Do Sticky Wages Matter? New Evidence from Matched Firm-Survey and Register Data*

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Abstract: We study wage rigidity using novel data on contractual wages from a Swiss firm survey, matched with income and the employment history from social security register data. We exploit the discontinuity around the origin of the employee-level wage growth distribution to identify the causal effects of wage rigidities after an unexpected 1% decline of the price level caused by the unexpected removal of an exchange rate floor policy. Locally, that is near the origin of the wage growth distribution, downward nominal wage rigidities cause a 4.5% decline of income and a 0.7 percentage point increase of the unemployment rate. We then construct sampling weights using the social security register data to estimate representative aggregate statistics. In the aggregate, income declines by 0.3% and the number of unemployed persons increases by 1.2%.

JEL classification: E30, E40, E50

Keywords: Downward nominal wage rigidity, income, employment, unemployment, deflation

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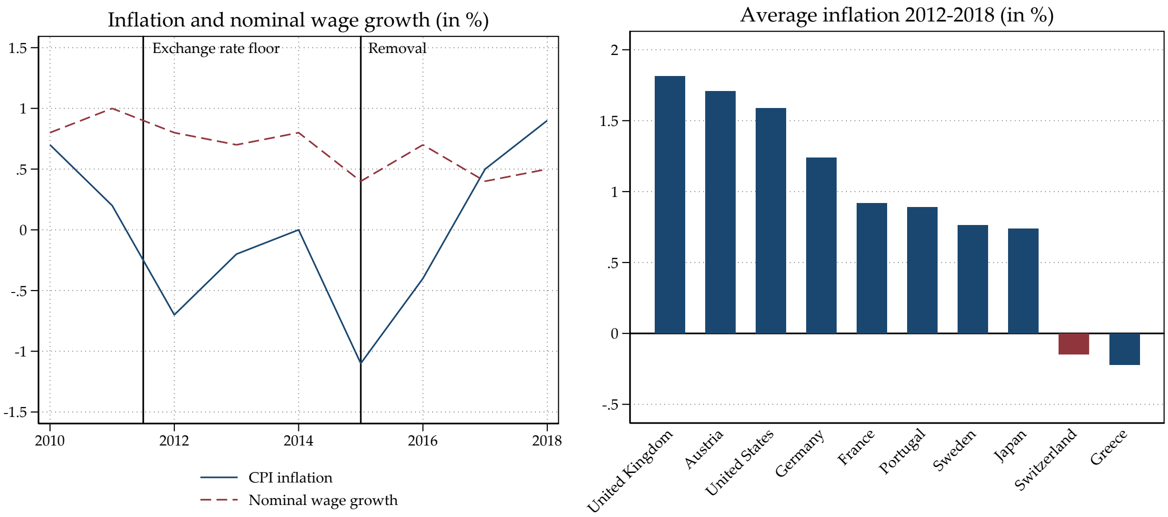
1 Introduction

To model involuntary unemployment and inefficient business cycles, macroeconomists assume that wages are infrequently adjusted (see Erceg et al., 2000; Schmitt-Grohé and Uribe, 2016; Born et al., 2019). Downward nominal wage rigidity, i.e. that wages are rarely cut, is one reason why central banks aim for a positive inflation target (see Tobin, 1972; Issing et al., 2003; Bernanke, 2003). It is controversial, however, whether such wage rigidities exist, and whether they cause economically relevant distortions (see Issing et al., 2003; Basu and House, 2016). Indeed, studying the allocative effects of downward nominal wage rigidity is challenging because most central banks target and achieve positive inflation, such that these rigidities rarely bind.

This paper provides evidence on downward nominal wage rigidities in a deflationary environment. We use a Swiss firm survey on employee's contractual wages—covering almost half of all employees—matched with social security register data on income and employment—covering the universe of the working age population. For identification we exploit the discontinuity around the origin of the employee-level wage growth distribution from the biennial firm survey. First, we define a treatment group (employees with wage freezes) and a control group (employees with small wage cuts, that is flexible wages). Second, we estimate the causal impact of downward nominal wage rigidities on income and labor market outcomes using the matched social security register data. We compare the outcomes of the two groups in a difference-in-differences model: We exploit a unique natural experiment, namely an unexpected 1% decline in the price level caused by an unexpected removal of the Swiss National Bank's exchange rate floor policy. We therefore follow a growing number of studies exploiting this 'Swiss franc shock' to measure the impact of an unexpected appreciation on the price-setting behavior of firms and exchange rate pass-through to prices (see Auer et al., 2019, 2021; Bonadio et al., 2020; Kaufmann and Renkin, 2019).

Switzerland is an interesting case to study for several reasons. First, downward nominal wage rigidities are more likely to bind during deflation. The left panel of Figure 1 shows that CPI inflation amounted to 0% in 2014. In the wake of the removal of the exchange rate floor in January 2015, inflation fell to -1% in 2015 and -0.2% in 2016. Meanwhile, aggregate nominal wages continued to increase. Second, Switzerland experienced particularly low inflation in international comparison for a prolonged period (see right panel). This matters because it has been argued that downward nominal wage rigidities may vanish in a persistent low-inflation environment (see, e.g., Issing et al., 2003). Third, Switzerland's labor market is relatively flexible.[1] Therefore, downward nominal wage rigidities are not mainly caused by legal provisions (see Duarte, 2008, for an example on Portugal). In particular, there is no federal minimum wage;[2] minimum wages introduced by single cantons are relatively low (see Berger and Lanz, 2019); only 20% of the working age population is subject to a (sectoral or cantonal) minimum wage agreement; finally, only 13% of the working age population is member of a labor union.[3]

Figure 1 — Inflation and wage growth



Source: SFSO, OECD, own calculations, see Table C.10 in Online Appendix C.

Our main findings can be summarized as follows. 7.7% of all employees experience a biennial base wage freeze in 2014. Despite slightly negative CPI inflation, base wage increases (70.9%) are more frequent than base wage cuts (21.4%). Total wages, which include irregular payments such as bonuses, are less rigid. Only 2.3% of employees' total wages are freezes, while 34% are wage cuts. Although total wages are more flexible than base wages, the latter make up for a larger share of total compensation. The base income accounts on average for

[1]See Figure C.1 in Online Appendix C for a comparison of labor market indexes between countries from the OECD.

[2]Even employees working in the public sector may experience wage cuts up to -4% (see Tages-Anzeiger, 2019).

[3]These data are for 2018 and 2019 and stem from www.bfs.admin.ch/bfs/de/home/statistiken/arbeit-erwerb/erwerbstaetigkeit-arbeitszeit/erwerbspersonen/eintritte-austritte-erwerbsbevoelkerung.html, www.bfs.admin.ch/bfs/de/home/statistiken/arbeit-erwerb/gesamtarbeitsvertraege-sozialpartnerschaft/lohnverhandlungen.html, www.bfs.admin.ch/bfs/de/home/statistiken/arbeit-erwerb/gesamtarbeitsvertraege-sozialpartnerschaft/gewerkschaften.html (accessed on 12/11/2020).

91% of employees' income, as well as firms' payroll payments. Therefore, base wage rigidities affect a substantial fraction of firms' labor costs. For this reason, we identify employees with wage freezes using the base wage rather than the total wage.

We then estimate the impact of these base wage rigidities on income and unemployment in the wake of the Swiss franc shock. Locally, that is at the origin of the base wage distribution, the difference between the treatment group and the control group is large. Compared to the control group, income and employment income decline by 4.5% and 11%, respectively. Moreover, the probability of becoming unemployed is 0.7 percentage points higher for the treatment group. Because only 7.7% of the population is affected by base wage freezes, however, these results are not representative for the entire economy. Therefore, we use the difference-in-differences model to make local counterfactual predictions, which we then aggregate to representative aggregate statistics. Downward nominal wage rigidities have relevant aggregate effects. They reduce aggregate income and employment income by 0.3% and 0.9%, respectively, and increase the number of unemployment persons by 1.2%.

These results are robust to controlling for a variety of individual, firm, and sector characteristics. First, in the baseline estimates, we control for time effects interacted with dummies for firms, contract type, job type, education, gender, employment status, and whether the individual changed employer in 2014 or 2013. Second, as a robustness test, we control for time effects interacted with percentiles of the wage level distribution, so that we identify the effects from variation between individuals with similar wages.

To the best of our knowledge, this paper is the first to provide local and representative aggregate evidence on the causal effects of downward nominal wage rigidities in a deflationary environment. It is closely related to Fehr and Goette (2005), de Ridder and Pfajfar (2017), and Kurmann and McEntarfer (2019). Fehr and Goette (2005) also provide evidence on the relationship between downward nominal wage rigidities and unemployment in Switzerland. de Ridder and Pfajfar (2017) also measure the effect of downward nominal wage rigidities in response to plausibly exogenous macroeconomic shocks. Kurmann and McEntarfer (2019) also exploit administrative worker-level data. What sets our paper apart from Fehr and Goette (2005) and de Ridder and Pfajfar (2017) is that we exploit employee-level rather than regional information to identify the impact of wage rigidities. Our paper also differs from Kurmann and McEntarfer (2019) because it provides representative aggregate evidence for Switzerland rather than evidence for one U.S. state. Finally, we study the impact of an outright deflationary shock while the other studies focus on periods with relatively low, but positive, inflation

In what follows we first provide an overview of the existing literature. Then, we present the data set, sampling issues, and descriptive statistics. After explaining the identification and estimation strategy, we present the results. The last section concludes.

2 Literature

Whether wages are sticky or flexible matters for macroeconomic models and optimal monetary policy. Infrequent nominal and real wage adjustments are a popular assumption to generate real effects of monetary policy and inefficient business cycles (Erceg et al., 2000; Blanchard and Galí, 2010). Moreover, downward nominal wage rigidity is one reason why central banks aim for positive inflation (Billi and Kahn, 2008; Kim and Ruge-Murcia, 2009). A somewhat higher inflation target facilitates real wage cuts during recessions and therefore mitigates the adverse effects on the labor market (Tobin, 1972; Akerlof et al., 1996; Schmitt-Grohé and Uribe, 2013). Indeed, downward nominal wage rigidity has become a popular feature in monetary macroeconomic models, in particular because inflation has remained subdued in the wake of the Global Financial Crisis (Schmitt-Grohé and Uribe, 2016; Born et al., 2019).

There are two early strands of literature providing empirical evidence on nominal wage rigidity (see Basu and House, 2016, for an overview).[4] The first strand uses aggregate or sectoral time series to document that nominal wages hardly fall, and real wages increase, during severe recessions (see e.g. Eichengreen and Sachs, 1985). Whether real wages are counter-cyclical, however, depends on the time period (Basu and Taylor, 1999), as well as on the nature of the macroeconomic shock (Sumner and Silver, 1989).

The second strand analyzes disaggregate wage or income data from household surveys (see Bils, 1985; Solon et al., 1994; McLaughlin, 1994; Kahn, 1997; Card and Hyslop, 1997; Altonji and Devereux, 2000; Fehr and Goette, 2005). These surveys suffer from reporting error; therefore, accounting for measurement error in reported wages is key. Most studies therefore attribute small wage changes to wage freezes (e.g. Bauer et al., 2007). Other studies prefer to statistically clean individual wage series from measurement errors (Gottschalk, 2005; Barattieri et al., 2014). These studies find that, after accounting for measurement errors, wage rigidity is important in

[4]Following the seminal work by Bewley (1999) a third strand asks firms whether and why they are hesitant to adjust or cut wages. The ECB's Wage Dynamic Network has assembled large cross-country surveys to analyze wage, price, and employment adjustments to shocks (Bertola et al., 2012).

the U.S. and Europe (Bauer et al., 2007; Barattieri et al., 2014).

Another possibility to avoid the measurement error problem is to obtain more accurate data from personnel files, firm surveys, register data, or firms' payroll data (see, e.g., Knoppik and Beissinger, 2003; Fehr and Goette, 2005; Le Bihan et al., 2012; Jardim et al., 2019; Elsby and Solon, 2019). Personnel files come with the downside that they may not be representative for the entire economy. Register data on income are more accurate and representative, but often lack working hours. Therefore, wage changes may be an artifact of changes or measurement errors in working hours. More recent studies emphasized the advantages of firm survey data. They are regarded to be more accurate than household survey data, and comprise detailed information on income, working hours, as well as socio-economic characteristics of the employees.

To identify the allocative consequences of downward nominal wage rigidity, most studies compare regions, sectors, firms, or time periods where downward nominal wage rigidity binds to varying degrees. Fehr and Goette (2005) and Bauer et al. (2007) find that higher wage rigidity is associated with higher unemployment across Swiss and German regions, respectively. Kurmann and McEntarfer (2019) compare U.S. firms with different degrees of wage rigidity during the Global Financial Crisis. Firms with rigid wages reduce employment by 1.2% relative to those with flexible wages. Similarly, Pischke (2018) analyzes adjustment in different segments of the U.S. housing sector in response to the burst of the U.S. housing bubble. Similarly, Ehrlich and Montes (2020) show that German firms with higher wage rigidity exhibit higher layoff rates. They use the share of workers with collectively bargained wages as an instrument to account for potential endogeneity of their wage rigidity variable. A few studies measure the impact of wage rigidity to plausibly exogenous shocks. de Ridder and Pfajfar (2017) combine regional variation in wage rigidity in the U.S. with aggregate monetary and fiscal policy shocks. They find a stronger impact of monetary policy shocks on real activity in states with sticky wages compared to states with flexible wages. Faia and Pezone (2018) provide evidence that monetary policy announcements induce higher volatility in stock returns for Italian firms that are more constrained by legally fixed wages. Finally, Kaur (2019) measures the response of Indian districts with varying degree of wage rigidity to exogenous rainfall shocks.

The view that wage stickiness is a pervasive phenomenon and has allocative consequences is controversial. First, evidence from accurate payroll data and firm surveys suggests that wages are more flexible than previously thought (Elsby and Solon, 2019). Wage cuts are rare only when legally prohibited or in environments with high inflation.[5] Second, total wages are more flexible than base wages because of bonus payments (Altonji and Devereux, 2000; Nickell and Quintini, 2003; Babecký et al., 2019; Grigsby et al., 2019; Kurmann and McEntarfer, 2019). Therefore, bonus payments are an additional margin firms may use to cut nominal wages during recessions. Third, downward nominal wage rigidity may be the result of an optimal implicit contract between the employee and the firm and thus may not have allocative effects. According to Barro (1977), firms' marginal cost of labor depends on the present discounted value of all wage payments during the duration of the contract. But this present value may differ from the current level of the wage. Finally, the allocative effects may be small because firms optimally compress wage increases as well as decreases when wage rigidities are present (Elsby, 2009; Stüber and Beissinger, 2012). Finally, if the wage setting behavior of firms is state dependent, wages may be flexible when it matters. For example, Issing et al. (2003) argue that downward nominal wage rigidities may vanish in a deflationary environment. In addition, Grigsby et al. (2019) find wages are more downward flexible during recessions.

What sets our paper apart is that we analyze an accurate representative data set combining firm-survey and register data. These data allow us to base our identification scheme on the employee-level wage growth distribution and follow income and the employment history of these employees over time. In addition, we focus on a time period with mild deflation and a plausibly exogenous deflationary shock, leading to a 1% decline in the price level. We are therefore able to examine whether downward nominal wage rigidities have allocative effects when they become effectively more binding, or, whether these rigidities vanish in a deflationary environment.

3 Data

We use a biennial firm survey matched at the employee level to social security register data (see Figure 2).[6] Both data sets comprise an anonymous identifier based on the social security number. Because the social security data cover the entire Swiss working age population, we

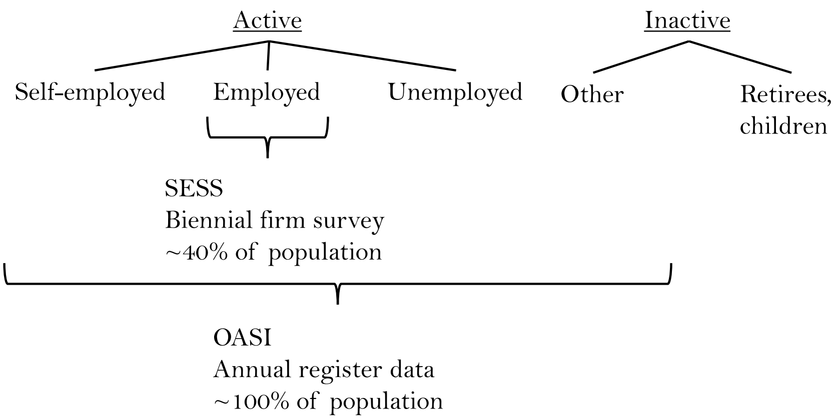
[5]For example Duarte (2008) emphasizes the role of legal restrictions for downward nominal wage rigidities in Portugal.

[6]Table C.10 in Online Appendix C provides information on the data sources. The data resemble the ideal described in Fehr and Goette (2005): "The ideal data set for examining nominal wage rigidity would be a representative sample of firms' personnel files including precise information on wages, individuals' productivity, and other individual characteristics. Unfortunately, there is no study with such a data set to our knowledge."

can match virtually all observations from the firm survey to the social security data.[7]

We use the firm survey primarily to compute the employee-level wage growth distribution because we can construct a measure of the contractual wage. In addition, it comprises a range of socio-economic, contract, firm, and sector characteristics that allow us to analyze various dimensions of the Swiss labor market. The social security data serve two purposes. First, the data allow to measure individuals' annual income from all occupations and the employment history. Second, because it covers the entire working-age population, we can use these data to correct for sample selection issues.

Figure 2 — Structure of the data



Notes: The braces indicate the population of the firm survey (SESS) and the social security register data (OASI), respectively.

3.1 Swiss Earnings Structure Survey

The Swiss Earnings Structure Survey (SESS) is a biennial firm survey conducted by the Swiss Statistical Office (SFSO). We obtained three waves for 2012, 2014, and 2016. Each wave comprises about 1.6 mio. individuals, that is 40% of all Swiss employees (see Figure 2).[8] Because the data is provided by firms on a biennial basis, we regard the data to be of very high quality.

