Wage Rigidity:

A Look Inside the Firm.

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Abstract: This paper tests for nominal salary rigidity using panel data from two large service-sector firms. Distributions of the firms' salary changes exhibit nominal rigidity: few nominal wage cuts, a pile-up of observations at zero, and positive skewness and asymmetry. In addition, these characteristics become more pronounced in periods of low inflation. These results are much stronger than those found in the previous literature. Further analysis shows that the sizable measurement error in the PSID and the fact that establishment surveys typically follow average wages within jobs may bias results in the previous literature toward rejecting downward nominal wage rigidity.

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As inflation drops to around 2 percent, there has been renewed interest in determining if inflation "greases the wheels" of the labor market. If true, when firms experience negative shocks, their ability to lower costs by cutting real wages is limited because workers resist nominal pay declines. The lower the inflation rate, the less wiggle room firms have to adjust real wages without cutting nominal pay, and firms must resort to other means to reduce costs. One standard response is to cut jobs. The existence of downward nominal wage rigidity, therefore, can imply a permanent relationship between inflation and unemployment.

While of obvious concern to economists and policy makers alike, documenting the existence and extent of nominal wage rigidity has always been difficult. Tests of nominal wage rigidity traditionally involved estimating the macroeconomic correlation between real wages and business cycle variables. One implication of nominal wage rigidity is that real wages are negatively correlated with the business cycle. However, tests based on this prediction have been notoriously inconclusive.[1]

Recent empirical work takes a different tack. Instead of focusing on the macro implications, the current literature tests for downward nominal wage rigidity (DNWR) at the micro level. In order to perform such tests, specific information is needed. The greasing-the-wheels scenario refers to behavior occurring inside firms, within jobs, and to wages for individuals. Therefore, an ideal dataset would follow wages of employees in specific jobs within establishments. In addition, the micro data must be unusually free of measurement error, as the focus is on the razor's edge between wage changes above and below zero. Even measurement error that is small and normally distributed around zero can have large effects on the proportion of negative observations. Finally, if wages are rigid, adjustments can be made on margins besides employment. For example, firms can reduce other forms of compensation such as health care or bonuses, vary hours, demote workers, adjust quality, and workers can quit rather than take a pay cut. Because firms and workers can respond in a variety of ways, downward nominal wage rigidity may be near impossible to identify on an aggregate level.

So far, two main types of micro data have been used: survey datasets that track

1. See Abraham and Haltiwanger (1994) and Fleischman (1997) for good reviews of the literature. See also Estevão and Wilson (1998) for a discussion of the inappropriateness of wage cyclicality tests for evaluating nominal wage rigidity models.

individuals, in particular, the Panel Study of Income Dynamics (PSID); and establishment datasets that track jobs, such as the data used in constructing the Employment Cost Index or the Federal Reserve Bank of Cleveland Salary Survey. Research using both types of datasets has looked at the shape of the distribution of wage changes and how that distribution varies with inflation. Downward nominal wage rigidity implies that the distribution have few negative observations, a pile-up of observations at zero, and be positively skewed. The last two characteristics should become more pronounced in periods of low inflation.

Studies using the PSID have found mixed evidence of nominal wage rigidity. With about 20 percent of all wage changes in the PSID below zero, employers do not appear to shy away from wage cuts. However, all the studies show that the distribution of wage changes in the PSID has a spike at zero and some degree of positive skewness. Beyond that, Lebow, Stockton, and Wascher (1995) find that for hourly wage earners (but not salaried workers) the relative mass in the upper and lower tails of the distribution of wage changes is significantly negatively correlated with inflation while the spike at zero is not. Kahn (1997) finds significant evidence that the distribution of wage changes is truncated below zero--again just for hourly wage earners. Card and Hyslop (1996), using both the PSID and matched CPS samples, show evidence of wage rigidity in the annual distributions of wage changes but "only a weak statistical relationship between the rate of inflation and the pace of relative wage adjustments across local labor markets." McLaughlin (1998), building on earlier work (1994), reports that although the distribution of wage earners is skewed to the right, there is no correlation between the skewness measure and inflation.

The PSID has the advantage of following large numbers of people over the period from 1968 to the present. It contains detailed information on individuals' personal characteristics, wage, industry, and union status. But, it has two serious drawbacks. First, the PSID and other datasets that use self-reported information contain far less accurate data on wages and salaries than would an establishment level dataset (Bound et al., 1989 and Akerlof, Dickens, and Perry, 1996). And, in addition to general measurement error, these datasets are plagued by recall and rounding bias. This is particularly relevant when quantifying the extent and the sensitivity to inflation of changes in wages around zero (Shea, 1997). Second, standard aggregate and longitudinal datasets cannot identify within-firm job switching, which is especially important because there is probably more

flexibility in wages for those workers who change jobs, even inside firms.[2] Unfortunately, the PSID measures of job switching and tenure are also very inaccurate (Brown and Light, 1992). More problematic, after 1979, the PSID does not inquire about intra-firm job changes.

The other type of data used is from establishments. Most recently, Lebow, Saks, and Wilson (1999), uses the micro data from the Employment Cost Index produced by the Bureau of Labor Statistics. This dataset contains detailed compensation data on roughly 20,000 jobs in about 4,500 establishments over the period from 1980 to the present. Lebow et al. find that the number of nominal wage cuts is about half what one would expect in the absence of DNWR. These results are less extreme when benefits are included but the amount of rigidity in total compensation is still substantial. Another study with establishment data, Groshen and Schweitzer (1996), uses the Federal Reserve Bank of Cleveland Community Salary Survey (CSS), an extensive dataset that tracks the average salaries of multiple occupations at almost 200 employers over 37 years. They find evidence of inflation-induced inter-occupational wage-changes which they argue is consistent with some grease-effect of inflation.

Unlike the PSID, these establishment datasets follow jobs rather than individuals, giving a clearer picture of within job and establishment wage changes. The data are also more accurate because they are taken from establishment records rather than self-reports. Finally, the micro-ECI data contain detailed information on benefits, allowing tests to see if firms respond to wage and salary rigidity by varying other forms of compensation.

However, these data also have limitations. By tracking jobs rather than individuals, the unit of observation in salary survey data is the average wage or salary for all workers in a given job. Therefore, wages can vary simply if the number or type of workers in the job varies. For example, the average wage in a job could drop if a more tenured worker retired, even if all workers received pay raises. The opposite is also true, if a more senior worker joined the job group, the wage change might be coded as a raise even if everyone had pay cuts. Or one large pay raise could mask cuts and vice versa. Moreover, because the data just track jobs, they cannot present a dynamic picture of how firms might alter workforce hours and promotions or demotions in response to changes in aggregate conditions.

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2. Two of the traditional explanations of DNWR--money illusion and perception of one's ranking relative to one's peers--should both be less relevant if the tasks performed, peer group, and job are changed.

From the above discussion, it is clear that what is needed is a more comprehensive dataset. This paper seeks to fill this gap by using two firm-level datasets that track *both* individuals and jobs. While limited in scope--only two service-producing firms in the private sector are studied here--these datasets contain very detailed information on the salary, job, age, sex, and tenure of the workers. The dataset for the first firm follows about 6,000 workers from 1982 to 1994 and the dataset for the second firm spans 1969 to 1988 and follows about 12,000 workers.

The results using these data show strong evidence of nominal wage rigidity: Workers in both firms experience almost no nominal wage cuts, a number of workers receive constant nominal salaries across years, and the distribution is positively skewed. In addition, the skewness is negatively correlated with inflation. These results indicate a much more sizable and significant degree of downward nominal wage rigidity than those found in the PSID and suggest that the significant measurement error in that dataset may cloud those results.

