Evidence of Nominal Wage Stickiness from Microdata

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For much of this century, sticky nominal wages have been considered a key reason that nominal shocks to the economy may have real effects. Historical explanations of sticky nominal wages often rely on money illusion, a concept unpopular with neoclassical economists because it implies irrationality. More modern explanations cite menu costs (for instance, George Akerlof and Janet Yellen, 1985) and imperfect information about the rate of inflation (for instance, Edmund S. Phelps, 1970).

Tests of sticky nominal wages have looked at their indirect effects, particularly regarding the countercyclicality of real wages (for instance, see Gary Solon et al., 1994). There has been little direct empirical analysis. This paper addresses that shortcoming by examining longitudinal microeconomic data on the distribution of annual nominal wage and salary changes of workers who remain on the same job.

This paper finds that there are some workers whose wages or salaries exhibit nominal stickiness. Specifically, it finds:

(1) A significant fraction of workers remaining on the same job over a year receive the same nominal wage/salary in consecutive years.

(2) When a given real wage/salary change requires a small nominal change, it is less likely to occur than when it requires a larger nominal change. Over the period studied, between 1 and 2 percent of workers would have received a small pay change in the absence of menu costs, but instead received none.

(3) There is also evidence of *downward* nominal wage stickiness, but with important differences between wage earners and salary earners. Wage earners receive nominal wage cuts less frequently than would be expected on the basis of distributions of real wage changes. In the period studied, approximately 9.4 percent of wage earners would have received a nominal wage reduction in the absence of downward wage rigidities, but instead do not.[1] In contrast, salary earners do not receive pay cuts less frequently than would be expected, particularly in later years.

The frequency of zero nominal pay changes combined with the relative infrequency of small pay changes provide micro-level evidence of the presence of "menu costs," which can lead firms to postpone small pay rate changes. Menu costs in pay rate adjustments may include the administrative costs of changing payrolls and the costs of performance appraisal and negotiations that generally accompany wage/salary changes. While there is considerable debate over whether menu costs can have a profound impact on aggregate fluctuations and create nonneutrality of money, this paper does not address the macroeconomic implications of menu costs in wage/salary adjustments.[2] Instead, it asks whether the distribution of annual wage and salary adjustments shows microeconomic evidence of menu costs, a necessary but not sufficient condition for macroeconomic effects.

Menu costs are not enough to explain the sharp drop in wage distributions below nominal zero. The phenomenon strongly suggests that either workers or firms resist nominal pay cuts, as would be predicted by traditional Keynesians. Because of this resistance,

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[1] These results are broadly compatible with Harry Holzer and Edward Montgomery (1993), who find that wages in a sample of firms in the early 1980's responded less to negative than to positive demand shocks. That paper did not distinguish between nominally negative and nominally positive wage changes.

[2] For instance, see Akerlof and Yellen (1985), N. Gregory Mankiw (1985), and Laurence Ball and David Romer (1990), who claim menu costs have important macroeconomic effects. Andrew Caplin and Daniel Spulber (1987) find no macroeconomic impact when the timing of price adjustments is endogenously affected by inflation.

implicit labor contracts would tend to insure against nominal wage decreases. The paper shows that most of the bunching at zero is accounted for by a combination of menu costs and downward nominal rigidity, rather than any other factors.

Money illusion had dropped from discussion among most macroeconomists[3] but recently has received attention in opinion surveys (Daniel Kahneman et al., 1986; Alan Blinder and Don Choi, 1990; Eldar Shafir et al., 1992; Truman Bewley, 1997) that indicate that workers and employers seem to care about nominal pay rate changes.[4] Such research develops psychological reasons why nominal pay rates may have special value as a sort of anchor or focal point. People tend to perceive their nominal pay rate changes as an indication of accomplishment and advancement or of personal success. The psychological importance of increasing nominal pay is also evident in a recent survey (George Loewenstein and Nachum Sicherman, 1991) that finds people prefer upward-sloping earnings profiles to flat earnings profiles even when informed that the present value of the upward-sloping profile is less than that of the flat profile.

I. A Brief Description of the Data

This work is based on the Panel Study of Income Dynamics (PSID) covering the period 1970 through 1988. The PSID interviewed the same individuals every year to form a longitudinal data base. The decades of the 1970's and 1980's are good ones for the study of nominal pay rate stickiness, as they span periods of very different inflation experiences.[5]

For each household head observed in the same job for two contiguous years of the survey, I calculate the annual percentage wage or salary changes. My sample excludes nonheads of households, people who change employers during a year, and people with missing wage or salary data for those years. Also excluded are people who work only for themselves.[6] Note that a single individual could account for as many as 18 observations (the first covering the annual changes from 1970 to 1971, the last covering changes from 1987 to 1988), although for most there are far fewer.

The analysis uses data on hourly pay rates of workers. The PSID began collecting pay rate data for wage workers beginning in 1970 and began collecting pay data for salary workers beginning in 1976. The PSID converts salaries into an hourly pay rate and codes only this number.

II. The Evidence

A. *Evidence from Descriptive Data*

Figure 1, which is a simple histogram of all annual percentage changes in nominal hourly

[3] Money illusion was subjected to empirical testing primarily in the decade following Milton Friedman (1968). Generally, these studies are based on time-series macroeconomic data and investigate whether nominal values affect labor supply decisions and, to a lesser extent, consumption decisions. Some of these studies find indirect evidence of money illusion, others do not (Ray Fair, 1971; Michael Wachter, 1972; Michael Abbott and Orley Ashenfelter, 1976; Beth Niemi and Cynthia Lloyd, 1981). For a somewhat more recent approach using microeconomic data, see Elizabeth Gustafson and Lawrence Hadley, 1989.

[4] For instance, 62 percent of respondents to an opinion survey by Kahneman et al. (1986) felt it is unfair (38 percent felt it is acceptable) to decrease (nominal) wages by 7 percent in a community experiencing substantial unemployment but no inflation. On the other hand, only 22 percent felt it unfair (and 78 percent acceptable) for a company to increase salaries only 5 percent in a community experiencing the same unemployment but with inflation of 12 percent (thus resulting in the same 7-percent decrease in real wages).

[5] Wage and price controls in the United States were in force between August 1971 through April 1974, but they had little to no impact on wages (Robert Gordon, 1973). To the extent that wage controls imposed constraints, they would be likely to bias *against* the key results here of downward nominal wage rigidity and few small wage increases. (Note that wages were actually frozen for only two very short periods.)

[6] The study is limited to heads of households because the PSID directly interviews them and directs its full set of labor market questions only to them. Household heads may be different from nonheads. For instance, there may be more resistance to wage cuts of the family's main earner. This caveat should be kept in mind when considering the results here. The alternative large longitudinal data survey, the NLS, was not administered annually during this period and is therefore less useful for a study of relatively short-term sticky nominal wages and salaries.

The self-employed are excluded because their pay is not set, but is determined instead by demand conditions for their products or services. Analysis also excludes observations from the special SEO sample of oversampled low-income people.

