Does Short-Time Work Prevent Unemployment?

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Abstract

We study whether the Swiss short-time work (STW) scheme reduced layoffs during and in the aftermath of the Great Recession. While the few previous firm-level studies compare firms that used and did not use STW, we focus on firms that applied for STW at cantonal employment agencies, responsible for approving STW in Switzerland. Based on a quarterly establishment-level panel dataset linking administrative datasources, we compare the evolution of layoffs, hiring, and employment before and after application for STW between establishments that applied successfully and those that did not, exploiting the substantial differences in approval practices across cantons. We find strong evidence that STW prevents rather than postpones layoffs, especially in the quarters immediately following the application. These effects are concentrated among low- and middle-qualified workers. The estimates suggest that the savings in terms of unemployment benefit payments may be large enough to fully compensate the spending on STW benefits in the Swiss case.

JEL Classification: E24, J23, J63, J65

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

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1 Introduction

Major economic downturns lead to large increases in permanent layoffs. Previous research shows that these job displacements can leave scars: large and persistent earnings and welfare losses for the affected workers that prevail even when the economy recovers, for instance due to lost firm-specific human capital (see Davis and von Wachter, 2011; Yagan, 2017; Lachowska et al., 2018, among others). Most governments thus aim at mitigating the effects of recessions on unemployment. One instrument that gained widespread popularity during the Great Recession 2007–2009 was short-time work (STW)[1] programs. Countries such as Germany, Italy, and Japan spent large amounts on STW benefits during the recession.[2] These programs, geared to firms that face a temporary drop in demand, provide income support to workers whose working hours are reduced. By stimulating work sharing, STW may help to distribute the burden of recessions to a larger number of workers. The main concerns with these schemes is that they 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 only postpones rather than prevents dismissals. Given these concerns and the costs associated with STW programs, understanding whether STW schemes induce firms to hoard labor is of central policy relevance.

This study investigates whether the Swiss short-time work scheme helped to prevent unemployment during and in the aftermath of the Great Recession. Our analysis exploits that not all establishments that aim at introducing STW in Switzerland are able to do so. The reason is that STW has to be approved by the cantonal employment agencies.[3] We focus exclusively on firms that applied for STW. In contrast, the few prior firm-level studies on the effectiveness of STW are based on the comparison of firms that use STW with firms that do not, and likely did not want to. This comparison is susceptible to biases because of firms' self-selection into STW: firms that recourse to STW are likely to

[1] In some countries also called partial unemployment (e.g. France) or temporary unemployment (e.g. Belgium).

[2] 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, more than 90'000 workers were covered by STW in 2009. The Swiss unemployment insurance spent more than 1.1 billion Swiss Francs on STW benefits (SECO, 2013).

[3] Many countries do have similar formal application procedures as Switzerland during which eligibility is tested by governmental authorities. However, not everywhere do local authorities have to approve the application (Arpaia et al., 2010; Walz et al., 2012).

differ in observed and unobserved ways from firms that do not make use of STW. In fact, the counterintuitive result of most existing firm-level studies that STW *increases* rather than reduces dismissals could be a direct consequence of the strong selection into STW.[4]

To conduct our analyses, we construct a novel quarterly establishment-level panel dataset that links register data from the universe of short-time work applications for the 2009–2014 period with the Swiss unemployment register and the Swiss Job Statistics. In every quarter around an establishments' application for STW, we then compare the evolution of dismissals—defined as the number of workers from an establishment that register themselves as unemployed—, hirings, and employment between establishments whose application was approved to establishments whose application was denied. We also count the number of daily unemployment allowances collected by workers that were previously employed at establishments that applied for STW.

We find strong evidence that short-time work prevents layoffs. Establishments whose application for STW is denied dismiss roughly 12 percent of their workforce in the three quarters immediately following application. By contrast, establishments whose application is approved lay off only around 5 percent. Importantly, firms that applied successfully dismiss less workers even in the period after they ended collecting STW, suggesting that dismissals are not just postponed. Moreover, we find no evidence that the evolution of dismissals and hirings in the two groups differed in the period *before* the application for STW. Based on these insights, we devise an event study Difference-in-Differences (DiD) approach which compares the change in layoffs and hirings after the application for STW relative to before between the two groups of establishments. Because we observe firms' outcomes in the pre-application period, we can control for fixed effects for each short-time work case. These account, among others, for all time-invariant information that is contained in establishments' application for STW. The DiD estimates suggest that approving STW reduces layoffs by 10 percent of the establishment's workforce in the three years following application. These effects are highly statistically significant. It is mainly workers with compulsory and vocational education whose jobs are saved due to short-time work. Moreover, the effects are larger for small firms and the larger the shortfall in demand an establishment expects at the time of application.

[4]In Switzerland, for instance, only 3 out of 100 firms applied for short-time work in 2009. To address the selection concerns, the existing studies either apply matching approaches or use different instrumental variables to instrument STW take-up. It is, however, unclear whether these fixes overcome the selection concern, and they are susceptible to biases from omitted variables. Moreover, the validity of the exclusion restrictions imposed in the IV estimations may often be questioned.

These results are very robust to various sensitivity checks. In particular, we thoroughly assess whether the before-after comparison of firms by approval status is misleading because cantonal employment agencies take firms' economic situation into consideration when approving or denying STW. We first provide evidence that the expected shortfall in labor demand of firms that applied successfully and those that did not are surprisingly similar, potentially providing an explanation for the similar pre-trends in outcomes. If anything, our comparison of outcomes in the two groups of firms may underestimate the causal impact of STW, as firms whose application was denied seem to be economically "healthier" (e.g. more competitive) than firms whose application was approved. The reason is that cantons deny short-time work among others if the shortfall in demand is perceived to be too small. Moreover, we show that the results are almost unchanged if we conduct several robustness checks that would likely fail if cantons' selection had a systematic influence on our results. Finally, we present Instrumental Variable (IV) estimates that directly exploit the idiosyncrasies in cantonal approval decisions. These results suggest that the negative impact of STW on dismissals may be even larger than in our baseline regressions, consistent with the idea that firms with a comparatively low shortfall in labor demand are overrepresented among firms whose STW application was denied.

Our findings help to reconcile the divergent results in the existing international micro- and macro-level literature about the effectiveness of STW. Several macro-level studies relate the use of STW to changes in employment and unemployment on the country- or region-level. These studies typically find a strong positive correlation between short-time work 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).[5] Firm-level evidence on the effectiveness of STW is surprisingly limited. Moreover, in contrast to the macro studies, the existing micro level studies—including prior policy reports on the Swiss short-time work scheme[6]—are much less optimistic about the effectiveness of STW programs. With the exception of Boeri and Bruecker (2011), the micro studies suggest no or even *negative* effects of short-time work on employment (Kruppe and Scholz, 2014; Calavrezo et al., 2009, 2010).

[5]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, 2016).

[6]See Frick and Wirz (2005), Hollenstein and Marty (1996), and Frick et al. (1989).

Our study contributes to this literature in three important respects. First, our paper adds to the limited micro-level evidence on the effectiveness of STW by using a comprehensive data set. Second, by focusing on firms that all applied for STW, we arguably come closer to the causal effect of STW on firm-level dismissals and employment than the existing micro-level studies on STW. A third important contribution of our paper is that we quantify the direct fiscal costs and benefits of the short-time work scheme for the Swiss unemployment insurance. Using our matched dataset, we provide direct estimates about the extent to which STW reduced spending on unemployment benefits for dismissed workers. This cost-benefit analysis suggests that the fiscal benefits of STW may in fact be large enough to fully compensate the total spending on short-time work benefits. In turn, these results indicate that the deadweight losses associated with the Swiss short-time work scheme are limited.

This study is organized as follows. Section 2 characterizes the Swiss short-time work scheme and presents some descriptive statistics about short-time work take-up in Switzerland. In section 3, we present the dataset. Section 4 discusses the approval decision. In section 5, we study whether the Swiss short-time work prevented unemployment during and in the aftermath of the Great Recession. Section 6 uses this results in order to make an assessment of the direct costs and direct financial benefits of the Swiss short-time work scheme. Section 7 summarizes our main findings and concludes.

2 Short-time work in Switzerland

2.1 The Swiss short-time work scheme

Short-time work benefits are a temporary subsidy for the wages of workers reducing their work hours in firms that face temporary declines in demand. The unemployment insurance act in Switzerland provides firms with the opportunity to apply for short-time work benefits at the cantonal employment agencies. The unemployment insurance replaces 80 percent of the loss in insured income[7] due to the working-time reduction. Firms continue to pay wages for the hours actually worked. Moreover, firms have to cover short-time work benefits during the first two (in the first six month) or three (from the seventh month onwards) days of every month in which they collect short-time work benefits. Short-time work benefits are paid for a maximum of 12 months within two

[7]The maximum insured income during our sample period was CHF 126'000.

years.[8] These regulations aim at limiting the scope for deadweight effects of the short-time work scheme.

Firms and affected workers have to meet certain eligibility criteria in order to obtain short-time work benefits. We discuss the criteria in detail in Section 4. The cantonal employment agencies are responsible for deciding whether an establishment meets these eligibility criteria[9]. Hence, those who decide (cantonal employment agencies) are not the same as those who pay (unemployment insurance). Workers with a temporary contract, temporary agency workers, and trainees are excluded from short-time work benefits.

The government changed certain legal provisions regarding short-time work during our sample period. During the economic crisis in 2008 and 2009, the Swiss government relaxed the eligibility criteria in order to incentivize firms to apply for short-time work. On April 1 2009, the maximum duration of short-time work benefits was extended from 12 to 18 month and the number of days firms had to cover short-time work benefits was reduced from two (during the first six months) and three (from the seventh month onwards) to one day per month. On April 1 2010, the maximum duration was increased further to 24 month. The cost reduction for employers expired in December 2011. At the same time, the maximum duration of short-time work benefits was reduced to 18 months and at the end of 2013 to its normal level of 12 months. Another temporary change in the short-time work scheme occurred at the beginning of 2012: From January 2012 to January 2014 the strong appreciation of the Swiss Franc was officially qualified as a reason to grant short-time work benefits.

The Swiss short-time work scheme is similar to those in other countries in many respects. However, it stands out in two important respects. First, Swiss firms can apply for short-time work without an explicit agreement between the social partners. It is sufficient if the affected workers agree to the introduction of short-time work. Second, Switzerland's short-time work scheme is quite generous for the covered workers. Covered workers earn between 80–100 percent of their prior earnings. This replacement rate is well above the average of the countries surveyed in Hijzen and Venn (2011). Similarly, the maximum duration of short-time work was comparatively high during the Great Recession. Only Finland and Japan allowed the use of short-time work for more than 24 months. However, the maximum duration of 12 months that applies under normal

[8]However, the Swiss government can depart from these legal provisions if exceptional circumstances (e.g. high unemployment) require action. It can extent the maximum duration of short-time work benefits and reduce the costs of employers.

[9]The unemployment insurance only checks the compliance with few formal criteria.

circumstances is close to the international average (Hijzen and Venn, 2011; Boeri and Bruecker, 2011).

2.2 Short-time work during and after the Great Recession

Switzerland's export-oriented sectors were strongly hit by the Great Recession. Value added in the banking and manufacturing sector 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 and unemployment (according to the ILO-Definition) increased from 3.9 to 4.8 percent.

As a reaction to the sharp shortfall in demand during the crisis, firms started to apply for short-time work. Figure 1 reports the number of employees covered by short-time work benefits per month from 2008 to 2014. The use of short-time work peaked mid-2009 when more than 90'000 workers, accounting for roughly 2 percent of the total workforce, received short-time work benefits. However, these aggregate numbers disguises large regional differences. In some cantons more than 10 percent of all workers were covered by short-time work in 2009 and in some regional labor markets, so-called NUTS-III regions (similar to commuting zones), coverage rates exceeded 15 percent (see Figure A.1 and Table A.2 in the Appendix).

