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

David Card
Princeton University

and

Dean Hyslop
University of California, Los Angeles

December 1995

* Prepared for the NBER Conference on Monetary Policy and Low Inflation, January 1996. We are grateful to Christina and David Romer for comments and suggestions, and to John DiNardo for many helpful discussions on the material and methodology in this paper. We also thank David Lee for extraordinary research assistance.
Does Inflation "Grease the Wheels of the Labor Market"?

ABSTRACT

One of the basic tenets of Keynesian economics is that labor market institutions cause downward nominal wage rigidity. We attempt to evaluate the evidence that relative wage adjustments occur more quickly in higher-inflation environments. Using matched individual wage data from consecutive years, we find that about 6-10 percent of workers experience wage rigidity in a 10-percent inflation environment, while this proportion rises to over 15 percent when inflation is less than 5 percent. By invoking a simple symmetry assumption, we generate counterfactual distributions of wage changes from the distributions of actual wage changes. Using these counterfactual distributions, we estimate that, over the sample period, a 1 percent increase in the inflation rate reduces the fraction of workers affected by downward nominal rigidities by about 0.5 percent, and slows the rate of real wage growth by about 0.06 percent. Using state-level data, the analysis of the effects of nominal rigidities is less conclusive. We find only a weak statistical relationship between the rate of inflation and the pace of relative wage adjustments across local labor markets.

David Card
Department of Economics
Princeton University
Princeton NJ 08544
and NBER

Dean Hyslop
Department of Economics
UCLA
Los Angeles, CA 90095
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. A direct implication of this downward nominal rigidity hypothesis is that inflation eases labor market adjustments by speeding the decline of relative wages for individuals and markets buffeted by negative shocks.\footnote{This hypothesis is spelled out e.g. in Tobin (1972).} According to this argument, very low levels of inflation impede labor market efficiency, whereas modest inflation serves to "grease the wheels" of the labor market and reduce frictional unemployment. In sharp contrast to the traditional Keynesian view, an emerging orthodoxy among many economists and central bankers is that \textit{stable} aggregate prices reduce labor market frictions and lead to the lowest 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 micro panel data to examine the evolution of individual real wages over time.\footnote{Previous studies of the extent of nominal rigidity in individual wage data include McLaughlin (1994) and Kahn (1995).} 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 environment 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 micro-level 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 multi-year 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 also enables us to examine the extent of nominal rigidity over longer time frames (1, 2, and 3 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 on the same job. In a typical year during the 1980s, 15-20 percent of non-job changers had measured nominal wage declines, and a similar fraction had nominal wage increases in excess of 10 percent. Second, the most likely nominal wage change is 0; on average during the 1980s, about 15 percent of non-job changers had 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 1.0-1.5 percentage point increase in the incidence of nominal rigidity.

The presence of a large "spike" at 0 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 nominal rigidities in a typical

---

3 Of course, some fraction of this measured variation is attributable to survey measurement error.
year in the 1980s resulted in a maximum of about a 1 percentage point faster real wage growth for non-job-movers, on average.

Our market-level analysis uses state-level average wages and unemployment from 1976 to 1991. The wage data are constructed from the annual March Current Population Survey, 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 in states with higher unemployment fall (relative to national trends), while real wages in states with lower unemployment rise. However, we find little evidence that the rate of local wage adjustment is faster in a higher-inflation environment. Taken in combination with our micro-level findings, these results imply that nominal rigidities have at most a small effect on the aggregate economy, and that any efficiency gains from the "greasing" effect of higher inflation are probably quite modest.

I. Descriptive Analysis of the Distribution of Individual Wage Changes

a. 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 Current Population Survey. 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

---

4 The data pertain to the individual's main job as of the survey week, and are not collected for self-employed workers.
approximately one-eighth of all wage and salary workers in the overall sample) will be interviewed again in 12 months and asked the same earnings questions. The other half were interviewed 12 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 2 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.\

For most of our analysis of the CPS data we restrict attention to the roughly 50 percent of individuals who were paid by the hour in both years of the panel. Ideally, since most models of nominal wage rigidity pertain to workers who stay on 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 (2-digit) industry and occupation in the two years, and those who report a change in industry or occupation. Finally, in order to minimize the confounding effects that institutionally determined minimum wage rates may have on the analysis of nominal rigidities, most of our analysis using the CPS samples also excludes observations which are directly affected by minimum wage regulations.

---

5 Details of the matching algorithm and other information on the CPS samples are presented in the Data Appendix. We do not use imputed wage data that are allocated in the CPS files to non-respondents.

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 Of course many of the observed industry or occupation switches are attributable to misclassification error (see Krueger and Summers (1988) for a discussion of industry misclassification rates). Changes in the industry and occupation coding system introduced between 1981 and 1983 necessitate slightly different procedures in these years - see note (a) to Table A1.

8 DiNardo, Fortin, and Lemieux (1995) present evidence that minimum wages exert a major influence on the lower tail of the wage distribution. We consider workers to be directly affected by the minimum wage if either w,
Our second source of data is the Panel Study of Income Dynamics (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. Although the PSID has far fewer observations than the CPS panels, and tends to over-represent 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. Third, the PSID follows individuals who change addresses, while the CPS cross-sections can only be matched 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.

\[ mw_t, \text{or } w_t = \log(wage \text{ in year } t), \text{mw}_t = \log(\text{minimum wage in year } t), \text{and } \pi_t = \text{the rate of inflation}. \]

\[ \text{Binding minimum wages may either increase or decrease the extent of measured rigidity, depending on whether the nominal minimum wage is constant or increases. Table 1 provides a comparison of the rigidity rates obtained from including and excluding individuals affected by the minimum wage.} \]

\[ \text{Because of changes in household composition, labor force entry and withdrawal, and the aging and refreshing of the PSID sample, the two panels have only about xx percent of individuals in common.} \]

\[ \text{This point, however, should be tempered by the evidence provided by Brown and Light (1992) that substantial errors occur when measuring job changes in the PSID. Their recommended procedure, and that adopted in this paper, is to assume "a job change has occurred whenever reported tenure is less than elapsed time since the previous interview" (page 221).} \]

\[ \text{Regular monthly earnings data is only available in the CPS beginning in January 1979. For the period from 1973 to 1978 earnings data was collected in the May CPS; in future work we hope to be able to use matched May CPS data from this period.} \]
b. 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 1a contains the histograms for the 14 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 wage changes at +/-0.35: the masses at the upper and lower extremes represent the cumulative fraction in each tail of the distribution. A vertical line at minus the annual inflation rate (-πt) is drawn for each year to identify the real wage change associated with fixed nominal wages.  

