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

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Abstract: We study the causal effects of downward nominal wage rigidity after a deflationary monetary policy shock using Swiss data on employee-level contractual wages matched with income and employment from social security register data. We exploit the discontinuity around the origin of the wage growth distribution to compare the outcomes of individuals with wage freezes (treatment group) and small wage cuts (control group) before and after an unexpected decision by the Swiss National Bank leading to a 1% decline of the price level. Locally (that is, near the origin of the wage growth distribution), downward nominal wage rigidities cause a 4.4% decline in income and a 0.7 percentage point increase in the probability of unemployment. In the aggregate, income declines by 0.3% and the probability of unemployment increases by 0.05 percentage points.

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1 Introduction

A large empirical literature documents that monetary policy affects the real economy. To replicate these findings, theoretical monetary macroeconomic models often introduce frictions in the form of two types of wage rigidities (see Erceg et al., 2000; Blanchard and Galí, 2010; Schmitt-Grohé and Uribe, 2016; Born et al., 2019; Schoefer, 2021). On the one hand, infrequent wage adjustments cause involuntary unemployment and inefficient business cycles because the actual wage may deviate from the optimal desired wage. On the other hand, downward nominal wage rigidity (that is, a friction impeding wage cuts), may exacerbate recessions because the real wage increases after a deflationary shock. Olivei and Tenreyro (2007, 2010) provide convincing empirical evidence that infrequent wage adjustments amplify the response of the real economy to monetary policy shocks. It is more controversial, however, whether downward nominal wage rigidities matter for real economic outcomes (see Issing et al., 2003; Basu and House, 2016).[1] Indeed, providing empirical evidence on the interaction between downward rigid wages and monetary policy is challenging for at least two reasons. First, most central banks aim for positive inflation so that this friction may rarely bind.[2] Second, monetary policy usually responds endogenously to economic fluctuations which hampers estimating its causal effect on the economy.

This paper provides empirical evidence on the allocative effects of downward nominal wage rigidities after an exogenous deflationary shock. The analysis benefits from three distinct advantages of the Swiss case. First, the firm survey allows us to define a treatment group (employees with base wage freezes) and a control group (employees with small base wage cuts; that is, flexible wages). Because the survey comprises information about various income components and the activity rate, we can compute a measure of the contractual wage and remove income from bonuses and other irregular payments. We show that this distinction matters because changes in the activity rate and volatile bonus payments conceal downward nominal wage rigidity in the base wage. Second, we can match the firm survey to social

[1] There are various arguments against the relevance of downward nominal wage rigidity. They may be the result of an optimal implicit contract between the employee and the firm and thus may not cause allocative inefficiencies (Barro, 1977). The allocative inefficiencies may be small because firms optimally compress wage increases as well as decreases when wage rigidities are present (Elsby, 2009; Stüber and Beissinger, 2012). If the wage setting behavior of firms is state dependent, wages may be flexible when it matters most (Issing et al., 2003; Grigsby et al., 2021). 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., 2021; Kurmann and McEntarfer, 2019). Therefore, bonus payments are an additional margin firms may use to cut nominal wages during recessions.

[2] Indeed, one reason why the central bank may aim for a positive inflation target to facilitate real wage cuts and therefore mitigate the adverse effects of recessions on the labor market (see Tobin, 1972; Akerlof et al., 1996; Bernanke, 2003; Schmitt-Grohé and Uribe, 2013; Billi and Kahn, 2008; Kim and Ruge-Murcia, 2009).

security register data on income and employment (covering the universe of the working age population). In contrast to the firm survey, the social security data comprise incomes of the unemployed, inactive, or self-employed. This is key to estimate the impact of downward nominal wage rigidity on income and the employment history. Third, we analyze the impact in response to an exogenous deflationary shock. We exploit a unique natural experiment, namely an 1% decline in the price level caused by an unexpected removal of the Swiss National Bank's exchange rate floor policy in January 2015 ('Swiss franc shock').[3] Importantly, CPI inflation stood at 0% in 2014 and fell to negative territory after the shock. This mildly deflationary environment therefore lends itself to analysis of the role of binding downward nominal wage rigidities. For estimation, we use a difference-in-differences model with, among other controls, firm-level time effects. We therefore identify the effects from variation before and after the Swiss franc shock between individuals working at the same firm with downward rigid and flexible wages.

Our main findings can be summarized as follows. Locally, that is, around the origin of the wage change distribution, the difference between the treatment group and the control group is large. Compared to the control group, income declined by 4.4%. Moreover, the probability of becoming unemployed is 0.7 percentage points higher for the group with downward rigid wages. Employment income, which does not include unemployment benefits or income from self-employment, declined by 10.8%. The difference in the response of income and employment income suggests that unemployment benefits partly offset the negative impact of downward nominal wage rigidities. We use the estimates to make local counterfactual predictions for the treatment group, which we then aggregate with actual observations for other individuals to representative aggregate statistics. Because only 7.7% of the population is affected by wage freezes in 2014 the aggregate effects are smaller than the local effects. Downward nominal wage rigidities still have relevant aggregate effects. They reduce aggregate income and employment income by 0.3% and 0.9%, respectively. In addition, they increase (reduce) the probability of unemployment (working) by 0.05 (0.08) percentage points.

Two comments on the interpretation of our estimates are in order. First, we compute the aggregate effects focusing on individuals with wage freezes in 2014. However, individuals that received a raise in 2014 may be subject to downward rigid wages, which were not binding

[3] We therefore follow a growing number of studies exploiting this 'Swiss franc shock' to measure the impact of an unexpected appreciation on the price-setting behavior of firms and exchange rate pass-through to prices (see Auer et al., 2019, 2021; Bonadio et al., 2020; Kaufmann and Renkin, 2019).

because they received an increase in their real wage. If this is the case, and the Swiss franc shock makes the friction binding in 2015 and 2016, we underestimate the aggregate effect by classifying them as non-treated individuals. Second, we analyze the impact on income and employment income, in addition to unemployment. We can therefore show to what extent unemployment benefits offset the allocative effects of downward nominal wage rigidities. This is relevant for policy makers because the central bank may ignore downward rigid wages, perhaps, if sufficiently strong automatic stabilizers are in place.[4]

To the best of our knowledge, this paper is the first to provide local and representative aggregate evidence on the causal effects of downward nominal wage rigidities in response to an exogenous deflationary shock. Olivei and Tenreyro (2007, 2010) analyze the role of staggered wage setting by measuring the output response to an exogenous monetary policy shock in a quarter-dependent vector-autoregression for the United States and other countries. Many firms in the United States take decisions on wage contracts in the fourth quarter. The authors find that a monetary policy shock after the negotiation has a larger and quicker effect on output than a shock occurring before the negotiation. This speaks to the relevance of infrequent wage adjustments in the form of staggered wage contracts.[5] As they do not distinguish between negative and positive shocks it remains unclear whether downward nominal wage rigidities are relevant, however. Such empirical evidence is provided by Fehr and Goette (2005), Bauer et al. (2007), de Ridder and Pfajfar (2017), and Kurmann and McEntarfer (2019). What sets our paper apart from Fehr and Goette (2005), Bauer et al. (2007) and de Ridder and Pfajfar (2017) is that we exploit employee-level rather than regional information to identify the impact of downward nominal wage rigidities on unemployment.[6] We can therefore control for a range of firm-level and worker-level characteristics. Kurmann and McEntarfer (2019) also exploit administrative worker-level data. Our paper differs from their study for two reasons. First,

[4]In addition, aggregate nominal income is related to the optimal target of a central bank in the presence of wage rigidity. In the presence of wage stickiness, the central bank should stabilize wage and price inflation, as well as a measure of real activity (Erceg et al., 2000; Giannoni and Woodford, 2004). Some researchers have therefore suggested that the central bank should stabilize nominal GDP (see, e.g., Beckworth and Hendrickson, 2020, and references therein).

[5]Other studies exploit various institutional restrictions on wage adjustments to measure the impact of sticky wages. Duarte (2008) emphasizes the role of legal restrictions for downward nominal wage rigidities in Portugal. 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. 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.

[6]Kaur (2019) also exploits regional variation measuring the response of Indian districts with varying degree of wage rigidity to exogenous rainfall shocks. Exploiting sectoral rather than regional variation, Pischke (2018) analyzes employment adjustment in different segments of the housing market in response to the burst of the U.S. housing bubble.

we study the impact of an outright deflationary shock while they focus on a period with relatively low, but positive, inflation. Second, we provide representative aggregate evidence for Switzerland rather than evidence for one U.S. state.

In what follows we present the data set. Then, we provide information on the economic environment, and explain the identification as well as the estimation strategies. Finally, we present the results before offering some concluding remarks.

2 Data

We use a biennial firm survey and social security register data.[7] Both data sets comprise employee-level information with an anonymous identifier based on the social security number. Because the social security data cover the entire Swiss working age population, we can match virtually all observations from the firm survey to the social security data.[8]

2.1 Swiss Earnings Structure Survey

The Swiss Earnings Structure Survey (SESS) is a biennial firm survey conducted by the Swiss Statistical Office (SFSO). We obtained three waves for 2012, 2014, and 2016. Each wave comprises about 1.6 million individuals, that is 40% of all Swiss employees.[9] Because the data is provided by firms rather than households, we regard the data to be of high quality and subject to little reporting error.[10]

The SFSO chooses firms according to a stratified sampling scheme. Once a firm is chosen to be in the sample, participation is mandatory.[11] Firms can chose between a paper-based and an online questionnaire, or submit the information directly via an electronic interface. About

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

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

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

[10]Earlier studies measuring wage rigidities often use information 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. 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). More recent studies avoid the measurement error problem is to obtain more accurate data from personnel files, firm surveys, register data, or firms' payroll data (see, e.g., Knoppik and Beissinger, 2003; Fehr and Goette, 2005; Le Bihan et al., 2012; Jardim et al., 2019; Elsby and Solon, 2019). Other researchers ask managers directly why they are hesitant to adjust or cut wages, following the seminal work by Bewley (1999). For example, 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).

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

half of the firms in the SESS respond with the paper questionnaire.[12] Medium (large) firms can choose to report every second (third) employee. If they do so, they are advised to randomize the selection. Nevertheless, about 75% of medium and large firms report all employees.

