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

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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.

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 play a critical role in the transmission of monetary policy to the real economy.[1] Some authors, however, still question whether *downward* nominal wage rigidity matters for real economic outcomes (see Issing *et al.* 2003; Basu and House 2016).[2] 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 bind rarely.[3] 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 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 estimating the impact of downward nominal wage rigidity on income and the employment history. Third, we analyse the impact in response to an exogenous deflationary shock. We exploit a unique natural experiment, namely a 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').[4] Importantly, consumer price index (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 who received a raise in 2014 may be subject to downward rigid wages, which were not binding 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, then we underestimate the aggregate effect by classifying them as non-treated individuals. Second, we analyse 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 policymakers because the central bank may ignore downward rigid wages, perhaps, if sufficiently strong automatic stabilizers are in place.[5]

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) analyse 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 USA and other countries. Many firms in the USA 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.[6] 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.[7] 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, 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 US state.

In what follows we present the dataset. 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.

I. Data

We use a biennial firm survey and social security register data.[8] Both datasets 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.[9]

Swiss Earnings Structure Survey

The Swiss Earnings Structure Survey (SESS) is a biennial firm survey conducted by the Swiss Federal 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.[10] Because the data are provided by firms rather than households, we regard the data to be of high quality and subject to little reporting error.[11]

The SFSO chooses firms according to a stratified sampling scheme. Once a firm is chosen to be in the sample, participation is mandatory.[12] Firms can chose between a paper-based and an online questionnaire, or submit the information directly via an electronic interface. About half of the firms in the SESS respond with the paper questionnaire.[13] Medium (large) firms can choose to report every second (third) employee. If they do so, then 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.[14] Firms report either the contractually agreed or the actual number of working hours.[15] 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.[16] If this standardization factor changes compared to 2014, then we standardize the incomes in 2012 and 2016 to the factor in 2014.[17]

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 and 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.[18]

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, then we assume that a person stayed at the same firm during the entire period. This is a proxy only for job stayers. For example, an employee may change job within the same firm. We address the latter by constructing alternative proxies for job stayers using information on their occupation category, management function and contract type (e.g. hourly or monthly payments). We then show results for a subset of individuals for whom 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, then we drop the observation with a temporary contract (0.7% of the sample). If both contracts are permanent, then 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 that we remove in each wave of the survey declines from 2.2% in 2012 to 1.5% in 2016.

Old-age and Survivors' Insurance

The social security data stem from the Old-age and 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).[19] Therefore we observe the entire working-age population, including inactive individuals with zero employment income.[20] We obtained data from 2008 to 2016, with about 5 million individuals each year.[21]

We compute various outcome variables of interest. Total income includes income from employment, income from self-employment and unemployment benefits, as well as payments from insurances (e.g. compensation for mandatory military service).[22] Employment income excludes income from self-employment and 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, unemployment income includes income from insurance receipts after accidents, remuneration of limited partnerships, and daily disability insurance payments (see OASI/DI Information Centre 2022).

Because we know the source of income, the data allow us to measure the employment history of the individuals. We define an unemployment indicator that equals unity if the individual received unemployment benefits in a given year.[23] In addition, we construct an indicator that 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 under-represented such that we need appropriate sampling weights to compute representative aggregate statistics. Second, our sampling decisions are unlikely to remove observations randomly.[24] Third, computing the biennial wage change selects a subset of individuals who are less likely to be unemployed. Because the social security data cover 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.

II. 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 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.

Economic environment and the Swiss franc shock

In the wake of the global financial crisis, the Swiss National Bank (SNB) lowered its interest rate target close to zero. Conventional monetary policy was constrained effectively 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 2022). 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, for example, 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.