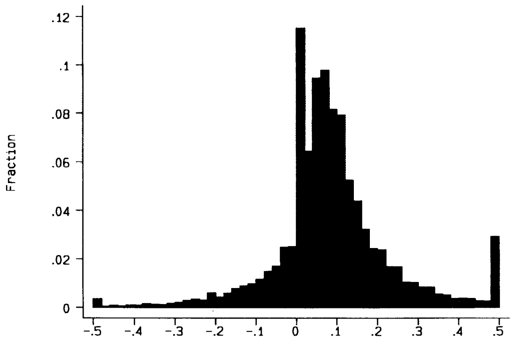

FIGURE 1. DISTRIBUTION OF ANNUAL PROPORTIONATE CHANGE IN PAY, 1970–1988

pay (for job stayers) for the period spanning 1970 to 1988, illustrates the important role of (nominal) zero.[7] First, there is a large spike at the zero point, averaging 7.66 percent over the sample. Second, the entire distribution seems to exhibit a discontinuous fall below the zero nominal pay change point. Third, the bar directly above zero seems lower than one might expect, given the pattern at higher values.

As Table 1 shows, there are important differences among types of workers. Divided into occupational groups, the drop below nominal zero is smallest for the professional and managerial group and greatest for unskilled occupations. Similarly, unskilled workers have the largest spike at zero while professionals and managers have the smallest.

While employers are legally prohibited from giving nominal wage cuts to (covered) employees receiving the minimum wage, this minimum-wage effect does not explain the low frequency of nominal wage decreases for less skilled blue collar and service workers. Excluding minimum wage workers hardly changes the percentage of laborers, operatives, and service occupations receiving nominal pay cuts—not surprising in light of the fact that less than 1 percent of this sample (of people on the job for at least a year) received minimum wages.

However, the key distinction seems to be between wage and salary earners. The pay changes of wage earners exhibit extreme downward nominal stickiness and a higher spike at zero than salary earners. This can be seen in the histograms of annual percentage hourly pay changes of wage earners and salary earners, shown for a sample of years (Figures 2 and 3), and in Tables 1 and 3. Between 1977 and 1988,[8] on average only 10.56 percent of *wage* earners received a nominal pay cut from their current employer, while 24.34 percent of *salary* earners received a nominal cut. In this same period, for 10.51 percent of wage earners the nominal wage was unchanged each year, while less than half that percentage of salary earners (4.68 percent) saw no pay change.

Pay change distributions look similar for salaried people, regardless of occupation. If we further categorize the occupations in Table 1 into salary versus wage earners, we find that 24.3 percent of professional/managerial salary earners have negative nominal pay changes, compared to 24.4 percent of salary earners in other occupations. Likewise, distributions of pay changes look similar for wage earners in all four major occupational groups, with the percentage with negative wage changes ranging only between 9.3 percent and 11.1 percent. In contrast, distributions of pay changes for wage and salary earners within a single occupation tend to be quite different. For instance, in skilled blue collar occupations, 10.4 percent of wage earners have negative pay changes compared to 26.9 percent of salary earners. Because of these large wage/salary differences, more detailed discussion of results focuses on the data disaggregated in this way.

The likelihood of downward nominal pay changes is only slightly different in union and nonunion environments (see Table 1). Among salaried workers, unionized workers are less likely to receive pay cuts (presumably because of union bargaining strength), while among wage earners, unionized workers are slightly more likely to receive pay cuts. This remains true even holding major occupational group constant (not shown). The union/nonunion difference is much more marked in the size of the spike at nominal zero for wage earners. There are far more nonunionized wage earners

[7] In Figure 1 and the other histograms, tails of the distribution are massed at the extremes, in order to allow a better view of the intermediate categories.

[8] These are the only years for which both salary and wage data are available in the PSID.

TABLE 1—CHANGES IN NOMINAL PAY BY CATEGORY, 1976–1988

Category	Percent with zero nominal pay change	Percent with negative nominal pay change
All workers	7.47	17.75
Only minimum wage workers	26.85	6.48
By occupation:		
Professional and managerial occupations	5.42	22.43
Other white collar occupations	6.86	15.92
Skilled blue collar occupations	7.97	15.75
Laborers, operatives, and service occupations	10.21	13.71
Excluding minimum wage workers	9.44	13.07
By payment type and union status:		
Wage earners	10.51	10.56
Nonunionized	14.22	9.45
Unionized	6.47	11.77
Salary earners	4.68	24.34
Nonunionized	4.75	25.00
Unionized	4.22	20.02

receiving exactly zero pay change than unionized ones, even controlling for major occupational group. Both of these differences between union and nonunion wage earners are likely due to different union/nonunion industrial composition.

B. *Estimation Methodology*

Most readers' evaluation of the persuasiveness of this paper's main thesis, that nominal zero affects wage distributions, will be based on the visual impact of the histograms presented in this paper. Nevertheless, it is necessary to test the statistical significance of these findings. To do this, I calculate the proportion of pay changes that fall into each ''bar'' of a histogram centered around the annual median pay change.[9] In addition to the base distribution expected for that ''bar'' given its relation to the mean pay change, the estimation allows the bar that contains nominal zero, the bars that are nominally negative, and the bars that lie close to nominal zero to be either higher or lower than ceteris paribus expected based only on their relation to the median pay change. For instance, a linear specification of this estimation is:

$$(1)\quad Prop_{rt} = \alpha_r + \beta_1 DNEG_{rt} + \beta_2 D1_{rt} + \beta_3 D2_{rt} + \beta_4 DN1_{rt} + \gamma D0_{1t} + \mu_{1t} \quad \forall\, r = 1, \ldots 12.$$

Here, $Prop_{rt}$ is the proportion of workers whose annual pay change in year t falls in the range between ''the median pay change minus r percentage points'' and ''the median pay change minus $r + 1$ percentage points''; $D0_{rt}$ is a dummy variable that takes on the value of one if *nominal* zero falls within the rth percentage range during year t; $DNEG_{rt}$ is a dummy variable that takes on the value of one whenever the *nominal* pay change (in the rth percentage

[9] For this test, it does not matter whether I use the percentage change in real or nominal wages, since each year they differ only by a constant factor.

