Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data*

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Abstract

Using administrative payroll data from the largest U.S. payroll processing company, we measure the extent of nominal wage rigidity in the United States. The data allow us to define a worker's per-period base contract wage separately from other forms of compensation such as overtime premiums and bonuses. We provide evidence that firms use base wages to cyclically adjust the marginal cost of their workers. Nominal base wage declines are much rarer than previously thought with only 2% of job-stayers receiving a nominal base wage cut during a given year. Approximately 35% of workers receive no base wage change year over year. We document strong evidence of both time and state dependence in nominal base wage adjustments. In addition, we provide evidence that the flexibility of new hire base wages is similar to that of existing workers. Collectively, our results can be used to discipline models of nominal wage rigidity.

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1 Introduction

It is well-known that aggregate wages exhibit muted cyclicality (Abraham and Haltiwanger, 1995) None of the last six American recessions were accompanied by declines in nominal average hourly earnings, while even real wages did not fall during the recessions of 2001 and 2009 (Grigsby, 2019). The potential sluggish response of firms' marginal cost of labor has important allocative effects in many models. To capture the stickiness observed in aggregate wages, many macroeconomic models rely on exogenously specified nominal wage rigidity.[1] However, the literature using micro data to document nominal wage adjustment is somewhat underdeveloped. This stands in stark contrast to the large literature using micro data to establish key moments of output price adjustment, which have guided theories of nominal price rigidities.[2] As a result, the nature of nominal wage stickiness remains a central question within the literature. For example, at the 2014 Jackson Hole Symposium, Janet Yellen speculated that downward nominal wage rigidity was an important reason why wages neither fell during the Great Recession nor increased faster during the subsequent recovery.

The goal of this paper is to measure the extent of nominal wage rigidity using micro data from the United States. Doing so is challenging given the complex nature of worker compensation which includes guaranteed contract earnings as well as commissions, tips, bonuses, performance pay, and overtime premiums. The purpose of incorporating wage rigidity into macroeconomic theories is to generate muted cyclicality of firms' marginal cost of labor. If different compensation components differ in their cyclical responsiveness, then so too will their impact on the cyclical response of firms' marginal costs. Similarly, if new hire wages are more easily adjusted than those of incumbent workers, then examining the wage adjustments of job-stayers only will yield an incomplete representation of the degree of wage flexibility in the economy. Furthermore, in many theories of employment dynamics, the *present value* of worker earnings determine labor market fluctuations. Therefore, it is important to measure the cyclicality and persistence of each component of compensation separately. The most persistent and cyclical components of compensation will better match the allocative notion of a wage in models with wage rigidity.

The empirical demands for a complete study of wage rigidity are therefore great, which is a major reason why the literature measuring nominal wage adjustment in micro data has remained underdeveloped. Few datasets separately identify the various components

[1] See Christiano et al. (2005) and Smets and Wouters (2007) for classic references incorporating nominal wage rigidities into dynamic stochastic general equilibrium models of the aggregate economy. More recently, see Beraja et al. (2019).

[2] See, for example, Bils and Klenow (2004), Klenow and Kryvtsov (2008), and Nakamura and Steinsson (2008) for important contributions.

of compensation, which precludes determination of the relevant wage concept for rigidity. Moreover, existing data sets are ill-suited to measure micro-level nominal wage adjustment, which are the key moments to which rigidity parameters are calibrated. Household surveys often define the nominal wage by dividing self-reported earnings by self-reported hours.[3] Any measurement error in either earnings, hours worked, or self-reported hourly wages can result in a substantial upward bias in the volatility of individual wage changes. Administrative datasets, on the other hand, have high quality panel data on quarterly or annual earnings but usually lack the measures of individual hours worked necessary to construct a wage.

In this paper, we use administrative data from ADP, LLC – one of the world's largest payroll processing companies – to produce a series of new facts about aggregate nominal wage adjustment in the U.S. over the last decade. Our data set is unique in that it: (1) includes administrative anonymized records of workers' per-period nominal wage, (2) has a sample of about 20 million workers per month which are generally representative of the US population, (3) provides data for the universe of workers within a firm, (4) allows workers to be tracked both within and across firms over time, (5) includes administrative records on various other forms of compensation including bonuses and overtime, and (6) spans multiple years so as to examine business cycle variation. The data allow us to compute measures of nominal wage adjustments separately by compensation component, as well as for job-stayers, job-changers, and new hires. Collectively, our results paint a relatively complete picture of nominal wage adjustments for workers and firms in the U.S. from 2008 to 2016.

We begin the paper by describing the composition of worker compensation. We measure a worker's contract "base" wage as either their hourly wage (for workers paid hourly) or their per pay-period contracted compensation (for salaried workers), which represents their contractually obligated annual salary divided by the annual number of pay-periods during the year: that is, their contracted weekly, bi-weekly or monthly earnings. For most workers, "base earnings" comprise essentially all annual earnings. However, for some workers, bonuses, commissions, performance pay and overtime premiums are also important. For ten percent of non-commission workers, non-base pay accounts for at least 11% of annual earnings.

The second part of the paper argues that base wages are the component of earnings that matches the model-relevant outcome of wage rigidity: the cyclicality of marginal cost. We show that base wage changes are highly procyclical, while bonuses and overtime pay are

[3] Many papers measure the frequency of nominal wage adjustments using household data. See, for example, Kahn (1997), Barattieri et al. (2014), Daly and Hobijn (2014), and Elsby et al. (2016). Separately, Lebow et al. (2003) and Fallick et al. (2016) use data from the BLS's Employment Cost Index (ECI) to measure nominal wage rigidity. Unlike the household surveys, the unit of analysis in the ECI is a job not a worker. To the extent that workers who populate a specific job are heterogeneous with respect to underlying skills, nominal wage variation will occur if the quality of workers filling a job shifts over time.

roughly acyclical. This, coupled with the observation that base pay is far more persistent than bonus or overtime income, suggests that base wages are the most relevant component of compensation for standard models which use wage rigidities to generate sluggish cyclical movements in marginal cost. Our findings imply that bonuses and overtime are the labor market analogue to sales in the output pricing literature.

We show strong evidence of downward nominal base wage rigidity for a sample of workers who remain continuously employed with the same firm. We refer to this sample as our "job-stayer" sample. Nominal base wage cuts are exceedingly rare on-the-job. During our entire sample period, only 2.5% of all workers received a nominal base wage cut during a year. On average, about one-third of both hourly and salaried workers who remain on the job received no nominal wage adjustment during a given year. Therefore, about two-thirds of both hourly and salaried workers receive a positive nominal base wage increase during a given year. The patterns are similar for both high and low wage workers, for workers paid hourly and workers who are salaried, and regardless of whether or not workers receive an annual bonus or overtime compensation. Our results imply a duration of nominal base wages for the typical worker who remains continuously employed on the same job of about 6 quarters.

Next, we document exhibits strong evidence of both time and state dependence in nominal wage adjustment. Wage adjustment is periodic, with the majority of adjustments occurring 12 months after the last adjustment. Wage adjustment is synchronized within a firm, with most workers in a firm receiving wage adjustment in the same month. Finally, wage adjustment shows seasonal patterns at the monthly level, albeit with limited quarterly seasonality. These patterns reinforce theories of time dependent wage adjustment, such as the staggered contracting model of Taylor (1980). We also highlight evidence of state dependence. Although nominal base wage cuts are very rare for job-stayers over our entire sample period, 6 percent of workers received nominal base wage cuts during the Great Recession. We also document that upwards of 10 percent of workers in industries hit hardest during the Great Recession (including both manufacturing and construction) experienced nominal base wage declines. In addition, the share of workers who see any base wage changes falls during the Great Recession. These results suggest that any model with a constant fraction of wage adjustments will struggle to match the wage setting patterns over a severe business cycle.[4]

We then provide evidence that the base wages of new hires are no more flexible than the base wages of job-stayers. The literature has argued that the wages of new hires are allocative for employment decisions (Pissarides, 2009). In addition, many authors have shown that the wages of job-changers are more pro-cyclical than those of job-stayers (Bils,

[4]Preliminary estimates from Cajner et al. (2020) using the ADP data show that nearly 7 percent of workers received nominal base wage cuts during the first few months of the Pandemic Recession.

1985). We replicate this in our data and find that approximately 27% of job-changers receive a wage cut on job switch with almost everyone seeing a wage change. However, because the selection of workers who switch jobs is not constant throughout a recession, this result does not imply that firms may circumvent incumbent wage rigidity by hiring new workers.[5] Indeed, we show that much of the excess cyclicality of job-changers' wages vanishes once one controls for observable characteristics of job-switchers. To further account for this selection, we propose a matching estimator in which the evolution of new hires' wages is compared to that of a worker in the destination firm of the same age, sex, industry, and skill (as proxied by lagged wages conditional on tenure). The matching estimator reveals that new hires' wages evolve almost identically to the wages of their matched job-stayers over the business cycle, suggesting no excess wage rigidity for job-stayers relative to new hires. These results complement the findings in Gertler et al. (2016) and Hazell and Taska (2019) documenting that new wage hires are not more flexible than those of existing workers once properly controlling for selection.

Although the data show signs of downward nominal base wage rigidity, this does not imply that compensation for job-stayers does not fall. In the final section of the paper, we show that bonuses, overtime, commissions, and job-changing all lead to meaningful fluctuations in workers' total earnings per hour. Aggregating all of these margins of adjustment reveals that approximately 17 percent of workers saw declines in average hourly earnings in all periods of our sample including the 2009 recession. However, as highlighted earlier in the paper, bonuses and overtime are not important components for the cyclicality of firm marginal cost. Taking all of our results together, this paper argues that there is an important distinction between *compensation* flexibility – the fluctuations in workers' realized earnings – and *contract* rigidity – fluctuations in the terms governing a worker's employment contract, such as the base wage, and bonus and overtime schedules. Contract rigidity is allocative for employment decisions and thus is the notion considered in most models employing wage rigidity, while compensation flexibility determines realized aggregate hourly earnings dynamics for a given worker.[6]

2 Data and Variable Definitions

2.1 Overview of ADP Data

We use anonymized administrative individual panel data provided by ADP. ADP is a large, international provider of human resources services including payroll processing, benefits man-

[5] See Solon et al. (1994) for a discussion of selection at business cycle frequencies.

[6] In essence, contract rigidity determines the user cost of labor, which Kudlyak (2014) shows is the allocative cost of employment.

agement, tax services, and compliance. ADP has over 650,000 clients worldwide, and now covers payroll for over 20 million individual workers in the United States per month. The data to which we have access start in May 2008 and extends through December 2016.

The data contain monthly aggregates of anonymized individual paycheck information, as well as all relevant information needed for human resources management. Crucially, we observe, without measurement error, the statutory per-period contract rate for all employees. For hourly workers, this contract rate is simply the worker's hourly wage. For salaried workers, it constitutes the pay that the worker is contractually obligated to receive each pay period (weekly, bi-weekly, or monthly). Given the data is aggregated to the monthly level, the per-period contract rate is measured as of the last pay period of the month.

In addition to the administrative wage information, the data contain all other information that would appear on the worker's paycheck, such as the worker's gross earnings per pay period, taxes paid, and any taxable benefits provided by the firm. Additionally, the data contain other payroll information including whether the worker is paid hourly, the frequency at which the worker is paid and the number of hours paid during the month. For hourly workers, the exact number of hours paid is reported. Hours for salaried workers are often missing or are set to 40 hours per week in all periods. We also observe various additional geographic and demographic characteristics of a worker as well as details about the job, such as worker tenure, firm size, and industry. Selection into the ADP data is at the firm level. As a result, we can measure wage distributions within and across firms over time.[7] Finally, the presence of consistently-defined anonymized worker and firm identifiers permits the study of individual worker dynamics across ADP clients. Given our sample size, we have millions of individuals moving from one ADP firm to another ADP firm.[8]

We make two major sample restrictions for our analysis. First, we restrict attention to prime age workers between 21 and 60 years old, inclusive. Second, ADP has two products separately targeted to firms with greater than or less than 50 employees. We only have access to data for firms with more than 50 employees throughout our sample period, and thus constrain our analysis to mid-size and larger employers.

The full dataset covers over 50 million unique individuals and over 141 thousand firms. To reduce computational burden, we create two subsamples of the full data. The first chooses one million unique employees, and follows them through their entire tenure in the sample

[7]Strictly speaking, our definition of a firm is an ADP-provided client code. This will usually be an autonomous firm, rather than any individual establishment. One possible exception to this rule arises if large conglomerates have multiple subsidiaries, all of which separately hire ADP to handle their payroll. In this case, each subsidiary would count as a separate ADP client. As a result, our ADP firms are a combination of both Census notions of firms and establishments.

[8]All worker and firm identifiers in the ADP data are unique and consistently defined over time. However, they are constructed in a way that all workers and firms remain anonymous for research purposes.

across all firms for which they work. This is the primary dataset for analysis. Second, we separately draw all instances in which a worker switches jobs during our sample period ("job-changers"). These are workers who show up in multiple firms during their time in the ADP database. This allows us to explore the patterns of wage changes for new hires. However, these two datasets are ill-suited to study questions at the firm level; we therefore construct a third subsample of three thousand unique ADP clients, drawing all workers from those firms in the process. The random employee and firm-level subsamples remain large, with roughly 22 million and 68 million unique employee-month observations, respectively. A full discussion of the construction of our samples and variable definitions can be found in the online appendix.

2.2 Representativeness of ADP Data

Given this is the first paper using the ADP data, a deeper discussion of the representativeness of the ADP sample is warranted. We have relegated much of this discussion to the online appendix. In particular, we compare the firm size distribution in the ADP data to that of the Census' Business Dynamic Statistics (BDS) during similar years. Conditional on having at least 50 employees, we find that the ADP sample under-represents firms with at least 5,000 employees as these firms are more likely to process their payroll in house. To account for the concern that the data do not perfectly represent the universe of all U.S. firms with at least 50 employees, all subsequent analyses in the paper have been weighted so as to match the BDS's firm size by industry mix of employment shares for firms with greater than 50 employees. We compute our weights separately for each year between 2008 and 2016. By re-weighting the data, we control for potential sample selection along these key observable dimensions. Given that patterns of wage adjustments do not differ markedly by firm size, our weighted results (shown in the paper) and our unweighted results (shown in the appendix) are nearly identical to each other.

The online appendix also explores the representativeness of our sample along demographic dimensions (by comparing to the Current Population Survey: the "CPS") and with respect to earnings dynamics (by comparing to results in Guvenen et al. (2014) using the universe of social security records.). We also explore the robustness of our results using a separate sample available from ADP that contains payroll information for firms with less than 50 employees. This dataset starts in 2013. After performing all of these benchmarking and robustness exercises, we are confident that the ADP data provides a representative picture of nominal wage adjustments for U.S. workers over the 2008 to 2016 period.

3 The Nature of US Worker Compensation

The ADP data include many detailed administratively recorded measures of worker compensation. In this section, we describe the composition of worker compensation.

3.1 Base Wages and Base Earnings

Employers participating in the ADP payroll services are required to report the contractually obligated per-period wage rate for each worker. This field is filled in by a manager within the firm or one of the firm's human resources (HR) representatives. We refer to a worker's per-period contract wage as their "base" wage. For workers who are paid hourly, this is the worker's hourly wage. In most firms, hourly workers manually enter the hours worked each day. Some firms use a virtual time clock within the ADP software where hourly workers punch-in when they start working and punch-out when they stop working. Other hourly workers have a default number of hours paid each day and can manually enter an override if they work more or less than the default. All hours worked entered by the employees are then approved by a manager or HR representative. If more than forty hours are worked during the week, the hourly workers accrue overtime compensation at an overtime premium. Functionally, the per pay-period base earnings in hourly workers' paychecks are the product of the administratively-recorded number of hours worked and the administratively-recorded hourly wage.

All salaried workers have an administrative field recording their contracted salary rate paid weekly, bi-weekly or monthly. Again, the annual contracted base compensation for salaried workers is an administrative field entered by a manager within the firm or one of the firm's HR representatives. The per-period base wage is their annual contracted base earnings divided by the number of yearly pay-periods. As highlighted below, this per-period base wage is the primary component of the worker's compensation during the pay period.

In addition to a worker's base contact wage, the data also contain separate administrative records of a workers' "monthly gross earnings." A worker's base pay is only one part of their monthly gross earnings. During a given month, a worker may also receive tips, commissions, overtime payments, performance pay, bonuses, cashed-out vacation days and meal and travel reimbursements.[9] Monthly gross earnings is literally the sum of all paychecks (before taxes) earned by the worker during the month. Any additional compensation such as bonuses, tips, commissions, or performance pay are manually entered by a worker's manager or HR

[9]The fact that meal and travel reimbursements can show up in workers' paychecks implies that there is not a one-to-one mapping between monthly gross earnings in our dataset and monthly W-2 earnings. We do not have a worker's W-2 earnings in the data provided to us by ADP.

representative during the pay period.

To isolate the importance of base wages in worker earnings, we define the concept of a worker's "monthly base earnings" using information on their base wage. If the worker is an hourly worker, their monthly base earnings is their base wage times the total number of hours worked during the month. If the worker is a salaried worker, their monthly base earnings is their base per-period payment rate times the number of pay periods during the month. Any difference between a worker's monthly gross earnings and their monthly base earnings is the result of the worker earning some combination of bonuses, tips, overtime, reimbursements or other non-standard payments during the month.

Such non-standard payments are not likely to accrue every month for a given worker. To see how important these sources are for a typical worker at an annual frequency, we also aggregate our data to calendar years. When doing so, we restrict our analysis to workers who remain continuously employed with the same firm for all twelve calendar months of a given year. We refer to this sample as our "full-year" employee sample.

3.2 Measuring Overtime, Bonuses, and Commissions

Ideally, one would decompose workers' "residual earnings" – gross earnings less base earnings – into various sub-components. However, such disaggregation is challenging in the ADP data. Firms are not required to separately report the different subcomponents that comprise residual earnings. Despite the limitation, we make four refinements to our residual earnings measures in order to identify worker overtime, commissions, and bonuses. First, we impute the amount of monthly overtime premiums paid to hourly workers. For workers paid hourly, firms report total hours worked (inclusive of overtime) and total earnings from hourly work (inclusive of overtime). Some firms additionally report total *non-base* hours and earnings related to hours. Comparing these measures to our measures of base earnings allows for an imputation of overtime hours worked. Reassuringly, we find that about 80% of the individual-month observations with imputed monthly overtime hours had an overtime wage equal to exactly 1.5 times their contract wage. This suggests that our imputation procedure is detecting overtime hours fairly well for hourly workers.[10] Additionally, given data limitations on actual reported hours, we cannot measure overtime earnings for salaried workers. Such earnings, to the extent that they occur, will be lumped in with our measure of bonuses for these workers which we discuss next.

[10]There may be measurement error in our imputation procedure if reported earnings also include other forms of compensation like bonuses, tips and meal reimbursements. To minimize the effects of such measurement error in our imputation, we only classify overtime compensation for hourly workers in the case where their residual earnings imply an imputed overtime premium of between 1.45 and 1.55 times their contract wage. We discuss our imputation procedure in greater detail in the online appendix.

Second, we define large residual earnings to be any residual earnings *net of overtime premiums* that accrue to a worker in a given month and that exceed 1% of their annual earnings. For example, if a worker earned $50,000 during a given calendar year, we would classify that worker as having large residual earnings during a given month if residual earnings net of overtime pay exceeded $500 during that month. By making this restriction, we exclude any small payments made to the worker during a given month such as small meal reimbursements or small measurement error in our overtime imputation.

Third, we compute the number of months a worker receives large residual payments during a given year. We define a worker to be a "commission worker" if they receive large residual earnings net of overtime in four or more calendar months during a given year. We are interpreting "commission workers" broadly in that these workers could have large residual payments in four or more months during a given year due to sufficiently frequent commissions, tips, performance pay, mis-measured overtime pay, or even large meal and travel reimbursements. We can then segment workers into "non-commission workers" and "commission workers." According to this definition, roughly 14 percent of workers each year during our sample can be classified as commission workers.

Finally, for non-commission workers, we define a worker as having received a "bonus" if that worker received a large residual earnings payment net of overtime in at least one month but no more than three months during a given calendar year. Again, our definition of bonus is broad in that it applies to any large infrequent extra payments received by workers during a year. This can include bonuses but it also could include infrequent meal and travel reimbursements as well as unmeasured infrequent overtime compensation for salaried workers. During our sample period and given our definition, roughly 49 percent of non-commission workers receive a "bonus" during a given year. Most of these extra infrequent payments accrue to workers in December, February and March suggesting that many of them are likely linked to annual bonuses.

