# 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. ${ }^{1}$ 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. ${ }^{2}$ The potential sluggish response of firms' marginal cost of labor has important allocative effects in many models. For example, to capture the stickiness observed in aggregate wages, many macroeconomic models rely on exogenously specified nominal wage rigidity. ${ }^{3}$ However, the literature using micro data to document nominal wage adjustment is somewhat underdeveloped. This stands in stark contrast to the large literature which has developed using micro data to establish key moments of output price adjustment, which have guided theories of nominal price rigidities. ${ }^{4}$ 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 contributor both to why wages did not fall more during the Great Recession and why they did not increase at a faster rate 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

[^1]is a major reason why the literature measuring nominal wage adjustment in micro data has remained underdeveloped. Few datasets separately identify the various components 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. 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 (henceforth known as ADP) - 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. during 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 example, for ten percent of workers, non-base pay accounts for $10 \%$ 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 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 then show that the adjustment of base wages exhibits strong evidence of downward nominal 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 $2.4 \%$ 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 that nominal wage adjustment exhibits strong evidence of both time and state dependence in the micro data. 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, 6.6 percent of salaried 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. 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.

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 cyclical than those of job-stayers (Bils, 1985). We replicate this in our data - approximately $40 \%$ of job-changers receive a wage cut on job switch with hardly any seeing no 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. ${ }^{5}$ To 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, and skill (as proxied by lagged wages). The matching estimator reveals that new hire 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 Hazell and Taska (2019) which documents that posted wages on the near universe of online job boards display similar adjustment patterns as the base wage changes of existing workers.

Although the data show signs of downward nominal base wage rigidity, this does not imply that worker 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 20 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 workers' 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 fluctuations in aggregate hourly earnings for a given worker. ${ }^{6}$

To summarize, our paper has five principal contributions. First, we document that fluctuations in base wages are the best proxy for contract rigidity because they have the largest impact on the model-relevant outcome of wage rigidity: the cyclicality of marginal costs. Second, we provide high quality measurement of base wage adjustment for a large share of the U.S. workforce, and highlight exceptional asymmetry in base wage adjustment suggesting the presence of downward nominal wage rigidities. Third, we provide new evidence of state and time dependence in the adjustment of base wages. Fourth, we show that new hires' wages are no more flexible than those of job-stayers, so that job-stayer base wages are a sufficient statistic for wage rigidity. Finally, we show that the various margins of adjustment outside of the base wages of job-stayers give rise to large compensation flexibility,

[^2]suggesting that downward nominal base wage rigidity does not preclude downward earnings adjustments; such adjustment, however, do not vary cyclically. Throughout the paper, we compare and contrast our findings with the existing literature.

## 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 management, 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 starts 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 worked during the month. For hourly workers, the exact number of hours worked 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

[^3]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 three random subsamples of the full data. The first chooses one million unique employees, and follows them through their entire tenure in the sample across all firms for which they work. This is the primary dataset for analysis. Second, we separately draw a sample of 1 million workers who change jobs during our sample period ("job-changers"). These are workers who show up in multiple firms during their time in the ADP database. This will allow us to explore fully the patterns of wage changes for workers who switch between ADP firms including 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-level, job-changer and firm-level subsamples remain large, with roughly 25 million, 27 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 the distribution of firm size from the Census' Business Dynamic Statistics (BDS) during similar years. Conditional on having at least 50 employees, we find that the ADP sample underrepresents 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

[^4]for firms with greater than 50 employees. ${ }^{9}$ 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 robustness of our sample along demographic dimensions (by comparing to 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 data set 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 workers per-period contract wage as their "base" wage. For workers who are paid hourly, this is the workers' 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 worked each day (i.e., 8 hours) 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 workers' administratively entered hourly wage times the hours they worked determines their per pay-period base wages in their paycheck.

All salaried workers have an administrative field recording their weekly, bi-weekly or

[^5]monthly contracted salary rate depending on the frequency of their pay period. 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 workers' base contact wage, we also observe separate administrative records of a workers' "monthly gross earnings." 10 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. ${ }^{11}$ 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 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 wage 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

[^6]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. ${ }^{12}$ 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.

Second, we define large residual earnings to be any residual earnings net of overtime premiums that accrue to a worker in a given month that exceeds $1 \%$ 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 frequency 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 10 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

[^7]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 30 percent of noncommission 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 shows 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 2.5 percent of their annual earnings from sources other than their base pay. This suggests that for most workers, base pay is their primary form of compensation. However, for some 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 10 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 stems from the small amounts of monthly residual earnings that we do not classify either as bonuses or overtime. 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 for non-commission workers. We pool together

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

|  | All |  | Hourly |  |  | Salaried |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Share | Share | Share | Share | Share | Share | Share |
|  | Base | Base+ | Base | Base+ | Base+ | Base | Base+ |
| $10^{\text {th }}$ percentile | 89.8\% | 97.8\% | 91.6\% | 92.9\% | 98.6\% | 86.5\% | 96.0\% |
| $25^{\text {th }}$ percentile | 93.8\% | 99.4\% | 94.8\% | 95.8\% | 99.6\% | 91.6\% | 99.0\% |
| Median | 97.5\% | 100\% | 97.8\% | 98.5\% | 100\% | 96.5\% | 100\% |
| $75^{\text {th }}$ percentile | 99.7\% | 100\% | 99.8\% | 100\% | 100\% | 100\% | 100\% |
| $90^{\text {th }}$ 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) | 619 | 619 | 387 | 387 | 387 | 222 | 222 |

Notes: Table shows the distribution of the share of worker 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 who are paid hourly and salaried. ${ }^{13}$ 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 $37 \%$ of hourly employees accrue overtime hours. Focusing on a sample of hourly employees who works continuously with the same employer during 12 consecutive calendar months, y percent of these individuals 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 are working much less than 40 hours a week which means they are far from

[^8]Figure 1: Bonus and Overtime Share by Employee Base Wage Percentile, Full-Year JobStayers, Excluding Commission 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. Commission workers are excluded. Panel A includes all workers while Panel B includes only workers paid hourly.
the overtime threshold. As base wages increases, the share of overtime earnings increases up through around the 50th percentile. For the median percentile of the base wage distribution for hourly workers, only $2.5 \%$ of annual earnings comes from overtime. After the 50th percentile, the share of earnings in overtime falls monotonically. ${ }^{14}$

Figure 2 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 about just under half work less than 10 overtime hours per month. 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 fifty percent of all hourly workers who work overtime during a given year work less 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. But, as with the monthly distribution, there

[^9]Figure 2: Distribution of Overtime Hours and Share of Earnings for those With Positive Overtime Hours

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.

## 4 The Cyclicality of Different Wage Concepts

Wage rigidity is commonly employed in modern macro models in order to generate a muted cyclical movements in firms' marginal cost. ${ }^{15}$ 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

[^10]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 constitute the only relevant margin for the cyclical adjustment of firms' marginal cost, and are thus the relevant notion for wage rigidity. In this section, we study the cyclical patterns of each compensation component separately in order to inform our choice of compensation components when we turn to 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 payments represent an upward shift in the marginal cost schedule. Suppose a worker earns overtime for working over 40 hours per week. 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 affects 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 has no additional influence on the worker's marginal cost, but does affect the average cost of employment, which may be important for models in which, for example, financial frictions render the average cost of employment allocative (Schoefer, 2016).