I also use my datasets to examine the costs of using data that follows only individuals or only jobs. Testing for the impact of intra-firm movement, workers who switch jobs and/or hours within the firm show much less evidence of nominal wage rigidity. However, they make up a relatively small fraction of total observations in my samples and, therefore, results controlling for intra-firm hours and worker movement are similar to the those calculated using the total sample. In the case of establishment data, salary data constructed from averaging salary levels within job code appear much more flexible that the data on individuals from which it was constructed. This indicates that the micro-ECI data, for example, may understate the true extent of downward nominal wage rigidity.

The structure of the paper is as follows: after the introduction, section two describes and summarizes the datasets; section three presents the results; and section four discusses the conclusions.

II. THE DATA

This section provides a brief overview of the two datasets used in the paper.[3] Firm 1 is a large--between 7,000 and 8,000 employees--non-profit organization in the private service-producing sector. The dataset for this firm contains observations for its support staff--about

3. For more detailed information see Wilson (1997).

1,700 workers per year from 1982 to 1994. In total it includes about 22,000 observations on approximately 6,000 workers. The variables are salary, standard hours per week, job code (including level and job title), tenure, age, sex, and an identification number for each support staff employee as of January 1 of each year.

The pay structure for these support staff workers is fairly standard for large firms. Jobs are classified into four pay grades based on the skill, training, experience, and education required to perform the tasks of the job. Within each grade are numerous job titles. The salaries for all the jobs in a given pay grade are restricted to fall within the minimum and maximum salaries that define each grade's pay range. Benefits are constant across levels. Workers are labeled as "stayers" if they did not switch pay grades during a given year. Workers are classified as "movers" in a given year if they switched pay grades.[4]

The information for firm 2 comes from the personnel records of management workers in a large, for-profit, service-sector firm. The dataset was compiled by George Baker, Michael Gibbs, and Bengt Holmström (BGH) and is detailed in BGH (1993, 1994a, and 1994b). As of December 31 of each year from 1969-1988, observations exist on the following variables: employee i.d., age, sex, race, education, performance ranking, salary, job level, and a dummy for title switch.[5] In total the dataset includes about 62,000 person/year observations for about 12,000 individuals. The management staff comprises roughly 20 percent of the firm's total workforce over the time period. The number of management employees in firm 2 more than quadrupled during the 20-year sample period to over 5,000 by 1988.

Unlike the dataset for the first firm, this firm's pay structure is not known explicitly--i.e. this dataset does not contain the official pay compensation matrix, pay range, and annual salary budget or "raise allocations". To circumvent this problem, BGH generated a hierarchy using

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4. This classification is more restrictive than that in the literature. In the PSID studies, the term "stayers" refers to workers who do not switch firms and includes those who switch jobs within the firm.

5. Unlike firm 1, for firm 2 the date the worker enters the firm is not available. Therefore, tenure is calculated and is known only for employees who entered the firm after 1969. Because firm employment grew strongly over the period, this restriction eliminates fewer than 20 percent of the sample, leaving more than 48,000 worker-year observations.

the transition matrix across job titles.[6] The hierarchy allows measures of promotion and job switching to be identified.[7]

Figure 1 and table 1 summarize the datasets and describe the variables used in this study. Although the variables in the two datasets are very similar, there are interesting contrasts. The time periods spanned by the two datasets differ. Data for firm 1 are limited to the 1982-1994 period, while data from firm 2 span 1969 to 1988--close to the period covered by the PSID dataset. The firms are from separate industries and firm 1 is non-profit while firm 2 is for profit. Workers in firm 1 are in support occupations and from a local labor market while firm 2 workers are in managerial occupations and are drawn from a national labor market.

Figure 1 - Summary of Datasets

Firm 1	Firm 2
• Non-Profit	• For Profit
• Support Staff Workers	• Managerial Workers
• 1/1/82 to 1/1/94	• 12/31/69 to 12/31/88
• Variables in panel: i.d., salary, job code, pay grade, age, sex, year began at the firm	• Variables in panel: i.d., salary, pay grade, job switch, age, sex, race, education, and performance evaluation
• Add'l variables: pay structure across time, raise allocations, method for classifying workers	• Add'l variables: return on assets, normalized net income and assets
• One geographical location	• Multiple geographical locations

6. To assign jobs to levels they begin with 14 major job titles--about 90 percent of all observations. Titles assigned to level 1, the lowest level in the hierarchy, are selected based on hiring patterns. Almost all workers holding these titles are hired in rather than transferred from other positions. To determine titles in other levels, the salary matrix plays a larger role. Titles that represented the primary source of shifts from level 1 are assigned to level 2 and so on. The assignment of titles to levels is fairly straightforward and a diagram of the hierarchy is presented in the first graph in BGH (1994a).

7. For this paper, the dataset does not contain the job code variable, only the job switching measure.

Table 1 - Summary Statistics for Both Firms*

	Firm 1		Firm 2	
Variable	Mean	Std.Dev.	Mean	Std.Dev.
Real Salary ($85)	15,612	4,075	46,710	22,955
Real Sal Growth (%)	3.0	13.5	4.2	6.5
Age (Years)	40	12	41	9.5
Sex (F=1,M=0)	.83	.37	--	--
Education (Years)	--	--	15.3	2.5
Performance(Scale=1-4)	--	--	1.93	0.72
Move (Y=1,N=0)	0.11	0.31	0.19	0.40
Pay Grade (1-4 & 1-7)	3.31	0.72	2.60	1.12
Tenure (Years)	8.24	7.12	5.50	3.48
No. of Obs (Total)	15,992	--	47,012	--

* For observations where salary growth exists.
Note: Real salary figures are deflated by the CPI (1985=100). For firm 2, sex and race dummies exist but are not labeled at the request of the firm. Performances are ranked from 4 to 1 with 1 being the highest. Pay grades for firm 1 go from 1 to 4 and for firm 2 from 1 to 7. Tenure calculations for firm 2 begin with entrants in 1970.

III. TESTS FOR NOMINAL WAGE RIGIDITY

A. Basic Results

The standard way of testing for nominal wage rigidity in disaggregated data is to look at the distribution of nominal salary changes. If workers resist pay cuts and wages are rigid, the distribution should contain very few observations below zero, a spike at zero (arguably the minimum acceptable wage change if DNWR exists), and be positively skewed. However, these features are not enough. If wages are nominally rigid than the asymmetry and massing at zero should become more pronounced in low inflation times.

Beginning with the shape of the distribution, chart 1 presents the distribution of salary changes for workers who did not switch pay grades for both firm 1 and firm 2. For firm 1, the chart presents only those nominal salary changes for which the standard weekly hours of the worker were also unchanged. From the chart, the salaries in these firms appear to display all the signs of nominal wage rigidity.

In contrast to the other survey and establishment data where almost 20 percent of changes in salary/hour are negative, both firms have only a trivial number of total nominal salary changes below zero--in total for firm 1 there were only 614 cases of negative nominal salary growth. Nominal salaries fell in only 13 cases when hours worked did not change, and only 4 of those were declines in the wages of stayers. In firm 2 only 24 salaries ever fell; 19 of them were stayers. (Real salaries fell for about 25 percent of the observations in both firms.) A massing at zero is visible in both cases. In firm 1, 2.5 percent of the observations for stayers had no salary change compared with 8.2 percent of the observations in firm 2 (table 2). Finally, both distributions are positively skewed with skewness coefficients for stayers' salaries of 1.4 and 1.6 for firm 1 and firm 2, respectively.

