Downward Rigidity in the Wage for New Hires

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Wage rigidity is an important explanation for unemployment fluctuations. In benchmark models wages for new hires are key, but there is limited evidence on this margin. We use wages posted on vacancies, with job and establishment information, to measure the wage for new hires. We show that our measure of the wage for new hires is rigid downward and flexible upward, in two steps. First, wages change infrequently at the job level, and fall especially rarely. Second, wages do not respond to rises in unemployment, but respond strongly to falls in unemployment. Job information is crucial for detecting downward rigidity.

1 Introduction

Suppose there is downward wage rigidity, meaning that wages do not fall during recessions. Economists have long argued that unemployment should then rise, because the cost of labor remains high even as labor demand falls (Keynes, 1936). Downward wage rigidity for *new hires* is particularly important, because employment is a long term contract. Therefore the present value of wages matters to workers and firms (Barro, 1977). One important component of the present value of wages is the wage for new hires (Pissarides, 2009). Even if wages in continuing jobs change little, the present value of wages can still vary if the wage for new hires is flexible.

There is not yet consensus about wage rigidity for new hires. A key difficulty is job composition. Pissarides (2009) surveys some work on the wage for new hires using worker level survey data without job information. This work studies the average wage for new hires—controlling for worker characteristics but averaging across the jobs that hire these workers. However if job composition varies over time, then average wage changes reflect changing job composition, as well as wage changes for individual jobs (Gertler, Huckfeldt, and Trigari, 2020). As an example, consider an economy of high wage bankers and low wage baristas, and suppose that the share of barista hires increases. Then average wages for new hires may fall, even if wages fall for neither baristas nor bankers. Equally, if the share of barista hires decreases, then average wages might not fall and could even rise—even if wages fall for both baristas and bankers.

To confront the difficulty of job composition, this paper studies a measure of the wage for new hires with job information. Our dataset, provided by Burning Glass Technologies, contains wages posted in online vacancies, with job titles, establishment identifiers, and pay frequency, for the United States. The source is vacancies from online job boards and company websites. Our main sample covers January 2010 to June 2020—starting after the Great Recession, and finishing after the contractionary phase of the Pandemic Recession. We show that posted wages are a reasonable measure of the wage for new hires. Average posted wages in Burning Glass track quarterly measures of the average wage for new hires in survey data, at the state, occupation and industry level.

However our dataset can measure not only averages, but also *job level* variation in posted wages—by which we mean the wage across successive vacancies posted by the same job title and establishment. Consider a physical location of Starbucks, in Cambridge, Massachusetts, that regularly posts vacancies for baristas, and pays them an hourly wage. Our data tracks the hourly wage for baristas across multiple vacancies posted by the Starbucks. By studying job level wages, we can purge wages changes due to job composition.

Figure 1: Nominal Posted Wage Growth at the Job Level and Unemployment Changes
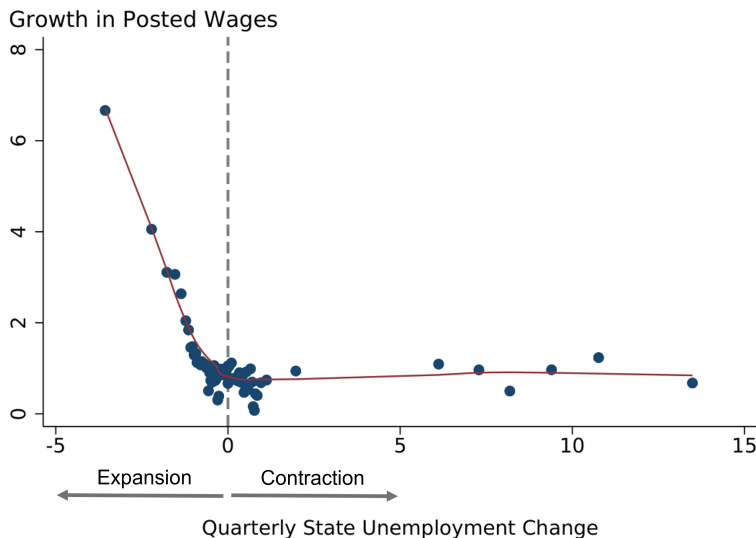
Notes: the graph plots wage growth of nominal posted wages, in percent, from Burning Glass; and state by quarter unemployment changes, in percentage points, from the Local Area Unemployment Statistics. The sample period is 2010Q1-2020Q2. To construct wage growth, we take the mean wage within each job and quarter, and then take log differences at the job level. We collect wage growth and unemployment changes into 100 bins, and add a non-parametric regression line.

In the main contribution of the paper, we find that our measure of the wage for new hires is rigid downward, but flexible upward. We present two pieces of evidence. First, we detect signs of a constraint on wage setting. The posted wage rarely changes between successive vacancies of the same job—typically changing once every 5-6 quarters. When wages do change for a given job, they are four times more likely to rise than to fall. Many papers (e.g. McLaughlin, 1994) document infrequent and asymmetric changes in continuing workers' wages—we show the same for a measure of new hires' wages.

Second, at the job level, the posted wage rises during expansions but does not fall during contractions. Figure 1 shows this result in a binned scatterplot. In the figure, the posted wage is averaged by job and quarter. On the vertical axis is wage growth between two consecutive vacancies for the same job. On the horizontal axis is the growth in quarterly state level unemployment between the quarters in which the vacancies are posted.[1] As state unemployment decreases, the posted wage rises strongly, with wages responding similarly to small and large declines. As state unemployment increases, wages do not fall—neither for small nor for large increases. Figure 1 isolates job level growth in posted wages, to remove variation from changing job composition that might obscure downward wage rigidity. We confirm the finding with regressions and show that wages are downwardly rigid with respect

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[1]Since many jobs do not post in consecutive quarters, sometimes the change in unemployment between postings is large. Average wage growth is greater than zero even during contractions, because some jobs experience positive wage growth during contractions.

to identified labor demand shocks. Real wages are also rigid downward and flexible upward. If firms select into vacancy posting according to the business cycle, then our estimates might be subject to selection bias. However we implement a standard Heckman (1979) selection correction, which suggests that selection bias in the job level regression is moderate.

Next, we show that job level information is important for our results. The average wage for new hires, the object of some previous work, shows no sign of downward rigidity— neither using Burning Glass, nor worker-level survey data from prior work. We find that job composition raises the variance of average wages, meaning regressions using average wages lack the power to detect downward rigidity. Intuitively, average wages aggregate across all types of jobs. Then average wage changes reflect either wage changes at the job level, or changes in job composition. In the data, the share of low wage jobs is volatile, which means average wages are also volatile. As a result, standard errors from regressions with average wages are twenty times larger than counterparts using job-level wages.

Our finding of downward wage rigidity for new hires is perhaps surprising. There is much evidence of downward wage rigidity for continuing workers—see, for example, Grigsby, Hurst, and Yildirmaz (2021) and Cajner et al (2020) for convincing analyses during the Great Recession and the Pandemic Recession. However wages could be more flexible for new hires than continuing workers. For example, firms might not cut wages for continuing workers to provide insurance, but still cut wages for new hires (Beaudry and DiNardo, 1991). Our results instead suggest parity between new hires and continuing workers, perhaps due to internal equity (Bewley, 2002).

Our findings can inform a range of models. For instance, wage rigidity for new hires is important in the canonical Diamond-Mortensen-Pissarides labor search model (e.g. Pissarides, 2009). Nominal wage rigidity for new hires is also important in New Keynesian models with hiring (Basu and House, 2016).[2]

There are four important limitations to our analysis. First, our main sample is small, being the subset of jobs in Burning Glass with wages, job title and establishment information, which post in multiple quarters; this sample is 0.8% of US vacancies. However we show the main sample is broadly representative of the US population on observables; and wages from the main sample track wages from representative survey data. A second caveat is that we measure rigidity only for jobs that post wages. Jobs that that do not post wages might be more flexible. Against this concern, we find the share of vacancies posting wages does not change during contractions. If vacancies that post wages were especially rigid, we might

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[2]Rigidity in posted wages also matters in models of directed search (Moen, 1997), since workers may form expectations from wage postings, which directs their search and affects vacancy creation. Continuing wages matter more than new hires' wages for unemployment fluctuations in other models, for instance with on the job search (Fukui, 2020).

expect the share of vacancies posting wages to fall during recessions. A third caveat is that we do not measure the realized wage paid to new hires. Wage bargaining could lead to more flexibility in realized wages than in posted wages. However we find wages are downwardly rigid even in occupations where wage bargaining is uncommon. A fourth caveat is that the Pandemic Recession is the main contraction over our sample period. Standard models focus on downward wage rigidity with respect to labor demand shocks, whereas the Pandemic Recession also involved labor supply shocks. However in robustness we study identified labor demand shocks. We also end our main sample in June 2020, including the contractionary phase of the Pandemic Recession but not the aftermath, a period during which labor demand shocks were arguably important (Guerrieri, Lorenzoni, Straub, and Werning, 2022).

Related literature. This paper contributes to three literatures. First, we contribute to the literature investigating the cause of unemployment fluctuations. Shimer (2005) shows that a standard calibration of the Diamond-Mortensen-Pissarides model leads to small unemployment fluctuations, compared with US data. Shimer (2004), Hall (2005), Hall and Milgrom (2008), Gertler and Trigari (2009) and Christiano, Eichenbaum, and Trabandt (2016) show that adding wage rigidity to the model leads to unemployment fluctuations as large as in the data. Pissarides (2009) emphasizes that in this model, the relevant wage is for newly hired workers. Our contribution is to argue wages for new hires are rigid downwards, but flexible upwards.

This paper contributes to a second literature that measures wage rigidity for new hires. In the seminal paper, Bils (1985) regresses the wage for new hires on unemployment to measure wage cyclicality. The wage data for new hires comes from surveys, on workers switching jobs or entering new jobs from unemployment, without job or establishment information. The regression controls for worker characteristics, but averages over the jobs into which workers are hired, which we term the average wage for new hires. Pissarides (2009) summarizes results from Bils (1985) and related papers. In these papers, point estimates suggest strong procyclicality, but the estimates tend to be imprecise and confidence intervals often include weak or zero procyclicality. Gertler and Trigari (2009) emphasize the challenge of job composition in interpreting these results.

Our work complements two papers that study wage rigidity for new hires and correct for job composition.[3] First, Gertler et al (2020) study wages for workers newly hired from unemployment. The average wage of workers hired from unemployment is likely less affected by job composition than the average wage of workers switching jobs. Gertler et al (2020)

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[3]Other important papers studying the wage for new hires in US data include Haefke, Sonntag, and Van Rens (2013), Hagedorn and Manovskii (2013), Kudlyak (2014), Basu and House (2016) and Bils, Kudlyak, and Lins (2022).

find weakly procyclical wages for workers hired from unemployment. Second, Grigsby et al (2021) introduce high quality payroll data on workers switching jobs. To control for job composition, they introduce a rich set of worker level controls available in their data, and also develop an estimator that matches workers switching jobs to observationally identical workers at the destination firm. With either adjustment, the wage for new hires is weakly procyclical. Our paper complements these papers in two respects. First, our data is at the job level instead of the worker level, meaning we can directly correct for job composition. The worker level controls in the two related papers may leave residual job composition. Second, both papers estimate that wages are rigid on average—we add evidence on the asymmetry of wage rigidity.

A third important paper is Martins, Solon, and Thomas (2012). Like us, the paper uses job level wages, by exploiting rich administrative data from Portugal with establishment and detailed occupation information. The paper identifies "entry jobs" which frequently hire workers, and then measures the cyclicality of real wages paid to workers newly hired into those jobs. Like us, they find wages are similarly rigid for new hires and continuing workers, though all wages are more cyclical in Portugal than in the United States.

Our paper contributes to a third literature that studies the consequence of downward rigidity for asymmetries in unemployment. For example, Dupraz, Nakamura, and Steinsson (2020) show that if wages for new hires are rigid downward and flexible upward, then unemployment rises sharply during contractions and falls more slowly during expansions. Our paper provides evidence for the asymmetric form of wage rigidity required by these papers, for a relevant kind of wage.[4]

2   Data

This section introduces the main job level dataset of posted wages. We study a dataset of wages posted on vacancies, with job title and establishment information. The dataset was developed by Burning Glass Technologies, a business analytics company. Burning Glass extracts information from the near-universe of job vacancy postings, using machine learning algorithms. There are 40,000 distinct online sources, primarily job boards and company websites. No more than 5% come from any one source. The company employs a deduplication algorithm, to avoid double counting vacancies that post on multiple job boards.

Our dataset covers January 2010 to June 2020. Therefore the dataset starts in the

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[4]Acharya, Bengui, Dogra, and Wee (2018), Chodorow-Reich and Wieland (2020), Cacciatore and Ravenna (2021), Guerrieri, Lorenzoni, Straub, and Werning (2021) and Barnichon, Debortoli, and Matthes (2022) also study asymmetries in unemployment dynamics, when wages for newly hired workers are rigid downward and flexible upward.

aftermath of the Great Recession, but excludes the Great Recession itself. Burning Glass began continuously collecting data in January 2010, meaning we cannot study earlier periods. Our dataset terminates after the contractionary phase of the Pandemic Recession, which officially took place over February-April 2020, but does not include the recovery after the contraction. We chose the end point to focus on a period in which labor demand shocks were relatively important, as we discuss at length in Section 4.2.

The dataset contains detailed information on the wage posted on vacancies, for around 20% of the full set of Burning Glass vacancies. Vacancies post either a wage range or a single wage. The data reports the pay frequency of the contract, for example, whether pay is annual or hourly; and the type of salary, e.g. base pay or bonus pay. Given pay frequency, we can measure hourly earnings for workers, i.e. the wage of the vacancy. The hours measure is an important advantage because in the United States, administrative data typically does not contain hours worked.

The data report establishment and job title information—henceforth, a "job" is a job title within an establishment whose wages are paid at a given frequency (e.g. annual or daily). These data are available for roughly half of the vacancies that post wages. Each physical location at which a firm employs workers is an establishment, measured by company name and zip code. Job titles are extracted from the text of the vacancies and cleaned using Burning Glass' algorithms. The dataset also records any education requirements associated with the vacancy, such as high school diploma or undergraduate degree, if they are present. There is occupation information at the 2- 3- or 6-digit SOC code level, and the industry associated with the vacancy at the 2 or 3 digit NAICS level. Vacancies from 2018 onwards measure the length of time for which the vacancy was posted; and the source of the vacancy, such as a job board or company website.[5]

We now describe the steps towards forming our main job level sample. The complete Burning Glass dataset has good coverage of the US economy, being the universe of online vacancies and 70% of total US vacancies, either online or offline (Carnevale, Jayasundera, and Repnikov, 2014). However we make significant restrictions that lead to a much smaller dataset, around 0.8% of total US vacancies.

Table 1, Panel A, lists the sample restrictions, going from the complete Burning Glass dataset of online vacancies, in row 1, and ending with the main regression sample in row 6. Moving from the first to the second row, we restrict to the set of vacancies that post wage information, either single wages or wage ranges. This step reduces the sample size by 82%, as the number of observations in the second column indicates. Moving from the second to the third row, we exclude vacancies posting a wage range. The sample falls by an additional

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[5]See Appendix Section C.1 for details of Burning Glass' algorithms.

63%. From the third to the fourth row, we drop vacancies that do not have job title and establishment information, reducing the sample by an additional 45%. From row 4 to row 5, we take the mean wage within each job and quarter cell, which reduces the sample by 3.6%. From row 5 to row 6, we restrict to jobs that post in multiple quarters, leading to an additional 66% sample size reduction. Row 6 is our main sample, which we will study unless stated otherwise.

Table 1, panels B and C, also report cell counts and other information for the main regression sample. Panel B shows that there are a large number of observations for all states and quarters in the main regression sample. The main sample contains a mixture of hourly and annual jobs, and contains a mixture of base pay or total pay wages. Panel C shows that almost all of the data is classified into detailed occupations. However there is more missing data for other fields, in part because Burning Glass collects several fields only after 2018. The length of vacancy posting, in the penultimate row of Panel C, is similar to vacancy lengths from official sources.[6]

The restrictions to arrive at the main sample are necessary. To exploit the job level information in our dataset, we must focus on vacancies with posted wages and job information. The jobs must be posted in multiple quarters, to allow a difference within the job and over time. This sample is large in absolute terms—numbering some 3.05 million vacancies and covering almost all occupations and counties. However the sample is small relative to the US population.

There are two concerns about measuring the wage for new hires using the main sample, which we try to address in the following subsection. First, the main sample may not be representative of the US population, because jobs that repeatedly post wages or job title information may be different from others. Second, wages posted on vacancies may not be an accurate measure of the wage for new hires, perhaps because of ex post bargaining or stale information.

2.1   Posted Wages as a Measure of the Wage for New Hires

We now show that our main sample of posted wages tracks measures of the wage for new hires from representative survey data—suggesting our main sample is reasonably representative and measures the wage for new hires adequately.

We construct an alternative measure of the wage for new hires from the Current Population Survey (CPS), for 2010Q1-2020Q4, at the state, industry and occupation level. The CPS is a worker level survey that is representative of the US population. The wage for new

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[6]The duration of vacancies is similar to the mean vacancy duration reported in Davis, Faberman, and Haltiwanger (2013) from the BLS's JOLTS survey, which is 20 days.

hires is from workers switching jobs over the previous quarter, identified as in Fallick and Fleischman (2004); or workers entering jobs from unemployment, identified as in Haefke et al (2013). We take the mean wage at the state, 2 digit SOC occupation, or 2 digit NAICS industry level.

Then, we validate wages in the main Burning Glass sample at the state by quarter, industry by quarter, or occupation by quarter level, by regressing log CPS wages on log wages from our main sample. To avoid attenuation bias in the regression coefficients, due to measurement error in Burning Glass wages, we adapt the method of Angrist and Krueger (1995). We half the data in each state-quarter and calculate mean state-quarter wages in each sub-sample. When we take the mean, we reweight by 6 digit occupation shares in the 2016 Occupational Employment Statistics, in order to ensure that the Burning Glass data are representative of the population of occupations. We then instrument for wages in one sub-sample with the other. This procedure uncovers an unbiased estimate of the population coefficient from a regression of log CPS wages on log Burning Glass wages at the state level, provided that measurement error in wages is independently distributed. We adopt a similar procedure in our industry and occupation regressions.[7]

Table 2 shows that wages in the main Burning Glass sample follow CPS wages reasonably closely. Panel A shows state level regressions, Panel B shows industry level regressions, and Panel C shows occupation level regressions. Column (1) is a simple bivariate regression; column (2) includes state fixed effects (in Panel A), industry fixed effects (in Panel B) or occupation fixed effects (in Panel C); and column (3) includes time fixed effects. The regression coefficient in column (1) is 1.06. So, when Burning Glass wages change by one percentage, wages for new hires from the CPS also change by roughly one percent. Going across the columns, the point estimate is not systematically different and remains close to 1 as we add in time or group fixed effects. Going down the panels, the fit is similar at the state, industry or occupation level. We cannot reject the coefficient equals 1 in most specifications.

Comparing average wages in the CPS and Burning Glass is useful, even though the CPS measures wages at the worker level, and Burning Glass at the job level. The reason is that *average* wages for new hires, in both job level and worker level datasets, should measure the same object. After all, all newly hired workers must be hired into a newly created job. Therefore the average wage for new hires, aggregating across workers, should equal the average wage for new hires, aggregating across jobs. If average wages in Burning Glass did not closely comove with average wages in the CPS, we might suspect mismeasurement in Burning Glass.

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[7]We include an additional two quarters of data of Burning Glass data, from 2020Q3 and 2020Q4, to compare Burning Glass and the CPS for the longest time period for which both datasets were available.

Still, this exercise has limitations. The partial R squared of the regression is sometimes high, for example 0.1 in the broad occupation regression with group fixed effects; but sometimes low, for example 0.03 in the state regression with state fixed effects; and the standard errors are relatively large in column (2) of Panel A. These patterns are not surprising, because survey wages are known to be noisy.[8]

There are likely three reasons why wages attached to online vacancies seem to measure the wage for new hires. First, for a representative survey of job-seekers, Hall and Krueger (2012) report that at least 30% and as many as 65% of workers do not bargain over the wage of the new vacancies to which they apply, and instead receive a wage dictated to them by their employer when they are hired.[9] Therefore for many newly hired workers, the wage attached to the vacancy is the relevant wage at the start of the match. Second, online vacancy posting is costly, which discourages firms from posting out-of-date wage information. The median cost of posting a vacancy on the largest four online job boards was $419 in 2017, and companies posting on their own websites typically pay monthly fees to subcontractors (see source in link). Gavazza, Mongey, and Violante (2018) show that company websites and online job boards are a large share of total recruiting costs for the typical US firm. Third, the duration of vacancies is short, which prevents "stale" vacancies. The median time for which vacancies are posted is 23 days, and 92% of vacancies are removed within the quarter.