For salaried workers, the base wage is less clearly a marginal cost notion for a given worker. 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 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:

$$
\begin{equation*}
y_{i j s t}=\alpha+\beta \Delta U_{s t}+\gamma_{i}+\gamma_{s}+\gamma_{j}+\Gamma X_{i t}+\epsilon_{i j s t} \tag{1}
\end{equation*}
$$

where $y_{i j s t}$ 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 $\left(\gamma_{s}\right)$ and industry fixed effects $\left(\gamma_{j}\right)$. Some measures of $y_{i j s t}$ 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, our specification includes individual fixed effects $\gamma_{i} .{ }^{16}$ Our key coefficient of interest is $\beta$ which measures how the various components of compensation respond to changes in the state unemployment rate between period $t-1$ and $t\left(\Delta U_{s t}\right)$. The regressions also include a vector of additional individual controls, $X_{i t}$, that can vary over time. These controls include the individual's age, sex, tenure, whether they work hourly, their payment frequency and firm size.

Panel A of Table 2 documents the cyclicality of various compensation components individually. 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, $y_{i j s t}$ 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 0.39 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 2.0 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. For these regressions, we restrict our sample to those individuals who are paid hourly and who

[^11]Table 2: Cyclicality of Various Forms of Compensation

| Panel A: Cyclicality of Compensation Components |  |  |  |  |  |
| :--- | :---: | :---: | :---: | :---: | :---: |
|  | \% Base Wage <br> Change | \% With <br> Overtime | Log | Overtime | With |
|  | $(1)$ | $(2)$ | $(3)$ | $(4)$ | Bonus |
|  | -0.39 | -0.16 | -0.02 | -0.54 | Bonus |
|  | $(0.02)$ | $(0.15)$ | $(0.01)$ | $(0.12)$ | $(0.00)$ |
| $\Delta$ Unemp. Rate (p.p.) |  |  |  |  |  |
|  |  | Y | Y | Y | Y |
| State and Industry FE | N | Y | Y | Y | Y |
| Individual FE | All | Hourly | Hourly | All | All |
| Workers Included | 450 | 267 | 174 | 486 | 203 |
| Observations (000s) | 3.85 | 62.3 | 3.96 | 40.3 | 8.27 |

Panel B: Cyclicality of Annual Compensation Per Hour

|  | \% Base | \%Base | \%Base | \%Base Plus | \%Base <br> + Bonus <br> Earnings |
| :--- | :---: | :---: | :---: | :---: | :---: |
|  | Carnings | Earnings | Overtime | Overtime |  |
|  | Change | Change | Change | Change | Change |
|  | $(1)$ | $(2)$ | $(3)$ | $(4)$ | $(5)$ |
| $\Delta$ Unemp. Rate (p.p.) | -0.67 | -0.71 | -0.64 | -0.55 | -0.63 |
|  | $(0.10)$ | $(0.11)$ | $(0.11)$ | $(0.11)$ | $(0.11)$ |
|  |  |  |  |  |  |
| State and Industry FE | Y | Y | Y | Y | Y |
| Individual FE | N | N | N | N | N |
| Workers Included | All | Salaried | Hourly | Hourly | All |
| Observations | 175 | 81 | 94 | 94 | 175 |
| Mean of Dep. Var. | 3.87 | 4.05 | 3.73 | 3.76 | 4.42 |

remain consistently employed with the same employer for 12 consecutive calendar months. Column 2 measures $y_{i j s t}$ as the percent of hourly workers who received overtime payments at any point during the calendar year. Column 3 measures $y_{i j s t}$ as the $\log$ of annual overtime hours conditional on overtime hours being positive. Controlling for individual fixed effects, we find that both the propensity to receive overtime and conditional log overtime hours are essentially acyclical. A one percentage point increase in the unemployment rate is found to decrease the propensity to receive overtime benefits by only 0.16 percentage points for hourly workers. To put this number in perspective, over the whole sample period, 63 percent of hourly workers receive positive overtime hours during the year.

This result may seem surprising given that there is evidence that that overtime hours in
manufacturing are procyclical. The US Bureau of Labor Statistics (BLS) records the average weekly overtime hours for production and nonsupervisory 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 $t$ over the 1956-2018 period yields a coefficient on the change in unemployment rate of 0.34 (with a standard error of 0.02 ). This implies a 1 percentage point increase in the unemployment rate reduces weekly overtime hours by about one-third of an hour. As we highlight in greater detail in the online appendix, there are three main reasons why we are finding relatively acyclical overtime movements in the ADP data. First, as highlighted above, even the cyclical variation in the BLS overtime data is quantitatively very small. Second, and more importantly, much of the cyclical variation in overtime hours in the BLS data that does exist is likely due to the changing compositional effect of workers over the business cycle. Specifically, we find procyclical effects of overtime hours in the ADP data (similar to that in the aggregate BLS data) when we exclude the individual fixed effects from our regressions. Finally, we show that the patterns for the non-manufacturing sector, conditional on individual fixed effects, differs from the manufacturing sector. The manufacturing sector, conditional on individual fixed effects, still has procyclical overtime hours. However, no such patterns exist in the non-manufacturing sector. Collectively, these results suggests that high overtime workers during normal times are the ones who exit the firm during recessions. Once accounting for this selection, overtime hours move from slightly procyclical to essentially acyclical in the ADP data particularly for the non-manufacturing sectors.

Columns 4 and 5 of Panel A of Table 2 examine the cyclicality of bonuses. These regressions restrict the sample to hourly and salaried workers who remained consistently employed with the same employer for 12 consecutive calendar months. Column 4 measures the percent of workers who received a bonus at any point during the calendar year while column 5 measures the log of annual bonuses conditional on positive bonus receipt. Bonus receipt is slightly countercyclical with a one percentage point increase in the unemployment rate increasing the fraction of workers receiving a bonus by 1 percentage point. This effect is relatively small in that during our entire sample 40 percent of workers receive an annual bonus. 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). ${ }^{17}$

[^12]To further help assess whether accounting for overtime payments and bonuses changes the cyclicality of individual compensation in any quantitatively meaningful sense, we report one final set of preferred results 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 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 in Table 1 and Panel A of Table 2. All regressions in Panel B impose this restriction.

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 rate in contract earnings. The extent to which the results in column 1 of Panel B differs from the results in column 1 of Panel A stems from both the differences in samples between the two panels and from time aggregation. In Panel B, we are conditioning 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. But, more importantly, Panel B measures wage changes over 24 months. 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 -0.67 percentage point decline in the growth rate of annual base wages per hour. Columns 2 and 3 of Panel B show the results separately for hourly and salaried workers. The semi-elasticity is slightly larger for salaried workers than for hourly workers ( -0.71 vs. -0.64 ). In all cases, base wages are highly procyclical.

In columns 4 and 5 we make broader measures of worker per hour compensation inclusive of overtime and bonuses. In column 4, we sum together annual base earnings base earnings plus annual overtime earnings for hourly workers and divide by annual hours worked. We then take the annual growth rate in per hour base plus overtime compensation as our measure of $y_{i j s t}$. In column 5, we sum together annual base earnings plus overtime plus bonuses for all
workers and divide that by annual hours worked. The goal of these last two regressions 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 slighlty dampens the cyclicality of base wages. These results stem from the fact that both overtime and bonus compensation is quantitatively small for most workers and from the fact that these sources of compensation are essentially acyclical. ${ }^{18}$

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 rigities.

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 therefore should seek to match sluggish 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.

[^13]
## 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 3 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 - $33 \%$ 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 2.4 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 $3.6 \%$ received a nominal base wage cut $(2.4 / 66)$.

Figure 3: 12-Month Nominal Base Wage Change Distribution, Job-Stayers


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.

