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

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Abstract

Abstract: This paper provides novel evidence on downward nominal wage rigidities and their allocative effects in Switzerland. We match individual wages from a bi-annual firm survey with information on annual income and employment from social security register data. We find relevant downward nominal wage rigidities in the base wage, which accounts for more than 90% of employment income. We then identify the allocative effects of downward nominal wage rigidities on income and employment after an unexpected 1% decline of the consumer price level. Base wage rigidities cause a decline of aggregate income (-0.39%) and employment income (-0.97%), as well as an increase of unemployment (2.11%).

JEL classification: E30, E40, E50

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

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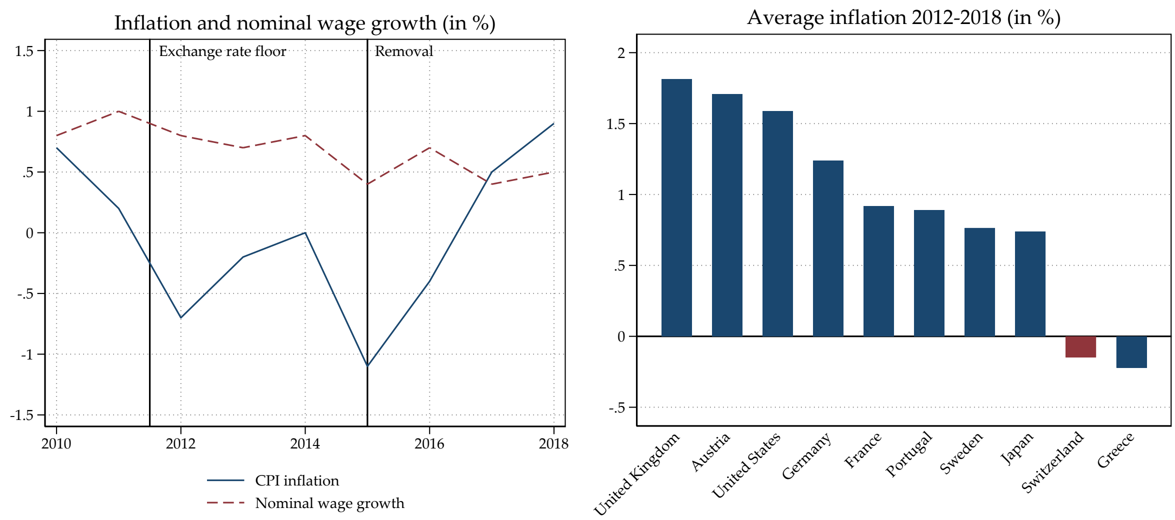
1 Introduction

Macroeconomic models often assume that wages are infrequently adjusted to generate involuntary unemployment and inefficient business cycles (see, e.g., Erceg et al., 2000; Schmitt-Grohé and Uribe, 2016; Born et al., 2019). However, whether such wage rigidities exist, and whether they cause economically relevant distortions, is controversial (see Basu and House, 2016). Although there is an extensive empirical literature on wage rigidities, only few studies examine the allocative effect in a deflationary environment. This study aims to fill this gap by analyzing Swiss social security data—covering the universe of the working age population—matched to a firm-survey on income and working hours—covering almost half of all Swiss employees. We then identify the causal effect of downward nominal wage rigidity using the discontinuity of the worker-level wage growth distribution near the origin and a surprise decline in the price level after the Swiss National Bank abandoned an exchange rate floor policy in January 2015 (see Figure 1).

Figure 1 — Inflation and wage growth



Source: SFSO, OECD, own calculations, see Table C.11 in the Online Appendix.

Switzerland is an interesting case to study for several reasons. First, downward nominal wage rigidities are more likely to bind during low inflation or deflation (see, e.g., Fehr and Goette, 2005). The left panel of Figure 1 shows CPI inflation was at 0% in 2014, before the exchange rate floor was abandoned; thereafter, inflation fell to -1% in 2015 and -0.2% in 2016. Meanwhile, aggregate nominal wages continued to increase. Second, low inflation or deflation is rare because most central banks have positive inflation targets. However, Switzerland experienced particularly low inflation in international comparison (see right panel). Third, Switzerland's labor market is relatively flexible.[1] Therefore, downward nominal wage rigidities, if they exist, are not caused by legal provisions.[2]

The aim of this study is twofold. First, we provide novel representative evidence on downward nominal wage rigidity in Switzerland for various wage components. The data allows to address a widespread sample selection issue: Computing the fraction of wage changes conditions on individuals observed for two consecutive periods. This selects individuals that are more likely to be employed and earn higher incomes. To compute representative descriptive statistics we construct new sampling weights based on social security data. Second, we identify allocative effects of downward nominal wage rigidity to a 1% decline in the price level, when the Swiss National Bank surprisingly abandoned an exchange rate floor. The identification strategy compares individuals with small wage cuts to those with wage freezes in 2014. To control for unobserved differences between individuals with wage freezes and small wage cuts, we use a two-step approach (see Heckman, 1979; Wooldridge, 1995). We estimate the expected value of unobserved characteristics conditional on experiencing a wage freeze with a Probit including a range of socio-economic characteristics in 2014 (i.e. the inverse Mills ratio). We then include the inverse Mills ratio as an additional regressor when estimating the impact of downward nominal wage rigidity on income and unemployment.

Our main findings can be summarized as follows. Downward nominal wage rigidity is a pervasive feature of the Swiss labor market. In 2014 7.7% of all observations were base wage freezes. In addition, wage increases (70.9%) were more frequent than wage cuts (21.4%), even though CPI inflation was slightly negative. For total wages, including payments for Sunday/night work, overtime, and bonuses, only 2.3% are wage freezes. Moreover, 34% of

[1] See 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%) depending on their evaluation (Tages-Anzeiger, 2019). In 2019, however, 96% of all employees working for the Swiss confederation received an evaluation that made them eligible for a raise.

observations are wage cuts. However, the base wage accounts for 91% of employment income. Therefore, base wage rigidities affect a substantial fraction of firms' labor costs.

We then estimate the impact downward nominal wage rigidities on income and unemployment after the 1% deflationary shock in January 2015. Locally, that is near the origin of the base wage distribution, the effects are large. By 2016, income and employment income decline by 5% and 12%, respectively. Moreover, the probability of becoming unemployed is 1.2 percentage points higher for individuals with wage freezes. Because only 7.7% of the population is affected by wage freezes, these results are not representative for the entire economy. Therefore, we use the difference-in-differences model to predict income and unemployment if all wages were flexible. Then, we aggregate this counterfactual prediction using sampling weights. We find that downward nominal wage rigidities reduce aggregate income and employment income by 0.39% and 0.97%, respectively, and increase unemployment by 2.11%.

Our paper is closely related to Fehr and Goette (2005), de Ridder and Pfajfar (2017), and Kurmann and McEntarfer (2019). As Fehr and Goette (2005), we analyze Switzerland because of low inflation and a relatively flexible labor market. Inflation was on average close to zero in the study by Fehr and Goette (2005). In our sample we even observe a period with a falling price level. As de Ridder and Pfajfar (2017) and Kurmann and McEntarfer (2019), we emphasize the importance of measuring the effect of wage rigidity to an identified macroeconomic shock.[3] The main difference to those studies is that we exploit a plausibly exogenous shock that leads to deflation, rather than only to disinflation (see Efing et al., 2015; Bonadio et al., 2020; Auer et al., 2018, 2019; Kaufmann and Renkin, 2019, for other applications). In addition, we are able to provide estimates based on a comparison between workers, rather than firms or regions. Finally, the data allow to compute representative statistics for the entire Swiss economy.

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.

[3]Similarly, Pischke (2018) analyzes adjustment in different segments of the U.S. housing sector and Kaur (2019) the response of Indian districts to rainfall shocks.

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). 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 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,

[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).

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. Firm survey data is usually more accurate than household survey data. In addition, these surveys comprise detailed information on income, its components, working hours, and 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, 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. de Ridder and Pfajfar (2017) combine regional variation in wage rigidity in the U.S. with monetary and fiscal policy shocks identified at the aggregate level. 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.

The view that wage stickiness is a pervasive phenomenon and has allocative consequences is controversial. First, evidence from accurate payroll data 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. 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), the 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. Indeed, Grigsby et al. (2019) find wages are more downward flexible during recessions.

3 Data

We use data on individual incomes, wages, and socio-economic characteristics from register data and a firm survey (see Figure 2).[5] The register data stem from social security payments collected by regional and sectoral social security branches. The Central Compensation Office (CCO) collects the data from the branches and makes them available to researchers. The firm survey is conducted by the Swiss Federal Statistical Office (SFSO). We can accurately match the data sets at the employee level with an anonymous identifier based on the social security number. The data allow to measure wage changes, income, and the unemployment history. In addition, we can construct sampling weights to compute representative statistics for the entire Swiss economy. Therefore, the combined data set is particularly suited to analyze wage rigidities and their impact on income and unemployment.[6]

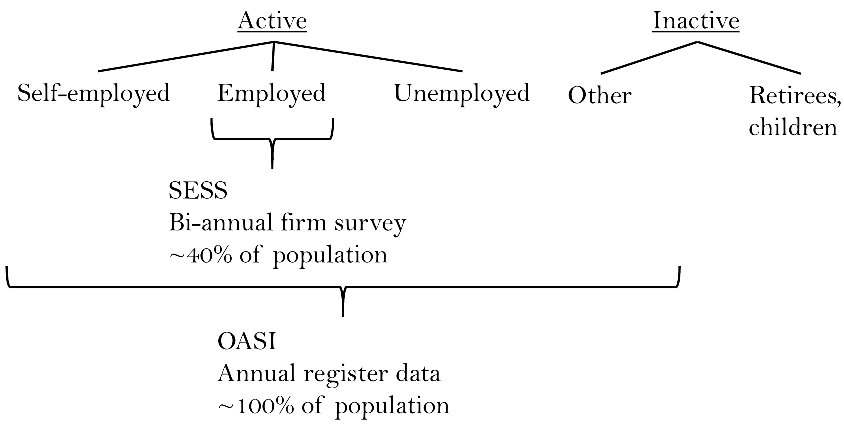
3.1 Old Age and Survivors Insurance

We use data on individual annual incomes and the unemployment history for the entire Swiss resident working-age population based on social security payments for the Old Age Survivors Insurance (OASI). Firms report these data for every employee when they pay the social security

[5]See Table C.11 in the Online Appendix for further information on the data sources.

