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Time-dependent or state-dependent wage-setting? Evidence from periods of macroeconomic instability

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ABSTRACT

Administrative data on monthly wages in Iceland during 1998–2010 provide new insight into nominal wage rigidity. Unlike the data used in previous work, ours have a higher frequency, minimal measurement error, and a long sample including a period of substantial macroeconomic instability. We find that the monthly frequency of nominal wage changes is 13 percent. Although nominal wage cuts are rare, their frequency rises following a large macroeconomic shock. Timing of wage changes is both time-dependent and state-dependent: we find evidence of synchronization of adjustment and contracts of fixed duration, but also that inflation and unemployment over the wage spell affect the timing of adjustment.

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1. Introduction

How rigid are nominal wages, and what factors determine the timing of wage adjustments? In the Keynesian paradigm, nominal wage rigidity is central to the explanation of variations in output and employment. An extensive literature uses models for analysis of monetary policy and business cycles that rest on the assumption that wages and prices are sticky. Still, detailed evidence on wage rigidity has been very scarce.

In this paper, we use a unique administrative dataset on monthly wages to provide new insight into nominal wage adjustment. The main focus is on the frequency of nominal wage changes as a measure of wage stickiness and on the factors influencing the timing of wage adjustments.[1] The dataset used has numerous advantages for studying wage-setting and wage rigidity, allowing us to present more accurate results than has been done in previous studies. Four main advantages are emphasized. First, the data are at monthly frequency, as are the majority of actual wage payments; hence, most wage changes that occur can be measured. Estimating the frequency of wage changes using lower-frequency data, as is done in most of the literature, could lead to biased results, as some changes are unobserved in the data. Second, wage changes are measured at the employer–employee level. Thus, the data allow for measuring actual wage changes for the same employee working at the same job for the same firm. Third, the data are collected directly from firms' payroll software rather than through interviews or postal surveys. This should limit measurement errors such as rounding or misreporting. Furthermore, it includes detailed and accurate information on both wages and working hours, segregated into daytime and overtime

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[1] Much of the previous research on wage rigidity using microdata has focused on the assessment of downward nominal wage rigidity. See, for example, McLaughlin (1994), Card et al. (1997), Kahn (1997), Smith (2000), Nickell and Quintini (2003), Dickens et al. (2007), and Elsby (2009).

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hours. Fourth, the dataset covers a long and continuous period characterized by substantial macroeconomic instability. Significant variability in macroeconomic variables, both in inflation and unemployment, allows us to test empirically for time-dependency and state-dependency of wage adjustments.

This papers presents a series of indicators of the degree of nominal wage rigidity. Wages are rigid at the microeconomic level: The mean monthly frequency of change in the base wage, our preferred measure of nominal wage rigidity, is measured at 12.9 percent. When changes due to union wage settlements are excluded, the frequency drops to 6.5 percent. There is a substantial seasonal component in wage changes, indicating synchronization in the timing of adjustments. In January, the frequency of wage increases is more than 50 percent, while other changes are staggered throughout the year. Moreover, there is ample heterogeneity in the distribution of wage spell duration, with most spells lasting less than a year but a distinct mass of spells lasting exactly 12 months.

Whether the timing of individual wage changes is exogenous or influenced by changes in the state of the economy has differing implications for the degree of monetary non-neutrality. If wage-setting is state-dependent, the output response to a monetary shock will be attenuated by a more pronounced response in wages selected for adjustment. In comparison, time-dependent wage-setting implies a larger and longer effect of monetary policy on employment and output. As a result, it is important to distinguish empirically between these determinants of the timing of wage adjustments. To this end, we first estimated a hazard model of wage change. The hazard function is mostly flat throughout the first year, but with a large spike at 12 months, consistent with time-dependent adjustment. Then an empirical model of wage adjustment was estimated, incorporating elements of both time-dependency and state-dependency of wage changes. The results provide strong evidence of state-dependent behavior, contradicting previous empirical studies. The timing of wage increases depends on cumulative inflation and unemployment over the current wage spell and also on the state when wages were adjusted in the past. In addition, large macroeconomic shocks are followed by an increased frequency of nominal wage cuts, contradictory to the notion of downward nominal wage rigidity. In contrast to findings in earlier studies, the results therefore highlight how the frequency of wage change, in addition to its magnitude, is determined endogenously in the economy.

This paper contributes to a recent but growing literature on the micro-level evidence of nominal wage rigidity. The study is most closely related to recent papers by Lünnemann and Wintr (2009), Le Bihan et al. (2012), and Barattieri et al. (2014). However, unlike ours, these papers have used data of lower frequency and/or datasets that are subject to various types of measurement errors, either in wages or in working hours, and cover periods of low macroeconomic variability. As we will discuss in greater detail, these issues affect both the accuracy of their results and their ability to identify empirically the underlying mechanisms of wage adjustment.

The remainder of the paper is organized as follows. Section 2 presents an overview of wage-setting and labor market institutions in Iceland. Section 3 describes the data. In Section 4 we present evidence of the frequency of wage changes, the distribution of wage spell duration, the size of wage changes, synchronization, and heterogeneity in wage adjustment. Section 5 provides econometric evidence on nominal wage adjustment. In Section 6 the results are compared to those in previous studies, and Section 7 concludes.

2. Wage-setting in the icelandic labor market

There are clear differences between the US and European labor markets, and within labor markets in Europe, as regards the institutions that affect wage formation. To interpret our results more effectively and put our findings into perspective, we present a short overview of the main characteristics of wage-setting in Iceland.

Iceland has a high degree of collectivization of wage bargaining, with union density among the highest among OECD countries. Furthermore, bargaining coverage is around 85 percent, a rate similar to that in the Nordic countries, Austria, France, Italy, and the Netherlands, but much higher than in the UK and the US (Du Caju et al., 2008). Icelandic private sector unions are organized on either a sectoral or an occupational basis and are affiliated with the Icelandic Confederation of Labour (ASI). Employers are highly organized as well. Centralized wage bargaining tends to produce nationwide settlements that provide for minimum wage increases and can then be followed by more decentralized and less dominant negotiations at lower levels. The structure of the union wage bargains is usually the same: wage increases take effect upon signing and then on January 1 each year.

During our sample period, the duration of union contracts was 3–4 years. As a rule, contracts contain some kind of trigger clauses according to which settlements can be revoked if the premises on which they are based – usually some type of CPI threshold – fail to hold. If assumptions do not hold, which has been the case more often than not, the contracting parties can either review the wage package within the settlement or revoke the settlement *en bloc*. Reviews generally result in wage increases that are nonetheless far smaller than those that would have been necessary to maintain the purchasing power originally intended when the agreements were signed.[2] Employees are commonly paid wages above the rates specified in the union contracts; therefore, contracts function as floors for the wage level and wage growth. Above these

[2] Table A.9 in Online Appendix D compares average yearly changes in the wage index, calculated by Statistics Iceland, and predetermined yearly wage increases resulting from union negotiations, which are lower than the growth in the wage index in all periods, indicating ample flexibility for wage adjustment at the discretion of firms.

wage levels are various margins for downward and upward wage adjustments, including allowances and bonuses, at the discretion of the firm. Wage-setting in Iceland therefore shares institutional features with both the unionized European labor market and the decentralized labor markets in the US and UK.

3. The data

This paper uses unpublished confidential administrative microdata for the Icelandic labor market. The dataset is constructed from data collected by Statistics Iceland through the *Icelandic Survey on Wages, Earnings and Labour Costs* (ISWEL), covering firms representing about 80 percent of the Icelandic private sector. Every month, firms in ISWEL submit, directly from their payroll software, standardized and detailed information on wages, labor costs, working hours, and background factors on both the firm and its workers.

Our dataset contains monthly data for the period from January 1998 to December 2010, except for workers in financial services which are available from January 2004. In a given firm, data are sampled for all workers aged 16–74, excluding firm owners and apprentices. For each worker, we have information on date of birth, gender, educational attainment, tenure with current employer, length of labor market experience, etc. The dataset contains 85,534 individual workers, 39,173 of whom are women, and the average age is 39 years. In all, our dataset contains roughly 2.6 million observations over the sample period of 13 years. Firms are categorized into five different industries and workers into seven different occupations.

Wages and hours are reported at monthly frequency. Hours are separated into two categories: daytime and overtime hours. The distinction between daytime and overtime hours is standardized and therefore comparable across firms. Our data include detailed information on wages and other payments, such as bonus payments, shift differential, sickness pay, overtime pay, piecework, irregular bonuses, lump-sum payments, and other irregular payments. Two measures of hourly wages are used in the paper: *base wages* and *regular wages*. Base wages are wages for daytime work divided by the number of daytime hours. Regular wages include, in addition to base wages, all regular monthly payments – including bonuses and allowances – divided by daytime hours. Our preferred measure is the base wage, which we believe to be the most relevant for macroeconomic interpretation, as it accounts for 73% of total average monthly payments and is compatible to the definition of wages used in previous studies. For comparison with the base wage, statistics are also reported for the regular wage, which includes both performance-related payments and other irregular payments.