The histograms show that real wage changes tend to be centered around 0, with a prominent "spike" at -πt (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 percent; in the mid-to-late 1980s, when inflation was at or below 5%, 15-20 percent of workers had constant nominal wages. Interestingly, it appears that there is a deficit in the distribution of wage changes to the left of -πt, suggesting that the distribution of real wage changes is being "swept up" to the floor imposed by rigid nominal wages. Nevertheless, there is still a considerable fraction of non-job-changers who experience reductions in their measured nominal wages -- typically from 15-20 percent.

Figure 1b presents the corresponding histograms of real wage changes for the PSID samples of hourly-rated workers in the same job in each year.  

---

12 Throughout the paper we measure inflation by πt = log(Pt/Pt−1).

13 We assume that workers are in the same job in both years (t and t-1) if tenure = tenure_t > date_t - date_{t-1} + 12, where tenure_t is the job tenure (in months) as at the year t interview date, and date_t is the calendar month in which the year t interview was conducted. Also, the measures of job tenure differ in the two periods: during the 1976-79
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.\textsuperscript{14} In particular, the PSID data also show a prominent spike in the distribution of real wages changes at $-\pi$. 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 1b show a deficit to the left of the spike, suggesting that the real wages of some workers who might otherwise have experienced large wage reductions were "held up" by nominal wage rigidity.

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 1b, leading her to conclude that there is significant downward nominal rigidity, and also 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. Unlike Kahn (1994), he finds only limited evidence of a spike in distribution of wage changes at zero nominal wage growth (see his Figure 4). Furthermore, McLaughlin finds no apparent deficit in the distribution of wage changes to the left of the spike. As of this writing we are unsure of the

\textsuperscript{14} Appendix Figure A1 shows the distributions of wage changes for all workers in the PSID who report wages in each year -- i.e. including non hourly-rated workers and those who change jobs. The patterns are similar to those in Figure 1b, 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.
reasons for the discrepancies between McLaughlin’s analysis and those presented here and by
Kahn.\footnote{Kahn (1994) is also puzzled by the differences between her findings and those of McLaughlin (1994). We are currently pursuing several alternative explanations.}

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 rigidity in decade-to-decade wage changes. To get a sense of the effects of different time frames, Figures 2a and 2b present histograms of real wage changes over 2 and 3 year time horizons, respectively, 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 1b, although the magnitude of the spike corresponding to rigid nominal wages is smaller. During the low-inflation period 1985-88, about 10 percent of hourly rated non-job-changers had constant wages over two years, compared with only 3 percent in the high-inflation period 1976-79. Over a three-year horizon, the fraction of observations with rigid wages is about 5 percent in the low-inflation era, and about 1 percent in the late 1970s. Some degree of nominal wage rigidity clearly persists over longer than a year. Furthermore, long-term rigidity is more pervasive during low-inflation periods than high-inflation periods.\footnote{Appendix Figures A2a and A2b contain the histograms for 2 and 3-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.}

Tables 1 and 2 summarize some of the information contained in the histograms in Figures 1 and 2. Table 1, which pertains to our CPS samples of hourly-rate workers, presents the annual
inflation rate, the unemployment rate,\textsuperscript{17} the median nominal wage change for all hourly-rated workers, the fraction 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 pertains to the PSID data, and shows the inflation rate and the fraction of workers with rigid nominal wages over 1-2- and 3-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 1 and 2, and Tables 1 and 2 present a reasonable \textit{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 percent report exactly the same nominal wage in one year as the next.\textsuperscript{18} Furthermore, the extent of rigidity is higher, the lower the rate of inflation. Indeed, the simple correlation between the inflation rates in column 1 of Table 1 and the fraction of rigid wages in column 6 is -0.96. Finally, inspection of the histograms in Figures 1 and 2 suggests that much 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 III 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

\textsuperscript{17} Measured as the average unemployment rate during the ending year of each change.

\textsuperscript{18} Note that any (classical) measurement error in wages is likely to lead to an over-statement of the probability of nominal wage declines and an understatement in the probability of rigid nominal wages.
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.

II. Is the Extent of Nominal Rigidity Overstated?

a. 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.19 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.20 The results of this analysis suggest that the incidence of rigid nominal wages is
slightly higher for non-hourly workers. For example, between 1979 and 1980, 7.4 percent of
"always hourly-rated" workers with no change in industry or occupation had rigid nominal wages,
versus 10.9 percent of "always non-hourly" workers. Similarly, between 1987 and 1988 16.4
percent of "always hourly" workers had rigid wages, versus 18.4 percent of "always non-hourly"
workers. There are some other differences between the distributions of real wage changes for
hourly 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

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

20 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.
pay for non-hourly rated workers with the same industry and occupation is about 25-50 percent 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 workers are larger than the errors in hourly pay for hourly 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.

b. 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 percent reported an even dollar wage amount in 1984 and another 14 percent 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 percent of individuals who reported an even wage amount in 1984 had rigid nominal wages between 1984 and 1985, versus a rigidity rate of only 9.2 percent for individuals who reported a wage amount not ending in either .00 or .50. Over the various years of our matched CPS
samples, individuals who reported an even dollar wage amount in the base year typically accounted for 55-60 percent 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 50 cent intervals. If actual wages are "spikey", 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 amount 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 line of reasoning comes from the fact that the R-squared coefficient for a conventional human capital model is slightly higher when the model is fit to the subsample of workers who report a "rounded" wage amount than when the same model is fit to the subsample of workers who report a wage that does not end in .00 or .50.\footnote{Specifically, we fit a model to the log hourly wage for hourly-rated workers who report a wage ending in .00 or .50 and for those with other wages. The included covariates included education, a cubic in experience, non-white, hispanic, female and non-white*female dummies, and indicators for living in the south, in a central city, and in the suburbs. In all years, the R-squared coefficients for the model are higher for the subsample of workers with even wage amounts.} Based on this evidence, one might conclude that the signal-to-noise ratio in measured wages is higher for people who report a "rounded" wage: a conclusion that is inconsistent with the hypothesis that rounding is a result of measurement error.

To further study this issue we used data from a January 1977 CPS supplement that collected self-reported wage information from workers and matching information from their employers.\footnote{The survey was conducted in part to assess the reliability of workers' self-reported earnings information.} Among hourly-rated workers paid above the minimum wage the probability of a "rounded" wage (ending in either .00 or .50) is 30.0 percent -- slightly under the rate of 37.8
percent in our matched 1979-80 CPS sample. The probability that the employer reports a "rounded" wage is lower (20.0 percent) but is far from negligible. Overall, the probability that the employer and employee reports agree exactly is 44.2 percent, with a significantly higher agreement rate (69.0 percent) conditional on the employer reporting a "rounded" wage.

