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Labour hoarding in Germany

Employment effects of short-time work during
the crises

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Contents

Abstract

During the crisis (2008-09) Germany experienced a huge decrease in GDP. Employment, however, remained surprisingly stable. A whole strand of literature has aimed at quantifying the contribution of short-time work to the German labour market miracle. In the course of this literature we estimate the treatment effect of short-time work on employment at establishment level using a dynamic propensity score matching approach. The analysis is based on data from the IAB Establishment Panel combined with administrative data on short-time work establishments from the Federal Employment Agency. Our results do not indicate any treatment effect of short-time work on employment.

Zusammenfassung

Während der Wirtschaftskrise (2008-09) erlebte auch Deutschland einen starken Rückgang des Bruttoinlandsprodukts. Die Beschäftigung blieb jedoch erstaunlich stabil. Seitdem zielen viele Publikationen darauf, den Anteil des arbeitsmarktpolitischen Instruments Kurzarbeit am sogenannten deutschen Arbeitsmarktwunder zu quantifizieren. Im Zuge dieser Literatur schätzen wir die kausale Wirkung von Kurzarbeit auf das Beschäftigungsniveau von Betrieben und nutzen dafür den Propensity Score Matching Ansatz. Die Analyse basiert auf Daten des IAB-Beschäftigtenpanels kombiniert mit administrativen Daten der Bundesagentur für Arbeit zur tatsächlichen betrieblichen Inanspruchnahme von Kurzarbeit. Unsere Ergebnisse geben keinerlei Hinweise auf eine kausale Wirkung von Kurzarbeit auf das betriebliche Beschäftigungsniveau.

Introduction

Facing the worldwide economic situation of 2008-09 with the strongest downturn since years, combating unemployment was one of the most important economic tasks of European Countries. During this economic crisis Germany experienced a huge decrease in GDP, but in contrast to other European Countries its employment remained surprisingly stable. This "German labour market miracle" is often attributed to the specific short-time work scheme: While labour market policies of most other countries intervene after unemployment sets in, short-time work mainly aims at preventing individual unemployment. In doing so it avoids passive income support and keeps human capital at the firm level by retaining qualified employees within the firm until the upswing.

Basically, short-time work (STW) is a reduction in working hours below standard normal working time[1] in order to compensate for fluctuations in labour demand. The idea is that through a reduction in working hours employment can be maintained, despite periods of declining sales. Due to such a reduction, labour input wage costs are lowered and output is reduced, while the necessity of dismissals is avoided. The use of STW is not necessarily connected with a utilization of STW benefits. If made use of, the STW allowance paid by the Employment Agency compensates for part of the loss of income of the employees affected.

The German STW scheme (*Kurzarbeit*), as a classical instrument of active labour market policy, originally dates back to 1927.[2] The scheme aims at a redistribution of a temporary shrunken volume of work in order to maintain employment and avoid dismissals. It was based on the idea of saving human capital for the firm and reducing the individual risk of unemployment. The STW scheme subsequently was adjusted until today. Nowadays the Third Volume of the Social Code (*Sozialgesetzbuch III*) regulates three types of short-time work: STW during firms' restructuring process (*Transferkurzarbeit*), which was analysed by Mosley/Kruppe (1996), STW due to seasonal labour shortage (*Saisonkurzarbeit*) and STW for economic reasons (*Konjunkturelle Kurzarbeit*).[3]

This paper focuses on the latter type of STW – STW for economic reasons – as it has been used most during the 2008-09 crisis, with up to 56,000 establishments making use of the program and 1.4 million employees receiving STW benefits (see figure 1).

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[1] Standard normal working time here means the usually worked hours agreed to on collective or firm level.

[2] Gesetz über Arbeitsvermittlung und Arbeitslosenversicherung (*AVAVG*) from 07-16-1927.

[3] While STW for economic reasons is regulated in §§ 96ff (§§ 169ff before April 2012) of Social Code Book III, some other regulations are also important for the use of the scheme, e.g. the Co-determination Act (*Mitbestimmungsgesetz*), the Law concerning Labour Relations at the Workplace (*Betriebsverfassungsgesetz*), the Protection Against Dismissal Act (*Kündigungsschutzgesetz*), and collective bargaining agreements on the national, regional and/or firm levels.

Figure 1
Usage of short-time work in Germany
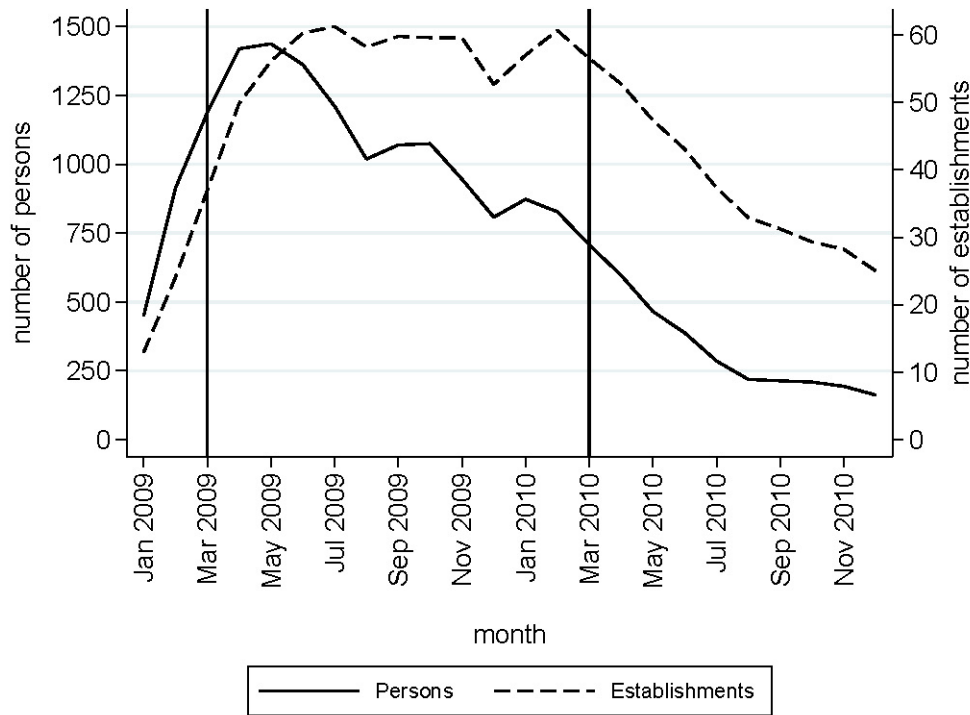
Numbers in thousands.

During the years 2009 and 2010 the Federal Employment Agency paid 4.7 Million Euros in STW benefits (Federal Employment Agency, 2012a). In the light of such cost, the effectiveness of the STW scheme has been the subject of a new strand of literature that analyses the employment preserving effect of STW. However, the contribution of STW to the German labour market miracle is still not without dispute. Other factors that may have contributed to the stable employment situation are the reduction of hours accumulated in working time accounts as well as the use of temporary or agency workers (Mosley/Kruppe, 1996; Burda/Hunt, 2011; Dietz/Stops/Walwei, 2011). These measures have become increasingly common and should provide a buffer for downward adjustment during the economic cycle. The enormous use of STW during the last recession is hence particularly surprising and undermines the importance of the question of STW being an effective measure.

We add to the existing line of literature by examining the job preserving effect of STW at the establishment level by applying methods of propensity score matching. In doing so we are able to provide evidence on the causal effect of STW on employment changes. As most OECD countries - amongst them many European countries - have operated or extended a STW program during the recession (Hijzen/Venn, 2011), the effectiveness of STW has also been disputed in many other countries. The analysis of

the German STW scheme hence provides insights that can also benefit other countries.

