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**Research Report**

## Does short-time work prevent unemployment?

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## Report

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## Does Short-time Work Prevent Unemployment?

Daniel Kopp and Michael Siegenthaler

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# Imprint

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# Does Short-Time Work Prevent Unemployment?

Study commissioned by the Supervisory Committee of the Equilibration  
Fund of the Swiss Unemployment Insurance

KOF Swiss Economic Institute, ETH Zurich

Daniel Kopp and Michael Siegenthaler\*

March 5, 2018

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# 1 Introduction

Many governments aim at mitigating the impact of recessions on unemployment. One instrument that gained widespread popularity during the severe economic downturn 2008–2009 is short-time work (STW) programs. These programs are public schemes that aim at preserving jobs by encouraging firms facing a temporary drop in demand to reduce work hours instead of laying off their workers. The purpose is to distribute the burden of recessions to a larger number of workers and to avoid dismissals of workers who are redundant in the short run but would be viable in the longer run. In order to make this solution attractive to employers and employees, short-time work schemes provide income support to workers whose hours are reduced. In the 25 countries that operated a short-time work scheme during the crisis, take-up rates increased substantially, covering temporarily over 4.5 million workers across the OECD (Hijzen and Martin, 2013). In Switzerland more than 90'000 workers, accounting for roughly 2 percent of the total workforce, were covered by short-time work mid-2009 (Hijzen and Venn, 2011). The Swiss unemployment insurance spent more than 1.1 billion Swiss Francs on short-time work benefits in 2009 (SECO, 2013).

However, short-time work schemes are no panacea. Their effectiveness may be hampered by deadweight and displacement effects (Hijzen and Venn, 2011). The former occur when short-time work subsidies are paid for jobs or working hours that employers would have retained even if short-time work benefits had not been paid. Displacement effects occur when short-time work schemes preserve jobs that are not viable without the subsidy even after business conditions recover. In this case, short-time work schemes may only postpone dismissals of workers rather than preventing them, and may lock in workers in unviable jobs in the meantime. Given the potential for negative side effects of short-time work programs and the large amount of public funds invested in short-time work, it is crucial to evaluate the impact and efficiency of short-time work schemes.

This study investigates whether the Swiss short-time work scheme helped to prevent unemployment during and in the aftermath of the Great Recession. To this end, we match data covering the universe of short-time work applications for the 2009–2014 period with the Swiss unemployment register data and the Swiss Job Statistics. The resulting establishment level panel dataset allows us to track dismissals, hirings, and employment of Swiss establishments before and after their application for short-time work benefits. Because cantonal agencies, responsible for handling these applications, deny short-time work applications in about 1 of 6 cases, we compare firms that applied for short-time

work and received short-time work benefits with firms that applied for short-time work but whose application was denied. We address selection during the approval decision by estimating an event-study Difference-in-Difference model with fixed-effects and an instrumental variable model that exploits the substantial idiosyncracies in the decision process within and across cantons.

This report contributes to an international literature evaluating short-time work schemes in different countries, including Switzerland. The results of these evaluations differ widely. The majority of country- or state-level evaluations suggest that short-time work schemes stabilize employment and reduce dismissals (Hijzen and Venn, 2011; Boeri and Bruecker, 2011; Cahuc and Carcillo, 2011; Abraham and Houseman, 2014). On the other hand, the relatively few existing firm-level studies are much less optimistic. Some studies even report that short-time work schemes increase unemployment and decrease employment (Calavrezo et al., 2009, 2010). The prior studies on the Swiss short-time work scheme reach the same negative conclusion (Frick and Wirz, 2005; Hollenstein and Marty, 1996; Frick et al., 1989). Rather than preventing unemployment, short-time work indemnity seems to increase layoffs by firms or be at best ineffective.

Yet, most of the existing evaluations of short-time work schemes have conceptual limits. On the one hand, cross-country studies usually compare employment or unemployment across countries that differ in their short-time work usage. The problem with this comparison is that short-time work usage is likely to be higher in countries in a strong downturn. The existing studies address the potential bias using instrumental variable estimations. But the validity of the instrumental variables may often be questioned. Moreover, as is often the case in cross-country studies, the results may be affected by failures to account for factors, which correlate with both, short-time work usage and changes in employment or unemployment. The existing firm-level studies rely on the comparison of firms, which receive short-time work benefits with firms that do not receive short-time work benefits. The central problem of these studies is that firms that recourse to short-time work benefits are likely to differ in observed and unobserved ways from firms that do not make use of short-time work benefits—after all, there are economic reasons why some firms apply for short-time work benefits and others do not. In fact, the counterintuitive result that short-time work increases rather than reduces unemployment could be a direct consequence of these studies' failures to compare firms that are in similar or, at best, identical situations.

Our study likely circumvents or at least mitigates some of these problems due to its focus on a sample of firms that all applied for short-time work. Hence, we avoid the

above mentioned self-selection problem which arises when comparing firms that applied for short-time work benefits with firms that did not. However, we still have to deal with selection during the decision process. Two observations help us to address this concern: We find large cantonal differences in approval practices that can not be accounted for by observable characteristics of applying establishments. Moreover, we find no systematic differences in the evolution of dismissals in the period *before* treatment if we compare firms that applied for short-time work and received it to firms that applied for short-time work and were denied. Based on these insights, we conduct a simple before-after comparison of firms depending on whether their short-time work application was approved or not. The resulting Difference-in-Differences (DiD) estimates that control for fixed effects for each short-time work case provide strong and very robust evidence that the Swiss short-time work scheme prevents dismissals. We reach the same conclusions with a set of Instrumental Variable (IV) regressions that directly exploit the idiosyncrasies in cantonal approval decisions. Using our DiD and IV models, we quantify the direct fiscal benefits of the Swiss short-time work scheme, which arise in the form of a reduction in spending on unemployment benefits. Our back-of-the-envelope computations suggest that the direct fiscal benefits may in fact be large enough to fully compensate the total fiscal spending on short-time work benefits in the Swiss case.

### Research Questions

This report answers the following research questions: What factors influence the approval decisions of cantonal employment agencies when handling short-time work applications? Does the Swiss short-time work scheme prevent dismissals or does it only postpone it? Does the effect of short-time work on unemployment differ across industries, firm size, and the expected demand shortfall? What are the educational credentials of workers whose jobs are saved by STW benefits? How many firms use short-time work until the legal maximum and do these firms dismiss workers when reaching this maximum? And finally: to what extent do the direct financial benefits of short-time work in terms of saved unemployment benefits cover the country's spending on short-time work benefits?

This report is organized as follows. Section 2 reviews the empirical and theoretical literature on short-time work programs. Section 3 characterizes the Swiss short-time work scheme, shows short-time work take-up in Switzerland over time, and illustrates that short-time work was of central importance in certain regional labor markets during the 2008-2009 recession. In section 4, we present the dataset used in the empirical analysis. Section 5 presents descriptive evidence on the data, take-up, and approval decisions of

the cantonal offices. In Section 6, we study in detail whether the Swiss short-time work prevented unemployment during and in the aftermath of the Great Recession. Section 7 uses the results from this evaluation in order to make an assessment of the direct costs and direct financial benefits of the Swiss short-time work scheme. Section 8 summarizes our main findings and concludes.

## 2 Literature review

The following overview presents the most important empirical studies investigating the effects of short-time work on employment and unemployment in the last 15 years. We discuss country-level and firm-level studies separately.

### 2.1 Country-level studies

Studies assessing the employment effects of short-time work schemes at the country-level usually relate the usage of short-time work programs to changes in employment or unemployment in the country. These studies differ primarily in terms of geographical and time coverage, and in terms of the strategy how they account for the endogeneity of short-time work. The latter refers to the econometric challenge that short-time work take-up likely depends on the labor market conditions in the countries. For instance, if short-time work take-up increases when the economy enters a recession, the observed relationship between short-time work and unemployment may primarily reflect policy or firm responses to the worsening of the economic conditions, rather than the causal effect of short-time work use on the employment—which is what we are interested in.

Hijzen and Venn (2011) compare the evolution of employment and average hours across 19 OECD countries with differences in short-time work take-up during the 2008–2009 crisis. They conclude that short-time work schemes helped to preserve jobs during the crisis, with the largest impact in Germany and Japan. However, the positive effect of short-time work on employment was restricted to permanent workers. In Hijzen and Martin (2013), the authors update their prior study by extending the dataset to 2010 and the country coverage to 23. To overcome the endogeneity of short-time work, they use the age of the short-time work program in a country as an instrumental variable for short-time work use. This approach is based on the idea that the age of the short-time work program does not relate to the country-specific business cycle but does have a positive effect on short-time work take-up because more employers are aware of its existence. Under the

assumption that the age of the short-time work program is not correlated with changes in employment except through its impact on short-time work take-up, they find that short-time work make employment and unemployment less sensitive to fluctuations in output. Their simulations suggest that short-time work schemes had a significant impact on preserving jobs during the crisis. However, the impact of short-time work schemes on employment was much smaller during the recovery following the crisis.

Similar results are reported by Arpaia et al. (2010). The authors evaluate the effect of short-time work benefits in the manufacturing sector during the 2008–2009 recession by means of a panel estimation with country fixed effects. Their dataset covers 27 European countries. Their results indicate that short-time work schemes had a significant positive effect on employment growth.

Boeri and Bruecker (2011) provide macro and micro evidence of the employment effect of short-time work. Their cross-country evidence draws on data from 16 OECD countries, including Switzerland. Similar to Hijzen and Martin (2013), they take the endogeneity of take-up rates into account by instrumenting short-time work take-up with the time elapsed since the first introduction of a short-time work scheme in a given country. Their estimates imply a positive employment effect of short-time work. However, they find that short-time work only prevents job losses if the output drop is larger than 2.6 percent.

Cahuc and Carcillo (2011) assess the effect of changes in short-time work take-up rates on unemployment and employment in 25 OECD countries. To account for endogeneity, they instrument take-up rates by institutional features of a countries' short-time work programs before the entry of the recession. The identifying assumptions are that take-up rates are positively correlated with the short-time work program generosity prior to the crisis, and that the features of short-time work programs before the crisis are not correlated with employment and unemployment other than through affecting take-up of short-time work. Their estimates indicate a negative effect of short-time work take-up rates on unemployment and a positive effect on employment. As Hijzen and Venn (2011), they find the positive effects to be restricted to permanent workers.

Hertweck and Brey (2016) also allow for non-linear effects of short-time work benefits on unemployment and test whether the effectiveness of short-time work depends on the business cycle. They find that short-time work dampens unemployment, but the dampening effect of short-time work on the unemployment rate diminishes at higher take-up rates. Moreover, they find that only countries with pre-existing short-time work schemes were able to fully exploit the benefits of short-time work. In line with the results of Boeri

and Bruecker (2011), they find the effects of short-time work to be strongest at very negative GDP growth rates.

Abraham and Houseman (2014) compare the responsiveness of employment to changes in output for manufacturing production workers in US states with and without short-time work schemes during the crisis in 2008–2009. They account for state and time fixed-effects and allow for different effects in the pre-recession period from 2006 to 2007 and the recession period 2008–2009. They find that manufacturers in states with short-time work schemes relied relatively more on hours reduction and less on employment reduction to adjust total hours worked. These effects are larger, the larger the number of full-time-equivalent workers drawing short-time work compensation in a state.

## 2.2 Firm-level studies

Firm-level studies have a major advantage compared to country- and state-level studies: there are less unobserved factors that could confound the relationship between short-time work and employment or unemployment. The main challenge for firm-level studies is that short-time work benefits are all but randomly assigned to establishments. Firms that apply for short-time work benefits are most likely in a more difficult economic situation than firms that do not. Researchers that compare establishments with and without short-time work may thus observe higher layoffs for firms with short-time work benefits relative to firms without, which they may falsely interpret as evidence that short-time work benefits lead to layoffs. In general, researchers likely underestimate the beneficial effects of short-time work benefits if they do not adequately address the problem that establishments applying for STW are negatively selected. In fact, they may even find that short-time work benefits lead to dismissals.

The existing firm-level studies apply different methods in order to overcome the potential selection bias. Focusing on the 1996–2004 period, Calavrezo et al. (2009) investigate the relationship between short-time compensation and the number of redundancies in French establishments. They apply a two-stage selection model that incorporates an estimation of the selection into short-time work take-up. They reach the counterintuitive conclusion that short-time work leads to more layoffs. In a follow-up study (Calavrezo et al., 2010), they study the impact of short-time work compensation on establishment exit. They apply propensity score methods to match observationally equivalent firms that did not draw short-time work benefits to firms that used short-time work benefits.

