Incidence and Evolution of Nominal Wage Rigidity in the US*

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Abstract

This paper documents the change in nominal wage rigidity in the US using the 1996-00 and 2008-13 panels of the Survey of Income and Program Participation (SIPP). Using the empirical methodology of Barattieri, Basu & Gottschalk (2014) to correct for measurement errors in self-reported wages, this paper finds evidence of (i) an increase in the frequency of wage adjustment among hourly job-stayers over the two periods, and (ii) conditional on wage adjustments, a higher proportion of wage cuts during the Great Recession relative to the subsequent recovery. These findings are robust when the methodology is applied to salaried workers. They can be seen in light of increasing labor market flexibility in the US over the recent decades.

JEL Classification: E24, J31, E32

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1 Introduction

This paper examines the change in the extent of nominal wage rigidity in the US. Using micro-data from the Survey of Income and Program Participation (SIPP) for the years 1996-00 and 2008-13, I document an increase in the frequency of wage adjustment over the two periods. I also document a relatively higher proportion of wage cuts, conditional on a wage change taking place, and a lower degree of wage rigidity during the Great Recession than the subsequent recovery. These patterns of nominal wage adjustments are robust across hourly and non-hourly workers.

The empirical patterns observed in this paper can be seen in light of the recent structural changes in the economic environment of the US labor market. A notable observation has been an increase in employer concentration, along with a simultaneous decline in worker bargaining power, that started around the late 1970s and continued well into the 2000s (Krueger 2018).[1] This is expected to have an important effect that can be understood in the context of a principal-agent model belonging to the class of implicit contract theory with a risk neutral firm and a risk averse worker (Azariadis 1975). The firm may no longer find it optimal to offer the worker with a fixed contract that provides implicit insurance against fluctuations in income or labor productivity.[2] Evidence of this has been documented in the form of increasing demand for flexible labor (Kalleberg 2009, Katz & Krueger 2016, 2019), and increasing volatility in the extensive and intensive margins relative to output (Galí & Van Rens 2014). Besides these margins, wage-setting comprises another important alternative for firms to adjust their labor force in response to shocks.

A highly influential premise in macroeconomic analysis has been the existence of downward rigidity in the nominal wage-setting process. This refers to firms' inability to cut wages, because of workers' reluctance from accepting wage cuts and the ensuing union pressure.[3] The weakening bargaining power of workers may have implications on the extent of downward wage rigidity. In particular, falling unionization and erosion of minimum wage could make the worker less reluctant from accepting wage cuts, making wages less downwardly rigid. This may be particularly true in a downturn, when layoffs into unemployment is the only other alternative to receiving wage cuts. This raises two related

[1]This has been attributed to an erosion of the minimum wage (David, Manning & Smith 2016), and a decline in unionization (Card 2001) and collective bargaining, which have fallen short in offsetting the effect of growing monopsony power in the labor market.

[2]Some earlier literature discussing how increased worker bargaining power through unionization may insure its members against aggregate shocks includes Agell & Lommerud (1992), Azariadis (1975), Horn & Svensson (1986).

[3]This premise was first propounded by Keynes in 1936. He cited nominal wage rigidity, as a result of workers' refusal to accept wage cuts, as a reason why labor markets do not clear in a recession and instead exhibit high unemployment.

questions: Has nominal rigidity in the wage-setting process become a less binding constraint over time, as worker bargaining power has weakened? During a recession, conditional on the worker experiencing a wage change, is there a higher incidence of receiving a wage cut?

This paper attempts to address these questions using microdata from the SIPP. Following the empirical methodology used in Barattieri, Basu & Gottschalk (2014) that corrects for measurement errors in self-reported wages, I arrive at the distribution and frequency of nominal wage adjustments for hourly job-stayers in the 2008-13 panel of the SIPP. To answer the first question and understand the change in wage rigidity over time, I compare these measures with the ones computed using the 1996-00 round of the SIPP in Barattieri et al. (2014). I find that overall, within-jobs wages are still more rigid than flexible, but there has been a relative increase in the frequency of wage adjustment in 2008-13. I also document a fall in the standard deviation of the distribution of wage changes over the two periods, with more workers experiencing smaller wage changes in 2008-13. This is consistent with falling aggregate wage growth rate among hourly workers that has been observed in the US over these two periods.[4] Next, to answer the second question and examine the cyclical variation in nominal wage adjustments, I focus on the 2008-13 panel of the SIPP. Applying the same measurement error correction procedure, I divide the sample into the Great Recession (2008-10) and recovery (2010-13). I find that wage cuts, as a proportion of all non-zero wage changes, were almost twice as high during the Great Recession than the subsequent recovery. Further, I show that these patterns of nominal wage adjustment observed for hourly workers are also robust for non-hourly workers who report their monthly salary in the SIPP.

The empirical findings of this paper have implications on two strands of literature that have partially attempted to understand the behavior of wages and examine its implications on nominal wage rigidity. The first strand documents an increase in wage volatility in the recent decades, but the implications on wage rigidities remain indirect. Galí & Van Rens (2014) document an increase in the relative volatility of hourly wage since the Great Moderation and interpret this as evidence of there being a consistent decline in wage rigidities. Champagne & Kurmann (2013) and Nucci & Riggi (2013) document an increased volatility in real wages from 1980s as a result of increasing incidence of deunionization and performance pay. Blanchard & Gali (2007) and Blanchard & Riggi (2013) argue that a reduction in wage rigidity is needed to account for the decrease in the macroeconomic effects of oil

[4]Another effect of weakening worker bargaining power has been a shrinking in the slice of pie going to workers. This has been documented in the form of falling share of national income accruing to labor, that started around the 2000s, even though firm profits have continued to rise. This trend has been partly responsible for the slowdown in the wage growth rate (*Council of Economic Advisers Issue Brief, October 2016*).

price shocks.

The second strand of literature is more closely related to estimating downward nominal wage rigidity from micro-level datasets. Whereas most of the studies agree that some downward rigidity in the wage-setting process exists, they offer mixed evidence on its extent and evolution. Elsby, Shin & Solon (2016) give a sense of the main findings of this literature by examining nominal wage rigidities using the Current Population Survey (CPS).[5] They show a modest upward secular trend in the frequency of hourly workers reporting the same nominal wage in successive years from 1980-2012, indicating an increase in wage rigidity over time. On the other hand, they document an equally substantial proportion of workers reporting wage cuts, suggesting high wage flexibility. The authors argue that these seemingly contradictory findings are an artifact of measurement errors contaminating self-reported wages in survey-based data.[6] To circumvent this problem one solution adopted by literature has been to correct for measurement errors.[7] More recently the literature has turned to administrative datasets. Jardim, Solon & Vigdor (2019) use data drawn from Unemployment Insurance Records of Washington state from 2005-15. They find that among job-stayers, wage cuts occur with a high frequency and wage freezes are much less common than household surveys report. Their findings resonate with Kurmann & McEntarfer (2019) who use US Longitudinal Employer Household Dynamics (LEHD) of Washington State. Grigsby, Hurst & Yildirmaz (2019) examine data from a payroll processing company for 2009-16 and find substantial downward rigidity in contractual wage and flexibility in realized compensation to workers. The short time horizon of the availability of administrative data sources has, however, prevented them from providing a longer view on the change in wage rigidity in the context of structural changes in the rest of the economy.[8]

[5] Other studies using CPS include Card & Hyslop (1997), Daly & Hobijn (2014), Daly, Hobijn, Lucking et al. (2012), Jo (2019).

[6] More recently, Jo (2019) examines the CPS (1979-2013) and SIPP (1984-2013) to determine empirical patterns of nominal wage change distribution. Without correcting for measurement errors, the author finds evidence of asymmetry with a relatively small proportion of wage cuts, and a large spike at zero in annual wage changes. These patterns are broadly in line with the rest of literature, and the evidence presented in this paper. The magnitude of patterns however, do not match with studies that employ measurement-error free sources of administrative datasets (such as Grigsby et al. (2019)). They are not directly comparable with this paper that explores quarterly wage changes instead of annual ones.

[7] Altonji & Devereux (2000) estimate a model employing the Panel Study of Income Dynamics (PSID) data that accounts for measurement errors, Gottschalk (2005) and Barattieri et al. (2014) test for structural breaks in individual wage histories from the SIPP. These studies find that nominal wage freezes are understated in reported wages. This paper is an extension of these studies in trying to correct for measurement error in self-reported individual wage data.

[8] A notable exception is Fallick, Lettau & Wascher (2016) who use employer-based data on jobs from Bureau of Labor Statistics' Employment Cost Index from 1982-2014 but find mixed evidence on the evolution of wage rigidity.

This paper tries to bridge the gap between the two related strands of literature discussed above. It uses the tools available for correcting measurement error in self-reported wages, to examine whether there has been an increase in wage flexibility over time. This paper closely relates to Elsby et al. (2016) in terms of taking a long-run view on wage rigidity. It overcomes the setback in their paper: of mis-reported wages biasing their estimates of wage rigidity. However, the main downside of this paper is the lack of continuity of the SIPP panels. At the same time, the 1996 and 2008 panels, even though comprising of different individuals, are both representative of the population. Together, they provide a snapshot of the economy at two points in time when important changes in the economic environment of the US labor market were taking place.

Finally, the empirical findings of this paper bring new evidence to the recent debate on cyclical variation in wage rigidity. This debate, which is aggravated by the already-existing lack of consensus over the extent of wage rigidity, has gained prominence especially in the aftermath of the Great Recession. The debate is based on two outcomes that can arise in a recession in a framework of nominal rigidity in wages: One, during recessions rigidities become more binding. This would happen in a scenario when firms that receive a negative shock would be less likely to adjust wages to the desired level. This outcome has been attributed to account for the increasing unemployment rate during the Great Recession, as firms adjust disproportionately less through the wage margin (Daly & Hobijn 2014, Grigsby et al. 2019, Jo 2019, Yellen et al. 2016). Two, during recessions, when layoffs into unemployment is the only other alternative, worker resistance to wage cuts would reduce and the latter would become increasingly common. Therefore, wages would be less downwardly rigid in a recession (Elsby et al. 2016, Jardim et al. 2019). The 2008 SIPP panel allows me to examine how nominal adjustments in wages and salaries vary over the business cycle. Documenting the distribution and frequency of these adjustments over the Great Recession and subsequent recovery, I find that wages and salaries were relatively less rigid, and workers experienced a higher incidence of pay cuts during the Great Recession. These empirical findings have implications on macro models that include nominal wage rigidity as a key friction in explaining aggregate fluctuations. The results show that nominal wage adjustments have increased overtime and vary over the business cycle. Therefore, models of nominal wage rigidity that assume a constant probability of workers receiving wage adjustment, particularly wage cuts, are at odds with this empirical evidence.

