

Kopp, Daniel; Siegenthaler, Michael

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## Short-time work and unemployment in and after the Great Recession

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Working Paper

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**Author(s):**

Kopp, Daniel; Siegenthaler, Michael

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## Short-Time Work and Unemployment in and after the Great Recession

Daniel Kopp and Michael Siegenthaler

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# KOF

ETH Zurich  
KOF Swiss Economic Institute  
LEE G 116  
Leonhardstrasse 21  
8092 Zurich, Switzerland

Phone +41 44 632 42 39  
Fax +41 44 632 12 18  
[www.kof.ethz.ch](http://www.kof.ethz.ch)  
[kof@kof.ethz.ch](mailto:kof@kof.ethz.ch)

# Short-Time Work and Unemployment in and after the Great Recession

Daniel Kopp and Michael Siegenthaler\*

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## Abstract

Can governmental policies mitigate the effects of recessions on unemployment? We study whether the Swiss short-time work (STW) program reduced unemployment in the 2009–2015 period using quarterly establishment-level panel data linking several administrative data sources. We compare changes in permanent layoffs into unemployment, hiring from unemployment, establishment survival and size between establishments that applied successfully to establishments that applied unsuccessfully for STW at cantonal employment agencies. The latter appear to be a valid control group for the former among others because of substantial idiosyncrasies in cantonal approval practices. We find strong evidence that STW increases establishment survival and prevents rather than postpones dismissals: the 7,880 establishments treated in 2009 would have dismissed approximately 20,500 workers into unemployment (0.46% of the labor force) until 2012. Most workers would have been dismissed in the quarters immediately following application and more than a third would have become long-term unemployed. We estimate that the savings in terms of unemployment benefit payments may have been large enough to compensate the spending on STW benefits for the Swiss unemployment insurance.

*JEL Classification:* E24, J23, J63, J65

*Keywords:* Great Recession, Labor demand, Layoffs, Labor hoarding, Short-time work, Unemployment, Work sharing

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\*ETH Zurich, KOF Swiss Economic Institute, Leonhardstrasse 21, CH-8092 Zurich, Switzerland. E-mails: kopp@kof.ethz.ch and siegenthaler@kof.ethz.ch. We thank Katharine G. Abraham, Kathrin Degen, Rafael Lalive, Daniel Lampart, Thomas Mathä, Christian Merkl, the members of the SECO advisory group for this project (especially Hans-Peter Egger, Jonathan Gast, and Bernhard Weber), and seminar participants at ETH Zurich, the University of Lausanne, SOLE, EALE, the Swiss Society of Economics and Statistics, the Young Swiss Economists Meeting, the IAB Workshop “Perspectives on (Un-)Employment”, and the IZA Summer School in Ammersee, in particular Jordy Meekes, for their valuable feedbacks and support. The authors acknowledge financial support from the Supervisory Committee of the Equilibration Fund of the Swiss Unemployment Insurance. There are no conflicts of interests arising from this funding.

# 1 Introduction

Major economic downturns lead to large increases in permanent layoffs. These job displacements pose a burden on government budgets. They can also leave scars: they may lead to large and persistent earnings and welfare losses for the affected workers that prevail even when the economy recovers (Davis and von Wachter, 2011; Yagan, 2017; Lachowska et al., 2018, among others). Governments thus aim at mitigating the effects of recessions on unemployment. One instrument that gained widespread popularity during the Great Recession 2007–2009 is short-time work (STW) programs. Countries such as Germany, Italy, and Japan spent large amounts on STW benefits during the recession (Boeri and Bruecker, 2011). These programs, geared to firms that face a temporary drop in demand, provide income support to workers whose working hours are reduced. However, these schemes could have deadweight and displacement effects (Hijzen and Venn, 2011). The former occur when STW subsidies are paid for jobs or working hours that employers would have retained even in the absence of the subsidy. The latter occur when the schemes preserve jobs that are not viable without the subsidy even after business conditions recover. In this case, STW programs only postpone rather than prevent dismissals. Given these concerns, it is highly policy-relevant to understand whether STW schemes prevent unemployment, and whether the savings in terms of unemployment benefit payments compensate their large costs.<sup>1</sup> Yet, we have little firm-level evidence on the causal effects of STW on permanent layoffs and unemployment (Cahuc, 2018).

This study investigates whether the Swiss STW scheme helped to prevent unemployment during and in the aftermath of the Great Recession. In contrast to previous studies that compare firms that use STW with firms that do not—a comparison that is susceptible to biases because of firms’ strong self-selection into STW<sup>2</sup>—, we focus solely on firms that aimed at introducing STW. In Switzerland, STW has to be approved by the employment agencies of each of the 26 cantons, and approval is far from certain in some cantons.<sup>3</sup> We show that establishments whose STW application was denied are a valid

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<sup>1</sup>In 2009, expenditure on STW amounted to 5 billion Euros in Germany, 5.5 billion Euros in Italy and roughly 6 billion Euros in Japan, between .1 and .3 per cent of GDP in these three countries (Boeri and Bruecker, 2011). In Switzerland, the unemployment insurance spent more than 1.1 billion Swiss Francs on STW benefits in 2009 (SECO, 2013).

<sup>2</sup>Firms that recourse to the program are likely to differ in observed and unobserved ways from firms that do not make use of it. In Switzerland, for instance, only 3% of all establishments applied for STW in 2009.

<sup>3</sup>Many countries have similar formal application procedures as Switzerland during which eligibility is tested by governmental authorities. However, local authorities usually do not have to approve the applications (Arpaia et al., 2010).

control group for establishments whose STW application was approved. One reason is that cantonal employment agencies differ substantially in the way they treat applications for STW. Approval rates range from 55% to close to 100%—differences that cannot be explained by differences in establishment characteristics at application. Another reason is that cantons deny STW not only if they think that the problems of an establishment are structural, but also if they think that the shortfall in demand is too small. Consequently, establishments that applied unsuccessfully are positively *and* negatively selected. *On average*, however, they face a similar shortfall in labor demand as treated firms according to a proxy reported in the STW application form. Indeed, pre-treatment trends in our outcome variables are very similar in the two groups of establishments.

Our main empirical analyses are thus based on a flexible event study Difference-in-Differences (DiD) approach that compares *changes* in outcomes of establishments that applied successfully to establishment that did not in the period before and after application, thus controlling for all time-invariant differences between establishments. Throughout, we use two control groups: all establishments whose applications were denied, and untreated establishments matched to treated establishments based on nearest-neighbor propensity score matching. The analyses are based on a quarterly establishment-level panel dataset that links data on establishments' applications for STW between 2007 and 2014 with the Swiss unemployment register 2009–2015 and the Swiss business censuses 2001, 2005, 2008, and 2011–2015, covering the universe of establishments in the respective years. To quantify the effect of STW on unemployment, we count the number of dismissed workers of treated and untreated establishments that register themselves as unemployed. We also count the number of hires from the two groups of establishments out of the pool of registered unemployed. Descriptively, we find that treated establishments lay off 3 percent of their workforce in the two quarters immediately following application, a small increase compared to pre-treatment levels. In contrast, establishments whose applications were denied dismiss approximately 8,5 percent of their workers—almost three times as many as in the quarters before application. The abrupt and substantial increase in dismissals just after application strongly suggests that the dismissals result from the decision on the STW application.

The event study estimates confirm that STW approval reduces dismissals into unemployment. The estimations suggest that approving STW leads to a cumulative reduction in permanent layoffs into unemployment of at least 10 percent of an establishment's workforce three years after application for STW. Importantly, the effect of STW on dismissals extends beyond the period during which treated establishment collect STW

benefits, suggesting that STW prevents dismissals permanently rather than postponing them. Moreover, we estimate that the workers that were not dismissed because of STW would have collected unemployment benefits for almost a year, almost twice the average unemployment duration in Switzerland. Overall, our estimates suggest that the 7,880 establishments that were treated in 2009 would have dismissed roughly 20,500 workers into unemployment over the course of the following three years, which is equivalent to 0.46 percentage points of the labor force in 2009. Many of these dismissed workers would have been low- or middle-educated and 8,275 would have become long-term unemployed. The results indicate that STW stimulates work sharing and helps to distribute the burden of recessions to a larger number of workers.

We present several important robustness checks and additional analyses that validate these results. For example, we show that the results are almost unchanged if we flexibly control for several types of unobserved confounders: for instance, the estimates remain very similar and highly statistically significant if we only compare firms within the same industry that apply for STW in the same canton and quarter. We also present results using an approach that combines the DiD strategy with an instrumental variable (IV) approach that directly exploits the idiosyncrasies in cantonal approval decisions. If anything, these IV estimations suggest that STW reduces unemployment even more than according to the baseline regressions. Finally, we study the impact of STW on establishment survival and growth using data from the business censuses. We find that STW increased the probability of establishment survival among establishments applying in 2009 and 2010 by 6–9 percentage points, and increased establishments’ growth in full-time equivalent employment by 10–15 percent—consistent with the effect size estimated based on the unemployment data. These estimates imply that the Swiss STW scheme saved 0.2–0.3 full-time jobs for every worker in the program.

Our findings help to reconcile the divergent results in the existing international micro and macro-level literature about the effectiveness of STW. The macro studies typically relate the use of STW to changes in employment and unemployment on the country- or region-level. Most such studies find a positive correlation between STW benefits and changes in employment and a negative correlation with changes in unemployment (Hijzen and Venn, 2011; Boeri and Bruecker, 2011; Cahuc and Carcillo, 2011; Abraham and Houseman, 2014).<sup>4</sup> As pointed out by Cahuc (2018), the findings relying on firm data

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<sup>4</sup>For the German case, these positive conclusions are generally supported by recent studies that simulate the effectiveness of STW using macro models calibrated with micro data (Balleer et al., 2016; Cooper et al., 2016; Niedermayer and Tilly, 2017).



are still scarce and, until recently, did not establish a consensus about the effectiveness of STW programs. With the exception of Boeri and Bruecker (2011), most of these studies suggest small or even negative effects of STW on employment in different contexts (Calavrezo et al., 2009, 2010; Kruppe and Scholz, 2014; Tracey and Polachek, 2018). Yet, these results should be interpreted cautiously because the counterintuitive result may be a direct consequence of the strong selection into STW—a bias that the matching and instrumental variable approaches of these studies probably only address partially.<sup>5</sup>

Two papers, contemporary to ours, tackle the selection issues convincingly and provide compelling evidence that STW can work. Cahuc et al. (2018) analyze the French STW program in the Great Recession. They instrument STW take-up of firms with the average response time to STW applications of the local administration in charge of managing the program and the geographical distance of the firm to the closest multi-establishment firm which used STW in 2008, thus exploiting firm-to-firm diffusion in knowledge about the program. Similar as we do, they find a strong positive impact of STW on employment and survival of firms. The effect is concentrated in firms that potentially face a large drop in revenues. Interestingly, they show that the cost per job saved is very low in STW programs relative to other employment policies because STW is more effective in targeting jobs at risk of being destroyed. Giupponi and Landais (2018) exploit plausibly exogenous eligibility rules based on firm size and industry affiliation to study the employment and welfare effects of STW in Italy. They find negative effects on hours worked but large and positive effects on employment. In contrast to the Swiss and the French scheme (Cahuc et al., 2018), the Italian STW scheme only postpones rather than prevents dismissals, hence only offering short-run insurance of workers against recessions. One possible explanation for the difference compared to our results is that the 2009 recession in Italy turned out to be long-lasting and deep, while the recession in Switzerland was sharp but brief (V-shaped). Another is that the particular Italian program analyzed by Giupponi and Landais (2018) may be more prone to displacement effects because it targets firms experiencing broadly defined economic shocks including a need for restructuring, bankruptcy procedures, and illiquidity issues.<sup>6</sup>

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<sup>5</sup>This literature generally uses the prior experience of firms with the program (i.e. the lagged take-up rate) to instrument for the use of STW (while often controlling for firms' revenue growth). However, firms that use STW may be cyclically more sensitive than firms that do not. Consequently, employment may fluctuate more strongly in firms with STW experience than in firms without, invalidating the exclusion restriction (Cahuc, 2018; Tracey and Polachek, 2018).

<sup>6</sup>The Swiss scheme analyzed here and the part of the French scheme analyzed by Cahuc et al. (2018) mainly target firms with a temporary drop in demand. Italy also has a scheme for such cases. However, Giupponi and Landais (2018) focus on another scheme.

Our study contributes to this literature in at least three important respects. First, our paper adds to the scarce micro-level evidence on the causal effect of STW on firm-level dismissals, employment, and establishment survival. We rely on a novel comprehensive data set that links several administrative sources and provides a methodological innovation compared to previous studies by focusing solely on establishments that aimed at introducing STW. Exploiting that denial of STW is partly idiosyncratic in Switzerland and addressing the remaining selection concerns in various ways, our study provides convincing evidence that STW can work. Second, our paper is the first to provide direct evidence on the effect of STW on inflows into and outflows out of unemployment. We show that STW indeed helps to limit the increase in unemployment in recessionary periods, as has long been hypothesized (see Boeri and Bruecker, 2011, for many examples). The link between the STW and unemployment register allows us to make a third contribution: a direct comparison of the fiscal costs and benefits of STW. We estimate that each approved STW case saves between 26 to 36.5 days of unemployment benefit payments *per worker in the firm* in the following three years. Comparing the fiscal benefits of STW to spending on STW benefits for the same cases, we find that the fiscal benefits of the STW scheme may have been large enough to fully compensate the total spending on STW benefits. This favorable result arises because the STW scheme has limited deadweight and displacement effects, but also because STW prevents many dismissals of workers that would have faced comparatively long unemployment spells.

This paper is organized as follows. Section 2 characterizes the Swiss STW scheme and discusses the use of STW during and in the aftermath of the Great Recession. Section 3 presents the data used in the empirical analyses. Section 4 discusses the cantonal approval decisions and presents the empirical strategy. Section 5 presents our estimates of the effects of the Swiss STW scheme on unemployment, employment, and establishment survival. Section 5.6 uses these results in order to assess the direct financial costs and benefits of the Swiss STW scheme. Section 6 summarizes our main findings and concludes.

## 2 Short-time work in Switzerland

### 2.1 The Swiss short-time work scheme

STW is geared towards firms that face a temporary low demand. Firms with STW can temporarily reduce the hours of work of some or all of their workers. The workers, in turn, are compensated for their income losses in the form of STW benefits. In the

case of Switzerland, these benefits are paid by the unemployment insurance financed via payroll taxes, which replaces 80 percent of workers' losses (up to a maximum insured income of 126k CHF, approximately 126k USD in 2010). Firms continue to pay wages for the hours that workers actually work. Workers with a temporary contract, temporary agency workers, and apprentices are not eligible for STW benefits. As in most other STW schemes, there is co-payment of firms: firms have to cover the benefits—80% of workers' wages—during the first two (in the first six month) or three (from the seventh month onwards) days of every month in which they collect STW benefits. In order to limit the scope for deadweight effects, STW benefits are paid for a maximum of 12 months within two years. However, the government relaxed these provisions as a response to the Great Recession. On April 1 2009, the number of days firms had to cover workers' benefits was reduced to one day per month. At the same time, the maximum duration during which firms can collect STW benefits was extended from 12 to 18 month. It was increased further to 24 month on April 1 2010. These measures were gradually phased out after the recession.<sup>7</sup>

The Swiss STW scheme is similar to those in other countries in many respects (Cahuc, 2018; Hijzen and Venn, 2011; Lydon et al., 2019, provide overviews and comparisons of such schemes). However, it stands out in three ways. First, Swiss firms can apply for STW if the affected workers agree to the introduction. There is no need for an explicit agreement between the social partners, as is customary in many countries. Second, Switzerland's STW scheme is generous. The replacement rate is well above the average of the countries surveyed in Hijzen and Venn (2011). Similarly, while the potential benefit duration of 12 months that applies under normal circumstances is close to the international average, it was comparatively high during the Great Recession. Only Finland and Japan allowed the use of STW for more than the 24 months in place in Switzerland (Hijzen and Venn, 2011; Boeri and Bruecker, 2011).

The third particularity of the Swiss STW scheme relates to the fact that firms that wish to introduce STW have to apply for STW benefits. This process is decentralized: the institution that decides on firms' applications for STW are the employment agencies of each of the 26 Swiss cantons. The institution that approves STW is thus different from the institution that pays the STW benefits, the federal unemployment insurance. When deciding on an application, cantonal agencies have to check whether the applying

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<sup>7</sup>The reduction to one day of co-payment expired in December 2011, and the maximum duration of STW benefits was reduced to 18 months. At the end of 2013, the maximum duration was reduced to its normal level of 12 months.

firm meets certain eligibility criteria. We discuss these criteria and the cantonal approval decision in detail in Section 4.1. Cantonal employment agencies decide on applications usually within a short period—on average within 10.5 days, and almost always within less than 30 days. Once approved, STW can be used without further conditions: there are no provisions regarding training and no prohibitions of dismissals for firms, and no job-search requirements for workers.

## 2.2 Short-time work during and after the Great Recession

The Great Recession hit Switzerland mid-2008 after a phase of high GDP and employment growth. The recession was most severe in late 2008 and the first half of 2009. While many domestic sectors only experienced a mild recession, the sharp drop in international demand strongly affected the export-oriented sectors. Value added in the Swiss banking and manufacturing sectors, for instance, dropped by more than 10 percent in 2009. In hotels and restaurants, value added fell by more than 5 percent. Overall GDP declined by 2.1 percent in 2009. Unemployment according to the ILO definition increased from 3.9 to 4.8 percent.