The results again point towards a negative effect of short-time work programs, i.e. an increased likelihood of market exit for firms that made use of short-time work benefits.

Boeri and Bruecker (2011) and Kruppe and Scholz (2014) both investigate the effect of short-time work on employment in German firms during the crisis 2008–2009. Kruppe and Scholz (2014) use propensity score matching to address the problem of firms’ self-selection into treatment. Their results indicate no significant effect of short-time work on employment. Boeri and Bruecker (2011) apply an instrumental variable approach. They argue that the Great Recession is by and large uncorrelated to previous business cycle shocks in Germany. They thus use a firm’s take-up rates in 2003 and 2006 as instrument for short-time work take-up in 2009. They find a significant positive effect of the share of short-time workers in a firm’s workforce on the change in employment.

The Swiss short-time work scheme has also been subject to evaluations. The newest one is Frick and Wirz (2005). Their analysis is based on data from the monthly and quarterly KOF business tendency surveys and a special survey about the use of short-time work by Swiss firms. Using these survey datasets, they assess whether short-time work allowances increased the probability that firms hoard labor during the crisis 2001–2003. To make firms that used short-time work and those that did not more comparable, they focus on firms that experienced a substantial decline in incoming orders during several quarters, and subsequently match treated and untreated firms based on the estimated propensity score. As in Calavrezo et al. (2009, 2010) their results indicate that short-time work indemnity increases (rather than decreases) the risk that firms reduce employment. The older Swiss studies (Frick et al., 1989; Hollenstein and Marty, 1996) did not find positive employment effects either.<sup>1</sup>

## 2.3 Summary and critical assessment

Do short-time work schemes stabilize employment and prevent unemployment? The existing empirical literature yields mixed results on this question. Most macroeconomic evaluations find a positive effect of short-time work benefits on employment and a negative on unemployment, with some evidence that these effects are restricted to permanent workers and that they are larger in sharp downturns. By contrast, with the exception of Boeri and Bruecker (2011), the existing firm-level studies suggest that short-time work

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<sup>1</sup>These studies also suffer from certain data limitations. For instance, Frick and Wirz (2005) have only qualitative information about the quarterly evolution of a firms’ employment and incoming orders. Furthermore, the sample on which the authors base their calculations is quite small (only 513 firms).

schemes do not prevent job losses. A non-negligible share of studies even report that short-time work schemes increase layoffs.

Both, macro- and micro-level studies have advantages and disadvantages. One of the advantages of macroeconomic evaluations is that they measure the net global impact of short-time work on employment or unemployment, considering all possible channels and taking into account general equilibrium effects. In contrast to firm-level evaluations, they also capture the indirect effects of short-time work arising from stabilizing aggregate demand. However, country- or state-level evaluations have to deal with some major challenges, too: First, the conclusions of macroeconomic studies are necessarily drawn from relatively small samples, which limits the ability to identify causal effects (Cahuc and Carcillo, 2011). Second, country-level studies are susceptible to endogeneity problems since the short-time work take-up rates are likely to depend on the evolution of employment and unemployment in a country. Another concern is reverse causality, as policy makers often increase the scale and generosity of short-time work schemes if the country is hit by a particularly severe economic downturn.<sup>2</sup> The existing studies usually address these concerns by applying instrumental variable methods. But it remains questionable whether the assumptions allowing for a causal interpretation of the results are met. In particular, the estimates may be biased due to failures to account for time-varying factors that systematically correlate with both, short-time work usage and changes in employment or unemployment. Third, despite significant efforts to improve the cross-country comparability of short-time work data, substantial problems remain due to differences in program coverage, the way take-up is recorded and differences in the corresponding reduction in working time (Hijzen and Venn, 2011).

Firm-level studies avoid some of these problems since they usually draw on much larger samples and are restricted to firms within the same economy and the same institutional framework. However, they generally struggle to cope with the fact that firms using short-time work likely differ in unobserved dimensions from firms that do not. The matching approaches applied in some studies are particularly susceptible to the omission of non-observed variables that affect short-time work take-up and the outcome variables simultaneously. The instrumental variable approaches, on the other hand, are based on assumptions about exclusion restrictions that may fail. These concerns are aggravated if data limitations restrict the sample to few observations or poorly measured variables. Moreover, the existing literature usually ignores that governmental bodies

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<sup>2</sup>In order to incentivize firms to take-up short-time work, Switzerland relaxed the eligibility criteria for short-time work in April 2009. Similar measures were taken in Germany, France, and Japan.



generally decide on whether to grant short-time work benefits or not, which may lead to further selection problems. These issues may explain why many firm-level studies reach the counterintuitive conclusion that participation in a short-time work scheme increases firms' layoffs.

## 3 Short-time work in Switzerland

### 3.1 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<sup>3</sup> 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 years.<sup>4</sup> 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. According to the law, establishments are eligible to short-time work benefits if the working-time reduction is presumed to be temporary and if it can be expected that the temporary reduction in working hours helps preserving jobs. 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 the accounting period. In contrast, there is no right for short-time work benefits if the working-time reduction is due to circumstances that are part of the usual operational risk of a firm, if the working-time reduction is customary in the respective industry, occupation or firm, and if the working-time reduction is due to normal seasonal demand fluctuations. Workers with a temporary contract, temporary agency workers, and trainees are excluded from short-time work benefits. The cantonal employment agencies and the

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<sup>3</sup>The maximum insured income during our sample period was CHF 126'000.

<sup>4</sup>However, the Swiss government can depart from these legal provisions if exceptional circumstances (e.g. high unemployment) require action. It can extend the maximum duration of short-time work benefits and reduce the costs of employers.

unemployment insurance are responsible for deciding whether a specific establishment meets these eligibility criteria.

The government changed certain legal provisions regarding short-time work during our sample period. In 2009 and 2010, 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. But in December 2011, it was reduced again to 18 months and at the end of 2013 to its normal level of 12 months. By December 2013, the cost reduction for employers expired too. 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.

### **3.2 Comparison to other short-time work schemes**

The Swiss short-time work scheme is similar to those in other countries in many respects.<sup>5</sup> First, as in the majority of other countries with short-time work schemes, Swiss firms have to give a justification of economic need in order to be eligible for short-time work. Second, Swiss firms bear part of the costs induced by short-time work by covering short-time work benefits for the first few days each month of short-time work benefit collection. Many other countries have similar regulations that require firms to cover part of the wages of short-time workers.<sup>6</sup> In terms of the costs to employers for engaging in short-time work, Switzerland ranks in the middle in an international comparison. Finally, in line with the institutional settings in countries such as the US or Canada, Switzerland does not require short-time workers to engage in compulsory training or job search activities, does not require a recovery plan of the firm nor does it prohibit the dismissal of workers

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<sup>5</sup>The comparison in this section draws heavily on Hijzen and Venn (2011) and Boeri and Van Ours (2013).

<sup>6</sup>In France, Japan, Poland or Portugal firms have to bear a fraction of the wage costs for hours not worked. In the United States, firms do not participate directly in the financing of short-time work but they may face higher unemployment-insurance premiums as a result of participating in short-time work. In contrast, employers in Belgium, Canada, Denmark, Finland, Ireland and Spain do not bear any costs of short-time work.

during the time of short-time work benefits usage and does not restrict the maximum hours reduction per worker.<sup>7</sup>

Switzerland's short-time work scheme 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 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 circumstances is close to the international average.

### **3.3 Short-time work during and after the Great Recession**

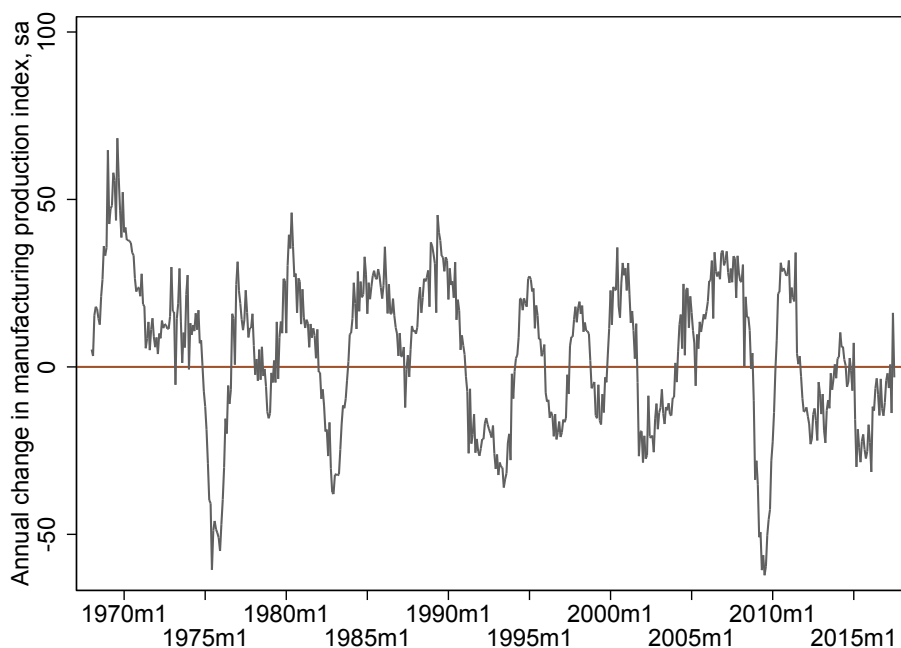
Switzerland's export-oriented sectors were strongly hit by the Great Recession. Value added in the banking and manufacturing sector dropped by more than 10 percent in 2009. In hotels and restaurants, value added fell by more than 5 percent. As the index of manufacturing production of the KOF Business tendency surveys shows, manufacturing production experienced the largest drop since the mid 1970s (see Figure 1). 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 2 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 received short-time work benefits. As is illustrated by Figure 2, take-up was largest among manufacturers since the manufacturing sector was most strongly and most immediately affected by the drop in international demand. The take-up of short-time work was supported by the Swiss government by relaxing the eligibility criteria for short-time work in April 2009. In 2009, the Swiss unemployment insurance spent more than 1.1 billion Swiss francs for short-time work benefits (SECO, 2013). 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

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<sup>7</sup>Germany, Finland, Ireland, and Norway depart from these provisions only by requiring affected workers to engage in active job search during the time they are not working.

Figure 1: Seasonally adjusted, annual change of the manufacturing production index



Source: KOF business tendency surveys.

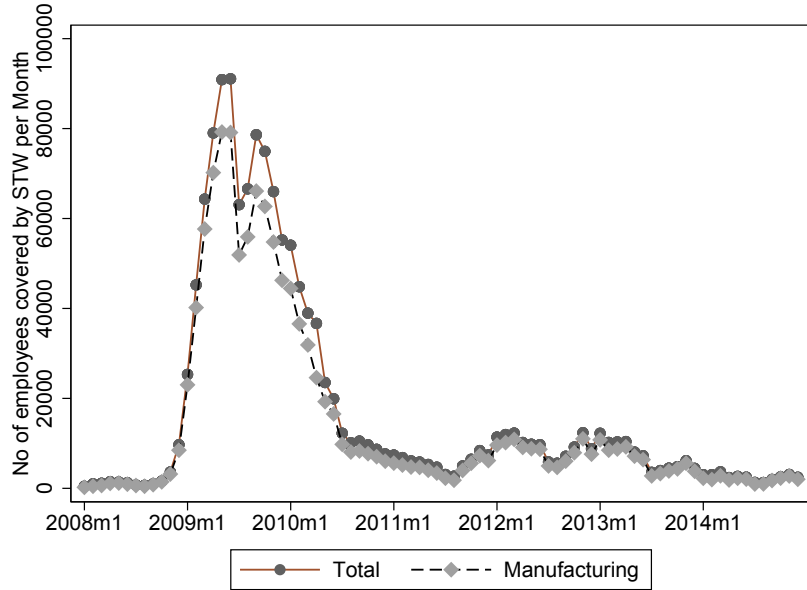
strong real appreciation of the Swiss franc, which put strong competitive pressures on Swiss exporters. The Swiss National Bank reacted by introducing an exchange rate peg of 1.20 for the Swiss franc relative to the Euro. Throughout 2012, 10'000 workers were covered by the short-time work scheme. Short-time work coverage declined substantially from mid-2013 onward.