3.3 The Composition of Worker Compensation

Table 1 shows the importance of annual base pay, bonuses and overtime as a share of annual earnings for non-commission workers using our full-year employee sample. The first two columns show results pooling together hourly and salaried workers. Column 1 shows the share of total annual earnings that comes from base pay. About one-quarter of all workers receive essentially all of their annual compensation from base earnings. The median worker earns only 2.7 percent of their annual earnings from non-base pay sources. This suggests that for most workers, base pay is their primary form of compensation. However, for some

Table 1: Share of Annual Base Earnings, Overtime and Bonuses out of Annual Gross Earnings, 2009-2016, Non-Commission Workers

	All		Hourly			Salaried	
	Share Base	Share Base+	Share Base	Share Base+	Share Base+	Share Base	Share Base+
10th percentile	88.9%	97.6%	90.4%	93.6%	97.2%	86.7%	98.5%
25th percentile	93.3%	99.2%	94.2%	96.4%	99.1%	91.7%	99.5%
Median	97.3%	100%	97.6%	98.8%	99.9%	96.6%	100%
75th percentile	99.7%	100%	99.6%	99.9%	100%	100%	100%
90th percentile	100%	100%	100%	100%	100%	100%	100%
Base Wages Included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bonuses Included		Yes		Yes	Yes		Yes
Overtime Included		Yes			Yes		
Sample Size (1,000s)	624	624	397	397	397	216	216

Notes: Table shows the distribution of the share of workers' annual base earnings and then annual base earnings plus annual bonuses and overtime earnings out of annual worker gross earnings for all workers, workers paid hourly and workers who are salaried (for the full year). We restrict attention to our sample of non-commission workers who remain continuously employed with the same firm for all twelve months of a calendar year.

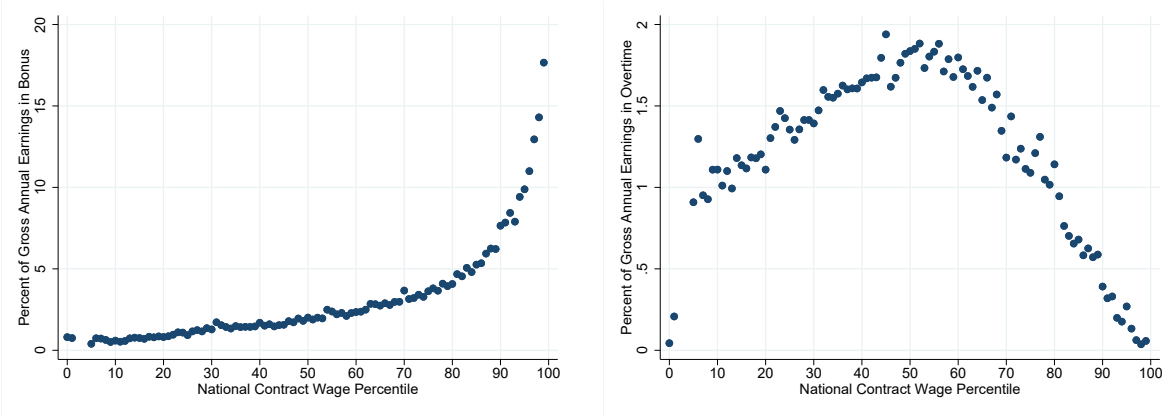
workers, other forms of compensation (e.g., bonuses, commissions, tips, overtime) comprise a more substantive portion of their annual earnings. Ten percent of non-commission workers earn at least 11 percent of their annual earnings from sources other than their base pay.

Additionally, nearly all remaining compensation for non-commission workers is in what we classify as either bonuses or overtime. Specifically, for the median non-commission worker, all of annual gross earnings are base earnings, overtime, or bonuses. This is important in that for the rest of the paper we are going to focus on these components of compensation. The remaining unexplained earnings in column 2 stem from the small amounts of monthly residual earnings that we do not classify either as bonuses or overtime, such as meal reimbursements. The remaining columns of the table show patterns separately for hourly and salaried workers. Bonuses are more important for salaried workers relative to workers paid hourly. However, overtime earnings are more important for workers paid hourly.

3.4 Heterogeneity in Bonuses and Overtime Across Workers

Panel A of Figure 1 shows the share of annual bonus income out of annual total earnings sorted by workers' base wage percentile. We pool together workers who are paid hourly

Figure 1: Bonus and Overtime Share by Employee Base Wage Percentile, Full-Year Job-Stayers



Panel A: Bonus Share
All Workers

Panel B: Overtime Share
Hourly Workers

Notes: Figure shows the share of bonus earnings out of total earnings (Panel A) and share of overtime earnings out of total earnings (Panel B) by worker percentile within the base earnings distribution. Sample restricted to job-stayers who remain in the sample for a full calendar year. Panel A includes all workers while Panel B includes only workers paid hourly.

and salaried.[11] Workers at the lower end of the base wage distribution receive, on average, only less than 0.5% of their annual earnings in bonuses. The share of earnings in bonuses increases monotonically throughout the wage distribution. The median worker earns about 2% of their annual earnings in bonuses. Systematically, workers at the top of the wage distribution earn a substantial amount of their annual earnings in bonuses. Therefore, while annual bonuses are not an important form of compensation for most workers, they can be substantial for high wage workers.

During a given month, only 30% of hourly employees accrue overtime hours, while 62 percent of hourly workers who are continuously employed by the same firm for 12 consecutive calendar months work overtime at any point during the year. Panel B of Figure 1 shows the share of annual overtime income out of annual total earnings sorted by workers' base wage percentile. For this panel we again exclude commission workers and restrict our analysis to hourly workers. Low wage workers who are paid hourly receive very little of their total compensation in overtime. Many of these workers work much less than 40 hours a week and are thus far from the overtime threshold. As base wages increase, the share of overtime earnings increases up through around the 50th percentile. For the median percentile of the

[11] To make the base wage percentile, we combine data on both hourly and salaried workers. For hourly workers, we use their base hourly wage. For salaried workers, to put things in the same hourly wage units, we divided their base weekly earnings by 40 hours.

base wage distribution for hourly workers, only 2% of annual earnings comes from overtime. After the 50th percentile, the share of earnings in overtime falls monotonically.[12]

4 The Cyclicality of Different Wage Concepts

Wage rigidity is commonly employed in modern macro models in order to generate muted cyclical movements in firms' marginal cost.[13] When firms' nominal marginal cost does not move one-for-one with their nominal productivity, then short run nominal shocks have allocative consequences for employment and output. Studies of rigid wages therefore should seek to measure the rigidity of compensation components which have the biggest impact on the cyclicality of marginal cost. We have shown above that base wages account for the largest share of workers' earnings; however, if base wages are fixed with the cycle while overtime pay and bonuses allow firms to adjust the cost of production, then base wages may be the incorrect notion of a wage for calibrating wage rigidity parameters. Similarly, if overtime and bonus pay do not systematically vary at the cyclical level, then base wages may be the relevant determinant of the cyclical stickiness of firms' marginal cost, and thus are the relevant notion for wage rigidity. In this section, we study the cyclical patterns of each compensation component separately in order to focus out study micro-level wage adjustments.

Before doing so, it is worth remarking on the relationship of each component to the economic costs of production. First consider hourly workers. For these workers, base wages are generally considered a pure marginal cost: in order to receive one more hour of labor, the firm must pay its employee the base hourly wage. Overtime is different. If the firm wishes to hire the worker for one more hour, the marginal cost of doing so is the base wage below the 40 hour mark, and one plus the overtime premium times the base wage above the 40 hour mark. Since almost every worker has a 50 percent overtime premium, this implies that the only time at which overtime pay changes the marginal cost of an employee is when the employee moves from having no overtime to having a positive amount of overtime: moving from 40 to 41 hours in the above example. After that point, overtime does not additionally change change the marginal cost of the worker's labor, but does affect the average cost of employment, which may be important for models in which, for example, financial frictions

[12] In the appendix, we explore the heterogeneity in overtime hours worked more extensively. About one-quarter of hourly workers with positive overtime hours work only between 1 and 5 hours of overtime during the month and just under half work less than 10 overtime hours. However, there is a long right tail of overtime hours with about twenty percent of workers with positive overtime hours accruing over 40 additional overtime hours during the month.

[13] For example, most DSGE models include some form of wage rigidity. For classic references, see Christiano et al. (2005) and Smets and Wouters (2007). Likewise, some models of frictional labor markets explicitly include wage rigidity. See, Hall (2005) and Shimer (2005).

render the average cost of employment allocative (Schoefer, 2016).

For salaried workers, the base wage is less clearly a notion of marginal cost. However, since the firm could adjust the number of salaried workers on the extensive margin, the marginal cost of employing a given worker is indeed the contracted base wage. In addition, salaried workers may often be compensated in part through incentive schemes, such as bonuses. In standard optimal contracting problems, a bonus rewards high output levels in order to incentivize effort which is productive but unobserved by the firm. The ex ante marginal cost to a firm of hiring additional effort is therefore the *expected* bonus paid to solicit that higher effort level. The realization of the bonus generates fluctuations in compensation without necessarily affecting the marginal cost of an unobserved labor input. This is the sense in which there is a difference between compensation flexibility – changes in realizations of a bonus payout – and contract flexibility – changes in the employment terms mapping output to a bonus payout. For this reason, bonuses may only be considered important for wage rigidity if their *expectation* varies at a cyclical level.

To test whether the expectation of the compensation components varies at the cyclical level, we separately measure the cyclicality of each form of compensation for our sample of job-stayers. Since our sample period contains only one aggregate recession, it is hard to tease out cyclical movements in different forms of compensation from our short time series. To overcome this problem, we exploit both cross-state and time series variation in business cycle conditions during our sample period by estimating the following regressions:

$$y_{ijst} = \alpha + \beta \Delta U_{st} + \gamma_i + \gamma_s + \gamma_j + \Gamma X_{it} + \epsilon_{ijst} \tag{1}$$

where y_{ijst} is various components of compensation for individual i who works in industry j in state s during year t. All regressions include state fixed effects (γ_s) and industry fixed effects (γ_j).[14] Some measures of y_{ijst} are within individual changes over time (e.g., the annual percentage change in base wages) while other measures are contemporaneous variables (e.g., whether the individual received overtime payments during the year). When we measure within individual changes over time, we omit individual fixed effects from the regression. However, when we use contemporaneous measures as our dependent variables, we show results both with and without individual fixed effects γ_i. Our key coefficient of interest is β which measures how the various components of compensation respond to changes in the state unemployment rate between period $t-1$ and t (ΔU_{st}). The regressions also include a vector of additional individual controls, X_{it}, that can vary over time, such as the individual's

[14]Excluding state fixed effects does not meaningfully alter the results in any regressions included in this paper, but their omission occasionally inflates standard errors. Using aggregate unemployment rates instead of state-level unemployment rates similarly has limited influence on the results of these regressions.

age, sex, tenure, whether they work hourly, their payment frequency and firm size.

Panel A of Table 2 documents the cyclicality of various individual compensation components. In the first column of Panel A, we restrict our sample to those individuals who continuously work at the same firm between December of year $t-1$ and December of year t (13 consecutive months). In this column, y_{ijst} measures the percentage change in the base wage of individual i working in industry j in state s between December of year $t-1$ and December of year t. As seen from Panel A column 1, individuals who reside in a state whose unemployment rate increased by one percentage point experienced nominal base wage growth that was 0.34 percent lower than average. The average unemployment rate increased by about 5 percentage points during the Great Recession. Therefore, our estimates imply that wage growth was about 1.7 percent lower during the Great Recession than during non-recessionary times. Given the mean annual wage growth in our sample was 3.9%, the response of wage growth to changes in the unemployment rate is quantitatively large.

Columns 2 and 3 of Table 2 show estimates of the cyclicality of overtime hours without and with individual fixed effects. For these regressions, we restrict our sample to those individuals who are paid hourly and who remain consistently employed with the same employer for 12 consecutive calendar months. Columns 2 and 3 measures y_{ijst} as an indicator equal to 100 if the worker received overtime payments at any point during the calendar year. Excluding individual fixed effects, we find that the propensity to receive overtime is procyclical. A one percentage point increase in the unemployment rate is found to reduce the propensity to receive overtime benefits by 0.6 percentage points for hourly workers. This is consistent with data from the US Bureau of Labor Statistics (BLS) on overtime hours within the manufacturing sector. The BLS records the average weekly overtime hours for production and non-supervisory employees in the manufacturing sector going back to 1956. Regressing average weekly overtime hours from the BLS data during year t on the change in the aggregate unemployment rate between years $t-1$ and t from 1956 to 2018 yields a coefficient on the change in unemployment rate of -0.34 (with a standard error of 0.02). In the online appendix, we show the overtime receipt falls when unemployment increases in both the manufacturing and non-manufacturing sectors when individual fixed effects are excluded.

Column 3 shows that the changing composition of the workforce over the business cycle contributes significantly to the measured cyclicality of worker overtime. Once we condition on individual fixed effects, we find that the propensity to receive overtime is much less cyclical. With individual worker fixed effects, a one percentage point increase in the unemployment rate reduces the propensity to receive overtime benefits by only 0.27 percentage points. To put this number in perspective, over the whole sample period, 62.5 percent of hourly workers receive positive overtime hours during the year. As highlighted in the appendix, overtime

Table 2: Cyclicality of Various Forms of Compensation

Panel A: Compensation Components					
Dependent Variable:	% Base Wage Change	% With Overtime	% With Overtime	% With Bonus	% With Bonus
	(1)	(2)	(3)	(4)	(5)
Δ Unemployment Rate (%)	-0.34	-0.60	-0.27	-0.33	-0.55
	(0.02)	(0.25)	(0.10)	(0.11)	(0.11)
Workers Included	All	Hourly	Hourly	All	All
State and Industry FE	Y	Y	Y	Y	Y
Individual FE	N	N	Y	N	Y
Observations (1,000s)	619	415	415	673	673
Mean of Dep. Var.	3.82	62.5	62.5	49.2	49.2

Panel B: Annual % Change in Compensation					
Dependent Variable:	Base Earnings	Base Earnings	Base Earnings	Base Plus Overtime	Base Plus Bonus
	(1)	(2)	(3)	(4)	(5)
Δ Unemployment Rate (%)	-0.41	-0.46	-0.39	-0.37	-0.37
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Workers Included	All	Salaried	Hourly	Hourly	All
State and Industry FE	Y	Y	Y	Y	Y
Individual FE	N	N	N	N	N
Observations (1,000s)	321	104	217	217	320
Mean of Dep. Var.	3.83	4.29	3.63	3.69	4.37

Notes: Table reports coefficients estimated from equation 1 with OLS. Standard errors clustered at the state-year level are reported in parentheses. All regressions control for state, industry, and firm size fixed effects, as well as linear controls for tenure, age, and payment frequency and type. Independent variable is the change in the state unemployment rate between period $t-12$ and t. Sample restricted to non-commission full-year job-stayers in Panel A, and two-year job-stayers in Panel B.

hours remain procyclical in the manufacturing sector even after controlling for individual worker fixed effects. However, the cyclicality of overtime hours in the non-manufacturing sectors completely disappears when worker fixed effects are included. These results suggest that workers in non-manufacturing sectors that receive overtime pay in non-recessionary times are more likely to be displaced during recessions. Given that the manufacturing sector represents only a small portion of aggregate employment, the acyclicality of overtime in the non-manufacturing sector generates the aggregate acyclicality of overtime.[15]

[15] In the online appendix, We also document that log overtime hours (conditional on overtime hours being positive) do not vary with changes in state unemployment rate in both regressions with and without state

Columns 4 and 5 of Panel A of Table 2 examine the cyclicality of bonus receipt. These regressions restrict the sample to hourly and salaried workers who remained consistently employed with the same employer for 12 consecutive calendar months. Columns 4 and 5 measures the percent of workers who received a bonus at any point during the calendar without and with individual fixed effects, respectively. Unlike with overtime hours, the inclusion of individual worker fixed effects has only a modest effect on the cyclicality of bonuses. With individual fixed effects, bonus receipt is slightly procyclical with a one percentage point increase in the unemployment rate decreasing the fraction of workers receiving a bonus by 0.55 percentage points. Given the 5 percentage point increase in the unemployment rate during the Great Recession, these results would imply a fall in the propensity to receive a bonus by 2.75 percentage points. This effect is relatively small, since 49 percent of workers receive an annual bonus during our entire sample. To some degree, these results are unsurprising. Many optimal contracting models suggest that incentive pay, such as bonuses, should filter out fluctuations in performance driven by factors outside of the control of the agent, such as the aggregate state of the economy (Lazear and Rosen, 1981).[16]

To assess whether accounting for overtime payments and bonuses changes the cyclicality of individual compensation quantitatively, we report the cyclicality of base wages augmented to include overtime and bonuses in Panel B of Table 2. To assess how the cyclicality of overtime and bonuses affects the cyclicality of the marginal cost of labor for continuing workers, our bonus, overtime and base wage measures must be in the same units given that bonuses often accrue annually and overtime hours are intermittent throughout the year. Given the infrequency of bonuses and overtime, we restrict our sample in Panel B to individuals who remain continuously employed with the same employer over two consecutive calendar years so we can examine calendar year over calendar year changes. This restriction necessarily implies smaller sample sizes than the samples used Panel A.

To measure the cyclicality of compensation inclusive of overtime and bonuses, we compute broader measures of compensation per hour for both hourly and salaried workers. Columns 1-3 measure the cyclicality of annual base earnings per hour for all workers, salaried workers and hourly workers, respectively. Base earnings per hour for hourly workers is simply their base hourly wage. To make base earnings per hour for salaried workers, we assume workers work 40 hours per week and 52 weeks per year. Given this adjustment for salaried workers is constant across years, the growth rate in base earnings per hour for salaried workers is completely determined by the growth in contract earnings. The differences between the results in column

fixed effects.

[16]The fact that many firms continued to give large bonuses during the Great Recession was covered frequently in the popular press. See, for example, "Bankers Reaped Lavish Bonuses During Bailouts" in the *New York Times*, July 30th 2009.

1 of Panel B those in column 1 of Panel A stem from both the differences in samples between the two panels and from time aggregation. Panel B conditions on workers who remain with the firm for 24 consecutive months. These workers are likely to be more positively selected than those who remain with the firm for one year. In addition, Panel B measures year-over-year wage changes. For some of these workers, they will have received multiple wage changes during the 24 month period potentially making their wages more cyclical. For workers who remain consistently employed for two consecutive calendar years, a one percentage point increase in the unemployment rate was associated with a 0.41 percentage point decline in the growth rate of annual base earnings per hour. Columns 2 and 3 of Panel B show the results separately for hourly and salaried workers. The semi-elasticity is roughly similar for salaried and hourly workers (-0.46 vs. -0.39). In all cases, base wages are notably procyclical.

In columns 4 and 5 we make broader measures of worker per hour compensation including overtime and bonuses. In column 4, we sum annual base earnings and annual overtime earnings for hourly workers, and divide by annual hours paid. We then take the annual growth rate in per hour base plus overtime compensation as our measure of y_{ijst}. In column 5, we add annual base earnings to overtime and bonuses for all workers, and divide that by annual hours paid. The goal is to see if per hour compensation becomes more cyclical when including overtime and bonuses. As seen from columns 4 and 5, the cyclicality of per hour compensation is relatively unchanged when including bonuses and overtime. If anything, including overtime and bonuses slightly dampens the cyclicality of base wages. These results stem from the fact that both overtime and bonus compensation are quantitatively small for most workers and that these sources of compensation are mostly acyclical.[17]

Collectively, the results in Table 2 suggest that the cyclicality of base wages drives the cyclicality of the marginal cost of labor for job-stayers. An analogue to the base wage versus bonuses and overtime distinction can be drawn from the output price setting literature. Eichenbaum et al. (2011) show that nominal rigidities take the form of inertia in reference prices and costs. As a result, short-term sales of output prices may be ignored for assessing the impact of nominal rigidities on aggregate fluctuations. Indeed, much of the empirical literature has excluded sales from their measure of price adjustments (Nakamura and Steinsson, 2008) or directly incorporated a differential adjustment cost for sales relative to reference prices (Kehoe and Midrigan, 2008; Midrigan, 2011). Given the lack of cyclicality of bonuses and overtime hours, it is natural to liken them to "sales" in the pricing literature when using micro data to discipline nominal wage rigidities.

[17]It should be noted that our estimates of the cyclicality of nominal wages for job-stayers (in a low-inflation environment) is similar to the estimates of real wage cyclicality for job-stayers documented in Bils (1985) and Devereux (2001) using micro data from household surveys. These papers, however, are not able to distinguish between the cyclicality of different components of compensation.

As noted above, it is often the user cost of labor which is allocative for employment decisions. Transient components of compensation are therefore broadly irrelevant for standard models of employment dynamics. In the online appendix, we show that base wage changes are highly persistent. We also show that while the propensity to receive a bonus or overtime payments at the individual level is highly persistent, the amount of bonus or overtime payment received conditional on receipt is approximately *i.i.d.* through time. Given the transient and acyclical nature of overtime and bonuses, we conclude that base wages almost entirely determine the cyclical movements in firms' marginal cost of labor. Models incorporating wage rigidity to mute cyclical fluctuations in the marginal cost of labor should therefore match movements in base wages, rather than earnings per hour. In the next three sections, we therefore use our micro data to explore the adjustment patterns of nominal base wages, starting with an examination of the wage dynamics for job-stayers.