Table 2 -- Summary Statistics for Stayers (simple averages, percent)

	Firm 1 Job Stayers (no hrs. change)	Firm 2 Job Stayers	PSID[3] Firm Stayers (LSW, 95)
Mean Salary Increases [1]	6.1	8.9	7.1
Median Salary Increases [1]	5.0	8.5	6.6
Standard deviation[1]	3.1	5.3	13.0
% w/ nominal salary cuts	negl.	0.1	17.1
% w/ salary change=0	2.5	8.2	8.1
% w/ real wage cuts	22.6	25.6	44.2
Skewness[1]	1.4	1.6	.08
Asymmetry[2]	10.6	6.5	6.8
# of obs.	12,983	37,914	19,632
% of total obs.	81.2	80.6	n.a.
Sample period	1982-94	1970-88	1971-88

[1] Calculated as total sample average for stayers.
[2] Calculated by dividing the number of observations above twice the median minus the number of observations below 0 by the total number of observations and multiplying by 100.
[3] Note that the PSID uses salary/hour measures rather than salary.

One problem with the skewness measure is that it is quite sensitive to outliers and may be driven by asymmetry anywhere in the distribution, not just from a lack of negative observations. Lebow, Stockton, and Wascher (1994) build another measure of asymmetry that is almost insensitive to outliers and better able to capture differences in the mass of observations in the tails. This measure is the difference between the cumulative frequency above two times the median and the cumulative frequency below zero: LSW = [1-F(2*median)]-F(0). This statistic is designed to capture relative differences in the size of the two tails--zero and two times the median being equidistant from the median--and targets exactly that part of the distribution affected by downward nominal wage rigidity--the mass below zero. This measure of asymmetry is zero if the distribution is symmetric and positive if the distribution contains disproportionately few salary cuts. As can be seen on table 2, this asymmetry measure is very positive for both firms, with 10.6 and 6.5 percent more observations above two times the median than below zero for firms 1 and 2 respectively.

One further characteristic of both distributions is the absence of small salary changes. Chart 2 shows a more truncated distribution, better highlighting the lack of salary changes near zero in both firms. This censoring around zero may reflect administrative costs to changing the salary of workers--so-called "menu costs". In the case of these firms, the costs of small salary changes do not appear to be outweighed by the benefits of higher wages.[8]

The next step is to determine if the distributions of stayers' salary changes are sensitive to inflation. If the firms' behavior is affected by rigidity then the higher the inflation rate, the greater the ability of the firms to give a full distribution of salary changes. In this case, skewness, the asymmetry measure, and the fraction of wage changes at zero would all fall in response to higher inflation.

Charts 3 and 4 show the distribution of salary changes for each firm during periods of high median wage change and low median wage change.[9] In chart 3, the distribution of salary

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8. Another plausible explanation for the lack of small salary changes may be that workers are more offended by very small pay gains than no change at all. A constant salary, especially in low inflation times, could be viewed as containing little signal value whereas a 1/2 percent raise or of only $300 or $400 a year is read by the worker as a signal of poor performance relative to one's peers. From the firm's perspective, the deleterious effect on worker morale and effort from a small pay raise may then be greater than no raise at all.

9. The firm's median wage change is often used in studies of wage rigidity as a way of capturing both average price and productivity changes for the firm. Tables in the appendix show results using 12-month changes

change for firm 1 shows much more truncation and skewness during the low wage change period than the high wage change period, consistent with binding nominal wage rigidity. For firm 2, the overall shape of the distribution also changes between the high and low median wage change periods; the skewness and asymmetry measures fall and the massing at zero is reduced noticeably in higher inflation times. Both firms, therefore, have additional visual evidence of downward salary rigidity.

Table 3 presents the results of more rigorous tests for the effects of nominal wage rigidity on the distribution of salary changes. In order not to confound changes in salary with changes in hours worked, salary levels rather than salary per hour measures are used here. Salary per hour measures will be used later, when direct comparisons are made with the previous literature. In the first column, the annual asymmetry measure for each firm is regressed on the average annual change in the median wage and the coefficient on the median wage change is presented. In order to control for the influence of other business cycle factors that may be correlated with the wage inflation rate, the annual average unemployment rate is added. The next two columns contain the results from regressing the annual skewness measure or the fraction of observations at zero for each year on the annual change in the median wage and unemployment.

The results in table 3 support binding nominal wage rigidity. For firm 1, the coefficient on the wage inflation variable in the asymmetry and skewness regressions are negative, indicating that as inflation rises asymmetry and skewness fall. Growth in the firm's median salary significantly reduces skewness, while the coefficient in the asymmetry equation is marginally significant. In terms of magnitude, the imbalance between the right and left tails decreases by 1-1/2 percentage points for every one percentage point rise in the median salary change and skewness falls by 0.4 when the change in the median increases by one percentage point. The only contradictory result, though not significant, is that inflation seems to increase the massing at zero.

For firm 2, the distribution of salary growth is consistently negatively related to the inflation variables. Median salary growth significantly reduces the asymmetry of the distribution and the massing at zero. Here, a one percentage point rise in median salary increase lowers asymmetry by about the same amount, lowers skewness by about 0.1, and the massing at zero

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in the log of the metropolitan CPI-U for firm 1 and the experimental CPI for firm 2. The results using the CPI are qualitatively the same.

drops almost 2 percentage points. All told, especially given the limited number of annual observations, the results provide notable evidence of binding downward wage rigidity.

Table 3--Sensitivity of Stayers Salary Distribution to Median Salary Growth (controlling for unemployment)

	Coefficient on Median Salary Growth		
Dependent Variable	Asymmetry	Skewness	Percent at Zero
Firm 1			
Coeff.	-1.475	-.430**	0.129
Std. err.	(.957)	(.155)	(.096)
R^2	.40	.50	.40
Deg. of F.	9	9	9
Firm 2			
Coeff.	-.953*	-.098	-1.802**
Std. err.	(.532)	(.144)	(.636)
R^2	.17	.09	.34
Deg of F.	16	16	16

** significant at the 5 percent level. * significant at the 10 percent level.
Note: Regression of dependent variable (annual measures of asymmetry, skewness, or percent at zero) on a constant, the firm's annual median salary growth and the annual average national or metropolitan unemployment rate.

B. Comparison with PSID

How do results using these firm-level datasets correspond to those in the previous literature? Table 4 shows a comparison of results using the firm data with results using the PSID. These results differ from those shown earlier in that, in order to make the samples as comparable as possible, I have constructed results using all the observations for the two firms (not just job stayers) and constructed a *salary per hour* measure where possible.[10] These results are compared to PSID results based on workers who do not switch firms, and are taken for the most part from Lebow, Stockton, and Wascher (1995). The PSID results are from the regression of asymmetry

10. Note that because the PSID uses all intrafirm workers the salary changes are for both job stayers and job switchers within the firm.

on the median wage and unemployment and refer only to salary workers. The "Kahn test" results using the PSID, discussed in more detail below, are for salaried workers and are taken from Kahn (1997).

As can be seen in comparing the summary statistics, the fraction of observations below zero is trivial in Firm 1 and Firm 2 but almost 20 percent in the PSID. The asymmetry and skewness statistics are much more positive for the two firms than for the PSID. Only the spike at zero seems more pronounced for the PSID. All these results are consistent with the greater measurement error in the PSID; the higher spike at zero in the PSID distribution reflecting rounding error on the part of respondents that would be absent in the firm data. (Indeed, Lebow, Stockton, and Wascher calculate that 40 percent of the spike at zero in the PSID is due to rounding.)