### 3.4 Regional relevance of short-time work

These aggregate numbers on the use of short-time work disguise large regional differences. Figure 3 reports the distribution of the share of total cantonal employment covered by short-time work for different years using a Box-Whisker-Plot. In the average canton, the coverage rate in 2009 was slightly less than 5 percent. However, there is a large heterogeneity across cantons. In the cantons of Jura and Neuchatel, 13 and 11 percent of all workers, respectively, were covered by short-time work in 2009.

Not surprisingly, the regional differences are even larger if we compare regional labor markets, so-called NUTS-III regions (similar to commuting zones). In Switzerland, there are 106 NUTS-III regions in total. Table A.1 in the Appendix reports summary statistics of the share of employees covered by short-time work in a regional labor market. In

Figure 2: Employees covered by short-time work benefits per month



Source: SECO, STW dataset

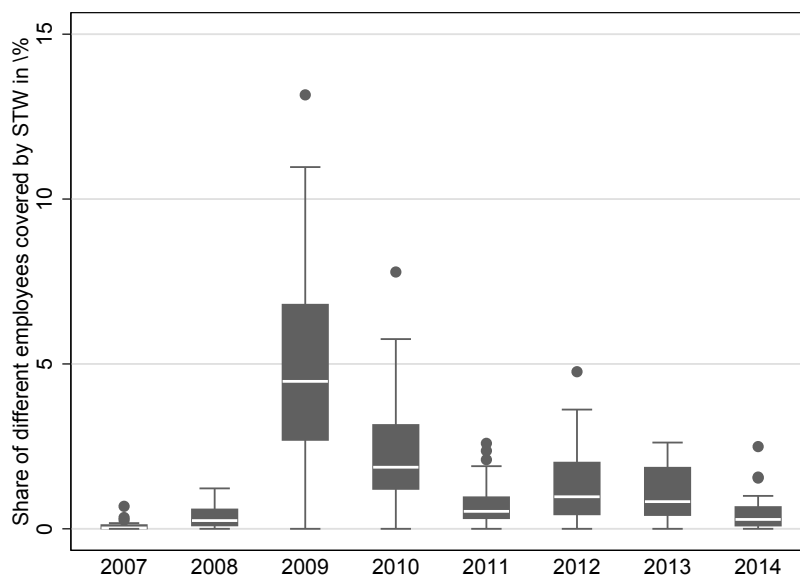
some regional labor markets, coverage rates exceeded 15 percent. Most of these strongly affected regions depend heavily on manufacturing production. Overall, these figures imply that the regional dimension of short-time work should be taken into account when discussing the pro and cons of short-time work.

## 4 Data

### 4.1 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 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

Figure 3: Distribution of the share of employees covered by short-time work across cantons by year



*Note:* This figure shows for each year the unweighted distribution of the share of employees across cantons by means of a Box-Whisker-Plot. The bottom and top of the box represent the first and third quartiles, respectively. The band inside the box is the median. The upper end of the whisker is the largest (lowest) data point still within 1.5 times the interquartile range of the upper (lower) quartile. The dots represent outliers.

*Source:* SECO, STW dataset

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). In the survey, firms report the number and the full-time equivalents (FTE) of 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. Apart from the main characteristics of the different data sources, it reports the overlap of the short-time work dataset with the UIR and the Job Statistics. 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, decision date, decision result, total employment, employees registered for short-time work, etc.). It also contains detailed information on the use of short-time work of those departments whose short-time work application was approved (month in which department was eligible, number of covered employees, missed hours, short-time work benefits, etc.). Since some establishments have several departments,<sup>8</sup> 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. Obviously, the dataset does not contain information about establishments that did not apply for short-time work. 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. Some establishments have approved as well as denied cases.
- Our sample drawn from the **Unemployment insurance register** covers the years 2006 to 2015 and contains all registered job seekers with former employers in manufacturing and trade (NACE-Codes 10-33 and 45-47). We also observe all unemployed workers if their former employer applied for short-time work (i.e. even if this employer does not belong to manufacturing and trade). 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 the registered 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,

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<sup>8</sup>2'152 out of 16'243 different short-time work cases between 2009 and 2014 are from multi-department establishments.

establishment identifiers are recorded for almost the universe of registered job seekers.<sup>9</sup> 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. 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.

- The **Job Statistics** is a quarterly survey of 18'000 secondary and tertiary sector enterprises (65'000 businesses) 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 application for short-time work was approved between 2009 and 2014. Unfortunately, there are only 262 establishments whose applications for short-time work were denied.

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<sup>9</sup>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.2 in the Appendix). Note, that this number will never reach 100% since not all new job seekers had previously a job.



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.

## 4.2 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. Our case definition differs from the definition used by SECO in its official statistics in two respects. The first difference relates to the treatment of renewals. Renewals arise frequently because establishments are required to renew their short-time work approval every three months.<sup>10</sup> In the official case definition, a renewal represents the start of a new case, as the cantonal offices are formally required to reconsider their decision on short-time work. In 99% of all cases, however, renewals are approved. We thus treat renewals as if they represented the continuation of a case that started earlier. The second difference relates to establishments that interrupt their collection of short-time work for short periods of time. As with renewals, it is unlikely that the establishments’ underlying economic situation has changed. We thus treat applications of establishments that collected STW benefits within the last 6 months as the continuation of the case before.

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

<sup>10</sup>In 2009 and 2010 this period was extended to six months.

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 (see Table 2). 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.

Table 2: Number of establishments and cases 2009-2014

	Number of ...
Establishments	12,570
Establ. with more than 1 case	2,786
Cases	16,243
Cases approved	13,565
Cases denied	2,678

*Source:* SECO, STW dataset

### 4.3 Definition of outcomes of interest

Using the case-level panel dataset detailed in the last section, 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.<sup>11</sup>
- **Share of hires:** This outcome exploits that the UIR records the establishment identifier of the *new* employer for most unemployed that leave unemployment to start a job.<sup>12</sup> We measure the per-period count of new hires by an establishment from the pool of registered job seekers. The “share of new hires” is the relationship between an establishment’s count of hires from the pool of registered job seekers 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 to 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

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<sup>11</sup>405 observations (0.1% of all observations) exhibit a share of more than 100%. We set these values to 100% in order to avoid that unrealistic outliers bias our results. We proceed in the same vein with the share of hires and the share of job seekers.

<sup>12</sup>The share of successful job seekers with known new employer increases from 36% in 2008 to 65% in 2009 to 77% in 2010. In the years 2011 to 2016 it stays between 80 and 87%. Hence, we underestimate the share of new hires from the pool of registered job seekers slightly.

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).

## 5 Descriptive evidence

### 5.1 Take-up

We start our empirical analysis by taking a closer look at the decision of establishments to apply for short-time work benefits. To what extent do establishments that apply for short-time work benefits differ from those that do not? To answer this question, Table 3 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 in 2009 or 2010. Overall, 20% of all establishments in the sample applied for short-time work. The covariates in the probit regressions are firm characteristics that are mostly taken from JOBSTAT.<sup>13</sup> Positive coefficients indicate that the respective variable is associated with a higher probability that an establishment applies for short-time work. We report regressions that control for an increasingly demanding set of fixed effects.

The results show that the propensity to apply for short-time work is positively associated with establishment size. Establishments with 100 to 499 employees have the highest probability to apply for short-time work. The estimated coefficients on the establishments' share of women turn from negative to positive as soon as we control for industry fixed effects. This suggests that the initial negative association (column 1) is due to the fact that women are underrepresented in industries that heavily use STW (in particular, in manufacturing). Interestingly, a higher share of cross-border commuters is associated with a higher probability of applying for short-time work. By contrast, a high share of part-time workers is associated with a lower probability, in line with the results of Boeri and Bruecker (2011). The negative relationship might partly reflect that establishments relying heavily on part-time workers might have greater flexibility to adjust

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<sup>13</sup>We have to restrict the sample to establishments operating in manufacturing and trade because our sample of the job statistics is restricted to these two sectors.

their workforce and working hours, which reduces the need to apply for short-time work benefits.

Another interesting result from Table 3 is that establishments reporting difficulties in recruiting workers have a higher probability to apply for short-time work benefits than establishments without problems to find adequate personal. One explanation for this finding is that firms with recruitment difficulties might be less inclined to lay off workers during a crisis because it will be costly to hire new ones when the economy recovers. If recruitment of appropriate workers is difficult, the human capital of the existing workforce may be more relevant to the establishment.

Column 3 includes the cantonal approval rate in the years 2009 and 2010 as a further explanatory variable. The regression shows that establishments' take-up of short-time work is positively related to the approval rate of a canton. It could be that a higher approval rate leads to a higher application rate, or that a higher application rate leads to more approvals. However, the correlation 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. More generally, 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). Hence, 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. By contrast, the two-digit industry fixed effects have substantial explanatory power. If we look at them more closely, we see that the propensity to apply is highest in the mechanical engineering, electrical and metal industries.

## 5.2 Cantonal approval decisions

Given that an establishment applied for short-time work, what influences the probability that a request is approved or denied? Understanding this question is of crucial importance for our investigation. The reason is that we will estimate the effects of short-time work by comparing firms that applied for short-time work and whose application was approved with firms that applied and whose application was denied.

We first take a look at the evolution of approval rates in different cantons.<sup>14</sup> The average approval rate across all cantons and the entire 2009–2014 period is 83.5%. However, it varies substantially between the cantons, and also within cantons over time. Cantonal

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<sup>14</sup>Table A.3 in the Appendix reports average approval rates for all cantons.

Table 3: Effects of firm characteristics on probability to apply for short-time work

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

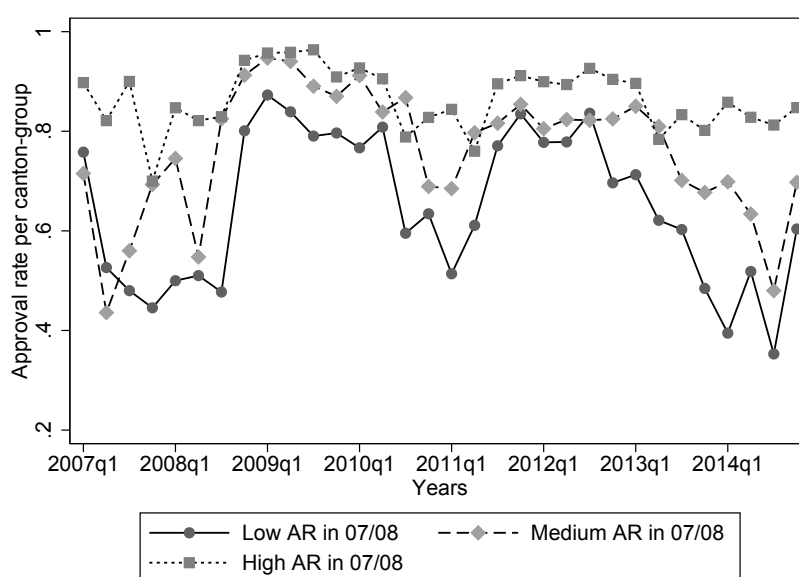
*Note:* The table reports the average marginal effects of a probit regression on the probability that an establishment applied for short-time work in 2009 and 2010. The Sample contains establishments from manufacturing and trade in 2005-2010.

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

*Source:* SECO; FSO

approval rates range from 40 to 100 percent. Figure 4 illustrates these differences in cantonal approval practices by plotting the approval rate of three canton groups over time. Cantons are allocated to the three groups based on their average approval rate in the first two sample years (2007 and 2008). We observe that cantonal approval rates are highest during the Great Recession and lower before and after it. The figure also suggests that cantonal differences in approval practices 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.

Figure 4: Cantonal approval rates for short-time work applications, by canton groups



*Note:* The cantons are allocated to the different groups according to their average approval rates in the pre-crisis period 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.

*Source:* SECO, STW dataset.

Table 4 reports the marginal effects of different firm and regional labor market characteristics on the approval probability in the years 2007 to 2014 applying a probit model. 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.