The SFSO chooses firms according to a stratified sampling scheme. Once a firm is chosen to be in the sample, participation is mandatory.[9] Firms can chose between a paper-based and

[7]There are a few observations that we cannot match. We suspect that this is due to reporting error.

[8]More precisely, employees at firms with at least 3 employees in the secondary and tertiary sector (Swiss Federal Statistical Office, 2018).

[9]However, the response rate is 82% in 2012 and decreases to 73% in 2016 (Swiss Federal Statistical Office, 2016, 2018).

an online questionnaire, or submit the information directly via an electronic interface. About half of the firms in the SESS report with the paper survey.[10] Medium (large) firms can choose to report every second (third) employee. If they do so, they are advised to randomize the selection. Nevertheless, about 75% of medium and large firms report all employees.

Firms are asked to provide employment income and working hours for October. They report various income components: base income, 13th month pay, bonus payments, pay for Sunday/night work, and overtime payments.[11] Firms report either the contractually agreed or the actual number of working hours.[12] In addition, the survey comprises detailed information on contract, employee, and firm characteristics. The SFSO validates and completes some of these characteristics with register data.

To compute the contractual wage, that is income at unchanged working hours, we exploit that the survey comprises actual income as well as a standardized full-time-equivalent income. We compute a standardization factor by dividing the full-time-equivalent income by the actual income.[13] If this standardization factor changes compared to 2014, we standardize the incomes in 2012 and 2016 to the factor in 2014.[14]

We apply the same standardization procedure to all income components and aggregate them to four different contractual wage measures. The total wage includes all payments net of social security contributions. The irregular wage includes bonus payments, payments for Sunday/night work, as well as payments for overtime. The regular wage amounts to the total wage net of irregular payments. The base wage corresponds to the regular wage without 13th month payments.

We can follow individuals over time because of an anonymous identifier based on the social security number. The firm identifier, however, is randomized in each wave. Therefore, we construct a proxy of whether an employee stayed at the same firm using information on tenure. If tenure increases by two years between each wave we assume that a person stays at the same

[10]In e-mail correspondence, the SFSO explained that in 2012 57% of firms used the paper survey. This share declined to 45% in 2016. The remaining firms used an electronic survey or directly transmitted the information via electronic personnel files.

[11]In Switzerland, some work contracts specify that the salary is payed in 13 installments, so that workers receive a 13th monthly payment in December.

[12]Firms can decide whether they report the working hours specified in the contract or the working hours the employee in fact worked during the year. For example, Swiss law permits that working hours do not have to be recorded for some, mostly high-income, jobs. In these cases, the firm cannot report the actual working hours.

[13]A change in the standardization factor may stem from changing agreed working hours (activity level) or changing actual working hours.

[14]We only do this if the change in the standardization factor is larger than 0.1% to avoid spurious changes in the activity rate. The reason is that the standardization by the SFSO is based on reported working hours, which may be subject to reporting error.

firm during this period. This is only a proxy for job stayers for two reasons. First, an employee may loose its job and then be rehired by the same firm. Second, an employee may change its job within the same firm.

We impose the following sampling decisions. Because workers can have multiple occupations, we observe some individuals twice in each wave. If this is the case, we drop the observation with a temporary contract (0.7% of the sample). If both contracts are permanent we drop the observation with the lower base income (2% of the remaining sample). We also drop the agriculture sector (0.01% of the sample). We remove a few observations with a negative income, which are likely due to reporting error (0.07% of the sample). Finally, we perform an outlier detection procedure using information from the presumably more accurate social security data (see Online Appendix A for details). We remove all observations from SESS that deviate more than 150% from a prediction based on income observed in the social security data. The share of outliers we remove in each wage declines from 2.2% in 2012 to 1.5% in 2016.

The main advantage of the firm survey is that we can compute the contractual wage growth distribution and observe many socio-economic, contract, firm, and sector characteristics. The firm survey has several disadvantages, however. First, individuals and firms may randomly enter or exit the data, depending on whether a firm chooses to report only a subset of employees, as well as on whether the SFSO chooses the firm to be in the sample. Second, the survey does not comprise the self-employed, the unemployed, or the inactive population. Therefore, the survey comprises no information on the employment history. Third, we only have three biennial waves, which hampers comparisons over time. Finally, because we need two consecutive wage observations to identify wage rigidities, we introduce a sample selection bias. Because of these disadvantages, we match the firm survey to social security data.

3.2 Old Age and Survivors Insurance

The data stem from social security payments for the Old Age Survivors Insurance (OASI). Firms report these data for every employee when they pay social security contributions to the regional or sectoral OASI branches. The Central Compensation Office (CCO) collects the data from the branches and makes them available to researchers. Even if individuals are not employed, they are registered with an OASI branch if they have to pay social security contributions. Social security contributions are due as of age 17 (if working) or age 20 (all

Swiss residents) until retirement at age 65 (64 for women).[15] We obtained data from 2008 to 2016, with about 5 mio. individuals each year.

We compute various outcome variables of interest. First, we compute various income measures taking into account all occupations. Overall income includes income from employment, income from self-employment, unemployment benefits, as well as payments from insurances (e.g. compensation for mandatory military service).[16] Employment income excludes income from self-employment, unemployment benefits, as well as other public insurance receipts. Similarly, we construct unemployment income as all payments from the unemployment insurance. Second, we construct variables that measure the employment history of the individuals. We define an unemployment indicator which equals unity if the individual received unemployment benefits in a given year.[17] In addition, we construct an indicator, which equals unity if an individual receives income from employment or self-employment in a given year. This indicator therefore measures whether an individual is working.

The register data is of very high quality and therefore, we impose few sampling decisions: We replace a very small share of negative incomes with 0 (0.03% of the sample).

The OASI data has several advantages. First, we observe the entire working age population, including inactive individuals with zero income (see Figure 2). Second, we observe incomes from all occupations subject to social security contributions. The so-called relevant salary is very broad and captures many income sources (see Information Center OASI/DI, 2020). For example, the data include income sources not covered by wage surveys, such as insurance receipts after accidents, remuneration of limited partnerships, or daily disability insurance payments. Third, because the data covers the entire working-age population we can exploit this information to construct own sampling weights.

3.3 Sampling weights

Analyzing wage rigidity with SESS data involves several sample selection problems. First, although the SFSO provides sampling weights, they will yield biased statistics if our sampling

[15] In a few cases, we observe individuals that still work during retirement.

[16] We exclude spells due to "splitting" of the income. This happens when the social security contributions of a divorced couple are split in two. By removing these spells, we attribute the income to the individual that earned the income.

[17] Therefore, we only measure individuals that are registered at a regional unemployment office to claim unemployment benefits. It is therefore lower than an unemployment rate that includes individuals not registered with an unemployment office, as defined by the International Labour Organization.

decisions remove observations in a non-random fashion. For example, if smaller firms are more likely to use the paper survey, these data suffer from more serious reporting error, which we remove in the outlier detection scheme. Second, computing the wage growth distribution requires two consecutive wage observations. Therefore, wage growth statistics are based on a sample selecting employees that are more likely to stay in the labor market.

Indeed, our sampling decisions introduce an upward bias in median income, and a downward bias in employment. Table B.1 in Online Appendix B comprises aggregate statistics for median income and employment based on different data sets and weighting schemes. The estimates are biased using the unweighted OASI data, as well as using the SESS data with official sampling weights. Conditioning on observing two consecutive wage observations exacerbates these biases. The median income is even higher, because we select individuals that are more likely to remain in the SESS over an extended period, and because low-income individuals probably face a higher risk of becoming unemployed.

To compute representative statistics we construct own sampling weights. We do so for two different sampling schemes. The first accounts for the sampling decisions and therefore yields weights for computing statistics for income and wage levels in 2014 and 2016. The second additionally accounts for conditioning on two consecutive wage observations and therefore yields weights for computing statistics for wage changes in 2014 and 2016.

The weights are computed as follows (see Online Appendix B for details). For each year and each sampling scheme we estimate a Probit model on the population of OASI data. The dependent variable is an indicator, which equals unity if the individual is included in the corresponding sampling scheme. The independent variables are a set of indicators for 400 percentiles of the employment income distribution, as well as dummy variables for being unemployed and self-employed. The sampling weights are then computed as the inverse conditional probability of being included in the sampling scheme. Online Appendix B shows that these weights yield representative aggregate statistics.[18]

3.4 Descriptive statistics

Because the data have not been analyzed before, and to provide a reference to other studies, this section provides some descriptive statistics.

Previous research has shown that bonus payments, hourly wages, or wages for job movers

[18]See Tables B.1 and B.3.

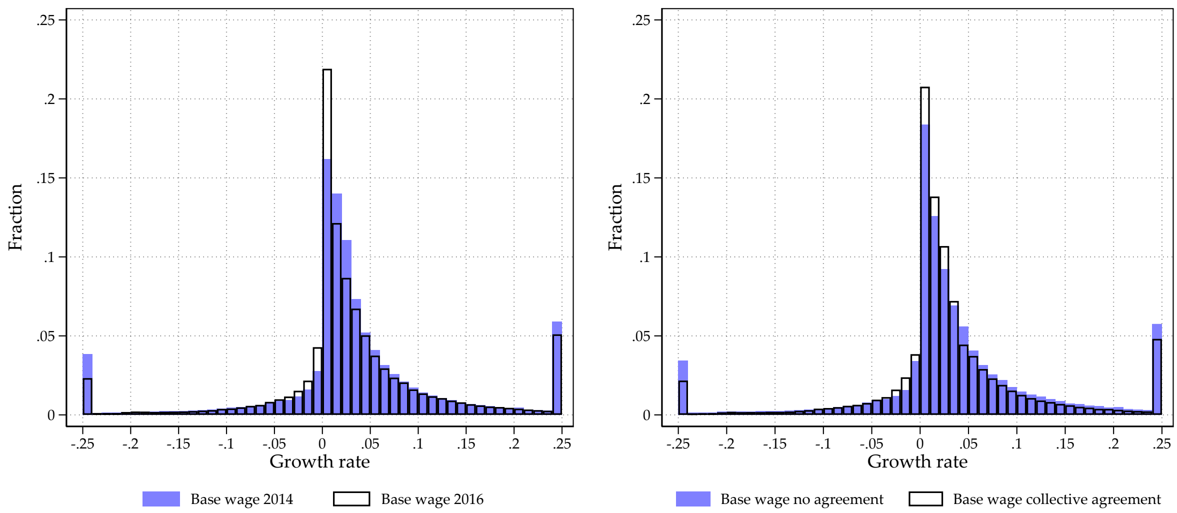
Table 1 — Descriptive statistics matched data set 2014

	Mean	Std.	Min.	Max.
Income (OASI)				
Income (in 1,000)	65.10	72.34	0.00	9,880.27
Employment income (in 1,000)	64.17	72.54	0.00	9,880.27
Unemployment benefits (in 1,000)	0.00	0.00	0.00	0.00
Income and wage (SESS)				
Employment income (in 1,000)	60.05	54.28	0.07	9,031.78
Total wage (in 1,000)	69.31	63.12	0.08	9,704.97
Share of base income	0.91	0.07	0.00	1.00
Share of regular income	0.97	0.06	0.00	1.00
Share of irregular income	0.03	0.06	0.00	1.00
Wage T-2 observed	0.49	0.50	0.00	1.00
Activity and contract				
Tenure at firm (years)	7.83	8.70	0.00	60.00
Manager	0.22	0.42	0.00	1.00
Open-ended contract	0.92	0.27	0.00	1.00
Hourly wage	0.20	0.40	0.00	1.00
Stays at company	0.82	0.39	0.00	1.00
Employee				
Age (years)	40.98	12.71	16.00	81.00
Women	0.52	0.50	0.00	1.00
University degree	0.19	0.39	0.00	1.00
Foreigner	0.28	0.45	0.00	1.00
Firm				
Public company	0.25	0.43	0.00	1.00
Collective agreement	0.42	0.49	0.00	1.00
Small firm	0.13	0.34	0.00	1.00
Medium firm	0.19	0.39	0.00	1.00
Large firm	0.68	0.47	0.00	1.00
Observations matched	1,517,784			
Observations SESS	1,523,987			

Notes: All statistics weighted using own sampling weights. Unless otherwise stated the variables are indicators with values of 1/0.

exhibit less wage rigidity (see e.g. Altonji and Devereux, 2000; Nickell and Quintini, 2003; Babecký et al., 2019; Grigsby et al., 2019; Kurmann and McEntarfer, 2019). We therefore examine how important these dimensions are for Switzerland. Table 1 shows that 91% of employment income stems from the base income.[19] We obtain similar results when calculating the average share of base income in the total payroll of firms (see Table C.1 in Online Appendix C). Irregular income, including bonus payments, accounts for 3% of employment income. This suggests that the base income is the most important component in employees' compensation and firms' labor cost. In addition, only 20% of employees are paid on an hourly basis. Finally, more than 80% of employees stay at the same company over two years, and more than 92% have a permanent contract. This suggests that factors typically associated with wage rigidities (the contractual base wage, staying on a job, permanent contract, monthly pay), play an important role in the Swiss labor market.

Figure 3 — Distribution of biennial base wage growth



Notes: The histograms are winsorized at an absolute biennial wage growth rate of 25%. Sampling weights are not taken into account.

Indeed, the biennial base wage growth distribution provides evidence of downward nominal wage rigidities (see Figure 3).[20] All histograms display a pronounced asymmetry around the origin. Small wage cuts occur less frequently than wage freezes and small wage increases. In addition, the Swiss franc shock leads to a leftward shift of the wage change

[19]Table C.2 in Online Appendix C shows descriptive statistics for 2016.

[20]Figure C.2 in Online Appendix C provides additional histograms for different wage measures, contract types, employees staying at the same company, and employees changing the company. The asymmetry is present along most dimensions. Wages are more flexible for job movers and for the total wage.

distribution in 2016 compared to 2014 (see left panel). After the deflationary shock, the share of wage freezes increases by more than the share of small wage cuts. This suggest that a nominal rigidity prevents wages from falling. Otherwise, we would observe more wage cuts. Splitting the sample between firms with collective and no collective agreements confirms that real rigidities are less important than nominal rigidities (see right panel).[21] Theory and empirical evidence suggest that labor unions care more about real than nominal wages (see, e.g., Babecký et al., 2010). If real rigidities would be important, we would observe more wage cuts for workers with a collective agreement. However, the histograms for the two groups are similar.

Table 2 — Wage rigidity statistics for 2014

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	70.9	21.4	7.7	26.4
Regular wage	67.2	27.3	5.5	16.8
Total wage	63.7	34.0	2.3	6.4
Employment income (SESS)	57.5	41.6	1.0	2.3
Employment income (OASI)	57.7	41.4	1.0	2.3

Notes: Wage rigidity statistics based on biennial wage changes according to different wage components. The regular wage includes the base wage and 13th monthly payments. The total wage includes the base wage, 13th monthly payments, and irregular payments (overtime, Sunday/night, and bonus payments). Wage freezes are defined as growth rates smaller than 0.02% in absolute value. The share of wage cuts prevented is defined as share freezes/(1−share raises).

Table 2 shows that in order to identify downward nominal wage rigidities we need data on working hours and wage components to compute the contractual base wage.[22] The reason is that a flexible bonus component causes small wage cuts even though its share in the total wage is relatively small.

First, we calculate the share of wage freezes, that is wage changes smaller than 0.02% in absolute size. Second, following Dickens et al. (2007), we compute the share of wage cuts prevented as the share of wage freezes divided by the share of wage freezes and cuts. We therefore implicitly assume that all wage freezes would have been wage cuts in the absence of downward nominal wage rigidity.[23] The base wage is the most rigid wage component. 7.7% of all base wage changes are freezes and 26.4% are prevented base wage cuts. These figures are

[21] The SESS measures collective wage agreements at the firm level rather at the employee level. The indicator is unity if most wages of a firm are affected by a collective wage agreement.

[22] Detailed wage rigidity statistics according to different time periods, weighting schemes, socio-economic characteristics, firm characteristics, and sectors are provided in Online Appendix C (Tables C.3 to C.8). The upshot is that downward nominal wage rigidity is a pervasive phenomenon across many dimensions.

[23] Therefore, our measures of wage rigidity include workers that have in principle flexible wages but, by accident, receive a productivity shock such that the firm does not want to change their wage. However, the probability of this occurring is arguably negligible.

similar, although slightly lower than the biennial wage rigidity statistics reported by Fallick et al. (2016) for the U.S. The total wage, which includes bonus payments, 13th month pay and pay for Sunday/night work, is more flexible. Only 2.3% of total wage changes are freezes and 6.4% are prevented wage cuts. If we use income, which does not account for changing working hours, the share of wage freezes and the share of wage cuts prevented is even lower.

We observe more wage increases than decreases in 2014 despite the fact that CPI inflation was slightly negative during this period. This does not come as a surprise, perhaps, because labor productivity growth is on average positive, which explains the higher share of positive (real) wage changes. Because individuals with positive wage changes are likely to have different characteristics than individuals with stagnant or falling wages, we therefore base our identification scheme on data near the origin of the base wage growth distribution.

4 Identification and estimation

To identify the causal effects of downward nominal wage rigidity, we use a plausibly exogenous, unexpected deflationary shock, as well as, information on the base wage growth distribution. This section first describes the Swiss franc shock episode, closely following Kaufmann and Renkin (2019). Then, we explain the identification strategy and the model.

