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*To my husband, my newborn son, and my parents,
with gratitude and love.*

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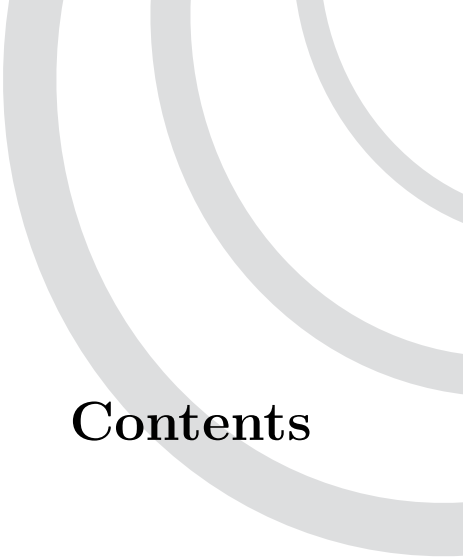
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General Introduction

Short-time work (STW), a social insurance program aimed at mitigating the adverse effects of economic shocks on employment, has been widely used during downturns. The Great Recession and the COVID-19 crisis are recent episodes in which firms and workers heavily relied on these programs. In Europe, STW has a long-standing history—not only during aggregate shocks but also, in some countries, in response to firm-specific economic fluctuations.

STW programs allow employers to temporarily reduce working hours while maintaining employment ties. Unlike traditional unemployment insurance (UI), which supports workers only after dismissal, STW preserves job matches and provides insurance value to both employers and employees. For firms, STW helps avoid the loss of firm-specific human capital and the costs associated with recruitment and training. For workers, it offers income replacement for reduced working hours while shielding them from long-term earnings losses and the re-employment risks that typically follow job separation. In the short term, STW supports firms facing liquidity constraints or temporary financial vulnerability by enabling temporary reductions in hours. In the medium term, it aims to facilitate a recovery in working hours while safeguarding employment relationships. Over the long run, the scheme contributes to stabilizing or increasing both working hours and employment, ultimately strengthening labor market resilience (Kopp and Siegenthaler, 2021).

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Despite its societal value, STW also generates social costs, particularly when program design features encourage distortive firm behaviors. These include moral hazard practices such as manipulating information to gain eligibility, reducing hours excessively despite stable demand, or enrolling workers without actually cutting hours. Poor targeting—especially failure to focus on temporarily vulnerable jobs with recovery potential—can increase program costs. In some cases, STW protects jobs that would have survived without support in the short run, leading to deadweight losses (Cahuc et al., 2024); in others, it may sustain non-viable jobs over time, obstructing efficient reallocation and reducing aggregate productivity (Giupponi and Landais, 2023).

In Belgium, STW has played a central role in employment preservation during economic crises. The country recorded the highest STW take-up rate in Europe during the Great Recession, with 5.6% of salaried employees enrolled (OECD, 2010),¹ and reached similarly high participation levels during the COVID-19 crisis, with 30% of employees enrolled in April 2020,² making it one of the most generous schemes in terms of coverage and benefits (Calligaris et al., 2023). Despite its scale and political salience, however, the full impact of STW—particularly on employment and firm survival—remains underexplored in the context of aggregate economic shocks.

This thesis contributes to filling this gap by examining the effectiveness of STW programs in preserving employment and supporting firm survival in Belgium. The central research question is: to what extent do STW programs preserve employment and prevent firm exit during aggregate economic shocks? Two related questions guide the analysis. The first is whether STW improves firm-level outcomes, such as employment and survival, during the Great Recession and the COVID-19 pandemic. The second concerns the heterogeneity of these effects across different types of firms and workers, shedding light on the potential benefits and costs associated with the policy.

To answer these questions, the thesis combines institutional analysis, a critical review of the literature, and two causal empirical evaluations using administrative data. A unifying element across the three chapters is a focus on

¹Compared to the OECD average of 0.5%.

²The OECD average during the COVID-19 period was 20%.

causal identification.

Chapter 1, co-authored with Muriel Dejemepe and Giulia Tarullo, critically reviews STW programs in Europe, highlighting their complementarities with unemployment insurance, the risks of moral hazard, and the importance of design features such as monitoring, co-insurance, and experience rating. The review emphasizes studies that provide causal estimates of STW's effects, clarifying their strengths and limitations, and sets the stage for the empirical contributions of the following chapters.

Chapter 2, co-authored with Bart Cockx and Gert Bijmens, evaluates the impact of STW in Belgium during the Great Recession. It exploits longstanding eligibility rules that favored blue-collar workers, combined with sectoral turnover shocks, to construct an instrumental variable (IV) strategy predicting STW uptake. Using administrative grouped data from 2004–2012, the analysis shows that STW significantly reduced working hours but had no average effect on employment or wages. However, small manufacturing firms—those most exposed to global supply disruptions—experienced short-term employment gains. This chapter responds directly to the first research question by estimating firm-level employment effects in the context of the Great Recession, while also highlighting heterogeneity across firm types.

Chapter 3 evaluates the impact of Belgium's simplified STW scheme introduced at the start of the COVID-19 crisis. A temporary reform in September 2020 created an eligibility threshold that is exploited using a Fuzzy Regression Discontinuity Design (RDD). The analysis uses a matched quarterly dataset combining STW use, firm financial indicators, and employment outcomes for firms close to the threshold. Results show that the simplified scheme substantially increased take-up and preserved jobs in firms partially affected by lockdowns, with effects lasting up to two years. These findings align with the literature on STW's effectiveness during large transitory shocks (Cahuc et al., 2024). White-collar-intensive firms responded strongly at the extensive margin, benefiting from relaxed administrative rules, while blue-collar-intensive firms—already enjoying easier access—responded mainly at the intensive margin, raising concerns about overuse. This pattern is consistent with Albertini et al. (2022), who document excessive reductions in hours with limited employment gains under a similarly generous STW scheme in France, especially among low-skill workers.

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The analysis also shows that lockdown-unaffected firms experienced no significant employment effects, implying that STW often supported jobs not at risk. Fully blue-collar firms in sectors such as construction and transportation appear responsible for much of this deadweight loss. By contrast, firms with more white-collar workers—where job-specific human capital and future hiring or training costs are higher—benefited from STW in the short and medium term, even without a direct lockdown shock. Descriptive statistics further suggest that these firms faced the tightest liquidity constraints. Overall, the findings indicate that STW’s effectiveness depends not only on the severity of shocks but also on the value of preserving firm–worker matches (Cahuc, 2024; Hallmann, 2023). This chapter complements the Great Recession analysis by addressing both research questions in the pandemic context and by documenting clear differences across firm types and workforce composition.

Chapters 2 and 3 focus on two major periods of economic disruption: the Great Recession and the COVID-19 pandemic. The nature and scope of these shocks differed substantially. The COVID-19 crisis was a broad, nearly exogenous shock, with lockdowns forcing widespread reductions in hours. In contrast, the Great Recession was financially driven and more sector-specific, primarily affecting manufacturing firms through global value chains. These differences suggest STW may have been more effective during the pandemic, particularly in preserving jobs at risk of immediate destruction (Cahuc, 2024).

Nevertheless, the generosity and simplification of STW schemes during the pandemic also posed challenges to cost-effectiveness. High take-up rates, driven by relaxed eligibility criteria, increased the risk of subsidizing jobs not at risk and encouraged excessive reductions in hours (Burdett and Wright, 1989; Albertini et al., 2022). These contrasting experiences offer valuable insights for refining STW design to better address future crises.

Taken together, the three chapters provide a comprehensive analysis of STW as a labor market policy. Chapter 1 situates STW within the broader institutional and theoretical framework, identifying the mechanisms through which it can preserve jobs and the risks that limit its effectiveness. Chapter 2 evaluates the effects of STW in Belgium during the Great Recession using an instrumental variable approach, while Chapter 3 studies the COVID-19 period with a regression discontinuity design. Although the chapters rely on different

methods and contexts, they are connected by a consistent concern for causal inference and by their contribution to understanding the trade-offs between job preservation, program costs, and labor market efficiency. Together, they provide new evidence on when and how STW is effective, where it generates inefficiencies, and what these insights imply for the design of crisis-response labor market policies.

Theory and Empirics of Short-Time Work: A Review

This chapter is co-authored with Muriel Dejemeppe and Giulia Tarullo.

Following widespread use during the COVID-19 crisis, short-time work (STW) policies have gained renewed attention. This paper critically reviews STW systems in Europe, focusing on their objectives, characteristics, and inefficiencies, such as excessive use and hindered worker reallocation. We also examine key contributions of STW impact evaluations. Finally, we discuss potential improvements to STW design features and identify avenues for future research on their long-term effects, labor market reallocation, and the cost-effectiveness of policy design. A key takeaway is the need for a careful balance between monitoring and financial disincentives to enhance the effectiveness of STW while minimizing negative consequences.

2.1 Introduction

Short-time work (STW) is a government program that subsidizes short-term reductions in employee working hours in response to temporary shocks. Unlike traditional unemployment insurance (UI), STW maintains employment relationships, avoiding excessive layoffs.¹ By preserving job relationships, STW protects firms and workers from bankruptcy and layoffs while generating broader economic benefits. Specifically, STW reduces the pool of unemployed workers, easing the fiscal burden on UI systems, which can be particularly relevant during recessions. In addition, STW stabilizes aggregate demand (Gehrke and Dengler, 2022) and has the potential to mitigate income inequalities (Christl et al., 2022), an effect that is particularly pronounced during aggregate economic shocks, when lower-income workers are likely to be most affected.

After gaining notoriety during the Great Recession, STW received renewed attention during the global COVID-19 crisis, a period that revealed the highest uptake in the history of many European countries. In April 2020, Germany, Italy, France, and Belgium had approximately 15%, 30%, 35%, and 30% of their salaried employment enrolled in STW schemes, respectively (Hijzen and Salvatori, 2022). The COVID-19 pandemic led to sharp, unexpected drops in product and labor demand due to mandated restrictions. The risk of high unemployment rates, along with the risk of firms losing valuable human capital and being forced into bankruptcy, triggered the need for governments to invest massively in STW schemes.

¹These programs are also referred to as short-time compensation schemes. In some European countries, such as Belgium, France, and The Netherlands, STW schemes are also known as “temporary unemployment” or “partial unemployment”. They should not be confused with temporary layoffs, which are common in the United States and Canada and allow the employer to temporarily suspend the employment relationship. In principle, temporary layoff policies enable dismissed workers to be rehired by their original employers (Feldstein, 1976). Temporary layoffs are distinct from STW schemes since the latter maintains the employment relationship unchanged. In addition, STW programs are distinct from furlough schemes, such as the one used in the United Kingdom, which allow for a total suspension of activity. While STW schemes may in some cases allow for a complete suspension of activity, they differ from furloughs in their ability to accommodate partial reductions in activity while maintaining the employment relationship.

Although STW has an important value to society, it also entails social costs associated with distortive behaviors among participating firms, often driven by program-specific design features. Such moral hazard behaviors include manipulating information to qualify for the program, excessively reducing working hours despite not facing financial distress, or enrolling workers without implementing any actual reductions in working hours. Ineffective targeting of vulnerable jobs that have recovery potential often increases the social cost of STW. For instance, STW may protect job matches that are likely to survive even in the absence of the program, resulting in deadweight losses. Moreover, the program’s value diminishes if it sustains non-viable jobs in the long run, hindering the efficient reallocation of workers to more productive firms and sectors and reducing aggregate productivity.

These issues were especially evident during COVID-19, when STW spending surged. As many countries rapidly simplified and expanded eligibility criteria of these programs, the risk of imperfectly targeting jobs genuinely at risk of destruction increased. At the same time, the high uncertainty surrounding this period raised concerns about the prolonged duration of several generous STW schemes in Europe. Policymakers questioned STW’s role in exacerbating labor shortages in certain sectors by delaying labor reallocation. As economies transitioned from crisis management to recovery, the underlying principles of these policies came under vigorous debate, particularly in relation to the required policy adjustments needed to navigate the recovery phase.

In this paper, we take stock of the STW policy instrument and provide a critical review of its implementation in Europe, focusing on countries with stringent employment protection regulations, namely Belgium, France, Germany, and Italy.² Our contribution is threefold. First, while previous surveys (Giupponi et al., 2022; Boeri and Cahuc, 2023) have compared STW with unemployment benefits in other contexts, such as the U.S. and the UK, and examined their different roles in providing insurance against negative economic fluctuations, we instead focus on the key features and objectives of

²According to OECD Employment Protection statistics, these countries are above the OECD average in terms of the strictness of the employment protection index for individual and collective dismissals in regular contracts (OECD, 2021).

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STW schemes in European labor markets. We examine how STW complements UI systems and interacts with existing labor market institutions (LMIs).³ Second, although other surveys discuss the benefits and costs of STW (Cahuc, 2024; Gehrke, 2024), we provide a deeper analysis of how STW generates value for society and offer a thorough discussion of the rationale behind firms' behavioral responses to STW schemes, distinguishing between ex-ante and ex-post moral hazard. Finally, we examine two critical design elements: monitoring mechanisms and financial disincentives, such as co-insurance and experience rating premia. Specifically, we explore their role in shaping the effectiveness of STW, drawing lessons from both the Great Recession and the COVID-19 crisis. These design features, which have not been thoroughly addressed in previous surveys, are instrumental in curbing misuse, containing fiscal costs, and mitigating STW's impact on labor reallocation.

The structure of the paper is as follows. Section 2.2 pinpoints the main objectives and general characteristics of STW programs, with a focus on the insurance value of such policies. This is fundamental to understand how STW serves as an insurance device for firms and workers. Section 2.3 summarizes lessons from recent STW impact evaluations during the Great Recession and COVID-19. We focus on differences in dynamic effects as well as heterogeneous effects, derived from the types of firms treated by the programs. Section 2.4 analyzes recent theoretical and empirical contributions to provide an overview of the conditions that determine the efficiency of STW policies (e.g., finding a balance between effective insurance provision to firms—and therefore workers—and minimizing distortion in labor demand choices). Section 2.5 then provides a critical summary of two design refinements necessary to improve STW effectiveness: monitoring and financial disincentives. Section 2.6 concludes with key policy lessons.

2.2 Insurance value of STW programs

This section outlines the objectives of STW systems, which are designed to support labor hoarding and prevent the destruction of job relationships

³For example, in the presence of hours and wage bargaining rigidities.

during temporary economic shocks. Since STW aims at preserving employment relationships that remain valuable to both firms and workers but are only temporarily unprofitable, we underscore STW insurance value to firms, workers, and the broader economy. We then examine how the European UI system, combined with financial cash frictions during economic crises, heightens the risk of excessive layoffs. Additionally, we discuss how binding institutional wage and working-hour agreements in many European countries limit firms' ability to adjust working hours in the absence of STW schemes. Finally, we define the core characteristics of STW programs—namely, the extent of government financial support and their transitory, yet expedient nature.

2.2.1 STW benefits to firms, workers, and the economy

Unlike UI, which supports dismissed workers, STW preserves job relationships, providing insurance value to both firms and employees. While UI compensates income loss after a layoff, STW prevents layoffs altogether, sparing both parties from associated costs. For firms, job separations result in the loss of firm-specific human capital and the need for re-hiring, along with associated costs, when the economy recovers. For workers, STW offers both financial support—via government subsidies for hours not worked—and protection against unemployment's long-term earnings losses and re-employment difficulties.⁴ Beyond financial effects, job loss can also harm psychological well-being and economic security, with such consequences worsening during downturns (DryDakis, 2021), further underscoring STW's insurance value.

At the aggregate level, STW generates positive effects through two channels: (i) net fiscal savings via reduced UI take-up, (ii) stabilization of consumption and demand, and (iii) spillover effects for non-users. First, by limiting layoffs, STW reduces UI claims, easing the fiscal burden from UI—a link explored in Section 2.4. Second, by mitigating job loss risk and income reductions, STW stabilizes demand during recessions (Jaravel, 2022). Faced with unemployment risk and borrowing constraints, workers may cut consumption, triggering a

⁴The worker receives STW allowances, which are computed as a rate of the gross wage for the hours not worked. This rate is referred to as the replacement rate.

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contractionary spiral. STW dampens this effect by reducing the incentive for precautionary savings (Gehrke and Dengler, 2022). Finally, STW benefits non-users by lowering their expected future risk of job loss (Cahuc, 2024) as firms internalize the availability of STW for future shocks.

2.2.2 Rationale for STW inclusion in the social insurance system

Several arguments highlight why layoffs can become inefficiently high during an economic shock without STW (Cahuc and Carcillo, 2011). First, when faced with a temporary economic downturn, firms encounter a trade-off between optimizing their long-term labor force needs—by retaining more workers than strictly necessary during the crisis—and avoiding short-term cash-flow shortages caused by ongoing labor costs. Short-term financial constraints, such as limited cash flow, and the worsening of financial market imperfections during economic shocks, hinder firms’ ability to engage in labor hoarding, leading to increased job destruction (Giroud and Mueller, 2017) and lower firm survival during downturns. In reality, when firms face unexpected declines in sales and profits, they may adjust their output prices or reduce production costs. To alleviate labor expenses, they may cut the wage bill, which can be achieved by trimming employment, decreasing working hours per employee, or lowering the wage rate. Nevertheless, the rigid LMIs in European economies make the last two responses unlikely, often leading to excessive job separations.

In most European countries, collective agreements regulate the working conditions of both unionized and non-unionized workers (Villanueva et al., 2022). These agreements, which set provisions for wages (including minimum thresholds), hours, and employment conditions, are typically negotiated at the national or industry level. Such arrangements impede the possibility of firm-worker negotiations on wages and working hours in response to negative economic shocks (Boeri et al., 2011), thus limiting the ability to reallocate job-match surpluses between employers and employees (Acemoglu, 1995; Hall and Lazear, 1984; Jäger et al., 2023). As a consequence, the downward rigidity of wages and hours can result in the destruction of job matches that could still offer a positive surplus for workers (Giupponi and Landais, 2023).

2.2. Insurance value of STW programs

In this context, STW provides firms with the flexibility needed to preserve job relationships that maintain positive surpluses. However, in most European countries, pay cuts are not a feasible alternative to layoffs, not only because of the constraints imposed by the industrial relations system, but also due to substantial size of the pay cut required to avoid layoffs. Such cuts risk harming employee morale and may incentivize workers to leave their jobs voluntarily (Bertheau et al., 2025; Davis and Krolikowski, 2025). Germany stands as an exception, where flexible wage bargaining between firms and workers can lead to mutually agreed-upon downward wage adjustments following negative shocks (Brinkmann et al., 2024).

Moreover, in European labor markets, UI programs offer firms little financial incentive to internalize the broader social costs of layoffs, such as increased government spending, lower tax revenues, and workers' long-term unemployment scarring (Boeri and Cahuc, 2023). An experience rating (ER) system can address this by linking firms' UI contributions to their layoff histories, thus incentivizing them to limit excessive dismissals (Blanchard and Tirole, 2008; Cahuc and Malherbet, 2004; Feldstein, 1976). In theory, full ER—where firms fully cover the social costs of layoffs—would eliminate excessive dismissals. However, during downturns, financial constraints may make full ER suboptimal, as it could worsen firms' financial distress, reducing hiring (Johnston, 2021) and increasing insolvency risk. Given these limitations, UI alone may not be sufficient to curb layoffs, underscoring STW's role in offering firms greater flexibility.

In contrast, strict employment protection laws (EPL) can deter excessive layoffs by restricting and discouraging dismissals. However, maintaining stringent EPL requirements during temporary economic shocks exacerbates the financial strain of firms affected by the downturn (Boeri and Cahuc, 2023). In such contexts, STW provides the necessary flexibility to manage labor costs without the negative impacts of rigid EPL.

2.2.3 Primary features of STW programs

Three underlying principles form the core of STW programs—temporariness, expediency, and government subsidization—, which are effectively reinforced

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through the program’s design features.

Internationally, STW programs share common design features, such as eligibility criteria for workers and firms, follow-up conditions, varying working time reduction schemes, and different co-financing levels by participating firms and workers (Cahuc and Carcillo, 2011; Giupponi et al., 2022).⁵ Workers, whether on open-ended or fixed-term contracts, must meet minimum past employment requirements, while firms must demonstrate a temporary decline in revenues or demand to qualify, driven by cyclical economic downturns or, in a few countries, by anticipated seasonal fluctuations. Once admitted, workers may be encouraged to accept job offers within six months, while firms are incentivized to provide training. Firms also have flexibility in reducing working hours, either through full suspension for limited periods or partial hour reductions, with varying maximum entitlement durations for STW. Workers receive compensation for both hours worked and lost earnings due to reduced hours. In most countries, the replacement rate for STW allowances exceeds that of UI, with lost wages reimbursed up to a predefined cap. Firms often share costs through co-insurance mechanisms—such as topping up worker subsidies for unworked hours—or experience-rated contributions based on past STW use.

The temporary nature of STW stems from its core function: subsidizing labor hoarding, which is itself a transitory phenomenon. Firms retain workers during downturns in anticipation that the job match will become profitable again once business conditions improve (Cahuc et al., 2014). Additionally, limiting the duration of STW support helps mitigate workers’ incentives to remain indefinitely in subsidized employment. Since STW allowances only partially compensate for lost income, prolonged reliance on the scheme may induce liquidity constraints for workers, encouraging them to exit and seek employment in less affected firms or sectors. Restricting the program’s duration also curtails potential adverse effects on labor reallocation, which is among the social costs of STW discussed in Section 2.4. However, economic theory suggests that the duration of STW support should be flexible when

⁵A summary of STW design features in key STW-user countries (e.g., Belgium, France, Germany, and Italy) during the Great Recession and COVID-19 is provided in Table A.1. of Bermúdez et al. (2023).

2.2. Insurance value of STW programs

economic shocks persist longer than initially anticipated (Balleer et al., 2020). Indeed, firms' ability to retain their workforce depends on the expectation that STW will provide coverage for the entire period of economic contraction, rather than being constrained by a pre-determined maximum duration that may not align with the actual length of the downturn. The COVID-19 crisis illustrated this rationale, as many countries extended the maximum potential benefit duration of STW to reassure firms of continued support throughout the pandemic. Belgium and France even introduced adaptive STW regimes, where eligibility duration was initially tied to a fixed calendar date but was later extended based on epidemiological indicators.

STW serves as an expedient policy instrument by swiftly delivering support to firms and workers in the face of economic shocks. Its streamlined application, approval, and payment processes ensure the rapid reduction of labor costs for firms while providing timely allowances to workers. Alternative liquidity-support measures, such as temporary loans, often prove less effective due to lengthier administrative procedures and delayed disbursements (Boeri and Cahuc, 2023). The speed of STW implementation is crucial for achieving its objectives and preventing unnecessary job losses. From a firm's perspective, those experiencing acute liquidity constraints may be unable to retain workers if financial support is not provided immediately. Similarly, for workers, delays in STW payments undermine the program's ability to alleviate household financial distress. This lack of expediency increases the likelihood that workers will opt out of the program in search of alternative employment, as discussed earlier. Recognizing these risks, governments across countries temporarily eased or suspended STW eligibility requirements during the height of the pandemic to ensure rapid support. This administrative simplification, unprecedented before COVID-19, was necessitated by the sudden and sweeping nature of lockdown measures. However, while granting firms maximum flexibility in reducing working hours, such broad relaxations raised concerns about potential misuse—specifically, that firms might claim STW benefits without implementing actual reductions in working time (see Section 2.4). As discussed in Section 2.5, monitoring remains a critical challenge, as ex-post monitoring by social security administrations is often limited due to the large scale of STW take-up (Boeri and Cahuc, 2023).

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As discussed above, while governments subsidize STW by providing allowances to workers for unworked hours, firms also bear certain costs. These arise when firms co-finance STW allowances alongside the government or when unions negotiate additional employer contributions to supplement state-funded payments. The primary objective of government subsidization is to ease the financial strain of labor hoarding for firms facing short-term liquidity constraints. However, co-financing mechanisms are designed to discourage firms from enrolling workers whose jobs are not at risk of redundancy and to mitigate the unintended reliance on subsidized hour reductions (Burdett and Wright, 1989), particularly in cases where monitoring actual working hours is challenging. As explored in Section 2.4, one of the key social costs of STW insurance stems from its potential to incentivize excessive reductions in working hours. The partial nature of government subsidization directly addresses this concern by ensuring that firms retain some financial responsibility. Nevertheless, during unanticipated aggregate shocks, firms typically face greater financial market frictions and more constrained access to credit, necessitating stronger government support. Reflecting this reality, in all countries with STW schemes, firms bore no costs during the initial waves of the COVID-19 crisis. Employer contributions were only gradually reinstated as part of the phased withdrawal from STW reliance.

2.3 Effects of STW on firm and worker outcomes

This section reviews key findings on the effects of STW on firm and worker outcomes. From the firm's perspective, STW effectively enable labor hoarding, thus benefiting firm survival, particularly for firms facing liquidity constraints. Moreover, because STW helps protect firms' accumulated human capital, it may indirectly influence future investment decisions in both human and physical capital. STW can thus have short- and long-term impacts, not only on employment but also on firm survival, profitability, and productivity. From the workers' perspective, STW provides insurance against i) income loss caused by reduced working hours and ii) the negative scarring effects of unemployment. This policy can therefore influence the worker's trajectory in the labor market, both in the short and long term.

2.3. Effects of STW on firm and worker outcomes

Earlier studies assess the effects of STW through cross-country macroeconomic evaluations during the Great Recession. Overall, these studies find that STW helped stabilize aggregate unemployment and employment during a temporary downturn (Boeri et al., 2011; Cahuc and Carcillo, 2011; Hijzen and Venn, 2011; Hijzen and Martin, 2013; Brey and Hertweck, 2020). In some cases, they also consider the indirect positive stabilizing effect of STW programs on aggregate demand (Gehrke and Dengler, 2022). Although cross-country analyses partially address the selection bias that arises at the firm level—since firms do not take up STW randomly but often due to financial difficulties—they are limited in their ability to account for country-specific responses to confounding factors, including concurrent policies or heterogeneous crisis impacts, which may jointly affect STW take-up and employment.

In contrast, microeconometric studies, although limited in capturing general equilibrium effects, focus on firm-level variation within a fixed institutional setting. By employing microeconometric techniques, these studies aim to identify causal effects of STW on firms and workers, provided that selection bias is adequately addressed. Early evaluations provide mixed conclusions, heavily influenced by the methodological approach used to identify causal effects (Bellmann and Gerner, 2011; Calavrezo et al., 2010; Tracey and Polachek, 2020). Estimating causal effects with firm-level or worker-level data is particularly challenging—not only due to data limitations, which can be sensitive and not rapidly available, but also because of selection bias, as STW participation is an endogenous firm decision. Firms opting into STW likely differ from non-users in both observable and unobservable characteristics, which may be correlated with firm outcomes. In this section, we review robust microeconometric evaluations that address selection concerns.

2.3.1 Effects of STW on the economic behavior of firms

This section examines the impact of STW on firm outcomes, including employment, survival, productivity, and profitability. The effects depend on program design, targeting features, and the nature of economic shocks. We distinguish between short-term (during STW) and medium-to-long-term

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(post-treatment) effects, and discuss heterogeneous effects based on firm characteristics.

Employment effects

A growing body of literature evaluates the impact of STW on employment using the framework of search and matching models calibrated with firm-level data (Albertini et al., 2022; Balleer et al., 2016; Gehrke et al., 2019; Niedermayer and Tilly, 2016; Cooper et al., 2017; Gehrke and Hochmuth, 2021). While not causal, these studies provide valuable insights into the underlying mechanisms. Most focus on Germany’s Kurzarbeit program during the Great Recession and consistently find a positive employment effect. However, some studies (Balleer et al., 2016; Gehrke and Hochmuth, 2021) suggest that, even when STW saves jobs, discretionary extensions during deep economic crises (i.e., increasing the generosity of STW programs) can lead to deadweight effects, subsidizing workers who are at no risk of job loss. Albertini et al. (2022) report similar findings for France during COVID-19, where STW prevented job losses but also protected jobs that would have survived without it.

The underlying mechanisms behind the short- and long-term STW employment effects are as follows. Firms respond to temporary productivity shocks by adjusting labor demand at the intensive margin via STW, preserving jobs that might otherwise be lost. In the short term, STW has a positive effect on employment by reducing hours per worker while preserving headcount employment (Cahuc et al., 2021). In the medium-to-long run, employment gains are likely to persist as hours of work per worker recover, given that firms are intended to use the scheme temporarily. This contrasts with the scenario without the program, where firms face separation costs related to hiring and training, as well as other recruitment frictions that hinder the recovery of pre-crisis employment levels.

However, STW may create deadweight losses by subsidizing firms that would not lay off workers even in the absence of the program. In the short term, it may reduce hours without affecting headcount employment in firms facing mild demand shocks, which would have retained workers regardless.

2.3. Effects of STW on firm and worker outcomes

Moreover, STW may delay the exit of non-viable jobs, potentially hindering long-term employment growth in both treated and untreated firms. Appendix Table A.1 summarizes these expected firm-level employment responses.

Recent micro-econometric evaluations for the Great Recession provide robust insights⁶ (see Table A.2) using credible econometric strategies that exploit exogenous variation arising from STW institutional features. Cahuc et al. (2021) evaluate STW in France, using single-establishment firms and an Instrumental Variable (IV) approach. They leverage variation in STW take-up based on administrative discretion in the approval rate of STW applications across regions. In the short term, they find a 28% drop in hours per worker but no employment gains, except for firms facing large demand shocks, highlighting strong heterogeneous responses.

Giupponi and Landais (2023) examine Italian firms with 15–25 full-time equivalents (FTEs), using an IV approach that exploits quasi-exogenous variation in firm eligibility. They find that STW led to a 40% reduction in hours per worker, offset by a 45% increase in headcount employment, although these effects dissipated rapidly after STW take-up. Two key characteristics of this setting are noteworthy. First, in Italy, the STW program targeted a broad spectrum of firms, including those facing bankruptcy, restructuring, reorganization, or severe demand shocks. This broad targeting, as suggested by Kopp and Siegenthaler (2021), may have increased the program’s susceptibility to protecting low-productivity matches that are non-viable in the medium-to-long run. Second, the combination of the Great Recession followed by a double-dip financial crisis in Italy made the economic shock more persistent than in other European countries, motivating a prolonged use of the program. This extended use, particularly concentrated among non-viable, low-productivity job matches, had implications for labor reallocation.⁷

Kopp and Siegenthaler (2021) analyze the impact of STW during the Great

⁶These studies rely on large administrative datasets with credible identification strategies.

⁷See further discussion in Section 2.4.2.

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Recession in Switzerland using an event study difference-in-difference (DID) design. They compare changes in the outcomes of successful and unsuccessful applicants, examining both average and dynamic effects around the event of application to the program. Their findings indicate that STW increased FTE employment per firm not only in the short term but also over the long run (4.5 years post-application), by 9%–17%. Switzerland’s STW program features stricter monitoring and firm selection, with higher denial rates compared to other European programs. It primarily targets firms facing temporary, cyclical shocks, rather than structural problems or seasonal fluctuations, reducing the likelihood of deadweight losses.⁸ Additionally, Switzerland’s recession, unlike Italy’s, was sharp but short-lived, allowing firms to recover more quickly following STW use.

Another source of heterogeneity in STW effects highlighted in the literature relates to firm characteristics such as liquidity and productivity levels. Firms with lower liquidity or credit constraints tend to cut more jobs in response to large temporary negative shocks (Giroud and Mueller, 2017; Melcangi, 2024), but they do not necessarily have non-viable jobs in the long run. STW helps these firms overcome liquidity constraints, enabling workers to return to pre-crisis employment levels once normal activity resumes, thereby fostering firm growth. Consistently, Giupponi and Landais (2023) find a greater short-term elasticity of employment with respect to hour reductions due to STW use among low-liquidity firms compared to firms with high-liquidity in the Italian scheme. Furthermore, they find that firms selecting into STW are generally low-productivity firms. However, the largest short- and medium-to-long-term employment gains per hour of STW reduction occur among firms in the highest productivity quartiles. In contrast, firms with very low productivity levels experience no significant employment gains from STW-induced hour reductions.

The COVID-19 period also offers valuable lessons (see Table A.2). This period

⁸Other programs in Europe, such as Germany’s “Kurzarbeit,” do not explicitly exclude the treatment of firms with seasonal fluctuations in demand, especially in sectors like construction (Giupponi and Landais, 2023). The program in France (“Chômage Partiel”) also does not exclude the treatment of firms with seasonal fluctuations (Cahuc and Nevoux, 2017).

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saw record take-up, driven by expanded and simplified access, leading to a lack of exogenous variation in firm participation, which complicates causal identification. Evidence from this period extends beyond STW programs to furlough schemes implemented in several countries to fully reduce working time. Some studies show that these job retention programs had positive effects on firm employment without raising concerns about their overall effectiveness (Bennedsen et al., 2023; Benkovskis et al., 2023; Montenegro and Hijzen, 2024; Meriküll and Paulus, 2023). Other studies, on the other hand, show that these positive effects on employment come at the cost of large deadweight losses due to firms’ behavioral responses (Brinkmann et al., 2024).

Bennedsen et al. (2023) evaluate the effectiveness of a furlough program in Denmark during the pandemic, using a selection-on-observables model with managers’ predicted layoffs and furlough use in the scenario of no STW take-up—collected before program implementation—as counterfactuals. Their analysis relies on a novel survey conducted at the outbreak of the crisis, matched with administrative data. They find that firms in furlough reduced their layoff rate by about 24 percentage points (p.p.) relative to firms in the counterfactual group, which in the absence of aid would have laid off workers during the first year of the pandemic. Benkovskis et al. (2023) evaluate a furlough scheme in Latvia using a local projection DID design. They find positive effects of STW on headcount employment, both in the short term and consistently through the end of 2021, for first-wave users relative to pre-pandemic levels and compared to non-users. Montenegro and Hijzen (2024) study Spain’s furlough program using a regression discontinuity design (RDD) that exploits variation in the program’s generosity, showing that each worker on STW resulted in one job saved, suggesting no deadweight effects in the short term. Meriküll and Paulus (2023) evaluate the STW program introduced during the COVID-19 period in Estonia.⁹ They construct firm counterfactuals using a propensity score matching approach with firms that did not take up the program but experienced a similar turnover decline as those that did. They find that firms in STW reduced layoffs relative to their counterfactual, and that about 20% of jobs supported by the program were saved in the short term.

⁹Although its official name refers to a temporary wage subsidy, it follows the structure of a STW program, as it is conditional on a reduction in working time.

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However, some studies report small or insignificant employment effects, likely due to deadweight losses. Brinkmann et al. (2024) use two RDD approaches, leveraging two sources of exogenous variation prior to and during the pandemic. Although not for the COVID-19 period, they exploit exogenous firm-level variation from the 2012 policy extension of the maximum duration of STW in a given spell. They find no significant employment effects at the firm level, with firms facing a shorter maximum duration adjusting wages instead of employment. These findings align with their worker-level analysis, which exploits variation in STW eligibility at the statutory retirement age during COVID-19, finding no difference in employment retention between eligible and ineligible workers at the same firm.

Importantly, their findings highlight wage negotiations as an alternative mechanism for preserving employment in Germany. Firms with shorter potential STW durations lowered wages more and preserved more jobs than those eligible for longer potential STW durations. Moreover, the absence of effects suggests that moral hazard plays a significant role in STW programs.

Other firm outcomes

This section focuses on the effects of STW on firm survival and other firm-level outcomes, including investment, productivity, profitability, and liquidity.

As noted in Section 2.2, in a setting of imperfect financial markets, STW provides insurance against bankruptcy by subsidizing costly labor hoarding when temporary declines in activity tighten financial constraints. Consequently, it is crucial to examine whether STW has implications for firm survival and, if so, to identify the types of firms that benefit the most. Table A.1 presents the expected firm-level survival responses to STW use.

STW can help firms survive in the short and medium-to-long term when, in its absence, they would face high immediate adjustment costs (e.g., laying off all of their workers). This risk is particularly relevant for smaller firms, which

2.3. Effects of STW on firm and worker outcomes

are more sensitive to temporary liquidity shocks and have limited capacity for costly labor hoarding. However, STW may only lead to short-term positive effects on survival, with no significant impact in the medium-to-long run, if firms cannot remain viable without operating under reduced working hours, thereby delaying bankruptcies. This effect has negative implications for worker reallocation during a crisis by postponing mass layoffs.