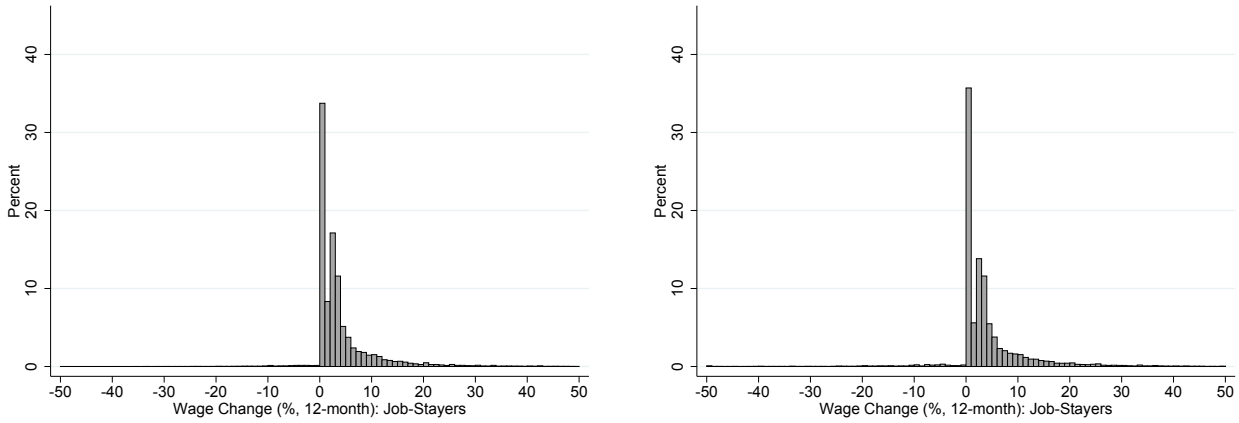
5 Nominal Base Wage Adjustments for Job-Stayers

This section explores the nature of nominal base wage adjustments for workers who remain continuously employed in the same job at the monthly, quarterly and annual frequencies. For the monthly, quarterly and annual samples, we ensure that workers are continuously employed with the same firm for one, three and twelve consecutive months, respectively.

Figure 2 plots the distribution of 12-month nominal base wage changes for all job-stayers pooled over all years of our sample. Panel A plots the distribution for hourly workers, while Panel B plots the distribution for salaried workers. Three key observations are apparent from the figure. First, a large share of workers - 32% of hourly and 35% of salaried - do not receive a nominal base wage change in a given year. Second, the patterns of nominal base wage adjustments for hourly workers and salaried workers are nearly identical. Given this, we often pool the data for hourly and salaried workers together going forward when describing base wage adjustments. Third, there is a clear asymmetry in the base wage change distribution, with the overwhelming majority of changes being wage increases. Only 2.5 percent of workers (combining hourly and salaried) in the U.S. who remained continuously employed with the same firm for 12 months received a nominal base wage decline. Of the roughly 66% of all individuals who receive a nominal base wage change over a given 12-month period, only 3.8% received a nominal base wage cut (2.5/66).

Table 3 provides a set of summary moments on the probability of base wage increases and base wage declines for three frequencies: monthly, quarterly and annual. The annual frequencies correspond to the data underlying Figure 2. While the asymmetry between nominal wage increases and nominal wage cuts is a feature of many existing empirical papers

Figure 2: 12-Month Nominal Base Wage Change Distribution, Job-Stayers



Panel A: Hourly Workers

Panel B: Salaried Workers

Notes: Figure shows the annual change in nominal base wages for workers in our employee sample (including commission workers) who remain employed on the same job for 12 consecutive months.

(see, e.g. Lebow et al. (2003); Kahn (1997); Card and Hyslop (1997)), the results in Figure 2 are quantitatively different from much of the existing literature. The 18.5% quarterly adjustment probability implies a mean duration of wage contracts of approximately 6 quarters if one were to assume a constant hazard of adjustment as in Calvo (1983).

The patterns of nominal wage adjustments for job-stayers are fairly robust across workers who are compensated in different ways. In the online appendix, we show that the patterns in Figure 2 are nearly identical if we restrict our sample to only non-commission workers, only commission workers, only non-commission workers who receive a bonus and only non-commission workers who do not receive a bonus. These findings suggest that base wage adjustments do not differ across workers who receive other types of compensation. Additionally, we show that the patterns of base wage adjustment are nearly identical for those workers who are paid hourly and who have substantive movements in monthly reported hours worked throughout the year. Even for workers whose hours appear allocative, there are essentially no nominal base wage cuts and roughly one-third of workers do not receive a year-over-year nominal base wage increase.

Table 4 shows additional moments of the base wage change distribution. For this table, we only report results pooling both hourly and salaried workers given the frequency of adjustment distributions were similar between the two groups.[18] During this period, mean

[18] To limit the effect of extreme outliers when computing mean wage changes, we winsorize both the top and bottom 1% of nominal wages and the top and bottom 1% of wage changes. We only do this when computing the size of wage changes conditional on a wage change occurring. This does not affect our frequency of wage

Table 3: Probability of Base Wage Change, Pooled 2008-2016 Sample of Job-Stayers

	All Workers (1)	Hourly (2)	Salaried (3)
Annual			
Probability of Positive Base Wage Change (%)	64.3	66.0	61.7
Probability of Negative Base Wage Change (%)	2.5	1.7	3.6
Quarterly			
Probability of Positive Base Wage Change (%)	18.5	19.6	16.8
Probability of Negative Base Wage Change (%)	0.9	0.7	1.2
Monthly			
Probability of Positive Base Wage Change (%)	6.3	6.6	5.9
Probability of Negative Base Wage Change (%)	0.4	0.3	0.6

Notes: Table shows the frequency of base wage increases and base wage decreases at different horizons for our sample of job-stayers during the 2008-2016 period. The first column pools together hourly and salaried workers while the second and third columns, respectively, show the frequency of changes for hourly and salaried workers separately. The top panel shows results at the annual horizon while the middle and bottom panels show results at the quarterly and monthly horizons. We use our full employee sample for this analysis.

Table 4: Base Wage Change Statistics, Pooled 2008-2016 Sample of Job-Stayers

	Monthly	Quarterly	Annual
Mean Unconditional Change (%)	0.4	1.0	3.9
Median Unconditional Change (%)	0.0	0.0	2.4
Standard Deviation of Unconditional Change (%)	2.4	3.5	6.3
Mean Conditional Change (%)	5.2	5.1	5.8
Median Conditional Change (%)	3.1	3.1	3.4
Standard Deviation of Conditional Change (%)	7.9	6.4	7.0
Mean Change, Conditional on Positive (%)	6.0	5.6	6.2
Median Change, Conditional on Positive (%)	3.3	3.3	3.5
Stan. Dev. Change, Conditional on Positive (%)	7.2	6.0	6.7
Mean Change, Conditional on Negative (%)	-9.6	-8.1	-7.0
Median Change, Conditional on Negative (%)	-7.7	-7.7	-6.3
Stan. Dev. Change, Conditional on Negative (%)	6.8	5.2	4.3

Notes: Table shows moments of the wage change distribution for different horizons for a sample of job-stayers in the ADP data between 2008 and 2016. For this table, we use our employee sample and pool together hourly and salaried workers. "Conditional changes" in rows 4-6 are conditional on a base wage occurring. Rows 7-9 and 10-12 condition on positive and negative wage changes, respectively.

and median nominal annual base wage growth for workers who remain on the same job equaled 3.9 percent and 2.4 percent, respectively.[19] Conditional on a base wage change occurring, annual mean and median base nominal wage growth was 5.8 and 3.4 percent. Unconditionally and conditional on a base wage change occurring, the standard deviation of annual nominal base wage growth during the full 2008-2016 period was 6.3 percent and 7.0 percent, respectively. Additionally, conditional on a positive base wage change occurring during a 12-month period, the mean and median size of the increase was 6.2 and 3.5 percent. The fact that the mean is much higher than the median reinforces the fact that some workers receive very large nominal base wage changes on the job, perhaps due to promotions. The mean and median size of a base wage cut, conditional on the worker experiencing a nominal base wage reduction, were both around 6-7 percent. While the frequency of base wage increases is much higher than of wage cuts, the mean size of a base wage increase is similar to the mean size of a cut.

6 Time and State Dependence in Nominal Wage Adjustment

We next provide evidence for both time and state dependence for our sample of job-stayers.

6.1 Time Dependence in Nominal Wage Adjustments

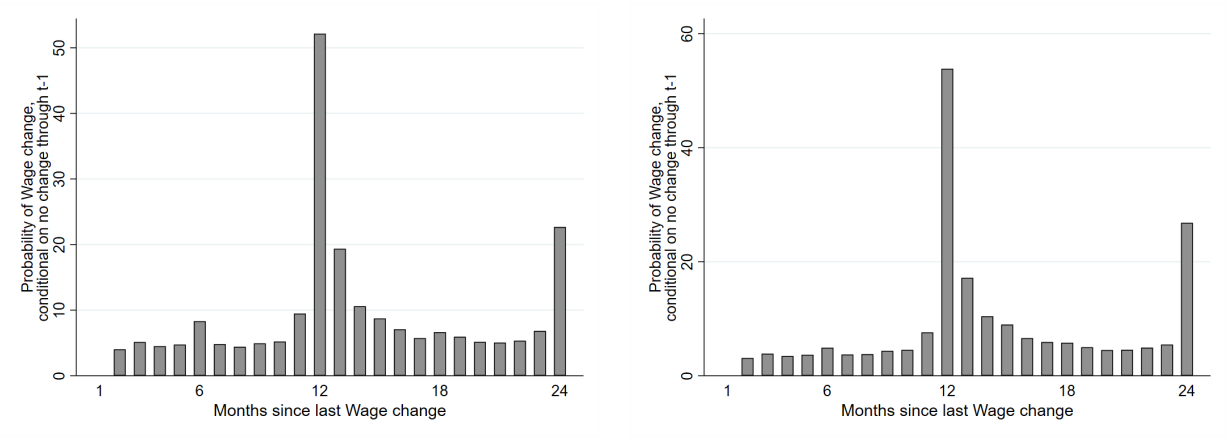
Many modern macro models assume some time dependence in wage setting. For example, Taylor (1979, 1980) emphasizes that staggered, periodically-adjusted wage contracts can amplify business cycle persistence in response to aggregate shocks. New Keynesian macro models in the spirit of Christiano et al. (2005) use a Calvo (1983) model of wage setting in which workers randomly have the opportunity to have wage adjustments. In this subsection, we use our detailed micro data to explore evidence of time dependence in wage adjustment for our sample of job-stayers.

To study time dependence in wage setting, Figure 3 plots the hazard function of base wage changes over our sample period. Specifically, the figure shows the probability of a one month base wage change between months $t-1$ and t conditional on a worker surviving to month $t-1$ without a base wage change at the same firm. The figure rejects the Calvo

change results in any way.

[19] It should be noted that our wage growth for job-stayers includes a combination of cohort, time and age effects. The presence of age effects implies that wage growth for job-stayers is higher than the wage growth for the economy as a whole. See Beraja et al. (2019) who make a similar point when comparing time series and panel data wage growth patterns in the CPS during the Great Recession.

Figure 3: Hazard Function of Base Wage Change, Job-Stayers



Panel A: Hourly Workers Panel B: Salaried Workers

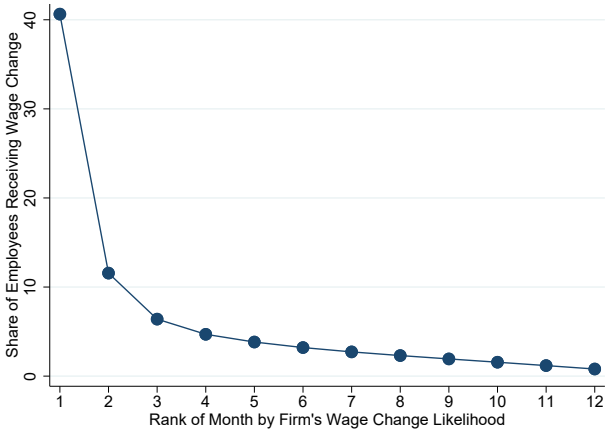
Note: Figure shows the hazard rate of a base wage change between $t-1$ and t conditional on surviving to $t-1$ without a base wage change at the same firm. Sample only includes individuals who remain at the same firm with at least two observed base wage changes. We use our employee sample between 2008 and 2016 for this analysis (conditional on remaining with the same firm).

prediction that the probability of wage change is constant over time at the individual level for job-stayers. In most months, the probability of a base wage change is roughly constant at about 3-4%. However, 12 months after the last wage increase, individuals are much more likely to get another base wage increase. Conditional on making it to month 11 with no base wage change, there is over a 50% probability that an individual gets a base wage increase in month 12. Note, given a little bit of calendar variation, there are small spikes at 11 and 13 months as well. We also see another spike in the hazard at 24 months.[20] This is consistent with a Taylor (1979) model of periodic wage contracts renegotiated once per year.

Figure 4 shows the time dependence in wage setting at the firm level. For this analysis, we use our firm-level sample. We restrict the firm-level sample to only include firms who remain in the sample for all 12 months during a given calendar year. For each firm-year pair, we compute the fraction of workers who received a nominal base wage change during each calendar month. We then rank the months within a given firm-year pair from the month with the highest fraction of nominal base wage changes to the month with the lowest fraction of nominal base wage changes. For example, for some firms the highest month may be September while for other firms the highest month may be January. We then take the

[20] In the online appendix, we show that only about 15 percent of workers who remain on the job for twelve calendar months receive multiple base wage changes during the year. Additionally, we show that workers who do receive base wage changes "off-cycle" (i.e., in months 2-10 from their last base wage change) experience much higher wage changes on average relative to those who receive a wage change on-cycle (one year after last base wage change).

Figure 4: Share Receiving Base Wage Change in Firm's Months with Most Wage Changes, Firm-Level Data



Note: Figure uses data from our firm sample. We restrict the sample to include only firms who remain consistently in the sample during a given calendar year. We compute for each calendar month within a firm-year pair, the fraction of workers who received a nominal base wage change during that month. We rank the months within a given firm-year pair from highest month of base wage changes to lowest month of base wage changes. We take the simple average across all firm-year pairs for each month rank. We restrict our analysis to only those firms who adjusted at least 25 percent of their workers base wages at some point during the calendar year.

simple average probability of a worker receiving a base wage change across firm-year pairs for each ranked month.[21]

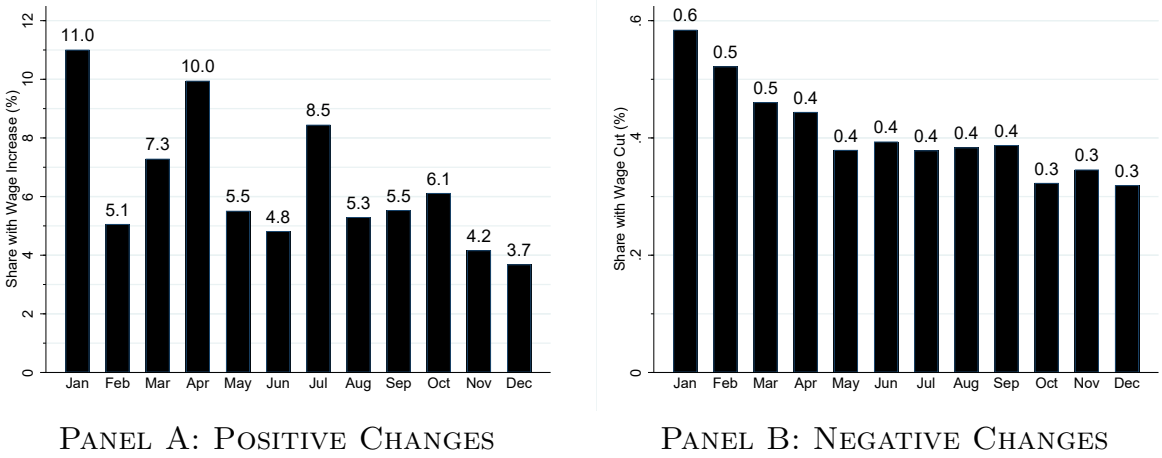
The figure shows that when a firm adjusts base wages, it tends to adjust many wages during one particular month of a given year. For example, a typical firm adjusts 40 percent of their workers' base wages in the month where they make the most base wage changes. Given that only about 65 percent of workers get a base wage change (in this population as a whole) and the fact that we are averaging over firms and not workers, the figure suggests that firms do most of their base wage changes in one month out of the year.[22] As a point of contrast, firms only adjust roughly 10 percent of their workers' base wages in the second highest ranked month. The fact that the share of base wages adjusted are roughly flat between the second highest ranked month and the lowest ranked month is consistent with the worker data where some adjustments occur off-cycle at a roughly constant hazard.

While the Calvo predictions may be rejected at the individual and firm level, Calvo may still be a good approximation for the aggregate economy if firms stagger the months

[21] We also restrict our sample to only firm-year pairs where the firm adjusted at least 25 percent of their workers' base wages at some point during the calendar year. This restriction is not too binding as 91% of firm-year pairs in our sample adjusted at least 25 percent of their workers' base wages during the year.

[22] This observation represents the labor market analogy to the price-setting rule employed in Midrigan (2011) in which multi-product firms enjoy economies of scale in coordinated output price adjustment.

Figure 5: Seasonality in Base Wage Changes, Job-Stayers, All Years



Panel A: Positive Changes

Panel B: Negative Changes

Notes: Figure plots the probability of a base wage adjustment in each calendar month, averaged over our full sample of job-stayers using our employee sample pooled between 2008 and 2016. Panel A shows probability of wage increase, while Panel B shows probability of wage cuts. This figure combines hourly and salaried workers.

in which they adjust wages. Indeed, this is the underlying intuition for the aggregation result in Taylor's staggered wage contract model. Instead of each individual probabilistically getting a wage change each period, individuals deterministically get a wage change at a fixed frequency but a constant fraction of the wage contracts adjust each period. To see whether Calvo is a good approximation for job-stayers in the aggregate economy, we explore the extent to which base wage changes are coordinated within a given calendar month.

Figure 5 shows the probability of base wage changes by calendar month pooling together hourly and salaried workers. For this analysis, we return to our employee sample and focus only job-stayers. Panel A shows the probability of wage increases, while Panel B shows the probability of wage decreases. The figure shows some slight seasonality in wage increases. The probability that a worker receives a base wage increase is highest in January. Three of the four next highest months are the beginning months of each calendar quarter (April, July and October). However, these differences mostly wash out at the quarterly frequency: 23.4 percent of workers receive a base wage increase in the first quarter of the year while 20.3 and 19.3 percent of workers see raises in the second and third quarters. Only 14 percent of workers receive a base wage raise in the last quarter of the year. While wage increases are somewhat seasonal, wage cuts are less so. January remains the modal month for wage cuts, however every month's probability of wage cuts is bounded between 0.3 and 0.6 percent. Given the lack of seasonality in base wage cuts, the seasonality of any wage adjustment mirrors that of wage increases.

Overall, this section shows strong evidence of time dependence in base wage adjustment

à la Taylor. Most base wage changes occur annually, usually at the beginning of a firm's fiscal year in January, April, or July. There is a roughly constant probability of base wage adjustment across the four quarters of the year, suggesting that models of Calvo adjustment may be a reasonable approximation of the base wage adjustment process in normal times. As we document next, base wage adjustment also displays evidence of state dependence.

6.2 State Dependence in Nominal Wage Adjustments

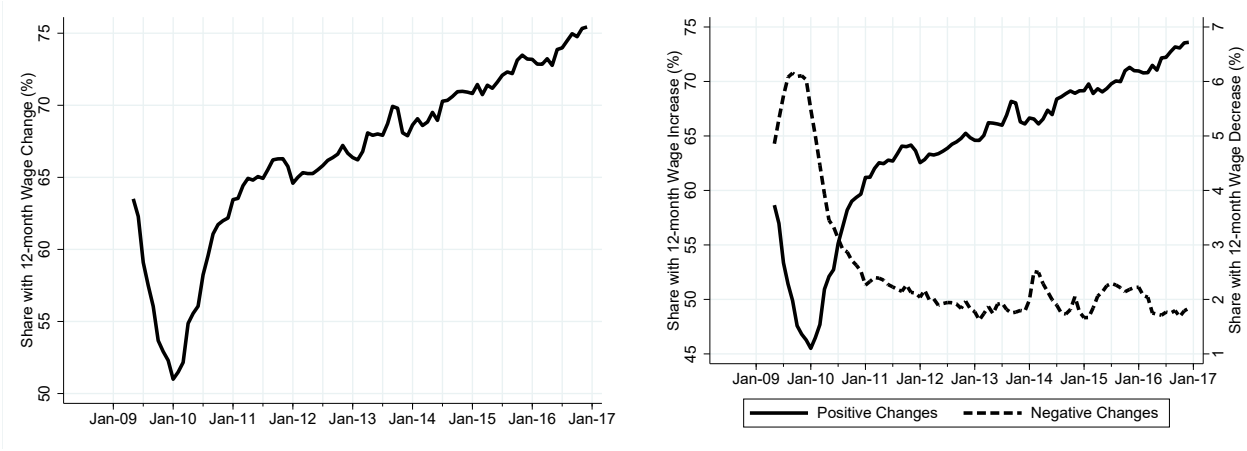
Wage adjustment is said to be state dependent if the probability of adjustment is related to some underlying state variable for the worker-firm match. There are two principal reasons why one might observe state dependence in nominal wage changes for job-stayers. The first arises if wage adjustment is subject to explicit costs. Non-convex adjustment costs, or "menu costs," are commonly employed in New Keynesian models of price setting in order to match moments of the price data. Fixed adjustment costs generate an inaction region whereby firms that are close to their optimal price in a frictionless economy do not adjust their prices until they move sufficiently far away from their optimal price. Thus, with a menu cost of adjusting prices, the state of the firm - its distance from the optimal pricing rule - is central to adjustment decisions. As a result, price changes are infrequent, and large when they occur. Although menu cost models of wage adjustments are rare, principally due to challenges arising from wage bargaining, the intuition gained from the output pricing literature helps guide analysis of state dependence in wage setting.