Table 1: Description of the data

	1996:03-2000:02	2008:08-2013:11
Total SIPP waves	12	16
Total people between 15 to 64 years (first wave)	39,095	66,672
Females	19,321	34,649
Hourly workers (first wave)	17,148	21,547
Females	8,931	11,577
Mean age	38	39.8
Mean wage (hourly workers)	$10.03	$13.3
5th percentile	$5	$7.25
95th percentile	$20.17	$28
Total people between 15 to 64 years (last wave)	29,975	30,566
Hourly workers (last wave)	12,574	9,495

Notes: This table reports the descriptive statistics of Survey of Income and Program Participation, 1996 and 2008 panels. The sample is restricted to workers between 15 and 64 years of age.

2 Data

This paper uses data from the SIPP which is a tri-annually collected, representative, panel survey administered by the US Census Bureau. The primary focus is on the 2008 panel, running from August 2008 to November 2013, and providing a time series of individual wages of up to 16 waves. The 2008 panel is compared to the 1996 panel, running from March 1996 to February 2000, and comprising 12 waves. Together, these two panels are the longest ones available for the SIPP.[9] Overall, the 1996-00 and 2008-13 periods of time had important differences and similarities. 1996-00 was a period of economic boom with record high employment to population ratios, whereas 2008-13 was a period of the Great Recession and subsequent recovery, when the economy recorded the highest unemployment rate since the twin recessions of the 1980s. The rate of inflation was low through both the periods, and was negative towards the end of the Great Recession.[10]

Table 1 reports descriptive statistics of the SIPP data. I restrict the sample to workers between 15 to 64 years of age. The SIPP 2008 panel interviewed around 66,000 persons

[9] The SIPP consisted of several panels prior to 1996, and two panels between 1996 and 2008. These panels are not the primary focus of this paper because of their shorter sample length. For completeness, Appendix A.5 extends the analysis in this paper to the remaining panels.

[10] Due to space considerations, I omit Table 1 of the original version of this paper that compares aggregate variables averaged over 1996-00 and 2008-13 to demonstrate similarities and differences across the two periods.

in the first wave, about a third of which comprised hourly workers. The mean hourly wage was $13.3. The sample dropped by a little more than half by the final wave of the survey, and the hourly workers still comprised a third of the sample. The attrition rate was relatively higher in SIPP 2008 due to the higher number of waves.[11]

My primary motive of using the SIPP over CPS that also reports individual wages is threefold: One, SIPP observes individual wages at a high frequency. This allows me to identify the measurement error component from raw wages using the methodology described in the next section. Two, SIPP panels provide consistent employer identifiers across waves for each job that the respondent has had. This allows me to examine frequencies of wage adjustment distinctly for job-stayers and job-switchers.[12] Three, SIPP easily matches individuals across waves, and keeps track of movers.[13]

The main downside of using the SIPP is the presence of "seam bias".[14] I overcome this problem by using wages observed during the interview month of each wave and drop recall-observations for the previous three months when the interview was not conducted. The importance of seam bias has been discussed in Appendix A.6. Another disadvantage of the SIPP is the discontinuity between it's panels, which prevents the data from being analyzed as a longer time series. Compared to the CPS, SIPP also has a smaller sample size.

3 Within-job Wage Rigidity

3.1 Methodology

The methodology of this paper is divided into two parts. The first part discusses within-job wage rigidity, largely following the empirical strategy of Barattieri, Basu & Gottschalk (2014) (henceforth, BBG). The second part, discussed in Section 4, relates to between-job

[11]While there is significant attrition in the SIPP, it would lead to an attrition bias in the wage rigidity estimate only if the measure of rigidity was different for those who attrite than those who stayed in the sample. There is no evidence of this.

[12]This information is missing in the CPS, where the job status of the respondent is missing after the 8-month break in the interviews.

[13]Apart from the CPS, SIPP offers advantages over the Panel Study of Income Dynamics (PSID) that also reports individual wage. The PSID comprises of annual interviews where earnings related information is gathered once a year and may combine wages and hours across all job changes that occurred over the course of that year. Annual interviews further prevent wage changes in jobs lasting less than a year from being captured. Kahn (1997), McLaughlin (1994) are some of the earlier works that used individual wage changes from the PSID to test for the presence of nominal wage rigidity. The first study finds limited evidence, while the latter finds more supporting evidence.

[14]Seam bias occurs when respondents are more likely to report inter-wave wage changes than intra-wave changes in a panel. This has been described in detail in Appendix A.6.

wage rigidity, and deviates from the BBG methodology.

The main goal of this methodology is to purge the impact of measurement errors from self-reported wages. This is done because measurement errors can bias the population frequency of wage change. In particular, they include classical reporting errors, which bias the proportion of wage freezes downwards, and wage changes upwards. They also include rounding errors, which may have the opposite effect (Elsby, Shin & Solon 2016). Under the assumption that the true wage changes in discrete steps and remains constant otherwise, the idea therefore, is to identify when a change in wage is a true change, and when it is noise.

Formally, suppose an individual's wage is observed for T periods under the same employer. Let there be m true nominal wage changes, or breaks, over those T periods. Then the reported wage at period t, y_t, would comprise of a constant wage $\bar{y}$ plus measurement error u_t:

$$\begin{aligned} y_t &= \bar{y}_1 + u_t \quad t = 1 \dots T_1 \\ &= \bar{y}_2 + u_t \quad t = T_1 + 1 \dots T_2 \\ &= \dots \\ &= \bar{y}_{m+1} + u_t \quad t = T_m + 1 \dots T \end{aligned} \tag{1}$$

Then, the objective is to estimate the constant wages in between m breaks, call $\{\bar{y}_1, ..., \bar{y}_{m+1}\}$, and the m break dates, $\{T_1, ..., T_m\}$, when the constant wage changes. BBG rely on a procedure of conducting structural break tests on an individual wage series.[15]

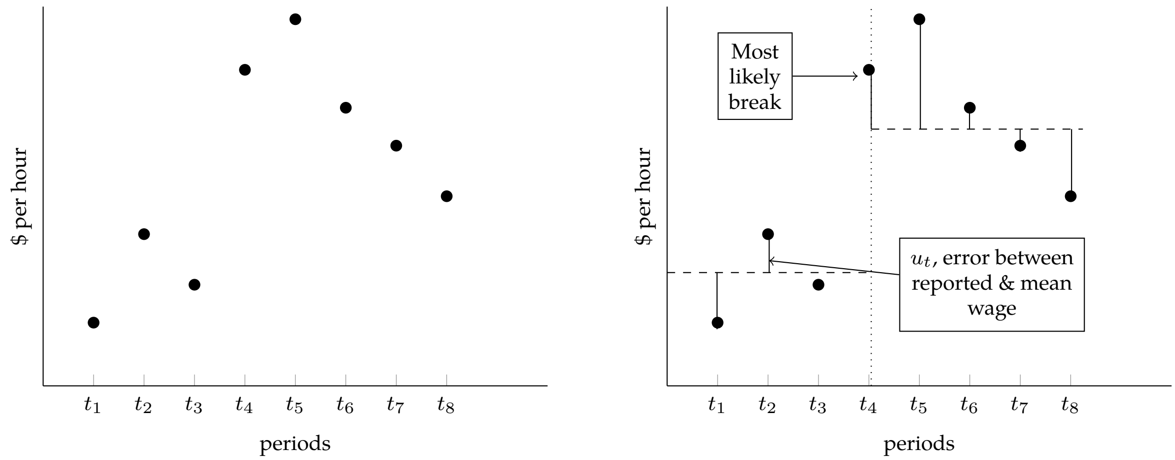
As a first step to account for measurement errors, I focus on hourly workers who directly report their pay in the SIPP. The reasons are threefold: One, workers may be more likely to accurately recall their hourly pay rate than their monthly earnings. Two, hourly wage is a closer proxy of base wage, than monthly earnings which may include non-base components such as bonuses, tips, and commissions (Grigsby, Hurst & Yildirmaz 2019). Three, directly reported hourly pay may suffer less from measurement error than indirectly computed hourly pay of non-hourly workers. This is because indirect hourly pay, computed by dividing pay per period by hours per period, may suffer from measurement error in reporting hours as well. This is discussed in detail in Section 3.4, where as a robustness check, I examine rigidity in monthly salaries among non-hourly workers.

The SIPP 2008-13 gives, for each individual, a wage series of up to 16 periods.[16] As an illustration, suppose an individual is observed for eight periods within the same job,

[15]This procedure was first developed by Bai & Perron (1998, 2003) in time series literature and applied in the context of wage series by Gottschalk (2005).

[16]Note that for running the break test, an individual has to be observed within the same job for at least three periods.

Figure 1: Individual wage series and break test, an illustration



(a) Reported wage series (b) Within-job break test

Notes: This figure illustrates the methodology in Section 3.1. Sub-figure (a) illustrates a hypothetical wage series of a respondent who was observed in the same job for eight waves in the SIPP. The wage series shown contains noise, and if not corrected for measurement error, will record a wage change in each wave. Sub-figure (b) illustrates how the wage observed in the fourth wave is being tested for whether it is a structural break in the wage series, or just noise. If it is a structural break, it is assumed to divide the wage series into two subintervals with a constant wage in each, and a jump at t_4.

with a wage series as shown in Figure 1(a). The objective is to identify the periods when the change in wage is a structural break in the series, and the periods when the change is noise. The summary of the algorithm to do so is described in a sequence of steps as follows:

Step 1 Pick a period. Assume that period to be the date of structural break (call, break date) in the wage series. Recall from equation (1) that break date is that period when the constant wage changes. This means that the break date divides the wage series into two subintervals, with a constant wage (equal to the mean wage of that subinterval) and a jump at the break date.

This is illustrated in Figure 1 (b) where t_4 is a potential break date, dividing the wage series into subintervals $\{t_1, t_2, t_3\}$ and $\{t_4, ..., t_8\}$. The dashed lines represent the mean wage in each subinterval. The wage at t for such a wage series can be expressed as:

$$\begin{aligned} y_t &= \bar{y}_1 + u_t \quad t = 1, 2, 3 \\ &= \bar{y}_2 + u_t \quad t = 4, 5, 6, 7, 8 \end{aligned}$$

Step 2 Compute the sum of squared errors between the reported wage and the constant wage of each subinterval.

In Figure 1 (b) the sum of squared errors associated with t_4 will be the sum of squares of the solid lines from the reported wage to the mean wage of each subinterval.

Step 3 Recall that Steps 1 and 2 were for a given period. Now repeat these steps for all periods of the wage series to get sum of squared residuals associated with each period.