As is emphasized in the introduction, our dataset is unique and has numerous advantages. In comparison with the most related studies, we argue that because our high-frequency administrative dataset has more detailed information and is less prone to measurement errors, it allows us to obtain more accurate results. Two amendments are made to the data, however. First, in our analysis outliers are excluded. An observed wage increase or wage decrease is classified as an outlier if it falls in the bottom 2 or the top 2 percent of the size distribution. Second, for some subsets of workers, there are changes in the base wage that take the form of a V-shape or an inverted V-shape; i.e., wage increases that are reversed by wage decreases in the subsequent month, or vice versa. A simple algorithm is used to exclude these trajectories from the analysis, as such variations are probably caused by misreporting of either working hours or wages and are therefore corrected the following month. One could argue that such temporary wage changes might arise from a response to a temporary increase in labor demand and should therefore be included in the measured frequency of change. We argue, however, that in order to respond to temporary changes in demand, firms would use margins not captured in the base wage, some of which are included in the regular wage. Online Appendix A provides a further description of the dataset, as well as a detailed description of the correction procedures and their effect on our results.

4. Wage adjustment and duration of wage spells

Workers and jobs are matched in relationships. Jobs are characterized by both the firm employing the worker and the worker's occupation at the firm. The hourly base wage paid to employee i working job j in month t is denoted as $w_{ij,t}$. Employer–employee relationships are assumed to be created in month t, when wage payments $w_{ij,t}$ are first observed, and destroyed in month $t+n$, when the last wage payment is observed. When a worker is no longer observed in a job because he or she leaves the firm or occupation or exits the labor market, the relationship ends. Therefore, for each relationship ij there is an n-long wage trajectory. Each wage trajectory can be divided into wage spells, where a wage spell is defined as a continuous period without a wage change.

For all relationships, ij, that have lasted for two consecutive months, monthly indicators, $I_{ij,t}$, are created, indicating whether wages have increased, decreased, or remained unchanged in month t. More precisely, an indicator for a wage increase is defined as:

$$I^{+}_{ij,t} = \begin{cases} 1 & \text{if } w_{ij,t} > w_{ij,t-1} \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

Table 1
Frequency of wage change, size of changes and duration of wage spells.

Wage measure	Mean frequency			Mean size		Wage spell
	Change (percent)	Increase (percent)	Decrease (percent)	Increase (percent)	Decrease (percent)	Implied duration (months)
Base wage	12.9	12.1	0.8	5.8	6.4	7.3
Raw dataset	18.2	15.5	2.7	6.7	6.1	5.0
Excluding union settlements	6.5	5.8	0.6	6.6	6.3	15.0
Regular wage	40.6	25.9	14.7	6.6	6.1	1.9
Excluding union settlements	31.4	18.5	12.8	6.8	6.0	2.7

Notes: All frequencies reported are in percentage per month. The mean size is the mean percentage change per month. The size of decrease is reported in absolute terms. Implied duration is calculated under the assumption that the hazard rate of wage change is the constant λ and the probability of a wage change is $f = 1 - e^{-\lambda}$. The mean implied duration is therefore $d = -1/\ln(1-f)$, where f is the mean frequency of wage change.

the indicator for a wage decrease as:

$$I^{-}_{ij,t} = \begin{cases} 1 & \text{if } w_{ij,t} < w_{ij,t-1} \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

and, finally, the indicator for unadjusted wages as:

$$I^{=}_{ij,t} = \begin{cases} 1 & \text{if } w_{ij,t} = w_{ij,t-1} \\ 0 & \text{otherwise} \end{cases} \tag{3}$$

Our preferred measure of the degree of wage rigidity is the frequency of wage changes. In our notation, the mean monthly frequency of wage change, in month t, is defined as:

$$f_t = \frac{\sum_{ij}(I^{+}_{ij,t} + I^{-}_{ij,t})}{\sum_{ij}(I^{+}_{ij,t} + I^{-}_{ij,t} + I^{=}_{ij,t})} \tag{4}$$

Analogously, the mean frequency of wage increases and decreases can be constructed using each of the two components in the numerator. Another important measure of wage adjustment is the size of wage changes. We use the indicators to define formulas for the size of wage increases and decreases as:

$$\Delta w^{+}_t = \frac{\sum_{ij}\left(I^{+}_{ij,t} * \left(\frac{w_{ij,t} - w_{ij,t-1}}{w_{ij,t-1}}\right)\right)}{\sum_{ij} I^{+}_{ij,t}}, \quad \Delta w^{-}_t = \frac{\sum_{ij}\left(I^{-}_{ij,t} * \left(\frac{w_{ij,t} - w_{ij,t-1}}{w_{ij,t-1}}\right)\right)}{\sum_{ij} I^{-}_{ij,t}} \tag{5}$$

where Δw^{+}_t and Δw^{-}_t are the average percentage wage increases and decreases, respectively, in a given month.

4.1. The frequency and size of wage changes

Our principal measures of the degree of nominal wage rigidity are the frequency of wage adjustments and the implied duration of wage spells. These measures follow the practice in earlier studies of wage and price stickiness but are also directly related to the key parameters in models of wage rigidity widely used in New Keynesian macroeconomic models.

Table 1 shows estimates of the frequency of wage changes, wage increases and decreases. We find that wages are changed infrequently. The mean monthly frequency of change in the base wage is 12.9 percent. Wage increases constitute the majority of wage adjustments – on average, 12.1 percent of wages are increased every month – but we also find evidence of nominal wage cuts with an average frequency of 0.8 percent. The amendments of the dataset described in Section 3 affect the estimated frequency of both wage increases and wage decreases. In the raw dataset, the estimated monthly frequency of wage adjustments is 18.2 percent, which breaks down into a 15.5 percent monthly frequency of wage increases and 2.7 percent frequency of wage decreases.

As is described in Section 2, the Icelandic labor market is highly unionized and wage bargaining is centralized. However, there is flexibility for wage adjustment at the individual firm level. We distinguish between wage changes that arise from union settlements, on the one hand, and sector- and firm-level bargaining, on the other, to report estimates of the frequency and size of wage changes excluding all union settlement changes. This is an *ad hoc* method where all dates during our sample period when there have been wage changes negotiated by unions have been historically identified. Because union bargaining takes place at the national level, the dates when a very large share of workers receive a wage change negotiated by the unions can be identified. Removing union settlement changes provides a rough estimate of wage adjustments that are at the discretion of firms. Excluding wage changes due to union settlements reduces the estimated monthly frequency of

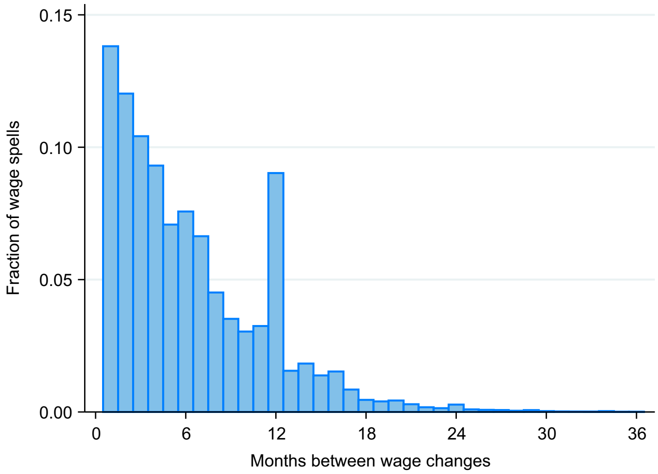

Fig. 1. Distribution of duration of wage spells.

wage change by half, to 6.5 percent.[3] Assuming a constant hazard of change, the measure of the mean frequency of change can be inverted to give a measure of the average duration of wage spells.[4] Corresponding to the mean frequency of wage changes, the implied duration of a wage spell is 7.3 months. If union settlements are excluded, the implied duration of spells increases to 15 months.

Table 1 also presents estimates of the frequency and size of adjustments in the regular wage, a broader definition of wages, for the purpose of comparison with our base wage estimates. The regular wage includes payments for daytime work, as does the base wage, but also all other regular monthly payments related to daytime work, such as allowances and bonuses, but it excludes overtime pay. The regular wage is much less sticky than the base wage. The mean monthly frequency of change in regular wages is roughly 40 percent, which implies a wage spell duration of only two months. The high frequency of change is explained to large extent by the much more frequent wage decreases than are found for the base wage measure, as 36 percent of adjustments in the regular wage are decreases. Furthermore, a significant amount of changes in regular wages are temporary, meaning that they are reversed in a subsequent month. According to this result, there exists a wage margin at which there is substantial flexibility, channeling temporary wage adjustments.