Figure 1 reports the number of employees covered by STW benefits per month from 2007 to 2014. Only very few firms used STW benefits prior to the recession. As a reaction to the sharp fall in international demand, demand for STW increased strongly in early 2009. The use of STW peaked mid-2009 when more than 90'000 workers, accounting for approximately 2 percent of the total Swiss workforce, received STW benefits. These aggregate numbers disguise large sectoral and regional differences, however. As is illustrated by Figure 1, take-up of STW was strongly concentrated in manufacturing. Therefore, regions with a high manufacturing share depended more strongly on STW. In certain Swiss cantons, more than 10 percent of all workers received STW benefits in 2009.<sup>8</sup>

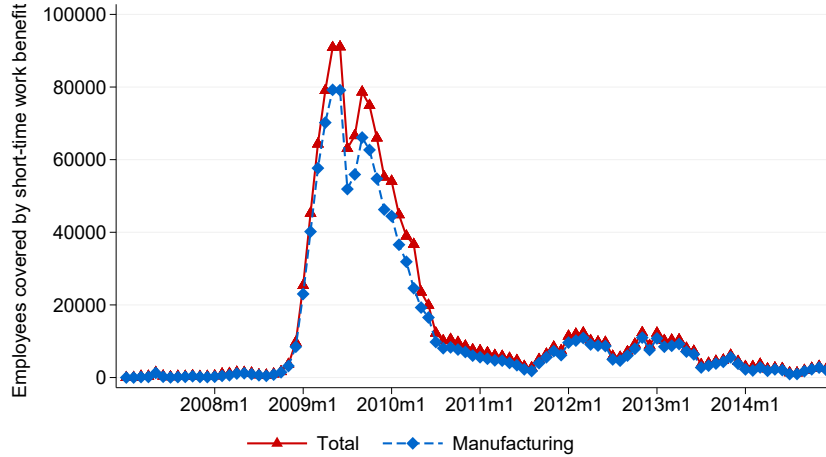
The use of STW declined markedly in 2010 and 2011. Take-up increased again in 2012 because of the Euro debt crisis. This crisis had a direct effect on exporters due to weak demand in many European countries. It also indirectly affected them because it led to a very strong real appreciation of the Swiss franc relative to the Euro, as the looming uncertainty about the fiscal stance of several European countries increased demand for Swiss francs, traditionally used as a safe haven currency. To ease the competitive pres-

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<sup>8</sup>In some regional labor markets (NUTS-III regions, similar to commuting zones), coverage rates exceeded 15 percent (see Figure A.1 and Table A.2 in the Appendix).

asures put on Swiss exporters and to counteract a possible deflation, the Swiss National Bank introduced an exchange rate peg of 1.20 for the Swiss franc relative to the Euro. Throughout 2012, 10'000 workers were covered by the STW scheme. STW coverage declined substantially from mid-2013 onward.

Figure 1: Employees covered by short-time work benefits per month



### 3 Data

Our main analyses are based on a link between three register datasets: (i) data about establishments’ STW applications and receipts in the 2007–2014 period, (ii) the Swiss unemployment insurance register (UIR) 2009–2015, and (iii) the Swiss business censuses (BC) 2001–2015. Table 1 presents an overview of these data sources.

Our main data source is the short-time work dataset, which covers all establishments that applied for STW in the years 2007 to 2014. On the one hand, the data contain information from the establishments’ application forms for STW sent to the cantonal employment agencies (such as application date, total employment at registration, or employees registered for STW). On the other hand, they reveal whether STW was approved and contain detailed information on the use of, and benefit payments to, establishments with STW. While our dataset contains the quantitative information from establishments’ application forms, it does not contain the mandatory written statements of establishments why they require STW benefits, what they did to prevent it, and why they think the demand shortfall is temporary.

We use the STW data to construct a dataset of individual “cases” of STW. A case starts when an establishment applies for STW. Establishments are formally required to

renew their STW approval every three or six months. Since renewals are approved in 99% of all cases, we treat renewals as the continuation of a case that started earlier. We also do not start a new case if the applying establishment collected STW benefits within the 6 months before application.<sup>9</sup> For each case, we then compute the “event time”  $\tau$ , i.e. the time elapsed since the quarter in which an establishment applied for STW. Event time is normalized to zero at application ( $\tau = 0$ ). In the infrequent case that an establishment has more than one STW case, the post-treatment period of the first case overlaps with the pre-treatment period of the second case. In the empirical analysis, we avoid this overlap by “cutting” the post- and pre-treatment periods of the two cases in such a way that both have the same length.<sup>10</sup> The resulting case-level panel dataset contains 16’243 cases from 12’570 different establishments in the years 2009 to 2014 (see Table 1). 2’786 establishments have more than one case. 13’565 of 16’243 (83.5%) cases were approved.

We merge this dataset of STW cases with two other data sources. The first is individual-level spell data from the Swiss Unemployment insurance register (UIR) covering the period 2009 to 2015. The register contains information on the universe of registered job seekers in Switzerland, including detailed demographic characteristics and information on individual unemployment spells (such as unemployment duration and benefit receipts). We can match the UIR to the STW dataset because the UIR contains the identifier of the previous employer of individuals that register themselves as unemployed. The UIR also contains the identifier of the new employer if a registered individual left unemployment with a job. However, the establishment identifiers of the previous employer are only recorded since 2009, and are still partially incomplete in 2009.<sup>11</sup> This explains why most of our analyses based on the UIR start in 2009.

We analyze the effect of STW on unemployment using the UIR data. An unemployed is every person that ever received unemployment benefits during the period she or he is registered at the unemployment insurance.<sup>12</sup> Based on the establishment identifiers in the

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<sup>9</sup>There is a very small number of cases defined this way—roughly 0.5%—where we observe a second decision on the same case that contradicts the first decision of the case. We drop these inconsistent cases from the estimation sample.

<sup>10</sup>For instance, if one case starts in March 2009 and another case in February 2010—i.e. 10 months later—we cut the post-treatment period of the first case at  $\tau = +5$  months and the pre-treatment period of the second at  $\tau = -5$  months.

<sup>11</sup>The share of job seekers with known last employer is approximately 56 percent in 2009. As we show below, the incompleteness of the data in 2009 does not affect our main results. We thus include 2009 in our baseline analysis. Starting in 2010, establishment identifiers are recorded for a very large share of registered job seekers that have a previous employer.

<sup>12</sup>As shown in the policy report that preceded this paper (Kopp and Siegenthaler, 2017), the results (and effect sizes) are very similar if we also consider individuals that register as job seekers at the Swiss public employment services but that are not eligible for unemployment benefits. The unemployed

UIR, we count the number of newly registered unemployed coming from an establishment that applied for STW benefits. Similarly, we count the number of unemployed hired by these establishments. Because the UIR contains the universe of registered job seekers, we assume that establishments in the STW dataset that do not appear in the UIR in a given quarter have zero flows into and out of (registered) unemployment in that quarter. The difference between dismissals into unemployment and hires from the pool of unemployed, appropriately normalized by establishments' employment at registration, is our main outcome of interest with this dataset. We term it *net share of dismissed workers*. Using the same data, we also study the effect of STW on total daily allowances per worker and long-term unemployment. A detailed definition of the outcomes based on the UIR can be found in subsection A in the appendix.

The third data source used in this paper is the Swiss business censuses (BC), which provide information on employment and establishment characteristics of the universe of private and public establishments in Switzerland in 2001, 2005, 2008, 2011–2015. Approximately 4 million employed persons in 389,000 workplaces are included in the census of 2008. The panel dataset is very reliable. Until 2008, the BC were based on mandatory surveys conducted in September of the respective year. Since 2011, the census is conducted annually based on register data and refers to December of each year. We use this comprehensive data to analyze establishment survival and establishment growth in terms of full-time equivalent (FTE). In the appendix, we validate these results based on a fourth data source: the Swiss Job Statistics 2005–2014. The advantage of this data relative to the censuses is their quarterly frequency. The disadvantage is the smaller sample coverage, especially concerning small establishments.

## 4 Empirical strategy

### 4.1 Cantonal approval decision

Our analyses are based on the before-after comparison of establishments that applied for STW and whose application was approved (henceforth termed the *treatment group*), and establishments that applied for STW and whose application was denied (the *control group*). This section provides a detailed discussion why this comparison represents fertile grounds to study the causal effects of STW in the Swiss case.

According to the law, Swiss establishments are eligible to STW benefits if the working-

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according to our definition account for 82 percent of all registered job seekers.

Table 1: Overview of data sources and sample overlap

	Short-time work applications	Unemployment insurance register	Business Censuses	Job Statistics
Acronym	STW dataset	UIR	BC	Jobstat
Time period	2007–2014	2009–2015	2001, 2005, 2008, 2011–2015	2005–2014
Industry (NACE)	all	all	all	10–33 & 45–47
Sampling frequency	Monthly	Monthly	Sep. until 2008, Dec. 2011–2015	Quarterly
Unit of observation	Establishment (department)*	Unemployment spell	Establishment	Establishment
Coverage	Universe	Universe	Universe	Survey
Approved STW cases	13,565	11,361**	11,877	4,106
Denied STW cases	2,678	2,155**	2,212	227

*Notes:* The table shows key characteristics of the datasets used in this study. “Approved STW cases” shows the number of approved STW cases in the period 2009 to 2014 according to the case definition discussed in section 3. Industry refers to NACE rev. 2 two-digit industry codes.

\* STW can be introduced for a single division (operational department) of an establishment under certain circumstances. The original data is thus at the department level. 2’152 out of 16’243 different STW cases between 2009 and 2014 are from multi-division establishments. For our analyses, we aggregate the data to the establishment level. We treat an application as approved in a given month if the application of at least one division of an establishment was approved.

\*\* The figures counts the number of employers collecting STW that show up in the UIR at least once in the 2009–2015 period. In our empirical analysis, we assume that establishments in the STW dataset that do not appear in the UIR have zero flows into and out of (registered) unemployment.

time reduction is temporary and if STW can be expected to help preserve jobs in the long run. Furthermore, the reduction has to be due to economic reasons, unavoidable, and to amount to at least 10 percent of the usual working time of the firm. STW is supposed to be denied if the working-time reduction is due to circumstances that are part of the firms’ usual operational risk, customary in the respective firm, or due to seasonal fluctuations.

The vague formulation of the eligibility criteria and the absence of clear instructions by the federal government how to implement them leaves substantial discretionary scope to the cantonal employment agencies. This scope is reflected in large differences in approval rates across cantons, ranging from 55% in the canton of Fribourg to exactly 100% in the canton of Uri (see Table A.3 in the Appendix). These differences cannot be explained by the characteristics of applying establishments.<sup>13</sup> Moreover, while cantonal approval rates vary within cantons, they are persistent: a canton that generally approves applications

<sup>13</sup>This is shown by Figure A.2 in the Appendix. The Figure reports the marginal effects of the different cantons on the probability that an establishment’s request for STW is approved controlling for a wide set of establishment and labor market characteristics.



for STW is more likely to approve an application in the future than a canton that handles applications more strictly (see Figure A.3 in the appendix).

Table 2: Establishment characteristics by cantonal approval decision

	(1) STW approved		(2) STW denied		(3) Matched control (weighted)	
	mean	sd	mean	sd	mean	sd
<i>Baseline characteristics</i>						
FTE employment	24.57	(45.77)	10.42	(22.15)	21.07	(36.61)
Manufacturing share	0.54	(0.50)	0.18	(0.39)	0.54	(0.50)
Construction share	0.15	(0.35)	0.29	(0.46)	0.19	(0.39)
Trade sector share	0.10	(0.30)	0.15	(0.36)	0.07	(0.26)
Other 3. sector share	0.21	(0.41)	0.37	(0.48)	0.19	(0.40)
Establishment age (years, BC)	12.06	(5.03)	10.14	(6.49)	12.30	(5.00)
Importer share (BC)	0.51	(0.50)	0.27	(0.45)	0.54	(0.50)
Exporter share (BC)	0.48	(0.50)	0.18	(0.39)	0.49	(0.50)
Share of female workers (BC)	0.27	(0.24)	0.28	(0.29)	0.25	(0.24)
Share of part-time workers (BC)	0.17	(0.20)	0.21	(0.26)	0.19	(0.21)
Share of foreign workers (BC)	0.29	(0.28)	0.33	(0.34)	0.31	(0.29)
Establishment growth 2005–2008 (BC)	-0.01	(0.62)	-0.23	(0.87)	-0.06	(0.67)
Establishment growth 2001–2008 (BC)	-0.25	(0.91)	-0.52	(1.06)	-0.31	(0.95)
<i>STW collection</i>						
Share of workers reg. for STW	0.71	(0.25)	0.68	(0.29)	0.67	(0.28)
Short STW duration	0.42	(0.49)	0.42	(0.49)	0.51	(0.50)
Prob. of STW approval ( <i>prop. score</i> )	0.89	(0.14)	0.64	(0.22)	0.89	(0.14)
Take-up of STW	0.76	(0.43)				
Share of workers covered by STW	0.62	(0.37)				
Duration of STW collection (months)	5.24	(5.88)				
Observations	13420		2642		1471	

*Notes:* The table provides descriptive statistics for establishments that applied for STW in the 2009–2014 period. The upper part of the table shows baseline establishment characteristics measured at application or before application if the information is from the business censuses (BC, almost always measured in September 2008). The lower part reveals characteristics of establishments’ application and use of STW. The table shows these statistics separately for establishments whose STW application was approved (column 1) and denied (column 2), and for a sample of untreated establishments matched to treated establishments based on nearest-neighbor matching (column 3, see text for details). “Prob. of STW approval” reveals the estimated propensity score used to match units. Mean and standard deviation in column 3 are weighted by the number of times that an untreated establishment is matched to a treated establishment. Variables derived from the BC are missing in a few cases. For these variables, the number of observations does not coincide exactly with the number of observations shown in the last row. “Establishment growth” is computed as a symmetric growth rate and is measured in full-time equivalent (FTE) employment. Establishment age is computed based on the first business census in which an establishment is observed. Industry affiliation is computed based on NACE (rev. 2) two-digit codes. “Short STW duration” is a dummy equal to one if an establishment registered for STW for a duration that is lower than the duration until STW has to be renewed for the first time (6 months in 2009/10 and 3 months in all other years).

Table 3: Effect of establishment characteristics and the business cycle on STW approval

	(1)	(2)	(3)	(4)
<i>Establishment size</i>				
0–4 employees	ref.	ref.	ref.	ref.
5–9 employees	0.04*** (0.007)	0.04*** (0.007)	0.04*** (0.007)	0.04*** (0.008)
10–19 employees	0.04*** (0.009)	0.04*** (0.008)	0.04*** (0.009)	0.04*** (0.010)
20–49 employees	0.06*** (0.010)	0.07*** (0.009)	0.06*** (0.010)	0.07*** (0.011)
50–99 employees	0.06*** (0.014)	0.07*** (0.013)	0.06*** (0.014)	0.05*** (0.018)
100–499 employees	0.11*** (0.014)	0.11*** (0.014)	0.11*** (0.014)	0.10*** (0.014)
<i>Industry</i>				
Manufacturing	ref.	ref.		ref.
Construction	-0.10*** (0.008)	-0.10*** (0.008)		-0.08*** (0.010)
Other 2. Sector	-0.07 (0.043)	-0.05 (0.038)		-0.07 (0.045)
Trade sector	-0.08*** (0.009)	-0.09*** (0.009)		-0.09*** (0.011)
Other 3. Sector	-0.08*** (0.007)	-0.08*** (0.007)		-0.08*** (0.008)
<i>Establishment age (BC)</i>				
4–9 years	0.03** (0.013)	0.03** (0.013)	0.02 (0.015)	0.00 (0.023)
At least 10 years	0.03** (0.014)	0.02 (0.015)	0.01 (0.017)	0.00 (0.023)
<i>Other establ. characteristics (BC)</i>				
Importer	0.02** (0.007)	0.02** (0.007)	0.02*** (0.007)	0.02** (0.008)
Exporter	0.05*** (0.007)	0.05*** (0.007)	0.05*** (0.007)	0.04*** (0.008)
FDI (0/1)	0.03** (0.013)	0.03** (0.013)	0.03** (0.013)	0.04*** (0.012)
Foreign owned (0/1)	0.01 (0.014)	0.02 (0.013)	0.02 (0.013)	0.04*** (0.012)
Share of female workers	-0.02** (0.011)	-0.03** (0.011)	-0.02* (0.011)	-0.02** (0.013)
Share of part-time workers	-0.02** (0.012)	-0.02* (0.012)	-0.03** (0.012)	-0.01 (0.014)
Share of foreign workers	-0.04*** (0.009)	-0.05*** (0.008)	-0.04*** (0.009)	-0.05*** (0.010)
Establ. important for reg. LM	0.00 (0.006)	-0.00 (0.006)	-0.00 (0.006)	-0.00 (0.007)
<i>STW experience</i>				
Prior STW application (0/1)	-0.13*** (0.012)	-0.13*** (0.012)	-0.13*** (0.012)	-0.05*** (0.016)
Prior STW approval (0/1)	0.23*** (0.010)	0.23*** (0.010)	0.23*** (0.010)	0.09*** (0.010)
Short STW duration (0/1)	-0.03*** (0.006)	-0.03*** (0.006)	-0.03*** (0.006)	-0.02*** (0.006)
<i>Share of workers reg. for STW</i>				
0–19%	ref.	ref.	ref.	ref.
20–39%	0.09*** (0.020)	0.09*** (0.019)	0.09*** (0.020)	0.11*** (0.026)
40–79%	0.12*** (0.019)	0.12*** (0.018)	0.12*** (0.019)	0.13*** (0.024)
80–99%	0.15*** (0.019)	0.15*** (0.019)	0.14*** (0.019)	0.15*** (0.025)
100%	0.10*** (0.019)	0.10*** (0.019)	0.10*** (0.019)	0.12*** (0.025)
<i>Business cycle</i>				
Cantonal unemployment rate	0.19*** (0.029)	0.15*** (0.029)	0.19*** (0.030)	0.28*** (0.033)
STW applications per canton	0.01*** (0.002)	0.01*** (0.002)	0.01*** (0.002)	0.01*** (0.002)
<i>Establishment growth BC</i>				
Employment growth 2005–08		0.00 (0.005)	0.00 (0.005)	0.01* (0.008)
Employment growth 2001–05		0.00 (0.004)	0.00 (0.004)	0.00 (0.004)
Observations	14599	14361	14266	8868
Period FE	Yes	Yes	Yes	Yes
Industry-period FE	No	No	Yes	No
Mc Fadden's adj. $R^2$	0.26	0.26	0.26	0.21

*Notes:* The table shows average marginal effects of establishment and labor market characteristics on the probability that an establishment's request for short-time work (STW) is approved. The marginal effects are derived from probit regressions. The estimation sample contains all establishments applying for STW in the 2009–2014 period that could be matched to at least one business census (BC). The pre-application establishment characteristics are either measured in the quarter of application for STW (applies, e.g., to establishment size, age, industry, and the number of STW applications in the canton) or, if they are derived from the BC, they almost always refer to 2008 (except for the variables measuring international exposure which refer to 2005). "Period FE" are fixed effects by quarter of application. Some variables from the BC are missing for a few establishments. In order to avoid losing these observations, we replace the missing with zeros and add indicator variables to the regression that are one if the respective variable is missing (omitted from the table). Column 4 is restricted to establishments applying in 2009 or 2010. In column 3, industry-period cells that do not contain treated and untreated establishments are dropped. We use the average of the cantonal labor force over the years 2010 to 2011 as numerator of the cantonal unemployment rate. The number of establishments that apply at the same time is constructed is the number of applications within a 2-week period around the application date. Establishment growth is measured as symmetric growth rates in full-time equivalents. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

However, STW is not randomly assigned to establishments. Table 2 reports characteristics of establishments with denied and approved applications. It shows that treated establishments are larger, more export-oriented, more likely to operate in the manufacturing sector, and they have a lower share of foreign workers. These findings are confirmed in a regression analysis of the factors that determine the probability of STW approval (see Table 3). The table also shows that the chances for approval are higher, *ceteris paribus*, if the same establishment applied successfully before. However, the probit regressions also provide additional evidence for the discretionary scope of cantons. First, they indicate that the success of an establishment’s application for STW does not only depend on its economic fundamentals, but also on political factors and the economic situation in a canton: its chances for approval are higher if the cantonal unemployment rate is high, and if the workload of the cantonal employment agencies is high (proxied by the number of establishments that apply at the same point in a 2-week window around the application). Second, the estimates suggest that many untreated establishments may have received STW had they applied in a different canton or at another point in time. The estimated average probability of treatment—the propensity score—among the control group is 64%, which is not much lower than the 89% probability in the treatment group (see the lower panel of Table 2).