Step 4 Pick the period with the minimum sum of squared errors. This period is the most likely break date.[17] The F statistic associated with this date is given by $F = \frac{rss-uss}{\frac{uss}{l-2}}$, where l is the number of periods over which the break test is run, rss is the restricted sum of squared errors around a constant mean over all l periods, and uss is the unrestricted sum of squared errors around constant means before and after the potential break (computed in Step 2).[18]

For brevity, I illustrate t_4 as the most likely break date for the hypothetical wage series in Figure 1 (b). It corresponds to the highest F-statistic, or the least unrestricted sum of squared errors among all periods in the wage series $\{y_1, \ldots, y_8\}$.[19]

Step 5 Now test for the significance of the most likely break date.

The significance cannot be tested with the standard F-test or Wald test designed for testing breaks in a structural break test. This is because we are specifically testing for the significance of the break date corresponding to the least sum of squared errors or equivalently the maximum of a set of F statistics. Therefore, the critical value has to correspond to one of a maximum F statistic which will be greater than the one of a single F-statistic.

The method of computing the critical values relies on simulating data for a given number of periods (say l), for 10,000 individuals, with measurement errors around a constant wage with no breaks.[20] The structural break test described in Steps 1

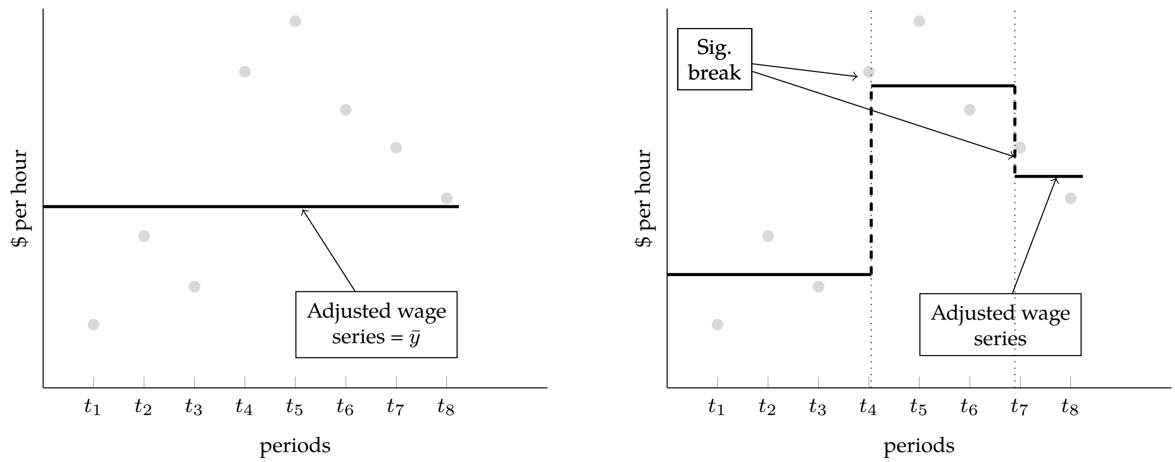
[17]Why is the date with minimum sum of squared errors the most likely break? Imagine a wage series with no measurement error, and only one break. Then the sum of squared errors corresponding to the break date would be zero. Therefore, the date with the least sum of squared errors is the most likely break date.

[18]Note for the restricted model, H_0: no structural breaks. For the unrestricted model, H_0: $\exists$ a structural break.

[19]Note, $\frac{\partial F}{\partial uss} < 0, \forall l > 2$ which is assumed, therefore least uss corresponds to maximum F statistic.

[20]The measurement error is assumed to follow AR(1) and detailed in Appendix A.1. The coefficients of the AR(1) process are taken from Gottschalk & Huynh (2010). They estimate a signal to noise ratio, and an autocorrelation of measurement error by matching the 1996 SIPP earnings records to uncapped W-2 earnings records. I assume that the structure of measurement error is constant across 1996 and 2008 panels. Although not shown, the results presented in this paper remain robust if I follow an alternative specification of coefficients estimated by Angrist & Krueger (1999) who match hourly wages from the CPS with employer records.

Figure 2: Test of significance, an illustration



(a) Most likely break insignificant

(b) Most likely break significant

Notes: This figure illustrates the methodology in Section 3.1. Sub-figure (a) illustrates the case where the most likely break, t_4, is insignificant. A constant wage equal to the mean wage is assigned as the adjusted wage series. Sub-figure (b) illustrates the case where t_4 is a significant break. Following this, the two subintervals are tested for more structural breaks and t_7 is found to be another significant break. This results in the adjusted wage series jumping at t_4 and t_7 and remaining constant otherwise.

through 4 is run, and the maximum F statistic for each individual wage series is computed. Assuming a significance level of α, the (1-α)th percentile of the maximum F distribution serves as the critical value for a given (l, α) tuple. This process is repeated for l = {3,...,16}.

In the illustration, the critical value for testing whether t_4 is a significant break in the wage series or not would be as follows: Simulate a large sample of constant wage series of eight periods. To each wage series, add noise. For each noisy wage series, run the structural breaks test and compute an F statistic associated with each of the eight periods. Obtain the maximum F statistic for each series. Assuming $\alpha = 5\%$, obtain the 95th percentile from the maximum F statistic distribution. This acts as the critical value for y_4 under the null that t_4 is not a significant break. In other words, if y_4 is larger than this critical value, we conclude that t_4 is a significant break.

Note, at this critical value, we are also falsely rejecting the null of no break (since the simulated wage series contains only measurement error and no breaks) α% of the time. Therefore, α represents the probability of type I error, i.e. falsely assigning a break when there is not one. Higher α will overestimate the true probability of wage change.

Step 6 Finally, as illustrated in Figure 2, once the break date has been tested for significance, two outcomes are possible:

One, the break date is insignificant as illustrated in Figure 2(a). This means that the most likely break date (with the maximum F statistic) is insignificant, and therefore, all other break dates (with a lower F statistic) will also be insignificant. In this case, the structural break test concludes that all observed changes in the reported wage series is noise and there is no structural break. A constant "adjusted" wage series, equal to the mean wage, is then assigned in place of the raw wage series.

Two, the break date is significant as illustrated in Figure 2(b) where t_4 is a significant break. In this case, the break date is a significant, discrete jump that divides the wage series into two subintervals. Now the algorithm for finding structural breaks is repeated on the subintervals, and the most likely break date within each subinterval is tested for significance. This process is repeated until one cannot find any more significant breaks. In the illustration, if t_4 is significant, then the break test is run on the two subintervals $\{t_1, t_2, t_3\}$ and $\{t_4, ..., t_8\}$. Whereas, no significant break is found in the former subinterval, t_7 is shown to be another significant break in the latter subinterval in Figure 2(b).

So far, following the algorithm described above gives an individual wage series with significant break dates. However, along each structural break test conducted, there is a probability of making type I and type II errors. As described above, type I errors leads one to falsely conclude that there is a break in the absence of one, thereby overestimating the true wage change. On the other hand, type II error leads one to falsely conclude that there is no break, when there is in fact one, thereby underestimating the true probability of wage change. This becomes problematic when one is aggregating the significant breaks in individual wage series to arrive at a population frequency of wage change. To circumvent this possibility of "overfitting or underfitting" breaks, BBG propose a scaling of the frequency of statistically significant breaks so as to correct for Type I and Type II errors. This transformed estimator serves as a consistent estimator of the probability of wage change for the population.[21] The transformation procedure is described in Appendix A.2.[22]

3.2 Main Results

This section explores nominal wage adjustments for workers who remain continuously employed within the same job. I start by documenting the wage change distributions

[21] The transformed estimator is therefore, adjusted for statistically significant breaks (after running the structural breaks test), and corrected for type I and type II errors.

[22] The validation of the BBG methodology has been assessed in Appendix A.4 using artificial data.

Figure 3: Self-reported wage changes (including measurement error)

(a) SIPP 1996-00

(b) SIPP 2008-13

Notes: This figure shows the log wage change distributions in 1996-00 and 2008-13 among hourly workers who stayed with the same employer. These distributions are based on self-reported wages, before applying the structural break test methodology. Wage freezes, or no wage change have been excluded from both the distributions to focus on nonzero wage changes. Each bin can be interpreted as a positive or negative wage growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

in 2008-13 and comparing them to their 1996-00 counterparts.[23] Figure 3 plots the self-reported wage growth distribution before correcting for measurement errors for the two panels. Each bin represents a wage change of 2.5%. The thin red line at zero indicates the workers who reported the same wages, across two or more waves, within the same job. This line has been truncated from the top so as to focus on the nonzero wage changes.

Due to the presence of measurement errors, the reported wage change distributions appear different from the ones that are typically observed in studies on wage rigidity that employ administrative and employer data. The latter are characterized by an unmistakable asymmetry, with a larger mass on positive wage changes. Whereas the 1996 panel distribution, in Figure 3(a), appears symmetric, the 2008 panel distribution, in Figure 3(b), appears slightly asymmetric. This may be because of the presence of dependent interviewing in the 2008 SIPP panel, that acted as a first level of correction for measurement errors.[24] Figure 4 describes the adjusted wage change distribution after applying the algorithm discussed in the previous section on individual wage series of the 2008 SIPP panel.

[23]Within-job wage rigidity is assessed for individuals who stay in the same job for at least two waves of the SIPP. Even though SIPP reports an employer ID of a worker that is supposed to be consistent through waves, there have been claims of erroneous coding of employer ID that are highlighted in Gertler, Huckfeldt & Trigari (2016). Therefore, instead of using employer ID, I rely on tracking the start date of a job. If the respondent reports the same start date for his/her primary job through two or more waves, then he/she is

Figure 4: Adjusted wage changes after applying structural break test

(a) SIPP 1996-00

(b) SIPP 2008-13

Notes: Sub-figure (a) is originally estimated in Barattieri et al. (2014). This figure shows the log wage change distributions in 1996-00 and 2008-13 among hourly workers who stayed with the same employer. These distributions are based on adjusted wages, after applying the structural break test methodology. Wage freezes, or no wage change have been excluded from both the distributions to focus on nonzero wage changes. Each bin can be interpreted as a positive or negative wage growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

For reference, the distribution in BBG for 1996-00 SIPP is also reproduced. Comparing Figure 4 to the raw wage change distributions in Figure 3, it is clear that negative wage changes get penalized more heavily while applying the methodology. This is consistent with prior literature that finds evidence that wage cuts are often overstated in self-reported wages (Altonji & Devereux 2000, Dickens et al. 2007, Gottschalk 2005).