To further address concerns about representativeness, Appendix Section C.2 provides a detailed comparison between our main sample and official, population-level data. To summarize, our main sample seems reasonably representative of the population. The occupation and industry mix as well as establishment characteristics are similar, and selection into the main sample is not correlated with business cycles. The main caveat is that the main sample under weights healthcare and over weights transportation and retail. In robustness exercises for our empirics, we reweight to the occupational or regional distribution of jobs, and find little change to our results.

2.2   Advantages of Job Level Data

Our job level dataset has an advantage relative to worker level survey data measuring the wage for new hires. We can track a measure of the wage for new hires at the job and

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[8]A concern is that the state level regression with time fixed effects has a coefficient of 0.85, well below 1. We suspect that this gap is sampling error, given that the other regressions with time fixed effects are at or above 1. Appendix Table 1 regresses the difference between state level wages in Burning Glass and the CPS on state unemployment and time fixed effects, and finds that the difference between these wages is uncorrelated with unemployment.

[9]Hall and Krueger (2012) find that 30% of workers knew their exact wage before being hired, and above 80% of workers "knew exactly or had a pretty good idea" of their wage before being hired.

Figure 2: An Example of a Job
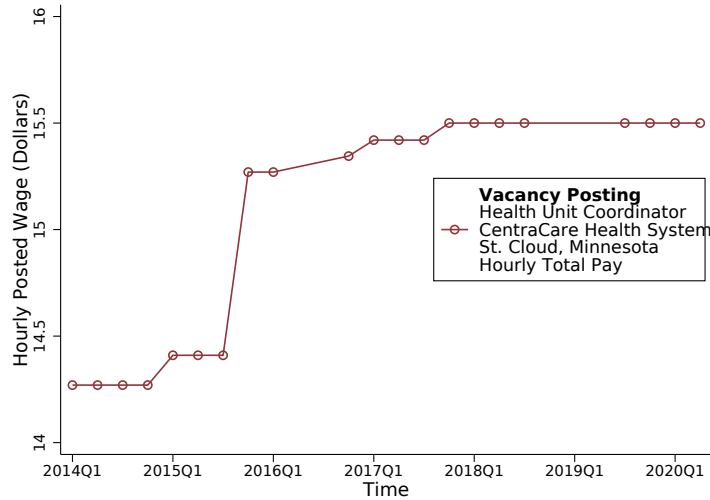
Notes: this graph plots an example of a job level wage. The hourly nominal posted wage is the mean within each quarter in which the job posts a vacancy.

the establishment level. We can track wages across multiple vacancies posted by the same job, within the same establishment. In coming sections, we use this feature to document downward rigidity in a measure of the wage for new hires faced by establishments.

Figure 2 displays job level variation. We present a job that posts multiple vacancies. The firm is CentraCare Health System. The establishment is the branch of the firm in St. Cloud, Minnesota. The job title is Health Unit Coordinator. The salary is hourly total pay. When the vacancy posts multiple times within the quarter, we take the average. Then according to our definition, a job is a health unit coordinator at the St. Cloud establishment of CentraCare. The job posts vacancies in 21 quarters over 7 years. We can track the wage across these vacancies—that is, we can track job level changes in the posted wage.[10]

Worker-level data cannot easily track job level variation in the wage for new hires. Workers are typically hired once into a job. So worker data cannot easily track the wage across *successive* workers, hired into the same type of job. Survey measures of wages, such as the Current Population Survey or the National Longitudinal Survey of Youth, typically measure workers' wages and do not contain job or establishment information.

In the sections to come, the job level data will let us document new findings about wage rigidity for new hires. However job level data has two further and related benefits. First, job level data can control for wage changes due to job composition. We will see that measures of

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[10]More generally, our measure of a job seems to capture the relevant heterogeneity in wages due to job level characteristics. In Appendix Table 2 we regress log wages on job fixed effects, finding that job fixed effects explain at least 82% of the variation in wages, meaning that the tasks associated with a given job title and establishment are relatively stable over time.

the wage that do not adjust for composition behave differently. Second, in standard models, variation in the wage for new hires at the job level is key for unemployment fluctuations. Therefore we are measuring a particularly important object in the data. This point is well known in the labor search literature (see, e.g. Gertler, Huckfeldt, and Trigari, 2020).

3 Constraint on Wage Setting for New Hires

This section presents evidence of a constraint on wage setting. We document that posted wages typically do not change between successive vacancies of the same job. Moreover when wages do change for a given job, they rarely fall.

3.1 Hazard Estimation of the Probability of Wage Changes

For the job in Figure 2, there is a distinctive pattern: the wage changes infrequently across vacancies. When the wage does change, it always rises. We now provide more systematic evidence of this pattern.

Measuring the frequency of wage change is difficult because the wages of a job are "missing" in the quarters without vacancy posting. Therefore we cannot directly observe the probability that the wage for new hires changes. We adapt a standard approach from the price setting literature to overcome this problem (Nakamura and Steinsson, 2008; Klenow and Kryvtsov, 2008). We treat the wage as a latent variable, which evolves stochastically when it is unobserved, and treat the observed sequence of wages as draws from the latent process. We estimate the latent process with a constant hazard model. We can then calculate the probability that the wage changes, even if jobs do not post in all quarters.

The constant hazard model has several desirable properties. If the observed wage does not change between successive vacancies, the latent wage also does not change. If the observed wage does change, the latent wage also changes. The latent wage can change multiple times if the observed wage changes once, and is more likely to change if the gap between successive vacancies is longer.[11] One can easily adapt this process to separately estimate the probability of wage increase and decrease. We use implied durations to measure for how long wages are unchanged. Following Nakamura and Steinsson (2008) and Klenow and Kryvtsov (2008), we view the constant hazard model as a convenient way to summarize the data. However the model will not accurately represent all features of the data generating process if, for example, there is state dependence in wage setting.

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[11]Appendix Section C.3 describes the hazard estimation procedure in full.

Figure 3: Distribution of Nominal Posted Wage Growth, Non-Zero Wage Growth Only
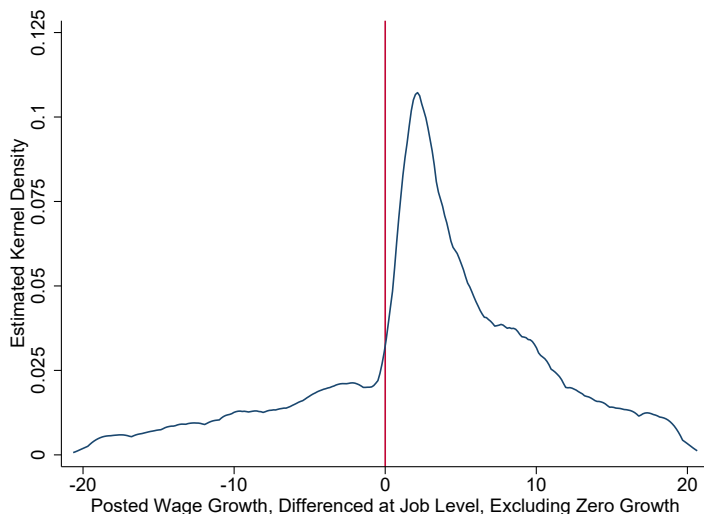
Notes: this graph is the distribution in the growth of nominal posted wages, excluding zeros, from Burning Glass, with a kernel density. Wages are averaged by job and quarter. Wage growth is the growth in wages between two consecutive vacancies posted by the same job, measured in percentage points. The wage growth distribution is truncated at ±20%. The sample period is 2010Q1-2020Q2.

3.2   Infrequent Wage Changes at the Job Level

We find that the nominal posted wage changes infrequently, suggesting a constraint on wage setting for new hires at the job level. Table 3 reports the results. Row (1) estimates the quarterly probability of wage change according to our method. Row (2) reweights vacancies at a granular level, to target the distribution of jobs from the 2016 Occupational Employment Statistics. Row (3) reweights to target the state distribution of jobs from the Quarterly Census of Employment and Wages. Row (4) drops jobs from the bottom quartile of the wage distribution, since minimum wages might cause infrequent changes. Across all columns, the probability of wage change is similar and low, between 0.17 and 0.19 per quarter. The corresponding implied durations of unchanged wages are 4-5 quarters.

The posted wage changes infrequently even as many successive vacancies are posted for a given job. Appendix Table 3 reports, for each vacancy, the length of time that has elapsed since a previous vacancy was posted for the same job. For over 90% of vacancies, less than 5 quarters have elapsed since the job posted a previous vacancy. Therefore jobs often post vacancies several times, over multiple quarters, without changing the wage. Data tracking individual workers' wages cannot easily measure the frequency of wage change for new hires. Workers are typically hired once into a job, but the object of interest is the wage across *successive* workers hired into the same type of job.

Infrequent changes in posted wages suggest a constraint on wage setting. We now present evidence suggesting this constraint is asymmetric. Figure 3 plots the distribution of wage

growth, after removing vacancies with zero wage change. We take the distribution of wage growth for new hires between two consecutive vacancies posted for the same job, and then exclude observations with zero wage growth. As before, we average wages within each job-quarter, meaning wage growth is quarterly. We truncate the plot at $\pm 20\%$ wage growth.

There are two clear points. First, conditional on a wage changing, wages for new hires rise more often than they fall. Secondly, wages "pile up" close to zero—there are many small positive wage increases, but far fewer small wage decreases. Both points suggest a downward constraint on wage setting for new vacancies of a given job.

We then estimate the probability of wage increases and decreases for new hires. The results are in Table 3, columns (3) and (4). As expected, wages are more likely to rise than to fall. For example, in the unweighted specification of Row 1, the probability of wage decreases is 0.04, whereas the probability of wage increases is 0.12. Table 3 shows that the finding is similar across several specifications, including after reweighting to target the occupational or regional distribution of jobs, or excluding low wage jobs to remove the effect of minimum wages.

Overall, the pattern of infrequent changes and especially rare decreases suggests an asymmetric constraint on wage setting. An alternative explanation for these patterns relates to vacancy fill rates. Vacancies posting a high wage might be filled quickly, whereas vacancies posting a low wage might be filled more slowly. Therefore jobs that initially post vacancies with a low wage might raise wages in the future, in order to raise vacancy filling rates. As a result, wage increases might be more likely than wage decreases, regardless of any constraint.

We can directly test for the relevance of vacancy fill rates using information on the length of time for which vacancies are posted. This variable, which is collected by Burning Glass, is a proxy for vacancy fill rates. Appendix Table 4 finds that the probability of wage increase and decrease is similar for vacancies that are posted for long or for short periods of time. This result suggests behavior relating to vacancy fill rates cannot explain the pattern of wage increases and decreases that we estimate.

Our finding—that a measure of the wage for new hires changes infrequently and falls especially rarely—has not been previously documented. We provide context with a better known fact: workers in continuing employment rarely experience wage changes.

Figure 4 shows that the duration of unchanged wages is similar for continuing jobs and for posted wages. The figure presents estimates from four papers that are representative of the literature, of the length of time for which wages are unchanged in continuing jobs. The first two papers, Card and Hyslop (1997) and Daly and Hobijn (2014), construct the frequency of wage change from worker level survey data, which is thought to overestimate the frequency of wage change due to measurement error. The third estimate, from Barratieri et

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Figure 4: Duration of Unchanged Nominal Wage for Continuing Workers and Posted Wages
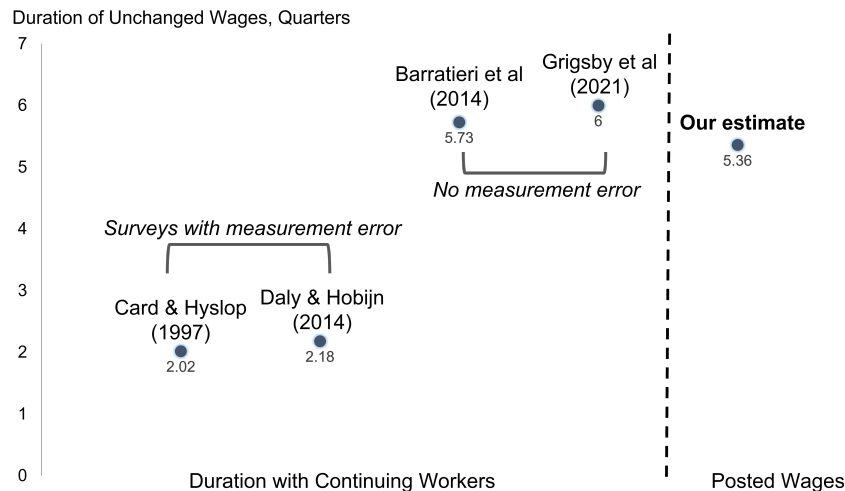
Notes: this graph plots the implied duration for which wages are unchanged from four papers that study continuing wages using payroll and survey data, alongside our estimate for posted wages using Burning Glass data.

al (2014), studies worker level data with a correction for measurement error. The final paper, Grigsby et al (2021), studies high quality payroll data. Two estimates are close to ours: the estimates of Grigsby et al (2021) and Barratieri et al (2014), which are both unaffected by measurement error.

The probability of wage cuts is also similar for posted versus continuing wages. Over 2008-2016, Grigsby et al (2012) report that 5% of base wage changes are wage cuts, and around 25% of changes in total wages (including bonus and overtime) are wage cuts. For a similar period, Kurmann and McEntarfer (2019) study high quality payroll data from Washington State, and find that around 25% of changes in total wages are wage cuts. Over the Pandemic Recession, Cajner et al (2020) report that 13% of base wage changes are wage cuts. We find a number in this range, i.e. 23% of wage changes are wage cuts.[12]

Our findings suggest that new and continuing wage changes are broadly governed by the same considerations, which relates to a debate on how wages are set for new hires. Some previous work conjectures parity in wage setting due to internal equity between new hires and continuing workers (Bewley, 2002). Our finding for posted wages supports this argument. However other plausible mechanisms predict differences between new hires and continuing workers. As one example, implicit contracting models imply that continuing wages should be rigid downwards, while wages for new hires should be *flexible* downwards (Beaudry and DiNardo, 1991). As a second example, continuing workers might have a reference point of their own past wage, and object to wage cuts because of morale. These considerations might matter less for new hires (Eliaz and Spiegler, 2014).

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[12]The summary statistics in Table 1 show that our data contains a mixture of base and total wages.

4   Wage Cyclicality

Motivated by our previous finding of asymmetric wage changes, this section asks whether posted wages respond differently to contractions versus expansions in the business cycle. We find that, across successive vacancies posted by the same job, the nominal posted wage does not fall during contractions, but does rise during expansions.

We study the response of wages to unemployment, following Bils (1985), and focus on regional business cycles. There were three major regional business cycles over 2010-2021. First, there was the Pandemic Recession, which had an uneven regional impact (Chetty et al, 2020). Second, there was a boom and bust in natural resource producing states, such as Texas, due to fluctuations in the global oil price between between 2010 and 2016. Third, our sample includes the recovery from the Great Recession, though the sample excludes the Great Recession itself.

We estimate a variant of the standard regression of Bils (1985) and add an additional term for asymmetry, motivated by our previous findings. We estimate the cyclicality of posted wages at the job level as

$$\Delta_{t,t-j} \log w_{ist} = \alpha + \gamma_t + \zeta_t I\left[\Delta_{t,t-j} U_{st} < 0\right] + \beta \Delta_{t,t-j} U_{st} + \delta I\left[\Delta_{t,t-j} U_{st} < 0\right] \Delta_{t,t-j} U_{st} + \varepsilon_{is,t,t-j}.$$
(1)

Here, w_{ist} is the nominal posted wage in job i and quarter t. Using the difference operator $\Delta_{t,t-j}$, we difference wages between two consecutive quarters $t - j$ and t in which the job posts a vacancy—this gap may be more than a single quarter. This step isolates job level wage changes. $\Delta_{t,t-j} U_{st}$ is the change in quarterly state unemployment, differenced over the same quarters as the vacancies, and $I\left[\Delta_{t,t-j} U_{st} < 0\right]$ is an indicator variable for whether unemployment is falling. γ_t is a time fixed effect, which isolates regional variation; we also interact the time fixed effect with an indicator for whether unemployment is rising or falling, denoted by $\zeta_t I\left[\Delta_{t,t-j} U_{st} < 0\right]$. β and δ measure the sensitivity of the posted wage to regional unemployment. A more negative number indicates greater sensitivity. If $\delta < 0$, then wages comove more with unemployment during expansions than contractions. If $\beta = 0$, then wages do not comove with unemployment during contractions.

4.1   Asymmetric Response of Wages to Unemployment

We now introduce the key empirical result. Figure 1, previously shown in the introduction, shows that when unemployment rises, the nominal posted wage does not fall—whereas wages do rise as unemployment falls. Moreover the comovement of wage growth and unemployment changes is piece-wise linear around zero unemployment changes. Therefore nominal wages

respond similarly to large and small changes in unemployment.

Table 4 confirms these results by estimating regression (1). Column (1) of the table estimates the regression without time fixed effects. The coefficient on ΔU_{st} has a slightly positive value of 0.05—meaning the posted wage does not respond to contractions. The coefficient on $\Delta U_{st} \times I(\Delta U_{st} < 0)$ has a significantly negative value of -1.4—therefore wages are more sensitive to expansions than contractions in unemployment. Summing the coefficients shows that wages are flexible upward: after a fall in unemployment of 1 pp, wages grow by 1.35%. The results are robust across several specifications. Column (2) is our preferred specification, which adds time fixed effects in order to isolate regional variation. The coefficient on ΔU_{st} becomes slightly negative, with a value of -0.05, and the coefficient on $\Delta U_{st} \times I(\Delta U_{st} < 0)$ is even more negative than in column (1). Column (3) adds state-specific trends and finds a similar result. Column (4) drops the $I[\Delta U_{st} < 0]\Delta U_{st}$ term from our benchmark regression to estimate the average sensitivity of wage growth to unemployment changes, while still including time fixed effects. On average wages do comove negatively and significantly with unemployment. The coefficient of -0.65 implies that a 1 pp fall in unemployment raises wages by 0.65%. However this average comovement is entirely driven by expansions and not contractions.

So far, we have studied nominal wages. Real wages are more relevant in many theoretical models, and could behave differently from nominal wages depending on price inflation. We now show there is similar rigidity and asymmetry in real wages. We study a version of regression equation (1), but replace the outcome with real wages. Real wages are deflated using regional prices measured at the census division level. We study two measures of the consumer price index, either including or excluding shelter. Correspondingly, we measure unemployment at the census division level for the analysis with real wages.

Table 5 shows with regressions that real wages are rigid downward and flexible upward. We estimate our preferred specification with time fixed effects. For comparison, column (1) of the table re-estimates nominal wage cyclicality at the census division level. Nominal wage rigidity is similar to the previous state level estimates. Column (2) then estimates real wage cyclicality, deflating by consumer prices excluding shelter. The coefficient on ΔU_{st} of -0.15 implies that when unemployment rises by 1 pp, real wages fall slightly. However the coefficient on $\Delta U_{st} \times I(\Delta U_{st} < 0)$ is significantly negative, meaning real wages are more sensitive to expansions than to contractions. Summing the coefficients shows that real wages are flexible upward: in response to a fall in unemployment of 1 pp, real wages grow by 1.2%. Column (3) estimates real wage cyclicality deflating by all consumer prices including shelter. The degree of downward wage rigidity is similar to column (2)—the coefficient on ΔU_{st} changes slightly to -0.10. The degree of upward wage flexibility is smaller, though

still significantly negative, since the coefficient in column (3) is closer to zero with a value of -0.37.

The coefficient on ΔU_{st} is similar in all three specifications, meaning nominal and real wages are both downwardly rigid. The coefficient on $\Delta U_{st} \times I\left(\Delta U_{st} < 0\right)$ is closer to zero for real wages than for nominal wages. However real wages are still flexible upwards, especially if the price level does not include shelter.[13] The similar behavior of nominal and real wages is probably because regional prices are sticky (Hazell, Herreno, Nakamura, and Steinsson, 2022).

4.2 Wage Rigidity with Respect to Labor Demand Shocks

We now turn to an identification challenge. In labor search models, the object of interest is typically wage rigidity with respect to labor demand shocks. However the major contraction during our sample period is the Pandemic Recession, which also included labor supply shocks—for instance from fears of disease. Labor supply shocks could mean that wages do not fall during contractions, even if there were no constraint on wage setting. We now describe various strategies that attempt to isolate labor demand variation.