Moreover, the analyses below are based on panel regressions that focus on within-establishment changes in outcomes. These regressions thus control for the time-invariant differences between establishments in the treatment and control group visible in Table 2. The most relevant question regarding the validity of our approach is thus whether treated and control establishments would have had similar post-application *growth rates of outcomes* without STW. There are at least three further reasons—apart from the between-canton idiosyncrasies in handling applications—suggesting that this question can be affirmed.

The first is that we do not find statistically significant differences in the evolution of outcomes in the *pre-treatment* period, as we show formally for all our main regression outcomes below. In order to provide first evidence along these lines, columns 2–4 of Table 3 show that there is only a very weak association between the probability of STW approval and establishments’ employment trends in the pre-recession period, as measured by growth in full-time equivalent employment in the 2001–2008 and 2005–2008 periods.<sup>14</sup> This holds even if we only compare establishments within the same industry (column 3)

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<sup>14</sup>For reasons explained in detail in section 5.5, we use *symmetric* employment growth rates here. The symmetric growth rate of establishment  $i$  between periods  $t-k$  and  $t$  is defined as:  $\Delta y_{i,t} = \frac{y_{i,t} - y_{i,t-k}}{0.5(y_{i,t} + y_{i,t-k})}$ .

or if we restrict the sample to establishments applying in 2009 and 2010 (column 4) or 2009 (not shown) to ensure that the pre-2008 trends should be relevant for the cantonal authorities when taking the STW decision.

Another important reason in favor of the validity of our approach is that the main results are very similar if we use a matched control sample that is comparable to treated establishments in terms of observable characteristics. We build this matched control sample using nearest-neighbor matching based on the estimated propensity score from the regression in column 1 of Table 3. We match one control unit to each treated unit with replacement, which implies that certain untreated establishments are matched to several treated establishments and certain establishments are never matched. Column 3 of Table 2 shows the characteristics of this matched control sample. By construction, the matched sample is very similar to treated establishments in terms of the establishments' characteristics that determine treatment, and the estimated propensity score is 89% as in the treatment group.

A final reason why the comparison by approval status works relates to the reasons why cantons deny STW in accordance with the eligibility criteria defined by the law. In practice, denials are usually due to one of the following three reasons.<sup>15</sup> The first is that establishments' problems are considered as structural rather than temporary; giving rise to the fear that STW might only postpone but not prevent dismissals. The second is that the shortfall in demand is deemed as too small, such that the firm might be able to deal with it on its own. The third is that the temporary drop in demand is considered seasonal rather than cyclical. Importantly, these three reasons have opposing implications regarding the composition of the control and treatment group. If cantons predominantly deny STW because they suspect that firms' problems are structural, treated establishments would be "healthier" on average (e.g., more competitive) than the control establishments, and would thus probably have dismissed less workers than the control establishments had they not received STW. Consequently, the before-after comparison of outcomes between the groups may *overestimate* the effect of STW. Conversely, if the main driver of cantonal denials is the perception that the shortfall in demand is too small, healthy establishments would be overrepresented in the control group, and we would likely *underestimate* the effect of STW. The sign of a possible bias of our estimates is ambiguous if cantons mainly

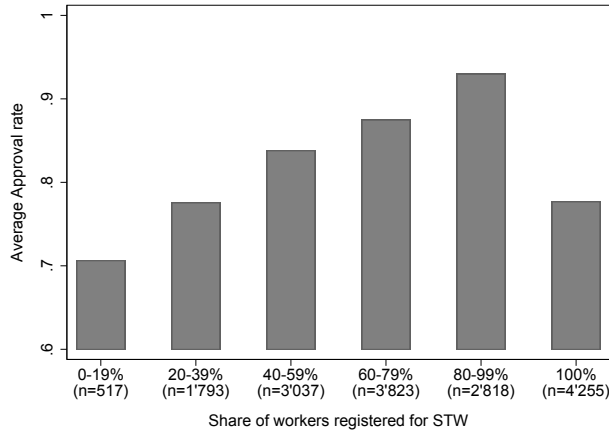
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<sup>15</sup>The discussion represents the result of conversations that we had with several experts experienced with or involved in the approval process. These specialists were part of an expert group that supervised and supported the writing of a policy report that preceded this paper (Kopp and Siegenthaler, 2017). The group comprised of cantonal representatives, government officials, and members of trade unions and employer associations.

deny STW because of seasonality.<sup>16</sup>

The comparability of the two groups thus hinges on the quantitative importance of the first two motives for denials. Figure 2 depicts the cantonal approval rates for STW applications depending on the share of workers that the establishment registers for STW at application—a proxy for the size of its shortfall in labor demand. The figure shows that cantons are more likely to deny STW to establishments that only register a small share of the workforce for STW—establishments that appear “healthy”<sup>17</sup>—and to establishments that register every worker—establishments that appear to have structural problems. This result also holds conditional on all observables that affect approval, as Table 3 shows. On average, however, the two motives seem to be similarly important—the share of workers registered for STW is 71% in the treatment group and 68% in the control group (see Table 2).

Figure 2: Expected shortfall in labor demand and approval decision



*Notes:* The figure shows the average cantonal approval rate depending on the share of workers that establishments register for STW at application. The latter serves as an indicator of the estimated shortfall in establishments’ labor demand at application. The figure suggests that cantons deny STW both to particularly “healthy” and particularly “unhealthy” establishments.

## 4.2 Regression model

Based on the evidence presented in the last section, our baseline approach is to compare the differences in the change in outcomes at application ( $\tau = 0$ ) between treatment

<sup>16</sup>The reason is that control establishments would be more likely to lay off workers shortly after the application for STW. But after a short period of around a quarter, they would probably dismiss less and hire more workers than treated establishments.

<sup>17</sup>Similarly, Table 3 shows that the approval rate is lower for establishments that apply for STW for a very short period (i.e. for less months than it takes until STW has to be renewed for the first time). This finding, too, suggests that cantons refuse STW to establishments if there is evidence that the establishment may be able to deal with the temporary drop on its own.

and the two control groups—all establishments with denied applications as well as the control sample matched based on the propensity score. We implement this approach by estimating the following event study Difference-in-Differences (DiD) model:

$$u_{i,t} = \delta_i + \gamma_t + \gamma_\tau + \sum_{\tau=-k}^k \beta_\tau STW_{i,t}^\tau + \epsilon_{i,t} \quad (1)$$

The dependent variable of this regression is the outcome of interest of an establishment (e.g. net share of dismissed workers) in STW case  $i$  and period  $t$ , denoted  $u_{i,t}$ . The central independent variables are the sequence of “event study” indicators for approval of STW, denoted  $STW_{i,t}^\tau$ . The model contains one of these indicator variables for each event period within the event window  $[-k, k]$ .  $STW_{i,t}^\tau$  is one in event time period  $\tau = k$  if case  $i$  is *approved*, although not all establishments whose applications were approved actually collect STW benefits (see Section 5.1). We focus on STW approval throughout the paper because even the *possibility* to recourse to STW may have an effect on establishments’ decisions whether or not to dismiss workers, and hence represents part of the treatment effect of interest. If STW works, we expect that establishments whose application was approved display a smaller increase in dismissals, relative to the pre-treatment period, than establishments whose application was denied, such that the DiD estimates  $\beta_\tau$  are negative in the treatment period (i.e.  $\tau \geq 0$ ). Because we estimate an entire sequence of DiD coefficients, we can evaluate the effect of STW approval for every period  $k$  around the time of an establishment’s application.

An important ingredient of the model are the case fixed effects  $\delta_i$ . These account for all observed and unobserved characteristics of an establishment correlated with  $u_{i,t}$  that do not change over the period of the case. These are, for instance, establishments’ size and productivity at the time of application as well as many factors that potentially influence cantonal approval practices. One important such factor is the mandatory statements that establishment hand in to the cantonal authorities when applying for STW benefits. In fact, due to the case fixed effects, all factors that affect cantons’ decisions but that do not lead to differential *changes* in dismissals at  $\tau = 0$  between treatment and control group do not matter for the validity of the approach. The specification also controls for a full set of period fixed effects  $\gamma_t$ , which account unobserved factors that affect all observations in a given time period equally such as common business cycle shocks or seasonality effects<sup>18</sup>,

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<sup>18</sup>The period and even time fixed effects absorb seasonal patterns that are common to all establishments. As shown by column 4 of Table 6 below, the results are virtually unchanged if we include industry-period effects that account for seasonal patterns specific to two-digit industries. Moreover, our results are very similar if we simply drop seasonal industries altogether.

and for event time fixed effects  $\gamma_\tau$ , an individual fixed effect for each event time period.<sup>19</sup>

For the event study model to work, we need to decide on certain technicalities. First, we have to define an “event window”  $k$  around the application date within which we estimate effects. In the case of the unemployment data, we settle on  $\pm 3$  years, i.e. we track the outcome over  $k = \pm 12$  quarters around the time of application.<sup>20</sup> Second, as all DiD effects are estimated *relative* to each other, we need to decide on a reference period. As is common in the literature, we normalize the coefficients relative to the event period just before application (i.e.  $\tau = -1$ ) by omitting the respective event study coefficient, which makes it easy to test for an impact of STW. Third, we impose two sample restrictions: we discard treated establishments with more than 500 employees since there are no untreated establishments with more than 500 employees, and we focus only on establishments that are present in the three periods  $\tau = -1$ ,  $\tau = 0$ , and  $\tau = 1$ .<sup>21</sup> Finally, we cluster standard errors at the level of establishments. Our standard errors thus account for the facts that the errors may be correlated within establishments over time and that certain establishments have several cases, which are unlikely independent.

## 5 Results

### 5.1 Descriptive Evidence

In this and the following subsections, we examine whether STW prevents unemployment. We start with a short discussion of take up rates of STW benefits among establishments whose application was approved. The lower part of Table 2 shows that the average take-up rate is 76%. Conditional on take up, establishments cover 62 percent of their workforce with STW benefits. Figure 3 illustrates the dynamics of the take up rate, the share of workers covered by STW benefits, and the share of hours covered by STW in the quarters after establishments’ STW application. We observe that the fraction of establishments that use STW increases rapidly, reaches its peak one quarter after the quarter of application, and then begins to taper off. Similarly, the share of workers

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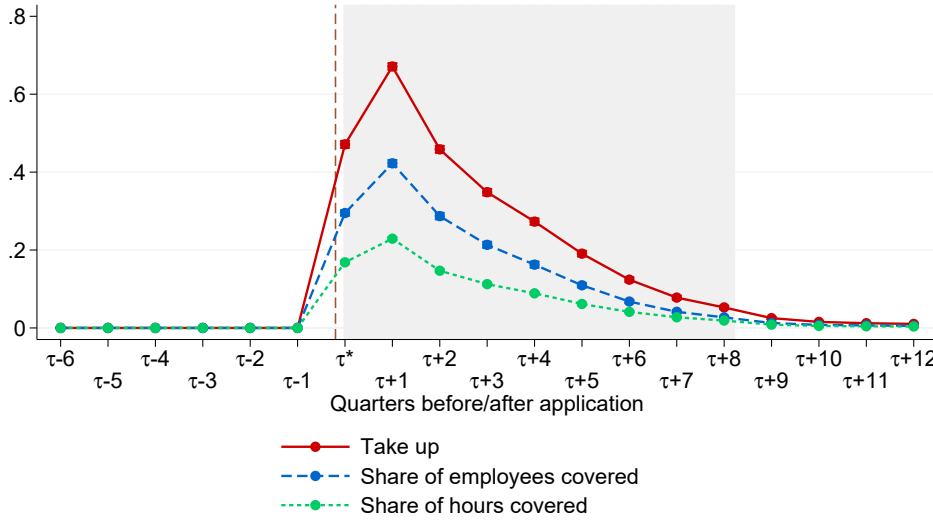
<sup>19</sup> $\gamma_t$  and  $\gamma_\tau$  can be separately identified because (i) we have both, treated and control units and (ii) the treated are treated at different points in time, so that the time and the event index do not coincide.

<sup>20</sup>The question is then how to deal with the fact that we have more than 12 periods before and after treatment in some cases. To address this, we “bin up” the endpoints, i.e. we build an event study dummy that is 1 in all periods  $k < -12$  for treated establishments and another one that is always one in all periods  $k \geq 12$  for treated establishments. We include these two dummies in all regressions but omit them from the output.

<sup>21</sup>Requiring that establishments have to be present at least in the periods from  $\tau = -1$  to  $\tau = 1$  ensures that our results are not driven by the cases for which we do not have *any* pre-treatment period.

covered by STW reaches a maximum of 42% in the first quarter after application. Since not all of these workers reduce their working time by 100%, the share of covered hours is smaller—slightly less than 25% one quarter after application. These numbers include establishments that never take-up STW. One striking result from the figure is that the large majority of establishments stops collecting STW benefits long before they reach the legal maximum of 8 quarters (marked by the grey area).<sup>22</sup> Establishments that recourse to STW benefits collect STW benefits on average for only 5.2 months (see Table 2). These results indicate that establishments use STW as a temporary measure and stop collecting benefits voluntarily.

Figure 3: Take-up and intensity of short-time work use



Notes: The figure shows the use of STW in establishments whose STW application was approved.  $\tau^*$  is the quarter of the STW application. “Take up” reports the average share of establishments actually collecting STW benefits among these treated establishments. “Share of employees covered” represents the number of workers covered by STW benefits as a share of total employment at application. “Share of hours covered” is the total number of hours covered by STW benefits as a share of total normal hours (as reported at registration). Treated establishments that did not use STW benefits are always included. The grey area marks the maximum period of two years during which treated establishments can collect STW benefits.

Figure 4 provides a descriptive analysis whether establishments whose STW application is approved dismiss less workers into unemployment than establishments whose STW application is rejected. The variable capturing the number of individuals that establishments dismiss into or hire from unemployment is the *net share of dismissed workers*<sup>23</sup>, which we plot in treated establishments and in the control group against event time  $\tau$ . Panel 4a shows that the share hovers around 1% per quarter in treated establishments in

<sup>22</sup>The reason the take-up rate is not exactly zero after 8 quarters is that some establishments interrupted their STW collection for some months and are therefore allowed to collect STW benefits even though  $\tau > 8$ .

<sup>23</sup>See section A in the Appendix for the definitions of this outcome variable.

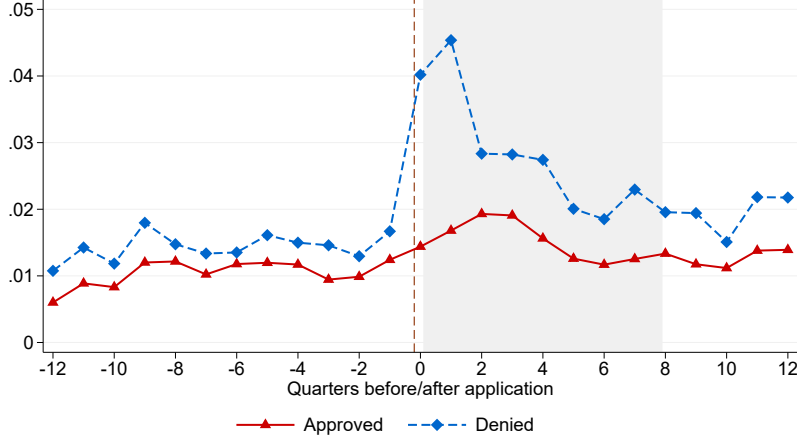


the pre-treatment period. In the quarters after the application, the share increases somewhat, reaching slightly less than 2% in the second and third quarter after application before declining again. Turning to the control group, we first note that the share evolves similarly to the treatment group before application. As Panel b of the figure shows, the small difference in the level of dismissals between the two groups can be explained by observable establishment characteristics. The evolution differs substantially in the treatment period, however: in the two quarters after the unsuccessful STW application, the net share more than triples in the control group. The share remains elevated—higher than in the pre-treatment period and higher than in treated establishments—in all the following quarters. Dismissals hence remain elevated in control establishments even after the maximum period of two years during which treated establishments can collect STW benefits, suggesting that dismissals are not just postponed but prevented. We obtain very similar results if we use the matched control sample (panel c) rather than all control establishments. Overall, Figure 4 provides strong evidence that denying the STW application forces establishments to dismiss workers during the subsequent three years, with a spike in dismissals immediately after application.

