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THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

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The Impact of Short-Time Work during the Great Recession[*]

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Abstract

We evaluate the effectiveness of Belgium's short-time work (STW) program during the Great Recession, a period when the country recorded the highest STW take-up rate in Europe. STW allows firms to reduce working hours in response to temporary shocks while avoiding layoffs, playing a key role in European labor market insurance systems. Using an instrumental variable strategy that exploits quasi-exogenous variation stemming from an institutional feature of the Belgian program, we estimate the causal effects of STW on employment and wages. We find that, while STW significantly reduces the volume of work per worker, it does not lead to statistically significant employment gains for the average treated firm. Importantly, positive employment effects are concentrated among small manufacturing firms, which are more likely to face binding liquidity constraints. These findings highlight the importance of targeting and screening in improving the cost-effectiveness of STW programs and minimizing deadweight losses.

Keywords: Short-time work; employment; wages; unemployment insurance.

JEL Codes: E24, J22, J23, J63, J65

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1 Introduction

Short-time work (STW), a program that subsidizes temporary reductions in working hours during economic shocks, has been shown to complement unemployment insurance by stabilizing aggregate employment (Cahuc, 2024). For firms, STW mitigates job separation costs—such as the loss of firm-specific human capital and rehiring costs—while reducing bankruptcy risks in imperfect financial markets. For workers, it provides an insurance against income losses, unemployment, and its long-term scarring effects. This paper examines the effects of STW program on employment and wages during the Great Recession in Belgium, the country with the highest STW take-up rate across Europe, with approximately 5.6% of salaried employees enrolled in 2009 (OECD, 2010).[1] As one of Europe's oldest and most extensively used STW programs, it has been advocated to play a dual role: mitigating aggregate economic shocks and supporting firms in sectors facing idiosyncratic downturns.

The COVID-19 crisis prompted an unprecedented expansion of STW programs across Europe. However, their broad and untargeted implementation—combined with reduced employer costs and widespread use across many sectors—poses significant challenges for causal impact evaluation. The crisis involved a broader and largely exogenous economic shock that affected multiple sectors, largely independent of firm-specific conditions. In addition to representing a massive liquidity shock for firms, it was accompanied by mandated reductions in working hours due to lockdown measures. In contrast, the Great Recession, as a financially driven crisis without a lockdown component, allows for greater variation in STW take-up across sectors and firms. Furthermore, because the program during that period was more narrowly targeted at blue-collar workers than white-collar workers, this institutional feature offers a useful setting to investigate the program's causal effects without relying on variation induced by policy changes. Insights from the Great Recession can thus provide valuable lessons for designing more targeted and effective programs during future economic shocks.

To evaluate the program's causal impact on firms' employment and wage outcomes, we employ rich micro-aggregated administrative data on employment and program take-up, combined with Value Added Tax (VAT) records from the National Bank of Belgium (NBB), covering the period from 2004 to 2012. Our identification strategy employs an instrumental variable (IV) approach with a long-difference estimator to isolate the causal effects of STW take-up. Specifically, we exploit an institutional feature of Belgium's program: firms employing blue-collar workers faced fewer application barriers and lower informational costs than those employing white-collar workers during the crisis. Using the pre-crisis share of blue-collar workers, interacted with a (leave-one-out)[2] sector-level turnover growth rate as an instrument, we estimate both short- and medium-run effects. This approach is similar to that of Cahuc et al. (2021), who instead use the local STW approval rate interacted with a measure of the shock.

We find that STW led to a substantial reduction in the volume of work per worker—by approximately 23%—without producing statistically significant average effects on employment or wages. These average effects, however, mask important heterogeneity. In particular, we find positive employment effects for manufacturing firms—a sector that experienced a large demand shock driven by disruptions in global supply

[1] Other countries with pre-crisis, well-established STW programs, such as Germany (3.2%), Italy (3.3%), and France (1.0%), had lower take-up rates.

[2] We leave out the unit of observation to ensure exogeneity.

chains, which likely resulted in tighter financial constraints, especially for small firms.[3] In contrast, no employment gains were observed in non-manufacturing sectors, suggesting that deadweight losses were more pronounced in firms less exposed to the crisis.

Interestingly, dynamic employment effects did not persist beyond the initial STW take-up for manufacturing firms, indicating that STW primarily supported low-productivity firms without recovery potential during the initial shock. Although STW helped stabilize employment and preserve firms' human capital during the crisis, these benefits diminished as the crisis persisted, consistent with Giupponi and Landais (2023).

Previous studies on STW programs during the Great Recession in Europe offer important lessons. Recent robust micro-econometric evaluations (Cahuc et al., 2024; Giupponi and Landais, 2023; Kopp and Siegenthaler, 2021) highlight STW's heterogeneous employment effects. These studies generally conclude that STW has large, positive short-run effects on employment at the extensive margin, particularly for firms facing significant temporary economic shocks. However, firms experiencing milder shocks often exhibit no significant benefits. Additionally, the persistence of employment effects depends on the firm's characteristics and the duration of the economic shock. While positive short-run employment effects are frequently observed, the benefits may not sustain in the medium-to-long run if the economic shock persists (Giupponi and Landais, 2023). Targeting firms that experience substantial temporary shocks, as Kopp and Siegenthaler (2021) shows, leads to positive employment effects that extend beyond the short run, particularly for smaller firms during the financial crisis.[4]

This paper contributes to the literature on STW programs in three main ways. First, it provides a novel micro-econometric evaluation of the impact of STW on employment and net firm retention in Belgium during the Great Recession, leveraging grouped data that are often more accessible than firm-level sources. By using aggregated data at the location-sector level, we capture effects net of reallocation or substitution across firms and workers within the same local labor market. In particular, our approach accounts for firm-level adjustments and labor market dynamics within each cell, allowing the estimates to reflect general equilibrium effects, rather than the partial equilibrium effects typically identified in prior micro-level evaluations (Cahuc et al., 2024; Giupponi and Landais, 2023; Kopp and Siegenthaler, 2021). As a result, our estimates capture net employment effects that incorporate both job creation and destruction, as well as firm entry and exit, thus accounting for potential negative reallocation responses induced by STW schemes.

Second, the analysis uncovers the dynamic effects of STW, highlighting how its impact evolves over time. Third, we document the program's effectiveness in stabilizing smaller firms, where liquidity constraints are more likely to bind. This finding underscores the importance of financial frictions in shaping the effectiveness of STW through labor hoarding decisions (Giroud and Mueller, 2019; Melcangi, 2024). Taken together, our results have important implications for the design and targeting of STW policies, suggesting the need for tailored support to financially vulnerable firms exposed to strong economic shocks.

[3]This relates to a strand of literature documenting that, during a financial recession, industries with greater external financial dependence are hit harder, and that credit constraints have a larger impact on employment—particularly for small firms (Duygan-Bump et al., 2015; Gertler and Gilchrist, 1994). The manufacturing sector was such an industry in European countries such as Belgium (Balta and Nikolov, 2013).

[4]For a detailed survey of the literature and rationales behind STW programs in Europe, see Bermúdez-Barrezueta et al. (2025).

The structure of this paper is as follows: Section 2 outlines the institutional framework of Belgium's STW program during the Great Recession. Section 3 describes the datasets used in the analysis. Section 4 describes the identification strategy. Section 5 discusses the main findings and their implications, and Section 6 concludes.

2 The STW Program During the Great Recession in Belgium

STW schemes[5] have existed in Belgium for several decades, enabling firms to temporarily reduce working time without terminating employment contracts. STW was formally introduced in 1954,[6] in the aftermath of World War II, to mitigate its economic consequences. The policy provided financial support to firms facing temporary declines in production or labor demand, aiming to avoid mass layoffs. Initially, the scheme applied to reductions in working time for four reasons: economic distress, force majeure events, exceptionally adverse weather, and technical accidents (Grais, 1983). In the 1970s, two additional reasons were added: strikes or lockouts and collective closures during annual holidays.

This paper focuses on the STW scheme for economic reasons, which is the most responsive to macroeconomic fluctuations. The program is used not only during aggregate downturns but also to cushion idiosyncratic firm- or sector-specific shocks in normal times—a feature that distinguishes Belgium from other European countries where take-up is minimal outside of crises. Between 2004 and 2008, prior to the Great Recession, 3.5% of salaried employees used STW at least once per year, rising to 5.6% at the peak of the crisis in 2009. The program has been adapted over time to improve its responsiveness to economic conditions.

This section reviews the key features of the Belgian STW scheme for economic reasons during the Great Recession. These include: (i) eligibility conditions for firms and workers; (ii) program generosity—namely, the replacement rate, duration limits, wage ceilings, and employer contributions; and (iii) application procedures and administrative requirements.

Eligibility and Conditionality Requirements

Firms are eligible to use STW if they face a temporary drop in demand due to economic reasons, such as a recession or firm-specific shocks. Reductions in activity stemming from internal factors—such as renovations, equipment maintenance, or poor organization—are not valid grounds for eligibility. If the use of STW is prolonged, local unemployment offices (UOs) may conclude that the economic decline is structural and restrict further access to the program. To apply, employers must notify the local UO, formally declare the reason for the reduction in activity, and confirm compliance with the eligibility criteria.

The Belgian labor market legislation distinguishes between blue- and white-collar workers, depending on whether the work involves primarily manual or intellectual tasks. This classification is enforced by payroll agencies and monitored by the Federal Public Service for Employment and social security authorities, leaving

[5] *Chômage Temporaire* or *Tijdelijke Werkloosheid* in French and Dutch, respectively.

[6] Since its creation in 1935, the National Office for Placement and Unemployment—the precursor to the National Employment Office (NEO)—offered an informal form of STW for blue-collar workers, even though no legal framework existed at the time. It was not until 1954 that the possibility of temporarily interrupting employment contracts was formally introduced into the March 10, 1900 law on employment contracts. These provisions were later incorporated into the Employment Contracts Act of July 3, 1978 (ONEM, 2010, 2020).

little room for manipulation by employers.[7]

Although this distinction has been largely harmonized since 2014, it remains a relevant criterion for STW eligibility. In particular, the conditions for eligibility are much stricter for white-collar than for blue-collar workers.

Originally, STW for economic reasons was available only to blue-collar workers. To prove a drop in demand, firms were required to declare the reason for using the program in their STW application. Controls were implemented randomly, and the declared information was in some cases cross-validated with data from institutions other than the NSSO. In July 2009, eligibility was expanded to white-collar workers, subject to stricter requirements. To access the program, firms needed to sign a collective bargaining agreement or adopt a corporate restructuring plan and demonstrate economic distress by meeting one of the following criteria: (i) a revenue decline of at least 20%; (ii) a 20% drop in production capacity; (iii) a 20% reduction in incoming orders; or (iv) at least 20% of declared workdays under STW for blue-collar workers. Despite the extension, take-up among white-collar workers remained low, accounting for only 0.8% of STW-subsidized days in 2009 (ONEM, 2009). Initially introduced as a temporary measure, STW coverage for white-collar workers became permanent in January 2012.

Overall, eligibility and conditionality requirements for STW are quite lenient for blue-collar workers compared to white-collar workers. The program relies on declarations that do not require formal proof and are only randomly verified at low rates. On average, approval rates of STW for blue-collar workers are high and consistent across regions, ranging from 96% to 99% during the period analyzed. Moreover, although random controls were implemented by the administration—particularly reinforced after 2011—these verifications are costly and in practice cover only a small proportion of STW applications. For instance, in 2011, 8,339 applications (6% of the total) were inspected, of which 15% presented infractions or anomalies (ONEM, 2011)

Generosity

We assess the generosity of the Belgian STW scheme during the Great Recession by examining four key parameters: (i) the number of compensated days per worker, (ii) the maximum duration, (iii) the net replacement rate, and (iv) employer cost-sharing, based on the framework from Cahuc and Carcillo (2011) and Hijzen and Venn (2011).

The scheme allows both full (i.e., zero hours worked) and partial reductions in activity (e.g., reduced working days per week). For blue-collar workers, full suspension is allowed for a maximum of 28 calendar days per month and can be renewed after the firm resumes regular working hours for at least one full week. Partial reductions can continue for up to 12 months (see Table A1 for details regarding differences between blue- and white-collar workers).

The net replacement rate—the ratio of income under STW to net income from regular work—was 60% for cohabitants without children and 65% for single individuals or household heads before the end of 2008. Benefits were capped at €1,906 per month, reducing the replacement rate for high earners. In January 2009,

[7] Ambiguous cases are occasionally settled in court.

the replacement rate increased to 70% (cohabitants) and 75% (others), and the monthly cap rose to €2,206.[8] The same replacement rate and cap applied to both blue- and white-collar workers, but the final income replaced could vary slightly due to employer-paid complements for white-collar workers, which in practice remained relatively low.

In contrast to some countries where employers contribute to STW costs, Belgian employers typically incur no direct cost (Hijzen and Venn, 2011). Although a modest employer contribution (€2 per STW day) was introduced in 2012, it remained limited. An experience rating system is also in place: firms in the construction sector faced additional contributions after 110 STW days per worker per year starting in 2005, with an extension to other sectors from 2012 (Tarullo, 2025). Despite these costs, the program remained relatively generous and inexpensive for employers.

Administrative Procedure for Benefit Claims

Access to STW benefits is managed by the NEO through its 30 local UOs. Firms must complete two steps to access benefits. First, they must notify affected employees at least seven days in advance. Second, they must submit an application to the relevant UO, providing details such as firm characteristics, the nature of the STW request, the type of use (total or partial suspension), a list of affected employees, and the coverage period. Local UOs verify whether eligibility criteria are met and notify firms of approval.

3 Data

We combine two different administrative data sources. The first consists of micro-aggregated administrative yearly[9] data from the National Social Security Office (NSSO) in Belgium. It contains employment information—headcount employment and volume of work in FTE—as well as STW statistics, including the number of worker and firm beneficiaries in the private sector. The data is grouped at the level of local UO location (l) and 3-digit economic sector (s)[10] to which a firm belongs during the period 2004–2012. Furthermore, we have data grouped at a finer level that includes firm size and employee status (e.g., blue-collar or white-collar), which allows us to retrieve information on the eligible population of workers within each cell, defined by the intersection of UO location and economic sector.

The second data source contains micro-aggregated data from VAT returns shared by the NBB. This dataset is also grouped at the UO location, economic sector and yearly level,[11] allowing us to merge it with the employment dataset. From these data, we use sales information to proxy demand shocks. The merge suggests that, on average, 93% (with a median of 100%) of firms within a cell file a VAT declaration.

[8] A withholding tax of 10.1% was deducted from benefits in 2009. This rate varied with economic conditions, reaching 18.75% in 2011.

[9] The original data were collected quarterly, but we aggregated them to the annual level by taking averages over four quarters to account for seasonal effects. We created a year definition that differs from the calendar year, as the decline in FTE began in the second half of the 2008 calendar year. Each year is thus defined as comprising four quarters: two from the second half of the previous calendar year and the first two quarters of the current calendar year. For example, we define 2008 as our reference year, encompassing the quarters 2007q3 to 2008q2.

[10] There are 30 regions corresponding to each of the UO locations to which firms report employment statistics and submit STW applications. For a map of these locations, refer to the following link. There are 221 sectors defined at the 3-digit NACE level (using the NACE Rev. 2.0 classification).

[11] Years are also defined as described in Footnote 9.

To address the structural break in the definition of NACE sectors[12] introduced in 2008, we harmonize sector codes using conversion tables provided by the NSSO. This procedure is described in Appendix A.2. We then merge the two datasets at the appropriate level of aggregation and apply a set of selection rules to evaluate the STW program for blue-collar workers in Belgium, as described below.

Sample construction

We select cells that include firms with 5 to 50 employees, which qualifies them as small firms according to the European Commission definition (EU recommendation 2003/361). This decision is motivated by two main considerations. First, the aggregated structure of the data complicates the tracking of multi-establishment firms. While financial data is recorded at the headquarters level, employment and STW applications are reported by establishment, making the linkage difficult. By focusing on smaller firms, which are mostly single-establishment (more than 90% in our sample), we mitigate this concern. Second, this sample selection aligns with previous studies. For instance, Giupponi and Landais (2023) study firms with between 5 and 25 FTEs during 2005–2015, and Cahuc et al. (2021) focus on single-establishment firms with more than four employees.

We exclude sectors that make only marginal use of STW. Temporary agency workers, while eligible, are highly sensitive to employment shocks and rarely benefit from STW due to low firing costs. During the crisis, these contracts were often the first to be terminated (Conseil supérieur de l'emploi, 2009). We also exclude sectors such as information and communication (J), financial and insurance activities (K), and real estate activities (L) (see Table A4).

To ensure consistency, we retain only cells that are observed in all six quarters of the pre-crisis period (2007Q1–2008Q2). Cells that disappear in later periods are retained with zero values. After constructing this balanced panel, we are left with 2,128 cells per year.[13] We also exclude cells composed entirely of white-collar workers during the pre-crisis period (2007Q1–2008Q2), since our analysis focuses on firms employing blue-collar workers, who were the primary beneficiaries of STW for economic reasons.

Our sample includes 30% of firms and employees in the private sector, and 50% of firms that used STW in the Belgian economy in 2009. Figure A1 in the Appendix shows the distribution of STW take-up—measured as both the fraction of firms and the average volume of work (in FTE) in STW—across 1-digit NACE sectors in our sample. Take-up was concentrated in blue-collar-intensive sectors such as manufacturing,[14] construction, and transportation. In these sectors, an average of 42% of firms used the program at least once in 2009, translating into approximately 20% of their total headcount employment.

Descriptive statistics for treatment and outcomes at the cell level (region-sector) in 2009 are presented in Table A3. These statistics are reported across quantiles of the fraction of firms using STW within each cell. Several key patterns emerge. First, cells with higher STW take-up rates tend to have larger

[12] In our original dataset, we use the NACE Rev.1.1 definition for the period 2005–2007 and the NACE Rev.2.0 definition for 2008–2014.