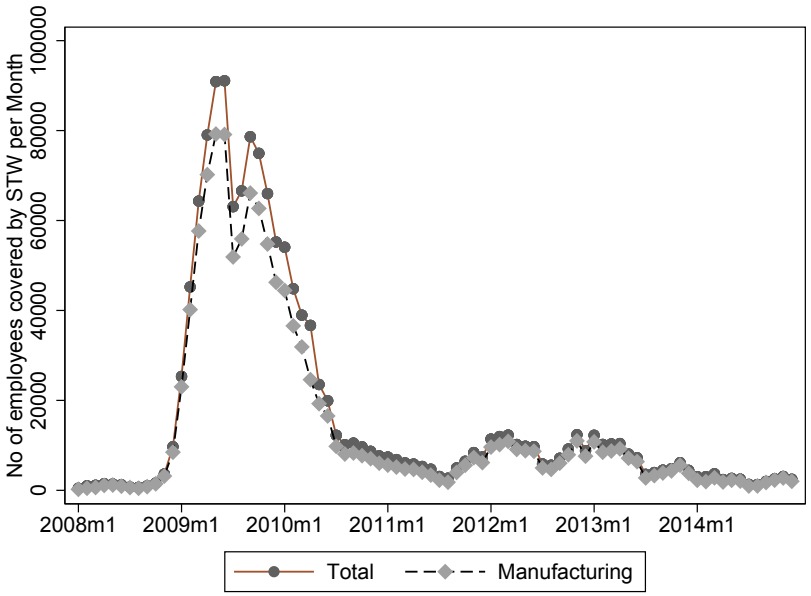
As is illustrated by Figure 1, take-up was largest among manufacturers since the manufacturing sector was most strongly and most immediately affected by the drop in international demand. After a strong decline in short-time work use in 2010 and 2011, take-up increased again somewhat in 2012 in the course of the Euro debt crisis. The crisis led to a very strong real appreciation of the Swiss franc, which put strong competitive pressures on Swiss exporters. The Swiss National Bank reacted by introducing an exchange rate peg of 1.20 for the Swiss franc relative to the Euro. Throughout 2012, 10'000 workers were covered by the short-time work scheme. Short-time work coverage declined substantially from mid-2013 onward.

3 Data

3.1 Data sources

Our analyses are mainly based on a link between the data of the Short-time work statistics (STW dataset henceforth), which contains information on all departments that applied

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



for short-time work in the 2007–2014 period, and the Swiss unemployment insurance register (UIR) dataset 2009–2015. The latter contains detailed information on the universe of individuals registered with the public employment service. Importantly, the register contains an establishment identifier of the last employer of every job seeker and the new employer in case they found a new job. These identifiers are recorded since 2009. Based on these identifiers, we count the number of newly registered unemployed (job seekers) coming from an establishment that applied for short-time work 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. We also do this if an establishment applied for STW but never shows up in the UIR. In addition, we merge data from the Job Statistics (JOBSTAT) to the merged datasets. The Job Statistics is a large quarterly survey of employment conducted by the Federal Statistical Office (FSO) containing information on workers who are subject to social security contributions at the last working day of the respective quarter. We use the quarterly employment data from this survey to carry out analyses on changes in FTE employment in firms applying for short-time work.

Table 1 presents a short overview over the three data-sources. The most important characteristics of the data sources can be summarized as follows:

The **Short-time work dataset** covers all public- and private-sector industries in the years 2007 to 2014. It contains detailed information about the departments that applied for short-time work benefits (application date, total employment, employees registered for short-time work, result of the decision, etc.). It also contains detailed information on the use of short-time work of those departments whose short-time work application was approved (number of covered employees, missed hours, short-time work benefits, etc.). Since some establishments have several departments,[10] and because the unemployment and employment data are sampled at the establishment level, we aggregate different departments within the same establishment to the establishment level. We treat an application in a given month as approved if the application of at least one department of an establishment was approved. For our main analyses, we collapse the dataset to a quarterly frequency. Within 2009 to 2014, the short-time work dataset contains 11'117 establishments with at least one approved case and 3'047 establishments with at least one denied case.

Our sample drawn from the **Unemployment insurance register** covers the years 2006 to 2015 and contains all registered job seekers if their former employer applied for short-time work. The UIR contains detailed demographic characteristics and on unemployment spells of all individuals registering with the public employment service. This can be job seekers who are eligible for unemployment benefits but also other individuals asking the public employment service for assistance. In our main specification, we focus on the effect of short-time work benefits on the flow of unemployed, i.e. job seekers that received some kind of unemployment benefits. They account for 82 percent of all registered job seekers. Our analyses require that we can identify the previous employer of unemployed workers and of the new employers if they found a job. For the 2010–2015 period, establishment identifiers are recorded for almost the universe of registered job seekers.[11] In 2009, the share of job seekers with known last employer is 53 percent. The remaining job seekers cannot be assigned to an establishment. The missing information in 2009 does not appear to be systematically related to the probability of short-time work approval. Our baseline analysis thus includes the year 2009. Our main results are unchanged if we run estimations without observations from 2009.

[10] 2'152 out of 16'243 different short-time work cases between 2009 and 2014 are from multi-department establishments.

[11] The share of job seekers for which the last employer is known is 84 percent in 2010 and 94 percent in 2015 (see Table A.1 in the Appendix). Note, that this number will never reach 100% since not all new job seekers had previously a job.

As Table 1 shows, most establishments in the short-time work dataset appear at least once in the UIR. The UIR contains 8'824 establishments with at least one approved case and 2'339 establishments with at least one denied case.

Table 1: Overview of data sources used in the report and sample overlap

	STW dataset	UIR	JOBSTAT
Time period	2007–2014	2009–2015	2005–2014
Industry (NACE rev. 2)	all	(all)*	10-33 & 45-47
Sampling frequency	Monthly	Monthly	Quarterly
Unit of observation	Department	Unempl. Spell	(Establishment)
Coverage	Universe	Universe	Survey
Establ. with approved STW case	11'117	8'824	2'634
Establ. with denied STW case	3'047	2'339	262
Datasource	SECO	SECO	FSO

* The UIR dataset covers the industries 10-33 & 45-47. It covers other industries only if the establishment is in the STW-Dataset.
Note: Some establishments have approved as well as denied STW cases. Hence, they appear twice in the figures above. Approved and denied cases refer only to the period 2009 to 2014. The figures in this table do not correspond exactly to those in subsequent tables because the latter refer to cases that start in the time period 2009 to 2014 whereas the figures here refer to any observation in the respective time period.

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 JOBSTAT 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 short-time work were approved between 2009 and 2014 but only 262 establishments whose applications were denied.

3.2 Construction of case-level panel dataset

Based on a link between the data sources discussed in the last section, we construct an analysis dataset that distinguishes between individual "cases" of short-time work.

Establishments are formally required to renew their short-time work approval every three months.[12] Hence, in the official case definition, a renewal represents the start of a new case. But since renewals are approved in 99% of all cases, we treat renewals as if they represented the continuation of a case that started earlier. Moreover, we treat applications of establishments that collected STW benefits within the last 6 months as the continuation of the case before. Hence, we do not start a new case if establishments interrupt their collection of short-time work for a short period of time. Based on this case definition, we rearrange our dataset into "event time". The event time represents the anchor in our case-level panel dataset. It is the time period elapsed since the quarter (or month) an establishment applied for short-time work. Event time is normalized to zero at this point in time ($\tau = 0$). We fix the estimation window in event time in the monthly dataset to $\tau = \pm 36$ months and in the quarterly dataset to $\tau = \pm 12$ quarters, i.e. we track firms' outcomes for at most 6 years around the application date (three years before an application and three years after it).

With our case definition, it is possible that the same establishment has more than one case during the sample period. This happens if the same establishment applies for short-time work several times and if the interruption between the last collection of STW benefits and the new registration is more than 6 months. If an establishment has several cases, the post-treatment period of one case overlaps with the pre-treatment period of another case. We treat these cases in the following way: We "cut" the post- and pre-treatment periods of the two cases in such a way that both have the same length. For instance, if one case starts in March 2009 and another one of the same establishment in February 2010—meaning that there are 10 months in between—we cut the post-treatment period of the first case at $\tau = +5$ months and the pre-treatment period of the second one at $\tau = -5$ months in order to avoid that both periods overlap.

In the final dataset, we end up with 16'243 cases from 12'570 different establishments in the years 2009 to 2014. 2'786 establishments exhibit more than one case. 13'565 of 16'243 cases were approved. Hence, the average approval rate is 83.5 percent.

Our main outcome variable is the net share of dismissed workers per quarter (difference between dismissed workers and hires normalized with the total employment at application) coming from a treated or untreated establishment. However, we also study the effect of STW on the share of dismissed workers, the share of new hires, the share of job seekers, total daily allowances per worker and an establishment's number of full-time

[12] In 2009 and 2010 this period was extended to six months.

equivalents. A detailed definition of the outcomes of interest is in subsection A in the Appendix.

4 Cantonal approval decision

Our analysis is generally based on the comparison before and after treatment of firms that applied for short-time work and whose application was approved (henceforth referred to as the *treatment group*), and firms that applied for short-time work and whose application was denied (the *control group*). By focusing only on firms that applied for STW, we arguably circumvent the main selection problem of previous firm-level studies on STW: the decision of firms whether or not to apply for short-time work. However, as previous micro-level studies on STW, our analysis has to cope with the likely non-random approval of STW applications by the cantonal employment agencies. This section therefore provides a detailed discussion of the main factors driving the decisions of the cantonal authorities to deny or approve STW.

According to the law, establishments are eligible to short-time work benefits in Switzerland if the working-time reduction is temporary and if it can be expected that the reduction in working hours helps preserving jobs in the long run. Furthermore, the working-time reduction has to be due to economic reasons, has to be unavoidable and has to amount to at least 10 percent of the usual working time of the firm. In contrast, short-time work benefits are denied if the working-time reduction is due to circumstances that are part of the firms' usual operational risk, if the working-time reduction is customary in the respective firm, or if the working-time reduction is due to seasonal fluctuations.

In practice, these elegibility regulations translate into three central reasons why cantonal employment agencies deny STW. First, STW is denied if establishments' problems are considered as structural rather than temporary, leading to the fear that STW might at best postpone but not prevent dismissals. Second, it is denied if the shortfall in demand is deemed as too small, such that the firm might be able to deal with the demand shock on its own.[13] Third, it is denied if firms' temporary drop in demand is considered seasonal rather than cyclical.

The three reasons have different implications regarding the causal interpretation of the before-after comparison of treatment and control group. If cantons predominantly deny STW because they suspect that problems are structural, our estimates may *overestimate*

[13]Moreover, establishments with a small demand shock might fall short of the requirement that the working-time reduction has to amount to at least 10 percent of the usual working time.

the effect of short-time work. The reason is that treated firms would be "healthier" (e.g., more competitive) in the counterfactual situation than the control firms, i.e. they would have dismissed less workers after the application for short-time work than the control firms even if their application for short-time work had not been approved. 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 deny STW because of seasonality.[14]

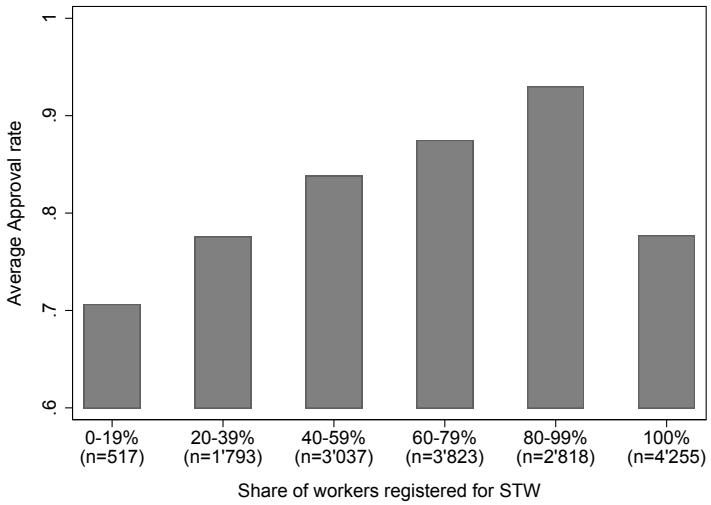
One consequence of this reasoning is that cantons are more likely deny applications of particularly healthy *and* of particularly unhealthy establishments. These groups may thus be overrepresented in the control group. Figure 2 provides evidence consistent with this expectation. It depicts the cantonal approval rates for STW applications depending on establishments' expected shortfalls in labor demand as reported when applying for STW. The latter is proxied by the share of workers that the establishments register for short-time work. We observe that approval rates are lowest for establishments that register less than 40% of their workforce, and for establishments that register the entire workforce for STW. The figure indeed suggests that cantons deny STW to establishments that appear too healthy and to those that appear too unhealthy. On average, however, the share of workers registered for short-time work is very similar in the two groups: 71% in the treatment group and 68% in the control group. We view these figures as indicating that the counterfactual shortfall in labor demand is quite similar in the two groups. If anything, establishments in the control group may be slightly healthier, in which case our estimates of the effect of STW based on the comparison of the two groups may be lower bounds for the true causal effect.

