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2 Does Inflation "Grease the Wheels of the Labor Market"?

David Card and Dean Hyslop

2.1 Introduction

One of the basic tenets of Keynesian economics is that labor market institutions tend to prevent nominal wage cuts—even in the face of high unemployment. An implication of this downward rigidity hypothesis is that inflation can ease labor market adjustments by speeding the decline in wages for individuals and markets buffeted by negative shocks.[1] According to this argument a modest level of inflation may serve to "grease the wheels" of the labor market and reduce frictional unemployment. In sharp contrast, an emerging orthodoxy among many economists and central bankers is that *stable* aggregate prices reduce labor market frictions and lead to the lowest possible levels of equilibrium unemployment.

In this paper we attempt to evaluate the evidence that relative wage adjustments occur more readily in higher-inflation environments. We focus on two types of evidence. First, at the individual level, we use panel microdata to examine the evolution of individual real wages over time.[2] According to the downward rigidity hypothesis, individual wage changes should exhibit significant asymmetries, with a greater degree of asymmetry, the lower the inflation rate. Second, at the market level, average wages in a local labor market should fall faster in response to a given negative shock in a high-inflation envi-

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1. This hypothesis is spelled out in Tobin 1972, for example.

2. Previous studies of the extent of nominal rigidity in individual wage data include McLaughlin 1994 and Kahn 1994. See also Lebow, Stockton, and Wascher 1995.

ronment than in low-inflation environments. This implies that the slope of the "cross-sectional Phillips-curve"—a graph of the relationship between market-specific real wage growth and the market-level unemployment rate—will be flatter in periods of low inflation, and steeper in periods of high inflation.

Our microlevel analysis is based on two complementary sources of data: rolling two-year panels constructed from matched Current Population Survey (CPS) files from 1979 to 1993, and multiyear panels from the Panel Study of Income Dynamics (PSID). The CPS provides relatively large and broadly representative samples, while the PSID provides better detail on job changing and enables us to examine the extent of nominal rigidity over longer time frames (one, two, and three years). Simple tabulations of both data sets lead to three basic conclusions. First, measured year-to-year changes in individual wages are quite variable, even for people who remain in the same job. In a typical year during the 1980s, 15–20% of non–job changers had measured nominal wage declines, and a similar fraction had nominal wage increases in excess of 10%.[3] Second, the *most likely* nominal wage change is zero: on average during the 1980s, about 15% of non–job changers report rigid nominal wages from one year to the next. Third, the fraction of workers with rigid wages is strongly negatively related to the inflation rate, with each percentage-point reduction in inflation leading to a 1.4 percentage-point increase in the incidence of nominal rigidity.

The presence of a large "spike" at zero in the distribution of measured nominal wage changes—or at minus the inflation rate in the distribution of real wage changes—leads to the question of what the distribution would look like in the absence of nominal wage rigidity. We use the simple assumption of symmetry to construct "counterfactual" distributions of real wage changes in the absence of rigidities. We then use the counterfactual distributions to measure the fraction of negative real wage changes "prevented" by nominal wage rigidities, and the net effect of nominal rigidities on average real wage growth. This exercise suggests that downward nominal rigidities in a typical year in the 1980s held up the real wage changes of workers by a maximum of about 1 percentage point per year.

Our market-level analysis uses state-level average wages and unemployment from 1976 to 1991. The wage data are constructed from the annual March CPS and are adjusted to reflect the varying composition of the workforce in each state in different years. Consistent with most of the recent literature on regional labor markets (e.g., Blanchflower and Oswald 1994, we find that local unemployment exerts a strong influence on local wage determination: real wages fall in states with higher unemployment (relative to national trends), while real wages rise in states with lower unemployment. However, we find little evidence that the rate of wage adjustment across local markets is faster in a higher-

3. Of course, some fraction of this measured variation is attributable to survey measurement error.

inflation environment. Taken in combination with our microlevel findings, these results imply that nominal rigidities have a small effect on the aggregate economy, and that any efficiency gains from the "greasing" effect of higher inflation are probably modest.

2.2 Descriptive Analysis of the Distribution of Individual Wage Changes

2.2.1 Data Sources

Our analysis of individual-level wage changes is based on information from two data sources that collectively span the period from 1976 to 1993. Our first source consists of the "merged monthly earnings files" from the 1979 to 1993 CPS. Each month, the CPS collects hourly or weekly earnings information from employed workers in the one-quarter of the sample frame who will not be interviewed in the next month.[4] One-half of this group (or approximately one-eighth of all wage and salary workers in the overall sample) will be interviewed again in twelve months and asked the same earnings questions. The other half were interviewed twelve months earlier and provided comparable earnings data at that time. By matching individuals from consecutive CPS samples it is therefore possible to construct a series of "rolling panels" with two years of wage information. A typical panel contains about 60,000 individuals, of whom roughly 50,000 report data on either their hourly or weekly wage in both years.[5]

For most of our analysis of the CPS data we restrict attention to the roughly 50% of individuals who report being paid by the hour in both years of the panel.[6] Ideally, since most models of nominal wage rigidity pertain to workers who stay in the same job, we would like to distinguish between individuals who changed employers and those who did not. Unfortunately, the CPS does not regularly collect information on job tenure or on the identity of specific employers. As a crude approximation, we distinguish between individuals who report the same (two-digit) industry and occupation in the two years, and those who report a change in industry or occupation.[7] Finally, in order to minimize the confounding effects that institutionally determined minimum-wage rates

4. The data pertain to the individual's main job as of the survey week, and are not collected for self-employed workers.

5. Details of the matching algorithm and other information on the CPS samples are presented in appendix 2A. We do not use imputed wage data that are allocated in the CPS files to nonrespondents.

6. This fraction is quite stable over the sample period. The advantage of using hourly-rated workers is that we can be sure their payment method is the same in both years. The CPS lumps all other payment periods (weekly, monthly, annual, and commission) into a single "other" category.

7. Many of the observed industry or occupation switches are presumably attributable to misclassification errors (see Krueger and Summers 1988). Changes in the industry and occupation coding system introduced between 1981 and 1983 necessitate slightly different procedures in these years—see table 2A.1, note a.

may have on the analysis of nominal rigidities, most of our analysis also excludes observations that are directly affected or potentially affected by minimum wage regulations.[8]

Our second source of data is the PSID. We constructed two four-year panels of wage observations from the PSID, for the period from 1976 to 1979, and from 1985 to 1988.[9] Although the PSID has far fewer observations than the CPS panels and tends to overrepresent certain groups (such as older workers), it has several other advantages that enhance its usefulness as a data source. First, individuals' wages and labor market experiences can be followed for several years in the PSID, while only consecutive-year matches are possible with the CPS. Second, the PSID questionnaire collects information on firm-specific (or job-specific) tenure, allowing us to draw a cleaner distinction between job movers and stayers.[10] Third, the PSID follows individuals who change addresses, while the CPS cross-sections can be matched only for people who remain at the same address. Finally, the PSID provides us with data from the mid-1970s, a period of high inflation that can be compared to the mid-1980s, when unemployment rates were similar but inflation rates were substantially lower.

2.2.2 The Distribution of Individual Wage Changes

We begin our analysis by presenting a series of histograms representing the distributions of year-to-year changes in real log hourly wage rates for the CPS and PSID samples described above. Figure 2.1 contains the histograms for the fourteen pairs of matched years from the CPS samples, based on wage changes for hourly-rated workers reporting the same industry and occupation in each year. For scale reasons we have censored the log real wage changes at ± 0.35: the masses at the upper and lower extremes represent the cumulative fractions in the respective tails of the distribution. A vertical line at minus the annual inflation rate ($-\pi_t$) is drawn for each year to identify the real wage change associated with fixed nominal wages.[11]

The histograms show that real wage changes tend to be centered around

8. DiNardo, Fortin, and Lemieux (1996) present evidence that minimum wages exert a major influence on the lower tail of the wage distribution. We consider a worker who is observed in periods $t - 1$ and t to be affected by the minimum wage if his or her wage is less than or equal to the contemporaneous minimum in either period. We consider a worker to be potentially affected if the wage in period $t - 1$ is below the minimum for year t.

9. We decided to use two separate panels of four years each, rather than a single panel of individuals who were in the PSID sample from 1976 to 1988, in order to reduce the attrition caused by changing household composition, labor force entry and withdrawal, and the aging and refreshing of the PSID sample.

10. Brown and Light (1992) note that the PSID tenure data contain errors that affect measured job changes. We adopt their recommended strategy of assuming that a job change has occurred whenever reported tenure is less than elapsed time since the previous interview.

11. Throughout the paper we measure inflation by the change in the logarithm of the CPI-U-X1. This series differs from the "official" CPI-U during 1979–82, since it uses a rental equivalence measure of housing cost comparable to the post-1982 CPI-U.

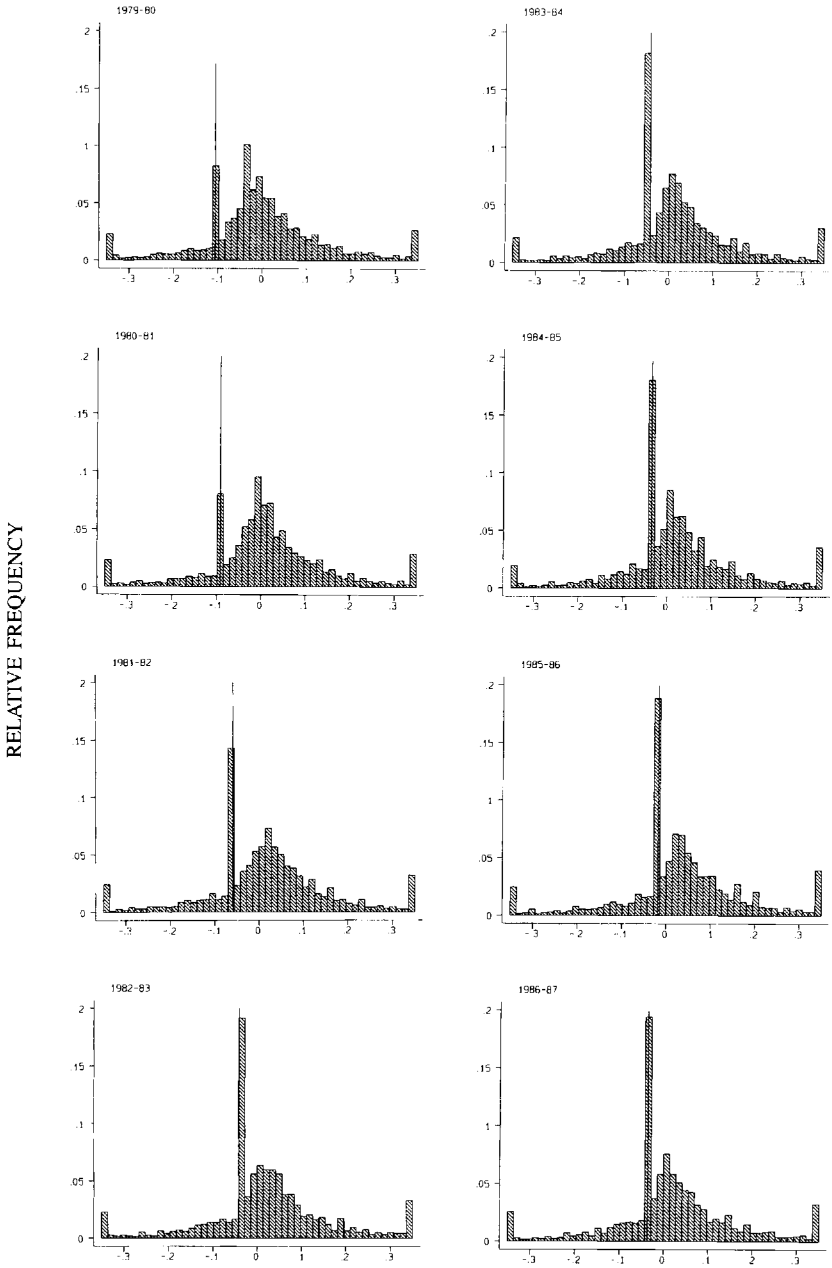
zero, with a prominent "spike" at $-\pi_t$ (i.e., at the point corresponding to fixed nominal wages). The size of the spike tends to be greater during periods of lower inflation: in the late 1970s when inflation was around 10%, the fraction of rigid nominal wages was 7–8%; in the mid to late 1980s, when inflation was at or below 5%, 15–20% of workers had constant nominal wages. Interestingly, it appears that there is a *deficit* in the distribution of wage changes to the left of $-\pi_t$, suggesting that the distribution of real wage changes is being "swept up" to the floor imposed by rigid nominal wages. Nevertheless, a considerable fraction of non–job changers report nominal wage cuts in any year—typically 15–20%.

Figure 2.2 presents the corresponding histograms of real wage changes for the PSID samples of hourly-rated workers in the same job in each year.[12] Despite some differences in the way the wage data are collected in the PSID and CPS surveys, and the more precise delineation of non–job changers in the PSID, the wage change distributions from the two data sources are fairly similar.[13] In particular, the PSID data also show a prominent spike in the distribution of real wages changes at $-\pi_t$. The spike is in the order of 10% during the high-inflation period 1976–79, and about 20% during the low-inflation period 1985–88. As in the CPS data, the wage change distributions in figure 2.2 show a deficit to the left of the spike, suggesting that the real wages of some workers who might otherwise experience nominal wage cuts are "held up" by downward rigidities.

Two earlier studies—by Kahn (1994) and McLaughlin (1994)—present comparable analyses of the extent of nominal rigidity in wage data derived from the PSID. Kahn uses data from 1970 to 1988 on non-self-employed household heads who have the same employer in consecutive years. Kahn's graphs of the distributions of wage changes are very similar to those presented in figure 2.2, leading her to conclude that there is significant downward nominal rigidity, and some evidence of "menu cost" effects (see below). McLaughlin uses data from 1976 to 1986 on household heads who report a wage or salary in consecutive years. Over this sample period he finds that about 7% of individuals have rigid nominal wages (see his figure 4). Nevertheless, McLaughlin concludes that there is little evidence of nominally induced asymmetries in the distribution of *real* wage changes. We believe that this conclusion arises from McLaughlin's decision to pool real wage changes from different years. As shown in figures 2.1 and 2.2, the spike in the distribution of real wage changes occurs at $-\pi_t$, which ranges from -2 to -11% in McLaughlin's

12. The measures of job tenure used in the two panels of the PSID differ: for the 1976–79 panel job tenure refers to the *position*, while for the 1985–88 panel it refers to the *employer.*

13. Appendix figure 2A.1 shows the distributions of wage changes for *all workers* in the PSID who report wages in each year—that is, including non-hourly-rated workers and those who change jobs. The patterns are similar to those in figure 2.2, except that the size of the spike is smaller—approximately one-half of the size observed for hourly-rated non–job changers—and there is more mass in the tails of the distribution.