Our first strategy to include labor demand variation in our regressions is to end the main sample in June 2020. Therefore the sample includes the contractionary phase of the Pandemic Recession, which officially took place during February-April 2020, but does not include the aftermath of the contraction. As such, we try to include the portion of the Pandemic period during which labor demand shocks were arguably important. Appendix Figure 1 plots the employment-population ratio from the start of 2019 onwards. The first half of 2020 contains a sharp contraction and rebound, whereas the period after June 2020 is a more gradual recovery. Existing theoretical and empirical work suggests that a large part of the sharp contraction in employment was due to labor demand. For instance, Guerrieri et al (2022) study a two sector model with downward nominal wage rigidity and incomplete markets. They argue that under plausible conditions, a shutdown of one sector represents a contractionary labor demand shock to the sector that remains open. Baqaee and Farhi (2022) consider a model with multiple sectors, input-output linkages, and downward nominal wage rigidity. They find that labor demand shocks can explain half of the contraction in real GDP during the Pandemic Recession. Empirically, Forsythe et al (2020) show that declines in vacancy posting were similarly large for in-person versus work-from-home occupations—the latter jobs are less likely to be affected by labor supply shocks.

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[13]Measures of real wages that exclude shelter are likely more relevant for labor search models, since the relevant real wage depends on producer prices, which do not including housing.

Appendix Figure 2 presents suggestive evidence that labor supply shocks may have become more important after June 2020. This figure repeats the main figure of the paper, Figure 1, in blue circles. In red triangles, we add observations from July 2020 - March 2021, which were also available at the time of writing. The data from July 2020 onwards suggests supply shocks, because wage growth is higher on average, and uncorrelated or even positively correlated with unemployment changes.

We now use four strategies to isolate labor demand shocks before June 2020, and tentatively argue there is downward wage rigidity with respect to these shocks. Our first strategy adds controls for labor supply shocks to the baseline regression equation (1). The controls are designed to absorb observable supply shocks, such as the effect of unemployment insurance (UI) extensions under the CARES act. However this strategy will not work well if we have failed to control for a large labor supply shock.[14]

The second strategy instruments for unemployment with a standard industry "shift share" measure of labor demand. The instrument is a weighted average of national industry employment growth, with weights depending on the state's employment share in each industry. The identification assumption is that differences in national employment growth across industries are due to labor demand and not labor supply shocks. Appendix Table 5 estimates the first stage regression corresponding to this instrument, and finds a strong first stage both for contractions and for expansions as measured by the instrument. As a by product, this instrument also corrects attenuation bias due to measurement error in state unemployment.[15]

The third strategy classifies sectors or occupations that, according to theory, were affected by labor demand and not labor supply shocks. We use two different classifications. The first classification follows Guerrieri et al (2022), who suggest sectors that were not shut down were subject to labor demand shocks. Therefore we re-estimate our baseline regression only for occupations amenable to remote work (Dingel and Neiman, 2020). The second classification follows Baqaee and Farhi (2022) who identify a set of sectors that faced labor demand shocks, in a calibration of their model. Importantly, their calibration strategy does not use information on wages.

Our final strategy develops another shift share instrument based on regional exposure to oil shocks, which uses variation only from before the Pandemic Recession. Specifically, we instrument for unemployment with the quarterly global oil price interacted with state

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[14]Specifically, we add as controls, the interaction of an indicator variable for 2020 onwards, with: the length of state shutdowns; quarterly Covid case and death counts; the state's median UI replacement after the CARES act; and the state's 2010-2019 share of female employment.

[15]Appendix Figure 3 suggests variation in industry employment growth during the Pandemic Recession was primarily determined by labor demand. For example, gambling had large employment falls,whereas home building materials had a large rises, which suggests changes in labor demand from the the goods market. See Chodorow-Reich, Darmouni, Luck, and Plosser (2022) for a similar instrument.

specific indicators and indicators for whether the oil price was increasing or decreasing. This regression contains many instruments and therefore is estimated by LIML as Angrist and Frandsen (2022) recommend. This regression only includes data from before 2020. This instrument exploits the large variation in the global oil price before 2020—Appendix Figure 4 shows that the oil price increased substantially between 2010-2014 and then fell sharply. The identification assumption is that oil price shocks particularly affect labor demand in states concentrating in natural resources, such as Texas (Acemoglu, Finkelstein, and Notowidigdo, 2013).

Table 6 shows the results. Reassuringly, across a range of identification strategies, wages seem to be rigid downward and flexible upward. The first column is the baseline regression estimates, which do not attempt to identify labor demand. Column (2) adds labor supply controls to the regression. Both regression coefficients change by a small amount. Column (3) studies the industry shift share instrument. The coefficient on ΔU_{st} declines slightly to -0.45, though is less precisely estimated. The coefficient is not significantly different from zero, and still suggests downward rigidity, albeit less than the baseline estimate. With the shift share instrument, the degree of asymmetry is even greater, since the coefficient on $\Delta U_{st} \times I\left(\Delta U_{st} < 0\right)$ takes a more negative value of -2.47. Columns (4) and (5) estimate wage cyclicality only for work from home occupations and for industries facing demand shocks, respectively. In both cases the degree of wage rigidity is similar to the baseline. Column (6) estimates wage rigidity instrumenting for unemployment with the oil price instrument, using only variation from before 2020. The estimates are less precise but broadly agree with the others—the coefficient on ΔU_{st} is positive, which suggests that wages do not fall in response to contractions. Again, there is a large negative coefficient on $\Delta U_{st} \times I\left(\Delta U_{st} < 0\right)$. Column (7) re-estimates the baseline specification excluding data from 2020 onwards, therefore restricting to a period where labor supply shocks were likely less important. The coefficients again suggest that wages were rigid downward and flexible upward, with similar magnitudes to the baseline.

4.3 Downward Wage Rigidity—Robustness Tests

The remainder of this section studies various robustness tests. Table 7 groups together some robustness tests about our key finding. Each row estimates versions of our benchmark regression, reporting the coefficient on $I\left[\Delta U_{st} < 0\right]\Delta U_{st}$ and its standard error. If this coefficient is negative, then wages are more rigid downward than upward.

In all cases, the coefficient changes little or becomes more negative. Row 1 is our baseline specification from column (1) of Table 4. Rows 2 and 3 explore forms of seasonal adjustment,

by re-estimating at annual frequency in row 2; or seasonally adjusting unemployment and employment with the Census Bureau's X-11 algorithm, in row 3. Rows 4 and 5 explore whether there are dynamic selection concerns, because jobs that re-post vacancies without filling them may set wages differently from the others. Row 4 controls for the length of time for which vacancies were posted; row 5 excludes jobs that post vacancies in consecutive quarters, these jobs are most likely to re-post without filling.[16] Row 6 verifies our results are robust to excluding jobs with bonus pay. Rows 7-9 study representativeness. Row 7 reweights to the occupation distribution at the 6 digit SOC level. Row 8 reweights to the regional distribution of employment with time invariant weights; row 9 does the same using time varying weights lagged by one quarter. Row 10 studies a broader definition of a job, as a job title within an establishment, while pooling across pay frequencies. Row 11 shows similar results if we also include wage ranges, and take the mean of the wage range.

Our Appendix contains further robustness exercises. These exercises use different sources of variation to show that downward wage rigidity is pervasive. First, we find that downward wage rigidity is common across occupations and industries. Appendix Table 7 estimates the baseline regression (1) for five broad occupations, and finds across all of them that wages are rigid downward and flexible upward. Appendix Table 8 estimates the baseline regression for 10 broad industry sectors, and detects similar pattterns across all sectors.

We also find downward rigidity at the establishment level. In principle, even though job level wages seem to be downwardly rigid, establishments might avoid wage rigidity by changing their mix of jobs. Therefore we re-estimate the baseline regression regression (1) at the establishment level, by replacing the outcome variable with the mean nominal establishment wage, averaging across all jobs posted by an establishment in a given quarter. Appendix Table 9 reports the results, finding a similar degree of rigidity.

One concern is that jobs posting only once—which do not enter the main sample of job level differences—have relatively flexible wages. One test for this concern is to ask whether jobs that post infrequently, but more than once, tend to have more flexible wages. Appendix Table 10 estimates downward wage rigidity, for jobs that post few or many times. Both kinds of jobs display similar of downward wage rigidity, suggesting that posting infrequently is not associated with more flexible wages.

Appendix Table 11 estimates downward wage rigidity separately for each source of the vacancy data, since the data is drawn from a mix of online job boards and company websites. All sources show downward wage rigidity, suggesting our result is not driven by the mechanics of a particular method of posting vacancies. Appendix Table 12 regresses wage growth, at

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[16]Additionally, Appendix Table 6 estimates the baseline regression separately for each quartile of the distribution of vacancy posting times, and finds similar results for each quartile.

the job level, on quarterly industry employment growth, at the 3 digit industry level. The regression shows that wages are rigid downward and flexible upward. Appendix Figure 5 graphs the data corresponding to this regression, in a binned scatterplot, and finds patterns consistent with the regression. Appendix Table 13 estimates our baseline regression by quartiles of establishment size, and finds that downward wage rigidity is pervasive across establishment size. Appendix Table 13 also estimates our baseline regression across the four quartiles of the wage distribution, and again finds that downward wage rigidity is widespread.[17]

Now, we consider two qualifications to our finding of downward wage rigidity. Both are important, but we believe neither diminishes our results entirely. First, we investigate whether our results are explained by selection into posting wages. The main sample contains only jobs posting wage information. However firms might post jobs without wage information—dropping from the regression sample—when they expect to renegotiate the wage during hiring. If true, then wage flexibility estimated from wage postings will understate true wage flexibility.

Therefore we estimate whether posting wages on vacancies is cyclical. To do so, we must study a different dataset from the main regression sample, to also include vacancies that do not post wages. For every job and quarter, we calculate the share of vacancies that post wages. We difference the share of vacancies posting wages at the job level, and then regress the job-level difference on unemployment changes. Other than the outcome variable, the regression is similar to the baseline job level regression (1).

Appendix Table 15 reports the result and does not detect selection into posting wages. The main coefficients of interest are in column (2), which estimates the regression specification with time fixed effects. The coefficient on ΔU_{st} is a precise zero. Therefore after a contraction, the share of vacancies posting wages does not change. The coefficient on $\Delta U_{st} \times I (\Delta U_{st} < 0)$ is also zero, meaning that the share of vacancies posting wages does not change during expansions—whereas, if there were selection, the share of vacancies posting wages would rise during expansions.

Section 6 will take a more structural approach to the same issue, modelling the decision to post vacancies with wages and making a selection correction. This approach also does not suggest selection bias. In principle, there could be selection into posting wages at the state level, instead of the job level. However in Appendix Section C.2 we show that at the

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[17]Our regression does not control for the length of time between posts, which is not predetermined with respect to unemployment changes and hence is a "bad control" (Angrist and Pischke, 2009). Appendix Table 14, Panel A controls for the lagged length of time between posts, which is predetermined, and finds the results unaffected. The current and lagged length of time are highly correlated (as Panel B shows), because the frequency of posting is largely a job level characteristic (as panel C shows).

state level, the share of vacancies posting wages is acyclical. Therefore selection into wage posting at the state level also seems unlikely.

A second qualification is that our data measures the posted wage. Suppose that workers bargain instead of accepting the wage posted on the vacancy, so there is a gap between posted and realized wages If this gap is cyclical, then posted wages may be less cyclical than realized wages, and our estimates will be biased. We suspect this concern—though reasonable—does not fully undermine our result.

To this end, we now show that posted wages are rigid even in occupations where wage bargaining is rare, or in occupations where workers typically accept the posted wage. We use data from the survey run by Hall and Krueger (2012), which asks workers first whether they bargained over wages, and second whether they accepted the posted wage. The survey also reports the 2 digit occupation of the new hire. We divide occupations into four quartiles, depending on whether either wage bargaining or accepting a posted wage is common. We estimate the baseline wage rigidity regression for every quartile.

This exercise is useful because of the following scenario: consider an occupation in which no worker bargains, or an occupation in which all workers accept the posted wage. Suppose we identify downward rigidity in posted wages for this occupation. There must also be downward rigidity in realized wages, because workers have accepted the posted wage and have not bargained. Our robustness test generalizes this idea by asking whether posted wages are still downwardly rigid in occupations where bargaining is unlikely, and accepting the posted wage is common.

Appendix Table 16 reports the results. In Panel A, we estimate wage rigidity separately for occupations in which wage bargaining is least common (column 1), through to occupations in which wage bargaining is most common (column 4). The coefficients are similar and do not seem to vary systematically across the groups. In Panel B, we estimate wage rigidity separately for occupations in which accepting a posted wage is least common (column 1), through to occupations in which wage posting is most common (column 4). Again, the coefficients are similar and do not seem to vary systematically across the groups.

The results suggest that a cyclical gap between realized and posted wages cannot easily explain our findings. Posted wages are downwardly rigid even in occupations where wage bargaining is rare or workers typically accept the posted wage. Moreover, wage rigidity is similar across all occupations, regardless of the degree of bargaining or accepting a posted wage.

5 Job Composition and the Average Wage for New Hires

So far, we have provided new evidence that a measure of the wage for new hires is rigid downward and flexible upward. Previous work does not detect this asymmetry. However previous work often studies the *average* wage for new hires, from worker level survey data that controls for worker characteristics and averages across jobs. This section shows that due to job composition, average wages may have higher variance than job level wages, meaning regressions with average wages have limited power to detect downward wage rigidity. We also discuss a related issue, which turns out to be less important during our sample period: job composition might create a form of omitted variable bias.

To start, we define measures of the wage for new hires at the average and job level. The wage for a newly hired worker in job type i, state s, and quarter t is w_{ist}. The share of new hires in job type i during the state-quarter is ν_{ist}. Our dataset of wage postings measures growth in the *job level* wage for new hires, $\Delta \log w_{ist}$. Some previous work, surveyed by Pissarides (2009), measures the *average* wage for newly hired workers, from survey data without information on jobs or establishments. Researchers study the change in the mean log wage $\Delta \overline{\log w_{st}} = \Delta \left[\sum_i \nu_{ist} \log w_{ist} \right]$.[18]

Estimates using average wages often have large standard errors. To understand this pattern, first note that average and job-level wage growth can differ if job composition changes. A first order expansion of average wages yields an additive relationship between average wage growth, job level wage growth, and composition:

$$\Delta \overline{\log w_{st}} \approx \sum_i \nu_{ist} \Delta \log w_{ist} + \sum_i \log w_{ist} \Delta \nu_{ist}. \tag{2}$$

Average wages can change, even if job-level wages do not change. Suppose that wages are unchanged at the job level during a given quarter—that is, the first term on the right hand side of equation (2) is zero. If the share of low wage hires increases, wages change due to composition. The second term on the right hand side of equation (2) falls, so average wages fall.

From inspecting equation (2), we can see how job composition can raise the variance of average wages relative to job-level wages. Given downward rigidity, job-level wage growth $\Delta \log w_{ist}$ is small. So the first term on the right hand side of equation (2) has low variance. However suppose that the share of job types ν_{ist} is volatile. Then the second term on the right hand side can have a high variance, so that average wage growth can have higher

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[18]Previous work often conditions on worker characteristics, by residualizing log wages against worker characteristics and then taking the average (e.g. Haefke et al, 2013); or equivalently by running regressions on worker level data while controlling for worker characteristics (e.g. Gertler et al, 2020).

variance than job-level wage growth. Consider the example of an economy with high wage bankers and low wage baristas. Suppose that sometimes there are relatively many banker jobs, and sometimes many barista jobs. Then average wages will vary, even if wages change little for either bankers or baristas.

As well as raising the variance of average wages, job composition might also create a form of omitted variable bias. Suppose that the share of low wage jobs rises during recessions. Then, from equation (2), average wages must systematically fall during recessions, which confounds estimates of wage rigidity (Solon et al, 1994). However, in Appendix Table 17, we find that the share of vacancy creation in low wage jobs was not cyclical during this period, different from earlier periods (Hagedorn and Manovskii, 2013; Gertler et al, 2020). This finding is consistent with other work over the Pandemic Recession, which finds that declines in vacancy posting were widespread and similarly large across most occupations and industries (Forsythe et al, 2020).[19] Therefore we do not focus on omitted variable bias due to job composition, though it is likely important at other times.

We now explain how job composition affects inference, more formally. Our benchmark regression estimates downward rigidity using job level wage variation. That is, we study the population regression function

$$\Delta \log w_{ist} = \alpha + \gamma_t + \beta_{\text{Job Level}} \Delta U_{st} + \delta_{\text{Job Level}} I \left[\Delta U_{st} < 0 \right] \Delta U_{st} + \varepsilon_{ist}, \tag{3}$$

where ε_{ist} has bounded variance. We are interested in $V \left[\hat{\delta}_{\text{Job Level}} \right]$, the variance of the OLS estimator of $\delta_{\text{Job Level}}$. If $\delta_{\text{Job Level}}$ is negative, then the wage for new hires is more rigid downward than upward at the job level.

Suppose a researcher only has access to average wages for new hires, as in prior work. A natural regression to study downward wage rigidity in average wages is

$$\Delta \overline{\log w_{st}} = \bar{\alpha} + \bar{\gamma}_t + \beta_{\text{Average}} \Delta U_{st} + \delta_{\text{Average}} I \left[\Delta U_{st} < 0 \right] \Delta U_{st} + \bar{\varepsilon}_{st}. \tag{4}$$

This regression is the analogue of our job-level regression, with average wages as the outcome variable. If estimates of δ_{Average} are negative, then one concludes that average wages are downwardly rigid. If average wages are noisy, then the variance of the OLS estimator of δ_{Average}, which we term $V \left[\hat{\delta}_{\text{Average}} \right]$, will be large.

In Appendix Section D, we show that job composition inflates the variance of $\hat{\delta}_{\text{Average}}$ relative to $\hat{\delta}_{\text{Job Level}}$. Thus regressions using average wages may lack the power to detect downward rigidity, even if it is present at the job level. From equation (2), the difference

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[19]In contrast to the pattern for vacancy creation, separations were much larger for low wage workers during the Pandemic Recession (Cajner et al, 2020).

Figure 5: Estimates of Downward Rigidity in Job Level and Average Wages

Regression Outcome Variable
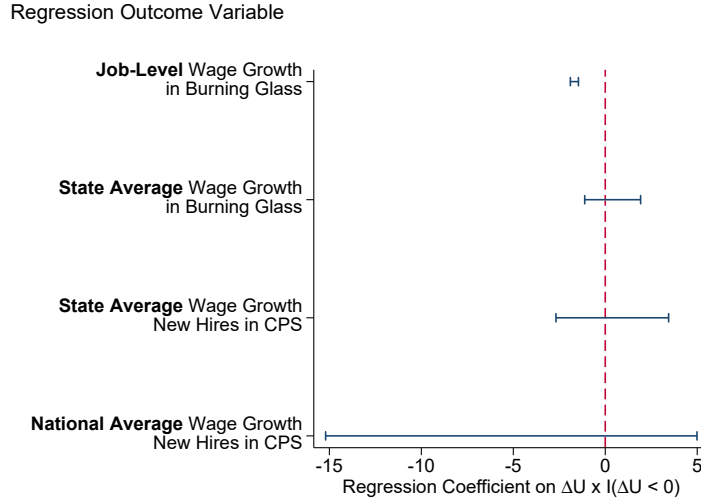
Notes: the top row reports the estimate of $\delta_{\text{Job Level}}$, which estimates downward rigidity with job-level data on posted wages from Burning Glass. The next three rows report various estimates of δ_{Average}, which estimates downward rigidity with average wages for new hires from Burning Glass. The second through fourth rows use, as the average wage measure, state-quarter average wages from Burning Glass, state-quarter average wages from the Current Population Survey controlling for worker information, and national average wages from the Current Population Survey. See Appendix Table 18 for details.

between job-level and average wage changes comes from changing job composition. In a regression with average wages, the residual variance is higher, creating noisier estimates. By contrast, regressions with job level data purge noise due to job composition, and become precise.

We now show that in the data, job composition makes estimates with average wages imprecise. To understand this point, we compare estimates of $\hat{\delta}_{\text{Average}}$ in equation (4) with our baseline estimate of $\hat{\delta}_{\text{Job Level}}$. For the outcome variable, we construct average posted wage at the state level, from Burning Glass; and the average wage for new hires from the Current Population Survey (CPS). For average wages in the CPS, we follow the state-of-the-art procedure in Haefke, Sonntag, and Van Rens (2013), by measuring averages after residualizing wages against 3 digit occupation and industry fixed effects, and demographic controls; we also include job switchers in the average as in Fallick and Fleischman (2004).