STW may have no short-term survival effects but could generate medium-to-long-run benefits if it shields firms initially capable of labor hoarding but unable to sustain it as the shock persists. Alternatively, delayed effects may stem from administrative lags, as bankruptcies take time to materialize. Finally, akin to its employment effects, STW may generate deadweight losses if it subsidizes firms that would have survived regardless, as these jobs were never at risk of being lost.

Giupponi and Landais (2023) find that in Italy during the Great Recession, STW increased firm survival by 10 p.p. one year after treatment, with stronger effects for liquidity-constrained firms. However, it did not have significant effects on survival for less productive firms¹⁰, aligning with its lack of employment effects in this group. Similarly, Kopp and Siegenthaler (2021) show that Swiss firms using STW during the 2008–2009 financial crisis were 5.5–9 p.p. more likely to remain operational 4.5 years later. Evidence on firm survival during COVID-19 is still limited, but Benkovskis et al. (2023) demonstrate that reduced firm exits explain a large share of STW’s positive employment effects during this period.

Beyond survival, STW can influence firm productivity and profitability. By preserving employment, it reduces future turnover costs and protects firm- and industry-specific human capital. This has direct implications not only for productivity but also for future investment decisions and can translate into higher profits when demand recovers. However, Giupponi and Landais (2023) find that while STW boosts short-term employment in Italy, it temporarily reduces per-employee productivity (proxied by value added per worker) due

¹⁰Productivity is defined as it was prior to the recession.

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to fewer hours worked, with no effect on productivity per hour. They also report no significant impact on physical capital investment in either the short or medium-to-long term.

Biancardi et al. (2022) also examine the impact of STW on firm performance in Italy during the Great Recession, considering the role of unionization and collective bargaining. Consistent with prior findings, they show that STW reduces working hours per employee, lowers labor costs, and decreases per capita labor productivity, with no significant effect on hourly productivity in the short term. Moreover, while STW results in a small short-term decline in profits, these effects dissipate after STW uptake. Additionally, they find that in highly unionized firms, STW minimally affects labor costs, as unions negotiate to prevent wage reductions while promoting work-sharing. Notably, this does not reduce firm profitability. In contrast, low-unionized firms using STW face significant labor cost savings per employee with larger declines in productivity and profits in the short term.

2.3.2 Effects of STW on worker outcomes

While STW primarily preserves employer-employee ties during shocks, most evaluations focus on firm outcomes, leaving limited empirical evidence on its effects on workers' labor market and income trajectories. Theoretically, by subsidizing labor hoarding, STW prevents layoffs and mitigates the long-term scarring effects of unemployment and income loss. This section reviews the evidence on the impact of STW on (i) earnings, (ii) hours worked, and (iii) employment probability, conditional on program take-up.

Assessing the dynamic effects of STW requires comparing STW-insured employees to different counterfactual groups, i.e., the scenarios workers would have faced in the absence of the program. Recent studies primarily adopt two counterfactuals: (i) unemployed individuals and (ii) employees similar to STW participants but not covered by the program. Some studies suggest that STW positively impacts earnings and employment prospects but highlight varying short- and long-term effects. Niedermayer and Tilly (2016), who calibrate a job search and matching model assuming non-negotiable contracts in terms of hours and wages, find that STW participation in Germany causes

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a sharp but temporary decline in earnings, albeit less severe than for unemployed individuals. They find no medium-term¹¹ earnings penalties for STW participants, unlike unemployed workers, due to STW's ability to preserve employment relationships and its higher replacement rates compared to UI systems.

Giupponi and Landais (2023) find that, in the short term, workers in STW have the same employment probability one year after program participation as similar workers employed in non-eligible firms. However, this probability declines over time for STW users. Three years after treatment, the employment probabilities of STW workers converge towards those of similar workers in non-eligible firms who were laid off. Likewise, earnings per employee in STW decline in the short term due to reduced hours and, in the medium-to-long run, become comparable to the earnings of similar laid-off workers in non-eligible firms. This suggests that in Italy, STW had no significant long-term effect on employment prospects, consistent with its lack of impact on firm headcount employment over the same period.

Overall, STW appears to provide only short-term insurance, particularly in Italy during the Great Recession, where the economic shock was more persistent than in other countries. In prolonged downturns, firms eventually resort to layoffs, reducing workers' earnings over time. Moreover, the effects of STW on workers vary by firm type. Giupponi and Landais (2023) show that in high-productivity firms (defined prior to the crisis), medium-term earnings of STW participants are significantly higher than those of laid-off workers in comparable firms. This suggests that STW improves long-term employment prospects only for workers in high-productivity firms.

During the COVID-19 period, Brinkmann et al. (2024) leverage exogenous worker-level variation in STW eligibility within firms, exploiting the statutory retirement age cutoff, beyond which individuals become ineligible. They focus on workers in firms that took up STW in April 2020 but had not done so in the previous three months. Using an RDD approach, they compare workers

¹¹They evaluate earnings up to 24 months after an STW or unemployment event.

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who reached the statutory retirement age up to three months before or in April 2020 (never-eligible workers) with workers in the same firm who reached the statutory retirement age up to three months after April 2020 (eligible workers). They find no difference in the probability of remaining employed at the same firm between eligible and ineligible older workers. They argue that this is the case because STW protects workers that have a high baseline retention probability. Although specific to older workers—who face higher layoff costs—this finding reveals potential deadweight losses by subsidizing workers unlikely to be laid off.

The empirical literature on STW reveals mixed effects on firms and workers. For firms, STW can preserve short-term employment by reducing working hours affecting headcount, yet it may lead to deadweight losses if subsidizing firms that would not have laid off workers. Short-term employment gains hinge on factors like liquidity and productivity, while medium- and long-term gains depend on the severity and duration of economic downturns. STW can also help prevent bankruptcy in the short term, but its effectiveness wanes if firms cannot remain viable without labor hoarding. For workers, STW offers short-term job security and mitigates income losses; however, its long-term impact is limited, especially for those in low-productivity firms. Overall, STW provides temporary relief, with effectiveness influenced by firm characteristics and economic conditions.

2.4 Efficient STW insurance

An efficient STW program balances providing insurance to firms and workers while minimizing labor demand distortions. From a social welfare perspective, efficiency is achieved when the net social costs do not exceed the net social gains (Section 2.2).¹² This section examines two inefficiencies: fiscal externalities resulting from firms' behavior and labor market reallocation distortions. While STW reduces UI expenditures by preserving jobs, it may create fiscal externalities due to firms' strategic behavior, driven by

¹²This leads to a net social cost minimization problem for a benevolent planner, who minimizes the costs of STW, given the utility of both STW participants (firms and workers) and non-participants.

asymmetric information. On the other hand, reallocation inefficiencies may arise from the protection of low-productivity jobs (Mohimont et al., 2024; Meriküll and Paulus, 2024; Diaz et al., 2025) and permanent contracts. We document the mechanisms behind these inefficiencies and review the latest theoretical and empirical insights.

2.4.1 Fiscal savings and costs of STW

STW creates a negative fiscal externality when firms overlook the burden that reduced working hours impose on public finances. However, it also generates savings by lowering UI expenditures. To assess STW's net fiscal impact, we first examine the UI-related externalities and their link to STW use, followed by an analysis of STW's primary inefficiency: firms' moral hazard responses.¹³

The value of a job consists of a private value, determined by the firm's production function and shared between firms (through profits) and workers (through wages), and a social value, reflecting its contribution to society (Cahuc et al., 2014). Job destruction affects both values, creating fiscal externalities when firms fail to account for UI costs or lost tax revenue. STW mitigates these UI fiscal externalities by preserving jobs, thereby reducing UI claims (Giupponi et al., 2022; Cahuc, 2024). Job preservation occurs when STW increases employment and, as a result, total hours of work, relative to the counterfactual without STW, where layoffs would have occurred and total hours would have been reduced.

However, it can also increase fiscal costs, primarily due to moral hazard. Firms may ex-ante manipulate eligibility to access benefits or ex-post overuse STW by excessively cutting hours (Burdett and Wright, 1989), even when they are financially stable or experiencing growth (Jaravel, 2022). Some firms, particularly those with seasonal fluctuations or poor management, have

¹³Adverse selection affects the net fiscal cost of social insurance programs but is less relevant in STW, as protecting at-risk jobs is a core objective. Teichgräber et al. (2022) suggest that when firm productivity is private information and unverifiable by policymakers, but hour reductions are observable, STW acts as a screening mechanism. Only the most productive matches—those reducing hours based on expected recovery—are screened.

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strong incentives to excessively benefit from cross-subsidies¹⁴, which reduce aggregate productivity in the economy (Boeri and Cahuc, 2023; Cahuc and Nevoux, 2017). Others may misuse the program by enrolling without actually reducing hours (Boeri and Cahuc, 2023). These behaviors occur because, in most cases, firms are not required to cover any costs of using STW (Burdett and Wright, 1989; Cahuc, 2019; Audenrode and A, 1994).

While evidence from the Great Recession suggests limited misuse (Giupponi and Landaïs, 2023; Biancardi et al., 2022; Kopp and Siegenthaler, 2021), there is some evidence of firm moral hazard during COVID-19. Lapeyre (2024) exploits quasi-experimental variation in employer STW contributions across industries in France, finding that firms eligible under a less generous scheme (with higher employer costs) claimed 3.5 times more STW hours under a more generous scheme by self-reporting eligibility. Moreover, using an event study with an IV approach, she shows that raising employer contributions from 0% to 10% of workers' gross hourly wages reduced STW take-up by 21% and 33%, in terms of workers and hours, respectively. This aligns with Albertini et al. (2022), who, through dynamic search and matching model simulations, show that a 10% increase in employer contributions in France would have reduced STW demand without negatively affecting employment. Furthermore, Bossler et al. (2023) use an experimental survey in Germany to document STW-related free-riding, finding evidence of firms misreporting working hours, likely resulting in deadweight effects that increase STW's fiscal cost.

Some studies have quantified the net fiscal costs of STW programs by evaluating their effects on employment and hours worked during the Great Recession. In Italy, each euro spent on STW costs society 1.38 euros, which is a lower burden than UI alone (1.5–2.5 euros per marginal euro) (Giupponi and Landaïs, 2023). In contrast, Kopp and Siegenthaler (2021) find that STW's fiscal benefits in Switzerland outweighed its costs during the Great Recession.

In summary, while STW reduces UI expenditures, it can generate negative fiscal

¹⁴Cross-subsidies occur when certain firms or industries face negative demand shocks and require STW benefits, while others, such as those with predictable seasonal fluctuations, do not require them but still benefit (Hamermesh, 1989).

externalities when firms overlook the costs imposed on public finances. While existing research has begun to examine the net fiscal impacts of STW, evidence remains limited—particularly for stable periods, where behavioral costs may be considerable. These costs depend on the program’s targeting and eligibility criteria, which influence firm behavior, such as moral hazard and misuse of benefits.

2.4.2 Reallocation inefficiencies

Efficient reallocation in the labor market ensures that workers and firms are optimally matched, thereby maximizing aggregate output in the economy (Cahuc et al., 2014). In practice, this implies the smooth movement of labor from contracting to expanding sectors (Cahuc, 2019). LMIs and insurance policies play a key role in shaping reallocation, especially during crises and recoveries. This section explores how STW policies may create reallocation inefficiencies and reviews current empirical and theoretical findings.

Inefficient firm/sector reallocation

STW can hinder labor market reallocation by prolonging the existence of low-productivity firms and job matches. During crises, workers typically move to more productive sectors, boosting aggregate productivity (Boeri et al., 2011; Cooper et al., 2017; Hijzen and Venn, 2011). However, prolonged use of STW may prevent this transition, particularly when it keeps low-productivity firms alive and excessively supports low-productivity job matches (Giupponi and Landais, 2023; Cahuc, 2024).

Similarly, the extent to which STW hinders reallocation likely depends on the duration of the shock. A key concern is that under persistent shocks, STW may crowd out vacancies in the medium-to-long run, subsidizing non-viable job matches and slowing the transition to more productive jobs. Specifically, STW affects job flows by limiting unemployment inflows. Since recessions are often characterized by job rationing—where many unemployed workers compete for scarce vacancies—STW, by preserving jobs, can further slow reallocation. This is especially true when the program is mainly used by firms that are likely to fail or by low-productive job matches that will eventually be destroyed. Consequently, STW can increase hiring costs for more productive

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firms and for new firms entering the market, tightening the labor market (Cooper et al., 2017; Giupponi and Landais, 2023; Jaravel, 2022). However, in deep recessions, when hiring is already low, this effect may be less relevant, but it becomes more important during recovery periods.

Empirical evidence from Italy’s STW program during the Great Recession suggests that STW reduced employment in non-treated firms. Giupponi and Landais (2023) exploit the spatial variation across more than 600 local labor markets and find that for every percentage-point increase in STW coverage, employment in non-treated firms declined by 0.94% during the period 2010-2013, relative to the pre-recession period (2005-2008). Moreover, by calibrating a matching model, they find that STW not only hindered reallocation but also reduced total factor productivity in Italy by approximately 2% during the crisis, relative to a counterfactual scenario without STW.

Although persistent shocks seem more prone to reallocation inefficiencies, Diaz et al. (2025) argue that STW does not necessarily perform better in terms of reallocation and aggregate productivity during short recessions. Using a calibrated search and matching model that incorporates sector-specific shocks and skill accumulation in the context of Spain’s furlough scheme, they find that in a short-lived recession, furlough schemes may crowd out labor hoarding that would have otherwise occurred in their absence. Without the program, firms might retain jobs in anticipation of improving economic conditions. This could lead to a sharper decline in aggregate output than in a prolonged downturn, where firms are less likely to sustain jobs with negative surplus due to greater uncertainty about future gains. Moreover, STW may hinder sectoral reallocation more during short recessions, as its discouraging effect on job search is amplified when uncertainty is lower, reducing the urgency for low-surplus workers to reallocate.

Whether STW significantly impeded reallocation during COVID-19 remains an open question, though early evidence suggests potential distortions. Unlike past recessions, COVID-19 had heterogeneous effects across sectors, with some industries growing (e.g., ICT, manufacturing, healthcare) while others

contracted (e.g., recreation, hospitality) (Barrero et al., 2021). Additionally, the crisis accelerated existing structural labor market trends, such as remote work and gig employment (Boeri and Cahuc, 2023). Mohimont et al. (2024) use a Dynamic Stochastic General Equilibrium (DGSE) model, calibrating it to the euro area and find that furlough programs, when protecting non-viable jobs, hamper reallocation and decrease welfare. Meriküll and Paulus (2024) find that Estonia’s STW program hindered within-industry reallocation to more productive firms, reducing aggregate productivity by 1.6%–1.7%. Coupled with their employment findings (Meriküll and Paulus, 2023), this suggests limited net economic benefits.

Overall, while STW has been shown to be cost-effective in some cases by preserving jobs in the short term during recessions, its positive impact must be weighed against the medium- to long-term consequences for job creation and productivity (Cahuc, 2024), which result in reallocation inefficiencies. This assessment depends on the program’s specific institutional targeting features, which determine the extent to which low-productivity, non-viable job matches are protected (Mohimont et al., 2024).

Inefficient reallocation of workers across different types of labor contracts

STW favors open-ended contracts with high replacement costs, limiting reallocation across contract types and leaving low-cost workers underserved (OECD, 2021). This may complicate the transition of temporary workers—who tend to be young and low-skilled—as well as the transition of the unemployed into permanent jobs (Cahuc and Carcillo, 2011; Cahuc et al., 2021). As a result, STW may exacerbate labor market segmentation between fixed-term and open-ended contracts (Hijzen and Venn, 2011). Giupponi and Landais (2023) provide evidence that STW predominantly protected open-ended contracts during the Great Recession in Italy, thereby tilting the employment structure towards these contracts. In contrast, Kopp and Siegenthaler (2021) do not find strong evidence that the reduction in dismissals was accompanied by a decline in the use of fixed-term contracts.

Section 2.4 has examined two inefficiencies of STW: the negative fiscal

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externalities arising from firms’ distortive behavior and the inefficiencies in labor market reallocation. Cahuc (2024) uses the Marginal Value of Public Funds (MVPF) framework to assess these inefficiencies, comparing the net social benefits to the net fiscal costs of the program (see Hendren (2016); Hendren and Sprung-Keyser (2020)). The MVPF is calculated as the ratio of the weighted sum of willingness to pay for STW to its net marginal cost to the government, accounting for users’ behavioral responses. The willingness to pay for STW reflects the value both users and non-users derive from the program, determined by its positive (Section 2.2) and negative effects. In this section, we have focused on the negative impacts on non-users, particularly those related to labor market reallocation inefficiencies (Section 2.4.2), and how firms’ behavioral responses, such as moral hazard, can lead to higher net fiscal costs (Section 2.4.1).

2.5 Revising design features for efficient STW insurance

Certain design features of STW can play a crucial role in curbing misuse to contain fiscal costs and mitigating its impact on labor reallocation. However, empirical evidence on how these features influence policy take-up remains scarce—a notable gap given the extensive research on UI design (Schmieder and Von Wachter, 2016; Spinnewijn, 2020) and the central role of STW in social insurance during recent economic crises, even surpassing UI. In this section, we examine the potential effects of two key design elements on STW take-up: monitoring and financial disincentives—specifically, co-insurance and ER premia. We link these mechanisms to the inefficiencies they aim to address and discuss the potential caveats they may entail.

2.5.1 Monitoring

Monitoring is a key tool to prevent misuse of STW by firms, addressing the asymmetric information between STW-insured firms and the government. It involves collecting information on firms that (plan to) take up STW and, based on this, restricting access to those for whom the scheme provides the greatest social benefits. When implemented ex-ante, monitoring reduces the likelihood that ineligible firms access STW—for instance, firms not facing

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economic or financial distress or those experiencing structural rather than temporary difficulties. This improves efficiency by preventing subsidies for (i) profitable matches that would survive without STW and (ii) matches that would dissolve regardless of STW support. When applied ex-post, monitoring ensures that reductions in working hours among STW-insured workers are not excessive, addressing the moral hazard concerns highlighted in Section 2.4.

Monitoring primarily targets firm behavior rather than that of employees or other participants in the job relationship, such as unions. Employers have greater scope for opportunistic behavior when deciding on both STW take-up and the extent of working time reductions. This can be explained by the fact that firms respond more strongly to changes in the firm-side rather than the worker-side of the match surplus (Giupponi et al., 2022). This effect is particularly pronounced when bargaining rigidities restrict the ability to adjust hours and wages, limiting flexibility to revise downward the job match surplus. By contrast, while unions do influence STW adoption and interact with firms in its implementation (Biancardi et al., 2022), concerns about distortions in their behavior are lower. Unions primarily seek to maximize member welfare, which aligns with preserving viable job relationships. Moreover, unions do not directly benefit from job matches, unlike firms. A similar rationale underpins the focus on employer-borne financial disincentives, as discussed in Section 2.5.1.

In practice, monitoring can be implemented ex-ante by tightening firm eligibility criteria—for example, requiring proof of a significant decline in output demand or revenue. Ex-post, monitoring actual hours worked and assessing whether value added per worker declines during STW use can curb misuse, preventing firms from falsely claiming STW while maintaining full operations (Giupponi and Landais, 2023). While most countries do not yet monitor hours worked or value added, an increasing number are conducting STW claim audits and integrating STW claims with tax or social security data systems (e.g., Belgium since 2012 and France since the COVID-19 pandemic).

Monitoring poses significant challenges for governments due to its high costs

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and potential for errors. During COVID-19, the widespread adoption of STW made large-scale monitoring practically unfeasible. The crisis further complicated monitoring efforts as pandemic-induced changes in work organization, particularly the expansion of remote work, reduced the observability of hours worked and weakened the relevance of statutory working hours. This raises concerns that firms extensively using remote work may have also claimed STW improperly (Boeri and Cahuc, 2023). Therefore, additional measures to curb ex-post STW misuse include financial levies linked to policy take-up, which incentivize employers to internalize the fiscal costs resulting from their opportunistic behavior.

Financial disincentives

STW programs use financial disincentives through co-insurance premiums or experience rating (ER) systems. Co-insurance levies rise with reduced hours and wages, paid concurrently with STW use. ER systems, similar to UI in the US or disability insurance in Europe, impose deferred, progressive premiums based on past STW uptake. These levies can be applied at both the intensive and extensive margins of STW use, either at the worker-level or based on firm-level participation. For example, in France, firms pay a worker-level co-insurance premium set at the intensive margin, amounting to 10% of the replacement wage for idle hours for workers earning up to 4.5 times the minimum wage. In Belgium, the ER imposes levies on firms based on their previous use of STW for individual blue-collar workers, with a threshold of 110 STW days per calendar year. In Italy, the firm-level ER system imposes progressive premia based on the duration of overall firm participation in STW, measured over a rolling three-year period.

Each system addresses a distinct form of ex-post STW misuse, though unintended consequences may arise. Specifically, the worker-level ER at the intensive margin can incentivize firms to optimize reductions in working time under STW, potentially improving its efficiency. Meanwhile, the system at the extensive margin ensures that only workers whose jobs are genuinely at risk of layoffs are enrolled in STW, minimizing misuse of this type. The firm-level ER, such as the Italian model, may mitigate negative effects on labor reallocation by reducing the likelihood of prolonged STW usage. Lastly, financial levies help discourage “false” STW claims, preventing firms from

2.5. Revising design features for efficient STW insurance

using the scheme as a way to provide paid vacation or from colluding with workers to claim subsidies without actually reducing working hours, thereby protecting the program's financial sustainability.

However, the first caveat of such STW taxes is that they may impose additional burdens on firms already grappling with negative shocks, potentially exacerbating the impact of these shocks on employment. Financial levies are likely to be most effective when STW use is driven by factors directly under the firm's control, such as poor management. In most cases, however, STW usage is due to external factors, such as a global pandemic. In these scenarios, financial levies aim to moderate a firm's STW usage to better reflect the social costs of the program. This raises a key puzzle: if higher financial levies reduce job retention, what are the costs and benefits? Does it enhance or diminish the productive capacity of firms and worker welfare? On one hand, firms may be more inclined to retain long-term viable job relationships that are likely to survive the economic shock, thus boosting their overall productive capacity.

A second caveat of financial levies lies in their potential distributional impact across workers. With STW financial taxes, labor hoarding benefits only those workers whose replacement costs (e.g., firing, hiring, and training expenses) outweigh the cost of STW during the policy period (OECD, 2021). This dynamic is more likely when replacement costs are higher and labor hoarding costs are lower (for example, with a short hoarding period and low tax rates). However, such financial incentives risk reinforcing labor market duality and segmentation. The incentives created by these policies tend to favor workers with high replacement costs (e.g., those on open-ended contracts with significant severance pay or those possessing firm-specific skills), while leaving workers with low replacement costs (e.g., those on fixed-term contracts or with less firm-specific human capital) underserved (OECD, 2021). Similarly, for workers at the bottom of the income distribution, the increase in taxes for firms tends to plateau faster than the benefits they receive from STW, particularly affecting lower-income workers and potentially limiting their access to the program initially.

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The impact of STW financial levies on labor markets remains an open question, requiring design-based methods and large administrative datasets. However, empirical measurement faces challenges. First, large-scale microdata that link STW tax rates to firm behavior are often not publicly available. Second, tax rates are endogenous by design, making it difficult to isolate the causal effect of taxes from other confounding factors, which can lead to negative selection biases among firms subject to higher tax rates. In other words, tax hikes are not random; they often reflect the financial health of a firm. Finally, in nearly all OECD countries, STW taxes on firms were suspended during both the Great Recession and the COVID-19 crisis (OECD, 2021). This policy change aimed to boost STW participation and alleviate liquidity constraints for firms impacted by the shocks. However, this suspension resulted in a lack of variation in regulatory frameworks, limiting the opportunities to use natural experiments for causal inference during periods of heightened STW take-up. To address selection biases, researchers, similar to those studying UI design (Landais, 2015; Johnston, 2021), might exploit kinks in the STW tax schedules (Lapeyre, 2024). However, it is important to note that the treatment effect identified through this approach would be local to the kink point in the tax rate schedule, which may limit the external validity of the findings.

This section explored how monitoring and financial disincentives—specifically co-insurance and experience rating premia—regulate STW take-up, thereby mitigating fiscal costs. Monitoring, whether implemented ex-ante or ex-post, helps prevent misuse by limiting access to STW for firms facing genuine temporary shocks and ensuring compliance with working-hour reductions. However, crises such as COVID-19 highlight its practical limitations when applied at large scale. Financial disincentives aim to curb opportunistic STW claims by shifting part of the fiscal burden onto firms, but they may also constrain struggling businesses and exacerbate labor market segmentation. Given their respective trade-offs, monitoring and financial disincentives should be seen as complementary tools for crisis response. Ex-post monitoring can help counterbalance laxer financial disincentives, but given its imperfections at scale, financial penalties remain essential to deter excessive use of the program.

2.6 Concluding remarks

This survey analyzed STW programs from both theoretical and empirical perspectives, assessing their objectives and the ways in which structural approaches and impact evaluations contribute to achieving those goals. It also identified the inefficiencies induced by STW programs, which can lead to social costs, and suggested potential improvements to enhance their effectiveness.

STW programs serve as critical social insurance tools by subsidizing reduced working hours to prevent layoffs. They help firms avoid bankruptcy, retain human capital, and minimize rehiring costs. For workers, STW mitigates income loss, stabilizes consumption, and prevents long-term unemployment effects, thereby sustaining aggregate demand. Their importance was evident during COVID-19, as their use surged across Europe.

Despite their benefits, STW programs have notable limitations. They impose substantial fiscal costs and can distort labor market reallocation. Asymmetric information may encourage firms to opportunistically reduce hours, inflating public expenditures. While preserving jobs, STW can also hinder worker transitions to higher-productivity sectors, slowing recovery. Prolonged reliance may sustain unproductive job matches and limit labor mobility. Moreover, STW often favors permanent workers, exacerbating labor market segmentation and disadvantaging those in temporary or precarious employment.

Empirical studies on STW during crises like the Great Recession and COVID-19 highlight its varied effects on employment, firm survival, productivity, and worker outcomes. Its effectiveness depends on firm characteristics such as liquidity, productivity, and unionization. STW best preserves jobs in firms facing temporary demand shocks, liquidity constraints, and high pre-shock productivity. Highly unionized firms adjust hours rather than wages, unlike less unionized firms. These findings emphasize the need for a nuanced approach to STW design that accounts for diverse firm conditions and labor market dynamics.

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Close monitoring and financial disincentives, such as co-insurance and experience rating, are key to enhancing STW effectiveness. Monitoring ensures that only firms in genuine distress qualify, mitigating moral hazard, though large-scale enforcement can be costly, especially in crises. Financial disincentives make firms internalize fiscal costs, curbing opportunism but requiring careful balance to avoid overburdening struggling firms. While essential, these tools must be calibrated to protect at-risk jobs during downturns. This trade-off highlights the need for further research to better understand the long-term consequences of STW, particularly its effects on labor reallocation and the cost-effectiveness of its design features. Future studies should also explore alternative policies, such as liquidity loans, to support firms during economic crises and ensure a more balanced and effective crisis response.

Appendix

2.A Additional Supplementary Material

Table A.1. Expected responses of STW on firm-level outcomes during a temporary downturn

Outcomes	Short-term (during take-up)	Medium- to long-term (after take-up)
Employment effects		
<i>Mechanical Effects</i>		
Volume of work in FTE per worker	↓	↑
Wage bill per worker	↓	↑
Jobs per firm	↑ = STW saves jobs	↑ = STW saves jobs
		[No effect = delays dismissals + hinders reallocation of workers] †
		[No effect = deadweight effects]†
Survival effects		
Likelihood of surviving	↑	↑
		[No effect = delays bankruptcies + hinders reallocation] †
	No Effect (if not enough bankruptcies in the counterfactual during the outbreak of the crisis)	↑
		[No effect = deadweight effects]†

Notes: (†) Responses described in brackets correspond to those not aligned with the objective of STW.

Table A.2. Summary of impact evaluations of STW during the Great Recession and COVID-19

Paper	Country	Program	Sample	Econometric Design	STW effects	
					Total effect	Short-run & Dynamics
Panel A: Great Recession						
Kopp and Siegenthaler (2021)	Switzerland	Kurzarbeit	Establishments with fewer than 500 employees	Event-study DiD / IV approach	- FTE employment per firm $\uparrow \approx 9\text{-}17\%$ (4.5 years after treatment). - Net share of dismissed workers $\downarrow \approx 11\%$ (3 years after treatment).	- Positive short- and long-run employment effects. - Larger effects in the first two quarters after (first) treatment.
Cahuc et al. (2021)	France	Activit�� Partielle	Single-establishment firms with more than 4 employees	IV approach	- Hours/worker $\downarrow$; no average headcount effect. - No significant survival effect. Heterogeneity: positive headcount effects for firms experiencing large demand shocks.	Heterogeneity: +42% (short run) and +69% (medium run) only for firms that experience large shocks in demand.
Giupponi and Landais (2023)	Italy	Casa Integrazione Guadagni Straordinaria	Firms with 5–25 FTE	IV approach	- Hours/worker $\downarrow \approx 40\%$. - Headcount $\uparrow \approx 45\%$. - Survival probability $\uparrow \approx 10$ p.p. Heterogeneity: larger effects for firms with low liquidity and higher pre-crisis productivity.	- Short-run effects that disappear after treatment. - Probability of surviving increases one period after treatment (survival is not studied in the short run).

Continued on next page

Table A.2. (Continued)

Paper	Country	Program	Sample	Econometric Design	STW effects	
					Total effect	Short-run & Dynamics
Biancardi et al. (2022)	Italy	Casa Integrazione Guadagni Straordinaria	Metal-engineering firms (>5 employees)	IV approach	• Labour costs and per-capita productivity ↓; no significant effect on hourly productivity. • Heterogeneity: higher STW use by firms with high unionization; larger short-run profit drop in low-unionized firms.	• Short-run effects that fade after treatment.
Panel B: COVID-19 period						
Bennedsen et al. (2023)	Denmark	Midlertidig-lønkompensation (Furlough Scheme)	Firms with more than 3 employees	OLS selection-on-observables with counterfactual using managers' predictions	• Negative selection: treated firms 16 p.p. more likely to lay off. • After accounting for selection, layoff rate ↓ 24 p.p. (first pandemic year).	N/A
Beykovskis et al. (2023)	Latvia	Dīkstāves pabalsts (Furlough Scheme)	Private-sector firms	Propensity Score Matching with DiD	• ~30% higher employment growth for firms in STW. • STW helped firms to survive.	• Effects significant up to 7 months after take-up.
Montenegro and Hijzen (2024)	Spain	ERTE (Furlough Scheme)	Non-agricultural establishments with 43–57 employees	Variation of Regression Discontinuity Design	• ≈1 job saved per STW worker (minimal deadweight). • STW promotes job finding; no congestion externalities; no evidence of blocking efficient reallocation.	• Positive employment effects persist up to one year after treatment.
Meriküll and Paulus (2023)	Estonia	Töötasu hüvitis (STW)	Non-financial private-sector firms	Nearest-neighbor Propensity Score Matching	• Firms' headcount ↑ ~20%.	N/A

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Table A.2. (Continued)						
Paper	Country	Program	Sample	Econometric Design	STW effects	
					Total effect	Short-run & Dynamics
Brinkmann et al. (2024)	Germany	Kurzarbeit (STW)	Universe of establishments	Regression Discontinuity in Time (RDIT)	• Negative selection in terms of employment trajectories prior the first year of take-up. • Take-up skewed to workers with higher wages, mid-level education, more complex job roles. • Extending the maximum duration of STW benefits is not significantly associated with higher employee retention. • Evidence of downward wage negotiation, for firms ineligible to an extension of the duration of the benefits (decentralized wage bargaining setting).	• Null employment effects persists up to 40 months after take-up. • Decreasing wage effects for ineligible firms (to an extension of the STW benefits duration) persist after take-up.

Notes: This table summarizes the studies discussed in Section 2.3.

The Impact of Short-Time Work during the Great Recession

This chapter is co-authored with Bart Cockx and Gert Bijnens.

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.

3.1 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.

¹Other 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.

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 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

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

³This 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).

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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

⁴For a detailed survey of the literature and rationales behind STW programs in Europe, see Bermúdez et al. (2023).

3.2. The STW Program During the Great Recession in Belgium

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.

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.

3.2 The STW Program During the Great Recession in Belgium

STW schemes⁵ 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 (Graiss, 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

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).

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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 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

⁷ Ambiguous cases are occasionally settled in court.

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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 A.1 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

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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, 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.

3.3 Data

We combine two different administrative data sources. The first consists of micro-aggregated administrative yearly⁹ data from the National Social Security

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

⁹The 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

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.

To address the structural break in the definition of NACE sectors¹² introduced in 2008, we harmonize sector codes using conversion tables provided by the NSSO. This procedure is described in Appendix 3.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

2007q3 to 2008q2.

¹⁰There 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).

¹¹Years are also defined as described in Footnote 9.

¹²In 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.

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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 A.4).

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.

¹³This 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.

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

Descriptive statistics for treatment and outcomes at the cell level (region-sector) in 2009 are presented in Table A.3. 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 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 ($\frac{Firms}{Total} \frac{in}{No.} \frac{STW_t}{Firms_t}$). 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 A.2 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

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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 3.4, the instrument is constructed by interacting the share of blue-collar workers in 2004¹⁷ with the national sectoral shock, combining a treatment exposure with a shift variable. We measure the shock ($g_{(s)2009}^{-(l)}$) 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):

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

¹⁶In Belgium, high-wage workers—particularly those earning above the wage cap (See Section 4.2 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.

¹⁷We 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).

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$$g_{(s)2009}^{-(l)} = \ln(T)_{(s)2009}^{-(ls)} - \ln(T)_{(s)2008}^{-(ls)} \quad (3.1)$$

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 3.4 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 A.4. 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 4.1. 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.

3.4 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 3.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 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 3.2 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

¹⁸Note 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).

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

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right-hand panel of Figure 3.2 confirms this result using residualized outcomes that control for location-sector-specific trends between 2004 and 2009 and other potential confounders.²⁰ Figure 3.2 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 3.3).

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.

²⁰This is done by regressing STW take-up on the full set of covariates in Equation (4.2), 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:

$$y_{(ls)t} = \gamma_0 + \gamma_{1t}z_{(ls)2009} + \gamma_{2t}b_{(ls)2004} + \gamma_{3t}g_{(s)2009}^{-(l)} + \gamma_{4t}x_{(s)2007}^{-(l)} + \phi_{1(ls)} + \phi_{2(ls)}t + \phi_{3(l)t} + u_{(ls)t} \quad (3.2)$$

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 $t \in 2005, \dots, 2012$). $b_{(ls)2004}$ is the blue-collar worker share, fixed in 2004, the first year of available data. $g_{(s)2009}^{-(l)}$ is the leave-one-out (LOO)²¹ growth rate of turnover from 2008 to 2009, representing the sectoral shock. $z_{(ls)2009} \equiv g_{(s)2009}^{-(l)} \cdot b_{(ls)2004}$ is the IV constructed as the interaction of the blue-collar worker share and the shock. $x_{(s)2007}^{-(l)}$ 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.

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

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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:

$$\begin{aligned} \Delta y_{(ls)t} = & \gamma_{1t}^* z_{(ls)2009} + \gamma_{2t}^* b_{(ls)2004} + \gamma_{3t}^* g_{(s)2009}^{-(l)} + \gamma_{4t}^* x_{(s)2007}^{-(l)} \\ & + \phi_{2(ls)}(t - 2004) + \phi_{3(l)t}^* + \Delta u_{(ls)t} \end{aligned} \quad (3.3)$$

Note that differencing causes us to lose 2004 from the sample. Equation (4.3) identifies the long-differenced parameters relative to 2004. $\phi_{2(ls)}$ is a cell-specific trend coefficient²³ 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 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.

²²We 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.