The upper part of Table 4 shows that the probability of approval increases significantly with firm size as well as with the fraction of employees that the establishment

registers for short-time work. Establishments that plan to cover only a small fraction of their workforce for short-time work (less than 20 percent) have a substantially lower approval probability than establishments that register a large fraction. According to experts involved in the decision process, this is because the unemployment insurance act requires that the working-time reduction has amount to at least 10 percent of the establishment's usual working time. If establishments envisage a working time reduction close to that threshold, the cantonal employment agencies might suspect that the establishment will fall short of this requirement and therefore deny the application. Similarly, requesting short-time work for a small fraction of the workforce might be interpreted as a sign that the establishment does not really need short-time work.

Table 4 shows several further factors that affect the approval probability. First, there are substantial differences between industry groups. The probability of approval is highest for establishments operating in the manufacturing industry and lowest for establishments in the construction sector. Establishments of the service sector also have a considerably smaller probability that their short-time work application is approved compared to the manufacturing industry. Second, approval is affected by the reported reason why establishments apply for short-time work. If establishments indicate that they apply for firm specific problems or "other reasons", they face a much lower probability of approval than firms that apply due to a cyclical drop in demand—which is by far the main reason why establishments apply for short-time work (i.e. 94% of all cases). Third, the probability for positive answer is substantially larger if an establishment already received short-time work benefits in the past.<sup>15</sup> Conversely, a negative decision in the past decreases the chances. Fourth, the approval probability increases with the number of departments of an establishment that apply for short-time work. Fifth, it increases significantly if the establishment is of high importance for the regional labor market. We measure the latter in terms of the establishment's employment share in total employment of a regional labor market.<sup>16</sup> Another factor that increases the chances of receiving short-time work benefits is the (normalized) cantonal unemployment rate.<sup>17</sup> Finally, we find evidence that the workload of the cantonal employment agencies plays a role, too. The probability of

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<sup>15</sup>We observe the establishments from 2005 onwards.

<sup>16</sup>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.

<sup>17</sup>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.



approval increases slightly if more establishments apply for short-time work at the same employment agency within two weeks around the application date (one week before and one week after application).<sup>18</sup> Overall, our evidence suggests that it is not only the situation of the firm that matters for the approval decision. Political factors or the specific economic situation in the canton seem to play a role as well.

Table 5 compares the characteristics of establishments whose application for short-time work was approved with the characteristics of establishments whose application was denied. Establishments with approved applications are on average much larger than establishments with denied ones. However, the share of workers an establishment registers for short-time work at application in total employment is very similar for both groups. So is the duration of the decision process. Importantly, not all establishments whose applications were approved actually make use of short-time work. In 24% of all approved cases, short-time work benefits were not collected. Those establishments that collect short-time work benefits use them for 6.8 months on average and cover 60 percent of their workforce.

### 5.3 Short-time work use

Let us take a closer look at the use of short-time work of those establishments whose application was approved. Figure 5a 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. 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.<sup>19</sup> 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 5b and 5c). More than 40 percent of the total workforce of an establishment are covered by short-time work benefits shortly after the application.<sup>20</sup> Since not all of these workers reduce their

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<sup>18</sup>We construct this variable 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.

<sup>19</sup>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.

<sup>20</sup>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.

Table 4: Effect of firm and region characteristics on approval probability

	Model 1	Model 2	Model 3	Model 4
<i>Firmsize</i>				
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***
<i>Industry</i>				
Manufacturing	ref.			
Construction	-.15***			
Other 2. Sector	-.073*			
Trade	-.1***			
Other 3. Sector	-.11***			
<i>Importance reg. labor market</i>				
Not important	ref.	ref.	ref.	ref.
Of medium importance	.0057	.014	.023**	.022**
Of high importance	.013	.028	.046**	.051**
<i>Other variables</i>				
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 5: 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

working time by 100%, the share of missed hours in normal hours is smaller—slightly less than 25% one quarter after application.

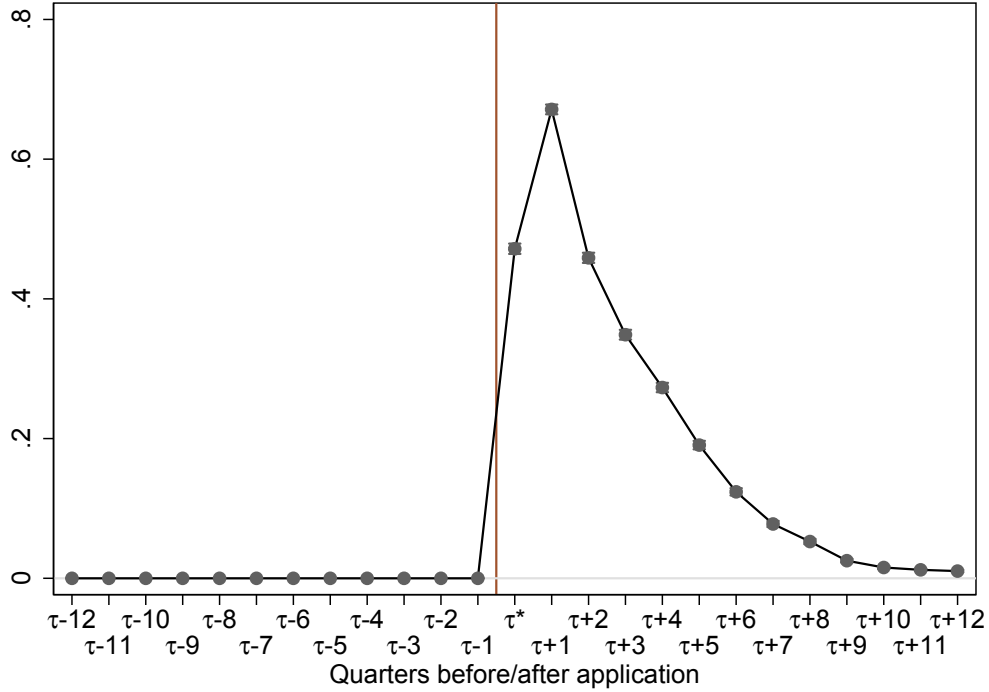
## 5.4 Definition of causal effect of interest

All empirical approaches that we follow below amount to comparing the evolution of the outcome variables of two groups: 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*). The control group is used to construct a counterfactual outcome for the treatment group had the treatment group’s short-time work application not been approved.<sup>21</sup>

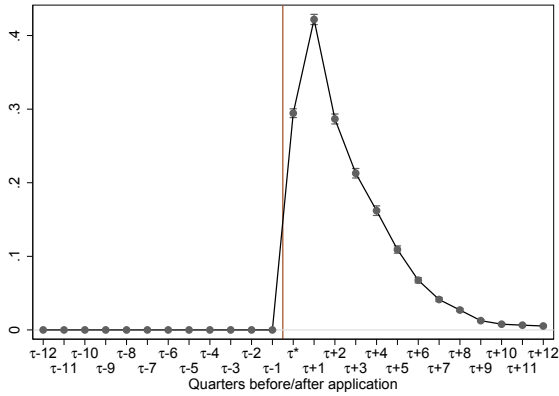
An important methodological implication of the split depending on establishments’ approval status is that we estimate the causal effect of the cantonal *approval of short-time work* on firms’ dismissals and other outcomes. We just saw, however, that roughly 1 of 4 establishments whose application was approved do not actually collect short-time work benefits. In the parlance of clinical trials, our estimates thus represent Intention-to-Treat (ITT) effects and not Treatment-Effects-on-the-Treated (ToT). The latter would

<sup>21</sup>In chapter 6.1.5, we provide a detailed discussion under which conditions these comparisons reveal a *causal* effect of short-time work.

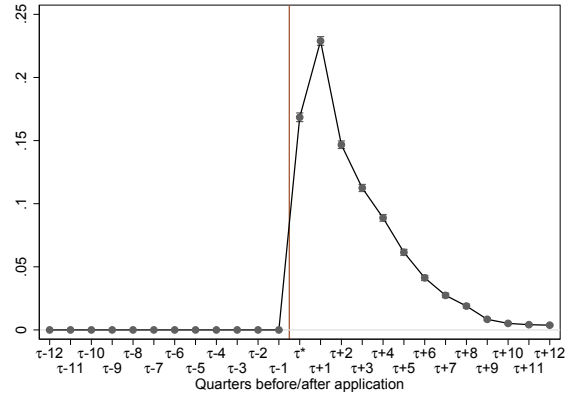
Figure 5: Intensity of short-time work use



(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.

### Intention-to-Treat Effects

In our application, the ITT measures whether cantonal decisions to approve short-time work or not affect unemployment. To illustrate what this means, consider the extreme case in which only 1 of 100 establishments whose application was approved decides to actually collect short-time work benefits. In this case, it would be no surprise if we find little differences in the share of dismissed workers when we compare establishments whose short-time work application was approved and establishments whose short-time work application was denied. After all, almost no establishment whose short-time work application was approved decided to actually use short-time work. We would conclude that the ITT is very small or even zero—the cantonal decision to approve short-time work does not affect dismissals. Note that the ITT would be very small even if short-time work strongly prevented dismissals in the single establishment that decided to collect benefits, i.e. if the effect on the establishment actually treated (the so-called Treatment-Effect-on-the-Treated, ToT) was large.

refer to the effect of actually *using* short-time work benefits on the outcomes (see box “Intention-to-Treat Effects” for further explanations).

There are two reasons why we focus on ITT. First, ITT arguably reflects a very relevant parameter, as it measures whether policy makers’ decisions to approve or deny short-time work affects unemployment. Second, a statistical analysis that would try to estimate ToT could be biased by another selection problem, arising from the decision of firms whether or not to collect short-time work benefits if they are allowed to. Because the decision to take up STW is a decision made by the establishment, it is unlikely that the decision is random and thus unrelated to its decisions to lay off or hire workers.

In our case, 76% of all firms collect short-time work benefits if their short-time work application was approved. This means that the so-called compliance with the cantonal approval decision is relatively large, and the difference between ITT and the ToT is relatively small in practice. We nevertheless provide an estimate for the ToT in section 6.2 below applying an appropriate method to address the selection concerns that arise in its estimation.

## 6 Does short-time work prevent unemployment?

In this section, we study whether the Swiss short-time work scheme prevented unemployment during and after the Great Recession. We proceed as follows. In section 6.1, we study whether establishments whose short-time work application is approved dismiss

less workers than establishments whose short-time work application is rejected. In section 6.2, we address concerns that the prior results are driven by the cantons' decision whom to approve or deny short-time work. We do this by means of instrumental variable estimations, which exploit certain random elements in cantons' approval decisions. The aim of section 6.3 is to get additional evidence regarding the question whether short-time work only postpones, rather than prevents, layoffs. Do firms that collect short-time work benefits until the legal maximum dismiss workers when the collection of short-time work benefits ends? How many firms exhaust the legal framework and collect short-time work benefits until the legal maximum? Finally, section 6.4 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.

## 6.1 Unemployment around short-time work decision

### 6.1.1 Descriptive evidence

How do the share of dismissed workers and the share of new hires<sup>22</sup> 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 not approved) around the time of the establishments' application for STW? Figure 6 gives an answer to this question by plotting the evolution of the share of dismissed workers against event time  $\tau$ .

Figure 6a shows that the share of dismissed workers in treated establishments hovers around 1.5% per quarter in the pre-treatment period. In the quarters after the application for short-time work, the share increases somewhat, reaching 2% in the second and third quarter after the application. After a slight decline, dismissals increase again slightly between  $\tau = 6$  and  $\tau = 8$ . This is no coincidence, as 6 (18 months) and 8 quarters (24 months), respectively, represent the maximum benefit duration for most short-time work cases in the sample. The evolution of the 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 Figure A.1 in the Appendix shows. However, it is very different after the application. Here, the share more than doubles 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.

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<sup>22</sup>See section 4.3 for the definitions of these outcome variables.

We observe less differences between the two groups of firms concerning the share of hires (Figure 6b). There is some evidence that the share increases in the control group after the failed application for STW, while treated establishments seem to hire almost no one around  $\tau = 0$ . However, the evidence on hires is less clear-cut than the evidence for dismissals. This is not the least because the average quarterly share of hires is very low in both groups (around 0.25%) and thus quite erratic. Because the share of hires is much lower than the share of dismissed workers, the evolution of the *net* share of dismissed workers—the share of dismissed workers minus the share of hires—is dominated by the differences in dismissals between the two groups.

Overall, Figure 6 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.