Our finding of infrequent wage adjustment is expected because the adjustment process is costly; both the bargaining process and the adjustment of wage payments require time and resources. However, because wage adjustment is costly, one would expect adjustments to be both infrequent and large when they do occur, even though adjustment to shocks is sluggish. Table 1 reports the mean size of wage increases and the mean absolute size of wage decreases. The mean size of increases in base wages is 5.8 percent and the mean absolute size of wage decreases is 6.4 percent. The average size of changes in regular wages is similar to that for base wages.

4.2. Duration of wage spells

What is the distribution of duration of wage spells? Previously, we have computed the duration of spells from the frequency of change under the assumption of constant probability of change. However, an alternative and more informative strategy is to explore the distribution of wage spell durations directly. Fig. 1 plots the distribution of duration of wage spells. As the figure demonstrates, there is a substantial heterogeneity in the duration of spells. Many of the spells are short, and 90 percent of them last one year or less. The mean wage spell duration is 6.4 months, one month less than the duration implied by the mean monthly frequency, and the median duration is 5 months. There is also a significant fraction of longer-lasting wage spells, some of them lasting more than 3 years. A distinct pattern is that substantial share of spells, almost 10 percent, last exactly one year.

The distribution of wage spell duration indicates that there is substantial heterogeneity in the frequency of wage change. The literature on heterogeneity of price rigidity, notably Carvalho (2006) and Nakamura and Steinsson (2010), has found that introducing heterogeneity in the frequency of price change into a model of price stickiness substantially increases the degree of monetary non-neutrality. In a model with heterogeneity in the frequency of price change, monetary shocks generate larger and more persistent real effects than a model in which all firms change prices with the same frequency. Similarly, Dixon and Kara (2011) study a model with wage contracts of various lengths, finding that even a small proportion

[3] This method, however, could underestimate the frequency of change at the discretion of the firm, as some of the changes excluded might be due not to union settlements but to firm-level or sectoral agreements.

[4] If constant probability λ of wage changes is assumed, the frequency of wage changes is $f = 1 - e^{-\lambda}$. This implies that the mean duration of a wage spell is $d = \frac{-1}{\ln(1-f)} = \frac{1}{\lambda}$.

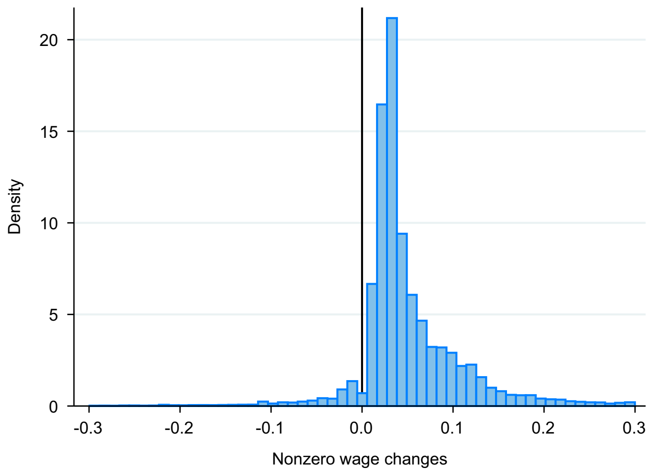

Fig. 2. Distribution of non-zero nominal wage changes. *Notes:* Each bar in the histogram represents the density for a percentage point interval of size of wage change, e.g. the bar at 1 percent shows wage changes of a size from 1 percent up to 2 percent. A zero-line is added for reference.

of long contracts can significantly increase output persistence following a monetary shock. Heterogeneity in the frequency of price and wage changes can generate an increase in output persistence because of strategic complementarity in price- and wage-setting. In the case of wages, because longer wage contracts adjust slowly to monetary shocks, shorter contracts will also adjust slowly, as the desired wage depends on the price level, which in turn depends on the wages of those with longer contracts. The spillover from longer to shorter contracts is therefore through the slow and more persistent effect of long contracts on the price level. In light of these results, our findings of heterogeneity in the duration of wage spells and a share of long-lasting spells may provide an empirical explanation for output persistence.

4.3. Downward nominal wage rigidity

Until recently, most of the literature studying nominal wage rigidity in microdata has focused on downward adjustment as a measure of rigidity. However, there is limited agreement in the literature concerning the degree to which nominal wages are downwardly rigid. Recently, researchers have used microdata to study distributions of wage changes to provide an empirical answer to this question (see, e.g., Dickens et al., 2007; Holden and Wulfsberg, 2008 for tests of downward nominal wage rigidity). In light of these studies, we investigate the properties of the distribution of wage changes in our data. Fig. 2 reports the distribution of wage changes. The size of wage change is measured only for wages that adjust between months, hence there are no zero changes in the distribution; however, a zero line is included as a reference point. First, we note that nominal wage cuts exist but account for only 5 percent of changes. Second, we see that the figure displays a substantial variation in the size of adjustment.

If wages are downwardly rigid, what properties should characterize the distribution of wage changes? The literature on downward nominal wage rigidity has identified a few properties: the distribution would be highly asymmetric, with no (or at least very low) density below zero and a large zero spike.[5] Fig. 3 plots the yearly distributions for 9 years, representing different labor market conditions, and includes both estimated kernel density and a fitted normal distribution for comparison. First, we see that there is substantial variation in the size of changes, even in years of high unemployment. Second, wage changes do not display a symmetric distribution and have much more clustering around the median as compared with the fitted normal distribution. Symmetry in the distributions was tested with a Jarque–Bera test of normality for each year, where the hypothesis of normality was rejected at the 1% level in all cases. Third, although they are not displayed in Fig. 3, a considerable share of wages are unchanged over longer periods, a feature that would translate into a zero spike.

The fact that nominal wage cuts are, on average, infrequent does not necessarily provide evidence for the notion of downward nominal wage rigidity, but indicates instead that shocks of sufficient magnitude to trigger wage cuts are rare. In autumn 2008, the Icelandic economy was hit hard by a financial crisis, with GDP contracting by almost 11 percent in the recession that followed and unemployment rising by more than 8 percentage points, peaking at 9.2 percent. Since the Great Recession falls within our sample period, we are able to study whether the frequency of nominal wage cuts is influenced by such large macroeconomic shocks.[6] Figure A.6 in Online Appendix D plots the monthly distributions of wage changes during the financial crisis and at the beginning of the recession. In November, there is a substantial mass below zero and a distinctive spike at -10%. A similar but smaller spike is also evident in the distribution for December. As we document in

[5] As is emphasized by Elsby (2009), under downward nominal wage rigidity, wage increases will also become more compressed because firms, when making decisions on wage increases, must compensate for the cost of not being able to cut wages if needed in the future.

[6] In Section 5, we study more closely how macroeconomic conditions affect the probability of wage changes.