RELATIVE FREQUENCY
1979-80
1983-84
1980-81
1984-85
1981-82
1985-86
1982-83
1986-87

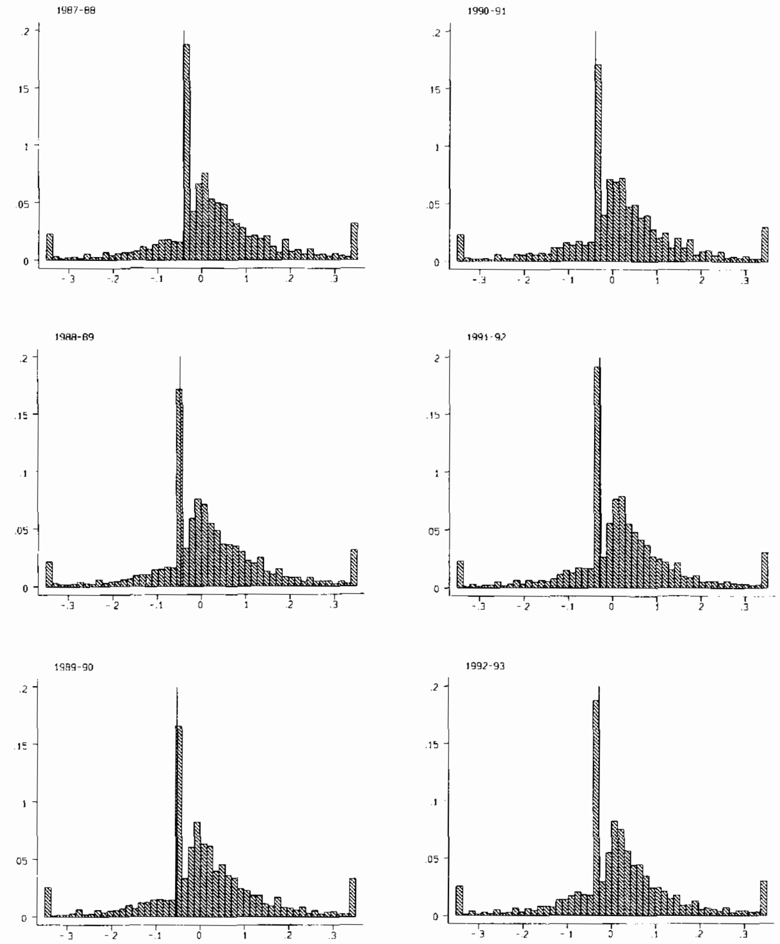


Fig. 2.1 Histograms of the distribution of log real wage changes, matched CPS samples from 1979–80 to 1992–93

sample. Pooling the data for different years thus obscures the spike in the real wage change distribution in any particular year.[14]

While most discussions of nominal wage rigidity implicitly focus on a yearly time frame, the degree of wage rigidity (either downward or upward) is clearly a function of the time horizon over which wage changes are measured. For example, we would expect to see a very high degree of nominal rigidity in week-to-week wage changes (at least in the U.S. labor market), but very little

14. Lebow, Stockton, and Wascher (1995) use PSID data for 1970–88 to measure rigidities among hourly- and non-hourly-rated workers. Their estimate of the fraction of workers with rigid nominal wages and nominal wage cuts is similar to ours.

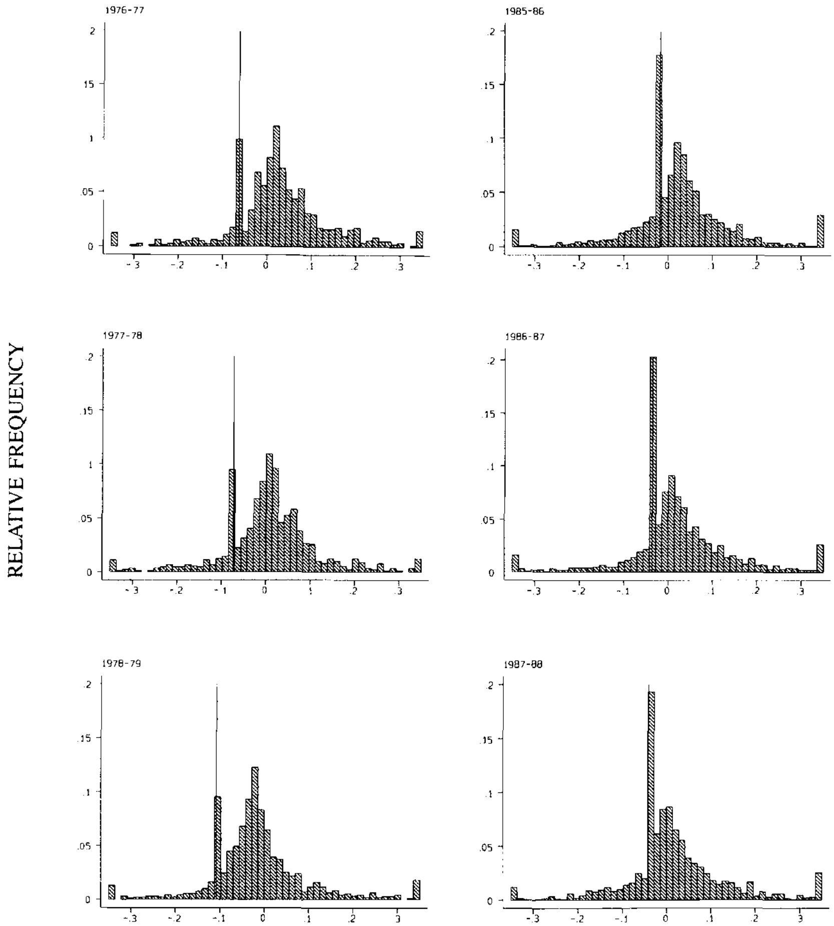


Fig. 2.2 Histograms of the distribution of log real wage changes, PSID samples 1976–79 and 1985–88, hourly-rated workers, same employer

rigidity in decade-to-decade wage changes. To get a sense of the effects of different time frames, figure 2.3 presents histograms of real wage changes over two- and three-year time horizons for hourly-rated workers in the PSID who remain with the same employer. These histograms have the same basic character as the year-to-year histograms in figure 2.2, although the magnitude of the spike corresponding to rigid nominal wages is smaller. During the low-inflation period 1985–88, about 10% of hourly rated non–job changers had constant wages over two years, compared with only 3% in the high-inflation period 1976–79. Over a three-year horizon, the fraction of observations with rigid wages is about 5% in the low-inflation era, and about 1% in the late 1970s. Some degree of nominal wage rigidity clearly persists more than a year.

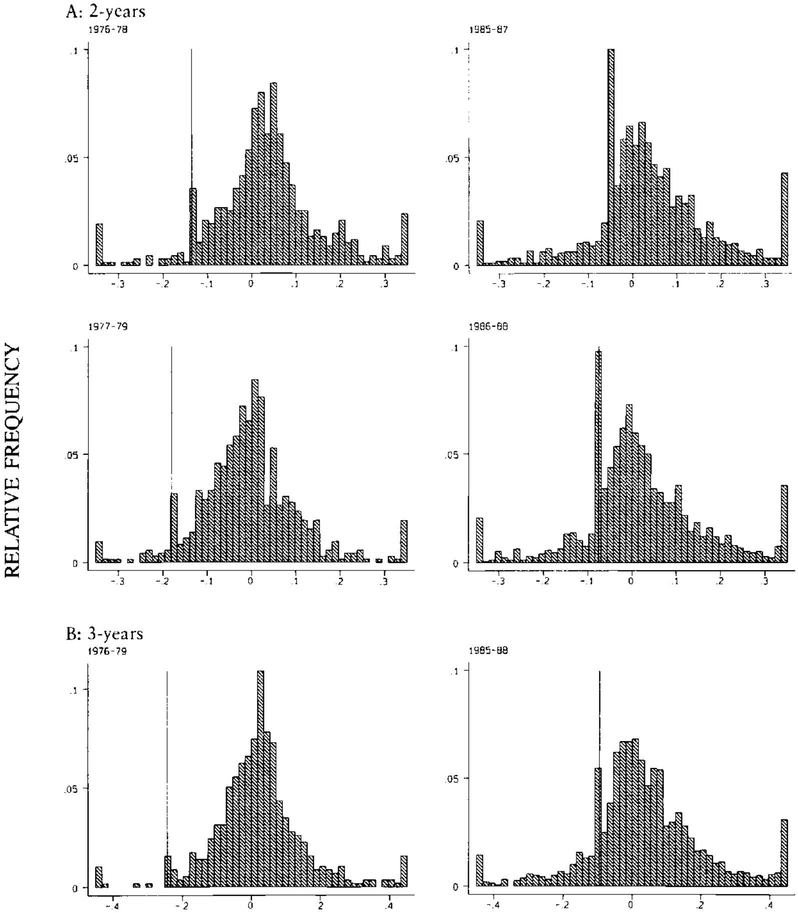


Fig. 2.3 Histograms of the distribution of log real wage changes, over two-year (*A*) and three-year (*B*) horizons, PSID samples, hourly-rated workers, same employer

Furthermore, long-term rigidity is more pervasive during low-inflation periods than during high-inflation periods.[15]

Tables 2.1 and 2.2 summarize some of the information contained in the histograms in figures 2.1–2.3. Table 2.1, which pertains to our CPS samples of hourly-rated workers, presents the annual inflation rate, the unemployment rate,[16] the median nominal wage change for all hourly-rated workers, the frac-

15. Appendix figure 2A.2 contains the histograms for two- and three-year wage changes for all workers from the PSID samples. These figures again show similar, although smaller, rigidity effects to those for hourly-rated non–job changers, closely matching the patterns for single-year wage changes.

16. Measured as the average unemployment rate during the ending year of each change.

Table 2.1 Characteristics of Wage Change Distributions in CPS Samples

	Aggregate Data		Median Nominal Wage Change	% of all Hourly Workers with[b]		% Rigid (exclude min. wage)[c]
	Inflation Rate[a]	Unemployment Rate		Nominal Cut	Rigid Wage	
1979–80	10.6	7.1	9.5	11.6	7.3	7.5
1980–81	9.1	7.6	9.4	12.1	7.2	7.8
1981–82	5.9	9.7	7.2	16.4	13.0	10.9
1982–83	4.1	9.6	4.9	17.7	17.1	14.8
1983–84	4.2	7.5	4.6	17.8	16.7	14.9
1984–85	3.5	7.2	4.4	18.4	16.4	15.2
1985–86	1.8	7.0	4.2	19.1	17.1	15.6
1986–87	3.6	6.2	4.1	19.1	17.3	16.1
1987–88	4.1	5.5	4.5	18.0	16.4	15.4
1988–89	4.7	5.3	4.7	17.2	15.5	14.8
1989–90	5.3	5.5	5.1	17.3	14.3	14.6
1990–91	4.1	6.7	4.9	18.2	14.9	15.7
1991–92	3.0	7.4	3.9	18.9	17.4	17.1
1992–93	2.9	6.8	3.6	20.3	17.1	16.6

Notes: Based on matched CPS samples. See text and appendix A for description of samples.

[a]Inflation rate is one hundred times the change in the log of the CPI-U-X1.

[b]Individuals who report being paid by the hour in both years, and who report the same two-digit industry and occupation in both years, except for 1982–83, 1983–84, and 1988–89. See table 2A.1, note a.

[c]Sample excludes individuals whose first-year wage does not exceed the minimum wage in either year, or whose second-year wage does not exceed the minimum wage in the second year.

tion of workers with measured nominal wage declines, and two estimates of the fraction of workers with zero nominal wage changes—one for all hourly-rated workers, and a second for the subsample of workers unaffected by minimum-wage regulations. Table 2.2 pertains to the PSID data, and shows the inflation rate and the fraction of workers with rigid nominal wages over one-, two-, and three-year time frames in the 1976–79 and 1985–88 periods. For comparison purposes we report both the overall fraction of workers with rigid nominal wages (columns 2 and 5), and the fraction of hourly rated non–job changers with rigid wages (columns 3 and 6).

Taken as a whole, we believe that the data in figures 2.1–2.3 and tables 2.1 and 2.2 present a reasonable prima facie case for the existence of downward wage rigidity for a significant fraction of workers. Although many non–job changers report nominal wage declines, the most likely outcome is for no change in nominal wages: between 6 and 17% report exactly the same nominal wage in one year as the next.[17] Furthermore, the extent of the rigidity is higher,

17. Note that any measurement error in wages is likely to lead to an overstatement of the probability of nominal wage declines and an understatement in the probability of rigid nominal wages. We consider the effects of measurement errors in more detail below.

Table 2.2 Characteristics of Wage Change Distributions in PSID Samples

Year	Inflation Rate[a] (1)	% Rigid All (2)	% Rigid Hourly[b] (3)	Year	Inflation Rate (4)	% Rigid All (5)	% Rigid Hourly[b] (6)
			One-Year Wage Changes				
1976–77	6.3	7.4	9.3	1985–86	1.8	8.8	15.6
1977–78	7.3	6.2	7.8	1986–87	3.6	10.1	16.5
1978–79	10.3	6.8	7.8	1987–88	4.1	10.6	16.0
			Two-Year Wage Changes				
1976–78	13.6	2.4	3.1	1985–87	5.4	4.7	7.9
1977–79	18.1	1.9	2.1	1986–88	7.6	5.3	8.4
			Three-Year Wage Changes				
1976–79	24.4	0.9	1.2	1985–88	9.5	2.8	4.7

Notes: The unemployment rates during the respective periods are 1977, 7.1%; 1978, 6.1%; 1979, 5.8%; 1986, 7.0%; 1987, 6.2%; 1988, 5.5%.

[a]Inflation rate is one hundred times the change in the log of the CPI-U-X1 over the relevant time period.

[b]Individuals who report being paid by the hour in the beginning and ending years, and report no change in "position" (1976–79) or "employer" (1985–88).

the lower the rate of inflation. A regression of the fraction of workers with rigid wages in table 2.1 on the inflation rate yields a coefficient of -1.39 ($t = 12.1$) with an R^2 coefficient of 0.92. This implies that each percentage-point decrease in the inflation rate increases the incidence of rigid wages among hourly-rated nonmovers by 1.4 percentage points. Finally, inspection of the histograms in figures 2.1–2.3 suggests that some of the mass at the rigid-wage spike represents workers who would have experienced even bigger real wage cuts in the absence of a nominal wage floor. In section 2.4 we present a more formal analysis of this issue. Before turning to this analysis, however, we consider two auxiliary questions: whether the extent of wage rigidity is systematically different for hourly-rated versus other workers; and whether the extent of measured nominal rigidity is affected by the tendency for workers to "round" their reported wages.

2.3 Is the Extent of Nominal Rigidity Overstated?

2.3.1 Hourly-Rated versus Other Workers

All of the CPS data analyzed in the last section, and most of the PSID data, pertain to workers who report that they were paid by the hour. In the matched CPS samples, however, only about one-half of workers report that they are paid by the hour in both the beginning and end years.[18] This raises the question of whether measures of nominal rigidity based on hourly-rated workers are representative of the overall labor force.

To get some evidence on this issue, we examined changes in reported weekly earnings for individuals in the CPS samples who reported being non-hourly-rated in both years of our two-year panels.[19] The results of this analysis suggest that the incidence of rigid nominal wages is slightly *higher* for non-hourly-rated workers. For example, between 1979 and 1980, 7.4% of "always hourly-rated" workers with no change in industry or occupation had rigid nominal wages, versus 10.9% of "always non-hourly-rated" workers. Similarly, between 1987 and 1988 16.4% of "always hourly-rated" workers had rigid wages, versus 18.4% of "always non-hourly-rated" workers. There are some other differences between the distributions of real wage changes for hourly-rated and non-hourly-rated workers. Most noticeably, the dispersion in real wage changes for non-hourly-rated workers tends to be larger: the interquartile range of the change in real weekly pay for non-hourly-rated workers with the same industry and occupation is about 25–50% higher than the interquartile range of the change in real hourly pay for hourly-rated workers with the same industry and occupation. We suspect that the measurement errors in weekly pay for non-hourly-rated workers are larger than the errors in hourly pay for hourly-rated workers, in part because workers are asked to report their "usual" weekly pay rather than a "straight-time" earnings measure. In any case, there is no evidence that nominal wage rigidity is lower for non-hourly-rated workers, and for simplicity we therefore confine our attention to hourly-rated workers in the remainder of this paper.

2.3.2 Rounding of Wages and the Incidence of Measured Rigidities

One of the most prominent features of observed wage distributions is the tendency for workers to report "rounded" wage amounts, like $5.00 per hour, or $7.50 per hour. Among hourly-rated workers in our matched 1984–85 CPS file, for example, 34% reported an even dollar wage amount in 1984, and another 14% reported a wage rate ending in 0.50. If some or all of this phenomenon is due to systematic rounding (or "heaping") of data drawn from an underlying continuous distribution, then one explanation for measured nominal wage rigidity is that individuals with small nominal wage changes tend to report the same rounded wage amount in consecutive surveys. A simple tabulation of the probability of zero nominal wage growth by the initial level of wages reveals some support for this hypothesis. In the 1984–85 CPS file 24.1% of individuals who reported an even wage amount in 1984 had rigid nominal

18. The fraction is similar for workers who report the same industry and occupation in both years and are therefore classified as non–job changers.

19. In principle we can construct an hourly wage for non-hourly-rated workers by dividing usual weekly earnings by usual weekly hours. However, any measurement error in reported hours will lead to excessive volatility in imputed hourly wages.

wages between 1984 and 1985, versus a rigidity rate of only 9.2% for individuals who reported a wage amount not ending in either .00 or .50. In our matched CPS samples, individuals who reported an even dollar wage amount in the base year typically account for 55–60% of all those with rigid nominal wages.