It is important to note, however, that the vague formulation of the eligibility criteria—and the absence of clear instructions by the federal government how to implement them—leave a substantial discretionary scope for the cantonal employment agencies in their decisions on STW applications. 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 cantonal differences in approval probabilities cannot be explained by the characteristics of applying firms.[15] Moreover,

[14]The reason is that control establishments would be more likely to lay off workers shortly after the application for short-time work. But after a short period of around a quarter, they would probably dismiss less and hire more workers than treated firms.

[15]This is shown by Table A.6 in the Appendix. The table reports the cantonal coefficients of a probit regression of the application probability on different firm and regional characteristics. These cantonal

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 short-time work. The latter serves as an indicator of the estimated shortfall in establishments' labor demand at application.

while cantonal approval rates vary within cantons over time, they are persistent: a canton that generally approves applications for short-time work today 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). A formal statistical analysis of the cantonal approval probability provides further evidence for the discretionary scope of cantons (see Appendix section C.2, for details). The success of an establishment's application for STW does not only depend on its economic fundamentals, but also on political factors and the general economic situation in a canton. Its chances are higher if the establishment is of high importance for the regional labor market, if the cantonal unemployment rate is high, and if the workload of the cantonal employment agencies is high.[16]

Table 2 reports characteristics of establishments with denied and approved applications, and reveals the extent to which the latter make use of short-time work benefits. There are noteworthy differences between the two groups: establishments with approved applications are larger than establishments with denied ones and operate more often in the manufacturing sector. On the other hand, the share of workers registered for short-time work at application and the duration from the moment of application to the decision

dummies are highly statistically significant and point to even larger differences between cantons than the unconditional differences.

[16]For details about the measurement and construction of the variables see the remarks with regard to Table A.5 in the Appendix.

of the cantonal authorities is very similar in both groups. Importantly, not all establishments whose applications were approved actually use short-time work. In 24% of all approved cases, short-time work benefits were not collected. Table 2 also reveals that those establishments that collect short-time work benefits use them for 6.8 months on average and cover 60 percent of their workforce. Figure A.2 in the Appendix reports more on the the intensity of short-time work use in the 12 quarters following the application.

Table 2: Establishment characteristics and STW usage by case and treatment status 2009-2014

	STW denied	STW approved
Employment at registration	12	28
Desired share of covered employees	.68	.71
Duration of stw decision process (days)	12	10
Manufacturing share	.18	.54
Construction share	.3	.15
Other 2. Sector share	.0034	.0032
Trade share	.15	.1
Other 3. Sector share	.37	.21
Establ. actually made use of STW	.	.76
Number of months establ used stw	.	6.8
Share stw employment in total employment	.	.6

Source: SECO STW dataset

5 Results

In this section, we present our main results with regard to the question whether short-time work prevents unemployment. We proceed as follows. In section 5.1, we study descriptively whether establishments whose short-time work application is approved dismiss less workers than establishments whose short-time work application is rejected. Section 5.2 presents our regression model and Section 5.3 reports the main regression results. Section 5.4 discusses whether our estimates reveal causal effects and in section 5.6, we address these concerns explicitly by means of instrumental variable estimations, which exploit certain random elements in cantons' approval decisions. Finally, section 5.7 studies whether

we see similar patterns regarding the effectiveness of short-time work if we focus on firms' employment rather than inflows into and outflows out of registered unemployment.

5.1 Descriptive Evidence

How does the net share of dismissed workers[17] evolve in treated establishments (i.e. establishments whose short-time work application was approved) and in the control group (i.e. establishments whose short-time work application was denied) around the time of the establishments' application for STW? Figure 3 gives an answer to this question by plotting the evolution of the share of dismissed workers against event time τ.

Panel 3a shows that the net share of dismissed workers in treated establishments hovers around 1% per quarter in the pre-treatment period. In the quarters after the application for short-time work, the share increases somewhat, reaching slightly less than 2% in the second and third quarter after the application before declining again. The evolution of the net share of dismissed workers is similar in the control group before application. The small difference in the levels is entirely explained by firm-size and industry as Panel 3b shows, that displays the evolution of the share of dismissed workers after adjusting for industry and firm-size. However, the evolution is very different after the application. Here, the share more than triples in the two quarters following the application for short-time work (which was not approved by the cantonal bodies). The share remains elevated—higher than in the pre-treatment period—in all the following post-treatment quarters. A decomposition of the net share of dismissed workers into the share of new hires and the share of dismissed workers shows, that the effect is almost entirely driven by the share of dismissed workers.

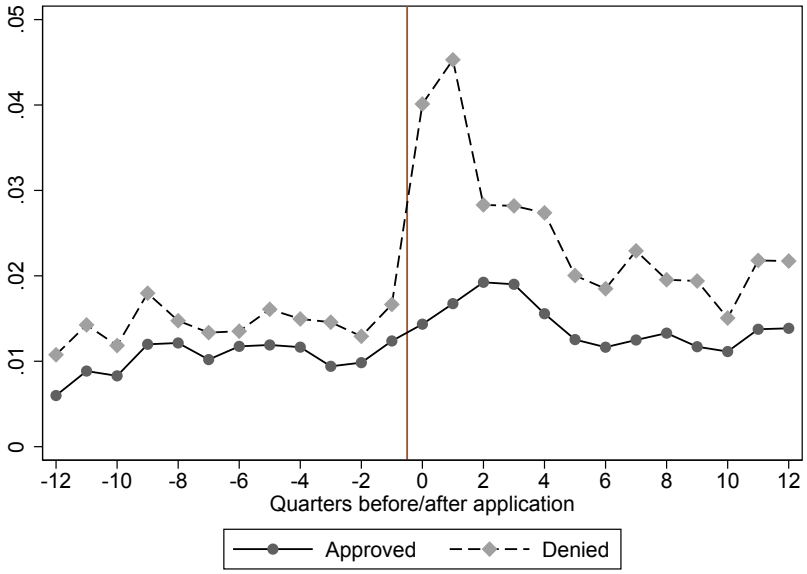
Overall, Figure 3 provides strong evidence that denying the short-time work application forces establishments to dismiss an above-average share of workers during the subsequent three years, with a spike in dismissals in the two subsequent periods. Conversely, we observe only slightly more dismissals among firms that receive short-time work benefits in the treatment periods (periods $0 \geq \tau \geq 8$) and thereafter. The figure suggests that approval of short-time work prevents unemployment.

5.2 Regression model

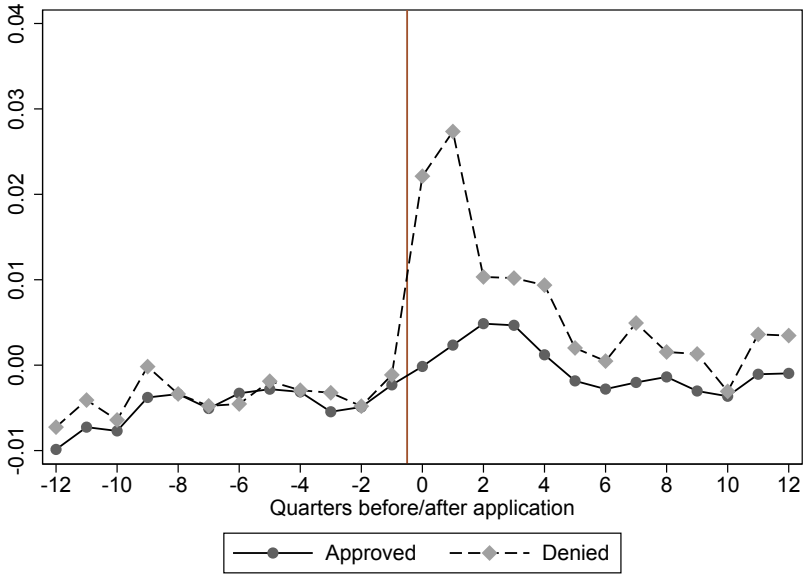
A striking feature of Figure 3 is the similar level and evolution of the net share of dismissed workers in firms with successful and unsuccessful STW applications prior to application.

[17] See section A in the Appendix for the definitions of this outcome variable.

Figure 3: Net share of dismissed workers and hires around short-time work application, by approval decision



(a) Net share of dismissals



(b) Adjusted net share of dismissals

Notes: The top panel displays the average net share of dismissed workers of establishments with approved and denied short-time work applications for each quarter around the short-time work application date. The vertical red line highlights the quarter of application. The bottom panel displays the residuals of a regression of the net share of dismissed workers on firm-size and industry of establishments with approved and denied short-time work applications for each quarter around the short-time work application date.

The parallel trends in dismissals—potentially a result of the idiosyncrasies in cantonal approval decisions documented in section 4—suggest that the two groups would follow common changes in dismissals absent treatment. A comparison of the difference in the change in dismissals at $\tau = 0$ relative to before between treatment and control group may thus provide a valid benchmark for how approval of short-time work affects dismissals.

We thus estimate the following flexible event study Difference-in-Differences (DiD) model for the outcome of interest in short-time work case i and period t, which we denote by $u_{i,t}$:

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

The dependent variable in this model is the share of dismissed workers, or, alternatively, one of the other outcomes studied in the paper. The central independent variables are the sequence of "event study" indicators for approval of short-time work, denoted $STW_{i,t}^{\tau}$. The model contains one of these indicator variables for each event period within the event window $[-k\ k]$. They are one in event time period $\tau = k$ if the case i of an establishment is approved. The specification controls for a full set of period fixed effects γ_t, which account unobserved factors that affect all observations in a given time period equally such as common business cycle shocks, and for event time fixed effects γ_τ, an individual fixed effect for each event time period.[18] Specified this way, we expect that the series of DiD estimates β_τ is negative in the treatment period (i.e. $\tau \geq 0$). The reason is that we expect that firms whose application was denied display a stronger increase in the share of dismissed workers, relative to the pre-treatment period, than firms whose application was approved. Because we estimate an entire sequence of these DiD coefficients, we can evaluate the effect of short-time work approval for every period k around the time of an establishment's application for short-time work.

An important ingredient of the model are the case fixed effects δ_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. 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.

[18] γ_t and γ_τ can be separately identified because (i) we have both, treated and control units and (ii) they are treated at different points in time, so that the time and the event index do not coincide.

Indeed, most of the factors that influence the probability to approve STW applications (according to the regressions in Table A.5) are correlated with establishments' dismissals. However, only one, firm size, is statistically significantly related to *changes* in dismissals over time.

For the event study model to work, we need to decide on certain technicalities. First, we have to specify how to deal with the fact that there are differences in the number of treatment periods (periods with $\tau \geq 0$) and pre-treatment periods by case. Rather than estimating an entire possible sequence of event study coefficients β_τ, the common approach in the literature is to define an "event window" k around the application date within which we estimate effects. We settle on $\pm$ 3 years, i.e. we track the outcome over $k = \pm 12$ quarters around the time of application.[19] 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. This makes it easy to test for an impact of short-time work on the outcome. Third, we need to settle on a mode to conduct statistical inference. The obvious choice here is to cluster standard errors at the level of establishments, which accounts for the facts that the regression errors may be correlated within establishments over time. It also takes care of the fact that certain establishments have several cases, which are unlikely to be independent. Finally, we decided to impose one sample restriction: we focus only on firms that are present in the three periods $\tau = -1$, $\tau = 0$, and $\tau = 1$.[20]

5.3 Main regression results

Figure 4a presents the sequence of event study coefficients, β_τ, and associated 90% confidence intervals, from our baseline event study model (equation 1). The outcome variable is the net share of dismissed workers (see section A in the Appendix for a detailed description how we built the outcomes used in the paper). Conditional on time, event time, and case fixed effects, there are no differences in the change of this outcome between

[19]The question is then how to deal with the fact that for some cases, we have more than 12 periods before and after treatment. The approach we follow here is to "bin up" the endpoints, i.e. we build an event study dummy that is 1 in all periods $k < -12$ for treated firms and another one that is always one in all periods $k > 12$ for treated firms. We include these two dummies in all regressions.