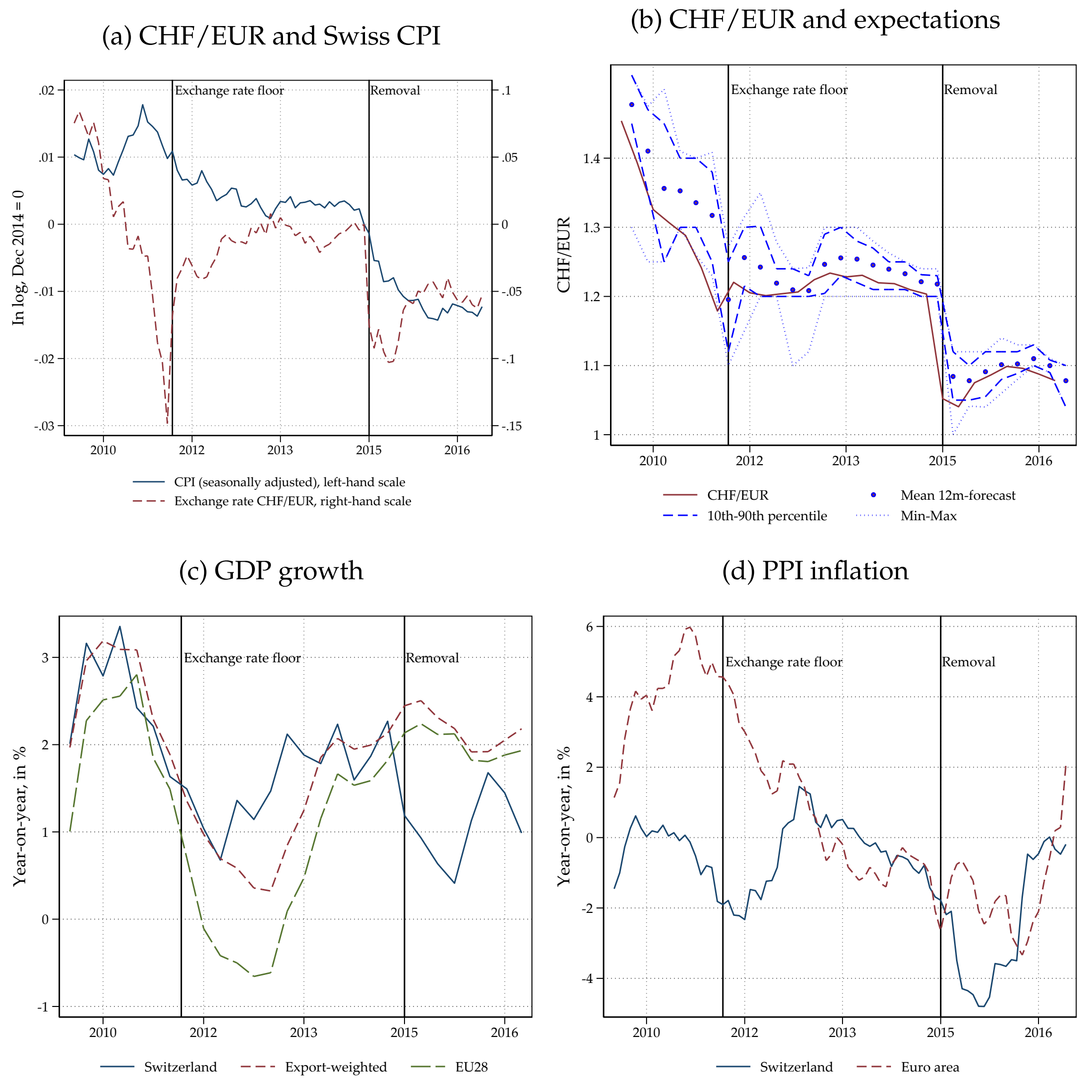
In the wake of the Global Financial Crisis, the SNB lowered its interest rate target close to zero. Because few central banks considered negative interest on reserves at the time, conventional monetary policy was effectively constrained (See SNB, 2009). Because of reserve absorbing operations and the effective lower bound on interest rates, the Swiss franc appreciated by about 30% (see Bäurle and Kaufmann, 2018; Canetg and Kaufmann, 2019). To stop this appreciation trend, the SNB established an exchange rate floor at CHF/EUR 1.20 in September 2011, promising to buy unlimited foreign currency, if necessary. As a consequence, the exchange rate remained close to CHF/EUR 1.20 over the following three years. On 15 January 2015, the SNB removed the floor because increasing pressure on the Swiss franc led to higher and higher exchange rate interventions.

The removal of the exchange rate floor led to an unexpected appreciation of the Swiss franc and a decline in the price level. Panel (a) of Figure 4 shows the Swiss CPI (left-hand scale) along with the CHF/EUR exchange rate (right-hand scale).[24] Indeed, the Swiss franc immediately

[24] Kaufmann and Renkin (2019) show that the Swiss franc appreciated also in trade-weighted terms and against currencies other than the euro.

appreciated by almost 20% and stabilized at a 10% stronger level by the end of 2015. The CPI moved sideways before the removal of the floor. Afterwards, we observe a decline by 1%.

Figure 4 — The Swiss franc shock



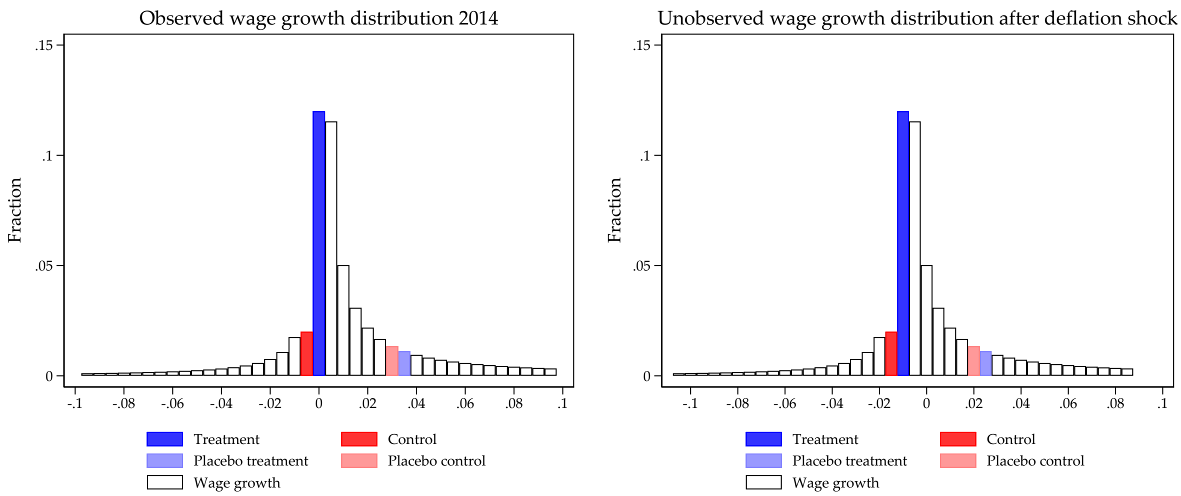
Notes: These figures are reproduced from Kaufmann and Renkin (2019). Panel (a): Monthly CPI and CHF/EUR exchange rate in log and normalized to 0 in Dec 2014. Panel (b): Quarterly CHF/EUR with expectations from the KOF Consensus Forecast, a survey of professional forecasters. Panel (c): Quarterly GDP growth in Switzerland, its main trading partners (export-weighted), and in the European Union. Panel (d): Monthly producer price inflation in Switzerland and the Euro area. Vertical lines denote the introduction and removal of the exchange rate floor at CHF/EUR 1.20. Survey data and export-weighted GDP are retrieved from KOF Swiss Economic Institute. Exchange rate data retrieved from the ECB. GDP and producer price data retrieved from the OECD.

Although most observers believed that the floor was a temporary policy, the end date of the policy was unknown. Indeed, its removal on 15 January 2015 surprised most economists and financial market participants. Panel (b) of Figure 4 shows forecasts of a panel of informed economists participating in the KOF Consensus Forecast, a survey of professional forecasters. One month before the removal of the floor, all survey participants forecast an exchange rate above CHF/EUR 1.20 over the next 12 months. This is also consistent with the high credibility of the floor found in financial market data (see Bonadio et al., 2020).

The international macroeconomic environment was relatively stable before and after the removal of the floor (see panel c). In 2013 and 2014, GDP growth in Switzerland's main trading partners stood at about 2%. After the removal of the floor Swiss GDP growth fell by 1 percentage point in 2015 and 1.5 percentage points in 2016. Meanwhile, GDP growth abroad remained relatively stable. Thus, the analysis is not confounded by a sudden change in foreign demand.

Although the exchange rate floor was introduced in 2011, it took time until relative prices in Switzerland and abroad converged. That is, the appreciation in 2010 and 2011 had relevant effects on prices and competitiveness of Swiss firms well into the exchange rate floor period. Panel (d) shows that producer price inflation in 2012 was lower in Switzerland than in the Euro area. Similarly, Panel (a) shows that the decline in the CPI came to a halt only by 2013. Therefore, we expect the effects of the appreciation before the floor to have an impact on firms well into the exchange rate floor period.

Figure 5 — Stylized depiction of the identification scheme



Notes: The treatment group is defined as individuals with base wage freezes in 2014 (left panel). The control group are individuals with small wage cuts (smaller than 0.5% in absolute value). After the deflationary shock, firms would like to cut wages for individuals with wage freezes (right panel). Because this may not be possible, we do not observe these wage changes, but rather, potential layoffs of individuals with wage freezes in 2014. We can use a comparison at another bin of the wage distribution as a placebo test. For individuals with higher productivity growth, and therefore higher higher real wage growth, the 1% deflation shock requires a smaller wage increase instead of a wage cut.

To identify the causal effects of downward nominal wage rigidities, we compare income and employment of individuals with base wage freezes in 2014 (treatment group) and those with small base wage cuts in 2014 (control group), before and after the Swiss franc shock. We focus on the base wage distribution because base wages are responsible for the asymmetry around the origin of the wage growth distribution. By contrast, irregular payments are much more flexible. By identifying the treatment and control groups using the total wage distribution would therefore select a relatively small sample of individuals that receive no irregular payments.

The left panel of Figure 5 shows a stylized depiction of our identification strategy. We use the base wage growth distribution in 2014 to determine a treatment group and a control group. The key assumption is that individuals with small wage cuts are similar to individuals with wage freezes, except for the wage rigidity. After the Swiss franc shock, the unobserved distribution of desired wage changes shifts to the left. That is, firms would like to cut wages for both, individuals in the treatment and control group. Our hypothesis is that this is not possible for the treatment group and, therefore, firms may instead lay off employees. Because the unemployed are not observed in the firm survey, we use the social security data to compare income and employment of the treatment group and control group before and after the Swiss franc shock.

In addition, we define placebo treatment and control groups using adjacent bins of the base wage growth distribution away from the origin. For individuals with high trend productivity growth the deflationary shock does not shift the desired wage change into negative territory. Therefore, we expect that there is no difference between the placebo treatment group and control group that experience large positive wage changes in 2014. In addition, for adjacent bins in negative territory, there should be no difference because these wages are not subject to a downward nominal wage rigidity.

Having defined a treatment and control group we estimate a difference-in-differences model:[25]

$$\begin{aligned} y_{i,t} &= \sum_{j \notin 2014} \mathbf{1}\{t=j\} \times \Big[\alpha_j \mathbf{1}\{\Delta w_{i,2014}=0\} + \delta_j \mathbf{1}\{\Delta w_{i,2014} < -c\} + \gamma_j \mathbf{1}\{\Delta w_{i,2014} > 0\}\Big] \quad (1) \\ &+ \sum_{j \notin 2014} \mathbf{1}\{t=j\} \times \Big[\mathbf{X}_{i,2014}\beta + \mathbf{Z}_{f,2014}\theta\Big] + \theta_i + \varepsilon_{i,t} \; . \end{aligned}$$

[25] See Bonadio et al. (2020); Kaufmann and Renkin (2017, 2019) for similar approaches.

The dependent variables ($y_{i,t}$) stem from the OASI data set and are available at annual frequency (total income, employment income, unemployment income, unemployment dummy). We saturate the model with time dummies for every year except 2014 ($\mathbf{1}\{t = j\}$), where $\mathbf{1}\{A\}$ denotes an indicator variable that equals 1 if the condition A is true and 0 otherwise. Then, we interact these dummies with a wage freeze dummy ($\mathbf{1}\{\Delta w_{i,2014} = 0\}$), dummies for large wage cuts ($\mathbf{1}\{\Delta w_{i,2014} < -c\}$), dummies for wage increases ($\mathbf{1}\{\Delta w_{i,2014} > 0\}$), and two matrices of control variables capturing observed and unobserved differences that affect selection into treatment at the individual and firm-level ($\mathbf{X}_{i,2014}$, $\mathbf{Z}_{f,2014}$). Finally, we control for individual fixed effects, which capture time constant unobserved characteristics (θ_i) and ε_{it} denotes an error term.

The main coefficients of interest (α_j) measure the impact of wage rigidities using variation for employees working at the same firm with wage freezes and absolute wage cuts smaller than c in 2014. In the main specification we set $c = 0.5\%$. Following Lee and Card (2008), we base inference on standard errors clustered according to the variable exhibiting a discontinuity, that is the unique values of the base wage growth distribution in 2014. Clustering at the firm level yields slightly larger standard errors. But all results are robust with respect to this alternative.

Ideally, the treatment and control groups differ only with respect to the nominal wage rigidity, but not with respect to other characteristics. However, Table C.9 in Online Appendix C shows that the average observed characteristics between treatment and control group are statistically significantly different. The significant differences do not come as a surprise, perhaps, given the large number of observations. In terms of economic relevance, the differences are relatively small. The main exceptions are that workers with wage freezes have a higher income than those with wage cuts, they are more likely to have a management function, they are 2.6 years older, and they are more likely to work in the public sector.

To account for observed differences that affect selection into treatment, the baseline model interacts time dummies with dummies for firms, contract type, job type, education, gender, employment status, and whether the individual changed employer in 2014 or 2013.[26]

[26] As a robustness test, we control for time effects interacted with percentiles of the wage level distribution. This captures that workers with relatively low income may be more likely affected by an implicit or explicit minimum wage. In addition, we include the inverse Mills ratio, which aims to control for unobserved differences that affect selection into treatment (see Appendix D).

5 Causal effects of downward nominal wage rigidity

We first discuss the causal impact of wage rigidities at the origin of the wage change distribution (local effects). Then, we estimate representative aggregate effects, before presenting estimates that control for measurement error in the base wage growth distribution. Finally, we discuss placebo and robustness tests.

5.1 Local effects

Wage freezes have a relevant negative impact on employees in the wake of the Swiss franc shock. Table 3 shows the evolution of total income, employment income, unemployment benefits, and the probability of being unemployed for employees with wage freezes compared to employees with small wage cuts.[27] For all outcomes, the estimates in 2015 and 2016 are statistically significant at conventional significance levels. Meanwhile, the estimates in 2013 are economically small not statistically significant.

Turning to the size of the effects, income declines by 2% and 4.5% in 2015 and 2016, respectively. Employment income even declines by 4 and 11%. Employment income falls more than total income because individuals becoming unemployed receive unemployment benefits. Indeed, by 2016 unemployment benefits increase by 7%, while the probability of becoming unemployed increases by 0.7 percentage points.

Table 3 — Relative effect between individuals with base wage freezes and cuts

	Income (in log)	Employment income (in log)	Unemployment benefits (in log)	Unemployed (1/0)
2013	0.004	0.007	-0.002	0.000
	(0.004)	(0.005)	(0.011)	(0.001)
2015	-0.021***	-0.041***	0.036**	0.004**
	(0.004)	(0.011)	(0.015)	(0.002)
2016	-0.044***	-0.108***	0.070***	0.007***
	(0.005)	(0.019)	(0.020)	(0.002)
Controls	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes
Adj. R-sq. (between)	0.81	0.42	0.33	0.33
Adj. R-sq. (within)	0.00	0.00	0.00	0.00
Observations	3,348,172	3,348,172	3,348,172	3,348,172

Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. The estimates are normalized to 0 in the base year 2014. ***/**/* denotes a statistically significant difference at the 1%/5%/10% level based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

[27] A graphical representation is given in Online Appendix C, Figure C.3.

One explanation for the decline in income is that firms reduced employees' wages in 2016. If this is the case, wages would be downward flexible when a negative shock hits and the downward nominal wage rigidity we observe in 2014 would be an artifact of the relatively stable economic environment. Another explanation is that workers are laid off but quickly find a new job at a lower wage at another employer. In this case, downward nominal wage rigidity does not matter much because Switzerland's flexible labor market allows for a quick reallocation of workers to other jobs.

To examine these hypotheses, we use a subset of individuals we observe in all three waves of the SESS. For those individuals, we can determine whether they experienced a wage freeze in 2014 and then experienced a wage cut by 2016. In addition, we can determine whether they stayed at the same firm using information on tenure. That is, we assume that an employee stays at the same firm if tenure increases by two years.

Table 4 — Outcomes for employees with wage freezes in 2014

	Same firm 2016	Different firm 2016
Freeze 2016	47	21
	(17,631)	(1,739)
Increase 2016	43	52
	(16,259)	(4,235)
Cut 2016	10	27
	(3,652)	(2,175)

Notes: Share of employees (in percent) with wage freezes in 2014 that experience a freeze, increase, and cut in 2016, depending on whether they work at the same or a different firm. Shares are measured in percent. Number of observations are given in parentheses. Statistics weighted using sampling weights for 2014.

Downward flexible wages for employees staying at the same company in 2016 are not the main drivers of the decline in income. Table 4 shows the share of individuals with a wage freeze in 2014, that experience a wage freeze, increase, or cut in 2016 (in percent) with the number of observations in parentheses. Only 10% of individuals with a wage freeze in 2014 experience a wage cut in 2016 if they work at the same firm. The share of wage cuts in 2016 is higher for employees changing the firm (27%). This suggests that firms hesitate to cut wages for their employees. By contrast, if employees with wage freezes in 2014 change their employer, some of them are willing to accept a lower wage.