Both distributions of Figure 4 demonstrate the typical features of wage change distributions that characterize wage rigidity literature: the asymmetric shape, the disproportionately higher positive changes than negative changes, and a spike at zero (which is truncated). At the same time, the distributions finds certain other features, that are difficult to detect in prior work that does not correct for measurement error: One, the distribution in 2008-13 is tighter than that of 1996-00, due to a relatively high mass at low wage growth rates (standard deviation of nonzero adjusted wage growth distribution fell from 0.27 in the 1996 panel to 0.22 in the 2008 panel). This could reflect the slowdown in aggregate

coded to be in the same job.

[24]Dependent interviewing, first introduced in the SIPP 2004 panel, is a procedure adopted to improve the data quality (Moore 2008). If respondents indicated that their hourly wages were unchanged from the previous interview, their response from the previous interview was filled in the current interview. Likewise, responses from the previous interviews are used to query any inconsistencies.

wage growth rate that started from the early 2000s and became especially pronounced during the Great Recession. Thus, one would expect a higher fraction of individuals experiencing low wage growth rates, and a lower fraction experiencing high wage growth rates within the same job.[25]

Two, there is a relatively lower mass on very small wage changes. This is evident in Figure 4 from comparing the bins immediately to the next of zero, to the ones in their vicinity. This lower mass could represent an adjustment cost to firms for changing wages of existing workers. These costs are analogous to "menu costs" found in price rigidity literature, that generate a region of no-action near the optimal wage.[26] If no-action regions are large, wage changes are infrequent, and large when they occur. Grigsby, Hurst & Yildirmaz (2019) also find evidence of a relative lower mass around zero in 2009-16. The SIPP data set gives the added benefit of comparing this lower mass on small wage changes across the two panels. In Figure 4(b), there is a spike in the mass immediately to the right of zero compared to Figure 4(a). This reflects an increasing fraction of job stayers experienced very small wage increases or a shrinking of the no-action region in 2008-13, relative to 1996-00. This can be interpreted as an indicator of falling wage rigidity in 2008-13.[27]

Formally, Table 2 (a) computes the frequency of wage adjustment for hourly workers after applying the structural break test, and subsequent correction procedure for type I and type II errors.[28] The quarterly frequency of adjusted and corrected wage adjustment, an indicator of wage flexibility, is 24.9% in 2008-13 compared to 16.3% in 1996-00 for hourly workers. This provides evidence that the incidence of wage flexibility has increased over the two periods, even though overall, wages within-jobs are still more rigid than flexible.[29]

[25] This compression of wage growth is also found in Elsby (2009), Stüber & Beissinger (2012).

[26] The no-action region represents a range of wages around the optimal wage. Due to high adjustment costs, firms do not change their wages, so long as they are within the range around the optimal wage.

[27] This point can be explained by way of a stark example. Suppose a worker's hourly wage is $100, and the annual wage growth rate is 10% in 1996 and 2% in 2008. Let us begin by assuming that the firm's no-action band is [$95,$105] and stays the same over the two periods. Then, in 1997 the firm will revise the wage of the worker upwards to $110, and in 2009 the firm will keep the wage at $100. Now the results in Table 2 discussed below indicate that the frequency of wage adjustment increased from 1996 to 2008. This could only happen if the no-action band changed over the two periods. In particular, the band would have to shrink to [$98,$102] to accommodate the 2% wage change.

[28] Table 2 reports bootstrapped standard errors. Note, Section 3.1, Step 5 discusses simulating data with measurement errors using a random draw, for getting critical values to test for the significance of a break. Different random draws generate different measurement errors and hence, critical values. Different critical values generate a distribution of each of the point estimates computed in Table 2. Thus, replicating over random draws of measurement errors provides the necessary variation needed for computing standard errors. The bootstrapping procedure is computationally intensive and I provide standard errors based on 100 replications in this version of the paper.

[29] Grigsby, Hurst & Yildirmaz (2019) compute the quarterly frequency of wage change for hourly workers to be close to 20.5% for 2009-16. Other papers in this literature express this probability in annual terms, preventing a direct comparison.

Table 2: Frequency of wage adjustment(%)

	(a) Within Job				(b) Between Jobs		
	Reported	Adjusted Total	Adjusted $\frac{\Delta w<0}{\Delta w\neq 0}$	Adjusted +Corrected	Reported	Adjusted Total	Adjusted $\frac{\Delta w<0}{\Delta w\neq 0}$
(i) 1996-2000							
Hourly	53.1	8.4	12.3	16.3	87.7	96.4	26.5
		(0.0020)	(0.0052)	(0.0010)		(0.0025)	(0.0019)
Salaried	65.4	3.0	24.5	14.0	96.4	99.7	33.8
		(0.0009)	(0.0059)	(0.0494)		(0.0002)	(0.0008)
(ii) 2008-2013							
Hourly	30.6	14.6	14.2	24.9	84.9	90.1	36.9
		(0.0022)	(0.0023)	(0.0031)		(0.0015)	(0.0009)
Recession	32.1	14.8	21.4	25.4	83.5	87.6	45.7
		(0.0022)	(0.0030)	(0.0028)		(0.0019)	(0.0016)
Recovery	28.9	13.9	12.3	23.1	85.0	90.2	36.4
		(0.0000)	(0.0018)	(0.0000)		(0.0014)	(0.0009)
Salaried	34.7	10.1	26.2	21.1	94.4	96.8	39.2
		(0.0017)	(0.0018)	(0.0047)		(0.0003)	(0.0006)
Recession	36.4	10.4	42.1	22.4	93.8	96.5	45.6
		(0.0017)	(0.0020)	(0.0067)		(0.0009)	(0.0025)
Recovery	32.5	9.6	21.2	18.8	94.4	96.8	39.0
		(0.0017)	(0.0012)	(0.0052)		(0.0003)	(0.0007)

Notes: Panel (a) reports quarterly self-reported, adjusted and adjusted+corrected (for type I & II errors) frequency of within-job wage changes. The 1996-00 hourly values were originally estimated in Barattieri et al. (2014). Panel (b) reports self-reported and adjusted frequency of wage changes among job-switchers. Both panels include wage cuts as a fraction of nonzero wage changes which represent the inner 98^{th} percentile of the wage change distribution. The bottom panel for 2008-13 is divided into Recession (2008-10) and Recovery (2011-13) to ascertain cyclical variation in the frequency of wage adjustment. Bootstrapped standard errors are reported in parenthesis.

Further, the quarterly frequency of reported wage changes is lower in 2008-13 (30.6%) than 1996-00 (53.1%). This reflects the same correction of measurement error that led to differences in self-reported wage change distributions observed over the two periods in Figure 3.[30]

3.3 Cyclical variation over the 2008 panel

The comparison between 1996-00 and 2008-13 is one between a large boom and a large recession followed by recovery. It may not be a suitable comparison to ascertain the state-dependence of wage adjustment as there were important changes that took place in the

[30] As mentioned earlier, this could be because of the introduction of dependent interviewing in the 2008 panel of the SIPP, that was absent in the 1996 panel.

Figure 5: Business cycle variation in adjusted wage changes

Notes: This figure shows the log wage change distributions among hourly workers who stayed with the same employer over the Great Recession, and subsequent recovery. These distributions are based on adjusted wages, after applying the structural break test methodology. Wage freezes, or no wage change have been excluded from both the distributions to focus on nonzero wage changes. Each bin can be interpreted as a positive or negative wage growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

US labor market across the two periods. To provide evidence on the state-dependence of wage rigidity, this paper estimates the distribution and frequency of wage adjustment during the Great recession (2008:08-2010:12) and the subsequent recovery (2011:1-2013:11) using the 2008-13 panel.

To estimate state-dependence of within-job wage flexibility, the iterative break tests are first run over the entire 2008-13 SIPP sample.[31] Next, there are two alternatives to arriving at state-dependence. One, to compute the adjusted probability of wage change for the full sample, and average it over quintiles of the size of wage growth (i.e. wage growth at significant break points), the number of periods over which the break tests were carried out, separately for recession and recovery.[32] However, this approach assumes that the size

[31]The sample is bifurcated into recession and recovery *after* doing the individual break tests (instead of *before*) so as to not reduce the sample size, which affects the quality of the iterative procedure.

[32]This approach is analogous to the one taken in BBG to examine heterogeneity in wage rigidity.

of wage growth is constant over the 2008-13 panel. In other words, the size of wage change, conditional on a wage change taking place, is assumed to be state-independent. To relax this assumption, I propose an alternative approach that makes the size of wage growth a function of the business cycle. After running the individual within-job break tests, the sample is divided into periods of recession and recovery. The sizes of wage change are computed for each of the two states separately, so as to allow them to vary over the business cycle. Finally, the corrected frequency of within-job wage change is aggregated for each of the two states individually. This method is explained in more detail in Appendix A.3.

Figure 5 plots the distributions of wage changes across recession and recovery within 2008-13. One observation is immediately apparent - conditional on observing a wage change, the propensity of a wage cut is higher during recession (21.4%) than recovery (12.3%).[33] Next, I formally compute the corrected and adjusted wage change statistic. Table 2(a)(ii) reports the quarterly frequency of within-job wage adjustment during the business cycle. Two observations are noteworthy: One, comparing the periods of 1996-00 and the recent recovery (2011-13), the frequency of wage adjustment is higher during the latter recovery (23.1%) than the former boom (16.3%). This acts as a robustness check against the argument that the period of Great Recession (2008-10) was what drove average the frequency of wage adjustment upwards over the entire 2008 panel relative to the 1996 panel. Two, the same frequency of wage adjustment over recovery (23.1%) is lower by a few percentage points than that over the Great Recession (25.4%).

A mild increase in nominal wage rigidity during the recent recovery compared to the recession has also been observed by Kurmann & McEntarfer (2019), Daly & Hobijn (2014) and Jo (2019).[34] There are two potential explanations put forth: One, if wage cuts during recession are affecting the non-base component of wages more, then they may only provide a relatively small reduction in total compensation. Therefore, they are compensated by smaller raises which leads to a higher incidence of wage freezes during recovery. This explanation may not apply to hourly workers observed in the SIPP who directly report hourly pay, as this measure is less likely to include non-base pay components. Two, Daly & Hobijn (2014) put forth an explanation of "pent up wage deflation" that suggests that

[33]Note, the plot presented in Figure 5 has been computed after running the structural breaks test and before correcting for measurement errors. I show the wage changes in this plot during the Great Recession and recovery based on their NBER dates. In the correction procedure to follow, I need a larger sample size, and therefore, divide the sample into recession (2008-10) and recovery (2011-13).