[20]The reason for this restriction is related to the fact that our sample starts in the first quarter of 2009. In this quarter, many establishments applied for short-time work. However, we do not observe any pre-treatment period for establishments that apply in this quarter. Requiring that firms have to present at least in the periods from $\tau = -1$ to $\tau = 1$ ensures that our results are not driven by the substantial number of cases for which we do not have any pre-treatment period.

treatment and control group in the period prior to the application for short-time work ($\tau < 0$). This is a direct consequence of the fact that the share of dismissals evolves in parallel in the two groups during this period (see Figure 3). This is strikingly different in the treatment period. Here, the increase in the share of dismissed workers is substantially smaller, and highly statistically significantly so, in firms whose short-time work application is approved. This suggests that the approval of short-time work prevents unemployment. Importantly, the figure also suggests that dismissals are not just postponed to the end of firms' collection of short-time work. Dismissals are lower in treated establishments in all post-treatment periods shown in the figure, even in periods more than two years after application when almost all treated firms do not collect short-time work benefits anymore (see Figure A.2).

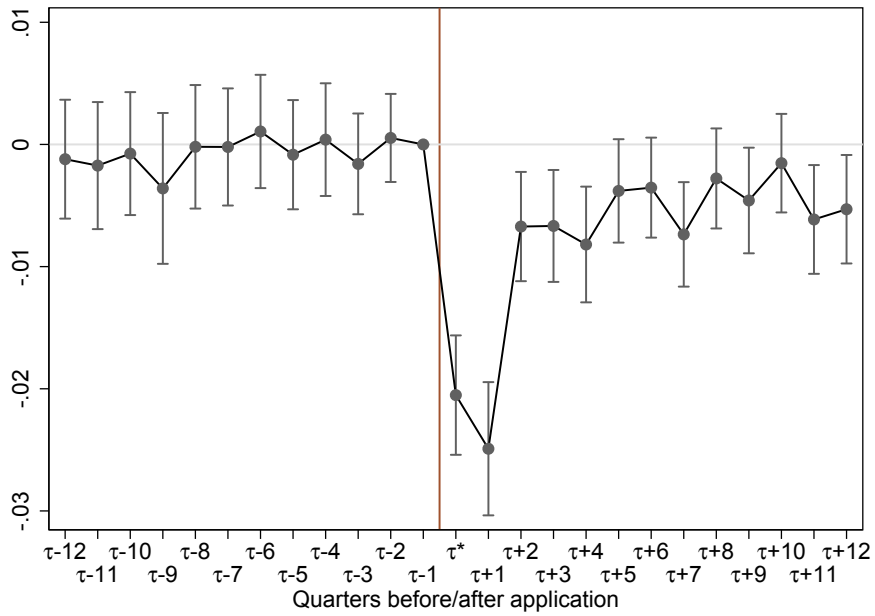
The estimates in Figure 4a present the DiD between the two groups in a specific quarter. To estimate the overall effect of the approval of short-time work, we can sum up these quarter-specific effects. We thus compute cumulative sums of β_τ from period $\tau = -1$ to period R, $E_R = \sum_{\tau=-1}^{R} \beta_\tau$, both for positive and negative R. In Figure 4b, we present these cumulative sums, and corresponding inference, for the share of dismissed workers, the share of hires, and the net share of dismissed workers.

The figure shows that the difference in the share of dismissed workers between treatment and control group sums to 10% over the first twelve quarters after application ($0 \geq \tau \leq 12$). These estimates roughly suggest that the approval of short-time work prevented unemployment of about a tenth of the establishment's workforce.[21] Figure 4b also suggests that treated establishments hire slightly less workers from the pool of unemployed than the control group. The effects on the hiring of unemployed, are, however, very small. We thus do not find strong evidence that the Swiss short-time work scheme comes at the expense of outsiders whose entry into employment is made more difficult—a concern regarding STW schemes sometimes raised (e.g., Cahuc and Carcillo, 2011). Because the effects on the share of hires from the pool of unemployed are very small relative to the effects regarding dismissals, the cumulative effect on the *net* share of dismissed workers is clearly negative (Figure 4b).

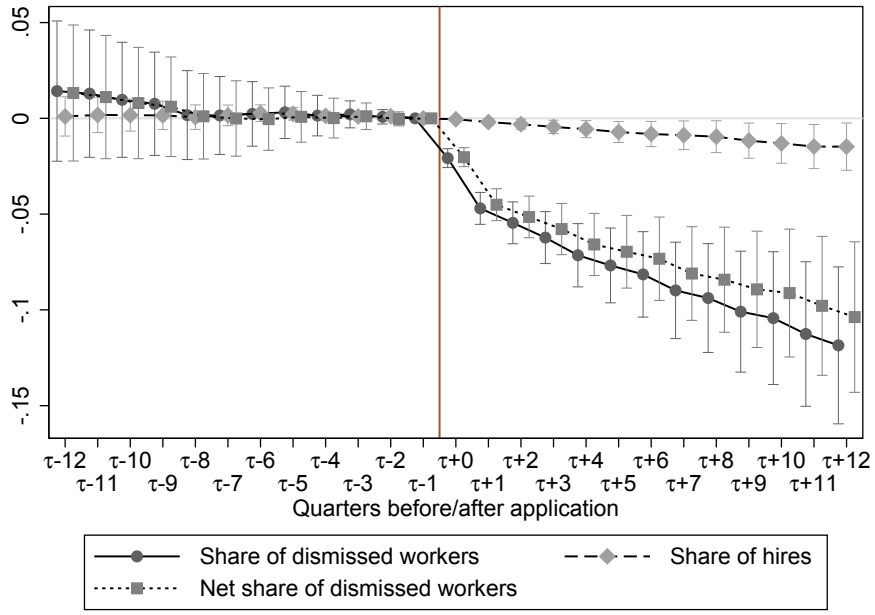
Columns 1–3 of Table 3 summarize the results for the share of dismissed workers, the share of hires, and the net share of dismissed workers in the form of a table. Our estimates suggest a more negative effect of approval of short-time work on dismissals

[21]Note, that these estimates represent Intention-to-Treat (ITT) effects and not Treatment-Effects-on-the-Treated (ToT) since in 24% of all approved cases, STW benefits were not collected. The ToT would thus be slightly higher given that the effect is similar for establishments that collected STW benefits and those that decided not to. In section 5.6 we provide an estimate for the ToT.

Figure 4: Event study: effect of short-time work on unemployment



(a) Effect on net share of dismissals



(b) Cumulative effect on share of dismissed workers, share of hires, and net share of dismissed workers

Notes: The top panel plots the sequence of event study coefficients, β_τ, and associated 90% 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. The bottom panel plots the cumulative effect of short-time work benefits, 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.

compared to most estimates from previous firm-level studies on short-time work. Kruppe and Scholz (2014), Calavrezo et al. (2009), and Frick and Wirz (2005) indeed find no or even negative (positive) effects of short-time work on employment (unemployment). Our estimates are most closely to the employment estimates reported in Boeri and Bruecker (2011). Their OLS estimates suggest that a one percent increase in the share of workers covered by short-time work within a firm raises employment by about 0.07 percent. At an average short-time work coverage rate of 50 percent, this implies employment gains due to short-time work of about 3.5 percent within a year for the average firm. Our estimates suggest an impact on dismissals of 6 percent of the workforce after one year.

Table 3 also contains the regression results if we use three further outcome variables. The fourth column shows the difference between treatment and control group in the evolution of the share of job seekers, which includes individuals that are registered at unemployment agencies but do not collect unemployment benefits. Comparing columns 1 (the share of dismissed workers claiming unemployment benefits) and column 4 (all registered job seekers) of Table 3, we note that it makes little difference whether we include these individuals or not.

Column 5 of Table 3 incorporates the unemployment duration of the dismissed workers. In particular, we compute the sum of all daily unemployment allowances that the dismissed workers collect during the subsequent unemployment spell. The outcome variable is the sum of all daily allowances of all workers dismissed in the respective quarter. As with the other variables, we normalize this outcome with the establishments' employment at $\tau = 0$. The DiD estimate is 25.8 days over the first 12 quarters after application. Since the average firm has 25 employees at application, the estimates suggest that granting short-time work saves roughly 625 daily allowances per case. If we repeat this exercise but count all days between registration and de-registration from the cantonal unemployment office (instead of counting daily allowances), we find a DiD estimate of 38.4 days (column 6 of Table 3). This effect is quantitatively in line with the effect on daily allowances considering that daily allowances are only paid for working days ($38.4 * 5/7 = 27.4$).[22]

5.4 Main robustness checks

The central concern with the estimates presented in the last section is that they are biased because control and treatment group differ in the underlying economic situation despite

[22]The slight difference likely arises because some job seekers remain registered at the unemployment agencies even if they are no longer eligible for unemployment benefits after reaching the maximum benefit duration.

Table 3: Effect of short-time work approval on different unemployment outcomes

	(1) dismissals	(2) hires	(3) net dismissals	(4) job seekers	(5) allowances	(6) days reg
STW $\tau-8$ *to* $\tau-4$	0.000	0.000	0.000	0.005	-1.852	-5.676
	(0.011)	(0.003)	(0.010)	(0.012)	(2.687)	(4.306)
STW $\tau-3$ *to* $\tau-1$	-0.002	-0.001	-0.001	-0.004	-0.859	-0.623
	(0.004)	(0.001)	(0.004)	(0.005)	(1.028)	(1.641)
STW τ *to* $\tau+3$	-0.062***	-0.004**	-0.058***	-0.068***	-12.191***	-20.071***
	(0.008)	(0.002)	(0.008)	(0.009)	(2.016)	(3.453)
STW $\tau+4$ *to* $\tau+8$	-0.032***	-0.005	-0.026***	-0.032***	-7.313***	-9.033**
	(0.010)	(0.003)	(0.010)	(0.012)	(2.474)	(3.721)
STW $\tau+9$ *to* $\tau+12$	-0.025***	-0.005*	-0.020**	-0.026***	-6.288***	-9.340***
	(0.009)	(0.003)	(0.008)	(0.010)	(2.083)	(3.211)
STW τ *to* $\tau+12$	-0.119***	-0.015**	-0.104***	-0.126***	-25.791***	-38.443***
	(0.025)	(0.007)	(0.024)	(0.029)	(5.963)	(9.100)
N	389242	389242	389242	389242	389242	389242
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

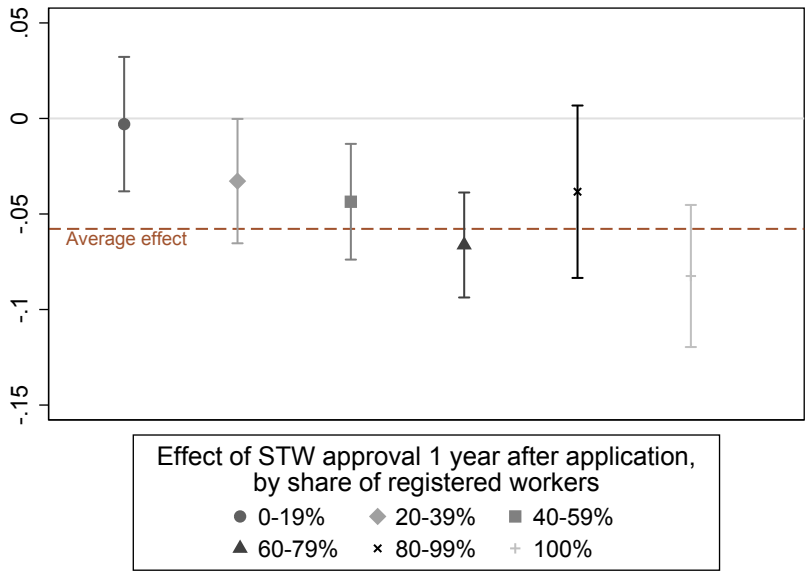
Notes: The dependent variables are the share of dismissed workers (column 1), the share of hires (column 2), the net share of dismissed workers (column 3), and the share of job seekers (column 4). The dependent variable in column 5 is sum of all (future) daily allowances (unemployment benefits) of all workers dismissed in the respective quarter by the firms, normalized by the number of workers at registration (total daily allowances per worker). The dependent variable in column 6 is sum of the total (future) days registered as unemployed of all workers dismissed in the respective quarter by the firms, normalized by the number of workers at registration. See section A for information on these outcomes. Baseline controls are period, event time, and case fixed effects. The table lists the sum of coefficients for indicated intervals. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the similar pre-trends. In section 4, we hypothesized that—as a consequence of the cantonal approval practices—particularly healthy and unhealthy may be overrepresented in the control group relative to the treatment group, but that the average counterfactual shortfall in labor demand may be similar in the two groups. In the discussion we used the share of workers that establishments register for short-time work as an indicator for the shortfall in establishments' labor demand at application. Our analysis does not exploit this information until now. Are our results similar if we condition the comparison between treatment and control establishments to establishments that register a similar share of workers for STW?