[13] This approach allows us to evaluate outcomes without conditioning on firm survival within a cell. In the post-crisis period, an average of 267 out of 2,128 cells cease to exist each year.

[14] Our sample covers 45% of manufacturing firms, 24% of manufacturing employment, and 27% of total manufacturing STW take-up.

shares of blue-collar workers and experience more severe demand shocks. Second, the volume of work per blue-collar worker remains relatively stable across quantiles. Finally, firms in the sample employ, on average, approximately 10 workers—a figure that is constant across the cell-level distribution of the fraction of firms in STW.

Treatment and outcomes definitions

This paper evaluates the short- and medium-run effects of STW take-up at the onset of the Great Recession in 2008. To enhance comparability with existing studies and minimize seasonal variation, we aggregate quarterly variables into annual data. Since the recession began in 2008Q3, we define years to run from Q3 to Q2 of the following year. In this grouping, 2009 corresponds to 2008Q3–2009Q2, and 2008 serves as our reference period. Figure A3 in the Appendix illustrates the evolution of turnover and STW take-up in FTE terms, showing a structural break in both series in 2009—the first period for which we estimate treatment effects.

Our treatment variable captures the intensity of STW use within a cell. Since treatment is determined at the firm level, we define the treatment as the share of firms taking up STW (for blue-collar workers) in a cell ($\frac{Firms \quad in \quad STW_t}{Total \quad No. \quad Firms_t}$). This share is computed annually as an average over the four quarters and represents the proportion of treated firms in a given location-sector cell. This enables estimation of firm-level treatment effects. We also explore an intensive-margin measure of treatment defined as the average volume of work in STW (in FTE) per firm in a cell. Although both definitions lead to similar results, the extensive margin treatment allows a more accurate estimation of the treatment effects at the firm level.

We focus on three outcomes that allow us to decompose the effects on blue-collar employment, as our analysis is restricted to blue-collar workers. Table A2 defines each outcome. The first outcome, volume of work (in FTE) per worker, captures intensive-margin adjustments due to STW. The second outcome measures headcount employment per firm, reflecting extensive-margin responses. The third outcome—total volume of work per firm—captures the net effect, combining changes in the volume of work (in FTE) per worker and headcount employment per firm.

Firms respond to temporary productivity shocks by adjusting labour demand at the intensive margin via STW, preserving jobs that might otherwise be lost. In the short term, STW is expected to have a positive effect on employment by reducing the average volume of work per worker while maintaining headcount employment that, in a counterfactual scenario, would have been laid off (Cahuc et al., 2021). Hence, if STW immediately protects jobs that would otherwise have been destroyed, we would also expect an increase in the total volume of work per firm.

In the medium to long term, employment gains are expected to persist if volume of work per worker recovers and jobs are retained beyond the STW take-up period, as the scheme use is intended to be temporary. Kopp and Siegenthaler (2021) documents both the short- and medium-term effects described above for Switzerland during the Great Recession: treated firms experienced gains in total employment—both in terms of headcount and total hours worked—in the short and medium run.

However, STW may also generate deadweight losses by subsidising firms that would not have laid off workers even in the absence of the program. In the short term, such deadweight effects result in a reduction in volume of work per worker without significantly affecting either headcount employment or total hours worked. For instance, Cahuc et al. (2021) finds this to be the case for firms facing small demand shocks, where workers would have been retained regardless of STW take-up, leading to "excessive" reductions in hours (Burdett and Wright, 1989). Moreover, STW may delay the exit of non-viable jobs, potentially hindering long-term employment growth in both treated and untreated firms. This would translate into no significant effect on headcount or total hours worked *after* STW take-up. Such a pattern is observed in the case of Italy during the Great Recession, where STW produced short-term employment gains without persistent effects in the medium term (Giupponi and Landais, 2023).

In terms of wages, we investigate the effect of STW on wage rates (i.e., the gross wage bill divided by working time)[15] and the gross wage bill per worker. While STW take-up mechanically reduces the volume of work per worker, it also mechanically reduces the wage bill per worker. We expect that wage rates do not decrease if labour hoarding is subsidised through STW take-up. This is the case particularly in contexts where labour markets are rigid and wage negotiations are more centralised (Giupponi and Landais, 2023; Brinkmann et al., 2024). Conversely, a positive effect on wage rates may reflect a composition-driven effect at the cell level, where the reduction in working time is concentrated among low-wage workers, while hours worked by high-wage workers are maintained unchanged.[16]

In our context, the analysis of the above mentioned employment outcomes ignores the impact of STW on firm failure and creation, as these outcomes are averages per firm. If STW saves jobs, it may also enhance at the limit firm survival. However, even so, it may simultaneously slow down the reallocation process by discouraging the creation of new firms. The net effect is unclear, and if the latter dominates, this could even lead to a negative effect on the volume of work per firm, as new firms are typically smaller than incumbent ones.

Definition of a Sectoral Shock

As explained in Section 4, the instrument is constructed by interacting the share of blue-collar workers in 2004[17] with the national sectoral shock, combining a treatment exposure with a shift variable. We measure the shock ($g^{-(l)}_{(s)2009}$) as the change in sectoral turnover (i.e., sales), T, between 2008 and 2009, marking the onset of the Great-Recession. To ensure the exogeneity of this shift variable, local sectoral shocks are aggregated using a leave-one-out (LOO) procedure, whereby local sectoral shocks are aggregated across all locations l except the one to which the unit of analysis pertains ($-(ls)$, where s indicates the economic sector) (See, e.g., Duggan et al., 2022):

$$g^{-(l)}_{(s)2009} = ln(T)^{-(ls)}_{(s)2009} - ln(T)^{-(ls)}_{(s)2008} \tag{1}$$

[15] Wages are deflated and expressed in real terms relative to the baseline year 2004.

[16] In Belgium, high-wage workers—particularly those earning above the wage cap (See Section 2 for details)—face a decreasing replacement rate, which may act as a disincentive for these workers to comply with STW take-up, as they bear the largest burden of the earnings loss.

[17] We define the blue-collar worker share using 2004 data, as it is the first year available. However, sensitivity tests were also conducted using the blue-collar worker share from other pre-crisis years, as well as the average over the entire pre-crisis period (2004–2008).

Note that by taking the logarithm of turnover, the shock is expressed in proportional terms. Further intuition on why the sectoral shock serves as a valid component of the instrument is provided in Section 4 below.

Descriptive statistics

To gain better insight into the functioning of the STW scheme in Belgium during the period of analysis, we present some descriptive statistics on STW take-up rates. First, STW exhibits counter-cyclicality (Brey and Hertweck, 2020); that is, take-up rates increase during economic downturns and decline during recoveries. The intensity of STW use varies considerably across sectors, as shown in Table A4. This variation reflects the distribution of eligible workers across sectors: sectors with a higher share of blue-collar workers tend to have greater take-up rates than those dominated by white-collar workers.

Take-up rates began to rise in 2008 and peaked in 2009 for the sample of firms analyzed, as illustrated in Figure 1. Specifically, the share of firms in the sample participating in STW increased from 16% in 2007 to over 22% in 2009. Figure A1 highlights the sectors with the highest STW participation in 2009, both in terms of the number of firms and the number of subsidized jobs. The three primary users of STW for economic reasons that year were the manufacturing, construction, and transportation sectors. On average, more than 40% of firms in these sectors used the program, accounting for approximately 20% of all jobs covered by STW in the sample.

Finally, Figure A2 in the Appendix reveals a notable pattern. It shows the evolution of employment per firm—both headcount and FTE—for blue- and white-collar workers. While headcount employment among blue-collar workers declined, the drop in FTE per firm was even more pronounced, reflecting the STW take-up during the Great Recession. In contrast, both headcount employment and FTE among white-collar workers followed parallel trends over time.

4 Identification strategy

Firms facing economic difficulties are typically more intensive users of STW. Therefore, estimating the impact of STW on employment or other indicators of firm performance by directly comparing outcomes between users and non-users is generally downward biased. This bias persists even after controlling for firm fixed effects, because STW is designed to protect firms experiencing temporary rather than structural difficulties. In Section D of the Appendix, we report such a downward bias when estimating the effect of STW on employment using ordinary least squares (OLS), even after accounting for cell fixed effects.[18]

To address this confounding, we employ an IV strategy, resembling a difference-in-differences design. We instrument STW uptake using the interaction between the predetermined share of blue-collar workers in a location-sector cell in 2004 and the magnitude of the negative sectoral shocks in turnover between 2008 and 2009. This approach is similar to that of Cahuc et al. (2021), who instead use the local STW approval rate interacted with a measure of the shock. They argue that applying for STW entails a fixed cost per job, which

[18]Note that, because we use firm data aggregated at the group level, some selection bias is already absorbed by aggregation at the location-sector cell level (Angrist, 1991; Cockx and Ridder, 2001).

decreases with the approval rate. Consequently, STW take-up increases more sharply with the approval rate when shocks are large than when they are small. Their empirical evidence supports this theoretical prediction, showing that the interaction leads to a relevant IV even after separately controlling for the approval rate and the magnitude of the shock.[19]

In our context, the blue-collar worker share plays a role analogous to the approval rate in their framework. Fixed application costs decline with the blue-collar worker share because the administrative process for STW is significantly more lenient for blue-collar than for white-collar workers. The left-hand panel of Figure 2 shows that, in cells where the decline in sectoral turnover between 2008 and 2009 is above the median, the change in STW take-up between 2004 and 2009 increases with the blue-collar worker share. In contrast, in cells with below-median shocks (which also include positive shocks), the change in take-up decreases with the blue-collar worker share. The right-hand panel of Figure 2 confirms this result using residualized outcomes that control for location-sector-specific trends between 2004 and 2009 and other potential confounders.[20] Figure 2 provides suggestive evidence that the IV is relevant.

We also argue for the exogeneity of our instrument after accounting for sector- and region-specific shocks. First, we contend that the predetermined blue-collar worker share in a given location-sector cell in 2004, well before the onset of the Great Recession, reflects long-standing production technologies and historical firm-location choices that could not have caused the negative outcomes observed during the crisis.

Nonetheless, firms with a high share of blue-collar workers may be more sensitive to broader downward trends. To address this concern, we allow for cell-specific linear trends and purge the instrument of potential endogenous variation by controlling for time shocks correlated with the 2004 blue-collar share, time shocks correlated with the sectoral turnover shock in 2009, and location-specific time shocks. In other words, we exploit the variation arising from sectoral deviations relative to the average trend, after controlling for the aforementioned confounders.

Our findings confirm that concerns about the validity of the unadjusted IV are warranted: without these corrections, the instrument is significantly related to pre-recession STW uptake and key outcomes. However, once these adjustments are introduced, placebo tests show that the instrument is no longer correlated with pre-treatment outcomes, supporting its exogeneity. These placebo tests are implemented, following Giupponi and Landais (2023), by presenting event-study plots of both the first-stage and reduced-form estimates (Figure 3).

We begin by detailing how these placebo tests are implemented. We then formally describe our strategy for estimating the short-run impact of STW take-up on 2009 labor market outcomes using two-stage least squares (TSLS), followed by an approach for estimating medium-run effects from 2010 to 2012.

[19]Our setting does not allow for the application of this approach, since there is little variation in local UO approval rates.

[20]This is done by regressing STW take-up on the full set of covariates in Equation (2), excluding the IV. The residuals from 2009 are then split by above- or below-median shocks and regressed separately on the blue-collar worker share defined in 2004.

Placebo tests

The first-stage and reduced-form regressions of the IV estimator of STW uptake in 2009 on the outcomes of interest (employment and wages) are estimated using a random growth model (Wooldridge, 2010, pp. 375–377), specified as follows:

$$y_{(ls)t} = \gamma_0 + \gamma_{1t} z_{(ls)2009} + \gamma_{2t} b_{(ls)2004} + \gamma_{3t} g^{-(l)}_{(s)2009} + \gamma_{4t} x^{-(l)}_{(s)2007} + \phi_{1(ls)} + \phi_{2(ls)} t + \phi_{3(l)t} + u_{(ls)t} \tag{2}$$

Here, $y_{(ls)t}$ denotes the outcome variable—such as the log of blue-collar workers per firm, the log of volume of work per blue-collar worker, the log of blue-collar volume of work per firm, the log of the gross (blue-collar) wage rate or the log of the gross (blue-collar) wage bill per worker—in location l and sector s in year t (where $t \in 2005, \ldots, 2012$). $b_{(ls)2004}$ is the blue-collar worker share, fixed in 2004, the first year of available data. $g^{-(l)}_{(s)2009}$ is the leave-one-out (LOO)[21] growth rate of turnover from 2008 to 2009, representing the sectoral shock. $z_{(ls)2009} \equiv g^{-(l)}_{(s)2009} \cdot b_{(ls)2004}$ is the IV constructed as the interaction of the blue-collar worker share and the shock. $x^{-(l)}_{(s)2007}$ is the log of the (LOO) sectoral turnover in 2007. This control accounts for the possibility that STW take-up depends not only on turnover growth but also on initial turnover levels, thereby capturing a measure of sector size. By estimating a model where γ_4 is restricted to zero, we test the robustness of the findings to the exclusion of this control. $\phi_{1(ls)}$ is a cell-specific time-invariant fixed effect, and $\phi_{2(ls)}$ is a cell-specific time trend slope, capturing persistent location-sector trends that could be correlated with $y_{(ls)t}$. Controlling for these trends is crucial to account for variation across cells that are in persistent decline (or growth), and therefore have an increasing (or decreasing) tendency to take up STW. Such sector-location combinations may be selective, as they are likely correlated with structurally less (or more) favorable outcomes. $\phi_{3(l)t}$ is a location-time-specific fixed effect that absorbs location-specific time shocks. Lastly, $u_{(ls)t}$ is the idiosyncratic error term.

To estimate this model, we take a long first difference relative to 2004, the beginning of our observation window. For any variable $w_{.t}$, we define $\Delta w_{.t} \equiv w_{.t} - w_{.2004}$.[22] Moreover, any parameter $\gamma_{.t}$ or $\phi_{.t}$ is defined as $\gamma^*_{.t} \equiv \Delta\gamma_{.t} = \gamma_{.t} - \gamma_{.2004}$ and $\phi^*_{.t} \equiv \Delta\phi_{.t} = \phi_{.t} - \phi_{.2004}$, respectively:

$$\Delta y_{(ls)t} = \gamma^*_{1t} z_{(ls)2009} + \gamma^*_{2t} b_{(ls)2004} + \gamma^*_{3t} g^{-(l)}_{(s)2009} + \gamma^*_{4t} x^{-(l)}_{(s)2007} + \phi_{2(ls)}(t - 2004) + \phi^*_{3(l)t} + \Delta u_{(ls)t} \tag{3}$$

Note that differencing causes us to lose 2004 from the sample. Equation (3) identifies the long-differenced parameters relative to 2004. $\phi_{2(ls)}$ is a cell-specific trend coefficient[23] and $\phi^*_{3(l)t}$ accounts for location-specific time shocks. We estimate this equation using a modified fixed effects estimator that absorbs the product

[21] That is, we construct a turnover measure at the (national) sectoral level, excluding the region for which we observe employment and other outcomes.

[22] We use long first differences rather than (short) first differences ($\Delta^s w_{.t} \equiv w_{.t} - w_{.t-1}$) because we aim to estimate the medium-run effects of STW take-up in 2009. Since the IV is based solely on the 2009 shock and not on shocks in later periods, short differences would weaken the instrument from 2010 to 2012. Long differences, in contrast, capture the cumulative effect of the IV from 2004 onward, maintaining its relevance for medium-run estimation.

[23] In a short-difference model, this would be multiplied by $t - (t - 1) = 1$.

of the fixed effect and the time trend. To obtain a firm-level estimator, we weight the data by the average number of firms in the two cells involved in the difference: $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$ (Angrist and Pischke, 2009).

We allow the coefficients on all five explanatory variables, γ_{1t}^* to γ_{4t}^* and $\phi_{3(l)t}^*$, to vary flexibly over time. This approach allows to control for time shocks correlated with the blue-collar worker share in 2004, the sectoral turnover shock in 2009, location effects, and the log of the (LOO) sectoral turnover in 2007, respectively. The random growth model requires dropping one time period (2004) and setting a reference year per variable to zero. We choose 2008 as a reference period, such that all estimates are relative to the pre-crisis baseline.

This choice facilitates the construction of event study plots tracing γ_{1t}^* over time. These plots allow us to visually inspect whether the IV is indeed unrelated to all pre-recession outcomes from 2005 to 2007, serving as a placebo test. We also formally test whether the 2005–2007 coefficients jointly differ significantly from zero.

The post-2008 coefficients represent Intention-to-Treat (ITT) effects for firms induced to take up STW in 2009 by a high value of the IV. Specifically, we are evaluating the effect of STW on a set of firms that have a high blue collar share and because of the financial crisis shock are induced to take-up the program.

By substituting the dependent variable in Equation (3) by $\Delta STW_{(ls)t}$, the long difference in STW take-up, we can also trace the evolution of the reduced form IV's effect on STW use over time. Under a valid IV, pre-2008 coefficients should be close to zero, while post-2009 coefficients inform us about instrument strength.

Finally, we replace $z_{(ls)2009}$ in Equation (3) by $STW_{(ls)t}$ to show that the instrument is not correlated with STW take-up in the pre-treatment period once the conditioning variables are controlled for, while it is strongly related to the take-up from the onset of the Great Recession.