5.2 Aggregate effects

The local effects are not representative for the entire Swiss economy because only 7.7% of observations in 2014 were base wage freezes. To show whether downward nominal wage rigidity has relevant aggregate effects, we use the difference-in-differences model to predict, for each individual in the treatment group, income and the probability of being unemployed. Then, we predict a counterfactual by setting the wage freeze dummy to zero. Finally, we aggregate the predictions (for treated individuals) and the actual data (for untreated individuals) with sampling weights for 2014. This strategy is likely to be conservative, for two reasons. First, we ignore that individuals with small wage increases may be affected by downward nominal wage rigidities. Second, we base these simulations on the differences-in-differences model including wage level time-effects, which yields estimates at the lower end of different specifications.

Table 5 — Aggregate predictions and counterfactuals

	Median income (in 1,000 CHF)			Median employment income (in 1,000 CHF)			Registered unemployed (in 1,000)		
	Pred.	Counterf.	% diff.	Pred.	Counterf.	% diff.	Pred.	Counterf.	% diff.
2013	58.50	58.50	0.00	58.44	58.42	0.03	7.87	7.86	0.23
2014	56.83	56.83	0.00	56.84	56.84	0.00	0.40	0.40	0.00
2015	58.16	58.24	-0.14	57.62	57.79	-0.30	20.72	20.50	1.11
2016	59.00	59.19	-0.32	57.01	57.52	-0.89	34.45	34.03	1.25

Notes: The table shows the aggregate effects of wage rigidity on median income, employment income, and registered unemployment. The predictions are evaluated at the actual model coefficients (Pred.). The counterfactual predictions set the treatment dummies to 0 (Counterf.). All statistics are computed at the individual level and then aggregated using own sampling weights.

The aggregate effects are smaller than the local effects. Nevertheless, Table 5 shows that wage freezes cause lower income and higher unemployment. After the 1% decline in the price level, median employment income falls by 0.9%. Because of unemployment benefits the impact on median total income is smaller (-0.3%). Finally, we observe an increase of the number of unemployed after 2014. Even without wage rigidities, unemployment increases after the removal of the exchange rate floor (counterfactual prediction). Because some wages are rigid, however, the number of unemployed is 1.1% and 1.2% higher in 2015 and 2016, respectively.

5.3 Accounting for measurement errors

Accounting for measurement errors in wage data is key when analyzing wage rigidity (see, e.g., Gottschalk, 2005). Although the firm survey is of high quality, the categorical wage freeze dummy may be mismeasured because of reporting error in income or hours worked. Measurement errors in categorical indicators result in a misclassification bias (Aigner, 1973; Card, 1996). To control for measurement error in the wage freeze dummy, we therefore follow Kane et al. (1999) and Black et al. (2000), who exploit two independent proxies for classifying wage freezes and small wage cuts. Black et al. (2000) show that, if two binary indicators are measured with errors, we can mitigate the misclassification bias by estimating a model on a subsample where both classifications are identical. Intuitively, if two independent indicators provide the same classification it is less likely that the indicators are measured with error for the corresponding observation.

We compute two potentially error-ridden wage freeze dummies based on the biennial wage change from SESS and the annual employment income change from OASI data. The two dummies are likely measured with error because both have advantages and disadvantages. The SESS dummy controls for working hours and measures the contractually agreed wage. However, it is more likely affected by reporting errors than the social security data and is based on a biennial wage change. By contrast, the OASI dummy is based on accurate register data and and on the annual change in income in 2014. The downside of the OASI dummy is that we do not control for working hours.

Based on these dummies we estimate the model on a subsample, where the SESS and OASI yield the same classification (wage freeze, wage increase, large wage cuts). Because employment income is more volatile than the base wage, we define a wage freeze as an absolute wage growth rate smaller than 0.05% (instead of 0.02%). In addition, we set the control group threshold $c = -0.1\%$.

Table 6 — Accounting for measurement error in wage freeze indicator

	Income (in log)	Employment income (in log)	Unemployment benefits (in log)	Unemployed (1/0)
2013	0.006	0.010	-0.039	-0.004
	(0.011)	(0.017)	(0.093)	(0.009)
2015	-0.090***	-0.146***	0.252***	0.029***
	(0.022)	(0.043)	(0.086)	(0.009)
2016	-0.063**	-0.228**	-0.529	-0.041
	(0.029)	(0.116)	(0.873)	(0.077)
Observations	2,005,166	2,005,166	2,005,166	2,005,166
Adj. R-sq. (between)	0.828	0.443	0.314	0.315
Adj. R-sq. (within)	-0.000	-0.000	0.000	-0.000

Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. They account for measurement error in the wage freeze indicator following the approach by Kane et al. (1999) and Black et al. (2000). The effect is normalized to 0 in the base year 2014. ***/**/* denotes a statistically significant difference at the 1%/5%/10% level based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

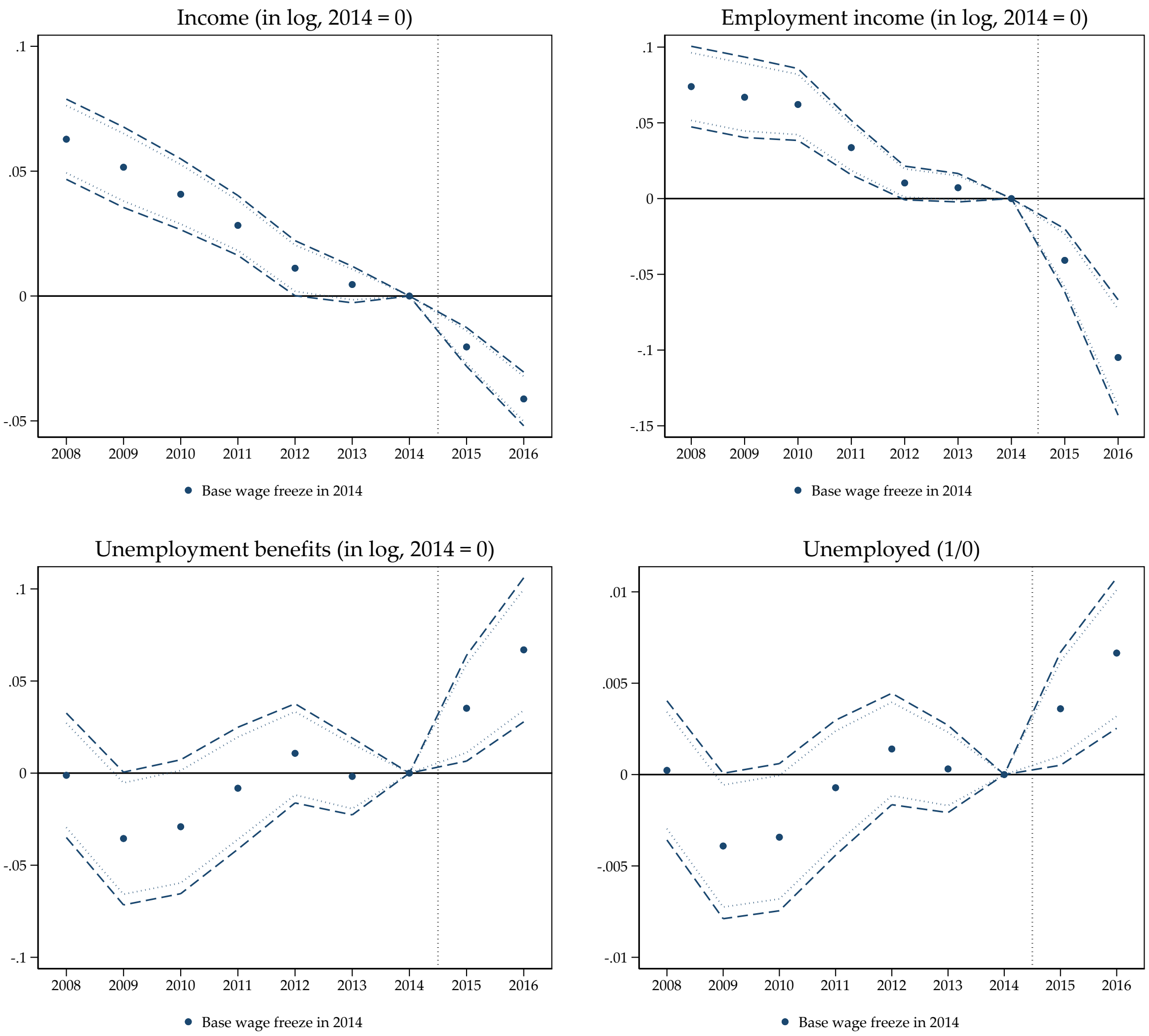
The results are based on a smaller sample and therefore less precisely estimated. Qualitatively, the effects are similar, however. Table 6 shows a a decline in (employment) income. The order of magnitude is similar as for the estimates based only on the SESS wage freeze indicator. If anything, the effects are larger. This is in line with the idea that the measurement errors mitigate the estimated effect. In addition, there is a (temporary) increase in unemployment benefits and an increase in the probability of being unemployed. The estimates for 2016 are not statistically significant, however.

5.4 Placebo tests

We conduct two types of placebo tests. First, we examine pre-treatment trends. Figure 6 shows that incomes of individuals with base wage freezes declines already before 2015. This does not come as a surprise, however, because the Swiss franc appreciated significantly between 2008 and 2011 and it took time until relative prices in Switzerland and abroad converged (see Figure 4). The pre-treatment trends therefore mirror the delayed effects of the appreciation before the exchange rate floor was introduced. During the exchange rate floor period (2011–2014), the point estimates stabilize and remain close to 0 as of 2012. For unemployment and unemployment benefits, the pre-treatment trends are mostly not statistically significant.

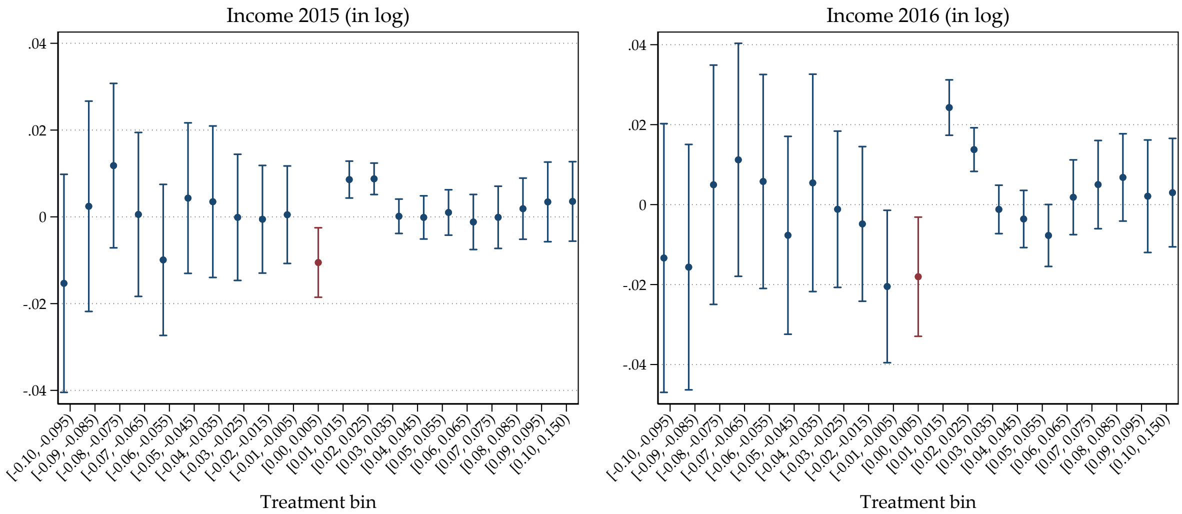
Second, we examine placebo treatments over the wage growth distribution in 2014. We define treatment bins with a width of 0.5 percentage points at different points of the wage growth distribution. The control groups are bins with the same width just below the treatment bins (see Figure 5). If the main estimates pick up the effects of downward nominal wage rigidity we should observe significant differences in outcomes only for bins close to the origin of the wage growth distribution. The left panel of Figure 7 shows that the only significantly negative coefficient for 2015 is the one for the treatment bin $[0, 0.005)$. The coefficients are significantly positive for two treatment bins covering small positive changes. This does not come as a surprise because the control group includes observations closer to the origin that are more likely to be affected by base wage rigidities. For example, for the treatment bin $[0.01, 0.015)$ the control group is $[0.005, 0.01)$. In this case, the placebo treatment bin includes individuals with higher productivity growth; therefore the 1% deflationary shock is less likely to make downward nominal wage rigidites a binding constraint than for individuals in the placebo control group. For 2016, the results are similar. The only difference is that we also find a significantly negative effect for the bin covering $[-0.01, -0.005)$.

Figure 6 — Pre-treatment trends



Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. The estimates are normalized to 0 in the base year 2014. The circles give the point estimates. The dashed (dotted) lines represent 95% (90%) confidence intervals based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

Figure 7 — Placebo treatments



Notes: Placebo treatments in different bins of the base wage growth distribution in 2014. We estimate the model defining the treatment group as a base wage change between $[c, c + 0.005)$. The control group is then defined as base wage changes between $[c - 0.005, c)$. The bin including wage freezes is highlighted in red. The circles give the point estimates. The bars represent 95% confidence intervals based on standard errors clustered according to unique values in the base wage growth distribution in 2014. Because some of the bins comprise few observations, we do not include the inverse Mills ratio.

5.5 Robustness tests

Table 7 shows a range of robustness tests. Panel (a) examines different samples and outcomes. The effect on income becomes more pronounced by restricting the sample to individuals that are observed over the entire period (balanced sample). Meanwhile, estimating the effect on real (employment) income does not change the results. We also estimate the impact on an indicator which is unity if an individual was either employed or self-employed. The probability of working falls by 0.5 and 1 percentage points in 2015 and 2016, respectively.

Panel (b) reports results using different controls. For brevity we only report the impact on total income. First, we include interactions among all the controls as time effects. That is, instead of including $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$ separately, we include a dummy for each group with the same characteristics of every variable in $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$. The results do not change. Second, we include the inverse Mills ratio interacted with time effects to control for unobserved differences that affect selection into treatment.[28] In line with the idea that unobserved factors

[28]See Online Appendix D, for a technical discussion and estimates of the Probit model. To obtain unbiased estimates, one of the explanatory variables in the Probit model to estimate the inverse Mills ratio should satisfy an exclusion restriction. It is difficult to argue that this requirement is satisfied for any of the observed variables. Still, we treat the inverse Mills ratio as an additional control and check whether our results change.

Table 7 — Robustness tests

(a) Other outcomes and samples

	Balanced sample income (in log)	Real income (in log)	Real employment income (in log)	Is working (1/0)
2013	0.017	0.004	0.007	-0.001
	(0.013)	(0.004)	(0.005)	(0.001)
2015	-0.158***	-0.021***	-0.041***	-0.005***
	(0.028)	(0.004)	(0.011)	(0.002)
2016	-0.127***	-0.044***	-0.108***	-0.010***
	(0.027)	(0.005)	(0.019)	(0.003)
Controls	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes
Adj. R-sq. (between)	0.74	0.81	0.42	0.31
Adj. R-sq. (within)	0.00	0.00	0.00	0.00
Observations	314,888	3,348,172	3,348,172	3,348,132

(b) Other controls (effect on income, in log)

	Firm TE interacted w/ controls	IMR	Wage level TE	Additional quantile controls
2013	0.005	0.004	0.003	0.005
	(0.004)	(0.004)	(0.003)	(0.003)
2015	-0.024***	-0.021***	-0.018***	-0.019***
	(0.005)	(0.004)	(0.004)	(0.004)
2016	-0.049***	-0.044***	-0.030***	-0.040***
	(0.006)	(0.005)	(0.006)	(0.006)
Controls	yes	yes	yes	yes
Interaction-TE	yes	no	no	yes
Wage level-TE	no	no	yes	no
IMR	no	yes	no	no
Adj. R-sq. (between)	0.81	0.81	0.81	0.81
Adj. R-sq. (within)	0.00	0.00	0.00	0.00
Observations	3,068,846	3,348,172	3,363,805	3,348,172

Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. Unless otherwise stated, the estimates measure the impact on total income. The effect is normalized to 0 in the base year 2014. Panel (a): The first column restricts the sample to individuals observed in the OASI data throughout 2013-2016. Panel (b): The first column reports results when including interactions of all the controls, that is we interact the time effects with a dummy for each group with the same characteristics of every variable in $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$. The second column includes the inverse Mills ratio interacted with time dummies as an additional control. The third column includes wage-level time-effects rather than firm-time effects. The last column shows results when controlling for a finer grid of quantile dummies for positive wage changes and wage changes smaller than $-c$. ***/**/* denotes a statistically significant difference at the 1%/5%/10% level based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

Table 7 — Robustness tests (continued)

(c) Other definitions of wage freezes (effect on income, in log)

	$c = -0.001$	$c = -0.1$	Treatment including positive changes $< 1\%$	Conditional on staying at company 2012-2014
2013	0.005	-0.000	-0.001	0.007**
	(0.003)	(0.005)	(0.002)	(0.003)
2015	-0.021***	-0.018**	-0.010**	-0.020***
	(0.003)	(0.009)	(0.005)	(0.004)
2016	-0.036***	-0.054***	-0.023***	-0.039***
	(0.005)	(0.011)	(0.008)	(0.006)
Controls	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes
Adj. R-sq. (between)	0.81	0.81	0.81	0.82
Adj. R-sq. (within)	0.00	0.00	0.00	0.00
Observations	3,348,172	3,348,172	3,348,172	2,778,009

(d) Export- and import-intensity

	Export-intensive	Non-import-intensive	Export- and non-import-intensive	Other
2013	0.003	0.000	0.011	0.005
	(0.004)	(0.005)	(0.007)	(0.004)
2015	-0.022***	-0.024***	-0.013*	-0.020***
	(0.005)	(0.006)	(0.008)	(0.004)
2016	-0.063***	-0.058***	-0.083***	-0.042***
	(0.008)	(0.009)	(0.012)	(0.006)
Controls	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes
Adj. R-sq. (between)	0.79	0.78	0.77	0.80
Adj. R-sq. (within)	0.00	0.00	0.00	0.00
Observations	2,565,366	2,294,718	2,162,581	3,076,283

Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. Unless otherwise stated, the estimates measure the impact on total income. The effect is normalized to 0 in the base year 2014. Panel (c): $-c$ denotes the lower threshold for defining the treatment group. The third column includes wage increases smaller than 1% in the treatment group, as those were likely also affected by the deflationary shock. The last column restricts the treatment group to individuals with a wage freeze that stayed at the same company between 2012 and 2014. Panel (d): The sample is split into export- and import-intensive firms. The categorization is based on input-output-tables for 2008 at the NOGA 2-digit level, where export-intensive (import-intensive) sectors are those with a share of exports (imports) in gross value added larger than the median. ***/**/* denotes a statistically significant difference at the 1%/5%/10% level based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

are economically negligible, the effects remain similar when adding the inverse Mills ratio from the model. The last column shows estimates controlling for 10 quantiles on both sides of the wage change distribution instead of only two indicators for negative and positive wage changes ($\mathbf{1}\{\Delta w_{i,2014} < -c\},\ \mathbf{1}\{\Delta w_{i,2014} > 0\}$). The results are almost identical to the baseline in Table 3.