Because of the appreciation phases before and after the exchange rate floor period, Switzerland experienced mild deflation since 2012 (see the left-hand 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, for example, Issing *et al.* 2003).[25]

The right-hand 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.[26]

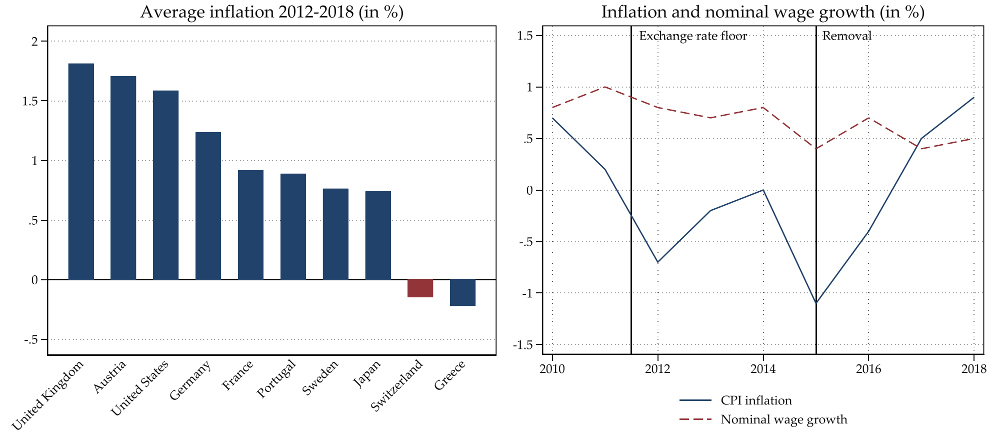

FIGURE 1. Inflation and wage growth. *Notes*: Sources: SFSO, OECD, own calculations; see Online Appendix A.

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-hand 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 (see right-hand panel). 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, 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 (see Figure 2 for an example). 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 there is no difference between the placebo treatment group and the 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.

Measuring downward nominal wage rigidity

To estimate the impact of the Swiss franc shock on individuals with downward rigid and flexible wages, we must 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,

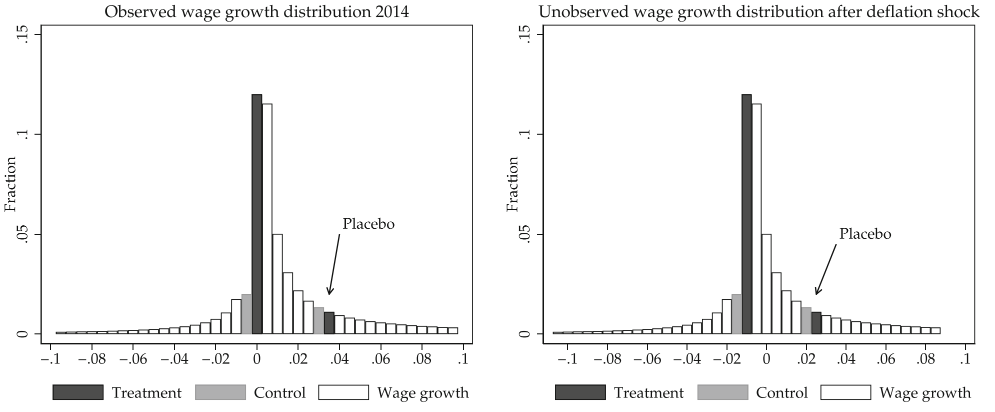

FIGURE 2. Stylized depiction of the identification scheme. *Notes*: The treatment group is defined as individuals with wage freezes in 2014 (left-hand panel). The control group are individuals with small wage cuts (smaller than 0.5% in absolute value). After the deflationary shock, firms would like to cut wages for individuals with wage freezes (right-hand panel). Because this may not be possible, we observe not 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.

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.[27]

In addition, most wages in Switzerland are negotiated in the autumn, 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 rigidity that we identify is a more persistent phenomenon than the staggered wage setting analysed by Olivei and Tenreyro (2007, 2010).[28]

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 shows that this distinction matters. It shows biennial wage rigidity statistics for 2014 based on various data sources.[29] 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.[30] 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.[31] 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, their 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 the base wage to identify individuals subject to downward rigid wages. The last two rows of Table 1 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.

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.