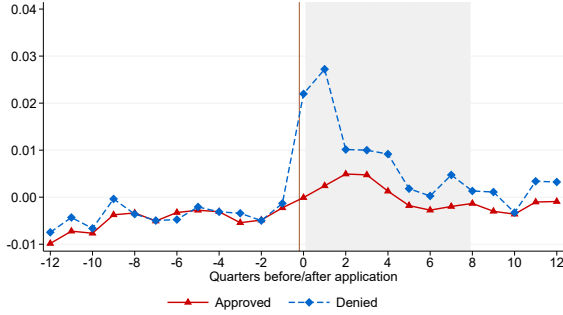
## 5.2 Effects on dismissals into unemployment

We now formally estimate the effect of STW approval on dismissals into unemployment. Figure 6a presents the sequence of event study coefficients,  $\beta_\tau$ , and associated 95% confidence intervals, from our baseline event study DiD model (equation 1) for the net share of dismissed workers. We start using all untreated establishments as the control group, but the results are very similar with the matched control group as shown in the next section. The regression suggests that STW approval has a strong and highly statistically significant negative impact on dismissals into unemployment. The overall effect of STW approval is the sum of these quarter-specific effects. Figure 6b presents this cumulative effect, and corresponding inference, for dismissals into unemployment (i.e. the share of dismissed workers), hiring from the pool of unemployed (the share of hires), and the sum of the two outcomes (the net share of dismissed workers). Columns 1–3 of Table 4 presents the original, first-differenced regressions for these outcomes. The figure shows that the cumulative DiD in the share of dismissed workers between treatment and control group is roughly 12% over the first twelve quarters after application. It also suggests that treated establishments hire slightly *less* workers from the pool of unemployed than the control group. The effects on hiring, are, however, very small. We thus find limited

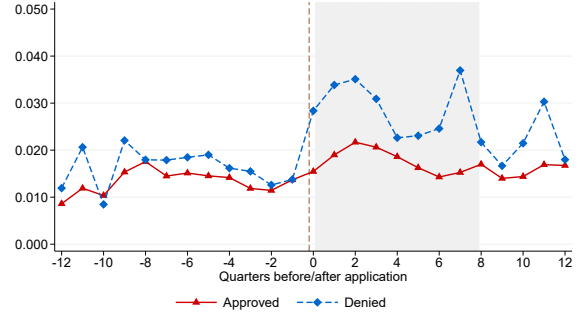
Figure 4: Dismissals around short-time work application by approval decision



(a) Net share of dismissed workers (all establishments)



(b) Adjusted net share of dismissed workers (all establishments)



(c) Net share of dismissed workers (matched control establishments)

*Notes:* The top panel displays the average net share of dismissed workers of establishments with approved and denied STW applications in the  $\pm 12$  quarters around STW application. The grey area marks the maximum period of two years during which treated establishments can collect STW benefits. We note a sizeable spike in dismissals in the two quarters following application for establishments whose STW application was rejected. Panel b repeats panel a, but shows averaged residuals of a regression of the net share of dismissed workers on establishment size and two-digit industry dummies (but no treatment dummy). The graph thus accounts for size and industry differences between treated and untreated establishments. It shows that the pre-treatment level differences visible in panel a are due to differences in establishment size and industry affiliation between treated and untreated establishments. In panel c, the control group contains only untreated establishments matched to treated establishments based on nearest-neighbor matching. Untreated establishments are weighted by the number of times that they are matched to a treated establishment. The sample is restricted to establishments that could be matched to the BC in this panel.

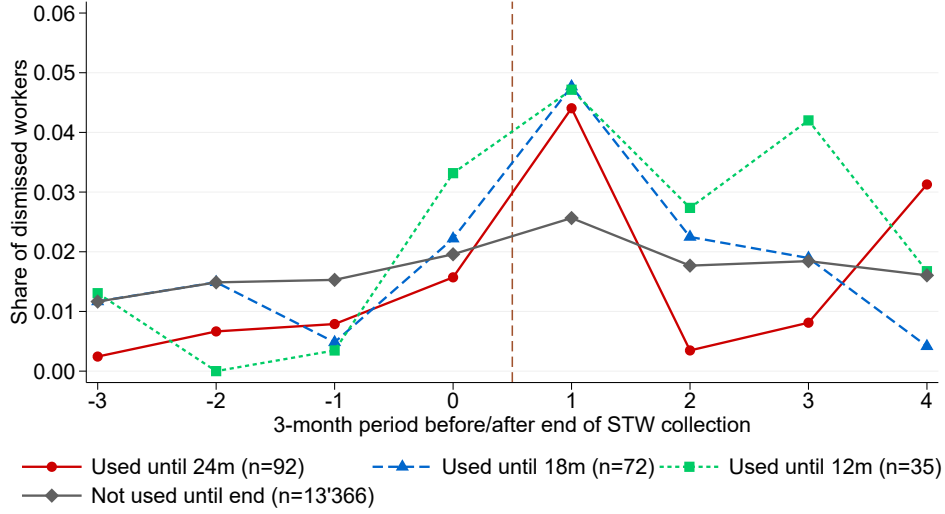
evidence that the Swiss STW program comes at the expense of unemployed outsiders whose entry into employment is made more difficult—a concern regarding STW schemes sometimes raised (e.g., Cahuc and Carcillo, 2011). The cumulative effect on the *net* share of dismissed workers is thus similar to the effect on dismissals and amounts to about a tenth of establishments’ workforce at application.

We can use the estimates of the first-year impact of STW on net dismissals (column 3 of Table 4) in order to approximate the deadweight losses associated with the Swiss STW scheme. Approximately 45% of the workers in treated establishments ever collect STW benefits in the first four quarters after application. In the same period, STW reduces net dismissals by about 5.8% of workers, or 12.9% for each worker actually covered by STW. This is only slightly below the 16.4% average share of hours covered by STW benefits during the same period (see Figure 3). These findings suggest that the scheme has limited deadweight effects and may indeed induce a close to one-to-one substitution from dismissals to hours worked. These results stand in contrast to micro-level studies that find small or negative effects of STW on dismissals and to the macroeconomic estimates reported by Boeri and Bruecker (2011), which suggest large deadweight losses, but are in line with the micro-level estimates for the German scheme provided by the same authors.

Similarly, Figure 6a suggests that the Swiss STW scheme has limited displacement effects: *all* post-treatment coefficients are negative and occasionally statistically significant, suggesting that dismissals are not just postponed to the end of the recession or the end of establishments’ collection of STW benefits. A direct alternative to show this is to zoom in on dismissals around benefit exhaustion, as is done in Figure 5. We observe only a very small increase in dismissals at benefit exhaustion in establishments that voluntarily stop collecting benefits. In contrast, establishments that collect STW benefits until the legal maximum (which is 12, 18 or 24 months, depending on establishment’s application date) indeed dismiss a sizeable share of their workforce in the three-month period directly following benefit exhaustion. Yet, the share of establishments that use STW benefits until exhaustion is less than 2 percent. Unlike the Italian STW scheme analyzed by Giupponi and Landais (2018), approval of STW in Switzerland thus seems to prevent dismissals into unemployment permanently.

In columns 4–6 of Table 4, we use the unemployment register to quantify the effect of STW on unemployment duration. Column 4 shows that STW prevents long-term unemployment. The outcome is the net share of dismissed workers if we only count dismissed workers that end up collecting unemployment benefits for at least 260 working days (i.e. one year). Taking the estimates at face value, they imply that the 7,880

Figure 5: Share of dismissed workers at short-time work benefit exhaustion



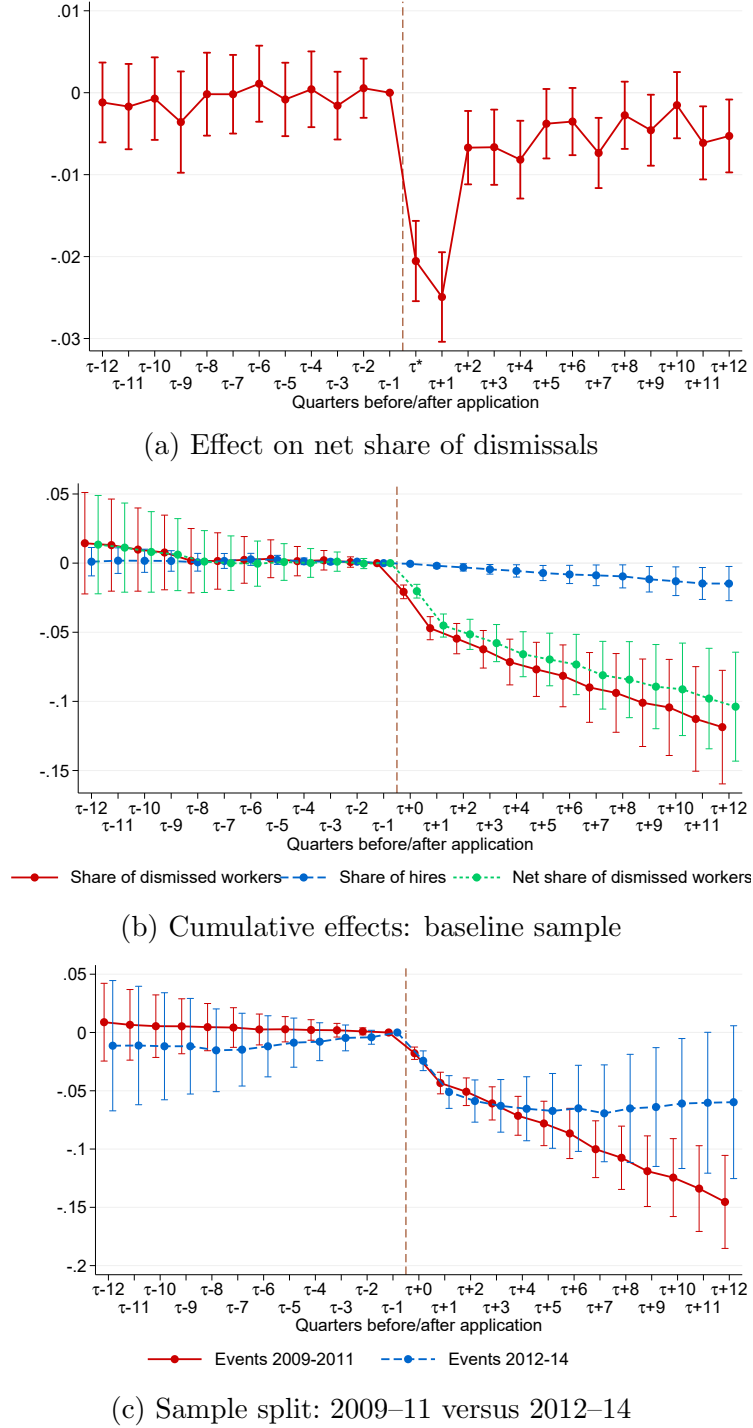
*Notes:* The figure depicts the share of dismissed workers in the 3-month periods before and after the end of STW collection. Period 1 starts immediately after the last month of benefit collection. The figure differentiates between establishments that did and establishments that did not collect STW until the legal maximum duration. Depending on the time of application, the maximal benefit duration is 12, 18, or 24 months, respectively. The legend reports the number of cases per group.

establishments that were treated in 2009 would have dismissed 8,275 workers that would have become long-term unemployed within the next three years, considering that the average establishment has a bit more than 25 employees at application ( $0,042 \cdot 25 \cdot 7,880$ ). In column 5, we directly estimate the effect of STW on the sum of daily unemployment allowances that the dismissed workers would have collected during their unemployment spell. We cumulate all future allowances of workers dismissed in a specific quarter and normalize this sum with establishments' employment at application. The DiD estimate is 25.8 days over the first 12 quarters after application (column 5 of Table 4), suggesting that granting STW saves roughly 645 daily allowances per case.<sup>24</sup> These estimates also imply that the 10.4% dismissed workers of treated establishments would have collected unemployment allowances for  $(25.8/0.104 =)$  250 working days on average (i.e. almost a year). This is remarkably longer than the average unemployment duration in Switzerland, which was half a year in 2009 (Cueni and Sheldon, 2013).

Overall, the results in this section provide compelling evidence that STW reduces the number of workers that are dismissed and end up as unemployed. In appendix section D, we study the heterogeneity of this effect for different establishments and workers. One important result from these analyses is that there are only small differences in the effects across broad industries: STW works in manufacturing but also in construction and the

<sup>24</sup>The result is very similar if we count all days between workers' registration and de-registration from the cantonal unemployment office (column 6 of Table 4). The difference in the size of the coefficients between columns 5 and 6 can be explained by the fact that allowances are only paid at working days.

Figure 6: Event study: effect of short-time work approval on unemployment



*Notes:* The top panel plots the sequence of event study coefficients,  $\beta_\tau$ , and associated 95% confidence intervals, from our baseline event study model (equation 1). The dependent variable is the net share of dismissed workers. We control for period, event time, and case fixed effects. Panel b plots the *cumulative* effect of STW approval, and corresponding inference, on the share of dismissed workers, the share of new hires and the net share of dismissed workers per quarter before and after application. We compute cumulative sums of  $\beta_\tau$  from period  $\tau = -1$  to period  $R$ ,  $E_R = \sum_{\tau=-1}^R \beta_\tau$ , both for positive and negative  $R$ . Panel c shows the cumulative effect of STW approval on the net share of dismissed workers separately for cases starting in the 2009–2011 period and cases starting in the 2012–2014 period. The former estimates exceptionally include data from the year 2008 to estimate pre-trends (even though the establishment identifier from the previous is generally missing in this year). Standard errors are clustered at the establishment level.

service sector. The largest differences are observed *within* the manufacturing sector: the effect on dismissals is much larger (more negative) in high-tech manufacturing compared to low-tech manufacturing, suggesting that STW preserves jobs in sectors with above-average productivity within manufacturing. We also find that the effects are larger for smaller establishments. Finally, estimating the effects separately by highest educational attainment of workers, we find that STW mainly saves the jobs of workers with compulsory and vocational education, suggesting that lower- and medium-educated workers are the main beneficiaries of the program.

Table 4: Effect of short-time work approval on different unemployment outcomes

	(1) dismissals	(2) hires	(3) net dismissals	(4) dismissals (long)	(5) allowances	(6) days registered
STW $\tau - 8$ to $\tau - 5$	-0.000 (0.009)	0.001 (0.003)	-0.001 (0.008)	-0.007 (0.005)	-1.617 (2.190)	-4.882 (3.638)
STW $\tau - 4$ to $\tau - 1$	-0.001 (0.006)	-0.001 (0.001)	-0.000 (0.006)	-0.003 (0.003)	-1.109 (1.571)	-1.428 (2.334)
STW $\tau$ to $\tau + 3$	-0.062*** (0.008)	-0.004** (0.002)	-0.058*** (0.008)	-0.022*** (0.004)	-12.204*** (2.021)	-20.089*** (3.461)
STW $\tau + 4$ to $\tau + 8$	-0.032*** (0.010)	-0.005 (0.003)	-0.026*** (0.010)	-0.012** (0.005)	-7.322*** (2.479)	-9.039** (3.729)
STW $\tau + 9$ to $\tau + 12$	-0.025*** (0.009)	-0.005* (0.003)	-0.020** (0.008)	-0.009** (0.004)	-6.299*** (2.088)	-9.361*** (3.218)
STW $\tau$ to $\tau + 12$	-0.119*** (0.025)	-0.015** (0.008)	-0.104*** (0.024)	-0.042*** (0.012)	-25.825*** (5.976)	-38.489*** (9.122)
<i>N</i>	387766	387766	387766	387766	387766	387766
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* The table presents event study estimates based on equation 1 of the effect of STW approval on unemployment. The estimation sample is the total sample of establishments applying for STW between 2009 and 2014 and the outcome covers the 2009–2015 period. Instead of presenting coefficients for each quarter separately, the table lists the sum of coefficients and corresponding standard errors for the indicated intervals. The dependent variables are the share of dismissed workers (column 1), the share of hires (column 2), the net share of dismissed workers (column 3), and the net share of dismissed workers that end up collecting unemployment benefits for at least 260 working days (column 4). The dependent variable in column 5 is sum of all (future) daily allowances (unemployment benefits) of all workers dismissed in the respective quarter, normalized by the number of workers at registration (total daily allowances per worker). The dependent variable in column 6 is the sum of (future) days registered as unemployed of all workers dismissed in the respective quarter, normalized by the number of workers at registration. See section A for information on these outcomes. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 5.3 Main robustness checks

This section presents the results of a series of important robustness and specification checks of the main results presented in the last section. Several further robustness checks—such as restricting the sample to small establishments or to the first application of establishment with several cases—are presented in Table A.8 in the appendix.