If the employer reports are treated as truth (the hypothesis underlying the original design of the supplement) then these data imply that just under one-half of the observed mass at "rounded" wage values is attributable to true spiking, with the remainder attributable to rounding errors. Under this assumption, rounding of wage reports can explain very little of observed wage rigidity. An alternative hypothesis, however, is that the underlying wage distribution has no spikes, and that both employees and employers tend to "round" their wage reports. We decided to evaluate the implications of this assumption by performing a simple simulation. Using some reasonable assumptions on the underlying wage model and on the probabilities of reporting a rounded wage amount in consecutive years, our simulations imply that rounding can account for a 4 percent rate of apparent nominal wage rigidity when the inflation rate is 5 percent and there is zero median wage growth. This is about one-fourth of the observed nominal rigidity rate in the CPS or PSID data for the mid-1980s. We believe this estimate is an upper bound on the fraction of observed nominal rigidity that can be attributed to rounding behavior. If some individuals who

---

23 The fraction of wages reported at even dollar or half-dollar amounts rose over the 1980s from 38 percent in 1979 to 48 percent in 1984 to 56 percent 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.

24 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 (p₁) a worker reports the true wage; with some probability (p₂) the worker rounds the wage to the nearest even 50 cent amount; and with some probability (1−p₁−p₂) 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 p₁=p₂=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.
report "rounded" wage amounts are truly paid such wages, 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.

A second feature of the data presented in Figures 1a and 1b argues against rounding as an explanation for all or even most of observed nominal wage rigidity. Provided that individuals round to the nearest even wage amount, rounding will cause true wage change observations above and below 0 to be drawn toward 0. Rounding cannot explain an apparent deficit in the distribution of observed real wage changes to the left of the spike at minus the inflation rate (-\(\pi\)) without simultaneously predicting a deficit to the right of -\(\pi\). In this regard, rounding by employees is similar to the existence of a "menu cost" that causes employers not to adjust a worker's actual wage if the optimal wage adjustment is small. To the extent that the histograms in Figures 1a and 1b show larger deficits to the left of spike than to the right, the data imply that rounding is an incomplete explanation for observed nominal wage rigidity.

III: Measuring the Effect of Inflation on Wage Rigidities

a. 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 3a under the assumptions that \(m=0\), that the inflation rate \(\pi\) is 5 percent, and that one half of individuals are affected by downward rigidities. As shown in 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 distribution at $-\pi$.\footnote{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 \textit{observed} wage changes will exist regardless of this possibility.} 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 equivalently, rounding in reported wage levels). For example, suppose that if the "optimal" nominal wage change is between $\pm x$ percent, then there is some probability that the nominal wage will not change. Figure 3b illustrates this scenario when menu costs are present for wage changes of up to $\pm 2$ percent, and the probability of non-adjustment declines symmetrically from 25 percent for a zero wage change to 0 for a 2 percent nominal wage change. To the extent that the density is not constant around $-\pi$, this assumption implies that menu costs induce \textit{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, then we would expect to see a deficit in the distribution of real wage changes immediately to the right of $-\pi$, a somewhat larger deficit to the left of $-\pi$, and a spike at $-\pi$ that is larger 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 \textit{shifted down} to 0 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 \textit{shifted up} to 0 to obtain an estimate of the net effect of downward rigidity in the presence of menu costs or rounding errors in the reporting of wage levels.
b. 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 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 individual 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. With regard to the second assumption, if the median nominal wage change is sufficiently positive, then wage changes at or above the median will be largely unaffected by nominal rigidities. Over our sample period the smallest median nominal wage increase is 3.8 percent (see Table 1). Thus we believe that assumption (2) is reasonable.

The third assumption is more problematic. Indeed, since much of the interest in downward nominal wage rigidity is driven by a concern over potential employment effects, the

---

26 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.
assumption that any employment effects may be ignored is troubling. One way to relax assumption (3) is to assume (as a "worst case scenario"): 

\[(3')\] a fraction \(2\alpha\) 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 percent 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 \(f_a(x)\) denote the probability density function of observed real wage changes in some period (for some given sample of workers). Let \(\tilde{f}_a(x)\) denote the counterfactual density function. Then assumptions (1)-(3) or (1)-(3') imply:

\[
\tilde{f}_a(x) = k_c f_a(x), \quad x \geq c
\]

\[
\tilde{f}_a(x) = k_c f_a(2c-x), \quad x < c
\]

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.

\(^{27}\) A closely related alternative approach to construct a counterfactual distribution for wage changes is to "symmetrise" the distribution around the mode of wage changes. This method is conceptually appealing if the underlying distribution is unimodal with the mode equal to the median. We also tried this approach, but found that the resulting distribution to be too sensitive to the empirical position of the mode. Also, in several years the mode is greater than the median, which is theoretically implausible.
Using the fact that $\int_{0}^{c} f(x) \, dx = .5(1 - F(c))$, where $F$ is the distribution function associated with $f$. Note that if $c = \mu$ (the observed median) then $F(c) = 0.5$ and $k_c = 1$. Otherwise, if $c$ is the $0.5 - \alpha$ quantile, then $k_c = 1/(1 + 2\alpha) = 1 - 2\alpha$.

c. 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 measure 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.

1. Density Effects

In principle, nominal wage rigidities can affect both people whose wages would have fallen in the absence of rigidity, and people whose wages would have otherwise risen. Thus, we further decompose the fraction 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 $-\pi_i$):

$$ su_i = \int_{-\pi_i}^{\infty} (\tilde{f}_e(x) - f_e(x)) \, dx = \tilde{F}_e(-\pi_i) - F_e(-\pi_i) $$
where the upper limit of integration excludes the mass-point at \(-\pi_c\), and \(\tilde{F}_w()\) and \(F_w()\) are the CDFs corresponding to \(\tilde{f}_w()\) and \(f_w()\) respectively. The latter is the cumulative density of the counterfactual distribution that has been "swept back" to the nominal-wage rigidity spike:

\[
(2) \quad s_h = \int_{-\pi_c}^{m_t} \left( \tilde{f}_w(x) - f_w(x) \right) \, dx = \left( \tilde{F}_w(m_t) - \tilde{F}_w(-\pi_c) \right) - \left( F_w(m_t) - F_w(-\pi_c) \right)
\]

where \(m_t\) is the median real wage change in year \(t\), and the lower limit of integration again excludes the mass-point at \(-\pi_c\). (Note that by assumption (2) above, we need only extend the upper bound of integration to the median). The total fraction of individuals affected by rigidities is \(s_u + s_h\), 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 \(f_w(x)\) and \(\tilde{f}_w(x)\) are available then \(s_u\) and \(s_h\) can be constructed directly.\(^{28}\) In the absence of menu costs, \(s_u\) provides an estimate of the density displaced by