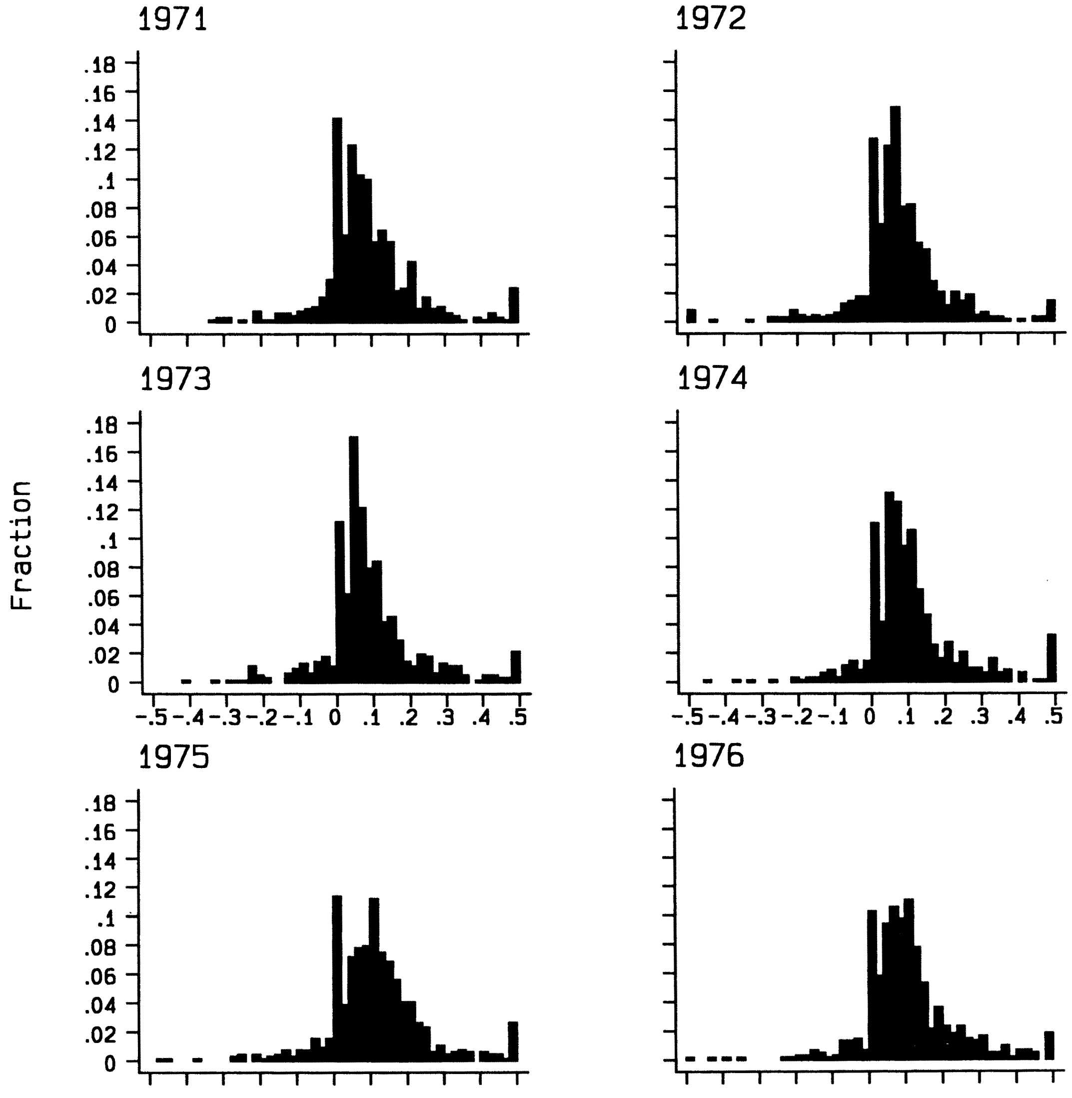

FIGURE 2. DISTRIBUTION OF ANNUAL PROPORTIONATE CHANGE IN WAGE

range in year t) is less than zero; $D1_{rt}$ and $D2_{rt}$ are dummy variables that take on the value of one in each of the two percentage ranges directly above nominal zero respectively; $DN1_{rt}$ is a dummy variable that takes on the value of one in the percentage range directly below nominal zero;[10] and α's, β's, and γ are estimated parameters. Only 12 histogram "bars" are estimated, since pay changes farther than 12 percent below the median always fall in the nominally negative range.

Equation (1) assumes that, in the absence of nominal rigidities, the proportion of workers receiving pay increases r percentage points below the median would be the same in all years except for a random factor. However, to allow nominal rigidities, if a given real pay change requires a negative nominal pay

[10] I also estimated additional specifications with a variable $DN2$ for the range below $DN1$. Results with this variable are never significant.