Short- and Medium-Run Effects of STW Take-Up in 2009

We consider the TSLS estimator. To determine the short-run effect of STW take-up in 2009 on any of the outcomes for that year, we estimate an equation similar to Equation (3), using data up to 2009. In this specification, $z_{(ls)2009}$ is replaced by $\Delta STW_{(ls)2009}$—i.e., the endogenous increase in STW take-up at the onset of the Great Recession between 2008 and 2009—and its coefficient is denoted by α_0 in 2009 and set to zero in all preceding periods:

$$\Delta y_{(ls)t} = \alpha_0 \Delta STW_{(ls)2009} I_{t,2009} + \alpha_{1t} b_{(ls),2004} + \alpha_{2t} g_{(s)2009}^{-(ls)} + \alpha_{3t} x_{(s)2007}^{-(ls)} + \varphi_{2(ls)}(t-2004) + \varphi_{3(l)t}^* + \Delta\epsilon_{(ls)t} \quad (4)$$

for $t \in 2005, \ldots, 2009$, where $I_{tj} = 1$ if $t = j$ and $I_{tj} = 0$ otherwise, with j denoting the first period of analysis, 2009. This equation is then estimated by TSLS, using $z_{(ls)2009}$ as an instrument for the endogenous change in the take-up rate.[24]

[24]The restriction of zero coefficients on the endogenous STW take-up in the pre-2009 period is justified by placebo tests, which show that the instrument is conditionally uncorrelated with both STW take-up and the outcomes in the pre-treatment period.

The variation our instrument exploits can be summarized as follows: among two cells with identical sectoral shocks but different blue-collar shares, the cell with a higher share of blue-collar workers is more likely to adopt STW. The variation captured is net of controls for the size of the sector (in terms of turnover in the past), sector-location trends, and time-varying confounders, such as the blue-collar worker share and the sectoral LOO growth rate of turnover.

In terms of inference, we follow recent developments in the weak instrument literature and implement the VtF adjustment to the F-statistic as proposed by Lee et al. (2023). The VtF procedure adjusts standard errors by incorporating information from both the first-stage and reduced-form models, yielding confidence intervals that are neither overly conservative nor anti-conservative. Specifically, we use the sample correlation between the TSLS and first-stage residuals to improve inference precision. In our main specification, which evaluates the impact across all firms satisfying the selection criteria, the F-statistic exceeds $10 + (100 \times \hat{r})$, where $\hat{r}$ denotes the sample correlation. According to Lee et al. (2023), this implies that conventional 95% confidence intervals based on ± 1.96 are valid. Although $\hat{r}$ may fall below 0.543—indicating the potential to construct narrower intervals—the large F-statistic supports the validity of standard inference.

Because we have only one IV, we cannot identify more than one treatment effect without additional assumptions. To identify dynamic lagged effects of STW in 2009 on outcomes in subsequent years, we follow the recursive specification of Giupponi and Landais (2023), which is based on the approach of Cellini et al. (2010) in a Regression Discontinuity Design context.

Identification relies on two key assumptions. First, the IV must not be correlated with STW use prior to 2009—an assumption that can be evaluated using the previously described placebo test. Second, the contemporaneous and lagged effects of STW take-up are assumed to remain constant over calendar time.

Under these assumptions, and noting that the TSLS-estimate of α_0 is a Wald estimator—i.e., the ratio of γ^{*RF}_{2009} to $\frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}}$ —the dynamic effects of STW in 2009 can be recovered recursively, beginning with the estimate $\hat{\alpha}_0$ for 2009:

$$\gamma^{*RF}_{2009} = \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \tag{5}$$

For $t > 2009$, we obtain the following recursive equations:

$$\gamma^{*RF}_{2010} = \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \tag{6}$$

$$\gamma^{*RF}_{2011} = \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2011}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} + \alpha_2 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}}$$

$$\gamma^{*RF}_{2012} = \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2012}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2011}}{\partial z_{(ls)2009}} + \alpha_2 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} + \alpha_3 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}}$$

Here, $\{\alpha_0, \alpha_1, \alpha_2, \alpha_3\}$ corresponds to the sequence of dynamic treatment effects of STW. For instance, from Equation (6), we solve for $\hat{\alpha}_1$ as: $\hat{\alpha}_1 = \frac{\hat{\gamma}^{*RF}_{2010} - \hat{\alpha}_0 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}}}{\frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}}}$. Subsequent coefficients $\hat{\alpha}_2$, $\hat{\alpha}_3$, and $\hat{\alpha}_4$ are estimated recursively similarly. Standard errors are calculated using the Delta Method. Results are presented in Section 5.[25]

5 Results

This section presents the main findings on the impact of STW on employment and wages.[26] We begin by reporting the first-stage and reduced-form estimates. We then discuss the short-run and dynamic effects of STW take-up. Finally, we explore heterogeneous responses across sectors—depending on their exposure to the financial crisis—and by firm size in Belgium.

5.1 First Stage and Reduced Form

Following the empirical strategy outlined in Section 4, we use the interaction between the blue-collar worker share and the LOO growth rate of turnover from 2008 to 2009 as an instrument for the fraction of firms in STW in each sector-location cell. Panels (a) and (b) of Figure 3 trace the evolution of the instrument's effect on STW use over 2005–2012. The figure plots the coefficients γ^*_{1t} from Equation (3), using two definitions of treatment: the fraction of firms in STW (extensive margin) and the average volume of work (in FTE) in STW per firm (intensive margin).

The figure confirms that the instrument is unrelated to differential pre-crisis behavior in STW take-up, ruling out selectivity. It also provides evidence of a strong first-stage relationship: the instrument explains an increase of approximately 37% in the fraction of firms in STW during 2009–2012. In addition, the instrument is associated with a 5.5 FTE increase in STW use per firm, starting from a pre-crisis baseline[27] near zero. While firms could take-up STW before the crisis, we focus on firms that responded to the financial crisis, conditional on sector-location trends and other controls (as detailed in Section 4).

We then estimate the first stage of Equation (4), defining treatment as the change between 2004 and 2009 and setting treatment to zero in all periods prior to 2009, which is supported by the placebo test. Doing so increases precision, relative to a regression that does not include the pre-treatment period (2005-2007).[28] Table 1 presents the TSLS first-stage estimates using both treatment definitions, confirming the graphical evidence.

The variation our instrument exploits can be summarized as follows: among two cells with identical sectoral shocks but different blue-collar shares, the cell with a higher share of blue-collar workers is more likely to adopt STW. The variation captured is net of controls for the size of the sector (in terms of turnover in the past), sector-location trends, and time-varying confounders, such as the blue-collar worker share and the sectoral LOO growth rate of turnover.

[25] These estimates are computed using the *nlcom* command in Stata.

[26] As noted in Section 3, our analysis defines both treatment and outcomes in terms of blue-collar workers. All results discussed in this section pertain exclusively to blue-collar workers.

[27] This is, relative to the cell-specific trend between 2004 and 2008, given that 2008 is the reference period.

[28] This increases precision for similar reasons as in a staggered difference-in-differences design, where it is more efficient to compare the treatment effect to the average of the pre-treatment periods (as, e.g., in Borusyak et al. (2024), or in Wooldridge (2021)) rather than to the last pre-treatment period (as, e.g., in Callaway and Sant'Anna (2021)). For further discussion, see (Roth et al., 2023, p. 2227).

The F-statistic averages 44 across the two specifications in Table 1. Column (1) corresponds to the full specification (Equation (4)); Column (2) omits the control for past level of sectoral turnover (i.e., $\alpha_{3t} = 0$). In the remainder of the analysis, we focus on these two specifications, emphasizing the extensive margin treatment for interpretative ease, though results using the intensive margin treatment are provided in Appendix.

Panel (c) of Figure 3 plots the evolution of the IV's effect on the (log) volume of work per blue-collar job from 2005 to 2012. Similar plots for other outcomes are available in the Appendix (Figures C6 and C7). These support our identification strategy by showing no pre-crisis differential behavior between firms with differing predetermined blue-collar shares subject to the same demand shock, conditional on confounders and sector-location trends.

Panel (c) also shows that the instrument is associated with a statistically significant 17% drop (at the 5% level) in volume of work per worker in 2009, confirming program take-up. However, the ITT effect on headcount employment is positive but not statistically significant. To corroborate these visual findings, the next section presents TSLS estimates of the impact of STW on employment and wage outcomes for the average treated firm.

5.2 Short- and Medium-Run Effects of STW Take-Up

We now turn to the main estimates from the TSLS specification outlined in Equation (4). Table 2 reports the effects of STW take-up on employment outcomes, while Table 3 presents results on wages.

We begin with short-run effects. As expected, STW take-up is associated with a reduction in the volume of work per worker, consistent with the design of the program. The estimated decline is approximately 23%[29], although the effect is not statistically significant. Similarly, we find positive but statistically insignificant effects on headcount employment and total volume of work per firm.

While these magnitudes are consistent with prior estimates of STW impacts during the Great Recession (summarized in Table D1), our results lack statistical precision. Turning to wages, we confirm the absence of differential trends before the crisis (Appendix Figure C7) and find no significant effects of STW take-up on either wage rates or wage bill per worker. The 18.5%[30] decline in wage bill per worker—although statistically insignificant—mirrors the reduction in volume of work per worker, reinforcing the notion that STW primarily subsidizes hours not worked rather than wages. This finding aligns with the ones of Giupponi and Landais (2023) and Biancardi et al. (2022), who emphasize the distinction between STW and wage subsidy programs.

In the Belgian context, where sector-level wage bargaining predominates,[31] the absence of wage effects is unsurprising, given that within-sector wages are relatively rigid. However, when we omit the control for past sectoral turnover (Column 2 in Table 3)—thus comparing firms exposed to similar shocks but operating in different economic sectors—the observed positive effect on wage rates suggests a compositional effect. Specifically, STW appears to have been used primarily for jobs in low-wage sectors by subsidizing unworked

[29] $(e^{-0.2612} - 1) \times 100\%$. Estimates are interpreted as semi-elasticities at the firm level, weighted by the number of firms in each cell. See Chapters 3.1.3 and 4.1.3 in Angrist and Pischke (2009).

[30] $(e^{-0.2045} - 1) \times 100\%$.

[31] In contrast, decentralized wage setting in countries like Germany allows for wage renegotiations as an alternative to layoffs; see Brinkmann et al. (2024); Mohimont et al. (2024).

hours, thereby raising the average wage rate in the cell defined by actual working hours. This pattern likely reflects the fact that higher-wage workers—who face larger earnings losses due to capped replacement rates—have weaker incentives to take up STW.[32]

Turning to dynamic effects, we would expect persistent employment gains only if STW helps maintain viable job matches that would otherwise be lost. However, if the program merely delays inevitable layoffs in low-productivity firms, its medium-term effects on headcount employment may be zero which, if protecting low productive matches in the short run, may hinder labour reallocation.

Figure 4 shows a recovery in volume of work per worker and in the wage bill per worker one year after take-up, supporting the interpretation of STW as a temporary insurance mechanism. Nonetheless, headcount employment and total volume of work (in FTE) remain statistically insignificant throughout the post-treatment period. Wage rates increase by about 11% in the year of take-up (Figure 5), but no further wage dynamics are observed thereafter.

Taken together, the results point to an insignificant average effect of STW on employment and wages for firms treated at the margin. While STW reduced working time per worker as intended, this did not translate into statistically significant job savings for the average firm. This finding is consistent with theoretical predictions from Cahuc et al. (2021) and Cahuc (2024), which highlight the heterogeneous nature of STW effectiveness and emphasize that program success depends on the severity of the shock experienced by the firm.

The next section explores this heterogeneity in greater depth by investigating whether employment gains were concentrated in sectors most severely affected by the Great Recession—particularly manufacturing, which was highly exposed to the global contraction in demand for durable goods and to tightening financial conditions. This analysis sheds light on which firms benefited most from STW and the extent to which program design should account for sector-specific vulnerabilities.

5.3 Heterogeneous STW Treatment Effects by Economic Sector

Although STW was available to firms across all sectors, we expect employment gains to be concentrated in those that experienced the largest demand shocks (Cahuc et al., 2021; Giupponi and Landais, 2023). In such sectors, the risk of job destruction in the absence of STW was the highest. To assess this, we compare the effects of STW across manufacturing and non-manufacturing sectors. This distinction is particularly relevant, as manufacturing—typically intensive in blue-collar labor—was severely affected by the global financial crisis. As shown by Hijzen and Venn (2011), almost the entire increase in STW take-up in Europe relative to the pre-crisis period was concentrated in the manufacturing sector.

We begin by validating the sectoral split using placebo tests. Our instrument predicts differential STW take-up in the post-crisis period, while no significant pre-crisis differences are observed in treatment exposure (Table 4 and Figure C3 in the Appendix) or in outcomes (Figure C8 in the Appendix). Furthermore, Figure 2 is replicated separately for manufacturing and non-manufacturing firms (see Figure B6 in Appendix), indicating

[32] This result is robust to the use of an intensive-margin treatment definition; see Figure D1 in the Appendix.

that the observed heterogeneity in effects is not simply driven by the construction of the instrument. Notably, non-manufacturing firms display a wide range of blue-collar worker shares and considerable variation in sectoral economic shocks.

Dynamic treatment effects at the extensive margin are presented in Figure 6, with qualitatively similar results obtained using the intensive margin treatment (Figure D2 in Appendix).

In the manufacturing sector, STW take-up leads to a 24% decline in volume of work per worker in the short term, offset by a 74% increase in headcount employment among blue-collar workers per firm.[33] Total volume of work per firm increases by 35%, although this effect is only marginally significant at the 10% level. These results suggest that headcount gains broadly offset the reduction in hours per worker. The lack of statistical significance for total volume of work may reflect spillover effects from non-treated firms within the same cell, which are absorbed by our identification strategy. Notably, the magnitude of these effects is comparable to the 40% increase in total hours worked found by Cahuc et al. (2024) among the firms most affected by the Great Recession.

Using the intensive margin treatment—defined as the average volume of STW (in FTE) per firm in a cell—we find that a one-FTE increase in STW use is associated with a 2.3% decline in volume of work per worker and a 5% increase in headcount employment. This results in a 2.6% increase in total volume of work per firm. In the median manufacturing cell, firms have about 10 employees and use approximately 1.2 FTE of STW annually. Therefore, an additional FTE of STW (roughly 0.1 FTE per worker) is associated with the preservation of approximately 0.5 jobs. These effects are proportionally larger than those reported by Biancardi et al. (2025), who study the effect of STW on larger Italian manufacturing firms during the Great Recession. They find that a 10% increase in STW use—equivalent to 9 hours per worker—is associated with a 1.4% increase in headcount employment, or about 0.9 jobs per firm (median firm size: 64 employees). Our estimates imply a stronger employment response to STW use, likely reflecting the more pronounced exposure to liquidity constraints faced by smaller firms in our sample.[34]

In contrast, STW take-up by non-manufacturing firms does not yield statistically significant employment gains in either the short or medium term. While the instrument predicts take-up (Table 4 and Figure C4, Panels a and b), the decline in hours per worker is not significant. Several factors may explain this: (i) demand shocks in these sectors were shorter-lived, reducing the likelihood of detecting effects over a one-year horizon; (ii) the burden of adjustment may have been more evenly spread across workers, diluting per-worker effects; and (iii) shocks were likely more idiosyncratic and temporary, further complicating identification at the firm level, particularly when spillovers from non-treated firms are accounted for.

Robustness checks support our interpretation. Excluding the construction sector—where STW is heavily used even in normal times (Tarullo, 2025)—improves the validity of the placebo test and yields qualitatively similar results, albeit with wider confidence intervals. Additionally, an alternative treatment definition—the share of total volume of work in STW (in FTE) per cell[35]— satisfies the placebo test and leads to comparable

[33] $(e^{-0.28} - 1) \times 100\%$ and $(e^{0.56} - 1) \times 100\%$, respectively.

[34] If 0.1 FTE corresponds to approximately 1 hour of STW per worker, then according to these estimates, STW would save 0.1 jobs. However, we find the effect for every hour of STW use is 5 times larger to the ones found in Biancardi et al. (2025).

[35] This share is calculated using the total contractual volume of work in the cell as the reference.

conclusions with a sufficiently strong first stage in the short term.[36]

Furthermore, our estimates could potentially suffer from sample selection bias from endogeneous transitions across firm size categories. For instance, if a firm transitions to a size category that falls outside the selected sample during the analysis period, the current selection may prevent us from tracking the firm over time. We assume that, on average, firms are unlikely to move to larger size categories during a crisis, as employment growth tends to be limited in such periods (Konings and Yergabulova, 2021). However, firms may shift to a smaller size category due to reductions in headcount. We consider that this downward reclassification likely results in an underestimation of the impact on headcount employment, as the counterfactual group may include firms that have downsized from larger categories.

Together with the absence of significant headcount effects in the pooled sample, these findings suggest that deadweight losses were largely concentrated in non-manufacturing sectors, consistent with the notion that these sectors were less severely affected by the crisis.

While manufacturing firms exhibit short-term employment gains in the year of STW take-up, these effects do not persist in the medium term. This contrasts with findings from France (Cahuc et al., 2024) (for firms suffering the largest idiosyncratic shocks) and Switzerland (Kopp and Siegenthaler, 2021) (for average treated firms), where STW preserved low-productivity but viable jobs in temporarily affected firms. In those cases, STW-induced employment gains persisted beyond the treatment period.

The pattern of our medium-term effects is consistent with STW functioning as a temporary insurance mechanism, with non-persistent gains in employment, like in the case of Italy (Giupponi and Landais, 2023). Although contrary to Italy, the manufacturing shock in Belgium was short-lived—similarly to France—evidence from Van den Bosch and Vanormelingen (2023) documents persistent effects on restructuring and total employment, which may explain the lack of persistent effects of STW. Structural adjustments—including downsizing, automation, and shifts in production strategy—may have undermined the long-term impact of STW on job preservation. While STW may stabilize employment temporarily, it cannot offset fundamental changes in labor demand. In the Belgian manufacturing sector, restructuring likely reflected permanent reductions in capacity or competitiveness, motivated by strong connections with global value chains, ultimately leading firms to reduce employment despite STW support.