### 6.1.2 Regression model

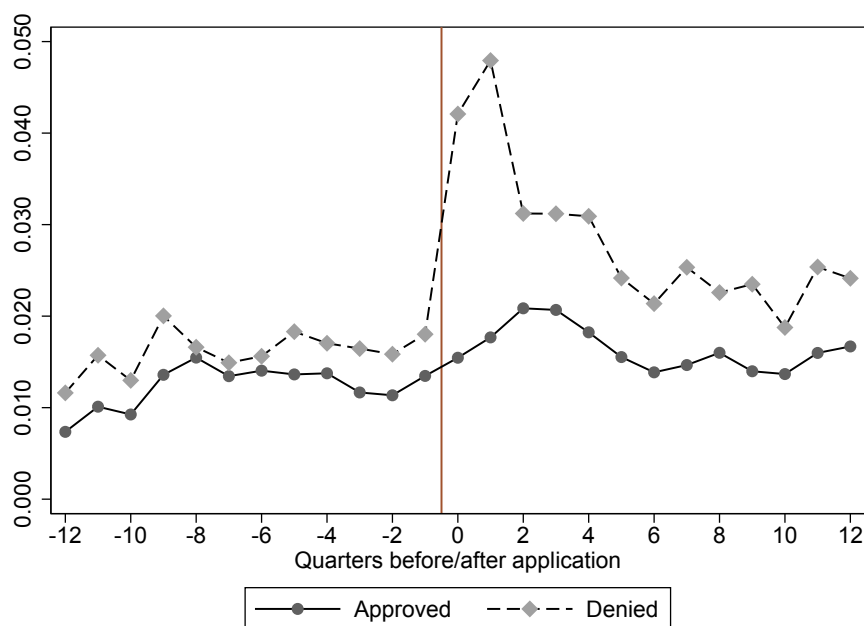
The evidence on the pre-application evolution of the share of dismissed workers, presented in the last section, suggests a straightforward way of analyzing the impact of the approval of short-time work on layoffs. In particular, there are only minor pre-application differences in the *change* of the share of dismissed workers from quarter to quarter between the two groups. These parallel trends suggest that the two groups would follow common changes in dismissals absent treatment. A Difference-in-Differences (DiD) approach, which compares 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 DiD model for the outcome of interest in short-time work case  $i$  and period  $t$ , which we denote by  $u_{i,t}$ :

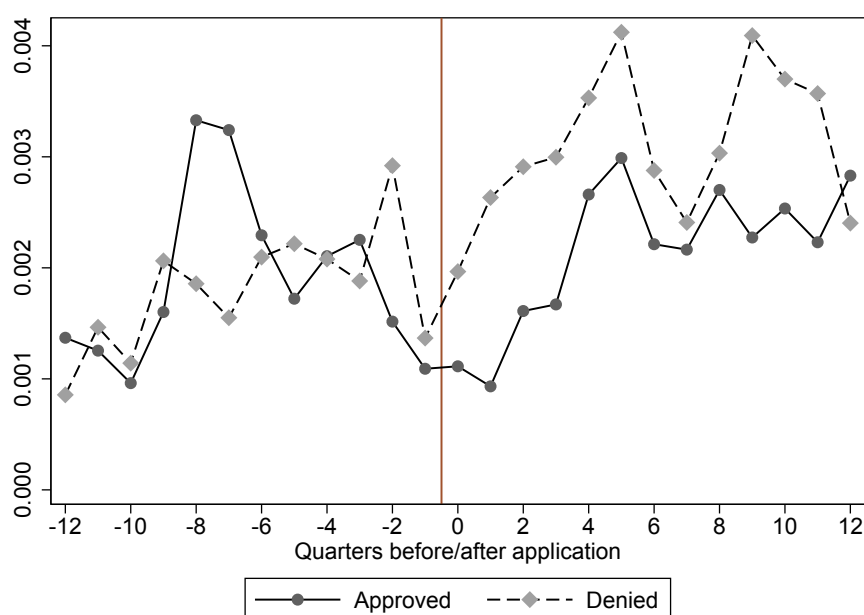
$$u_{i,t} = \delta_i + \gamma_t + \gamma_\tau + \sum_{\tau=-k}^k \beta_\tau STW_{i,t}^\tau + \epsilon_{i,t} \quad (1)$$

The dependent variable in this model is the share of dismissed workers, or, alternatively, one of the other outcomes specified in section 4.3. The independent variables of

Figure 6: Share of dismissed workers and hires around short-time work application, by approval decision



(a) Share of dismissed workers



(b) Share of new hires

*Notes:* The top panel displays the average 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 share of dismissals includes only dismissed workers who register at the Swiss Unemployment insurance. The bottom panel displays the share of new hires from the pool of registered job seekers of establishments with approved and denied short-time work applications.



interest 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 both, for a full set of period fixed effects  $\gamma_t$ , which account unobserved factors that affect all observations in a given time period equally such as common business cycle shocks, and for event time fixed effects  $\gamma_\tau$ , an individual fixed effect for each event time period.<sup>23</sup> The model also contains a full set of case fixed effects  $\delta_i$ . Among other, these account for all characteristics of an establishment that do not change over the period of the case. These are, for instance, firms’ size and productivity at the time of application. In fact, the case fixed effects control for all information that is contained on an establishment’s application for short-time work and that does not change over time (i.e. is fixed at  $\tau = 0$ ).

Due to the case fixed effects, the coefficients of interest  $\beta_\tau$  represent DiD estimates. They reflect the *difference* between treatment and control group *in the change* of  $u_{i,t}$  from the period before the application ( $\tau < 0$ ) to the period after the application ( $\tau \geq 0$ ). If short-time work helps to prevent unemployment, we would 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. Hence, we expect the series of  $\beta_\tau$  to be negative in the treatment period (i.e.  $\tau \geq 0$ ). 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.

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  $\beta_\tau$ , 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.<sup>24</sup> Second, as all DiD effects are estimated *relative* to each other, we need to decide on a reference period. As is common

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<sup>23</sup> $\gamma_t$  and  $\gamma_\tau$  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.

<sup>24</sup>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.

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  $\tau = 1$ .<sup>25</sup>

### 6.1.3 Main regression results

Figure 7a presents the sequence of event study coefficients,  $\beta_\tau$ , and associated 90% confidence intervals, from our baseline event study model (equation 1). The outcome variable is the net share of dismissed workers. Conditional on time and case fixed effects, there are no differences in the change of this outcome between 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, so that the DiD is close to 0 (see Figure 6). 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 these 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 5).

The estimates in Figure 7a 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  $\beta_\tau$  from period  $\tau = -1$  to period  $R$ ,  $E_R = \sum_{\tau=-1}^R \beta_\tau$ , both for positive and negative  $R$ . In Figure 7b, we present these cumulative sums, and corresponding inference, for the share of dismissed

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<sup>25</sup>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.

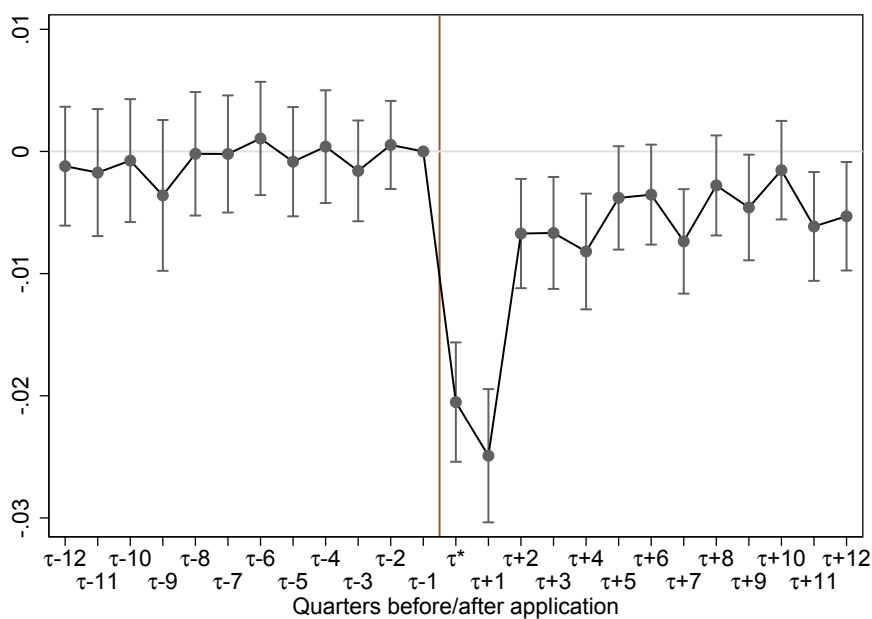
workers, the share of hires, and the net share of dismissed workers. The results confirm and strengthen our previous findings. Over the first twelve quarters after application ( $0 \geq \tau \leq 12$ ), the difference in the share of dismissed workers between treatment and control group sums to 10%. Taken at face value, these estimates roughly suggest that the approval of short-time work prevented unemployment of about a tenth of the establishment's workforce three years after application. Figure 7b 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, which is a fear sometimes raised in the academic literature (e.g., Cahuc and Carcillo, 2011). Because the effects on the share of hires are marginal relative to the effects regarding dismissals, the cumulative effect on the *net* share of dismissed workers is clearly negative (Figure 7b).

Columns 1–3 of Table 6 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 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.

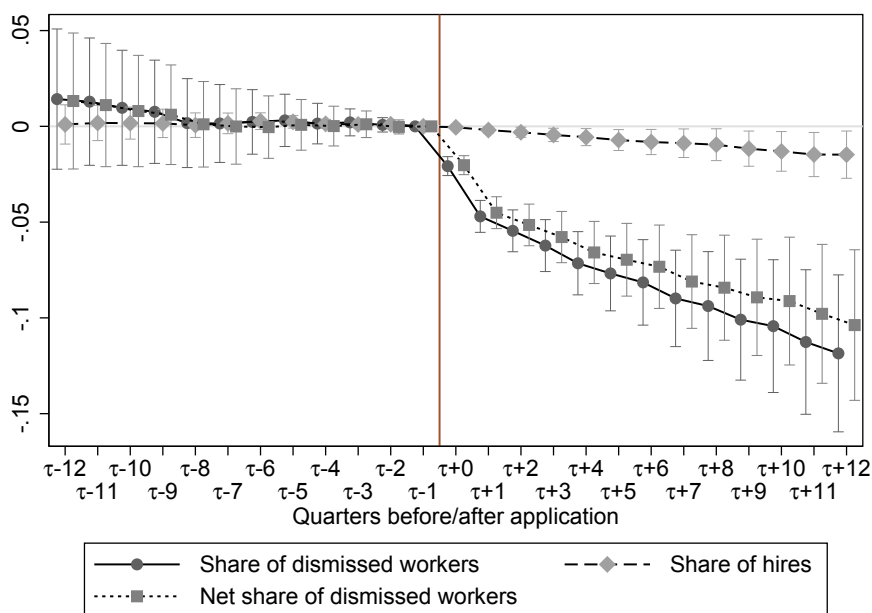
Table 6 also contains the regression results if we use three further outcome variables. The fourth column shows the difference between treatment and control group in the evolution of the share of job seekers, which includes individuals that are registered at unemployment agencies but do not collect unemployment benefits. Comparing columns 1 (the share of dismissed workers claiming unemployment benefits) and column 4 (all registered job seekers) of Table 6, we note that it makes little difference whether we include these individuals or not.

Column 5 of Table 6 incorporates the unemployment duration of the dismissed workers. In particular, we compute the sum of all daily unemployment allowances that the

Figure 7: 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,  $\beta_\tau$ , 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.

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—instead of counting daily allowances—count all days between registration and de-registration from the cantonal unemployment office, we find a DiD estimate of 38.4 days (column 6 of Table 6). The difference in the two results is explained by the fact that daily allowances are only paid for working days whereas the registration duration includes weekends and public holidays, too. Moreover, unemployed workers may remain registered at the unemployment agencies even if they are no longer eligible for unemployment benefits because they reached the maximum benefit entitlement. However, the latter factor seems to play a limited role, since the cumulative effect after 12 quarters in column 5 is only slightly lower than  $5/7$  of the corresponding effect in column 6 ( $38.4 * 5/7 = 27.4$ ).