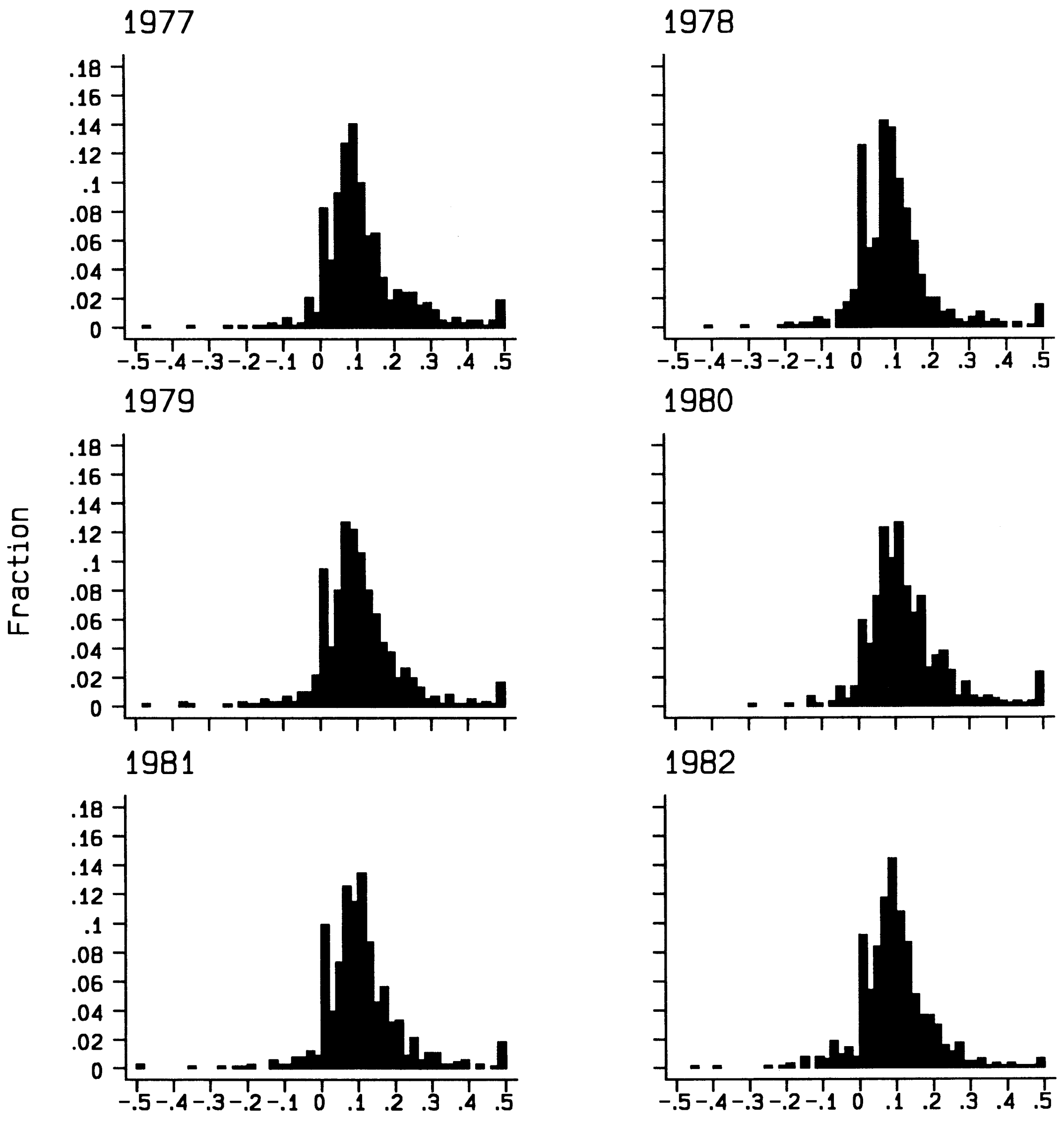

FIGURE 2. DISTRIBUTION OF ANNUAL PROPORTIONATE CHANGE IN WAGE—*Continued*.

decrease, the proportion of workers receiving that pay change will be altered by β_1. If a given real pay change implies only a small nominal change, menu costs may decrease the likelihood of a change, as captured in the β_2, β_3, and β_4 coefficients. Finally, the specification allows a spike γ at zero nominal pay change.

Note that this equation does not impose any functional form for the distribution of nominal pay rate changes.[11] Instead, the model estimates a predicted frequency of being, for instance, between 7 percent and 8 percent below

[11] I checked the results using a more parametric method that fit two truncated normal density functions to the (nominal) negative and positive wage change observations respectively, allowing for a spike at zero. The implications of both tests were similar.

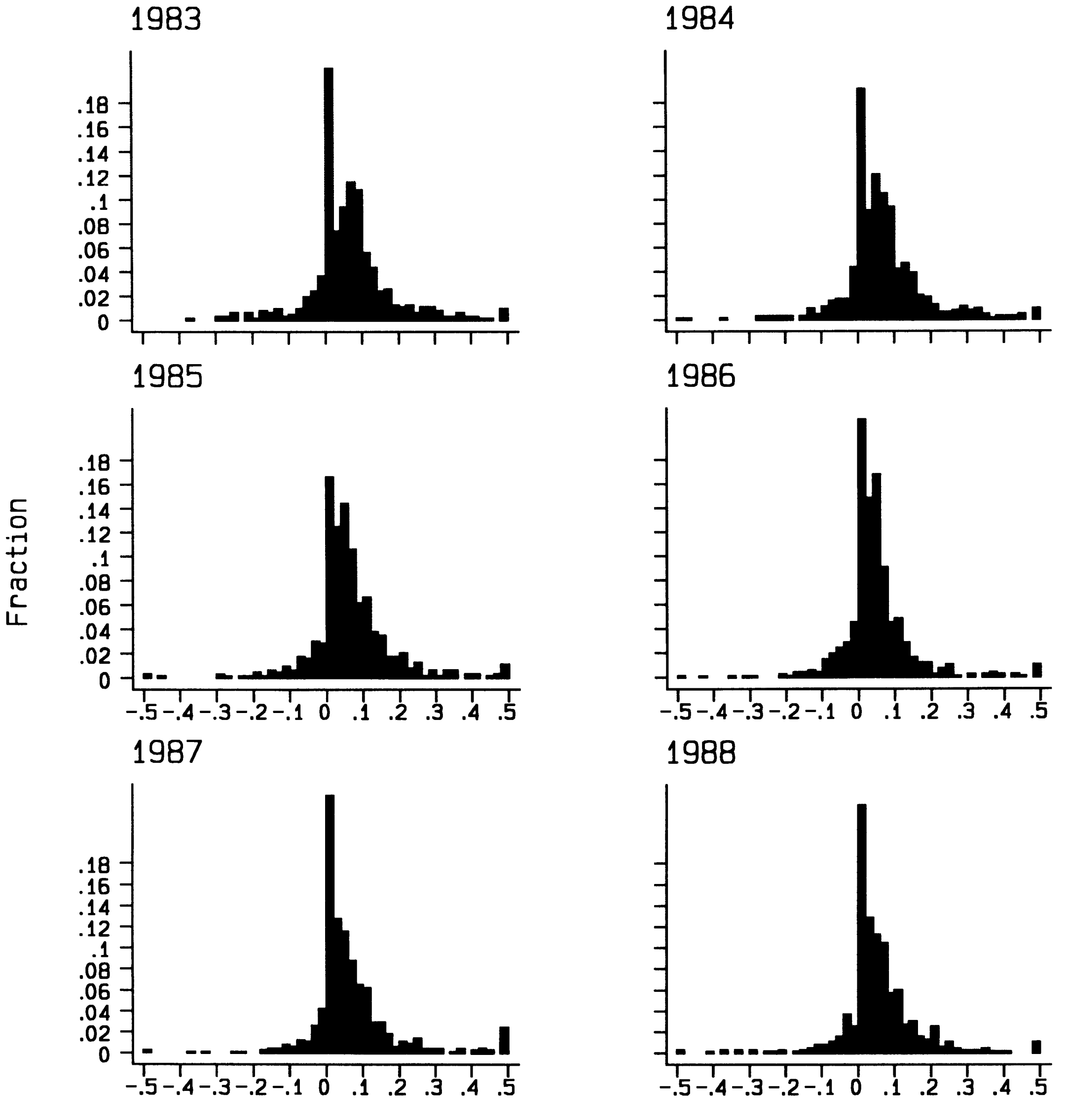

FIGURE 2—*Continued.*

the median pay, independently from being between 8 percent and 9 percent below the median pay. The approach cannot use information about the scarcity of pay changes more than 12 percent below the median pay—although their rarity seems striking in the histograms—because such changes always involve negative nominal pay changes. Thus, the power of the test derives completely from ranges that in some years represent positive nominal pay changes and in others represent negative nominal pay changes.