Figure 5 presents the estimated cumulative effect of STW on the net share of dismissed workers one year after application, depending on the share of workers that establishments register for STW. We assign firms into one of six groups, and then estimate our baseline event study model for each of the groups. The first coefficient in Figure 5 reveals that there is no discernible difference in the post-treatment evolution of dismissals in the few

firms that expect a small shortfall in labor demand at application, and thus register less than 20% of workers for STW. As expected, the difference in the evolution of dismissals tends to become larger, and more statistically significant, the larger the share of workers registered for STW. Importantly, if we weight each coefficient in Figure 5 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 which pools all firms (indicated in the figure with a dashed line). The estimated effect of STW is thus similar if we explictily condition on the share of workers registered for STW. The figure also implies that our results would be similar if we disregarded seemingly healthy firms (i.e. firms that planned to cover a low share of workers) and seemingly unhealthy firms (firms that planned to cover all workers with short-time work). Not surpringly 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 only identified from the comparison of firms that registered a similar share of workers (cf. column 1 and 2 of Table 4).

Figure 5: Effect of STW on dismissals depending on firms' expected shortfall in labor demand



Notes: Each coefficient represents the cumulative effect of STW on the net share of dismissed workers one year after approval, derived from our baseline event study model estimated separately for firms depending on the share of workers that they register for short-time work. 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 firms.

There are two other main concerns with the event study results in the last subsection. The first is that they are driven by differing seasonal patterns in treatment and control group, as cantons can deny STW if they think that the shortfall in demand is seasonal rather than cyclical. To address this concern, our baseline regressions control for period fixed effects, which absorb seasonal patterns that are common to all firms. As shown by column 4 of Table 4, the results are virtually unchanged if we use industry-specific period fixed effects which account for seasonal patterns specific to two-digit industries. Moreover, our results are very similar if we simply drop seasonal industries altogether.[23]

Another important concern is that our results are biased because firms are more willing to apply for STW if the chances of approval are higher. We indeed find some evidence consistent with this idea, as the probability of applying for short-time work during the Great Recession is weakly positively related to the average cantonal approval rate. If high approval rates increase firms' take-up of STW, cantons that generously handle short-time work applications may attract applications of a different sample of firms 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. Column 5 of Table 4 thus adds a full set of canton-period (in addition to the industry-period fixed effects) to the baseline regression. The canton-time effects ensure that we only compare firms that applied at the same point in time within the same canton. Arguably, these establishments faced the same cantonal approval practice. The inclusion of these dummies has no impact whatsoever on the event study results.

5.5 Heterogeneity

Section D in the Appendix discusses in detail whether the effects of the approval of short-time work on the net share of dismissals depend on certain establishment characteristics and whether short-time work primarily prevents unemployment of high-, medium-, or low-qualified workers. Here, we briefly present the main results.

It is mainly workers with compulsory and vocational education whose jobs are saved because of short-time work. In contrast, the differences across broad industries are not

[23] A firm-specific way to test for the importance of seasonality is to disregard establishments that are ever observed to hire the same worker that they previously dismissed. Such re-call behavior is much more common in seasonal industries. In a recent study, Föllmi et al. (2014) estimate that 52% of all recalls in Switzerland occur in the construction and hospitality industry. Dropping such firms has little effect on the estimated effects, too (see column 4 of Table A.10 in the appendix).

Table 4: Main robustness checks: effect of short-time work approval on net share of dismissed workers

	(1)	(2)	(3)	(4)	(5)
STW $\tau-8$ *to* $\tau-4$	0.000	-0.000	0.000	-0.001	-0.001
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
STW $\tau-3$ *to* $\tau-1$	-0.001	-0.001	-0.001	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
STW τ *to* $\tau+3$	-0.058***	-0.058***	-0.059***	-0.059***	-0.059***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
STW $\tau+4$ *to* $\tau+8$	-0.026***	-0.026***	-0.026***	-0.025**	-0.025**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
STW $\tau+9$ *to* $\tau+12$	-0.020**	-0.019**	-0.018**	-0.021**	-0.019**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
STW τ *to* $\tau+12$	-0.104***	-0.103***	-0.102***	-0.105***	-0.103***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
N	389242	389242	389293	389177	389177
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 dependent variable is the net share of dismissed workers. 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 of the of the share of workers registered for STW at application, as reported by firms in the short-time work application form. The table lists the sum of coefficients for indicated intervals. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

large. 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. 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. The effects are much more sizeable for small firms. Finally, we find that the effect of short-time work on the share of dismissed workers does not differ by year. Hence, the negative effect of short-time work benefits on unemployment is not restricted to the crisis-years but can be observed in the subsequent years 2011–2014, too.

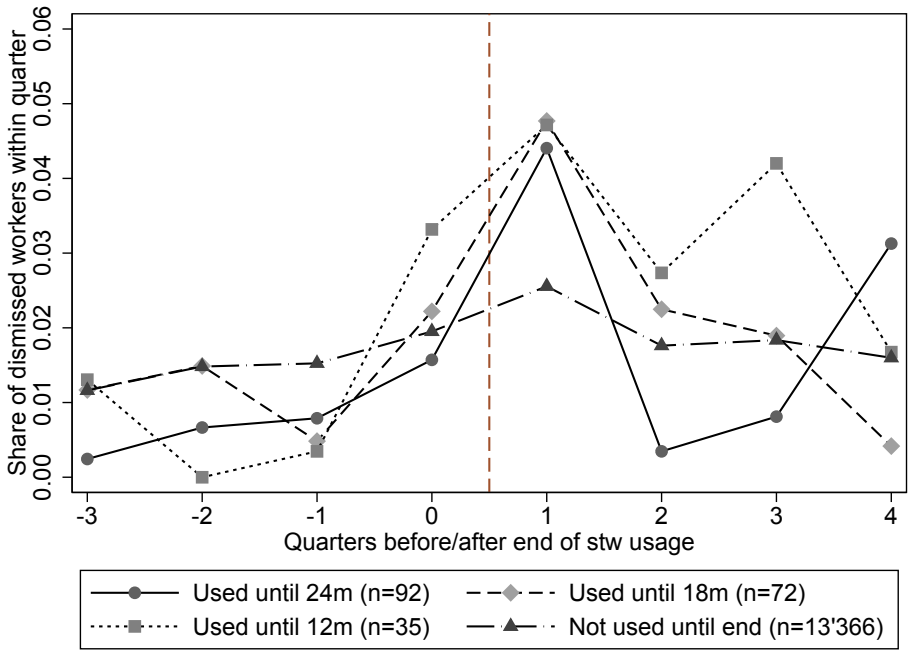
One of the major concerns regarding short-time work is that it only postpones rather than prevents layoffs. We saw that for the average firm this concern seems to be unfounded. But what about establishments that collect short-time work benefits until they reach the legal maximum duration? We might expect the latter to be more likely to be in a critical situation and therefore more prone to dismiss workers as soon as payments of short-time work benefits stop. And indeed, as Figure 6 shows, establishments that collect short-time work benefits until the maximum duration of 12, 18 or 24 months, respectively, dismiss a sizeable share of their workforce in the quarter directly following benefit expiration.[24] Hence, this group seems indeed to use short-time work benefits to postpone dismissals. However, as the number of cases per group reported in the legend of Figure 6 shows, the share of establishments that use short-time work benefits until they reach the maximum legal duration is very small. More than 98 percent of all establishments voluntarily stop collecting benefits even though the law would allow them to collect the benefits even longer. For this group, we do not observe a sizeable increase of dismissals at benefit expiration which is why on aggregate we see the huge negative effect of short-time work on unemployment reported before.

5.6 Instrumental variable estimates

In this section, we present further evidence on the effect of approval of short-time work on dismissals using instrumental variable (IV) methods. Intuitively, our focus here lies on certain idiosyncrasies in the decisions of cantonal employment agencies to deny or approve short-time work. As we noted in section 4, average cantonal approval rates

[24]Remember, that during normal times the maximum duration is 12 months. However, the government extended the maximum duration in 2009 and 2010 first to 18 and then to 24 months.

Figure 6: Share of dismissed workers around the end of short-time work collection



Notes: The figure depicts the share of dismissed workers in the quarters before and after the end of short-time work collection. Quarter 1 starts immediately after the last month of short-time work benefit collection. The figure differentiates between establishments that did and establishments that did not collect short-time work 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.

range from 55% to 100%. These cantonal approval rates change somewhat over time, but cantons with high approval rates at one point in time or in one industry tend to have high approval rates at another point in time or for other industries. These observations suggest that cantonal employment agencies differ in the strictness with which they handle short-time work applications. The consequence is that different cantons treat very similar short-time work cases differently. With our preferred instrument, we aim at exploiting these differences in approval practices across cantons.

The first set of two-stage least squares (2SLS) estimations that we run are based on the DiD model of Equation DA.1 in the Appendix, focusing on the effect of short-time work approval on the change in the net share of dismissed workers in the first year after application relative to the year before. Our preferred instrument for the indicator whether short-time work was approved is a canton's approval rate for all short-time work 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 the *change* in firms' dismissals at $\tau = 0$, apart from its direct effect on

approval of short-time work. Arguably, the lagged cantonal approval rate is unrelated to the unobserved characteristics of the applying establishments. After all, the firms' 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 increase in dismissals at $\tau = 0$. By excluding the approval rates in the establishment's own industry and by lagging the approval rate by one quarter, we aim at further increasing the probability that the exclusion restriction and the conditional independence assumption required for the 2SLS estimations are met. Moreover, we add a rich set of control variables to the 2SLS regressions.[25]

The first stage regressions presented in Panel A of Table 5 show that the lagged cantonal approval rate in other industries strongly predicts whether an establishment's short-time work application is approved. The instrument thus appears to be *relevant*. The coefficient estimated in the third column suggests that an increase in the lagged cantonal approval rate in other industries by, say, 10 percentage points, increases an establishments' approval probability by about 4 percentage points, conditional on detailed industry-period fixed effects and all the control variables that have been shown to affect cantonal approval decisions in section 4. In column 4, we add canton fixed effects to the model. The first stage coefficient becomes smaller in this case, but remains highly statistically significant. This suggests that our instrument may work even if we focus solely on changes in approval practices within the same canton. Yet, one problem with this regression is that certain cantons handle only very few short-time work applications in a given period, such that their approval rates are quite erratic. In these cantons, the lagged approval rate is likely to be a noisy measure of cantonal approval practices. We thus disregard cantons that handle less than 50 short-time work applications on average within two quarters.[26] As expected, dropping cases from these cantons markedly improves the size and precision of the first stage (column 5 of Table 5).

[25]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 firms 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 rates by one quarter. In the two periods prior to the application period ($\tau = -1$ and $\tau = -2$), we observe no systematic differences in the evolution of the outcomes between treatment and control group, limiting concerns that the cantonal approval decision in these periods is related to unobserved regional or industry-specific shocks. Note also that our regressions control for a set of time-varying canton-specific covariates and industry-time fixed effects, which further address this issue.

[26]All cases from the cantons of Uri, Schwyz, Nidwalden, Obwalden, Glarus, Fribourg, Basel (city), Appenzell Inner-Rhodes, Appenzell Outer-Rhodes, Schaffhausen, Grisons, and Valais are dropped.

The corresponding columns in Panel B of Table 5 show the second stage, i.e. the effect of short-time work approval on the net share of dismissed workers if we only exploit the variation in the approval decision that can be explained by the lagged cantonal approval rate in other industries. All estimates suggest that the approval of short-time work reduces dismissals substantially. The estimate in the third column, for instance, indicates that the approval of short-time work decreases the net share of dismissed workers in the first year after application by 13%. The estimated effects are even larger in columns 4 and 5 where we focus on changes in approval rates within the same canton, but they are also more imprecisely estimated. Importantly, the estimated effects in these IV regressions are at least twice as large as the estimates from the corresponding DiD regressions (see column 1). If anything, our prior DiD results may thus even understate the effects of short-time work approval on dismissals.