Firms are asked to provide employment income, as well as the activity rate or working hours for October. They report various income components: base income, 13th month pay, bonus payments, pay for Sunday/night work, and overtime payments.[13] Firms report either the contractually agreed or the actual number of working hours.[14] In addition, the survey comprises detailed information on contract, employee, and firm characteristics. The SFSO validates and completes some of these characteristics with register data.

To compute the contractual wage, which we define as the income at unchanged working hours, we exploit the fact that the survey comprises actual income as well as a standardized full-time-equivalent income. We compute a standardization factor by dividing the full-time-equivalent income by the actual income.[15] If this standardization factor changes compared to 2014, we standardize the incomes in 2012 and 2016 to the factor in 2014.[16]

We apply the same standardization procedure to all income components and aggregate them to four different contractual wage measures. The total wage includes all payments net of social security contributions. The irregular wage includes bonus payments, payments for Sunday/night work, as well as payments for overtime. The regular wage amounts to the total wage net of irregular payments. The base wage corresponds to the regular wage without 13th month payments.[17]

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

[12]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.

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

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

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

[16]We only do this if the change in the standardization factor is larger than 0.1% to avoid spurious changes in the activity rate.

[17]Note that the 13th month payment is often fixed in the contract. But, anecdotal evidence suggests that contracts also specify that the 13th month payment can be suspended, which may be a margin of adjustment in economic downturns.

firm during the entire period. This is only a proxy for job stayers. For example, an employee may change the job within the same firm. We address the latter by constructing a proxy for job stayers using information on their occupation category, management function, contract type (e.g. hourly or monthly payments). We then show results for a subset of individuals for which these characteristics did not change between 2012 and 2014 as a robustness test.

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

2.2 Old Age and Survivors Insurance

The social security data stem from the Old Age Survivors Insurance (OASI). Firms report these data for every employee when they pay social security contributions to the regional or sectoral OASI branches. The Central Compensation Office (CCO) collects the data from the branches and makes them available to researchers. Even if individuals are not employed, however, they are registered with an OASI branch. Social security contributions are due as of age 17 (if working) or age 20 (all Swiss residents) until retirement at age 65 (64 for women).[18] Therefore, we observe the entire working age population, including inactive individuals with zero income.[19] We obtained data from 2008 to 2016, with about 5 million individuals each year.[20]

We compute various outcome variables of interest. Total income includes income from employment, income from self-employment, unemployment benefits, as well as payments

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

[19]See also Figure A.1 in Online Appendix A.

[20]We regard the social security data to be of very high quality and impose few sampling decisions: We replace a very small share of negative incomes with 0 (0.03% of the sample).

from insurances (e.g. compensation for mandatory military service).[21] Employment income excludes income from self-employment, unemployment benefits, as well as other public insurance receipts. Unemployment income includes all unemployment insurance payments. Note that all income measures are broader than the income information from the SESS. For example, we observe incomes from all occupations. In addition, it includes income from insurance receipts after accidents, remuneration of limited partnerships, or daily disability insurance payments (see Information Center OASI/DI, 2020).

Because we know the source of income, the data allows us to measure the employment history of the individuals. We define an unemployment indicator, which equals unity if the individual received unemployment benefits in a given year.[22] In addition, we construct an indicator, which equals unity if an individual receives income from employment or self-employment in a given year. This indicator therefore measures whether an individual is working. Finally, we construct an indicator of whether an individual receives income from self-employment.

In addition, we use the social security data to correct for various sample selection issues. First, the SESS is a stratified survey; some groups are over- or underrepresented such that we need appropriate sampling weights to compute representative aggregate statistics. Second, our sampling decisions are unlikely to remove observations randomly.[23] Third, computing the biennial wage change selects a subset of individuals that are less likely to be unemployed. Because the social security data covers the universe of the working age population, we can construct sampling weights to correct for these biases and compute representative aggregate statistics. To preserve space, we explain the corresponding sampling issues and how we construct the weights in Online Appendix C.

3 Identification and estimation

To identify the causal effects of downward nominal wage rigidity, we use an unexpected deflationary monetary policy shock, as well as information on the base wage growth

[21] 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.

[22] 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.

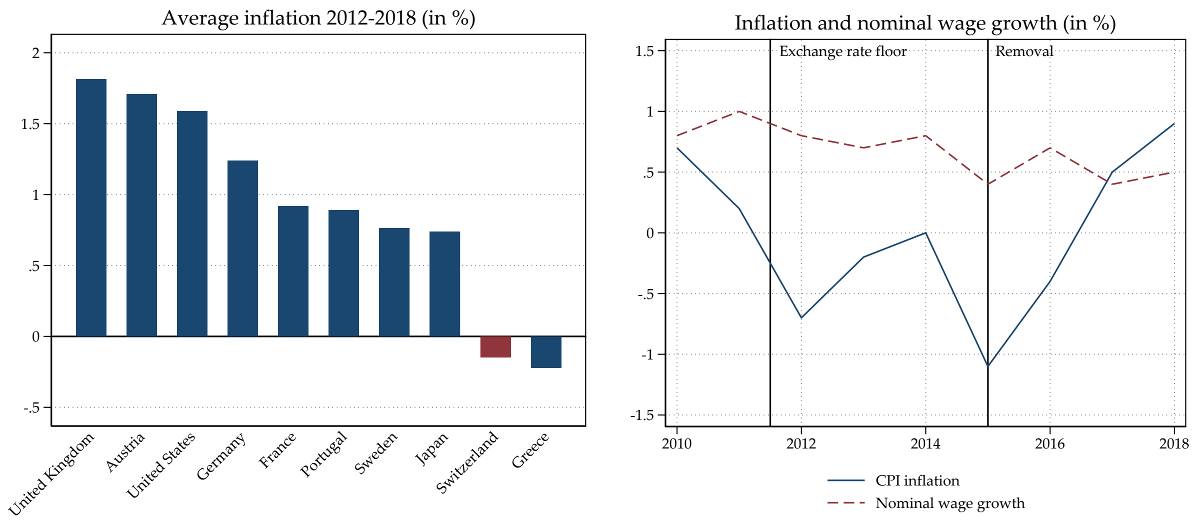
[23] For example, if smaller firms are more likely to use the paper survey, these data suffer from more serious reporting error, which we remove in the outlier detection scheme.

distribution. This section first describes the Swiss economic environment and the removal of the exchange rate floor policy. Then, we explain the identification scheme and the choice of the wage freeze indicator. Finally, we present the estimation strategy.

3.1 Economic environment and the Swiss franc shock

In the wake of the Global Financial Crisis, the SNB lowered its interest rate target close to zero. Conventional monetary policy was effectively constrained by a lower bound on interest rates (see SNB, 2009). Because of reserve absorbing operations, safe haven pressures related to the euro area debt crisis, and the effective lower bound on interest rates, the Swiss franc appreciated by about 30% until August 2011 (see Bäurle and Kaufmann, 2018; Canetg and Kaufmann, 2019). To stop the appreciation and deflationary pressure, the SNB established an exchange rate floor at CHF/EUR 1.20 in September 2011, promising to buy unlimited foreign currency, if necessary. As a consequence, the exchange rate remained close to CHF/EUR 1.20 over the following three years. On 15 January 2015, the SNB removed the floor because increasing pressure on the Swiss franc led to higher and higher exchange rate interventions (see also Bonadio et al., 2020). It is well established that the removal of the exchange rate floor was unexpected and that the international economic environment was relatively stable (see, e.g., Kaufmann and Renkin, 2019; Bonadio et al., 2020; Auer et al., 2021). To preserve space we provide supportive evidence of this view in Online Appendix D.

Figure 1 — Inflation and wage growth



Source: SFSO, OECD, own calculations, see Online Appendix A.

Because of the appreciation phases before and after the exchange rate floor period, Switzerland experienced mild deflation since 2012 (see left panel of Figure 1). Therefore, downward nominal wage rigidities were likely binding for a relevant share of employees. In addition, inflation was particularly low in international comparison for a prolonged period. This matters because it has been argued that downward nominal wage rigidities may vanish in a persistent low-inflation environment (see, e.g., Issing et al., 2003).[24]

The right panel of Figure 1 shows that the unexpected Swiss franc shock led to a fall in the price level. CPI inflation amounted to 0% in 2014. In the wake of the removal of the exchange rate floor in January 2015, inflation fell to -1% in 2015 and -0.2% in 2016. Meanwhile, aggregate nominal wages continued to increase.[25]

3.2 Identification scheme

As the macroeconomic environment was relatively stable in 2014, the Swiss franc shock in 2015 lends itself to the analysis of the interaction between unexpected deflation and downward rigid wages. We therefore compare income and employment of individuals with wage freezes in 2014 (treatment group) and those with small wage cuts in 2014 (control group), before and after the Swiss franc shock.

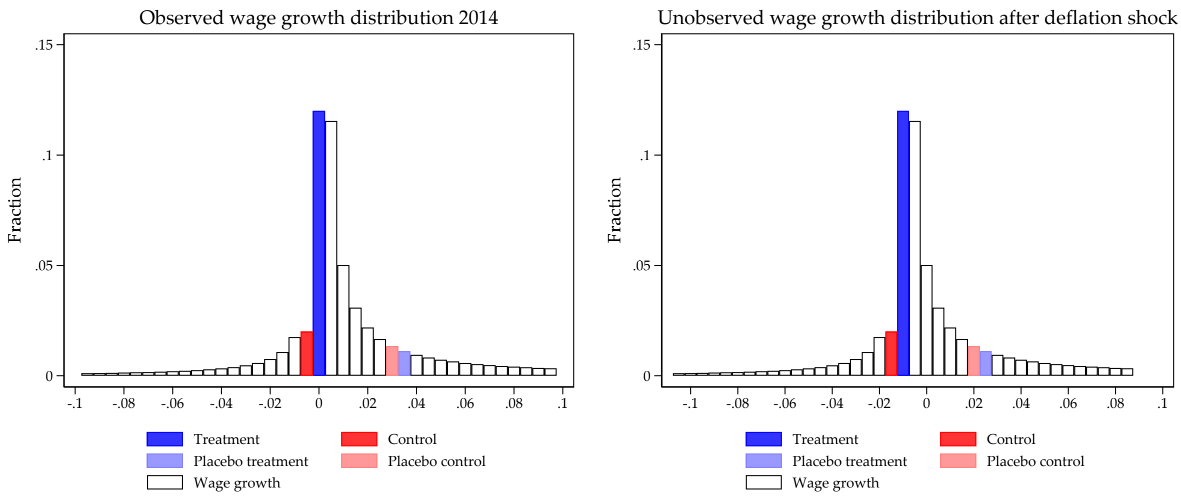
The left panel of Figure 2 shows a stylized depiction of our identification strategy. We use the wage growth distribution in 2014 to determine a treatment group and a control group. The key assumption is that individuals with small wage cuts are similar to individuals with wage freezes, except for the friction causing downward wage rigidity. After the Swiss franc shock, the unobserved distribution of desired wage changes shifts to the left. That is, firms would like to cut wages for individuals in both the treatment and control groups. Our hypothesis is that this is not possible for the treatment group and, therefore, firms may instead lay off employees.