[34]Jo (2019) finds that, controlling for inflation, the share of workers with no wage changes as well as those with wage cuts appear counter-cyclical, with relative changes in the former being larger. There are two points worth noting: One, the author aggregates microdata from the CPS to arrive at cyclical fluctuations in wage rigidity. Two, a more subtle point which also concerns this paper is that the author find that wage rigidity peaked during the recent recovery before falling.

the downward rigidity during the Great Recession created a backlog of pent up wage cuts that held down wage increases that were to happen during recovery. This explanation is based on their observation that wage rigidity increased during the Great Recession, relative to the years preceding it. However, due to the unavailability of sufficiently long panels of SIPP in the years preceding 2008, I am unable test their claim.[35]

An alternative explanation for the decline in nominal wage flexibility during recovery may lie in the behavior of inflation, which was negative by the end of the Great Recession and may have kept real wages the same or higher, even in the presence of nominal wage cuts. This may have led firms to freeze wages during recovery, as inflation began to recover, in an attempt to bring real wages back to their optimal level. As a way of testing this argument, I could estimate the frequency of real wage changes over the business cycle. If this explanation is true, then real wages should exhibit flexibility during recovery. In particular, there should be a higher incidence of real wage cuts, as a higher proportion of nominal wages display rigidity, and inflation recovers during recovery. I leave this test for a future version of this paper.

3.4 Robustness

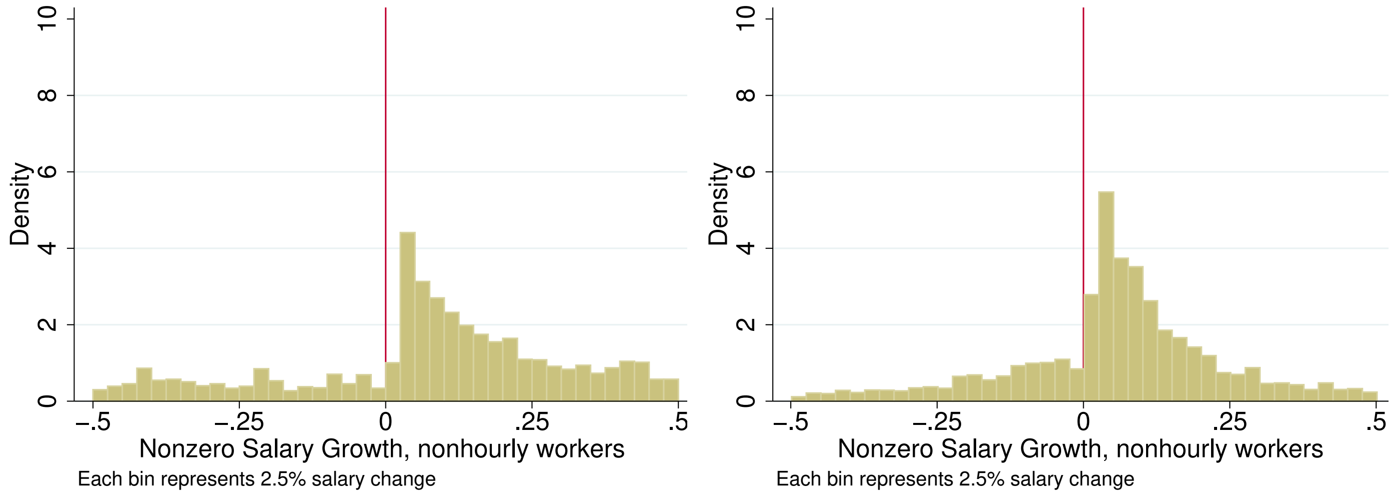
As a robustness check, I also estimate nominal salary adjustments among non-hourly workers. I refer to non-hourly workers as those who did not work in an hourly job throughout their SIPP tenure. I call these workers "salaried" to distinguish them from hourly workers. The SIPP reports monthly salary for all salaried workers, and hourly pay for a fraction of salaried workers. In order to compute an hourly salary for the remaining fraction, the measure of monthly salary could be divided by the hours worked in a month. However, hours worked on a job are not measured accurately in the SIPP.[36] Therefore, this paper reports adjustments in monthly salary.

Figure 6 reports the adjusted monthly salary change distribution among nonhourly workers for the 1996 and 2008 panels of the SIPP. The patterns that were apparent in the adjusted wage change distributions of hourly workers in Figure 4 are also observed for

[35]I report evidence of a fall in wage rigidity in the 2004 panel relative to the 2008 panel in Appendix A.5. However, the shorter length of the former panel may make the estimates of wage rigidity imprecise.

[36]Hours worked per month = weeks worked/month $\times$ hours worked/week. The SIPP does not report weeks worked by job, and instead reports total weeks worked on all jobs. Therefore, the weeks worked on primary job can only be ascertained by dropping all workers with multiple jobs. Further, appx. 30% of all workers report "varying hours" worked per week on a job. I drop all workers with multiple jobs and varying hours, to compute a measure of hourly salary = $\frac{\text{monthly earnings}}{\text{hours worker per month}}$ for all salaried workers. However, the resulting hourly salary does not match the hourly pay for the fraction of salaried workers who directly report it. Therefore, I refrain from using an indirectly computed measure of hourly salary.

Figure 6: Adjusted salary changes for non-hourly workers



(a) SIPP 1996-00

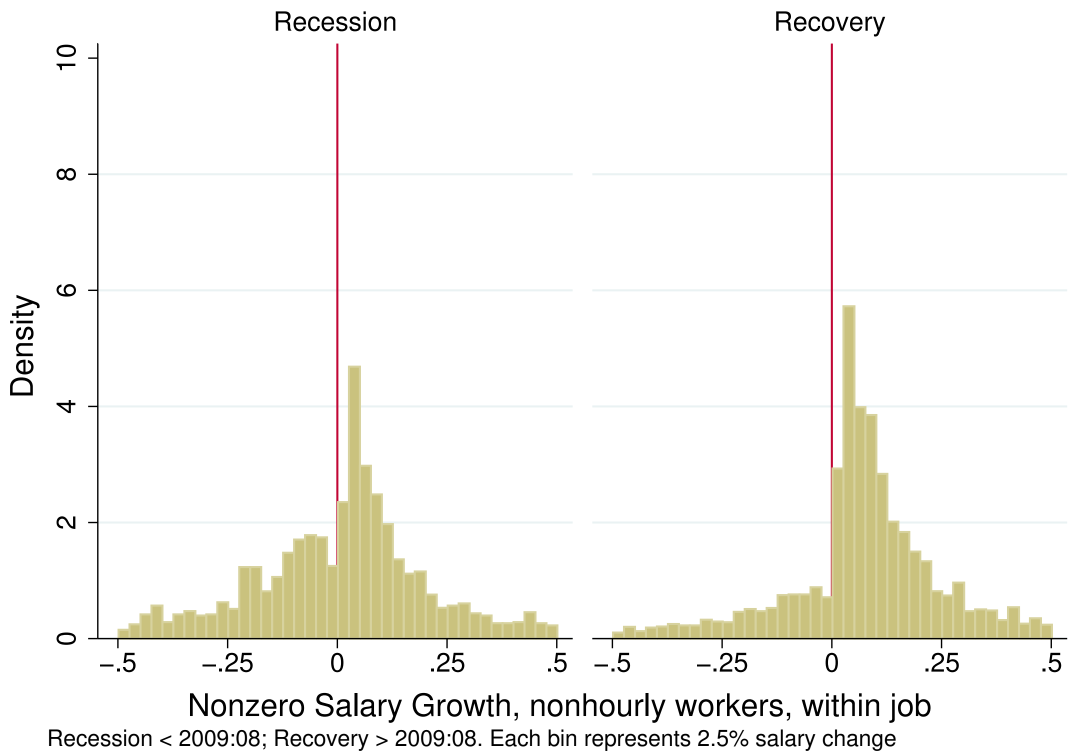
(b) SIPP 2008-13

Notes: This figure shows the log monthly salary change distributions among nonhourly workers who stayed with the same employer in 1996-00 and 2008-13. These distributions are based on adjusted salaries, after applying the structural break test methodology. Salary freezes, or no salary change have been excluded from both the distributions to focus on nonzero salary changes. Each bin can be interpreted as a positive or negative salary growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

their salaried counterparts. The key observation is that the mass on high positive salary growth during 1996-00 seems to have shifted to lower salary growth in 2008-13. This is true, albeit to a lesser extent, for negative salary growth too. Overall, as noted in Table 2, in the 1996 panel, salary cuts comprised 24.5% of all nonzero salary changes which increased to 26.2% in the 2008 panel. An increase of similar magnitude was seen for hourly workers. Further, the asymmetry that characterized the wage change distribution, with an overwhelming majority of changes being positive, is also observed in the salary change distribution. Finally, as with hourly workers, there is missing mass of very small salary changes, with a spike in the bin immediately next to zero on the positive side in the 2008 panel.

More formally, Table 2 also reports the overall adjusted and corrected frequency of within-job salary adjustment in 2008-13 (21.1%) and 1996-00 (14.0%). The overall level of salary adjustments seems to be a little lower compared to hourly wage adjustments in both periods. Grigsby, Hurst & Yildirmaz (2019) also report a slightly lower fraction of salaried workers reporting hourly pay changes (18% in their sample for 2008-16), compared to hourly workers (20.5% in their sample). At the same time, it is important to remember that the pay-periods of receiving salaries and wages are different (month and

Figure 7: Business cycle variation in adjusted salary changes



Notes: This figure shows the log monthly salary change distributions among nonhourly workers who stayed with the same employer over the Great Recession, and subsequent recovery. These distributions are based on adjusted salaries, after applying the structural break test methodology. Salary freezes, or no salary change have been excluded from both the distributions to focus on nonzero salary changes. Each bin can be interpreted as a positive or negative salary growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

hour, respectively) in the SIPP, which may account for this difference.[37] Overall, the adjusted and corrected quarterly frequency of pay changes for salaried workers is higher in 2008-13 compared to 1996-00, which reflects salary changes becoming more flexible over time. Therefore, this pattern of nominal pay adjustment seems to be robust across hourly and non-hourly workers.

Figure 7 reports the cyclical variation in salary adjustments over 2008-13. Salary cuts, as a proportion of all nonzero salary changes, were twice as high during the Great Recession (42.1%) than the subsequent recovery (21.2%). Formally, Table 2 (a) reports the business cycle differences in nominal salary adjustments for job-stayers. The frequency of salary adjustments is higher during the Great Recession (22.4%) than the recovery (18.8%).

[37] The relevant point of comparison between hourly and salaried workers is therefore, in the change in frequency of adjustment across the two panels.

Therefore, the two patterns observed during the Great Recession: of a spike in propensity of pay cut conditional on pay change taking place, and an increase in frequency of adjustments, are robust across hourly and non-hourly workers.