The remainder of the paper is organized as follows. Section 2 reviews the existing literature, while section 3 explains the institutional framework of the German STW scheme. In section 4 we present the data used in the analysis; our empirical strategy is explained in section 5. The results of our analysis as well as robustness checks are presented in section 6 and 7. Section 8 concludes.

1 Literature review

As mentioned in section 1, to this day the contribution of STW to the German labour market miracle is disputed within the literature. Burda/Hunt (2011), for instance, argue that working time accounts acted as a substitute for STW. Their empirical analysis finds that the stable labour market situation during the recession may be explained to 60 % by the previously reluctant hiring behaviour of employers together with wage moderation, that was related to a decline in the bargaining power of labour unions. On a similar note, Moeller (2010) stresses that firms profited from an increased flexibility thanks to the behaviour of unions and management as well as the labour market reforms between 2003 and 2005.

While these studies are concerned with explaining the German labour market miracle per se, several studies focus on the analysis of a potential employment preserving effect of STW. These papers can be divided into two broad categories, namely country level- and establishment level studies. Country level studies draw on national data of one or more countries to estimate the effect of STW on national employment, whereas establishment level studies analyse the role of STW take-up within firms.

In general, the existing country level studies find - with restrictions - an employment preserving effect of STW. Hijzen/Venn (2011), for instance, apply a difference-in-difference approach to data from the OECD (2010). The authors estimate that during the 2008-09 recession the German and Japanese STW schemes had the largest impact on employment. Differentiating between permanent and temporary jobs, the results provide clear evidence that STW preserved permanent employment. However, this is not true for temporary jobs. Drawing on the same data, Cahuc/Carcillo (2011) obtain very similar results using an instrumental variable approach. In their paper, features of STW schemes before the crisis serve as an instrument for national STW take-up rates. The estimated influence of the STW take-up rate on the permanent (temporary) employment rate is strongly positive (insignificant). An instrumental variable approach is also applied to the same data by Boeri/Bruecker (2011), where the time elapsed in each country since the first introduction of STW represents the instrument. Both their ordinary least squares (OLS) and instrumental variable (IV) estimates indi-

cate that during the last crisis STW contributed to the reduction of job losses, however, only if falls in output are sufficiently large. The authors stress that STW can actually increase employment losses during mild recessions and upturns. Their econometric results also point to the existence of large deadweight cost of STW in some countries. In line with these results, Van Audenrode (1994) shows that only generous enough STW programs are able to bring about an efficient level of both employment and working hours. Finally, using a panel estimation on 27 European countries from second quarter 1991 to third quarter 2009, Arpaia et al. (2010) also confirm that STW contributed to the reduction in employment losses during the last crisis.

When looking at studies carried out at the firm level, however, empirical evidence is not so clear-cut. Moreover, to our best knowledge so far only three firm-level studies analysing the employment preserving effect of STW exist for Germany. Bellman/Gerner (2011) use German data from the IAB establishment panel to conduct a matching among a set of establishments that use STW. The same is done for a set of firms without a scheme. In order to estimate the effect of the last crisis, a difference-in-difference estimator is applied to each set where the outcome variable is the change in employment from June 2008 to June 2009. As a result Germany STW did not contribute to the preservation of jobs during the last crisis. The results are confirmed by Bellmann/Gerner/Upward (2012), where they analysed the job and worker flows as firms reaction to the crises in relation to the use of STW and working-time accounts: The regression estimates do not show any evidence of increased labour hoarding by reducing layoffs. The micro analysis of Boeri/Bruecker (2011) is also based on the IAB establishment panel. The authors exploit information on firms' prior experience with the program to instrument the use of STW in 2009. Estimating the effect on the log employment change from June 2008 to June 2009 their IV results show that an increase in the share of short-time workers by 1 percent raises the firm's employment by about 0.37 percent. The authors calculate that the point estimates correspond to about 400,000 jobs saved by the German STW scheme. Note that the studies of Bellman/Gerner (2011) and Boeri/Bruecker (2011) analyse the effect of STW on the employment change between June 2008 and June 2009. We are well aware that this was done due to data limitations existing at the time the analyses were carried out. However, this may be problematic since in June 2009 the crisis was still in full effect and crucial firm-level employment changes may have taken place in later months. The study of Calavrezo/Duhautois/Walkowiak (2009) refers to the period from 1996 to 2004. The authors analyse the effect of STW on redundancies using French Data on 36,000 establishments. They conclude that STW does not protect from redundancies. Quite the contrary, the authors find a strong positive relation between the number of STW days per employee and the number of redundancies.

Finally, let us turn to studies which analyse the determinants of the demand for STW on the firm level. Using a probit regression Crimmann/Wiessner (2009) find a firm more likely to implement STW in one year if it experienced a bad profit situation in the previous year. Also, negative expectations about the future render a firm more likely to adopt a STW scheme. Moreover, the likelihood of an establishment using STW is found to increase with the skill level of the workforce. These results are confirmed by Crimmann/Wießner/Bellmann (2010a), who additionally find that export oriented firms are more likely to operate a STW scheme. Bohachova/Boockmann/Buch (2011) rather focus on the determinants of total versus subsidized labour hoarding (STW). They find that subsidized labour hoarding mainly occurs in firms that are exposed to high competition, while overall labour hoarding can be observed in firms facing low competition. Crimmann/Wießner/Bellmann (2010b) analyse the determinants of an establishment's decision for or against STW as well as the factors influencing the intensity of STW as measured by the share of employees working short-time. Interestingly, the same variable may affect the probability of STW differently than its intensity. In particular, establishments use STW more intensively the harder they are hit by the crisis. Boeri/Bruecker (2011) also regress the STW take-up rate, i.e. the intensity of STW, on firm characteristics. A higher share of workers with a university degree is found to negatively correlate with the STW take-up rate. The authors conclude that shocks rather than structural problems of firms determine STW take-up rates.

2 German institutional settings

Originally, the employer has to carry the entire burden of the entrepreneurial firm's risk. He must meet his payroll even in times of slack demand. The STW scheme enables the employer to reduce working time and hence the wage bill as an alternative to dismissals in the case of inevitable short-term economic adjustments. Hours can be cut up to 100%, while the short-time allowance partially protects affected employees against their resulting wage loss. Currently the replacement rates paid for the lost hours are 60% (67% for employees with at least one child) of the net wage loss. These are the same replacement rates used in calculating unemployment benefits. The employer adds the short-time allowance to the pay-check for hours worked and is reimbursed by the Federal Employment Agency.

The social security contributions are normally paid half and half by the employer and the employee; for the hours cut the employer also has to pay a percentage of the em-

ployee's part.[4] The standard duration for receiving short-time allowance for cyclical reasons is six months, but the maximum duration of STW was prolonged several times during the crisis up to 24 months.

The implementation of the short-time work scheme is highly flexible. The program is drawn up for almost instant use, in the case of an act of god, for example, from the first day on. Of course, the employer has to specify the planned volume of the working time reduction in advance. The firm as a whole does not have to shorten working time. Normally, the loss of work has to be temporary and take place for at least one third of all employees (excluding apprentices) of a firm or of a specific division of a firm for at least four weeks with a minimum of a 10% reduction in the firms standard working time for those employees affected.[5] The reduction can have a maximum of 100% with zero hours actually worked.

So the employer is able to use the program in a very controlled manner. An increase in working time (in cases of high short-term demand) is also possible without stopping the program if the general eligibility conditions for the period as a whole are still fulfilled.

Nevertheless, the loss of working time must not be usual for the firm, branch or season. It must not be caused by weather and not be exclusively for organizational reasons. Furthermore, all other possibilities of adjustment have to be checked and used, such as reducing overtime, vacations or shifting employees to other company branches or divisions.