In addition, we define placebo treatment and control groups using adjacent bins away from the origin of the wage growth distribution (light blue and red bars in Figure 2). For individuals with high trend productivity and therefore real wage growth, for example, the deflationary shock does not shift the desired wage change into negative territory. Therefore, we expect that

[24]Another reason why Switzerland is an interesting case to study is that Switzerland's labor market is relatively flexible. See Figure D.1 in Online Appendix D for a comparison of labor market indexes between countries from the OECD. Therefore, downward nominal wage rigidity is unlikely to be caused by legal provisions.

[25]Thus, the aggregate real wage was pro-cyclical during this period. This is in line with studies using 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).

Figure 2 — Stylized depiction of the identification scheme



Notes: The treatment group is defined as individuals with 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 real wage growth, the 1% deflation shock requires a smaller wage increase instead of a wage cut.

there is no difference between the placebo treatment group and control group that experience large positive wage changes in 2014. The same holds for adjacent bins in negative territory because these individuals are not subject to downward nominal wage rigidity.

3.3 Measuring downward nominal wage rigidity

To estimate the impact of the Swiss franc shock on individuals with downward rigid and flexible wages, we have to measure downward nominal wage rigidity. In what follows, we explain our preferred choice and how it differs from the existing literature.

Our indicator of downward nominal wage rigidity is a zero biennial base wage change between 2012 and 2014. There is ample evidence that total wages are more flexible than base wages because of bonus payments (Altonji and Devereux, 2000; Nickell and Quintini, 2003; Babecký et al., 2019; Kurmann and McEntarfer, 2019; Grigsby et al., 2021). As a consequence, using total wages would misclassify individuals with a downward rigid base wage, even if only a small share of the total wage stems from bonus payments.

In addition, most wages in Switzerland are negotiated in fall, to the best of our knowledge.

Because we use biennial wage changes to identify wage freezes, most firms have had the possibility to renegotiate wages. Therefore, the rigiditiy we identify is a more persistent phenomenon than the staggered wage setting analyzed by Olivei and Tenreyro (2007, 2010).[26]

Finally, we use the contractual wage standardized by hours worked from the firm survey rather than employment income from the social security data. The reason is that employment income may change because of hours worked, that is, the activity rate.

Table 1 — Wage rigidity statistics for 2014

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

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

Table 1 shows that this distinction matters. It shows biennial wage rigidity statistics for 2014 based on various data sources.[27] Because all measures may still be subject to some measurement error, we attribute wage growth rates smaller than 0.02% in absolute value to wage freezes.[28] In addition, following Dickens et al. (2007), we compute the share of wage cuts prevented as the share of wage freezes divided by the share of wage freezes and cuts.

The base wage is the most rigid wage component. 7.7% of all base wage changes are freezes and 26.4% are prevented base wage cuts.[29] The total wage, which includes bonus payments, 13th month pay and pay for Sunday/night work, is more flexible. Only 2.3% of total wage changes are freezes and 6.4% are prevented wage cuts. Although bonus payments lead to more downward wage flexibility, its share in the total wage is relatively small. In our data, the base income represents 91% of total income in 2014, while irregular income components including bonus payments constitute only 3% (see Online Appendix E). We therefore prefer

[26]As we estimate effects on annual outcomes over two years, and we only observe one shock in January, we cannot establish whether the response on employment is particularly quick or strong.

[27]We provide detailed wage setting statistics along many dimensions in Online Appendix E.

[28]Note that we therefore implicitly assume that all wage freezes would have been wage cuts in the absence of downward nominal wage rigidity. In addition, our measures of wage rigidity include workers that have in principle flexible wages but, by accident, receive a productivity shock such that the firm does not want to change their wage. However, the probability of this occurring is arguably negligible. We show in a robustness test that modifying this wage freeze tolerance level does not change our main results.

[29]These figures are similar, although slightly lower than the biennial wage rigidity statistics reported by Fallick et al. (2020) for the U.S.

the base wage to identify individuals subject to downward rigid wages. The last two rows show the importance of controlling for changes in the activity rate. Using income rather than the contractual wage reduces the share of wage freezes even further.

3.4 Estimation

Having defined a treatment and control group we estimate a difference-in-differences model:[30]

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

The dependent variables ($y_{i,t}$) stem from the OASI data set and are available at annual frequency (total income, employment income, unemployment income, unemployment dummy).[31] We saturate the model with time dummies for every year except 2014 ($\mathbf{1}\{t = j\}$), where $\mathbf{1}\{A\}$ denotes an indicator variable that equals 1 if the condition A is true and 0 otherwise. Then, we interact these dummies with a base wage freeze dummy ($\mathbf{1}\{\Delta w_{i,2014} = 0\}$), dummies for large wage cuts ($\mathbf{1}\{\Delta w_{i,2014} < -c\}$), dummies for wage increases ($\mathbf{1}\{\Delta w_{i,2014} > 0\}$), and two matrices of control variables capturing observed and unobserved differences that affect selection into treatment at the individual and firm-level ($\mathbf{X}_{i,2014},\ \mathbf{Z}_{f,2014}$). Finally, we control for individual fixed effects, which capture time constant unobserved characteristics (θ_i) and ε_{it} denotes an error term.

Ideally, the treatment and control groups differ only with respect to the nominal wage rigidity, but not with respect to other characteristics. However, we show in Online Appendix F that the average observed characteristics between treatment and control group are statistically significantly different. The significant differences do not come as a surprise, perhaps, given the large number of observations. In terms of economic relevance, the differences are relatively small. The main exceptions are that workers with wage freezes have a higher income than those with wage cuts, they are more likely to have a management function, they are 2.6 years older, and they are more likely to work in the public sector. Moreover, we suspect that the Swiss franc shock affected export-oriented firms more strongly than domestic firms. Therefore, to account for observed differences that affect selection into treatment, the baseline model interacts time

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

[31] All income variables are measured in natural logarithms, that is $\ln(1 + x)$.

dummies with dummies for firms, contract type, job type, education, gender, and whether the individual changed employer or was unemployed at some point between 2012 and 2014.[32]

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

4 Causal effects of downward nominal wage rigidity

We first discuss the causal impact of wage rigidities near the origin of the wage growth distribution (local effects). We then provide placebo tests before estimating representative aggregate effects. Finally, we discuss a range of robustness tests.

4.1 Local effects

Individuals with wage freezes are more affected by the Swiss franc shock than individuals with flexible wages. Figure 3 and Table 2 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.[33] For all outcomes, the estimates in 2015 and 2016 are statistically significant at conventional significance levels. Meanwhile, the estimates in 2013 are economically small and not statistically significant. This is consistent with the idea that the Swiss franc shock was not anticipated and the economic environment relatively stable.

We find that total income declines by 2.1% and 4.4% in 2015 and 2016, respectively. Employment income declines by 4.1% and 10.8%. Employment income falls more than total income because individuals becoming unemployed receive unemployment benefits. Indeed, by 2016 unemployment benefits for individuals with wage freezes increase by 7% relative to their peers with flexible wages, while the probability of becoming unemployed increases by 0.7

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

[33] A graphical representation is given in Online Appendix G.

percentage points.

Table 2 — Relative effect between individuals with base wage freezes and cuts

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

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

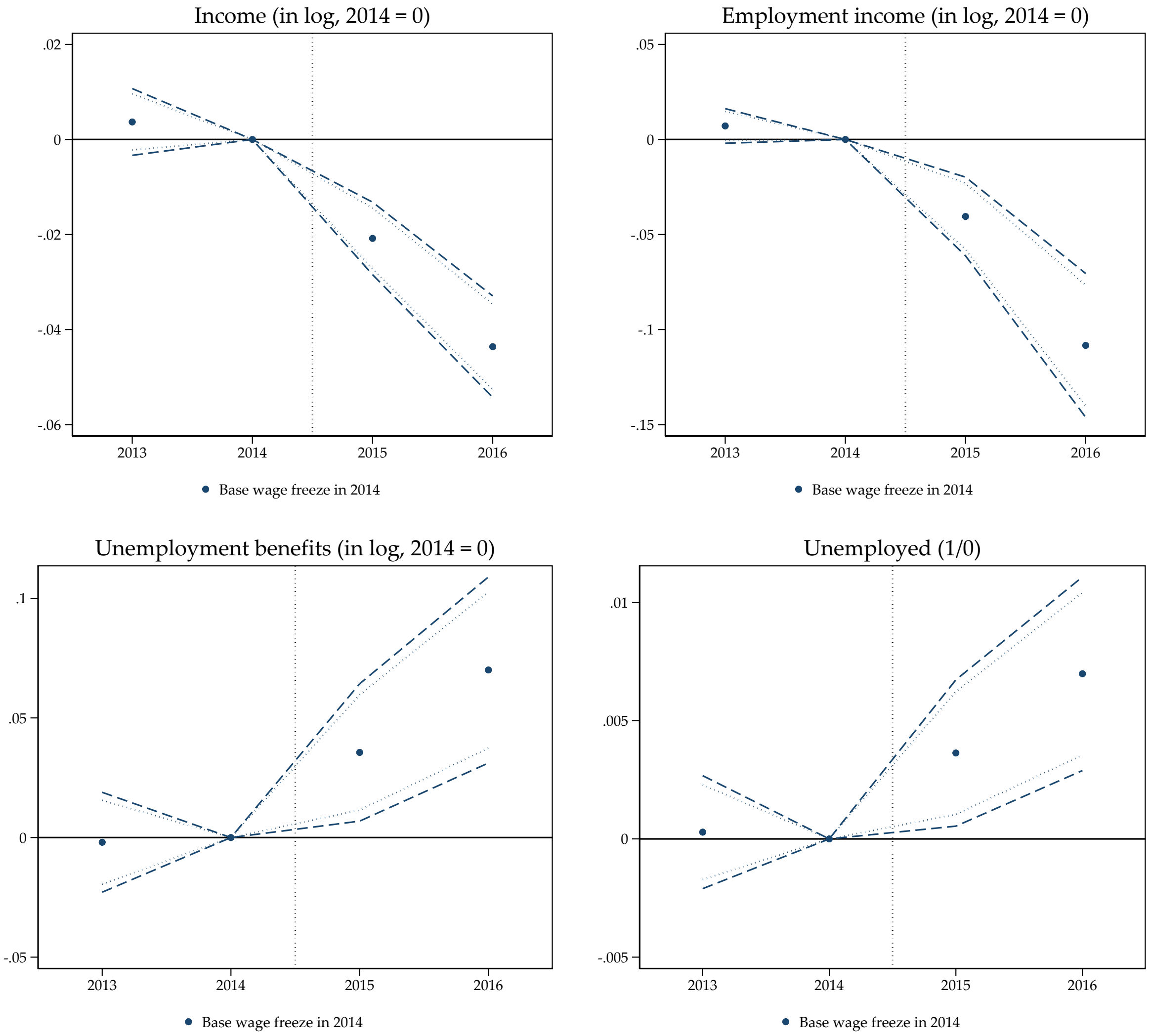
The results suggest that wage freezes cause allocative inefficiencies, as employment income falls and the probability of becoming unemployed increases. In addition, the fact that total income falls implies that unemployment benefits are not sufficient to compensate the distortion. This is not surprising, perhaps, as Swiss unemployment insurance initially covers 70% of the previous salary.[34]