We start the discussion, however, by noting that Figure 6 provides support for the validity of our approach: there are no pre-treatment differences in the change of the net

Table 5: Effect of short-time work approval on net share of dismissed workers (matched control samples)

	(1) baseline	(2) atet	(3) atet fitted	(4) atet capped weight	(5) atut
STW $\tau - 8$ to $\tau - 5$	-0.001 (0.008)	0.013 (0.024)	0.016 (0.029)	-0.008 (0.013)	-0.014 (0.016)
STW $\tau - 4$ to $\tau - 1$	-0.000 (0.006)	-0.005 (0.009)	-0.002 (0.010)	-0.011 (0.008)	-0.013 (0.011)
STW $\tau$ to $\tau + 3$	-0.058*** (0.008)	-0.051*** (0.014)	-0.051*** (0.014)	-0.046*** (0.012)	-0.042*** (0.014)
STW $\tau + 4$ to $\tau + 8$	-0.026*** (0.010)	-0.033 (0.022)	-0.033 (0.022)	-0.028* (0.015)	-0.025 (0.017)
STW $\tau + 9$ to $\tau + 12$	-0.020** (0.008)	-0.027 (0.018)	-0.026 (0.017)	-0.019* (0.012)	-0.012 (0.015)
STW $\tau$ to $\tau + 12$	-0.104*** (0.024)	-0.110** (0.045)	-0.109** (0.044)	-0.093*** (0.034)	-0.079* (0.041)
$N$	387766	295877	248661	295877	87699
Period FE	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes

*Notes:* The table presents event study estimates based on equation 1 of the effect of STW approval on the net share of dismissed workers. The estimation period is 2009–2015. Instead of presenting coefficients for each quarter separately, the table lists the sum of coefficients and corresponding standard errors for the indicated intervals. Column 1 shows our baseline estimates using the total sample of establishments applying for STW between 2009 and 2014 (column 3 of Table 4). In column 2, we estimate the average treatment effect on the treated using a control group that contains only untreated establishments matched to treated establishments based on nearest-neighbor matching. The corresponding propensity score estimation is shown in column 1 of Table 3. In this regression, untreated establishments are weighted by the number of times that they are matched to a treated establishment. Column 3 uses the same sample and weighting scheme as column 2. However, observations are only retained if the matched unit from the other treatment group is also present in the respective event period  $\tau$ . For instance, if the treated unit is present in the interval  $\tau \in [-10, 10]$  and the matched untreated unit in the interval  $\tau \in [-5, 15]$ , both units are only retained in the interval  $\tau \in [-5, 10]$ . In column 4, the maximum weight of an untreated unit is winsorized at the 99th percentile of the weights in order to reduce the influence of the very few untreated units that receive a large weight in columns 2 and 3. Column 5 is similar to column 2, but this time we match treated units to untreated units and estimate the average treatment effect on the untreated. Treated establishments are weighted by the number of times that they are matched to an untreated establishment. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

share of dismissed workers between treatment and control group conditional on time, event time, and case fixed effects—a direct consequence of the fact that the share evolves in parallel in the pre-treatment period in the two groups (see Figure 4). The absence of significant pre-trends holds for all other unemployment outcomes, too, as shown in Table 4. However, a valid concern with the pre-trends of these regressions is that the estimation sample starts in 2009 only. We thus have limited possibilities to estimate pre-trends for the very early cases in the sample. Below, we use the employment data from the business censuses to show parallel employment trends and survival rates for establishments treated in 2009 and 2010. Moreover, Figure 6c shows that pre-trends are parallel if we estimate separate event studies for cases starting 2009–2011 and 2012–2014. In the latter sample, we observe the outcome in the 12 quarters before treatment for (almost) all cases, allowing us to estimate the trends over the entire pre-event period.

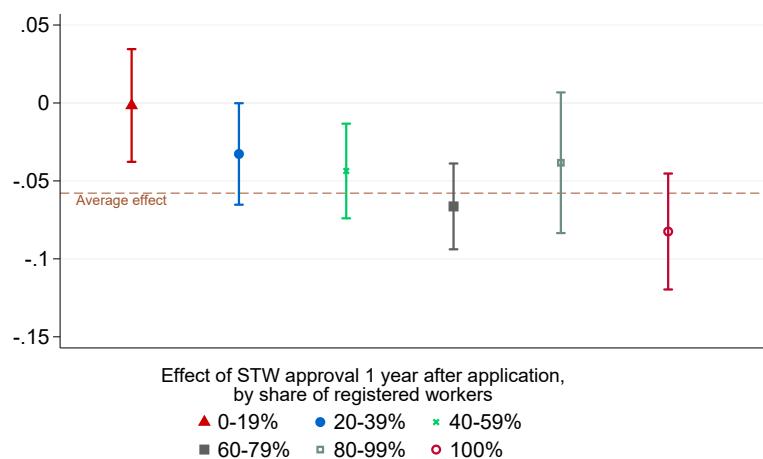
Despite the similarity in pre-trends by groups, the most important concern with our DiD approach remains that control and treatment group may differ in the underlying economic situation, explaining the disproportionate increase in dismissals that we observe in the control group. However, there are at least five further reasons that speak in favor of our baseline approach. The first is presented in the next section. The second is the timing of the effect. Dismissals increase abruptly and substantially after the decision to deny STW, strongly suggesting that the increase in dismissals is a result of the decision on the STW application.

Table 5 presents a third reason. It shows our event study regressions using the alternative control group matched based on nearest-neighbor matching, weighting untreated establishments by the number of times that they are matched to a treated establishment in the regressions. As established above, this control groups is very similar to treated establishments in terms of observed characteristics. Table 5 shows that both, the short- and the long-run effect with this alternative control group are close to the baseline results (repeated in column 1 for convenience). The remaining columns provide alternative specifications based on the matched control sample approach that confirm these results. Column 3 uses the same establishment sample and weighting scheme as column 2 but observations are only retained if the matched unit from the other group is also present in the respective event period  $\tau$ . In column 4, the maximum weight of an untreated unit is winsorized at the 99th percentile of the weights in order to reduce the influence of the very few untreated units that receive a large weight in columns 2 and 3. In column 5, we match one treated unit to each untreated unit. The column indicates that the treatment effect on the *untreated* may be slightly lower than the treatment effect on the treated.



A fourth reason in favor of our approach is that the results are similar if we condition the comparison between treatment and control establishments to establishments that expect a similar shortfall in labor demand at application. Figure 7 presents the cumulative effect of STW approval on the net share of dismissed workers one year after application depending on our proxy for this shortfall: the share of workers that establishments register for STW at application. The first coefficient in Figure 7 reveals that STW approval has no effect on dismissals among the few establishments that register less than 20% of workers. The estimated effect of STW becomes larger, and more statistically significant, the larger the share of workers registered for STW. These findings not only suggest that these establishments indeed face a larger drop in labor demand, but also that the effect of STW is largest where we expect it to be largest. Importantly, if we weight each coefficient in Figure 7 with the share of cases that it represents, we end up with an estimate of the effect of STW after one year that is very close to the baseline effect that pools all establishments (indicated in the figure with a dashed line). The figure also implies that our results would be similar if we disregarded seemingly healthy establishments (establishments that planned to cover a low share of workers) and seemingly unhealthy establishments.<sup>25</sup>

Figure 7: Effect of STW approval on dismissals depending on the share of workers that establishments register for STW at application



*Notes:* Each coefficient represents the cumulative effect of STW approval on the net share of dismissed workers one year after approval, derived from our baseline event study model estimated separately for establishments depending on the share of workers that they register for STW. The latter serves as an indicator of the shortfall in establishments' labor demand at application. All regressions control for period, event time, and case fixed effects. The dashed sienna line shows the estimate of the average effect of STW one year after approval based on all establishments.

<sup>25</sup>Not surprisingly given this evidence, our results are also very similar if we augment our baseline model with a full set of interactions between the period fixed effects and indicators for the six groups in terms of the share of registered workers. This saturated regression is identified only from the comparison of establishments that registered a similar share of workers (cf. columns 1 and 2 of Table 6).

A fifth reason in favor of our baseline approach is the striking robustness of our results to the inclusion of additional fixed effects that control for many possible confounders. Most importantly, our results are virtually unchanged if we add industry-period and canton-period fixed effects to our main specification (see columns 4 and 5 of Table 6 for the baseline and Table A.9 for the matched control sample). The former imply that we only compare establishments that operate in the same two-digit industry and the latter that we only compare establishments that applied at the same point in time within the same canton. The specification with canton-period effects also alleviates concerns that our results are biased due to compositional effects that could arise because establishments may be more willing to apply for STW in cantons in which the chances for approval are higher.<sup>26</sup>

## 5.4 Instrumental variable estimates

This section presents additional evidence on the effect of STW approval on dismissals into unemployment using an alternative identification strategy. Based on the findings from section 4.1 suggesting that cantonal employment agencies treat similar STW cases differently, we devise an instrumental variable for establishments' approval of STW that exploits the idiosyncrasies in the decisions of cantonal employment agencies to deny or approve STW. The aim is to isolate the exogenous cantonal variation in STW approval orthogonal to the economic situation of the applying establishments.

In order to estimate the IV regressions, we compute the cumulative net share of dismissed workers by subtracting hires from dismissals from period 0 until period  $k$ , denoted  $\sum_{\tau=0}^{\tau=k} u_{i,t}$ . We also estimate IV regressions for the *change* in dismissals around application, which allows us to combine the DiD and IV approach. In this case, we focus on establishments that are present in all periods within  $\pm 4$  quarters to the application for STW, and subtract the cumulative net share of dismissed worker in the four quarters just before application from the post-treatment share. Formally, the dependent variable is  $\sum_{\tau=0}^{\tau=3} u_{i,t} - \sum_{\tau=-4}^{\tau=-1} u_{i,t}$ . We estimate the following regression model for both outcomes:

$$\mathbf{u}_{i,t} = \gamma_t + \beta STW_i^T + \gamma X_{i,t} + \epsilon_{i,t} \quad (2)$$

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<sup>26</sup>In particular, if high cantonal approval rates induce more establishments to apply for STW, cantons that generously handle STW applications may attract applications of a different sample of establishments compared to cantons that handle applications strictly. If, moreover, stricter cantons attract firms with a different time path in dismissals than laxer cantons, this selection could lead to compositional biases that are not accounted for by the case fixed effects.

Table 6: Robustness of main event study DiD results to addition of various fixed effects

	(1)	(2)	(3)	(4)	(5)
STW $\tau - 8$ to $\tau - 5$	-0.001 (0.008)	-0.001 (0.008)	-0.000 (0.008)	-0.002 (0.008)	-0.003 (0.008)
STW $\tau - 4$ to $\tau - 1$	-0.000 (0.006)	-0.000 (0.006)	-0.001 (0.006)	-0.002 (0.006)	-0.002 (0.006)
STW $\tau$ to $\tau + 3$	-0.058*** (0.008)	-0.058*** (0.008)	-0.059*** (0.008)	-0.059*** (0.008)	-0.059*** (0.008)
STW $\tau + 4$ to $\tau + 8$	-0.026*** (0.010)	-0.026*** (0.010)	-0.026*** (0.010)	-0.025** (0.010)	-0.025** (0.010)
STW $\tau + 9$ to $\tau + 12$	-0.020** (0.008)	-0.019** (0.008)	-0.018** (0.008)	-0.021** (0.008)	-0.019** (0.008)
STW $\tau$ to $\tau + 12$	-0.104*** (0.024)	-0.104*** (0.024)	-0.102*** (0.024)	-0.105*** (0.024)	-0.103*** (0.024)
$N$	387766	387766	387817	387690	387690
Period FE	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	No	Yes	Yes
Share registered FE	No	Yes	No	No	No
Industry time FE	No	No	No	Yes	Yes
Canton time FE	No	No	No	No	Yes

*Notes:* The table presents event study estimates based on equation 1 of the effect of STW approval on the net share of dismissed workers. The estimation period is 2009–2015 and the sample contains all establishments applying for STW in the 2009–2014 period. Instead of presenting coefficients for each quarter separately, the table lists the sum of coefficients and corresponding standard errors for the indicated intervals. Industry-time FE are separate time effects for each NACE two-digit industry. The controls for “Share registered FE” are dummy variables controlling for interaction terms between a full set of period dummies and six indicator variables for the share of workers registered for STW at application, as reported by establishments in the STW application form. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

The variable of interest in these regressions is  $STW_i^\tau$ , an indicator variable that is one in case an establishment’s application for STW was approved. Our instrument for  $STW_i^\tau$  is the *cantonal approval rate for all STW applications outside of the establishment’s own industry in the two quarters prior to the specific application*. To be a valid instrument, the lagged approval rate needs to be unrelated to establishments’ dismissals (or the change in dismissals at  $\tau = 0$ ), apart from its direct effect on approval of STW. The main advantage of this instrument is that the lagged cantonal approval rate is likely to be unrelated to the underlying economic situation of an individual establishment at application. After all, establishments’ own situation, and the decision on its own case, have no effect on the instrument. As such, the instrument overcomes the main concern regarding the event study results in the last section: that the cantonal application decision is, for some unobserved reason, related to the establishment’s counterfactual increase in dismissals. The main concern with this instrument is that cantonal approval rates may be correlated to unobserved (persistent) region- or industry-specific shocks which in turn have an impact on (changes in) dismissals. We aim at limiting these concerns by excluding approval rates in the establishment’s own industry, by lagging the approval rate by one quarter<sup>27</sup>, by including industry-period fixed effects, and by adding all control variables that affect the probability of STW approval according to column 1 in Table 3 (among others the cantonal unemployment rate).

Table 7 presents the results of the IV regressions. Panel A of the table shows that the instrument strongly predicts whether an establishment’s STW application is approved. If we exploit cross-cantonal variation (column 2), an increase in the lagged cantonal approval rate in other industries by 10 percentage points increases an establishments’ approval probability by about 4.3 percentage points. The instrument also works if we focus solely on changes in approval practices within the same canton by adding canton fixed effects (column 4), but the first stage is weaker and less precisely estimated.

The IV estimates, presented in panel B of Table 7 for different specifications and different  $k$ , confirm that STW approval reduces the net share of dismissed workers (columns 2–6) and the sum of daily unemployment allowances (column 7). The estimate in the second column, using the level outcome and  $k = 3$ , indicates that the approval of STW decreases the net share of dismissed workers in the first year after application by  $-5.6\%$ . This estimate is identical to the corresponding DiD estimate based on equation 2 and the

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<sup>27</sup>The exclusion of the establishment’s own industry in the calculation of the share ensures that the instrument is unrelated to common (and potentially persistent) shocks to establishments within the same industry in a given canton. We also ensure that the instrument is unrelated to unobserved regional shocks in period  $\tau = 0$  by lagging the approval rate by one quarter.

first-differenced version of the outcome, shown in column 1 of the table. The IV estimate is  $-12.6\%$  if we use the first-differenced outcome (column 3),  $-9.7\%$  if we use the first-differenced outcome and add controls (column 4), and a staggering but imprecise  $-38.4\%$  if we use the first-differenced outcome and solely exploit time variation in approval practices within the same canton (column 5). Obviously, the IV estimates are less precise and much more dependent on the exact specification than those from our preferred DiD strategy, particularly so if we focus on within-canton variation only. Importantly, however, the IV estimates are not statistically significantly different from the corresponding baseline estimate. If anything, the IV point estimates are *more* negative than the corresponding DiD estimates. This holds, too, if we focus on the cumulative effects three year after treatment (columns 6 and 7 of Table 7).

Overall, the IV results confirm that STW reduces dismissals into unemployment. Moreover, they indicate that our baseline DiD results may *underestimate* the effect of STW approval on dismissals. This could be because cantons often deny STW to establishments that they deem “healthy”, or because the IV regressions identify another treatment effect: they focus on cases in which the instrument affects approval. These are probably the ambiguous cases—cases that are approved in one but would be denied in another canton. The effect of STW approval could be particularly large in these cases.

## 5.5 Effects on establishment survival and FTE employment

In this section, we explore how approval of STW affects establishment survival and establishment growth in terms of FTE employment using data from the Swiss business censuses. Recall that the censuses were conducted in 2001, 2005, 2008, and annually since 2011. In the following, we thus focus on establishments applying for STW between the fourth quarter of 2008 and the end of 2010 (i.e. after the census in September 2008 but at least one year before the census in December 2011), and on the first STW case if an establishment has several cases. We simplify the DiD approach by estimating the following FE regression model for outcome  $u_{i,t}$  of establishment  $i$  in period  $t$ :

$$u_{i,t} = \gamma_t + \gamma_i + \gamma_\tau + \beta_1 STW_i * I[t > 2008q3] + \beta_2 STW_i * I[t < 2008q3] + \epsilon_{i,t} \quad (3)$$

The coefficient of interest is the interaction  $I[t > 2008q3]STW_i$ , which reveals the effect of an STW approval on the outcome in the post-treatment censuses. The interaction  $I[t < 2008q3] * STW_i$  provides a placebo test for possible differences in outcomes in the two pre-approval periods (2001 and 2005). We control for establishment  $\gamma_i$ , year  $\gamma_t$ , and

Table 7: IV estimates of the effect of STW approval on dismissals into unemployment

	(1) Net dismissals FD OLS $0 \geq \tau \leq 3$	(2) Net dismissals level IV $0 \geq \tau \leq 3$	(3) Net dismissals FD IV $0 \geq \tau \leq 3$	(4) Net dismissals FD IV $0 \geq \tau \leq 3$	(5) Net dismissals FD IV $0 \geq \tau \leq 3$	(6) Net dismissals level IV $0 \geq \tau \leq 12$	(7) Allow- ances level IV $0 \geq \tau \leq 12$
<b>Panel A: First stage</b>							
Lagged appr. rate (other ind.)		0.434*** (0.023)	0.553*** (0.039)	0.480*** (0.040)	0.164*** (0.056)	0.448*** (0.028)	0.448*** (0.028)
<b>Panel B: Second stage</b>							
STW approved	-0.056*** (0.010)	-0.056* (0.029)	-0.123*** (0.042)	-0.097* (0.053)	-0.387* (0.224)	-0.151** (0.065)	-36.553*** (12.789)
Observations	5,466	13,254	5,706	4,846	5,706	10,000	10,000
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	No	No	No	No	Yes	No	No
Controls	No	Yes	No	Yes	No	Yes	Yes
RMSE	0.217	0.165	0.199	0.185	0.227	0.317	61.70

Robust standard errors in parentheses

\*\*\* p&lt;0.01, \*\* p&lt;0.05, \* p&lt;0.1

*Notes:* The table shows IV estimations based on equation 2. The dependent variable is the cumulative net share of dismissed workers over the period indicated in the column header. In columns 2, 6 and 7, we use the cumulative share in the post-treatment period (i.e. in  $0 \geq \tau \leq k$ ). In columns 1, 3, 4, and 5 the dependent variable is the difference in the net share of dismissed workers between the first four quarters after ( $0 \geq \tau \leq 3$ ) and before ( $-4 \geq \tau \leq -1$ ) application. In these columns, the estimations are restricted to cases with non-missing outcome in all periods  $-4 \geq \tau \leq 3$ . In column 1, we report the OLS estimate from a DiD regression based on this equation 2. The other columns report IV regressions. Panel A reports the first stage. The dependent variable is a dummy equal to one if an establishment's STW application was approved. The instrument is the cantonal approval rate in all other two-digit industries in the two quarters preceding application. Panel B reports the second stage. Controls are the variables in column 1 of Table 3. Standard errors are clustered at the establishment level. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.

event time fixed effects  $\gamma_\tau$ . The latter ensure that we only compare establishments that are treated in the same quarter.