A second reason for state dependence in nominal wage adjustments might arise in a framework with asymmetric rigidity. For instance, suppose that it is harder for firms to cut wages than to raise them, possibly due to morale or union concerns. Under this scenario, firms receiving a negative productivity shock would have a lower probability of being able to adjust wages to the desired level than firms receiving a positive productivity shock. This would imply that wages appear less flexible in downturns than in booms.

Figure 6 plots the time series of the probability of base wage adjustments for job-stayers using our employee sample pooling together both hourly and salaried workers. The left panel plots the extensive margin of base wage changes: the percent of all employees in month t who have a different base wage from month $t - 12$. As a reminder, our data starts in May 2008. That means the first observation in each of the panels in Figure 6 is for May 2009 and measures the fraction of job-stayers who received a base wage change between May 2008 and May 2009. The fact that our data spans the Great Recession allows us to explore business cycle variation in the probability of base wage adjustments.

As seen from the left panel of Figure 6, the probability of base wage adjustments for job-

Figure 6: Time Series of Nominal Base Wage Adjustments, Job-Stayers



Panel A: Has Wage Change

Panel B: Has Wage Change: Positive vs Negative

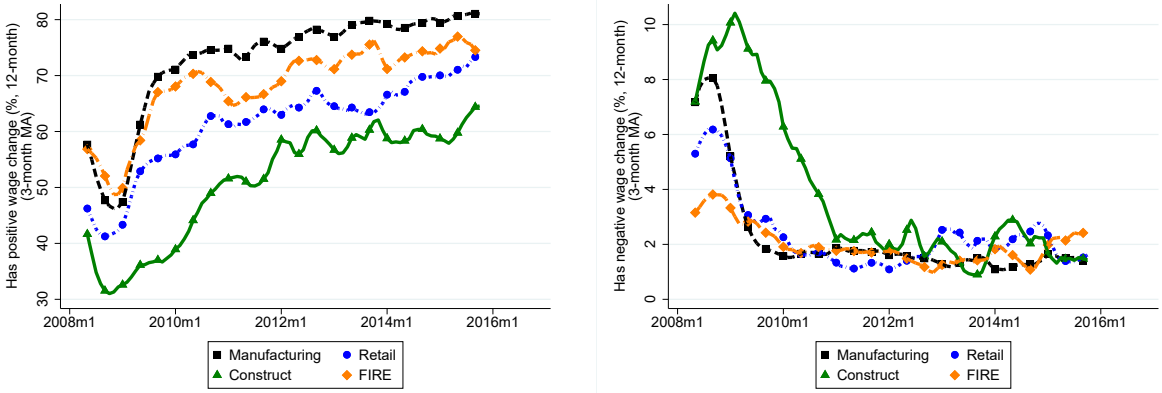
Notes: Figure plots the propensity to receive a 12-month base wage change over time for our employee sample of job-stayers between May 2009 and December 2016. For example, May 2009 measures the propensity for a base wage change between May 2008 and May 2009. Panel A measures whether the worker received any base wage change during the period. Panel B separately measures wage increases (left axis) and the propensity for wage decreases (right axis).

stayers exhibits striking pro-cyclicality. Only about 51 percent of continuing wage workers received a year-over-year wage change during the depths of the recession. However, after the recession ended, during the 2012 to 2014 period, between 65 and 70 percent of workers received a wage change. As of the end of 2016, nearly 75 percent of all workers received a nominal base wage change. While most of the time series variation was between the recession and non-recessionary periods, there is still a trend upwards in the share of workers receiving an annual base wage change between 2012 and 2016.

The right panel of Figure 6 separates the probability of a base wage change of job-stayers into the probability of a base wage increase (solid line - measured on the left axis) and the probability of a wage decline (dashed line - measured on the right axis). During the Great Recession, the propensity of base wage increases for job-stayers fell sharply while the propensity of base wage declines rose sharply. Although nominal base wage cuts are exceedingly rare for job-stayers during non-recessionary periods, upwards of 6 percent of all continuing workers received a nominal base wage cut during late 2009 and early 2010.

Figure 7 shows time series trends in the probability of a nominal base wage increase (left panel) and a nominal base wage cut (right panel) for job-stayers by industry. During the Great Recession, manufacturing and construction were two of the hardest hit industries. Roughly 10 percent of construction workers and 8 percent of manufacturing workers who

Figure 7: Time Series of Wage Changes by Industry, Job-Stayers



Panel A: Positive Change

Panel B: Negative Change

Notes: Figure shows the propensity to receive a 12-month base wage increase (Panel A) and decrease (Panel B) for job-stayers over the cycle, broken out for select broad industry groups. This figure makes use of our employee sample of job-stayers. "FIRE" refers to Finance, Insurance, and Real Estate.

remained at their job received a year-over-year nominal base wage cut during 2009. The comparable numbers for retail and finance, insurance, and real estate (FIRE) were 6 and 3 percent, respectively. By 2012, continuing workers in all industries had a roughly 2 percent probability of receiving a nominal base wage cut. The probability of a nominal base wage increase did not differ markedly across industries during the Great Recession. There are persistent level differences in the propensity of a nominal base wage increase across industries for job-stayers in all years. However, these differences remained relatively constant during the 2008-2016 period. These cross-industry patterns reinforce the time series patterns with respect to the state dependence of nominal base wage cuts of continuing workers. Not only were nominal base wage cuts more likely for job-stayers during the Great Recession, the propensity of nominal base wage cuts was highest in the industries hit hardest during the Great Recession. Firms in manufacturing and construction both were more likely to shed workers during the Great Recession and also were more likely to cut the base wages of the workers who remained with their firm.

An appropriate test for the key prediction of menu cost models – that firms adjust wages if the wage they would optimally set deviates sufficiently far from the existing wage – requires an empirical proxy for the deviation from optimal wage rates across individuals. To construct such a proxy, we pursue the following logic. Suppose that firms adjust wages to their optimal level every time they choose to make an adjustment. The deviation from optimal wages (which we refer to as the "optimality gap") is therefore zero at the beginning of each wage spell, where a wage spell is defined as a contiguous set of months during which

the worker has the same base wage. As optimal wages evolve over the wage spell, so too does the gap between current and optimal wages. Cumulative changes in state unemployment rates since the beginning of the worker's wage spell then serve as a potential proxy for the worker's wage gap so long as increases in the unemployment rate are correlated with drops in workers' optimal wage rates. This will be satisfied if optimal frictionless wages are lower in recessions, as is the prediction of most models.

We estimate the relationship between wage changes and our proxy of the optimality gap using linear regression. Specifically, let workers' wage spells be indexed by τ, and let the month when the wage spell began be $t_0(\tau)$. Letting i index workers, t months, and s the residence state of a worker, we estimate the following regression

$$y_{i\tau st} = \alpha + \beta \times U_{st_0(\tau)} + \gamma \times \left(U_{st} - U_{st_0(\tau)}\right) + X'_{it}\eta + \epsilon_{i\tau st}. \tag{2}$$

The dependent variable $y_{i\tau st}$ is some moment of the wage change distribution for worker i living in state s, such as indicators for whether wages increase or decrease between month t and $t+1$, or the worker's month-over-month wage change. The coefficients of interest are β and γ which measure the response of wage changes to shifts in the state unemployment rate at the beginning of the wage spell ($U_{st_0(\tau)}$) and to our proxy of the optimality gap – the cumulative change in unemployment rates since the beginning of the wage spell ($U_{st} - U_{st_0(\tau)}$) – respectively. We additionally include controls X_{it} for a variety of worker observables, such as age, sex, time since last wage adjustment (both linearly and with an indicator for multiples of twelve months), tenure, industry and firm size.

The results are presented in Table 5. Each column represents a different dependent variable. The dependent variables for the first three columns are indicators equal to 100 if the worker has a base wage increase, decrease, and any change (increase or decrease), respectively. The table shows that our proxy for the optimality gap strongly negatively predicts the likelihood that an individual sees a monthly wage increase. A one percentage point decline in cumulative unemployment rate changes since last wage spell is associated with a 63 basis point higher monthly likelihood of wage increases. This is about one-tenth of the baseline wage increase probability. Similarly, the cumulative change in the unemployment rate since the beginning of the wage spell positively predicts wage decreases: a one percentage point increase in the optimality gap proxy is associated with an increase in the monthly probability of a wage cut by 4.9 basis points, roughly one sixth of the baseline probability of wage cuts (0.28). The larger unemployment increases which have occurred since the worker's last adjustment, the more likely they are to have wage cuts and less likely to have wage increases. Overall, the optimality gap negatively predicts the probability of having a wage

Table 5: State Dependence - The Relationship Between Wage Changes and Cumulative Unemployment Change since Beginning of Wage Spell

	Has Wage Increase (1)	Has Wage Decrease (2)	Has Wage Change (3)	Wage Change (%) (4)	Wage Change, Conditional (%) (5)
$U_{st} - U_{st_0(\tau)}$	-0.63	0.049	-0.58	-0.043	-0.25
	(0.10)	(0.011)	(0.10)	(0.004)	(0.03)
$U_{st_0(\tau)}$	-0.19	0.007	-0.18	-0.015	-0.09
	(0.02)	(0.004)	(0.02)	(0.002)	(0.02)
Observations (000s)	9,240	9,240	9,240	9,227	652
Mean of Dep. Var.	7.15	0.28	7.43	0.33	4.55

Notes: Table reports the coefficients estimates from equation (2). Each column represents a different dependent variable. The dependent variables for the first three columns are indicators equal to 100 if the worker has a base wage increase, decrease, and any change (increase or decrease), respectively. Column 4 and 5 have the month-over-month percentage change in base wages unconditionally (column 4) and conditional on a wage change (column 5). Conditional wage changes winsorized at top and bottom 1%. The independent variables are the unemployment rate at the beginning of a worker's wage spell and change in the unemployment rate since the beginning of the wage spell. Unemployment rates measured in the worker's residence state and are on a percentage point scale. Regressions include control for workers' industry, firm size, age, tenure, sex, time since last wage adjustment (both linearly and with an indicator for multiples of 12 months), hourly status and payment frequency. Standard errors, clustered at the state-year level, reported in parentheses.

change, either up or down. Note, however, that the unemployment rate at the beginning of the wage spell has only limited association with the probability of seeing a wage change in any future month after controlling for the optimality gap proxy. That is, the prevailing conditions at the time of a wage reset do not strongly predict the length that the wage spell will persist after controlling for the future path of the unemployment rate.

These patterns are consistent with the prediction of a model with asymmetric menu costs. The further a worker drifts from her optimal wage level, the more attractive it becomes to pay a fixed wage adjustment cost to shift wages. However, this is particularly true as labor market conditions improve: wage cuts respond less to worsening labor market conditions.

Column 4 and 5 report the association between our proxy for the optimality gap with the month-over-month percentage change in base wages unconditionally (column 4) and conditional on a wage change (column 5).[23] In both cases, the size of wage changes decline as the cumulative unemployment rate increases enlarge, consistent with the notion that this measure proxies for the optimal wage gap. A one percentage point increase in the cumulative unemployment rate change is associated with a 25 basis point decline in the workers' wage changes, conditional on a change occurring.

[23] To limit the influence of outliers we trim the top and bottom 1% of the non-zero wage change distribution.

The results in this section show that time dependence is a good approximation of the wage adjustment process in normal times. However, we also document procyclical downward adjustment and countercyclical upward adjustment during the Great Recession. While we show that nominal wage cuts were more common during the Great Recession, the aggregate propensity of base wage declines only increased modestly in 2009 and 2010 relative to the 2012-2016 period (from 2% to 6%). Future research will need to assess whether the extent of state dependence documented above is quantitatively important. Specifically, without more structure it is unclear whether allowing for a slightly increased propensity of nominal wage cuts during recessions will meaningfully alter conclusions about cyclical employment fluctuations relative to the assumption that nominal wages can never be cut.

7 The Cyclicality of New Hire Base Wages

Although our focus thus far has been on the wages of job-stayers, a substantial literature argues that the flexibility of new hire wages is the key determinant of aggregate employment fluctuations (Pissarides, 2009). The fact that the wages of job-switchers is substantially more cyclical than that of job-stayers (Bils, 1985; Haefke et al., 2013; Martins et al., 2012) has therefore been cited as evidence of a limited role for wage rigidity in explaining employment fluctuations. However, the cyclicality of job-changers' wages may be large for two reasons. The first is that the wages of new hires are simply less rigid: that is, for a given worker in a given job, it is easier to reduce the wage paid to new hires in a recession than it is to reduce the wages of a similar incumbent worker in the same job. However, it may also be that excess wage cyclicality arises from shifts in the composition of job-changers, either in the workers who switch jobs or the sets of jobs to which they switch.

In this section, we therefore make efforts to control for the composition of workers switching jobs, in order to isolate rigidity in firms' marginal cost of comparable labor. First, we present the distribution of base wage changes for job-switchers, finding that a substantial share of job-switchers receive wage cuts. Next, we confirm that job-changers' base wages are more cyclical than those of job-stayers. Finally, we develop a matching method to control for shifts in the composition of workers and jobs in order to isolate rigidity in new hires' wages, and find that the wages of new hires are no more cyclical than those of similar job-stayers. From this, we conclude that the nominal base wage adjustments of job-stayers is a sufficient metric for gauging nominal wage flexibility.

Throughout this analysis, we use our job-changer sample and only include job-changers who either remain an hourly worker or remain a salaried worker. As discussed when defining the sample in detail in the online appendix, the job-changer sample is limited in that we

cannot observe the employment status of workers who transition between ADP jobs. If we observe a worker leaving one ADP firm in month t and then observe that worker arriving at another ADP firm in month $t+k$, we do not know whether the worker was not working between t and $t+k$ or whether they were working at a non-ADP firm. This makes it impossible for us to distinguish job-changers who had an unemployment spell before switching to a new job. For our primary sample, we only look at job-changers who exit an ADP firm in month t and then show up at a new ADP firm at any time within the next 12 months.

Figure 8 plots the distribution of 12-month nominal base wage changes for our primary sample of job-changers. The patterns are strikingly different from the patterns in Figure 2. First, essentially all job-changers receive a base wage change over a given year. Only about 7.5 percent of hourly job-changers and 5.2 percent of salaried job-changers do not receive a year-over-year base wage change. Second, the propensity for a base wage cut is very high for job-changers with 28.2% of workers paid hourly and 25.8% of salaried workers receiving a base wage decline during a job-change. Finally, the distribution of base wage changes is more symmetric around zero. Conditional on a job change and a base wage change, mean and median annual base wage growth was 15.4 and 8.7 percent respectively. Base wage growth is much larger on average for job-changers than for job-stayers. Moreover, the standard deviation of annual nominal base wage changes for job-changers is 36.6 percent - five times larger than the standard deviation of annual nominal base wage changes for job-stayers.[24]

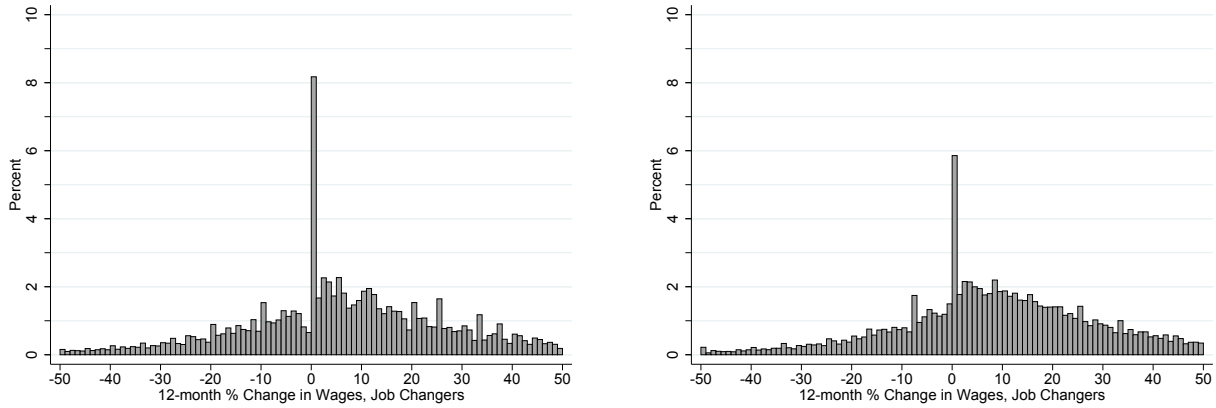
To study the shifts in this distribution over the business cycle, we estimate cyclicality regressions akin to that in equation (1). Specifically, we combine our job-stayer and job-changer samples and estimate the following regression with OLS:

$$\%\Delta w_{ijst} = \alpha_0 + \beta_0 \Delta U_{st} + \beta_1 \Delta U_{st} \times \text{Switcher}_{it} + \beta_2 \cdot \text{Switcher}_{it} + \Gamma X_{it} + \theta_j + \gamma_s + \epsilon_{ijst} \quad (3)$$

where i indexes an individual, t a month, s the state in which i lives in t, and j represents i's firm as of date $t+12$. As before, U_{st} is the unemployment rate, in percentage points, in state s in period t, ΔZ is an operator considering the difference in a variable Z between t and $t+12$, w_{ijst} is worker i's base wage in period t, and Switcher_{it} is an indicator equal to 1 if worker i switches employers between months t and $t+12$. We include a vector of controls X_{it} following Bils (1985), who controls for experience and education in similar cyclicality regressions. In place of experience, we control for five-year age bins, and in place

[24]The patterns of nominal wage changes of job-changers that we document using the ADP data are similar to the patterns found using French data in the 1990s as documented in Postel-Vinay and Robin (2002). Postel-Vinay and Robin (2002) document that about one-third of French workers experience a *real* wage decline as they move from job to job with no intervening unemployment spell during a period of relatively low inflation. Similarly, Sorkin (2018) uses linked employer-employee administrative data from the United States to document that many workers receive annual earnings declines when they change jobs.

Figure 8: 12-Month Base Wage Change Distribution for Job-Changers



Panel A: Hourly-to-Hourly Changers

Panel B: Salaried Changers

Notes: Figure shows the 12-month change in nominal base wages for workers in our job-changer sample. We include all data from 2008-2016. See text for additional details.

of education, we control for a worker's percentile within the national wage distribution in month t. In order to isolate cyclical fluctuations in a given firm's marginal cost, we include firm fixed effects θ_j in some specifications, while γ_s is a state fixed effect. The coefficients of interest are β_0, which reports the cyclicality of job-stayer wages, and β_1, which represents the excess wage cyclicality for job-changers.[25]

The results of this regression are reported in Columns (1)-(4) of Table 6. Standard errors, clustered at the month level, are reported in parentheses below the estimated coefficients. Column (1) includes controls only for worker age, sex, state, and initial wage percentile. This column shows that job-stayers' base wages are pro-cyclical, with a 1 percentage point increase in unemployment rates associated with a 0.33 percent decline in wage growth for job-stayers, in line with the coefficients in Table 2. However, job-changers' base wages are substantially more pro-cyclical: those who switch jobs exhibit 0.77 percent lower wage growth relative to job-stayers for every percentage point increase in the local unemployment rate. This excess cyclicality is statistically significant at the 1% level, but is somewhat lower than the cyclicality of wages uncovered by Gertler et al. (2016), who find, using the Survey of Income and Program Participation (SIPP), that a one percentage point increase in unemployment is associated with declines in wage growth of 0.45 percent for job-stayers and 1.44 percent for job-changers. This reduced cyclicality could be due to a difference in sample period, or because Gertler et al. only include men, while we include the full economy.

[25]Note that this formulation requires that an individual appear in the dataset in both period t and $t+12$. This will generally lead us to exclude workers who switch jobs very frequently, as the probability that such workers switch into firms not covered by ADP within 12 months may be large.

Table 6: Cyclicality of New Hire Wages

	(1)	(2)	(3)	(4)
ΔU_{st} (pp.)	-0.33	-0.22	-0.35	-0.35
	(0.03)	(0.02)	(0.03)	(0.03)
$\Delta U_{st} \times$ Switcher	-0.77	-0.80	-0.69	-0.18
	(0.24)	(0.21)	(0.12)	(0.17)
State Fixed Effects	Y	Y	Y	Y
Demographic and Wage Percentile Controls	Y	Y	Y	Y
Firm Fixed Effects	N	Y	Y	Y
Interacted Controls	N	N	Y	Y
Tenure Control	N	N	N	Y
Observations (000s)	12,514	12,514	12,514	12,514

Notes: Table reports cyclicality of wage growth for job-stayers and job-changers, estimated from Equation 3. The dependent variable in columns (1)-(5) the percentage base wage growth between month t and month $t + 12$, while the independent variable is the change, in percentage points, in state unemployment rates between t and $t+12$. All Columns include controls for demographics, namely fixed effects for 5-year age bins and worker sex, and the worker's percentile in the national base wage distribution as of month t. Columns (2)-(4) additionally include firm fixed effects, while columns (3)-(4) include fully interacted controls for firm, demographics, and wage percentile. Column (4) additionally controls for worker tenure in period t. Standard errors, clustered at the month level, are reported in parentheses.

As noted above, however, the excess cyclicality of job-changers' wages does not imply that new hires' wages are less rigid than those of job-stayers, as the composition of workers moving into or out of given jobs may change through the cycle. Indeed, simply controlling for observable characteristics of *workers* who switch jobs in these regressions does not fully control for the set of *jobs* to which the workers move. This idea has been well established in the literature by, for instance, Solon et al. (1994, 1997), Grigsby (2019), and Gertler et al. (2016) who show that much of the cyclicality of new hire wages may be explained by pro-cyclical match quality.