One explanation for the decline in income is that firms reduced employees' wages in 2016. If this is the case, wages would be downward flexible when a negative shock hits and the downward nominal wage rigidity we observe in 2014 would be an artifact of the relatively stable economic environment. Another explanation is that workers are laid off but quickly find a new job at a lower wage at another employer. In this case, downward nominal wage rigidity does not matter much because Switzerland's flexible labor market allows for a quick reallocation of workers to other jobs. Finally, as Pissarides (2009) highlights, allocative inefficiencies are mostly caused by wage stickiness of new hires.

To examine these hypotheses, we use a subset of individuals we observe in all three waves of the SESS. For those individuals, we can determine whether they experienced a wage freeze in 2014 and then experienced a wage cut by 2016. In addition, we can determine whether they stayed at the same firm using information on tenure. Therefore, we can examine whether

[34]See www.ahv-iv.ch/en/Social-insurances/Other-types-of-social-insurances/Unemployment-insurance-ALV (accessed on 06/09/2021).

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



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

wages of new hires are more downward flexible.

Table 3 — Outcomes for employees with wage freezes in 2014

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

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

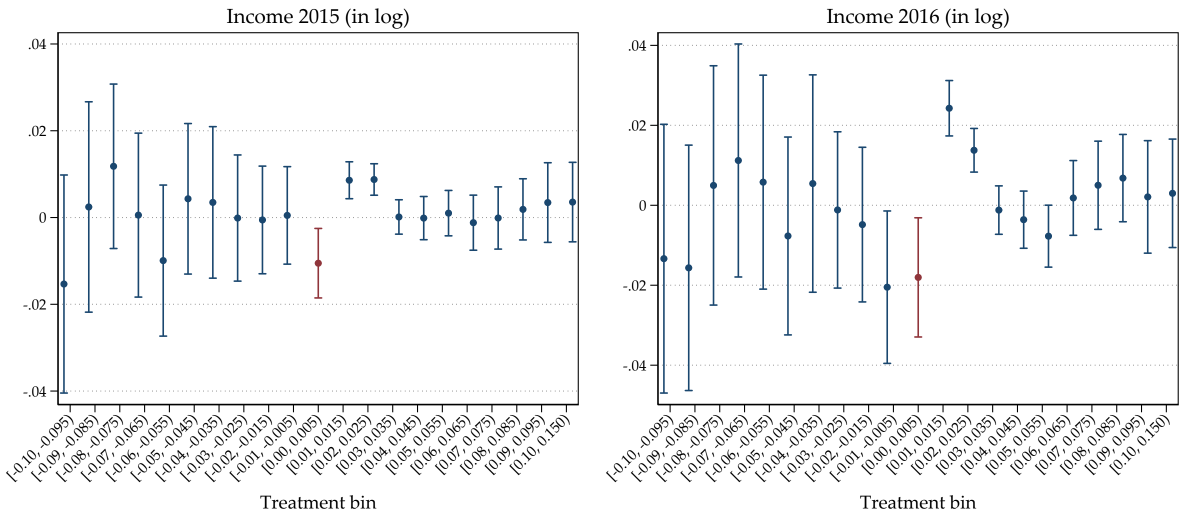
Individuals with wage freezes in 2014 are unlikely to receive a wage cut in 2016 regardless of whether they stay at the same firm or not. Table 3 shows the share of individuals with a wage freeze in 2014, that experience a wage freeze, increase, or cut in 2016 (in percent, with the number of observations in parentheses). Only 10% of individuals with a wage freeze in 2014 experience a wage cut in 2016 if they work at the same firm. Meanwhile, a roughly equal share of these individuals receive a freeze or increase in 2016. This implies that these wages are indeed downward rigid and upward flexible. The share of wage cuts in 2016 is higher for employees changing the firm (27%). If employees with wage freezes in 2014 change their employer, some of them are willing to accept a lower wage. But this share is still relatively low.

4.2 Placebo tests

To show that the local effects are only driven by differences close to the origin of the base wage distribution and that the Swiss franc shock was indeed unexpected, we conduct two types of placebo tests.

First, we examine placebo treatments over the wage growth distribution in 2014. We define treatment bins with a width of 0.5 percentage points at different points of the wage growth distribution. The control groups are bins with the same width just below the treatment bins (see Figure 2). If the main estimates pick up the effects of downward nominal wage rigidity we should observe significant differences in outcomes only for bins close to the origin of the wage growth distribution. The left panel of Figure 4 shows that the only significantly negative coefficient for 2015 is the one for the treatment bin $[0, 0.005)$. The coefficients are significantly positive for two treatment bins covering small positive changes. This does not come as a surprise because the control group includes observations closer to the origin that are more likely to be affected by base wage rigidities. For example, for the treatment bin $[0.01, 0.015)$ the control group is $[0.005, 0.01)$. In this case, the placebo treatment bin includes individuals with higher productivity growth; therefore the 1% deflationary shock is less likely to make downward nominal wage rigidities a binding constraint than for individuals in the placebo control group. For 2016, the results are similar. The only difference is that we also find a significantly negative effect for the bin covering $[-0.01, -0.005)$.

Figure 4 — 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.

Second, we examine pre-treatment trends. Between 2011 and 2014, the minimum exchange rate floor policy was in place. Therefore, we can test whether there were other factors before the Swiss franc shock that may distort our estimates. We discuss these results in more detail in Online Appendix G. The upshot is that the effects on income and unemployment are close to zero in 2013 once the adverse impact of the previous appreciation phase in 2011 dissipated.

4.3 Aggregate effects

The local effects are not representative for the entire Swiss economy because only 7.7% of observations in 2014 were base wage freezes. To show whether downward nominal wage rigidity has relevant aggregate effects, we use the difference-in-differences model to predict, for each individual in the treatment group, income and the probability of being unemployed. Then, we predict a counterfactual by setting the wage freeze dummy to zero. Finally, we aggregate the predictions (for treated individuals) and the actual data (for untreated individuals) with sampling weights for 2014. This strategy is likely to underestimate the true effects. This is because we ignore that individuals with wage increases in 2014 may be affected by downward nominal wage rigidities. Therefore, although the Swiss franc shock may cause these rigidities to bind, we classify them as untreated individuals.

Table 4 — Difference between aggregate predictions and counterfactuals

	Median income (% difference)	Median employment income (% difference)	Probability registered unemployed (pp difference)	Probability working (pp difference)
2013	0.00	0.03	0.00	-0.00
2014	0.00	0.00	0.00	0.00
2015	-0.14	-0.30	0.03	-0.04
2016	-0.32	-0.89	0.05	-0.08

Notes: The table shows the difference between predictions evaluated at the actual model coefficients and counterfactual predictions, based on treatment dummies set to 0. All predictions are computed at the individual level and then aggregated using own sampling weights. For income we compute the median. For the binary indicators, whether an individual is unemployed or working, we compute the mean. Therefore, the prediction gives the probability of being registered as unemployed or the probability of working.

As one would expect, the aggregate effects are smaller than the local effects because only 7.7% of all individuals experience wage freezes in 2014. Nevertheless, Table 4 shows that base wage rigidities cause a relevant decline in income. After the 1% decline in the price level, median employment income falls by 0.9% more compared to the counterfactual. The impact on median total income is smaller because of unemployment benefits (-0.3%). We also observe an increase in the probability of being unemployed by 0.05 pp compared to the counterfactual. Recall that our unemployment dummy only captures unemployed registered to receive unemployment benefits. It is possible, however, that people either exit the labor market, loose the eligibility for unemployment insurance, or become self-employed. We therefore compute a final aggregate prediction based on a dummy variable whether an individual is working (employment or self-employment). In line with the idea that some individuals exit the labor market or loose eligibility for unemployment insurance, the effect on the probability of working is slightly larger than the effect on the probability of being unemployed.[35]

[35] In a robustness test, we show that the probability of self-employment does not change.

4.4 Robustness tests

We perform a range of robustness tests varying the samples, as well as the measurement of wage freezes, outcomes, and controls.