The interpretation of these facts, however, depends crucially on the underlying explanation for spikes in the distribution of wages at dollar and fifty-cent intervals. If the *true* wage distribution contains spikes, and employees are more likely to report their true wage if it is an easily remembered amount like $5.00 or $7.50 per hour, then the measured rigidity rate for individuals who report an even wage may be a better estimate of the true rate of nominal rigidity than the overall rigidity rate for all wage earners. Some support for this hypothesis comes from the fact that the residual variance of a conventional wage equation is slightly *lower* when the model is fit to the subsample of workers who report a rounded wage amount than when the same model is fit to workers who report a wage that does not end in .00 or 50.[20] This evidence suggests that the noise in measured wages is lower for workers who report a rounded wage, contrary to the view that rounding is purely a result of measurement error.

To further explore this issue we used data from a January 1977 CPS validation study that collected self-reported wage information from workers and matching information from their employers (see Card 1996 for more information on this survey). Among hourly-rated workers paid above the minimum wage, the probability of a rounded wage (ending in either .00 or .50) is 30%—somewhat below the rate of 38% in our matched 1979–80 CPS sample.[21] The probability that the *employer* reports a rounded wage is lower (20%) but is far from negligible. Overall, 44% of employers and employees report exactly the same wage, with a significantly higher agreement rate (69%) conditional on the employer's reporting a rounded wage. Treating the employer reports as truth, these data imply that about one-half of the observed mass at rounded wage values is attributable to spikes in the true distribution of wages, with the other half attributable to rounding errors.[22]

To get an indication of the potential contribution of rounding behavior to measured rigidity rates, we decided to perform a simple simulation. In the

20. Specifically, we fit a model to the log hourly wage for hourly-rated workers in our pooled CPS files who report a wage ending in .00 or .50 and for those with other wages. The explanatory variables included education, a gender-specific cubic in experience, nonwhite and female dummies, and indicators for region and year. The residual standard error is slightly lower in the model for rounded wage observations than in the model for nonrounded observations. A similar finding holds by year.

21. The fraction of wages reported at even dollar or half-dollar amounts rose over the 1980s from 38% in 1979 to 48% in 1984 to 56% in 1992. We suspect that this trend may be due in part to inflation: at higher nominal wage levels, the percentage difference between "rounded" wage amounts is smaller, implying less "cost" to paying a "rounded" wage amount, and/or a smaller error in reporting a "rounded" amount.

22. Specifically, if 20% of employers report a rounded wage, and 69% of workers whose employer reports a rounded wage report the same wage, then 14% ($= 0.20 \times 0.69$) of workers report a "true" rounded wage.

simulation we assume that individual wage changes are generated from a continuous distribution, and that individuals have some probability of reporting either their true wage, a rounded wage, or their true wage plus a measurement error.[23] For plausible values of the parameters, the simulation implies that rounding generates a 4–5% rate of apparent nominal wage rigidity when the inflation rate is 5% and there is zero median wage growth. We believe this is an upper bound on the fraction of observed nominal rigidity that can be attributed to rounding behavior. If some of the observed rounding is due to spikes in the true distribution of wages at even wage amounts, or if the probability of reporting a rounded wage is less persistent over time than we have assumed, then the share of observed wage rigidity attributable to rounding is smaller.

An important feature of rounding behavior is its symmetry. Provided that individuals round their wages to the nearest even amount, rounding causes nominal wage changes *above and below* zero to be drawn toward zero. In this regard, rounding by employees is similar to "menu costs" that cause employers not to adjust wages if the optimal wage adjustment is small. By comparison, downward nominal rigidities exert an asymmetric effect on workers who would otherwise experience a nominal wage cut. In the next section we show how the symmetric effect of rounding or related phenomena can be used to empirically distinguish the contribution of downward rigidities to the total measured rigidity rate.

2.4 Measuring the Effect of Inflation on Wage Rigidities

2.4.1 Conceptual Framework

Suppose that in the absence of rigidities the distribution of real wage changes would be continuously distributed with some mean m. In the presence of rigidities, suppose that some individuals whose nominal wages would otherwise fall experience zero wage growth. This scenario is illustrated in figure 2.4A under the assumptions that $m = 0$, that the inflation rate π is 5%, and that one-half of individuals who would otherwise experience a negative real wage change are affected by downward rigidities. As illustrated by the figure, the net effect of downward nominal rigidity is to produce a *deficit* in the left-hand tail of the distribution of real wage changes (below $-\pi$) and a spike in the distribu-

23. In the simulation we assume that individual log wages are normally distributed according to a stationary autoregressive model, and that measured wages are generated as follows: with some probability (p_1) a worker reports the true wage; with some probability (p_2) the worker rounds the wage to the nearest even 50-cent amount; and with some probability ($1 - p_1 - p_2$) the worker reports the true wage plus a (normally distributed) random measurement error. We calibrated the model by fixing the cross-sectional standard deviation of true log wages and the correlation of true log wages across years at 0.45 and 0.95, respectively. We set $p_1 = p_2 = 0.45$ and assumed that three-quarters of individuals who round their wage report in one year also round their report in the next year.

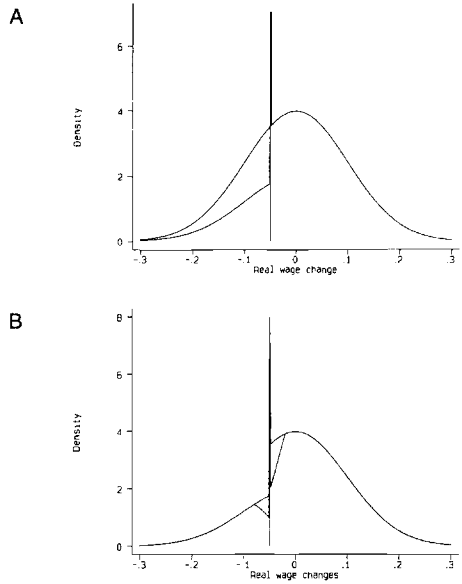


Fig. 2.4 Theoretical effects on the distribution of real wage changes. *A*, downward nominal rigidities and, *B*, downward nominal rigidities and menu costs

tion at $-\pi$.[24] It is easy to see that as the inflation rate falls (i.e., as $-\pi$ moves to the right) the effect of nominal rigidity becomes more pronounced.

A second source of nominal wage rigidity that we will attempt to separately identify is that due to menu costs or rounding in reported wage levels. For example, suppose that if the "optimal" nominal wage change is between $\pm x\%$, then there is some probability that the nominal wage will not change. Figure 2.4B illustrates this scenario when menu costs are present for wage changes of up to $\pm 2\%$, and the probability of nonadjustment declines symmetrically from 25% for a zero wage change to 0 for a 2% nominal wage change. To the extent that the density is not constant around $-\pi$, this assumption implies that menu costs induce *asymmetric* deficits in the observed distribution of real wage changes on either side of $-\pi$: if $-\pi$ lies in the left-hand tail of the distribution, there will be a larger menu-cost deficit to the right of $-\pi$ than to the left. If both downward rigidities and menu costs are present, we would expect to see a deficit in the distribution of real wage changes immediately to the left of $-\pi$, a somewhat larger deficit to the right of $-\pi$, and a spike at $-\pi$ that is larger

24. Note that if the effect of the rigidities is translated entirely into quantity effects (i.e., unemployment) there will be no spike. However, the deficit in the left-hand tail of the distribution of *observed* wage changes will exist regardless of this possibility.

than the "deficit" to the left of $-\pi$ (by the amount of the deficit to the right of $-\pi$). In principle, if the fraction of underlying wage changes that have been *shifted down* to zero can be estimated, then this fraction, suitably adjusted to take account of the different density on either side of the spike, can be subtracted from an estimate of the fraction of underlying wage changes that have been *shifted up* to zero to obtain an estimate of the net effect of downward rigidities.

2.4.2 Identifying a Counterfactual Wage-Change Distribution

The key issue in estimating the effect of nominal wage rigidities is the identification of a "counterfactual" distribution—a model for the distribution of real wage changes in the absence of downward wage rigidities and menu costs. The counterfactual that we adopt in this paper is based on the following three assumptions: (1) in the absence of rigidities, the distribution of wage changes would be symmetric; (2) the upper half of the distribution of observed wage changes is unaffected by rigidities; and (3) wage rigidities do not affect employment probabilities. Under these assumptions, the upper half of the distribution of observed wage changes can be used to infer what the lower half would have looked like in the absence of rigidities.

Although there is no *a priori* reason for imposing assumption 1, we believe that symmetry is a natural starting point for building a counterfactual distribution. Moreover, most conventional models of wage determination imply symmetry. For example, if real wage outcomes in consecutive periods are jointly normally distributed, or if the individual wage determination process is stationary, then symmetry holds.[25] An alternative approach, pursued by Kahn (1994), is to use the observed distribution of wage changes in other periods to infer the counterfactual in the absence of rigidities. An important objection to this alternative is that the *dispersion* of wage changes may be affected by inflation. Thus in this paper we rely on the symmetry assumption.

The second assumption, that wage changes above the median are unaffected by downward rigidities, may seem relatively innocuous. However, the presence of measurement errors in wages may lead downward nominal rigidities to exert some influence on the upper half of the observed wage-change distribution. Specifically, let Δw_t^* represent the true wage change of a given worker from period $t - 1$ to t, and let

$$\Delta w_t = \Delta w_t^* + \Delta u_t$$

represent the measured wage change, where Δu_t is the measurement error in wage growth. Suppose that Δu_t is symmetric with median zero. Then if the distribution of true wage changes Δw_t^* is *asymmetric* (as implied by the down-

25. At least for workers in middle age, the assumption of stationarity may be appealing. If the process generating w_{it}, the real wage of individual i in period t, is stationary, then $w_{it} - w_{it-1}$ has the same distribution as $w_{it-1} - w_{it}$, implying that wage changes are symmetric.

ward rigidity hypothesis) the median of observed wage changes will not necessarily equal the median of Δw_t^*. Indeed, if Δw_t^* has the shape illustrated in figure 2.4a, then the median of observed wage changes will tend to exceed the median of Δw_t^*.[26] We return to this issue in more detail below.

The third assumption is perhaps the most problematic. Indeed, since much of the interest in downward nominal wage rigidity is driven by a concern over potential employment effects, the assumption that any employment effects may be ignored is troubling. One way to relax assumption 3 is to assume (3′) a fraction 2α of jobs that would otherwise be observed—all associated with nominal wage changes below the median—are lost due to nominal wage rigidities. In this case, a counterfactual distribution can be constructed by taking the observed distribution of wage changes beyond the $0.5 - \alpha$ quantile, and building a symmetric lower tail. For example, if 2% of continuing jobs are lost because of downward wage rigidities, then an appropriate counterfactual is the symmetric distribution constructed from the observed distribution to the right of the 49th percentile. In the analysis below, we also construct such a "49th percentile counterfactual" distribution and derive summary statistics from this, as a robustness check on the results from the "median" counterfactual.[27]

Formally, let $f(x)$ denote the probability density function of *observed* real wage changes in some period (for some given sample of workers). Let $\tilde{f}(x)$ denote the counterfactual density function. Then assumptions 1–3 or 1–3′ imply

$$\tilde{f}(x) = k_c \cdot f(x), \qquad x \geq c;$$
$$\tilde{f}(x) = k_c \cdot f(2c - x), \qquad x < c,$$

where k_c is a constant and c is the point of symmetry. Under assumption 3, c is equal to the median observed wage change, while under assumption 3′, c is equal to the $0.5 - \alpha$ quantile. Using the fact that $\tilde{f}(x)$ must integrate to 1, it is easy to see that $k_c = 0.5/(1 - F(c))$, where F is the distribution function associated with f. Note that if $c = m$ (the observed median) then $F(c) = 0.5$ and $k_c = 1$. Otherwise, if c is the $0.5 - \alpha$ quantile, then $k_c = 1/(1 + 2\alpha) \approx 1 - 2\alpha$.

2.4.3 Measuring the Effects of Rigidities

Given an observed distribution of real wage changes and a particular counterfactual distribution, it is possible to develop a variety of measures of the effect of nominal rigidities. We focus on two simple summary statistics: a mea-

26. Intuitively, measurement errors smear some of the true mass at $-\pi_t$ to the left and right of the spike. Any measurement errors larger than π_t will therefore displace a nonzero mass to the right of the median of Δw_t^*.

27. An alternative is to construct the counterfactual distribution by imposing symmetry around the mode of the distribution of observed wage changes. This is equivalent to assuming that, in the absence of rigidities, the wage-change distribution would be symmetric with median equal to the mode. We tried this approach, but found that the resulting counterfactual distribution is extremely sensitive to the location of the mode. Also, in several years the mode is above the median, which would imply job gains, rather than job losses, from nominal rigidities.

sure of the fraction of people whose wages are affected by rigidities, and a measure of the net effect of rigidities on the average wage change.

Density Effects

In principle, nominal wage rigidities can affect workers whose wages would have fallen in the absence of rigidity, and people whose wages would have otherwise risen. Thus, we decompose the fraction of workers affected by rigidities into an estimate of the fraction whose wages were "held up," and an estimate of the fraction whose wages were "held down." The former is the cumulative density of the counterfactual distribution that has been "swept up" to the nominal wage rigidity spike (at $-\pi_t$):

$$(1) \qquad su_t = \int_{-\infty}^{-\pi_t^-} (\tilde{f}(x) - f(x))\, dx = \tilde{F}(-\pi_t^-) - F(-\pi_t^-),$$

where the upper limit of integration ($-\pi_t^-$) excludes the mass point at $-\pi_t$, and $\tilde{F}(x)$ and $F(x)$ are the cumulative distribution functions corresponding to $\tilde{f}(x)$ and $f(x)$ respectively. The latter is the cumulative density of the counterfactual distribution that has been "swept back" to the nominal-wage rigidity spike:

$$(2) \qquad sb_t = \int_{-\pi_t^+}^{m_t} (\tilde{f}(x) - f(x))\, dx = (\tilde{F}(m_t) - \tilde{F}(-\pi_t^+)) - (F(m_t) - F(-\pi_t^+)),$$

where m_t is the median real wage change in year t, and the lower limit of integration ($-\pi_t^+$) excludes the mass point at $-\pi_t$. (Note that by assumption 2 above, we need only extend the upper limit of integration to the median.) The total fraction of individuals affected by rigidities is $su_t + sb_t$, which is equal to the mass at the spike point (suitably normalized, if the point of symmetry for the construction of the counterfactual density is not equal to the median).

If estimates of $F(x)$ and $\tilde{F}(x)$ are available, then su_t and sb_t can be evaluated directly.[28] In the absence of any menu costs or "rounding," su_t provides an esti-

28. Alternatively, using the definition of the counterfactual density, it is easy to show that

$$(1') \qquad su_t = k_c \cdot (1 - F(2c + \pi_t)) - F(-\pi_t^-),$$

where F is the distribution function of observed wage changes in year t, c is the point of symmetry for the counterfactual, and k_c is the constant defined earlier. This expression can be evaluated directly using the empirical distribution function for observed real wage changes. If c is set to the median real wage change in year $t(m_t)$, this expression simplifies to $su_t = (1 - F(2m_t + \pi_t)) - F(-\pi_t^-)$, and if $m_t = 0$ (which is roughly true for most of our sample years) then $su_t = (1 - F(\pi_t)) - F(-\pi_t^-)$, which represents a simple difference between the fraction of real wage changes *above* π_t and the fraction *below* $-\pi_t$. Similarly, the fraction of the density swept back can be written as

$$(2') \qquad sb_t = k_c \cdot (F(2c + \pi_t) - F(2c - m_t)) - (F(m_t) - F(-\pi_t^+)),$$

which, if the point of symmetry is set to the median, reduces to $sb_t = F(2m_t + \pi_t) - .5 - (.5 - F(-\pi_t^+))$, or to $sb_t = F(\pi_t) - .5 - (.5 - F(-\pi_t^+))$, if $m_t = 0$. This last expression is simply the fraction of observed wage changes between π_t and the median minus the fraction between the median and $-\pi_t$.

mate of the fraction of workers affected by downward wage rigidities. In the presence of menu costs or rounding, however, su_t will tend to overstate the effect of downward rigidities. Nevertheless, if menu costs affect an equal fraction of workers who otherwise would receive small nominal increases and decreases (as assumed in figure 2.4b), then the *net* sweep-up $su_t - sb_t$ provides a lower-bound estimate of the fraction of workers affected by downward nominal wage rigidity. To see why, notice that the counterfactual density to the right of $-\pi_t$ is bigger than the counterfactual density to the left. Thus if equal *fractions* of the counterfactual are affected by menu costs, the total density swept back to $-\pi_t$ by menu costs (measured by sb_t) will exceed the total density swept up to $-\pi_t$ by menu costs.