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

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

|  | All Workers <br> $(1)$ | Hourly <br> $(2)$ | Salaried <br> $(3)$ |
| :--- | :---: | :---: | :---: |
| Annual |  |  |  |
| $\quad$ Probability of Positive Base Wage Change (\%) | 63.9 | 65.3 | 61.6 |
| Probability of Negative Base Wage Change (\%) | 2.4 | 1.8 | 3.6 |
|  |  |  |  |
| Quarterly <br> Probability of Positive Base Wage Change (\%) <br> Probability of Negative Base Wage Change (\%) | 18.5 | 19.5 | 16.7 |
|  |  |  |  |
| Monthly | 0.9 | 0.7 | 1.3 |
| Probability of Positive Base Wage Change (\%) |  |  |  |
| Probability of Negative Base Wage Change (\%) | 0.3 | 6.6 | 5.8 |
| Notes: | 0.6 | 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.
frequencies correspond to the data underlying Figure 3. While the asymmetry between nominal wage increases and nominal wage cuts is a feature of many existing empirical papers (see, e.g. Lebow et al. (2003); Kahn (1997); Card and Hyslop (1997)), the results in Figure 3 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 3 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 noncommission 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 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,

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

|  | Monthly | Quarterly | Annual |
| :--- | :---: | :---: | :---: |
| Mean Unconditional Change (\%) | 0.3 | 1.0 | 3.9 |
| Median Unconditional Change (\%) | 0.0 | 0.0 | 2.4 |
| Standard Deviation of Unconditional Change (\%) | 2.6 | 3.7 | 6.5 |
|  |  |  |  |
| Mean Conditional Change (\%) | 5.2 | 5.1 | 5.8 |
| Median Conditional Change (\%) | 3.1 | 3.1 | 3.4 |
| Standard Deviation of Conditional Change (\%) | 8.0 | 6.5 | 7.0 |
| Mean Change, Conditional on Positive (\%) |  | 6.2 | 5.7 |
| Median Change, Conditional on Positive (\%) | 3.3 | 3.3 | 6.3 |
| Stan. Dev. Change, Conditional on Positive (\%) | 7.7 | 6.4 | 7.0 |
|  |  |  |  |
| Mean Change, Conditional on Negative (\%) | -10.7 | -8.7 | -7.3 |
| Median Change, Conditional on Negative (\%) | -8.3 | -7.7 | -6.6 |
| Stan. Dev. Change, Conditional on Negative (\%) | 8.1 | 5.8 | 4.6 |

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.
we only report results pooling both hourly and salaried workers given the frequency of adjustment distributions were similar between the two groups. ${ }^{19}$ During this period, mean and median nominal base wage growth for workers who remain on the same job equaled 3.9 percent and 2.4 percent, respectively. ${ }^{20}$ 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 6.5 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 6.3 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

[^14]both around 7 percent. While the frequency of base wage increases is much higher than wage cuts, the mean size of a base wage increase is very similar to the mean size of a cut.

## 6 Time and State Dependence in Nominal Wage Adjustment

In the this section, we 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 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 this subsection, we use our detailed micro data to explore evidence of time dependence in wage adjustment for our sample of job-stayers.

Figure 4 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 90 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.

To formally study time dependence in wage setting, Figure 5 plots the hazard functions of base wage adjustment for the subset of job-staying employees who experience at least two 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 the worker surviving to month $t$ without a base wage change at the same firm.

The figure shows evidence rejecting the Calvo 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

Figure 4: Number of Nominal Base Wage Changes over a Calendar Year, Job-Stayer Sample


## Panel A: Hourly Workers



Panel B: Salaried Workers

Note: Table shows the average 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 and average over the calendar years.

Figure 5: Hazard Function of Base Wage Change, Job-Stayers

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. ${ }^{21}$

Figure 6 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 of all 12 months during a given calendar year. Then, 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 simple average probability of a worker receiving a base wage change across firm-year pairs for each ranked month. ${ }^{22}$

The figure shows that when a firm adjusts base wages, it tends adjust many wages during one particular month of a given year. For example, a typical firm adjusts 50 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 suggest that firms do most of their base wage changes in one month out of the year. ${ }^{23}$ 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 are occurring 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 macro economy if firms stagger the months in which they adjust wages. Indeed, this is the underlying intuition behind the 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.

[^15]Figure 6: 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 then 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 then rank the months within a given firm-year pair from highest month of base wage changes to lowest month of base wage changes. We then take the simple average across all firm-year pairs for each month rank. When making the figure, 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.

Figure 7 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. The figure shows some slight seasonality in the data. The probability that a worker receives a base wage change 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 change in the first quarter of the year while 21.1 and 21.5 percent of workers receive a base wage change in the second and third quarters. Only 16.6 percent of workers receive a base wage change in the last quarter of the year.

Overall, the evidence presented in this section shows strong evidence of time dependence in base wage adjustment. The majority of base wage changes occur annually, usually at the beginning of a firm's fiscal year - either in January, April, or July. However, 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. However, as we document next, base wage adjustment also displays evidence of state dependence.

Figure 7: Seasonality in Base Wage Changes, Job-Stayers, All Years


Note: 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. This figure combines hourly and salaried workers.

### 6.2 State Dependence in Nominal Wage Adjustments

There are two principal reasons why one might observe state dependence in nominal wage changes for job-stayers. The first is if there is some explicit cost for firms when adjusting the wages of existing workers. 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 price 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 for job-stayers 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 concerns over morale or because of union pressure. 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 would then appear less flexible in downturns than in booms.

Figure 8 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 8 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 8, the probability of base wage adjustments for jobstayers exhibit striking pro-cyclicality. Only about 55 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 8 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 9 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 remained on 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

Figure 8: Time Series of Nominal Base Wage Adjustments, Job-Stayers


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 the propensity for wage increases (left axis) and the propensity for wage decreases (right axis).
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.

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.

Figure 9: 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.

## 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 in many models (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 confirm that job-changers' wages are more cyclical than those of job-stayers across a variety of wage notions and present the distribution of wage changes for job-switchers, finding that a substantial share of job-switchers receive wage cuts. Next, we develop a matching method to control for 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 that of similar jobstayers. 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. As discussed when defining the sample in detail in the online appendix, a limitation of the job-changer sample is 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. Throughout, we only include individuals who either remain an hourly worker or remain a salaried worker.

Figure 10 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 3. First, essentially all job-changers receive a base wage change over a given year. Only about 6.5 percent of hourly job-changers and 3.3 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 $39.2 \%$ of workers paid hourly and $31.2 \%$ 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 8.0 and 4.6 percent respectively. Base wage growth is much larger on average for job-changers than it is for job-stayers. Moreover, the standard deviation of annual nominal base wage changes for job-changers is 29.3 percent almost 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 jobchanger samples and estimate the following regression with OLS:

$$
\begin{equation*}
\% \Delta w_{i j s t}=\alpha_{0}+\beta_{0} \Delta U_{s t}+\beta_{1} \Delta U_{s t} \times \operatorname{Switcher}_{i t}+\beta_{2} \cdot \operatorname{Switcher}_{i t}+\Gamma X_{i t}+\theta_{j}+\epsilon_{i j s t} \tag{2}
\end{equation*}
$$

where $i$ indexes an individual, $t$ a month, $s$ the state that $i$ lives in in $t$, and $j$ represents $i$ 's firm as of date $t$. As before, $U_{s t}$ is the unemployment rate, in percentage points, in state $s$ in

[^16]Figure 10: 12-Month Base Wage Change Distribution for Job-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.
period $t, \Delta Z$ is an operator considering the difference in a variable $Z$ between $t$ and $t+12$, $w_{i j s t}$ is worker $i$ 's base wage in period $t$, and Switcher $_{i t}$ is an indicator equal to 1 if worker $i$ switches employers between months $t$ and $t+12$. We include a vector of controls $X_{i t}$ 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 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 $\theta_{j}$. The coefficients of interest are $\beta_{0}$, which reports the cyclicality of job-stayer wages, and $\beta_{1}$, which represents the excess wage cyclicality for job-changers. ${ }^{25}$

The results of this regression are reported in Columns (1)-(4) of Table 5. Standard errors, clustered at the month level, are reported in parentheses below the estimated coefficients. Column (1) excludes all control variables and simply constructs the raw linear relationship between wage growth and state level unemployment. This column shows that job-stayers' base wages are mildly pro-cyclical, with a 1 percentage point increase in unemployment rates being associated with a 0.27 percent decline in wage growth for job-stayers. However, job-changers' base wages are substantially more pro-cyclical: those who switch jobs exhibit approximately 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 $10 \%$ level.