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

3.4. Identification strategy

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 (4.3) 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 (4.3) 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 (4.3), 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:

$$\begin{aligned} \Delta y_{(ls)t} = & \alpha_0 \Delta STW_{(ls)2009} I_{t,2009} + \alpha_{1t} b_{(ls),2004} + \alpha_{2t} g_{(s)2009}^{-(ls)} \\ & + \alpha_{3t} x_{(s)2007}^{-(ls)} + \varphi_{2(ls)}(t - 2004) + \varphi_{3(l)t}^* + \Delta \epsilon_{(ls)t} \end{aligned} \quad (3.4)$$

for $t \in 2005, \dots, 2009$, 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

²⁴The 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.

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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_{2009}^{*RF} 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:

$$\gamma_{2009}^{*RF} = \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \quad (3.5)$$

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

$$\begin{aligned} \gamma_{2010}^{*RF} &= \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \\ \gamma_{2011}^{*RF} &= \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2011}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} + \alpha_2 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \\ \gamma_{2012}^{*RF} &= \alpha_0 \cdot \frac{\partial \Delta STW_{(ls)2012}}{\partial z_{(ls)2009}} + \alpha_1 \cdot \frac{\partial \Delta STW_{(ls)2011}}{\partial z_{(ls)2009}} + \alpha_2 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}} \\ &\quad + \alpha_3 \cdot \frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}} \end{aligned} \quad (3.6)$$

Here, $\{\alpha_0, \alpha_1, \alpha_2, \alpha_3\}$ corresponds to the sequence of dynamic treatment effects of STW. For instance, from Equation (3.6), we solve for $\hat{\alpha}_1$ as:

$\hat{\alpha}_1 = \frac{\hat{\gamma}_{2010}^{*RF} - \hat{\alpha}_0 \cdot \frac{\partial \Delta STW_{(ls)2010}}{\partial z_{(ls)2009}}}{\frac{\partial \Delta STW_{(ls)2009}}{\partial z_{(ls)2009}}}$. 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 3.5.²⁵

3.5 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.

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

²⁶As noted in Section 3.3, 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.

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3.5.1 First Stage and Reduced Form

Following the empirical strategy outlined in Section 3.4, 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 3.3 trace the evolution of the instrument's effect on STW use over 2005–2012. The figure plots the coefficients γ_{1t}^* from Equation (4.3), 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 baseline²⁷ 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 3.4).

We then estimate the first stage of Equation (3.4), 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 3.1 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

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

²⁸This 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).

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.

The F-statistic averages 44 across the two specifications in Table 3.1. Column (1) corresponds to the full specification (Equation (3.4)); 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 3.3 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.

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

We now turn to the main estimates from the TSLS specification outlined in Equation (3.4). Table 3.2 reports the effects of STW take-up on employment outcomes, while Table 3.3 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

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).

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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 3.3)—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 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.

³⁰ $(e^{-0.2045} - 1) \times 100\%$.

³¹In 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).

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

Figure 3.4 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 3.5), 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.

3.5.3 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

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instrument predicts differential STW take-up in the post-crisis period, while no significant pre-crisis differences are observed in treatment exposure (Table 3.4 and Figure C3 in the Appendix) or in outcomes (Figure C8 in the Appendix). Furthermore, Figure 3.2 is replicated separately for manufacturing and non-manufacturing firms (see Figure B6 in Appendix), indicating 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 3.6, 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

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

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 3.4 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 cell³⁵—satisfies the placebo test and leads to comparable conclusions with a sufficiently strong first stage in the short term.³⁶

Furthermore, our estimates could potentially suffer from sample selection bias from endogenous 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

³⁴If 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).

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

³⁶These additional results are available upon request.

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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 for small, manufacturing crisis-exposed firms.

3.6 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.

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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.

3.7 Tables and Figures

Tables

Table 3.1. 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*** (0.1282)	0.8618*** (0.1263)	7.8939*** (1.2365)	8.3490*** (1.2619)
Trend FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Control: Share	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Control: Shock	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Region-specific trends	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Control: Log of turnover in 2007	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>
Restricted model	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
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 $\frac{1}{N_{it} + N_{t,2004}}$, 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 (3.4) for the period 2004–2009.

3.7. Tables and Figures

Table 3.2. 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 (0.1680)	-0.2270 (0.1559)	0.1428 (0.2409)	0.0835 (0.2145)	-0.0873 (0.3851)	-0.1121 (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 $\frac{1}{N_{it} + \frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t .

Table 3.3. TSLS estimates (wage effects)

	Gross Wage rate		Gross Wage bill per employee	
	(1)	(2)	(3)	(4)
Treatment x I[t=2009]	0.0567 (0.0476)	0.1119*** (0.0426)	-0.2045 (0.1490)	-0.1151 (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 $\frac{1}{N_{it} + \frac{1}{N_{i,2004}}}$, where N_{it} is the number of firms in each cell in period t .

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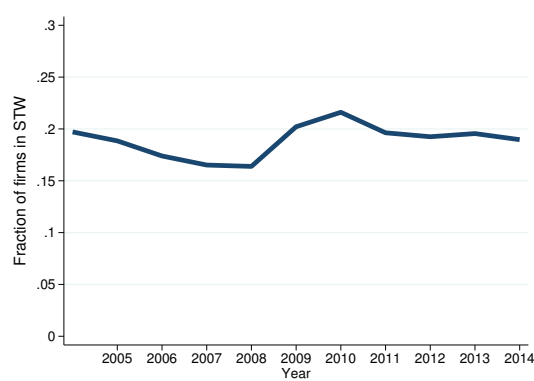
Table 3.4. First stage estimates: heterogeneity across economic sectors.

	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)
$Z_{i,t,2009} \times \mathbb{I}[t=2009]$	0.3966*** (0.1222)	5.2315*** (1.4573)	0.5002*** (0.1175)	6.0508*** (1.3443)	2.0467*** (0.4087)	13.6560*** (2.9004)	2.0399*** (0.4020)	13.5923*** (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 (3.4) for the period 2004–2009 for manufacturing and non-manufacturing firms.

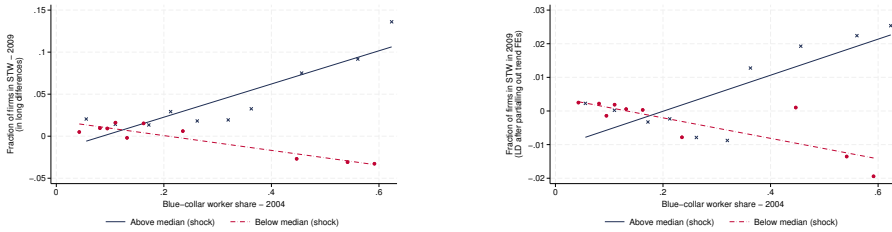
Figures

Figure 3.1. 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 3.2. 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.

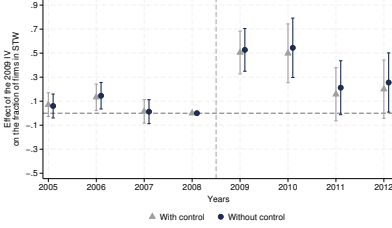


Notes: This figure displays in the left-hand side panel the average change in the fraction of firms in STW in 2009 with respect to 2004 and the blue-collar worker share in 2004, above and below the median of a turnover shock experienced in 2009 with respect to 2008. In the right-hand side panel we plot 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.

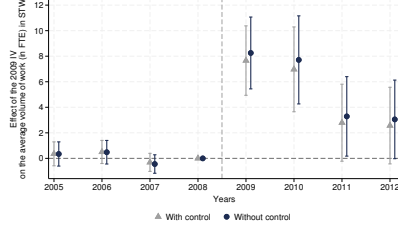
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Figure 3.3. Placebo test (First stage and Reduced Form)

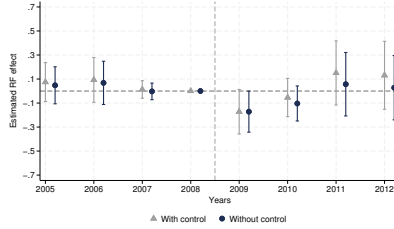
(a) First stage estimator (extensive margin treatment)



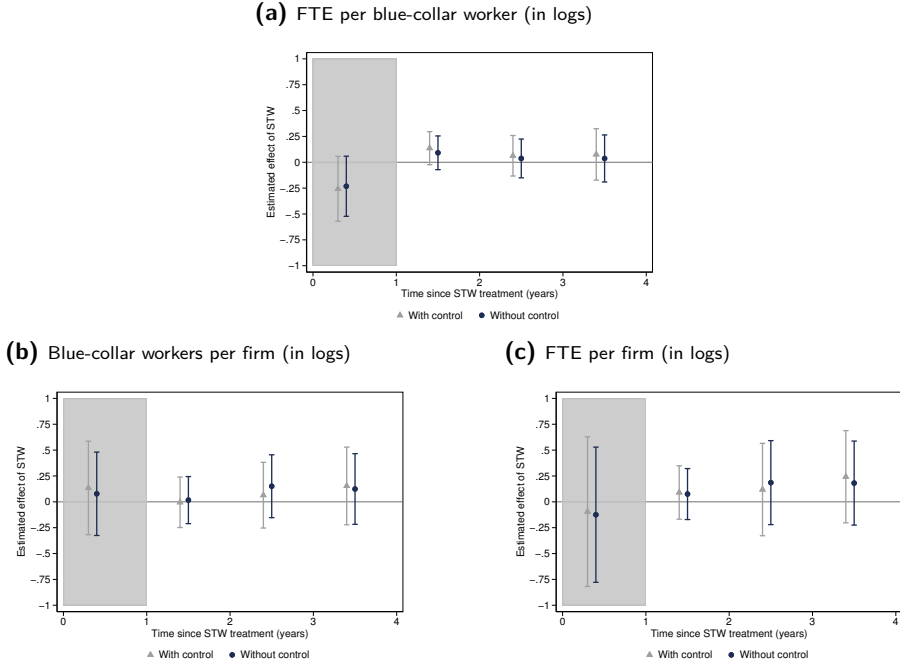
(b) First stage estimator (intensive margin treatment)



(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 (4.3) 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 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 3.4. Dynamic Treatment Effects: employment decomposition

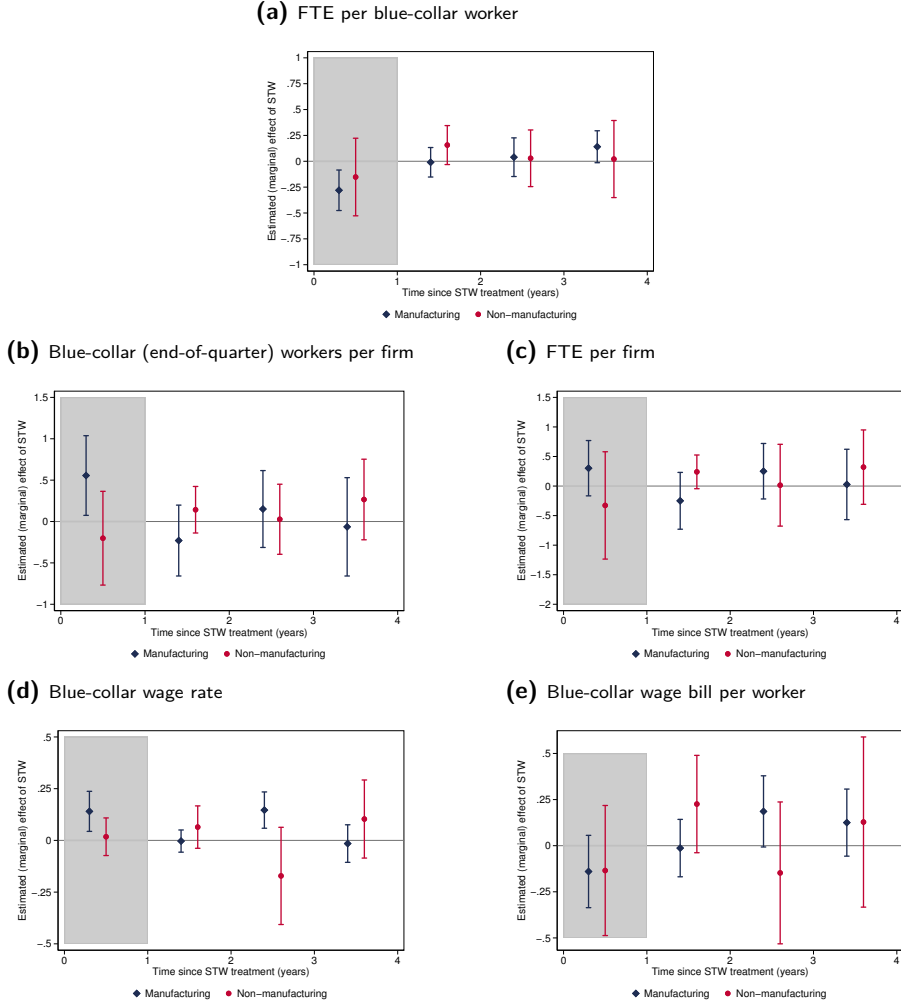
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic standard errors calculated with the Delta Method. These graphs report the sequence of dynamic treatment effects $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \dots, \hat{\alpha}_4\}$. Two specifications are displayed: (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.

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Figure 3.5. Dynamic Treatment Effects: wage outcomes



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 $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \dots, \hat{\alpha}_4\}$. 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 3.6. Dynamic Treatment Effects: Employment Decomposition by Sector

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 $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \dots, \hat{\alpha}_4\}$. 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 $\gamma_{4t}^* \neq 0$.

Appendix

3.A Policy and Data

3.A.1 The policy

Table A.1. Modalities of STW in Belgium during the Great Recession.

Type of worker	Types of reduction in working time	Maximum Duration
<i>Blue-collar employees</i>	Partial - small suspension (≥ 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)
<i>White-collar employees</i>	Partial (≥ 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.

3.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.

3.A.3 Descriptive statistics

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

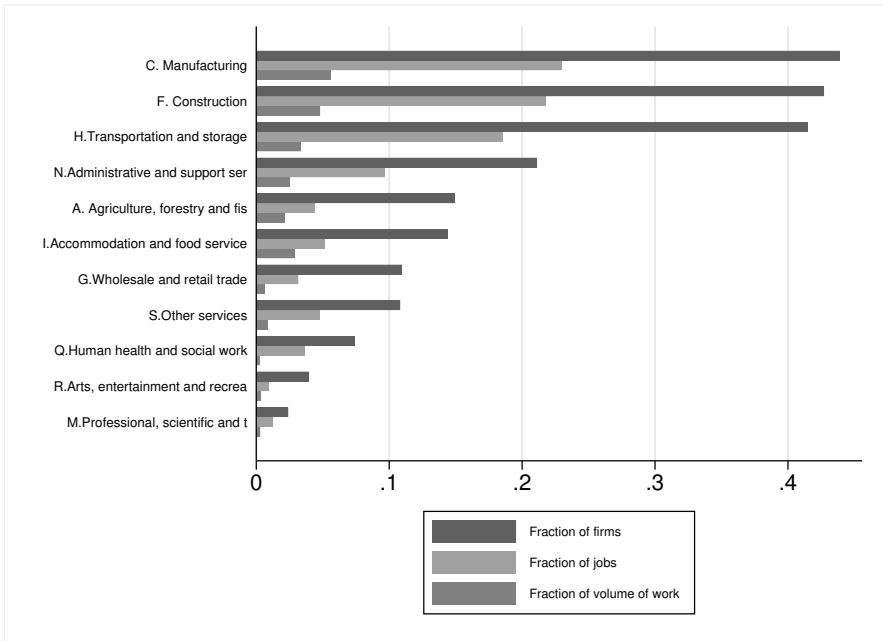
Notes: Wages are deflated taking into account the year 2004 as a baseline. The wage rate is computed by dividing the total gross (before-tax) wage bill (for blue-collar workers) by the total volume of work in FTE (for blue-collar workers). The gross wage bill does not account for employer social security 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.

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Table A.4. Evolution of STW take-up by sector.

	2007			2008			2009			2010			2011		
	%	Firms	%	%	Firms	%	%	Firms	%	%	Firms	%	%	Firms	%
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%
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%
E. 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%
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%
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%
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%
J. Information and communication	1.0%	1,509	0.0%	24,144	1.0%	1,587	0.0%	25,594	1.0%	1,580	1.0%	25,290	1.0%	1,585	1.0%
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%
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%
M. Professional, scientific and technical activity	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%
N. Administrative and support service	15.0%	2,509	6.0%	41,270	15.0%	2,614	6.0%	42,177	20.0%	2,546	8.0%	40,915	25.0%	3,118	11.0%
Q. Human health and social work	5.0%	3,715	2.0%	74,988	6.0%	3,806	3.0%	78,698	7.0%	4,137	4.0%	83,090	3.0%	3,636	2.0%
R. Arts, entertainment and recreation	3.0%	1,306	1.0%	21,224	3.0%	1,302	1.0%	21,665	4.0%	1,338	1.0%	22,190	4.0%	1,358	1.0%
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%

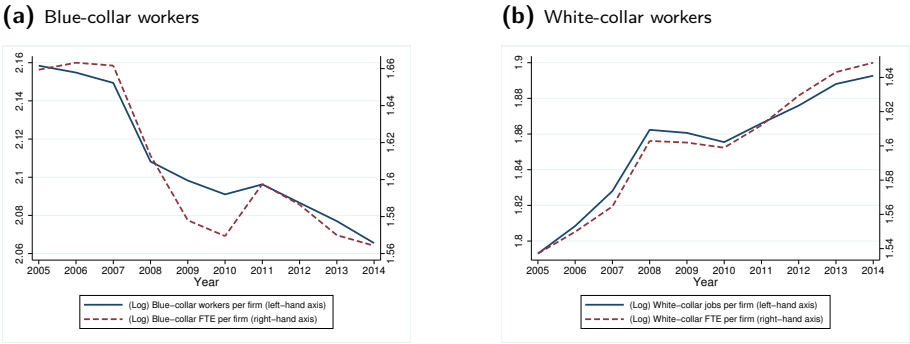
Notes: The table reports the annual evolution of STW take-up over 2007–2011. Take-up rates are expressed as the percentage of firms and of jobs. “STW take-up” is defined as having used STW at least once during the period of analysis.

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

Notes: This figure is elaborated with the data after the selection procedure described in Section 3.3.

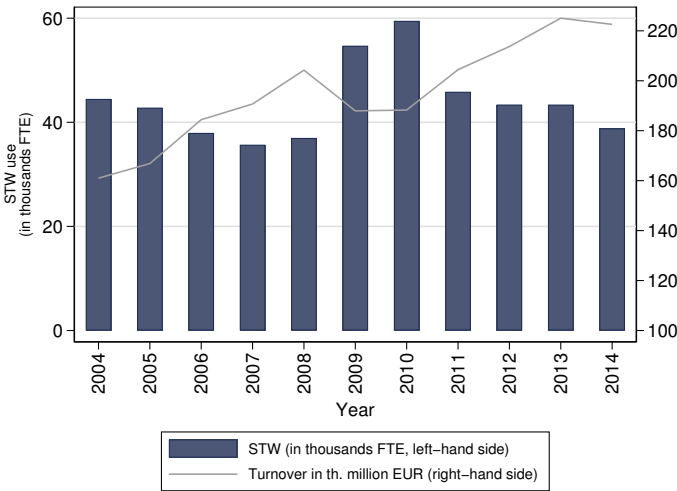
3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

Figure A2. Evolution of employment per firm: blue-collar vs 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 3.4. 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.



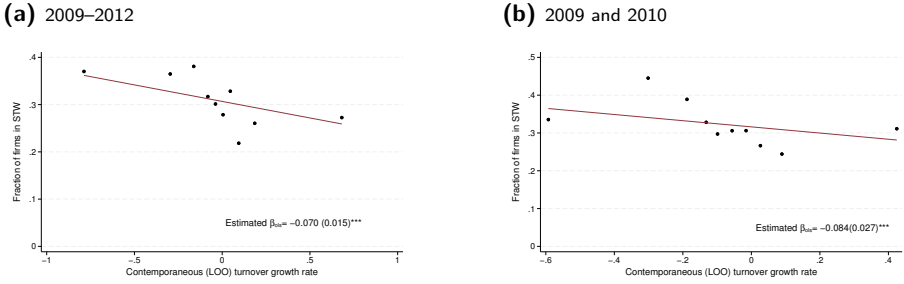
Notes: These graphs shows the evolution of STW in FTE and turnover in thousands of million EUR during the period 2004-2014. This graphs displays statistics not using calendar years but instead the new definition of years explained in Section 3.4. This new definition was adopted since the shock was initially encountered in the last two quarters of 2008.

3.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.

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



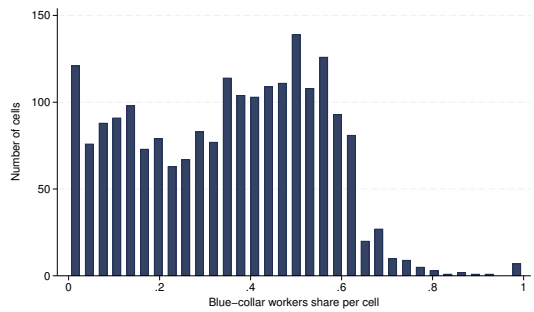
Notes: $\hat{\beta}_{OLS}$ is the estimator from regressing the fraction of firms in STW on the contemporaneous growth rate of FTE with respect to t-2. Panel (a) and (b) controls for year fixed effects. Data is aggregated at the level of 10 equal-sized bins. The definition of years is the one described in Section 3.4.

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.

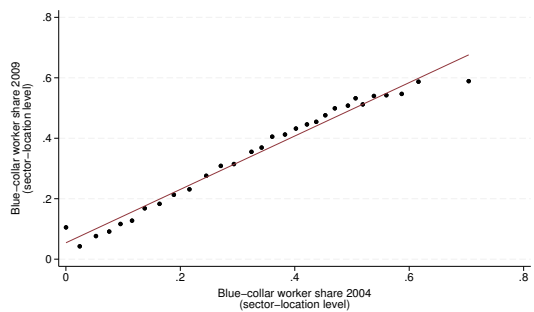
3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

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 3.4.

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

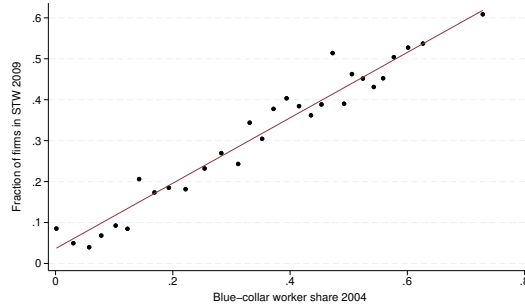


Notes: Data aggregated at the level of 30 equal-sized bins.

3.B. Graphical evidence of IV components

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 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

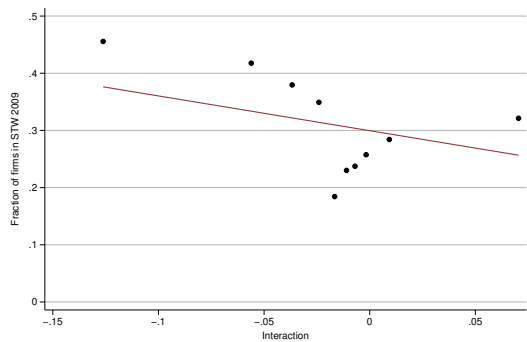


Notes: The relationship displayed in the following graphs controls for the leave-one-of growth rate of turnover. Data aggregated at the level of 30 equal-sized bins.

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.

3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

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

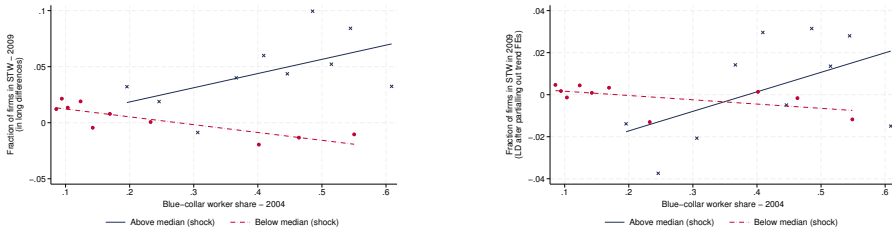


Notes: The top 1% of the interaction is winsorized. Data aggregated at the level of 10 equal-sized bins. This relationship controls for the leave-one-of growth rate of turnover, as well as the log of turnover in 2009.

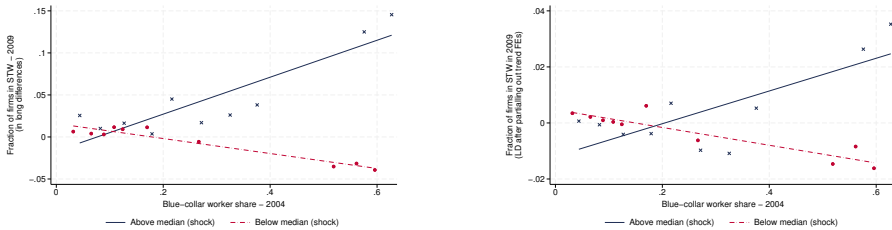
3.B. Graphical evidence of IV components

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



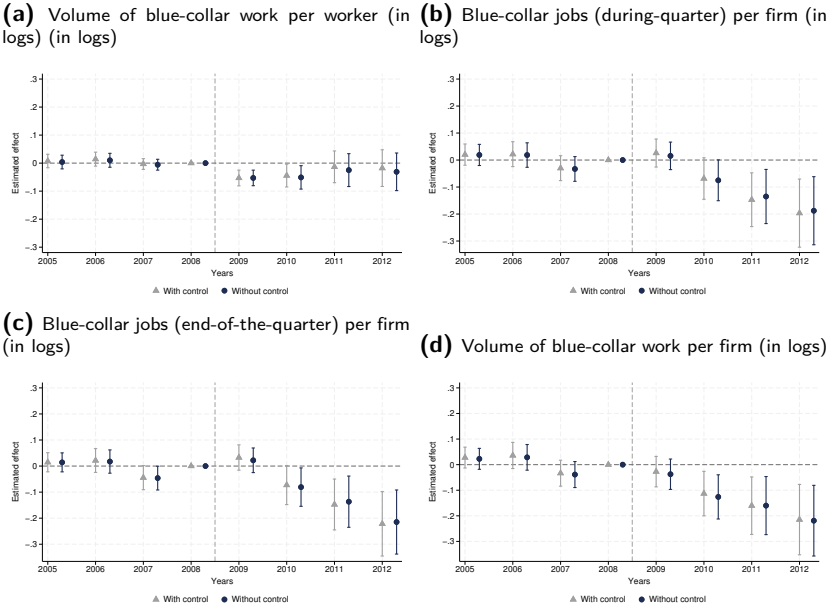
Notes: This figure displays in the left-hand side panels the average change in the fraction of firms in STW in 2009 with respect to 2004 and the blue-collar worker share in 2004, above and below the median of a turnover shock experienced in 2009 with respect to 2008, for the manufacturing and non-manufacturing 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.

3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

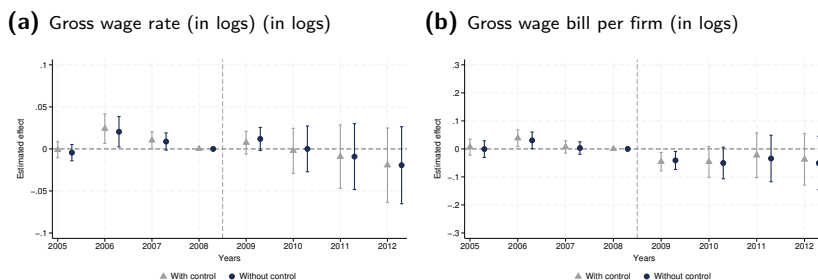
3.C Placebo tests

3.C.1 OLS model

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



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.

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.

3.C.2 First stage

3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

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

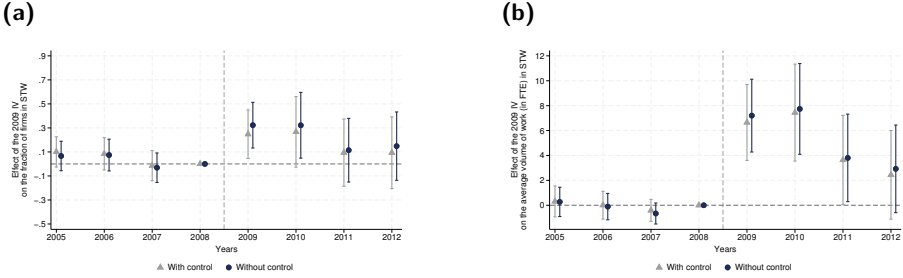


Figure C4. Placebo test: Non-manufacturing sector take-up.

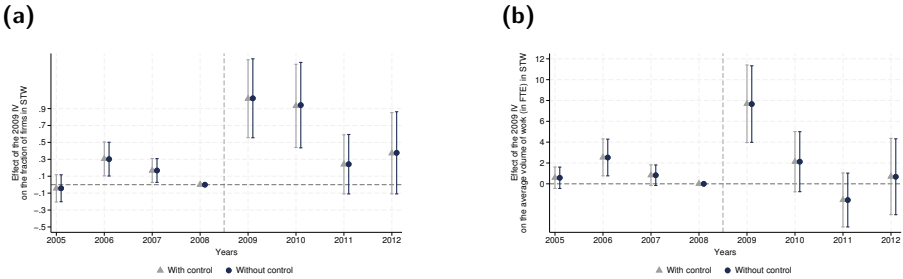
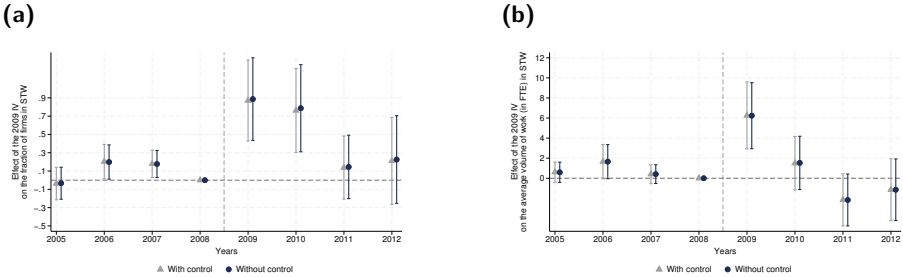


Figure C5. Placebo test: Non-manufacturing (without construction) sector take-up.

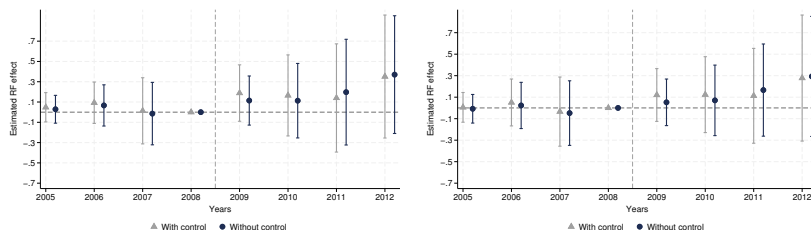


Notes: The vertical bars correspond to 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. Dependent variables are defined in long 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). Two models are presented: a model with control ($\gamma_{4t}^* \neq 0$, defined by the triangle) and a model without control ($\gamma_{4t}^* = 0$, defined by a dot). 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).

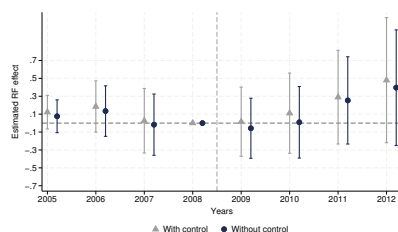
3.C.3 Reduced form

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)



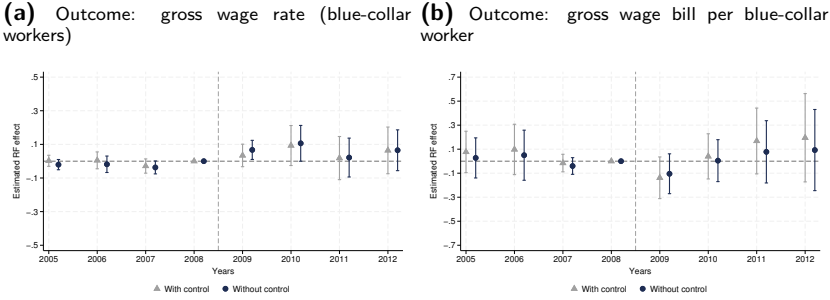
(c) Volume of blue-collar work per firm (in logs)



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.

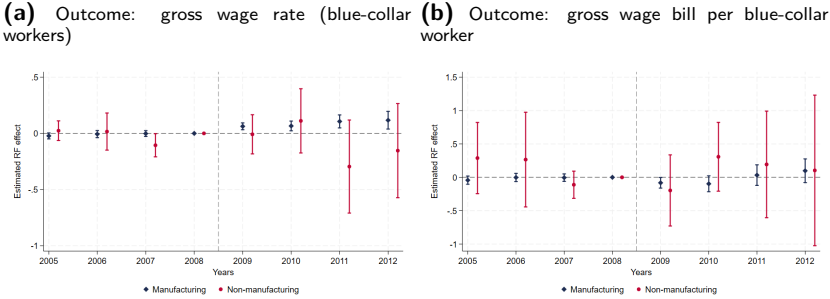
3. THE IMPACT OF SHORT-TIME WORK DURING THE GREAT RECESSION

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



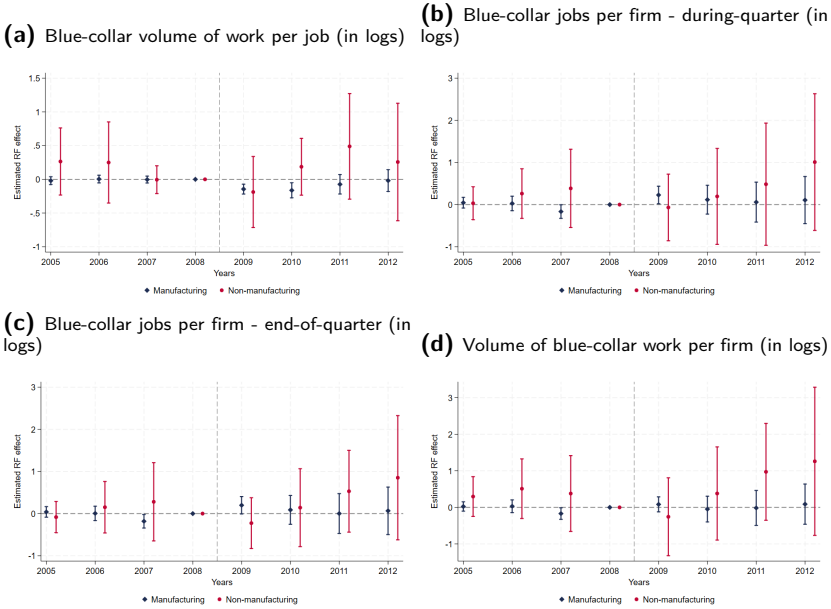
Notes: The vertical bars show 95% Confidence Intervals (CI) for robust heteroskedastic clustered standard errors at the location-sector level. These graphs report the coefficients from a pooled regression of the outcomes on the instrument defined in 2009. The instrument (Z_{2009}) is defined as the interaction 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 C9. Placebo test (reduced form) by economic sector: wage outcomes.



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.

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



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.

3.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	Heterogeneous Treatment effects	Interpretation
							β (SE)	β (SE)	
Kopp and Siegenthaler (2021)	Switzerland	Kurzarbeit	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	Activit�� Partielle	Single-establishment firms with at least 4 employees	IV approach (surated weighted model)	STW take-up	2009	-0.236 0.238 (0.060) 0.102 (0.067) -0.027 (0.079)		Hours per capita: -25% Headcount employment: +33% Total hours worked: no significant effect. Firm closure: no significant effect.
							-0.511 (0.038)		Hours of work per worker: -40%
							0.382 (0.036) 0.032 (0.028) -0.556 (0.046) 0.104 (0.035)		Headcount employment: + 45% Wage rate: no significant effect Wage bill per employee: -45% Firm survival probability (one year after take-up): +10 p.p.
Giupponi and Lauthais (2023)	Italy	Cassa Integrazione Guadagni Straordinaria	Firms with 5 to 25 FTE	IV approach	STW take-up	Average between 2009 and 2014	-0.020 (0.011)		Hours of work per worker: a 10% increase in STW hours reduces working hours by 0.2%
Blacard et al. (2025)	Italy	Cassa Integrazione Guadagni Straordinaria	Sample of metal engineering firms with more than 3 employees	IV approach	Hours of worked subsidized by STW	Average between 2009 and 2015	0.138 (0.030)		Headcount employment: no significant effect Total hours worked: no significant effect Total employment by 1.4%
							0.117 (0.032)		Total hours of work: a 10% increase in STW hours increases total working hours by 1.2%
							-0.055 (0.023)		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.