We study quarter-by-state data for 2010-2020Q2, as in our benchmark regression. We report the standard error of $\hat{\delta}_{\text{Average}}$ to contrast with the standard error of $\hat{\delta}_{\text{Job Level}}$. In both cases, we cluster standard errors at the state level. This procedure consistently estimates the standard deviation of the estimators $\hat{\delta}_{\text{Average}}$ and $\hat{\delta}_{\text{Job Level}}$, given that the regressor ΔU_{st} varies at the state level.

Figure 5 reports the standard error of downward wage rigidity estimates, from job level and average wages. The difference in precision between the estimates using average and job level wages is enormous, meaning job composition does, indeed, inflate the variance of

estimators of downward rigidity. The top row of Figure 5 reports the standard error of our job level estimate of downward wage rigidity, $\hat{\delta}_{\text{Job Level}}$. The second row reports the standard error of $\hat{\delta}_{\text{Average}}$, the estimate of downward rigidity from average wages, using average posted wages from Burning Glass. The third row reports the standard error of $\hat{\delta}_{\text{Average}}$, with average wages for new hires from the Current Population Survey. The fourth row estimates $\hat{\delta}_{\text{Average}}$ using national wage growth for new hires and national unemployment changes, for 1984-2007. The sample period and measure of wages is the same as Haefke et al (2013). In all the regressions that use average wages instead of job level wages, the standard error is far higher. Therefore the variance due to job composition is large in practice, and precludes researchers from detecting downward rigidity in average wages. Appendix Table 18 reports the point estimates and standard errors from the regressions in Figure 5.[20,21]

6   Selection into Vacancy Posting

So far, we have exploited information on jobs, contained in vacancy postings, to estimate wage rigidity. However, selection into vacancy posting may affect our estimates. To understand such effects, this section studies a sample selection model following Heckman (1979), in order to apply a selection correction.[22]

In the model, time is discrete and there is a finite set of jobs. In period t and region s, job i decides whether to post a vacancy with a wage. The decision depends on a processes for aggregate unemployment U_{st} and an independent process for idiosyncratic productivity ϕ_{ist}. The decision also depends on the latent wage w_{ist}^*—the wage that jobs would pay, should they post a vacancy. When the job posts a vacancy, the actual and latent wage are equal, so $w_{ist}^* = w_{ist}$. Otherwise the latent wage is not observed.

We focus on a log-linear relationship between latent wages, unemployment and idiosyncratic productivity

$$\log w_{ist}^* = \alpha - \beta U_{st} + \gamma \phi_{ist}, \tag{5}$$

with $\gamma \geq 0$. Wages depend on aggregates, summarized by unemployment; and on the particulars of the job, summarized by idiosyncratic productivity. β is the measure of wage rigidity to be estimated. This equation resembles a simplified version of our benchmark regression,

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[20]Appendix Table 18 also considers a measure of average wages that controls for worker fixed effects, constructed by Basu and House (2016).

[21]The job-level estimates not more precise simply because Burning Glass contains many jobs. The degrees of freedom in the regression is the number of states, and not the number of jobs, and standard errors are clustered at the state level.

[22]Martins, Solon, and Thomas (2012) provide an alternative method, by estimating wage cyclicality for new hires only for a sample of "entry jobs", which hire at all phases of the business cycle.

but replaces the regression residual with a structural idiosyncratic productivity term.

Jobs choose to post a vacancy if the value is high enough, given previous employment at the job $n_{is,t-1}$. The value of a vacancy is $V_{ist} = V\left(U_{st}, \phi_{ist}, n_{is,t-1}\right)$, which is decreasing in its first argument, increasing in its second, and decreasing in its third. Higher unemployment signals unprofitable aggregate conditions that lower the value of a vacancy, whereas higher productivity raises the value. Higher past employment in the job may lower the value, due to decreasing returns to scale. The dependence on U_{st} and ϕ_{ist} account for the indirect effect on vacancy value via latent wages.

In the model, jobs post vacancies when the value is positive. Therefore the vacancy posting decision is

$$\xi_{ist} = 1\left\{V\left(U_{st}, \phi_{ist}, n_{is,t-1}\right) \geq 0\right\}, \tag{6}$$

where $\xi_{ist} \in \{0, 1\}$ is an indicator for whether the job posts vacancies. This simple sample selection model captures a key feature of the data—jobs enter and exit the vacancy data depending on idiosyncratic and aggregate shocks as well as wage rigidity.

Now, we use the simple model to make two points: first, regressions with average wages are biased by selection into vacancy posting; second, job level regressions should reduce this bias.

To show selection bias, we take expectations of the wage equation (5) conditioning on a vacancy having positive value and being posted, which implies

$$E\left[\log w_{ist}|U_{st}, V_{ist} \geq 0\right] = \alpha - \beta U_{st} + \gamma E\left[\phi_{ist}|V_{ist} \geq 0\right]. \tag{7}$$

We have a regression equation with an omitted variable bias. Average wages amongst jobs posting vacancies at time t is the outcome variable, and state unemployment is the regressor. The regression residual has a non-zero mean because the value of a vacancy V_{ist} depends on unemployment—this term represents selection bias. Estimates of β from regressing average wages on unemployment are always biased towards zero.

Equation (7) shows that due to selection, wages may seem rigid even if they are truly flexible. When aggregate unemployment U_{st} is high, then only jobs with high idiosyncratic productivity ϕ_{it} post vacancies. However when aggregate unemployment is low, low productivity jobs also post vacancies, which lowers average wages all else equal.

According to the model, regressions that difference wages at the job level should reduce selection bias. Taking a difference of equation (7) within the same job over any periods t

and $t - j$ in which the job posts a vacancy implies

$$E\left[\Delta_{t,t-j}\log w_{ist}|U_{st}, U_{s,t-j}, V_{ist} \geq 0, V_{is,t-j} \geq 0\right] = \alpha - \beta\Delta_{t,t-j}U_{st} + \gamma E\left[\Delta_{t,t-j}\phi_{ist}|V_{ist} \geq 0, V_{is,t-j} \geq 0\right],$$
(8)

which is a regression equation differencing wages at the job level. This equation resembles the baseline job level regression (1), with a selection bias term. If ϕ_{ist} is constant, so that idiosyncratic productivity does not vary over time, then the final term vanishes and there is no selection bias. If most of the variation in ϕ_{ist} is time invariant, then the degree of residual selection bias will be small. Importantly, the residual bias in the job level regression does not have a definite sign, unlike the bias in the regression with average wages—selection in the job level regression is now a more complicated function of both U_{st} and $U_{s,t-j}$, via their effects on V_{ist} and $V_{is,t-j}$.

Intuitively, the job level regression should reduce selection bias because this regression compares the same job over time, and removes variation from a changing selection of jobs. In the particular case of constant idiosyncratic productivity, the effect of selection on the job's wages is the same in both periods, meaning that a difference of wages removes the effect of selection entirely.

Therefore in practice, job level regressions will reduce selection bias if the relevant form of productivity is mostly time invariant at quarterly frequency. The available evidence tentatively supports this view. For example, Lachowska, Mas, Saggio, and Woodbury (2022) study quarterly data from Washington State and find that 93% of the firm component of wages is explained by time invariant firm characteristics. However, this evidence might not apply to our setting. We therefore turn to the data and a selection correction to see whether job level regressions reduce selection bias.

6.1   Selection Correction for the Job Level Regression

Now, we implement a standard Heckman (1979) correction, in order to test whether our job level regressions are affected by selection bias.

To motivate the correction, observe that the vacancy posting decision (6) is a standard selection equation, which captures two ways in which past employment affects the likelihood of posting a vacancy. First, if there are decreasing returns to scale in employment, then the right hand side of the inequality in equation (6) falls as $n_{is,t-1}$ increases. When jobs employ more workers, the value to hiring an additional worker by posting a vacancy may be lower. Second, jobs with different values of past employment, or jobs on different employment trends, will tend to have different conditional distributions of idiosyncratic productivity ϕ_{ist}. For instance, jobs with growing employment may be receiving positive idiosyncratic

shocks—and therefore be more likely to post vacancies.

The selection equation (6) suggests a standard Heckman two step procedure for correcting selection bias. In the first step, we estimate the probability of posting a vacancy, separately for periods t and $t - j$, as a function of past employment in the job. In the second step, we add estimates of the probability of vacancy posting in both periods as controls in our benchmark job level regression, equation (8). Following the standard Heckman logic, these controls absorb the selection bias term in equation (8).

We use the Heckman estimator proposed by Das, Newey, and Vella (2003), which does not impose parametric assumptions about selection, and flexibly controls for the effect of aggregate variables on selection. Appendix Section E contains more details.

The intuition underlying the Heckman estimator is straightforward. Jobs select into vacancy posting when they receive idiosyncratic shocks. The Heckman estimator "backs out" the idiosyncratic shocks, using the relationship between whether a job posts a vacancy and its past employment, in order to create a control for the effect of selection on wages.

We motivated this estimator as a correction for bias from selection into vacancy posting. However this estimator also corrects bias from selection into choosing to post a wage on a vacancy—since our model can be reinterpreted as framework for choosing whether to post wages on a vacancy, given aggregate and idiosyncratic conditions.

To implement the Heckman correction, we need to observe the employment of the job, even when the job does not post vacancies—Burning Glass cannot provide this information by definition. In order to obtain selection relevant information on jobs, we create a dataset that merges Burning Glass to annual establishment level employment from Dun & Bradstreet (D&B). Dun & Bradstreet is a business analytics company that collect information on the universe of establishments in the United States from 1990-2020 inclusive, for the purpose of credit scoring and marketing. D&B collects data on employment at the start of the year, industry classification and establishment age. We do a fuzzy merge between establishments in Burning Glass and D&B, matching on firm name, city, county and state (we describe our merge algorithm in Appendix Section C.5). For cost reasons, we purchased data from D&B only on the largest 30% of employers in Burning Glass. We achieve a high merge rate—75% of our main sample matches to establishments in Dun & Bradstreet.[23]

Dun & Bradstreet is known to measure employment poorly for very small establishments. Once these establishments are removed, Dun & Bradstreet appears to match official employment sources at the industry and regional level (Haltiwanger, Jarmin, and Miranda, 2013; Barnatchez, Crane, and Decker, 2017). Our extract from Dun & Bradstreet does not include very small establishments, suggesting our extract should measure establishment employment

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[23]Dun & Bradstreet was used by, for instance, Crouzet and Mehrotra (2020).

reasonably well. Appendix Section C.5 compares our D&B extract to official data from the Business Dynamics Statistics (BDS), and find a reasonably close match.

6.2   Selection Corrected Estimates of Job-Level Regression

We apply our selection correction to the merged dataset of Dun & Bradstreet employment and Burning Glass vacancies. Table 8 reports the results, and shows that selection bias is moderate in the job level regressions. Column (1) reports our baseline job level regression with time fixed effects, without controlling for selection bias, but restricting to the merged subsample. The estimates are similar to the full regression sample: wages are rigid downward and flexible upward, with regression coefficients on ΔU_{st} and $I\left(\Delta U_{st} < 0\right) \times \Delta U_{st}$ of -0.05 and -1.78, respectively. Column (2) corrects for selection. The results are broadly similar. The estimate of downward wage rigidity—the coefficient on the ΔU_{st} regressor—becomes slightly closer to zero, with a value of -0.005. The estimate of upward wage flexibility—the coefficient on the $I\left(\Delta U_{st} < 0\right) \times \Delta U_{st}$ regressor—also becomes closer to zero. The estimate is -1.48, a 17% change versus the estimate without the selection correction. Column (3) reports the difference in estimates between the regression coefficients with and without selection corrections, along with the standard errors of the difference.[24]

This selection correction suggests that in practice, the baseline job level regression is not greatly affected by bias from selection into vacancy posting. If anything, selection biases the job level regression towards estimating too little wage rigidity. However our selection correction is based on strong assumptions. Notably, we require that lagged establishment employment does not enter the wage equation (5) other than through its relationship with idiosyncratic productivity, which is necessary for identification (Das et al, 2003).

To strengthen our finding of moderate selection bias, we close this section with a reduced form approach to testing for selection. Equation (8) shows that selection bias will be strong for types of jobs that occasionally post vacancies. However for jobs that constantly post vacancies, regardless of aggregate or idiosyncratic conditions, selection bias will be small.

In practice, we can look for selection bias by comparing wage rigidity across occupations with low and high turnover. If wage rigidity were very different across occupations with high turnover—which are unlikely to be subject to selection bias—compared with low turnover occupations, then the latter group may be subject to selection bias.

Appendix Table 20 shows that wage rigidity is similar across occupations with low and

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[24]Appendix Table 19 carries out several robustness tests. We aggregate the regressions to the establishment level and to the annual level data; we use a simple control for employment growth instead of the full selection correction; we include entering and exiting establishments; we trim the propensity score for entry to values between zero and one; and we include lagged wages in the selection equation.

high turnover. We construct a measure of turnover, as the ratio of Burning Glass vacancies to occupation employment from the Occupational Employment Statistics. We then estimate our baseline regression equation (8), with time fixed effects, for four quartiles of the occupational turnover distribution. The estimates, both of downward wage rigidity and upward wage flexibility, are similar across the four quartiles. By itself, this test cannot fully rule out selection bias. However the test does suggest that selection bias is not a key feature of the data.[25]

7   Conclusion

We use a new dataset to show downward rigidity in a measure of the wage for new hires, at the job level. We present two pieces of evidence. First, the posted wage rarely changes between successive vacancies at the same job. When wages do change for a given job, they are three times more likely to rise than to fall. These findings suggest a downward constraint on the wage in newly created jobs. Second, at the job level, the posted wage rises during expansions but does not fall during contractions, meaning wages are rigid downward and flexible upward at the job level.

One important question that our paper does not answer is *why* the wage for new hires might be more rigid downward than upward at the job level. Some plausible mechanisms for downward wage rigidity apply to continuing workers and not for new hires. For example, firms might offer implicit contracts in the form of downwardly rigid wages to continuing workers, and not extend the same insurance to new hires (Beaudry and DiNardo, 1991). We hope future work can investigate the mechanisms behind downward rigidity for new hires, such as internal equity between continuing workers and new hires (Bewley, 2002).

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[25]One limitation of this test is that the Occupational Employment Statistics measures employment whereas Burning Glass measures vacancies; however official sources do not report disaggregated vacancy data.

8 Tables

Table 1: Summary Statistics

Panel A: Sample Formation

	Observations	Occupations	Counties
All Burning Glass	257,427,611	835	3,142
Range and point wages info	46,031,304	835	3,141
Point wage info	16,967,648	833	3,141
Point wage + job title	9,318,188	828	3,141
Point wage + job title, quarter collapse	8,990,702	828	3,141
Main sample	3,050,360	818	3,116

Panel B: Cell Counts for Main Sample

	10th p'tile	Median	90th p'tile
Observations per state	8,851	41,410	119,023
Observations per quarter	24,060	64,142	140,133

Panel C: Information for Main Sample

Fraction Missing (pp)	6 digit occupation 4	Length of posting 76	
Fraction Missing (pp)	Sector 14	3 digit industry 22	Vacancy Source 67
Share by Pay Frequency (pp)	Hourly 63	Annual 28	Monthly 6
Share by Salary Type (pp)	Base Pay 55	Total Pay 40	Bonus 5
Length of Vacancy (days)	10th p'ctile 6	Median 23	90th p'ctile 44
Non-zero Wage Growth (%)	10th p'ctile -8.0	Median 1.1	90th p'ctile 9.5

Notes: in Panel A, an occupation is at the 6 digit SOC level, the definition of each part of the sample formation is described in the main text. In Panel C, the first two rows report the share of vacancies missing information for each of the variables in the corresponding columns. We report only the three most common kinds of pay frequency and salary type. Wage growth is normalized by the number of quarters between the vacancy posting.

Table 2: Average Nominal Wages in Burning Glass vs. Wage for New Hires in CPS

	Log Average Wage for New Hires, Current Population Survey		
	(1)	(2)	(3)
Panel A: Outcome is Average Wages by State			
Log Average Wage,	1.06	1.09	0.85
Burning Glass	(0.19)	(0.35)	(0.14)
Observations	2,244	2,244	2,244
Within R-squared	0.06	0.03	0.05
Panel B: Outcome is Average Wages by Industry Sector			
Log Average Wage,	1.26	1.01	1.17
Burning Glass	(0.20)	(0.23)	(0.23)
Observations	852	852	852
Within R-squared	0.08	0.03	0.09
Panel C: Outcome is Average Wages by Broad Occupation			
Log Average Wage,	1.37	0.95	1.34
Burning Glass	(0.15)	(0.18)	(0.15)
Observations	963	963	963
Within R-squared	0.08	0.10	0.07
Time Fixed Effect			✓
Group Fixed Effect		✓	

Notes: this table presents regressions of average nominal wages in survey data on average wages in Burning Glass. The dependent variable is 100 times the log of the hours-weighted mean wage for newly hired workers from the 2010-2020 Current Population Survey, by quarter and state. We include both workers hired from unemployment (as in Haefke et al, 2013) and workers hired from other jobs (as in Fallick and Fleischman, 2004). Wages are trimmed at the first and 99th percentile. The wage is usual hourly earnings for hourly and non-hourly workers including overtime, for non-farm workers; measured from the CPS Outgoing Rotation Group. In Panel A, the dependent variable is the mean by state and quarter; in Panel B, the dependent variable is the mean by quarter and 2 digit NAICS industry; in Panel C, the dependent variable is the mean by quarter and 2 digit SOC occupation. The regressor is 100 times the log of average wages from 2010-2020 from Burning Glass. The sample is the sample of Table 4, i.e. the main regression sample, with additional quarters of data 2020Q3 and 2020Q4 from Burning Glass. Salaries are trimmed at the 5th and 95th percentiles in each 2 digit occupation group, within each pay frequency and salary type. To uncover group-quarter salaries, we regress $\log(\text{salary}_{ist}) = \alpha + \sum_{p,s} \beta_{ps} D_{ps} + \sum_{s,t} \gamma_{st} W_{st} + \text{error}_{ist}$ where D_{ps} denotes a set of salary type by pay frequency dummies and W_{st} is a set of group (i.e. state, 2 digit industry or 2 digit occupation) by quarter dummies. Observations are reweighted by the 2014-2016 OES. Then W_{st} is the log mean salary in the group-quarter. We split the sample in half in each state-quarter, and instrument for salaries in one sub-sample with salaries in the other, to overcome measurement error. The regressions are weighted by the number of CPS observations in each group (i.e. state, industry or occupation) and quarter, using sampling weights. Standard errors are clustered by group and quarter.

Table 3: Quarterly Probability of Nominal Posted Wage Change at the Job Level

	Prob. Change	Duration of Unchanged Wages	Prob. Decrease	Prob. Increase	Obs.
	(1)	(2)	(3)	(4)	(5)
Unweighted	0.17	5.36	0.04	0.12	1,746,812
Occupation weight	0.19	4.79	0.04	0.13	1,746,812
Region weight	0.17	5.45	0.04	0.12	1,746,812
No low wage jobs	0.18	5.07	0.03	0.12	1,266,448

Notes: we study the main sample of Burning Glass data. We estimate the probability of job-level wage change using a similar method to Nakamura and Steinsson (2008). We assume that the hazard rate of job change/increase/decrease is constant and identical for all jobs in the same 2 digit SOC code occupation. We then estimate the hazard rate of job change/increase/decrease by maximum likelihood. We then calculate the implied duration and probability of change/increase/decrease for each occupation, and then take the median across occupations, weighted by the number of vacancies. In Row (2), we reweight to target the distribution of jobs at the 6 digit SOC level from the 2014-2016 OES. In Row (3) we reweight to target the distribution of employment across states from the 2010 QCEW. In Row (4) we drop jobs in the bottom quartile of the wage distribution. Counts refer to the number of differenced observations.

Table 4: Nominal Posted Wage Growth and Unemployment Changes at the Job Level

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
	(1)	(2)	(3)	(4)
ΔU_{st}	0.05	-0.05	-0.05	-0.65
	(0.01)	(0.03)	(0.03)	(0.10)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.40	-1.68	-1.68	
	(0.10)	(0.11)	(0.10)	
Observations	1,789,179	1,789,179	1,789,179	1,789,179
Time Fixed Effect		✓	✓	✓
State Fixed Effect			✓	

Notes: this table presents estimates regressing nominal job-level wages on unemployment. The dependent variable is quarterly percent growth in nominal posted wages, from the Burning Glass main sample. Wage growth is trimmed at the 1st and 99th percentiles. The regressors are the change in state-quarter unemployment, and the change interacted with an indicator for whether unemployment is decreasing, from the 2010-2020 LAUS, in percentage points. Standard errors are in parentheses, clustered by state. Column (1) presents estimates without controls. Column (2) adds in time fixed effects, interacted with an indicator variable for whether unemployment is decreasing. Column (3) adds in state fixed effects. Column (4) presents estimates without asymmetries, by only including unemployment changes and time fixed effects as regressors. Counts refer to the number of differenced observations.