To sweep out some selection in the jobs workers move to over the course of a cycle, Column (2) adds destination firm fixed effects to the regression. Doing so reduces the coefficient on unemployment rate changes for job-stayers' wages to 0.22, but does not meaningfully change the excess cyclicality for job-switchers. The fact that the cylicality of wages for job-switchers increases slightly suggests that workers disproportionately move to firms with above average wage growth in recessions. Next, column (3) includes a full suite of interacted control variables: that is, fixed effects for the cross-product of age group, wage percentile, sex, and destination firm. In this regression, the cyclicality of job-stayer wages increases to 0.35, but excess cyclicality falls to 0.69, suggesting that the movement of particular *types* of worker to different types of firm is part of the reason why job-changers' wages are more cyclical.

Finally, column (4) additionally controls for the employment tenure of workers in period t; that, is the workers' tenure in the last month of employment at their source firm.[26] Doing so reveals that job-stayers' wage growth is as cyclical as we have found thus far, with a coefficient on the unemployment rate of -0.35. However, controlling for tenure removes much of the excess cyclicality of job-changers' wages, which we now estimate to be -0.18. Given a standard error of 0.17, we are unable to reject the hypothesis that changers have no excess wage cyclicality once one controls for observable characteristics of the worker and firm.

Column (4) controls for all determinants of wage growth which are fixed over time for a given worker type by firm pair. However, this still does not capture excess flexibility in a given worker's wage, as there may be worker type-by-firm-specific fluctuations in desired wage growth over time. For instance, if firms disproportionately constrain wage growth for low wage workers relative to high wage workers during a recession, and low wage workers are more likely to switch employers, one might see higher wage cyclicality for job-changers even if their wages are no more flexible.

Ideally, we seek to compare a job-changer to a counterfactual version of herself in which she were always an incumbent at her destination firm. We therefore construct a matching estimator to understand the rigidity of a given firm's marginal cost of labor. The goal of this estimator is to compare the wage behavior of a job-changer with an observationally identical job-stayer in the destination firm. Define $w_{i,j,t}$ as the base wage (in nominal dollars per hour) of worker i who works at firm j in month t. As above, we assume that salaried employees work 40 hours per week when making their hourly base wage measure. Next, define $p_{i,t}$ to be individual i's percentile within the national wage distribution in month t. In the absence of data on individual skills, we treat this as a measure of a worker's quality in t.

To construct our matching estimator, we begin by examining the base wage adjustment of job-stayers. Denote by $\mathcal{I}_t^j(p, X)$ the set of workers who work for firm j continuously between months t and $t + 12$, lie in wage percentile p and have observable characteristics X in t:

$$\mathcal{I}_t^j(p, X) = \{i : (p_{i,t} = p) \cap (X_{i,t} = X) \cap (i \text{ works for } j \text{ in } t \text{ and } t + 12)\} \tag{4}$$

Denote by $N_t^j(p, X)$ the size of this set: i.e. the number of job-stayers at firm j at percentile p with observable characteristics X. We use worker age, grouped into 5 year bins, a set of tenure bins, sex, industry, and residence state as the set of characteristics X.[27] For every combination of wage percentile and observables, we construct the mean wage change

[26] Some workers have missing tenure information. To avoid making the assumption that tenure information is missing at random, we assign these workers a tenure of 0, and control for a dummy variable equal to 1 if the worker is missing tenure information. We further interact this dummy variable with a switcher flag and year fixed effects to account for shifting biases arising from improved data quality over time.

[27] Specifically, we use tenure groups of < 1 year, 1-3 years, 3-5 years, 5-10 years, and 10+ years.

for job-stayers within firm j:[28]

$$\Delta_t^j(p, X) = \frac{\sum_{i \in \mathcal{I}_t^j} (\ln w_{i,j,t+12} - \ln w_{i,j,t})}{N_t^j(p, X)} \tag{5}$$

The goal is to compare this $\Delta_t^j(p, X)$ to the wage changes of similar job-changers into firm j. We construct the 12-month base wage change for every switcher in our primary sample as they move from firm j to j'. Then we compare each job-changer i moving to firm j' with a matched job-stayer in j with the same initial observable characteristics. If the wages of new hires are more flexible than that of job-stayers, one would expect excess cyclicality of job-stayers to persist even after controlling for the wage change of this matched job-stayer. If, however, the excess wage cyclicality of job-changers purely reflects changes in the set of workers moving into a given job, then the wage changes of job-changers should exhibit no excess cyclicality once one controls for the wage evolution of similar job-stayers at the destination firm.

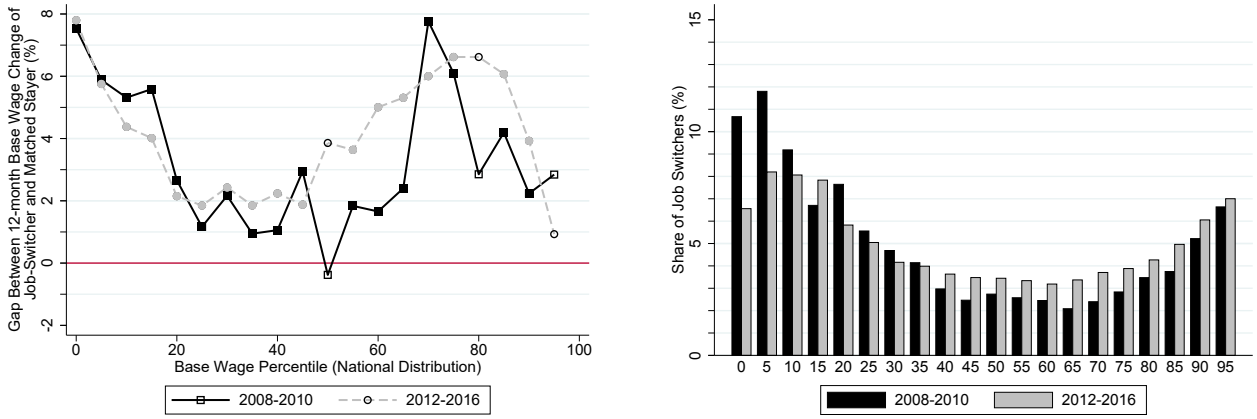
This matching exercise yields two key results. First, the wage change of the average job-changer is always higher than the wage change of their matched job-stayer. In 2009, an average job-changer has 3.0 percentage points higher wage growth than their matched job-stayer. The median job-changer has no gap between her wage growth and the wage growth of her matched job-stayer. This is *prima facie* evidence that a firm is unable to simply lay off job-stayers and replace them with a cheaper equivalent new hire in order to overcome downward nominal wage rigidity, as job-changers generally see larger wage increases.

Secondly, the gap between job-changers' wage growth and that of their matched job-stayers is largely acyclical. Regressing the gap between job-changers' wage growth and that of their matched job-stayer on changes in the state unemployment rate and the controls in column (4) of Table 6 yields a coefficient on the unemployment rate of -0.15, with standard error 0.33. This is in line with the lack of excess cyclicality reported in column (4), albeit estimated with more noise given the requirement that we find a matched job-stayer.

This lack of cyclicality is depicted visually in Figure 9. Panel A plots the gap between job-changers' wage growth and that of their matched stayer for workers who switch jobs in 2008, 2009, or 2010 (black line), and for those who switch in the years 2012-2016 (gray dashed line). The x-axis plots the wage percentile of the worker in her last month of employment with her source firm. The plot shows that the gap between changers and matched stayers is similar in the recession and recovery periods throughout the distribution. Until approximately the 45th percentile of income, the black and gray lines track each other almost exactly. Thereafter,

[28] To maximize power, we use the full ADP data for this exercise, rather than any of our subsamples.

Figure 9: Matching Estimator: Gap Between Job-Changer Wage Growth and Wage Growth of Similar Job-Stayer in Destination Firm



Panel A: Gap Between Job-Changer and Matched Job-Stayer Wage Growth

Panel B: Share of Job-Changers by Initial Wage Percentile

Notes: Panel A shows the average gap between the wage growth of job-changers and a matched job-stayer. Matched job-stayers have the same destination firm, initial wage ventile, sex and 5-year age bin. We limit attention to job-changers who stay in the same state and 2-digit NAICS industry. The black squares show the gap in the recession (2008-2010) period, while the gray circles present the patterns for the recovery (2012-2016). Hollow markers represent statistically significant differences between recession and recovery periods at the 5% level. Panel B plots the distribution of job-changers by initial wage ventile.

a gap between the lines opens up. This may have two causes. First, it's possible that new hire wages are relatively more flexible for high wage workers.

Alternatively, it may be the result of noise, as there are far fewer job-switchers in the middle of the wage distribution than at the left tail during the recession. Only three ventiles – corresponding to the 50th, 80th and 95th percentiles – had statistically significant differences between recession and recovery periods at the 5% level, represented by the hollow markers on the plot. Indeed, Panel B of Figure 9 shows the share of job-switchers in each wage ventile in 2008-10 (black bars) and 2012-16 (gray bars). The majority of job-switchers come from the bottom of the wage distribution, especially during the Great Recession. This changing composition illustrates the importance of controlling for selection when estimating the excess cyclicality of job-switchers. The matching exercise we present here argues against substantial excess cyclicality for switchers once one controls for this selection.

This is an important result, as it suggests that firms do not avoid rigidity in incumbent workers' wages by hiring similar workers at a lower wage. This section argues that the behavior of new hires' wages mimics the adjustment patterns of job-stayers' wages once one controls for selection. For this reason, we conclude that rigidity in the base wage of job-stayers is a sufficient statistic for rigidity in a firm's marginal cost of labor, which is the

relevant notion of a wage in many models. This is one of the key results of the paper.

It is important to note that, by conditioning on wages a year prior to the job move, this matching exercise focuses on lateral employment moves. If moves up or down the job-ladder had more flexible wages, then the conclusion that new hire wages are not significantly more flexible than job-stayer wages may be premature. The importance of this caveat grows with the set of matching criteria used. For this reason, we elect not to match on additional worker or firm characteristics, such as firm size.[29] Similarly, we select on transitions to firms that continue to employ workers with a similar pre-existing profile to the switcher. In addition, our sample period is such that we have only one aggregate cyclical downturn to consider - the Great Recession. It is possible that the behavior of the labor market may differ across business cycles. Finally, the result that new hires' and incumbents' wages evolve similarly may arise through internal equity concerns, or if firing and hiring costs are larger than the cost of adjusting wages. Our conclusions may cease to hold if hiring and firing costs fall substantially, the cost of adjusting incumbents' wages rises, or cultural or organizational shifts mitigate the influence of internal equity concerns.

One final caveat concerns the extent to which the base wage is allocative for new hires. In many models, the present value of the employment relationship determines whether a worker and a firm initiate a match. This present value will be highly sensitive to the base wage, but may also depend on large signing bonuses. To assess the importance of such signing bonuses, we define a worker's signing bonus to be a large (at least one percent of annual income) residual earnings payment given to a worker in her first month of employment with a firm. Here we exploit the fact that we observe an employee's start date in order to adjust for the number of paychecks the worker receives in her first month. Defined thus, signing bonuses are relatively rare, with 5% of non-commission job-switchers receiving them. What's more, these bonuses are mildly counter-cyclical: 5.6% of new hires received signing bonuses during the recession years of 2008-2010, while 4.7% of new hires received a signing bonus between 2011 and 2016. These estimates are likely upper bounds given that our measure of signing bonuses could include any other large residual payment including travel reimbursements.

Collectively, our new hire results yield a few key insights. First, we confirm that job-changers have more cyclical wages than do job-stayers. However, after controlling for selection as best we can, job-changers' wages do not respond more to business cycle conditions than do the wages of job-stayers. This suggests that the majority of the increased cyclicality of job-changers can be explained through composition effects, as posited by Gertler et al. (2016), rather than arising from excess wage flexibility of new hires. The results presented in

[29] The results of the matching estimator do not change if we exclude industry, residence state, or sex from our set of matching criteria.

this section complement those of Hazell and Taska (2019), who document that posted wages adjust about as frequently as the wages of incumbent workers do. New hire wages at the job level do not appear substantially more flexible than those of job-stayers do, suggesting that internal equity concerns may be important in wage setting. As a result, we conclude that the distribution of nominal base wage adjustments of job-stayers is a sufficient statistic for the degree of allocative nominal wage rigidity in the economy insofar as the base wage of job-stayers pins down the model-relevant outcome of wage rigidity: the responsiveness of firms' marginal cost to shocks.

8 Discussion

In this section, we highlight the distinction between realized compensation dynamics for an individual worker and contract rigidity from the perspective of the firm. A worker who remains continuously employed with the same firm may receive declines in realized compensation per hour even when their labor contract is rigid. These declines could occur from transitory drops in overtime or bonuses across years. These movements in compensation expose workers to a potentially risky income stream. However, as we highlight above, these forms of compensation are not used to adjust the firm's marginal cost of a worker at business cycle frequencies. This echoes the theoretical point of Fischer (1977), who shows monetary non-neutrality when future wage adjustments are predetermined by employment contracts. If the schedules for overtime pay and bonuses are pre-determined, realized changes in compensation from these components based on rigid contracts do not reflect flexibility.

We begin by gauging the importance of bonuses and overtime for earnings volatility for our sample of job-stayers. For this analysis, we restrict our employee sample to include only non-commission workers who remain continuously employed with the same firm for twenty-four consecutive calendar months. The twenty-four consecutive calendar month restriction is necessitated by the fact that bonuses and overtime accrue annually. This is the same sample used in the bottom panel of Table 2. Given that our data starts mid-year in 2008, our bonus sample pools together workers for all two-year periods between 2009 and 2016.

Figure 10 shows the annual change in four forms of compensation. To assess how bonus and overtime adjustments affect the variation in broader measures of per period compensation, our bonus, overtime and base wage measures must be in the same units. As a result, we create per hour measures of compensation by dividing various measures of annual earnings by the number of hours paid. Panel A measures annual base earnings per hour, panel B measures annual base earnings plus bonuses per hour and panel C measures annual base earnings plus bonuses and overtime per hour. We assume that salaried workers always work

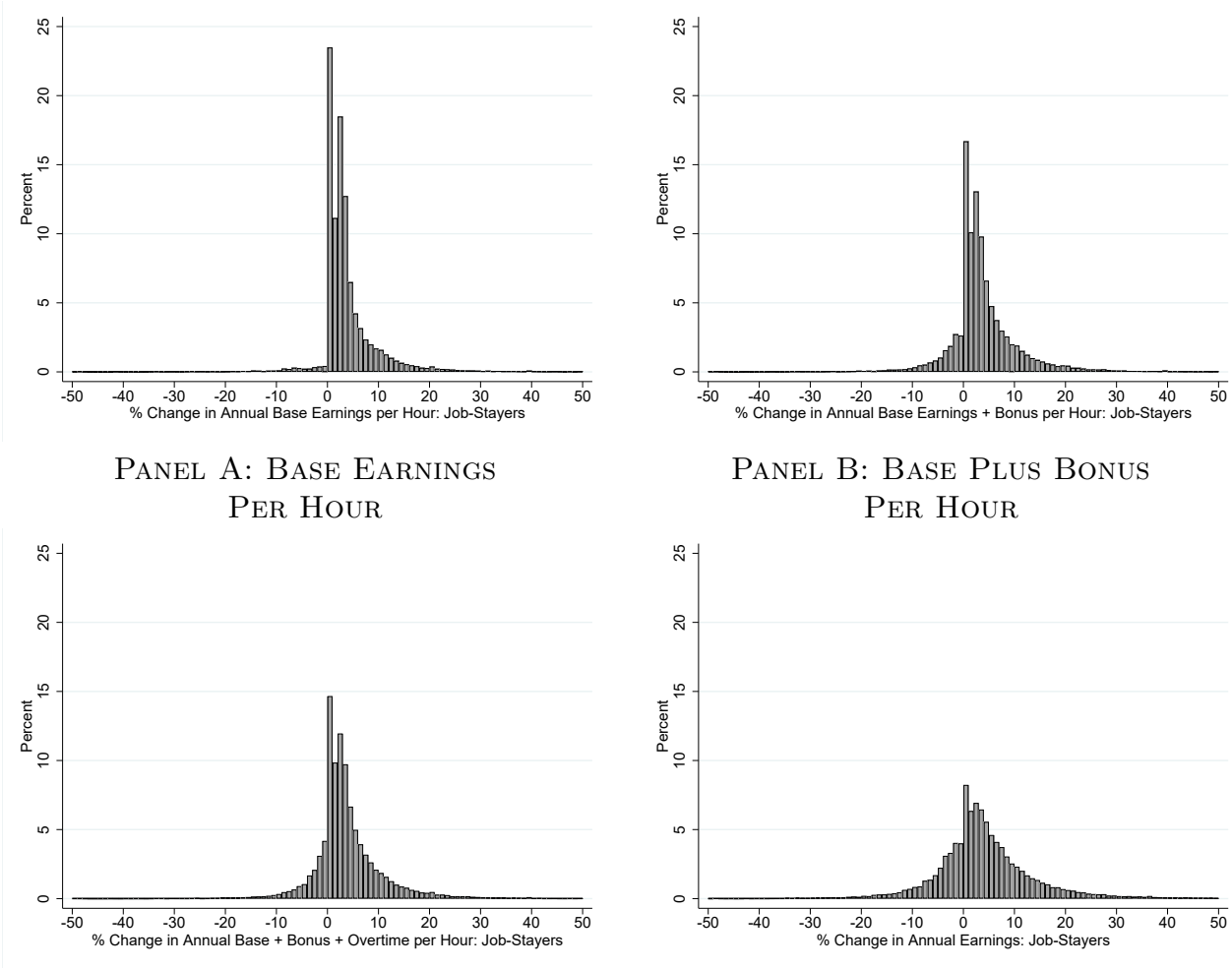
40 hours per week and 52 weeks per year. Such a normalization for salaried workers does not affect the results in Panel A of this figure at all. The normalization of hours paid for salaried workers only matters in the extent to which it converts annual bonuses and annual overtime into a per-hour measure. Panels A-C measure changes in per-hour compensation holding the number of hours paid fixed. Panel D measures the change in total annual earnings which also allows for annual hours worked to vary for those who are paid hourly. All panels pool together both hourly and salaried workers with harmonized y-axes for comparability.

Not surprisingly, the patterns in Panel A are very similar to those presented in Figure 2. The only difference between the two figures results both from selection on workers who remain with the firm for two-consecutive years and from time aggregation. In Figure 2, we restrict workers to those who remain with the same employer for only 12 consecutive months while Panel A of Figure 10 includes workers who remain with the firm for 24 consecutive calendar months. Additionally, Panel A of Figure 10 measures nominal wage adjustments between calendar year $t-1$ and calendar year t. Wage changes that occur prior to December of $t-1$ will show up as an additional nominal wage adjustment relative to Figure 2. As a result, the variance of changes is slightly higher for annual base earnings per hour relative to 12-month changes in base wages. Despite these differences, only 3.9% of workers who remain with their firm for two-years receive a year-over-year nominal base earnings per hour reduction. The patterns in this panel are nearly identical to the patterns shown in Figure 2.

Panel B of Figure 10 shows the annual change in annual base earnings plus annual bonuses per hour. Relative to Panel A, the change in hourly compensation inclusive of bonuses is much more dispersed. While only 3.9 percent of workers received a decline in their annual base earnings per hour, 14.8 percent of workers saw reduced per hour compensation inclusive of bonuses. Additionally, accounting for bonuses increases the standard deviation of nominal wage changes from 5.3 percent to 6.3 percent. Bonuses have long been considered a potential source of additional earnings volatility for a given worker (Shin and Solon, 2007). While bonuses do not adjust cyclically, they do vary substantively across years for a given worker.

Panel C of Figure 10 presents the annual change in total per-hour compensation (inclusive of base pay, bonuses and overtime). As with the results above, adding in overtime provides an additional margin of downward earnings adjustments in terms of worker take-home compensation. Comparing the results between Panels B and C, the fraction of workers who received an annual reduction in per hour compensation during our sample period increases to 17.2 percent. As we discuss below, many other studies using administrative earnings data measure the extent to which total per-hour compensation adjusts downward. As seen from our results, the downward adjustment in total compensation per-hour for a given worker who remains on the same job is driven by year-over-year fluctuations in bonuses and overtime

Figure 10: Change in Various Measures of Compensation: 24-month Job-Stayers, Excluding Commission Workers



Panel A: Base Earnings Per Hour

Panel B: Base Plus Bonus Per Hour

Panel C: Base Plus Bonus And Overtime Per Hour

Panel D: Annual Earnings

Notes: Figure plots the distribution of year-over-year changes in annual base earnings per annual hours paid (Panel A), annual base earnings plus bonuses per annual hours paid (Panel B), annual base earnings plus bonuses and overtime per annual hours paid (Panel C) and total annual earnings (Panel D). Figure restricts attention to a sample of 24-month job-stayers excluding commission workers, between 2009 and 2016. Panels pool together hourly and salaried workers. For hourly workers, we use actual hours worked during the year. For salaried workers, we assume they are paid for 40 hours per week and 52 weeks per year.

rather than downward adjustments in base pay. However, from the perspective of the firm, these forms of compensation do not affect the cyclicality of marginal cost.