Panel (c) examines different definitions of wage freezes. First, we vary the threshold for defining the control group (c). Then, we define a new treatment group including small positive growth rates smaller than 1%. Finally, we compute wage changes conditional on staying at the same company between 2012 and 2014. The results remain similar. Only when including small positive changes, the effect becomes somewhat smaller. This is in line with the idea that downward nominal wage rigidity is less likely to be a binding constraint for individuals with positive wage growth.

The deflationary shock was associated with a substantial appreciation of the Swiss franc. Therefore, export-oriented firms potentially suffer more from the shock. However, large export-oriented firms also import a larger fraction of intermediate inputs and therefore may benefit from the appreciation (see Kaufmann and Renkin, 2019). If wage rigidities are equally important across all sectors, they should therefore bind more strongly for export-oriented firms with a relatively low share of imports in value added. To test this hypothesis, panel (d) estimates the impact on employees' total income, distinguishing between sectors according to their export- and import-intensity. This categorization is based on sectoral input-output-tables for 2008 at the NOGA 2-digit level, which are the last available data for Switzerland (Nathani et al., 2015).[29] The number of observations is smaller than in the baseline because we were not able to match all sectors from the survey to the input-output-tables. We define export/import-intensive sectors as those having a share of exports/imports in gross value added larger than the median.

Compared to our main estimates, the effect on income in 2016 is larger for export-intensive sectors (-7% instead of -5%). Incomes of employees in non-import-intensive sectors fall more strongly too. For sectors that are, at the same time, export- and non-import-intensive, the effect is largest (-9.4%). However, in line with the idea that the appreciation was caused by a turn to a more restrictive monetary policy that affects the entire economy, the last column shows that the effect is statistically significant also for domestically oriented sectors with a relatively high

[29] We transform the tables from the NOGA 2002 classification to the NOGA 2008 classification with a conversion key provided by the SFSO (see Table C.10 in Online Appendix C).

import share.

6 Concluding remarks

We show that downward nominal wage rigidities exist in an economy with persistent mild deflation and a flexible labor market. In addition, these rigidities have relevant allocative effects. We compare individuals with rigid and flexible wages after an unexpected 1% decline of the price level. On average, people with rigid wages experience a decline in income (employment income) by 4.5% (11%). Moreover, the probability of becoming unemployed increases by 0.7 percentage points. A key novelty of this study is that it provides representative aggregate allocative effects. Downward nominal wage rigidities cause a fall in aggregate income (employment income) by 0.3% (0.9%). In addition, the number of unemployed persons increases by 1.2%. Therefore, even though downward nominal wage rigidities affect only a modest share of employees, these rigidities matter in the aggregate.

Our findings have implications for monetary policy and the optimal level of inflation. On the one hand, zero or slightly negative inflation is desirable because it minimizes the costs of money holdings (see Friedman, 1969). In addition, deviations of inflation from zero are costly because of misallocation of resources due to relative price distortions (see e.g. Yun, 2005). On the other hand, some researchers and central bankers argue that somewhat positive trend inflation is desirable because it relaxes the effective lower bound on interest rates (see e.g. Andrade et al., 2019) and reduces distortions caused by downward nominal wage rigidities (see e.g. Tobin, 1972; Kim and Ruge-Murcia, 2009).

At least since the Global Financial Crisis most central bankers acknowledge that the effective lower bound is a relevant constraint. The importance of downward nominal wage rigidities for the optimal level of inflation is more controversial. Issing et al. (2003), for example, argue that "[...] the importance in practice of downward nominal rigidities is highly uncertain and the empirical evidence is not conclusive, particularly for the euro area." and "[...] it would seem difficult to rule out the possibility that such rigidities would decline and even vanish in the context of a permanent and fully credible move to a low inflation environment." In a similar vein, the Swiss National Bank justifies its positive inflation target, which is in practice lower than that of the ECB, exclusively with measurement problems in CPI inflation.[30]

[30] See www.snb.ch/en/iabout/monpol/id/qas_gp_strat_1#t9 (acessed on 13/11/2020).

Our findings suggest that downward nominal wage rigidities do not vanish even during a prolonged period of mild deflation. In addition, downward nominal wage rigidities have negative effects on income and employment after an exogenous deflationary shock. We therefore conclude that central banks and researchers should take into account downward nominal wage rigidity when choosing the monetary policy strategy, in particular, the type and level of the nominal target.

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Do Sticky Wages Matter? New Evidence from Matched Firm-Survey and Register Data

Online Appendix

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This Online Appendix discusses the treatment of outliers, the construction of sampling weights, and provides additional results.

A Treatment of outliers

We consider the OASI register data to be of higher quality than the SESS data because of potential reporting errors in the firm survey. Therefore, we use the OASI data to detect outliers in the SESS. For each year t we estimate a separate linear regression for annual log-employment income net of social security contributions ($y_{i,t}$):

$$y_{i,t}^{\text{SESS}} = \alpha_t + \beta_t y_{i,t}^{\text{OASI}} + \varepsilon_{i,t}, \quad t \in \{2012, 2014, 2016\}$$

where i denotes individuals and $\varepsilon_{i,t}$ is an iid error term. We estimate the coefficients α_t, β_t using an outlier-robust regression by Yohai (1987) implemented by Jann (2010).[1] Outliers are defined as observations that deviate more than 150% from the prediction of the linear model:

$$\text{Outlier}_{i,t} = \begin{cases} 1 & , \ |y_{i,t}^{\text{SESS}} - \hat{\alpha}_t - \hat{\beta}_t y_{i,t}^{\text{OASI}}| > 1.5 \\ 0 & , \ |y_{i,t}^{\text{SESS}} - \hat{\alpha}_t - \hat{\beta}_t y_{i,t}^{\text{OASI}}| \leq 1.5 \end{cases}$$

where $\hat{\alpha}_t, \hat{\beta}_t$ denote the parameter estimates.

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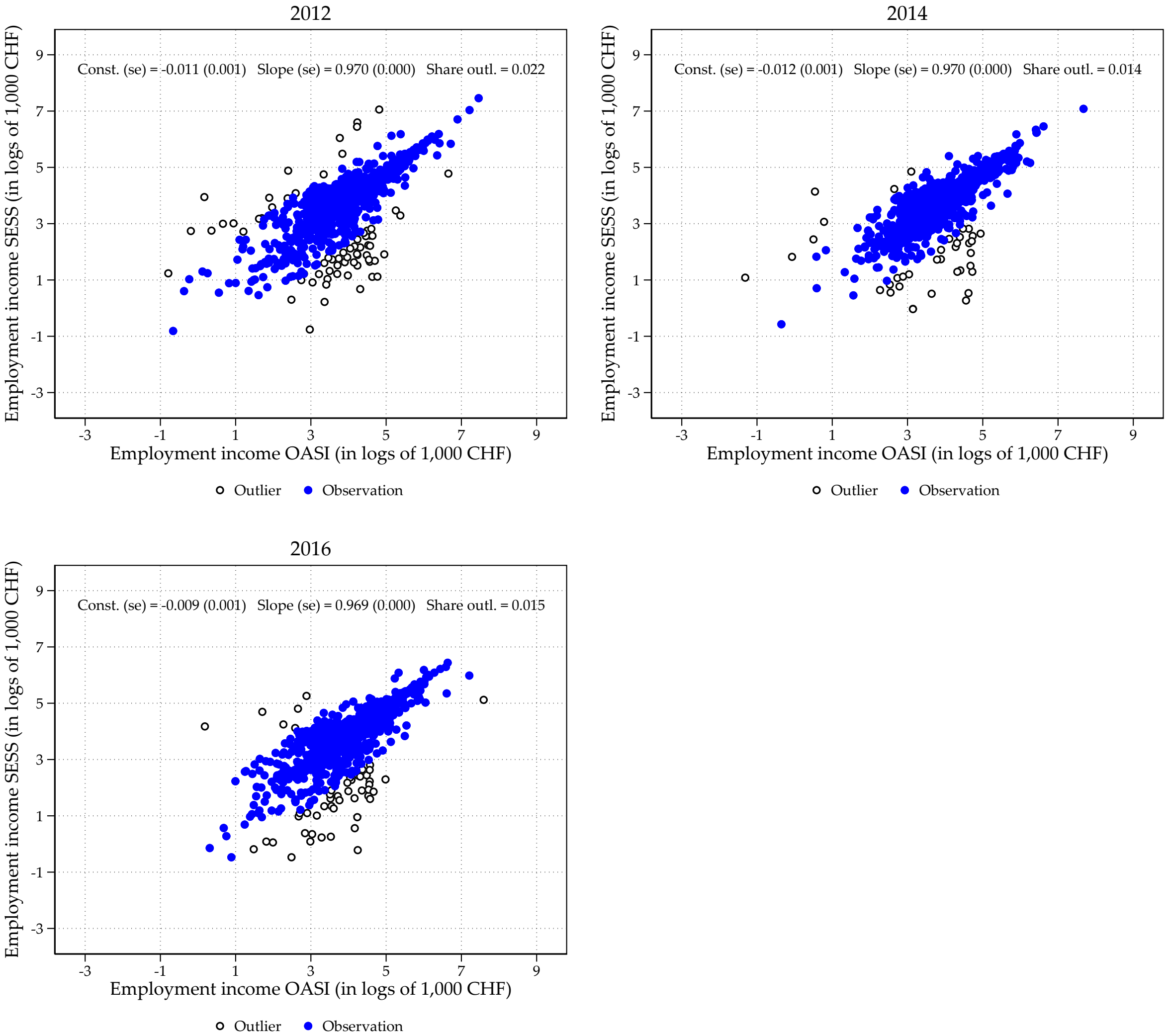
[1]Other outlier-robust estimators yield similar results.

We allow for relatively large differences between the two data sources. The reason is, that OASI data and the SESS do not measure exactly the same income. The SESS comprises only one income source for an individual that is employed for October. Meanwhile, OASI comprises all income sources for individuals employed any time for the entire year.

Figure A.1 shows that the two data sources are on average strongly related. This confirms both data sets are of high quality. The share of outliers is small and falls from 2.2% in 2012 to 1.5% in 2016.

Figure A.1 — Detection of outliers



Notes: The figure shows a 0.2% random sample of observations smaller than CHF 5,000,000. The outlier-robust regression coefficients and the share of outliers are based on all data. Outliers are observations where the SESS income deviates more than 150% from the predicted value based on the OASI data.

B Sampling weights

Analyzing wage rigidity with the SESS raises various sample selection issues. First, our sampling decisions are unlikely to randomly remove observations. Second, analyzing wage rigidity requires two consecutive wage observations. Therefore, conditioning on observing a wage change selects individuals that are more likely to stay in the labor market for an extended period.

These sampling problems introduce relevant biases in aggregate statistics (see Tables B.1 and B.3). The official statistics on median net income amount to CHF 57,000. Our own calculations with the SESS data show a higher income at CHF 60,000 (Table B.1 Panels a and b). This bias stems from the sampling decisions. If we additionally condition on observing a biennial wage change the upward bias becomes even more pronounced (panels c and d).

To compute representative aggregate statistics we therefore construct new sampling weights accounting for the sampling decisions and conditioning on observing a biennial wage change. We use information from the OASI data, which cover the population of Swiss residents. For each year and each subsample, we estimate the probability of being observed with a Probit model:

$$P[\mathbf{1}\{i \in \tilde{I}\}|\mathbf{x}_i] = \Phi(\mathbf{x}_i\beta)$$

where $\mathbf{1}\{i \in \tilde{I}\}$ is an indicator that equals one if individual i is observed in the subsample $\tilde{I} \subseteq I$ of population I. For ease of exposition, we do not add time subscripts. But we estimate a separate Probit for each year. $\mathbf{x}_i$ comprises variables that explain whether an individual is observed in the subsample. We control for 400 percentiles of the employment income distribution, as well as dummy variables for unemployment and self-employment.

Then, we use the inverse the probability that an individual with characteristics $\mathbf{x}_i$ is included in the sample as sampling weight:

$$s_i = \begin{cases} 1/P[\mathbf{1}\{i \in \tilde{I}\}|\mathbf{x}_i, i \in \tilde{I}] & , \ i \in \tilde{I} \\ 1/P[\mathbf{1}\{i \notin \tilde{I}\}|\mathbf{x}_i, \mathbf{1}\{i \notin \tilde{I}\}] = 1/\left(1 - P[\mathbf{1}\{i \in \tilde{I}\}|\mathbf{x}_i, \mathbf{1}\{i \notin \tilde{I}\}]\right) & , \ i \notin \tilde{I} \end{cases}$$

If the probability of observing an individual with characteristics $\mathbf{x}_i$ is high, the weight is low because there many other individuals with similar characteristics in the sample. The formula differs between individuals observed in the subsample ($i \in \tilde{I}$) and individuals not observed in the subsample ($i \notin \tilde{I}$). However, in our application only the weights for observed individuals matters because we compute the statistics only on the subsample with SESS data. Therefore, we obtain representative statistics for the population of all employees in Switzerland.

Table B.2 provides selected coefficient estimates, excluding the indicators for 400 percentiles of the employment income distribution for brevity. The coefficients have the expected sign.

Table B.1 — Replication net and gross income SESS

(a) Conditional on being in SESS 2014

	Official (net)	SESS (net)	Official (gross)	SESS (gross)
Median income (in 1,000 CHF)	57.41	60.33	67.00	69.29
Observations (in 1,000)	.	1,523.99	.	1,523.99

(b) Conditional on being in SESS 2016

	Official (net)	SESS (net)	Official (gross)	SESS (gross)
Median income (in 1,000 CHF)	57.21	60.53	67.60	69.60
Observations (in 1,000)	.	1,665.34	.	1,665.34

(c) Conditional on observing biennial wage change 2014

	Official (net)	SESS (net)	Official (gross)	SESS (gross)
Median income (in 1,000 CHF)	57.41	66.23	67.00	76.65
Observations (in 1,000)	.	859.99	.	859.99

(d) Conditional on observing biennial wage change 2016

	Official (net)	SESS (net)	Official (gross)	SESS (gross)
Median income (in 1,000 CHF)	57.21	68.18	67.60	78.98
Observations (in 1,000)	.	960.73	.	960.73

Notes: Official median income and employment stem from the SFSO. We adjust the official gross income reported by SFSO by our own estimate of the federal social security charges in 2014 and 2016 (14.32% and 15.37%). The sample estimates are based on two subsamples. Panels (a) and (b) restrict the sample to observations in the SESS after our sampling decisions. Panels (c) and (d) additionally restrict the sample to those individuals in the SESS with two consecutive wage observations.

Table B.2 — Probit models weighting

(a) Conditional on being in SESS after sampling decisions (2014)

	1/0 (in SESS)
Unemployed	-0.294***
	(0.003)
Self-employed	-0.175***
	(0.004)
Constant	-3.083***
	(0.013)
Observations	5,576,637
Pseudo R-sq.	0.170

(b) Conditional on being in SESS after sampling decisions (2016)

	1/0 (in SESS)
Unemployed	-0.341***
	(0.003)
Self-employed	-0.132***
	(0.005)
Constant	-3.015***
	(0.012)
Observations	5,593,395
Pseudo R-sq.	0.171

(c) Conditional on observing biennial wage change after sampling decisions (2014)

	1/0 (in SESS)
Unemployed	-0.483***
	(0.005)
Self-employed	-0.129***
	(0.005)
Constant	-3.443***
	(0.022)
Observations	5,576,637
Pseudo R-sq.	0.152

(d) Conditional on observing biennial wage change after sampling decisions (2016)

	1/0 (in SESS)
Unemployed	-0.531***
	(0.005)
Self-employed	-0.127***
	(0.006)
Constant	-3.262***
	(0.017)
Observations	5,593,395
Pseudo R-sq.	0.164

Notes: Probit model coefficients for estimating weights. Indicators for 400 percentiles of the employment income distribution not reported for brevity. ***/**/* denotes statistical significance at the 1%/5%/10% level.

In particular, unemployed and self-employed individuals are less likely to be included in the SESS.

In the main text, we show these sampling weights allow to recover the official median income and employment statistics in 2014. Table B.3 shows our sampling weights accurately recover these aggregate statistics for 2016 as well (first and fourth column).

Table B.3 shows aggregate statistics for income and employment based on different data sets and weighting schemes. It shows that the sampling weights recover the official median income and employment statistics in 2014 and 2016. For example, for 2014 median income (employment) amounted to CHF 57,410 (4,824,800 persons). Using our sampling weights, we obtain an estimate of CHF 56,670 (4,814,020 persons).