Between 2009 and 2010, significant restructuring occurred in blue-collar-intensive subsectors such as textiles, automotive, and steel (Eurofound, 2010). Van den Bosch and Vanormelingen (2023) show that job reallocation in manufacturing was productivity-enhancing, even with labor hoarding subsidies in place. This suggests that STW did not hinder labor reallocation, allowing movement toward more productive firms. While some have expressed concern that STW might delay necessary adjustments, evidence from Italy indicates that such negative reallocation effects were minimal during the Great Recession (Giupponi and Landais, 2023).

Finally, it is important to emphasize that our effects pertain to firms that were first compliers to the financial crisis shock—micro and small firms with 5 to 50 employees. While our findings cannot be generalized to later compliers or larger firms, they indicate that STW was effective in preserving jobs in the short term

[36]These additional results are available upon request.

for small, manufacturing crisis-exposed firms.

6 Conclusion

STW programs are key labor market interventions during economic downturns, offering insurance to firms and workers by preventing mass layoffs, preserving firm-specific human capital, and stabilizing aggregate demand. This paper evaluates the causal impact of Belgium's STW program during the Great Recession, a period during which take-up peaked and program expenditures rose by over 140% relative to pre-crisis levels.

Although the COVID-19 crisis prompted an unprecedented expansion of STW programs across Europe, their broad and untargeted implementation—combined with reduced employer costs and widespread use across many sectors—poses significant challenges for causal impact evaluation. The crisis involved a broader and largely exogenous economic shock that affected multiple sectors, largely independent of firm-specific conditions. In addition to representing a massive liquidity shock for firms, it was accompanied by mandated reductions in working hours due to lockdown measures. In contrast, the Great Recession, was a financially driven crisis without a lockdown component, which allow for greater variation in STW take-up across sectors and firms. Furthermore, because the program during that period was more narrowly targeted at blue-collar workers than white-collar workers, this institutional feature offers a useful setting to investigate the program's causal effects without relying on variation induced by policy changes.

We develop an IV strategy based on pre-crisis variation in blue-collar employment shares and differential sectoral turnover shocks, exploiting detailed administrative data aggregated at the location-sector level. Our results show that while STW significantly reduced the volume of work per worker (by about 23%), it did not lead to statistically significant average effects on employment or wages. However, these aggregate effects mask considerable heterogeneity. Positive employment effects are concentrated in the manufacturing sector, where the shock was most severe. At the intensive margin, a one-FTE increase in STW is associated with a 5% rise in headcount employment in manufacturing, but no significant effect in the non-manufacturing sector, suggesting that deadweight losses were largely driven by firms on average less exposed to the crisis.

Importantly, while STW resulted in positive short-term (headcount) employment effects in manufacturing, we find no evidence of persistence beyond the year of take-up. This lack of dynamic effects likely reflects the temporary nature of the support and the fact that the firms most affected at the onset were structurally weaker. The STW effect identified in our analysis primarily concerns (small) firms initially affected by the demand shock, which are likely to be low-productivity. Although STW helped these firms stabilize employment and preserve firm-specific human capital during the crisis, the benefits faded as the crisis persisted (Giupponi and Landais, 2023; Diaz et al., 2025). Restructuring and firm-level adjustments—including automation, downsizing, and shifts in production strategy—undermined the long-term preservation of jobs. Our results are consistent with productivity-enhancing job reallocation, as STW helped prevent premature layoffs without necessarily impeding the eventual reallocation of labor toward more productive firms (Van den Bosch and Vanormelingen, 2023).

While our findings provide novel insights, some limitations must be acknowledged. First, because our analysis is based on grouped data, we are unable to track reallocation effects across sectors and locations. Second, we focus on firms with 5–50 employees, a decision motivated by data consistency and the difficulty of tracking economic activity in multi-establishment firms. Although this allows us to study a sample more vulnerable to financial shocks—and likely to benefit more from liquidity support—the exclusion of larger firms may limit the generalizability of our findings. Future research should examine how STW affected larger firms, particularly given existing evidence that such firms respond differently to credit shocks (Beck et al., 2008). Lastly, we do not account for within-sector heterogeneity in STW effectiveness. It is possible that persistent job-saving effects were concentrated in temporarily low-productivity firms that faced short-term demand shocks but had large growth prospects—firms for which STW might have also functioned as a medium-term insurance mechanism.

From a policy perspective, our results underscore the importance of effective targeting. While STW provides critical social value, its success hinges on the ability to reach firms at high risk of job destruction while minimizing distortions (Cahuc, 2024). Poor targeting—often resulting from information asymmetries between firms and policymakers—can lead to substantial deadweight losses. Screening mechanisms based on verifiable financial indicators, such as recent declines in turnover or low liquidity, may enhance program efficiency, particularly in sectors less exposed to aggregate economic shocks. Additionally, extending access to firms in sectors with forecasted demand declines could help prioritize support for those most in need. However, implementation remains challenging due to the idiosyncratic nature of firm-level shocks and the administrative capacity required for timely intervention.

In sum, STW programs play a vital role in stabilizing labor markets during crises. By improving the design and targeting of STW, policymakers can better balance the objectives of job preservation and economic efficiency.

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Tables and Figures

Tables

Table 1: First stage estimates

	Fraction of firms in STW		Average volume of work (in FTE) in STW per firm	
	(1)	(2)	(3)	(4)
Z_rs,2009 I[t=2009]	0.8551***	0.8618***	7.8939***	8.3490***
	(0.1282)	(0.1263)	(1.2365)	(1.2619)
Trend FE	Yes	Yes	Yes	Yes
Control: Share	Yes	Yes	Yes	Yes
Control: Shock	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Control: Log of turnover in 2007	Yes	No	Yes	No
Restricted model	No	Yes	No	Yes
F-stat (Stock and Yogo)	44.4782	46.5351	40.7549	43.7736
R-squared	0.5084	0.5071	0.5325	0.5268
Obs	9350.0000	9380.0000	9350.0000	9380.0000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. The dependent variable is the fraction of firms in STW in a cell in Columns (1)-(2), and the average volume of work (in FTE) in STW per firm in a cell in Columns (3)-(4). Regressions are weighted by $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t. This table presents the first stage results from the estimation of Equation (4) for the period 2004–2009.

Table 2: TSLS estimates (employment effects decomposition)

	Growth in (BC) volume of work per worker		Growth in (BC) jobs per firm		Growth in (BC) volume of work per firm	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x I[t=2009]	-0.2612	-0.2270	0.1428	0.0835	-0.0873	-0.1121
	(0.1680)	(0.1559)	(0.2409)	(0.2145)	(0.3851)	(0.3489)
Trend FE	Yes	Yes	Yes	Yes	Yes	Yes
Control: Share	Yes	Yes	Yes	Yes	Yes	Yes
Control: Shock	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Control: Log of turnover in 2007	Yes	No	Yes	No	Yes	No
Restricted model	No	Yes	No	Yes	No	Yes
F-stat (Kleibergen-Paap)	44.0273	47.5054	44.0273	47.5054	44.0273	47.5054
Placebo Test (Joint F-test p-value)	0.2667	0.4921	0.2668	0.1062	0.5722	0.3485
R-squared	0.0580	0.0590	0.0359	0.0347	0.0520	0.0479
Obs	9296.0000	9296.0000	9296.0000	9296.0000	9296.0000	9296.0000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. The dependent variables are expressed in logs, therefore the effects can be interpreted as semi-elasticities. The dependent variable Growth in jobs per firm is measure at the end of the quarter. The Placebo Test indicates whether there are responses (in the RF model) that are jointly statistically significantly different from zero in the pre-crisis period. A rejection of this test invalidates the placebo test. Regressions are weighted by $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t.

Table 3: TSLS estimates (wage effects)

	Gross Wage rate		Gross Wage bill per employee	
	(1)	(2)	(3)	(4)
Treatment x I[t=2009]	0.0567	0.1119***	-0.2045	-0.1151
	(0.0476)	(0.0426)	(0.1490)	(0.1426)
Trend FE	Yes	Yes	Yes	Yes
Control: Share	Yes	Yes	Yes	Yes
Control: Shock	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Control: Log of turnover in 2007	Yes	No	Yes	No
Restricted model	No	Yes	No	Yes
F-stat (Kleibergen-Paap)	44.0273	47.5054	44.0273	47.5054
Placebo Test (Joint F-test p-value)	0.4922	0.2004	0.6633	0.4521
R-squared	0.3098	0.2695	0.0762	0.0624
Obs	9296.0000	9296.0000	9296.0000	9296.0000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. The dependent variables are expressed in logs, therefore the effects can be interpreted as semi-elasticities. The Placebo Test indicates whether there are responses (in the RF model) that are jointly statistically significantly different from zero in the pre-crisis period. We set that a rejection at the 5% level of this test invalidates the placebo test. Regressions are weighted by $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t.

Table 4: First stage estimates: heterogeneity across economic sectors.

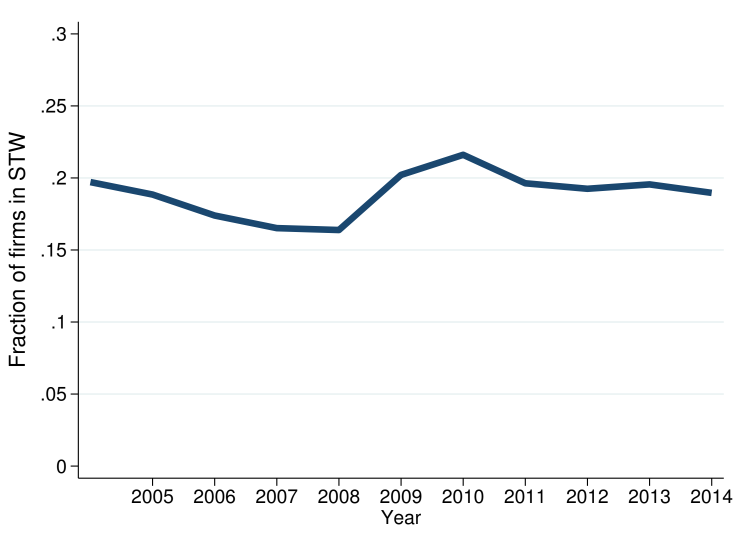
	Manufacturing				**Non-manufacturing**			
	Fraction of firms in STW		Average volume of work (in FTE) in STW per firm		Fraction of firms in STW		Average volume of work (in FTE) in STW per firm	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Z\_rs, 2009 \times \mathbb{I}[t{=}2009]$	0.3966***	5.2315***	0.5002***	6.0508***	2.0467***	13.6560***	2.0399***	13.5923***
	(0.1222)	(1.4573)	(0.1175)	(1.3443)	(0.4087)	(2.9004)	(0.4020)	(2.8385)
Trend FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control: Share	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control: Shock	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control: Log of turnover in 2007	Yes	No	Yes	No	Yes	No	Yes	No
Restricted model	No	Yes	No	Yes	No	Yes	No	Yes
F-stat (Stock and Yogo)	10.5285	12.8866	18.1299	20.2586	25.0836	22.1678	25.7435	22.9300
R-squared	0.5061	0.5425	0.4959	0.5388	0.5722	0.5980	0.5708	0.5957
Obs	4655.0000	4655.0000	4685.0000	4685.0000	4695.0000	4695.0000	4695.0000	4695.0000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. The dependent variable is the fraction of firms in STW in a cell in Columns (1)-(2) and (5)-(6), and the average volume of work (in FTE) in STW per firm in a cell in Columns (3)-(4) and (7)-(8). Regressions are weighted by $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t.

This table presents the first stage results from the estimation of Equation (4) for the period 2004–2009 for manufacturing and non-manufacturing firms.

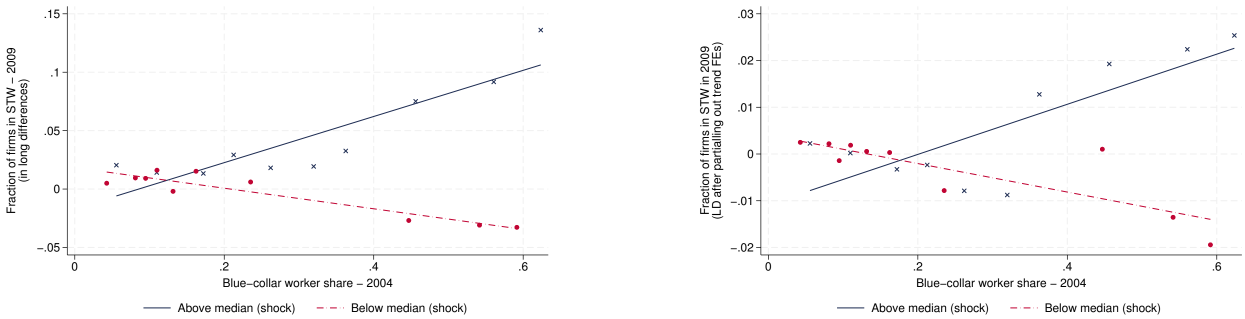
Figures

Figure 1: Fraction of firms (with 5-49 employees) in STW in Belgium (2005-2014).



Notes: This figure displays the average fraction of firms in STW to the total number of firms in a year. Each year t does not correspond to a calendar year, but it is defined as the combination of the last two quarters of year $t-1$ and the first two quarters of a year t. For instance, 2009 corresponds to the period 2008Q3–2009Q2.

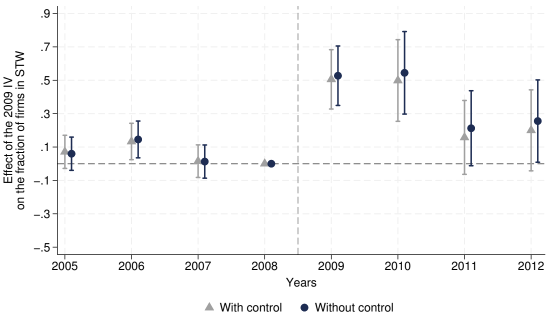
Figure 2: Change in the fraction of firms in STW from 2004 to 2009 and the blue-collar worker share in 2004 across intensity of sectoral turnover decline.



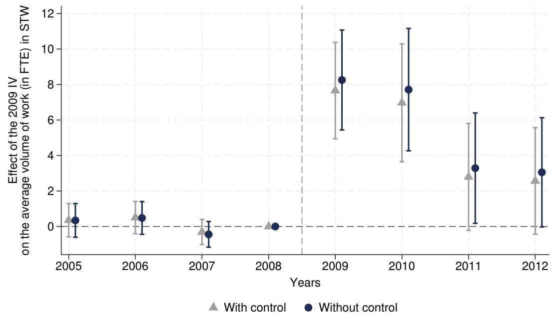
Notes: This figure displays in the left-hand side panel the average change in the fraction of firms in STW in 2009 with respect to 2004 and the blue-collar worker share in 2004, above and below the median of a turnover shock experienced in 2009 with respect to 2008. In the right-hand side panel we plot the residuals obtained from a regression of the same change in the fraction of firms in STW in 2009 with respect to 2004, on location-sector specific trends between 2004 and 2009 and potential confounders (e.g., the blue-collar worker share in 2004 interacted with time indicators, the growth rate of turnover interacted with time indicators in 2009, and location-trend specific fixed effects). LD stands for long differences.

Figure 3: Placebo test (First stage and Reduced Form)

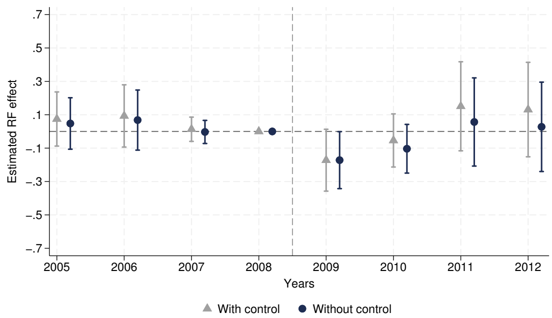
(a) First stage estimator (extensive margin treatment)



(b) First stage estimator (intensive margin treatment)



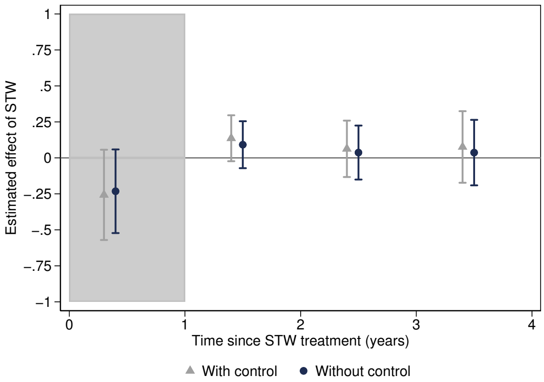
(c) Reduced form estimator (outcome: volume of work per worker)



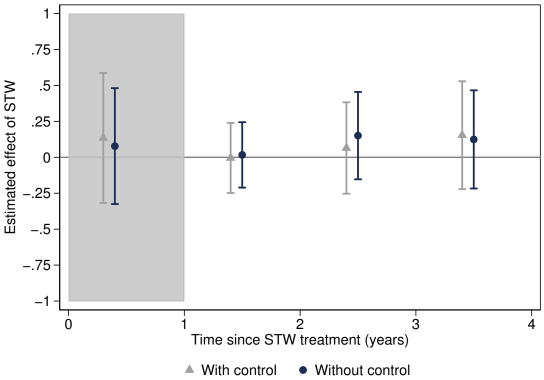
Notes: The vertical bars correspond to 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. The instrument (z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share fixed in 2004. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (i.e., the reference year). This graph plots Equation (3) with three distinct dependent variables. Two models are presented: a model with $\gamma^*_{4t} \neq 0$ (i.e., the most flexible model with the log of the (LOO) sectoral turnover in 2007 defined with the triangle in gray) and a model with $\gamma^*_{4t} = 0$ (i.e., a model without such control defined with the dot in blue). The outcomes in Panel (a) and (b) are the fraction of firms in STW and the average volume of work (in FTE) in STW per firm, respectively. The outcome in Panel (c) is the volume of work in FTE per (blue-collar) worker in logs. The effect in Panel (c) can be interpreted as a semi-elasticity. The F joint significance test for the pre-crisis period is not rejected at the 5% level for Panel (a) (p-value of 0.12 and 0.08), Panel (b) (0.26 and 0.15) and Panel (c) (0.78 and 0.77) in all the specifications.