Wage Effects

In constructing a measure of the effect of nominal rigidities on average wage growth, we similarly distinguish between the effect for individuals whose wages are "held up" by rigidities and the effect for those whose wages are "held back." The effect on the former group is

$$(3)\qquad \begin{aligned} wsu_t &= \int_{-\infty}^{-\pi_t^-} (\tilde{f}(x) - f(x))(-\pi_t - x)\,dx \\ &= -\pi_t su_t - E(\Delta w | \Delta w < -\pi_t; \tilde{f}) \times \tilde{F}(-\pi_t^-) \\ &\quad + E(\Delta w | \Delta w < -\pi_t; f) \times F(-\pi_t^-), \end{aligned}$$

which we refer to "wage sweep-up," while the effect on the latter group is

$$(4)\qquad \begin{aligned} wsb_t &= -\int_{-\pi_t^+}^{m_t} (\tilde{f}(x) - f(x))(-\pi_t - x)\,dx \\ &= \pi_t sb_t + E(\Delta w | -\pi_t < \Delta w \leq m_t; \tilde{f}) \times (\tilde{F}(m_t) - \tilde{F}(-\pi_t^+)) \\ &\quad - E(\Delta w | -\pi_t < \Delta w \leq m_t; f) \times (F(m_t) - F(-\pi_t^+)), \end{aligned}$$

which we refer to as "wage sweep-back." Again, if estimates of the densities $f(w)$ and $\tilde{f}(x)$ are available, these expressions can be evaluated directly. Alternatively, they can be estimated using estimates of the fractions of individuals in various wage-change intervals, and the mean wage change within these intervals.[29]

29. Specifically, using the definition of the counterfactual density, it is straightforward to show that

$$(3')\qquad \begin{aligned} wsu_t = k_c \cdot (1 - F(2c + \pi_t) \cdot \{E(\Delta w | \Delta w \geq 2c + \pi_t) - \pi_t\} \\ - F(-\pi_t^-) \cdot \{-\pi - E(\Delta w | \Delta w \leq -\pi_t)\}, \end{aligned}$$

where the expectations are taken with respect to the actual distribution of wage changes. This expression can be evaluated using estimates of the fractions of real wage changes in the upper and lower tails of the observed wage-change distribution and estimates of the conditional mean wage changes in the two tails. A similar expression can be developed for wsb_t in terms of the fractions of wage changes in the intervals $[-\pi_t^+, c]$ and $[c, 2c + \pi_t]$, and the mean wage changes within these intervals.

Effects of Measurement Error

The nominal rigidity measures developed in equations 1–4 implicitly ignore any errors in reported wages. Random measurement errors will have several effects on the observed distribution of wage changes relative to the true underlying distribution. Most notably, the observed fraction of workers with rigid wages will be *lower* than the true fraction. In particular, assuming that the observed wage in period t w_t differs from the actual wage w_t^* by an error u_t, the observed wage change is

$$\Delta w_t = \Delta w_t^* + \Delta u_t.$$

If the distribution of true wage changes is continuous, apart from a spike at $-\pi_t$, only individuals with truly rigid wages who accurately report their wage change contribute to observed rigidity. The fraction of individuals with observed wage rigidity is therefore

$$P(\Delta w_t = 0) = R \times P(\Delta w_t^* = 0),$$

where $R = P(\Delta u_t = 0 | \Delta w_t^* = 0)$ is the probability of accurately reporting the true wage change, conditional on rigid wages. We are unaware of any direct estimates of R. However, evidence from the January 1977 CPS validation survey provides an indication of the magnitude of this probability. In that survey 44% of hourly-rated workers report exactly the same wage as their employers report. Treating the employers' reports as error free, this estimate suggests that R lies between 0.2 ($=0.44^2$) and 0.44, depending on the persistence in individuals' probabilities of making an error-free wage report.[30] If employers have about the same probability of making an erroneous wage report as employees, however, then this estimate suggests a range for R between 0.44 and 0.66 ($=0.44^{1/2}$), again depending on the persistence in the likelihood of making an error-free wage report. These estimates suggest that the observed fraction of rigid wages may understate the true rigidity rate by 30–80%.

A second implication of measurement error is that the observed distribution of wage changes will tend to show less evidence of menu costs than the true distribution. Specifically, suppose that with probability R individuals report their true wage change, and with probability $(1 - R)$ they report their true wage change plus a continuously distributed measurement error Δu_t. Then a fraction $(1 - R)$ of the true mass at $-\pi_t$ is transformed into a distribution of observed wage changes centered on $-\pi_t$ with the density function of Δu_t. Assuming that Δu_t has a "bell-shaped" distribution, this will add relatively more mass to the observed distribution just to the left and right of $-\pi_t$, partially "filling in" any deficit created by menu costs or rounding effects.

30. If the same individuals provide an error-free wage report in consecutive years, then the probability of an error-free wage change is 0.44. If the probability of an error-free wage report is independent over time, then the likelihood of an error-free change is 0.44^2.

A third implication of measurement error, mentioned above, is that nominal rigidities in the lower half of the wage-change distribution may spill over to the upper half, leading to a violation of the assumption that observed wage changes above the median are unaffected by rigidities. In particular, the addition of a symmetric measurement error to a right-skewed distribution of true wage changes, such as illustrated in figure 2.4A, will tend to lead to a measured median above the true median wage change.

Figure 2.5 displays the qualitative effects of measurement error on the observed distribution of wage changes. As illustrated in the figure, reporting errors attenuate the magnitude of the spike in the observed distribution at $-\pi_t$, while adding "shoulders" to either side of the spike. In the figure some of the displaced mass spills over above the median, causing an upward bias in the observed median relative to the true median.

To get some idea of the quantitative effect of measurement errors on the accuracy of our rigidity measures, we performed a series of simulations in which we added measurement errors to a distribution of true wage changes like the one in figure 2.4B and then formed estimates of *su, sb, wsu,* and *wsb.* A complete description of the simulations is presented in appendix B, with a table showing the actual and estimated levels of sweep-up (*su*), sweep-back (*sb*) and wage sweep-up (*wsu*). Although limited in scope, the simulations show that the addition of measurement error leads to *downward* biases in our estimates of downward rigidity effects. The estimates of wage sweep-up, for example, are downward biased by 10–30% under a plausible range of assumptions.

2.4.4 Kernel Density Estimates of the Actual and Counterfactual Distributions

As a preliminary step in describing the extent of nominal rigidities in our CPS and PSID samples, we used standard kernel estimation techniques to construct smoothed estimates of the densities of real wage changes, and corresponding estimates of the counterfactual densities. In contrast to simple histograms, which can display irregular "jumps," kernel density methods compute a weighted average of the density *near to* each point. In particular, the kernel estimator for the density at some value x is

$$\tilde{f}(x) = \frac{1}{nh}\sum_{i=1}^{n} K\left(\frac{x - x_i}{h}\right),$$

where n is the number of observations, h is a *bandwidth* parameter (sometimes called the *window width*), and $K(\cdot)$ is a kernel or weighting function, which integrates to 1 over the range of x.[31] The smoothed kernel estimates give a

31. Silverman (1986) provides a full treatment of the issues involved with density estimation. We estimate each of the densities of 250 equispaced points (x) in the range (-0.35, 0.35) using an Epanechnikov kernel and a fixed bandwidth, h = 0.005. We also tried other bandwidths and found that the resulting distributions were qualitatively similar.

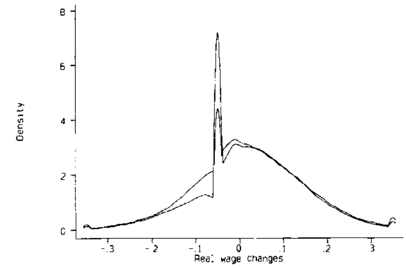


Fig. 2.5 Theoretical effect of measurement error on the distribution of real wage changes in the presence of menu costs and downward rigidities

clearer picture of the differences between the actual and counterfactual distributions of wage changes than can be obtained using simple histograms.

The actual and median-counterfactual densities for the CPS samples are shown in figure 2.6. As is true of the simple histograms in figure 2.1, the smoothed densities of the observed data show noticeable spikes at the point corresponding to rigid nominal wages (i.e., at minus the inflation rate), with a larger spike in years with lower inflation rates. A comparison of the actual and counterfactual distributions shows a deficit in the left tail of the actual distribution, and a small but typically noticeable deficit to the right of the spike point. These two characteristics are consistent with the stylized graph in figure 2.4B. The observed data seem to show both downward nominal rigidity effects *and* the presence of menu costs associated with small wage changes.

To better pinpoint the differences between the actual and counterfactual distributions, figure 2.7 presents graphs of the cumulative deviation between the two distributions at each point up to the median. For each wage change below the median, we compute the fraction of the actual distribution "missing" from the counterfactual distribution between that point and $-\pi_t$. Specifically, for each point below the spike (i.e., for each wage change $\Delta w < -\pi_t$), we estimate

$$G(\Delta w) = \frac{\int_{\Delta w}^{-\pi_t^-} (\tilde{f}(x) - f(x))\, dx}{\int_{\Delta w}^{-\pi_t^-} \tilde{f}(x)\, dx}.$$

Similarly, for each point between the spike and the median (i.e., for each wage change $-\pi_t < \Delta w < m_t$), we estimate

$$G(\Delta w) = \frac{\int_{-\pi_t^+}^{\Delta w} (\tilde{f}(x) - f(x))\, dx}{\int_{-\pi_t^+}^{\Delta w} \tilde{f}(x)\, dx}.$$

In practice, we set the limits of integration around the spike point to be $-\pi_t^- = -\pi_t - 0.0025$ and $-\pi_t^+ = -\pi_t + 0.0025$. If nominal rigidities prevent some individuals' real wages from falling faster than the inflation rate, then $G(\Delta w)$ will be positive for all $\Delta w < -\pi_t$. Indeed, in the simple case where a fixed fraction f of real wage declines bigger than $-\pi_t$ are prevented, $G(\Delta w)$ will equal f. Similarly, to the extent that menu costs prevent some individuals' nominal wages from rising, $G(\Delta w)$ will be positive for all $-\pi_t < \Delta w < m_t$.

In figure 2.7 we have graphed the estimated $G(\Delta w)$ functions for each year after renormalizing the real wage changes in a particular year relative to the spike point. That is, we graph $G(\Delta w + \pi_t)$, which is equivalent to graphing the deficits in the distributions of *nominal* wage changes. Inspection of the graphs suggests that in most years $G(\Delta w)$ is roughly constant for Δw in the left-hand tail of the distribution, and in the range from one-quarter to one-half; below, but near to, $-\pi_t$ the fraction displaced shows a sharp increase to one-half or more; and above $-\pi_t$ $G(\Delta w)$ falls off steadily from about one-half. These patterns suggest that a substantial fraction of wages are affected by downward nominal rigidity, and that, near to zero nominal change, menu costs may account for at least one-half and perhaps more of observed rigidity.

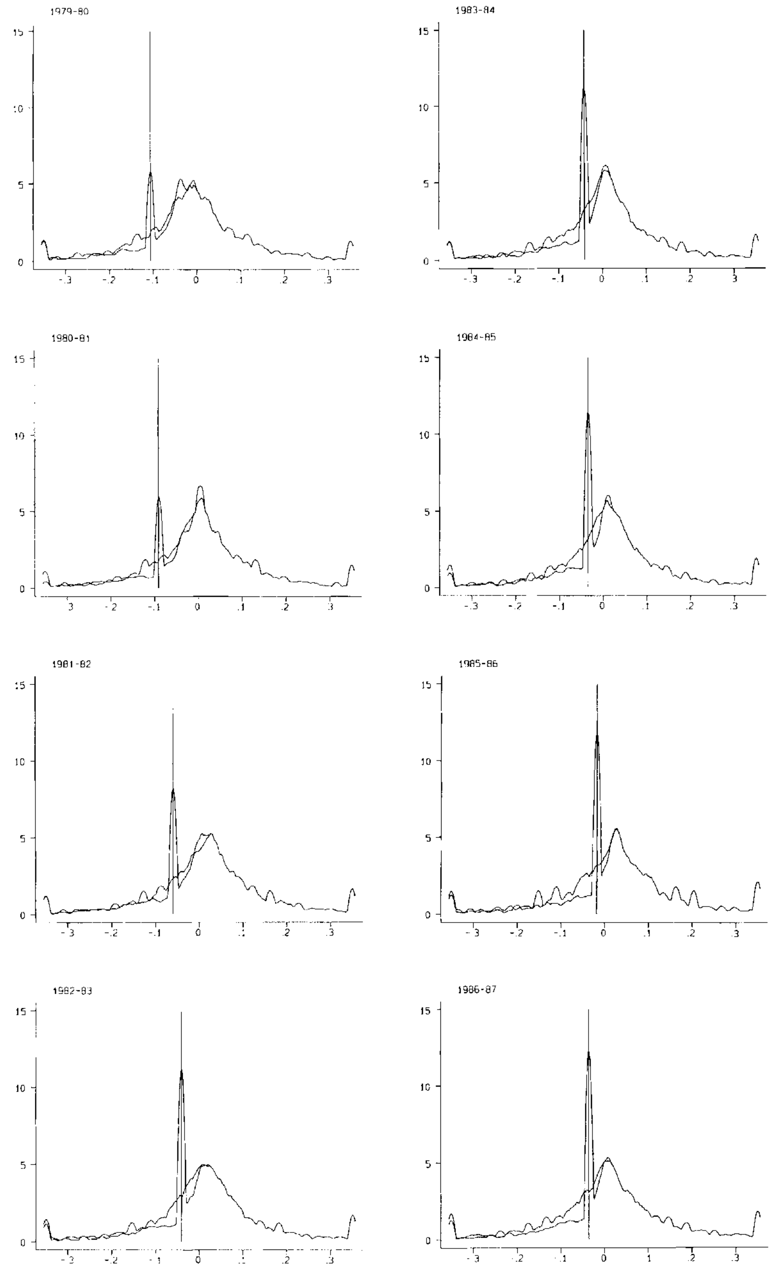
2.4.5 Estimates of the Effects of Nominal Rigidities

Tables 2.3 and 2.4 present estimates of the four summary measures of the effect of nominal wage rigidity (su_t, sb_t, wsu_t, wsb_t) defined by equations 1–4, using our CPS samples of hourly-rated non–job changers. In implementing the formulas we restrict the upper and lower limit of integration for real wage changes to ± 0.3, in order to reduce the effect of any outliers in the extreme tails of the wage-change distributions. Table 2.3 contains estimates of the density displacement effects su_t and sb_t for two choices of the point of symmetry: the median real wage change, and the 49th percentile real wage change. Recall that the latter is appropriate under the assumption that 2% of potential wage change observations are missing because of employment responses to downward wage rigidity.

Consider first the estimated sweep-up effects (su_t) presented in columns 2 and 3. Under the median counterfactual, nominal wage rigidities are estimated to affect between 5.4 and 7.3% of hourly-rated non–job changers during the high-inflation years from 1979 to 1982, and between 9.7 and 13.5% of workers during the low-inflation period later in the sample. Using the 49th-percentile counterfactual the estimated effects are fairly similar: between 6.5 and 6.8% during the high-inflation years, and between 10.6 and 14.5% during the low-inflation years.