[^17]Column (2) additionally includes age, sex, and wage percentile controls. Adding these controls uncovers that job-stayers' is 0.30 percentage points lower for every one percentage point increase in unemployment rates, while job-changers still see excess wage growth cyclicality of approximately 0.74 percent. This 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 down to a difference in sample period, or because Gertler et al. only include men, while we include the full economy.

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. $(1994,1997)$, and more recently by 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 of the selection in the jobs workers move to over the course of a cycle, Column (3) adds destination firm fixed effects to the regression. Doing so reduces the coefficient on unemployment rate changes for job-stayers wages to 0.21 , but inflates excess wage cyclicality for job switchers, which we now estimate to be 0.83 . This suggests that workers leave firms that decrease wages by more during recessions. Finally, column (4) 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 0.32 , but excess cyclicality falls to 0.67 , suggesting that the movement of particular types of worker to different types of firm is part of the reason why job-changer wages are more cyclical.

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 and after conditioning on fixed characteristics of employees and firms.

Ideally, we seek to compare a job-changer to a counterfactual version of herself in which

Table 5: Cyclicality of New Hire Wages

|  | $(1)$ | $(2)$ | $(3)$ | $(4)$ | $(5)$ |
| :--- | :---: | :---: | :---: | :---: | :---: |
| $\Delta U_{s t}$ (pp.) | -0.27 | -0.30 | -0.21 | -0.32 |  |
| $\Delta U_{s t} \times$ Switcher | $(0.02)$ | $(0.02)$ | $(0.02)$ | $(0.02)$ |  |
|  | -0.77 | -0.74 | -0.83 | -0.67 | -0.08 |
|  | $(0.08)$ | $(0.08)$ | $(0.08)$ | $(0.08)$ | $(0.11)$ |
| Sample |  |  |  |  |  |
| Demographic, Wage Pctile Controls | All | N | All | All | All |
| Switchers |  |  |  |  |  |
| Firm Fixed Effects | N | N | Y | Y | Y |
| Interacted Controls | N | N | N | Y | Y |
| $\Delta w$ of Matched Job Stayer Control | N | N | N | N | Y |
| Observations (000s) | 11,009 | 11,008 | 11,008 | 11,008 | xx |

Notes: Table reports cyclicality of wage growth for job-stayers and job-changers, estimated from Equation 2. The dependent variable in all instances is 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 $t+12$. Columns (2)-(5) 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 (3)-(5) additionally include firm fixed effects, while columns (4) and (5) include fully interacted controls for firm, demographics, and wage percentile. Finally, column (5) subsets to job-changers and additionally controls for the wage change of a matched job-stayer in her destination firm, as described in the text. Standard errors, clustered at the month level, are reported in parentheses.
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. Define $w_{i, j, t}$ as the base wage (measured 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 $t+12$, who lie in wage percentile $p$ and have observable characteristics $X$ in $t$ :

$$
\begin{equation*}
I_{t}^{j}(p, X)=\left\{i:\left(p_{i, t}=p\right) \cap\left(X_{i, t}=X\right) \cap(i \text { works for } j \text { in } t \text { and } t+12)\right\} \tag{3}
\end{equation*}
$$

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, and sex as the set of characteristics $X$. For every combination of wage percentile and observables, we construct the mean wage change for job-stayers within firm $j:{ }^{26}$

[^18]\[

$$
\begin{equation*}
\Delta_{t}^{j}(p, X)=\frac{\sum_{i \in \mathcal{I}_{t}^{j}}\left(\ln w_{i, j, t+12}-\ln w_{i, j, t}\right)}{N_{t}^{j}(p, X)} \tag{4}
\end{equation*}
$$

\]

The goal is to compare this $\Delta_{t}^{j}(p, X)$ to the wage changes of similar job-changers into firm $j$. We therefore turn to our main job-changer sample and construct the 12 -month base wage change for every switcher in our primary sample as they move from firm $j$ to $j^{\prime}$. Then we compare each job-changer $i$ moving to firm $j^{\prime}$ with a matched job-stayer at that same firm with the same initial wage percentile, age group, and sex. 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.

To test this, we re-estimate equation (1) for the sample of job-switchers, controlling for the wage change of a switcher's matched job-stayer, $\Delta_{t}^{j}(p, X)$. The results are presented in Column (5) of Table 5. Observe that once one controls for the wage change of a changer's matched stayer, a one percentage point increase in the unemployment rate is associated with only a 5 basis point decline in base wages for job-changers. ${ }^{27}$ This decline is statistically insignificant and substantially smaller than the unconditional excess cyclicality of job-changers' base wages estimated in column 1. Once one controls for the wage behavior of similar job-stayers, one finds no excess wage cyclicality for new hires into the firm.

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. The evidence in this section is that the adjustment patterns of job-stayers' wages is a sufficient statistic for the behavior of new hires' 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.

However, 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

[^19]additional worker or firm characteristics, such as industry or firm size. 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 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 in some contexts. 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 $5.0 \%$ 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. Considering large residual earnings payments in either of the first two months of employment reveals that $9.7 \%$ of new hires receiving a signing bonus in recession years, compared with $8.7 \%$ in non-recession periods. The average signing bonuses is $\$ 5,044$, accounting for $31.6 \%$ of monthly income, indicating that signing bonuses are concentrated among high earners. ${ }^{28}$

Collectively, our new hire results yield a few key insights. First, we confirm that jobchangers have more cyclical wages than do job-stayers. Furthermore, 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 this section complement those of Hazell and Taska (2019), who documents that posted wages adjust about as frequently as do the wages of incumbent workers. New hire wages at the

[^20]job level do not appear substantially more flexible than do those of job-stayers, 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 set our main results within the context of the existing literature. In doing so, we highlight the distinction between realized compensation dynamics for an individual worker and compensation 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 base contract wage is downwardly rigid. These declines could occur from the transitory nature of overtime and bonuses across years. These movements in compensation expose workers to a potentially risky income stream. However, as we highlight above, from the perspective of the firm, these forms of compensation are not used to adjust the marginal cost of a worker at business cycle frequencies.

### 8.1 The Importance of Bonuses and Overtime in Compensation Dynamics for Job-Stayers

In this subsection, we gauge the importance of bonuses and overtime in contributing to 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 11 shows the annual change in four forms of compensation. To assess how bonus and overtime adjustments affects 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 worked. 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 worked 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 worked 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.

Not surprisingly, the patterns in Panel A are very similar to those presented in 3. 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 3, we restrict workers to those who remain with the same employer for only 12 consecutive months while Panel A of Figure 11 includes workers who remain with the firm for 24 consecutive calendar months. Additionally, Panel A of Figure 11 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 3. 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 about $3 \%$ of workers who remain with their firm for two-years receive an year-over-year nominal base wage per hour reduction. Overall, the patterns in this panel are nearly identical to the patterns shown in Figure 3.

Panel B of Figure 11 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 3.6 percent of workers received a decline in their annual base wage per hour, 17.9 percent of workers saw reduced per hour compensation inclusive of bonuses. Additionally, accounting for bonuses increases the standard-deviation of nominal wage changes from 5.0 percent to 8.3 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 cyclicality, they do vary substantively across years for a given worker.