For completeness, Panel D shows the annual change in take-home compensation for job-stayers. This panel includes variation in weeks worked as well as hours worked for hourly workers. This figure is comparable to annual earnings volatility for a given worker on the same job using panel survey data or administrative tax or Social Security records. Given the

extent to which annual hours worked for a given job-stayer varies over time, annual changes in total compensation is even more variable than annual changes in total compensation per hour (standard deviations of 9.5 and 6.4, respectively).

With the above results in the prior section as a backdrop, we end the paper by highlighting how our estimates compare with other recent estimates of nominal wage rigidity in the literature using administrative data.[30] Administrative data sets containing measures of both earnings and hours are exceedingly rare in the United States. Kurmann and McEntarfer (2018) and Jardim et al. (2019) use data from the US Longitudinal Employer Household Dynamics (LEHD) and Washington State Unemployment Insurance Records to examine nominal earnings-per-hour adjustments for a sample of job-stayers who reside in Washington state over a two-year period. They focus their sample on residents of Washington state because Washington requires employers to report the hours worked of their employees as part of their Unemployment Insurance program. Focusing on a sample of two-year job-stayers, both papers define the nominal wage as total annual earnings (inclusive of bonuses and overtime) divided by total annual hours worked. This measure is directly comparable to our results in Panel C of Figure 10. Their findings with high quality administrative data from Washington are consistent with our patterns for measures of annual earnings per hour inclusive of bonuses and overtime.[31] While such measures are very informative about earnings per hour volatility, they are not well-suited to consider whether fluctuations are driven by base wage changes or other sources like bonuses and overtime premiums. However, as we discuss throughout the paper, it is the nominal adjustment of base wages that determines the cyclicality of the marginal cost of workers from the firm's perspective. Distinguishing between base wages and overtime/bonuses in the wage adjustment literature is akin to distinguishing between reference prices and sales in the pricing literature.

There is a larger set papers measuring nominal wage adjustments using administrative data in Europe.[32] Some of this literature focuses on year-to-year variation in annual earnings per hour inclusive of bonuses and overtime. These papers almost always find changes in per hour total compensation that are similar to what we document in Panel C of Figure 10. For example, both Nickell and Quintini (2003) and Elsby et al. (2016) use data from Britain's

[30] Our paper is closest in spirit to the results in Altonji and Devereux (2000) and Fehr and Goette (2005) who use administrative payroll data similar to ours from one and two large firms, respectively, in the 1990s. Within the companies studied by these authors, the patterns of base wage adjustment for job-stayers closely match the patterns we document for the whole U.S. economy during the 2008-2016 period.

[31] For example, Jardim et al. (2019) report that roughly 29% of two-year job-stayers in Washington received per hour annual earnings cuts during the 2009-2010 period and that 24% received per hour annual earnings cuts during the 2012-2016 period. The additional 5% downward adjustment during the Great Recession is consistent with the increased propensity of base wage cuts we document during the 2009-2010 period.

[32] See Elsby and Solon (2018) for a recent survey of this literature.

New Earnings Survey which collects administrative data on wages and hours from employers to document that between 15% and 25% of British job-stayers receive annual declines in earnings per hour post-1990. Ehrlich and Montes (2019) use administrative data from Germany and finds a large mass of job-stayers who receive annual declines in compensation per hour. However, the patterns in these papers stand in stark contrast to papers that specifically focus on a worker's per-period base (contract) wage. Using administrative data that allows for a distinction between the base wage and other forms of compensation, Sigurdsson and Sigurdardottir (2016) using Icelandic data, Carneiro et al. (2014) using Portugese data, and Ekberg (2004) using Swedish data all find that reductions in nominal base wages are exceedingly rare (less than 5%) for their samples of job-stayers.[33] Collectively, these papers highlight that base wages are, in fact, downwardly rigid. However, bonuses and overtime vary (both up and down) from year-to-year. Given that bonuses and overtime are relatively acyclical, they contribute to worker per-hour earnings volatility despite not being the appropriate moment to discipline models incorporating the sluggish cyclical adjustment of firms' marginal costs of labor.

9 Conclusion

This paper measures nominal wage adjustments for millions of US workers from 2008 to 2016 using administrative payroll records from ADP, the largest payroll processor in the United States. The payroll data allow us to measure – without error – a worker's per period base contract wage. For hourly workers, their base wage is their contracted hourly wage. For salaried workers, their base wage is their per-pay-period contracted earnings which is their annual base salary divided by the number of annual pay periods. The data also allow us to measure other forms of compensation including commissions, overtime payments, and bonuses. We perform a variety of exercises to highlight that base wages are the component of earnings that matches the model-relevant outcome of wage rigidity: the cyclicality of the marginal cost of labor. We highlight that base wages of job-stayers are highly procyclical while bonuses and overtime payments are roughly acyclical. We conclude that bonuses and overtime payments are akin to sales in the output pricing literature.

Our results show there is essentially no downward nominal base wage adjustment during non-recessionary times for workers who remain continuously employed with the same firm. Further, we show detailed evidence of time and state dependence in worker base wage ad-

[33] Sigurdsson and Sigurdardottir (2016) also document evidence of both time and state dependence in nominal base wage adjustments within their sample of job-stayers that is similar to what we find in the US data. Le Bihan et al. (2012) use detailed firm level data from France to also highlight the importance of time dependence in firm base wage adjustments.

justment. Finally, we show that the base wages of new hires are no more cyclical than the base wages of existing workers, once one adjusts for the shifting composition of job-changers. Collectively, our results paint a relatively complete picture of nominal wage adjustments for workers and firms in the United States during the 2008 to 2016 period.

The goal of the paper is to provide a set of moments that can be used to both guide and discipline models of nominal wage adjustments. However, the differential adjustment patterns of various compensation forms that we highlight urges careful theoretical consideration over the correct notion of "wage rigidity." The evidence presented in this paper suggests that workers' *contracts*, which specify base wages, and a bonus and overtime schedule, may be quite rigid, particularly on the downside. However, workers' *realized compensation*, which includes base pay, and realized bonuses and overtime payments, may be quite flexible, as stipulated by the (explicit or implicit) contract governing their employment relationship.

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Online Appendix: "Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data" by John Grigsby, Erik Hurst and Ahu Yildirmaz

Appendix A Data Details and Sample Construction

In the main text, we outlined a broad overview of our sample construction. In this appendix, we provide additional details.

Our principal sample is of one million random employees in the ADP database. To draw this sample, we first construct a list of all the unique IDs of employees who are between the age of 21 and 60 for at least one month in the data. Then we randomly select one million such IDs, and draw the complete history of each of these individuals in the data. By drawing their complete history, we track workers as they switch across employers, so long as they remain employed with a firm that contracts with ADP.

We construct a similar sample of 3,000 random firms. For this, we construct a list of all unique client codes in the data, and randomly select 3,000 such firms. We then draw all of the information for workers who work for those firms in each month. The first three rows of Table A1 show the number of unique employees, firms, and monthly observations in the employee and firm samples.

Approximately 0.4 percent of worker-firm observations appear multiple times in the same month. This principally results from workers who work multiple jobs within the same firm in that month, such as a line worker who is also a shift manager. The rarity of such events mean that they will not bias our estimates substantially. Since our focus is on the extent to which wages adjust, we only include the observation with the smallest base wage in the month. The intent is to look at fluctuations in wages for a worker in a given job.

For some analysis, we focus on individuals who switch firms. In order to maximize sample size for such analyses, we additionally draw the complete set of job-switches in the data. To construct this sample, we proceed iteratively. First, we bring in the complete data for May of 2008: the first month of our sample. Then we merge in the worker's wage and firm in May 2009 (if the worker only works for one employer in May 2009), and calculate the worker's wage change between the two years. We then do the same for June of 2008, and append this to our file. We continue to iteratively add months, keeping only the first and last month of a worker's employment with a particular firm.

Once we have a complete set of worker-firm employment spells and their associated 12-month wage changes, we first drop all firms who appear to undergo a merging of client codes as described below. We then drop one-month employment spells with a given firm, and workers who have overlapping employment spells at more than one firm; that is we restrict attention to single job-holders. This additionally removes about 8% of workers. We then keep only the last month of employment at each firm to arrive at our sample of job-changers.

When creating our job-changer sample, three additional issues are worth noting. First, we stress that we are measuring wage changes for workers who move from one ADP firm to another ADP firm. An implicit assumption we make throughout the paper is that the patterns of nominal base wage adjustments for workers who migrate across ADP firms are similar to the patterns of nominal base wage adjustment for workers who migrate to and from non-ADP firms.

Second, as noted above, multiple establishments within a firm sometimes contract separately with ADP or, on occasion, firms will spin off into multiple units each contracting separately with ADP. In this case, a movement from one establishment within a firm to another establishment within the same firm will look like a job-change. To account for such flows, we measure the percent of job-changers leaving a given firm in month t and showing up at another ADP firm in month $t+1$ or month $t+2$ using the universe of our data. If more than twenty percent of job-changers leaving firm i subsequently show up in firm j with no intervening employment spell elsewhere between t and $t+2$, we treat switches from i to j as within firm movements over this time period, and do not include them in our job-changer sample. In addition, if a worker's reported tenure does not reset after switching firms, we exclude that worker from the job-changer sample. Removing such dubious switches excludes around 20 percent of observed job changes. We also restrict our analysis to include only those workers who switch between either hourly jobs or who switch between salaried jobs. We exclude those who switch between the two types of jobs. These switches across payment types are relatively rare – only 5.9% of job switches involve such a change – but generate large swings in base wages in almost all cases. All of these restrictions leave us with 2.9 million unique job-switches with non-missing year-over-year wage changes.

Finally, the choice of timing with respect to job changes is more nuanced given the nature of our data. When we see a worker at firm i in month t and then see a worker at firm j in month $t+12$, the worker may have multiple other jobs in the interim. Because we only measure labor market outcomes for ADP firms, if a worker disappears from our dataset for a short time but reappears later, we are not able to distinguish if the worker was not employed or whether the worker was employed but at a non-ADP firm. For many applications, such distinctions are not important. However, it is worth keeping such timing issues in mind

when interpreting our wage adjustment measures for job-changers. In some specifications we explore the sensitivity of our results to this timing issue by restricting our job-changing sample to only include individuals who left firm i in month t and appeared in firm j in month $t+1$. There are 1.1 million such switches in the data, approximately 37% of all observed job switches. For these workers, any intervening unemployment spell would be short; this can therefore be thought of as a sample of employed-to-employed (E-E) flows. This restriction does not change the conclusions of section 7: we are unable to detect statistically significant differences in the cyclicality of wage changes for job-changers who have short gaps in ADP employment relative to those who have long gaps in employment. This is likely due to measurement error in our construction of E-E flows, as the existing literature has found that E-E flows drive much of the cyclicality in job-changers' wages (Gertler et al., 2016).

All replication codes from the paper are available on the AER website and the authors' websites. Given the confidential nature of the ADP data, we were only able to access the ADP data through APD provided laptops that directly accessed ADP servers. Given the sensitive nature of the data, we are not able to post any of the data online for replication purposes. However, replication is possible for researchers who obtain access to the ADP data. In terms of replication, we only use ADP's anonymous employee level database (as described in the text and in more detail above). The variable names in our code are the ones provided by ADP in their employee level data files. Researchers with access to the data can use our code directly with the ADP data to create all measures within our paper. Researchers interested in accessing the confidential ADP data can send requests to the ADP Research Institute (Ahu Yildirmaz at ahu.yildirmaz@adp.com). ADP Research Institute collaborates with researchers outside the institute, and through these research collaborations, researchers have the opportunity to conduct research based on ADP's anonymized, aggregated and de-identified payroll data.

Appendix B Benchmarking ADP Data

In this section of the appendix, we benchmark the ADP data to various other data sources.

Appendix B.1 Firm Size Distribution

There are two areas of concern regarding the representativeness of the ADP data by firm size. First, the patterns we highlight in the paper apply only to firms with more than 50 employees. To the extent that the nature of nominal wage adjustments differs by firm size, the patterns we document within our sample may not be representative of the US economy

as a whole. Below, in Appendix E, we show that there are only modest differences in nominal wage adjustments by firm size within our sample, we conjecture that any potential bias in our headline results from excluding firms with fewer than 50 employees is likely to be small. Furthermore, from 2013 onward, we also have access to ADP's data for firms with fewer than 50 employees. As we also highlight in Appendix E, these data reinforce that any potential bias from excluding small firms from the main results in our paper is likely to be minor.

The second concern is whether ADP clients are representative of firms with more than 50 employees. According to industry reports, roughly 50 percent of US firms in recent years report outsourcing their payroll services to payroll processing companies.[34] As noted in the main text, ADP processes payroll for about 20 million US workers per month. While ADP is the largest payroll processing company, the industry has many competing firms including Intuit, Workday, and Paychex.

According to these same industry surveys, very large firms (firms with more than 10,000 employees) are less likely to outsource their payroll functions. Appendix Table A1 highlights the employment-weighted firm size distribution in our "employee sample" (column 1) and in our "firm sample" (column 2). For the results in this table, we pool our data over the entire 2008-2016 period. By design, we randomly drew 1 million employees for our employee sample and 3,000 firms for our firm sample. Our employee sample includes roughly 250 thousand distinct firms while our firm sample includes roughly 3.3 million distinct employees. The number of actual observations is much larger for each sample because we observe employees for multiple months. For our employee sample, we track employees across all months between 2008 and 2016 that they are employed at *any* ADP firm. For our firm sample, we track all employees in that firm across all months that they remain employed at that firm.

For comparison, column 3 of Appendix Table A1 includes the firm size distribution from the U.S. Census's Business Dynamics Statistics (BDS) over the same time period restricting our attention to only firms with more than 50 employees.[35] As seen from the table and consistent with industry surveys, ADP under-represents very large employers (those with at least 5,000 employees). According to BDS data, nearly 46 percent of all employment in firms with more than 50 employees is in firms with more than 5,000 employees. The ADP data only have 19 percent of employment (in our employee sample) in firms with more than 5,000 employees.[36] As noted above, some of this difference also results from the fact that

[34]See, for example, https://www.feicanada.org/ajaxfilemanager/uploaded/RH_FERF_Benchmarking2014.pdf.

[35]According to BDS data, 72% of all U.S. employment during this time period is in firms with more than 50 employees.

[36]We also explore how the industry distribution of the ADP sample compares to the industry distribution in the BDS. We are unable to report ADP's precise industry distribution for disclosure reasons. The ADP sample has a slight over-representation amongst the manufacturing and broad service sectors, and a

Table A1: Firm Size Distribution in ADP Samples and the BDS, Pooled 2008-2016 Data

	ADP Employee Sample	ADP Firm Sample	BDS Data
Number of Employees	1,000,000	3,296,701	.
Number of Firms	254,729	3,000	.
Number of Observations	21,750,672	68,267,166	.
% Firm Size: 50-499	38.4	31.3	29.5
% Firm Size: 500-999	13.6	13.9	7.3
% Firm Size: 1000-4999	25.2	22.2	17.5
% Firm Size: $\geq$ 5000	19.0	32.5	45.6

Notes: Table reports the share of employees in firms of various sizes in our random samples of the ADP data, stratified at the employee (Column 1) and firm levels (column 2). Column 3 reports the associated employee-weighted firm size distribution reported in the Census' Business Dynamics Statistics (BDS) data. All numbers span the period 2008-2016. In addition, the first three rows show the number of unique employees, firms, and observations in each of our ADP subsamples.

the ADP definition of a firm is different from Census definitions.

To account for the concern that the data do not perfectly represent the universe of all U.S. firms with at least 50 employees, all analyses in the main text are weighted to match the BDS's firm size by industry mix of employment shares for firms with greater than 50 employees. We compute our weights for each year between 2008 and 2016. By re-weighting the data, we control for sample selection along these key observable dimensions. Although there may yet remain selection into the sample along unobservable dimensions, we consider these potential selection issues to be small once controlling for firm size and industrial mix.

Appendix B.2 Demographics and Worker Tenure

Appendix Table A2 shows some additional summary statistics for our ADP employee sample pooling across all years (column 1) and for selected individual years (columns 2-4). In particular, we show statistics for 2008 (our first year of data), 2012 (a middle year of data), and 2016 (our last year of data). The age, sex, and tenure distributions in our ADP sample match well the age, sex, and tenure distributions of workers in nationally representative surveys such as the Current Population Survey (CPS). Additionally, according to the BLS, median tenure for workers over the age of 25 was about 65 months in 2012 and 2014 and was about 60 months in 2016. About one-fifth of our sample is paid weekly while three-quarters is paid bi-weekly. Less than five percent are paid monthly.

Given that ADP is growing over time, so too is our sample. Of our 1 million workers in our employee sample, only 211,000 are in our sample in 2008 while 366,000 are in our

complementary underweight in retail trade, construction, and agriculture.

Table A2: Statistics for Employee Sample, Selected Years

	All	2008	2012	2016
Number of Workers	1,000,000	210,573	373,168	365,739
Number of Firms	254,729	67,165	117,947	94,582
Number of Observations	23,382,167	1,335,656	2,841,901	2,891,002
Age: 21-30 (%)	25.2	25.2	24.4	26.6
Age: 31-40 (%)	24.0	25.4	23.8	24.2
Age: 41-50 (%)	24.1	25.6	24.4	22.3
Age: 51-60 (%)	20.6	18.0	21.2	20.8
% Male	53.9	54.1	53.7	55.0
Average Tenure (months)	69.2	71.8	69.5	67.2
% Paid Weekly	20.3	21.3	21.0	20.6
% Paid Bi-Weekly/Semi-Monthly	76.6	75.8	75.8	75.5
% Paid Monthly	3.2	3.0	3.1	3.9
% Hourly	65.4	65.4	66.0	64.5

Notes: Descriptive statistics for our employee sample in all years, 2008, 2012, and 2016. All data weighted to be representative of BDS firm size by industry distribution for firms with more than 50 employees. This table does *not* select on employees being between 21 and 60 years old

sample in 2016. Despite the growing sample size over time as ADP expands its business, the demographic composition of workers is essentially constant over time. One exception is that average tenure is falling over time. Given that the Great Recession occurred early in our sample, it is not surprising that average tenure fell as many workers became displaced during the recession and eventually re-entered employment as the recovery took hold post-2012. Indeed, the roughly 6-month decline in worker tenure between 2012 and 2016 is also found in BLS data. However, worker tenure in the ADP data is higher in 2008 than similar 2008 numbers reported by the BLS.

For our sample, 65 percent are paid hourly with the remaining 35 percent being classified as salaried workers. According to data from the CPS monthly supplements, only 57 percent of employed workers in the U.S. between the ages of 21 and 60 report being paid hourly during this time period. The difference between the CPS and ADP data may arise as the distinction between hourly and salaried workers is sometimes unclear within the ADP dataset. Some workers in the ADP data are automatically entered as having worked 40 hours each week at a given hourly wage. These workers are therefore classified as hourly wage workers. However, on many levels, these workers operate as if they were salaried: their reported hours never vary across weeks. For these workers, their hourly contract wage is just

their weekly salary divided by 40. Furthermore, these workers may report being salaried in survey data such as the CPS. For our purposes, however, we consider these workers as hourly, matching the ADP-provided definition. Additionally, with respect to wage changes, all changes in per-period earnings for these workers will be associated with a change in the hourly wage given that from the payroll system's perspective hours are fixed at 40 hours per week. Despite these differences in classification, the fraction reporting being paid hourly in the ADP data is broadly similar to the CPS averages.

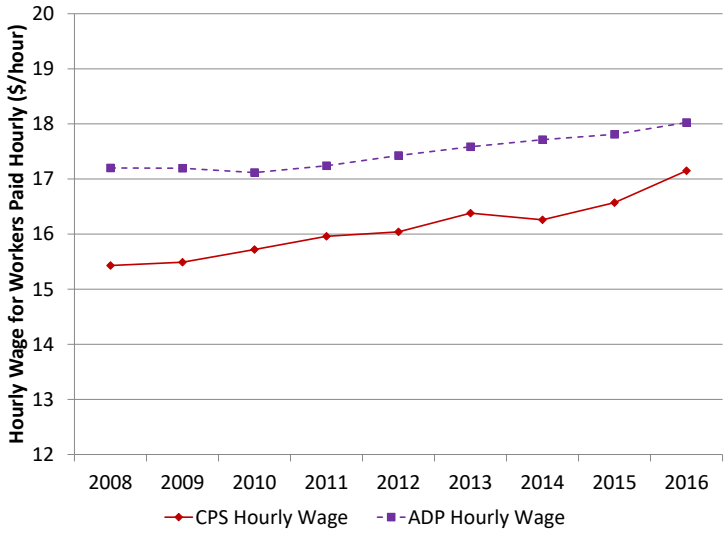
Appendix B.3 CPS Comparison Average Hourly Wage for Hourly Workers

Appendix Figure A1 compares the average hourly base wages for hourly workers in our ADP sample to average hourly wages in a similarly defined sample of 21-60 year olds in the CPS. To get the hourly wage in the CPS, we use data from the outgoing rotation of respondents from the CPS monthly surveys. In the outgoing rotation, workers are asked if they are paid hourly and if so their hourly wage. For hourly workers, hourly wages are slightly higher in the ADP sample than in the CPS. This may be the result of the fact that, as discussed above, some salaried workers are classified as being hourly within the ADP data. Additionally, the ADP dataset does not include workers at small firms who are, on average, paid slightly less than workers at larger firms. The differences, however, between the ADP sample and the CPS sample are small and the trends are very similar suggesting that the ADP data is roughly representative of the entire U.S. population.