The last two columns of the table present results testing the sensitivity of the distributions to inflation. The coefficient on the change in the median wage on the regression of asymmetry on median wage change and unemployment is negative, sizeable, and significant to the 10 percent level for firm 1 and firm 2. For salaried workers in the PSID the coefficient is significantly positive. The final set of results is for the test detailed in Kahn (1997). This tests estimates whether histogram bars a given distance from the median of the wage distribution are truncated when they fall below zero. This test makes no assumptions about the underlying shape of the distribution and is targetted directly at identifying effects of downward nominal wage rigidity.[11] The results from firm 1 and firm 2 suggest that the mass of the bars is cut 75 to 100 percent if it goes below zero. However, Kahn actually finds the bars' mass increases about 30 percent. The results are more supportive of DNWR, when only wage earners are used, but are still not as sizable as in those shown here. In sum, the firm results differ markedly from those using the PSID and are much more supportive of nominal wage rigidity.

11. For a detailed discussion of the strengths and weaknesses of the various tests for DNWR see Lebow, Saks, and Wilson (1999).

Table 4 -- Comparison with PSID (For Firms' Total Sample, Salary/Hour)

	Median	%<0	%=0	LSW Asym.	Skewness	LSW Asym. Regn[2]	Kahn Test
Firm 1	5.3	0.4	2.2	16.0	1.53	-1.76*	-0.79**
Firm 2[1]	9.1	0.1	6.8	8.5	1.75	-1.12*	-1.00**
PSID[2]							
Wg. & Sal. Earners	6.6	17.8	8.1	6.8	0.08	.70**	0.33**
Wage Earners	6.5	11.9	10.6	9.7	0.48	-.88**	-.47**

**indicates significance at the 5% level, and * indicates significance at the 10% level.

1. Results are not per hour since no hours measures exist.

2. Results in columns 1-6 are for all stayers and are taken from Lebow, Stockton, and Wascher (1995). Results in column 7 are taken from the proportional models in Kahn (1997) and are for salaried workers only in row 3.

One further criticism of the PSID, discussed earlier, was the fact that it was difficult to control for intra-firm job and hours changes. Recall that even with resistance to nominal pay cuts for a given job, firms may alter the wages of their workers in other ways. One way firms could manipulate salaries is by moving their workers into different jobs or pay grades. For example, in times of low inflation firms may choose to demote a greater proportion of their workforce as a way of giving lower salaries. Alternatively, the firm may also manipulate the hours of workers when it cannot adjust the salary. For example, firms might keep a worker's salary constant but increase hours, causing the hourly cost to the firm of the worker to fall, even when their salaries did not.

The question, then, is whether this manipulation of hours and worker placement has a significant impact on the overall distribution of firm wages. Charts 5 and 6 address this question. In chart 5, the first panel shows the distribution of stayers with fixed hours, as seen before in chart 1. The second panel plots the distribution of salary changes for workers who did not switch pay grades but who did change standard hours. As can be seen, the latter distribution is far more uniform and shows no evidence of truncation below zero. By changing hours, the firm is able to gain considerable flexibility in pay for those workers. The third panel shows the distribution of wage changes for movers. This distribution also shows less positive skewness than that of stayers with fixed hours although relatively few observations fall below zero. The final panel is of the entire distribution of salary changes. Even though close to 20 percent of the sample is made up

of workers who change hours or pay grades, they seem to have little impact on the overall distribution of wages--it looks very similar to the top panel. The same can be seen in chart 6, for firm 2, movers have a slightly less truncated distribution, but the overall distribution appears very similar to that of stayers--a massing at zero, few negative wage changes, and positive skewness.

The charts do not allow for a particularly rigorous comparison between the distribution of stayers and totals, however. To get a clearer picture, results from regression tests of the sensitivity of the total distribution of salaries to the inflation measures are shown in table 5. The first row of results for each firm are the results for salary per hour measures for workers who do not switch jobs. The second row of results for firm 1 shows the results using just the group of workers who switches jobs within the firm. The last row of results for each firm shows the regressions using the total sample. Although the evidence of nominal wage rigidity is much weaker for workers who switch jobs within the firm, these workers' share of the total workforce of each firm is probably not enough to drive the results.[12] As can be seen, there is little difference between the evidence for DNWR using the stayers or the total sample. These results suggest that, if the fraction of intra-firm job switchers is similar to that in the PSID, then not being able to control for these workers is of second-order importance.

12. See appendix tables for the results of direct tests on the sensitivity of grade switching and hours changes to inflation.

Table 5 -- Sensitivity of Total Distribution for Salary per Hour to Inflation (controlling for unemployment)

Dependent Var.		Asymmetry	Skewness	Percent at Zero
		Coefficient on Median Salary Growth		
Firm 1				
Stayers	Coef. (Std. err.)	-1.565 (.953)	-.409** (.111)	.097 (.090)
	R^2	0.40	0.35	0.35
Movers	Coef.	-0.204 (.377)	-.035 (.249)	-.385 (.287)
	R^2	0.06	0.04	0.20
Total	Coef.	-1.764 (.898)*	-.214 (.158)	.045 (.072)
	R^2	0.49	0.18	0.23
Firm 2[1]				
Stayers	Coef. (Std. err)	-.953* (.532)	-.097 (.144)	-1.802** (.636)
	R^2	0.17	0.09	0.34
Total	Coef.	-1.117* (.582)	-.018 (.157)	-1.494** (.535)
	R^2	0.21	0.00	0.33

[1] Not on a per hour basis.
** indicates significance at the 5 percent level, and * indicates significance at the 10% level.
Note: Regression of dependent variable (asymmetry, skewness, or percent at zero) on constant, median salary change and the annual average national or metropolitan unemployment rate.

C. Comparison with Establishment Datasets

Recall that establishment surveys such as the Cleveland Community Salary Survey (Groshen and Schwitzer, 1996) and micro-ECI data (Lebow, Saks, and Wilson, 1999) track jobs rather than individuals. Therefore, tests are based on changes in average salary in sampled jobs. As discussed above, the averaging of salary and wage levels can mask negative wage changes or lead to the appearance of them. Does this matter?

While there are no data on individuals within the micro-ECI and CCS surveys, the data for firm 1 contains individual job codes as well as individual identifiers. Thus, using this data, salary levels and growth rates by job can be constructed and compared to distributions constructed using salary growth by individuals. Since the micro-ECI data are salary per hour, for the comparison,

salary per hour measures are also constructed with the firm 1 data.[13]

Turning to chart 7, the upper panel depicts the distribution of changes in salary per hour for all individuals in firm 1 while the bottom panel shows the distribution of changes in salary per hour for all jobs. The difference between the two panels is striking. Averaging individual salaries within jobs leads to measures of growth in salary per hour that are much more symmetric. The distribution for jobs also has a far greater number of negatives. Not surprisingly, the distribution also becomes smoother--masking possible evidence of menu costs.

Table 6 presents summary statistics and tests for the two samples. Turning to the regression results, the coefficient on the measure of median salary growth is sizable, negative, and significant when using distributions of the individual salary measures. However, when salaries are averaged by jobs, the evidence for downward nominal wage rigidity becomes smaller and insignificant. Results from the Kahn test are even more dramatic. The *neg* coefficient in the first row, indicates that bars of the salary per hour distribution are reduced by almost 80 percent when the bars fall below zero. This result is completely reversed if job averages are used. The results from chart 7 and table 6 provide compelling evidence that, at least for firm 1, constructing average salary measures within job cells alters the nature of the wage change distribution. If the results hold true for the large establishment surveys, it suggests that the previous work may seriously underestimate the extent of downward nominal wage rigidity.