3. THE IMPACT OF SHORT-TIME WORK DURING 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*** (0.0066)	-0.0561*** (0.0164)	-0.0528*** (0.0160)	-0.0436*** (0.0088)	0.0463 (0.0335)	0.0392 (0.0325)	-0.1240*** (0.0112)	-0.0250 (0.0402)	-0.0285 (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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. 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 $\frac{1}{N_{it} + N_{it,2004}}$, where N_{it} is the number of firms in t . 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.

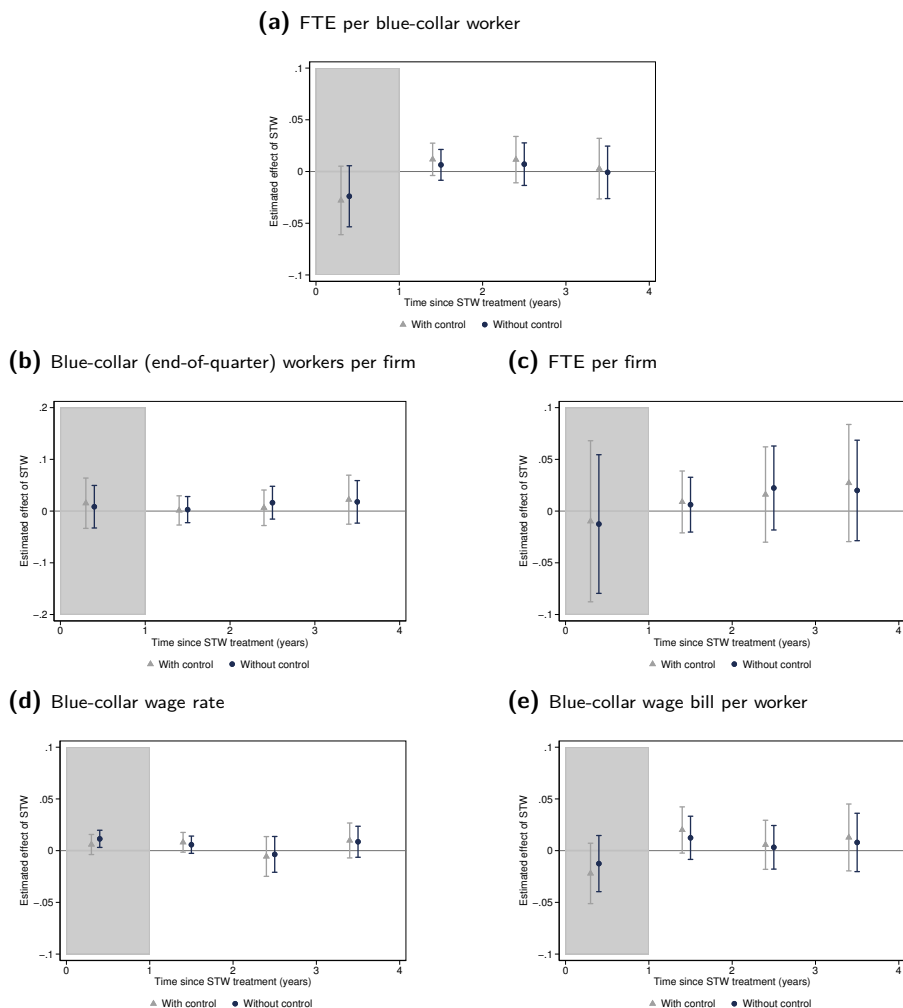
Table D3. OLS estimates - wage outcomes

	Gross Wage rate			Gross Wage bill per employee		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x I[t=2009]	0.0056 (0.0093)	-0.0041 (0.0095)	0.0032 (0.0096)	0.0783*** (0.0043)	-0.0495** (0.0204)	-0.0495** (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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust heteroskedastic standard errors in parentheses, clustered at the cell level. 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 $\frac{1}{N_{(t)2t} + N_{(t)2004}}$, where N_{it} is the number of firms in t .

3.D. Additional Supplementary Material

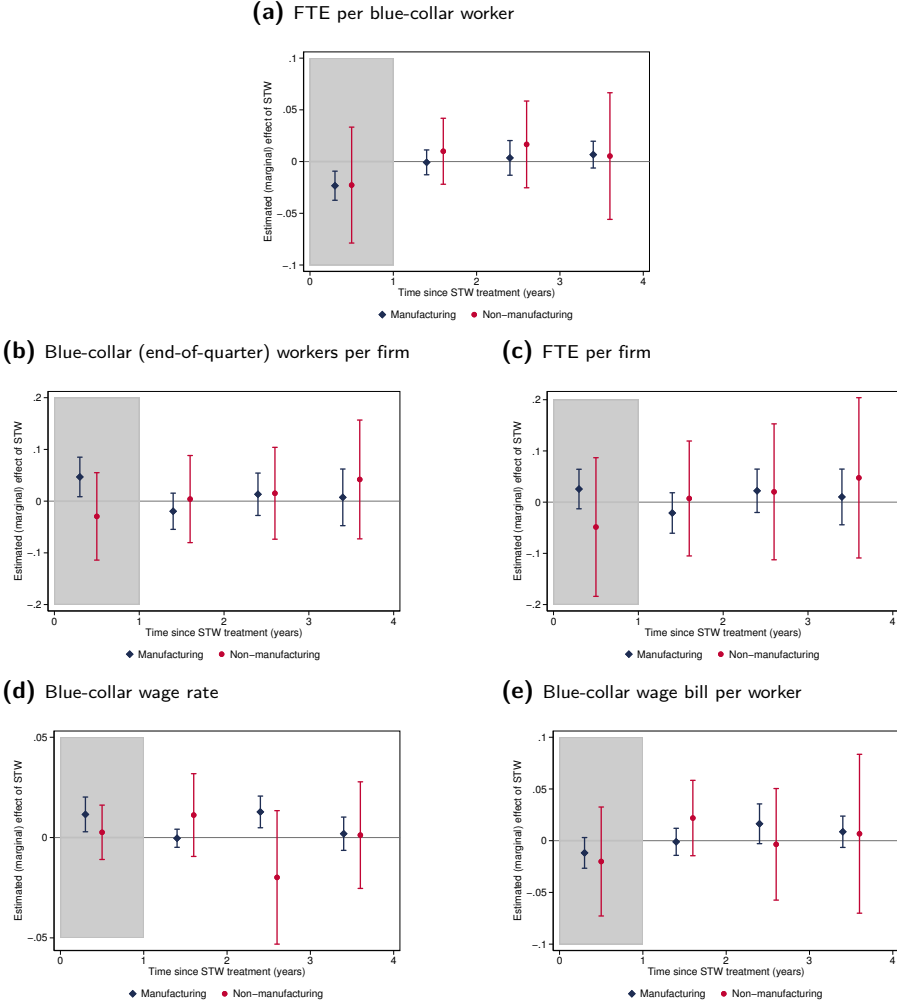
Figure D1. Average Dynamic Treatment Effects (Intensive Margin Treatment): Employment Decomposition



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 $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \dots, \hat{\alpha}_4\}$. 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.

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



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 $\{\alpha_j : \hat{\alpha}_0, \hat{\alpha}_1, \dots, \hat{\alpha}_4\}$. 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.

Protecting Jobs and Firms: The Impact of Short-Time Work during COVID-19

This chapter is solo-authored.

Short-Time Work (STW) programs aim to protect jobs and firms, but their effectiveness during the COVID-19 pandemic remains unclear. This paper evaluates STW during the pandemic, a period marked by economic lockdowns and high program uptake. Using a fuzzy regression discontinuity design, I analyze a Belgian policy reform that restricted access to firms that had used the program for less than 20% of contractual days at the peak of the crisis. The results show that STW effectively preserved employment in firms partially affected by lockdown measures, but had no significant effect on firms in essential sectors unaffected by lockdowns, indicating deadweight losses. These heterogeneous responses align with theoretical predictions, which suggest that STW is more effective for firms facing large and unexpected productivity shocks.

4.1 Introduction

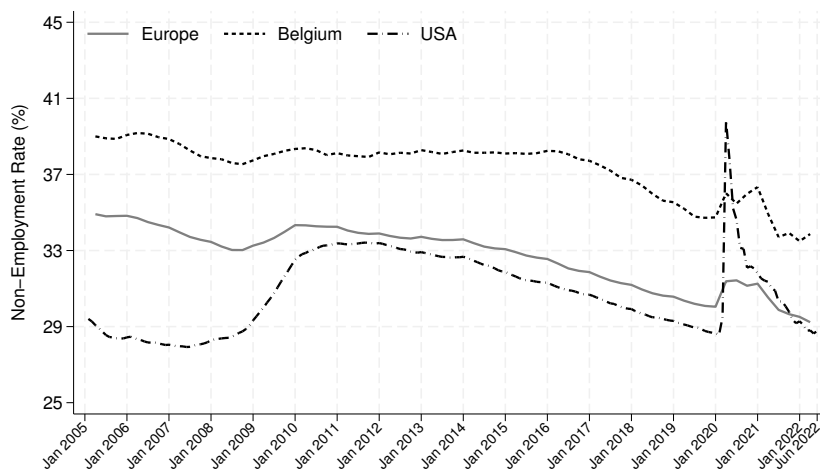
The COVID-19 pandemic led to unprecedented government-mandated lockdowns and social distancing measures, significantly disrupting numerous economic sectors. Timely government interventions were crucial in mitigating employment fluctuations globally. Countries adopted various labor market strategies based on their institutional frameworks. For instance, the U.S. expanded support for the unemployed through extended unemployment benefits, temporary layoffs,¹ and payroll subsidies, while countries such as Belgium, France, Germany, Italy, and Spain prioritized preserving employer-employee relationships via Short-Time Work (STW) programs, each with distinct designs. STW allows employers to reduce working hours during downturns, with employees receiving partial compensation for the reduced hours. A key distinction between STW and temporary layoffs is that STW maintains the employment contract, thereby limiting job destruction and preventing firm closures (Albertini et al., 2022). This approach may help explain why employment fluctuations were more subdued in Europe compared to the U.S. (Giupponi et al., 2022) (see Figure 4.1).

This paper evaluates the effects of STW on employment and firm bankruptcy during the pandemic in Belgium—a period marked by government-mandated lockdowns and historically high STW take-up rates. In March 2020, Belgium introduced a generous² simplified STW procedure that substantially reduced the administrative burden, aiming to improve coverage and expedite support across a broad range of sectors. Unlike other OECD countries—such as France, Spain, and Germany—which constantly modified their STW programs in response to the evolving health crisis,³ Belgium implemented only one temporary change in September 2020 during the first three years of the pandemic.

¹This policy allows the employer to temporarily suspend the employment relationship. In principle, temporary layoff policies enable dismissed workers to be rehired by their original employers (Feldstein, 1976).

²Generosity accounts for both eligibility requirements and the subsidy level through the replacement rate. According to Calligaris et al. (2023), Belgium had one of the most generous schemes at the crisis peak and throughout the first two years of the pandemic.

³Adaptations included employer cost-sharing, tighter eligibility criteria for firms and workers, and sector-specific duration caps.

Figure 4.1. Non-employment rate evolution, 2005–2022.

Notes: This figure reports the evolution of the non-employment rate, defined as one minus the employment rate. It therefore includes both the unemployed and those out of the labor force. Employment data are drawn from the OECD. This is an updated version of Figure 1b in Giupponi, Landais, and Lapeyre (2022), now including Belgium.

To estimate the impact of STW on firm-level employment dynamics and risk of bankruptcy, I use a novel quarterly administrative dataset and implement a Fuzzy Regression Discontinuity Design (RDD), exploiting variation in eligibility conditions induced by the September 2020 policy reform. This reform restricted access to the simplified STW procedure for firms and workers not directly affected by lockdowns—but only for one month. Access was then re-expanded from October 2020 to June 2022 with the onset of the second COVID-19 wave. Specifically, in September, simplified STW was available to firms that used the program for more than 20% of contractual days during the peak crisis (2020-Q2). Additionally, workers in lockdown-affected sectors, as designated by the Minister of Employment, retained automatic access. Firms not meeting the 20% threshold could still apply but under the traditional and more stringent approval process—particularly for white-collar workers.⁴ This framework introduces

⁴For white-collar workers, eligibility required demonstrating a formal quarterly turnover decline relative to the same quarter in the previous year, based on a rolling reference window covering the four quarters prior to the intended start of STW use.

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the “fuzziness” to the design.

Given that not all eligible firms took up the program—or enrolled all their workers—I estimate the Local Average Treatment Effects (LATE) for firms near the 20% threshold. The within-firm variation in worker eligibility enables the identification of treatment effects for two groups: (i) partially lockdown-affected firms—those with workers in lockdown-affected sectors and others eligible under the 20% rule—and (ii) lockdown-unaffected firms, composed entirely of workers not directly affected by lockdowns. This classification permits a test of theoretical predictions that STW is most effective in firms experiencing the most severe productivity shocks—typically those operating below full capacity during the pandemic (Cahuc, 2024; Cahuc et al., 2021; Diaz et al., 2025). The design’s validity is supported by continuity in firm density around the 20% threshold and placebo tests showing smooth pre-reform STW take-up behavior across the cutoff.

I find that simplified STW access increased take-up by 11 percentage points (p.p.) relative to ineligible firms, but only partially lockdown-affected firms experienced significant employment gains—ranging from 48% to 58% in the quarter of take-up. At the intensive margin, a 10 p.p. increase in STW days is associated with a 25% increase in jobs relative to the counterfactual. These job-saving effects persist up to two years post-treatment. By the end of 2022, treated firms reported 31% to 50% higher total volume of work relative to their counterfactuals. STW appears most effective for firms at high risk of job destruction, particularly those partially affected by lockdowns—an empirical channel largely unexplored in the current COVID-19 literature. However, I find no significant effects on firm bankruptcy within two years of treatment for either firm type. Importantly, during the COVID-19 period, moratorium procedures were in place: the tax administration and ONSS/RSZ were not permitted to declare firms bankrupt due to unpaid taxes or social security debts until July 2022. After this date, the number of bankruptcies began to rise. This suggests that firm bankruptcies were likely influenced by the institutional framework in effect during that period, and that survival effects may only manifest over a longer time horizon.

Motivated by the differential restrictiveness of the September reform for firms with large white-collar workforces located below the 20% cutoff, I examine heterogeneous effects by the share of white-collar workers. This analysis is

informed by theory predicting that labor hoarding is most efficient in firms with high adjustment costs and accumulated firm-specific human capital—i.e., white-collar-intensive firms (Biddle, 2014). Identifying which types of workers benefited most helps inform how STW design can be refined to maximize effectiveness. The results shows that among partially lockdown-affected firms, those with above-median shares of white-collar workers located just above the 20% cutoff were more likely to take up STW than their counterparts just below the cutoff—a pattern consistent with the policy being more binding for these firms. This prediction, however, does not hold for lockdown-unaffected firms, where take-up was higher among firms with below-median white-collar shares. Alternative strategies such as teleworking and task restructuring, together with a less intense shock for the latter group of firms, might explain this divergence.

In terms of intensity of use, firms with lower shares of white-collar workers—regardless of lockdown exposure—showed a larger discontinuous increase in the fraction of days in STW at the cutoff. This suggests that the simplified STW procedure was used more intensively by firms with higher shares of blue-collar workers, independent of whether they were directly affected by lockdown restrictions. While the higher usage in these firms can partly be attributed to the nature of blue-collar jobs—which are less amenable to remote arrangements and more likely to require physical presence—the significant differential in STW days suggests that administrative simplification itself may have incentivized overuse. If the shock alone were driving take-up, similar STW use levels would be expected under the alternative STW scheme, which imposed significantly lower administrative costs on blue-collar-intensive firms. The evidence, however, points to the generous design of the simplified procedure as a key driver of intensive STW utilization in blue-collar-intensive firms.

Heterogeneity also emerges in the effectiveness of STW across firms with different white-collar workers shares. Among partially lockdown-affected firms, STW preserved both employment and total volume of work in firms with above-median white-collar worker shares over the short and medium term. In contrast, blue-collar-intensive firms,⁵ although they also benefited in terms of headcount in the medium term, did not experience gains in total

⁵This set of firms is analyzed using the intensive margin treatment, which, by being unconditional on STW take-up, reflects a combination of extensive and intensive margin treatment.

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volume of work by the end of the period—consistent with a more persistent reduction in hours per worker. For lockdown-unaffected firms, medium-term employment gains were observed only in white-collar-intensive firms, although without increases in total volume of work. Fully blue-collar firms in this group—analyzed separately—show no employment effects, suggesting that this group is the main driver of the significant deadweight losses of the program, resulting in insignificant average employment effects for lockdown-unaffected firms. Interestingly, this group comprises firms more familiar with STW use prior to the pandemic, particularly in sectors such as construction, manufacturing and transportation (Bermúdez et al., 2025; Tarullo, 2025). Together, these findings highlight the critical role of STW design in shaping both the effectiveness and fiscal efficiency of STW programs.

While the positive impact on employment in firms in non-essential sectors closed due to administrative lockdowns is evident (Cahuc, 2024), this study focuses on firms that were partially affected by such measures. Although some studies highlight STW’s effectiveness in preserving employment during the pandemic, others indicate that these employment gains were accompanied by significant deadweight losses, especially during periods of generous and simplified STW programs with high take-up rates (Albertini et al., 2022; Brinkmann et al., 2024). This paper complements the previous existing research.

Although there are empirical and theoretical contributions that have studied the effects of STW during the Great Recession, there is a dearth of evidence for the COVID-19 period. Robust causal impact evaluations for the Great Recession leverage variation in take-up rates across firms driven by differences in firm eligibility (Giupponi and Landais, 2023) and idiosyncratic institutional approval rates in the aggregate (Kopp and Siegenthaler, 2021) and across locations (Cahuc et al., 2021). These studies offer important lessons, which depend on the program design features and the persistence of the economic shock in each setting.

Firstly, in France, STW was found to be effective at saving jobs in the short term (i.e., during STW treatment) and in the longer run (i.e., after treatment), but only for firms that faced temporary and large economic shocks, while no benefits were exhibited for firms experiencing milder demand shocks

(Cahuc et al., 2021). In particular, the protection of jobs in the latter group determines the extent of the program’s deadweight losses. For instance, if STW protects mostly jobs that are not at risk of being destroyed, or in firms whose productivity shock is sufficiently small, it reduces excessive hours of work without positive effects on headcount employment. For Belgium, Bermúdez et al. (2025) reaches a similar conclusion. Headcount employment gains were concentrated in sectors that were hardest hit by the economic shock, such as manufacturing. Although non-manufacturing sectors used the program, this use did not lead to effective preservation of headcount employment, increasing the overall fiscal cost of the program.

Secondly, the persistence of employment effects depends on the characteristics of firms selected by the program and the nature of the economic shock. The benefits in terms of job savings are not sustained after STW treatment when the program protects mostly low-productivity firms during a persistent economic shock, as shown by Giupponi and Landais (2023) for the Italian STW program during the Great Recession. However, when institutional design features target productive⁶ and temporarily affected firms, STW has proven effective not only in the short run but also with persistent effects after treatment, as Kopp and Siegenthaler (2021) finds for Switzerland during the Great Recession. These findings altogether highlight a trade-off between offering extended coverage to secure the largest number of jobs at risk of being destroyed in the economy and constraining behavioral responses from a generous government transfer, such as excessive use of the program.

While the effects of STW on job savings and avoiding bankruptcies are recognized to be positive during the COVID-19 period, especially for firms closed because of administrative lockdown restrictions (Cahuc, 2024), there are very few robust impact evaluations backing up these effects. In particular, very little is known regarding the size and dynamics of these positive effects after treatment during the COVID-19 period. Even though some studies recognize the positive effects of STW without raising much concern about the overall effectiveness of the program (Bennedsen et al., 2023; Benkovskis et al., 2023; Montenegro and Hijzen, 2024), another set of studies acknowledges that these positive effects came at the cost of large deadweight losses arising from

⁶Based on past financial indicators.

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behavioral responses of firms (Albertini et al., 2022; Brinkmann et al., 2024; Lapeyre, 2024; Kagerl, 2024).

Overall, the evidence investigating the effectiveness of STW programs is still relatively scarce for the COVID-19 period due to at least three factors. First, the introduction of simplified procedures and increased coverage reduced the sources of variation in program design features that allow for the identification of causal responses, making it more challenging to evaluate STW programs during the COVID-19 period compared to other periods when variation in eligibility was more likely. Second, even when there is variation in firm-level eligibility to STW programs, this variation often results from permanent policy reforms that apply sequentially. As a result, firms are assigned to treatment based on past policy changes, creating path dependency in the assignment variable. This complicates the estimation of dynamic treatment effects, as researchers must account not only for the immediate impact of the policy but also for its lagged effects over time. Third, high-frequency administrative microdata needed to evaluate firm and worker responses are not easily available and are subject to personal data protection rules.

In this paper, I address these challenges. First, the Belgian COVID-19 setting offers a credible and unexpected quasi-exogenous source of variation in eligibility for the simplified STW procedure across firms. The eligibility rule that introduces variation in simplified STW access is based on past administrative information, which cannot be manipulated. This contrasts with Lapeyre (2024), who provides evidence of manipulation of self-reported information to qualify for the more generous STW program in France during the COVID-19 pandemic. Second, the policy change that I exploit occurs only during one month in September and was abolished in October 2020, which facilitates the identification of dynamic treatment effects and makes path dependency less likely. Third, this is the first study that exploits administrative quarterly granular firm-level microdata on STW programs in Belgium, the country with a large history of STW use—even outside recessions. Previous studies on the Great Recession rely on yearly data, which do not allow for capturing timely responses in employment thoroughly. Fourth, I am able to retrieve firms' past financial characteristics by merging social security information with annual accounts and VAT records, which allows to characterize firms selected into the program.

4.2. The Belgian STW Program During the COVID-19 Crisis

The remainder of this paper is organized as follows. Section II provides details of the Belgian STW program during the COVID-19 period, including the different discretionary changes that took place in 2020 as a response to the pandemic. Section III details the different sources of administrative data and describes the sample used in the RDD. Section IV describes the RDD method used to evaluate the policy instrument. Section V presents the key empirical findings, and Section VI concludes.

4.2 The Belgian STW Program During the COVID-19 Crisis

This section first describes the main principles of STW programs in Belgium, then outlines their evolution during the onset of the pandemic. Finally, it provides insights into the policy reform that I will exploit to identify exogenous variation in the subsequent analysis.

STW in Belgium is a well-established program, formally introduced after the Second World War as part of the unemployment insurance system. Initially restricted to blue-collar workers,⁷ the program was expanded in 2012 to cover white-collar workers, subject to more stringent eligibility requirements. These requirements included the submission of formal evidence of declining turnover and the preparation of a business recovery plan to be implemented once economic conditions improved.

As in other countries, three core principles underpin Belgium's STW programs: temporariness, expediency, and government subsidization (Bermúdez et al., 2023). First, STW allows employers to reduce working hours, conditional on a temporary decline in revenues or demand, typically driven by cyclical downturns. These reductions can extend to a complete cessation of work hours. Second, employees are rapidly and partially compensated for reduced hours. Prior to the COVID-19 crisis, employees received 65% of their gross wage (i.e., the replacement rate), paid by the

⁷Distinctively, the Belgian employment legislation differentiates between blue- and white-collar workers. Although this distinction has been largely harmonized since 2014, it remains a relevant criterion for STW eligibility. Classification depends on whether the worker primarily performs manual or intellectual tasks. This distinction is enforced by payroll agencies and monitored by the Federal Public Service for Employment and the social security authorities.

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government. This applied up to a cap equal to 1.6 times the minimum wage (€2,619 per month in 2019).⁸ For wages above this cap, the replacement rate declines, resulting in greater earnings losses for higher-wage workers. During the COVID-19 crisis, the replacement rate increased to 70% of gross wages, and the maximum benefit rose to €2,754 per month. Importantly, this benefit was not exempt from income tax and social insurance contributions, although the burden was lower for low-wage workers. As a result, the replacement rate reached 78% of net wages for workers earning 67% of the average gross wage, and 65% for those earning the average gross wage (BNB, 2020). Third, during the COVID-19 crisis, firms incurred no costs when using the program.⁹

At the onset of the crisis, during the first lockdown, a simplified STW procedure was introduced in March 2020 to address rising infections and lockdown measures, replacing the pre-existing system. This simplified procedure featured a higher replacement rate, no requirement to demonstrate a decline in activity, no cost to employers, and no approval process. Unlike other European countries—such as Germany, France, Switzerland, Spain, and Italy—where simplified and generous STW programs were gradually adjusted in response to the evolution of infections and lockdown measures, Belgium implemented only one temporary policy change in September 2020.¹⁰ However, this restriction was short-lived, and the initially introduced simplified procedure remained in place without any restriction from October 2020 until June 2022, when the program returned to its pre-crisis design. The continued relaxation of the program was motivated by the resurgence of infections during the second wave.

In other countries, adjustments typically involved tightening eligibility criteria or increasing employer contributions. In Belgium, all employers could access the simplified procedure without eligibility requirements, except during September. To participate, firms were required to submit a monthly form declaring the number of hours per day subsidized by the program for each worker. Moreover, while pre-pandemic rules imposed stricter eligibility

⁸Assuming the intersectoral minimum gross wage of €1,625.72 per month (Banque Nationale de Belgique [BNB], 2020).

⁹Employers could top up salaries, and these top-ups were not subject to Social Security Contributions.

¹⁰The policy change in September was announced on July 17. See Article 15, Belgian Official Gazette, 17 July 2020.

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conditions for white-collar workers compared to blue-collar workers—for instance, requiring proof of a formal decline in turnover—the simplified procedure did not differentiate between worker types.

In September 2020, as infections declined and the economy experienced a partial recovery, eligibility for the simplified procedure was restricted to firms and workers particularly hit by the crisis. On the one hand, workers in specific ‘joint committees’ (JCs)¹¹ performing non-essential activities—systematically shut down during lockdowns where safety measures or telework could not be implemented—remained eligible for the simplified procedure without additional requirements. On the other hand, firms that had used STW for more than 20% of contractual days at the peak of the crisis (2020-Q2) were automatically eligible for the simplified program.

The list of JCs classified as those the most affected by lockdown measures was explicitly defined in a Ministerial Decree.¹² Workers in these JCs accounted for 38% of total employment and were primarily concentrated in manufacturing (21%), wholesale and retail trade (16%), and accommodation and food services (12%). For all other workers—those not included in the list of protected sectors—firms were required to submit a formal request to participate in the simplified program, demonstrating that they had used STW for more than 20% of contractual working days during the second quarter of 2020, as declared to the National Social Security Office (ONSS/RSZ). The main causal analysis focuses on firms employing workers not covered by the list but eligible for STW if they satisfied the firm-level 20% eligibility criterion. Specifically, these workers were eligible for the simplified procedure only if their employer met the 20% threshold. The variation in eligibility across workers within a firm allows for the examination of STW effects on two types of firms: (i) partially lockdown-affected firms—those with workers in lockdown-affected sectors and others only eligible under the 20% rule—and (ii) firms with only workers in lockdown-unaffected sectors. As shown below, STW take-up effects differ significantly between these two groups.

¹¹A Joint Committee (JC) is a sector-specific body in Belgium composed of representatives from both employer and employee organizations. These committees are responsible for negotiating collective labor agreements (CLAs) that establish employment conditions, including wages, working hours, and other benefits, for all workers and employers within their respective sectors. The agreements reached by JCs are binding and apply to all entities within the sector, ensuring standardized labor conditions across the industry.

¹²See [Ministerial Decree of September 10, 2020](#).

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Firms located below the 20% threshold could not access the simplified STW program in September for lockdown-unaffected workers. These employers could only apply for the STW scheme for economic reasons, which was reintroduced from September to December 2020. However, access to this program was conditional on meeting additional requirements for both blue- and white-collar workers. For blue-collar workers, firms had to submit a form citing economic justification. For white-collar workers, they were required to demonstrate a turnover decline of at least 10% compared to the same quarter of the previous year. In both cases, approval by the local unemployment office was mandatory. In practice, take-up of the economic reasons program was low, covering less than 1% of total FTEs across all types of STW¹³ during this period.

4.3 Data

This section describes the main data sources used in the analysis, outlines the characteristics of the sample selected for implementing the identification strategy, and presents descriptive statistics for the firms analyzed in this paper.

The data consist of all single-establishment legal entities in the private sector with at least five employees at the end of at least one quarter between 2003 and 2022. These entities are drawn from three administrative sources, linked through unique firm identifiers. The restriction to legal entities and the exclusion of smaller firms are motivated by privacy concerns. The final sample accounts for 60% of total private-sector employment and 70% of jobs (in full-time equivalents) in STW during the period 2018–2022.

National Social Security Office (ONSS/RSZ): this dataset provides quarterly information at the firm and worker-type level regarding employment and STW statistics. This data is collected through the DmfA declaration,¹⁴ which employers are required to submit at the end of each quarter. Specifically, the dataset contains information on the volume of work in FTE, headcount employment at the end and during the quarter, gross wages, and labor costs. It also includes detailed information on the use of STW programs, such as

¹³Other types of STW, apart from economic reasons and the simplified procedure for corona reasons, include STW for weather conditions and for strikes or lockouts.

¹⁴‘Déclaration multifonctionnelle’ in French.

the volume of work in FTE under STW, the number of jobs and days in STW, and the type of STW program used. Data are grouped at the level of worker-types, defined as the combination of various worker characteristics within each firm. This allows retrieving employment and STW information relative to the combination of different categories of workers, including the type of task performed (blue-collar or white-collar), the type of working-time contract (full-time, part-time, or other¹⁵), sex (female or male), age group (30 years old or less, between 31 and 54, and 55 or older), and sectoral affiliation or JC. This information is essential to determine a firm's eligibility for the simplified STW program according to the cutoff rule exploited in this study. Additionally, this dataset provides the total number of contractual working days declared in 2020-Q2 by establishment, which was used to apply the eligibility rule introduced in September 2020.

National Office of Employment (ONEM/RVA): this dataset contains monthly data on STW take-up between March 2020 and June 2022. It includes the number of days in STW, the amount of subsidies received per firm, and a categorical variable that indicates the type of STW the firm participated in (i.e., for economic or corona reasons). However, this dataset does not contain STW statistics across sectoral JCs as in the quarterly ONSS/RSZ dataset. Importantly, these statistics reflect the initial reports submitted by firms prior to any verification process. This allows for the identification of firms' immediate responses, particularly in September 2020. Verified statistics, available in the ONSS/RSZ dataset, are reported only on a quarterly basis. Importantly, both sources provide similar information for the key period analyzed, namely 2020-Q3. Both datasets are combined throughout the empirical analysis for the treatment definition.

National Bank of Belgium (NBB): Two distinct datasets from the NBB are used. The first consists of data from firms' quarterly and monthly VAT declarations,¹⁶ which contain information on turnover, input costs, and investments at the firm level. Firms exempt from VAT declarations—such as those with annual turnover below €25,000 or those that are part of VAT units

¹⁵Other working-time contracts include: special contracts (i.e., temporary agency work, seasonal work, casual work in agriculture and horticulture and casual work in the hotel and catering industry) or statutory civil servant contracts in the public sector.

¹⁶The information of firms subject to monthly reporting is aggregated at the quarterly level.

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with consolidated reporting—are not included in this dataset. Overall, more than 80% of firms selected for the analysis report VAT data. The second dataset contains detailed information from firms' annual accounts, available from 2018 to 2022. Although the majority of Belgian companies are required to file annual accounts, some exemptions apply.¹⁷ On average, approximately 88% of firms in the sample filed annual accounts at least once between 2018 and 2022.¹⁸ From these data, I retrieve financial indicators including liquidity (proxied by the sum of cash and short-term investments), leverage (measured as the ratio of short- and long-term debt to total assets), and profitability (proxied by the Return on Assets, ROA). In addition, two measures of labor productivity are constructed: turnover per worker and value-added per FTE. Value-added is calculated from VAT data as total turnover minus intermediate inputs or from annual accounts as total revenues plus unsold stock minus the cost of goods and services used in production.¹⁹

Together, these datasets provide comprehensive information on employment, STW participation, and firm performance. They are crucial for characterizing firms' financial positions and employment trajectories before, during, and after the implementation of STW policies during the COVID-19 crisis.

¹⁷For instance, sole proprietorships, small businesses whose partners have unlimited liability, large businesses whose partners have unlimited liability (provided none of the partners is a legal entity), agricultural companies, some hospitals, and some professional associations, schools and institutions of higher education, are not obliged to file annual accounts and in certain cases, they have to submit instead a social balance sheet.

¹⁸The reporting coverage of annual accounts is slightly broader than that of VAT data, as nearly all legal entities are required to file annual accounts. In contrast, not all legal entities are obligated to register for VAT. Businesses engaged in VAT-exempt activities—such as certain educational, medical, financial, and cultural services—are not required to register. Additionally, small businesses below specific turnover thresholds (i.e., €25,000 in annual turnover) are also exempt from VAT registration. It is worth noting that non-missing turnover data are more prevalent in VAT declarations, as turnover is a mandatory field for firms filing VAT returns, but not for those submitting annual accounts.

¹⁹Since total revenues is not a mandatory field for all firms declaring annual accounts, I use gross margin which corresponds to the value of revenues net of cost of goods (excluding labour) sold. In other words, I approximate value added as gross margin minus labour cost plus unsold stock.

Sample Selection

I select firms that report a positive number of employees at the end of each quarter in the ONSS/RSZ dataset consecutively from 2018-Q3 to 2020-Q3. In other words, firms in the sample remained continuously active throughout the period from 2018-Q3 to 2020-Q3.²⁰ Within this set, firms are grouped into three categories based on their sector composition and eligibility under the rule introduced in September 2020:

(1) Fully lockdown-affected firms: All employees of these firms are represented by JCs that were highly affected by lockdowns. As a result, these firms were automatically eligible for the simplified procedure throughout the entire period from 2020-Q2 to 2022-Q2, since all their workers belonged to the list of lockdown-affected sectors. No application or approval was required for this group. Because these firms were continuously eligible for the simplified procedure—before, during, and after September—they are excluded from the main analysis and instead retained for placebo tests.

(2) Partially lockdown-affected firms: These firms employ a mix of workers. Some employees belong to the list of lockdown-affected JCs and were automatically eligible for the simplified procedure. Others, not covered by the list, were subject to the firm-level 20% cutoff rule to access the simplified STW procedure.

(3) Lockdown-unaffected firms: These firms do not employ any workers included in the list of lockdown-affected JCs. As a result, they were eligible for the simplified STW procedure only if they reported more than 20% of total working days in STW (for economic or corona reasons) during 2020-Q2.

Table 4.1 describes the number of firms in each of these categories. Firms in group (1), representing 42% of the total, are not the focus of the main analysis, as they were not subject to the 20% cutoff rule that generates variation in eligibility for the simplified procedure. The analysis therefore focuses on the remaining 58% of firms in the sample.

I focus on two types of firms: (i) partially lockdown-affected firms

²⁰This implies that firms must be active in September 2020 for the policy reform to apply to them.

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(representing 19% of firms in 2020-Q3) and (ii) lockdown-unaffected firms (39% of firms in 2020-Q3).

Table 4.1. Number of firms in selected sample

Group	Firm types	Cut-off rule	Number of firms selection
1	Fully lockdown-affected firm	No	38,375
2	Partially lockdown-affected firms	Yes	17,530
3	Lockdown-unaffected firms	Yes	35,566
Total			91,784
Total of groups 2 and 3			53,096

Notes: This table reports the number of firms with positive employment at the end of each quarter consecutively from 2018-Q3 to 2020-Q3, disaggregated by firm eligibility type. Groups 2 and 3, which together account for 58% of firms, correspond to the firms analyzed in this paper.