4.4.1 Subsamples

Table 5 — Robustness tests: Subsamples

(a) Sectors (effect on income, in log)

	Export-intensive	Non-import-intensive	Export- and non-import-intensive	Private sector	Public sector
2013	0.003	0.000	0.011	0.001	0.008
	(0.004)	(0.005)	(0.007)	(0.003)	(0.005)
2015	-0.022***	-0.024***	-0.013*	-0.026***	-0.010
	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)
2016	-0.063***	-0.058***	-0.083***	-0.042***	-0.044***
	(0.008)	(0.009)	(0.012)	(0.007)	(0.009)
Controls	yes	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes	yes
Adj. R-sq. (between)	0.79	0.78	0.77	0.82	0.77
Adj. R-sq. (within)	0.00	0.00	0.00	0.00	0.00
Observations	2,565,366	2,294,718	2,162,581	2,359,542	988,630

(b) Individuals with same characteristics 2012-2014 (effect on income, in log)

	Same firm	Same contract	Same occupation	Same function	All
2013	0.007**	0.002	0.006	0.005	0.005
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
2015	-0.020***	-0.022***	-0.018***	-0.021***	-0.019***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
2016	-0.039***	-0.042***	-0.037***	-0.038***	-0.033***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Controls	yes	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes	yes
Adj. R-sq. (between)	0.82	0.82	0.82	0.81	0.82
Adj. R-sq. (within)	0.00	0.00	0.00	0.00	0.00
Observations	2,778,009	2,852,516	2,537,663	2,923,038	1,909,946

Notes: The estimates measure the effect on the treatment group (wage freezes in 2014) relative 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. Panel (a): The sample is split into export- and import-intensive firms, as well as firms in the public and private sector. The first categorization is based on input-output-tables for 2008 at the NOGA 2-digit level, where export-intensive (import-intensive) sectors are those with a share of exports (imports) in gross value added larger than the median. Panel (b): The sample is restricted to individuals that, over the period 2012-2014, stay at the same firm (based on tenure), have the same contract type (open-ended and temporary, each with a distinction between monthly and hourly pay), work in the same occupation (ISCO 2-digit-level, 50 categories, see Online Appendix E), or have the same management function (no, basic, lower, middle, upper management). The last column restricts the sample to individuals that have no change in any of these characteristics. ***/**/* denotes a statistically significant difference at the 1%/5%/10% level based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

Table 5, panel (a) shows estimates for various sectors. The deflationary shock was associated with a substantial appreciation of the Swiss franc. Therefore, export-oriented firms may have been more exposed to the shock. However, large export-oriented firms also import a larger fraction of intermediate inputs and therefore may have benefitted from the appreciation (see Kaufmann and Renkin, 2019). If wage rigidities are equally important across all sectors, they should therefore bind more strongly for export-oriented firms with a relatively low share of imports in value added. To test this hypothesis we estimate the impact on employees' total income, distinguishing between sectors according to their export- and import-intensity.[36] We define export/import-intensive sectors as those having a share of exports/imports in gross value added larger than the median.

Compared to our main estimates, the effect on income in 2016 is larger for export-intensive and non-import-intensive sectors (-6% instead of -4%). For sectors that are, at the same time, export- and non-import-intensive, the effect is largest (-8%).

In addition, we distinguish between the private and public sector. If wage rigidity is mostly a public sector phenomenon, we may expect the effect to be smaller in the private sector. However, the fourth and fifth columns of panel (a) show that the effects are only marginally larger in the public sector. The most important difference is that the effects are more delayed in the public sector.

Panel (b) restricts the sample to job stayers according to various proxies. This ensures that the treatment and control groups are more comparable. However, this comes at the cost of a smaller, and possibly less representative, sample. The effects are smaller in absolute size, but qualitatively quite similar compared to our preferred baseline specification.

4.4.2 Measurement of treatment

Accounting for measurement errors in wage data is key when analyzing wage rigidity (see, e.g., Gottschalk, 2005). Although the firm survey is of high quality, the categorical wage freeze dummy may be mismeasured because of reporting error in income or hours worked. Measurement errors in categorical indicators result in a misclassification bias (Aigner, 1973; Card, 1996). To control for measurement error in the wage freeze dummy, we therefore follow Kane et al. (1999) and Black et al. (2000), who exploit two independent proxies for classifying

[36]This classification is based on sectoral input-output-tables for 2008 at the NOGA 2-digit level, which are the last available data for Switzerland (Nathani et al., 2015). We transform the tables from the NOGA 2002 classification to the NOGA 2008 classification with a conversion key provided by the SFSO (see Online Appendix A). The number of observations is smaller than in the baseline because we were not able to match all sectors from the survey to the input-output-tables. Therefore, the results may be less representative.

Table 6 — Robustness tests: Measurement of treatment

(a) Accounting for measurement error

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

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

	$c = 0.001$	$c = 0.1$	Treatment including positive changes $< 1\%$	Treatment tolerance 0.01%	Treatment tolerance 0.05%
2013	0.005	-0.000	-0.001	-0.001	0.006
	(0.003)	(0.005)	(0.002)	(0.003)	(0.004)
2015	-0.021***	-0.018**	-0.010**	-0.021***	-0.023***
	(0.003)	(0.009)	(0.005)	(0.004)	(0.004)
2016	-0.036***	-0.054***	-0.023***	-0.042***	-0.040***
	(0.005)	(0.011)	(0.008)	(0.005)	(0.007)
Controls	yes	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes	yes
Adj. R-sq. (between)	0.81	0.81	0.81	0.81	0.81
Adj. R-sq. (within)	0.00	0.00	0.00	0.00	0.00
Observations	3,348,172	3,348,172	3,348,172	3,348,172	3,348,172

Notes: The estimates measure the effect on the treatment group (wage freezes in 2014) relative 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 estimates account for measurement error in the wage freeze indicator following the approach by Kane et al. (1999) and Black et al. (2000). Panel (b): The estimates are based on various definitions of the treatment and control groups. $-c$ denotes the lower threshold for defining the control group (small wage cuts). The third column includes wage increases smaller than 1% in the treatment group, as those may also be affected by the deflationary shock. The last two columns vary the tolerance level in which we attribute small wage growth rates to wage freezes (0.01% and 0.05% in absolute size). ***/**/* 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.

wage freezes and small wage cuts. Black et al. (2000) show that, if two binary indicators are measured with errors, we can mitigate the misclassification bias by estimating a model on a subsample where both classifications are identical. Intuitively, if two independent indicators provide the same classification it is less likely that the indicators are measured with error for the corresponding observation.

We compute two potentially error-ridden wage freeze dummies based on the biennial wage change from SESS and the annual employment income change from OASI data.[37] Based on these dummies we estimate the model on a subsample, where the SESS and OASI yield the same classification (wage freeze, wage increase, large wage cuts). Because employment income is more volatile than the base wage, we define a wage freeze as an absolute wage growth rate smaller than 0.05% (instead of 0.02%). In addition, we set the control group threshold $c = 0.1\%$.

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

In addition, panel (b) examines various other 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 of less than 1%. Finally, we vary the tolerance level to define wage freezes (0.01% and 0.05% in absolute size). The results sometimes show larger and sometimes smaller effects. In particular, when including small positive changes, the effect becomes 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 and that only part of these individuals are subject to downward rigid wages.

Table 7 — Robustness tests: Controls and outcomes

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

	Firm TE interacted with controls	IMR	Wage level TE	Sector TE	Wage level × sector TE
2013	0.005	0.004	0.003	-0.000	0.003
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
2015	-0.024***	-0.021***	-0.018***	-0.021***	-0.019***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
2016	-0.049***	-0.044***	-0.030***	-0.037***	-0.033***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)
Controls	yes	yes	yes	yes	yes
Firm-TE	no	no	no	no	no
Interaction-TE	yes	no	no	no	no
Wage level-TE	no	no	yes	no	yes
Sector-TE	no	no	no	yes	yes
IMR	no	yes	no	no	no
Adj. R-sq. (between)	0.81	0.81	0.81	0.81	0.81
Adj. R-sq. (within)	0.00	0.00	0.00	0.00	0.00
Observations	3,068,846	3,348,172	3,363,805	3,363,805	3,348,172

(b) Other outcomes

	Real income (in log)	Real employment income (in log)	Is working (1/0)	Is self-employed (1/0)
2013	0.004	0.007	-0.001	-0.000
	(0.004)	(0.005)	(0.001)	(0.001)
2015	-0.021***	-0.041***	-0.005***	0.001
	(0.004)	(0.011)	(0.002)	(0.001)
2016	-0.044***	-0.108***	-0.010***	0.001
	(0.005)	(0.019)	(0.003)	(0.001)
Controls	yes	yes	yes	yes
Firm-TE	yes	yes	yes	yes
Adj. R-sq. (between)	0.81	0.42	0.31	0.72
Adj. R-sq. (within)	0.00	0.00	0.00	-0.00
Observations	3,348,172	3,348,172	3,348,132	3,348,132

Notes: The estimates measure the effect on the treatment group (wage freezes in 2014) relative 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 reports results when including interactions of all the controls, that is we interact the time effects with a dummy for each group with the same characteristics of every variable in $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$. The second column includes the inverse Mills ratio interacted with time dummies as an additional control. The third column includes wage-level time-effects rather than firm-time effects. The fourth column replaces the firm-time effects with sector-time effects. The last column replaces the firm-time effects with sector-wage-level time effects. Panel (b): We change the dependent variables to real income, real employment income, an indicator whether an individual is working (any occupation including self-employment), and an indicator whether an individual is self-employed. ***/**/* 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.

4.4.3 Controls and outcomes

Table 7, panel (a) reports results using different controls. For brevity we only report the impact on total income. First, we include interactions among all the controls as time effects. That is, instead of including $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$ separately, we include a dummy for each group with the same characteristics of every variable in $\mathbf{X}_{i,2014}$ and $\mathbf{Z}_{f,2014}$. The results do not change.

Next, we include the inverse Mills ratio interacted with time effects to control for unobserved differences that affect selection into treatment.[38] In line with the idea that unobserved factors are economically negligible, the effects remain similar when adding the inverse Mills ratio to the model. The third and fourth columns replace the firm time-effects with wage-level and sector time-effects, respectively. The former interacts time dummies with dummies for 50 percentiles of the individuals' wage level. These controls capture that workers with relatively low income may be more likely affected by an implicit or explicit minimum wage. The latter interacts the time dummies with 18 sector dummies to identify the effects from variation between firms rather than individuals. The effects are somewhat lower but still qualitatively in line with our baseline estimates. This suggests that downward rigid wages may be partly but not only caused by minimum wage legislation. The last column shows similar results when the effects are identified from variation between individuals with similar wage levels in the same sector.

Panel (b) examines different outcomes. Estimating the effect on real (employment) income, that is individual income deflated by the CPI, does not change the results. We also estimate the impact on an indicator which is unity if an individual was either employed or self-employed. The probability of working falls by 0.5 and 1 percentage points in 2015 and 2016, respectively. There is no effect on the probability of becoming self-employed. This suggests that individuals loosing their job due to downward rigid wages do not exit unemployment by switching to self-employment.