There are several limitations of the specification in (1). First, this specification does not impose that the predicted proportion of observations in all categories sums to 100 percent. The system fails to recognize that observations subtracted from one category must be added elsewhere. Thus, the model in (1) cannot be formally correct. To solve this problem, we instead estimate:

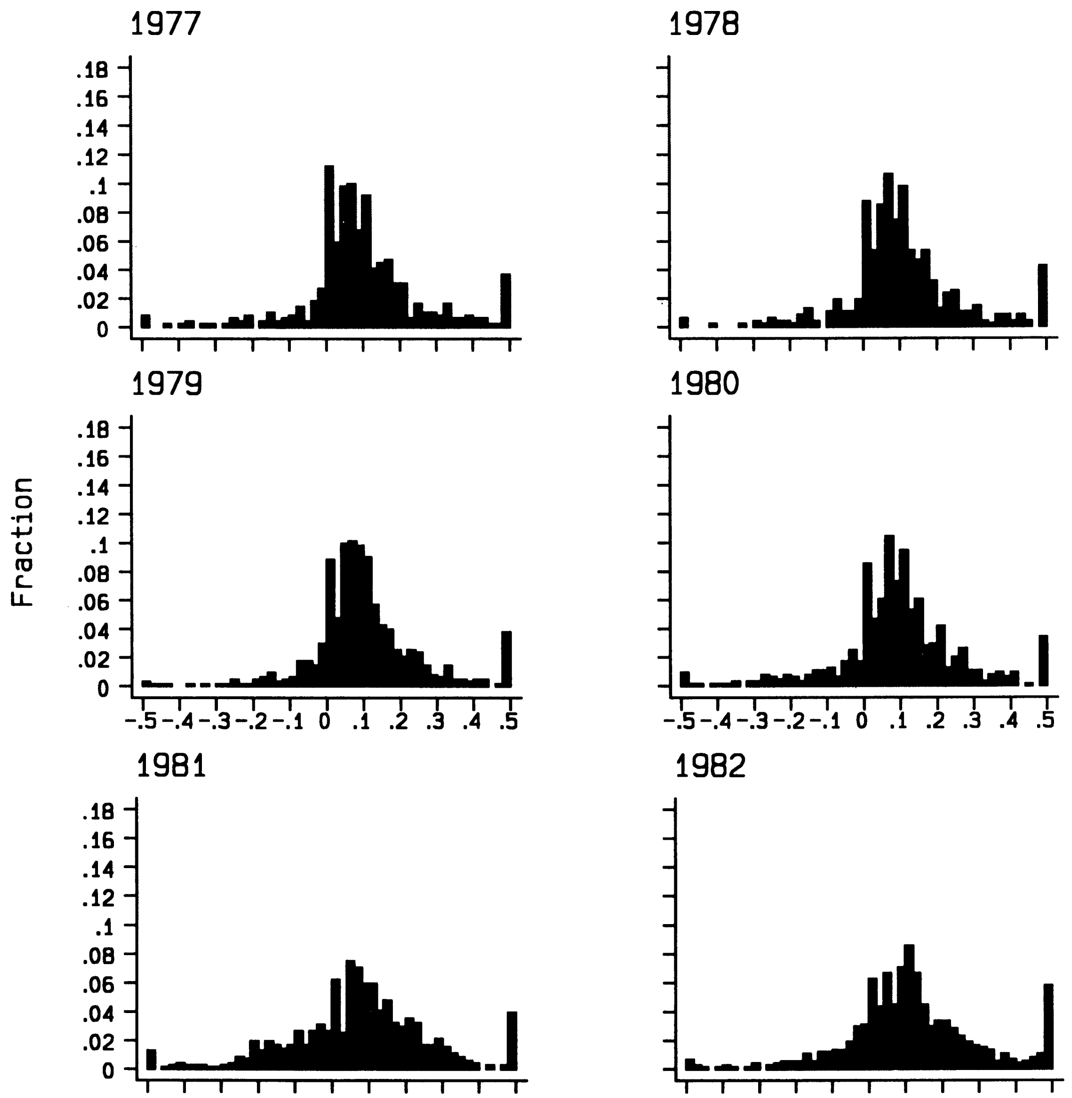

FIGURE 3. DISTRIBUTION OF ANNUAL PROPORTIONATE CHANGE IN SALARY

(2) (linear model)

$$Prop_{rt} = \alpha_r + \beta_1 DNEG_{rt} + \beta_2 D1_{rt} + \beta_3 D2_{rt} + \beta_4 DN1_{rt} + (\gamma - [\beta_1(12 - r) + \beta_2 + \beta_3 + \beta_4])\, D0_{rt} + \mu_{rt}$$

$\forall\, r = 1, \dots 12.$

This specification assumes that people who would otherwise have received a nominally small or negative pay change but do not, instead receive a zero wage change. The term in square brackets captures the "pile-up" that this creates at nominal zero. The term $\beta_1(12 - r)$ includes only unenacted negative pay changes within 12 percentage points below the median. Since pay changes farther than 12 percent below the median always fall in the nominally negative range, the mass at zero due to

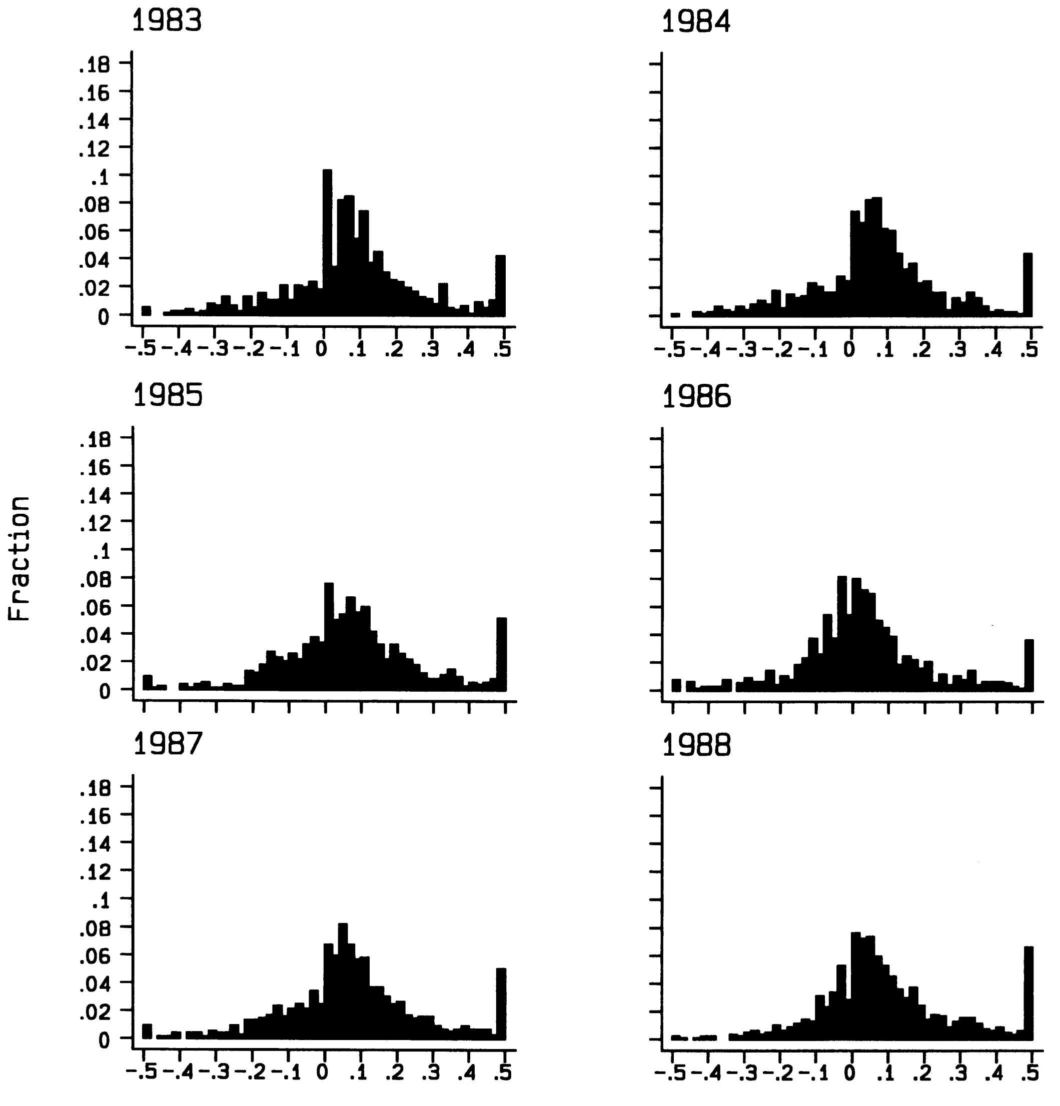

FIGURE 3—*Continued*.