Running variable, treatment and outcomes

The running variable used in the subsequent analysis described in Section 4.4, is constructed from ONSS/RSZ records on firm-level STW use in 2020-Q2. Specifically, I calculate the ratio between the number of days in STW (for both economic- and COVID-related reasons)²¹ and the total number of contractual working days declared by employers in 2020-Q2, multiplied by 100%. I re-center the 20% threshold to zero such that the cutoff is 0%. The minimum and maximum value of the cutoff are -20% and 80% since there are no firms that report more than 100% of working days in STW.

I evaluate two treatment definitions at the firm level. The first captures the extensive margin of STW take-up in September, based on monthly ONEM/RVA statistics. It equals one if the firm claimed at least one day of STW (for either economic or corona-related reasons) during September, and zero otherwise. The second captures the intensive margin, measured as the fraction of days in STW relative to total contractual days. This measure can be constructed from both monthly and quarterly statistics. In Section 4.5, I show that estimates based on monthly and quarterly data using the fraction

²¹The official rule considers both types of programs for the definition of the running variable.

of days in STW as the treatment are similar. The main analysis relies on the monthly definitions for September; however, I conduct placebo tests using the quarterly data to demonstrate that measurement error in these statistics is negligible. Table B.1 summarizes the details of these definitions.

In terms of outcomes, since STW is a job-match-specific policy, I explore firm-level employment responses to STW take-up by examining employment outcomes within firms. Specifically, I define employment outcomes at the cell level, within firms. A cell is defined as a particular combination of worker characteristics such as age, sex, worker type (blue-collar or white-collar), working-time contract type, and the JC to which the worker belongs. The latter characteristic enables the identification of the eligible population affected by the September 2020 policy change. Moreover, this cell-level analysis allows for a precise decomposition of the employment effect of STW into changes in the volume of work per worker (i.e., the intensive margin response) and changes in headcount employment (i.e., the extensive margin response). Although this granular analysis ignores potential spillover effects within firms, sensitivity analyses are conducted by collapsing the data at the firm level and assessing the impact on workers not directly affected by the policy reform but employed in partially lockdown-affected firms, using cell-level data (see discussion in Section 4.4.3).

First, to assess whether treatment intensity differs across either side of the threshold, I examine firms' responses in first-order outcomes that capture intensive-margin adjustments following STW take-up. These outcomes include: (1) the volume of work (in FTE) per worker (in logs),²² and (2) the wage bill per worker (in logs). Firms that take up STW are expected to reduce the average volume of work per worker as a mechanical effect of the program utilization, which subsidizes reductions in working time. Similarly, the average wage bill per worker is expected to decline because the government partially subsidizes the reduction in working time.²³

Second, to examine firms' behavioral responses, I evaluate total

²²Sensitivity analyses are also conducted using alternative definitions of the outcomes—for example, using untransformed outcomes or outcomes normalized relative to the average firm level in 2020-Q1, two quarters prior to the policy change.

²³The subsidized amount is directly paid to the worker and it is not reported as part of firm-level wages. The employee bears the loss resulting from the partial subsidization.

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employment effects, measured as the total volume of work in FTEs (in logs) and the total number of jobs (in logs) at the cell level within firms. Finally, I assess the effect of STW on the likelihood of bankruptcy following treatment—that is, for firms that remained active up to 2020-Q3. This outcome is defined at the firm level and takes a value of one if the firm exits or reports zero employment at the end of a quarter in the ONSS/RSZ statistics, and zero otherwise. I focus on bankruptcy outcomes after 2020-Q3, since the sample is selected conditional on firms surviving up to that point in time.²⁴ Note that for the survival analysis, the dataset is collapsed at the firm level rather than at the cell level. The evaluation of STW effects on other performance indicators is left for future research.

Descriptive statistics

Figure A1 in the Appendix illustrates the overall evolution of STW subsidy expenditure from March 2020 to June 2022. Total expenditure includes subsidies for both corona-related and economic reasons. STW spending peaked in April 2020 at €800 million, before falling sharply—by a factor of eight—by September 2020. Expenditure rose again during the second wave of infections, doubling relative to September levels. After April 2021, spending gradually declined, although it remained above pre-crisis levels through the end of 2023 (not shown in the figure). In 2021, subsequent COVID-19 waves occurred from March to June 2021 and from November 2021 to January 2022. The latter, which was the fifth wave of the pandemic, concluded at the end of February 2022. After this period, smaller waves continued until mid-June 2023. COVID-19 waves after 2020 were associated with significantly lower STW take-up rates compared to earlier waves.

Table B.2 (in Appendix) shows descriptive statistics for the firms included in the evaluation sample—those close to the 20% cutoff in 2020-Q3. For comparison purposes, it also shows descriptive statistics for all firms (around the 20% cutoff) including those not considered in the evaluation sample. Firm selection depends on the bandwidth chosen, which is discussed in the next

²⁴Sensitivity analyses were conducted by selecting firms that were active up to 2020-Q2. However, the results remain similar to those obtained using the current sample selection. Moreover, since firms in STW are more likely to survive during take-up—given that bankrupt firms are not eligible for STW—the effect on bankruptcy has been assessed in periods following take-up in the literature (Giupponi and Landais, 2023; Kopp and Siegenthaler, 2021).

section (Section 4.4). I report statistics using a bandwidth of 5 p.p. on each side around the threshold, which is close to the MSE-optimal bandwidth on each sample.

Overall, Table B.2 shows that firms just below (i.e., ineligible) and just above (i.e., eligible) the 20% threshold are similar in terms of observable characteristics in both groups of firms. Partially lockdown-affected firms are mostly concentrated in construction (42%), retail and transportation (36%),²⁵ and manufacturing (11%). Lockdown-unaffected firms are also concentrated in retail and transportation (31%), but have a higher representation in professional, scientific, education, security, and human health activities (23%) as well as in financial and insurance activities (5%). By contrast, they are less represented in construction (22%) relative to partially-lockdown affected firms.

Although lockdown-unaffected firms include a higher share of smaller firms, their sectoral distribution more closely resembles that of the overall sample than that of partially lockdown-affected firms. In contrast, partially lockdown-affected firms are larger on average and concentrated in more blue-collar-intensive sectors, with higher shares of blue-collar and male workers. Full-time contracts are also more common among these firms. Lockdown-unaffected firms mirror the overall sample in most employment outcomes but exhibit lower wage rates and labor productivity (measured by turnover per worker). By contrast, partially lockdown-affected firms—employing more workers in both headcount and FTE terms—report higher wage rates and labor productivity than the overall sample.

Firms taking up at least one day of STW (treated group) are more prevalent in manufacturing and less so in financial and insurance activities—sectors where telework was more common after April 2020. They are also underrepresented among very small firms (fewer than 10 employees) and are, on average, larger, with 52 employees compared to 32 and 19 among eligible and ineligible firms in partially and lockdown-unaffected groups, respectively. Treated firms tend to have higher wage bills and turnover but lower labor productivity (turnover per worker), likely due to reduced activity and working hours. Their workforce is more male-dominated and slightly

²⁵The representation of firms in food services is very small in this group compared to firms that are fully affected by lockdown measures.

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more concentrated among workers aged 55+ with full-time contracts—especially in lockdown-unaffected sectors—consistent with STW’s goal of preserving firm-specific human capital. On average, treated firms used STW for 42 days per quarter, representing 9

4.4 Identification Strategy

This section describes the identification strategy, based on a fuzzy Regression Discontinuity Design (RDD). It is divided into three parts. The first part outlines the identification strategy used to estimate the causal effects of STW on firm outcomes in the short run. The second discusses the identification of dynamic effects in Belgium during the COVID-19 crisis. Finally, I describe several validity and sensitivity checks that support the validity of the RDD in this evaluation.

4.4.1 A Fuzzy Regression Discontinuity Design

Identifying the causal effect of STW on firm outcomes is challenging, as firms that select into the program are likely to differ from non-participants on both observable and unobservable characteristics. These differences are plausibly correlated with both STW take-up and the outcomes of interest. For example, firms opting to take up STW may already be underperforming relative to others and more likely to experience poor outcomes. Simple comparisons between users and non-users would therefore lead to biased estimates.

To overcome this challenge, I leverage quasi-experimental variation stemming from a temporary change in firm eligibility for the simplified STW program, which occurred in September 2020. As described in Section 4.2, workers in lockdown-unaffected sectors could access the simplified program only if their firms had used STW (for either economic or corona reasons) for more than 20% of total contractual working days in the second quarter of 2020. Firms below this threshold were not automatically eligible. They could only apply for the pre-existing STW scheme under a more burdensome approval process,²⁶ requiring written justification, supporting documents, and authorization from the unemployment agency. This procedural difference created variation in the propensity to take up STW. I employ the fuzzy RDD

²⁶The pre-crisis approval process was more burdensome for enrolling white-collar workers.

4.4. Identification Strategy

that exploits the 20% threshold in STW use during 2020-Q2 to identify treatment effects on firm outcomes.

The design compares firms with similar percentages of STW use in 2020-Q2 that narrowly fall on either side of the 20% cutoff, resulting in different eligibility conditions in September (2020-Q3). Firms just above the threshold were eligible for the simplified STW procedure, while those just below were not. As shown in Section 4.3, these firms are otherwise very similar across observable characteristics. The running variable is continuous, based on the ratio of STW days to total contractual working days, and is determined at the firm level. Importantly, the running variable is measured prior to the September policy change. For firms close to the threshold, eligibility is as good as random, minimizing concerns about unobserved differences.

The fuzziness of the RDD arises for several reasons. First, compliance with non-eligibility is imperfect: some non-eligible firms still benefited from STW through the classic program for economic reasons. Second, the available data complicates the precise measurement of actual treatment. ONEM/RVA monthly statistics do not include information on the joint committee to which workers belong, preventing a full separation of workers exclusively affected by the policy change. Quarterly data allow for this distinction but do not capture extensive margin (take-up) responses with sufficient precision for the month of September. Finally, and most importantly, take-up decisions are made at the firm level in response to economic conditions, so not all eligible firms necessarily participate or enroll all of their workers. To account for imperfect compliance, I use an instrumental variable estimator, scaling the Intention-to-Treat (ITT) estimate by the difference in the probability of STW take-up or the fraction of days in STW at the cutoff.

First, I identify the effect of being above the cutoff on quarterly outcomes defined at the end of 2020-Q3, estimating the following reduced form regression:

$$y_{ic} = \delta + \theta^{ITT} D_{ic} + \lambda_1 z_i D_{ic} + \lambda_2 z_i + u_{ic} \quad (4.1)$$

where y_{ic} corresponds to employment outcomes (i.e., log of volume of work per worker, log of jobs, log of volume of work, wage rate in logs, gross wage bill

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per worker in logs) in cell c of firm i . This allows to decompose the effect on employment at the intensive and extensive margin on each cell. y_{ic} is replaced by y_i when evaluating the effect on the firm likelihood of bankruptcy for a firm i .²⁷ $D_{ic} = \mathbb{1}[z_i > 0 \wedge c \in \mathbb{W}]$ is an indicator variable for being above the cutoff, therefore being eligible to use the simplified STW procedure for a cell c belonging to the set of lockdown-unaffected workers W .²⁸ The associated parameter θ^{ITT} is the effect of eligibility on the treatment at the cutoff. $z_i = f(x_i - 20\%)$ defines the re-centered to zero running variable at the firm-level, where x_i is the running variable as a percent of STW use (in days) from the total working days in 2020-Q2, and $f(\cdot)$ represents a linear function. $\lambda_1 z_i D_{ic}$ and $\lambda_2 z_i$ capture a linear relationship between the outcome and the running variable on the right- and left-hand side of the cutoff, respectively. μ_{ic} is the idiosyncratic error component clustered and robust to heteroskedasticity at the firm level.

Since I want to estimate the marginal treatment effect of STW take-up (induced by the policy change) on the outcomes at the cutoff z_i , I scale the ITT effect by the effect of differential eligibility on STW take-up (θ^{FS}), replacing y_{ic} by STW_i in Equation (4.1).

Under the assumptions of local continuity and local mean independence, the fuzzy RDD identifies a Local Average Treatment Effect (LATE). I adopt the weaker Local Instrumental Variable (LIV) assumption that replaces local mean independence by the exclusion restriction, random assignment at the cutoff, and monotonicity, as discussed by Hahn et al. (2001) to identify a LATE at the cutoff. The local LATE can be seen as the Wald estimator (i.e., θ^{RF}/θ^{FS}).

I estimate a local linear regression on either side of the cutoff, using triangular weights centered at 20%. Bandwidth selection follows Calonico et al. (2014b), with the MSE-optimal bandwidth for point estimates. For comparability across outcomes, I fix the bandwidth to the MSE-optimal bandwidth obtained for the outcome volume of work (in FTE) per worker.

Following Korting et al. (2023), I use evenly spaced bins and apply the mimicking variance method to determine the number of bins, which yields

²⁷For this analysis, I collapse the data at the firm level.

²⁸Recall that workers in lockdown-affected JCs were automatically eligible to the simplified procedure in September.

a relatively large number. Table B.3 summarizes the number of bins used when imposing the MSE-optimal bandwidth at the cell level. For visualization purposes, I present graphs using 10 bins on each side of the cutoff.

For correct inference, I follow Cattaneo et al. (2020) and report robust bias-corrected confidence intervals since advances in the econometrics literature warn that conventional confidence intervals can be highly sensitive to bandwidth choices or misspecification errors, which may be a reason to over-reject the null hypothesis of no treatment effect (Calonico et al., 2014b). Robust bias-corrected CIs remove the estimated bias from the RD point estimate. Additionally, they allow the variance term to account for the contribution of the bias correction step to the variability of the bias-corrected point estimator (Cattaneo et al., 2020).

4.4.2 Identification and Estimation of Dynamic Causal Effects

This section discusses the identification of dynamic treatment effects in the presence of a single differential treatment assignment period and plausible extensions if the required assumptions are not satisfied.

In standard experimental designs with one-time randomization and imperfect compliance, we can identify treatment effects for compliers using assignment as an instrument. The fuzzy RDD here follows the same intuition: the September eligibility rule serves as an instrument for actual treatment status. Recent studies (Cellini et al., 2010; Baron, 2022; Ruggieri, 2025; Hsu and Shen, 2024) extend the fuzzy and sharp RDD to dynamic settings where there is sequential treatment and path dependency. In such cases, non-eligible units in the first period may later cross the threshold, becoming eligible and complicating identification. In other words, in such settings individuals are exposed to differential treatments at different points in time.

In my setting, differential assignment was only enforced in September 2020. From October onward, all firms were once again eligible for the simplified STW program. Thus, the design approximates a static setting, in which firms' eligibility changed only once and treatment exposure is not, a priori, expected to differ at the cutoff in subsequent periods. Nonetheless, informational frictions may have delayed take-up among previously ineligible

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firms, violating the static compliance assumption (Ferman and Tecchio, 2025). Firms that were not eligible in September might have been less likely to take up STW in later periods relative to firms that were eligible, simply because they were not informed that the original administrative rules had been reinstated. This is supported by Figure C.2.3, which shows that the fraction of days in STW continues to exhibit a discontinuity in 2020-Q4, suggesting persistence. However, from 2021-Q1 onward, the discontinuity disappears.

In Section 4.5, I present results assuming static compliance to estimate dynamic LATEs at the cutoff but discuss a potential approach that can be applied to account for persistence in treatment exposure at the cutoff in Appendix 4.C.1. Specifically, Section 4.5 presents estimates from the following dynamic LATEs at the cutoff:

$$\Delta_t(C_1) = \frac{\theta_t^{ITT}}{\theta_{t=1}^{FS}} \quad \forall \quad t \in \{1, \dots, T\} \quad (4.2)$$

That is, the effect at time t for first-period compliers C_1 is defined as the ratio between the ITT estimate at t and the first-stage estimate at $t = 1$, for all $t \in \{1, \dots, T\}$. In other words, assuming that treatment is only differentially accessed in period $t = 1$, I estimate per-period reduced-form estimates scaled by the first period differential treatment effect in take-up. Each of these estimates can be interpreted as the total effect at each period for first-period compliers.

The implication of this approach, relative to one that accounts for dynamic compliance is that, the ITT estimate in $t = 2$ ignores the contribution of contemporaneous effects that can be identified from new compliers being treated in that period. To illustrate this formally, Equation A4.4 in the Appendix shows that, under the assumption of homogeneous treatment effects across calendar periods, the ITT in the second period can be decomposed into the sum of contemporaneous effects for newly treated compliers and lagged effects from first-period compliers. This suggest that reduced form estimates might be informative about total effects while LATE estimates may overstate the contribution of first-period compliers by not accounting for new compliance in period $t = 2$ (Equation A4.5).

4.4.3 Validity and sensitivity checks

This section presents a series of validity checks to support the proposed identification strategy and its underlying assumptions, as well as robustness checks for the non-parametric estimation approach, following Cattaneo et al. (2020).

Validity checks of identifying assumptions. The identification of treatment effects in the fuzzy RDD framework relies on two key assumptions: (i) no local selection on unobserved characteristics or potential gains, and no heterogeneity in treatment assignment (i.e., no defiers at the cutoff);²⁹ and (ii) local continuity of potential outcomes (“smoothness”) at the cutoff.

The first assumption implies no selective sorting across the treatment assignment threshold. A potential concern would be firms manipulating their position relative to the threshold to obtain automatic access to the simplified STW program. However, this is likely not the case since the eligibility restriction was announced by Royal Decree on July 17, 2020, and applied from September 1 to September 30, based on past reports of contractual working days and days in STW during the second quarter of 2020. This design makes manipulation unlikely. Although employers submit quarterly employment declarations in the months following each quarter, STW declarations are cross-validated across several institutions beyond the National Social Security Office, further reducing the likelihood of manipulation. To corroborate this, I formally test for manipulation using the local polynomial density estimator proposed by Cattaneo et al. (2018). The estimates provide no evidence of discontinuities in the density of the running variable at the cutoff (Figure C.2.1).

The second assumption (i.e., the local continuity of potential outcomes across the cutoff) cannot be tested directly. By examining whether predetermined covariates are smooth at the cutoff I explore the plausibility of this assumption. These covariates include firm-level financial indicators and worker characteristics, all measured at the end of 2019. Tables C.2.1 and C.2.2 show that financial indicators are smooth across the cutoff for partially lockdown-affected and lockdown-unaffected firms. Among worker

²⁹This assumption boils down to the exclusion restriction, random assignment, and monotonicity in the context of a LIV.

4. PROTECTING JOBS AND FIRMS: THE IMPACT OF SHORT-TIME WORK DURING COVID-19

characteristics, there is a small but statistically significant difference in the white-collar worker share at the threshold for partially lockdown-affected firms. Particularly, in this group of firms, those above the 20% cutoff have a slightly larger share of white-collar workers. To assess whether this imbalance is a concern, I split the sample by the median fraction of white-collar workers and find no imbalance within these subgroups. This heterogeneity analysis will be further discussed in the results section. Regarding predetermined outcomes, a pre-trends test fails to reject smoothness across the cutoff for both groups of firms. Figure C.2.2 confirms visually this for two main outcomes of interest, volume of work per worker and headcount employment.

I also test for discontinuities in placebo outcomes that are unlikely to be affected by STW eligibility. Specifically, I assess whether eligible firms report disproportionately higher use of vacation (in FTE) per worker or equivalent performance time³⁰ (in FTE) per worker. A statistically significant discontinuity in such outcomes might indicate that the estimated effect is capturing other mechanisms unrelated to STW. Table C.2.3 shows no evidence of such discontinuities. However, the results suggest that vacation days may have served as a substitute for STW, particularly for ineligible workers in firms directly affected by COVID-19. This implies that the observed drop in FTE per worker for eligible firms may have been even larger had alternative adjustment mechanisms not been used by employers and workers on the left-hand side of the cutoff.

Furthermore, I test for differential treatment effects prior to 2020-Q3. Figure C.2.3 presents the θ^{FS} estimates obtained from Equation (4.1), replacing the outcome with the treatment variable defined as the average monthly fraction of days in STW, using ONSS/RSZ quarterly data, for each period from 2018Q3 to 2022-Q4. No statistically significant effects are found for estimates prior to 2020-Q3. I also test for differential treatment effects prior to September 2020 using ONEM/RVA monthly statistics on STW take-up. Figure C.2.4 presents these results, using as treatment the monthly take-up rate and the monthly fraction of days in STW for partially

³⁰In the Belgian social security system, equivalent performance periods (or assimilated periods) refer to periods during which an employee is not actively working but which are treated as if they were periods of actual work for specific purposes. These periods can affect entitlement to and the calculation of certain social benefits, such as retirement pensions, unemployment benefits, and sick pay. They do not include holidays, unpaid leave, or short-time work, but cover other types of authorized work absences.

lockdown-affected and lockdown-unaffected firms, respectively. I find no statistically significant difference in the monthly likelihood of taking up STW prior to September for either type of firm. However, for the fraction of days in STW computed from monthly statistics, I find a statistically significant effect in May 2020. It is worth noting that, by contrast, there is no statistically significant difference in the fraction of days in STW when using ONSS/RSZ quarterly statistics for 2020-Q2—the quarter that includes May—thereby providing some validation for the assumed smoothness condition.

Sensitivity and Robustness Checks. First, I assess the sensitivity of the LATE estimates to different bandwidth choices, widening and narrowing from the benchmark bandwidth of ± 3.5 percentage points. Tables C.2.4 and C.2.5 present results for both firm types and for the treatment defined as the take-up rate in September.³¹ Results remain robust across bandwidth choices.

Second, I perform a “donut” robustness check by excluding observations within $|z_i| < 0.1$ and $|z_i| < 0.2$ of the cutoff. Table C.2.8 shows that results are robust to the exclusion of up to 124 firms (or 554 observations). Third, I demonstrate that there is no statistically significant effect when I impose artificial cutoffs at different percentage points (Table C.2.9). Fourth, I test the sensitivity of the estimates by replacing the log transformation with both untransformed outcomes and the $\log(x + 1)$ transformation. The log-like transformation specification imposes less selection, as it retains worker combinations reporting zero volume of work, rather than excluding them as would occur with a standard log transformation. While conclusions remain qualitatively unchanged, the estimates are slightly more conservative under this alternative transformation.³²

Finally, I assess the sensitivity of the cell-level estimates to the presence of spillover effects within firms by (i) estimating treatment effects at the firm level,³³ and (ii) for partially lockdown-affected firms, by estimating the

³¹The test for the treatment defined as the monthly fraction of days in STW is presented in Tables C.2.6 and C.2.7.

³²These results are available upon request. Main results are reported using the standard log transformation, given criticisms of log-like transformations such as $\log(x + 1)$ (Chen and Roth, 2024).

³³The first stage for partially affected firms is weaker when I include the set of workers not concerned by the policy change. This is not the case for lockdown-unaffected firms. In both cases, results remain qualitatively the same. These results are available upon request.

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treatment effects on the group of workers who were not directly concerned by the policy reform in September (Figure C.3.7 in Appendix). Since the firm-level analysis yields similar conclusions, and the cell-level analysis of non-concerned workers in partially lockdown-affected firms also points to positive headcount employment effects, it does not appear that positive effects for some workers are offset by negative effects for others within the same firm.

Taken together, these tests and robustness checks provide no evidence of sorting or non-linearities around the cutoff and confirm the robustness of the estimates. This supports the validity of the fuzzy RDD design in this context. I now turn to examine the impact of STW on employment and firm exits, both in the short and medium run.

4.5 Main Results

In this section, I present the main results on the impact of STW on firm employment and the likelihood of bankruptcy for two types of firms: (i) partially lockdown-affected firms and (ii) lockdown-unaffected firms.

In the first part, I discuss the impacts of STW on employment and the likelihood of bankruptcy in both the short and medium term. I structure this discussion by first providing insights into the impact of eligibility on STW take-up (extensive margin treatment) and the fraction of days in STW (intensive margin treatment) in the short term (i.e., the first-stage estimates). Then, I investigate the impact of eligibility on employment outcomes (i.e., reduced-form estimates). Finally, I discuss the dynamic local LATE effects of STW take-up on employment and the likelihood of bankruptcy in the short and medium term.

In the second part, I explore a dimension of heterogeneity related to the use of STW in the Belgian context. In particular, I evaluate heterogeneous effects below and above the median firm white-collar worker share. This is motivated by the fact that, in firms with higher shares of white-collar workers, the restrictions introduced by the September 2020 reform were more binding, as the alternative conditions for STW take-up—when firms were not eligible for the simplified procedure—were considerably stricter for white- than for blue-collar workers.

The heterogeneous analysis by firm type—based on the share of white-collar workers—contributes to the broader literature on STW by uncovering how workforce composition interacts with firms’ incentives and constraints in using the program. It highlights that theoretical predictions, which emphasize efficient labor hoarding (Biddle, 2014) in firms with high adjustment costs and firm-specific human capital (typically white-collar-intensive), may not align with observed patterns of STW use. In practice, blue-collar-intensive firms used STW more persistently during the pandemic, driven by institutional factors such as sectoral agreements, labor shortages, the intensity of the shock, and prior experience with the program. Understanding which type of workers benefited more from the policy is essential for assessing how program design can be improved to enhance efficiency during economic shocks.

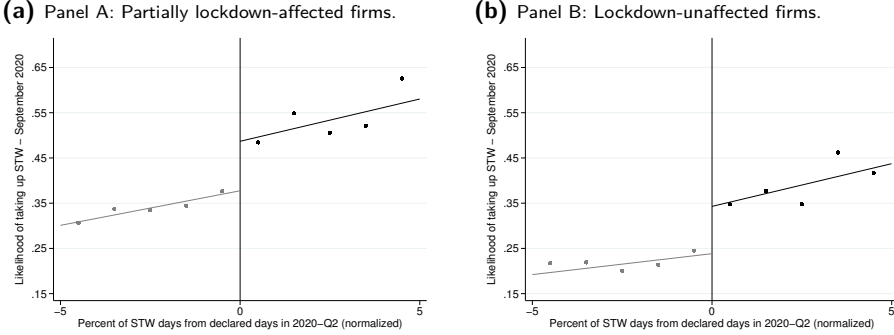
A. Impacts of STW on Employment and Firm Bankruptcy

First-stage estimates. I begin by exploring the effect of the cutoff rule introduced in September 2020, which provided differential eligibility conditions for STW for each of the two types of firms analyzed. Figure 4.2 (Panel A) plots the empirical relationship between the average (firm-level) STW take-up rate and the percentage of contractual days in STW in 2020-Q2 (normalized to zero) for partially lockdown-affected firms. Averages are taken within a 1 percentage point interval. Similarly, Figure 4.2 (Panel B) shows the same relationship for lockdown-unaffected firms. One can observe an upward trend, with clear evidence of a discontinuity at the 20% cutoff in the share of STW days relative to declared days in 2020-Q2, for both types of firms. The differential take-up rate at the cutoff is 10.6 p.p. (SE: 2.5) for partially lockdown-affected firms and 11 p.p. (SE: 1.8) for lockdown-unaffected firms.

To evaluate whether treatment at the intensive margin also jumps discontinuously across the threshold, Figure 4.3 (Panels A and B) shows the relationship between the average (firm-level) fraction of days in STW taken up in September and the percentage of contractual days in STW in 2020-Q2. Whereas the relationship is increasing for partially lockdown-affected firms, this is not the case for lockdown-unaffected firms. This is because firms partially affected by lockdowns were more likely to use the program and did so more intensively, as some of their workers remained under operating constraints in September 2020, making them eligible for the simplified STW

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Figure 4.2. First-stage: STW take-up rate in September as a function of percent of contractual days in STW in 2020-Q2.



Notes: This figure shows the empirical relationship between the average STW take-up rate and the percent of contractual days in STW in 2020-Q2 (normalized to zero) within the 5 p.p. bandwidth around the cutoff, for (i) partially lockdown-affected firms (Panel A) and (ii) lockdown-unaffected firms (Panel B), respectively. It corresponds to the “first stage” estimate (θ^{FS}), that is obtained by estimating Equation (4.1), replacing the dependent variable with STW_i , the treatment. In both panels, the horizontal axis is the percent of STW days from declared contractual days in 2020-Q2 normalized to zero in 5 bins to the left- and right-hand side of the cutoff. Each bin is of the size of 1 p.p. The MSE-optimal estimated bandwidth for each panel is 3.5 and 3.7, respectively, and estimates within these bandwidths are 10.6 (SE: 2.5) and 11.0 (SE: 1.8), respectively.

procedure.

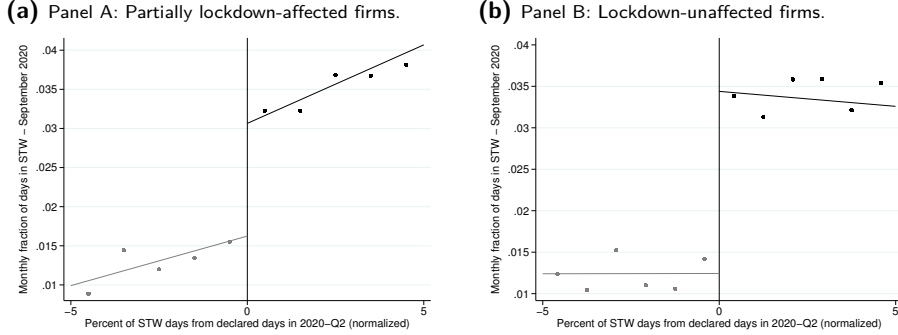
In contrast, there is no monotonically increasing relationship between the past intensity of use and the fraction of days in STW in September for lockdown-unaffected firms, suggesting that affected workers at the peak of the crisis were not substantially concerned by a higher fraction of days in STW in September, as these sectors were able to continue operations during this month. Interestingly, the fraction of days in STW taken up in September increases discontinuously at the cutoff by 1.5 p.p. (SE: 0.25) and 2.0 p.p. (SE: 0.31) for the two types of firms, respectively.

Figure C.3.1 in Appendix shows the same relationship using the monthly fraction of days in STW calculated from quarterly data for 2020-Q3. Estimates are of similar magnitude to those using the treatment defined in September, suggesting that the discontinuity corresponds mostly to the month of the policy change and not to other months in the quarter.

Taken together, the previous figures and formal estimates demonstrate

4.5. Main Results

Figure 4.3. First-stage: Fraction of days in STW in September as a function of percent of contractual days in STW in 2020-Q2.



Notes: This figure shows the empirical relationship between the average fraction of days in STW in September and the percent of contractual days in STW in 2020-Q2 (normalized to zero) within the 5 p.p. bandwidth around the cutoff, for (i) partially lockdown-affected firms (Panel A) and (ii) lockdown-unaffected firms (Panel B), respectively. The dependent variable is constructed by approximating the total contractual number of day for each firm from quarterly ONSS/RSZ data, since this information is not available at the monthly level. It corresponds to the “first stage” estimate (θ^{FS}), that is obtained by estimating Equation (1) by replacing the dependent variable with STW_i , the treatment. In both panels, the horizontal axis is the percent of STW days from declared contractual days in 2020-Q2 normalized to zero in 5 bins to the left- and right-hand side of the cutoff. Each bin is of the size of 1 p.p. The MSE-optimal estimated bandwidth for each panel is 4.8 and 7.6, respectively, and estimates within these bandwidths are 1.5 (SE: 0.25) and 2.0 (SE: 0.31), respectively.

that the identification strategy can adequately capture the differential take-up rate at the cutoff in September, which is information that I rely on to estimate the effects of STW programs on firm outcomes.

Reduced-form estimates. I now show the average volume of work per worker (in 0.6 percentage point bins of the running variable) and the average number of jobs (in 0.45 and 0.6 percentage point bins) around the 20% cutoff, for both types of firms studied. Figure C.3.2 illustrates how simplified firm access to STW affects (cell-level) individual working time. Specifically, the figure shows that the volume of work (in FTE) per worker decreases when firms locate above the 20% cutoff, suggesting STW utilization. Additionally, Figure C.3.2 shows how simplified access to STW positively affects headcount employment, unconditional on STW take-up. Both relationships are statistically significantly different from zero (at the 5% level) for partially lockdown-affected firms.

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However, this is not the case for lockdown-unaffected firms. This is likely because lockdown-unaffected firms are more selective in their allocation of STW to workers: first, not all firms above the cutoff take up the program, and second, if they do, they most likely do not reduce hours of work for all their workers.

Short-term impact of STW take-up. To confirm the previous graphical exploration, I estimate (LATE) treatment effects of STW take-up using employment and wage outcomes and report the results in Table 4.2.³⁴ First, one can notice that access to the simplified procedure of STW does not positively affect headcount employment for the majority of firms that take up STW because of the policy change. However, STW take-up translates into a 57% reduction in the volume of work per worker and a 58% increase in headcount employment for partially lockdown-affected firms, whereas no significant effect is observed for lockdown-unaffected firms. Notably, this latter group does not exhibit a reduction either in the volume of work per worker or in the wage per worker, which are mechanical effects of program use in the short term. This suggests that while lockdown-unaffected firms above the cutoff took up the program to a greater extent than those below the cutoff, on average, they did not benefit in terms of job savings, resulting in deadweight effects in the short run.

In this section, I have provided evidence that the overall effectiveness of STW take-up during the COVID-19 period depended on the extent to which the program protected jobs at high risk of destruction. Overall, STW appears more effective for firms facing the highest risk of job destruction, which in the COVID-19 setting are those partially affected by lockdown measures—an empirical channel that has not yet been uncovered by the current literature for the pandemic period. Moreover, the evidence suggests that high-wage workers were protected in these firms by STW, presumably because these firms had future prospects of recovery once the economic cycle stabilized.

Importantly, the effectiveness of the program does not only depend on the short-run effects but also requires examining how differential STW take-up in September impacted firm employment trajectories, which is discussed in the

³⁴Table C.3.1 in Appendix shows the same estimates but using the log-like transformation $\log(x + 1)$. The decomposition is such that, for each cell, $\log(FTE + 1) = \log(\frac{FTE+1}{jobs+1}) + \log(jobs + 1)$. These estimates are more conservative in magnitudes.

4.5. Main Results

Table 4.2. STW take-up impact on employment outcomes.

	Partially Lockdown-affected firms		Lockdown-unaffected firms	
	Coefficient (SE)	Robust bias-corrected 95% CIs	Coefficient (SE)	Robust bias-corrected 95% CIs
Panel A. First stage.				
	0.113*** (0.0257)		0.115*** (0.0185)	
Panel B. Treatment Effects.				
Volume of work (in FTE) per worker	-0.570*** (0.1793)	[-1.459 , -0.487]	-0.1298 (0.1305)	[-0.5887 , 0.1205]
Headcount employment (during-quarter)	0.580** (0.2911)	[0.268 , 1.936]	0.0087 (0.1610)	[-0.8275 , 0.1042]
Headcount employment (end-of-quarter)	0.474* (0.2792)	[-0.028 , 1.573]	0.0236 (0.1521)	[-0.8128 , 0.0689]
Total Volume of work (in FTE)	0.0098 (0.3208)	[-0.750 , 1.008]	-0.1211 (0.2135)	[-1.208 , 0.0165]
Wage per unit of work (EUR per FTE)	-0.160 (0.1022)	[-0.717 , -0.113]	0.0205 (0.1060)	[-0.119 , 0.500]
Wage per worker (EUR)	-0.684*** (0.2198)	[-1.918 , -0.688]	-0.1176 (0.1798)	[-0.548 , -0.468]
MSE-optimal Bandwidth	3.9		3.7	
Number of Observations	6,316		9,118	
Number of Firms (clusters)	1,941		1,139	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In Panel A, the first-stage estimates are obtained by estimating Equation (4.1), replacing y_{it} with STW_i , an indicator variable for whether the firm takes up STW in September. In Panel B, the presented coefficients are obtained by estimating Equation (4.2) for $t = 1$, the first period of analysis, which corresponds to the quarter of the policy change (2020-Q3). RDD estimates are derived from local polynomial nonparametric regressions of order 1, using a symmetric bandwidth. Since outcomes are expressed in logs, estimates can be interpreted as semi-elasticities. I employ the MSE-optimal bandwidth, selected using the volume of work (in FTE) per worker as the benchmark outcome, following the data-driven procedure of Calonico et al. (2014a). This same bandwidth is used for all other outcomes to enable the decomposition of the employment effect into intensive and extensive margins. Heteroskedasticity-robust standard errors clustered at the firm level are reported in parentheses. Robust bias-corrected confidence intervals, as proposed by Calonico et al. (2014b), are shown in brackets. I set the bandwidth for estimating the bias correction to the same MSE-optimal bandwidth (i.e., $\rho = 1$).