Panel A of Table 8 studies the effect of STW approval on establishment survival. The outcome variable is a dummy equal to one if the establishment exists in a given census. If we use all untreated establishments as control group (column 1), we estimate a sizeable positive effect of STW approval on establishment survival. However, these estimates are misleading, as illustrated by the highly-significant positive pre-treatment coefficient (i. e.  $I[t < 2008q3] * STW_i$ ). The explanation is simple: treated establishments are larger and slightly older than the control establishments. Consequently, treated establishments have a higher survival rate in *every* period, including the pre-treatment periods. A more adequate comparison is thus the one between treated establishments and the matched control group, as these are comparable in terms of establishment size and age. Indeed, as shown in columns 2–5 of the table, there is no evidence that survival rates differ in the pre-treatment period if we compare these two groups—a finding that is corroborated if we estimate an effect for each census year separately before and after treatment using a

flexible event study version of equation 3 (see Figure A.4 in the appendix).

The DiD estimates based on the matched control group suggest that STW approval increases establishment survival between 5.8 (in the least demanding specification, column 2) to 9 percentage points (in the most demanding specification, column 4). These effects are substantial in relative terms given the baseline (pre-treatment) survival rate of 86.8%, and substantially larger than those found for the French scheme by Cahuc et al. (2018). They find that STW reduces death rates of firms by 9 percentage point in the 20% of firms with the lowest predicted revenue growth, but has no effect on the other 80% of firms. We find corroborative evidence for a positive effect of STW on establishment survival using the Job statistics (see section F in the appendix).

The fact that STW has a large positive impact on establishment survival implies that our employment regressions face a first-order sample selection problem. If we focused on surviving establishments, we would likely underestimate the effect of approval, as establishments with negative employment dynamics drop out of the control group while STW approval keeps them in the treatment group. Another empirical challenge is that there is a sizeable number of micro-establishments in the dataset, rendering employment regression based on a log-transformed outcome unattractive.<sup>28</sup>

We address these challenges by computing *symmetric* growth rates of employment, as is customary in studies based on establishment-level microdata (e.g., Davis et al., 1996; Chodorow-Reich, 2014). We focus on establishment existing in 2008 and compute symmetric growth rates of full-time equivalent (FTE) employment relative to 2008 in each period  $t$ :  $FTE_{it} - FTE_{i,2008} / (0.5 * FTE_{i,2008} + 0.5 * FTE_{it})$ . In the case an establishment does not exist in period  $t$ , employment is set to zero ( $FTE_{i,t} = 0$ ), implying that our panel is fully balanced and that the growth rate is bounded between  $-2$  and  $2$ . Defining the outcome in this way has the important advantages that it accommodates establishment entry and exit for all periods and that it limits the influence of outliers. Since this outcome is first-differenced, we estimate equation 3 without case fixed effects.

Columns 2–5 of Table 8 show that STW approval increased employment growth relative to 2008 in the post-approval period. Since symmetric growth rates represents a second-order approximation of the log difference growth rate around 0, the estimated coefficients suggest an increase in FTE employment by about 10–16 percent depending

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<sup>28</sup>The problem becomes readily apparent if we consider the extreme example of a firm that has one worker. If the firm grows, its change in the log-transformed outcome will be a large positive number. On the other hand, it cannot shrink unless it goes out of business. The consequence is a mechanical negative correlation between the initial size and the subsequent growth for establishments that initially have very few workers (see Mata, 1994).

on the specification. In section F in the appendix, we corroborate this result with similar evidence using data from the quarterly Job statistics. Moreover, the magnitude of the effect on FTE employment is quantitatively in line with the 10% effect on dismissals into unemployment estimated above. The estimates also imply that the Swiss STW scheme saved 0.2–0.3 full-time jobs for every worker in the program. These estimates lie in-between the 0.17 and 0.35 jobs saved per worker on STW that Cahuc et al. (2018) and Boeri and Bruecker (2011) report for the French and German STW scheme, respectively.

## 5.6 Cost-benefit analysis

In this section, we use our results to carry out a cost-benefit analysis for the Swiss STW scheme. We focus on the *financial costs and benefits* for the unemployment insurance by comparing the direct financial benefits of STW—arising from the effect of STW on unemployment benefit payments shown above—to the direct financial costs of STW, i.e. the sum of STW benefits paid out to the workers covered by the program. As we discuss below, this cost-benefit analysis is partial and likely *underestimates* the fiscal benefits of STW because it ignores important potential benefits. However, it provides, for the first time, an estimate of the extent to which spending on STW benefits is compensated by savings in terms of spending on unemployment benefits.

Table 9 presents the cost-benefit analysis for the year 2009. The top panel shows the direct financial benefits of STW. According to our OLS estimates, approval of STW leads to a decrease in 26 daily allowances per employee and case within three years after application (Table 4, column 5). The IV estimates suggest a decrease by 36.5 daily allowances per employee and case (see Table 7, column 7). For the following calculations, we use both estimates to bound the effect of interest. Multiplying the estimated decrease in the number of daily allowances per employee by the average amount of a daily allowance<sup>29</sup> and the average number of employees per establishment, we get the gross financial benefit per STW case. Since there are 7882 cases that started in 2009, the gross financial benefits of the program amount to CHF 856 Mio (DiD estimate) or CHF 1'580 Mio (IV) in 2009.

These benefits can be compared to the total spending on STW per case. According to

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<sup>29</sup>The STW dataset contains information on STW benefits, missed hours due to STW and normal working hours of an establishment. Hence, we can estimate the average insured income of a worker in our sample in the year 2009 and then calculate the average daily allowance that a short-time worker would receive in case of unemployment. The value we get is CHF 167, slightly above the daily allowance for the average unemployed in 2009 (SECO, 2013). The difference is to be expected because workers covered by STW are much more likely to be male and work full-time than the average unemployed.



Table 8: Effect of STW approval on establishment survival and growth

VARIABLES	(1) all establ.	(2) matched establ. atet	(3) matched establ. atet	(4) matched establ. atet	(5) matched establ. only 1 case
<b>Panel A: Establishment survival</b>					
$I[t < 2008q3]*STW$ approved	0.123*** (0.013)	-0.032 (0.056)	-0.040 (0.050)	-0.014 (0.039)	-0.008 (0.037)
$I[t > 2008q3]*STW$ approved	0.054*** (0.014)	0.058** (0.023)	0.074*** (0.021)	0.090*** (0.020)	0.078*** (0.023)
Observations	78,792	74,552	74,512	74,512	52,240
<b>Panel B: Establishment growth</b>					
$I[t < 2008q3]*STW$ approved	0.271*** (0.029)	0.008 (0.051)	0.044 (0.057)	0.076 (0.057)	0.065 (0.063)
$I[t > 2008q3]*STW$ approved	0.186*** (0.028)	0.099* (0.059)	0.132** (0.057)	0.159*** (0.054)	0.150** (0.059)
Observations	75,000	73,536	73,496	73,496	51,440
Period FE	Yes	Yes	Yes	Yes	Yes
Event-period FE	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes
Industry-period FE	No	No	Yes	Yes	Yes
Canton-period FE	No	No	No	Yes	Yes

*Notes:* The table presents estimates of the effect of STW approval on establishment survival (panel A) and establishment growth (panel B). The estimation sample covers the business censuses 2001q3, 2005q3, 2008q3, and 2011q4–2015q4. We focus on the first STW case if an establishment has several cases (and drop establishments with several cases in column 5). We focus on firms applying for STW between 2008q4 and 2010q4. Therefore, the interaction  $I[t > 2008q3]*STW$  approved reveals the effect of an STW approval on establishments' outcomes in the post-treatment censuses. The interaction  $I[t < 2008q3]*STW$  approved tests for possible differences in outcomes in the two pre-approval periods (placebo test). Column 1 is based on a DiD between treated and all untreated establishments. In columns 2–5, we estimate the average treatment effect on the treated (atet) using a control group that contains only untreated establishments matched to treated establishments based on nearest-neighbor matching. In these columns, untreated establishments are weighted by the number of times that they are matched to a treated establishment. The dependent variable in panel A is a dummy equal to one if the establishment exists in a given census. The dependent variable in panel B is establishment growth in full-time equivalents (FTE) relative to 2008q3. Growth rates are computed as symmetric growth rates:  $(FTE_{it} - FTE_{i,2008}) / (0.5 * FTE_{i,2008} + 0.5 * FTE_{it})$ . To account for establishment entry and exit, non-existent establishments are treated as zeros. Both panels control for establishment fixed effects (we estimate FE regressions in panel A and use a first-differenced outcome in panel B). Event-period fixed effects are individual dummies for each  $\tau$ . Industry-time FE are separate time effects for each NACE two-digit industry. Standard errors are clustered at the establishment level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

our data, the unemployment insurance spend 1'256 Mio CHF on STW benefits on cases that started in 2009.<sup>30</sup> The net benefit (or cost) of the scheme is simply the difference between the estimated costs and benefits. We find that the net financial benefit lies somewhere between CHF -400 Mio (DiD) and CHF -55 Mio (IV). Hence, the reduction in unemployment benefit payments may compensate two thirds of the spending on STW benefits or it may almost compensate the spending entirely.

These calculations likely underestimate the cost-effectiveness of the Swiss STW scheme because they disregard several potentially important benefits of STW *outside* of the unemployment insurance system. These include the direct cost savings in other social programs (e.g., social welfare) as well as the potentially large fiscal and societal benefits of avoiding the psychological costs and social costs of and the human capital losses caused by unemployment (to the extent that they do not arise for a short-time workers). Our cost-benefit analysis also disregard (i) that STW may act as an automatic stabilizer and thus prevent sharper recessions (Balleer et al., 2016); and (ii) that STW is likely to be more *equitable*. If firms resort to layoffs rather than reducing hours of work per worker, the costs of recessions are concentrated on a small number of workers who suffer large losses in income and other job-related benefits (Abraham and Houseman, 1994; Cahuc and Carcillo, 2011).

Importantly our cost-benefit analysis also disregards one important potential indirect cost of STW programs. STW may slow down the structural change from unproductive to productive sectors by preventing the destruction of unprofitable economic structures, by binding capital in unproductive establishments and by hindering efficiency-enhancing labor mobility. Giupponi and Landais (2018) provide evidence that such negative re-allocation effects of STW programs on untreated firms exist and that they can depress productivity growth in regional labor markets.

## 6 Conclusions

This study investigates whether the Swiss STW program prevented unemployment during and in the aftermath of the Great Recession. Our analysis exploit that establishments in Switzerland have to apply for STW at cantonal employment agencies. Consequently, many establishments that aim at introducing STW are not able to do so, giving rise

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<sup>30</sup>This amount is slightly higher than the 1.1 Billion CHF that the unemployment insurance spent in 2009 as reported by SECO (2013). The reason is that the latter refers to all payments for STW benefits in 2009 whereas the former refers to payments made for all cases *starting* in 2009.

Table 9: Cost benefit analysis of the Swiss short-time work scheme in 2009 (in CHF)

<b>Financial benefits</b>	<b>DiD</b>	<b>IV</b>
Decrease in daily allowances per employee	26	36.5
Daily allowance per employee when unemployed	167	167
Average number of employees per establishment	25	25
Cost savings per case	108'850	152'400
Total cost savings in 2009	856 Mio	1'201 Mio
<b>Financial costs</b>		
Costs per case	159'300	159'300
Total costs in 2009	1'256 Mio	1'256 Mio
<b>Net financial benefits of STW</b>		
Net financial benefits per case	-50'750	6'900
Net financial benefits in 2009	<b>-400 Mio</b>	<b>-55 Mio</b>

*Notes:* This table compares financial costs and benefits of the Swiss STW program for the unemployment insurance. The estimated decrease in the number of daily allowances per employee and the costs of STW are taken from the baseline DiD event study (column 5 of Table 4) and IV regressions (column 7 of Table 7). The reduction in UI benefits per case are calculated by multiplying the estimated decrease in daily allowances per employee by the cost of a daily allowance and the average number of employees per case. Multiplying this value by number of cases that started in 2009 (7882), we get the gross financial benefit of STW in 2009. The financial costs of STW are calculated from data on STW benefit payments to cases starting in 2009.

to a natural control group for establishments that are allowed to introduce STW. We merge administrative information from all STW applications for the years 2009–2014 with the Swiss unemployment register and business censuses and track dismissals, hiring, survival and employment of establishments before and after their application for STW benefits. Using a flexible event study DiD model, we find robust and highly statistically significant evidence that STW prevents permanent layoffs into unemployment. We also show that the STW program mainly saves jobs of workers with compulsory and vocational education, and that many dismissed workers would have become long-term unemployed. We present similar results applying an Instrumental Variable (IV) approach that directly exploits the idiosyncrasies in cantonal approval decisions.

We then use our estimates to approximate the deadweight losses associated with the Swiss STW scheme. We find that the scheme has limited deadweight effects and may indeed induce a close to one-to-one substitution from a reduction in dismissals to a reduction in hours worked. Moreover, we find strong evidence that the scheme has limited displacement effects, as dismissals are prevented permanently and not just postponed to the end of establishments' collection of STW benefits. We also use our estimates to quantify the financial costs and benefits of the STW program for the Swiss unemployment

insurance. The calculations suggest that the direct fiscal benefits of the program—which arise in the form of a reduction in spending on unemployment benefits—, may be large enough to compensate the total fiscal spending on STW benefits.

Two important limitations of our analysis have to be kept in mind. The first concerns external validity. We assess the Swiss STW scheme during and in the aftermath of a recession that turned out to be V-shaped. The fast recovery may have favored the effectiveness of the Swiss STW scheme. Second, our establishment-level estimates do not take into account two possible general equilibrium effects: the possible contribution of STW to stabilizing aggregate demand and to depressing productivity growth by hindering the efficient reallocation of production factors. Studying these effects requires another research design than the one we propose in this paper. Moreover, these effects were arguably of second-order importance so far, as we did not even have convincing micro-level evidence whether STW program actually prevent permanent layoffs. Given the growing evidence that this is the case, answering whether the general equilibrium costs and benefits of STW programs arise—and how large they are—are important questions for future research.

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# Appendix

## A Data and Variables

Table A.1: Share of registrations for which the last employer is known

	Registrations	Employer known	Share
2006	2,080	103	5
2007	8,895	293	3
2008	46,991	1,866	4
2009	124,566	65,589	53
2010	105,535	88,418	84
2011	105,918	92,692	88
2012	112,509	100,674	89
2013	113,482	102,929	91
2014	112,762	104,350	93
2015	121,692	113,814	94
2016	87,305	81,540	93

*Notes:* The first column reports the number of registrations of job seekers at the unemployment insurance in the respective year. The second column reports the number of registrations for which the last employer is known and the third column shows the respective share in total registrations.

*Source:* SECO, Unemployment register

The following outcome variables are used to evaluate the Swiss short-time work scheme.

- **Share of dismissed workers/share of unemployed:** This outcome variable is based on the number of workers, previously employed at the establishment of interest, that register themselves as unemployed in period  $t$ . We consider someone to be unemployed if he or she draws unemployment benefits at least once during the period he or she is registered at the unemployment agency. We normalize this count by the establishment's employment at the time of STW application (i.e.  $\tau = 0$ ), as recorded in the application form. Although the share only captures dismissed workers if they claim unemployment benefits, we refer to this outcome as the “share of dismissed workers” or “share of unemployed” below for ease of exposition.<sup>31</sup>

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<sup>31</sup>405 observations (0.1% of all observations) exhibit a share of more than 100%. We set these values to 100% in order to avoid that unrealistic outliers bias our results. We proceed in the same vein with the share of hires and the share of job seekers.

- **Share of hires:** This outcome exploits that the UIR records the establishment identifier of the *new* employer for most unemployed that leave unemployment to start a job.<sup>32</sup> We measure the per-period count of new hires by an establishment from the pool of registered job seekers. The “share of new hires” is the relationship between an establishment’s count of hires from the pool of unemployed in period  $t$  and its employment at STW application.
- **Net share of dismissed workers/net share of unemployed:** Our main outcome of interest represents the difference between the share of dismissed workers and the share of new hires. It summarizes the net effect of approval of STW on the pool of registered unemployed.
- **Share of job seekers:** This share represents the number of workers, previously employed or still employed at the establishment of interest, that register themselves at an unemployment agency in period  $t$ , relative to the establishment’s employment at STW application. In contrast, the share of dismissed workers, we also count workers that register themselves at the unemployment agency but do not draw unemployment benefits. These job seekers are likely to be still employed at the establishment of interest.
- **Total daily allowances per worker:** In each quarter, this variable reflects the sum of all (subsequent) daily allowances that workers, dismissed in the respective quarter, collect during the subsequent unemployment spell. As with the other variables, we normalize this outcome with the establishments’ employment at  $\tau = 0$ .