Table 5: Regression of Posted Wage Growth on Census Division Unemployment Changes

	Wage Growth at the Job Level		
	Nominal Wages	Real Wages (Price Level Excludes Shelter)	Real Wages (Price Level Includes Shelter)
ΔU_{st}	-0.13	-0.15	-0.10
	(0.04)	(0.09)	(0.08)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.85	-1.02	-0.37
	(0.14)	(0.05)	(0.14)
Observations	1,696,741	1,696,560	1,696,552
Time Fixed Effect	✓	✓	✓

Notes: In all panels, the dependent variable is quarterly percent growth in posted wages, from the Burning Glass main sample. Wages are averaged within each job and quarter. Wage growth is trimmed at the 1st and 99th percentiles. In column (1), the outcome is nominal wages. In column (2) the outcome is real wages, deflated by the census division level consumer price index (see Appendix Section C.4 for details on how we construct price indices). In column (3) the outcome is real wages, deflated by the census division level consumer price index excluding shelter. The regressor are the change in census division-quarter unemployment, in percentage points, and an interaction of the change with an indicator for whether unemployment is falling, from the 2010-2020 LAUS, in percentage points. Standard errors are in parentheses, clustered by census division. We control for time fixed effects, interacted with an indicator variable for whether unemployment is decreasing. Counts refer to the number of differenced observations.

Table 6: Nominal Posted Wages and Unemployment—Identifying Labor Demand Variation

	Baseline	Labor Supply Controls	Shift Share Instrument	Work from Home Only	Demand Shock Industries	Oil Instrument (pre-2020)	Baseline (pre-2020)
			Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$				
ΔU_{st}	-0.05	-0.08	-0.45	-0.05	-0.02	1.47	-0.33
	(0.03)	(0.03)	(0.24)	(0.03)	(0.04)	(1.76)	(0.13)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.68	-1.64	-2.47	-1.35	-1.72	-3.13	-1.38
	(0.11)	(0.10)	(0.32)	(0.13)	(0.13)	(1.84)	(0.13)
Observations	1,789,179	1,782,126	1,789,179	442,952	323,258	1,535,117	1,535,117
Robust F Stat.			65.4				
Time Fixed Effect	✓	✓	✓	✓	✓	✓	✓
Estimation Method	OLS	OLS	2SLS	OLS	OLS	LIML	OLS

Notes: this table presents estimates regressing nominal job-level wages on unemployment, while identifying labor demand variation. Column (1) presents the baseline estimate from column (2) of Table 4. Column (2) repeats Column (1) but controls for a dummy variable for 2020Q1 onwards, interacted with: the length of time for which states shut down businesses (from Chetty et al, 2020); quarterly Covid case and death counts (from Chetty et al, 2020); the state's median UI replacement after the passage of the CARES act (from Ganong, Noel, and Vavra, 2020); and the state's 2010-2019 share of female employment (from the Current Population Survey). Column (3) instruments unemployment changes with a shift share instrument, and the instrument interacted with an indicator for whether for whether it is increasing; and also controls for time fixed effects interacted with whether the shift share instrument is increasing; for 2010-2020. The shift share instrument is measured at the 6 digit NAICS level from the QCEW, we calculate state industry shares using annual employment from 2009. Column (4) re-estimates the regression of column (1), but restricts to work-from-home occupations as classified by Dingel and Neiman (2020). Column (5) re-estimates the regression of column (1), but restricts to occupations affected by labor demand shocks as classified by Baqaee and Farhi (2022). Column (6) instruments unemployment changes with an instrument based on the growth in the Brent Crude oil price, averaged by quarter, and controls for time fixed effects interacted with whether the oil price is increasing. The instrument is a set of variables $A_s \Delta \log$ oil price$_t + B_s \Delta \log$ oil price$_t \times I$ ($\Delta \log$ oil price$_t < 0$), where A_s and B_s are state fixed effects estimated in the first stage. In this column, the sample is 2010-2019. Column (7) is the same as column (1) but restricts the sample to 2010-2019. Each regression is estimated by OLS; with the exception of the shift share instrument (estimated by 2SLS) and the oil instrument (estimated by LIML). Counts refer to the number of differenced observations.

Table 7: Robustness—Nominal Posted Wages on Unemployment at the Job Level

Specification	Coefficient $\Delta U_{st} \times I(\Delta U_{st} < 0)$	Standard Error	Obs.
1. Baseline	-1.68	0.11	1,789,176
2. Annual	-1.58	0.13	809,187
3. Seasonally Adjust (X-11)	-2.05	0.12	1,789,175
4. Control for Vacancy Length	-2.73	0.19	447,444
5. Drop Consecutive Quarters	-1.58	0.12	772,654
6. Drop Bonuses	-1.68	0.11	1,701,819
7. Occupation Weights	-1.70	0.13	1,717,861
8. Region Weights	-1.63	0.11	1,789,176
9. Time Varying Weights	-1.26	0.09	1,789,176
10. Pool across Pay Types	-1.68	0.11	1,788,721
11. Including Ranges	-1.71	0.11	4,321,421

Notes: the first row reports the coefficient on $\Delta U_{st} \times I(\Delta U_{st} < 0)$ from the baseline regression, that is, column (2) of Table 4. The second row re-estimates the baseline regression at annual frequency. The third row seasonally adjusts unemployment and employment, at the state-quarter level for 1980-2021 data. The fourth row controls for the length of time for which vacancies are posted, taking the average by job and quarter. The fifth row excludes vacancies posted in the quarter immediately after another vacancy of the same job. The sixth row excludes vacancies with bonus pay. The seventh row reweights to target mean employment in each six digit occupation over 2010-2020. The eighth row reweights to target mean employment in each state over 2010-2021; the ninth row reweights to target employment in the previous quarter. The tenth row takes the mean wage across job titles, averaging over pay frequencies. The eleventh row includes vacancies posting a wage range, taking the mean of the range.

Table 8: Correcting Selection Bias—Nominal Posted Wages and Unemployment

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$		
	Without Selection Correction	With Selection Correction	Difference Due to Selection Bias
ΔU_{st}	-0.054	-0.005	0.049
	(0.014)	(0.023)	(0.024)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.783	-1.478	0.305
	(0.098)	(0.076)	(0.041)
Time Fixed Effect	✓	✓	✓

Notes: column (1) estimates the baseline specification from column (2) of Table 4, restricting to the subset of the main sample that merges to Dun & Bradstreet. Column (2) re-estimates the same specification but applies the selection correction outlined in the main text. Column (3) tests the null hypothesis that there is no selection bias. Each entry in Column (3) reports the difference between the corresponding coefficients in Panel A and B, and the standard error of this difference in parentheses. Counts refer to the number of differenced observations.

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Appendix—For Online Publication

A   Additional Figures

Figure 1: Employment-Population Ratio in United States during 2019-2021
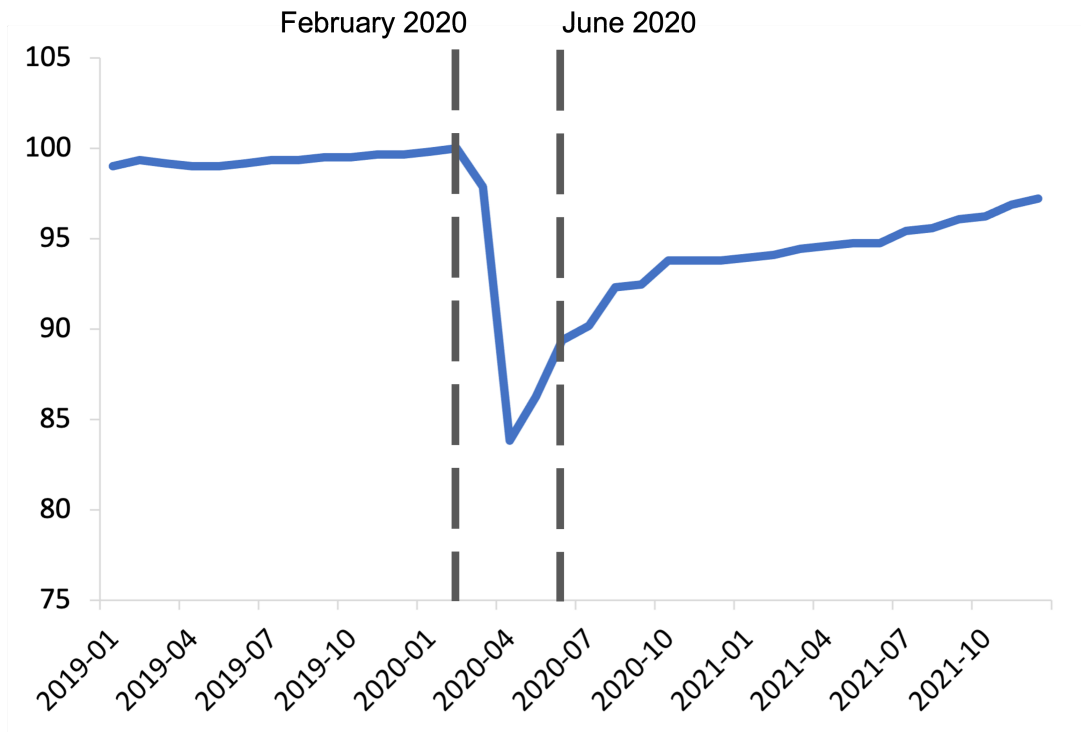
Notes: this graph plots the employent-population ratio, by month, from January 2019 to December 2021. The employment-population ratio is provided by the Bureau of Labor Statistics. The series is rescaled to have a value of 100 in February 2020.

Figure 2: Nominal Wages and Unemployment—pre vs. post June 2020

Notes: the graph plots binned wage growth of nominal posted wages, in percent, from Burning Glass; and binned state by quarter unemployment changes, in percentage points, from the Local Area Unemployment Statistics. In blue circles is data for 2010Q1-2020Q2, in red trianges is data for 2020Q3-2021Q1. To construct wage growth, we take the mean wage within each job and quarter, and then take log differences at the job level. We use 50 bins and add a non-parametric regression line, separately for each series.

Figure 3: Industry Employment Growth During the Pandemic Recession
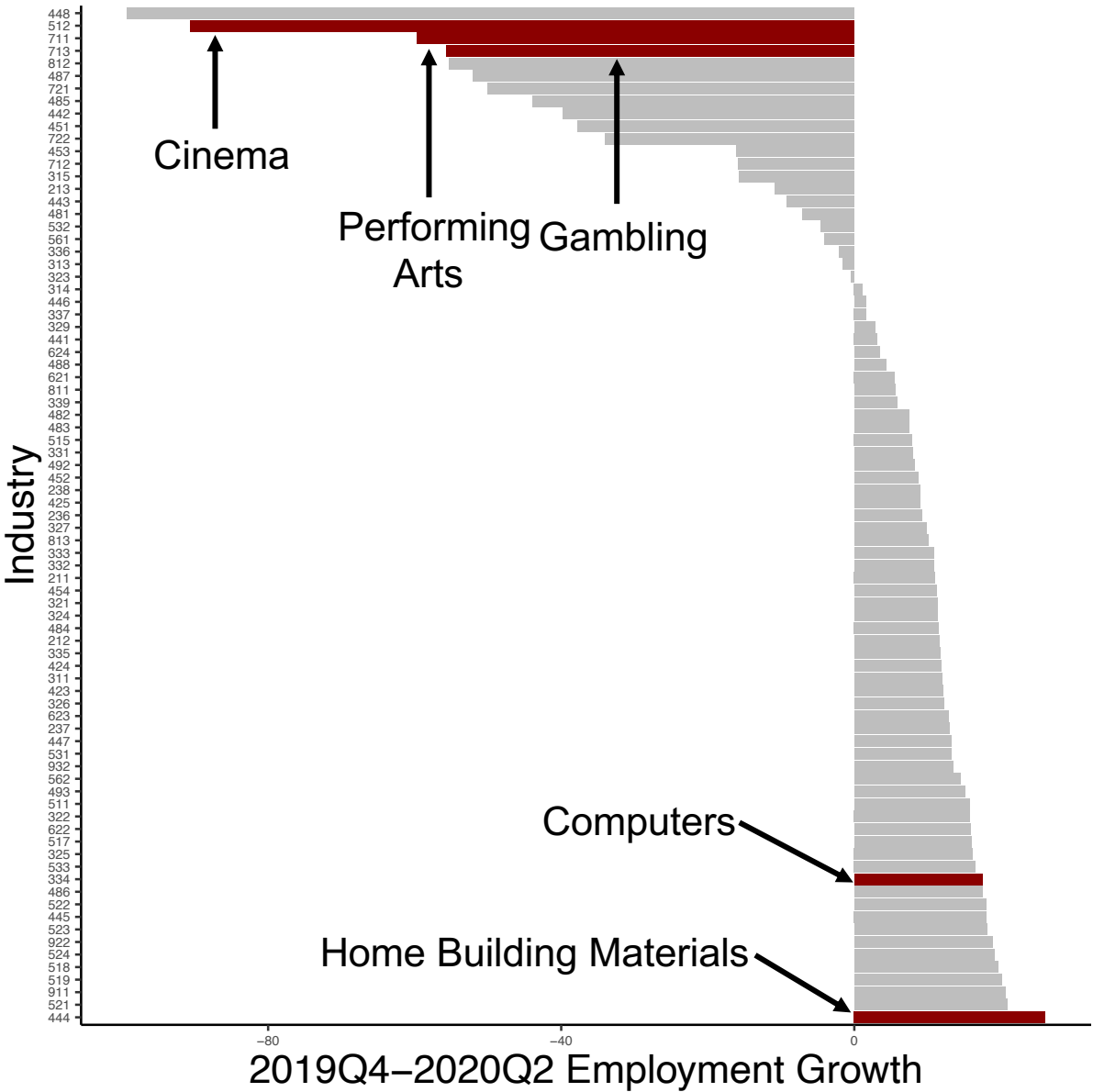
Notes: this graph plots industry employment growth, in percentage points, at the 3 digit NAICS level, from the Current Employment Statistics, between the last quarter of 2019 and the second quarter of 2020. Each observation is demeaned by national average employment growth over the period.

Figure 4: Brent Crude Oil Price ($/Barrel)
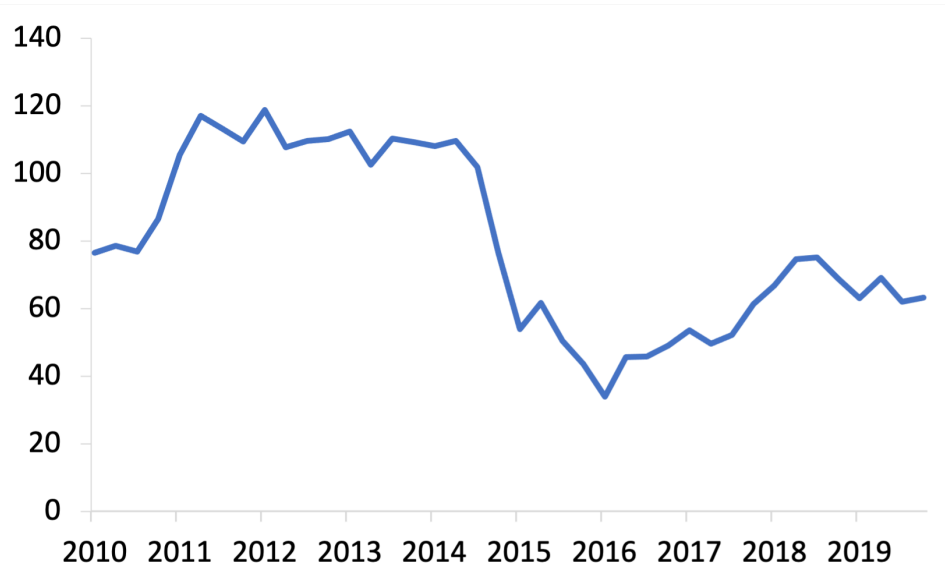
Notes: this graph plots the quarterly average of the Brent Crude oil price, for 2010-2020. The units are dollars per barrel of oil.

Figure 5: Nominal Wages and Industry Employment
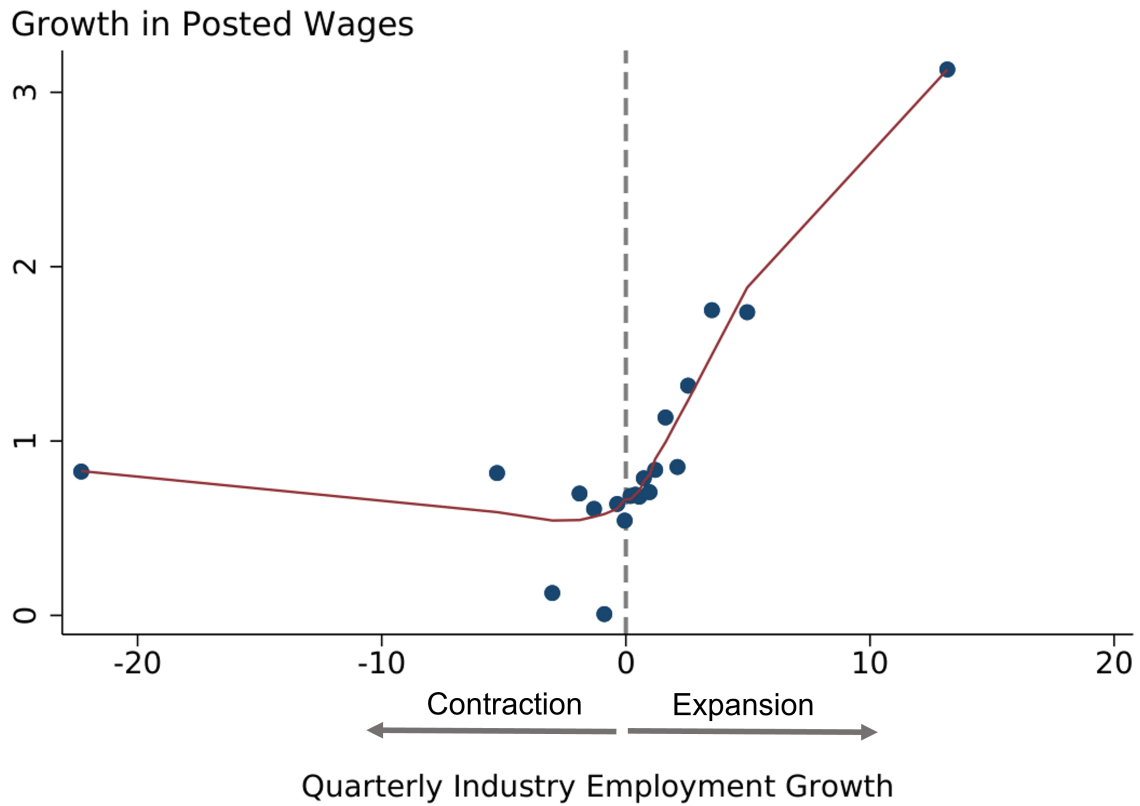
Notes: the graph plots binned wage growth of nominal posted wages, in percent, from Burning Glass; and binned 3 digit NAICS industry by quarter employment growth, in percentage points, from the Current Employment Statistics. The sample period is 2010Q1-2020Q2. To construct wage growth, we take the mean wage within each job and quarter, and then take log differences at the job level. We use 20 bins and add a non-parametric regression line.