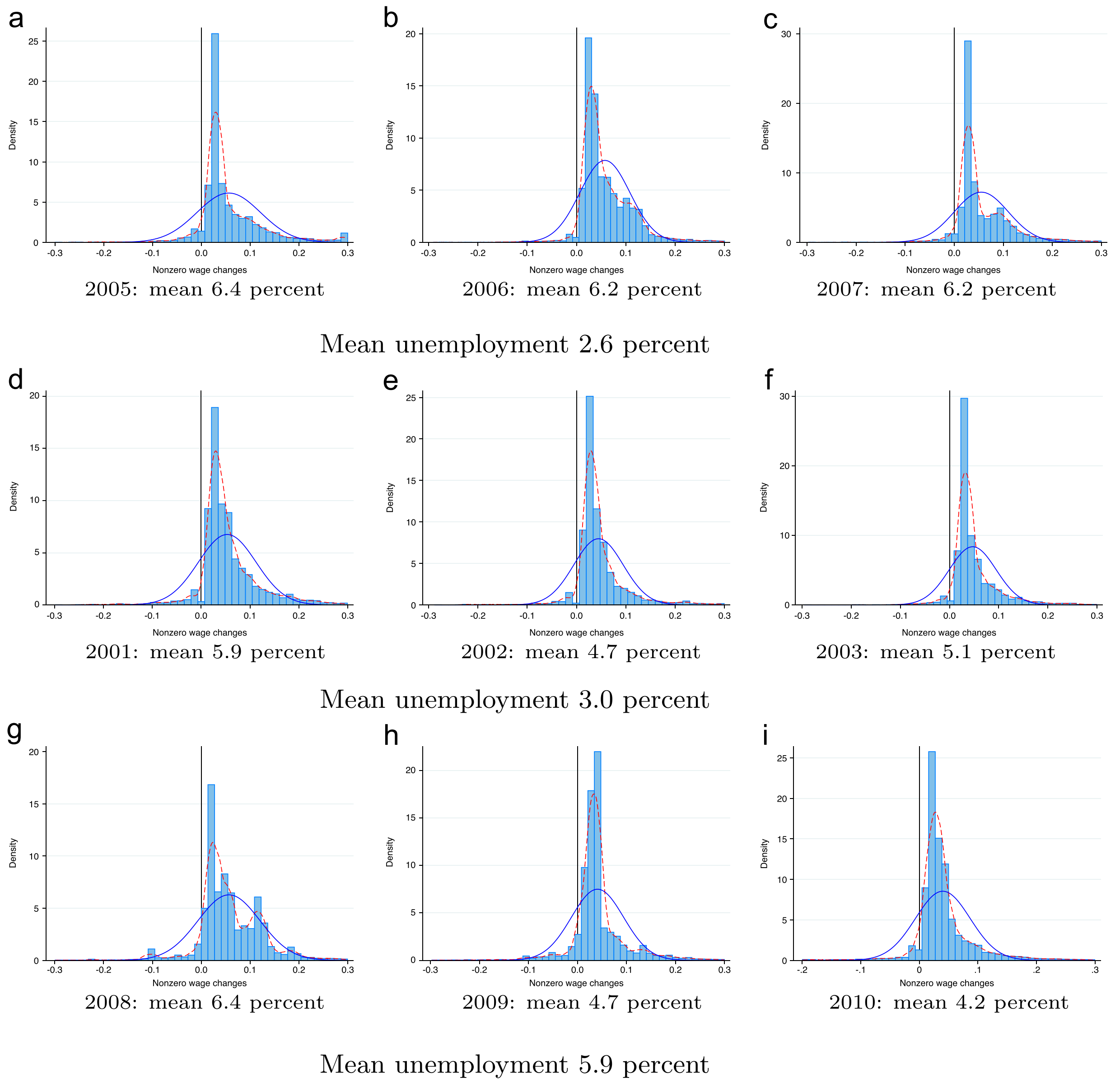

Fig. 3. Distribution of non-zero nominal wage changes. *Notes:* Histograms of non-zero nominal wage changes, a fitted normal distribution (solid line) and estimated kernel density (dashed line). A zero-line is added for reference. The bandwidth parameter for the Epanechnikov kernel function, *h*, is set to 0.01. For each year we calculate the mean size of wage change and perform a Jarque–Bera test of normality, rejecting the null hypothesis of normality at the 1% significance level in all cases.

Online Appendix B, a substantial slowdown in wage growth at the end of 2008 is evident for all occupations and in all sectors, and large wage decreases are observed in the construction, transport, and financial sectors. Moreover, with inflation measuring above 17% in November 2008, workers receiving a 10% cut to their nominal wage were faced with an enormous drop in their real wage. These observations suggest that nominal wage cuts appear to have been one channel of adjustment in the Icelandic labor market during the Great Recession, contradicting the general notion of a strict downward nominal rigidity.

4.4. Staggering and synchronization

The timing of wage adjustments is an important determinant of how aggregate shocks affect the real economy. More precisely, the degree of synchronization, which indicates how large a fraction of wages are adjusted at the same time, and the degree of staggering, which refers to how wage adjustments are distributed over time, play an important role in determining the effectiveness of monetary policy. Seasonality in wage changes is one form of synchronization.

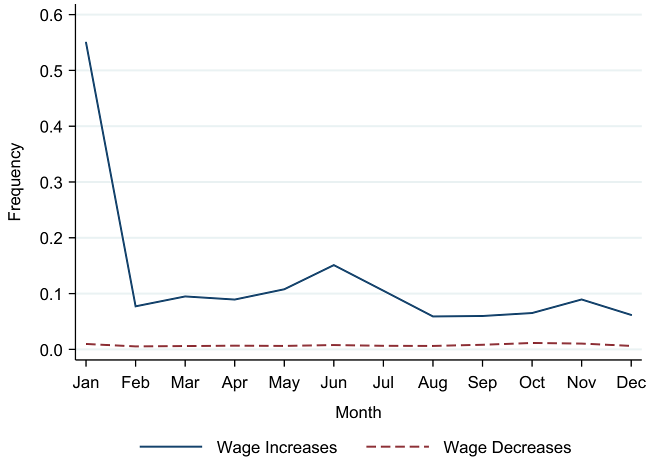

Fig. 4. Frequency of nominal wage increases and decreases by month.

Fig. 4 plots the frequency of wage change by month, separated into increases and decreases, for the average year in our sample. While a substantial share of wage increases are distributed over the course of the year, a clear seasonal pattern is revealed: on average, 50 percent of wages increase in January. In other months of the year, the frequency of increase ranges from 7 percent to 16 percent.[7] The frequency of wage decreases is low in all months and does not display a clear seasonal pattern. The January peak corresponds to the implementation of wage increases negotiated by unions, as those changes generally take place in January. When union settlement changes are excluded, the mean monthly frequency of wage changes is largely flat over the year (see Figure A.8 in Online Appendix D). The timing of wage increases is therefore characterized by a combination of synchronization in the beginning the year and substantial staggering over the course of the year, with positive frequency in every month.

Olivei and Tenreyro (2007) show that if wage contracts are not uniformly staggered, monetary policy affects the real economy differently at different points in time. Hence, a monetary policy shock generates more marked responses in economic activity at times when wages are more rigid. However, what is critical for the effectiveness of monetary policy is not the time at which wage changes are implemented but when they are decided (Olivei and Tenreyro, 2010). Therefore, if seasonal wage changes are predetermined, as in the case of the changes due to union settlements in Iceland, observed seasonality in wage adjustment will not directly affect the size of real effects of a monetary policy shock.

4.5. Heterogeneity across firms and workers

In macroeconomic models with nominal wage rigidity, wage contracts are staggered, but firms and workers are generally assumed to be homogeneous and contracts identical. As we have shown, wage contracts are not homogeneous in duration, nor are changes uniformly staggered. To shed further light on the validity of these assumptions, we briefly explore heterogeneity in wage-setting. In Online Appendix B we study the frequency and the size of wage changes by industry and occupation. The average frequency does not vary much across industries, apart from the financial services sector, where changes are larger but less frequent. There are greater differences across occupations, where wage adjustment is less frequent for managers and specialists compared to, e.g., blue-collar workers.

As another form of heterogeneity, wage-setting may differ according to firm size, as larger firms may be more able to apply firm-level wage contracts rather than contracts like those negotiated by unions. The cost of changing wages may also be relatively higher in small firms, as the action of changing wages may involve some fixed cost. Table 2 reports the frequency and size of wage adjustment for four different firm size categories. Smaller firms are found to adjust wages less frequently than large firms; the frequency of change in firms with fewer than 20 employees is 11 percent, as opposed to about 13 percent for firms with 150 employees.

5. Time-dependent and state-dependent wage-setting

Is timing of wage changes exogenous, or is it an outcome of an optimizing behavior of firms and workers, influenced by the state of the economy? In the standard models of time-dependent wage-setting, duration of wage contracts is exogenous and adjustments are made every *n*th period (Taylor, 1980) or randomly (Calvo, 1983). In contrast, under state-dependent

[7] A graph of changes in regular wages by month, available in Figure A.7 in Online Appendix D, shows the seasonal pattern for regular wages is similar to that for base wages.

Table 2
Frequency and size of wage change by firm size.

Number of employees[a]	Mean frequency			Mean size		Wage spell
	Change (percent)	Increase (percent)	Decrease (percent)	Increase (percent)	Decrease (percent)	Implied duration (months)
10–19	11.0	10.2	0.8	5.8	5.9	8.6
20–49	12.0	11.4	0.6	5.7	7.0	7.8
50–149	13.1	12.2	0.9	5.7	6.1	7.1
≥ 150	12.9	12.2	0.7	5.8	6.4	7.2

Notes: All frequencies reported are in percentage per month. The mean size is the mean percentage change per month. The size of decrease is reported in absolute terms. Implied duration is calculated under the assumption that the hazard rate of wage change is the constant λ and the probability of a wage change is $f = 1 - e^{-\lambda}$. The mean implied duration is therefore $d = -1/\ln(1-f)$, where f is the mean frequency of wage change.

[a] As Statistics Iceland does not include firms in their sample that have less than 10 workers the smallest subgroup strictly includes firms with 10–19 employees.

wage-setting, nominal rigidities arise because of fixed "menu costs" associated with changing wages; e.g., the cost of commencing wage negotiations.[8] According to the menu cost model, changes in the state of the economy drive wages away from their optimum level, eventually triggering adjustment.[9] This implies that wages are not adjusted at random; rather, wages that are "selected" for adjustment are those furthest from their optimum level.