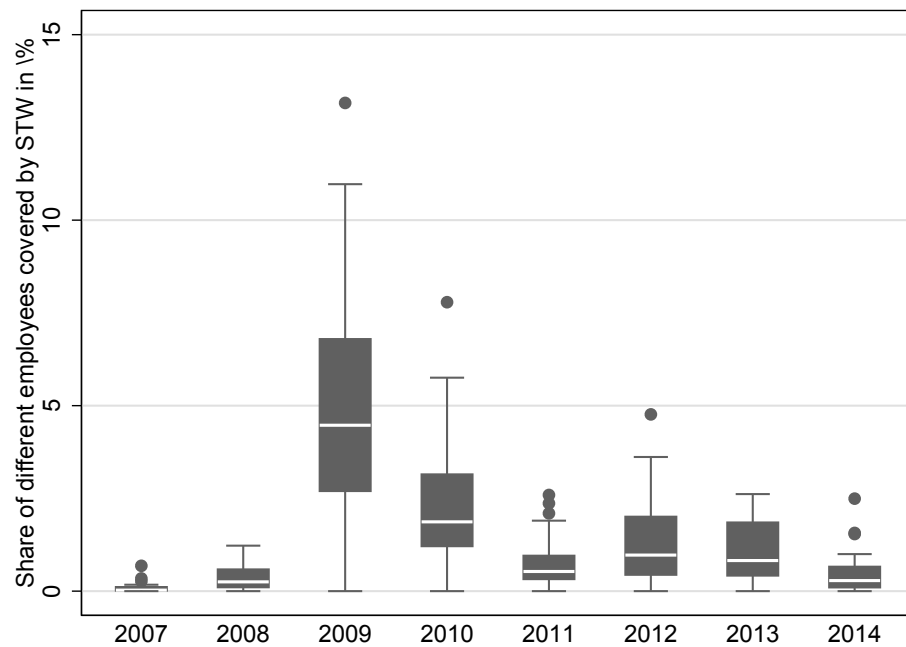
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<sup>32</sup>The share of successful job seekers with known new employer increases from 36% in 2008 to 65% in 2009 to 77% in 2010. In the years 2011 to 2016 it stays between 80 and 87%.



## B Descriptive Statistics

Figure A.1: Box-Whisker-Plot of the cantonal share of workers covered by STW (un-weighted)



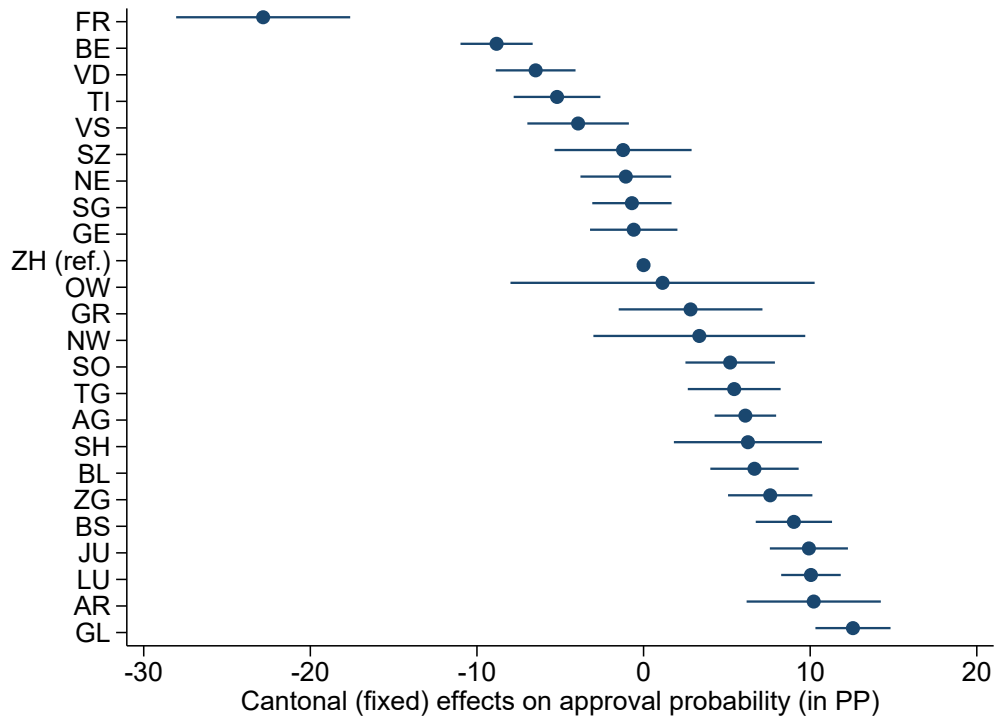
*Note:* This figure shows the distribution of the share of workers covered by STW across cantons by year. I.e. in 2009, the STW coverage rate per canton ranged between 0 and almost 14% with an average of slightly below 5%.

Table A.2: Share of employees covered by short-time work in NUTS-III-regions

	P5	Median	Mean	P95
2007	0.00	0.02	0.09	0.54
2008	0.00	0.15	0.52	2.32
2009	0.00	4.25	5.43	18.12
2010	0.00	1.64	2.48	7.79
2011	0.00	0.49	0.79	2.39
2012	0.00	0.75	1.26	4.14
2013	0.00	0.72	1.07	4.58
2014	0.00	0.27	0.43	1.61

*Notes:* The table reports the share of employees covered by STW benefits in total employment in a NUTS-III-region (similar to commuting zones). In Switzerland, there are 106 NUTS-III regions in total. *Source:* SECO; FSO

Figure A.2: Cantonal effects on approval probability controlling for establishment and labor market characteristics



*Note:* The figure reports average marginal effects of the different cantons on the probability that an establishment's request for short-time work is approved controlling for a whole set of establishment and labor market characteristics. We use the same set of covariates as in column (1) of table 3. Note, that the cantonal (fixed) effects are not identified for UR and AI since there are no denied cases in the sample period. ZH is the reference category.

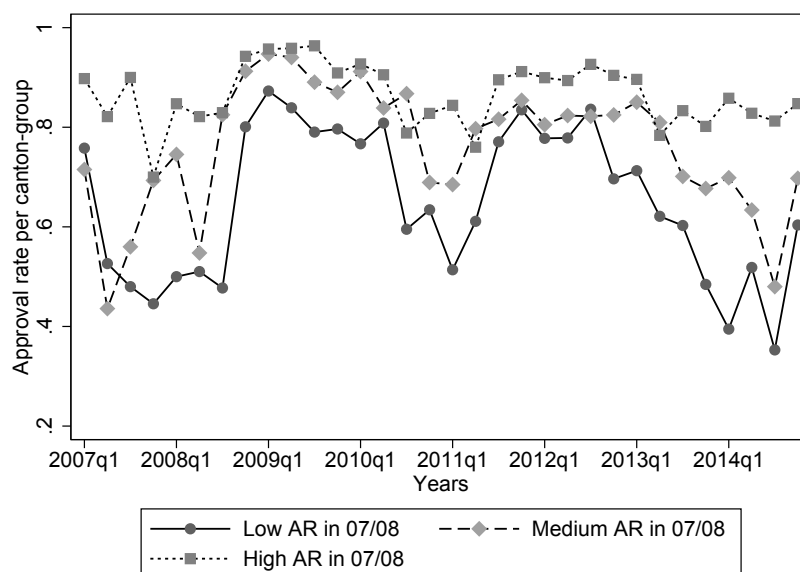
Table A.3: Number of approved and denied cases by canton

	No cases approved	No cases denied	Share approved
ZH	2,099	402	.84
BE	1,515	398	.79
LU	698	53	.93
UR	70		1
SZ	223	50	.82
OW	61	11	.85
NW	84	7	.92
GL	176	2	.99
ZG	358	38	.9
FR	167	135	.55
SO	550	37	.94
BS	271	26	.91
BL	471	36	.93
SH	161	10	.94
AR	111	3	.97
AI	44	1	.98
SG	1,066	157	.87
GR	149	29	.84
AG	1,188	121	.91
TG	541	41	.93
TI	717	281	.72
VD	740	376	.66
VS	376	153	.71
NE	720	116	.86
GE	519	175	.75
JU	490	20	.96
Total			.84

*Notes:* The table shows the number of approved and denied cases as well as the share of approved cases by canton in the time period 2009 to 2014.

*Source:* SECO, STW dataset

Figure A.3: Cantonal approval rates for short-time work applications, by canton groups



*Note:* Cantons are allocated to the three groups based on their average approval rate in the first two sample years (2007 and 2008). Cantons with low approval rates in 2007/2008 are: Fribourg, Basel-Stadt, Basel-Landschaft, Ticino, Vaud, Geneva, Bern. Cantons with medium approval rates in 2007/2008 are: Zürich, Zug, Solothurn, Aargau, Valais, Neuchatel. Cantons with high approval rates in 2007/2008 are: Luzern, Uri, Schwyz, Obwalden, Nidwalden, Glarus, Schaffhausen, Appenzell-Innerrhoden, Appenzell-Ausserrhoden, St. Gallen, Graubünden, Thurgau, Jura.

## C Application decision

To what extent do establishments that apply for STW benefits differ from those that do not? Table A.4 reports the results of probit regressions that are based on repeated cross-sections of establishments from manufacturing and trade in the years 2005 to 2010. Data-source is from the Swiss Job Statistics. The outcome variable is a dummy equal to one if an establishment applies for STW benefits during that period. Overall, 20% of all establishments applied for STW. The covariates in the probit regressions are establishment characteristics that are mostly taken from the Swiss Job Statistics.<sup>33</sup>

According to Table A.4 the propensity to apply for STW is positively associated with establishment size but negatively associated with the share of part-time workers. The latter result is in line with Boeri and Bruecker (2011) and might partly reflect that establishments relying heavily on part-time workers might have greater flexibility to adjust their workforce and working hours, which reduces the need to apply for STW benefits. Establishments reporting difficulties in recruiting workers have a slightly higher probability to apply for STW benefits than establishments without problems to find

<sup>33</sup>We have to restrict the sample to establishments operating in manufacturing and trade because our sample of the job statistics is restricted to these two sectors.

adequate personal. If recruitment of appropriate workers is difficult, the human capital of the existing workforce may be more relevant to the establishment. Hence, establishments with recruitment difficulties are less inclined to lay off workers during a crisis because the hiring of new ones during the recovery might be very costly. The analysis further suggests that time-invariant differences between cantons—such as the average approval rate on STW applications—play a small role in explaining the cross-sectional variation in the probability that an establishment applies for STW. The explanatory power of the model—reflected in the adjusted R-squared reported at the bottom of the table—hardly changes if we include canton fixed effects (cf. columns 2, 3 and 4).<sup>34</sup> By contrast, the two-digit industry fixed effects have substantial explanatory power. The propensity to apply is highest in the mechanical engineering, electrical and metal industries.

## D Heterogeneity of the effect of STW

In this section, we study whether the effects of the approval of STW on the net share of dismissals depend on certain establishment characteristics. We also study whether STW primarily prevents unemployment of high-, medium-, or low-qualified workers.

Table A.5 starts by answering the second question. The table provides our baseline event study estimates for the share of dismissed workers by highest educational attainment. The information on workers' educational credentials is recorded at registration at unemployment agencies. The table suggests that it is mainly workers with compulsory and vocational education whose jobs are saved because of STW. The estimated effects on the share of dismissed workers are quantitatively much lower for the other groups of workers.<sup>35</sup>

We now turn to the analysis whether the effect of STW on dismissals is larger for some groups of establishments. In order to provide a straightforward analysis of this question, we use a simplified regression model here. We first restrict the sample to establishments that are present in all periods within  $\pm 4$  quarters to the application for STW.<sup>36</sup> For these establishments, we then sum up all dismissals and all new hires in the four quarters before

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<sup>34</sup> Note, that there is a positive correlation between establishments take-up of STW benefits and the cantonal approval rate but it is not large. An increase in the cantonal approval rate of 10 percentage points is associated with a 0.8 percentage point higher application rate. Moreover, it is not clear whether a higher approval rate leads to a higher application rate, or vice versa.

<sup>35</sup> Note that we do not know the number of employed workers by educational attainment. We thus normalize each share by the total employment of an establishment at registration for STW. Part of the reason for the lower effect is thus that high-qualified workers represent a smaller share in the workforce of establishments in general.

<sup>36</sup> The results are similar if we do not impose this sample restriction here. But the restriction can be seen as a further robustness check for our results, as we do not use cases where we observe only a small number of pre- and/or post-treatment periods.

Table A.4: Average marginal effects on probability that establishment applied for short-time work in 2009 and 2010

	Model 1	Model 2	Model 3	Model 4
<i>Firmsize</i>				
0 to 4 employees	ref.	ref.	ref.	ref.
5 to 9 employees	.051***	.052***	.052***	.052***
10 to 19 employees	.095***	.091***	.091***	.091***
20 to 49 employees	.14***	.12***	.12***	.12***
50 to 99 employees	.16***	.13***	.12***	.12***
100 to 499 employees	.19***	.14***	.14***	.14***
>500 employees	.1***	.078***	.074***	.077***
<i>Other variables</i>				
Share of women	-.025***	.017*	.017*	.013
Share of part-time workers	-.039***	-.026**	-.028***	-.028***
Share of Cross-border commuters	.072***	.053***	.058***	.054***
2. Sector	ref.			
3. Sector	-.14***			
Difficulties in recruiting workers	.01**	.0074*	.007*	.007*
Cantonal approval rate 09/10			.082***	
Industry FE (NOGA2)	No	Yes	Yes	Yes
Canton FE	No	No	No	Yes
Observations	22581	22451	22451	22451
Adj_R2_McFadden	.23	.31	.31	.31

The Sample contains establishments from manufacturing and trade in 2005-2010

Source: SECO/BFS

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.5: Effect of STW approval on share of dismissed workers by highest educational attainment

	(1) compuls	(2) vocat educ	(3) upper sec	(4) prof educ	(5) univers educ
STW $\tau - 8$ to $\tau - 5$	-0.011* (0.006)	0.000 (0.006)	0.002 (0.002)	-0.001 (0.002)	0.002 (0.002)
STW $\tau - 4$ to $\tau - 1$	-0.001 (0.004)	-0.000 (0.005)	0.001 (0.002)	-0.003* (0.002)	-0.003*** (0.001)
STW $\tau$ to $\tau + 3$	-0.019*** (0.005)	-0.031*** (0.007)	-0.003 (0.002)	-0.003** (0.002)	-0.005*** (0.002)
STW $\tau + 4$ to $\tau + 8$	-0.018*** (0.007)	-0.013* (0.007)	0.000 (0.003)	-0.001 (0.002)	-0.002 (0.002)
STW $\tau + 9$ to $\tau + 12$	-0.018*** (0.006)	-0.013* (0.007)	0.002 (0.002)	-0.002 (0.002)	-0.001 (0.001)
STW $\tau$ to $\tau + 12$	-0.055*** (0.016)	-0.057*** (0.019)	-0.001 (0.007)	-0.006 (0.005)	-0.008** (0.004)
$N$	387766	387766	387766	387766	387766
Period FE	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes

*Notes:* The dependent variables are the share of dismissed workers by the respective highest educational attainment mentioned in the column header, normalized by the number of workers at registration. Base-line controls are period, event time, and case fixed effects. The table lists the sum of coefficients for indicated intervals. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

application (i.e. in  $-4 \geq \tau \leq -1$ ) and after application (i.e. in  $0 \geq \tau \leq 3$ ). Subtracting the latter from the former, we get the *increase* in the number of dismissed workers and the number of hired workers in the treatment period relative to the period just before treatment. Combining these two variables, we construct the before-after increase in the net share of dismissed workers, and then run a simple OLS regression of this outcome on a set of period fixed effects and an indicator variable that is one in case an establishment's application for STW was approved, i.e. we estimate

$$\sum_{\tau=0}^{\tau=3} u_{i,t} - \sum_{\tau=-4}^{\tau=-1} u_{i,t} = \gamma_t + \beta STW_i^\tau + \gamma X_{i,t} + \epsilon_{i,t} \quad (\text{DA.1})$$

The coefficient  $\beta$  is an estimate of the DiD between treated and control establishments, focusing on the (cumulative) effect in the first year after treatment relative to the year before.<sup>37</sup> In order to see whether the effects of STW approval are different for different establishments, we estimate this regression for different subsamples of establishments.

Tables A.6 and A.7 show the results of this exercise. Table A.6 presents the effects of STW approval for broad industry groups (manufacturing, construction, trade, and other service sector industries). We observe similar negative effects of STW in these industry groups. There are, however, noteworthy differences in the estimated effects between high-tech and low-tech manufacturing. The effects on high-tech manufacturers is much larger than the effect on low-tech manufacturers, where the effect is statistically insignificant and about four times smaller. High-tech manufacturing encompasses, among others, the manufacturing of chemical and pharmaceutical products, of computers, electronic or electrical equipment, and of machinery and transport equipment.

There are also noteworthy differences in the estimated effects depending on establishment size (Table A.7). The effects are much more sizeable for small establishments. There are no differences in dismissals in the following year between large establishments (with more than 50 employees) whose application is approved and large establishments whose application is denied.

Finally, in unreported regressions, we find that the effect of STW on the share of dismissed workers does not differ by year. Hence, the negative effect of STW benefits on unemployment is not restricted to the crisis-years but can be observed in the subsequent years 2011–2014, too.

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<sup>37</sup>We conducted a similar analysis focusing on the effect within the first two years after application. This yielded very similar results regarding the heterogeneity in the effects.



Table A.6: Effect of short-time work approval on net share of dismissed workers, by broad industry

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
VARIABLES	Manufac- turing	High-tech manuf.	Low-tech manuf.	Const- ruction	Trade	Other services
STW approved	-0.053*** (0.017)	-0.130*** (0.045)	-0.029 (0.018)	-0.052*** (0.016)	-0.071*** (0.022)	-0.057*** (0.015)
Observations	2,445	833	1,612	1,070	681	1,574
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Share approved	0.886	0.917	0.870	0.551	0.639	0.619

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The dependent variable is the difference in the net share of dismissed workers between the first year after (i.e. in  $0 \geq \tau \leq 3$ ) and before (i.e. in  $-4 \geq \tau \leq -1$ ) application. The estimations are restricted to cases with non-missing outcome in all periods  $-4 \geq \tau \leq 3$ . They are further restricted to the sample of establishments indicated in the column header. STW approved is a dummy variable indicating approval of an STW application. The only controls are period fixed effects. High-tech manufacturers (column 2) are establishments in NACE rev. 2 two-digit sections 20, 21, 26–30 (excluding three-digit industry 30.1), and three-digit industries 25.4 and 32.5, following the definition of Eurostat. Low-tech manufacturers (column 3) are establishments from all other manufacturing industries. The “share approved” shows the fraction of establishments in the respective subgroup whose STW application was approved. Standard errors are clustered at the establishment level. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.