Estimation

Having defined a treatment and control group, we estimate a difference-in-differences model:[32]

$$y_{i,t} = \sum_{j \notin 2014} \mathbf{1}\{t = j\} \times \left[\alpha_j \; \mathbf{1}\{\Delta w_{i,2014} = 0\} + \delta_j \; \mathbf{1}\{\Delta w_{i,2014} < -c\} + \gamma_j \; \mathbf{1}\{\Delta w_{i,2014} > 0\}\right]$$

$$+ \sum_{j \notin 2014} \mathbf{1}\{t = j\} \times \left[\mathbf{X}_{i,2014} \, \boldsymbol{\beta}_j + \mathbf{Z}_{f,2014} \, \boldsymbol{\theta}_j\right] + \theta_i + \varepsilon_{i,t}. \tag{1}$$

The dependent variables ($y_{i,t}$) stem from the OASI dataset and are available at annual frequency (total income, employment income, unemployment income, unemployment dummy).[33] 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 that 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 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.[34]

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.

III. 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.

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. 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 was 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 percentage points.

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 replaces 70% of the salary.[35]

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

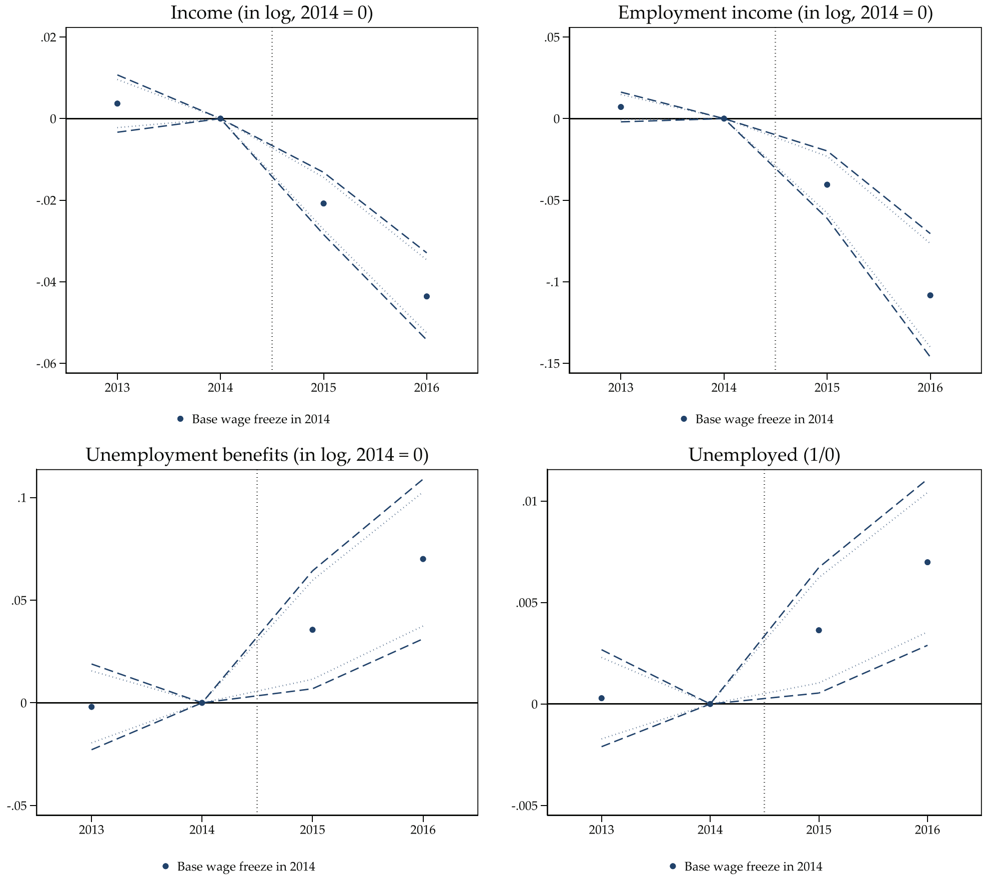

FIGURE 3. Relative effect between individuals with base wage freezes and cuts. *Notes*: The estimates measure the evolution of the treatment group (wage freezes in 2014) to the control group (small wage cuts in 2014) after a 1% decline of the price level. The estimates are normalized to 0 in the base 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.

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 who experience a wage freeze, increase or cut in 2016 (in per cent, 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 firm (27%). If employees with wage freezes in 2014 change their employer, then some of them are willing to accept a lower wage. But this share is still relatively low.

Placebo tests

To show that the local effects are driven only 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.