these workers is constant and forms part of the γ parameter. γ also includes employees who may not receive a pay adjustment for other reasons, such as those who have not had a salary review between surveys.[12] Estimation of both (1) and (2) indicates that, in fact, the latter specification that satisfies adding-up also fits the data much better.[13]

A second limitation that applies to both (1) and (2) is that it is unlikely that a linear model

[12] Because of administrative costs of changing pay rates, adjustments occur not continuously but rather only at intervals, often yearly. However, some wage/salary reviews might not be on an annual cycle, or may be delayed in some years. Moreover, although each two consecutive PSID surveys are approximately a year apart, any individual may easily be surveyed at an 11-month interval.

[13] My evaluation of which model fits best, both here and elsewhere in this paper, is based on which model has a smaller value for the distance function being minimized. Since both models have the same number of parameters, any standard sample information criterion would select this model.

will fit well, since it unrealistically assumes that being nominally negative decreases the number of workers observed within a category by the same absolute amount, whether that category would otherwise be expected to contain 10 percent of workers or 0.5 percent of workers. One unappealing consequence of this assumption is that the results may easily predict negative probabilities.

Consequently, we also estimated an alternative specification:

(3) (proportional model)

$$Prop_{rt} = \alpha_r(1 + \beta_1 DNEG_{rt} + \beta_2 D1_{rt} + \beta_3 D2_{rt} + \beta_4 DN1_{rt}) + \left(\gamma - \left[\beta_1 \sum_{j>r} \alpha_j + \beta_2 \alpha_{r-1} + \beta_3 \alpha_{r-2} + \beta_4 \alpha_{r+1}\right]\right) D0_{rt} + \mu_{rt} \qquad \forall\, r = 1, \ldots 12.$$

Here, the β coefficients cut out a *proportion* of the density α_i that we would otherwise observe.[14]

Table 2 reports the results of estimation of equations (2) and (3). There are 18 observations for each equation corresponding to the years 1971–1988.[15] There are cross-equation constraints imposed by estimating a single set of β's and γ, and the error terms μ_{rt} are also allowed to covary across equations.[16]

The results are very robust to changes in functional form. Both specifications (2) and (3) yield the same qualitative conclusions. I have also estimated a model without the pile-up term in square brackets, as well as several additional functional forms. All yield similar results.[17] Since equation (3) has more theoretical appeal and also fits the data better than equation (2), discussion below concentrates on these results.

C. *Estimation Results*

The results of this estimation applied to the entire sample are shown in the first two columns of Table 2. The large positive coefficient on $D0$ reflects the sharp spike at nominal zero observed in the histograms. The coefficients on $D1$, $D2$, and $DN1$ indicate that, ceteris paribus, we are less likely to observe pay changes when they entail small nominal changes, a result consistent with the existence of menu costs. The coefficient on *DNEG* is indistinguishable from zero in the proportional model but significantly negative in the linear model. Considering only this estimation, we would remain agnostic about the existence of downward nominal stickiness.

The remaining columns of Table 2 repeat the estimation separately for wage and salary earners. For wage earners, there is no ambiguity about downward nominal stickiness. The point estimate of the (negative) coefficient on *DNEG*, the dummy for negative wage changes, is large and statistically significant in all specifications. Using equation (3) parameters, negative nominal observations occur 47 percent less often than would be predicted by their distance from the median. Based on this estimation (and average wage distributions), 9.4 percent of wage earners did not receive nominal wage cuts that they would have otherwise received, because of downwardly sticky wages.

The results for salary earners are sharply different. In both functional specifications, the sign on *DNEG* is positive implying that pay changes are *more* likely if they require a pay cut.

In part because of these unexpected results for salary earners, it is important to test a key assumption of the model, that the spike at zero equals a constant term (γ) plus the

[14] Note that the γ coefficient enters linearly. If it did not, the equation would not satisfy the adding-up constraint. Moreover, theoretically it is reasonable to expect that the fraction of workers who are not reviewed between two surveys will be independent of α_i. Empirical estimation of a specification where γ's impact is proportional to α_i yields results similar to equation (3), but fits the data less well.

[15] "1971" wage change represents the change of wage between spring 1970 and spring 1971.

[16] This differs from standard "seemingly unrelated regression" only in that it imposes cross-equation constraints. It can be interpreted as a GLS regression with 216 observations (18 times 12, where 12 is the number of categories) with heteroskedasticity dependent on the category and with correlation across categories for observations in the same year.

[17] These results are available on request from the author.

TABLE 2—DEPENDENT VARIABLE: FREQUENCY OF PERCENTAGE PAY CHANGES WITHIN EACH PERCENTAGE BELOW MEDIAN

	All workers		Wage earners		Salary earners		
	Proportional model	Linear model	Proportional model	Linear model	Proportional model	Linear model	Proportional model with time
*D*0	5.145*	3.438*	4.434*	1.903*	4.082*	3.610*	3.820*
	(0.219)	(0.323)	(0.375)	(0.382)	(0.661)	(0.964)	(0.763)
DNEG	0.042	−0.439*	−0.473*	−1.496*	0.325*	0.458*	−0.381*
	(0.034)	(0.051)	(0.033)	(0.0638)	(0.074)	(0.122)	(0.160)
*D*1	−0.279*	−0.836*	−0.386*	−1.331*	−0.259*	−0.693*	−0.334*
	(0.030)	(0.093)	(0.042)	(0.138)	(0.123)	(0.307)	(0.112)
*D*2	−0.208*	−0.686*	−0.150*	−0.641*	−0.193	−0.557	−0.238
	(0.030)	(0.102)	(0.036)	(0.123)	(0.136)	(0.360)	(0.124)
*DN*1	−0.339*	−0.333*	0.036	0.140	−0.547*	−0.951*	−0.285*
	(0.057)	(0.090)	(0.048)	(0.080)	(0.158)	(0.296)	(0.137)
year**D*0	—	—	—	—	—	—	−0.134*
							(0.113)
year**DNEG*	—	—	—	—	—	—	0.059*
							(0.017)