Figure 4: Dynamic Treatment Effects: employment decomposition

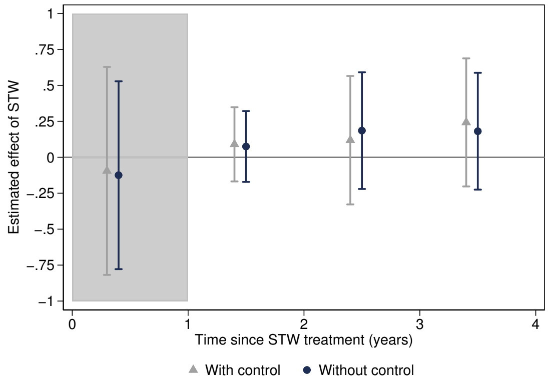
(a) FTE per blue-collar worker (in logs)



(b) Blue-collar workers per firm (in logs)



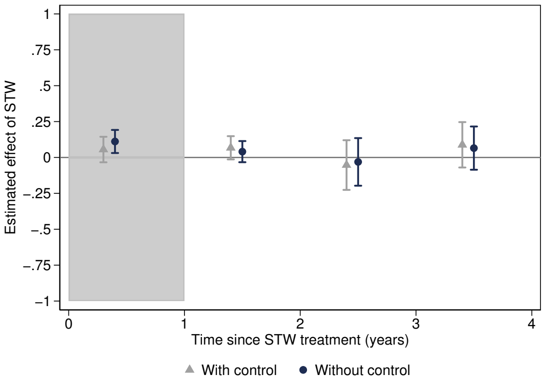
(c) FTE per firm (in logs)



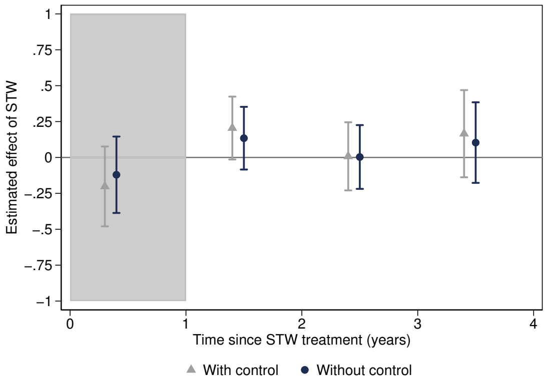
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \cdots, \hat{\alpha}_4\}$. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). Panel (a) shows the effect of STW treatment on FTE per blue-collar worker (our first order outcome). Panel (b) shows the effect of STW treatment on headcount employment per firm as a stock at the end of each quarter. Panel (c) shows the effect of STW treatment on volume of work in FTE per firm. The coefficients displayed in the following graph can be interpreted as semi-elasticities.

Figure 5: Dynamic Treatment Effects: wage outcomes

(a) (Gross) wage rate for blue-collar workers



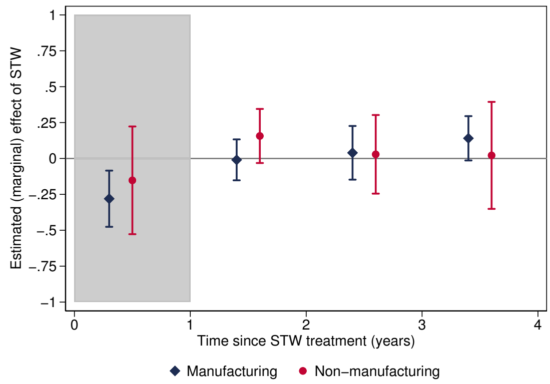
(b) (Gross) wage bill per blue-collar worker



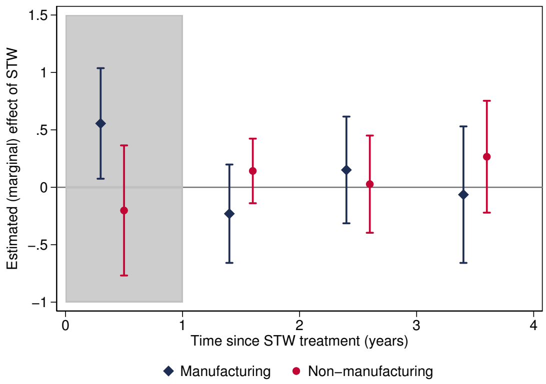
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \cdots, \hat{\alpha}_4\}$. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). Panel (a) shows the effect of STW treatment on FTE per blue-collar worker (our first order outcome). Panel (b) shows the effect of STW treatment on headcount employment per firm as a stock at the end of each quarter. Panel (c) shows the effect of STW treatment on volume of work in FTE per firm. The coefficients displayed in the following graph identify the dynamic treatment effect of receiving STW in $t = 0$ on outcomes in the following years ($t > 1$).

Figure 6: Dynamic Treatment Effects: Employment Decomposition by Sector

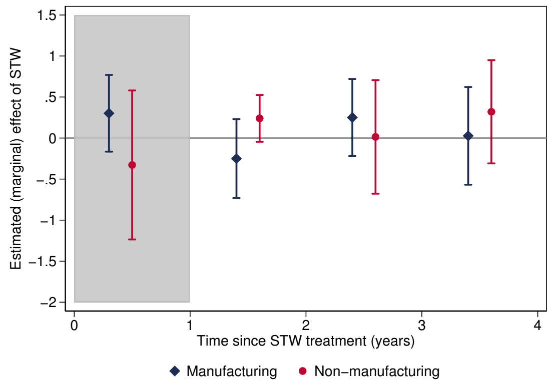
(a) FTE per blue-collar worker



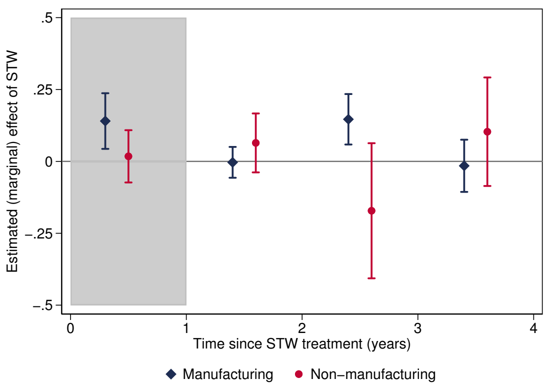
(b) Blue-collar (end-of-quarter) workers per firm



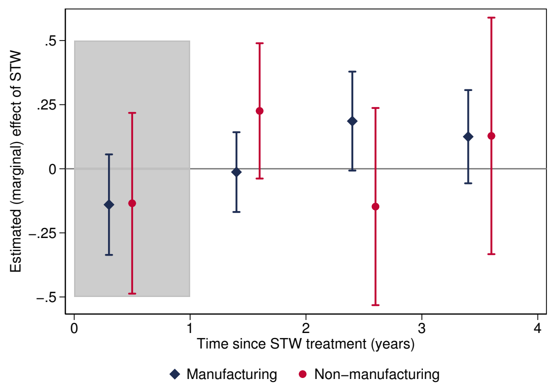
(c) FTE per firm



(d) Blue-collar wage rate



(e) Blue-collar wage bill per worker



Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \cdots, \hat{\alpha}_4\}$. Panel (a) shows the effect of STW treatment on FTE per blue-collar worker (our first order outcome). Panel (b) shows the effect of STW treatment on headcount employment per firm at the end-of-the-quarter. Panel (c) shows the effect of STW treatment on headcount employment per firm during the quarter. Panel (d) shows the effect of STW treatment on volume of work in FTE per firm. The coefficients displayed in the following graph can be interpreted as semi-elasticities. The model presented is the one with $\gamma^*_{4t} = 0$ (i.e., a model without control). Similar results are obtained when imposing $\gamma^*_{4t} \neq 0$.

Appendices

A Policy and Data

A.1 The policy

Table A1: Modalities of STW in Belgium during the Great Recession.

Type of worker	**Types of reduction in working time**	**Maximum Duration**
Blue-collar employees	Partial - small suspension ($\geq$ 3 days of work per week)	12 months
	Partial – long suspension ($<$ 3 days of work per week)	3 months
	Total reduction of activity	4 weeks (28 calendar days)
White-collar employees	Partial ($\geq$ 2 days of work per week)	26 weeks*
	Total	16 weeks**

Notes: (*) This count of weeks is net of the number of weeks under partial suspension divided by two. (**) This count of weeks is net of the number of weeks under total suspension divided by two.

A.2 Sector NACE structural break

One of the key dimension of aggregation in our dataset is the economic sector defined at the 3-digit NACE level. Therefore, a relevant step is the proper definition of the sector dimension. There is a change in the definition that conflicts with the period of our analysis (e.g., 2006-2014). This change was the shift of the NACE Rev. 1.1 to the NACE Rev. 2.0 definition of economic sectors. The latter definition was adopted from January 1st, 2008 and reflects the technological developments and structural changes of the economy. Changes in economic structures and organizations, as well as technological developments, give rise to new activities and products, which may supersede existing activities and products. This results in shifts of lower levels of NACE codes (e.g., 4- and 5-digit levels) across 3-digit NACE codes. Since we do not observe lower levels than 3-digit NACE sectors in our dataset, we require to build a homogeneous definition based on 3-digits NACE sectors. We based the construction of this definition on the conversion provided by the public administration (NSO) at the 5-digit level for the year 2007. We also take into account the representation of each sector in terms of employment in FTE to do this conversion. The procedure consists on building clusters of NACE codes that are homogenous before and after the break, minimizing as much as possible the structural break in the series. To do this, we revised the 238 codes in NACE Rev. 2.0 and the 193 codes in NACE Rev.1.1 together with the ONSS conversion at the 5-digit level that we obtained from this institution. First, we trim the 5-digit NACE codes at the 3-digit for both the NACE Rev.2.0 and NACE Rev.1.1 in the ONSS file. This step shows that the majority of 3-digit NACE sectors had a 1:1 conversion (e.g., one sector is matched to a single sector between the two classifications). However, a considerable amount of sectors had a 1:N (e.g., one sector in 2008 is matched to many sectors in 2003) correspondence or an N:1 correspondence (e.g., one sector in 2008 is matched to many sectors in 2003). Therefore, as a next step, we apply some rules to convert cases 1:N and N:1.In case 1:N, we assign the NACE Rev.2.0 code to the NACE Rev.1.1 code with the highest value of employment in FTE from among the N NACE Rev.1.1 codes. In case N:1, sector clusters are created. In this sense, all NACE Rev.2.0 sectors and their corresponding NACE Rev.1.1 counterparts will form a single cluster. This conversion finally leads to 134 clusters, balanced for the period 2003-2021. This conversion will be useful to implement the identification strategy using the blue-collar workers share in $t-2$ as an IV component.

A.3 Descriptive statistics

Table A2: Outcome variables description

Name	**Description**
Log of (blue-collar) volume of work per job	Ratio of effective blue-collar jobs' volume of work in FTE to the total number of blue-collar jobs in a cell. Both, numerator and denominator are quarterly during-quarter values aggregated as an average over the four quarters of a year.
Log of (blue-collar) jobs per firm	Ratio of blue-collar jobs to the total number of firms in a cell. The numerator is an average of quarterly during-quarter values aggregated over four quarters of a year. This allows for an exact decomposition of employment outcomes. The denominator is stock value measured at the end of each quarter and expressed as an average over the four quarters in a year.
Log of (blue-collar) volume of work per firm	Ratio of effective blue-collar jobs' volume of work in FTE to the total number of firms in a cell. The numerator is a during-quarter value aggregated as an average over four quarters in a year. The denominator is stock value measured at the end of each quarter and expressed an average over the four quarters in a year.
Log of (blue-collar) wage rate	Ratio of the gross wage bill for blue-collar jobs to the total volume of blue-collar work in FTE in a cell.
Log of (blue-collar) wage bill per worker	Ratio of the gross wage bill for blue-collar jobs to the total number of blue-collar jobs in a cell.

Notes: This table provides a description of the main outcomes analyzed. A year is defined as the combination of four quarters: the last two from the previous calendar year and the first two from the current year. For instance, 2009 refers to averages over 2008q3, 2008q4, 2009q1, and 2009q2.

Table A3: Descriptive statistics by quantile of fraction of firms in STW in 2009.

	Fraction of firms in STW by 4 quantiles				
	1	2	3	4	Total
Fraction of firms in STW	0.00	0.11	0.38	0.79	0.31
	(0.00)	(0.06)	(0.09)	(0.18)	(0.30)
Share of Blue-collar jobs in 2004	0.24	0.20	0.42	0.49	0.35
	(0.20)	(0.14)	(0.15)	(0.14)	(0.20)
(Leave-one-out, negative) Growth rate of turnover in 2009	0.01	0.04	0.05	0.05	0.04
	(0.57)	(0.13)	(0.21)	(0.42)	(0.36)
Volume of work per (BC) job (in FTE)	0.68	0.61	0.69	0.65	0.66
	(0.18)	(0.16)	(0.11)	(0.11)	(0.14)
(BC) Jobs per firm (during-quarter)	7.11	7.49	11.42	13.45	9.99
	(6.34)	(4.55)	(4.59)	(5.83)	(5.86)
(BC) Jobs per firm (end-of-quarter)	6.47	6.39	10.55	12.66	9.14
	(5.84)	(3.53)	(4.31)	(5.56)	(5.45)
(BC) Volume of work per firm (in FTE)	4.99	4.32	7.87	8.79	6.63
	(4.84)	(2.67)	(3.47)	(4.30)	(4.26)
Wage rate (euros per FTE per quarter)	5317.48	5117.46	5722.47	5693.30	5493.97
	(901.02)	(915.19)	(866.93)	(694.16)	(890.20)
Wage bill per (BC) job (euros per job per quarter)	3660.26	3206.85	3989.66	3711.29	3690.61
	(1254.40)	(1165.95)	(957.81)	(843.66)	(1096.41)
Number of firms	2,726	33,564	15,420	5,709	57,419
Number of cells (l $\times$ s)	430	397	660	374	1,861

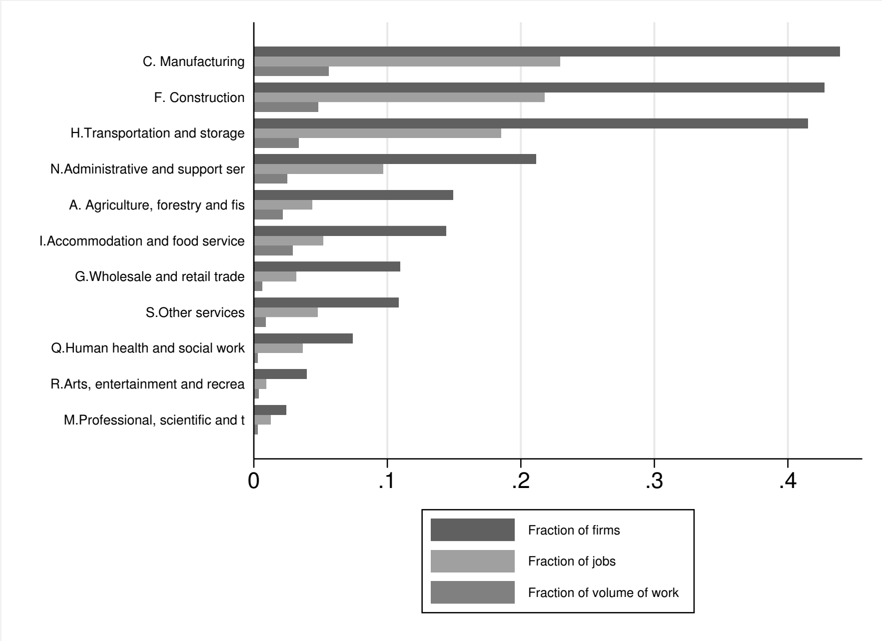
Notes: Wages are deflated taking into account the year 2004 as a baseline. The wage rate is computed by dividing the total gross (before-tax) wage bill (for blue-collar workers) by the total volume of work in FTE (for blue-collar workers). The gross wage bill does not account for employer social security contributions. The leave-one-out growth rate of turnover is multiplied by -1; in other words, a higher growth rate represents a more negative shock in demand. Wages are expressed in real terms taking a reference year the year 2004.

Table A4: Evolution of STW take-up by sector.