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 time effects	Yes	Yes	Yes	Yes
Adjusted R-squared (between)	0.81	0.42	0.33	0.33
Adjusted R-squared (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. Standard errors are given in parentheses.
***, **, * denote a statistically significant difference at the 1%, 5%, 10% level, respectively, based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

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 per cent (number of observations in parentheses). Statistics weighted using sampling weights for 2014.

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, then we should observe significant differences in outcomes only for bins close to the origin of the wage growth distribution. The left-hand 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

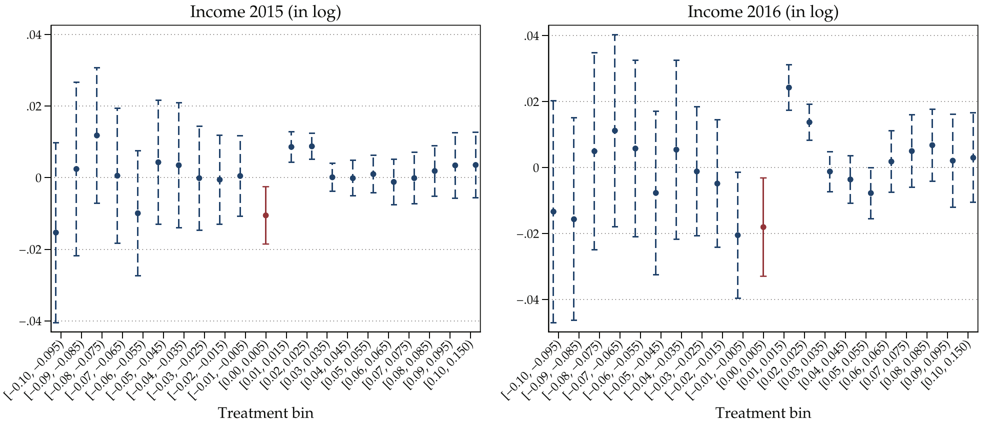

FIGURE 4. Placebo treatments. *Notes*: Placebo treatments in different bins of the base wage growth distribution in 2014, represented by dashed lines. We estimate the model defining the treatment group as a base wage change in $[c, c + 0.005)$. The control group is then defined as base wage changes in $[c - 0.005, c)$. The bin including wage freezes is represented by the solid line. 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.

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 $[-0.01, -0.005)$.

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.

Aggregate effects

The local effects are not representative of 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.

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 percentage points compared to the counterfactual. Recall that our unemployment dummy captures only unemployed registered to receive unemployment benefits. It is possible, however, that people

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.

exit the labour market, lose eligibility for unemployment insurance, or become self-employed. We therefore compute a final aggregate prediction based on a dummy variable for whether an individual is working (employment or self-employment). In line with the idea that some individuals exit the labour market or lose eligibility for unemployment insurance, the effect on the probability of working is slightly larger than the effect on the probability of being unemployed.[36]

Robustness tests

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

Subsamples Panel A of Table 5 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 benefited from the appreciation (see Kaufmann and Renkin 2019). If wage rigidities are equally important across all sectors, then 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 intensities.[37] 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 sectors. If wage rigidity is mostly a public sector phenomenon, then we may expect the effect to be smaller in the private sector. However, the fourth and fifth columns of panel A of Table 5 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 of Table 5 restricts the sample to job stayers according to various proxies. This ensures that the treatment and control groups are more comparable. However, this comes

TABLE 5
ROBUSTNESS TESTS: SUBSAMPLES

Panel 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 time effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared (between)	0.79	0.78	0.77	0.82	0.77
Adjusted R-squared (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
Panel B: Individuals with same characteristics 2012–14 (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 time effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared (between)	0.82	0.82	0.82	0.81	0.82
Adjusted R-squared (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. In 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. In panel B, the sample is restricted to individuals that, over the period 2012–14, 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 (none, basic, lower, middle, upper management). The last column restricts the sample to individuals who have no change in any of these characteristics. Standard errors are given in parentheses.
***, **, * denote a statistically significant difference at the 1%, 5%, 10% level, respectively, based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

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.

