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JOBS SAVED? EVALUATING THE IMPACT OF SHORT-TIME WORK IN PORTUGAL DURING COVID-19

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To my friends, for the laughter, comradery and encouragement.

To my girlfriend, for her love.

To my family, for everything.

Abstract: The COVID-19 pandemic presented a global economic challenge, prompting the unprecedentedly widespread implementation of job retention schemes. In Portugal, the government implemented *layoff simplificado*, a short-time work (STW) scheme designed to maintain jobs by subsidizing firms facing significant economic hardship. Using novel firm-level data and a differences-in-differences approach, this dissertation estimates the causal impact of *layoff simplificado* on employment and wages. Outcomes for firms that placed their entire workforce on STW in April 2020 are compared to a next-in-line control group of firms that remained untreated but experienced a similarly large revenue decline. I demonstrate that not using the next-in-line control group yields highly biased estimates due to strong negative self-selection into treatment by lower productivity firms. I conclude that the STW policy was effective in supporting both employment and wages, saving up to 50,000 jobs - equivalent to a 15% increase in unemployment had the policy not existed - and raising the firm-level average annual wage by as much as 1,500€. Furthermore, I find that although the policy generated relevant deadweight losses – meaning it supported jobs that were not at risk of being lost -, its fiscal savings may have been sufficient to offset its overall cost. The results of this dissertation contribute to the micro-level evidence on the efficacy of STW policies as well as the literature on the effectiveness of the COVID-19's policy-response in the short and medium-run.

JEL codes: J21, J22, J38, J63, H20

Keywords: COVID-19, Employment, Recession, Short-time work, Differences-in-differences

Resumo: A pandemia de COVID-19 constituiu um desafio económico global, levando à aplicação, sem precedentes, de políticas de retenção de emprego. Em Portugal, o Governo implementou o *lay-off simplificado*, um regime de redução temporária do horário de trabalho (*short-time work* - STW) com o objetivo de preservar postos de trabalho através da atribuição de apoios a empresas com quedas significativas de atividade. Recorrendo a microdados ao nível empresarial e a uma estratégia de *diferences-in-differences*, esta dissertação estima o impacto causal do *lay-off simplificado* sobre o emprego e os salários. Para este fim, comparam-se os resultados das empresas que, em abril de 2020, tinham todos os seus trabalhadores no programa de STW com um grupo de controlo *next-in-line* composto por empresas não tratadas, mas que experienciaram quedas similares de vendas. Ademais, demonstra-se que a não utilização deste grupo de controlo conduz a estimativas fortemente enviesadas, devido à autosseleção negativa para o tratamento de empresas com menor produtividade. Concluo que a política de STW foi eficaz no apoio ao emprego e aos salários, salvando até 50,000 postos de trabalho - o equivalente a um aumento de 15% do número de indivíduos desempregados se a medida não tivesse existido - e aumentando o salário médio anual das empresas em até 1,500€. Embora a política tenha gerado perdas de ineficiência relevantes, as poupanças fiscais decorrentes da sua implementação poderão ter sido suficientemente grandes para compensar o custo total do programa. Os resultados desta dissertação contribuem para a literatura de evidência microeconómica sobre a eficácia das políticas de STW e contribuem para a literatura que avalia, a curto e médio prazo, a resposta de política pública à COVID-19.

Códigos JEL: J21, J22, J38, J63, H20

Palavras-chave: COVID-19, Emprego, Recessão, Redução temporária do horário de trabalho, Diferenças-em-diferenças

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1. Introduction

What happens when a pandemic shuts down the global economy overnight? The answer appears to be an unprecedentedly large-scale use of job retention schemes.

Job retention policies seek to preserve jobs when companies face a temporary drop in demand by reducing their labour costs and supporting the incomes of workers whose hours are being reduced. In this way, they seek to provide an alternative to permanent layoffs, encouraging firms to adjust at the intensive rather than the extensive margin: reducing the number of hours worked per employee, rather than the number of employees.