Appendix B.4 Distribution of Changes in Annual Earnings

Although our paper is the first to use large-scale administrative data to measure wage adjustment in the United States, we are not the first to consider fluctuations in labor earnings. In particular, Guvenen et al. (2015) estimate a life cycle earnings process using earnings records provided by the Social Security Administration (SSA). Although their dataset has no measure of hours nor any breakdown of earnings by type (precluding a study of wage rigidity or how adjustment patterns differ by compensation component), it has the advantage of covering the universe of American workers over a long time span. As a result, their data are free of sample selection, and represent a logical benchmark for our ADP dataset. We benchmark our annual earnings changes to Guvenen et al. (2015)'s Figure 1, which plots the distribution of individuals' log annual earnings changes between 1998 and 1997, a period of relative calm in the labor market. Since our data do not extend back to 1997, we consider a year we deem relatively similar within our time period - the recovery years of 2015-2016.

Figure A1: Hourly Wage Comparison ADP vs. CPS, 2008-2016



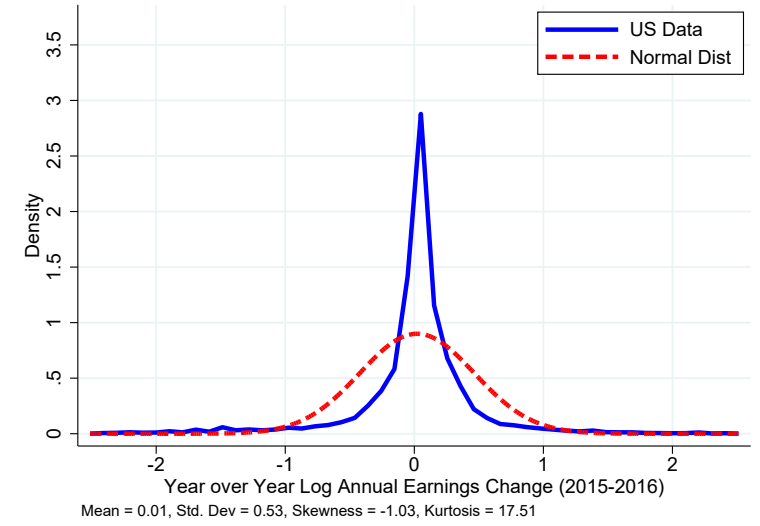
Notes: Figure shows the average hourly wage for hourly workers in our ADP sample and in a similarly defined sample of CPS respondents. Specifically, the CPS sample is restricted to workers between the ages of 21 and 60 who are paid hourly. For the average hourly wage for workers paid hourly in the CPS, we use data from the monthly outgoing rotation files from the CPS. In the outgoing rotation files, workers paid hourly are asked to report their hourly wage. ADP hourly wages reflect base wages. The CPS data are weighted by the corresponding survey weights for the respective samples.

The central challenge in benchmarking our data to annual earnings records is that the ADP data do not follow a worker if they move to employers who are not clients of ADP. This can generate large swings, both positive and negative, in annual earnings, which are not observed in datasets with the universe of employment, such as the SSA. Furthermore, since a great deal of annual earnings fluctuations arise from employment transitions, simply conditioning on workers appearing in the ADP data for a full 12 months will also lead to inaccurate fluctuations in annual earnings.

Our approach is somewhere in between the two extremes of treating all worker-years equally, and considering only full-year employment, in that we consider workers who appear in the ADP data for approximately the same number of months in both 2015 and 2016. Specifically, let N_i be the number of months that worker i appears in the ADP data in 2015. We consider only the annual earnings changes for workers who appear between $N_i - x$ and $N_i + x$ months in 2016, where x is a parameter that we set to 3 by default. For example, a worker who appears in the ADP data for 10 months in 2015 must appear in the ADP data for 7 to 12 months in 2016.

Appendix Figure A2 plots the distribution of log annual earnings changes in the ADP data. The figure matches the SSA data well but imperfectly. We estimate a mean earnings

Figure A2: Comparison of Annual Earnings Changes in ADP Data with SSA Earnings data



Notes: This figure plots the distribution of year-over-year annual earnings changes for workers in the ADP data between 2015 and 2016. We limit attention to workers who appear in 2015 and 2016 for the same number of months, plus or minus 3. The blue line plots the realized distribution in the ADP dataset, while the red dashed line plots the normal distribution implied by the mean and variance of annual earnings changes. ADP data are weighted to reflect the aggregate firm size × industry mix.

change of 0.01, in line with Guvenen et al. (2015). The standard deviation of annual earnings changes is 0.53 (compared with 0.51 in the SSA), while the skewness is -1.03 (vs -1.07) and kurtosis is 17.51 (vs 14.93).[37] We interpret these small differences to be the result of imperfectly capturing the annual earnings changes for job-changers and transitions to unemployment. Overall, however, we find the similarity of this figure to that in Guvenen et al. (2015) encouraging.

Appendix C Calculating Compensation Measures

This section details our construction of relevant compensation measures.

Appendix C.1 Base Wages

The ADP data show an employee's per period base payment rate. This administratively-recorded variable indicates the amount that an individual is contracted to earn every pay period. For hourly workers, this is literally an individual's hourly wage, while it represents

[37] Varying x from 2 up to 4 does not have substantial impact on the results, but increasing x above 4 or decreasing it below 2 reduces the similarity between the ADP and the SSA data - the implied kurtosis is decreasing in x and standard deviation is increasing.

a salaried worker's payment every week, if paid weekly, or every two weeks if paid biweekly, etc. Although these variables are administratively recorded, some employees still appear to have occasional errors in them, presumably resulting from keystroke errors. To deal with these issues, we clean the data in four ways. First, we code salaried workers who earn less than $100 per pay period and have meaningful variation in hours worked as hourly workers. Second, we winsorize wage rates below the federal minimum wage for service workers who receive tips. Some of these individuals may be unpaid interns who receive, for instance, transportation benefits from their employer. Third, we drop employees whose status codes indicate that their employment has been terminated. Finally, in our base wage change analysis we exclude workers who remain on the job but transition between being hourly and salaried. To compare the wages of hourly and salaried workers, we make the assumption that all salaried workers work 40 hours per week. This assumption does not affect our wage change calculations given that we exclude workers who transition between hourly statuses; however, it is worth bearing in mind when we present statistics by employee wage percentile.

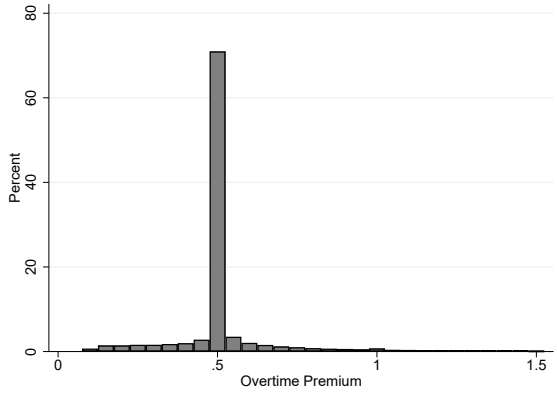
Appendix C.2 Overtime Pay

In addition to the base payment rate and gross earnings variables, the ADP data include four separate earnings variables and four separate hours variables, denoting subcategories of compensation. These earnings and hours variables represent base earnings, overtime pay, or some combination of the two. These variables are not required for ADP clients to input. As a result, their quality and coverage are not comparable to that of gross earnings or base per period payment rates. Nevertheless, we use these variables to attempt to distinguish between overtime pay where possible.[38]

To do so, we restrict our overtime imputation to those workers paid hourly. Given that the hours variables are essentially always set to 40 hours per week for salaried workers, we cannot separately distinguish overtime payments from bonuses and commissions for salaried workers. This implies that, by definition, we will have no overtime measures for salaried workers. For hourly workers, we infer overtime premiums implied using the hours and earnings subcategories. Specifically, we calculate implied base wages as base earnings divided by base hours, and overtime wage as overtime pay divided by overtime hours. The ratio of these implied wage rates to the administratively-recorded base wage provides a check on the validity of these implied wages. Most implied base wages, for instance, are exactly equal to the administrative base wage, and almost all lie between 1 and 1.1 times the contract

[38] When available, the sum of these four earnings variables plus a variable defined as "earnings not related to hours" is always equal to the administratively-recorded gross earnings variables in our sample. However, the composition of earnings type across these five earnings categories is measured with error.

Figure A3: Distribution of Overtime Premiums



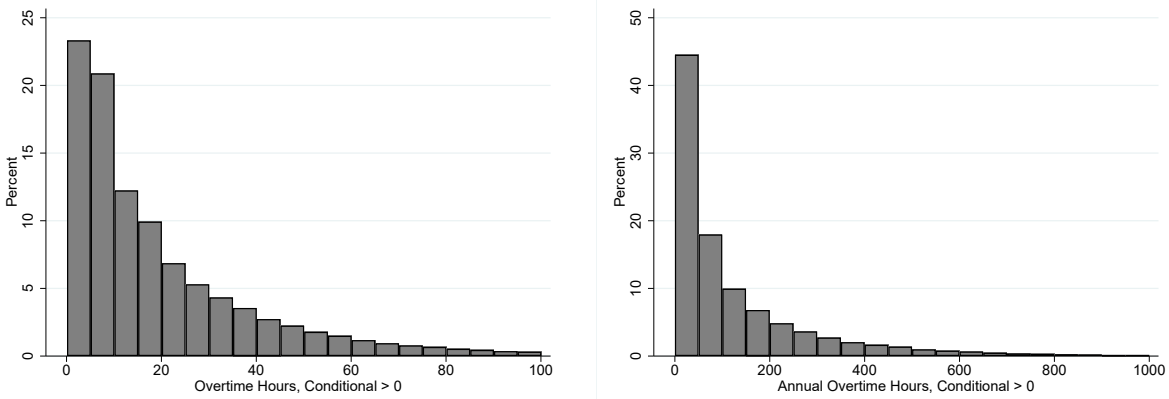
Notes: Figure shows the distribution of imputed overtime premiums for our employee sample for those with imputed overtime premiums greater than 1.1.

wage. Overtime wage rates, conversely, have large mass points at 1.5 times contract wages: about 80% of hourly workers with overtime premiums above 1.1 have implied wage rates which are 1.45-1.55 times their base wage with almost all of them exactly equal to 1.5. The distribution of our imputed overtime premium is shown in Appendix Figure A3.

If the overtime wage is no more than 1.1 times the base contract wage, we declare overtime earnings to be part of base pay - although the worker may have worked overtime, she did not see any increased wage as a result, and so overtime cannot be a source of wage adjustment. Next, we declare earnings in the overtime subcategory to be true overtime earnings for any worker whose imputed overtime wage is between 1.45 and 1.55 times their base earnings. There is no clear way to classify the remaining 20 percent of individuals with imputed overtime wages between 1.1 and 1.45 times their contract wage or those with overtime wages above 1.55 times their contract wage. As our base methodology, we include such earnings in our bonus measure. However, we also explored excluding those individuals from our sample all together when discussing the composition of compensation and cyclicality of compensation in Sections 3 and 4. Our key results were essentially identical under the two methods.

Appendix Figure A4 explores the heterogeneity in overtime hours worked more extensively. Focusing on those with positive overtime hours, Panel A shows that about one-quarter of those hourly workers with positive overtime hours work only between 1 and 5 hours of overtime during the month and just under half work less than 10 overtime hours. The median individual with positive overtime hours during the month is working about 11 extra hours during the month. However, there is a long right tail of overtime hours with about twenty percent of workers accruing over 40 additional overtime hours during the month. Panel B shows that 45 percent of all hourly workers who work overtime during a given year work less

Figure A4: Distribution of Overtime Hours for those With Positive Overtime Hours



Panel A: Monthly Overtime Hours Distribution

Panel B: Annual Overtime Hours Distribution

Notes: Figure shows the distribution of monthly hours of overtime worked (Panel A) and annual hours of overtime worked (Panel B) conditional on overtime hours being positive during the respective time periods. Both panels restrict the sample to only those workers paid hourly whose overtime premium is approximately 1.5 times their contract wage. See text for additional details. Panels B further restricts the sample to those who remain continuously employed with the same employer for 12 consecutive calendar months.

than 50 annual overtime hours. Again, most annual overtime recipients work very little overtime during the year. This is consistent with the low overtime share of earnings for hourly workers highlighted in Table 1 and Figure 1 of the main text. But, as with the monthly distribution, there is a long tail of overtime hours with one-quarter of annual overtime recipients working over 200 annual overtime hours per year. Additionally, overtime earnings are a small component of annual earnings. Half of all overtime recipients receive less than 2 percent of their annual earnings from overtime. Only about 5 percent of overtime recipients receive more than 10 percent of their annual earnings from overtime compensation.

Appendix D Robustness of Nominal Wage Adjustments for Job-Stayers

In this section, we present robustness exercises for our key results - the distribution of nominal wage adjustments for job-stayers.

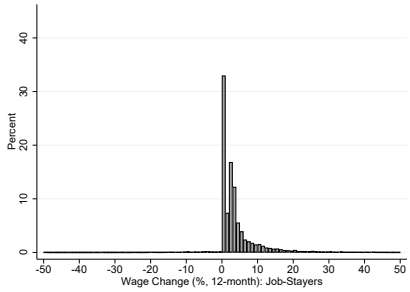
Appendix D.1 Similarity in Patterns across Compensation Arrangements

The patterns of nominal wage adjustments for job-stayers are fairly robust across workers who are compensated in different ways. The top panel of Figure A5 shows the patterns of nominal base wage adjustments separately for non-commission workers (left) and commission workers (right). The bottom panel shows similar patterns for non-commission workers who do not receive a bonus (left) and non-commission workers who do receive a bonus (right). All of those panels pool together hourly and salaried workers. The patterns are strikingly similar across the four groups. Notice that essentially none of the groups receive a nominal cut to their base wage. All groups have between 30 and 40 percent of workers receiving no nominal base wage adjustments during the 12-month period. Non-commission workers who receive an annual bonus are the *most* likely to get a nominal base wage increase during the year. These workers both receive a bonus and are more likely to receive a wage increase. As seen above, these workers are more likely to be high earning workers. Conversely, roughly 40 percent of commission workers receive no nominal base wage change during the year. Finally, the patterns of nominal base wage adjustment for workers who receive essentially all of their earnings from base pay – non-commission workers who do not receive a bonus – are nearly identical to the patterns for all workers highlighted in Figure 2.

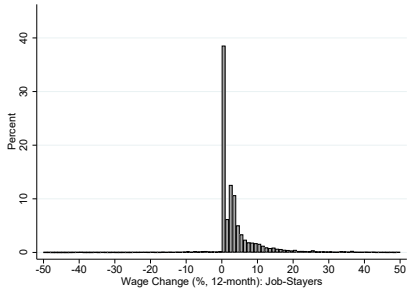
Figure A6 explores the extent to which nominal base wages are allocative. Specifically, we focus on our sample of hourly workers whose monthly hours worked fluctuates over the year. The number of pay weeks in the month varies over time, so we adjust our monthly hours for the number of pay periods making a measure of hours worked per week. We restrict the sample to only include those households whose hours worked per week varies substantively over the year.

The left hand panel of Figure A6 shows that wages are potentially allocative for these workers. Exploiting the panel nature of the data, we show that one-year base wage changes are associated with one-year hours worked changes, with an elasticity of 0.23. The right hand panel of the figure shows the one-year distribution of nominal base wage changes. It is nearly identical to the results shown in Figures 2 of the main text and Figure A5. Even for workers whose hours fluctuate, there are essentially no nominal base wage cuts and roughly one-third of workers do not receive a year-over-year nominal base wage increase.

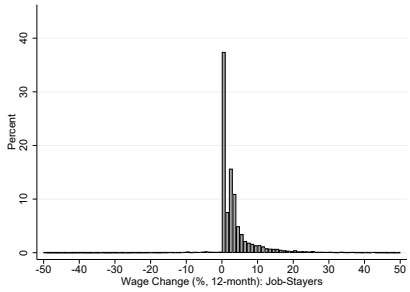
Figure A5: 12-month Changes in December Base Wages, 24-month Job-Stayers



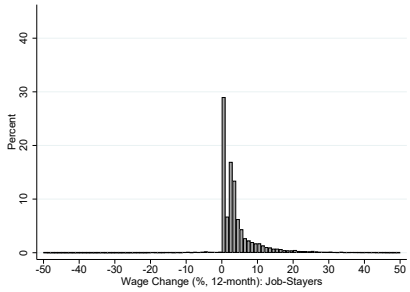
Panel A: Non-Commission in Year $t-1$



Panel B: With Commission in Year $t-1$



Panel C: No Bonus in Year $t-1$



Panel D: With Bonus in Year $t-1$

Notes: Figure plots the 12-month change in December contract wages between year $t-1$ and t for workers who remain on a job for at least 24 months. Panel A plots the distribution of changes for workers who do not work for commission in year $t-1$, while Panel B plots the distribution for commission workers in $t-1$. Panels C and D plot the distribution for workers who did and did not receive a bonus in year $t-1$, respectively, excluding workers who work for commission.

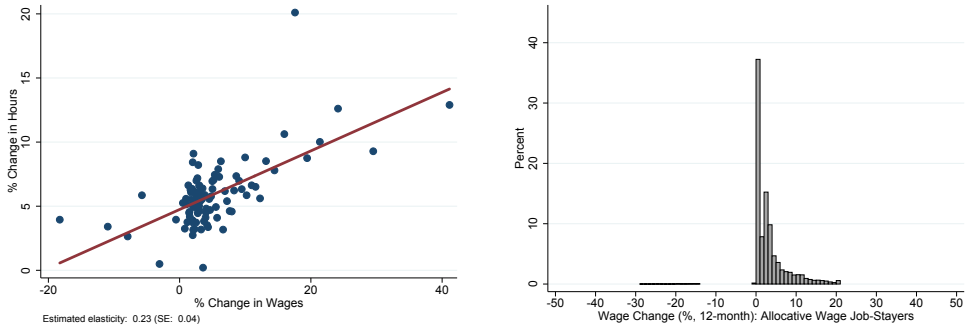
Appendix D.2 Higher Order Moments of the Base Wage Change Distribution

Table A3 shows higher order moments of the base wage change distribution for job stayers. In particular, we highlight both the skewness and kurtosis of the unconditional and conditional base wage change distribution.

Appendix D.3 Robustness to weighting and sampling

The analysis presented in the main text shows the distribution of wage changes for workers in large firms, weighted to match the firm size $\times$ industry mix implied by the Census' BDS. Since a firm in the ADP data is defined by a unique ADP client, our weighting procedure may introduce bias if, for instance, large firms are especially likely to have multiple sub-units each of which separately contracts with ADP. To explore the potential bias, we show some of key results without any additional weighting.

Figure A6: 12-month Base Wage Changes, Job-Stayers, Hourly Workers w/ Variable Hours



Panel A: Changes in Hours vs Changes in Wages

Panel B: Base Wage Change Distribution

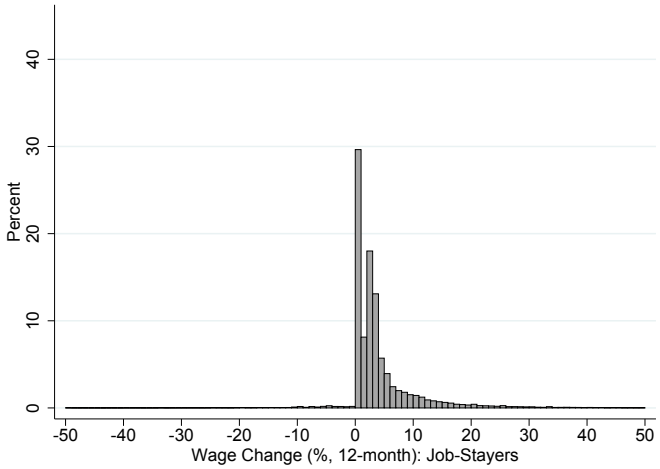
Notes: Figure shows results from a sub-sample of hourly workers whose weekly hours varies over the year and who remained continuously employed with the same firm during the 12-month period. We pool results over the entire 2008-2016 period. The left hand picture shows the relationship between the percent change in nominal base wages over the 12 months and the percent changes in hours worked. Each dot is a percentile of the wage change distribution. The right panel shows the distribution of the 12-month nominal base wage change.

Table A3: Base Wage Change Statistics, Pooled 2008-2016 Sample of Job-Stayers

	Monthly	Quarterly	Annual
Unconditional			
Skewness of Wage Changes (%)	9.4	5.0	2.7
Kurtosis of Wage Changes (%)	155.8	42.8	13.3
Conditional on Any Wage Change			
Skewness of Wage Changes (%)	2.0	1.9	2.2
Kurtosis of Wage Changes (%)	13.1	11.1	10.5

Notes: Table shows higher order moments of the wage change distribution for different horizons for a sample of job-stayers in the ADP data between 2008 and 2016. For this table, we use our employee sample and pool together hourly and salaried workers. All data are weighted to be nationally representative of sample of workers working in firms with more than 50 employees.

Figure A7: 12-month base wage change distributions for job-stayers: unweighted



Notes: Figure plots the unweighted distribution of 12-month base wage changes for our employee sample of job-stayers. Figure is analogous to Figure 2 of the main text except pooling over hourly and salaried workers and not weighting to match the BLS's firm-size by industry distribution.