4 Between job wage change

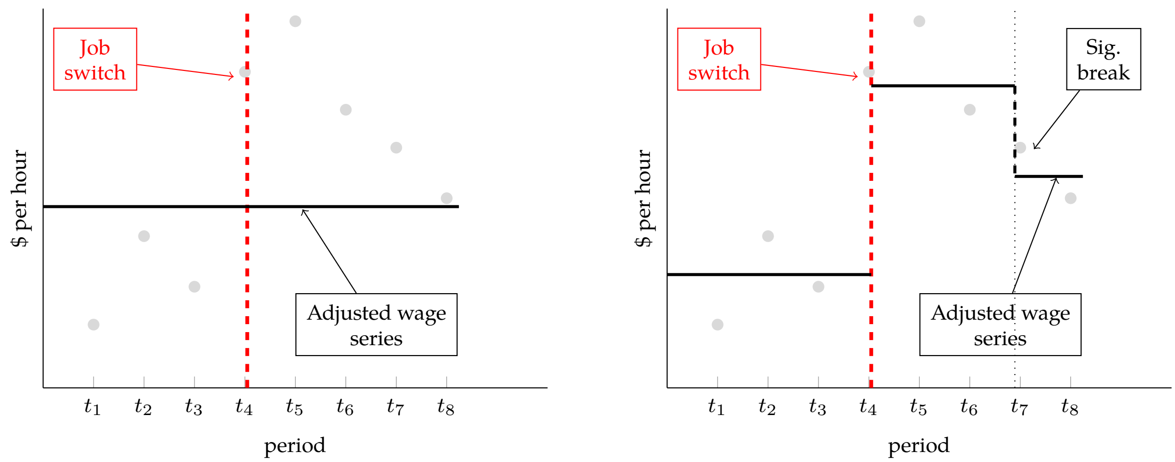
4.1 Methodology

Most studies of downward nominal wage rigidities focus on wage changes of those who remain in the same job (Daly & Hobijn 2014, Elsby et al. 2016, Jardim et al. 2019, Kurmann & McEntarfer 2019). This is because it is typical and common to experience a wage change at the time of switching jobs. However, for models of labor search and matching in which wage changes as a result of workers moving across firms, or due to workers getting outside offers, the question of wage adjustments between-jobs is equally crucial. More recently, Grigsby, Hurst & Yildirmaz (2019) is the only study that documents between-job wage rigidity using administrative data from a large payroll processing company in the US. However, their between-job worker sample is limited to workers who make a job-switch from one firm captured in their sample (that is not nationally representative) to another. They implicitly assume that the patterns of nominal wage adjustments for workers switching across firms within their sample would be similar to the patterns that would emerge if workers switched to a firm not captured by their sample. In this paper, the SIPP follows job-switchers throughout the panel, thereby maintaining representability among them. This section therefore, focuses on documenting wage change among job switchers as a way to examine how between-job wage rigidity has changed over time and to serve as a robustness check for Grigsby et al. (2019).

In order to arrive at a frequency of between-job wage adjustment, I deviate from the BBG methodology. In a nutshell, their methodology involves redoing the structural break tests discussed in Section 3.1, while feeding into the algorithm the periods of job changes as structural breaks. The resulting breaks in job-changing periods are then tested for significance. However, now the critical values associated with job changes do not correspond to the maximum of F statistics that was formerly used, as the job change dates may not be the most likely break dates.

To compute the critical values corresponding to job change, BBG simulate wage series of 10,000 individuals for a given number of periods (call l), with measurement errors and no break, run the structural break test, and take the F-statistic of the median observation of each individual wage series. The median F for each individual is then ranked, and the 95th

Figure 8: Between-job wage changes, an illustration



(a) Between job $\Delta w = 0$

(b) Between job $\Delta w \neq 0$

Notes: This figure illustrates the methodology in Section 4.1 where the respondent reports a job change at t_4. Sub-figure (a) illustrates the case where the adjusted wage before and after the job change does not change. In this case, the job change is classified as one without a wage change. In Sub-figure (b) the adjusted wage is higher in the new job, classifying the job change at one with a positive wage change.

percentile is taken as the critical value for a given (α, l) tuple. To test for the significance of the F-statistic corresponding to the job-switch, it is compared with this critical value corresponding the the median F statistic. This paper deviates from this method because the assumption that a job change occurs at the median of an individual's SIPP tenure may not be plausible. For the 2008 panel of the SIPP, a job change occurs at the median date of the respondent's SIPP tenure only 18.2% of the times.[38]

Here, an alternative approach from BBG is proposed, and illustrated in Figure 8.

Step 1 Conduct the tests for within-job structural breaks and arrive at an adjusted individual wage series within each job, as described in Section 3.1.

Step 2 Identify the job change. This is illustrated in Figure 8 where t_4 is now identified as the period when the respondent switched jobs. Then the steps in Section 3.1 give us within-job adjusted wage series for periods $\{t_1, ..t_3\}$ and $\{t_4, ..., t_8\}$.

Step 3 The adjusted wage before and after the date of job change is compared. If there is no change in the adjusted wages, then that job switch is assigned as one without a wage change. If the adjusted wages differ, the job change is associated with a positive

[38]The frequency of between-job wage change computed in BBG (2014) amounts to 69.1% for the 1996-00 SIPP, and a similar statistic is obtained for SIPP 2008-13 following their methodology. However, this statistic underestimates the frequency of between jobs wage adjustment that is found in other studies in the literature trying to compute the same.

Figure 9: Adjusted wage changes, between-jobs

(a) SIPP 1996-00 (b) SIPP 2008-13

Notes: This figure shows the log wage change distributions in 1996-00 and 2008-13 among hourly workers who switched jobs. These distributions are based on adjusted wages, after applying the methodology discussed in Section 4.1. Wage freezes, or no wage change are included in both the distributions. Each bin can be interpreted as a positive or negative wage growth of 2.5%. The inner 98 percentiles of the distributions are plotted.

or negative wage change. In Figure 8 (a) and (b), the job switch at t_4 is one with no wage change, and a positive wage change, respectively.[39]

4.2 Main Results

Figure 9 (a) and (b) plot the distribution of between-job wage adjustment in 1996-00 and 2008-13. These distributions appear strikingly different from their within-job counterparts. The following observations are apparent: One, as expected there is much more wage adjustment for job-changers than job-stayers. These distributions include a spike at zero, which is much smaller than the truncated spike for job-stayers observed in Figure 4. Two, the proportion of wage cuts is much higher between-jobs than with-in jobs. Specifically, between-job wage cuts accounted for 36.9% of all wage changes among hourly workers in 2008-13. The distribution of 2008-13, as well as the proportion of wage cuts bear similarity to the one documented by Grigsby et al. (2019). Three, the 2008-13 distribution

[39]Note, this method does not correct for Type I and Type II errors, and if the within-job adjusted wage before or after the job switch is affected by these errors, then this method may not be reliable. At the same time, this method is biased towards getting a high frequency of wage adjustment, and high wage flexibility between-jobs is more consistent with literature.

has a larger mass on the negative side than the 1996-00 distribution. As reported in Table 2(b) , in 1996-00 the corresponding proportion of wage cuts as fraction of total wage changes was 26.5%. Finally, the spike at zero is lesser in 1996-00 compared to 2008-13, reflecting a marginal rise in wage freezes among job-switchers over time. Wages between-jobs still remain more flexible than rigid.

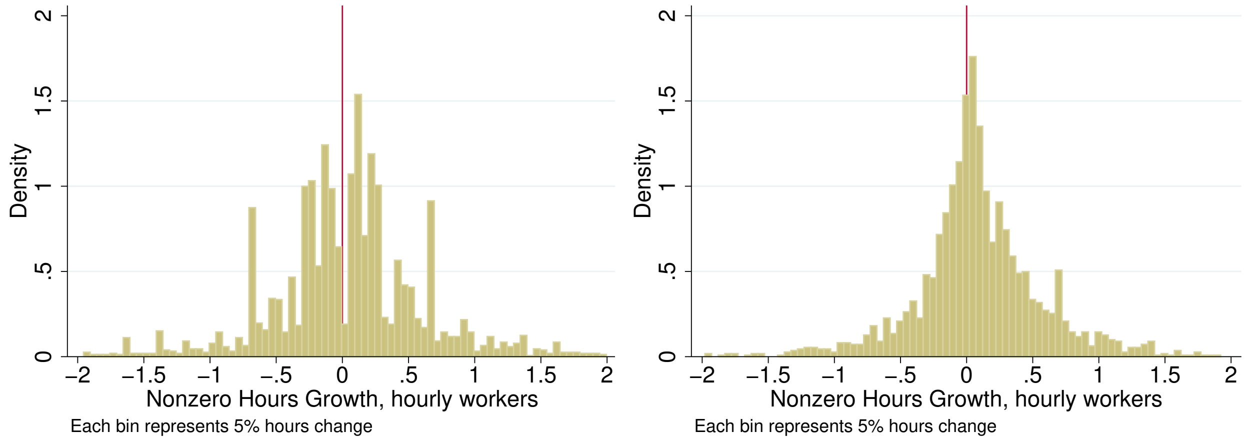
Formally, Table 2 (b) also documents the frequency of wage adjustment among job switchers across the two SIPP panels for hourly and salaried workers. As seen in Figure 9, essentially all job switchers receive a pay change. Further, the proportion of switchers receiving a pay cut conditional on a pay change was higher during the recent recession than the subsequent recovery.

5 Hours flexibility

As a final application of the methodology discussed in this paper, I attempt to examine downward flexibility in hours. Much like wages, hours reported are subject to measurement errors. Bound, Brown & Mathiowetz (2001) review a number of empirical studies concerning the quality of self-reported hours. They document a common finding: interview respondents overestimate the number of hours worked. I attempt to use the structural breaks tests to purge the impact of measurement error in hours. I use the same methodology discussed in Section 3.1 with one difference. While running Monte Carlo simulations to obtain critical values, I use estimates of the structure of measurement error in hours reported in Angrist & Krueger (1999). This study matches reported hours worked in the CPS with employer records and defines measurement error as the difference between the two. Their matched records are used to estimate the variance of employer recorded hours (signal) and the variance of the difference between true and reported hours (noise). The resulting signal to noise ratio in hours is almost twice as high as the one they report for hourly wages. The structure of measurement error is detailed in Appendix A.1.