In the OECD, up to 20% of employment was, at the peak, supported by job retention policies, which proved to be national government's main instrument to counter the economic fallout. As per OECD (2021)'s report, while preliminary evidence points to these job retention schemes having been effective in limiting job losses, the magnitude and permanence of this effect is subject to much uncertainty and requires further empirical study. In Portugal, the government's response to support firms and jobs was the unprecedently large-scale implementation of a short-time work scheme (STW) called *layoff simplificado*.

At this point a key initial distinction must be made. In the Portuguese context, the use of the word layoff has a significantly different connotation from how layoff is typically used in the international labour literature. In the Portuguese context, *layoff simplificado* is a job retention scheme – namely a short-time work scheme as defined by the OECD - wherein the workers' contract remains in force even if their work is fully suspended (OECD, 2021). In the labour literature, layoff is used for workers who have been dismissed from their jobs. To avoid confusion, henceforth the term layoff will be used in the latter ascension (job dismissal), whereas the full expression *layoff simplificado* will refer to the Portuguese STW scheme.

This dissertation examines the government's STW-related response to the pandemic by addressing several research questions. Was the unprecedentedly large-scale use of STW effective in maintaining employment in the Portuguese context? Did average wages increase at the firm level? If so, were these effects transitory or did they persist after the economy recovered? Were these effects heterogenous across firms? What are the policy implications of the program: was it cost-effective, and to what extent did it subsidize jobs that would have been retained in the absence of the policy?

In answering these questions, this dissertation explores a gap in the STW literature: most of the recent evidence on the efficacy of STW still uses data stemming from the Great Financial Crisis (Cahuc et al., 2021; Giupponi & Landais, 2023). The existing literature addressing specifically the COVID-19 crisis focuses mostly on the early effects of the crisis and mostly relies either on theoretical models (Albertini et al., 2022; Osuna & García Pérez, 2022) or on survey-based evidence (Adams-Prassl et al., 2020). Therefore, this dissertation contributes to the relatively scarce micro-level evidence on the causal effect of STW on employment as well as the overall understanding of the appropriateness of the COVID-19's policy-response both in the short and medium-term.

By addressing this gap, this dissertation offers several key findings. First, it demonstrates the biases that can arise when relying on inadequate counterfactuals, particularly in the context of COVID-era STW policies. To address this, a more credible *next-in-line* control group is proposed, using untreated firms that experienced revenue declines similar to those of treated firms. The results indicate that the estimated treatment effects not only increase in magnitude as the control group is restricted to more severely affected firms, but can even change sign from negative to positive. Second, it finds the policy had positive effects on firms' employment and workers' wages, which are evident in the year of policy implementation and persist for two subsequent years, suggesting that job losses were permanently avoided rather than merely postponed. Third, I do not find significant differences in policy effects across firms with different levels of liquidity, but the policy did support workers' wages significantly more in the least productive firms. Additionally, despite preventing the loss of a non-negligible number of jobs, the STW scheme also entailed significant deadweight losses by supporting jobs that were not actually at risk. Finally, depending on the chosen control group, the program's cost may have been fully offset by the fiscal savings from reduced unemployment benefit payments.

The rest of this dissertation is structured as follows: section 2 reviews the relevant literature on STW and briefly overviews other job retention schemes used during the COVID-19 crisis; section 3 describes the *layoff simplificado* policy and other related measures in Portugal; section 4 introduces the data, the model and estimation results using a biased control group; section 5 presents the main results of the dissertation; section 6 conducts a policy cost-effectiveness analysis and estimates deadweight losses; section 7 concludes, discusses the limitations of the dissertation and points to future avenues for research.

2. Literature Review

2.1. The Theoretical

In the past decade, interest in short-time work has risen substantially due to the increasing use of these policies during the Great Financial Crisis and, especially, during the recent COVID-19 pandemic. In simple terms, the purpose of STW schemes is to prevent firms from resorting to excessive and inefficient layoffs during a crisis, dismissing workers whose jobs would be viable once economic activity recovers (Hijzen & Venn, 2011).