\(^{28}\) Alternatively, using the definition of the counterfactual density it is easy to show that

\[
(1') \quad s_u = k_c \left( 1 - F(2c+\pi_c) \right) - F_w(-\pi_c),
\]

where \(F\) is the distribution function of observed wage changes in year \(t\), \(c\) is the point of symmetry for the counterfactual, \(k_c\) is the constant defined earlier, and \(F(x)\) denotes the cumulative density up to \(x\) but excluding any mass-point at \(x\). 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

\[
\begin{align*}
 s_u & = (1 - F(2m_t + \pi_c)) - F_w(-\pi_c), \\
 \text{and if } m_t & = 0 \text{ (which is roughly true for most of our sample years) then} \\
 s_u & = (1 - F(\pi_c)) - F_w(-\pi_c),
\end{align*}
\]

which represents a simple difference between the fraction of real wage changes above \(\pi\) and the fraction below \(-\pi\). Similarly, the fraction of the density swept back can be written as:

\[
(2') \quad s_h = k_c \left( F(2c+\pi_c) - F(2c-m_t) \right) - \left( F(m_t) - F_w(-\pi_c) \right),
\]

which, if the point of symmetry is set to the median, reduces to

\[
\begin{align*}
 s_h & = F(2m_t + \pi_c) . .5 - \left( .5 - F_w(-\pi_c) \right),
\end{align*}
\]
downward wage rigidities. However, in the presence of menu costs, su, overestimates to true
effect due downward nominal wage rigidity. If the rigidity effects of menu costs act
symmetrically around zero nominal change, in the sense described in figure 3(b), then the sweep-
up net of the sweep-back effect will provide a lower bound for the effect of downward nominal
wage rigidity. That is, the total density displaced due to menu costs will be lower below the
spike point than above it, and the net density swept-up (su - sb) will underestimate the true effect
due to the downward nominal rigidity.

2. 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

\[
(3) \quad w_{su} = \int_{x_s}^{x_t} (\tilde{f}_w(x) - f_w(x))(-\pi_t - x) \, dx
\]

\[
= -\pi_s su_s - E(\Delta w|\Delta w < -\pi_s) \tilde{f}_w(-\pi_s) + E(\Delta w|\Delta w < -\pi_s) f_w(-\pi_s) F_w(-\pi_s),
\]

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

\[
(4) \quad w_{sb} = \int_{x_s}^{x_t} (\tilde{f}_w(x) - f_w(x))(-\pi_t - x) \, dx
\]

\[
\text{or, to}
\]

\[
b_{sb} = F(\pi_s) - .5 - (.5 - F_w(-\pi_s)).
\]

if m=0. This last expression is simply the fraction of observed wage changes between \(\pi_s\) and the median, minus
the fraction between the median and \(-\pi_s\).
\[
\begin{align*}
\pi_s, sb, + E(\Delta w | -\pi_s < \Delta w \leq m_s) \cdot (\tilde{f}_w(m_s) - \tilde{F}_w(-\pi_s)) \\
- E(\Delta w | -\pi_s < \Delta w \leq m_s) \cdot f_w(m_s) \cdot (F_w(m_s) - F_w(-\pi_s)),
\end{align*}
\]

which we refer to as "wage sweep-back".

Again, if estimates of the densities \( f_w(x) \) and \( \tilde{f}_w(x) \) are available, these expressions can be evaluated directly.\(^{29}\) Also, \( w_{su} \) and \( (w_{su} - w_{sh}) \) provide upper- and lower-bound on the aggregate wage effects of nominal downward rigidity in the absence, and presence, of menu costs respectively.

d. Kernel Density Estimates of the Actual and Counterfactual Distributions

We have used standard kernel estimation techniques to construct smoothed estimates of the densities of real wage changes in our CPS and PSID samples, and estimates of counterfactual densities using various percentiles of the distribution as a point of symmetry. In contrast to histograms, which simply compute the relative frequency of the data which lies in various intervals of the range of wage changes, kernel density estimation computes a weighted average of the data near to some point, for various values of wage changes. In particular, the kernel estimator for the density at some value \( x \) is

\[
\hat{f}_w(x) = \frac{1}{nh} \sum_{i=1}^{n} K\left( \frac{x - x_i}{h} \right),
\]

\(^{29}\) Alternatively, using the definition of the counterfactual density, it is straightforward to show that

\[
\text{counterfactual density, it is straightforward to show that}
\]

\[
\begin{align*}
(w_{su}) = k_s \cdot (1 - F(2\pi_s) - \{ E(\Delta w | \Delta w \geq 2\pi_s) \cdot \pi_s \} \cdot F_w(\pi_s) \cdot \{ -\pi_s \cdot E(\Delta w | \Delta w \leq -\pi_s) \})
\end{align*}
\]

which 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 \( w_{sh} \).
where \( n \) is the number of observations, \( h \) is the \textit{bandwidth} (sometimes called the \textit{window width}), and \( K(.) \) is the kernel, which satisfies

\[
\int_{-\infty}^{\infty} K(x) dx = 1. \quad (30)
\]

The use of kernel estimates provides a clearer visual appreciation of the differences between the actual and predicted distributions of wage changes, than is obtained using simple histograms.

The actual and median-counterfactual densities for the CPS samples are shown in Figure 4. As is true of the simple histograms in Figure 1a, 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 3b. The observed data seem to show both downward nominal rigidity effects \textit{and} the presence of menu costs associated with small wage changes.

To better pinpoint the differences between the actual and counterfactual distributions, Figure 5 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 \(-\pi_n\). Specifically, for each point below the spike (i.e., for each wage change \( \Delta w < -\pi_n \)), we estimate

\[ \frac{1}{n} \sum_{i=1}^{n} \mathbb{1}_{\{\Delta w_i < -\pi_n\}} \]

\[ \hat{F}(\Delta w) = \frac{1}{n} \sum_{i=1}^{n} \mathbb{1}_{\{\Delta w_i < -\pi_n\}} \]

\[ \hat{F}(\Delta w) = \frac{1}{n} \sum_{i=1}^{n} \mathbb{1}_{\{\Delta w_i < -\pi_n\}} \]

---

\( ^{30} \) Silverman (1986) provides a full treatment of the issues involved with density estimation. We estimate each of the densities at 250 equi-spaced 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.
\[
G(\Delta w) = \frac{\int_{-\pi}^{\pi} (\hat{f}_u(x) - f_u(x)) \, dx}{\int_{-\pi}^{\pi} \hat{f}_u(x) \, dx}.
\]

Similarly, for each point between the spike and the median (i.e., for each wage change \(-\pi < \Delta w < m\)), we estimate
\[
G(\Delta w) = \frac{\int_{-\pi}^{\Delta w} (\hat{f}_u(x) - f_u(x)) \, dx}{\int_{-\pi}^{\Delta w} \hat{f}_u(x) \, dx}.
\]