Table 5: IV estimates of the effect of short-time work approval on the change in the net share of dismissed workers

	(1)	(2)	(3)	(4)	(5)
	OLS	2SLS	2SLS	2SLS	2SLS
		appr.	appr.	appr.	appr.
		rate	rate	rate	rate
VARIABLES					w/o small
Panel A: First stage					
Lagged appr. rate (other ind.)		0.551***	0.408***	0.161***	0.286***
		(0.039)	(0.036)	(0.056)	(0.063)
Panel B: Second stage					
STW approved	-0.056***	-0.122***	-0.133**	-0.388*	-0.262*
	(0.010)	(0.042)	(0.059)	(0.227)	(0.137)
Observations	5,488	5,730	5,650	5,730	4,873
Industry-period FE	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	No	No	Yes	Yes
Controls	No	No	Yes	No	No
RMSE	0.216	0.198	0.197	0.227	0.206

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows IV estimations. The dependent variable of the second stage regression (Panel B) 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 $-4 \geq \tau \leq 3$. Panel A shows the corresponding first stage regressions. In column 1 we report the OLS estimate from our event study model. The instrument for the IV-estimates is the cantonal approval rate in all other two-digit industries in the two quarters preceding application. Controls are the set of variables from Table A.5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 6, we run IV regressions using the level—rather than the change—in the net share of dismissed workers as the outcome of interest. The advantage of this approach is a much larger sample size since it does not require pre-treatment outcomes. The disadvantage, of course, is that we have to assume conditional independence and the exclusion restriction for the level of the net share of dismissed workers, which is more restrictive than the corresponding assumptions for the change in the share. If we focus on the cumulative effects in the first four quarters after application, we find that the approval of short-time work reduces the net share of dismissed workers by about 8% (column 1 of Table 6). This effect is somewhat smaller than the first-differenced IV estimates in Table 5, but somewhat larger than the corresponding event study estimates. In the second column of Table 6, the outcome is the cumulative net share of dismissed workers 12 quarters after application (column 2). The regressions suggest that the approval of short-time work reduces the net share of dismissed workers by 23%, in the following twelve quarters. Similar comments apply if we use the sum of all daily allowances of the dismissed workers as the outcome variable of interest (column 3–4 of Table 6). As was the case in Table 5, the IV estimates in Table 6 are larger than the corresponding event study estimates.

Two comments on these IV results are noteworthy. First, the results speak only about the effects on the subgroup of establishments whose approval status is affected by the instrument (the so-called compliers). In statistical terms, the causal effects are thus only locally identified (hence the term local average treatment effect, LATE). The estimated effects arguably focus on ambiguous cases: the cases that are approved in one but would be denied in another canton or in the same canton but at another point in time. It is possible that the effect of short-time work approval on dismissals in these close cases differs from the effect for the "average" case. Whether the effect is smaller, similar, or larger is unclear, and likely depends on what is driving cantonal approval decisions.[27] Second, the IV results—as well as the event study results in the last section—represent the effect of short-time work approval on dismissals, irrespective of the fact whether establishments claimed short-time work benefits if their application was approved. The IV approach provides one way how we can estimate the effect for those firms that actually used short-time work once their application was approved (an estimate of ToT for the

[27]If cantonal differences are driven by differences in the assessments whether establishment face structural rather than temporary problems, the close cases are probably those where short-time work has a large effect on dismissals. Conversely, if cantonal differences are driven by the uncertainty about whether a drop in demand is sufficiently strong, the close cases may be those where short-time work does no matter that much.

Table 6: IV estimates of the effect of short-time work approval on the net share of dismissed workers and on total daily allowances per worker

	(1) Net dismissals appr. rate	(2) Net dismissals appr. rate	(3) Allow-ances appr. rate	(4) Allow-ances appr. rate	(5) Net dismissals appr. rate
VARIABLES	$0 \geq \tau \leq 3$	$0 \geq \tau \leq 12$	$0 \geq \tau \leq 3$	$0 \geq \tau \leq 12$	$0 \geq \tau \leq 12$
STW approved	-0.081**	-0.235***	-16.668*	-48.523***	
	(0.033)	(0.075)	(9.278)	(15.225)	
STW used					-0.315***
					(0.102)
Observations	14,370	10,795	14,370	10,795	10,795
Industry-period FE	Yes	Yes	Yes	Yes	Yes
Canton FE	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes
RMSE	0.173	0.331	46.20	65.38	0.351

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents IV estimates of the effect of short-time work approval (columns 1–4) and short-time work use (column 5) on the net share of dismissed workers and the sum of all (future) daily allowances (unemployment benefits) of all workers dismissed by the firms, normalized by the number of FTE workers at registration (total daily allowances per worker, columns 3–4). These outcomes are computed over the first four quarters after application in columns 1 and 3, and over the first 12 quarter (i.e. quarters $0 \geq \tau \leq 12$) in columns 2, 4, and 5. Short-time work used is an indicator equal to one if a firm claims STW benefits. The instrument is the lagged cantonal approval rate in other two-digit industries. Controls are the set of variables from Table A.5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

subgroup of compliers). The approach is simple: We rerun the prior IV regressions, but rather than instrumenting the dummy variable whether short-time work was approved, we instrument an indicator whether short-time work was *used*. The IV approach overcomes the selection concerns that arise from firms' decisions to claim short-time work benefits by only exploiting the variation in short-time work use that can be explained by the lagged cantonal approval rate in other industries. It works under the modified exclusion restriction that approval of short-time work does not affect firms that do not use short-time work if their application is approved. The results are shown in column 5 of Table 6. The second stage estimates suggest that the use of short-time work reduces the net share of dismissed workers in the three years following application by 31.5%. This estimate is about 34% larger than the corresponding (local) ITT (cf. column 2 in Table 6). It is in line with what we could guess from ITT and the take-up rate of short-time work benefits.[28]

5.7 Effects of short-time work on employment

In this section, we explore how approval of short-time work affects firms' FTE employment. The analysis is based on a link between the STW dataset and the Job Statistics. 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.[29] 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.[30] 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 short-time work participated in the Job Statistics only in one of four (4'034 of 16'243) cases. Moreover, the sample overlap is smaller regarding firms

[28] In the estimation sample, 6,772 of the 8,830 establishments (77%) whose short-time work is approved use short-time work. The ITT in column 1 of Table 6 is -0.235. Dividing this by 77%, we get -0.31.

[29] 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).

[30] Our results do not depend on the exclusion of these cases, but they tend to be more precisely estimated.

whose short-time work 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. Note also that the analysis 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 5.2. Rather, we estimate the following simplified variant of it:

$$u_{i,t} = \gamma_t + \gamma_\tau + \beta_1 STW_i + \beta_2 STW_i * I[\tau \geq 0] + \gamma X_{i,t} + \epsilon_{i,t} \tag{2}$$

Equation 2 represents a simple DiD regression model. STW_i is an indicator variable whether an establishment's short-time work 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 (δ_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 short-time work 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 7 reports the results. We find clear evidence that establishment's whose short-time work 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 short-time work 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 of time after the short-time work 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.[31] Moreover, we find corroborating evidence that short-time work approval reduces establishment closures if we use the unemployment data to generate a proxy variable for establishment closures (see appendix Table A.11).

Table 7: 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
	Dropout	Dropout	Dropout	Dropout
VARIABLES	all τ	all τ	all τ	$-4 \geq \tau < 4$
$I[\tau \geq 0]$*STW approved	-0.104***	-0.091***	-0.091***	-0.043***
	(0.035)	(0.029)	(0.029)	(0.016)
STW approved	-0.050**			-0.097***
	(0.023)			(0.030)
Observations	112,016	112,016	112,016	30,441
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	0.950	0.950	0.950	0.952
Number of cases		4,034	4,034	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows DiD estimates of the effect of short-time work 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. Short-time work 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 ($-4 \geq abs(\tau) < 4$).

The fact that denial of short-time work has a large positive impact on the probability to drop out from the Job Statistics implies that our employment regressions face a non-trivial sample selection problem. If we focus on firms with non-missing employment data

[31]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.

(i.e. surviving firms), 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 short-time work keeps them in the treatment group. Hence, surviving control firms are likely to be positively selected. Another empirical challenge for our employment regressions is that there is a sizeable number of microfirms in the dataset. Apart from aggravating the selection problem discussed above, microfirms also pose a problem to the usual way economist look at firm size, which is to use a log-transformed outcome. 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). Our results would likely be downward biased by this problem because there are more microfirms in the control than in the treatment group.

In Table 8, we address these estimation challenges in the following way. We deal with the problem caused by the presence of microfirms 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 2 with and without case fixed effects.

The employment regressions suggest that the approval of short-time work 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 short-time work 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 short-time work on dismissals is close to zero for larger establishments (see section D). Overall, the evidence presented in Tables 7 and 8 suggests that short-time work 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 inflows into and outflows out of the pool of registered unemployed.

6 Cost-benefit analysis

In this section, we use our previous estimates on the impact of short-time work on unemployment to carry out a cost-benefit analysis for the Swiss short-time work scheme. We focus on the *direct financial effects* of short-time work for the unemployment insurance in the year 2009. In particular, we compare the direct financial benefits of short-time work—arising from lower unemployment benefit payments—and compare them to the direct financial costs of short-time work, i.e. the amount of short-time work benefits paid out to the workers covered by short-time work. As we discuss in detail below, such a cost-benefit analysis is necessarily partial, and ignores important potential benefits and costs of the short-time work. It is nevertheless an interesting exercise, as it informs policy makers about the extent to which spending on short-time work benefits is directly compensated by savings in terms of spending on unemployment benefits.

We estimate the net financial benefits of short-time work for the unemployment insurance in Table 9. In the top panel of the table, we compute the direct financial benefits of short-time work. According to our OLS estimates, the approval of short-time work leads to a decrease in 26 daily allowances per employee within three years after the application (Table 3, column 5). Our IV estimates are substantially larger. According to our preferred IV estimation, short-time work approval leads to a decrease in 48 daily allowances per employee after three years (see Table 6, column 5). For the following calculations, we use both, the OLS estimate as a lower and the IV estimate as an upper bound. Multiplying the estimated decrease in the number of daily allowances per employee by the average amount of a daily allowance[32] and the average number of employees per establishment, we get the gross financial benefit of short-time work per case. Since there are 7882 cases that started in 2009, we estimate a gross financial benefit of short-time work in 2009 of CHF 856 Mio or CHF 1'580 Mio, depending on whether we use the OLS or IV estimate.

[32]In our short-time work dataset we have information about short-time work benefits, missed hours due to short-time work 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 a short-time worker would receive in case of unemployment. The value we get is CHF 167. This estimate makes sense. According to SECO (2013) the average daily allowance in 2009 amounted to CHF 137. However, the average amount for men was CHF 155. Since workers covered by short-time work are much more likely to be male and work full-time than the average unemployed, it is no surprise that the average daily allowance for short-time workers is higher than the average.

Table 8: DiD estimates of the effect of short-time work approval on FTE employment

VARIABLES	(1) OLS FTE ≥ 5	(2) FE FTE ≥ 5	(3) OLS FTE ≥ 10	(4) FE FTE ≥ 10	(5) OLS FTE ≥ 25	(6) FE FTE ≥ 25	(7) OLS FTE ≥ 50	(8) FE FTE ≥ 50
Panel A: Missings as missing								
$I[\tau \geq 0]$*short-time work approved	0.004	-0.011	0.112**	0.038	0.068	0.013	0.015	-0.033*
	(0.047)	(0.020)	(0.050)	(0.024)	(0.045)	(0.024)	(0.037)	(0.018)
short-time work approved	0.165***		0.215***		0.297***		0.265***	
	(0.043)		(0.052)		(0.052)		(0.042)	
Observations	61,698	61,698	61,698	61,698	61,698	61,698	61,698	61,698
Panel B: Missings as zeros								
$I[\tau \geq 0]$*short-time work approved	0.047	0.014	0.113***	0.074**	0.069**	0.017	0.023	-0.010
	(0.046)	(0.038)	(0.041)	(0.031)	(0.034)	(0.021)	(0.028)	(0.016)
short-time work approved	0.180***		0.210***		0.258***		0.221***	
	(0.043)		(0.047)		(0.043)		(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 short-time work 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. short-time work 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.

These benefits can be compared to the total spending on short-time work. According to the STW dataset, which contains the total spending on short-time benefits for each case, the unemployment insurance spend 1'256 Mio CHF on short-time work benefits for all cases that started in 2009.[33] The estimated costs and benefits imply that the net financial benefit of short-time work in 2009 is somewhere between CHF -400 Mio (lower bound) and CHF +324 Mio (upper bound). Depending on which estimate one prefers, we thus find that the reduction in unemployment benefit payments compensate two thirds or even overcompensate the total spending on short-time work benefits. Moreover, even the OLS results suggest that the deadweight losses associated with the Swiss short-time work scheme are quite limited, i.e. there are not so many cases in which employers get paid for jobs that they would have retained even if short-time work benefits had not been paid. These results stand in contrast to the macroeconomic estimates reported by Boeri and Bruecker (2011), which suggest large deadweight losses associated with the short-time work scheme, and support the microeconomic estimates provided by the same authors suggesting very moderate deadweight losses. Needless to say, other studies that find no effect of short-time work on dismissals and unemployment also suggest large deadweight effects.