From the worker's point of view, avoiding being dismissed is particularly relevant since job displacement may have permanent scarring effects even after the recovery has taken place. For instance, Gibbons and Katz (1991) argue that laid off workers suffer a post-displacement wage penalty due to negative signalling, wherein they are perceived to be of low ability. Evidence from the Great Recession supports the notion that workers' earnings may not return to pre-displacement trajectories (Lachowska et al., 2020).

For firms, STW schemes provide the opportunity to reduce labour costs while maintaining their existing trained workforce. Consequently, STW take-up rates are higher when companies face higher dismissal costs and exhibit higher levels of firm-specific human capital (Lydon et al., 2019). As such, STW should have a positive effect on firms' survival as it permits a relatively inexpensive downward adjustment in working hours (Kopp & Siegenthaler, 2021; Rinne & Zimmermann, 2012). While Cahuc et al. (2021) do not find effects of the policy on firm's survival, Kopp and Siegenthaler (2021) and Giupponi and Landais (2023) support this proposition, especially for pre-crisis low-liquidity firms. Interestingly, the latter propose framing labour hoarding with recourse to STW, as a transfer of resources across time - firms incur costs today to retain workers during low productivity periods in exchange for future gains. Thus, STW policies mitigate excessive layoffs by alleviating firms' – particularly small firms – liquidity constraints (Kopp & Siegenthaler, 2021).

Arguably, STW schemes can also prove both efficient and equitable. Unlike layoffs, which impose severe income and welfare losses on a small subset of workers, STW distributes the burden of reduced labour demand across a larger workforce, minimizing inequities and simultaneously preserving valuable job matches (Abraham & Houseman, 1993). Likewise, STW is considered more cost-efficient than other job retention schemes since, by preventing dismissals and reducing unemployment benefit payments, it partially or even fully offsets the

program's direct costs (Cahuc et al., 2021; Kopp & Siegenthaler, 2021). Moreover, STW can sustain aggregate demand and act as an automatic stabilizer, with higher take-up rates during recessions helping to absorb economic shocks (Balleer et al., 2016).

However, as with other government support programs, the calibration of STW schemes must account for potential drawbacks, namely sustaining inefficient levels of employment or working hours.

Abraham and Houseman (1993) address the broader context of job security regulations, acknowledging concerns that strong job protections may impede employers' ability to adapt to economic fluctuations. However, they argue that work-sharing measures can complement strong job-security provisions, enabling employers to adjust working hours rather than resorting to layoffs. Similarly, Burdett and Wright (1989) highlight that, while effectively reducing the reliance on inefficient temporary layoffs in favour of variations in hours per worker, STW systems can distort hours per worker, potentially leading to underemployment. Additionally, Van Audenrode (1994) emphasizes that the efficacy of STW programs is closely tied to their generosity, with insufficiently generous STW schemes failing to induce the desired flexibility, resulting in inefficiencies in both employment and working hours. By contrast, countries with more generous STW programs exhibit quicker and larger adjustments in working hours in response to labour demand changes

Several authors also emphasize the potential deadweight and displacement effects of STW schemes. The former occurs when subsidies are paid to firms for retaining jobs they would have kept regardless of the program. The latter arises when STW preserves jobs that remain unviable after recovery, locking workers into low-productivity positions and hindering job creation and labour market mobility from low to high productivity firms (Cahuc, 2024).

Imposing stringent eligibility requirements and having firms bear some of the cost of STW should help mitigate deadweight effects. Brey and Hertweck (2020) highlight that relaxed eligibility criteria during the Great Recession led to substantial deadweight losses. Likewise, Boeri and Bruecker (2011) estimate average deadweight losses for several OECD countries of 66%, noting that these are particularly relevant in milder rather than severe recessions.[1]

[1] To calculate the deadweight loss of STW, the number of jobs actually saved through STW was related to the total number of STW participants.