[6]Our data set is close to the ideal data set described by 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."

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.

contributions to the regional or sectoral OASI branches. 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 (women 64). In few cases, we observe individuals that still work during retirement. The CCO then collects these data from the branches. We use annual data from 2008–2016, with about 5 mio. individuals each year.

We construct various income measures for each individual in the data set. Overall income is defined as the sum of all incomes received in a given year. This includes income from self-employment, payments from insurances (e.g. compensation for mandatory military service), income from employment, as well as unemployment benefits.[7] Importantly, our overall income measure includes all occupations, as well as, income when an individual changed occupation. We then compute employment income excluding 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. Finally, we construct an unemployment indicator which equals one if the individual received

[7] 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.

unemployment benefits in a given year.[8] 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 individuals even if they are not working and have zero income. Second, we observe incomes from all occupations subject to social security contributions.[9] Third, the data allow to distinguish between income from employment, self-employment, or unemployment benefits. Therefore, we can reconstruct the unemployment history. Fourth, the data comprise an anonymous social security number to match them to a firm survey on wages and socio-economic characteristics. Because OASI covers almost the entire Swiss working age population, we can match virtually all observations from the survey to the social security data.[10]

3.2 Swiss Earnings Structure Survey

The Swiss Earnings Structure Survey (SESS) is a bi-annual stratified firm survey conducted by the SFSO. The targeted population are employees working at Swiss firms. Therefore, the survey does not comprise the self-employed, the unemployed, or the inactive population. We use three waves for 2012, 2014, and 2016. Each wave comprises about 1.6 mio. individuals.

The SESS covers about 40% of the population, that is employees working at a Swiss firm (see Figure 2).[11] Once a firm is chosen to be in the sample, participation is mandatory. Still, the response rate was 82% in 2012 and decreased to 73% in 2016 (Swiss Federal Statistical Office, 2016, 2018). Firms can chose between a paper-based and an online questionnaire, or submit the payroll information directly via an electronic interface from their accounting software. Medium and large firms can choose to report every second and every third employee, respectively. If they do so, they are advised to randomize the selection. About 3/4 of medium and large firms

[8]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.

[9]The relevant salary is very broad and captures many income sources (Information Center OASI/DI, 2020). For example, the data include incomes that are usually not covered by wage surveys, such as insurance receipts after accidents, remuneration of limited partnerships, or daily disability insurance payments.

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

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

still 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. Firms report either the contractually agreed or actual number of working hours. In addition, the survey comprises detailed information on contract, employee, and firm characteristics.[12]

The SESS allows us to measure wage rigidity, because we observe income as well as a standardized full-time equivalent income. In what follows, we explain how we construct a measure of the contractual wage, that is, income corrected for changes in working hours. 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 construct three different 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 the anonymous social security number. We cannot do so for firms because the firm identifier is randomized in each wave. Therefore, we construct a proxy of whether an individual stayed at the same company using information on tenure. If tenure increases by two years between each wave we assume that a person stays at the same company.

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 observations have a permanent contract we drop the observation with the lower base income (2% of the remaining sample). Because most farmers are self-employed we drop the agriculture sector (0.01% of

[12]The SFSO validates and completes some of these characteristics with register data.

[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.

the sample). Also, we drop very few observations with a negative income (0.07% of the sample). Because OASI is likely to exhibit less reporting error we additionally perform an outlier detection procedure (see Online Appendix A for details). Using outlier-robust estimates from a linear regression we predict SESS income with the employment income from OASI.[15] We remove all observations from SESS that deviate more than 150% from this prediction.[16] In line with the idea that reporting error in the SESS declined over time, as more firms switched from paper-based to electronic surveys, the share of outliers declines from 2.2% in 2012 to 1.5% in 2016.[17]

3.3 Representativity and weighting

Analyzing wage rigidity with SESS data involves several sample selection problems. The SESS is not a random sample of Swiss employees, but rather, a stratified firm survey. Although we obtained sampling weights from the SFSO, these are not valid because our sampling decisions are unlikely to randomly remove observations. For example, we suspect that smaller firms are more likely to use the paper survey and therefore these responses suffer from more serious reporting error. In addition, analyzing wage rigidity requires two consecutive wage observations. Because medium and small firms randomize workers, if they only report part of their workforce, the sample is biased towards large firms if we compute wage changes at the worker level.

Table 1 shows aggregate statistics for income and employment based on different data sets and weighting schemes. The first two columns show aggregate representative statistics for median income and employment for comparison. The third to fifth columns show our own estimates based on OASI and SESS data. The sampling decisions introduce an upward bias in income, and a downward bias in employment (panel a). This is the case using the unweighted OASI data, as well as using the SESS data with official sampling weights. Conditioning on

[15] All variables are transformed using the natural logarithm.

[16] Note that we may expect relatively large differences between the two sources for people becoming unemployed, as well as, people having multiple occupations. Therefore, we remove only very large deviations from the SESS.

[17] About half of the firms in the SESS report with a paper survey form, which is likely affected by more serious reporting error. 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.

observing a wage change exacerbates these biases (panel b). The median income is even higher because we select individuals that are more likely to remain in the SESS over an extended period and these employees earn higher incomes.

Table 1 — Data and weighting 2014

(a) Conditional on being in SESS after sampling decisions

	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 observing bi-annual wage change after sampling decisions

	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

Notes: Aggregate statistics refer to official statistics or estimates based on the population of OASI data. Sample estimates give statistics based on observations in the SESS after our sampling decisions (panel a) and restricted to individuals with two consecutive wage observations (panel b). Because the SFSO reports only gross median income, we subtract an estimate of the social security charges in 2014 (14.32%). See Table C.11 in the Online Appendix for data sources.

Therefore, we construct new sampling weights to compute representative statistics (see Online Appendix B for details). For each year and each sampling scheme we estimate a separate Probit model on the population of OASI data. The dependent variable is an indicator variable, 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.

Using this procedure, we are able to recover representative aggregate statistics (see fourth column in panels (a) and (b)). Although the two samples make up only 1/3 and 1/5 of the population, we accurately estimate the median income and employment in 2014.[18] Unless otherwise stated, we therefore report weighted statistics using appropriate weights for each sampling scheme.

3.4 Descriptive statistics

Previous research has shown that bonus payments, hourly wages, or wages for individuals changing jobs 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). In what follows, we confirm these findings for Switzerland. However, we also find these categories account for a relatively small share of the population.

Table 2 shows that 91% of income stems from the base income.[19] Irregular income, including bonus payments, accounts for 3% of employment income. 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 an open ended contract.

How rigid are wages in Switzerland? Recall that inflation was slightly negative between 2012 and 2014. Nevertheless, we observe more base wage increases than decreases (Table 3). The table also reports two measures of downward nominal wage rigidity. The share of wage freezes attributes wage changes smaller than 0.02% to 0%. The share of wage cuts prevented is calculated as the share of wage freezes divided by the share of wage freezes and cuts (Dickens et al., 2007). 7.7% of all base wage changes are freezes and 17.9% of wage cuts are prevented. This figures are slightly lower than the bi-annual wage rigidity statistics reported by Fallick et al. (2016) for the United States. In addition, The degree of wage rigidity depends on how we measure the wage. Downward rigidities mostly affect the base wage. The total wage, which includes bonus payments, 13th month pay and pay for Sunday/night work, is more flexible. Finally, if we use income instead of the wage measures, the share of wage freezes and the share of wage cuts prevented is even lower. This highlights that we need wage components and

[18] Results for 2016 are reported in Online Appendix B.

[19] We also computed the share of the base income in the total payroll paid by firms according to firm size (see Online Appendix C). This share ranges from 88% to 92% in 2014 and 2016 across different firm sizes.

Table 2 — 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 (except share of base income in firm's payroll). Unless otherwise stated the variables are indicators with values of 1/0.