#### 6.1.4 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 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.<sup>26</sup>

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

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<sup>26</sup>Note that we do not know the number of employed workers by educational attainment. We thus normalize each share by the total employment of an establishment at registration for 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 6: Effect of short-time work approval on different unemployment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	dismissals	hires	net dismissals	job seekers	allowances	days reg
STW $\tau - 8$ to $\tau - 4$	0.000 (0.011)	0.000 (0.003)	0.000 (0.010)	0.005 (0.012)	-1.852 (2.687)	-5.676 (4.306)
STW $\tau - 3$ to $\tau - 1$	-0.002 (0.004)	-0.001 (0.001)	-0.001 (0.004)	-0.004 (0.005)	-0.859 (1.028)	-0.623 (1.641)
STW $\tau$ to $\tau + 3$	-0.062*** (0.008)	-0.004** (0.002)	-0.058*** (0.008)	-0.068*** (0.009)	-12.191*** (2.016)	-20.071*** (3.453)
STW $\tau + 4$ to $\tau + 8$	-0.032*** (0.010)	-0.005 (0.003)	-0.026*** (0.010)	-0.032*** (0.012)	-7.313*** (2.474)	-9.033** (3.721)
STW $\tau + 9$ to $\tau + 12$	-0.025*** (0.009)	-0.005* (0.003)	-0.020** (0.008)	-0.026*** (0.010)	-6.288*** (2.083)	-9.340*** (3.211)
STW $\tau$ to $\tau + 12$	-0.119*** (0.025)	-0.015** (0.007)	-0.104*** (0.024)	-0.126*** (0.029)	-25.791*** (5.963)	-38.443*** (9.100)
<i>N</i>	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 4.3 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. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 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 (0.008)	0.001 (0.008)	0.002 (0.003)	-0.003 (0.002)	0.000 (0.002)
Appl. $\tau - 3$ to $\tau - 1$	-0.000 (0.002)	-0.001 (0.003)	0.001 (0.001)	-0.001 (0.001)	-0.002** (0.001)
Appl. $\tau$ to $\tau + 3$	-0.019*** (0.005)	-0.031*** (0.007)	-0.003 (0.002)	-0.003** (0.002)	-0.005*** (0.002)
Appl. $\tau + 4$ to $\tau + 8$	-0.018*** (0.007)	-0.013* (0.007)	0.000 (0.003)	-0.001 (0.002)	-0.002 (0.002)
Appl. $\tau + 9$ to $\tau + 12$	-0.018*** (0.006)	-0.013* (0.007)	0.002 (0.002)	-0.002 (0.002)	-0.001 (0.001)
Appl. $\tau$ to $\tau + 12$	-0.055*** (0.016)	-0.057*** (0.019)	-0.001 (0.007)	-0.006 (0.005)	-0.008** (0.004)
$N$	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. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

that are present in all periods within  $\pm 4$  quarters to the application for short-time work.<sup>27</sup> 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

$$\sum_{\tau=0}^{\tau=3} u_{i,t} - \sum_{\tau=-4}^{\tau=-1} u_{i,t} = \gamma_t + \beta STW_i^\tau + \gamma X_{i,t} + \epsilon_{i,t} \quad (2)$$

The coefficient  $\beta$  is an estimate of the DiD between treated and control establishments, focusing on the (cumulative) effect in the first year after treatment relative to the year

<sup>27</sup>The results are similar if we do not impose this sample restriction here. But the restriction can be seen as a further robustness check for our results, as we do not use cases where we observe only a small number of pre- and/or post-treatment periods.

before.<sup>28</sup> 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 8, 9, and 10 show the results of this exercise. Table 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 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. In Table 10, we test whether the effect of short-time work on dismissals is larger, the larger the shortfall in labor demand that the establishment expects at the time of application. We can examine this, as establishments report the number of workers they plan to cover by short-time work at the time of application. The share of workers registered for short-time work in total employment can be seen as an estimate of the size of the temporary labor demand shock that the establishment faces. We indeed find that the estimated effect of short-time work on the net share of dismissals is larger, the larger the share of workers registered for short-time work (see Table 10).

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.

### **6.1.5 Do the event study estimates reveal causal effects?**

Do the results from the regressions in the last section represent the causal effects of the approval of STW on unemployment, as we have sometimes interpreted them? Not necessarily. There are two main concerns here. The first is related to the possibility that

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<sup>28</sup>We conducted a similar analysis focusing on the effect within the first two years after application. This yielded very similar results regarding the heterogeneity in the effects.



Table 8: Effect of short-time work approval on net share of dismissed workers, by broad industry

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

Robust standard errors in parentheses

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

*Notes:* The dependent variable is the difference in the net share of dismissed workers between the first year after (i.e. in  $0 \geq \tau \leq 3$ ) and before (i.e. in  $-4 \geq \tau \leq -1$ ) application. The estimations are restricted to cases with non-missing outcome in all periods  $-4 \geq \tau \leq 3$ . They are further restricted to the sample of establishments indicated in the column header. 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 9: Effect of short-time work approval on net share of dismissed workers, by firm size

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

Robust standard errors in parentheses

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

*Notes:* The dependent variable is the difference in the net share of dismissed workers between the first year after (i.e. in  $0 \geq \tau \leq 3$ ) and before (i.e. in  $-4 \geq \tau \leq -1$ ) application. The estimations are restricted to cases with non-missing outcome in all periods  $-4 \geq \tau \leq 3$ . They are further restricted to the establishment size groups indicated in the column header. 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.

Table 10: Effect of short-time work approval on net share of dismissed workers, by employment share of workers registered for short-time work

VARIABLES	(1) OLS Share of reg. workers Quintile 1	(2) OLS Share of reg. workers Quintile 2	(3) OLS Share of reg. workers Quintile 3	(4) OLS Share of reg. workers Quintile 4	(5) OLS Share of reg. workers Quintile 5	(6) OLS Share of reg. workers Quintile 6
STW approved	0.012 (0.015)	-0.032** (0.015)	-0.037** (0.016)	-0.050*** (0.014)	-0.057*** (0.022)	-0.110*** (0.020)
Observations	239	750	1,148	1,245	866	1,540
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Share approved	0.619	0.661	0.735	0.780	0.864	0.631

Cluster-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  $-4 \geq \tau \leq 3$ . Cases are assigned to quintiles of the share of workers registered for short-time work at short-time work application in total employment. These quintiles indicate an establishment's expected shortfall in labor demand to be covered by short-time work. Firms in the lowest quintile (quintile 1) want to cover a low share of their employees with short-time work, firms in the highest quintile (5) want to cover almost all workers. 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.

cantons that usually approve short-time work application attract a different set of firms than cantons that usually deny short-time work. We discuss this concern below. The second concern is that cantons' decisions whom to approve short-time work could be systematically related to the outcome that we study. Importantly, due to the case fixed effects, the outcome is not establishment's dismissals but *changes in dismissals* at  $\tau = 0$  relative to the pre-application period. Any characteristic that affects cantons' decisions, but that does not lead to differential changes in dismissals at the application date between treatment and control group, does not matter for the validity of the approach. Indeed, most of the factors that influence the probability to approve a short-time work application (according to the regressions in Table 4) are correlated with the share of dismissed workers. However, only firm size is weakly but statistically significantly related to changes in the net share of dismissed workers around application. This suggests that many factors that influence cantonal approval practices are controlled for once we focus on a first-differenced outcome by including case fixed effects.

Despite this evidence, it is conceivable that our results are biased because cantons assign short-time work to establishments that would have systematically different changes in dismissals had they not been treated compared to firms that they deny short-time work. Obviously, this requires that cantons are able to extract information from the application form that predicts the change in the outcome of interest. Let us discuss the evidence for this by focusing on the three most important factors that affect cantons' decisions to approve short-time work.

A first important reason why short-time work could be denied according to the law is that cantons consider establishments' problems as structural rather than temporary. To the extent that the cantons' decisions to deny short-time work because of structural problems leads to a differential increase in dismissals at  $\tau = 0$  relative to before between groups, our estimates would likely *overestimate* the beneficial effects of short-time work. The reason is that treated firms would be "healthier" than the control firms (e.g., more competitive), and thus dismiss less workers after the application for short-time work even if their application for short-time work had not been approved. However, there is evidence that the suspicion of structural problems is not quantitatively central for cantonal approval decisions. Among others, there are only small differences between treatment and control group in the share of dismissed workers prior to treatment.<sup>29</sup>

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<sup>29</sup>In a similar vein, establishments that successfully applied for short-time work before have a substantial higher (not lower) probability to receive short-time work again. Moreover, experts admit that it is hard to prove in practice that an establishment faces structural rather than temporary shortfall in demand.

The second central factor for denials of short-time work is that firms' temporary drop in demand is considered to be related to seasonal rather than business cycle factors. If this mattered, firms in the control group would be more likely to face a short seasonal downturn than firms in the treatment group. It is unclear how this would affect our estimates. 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. To limit such concerns, our estimations control for seasonality with the help of fixed effects: the baseline regressions control for period fixed effects, which absorb seasonal patterns that are common across firms. The results are unchanged if we use industry-specific period fixed effects (as is shown by column 3 of Table 11), and if we drop seasonal industries altogether.<sup>30</sup> These results speak against the fact that the evidence of the OLS regressions is driven by differences in seasonal effects across groups.

A third central reason to decline short-time work is that an establishment does not face a sufficiently large temporary decline in demand. Establishments are only eligible to short-time work if the working-time reduction amounts to at least 10 percent of the usual working time in the accounting period. If the cantonal employment agencies suspect the establishment to fall short of this requirement because the temporary decline in demand is too small, they might decline the application.<sup>31</sup> In this case, our estimates would likely *underestimate* the effect of short-time work on dismissals. The reason is that treated firms would face larger demand reductions on average than the control group, and would thus likely dismiss more workers than the control group had they not received treatment. Given these concerns, we run a regression that only compares treatment and control firms that expect a *similar* shortfall in labor demand at the time of the application. Our data allow us to do this, as the expected share of hours lost is an estimate of the size of the temporary labor demand shock that the establishment faces (see the last section). We use this variable to assign firms into quintiles of the expected share of hours lost. We then add a full set of interactions between the period fixed effects and indicators for these

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<sup>30</sup>A firm-specific way to test for the importance of seasonality is to disregard establishments who 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.4 in the appendix).

<sup>31</sup>Our analysis of the approval process supports the notion that this reason was indeed important since we observe that establishments envisaging to cover only a small fraction of their workforce by short-time work benefits—and are therefore much more likely to fall below the threshold of 10 percent working time reduction—have a much lower probability to get a positive response.

five quintiles to our regressions. If we do this, the results remain virtually unchanged (see column 5 of Table 11).

As mentioned above, a second conceptual concern with the event study results is that they might be biased because firms are more willing to apply for short-time work if the chances of approval of the application are higher. In section 5.1, we 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 short-time work, cantons that generously handle short-time work applications would attract and approve more applications of firms that would not even apply in other cantons. Because the impact of short-time work on dismissals is likely to be lower for these firms, our event study estimates would probably understate the effect of short-time work in this case. To examine the quantitative relevance of this concern, column 4 of Table 11 adds a full set of canton-period fixed effects to the regression. These canton-time effects ensure that we only compare firms that applied at the same point in time within the same canton. These firms thus faced the same cantonal approval practice. The inclusion of these dummies has no impact whatsoever on the event study results.

The discussion shows that it is unclear whether the event study estimates bear a causal interpretation or not. Moreover, they may either over- or understate the true effect of short-time work, depending on what is driving cantons' decisions to deny or approve short-time work. In general, however, we are confident that our DiD estimates by approval decision provide a valid and relevant benchmark for the causal effect of interest. There are three reasons for this. First, the results pass several robustness tests that would likely fail if the above-mentioned concerns played a major role. Second, there appear to be idiosyncrasies in cantonal approval decisions, which make it more likely that the cantonal selection is—at least on average across cantons—unrelated to the outcome of interest. Third, we do not find any evidence for differences in the evolution of the main outcomes of interest in the periods leading up to the short-time work application. Usually, such evidence is interpreted as favoring the main identifying assumption of a DiD analysis: that treatment and control group would follow *a common trend* absent treatment.