An alternative explanation for nominal wage rigidity is the existence of informational friction. In the inattentiveness model pioneered by Caballero (1989) and Reis (2006), there is no physical adjustment cost, but agents are unable to costlessly acquire, absorb, and process the information they need in order to decide whether to adjust. This friction leads agents to optimally choose to be inattentive and to update their information sets and perform adjustments sporadically at predetermined dates. The inattentiveness model therefore gives rise to adjustment that is *recursively time-contingent*. That is, timing of wage adjustment does not depend on the evolution of the state of the economy since the last adjustment, as in the menu cost model, but rather on the state at the last adjustment. In comparison to the model in Taylor (1980), for example, the inattentiveness model therefore provides a micro-foundation for time-dependent wage contracts where, instead of being exogenous and fixed, the intervals between adjustments depend on the state when wages were set in the past.[10] Woodford (2009) develops a model where information about both state and duration since the last adjustment is costly, but partial information about the current conditions is available without cost. In the model with costly memory, optimal adjustment follows a state-dependent rule where agents use the costless information about the current state to make decision on whether to incur the information cost and to adjust.

Understanding to what extent wage adjustment is time-dependent, either exogenously or endogenously, or state-dependent is of great importance due to the different implications these mechanisms have for monetary non-neutrality. If wage-setting is state-dependent, the real effect of monetary policy is attenuated by a more pronounced response in wages selected for adjustment. Hence, under time-dependent wage-setting, monetary policy shocks will have a longer-lasting effect on output and employment relative to state-dependent adjustment. Our methodology to distinguish empirically among factors driving the probability of wage adjustment is twofold. First, following the literature, we estimate a hazard model for wages and analyze the hazard function that describes the conditional probability of a wage change at each point of duration. Second, we estimate an empirical model of wage adjustment that incorporates the different features of models with time- and state-dependent wage-setting. In order to be able to identify whether wage adjustment is state-dependent, we need a sample period featuring sufficient variation in macroeconomic variables. Our data offer a rare opportunity to evaluate the empirical importance of both state-dependent and time-dependent elements of wage-setting, as our sample covers a long period featuring substantial macroeconomic variability. Fig. 5 displays monthly inflation, measured as the twelve-month change in the CPI, and the monthly unemployment rate. During the sample period, unemployment fluctuates widely and inflation is volatile. In addition, the period covers the Great Recession, when inflation spiked following a currency depreciation of more than 50 percent and unemployment rose from below 1 percent to 9.2 percent.

5.1. *The hazard of wage change*

The hazard function of wage change differs in shape, depending on whether the duration of wage spells is a function of time or the state of the economy. Time-dependent wage contracts with fixed duration, such as in the Taylor model or, under

[8] For early development of models with menu costs, see, e.g., Barro (1972) and Sheshinski and Weiss (1977). For further development of the model and studies of the implications of monetary policy in this class of modes, see, e.g, Caplin and Spulber (1987), Dotsey et al. (1999) and Golosov and Lucas (2007).

[9] We are not aware of a micro-founded model of state-dependent wage-setting like that for price-setting. However, we draw inferences from menu cost models of price-setting about how wage-setting under state-dependency will differ from wage-setting under time-dependency.

[10] In contrast to Caballero (1989) and Reis (2006), Bonomo and Carvalho (2004) present a model where there is both information-gathering cost and adjustment cost, which are borne together. This results in a time-dependent rule with fixed wages between adjustment dates rather than a preset path, which results from having only information cost.

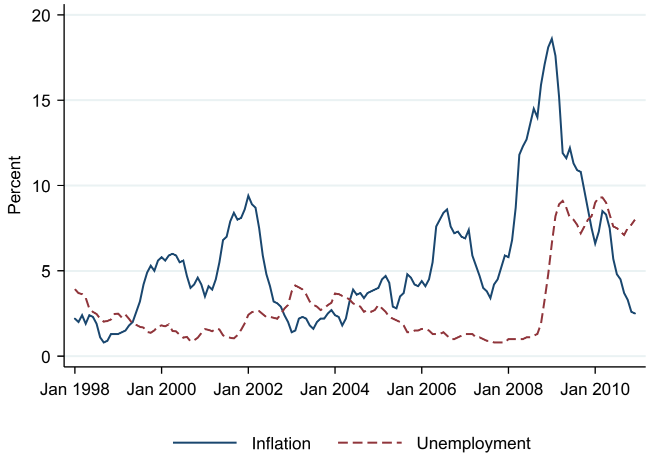

Fig. 5. Inflation and unemployment. *Notes:* Inflation is 12 month change in statistic Iceland's consumer price index. Monthly unemployment is the registered unemployment rate as measured by the Directorate of Labour.

some conditions, the inattentiveness model, give rise to a hazard function with spikes at the duration of contracts. Thus, if the labor market is characterized by one-year wage contracts, the hazard function would display a spike at twelve months, but the hazard would otherwise be zero. If wage spells have random duration, as in the Calvo model, the hazard function is flat. State-dependent wage-setting can give rise to various shapes of the hazard function, but if, for example, wages are more likely to change the longer they have remained unchanged, the hazard function will be upward-sloping.

When we explored the distribution of wage spell duration in Section 4.2, we found substantial heterogeneity in the length of spells, with a large share of spells lasting less than one year and the longest spells more than three years. However, the presence of a number of right- or left-censored wage spells affects these results. New wage spells begin either at the start of a new relationship or immediately after a wage change. Because of censoring, we do not have exact information on the length of all wage spells. In order to account for right-censoring, we estimate a hazard function.

In discrete time, where T is a random variable denoting the duration of a generic wage spell, the hazard function $\lambda(t)$ is explicitly defined as:

$$\lambda(t) = Pr[T = t | T \geq t] = \frac{f(t)}{S(t-1)} \tag{6}$$

where $f(\cdot)$ is the probability density function and $S(\cdot)$ is the survival function, describing the lifetime of wage spells. The hazard function therefore describes the probability of a wage changes in period t, conditional upon the survival of a wage spell until the beginning of period t.[11]

We estimate a discrete time hazard model for wage changes, where failure is defined as an observed wage change. We control for age, gender, education, tenure, labor market experience, and whether the worker is a foreign citizen. Standard errors are clustered at the firm level to account for interdependency in the probability of wage adjustment within firms. Wage spells longer than 24 months are truncated, and left-censored spells are dropped. Fig. 6 plots the hazard function based on the estimated hazard model.

The hazard function for wage changes is mostly flat during the first year, with the monthly hazard ranging from 10 to 15 percent. At twelve months, there is a large spike: given that wages have not been changed during the year, the probability of change is more than 40 percent. After the first year, the hazard function has smaller spikes, but the survival probability of a generic wage spell has dropped to 12 percent. A significant twelve-month spike is consistent with time-dependent contracts of fixed one-year duration, but substantial variation in the hazard rate contradicts the prediction of the Calvo model. However, the fact that the hazard rate is positive and large throughout the first year indicates that a large share of contracts are different in nature than fixed-duration Taylor-type wage contracts.

Estimates of hazard functions using pooled data with heterogeneous subgroups will yield a downward bias in the estimated slope (Kiefer, 1988). Following Nakamura and Steinsson (2008), who estimate hazard functions for price change, we estimate separate hazard functions for all industries and occupations. The results are presented in Online Appendix D. As in Section 4.5, we find limited evidence of heterogeneity. In all cases, the estimated hazard functions are relatively flat throughout the first year and display a significant twelve-month spike.

[11] For further description of the discrete-time hazard model, see, for example, Kalbfleisch and Prentice (2002).

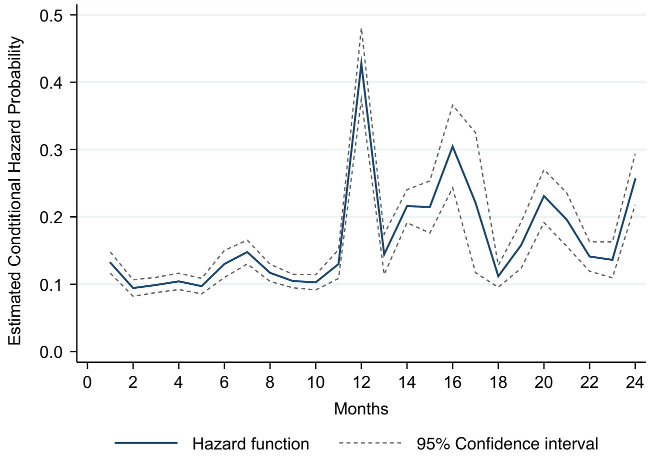

Fig. 6. Hazard function of wage changes. *Notes:* Wage spells longer than 24 months are truncated and left-censored spells are dropped. Standard errors are clustered at the firm level.

5.2. Empirical model of wage changes

In order to study the underlying process of wage adjustment, we estimate an empirical model of wage changes that incorporates both indicator variables for time and elapsed duration, suggested by models of time-dependent wage-setting, and variables capturing variations in the state of the economy over the wage spell.