The estimated density sweep-back effects (sb_t) in columns 4 and 5 are generally much smaller than the sweep-up effects, although in some years sweep-back accounts for up to one-third of total nominal rigidity. If the sweep-back effects are interpreted as estimates of the effect of menu costs to the right of the spike, and if menu costs have a symmetric effect on negative and positive

KERNEL DENSITY ESTIMATES



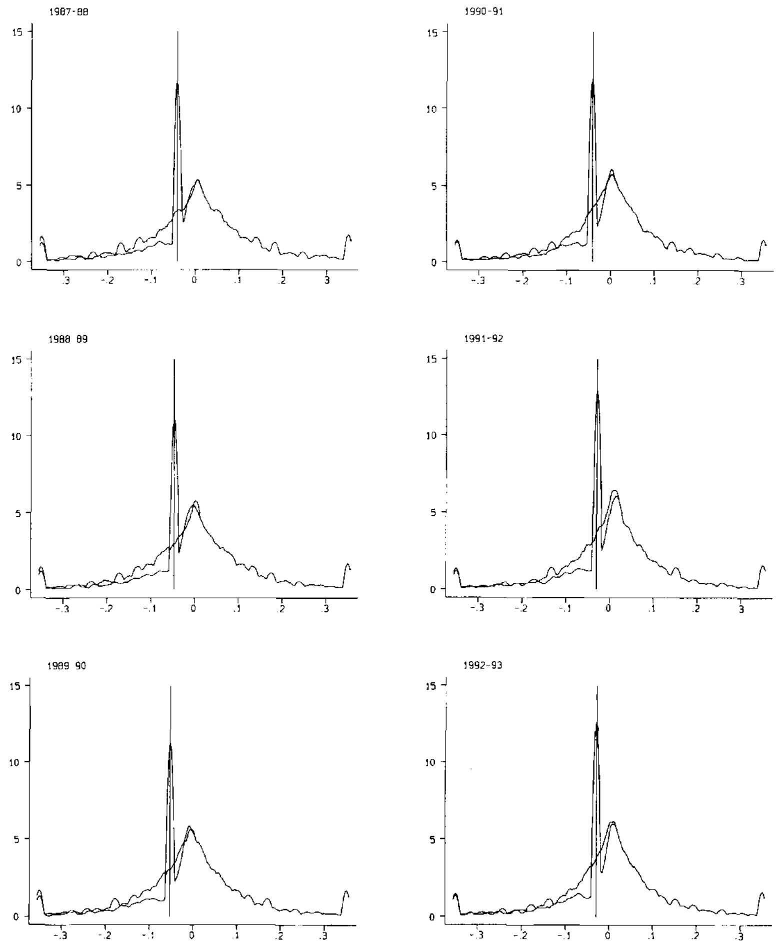
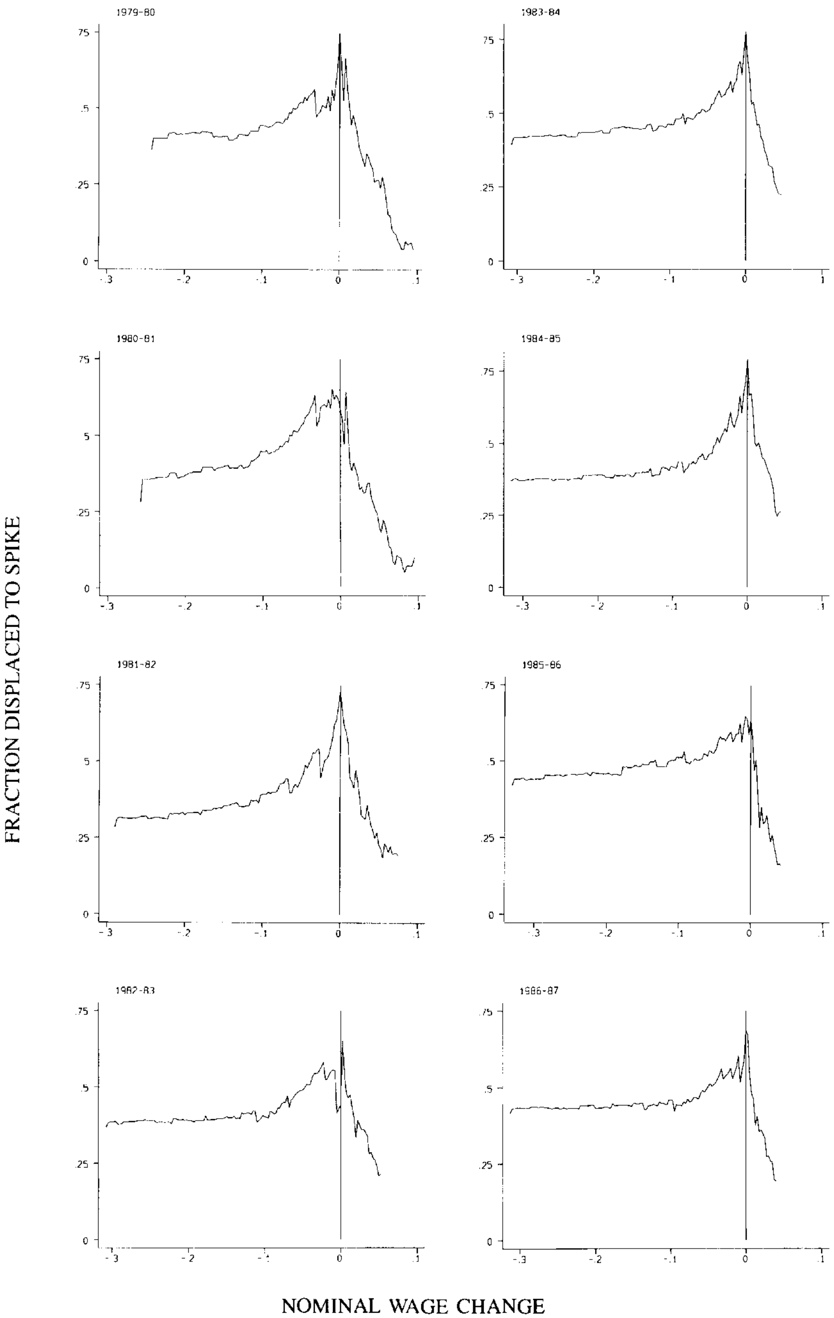


Fig. 2.6 Smoothed (kernel) estimates of actual and counterfactual densities of real wage changes, CPS samples from 1979–80 to 1992–93

wage changes, then the difference ($su_t - sb_t$) provides a *lower-bound* estimate of the fraction of people affected by downward nominal wage rigidities. In the mid-1980s this fraction is around 10–12%.

Simple regressions of our estimates of su_t on the inflation rate in year t yield statistically significant coefficients of −0.81 and −0.97 using the median and 49th-percentile counterfactuals respectively, with t-statistics of 4.1 and 4.9. Analogous regressions of the net sweep-up effects ($su_t - sb_t$) on the inflation rate yield smaller and less significant coefficients of −0.44 and −0.73, with t-statistics 1.3 and 2.2 These estimates suggest that higher inflation helps to reduce the effect of downward nominal rigidities. A 5 percentage-point increase in the inflation rate is associated with a 2.2 to 5.0 percentage-point reduction


FRACTION DISPLACED TO SPIKE
1979-80
1983-84
1980-81
1984-85
1981-82
1985-86
1982-83
1986-87
NOMINAL WAGE CHANGE

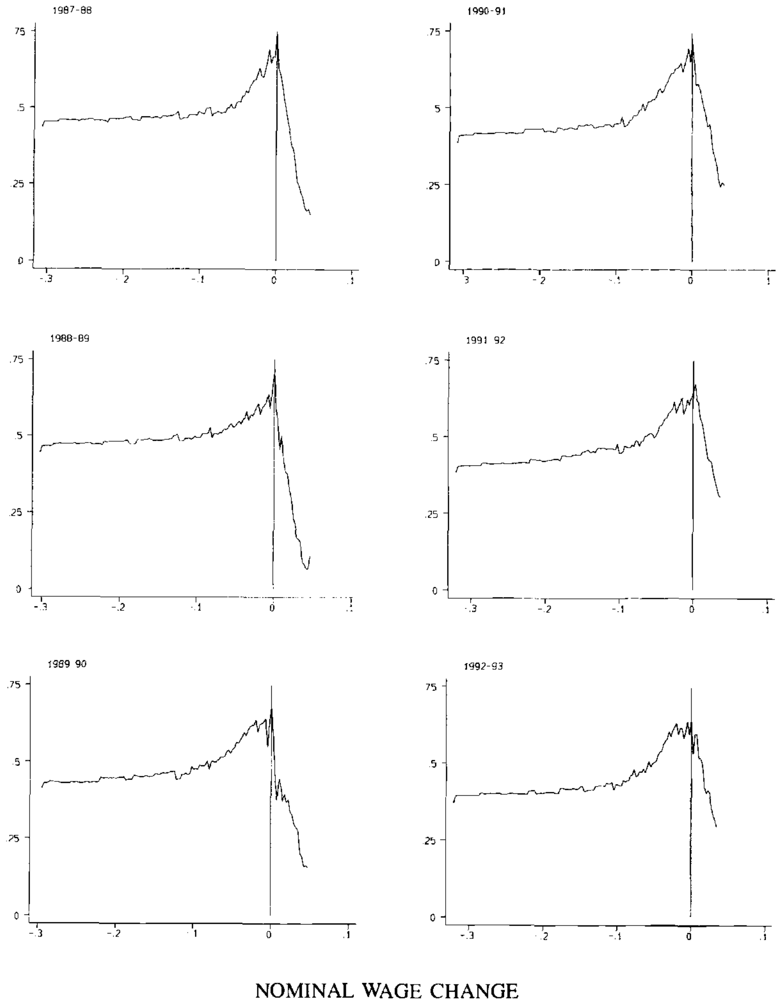


Fig. 2.7 Cumulative fraction of counterfactual density affected by rigidities, CPS samples from 1979–80 to 1992–93

in the fraction of nonmovers who are affected by downward nominal rigidity. As noted above, we suspect that this estimate is downward biased in magnitude to the extent that measured wage changes are incorrectly reported to the CPS.

Table 2.4 contains the estimated wage effects wsu_t and wsb_t associated with nominal rigidities. These vary over the sample period with larger effects in low-inflation years. Again, the estimates of wsu_t and wsb_t from the median and 49th-percentile counterfactuals are fairly similar. The estimates imply that nominal rigidities raised the mean real wages of non–job changers who would otherwise have suffered nominal wage declines by between 0.3 and 1.2%, with

Table 2.3 **Estimated Fraction of Non-Job Changers Affected by Nominal Wage Rigidities**

Year	Inflation Rate (1)	Density Swept-up[a] Counterfactual: Median (2)	Density Swept-up[a] Counterfactual: 49th Percentile (3)	Density Swept-back[b] Counterfactual: Median (4)	Density Swept-back[b] Counterfactual: 49th Percentile (5)
1979–80	10.6	6.86	6.54	0.76	2.01
1980–81	9.1	6.20	5.92	2.02	2.88
1981–82	5.9	6.31	6.56	4.54	5.31
1982–83	4.1	9.98	11.60	4.79	4.17
1983–84	4.2	10.43	11.54	4.44	4.41
1984–85	3.5	10.84	11.24	4.49	4.92
1985–86	1.8	12.72	13.96	2.87	2.66
1986–87	3.6	13.45	13.49	2.66	3.63
1987–88	4.1	13.85	14.10	1.57	2.33
1988–89	4.7	13.04	14.09	1.82	1.77
1989–90	5.3	11.39	12.42	2.72	3.17
1990–91	4.1	10.79	12.09	4.89	4.59
1991–92	3.0	11.75	12.09	5.32	5.98
1992–93	2.9	11.10	12.13	5.45	5.43

Notes: Samples are based on matched CPS samples of hourly-rated workers who report the same industry and occupation code in consecutive years, and whose wages are not affected by the minimum wage in either year.

[a]Estimated percentage of workers who would have experienced a nominal wage cut in the absence of rigidities.

[b]Estimated percentage of workers who would have experienced a nominal wage increase in the absence of rigidities.

an average effect of about 1% in the low-inflation years of the mid-1980s. On the other hand, nominal rigidities do not seem to have had a large *negative* effect on people whose nominal wages otherwise would have risen. The maximum estimated wage sweep-back effect is 0.2%, and the estimates are typically less than 0.1%. On net, our estimates imply that nominal rigidities may have contributed to about 1% higher average growth for hourly-rated non–job changers in the mid-1980s, with smaller effects in the earlier and later years of our sample period.

One interesting question that the estimated sweep-up effects in tables 2.3 and 2.4 do *not* address is how far down in the lower tail of the counterfactual wage-change distribution are individuals with observed rigid wages drawn from. For example, one might argue that the institutional forces that generate downward rigidities have limited power to resist large wage cuts. In this case, most of the measured sweep-up in table 2.3 should arise from the interval of real wage changes just below $-\pi_t$.[32] Of course, if downward rigidities do pre-

32. This ignores measurement errors in wage changes. Given an observed wage change in the lower tail of the observed wage-change distribution, the best estimate of the true wage change is less negative.

Table 2.4 Estimated Effect of Nominal Wage Rigidities on Average Real Wage Changes

		Wage Swept-Up Counterfactual[a]		Wage Swept-Back Counterfactual[b]	
Year	Inflation Rate (1)	Median (2)	49th Percentile (3)	Median (4)	49th Percentile (5)
1979–80	10.6	0.54	0.51	0.00	0.03
1980–81	9.1	0.35	0.32	0.01	0.11
1981–82	5.9	0.25	0.30	0.16	0.16
1982–83	4.1	0.75	0.83	0.09	0.06
1983–84	4.2	0.81	0.82	0.06	0.09
1984–85	3.5	0.93	0.99	0.08	0.06
1985–86	1.8	0.87	0.95	0.03	0.03
1986–87	3.6	1.17	1.20	0.03	0.05
1987–88	4.1	1.13	1.20	0.00	0.01
1988–89	4.7	1.10	1.18	0.01	0.00
1989–90	5.3	0.93	0.96	0.00	0.05
1990–91	4.1	0.71	0.80	0.07	0.05
1991–92	3.0	0.71	0.74	0.08	0.09
1992–93	2.9	0.72	0.78	0.07	0.07

Notes: Samples are based on matched CPS samples of hourly-rated workers who report the same industry and occupation code in consecutive years, and whose wages are not affected by the minimum wage in either year.

[a]Estimated effect of nominal rigidities on average real wage change for workers who otherwise would have experienced a nominal wage cut, expressed in percentages.

[b]Estimated effect of nominal rigidities on average real wage change for workers who otherwise would have experienced a nominal wage increase, expressed in percentages. A positive entry means that rigidities reduced wages for this group.

vent large wage cuts, we might expect some wage-change observations to be missing from the lower tail of the distribution, consistent with our 49th-percentile counterfactual. Appendix tables 2A.3 and 2A.4 decompose the estimates of su_t and wsu_t into fractions attributable to nominal wage changes in three intervals: less than a 10% cut, from a 10 to 20% cut, and more than a 20% nominal cut. About 70% of the density swept up to the nominal rigidity spike is attributable to the interval of 0–10% nominal cuts. Another 20% is attributable to nominal cuts of 10 to 20% and only 10% is attributable to nominal cuts over 20%. The decomposition of wage sweep-up, however, is different, since wages swept up from farther in the tail contribute more to wsu_t. Indeed, roughly one-third of total estimated wage sweep-up is attributable to each of the three ranges.

The correlations of the estimated wage sweep-up (wsu_t) and net wage sweep-up ($wsu_t - wsb_t$) effects with the aggregate inflation rate are negative and significant. Regressions of wsu_t and ($wsu_t - wsb_t$) on the corresponding inflation rates over the fourteen-year sample period yield coefficient estimates between -0.057 and -0.079, with t-statistics between 1.8 and 2.5. These estimates imply that a rise in the inflation rate from 3% to 8% is associated with about 0.3% slower real average wage growth for non–job changers. We conclude that downward nominal wage rigidities exert a small but measurable effect on average wage growth, with a bigger effect in low-inflation years. Again, evidence from our simulations suggest that, if anything, these estimates may be downward biased in magnitude by the effects of reporting errors in the CPS wage data.

The conclusion that lower inflation rates increase the incidence of downward rigidity provides one possible insight into the "fact" that individuals seem to dislike inflation (see Shiller, chap. 1 in this volume). Our estimates suggest that a lower inflation rate acts like a higher "minimum wage" for the rate of growth of real wages. Indeed, the similarity between the histograms in figures 2.1 and 2.2 and histograms of real wage *levels* in the presence of a binding minimum wage is remarkable. The data in figure 2.7 suggest that between one-quarter and one-half of non–job changers who might have expected a nominal wage cut in the absence of any rigidities instead have rigid nominal wages. If workers have an implicit "guarantee" that their real wage will fall by no more than the inflation rate, their preference for a lower inflation rate is understandable.