Table 11: Some 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 (0.010)	-0.000 (0.010)	0.000 (0.010)	-0.001 (0.010)	-0.001 (0.010)
STW $\tau - 3$ to $\tau - 1$	-0.001 (0.004)	-0.001 (0.004)	-0.001 (0.004)	-0.003 (0.004)	-0.003 (0.004)
STW $\tau$ to $\tau + 3$	-0.058*** (0.008)	-0.058*** (0.008)	-0.059*** (0.008)	-0.059*** (0.008)	-0.059*** (0.008)
STW $\tau + 4$ to $\tau + 8$	-0.026*** (0.010)	-0.026*** (0.010)	-0.026*** (0.010)	-0.025** (0.010)	-0.025** (0.010)
STW $\tau + 9$ to $\tau + 12$	-0.020** (0.008)	-0.019** (0.008)	-0.018** (0.008)	-0.021** (0.008)	-0.019** (0.008)
STW $\tau$ to $\tau + 12$	-0.104*** (0.024)	-0.103*** (0.024)	-0.102*** (0.024)	-0.105*** (0.024)	-0.103*** (0.024)
<i>N</i>	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 “application intensity” are dummy variables controlling for interaction terms between a full set of period dummies and indicator variables of the quintile of the share of total hours lost because of the drop in demand, as reported by firms in the short-time work application form. The table lists the sum of coefficients for indicated intervals. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 6.2 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 5.2, average cantonal approval rates range from 60% to almost 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. Importantly, these differences persist or get even larger if we account for differences in observable cantonal characteristics like the industry composition or the average establishment size. 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 2, 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 (see box “Instrumental Variable (IV) Estimation” for more details on the method). 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.<sup>32</sup>

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<sup>32</sup>The exclusion of the establishment’s own industry in the calculation of the share ensures that the instrument is unrelated to common (and potentially persistent) shocks to firms within the same industry

The first stage regressions presented in Panel A of Table 12 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 first 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 5.2. In column 2, 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.<sup>33</sup> As expected, dropping cases from these cantons markedly improves the size and precision of the first stage (column 3 of Table 12).

The corresponding columns in Panel B of Table 12 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 first 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 2 and 3 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, which suggest negative effects of -5% to -7% (see Table 8). If anything, our prior DiD results may thus even understate the effects of short-time work approval on dismissals.

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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.

<sup>33</sup>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.



## Instrumental Variable (IV) Estimation

The method of instrumental variables (IV) is used to estimate causal relationships when controlled experiments are not feasible. IV methods are applied if the covariate of interest (here: the approval status of an establishment) is correlated with the error term in a regression (i.e. is *endogenous*). In our case, we are most worried that the approval decisions of cantonal employment agencies are based on certain establishment or case characteristics that we do not observe which are, at the same time, correlated with the change in dismissals at application. In this situation, the above event study estimates may be biased and inconsistent. Instrumental variable methods may overcome the endogeneity problem and allow for consistent estimation. An instrument is a variable that is correlated with the endogenous explanatory variable, but is uncorrelated with the error term in the regression model. In our case, a valid instrument has to fulfill the three following assumptions:

1. **Relevance or first stage:** The instrument needs to have a sufficiently strong impact on cantonal approval decisions. This is a testable assumption.
2. **Conditional independence:** The instrument needs to be as good as randomly assigned conditional on the covariates, i.e. it has to be unrelated to unobserved third factors that affect the share of dismissals.
3. **Exclusion restriction:** The instrument needs to affect the share of dismissals only through its effect on the approval status. There cannot be a second (indirect) channel through which the instrument affects dismissals.

In the rest of Table 12, we use an indicator whether an establishment applied for short-time work benefits before as a second instrumental variable. The chances that a canton approves short-time work are substantially higher for an establishment that applied for short-time work before. An important reason for this is that cantonal employment agencies are likely to scrutinize applications of these establishments less closely and are thus less likely to reject their applications compared to applications of observationally equivalent establishments that never applied before. At the same time, the approval in the second case may have little bearing with the specific situation in which the establishment is in the current case, and may thus be largely unrelated to the change in dismissals at this second application apart from influencing the approval probability. Because there are scenarios why the conditional independence assumption and the exclusion restriction could fail,<sup>34</sup> the results based on this instrument should be interpreted with caution. Nevertheless, it is reassuring that the estimated second stage effects are comparable to

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<sup>34</sup>For instance, a firm that applies for the second or third time could be characterized by above-average cyclicality in demand. The cantonal agencies know this and generally approve its applications. In this

those from the preferred specification if we use this instrument or if we combine it with our preferred instrument and additionally control for canton FE (cf. columns 4 and 5 of Table 12). Similarly, it is reassuring that the p-value of the Hansen  $J$  test, shown at the bottom of the table, suggests that the overidentifying restrictions are valid.

In Table 13, 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 13). This effect is somewhat smaller than the first-differenced IV estimates in Table 12, but somewhat larger than the corresponding event study estimates. In the second and third column of Table 13, the outcome is the cumulative net share of dismissed workers 12 quarters after application. We use the preferred instrument without canton FE (column 2) and combine both instruments used before in a specification with canton FE (column 3). The regressions suggest that the approval of short-time work reduces the net share of dismissed workers by 23% or 28%, respectively, 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 4–6 of Table 13). As was the case in Table 12, the IV estimates in Table 13 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). In the case of our main instrumental variable, 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.<sup>35</sup> Second, the IV results—as well as the event study

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case, the dummy for a previous application would be related directly to the change in the outcome at  $\tau = 0$ , and the exclusion restriction would fail.

<sup>35</sup>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

Table 12: IV estimates of the effect of short-time work approval on the change in the net share of dismissed workers

VARIABLES	(1)	(2)	(3)	(4)	(5)
	2SLS	2SLS	2SLS	2SLS	2SLS
	appr.	appr.	appr.	altern.	Both IV
	rate	rate	rate	appr.	combined
			w/o small	rate	
<b>Panel A: First stage</b>					
Lagged appr. rate (other ind.)	0.408***	0.157***	0.247***		0.151***
	(0.036)	(0.051)	(0.056)		(0.052)
Establishment applied before				0.105***	0.105***
				(0.012)	(0.012)
<b>Panel B: Second stage</b>					
short-time work approved	-0.133**	-0.360	-0.266*	-0.180***	-0.197***
	(0.059)	(0.224)	(0.154)	(0.066)	(0.065)
Observations	5,650	5,650	4,830	5,708	5,650
Industry-period FE	Yes	Yes	Yes	Yes	Yes
Canton FE	No	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
RMSE	0.197	0.217	0.203	0.200	0.200
F stat first stage					38.47
p-value Hansen J stat					0.392

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 columns 1–3, the instrument is the cantonal approval rate in all other two-digit industries in the two quarters preceding application. In column 4, the instrument is the contemporaneous approval rate in construction and the service sector for manufacturers, and the approval rate in manufacturing for all other firms. In column 5, the instrument is an indicator whether the establishment applied for short-time work in the past. Column 6 combines these instruments and presents a Hansen J-test of the validity of the overidentifying restrictions. Controls are the set of variables from Table 4. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.

Table 13: IV estimates of the effect of short-time work approval on the net share of dismissed workers and on total daily allowances per worker

VARIABLES	(1) Net dismissals appr. rate $0 \geq \tau \leq 3$	(2) Net dismissals appr. rate $0 \geq \tau \leq 12$	(3) Net dismissals Both IV combined $0 \geq \tau \leq 12$	(4) Allow- ances appr. rate $0 \geq \tau \leq 3$	(5) Allow- ances appr. rate $0 \geq \tau \leq 12$	(6) Allow- ances Both IV combined $0 \geq \tau \leq 12$	(7) Net dismissals appr. rate $0 \geq \tau \leq 12$
STW approved	-0.081** (0.033)	-0.235*** (0.075)	-0.281*** (0.090)	-16.668* (9.278)	-48.523*** (15.225)	-32.405* (19.304)	
STW used							-0.315*** (0.102)
Observations	14,370	10,795	10,795	14,370	10,795	10,795	10,795
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	No	No	Yes	No	No	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
RMSE	0.173	0.331	0.332	46.20	65.38	64.71	0.351
F stat first stage			61.65			61.65	
p-val Hansen J stat			0.894			0.965	

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 1–6) and short-time work use (column 7) 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 4–6). These outcomes are computed over the first four quarters after application in columns 1 and 4, and over the first 12 quarter (i.e. quarters  $0 \geq \tau \leq 12$ ) in columns 2, 3, and 5–7. Short-time work used is an indicator equal to one if a firm claims STW benefits. In columns 1, 2, 4, 5, and 7, the instrument is the lagged cantonal approval rate in other two-digit industries. In columns 3 and 6, we combine the two instruments also used in Table 12. Controls are the set of variables from Table 4. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01.

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 (see section 5.4 for a discussion). 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 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 7 of Table 13. 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. columns 1 and 7 Table 13). It is in line with what we could guess from ITT and the take-up rate of short-time work benefits.<sup>36</sup>

### 6.3 Unemployment around short-time work expiration

One of the major concerns regarding short-time work is that it only postpones rather than prevents layoffs. Figure 6a shows that firms whose application for short-time work is approved dismiss less workers than the control group for at least 12 quarters after the application. This speaks against the fact that short-time work only postpones dismissals, as almost no treated firm still collects short-time work benefits 12 quarters after application. In this section, we take a closer look at what happens to layoffs when short-time work benefits run out. We differentiate between firms that voluntarily stop collecting benefits, and establishments that are forced to stop collecting benefits because they reach the legal maximum benefit entitlement. The latter are more likely to be in a critical situation and are therefore more prone to dismiss workers as soon as payments of short-time work benefits stop.

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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.

<sup>36</sup>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 13 is -0.235. Dividing this by 77%, we get -0.31.

How many establishments collect short-time work benefits until the maximum duration of 12, 18 or 24 months? The answer to this question—provided in Table 14—is strikingly few. In 98.5 percent of 13’565 approved cases over the years 2009 to 2014, the establishments did not collect short-time work benefits until the legal maximum. Establishments collected benefits until the maximum in only 1.5% of all cases. In 35 cases, short-time work benefits were collected until the legal maximum of 12 months. In 72 and 92 cases, benefits were collected until the legal maximum of 18 months and 24 months, respectively.

Table 14: Number of cases that reach legal maximum duration of short-time work, 2009–2014

	No of cases	Percent
STW not used until legal max	13,366	98.53
STW used until legal max 12 months	35	0.26
STW used until legal max 18 months	72	0.53
STW used until legal max 24 months	92	0.68

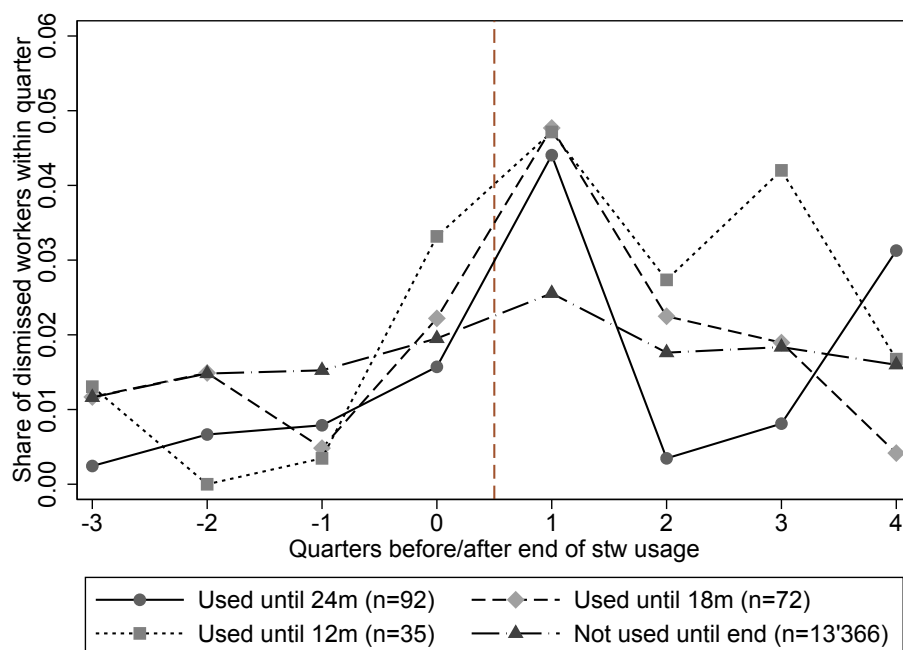
Source: SECO STW dataset

Figure 8 shows the share of dismissed workers around the last month in which an establishment collects short-time work benefits. The figure provides strong evidence that establishments that reach the maximum duration of short-time work dismiss a sizeable share of their workforce in the quarter following benefit expiration. This suggests that dismissals are indeed postponed, rather than prevented, in the few establishments that collect short-time work benefits until the legal maximum. However, for the large majority of establishments that stops collecting short-time work benefits voluntarily before they reach the legal maximum duration, we observe only a very small increase in the share of dismissals at the end of the short-time work case.