In practice, we set the limits of integration around the spike point to be \(-\pi^- = -\pi - 0.0025\) and \(-\pi^+ = -\pi + 0.0025\). If downward nominal rigidities prevent some individuals' real wages from falling faster than the inflation rate, then \(G(\Delta w)\) will be positive for all \(\Delta w < -\pi\). Indeed, in the simple case where a fixed fraction \(f\) of real wage declines bigger than \(-\pi\) 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 \(-\pi < \Delta w < m\).
For each year we have graphed the $G(\Delta w)$ functions in Figure 5 after re-normalizing the real wage changes in a particular year relative to the spike point. That is, we graph $G(\Delta w + \pi_0)$, 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 $\Delta w$ in the left-hand tail of the distribution, and in the range from one-quarter to one-half; below, but near to, $-\pi$, the fraction displaced shows a sharp increase to one-half or more; and above $-\pi$, $G(\Delta w)$ falls off steadily from about one-third. 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 around one-half of the observed rigidity.

e. Estimates of the Effects of Nominal Rigidities

We have used the differences in the actual and counterfactual distributions illustrated in Figures 4 and 5 to estimate the four summary measures of the effect of nominal wage rigidity ($su$, $sb$, $wsu$, $wsh$) described in equations (1) - (4). In implementing the formulas we restrict the 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, and we again choose the limits of integration "just below" and "just above" the spike point to be $-\pi$, minus and plus 1/4 percentage point, respectively. Table 3 presents estimates of the density displacement effects $su$, and $sb$, for two choices of the point of symmetry in each year: the median real wage change, and the 49th percentile real wage change. Recall that the latter is appropriate under the assumption that 2 percent of potential wage change observations are missing because of employment responses to downward wage rigidity.
Consider first the estimated sweep-up effects ($su$) presented in columns 2 and 3. Under the median counterfactual, nominal wage rigidities are estimated to affect between 5.4 and 7.3 percent of hourly-rated non-job-changers during the high inflation years from 1979 to 1982, and between 9.7 and 13.5 percent 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 percent during the high inflation years, and between 10.6 and 14.5 percent during the low inflation years.

The estimated density sweep-back effects ($sb$) in columns 4 and 5 are generally much smaller than the sweep-up effects, although there is a statistically significant negative relationship between sweep-back and the inflation rate. If the sweep-back effects are interpreted as estimates of the effects of menu costs, and if menu costs have the same relative effect on wage change observations on either side of the spike point, then the difference ($su - sb$) 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 percent.

Simple regressions of the $su$ effects on $\pi$, yield statistically significant coefficients of -0.64 and -0.76 using the median and 49th-percentile counterfactuals respectively, with $t$-statistics of 3.3 and 4.4. Analogous regressions of the net sweep-up effects ($su - sb$) on $\pi$, yield smaller and much less significant coefficients of -0.31 and -0.52, with $t$-statistics 0.9 and 1.7. As the true nominal rigidity effect is bounded between $su$ and ($su - sb$), this provides some evidence that inflation helps to reduce the effects of downward nominal wage rigidities. For example, using a ballpark average of these four coefficients of -0.5 means that a 5 percentage point increase in inflation (from 3 percent to 8 percent, say) is associated with a 2.5 percent drop in the fraction of workers whose wages are affected by nominal rigidities.
Table 4 presents the estimated average real wage change effects \( w_{su} \) and \( w_{sb} \). These vary over the sample period with generally larger effects in low-inflation years. Again, the estimates 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 1.3 percent, with an average effect of about 1 percent 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 percent, and the estimates are typically less than 0.1 percent. On net, our estimates imply that nominal rigidities may have contributed to about 1 percent 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.

The correlation of the wage sweep-up (\( w_{su} \)) and net wage sweep-up (\( w_{su} - w_{sb} \)) effects with the aggregate inflation rate are negative and significant. Regressions of the wage effects -- i.e. \( w_{su} \) or \( (w_{su} - w_{sb}) \), obtained using either the median or 49th-percentile counterfactuals -- on the corresponding inflation rates over the 14 years in our CPS sample yield coefficients between -0.057 and -0.064, with t-statistics between 1.9 and 3.5. These estimates imply that a rise in the inflation rate from 3 percent to 8 percent is associated with about 0.3 percent 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.

This conclusion provides one possible insight into the “fact” that many individuals dislike inflation (see Shiller (1995)). Our estimates suggest that a lower inflation rate acts like a higher minimum wage in first differences. Indeed, the similarity between the histograms in Figures 1a and 1b and histograms of real wage levels in the presence of a binding minimum wage is
remarkable. The patterns in Figure 5 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. Since many 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.

IV. 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 focusses 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,

\[
\Delta w_i = b'U_j + \varepsilon_i, \tag{5}
\]

where \( \Delta w_i \) is the real wage change for individual \( i \) in market \( j \) (over some specific time horizon) and \( \varepsilon_i \) 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:

\[
\Delta w_i = b'U_j + \varepsilon_i, \quad b'U_j + \varepsilon_i > -\pi \\
= I_{1-b}(-\pi) + (1-I_{1-b}) \cdot (b'U_j + \varepsilon_i), \quad b'U_j + \varepsilon_i < -\pi, \tag{6}
\]
where $I_{ij}$ is a random indicator variable with mean $f$. 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:

$$(7) \quad \mathbb{E}(\Delta w_{ij} U_j, \pi) = a(\pi) + b(\pi) U_j,$$

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 micro data 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. We then fit a variety of models of the form:

$$(8) \quad w_{jt} - w_{j,t-1} = a_t + b_t U_j + \epsilon_{jt},$$

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

---

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

32 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.
Several 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 non-random movements in and out of the labor market, this is not a problem, since with a fixed population $E(\Delta w_{ij}) = E(w_{ij}) - E(w_{ij,1})$ (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 et al (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.\footnote{We plan to investigate this issue more thoroughly using matched March CPS files.} Second, equation (8) implies that real wage growth rates are linear in the unemployment rate, whereas most of the Phillips curve literature suggests that the effect is nonlinear. In the actual estimation, we therefore use the log of the unemployment rate as a cyclical indicator.

Finally, 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 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.\footnote{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.}

Some evidence on this specific issue, and on the general performance of equation (8), is presented in Table 5. Here we summarize the results of estimating various versions of (8) without allowing the coefficient $b$ to vary across years. As shown in row 1 of the table, using adjusted wage growth, when $b$ is restricted to be constant, the resulting estimates suggest that wage growth is relatively responsive to local unemployment: for example, a doubling of the unemployment reduces the rate of growth of wages by 1.7 percent per year. The specifications in rows 2 and 3 add in lags of the unemployment rate. Consistent with the conventional Phillips curve, but contrary to the wage curve specification, these are statistically insignificant. A similar conclusion holds when region effects representing the 9 Census divisions are added to the model (rows 4 and 5); when region-by-year effects are added (rows 6 and 7); when the model is estimated using average observed wages, rather than adjusted average wages (row 8); and when the model is estimated without weighting the state-level observations by relative population (row 9).