There are further reasons why these results are favoring the cost-effectiveness of the Swiss short-time work scheme. Our focus on the costs and benefits for the unemployment insurance disregards a number of important potential benefits of short-time work:

- Our analysis disregards potential financial benefits of a reduction in unemployment that arise outside of the unemployment insurance. An example is the possible cost savings in other social programs (e.g., social welfare).

- Since short-time work prevents unemployment, it avoids the psychological and social costs associated with unemployment (to the extent that they do not arise for a short-time workers).

- Short-time work may help to avoid losses of human capital caused by unemployment, which would subsequently lead to lower earnings for the affected workers and hence to lower tax revenues for the government.

[33] 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 short-time work benefits in 2009 whereas the former refers to payments made for all cases *starting* in 2009.

- Dismissed workers might reduce their consumption spending more than workers receiving short-time work benefits because dismissed workers are likely to face considerable uncertainty about future earnings. Since short-time work prevents dismissals, it may stabilize aggregate demand through this impact on workers' consumption. Indirectly, short-time work might therefore help to prevent sharper recessions and thus avoid potentially long-lasting costs incurred by the destruction of healthy production units.

- Finally, since short-time work schemes promote work-sharing, they are likely to be more *equitable*. If firms resort to layoffs, the costs of adjustment to recessions are concentrated on a relatively small number of workers who suffer large losses of income and other job-related benefits (Abraham and Houseman, 1994; Cahuc and Carcillo, 2011).

It is important to highlight, that our cost-benefit analysis also disregards potential indirect costs of short-time work. In particular, by preventing the destruction of unprofitable economic structures, by binding capital in unproductive sectors and by hindering efficiency-enhancing labor mobility, short-time work might slow down the structural change from unproductive to productive sectors and thus eventually slow down productivity growth.

Table 9: Cost benefit analysis of the short-time work scheme in 2009

Financial benefits of short-time work	**OLS**	**IV**
Estimated decrease in daily allowances per employee	26	48
Daily allowance per employee when unemployed (in CHF)	167	167
Average number of employees per establishment	25	25
Cost savings per case (in CHF)	108'850	200'400
Total cost savings in 2009 (in CHF)	856 Mio	1'580 Mio
Financial costs of short-time work		
Costs of STW per case (in CHF)	159'300	159'300
Total costs of STW in 2009 (in CHF)	1 256 Mio	1 256 Mio
Net financial benefits of short-time work		
Net financial benefits of STW per case (in CHF)	-50'750	41'100
Net financial benefits of STW in 2009 (in CHF)	**-400 Mio**	**324 Mio**

Notes: The estimated decrease in the number of daily allowances per employee and the costs of short-time work are measured relative to establishments whose short-time work application was denied. The reduction in UI benefit payments per case are calculated by multiplying the estimated decrease in daily allowances per employee by the cost of a daily allowance in CHF and the average number of employees per case. Multiplying this value by 7882 (number of cases that started in 2009) we get the gross financial benefit of short-time work in 2009.

7 Conclusions

This study investigates whether the Swiss short-time work scheme achieved its aim to prevent unemployment during and in the aftermath of the Great Recession. Our analysis exploits that firms in Switzerland have to apply for short-time work at cantonal employment agencies. We merge information from all short-time work applications for the years 2009–2014 with the unemployment register and the Job Statistics. The resulting quarterly establishment-level panel dataset allows us to track dismissals, hirings, and employment of the establishments before and after their application for short-time work benefits. Using a flexible event study model, we find very robust and highly statistically significant evidence that short-time work prevents layoffs. We find that it is mainly workers with compulsory and vocational education whose jobs are saved due to short-time work. In order to test the robustness of our results, we apply an Instrumental Variable (IV) approach that directly exploits the idiosyncrasies in cantonal approval decisions. These IV estimations corroborate our results from the event study estimates. In fact, the estimated negative effects of short-time work on dismissals are even two to three times larger than the corresponding event study estimates. Our cost-benefit calculations based on these results suggest that the direct fiscal benefits of the Swiss short-time work scheme—which arise in the form of a reduction in spending on unemployment benefits—, may in fact be large enough to fully compensate the total fiscal spending on short-time work benefits.

Two important limitations of our analysis have to be kept in mind. The first concerns external validity. We assess the Swiss short-time work scheme during and in the aftermath of the 2008/2009 recession. In Switzerland, this recession was V-shaped, with a sharp downturn and a fast recovery that started already in the third quarter of 2009. The quick recovery may have favored the efficiency of the short-time work scheme. Second, our firm-level estimates do not take into account general equilibrium effects, such as the indirect effects of short-time work on stabilizing aggregate demand. They also do not capture possible long-run effects of short-time work such as a slowdown of the structural change from unproductive to productive firms or the potential benefits from avoiding the destruction of healthy production units. Whether these aggregate costs and benefits 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 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 short-time work application (i.e. $\tau = 0$), as recorded in the application form. We do this because larger firms dismiss more workers. 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.[34]

- **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.[35] 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 short-time work 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 short-time work 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 short-time work 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 $\tau = 0$.

- **FTE employment**: Full-time equivalent (FTE) employment of the establishment of interest in period t. This outcome is directly taken from the JOBSTAT dataset. It is available only for a subsample of establishments (see Table 1).

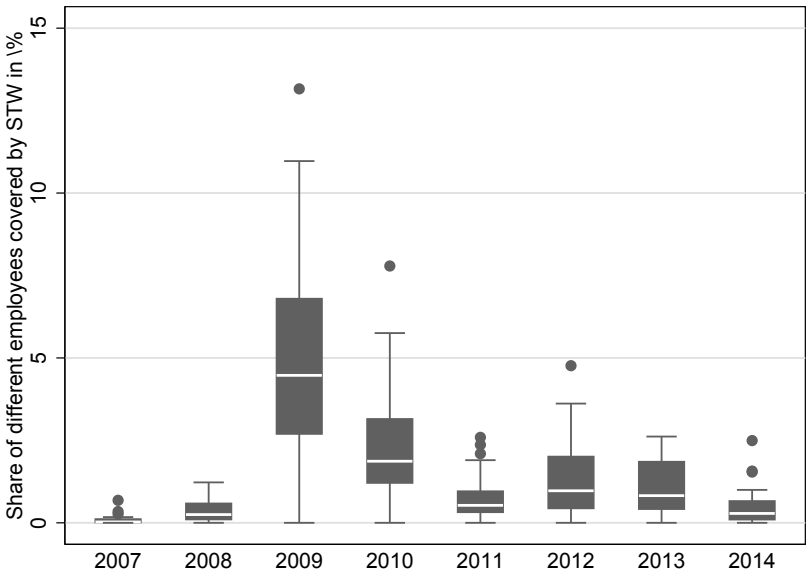
B Descriptive Statistics

Figure A.2a shows the share of all establishments that collect short-time work benefits for at least one worker against event time. The frequency of the dataset is quarterly.

[34] 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.

[35] 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%.

Figure A.1: Box-Whisker-Plot of the cantonal share of employees covered by short-time work (unweighted)

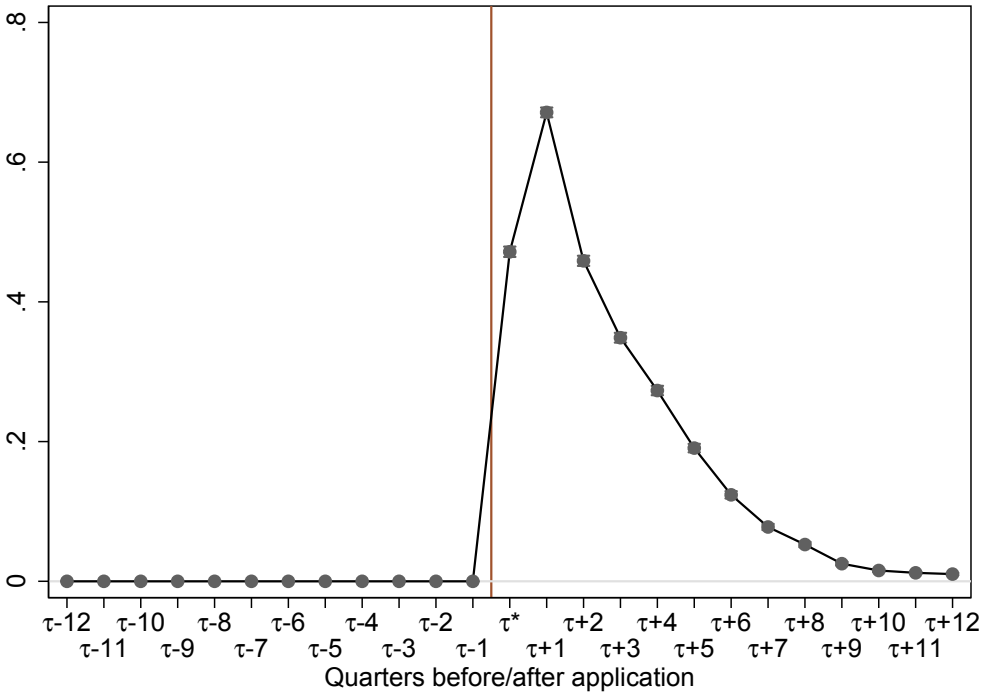


Hence, $\tau = 0$ represents the quarter in which establishments applied for short-time work. We observe that the fraction of establishments that use short-time work increases rapidly and reaches its peak one quarter after the quarter of application (i.e. $\tau = 0$). Then the share tapers off and reaches zero after ten quarters.[36] We observe similar trajectories for the share of employees covered by short-time work as well as the share of missed hours in the normal hours of an establishment (Figures A.2b and A.2c). More than 40 percent of the total workforce of an establishment are covered by short-time work benefits shortly after the application.[37] Since not all of these workers reduce their working time by 100%, the share of missed hours in normal hours is smaller—slightly less than 25% one quarter after application.

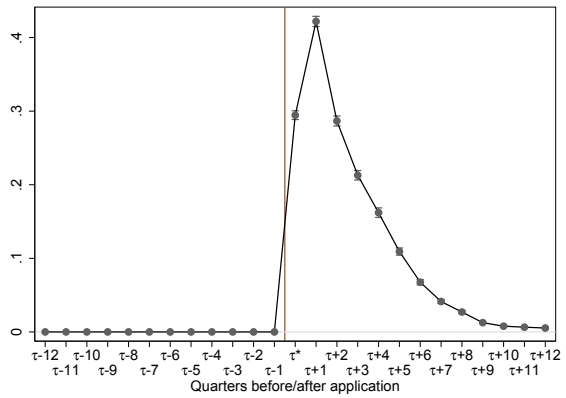
[36]The reason why some establishments are still using short-time work benefits after 24 months (which is the maximum legal duration of short-time work benefits collection) is that these establishments interrupted their short-time work usage for some months and are therefore allowed to collect short-time work benefits even 25 or 26 months after application.

[37]Note, that this figure includes all establishments with approved applications, also establishments that did not make use of short-time work benefits. The same holds for the share of missed hours.

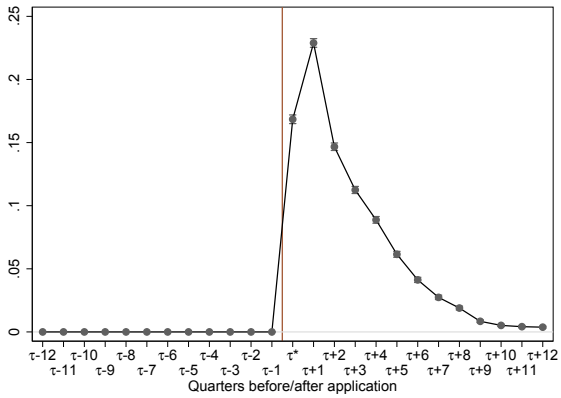
Figure A.2: Intensity of short-time work use



(a) Share of firms taking up STW



(b) Employees covered by short-time work



(c) Share of missed hours

Notes: Panel (a) reports the average share of establishments with approved short-time work benefit applications that actually collects short-time work benefits in a given quarter. Panel (b) depicts the average share of workers that is covered by short-time work benefits in an establishment's total employment at application. Panel (c) displays the average share of missed hours due to short-time work in an establishment's total normal hours. In panel (b) and (c), establishments that did not use short-time work benefits are included. For these firms, the shares are set to zero.