4. PROTECTING JOBS AND FIRMS: THE IMPACT OF SHORT-TIME WORK DURING COVID-19

next section.

Dynamic effects of STW take-up on employment. I now explore the dynamics of the effects found on employment and wages. I plot the treatment effects estimated from separate regressions using Equation (4.2) for each quarter after the policy change and up to 2022-Q4, the last period of data available. I focus on the outcomes previously analyzed in Table 4.2 for partially lockdown-affected firms (Figure 4.4)³⁵ and lockdown-unaffected firms (Figure 4.5). The effects can be interpreted as total effects, or reduced-form estimates at each point in time, scaled by the first-period differential STW take-up.

For partially lockdown-affected firms, Panel (a) in Figure 4.4 reveals that the volume of work per worker dropped consistently during the first two consecutive quarters, while it recovered in 2021, when the economy slightly rebounded and COVID-19 infections slowed down. From the end of 2021 and up to the end of 2022, volume of work per worker decreased again. This coincides with the fourth wave of COVID-19 infections, where a new set of restrictions came into effect in 2021-Q4. Interestingly, the positive gains in employment observed in the short run are persistent over time (Panels b and c in Figure 4.4). Robust bias-corrected 95% confidence intervals suggest persistent positive effects on headcount employment and total working hours up to two years after the policy change.³⁶ The effect on wages per worker closely tracks the evolution of the volume of work per worker, consistent with no significant effect on wage rates in the medium term, although there is a slight decline in wage rates in the short term.

Turning to lockdown-unaffected firms, Figure 4.5 reveals no statistically significant effect on the volume of work per worker up to 2021-Q2. However, Figure C.3.4 in Appendix shows that when including observations that drop completely working time by using the log-like transformation $\log(x + 1)$, the negative mechanical effect on volume of work per worker is statistically

³⁵The analogous figure using the $\log(x + 1)$ transformation of the outcomes is presented in the Appendix in Figure C.3.3.

³⁶Since I fix the bandwidth to be the MSE-optimal bandwidth for 2020-Q3, conventional estimates may fall outside the robust bias-corrected confidence intervals. When computing MSE-optimal bandwidths separately for each period and using them accordingly, estimates are more likely to fall within the corresponding robust bias-corrected intervals. However, the latter approach is not ideal for tracking the effects on the same firms over time.

significant during the last two quarters of 2020, including the one of the policy change. This suggests that STW was used to reduced fully working time among some workers in lockdown-unaffected sectors. However, even if these firms reduced working time per worker because of STW utilization, there are no statistically significant gains in employment (headcount or total hours worked) in either the short or medium term for lockdown-unaffected firms, even when using the less selective outcome transformation.

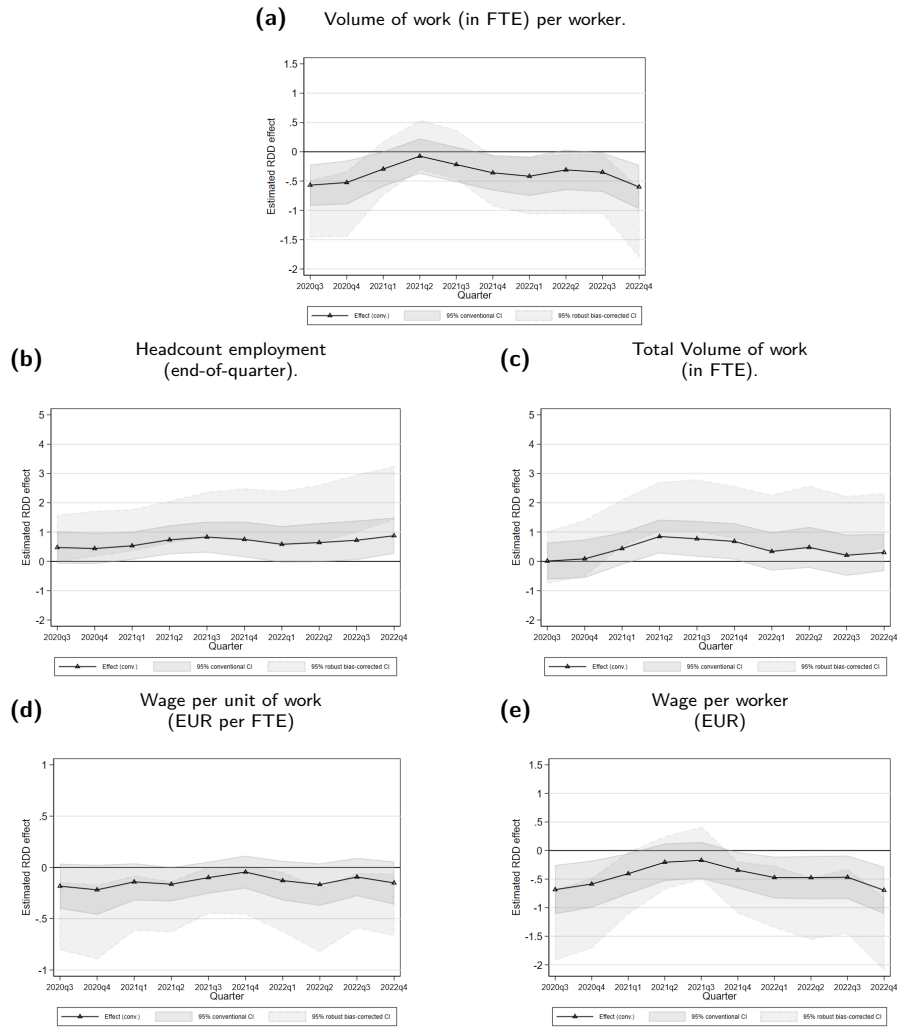
Importantly, the employment gains among partially lockdown-affected firms did not come at the expense of layoffs of workers who were not directly affected by the policy change—that is, those belonging to JCs classified as most affected by lockdown measures. Figure C.3.7 in the Appendix shows that workers in lockdown-affected JCs—who were eligible for the simplified procedure regardless of their firm’s status under the policy reform—also experienced improved job retention, relative to similar workers in firms that were ineligible for the simplified scheme for non-affected workers.

Dynamic effects of STW take-up on firm bankruptcies. When examining firm bankruptcies from a dynamic perspective, results show that STW take-up did not result in a statistically significant effect on bankruptcies relative to the counterfactual of no STW utilization, for either type of firm. Importantly, during the COVID-19 period, moratorium procedures were in place: the tax administration and ONSS/RSZ could not declare firms bankrupt due to unpaid taxes or social debts until July 2022. After this date, the number of bankruptcies began to rise.

Interestingly, although the estimated effect turns negative from 2022-Q3 for partially lockdown-affected firms, it is not statistically different from zero. Taken together with the positive employment effects, this suggests that while STW shielded jobs from destruction in partially lockdown-affected firms, these jobs were not necessarily concentrated in firms at higher risk of bankruptcy. Rather, firm bankruptcies were likely influenced by the institutional framework in place during that period.

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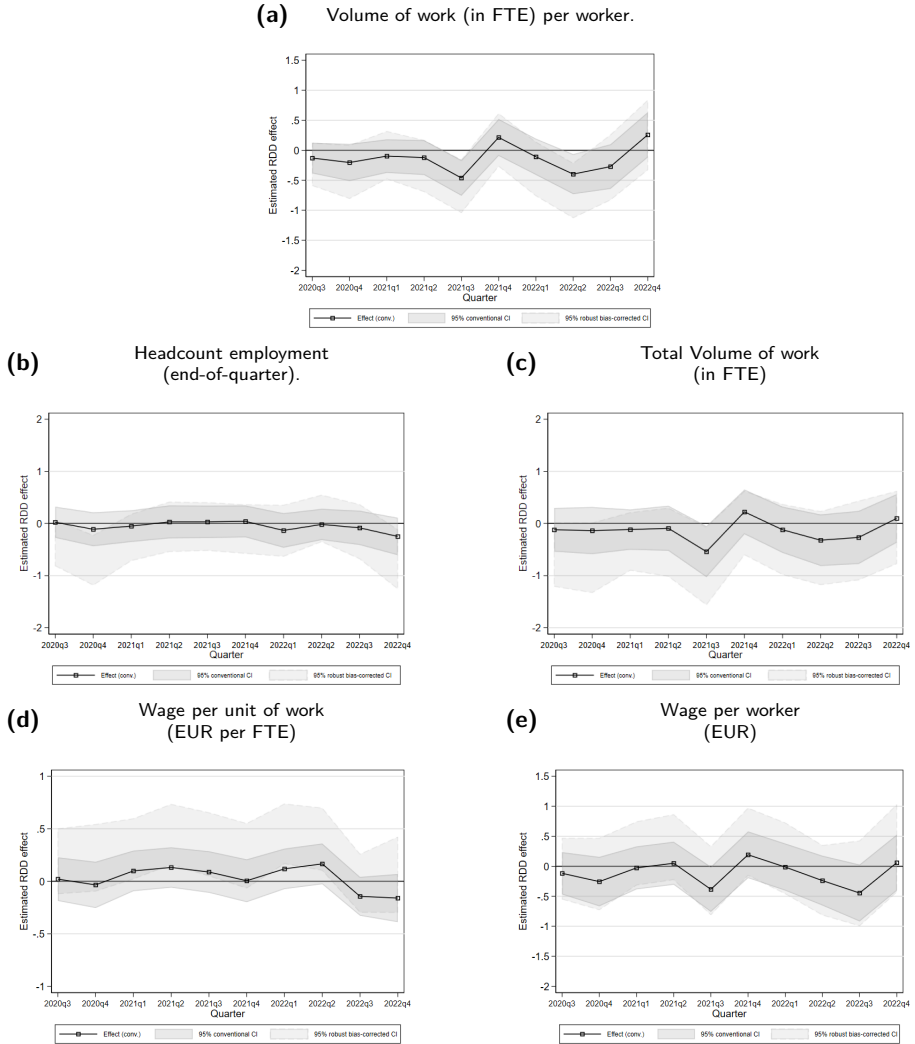
Figure 4.4. Dynamic Treatment Effects: Partially lockdown-affected firms.



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020. 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020-Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 3.9 around the cutoff.

4.5. Main Results

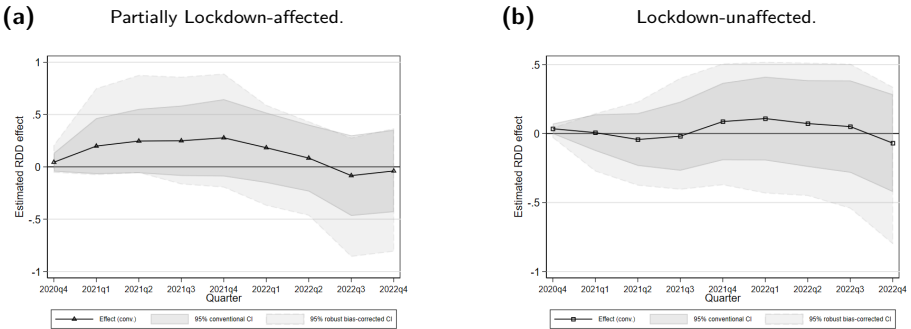
Figure 4.5. Dynamic Treatment Effects: Lockdown-unaffected firms.



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020. 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020-Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 3.7 around the cutoff.

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Figure 4.6. Dynamic Treatment Effects: Likelihood of Bankruptcy.



Notes: For these estimates the dataset is collapsed at the firm level. These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020. 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020-Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 5 around the cutoff.

B. Heterogeneous effects across firm share of white-collar workers.

In the previous subsection, I showed that STW preserved jobs in the short and medium term, but only for partially lockdown-affected firms. However, it did not significantly reduce the likelihood of firm bankruptcies. The underlying mechanism is that these sectors were severely affected by COVID-19 lockdown restrictions, which was one of the main motivations for public authorities to provide special treatment to firms at the end of the first wave of COVID-19 infections. This finding is consistent with previous literature showing that the positive effects of STW are concentrated in firms facing the largest demand shocks, as documented in the context of the Great Recession (Cahuc et al., 2021).

In this section, I explore a dimension of heterogeneity related to the use of STW across firms with different workforce compositions, defined by their share of white-collar workers. In firms with higher shares of white-collar workers, the restrictions introduced by the September 2020 reform were more binding, as the alternative conditions for STW take-up—when firms were not eligible for the simplified procedure—were considerably stricter for white- than for blue-collar workers.

In Belgium, prior to the pandemic, eligibility criteria for STW access had been more stringent for white-collar workers than for blue-collar workers (see Section 4.2 for details), increasing the administrative burden and application costs for employers seeking to enroll white-collar workers in the program. As a result, employers have historically had greater experience using STW for blue-collar workers due to more lenient eligibility rules.³⁷

Given this context, firms with a larger share of white-collar workers located just below the 20% eligibility cutoff are expected to be less likely to take up STW—relative to firms just above the cutoff—than firms with a smaller fraction of white-collar workers. Furthermore, it is plausible that firms with little or no prior experience—typically white-collar-intensive firms, where deadweight losses are likely less concentrated—would benefit the most

³⁷Calavrezo and Walkowiak (2025) show that prior use of STW before the pandemic is positively correlated with its adoption during the pandemic in France.

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from the program. However, given the context of the pandemic, blue-collar-intensive firms located above the 20% cutoff—even if not more likely to take up the simplified procedure of STW relative to their counterparts—may still have used a statistically significantly larger fraction of days in STW compared to those below the cutoff that had access to the alternative program.

In the following analysis, I examine the effects for firms below and above the median share of white-collar workers. It is important to note that the distribution of white-collar workers differs substantially between the two types of firms studied. Figure A2 in the Appendix illustrates these distributions. Among partially lockdown-affected firms, 50% have fewer than 25% white-collar workers, meaning these firms are, on average, blue-collar intensive.

By contrast, the distribution for lockdown-unaffected firms is closer to a bimodal distribution, with firms either predominantly blue-collar or predominantly white-collar. While this distribution resembles that of the full sample of firms,³⁸ its median is considerably higher. Specifically, 50% of firms in lockdown-unaffected sectors have more than 75% of their workforce composed of white-collar workers, indicating that these firms are, on average, white-collar intensive. Moreover, since half of the firms below the median in lockdown-unaffected sectors are fully blue-collar intensive (i.e., with 0% white-collar workers), I analyze this group separately. This is particularly relevant, as these firms had greater prior experience with the program and are concentrated in sectors with higher experience-rated STW contributions under normal conditions (Tarullo, 2025).³⁹

This heterogeneity analysis provides important insights into the mechanisms driving STW utilization. While theoretical predictions suggest that white-collar-intensive firms—facing higher adjustment costs and employing workers with more firm-specific human capital—would be more inclined to use STW, the empirical evidence indicates more persistent use among blue-collar-intensive firms during the pandemic. This pattern reflects

³⁸Including firms not affected by the policy change because 100% of their workforce belonged to protected sectors.

³⁹These firms are primarily concentrated in sectors such as construction and manufacturing.

stronger exposure to lockdown restrictions, more favorable sectoral agreements,⁴⁰ and greater familiarity with the program in blue-collar-intensive firms. These contrasting dynamics underscore the importance of workforce composition in shaping firms' incentives to use STW and the overall effectiveness of program design.

First-stage estimates. For partially lockdown-affected firms, among those with an above-median white-collar worker share, firms above the 20% cutoff display a greater differential likelihood of STW take-up relative to their counterfactuals below the cutoff, compared to firms with a below-median white-collar worker share. This confirms the mechanism discussed previously concerning the differential costs faced by employers when enrolling blue- and white-collar workers in STW.

Table C.3.3 in the Appendix presents the first-stage estimates. It shows that STW take-up was significantly and positively affected for partially lockdown-affected firms above the 20% cutoff with a larger fraction of white-collar workers (i.e., above the median). However, there was no differential take-up effect for firms that were blue-collar-intensive (i.e., below the median). This suggests that firms with larger shares of blue-collar workers on the left-hand side of the cutoff had nearly the same likelihood of taking up the program as those on the right-hand side that were eligible for the simplified STW procedure.

Remarkably, this prediction does not hold when analyzing lockdown-unaffected firms. Table C.3.3 shows that white-collar-intensive firms (i.e., those with more than 75% white-collar workers) above the 20% cutoff did not take up STW to a greater extent than their counterfactuals below the cutoff, compared to below-median firms. Even when further disaggregating below-median fraction firms by quartiles, although the differential take-up rate increases from the first to the second quartile of the white-collar share, the difference (around 3 percentage points) is not statistically significant at the 5% level.⁴¹

It is important to note that while partially lockdown-affected firms were likely unable to operate due to lockdown measures, lockdown-unaffected firms

⁴⁰Favorable sectoral agreements included top-up payments when using STW.

⁴¹These results are not presented but are available upon request.

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did not experience such an operational halt. Alternative strategies such as teleworking and task restructuring, together with a less intense shock, likely explain why the above prediction does not hold in the case of lockdown-unaffected firms.

Interestingly, when analyzing the treatment at the intensive margin, it is not the case that firms above the median white-collar share used a larger fraction of days in STW, even among partially lockdown-affected firms. In fact, blue-collar-intensive firms (below the median) that located above the 20% cutoff used a larger fraction of days in STW relative to their counterfactuals below the cutoff, compared to firms with higher shares of white-collar workers. This pattern is observed not only among partially lockdown-affected firms but also among lockdown-unaffected firms. This suggests that, on average, firms with larger fractions of blue-collar workers—although not more likely to react significantly in terms of take-up among partially lockdown-affected firms—used STW more intensively when they did.

To analyze STW (LATE) treatment effects in the following section, I rely on the extensive margin treatment to explore effects among above-median white-collar share firms in partially lockdown-affected sectors (Figure 4.7). However, given the relatively weak first stage for below-median firms (Table C.3.3) in this group, I focus on the intensive margin treatment to draw conclusions regarding the effects of STW utilization (Table C.3.4). For lockdown-unaffected firms, I rely on extensive margin treatment effects for above-median firms and compare both extensive and intensive margin treatment effects for below-median firms. Furthermore, since half of the below-median fraction firms are fully blue-collar-intensive (i.e., with 0% white-collar workers), I further investigate the behavior of these firms separately.

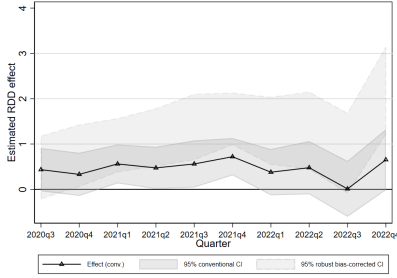
Short- and medium-term effects according to the fraction of white-collar workers. In this subsection, I discuss the results regarding the (LATE) effects of STW take-up and the fraction of days in STW for firms with below- and above-median fractions of white-collar workers, depending on the preferred specification.⁴²

⁴²The preferred specification is selected according to the first-stage results presented above.

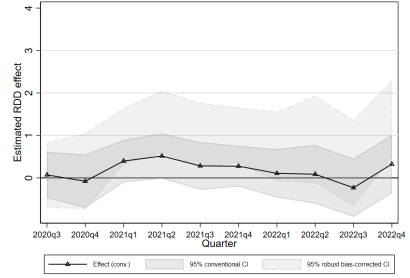
Figure 4.7. Heterogeneous effects across the fraction of white-collar workers: Partially lockdown-affected firms.

Panel A: Above median firms (Treatment: STW take-up).

(a) Headcount employment (end-of-quarter)

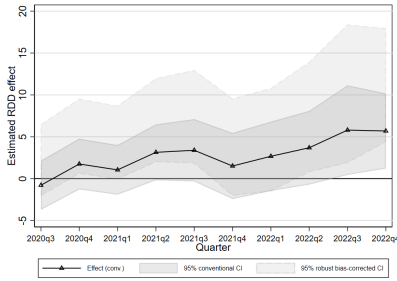


(b) Total volume of work (in FTE)

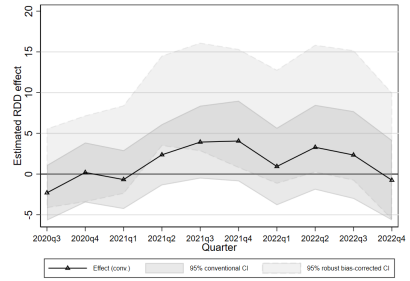


Panel B: Below median firm (Treatment: Fraction of days in STW).

(c) Headcount employment (end-of-quarter)



(d) Total volume of work (in FTE)



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2) for two splits of the sample of partially lockdown-affected firms according to the median value of the fraction of white-collar workers. 95% conventional confidence intervals (darker shaded area with solid line) and 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020 in Panel A and as the fraction of days in STW in Panel B. The coefficients are obtained from a separate regression for each quarter starting from 2020-Q3, the quarter when the policy change occurs. All estimates use a common bandwidth of 3.7 and 4.8 around the cutoff, for Panel A and Panel B, respectively. The number of observations (firms) is 3,460 (1,047) and 4,316 (1,241), for Panel A and Panel B, respectively.

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Among partially lockdown-affected firms, results reveal some differences depending on the firm-level workforce composition. In firms with an above-median fraction of white-collar workers (Panel A in Figure 4.7), STW substantially protected headcount employment in both the short and medium term. These firms also experienced positive and persistent effects on the total volume of work in the medium term. By the end of 2022, firms in STW had, on average, 32% more total volume of work relative to their counterfactual. The protection of headcount and total volume of work aligns with the severity of the operational constraints imposed by the lockdown, suggesting that declines in working time were justified by firms' limited operational capacity.

Among firms with less than 25% white-collar workers—i.e., blue-collar-intensive firms—(Panel B in Figure 4.7) STW led to a slightly more persistent decline in volume of work per worker compared to firms with an above-median share of white-collar workers. Contrary to firms with larger shares of white-collar workers, a larger fraction of days in the program⁴³ is not associated with a statistically significant short-term headcount employment effect. Although blue-collar workers seem to benefit in terms of headcount in the medium term, these gains did not translate into sustained improvements in total volume of work. While total volume of work increased until 2020-Q2, it declined thereafter. This suggests that blue-collar-intensive firms in partially lockdown-affected sectors were more persistent users of the program, since by the end of 2022-Q4 their total hours of work had not increased, although they experienced positive gains in headcount.

Among lockdown-unaffected firms, the pattern of results shows important differences across firm-level workforce compositions. For white-collar-intensive firms (those with more than 75% white-collar workers), STW did not generate short-term employment gains but did result in positive headcount effects in the medium term (Panel A in Figure 4.8). While these headcount effects coincided with gradual increases in total volume of work over the period, the latter effect is not statistically significant at the end of the period. The delayed gains in headcount employment are consistent with the slower adjustment of white-collar employment at the extensive margin

⁴³Although for firms with larger shares of white-collar workers I cannot use the intensive margin treatment to evaluate the effect of STW, the extensive margin treatment can be interpreted as the effect of a firm going from a take-up of 0% of STW to 100% of days in STW.

due to higher firing costs, with layoffs likely materializing in the counterfactual gradually over time. Moreover, the increase in headcount coincides with a persistent reduction in volume of work per worker, which only recovered toward the end of the observation period.⁴⁴ This pattern helps explain the absence of significant cumulative effects on total volume of work. It remains unclear whether the medium-term drop in volume of work per worker reflects worker preferences, employer behavior under STW, or broader structural changes. Recent empirical evidence shows that the COVID-19 crisis structurally affected working time among white-collar workers; for instance, Vogtenhuber et al. (2024) document persistent earnings losses among Austrian white-collar workers following STW use, driven by reductions in working hours, fewer promotion opportunities, and weaker wage growth among workers who remained with the same employer.

Among firms with a below-median fraction of white-collar workers within the lockdown-unaffected group (i.e., less than 75% white-collar workers), two distinct subgroups can be identified. The first group consists of firms with a mixed workforce composition, with a white-collar share ranging from 1% to 75% (Panel B.1 in Figure 4.8). For these firms, STW generated short-term headcount employment gains. However, interestingly, although these gains persisted up to 2020-Q1, they dissipated after the simplified STW procedure was abolished and the pre-crisis STW scheme for economic reasons was reinstated. Cumulative gains in total volume of work are also observed up to 2020-Q1 but dissipate afterwards. Overall, these patterns suggest that STW temporarily delayed dismissals and protected jobs in the short term among firms with mixed workforces. While these findings may have important implications for labor market reallocation dynamics in Belgium, a full exploration of these long-term effects falls beyond the scope of the present analysis.

The second group corresponds to firms composed entirely of blue-collar workers (Panel B.2 in Figure 4.8). For this group, STW did not result in headcount employment gains in either the short or medium term. These results are also confirmed when defining treatment at the intensive margin, using the fraction of days in STW. The lower exposure to lockdown measures

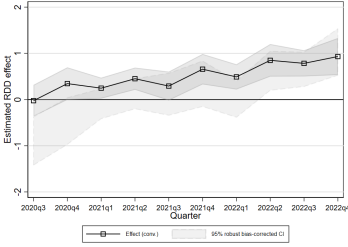
⁴⁴Results on average volume of work per worker are not presented in the main draft but are available upon request.

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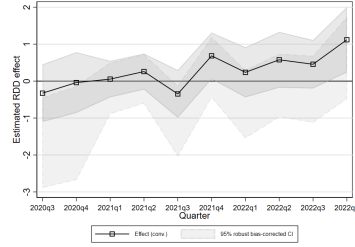
Figure 4.8. Heterogeneous effects across the fraction of white-collar workers: Lockdown-unaffected firms (Treatment: STW take-up.).

Panel A: Above median firms.

(a) Headcount employment (end-of-quarter)

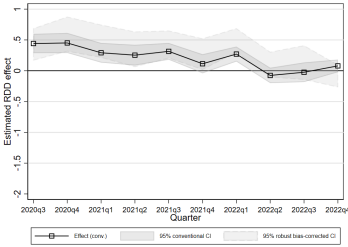


(b) Total volume of work (in FTE)

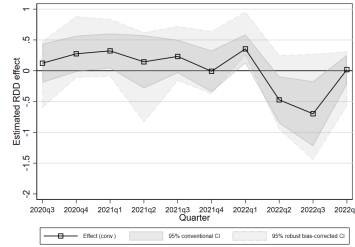


Panel B.1: Below median firms (second quantile).

(c) Headcount employment (end-of-quarter)

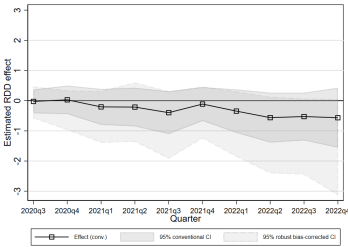


(d) Total volume of work (in FTE)

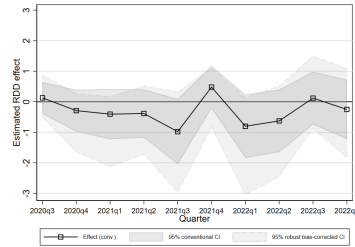


Panel B.2: Below median firms (first quantile or fully blue-collar intensive).

(e) Headcount employment (end-of-quarter)



(f) Total volume of work (in FTE)



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2) for two splits of the sample of lockdown-unaffected firms according to the median value of the fraction of white-collar workers. 95% conventional confidence intervals (darker shaded area with solid line) and 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020 in Panel A and Panel B. The coefficients are obtained from a separate regression for each quarter starting from 2020:Q3, the quarter when the policy change occurs. All estimates use a common bandwidth of 3.7 and 4.8 around the cutoff, for Panel A and Panel B, respectively. The number of observations (firms) is 3,460 (1,047) and 4,316 (1,241), for Panel A and Panel B, respectively.

and their greater prior experience with the traditional STW program for economic reasons may explain why deadweight losses appear more concentrated among these firms, reflecting the preservation of jobs that were not at risk of destruction.

When reconciling the findings of no significant treatment effects for the average treated lockdown-unaffected firms with the above-presented results, the evidence suggests that deadweight effects are driven by the protection of fully blue-collar-intensive firms. Although medium-term positive gains in headcount occur in white-collar-intensive firms, the smaller size of the differential take-up rate of STW and the absence of a statistically significant differential effect in the fraction of days in STW between firms located below and above the cutoff help explain why these effects contribute less to the overall result. While firms with a larger share of white-collar workers appear to benefit more in terms of headcount in the medium term, the largest use of the program was concentrated in blue-collar-intensive firms, increasing the overall deadweight costs of the program in sectors that were not directly affected by lockdown measures.

Finally, the effects on firm bankruptcy are not investigated in this heterogeneity analysis due to limited variation when splitting the sample across quartiles.

4.6 Conclusion

Despite the substantial expenditures on policies aimed at preserving employment and preventing firm exits during the COVID-19 crisis, few studies have assessed their unintended consequences using quasi-experimental designs. This paper provides new causal evidence on the impact of Belgium’s generous and simplified STW program on employment and firm survival during the COVID-19 pandemic by exploiting variation in eligibility rules stemming from the implementation of a temporary policy reform in September 2020. The analysis reveals important takeaways.

First, simplified STW access significantly increased program take-up, but employment gains were only concentrated among firms that were partially affected by lockdowns. These employment gains persisted for up to two years, with treated firms exhibiting higher total volume of work by the end

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of 2022. Overall, STW appears most effective for firms at high risk of job destruction, particularly those partially affected by lockdowns—an channel largely unexplored in the current COVID-19 empirical literature. This confirms the theoretical predictions that STW is most effective in firms experiencing the most severe productivity shocks—typically those operating below full capacity during the pandemic (Cahuc, 2024; Cahuc et al., 2021; Diaz et al., 2025). However, no significant effects on firm bankruptcy were observed, likely due to concurrent moratorium protections that remained in place until mid-2022.

Second, program design critically shaped both take-up and intensity of use. Firms with more blue-collar workers, regardless of lockdown exposure, used the simplified STW procedure more intensively. Specifically, results highlight that administrative simplification, not just shock severity, contributed to high take-up—pointing to potential overuse in sectors less affected by the crisis but more familiar with STW.

Third, STW effectiveness varied substantially by workforce composition. For partially lockdown-affected firms, those with above-median shares of white-collar workers benefited more from STW in terms of both headcount and volume of work. Conversely, blue-collar-intensive firms, despite showing medium-term headcount gains, experienced persistent reductions in hours per worker, leading to no significant increase in total volume of work. For lockdown-unaffected firms, although on average they did not benefit from headcount gains, the findings suggest heterogeneous responses of the program across firms' white-collar worker shares. Headcount employment effects were limited and observed in firms with a mixed workforce composition, while fully blue-collar-intensive firms—such as firms in the construction, manufacturing, and transportation sectors—in this group exhibited no positive employment responses, highlighting that this subgroup drove the deadweight losses and limited aggregate effectiveness, increasing the fiscal cost of the program.

These findings underscore the importance of targeting STW programs to preserve jobs at the highest risk of destruction. While our analysis does not support designing eligibility solely around worker categories, it highlights the relevance of conditioning access on sectoral exposure to economic shocks and the presence of high job adjustment costs—factors often associated with white-collar-intensive firms and strategic occupations. Alternative targeting criteria may also include labor shortages in critical sectors preceding the crisis

which could also occurred in blue-collar intensive firms. The evidence suggests that broad-based and administratively simplified access, while facilitating rapid use, may substantially increase fiscal costs and invite strategic behavior, particularly in firms with prior experience using STW. Policymakers may mitigate such inefficiencies through differentiated administrative requirements, firm-level financial assessments, and the introduction of employer cost-sharing devices once the most acute phase of a crisis subsides. More generally, the integration of real-time economic monitoring using high-frequency administrative data can inform dynamic adjustments in program design, thereby improving the labor market effectiveness and fiscal sustainability of STW schemes.

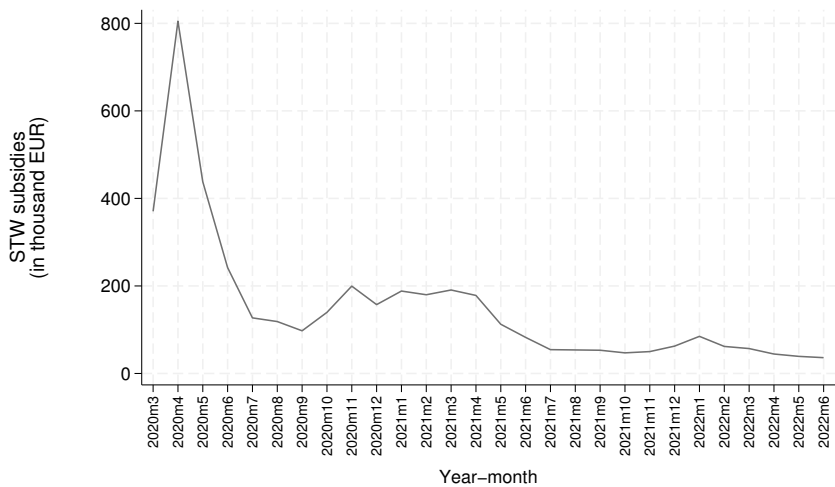
This study also presents several limitations that offer directions for future research. The estimated effects are local to firms near the eligibility threshold and should not be interpreted as externally valid across the full distribution of firms. While the empirical strategy accounts for within-firm variation and assumes no spillovers at the firm level, it cannot rule out broader general equilibrium effects. In particular, the preservation of certain job matches in lockdown-unaffected sectors may have delayed labor reallocation by sustaining unproductive matches and postponing necessary separations. Such frictions may attenuate aggregate productivity gains and dampen the pace of recovery. These allocative effects lie beyond the scope of the present analysis but warrant further investigation to assess the long-run efficiency consequences of large-scale STW use. Future evaluations of STW programs should continue to explore dynamic effects and heterogeneity across firms and workers to inform optimal crisis response strategies.

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Appendix

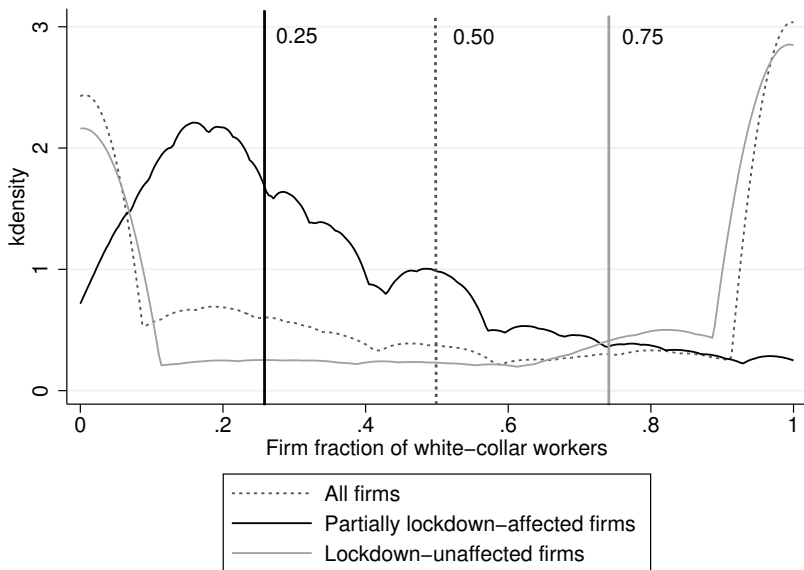
4.A Additional Figures

Figure A1. Evolution of STW Subsidies payments from March 2020 to June 2022.



Notes: This figure shows the overall evolution of STW subsidy expenditure from March 2020 to June 2022. Total expenditure includes subsidies for both corona-related and economic reasons.

Figure A2. Distribution of the firm-level fraction of white-collar workers.



Notes: This figure shows the distribution of the fraction of white-collar workers at the firm level defined at the end of 2019.

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4.B Additional Tables

Table B.1. Treatment definitions.