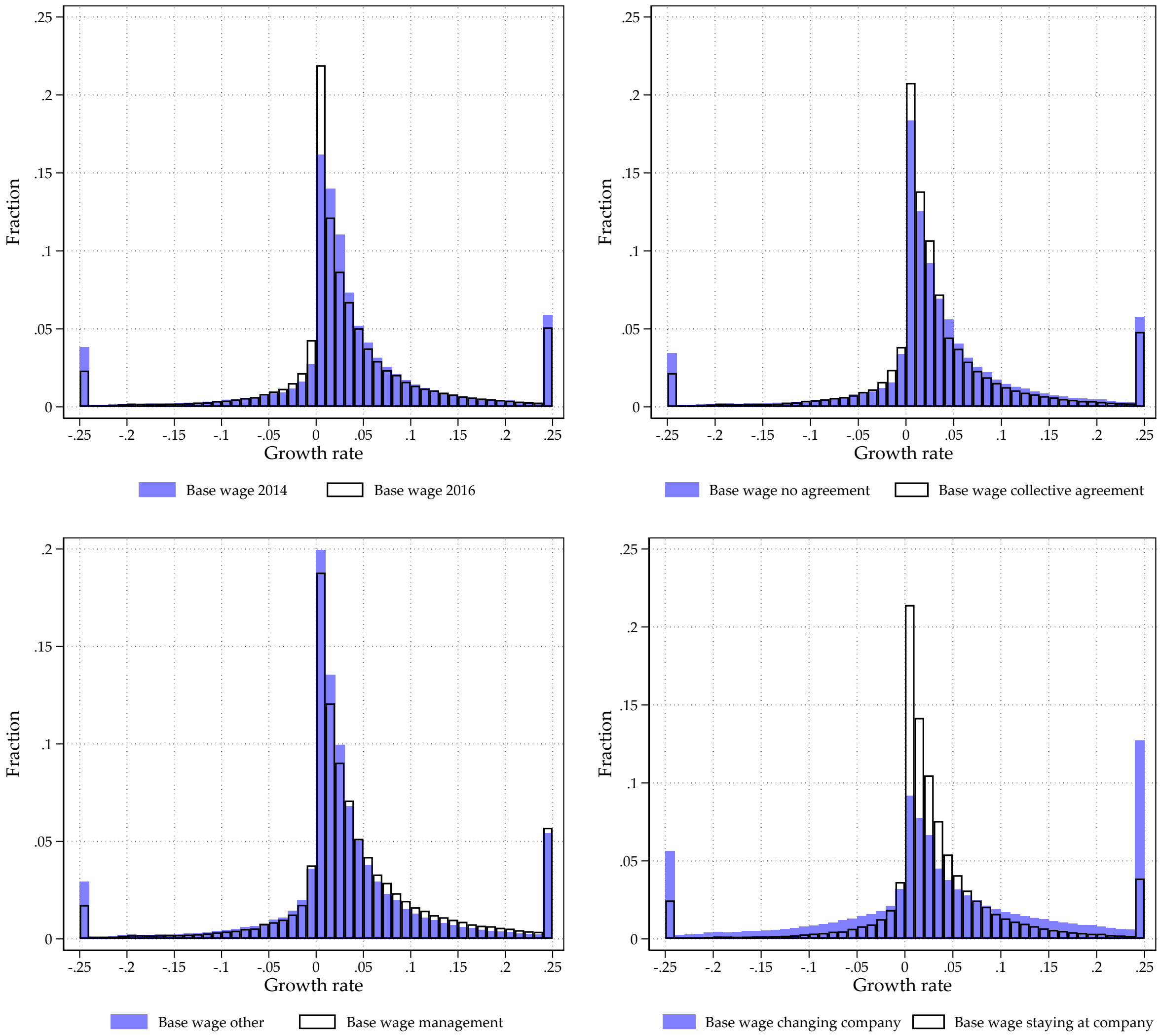
Table 3 — Wage rigidity statistics for 2014

	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	17.9
Regular wage	67.2	27.3	5.5	10.1
Total wage	63.7	34.0	2.3	3.4
Employment income (SESS)	57.5	41.6	1.0	1.2
Employment income (OASI)	57.7	41.4	1.0	1.2

Notes: Wage rigidity statistics based on bi-annual 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). 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).

accurate measures of working hours to identify downward nominal wage rigidity.

Figure 3 — Distribution of bi-annual base wage growth



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

Figure 3 shows bi-annual base wage growth distributions for various characteristics and time periods. All histograms display a pronounced asymmetry around the origin. Small wage increases are more frequent than small wage cuts. In addition, the Swiss franc appreciation in 2015 led to a leftward shift of the wage change distribution in 2016. After the removal of the Swiss franc floor in 2015, the CPI fell by 1.1% and 0.4% in 2015 and 2016, respectively. However, the share of wage freezes has increased by more than the share of small wage cuts. This suggest that wages do not fall because of a nominal rigidity. 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.[20] The histograms for the two groups are similar.[21] Also, there is little difference between the histogram for employees with and without a management function. Therefore, although managers may have more flexible bonus payments, their base wage is still relatively downward rigid. Finally, we observe that wages are more flexible for employees moving to another company.

Table 4 — 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	17.9
Activity and contract				
Tenure shorter than 5 years	70.6	25.1	4.3	8.6
Tenure longer or 5 years	71.1	19.5	9.4	24.1
No management	70.5	22.4	7.1	15.8
Management	69.7	21.0	9.4	22.3
Temporary contract	60.9	33.4	5.8	8.6
Open-ended contract	71.3	20.9	7.8	18.6
Monthly pay	72.8	18.4	8.8	23.8
Hourly pay	62.1	35.3	2.6	3.7
Changed firm	61.7	35.5	2.8	3.9
Stayed at firm	73.1	18.1	8.8	24.4
Employee				
Older than or 40 years	67.1	23.0	9.9	21.5
Younger than 40 years	77.3	18.8	4.0	10.6
Men	72.4	18.3	9.3	25.3
Women	69.8	23.7	6.5	13.8
University degree	70.2	22.1	7.7	17.4
No university degree	72.0	21.0	7.0	16.7
Foreigner	70.3	21.8	7.9	18.2
Swiss	72.8	20.3	6.9	16.9
Firm				
Private sector	71.7	21.8	6.6	15.1
Public sector	68.7	20.4	10.9	26.6
No collective agreement	67.9	24.1	8.0	16.6
Collective agreement	73.1	19.8	7.1	17.9
Small firm	60.6	29.4	10.0	17.0
Medium firm	62.9	28.4	8.7	15.4
Large firm	73.4	19.4	7.3	18.8

Notes: Wage rigidity statistics based on bi-annual 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.

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

[21] Theory and empirical evidence suggest labor unions care more about real than nominal wages (Babecký et al., 2010).

Table 4 shows results for various contract, employee, and firm characteristics.[22] For brevity, we focus on results that differ substantially from the overall statistics. Wages are more flexible for employees that are young and have a short tenure. Wage cuts are more common early in an employee's career. Workers with hourly pay, temporary contracts, or that change the firm exhibit more flexible wages.[23] Women experience more flexible wages than men.[24] Finally, there are relatively small differences between nationality, education level, and firm size.

4 Identification and estimation

The previous section showed that, on the one hand, downward nominal wage rigidity is important because the base wage accounts for a large fraction of income. On the other hand, only 7.7% of all bi-annual base wages changes are freezes. We next ask whether base wage rigidites have allocative effects and whether these effects matter at the aggregate level.

We exploit a unique natural experiment that led to a sharp appreciation of the Swiss franc and a substantial decline in the price level (see Bonadio et al., 2020; Kaufmann and Renkin, 2019, for a detailed description). Figure 4 shows the Swiss CPI (left-hand scale) along with the CHF/EUR exchange rate (right-hand scale). Both series are measured in natural logarithms and are normalized to 0 in December 2014. There are two sharp appreciation phases. Before the exchange rate floor was introduced in September 2011, the Swiss franc appreciated by about 30% over one year. With some delay, this led to a decline in the price level that came to a halt after the SNB introduced the exchange rate floor in September 2011. During most of the exchange rate floor period, the exchange rate and the price level remained stable. In January 2015, the SNB surprisingly abandoned its exchange rate policy, which led to a 10% appreciation. In addition, the CPI declined on average by 1.1% in 2015 and 0.4% in 2016.

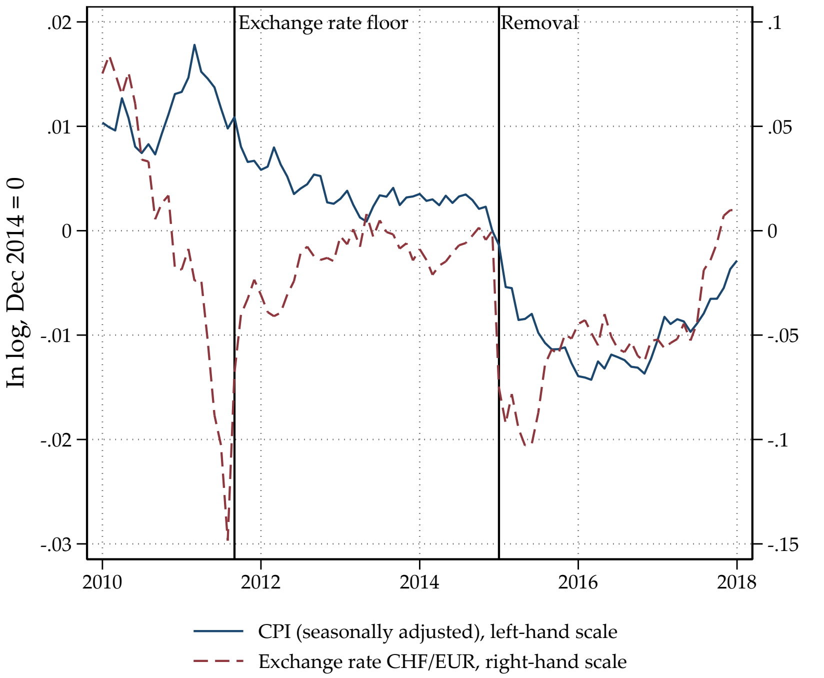
The left panel of Figure 5 shows a stylized depiction of our identification strategy. We identify the role of wage rigidity by comparing individuals with wage freezes (treatment group) in 2014 to individuals with small wage cuts (control group). The key assumption is

[22] Detailed wage rigidity statistics according to socio-economic and firm characteristics are provided in Online Appendix C.