In addition, the implementation of STW needs to be agreed upon by the workers council. This is based on the fact that a change in working hours is a change in the employment contract that underlies co-determination. The workers council has extensive rights regarding the need, the volume and the circumstances of short-time work. Employees may for example give their approval only if the short-time allowance is granted by the employment service. Also the workers council can initially demand short-time work and, if the employer does not agree, call a unification office (*Einigungsstelle*). This initiation right is derived from the workers councils' function of contributing in order to avoid dismissals. In smaller firms or even if no workers council exists the introduction of STW as a change in the work contract has to be agreed by every individual employee.

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[4] Before February 1st 2009 this share amounted to 80 percent. In the course of the crisis the program became more generous, and 50 percent of this payment were reimbursed by the Federal Employment Agency from February 1st 2009 to March 31st 2012. Between July 1st 2009 and March 31st 2012, 100 percent were reimbursed from the 6th month of STW on or if short-time workers participated in training measures.

[5] Between February 2009 and December 2010 the wage cut could affect less than one third of all employees. In this case only workers with a wage loss of more than ten percent were entitled to STW compensation.

Possibly, this is one reason for the low use in small firms. In case of illness the benefit is paid as long as wages would also be paid (in general up to 6 weeks). In contrast, for days with loss of work for other reasons, such as vacations or public holidays, no compensation is paid.

3 Data

3.1 Data sources

In order to analyse how the use of STW affects employment at the establishment level we use three different data sets, which can be combined via the establishment number. This number is administered by the Federal Employment Agency and uniquely identifies an establishment.

First, the information about firm characteristics is taken from the establishment panel of the Institute for Employment Research (IAB). This representative panel data set is a yearly survey of by now approximately 16,000 establishments, where the large majority of establishments are interviewed several years in a row. The sampling is based on the establishment file of the Federal Employment Agency, which contains all establishments with at least one employee subject to social security as of June 30th of the year previous to the interview. As a result of the sampling design large establishments, small federal states, small industries and the manufacturing industry in East Germany are over-represented (Fischer et al., 2009). The IAB establishment panel is a rich data source providing information on general firm characteristics such as industry, firm size and the composition of the work force. Moreover, information is given on the profit situation and business volume, the experienced competitive pressure as well as a variety of several other firm characteristics used as control variables in the analysis. Most of our control variables are constructed from the 2009 wave of the panel. However, we also use information from earlier waves to capture the pre-crisis condition of the establishments. The establishment panel also provides information on short-time work. Establishments were asked two questions: "Did you have to make use of short-time work in the first half of 2009?" and "If so, how many employees did this affect?". However, we do not exploit this information to identify STW firms or the extent to which they used STW.

Instead, we rely on the so-called billing lists (*Abrechnungslisten*) of the Federal Employment Agency, which contain monthly information on all German STW establishments. Our data range from January 2009 to December 2010, and hence cover the latest period of extensive STW usage (Federal Employment Agency, 2012b). The billing lists are a unique data source as they provide detailed information on the use of STW at the establishment level. They include the months during which the STW establishments operated a scheme, the exact number of short-time workers and regular em-

ployees along with the average percentage of hours cut due to short-time work.[6] All numbers refer to actually realized STW. The information on STW provided in the billing lists is hence superior to the information contained in the IAB establishment panel, which merely tells us whether an establishment did operate a scheme in the first half of 2009 along with the number of short-time workers if applicable. For this reason STW firms along with their usage of STW are solely identified from the billing lists.

As a final data source, we draw on the employee history of the IAB (*Beschäftigtenhistorik - BeH*), which contains information on all persons marginally employed or subject to social security contributions. The data available to us are recorded on a daily basis ranging from January 1st 2008 to December 31st 2010 and include information on the beginning and end of employment as well as the educational level of each employee. We use this data set to compute various outcome variables. For this purpose we aggregate the person level data to the establishment level and coarsen the information to be exact to the month. In doing so we obtain at the establishment level the monthly number of employees subject to social security and the monthly turnover, both in total and differentiated by employees' education.

3.2 Combination of the data sets

We start by merging the IAB establishment panel and the billing lists via the establishment number. Only firms that occur in both the establishment panel and the billing lists are defined as STW establishments. In case the total number of employees given in the billing lists differs from the establishment panel by more than 50 %, we exclude the respective establishment from our data. In doing so, we ensure to merge information pertaining to the same establishment. Table 1 shows the number of STW firms according to the question asked in the establishment panel and according to the billing lists, after having merged the two data sets.

It may be surprising at first, that only 67% of the STW establishments found in the billing lists state that they conducted STW in the establishment panel. The reason for this deviation is that the establishment panel only asks about short-time work in the first half of 2009. When we exclude all establishments that start STW after June 2009 from the billing lists, we find a 90% match between the billing lists and the respective information in the establishment panel (see lower part of Table 1).

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[6] The average percentage of hours cut is given in categories as follows: 25 percent, 26 to 50 percent, 51 to 75 percent, 76 to 99 percent, 100 percent.

Table 1

STW firms according to IAB establishment panel and billing lists

No establishments excluded.			
	from establishment panel		
from billing list	STW establishments	Non-STW establishments	Total
STW establishments	1156 66.9%	571 33.1%	1727 100%
Non-STW establishments	259 2.3%	11096 97.7%	11355 100%
Total	1415 10.8%	11667 89.2%	13082 100%

Establishments starting STW after June 2009 excluded from billing lists.			
	from establishment panel		
from billing list	STW establishments	Non-STW establishments	Total
STW establishments	1125 90.4%	119 9.6%	1243 100%
Non-STW establishments	259 2.3%	11096 97.7%	11355 100%
Total	1383 11.0%	11251 89.0%	12589 100%

Source: Own calculations

Figure 2 plots the number of establishments starting STW per month. The left panel only uses the data from the billing lists, while the right panel is based on the combined data set (billing lists and establishment panel). From the billing lists we know that most establishments began implementing STW in April of 2009. In our combined data set, the starting times are slightly distorted, because large establishments - which are over-represented in the establishment panel - tend to start STW earlier on.

The combined data set serves as a basis to conduct the propensity score matching. From the matching we obtain the establishment numbers of treated and non-treated firms. Therefore, we are able to add the number of employees and the turnover from the employee history.