Table 6 -- Rigidity Tests for Distributions of Changes in Salary per Hour for Jobs vs. Individuals for Firm 1

				Asymmetry Regn[1]	Kahn Test[2]
	%<0	%=0	Asymmetry	Coef. on Median Sal. Growth	(Neg coef.)
By Individuals	0.4	2.2	16.0	-1.76** (.90)	-.79** (.05)
By Jobs	5.3	3.3	4.7	-.33 (.81)	.29* (.15)

**indicates significance at the 5% level, and * indicates significance at the 10% level.
1. OLS regression of annual asymmetry measure on inflation measure, unemployment rate, and a constant.
2. Results of SUR of the fraction of salary changes in a given year t, in the region between r and r-1 percentage points below the median on a constant and a set of dummy variables. n is the proportion of negative salary-change observations estimated to be piled-up at zero.

13. The earlier distributions were not constructed using measures of salary per hour measures but with the salary itself. This was done to prevent attributing changes in salaries to changes in hours alone.

IV. CONCLUSION

Strong evidence of downward nominal wage rigidity has been particularly difficult to find. Traditional studies relying on the correlation of wages with business cycle variables have been remarkably inconclusive. More recent work using micro datasets such as the PSID have been troubled by issues of measurement error and by the difficulty of controlling for across and within firm movement. Data on establishments also have problems, most notably, that the unit of observation is the average salary for jobs and is thus variations in worker composition alone can generate or obscure wage cuts. Economists have even resorted to telephone polls to determine if workers receive nominal pay cuts on a regular basis. (Akerlof, Dickens, and Perry, 1996)

This paper uses datasets on two firms that contain extensive information on the pay structure of the firms and track all support staff or managerial workers over a period of at least 12 years. Because these data are establishment data, they should be considered more accurate than individual survey data. In addition, because they contain both individual and job related information, the salary changes for both jobs and workers can be calculated--allowing for a far clearer measurement of wage rigidity than seen in the past. Moreover, the importance of hours and job switching for overall wage rigidity can be calculated.

Data for these two firms show substantial evidence of downward nominal wage rigidity. Exceptionally few workers receive nominal pay cuts, a significant number of workers receive no change in their nominal salaries across years, and the distribution of salary changes for workers who do not switch jobs within the firm is positively skewed and asymmetric. For both firms, the measures of skewness and asymmetry are negatively related to measures of inflation--a further sign of downward nominal wage rigidity.

Studies using the PSID have found a significant number of nominal pay cuts and less sensitivity of the distribution to measures of inflation. One explanation for this is that the high level of measurement error associated with individual responses obscures the true level of DNWR. The evidence from these two firms is consistent with that explanation. Another explanation is that the PSID cannot identify intra-firm job switching. In this paper, the rate of promotion and demotion, and the standardized hours of workers are all sensitive to inflation. However, the results based on the distributional characteristics of the salary changes for all workers are basically the same as for those who do not switch jobs or change hours worked over the year.

Using the detailed firm level data here also sheds light on the possible cost of using establishment data on wages for jobs where the wage measure is the average wage of all workers in that job. With my firm data, comparing distributions constructed using salary measures of the individuals to wage measures for jobs constructed by averaging those individual salaries shows forcefully that averaging acts to increase the incidence of negative wage changes and reduce the sensitivity of the distribution to inflation. Judgments based on only data for jobs in my data would dramatically underestimate the true extent of downward nominal wage rigidity.

Although only two companies are used in this study, the results raise larger issues about tests for nominal wage rigidity. They point to the importance of datasets that track the wages associated with both workers and jobs to gain a clearer understanding of the source, extent, and costs of wage rigidity.

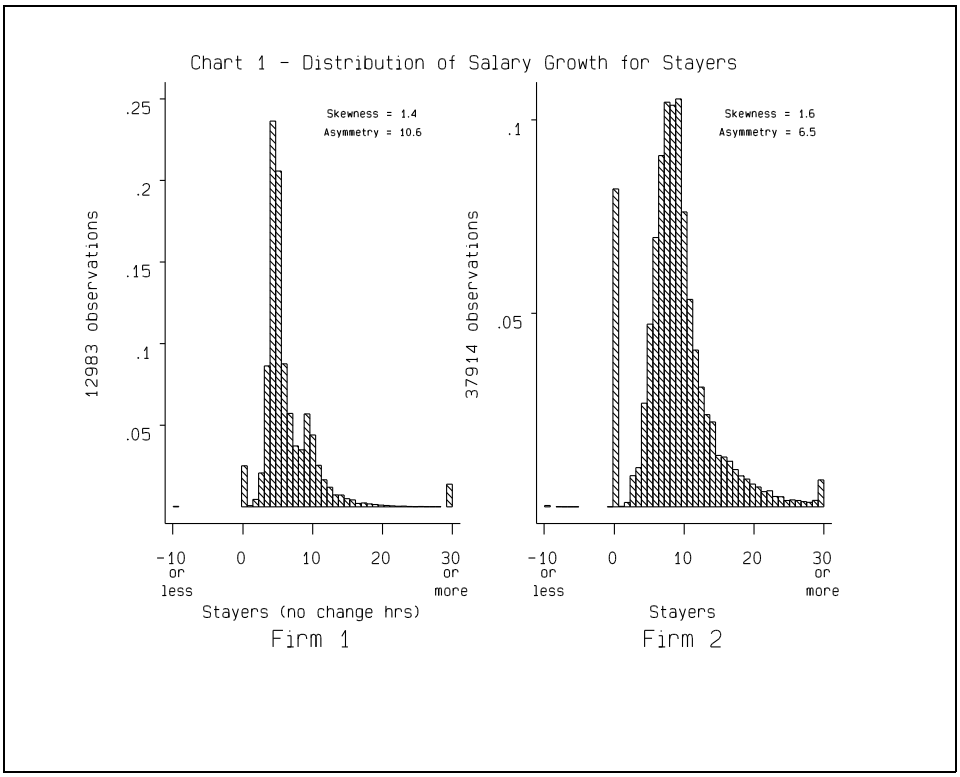


Chart 1 - Distribution of Salary Growth for Stayers
12983 observations
Skewness = 1.4
Asymmetry = 10.6
.25
.2
.15
.1
.05
-10 or less
0
10
20
30 or more
Stayers (no change hrs)
Firm 1
37914 observations
Skewness = 1.6
Asymmetry = 6.5
.1
.05
-10 or less
0
10
20
30 or more
Stayers
Firm 2