## 6.4 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

Figure 8: 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.

industries by (NUTS-II) Swiss regions. The survey thus covers a sizeable share of employment in Switzerland.<sup>37</sup> Until 2011, the FSO collected the employment data at the establishment level with very few exceptions. From 2011 onward, the survey is generally collected at the firm level. This leads to a sizeable structural break in the employment series of multi-establishment firms in the middle of the estimation sample. We thus drop 298 cases where we observe a change in the collection unit from establishment to firm in 2011.<sup>38</sup> Despite the relatively large coverage of workers in the Job Statistics, the overlap between the STW dataset and the Job Statistics proved to be quite small: establishments that applied for 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 whose short-time work application was denied. The reason is that the Job Statistics

<sup>37</sup>In 2015, the survey encompassed roughly 18'000 firms with 65'000 establishments and over 2 million workers (more than one third of total employment in Switzerland).

<sup>38</sup>Our results do not depend on the exclusion of these cases, but they tend to be more precisely estimated.

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 6.1.2. Rather, we estimate the following simplified variant of it:

$$u_{i,t} = \gamma_t + \gamma_\tau + \beta_1 STW_i + \beta_2 STW_i * I[\tau \geq 0] + \gamma X_{i,t} + \epsilon_{i,t} \quad (3)$$

Equation 3 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 ( $\delta_i$ ). If we control for case fixed effects, all time-invariant differences between firms—and hence also the variable  $STW_i$ —are absorbed from the regressions.

Using this regression model, we first study whether the approval of 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 15 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.<sup>39</sup> 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.5).

Table 15: DiD estimates of the effect of short-time work approval on the probability to drop out of the Job Statistics

	(1)	(2)	(3)	(4)
	OLS	FE	FE	OLS
VARIABLES	Dropout all $\tau$	Dropout all $\tau$	Dropout all $\tau$	Dropout $-4 \geq \tau < 4$
$I[\tau \geq 0]$ *STW approved	-0.104*** (0.035)	-0.091*** (0.029)	-0.091*** (0.029)	-0.043*** (0.016)
STW approved				-0.097*** (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 (i.e. surviving firms), we would likely underestimate the effect of approval. The reason

<sup>39</sup>The Job Statistics is a rotating survey. Hence, another reason to drop out of the sample is the sample rotation scheme. However, it appears very unlikely that the rotation scheme is systematically related to differential changes in drop out probabilities before and after application for treatment and control group.

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 16, 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 3 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 6.1.4). Overall, the evidence presented in Tables 15 and 16 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.

## 7 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 17. 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 6, 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 13, 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<sup>40</sup> 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

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<sup>40</sup>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 16: DiD estimates of the effect of short-time work approval on FTE employment

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

Robust standard errors in parentheses

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

Notes: The table shows DiD estimates, with and without case FE, examining the effect of 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.

in 2009 of CHF 856 Mio or CHF 1'580 Mio, depending on whether we use the OLS or IV estimate.

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 work benefits for each case, the unemployment insurance spend 1'256 Mio CHF on short-time work benefits for all cases that started in 2009.<sup>41</sup> 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. The reason why the direct financial effect of short-time work on the unemployment insurance could be positive even though the number of workers covered by (or the share of missed hours due to) short-time work is larger than the number of jobs saved, is that the average unemployment duration is longer than the average duration of short-time work benefit collection. An unemployment spell of a job seeker coming from an establishment whose application was denied lasted on average 9.5 months in the period 2009–2014 whereas the average duration a firm collects short-time work benefits amounts to 6.8 months.

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).

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<sup>41</sup>This amount is slightly higher than the 1.1 Billion CHF that the unemployment insurance spent in 2009 as reported by SECO (2013). The reason is that the latter refers to all payments for short-time work benefits in 2009 whereas the former refers to payments made for all cases *starting* in 2009.

- 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.
- 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 17: Cost benefit analysis of the short-time work scheme in 2009

<b>Financial benefits of short-time work</b>	<b>OLS</b>	<b>IV</b>
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
Reduction in UI benefit payments per case (in CHF)	108'850	200'400
Total cost savings in 2009 (in CHF)	856 Mio	1'580 Mio
<b>Financial costs of short-time work</b>		
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
<b>Net financial benefits of short-time work</b>		
Net financial benefits of STW per case (in CHF)	-50'750	41'100
Net financial benefits of STW in 2009 (in CHF)	<b>-400 Mio</b>	<b>324 Mio</b>

*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.

## 8 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. These agencies review these applications and approve or deny short-time work. Applications are denied in about 1 of 6 cases, and the approval rates differ considerably between cantons. 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—defined as the number of workers from an establishment that register themselves as unemployed—, hirings, and employment of the establishments before and after their application for short-time work benefits.

We find that establishments whose short-time work application was denied dismiss much more workers in the quarters following application than establishments whose application was approved, particularly in the first two quarters after application. In contrast, the evolution of dismissals is very similar in the two groups in the three years prior to the application. Using a flexible event study model that controls for fixed effects for each short-time work case, we indeed find very robust and highly statistically significant evidence that short-time work prevents layoffs. One year after application, the additional dismissals of establishments with denied short-time work applications amount to 6 percent of the establishment’s workforce compared to establishments with approved applications. After three years, the difference amounts to 10 percent. We find that it is mainly workers with compulsory and vocational education whose jobs are saved due to short-time work. We also show that the effects of short-time work approval are larger for small firms and the larger the shortfall in demand an establishment expects at the time of application. By contrast, differences across broad industry categories are very small, but the effect is larger for high-tech compared to low-tech manufacturers.

Importantly, our evidence speaks against the possibility that short-time work postpones rather than prevents dismissals. To better understand this result, we also study what happens to layoffs when short-time work benefits run out. We find that less than 2% of all short-time work cases reach the legal maximum short-time work duration. In these few cases, there is a clear increase in dismissals in the quarter following benefit expiration. In the remaining more than 98% of cases, however, there is only a very small increase in dismissals when they stop collecting short-time work benefits.



Although we believe that our DiD estimates provide a valid benchmark for the causal effect of approval of short-time work on dismissals, it is conceivable that our DiD estimates over- or understate the true effect of short-time work. The direction of a possible bias depends on what is driving cantons' decisions to deny or approve short-time work. In order to see whether our results are robust, we thus apply an Instrumental Variable (IV) approach that directly exploits the idiosyncrasies in cantonal approval decisions. These IV estimations corroborate our results from the DiD estimates. In fact, the estimated negative effects of short-time work on dismissals are even two to three times larger than the corresponding DiD estimates. Our estimates based on the Job Statistics provide further support for our unemployment estimates. We observe that establishments whose short-time work application is denied have a higher risk of dropping out of the Job Statistics than establishments that receive benefits. This indicates that some establishments have to close down. We also find suggestive evidence that approval of short-time work increases FTE employment, especially in small firms.

In a last step, we use our estimates to quantify the direct fiscal benefits of the Swiss short-time work scheme—which arise in the form of a reduction in spending on unemployment benefits—, and compare them to the spending on short-time work benefits. These back-of-the-envelope computations suggest that the direct fiscal benefits may in fact be large enough to fully compensate the total fiscal spending on short-time work benefits even though the number of workers covered by (or the share of missed hours due to) short-time work is larger than the number of jobs saved. The reason is that the average unemployment duration is longer than the average duration of short-time work benefit collection. Hence, our estimates indicate that the deadweight losses associated with the Swiss short-time work scheme are quite limited, i.e. there are not many cases in which employers get paid for jobs that they would have retained even if short-time work benefits had not been paid.

Overall, our results provide strong and very robust evidence that the Swiss short-time work scheme prevents unemployment. However, some 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. This rather quick recovery may have favored the efficiency of the short-time work scheme.<sup>42</sup> Second, the point estimates of our OLS and IV

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<sup>42</sup>However, the quick recovery might in turn—at least in part—have been a result of the short-time work scheme.

estimation differ quite substantially, which prevents us from drawing strong conclusions on the extent to which the direct fiscal benefits of short-time work cover the direct costs. Third, our firm-level estimates do not take into account general equilibrium effects. In particular, they do not account for 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 Further Tables

Table A.1: 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.2: 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

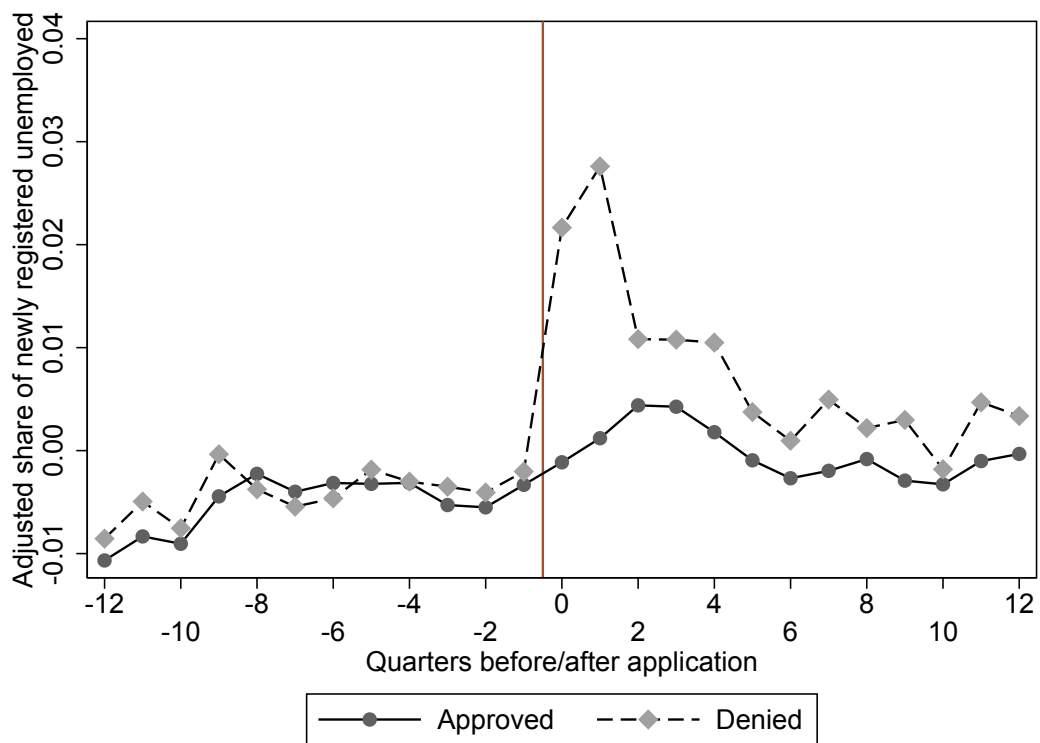
Table A.3: Number of approved and denied cases by canton

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

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

*Source:* SECO, STW dataset

Figure A.1: Adjusted Share of dismissed workers, by approval decision



*Notes:* The figure plots the average residuals of a regression of the 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.

Table A.4: 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 (0.012)	0.000 (0.012)	0.000 (0.011)	-0.006 (0.013)	0.004 (0.012)	0.003 (0.010)
Appl. $\tau - 3$ to $\tau - 1$	0.002 (0.005)	-0.001 (0.005)	-0.001 (0.005)	-0.007 (0.005)	0.001 (0.005)	0.000 (0.004)
Appl. $\tau$ to $\tau + 3$	-0.057*** (0.010)	-0.054*** (0.010)	-0.059*** (0.009)	-0.080*** (0.011)	-0.055*** (0.010)	-0.057*** (0.008)
Appl. $\tau + 4$ to $\tau + 8$	-0.015 (0.012)	-0.024** (0.012)	-0.025** (0.011)	-0.025* (0.013)	-0.021* (0.012)	-0.022** (0.010)
Appl. $\tau + 9$ to $\tau + 12$	-0.006 (0.010)	-0.018* (0.009)	-0.019** (0.009)	-0.022** (0.011)	-0.017* (0.010)	-0.014* (0.008)
Appl. $\tau$ to $\tau + 12$	-0.079*** (0.029)	-0.096*** (0.028)	-0.103*** (0.026)	-0.128*** (0.031)	-0.092*** (0.028)	-0.093*** (0.024)
$N$	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

*Notes:* 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.5: 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