Figure 2
Establishments starting short-time work

in billing lists

in billing lists and establishment panel
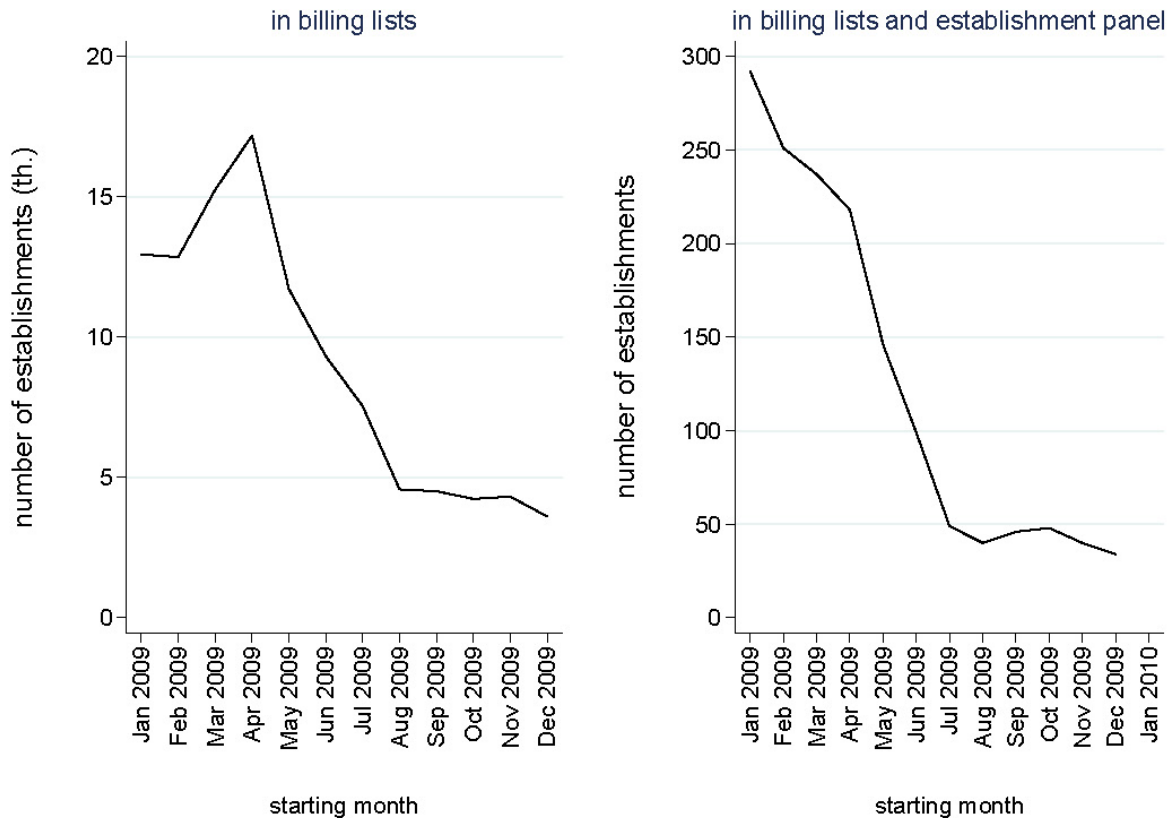
Note: While the left panel displays the number of establishments from the billing lists only, the right panel shows the number of establishments that are contained in both the billing lists and the establishment panel. Note that the scale for the left and right-hand panel differs.

Source: Own calculations

4 Methods

4.1 The evaluation problem

Our empirical strategy builds on the evaluation problem framework of Rubin (1974) and Rosenbaum/Rubin (1983, 1985). We are interested in estimating the effect of a binary treatment represented by $d \in 0,1$ on various outcome variables $Y_i (i = 1, \ , N)$. In our case, treatment is the implementation of a STW program at the firm level. Since we want to provide insight on the employment effect of the program, we choose as our first outcome variable the number of employees liable to social security. To ensure comparability across firms this number is measured relative to the number of employees as of the first quarter of 2008. Our second outcome variable is the establishment's turnover, i.e. voluntary and involuntary quits as a percentage of total employment. Potential outcomes are denoted by Y_i^d, where d=1 if establishment i is treated and zero otherwise.

The parameter of interest is the average treatment effect on the treated (ATT), which can be represented as

$$\gamma = E[Y_i^1 \mid d_i = 1] - E[Y_i^0 \mid d_i = 1]. \tag{1}$$

The fundamental evaluation problem results from the fact that only $E[Y_i^1 \mid d_i = 1]$ can be observed. The counterfactual $E[Y_i^0 \mid d_i = 1]$ represents the expected outcome of non-STW establishments had they operated a scheme, and cannot be observed within the data. The ATT must hence be estimated by

$$\gamma = E[Y_i^1 \mid d_i = 1] - E[Y_i^0 \mid d_i = 0]. \tag{2}$$

For equation (2) to be identified certain assumptions need to be fulfilled (Caliendo/Kopeinig, 2008).

Assumptions

$$Y^0, Y^1 \coprod d \mid X \tag{i}$$

$$0 < P(d = 1 \mid X) < 1 \tag{ii}$$

Assumption (i) is referred to as selection on observables[7,] where X is a set of observable covariates, which are not affected by treatment. Selection on observables states that given X the potential outcomes are independent of treatment assignment. Imagine two establishments with the exact same values of X. These two establishments have the same probability of receiving treatment, which in turn means that their respective outcomes are not correlated to treatment assignment, but only to the values of X. For assumption (i) to be fulfilled, all variables that influence the probability of treatment and the potential outcome simultaneously have to be observed.

Due to the richness of the data at hand, we are confident that assuming selection on observables is justified in our case. The IAB establishment panel – from which the explanatory variables are constructed– does not only provide us with data on general firm characteristics such as firm size, industry and the composition of the workforce (including the share of agency workers and the share of workers with a fixed-term contract). It also contains information on all relevant factors that determine an establishment's decision to implement short-time work.[8] To name the most important ones, we can control

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[7] In the literature the terms conditional independence assumption (CIA) or unconfoundedness are used as synonyms.

[8] As explained in section 2 various studies have examined the factors driving an establishment's decision to implement STW.

for the existence of working time accounts, difficulties finding skilled workers, competitive pressure, business volume and profit situation. Most importantly, the establishment panel interviews establishments on their expectations on how the two latter measures will develop.

The common support assumption (ii) ensures that establishments with the same values of X must have a positive probability of both being treated and non-treated. Put differently, it states that any combination of X values in the treatment group must also occur in the non-treatment group, ruling out perfect predictability of treatment assignment.

To estimate the ATT, equation (2) requires taking into account a whole set of variables. In order to avoid the curse of dimensionality we use the propensity score $p(x)$ as a balancing score as suggested by Rosenbaum/Rubin (1983). The propensity score is estimated using a logit regression. The treatment and control group are constructed using a nearest neighbour matching with replacement[9] within a calliper of 0.05, where a maximum of 5 non-STW establishments can be matched to each STW establishment. We choose this matching algorithm as it yields the best results in terms of bias reduction between the treatment and control group. The estimation of the propensity score and the matching procedure are conducting using the STATA module psmatch2 (Leuven/Sianesi, 2003). The estimated treatment affect is computed as

$$\gamma = E[Y^1 \,|\, d = 1, p(x)] - E[Y^0 \,|\, d = 0, p(x)]. \tag{3}$$

4.2 Dynamic treatment evaluation

From our combined data set (IAB establishment panel and billing lists) we know that establishments start STW at different points in time (see the right-hand panel of figure 2). Hence there is no fixed point in time at which treatment starts - the timing of treatment is non-static. As explained in section 3, establishments can only implement the treatment STW as long as they suffer from a considerable lack of work. Whether or not we observe a treatment consequently depends (amongst other things) on the duration the lack of work persist. Fredriksson/Johansson (2008) show that in this case estimating the treatment effect based on a static propensity score matching approach yields biased estimates. Moreover, the starting point of the STW program is likely to be linked to the outcome variables: Imagine two identical establishments that implement the same STW scheme only at different points in time. Even in this case it would be implausible to assume that the program had the same employment effect in both establishments. As an alternative to the standard static matching approach, Fredriksson/Johansson (2008) propose to match individuals that are treated at time t with indi-

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[9]   Matching with replacement implies that an establishment of the comparison group may serve as a match for several treated establishments.

viduals that are not treated at time t. This is what we will henceforth refer to as dynamic matching.

The concrete implementation of the dynamic matching follows Sianesi (2004). In our data, we are able to observe the occurrence of STW between January 2009 and December 2010. The right-hand panel of figure 2 shows that the vast majority of all establishments in our combined data set starts STW in 2009. For this reason we define the first through fourth quarter of 2009[10] as possible starting points for STW. Our first treatment group are establishments that start STW in 2009 *Q1*. These are matched to establishments that have not started STW until 2009 *Q1*.