We model the decision whether or not to adjust wages as the following selection process:

$$y_{ij,t} = \begin{cases} 1 & \text{if } y^*_{ij,t} > 0 \\ 0 & \text{if } y^*_{ij,t} \leq 0 \end{cases} \tag{7}$$

where the latent variable $y^*_{ij,t}$, triggering a wage change at time t for worker i employed by firm j, is described as follows:

$$y^*_{ij,t} = \mathbf{z}_{ij,t}\gamma + \eta_{ij,t}, \quad \eta_{ij,t} \sim N(0,1) \tag{8}$$

The set of covariates is collected in a row vector denoted by $\mathbf{z}_{ij,t}$ which includes factors aimed at capturing both possible time-dependent and state-dependent components of wage adjustment. In order to capture time-dependency, the empirical model features indicator variables for the elapsed duration of the wage spell. In addition, we include monthly seasonal dummies.

To test for state-dependency, we include transformations of a set of variables capturing the state of the economy. This includes the natural logarithm of the price level, p_t, the monthly unemployment rate, u_t, and, as a proxy for firm-level productivity, firm size, $s_{j,t}$, measured as the natural logarithm of the number of workers employed in firm j in month t. For a generic variable x_t we compute the cumulative change over the wage spell, $(x_{t-1} - x_{t-\tau_{-1}})$, where $t-1$ is the end date of the wage spell, and τ_{-1} denotes the duration of the wage spell. The economic rationale for the use of such cumulated variables, an empirical approach introduced by Cecchetti (1986), is to include a proxy to measure the disequilibrium between the actual wage and the optimal wage. Since duration of wage spells varies across individuals and over time, this setup allows us to test for state-dependency in wage-setting as described by the menu cost model. In addition, the empirical model includes the absolute value of the size of the previous wage change. According to the state-dependent framework, this provides an indication of the degree of wage rigidity. Large wage changes may indicate large costs associated with wage adjustments, which are therefore conducted less frequently, giving rise to a negative relationship between the size of the previous change and the current probability of adjustment.

As emphasized above, an important question is whether the timing of wage adjustment depends on the state of the economy during the wage spell, as in the menu cost model, or at the time when wages were set in the past, as in the inattentiveness model (Reis, 2006). In order to study this empirically, the model includes the cumulative change in the macroeconomic variables over the previous wage spell, $(x_{t-\tau_{-1}-1} - x_{t-\tau_{-1}-\tau_{-2}})$, where τ_{-2} denotes the duration of the previous spell. This set of variables measures the state of the economy both at the time of and leading up to the last adjustment. Wages may also be contingent on the current state rather than cumulated state over the entire wage spell; e.g., if memory is costly but agents costlessly observe information about the current state (Woodford, 2009). In order to embody this in the empirical framework, the lagged one-period change in macroeconomic variables, $(x_{t-1} - x_{t-2})$, is included as a measure of the current state.

5.3. Estimation results

The empirical model of wage adjustment is estimated separately for wage increases and wage decreases. Table 3 gives estimates for the probability of wage increases. Standard errors are clustered at the firm level to allow for arbitrary correlation over time in the error term for a given firm. The right half of each column reports the marginal effects on the

Table 3
Probit estimates of the probability of wage increase.

Variable	(1)		(2)		(3)		(4)	
	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect
January	1.686***	0.295***	1.676***	0.284***	1.683***	0.284***	1.691***	0.285***
	(0.069)	(0.009)	(0.068)	(0.008)	(0.070)	(0.008)	(0.071)	(0.008)
February	0.238***	0.042***	0.122**	0.021**	0.124**	0.021**	0.161***	0.027***
	(0.054)	(0.009)	(0.058)	(0.010)	(0.057)	(0.009)	(0.059)	(0.010)
March	0.327***	0.057***	0.353***	0.060***	0.366***	0.062***	0.347***	0.058***
	(0.051)	(0.008)	(0.054)	(0.008)	(0.057)	(0.009)	(0.057)	(0.009)
April	0.283***	0.049***	0.292***	0.050***	0.301***	0.051***	0.246***	0.041***
	(0.057)	(0.010)	(0.057)	(0.009)	(0.059)	(0.009)	(0.058)	(0.009)
May	0.335***	0.059***	0.345***	0.059***	0.351***	0.059***	0.260***	0.044***
	(0.043)	(0.007)	(0.044)	(0.008)	(0.045)	(0.008)	(0.044)	(0.008)
June	0.557***	0.097***	0.611***	0.104***	0.621***	0.105***	0.517***	0.087***
	(0.055)	(0.009)	(0.056)	(0.009)	(0.056)	(0.009)	(0.051)	(0.008)
July	0.297***	0.052***	0.297***	0.050***	0.306***	0.052***	0.198***	0.033***
	(0.050)	(0.009)	(0.049)	(0.008)	(0.049)	(0.008)	(0.047)	(0.008)
August	−0.061	−0.011	−0.110**	−0.019**	−0.108**	−0.018**	−0.158***	−0.027***
	(0.056)	(0.010)	(0.049)	(0.009)	(0.051)	(0.009)	(0.050)	(0.009)
September	−0.053	−0.009	−0.042	−0.007	−0.039	−0.007	−0.097*	−0.016*
	(0.052)	(0.009)	(0.051)	(0.009)	(0.053)	(0.009)	(0.051)	(0.009)
October	−0.024	−0.004	0.006	0.001	0.008	0.001	−0.093	−0.016
	(0.056)	(0.010)	(0.060)	(0.010)	(0.062)	(0.010)	(0.061)	(0.010)
November	0.236***	0.041***	0.309***	0.052***	0.312***	0.053***	0.281***	0.047***
	(0.059)	(0.010)	(0.063)	(0.010)	(0.065)	(0.010)	(0.064)	(0.010)
Cumulative change in unemployment,current spell	−0.095***	−0.017***	−0.098***	−0.017***	−0.102***	−0.017***	−0.080***	−0.013***
	(0.008)	(0.001)	(0.009)	(0.001)	(0.010)	(0.001)	(0.009)	(0.001)
Cumulative inflation, current spell	0.032***	0.006***	0.009***	0.001***	0.010***	0.002***	0.007**	0.001***
	(0.003)	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)
Cumulative growth in firm size	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Size of previous change	−0.007***	−0.001***	−0.004***	−0.001***	−0.005***	−0.001***	−0.005***	−0.001***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Cumulative change in unemployment, last spell					−0.009	−0.002	−0.009	−0.002
					(0.008)	(0.001)	(0.009)	(0.001)
Cumulative inflation, last spell					0.011***	0.002***	0.011***	0.002***
					(0.002)	(0.000)	(0.002)	(0.000)
Change in current unemployment							−0.207***	−0.035***
							(0.029)	(0.005)
Current inflation							0.037***	0.006***
							(0.007)	(0.001)
Duration dummies	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Wald test	0.000		0.000		0.000		0.000	
Log pseudolikelihood	−590,725		−577,763		−565,586		−563,918	
Observations	1,857,628	1,857,628	1,857,628	1,857,628	1,823,183	1,823,183	1,823,183	1,823,183

Notes: The probit model is estimated by maximum likelihood. Robust standard errors, clustered at the firm level, are in parenthesis. All specifications include variables controlling for age, age^2, experience, tenure, education, gender, foreign citizenship, as well as sets of firm size, industry, and occupation dummies. Marginal effects on the probability of a wage increase of one unit increase in each variable are evaluated at the sample average. Standard errors of marginal effects are calculated using the delta method. The hypothesis that monthly coefficients are jointly zero is tested with a Wald test. Complete estimation results are presented in Table A.16 in Online Appendix D.

* Significance at the 10 percent level.

** Significance at the 5 percent level.

*** Significance at the 1 percent level.

probability of a wage increase, evaluated at the sample average. We control for firm characteristics by including industry dummies and dummies for firm size, and control for worker characteristics by including age, age^2, tenure, labor market experience, education, and gender, as well as a dummy indicating whether a worker is a foreign citizen.

Consistent with the evidence presented in Section 4.4, we find that there is substantial seasonality in wage increases. Under all specifications reported in Table 3, the January dummy has a large, positive, and significant coefficient. The coefficients other month dummies in the first half of the year are also positive and significant, although much smaller in size, indicating that wages are more likely to be increased in the first half of the year. Recalling that the Calvo model predicts that the probability of wage change is constant and the same in every period, we test this prediction by applying a Wald test of joint significance of monthly coefficients, rejecting the hypothesis that they are all zero at the 1% significance level under all specifications. We include dummies for elapsed duration of wage spells for one to 12 months of duration and estimate a substantial increase in the probability of a wage increase when spells have lasted 12 months (see Table A.16 in Online Appendix D). These results are consistent with our hazard function estimates, which provide evidence for wage contracts of fixed one-year duration. However, when we include both sets of time variables – i.e., full sets of both seasonal and duration dummies – this effect is no longer significant, as the seasonal effect dominates.