To prevent displacement effects, STW policies must consider the structural viability of firms and the persistence of economic shocks. A persistent shock may cause STW to have negative effects on labour reallocation, by keeping workers in low productivity firms where employment matches do not survive in the medium-run (Giupponi & Landais, 2023). Note that the COVID-19 shock might be permanent which can impact the efficacy and trade-offs of the implemented policies (Barrero et al., 2021). To be effective without hindering job creation, the OECD argues that STW schemes should be timely, targeted and temporary: they should provide timely support to workers and firms facing temporary economic downturns; supporting structurally unviable firms may hinder job creation and reallocation to more productive sectors (OECD, 2021).

2.2. The Empirical

Macroeconomic estimates, which have been the mainstay of the STW literature, evaluate the effects of STW at the national level and typically depend on data from various countries and a reduced number of observations. This limits their identification ability, as countries differ in many ways that affect labour markets, namely labour regulations. Nevertheless, most macroeconomic estimates point to positive effects on employment by allowing adjustment to occur at the intensive rather than the extensive margin (Abraham & Houseman, 1993; Cahuc & Carcillo, 2011).

Microeconomic estimates, on the other hand, are relatively scarce and have not been consensual in determining the efficacy of STW (Cahuc et al., 2018). Here, the main difficulty facing economists is a selection issue where firms that use STW may be inherently different from those that do not, biasing estimates (Hijzen & Martin, 2013).

In attempting to overcome this selection issue, Calavrezo et al. (2010) apply a propensity score matching (PSM) methodology and find that firms using STW are comparatively more likely to exit the market the year after implementation. However, this result may indicate unresolved selection bias issues, as highlighted by the critiques of Hijzen and Venn (2011) and Cahuc and Carcillo (2011). Similarly, using firm-level Japanese data, Kato and Kodama (2019) find no significant effect on employment, but find a long-run positive effect on firm performance as STW helps preserve firm-specific human capital that would otherwise be lost through temporary layoffs in a recession.

Recently, to address selection issues, a few papers have provided compelling firm-level evidence on the causal effects of STW during the Great Recession. Cahuc et al. (2021) analyse French administrative data and use an instrumental variables (IV) strategy that exploits variations in local approval rates and the severity of revenue shocks for identification. They find that STW effectively safeguards jobs, enabling rapid recovery post-recession. However, they also highlight substantial windfall effects, as less-affected firms also use STW to reduce working hours for jobs that were not at risk of being destroyed. In Switzerland, Kopp and Siegenthaler (2021) use a differences-in-differences approach, comparing outcomes for firms that successfully applied for STW with those who were unsuccessful. They find that STW take-up reduces layoffs and increases firms' survival rates, with these effects extending beyond the period of STW benefits, indicating that jobs were permanently rather than temporarily saved. In Italy, Giupponi and Landais (2023) uses administrative social security data and an IV strategy exploiting a variation in eligibility rules. The findings reveal that STW significantly increases the number of employees in a firm while reducing hours worked per employee. However, unlike in Kopp and Siegenthaler (2021), these employment effects do not hold after STW use ends, due to the persistent nature of the shock in the Great Recession in Italy.

In the context of the COVID-19 pandemic – where the literature on STW is still scarce -, several authors have relied on economic models. Albertini et al. (2022), calibrate their model with French data and find that STW was successful in preserving jobs and supporting consumption, at the expense of excessive hours reduction. Dengler and Gehrke (2021)'s model, using German data, supports STW's consumption stabilizing effect and Osuna and García Pérez (2022)'s model finds that Spain's STW had a sizeable effect in reducing the increase in unemployment and job destruction.

2.3. International Responses During COVID-19

It is worth noting that, although the *layoff simplificado* policy constitutes a short-time work scheme, it was not the only job retention measure employed by national governments to mitigate significant increases in unemployment. While all types of job retention schemes have the common goal of maintaining in force the contract between firms and workers, reducing labour costs and supporting workers' incomes, in general three categories can be distinguished. However, significant overlap exists between these categories, and the

definition of each is not consensual, particularly after the flexibilization of the legislation during the pandemic (Eichhorst et al., 2022; Müller et al., 2022).