By matching the first treatment and comparison group we obtain our first matched sample. The matched sample is used to estimate the average treatment effect on the treated (ATT) for four quarters after the start of STW[11,] where the outcomes of interest are the number of employees liable to social security relative to 2008 *Q1* as well as the turnover. The ATT measures the effect of STW on the outcome as the difference between the mean outcome within the treatment group and the mean outcome within the matched control group. We obtain four treatment effects - one for each quarter after the start of STW - that represent the average impact of STW in that quarter Sianesi (2004). Now, we repeat this procedure for quarters 2009 *Q2* through 2009 *Q4*. Our second matched sample is hence made up of establishments that start STW in 2009 *Q2* (treatment group) and matched establishments that have not implemented STW until 2009 *Q2* (control group). For each matched sample the ATT is estimated for four quarters after the start of STW. We do so because this is the maximum number of quarters after treatment we are able to observe for each matched sample.

Choosing this dynamic matching approach implies a crucial definition of the non-treated establishment: In each quarter the non-treated establishments may include both firms that never implement STW and firms that start to operate a scheme in later quarters. Table 2 shows for each quarter of 2009 *Q1* through 2009 *Q4* how many firms of the control group do indeed start STW later on.

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[10] Henceforth referred to as 2009 *Q1* through 2009 *Q4*.

[11] For the first matched sample we hence estimate the ATT for the quarters 2009 *Q2* through 2010 *Q1*.

Table 2
Establishments of control group starting STW in later quarter

Starting point of STW	# of establishments in matched control group	# thereof starting STW in later quarter
2009 *Q1*	1607	424
2009 *Q2*	1349	187
2009 *Q3*	496	53
2009 *Q4*	481	32

Source: Own calculations

Following the wording of Stephan (2008) the treated establishments are "joining" the STW program in the respective quarter, while the non-treated establishments are "waiting", meaning they do not start treatment until the end of the quarter. In this case the estimated treatment effects represent the impact of implementing STW in a given quarter compared to waiting longer (Stephan, 2008). We choose this "joining" versus "waiting" approach for the definition of the treatment and control group for the following reason. In our non-experimental setting, firms may expect the experienced decline in demand to end soon and hence decide not to implement STW. Restricting the comparison group to firms who never participated in STW would thus be based on expected future outcomes. As a consequence matching conditional on observables may not remove selectivity entirely (Stephan, 2008). When treatment is non-static the treatment effect should hence correctly be measured using the "joining" versus "waiting" approach. This is undermined by Stephan (2008), who shows empirically that the definition of non-treatment matters. In her study, the estimated treatment effect of active labour market programs is larger when the comparison group is composed of "waiting" individuals as compared to a comparison group that only includes individuals that never took part in the program.

5 Results

To answer the question whether STW has a causal effect on the preservation of employment at the establishment level, we conduct a dynamic propensity score matching as described in section 5.

In a first step, the propensity score is estimated using a logit regression. As described above, this was done separately for each subgroup from 2009 *Q1* through 2009 *Q4*. We prefer the logit over the probit model, as it has a larger probability mass at its margins. In a binary treatment case the two models, however, produce similar results (Caliendo/Kopeinig, 2008). In our logit regression, we include a vast number of varia-

bles for the sample members' characteristics.[12] In the following, the explanatory variables used to estimate the propensity score are described.

Following Boeri/Bruecker (2011) we control for the profit situation in 2007 as a pre-crises indicator. This is done to control for the possibility that structural characteristics may influence a firm's decision to implement STW. The profit situation in 2008 – immediately before the use of STW – is expected to exert a positive influence on the probability of STW. Burda/Hunt (2011) as well as Bohachova/Boockmann/Buch (2011) argue that expectations about a near end of the recession are likely to affect employers' labour hoarding decisions. For this reason we control for firms' 2008 expectations about profit development in 2009. Moreover, expectations about the future may be driven by recent changes in the profit situation. Consequently, we include two dummy variables that measure the change in the profit situation from 2007 to 2008.

Important context variables are functional equivalents for short-time work, i.e. "other potential personnel alternative strategies combining flexibility in response to market contingency with avoidance of excessive turnover costs" (Mosley/Kruppe, 1996: 600). Therefore we include information on the existence of working time accounts and the respective share of agency workers, marginally employed, part-time workers and workers with a fixed-term contract. However, a firm's flexibility may also be influenced by the existence of a works council, collective agreements and firm size.

As we know from Möller (2010) the crisis hit those firms the hardest which had a high share of exports and experienced high competitive pressure. As STW is an instrument of labour hoarding we control for the share of high and low qualified workers, respectively. Furthermore we control whether firms' have difficulties finding skilled workers. Industry and region are included as further control variables. The results of the logit regression referring to the 2009 *Q1* sample are presented in Table A.2 of the appendix.[13]

The control variables included in the logit regression make it likely that the outcome of the treated and control group, given the propensity score, differs only due to treatment, and hence, the conditional independence assumption holds. The matching procedure successfully reduces the bias between the treatment and the control group. After matching in almost all cases bias is below five percent. Table A.2 of the appendix (referring to the matching conducted for 2009 *Q1*) shows that after matching the means of

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[12] For a detailed list of these variables see Table A.1 of the appendix, which displays the summary statistics for the 2009 *Q1* sample. The remaining summary statistics are available upon request.

[13] The results for the 2009 *Q2* through 2009 *Q4* samples are presented in Table A.3 through A 5 of the appendix.

the covariates do not differ significantly anymore. The pseudo R^2 from a logit regression based on the matched sample shows that within the matched sample treatment participation can no longer be explained by the covariates.

After matching we calculate our first outcome variable for each firm: the level of employment subject to social security contributions. The employment level is always measured relative to 2008 *Q1* to ensure comparability across firms. This is done on a quarterly basis from start quarter of STW of the treatment group onwards. Due to data restrictions (right censoring) we include four quarters after the entry in STW. Then we estimate the difference in the mean outcome between the treatment and control group, the average treatment effect on the treated (ATT, see section 5).

If STW has the assumed affect, namely preventing individual unemployment by keeping the individuals employed, there should be a difference between the two groups, in the way that employment falls significantly faster in control establishments than in treated ones. In other words, the ATT would be positive. But what we observe is no significant difference in the development of the employment level between the two groups. The detailed results are displayed in Table A.6 of the appendix. Figure 3 displays these results graphically. Each panel shows the ATT for the four sub-samples (starting STW in 2009 *Q1* through 2009 *Q4*) for four quarters after treatment, respectively. One can see that the 95% confidence interval for the ATT always includes zero, making a statement about the direction of the treatment effect impossible.