Panel C of Figure 11 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 19.3 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

Figure 11: Change in Various Measures of Compensation: 24-month Job-Stayers, Excluding Commission Workers


Notes: Figure plots the distribution of year-over-year changes in annual base earnings per annual hours worked (Panel A), annual base earnings plus bonuses per annual hours worked (Panel B), annual base earnings plus bonuses and overtime per annual hours worked (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 work 40 hours per week and 52 weeks per year.
remains on the same job is driven by year-over-year fluctuations in bonuses and overtime rather than downward adjustments in base pay. However, from the perspective of the firm, these forms of compensation do not vary cyclically.

For completeness, Panel D shows the annual change in take home compensation for jobstayers. 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 8.3 and 9.3 , respectively).

### 8.2 Discussion of Existing Literature

With the results in the prior section as a backdrop, we end the paper by highlighting how our estimates compare with many of the existing estimates of nominal wage rigidity in the literature. Our results differ in two important ways. First, our administrative data minimizes the impact of measurement error. Second, we highlight that base wages are a better match to the notion of the cyclicality of the marginal cost of a worker than broader measures of compensation inclusive of bonuses and overtime. ${ }^{29}$

Using the panel component of the CPS, Daly and Hobijn (2014) report that roughly 85 percent of job-stayers' wages (measured by earnings per hour) change annually during a sample period that overlaps with ours. ${ }^{30}$ As noted above, we find that only about twothirds of job-stayers receive an annual nominal base wage change during the 2008-2016 period. Likewise, Elsby et al. (2016) use data from the CPS to document that roughly $20 \%$ of workers receive annual earnings per hour reductions in the CPS during the 2008-2012 period. As highlighted above, we find only $2-6 \%$ of workers receiving a base wage cut during our sample period. The CPS results may differ from ours for two reasons. First, it is well documented that there is a large amount of measurement error in both earnings and hours in household survey. ${ }^{31}$ The large measurement error in earnings and hours in household surveys could explain the higher variance of wage changes in the CPS. Second, it is unclear in household surveys whether workers are reporting only their base earnings or a broader measure of earnings inclusive of bonuses and overtime. This ambiguity makes it harder to make comparisons between household surveys and administrative data sets like ADP.

[^21]Using data from the Survey of Income and Program Participation, Barattieri et al. (2014) try to account for the measurement error in wages and hours in household data by looking for structural breaks in their individual hourly wage series. Their primary focus is on workers who are paid hourly. Given that they focus on an individual's self-reported hourly wage (reported in dollars per hour), their wage measure is similar in concept to our measure of a base wage for hourly workers. When they make their correction for measurement error, they find that the frequency of quarterly wage changes for job-stayers falls from over 50 percent to between 15 and 20 percent - depending on their adjustment procedure - during their period of study. Our quarterly frequency of base wage changes for job-stayers who are paid hourly is 20 percent which is at the upper range of their estimates. More importantly, however, they estimate a much larger fraction of downward wage adjustments. Specifically, they find that 12 percent of all quarterly wage changes for job-stayers are downward changes. That is three and half times larger than our administrative data reports. As Barattieri et al. (2014) highlight, there is substantial measurement error in household surveys with respect to measuring how nominal wages adjust. The fact that their patterns still differ relative to the ADP results is consistent with some residual measurement error remaining even after implementing their structural break procedure.

A more recent literature has emerged using firm-level data to measure wage stickiness. Both 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 or other administrative payroll data, 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 could occur if the quality workers filling a job shifts over time. Consistent with this fact, the nominal base wage variation in the ECI for a given job is much larger than what we document in the payroll data for job-stayers.

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 11. 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. 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. 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.

Our paper is closest in spirit to the results in Altonji and Devereux (2000) and Fehr and Goette (2005). Altonji and Devereux (2000) uses administrative payroll data similar to ours for one large financial service company during 1996 and 1997 while Fehr and Goette (2005) uses administrative payroll data from two Swiss firms in the early 1990s. Within these companies studies by these authors, the patterns of base wage adjustment for jobstayers closely match the patterns we document for the whole U.S. economy during the 2008-2016 period. Additionally, these papers also find that bonuses allow for more downward adjustment of worker annual compensation per hour. That firm-level payroll data from an earlier period within the U.S. and Switzerland broadly matches the job-stayer results from the recent ADP data highlights the importance of using administrative payroll data to measure base wage rigidities.

Finally, there is a growing literature documenting 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 11. For example, both Nickell and Quintini (2003) and Elsby et al. (2016) use data from Britain's 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. ${ }^{33}$ However, these patterns 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

[^22]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. Sigurdsson and Sigurdardottir (2016) also documents 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. ${ }^{34}$ 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 main continuously employed with the same firm. Further, we show detailed evidence of time and state dependence in worker base wage adjustment. 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 pat-

[^23]terns 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: <br> "Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data" (Not for Publication) 

## Appendix A Additional Discussion of Sample Construction

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

## Appendix A. 1 Drawing Randomized Samples

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.

For some analysis, we focus on individuals who switch firms. In order to maximize sample size for such analyses, we additionally draw a sample of one million job-changers. To construct this sample, we begin with the list of unique employee IDs in the data. Next we restrict this list to include only workers who appear as working for at least two unique firms in different months during our sample period. Such workers must appear in firm $j$ in some month $t$, and in a different firm $j^{\prime}$ in month $t^{\prime}$, and thus must have switched employer at some point between May 2008 and December 2016. From this restricted list, we then draw the complete history of one million randomly selected 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.

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 $t+1$. For these workers, any intervening unemployment spell would be short. ${ }^{35}$

## 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 Section Appendix E of this appendix, we show that there are only modest

[^24]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 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. ${ }^{36}$ 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. 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 91,500 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 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. ${ }^{37}$ 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 has about 20 percent of employment (in our employee sample) in firms with more than 5,000 employees. ${ }^{38}$ As noted above, some of this difference also results from the fact that the ADP

[^25]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 | $1,000,000$ | $3,296,701$ | $\cdot$ |
| Number of Firms | 91,577 | 3,000 | $\cdot$ |
| Number of Observations | $24,831,316$ | $68,267,166$ | $\cdot$ |
| \% Firm Size: $50-499$ |  |  |  |
| \% Firm Size: $500-999$ | 37.8 | 31.3 | 29.5 |
| \% Firm Size: $1000-4999$ | 13.6 | 13.9 | 7.3 |
| \% Firm Size: $\geq 5000$ | 25.1 | 22.2 | 17.5 |

$\overline{\overline{\text { Notes: }} \text { Table reports the share of employees in firms of various sizes in our random samples of the }}$ 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.
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 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 for each year between 2008 and 2016. By reweighting 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

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 matches 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
complementary underweight in retail trade, construction, and agriculture.
in our employee sample, only 220,000 are in our sample in 2008 while 377,000 are in our 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 distinction 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 post2012. 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.