Figure 6: State Level Coverage in Burning Glass
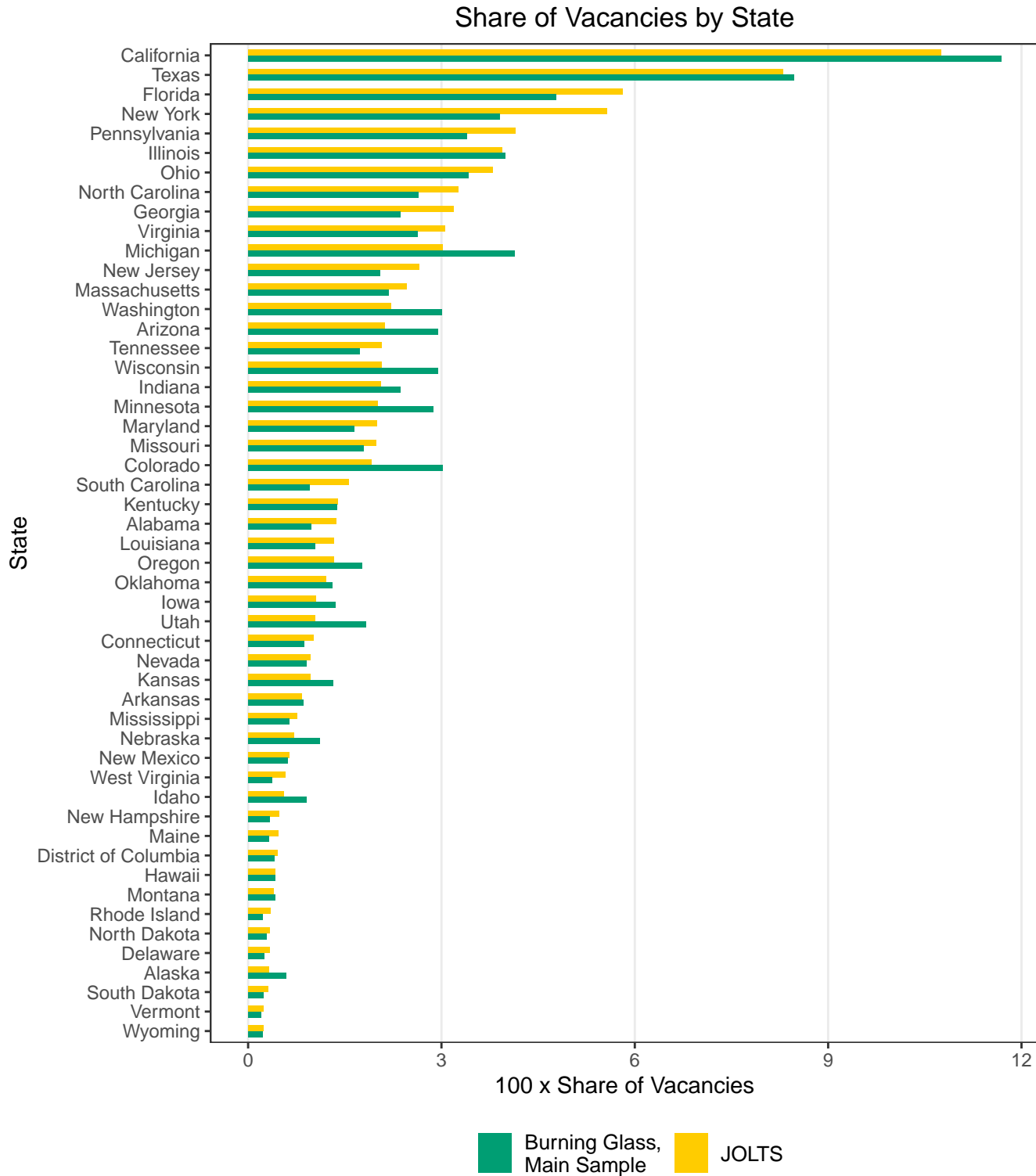
Notes: this graph compares the state level share of vacancies in Burning Glass and JOLTS. In green, is the share of vacancies in each state, in the Burning Glass main sample, for 2010-2020Q2. In yellow is the share of vacancies in each state, for 2010-2020, according to JOLTS.

Figure 7: Broad Occupation Coverage in Burning Glass

Share of Vacancies or Employment by Occupation
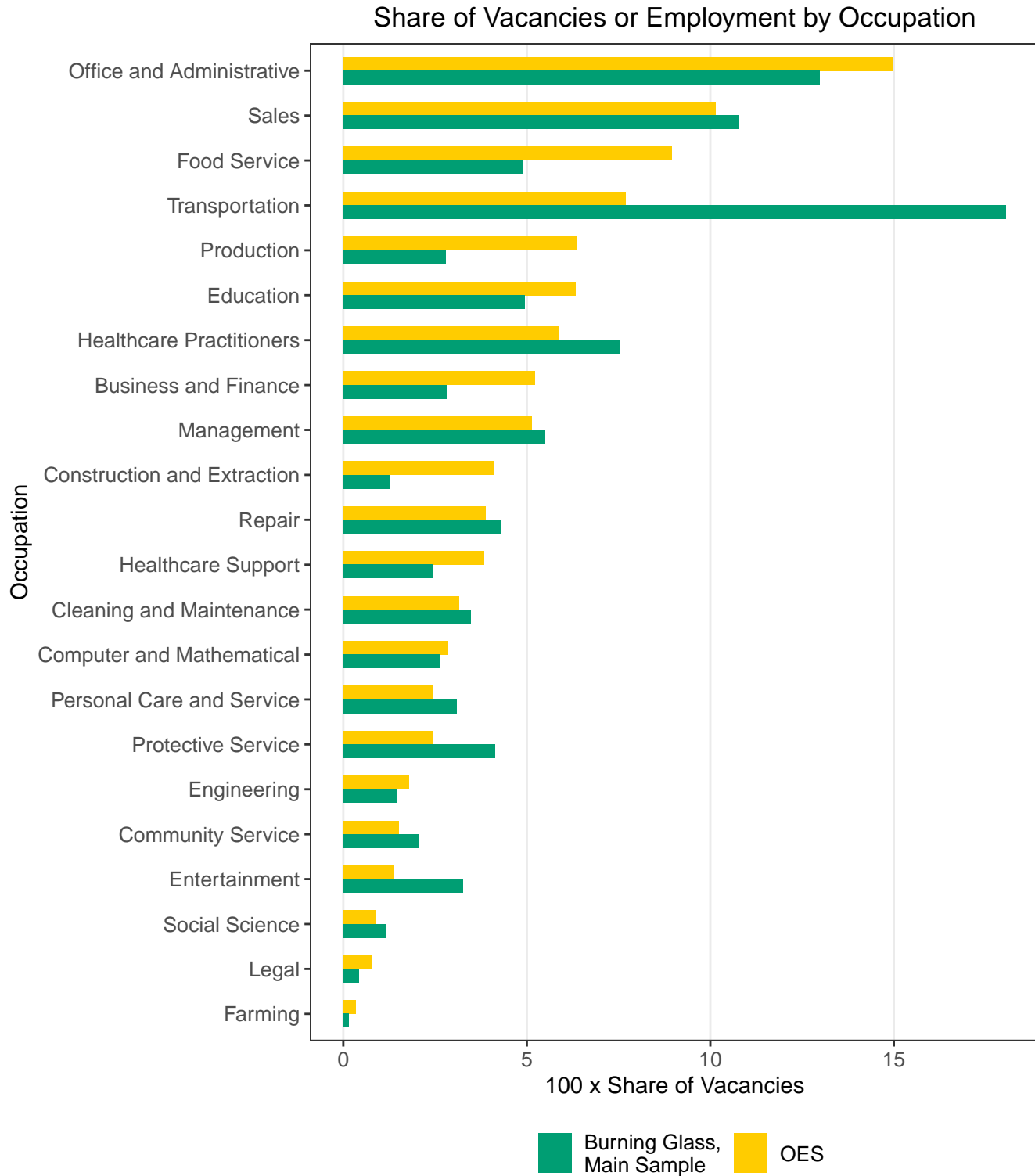
Notes: this graph compares the occupation level share of vacancies in Burning Glass versus employment in the Occupational Employment Statistics, at the 2 digit occupation level. In green, is the share of vacancies in each occupation, in the Burning Glass main sample, for 2010-2020Q2. In yellow is the share of employment in each occupation, for 2010-2020, according to the Occupational Employment Statistics.

Figure 8: State Level Coverage in Dun & Bradstreet and Burning Glass Merged Sample
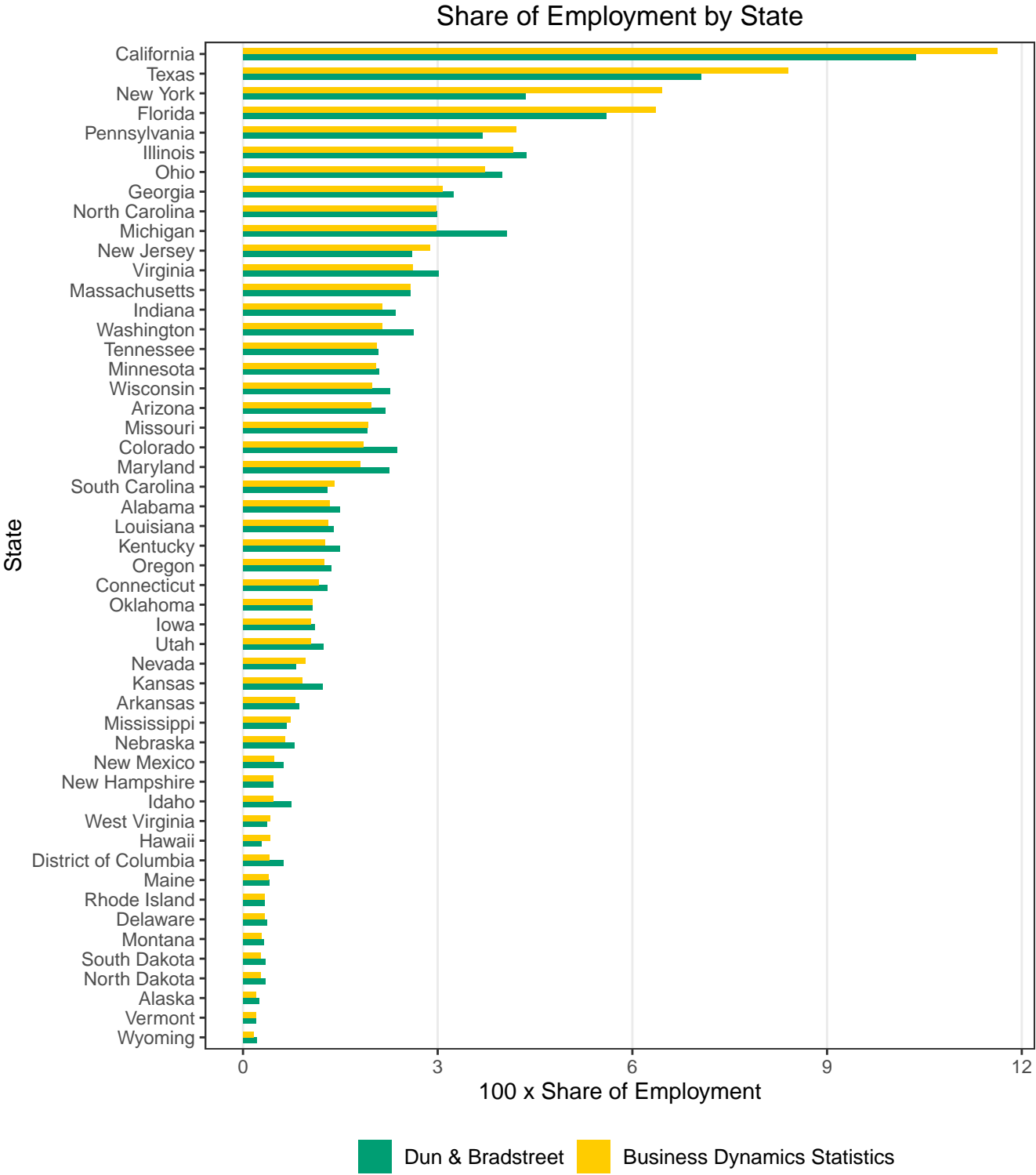
Notes: this graph compares the state level share of employment in Dun & Bradstreet and the Business Dynamics Statistics. In green, is the share of employment in each state, in the Dun & Bradstreet establishments that merge to the Burning Glass main sample, averaged over 2010-2020. In yellow is the share of employment in each state, according to the 2019 Business Dynamics Statistics.

Figure 9: Industry Coverage in Dun & Bradstreet and Burning Glass Merged Sample

Share of Employment by Sector
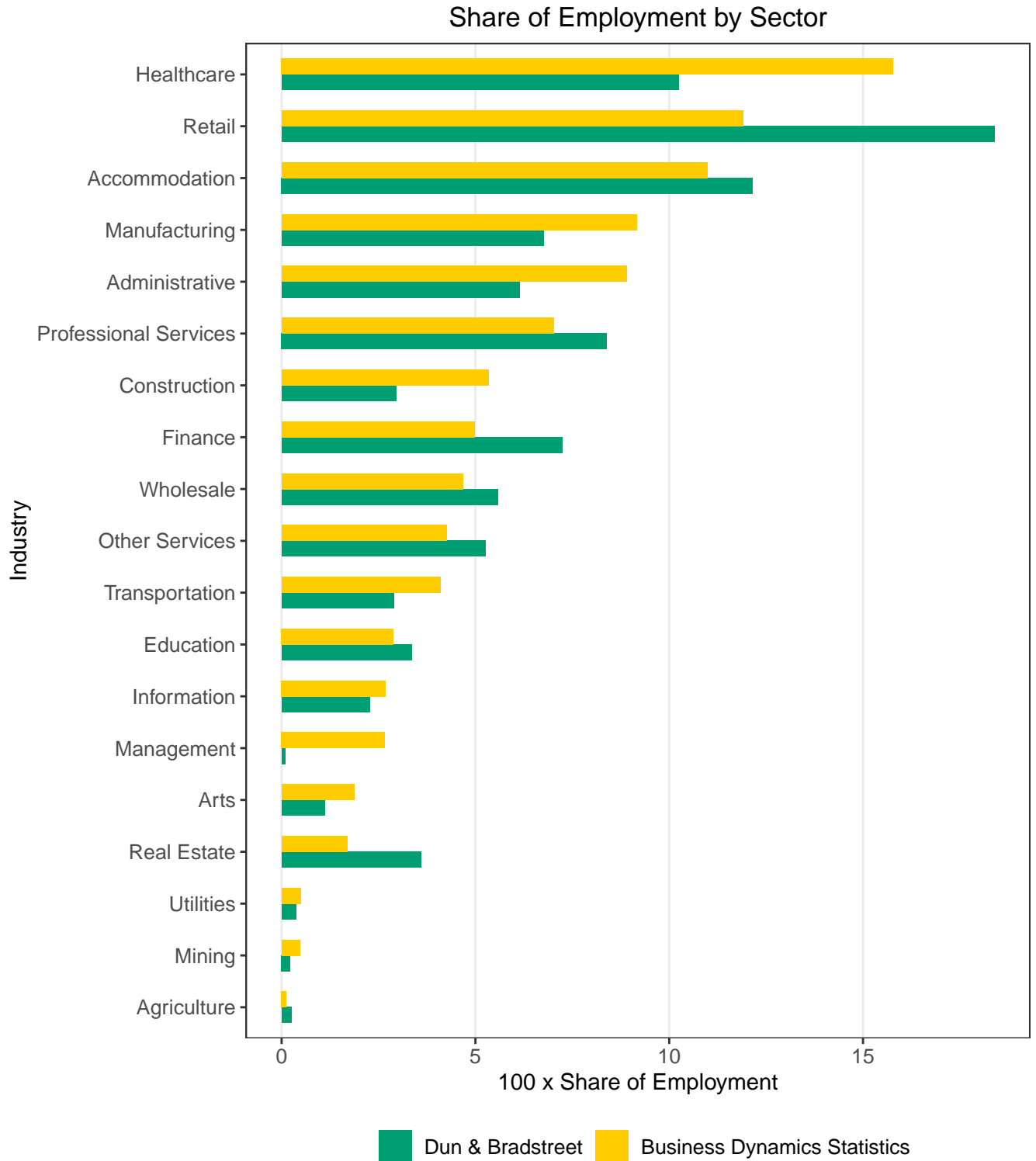
Notes: this graph compares the industry level share of employment in Dun & Bradstreet and the Business Dynamics Statistics, measured at the 2 digit industry level. In green, is the share of employment in each industry, for the Dun & Bradstreet establishments that merge to the Burning Glass main sample, averaged over 2010-2020. In yellow is the share of employment in each industry, according to the 2019 Business Dynamics Statistics.

B Additional Tables

Table 1: Difference of State Wages in Burning Glass and CPS vs. Unemployment

	Difference between Quarterly State Wage Growth in Burning Glass and CPS			
	(1)	(2)	(3)	(4)
ΔU_{st}	0.12	0.13	0.06	0.06
	(0.38)	(0.38)	(0.38)	(0.38)
Observations	2,193	2,193	2,193	2,193
Time Fixed Effect	✓	✓	✓	✓
State Fixed Effect		✓		✓
Weights	QCEW	QCEW	CPS	CPS

Notes: this table regresses the difference in Burning Glass and Current Population Survey (CPS) wages on unemployment. The dependent variable is 100 times the growth of quarterly state wages in Burning Glass, minus 100 times the growth of quarterly state wages in the CPS. We measure quarterly state wages in Burning Glass and the CPS in the same way as Table 2, Panel A. The regressor is the change in state-quarter unemployment, from the 2010-2020 Local Area Unemployment Statistics, in percentage points. Standard errors are in parentheses, clustered by state. Columns (1) and (3) include time fixed effects, columns (2) and (4) include both time and state fixed effects. Column (1) and (2) are weighted by mean state employment during 2010-2020 from the Quarterly Census of Employment and Wages. Columns (3) and (4) are weighted by the quarterly size of the state, measured from the CPS.

Table 2: Variance Decomposition of Log Wages with Job Fixed Effects

Specification	Share of Variance Explained by Job FEs
Job FEs only	0.99
Job, Time FEs	0.98
Job, State-Time FEs	0.98
Job, State-Time FEs, at least 5 obs per job	0.94
Job FEs only, trimmed wages	0.98
Job title FEs	0.82

Notes: this table regresses log wages, for each job and quarter, on job fixed effects—recall that a job is a job title by establishment. Column (1) reports the specification, and column (2) reports the share of the total variance in wages that is explained by job fixed effects. Row 1 is the baseline specification. We add in time fixed effects in row 2, state-time fixed effects in row 3, and restrict to at least five observations per job in row 4 (recall that the main regression sample has at least two observations per job). Row 5 repeats row 1 but trims the most extreme 5% of wages within each year, 6 digit occupation, pay frequency and salary type. Row 6 repeats Row 1, but uses a different definition of a job. Specifically, we regress log wages only on job title fixed effects, instead of job title by establishment fixed effects. The regression is for the main Burning Glass sample, over 2010-2020Q2.

Table 3: Probability of Posted Wage Change By Gap Between Posting

Gap Between Postings (Quarters)	Quarterly Prob. Change	Quarterly Prob. Increase	Quarterly Prob. Decrease	Share of Observations (pp)
1	0.22	0.14	0.08	57
2	0.18	0.12	0.05	18
3	0.17	0.13	0.03	8
4	0.15	0.12	0.02	6
5	0.16	0.12	0.02	3
6	0.15	0.11	0.02	2
7	0.14	0.10	0.02	1
8	0.14	0.10	0.01	1

Notes: we study the main sample of Burning Glass data. We estimate the probability of job-level wage change (column 2), increase (column 3) and decrease (column 4) in the same way as table 3. Column 1 lists the number of quarters between the vacancy postings. In row (1), we estimate the probability only for vacancies that post in consecutive quarters (i.e. with a one quarter gap). In Row (2) we estimate the probability for vacancies that post with a two quarter gap, and so on. The final column reports the share of vacancies that post, given a number of quarters between postings.

Table 4: Quarterly Probability of Posted Wage Change—Heterogeneity by Length of Vacancy

	Prob. Change	Duration of Unchanged Wages	Prob. Decrease	Prob. Increase	Observations
	(1)	(2)	(3)	(4)	(5)
Above Median Vacancy Length	0.23	3.74	0.04	0.14	225,188
Below Median Vacancy Length	0.23	3.89	0.04	0.14	222,683

Notes: we study the main sample of Burning Glass data. We estimate the probability of job-level wage change (column 1), duration of unchanged wages (column 2) and probability of increase and decrease (columns 3 and 4) in the same way as table 3. In row (1), we estimate the probabilities for vacancies that post for an above median length of time. In row (2) we estimate the probabilities for vacancies that post for a below median length of time. The sample is vacancies from the main sample with information on the length of posting time.

Table 5: First Stage Regressions for Shift Share Instrument

	Δunemployment$_{st}$			Δunemployment$_{st} \times I\,(\Delta$unemployment$_{st} < 0)$		
	(1)	(2)	(3)	(4)	(5)	(6)
Δlog (shift share$_{st}$)	-0.75	-0.60	-0.59	-0.00	-0.01	-0.01
	(0.03)	(0.05)	(0.05)	(0.00)	(0.01)	(0.01)
Δlog (shift share$_{st}$) $\times$	0.54	0.27	0.26	-0.30	-0.31	-0.31
$I\,(\Delta$log (shift share$_{st}$) $> 0)$	(0.03)	(0.05)	(0.05)	(0.03)	(0.02)	(0.02)
Observations	1,789,179	1,789,179	1,789,179	1,789,179	1,789,179	1,789,179
Time Fixed Effect		✓	✓		✓	✓
State Fixed Effect			✓			✓

Notes: this table presents first stage regressions of the shift share instrument. In Columns (1)-(3) the outcome variable is the change in quarter by state unemployment, from the 2010–2020Q2 Local Area Unemployment Statistics. In Columns (4)-(6) the outcome variable is the change in unemployment interacted with an indicator for whether unemployment is declining. In all columns, the regressors are the quarterly change in the shift share instrument, and the change in the shift share interacted with an indicator for whether the shift share is increasing. In columns (1) and (4) there are no controls. Columns (2) and (5) have time fixed effects. Columns (3) and (6) add state fixed effects. The shift share instrument is measured at the 6 digit NAICS level from the QCEW, we calculate state industry shares using annual employment from 2009. Standard errors are in parentheses, clustered by state.