	2007				2008				2009				2010				2011			
	%	**Firms**	%	**Jobs**	%	**Firms**	%	**Jobs**	%	**Firms**	%	**Jobs**	%	**Firms**	%	**Jobs**	%	**Firms**	%	**Jobs**
A. Agriculture, forestry and fishing	12.0%	968	3.0%	16,629	14.0%	935	5.0%	15,448	15.0%	978	4.0%	17,124	13.0%	1,033	4.0%	17,978	14.0%	1,063	4.0%	18,426
C. Manufacturing	29.0%	8,449	12.0%	138,022	31.0%	8,340	14.0%	136,741	44.0%	8,115	23.0%	131,935	40.0%	7,951	19.0%	129,074	36.0%	7,803	16.0%	127,619
F. Construction	35.0%	8,427	18.0%	115,394	37.0%	8,586	19.0%	116,980	43.0%	8,481	22.0%	113,817	41.0%	8,588	20.0%	113,873	39.0%	8,672	19.0%	115,903
G.Wholesale and retail trade	7.0%	16,081	2.0%	216,583	8.0%	16,219	2.0%	218,463	11.0%	15,976	3.0%	213,757	10.0%	15,986	3.0%	213,731	9.0%	16,164	2.0%	218,314
H.Transportation and storage	21.0%	3,601	7.0%	62,871	25.0%	3,630	10.0%	62,632	41.0%	3,510	19.0%	58,844	35.0%	3,460	13.0%	57,985	31.0%	3,443	11.0%	57,970
I.Accommodation and food service	12.0%	5,896	4.0%	84,149	12.0%	5,914	4.0%	84,069	14.0%	5,895	5.0%	83,245	14.0%	6,033	5.0%	85,620	14.0%	6,002	5.0%	86,249
J.Information and communication	1.0%	1,509	0.0%	24,144	1.0%	1,587	0.0%	25,994	1.0%	1,580	1.0%	25,290	1.0%	1,585	1.0%	25,021	1.0%	1,603	0.0%	25,936
K.Financial and insurance activities	0.0%	1,552	0.0%	18,830	0.0%	1,664	0.0%	20,046	1.0%	1,627	0.0%	19,610	1.0%	1,623	0.0%	19,665	1.0%	1,652	0.0%	19,906
L. Real estate activities	2.0%	546	0.0%	7,221	2.0%	582	1.0%	7,569	2.0%	590	1.0%	7,442	2.0%	637	1.0%	7,880	2.0%	679	1.0%	8,354
M.Professional, scientific and technical activ.	1.0%	4,056	1.0%	52,346	2.0%	4,186	1.0%	54,129	2.0%	4,193	1.0%	53,851	2.0%	4,247	1.0%	53,694	2.0%	4,325	1.0%	54,881
N.Administrative and support service	15.0%	2,509	6.0%	41,279	15.0%	2,614	6.0%	42,177	20.0%	2,546	8.0%	40,915	25.0%	3,118	11.0%	55,806	25.0%	3,257	11.0%	60,225
Q.Human health and social work	5.0%	3,715	2.0%	74,988	6.0%	3,866	3.0%	78,698	7.0%	4,137	4.0%	83,090	3.0%	3,636	2.0%	70,343	3.0%	3,574	2.0%	69,006
R.Arts, entertainment and recreation	3.0%	1,306	1.0%	21,524	3.0%	1,302	1.0%	21,665	4.0%	1,338	1.0%	22,190	4.0%	1,358	1.0%	20,544	4.0%	1,385	1.0%	21,026
S.Other services	8.0%	3,217	3.0%	39,144	8.0%	3,256	4.0%	39,146	11.0%	3,099	5.0%	37,352	11.0%	3,167	5.0%	38,554	11.0%	3,200	5.0%	39,031

Source: National Social Security Office (NSSO). Elaborated by authors.

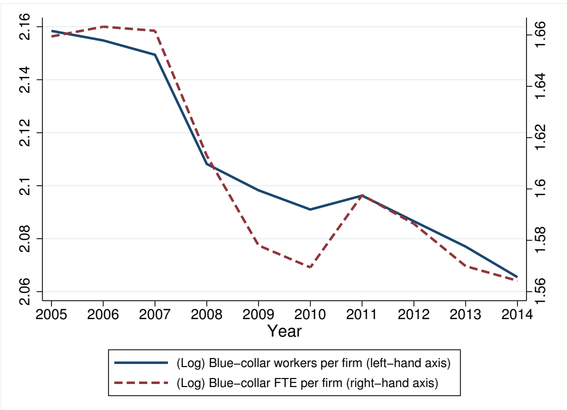
Figure A1: STW take-up by sector in 2009.



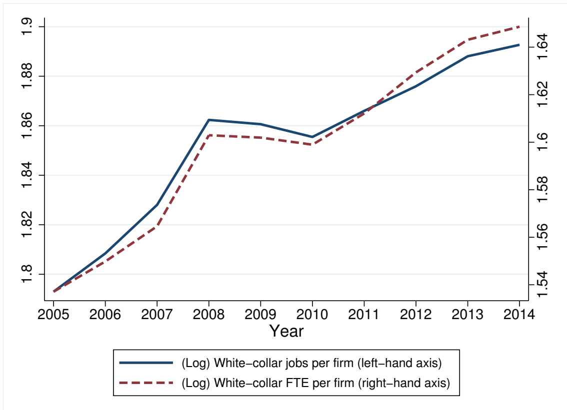
Notes: This figure is elaborated with the data after the selection procedure described in Section 3.

Figure A2: Evolution of employment per firm: blue-collar vs white-collar workers.

(a) Blue-collar workers

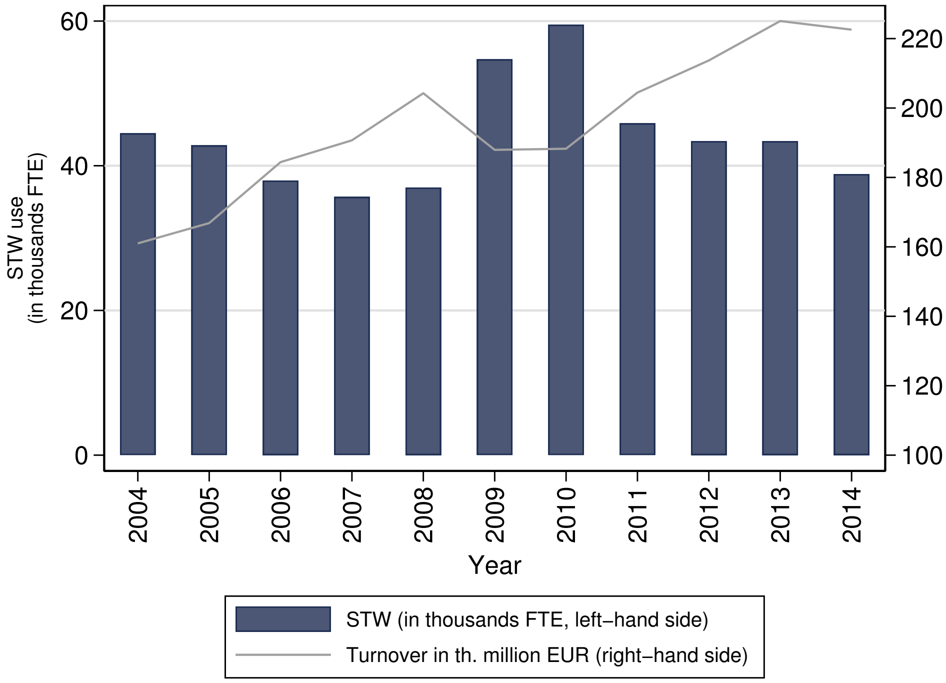


(b) White-collar workers



Notes: These figures show the comparison between the evolution of the number of jobs and volume of work in FTE per firm for each type of worker. Notice that we use the definition of "calendar" years in these graphs and not the definition of years described in Section 4. We can see than in 2008, the average blue-collar worker per firm already starts to decline. This is likely the case given that in 2008q3 and 2008q4, we observe drops in aggregate employment outcomes.

Figure A3: Evolution of turnover and STW take-up.



Notes: These graphs shows the evolution of STW in FTE and turnover in thousands of million EUR during the period 2004-2014. This graphs displays statistics not using calendar years but instead the new definition of years explained in Section 4. This new definition was adopted since the shock was initially encountered in the last two quarters of 2008.

B Graphical evidence of IV components

In this section, I present graphical evidence of the correlations of each of the components of the interaction used to instrument STW treatment during the Great Recession. I focus on the treatment at the extensive margin, defined as the ratio of firms in STW to the total number of firms in the cell.

First, we plot the correlation between the (leave-one-out) growth rate of turnover in 2009 and the fraction of firms in STW (Figure B1). We can observe that the proportion of firms in STW in a sector-region cell and the drop in turnover are inversely correlated.

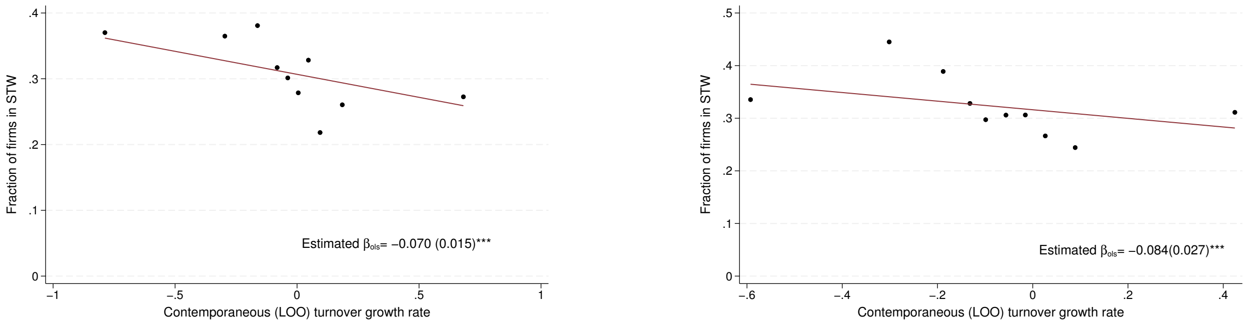
In Figure B2, we document the heterogeneity of blue-collar workers' shares across sectoral-regional cells. This figure shows the cross-sectional variation in 2004 (i.e., the pre-crisis period). The fractions of blue-collar workers varies widely from 0 to 1 across cells. The median share of blue-collar workers is 0.37.

In Figure B3, we display the blue-collar worker share in 2004 versus the blue-collar worker share in 2009 of all cells. We can observe that cells where the blue-collar workers share is high in 2004 also remain with a high share in 2009. The slope is close to 1. This fact suggests that cells with a larger exposure to the program in 2007 also had a larger exposure during the crisis. Moreover, this share was not adjusted significantly because of the crisis.

Figure B4 shows that there is a positive relationship between the blue-collar workers share of the cell in 2004 and take-up (in terms of number of firms in STW) in 2009, during the Great Recession. This relationship

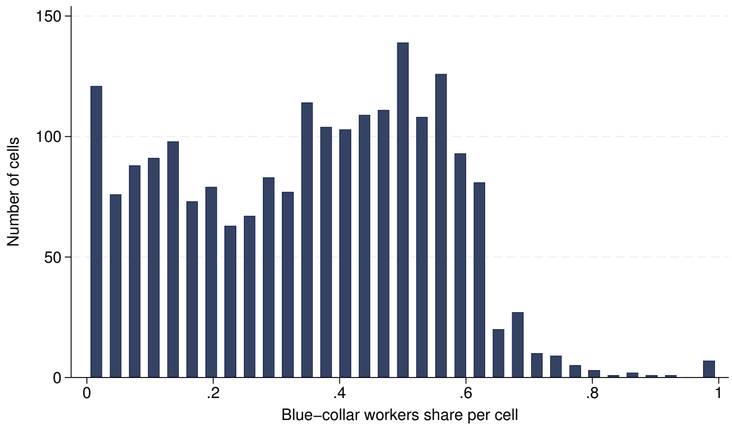
Figure B1: Correlation between the fraction of firms in STW and the contemporaneous (sectoral) growth rate of turnover.

(a) 2009–2012 **(b)** 2009 and 2010



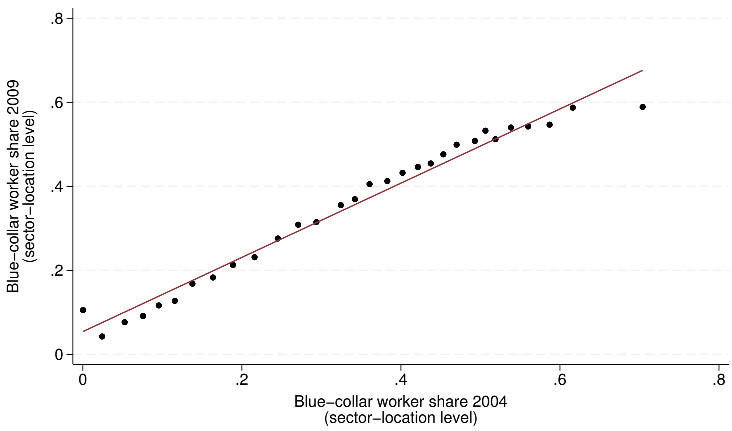
Notes: β_{OLS} is the estimator from regressing the fraction of firms in STW on the contemporaneous growth rate of FTE with respect to t-2. Panel (a) and (b) controls for year fixed effects. Data is aggregated at the level of 10 equal-sized bins. The definition of years is the one described in Section 4.

Figure B2: Histogram of the share of blue-collar workers at the sectoral-regional level in 2004



Notes: The blue-collar worker share is computed as an average over four quarters in a year of flow values of employment. We use the year definition as in Section 4.

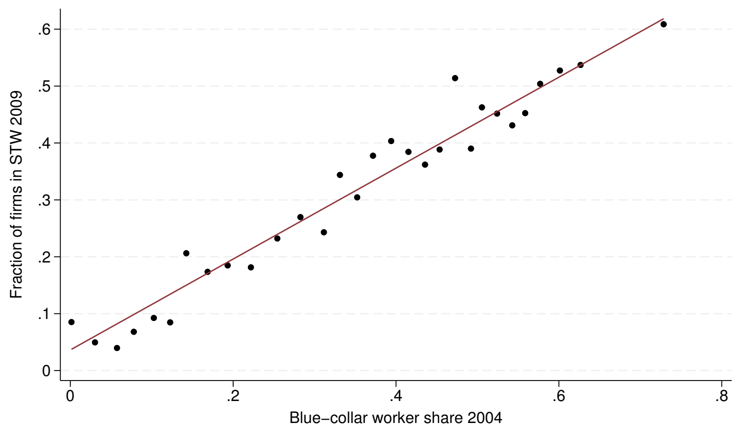
Figure B3: Blue-collar worker share in 2004 vs blue-collar worker share in 2009



Notes: Data aggregated at the level of 30 equal-sized bins.

holds even after controlling for by the llo growth rate of turnover.

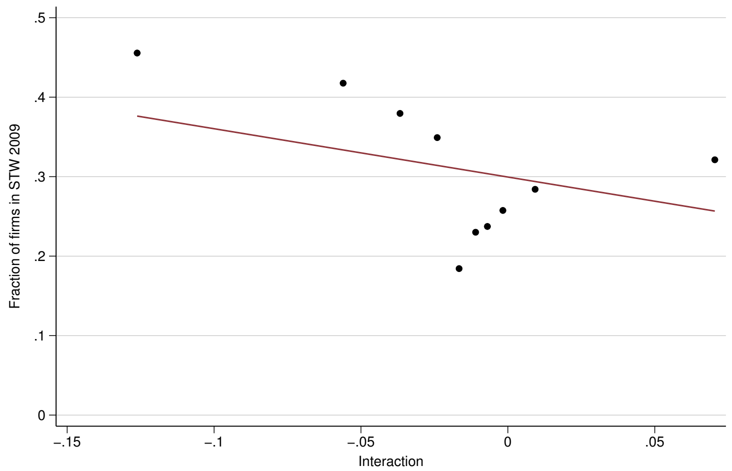
Figure B4: Fraction of firms in STW in 2009 vs blue-collar worker share in 2004



Notes: The relationship displayed in the following graphs controls for the leave-one-of growth rate of turnover. Data aggregated at the level of 30 equal-sized bins.

Overall, both components of our instrument are correlated with the use of STW during the crisis. We use the interaction between shocks predicted by turnover fluctuations and the share of blue-collar workers prior to the crisis as an instrument to predict STW take-up in 2009. The relationship between these two variables is displayed in Figure B5.

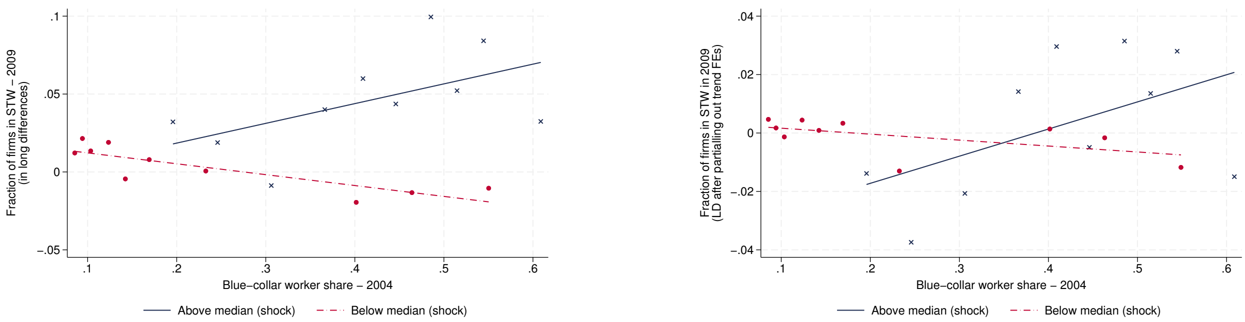
Figure B5: Fraction of firms in STW in 2009 vs interaction



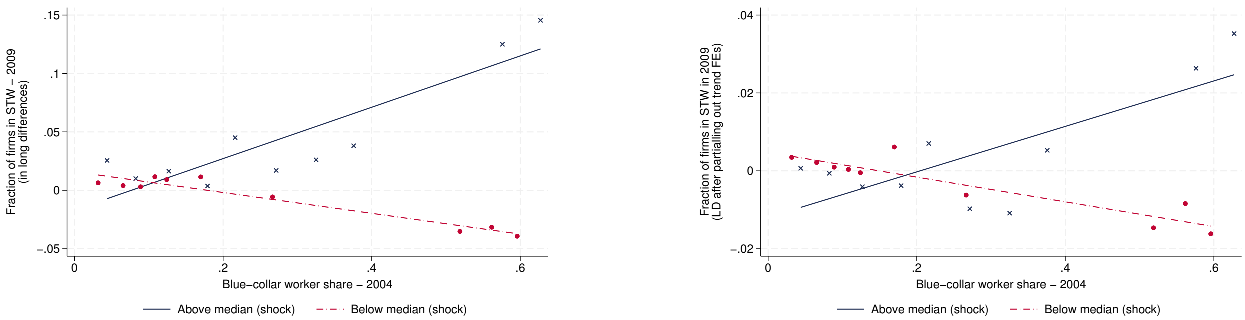
Notes: The top 1% of the interaction is winsorized. Data aggregated at the level of 10 equal-sized bins. This relationship controls for the leave-one-of growth rate of turnover, as well as the log of turnover in 2009.