Table A2: Statistics for Employee Sample, Selected Years

|  | All | 2008 | 2012 | 2016 |
| :--- | :---: | :---: | :---: | :---: |
| Number of Workers | $1,000,000$ | 220,817 | 388,214 | 377,023 |
| Number of Firms | 91,577 | 37,269 | 52,478 | 45,519 |
| Number of Observations | $24,831,316$ | $1,424,364$ | $3,017,746$ | $3,053,743$ |
|  |  |  |  |  |
| Age: 21-30 (\%) | 25.4 | 25.8 | 24.5 | 26.7 |
| Age: 31-40 (\%) | 24.0 | 25.3 | 23.8 | 24.2 |
| Age: 41-50 (\%) | 23.9 | 25.2 | 24.4 | 22.2 |
| Age: 51-60 (\%) | 20.5 | 17.7 | 21.2 | 20.7 |
|  |  |  |  |  |
| \% Male | 54.0 | 54.1 | 53.9 | 55.2 |
| Average Tenure (months) | 68.5 | 71.0 | 68.9 | 66.6 |
|  |  |  |  |  |
| \% Paid Weekly | 20.7 | 21.4 | 21.7 | 21.0 |
| \% Paid Bi-Weekly/Semi-Monthly | 76.0 | 75.5 | 75.1 | 75.1 |
| \% Paid Monthly | 3.3 | 3.1 | 3.2 | 3.9 |
| \% Hourly | 65.8 | 66.1 | 66.2 | 64.6 |

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

## Appendix B. 3 Fraction Paid Hourly

For our sample, 66 percent are paid hourly with the remaining 34 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 actual hours never vary across weeks and not actually tracked in any meaningful sense. 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. 4 CPS Comparison Average Hourly Wage for Hourly Workers

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. 5 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 wage rigidity varies by compensation component), it has the advantage of covering the universe of American workers over a long time span. As a result, their data is free of any form of sample selection, and represents a logical benchmark for our ADP dataset. We benchmark our annual earnings changes to Guvenen et al. (2015)'s Figure 1,

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 ADP data is weighted so it is representative of the aggregate industry $\times$ size distribution. The CPS data is weighted by the corresponding survey weights for the respective samples.
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.

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.

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 change of 0.01 , in line with Guvenen et al. (2015). The standard deviation of annual earnings changes is 0.56 (compared with 0.51 in the SSA), while the skewness is -0.88 (vs -1.07 ) and kurtosis is 16.98 (vs 14.93). ${ }^{39}$ We interpret these small differences to be the result of imperfectly capturing the annual earnings changes for job-changers, as well as transitions to unemployment. Overall, however, we find the similarity of this figure to that in Guvenen et al. (2015) to be encouraging.

## Appendix C Calculating Compensation Measures

This section details our construction of relevant compensation measures.

[^26]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 $\times$ industry mix.

## Appendix C. 1 Base Wages

The ADP data show an employee's per period base payment rate. This administrativelyrecorded 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 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 workers. 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 from hourly to salaried status or vice versa; 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 is 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. ${ }^{40}$

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 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 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

[^27]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.
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 C. 3 Employer-Provided Fringe Benefits

Although not highlighted in the main text, the ADP data include measures of fringe benefits. Fringe health benefits were not required to be reported on a person's paycheck (for tax reasons) until 2012. Starting in 2012, as part of the Affordable Care Act, employers were required to report their contributions to employee health benefits. As a result, components of our fringe benefit information are only reliable from 2012 onwards. We focus on the two largest forms of fringe benefits - health insurance and pension benefits. We define health insurance to be the sum of employer-provided health and accident plans, employer contributions to Health Savings Accounts (HSAs), Medical Spending Accounts (MSAs), and other Section 125 Medical Cafeteria plans. Pension benefits are the sum of employer contributions to $403(\mathrm{~b}), 501(\mathrm{c}), 414(\mathrm{~h}), 401(\mathrm{k}), 408(\mathrm{p}), 408(\mathrm{k})$, and Roth 457 plans. ${ }^{41}$

Using these data, we create a measure of "annual fringe benefits" by summing the monthly employer-provided health benefits and retirement contributions over all months of a year. Using these measures, we construct a worker's "total annual compensation" by summing their annual gross earnings with their annual employer-provided fringe benefits. When analyzing measures of broader compensation including fringe benefits, our analysis is limited to workers who remain continuously employed with the same firm for a full year during the 2012-2016

[^28]Table A3: Share of Fringe Benefits out of Total Compensation, 2012-2016 Period

|  | All | Hourly | Salaried |
| :--- | :---: | :---: | :---: |
| $10^{\text {th }}$ percentile | $0.0 \%$ | $0.0 \%$ | $0.0 \%$ |
| $25^{\text {th }}$ percentile | $1.3 \%$ | $0.0 \%$ | $4.4 \%$ |
| Median | $8.2 \%$ | $6.5 \%$ | $9.5 \%$ |
| $75^{\text {th }}$ percentile | $17.1 \%$ | $18.3 \%$ | $15.9 \%$ |
| $90^{\text {th }}$ percentile | $25.1 \%$ | $27.1 \%$ | $22.2 \%$ |
|  |  |  |  |
| Mean Total Fringe Share | $10.7 \%$ | $10.6 \%$ | $10.8 \%$ |
| Mean Health Fringe Share | $7.9 \%$ | $8.7 \%$ | $6.7 \%$ |
| Mean Pension Fringe Share | $2.8 \%$ | $2.0 \%$ | $4.0 \%$ |
|  |  |  |  |
| Sample Size (Thousands) | 630 | 370 | 260 |

Notes: Table shows the distribution of the share of worker fringe benefits out of total worker compensation during the 2012-2016 period for all workers in our full time employee sample (column 1) and then separately for hourly and salaried workers (columns 2 and 3).
period.
Table A3 shows the distribution of the share of total compensation that is in fringe benefits for all workers in our full-year employee sample. Fringe benefits accounts for $8.2 \%$ of the median worker's total compensation, but there is large variation around this number, with $10 \%$ of workers receiving more than $25 \%$ of their compensation through fringe benefits, and many workers receiving no fringe benefits at all. Hourly workers tend to receive fewer fringe benefits than do salaried workers: the median hourly worker has $6.5 \%$ of their total compensation in fringe benefits, compared with $9.5 \%$ for salaried workers, However, the right tail of fringe benefits is thicker for hourly workers than for salaried workers, with the 90th percentile of hourly workers receiving $27.1 \%$ of their compensation from these fringe benefits, compared with $22.2 \%$ for salaried workers. Encouragingly, the numbers presented in this table match those found by the BLS in their Employer Cost for Employee Compensation (ECEC) reports. For example, the June 2016 report finds that $7.6 \%$ of workers' total compensation is accounted for by the cost of health insurance, and $3.9 \%$ is accounted for by retirement and savings account contributions. ${ }^{42}$

Figure A4 shows the share of annual fringe benefits provided by the employer out of a worker's annual total compensation (earnings plus fringe) as a function of the worker's base wage percentile. Fringe benefits are much less important for lower wage workers (below the 20th percentile). However, from the 20th percentile through the 95th percentile of the wage

[^29]Figure A4: Fringe Benefit Share by Employee Base Wage Percentile, Full-Year Job-Stayers, Excluding Commission Workers


Notes: Figure shows the share of fringe benefits out of total compensation by worker percentile within the base earnings distribution. Sample restricted to job-stayers who remain in the sample for a full calendar year. Commission workers are excluded. Data covers the 2012-2016 period.
distribution, the share of total compensation in fringe is roughly constant in the 12 to 14 percent range. For top wage earners, fringe becomes a smaller fraction of total compensation. This is likely because fringe benefits are not usually provided on bonus income and because tax exempt employer-provided retirement contributions are capped.

## Appendix D Robustness 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

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


Panel A: Non-Commission in $\operatorname{Year} t-1$


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

Panel B: With Commission 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 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.
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 3.

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

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.
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 3 of the main text and 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.

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

Table A4 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.