Table B.3 — Data and weighting

(a) Conditional on being in SESS after sampling decisions 2014

	Aggregate statistics		Sample estimates		
	Official statistics	OASI population	OASI unweighted	OASI own weights	SESS official weights
Median income (in 1,000 CHF)	57.41	55.69	75.17	56.76	60.33
Employment (in 1,000)	4,824.80	4,895.73	1,523.99	4,814.02	3,974.69
Observations (in 1,000)	.	4,895.73	1,517.78	1,454.88	1,523.99

(b) Conditional on being in SESS after sampling decisions 2016

	Aggregate statistics		Sample estimates		
	Official statistics	OASI population	OASI unweighted	OASI own weights	SESS official weights
Median income (in 1,000 CHF)	57.21	56.40	75.03	57.24	60.53
Employment (in 1,000)	4,915.50	4,971.26	1,665.34	4,907.56	3,733.10
Observations (in 1,000)	.	4,971.26	1,659.21	1,594.97	1,665.34

(c) Conditional on observing biennial wage change after sampling decisions 2014

	Aggregate statistics		Sample estimates		
	Official statistics	OASI population	OASI unweighted	OASI own weights	SESS official weights
Median income (in 1,000 CHF)	57.41	55.69	80.37	56.60	66.23
Employment (in 1,000)	4,824.80	4,895.73	859.99	4,826.18	1,561.71
Observations (in 1,000)	.	4,895.73	857.90	832.59	859.99

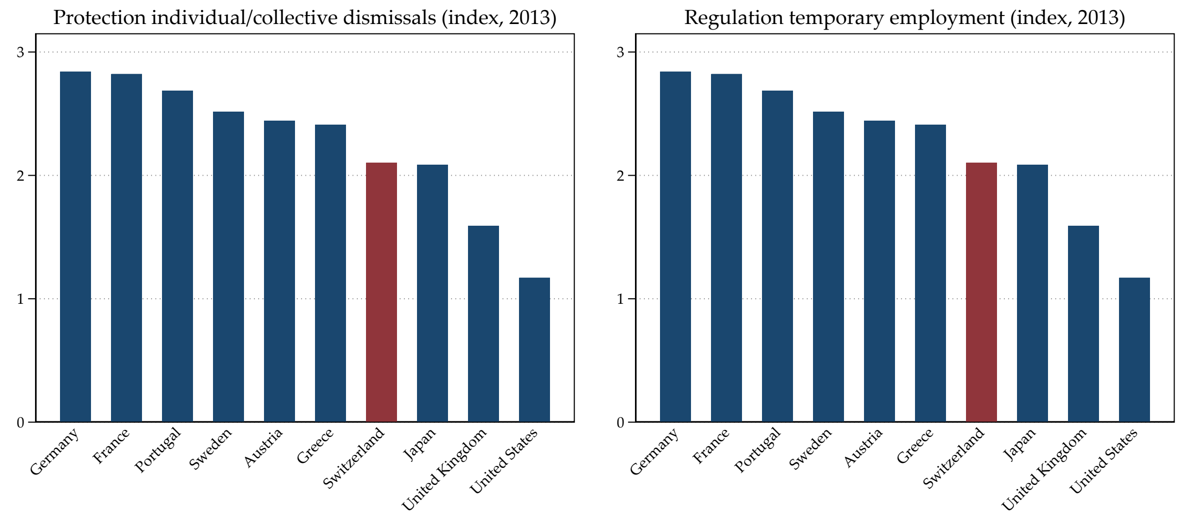
(d) Conditional on observing biennial wage change after sampling decisions 2016

	Aggregate statistics		Sample estimates		
	Official statistics	OASI population	OASI unweighted	OASI own weights	SESS official weights
Median income (in 1,000 CHF)	57.21	56.40	81.94	56.64	68.18
Employment (in 1,000)	4,915.50	4,971.26	960.73	4,959.38	1,425.73
Observations (in 1,000)	.	4,971.26	959.10	935.21	960.73

Notes: Official median income and employment stem from the SFSO. We adjust the official gross income reported by SFSO by our own estimate of the federal social security charges in 2014 and 2016 (14.32% and 15.37%). The sample estimates are based on two subsamples. Panel (a) restricts the sample to observations in the SESS after our sampling decisions. Panel (b) additionally restricts the sample to those individuals in the SESS with two consecutive wage observations.

C Additional results

Figure C.1 — Labor market regulation



Source: OECD, see Table C.11.

Table C.1 — Share base wage in total payroll by firm size (number of employees)

(a) 2014

	Mean	Std.	Min.	Max.
0-19	0.92	0.06	0.00	1.00
20-49	0.91	0.05	0.54	1.00
50-249	0.90	0.04	0.39	1.00
250-999	0.89	0.05	0.46	1.00
1000-	0.90	0.04	0.63	1.00
Total	0.90	0.04	0.00	1.00
Observations matched	1,517,784			
Observations SESS	1,523,987			

(b) 2016

	Mean	Std.	Min.	Max.
0-19	0.92	0.06	0.10	1.00
20-49	0.90	0.06	0.26	1.00
50-249	0.89	0.05	0.42	1.00
250-999	0.88	0.05	0.44	1.00
1000-	0.90	0.04	0.63	1.00
Total	0.90	0.05	0.10	1.00
Observations matched	1,659,212			
Observations SESS	1,665,338			

Notes: Share of base wage payments in total payroll at the firm level by firm size according to the number of employees. Unweighted statistics.

Table C.2 — Descriptive statistics matched data set 2016

	Mean	Std.	Min.	Max.
Income (OASI)				
Income (in 1,000)	65.73	79.20	0.00	16,757.25
Employment income (in 1,000)	64.93	79.30	0.00	16,757.25
Unemployment benefits (in 1,000)	0.00	0.00	0.00	0.00
Income and wage (SESS)				
Employment income (in 1,000)	60.16	62.73	0.25	15,105.29
Total wage (in 1,000)	70.26	73.09	0.20	16,723.77
Share of base income	0.91	0.07	0.01	1.00
Share of regular income	0.97	0.07	0.02	1.00
Share of irregular income	0.03	0.07	0.00	0.98
Wage T-2 observed	0.51	0.50	0.00	1.00
Activity and contract				
Tenure at firm (years)	7.99	8.88	0.00	64.00
Manager	0.21	0.41	0.00	1.00
Open-ended contract	0.93	0.25	0.00	1.00
Hourly wage	0.18	0.39	0.00	1.00
Stays at company	0.81	0.39	0.00	1.00
Employee				
Age (years)	41.62	12.76	17.00	80.00
Women	0.54	0.50	0.00	1.00
University degree	0.20	0.40	0.00	1.00
Foreigner	0.29	0.45	0.00	1.00
Firm				
Public company	0.25	0.43	0.00	1.00
Collective agreement	0.42	0.49	0.00	1.00
Small firm	0.13	0.34	0.00	1.00
Medium firm	0.20	0.40	0.00	1.00
Large firm	0.67	0.47	0.00	1.00
Observations matched	1,659,212			
Observations SESS	1,665,338			

Notes: All statistics weighted using own sampling weights. Unless otherwise stated the variables are indicators with values of 1/0.

Table C.3 — Wage rigidity statistics for 2016

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	69.2	21.0	9.8	31.7
Regular wage	67.7	29.4	2.9	9.0
Total wage	62.6	36.2	1.2	3.2
Employment income (SESS)	55.6	43.8	0.5	1.2
Employment income (OASI)	53.5	45.4	1.1	2.5

Notes: Wage rigidity statistics based on biennial wage changes according to different wage measures. The regular wage includes the base wage and 13th monthly payments. The total wage includes the base wage, 13th monthly payments, and irregular payments (overtime, Sunday/night, and bonus payments). The share of wage cuts prevented is defined as share freezes/(1-share raises).

Table C.4 — Wage rigidity statistics unweighted

(a) 2014

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	75.1	16.8	8.1	32.5
Regular wage	72.6	21.5	5.9	21.7
Total wage	68.4	29.5	2.1	6.5
Employment income (SESS)	64.0	35.5	0.5	1.4
Employment income (OASI)	69.3	29.6	1.1	3.6

(b) 2016

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	72.4	16.8	10.8	39.2
Regular wage	72.7	24.1	3.2	11.7
Total wage	68.2	30.8	0.9	3.0
Employment income (SESS)	62.8	36.7	0.5	1.4
Employment income (OASI)	65.6	33.0	1.4	4.0

Notes: All statistics based on own sampling weights.

Table C.5 — Wage rigidity statistics excluding hourly wages

(a) 2014

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	72.8	18.4	8.8	32.3
Regular wage	69.6	24.2	6.2	20.4
Total wage	65.5	32.1	2.5	7.1
Employment income (SESS)	61.2	38.0	0.8	2.1
Employment income (OASI)	61.7	37.2	1.1	3.0

(b) 2016

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Base wage	70.0	19.3	10.7	35.7
Regular wage	69.7	27.3	3.0	9.9
Total wage	63.7	35.2	1.0	2.9
Employment income (SESS)	58.5	41.0	0.6	1.4
Employment income (OASI)	56.2	42.5	1.3	3.0

Notes: All statistics based on own sampling weights.

Table C.6 — Base wage rigidity statistics for various characteristics 2014

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Overall	70.9	21.4	7.7	26.4
Activity and contract				
Tenure shorter than 5 years	70.6	25.1	4.3	14.7
Tenure longer or 5 years	71.1	19.5	9.4	32.6
No management	70.5	22.4	7.1	24.0
Management	69.7	21.0	9.4	30.9
Temporary contract	60.9	33.4	5.8	14.7
Open-ended contract	71.3	20.9	7.8	27.1
Monthly pay	72.8	18.4	8.8	32.3
Hourly pay	62.1	35.3	2.6	6.8
Changed firm	61.7	35.5	2.8	7.3
Stayed at firm	73.1	18.1	8.8	32.8
Employee				
Older than or 40 years	67.1	23.0	9.9	30.0
Younger than 40 years	77.3	18.8	4.0	17.5
Men	72.4	18.3	9.3	33.6
Women	69.8	23.7	6.5	21.6
University degree	70.2	22.1	7.7	25.8
No university degree	72.0	21.0	7.0	25.1
Foreigner	70.3	21.8	7.9	26.7
Swiss	72.8	20.3	6.9	25.3
Firm				
Private sector	71.7	21.8	6.6	23.1
Public sector	68.7	20.4	10.9	34.8
No collective agreement	67.9	24.1	8.0	25.0
Collective agreement	73.1	19.8	7.1	26.4
Small firm	60.6	29.4	10.0	25.4
Medium firm	62.9	28.4	8.7	23.5
Large firm	73.4	19.4	7.3	27.3

Notes: Wage rigidity statistics based on biennial base wage growth according to contract, employee and firm characteristics. Wage freezes are defined as growth rates smaller than 0.02% in absolute value. The share of wage cuts prevented is defined as share freezes/(1-share raises). All statistics weighted using own sampling weights.

Table C.7 — Base wage rigidity statistics for various characteristics 2016

	Share wage raises (in %)	Share wage cuts (in %)	Share wage freezes (in %)	Share wage cuts prevented (in %)
Overall	69.2	21.0	9.8	31.7
Activity and contract				
Tenure shorter than 5 years	70.6	23.4	6.1	20.7
Tenure longer or 5 years	68.6	19.9	11.6	36.8
No management	69.0	21.5	9.5	30.7
Management	72.3	18.1	9.6	34.7
Temporary contract	66.5	26.3	7.2	21.4
Open-ended contract	69.3	20.8	9.9	32.1
Monthly pay	70.0	19.3	10.7	35.7
Hourly pay	64.6	31.8	3.6	10.3
Changed firm	63.4	31.6	5.0	13.5
Stayed at firm	70.6	18.5	10.9	37.1
Employee				
Older than or 40 years	64.8	22.9	12.4	35.1
Younger than 40 years	76.9	17.8	5.3	23.0
Men	67.7	21.0	11.4	35.2
Women	70.5	21.1	8.5	28.7
University degree	68.0	21.9	10.1	31.6
No university degree	74.1	19.0	6.8	26.4
Foreigner	69.2	21.4	9.5	30.7
Swiss	69.4	19.9	10.7	35.0
Firm				
Private sector	68.2	22.4	9.3	29.3
Public sector	71.6	17.6	10.8	38.0
No collective agreement	69.0	20.7	10.3	33.1
Collective agreement	69.5	21.4	9.1	29.8
Small firm	58.8	29.9	11.3	27.4
Medium firm	61.4	26.1	12.5	32.5
Large firm	71.6	19.3	9.1	31.9

Notes: All statistics based on own sampling weights.

Table C.8 — Descriptive statistics 2014-2016 (detailed results)

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Overall	0.70	0.21	0.09	0.29	1,820,712	27,890	0.91	0.97	3,237,213	37,020
Competence level for job										
Simple tasks	0.60	0.30	0.11	0.27	84,698	7,476	0.92	0.97	177,407	20,364
Practical work	0.71	0.21	0.08	0.26	479,432	19,185	0.91	0.97	883,822	35,393
Special knowledge	0.72	0.17	0.11	0.38	371,670	15,682	0.90	0.96	600,993	31,424
Complex work/problem solving	0.71	0.20	0.08	0.30	494,293	18,236	0.91	0.97	793,391	33,758
Missing	0.69	0.23	0.09	0.27	390,619	16,854	0.91	0.96	781,600	31,192
Job type										
Upper Management	0.66	0.22	0.12	0.36	47,313	12,440	0.88	0.92	98,482	32,663
Middle Management	0.73	0.17	0.10	0.37	147,307	11,952	0.88	0.93	232,007	27,538
Lower Management	0.70	0.18	0.12	0.40	166,510	12,674	0.90	0.95	275,410	28,435
Basic Management	0.72	0.19	0.09	0.32	137,622	10,108	0.91	0.97	227,531	25,253
Without Managament Function	0.70	0.22	0.09	0.28	1,272,359	24,928	0.91	0.97	2,291,468	36,543
Missing	0.70	0.29	0.01	0.05	49,601	289	0.95	1.00	112,315	312
Basis for pay										
Hours	0.71	0.20	0.09	0.31	1,692,415	26,893	0.91	0.97	2,972,620	36,941
Lessons	0.61	0.37	0.02	0.05	83,932	1,756	0.93	0.99	122,879	3,326
Other (e.g. commission)	0.61	0.30	0.09	0.23	44,365	2,562	0.90	0.95	93,826	10,162
Conract type										
Open-ended (monthly pay)	0.71	0.19	0.10	0.34	1,418,830	24,825	0.90	0.97	2,379,321	36,582
Open-ended (ann. working time)	0.75	0.16	0.09	0.37	270,695	4,137	0.90	0.95	427,938	10,080
Open-ended (hourly pay)	0.64	0.33	0.03	0.08	74,468	7,867	0.94	0.98	215,007	24,614
Temporary (monthly pay)	0.67	0.26	0.07	0.21	49,325	3,515	0.93	0.99	132,895	11,119
Temporary (hourly pay)	0.57	0.38	0.05	0.11	7,312	1,479	0.95	0.99	33,950	5,656

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Open-ended (w. commission)	–	–	–	–	11	2	0.98	0.98	95	7
Temporary (w. commission)	0.53	0.47	0.00	0.00	71	8	0.91	0.96	119	13
Occupation (ISCO 2-digit)										
Commissioned armed forces officers	0.31	0.67	0.02	0.03	794	39	0.88	0.95	972	60
Non-commissioned armed forces officers	–	–	–	–	49	15	0.90	0.97	76	25
Armed forces occupations, other ranks	0.80	0.17	0.03	0.15	63	10	0.92	0.99	138	16
Managers, w/o further details	0.81	0.11	0.08	0.44	48,859	4,890	0.90	0.95	72,177	11,650
Chief executives, senior officials and legislators	0.73	0.17	0.11	0.39	31,202	7,308	0.88	0.93	54,490	22,404
Administrative and commercial managers	0.75	0.15	0.10	0.40	31,329	5,122	0.88	0.92	51,610	13,609
Production and specialized services managers	0.69	0.25	0.07	0.21	30,956	4,953	0.90	0.96	44,962	12,872
Hospitality, retail and other services managers	0.87	0.07	0.06	0.47	10,257	609	0.91	0.98	13,072	2,393
Professionals, w/o further details	0.79	0.13	0.08	0.38	25,315	1,411	0.92	0.97	42,619	3,123
Science and engineering professionals	0.77	0.10	0.13	0.56	27,570	3,263	0.90	0.96	48,725	9,272
Health professionals	0.72	0.19	0.09	0.34	39,514	1,602	0.91	0.97	67,132	4,606
Teaching professionals	0.65	0.30	0.05	0.13	133,718	3,202	0.93	0.99	202,747	6,596
Business and administration professionals	0.68	0.20	0.12	0.37	46,699	5,280	0.91	0.96	81,801	13,451

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Information and communications technology professionals	0.78	0.11	0.11	0.48	32,601	3,073	0.91	0.96	55,468	7,866
Legal, social and cultural professionals	0.72	0.15	0.13	0.47	35,479	3,385	0.92	0.98	57,616	8,262
Technicians and associate professionals, w/o further details	0.81	0.10	0.09	0.47	91,749	4,867	0.91	0.97	132,211	10,249
Science and engineering associate professionals	0.71	0.20	0.09	0.30	73,042	6,838	0.90	0.96	123,118	17,560
Health associate professionals	0.66	0.23	0.11	0.32	76,303	2,503	0.90	0.96	124,392	6,964
Business and administration associate professionals	0.72	0.15	0.13	0.47	97,555	8,971	0.91	0.96	162,864	22,375
Legal, social, cultural and related associate professionals	0.66	0.25	0.09	0.26	17,369	2,498	0.93	0.98	32,169	7,026
Information and communications technicians	0.73	0.17	0.10	0.37	15,652	1,511	0.89	0.95	26,239	3,864
Clerical support workers, w/o further details	0.76	0.20	0.04	0.18	1,598	40	0.91	0.96	1,958	251
General and keyboard clerks	0.70	0.20	0.10	0.34	50,903	8,731	0.92	0.98	99,499	25,961
Customer services clerks	0.70	0.22	0.09	0.29	12,263	1,692	0.94	0.98	21,274	5,144
Numerical and material recording clerks	0.71	0.18	0.11	0.37	25,516	3,330	0.91	0.97	42,598	9,194
Other clerical support workers	0.75	0.23	0.02	0.08	24,767	995	0.93	0.98	30,687	2,592