[23] See also the histograms for wage growth for these categories in Online Appendix C.

[24] This finding is also related to the fact that 25% of women receive hourly wages (12% of men).

Figure 4 — The Swiss franc shock

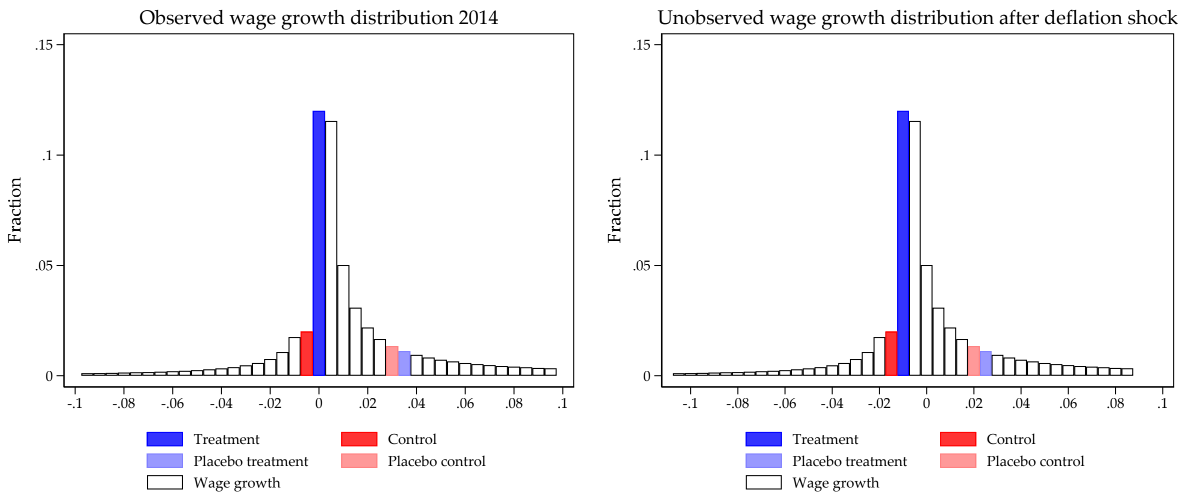


Source: SNB, SFSO, own calculations, see Table C.11 in the Online Appendix.

that individuals with small wage cuts are similar to individuals with wage freezes, except for the wage rigidity. In 2015 and 2016, the wage change distribution shifts to the left because of the deflationary shock (right panel). This implies that for some individuals with wage freezes (treatment group), the shock would require a wage cut. The distribution after the deflationary shock is unobserved, however, because firms may instead lay off employees. But we observe the incomes and unemployment history of those individuals in the OASI data. Therefore, we can estimate differences in income and labor market outcomes between treatment and control group in 2015 and 2016.

The figure also shows that we can define placebo treatments, comparing individuals in adjacent bins in other parts of the wage growth distribution. If the effects we identify stem from the discontinuity near the origin of the wage growth distribution, the outcomes are similar for the placebo treatment and control groups. On the one hand, individuals with relatively large wage cuts have flexible wages to begin with. On the other hand, individual with large wage increases are those with higher productivity growth. Therefore, even a 1% deflationary shock

Figure 5 — Stylized depiction of 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.

may not require a wage cut, but rather, a smaller wage increase.

Ideally, the treatment and control groups differ only with respect to the nominal wage rigidity, but not with respect to other characteristics. Table 5 shows that this is not strictly the case. Given the number of observations, it does not come as a surprise that the average characteristics between treatment and control group are statistically significantly different. For example, individuals with wage freezes tend to earn higher incomes and obtain less income from unemployment benefits. In addition, their tenure tends to be longer and they tend to be older.

We therefore use a two-step approach to control for the fact that individuals with certain unobserved characteristics related to selection into treatment are differently affected by the deflationary shock. First, 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, depends linearly on observed ($\mathbf{x}_{i,2014}$) and unobserved

Table 5 — 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
Unemployment benefits (in 1,000)	-0.08***	0.01	0.04	68,662	0.12	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.

($\nu_{i,2014}$) characteristics:[25]

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

Based on this assumption we can estimate the inverse Mills ratio from a Probit with 2014 data on the treatment and control group:

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

As control variables, we include a wide range of socio-economic, contract, and firm characteristics (see Online Appendix C). Then, we compute the inverse Mills ratio for each individual (see, e.g., Wooldridge, 1995):

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

We see that the inverse Mills ratio gives the expected value of the unobserved characteristics relevant for selection into treatment conditional on observed characteristics and the treatment dummy.

Second, we estimate a difference-in-differences model on the matched data set including the inverse Mills ratio:[26]

$$\begin{aligned} y_{i,t} \;=\; & \sum_{j \notin 2014} \mathbf{1}\{t = j\} \times \Big[\alpha_j \mathbf{1}\{\Delta w_{i,2014} = 0\} + \beta_j \lambda_{i,2014} \\ & + \delta_j \mathbf{1}\{\Delta w_{i,2014} < -c\} + \gamma_j \mathbf{1}\{\Delta w_{i,2014} > 0\} + \eta_{f,2014}\Big] + \theta_i + \varepsilon_{i,t} \;. \end{aligned} \tag{1}$$

where $y_{i,t}$ is the outcome variable of interest. We saturate the model with time dummy variables 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 dummy variables with an indicator for wage freezes ($\mathbf{1}\{\Delta w_{i,2014} = 0\}$), the inverse Mills ratio ($\lambda_{i,2014}$), indicators for large wage cuts ($\mathbf{1}\{\Delta w_{i,2014} < -c\}$), wage increases ($\mathbf{1}\{\Delta w_{i,2014} > 0\}$), and firm fixed effects

[25] We drop the constant for readability.

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

($\eta_{f,2014}$). Finally, we control for individual fixed effects (θ_i) and ε_{it} denotes an error term. The firm fixed effects capture that some firms and sectors may be more strongly affected by the deflationary shock. Moreover, the individual fixed effects capture differences of individuals that remain constant over time, such as gender, for example.

The main coefficients of interest (α_j) identify 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 our baseline model, we set $c = 0.5\%$.

5 Causal effects of wage rigidity

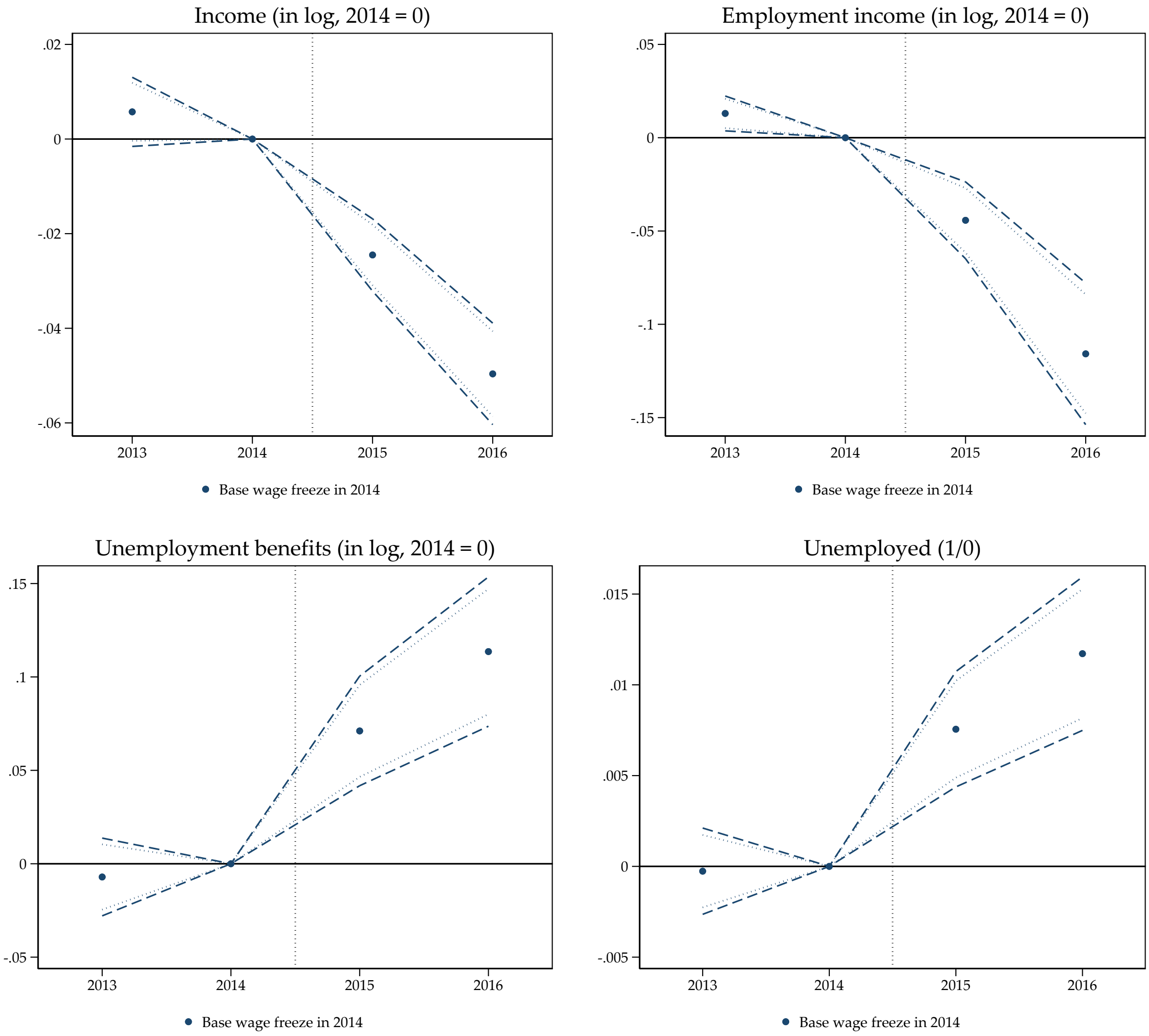
We first discuss the average impact of wage rigidities close to the origin of the wage change distribution. Then, we estimate the impact on aggregate income and unemployment. Thereafter, we present estimates that are robust with respect to measurement errors. Finally, we discuss placebo and robustness tests.