2.5 Market-Level Evidence

While our analysis of individual wage data provides reasonably strong evidence that nominal rigidities affect the underlying distribution of real wage changes, much of the interest in nominal rigidities focuses at a higher level of aggregation. In this section we therefore examine the evidence that *state-level* average real wages fall more quickly in response to a given level of labor market slack in periods of high inflation than in periods of low inflation.

As a point of departure, consider a collection of workers indexed by i in some local labor market j. Let U_j represent a measure of slack in market j in some period (e.g., the difference between a market demand shock and a market supply shock). Suppose that, in the absence of rigidities,

$$\Delta w_{ij} = b^* U_j + \varepsilon_{ij}, \tag{5}$$

where Δw_{ij} is the real wage change for individual i in market j (over some specific time horizon) and ε_{ij} is a random term reflecting idiosyncratic factors. In the presence of downward nominal rigidities, suppose that a fraction f of nominal wage cuts required by equation 5 do not take place:

$$\begin{aligned} (6)\quad \Delta w_{ij} &= b^* U_j + \varepsilon_{ij}, \qquad b^* U_j + \varepsilon_{ij} > -\pi \\ &= I_{ij}(-\pi) + (1 - I_{ij}) \cdot (b^* U_j + \varepsilon_{ij}), \qquad b^* U_j + \varepsilon_{ij} < -\pi, \end{aligned}$$

where I_{ij} is a random indicator variable with mean f.[33] Equation 6 implies that a regression of the average wage change observed in market j on the slack variable U_j has a coefficient that varies with the aggregate inflation rate:

33. Formally, equation 6 is a Tobit model with random censoring at $-\pi$.

$$E(\Delta w_{ij} | U_j, \pi) = a(\pi) + b(\pi) \cdot U_j, \tag{7}$$

with a smaller coefficient $b(\pi)$, the lower the inflation rate and the higher the fraction f of individuals affected by downward rigidities. If the measure of labor market slack is the unemployment rate, then equation 7 implies that the "cross-sectional Phillips curve" is *flatter* in periods with low inflation than in periods with high inflation.

To test this prediction, we used individual microdata from the March CPS files from 1977 to 1992 to construct estimates of the average wage of workers in each state from 1976 to 1991. Specifically, we constructed two estimates of the average hourly wage for each state in each year: a simple average, and an adjusted average that accounts for differences in the observed characteristics of the workers in each state.[34] We then fit a variety of models of the form

$$w_{jt} - w_{jt-1} = a_t + b_t \log U_{jt} + e_{jt}, \tag{8}$$

where w_{jt} is the average wage index for state j in year t, a_t represents a year dummy, U_{jt} is the measured unemployment rate in the state in year t, and e_{jt} represents a residual. Finally, we analyzed the covariation between b_t (the slope coefficient in year t) and the inflation rates between years $t - 1$ and t.

Two aspects of the specification in equation 8 deserve comment. First, equation 8 describes the change in the average wage, while equation 7 describes the average individual-level wage change. In the absence of selection biases associated with nonrandom movements in and out of the labor market, this is not a problem, since with a fixed population $E(\Delta w_{ijt}) = E(w_{ijt}) - E(w_{ijt-1})$ (taking expectations over individuals in state j). While there is some evidence of a cyclical component in the gap between the average wage change for continuing workers and the change in average wages for all workers (see Solon, Barsky, and Parker 1994), this issue is somewhat less important in our application because an individual has to be unemployed (or out of the labor force) for an entire year in order not to have a wage in the March CPS data.

Second, although equation 8 is consistent with the original formulation of the Phillips curve, it is inconsistent with the formulation of the so-called wage curve recently popularized by Blanchflower and Oswald (1994). In particular, Blanchflower and Oswald argue that the wage *level* in a local labor market depends on the unemployment rate, while equation 8 implies that the rate of change of wages depends on the unemployment rate. A simple way to compare the two alternatives is to introduce the lagged unemployment rate into equation 8. If the correct model specifies the level of wages as a function of the level of unemployment, then the first difference of wages will depend on current and

34. To construct the adjusted average, we first estimated a wage-prediction equation for each year that included various observable characteristics (education, labor market experience, dummies for race, gender, Hispanic status) as well as dummies for each state of residence. We then used the coefficients to predict a wage for each individual, assuming that the individual lived in California. Finally, we constructed the average deviation of the observed wage from the predicted wage: this is our adjusted average (log) wage.

lagged unemployment with equal and opposite coefficients. If the correct model specifies the rate of growth of wages as a function of the unemployment rate, then lagged unemployment will have an insignificant effect on wage growth.[35]

Some evidence on this specific issue, and on the general performance of equation 8, is presented in appendix table 2A.5 where we summarize the results of estimating various versions of equation 8 *without allowing the coefficient* b *to vary across years*. In brief, the estimates suggest that wage growth is fairly responsive to local unemployment: a doubling of the unemployment typically reduces the rate of wage growth by 1.7–2.4% per year. Moreover, consistent with the specification of the conventional Phillips curve, but contrary to the wage-curve approach, lagged valued of local unemployment exert no significant effect on wage growth. These conclusions are robust to minor changes in specification, including the addition of dummies capturing permanent differences in wage growth across regions or states, the introduction of region times year effects capturing region-specific cycles, alternative weighting schemes, and the use of raw versus adjusted average wages for each state.

Using these findings, we proceeded to estimate a series of models that exclude lagged unemployment, but allow the coefficient on current unemployment to vary across years. Estimates of the critical coefficients b_t from five such specifications are reported in table 2.5. For reference, the top row in the table gives the estimates of the unemployment slopes from identical specifications when the slope b_t is constrained to be constant across years. The year-specific estimates of b_t are then tabulated, along with the estimated coefficients from simple ordinary least squares (OLS) regressions of the estimated b_ts on the inflation rate. Across the different specifications there is a tendency for unemployment to exert a bigger (more negative) effect on local wage determination in high inflation years. However, the correlation of b_t and π_t is weak: the biggest t-ratio (for the model in column 4) is around one.

The estimates in the bottom row of table 2.5 imply that a 5 percentage-point increase in inflation leads to an increase in the magnitude of the slope coefficient relating wage growth to local unemployment of between 0 and 0.012. To understand the implications of these estimates, suppose that $b_t = -0.034$ in an average year (as in column 2 of table 2.5). Then real wage growth is about 2.3 percentage points per year slower in a state with an 8% unemployment rate than in a state with a 4% unemployment rate. Raising the inflation rate by 5 percentage points would widen this gap by an additional 0 to 0.7 percentage

35. It is also possible to formulate a test based on a model for the level of wages. Specifically, the wage-curve hypothesis suggests that only the current unemployment rate affects the level of wages (controlling for state effects), while the Phillips-curve specification implies that lagged unemployment terms enter in the model with equal (negative) coefficients. Our findings from this approach are consistent with the results based on a model in first-differences.

Table 2.5 **Estimated Effects of State Unemployment on Real Wage Growth**

	Additional Control Variables Included in Models				
	Year	Year & Region	Year × Region	Year & State	Year × Region & State
Pooled slopes[a]	−0.025	−0.034	−0.025	−0.048	−0.056
	(0.005)	(0.006)	(0.007)	(0.007)	(0.012)
Year-specific slopes[b]					
1976–77	0.018	0.008	0.002	−0.004	−0.028
	(0.019)	(0.019)	(0.029)	(0.020)	(0.031)
1977–78	−0.020	−0.035	−0.027	−0.049	−0.057
	(0.020)	(0.020)	(0.031)	(0.021)	(0.034)
1978–79	0.001	−0.015	−0.005	−0.033	−0.040
	(0.020)	(0.021)	(0.030)	(0.022)	(0.033)
1979–80	−0.053	−0.068	−0.016	−0.088	−0.055
	(0.020)	(0.020)	(0.030)	(0.021)	(0.033)
1980–81	−0.042	−0.056	−0.034	−0.074	−0.067
	(0.018)	(0.018)	(0.026)	(0.019)	(0.028)
1981–82	−0.022	−0.037	−0.061	−0.056	−0.100
	(0.019)	(0.019)	(0.030)	(0.020)	(0.032)
1982–83	−0.047	−0.060	−0.057	−0.080	−0.087
	(0.018)	(0.019)	(0.026)	(0.020)	(0.028)
1983–84	−0.044	−0.058	−0.025	−0.076	−0.056
	(0.017)	(0.018)	(0.025)	(0.019)	(0.028)
1984–85	−0.018	−0.029	−0.040	−0.046	−0.071
	(0.019)	(0.019)	(0.029)	(0.020)	(0.031)
1985–86	−0.016	−0.024	0.018	−0.036	−0.011
	(0.016)	(0.017)	(0.030)	(0.017)	(0.033)
1986–87	−0.062	−0.066	−0.030	−0.077	−0.060
	(0.015)	(0.015)	(0.028)	(0.016)	(0.031)
1987–88	−0.004	−0.008	−0.023	−0.020	−0.050
	(0.015)	(0.016)	(0.025)	(0.016)	(0.028)
1988–89	−0.027	−0.033	−0.017	−0.050	−0.045
	(0.020)	(0.020)	(0.026)	(0.021)	(0.029)
1989–90	−0.030	−0.040	−0.069	−0.063	−0.099
	(0.025)	(0.025)	(0.028)	(0.027)	(0.031)
1990–91	0.019	0.006	0.010	−0.012	−0.017
	(0.023)	(0.023)	(0.027)	(0.024)	(0.029)
Effect of inflation rate	−0.097	−0.197	−0.041	−0.251	−0.146
on estimated slope[c]	(0.275)	(0.273)	(0.286)	(0.286)	(0.298)

Notes: Standard errors are in parentheses. Models are estimated on sample of 756 state times year observations. See note to table 2A.5.

[a]Estimated effect of unemployment on wage growth in model with constant coefficient.

[b]Estimated effects of unemployment on wage growth in model with year-specific coefficients.

[c]Estimated coefficient from OLS regression of year-specific unemployment effects on annual inflation rate (change in log CPI-U-X1).

points.[36] The upper range of this interval represents a sizeable increase in the "flexibility" of wages to local demand conditions between a low- and high-inflation regime. However, the imprecise nature of our estimates makes it impossible to distinguish such a possibility from the alternative that higher inflation has *no* effect on the rate of relative wage adjustment.

2.6 Conclusions

A traditional concern about very low inflation is that nominal wages are downward rigid. In this paper we have attempted to assemble two types of evidence on the extent of such rigidities: microlevel evidence based on the distribution of individual-specific wage changes; and market-level evidence based on the rate of adjustment of average real wages in a state to the state unemployment rates. Our microanalysis reveals three key insights. First, although many individuals experience (measured) nominal wage reductions from one year to the next, there is a substantial spike at zero in the distribution of nominal wage changes. Second, the magnitude of this spike is very highly correlated with inflation. In the high-inflation era of the late 1970s, 6–10% of workers with the same job reported exactly the same wage from one year to the next. In the low-inflation era of the mid-1980s, this fraction rose to over 15%. Third, informal and formal analyses suggest that most (but not all) of workers with rigid nominal wages would have had an even bigger decline in their real wage in the absence of rigidities. For the mid-1980s we estimate that downward nominal rigidities may have "held up" average real wages by 1% per year.

Our market-level analysis of real wage responses to local unemployment is less conclusive. As previous researchers have noted, real wages grow more quickly in local labor markets with low unemployment, and decline in local labor markets with high unemployment. In principle, the existence of downward nominal rigidities implies that the rate of adjustment to negative shocks will be faster, the higher the aggregate inflation rate. Empirically, however, we find only weak evidence of such an effect. Based on both types of evidence, we conclude that the overall impact of nominal wage rigidities is probably modest.

36. An increase in the unemployment rate from 4% to 8% is a 0.69 point change in the log unemployment rate. Multiplying this by the baseline coefficient estimate (-0.034) implies a 2.3 percentage-point reduction in the growth of log wages. The coefficients in the bottom row of table 2.5 imply that a 5 percentage-point increase in the inflation rate will raisc thc absolute magnitude of the unemployment coefficient by from 0.002 to 0.010, leading to a net unemployment coefficient of -0.036 to -0.044. In this case, the effect of doubling the uncmployment rate is to slow the rate of growth of wages by from 2.5 to 3.0 percentage points per year.

Appendix A
Data Description and Sources

This appendix describes the construction of our matched CPS panels. We begin with the merged monthly "outgoing rotation group" files that pool the CPS sample observations in the two outgoing rotation groups (rotation groups 4 and 8) of each month of a given calendar year. The CPS sample design implies that households in rotation group 4 in a given month will be in rotation group 8 in the same month in the next year. For example, in the 1979 CPS sample there are 164,626 individuals age sixteen and older in rotation group 4, drawn from 80,557 uniquely identified households. All of these individuals were potentially reinterviewed in 1980. Since the CPS sample frame is based on physical addresses, rather than specific individuals or families, any family that moves between 1979 and 1980 is "replaced" in the sample by the family that moves into their old housing unit. Moreover, individuals who move out of a family are not tracked to their new address. Finally, since the CPS does not assign unique person identifiers to individuals within households, there is some slip-

Table 2A.1 Matched CPS Sample Selection

Year	Total Number of Hourly-rated Workers in Matched CPS Sample	% with Same Industry & Occupation[a]	. . . And Unaffected by Minimum Wage[b]
1979–80	19,792	58.9	47.3
1980–81	22,362	59.8	48.1
1981–82	22,127	61.5	52.9
1982–83	21,768	32.8	28.5
1983–84	21,737	47.7	42.4
1984–85	10,491	57.0	51.2
1985–86	5,904	54.9	50.2
1986–87	23,187	56.1	51.5
1987–88	21,906	55.8	51.9
1988–89	21,751	55.2	52.0
1989–90	22,952	55.3	50.4
1990–91	23,365	56.0	48.9
1991–92	23,089	55.7	50.5
1992–93	22,847	56.3	52.2

[a]The industry and occupations are matched using detailed (two-digit) industry and occupation codes for all years except 1982–83, 1983–84, and 1988–89. Matching for the 1983–84 sample is based on three-digit 1980 census codes; for the 1982–83 sample, the industry is matched using the detailed (two-digit) codes which are comparable across years, while occupation was matched using an algorithm devised to convert 1970 census three-digit occupation codes to their 1980 census counterparts; and for the 1988–89 sample, occupation was matched using the detailed codes, and an algorithm was devised to match the detailed industry codes. The matching algorithms used for the 1982–83 and 1988–89 samples are available from the authors on request.

[b]Observations are assumed to be affected by minimum wage effects if *either* $w_{t-1} \leq \max(mw_{t-1}, mw_t)$, *or* $w_t \leq mw_t$.

Table 2A.2 PSID Sample Selection

Year	Total Number of Workers in 4-Year Panel	% Hourly-rated with Same Employer[a]
1976–77	1,965	41.2
1977–78	1,992	45.0
1978–79	2,214	41.3
1985–86	4,507	45.9
1986–87	4,447	45.0
1987–88	4,443	45.1

[a]Workers are treated as having changed employer if their reported tenure, in months, is less than the number of months since their previous interview. During 1976–79, tenure relates to time in the same *position,* while during 1985–88, tenure relates to time with the same employer.

Table 2A.3 Decomposition of Density Sweep-Up over the Range of Nominal Wage Changes

		Density Swept-up From[a]			
Year	Inflation Rate	All Negative Wage Changes	Wage Changes between −0.1 & 0	Wage Changes between −0.2 & −0.1	Wage Changes < −0.20
1979–80	10.6	6.86	5.11	1.34	0.42
1980–81	9.1	6.20	5.22	0.42	0.56
1981–82	5.9	6.31	5.55	0.53	0.23
1982–83	4.1	9.98	6.54	2.07	1.37
1983–84	4.2	10.43	7.27	2.21	0.94
1984–85	3.5	10.84	7.45	1.86	1.53
1985–86	1.8	12.72	9.41	2.16	1.15
1986–87	3.6	13.45	9.20	2.26	1.99
1987–88	4.1	13.85	9.38	3.04	1.42
1988–89	4.7	13.04	8.79	2.87	1.37
1989–90	5.3	11.39	8.02	2.26	1.12
1990–91	4.1	10.79	7.97	2.48	0.34
1991–92	3.0	11.75	8.74	2.15	0.87
1992–93	2.9	11.10	8.09	2.07	0.94

Note: Samples are based on matched CPS samples of hourly-rated workers who report the same industry and occupation code in consecutive years, and whose wages are not affected by the minimum wage in either year.