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

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

5 Concluding remarks

This paper identifies allocative effects of downward nominal wage rigidities on income and employment after an unexpected 1% decline in the price level. Individuals with downward rigid wages experience a decline in income (employment income) of 4.4% (10.8%). Moreover, the probability of becoming unemployed increases by 0.7 percentage points. We also provide representative aggregate estimates. Downward nominal wage rigidities cause a fall in aggregate income (employment income) of 0.3% (0.9%). In addition, they are responsible for an increase (decrease) in the probability of unemployment (working) of 0.05 (0.08) percentage points. Therefore, even though only 7.7% of individuals are subject to a wage freeze, this friction translates into relevant aggregate effects.

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

At least since the Global Financial Crisis most central bankers acknowledge that the effective lower bound is a relevant constraint.[39] The importance of downward nominal wage rigidities for the optimal level of inflation has been more controversial (see e.g. Basu and House, 2016). Recently the ECB (2021) concluded after its strategy review that "by taking account of downward nominal wage rigidities, an inflation buffer reduces the risk of macroeconomic downturns being predominantly reflected in an excessive rise in unemployment."

Our findings indeed support the view that downward nominal wage rigidities persist and have allocative effects in a deflationary environment. We therefore conclude that central banks and researchers should take into account downward nominal wage rigidity when choosing the monetary policy strategy, in particular, the type and level of the nominal target.

[39] The Federal Reserve, for example, acknowledges that the effective lower bound will probably be more often binding (see Board of Governors of the Federal Reserve System, 2021).

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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 provides information on the data sources, discusses the treatment of outliers, the construction of sampling weights, the monetary policy shock used for identification and provides additional descriptive statistics, information on the treatment and control groups, as well as pre-treatment trends.

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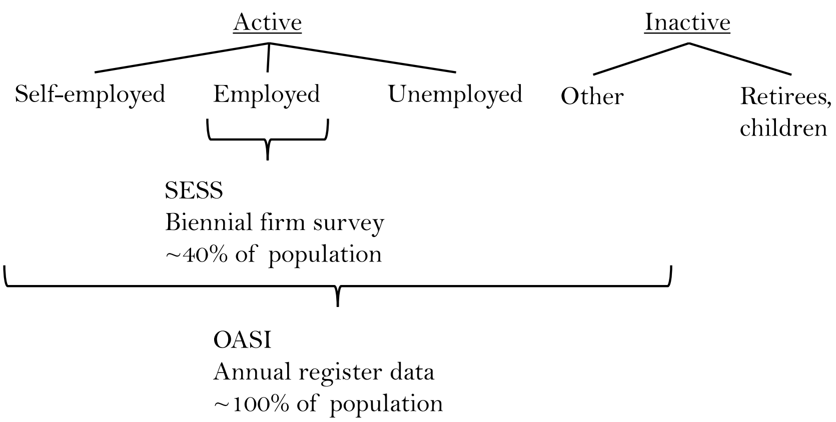
A Data sources

Table A.1 — 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.html
Inflation abroad	OECD	https://data.oecd.org/price/inflation-cpi.htm
Gross median income	SFSO	https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/wages-income-employment-labour-costs.assetdetail.8786111.html
Average social security charges 2014	Federal Social Insurance Office	Page 30 https://fak-basel.ch/wp-content/uploads/2015/10/Soz.vers_Statistik-2013.pdf
Average social security charges 2016	Federal Social Insurance Office	https://www.bsv.admin.ch/bsv/de/home/sozialversicherungen/ueberblick/grsv/statistik.html?cq_ck=1481195805050#-1422866446
Employment	SFSO	https://www.bfs.admin.ch/bfs/de/home/statistiken/industrie-dienstleistungen/unternehmen-beschaeftigte/beschaeftigungsstatistik/beschaeftigte.assetdetail.12967634.html
Input-Output-Table	SECO	https://www.seco.admin.ch/seco/de/home/Publikationen_Dienstleistungen/Publikationen_und_Formulare/Aussenwirtschafts/Globalisierung/die-volkswirtschaftliche-bedeutung-der-globalen-wertschoepfungsk.html and Nathani et al. (2015)
Conversion keys NOGA 2002 to 2008	SFSO	https://www.bfs.admin.ch/bfs/en/home/statistics/industry-services/nomenclatures/noga/publications-noga-2008.assetdetail.239927.html

Notes: All links accessed on 28/10/2021.

Figure A.1 — Structure of the data



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

B 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:

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

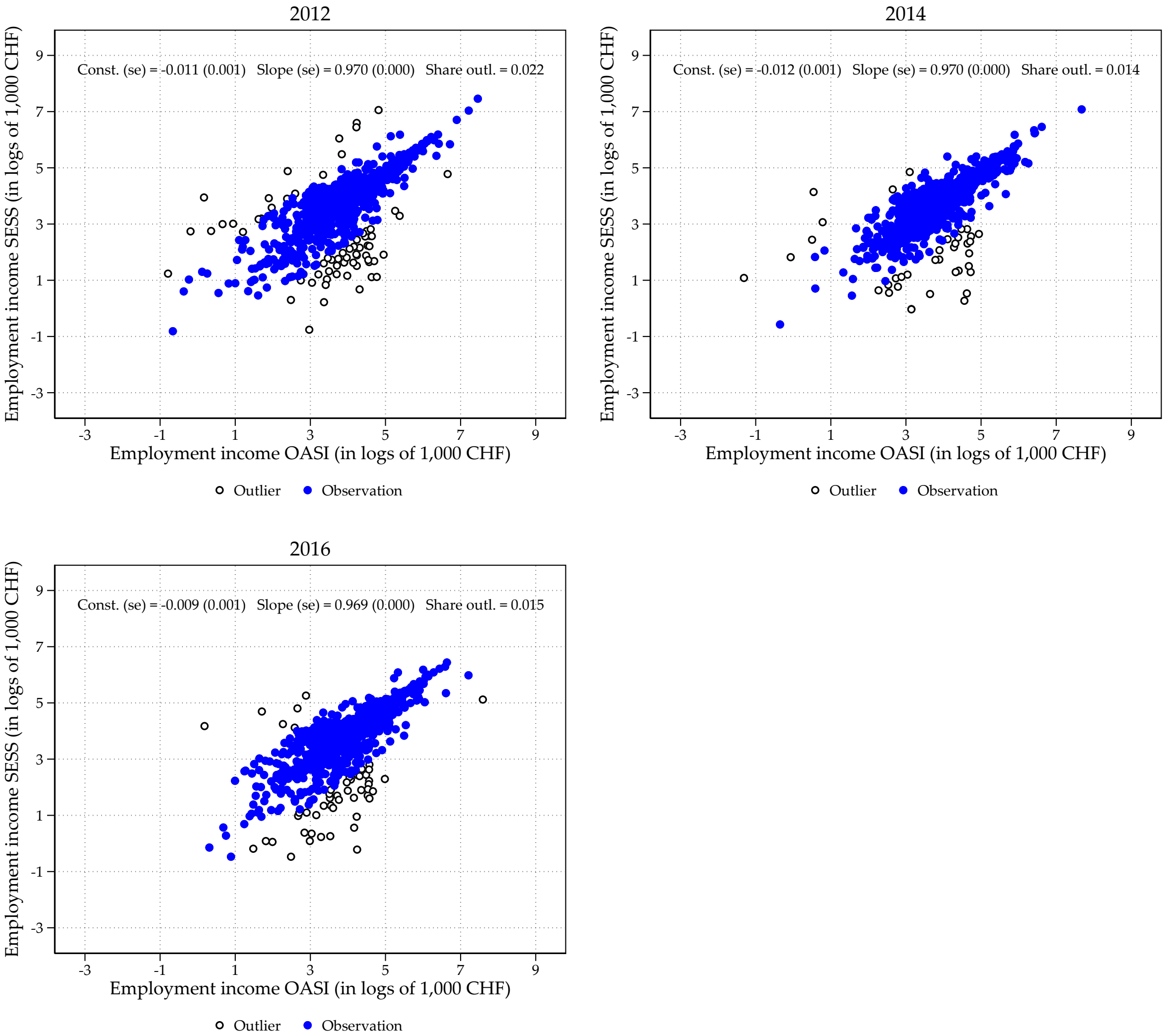
where $\hat{\alpha}_t, \hat{\beta}_t$ denote the parameter estimates.

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 B.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.

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

Figure B.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.

C Sampling weights

Analyzing wage rigidity with SESS data involves several sample selection problems. First, although the SFSO provides sampling weights, they will yield biased statistics if our sampling decisions remove observations in a non-random fashion. For example, if smaller firms are more likely to use the paper survey, these data suffer from more serious reporting errors, which we remove in the outlier detection scheme. Second, computing the wage growth distribution requires two consecutive wage observations. Therefore, wage growth statistics are based on a sample of employees that are more likely to stay in the labor market.

Indeed, our sampling decisions introduce an upward bias in median income, and a downward bias in employment. Table B.1 comprises aggregate statistics for median income and employment based on different data sets and weighting schemes. The official statistics on median net income amount to CHF 57,000. Our own calculations with the SESS data show a higher income at CHF 60,000 (Table C.1 panels a and b). This bias stems from the sampling decisions. If we additionally condition on observing a biennial wage change the upward bias becomes even more pronounced (panels c and d). The estimates are biased using the unweighted OASI data, as well as using the SESS data with official sampling weights. Conditioning on observing two consecutive wage observations exacerbates these biases. The median income is even higher, because we select individuals that are more likely to remain in the SESS over an extended period, and because low-income individuals probably face a higher risk of becoming unemployed.

To compute representative aggregate statistics we therefore construct new sampling weights. The first accounts for the sampling decisions and therefore yields weights for computing statistics for income and wage levels in 2014 and 2016. The second additionally accounts for conditioning on two consecutive wage observations and therefore yields weights for computing statistics for wage changes in 2014 and 2016. We use information from the OASI data, which cover the population of Swiss residents. For each year and each subsample, we estimate the probability of being observed with a Probit model:

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

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

Then, we use the inverse of the probability that an individual with characteristics $\mathbf{x}_i$ is

Table C.1 — Replication net and gross income SESS

(a) Conditional on being in SESS 2014

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

(b) Conditional on being in SESS 2016

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

(c) Conditional on observing biennial wage change 2014

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

(d) Conditional on observing biennial wage change 2016

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

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

included in the sample as sampling weight:

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

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

Table C.2 provides selected coefficient estimates, excluding the indicators for 400 percentiles of the employment income distribution for brevity. The coefficients have the expected sign. In particular, unemployed and self-employed individuals are less likely to be included in the SESS.