Table 6: Nominal Posted Wages and Unemployment—Heterogeneity by Posting Time

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
Vacancy posting time:	Quartile 1	Quartile 2	Quartile 3	Quartile 4
ΔU_{st}	-0.11	-0.09	-0.02	0.01
	(0.07)	(0.06)	(0.03)	(0.04)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-2.55	-2.61	-2.98	-2.78
	(0.25)	(0.24)	(0.24)	(0.18)
Observations	112,648	114,703	109,083	111,009
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity by the length of vacancy posting. We estimate the baseline regression, that is, column (2) of Table 4. We estimate wage rigidity separately for vacancies according to the length of time that vacancies were posted, split into quartiles. The sample is vacancies from the main sample with information on the length of time for which vacancies were posted.

Table 7: Heterogeneity in Wage Cyclicality by Occupation

| | Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$ | | | | |
	Management	Services	Sales	Construction	Production
ΔU_{st}	-0.10	0.02	-0.04	-0.07	-0.06
	(0.03)	(0.03)	(0.04)	(0.06)	(0.05)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.30	-2.03	-1.64	-1.80	-2.18
	(0.14)	(0.18)	(0.11)	(0.16)	(0.15)
Observations	549,943	312,401	399,432	99,121	356,963
Time Fixed Effect	✓	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity by occupation. We estimate the baseline regression, that is, column (2) of Table 4. We estimate wage rigidity separately for five broad occupations according to the SOC classification. Standard errors are in parentheses, clustered by state.

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Table 8: Heterogeneity in Wage Cyclicality by Industry

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$				
Panel A:	Natural Resources	Construction	Manufacturing	Trade & Utilities	Information
ΔU_{st}	0.37	0.01	-0.08	-0.09	-0.17
	(0.06)	(0.10)	(0.05)	(0.09)	(0.16)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.87	-1.24	-1.67	-2.18	-1.46
	(0.22)	(0.25)	(0.10)	(0.15)	(0.20)
Observations	11,541	20,042	82,892	365,602	48,970
Panel B:	Finance	Business Services	Education & Health	Leisure	Other Services
ΔU_{st}	-0.03	-0.01	-0.08	0.04	-0.08
	(0.03)	(0.03)	(0.03)	(0.06)	(0.05)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.92	-1.72	-1.48	-2.16	-1.62
	(0.12)	(0.19)	(0.13)	(0.16)	(0.29)
Observations	123,946	140,015	456,701	112,679	27,809
Time Fixed Effect	✓	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity by industry. We estimate the baseline regression, that is, column (2) of Table 4. We estimate wage rigidity separately for ten broad industries, at the 2 digit NAICS level. Standard errors are in parentheses, clustered by state.

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Table 9: Nominal Posted Wages and Unemployment at the Establishment Level

| | Nominal Wage Growth at Establishment Level, $\Delta \log w_{jst}$ | | | |
	(1)	(2)	(3)	(4)
ΔU_{st}	-0.09	-0.14	-0.13	-0.81
	(0.01)	(0.06)	(0.06)	(0.11)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.45	-1.76	-1.79	
	(0.16)	(0.19)	(0.21)	
Observations	2,120,212	2,120,212	2,120,212	2,120,212
Time Fixed Effect		✓	✓	✓
State Fixed Effect			✓	

Notes: this table presents estimates regressing nominal establishment-level wages on unemployment. The dependent variable is quarterly percent growth in nominal posted wages, from the Burning Glass main sample. Wage growth is trimmed at the 1st and 99th percentiles. The regressors are the change in state-quarter unemployment, and the change interacted with an indicator for whether unemployment is decreasing, from the 2010-2020 LAUS, in percentage points. Standard errors are in parentheses, clustered by state. Column (1) presents estimates without controls. Column (2) adds in time fixed effects, interacted with an indicator variable for whether unemployment is decreasing. Column (3) adds in state fixed effects. Column (4) presents estimates without asymmetries, by only including unemployment changes and time fixed effects as regressors.

Table 10: Nominal Posted Wages and Unemployment—Heterogeneity by Posting Frequency

| | Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$ | | | |
Number of vacancy postings:	Quartile 1	Quartile 2	Quartile 3	Quartile 4
ΔU_{st}	-0.05	-0.05	-0.02	-0.07
	(0.04)	(0.02)	(0.02)	(0.03)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.71	-1.75	-1.72	-1.37
	(0.12)	(0.14)	(0.10)	(0.11)
Observations	728,431	396,766	365,418	298,563
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity by the frequency of vacancy posting. We estimate the baseline regression, that is, column (2) of Table 4. We estimate wage rigidity separately for vacancies according to the number of times that vacancies were posted, split into four groups. Note that although we use the word quartile in the table, we use the jobs that posted two times in first column and jobs that posted three times in the second column. In the last two columns, we split the rest of the sample evenly into two groups.

Table 11: Heterogeneity in Wage Cyclicality by Source of Vacancy

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
	Company Website	Job Board	Government	Education
ΔU_{st}	-0.17	-0.18	1.17	-0.27
	(0.20)	(0.13)	(0.94)	(0.49)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.62	-1.34	-2.20	-1.19
	(0.24)	(0.11)	(0.77)	(0.56)
Observations	120,291	325,859	52,004	51,056
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity by source of vacancy. We estimate the baseline regression, that is, column (2) of Table 4. We estimate wage rigidity separately for vacancies from different sources, as listed in the table. The sample is vacancies from the main sample with information on the source of the vacancy. Standard errors are in parentheses, clustered by state.

Table 12: Regression of Nominal Posted Wage Growth on Industry Employment Growth

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jit}$			
	(1)	(2)	(3)	(4)
$\Delta \log (\text{employment}_{it})$	0.00	-0.01	-0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta \log (\text{employment}_{it}) \times$	0.05	0.06	0.05	
$I(\Delta \log (\text{employment}_{it}) > 0)$	(0.02)	(0.01)	(0.01)	
Observations	934,571	934,571	934,571	934,571
Time Fixed Effect		✓	✓	✓
Industry Fixed Effect			✓	

Notes: this table presents estimates regressing nominal job-level wages on industry employment, at the 3 digit NAICS level. The dependent variable is quarterly percent growth in nominal posted wages, from the Burning Glass main sample. Wage growth is trimmed at the 1st and 99th percentiles. The regressors are the growth in industry by quarter employment, and the change interacted with an indicator for whether employment is increasing, from the 2010-2020 Current Employment Statistics, in percentage points. Standard errors are in parentheses, clustered by industry. Column (1) presents estimates without controls. Column (2) adds in time fixed effects, interacted with an indicator variable for whether employment is increasing. Column (3) adds in industry fixed effects. Column (4) presents estimates without asymmetries, by only including employment changes and time fixed effects as regressors.

Table 13: Heterogeneity in Wage Cyclicality by Establishment Size and Occupation Wage

Position in size or wage distribution:	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	Panel A: Heterogeneity by Establishment Size			
ΔU_{st}	0.01	-0.03	-0.16	-0.08
	(0.02)	(0.02)	(0.09)	(0.04)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.72	-1.70	-1.69	-1.54
	(0.09)	(0.09)	(0.14)	(0.17)
Observations	493,595	409,782	439,290	446,508
	Panel B: Heterogeneity by Occupation Wage			
ΔU_{st}	-0.02	-0.09	-0.03	-0.06
	(0.05)	(0.04)	(0.02)	(0.03)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-2.04	-1.71	-1.75	-1.27
	(0.17)	(0.11)	(0.13)	(0.13)
Observations	429,969	393,034	406,400	402,589
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity. We estimate the baseline regression, that is, column (2) of Table 4. In Panel A, we estimate wage rigidity separately for vacancies in the first through fourth quartiles of the establishment size distribution. Establishment size is the total number of vacancies posted by an establishment, in the main sample, during 2010-2020. In Panel B, we estimate wage rigidity separately for vacancies in the first through fourth quartiles of the wage distribution. Wages are the median wage, within the 6 digit occupation, measured from the 2014-16 Occupational Employment Statistics. Standard errors are in parentheses, clustered by state.

Table 14: Controlling for lagged gap between postings

Panel A: Controlling for lag of gap			
	(1)	(2)	(3)
ΔU_{st}	-0.040	-0.039	-0.037
	(0.017)	(0.016)	(0.015)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.771	-1.762	-1.757
	(0.107)	(0.107)	(0.107)
Lagged Gap		0.050	
		(0.005)	
Observations	708,015	708,015	707,999
Time Fixed Effect	✓	✓	✓
Interacted lagged gap FE			✓

Panel B: Correlation between gap and its lag	
	Gap between postings
Previous gap between postings	0.093
	(0.004)
Observations	714,648
Time Fixed Effect	✓

Panel C: Job specificity of gap between postings	
	Gap between postings
Observations	513,239
R-squared	0.41
Job Fixed Effect	✓

Notes: Panel A presents estimates regressing nominal job-level wage growth on unemployment changes. The dependent variable is the quarterly percent growth in nominal posted wages, from the Burning Glass main sample. Wage growth is trimmed at the 1st and 99th percentiles. The regressor is the change in state-quarter unemployment, from the 2010-2020 LAUS, in percentage points. Standard errors are in parentheses, clustered by state. Panel B shows the result for estimating a regression with the current gap between postings as the dependent variable and the lagged value of the gap between postings as the explanatory variable. Panel C displays the R-squared of the regression of the gap between postings on job fixed effects. In all panels, we limit our sample such that it only includes observations for which the lagged value of the gap between postings is not missing. Counts refer to the number of differenced observations.

Table 15: Change in Share of Vacancies with Wages at Establishment by Occupation Level

	Change in Share of Vacancies with Posted Wages			
	(1)	(2)	(3)	(4)
ΔU_{st}	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	0.00	0.00	0.00	
	(0.00)	(0.00)	(0.00)	
Observations	2,975,670	2,975,670	2,975,670	2,975,670
Time Fixed Effect		✓	✓	✓
State Fixed Effect			✓	

Notes: this table presents estimates regressing the change in the share of vacancies with wages on the change in unemployment. The dependent variable is quarterly change in the share of vacancies that post wages, within each establishment and 6 digit occupation. The regressors are the change in state-quarter unemployment, and the change interacted with an indicator for whether unemployment is decreasing, from the 2010-2020 LAUS, in percentage points. Standard errors are in parentheses, clustered by state. Column (1) presents estimates without controls. Column (2) adds in time fixed effects, interacted with an indicator variable for whether unemployment is decreasing. Column (3) adds in state fixed effects. Column (4) presents estimates without asymmetries, by only including unemployment changes and time fixed effects as regressors. The sample is all establishment by occupation observations that post at least one vacancy with a wage in the lagged period.

Table 16: Heterogeneity in Wage Cyclicality by Degree of Wage Bargaining or Posting

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
Degree of bargaining or posting in occupation:	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	Panel A: Heterogeneity by Degree of Wage Bargaining			
ΔU_{st}	-0.09	-0.03	-0.05	-0.02
	(0.03)	(0.04)	(0.05)	(0.04)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.33	-1.69	-2.16	-1.91
	(0.14)	(0.11)	(0.15)	(0.13)
Observations	530,381	517,577	362,682	307,220
	Panel B: Heterogeneity by Degree of Wage Posting			
ΔU_{st}	-0.04	-0.05	-0.02	-0.08
	(0.04)	(0.05)	(0.03)	(0.03)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.96	-1.63	-1.73	-1.47
	(0.15)	(0.11)	(0.12)	(0.14)
Observations	458,973	424,510	413,339	421,036
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity. We estimate the baseline regression, that is, column (2) of Table 4. In Panel A, we estimate wage rigidity separately for vacancies in the first through fourth quartiles of the share of workers within an occupation that engage in wage bargaining. In Panel B, we estimate wage rigidity separately for vacancies in the first through fourth quartiles of the share of workers within an occupation that receive a job with a posted wage. The share of workers within an occupation that either receive a bargained or posted wage is provided by Hall and Krueger (2012). Standard errors are in parentheses, clustered by state.

Table 17: Regression of State Share of High Wage Jobs on Unemployment

	Change in State Share of High Wage Jobs			
	(1)	(2)	(3)	(4)
ΔU_{st}	0.11	0.09	-0.05	-0.08
	(0.18)	(0.18)	(0.17)	(0.17)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$			0.77	0.88
			(0.86)	(0.96)
Observations	2,040	2,040	2,035	2,035
Time Fixed Effect	✓	✓	✓	✓
State Fixed Effect		✓		✓

Notes: In all columns, the dependent variable is the growth in the share of high wage vacancies in the state, in percentage points. High wage vacancies have wages above the national median, within the pay frequency and salary type. In columns (1) and (2), the independent variable is state unemployment changes, in percentage points, from the 2010-2020 LAUS. Column (2) also controls for state trends. Columns (3) and (4) repeat columns (1) and (2), but include an additional regressor, the interaction of unemployment changes with an indicator for whether unemployment is falling. We also control for the interaction of this indicator with a time fixed effect. We weight by mean state employment over 2010-2020 from the QCEW. Standard errors are in parentheses, clustered by state.

Table 18: Regression of Average Nominal Wage Growth on Unemployment Changes

	Change in State Average of		Change in National Average of Wage for New Hires in	
	Posted Wages in Burning Glass	Wage for New Hires in Current Population Survey	Current Population Survey	National Longitudinal Survey of Youth
	(1)	(2)	(3)	(4)
ΔU_{st}	-0.29	-0.10	3.77	-1.78
	(0.16)	(0.59)	(3.47)	(3.17)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	0.40	0.38	-5.11	2.94
	(0.78)	(1.56)	(5.15)	(4.31)
Observations	2,085	1,627	83	89
Time Fixed Effect	✓	✓	✓	✓
Occupation FE		✓		✓
Industry FE		✓	✓	✓
Demographics		✓	✓	✓

Notes: In column (1), the dependent variable is the percent growth in average state wages, from Burning Glass. The independent variable is state unemployment changes, in percentage points, from the 2010-2020 LAUS, with time fixed effects; as well as the interaction of unemployment changes with an indicator for whether unemployment is falling, and the interaction of this indicator with a time fixed effect. Column (2) repeats Column (1) but replaces the dependent variable with the percentage growth in average state wages, from the Current Population Survey, first we residualize wages against 3 digit occupation and industry fixed effects, and demographic controls (gender, race, marital status, education and a fourth order polynomial in experience) and then take the mean. Columns (1) and (2) are weighted mean state employment over 2010-2020 from the QCEW, and standard errors are clustered by state. In column (3), the dependent variable is the percent growth in average wages for new hires, from the Current Population Survey, for 1984-2007, as constructed by Haefke et al (2013). The independent variable is national unemployment changes, in percentage points; as well as the interaction of unemployment changes with an indicator for whether unemployment is falling. Column (4) repeats Column (3), but replaces the outcome with the measure of the wage for new hires constructed by Basu and House (2016) from the National Longitudinal Survey of Youth; this measure controls for occupation, industry and demographic characteristics, as well as the cumulative tightness measure of Hagedorn and Manovskii (2013). Columns (3) and (4) use robust standard errors.

Table 19: Correcting Selection Bias—Robustness

	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$					
	Control for employment growth	Trim probabilities	Lagged wage in selection equation	Including entrants	Establishment Level	Annual
ΔU_{st}	-0.05	-0.01	-0.06	-0.00	-0.02	-0.12
	(0.01)	(0.02)	(0.02)	(0.02)	(0.05)	(0.08)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.78	-1.45	-1.67	-1.48	-0.87	-1.38
	(0.10)	(0.07)	(0.11)	(0.07)	(0.10)	(0.14)
Observations	1,450,109	1,449,058	610,384	1,458,110	798,460	686,647
Time Fixed Effect	✓	✓	✓	✓	✓	✓

Notes: this table reports robustness of the main selection corrected regression specification, column (2) of Table 8. In the first column, we remove the Heckman selection terms, and instead control for employment growth between the start of the year and the start of the previous year, in the establishment. In the second column, we repeat the baseline Heckman correction, but drop observations with a predicted probability of selection outsize the unit interval, following the recommendation of Das et al (2003). In column (3), we re-estimate the baseline Heckman correction but add the lagged wage to the variables in the selection equation. In column (4) we also include entrants. In columns (5) and (6), we aggregate the selection equations and selection-corrected regressions to the establishment and annual level, respectively. Standard errors are in parentheses, clustered by state.

Table 20: Heterogeneity in Wage Cyclicality by Turnover

Ratio of vacancies to employment:	Nominal Wage Growth at the Job Level, $\Delta \log w_{jst}$			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
ΔU_{st}	-0.08	-0.04	-0.06	-0.01
	(0.02)	(0.02)	(0.08)	(0.02)
$\Delta U_{st} \times I(\Delta U_{st} < 0)$	-1.64	-1.71	-1.55	-1.80
	(0.07)	(0.13)	(0.15)	(0.16)
Observations	434,176	473,794	435,034	374,856
Time Fixed Effect	✓	✓	✓	✓

Notes: this table reports estimates of downward wage rigidity with heterogeneity. We estimate the baseline regression, that is, column (2) of Table 4. We calculate the ratio of vacancies to employment within each occupation. Vacancies are the number of observations in each 6 digit occupation, for the main sample, and employment is measured at the 6 digit level from the 2014-16 Occupational Employment Statistics. W estimate wage rigidity separately for observations in the first through fourth quartiles of the distribution of the ratio of vacancies to employment. Standard errors are in parentheses, clustered by state.

Table 21: Detailed Occupations in Burning Glass vs. Occupational Employment Statistics

	6 Digit SOC Occupation Shares in Burning Glass		
	(1)	(2)	(3)
Occupation Shares in OES	0.84	0.86	0.92
	(0.05)	(0.05)	(0.06)
Observations	666	666	666
2 Digit Occupation Fixed Effect		✓	
3 Digit Occupation Fixed Effect			✓

Notes: the outcome variable is the share of vacancies within each 6 digit occupation, for the main Burning Glass sample. The regressor is the share of employment within each 6 digit occupation, from the 2014-16 Occupational Employment Statistics. Column (1) has no controls, column (2) adds 2 digit occupation fixed effects, and column (3) adds 3 digit occupation fixed effects. Robust standard errors are in parentheses.

Table 22: County Employment in County Business Patterns vs. in Burning Glass

	Log County Vacancies in Burning Glass		
	(1)	(2)	(3)
Log County Employment	0.99	1.00	0.98
	(0.01)	(0.01)	(0.01)
Observations	3,025	3,025	3,025
Census Division Fixed Effect		✓	
State Fixed Effect			✓

Notes: the outcome variable is 100 times the log number of vacancies within each county, for the main Burning Glass sample. The regressor is 100 times log employment for each county, as measured by the 2016 County Business Patterns. Column (1) has no controls, column (2) adds fixed effects for the census division, and column (3) adds fixed effects for the state. Robust standard errors are in parentheses.

Table 23: Testing Whether Coverage of Dataset is Cyclical

	(1)	(2)	(3)	(4)
	Change in Share of Coverage at the State by Quarter Level			
	Panel A: Outcome is Δ(Total Burning Glass Vacancies$_{st}$/JOLTS Vacancies$_{st}$)			
ΔU_{st}	0.10	0.11	0.15	0.16
	(0.35)	(0.35)	(0.36)	(0.36)
Observations	2,040	2,040	2,040	2,040
	Panel B: Outcome is Δ(Burning Glass Vacancies in Main Sample$_{st}$/Total Burning Glass Vacancies$_{st}$)			
ΔU_{st}	0.02	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,091	2,091	2,091	2,091
Time Fixed Effect	✓	✓	✓	✓
State Fixed Effect		✓		✓
Weights	Employment	Employment	Vacancies	Vacancies

Notes: this table studies whether the coverage of various datasets is cyclical. In Panel A, the outcome variable is the change in the share of total 2010-2020Q2 Burning Glass vacancies in each quarter and state, relative to seasonally adjusted total vacancies in each state and quarter from the seasonally adjusted 2010-2020Q2 Job Opening and Labor Turnover Survey, measured in percentage points. We apply the rescaling formula of Carnevale et al (2014) so that vacancies in JOLTS and Burning Glass are expressed in the same units. In Panel B, the outcome variable is the change in the share of Burning Glass vacancies in each quarter and state that are in the main regression sample, relative to total vacancies in Burning Glass in the state and quarter, measured in percentage points. The main regression sample is vacancies with job title information, that post a point wage, and post in multiple quarters. In all panels, the regressor is quarter by state unemployment changes, in percentage points, from the 2010-2020Q2 Local Area Unemployment Statistics. Columns (1) and (2) are weighted by average state employment for 2010-2020 from the Quarterly Census of Employment and Wages. Columns (3) and (4) are weighted by average state vacancies for 2010-2020 from the full sample of Burning Glass vacancies. Standard errors are in parentheses, clustered by state.