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Service and sales workers, w/o further details	0.77	0.16	0.08	0.32	5,185	853	0.93	0.98	10,485	2,153
Personal service workers	0.67	0.24	0.10	0.29	52,739	4,915	0.92	0.98	110,821	14,786
Sales workers	0.81	0.16	0.04	0.19	84,706	3,712	0.92	0.98	145,972	11,508
Personal care workers	0.64	0.28	0.07	0.20	43,655	2,476	0.90	0.96	83,302	5,601
Protective services workers	0.79	0.14	0.07	0.35	27,399	1,007	0.91	0.96	42,187	2,662
Market-oriented skilled agricultural workers	0.47	0.09	0.44	0.82	1,490	408	0.93	0.99	4,126	1,850
Market-oriented skilled forestry, fishery and hunting workers	0.77	0.06	0.16	0.71	224	47	0.92	0.99	347	175
Craft and related trades workers, w/o further details	0.73	0.16	0.11	0.39	8,356	466	0.91	0.97	10,806	1,004
Building and related trades workers, excluding electricians	0.61	0.25	0.14	0.37	17,038	2,088	0.91	0.98	45,402	8,127
Metal, machinery and related trades workers	0.73	0.19	0.09	0.32	27,917	3,407	0.89	0.96	55,405	10,110
Handicraft and printing workers	0.73	0.17	0.10	0.37	7,149	1,036	0.90	0.97	14,606	3,244
Electrical and electronic trades workers	0.72	0.21	0.07	0.24	11,437	1,936	0.91	0.97	24,389	5,351
Food processing, wood working, garment and other craft and related trades workers	0.69	0.23	0.08	0.25	11,524	1,946	0.92	0.97	26,563	7,184

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Plant and machine operators and assemblers, w/o further details	0.50	0.32	0.18	0.35	325	66	0.92	0.98	691	216
Stationary plant and machine operators	0.62	0.26	0.12	0.31	18,092	2,228	0.88	0.95	31,761	6,008
Assemblers	0.70	0.22	0.08	0.28	11,473	1,127	0.90	0.97	20,062	3,504
Drivers and mobile plant operators	0.56	0.34	0.10	0.22	35,627	2,547	0.91	0.96	60,805	7,290
Elementary occupations, w/o further details	0.58	0.31	0.11	0.25	39,427	3,429	0.92	0.97	77,522	8,974
Cleaners and helpers	0.57	0.30	0.14	0.31	15,890	2,875	0.93	0.98	42,757	11,109
Agricultural, forestry and fishery labourers	0.62	0.26	0.12	0.32	642	162	0.93	0.99	1,674	784
Labourers in mining, construction, manufacturing and transport	0.67	0.25	0.08	0.24	25,998	2,452	0.89	0.95	50,437	6,611
Food preparation assistants	0.56	0.13	0.31	0.70	113	46	0.92	0.98	296	256
Street and related sales and service workers	–	–	–	–	4	1	–	–	4	2
Work permit										
Swiss	0.70	0.22	0.09	0.29	1,355,166	26,184	0.91	0.97	2,257,572	36,822
Short-term resident (L)	0.65	0.28	0.06	0.19	1,655	564	0.93	0.98	14,057	5,321
Resident (B)	0.74	0.19	0.07	0.26	78,527	9,469	0.92	0.97	224,963	26,637
Resident (C)	0.71	0.20	0.09	0.31	267,024	16,448	0.91	0.96	464,460	32,695
Cross-border worker (G)	0.71	0.19	0.10	0.34	116,259	7,806	0.91	0.96	221,251	20,292
Other	0.57	0.38	0.04	0.10	2,081	1,016	0.93	0.97	7,022	3,792
Education										
University	0.75	0.17	0.08	0.31	244,153	10,739	0.91	0.96	421,297	23,954
U Applied Sciences	0.76	0.16	0.08	0.33	163,743	9,697	0.91	0.96	264,837	21,530

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Federal Certificate	0.70	0.19	0.10	0.34	212,402	13,828	0.90	0.96	349,355	28,830
Teacher Certificate	0.55	0.33	0.11	0.26	17,941	2,410	0.92	0.99	37,486	7,372
Higher School Certificate	0.67	0.21	0.13	0.38	52,360	6,788	0.93	0.97	102,593	19,393
Vocational Training	0.70	0.21	0.09	0.30	677,486	22,184	0.91	0.97	1,247,472	36,074
On-the-job Training	0.63	0.26	0.11	0.30	68,475	6,110	0.91	0.97	125,896	17,593
Compulsory Education	0.69	0.23	0.08	0.27	171,677	9,570	0.92	0.97	333,414	24,565
Missing	0.71	0.23	0.06	0.20	212,475	1,389	0.92	0.97	354,863	4,714
Region										
Leman	0.70	0.21	0.09	0.29	290,741	5,755	0.92	0.97	529,318	15,680
Espace Mittelland	0.71	0.24	0.06	0.20	470,712	7,280	0.91	0.97	767,480	16,997
Northwest	0.72	0.21	0.07	0.25	219,224	4,523	0.91	0.96	396,212	10,808
Zurich	0.72	0.18	0.10	0.36	486,718	6,439	0.91	0.96	835,040	14,526
East	0.67	0.21	0.12	0.36	160,038	4,503	0.91	0.97	304,867	11,892
Central	0.68	0.24	0.08	0.25	145,859	4,912	0.91	0.97	263,885	11,497
Ticino	0.57	0.23	0.20	0.46	47,420	2,330	0.91	0.97	92,523	6,493
Firm size (number of employees)										
0-19	0.58	0.32	0.10	0.23	37,893	11,294	0.94	0.98	184,117	32,819
20-49	0.61	0.28	0.11	0.29	57,187	8,042	0.93	0.97	177,498	15,549
50-249	0.62	0.27	0.11	0.28	283,583	11,113	0.91	0.97	620,758	15,890
250-999	0.67	0.23	0.09	0.29	310,630	1,951	0.90	0.96	558,839	2,861
1000-	0.74	0.18	0.08	0.30	1,131,419	2,184	0.91	0.97	1,648,113	2,619
Collective agreements										
GAV (association)	0.73	0.20	0.06	0.24	350,050	6,244	0.91	0.97	634,545	17,599
GAV (private and public)	0.71	0.18	0.10	0.36	307,198	1,919	0.91	0.97	493,648	4,045
Collective agreement (without GAV)	0.69	0.24	0.07	0.23	71,922	967	0.92	0.98	120,861	2,219
No collective agreements	0.69	0.22	0.09	0.30	1,039,370	22,139	0.91	0.96	1,841,724	35,015
Missing	0.59	0.35	0.06	0.15	52,172	874	0.92	0.98	146,435	2,434

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Sectors (NACE 1-digit sections)										
Mining and quarrying	0.63	0.27	0.11	0.29	1,621	197	0.91	0.98	3,196	366
Manufacturing	0.72	0.18	0.10	0.35	302,379	7,352	0.89	0.95	537,811	15,587
Electricity, gas a. steam supply	0.72	0.20	0.07	0.27	22,239	428	0.88	0.95	32,656	777
Water supply	0.73	0.15	0.12	0.45	7,599	611	0.90	0.97	13,047	1,292
Construction	0.62	0.26	0.12	0.32	54,097	1,580	0.91	0.98	116,038	6,274
Trade; rep. of motor vehicles a. moto.	0.77	0.18	0.05	0.22	217,818	3,236	0.91	0.98	388,139	11,049
Transportation and storage	0.69	0.27	0.04	0.14	173,269	1,199	0.91	0.96	234,770	3,120
Accomod. and food serv. act.	0.62	0.28	0.10	0.26	18,979	1,156	0.92	0.99	65,781	4,749
Information and communication	0.70	0.17	0.13	0.43	75,542	2,459	0.91	0.95	130,299	5,760
Financial and insurance activities	0.67	0.18	0.15	0.44	152,048	2,597	0.89	0.92	252,303	6,100
Real estate activities	0.71	0.20	0.08	0.29	5,744	670	0.93	0.98	16,040	2,096
Prof., scientific and tech. act.	0.69	0.22	0.09	0.29	64,326	3,277	0.92	0.96	147,826	9,915
Admin. and support serv. act.	0.62	0.26	0.12	0.31	42,910	2,064	0.93	0.98	123,657	4,970
Public administration and defence	0.75	0.16	0.09	0.35	173,258	1,101	0.92	0.99	255,820	1,753
Education	0.67	0.26	0.07	0.21	174,887	2,398	0.94	0.99	279,245	4,516
Human health and social work act.	0.67	0.23	0.11	0.32	304,565	3,869	0.91	0.97	517,322	8,707
Arts, entertainment and recreation	0.64	0.27	0.10	0.27	12,904	795	0.94	0.98	30,360	2,224
Other service activities	0.56	0.38	0.06	0.14	16,527	1,440	0.94	0.98	45,015	4,606

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Sectors (NACE 2-digit divisions)										
O. mining and quarrying	0.63	0.27	0.11	0.29	1,617	193	0.91	0.98	3,188	354
Mining support service activities	–	–	–	–	4	4	–	–	46	14
Manufacture of food products	0.76	0.18	0.05	0.23	28,406	635	0.91	0.97	54,660	1,764
Manufacture of beverages	0.65	0.21	0.13	0.38	957	82	0.91	0.97	2,817	213
Ma. of tabacco products	0.83	0.15	0.02	0.12	81	9	0.87	0.92	1,334	19
Ma. of textiles	0.60	0.24	0.15	0.38	3,031	227	0.91	0.97	6,281	518
Ma. of wearing apparel	0.59	0.28	0.14	0.33	1,007	131	0.94	0.98	1,901	353
Ma. of leather and related products	0.52	0.36	0.12	0.25	340	62	0.94	0.99	747	140
Ma. of wood a. of prod. of wood a. cork	0.47	0.38	0.16	0.30	2,341	215	0.91	0.97	9,232	1,143
Ma. of paper and paper products	0.59	0.27	0.14	0.33	4,488	73	0.87	0.94	7,417	148
Printing and reprod. of recorded media	0.31	0.45	0.24	0.35	2,590	188	0.91	0.97	7,473	607
Ma. of coke and refined petroleum prod.	0.68	0.23	0.09	0.28	69	8	0.88	0.95	298	18
Ma. of chemicals and chemical prod.	0.78	0.16	0.06	0.27	15,381	537	0.88	0.93	28,495	939
Ma. of pharmaceutical prod. a. prep.	0.85	0.11	0.03	0.22	42,705	217	0.89	0.91	57,290	368
Ma. of rubber and plastic products	0.61	0.29	0.10	0.26	8,493	359	0.88	0.95	17,805	800
Ma. of o. non-metallic mineral prod.	0.69	0.21	0.10	0.31	4,674	363	0.90	0.97	12,333	843
Manufacture of basic metals	0.61	0.24	0.15	0.38	5,033	106	0.88	0.95	10,805	198

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Ma. of fab. metal prod., except mach.	0.64	0.23	0.12	0.35	17,926	754	0.90	0.96	41,311	2,293
Ma. of computer and electronic prod.	0.71	0.17	0.12	0.41	78,771	1,267	0.89	0.95	127,381	2,474
Manufacture of electrical equipment	0.79	0.14	0.07	0.33	27,542	562	0.90	0.96	39,683	1,055
Ma. of machinery and equipment n.e.c.	0.73	0.15	0.12	0.43	37,202	1,093	0.89	0.96	69,450	2,241
Ma. of motor vehicles	0.67	0.19	0.14	0.44	1,931	142	0.89	0.96	3,497	261
Ma. of o. transport equipment	0.65	0.18	0.18	0.50	2,712	146	0.89	0.95	9,849	267
Manufacture of furniture	0.76	0.10	0.14	0.60	2,424	154	0.92	0.98	5,957	399
Other manufacturing	0.75	0.17	0.08	0.32	11,111	242	0.88	0.93	18,018	748
Rep. and install. of mach. and eq.	0.57	0.26	0.17	0.40	3,164	141	0.90	0.97	6,475	587
Electricity, gas a. steam supply	0.72	0.20	0.07	0.27	22,239	428	0.88	0.95	32,834	778
Water collection, treatment and supply	0.70	0.17	0.13	0.44	1,011	59	0.91	0.98	1,704	138
Sewerage	0.69	0.22	0.10	0.31	1,553	214	0.90	0.96	2,813	458
Waste collection and treatment	0.75	0.12	0.13	0.51	5,016	335	0.90	0.96	8,661	695
Remediation act. and o. waste man. serv.	–	–	–	–	19	7	–	–	55	19
Construction of buildings	0.62	0.32	0.06	0.15	24,833	501	0.91	0.98	47,767	1,356
Civil engineering	0.52	0.29	0.19	0.40	9,741	163	0.90	0.97	17,437	336
Specialised construction activities	0.68	0.14	0.19	0.57	19,523	927	0.91	0.98	51,684	4,734
Trade a. rep. of motor vehicles a. moto.	0.71	0.13	0.16	0.54	9,947	387	0.90	0.97	23,194	2,246

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Wholesale trade, exc. of motor vehicles	0.71	0.20	0.08	0.29	54,371	1,675	0.91	0.96	112,098	4,676
Retail trade, exc. motor vehicles	0.79	0.17	0.04	0.19	153,500	1,217	0.92	0.98	259,361	5,003
Land transp. a. transp. via pipelines	0.69	0.25	0.07	0.21	83,658	723	0.90	0.96	116,714	2,024
Water transport	0.75	0.14	0.11	0.44	164	19	0.91	0.96	709	63
Air transport	0.58	0.39	0.03	0.07	9,200	32	0.92	0.94	13,134	83
Warehousing and sup. act. for transport.	0.60	0.33	0.06	0.16	22,466	262	0.91	0.96	37,980	649
Postal and courier activities	0.72	0.26	0.02	0.07	57,781	170	0.91	0.97	69,610	361
Accommodation	0.56	0.25	0.19	0.43	6,603	514	0.93	0.99	26,672	1,510
Food and beverage service activities	0.64	0.29	0.07	0.20	12,376	648	0.92	0.99	41,761	3,331
Publishing activities	0.52	0.20	0.28	0.58	8,327	465	0.90	0.96	14,887	955
Motion picture	0.57	0.36	0.07	0.16	1,301	311	0.96	0.98	3,887	764
Programming and broadcasting activities	0.63	0.14	0.23	0.63	10,953	85	0.89	0.95	13,459	148
Telecommunications	0.80	0.16	0.05	0.22	33,245	235	0.90	0.94	44,271	409
Computer progr., consult. and rel. act.	0.70	0.17	0.13	0.44	20,173	1,303	0.92	0.95	49,830	3,482
Information service activities	0.69	0.19	0.12	0.39	1,543	91	0.90	0.95	5,034	235
Financial service activities	0.62	0.19	0.19	0.49	84,440	933	0.88	0.91	143,275	1,955
Insu., reinsurance and pension funding	0.74	0.17	0.10	0.37	50,108	474	0.89	0.93	74,013	818
Act. aux. to financial s. a. insu. act.	0.70	0.21	0.09	0.31	17,500	1,221	0.90	0.94	36,323	3,552
Real estate activities	0.71	0.20	0.08	0.29	5,744	670	0.93	0.98	16,695	2,097
Legal and accounting activities	0.59	0.29	0.12	0.29	9,456	496	0.91	0.95	23,326	2,260

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Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Act. of head offices; man. consult. act.	0.71	0.19	0.10	0.34	24,097	609	0.90	0.95	44,993	1,624
Architectural and engineering act.	0.76	0.16	0.08	0.34	14,605	1,065	0.91	0.97	42,475	3,624
Scientific research and development	0.71	0.24	0.05	0.19	11,832	618	0.94	0.97	23,596	1,106
Advertising and market research	0.55	0.36	0.08	0.19	2,387	266	0.96	0.98	8,359	856
O. prof., scientific and technical act.	0.64	0.23	0.12	0.34	1,504	216	0.93	0.97	5,571	962
Veterinary activities	0.79	0.19	0.02	0.11	445	63	0.95	0.99	1,273	319
Rental and leasing activities	0.51	0.27	0.23	0.46	1,528	70	0.91	0.97	3,315	185
Employment activities	0.63	0.31	0.06	0.16	9,693	971	0.93	0.97	43,513	1,866
Travel agency, tour operator reserv.	0.76	0.12	0.12	0.50	5,465	161	0.91	0.97	10,046	456
Security and investigation act.	0.67	0.29	0.04	0.12	3,561	74	0.95	0.97	10,891	197
Serv. to build. and landscape act.	0.62	0.22	0.16	0.42	18,299	639	0.92	0.98	54,039	1,993
Office admin., office support act.	0.47	0.43	0.10	0.18	4,364	169	0.94	0.98	8,852	436
Public administration and defence	0.75	0.16	0.09	0.35	173,258	1,101	0.92	0.99	258,248	1,754
Education	0.67	0.26	0.07	0.21	174,887	2,398	0.94	0.99	286,729	4,517
Human health activities	0.68	0.20	0.12	0.37	205,860	1,254	0.90	0.97	329,340	4,384
Residential care activities	0.65	0.25	0.10	0.28	75,910	1,976	0.90	0.96	147,696	3,175
Social work act. without accommodation	0.63	0.31	0.06	0.16	22,795	739	0.94	0.99	46,205	1,699
Creative, arts and entertainment act.	0.58	0.19	0.23	0.55	1,605	133	0.93	0.97	4,826	409