Based on the results in Table 5 we then proceeded to estimate a series of specifications that excluded lagged unemployment, but allowed the coefficient on current unemployment to vary across years. Estimates of the key parameter $b$, from one such specification, which also included region effects, are plotted in Figure 6 against the inflation rate for the same year. As shown in the Figure, the estimates of $b$ vary somewhat from year to year, with most of the individual
estimates being significantly different from 0 at conventional significance levels.\(^\text{35}\) However, the magnitude of the estimated coefficients is only weakly negatively related to the inflation rate (the t-statistic for the implied regression coefficient is 0.9). Thus, at the state level of aggregation, we find only very limited evidence that the rate of adjustment of wages to local labor market shocks is faster in periods of higher inflation.

V. 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: micro-level 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 micro analysis 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 0 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 percent 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 percent. 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. During the mid-1980s, we estimate that downward nominal rigidities may have "held up" average real wages by 1 percent per year.

\(^{35}\) Only 3 of the 15 coefficients shown in the Figure (for 1976-77, 1987-88, and 1990-91) are not statistically different from 0.
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.
References


Kahn, Shulamit. "Evidence of Nominal Wage Stickiness from Microdata", Manuscript, Boston University School of Management.


Data Appendix

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 16 and older in rotation group 4, drawn from 80,557 uniquely identified households. All of these individuals were potentially re-interviewed 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 slippage in matching if an individual mis-reports a key characteristic (like race or age), or if a household contains two very similar people. These limitations imply that about 25-30 percent of individuals are unmatchable.

We matched individuals in rotation group 4 of year t with individuals in 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 percent more successful matches than a strict requirement that age increments by 1). The overall match rates are between 70 and 75 percent in every year except 1984-85 and 1985-86. For example, 74.5 percent of the 164,626 individuals in rotation group 4 of the 1979 sample are successfully matched to a 1980 observation, and 74.4 percent 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 percent of all individuals in the 1984 sample) and 1985-86 (18.3 percent of all individuals in
the 1985 sample).
### Table 1: Characteristics of Wage Change Distributions, CPS Samples

<table>
<thead>
<tr>
<th></th>
<th>Aggregate Data:</th>
<th>Median Nominal Wage Change</th>
<th>Percent of all Hourly Workers with $^b$</th>
<th>Percent Rigid (exclude min. wage)$^c$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Inflation Rate$^a$</td>
<td>Unemployment Rate</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>1979-80</td>
<td>12.7</td>
<td>7.1</td>
<td>9.6</td>
<td>11.6</td>
</tr>
<tr>
<td>1980-81</td>
<td>9.8</td>
<td>7.6</td>
<td>9.5</td>
<td>12.1</td>
</tr>
<tr>
<td>1981-82</td>
<td>6.0</td>
<td>9.7</td>
<td>7.2</td>
<td>16.4</td>
</tr>
<tr>
<td>1982-83</td>
<td>3.2</td>
<td>9.6</td>
<td>5.1</td>
<td>17.7</td>
</tr>
<tr>
<td>1983-84</td>
<td>4.2</td>
<td>7.5</td>
<td>5.0</td>
<td>17.8</td>
</tr>
<tr>
<td>1984-85</td>
<td>3.6</td>
<td>7.2</td>
<td>4.9</td>
<td>18.4</td>
</tr>
<tr>
<td>1985-86</td>
<td>1.8</td>
<td>7.0</td>
<td>4.5</td>
<td>19.1</td>
</tr>
<tr>
<td>1986-87</td>
<td>3.6</td>
<td>6.2</td>
<td>4.4</td>
<td>19.2</td>
</tr>
<tr>
<td>1987-88</td>
<td>4.1</td>
<td>5.5</td>
<td>4.9</td>
<td>18.0</td>
</tr>
<tr>
<td>1988-89</td>
<td>4.7</td>
<td>5.3</td>
<td>5.1</td>
<td>17.2</td>
</tr>
<tr>
<td>1989-90</td>
<td>5.3</td>
<td>5.5</td>
<td>5.4</td>
<td>17.3</td>
</tr>
<tr>
<td>1990-91</td>
<td>4.1</td>
<td>6.7</td>
<td>5.0</td>
<td>18.2</td>
</tr>
<tr>
<td>1991-92</td>
<td>3.0</td>
<td>7.4</td>
<td>4.1</td>
<td>19.0</td>
</tr>
<tr>
<td>1992-93</td>
<td>2.9</td>
<td>6.8</td>
<td>3.9</td>
<td>20.3</td>
</tr>
</tbody>
</table>

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

$^a$ Inflation rate is 100 times the change in the log of the CPI.

$^b$ Individuals who report being paid by the hour in both years, and who report the same 2 digit industry and occupation in both years, except for 1982-83 and 1983-84 -- see appendix Table A1, note (a).

$^c$ Sample excludes individuals whose real wage in either year is less than the minimum wage in the second year.
Table 2: Characteristics of Wage Change Distributions in PSID Samples

<table>
<thead>
<tr>
<th>Year</th>
<th>Inflation Rate¹</th>
<th>Percent Rigid:</th>
<th>Inflation Rate</th>
<th>Percent Rigid:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Rate</td>
<td>All</td>
<td>hourly</td>
<td>All</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>1976-77</td>
<td>6.3</td>
<td>7.4</td>
<td>9.3</td>
<td>1.8</td>
</tr>
<tr>
<td>1977-78</td>
<td>7.3</td>
<td>6.2</td>
<td>7.8</td>
<td>3.6</td>
</tr>
<tr>
<td>1978-79</td>
<td>10.3</td>
<td>6.8</td>
<td>7.8</td>
<td>4.1</td>
</tr>
</tbody>
</table>

1-Year Wage Changes

2-Year Wage Changes

<table>
<thead>
<tr>
<th>Year</th>
<th>Inflation Rate</th>
<th>Percent Rigid:</th>
<th>Inflation Rate</th>
<th>Percent Rigid:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>13.6</td>
<td>2.4</td>
<td>3.1</td>
<td>5.4</td>
</tr>
<tr>
<td>1977-79</td>
<td>18.1</td>
<td>1.9</td>
<td>2.1</td>
<td>7.6</td>
</tr>
</tbody>
</table>

3-Year Wage Changes

<table>
<thead>
<tr>
<th>Year</th>
<th>Inflation Rate</th>
<th>Percent Rigid:</th>
<th>Inflation Rate</th>
<th>Percent Rigid:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>24.4</td>
<td>0.9</td>
<td>1.2</td>
<td>9.5</td>
</tr>
</tbody>
</table>

Notes: The unemployment rates during the respective periods are as follows:

<table>
<thead>
<tr>
<th>Year</th>
<th>Unemployment</th>
</tr>
</thead>
<tbody>
<tr>
<td>1977</td>
<td>7.1</td>
</tr>
<tr>
<td>1978</td>
<td>6.1</td>
</tr>
<tr>
<td>1979</td>
<td>5.8</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Year</th>
<th>Unemployment</th>
</tr>
</thead>
<tbody>
<tr>
<td>1986</td>
<td>7.0</td>
</tr>
<tr>
<td>1987</td>
<td>6.2</td>
</tr>
<tr>
<td>1988</td>
<td>5.5</td>
</tr>
</tbody>
</table>

¹ Inflation rate is 100 times the change in the log of the CPI over the relevant time period.
² Individuals who report being paid by the hour in the beginning and ending years, and report no change in job ("position" or "employer").
<table>
<thead>
<tr>
<th>Year</th>
<th>Inflation Rate</th>
<th>Density swept-up&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Density swept-back&lt;sup&gt;b&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Counterfactual:</td>
<td>Counterfactual:</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Median</td>
<td>49th Percentile</td>
</tr>
<tr>
<td>1979-80</td>
<td>12.7</td>
<td>6.06</td>
<td>6.64</td>
</tr>
<tr>
<td>1980-81</td>
<td>9.8</td>
<td>7.30</td>
<td>6.54</td>
</tr>
<tr>
<td>1981-82</td>
<td>6.0</td>
<td>5.36</td>
<td>6.75</td>
</tr>
<tr>
<td>1982-83</td>
<td>3.2</td>
<td>10.29</td>
<td>10.94</td>
</tr>
<tr>
<td>1983-84</td>
<td>4.2</td>
<td>10.49</td>
<td>10.74</td>
</tr>
<tr>
<td>1984-85</td>
<td>3.6</td>
<td>9.66</td>
<td>10.63</td>
</tr>
<tr>
<td>1985-86</td>
<td>1.8</td>
<td>13.47</td>
<td>14.37</td>
</tr>
<tr>
<td>1986-87</td>
<td>3.6</td>
<td>13.29</td>
<td>13.91</td>
</tr>
<tr>
<td>1987-88</td>
<td>4.1</td>
<td>13.16</td>
<td>14.51</td>
</tr>
<tr>
<td>1988-89</td>
<td>4.7</td>
<td>13.27</td>
<td>13.60</td>
</tr>
<tr>
<td>1989-90</td>
<td>5.3</td>
<td>12.89</td>
<td>12.44</td>
</tr>
<tr>
<td>1990-91</td>
<td>4.1</td>
<td>11.03</td>
<td>11.69</td>
</tr>
<tr>
<td>1991-92</td>
<td>3.0</td>
<td>11.38</td>
<td>13.35</td>
</tr>
<tr>
<td>1992-93</td>
<td>2.9</td>
<td>10.34</td>
<td>12.59</td>
</tr>
</tbody>
</table>

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.

<sup>a</sup> Estimated percent of workers who would have experienced a nominal wage cut in the absence of rigidities.

<sup>b</sup> Estimated percent of workers who would have experienced a nominal wage increase in the absence of rigidities.
Table 4: Estimated Effect of Nominal Wage Rigidities on Average Real Wage Changes

<table>
<thead>
<tr>
<th>Year</th>
<th>Inflation Rate (1)</th>
<th>Wage swept-up(^a) Counterfactual: Median (2)</th>
<th>Wage swept-up(^a) Counterfactual: 49th Percentile (3)</th>
<th>Wage swept-back(^b) Counterfactual: Median (4)</th>
<th>Wage swept-back(^b) Counterfactual: 49th Percentile (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1979-80</td>
<td>12.7</td>
<td>0.32</td>
<td>0.41</td>
<td>0.00</td>
<td>0.04</td>
</tr>
<tr>
<td>1980-81</td>
<td>9.8</td>
<td>0.48</td>
<td>0.29</td>
<td>0.00</td>
<td>0.04</td>
</tr>
<tr>
<td>1981-82</td>
<td>6.0</td>
<td>0.31</td>
<td>0.34</td>
<td>0.18</td>
<td>0.22</td>
</tr>
<tr>
<td>1982-83</td>
<td>3.2</td>
<td>0.85</td>
<td>0.80</td>
<td>0.09</td>
<td>0.08</td>
</tr>
<tr>
<td>1983-84</td>
<td>4.2</td>
<td>0.81</td>
<td>0.75</td>
<td>0.07</td>
<td>0.11</td>
</tr>
<tr>
<td>1984-85</td>
<td>3.6</td>
<td>0.90</td>
<td>0.66</td>
<td>0.09</td>
<td>0.08</td>
</tr>
<tr>
<td>1985-86</td>
<td>1.8</td>
<td>1.06</td>
<td>1.07</td>
<td>0.03</td>
<td>0.04</td>
</tr>
<tr>
<td>1986-87</td>
<td>3.6</td>
<td>1.26</td>
<td>1.06</td>
<td>0.03</td>
<td>0.06</td>
</tr>
<tr>
<td>1987-88</td>
<td>4.1</td>
<td>1.22</td>
<td>1.09</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>1988-89</td>
<td>4.7</td>
<td>1.08</td>
<td>1.03</td>
<td>0.01</td>
<td>0.03</td>
</tr>
<tr>
<td>1989-90</td>
<td>5.3</td>
<td>1.09</td>
<td>0.80</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>1990-91</td>
<td>4.1</td>
<td>0.85</td>
<td>0.86</td>
<td>0.08</td>
<td>0.09</td>
</tr>
<tr>
<td>1991-92</td>
<td>3.0</td>
<td>0.60</td>
<td>0.73</td>
<td>0.09</td>
<td>0.10</td>
</tr>
<tr>
<td>1992-93</td>
<td>2.9</td>
<td>0.58</td>
<td>0.79</td>
<td>0.08</td>
<td>0.09</td>
</tr>
</tbody>
</table>

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 worker who otherwise would have experienced a nominal wage increase, expressed in percentages. A positive entry means that rigidities reduced wages for this group.