[a]Computed assuming "median" counterfactual wage-change distributions.

page in matching if an individual misreports a key characteristic (like race or age), or if a household contains two very similar people. These limitations imply that about 25–30% of individuals are unmatchable.

We matched individuals in rotation group 4 of year *t* with individuals in

Table 2A.4 Decomposition of Wage Sweep-Up over the Range of Nominal Wage Changes

		Density Swept-up From[a]			
Year	Inflation Rate	All Negative Wage Changes	Wage Changes between −0.1 & 0	Wage Changes between −0.2 & −0.1	Wage Changes < −0.20
1979–80	10.6	0.54	0.19	0.21	0.14
1980–81	9.1	0.35	0.17	0.06	0.12
1981–82	5.9	0.25	0.18	0.08	−0.01
1982–83	4.1	0.75	0.18	0.27	0.29
1983–84	4.2	0.81	0.27	0.31	0.24
1984–85	3.5	0.93	0.24	0.28	0.40
1985–86	1.8	0.87	0.39	0.30	0.18
1986–87	3.6	1.17	0.36	0.32	0.49
1987–88	4.1	1.13	0.33	0.44	0.36
1988–89	4.7	1.10	0.33	0.40	0.36
1989–90	5.3	0.93	0.28	0.32	0.33
1990–91	4.1	0.71	0.25	0.37	0.09
1991–92	3.0	0.71	0.26	0.25	0.19
1992–93	2.9	0.72	0.23	0.28	0.21

Note: Samples are based on matched CPS samples of hourly-rated workers who report the same industry and occupation code in consecutive years, and whose wages are not affected by the minimum wage in either year.

[a]Computed assuming "median" counterfactual wage-change distributions.

rotation group 8 in year $t + 1$ by household identity number, interview month, sex, race, ethnicity, and age. We allowed for errors in age of plus or minus one year in the matching algorithm (this gives about 6% more successful matches than a strict requirement that age increments by one). The overall match rates are between 70 and 75% in every year except 1984–85 and 1985–86. For example, 74.5% of the 164,626 individuals in rotation group 4 of the 1979 sample are successfully matched to a 1980 observation, and 74.4% of the 164,942 individuals in rotation group 4 of the 1992 sample are successfully matched to a 1993 observation. In July 1985 the CPS implemented a new sample frame: only individuals in the January–June 1985 CPS are matchable to observations in 1984, and only individuals in the October–December 1985 CPS are matchable to observations in 1986. These limitations lead to much lower match rates for 1984–85 (37.0% of all individuals in the 1984 sample) and 1985–86 (18.3% of all individuals in the 1985 sample).

Table 2A.5 **Estimated Models for the First-Difference of State-Average Log Wages, 1976–91**

Dependent Variable	Estimated Coefficients of Log State Unemployment Rate: Current	Lag 1	Lag 2	Lag 3	Residual Standard Error	Other Controls Included
Adjusted log wage (weighted)	−0.025 (0.005)	—	—	—	0.042	year effects
Adjusted log wage (weighted)	−0.044 (0.011)	0.021 (0.011)	—	—	0.042	year effects
Adjusted log wage (weighted)	−0.038 (0.011)	−0.004 (0.016)	0.002 (0.015)	0.021 (0.011)	0.042	year effects
Adjusted log wage (weighted)	−0.034 (0.006)	—	—	—	0.042	year and region effects
Adjusted log wage (weighted)	−0.048 (0.011)	0.016 (0.011)	—	—	0.042	year and region effects
Adjusted log wage (weighted)	−0.025 (0.007)	—	—	—	0.040	year × region effects
Adjusted log wage (weighted)	−0.023 (0.014)	−0.003 (0.014)	—	—	0.040	year × region effects
Unadjusted log wage (weighted)	−0.029 (0.012)	−0.002 (0.012)	—	—	0.048	year and region effects
Adjusted log wage (unweighted)	−0.049 (0.012)	0.018 (0.012)	—	—	0.038	year and region effects

Notes: All models are fit to sample of 765 observations (51 states times 15 year-to-year changes). The dependent variable is the change from year $t - 1$ to year t in the state average wage, derived from March CPS data for all individuals who worked positive weeks and reported positive earnings (age 16–68). In all but one row, the state average wage is adjusted for the characteristics of workers in the state (using a year-specific wage prediction model). In all but one row, the estimates are obtained by weighted OLS, using as weights the relative number of workers in the state in 1976. Standard errors are in parentheses.

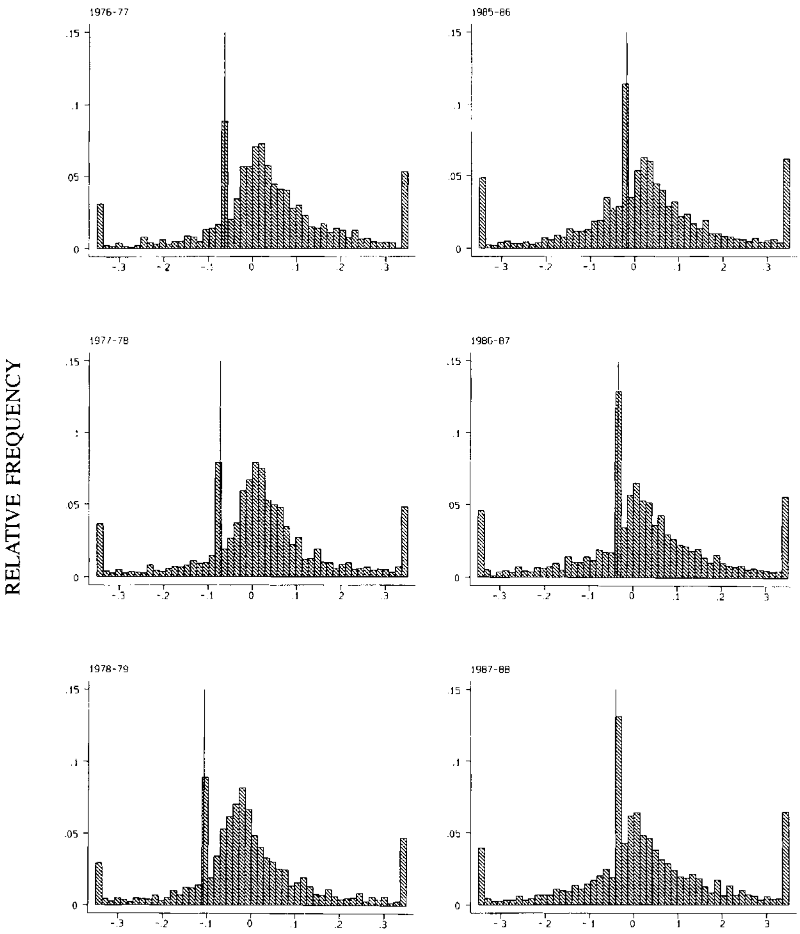


Fig. 2A.1 Histograms of real wage changes for all workers in PSID samples, 1976–79 and 1985–88

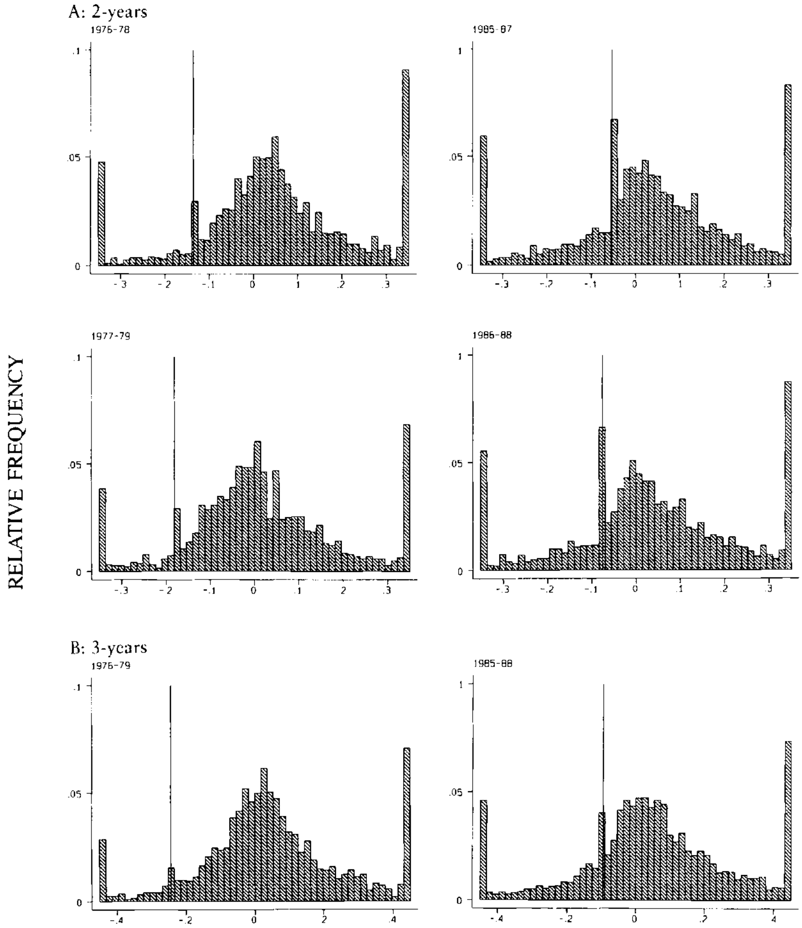


Fig. 2A.2 Histograms of real wage changes for all workers in PSID samples, over two-year and three-year horizons, 1976–79 and 1985–88

Appendix B
Simulations of the Effect of Measurement Error

This appendix describes the simulations we used to evaluate the effect of measurement error on our estimates of sweep-up, sweep-back, wage sweep-up, and wage sweep-back. The simulations all begin with an underlying distribution of real wage changes in the absence of any rigidities. We assume that this is a normal distribution with mean zero and standard deviation 0.12. The standard deviation of 0.12 is based on estimates of the dispersion in the upper half of the distribution of observed real wage changes in our CPS samples. To this underlying distribution we then add downward rigidities affecting a fraction of workers who would otherwise receive a nominal wage cut, and menu-cost rigidities affecting some individuals who would otherwise experience a "small" nominal wage change. Finally, we added a simple model of measurement error: with probability R the measurement error in the observed wage change is zero; with probability $(1 - R)$ the measurement error is drawn from a normal distribution with mean zero.

In all simulations we adjusted the standard deviation of the measurement error component so that the overall contribution of measurement errors to the variance of observed real wage changes is 20%. Most available evidence suggests that this is probably a lower bound on the share of observed wage changes attributable to reporting errors (see, e.g., McLaughlin 1994). However, even large changes in the fraction of the variance of observed wage changes attributable to measurement error have relatively little effect in our simulations, holding constant the probability of an accurately reported wage change (R).

We modeled the effect of menu costs as follows. For all observations that would otherwise obtain an absolute nominal wage change Δw of less than or equal to g, we assume that a fraction $0.5(1 - |\Delta w|/g)$ have rigid nominal wages. We set g to either 0.03 or 0.06.

In the simulation model the rate of measured wage rigidity at any inflation rate is determined by three factors: the fraction of workers affected by downward nominal rigidities (i.e., the fraction "swept up"); the fraction affected by menu costs; and the fraction of individuals who accurately report their true wage change (R). We developed three scenarios that combine these factors so as to generate observed rigidity rates of about 8–9% at 10% inflation and observed rigidity rates of 12–14% at 5% inflation. One of these combines a relatively high estimate of R (0.66) with a midrange estimate of the probability that a nominal wage cut is affected by downward rigidity (0.5) and a narrower range of menu costs ($\pm 3\%$). The second combines a higher rate of menu-cost rigidity with a more moderate estimate of R (0.50). The third assumes a very high probability of downward rigidity, conditional on a negative nominal wage change (0.7).

Table 2B.1 **Evaluation of Estimated Rigidity Effects in Presence of Measurement Errors**

Width of Interval Affected by Menu Costs	Probability of Downward Nominal Rigidity	Probability of No Error in Δw	Inflation Rate	Based on True Wage Changes, Fraction Affected by: Menu Costs	Downward Rigidity	True *wsu*	Based on Observed Wage Changes: Fraction Rigid	*su*	*sb*	*wsu*	Ratio: Observed ÷ True *wsu*
				Scenario 1							
±0.03	0.50	0.66	0.10	0.035	0.093	0.007	0.087	0.081	0.006	0.007	1.00
			0.05	0.046	0.157	0.013	0.136	0.123	0.013	0.010	0.77
			0.02	0.049	0.206	0.019	0.169	0.153	0.015	0.014	0.74
				Scenario 2							
±0.06	0.50	0.50	0.10	0.071	0.087	0.006	0.079	0.069	0.010	0.005	0.85
			0.05	0.091	0.147	0.013	0.119	0.100	0.018	0.009	0.69
			0.02	0.097	0.191	0.019	0.143	0.135	0.008	0.013	0.68
				Scenario 3							
±0.03	0.70	0.50	0.10	0.035	0.130	0.009	0.083	0.087	−0.003	0.007	0.78
			0.05	0.046	0.223	0.019	0.134	0.130	0.003	0.013	0.68
			0.02	0.049	0.290	0.027	0.168	0.154	0.013	0.016	0.59

Notes: Based on simulations of wage changes and rigidity effects. In all cases, the real wage change that would be observed in the absence of rigidities is assumed to be normally distributed with mean zero and standard deviation 0.12. Also, the ratio of the variance of the measurement error in wage changes to the total variance of observed wage changes is set to 0.20.

Table 2B.1 summarizes the true and observed nominal rigidity effects under each scenario at three different inflation rates (10%, 5%, and 2%). In scenario 1, which has a "high" value of R, the true fraction of workers affected by downward rigidity varies from 9 to 21%, and between 3.5 and 5% of workers are affected by menu costs. The true wage sweep-up effect is relatively modest, ranging from 0.7 to 1.9%. (The wage sweep-back effects are uniformly close to zero in all our simulations and are not shown.) Depending on the inflation rate, the observed density displacement and wage effects in this scenario are downward biased by 0–30%.

In scenario 2, which has a "high" fraction of workers affected by menu costs and/or rounding, the true sweep up effects are (virtually) the same as in scenario 1 and the measured effects are also similar. (The sweep-up effects are just slightly smaller in scenario 2 than scenario 1 because we first allow the effect of menu costs and then impose downward rigidities. With more rigidity attributable to menu costs, the net effect of downward rigidity is lessened.) Finally, in scenario 3, which has a "high" probability of downward rigidity for those who would otherwise experience wage cuts, the true sweep-up effects are slightly larger but the measured effects are about the same as in the other scenarios, implying slightly larger downward biases.

The last column of table 2B.1 shows the ratio of estimated wage sweep-up to true wage sweep-up. Note that estimated wage sweep-up is typically downward-biased by 20–30%, with a larger bias the lower the inflation rate.

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Comment John Shea

Many economists believe that nominal labor market frictions cause excessive employment fluctuations. One often-mentioned type of nominal friction is downward nominal wage rigidity (DNWR), in which workers are either unwilling to accept reductions in nominal wages, or resent nominal wage cuts so much that firms optimally do not try to impose them. To see how DNWR can generate excessive employment volatility, consider figure 2C.1, which plots labor demand and supply curves relating employment (L) to the real wage (W). Under DNWR, workers will not work for less than last period's nominal wage, so labor supply becomes infinitely elastic at a real wage of $w(t-1)/(1+\pi)$, where π is this period's inflation rate and $w(t-1)$ is last period's real wage. Evidently, labor-demand shifts generate excessive employment volatility whenever labor demand intersects the flat portion of labor supply—that is, whenever the downward constraint on nominal wages binds.