	<i>Treatment variable</i>	<i>Definition</i>	<i>Type</i>	<i>Level</i>	<i>Period</i>	<i>Source of information</i>
1	Take-up Corona & Econ. Reasons (monthly)	It takes values of 1 if the firm took up at least 1 day of STW for Corona or Economic reasons in September 2020 and 0 otherwise.	Discrete	Firm	2020-Sep	ONEM/RVA
2	Fraction of days in STW Corona & Econ. Reasons (monthly)	It is calculated as the ratio between days in STW in September 2020 and contractual days [†] in 2020-Q3 divided by 3.	Continuous	Firm	2020-Sep	ONEM/RVA and NSSO
3	Fraction of days in STW Corona & Econ. Reasons 2020q3 (monthly - retrieved from quarterly data)	It is calculated as the ratio between days in STW in 2020q3 and contractual days [†] in 2020-Q3. This fraction is multiplied by 3 to be compared to monthly take-up.	Continuous	Firm	2020-Q3	NSSO

Notes: (†) Contractual days are defined as the sum of 1) paid days, 2) days in every category of STW, 3) unpaid leaves, and 4) equivalent performance days. “Contractual days” must not be confused with “contractual working days” as defined in the DMfA declaration for the purpose of calculating the eligibility rule cutoff. Definitions (1) and (2) are used in the main analysis. Definition (3) is used in Appendix.

Table B.2. Descriptive statistics by firm group (2020-Q3).

Mean	Partially Lockdown-affected firms			Lockdown-unaffected firms			All firms [§]
	Not eligible [†]	Eligible [‡]	Treated [¶] firms	Not eligible	Eligible	Treated firms	
<i>Economic Sector</i>							
Agriculture (A)	1.0%	0.7%	0.5%	1.2%	0.5%	0.8%	0.7%
Mining, Electricity and Water Supply (B, D, E)	1.8%	1.4%	1.6%	0.8%	0.2%	0.6%	0.2%
Manufacturing (C)	7.2%	8.9%	10.6%	12.0%	11.7%	25.1%	15.6%
Construction (F)	43.8%	40.3%	45.7%	22.2%	22.4%	24.0%	22.3%
Retail, transportation, and food services (G, H, I)	36.2%	38.2%	31.5%	30.9%	30.1%	24.5%	31.5%
Information and communication (J)	0.0%	0.3%	0.2%	0.1%	0.2%	0.3%	3.5%
Financial and insurance activities (K)	0.1%	0.1%	0.0%	5.7%	4.9%	2.4%	3.3%
Real State Activities (L)	0.1%	0.2%	0.0%	2.0%	3.7%	1.6%	1.5%
Professional, scientific, education, security human health and others (M, N, O, P).	8.1%	9.0%	9.0%	22.5%	24.3%	18.8%	18.8%
Other services: arts, entertainment, etc (R, S, T, U)	1.5%	0.9%	1.0%	2.5%	2.0%	1.9%	2.5%
<i>Size</i>							
Less than 10 employees	40.0%	41.6%	20.8%	70.7%	72.0%	45.8%	58.7%
With between 10 and 49 employees	51.0%	47.1%	56.2%	24.5%	23.0%	37.0%	34.1%
With between 50 and 199 employees	7.6%	9.6%	18.8%	3.7%	3.9%	12.3%	5.8%
At least 200 employees	1.4%	1.7%	4.3%	1.1%	1.1%	4.9%	1.3%
<i>Worker characteristics within firms</i>							
Woman jobs (end-of-quarter)	20.4%	21.0%	18.0%	35.3%	38.0%	28.2%	32.1%
Blue-collar jobs	68.6%	66.8%	72.1%	55.1%	54.8%	71.7%	50.0%
≤ 30 years old jobs	22.4%	22.6%	19.9%	27.1%	26.8%	21.0%	26.0%
31-54 years old jobs	60.4%	60.3%	61.6%	56.9%	57.5%	60.0%	58.8%
≥55 years old jobs	17.2%	17.1%	18.5%	16.0%	15.8%	19.0%	15.1%
Full-time jobs	82.0%	81.1%	82.7%	72.5%	72.2%	79.5%	76.5%
Part-time jobs	16.5%	17.5%	15.3%	26.3%	26.7%	20.0%	22.2%
Special jobs	1.5%	1.3%	2.0%	1.2%	1.1%	0.5%	1.3%
<i>Outcomes</i>							
Average volume of work in FTE	18.6	20.1	35.7	13.0	12.1	35.3	14.4
Average jobs	29.6	33.4	61.5	19.5	18.5	53.9	22.4
Average jobs (end-of-quarter)	25.9	28.9	52.2	17.7	17.3	50.6	20.2
FTE per worker	0.7	0.6	0.6	0.7	0.7	0.6	0.6
(Gross) Wage per worker (in EUR)	5,256	5,059	4,876	5,544	5,448	5,364	5,779
(Gross) Wage rate (in EUR)	8,378	8,260	8,397	8,159	8,152	8,512	8,848
(Gross) Wage bill (in EUR)	167,604	189,553	333,073	120,232	112,912	343,277	144,272
Turnover per worker (Labor Prod.; in EUR)	74,880	76,931	61,143	59,099	58,800	53,844	92,598
Turnover (in EUR)	1,643,748	2,070,369	3,255,686	1,347,411	1,029,696	3,586,706	1,695,383
<i>STW use in Sep 2020</i>							
Share of firms in STW (corona)	0.11	0.42	0.73	0.06	0.25	0.73	
Share of firms in STW (corona + econ reasons)	0.26	0.45	1.00	0.14	0.27	1.00	
Fraction of days in STW (corona)	0.00	0.03	0.05	0.00	0.03	0.08	
Fraction of days in STW (corona + econ reasons)	0.01	0.04	0.07	0.01	0.03	0.11	
Average days in STW (corona + econ reasons) (in a month) per firm	6.7	20.0	37.1	4.7	15.0	47.6	
	Cells	5,015	4,647	4,178	7,274	6,330	3,996
	Number of firms	1,476	1,383	1,011	1,661	1,494	633

Notes: This table shows averages for three groups of firms: (i) Partially lockdown-affected firms, (ii) Lockdown-unaffected firms, and (iii) all firms including those not used in the analysis (i.e., Group 1 in Table 4.1). It shows statistics of firms that fall within a bandwidth of 5 p.p. to the left- and right-hand side of the cutoff of 20%. (†) Not eligible firms are those that do not satisfy the cutoff criterion and report less than 20% of contractual days in STW in 2020-Q2. (‡) Eligible firms are those that satisfy the cutoff criterion and report at least 20 of contractual days in STW in 2020-Q2. (¶) Treated firms are those that take up at least one day of STW in September 2020. (§) All firms corresponds to the set of firms selected with the same criteria described in Section 4.3 and that includes the fully lockdown-affected firms described in Table 4.1. STW use in September is measured by the total number of days in the program for both economic and corona-related reasons.

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Table B.3. Summary of optimal bandwidths and bins for different treatment definitions (cell-level graph).

Treatment	Partially Lockdown-affected firms			Lockdown-unaffected firms		
	bins left	bins right	h_mserd	bins left	bins right	h_mserd
Take-up (Corona + Econ)	78	80	2.65	130	121	3.43
Fraction of days in STW (Corona + Econ) (using monthly statistics)	99	95	4	119	124	5.71

Notes: This table displays the Mean Squared Error Optimal Bandwidth in the column “h_mserd”.

4.C Further Discussion and Results

4.C.1 Accounting for dynamic compliance

To account for dynamic compliance, or persistent take-up, we can rely on the approach of Ferman and Tecchio (2025), which extends Angrist et al. (2024). This approach allows to identify dynamic LATEs in settings with an irreversible binary treatment and time-invariant instruments. Importantly, treatment effects depend on treatment duration but are assumed homogeneous across calendar quarters.

Dynamic LATEs can be identified recursively, correcting for the bias introduced by new complier groups taking up treatment over time. For example, firms not treated in $t = 1$, because they were ineligible, but taking up treatment in $t = 2$ (i.e., the “switchers”) can bias reduced-form estimates in $t = 2$. In order to correct for this bias we can rely on the following specification. The local LATE for compliers at $t = 1$ can be expressed as the standard Wald estimator:

$$\Delta_1^0(C_1) = \frac{\theta_1^{ITT}}{\theta_1^{FS}} \quad for \quad t = 1 \quad (4.3)$$

Importantly, we assume that there is no difference in prior treatment likelihood between firms on either side of the cutoff.

In $t = 2$, the reduced-form estimate can be decomposed into contemporaneous and lagged effects:⁴⁵

⁴⁵Implicitly, Equation (4.4) suggest that the ITT effect in period $t = 2$ does not only

4.C. Further Discussion and Results

$$\theta_2^{ITT} = \Delta_2^0(C_1)(\theta_2^{FS} - \theta_1^{FS}) + \Delta_2^1(C_1)\theta_1^{FS} \quad (4.4)$$

Assuming treatment effects are homogeneous across periods (i.e., $\Delta_2^0(C_1) = \Delta_1^0(C_1) = \Delta^0(C_1)$), we can retrieve $\Delta_2^1(C_1)$ (which is equal to $\Delta^1(C_1)$), the one-period lagged effect of being treated in the past (i.e., $t = 1$), using Equation (4.3) and (4.4) such that:

$$\Delta_2^1(C_1) = \frac{\theta_2^{ITT}}{\theta_1^{FS}} - \frac{(\theta_2^{FS} - \theta_1^{FS})}{\theta_1^{FS}} \frac{\theta_1^{ITT}}{\theta_1^{FS}} \quad (4.5)$$

Extending this procedure to several periods, we have that for each period $t \in \mathcal{T}$ ($\mathcal{T} := \{1, \dots, T\}$) and lag $\tau \in \{0, \dots, t-1\}$, $\Delta_t^\tau(C_1) = \Delta^\tau(C_1)$. That is, we estimate a set of dynamic local LATEs for first-period compliers C_1 , at each t : $\{\Delta^0(C_1), \Delta^1(C_1), \Delta^2(C_1), \Delta^3(C_1), \dots, \Delta^{t-1}(C_1)\}$.

Overall, we can calculate each additional lagged effect as:

$$\Delta_t^{t-1}(C_1) = \frac{1}{\theta_1^{FS}} \left(\theta_t^{RF} - \sum_{\tau=0}^{t-2} \Delta^\tau(C_1) (\theta_{t-\tau}^{FS} - \theta_{t-\tau-1}^{FS}) \right) \quad (4.6)$$

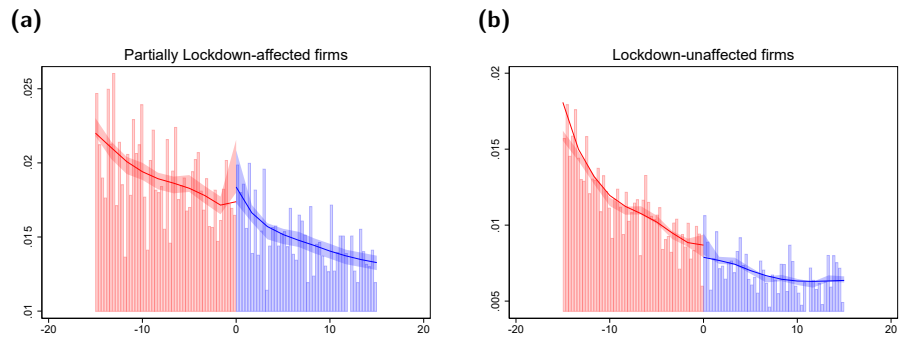
To see the analogy with the static framework, for which I discuss results in Section 4.5, I assume $\theta_{t-\tau}^{FS} - \theta_{t-\tau-1}^{FS} = 0$.

4.C.2 Validity and sensitivity checks

depend on the effect of being treated in the past in $t = 1$, but also on the effects of some latent groups (i.e., never treated and compliers that were assigned to the ineligible group in $t = 1$) that switch into treatment in the second period as explained in detail in Ferman and Tecchio (2025).

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Figure C.2.1. Manipulation Testing based on Density Discontinuity.



Notes: These figures test for manipulation of the running variable for i) Partially lockdown-affected firms (Panel a) and ii) Lockdown-unaffected firms (Panel b). I use the package *rddensity* proposed by Cattaneo et al. (2018). The density test is not rejected in Panel (a) with a p-value of 0.5313, and in Panel (b) with a p-value of 0.8412.

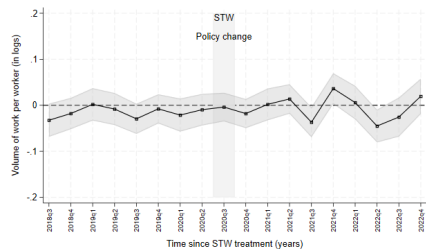
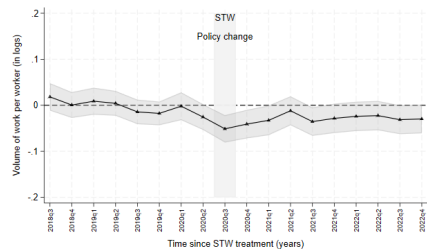
Figure C.2.2. ITT estimates of the outcomes (pre-determined outcomes test).

Panel A: Partially lockdown-affected firms.

Panel B: Lockdown-unaffected firms.

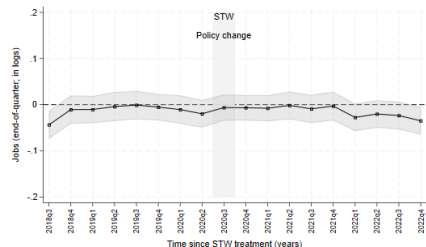
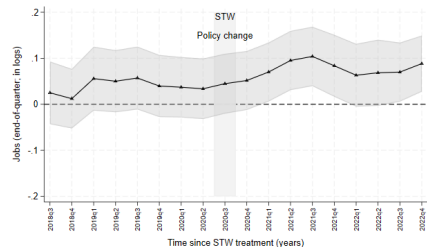
(a) Volume of work (in FTE) per worker.

(a) Volume of work (in FTE) per worker.



(b) Headcount employment (end-of-quarter).

(b) Headcount employment (end-of-quarter).



Notes: These figures display ITT estimates from estimating Equation (4.1) at each quarter separately. Treatment assignment is fixed to September 2020. Panel A and Panel B display estimates for partially lockdown-affected and lockdown-unaffected firms, respectively.

4.C. Further Discussion and Results

Table C.2.1. Test of predetermined covariates: Partially Lockdown-affected firms.

Variable	CER-optimal bandwidth	P-value	95% CI (left)	95% CI (right)	Effective Obs. Left	Effective Obs. Right
<i>Financial indicators (in 2019)</i>						
Turnover (in logs)	4.04	0.830	-0.23	0.29	1059	997
Turnover per FTE (in logs)	4.99	0.840	-0.17	0.14	1301	1193
Turnover per worker (in logs)	5.09	0.980	-0.16	0.16	1324	1225
Value Added (in logs)	4	0.290	-0.12	0.42	944	875
Value Added per FTE (in logs)	4.76	0.650	-0.12	0.19	1120	1010
Value Added per worker (in logs)	4.75	0.570	-0.11	0.21	1116	1009
Return-on-Assets (ROA) [†]	2.3	0.490	-0.02	0.04	654	667
Net Leverage [‡]	3.32	0.760	-0.09	0.06	751	721
Debt-to-EBITDA [*]	3.5	0.250	-28.79	110.2	796	759
Total Leverage [¶]	3.27	0.820	-0.07	0.05	739	717
Long-run Leverage [§]	4.47	0.140	-0.07	0.01	1022	945
Liquidity (in logs)	4.68	0.410	-0.45	1.09	183	173
Labour Cost (in logs)	3.64	0.590	-0.17	0.29	1030	1010
Profits (or Losses) After Taxes (in logs)	4.28	0.800	-0.3	0.38	1046	986
Profits (or Losses) Before Taxes (in logs)	4.75	0.520	-0.22	0.43	1191	1081
Operating (Net) Profit (in logs)	4.03	0.580	-0.22	0.4	1011	967
Cash Investments (in logs)	4.34	0.160	-0.45	2.8	173	166
Short-run Debt (in logs)	3.73	0.050	0	0.53	1046	1034
Long-run Debt (in logs)	4.07	0.990	-0.36	0.36	935	872
Current Assets (in logs)	3.47	0.070	-0.02	0.52	988	972
<i>Share of workers within the firm (pre-determined)</i>						
Part-time contracts	3.05	0.890	-0.04	0.03	868	893
Full-time contracts	3.39	0.510	-0.05	0.03	984	977
≥ 55 years old workers	3.51	0.480	-0.02	0.04	1025	1003
31-54 years old workers	5.24	0.830	-0.03	0.03	1552	1436
≤ 30 years old workers	4.09	0.220	-0.06	0.01	1200	1155
White-collar jobs	3.12	0.040	0	0.1	897	925
Women	2.89	0.190	-0.01	0.07	830	839

Notes: This table presents the test of predetermined covariates for financial indicators defined in 2019 and characteristics of workers also defined in 2019. All the ratios are not expressed in logs. The definition of some financial indicators is the following: (†) Return-on-Assets is defined as the ratio between Profit After Taxes and Total Assets. (‡) Net Leverage is defined as the ratio between the sum of short-run debt, long-run debt minus cash in the numerator, and total assets in the denominator. (*) Debt-to-EBITDA is the ratio between the sum of short-run debt and long-run debt in the numerator, and profits minus depreciation in the denominator. (§) Long-run Leverage is the ratio between long-run debt and total assets. (¶) Total leverage is the ratio between the sum of short-run and long-run debt in the numerator and total assets in the denominator.

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Table C.2.2. Test of predetermined covariates: Lockdown-unaffected firms.

Variable	CER-optimal bandwidth	P-value	95% CI (left)	95% CI (right)	Effective Obs. Left	Effective Obs. Right
<i>Financial indicators (in 2019)</i>						
Turnover (in logs)	3.86	0.860	-0.29	0.24	1017	893
Turnover per FTE (in logs)	3.45	0.140	-0.05	0.37	896	792
Turnover per worker (in logs)	3.58	0.210	-0.08	0.35	928	819
Value Added (in logs)	4.22	0.720	-0.2	0.29	995	882
Value Added per FTE (in logs)	3.49	0.170	-0.06	0.34	821	735
Value Added per worker (in logs)	3.97	0.180	-0.06	0.34	943	833
Return-on-Assets (ROA) [†]	3.21	0.090	-0.16	0.01	926	887
Net Leverage [‡]	6.21	0.620	-0.13	0.22	1419	1185
Debt-to-EBITDA [*]	2.63	0.100	-9.85	122.95	555	546
Long-run Leverage [§]	3.89	0.840	-0.09	0.07	843	788
Total Leverage [¶]	6.59	0.500	-0.1	0.21	1549	1279
Liquidity (in logs)	5.44	0.580	-0.54	0.97	188	223
Labour Cost (in logs)	2.54	0.060	-0.5	0.01	722	718
Profits (or Losses) After Taxes (in logs)	4.04	0.440	-0.53	0.23	956	881
Profits (or Losses) Before Taxes (in logs)	3.86	0.660	-0.45	0.28	931	852
Operating (Net) Profit (in logs)	4.95	0.890	-0.33	0.29	1251	1116
Cash Investments (in logs)	6.33	0.790	-1.37	1.04	243	261
Short-run Debt (in logs)	4.17	0.970	-0.27	0.25	1218	1138
Currenet Assets (in logs)	3.6	0.520	-0.37	0.19	1040	980
Long-run Debt (in logs)	3.96	0.940	-0.41	0.44	853	799
<i>Share of workers within the firm (pre-determined)</i>						
Part-time contracts	4.89	0.900	-0.05	0.04	1614	1460
Full-time contracts	4.23	0.860	-0.05	0.06	1376	1259
≥ 55 years old workers	5.72	0.720	-0.02	0.04	1927	1669
31-54 years old workers	5.00	0.710	-0.05	0.03	1657	1474
≤ 30 years old workers	4.13	0.900	-0.05	0.04	1339	1234
White-collar jobs	4.43	0.900	-0.07	0.08	1440	1320
Women	4.27	0.880	-0.07	0.08	1391	1270

Notes: This table presents the test of predetermined covariates for financial indicators defined in 2019 and characteristics of workers also defined in 2019. All the ratios are not expressed in logs. The definition of some financial indicators is the following: (†) Return-on-Assets is defined as the ratio between Profit After Taxes and Total Assets. (‡) Net Leverage is defined as the ratio between the sum of short-run debt, long-run debt minus cash in the numerator, and total assets in the denominator. (*) Debt-to-EBITDA is the ratio between the sum of short-run debt and long-run debt in the numerator, and profits minus depreciation in the denominator. (§) Long-run Leverage is the ratio between long-run debt and total assets. (¶) Total leverage is the ratio between the sum of short-run and long-run debt in the numerator and total assets in the denominator.

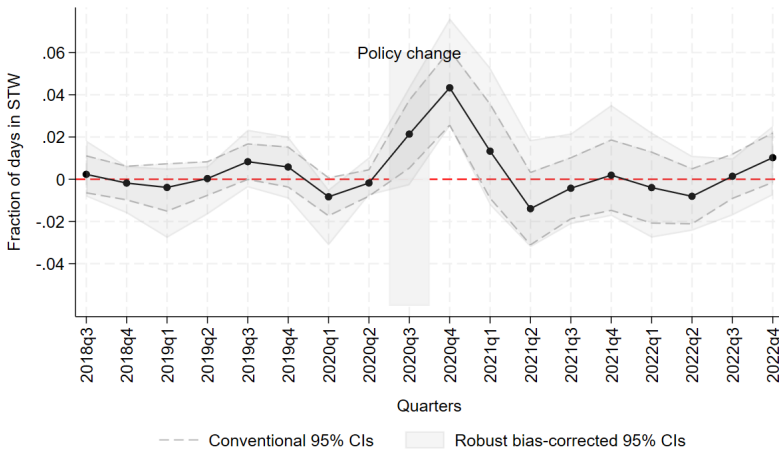
4.C. Further Discussion and Results

Table C.2.3. Placebo outcomes sensitivity analysis.

Placebo Outcome	CER-Optimal Bandwidth	RDD (ITT) estimator	Robust (BC) Inference		Effective Number of Obs.	Clusters or firms
			p-value	Conf. Int.		
Sample I: Partially lockdown affected firms						
Vacations in FTE per worker	1.86	-0.145	0.021	[-0.253, -0.0203]	2994	987
Other equivalent performance in FTE per worker	2.6	-0.1	0.375	[-0.380, 0.143]	2190	1017
Sample II: Lockdown-unaffected firms						
Vacations in FTE per worker	3.47	0.068	0.106	[-0.022, 0.2330]	3550	1299
Other equivalent performance in FTE per worker	3.45	-0.098	0.084	[-0.561, 0.035]	2455	1117

Notes: This table shows the ITT RDD estimates for two different placebo outcomes that can affect working time others than STW. The first one is vacations in FTE per worker, and the second one is other equivalence performance time in FTE per worker.

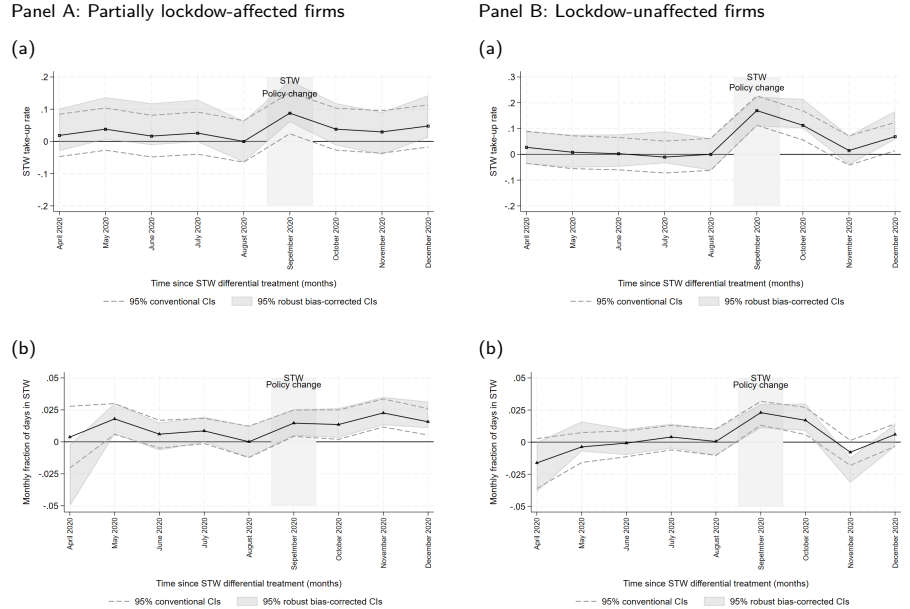
Figure C.2.3. Monthly fraction of days in STW as a function of percent of contractual days in STW in 2020-Q2 (using quarterly ONSS/RSZ statistics).



Notes: This figure reports the coefficient θ^{FS} from estimating Equation (4.1) using the monthly fraction of days in STW (proxied with quarterly data) as an outcome variable for each time period independently. The estimation is performed for the total number of firms included in the analysis. Estimates are obtained using a local polynomial density estimator with a bandwidth of 5.p.p.

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Figure C.2.4. Monthly take-up of STW as a function of percent of contractual days in STW in 2020-Q2 (using ONEM/RVA monthly statistics).



Notes: These figures show the coefficient θ from estimating Equation (4.1), using the monthly take-up rate (Figures a) and the monthly fraction of days in STW (Figures b) as outcomes. Estimates are obtained from separate regressions for each month. Effects are reported taking as a reference period one month prior to September 2020. The coefficient associated with the month of August is not statistically significantly different from zero. Panel A presents results for partially lockdown-affected firms, and Panel B for lockdown-unaffected firms. The bandwidth is 4 percentage points (on each side of the cutoff) for partially lockdown-affected firms, and 3.5 or 4 percentage points for lockdown-unaffected firms, depending on the outcome. Conventional 95% confidence intervals based on robust standard errors clustered at the firm level, as well as robust bias-corrected confidence intervals, are reported.

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Table C.2.4. Bandwidth sensitivity analysis for treatment defined as the take-up rate in September: partially lockdown-affected firms.

R	Bandwidth	Conventional			Robust bias-corrected 95% CI	Number of observations	No. firms
	(0.1*R)	RDD LATE effect	Standard Error	P-value			
Panel A. Outcome: volume of work per worker in logs in FTE							
0	3.5	-0.644	0.233	0.006	[-1.740 , -0.474]	6580	1946
1	3.85	-0.62	0.207	0.003	[-1.532, -0.414]	7202	2127
2	4.2	-0.589	0.19	0.002	[-1.407, -0.385]	7905	2326
3	4.55	-0.556	0.179	0.002	[-1.331, -0.368]	8605	2509
-1	3.15	-0.686	0.267	0.010	[-2.083, -0.619]	5983	1768
-2	2.8	-0.783	0.339	0.021	[-2.619, -0.730]	5306	1563
-3	2.45	-0.971	0.488	0.046	[-3.646, -0.877]	4702	1385
Panel B. Outcome: jobs in logs							
0	3.5	0.643	0.337	0.056	[0.302 , 2.192]	6812	1950
1	3.85	0.525	0.294	0.074	[0.304, 1.961]	7448	2131
2	4.2	0.405	0.27	0.134	[0.286, 1.814]	8176	2331
3	4.55	0.349	0.256	0.173	[0.196, 1.653]	8893	2508
-1	3.15	0.794	0.395	0.044	[0.249, 2.454]	6191	1772
-2	2.8	0.89	0.496	0.073	[0.295, 3.048]	5498	1567
-3	2.45	1.04	0.682	0.127	[0.368, 4.165]	4869	1389

Notes: This table test for the sensitivity of the estimates (for two main outcomes) across bandwidths for partially lockdown-affected firms. The benchmark bandwidth is defined as 3.5 p.p.

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Table C.2.5. Bandwidth sensitivity analysis for treatment defined as the take-up rate in September: lockdown-unaffected firms.

R	Bandwidth	Conventional			Robust bias-corrected 95% CI	Number of observations	No. firms
	(0.1*R)	RDD LATE effect	Standard Error	P-value			
Panel A. Outcome: volume of work per worker in logs in FTE							
0	3.5	-0.021	0.156	0.893	[-1.675, 0.177]	9065	2021
1	3.85	-0.025	0.144	0.864	[-1.564, 0.227]	10025	2243
2	4.2	-0.051	0.142	0.72	[-0.486, 0.299]	10962	2432
3	4.55	-0.091	0.138	0.512	[-0.413, 0.359]	11851	2615
-1	3.15	-0.047	0.175	0.787	[-0.812, 0.140]	8203	1835
-2	2.8	-0.087	0.204	0.669	[-0.975, 0.143]	7163	1616
-3	2.45	-0.117	0.2	0.56	[-1.086, 0.016]	6357	1441
Panel B. Outcome: jobs in logs							
0	3.5	0.007	0.172	0.966	[-0.995, -0.016]	9354	2024
1	3.85	0.010	0.157	0.952	[-0.789, 0.106]	10342	2246
2	4.2	-0.003	0.155	0.987	[-0.696, 0.190]	11307	2436
3	4.55	0.001	0.15	0.994	[-0.635, 0.230]	12221	2619
-1	3.15	-0.106	0.206	0.608	[-1.136, 0.048]	8471	1838
-2	2.8	-0.398	0.288	0.167	[-0.710, 0.987]	7397	1618
-3	2.45	-0.407	0.289	0.159	[-0.340, 1.369]	6564	1443

Notes: This table test for the sensitivity of the estimates (for two main outcomes) across bandwidths for lockdown-unaffected firms. The benchmark bandwidth is defined as 3.5 p.p.

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Table C.2.6. Bandwidth sensitivity analysis for treatment defined as the fraction of days in STW in September: partially lockdown-affected firms.

R	Bandwidth	Conventional			Robust bias-corrected 95% CI	Number of observations	No. firms
	(0.1*R)	RDD LATE effect	Standard Error	P-value			
Panel A. Outcome: volume of work per worker in logs in FTE							
0	3.5	-4.902	1.619	0.002	[-11.338,-2.925]	6580	1946
1	3.85	-4.864	1.501	0.001	[-10.383,-2.533]	7202	2127
2	4.2	-4.700	1.413	0.001	[-9.907,-2.475]	7905	2326
3	4.55	-4.439	1.334	0.001	[-9.665,-2.592]	8605	2509
-1	3.15	-5.177	1.827	0.005	[-12.775,-3.353]	5983	1768
-2	2.8	-5.403	2.012	0.007	[-14.353,-4.008]	5306	1563
-3	2.45	-5.954	2.341	0.011	[-17.056,-4.988]	4702	1385
Panel B. Outcome: jobs in logs							
0	3.5	4.588	2.724	0.092	[-0.284, 14.832]	6812	1950
1	3.85	3.922	2.455	0.11	[0.291, 14.063]	7448	2131
2	4.2	3.11	2.273	0.171	[0.775, 13.601]	8176	2331
3	4.55	2.698	2.147	0.209	[0.473, 12.654]	8893	2514
-1	3.15	5.533	3.152	0.079	[-2.047, 15.182]	6191	1772
-2	2.8	5.526	3.465	0.111	[-1.965, 16.567]	5498	1567
-3	2.45	5.527	3.926	0.159	[-1.625, 18.837]	4869	1389

Notes: This table test for the sensitivity of the estimates (for two main outcomes) across bandwidths for partially lockdown-affected firms. The benchmark bandwidth is defined as 3.5 p.p.

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Table C.2.7. Bandwidth sensitivity analysis for treatment defined as the fraction of days in STW in September: lockdown-unaffected firms.

R	Bandwidth	Conventional			Robust bias-corrected 95% CI	Number of observations	No. firms
	(0.1*R)	RDD LATE effect	Standard Error	P-value			
Panel A. Outcome: volume of work per worker in logs in FTE							
0	3.5	-0.536	0.634	0.398	[-1.854, 1.712]	13436	2985
1	3.85	-0.610	0.617	0.323	[-1.831, 1.649]	14796	3288
2	4.2	-0.675	0.605	0.264	[-1.817, 1.585]	16081	3572
3	4.55	-0.736	0.603	0.222	[-1.828, 1.543]	17662	3955
-1	3.15	-0.438	0.65	0.500	[-1.887, 1.749]	11921	2630
-2	2.8	-0.193	0.679	0.776	[-2.403, 1.359]	10616	2362
-3	2.45	-0.104	0.726	0.886	[-3.030, 0.941]	9204	2056
Panel B. Jobs in logs							
0	3.5	0.021	0.6642961	0.975	[-2.504, 1.341]	13862	2989
1	3.85	0.250	0.6406158	0.696	[-2.572, 1.141]	15261	3292
2	4.2	0.392	0.6233348	0.529	[-2.445, 1.159]	16576	3576
3	4.55	0.508	0.6169171	0.41	[-2.326, 1.231]	18200	3960
-1	3.15	0.002	0.6869804	0.997	[-2.869, 1.085]	12294	2634
-2	2.8	-0.003	0.7239477	0.996	[-3.342, 0.792]	10953	2366
-3	2.45	0.043	0.7810236	0.956	[-4.396, 0.046]	9497	2059

Notes: This table test for the sensitivity of the estimates (for two main outcomes) across bandwidths for lockdown-unaffected firms. The benchmark bandwidth is defined as 3.5 p.p.

4.C. Further Discussion and Results

Table C.2.8. Donut hole sensitivity analysis for RDD (LATE) estimates – Treatment: take-up in September

Donut-hole Radius	Bandwidth	RD (LATE) Estimator	Robust Inference		Number of Observations	Number of clusters (or firms)	Excluded firms		
			P-value	Conf. Intervals			Left	Right	Total
Sample I: Firms with protected sectors									
Panel A. Outcome: Volume of work per worker in FTE									
0	3.5	-0.6442	0.001	[-1.740, -0.474]	6580	1946	0	0	0
0.1	3.5	-0.4032	0.029	[-0.875, -0.047]	6389	1882	21	43	64
0.2	3.5	-0.713	0.008	[-2.338, -0.747]	6177	1827	52	67	119
Panel B. Outcome: Jobs in logs									
0	3.5	0.643	0.010	[0.302, 2.192]	6812	1950	0	0	0
0.1	3.5	0.902	0.000	[1.082, 2.558]	6611	1886	21	43	64
0.2	3.5	1.276	0.000	[2.696, 5.431]	6394	1831	52	67	119
Sample II: Firms with only non-protected sectors									
Panel A. Outcome: Volume of work per worker in FTE									
0	4	-0.0345	0.503	[-0.524, 0.257]	10413	2309	0	0	0
0.1	4	0.1072	0.639	[-0.442, 0.721]	10176	2243	15	51	66
0.2	4	0.3051	0.138	[-0.219, 1.578]	9881	2185	40	84	124
Panel B. Outcome: Jobs in logs									
0	4	0.006	0.187	[-0.738, 0.144]	10745	2312	0	0	0
0.1	4	0.187	0.637	[-0.419, 0.685]	10499	2246	15	51	66
0.2	4	-0.587	0.000	[-3.471, -1.412]	10191	2188	40	84	124

Notes: This table displays RDD (LATE) estimates using as treatment the take-up in September from monthly statistics (ONEM/RVA). The robust inference p-value and confidence intervals, corresponds to the robust bias correction.

4. PROTECTING JOBS AND FIRMS: THE IMPACT OF SHORT-TIME WORK DURING COVID-19

Table C.2.9. Artificial cutoffs – Treatment: monthly fraction of jobs in STW in September 2020.