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Estimated Coefficients of Log State Unemployment Rate:</th>
<th>Residual Standard Error (5)</th>
<th>Other Controls Included:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Current (1) Lag 1 (2) Lag 2 (3) Lag 3 (4)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Adjusted Log Wage (weighted)</td>
<td>-0.025 (0.005) --- --- ---</td>
<td>0.042</td>
<td>year effects</td>
</tr>
<tr>
<td>2. Adjusted Log Wage (weighted)</td>
<td>-0.044 (0.011) 0.021 --- ---</td>
<td>0.042</td>
<td>year effects</td>
</tr>
<tr>
<td>3. Adjusted Log Wage (weighted)</td>
<td>-0.038 (0.011) -0.004 0.002 0.021</td>
<td>0.042</td>
<td>year effects</td>
</tr>
<tr>
<td>4. Adjusted Log Wage (weighted)</td>
<td>-0.034 (0.006) --- --- ---</td>
<td>0.042</td>
<td>year and region effects</td>
</tr>
<tr>
<td>5. Adjusted Log Wage (weighted)</td>
<td>-0.048 (0.011) 0.016 --- ---</td>
<td>0.042</td>
<td>year and region effects</td>
</tr>
<tr>
<td>6. Adjusted Log Wage (weighted)</td>
<td>-0.025 (0.005) --- --- ---</td>
<td>0.040</td>
<td>year, region, and year*region effects</td>
</tr>
<tr>
<td>7. Adjusted Log Wage (weighted)</td>
<td>-0.023 (0.014) -0.003 --- ---</td>
<td>0.040</td>
<td>year, region, and year*region effects</td>
</tr>
<tr>
<td>8. Unadjusted Log Wage (weighted)</td>
<td>-0.029 (0.012) -0.002 --- ---</td>
<td>0.048</td>
<td>year and region effects</td>
</tr>
<tr>
<td>9. Adjusted Log Wage (unweighted)</td>
<td>-0.049 (0.012) 0.018 --- ---</td>
<td>0.038</td>
<td>year and region effects</td>
</tr>
</tbody>
</table>

Note: All models are fit to sample of 765 observations (51 states x 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 rows expect row 8, the state average wage is adjusted for the characteristics of workers in the state (using a year-specific wage prediction model). In all rows except row 9, the estimates are obtained by weighted OLS, using as weights the relative number of workers in the state in 1976. Standard errors in parentheses.
Figure 1a: Histograms of the Distribution of Log Real Wage Changes, Matched CPS Samples from 1979-80 to 1992-93
Figure 1: Histograms of the Distribution of Log Real Wage Changes, PSID Samples 1976-79 and 1985-88, Hourly-rated workers, same employer.
Figure 2: Histograms of the Distribution of Log Real Wage Changes, Over 2-year and 3-year Horizons, PSID Samples, Hourly-rated workers, same employer
Figure 3a: Effect of Downward Nominal Rigidity on the Distribution of Real Wage Changes -- Theoretical Illustration
Figure 3b: Effect of Downward Nominal Rigidity and Menu Costs on the Distribution of Real Wage Changes -- Theoretical Illustration
Figure 4: Smoothed (Kernel) Estimates of Actual and Counterfactual Densities of Real Wage Changes, CPS Samples from 1979-80 to 1982-83
Figure 4 (Continued): Smoothed (Kernel) Estimates of Actual and Counterfactual Densities of Real Wage Changes, CPS Samples from 1983-84 to 1986-87
Figure 4 (Continued): Smoothed (Kernel) Estimates of Actual and Counterfactual Densities of Real Wage Changes, CPS Samples from 1989-90 to 1992-93.

Kernel density estimates
Figure 5: Cumulative Fraction of Counterfactual Density affected by Rigidities, CPS Samples from 1979-80 to 1991-92
Figure 6: Rate of Adjustment of Real Wages to Local Unemployment versus Aggregate Inflation Rate
Table A1: CPS Merged ORG Sample Selection

<table>
<thead>
<tr>
<th>Year</th>
<th>Hourly-rated workers: Sample size</th>
<th>Industry and Occupation Match-rate&lt;sup&gt;a&lt;/sup&gt;</th>
<th>... And Unaffected by minimum wage&lt;sup&gt;b&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>1979-80</td>
<td>20055</td>
<td>58.2</td>
<td>47.5</td>
</tr>
<tr>
<td>1980-81</td>
<td>22620</td>
<td>59.1</td>
<td>50.1</td>
</tr>
<tr>
<td>1981-82</td>
<td>22431</td>
<td>60.7</td>
<td>51.7</td>
</tr>
<tr>
<td>1982-83</td>
<td>22020</td>
<td>32.4</td>
<td>28.1</td>
</tr>
<tr>
<td>1983-84</td>
<td>21955</td>
<td>47.2</td>
<td>42.0</td>
</tr>
<tr>
<td>1984-85</td>
<td>10629</td>
<td>56.3</td>
<td>50.5</td>
</tr>
<tr>
<td>1985-86</td>
<td>5967</td>
<td>54.3</td>
<td>49.7</td>
</tr>
<tr>
<td>1986-87</td>
<td>23452</td>
<td>55.5</td>
<td>50.9</td>
</tr>
<tr>
<td>1987-88</td>
<td>22176</td>
<td>55.1</td>
<td>51.3</td>
</tr>
<tr>
<td>1988-89</td>
<td>22045</td>
<td>54.4</td>
<td>51.3</td>
</tr>
<tr>
<td>1989-90</td>
<td>23241</td>
<td>54.6</td>
<td>51.1</td>
</tr>
<tr>
<td>1990-91</td>
<td>23663</td>
<td>55.3</td>
<td>50.4</td>
</tr>
<tr>
<td>1991-92</td>
<td>23401</td>
<td>54.9</td>
<td>50.5</td>
</tr>
<tr>
<td>1992-93</td>
<td>23117</td>
<td>55.6</td>
<td>51.1</td>
</tr>
</tbody>
</table>

Notes:

<sup>a</sup> The industry and occupations are matched using detailed (2-digit) industry and occupation codes for all years except 1982-83 and 1983-84: for the 1983-84 sample, matching is based on 3-digit 1980 census codes; for the 1982-83 sample, industry is matched using the detailed (2-digit) codes which are comparable across years, while occupation was matched using an algorithm devised to convert 1970 census 3-digit occupation codes to their 1980 census counterparts. This algorithm is available from the authors on request.

<sup>b</sup> Observations are assumed to be affected by minimum wage effects if either \( w_i \leq mw_{i} \) or \( w_{i+1} \leq mw_{i+1} + \pi_i \).
<table>
<thead>
<tr>
<th>Year</th>
<th>All workers: Sample size</th>
<th>Hourly-rated workers, same employer: Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>1976-77</td>
<td>1965</td>
<td>41.2</td>
</tr>
<tr>
<td>1977-78</td>
<td>1992</td>
<td>45.0</td>
</tr>
<tr>
<td>1978-79</td>
<td>2214</td>
<td>41.3</td>
</tr>
<tr>
<td>1985-86</td>
<td>4507</td>
<td>45.9</td>
</tr>
<tr>
<td>1986-87</td>
<td>4447</td>
<td>45.0</td>
</tr>
<tr>
<td>1987-88</td>
<td>4443</td>
<td>45.1</td>
</tr>
</tbody>
</table>

Notes:  
* 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, tenures relates to time with the same employer.
Figure A1: Histograms of Real Wage Changes for All Workers in PSID Samples, 1976-79 and 1985-88
Figure A2: Histograms of Real Wage Changes for All Workers in PSID Samples, Over 2-year and 3-year Horizons, 1976-79 and 1985-88