Measurement of treatment Accounting for measurement errors in wage data is key when analysing wage rigidity (see, for example, Gottschalk 2005). Although the firm survey is of high quality, the categorical wage freeze dummy may be mismeasured because of reporting error in income or hours worked. Measurement errors in categorical indicators result in a misclassification bias (Aigner 1973; Card 1996). To control for measurement error in the wage freeze dummy, we therefore follow Kane *et al.* (1999) and Black *et al.* (2000), who exploit two independent proxies for classifying wage freezes and small wage cuts. Black *et al.* (2000) show that if two binary indicators are measured with errors, then 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, then it is less likely that the indicators are measured with error for the corresponding observation.

TABLE 6
ROBUSTNESS TESTS: MEASUREMENT OF TREATMENT

Panel 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 time effects	Yes	Yes	Yes	Yes
Adjusted R-squared (between)	0.828	0.443	0.314	0.315
Adjusted R-squared (within)	−0.000	−0.000	0.000	−0.000
Observations	2,005,166	2,005,166	2,005,166	2,005,166

Panel 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 time effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared (between)	0.81	0.81	0.81	0.81	0.81
Adjusted R-squared (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. In 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). In 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). Standard errors are given in parentheses.
***, **, * denote a statistically significant difference at the 1%, 5%, 10% level, respectively, based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

We compute two potentially error-ridden wage freeze dummies based on the biennial wage change from the SESS and the annual employment income change from OASI data.[38] Based on these dummies, we estimate the model on a subsample, where the SESS and OASI data 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 at $c = 0.1\%$.

The results are based on a smaller sample and therefore less precisely estimated. Qualitatively, the effects are similar, however. Panel A of Table 6 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 of Table 6 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 show sometimes 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 some of these individuals are subject to downward rigid wages.

Controls and outcomes Panel A of Table 7 reports results using different controls. For brevity, we report only 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.[39] 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 of panel A of Table 7 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 incomes may be more likely to be 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 of Table 7 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 that 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 losing their job due to downward rigid wages do not exit unemployment by switching to self-employment.

TABLE 7
ROBUSTNESS TESTS: CONTROLS AND OUTCOMES

Panel A: Other controls (effect on income, in log)	Firm time effects interacted with controls	IMR	Wage level time effects	Sector time effects	Wage level × sector time effects
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 time effects	No	No	No	No	No
Interaction time effects	Yes	No	No	No	No
Wage-level time effects	No	No	Yes	No	Yes
Sector time effects	No	No	No	Yes	Yes
IMR	No	Yes	No	No	No
Adjusted R-squared (between)	0.81	0.81	0.81	0.81	0.81
Adjusted R-squared (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
Panel 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 time effects	Yes	Yes	Yes	Yes	
Adjusted R-squared (between)	0.81	0.42	0.31	0.72	
Adjusted R-squared (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. In 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. In panel B, we change the dependent variables to real income, real employment income, an indicator of whether an individual is working (any occupation including self-employment), and an indicator of whether an individual is self-employed. Standard errors are given in parentheses.
***, **, * denote a statistically significant difference at the 1%, 5%, 10% level, respectively, based on standard errors clustered according to unique values in the base wage growth distribution in 2014.

IV. 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, for example, 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, for example, Andrade *et al.* 2019), and reduces distortions caused by downward nominal wage rigidities (see, for example, 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.[40] The importance of downward nominal wage rigidities for the optimal level of inflation has been more controversial (see, for example, Basu and House 2016). Recently, the European Central Bank (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.