5.1 Local effects

Wage freezes have a relevant negative impact on income and unemployment. Figure 6 and Table 6 show 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. We follow Lee and Card (2008) and cluster standard errors according to the forcing variable exhibiting a discontinuity. In our case, these are unique values in the base wage growth distribution in 2014.[27] The estimates in 2015 and 2016 are statistically significant at conventional significance levels. Income declines by 2.4% and 4.9% in 2015 and 2016, respectively. Employment income even declines by 4.4 and 11.6%. The reason why employment income falls more than total income is, that individuals that become unemployed receive unemployment benefits. Indeed, by 2016 unemployment benefits increase by 11.3%, while the probability of becoming unemployed increases by 1.2 percentage points.

[27] Clustering at the firm level yields slightly larger standard errors. But all results are robust with respect to this alternative.

Figure 6 — 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 6 — 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	0.006	0.013***	-0.007	-0.000
	(0.004)	(0.005)	(0.011)	(0.001)
2015	-0.025***	-0.044***	0.071***	0.008***
	(0.004)	(0.011)	(0.015)	(0.002)
2016	-0.050***	-0.116***	0.114***	0.012***
	(0.005)	(0.019)	(0.020)	(0.002)
Observations	3,348,220	3,348,220	3,348,220	3,348,220
Adj. R-sq. (between)	0.810	0.421	0.297	0.294
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. The estimates are normalized to 0 in the base year 2014. 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.

5.2 Aggregate effects

The local effects are substantial. Because only 7.7% of observations in 2014 were wage freezes, however, these effects are not representative for the entire Swiss economy. To show whether downward nominal wage rigidity has relevant aggregate effects, we use the model estimates to predict, for each individual, income and the probability of being unemployed. Then, we predict a counterfactual the wage freeze dummy set to zero. Finally, we aggregate the predictions with own sampling weights for 2014.

Not surprisingly, perhaps, the aggregate effects are smaller than the local effects. Nevertheless, Table 7 shows that wage freezes cause lower income and higher unemployment. After the 1% decline in the price level, employment income falls by 1.0%. Because of unemployment benefits the impact on total income is smaller (-0.4%). Finally, we observe an increase of the number of unemployed after 2014. Even without wage rigidities, unemployment would have increased after the removal of the exchange rate floor. But because some wages are rigid unemployment was 2.1-2.3% higher.

5.3 Accounting for measurement errors

Controlling for measurement error in wage data is key when analyzing wage rigidity (see e.g. Gottschalk, 2005). In our case, the wage freeze dummy may be measured with error.

Table 7 — 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	58.49	0.01	58.43	58.39	0.07	8.01	8.03	-0.22
2014	56.82	56.82	0.00	56.82	56.82	0.00	0.48	0.48	0.00
2015	58.17	58.26	-0.16	57.63	57.81	-0.31	20.63	20.16	2.33
2016	59.00	59.24	-0.39	57.05	57.60	-0.96	34.32	33.61	2.12

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.

Measurement errors in categorical indicators result in a so-called misclassification bias (Aigner, 1973; Card, 1996). To control for measurement error in the wage freeze classification, we 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 classifications 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 classifications agree for an observation, it is less likely that the observation is classified incorrectly.

Table 8 — Accounting for measurement error in wage freeze indicator

	Income (in log)	Employment income (in log)	Unemployment benefits (in log)	Unemployed (1/0)
2013	0.003	0.005	-0.018	-0.001
	(0.010)	(0.016)	(0.088)	(0.009)
2015	-0.084***	-0.142***	0.195**	0.022**
	(0.022)	(0.044)	(0.095)	(0.010)
2016	-0.056*	-0.222**	-0.586	-0.047
	(0.029)	(0.111)	(0.868)	(0.077)
Observations	2,005,166	2,005,166	2,005,166	2,005,166
Adj. R-sq. (between)	0.827	0.442	0.283	0.280
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. The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014). 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.

We compute two potentially error-ridden classifications based on the bi-annual wage change from SESS and the annual employment income change from OASI data.[28] Then, we estimate the model on a subsample, where SESS and OASI yield the same classification. Because employment income is more volatile than the base wage, we define absolute base wage changes smaller than 0.05% as wage freezes (instead of 0.02%). In addition, we set the control group threshold $c = -0.1\%$.

The results are based on a smaller sample and therefore less precisely estimated. Qualitatively, we find similar effects, however. Table 8 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. In addition, there is a (temporary) increase in unemployment benefits and an increase in the probability of being unemployed.

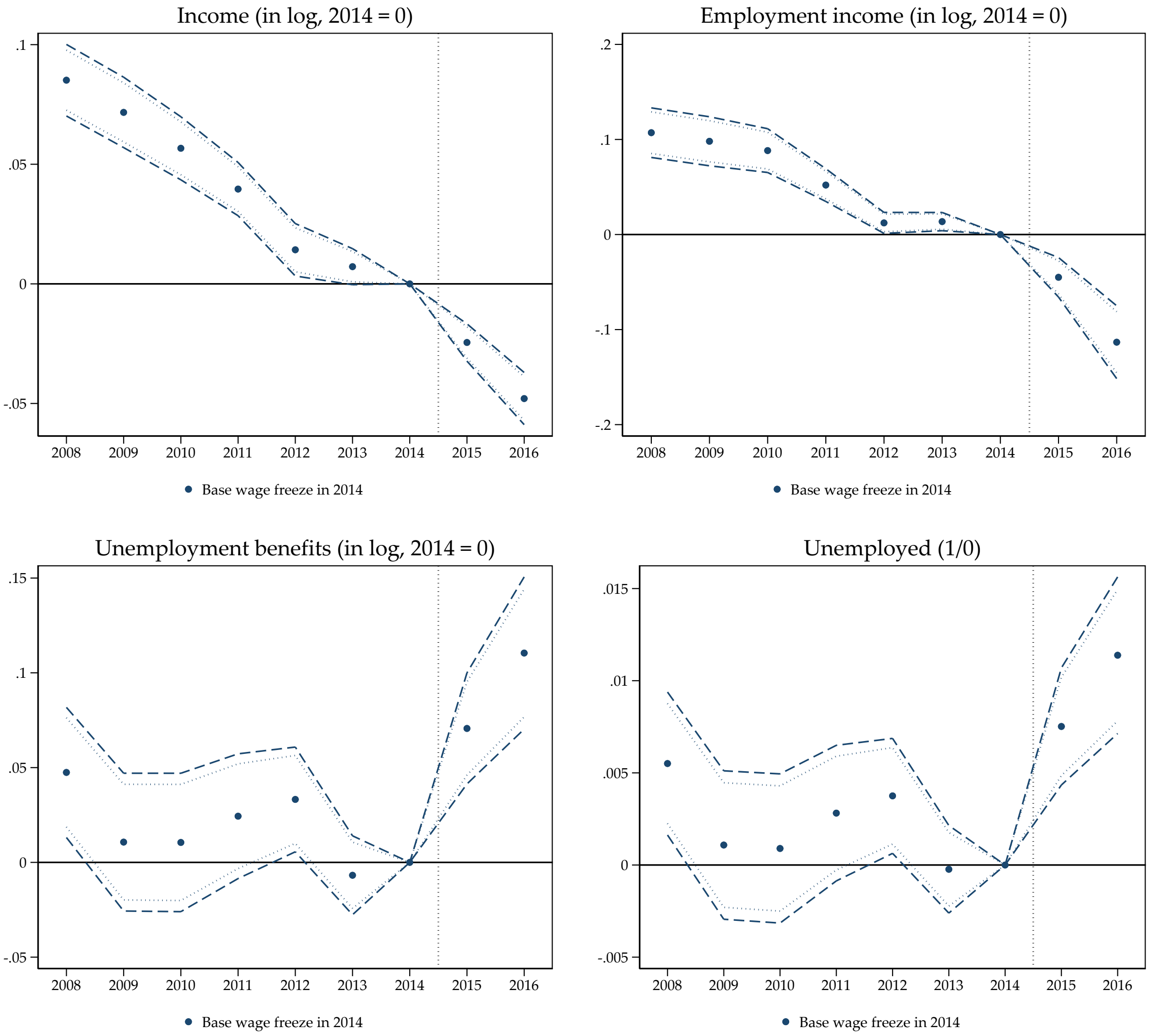
5.4 Placebo tests

We conduct three types of placebo and specification tests. First, we examine pre-treatment trends. Figure 7 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. During the exchange rate floor period (2011–2014), the point estimates are close to 0. Similarly, for unemployment we observe an uptick in 2012, which may stem from a delayed impact of the sharp appreciation in mid-2011. The value for 2013, however, is not significantly different from the value in 2014.