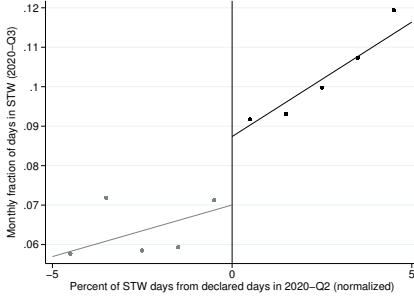
Alternative Cutoff	MSE-Optimal Bandwidth	RD (LATE) Estimator	Robust Inference		Number of (Eff.) Observations		Number of firms		
			P-value	Conf. Int.	Left	Right	Left	Right	
Sample I: Firms with directly affected workers									
Panel A. Outcome: Volume of work per worker in FTE									
-5	1.245	-2.402	0.917	[-14.001, 15.572]	1274	1342	368	385	
-4	0.678	0.072	0.808	[-0.5110, 0.656]	746	680	203	190	
-3	0.465	-0.750	0.506	[-1.725, 0.851]	486	378	147	107	
-2	0.594	-4.973	0.421	[-22.509, 53.831]	575	511	162	154	
0	3.338	-0.645	0.000	[-1.883, -0.557]	3204	3113	947	918	
2	1.589	1.636	0.058	[-16.209, 0.280]	1473	1342	434	413	
3	1.556	1.804	0.942	[-4.169, 4.493]	1349	1341	421	387	
4	0.784	-1.807	0.281	[-9.588, 2.786]	606	647	189	175	
5	1.072	-3.418	0.694	[-25.883, 17.229]	906	906	246	276	
Panel B. Outcome: Jobs in logs									
-5	1.245	-0.819	0.141	[-1.595, 11.221]	1314	1378	369	385	
-4	0.678	-1.324	0.146	[-3.107, 0.4597]	765	695	203	190	
-3	0.465	1.061	0.906	[-1.988, 1.761]	502	392	147	107	
-2	0.594	-1.119	0.947	[-16.179, 17.315]	592	526	163	154	
0	3.338	0.709	0.012	[0.285, 2.293]	3316	3224	949	920	
2	1.589	-6.750	0.053	[-0.440, 61.683]	1529	1392	435	414	
3	1.556	-0.127	0.914	[-2.393, 2.671]	1397	1385	423	388	
4	0.784	-0.928	0.133	[-5.988, 0.793]	629	667	189	177	
5	1.072	19.526	0.531	[-184.777, 95.297]	938	937	248	277	
Sample II: Firms with only non-directly affected workers									
Panel A. Outcome: Volume of work per worker in FTE									
-5	1.974	0.88249	0.983	[-19.493, 19.931]	3363	2964	1163	1029	
-4	0.922	1.5468	0.925	[-33.234, 36.588]	1445	1255	452	428	
-3	0.519	3.3003	0.877	[-15.743, 18.444]	773	702	289	258	
-2	0.433	-5.8623	0.925	[-27.609, 25.080]	626	606	256	260	
0	4.426	-0.36941	0.860	[-1.635, 1.3652]	6182	5408	2397	1939	
2	3.523	-44.613	0.478	[-369.647, 173.183]	2513	4051	580	1096	
3	2.971	-215.9	0.641	[-5528.06, 3404.33]	3478	3422	827	1006	
4	1.53	-10.357	0.376	[-42.950, 16.210]	1829	1701	683	691	
5	1.647	-1.3163	0.518	[-6.702, 3.380]	1993	1701	617	600	
Panel B. Outcome: Jobs in logs									
-5	0.847	406.55	0.746	[-7757.18, 5556.49]	1401	1417	597	530	
-4	1.111	23.045	0.421	[-36.965, 88.465]	1823	1640	695	648	
-3	0.879	-42.733	0.436	[-175.246, 75.618]	1331	1252	470	472	
-2	0.242	648.09	0.806	[-58548.9, 45519.1]	341	344	185	184	
0	4.426	-0.0081	0.333	[-2.850, 0.965]	3791	3482	1445	1296	
2	2.437	-53.729	0.619	[-298.319, 177.465]	2611	3014	581	768	
3	2.246	-796.79	0.842	[-37104, 30242.3]	2633	2807	828	868	
4	0.989	89.28	0.456	[-149.489, 332.77]	1341	1198	583	595	
5	0.966	-204.82	0.761	[-1409.2, 1031.25]	1164	982	560	536	

Notes: This table shows the RDD (LATE) estimates for two different main outcomes and two different set of firms for different cutoffs above and below the 0 cutoff which represents the 20% threshold. Panel A display estimates for the volume of work in FTE per worker and Panel B display estimates for the number of jobs in logs. The selected bandwidth is obtained by minimizing the MSE on each estimation. The treatment used for the estimation of the RDD (LATE) estimates is the fraction of jobs in STW in September 2020.

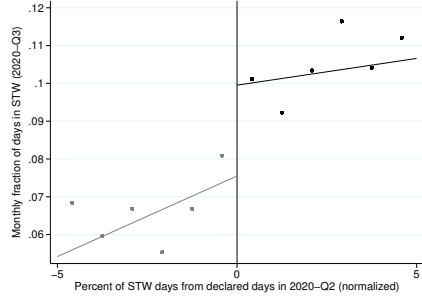
4.C.3 Additional results

Figure C.3.1. Fraction of monthly days in STW (proxied with 2020Q3 ONSS data) as a function of the percent of contractual days in STW in 2020-Q2.

(a) Panel A: Partially lockdown-affected firms.



(b) Panel B: Lockdown-unaffected firms.

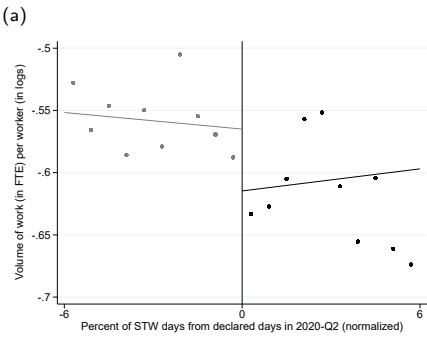


Notes: This figure shows the empirical relationship between the average STW take-up rate and the percent of contractual days in STW in 2020-Q2 (normalized to zero) within the 5 p.p. bandwidth around the cutoff, for (i) partially lockdown-affected firms (Panel A) and (ii) lockdown-unaffected firms (Panel B), respectively. It corresponds to the “first stage” estimate (θ^{FS}), that is obtained from estimating Equation (1) by replacing the dependent variable with STW_i , the treatment. In both panels, the horizontal axis is the percent of STW days from declared contractual days in 2020-Q2 normalized to zero in 5 bins to the left- and right-hand side of the cutoff. Each bin is of the size of 1 p.p. The estimates within the bandwidth of 5 p.p. around the cutoff are 1.7 (SE: 0.6) and 2.4 (SE: 0.5), for each groups of firms, respectively.

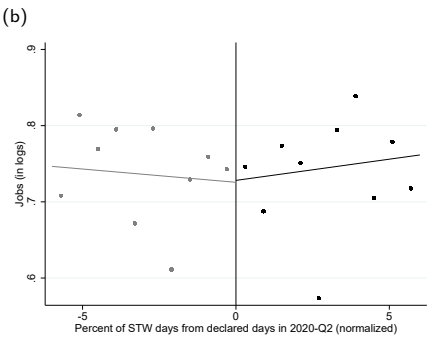
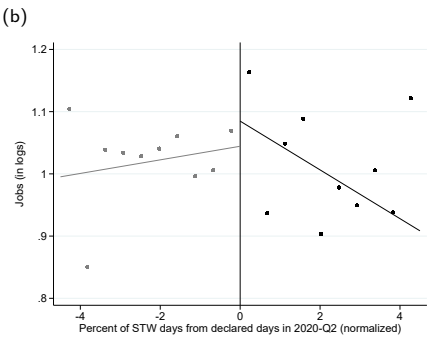
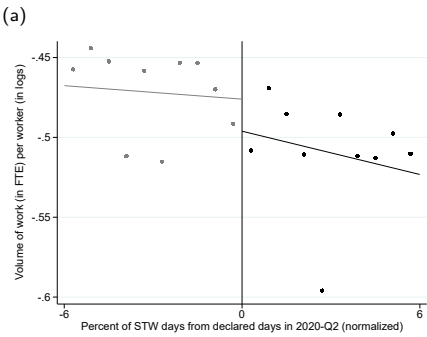
4. PROTECTING JOBS AND FIRMS: THE IMPACT OF SHORT-TIME WORK DURING COVID-19

Figure C.3.2. ITT estimates.

Panel A: Partially lockdown-affected firms.



Panel B: Lockdown-unaffected firms.



Notes: The MSE-optimal bandwidth used for each figure is determined by the data-driven procedure of Calonico et al. (2014a). Panel A displays an ITT RDD estimate of -0.05 (SE: 0.014) [95% robust bias-corrected CI: -0.12; -0.04] for Figure (a) and an ITT RDD estimate of 0.04 (SE: 0.032) [95% robust bias-corrected CI: 0.025; 0.185] for Figure (b). Panel (B) displays an ITT RDD estimate of -0.02 (SE: 0.012) [95% robust bias-corrected CI: -0.051; 0.016] for Figure (a) and an ITT RDD estimate of 0.003 (SE: 0.014) [95% robust bias-corrected CI: -0.056; 0.018] for Figure (b).

4.C. Further Discussion and Results

Table C.3.1. STW take-up impact on employment outcomes (log-like transformation: $\log(x+1)$)

	Partially Lockdown-affected firms		Lockdown-unaffected firms	
	Coefficient (SE)	Robust bias-corrected 95% CIs	Coefficient (SE)	Robust bias-corrected 95% CIs
Panel A. First stage.				
	0.114*** (0.023)		0.110*** (0.018)	
Panel B. Treatment Effects.				
Volume of work (in FTE) per worker	-0.338*** (0.179)	[-0.941 , -0.425]	-0.105 (0.058)	[-0.306 , 0.018]
Headcount employment (during-quarter)	0.482** (0.291)	[0.494 , 1.697]	0.0440 (0.113)	[-0.516 , 0.137]
Headcount employment (end-of-quarter)	0.379* (0.212)	[0.348 , 1.476]	0.051 (0.107)	[-0.513, 0.108]
Total Volume of work (in FTE)	0.144 (0.196)	[0.012, 1.046]	-0.060 (0.115)	[-0.669 , 0.002]
Wage per unit of work (EUR per FTE)	-0.158 (0.099)	[-0.560 , -0.023]	0.021 (0.106)	[-0.119, 500]
Wage per worker (EUR)	-0.665*** (0.212)	[-1.497, -0.390]	-0.118 (0.180)	[-0.548 , 0.468]
MSE-optimal bandwidth	3.9		3.7	
Number of Observations	7,240		9,389	
Number of firms	2,156		2,141	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In Panel A, the first-stage estimates are obtained by estimating Equation (4.1), replacing y_{it} with STW_i , an indicator variable for whether the firm takes up STW in September. In Panel B, the presented coefficients are obtained by estimating Equation (4.2) for $t = 1$, the first period of analysis, which corresponds to the quarter of the policy change (2020Q3). RDD estimates are derived from local polynomial nonparametric regressions of order 1, using a symmetric bandwidth. Since outcomes are expressed in logs, estimates can be interpreted as semi-elasticities. I employ the MSE-optimal bandwidth, selected using the volume of work (in FTE) per worker as the benchmark outcome, following the data-driven procedure of Calonico et al. (2014a). This same bandwidth is used for all other outcomes to enable the decomposition of the employment effect into intensive and extensive margins. Heteroskedasticity-robust standard errors clustered at the firm level are reported in parentheses. Robust bias-corrected confidence intervals, as proposed by Calonico et al. (2014b), are shown in brackets. For partially lockdown-affected firms, I set the bandwidth for estimating the bias correction to 6.5 (i.e., $\rho = 0.6$), which corresponds to the data-driven optimal bandwidth selected for bias estimation.

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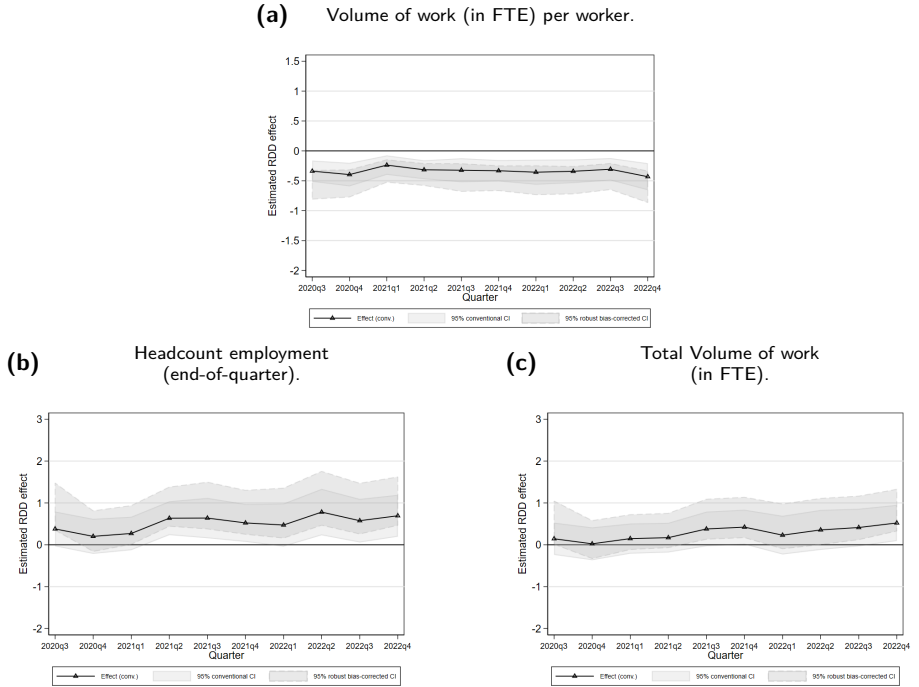
Table C.3.2. Impact of the fraction of days in STW on employment outcomes

	Partially Lockdown-affected firms		Lockdown-unaffected firms	
	Coefficient (SE)	Robust bias-corrected 95% CIs	Coefficient (SE)	Robust bias-corrected 95% CIs
Panel A. First stage.				
	0.015*** (0.0026)		0.022*** (0.0024)	
Panel B. Treatment Effects.				
Volume of work (in FTE) per worker	-3.974*** (1.1363)	[-8.8345 , -2.7122]	-0.902 (0.5474)	[-2.201 , 0.855]
Headcount employment (during-quarter)	2.537 (2.1586)	[1.1397 , 13.4626]	0.343 (0.6569)	[-2.6425 , 1.139]
Headcount employment (end-of-quarter)	1.825 (2.0611)	[-0.1689 , 11.5862]	0.339 (0.6245)	[-2.4726 , 1.120]
Total Volume of work (in FTE)	-1.438 (2.2321)	[-4.6368 , 7.6923]	-0.560 (0.8566)	[-3.8961 , 1.0461]
Wage per unit of work (EUR per FTE)	-1.032 (0.6938)	[-4.2252 , -0.1539]	-0.293 (0.4331)	[-0.7425 , 1.799]
Wage per worker (EUR)	-4.749*** (1.4122)	[-11.5229 , -3.6722]	-1.195 (0.7501)	[-2.3168 , 1.949]
MSE-optimal Bandwidth	4.76		5.81	
Number of Observations	8,634		14,557	
Number of Firms	2,884		3,394	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In Panel A, the first-stage estimates are obtained by estimating Equation (4.1), replacing y_{ic} with STW_i , which is the monthly fraction of days in STW. In Panel B, the presented coefficients are obtained by estimating Equation (4.2) for $t = 1$, the first period of analysis, which corresponds to the quarter of the policy change (2020Q3). RDD estimates are derived from local polynomial nonparametric regressions of order 1, using a symmetric bandwidth. I employ the MSE-optimal bandwidth, selected using the volume of work (in FTE) per worker as the benchmark outcome, following the data-driven procedure of Calonico et al. (2014a). This same bandwidth is used for all other outcomes to enable the decomposition of the employment effect into intensive and extensive margins. Heteroskedasticity-robust standard errors clustered at the firm level are reported in parentheses. Robust bias-corrected confidence intervals, as proposed by Calonico et al. (2014b), are shown in brackets.

4.C. Further Discussion and Results

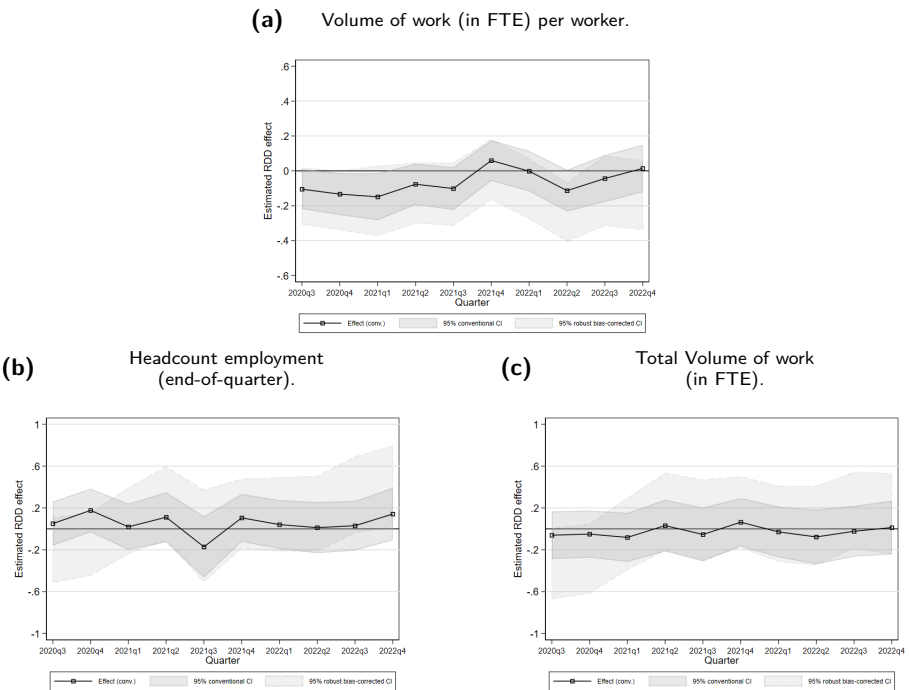
Figure C.3.3. Dynamic treatment effects (take-up rate): partially lockdown-affected firms (with log-like transformation: $\log(x + 1)$)



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (lighter shaded area with solid line). 95% robust bias-corrected confidence intervals are also shown (darker shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 3.9 around the cutoff. Outcomes are transformed using the $\log(x + 1)$ log-like transformation to deal with the presence of zeros. I set the bandwidth for estimating the bias correction to 6.5 (i.e., $\rho = 0.6$), which corresponds to the data-driven optimal bandwidth selected for bias estimation.

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Figure C.3.4. Dynamic treatment effects (take-up rate): lockdown-unaffected firms (with log-like transformation: $\log(x + 1)$).



Notes: These figures show treatment effects (solid black lines with squares), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (lighter shaded area with solid line). 95% robust bias-corrected confidence intervals are also shown (darker shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 3.9 around the cutoff. Outcomes are transformed using the $\log(x + 1)$ log-like transformation to deal with the presence of zeros. I set the bandwidth for estimating the bias correction to the same MSE-optimal bandwidth (i.e., $\rho = 1$).

4.C. Further Discussion and Results

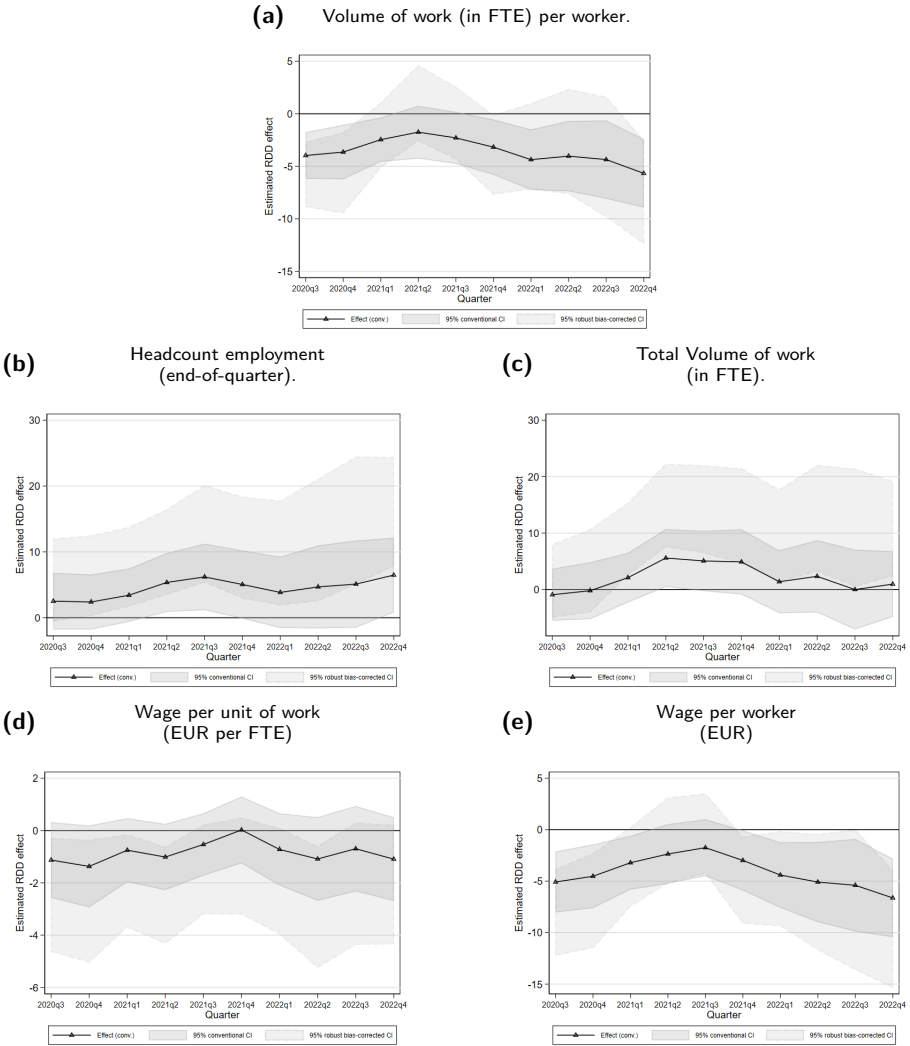
Table C.3.3. First stage (STW Take-up): heterogeneous effects across the fraction of white-collar workers.

	1. Partially lockdown-affected firms		2. Lockdown-unaffected firms	
	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)
Conventional	0.0476 (0.0303)	0.1863*** (0.0242)	0.1507*** (0.0209)	0.0744*** (0.0169)
Robust	0.0263 (0.0352)	0.1927*** (0.0372)	0.1688*** (0.0336)	0.0453 (0.0277)
Bandwidth (Left and right)	3.900	6.6642	5.3718	3.4228
B-C Robust CI (left)	-0.0427	0.1198	0.1030	-0.0091
B-C Robust CI (right)	0.0952	0.2656	0.2347	0.0996
F-stat (Stock and Yogo)	2.4649	59.1526	51.8229	19.3696
Observations	3,660	5,794	8,460	3,308
Firms	1,856	1,759	1,923	741

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered robust standard errors in parentheses at the firm level. The median fraction of white-collar workers for partially lockdown-affected firms is 0.25. The median for lockdown-affected firms is 0.75.

4. PROTECTING JOBS AND FIRMS: THE IMPACT OF SHORT-TIME WORK DURING COVID-19

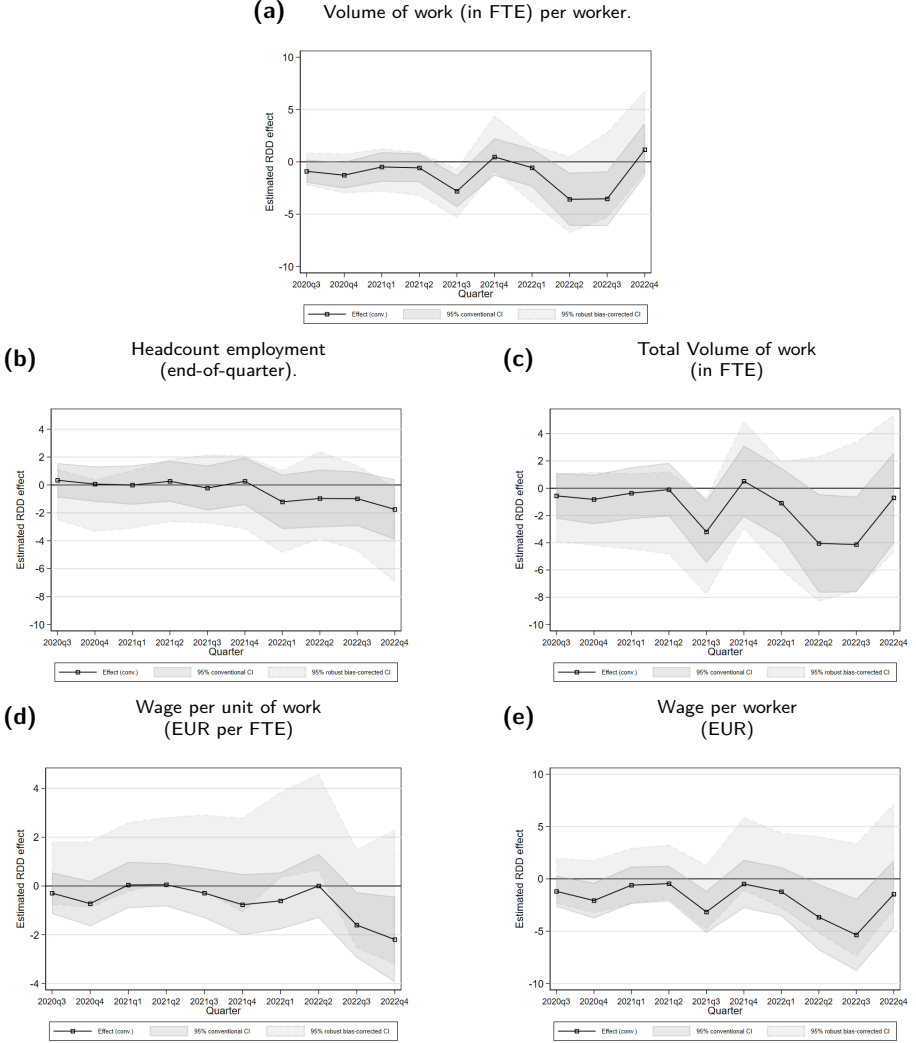
Figure C.3.5. Dynamic treatment effects (fraction of days in STW): partially lockdown-affected firms.



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 4.76 around the cutoff.

4.C. Further Discussion and Results

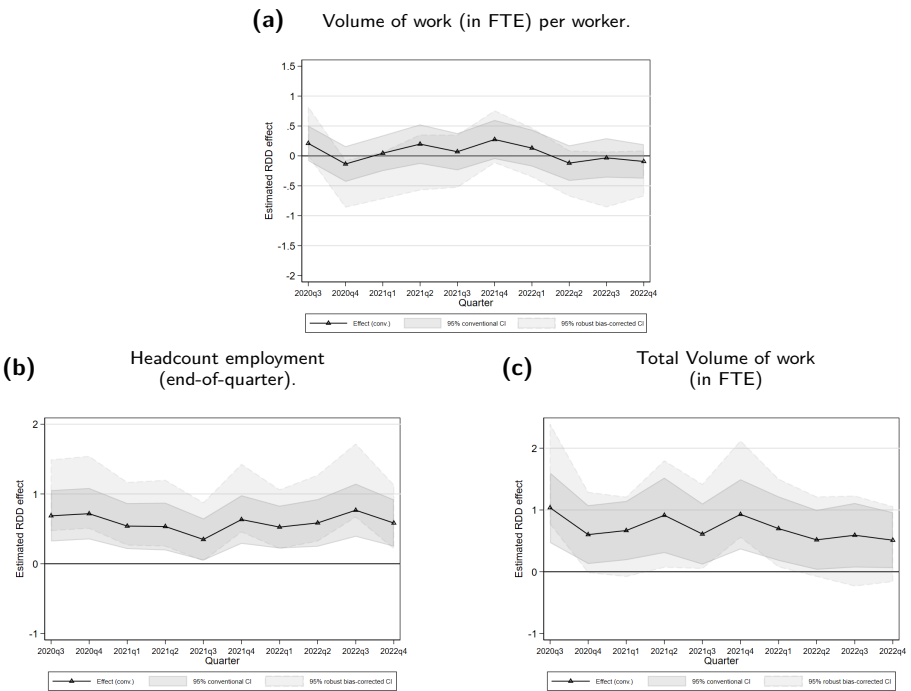
Figure C.3.6. Dynamic treatment effects (fraction of days in STW): lockdown-unaffected firms.



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). The treatment is defined as the fraction of days in STW in September 2020. 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 5.81 around the cutoff.

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Figure C.3.7. Dynamic treatment effects: partially lockdown-affected firms (workers not concerned by the policy change).



Notes: These figures show treatment effects (solid black lines with triangles), based on the RDD estimator of Equation (4.2), as well as 95% conventional confidence intervals (darker shaded area with solid line). The treatment is defined as an indicator variable that is equal to one if the firm takes up STW in September 2020. 95% robust bias-corrected confidence intervals are also shown (lighter shaded area with dashed line). The coefficients are obtained from a separate regression for each quarter starting from 2020Q3, the quarter when the policy change occurs. All specifications use a common bandwidth of 3.9 around the cutoff.

4.C. Further Discussion and Results

Table C.3.4. First stage (fraction of days in STW): heterogeneous effects across the fraction of white-collar workers.

	1. Partially lockdown-affected firms		2. Lockdown-unaffected firms	
	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)
Conventional	0.0223*** (0.0033)	0.0072** (0.0032)	0.0349*** (0.0039)	0.0057* (0.0029)
Robust	0.0233*** (0.0051)	0.0095** (0.0044)	0.0411*** (0.0061)	0.0033 (0.0041)
Bandwidth (Left and right)	7.0727	4.1835	5.5460	5.6162
B-C Robust CI (left)	0.0133	0.0010	0.0292	-0.0047
B-C Robust CI (right)	0.0334	0.0181	0.0531	0.0114
F-stat (Stock and Yogo)	45.0270	5.0653	80.3950	3.7191
Observations	7,074	3,728	8,752	5,692
Firms	2,027	1,125	2,000	1,273

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Clustered robust standard errors in parentheses at the firm level. The median fraction of white-collar workers for partially lockdown-affected firms is 0.25. The median for lockdown-affected firms is 0.75.

General Conclusion

This thesis investigates the effectiveness of STW programs as countercyclical labor market instruments during periods of severe economic downturn. It offers a comprehensive account of how STW can prevent job destruction and support firm survival when productivity shocks are temporary but firms retain growth potential. Drawing on Belgium's experience during the Great Recession and the COVID-19 crisis, the thesis contributes to both the academic literature and contemporary policy debates on labor market resilience.

A core insight emerging from this research is that STW can effectively preserve employment relationships during large-scale shocks, but its success critically depends on institutional design and policy targeting.

The first chapter presents the theoretical foundations from the literature and compares institutional designs across Europe. It shows that, unlike traditional unemployment insurance, STW allows firms to preserve firm-worker matches and firm-specific human capital. However, its effectiveness hinges on three key features: credible monitoring, meaningful co-insurance, and eligibility conditions that reflect actual firm-level distress. Without these features, STW risks generating inefficiencies through ex-ante or ex-post moral hazard.

The second chapter presents a causal impact evaluation based on a

5. GENERAL CONCLUSION

quasi-experimental design of Belgium's STW policy during the Great Recession—a period marked by high take-up rates and longstanding eligibility rules favoring blue-collar workers. Exploiting variation in pre-crisis workforce composition and sectoral turnover shocks with an instrumental variable (IV) approach, the analysis finds that STW reduced hours worked without significantly affecting average employment or wages. Nonetheless, small manufacturing firms—those exposed to global supply chain disruptions and financial constraints—experienced short-term employment gains. This suggests that the effectiveness of STW depends strongly on the interaction between firm characteristics and sectoral exposure.

The third chapter turns to the COVID-19 crisis, during which Belgium expanded and simplified its STW scheme. Exploiting a temporary policy reform that introduced an eligibility threshold, the chapter applies a fuzzy regression discontinuity design (RDD) to estimate the program's causal impacts. The results show that access to the simplified scheme significantly increased take-up and preserved employment in partially lockdown-affected firms, with effects persisting for up to two years. White-collar-intensive firms—previously constrained by stricter administrative requirements—responded strongly at the extensive margin to the reform. By contrast, blue-collar-intensive firms, already enjoying easier access, responded mainly at the intensive margin, suggesting that the simplified and generous scheme may have encouraged overreliance. Among lockdown-unaffected firms, no significant employment effects were found, and blue-collar sectors less affected by demand shocks, such as construction and transportation, appear to have generated substantial deadweight losses. By contrast, white-collar-intensive firms benefited more from STW even in the absence of direct lockdown shocks, reflecting both the higher value of preserving job-specific human capital and their tighter liquidity constraints. These findings reinforce the idea that STW's effectiveness is shaped not only by shock severity but also by the economic value of preserving firm-worker matches.

Together, these chapters provide new evidence on the conditions under which STW is effective, where inefficiencies arise, and how program design can maximize social returns. Several concrete policy recommendations follow directly from these findings.

From the analysis of the Great Recession in Chapter 2, a first lesson concerns targeting blue-collar jobs. The results show that although STW substantially reduced working hours, average employment effects were limited, with the exception of small manufacturing firms that were highly exposed to global supply chain disruptions. By contrast, non-manufacturing firms showed little employment response despite widespread take-up, indicating significant deadweight losses. This suggests that eligibility conditions should be tied more closely to indicators of firm-level distress, such as sales declines or liquidity shortages, to ensure that support reaches jobs that are temporarily affected but have recovery potential. Conditioning support on commitments to preserve employment could further improve program cost-effectiveness, ensuring that employers allocate the program to job matches with valuable prospects. The findings also imply that a one-size-fits-all approach across sectors may be inefficient: more stringent criteria are needed in less-affected industries, while firms more exposed to economic shocks may justifiably receive greater support.

The analysis of the COVID-19 crisis in Chapter 3 highlights a different set of policy implications. The results show that white-collar-intensive firms benefited from the simplified scheme once administrative barriers were removed, reflecting both the high value of preserving their firm-specific human capital and their tighter liquidity constraints. This supports recent reforms that relax administrative requirements for white-collar firms, such as eliminating the need for collective agreements and formal applications to the Federal Public Service (FPS) Employment, Labour and Social Dialogue. At the same time, the evidence points to overreliance among blue-collar-intensive firms, which already had easier access and responded mainly at the intensive margin. For these firms, stricter eligibility conditions tied to observable financial distress would help limit deadweight losses, particularly in sectors like construction and transportation that were less directly affected by lockdowns. Finally, Belgium's prolonged use of a highly generous STW scheme during the pandemic—longer than in many peer countries—illustrates the risks of sustaining broad-based support once the acute phase of a crisis has passed. Future schemes should therefore incorporate dynamic adjustment mechanisms, gradually tightening eligibility and generosity as the crisis evolves to maintain cost-effectiveness while still protecting viable jobs.

Beyond these policy lessons, the thesis makes broader contributions to the literature on labor market policy in times of crisis. It provides one of the

5. GENERAL CONCLUSION

few causal evaluations of STW in Belgium, examines dynamic impacts rather than only short-term effects, and highlights the importance of heterogeneity across firms, sectors, and worker types. These findings resonate with recent international research (e.g., Cahuc et al., 2024; Giupponi and Landais, 2023; Kopp and Siegenthaler, 2021), while offering context-specific insights for countries with similar institutional frameworks.

Some limitations remain. The reliance on grouped and firm-level data restricts the analysis of worker-level heterogeneity, including impacts by gender, occupation, or contract type. Moreover, long-term outcomes—such as productivity and firm exit—could not be fully captured within the available period. Future research could address these limitations while also extending the analysis to new questions. For instance, future work could examine how STW interacts with structural transformations such as digitalization and the green transition, or with geopolitical shocks that reshape production, such as the Ukrainian conflict. In addition, assessing the distributional consequences of STW—who gains the most, and whether it reinforces or mitigates labor market dualism—would be crucial for equity considerations. Comparative studies across institutional contexts would also help evaluate the transferability of STW to emerging and developing economies.

This thesis underscores the critical role of well-designed labor market institutions in mitigating the effects of economic shocks. When timely, targeted, and administratively coherent, STW programs can function as effective automatic stabilizers—preserving jobs, firm-specific human capital, and productive capacity. Yet their success depends on careful targeting, dynamic adaptability, and the alignment of program generosity with crisis severity. In an era increasingly marked by systemic shocks—from pandemics to technological disruption, geopolitical instability, and climate risks—ensuring that labor market policies are both efficient and resilient remains a pressing challenge. This thesis contributes to understanding how that balance can be achieved.

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