Figures 10 (a) and (b) document preliminary evidence of nonzero changes in average weekly hours worked in the 2008 SIPP for job stayers and job switchers respectively. I exclude workers who report overtime hours (i.e. greater than 40 hours per week). Panel (a) shows that the within-job hours distribution exhibits spikes. This may be an artifact of rounding error which is much more prevalent in hours data. Overall, hours distribution does not seem to demonstrate rigidity in either direction, with an almost equal number of positive and negative changes. Panel (b) reports hours adjustment distribution, conditional on job switching. The distribution is concentrated around smaller hours changes and appears symmetric. Negative hour adjustments account for 40.6% of all nonzero

Figure 10: Adjusted hours changes for non-hourly workers



(a) SIPP 2008-13: Within-job

(b) SIPP 2008-13: Between-job

Notes: This figure shows the log hours change distributions among hourly workers who stayed with the same employer (sub-figure a), and switched jobs (sub-figure b). These distributions are based on adjusted hours, after applying the structural break test methodology. Hour freezes, or no hours change have been excluded from both the distributions to focus on nonzero hour changes. Each bin can be interpreted as a positive or negative hours growth of 5%. The inner 98 percentiles of the distributions are plotted.

changes. Finally, although not shown here, I preliminarily compute the frequency of hours adjustment by applying the structural breaks tests methodology and correction procedure on hours data for the SIPP panels. Within-job hours worked are still more rigid than flexible, but over the two rounds, the frequency of adjustment seems to have mildly increased. I compute the adjusted and corrected quarterly frequency of hours change to be 7.7% in 1996-00 and 11% in 2008-13. The extent and change in rigidity of hours worked is still an open question in literature, and a fruitful area of future research for this project.

6 Conclusion

This paper measures nominal wage adjustments using the SIPP 2008 panel to ascertain how wage rigidity has changed from the 1996 panel. It corrects for measurement error in self-reported wages to provide a distribution and frequency of nominal wage adjustments. This paper documents two stylized facts about the US labor market in the recent decades: One, even though overall, within-job wages are still more rigid than flexible, there has been a relative increase in the frequency of nominal wage adjustments from 1996-00 to 2008-13. Two, conditional on these wage adjustments taking place, the propensity of nominal wage cuts was much higher during the Great Recession than the subsequent recovery. These findings are robust for hourly and nonhourly workers.

The first fact is in line with literature that has documented an increase in volatility of wages along with the intensive and extensive margins in the recent decades. This has been attributed to a weakening of worker bargaining power and an increasing demand for flexible labor. The second fact adds to the debate on the state-dependence of wage adjustments, bringing more evidence to the side of the debate that argues in favor of an increase in wage flexibility during recessions.

I close by noting that, by only examining the extent of nominal wage rigidity over the two SIPP panels, this paper does not give a complete picture of how it has evolved from the 90s. The main setback of using the SIPP dataset is the absence of information between the two panels. Given the non-availability of administrative data sources dating back to the 1990s, the question of evolution therefore, becomes a theoretical one. I leave that as an area of future research for this project.

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A Appendix

A.1 Structure of Measurement Error

The measurement error is assumed to be nonclassical. Bound, Brown & Mathiowetz (2001) show that measurement error in earnings is mean reverting, with persons with low earnings overstating their income, and vice versa. Let measurement error be denoted by v. Let us assume it follows an AR(1) process, with ρ expressing the autocorrelation, and e_t being noise:

$$v_t = \rho v_{t-1} + e_t$$

s.t. $e_t \sim N(0, \sigma_e^2)$ & $v_0 \sim N(0, \sigma_v^2)$. Then can show that,

$$\sigma_v^2 = \frac{\sigma_e^2}{1-\rho^2} \implies \sigma_e^2 = \sigma_v^2(1-\rho^2)$$

Given σ_v^2 and ρ from Gottschalk & Huynh (2010), can back out v_t.

A.2 Correction for Type I and Type II Errors

After doing the structural breaks tests on individual wage series through the iterative procedure described in Section 3.1, we want to aggregate the data to compute a population estimate of the frequency of wage change. However, simply tabulating the frequency of breaks does not give us a consistent estimator.

Formally, let the true probability of wage change be π, and the frequency of significant breaks in wages be denoted by $\hat{\pi}$. Without type I and type II errors $\hat{\pi}$ would be a consistent estimator of π, i.e. $p\lim(\hat{\pi}) = \pi$. However, in the presence of type I and type II errors this would not be the case. To explain, let the break tests be conducted P times on the SIPP panel. Out of these, on average, $P\pi$ tests were conducted in periods when the true wage changed, and $P(1-\pi)$ in periods when the wages did not change. To compute the average frequency of breaks detected we have to factor in:

- Probability of making type I error, α. This refers to the probability of falsely rejecting the null of no break (or accepting the alternate that there is a break) when there is in fact no break. This means $\alpha P(1-\pi)$ of the tests with no wage change would be falsely classified as significant breaks.
- Probability of making type II error, β. This refers to the probability of falsely accepting the null of no breaks (or rejecting the alternate that there is a break) when there is in fact a break. This means out of the $P\pi$ wage changes, only $(1-\beta)P\pi$ would

be correctly detected as significant breaks, and $\beta P\pi$ would be falsely rejected as no breaks.

The net impact of type I and type II errors on the average frequency of breaks would be:

$$p\lim(\hat{\pi}) = \frac{\alpha P(1-\pi) + (1-\beta)P\pi}{P} = \alpha + ((1-\beta)-\alpha)\pi$$

Denote power of the test, $(1-\beta) = \gamma$, then the last line implies: $p\lim\left[\frac{\hat{\pi}-\alpha}{\gamma-\alpha}\right] = \pi$. Defining $\tilde{\pi} = \frac{\hat{\pi}-\alpha}{\gamma-\alpha}$ therefore proves that $\tilde{\pi}$ is a consistent estimator of the true probability of wage change, π. $\tilde{\pi}$ is the adjusted+corrected probability documented in Table 2.

The next natural question is, how to compute γ or the power of the test? For this, BBG undertake the following steps:

Fix the number of periods, $l = (3, 4, ..., 16)$ and α = 5%.

- Simulate 10,000 individual wage series, and assign a measurement error to each.
- Assign to each series a randomly selected break date.
- To each break date, assign a break of size equal to the median wage change of a given quintile of the adjusted wage change distribution from the SIPP panel.[40]
- Apply structural break test to this new simulated wage series with breaks and compute maximum F of each series.
- Test for the significance of maximum F using the critical values computed in Step 5 of Section 3.1.
- Recall that in this simulated wage series with breaks, the null hypothesis is in fact false. Then, the probability of making type II errors is the average number of times the maximum F statistic is insignificant (proportion of times we falsely accept null of no breaks in a wage series with breaks). The power of test is the average number of times the maximum F statistic is significant (proportion of times we reject null of no breaks when it is false).
- Therefore, power of test can be computed for a given l, α and quintile of the size of wage change.

In order to go from micro data on individual wage series to $\tilde{\pi}$, $\hat{\pi}$ are averaged over l and break size (i.e. the quintile of the size of wage change), so as to ensure: $\tilde{\pi}(l, \text{break size}) = \frac{\hat{\pi}(l, \text{break size}) - \alpha}{\gamma(l, \text{break size}) - \alpha}$ where α is assumed to be constant at 5%. Finally $\tilde{\pi}(l, \text{break size})$ is aggregated over $(l, \text{break size})$.

[40]i.e. take an absolute of the wage change distributions shown in Figure 4. Divide it into five quintiles. Take the median wage change of each quintile.

A.3 Method of computing state-dependent frequency of wage adjustment

To compute how the frequency of wage adjustment differed across the business cycle, I first run the within-job break test discussed in Section 3.1 over the entire sample. This gives me wage series with breaks for all individuals. Notice that I do not divide the sample into recession and recovery before doing this step. This is because the quality of structural breaks test relies on the number of waves over which it is tested. Therefore, I do not divide the number of waves before this step.

The goal is to now aggregate this series, and do so separately for the Great Recession and Recovery. I define the period of recession as 2008:08-2010:12, and recovery as 2011:01-2013:11. I choose these time periods because they roughly divide the sample periods into half, and maintain a large number of observations over both states. Now, I am faced with two options of aggregating the frequency over the two states:

- Option I: Average $\hat{\pi}$, the frequency of significant breaks, over l and break size (i.e. the quintile of the size of wage change) seperately for the two states. This would ensure: $\tilde{\pi}(l, \text{break size}, \text{state}) = \frac{\hat{\pi}(l, \text{break size}, \text{state}) - \alpha}{\gamma(l, \text{break size}) - \alpha}$. Then, $\tilde{\pi}(l, \text{break size}, \text{state})$ can be aggregated over $(l, \text{break size})$ separately over the two states. The problem with this alternative is that it assumes break sizes are state-independent. To relax this assumption, I follow another alternative.

- Option II: After running the breaks test, I divide the sample into recession and recovery. I then compute break sizes separately for the two samples, making it state-dependent. On the basis of these break sizes, I compute power. I then compute $\tilde{\pi}$: $\tilde{\pi}(l, \text{break size}, \text{state}) = \frac{\hat{\pi}(l, \text{break size(state)}) - \alpha}{\gamma(l, \text{break size(state)}) - \alpha}$. Finally, I aggregate $\tilde{\pi}(l, \text{break size}, \text{state})$ over $(l, \text{break size})$, separately over the two states.

A.4 Validating BBG methodology using Monte Carlo simulations

To understand how the estimator of wage flexibility performs, this section runs Monte Carlo simulations assuming that the true frequency of wage change is known.[41] These simulations are performed as follows: Pick a sample size (N), period ($l = 3, \ldots, 16$), level of significance (α), break size (Δw), and set the true frequency of wage change to π. Assign measurement error[42] to all workers, and a random break of size Δw to the πN workers.

[41] Part of this section re-runs the simulations performed in BBG, with the addition of a longer sample and smaller break sizes that were observed in 2008 SIPP.

[42] Appendix A.1 describes the structure of measurement error

Table 3 reports the resulting statistics after running the structural breaks test on simulated data for a given level of $l, \alpha, \Delta w, \pi$, and N. It documents the adjusted frequency of wage change ($\hat{\pi}$), the transformation of adjusted frequency that is corrected for type I and type II errors ($\tilde{\pi}$), and the power of the test (γ). The final statistics reported in Table 2 using actual data are tantamount to the average across cells of Table 3 for each π. For $\pi = 0.15$, the average $\hat{\pi}$ = 0.11 and $\tilde{\pi}$ = 0.17. For $\pi = 0.3$, the average $\hat{\pi}$ = 0.17 and $\tilde{\pi}$ = 0.31.