Figure 3
Treatment effect (ATT) of short-time work - change in employment
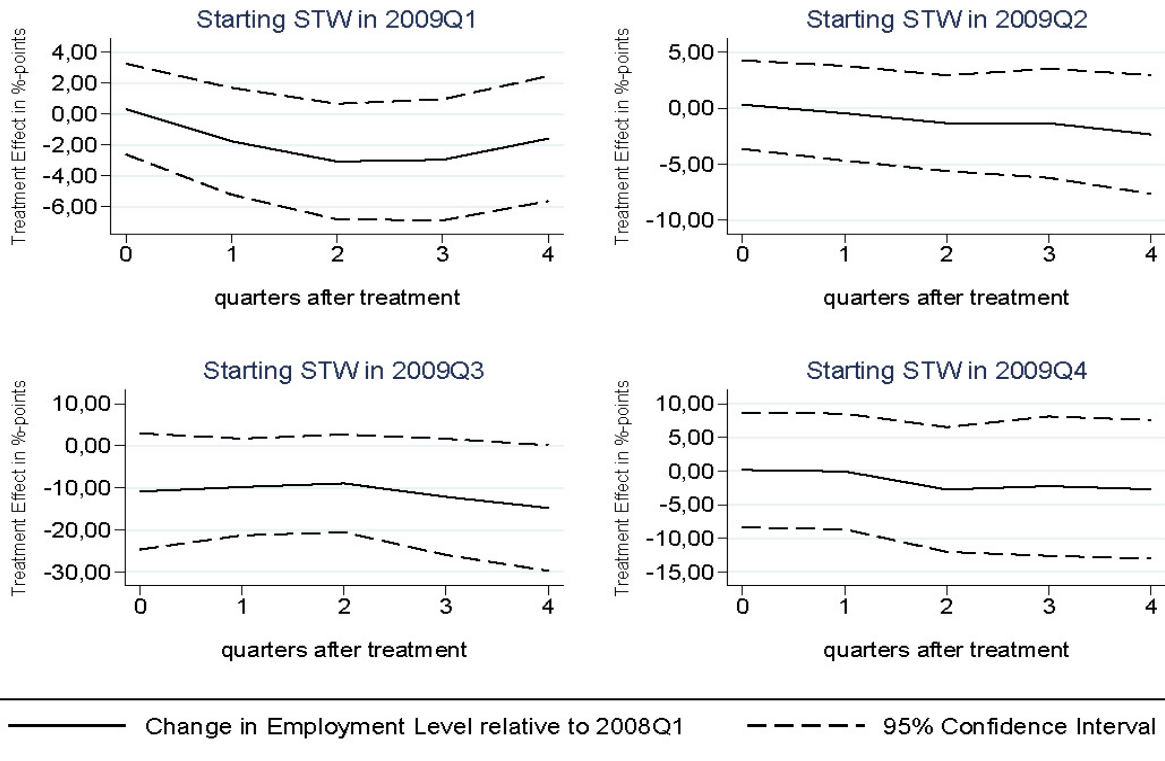
Source: Own calculations

However, firms' workforce may not be homogenous in terms of their level of qualifications. From a theoretical point of view, there could be different effects for different skill groups. While especially low skilled workers are more easily to hire, firms may have more difficulties in recruiting middle and at most high skilled workers. Additionally, the last two groups may have firm specific skills and establishments are highly interested in keeping them. To control for this fact, we calculated a second outcome variable, differentiating the level of employment subject to social security contributions into three groups: high, middle and low skilled workers. But again, the treatment effect on the treated was constantly insignificant. The insignificant results obtained above may be driven by the fact that STW is designed to act on dismissals but not on hires. The outcome variables used above do not account for this effect as they use the net employment change. For this reason we calculate a third outcome variable, the monthly turnover rate, defined as the share of employees leaving the firm (in-)voluntarily. Nevertheless, again we find no significant difference between those firms using STW and those who do not (see figure 4).

Figure 4
Treatment effect (ATT) of short-time work – turnover
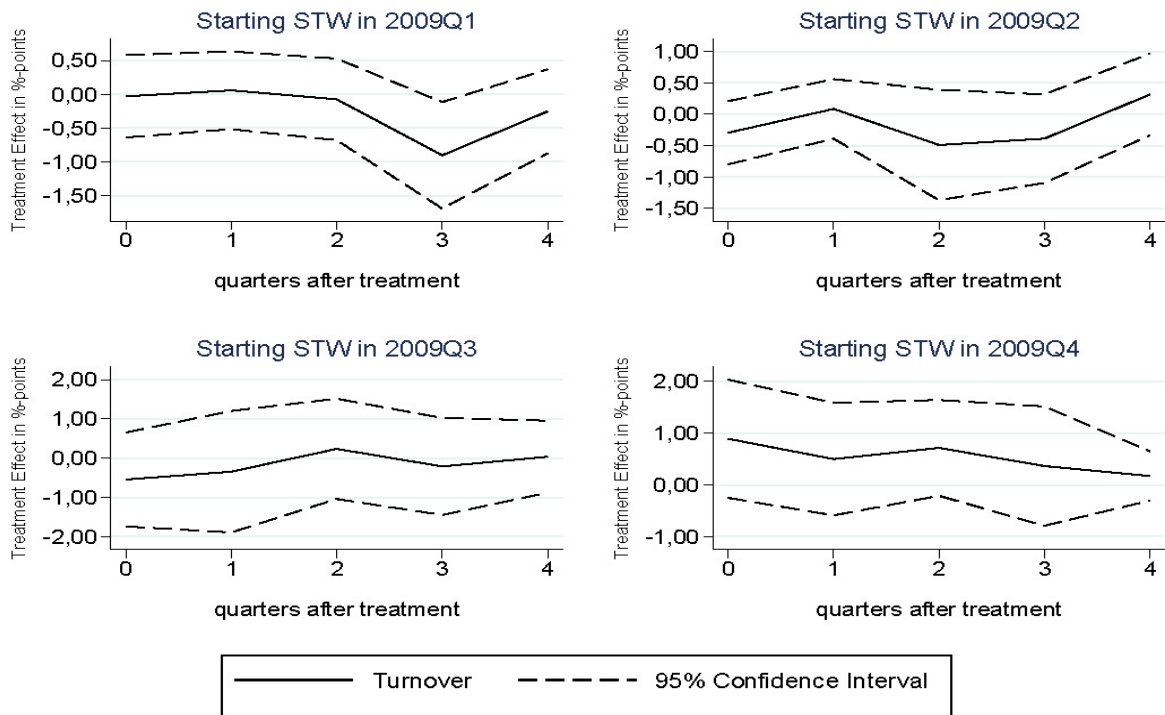
Source: Own calculations

Combining the arguments for the second and third outcome, we generate a fourth outcome variable: the monthly turnover rate differentiated by skill level. Just like the results before, we could not find any significant effect.

6 Robustness

As described in section 6 the sample used to implement the propensity score matching includes firms with different branches of economic activity. It could hence be argued that these firms operate on different markets and that our matching does not capture this fact. For this reason we conduct the matching exercise with a sample that is restricted only to manufacturing firms. Due to the small case numbers we are not able to conduct a dynamic matching as described in section 5.2. Instead, we carry out a standard static matching, where treated establishments are simply defined by the fact that they implemented STW at some point during the observational period. The matching algorithm as well as the explanatory variables are the same as in section 6. When running the matching only on manufacturing firms the bias between the treated and control group is reduced well: The mean of the absolute bias is reduced from 22.6 to 2.3; the pseudo R^2 obtained from repeating the logit regression on the matched sample amounts to 0.006, meaning that with the matched sample treatment participation cannot be explained by the covariates. As before, we calculate the treatment effect of

STW on the level of employment subject to social security relative to 2008 *Q1* as well as the turnover. For both outcome variables, we do not find any treatment effect of the STW scheme. This strongly supports the results obtained in section 6.

As a second robustness check, we conduct a matching with multiple treatments (Lechner, 2002). We do so for the following reason. This papers aims at analysing the causal effect of STW on employment at the establishment level. To this extent we ask how the fact that STW is implemented within a firm influences employment, i.e. we use a binary treatment indicator. However, it is not hard to imagine that employment is driven by the intensity of STW, not by its pure existence within a firm. In this case, the intensity of STW would be the treatment. To check the robustness of our results we thus allow treatment to take the values of no STW, low intensity of STW and high intensity of STW[14]. Due to small case numbers, we run a static matching using the same matching algorithm and control variables as in section 6. Essentially, the multiple treatment approach estimates the ATT of the scheme for three subsamples (no versus low intensity, no versus high intensity, low versus high intensity). For each subsample the matching worked well reducing the mean of the absolute bias to 2.03 at the highest. With respect to the ATT, we do not obtain significant results of the treatment effect of STW on the employment level relative to 2008 *Q1* or the turnover in any of the subsamples. Again, this supports the results obtained in section 6.