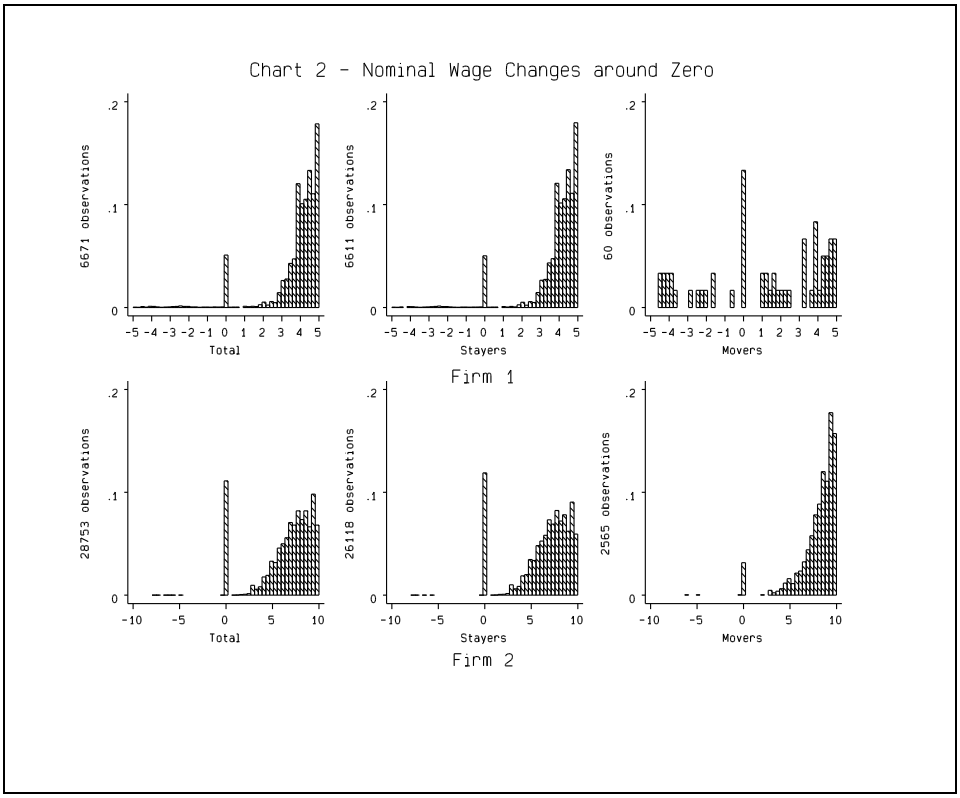


Chart 2 - Nominal Wage Changes around Zero
6671 observations
Total
6611 observations
Stayers
60 observations
Movers
Firm 1
28753 observations
Total
26118 observations
Stayers
2565 observations
Movers
Firm 2

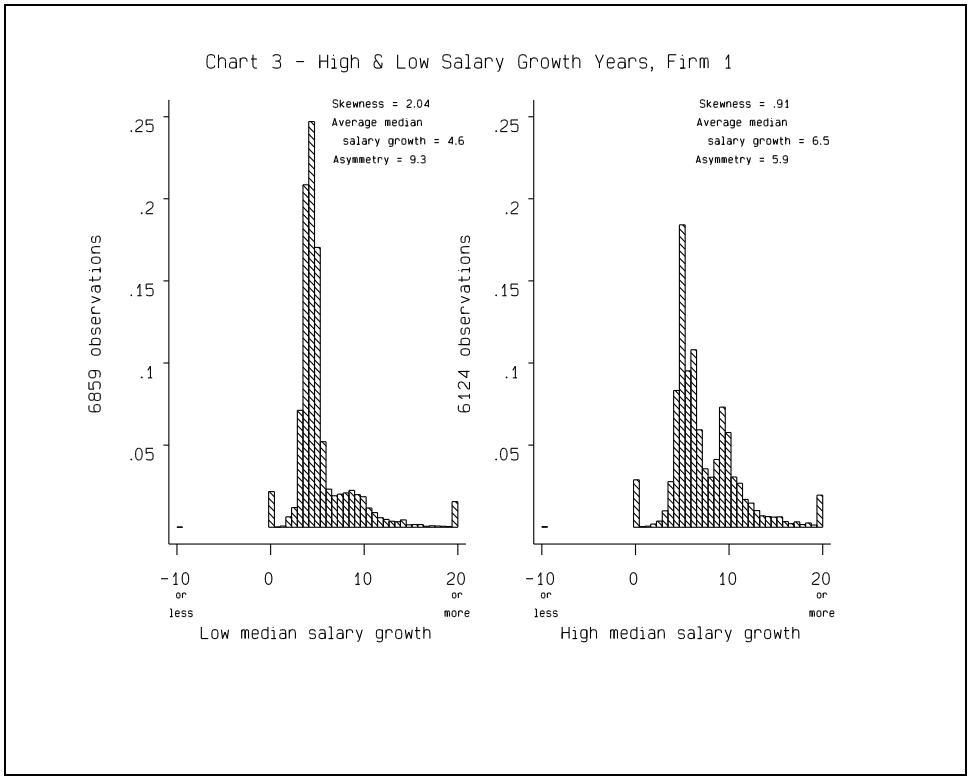


Chart 3 - High & Low Salary Growth Years, Firm 1
Skewness = 2.04
Average median
salary growth = 4.6
Asymmetry = 9.3
6859 observations
.25
.2
.15
.1
.05
-10
or
less
0
10
20
or
more
Low median salary growth
Skewness = .91
Average median
salary growth = 6.5
Asymmetry = 5.9
6124 observations
High median salary growth

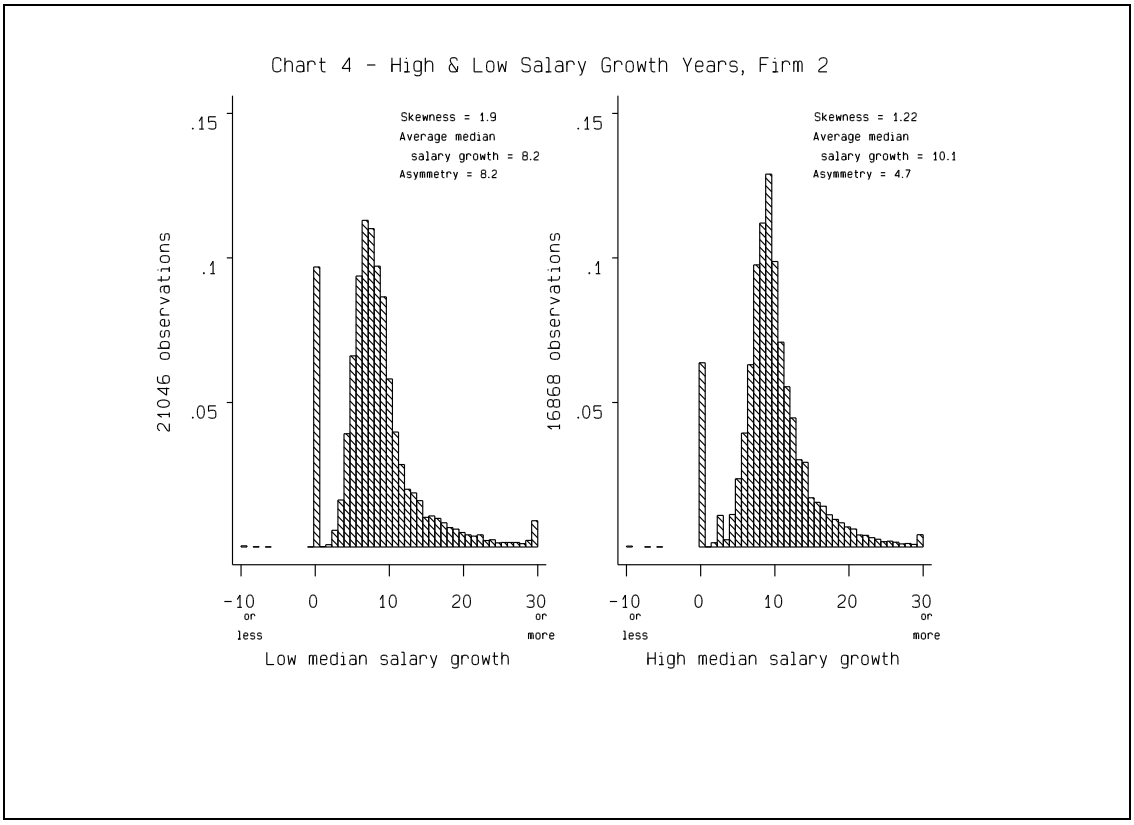


Chart 4 - High & Low Salary Growth Years, Firm 2
Skewness = 1.9
Average median
salary growth = 8.2
Asymmetry = 8.2
21046 observations
.15
.1
.05
-10
or
less
0
10
20
30
or
more
Low median salary growth
Skewness = 1.22
Average median
salary growth = 10.1
Asymmetry = 4.7
16868 observations
High median salary growth