Second, we examine placebo treatments over the wage growth distribution in 2014. We define treatment bins with a width 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 we really pick up 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 8 shows for 2015, the only significantly negative coefficient is the one for the treatment bin $[0, 0.005)$. The coefficients are significantly positive for two treatment bins

[28] We treat the two classifications as noisy measures of the true classification because both measures have advantages and disadvantages. The SESS indicator controls for working hours and measures the contractually agreed wage. However, it is more likely affected by reporting errors. In addition, we only observe bi-annual wage changes. By contrast, OASI income has the advantage that we can compute the annual change between 2014 and 2013. The downside of OASI data is that we do not observe working hours, and we include multiple occupations and income sources.

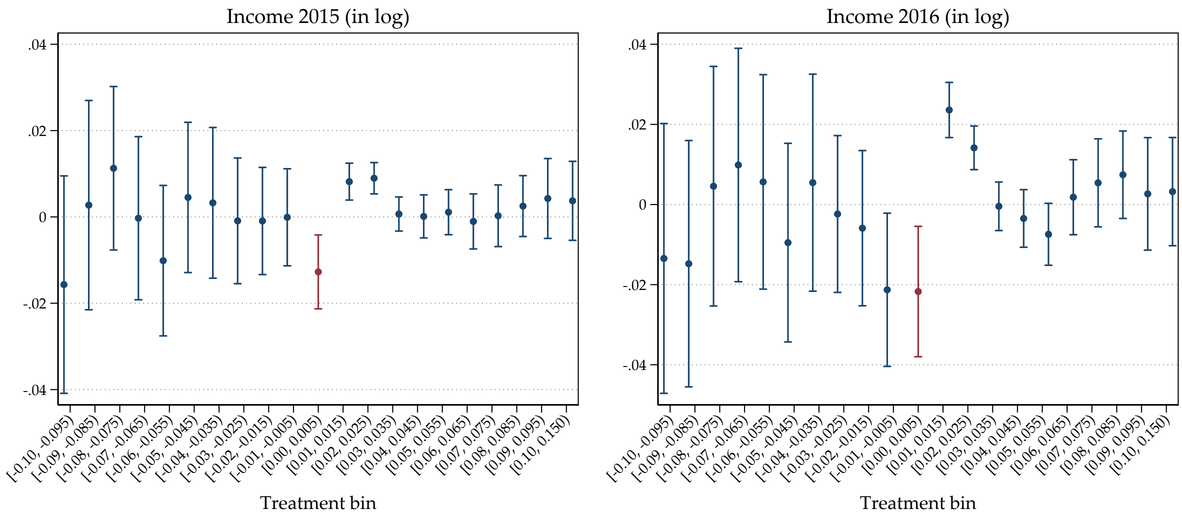
Figure 7 — Pre-treatment trends



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.

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 2016, the results are similar. The only difference is that we also find a significantly negative effect for the bin covering $[-0.01, -0.005)$.

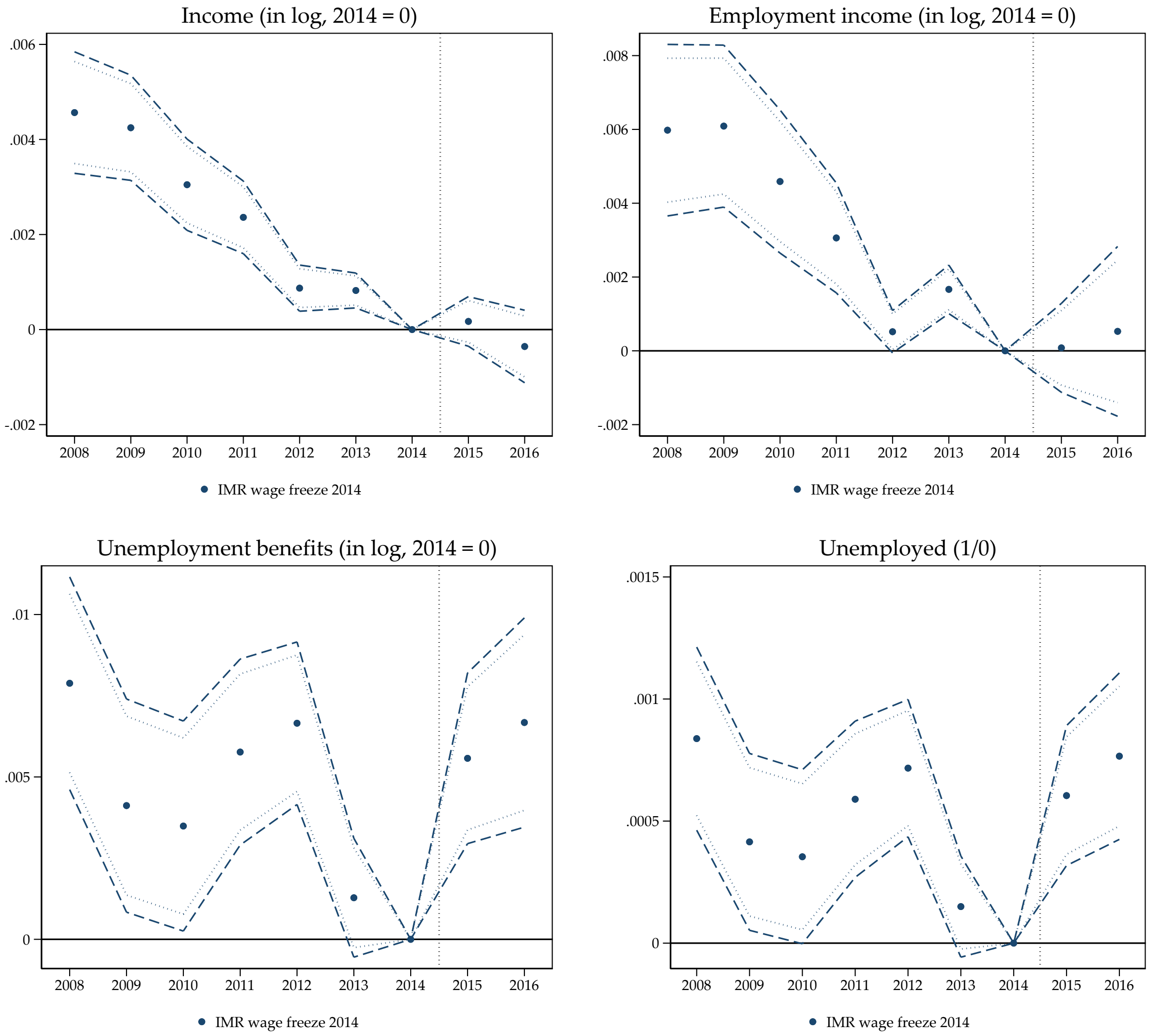
Figure 8 — Placebo treatments



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.

Third, we examine the interaction term with the inverse Mills ratio. These interactions show whether individuals in the treatment group experience different outcomes because of unobserved factors explaining selection into treatment. Given the large number of observations, we expect this interaction to be statistically significant. Compared to the causal effects the interactions with the inverse Mills ratio are small in absolute value (see Figure 9). Therefore, although our treatment and control groups are not entirely random with respect to unobserved characteristics, the impact of these characteristics is economically small.

Figure 9 — Interaction with inverse Mills ratio



Notes: Coefficient on interaction term with inverse Mills ratio in 2014 for total income, employment income, unemployment benefits, and the unemployment indicator. The effect is 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.

5.5 Robustness tests

Table 9 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 1 and 1.5 percentage points in 2015 and 2016, respectively.

Panel (b) reports results using different controls. For brevity we only report the impact on total income. In line with the idea that unobserved factors have an economically small effect on the results, we find similar effects when dropping the inverse Mills ratio from the model. We also control for age- and job type-time effects. The estimates are smaller when controlling for age-time effects, but are still statistically significant. 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 6.

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 because 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.