Our results are not in line with earlier firm-level studies. Boeri/Bruecker (2011), for instance, do find a causal effect of STW on the employment change between June 2008 and June 2009 using an IV approach. For this reason we conduct an IV regression a la Boeri/Bruecker (2011) using the same data from the IAB establishment panel and estimate two models. For the first regression the dependent variable is the log employment change between June 2008 and June 2009 as in Boeri/Bruecker (2011). For the second regression we use as a dependent variable the log employment change between June 2008 and June 2010. The first IV regression estimates a significant influence of the share of short-time workers on the log employment change, supporting the results of Boeri/Bruecker (2011). This result is, however, not maintained when estimating the second IV regression. This means that as soon as the employment change between 2008 and 2010 is used as a dependent variable, we do not find a significant influence of short-time work on employment using an IV approach a la Boeri/Bruecker (2011).

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[14] If an establishment implements STW the intensity is defined as high if at least two of the three following conditions are fulfilled. STW is operated for at least 12 months; the average share of short-time workers over the implementation period amounts to at least two thirds of the work-force or the average lack of work over the implementation period amounts to at least 38 percent. Otherwise an STW establishment is defined to have a small intensity of STW.

7 Conclusion

In this paper we analyse the employment preserving effect of short-time work (STW) at the establishment level. To this extent we aim at identifying the causal effect of STW on changes in firms' level of employment. Our analysis exploits a rich data sources that combines administrative data from the Federal Employment Agency and survey data from the Institute for Employment Research. Applying a dynamic propensity score matching approach, we estimate the treatment effect of STW as the average treatment effect on the treated (ATT), where the timing of treatment is taken into account. We compute several outcome variables: the level of employment relative to the first quarter of 2008 (both in total as well as differentiated by the qualificational level of employees) as well as the turnover (again in total and differentiated by the qualificational level of employees).

For all outcome variables the estimated treatment effect is insignificant. Our results hence do not indicate that changes in the level of employment differ between STW and non-STW establishments. In other words, non-STW establishments do not reduce jobs significantly faster than establishments with STW. This applies to the workforce in total as well as differentiated by qualificational level. This study indicates that STW is not used to keep skilled workers either. In sum, our analysis cannot confirm an employment preserving effect of STW. It seems that establishments without STW use other mechanisms than STW to hoard labour.

These results are in line with Burda/Hunt (2011) who argue that during the 2008-09 crisis working time accounts acted as a substitute for STW. However, our results contradict earlier establishment level studies by Boeri/Bruecker (2011) and Bellmann/Gerner (2011). These studies found that STW did contribute to the preservation of jobs during the last recession. While these studies use the employment change between June 2008 to June 2009 as the outcome variable, our analysis focuses on the employment change between June 2008 and June 2010. This may explain the differing results, since the crisis was still in full effect as of June 2009 and crucial employment changes may have taken place in the months afterwards. In the light of our results a relaxation of the eligibility criteria of STW as well as an anew prolongation of the maximum period of STW - for which a motion is submitted to the German ment - should be discussed thoroughly.

As our results are obtained on the firm level, we are not able to evaluate the effect of STW on individual employment biographies. It may well be that (although we do not find an employment preserving effect of STW on the establishment level) STW does prevent individual unemployment.

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Appendix

Table A.1
Summary statistics

Variable	Mean	Std. Dev.	N
profit situation bad in 2007	0.067	0.25	13082
profit situation good in 2008	0.349	0.477	13082
profit situation bad in 2008	0.071	0.257	13082
no or low competition	0.263	0.44	13082
share of exports in 2008	6.703	17.68	10855
business volume down	0.3	0.458	13082
business volume up	0.145	0.352	13082
unclear	0.101	0.301	13082
deteriorated	0.262	0.44	10848
improved	0.265	0.442	10848
working time accounts (dummy)	0.422	0.494	13055
collective agreement (dummy)	0.471	0.499	13046
works council (dummy)	0.296	0.457	13043
difficulties finding skilled workers (dummy)	0.304	0.46	13044
share of agency workers	0.016	0.087	12962
share of marginally employed	0.101	0.171	13082
share of part-time workers	0.239	0.264	12884
share with fixed-term contract	0.06	0.144	13014
share of low qualified	0.184	0.276	12632
share of high qualified	0.114	0.216	12632
regional dummy	0.414	0.493	13082

Source: own calculations

Table A.2
Results of the matching procedure: Establishments starting STW in 2009Q1

	Logit Model	Mean Before Matching				Mean After Matching		
		Treated	Control	p-Value		Treated	Control	p-Value
profit situation bad in 2007	-.083	0.075	0.075	0.970		0.075	0.083	0.575
profit situation good in 2008	-.386***	0.383	0.427	0.019		0.384	0.372	0.627
profit situation bad in 2008	.661***	0.168	0.073	0.000		0.165	0.165	0.968
profit situation sufficient in 2008	reference category							
no or low competition	-.381**	0.085	0.188	0.000		0.085	0.097	0.413
share of exports in 2008	.004*	24.051	5.914	0.000		23.855	24.812	0.513
expectations about 2009								
-- business volume down	1.010***	0.730	0.311	0.000		0.728	0.719	0.674
-- business volume up	-.451**	0.042	0.152	0.000		0.043	0.043	0.979
-- unclear	.277	0.070	0.096	0.019		0.070	0.070	1.000
-- business volume stable	reference category							
change in profit situation from 2007 to 2008								
-- deteriorated	0.284**	0.402	0.251	0.000		0.399	0.385	0.592
-- improved	0.076	0.191	0.268	0.000		0.192	0.199	0.722
working time accounts (dummy)	.094	0.697	0.391	0.000		0.697	0.702	0.831
collective agreement (dummy)	-.195*	0.495	0.422	0.000		0.492	0.492	0.983
works council (dummy)	-.138	0.514	0.217	0.000		0.512	0.505	0.785
difficulties finding skilled workers (dummy)	.205**	0.541	0.303	0.000		0.539	0.535	0.868
share of agency workers	-.733	0.045	0.017	0.000		0.045	0.045	0.973
share of marginally employed	.166	0.034	0.117	0.000		0.034	0.035	0.781
share of part-time workers	-3.113***	0.064	0.234	0.000		0.064	0.065	0.911
share with fixed-term contract	-1.039*	0.049	0.044	0.237		0.049	0.047	0.736
share of low qualified	.667***	0.220	0.188	0.004		0.219	0.225	0.643
share of high qualified	-1.191***	0.093	0.080	0.059		0.093	0.094	0.877
regional dummy	.203*	0.437	0.409	0.130		0.439	0.442	0.899
industry dummies	yes							
firm size dummies	yes							
observations	9,481	732	8,749			729	1,607	
LR Ch^2	1804.80							
Pseudo R^2	0.350							

Note: * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent

Source: own calculations

Table A.3
Results of the matching procedure: Establishments starting STW in 2009Q2