Table A4: Base Wage Change Statistics, Pooled 2008-2016 Sample of Job-Stayers

|  | Monthly | Quarterly | Annual |
| :--- | :---: | :---: | :---: |
| Unconditional |  |  |  |
| Skewness of Base Wage Changes (\%) | 9.7 | 5.3 | 2.8 |
| Kurtosis of Base Wage Changes (\%) | 175.4 | 49.5 | 14.4 |
|  |  |  |  |
|  |  |  |  |
| Conditional on Any Wage Change |  | 2.1 | 2.4 |
| Skewness of Base Wage Changes (\%) | 2.1 | 13.6 | 12.1 |
| Kurtosis of Base Wage Changes (\%) | 15.9 | 10. |  |

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 plots the unweighted distribution of 12 -month base wage changes for jobstayers. The only difference between this figure and Figure 3 of the main text is that here we do not weight data in order to match the firm size $\times$ 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 $\times$ 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 Heterogeneity in Base Wage Adjustment for JobStayers

Figure A8 plots the probability that a worker receives a year-over-year base wage change according to her initial position in the national base wage distribution. We calculate the wage distribution within hourly and salaried bins, and plot the patterns separately for each payment type. The black solid line shows the patterns for hourly workers, while the gray dashed line shows the patterns for salaried workers. The figure shows little systematic difference in the probability of receiving a base wage increase by initial wage percentile. However, those at the very top of the salaried distribution, for whom bonus income is a substantial portion of earnings, are less likely to receive an annual wage increase. Similarly,

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 3 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 A8: Probability of base wage adjustment by initial wage percentile, 2008-2016


Panel A: Positive Changes


Panel B: Negative Changes

Notes: Figure shows the probability that a worker receives a year-over-year base wage increase (Panel A) or decrease (Panel B) by the worker's initial position in the national wage distribution for workers of her payment type (i.e. hourly or salaried). This plot covers the period 2008-2016, and plots the patterns separately for hourly workers (black solid lines with diamond markers) and salaried workers (gray dashed lines with triangle markers).
there is little relationship between wage level and the probability of receiving a base wage cut for salaried workers, with the probability of a wage cut bounded between $1.5 \%$ and $2.2 \%$ for much of the distribution. Low wage hourly workers, however, are less likely to receive a base wage cut than are high wage hourly workers, possibly due to minimum wage constraints or union contracts.

## Appendix D. 5 Mean Base Wage Change Size by Time Since Last Base Wage Change

Figure 5 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 who wait longer between wage changes should observe larger average changes in absolute value. We explore this prediction next.

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, 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

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.
than 50 employees from our analysis.
Figure A10 shows the distribution 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, $63.4 \%$ of hourly workers and $66.5 \%$ of salaried workers in firms with under 500 employees receive a base wage change. The comparable numbers for firms with $5000+$ employees are $78.9 \%$ and $76.8 \%$, 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 explains why our weighted results and unweighted results are so similar to each other.

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. Figure A11 plots the distribution of 12-month base wage changes for jobstayers in this small firm sample for the period 2014-2016. ${ }^{43}$ 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 $2.4 \%$. 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 these small firms leads to a corrected annual probability of base wage change of $62.2 \%(0.271 \times 51.3 \%+(1-0.271) \times$ $66.3 \%$ ). Using the same procedure, the probability of a year-over-year base wage cut is $2.3 \%$ for all 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 ( $66.3 \%$ vs. $62.2 \%$ and $2.4 \%$ vs. $2.3 \%$ ). To summarize, we conclude that the omission of workers in firms with less than 50 employees is not biasing our results substantively.

[^30]Figure A10: Share with Base Wage Change by Firm Size and Industry, All Years


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.

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


Panel A: $\underset{\text { Unweighted }}{\geq 50 \text { Employee Firms - }}$


## 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 2 we explore the robustness of the regressions shown in top panel of Table 2 of the main text to the exclusion of individual fixed effects. Comparing across the tables, the individual fixed effects only affect the cyclicality of overtime receipt (column 2) and the cyclicality of bonus receipt (column 4). With individual fixed effects, the propensity of receiving overtime is unrelated to the unemployment rate as seen in column 2 of Table 2 in the main text; a one percentage point increase in the unemployment rate was associated with only a -0.16 percentage point decline in the propensity to accrue overtime hours. However, excluding individual fixed effect, overtime receipt becomes more procyclical with a coefficient of -0.85 (standard error $=0.10$ ) as seen in column 2 of Appendix Table A5. These results suggests that it is important to control for individual fixed effects when interpreting the cyclicality of overtime receipt. These findings are consistent with evidence of the changing selection on worker attributes over the business cycle. Specifically, the results are consistent with workers who usually receive overtime hours being less likely to be employed at the firm during recessions. The fixed effect regressions highlight that a given worker is no more likely to work overtime hours when unemployment is high.

The bottom panel of Figure 2 explores the overtime results in greater detail. The first two
columns restricts the sample to those workers in the manufacturing industries. The second two columns restricts the sample to those workers in the non-manufacturing industries. For each industry subset, we highlight result 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 the manufacturing industry given the BLS tracks annual manufacturing hours. The BLS data highlights that overtime hours within the manufacturing sector are pro-cyclical. The BLS data do not control for individual fixed effects. As seen from columns 1 and 3, the propensity to receive overtime is equally pro-cyclical in both the manufacturing and non-manufacturing industries without controlling for individual fixed effects. However, as seen in column 2, controlling for individual fixed effects explain about half of the observed pro-cyclicality of overtime hours within the manufacturing sector but overtime hours still remain pro-cyclical at standard statistically significant levels (a coefficient of -0.44 with a standard error of 0.20 ). However, within the non-manufacturing sectors which make up the bulk of employment there is no statistically 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 once 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:

$$
\begin{equation*}
y_{i t}=\rho y_{i t-1}+\alpha_{i}+\epsilon_{i t} \tag{A1}
\end{equation*}
$$

where $i$ indexes a worker-firm pair, $t$ represents a year and $\alpha_{i}$ is a worker-job fixed effect. $y_{i t}$ 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.

Table A6 reports the estimated autocorrelation coefficient $\rho$ 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 .

Table A5: Robustness of Cyclicality of Various Forms of Compensation

| Panel A: Cyclicality of Compensation Components |  |  |  |  |  |  |  |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | \% Base Wage | \% With | Log | \% With | Log |  |  |
|  | Change | Overtime | Overtime | Bonus | Bonus |  |  |
|  | $(1)$ | $(2)$ | $(3)$ | $(4)$ | $(5)$ |  |  |
| $\Delta$ Unemployment Rate (\%) | -0.39 | -0.85 | -0.04 | -0.29 | -0.03 |  |  |
|  | $(0.02)$ | $(0.10)$ | $(0.00)$ | $(0.09)$ | $(0.00)$ |  |  |
| State and Industry FE |  |  |  |  |  |  |  |
| Individual FE | Y | Y | Y | Y | Y |  |  |
| Workers Included | N | N | N | N | N |  |  |
| Observations (000s) | All | Hourly | Hourly | All | All |  |  |
| Dependent Variable Mean | 450 | 267 | 174 | 486 | 203 |  |  |

Panel B: Cyclicality of Overtime for Manufacturing and Non-Manufacturing Sectors

|  | Manufacturing |  | Non-Manufacturing |  |
| :--- | :---: | :---: | :---: | :---: |
|  | \% With | \% With | \% With | \% With |
|  | Overtime | Overtime | Overtime | Overtime |
|  | $(1)$ | $(2)$ | $(3)$ | $(4)$ |
| $\Delta$ Unemployment Rate (\%) | -0.70 | -0.44 | -0.89 | -0.10 |
|  | $(0.24)$ | $(0.20)$ | $(0.13)$ | $(0.18)$ |
|  |  |  |  |  |
| State and Industry FE | Y | Y | Y | Y |
| Individual FE | N | Y | N | Y |
| Workers Included | Hourly | Hourly | Hourly | Hourly |
| Observations (000s) | 62 | 45 | 205 | 145 |
| Mean of Dep. Var. | 73.5 | 73.8 | 59.8 | 58.3 |

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.