6 Concluding remarks

We show that downward nominal wage rigidities are a pervasive phenomenon in a country with low inflation and a flexible labor market. In addition, these rigidities have relevant allocative effects. We compare individuals with sticky and flexible wages after an unexpected 1% decline in the price level. On average, people with sticky wages experience a decline

Table 9 — 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	0.015	0.006	0.013***	0.000
	(0.013)	(0.004)	(0.005)	(0.001)
2015	-0.183***	-0.024***	-0.044***	-0.009***
	(0.029)	(0.004)	(0.011)	(0.002)
2016	-0.159***	-0.050***	-0.116***	-0.015***
	(0.027)	(0.005)	(0.019)	(0.003)
Observations	314,944	3,348,220	3,348,220	3,348,180
Adj. R-sq. (between)	0.735	0.810	0.420	0.276
Adj. R-sq. (within)	0.001	0.000	0.000	0.000

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

	No inverse Mills ratio	Age time effects	Job type time effects	Additional quantile controls
2013	0.006	0.001	0.006	0.005
	(0.004)	(0.003)	(0.004)	(0.004)
2015	-0.025***	-0.008**	-0.023***	-0.022***
	(0.004)	(0.004)	(0.004)	(0.004)
2016	-0.050***	-0.013**	-0.048***	-0.044***
	(0.005)	(0.005)	(0.006)	(0.006)
Observations	3,348,220	3,017,503	3,207,520	3,348,220
Adj. R-sq. (between)	0.810	0.830	0.810	0.811
Adj. R-sq. (within)	0.000	0.000	0.000	0.000

(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	0.008**	-0.001	0.000	0.005
	(0.003)	(0.005)	(0.002)	(0.004)
2015	-0.025***	-0.020**	-0.013**	-0.021***
	(0.003)	(0.009)	(0.005)	(0.004)
2016	-0.042***	-0.058***	-0.027***	-0.042***
	(0.005)	(0.011)	(0.008)	(0.006)
Observations	3,348,220	3,348,220	3,348,220	2,778,043
Adj. R-sq. (between)	0.810	0.810	0.810	0.820
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. 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 last column shows results when controlling for a finer grid of quantile dummies for positive wage changes and wage changes smaller than $-c$. 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. ***/**/* 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.

in income (employment income) by 5% (12%). Moreover, the probability of becoming unemployed raises by 1.2 percentage points. A key novelty of this study is that we compute representative aggregate allocative effects. Downward nominal wage rigidities cause a fall in income (employment income) by 0.39% (0.97%). In addition, unemployment increases by 2.11%. Therefore, even though downward nominal wage rigidities affect only a modest share of employees, these rigidities matter at the aggregate level.

These findings have implications for monetary policy and the optimal level of the inflation target. Choosing the inflation target too low makes it more likely that downward nominal wage rigidities bind. If this is the case, our study suggests that unemployment rises. Therefore, downward nominal wage rigidities should be taken into account when discussing the optimal level of inflation in theory and in practice.

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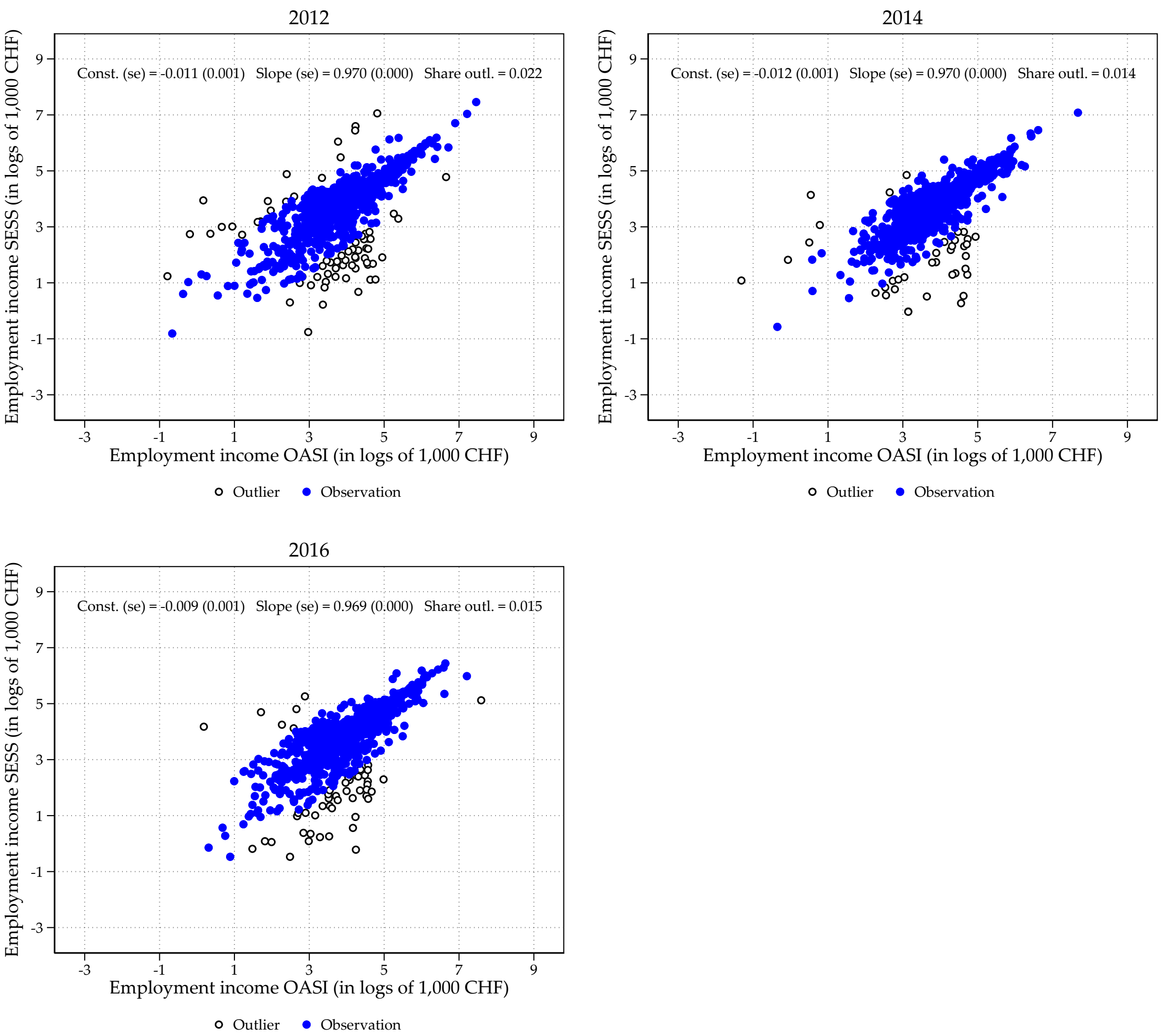
[1] Other outlier-robust estimators yield similar results.

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, the SESS is a stratified survey. However, the sampling weights from the SFSO are not valid because the sampling decisions are unlikely to randomly remove observations. Moreover, 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. In addition, because small and medium firms can choose to report only part of their workforce, the individuals selected are more likely to work at a large firm.

These sampling problems introduce relevant biases in aggregate statistics (Table B.1). 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 (Panels a and b). This bias stems from the sampling decisions. If we additionally condition on observing a bi-annual 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 bi-annual 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.[2] $\mathbf{x}_i$ comprises variables that explain whether an individual is observed in the subsample. We control for 400 percentiles of the employment income distribution according to OASI, as well as dummy variables for unemployment and self-employment.

We then compute the probability that an individual with characteristics $\mathbf{x}_i$ is included in the sample:

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

Then, we use the inverse of this probability 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

[2]For ease of exposition, we do not add time subscripts. But we estimate a separate Probit for each year.

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 bi-annual 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 bi-annual 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.

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.[3] The coefficients have the expected sign. 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).

[3] We do not report the coefficients on indicators for 400 percentiles of the employment income distribution for brevity.

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 bi-annual 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 bi-annual 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.

Table B.3 — Data and weighting 2016

(a) Conditional on being in SESS after sampling decisions

	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

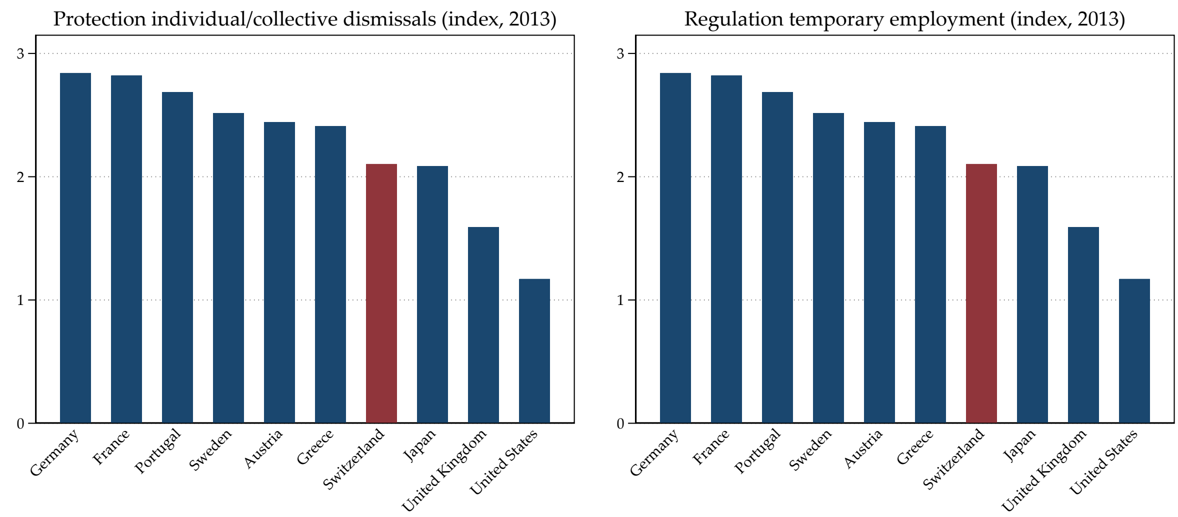
(b) Conditional on observing bi-annual wage change after sampling decisions

	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

(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. 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	23.2
Regular wage	67.7	29.4	2.9	5.0
Total wage	62.6	36.2	1.2	1.6
Employment income (SESS)	55.6	43.8	0.5	0.6
Employment income (OASI)	53.5	45.4	1.1	1.3

Notes: Wage rigidity statistics based on bi-annual 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	24.1
Regular wage	72.6	21.5	5.9	13.8
Total wage	68.4	29.5	2.1	3.5
Employment income (SESS)	64.0	35.5	0.5	0.7
Employment income (OASI)	69.3	29.6	1.1	1.9

(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	32.2
Regular wage	72.7	24.1	3.2	6.6
Total wage	68.2	30.8	0.9	1.5
Employment income (SESS)	62.8	36.7	0.5	0.7
Employment income (OASI)	65.6	33.0	1.4	2.1

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	23.8
Regular wage	69.6	24.2	6.2	12.8
Total wage	65.5	32.1	2.5	3.8
Employment income (SESS)	61.2	38.0	0.8	1.1
Employment income (OASI)	61.7	37.2	1.1	1.5

(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	27.7
Regular wage	69.7	27.3	3.0	5.5
Total wage	63.7	35.2	1.0	1.5
Employment income (SESS)	58.5	41.0	0.6	0.7
Employment income (OASI)	56.2	42.5	1.3	1.5

Notes: All statistics based on own sampling weights.