Chart 5 - Distribution of Salary Growth, Firm 1

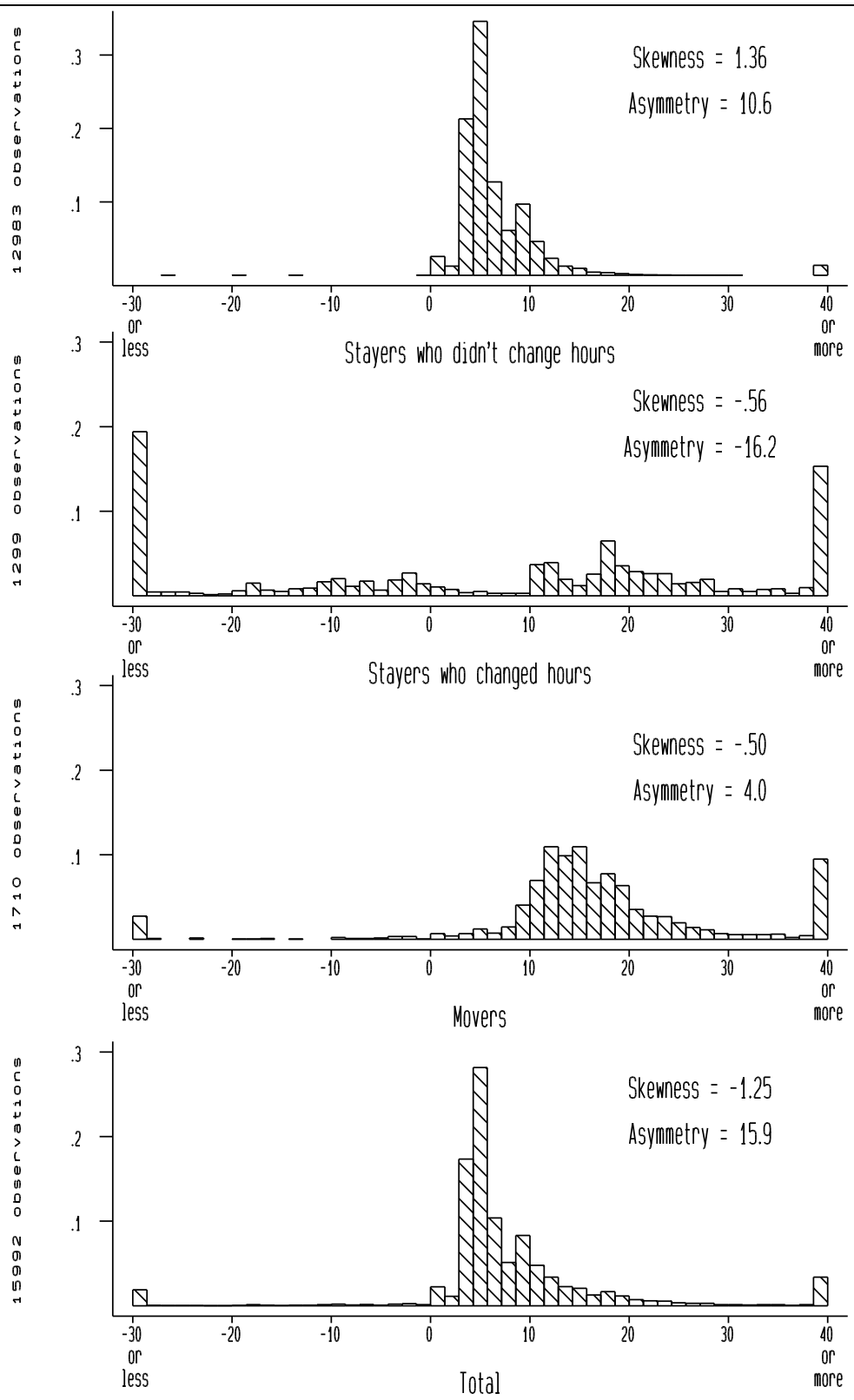


Chart 6 - Distribution of Salary Growth, Firm 2

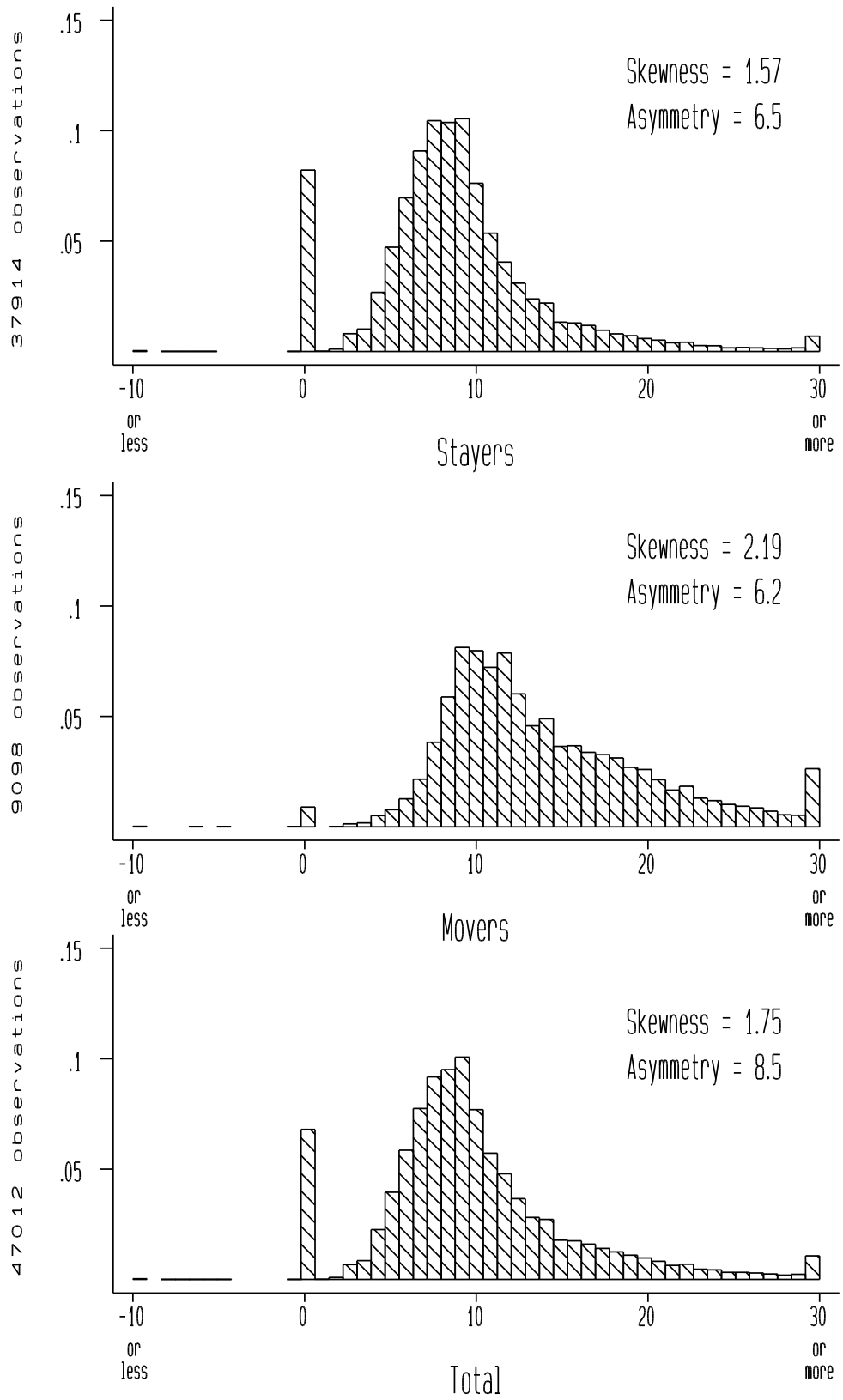
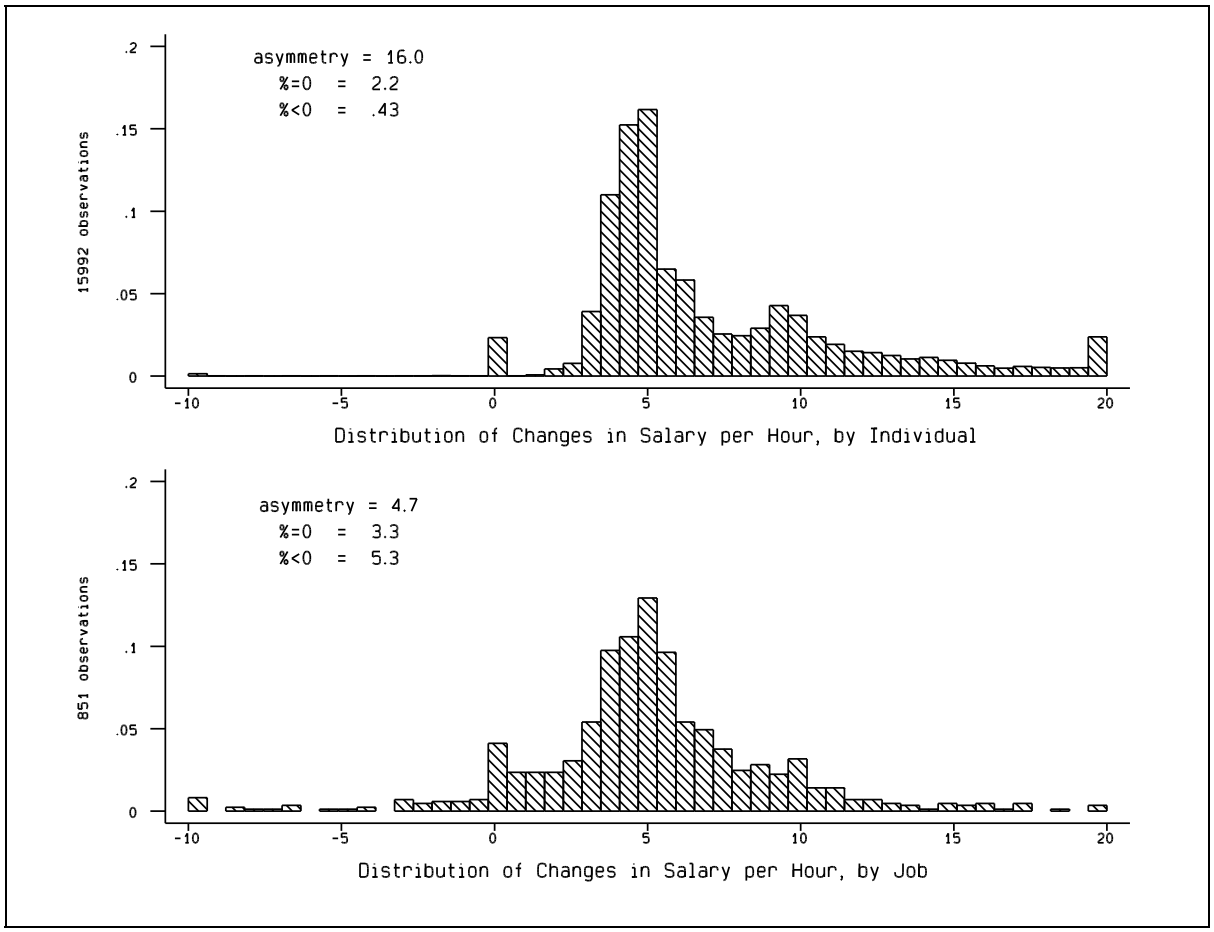


Chart 7--Comparison of Distributions of Salary per hour Growth by Individual and by Job, Firm 1



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Appendix

Table A1--Sensitivity of Stayers Salary Distribution to Inflation (controlling for unemployment)

Dependent Var.	Asymmetry		Skewness		Percent at Zero	
Independent Inflation Var.	Inflation	Median Salary Growth	Inflation	Median Salary Growth	Inflation	Median Salary Growth
Firm 1 Coeff.	-0.750	-1.475	-.271	-.430**	0.228**	0.129
Std. err.	(1.199)	(.957)	(.223)	(.155)	(.093)	(.096)
R^2	.27	.40	.20	.50	.57	.40
Deg. of F.	9	9	9	9	9	9
Firm 2 Coeff.	-.169	-.952*	-.055	-.097	-.300	-1.802**
Std. err.	(.302)	(.532)	(.075)	(.144)	(.400)	(.636)
R^2	.03	.17	.09	.09	.04	.34
Deg of F.	16	16	16	16	16	16

** indicates significance at the 5 percent level, and * indicates significance at the 10% level.

Note: Regression of dependent variable (asymmetry, skewness, or percent at zero) on constant, inflation variable (Firm 1-change in annual CPI-X or change in firm's median salary, Firm 2-change in annual metropolitan CPI-U or change in firm's median salary) and the annual average national or metropolitan unemployment rate.)