Table 3 presents estimates for three specifications of the empirical model, aimed at capturing the elements of state contingency suggested by different classes of wage-setting models. In the first two columns, forming our baseline specification, we include variables suggested by the menu cost model of state-dependent adjustment. We find that the probability of wage increase reacts both to cumulative unemployment and inflation over the wage spell, strongly indicating state-dependent behavior. According to the estimated marginal effect, a 1 percentage point increase in cumulated unemployment over the wage spell reduces the probability of a wage increase by 1.7 percentage points. Recall that, as is presented in Table 1, the mean monthly probability of wage increase is 12.1 percent over the sample period. Also in line with economic intuition, cumulative inflation increases the probability of wage increase, with a marginal effect 0.1 percentage points.[12] Cumulated employment growth, included as a proxy for firm-level productivity growth, is not found to affect the probability of wage increase.[13] In addition, we find that the size of the previous wage change is negatively related with the probability of wage increase. The third and fourth columns of Table 3 show estimates from a model specification that incorporates determinants of wage adjustment suggested by models of nominal rigidities with costly information. First, in column three, we include cumulated inflation and change in unemployment during the previous wage spell, as a measure of the state of the economy at the time wages were last adjusted. Cumulative inflation during last spell has a positive effect on the probability of wage change in the current period, whereas the effect of unemployment, although negative, is not statistically significant. The estimates of the combined model, reported in column four, present clear evidence of state-dependent behavior. The probability of wage increase is related positively to current inflation but negatively to current unemployment. Overall, these results imply that the timing of wage changes has a strong link to macroeconomic conditions, not only as shocks accumulate over the wage spell, but also through contemporaneous effects of large shocks. Our findings of state-dependency of the timing of wage adjustments are consistent with the inattentiveness model, but more strongly so with the menu cost model and models with availability of costless information.

Table 4 presents probit model estimates for the case of wage decreases. Unlike with wage increases, no stark seasonal pattern is revealed, although wages appear more likely to decrease in January, as well as in November. Shorter wage spells, lasting three months or less, are more likely to end with a wage decrease than spells lasting more than a year. This could be interpreted as evidence of short-term wage adjustment; i.e., wage increases that are reversed after a short period. According to our results, macroeconomic shocks trigger an immediate response in wage cuts. Contemporaneous unemployment and inflation influence the frequency of wage decreases, whereas, unlike wage increases, a cumulative change in macroeconomic conditions over the current and past wage spells does not have a significant effect.[14]

The reader might wonder whether and how the econometric results of state-dependent wage adjustment are influenced by the macroeconomic instability during the 2008 financial crisis and the Great Recession. To explore the sensitivity of the results, the model was estimated on a sample excluding the years 2008–2010. Results are provided in Tables A.10 and A.11 in Online Appendix D. Although the estimated effect of cumulative unemployment on wage increases is weaker in the shorter sample, the effect of cumulated inflation is substantially stronger. This supports our previous conclusion on state-dependency. However, the results for wage decreases are driven by responses to the macroeconomics shocks in the beginning of the Great Recession. Interestingly, when the Great Recession is excluded from our sample, there is a considerably stronger effect from macroeconomic conditions at last adjustment on the current probability of wage increase.

[12] In Table A.12 in Online Appendix D we include the Central Bank of Iceland's policy interest rate in the set of macroeconomic variables. In March 2001, the Central Bank was granted instrument independence and the current inflation targeting monetary policy was introduced, replacing a fixed exchange rate policy. This restricts the estimation period for this empirical specification to April 2001–December 2010. According to our results, a 1 percentage point increase in cumulative change in the interest rate reduces the probability of a wage increase by 0.7 percentage points.

[13] Boivin et al. (2009) find that prices appear stickier in response to macroeconomic disturbances then to sector-specific shocks. Due to data limitations, we are unable to study sensitivity to firm- or sectoral-level variation other than changes in firm-level employment growth.

[14] In Online Appendix C, we estimate a selection model of wage change that allows us to explore jointly the determinants of both probability and size of wage adjustment. In line with economic intuition, cumulative change in inflation has a positive effect on the size of wage change, whereas cumulative unemployment has a negative effect.

Table 4
Probit Estimates of the probability of wage decrease.

Variable	(1)		(2)		(3)		(4)	
	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect	Probit coefficient	Marginal effect
January	0.296***	0.005***	0.282***	0.004***	0.283***	0.004***	0.291***	0.004***
	(0.059)	(0.001)	(0.061)	(0.001)	(0.061)	(0.001)	(0.059)	(0.001)
February	−0.106**	−0.002**	−0.129**	−0.002**	−0.123**	−0.002**	−0.178***	−0.003***
	(0.049)	(0.001)	(0.051)	(0.001)	(0.050)	(0.001)	(0.052)	(0.001)
March	0.050	0.001	−0.046	−0.001	−0.070	−0.001	−0.052	−0.001
	(0.082)	(0.001)	(0.081)	(0.001)	(0.084)	(0.001)	(0.080)	(0.001)
April	0.076	0.001	−0.010	−0.000	−0.015	−0.000	0.056	0.001
	(0.077)	(0.001)	(0.081)	(0.001)	(0.083)	(0.001)	(0.075)	(0.001)
May	0.043	0.001	−0.006	−0.000	−0.007	−0.000	0.113	0.002
	(0.072)	(0.001)	(0.075)	(0.001)	(0.077)	(0.001)	(0.071)	(0.001)
June	0.072	0.001	0.035	0.001	0.034	0.000	0.147**	0.002**
	(0.075)	(0.001)	(0.078)	(0.001)	(0.079)	(0.001)	(0.071)	(0.001)
July	0.026	0.000	−0.014	−0.000	−0.017	−0.000	0.104*	0.001*
	(0.061)	(0.001)	(0.062)	(0.001)	(0.065)	(0.001)	(0.056)	(0.001)
August	0.044	0.001	0.004	0.000	−0.005	−0.000	0.049	0.001
	(0.079)	(0.001)	(0.080)	(0.001)	(0.081)	(0.001)	(0.072)	(0.001)
September	0.188**	0.003**	0.149	0.002*	0.149	0.002*	0.213**	0.003***
	(0.088)	(0.001)	(0.093)	(0.001)	(0.094)	(0.001)	(0.084)	(0.001)
October	0.309*	0.005*	0.290	0.004*	0.299*	0.004*	0.414**	0.006**
	(0.172)	(0.003)	(0.178)	(0.003)	(0.181)	(0.002)	(0.171)	(0.002)
November	0.286***	0.004***	0.279***	0.004***	0.287***	0.004***	0.344***	0.005***
	(0.100)	(0.001)	(0.100)	(0.001)	(0.102)	(0.001)	(0.095)	(0.001)
Cumulative change in unemployment, current spell	0.011	0.000	0.013	0.000	0.015	0.000	−0.012	−0.000
	(0.011)	(0.000)	(0.011)	(0.000)	(0.012)	(0.000)	(0.012)	(0.000)
Cumulative inflation, current spell	0.000	0.000	0.009*	0.000	0.008	0.000	0.013**	0.000**
	(0.006)	(0.000)	(0.006)	(0.000)	(0.006)	(0.000)	(0.006)	(0.000)
Cumulative growth in firm size	−0.002**	−0.000**	−0.002**	−0.000**	−0.002**	−0.000**	−0.002**	−0.000**
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Size of last change	0.008***	0.000***	0.007***	0.000***	0.007***	0.000***	0.007***	0.000***
	(0.002)	(0.000)	(0.002)	(0.000)	(0.002)	(0.000)	(0.002)	(0.000)
Cumulative change in unemployment, last spell					−0.004	−0.000	−0.004	−0.000
					(0.008)	(0.000)	(0.008)	(0.000)
Cumulative inflation, last spell					−0.010**	−0.000**	−0.009**	−0.000**
					(0.004)	(0.000)	(0.004)	(0.000)
Change in current unemployment							0.193***	0.003***
							(0.032)	(0.001)
Current inflation							−0.074***	−0.001***
							(0.016)	(0.000)
Duration dummies	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Wald test	0.000		0.000		0.000		0.000	
Log pseudolikelihood	−69,217		−68,826		−66,169		−65,866	
Observations	1,858,508	1,858,508	1,858,508	1,858,508	1,824,028	1,824,028	1,824,028	1,824,028

Notes: The probit model is estimated by maximum likelihood. Robust standard errors, clustered at the firm level, are in parenthesis. All specifications include variables controlling for age, age^2, experience, tenure, education, gender, foreign citizenship, as well as sets of firm size, industry, and occupation dummies. Marginal effects on the probability of a wage increase of one unit increase in each variable are evaluated at the sample average. Standard errors of marginal effects are calculated using the delta method. The hypothesis that monthly coefficients are jointly zero is tested with a Wald test. Complete estimation results are presented in Table A.17 in Online Appendix D.

* Significance at the 10 percent level.

** Significance at the 5 percent level.

*** Significance at the 1 percent level.

This suggests that information friction may play a larger role, relative to adjustment costs, in explaining infrequent wage changes during periods of more stable economic conditions.