Table C.6 — 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	23.2
Activity and contract				
Tenure shorter than 5 years	70.6	23.4	6.1	13.0
Tenure longer or 5 years	68.6	19.9	11.6	29.1
No management	69.0	21.5	9.5	22.1
Management	72.3	18.1	9.6	26.5
Temporary contract	66.5	26.3	7.2	13.6
Open-ended contract	69.3	20.8	9.9	23.7
Monthly pay	70.0	19.3	10.7	27.7
Hourly pay	64.6	31.8	3.6	5.7
Changed firm	63.4	31.6	5.0	7.8
Stayed at firm	70.6	18.5	10.9	29.5
Employee				
Older than or 40 years	64.8	22.9	12.4	27.0
Younger than 40 years	76.9	17.8	5.3	14.9
Men	67.7	21.0	11.4	27.1
Women	70.5	21.1	8.5	20.1
University degree	68.0	21.9	10.1	23.1
No university degree	74.1	19.0	6.8	17.9
Foreigner	69.2	21.4	9.5	22.1
Swiss	69.4	19.9	10.7	26.9
Firm				
Private sector	68.2	22.4	9.3	20.7
Public sector	71.6	17.6	10.8	30.6
No collective agreement	69.0	20.7	10.3	24.8
Collective agreement	69.5	21.4	9.1	21.3
Small firm	58.8	29.9	11.3	18.8
Medium firm	61.4	26.1	12.5	24.0
Large firm	71.6	19.3	9.1	23.5

Notes: All statistics based on own sampling weights.

Table C.7 — 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.7 – 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.7 – 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.7 – 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.7 – 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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	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.7 – 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.7 – 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.7 – 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.7 – 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.7 – 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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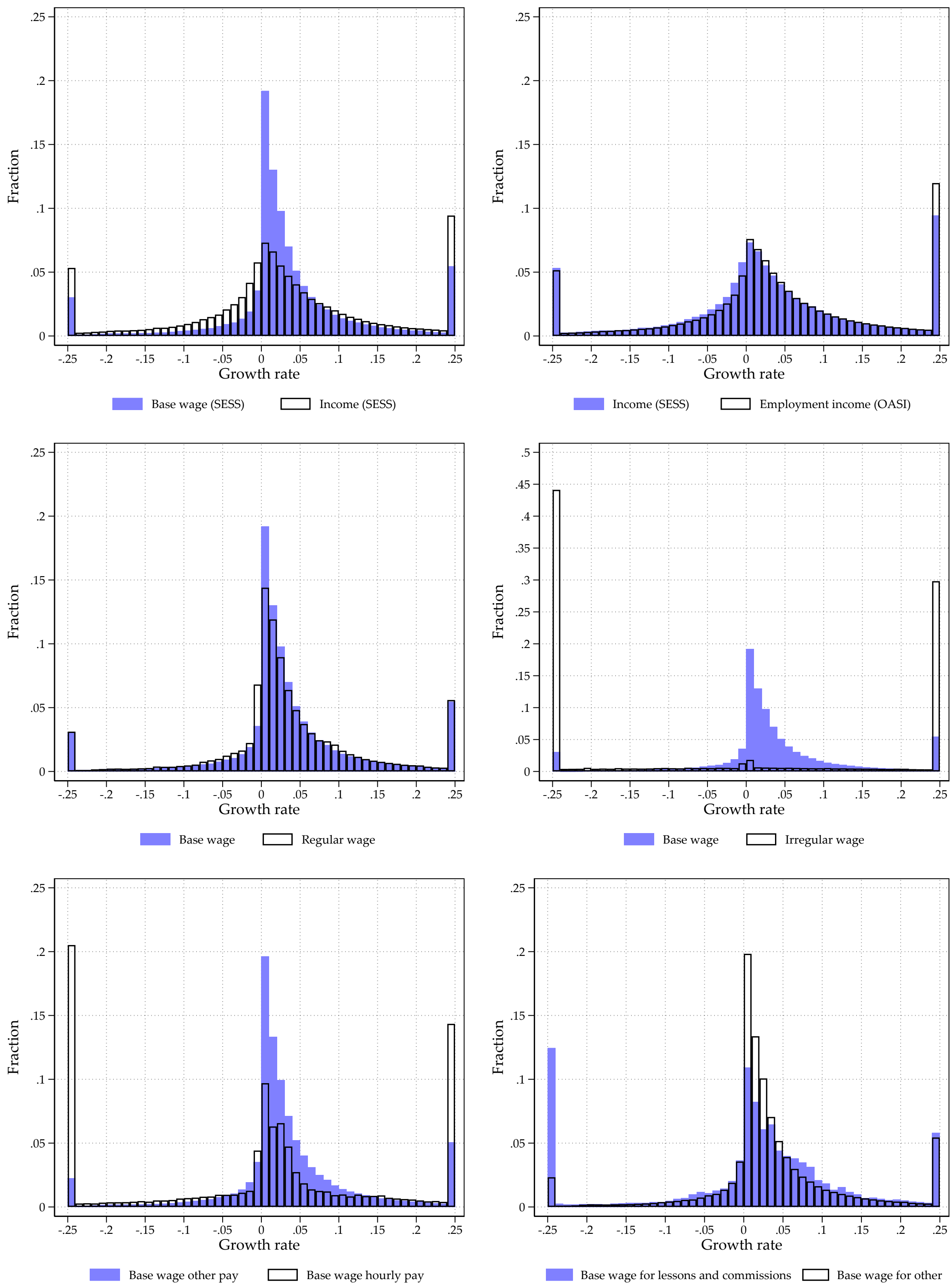
Table C.7 – continued from previous page

	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 bi-annual 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).

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.8 — Probit for inverse Mills ratio

	1/0 (wage freezes/small cuts)
Unemployed	-0.166**
	(0.065)
Open-ended (ann. working time)	0.284***
	(0.019)
Open-ended (hourly pay)	-1.072***
	(0.034)
Temporary (monthly pay)	0.136***
	(0.043)
Temporary (hourly pay)	-0.902***
	(0.099)
Lessons	-0.170***
	(0.045)
Other (e.g. commission)	0.153***
	(0.036)
Stays at company=1	0.586***
	(0.019)
Observations	79,193
Pseudo R-sq.	0.087

Notes: 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 C.8 — Probit for inverse Mills ratio (continued)

	1/0 (wage freezes/small cuts)
Collective agreement	0.029**
	(0.014)
20-49	-0.109**
	(0.050)
50-249	-0.214***
	(0.042)
250-999	-0.301***
	(0.042)
1000-	-0.392***
	(0.042)
Manufacturing	1.175***
	(0.238)
Electricity, gas a. steam supply	1.161***
	(0.245)
Water supply	1.512***
	(0.256)
Construction	0.857***
	(0.241)
Trade; rep. of motor vehicles a. moto.	1.042***
	(0.238)
Transportation and storage	0.757***
	(0.238)
Accomod. and food serv. act.	1.382***
	(0.242)
Information and communication	1.633***
	(0.239)
Financial and insurance activities	1.317***
	(0.238)
Real estate activities	1.291***
	(0.268)
Prof., scientific and tech. act.	1.097***
	(0.239)
Admin. and support serv. act.	1.305***
	(0.239)
Public administration and defence	1.370***
	(0.239)
Education	1.291***
	(0.239)
Human health and social work act.	1.230***
	(0.238)
Arts, entertainment and recreation	1.331***
	(0.247)
Other service activities	1.266***
	(0.247)
Public company	0.197***
	(0.019)
Observations	79,193
Pseudo R-sq.	0.087

Notes: 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 C.8 — Probit for inverse Mills ratio (continued)

	1/0 (wage freezes/small cuts)
Women	-0.194***
	(0.013)
Middle Management	0.048
	(0.038)
Lower Management	-0.128***
	(0.038)
Basic Management	-0.028
	(0.041)
Without Managament Function	-0.267***
	(0.034)
U Applied Sciences	0.272***
	(0.027)
Federal Certificate	0.314***
	(0.024)
Teacher Certificate	0.278***
	(0.075)
Higher School Certificate	0.611***
	(0.044)
Vocational Training	0.493***
	(0.020)
On-the-job Training	0.372***
	(0.034)
Compulsory Education	0.577***
	(0.027)
Observations	79,193
Pseudo R-sq.	0.087

Notes: 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 C.9 — 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

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