		Mean Before Matching				Mean After Matching		
	Logit Model	*Treated*	*Control*	*p-Value*		*Treated*	*Control*	*p-Value*
profit situation bad in 2007	.107	0.074	0.077	0.821		0.074	0.075	0.959
profit situation good in 2008	-.041	0.480	0.412	0.005		0.481	0.475	0.859
profit situation bad in 2008	.418**	0.102	0.075	0.043		0.100	0.110	0.625
profit situation sufficient in 2008	*reference category*							
no or low competition	-.200	0.109	0.175	0.000		0.109	0.096	0.530
share of exports in 2008	.003	17.223	5.863	0.000		17.093	16.92	0.919
expectations about 2009								
-- business volume down	1.306***	0.703	0.303	0.000		0.702	0.704	0.952
-- business volume up	-.606**	0.037	0.158	0.000		0.037	0.037	0.971
-- unclear	.652***	0.081	0.099	0.224		0.081	0.078	0.860
-- business volume stable	*reference category*							
change in profit situation from 2007 to 2008								
-- deteriorated	.214	0.295	0.254	0.063		0.293	0.310	0.593
-- improved	.312**	0.290	0.266	0.278		0.291	0.271	0.524
working time accounts (dummy)	.048	0.629	0.373	0.000		0.628	0.639	0.745
collective agreement (dummy)	-.111	0.476	0.416	0.015		0.477	0.480	0.924
works council (dummy)	-.128	0.371	0.198	0.000		0.370	0.374	0.888
difficulties finding skilled workers (dummy)	.026	0.466	0.289	0.000		0.467	0.469	0.967
share of agency workers	-.933	0.035	0.017	0.000		0.035	0.038	0.668
share of marginally employed	-.159	0.050	0.119	0.000		0.051	0.051	0.911
share of part-time workers	-.942*	0.094	0.227	0.000		0.094	0.094	0.950
share with fixed-term contract	-.846	0.043	0.042	0.873		0.043	0.044	0.806
share of low qualified	.353	0.178	0.191	0.360		0.178	0.172	0.738
share of high qualified	.186	0.104	0.075	0.001		0.104	0.104	0.988
regional dummy	.005	0.439	0.412	0.274		0.440	0.425	0.670
industry dummies	yes							
firm size dummies	yes							
observations	7,876	431	7,445			430	1,349	
LR Ch^2	782.30							
Pseudo R^2	0.234							

Note: * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent

Source: own calculations

Table A.4
Results of the matching procedure: Establishments starting STW in 2009Q3

	Logit Model	Mean Before Matching				Mean After Matching		
		Treated	Control	p-Value		Treated	Control	p-Value
profit situation bad in 2007	.124	0.067	0.075	0.755		0.067	0.057	0.749
profit situation good in 2008	-.094	0.454	0.417	0.418		0.454	0.420	0.603
profit situation bad in 2008	.166	0.092	0.074	0.444		0.092	0.091	0.964
profit situation sufficient in 2008	reference category							
no or low competition	-.024	0.134	0.174	0.256		0.134	0.138	0.940
share of exports in 2008	.002	18.008	6.359	0.000		18.008	17.376	0.856
expectations about 2009								
-- business volume down	1.252***	0.664	0.307	0.000		0.664	0.679	0.805
-- business volume up	-.290	0.059	0.156	0.004		0.059	0.047	0.687
-- unclear	.451	0.076	0.097	0.438		0.076	0.084	0.812
-- business volume stable	reference category							
change in profit situation from 2007 to 2008								
-- deteriorated	0.274	0.328	0.253	0.064		0.328	0.343	0.806
-- improved	.006	0.227	0.269	0.304		0.227	0.198	0.592
working time accounts (dummy)	-.002	0.605	0.385	0.000		0.605	0.590	0.813
collective agreement (dummy)	-.317	0.429	0.412	0.717		0.429	0.444	0.815
works council (dummy)	.083	0.370	0.191	0.000		0.370	0.351	0.768
difficulties finding skilled workers (dummy)	.092	0.479	0.293	0.000		0.479	0.425	0.407
share of agency workers	.135	0.043	0.019	0.009		0.043	0.040	0.834
share of marginally employed	.542	0.056	0.103	0.002		0.056	0.057	0.929
share of part-time workers	-1.437	0.100	0.208	0.000		0.100	0.105	0.804
share with fixed-term contract	-2.528	0.032	0.040	0.427		0.032	0.034	0.771
share of low qualified	.263	0.166	0.173	0.781		0.166	0.186	0.528
share of high qualified	1.028*	0.137	0.079	0.001		0.137	0.121	0.525
regional dummy	-.295	0.378	0.425	0.301		0.378	0.376	0.979
industry dummies	yes							
firm size dummies	yes							
observations	6,482	119	6,363			119	496	
LR Ch^2	199.92							
Pseudo R^2	0.168							

Note: * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent
Source: own calculations

Table A.5
Results of the matching procedure: Establishments starting STW in 2009Q4

	Logit Model	**Mean Before Matching**				**Mean After Matching**		
		Treated	*Control*	*p-Value*		*Treated*	*Control*	*p-Value*
profit situation bad in 2007	.407	0.080	0.075	0.841		0.080	0.104	0.522
profit situation good in 2008	.256	0.549	0.414	0.004		0.549	0.529	0.770
profit situation bad in 2008	.238	0.089	0.074	0.551		0.089	0.110	0.595
profit situation sufficient in 2008	*reference category*							
no or low competition	.026	0.133	0.175	0.239		0.133	0.117	0.719
share of exports in 2008	.007	17.681	6.114	0.000		17.681	18.023	0.923
expectations about 2009								
-- business volume down	.347	0.496	0.303	0.000		0.496	0.496	1.000
-- business volume up	-.699*	0.071	0.158	0.012		0.071	0.078	0.840
-- unclear	.316	0.097	0.097	0.983		0.097	0.103	0.895
-- business volume stable	*reference category*							
change in profit situation from 2007 to 2008								
-- deteriorated	.037	0.248	0.253	0.893		0.248	0.230	0.756
-- improved	.001	0.265	0.269	0.929		0.265	0.278	0.853
working time accounts (dummy)	.704***	0.708	0.378	0.000		0.708	0.733	0.680
collective agreement (dummy)	.222	0.522	0.410	0.016		0.522	0.520	0.979
works council (dummy)	-.451	0.363	0.187	0.000		0.363	0.398	0.586
difficulties finding skilled workers (dummy)	-.034	0.451	0.289	0.000		0.451	0.455	0.958
share of agency workers	.040	0.045	0.018	0.005		0.045	0.037	0.477
share of marginally employed	1.804	0.050	0.104	0.001		0.050	0.054	0.809
share of part-time workers	-3.199***	0.081	0.211	0.000		0.081	0.081	0.987
share with fixed-term contract	-1.802	0.038	0.040	0.856		0.038	0.039	0.920
share of low qualified	.074	0.153	0.174	0.426		0.153	0.153	0.983
share of high qualified	-.166	0.108	0.078	0.089		0.108	0.107	0.956
regional dummy	.289	0.487	0.425	0.187		0.487	0.489	0.979
industry dummies	yes							
firm size dummies	yes							
observations	6,357	113	6,244			113	481	
LR Ch^2	160.45							
Pseudo R^2	0.1414							

Note: * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent

Source: own calculations

Table A.6
Treatment effect of short-time work

Starting STW in	Quarters after treatment	Mean of Treated	Difference to mean of controls	Standard Error	t-Value
2009Q1	0	-31.850	0.3127	14.543	0.21
	1	-58.046	-1,771	1,7109	-1,04
	2	-7,8108	-3,0802	1,8515	-1,66
	3	-9,0314	-2,9711	1,9385	-1,53
	4	-11,14	-1,6032	2,0021	-0,8
2009Q2	0	-2,2729	0,3129	1,9535	0,16
	1	-2,9929	-0,4551	2,0892	-0,22
	2	-4,5237	-1,3368	2,114	-0,63
	3	-7,8356	-1,3457	2,4086	-0,56
	4	-7,711	-2,3414	2,6193	-0,89
2009Q3	0	-4,3602	-10,8353	6,8383	-1,58
	1	-7,0326	-9,7802	5,6958	-1,72
	2	-9,3823	-8,9195	5,7476	-1,55
	3	-11,8346	-12,1248	6,8357	-1,77
	4	-12,2417	-14,7603	7,3946	-2
2009Q4	0	2,5387	3,0289	3,9214	0,77
	1	-0,3284	1,8282	3,8258	0,48
	2	-0,9436	0,6121	4,0651	0,15
	3	-0,1963	0,5844	4,1403	0,14
	4	0,2435	0,0169	4,3624	0

Source: own calulations

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