6. Comparison with previous microdata studies

A key result that emerges from our analysis is that, despite a similar degree of nominal wage rigidity, our results about the underlying wage-setting mechanism are very different from what is found in previous microdata studies. In order to establish this, we first describe how our findings fit a set of stylized facts about the degree of nominal wage rigidity. Next, it is highlighted how our results contradict previous results about the nature of this friction. Finally, it is demonstrated how the quality of the data is necessary for establishing the main results.

Lünnemann and Wintr (2009) report evidence on nominal wage rigidity using monthly data from Luxembourg. After extensive cleaning of the dataset to limit measurement error, they report a monthly frequency between 9 and 14 percent, compared to 57% in their raw dataset. Their measure is reduced to 4.8 percent when a multiple break test is applied, implying that measurement problems affect their overall results. Using quarterly data for France, Le Bihan et al. (2012) report a 38 percent quarterly frequency of wage change. For comparison, our measured monthly frequency of wage change can be converted to a mean quarterly frequency of 34 percent.[15] Barattieri et al. (2014) estimate the frequency of wage changes in the US using data from a survey conducted among workers every four months. They report a quarterly frequency of within-job wage changes between 16.3 and 21.6 percent after having adjusted for measurement errors using a break-point test.[16] This result for the less unionized US labor market is similar to the 18.3 percent quarterly frequency measured in our data when excluding union settlement changes.

We find qualitative similarities with three other stylized facts reported in previous studies. First, wage adjustment is synchronized, either in January, as is reported in Lünnemann and Wintr (2009), or in the third quarter, as is reported in Le Bihan et al. (2012). However, Barattieri et al. (2014) do not find evidence of much seasonality in US data. Although this may reflect institutional differences between the European and the US labor markets, Olivei and Tenreyro (2007, 2010) have argued that adjustments of wages and prices in the US are more likely to occur in the second half of the year. Second, there is some heterogeneity, albeit limited, across sectors and occupations, but, as is reported in Le Bihan et al. (2012) and Lünnemann and Wintr (2009), there are clearer differences across firm size, as wages are found to change more frequently within larger establishments. Third, both Le Bihan et al. (2012) and Barattieri et al. (2014) estimate a hazard function of wage change that displays a distinctive spike after one year of duration.

Despite the fact that our results are consistent with stylized facts about the degree of nominal wage rigidity, our results contradict previous conclusions about the nature of this friction.[17] First, wage cuts are rare but their frequency is sensitive to macroeconomic shocks, contradicting the general notion of downward nominal wage rigidity. Le Bihan et al. (2012) and Barattieri et al. (2014) report a low frequency of nominal wage cuts and right-skewed size distribution, which they interpret as evidence for downward rigidity of nominal wages. Second, ours is the first study to provide microdata evidence for state-dependent wage-setting.[18] Le Bihan et al. (2012) estimate a model for the probability of wage changes similar to our baseline specification. In a regression without duration dummies, they find a positive effect of cumulated inflation and a negative effect of cumulated unemployment on the probability of wage increases, but when duration dummies are included, the signs of the two coefficients are reversed and no longer conform to economic intuition. The authors' interpretation of the results is that, as long as time-dependency is accounted for, there is no evidence of state-dependency. When comparing our results to those in Le Bihan et al. (2012), we emphasize two features of their dataset. First, using quarterly rather than monthly data reduces the number of observations for each spell and the accuracy of measuring the actual timing of wage adjustments. Second, the macroeconomic variability in their data is very low, particularly in comparison with that in our dataset. During 1998–2005, inflation in France remained at a low level, around 2%, and unemployment remained persistent and high, fluctuating between 8% and 11%. As a result of those features, we believe that it may be difficult to identify state-dependency empirically, even though the timing of wage adjustments may depend on the state of the economy.

In order to assess the importance of the qualities of our data, two exercises were performed. The results from these exercises are reported in Online Appendix D. First, the dataset was collapsed to a quarterly frequency.[19] The quarterly

[15] The monthly frequency, f_m, is converted to quarterly frequency, f_q, using the formula $f_q = 1-(1-f_m)^3$.

[16] Note that Barattieri et al. (2014) use surveys conducted at the individual level, unlike the firm-level data used in this paper and in both Lünnemann and Wintr (2009) and Le Bihan et al. (2012). This allows them to study between-job wage changes, which they find substantially more frequent than within-job wage changes.

[17] We emphasize that although the measurement error-adjusted frequency reported in other studies is similar to our measured frequency, their unadjusted measures are substantially larger, revealing extensive measurement errors.

[18] Recent studies using European survey data provide evidence of state-dependent wage-setting behavior. Druant et al. (2009) find that wage-setting in 15 European countries has both a time component and a state component: more than half of firms report that they change wages in a specific month, and one-third of firms have an internal policy of adapting wages to inflation. Montornés and Sauner-Leroy (2009), using survey data for French companies, find evidence of state-dependent and backward-looking wage-setting behavior.

[19] Wages in the last month of each quarter are set to represent average monthly wages in that quarter. This specification was chosen to simulate the structure of the dataset used by Le Bihan et al. (2012), which is survey data collected in the end of each quarter. However, the results are not sensitive to this specification, and using, e.g., the first month in each quarter generated very similar results.

frequency of wage change is measured at 37 percent, which is similar but slightly greater than was obtained under the assumption of constant monthly frequency throughout the quarter. Also, similar to what was found in the monthly data, wage changes are most frequent in the first quarter, and the estimated hazard function displays a spike after four quarters. The empirical model was then estimated using the quarterly aggregated data. First, when duration dummies are excluded, there is a negative effect of cumulative unemployment on the probability of wage increase and a positive effect of cumulative inflation. However, similar to what is found in Le Bihan et al. (2012), when elapsed duration is accounted for, the sign of the coefficient on cumulated inflation is reversed, contradicting economic intuition and our main results. In the fully specified model, the coefficients of cumulative inflation and unemployment, both over the current and last spells, have opposite signs compared to our main results. Overall, the results on state-dependent behavior are therefore either less clear or contradictory to our main results.

As a second exercise, the measure of base wages was reconstructed without making use of the distinction between daytime and overtime hours and compensation by erroneously taking the sum of daytime and overtime pay and divide by total hours.[20] An important measurement issue in previous studies, such as Le Bihan et al. (2012) and Lünnemann and Wintr (2009), is that the data used do not explicitly report overtime hours and overtime compensation. As Lünnemann and Wintr (2009) emphasize, this may lead to an upward bias in the frequency because, if overtime hours are remunerated at different rates than daytime hours, changes in working hours will lead to a measured change in the average hourly wage. The results demonstrate that not being able to distinguish between daytime and overtime hours generates substantial measurement error. Using the erroneous measure, the monthly frequency of wage change increases to 55 percent, with the relative shares of increases and decreases being 60:40. The average wage-spell duration is substantially reduced by this measurement error, resulting in a downward-sloping hazard function. Similarly, Barattieri et al. (2014) estimate a downward-sloping hazard function when using their original reported dataset. In addition, the empirical model was estimated using the erroneous wage measure, which results in lower coefficient estimates and reduced statistical significance compared to our main results.

7. Conclusion

In this paper, we use unique administrative microdata to provide new insight into nominal wage rigidity at the microeconomic level. Several stylized facts emerge from our analysis. The monthly frequency of wage change is 12.9 percent but drops to 6.5 percent when wage changes due to union settlements are excluded. Wage changes are synchronized, with half of changes taking place in January and other changes staggered over the course of the year. A mass of spells has a duration of exactly one year, but there is also ample heterogeneity in the distribution of wage spell duration. Infrequent wage changes observed in the data may be rationalized by models in which the timing of wage changes is a function of time or the state of the economy. Consistent with existing evidence, we find evidence of time-dependent wage adjustment. However, contradicting previous studies, we also find strong evidence for state-dependent behavior, as cumulated inflation and unemployment over current and past wage spells are important determinants of the timing of wage changes. Furthermore, the frequency of nominal wage cuts is responsive to macroeconomic shocks.

In the New Keynesian framework, the workhorse for the analysis of monetary policy, rigidity of nominal wages plays a critical role in explaining variations in output and employment. The model generally features the simplification that although the size of wage changes is an outcome of optimization, the timing is exogenous and not affected by policy or the economic environment. It is clear from our results that frequency of wage changes is determined endogenously in the economy. The failure of macroeconomic models to account for this feature could result in false conclusions about the real effect of monetary policy. Our results therefore convince us of the need for further investigation, both theoretical and empirical, of the mechanisms governing the timing of wage adjustment.

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[20] Note that in the measure of base wages used throughout the paper, both overtime compensation and overtime hours were excluded. The frequency of change in hourly overtime pay – i.e., overtime compensation divided by overtime hours – is 44%.

Appendix A. Supplementary data

Supplementary data associated with this paper can be found in the online version at http://dx.doi.org/10.1016/j.jmoneco.2016.01.001.

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