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 short-time work 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

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

C Application and approval decision

C.1 Application decision

To what extent do establishments that apply for short-time work benefits differ from those that do not?

Table A.4 reports the results of probit regressions that are based on a cross-section of establishments from manufacturing and trade in the years 2005 to 2010. The outcome variable is a dummy equal to one if an establishment applies for short-time work benefits during that period. Overall, 20% of all establishments applied for short-time work. The covariates in the probit regressions are firm characteristics that are mostly taken from JOBSTAT.[38]

According to Table A.4 the propensity to apply for short-time work 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 short-time work benefits. Establishments reporting difficulties in recruiting workers have a slightly higher probability to apply for short-time work benefits than establishments without problems to find adequate personal. If recruitment of appropriate workers is difficult, the human capital of the existing workforce may be more relevant to the establishment. Hence, firms 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 short-time work applications—play a small role in explaining the cross-sectional variation in the probability that an establishment applies for short-time work. 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).[39] 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.

[38] 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.

[39] Note, that there is a positive correlation between establishments take-up of short-time work 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.

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
Firmsize				
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***
Other variables				
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

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

C.2 Approval decision

Table A.5 reports the marginal effects of probit regressions of the approval probability in the years 2007 to 2014 on different firm and labor market characteristics as well as on features of the short-time work application process. The firm characteristics used in the regressions are reported on the application form for short-time work.The columns further to the right control for an increasing set of fixed effects. Note, that establishments with a "high importance" for the regional labor market employ more than 0.31 percent (= 95th percentile of the distribution) of the whole labor force in a NUTS-III-region. Establishments that are "not important" employ less than 0.059 percent (= 50th percentile of the distribution) of the labor force in a NUTS-III-region.

The normalized cantonal unemployment rate is constructed as the ratio of the cantonal unemployment rate in the respective quarter to the average unemployment rate over the years 2007 to 2014. As numerator of the yearly cantonal unemployment rate we use the average of the cantonal labor force over the years 2010 to 2011 since we lack data for the years prior to 2010. The normalized number of establishments that apply at the same time is construct by taking the ratio of the number of applications within a 2-week period around the application date to the average number of applications during a 2 week period in the same canton between 2007 and 2014.

Table A.5: Effect of firm and region characteristics on approval probability

	Model 1	Model 2	Model 3	Model 4
Firmsize				
0 to 4 employees	ref.	ref.	ref.	ref.
5 to 9 employees	.055***	.053***	.052***	.061***
10 to 19 employees	.068***	.063***	.062***	.069***
20 to 100 employees	.095***	.093***	.09***	.1***
>100 employees	.11***	.11***	.11***	.12***
Industry				
Manufacturing	ref.			
Construction	-.15***			
Other 2. Sector	-.073*			
Trade	-.1***			
Other 3. Sector	-.11***			
Importance reg. labor market				
Not important	ref.	ref.	ref.	ref.
Of medium importance	.0057	.014	.023**	.022**
Of high importance	.013	.028	.046**	.051**
Other variables				
No of departments that applied	.034***	.026***	.022**	.02**
Establishment applied before	-.11***	-.11***	-.095***	-.1***
Establishment received stw in past	.24***	.25***	.22***	.23***
Norm. cantonal unemployment rate	.13***	.16***	.13***	
Norm. no estab that applied at same time	.018***	.017***	.0091***	
Period FE	Yes	Yes	Yes	Yes
Industry-period FE (NOGA2)	No	Yes	Yes	Yes
Canton FE	No	No	Yes	Yes
Canton-period FE	No	No	No	Yes
Observations	18615	15628	15552	14117
Adj_R2_McFadden	.23	.18	.24	.23

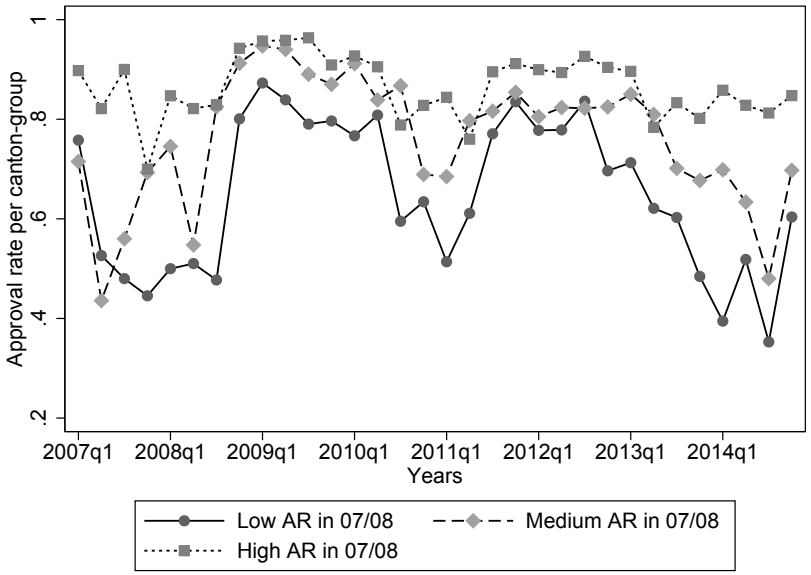
Note: The table reports the average marginal effects of different firm and labor market characteristics on the approval probability in the years 2007 to 2014 applying a probit model.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: SECO, STW dataset

Table A.6: Cantonal (fixed) effects on approval probability

	Cantonal Coefficients
ZH	ref.
BE	-.099***
LU	.12***
UR	0
SZ	.012
OW	-.00072
NW	.056
GL	.17***
ZG	.058***
FR	-.25***
SO	.039**
BS	.098***
BL	.078***
SH	.083***
AR	.097***
AI	.11**
SG	-.018
GR	.018
AG	.062***
TG	.06***
TI	-.11***
VD	-.11***
VS	-.026*
NE	-.053***
GE	-.034**
JU	.12***
Control Var model 3	Yes
Observations	15552
Adj R2 McFadden	.24

Note: The table reports the average marginal effects of the different cantons on the approval probability in the years 2007 to 2014 applying probit model 3 from the previous regression.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
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, Neuchâtel. 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.

D Heterogeneity of the effect of short-time work

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

Table A.7 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 short-time work. The estimated effects on the share of dismissed workers are quantitatively much lower for the other groups of workers.[40]

[40]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 short-time work. Part of the reason for the lower effect is thus that high-qualified workers represent a smaller share in the workforce of firms in general.

Table A.7: Effect of short-time work approval on share of dismissed workers by highest educational attainment

	(1) compuls	(2) vocat educ	(3) upper sec	(4) prof educ	(5) univers educ
Appl. $\tau-8$ *to* $\tau-4$	-0.012	0.001	0.002	-0.003	0.000
	(0.008)	(0.008)	(0.003)	(0.002)	(0.002)
Appl. $\tau-3$ *to* $\tau-1$	-0.000	-0.001	0.001	-0.001	-0.002**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)
Appl. τ *to* $\tau+3$	-0.019***	-0.031***	-0.003	-0.003**	-0.005***
	(0.005)	(0.007)	(0.002)	(0.002)	(0.002)
Appl. $\tau+4$ *to* $\tau+8$	-0.018***	-0.013*	0.000	-0.001	-0.002
	(0.007)	(0.007)	(0.003)	(0.002)	(0.002)
Appl. $\tau+9$ *to* $\tau+12$	-0.018***	-0.013*	0.002	-0.002	-0.001
	(0.006)	(0.007)	(0.002)	(0.002)	(0.001)
Appl. τ *to* $\tau+12$	-0.055***	-0.057***	-0.001	-0.006	-0.008**
	(0.016)	(0.019)	(0.007)	(0.005)	(0.004)
N	389242	389242	389242	389242	389242
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. Baseline controls are period, event time, and case fixed effects. The table lists the sum of coefficients for indicated intervals. * p < 0.1, ** p < 0.05, *** p < 0.01.

We now turn to the analysis whether the effect of short-time work on dismissals is larger for some groups of firms. In order to provide a straightforward analysis of this question, we use a simplified regression model here. We first restrict the sample to firms that are present in all periods within ±4 quarters to the application for short-time work.[41] For these firms, we then sum up all dismissals and all new hires in the four quarters before 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 short-time work was approved, i.e. we estimate

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

[41]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.

The coefficient β 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.[42] In order to see whether the effects of short-time work approval are different for different firms, we estimate this regression for different subsamples of firms.

Tables A.8 and A.9 show the results of this exercise. Table A.8 presents the effects of short-time work approval for broad industry groups (manufacturing, construction, trade, and other service sector industries). We observe similar negative effects of short-time work 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.9). The effects are much more sizeable for small firms. There are no differences in dismissals in the following year between large firms (with more than 50 employees) whose application is approved and large firms whose application is denied.

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

[42] 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.8: Effect of short-time work approval on net share of dismissed workers, by broad industry

VARIABLES	(1) OLS Manufac-turing	(2) OLS High-tech manuf.	(3) OLS Low-tech manuf.	(4) OLS Const-ruction	(5) OLS Trade	(6) OLS Other services
STW approved	-0.053***	-0.130***	-0.029	-0.052***	-0.071***	-0.057***
	(0.017)	(0.045)	(0.018)	(0.016)	(0.022)	(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. Short-time work approved is a dummy variable indicating approval of an STW application. The only controls are period fixed effects. High-tech manufacturers (column 2) are firms 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 firms from all other manufacturing industries. The "share approved" shows the fraction of firms in the respective subgroup whose short-time work application was approved.

Table A.9: Effect of short-time work approval on net share of dismissed workers, by firm size

VARIABLES	(1) OLS size <10	(2) OLS size 10-19	(3) OLS size 20-49	(4) OLS size 50+
STW approved	-0.072***	-0.030***	-0.019*	0.001
	(0.011)	(0.010)	(0.010)	(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. Short-time work approved is a dummy variable indicating approval of a short-time work application. The only controls are period fixed effects. The "share approved" shows the fraction of firms in the respective subgroup whose short-time work application was approved.

E Further robustness checks

Table A.10: 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
Appl. $\tau-8$ *to* $\tau-4$	0.008	0.000	0.000	-0.006	0.004	0.003
	(0.012)	(0.012)	(0.011)	(0.013)	(0.012)	(0.010)
Appl. $\tau-3$ *to* $\tau-1$	0.002	-0.001	-0.001	-0.007	0.001	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Appl. τ *to* $\tau+3$	-0.057***	-0.054***	-0.059***	-0.080***	-0.055***	-0.057***
	(0.010)	(0.010)	(0.009)	(0.011)	(0.010)	(0.008)
Appl. $\tau+4$ *to* $\tau+8$	-0.015	-0.024**	-0.025**	-0.025*	-0.021*	-0.022**
	(0.012)	(0.012)	(0.011)	(0.013)	(0.012)	(0.010)
Appl. $\tau+9$ *to* $\tau+12$	-0.006	-0.018*	-0.019**	-0.022**	-0.017*	-0.014*
	(0.010)	(0.009)	(0.009)	(0.011)	(0.010)	(0.008)
Appl. τ *to* $\tau+12$	-0.079***	-0.096***	-0.103***	-0.128***	-0.092***	-0.093***
	(0.029)	(0.028)	(0.026)	(0.031)	(0.028)	(0.024)
N	227697	315605	316305	239340	309564	347217
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 dependent variable is the difference between new job seekers and hired job seekers (net hires), expressed as a fraction of the firm size reported at short-time work application. Baseline controls are period event time, and case fixed effects. The table lists the sum of coefficients for indicated intervals. Column 1 disregards short-time work application made in 2009. Column 2 is restricted to firms observed over the entire period from $t-4$ to $t+9$. Column 3 is restricted to firms with at most 25 workers at registration. Column 4 disregards firms that are observed to call back some of their workers. Column 5 is restricted to the first application of a firm. Column 6 disregards applications from firms that applied for short-time work for several departments. Column 7 is restricted to the BESTA sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: OLS estimates of firm-closure probability

	After 1Y	After 2Y	After 3Y
STW denied	ref.	ref.	ref.
STW approved	-.0024	-.0076**	-.02***
0 to 9 employees	ref.	ref.	ref.
10-19 employees	-.0021**	-.0095***	-.014***
20-49 employees	-.0025*	-.01***	-.012***
Establishment characteristics	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes
Observations	14667	12974	11008
Mean firm closure prob	.0038	.012	.02

Notes: Only establishments with less than 50 employees. As soon as the number of registered unemployed (minus new hires) is larger than the firm's employment at application, we consider the firm as closed.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SECO, STW dataset