Table A.7: Effect of short-time work approval on net share of dismissed workers, by establishment size

	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
VARIABLES	size <10	size 10-19	size 20-49	size 50+
STW approved	-0.072*** (0.011)	-0.030*** (0.010)	-0.019* (0.010)	0.001 (0.015)
Observations	3,353	1,104	780	551
Period FE	Yes	Yes	Yes	Yes
Share approved	0.642	0.785	0.832	0.922

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The dependent variable is the difference in the net share of dismissed workers between the first year after (i.e. in  $0 \geq \tau \leq 3$ ) and before (i.e. in  $-4 \geq \tau \leq -1$ ) application. The estimations are restricted to cases with non-missing outcome in all periods  $-4 \geq \tau \leq 3$ . They are further restricted to the establishment size groups indicated in the column header. STW approved is a dummy variable indicating approval of a STW application. The only controls are period fixed effects. The “share approved” shows the fraction of establishments in the respective subgroup whose STW application was approved. Standard errors are clustered at the establishment level. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.

## E Further robustness checks

Table A.8: Further robustness checks for effect of short-time work approval on net share of dismissed workers

	(1) no 2009	(2) p4tot9	(3) only small	(4) no return	(5) only first	(6) one department
STW $\tau - 8$ to $\tau - 5$	0.005 (0.009)	-0.001 (0.010)	-0.001 (0.009)	-0.004 (0.011)	0.004 (0.010)	0.002 (0.009)
STW $\tau - 4$ to $\tau - 1$	0.004 (0.007)	-0.000 (0.007)	0.001 (0.007)	-0.009 (0.008)	0.001 (0.008)	0.001 (0.007)
STW $\tau$ to $\tau + 3$	-0.057*** (0.010)	-0.054*** (0.010)	-0.059*** (0.009)	-0.081*** (0.011)	-0.055*** (0.010)	-0.057*** (0.008)
STW $\tau + 4$ to $\tau + 8$	-0.015 (0.012)	-0.024** (0.012)	-0.025** (0.011)	-0.025* (0.013)	-0.021* (0.012)	-0.022** (0.010)
STW $\tau + 9$ to $\tau + 12$	-0.006 (0.010)	-0.018* (0.009)	-0.019** (0.009)	-0.022** (0.011)	-0.017* (0.010)	-0.014* (0.008)
STW $\tau$ to $\tau + 12$	-0.079*** (0.029)	-0.096*** (0.028)	-0.103*** (0.026)	-0.128*** (0.031)	-0.092*** (0.028)	-0.093*** (0.024)
<i>N</i>	226756	314284	316305	239099	308429	346860
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	Yes	Yes	Yes	Yes

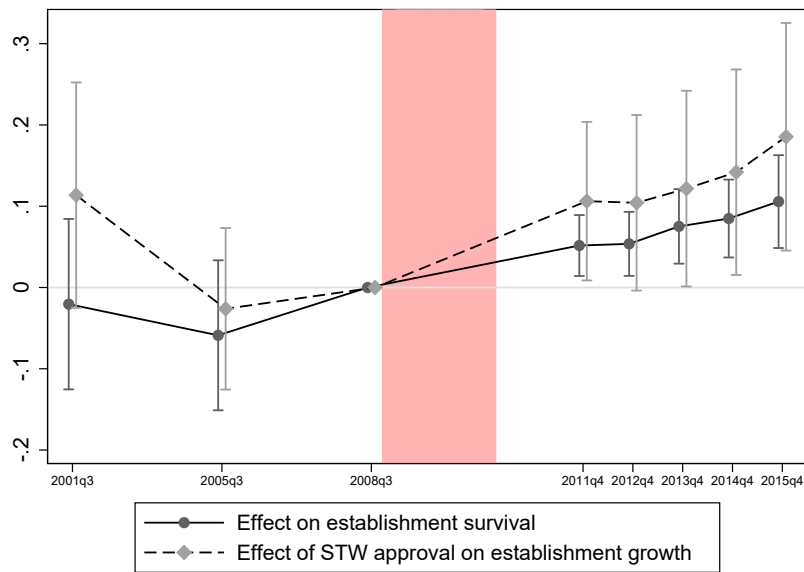
The table presents event study estimates based on equation 1 of the effect of STW approval on the net share of dismissed workers. The estimation period is 2009–2015. Baseline controls are period, event time, and case fixed effects. Instead of presenting coefficients for each quarter separately, the table lists the sum of coefficients and corresponding standard errors for the indicated intervals. Column 1 disregards STW application made in 2009. Column 2 is restricted to establishments observed over the entire period from  $t - 4$  to  $t + 9$ . Column 3 is restricted to establishments with at most 25 workers at registration. Column 4 disregards establishments that are observed to call back some of their workers. Column 5 is restricted to the first application of establishments. Column 6 disregards applications from establishments that applied for STW for several departments. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.9: Main robustness checks using matched control sample

	(1)	(2)	(3)	(4)	(5)
STW $\tau - 8$ to $\tau - 5$	0.013 (0.024)	0.016 (0.022)	-0.015 (0.012)	0.020 (0.017)	0.016 (0.016)
STW $\tau - 4$ to $\tau - 1$	-0.005 (0.009)	-0.004 (0.009)	-0.011 (0.008)	-0.001 (0.009)	-0.003 (0.009)
STW $\tau$ to $\tau + 3$	-0.051*** (0.014)	-0.049*** (0.013)	-0.060*** (0.016)	-0.044*** (0.011)	-0.042*** (0.012)
STW $\tau + 4$ to $\tau + 8$	-0.033 (0.022)	-0.030 (0.022)	-0.048** (0.022)	-0.020 (0.016)	-0.020 (0.016)
STW $\tau + 9$ to $\tau + 12$	-0.027 (0.018)	-0.024 (0.016)	-0.035** (0.018)	-0.022* (0.013)	-0.020 (0.013)
STW $\tau$ to $\tau + 12$	-0.110** (0.045)	-0.104** (0.043)	-0.144*** (0.046)	-0.086** (0.034)	-0.083** (0.036)
$N$	295877	295877	295877	295752	295752
Period FE	Yes	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes	Yes
Case FE	Yes	Yes	No	Yes	Yes
Share registered FE	No	Yes	No	No	No
Industry time FE	No	No	No	Yes	Yes
Canton time FE	No	No	No	No	Yes

*Notes:* The table presents event study estimates based on equation 1 of the effect of STW approval on the net share of dismissed workers. The estimation period is 2009–2015. Instead of presenting coefficients for each quarter separately, the table lists the sum of coefficients and corresponding standard errors for the indicated intervals. We estimate the average treatment effect on the treated using a control group that contains only untreated establishments matched to treated establishments based on nearest-neighbor matching. The corresponding propensity score estimation is shown in column 1 of Table 3. In all regressions, untreated establishments are weighted by the number of times that they are matched to a treated establishment. Industry-time FE are separate time effects for each NACE two-digit industry. The controls for “Share registered FE” are dummy variables controlling for interaction terms between a full set of period dummies and six indicator variables for the share of workers registered for STW at application, as reported by establishments in the STW application form. Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Figure A.4: Event study estimates of the effect of STW approval on establishment survival and growth



*Notes:* The figure shows event study estimates and associated 95% confidence interval of the effect of STW approval on establishment survival and establishment growth. The estimation sample covers the business censuses 2001q3, 2005q3, 2008q3, and 2011q4–2015q4. We focus on the first STW case if an establishment has several cases and on establishments applying for STW between 2008q4 and 2010q4 (i.e. the period highlighted by the shaded red area). We estimate the average treatment effect on the treated using a control group that contains only untreated establishments matched to treated establishments based on nearest-neighbor matching. Untreated establishments are weighted by the number of times that they are matched to a treated establishment. The dependent variable in the model “effect on establishment survival” is a dummy equal to one if the establishment exists in a given census. The dependent variable in the model “effect on establishment growth” is establishment growth in full-time equivalents (FTE) relative to 2008q3. Growth rates are computed as symmetric growth rates:  $(FTE_{it} - FTE_{i,2008}) / (0.5 * FTE_{i,2008} + 0.5 * FTE_{it})$ . To account for establishment entry and exit, non-existent establishments are treated as zeros. We control for case, (two-digit) industry-year, and event time fixed effects. Standard errors are clustered at the establishment level.

## F Effects on survival and employment in the Job Statistics

In this section, we explore how approval of STW affects establishments' FTE employment based on data from the Job Statistics. The Job Statistics is a quarterly survey of 18'000 secondary and tertiary sector firms (65'000 establishments) and contains information about the enterprise and the workforce (number of female/male employees, part- or full-time, etc.). Our sample covers the years 2005 to 2014 and all establishments from manufacturing and trade that participated in the survey. Note that the sample overlap between Job Statistics and the STW dataset is much smaller than the one between the UIR and the STW dataset. As shown in Table 1, there are 2'634 establishments in the Job Statistics whose applications for STW were approved between 2009 and 2014 but only 262 establishments whose applications were denied.

In the Job Statistics, establishments report quarterly figures on total and full-time equivalent (FTE) employment. The sampling of the Job Statistics is largely non-random. However, it is designed to produce official figures on quarterly employment for detailed industries by (NUTS-II) Swiss regions. The survey thus covers a sizeable share of employment in Switzerland.<sup>38</sup> Until 2011, the FSO collected the employment data at the establishment level with very few exceptions. From 2011 onward, the survey is generally collected at the firm level. This leads to a sizeable structural break in the employment series of multi-establishment firms in the middle of the estimation sample. We thus drop 298 cases where we observe a change in the collection unit from establishment to firm in 2011.<sup>39</sup> Despite the relatively large coverage of workers in the Job Statistics, the overlap between the STW dataset and the Job Statistics proved to be quite small: establishments that applied for STW participated in the Job Statistics only in one of four (4'034 of 16'243) cases. Moreover, the sample overlap is smaller regarding firms whose STW application was denied. The reason is that the Job Statistics samples only relatively few small firms, and small firms have a higher chance for denial. Our micro-level dataset from the Job Statistics is restricted to establishments in manufacturing and trade.

Due to the relatively small number of establishments whose application was denied, we do not estimate the demanding event study regression model presented in section 4.2. Rather, we estimate the following simplified variant of it:

$$u_{i,t} = \gamma_t + \gamma_\tau + \beta_1 STW_i + \beta_2 STW_i * I[\tau \geq 0] + \gamma X_{i,t} + \epsilon_{i,t} \quad (\text{FA.2})$$

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<sup>38</sup>In 2015, the survey encompassed roughly 18'000 firms with 65'000 establishments and over 2 million workers (more than one third of total employment in Switzerland).

<sup>39</sup>Our results do not depend on the exclusion of these cases, but they tend to be more precisely estimated.

Equation FA.2 represents a simple DiD regression model.  $STW_i$  is an indicator variable whether an establishment’s STW application was approved. The interaction term between the approval dummy and the post-application period,  $STW_i * I[\tau \geq 0]$ , is the coefficient of interest and represents the extent to which the outcome variable,  $u_{i,t}$ , changed differently between control and treatment group in the periods after application relative to the periods before. We present models that control and that do not control for case fixed effects ( $\delta_i$ ). If we control for case fixed effects, all time-invariant differences between firms—and hence also the variable  $STW_i$ —are absorbed from the regressions.

Using this regression model, we first study whether the approval of STW affects panel attrition. The outcome variable used in the table is one, and stays one, if a firm permanently drops from the sample of the Job Statistics. If an establishment does not answer to the survey but participates in at least one future survey, we do not consider it as a drop out but rather set the variable to zero. The estimation sample covers the 2005–2014 period and is restricted to at most 24 event time periods prior and posterior to the event (i.e.  $abs(\tau) \leq 24$ ) for each case.

Table A.10 reports the results. We find clear evidence that establishment’s whose STW application is approved have a lower chance of dropping out of the Job Statistics. The estimated treatment effects are large: the average drop-out rate in the estimation sample is 18%. Hence, the estimated effects suggest that STW approval reduces the drop out probability by 56%. Column 4—restricted to event time periods within at most one year around the application—shows that the impact of treatment on the probability to stay in the sample becomes apparent within a short period after the STW decision.

Unfortunately, our data do not allow us to distinguish whether a firm drops from the survey because it does not want to answer to the survey or whether it drops because it has to close down. However, establishments usually participate in the survey when asked to participate in it—the response rates to the latest Job Statistics were 81% in manufacturing and even 95% in trade. In the view of these high response rates, it appears likely that some of the excess drop-outs that we observe in the control group in the post-treatment period represent establishment closures, in line with the results presented in the main part of the paper (section 5.5).<sup>40</sup>

The fact that STW approval has a large negative impact on the probability to drop out from the Job Statistics implies that our employment regressions face a non-trivial sample selection problem, as discussed in the main text. If we focus on surviving firms,

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<sup>40</sup>The Job Statistics is a rotating survey. Hence, another reason to drop out of the sample is the sample rotation scheme. However, it appears very unlikely that the rotation scheme is systematically related to differential changes in drop out probabilities before and after application for treatment and control group.

Table A.10: DiD estimates of the effect of short-time work approval on the probability to drop out of the Job Statistics

	(1)	(2)	(3)	(4)
	OLS	FE	FE	OLS
VARIABLES	Dropout all $\tau$	Dropout all $\tau$	Dropout all $\tau$	Dropout $-4 \geq \tau < 4$
$I[\tau \geq 0]$ *STW approved	-0.104*** (0.035)	-0.084*** (0.029)	-0.084*** (0.029)	-0.043*** (0.016)
STW approved	-0.034* (0.020)			-0.087*** (0.029)
Observations	114,243	114,243	114,243	30,368
Period FE	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes
Industry-period FE	No	No	Yes	No
Case FE	No	Yes	Yes	No
Share approved	0.943	0.943	0.943	0.949
Number of cases		4,026	4,026	

Robust standard errors in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

*Notes:* The table shows DiD estimates of the effect of STW approval on the probability to drop out of the sample of the Job Statistics. The dependent variable is a dummy equal to one in all quarters after an establishment permanently leaves the sample of the Job Statistics. STW approved is a dummy equal to one if an establishment's application was approved, and  $I[\tau \geq 0]$  indicates post application periods. The estimation sample covers the period 2005–2014 and is restricted to at most 24 event time periods prior or posterior to the event (i.e.  $abs(\tau) \leq 24$ ) for each case. In column 4, the sample is restricted to event time periods within at most four quarters around the application ( $-4 \geq abs(\tau) < 4$ ). Standard errors are clustered at the establishment level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

we would likely underestimate the effect of approval. The reason is that firms with negative employment dynamics drop out of the control group while the approval of STW keeps them in the treatment group. Another empirical challenge for our employment regressions is that there is a sizeable number of micro-firms in the dataset.

In Table A.11, we address these estimation challenges in the following way. We deal with the problem caused by the presence of micro-firms by estimating linear probability models for a simple binary indicator whether an establishment's number of FTE workers exceeds a certain threshold. We address the possible selection bias due to non-random panel attrition by presenting two very similar regressions in Panel A and B of the table. Panel A shows regressions that focus on surviving firms (i.e. on the employment dummies of firms with non-missing employment data). Panel B shows the same regressions if we treat missing values in the outcome variables as zeros. In the presence of non-random attrition, the former delivers a lower bound to the true effect, while the latter is an upper bound for the true effect under the assumption that FTE employment of attriters stays



permanently below the respective employment threshold. The table presents estimates of equation FA.2 with and without case fixed effects.

In line with our main results, the employment regressions based on the Job Statistics suggest that the approval of STW increases FTE employment. The coefficients for the interaction term  $STW_i * I[\tau \geq 0]$  are generally positive and some statistically significant despite the small number of firms in the control group. The evidence for a positive impact of STW approval on FTE employment is largest for firms with around 10 FTE workers. The estimated effects are close to zero if we focus on the effects for larger establishments, consistent with our finding that the effect of STW on dismissals is smaller for larger establishments (see section D). Overall, the evidence presented in Tables A.10 and A.11 suggests that STW prevents firms from dismissing workers and/or from having to close down. The results from the Job Statistics thus corroborate our findings based on the business census data.

Table A.11: DiD estimates of the effect of short-time work approval on FTE employment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
	FTE	FTE	FTE	FTE	FTE	FTE	FTE	FTE
	$\geq 5$	$\geq 5$	$\geq 10$	$\geq 10$	$\geq 25$	$\geq 25$	$\geq 50$	$\geq 50$
<b>Panel A: Missing as missing</b>								
$I[\tau \geq 0]$ *STW approved	0.004 (0.047)	-0.011 (0.020)	0.112** (0.050)	0.038 (0.024)	0.068 (0.045)	0.013 (0.024)	0.015 (0.037)	-0.033* (0.018)
STW approved	0.165*** (0.043)		0.215*** (0.052)		0.297*** (0.052)		0.265*** (0.042)	
Observations	61,698	61,698	61,698	61,698	61,698	61,698	61,698	61,698
<b>Panel B: Missing as zeros</b>								
$I[\tau \geq 0]$ *STW approved	0.047 (0.046)	0.014 (0.038)	0.113*** (0.041)	0.074** (0.031)	0.069** (0.034)	0.017 (0.021)	0.023 (0.028)	-0.010 (0.016)
STW approved	0.180*** (0.043)		0.210*** (0.047)		0.258*** (0.043)		0.221*** (0.034)	
Observations	77,848	77,848	77,848	77,848	77,848	77,848	77,848	77,848
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case FE	No	Yes	No	Yes	No	Yes	No	Yes
Share approved	0.959	0.959	0.959	0.959	0.959	0.959	0.959	0.959

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The table shows DiD estimates, with and without case FE, examining the effect of STW approval on dummy variables equal to one if an establishment's FTE employment in a given quarter exceeds the threshold shown in the column header. In Panel A, missing employment data is treated as missing. In Panel B, we first linearly interpolate the employment data if an establishment has missing employment data for some periods but non-missing data in later periods. The dummy variables are then build using the interpolated data, treating missing values as zeros. "STW approved" is a dummy equal to one if an establishment's application was approved, and  $I[\tau \geq 0]$  indicates post application periods. The estimation sample covers the period 2005–2014 and is restricted to (i) firms that have non-missing employment data in the period before application ( $\tau = -1$ ) and (ii) to at most 24 event time periods prior or posterior to the event (i.e.  $abs(\tau) \leq 24$ ) for each case. Standard errors are clustered at the establishment level. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.