Notes: Dependent variable measured as percentages. Standard errors in parentheses.
DNEG is a dummy variable for negative nominal pay changes.
*D*0 is a dummy variable for exactly zero nominal pay change.
*D*1 is a dummy variable for the percentage directly above zero nominal pay change.
*D*2 is a dummy variable for the second percentage above zero nominal pay change.
*DN*1 is a dummy variable for the percentage directly below zero nominal pay change.
All specifications also include a set of dummies for the percentage below the median pay change.
See text for more detailed explanation of methodology. Proportional model is equation (3) in text; linear model is equation (2).
* Indicates significance at the 5-percent level.

piling-up of unenacted negative and small positive nominal wage changes. To test this model, I allow the spike at zero to be equal to a constant term plus a *multiple* (ϕ) of the unenacted wage changes. In the proportional model, this becomes:

$$(4)\quad Prop_r = \alpha_r(1 + \beta_1 DNEG_r + \beta_2 D1_r + \beta_3 D2_r + \beta_4 DN1_r) + \left(\gamma - \phi\left[\beta_1 \sum_{j>r} \alpha_j + \beta_2\alpha_{r-1} + \beta_3\alpha_{r-2} + \beta_4\alpha_{r+1}\right]\right) D0_r + \mu_r \qquad \forall\, r = 1, \ldots 12.$$

If the pile-up model is correctly specified, ϕ should equal 1. This test was also performed on the linear functional specification (2). Implications were similar except where noted.

For wage earners, the point estimate of the multiple (ϕ) is *greater than* one, suggesting that the cut-out observations plus more are piled at nominal zero. However, Wald, likelihood ratio tests, and score tests give mixed results of the test that this multiple significantly differs from one.[18] We can conclude

[18] All three tests should be regarded with caution, because the sample is small and thus actual and asymptotic distributions may differ. A multiple ϕ significantly greater than one could reflect the fact that when inflation is low, there will tend to be simultaneously: (1) less frequent

TABLE 3—EVIDENCE ON THE DISTRIBUTION OF CHANGES IN NOMINAL PAY

	Wage earners		Salary earners		
End year	Percent at zero	Percent less than zero	Percent at zero	Percent less than zero	Inflation (percent change March to March)
1971	10.42	11.20	—	—	4.3
1972	10.33	11.58	—	—	3.4
1973	7.44	10.68	—	—	5.1
1974	7.19	8.31	—	—	10.1
1975	8.07	9.32	—	—	10.2
1976	7.24	8.06	—	—	6.1
1977	6.34	5.82	7.96	13.67	6.8
1978	8.12	8.29	5.53	12.98	6.6
1979	6.34	7.64	6.49	13.61	10.4
1980	3.77	4.59	5.75	16.95	14.7
1981	7.33	6.11	3.53	28.39	10.0
1982	6.48	8.21	3.93	19.27	6.6
1983	14.68	14.52	6.75	22.78	3.9
1984	13.28	14.22	4.27	25.48	4.5
1985	10.60	13.77	4.73	30.54	3.7
1986	15.58	16.49	2.06	42.42	1.6
1987	15.50	12.61	3.19	27.62	3.8
1988	17.04	13.54	3.89	27.20	3.8

only that the pile-up assumption is not clearly rejected by the data.

When we apply the same specification test to salary earners, the results are dramatically different. The estimated value of the multiple (ϕ) has a counterintuitive negative sign. It is highly significantly negative in the proportional specification but insignificant in the linear one.

The explanation for the counterintuitive salary earners' results lies in time trends in the distribution of pay changes. Table 3 gives descriptive data on these time trends, some of which are attributable to inflation and productivity shifts that tend to shift the entire distribution of nominal pay changes. To model time trends, we first regress annual data for the percent receiving negative pay changes on independent variables including median nominal pay rate changes, squared median pay rate changes, inflation, unemployment rates, and either a time trend or a dummy variable for the years 1983+.[19] The percent receiving exactly

wage evaluations in general and thus a greater spike, and (2) more nominally negative and small observations and thus more pile-up.

[19] The pile-up model suggests that an increase in the median percent pay change would decrease the mass at zero. This is exactly what is found in this simple linear regression. Extending the reasoning of the pile-up model given a bell-shaped distribution of percentage wage changes, it can be shown that the median wage change should have a nonlinear, concave effect on the percentage less than zero. To investigate nonlinearity, some specifications include the median percentage wage change squared; this variable does not contribute significantly to explanatory power.

a zero nominal pay change is also regressed on the same variables.

For wage earners, no significant time trend is identified in either of the dependent variables, controlling for the other factors. For salary earners, however, time both lowers the spike at zero and increases the "percent < zero."

The same time trends are evident if we reestimate (3) adding two interaction terms: time**D*0 and time**DNEG*.[20] For wage earners, point estimates of the time terms are small and statistically insignificant and do not add to the statistical power of equation (3). For salary earners, on the other hand, the specification using time not only adds significant explanatory power, but the estimation results are no longer counterintuitive. Table 2 includes these results for salary earners.

With time trend interactions, the coefficient on *DNEG* (for salary earners) is significantly negative but the interaction of *DNEG* with time is significantly positive, with the two effects exactly balancing around 1982. Thus the results suggest that before 1982 but not after, real salary changes were less likely if they required negative nominal pay changes.

This estimation also suggests two reasons that salary earners' spike at zero falls over time. First, because of less downward nominal rigidity (as measured by the positive interaction between year and *DNEG*), there will be less piling-up of what would otherwise be negative pay changes. Second, the negative interaction term year**D*0 indicates that the non-pile-up part of the spike may decrease over time, with point estimates suggesting a decrease of 1.5 percentage points between 1977 and 1988.

Time trends explain not only salary earners' counterintuitive positive parameter on *DNEG* in equations (2) and (3), but also the "wrong" sign on the multiple ϕ in equation (4), which captures the pile-up of cut-out observations onto the spike. Since later years had both lower median nominal salary changes and a lower spike at zero, we observe a negative ϕ when equation (4) is estimated for salary earners. However, when time interactions are added to equation (4), the multiple (ϕ) becomes positive and statistically indistinguishable from one. Thus, allowing for the time trend eliminates all anomalous salary results.

The analyses of this section can be summarized succinctly. Downward nominal pay stickiness for wage earners continued through both the 1970's and the 1980's. For salary earners, there may have been downward nominal pay stickiness in the 1970's but by the mid-1980's, there was no evidence of downward pay stickiness.

D. *Menu Costs*

Table 2 also sheds light on whether menu costs lead to infrequent small pay changes. The results show that it is less likely for both wage earners and salary earners to receive a pay increase of <1 percent than would be predicted solely on the basis of distance from the median pay change. Even nominal pay increases up to 2 percent are less likely than would be expected, particularly for wage earners. For salary earners, it is also less likely ceteris paribus to receive a pay *decrease* of < $|1|$ percent. (For wage earners, there is no significant drop-off for small negative ranges beyond the general drop-off below zero.) Using equation (3) estimates (with time variables for salary earners), I estimate that on average during the period 1977–1988, 1.66 percent of wage earners and 1.94 percent of salary earners who would have received small nominal wage changes did not because of menu costs.