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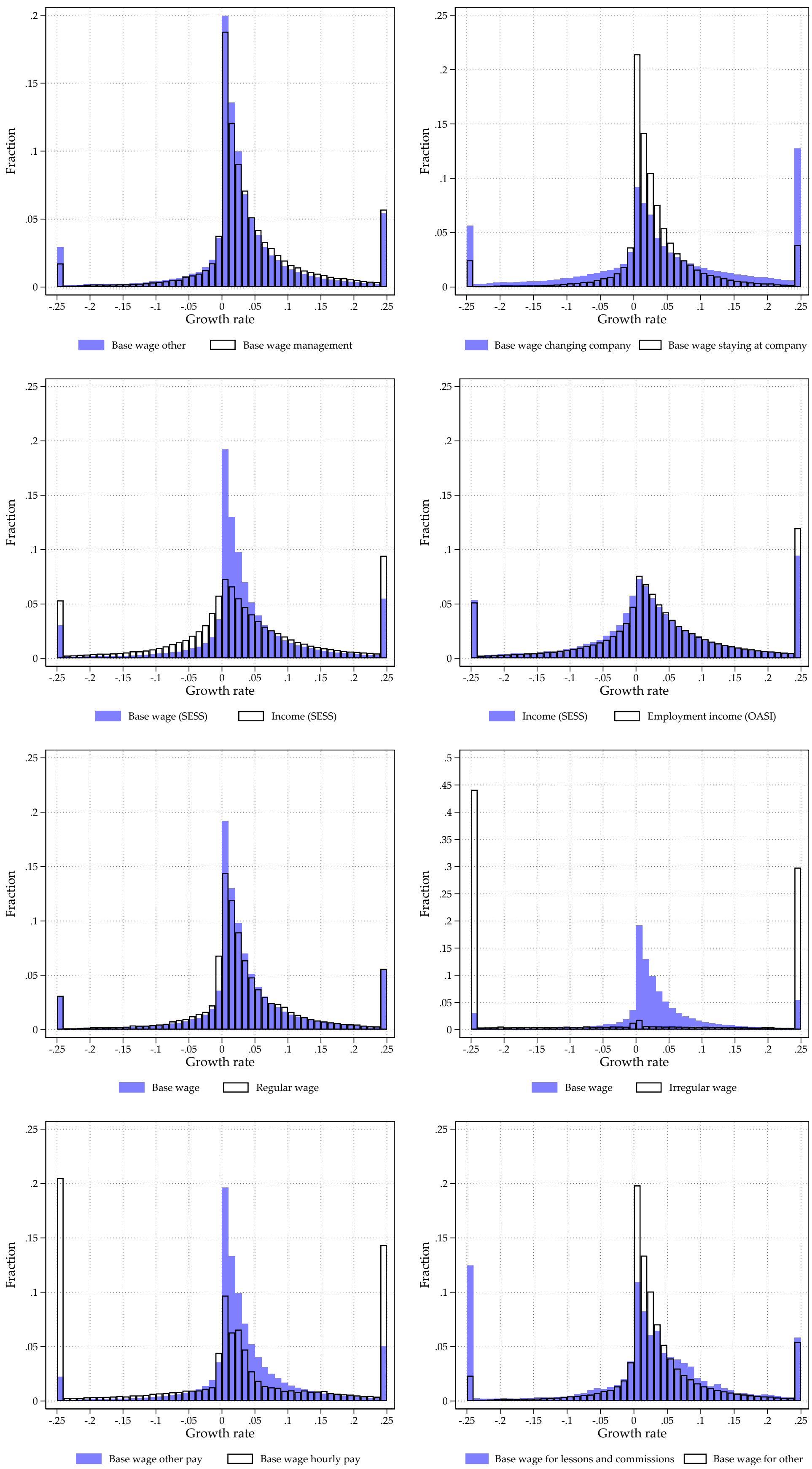
Table C.8 – continued from previous page

	Wage growth statistics (share)						Share in total income			
	Raise	Cut	Freeze	Cut prev.	Obs.	Firms	Base	Regular	Obs.	Firms
Libr., arch., museums and o. cult. act.	0.79	0.14	0.07	0.34	5,815	163	0.93	0.98	10,414	352
Gambling and betting activities	0.49	0.42	0.09	0.17	1,360	36	0.92	0.98	3,071	58
Sports activities and amusement	0.54	0.37	0.09	0.20	4,124	472	0.95	0.98	14,149	1,445
Activities of membership organisations	0.61	0.32	0.07	0.17	12,145	1,132	0.94	0.99	33,145	2,711

Notes: The left panel gives biennial base wage rigidity statistics. Wage freezes are defined as growth rates smaller than 0.02% in absolute value. The share of wage cuts prevented is defined as share freezes/(1-share raises). The right panel provides the share of the base and regular income in total income. Regular income includes the base income and 13th month payments. Total wage includes the base wage, 13th month payments, and irregular payments (overtime, Sunday/night, and bonus payments). All statistics are weighted using our own sample weights (see Section B). Due to confidentiality restrictions results are only published if they are based on at least 60 employees and five firms.

Figure C.2 — Distribution of wage and income growth



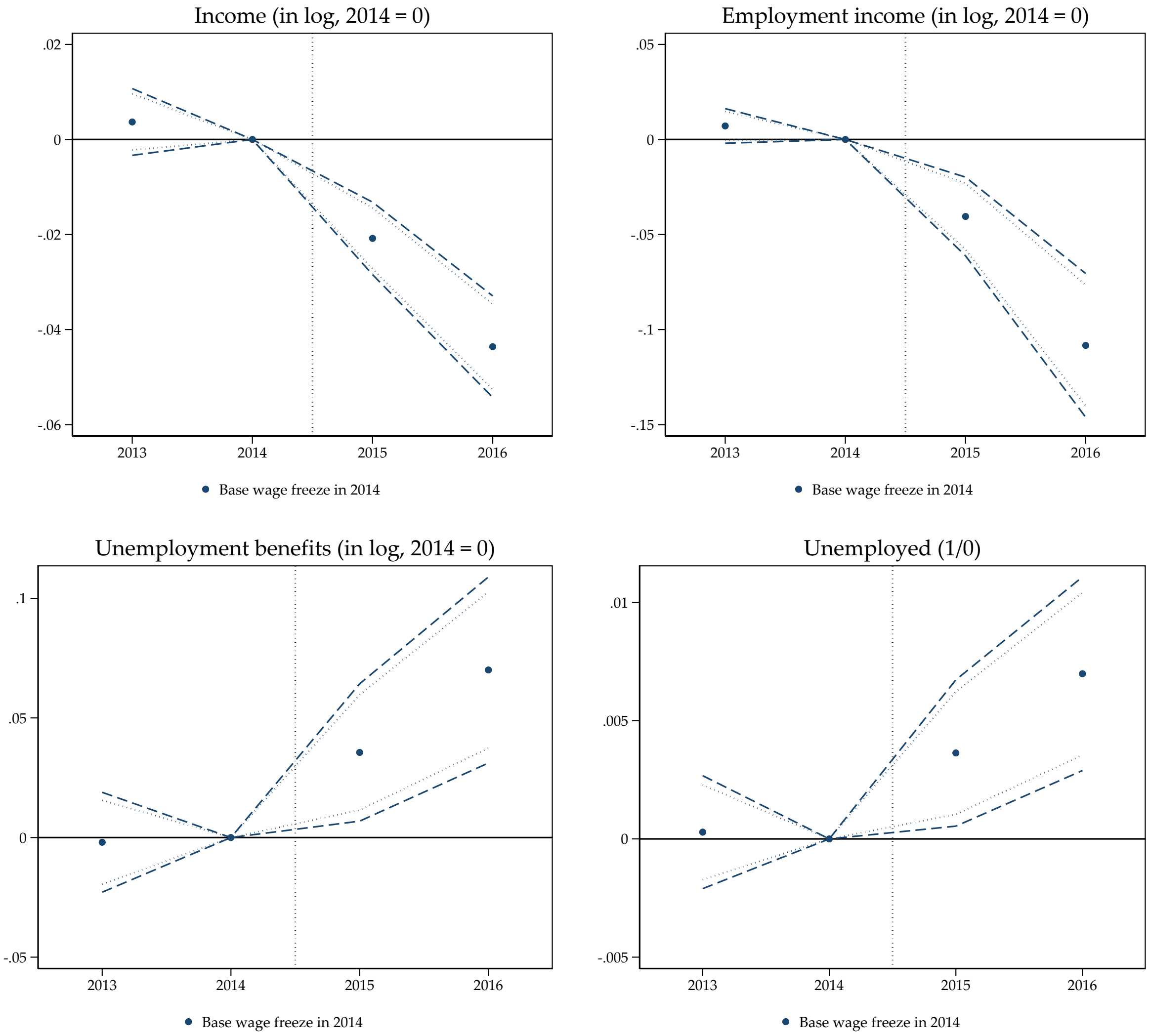
Notes: Histograms winsorized at an absolute value of 25%. Not accounting for sampling weights.

Table C.9 — Difference in means between treatment and control group

	Difference means freezes − cuts	Std. err. freezes − cuts	Mean freezes	Obs.	Mean small cuts	Obs.
Income (OASI)						
Income (in 1,000)	15.57***	0.71	100.93	68,662	85.36	10,532
Employment income (in 1,000)	15.15***	0.68	99.84	68,662	84.70	10,532
Income and wage (SESS)						
Employment income (in 1,000)	12.17***	0.51	82.94	68,790	70.77	10,591
Total wage (in 1,000)	14.91***	0.61	97.21	68,790	82.30	10,591
Share of base income	-0.01***	0.00	0.89	68,790	0.90	10,591
Share of regular income	-0.01***	0.00	0.96	68,790	0.96	10,591
Share of irregular income	0.01***	0.00	0.04	68,790	0.04	10,591
Activity and contract						
Tenure at firm (years)	1.38***	0.11	14.81	68,790	13.43	10,591
Manager	0.11***	0.00	0.35	68,256	0.24	10,540
Open-ended contract	0.01***	0.00	0.98	68,790	0.97	10,591
Hourly wage	-0.08***	0.00	0.01	68,790	0.10	10,591
Stays at company	0.10***	0.00	0.94	68,790	0.84	10,591
Employee						
Age (years)	2.64***	0.10	49.73	68,790	47.09	10,591
Women	-0.11***	0.01	0.40	68,790	0.50	10,591
University degree	-0.04***	0.00	0.23	61,471	0.26	9,787
Foreigner	-0.00	0.00	0.21	68,790	0.21	10,591
Firm						
Public company	0.08***	0.01	0.40	68,790	0.33	10,591
Collective agreement	-0.04***	0.01	0.37	65,074	0.40	9,750
Small firm	0.02***	0.00	0.07	68,790	0.05	10,591
Medium firm	-0.00	0.00	0.17	68,790	0.17	10,591
Large firm	-0.02***	0.00	0.76	68,790	0.78	10,591

Notes: Tests for difference in means between treatment (wage freezes) and control group (small wage cuts). ***/**/* denotes a statistically significant difference at the 1%/5%/10% level.

Figure C.3 — Relative effect between individuals with base wage freezes and cuts



Notes: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. The estimates are normalized to 0 in the base year 2014. The circles give the point estimates. The dashed (dotted) lines represent 95% (90%) confidence intervals based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

Table C.10 — Data sources

Name	Source	URL
Social security data (OASI)	CCO	https://www.zas.admin.ch/zas/de/home/partenaires-et-institutions-/statistique.html
Wages, socio-economic, and firm data (SESS)	SFSO	https://www.bfs.admin.ch/bfsstatic/dam/assets/6468399/master
Labor market regulation index	OECD	https://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection.htm
Swiss inflation	SFSO	https://www.bfs.admin.ch/bfs/en/home/statistics/prices/consumer-price-index.html
CHF/EUR exchange rate	SNB	https://data.snb.ch/en/topics/ziredev#!/cube/devkum
Wage index	SFSO	https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/wages-income-employment-labour-costs/wage-evolution.html
Inflation abroad	OECD	https://data.oecd.org/price/inflation-cpi.htm
Gross median income	SFSO	https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/wages-income-employment-labour-costs.assetdetail.8786111.html
Average social security charges 2014	Federal Social Insurance Office	Page 30 https://fak-basel.ch/wp-content/uploads/2015/10/Soz.vers_Statistik-2013.pdf
Average social security charges 2016	Federal Social Insurance Office	https://www.bsv.admin.ch/bsv/de/home/sozialversicherungen/ueberblick/grsv/statistik.html?cq_ck=1481195805050#-1422866446
Employment	SFSO	https://www.bfs.admin.ch/bfs/de/home/statistiken/industrie-dienstleistungen/unternehmen-beschaeftigte/beschaeftigungsstatistik/beschaeftigte.assetdetail.12967634.html
Input-Output-Table	SECO	www.seco.admin.ch/seco/de/home/Publikationen_Dienstleistungen/Publikationen_und_Formulare/Aussenwirtschafts/Globalisierung/die-volkswirtschaftliche\-bedeutung-der-globalen-wertschoepfungsk.html and Nathani et al. (2015)
Conversion keys NOGA 2002 to 2008	SFSO	www.bfs.admin.ch/bfs/en/home/statistics/industry-services/nomenclatures/noga/publications-noga-2008.assetdetail.239927.html

D Inverse Mills ratio

Including the inverse Mills ratio aims to control for the fact that individuals with certain unobserved characteristics related to selection into treatment are differently affected by the deflationary shock. In a first step, we estimate the inverse Mills ratio to control for unobserved factors conditional on observed characteristics and selection into treatment (see Heckman, 1979). Let us assume that the continuous selection process into treatment in 2014, that is the unobserved wage change absent wage rigidities ($\Delta w^*_{i,2014}$), depends linearly on observed ($\mathbf{x}_{i,2014}$) and unobserved ($\nu_{i,2014}$) characteristics:[2]

$$\Delta w^*_{i,2014} = \mathbf{x}_{i,2014}\beta + \nu_{i,2014}, \quad \nu_{i,2014} \sim iid\; N(0, \sigma^2_\nu)$$

Based on this assumption we can estimate the inverse Mills ratio from a Probit model, where we restrict the sample to the treatment and control group in 2014:[3]

$$P[\Delta w_{i,2014} = 0 | \mathbf{x}_{i,2014}] = \Phi(\mathbf{x}_{i,2014}\beta)\ ,$$

As control variables, we include a variety of employee and firm characteristics from the SESS.[4] For example, we control for 30 percentiles of the log wage level, age, employment status between 2012 and 2014, whether an individual stayed at the same company between 2012 and 2014, education, job type, whether a firm has collective agreements, firm size, gender, and sector.

Then, we compute the inverse Mills ratio for each individual as (see, e.g., Wooldridge, 1995):

$$\lambda_{i,2014} = E[v_i | \mathbf{x}_{i,2014}, \Delta w_{i,2014} = 0] = \frac{\phi(\mathbf{x}_{i,2014}\beta)}{\Phi(\mathbf{x}_{i,2014}\beta)}$$

The inverse Mills ratio measures the expected value of the unobserved characteristics affecting selection into treatment conditional on observed characteristics.

Table D.1 shows the estimates of the Probit model. For brevity, we do not report coefficients for the age and wage level percentiles. The coefficients have the expected sign. For example, individuals that were unemployed sometime between 2012 and 2014 were less likely experiencing a wage freeze. Similarly, individuals that stayed at the same firm during this period were more likely to experience a wage freeze.

[2] We drop the constant for readability.

[3] For the remaining observations, we set the inverse Mills ratio to zero.

[4] See Table C.10 in Online Appendix C.

Table D.1 — Probit for inverse Mills ratio

	1/0 (wage freezes/small cuts)
Unemployed (before 2015)	-0.154***
	(0.039)
Stayed at firm (before 2015)	0.574***
	(0.019)
Middle Management	0.088**
	(0.038)
Lower Management	-0.033
	(0.040)
Basic Management	0.102**
	(0.044)
Without Managament Function	-0.134***
	(0.038)
Woman	-0.110***
	(0.014)
U Applied Sciences	0.271***
	(0.027)
Federal Certificate	0.311***
	(0.024)
Teacher Certificate	0.253***
	(0.076)
Higher School Certificate	0.612***
	(0.044)
Vocational Training	0.517***
	(0.020)
On-the-job Training	0.439***
	(0.035)
Compulsory Education	0.611***
	(0.027)
Observations	79,259
Pseudo R-sq.	0.082

Notes: Model estimated on data for treatment (wage freezes) and control group (small wage cuts). Coefficients for age and tenure percentiles not shown for brevity. Standard errors in parentheses. ***/**/* denotes a statistically significant coefficient at the 1%/5%/10% level.

Table D.1 — Probit for inverse Mills ratio (continued)

	1/0 (wage freezes/small cuts)
Manufacturing	1.307***
	(0.238)
Electricity, gas a. steam supply	1.419***
	(0.246)
Water supply	1.703***
	(0.256)
Construction	1.053***
	(0.241)
Trade; rep. of motor vehicles a. moto.	1.134***
	(0.239)
Transportation and storage	0.957***
	(0.239)
Accomod. and food serv. act.	1.406***
	(0.242)
Information and communication	1.703***
	(0.240)
Financial and insurance activities	1.544***
	(0.239)
Real estate activities	1.469***
	(0.269)
Prof., scientific and tech. act.	1.236***
	(0.240)
Admin. and support serv. act.	1.424***
	(0.240)
Public administration and defence	1.655***
	(0.239)
Education	1.520***
	(0.239)
Human health and social work act.	1.470***
	(0.239)
Arts, entertainment and recreation	1.513***
	(0.248)
Other service activities	1.461***
	(0.248)
Collective agreement	0.015
	(0.014)
Observations	79,259
Pseudo R-sq.	0.082

Notes: Model estimated on data for treatment (wage freezes) and control group (small wage cuts). Coefficients for age and tenure percentiles not shown for brevity. Standard errors in parentheses. ***/**/* denotes a statistically significant coefficient at the 1%/5%/10% level.

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