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NOTES

1. Also, in the canonical dynamic stochastic general equilibrium model by Christiano *et al.* (2005), monetary policy has relevant real effects if wages are rigid, irrespective of the degree of price rigidity. Without wage rigidity, monetary policy has limited effects even if prices are rigid.
2. There are various arguments against the relevance of downward nominal wage rigidity. It 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 behaviour of firms is state-dependent, then 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 that firms may use to cut nominal wages during recessions.
3. Indeed, one reason why the central bank may aim for a positive inflation target is to facilitate real wage cuts and therefore mitigate the adverse effects of recessions on the labour market (see Tobin 1972; Akerlof *et al.* 1996; Bernanke 2003; Schmitt-Grohé and Uribe 2013; Billi and Kahn 2008; Kim and Ruge-Murcia 2009).
4. 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 behaviour of firms and exchange rate pass-through to prices (see Auer *et al.* 2019, 2021; Bonadio *et al.* 2020; Kaufmann and Renkin 2019).
5. 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, for example, Beckworth and Hendrickson 2020, and references therein).
6. 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.
7. Kaur (2019) also exploits regional variation measuring the response of Indian districts with varying degrees of wage rigidity to exogenous rainfall shocks. Exploiting sectoral rather than regional variation, Pischke (2018) analyses employment adjustment in different segments of the housing market in response to the burst of the US housing bubble.
8. Online Appendix A provides information on the data sources. The data resemble the ideal described in Fehr and Goette (2005, P. 783): ‘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.’
9. There are very few observations that we cannot match. We suspect that this is due to reporting error.
10. More precisely, each wave included employees at firms with at least three employees in the secondary and tertiary sectors (SFSO 2018).
11. 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 to obtain more accurate data from personnel files, firm surveys, register data or firms’ payroll data (see, for example, 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 Wage Dynamic Network of the European Central Bank has assembled large cross-country surveys to analyse wage, price and employment adjustments to shocks (Bertola *et al.* 2012).
12. The response rate is 82% in 2012, and decreases to 73% in 2016 (SFSO 2016, 2018).
13. In email 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 transmitted the information directly via electronic personnel files.
14. In Switzerland, some work contracts specify that the salary is payed in 13 installments. Therefore workers receive an additional 13th monthly payment in December.
15. Firms can decide whether they report the working hours specified in the contract or the working hours that 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.
16. A change in the standardization factor may stem from changing agreed working hours (activity level) or changing actual working hours.

17. We do this only if the change in the standardization factor is larger than 0.1%, to avoid spurious changes in the activity rate.
18. Note that the 13th month payment is often fixed in the contract. But anecdotal evidence suggests that some contracts specify that the 13th month payment can be suspended, which may be a margin of adjustment in economic downturns.
19. In a few cases, we observe individuals who still work during retirement.
20. See also Figure A.1 in Online Appendix A.
21. 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).
22. 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 who earned the income.
23. Therefore we measure only individuals who 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.
24. For example, if smaller firms are more likely to use the paper survey, then these data suffer from more serious reporting error, which we remove in the outlier detection scheme.
25. Another reason why Switzerland is an interesting case to study is that Switzerland's labour market is relatively flexible. See Figure D.1 in Online Appendix D for a comparison of labour market indexes between countries from the OECD. Therefore downward nominal wage rigidity is unlikely to be caused by legal provisions.
26. Thus the aggregate real wage was procyclical 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, for example, Eichengreen and Sachs 1985). Whether real wages are countercyclical, however, depends on the time period (Basu and Taylor 1999), as well as on the nature of the macroeconomic shock (Sumner and Silver 1989).
27. In addition, bonus payments may serve as an additional margin of adjustment to circumvent downward rigid base wages (see, for example, Babecký *et al.* 2019). We show in a separate paper that downward flexible wage components, such as bonuses, do not fully offset the allocative effects of downward rigid base wages (see Funk and Kaufmann 2022).
28. As we estimate effects on annual outcomes over two years, and we observe only one shock in January, we cannot establish whether the response on employment is particularly quick or strong.
29. We provide detailed wage setting statistics along many dimensions in Online Appendix E.
30. 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.
31. These figures are similar to, although slightly lower than, the biennial wage rigidity statistics reported by Fallick *et al.* (2020) for the USA.
32. See Bonadio *et al.* (2020) and Kaufmann and Renkin (2019) for similar approaches.
33. All income variables are measured in natural logarithms, that is, $\ln(1+x)$.
34. 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 to be 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).
35. See www.ahv-iv.ch/en/Social-insurances/Other-types-of-social-insurances/Unemployment-insurance-ALV (accessed 30 January 2022).
36. In a robustness test, we show that the probability of self-employment does not change.
37. 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.
38. 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.
39. 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.
40. 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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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

A Data sources
B Treatment of outliers
C Sampling weights
D Economic environment and Swiss franc shock
E Descriptive statistics
F Selection into treatment and inverse Mills ratio
G Pre-treatment trends