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

Table C.2 — Probit models weighting

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

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

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

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

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

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

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

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

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

Table C.3 — Data and weighting

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

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

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

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

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

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

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

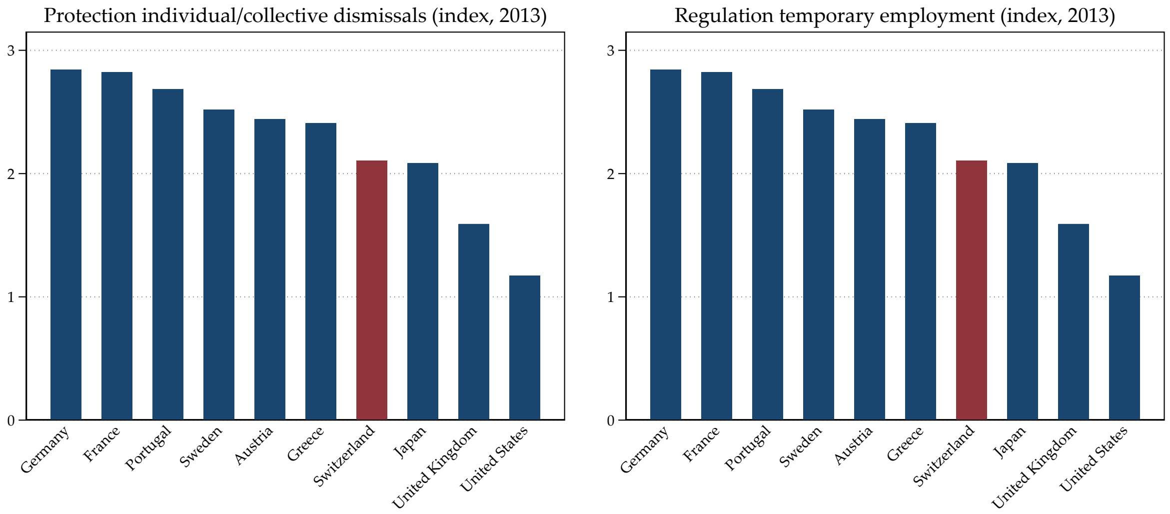
	Aggregate statistics		Sample estimates		
	Official statistics	OASI population	OASI unweighted	OASI own weights	SESS official weights
Median income (in 1,000 CHF)	57.21	56.40	81.94	56.64	68.18
Employment (in 1,000)	4,915.50	4,971.26	960.73	4,959.38	1,425.73
Observations (in 1,000)	.	4,971.26	959.10	935.21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

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

D Economic environment and Swiss franc shock

One reason why the Swiss economy is an interesting case in which to study downward nominal wage rigidity is its labor market flexibility. As a result, downward nominal wage rigidities are not mainly caused by legal provisions (see Duarte, 2008, for an example on Portugal). In particular, there is no federal minimum wage; minimum wages introduced by single cantons are relatively low (see Berger and Lanz, 2020); only 20% of the working age population is subject to a (sectoral or cantonal) minimum wage agreement; and finally, only 13% of the working age population is a member of a labor union.[2] This is supported by indices on labor market regulation by the OECD (see Figure D.1). Switzerland's labor market is less regulated than many European countries, although it is more regulated than the US and UK.

Figure D.1 — Labor market regulation



Source: OECD, see Table A.1.

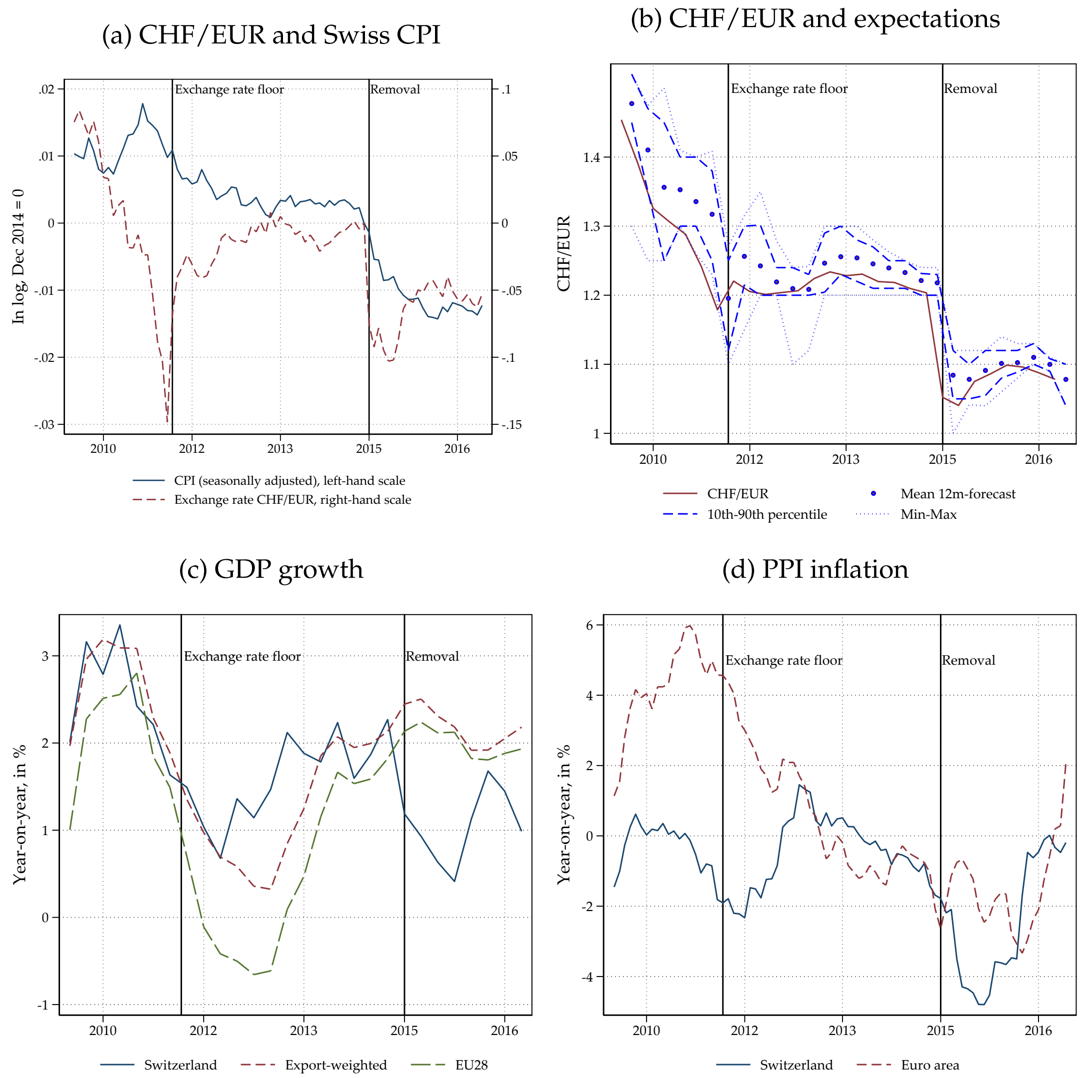
Another reason why Switzerland is an interesting case is that the Swiss franc shock in 2015 can be regarded as an unexpected exogenous economic shock that made downward nominal wage rigidities more binding. At that time, the international economic environment was relatively stable.

The removal of the exchange rate floor led to an unexpected appreciation of the Swiss franc and a decline in the price level. Panel (a) of Figure D.2 shows the Swiss CPI (left-hand scale)

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

along with the CHF/EUR exchange rate (right-hand scale).[3] The Swiss franc immediately appreciated by 10% and stabilized at a level 5% stronger than prior to the shock by the end of 2015. The CPI moved sideways before the removal of the floor. Afterwards, we observe a decline of 1%.

Figure D.2 — The Swiss franc shock



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

Although most observers believed that the floor was a temporary policy, the end date of the policy was unknown. Its removal on 15 January 2015 surprised most economists and

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

financial market participants. Panel (b) of Figure D.2 shows forecasts of a panel of informed economists participating in the KOF Consensus Forecast, a survey of professional forecasters. One month before the removal of the floor, all survey participants forecast an exchange rate above CHF/EUR 1.20 over the next 12 months. This is also consistent with the high credibility of the floor found in financial market data (see Bonadio et al., 2020).

The international macroeconomic environment was relatively stable before and after the removal of the floor (see panel c). In 2013 and 2014, GDP growth in Switzerland's main trading partners stood at about 2%. After the removal of the floor Swiss GDP growth fell by 1 and 1.5 percentage points in Q1 and Q2 2015, respectively. Meanwhile, GDP growth abroad remained relatively stable. Thus, the analysis is not confounded by a sudden change in foreign demand.

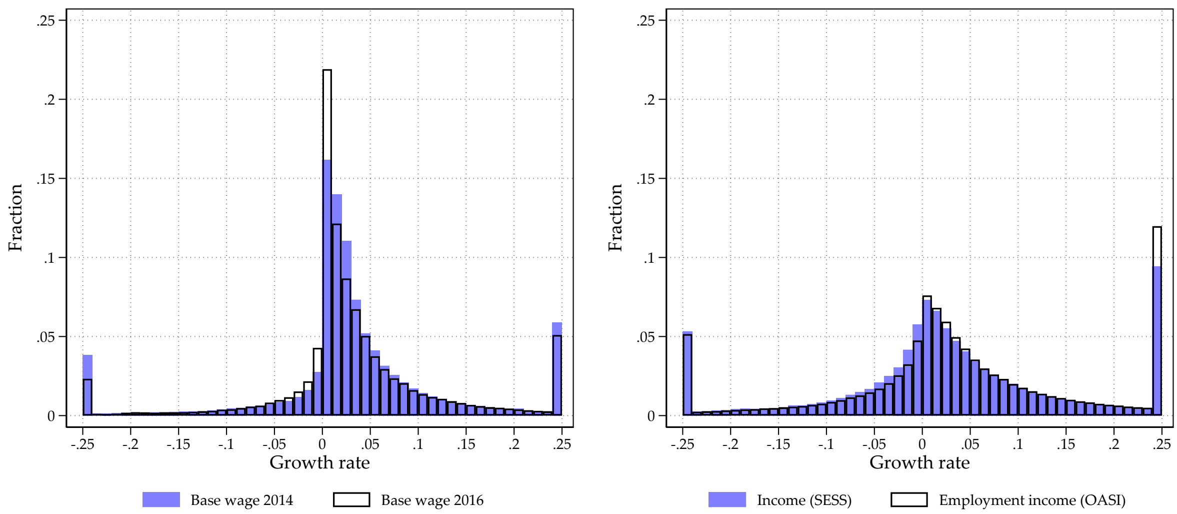
Although the exchange rate floor was introduced in 2011, it took time until relative prices in Switzerland and abroad converged. That is, the appreciation in 2010 and 2011 had relevant effects on prices and competitiveness of Swiss firms well into the exchange rate floor period. Panel (d) shows that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

E Descriptive statistics

Because our data has not been analyzed before, this section provides additional descriptive statistics. We first show histograms of the base wage and income growth distributions. Then, we provide information on the importance of irregular payments in employees' compensation. Finally, we report wage setting statistics along various dimensions.