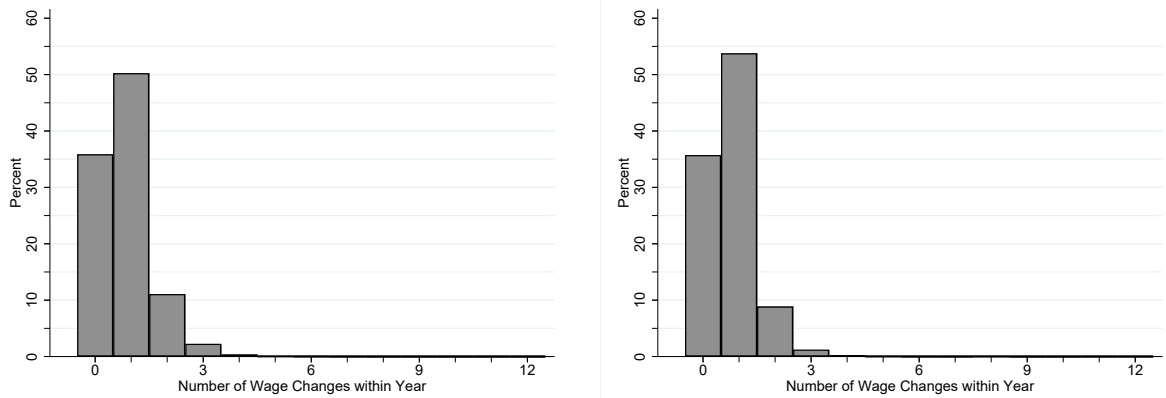
Figure A7 plots the unweighted distribution of 12-month base wage changes for job-stayers. The only difference between this figure and Figure 2 of the main text is that here we do not weight data in order to match the firm size × industry mix implied by the BDS. The patterns presented in this figure are almost identical to those in the main text, suggesting that our choice of weighting does not drive our results. While we only show this robustness for our base wage change results for job-stayers, the unweighted versions of other key results in the paper are also unchanged (e.g., bonuses, job-stayers, etc.).

The reason that our results are relatively insensitive to our weighting procedure is that the ADP data's firm size × industry mix is fairly representative of the US economy, and there are only relatively small differences in wage adjustment patterns across firm size and industry. We highlight this second fact in the next section.

Appendix D.4 Mean Base Wage Change Size by Time Since Last Base Wage Change

Figure 3 from the main text provides evidence of time dependence in base wage adjustment. The majority of base wage changes occur annually. However, basic models of purely time dependent wage setting often have predictions regarding the average size of wage changes. These models predict that under standard productivity processes with positive drift, individuals who are able to renegotiate their wage every month would negotiate smaller increases in their wages than those who renegotiate only once per year, on average. As a result, those

Figure A8: Number of Nominal Base Wage Changes over a Calendar Year, Job-Stayer Sample



Panel A: Hourly Workers Panel B: Salaried Workers

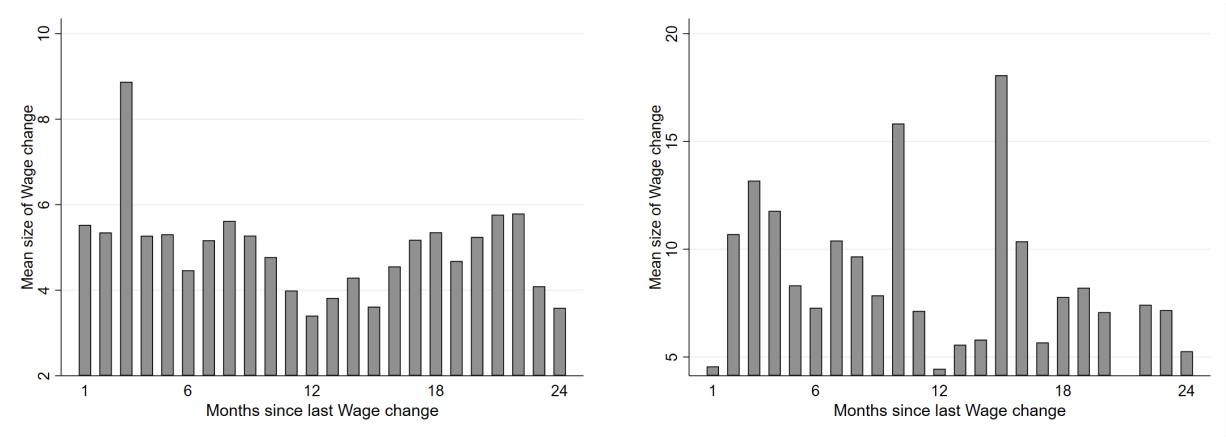
Note: Figure shows the distribution of the number of nominal base wage changes for hourly workers (left panel) and salaried workers (right panel) during a calendar year. We use our employee sample for this analysis and restrict our sample to those workers who remain continuously employed with the same firm during a 12-month calendar year. We use all data between 2008 and 2016.

who wait longer between wage changes should observe larger average changes in absolute value. We explore this prediction next.

Appendix Figure A8 plots the histogram of number of base wage changes during a given calendar year for workers in our full-year employee sample. Roughly 35 percent of job-stayers receive no base wage change during a 12-month period. Over 50 percent of both hourly and salaried workers receive exactly one base wage change during a 12-month period when they remained continuously on the job. Therefore, roughly 85 percent of job-stayers receive either zero or one nominal base wage change during a given year. Multiple nominal base wage changes within a year are rare for continuing employees.

Figure A9 shows the average size of the base wage change for job-stayers by the time since last base wage change. Since the vast majority of base wage changes for job-stayers are positive, this figure only includes workers who received a positive base wage change. While most base wage changes occur at 12-month frequencies, Figure A9 shows that the size of the base wage changes at these annual frequencies are much smaller than wage changes that occur at other times of the year. These predictions are not consistent with a standard Calvo (or Taylor) model at the individual level. However, the patterns could be consistent with a broader model of selection. If the workers who get these base wage changes that occur off-cycle are positively selected in some way, this could explain why they receive higher base wage increases. For example, if the worker receives an outside offer, the firm may have to raise the worker's base wage earlier than their annual cycle in order to retain the worker. Or, if a worker is promoted internally and the promotions are distributed throughout the year,

Figure A9: Mean Size of Base Wage Changes by Time Since Last Change, Job-Stayers



Panel A: Hourly Workers Panel B: Salaried Workers

Note: Figure shows the mean size of base wage increases for workers receiving a base wage increase t months after their last base wage change. Sample only includes individuals with at least two base wage changes. Additionally, we restrict our analysis to the job-stayer sample.

workers who receive a base wage change off cycle would also get larger base wage changes.

Appendix E Nominal Wage Adjustments for Job-Stayers by Firm Size and Industry

In this section, we document the extent to which wage adjustment varies by firm size. Additionally, we explore the potential bias in our key results from excluding firms with less than 50 employees from our analysis.

Appendix Figure A10 shows the probability of annual wage changes over the 2008-2016 period by firm size and industry. The top panel shows patterns for hourly workers while the bottom patterns for salaried workers. The figure shows that base wage changes are monotonically increasing in firm size for both hourly and salaried workers. In a given 12-month period, 64.4% of hourly workers and 66.9% of salaried workers in firms with under 500 employees receive a base wage change. The comparable numbers for firms with 5000+ employees are 80.2% and 77.1%, respectively. These results complement the finding in the literature documenting that workers receive higher wages in larger firms (Brown and Medoff, 1989). Not only are workers in large firms receiving higher wages, they also have a higher frequency of nominal base wage adjustments. All of the variation across firm size groups is in the propensity to receive a nominal base wage increase. While nominal base wage cuts are rare for all workers, there is no systematic variation in the propensity of a nominal base

wage cut with firm size. While there are differences in nominal base wage adjustment across firm size, the differences are relatively small. The small differences by firm size explain why our weighted results and unweighted results are so similar to each other.

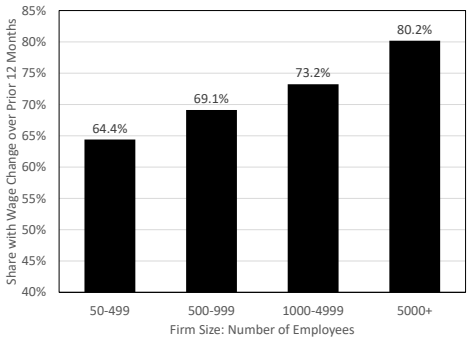
Appendix Figure A10 also shows that there is some degree of heterogeneity across industries with respect to base wage changes. For example, both hourly and salaried workers in the manufacturing industry are much more likely to receive a base wage change than workers in construction during our sample period. This is in part due to the differential cyclical patterns of construction workers documented in section 6. Again, while there are some differences across industries in the extent of nominal base wage adjustments, the differences are quantitatively small so that our weighted and unweighted results are not that different from each other.

In order to further study the influence of excluding small firms with less than 50 employees from our baseline analysis, we use an additional dataset from ADP. This dataset originates from a payment product which is primarily marketed to firms with less than 50 employees. The dataset begins in June 2013 and contains similar measures of base wages and gross earnings to our main dataset that covers the 2008-2016 period for firms with more than 50 employees. Appendix Figure A11 plots the distribution of 12-month base wage changes for job-stayers in this small firm sample for the period 2014-2016.[39] The patterns for small firms are qualitatively similar to our patterns for mid-size and larger firms - there remains a striking lack of wage cuts over a 12-month period among workers in small firms, as well as a substantial share of employees not receiving a wage change in a given year. Specifically, 48.7% of workers in small firms receive no wage change while 2.2% of workers receive a wage cut. As a reminder, the comparable numbers in firms with more than 50 employees were 34% and 2.5%. This findings are consistent with the results above that base wages adjust less frequently for workers in smaller firms.

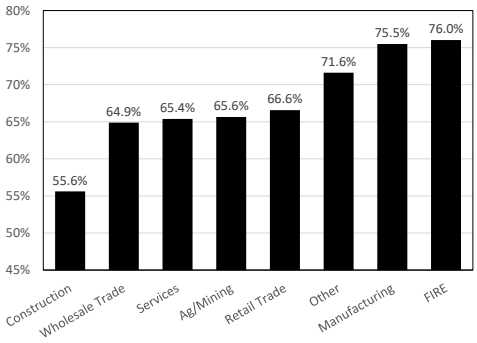
How much can the exclusion of small firms from our main analysis bias our results? The BDS shows that 27.1% of workers were employed in small firms in 2016. We can use the results in Appendix Figure A11 to compute a new measure of the probability of nominal base wage adjustments inclusive of workers in small firms. Accounting for small firms leads to a corrected annual probability of base wage change of 60.7% ($0.271 \times 51.3\% + (1 - 0.271) \times 64.3\%$), while the probability of a year-over-year base wage cut is 2.4% for all

[39] We do not use this small firm sample in our main analysis for three reasons. First, this dataset does not contain any information on overtime, nor sufficient information needed to construct bonus payments reliably. Second, we are unable to track workers as they move from the small firm to the large firm sample. As a result, the rarity of job-changing from small ADP firm to another small ADP firm confounds our ability to measures wage adjustment of job-changers. Finally, the lack of a sufficiently long time series precludes the study of state dependence in wage adjustment in this dataset.

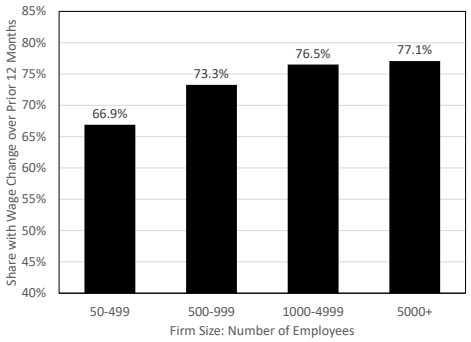
Figure A10: Share with Base Wage Change by Firm Size and Industry, All Years



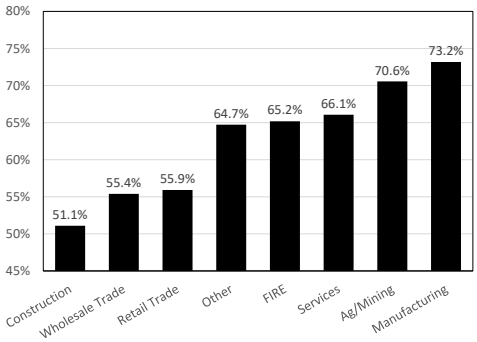
Panel A: Hourly Workers by Size



Panel B: Hourly Workers by Industry



Panel C: Salaried Workers by Size

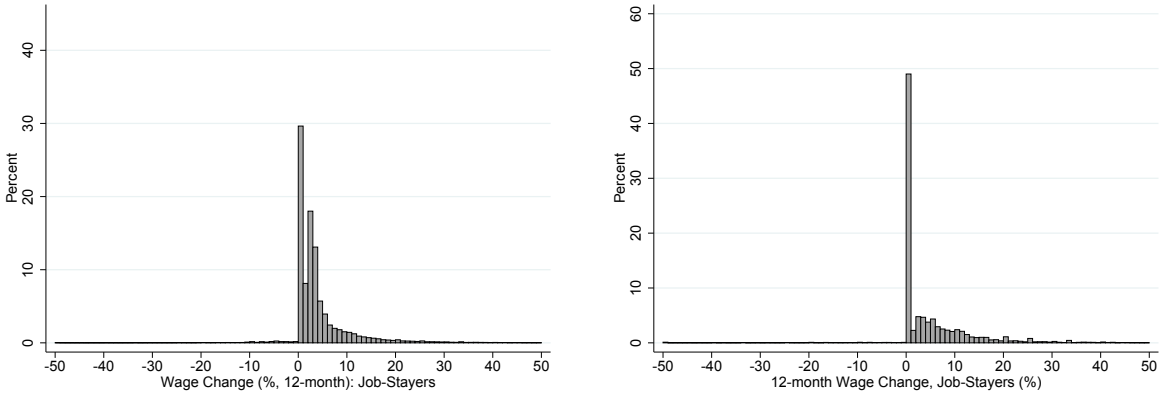


Panel D: Salaried Workers by Industry

Note: Figure shows the probability of receiving a base wage change over a 12-month period by firm size and industry for our employee sample of job-stayers in the ADP data between 2008 and 2016. For this figure, we use our employee sample, and separately plot the patterns for hourly workers (Panels A and B) and salaried workers (Panels C and D). All data are weighted to be nationally representative of sample of workers working in firms with more than 50 employees.

workers inclusive of those at small firms. Note, that these adjusted probabilities are very close to those reported in the main text including only workers in firms with more than 50 employees (64.3% vs. 60.7% and 2.5% vs. 2.4%). We conclude that the omission of workers in firms with less than 50 employees does not bias our results substantively.

Figure A11: 12-month base wage change distributions for job-stayers: firms with less than 50 employees



Panel A: ≥ 50 Employee Firms - Unweighted

Panel B: < 50 Employee Firms - Unweighted

Notes: Left panel of the figure plots the 12-month base wage change for job-stayers for a sample of workers employed in firms with more than 50 employees. For this figure, we show the raw unweighted data. Right panel of the figure plots the distribution of 12-month base wage changes for job-stayers for a sample of workers employed in firms with less than 50 employees, The results in this figure are produced using an ADP data product which covers the period Jan 2014 through December 2016 and only includes firms with less than 50 employees. The data in this figure are also otherwise unweighted.

Appendix F Robustness of Cyclicality Regressions

In the top panel of Appendix Table A4 we ask whether the intensive margin of overtime and bonuses varies with changes in state unemployment. The regression in Panel A is the same as the cyclicality regression in the main text except that y_{ijst} is the log of overtime hours (columns 1 and 2 for those with positive overtime hours) and the log of bonus payments (columns 3 and 4for those with positive bonuses). Columns 1 and 3 exclude individual fixed effects while columns 2 and 4 include individual fixed effects. Regardless of individual fixed effects, the amount of overtime hours (conditional on overtime hours existing) and the size of bonuses (conditional on bonuses being positive) are relatively acyclical.

The bottom panel of Appendix Table A4 explores the cyclicality of overtime receipt across industries. The first two columns restrict the sample to those workers in manufacturing industries, while the second two columns consider those in non-manufacturing industries. For each industry subset, we highlight results without and with individual fixed effects. The dependent variable for these regressions is the propensity to receive overtime hours. Otherwise, the regressions are the same as in the top panel. We split the sample by industry since the BLS tracks annual manufacturing hours. The BLS data highlight that overtime hours within the manufacturing sector are pro-cyclical. The BLS data do not control for

Table A4: Robustness of Cyclicality of Various Forms of Compensation

Panel A: Cyclicality of Compensation Components				
	Log Overtime (1)	Log Overtime (2)	Log Bonus (3)	Log Bonus (4)
Δ Unemployment Rate (%)	-0.04	-0.02	-0.02	-0.01
	(0.01)	(0.00)	(0.01)	(0.00)
Workers Included	Hourly	Hourly	All	All
State and Industry FE	Y	Y	Y	Y
Individual FE	N	Y	N	Y
Observations (000s)	273	273	348	348
Mean of Dep. Var.	3.95	3.95	7.95	7.95

Panel B: Cyclicality of Overtime for Manufacturing and Non-Manufacturing Sectors				
	Manufacturing		Non-Manufacturing	
	% With Overtime (1)	% With Overtime (2)	% With Overtime (3)	% With Overtime (4)
Δ Unemployment Rate (%)	-0.82	-0.54	-0.54	-0.19
	(0.23)	(0.13)	(0.29)	(0.13)
Workers Included	Hourly	Hourly	Hourly	Hourly
State and Industry FE	Y	Y	Y	Y
Individual FE	N	Y	N	Y
Observations (000s)	94	94	320	320
Mean of Dep. Var.	74.5	74.5	59.9	59.9

Notes: Table reports robustness specifications for Table 2 of the main text. The top panel is the same as the top panel of Table 2 of the main text except we exclude individual fixed effects from all regression columns. The bottom panel shows overtime receipt cyclicality separately for workers in the manufacturing and non-manufacturing sectors (with and without individual fixed effects). Otherwise, the regressions are the same as show in Table 2 of the main text. See main text for additional details.

individual fixed effects. As seen from columns 1 and 3 of Panel B, the propensity to receive overtime is slight more pro-cyclical in manufacturing than in non-manufacturing industries without controlling for individual fixed effects. However, as seen in column 2, controlling for individual fixed effects explains about a third of the observed pro-cyclicality of overtime hours within the manufacturing sector. Even with individual fixed effects, overtime remains statistically significantly pro-cyclical in the manufacturing sector with a coefficient of -0.54 and standard error of 0.13. However, within the non-manufacturing sectors – which make

Table A5: Annual Persistence of Base Wage, Bonuses, and Overtime, sample of full-year job-stayers, 2009-2016

	Log December Base Wage (1)	% With Bonus (2)	Log Annual Bonus (3)	% With Overtime (4)	Log Annual Overtime Hours (5)
y_{it-1}	0.82	0.57	-0.02	0.72	0.05
	(0.01)	(0.00)	(0.02)	(0.01)	(0.02)
Observations (000s)	463	463	115	307	235
Individual FE	Y	N	Y	N	Y
Workers Included	All	All	All	Hourly	Hourly

Notes: Table reports OLS-estimated AR(1) coefficients from appendix equation (A1). White heteroskedasticity robust standard errors clustered at the employee-level reported in parentheses. We use the employee sample restricting our analysis to workers who remain continuously employed with the same firm for two consecutive calendar years.

up the bulk of employment – there is no significant relationship between the propensity to work overtime and unemployment rates once controlling for individual fixed effects. These results highlight that even if overtime is procyclical in the manufacturing sector, the same patterns do not exist in other sectors after controlling for individual fixed effects.

Appendix G Persistence Regressions

In this section of the Online Appendix, we estimate autocorrelation coefficients for our various components of compensation. To do so, we take advantage of within worker wage dynamics within the same job. Specifically, we estimate OLS regressions of the form:

$$y_{it} = \rho y_{it-1} + \alpha_i + \epsilon_{it} \tag{A1}$$

where i indexes a worker-firm pair, t represents a year and α_i is a worker-job fixed effect. y_{it} represents the value of a particular compensation measure for job i in year t. For this exercise we use our sample of workers who remain continuously employed on the same job (job-stayers) for two consecutive calendar years, in order to measure annual adjustments in bonus and overtime compensation.

Appendix Table A5 reports the estimated autocorrelation coefficient ρ of various compensation measures at the individual level. We explore the persistence of log base wages (first column) and various specifications of overtime and bonus receipt (columns 2 through 5). As seen from the table, base wages are highly persistent with an annual autocorrelation

of 0.82. Columns 2 and 4 show that that propensity to accrue overtime hours and to receive bonuses, respectively, are also highly persistent over time. If a worker receives a bonus this year, they are very likely to receive a bonus next year. Likewise, people who accrue overtime this year are also likely to accrue overtime next year. However, the amount of overtime compensation and the amount of bonus receipt, conditional on receipt, are essentially i.i.d.. Indeed regressing the share of pay in bonuses and overtime (not shown) on its lagged value yields a negative coefficient, suggesting that high bonus years tend to be followed by low bonus years. These results suggest that base wage adjustments may be a far better measure of permanent wage adjustments than are bonus payments. Given this, the ability to adjust base wages is likely more important for changes in the user cost of labor than the ability to adjust bonuses.