The rarity of observed small pay changes may be because of measurement error rather than menu costs. That is, some people tend to report their pay in round numbers. In this case,

[20] In this specification, adding-up is maintained by assuming that if time lowers the γ term of the spike, it also increases the density at all other points of the distribution [including those outside the range studied] such that the two effects just balance out. Similarly, if time decreases the impact of *DNEG*, fewer unenacted observations (that would have otherwise been negative) get piled up on the spike at zero. The exact program is available from the author on request.

An alternative way to measure the impact of time on the distribution is reestimate equation (3) over the two time subperiods, 1971–1980 and 1981–1988 for wage earners. The coefficients change very little across the two periods and the hypothesis of no structural shift could not be rejected for either the model as a whole or for the individual coefficients on *DNEG* and *D*0.

a small change in the true pay rate would appear as either a zero pay change or a large pay change, depending on whether the pay rate is altered enough to change the rounding. (On the other hand, classical measurement error would lead to observing no spike at zero, insofar as people whose true pay rate did not change would randomly report a somewhat different pay rate each year.)

In the period 1971 through 1988, 28 percent of wage earners received a round-number hourly wage, where a round number is defined as an even dollar or half-dollar amount. (The discussion here is limited to wage earners, because in the PSID wage earners directly quote their hourly pay rate, while salary earners may quote an annual, monthly, or weekly rate.) People reporting round-number wages account for about 50 percent of the total spike at zero. While this is a very large number, we cannot conclude that half of the spike is reporting error. Instead, it is quite possible that the likelihood of receiving a zero nominal wage change *is* truly higher for people with round-number wage rates. Employers may set wages in round numbers and not change them until the optimal wage change is large enough to catapult the worker to a different round number.

Even assuming that all people who begin a period with a round-number wage and do not experience any wage change represent reporting error, there is still evidence of menu costs, although the impact is smaller. Taking these people out of the estimation and reestimating equation (3) causes the parameter of the unexplained portion of the spike to fall 55 percent. The magnitude of the menu cost coefficient on $D1$ drops 35 percent but remains significantly negative. On the other hand, the coefficient on $D2$ is more than halved and becomes indistinguishable from zero. Using these estimates, 1.04 of wage earners would have received a small wage change but did not.

Alternatively, we can reestimate the same model excluding *all* people with round-number wages (at either the beginning or the end of the period). Again, menu costs are smaller but still present. The parameter on $D0$, the spike, falls much less (by only 22 percent), the parameter on $D1$ (the percentage range above 0) drops about the same percent as above (37 percent), while the parameter on $D2$ drops by only 15 percent and remains significant. An anomalous result is that the coefficient on $DN1$ (the percentage range below 1) becomes significantly positive.

III. Conclusion

The evidence presented here indicates substantial stickiness of nominal *wages* for wage earners remaining with the same employer over the year. This stickiness is of both types: sparse negative nominal wage changes which affect an estimated 9.4 percent of wage earners, and menu costs of small wage changes affecting a smaller 1.04 to 1.66 percent of wage earners. Practically all of the spike at nominal zero wage change for wage earners can be accounted for by the "pile-up" from otherwise negative or small positive observations.[21] This pattern of wage stickiness implies clear money illusion in its focus on nominal rather than real values.[22]

This conclusion seems startlingly different from that arrived at by Kenneth McLaughlin (1994) who, using the same data set, concluded that his results combine to "paint a sharp picture of wage variability" (p. 386). McLaughlin's conclusion rests more on his findings regarding real wages than nominal wages, however—a subject which I do not address. His evidence on nominal wages is essentially similar to mine, with small differences attributable to somewhat different samples.[23] While he too finds "some evidence of nominal wage rigidity" (pp. 385) in the spike at zero, he rejects nominal downward stickiness because of statistically insignificant skewness statistics. Skewness statistics, however, tend to be highly dominated by

[21] This is calculated from the parameters of the estimated equation (3) and actual wage distributions.

[22] Using different approaches, David Card and Dean Hyslop (1995) and George Akerlof et al. (1996) have corroborated the results in this paper.

[23] I exclude the SEO subsample because it heavily oversamples low-income workers. McLaughlin includes it because he had found that it did not substantially change his early results. In addition, I use a somewhat wider time period than did McLaughlin. Finally, differences in the appearance of our histograms seem to be driven by cosmetic decisions regarding the width of categories in the graph rather than substantive differences.

observations far from the median and are therefore not a powerful test of rigidity near the nominal zero point.[24] The primary contribution of the present paper is to apply direct tests of downward nominal rigidity and menu costs based on fitting the distribution of nominal wage changes. Disaggregating by year gives additional accuracy to these fitted distributions. These direct tests identify both downward nominal stickiness and sparse small wage changes.

One can imagine many factors such as increasing foreign competition and declining inflation that could lead to a rise in the frequency of nominal wage cuts in the 1980's. It is striking that even in the 1980's we still find confirmation of downward wage rigidity and further, that controlling for median nominal wage changes, there is no evidence that downward wage rigidity diminished from the 1970's to the 1980's.

The distinction between wage and salary earners appears much more important in determining the pay rate change distributions than occupation. For *salaried* workers, while there is some weak evidence of downward nominal wage stickiness in the 1970's, there seems to be no statistically identifiable evidence of this kind of stickiness remaining by the mid-1980's. On the other hand, there is evidence of menu costs associated with small salary changes affecting an estimated 1.94 percent of salary earners. On average for the period 1977–1988, I estimate that 32 percent of the predicted spike at zero can be explained by pile-up from menu costs.[25]

Exactly why salaries are not downwardly rigid is, at this point, only conjecture. Even considering only positive changes, salary changes have higher variance than wage changes, suggesting more responsiveness to changing demand or to merit. Another possible reason for the salary results may be that the analysis uses hourly pay rates, the only data available from the PSID, but that salaries refer to longer durations, often a year. Annual salaries may be nominally sticky, but if hours vary, the hourly pay need exhibit no stickiness. To investigate whether the absence of observed stickiness among salary earners is due to this data limitation, equation (3) was reestimated for salaried workers who did not change usual hours during the year. For this subsample comprising 47 percent of the total salaried sample, results were qualitatively the same as for the entire sample and indicate no drop below nominal zero. Further research is needed to determine whether these differences between wage and salaried workers are corroborated in other data sets with better salary data.

The evidence estimated here can provide an important input into discussions of the macroeconomic ramifications of sticky wages. They provide a challenge to future research to determine whether menu costs or downwardly sticky wages of the magnitudes estimated here create significant macroeconomic impacts.

[24] This argument must be credited to McLaughlin himself, from a personal conversation discussing our results.

[25] For these calculations for salary earners, I use the version of equation (3) with time trends.

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