We exploit the base wage growth distribution to identify the causal impact of downward nominal wage rigidity. The biennial base wage growth distribution in 2014 displays a pronounced asymmetry around the origin (see left panel of Figure E.1). Small wage cuts occur less frequently than wage freezes and small wage increases. In addition, the Swiss franc shock leads to a leftward shift of the wage change distribution in 2016 compared to 2014. After the deflationary shock, the share of wage freezes increases by more than the share of small wage cuts. This suggest that a nominal rigidity prevents wages from falling. Otherwise, we would observe more wage cuts.

Figure E.1 — Distribution of biennial base wage and income growth



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

The right panel shows that the asymmetry is not present if we compute biennial income growth rather than base wage growth. Indeed, income fluctuates more strongly because of irregular payments, such as bonuses, and changes in the activity rate. Therefore, we prefer the base wage to measure individuals affected by downward nominal wage rigidity.

Previous research has shown that bonus payments, hourly wages, or wages for job movers exhibit less wage rigidity (see e.g. Altonji and Devereux, 2000; Nickell and Quintini, 2003; Babecký et al., 2019; Grigsby et al., 2021; Kurmann and McEntarfer, 2019). In Switzerland, however, irregular payments account for a relatively small share of total compensation. Tables E.1 and E.2 show that 91% of employment income stems from base income. We obtain similar results when calculating the average share of base income in the total payroll of firms (see Table E.3). Irregular income, including bonus payments, accounts for 3% of employment income. This suggests that the base income is the most important component in employees' compensation and firms' labor cost. In addition, only 20% of employees are paid on an hourly basis. Finally, more than 80% of employees stay at the same company over two years, and more than 92% have a permanent contract. This suggests that factors typically associated with wage rigidities (the contractual base wage, staying on a job, permanent contract, monthly pay), play an important role in the Swiss labor market.

Table E.1 — Descriptive statistics matched data set 2014

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

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

Table E.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 E.3 — Share base wage in total payroll by firm size (number of employees)

(a) 2014

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

(b) 2016

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

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

We provide base wage setting statistics along various dimensions without commenting on them in detail because they may be useful to researchers calibrating macroeconomic models. The main point to note is that downward nominal wage rigidity is a pervasive phenomenon. Wages are more downward flexible for hourly pay, workers with shorter tenure, and workers changing the firm.

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

Notes: Wage rigidity statistics based on biennial wage changes according to different 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 based on own sampling weights.

Table E.5 — Base wage rigidity statistics for various characteristics 2016

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

Notes: Wage rigidity statistics based on biennial wage changes according to different 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 based on own sampling weights.

Table E.6 — Detailed base wage rigidity statistics 2014-2016

	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 E.6 – 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 E.6 – 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 E.6 – 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 E.6 – continued from previous page

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

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Table E.6 – continued from previous page

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

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Table E.6 – 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 E.6 – 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 E.6 – 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 E.6 – 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 E.6 – 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 E.6 – continued from previous page

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

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

F Selection into treatment and inverse Mills ratio

Table F.1 shows the difference in the mean of various characteristics between the treatment and control groups, along with the estimated standard error. We see that the two groups differ in a statistically significant way. Economically, however, most of the characteristics are similar.

Table F.1 — Difference in means between treatment and control group

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

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

We control for most of these characteristics in our model. But there may be unobserved characteristic affecting selection into treatment. We therefore include the inverse Mills ratio in our model in a robustness check. We aim to control for the fact that individuals with certain unobserved characteristics related to selection into treatment are differently affected by the deflationary shock.

In a first step, we estimate the inverse Mills ratio (see Heckman, 1979). Let us assume that the continuous selection process into treatment in 2014 (that is, the unobserved wage change absent wage rigidities ($\Delta w^*_{i,2014}$)), depends linearly on observed ($\mathbf{x}_{i,2014}$) and unobserved

($\nu_{i,2014}$) characteristics:[4]

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

Based on this assumption we can estimate the inverse Mills ratio from a Probit model, where we restrict the sample to the treatment and control group in 2014:[5]

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

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

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

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

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

Table F.2 shows the estimates of the Probit model. For brevity, we do not report coefficients for the age and wage level percentiles. The coefficients have the expected sign. For example, individuals that were unemployed sometime between 2012 and 2014 were less likely experiencing a wage freeze. Similarly, individuals that stayed at the same firm during this period were more likely to experience a wage freeze.

[4]We drop the constant for readability.

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

Table F.2 — Probit for inverse Mills ratio

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

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

Table F.2 — Probit for inverse Mills ratio (continued)

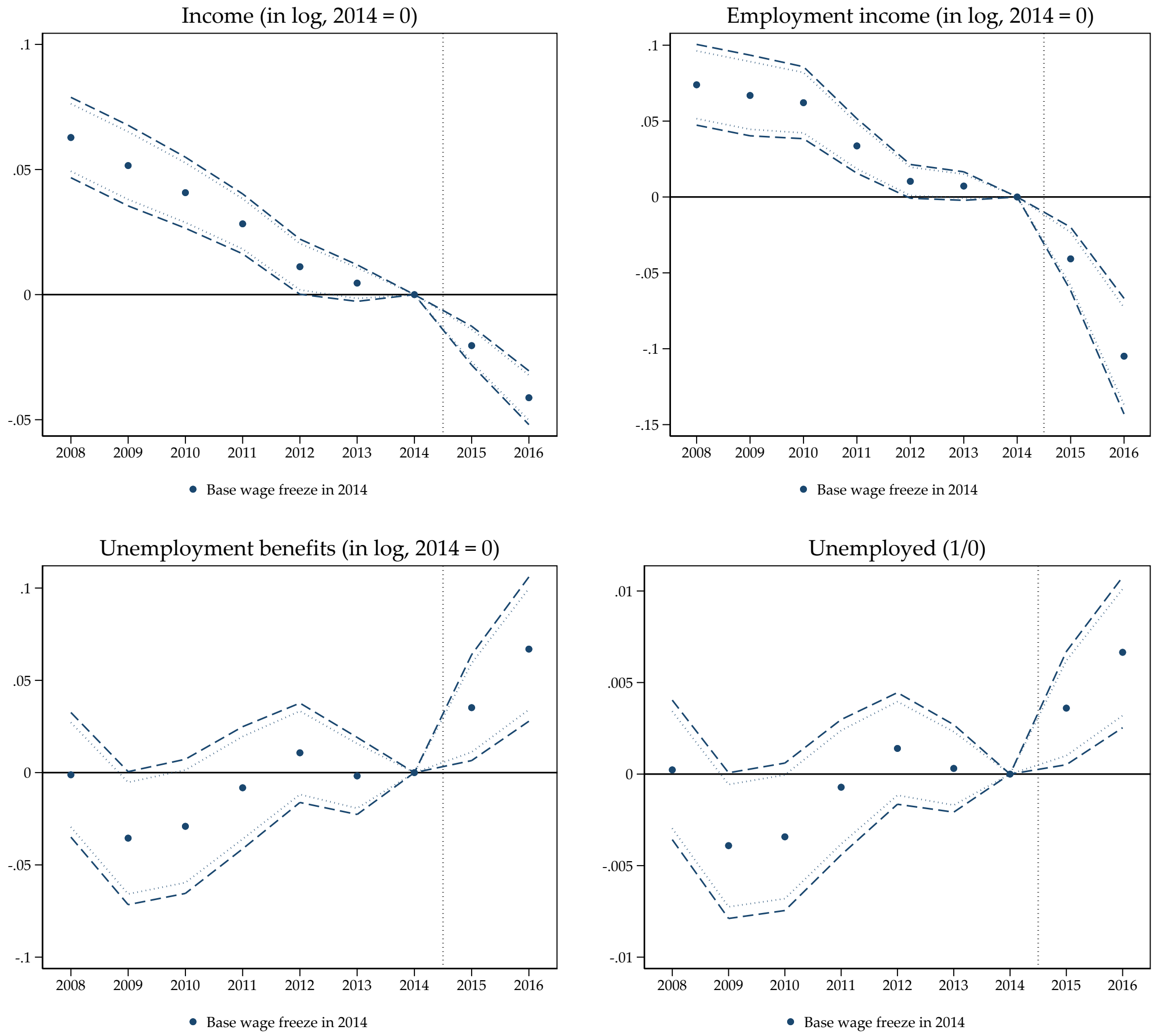
	1/0 (wage freezes/small cuts)
Manufacturing	1.307***
	(0.238)
Electricity, gas a. steam supply	1.419***
	(0.246)
Water supply	1.703***
	(0.256)
Construction	1.053***
	(0.241)
Trade; rep. of motor vehicles a. moto.	1.134***
	(0.239)
Transportation and storage	0.957***
	(0.239)
Accomod. and food serv.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

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

G Pre-treatment trends

We estimate pre-treatment trends to show that the effects can indeed be attributed to the Swiss franc shock. Between 2011 and 2014, the minimum exchange rate floor policy was in place and the international environment was relatively stable. Therefore, we can test whether there is a difference between the treated and non-treated group in absence of a deflationary shock. Figure G.1 shows that incomes of individuals with base wage freezes were quite stable from 2012-2014, but declined before that. This does not come as a surprise, however, because the Swiss franc appreciated significantly between 2008 and 2011 and it took time until relative prices in Switzerland and abroad converged (see Figure D.2). We believe that the pre-treatment trends mirror the delayed effects of the appreciation before the exchange rate floor was introduced. For unemployment and unemployment benefits, the pre-treatment trends are mostly not statistically significant. However, in line with the idea that the first appreciation phase has delayed effects, we observe an increase during 2009-2011.

Figure G.1 — 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.

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