Table 24: Testing Whether Coverage of Dataset Varies by Urban Status

	Change in Share of Coverage at the State by Quarter Level, Heterogeneity by Urban Share			
	(1)	(2)	(3)	(4)
	Panel A: Outcome is Δ(Total Burning Glass Vacancies$_{st}$/JOLTS Vacancies$_{st}$)			
$\Delta U_{st} \times I$ (low urban share)	0.40	0.42	0.53	0.55
	(0.39)	(0.39)	(0.38)	(0.39)
$\Delta U_{st} \times I$ (high urban share)	0.08	0.09	0.12	0.14
	(0.30)	(0.30)	(0.31)	(0.31)
Observations	2,040	2,040	2,040	2,040
	Panel B: Outcome is Δ(Burning Glass Vacancies in Main Sample$_{st}$/Total Burning Glass Vacancies$_{st}$)			
$\Delta U_{st} \times I$ (low urban share)	0.02	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta U_{st} \times I$ (high urban share)	0.02	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,091	2,091	2,091	2,091
Time Fixed Effect	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓
Weights	Employment	Employment	Vacancies	Vacancies

Notes: this table repeats the analysis of 23, while modifying the regressors. In this table, the regressors are the change in quarterly state unemployment, interacted with whether the state has an above or below weighted median share of the population in urban areas. We rank states by their share in urban areas, as measured by the Census Bureau in 2010. The median is weighted by average state employment over 2010-2020.

Table 25: Statistics for Dun & Bradstreet / Burning Glass Merged Sample

	Business Dynamics Statistics	Burning Glass / Dun & Bradstreet
Panel A: Establishment Age (Years)		
0-5	20	14
6-10	14	24
11+	66	62
Panel B: Establishment Employment		
1-19	24	6
20-499	54	56
500+	22	38
Panel C: Establishment Idiosyncratic Shocks (pp)		
Job creation rate	12	13
Job destruction rate	10	16
Reallocation rate	20	27

Notes: this table compares information from the 2019 Business Dynamics Statistics (BDS), to information on establishments in Dun & Bradstreet that merge to the main Burning Glass sample. In column (1) is information from the BDS, and in column (2) is the corresponding information from the Burning Glass main sample. Panel A reports the share of establishments in each age bucket, where the buckets are 0-5 years in operation, 6-10 years in operation, and at least 11 years in operation. Panel B reports the share of establishments in each size bucket, where the buckets are 1-19 employees, 20-499 employees, and at least 500 employees. Panel C reports measures of establishment idiosyncratic shocks. In column (1) we report the job creation rate, job destruction rate and reallocation rate,as reported by the BDS and originally defined by Davis, Haltiwanger, and Schuh (1998). In column (2) we report the median job creation, job destruction and reallocation rates from the Burning Glass main sample.

C Data Appendix

C.1 Description of Burning Glass Algorithm

C.1.1 Classifying Job Titles

This subsection describes the algorithm that Burning Glass uses to extract job titles from the text of vacancy postings, Burning Glass' "CleanTitle" field. The wage posted in the vacancy is not used at any stage in this process. There is natural language processing model that segments the posting into different parts (such as company description, position description, and so on) and then finds the text containing the raw job title. The raw job title is then cleaned and standardized as follows. First, the title is cleaned by matching to an existing dataset of job titles maintained by Burning Glass. Second, there is a step to remove any location identifiers from the text of the job title (since these populate the separate location field). Third, there is a further step to standardize punctuation and other aspects of writing.

C.1.2 Measuring Length of Vacancy Posting

Burning Glass' algorithm checks daily for new vacancy postings, when a new vacancy is posted, it is assigned a start date. The end date is the minimum of (a) the first seen date + 60 days, or (b) the last seen date; we discard the vacancy length information for vacancies that record a length of 60 days. The last seen date is the date that the posting is either (i) removed from the internet; or (ii) includes some text saying it is no longer available, not accepting applications. Burning Glass' algorithm checks daily if (i) or (ii) is true.

C.2 Representativeness of the Main Sample

This section expands on the discussion in subsection 2.1. We show that our main Burning Glass Sample is broadly representative of the population of US employment and establishments; with notable caveats that we will make clear.

Appendix Figure 6 shows that the main regression sample is broadly representative at the regional level. We calculate the share of vacancies in each state over 2010-2020, for our main sample in Burning Glass; and from the official source, the Bureau of Labor Statistics' Job Opening and Labor Turnover Survey. Overall, the distribution of vacancies at the state level match across the two datasets.

Appendix Figure 7 shows that the main regression sample is also mostly representative at the broad occupation level. We calculate the share of employment over 2010-2020 for the main sample in Burning Glass; and from official data, namely, the Occupational Employ-

ment Statistics (OES) for 2010-2020. The distribution of jobs across occupations is broadly similar across the two sources. The main discrepancy is in transportation, which is heavily overweighted in Burning Glass. One innocuous reason for the difference could be that transporation is an occupation with high turnover; so, will be over-represented in vacancies relative to employment.

Appendix Table 21 shows that the main sample is also reasonably representative at the detailed occupation level. We construct the employment share within each 6 digit SOC occupation from the OES, and corresponding shares for vacancies from the main Burning Glass sample. We regress the Burning Glass share on the OES share. Column (1) reports a coefficient of 0.84—meaning, an occupation with a 1 pp higher share in the OES has a 0.84 pp higher share in Burning Glass, suggesting relatively similar representation. The coefficient is similar in Columns (2) and (3), which add 2 and 3 digit occupation fixed effects, respectively. Indeed, column (3) has a regression coefficient of 0.92, suggesting that detailed occupation shares in Burning Glass and the OES are very similar.

Appendix Table 22 shows that the main sample is also reasonably representative at the county level. We construct log employment at the county level, from the 2016 County Business Patterns (CBP), and construct log vacancies at the county level for the Burning Glass main sample. We regress Burning Glass log vacancies on CBP log employment. Column (1) reports a coefficient of 0.99—meaning, county with 1 percent higher employment in the CBP has 1 percent higher vacancies in Burning Glass, suggesting relatively similar representation. The coefficient is similar in Columns (2) and (3), which add census division and state fixed effects, respectively.

Appendix Table 23 shows that selection is not cyclical—neither selection into online vacancies in general, nor selection into the main regression sample. In Panel A, the outcome variable is the quarter by state change in the full set of vacancies in Burning Glass, relative to JOLTS. Here, the denominator is all online vacancies posted in the state and quarter; whereas the numerator is the number of vacancies in the state and quarter from official sources. The regressor is the change in quarter by state unemployment. Column (1) has a regression coefficient of 0.1, meaning that a 1 pp increase in unemployment leads to a statistically and economically insignificant decline in ratio of online vacancies to total vacancies in the state. Column (2) reports a similarly small estimate, after added in state fixed effects. Columns (3) and (4) show similarly small numbers when weighting by total vacancies in each state, instead of employment. Panel B instead asks whether there is cyclical selection within Burning Glass, between the full set of online vacancies and the main sample. Specifically, the outcome is the quarter by state change in the number of vacancies in the main sample, relative to the full set of Burning Glass vacancies posted in the state and quarter. The estimates in Panel

B are tiny, suggesting no cyclical selection. For example, column (1) reports an estimate of 0.02, meaning that a 1 pp increase in state unemployment leads to a 0.02 pp increase in vacancies in the main sample, relative to the full set.

Next, we show that our main sample is also broadly representative of the population of establishments. To use information on establishments, we merge the Burning Glass main sample to establishment level information from Dun & Bradstreet. Section 6.1 describes the Dun & Bradstreet dataset, and the merge to Burning Glass, in detail. Briefly, Dun & Bradstreet is a business analytics company that collect information on the universe of establishments in the United States from 1990-2020. D&B collects data on employment at the start of the year, industry classification and establishment age. For cost reasons, we purchased data from D&B only on the largest 30% of employers in Burning Glass. We achieve a high merge rate—75% of our main sample matches to establishments in Dun & Bradstreet. Dun & Bradstreet is known to measure employment poorly for very small establishments. Once these establishments are removed, Dun & Bradstreet appears to match official employment sources at the industry and regional level (Haltiwanger et al, 2013). Our extract from Dun & Bradstreet does not include very small establishments, suggesting our extract should measure establishment employment reasonably well.

Appendix Figure 8 shows that state level coverage of establishments is similar in the Burning Glass main sample and in official sources. Appendix Figure 9 shows the main sample is mostly representative of the distribution of establishments across industries—other than being somewhat under-weight healthcare and over-weight retail. Appendix Table 25, Panel A shows that the distribution of establishment age is similar in the main sample and in official sources. Panel B shows that our merged dataset is under-weight small establishments but otherwise representative—probably because the data we purchased from D&B excludes small establishments. Panel C shows that standard measures of the idiosyncratic shocks facing establishments—namely job creation and job destruction as in Davis, Haltiwanger, and Schuh (1998)—are similar in the two datasets, suggesting that our D&B subsample adequately measures establishment outcomes at annual frequency.

C.3   Hazard Estimation of the Frequency of Wage Change

This subsection describes the procedure for estimating the hazard rate of the latent wage change. We assume the hazard rate of the latent wage change is constant across time and common across all jobs within each 2 digit SOC occupation. Let $\{w_{it}\}$ be the sequence of log wages for job i and quarter t. Let γ_{it} be the gap in quarters between the wage at t and wage in the previous vacancy that was posted. Let I_{it} be an indicator for whether the wage

changed, where $I_{it} = 1$ if $w_{it} \neq w_{i,t-\gamma_{it}}$. The quarterly hazard rate of wage change, assumed to be time-invariant, is given by λ, which we estimate by maximum likelihood.

The likelihood function is then

$$L = \prod_i \prod_t \left(1 - e^{-\lambda\gamma_{it}}\right)^{I_{it}} \left(e^{-\lambda\gamma_{it}}\right)^{1-I_{it}}.$$

The first order condition $\partial \log L / \partial \lambda$ implicitly defines the maximum likelihood estimate $\hat{\lambda}$ as

$$\sum_i \sum_t \frac{I_{it}\gamma_{it}}{e^{\hat{\lambda}\gamma_{it}} - 1} = \sum_i \sum_t \left(1 - I_{it}\right)\gamma_{it}.$$

With a hazard estimate in hand, we can calculate the other statistics as follows. The probability of a wage change for each occupation is $f = 1 - e^{-\lambda}$. The implied duration of time for which a wage is unchanged is $d = 1/\lambda$. The overall probability of wage change is the median probability across occupations, weighted by the number of vacancies in each occupation. Similarly, the overall implied duration is the the weighted median of the implied duration for each occupation. We discard left-censored wage spells. We can calculate the hazard rate of wage increase and decrease in an analogous way, and thereby calculate the probability of wage increase and wage decrease.

C.4   Construction of Census Region Level Price Measures

In this subsection, we describe how we construct measures of the consumer price index at the quarter by census division level. These measures were not available before this paper—the closest analogue is the state level inflation series of Hazell et al (2022), which is not available after 2017.

The Bureau of Labor Statistics (BLS) publishes quarterly census division level prices after 2018, but not before 2018. We now describe how we calculate census division level prices before 2018. The BLS reports MSA level inflation for the largest 20 metro areas in the United States. For census divisions in which MSA level inflation data is available from the BLS, we take the mean price level across MSAs within each census division to create a division level series for quarterly prices.

Then, we develop a procedure to calculate prices in census divisions for which MSA level inflation data is unavailable from the BLS. These census divisions contain only "mid-size" MSAs as defined by the BLS, instead of the "large" MSAs for which the BLS reports dedicated a dedicated inflation series. Therefore we assign to these census divisions the series reported by the BLS for inflation for mid-size cities from the corresponding census region. Recall that

census divisions are a grouping of states into 9 groups, whereas census regions are a coarser grouping of states into 4 groups. As it turns out, we do not have to assign multiple census divisions to the same census region during this step.

We splice together our series for census division level inflation, with the BLS series, in the final quarter of 2017. We use the same procedure, separately, for the measures of census division prices including and excluding shelter prices.

C.5 Details on Dun & Bradstreet

C.5.1 Merge

We merge the main Burning Glass sample to Dun & Bradstreet as follows. First, we clean firm names in Burning Glass and Dun & Bradstreet in order to carry out the match. The firm name cleaning algorithm closely follows the algorithm developed in Hazell et al (2021), which uses a combination of standard cleaning procedures and a machine learning algorithm. We began with a list of (unclean) unique employer names in Dun & Bradstreet and Burning Glass. Then we clean both sets of employer names in the same way. We truncate employer names to 128 characters, and then we manually correct the names of some large employers, making use of code from the NBER Patent Data Project. We additionally stripped common words ("The", "Corp.", "Company", etc.), all non-alphanumeric punctuation, spacing, and capitalization. Next, we implemented a fuzzy matching algorithm, called *dedupe,* to create clusters of similar employer names. Dedupe makes use of a combination of squared edit distance comparisons subject to a confidence score threshold (which we chose to be 0.5, or 50% based on sample performance), as well as a small sample of names with manual labelling provided as training. For computational reasons, we employ blocking to limit the number of comparisons for each name to roughly 90 percent of each group of names sharing the first two letters. Within each cluster of names generated by dedupe, we set all names to that of the most common employer. Finally, we merge this crosswalk back on to the original (unclean) firm names and set the names to the new, cleaned versions to complete the process.

Then, we match Burning Glass establishments to Dun & Bradstreet establishments, based on City, County, State and cleaned firm name. Here, City is a field developed by Burning Glass, which roughly corresponds to a metropolitan statistical area—Dun & Bradstreet reports a similar field. In order to save money, we merge Burning Glass only to the largest 30% of establishments in Dun & Bradstreet. We are able to achieve a 75% match rate for the Burning Glass main sample.

C.5.2   Validating the D&B Dataset

To validate our measure of establishment employment, we compare to official data from the Business Dynamics Statistics (BDS), and find a reasonably close match. Appendix Figure 8 shows that state level coverage of establishments is similar in the Burning Glass main sample and the BDS. Appendix Figure 9 shows the main sample is mostly representative of the distribution of establishments across industries—other than being somewhat under-weight healthcare and over-weight retail. Appendix Table 25, Panel A shows that the distribution of establishment age is similar in the main sample and the BDS, though our extract is under-weight young establishments. Panel B shows that our merged dataset is under-weight small establishments but otherwise representative—probably because the data we purchased from D&B excludes small establishments. Panel C shows that standard measures of the idiosyncratic shocks facing establishments—namely job creation and job destruction as in Davis, Haltiwanger, and Schuh (1998)—are similar in the two datasets. This step is important because our selection correction will use establishment information to measure idiosyncratic shocks.

D Job Composition and Variance of Wage Cyclicality Estimates

This section formally proves that job composition raises the variance of wage cyclicality estimates. As such, regressions that do not correct for job composition may lack the power to detect downward wage rigidity.

Proposition 1. *For $S, T < \infty$, and if $\sum_i \log w_{ist} \Delta \nu_{ist}$ and $\sum_i \log w_{ist} \Delta \nu_{ist}$ are independent conditional on ΔU_{st}, then*

$$V\left[\hat{\delta}_{Average}|\Delta U_{st}\right] > V\left[\hat{\delta}_{Job\ Level}|\Delta U_{st}\right] \quad and \quad V\left[\hat{\beta}_{Average}|\Delta U_{st}\right] > V\left[\hat{\beta}_{Job\ Level}|\Delta U_{st}\right]$$

Proof. Summing regression equation (3) over i yields

$$\sum_i \nu_{ist} \Delta \log w_{ist} = \alpha + \gamma_t + \beta \Delta U_{st} + \delta_{\text{Job Level}} I\left[\Delta U_{st} < 0\right] \Delta U_{st} + \varepsilon_{st} \tag{9}$$

where $\varepsilon_{st} = \sum_i \nu_{ist} \varepsilon_{ist}$. We can substitute equation (2) into equation (4) to rewrite the regression that uses average wages as

$$\sum_i \nu_{ist} \Delta \log w_{ist} + \sum_i \log w_{ist} \Delta \nu_{ist} = \bar{\alpha} + \bar{\gamma}_t + \bar{\beta} \Delta U_{st} + \delta_{\text{Average}} I\left[\Delta U_{st} < 0\right] \Delta U_{st} + \bar{\varepsilon}_{st}. \tag{10}$$

For notational simplicity, we can rewrite equation (9) as

$$y_{st} = \mathbf{x}'_{st} \mathbf{b} + \varepsilon_{st}$$

and equation (10) as

$$y_{st} + u_{st} = \mathbf{x}'_{st} \bar{\mathbf{b}} + \bar{\varepsilon}_{st}$$

where

$$y_{st} \equiv \sum_i \nu_{ist} \Delta \log w_{ist}$$

$$u_{st} \equiv \sum_i \log w_{ist} \Delta \nu_{ist}.$$

$\mathbf{x}'_{st} \mathbf{b}$ and $\mathbf{x}'_{st} \bar{\mathbf{b}}$ collect the covariates and coefficients in regressions (9) and (10) respectively. The OLS estimator of $\mathbf{b}$, which we term $\hat{\mathbf{b}}$, is

$$\hat{\mathbf{b}} = \left(\frac{1}{ST} \sum_{s,t} \mathbf{x}_{st} \mathbf{x}'_{st}\right)^{-1} \left(\frac{1}{ST} \sum_{s,t} \mathbf{x}_{st} y_{st}\right).$$

The variance of $\hat{\mathbf{b}}$ conditional on $\mathbf{x}_{st}$ is

$$
\begin{aligned}
V\left[\hat{\mathbf{b}}|\mathbf{x}_{st}\right] &= V\left[\left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}\left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}y_{st}\right)|\mathbf{x}_{st}\right] \\
&= \left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}\frac{1}{(ST)^2}V\left[\left(\sum_{s,t}\mathbf{x}_{st}y_{st}\right)|\mathbf{x}_{st}\right]\left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}
\end{aligned}
$$

The OLS estimator of $\bar{\mathbf{b}}$, which we term $\hat{\bar{\mathbf{b}}}$, is

$$
\hat{\bar{\mathbf{b}}} = \left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}\left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\left(y_{st}+u_{st}\right)\right).
$$

Then the variance of $\hat{\bar{\mathbf{b}}}$ conditional on $\mathbf{x}_{st}$ is

$$
V\left[\hat{\bar{\mathbf{b}}}|\mathbf{x}_{st}\right] = V\left[\hat{\mathbf{b}}|\mathbf{x}_{st}\right] + \left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}\frac{1}{(ST)^2}V\left[\sum_{s,t}\mathbf{x}_{st}u_{st}|\mathbf{x}_{st}\right]\left(\frac{1}{ST}\sum_{s,t}\mathbf{x}_{st}\mathbf{x}'_{st}\right)^{-1}
$$

(11)

The second term in equation (11) is a matrix with strictly positive entries on its leading diagonal for $S,T < \infty$. Hence every entry on the leading diagonal of $V\left[\hat{\bar{\mathbf{b}}}|\mathbf{x}_{st}\right]$ is greater than the corresponding entry on the leading diagonal of $V\left[\hat{\mathbf{b}}|\mathbf{x}_{st}\right]$.

E   Selection Correction

This subsection explains how we implement the non-parametric Heckman estimator of Das et al (2003). We implement the selection correction in two steps as follows:

1. In the first step, we estimate the probability of vacancy posting p_{ist}, for a job i in a state s and quarter t. We estimate a regression of ξ_{ist} on $n_{is,t-1}$ and $n_{is,t-2}$, establishment employment at the start of the year and the previous year. Recall that ξ_{ist} is an indicator for whether the vacancy posts. We interact the regressors with state-by-time fixed effects in a third order polynomial series regression. Similar, we estimate $p_{is,t-j}$ from a regression of $\xi_{is,t-j}$ on $n_{is,t-j-1}$ and $n_{is,t-j-2}$, interacted with state-by-time fixed effects in a third order series regression.

2. Then, we re-estimate our baseline regression equation. However, we include as extra regressors, a third order polynomial series regression in our estimates of $p_{ist}, p_{is,t-j}$ as well as $n_{is,t-1}$ and $n_{is,t-2}$.