Table A6: Annual Persistence of Base Wage, Bonuses, and Overtime, sample of full-year job-stayers, 2009-2016

|  | Log December <br> Base Wage <br> $(1)$ | \% With <br> Bonus <br> $(2)$ | Log Annual <br> Bonus <br> $(3)$ | \% With <br> Overtime <br> $(4)$ | Log Annual <br> Overtime Hours |
| :--- | :---: | :---: | :---: | :---: | :---: |
|  | $0.82^{* * *}$ | $0.57^{* * *}$ | -0.02 | $0.72^{* * *}$ | $0.05^{* * *}$ |
| $y_{i t-1}$ | $(0.01)$ | $(0.00)$ | $(0.02)$ | $(0.01)$ | $(0.02)$ |
|  |  |  |  |  |  |
| Observations | 463418 | 463418 | 114608 | 307396 | 235340 |
| Individual FE | Y | N | Y | N | Y |
| Workers Included | All | All | All | Hourly | Hourly |

Notes: Table reports OLS-estimated $\operatorname{AR}(1)$ 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.

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 suggests 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.


[^0]:    *We thank Mark Aguiar, Susanto Basu, Steve Davis, Fatih Guvenen, John Haltiwanger, Jonathon Hazell, Pete Klenow, Alan Krueger, Marianna Kudlyak, Andre Kurmann, Alex Mas, Ben Schoefer, John Shea, Rob Shimer, Gary Solon, Joe Vavra, Emi Nakamura and three anonymous referees for helpful comments. Per our use agreement, ADP approved the paper topic ex-ante. Additionally, ADP reviewed the paper prior to distribution with the sole focus of making sure that the paper did not release information that would compromise the privacy of their clients or would reveal proprietary information about the ADP business model. Authors' contact information: jgrigsby@uchicago.edu, erik.hurst@chicagobooth.edu. and Ahu.Yildirmaz@ADP.com.

[^1]:    ${ }^{1}$ See, for example, Abraham and Haltiwanger (1995) and the cites within.
    ${ }^{2}$ See, for example, Grigsby (2019).
    ${ }^{3}$ 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).
    ${ }^{4}$ See, for example, Bils and Klenow (2004), Klenow and Kryvtsov (2008), and Nakamura and Steinsson (2008) for important contributions.

[^2]:    ${ }^{5}$ See Solon et al. (1994) for a discussion of selection at business cycle frequencies, and Gertler et al. (2016) for an argument that much of the observed cyclicality of job-changers is due to selection in workers' match quality.
    ${ }^{6}$ In essence, contract rigidity determines the user cost of labor, which Kudlyak (2014) shows is the allocative cost of employment.

[^3]:    ${ }^{7}$ 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.

[^4]:    ${ }^{8}$ 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. Firm and worker names, detailed addresses, firm employer identification numbers, and worker social security numbers are all purged from the data available to researchers. The ADP data use agreement prohibits using the data to explore wage patterns of any individual worker or firm.

[^5]:    ${ }^{9}$ The ADP data matches well the industry composition of employment in the BDS.

[^6]:    ${ }^{10}$ Throughout our analysis, we abstract from discussing fringe benefits in worker compensation. In the online appendix, we discuss the importance of fringe benefits in worker compensation in greater detail.
    ${ }^{11}$ 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.

[^7]:    ${ }^{12}$ 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 implies 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.

[^8]:    ${ }^{13}$ 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.

[^9]:    ${ }^{14}$ The decline in overtime compensation for high wage hourly workers may be an artifact of the way some firms designate hourly workers. There are some high wage hourly workers who report working 40 hours per week during every week of the year. These workers, at least with respect to hours worked, look like they may in fact be salaried.

[^10]:    ${ }^{15}$ 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).

[^11]:    ${ }^{16}$ In the online appendix, we show a variety of robustness specifications to the results in Table 2. One set of specifications exclude individual fixed effects for all specifications. Across all the specifications we explored, base wages are predominantly responsible for the cyclicality of worker compensation.

[^12]:    ${ }^{17}$ 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.

[^13]:    ${ }^{18}$ It should be noted that our estimates of the cyclicality of nominal wages for job-stayers 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.

[^14]:    ${ }^{19}$ 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 change results in any way.
    ${ }^{20}$ 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 are higher than the wage growth for the economy as a whole. See Beraja et al. (2016) who make a similar point when comparing time series and panel data wage growth patterns in the CPS during the Great Recession.

[^15]:    ${ }^{21}$ In the online appendix, we show that workers who 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).
    ${ }^{22}$ 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.
    ${ }^{23}$ 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.

[^16]:    ${ }^{24}$ 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 the change jobs.

[^17]:    ${ }^{25}$ 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.

[^18]:    ${ }^{26}$ To maximize power, we use the full ADP data for this exercise, rather than any of our subsamples.

[^19]:    ${ }^{27}$ The sample size in this column is substantially smaller as we only include workers who switch jobs and appear at another ADP employer 12-months after leaving her prior employer. In addition, we must match the worker to a similar job-stayer, implying that she must go to a firm which has at least one worker who stays on the firm between $t$ and $t+12$, who is of the same wage percentile, sex, and age group.

[^20]:    ${ }^{28}$ The conditional size of these bonuses exhibits mild cyclical variation. During recession years, the average signing bonus accounts for $28.6 \%$ of workers' monthly paycheck, compared with $32.8 \%$ in the non-recession years of our sample. However, the rarity of signing bonuses makes it unlikely that they will generate substantial cyclical variation in the cost of new hires for a majority of firms.

[^21]:    ${ }^{29}$ There is a long literature, surveyed by Bewley (2004) and Howitt (2002), examining the root causes of nominal wage rigidity. In a series of interviews with business managers responsible for compensation policy, studies have documented that the primary resistance to wage cuts arises from concerns over damaging worker morale. See, for instance, Kaufman (1984), Blinder and Choi (1990), Agell and Lundborg (1995, 1999), Campbell III and Kamlani (1997), and Bewley (1999).
    ${ }^{30}$ One of the earliest papers to estimate the extent of nominal wage rigidity using household level data was Kahn (1997), who used data from the Panel Study of Income Dynamics (PSID) to find that about $92 \%$ of workers receive a nominal wage change during a given year.
    ${ }^{31}$ There is a literature documenting sizable measurement error in both income and hours in household surveys using audit studies. Bound and Krueger (1991) finds that just about forty percent of cross-sectional variance of the change in income for men in household surveys can be explained by measurement error. Bound et al. (1989) documents that the measurement error in reported hours in household surveys is even larger than the measurement error in income.

[^22]:    ${ }^{32}$ See Elsby and Solon (2018) for a recent survey of this literature.
    ${ }^{33}$ Ehrlich and Montes (2019) uses administrative data from Germany and finds a large mass of jobstayers who receive annual declines in compensation per hour. However, they find evidence of downward compensation per hour rigidities in that there are less declines than predicted by their model.

[^23]:    ${ }^{34}$ 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.

[^24]:    ${ }^{35}$ 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, but generate large swings in base wages in almost all cases.

[^25]:    ${ }^{36}$ See, for example, https://www.feicanada.org/ajaxfilemanager/uploaded/RH_FERF_ Benchmarking2014.pdf.
    ${ }^{37}$ According to BDS data, $72 \%$ of all U.S. employment during this time period is in firms with more than 50 employees.
    ${ }^{38}$ 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

[^26]:    ${ }^{39}$ 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.

[^27]:    ${ }^{40}$ 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.

[^28]:    ${ }^{41}$ We exclude all tax measures from our analysis including employer paid payroll taxes. Additionally, we note that our fringe benefit and bonus measures (in the main text) do not include stock options. ADP has data on when stock options are exercised (for tax reasons), but not when the options were granted.

[^29]:    ${ }^{42}$ See, for instance https://www.bls.gov/news.release/ecec.nr0.htm. The aggregate fringe benefit share does not match exactly, as the BLS includes paid leave, bonuses, and legally-required benefits such as social security payments, the first two of which will be included in our measure of gross earnings.

[^30]:    ${ }^{43}$ 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.