Figure B6: Change in the fraction of firms in STW from 2004 to 2009 and the blue-collar worker share in 2004 across intensity of sectoral turnover decline by economic sector.

I. Manufacturing



II. Non-manufacturing



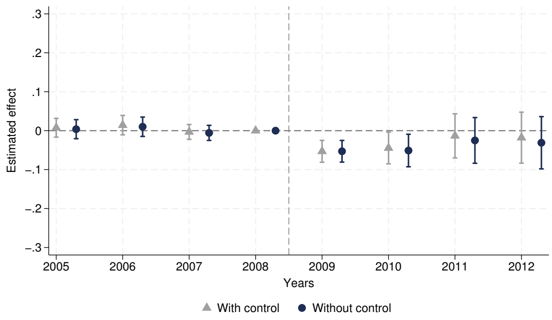
Notes: This figure displays in the left-hand side panels the average change in the fraction of firms in STW in 2009 with respect to 2004 and the blue-collar worker share in 2004, above and below the median of a turnover shock experienced in 2009 with respect to 2008, for the manufacturing and non-manufacturing sectors. In the right-hand side panels we plot the residuals obtained from a regression of the same change in the fraction of firms in STW in 2009 with respect to 2004, on location-sector specific trends between 2004 and 2009 and potential confounders (e.g., the blue-collar worker share in 2004 interacted with time indicators, the growth rate of turnover interacted with time indicators in 2009, and location-trend specific fixed effects) for both sectors respectively. LD stands for long differences.

C Placebo tests

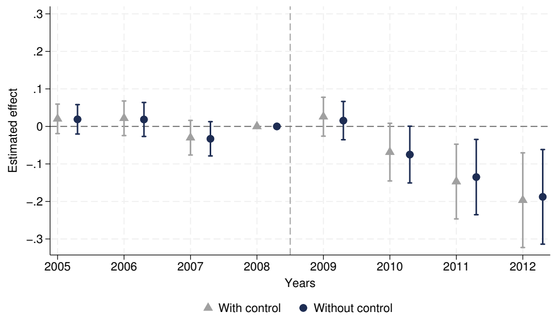
C.1 OLS model

Figure C1: Placebo test (OLS): employment outcomes.

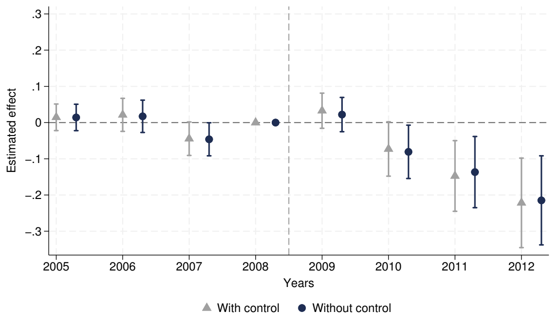
(a) Volume of blue-collar work per worker (in logs) (in logs)



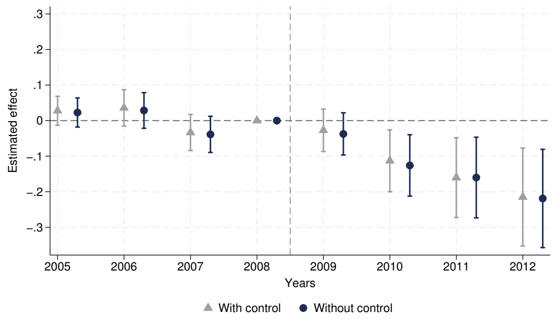
(b) Blue-collar jobs (during-quarter) per firm (in logs)



(c) Blue-collar jobs (end-of-the-quarter) per firm (in logs)



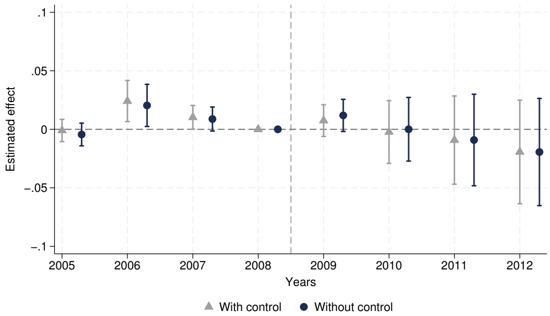
(d) Volume of blue-collar work per firm (in logs)



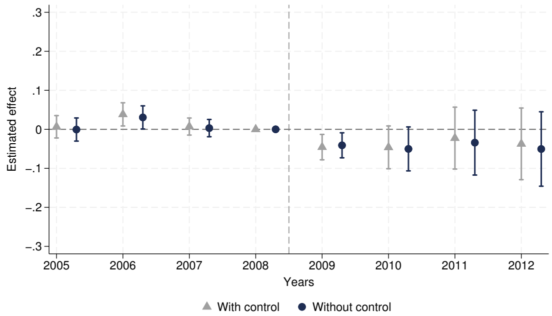
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the treatment defined in 2009. The treatment is defined as the fraction of firms in STW within a cell in 2009. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). All the effects can be interpreted as a semi-elasticities. Two specifications are displayed: (i) the most flexible model with control (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). The joint significance F-test p-value in the pre-2008 period is as follows for model (i) and (ii), respectively: Panel (a) has a p-value of 0.40 and 0.44. Panel (b) has a p-value of 0.10 and 0.09, respectively. Panel (c) has a p-value of 0.02 and 0.02, respectively, so the test is rejected. Panel (d) has a p-value of 0.01 and 0.01, respectively, so the test is rejected.

Figure C2: Placebo test (OLS): gross wages.

(a) Gross wage rate (in logs) (in logs)



(b) Gross wage bill per firm (in logs)



Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the treatment defined in 2009. The treatment is defined as the fraction of firms in STW within a cell in 2009. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). All the effects can be interpreted as a semi-elasticities. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). The joint significance F-test p-value in the pre-2008 period is as follows for model (i) and (ii), respectively: Panel (a) has a p-value of 0.05 and 0.10. Panel (b) has a p-value of 0.06 and 0.12, respectively.

C.2 First stage

Figure C3: Placebo test (first stage): Manufacturing sector take-up.

(a) **(b)**

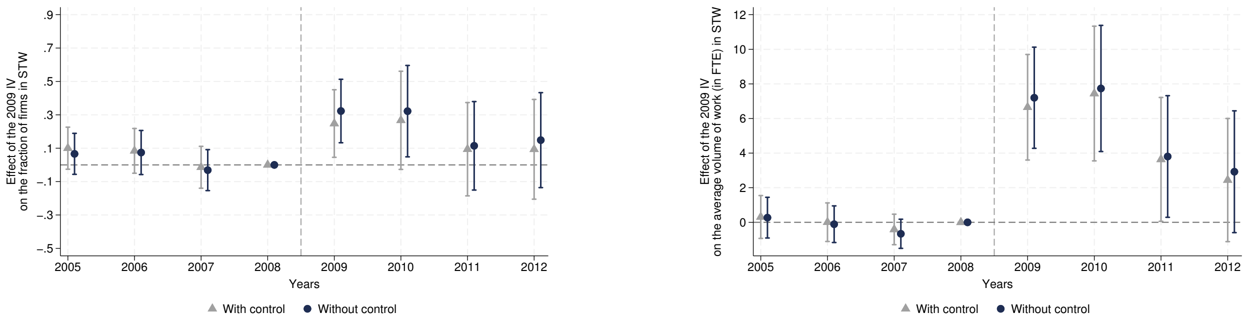


Figure C4: Placebo test: Non-manufacturing sector take-up.

(a) **(b)**

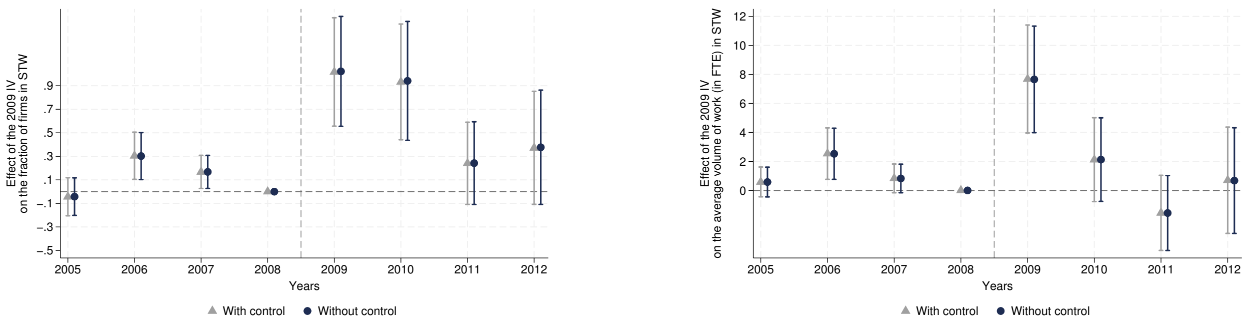
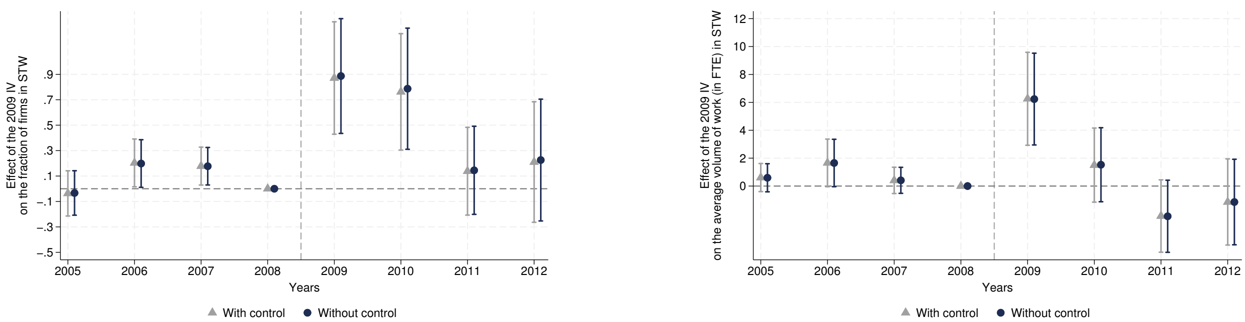


Figure C5: Placebo test: Non-manufacturing (without construction) sector take-up.

(a) **(b)**

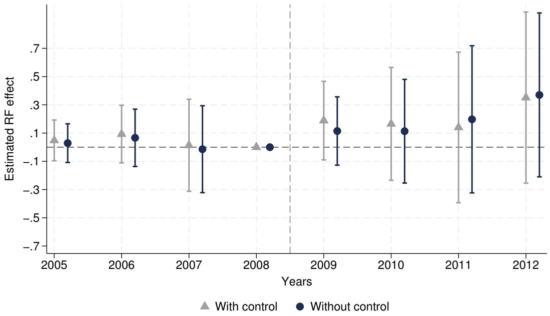


Notes: The vertical bars correspond to 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. The instrument (z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share in 2004. Dependent variables are defined in long differences with respect to the year 2004 for manufacturing and non-manufacturing sectors. We plot the effects relative to the average trend between 2004 and 2008 (i.e., the reference year). This graph plots Equation (3) with three distinct dependent variables. Two models are presented: a model with control ($\gamma^*_{4t} \neq 0$, defined by the triangle in gray) and a model without control ($\gamma^*_{4t} = 0$, defined by the dot in blue). The outcomes in Panel (a) and (b) are the fraction of firms in STW and the average volume of work (in FTE) in STW per firm, respectively. The F joint significance test for the pre-crisis period is not rejected at the 5% level for Panel (a) (p-value of 0.57), Panel (b) (p-value of 0.29) for the manufacturing sector. The F joint significance test for the pre-crisis period is rejected at the 5% level for Panel (a) (p-value of 0.004), Panel (b) (p-value of 0.0412) for the non-manufacturing sector. However, it is not rejected for this later group for the treatment defined as the fraction of volume of work (in FTE) in STW in the cell (p-value of 0.1180) (results not displayed but available under request). In Figure C5a, when we remove the construction sector, the F joint significance test is rejected at the 5% level for Panel (a) but not rejected at the 5% level for panel (b) (p-value of 0.2678).

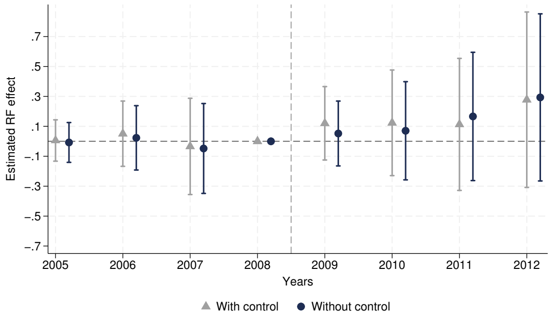
C.3 Reduced form

Figure C6: Placebo test (reduced form): employment outcomes.

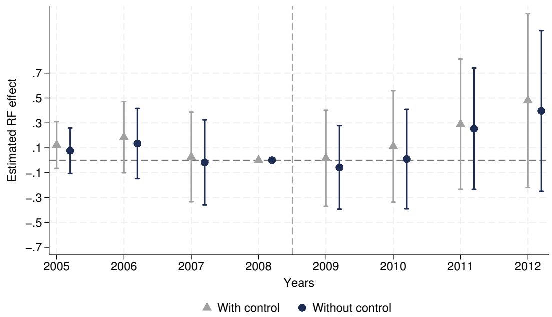
(a) Blue-collar jobs per firm - during-quarter (in logs)



(b) Blue-collar jobs per firm - end-of-quarter (in logs)



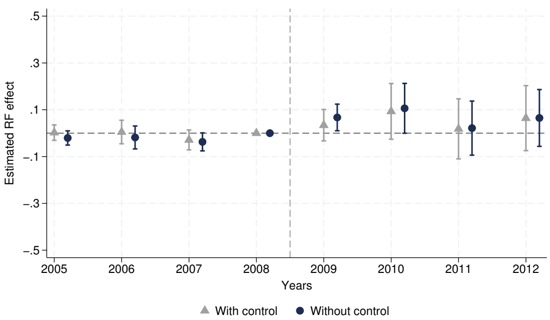
(c) Volume of blue-collar work per firm (in logs)



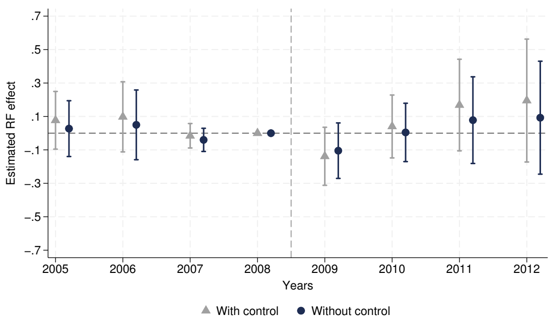
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. The instrument (Z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share fixed in 2004. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). All the effects can be interpreted as a semi-elasticities. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). The joint significance F-test for the pre-crisis period is not rejected at the 5% level for all the outcomes and all the specifications.

Figure C7: Placebo test (reduced form): wage outcomes.

(a) Outcome: gross wage rate (blue-collar workers)



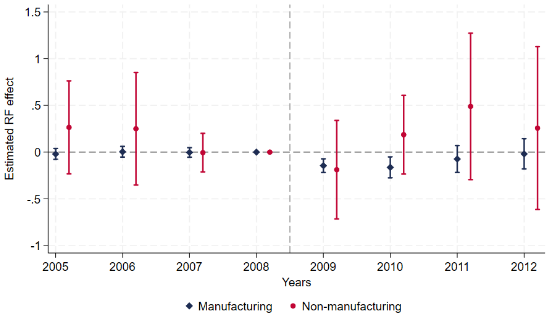
(b) Outcome: gross wage bill per blue-collar worker



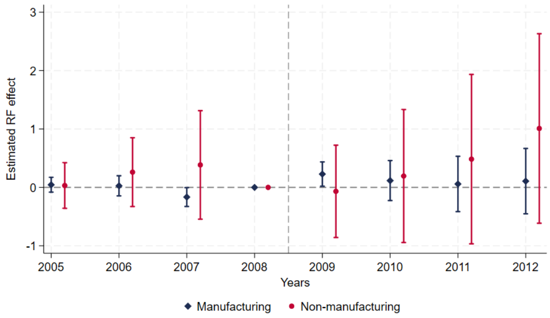
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. The instrument (Z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share fixed in 2004. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). The outcomes in Panel (a) and (b) are the fraction of firms in STW and the volume of work in FTE per (blue-collar) worker in logs, respectively. The effect in Panel (b) can be interpreted as a semi-elasticity. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). For the gross wage rate outcome and gross wage bill outcome, the joint significance F-test for the pre-crisis period is only rejected for the restricted model without control at the 5% level.