Now consider figure 2C.2, which shows how DNWR interacts with inflation. When inflation is low, labor supply flattens at a high real wage, and excessive employment fluctuations are likely. When inflation is high, however, labor supply does not flatten until the real wage is low, and excessive employment fluctuations are less likely. This is the sense in which inflation "greases the wheels of the labor market" under DNWR—by making a wider range of real wage outcomes acceptable to workers, inflation can prevent excessive employment responses to negative labor-demand shocks.[1]

David Card and Dean Hyslop's paper uses two methods to assess the empirical significance of downward nominal wage rigidity for the United States. The first method examines the distribution of individual wage changes in U.S. microeconomic data. The second method examines the interaction between the inflation rate and the slope of the Phillips curve, using panel data for U.S. states. I will discuss each method in turn.

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1. This discussion ignores the question of why workers would accept declining real wages imposed by inflation but would not accept declining real wages imposed by nominal wage cuts. One possibility, of course, is that workers suffer from nominal illusion. Tobin (1972) suggests instead that workers care about relative wages in addition to absolute wages, and that workers rationally believe that inflation is more likely than nominal wage cuts to spread the pain across all workers equally.

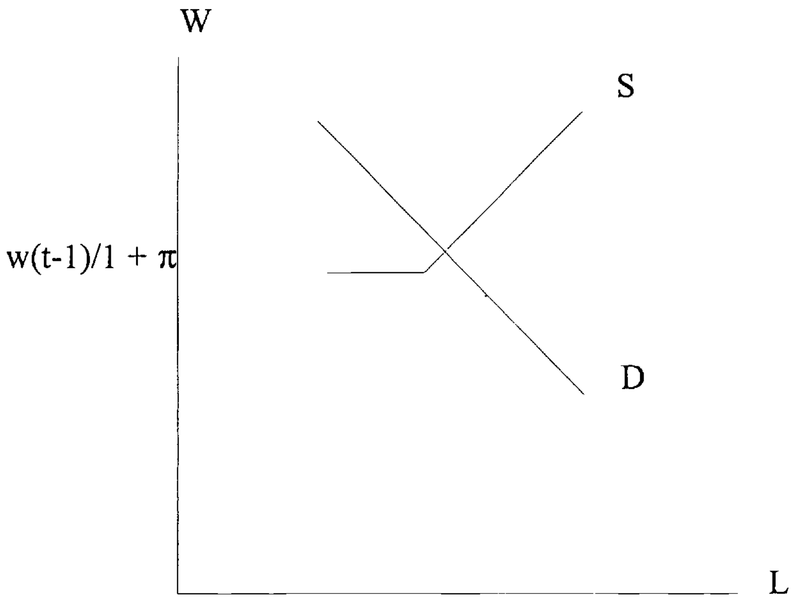

Fig. 2C.1 Downward nominal wage rigidity

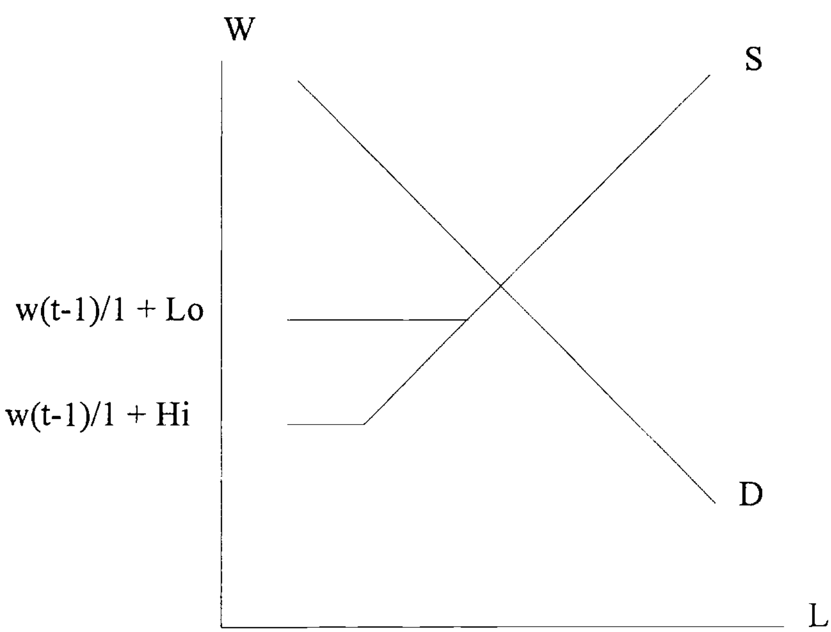

Fig. 2C.2 How inflation greases the wheels

Method One: Wage Distributions

If wages are downwardly rigid, then the distribution of workers' observed real wage changes should be skewed to the right, the more so the lower is the inflation rate. The first part of the paper tests this implication of DNWR by examining reported year-to-year real wage changes in the Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID).[2] The authors begin by constructing a counterfactual wage-change distribution that would hold in the absence of wage rigidity; this distribution is constructed by taking a mirror image of the upper half of the observed distribution. The authors then use the shortfall in the nominal-wage-cut region of the observed distribution relative to the counterfactual to estimate the fraction of workers whose real wages are propped up by DNWR (the "sweep up"), as well as the impact of DNWR on aggregate wage growth. The authors find that nominal wage cuts are not rare; the fraction of hourly-rated CPS workers reporting a nominal wage cut ranges from 11.6% in 1979–80 to 20.3% in 1992–93. Despite this, there is still some evidence of DNWR; averaging over the year-by-year results in table 2.3, the authors find that 10.6% of sample workers have their wages propped up in a typical year. As expected, DNWR binds more when inflation is low; the sweep up is 6.20% in 1980–81, but 13.85% in 1987–88. Overall, the authors find that the economic impact of DNWR is small; eliminating downward rigidity would have reduced real wage growth by only 0.78% per year between 1979 and 1993.

While Card and Hyslop's conclusions—downward rigidity exists, but it does not exert a very large impact on the labor market—accord with my priors, I have some concerns with the details of their methodology. In particular, there are three potential reasons why the authors' numbers might not reflect the true impact of DNWR on the U.S. economy.

First, Card and Hyslop's baseline sample is restricted to hourly workers who do not switch jobs from one year to the next. But hourly stayers make up only half of the working population.[3] Including job switchers and salaried workers would raise the authors' estimates of wage flexibility and reduce the estimated impact of DNWR. For instance, Lebow, Stockton, and Wascher (1995) examine the distribution of individual wage changes in the PSID. They find that 11.9% of hourly stayers experience nominal wage cuts in a typical year, compared to 19.3% of all workers, 17.8% of all stayers, and 24.8% of movers. They find that 9.7% of hourly wage stayers have their wages swept up in a typical year, compared to 7.4% of all workers, 6.8% of all stayers, and 5.1% of all movers. These figures suggest that the authors' sample-selection criteria cause them to overstate the average sweep-up by about 30% (9.7 divided by 7.4

2. Other recent studies examining the distribution of individual wage changes include McLaughlin 1994, Kahn 1995, and Lebow, Stockton, and Wascher 1995, the last of which is the closest to the present paper.

3. These figures are based on table 1 in Lebow, Stockton, and Wascher 1995.

equals 1.31).[4] The authors' sample may also overstate the sensitivity of sweep-up with respect to inflation; for instance, Lebow et al.'s regression of sweep-up on inflation yields a coefficient of -0.75 (with a t-statistic of -2.5) for hourly stayers, but only -0.35 (-1.2) for all stayers.

Second, the authors assume that the wage distribution would be symmetric absent DNWR. This assumption is obviously important to the quantitative results; if the counterfactual were assumed to be negatively skewed, for instance, the gap between the counterfactual and reality would be larger and the estimated impact of DNWR would be greater. To my knowledge, there is little evidence available on the shape of the distribution of microlevel shocks in the U.S. economy. Davis and Haltiwanger (1992), however, show the plant-level job destruction is much more cyclical than plant-level job creation—job destruction rises much more sharply in recessions than job creation rises in booms. This suggests that plant-level shocks may be negatively skewed, at least during recessions. On the other hand, the shock distribution and the wage distribution need not look alike. In particular, even if wages are flexible downward, bad microshocks would presumably in many cases lead to voluntary separations rather than wage cuts (McLaughlin 1991), which would counteract negative skewness in the shocks and could even create positive skewness in the wage distribution. Obviously, we need more evidence on the distribution of microlevel shocks and the determinants of voluntary separations before we can assess whether a symmetric counterfactual is plausible or not.

Third, Card and Hyslop's calculations assume that individuals' reported nominal wages are accurate. There is good reason to believe that individually reported nominal wages contain measurement error; for instance, the authors cite a January 1977 CPS survey in which employees and their employers agree on the wage only 44% of the time. As the authors show in appendix B, measurement error in the level of wages can cause their methodology to understate effects of DNWR considerably. One channel that the authors do not emphasize, but that seems important to me, is that measurement error might cause the data to vastly overstate the true fraction of workers receiving nominal wage cuts. I have heard several colleagues express disbelief at the notion that between 10 and 20% of hourly stayers experience nominal wage cuts from one year to the next. To see whether measurement error could explain such a result, I perform some calculations using a small sample of union workers from the PSID. In Shea (1995), I combine PSID information on individuals' industry, occupation, union affiliation, and county of residence with outside information about pattern bargaining, contract settlements, and the location of particular employers to match individual PSID household heads to the provisions of particular long-

4. In truth, Card and Hyslop's figures are probably not off by as much as 30%. The authors work primarily with CPS data, in which the distinction between movers and stayers is not as precise as in the PSID; thus, the authors' sample already includes some movers. Also, Card and Hyslop provide evidence contrary to the finding in Lebow, Stockton, and Wascher (1995) that salaries are more flexible than wages.

Table 2C.1 **Percentage of Workers with Nominal Wage Cuts**

Year	Sample Size	Contract	Reported
1981–82	79	0	11.4
1982–83	69	5.8	11.6
1983–84	55	1.8	25.5
1984–85	59	0	16.9
1985–86	57	0	35.1
1986–87	60	0	31.7

term union contracts. Here, I consider a subset of the sample from Shea (1995) for which hourly wages are reported at both t and $t + 1$, and for which reported tenure at time $t + 1$ is greater than twelve months. These restrictions leave 379 observations, ranging from 1981–82 through 1986–87.

Table 2C.1 reports statistics on nominal wage cuts for my sample, broken down by year. For each year, I report the number of observations, the percentage of observations whose published union settlements imposed nominal wage cuts, and the percentage of observations reporting nominal wage cuts in the PSID.[5] The figures are startling; overall, I find that only 1.3% of my sample observations have "true" nominal wage cuts according to their contracts, but that 21.1% of my sample report nominal wage cuts. Taken literally, these results suggest that measurement error could explain all of the evidence for downward nominal wage flexibility found in Card and Hyslop's sample. It is possible, of course, that contract information understates the incidence of "true" nominal wage cuts. For instance, contemporaneous accounts in the Bureau of Labor Statistics' *Current Wage Developments* indicate that some unionized trucking companies deviated from the trucking pattern bargain during the 1980s and imposed nominal wage cuts in the face of competition from nonunion companies. For robustness, I redid my experiment excluding truckers, and found that the gap between the reported and published incidence of nominal wage cuts was virtually unchanged (1.6 versus 21.0%). Another possibility is that my findings reflect the fact that senior union workers whose positions have been eliminated are typically allowed to "bump" less senior workers at the next highest pay rung, who in turn can move down a pay rung and bump even less senior workers, and so on.[6] I know of no data on the fraction of union workers who are bumped in a typical year. I would note, however, that the incidence of reported nominal wage cuts was lower during the 1982–83 recession than during the subsequent recovery, which seems inconsistent with

5. Hourly wages in the PSID are reported as of the time of interview. Since almost all PSID interviews occur during the spring, I compute "contract" wage changes over the interval April 1, year t through March 31, year $t + 1$. Contract wage changes are estimated using union settlement information published in various issues of the Bureau of Labor Statistics periodical *Current Wage Developments* and in the Bureau of National Affairs periodical *Government Employee Relations Reporter.* Contract wage changes include any changes imposed as a result of unexpected ex post contract renegotiations or reopenings.

6. I thank Chris Erickson and the authors for independently pointing out this possibility to me.

bumping being responsible for the bulk of reported nominal wage cuts in my sample. I also redid my experiment separating workers who report changing occupations from workers who do not; the incidence of reported nominal wage cuts among occupation switchers was only slightly higher (21.4%) than among occupation stayers (21.0%). Given that bumped workers should have a higher incidence of occupation switches than unbumped job stayers, this result again suggests that bumping is not very important in my sample.

My conclusion from this section is that it is difficult to say how important DNWR is to the labor market using the distribution of individual wage changes alone. We can adjust Card and Hyslop's sweep-up estimates to account for the exclusion of movers and salaried workers rather easily. But with existing data, it is hard to say how much we should adjust the authors' estimates for measurement error or for asymmetry in the counterfactual distribution.

Method Two: Phillips Curves

Given the problems with using individual wage distributions, the authors should be commended for formulating an alternative approach to estimating the impact of DNWR on the labor market. Recall from figures 2C.1 and 2C.2 that DNWR increases (decreases) the sensitivity of employment (wages) to labor-demand shocks, the more so the lower is the inflation rate. In the latter part of their paper, Card and Hyslop test this implication by looking for interactions between inflation and the slope of the Phillips curve in the United States. Since such interactions would probably be impossible to detect in aggregate data, the authors cleverly exploit cross-state variation in unemployment and wage growth to estimate separate Phillips curves year-by-year from 1976 through 1991. The authors find that higher state-level unemployment significantly reduces state-level wage growth in each year. They also find that the Phillips curve (plotted with unemployment on the horizontal axis) is steeper when inflation is high, consistent with DNWR, but that this interaction is imprecisely estimated and insignificantly different from zero.

I think the authors' approach has excellent potential as a tool for assessing the impact of downward nominal rigidity and other sorts of frictions on the labor market. I have two suggestions for making this tool sharper. First, the authors need more degrees of freedom. With fifty U.S. states, the authors have enough cross-section observations to estimate the year-by-year Phillips curve slopes reasonably precisely. However, with only sixteen years of data, the authors do not have enough slopes to estimate the interaction between inflation and the slope precisely. The authors could alleviate this problem either by getting more years of data for the United States, or by including other countries for which regional wage and employment information is available.[7]

7. Of course, expanding the data set would limit the extent to which the authors could correct wages for the skill composition of the workforce (as they currently do using the CPS). But this shouldn't be problematic if the skill distribution at the regional level does not vary much over the business cycle, an issue the authors could investigate directly with the CPS or the PSID.

Second, the authors need to pay careful attention to endogeneity issues. What the authors presumably want to estimate each year is the relative responsiveness of wages and unemployment to labor-demand shocks. An ordinary least squares regression of wage growth on unemployment will estimate the Phillips curve consistently only if all cross-state variation in unemployment is due to cross-state variation in the position of the labor-demand curve. It is easy to think of reasons why this condition would not hold. For instance, suppose that nominal wage growth is predetermined for union workers, but flexible for nonunion workers, and suppose that states differ in the extent of unionization. Now suppose the inflation rate changes unexpectedly. Real wages and unemployment would move in the same direction as firms moved along their labor-demand curves, causing the Phillips curve to shift, and this shift would be more pronounced in more heavily unionized states. In this example, then, unexpected inflation shocks would bias the estimated cross-section Phillips curve toward zero. Of course, what the authors are most interested in is not the slope of the Phillips curve, but rather the interaction of the slope with inflation. In this example, if the conditional variance of inflation is uncorrelated with the level of inflation, then the authors have nothing to fear. But if unexpected inflation shocks are more likely at higher levels of inflation, then high-inflation periods will also be periods in which the slope estimates are more biased toward zero, masking the interaction between inflation and the slope predicted by DNWR. To avoid such problems, the authors should estimate their Phillips curves instrumenting for state-level unemployment, using measures of state-level labor demand.[8]

Conclusion

Overall, I find Card and Hyslop's central conclusion—downward nominal rigidity has a positive but economically small impact on the labor market—sensible and well-founded. The reader should be cautioned, however, that the authors' results in no way prove that nominal rigidities are unimportant to labor market fluctuations. Downward nominal rigidities are only one type of nominal friction; even if downward rigidity is not important, generalized nominal wage stickiness or nominal illusion may still matter.

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8. For example, the authors could capture the lahor-demand-driven element of cross-state unemployment variation by computing a weighted average of disaggregated national-industry-level employment or output growth rates, using state-level industry-employment shares as weights, as in Blanchard and Katz 1992.

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