Table A2 -- Sensitivity of Grade Switching to Inflation (controlling for the unemployment rate)

	Movers		Promotion		Demotion	
	Inflation	Median Salary Growth	Inflation	Median Salary Growth	Inflation	Median Salary Growth
Firm 1 Percent coeff.	.841*	0.424	.926*	0.397	-.085	.028
Std. err.	(.339)	(.359)	(.320)	(.367)	(.075)	(.069)
R^2	.72	.59	.74	.55	.23	.14
Firm 2 Percent coeff.	.105	.377	.317*	.462	-.211*	-.085
Std. err.	(.179)	(.333)	(.163)	(.327)	(.060)	(.151)
R^2	.23	.28	.35	.28	.44	.03

* indicates significance at the 10 percent level.

Note: Regressions in the top rows are of the fraction of movers, promotions, or demotions in each year regressed on a constant, the inflation measures, and the unemployment rate.

Table A3 -- Sensitivity of Hours Change to Inflation (Controlling for Unemployment Rate)

	Hours Changed		Hours Increased		Hours Decreased	
	Inflation	Median Salary Change	Inflation	Median Salary Change	Inflation	Median Salary Change
Percent Coeff.	.559*	.081	.488*	.115*	.071	-.033
Std. err.	(.166)	(.217)	(.131)	(.179)	(.159)	(.141)
R^2	.74	.43	.66	.17	.42	.41

* indicates significance at the 10 percent level.

Note: Regressions in the top rows are of the fraction of workers with hours changes, increases, and decreases in each year regressed on a constant, the inflation measures, and the unemployment rate.