Table 3 reveals that the corrected estimator ($\tilde{\pi}$) is imprecise when the number of waves (denoted by sample lengths l) are small, and the break sizes (Δw) are small.[43] Whereas the former point is not a cause of concern for the relatively longer sample of 2008, the latter point may be. This is because break sizes were smaller, as wages grew at a lower rate in 2008-13 than 1996-00.[44]

However, despite the possibility of imprecision of $\tilde{\pi}$ in 2008-13, there is still evidence of a decline in wage rigidity over the two SIPP panels. Notice that π is increasing in $\hat{\pi}$ so long as $\gamma > \alpha$.[45] Therefore, given that $\gamma > \alpha$ in the 2008-13 panel[46], $\hat{\pi}_{2008} > \hat{\pi}_{1996} \implies \pi_{2008} > \pi_{1996}$. This is shown in Table 2 where the within-job adjusted wage of the 1996 panel is 8.4% and that of the 2008 panel is 14.6%. It is important to note that because the break sizes are smaller in 2008-13, $\hat{\pi}_{2008}$ is an underestimate of the $\hat{\pi}$ when break sizes are high. However, despite being an underestimate, $\hat{\pi}_{2008}$ being higher than $\hat{\pi}_{1996}$ points to the possibility of the true π being higher in 2008-13.[47]

[43] To understand how break sizes affect probability of wage change, I rewrite two equations derived in Appendix A.2:

$$\text{Adjusted probability } \hat{\pi} = \alpha(1-\pi) + \pi\gamma; \text{Adjusted+Corrected probability } \tilde{\pi} = \frac{\hat{\pi} - \alpha}{\gamma - \alpha}$$

Recall power of the test is probability of correctly rejecting the null of no breaks. When break sizes are high, power of the test (γ) is high. If $\gamma \to 1$, then $\hat{\pi} \to \alpha(1-\pi) + \pi$ and $\tilde{\pi} \to \pi$, i.e. $\hat{\pi}$ overestimates the true π, but $\tilde{\pi}$ comes close to it. This can be seen in the bottom right cells of both sections of Table 3. On the contrary, when break sizes are small, the power of the test (γ) is low. This is because the structural breaks test rejects the false null of no breaks less frequently as it is not able to distinguish between wage changes due to measurement error and breaks. In an extreme case where $\gamma \to 0$, $\hat{\pi}$ is underestimated, and $\tilde{\pi}$ overestimated, compared to the case with $\gamma \to 1$.

[44] This can be observed in Figure 4 which demonstrates a relatively higher mass on low wage growth rates in the 2008 panel compared to the 1996 panel.

[45] This follows from $\hat{\pi} = \alpha(1-\pi) + \pi\gamma \implies \frac{d\pi}{d\hat{\pi}} = \frac{1}{\gamma - \alpha}$

[46] Due to paucity of space, I do not present this evidence.

[47] One could also argue that $\hat{\pi}_{2008} > \hat{\pi}_{1996}$ because of the higher length of sample in 2008. However, this remains the case even when I limit the sample of 2008 to twelve waves to make it comparable with the 1996 sample.

Table 3: Simulations for N = 500

		$\pi = 0.15$					$\pi = 0.3$				
$l \downarrow$	$\Delta w \rightarrow$	0.25	0.5	1	1.5	2	0.25	0.5	1	1.5	2
3	Adjusted $\hat{\pi}$	0.04	0.04	0.05	0.06	0.06	0.04	0.05	0.06	0.08	0.08
	Corrected $\tilde{\pi}$	0.40	0.18	0.14	0.12	0.11	0.42	0.28	0.25	0.44	0.37
	Power γ	0.05	0.06	0.08	0.12	0.14	0.05	0.06	0.08	0.12	0.14
6	Adjusted $\hat{\pi}$	0.05	0.05	0.06	0.08	0.13	0.05	0.05	0.08	0.13	0.21
	Corrected $\tilde{\pi}$	0.40	0.09	0.08	0.09	0.15	0.42	0.09	0.25	0.25	0.29
	Power γ	0.06	0.10	0.18	0.37	0.58	0.06	0.10	0.18	0.37	0.58
12	Adjusted $\hat{\pi}$	0.06	0.07	0.13	0.17	0.19	0.07	0.09	0.21	0.29	0.33
	Corrected $\tilde{\pi}$	0.62	0.26	0.18	0.16	0.16	0.42	0.46	0.35	0.31	0.31
	Power γ	0.07	0.14	0.49	0.80	0.97	0.07	0.14	0.49	0.80	0.97
15	Adjusted $\hat{\pi}$	0.06	0.07	0.11	0.18	0.19	0.07	0.10	0.20	0.31	0.33
	Corrected $\tilde{\pi}$	0.33	0.15	0.14	0.15	0.15	0.67	0.35	0.31	0.31	0.30
	Power γ	0.08	0.18	0.52	0.88	0.99	0.08	0.18	0.52	0.88	0.99

Notes: Varying true probability of wage change (π), sample length(l), size of wage change(Δw), given worker sample (N) = 500, and level of significance (α) = 0.05.

A.5 Wage adjustment in other panels of the SIPP

The SIPP consisted of several panels before 1996, and two panels between 1996 and 2008. There are three reasons why I have not included them in the main analysis of the paper: One, all panels before 1996 (these include ones starting in each year from 1985 to 1993), and the 2001 panel have a short sample length, with a maximum of 9 waves. I show in Appendix A.4 that a short sample length makes the probability of wage change imprecise. Therefore, apart from the 1996 panel covering 12 waves, I used the longest panel for which data is available i.e. the 2008 panel covering 16 waves. Two, SIPP has a 2004 panel that was originally slated for 12 waves. However, due to Census budget shortfalls, the SIPP cut the sample by half post wave 8. Wave 1 began with 44,600 households and wave 12 ended with 16,000 households. Three, the pre-1996 panels of the SIPP had difficulty assigning longitudinally consistent job identification numbers. These IDs are a key input in determining within- and between-job wage changes. Whereas a correction for the miscoded IDs is available for the 1990-1993 panels (Stinson 2003), no such revision is available for the panels from the 1980s.

In Table 4, I supplement the main analysis by documenting the quarterly frequency of within-job wage adjustment in the 1990-93, 2001 and 2004 (up to wave 8) panels of the

Table 4: Quarterly frequency of within-job hourly wage adjustment(%)

Panel	Period	No. of waves	Self-Reported	Adjusted	Adjusted & Corrected
1990	1990:2-1992:9	8	51.9	5.2	14.6
1991	1991:2-1993:9	8	50.7	5.0	17.4
1992	1992:2-1995:1	9	50.9	5.4	10.7
1993	1993:2-1996:1	9	50.3	5.5	7.4
1996	1995:12-2000:2	12	53.1	8.4	16.3
	1995:12-1998:1	6	54.1	8.6	15.6
	1998:2-2000:2	6	51.9	7.6	14.0
2001	2001:2-2004:1	9	52.7	6.6	10.5
2004	2004:2-2006:9	8	37.8	11.2	28.2

Notes: This table reports the self-reported, adjusted and adjusted & corrected (for Type I and Type II errors) quarterly frequency of wage adjustment among hourly job stayers. The 2004 panel contains 12 waves, but experienced a cut in sample after wave 8 due to Census budget shortfalls. I therefore, do not include wave 9-12 in the current analysis. Besides, SIPP also contains panels beginning in each year from 1985-89. These have not included in the current analysis owing to incorrect coding of job IDs in these panels.

SIPP. There are two key observations: One, the adjusted and corrected probabilities rise sharply around 2004-2006. This points to the possibility that the high probability of wage change documented in the 2008 panel may have started prior to the Great Recession. This evidence is in line with the pattern reported in San Francisco Fed's Wage Rigidity Meter[48] that shows a fall in the fraction of hourly workers reporting yearly constant wages after 2004. Two, the reported probability is lower in the 2004 panel due to the introduction of dependent interviewing in the SIPP. This pattern was also observed in the 2008 panel.

A.6 Seam Bias in the SIPP

Individuals within the SIPP panel are interviewed every four months, a period referred to as a wave. In each interview, respondents provide employment information about the previous four months since the last interview. This leads to a majority of changes in variables (such as within-job earnings and wages) to seemingly occur in between waves, as opposed to during waves. This problem is referred to as "seam bias". In the context of this paper, seam bias would lead to wages changing less frequently intra-wave, i.e. between the first and last month of a wave, and more frequently inter-waves, i.e. between the last month of a wave and the first month of the next wave.

[48] `https://www.frbsf.org/economic-research/indicators-data/nominal-wage-rigidity/`

A common way to address this problem is to use wages only for the fourth month of a wave when the interview is conducted and drop the recall-observations for the previous three months when the interview is not conducted. By not using observations of the non-interview months, I do not measure intra-wave wage changes, thereby eliminating the possibility of them being different from inter-wave wage changes. The downside is underutilization of data. Another implication in the present context is that within-job wage adjustment assumes that an individual is employed within the same job for at least four months since wages are observed tri-annually in the interview month (i.e. the fourth month of a wave), instead of monthly.

To understand the importance of seam bias, I compare the baseline estimates of wage change computed from wages reported in the interview month with those from wages reported off the interview month. These include wages reported in the first, second and third month preceding the interview month. Finally, I also compute estimates from wages measured in all months.[49]

The findings are reported in Table 5. The top panel (a) of this table compares the adjusted & corrected (for Type I and Type II errors) quarterly frequency of wage adjustment among hourly job stayers when wages are observed in months 4 (baseline), 1,2, and 3 of a wave. Overall these statistics appear similar for both periods, with no consistent pattern of differences. The bottom panel (b) reports the monthly frequency when wages are observed in all waves. This implicitly assumes that workers stay on the job for at least one month, which is different from the assumption made in the top panel that workers stay on the job for at least four months. The monthly frequency is expectedly lower than the quarterly frequency because wages have a higher chance of changing, when measured over a longer time period than a shorter one. Note, also that the monthly frequency does not correct for seam bias, and therefore, is biased downward by the low frequency of wage change within a wave. The increase in frequency of wage change from the 1996 to 2008 panel remains robust regardless of the correction of seam bias.

[49]Note, this case is not directly comparable to the other cases where wages are observed tri-annually because in this case I implicitly assume that the individual remains in the same job for at least one month.

Table 5: Adjusted & Corrected frequency of within-job hourly wage change(%)

Wages observed in:	1996-2000	2008-2013
(a) Quarterly frequency		
Fourth month of wave (Baseline)	16.3	24.9
First month of wave	13.7	24.7
Second month of wave	14.1	24.8
Third month of wave	16.0	24.8
(b) Monthly frequency not corrected for seam bias		
All months of wave	3.8	4.2

Notes: The top panel of this table compares the adjusted & corrected (for Type I and Type II errors) quarterly frequency of wage adjustment among hourly job stayers when wages are observed in months 1,2,3 and 4 of a wave. The bottom panel reports the monthly frequency when wages are observed in all waves. This implicitly assumes that workers stay on the job for at least one month, which is different from the assumption made in the top panel that workers stay on the job for at least four months.