Figure C8: Placebo test (reduced form) by economic sector: employment outcomes.

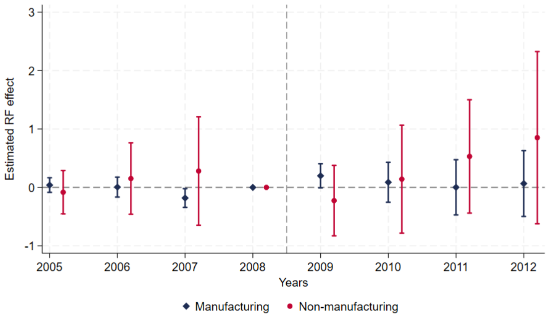
(a) Blue-collar volume of work per job (in logs)



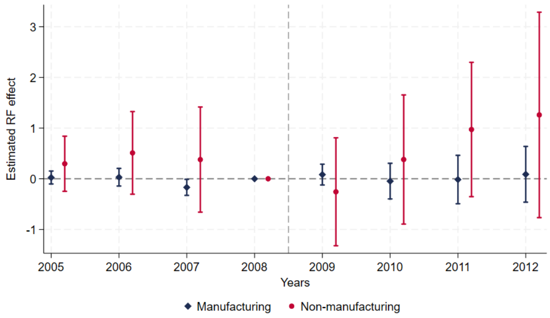
(b) Blue-collar jobs per firm - during-quarter (in logs)



(c) Blue-collar jobs per firm - end-of-quarter (in logs)



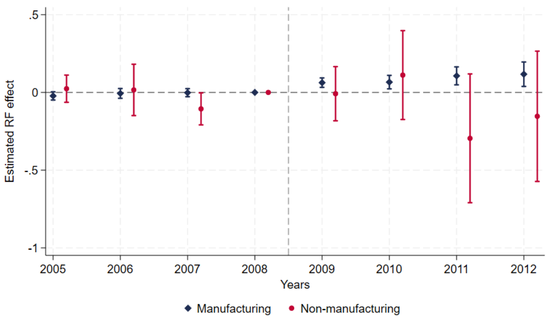
(d) Volume of blue-collar work per firm (in logs)



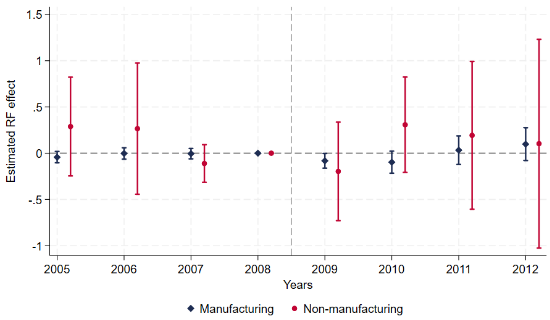
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the employment outcomes on the instrument defined in 2009. The instrument (Z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share in 2004. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). All the effects can be interpreted as a semi-elasticities. The model which does not control for log of (LOO) turnover in 2007 is plotted for both economic sectors, manufacturing (in blue) and non-manufacturing (in red). The joint significance F-test for the pre-crisis period is not rejected at the 5% level for all the outcomes and all the specifications.

Figure C9: Placebo test (reduced form) by economic sector: wage outcomes.

(a) Outcome: gross wage rate (blue-collar workers)



(b) Outcome: gross wage bill per blue-collar worker



Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the wage outcomes on the instrument defined in 2009. The instrument (Z_{2009}) is defined as the interaction of the shock in 2009 and the blue-collar worker share fixed in 2004. Dependent variables are defined in long differences with respect to the year 2004. We plot the effects relative to the impact in 2008 (the reference year). All the effects can be interpreted as a semi-elasticities. The model which does not control for log of (LOO) turnover in 2007 is plotted for both economic sectors, manufacturing (in blue) and non-manufacturing (in red). The joint significance F-test for the pre-crisis period is not rejected at the 5% level for all the outcomes and all the specifications.

D Additional Supplementary Material

Table D1: Firm-Level Average Treatment Effects of STW During the Great Recession (robust micro-level impact evaluations).

Citation	Country	Program	Sample	Econometric Design	Treatment	Effect period	STW treatment effect: Average Effect β (SE)	Average Effect: Interpretation	Heterogeneous Treatment effects: β (SE)	Heterogeneous Treatment effects: Interpretation
Kopp and Siegenthaler (2021)	Switzerland	Kurzabeit	Establishments with fewer than 500 employees	Event-study DiD / IV	STW take-up	Average between 2009 and 2014	-0.105 (0.024) 0.167 (0.053)	Net share of dismissed workers: -11% FTE employment per firm: +9% to 17%		
Cahuc et al. (2024)	France	Activité Partielle	Single-establishment firms with more than 4 employees.	IV approach (saturated weighted model)	STW take-up	2009	-0.226 0.328 (0.096) 0.102 (0.067) -0.027 (0.079)	Hours per capita -23% Headcount employment: +33% Total hours worked: no significant effect. Firm closure: no significant effect.	*Firms with the largest shocks in demand* -0.037 0.394 (0.109) 0.357 (0.080) -0.038 (0.056)	 Headcount employment: +45% Total hours worked: +40%
Giupponi and Landais (2023)	Italy	Casa Integrazione Guadagni Straordinaria	Firms with 5 to 25 FTE.	IV approach	STW take-up	Average between 2009 and 2014	-0.511 (0.036) 0.382 (0.036) 0.032 (0.028) -0.556 (0.046) 0.104 (0.038)	Hours of work per worker: -40% Headcount employment: + 45% Wage rate: no significant effect Wage bill per employee: - 45% Firm survival probability (one year after take-up): +10 p.p.		
Biancardi et al. (2025)	Italy	Casa Integrazione Guadagni Straordinaria	Sample of metal engineering firms with more than 5 employees.	IV approach	Hours of worked subsidized by STW	Average between 2009 and 2015	-0.020 (0.011) 0.138 (0.030) 0.117 (0.032) -0.055 (0.025)	Hours of work per worker: a 10% increase in STW hours reduces working hours by 0.2% Headcount employment: a 10% increase in STW hours increases total employment by 1.4% Total hours of work: a 10% increase in STW hours increases total working hours by 1.2% Wage bill per employee: a 10% increase in STW hours reduces average wage per employee by 0.5%		

Notes: This table summarizes the main effects of STW take-up at the firm level during the Great Recession. It includes robust causal impact evaluations from countries with long-established STW programs implemented well before the Great Recession.

Table D2: OLS estimates - employment outcomes

	Growth in volume of work per (BC) worker			Growth in (BC) jobs (end-of-period) per firm			Growth in volume of (BC) work per firm		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x I[t=2009]	−0.0727***	−0.0561***	−0.0528***	−0.0436***	0.0463	0.0392	−0.1240***	−0.0250	−0.0285
	(0.0066)	(0.0164)	(0.0160)	(0.0088)	(0.0335)	(0.0325)	(0.0112)	(0.0402)	(0.0394)
Within FE	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*
Trend FE	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Long differences	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Share	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Shock	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Region-specific trends	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Log of turnover in 2007	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*
Restricted model	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
R-squared	0.9697	0.7736	0.7722	0.9839	0.7910	0.7905	0.9801	0.8022	0.8014
Obs	12601.0000	9296.0000	9296.0000	12601.0000	9296.0000	9296.0000	12601.0000	9296.0000	9296.0000

Notes: Robust heteroskedastic standard errors in parentheses, clustered at the cell level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[a] The displayed estimates with grouped data do not control for the endogeneity of STW take-up, but only account for a rich set of controls. Regressions are weighted by $\frac{1}{\frac{1}{N_{it}}+\frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in t.
[b] The effects for the first and second outcome do not add up to the third since the second outcome is measured at the end of the quarter and not during the quarter as the rest of outcomes.

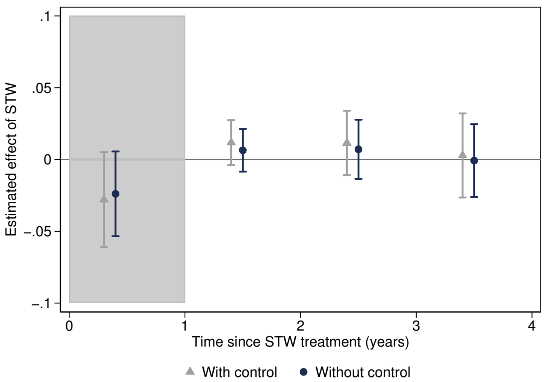
Table D3: OLS estimates - wage outcomes

	Gross Wage rate			Gross Wage bill per employee		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x I[t=2009]	0.0056	−0.0041	0.0032	0.0783***	−0.0495**	−0.0495**
	(0.0093)	(0.0095)	(0.0096)	(0.0043)	(0.0204)	(0.0204)
Trend FE	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Share	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Shock	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Region-specific trends	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Control: Log of turnover in 2007	*No*	*Yes*	*No*	*No*	*No*	*No*
Restricted model	*No*	*No*	*Yes*	*No*	*Yes*	*Yes*
R-squared	0.9785	0.8680	0.8628	0.9716	0.7883	0.7883
Obs	12601.0000	9296.0000	9296.0000	12601.0000	9296.0000	9296.0000

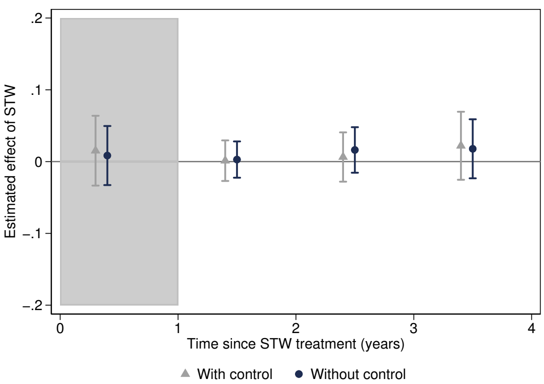
Notes: Robust heteroskedastic standard errors in parentheses, clustered at the cell level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[a] The displayed estimates with grouped data do not control for the endogeneity of STW take-up, but only account for a rich set of controls. Regressions are weighted by $\frac{1}{\frac{1}{N_{(ls)t}}+\frac{1}{N_{(ls)2004}}}$, where N_{it} is the number of firms in t.

Figure D1: Average Dynamic Treatment Effects (Intensive Margin Treatment): Employment Decomposition

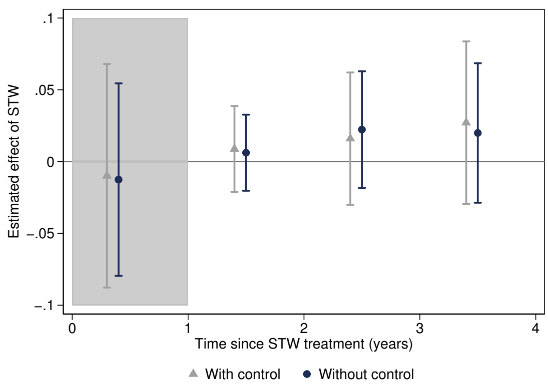
(a) FTE per blue-collar worker



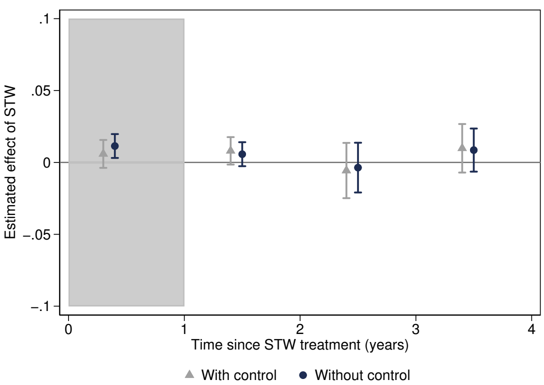
(b) Blue-collar (end-of-quarter) workers per firm



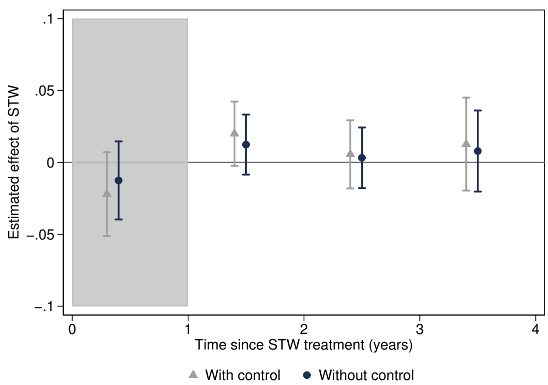
(c) FTE per firm



(d) Blue-collar wage rate



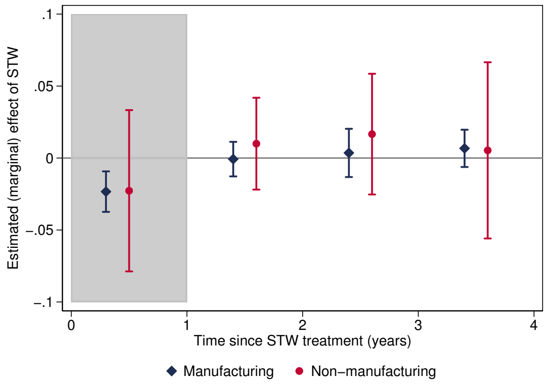
(e) Blue-collar wage bill per worker



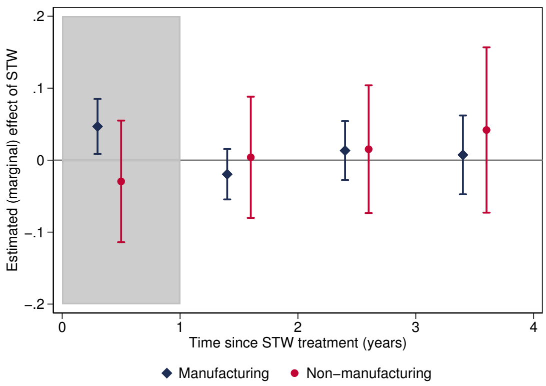
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \cdots, \hat{\alpha}_4\}$. Two specifications are displayed: (i) the most flexible model controlling for the log of turnover in the 2007 (triangle in gray), (ii) a flexible specification without controlling for the log of turnover in the 2007 (dot in blue). Panel (a) shows the effect of STW treatment measured as the average volume of work (in FTE) in STW, on FTE per blue-collar worker (our first order outcome). Panel (b) shows the effect of STW treatment on headcount employment per firm at the end-of-the-quarter. Panel (c) shows the effect of STW treatment on volume of work in FTE per firm. Panel (d) shows the effect of STW treatment on the blue-collar wage rate. Panel (e) shows the effect of STW treatment on the blue-collar wage bill per worker. The coefficients displayed in the following graph can be interpreted as semi-elasticities with respect to an increase of 1 FTE in STW per firm.

Figure D2: Dynamic Treatment Effects (Intensive Margin Treatment): Employment Decomposition by Sector

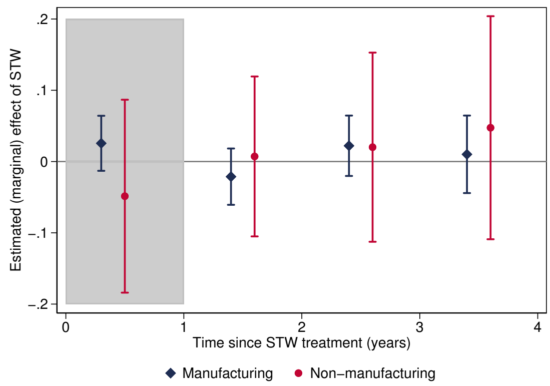
(a) FTE per blue-collar worker



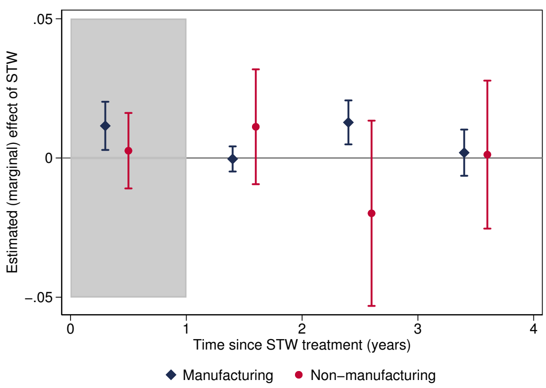
(b) Blue-collar (end-of-quarter) workers per firm



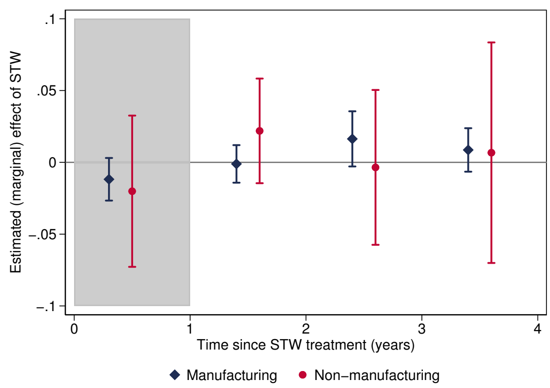
(c) FTE per firm



(d) Blue-collar wage rate



(e) Blue-collar wage bill per worker



Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \cdots, \hat{\alpha}_4\}$. Panel (a) shows the effect of STW treatment measured as the average volume of work (in FTE) in STW, on FTE per blue-collar worker (our first order outcome). Panel (b) shows the effect of STW treatment on headcount employment per firm at the end-of-the-quarter. Panel (c) shows the effect of STW treatment on volume of work in FTE per firm. Panel (d) shows the effect of STW treatment on the blue-collar wage rate. Panel (e) shows the effect of STW treatment on the blue-collar wage bill per worker. The coefficients displayed in the following graph can be interpreted as semi-elasticities with respect to an increase of 1 FTE in STW per firm.