Firstly, STW tends to differ from furloughs as the former supports firms in financing hours not worked, depending on the extent of working time reduction which can include full suspension; the latter typically provides direct payment to workers only if hours worked are reduced to zero. In fact, the UK's furlough scheme for example, where the government covered up to 80% of the workers' wage, initially excluded partial reduction in work - despite this, many workers continued to work while furloughed (Adams-Prassl et al., 2020). Nonetheless, estimates from the UK (Görtz et al., 2023) and Denmark (Bennedsen et al., 2020) indicate that, in the absence of a furlough policy, employers were more likely to opt for layoffs, effectively preserving jobs.

Second, wage subsidy policies preserve employment through subsidizing companies' wage bills, i.e., they support firms in case of a substantial drop in revenue, without necessarily linking this to a reduction in working hours. This was the policy adopted by the Australian government's Job Keeper Payment program, which is estimated to have saved as many as 800,000 jobs (Watson et al., 2022).

Finally, other countries - most notably the United States - relied primarily on traditional unemployment insurance during the pandemic. As noted by Giupponi et al. (2022), the core objective of the U.S. policy response was to insure workers' incomes against job loss through a substantial increase in the generosity of unemployment benefits. In contrast, European countries focused on preserving employment relations by subsidising reductions in working hours and supporting temporary layoffs. As a result, the United States experienced a sharper rise in unemployment relative to European countries. The authors conclude that, even though STW primarily protected labour market insiders - such as higher-income individuals - and unemployment insurance benefited more vulnerable outsiders - including younger or less-educated workers - the European strategy of prioritising employment retention was likely appropriate. STW proved effective in mitigating the social costs of excessive layoffs during the recession, while being more cost-effective. Moreover, the moral hazard associated with STW - where employers may overly rely on subsidized reductions in working hours - was considered more limited than that arising from unemployment insurance - which can reduce job search incentives among unemployed individuals.

3. Framing the Policy

3.1. *Layoff Simplificado*: the Portuguese Short-Time Work Solution

The economic crisis caused by the COVID-19 pandemic had nearly immediate and grave consequences on the health of the world economy, dragging national economies into a severe recession which was, at the time, unclear how long-lasting it would be and how it would reshape future economic activity.

The Portuguese economy was not an exception. For instance, using data from the Portuguese Institute of Employment and Professional Training, Ferreira et al. (2020) point to an increase in the number of unemployed individuals in continental Portugal of over 36%, when comparing figures from May 2020 and May 2019, as well as a disproportionate impact on less-educated workers. The authorities' actions to curtail the fallout were substantial: pandemic related expenses in 2020 reached as high as 3% of the nation's GDP, which, when combined with an even more substantial drop in fiscal revenue, reflected in a public deficit of 5.7% in 2020, compared with the 0.1% fiscal surplus of 2019 (Banco de Portugal, 2021).

Layoff simplificado – officially known as *Apoio Extraordinário à Manutenção de Contrato de Trabalho* - was introduced in Portugal through Decree-Law No. 10-G/2020 of March 26, building upon a framework first established in 1983 to regulate the suspension or temporary reduction of working hours, with the goal of simplifying and expanding the use of the Portuguese STW scheme. The program was initially planned to end in June 2020 but was later extended until the end of July. In January 2021, due to the development of the pandemic, it was again prolonged for 6 months by Decree-Law No. 6-C/2021 of 15 January.

Eligibility to the support was granted to firms experiencing "business crisis" determined by: (i) total or partial closures due to mandated shutdowns; (ii) significant disruptions in supply chains or cancellations of orders; or (iii) a sudden and severe revenue drop of at least 40% over the 30 days preceding the application, compared to the average of the two prior months, or the same period in the previous year, or, for firms operating for less than 12 months, the average since inception.

Under *layoff simplificado*, workers were entitled to receive at least two-thirds of their gross monthly wage, with a minimum equal to the national minimum wage and a maximum capped at three times the minimum wage, with employers responsible for directly compensating employees, and Social Security reimbursing 70% of the amount paid. Additionally, employers

were exempted from paying their share of Social Security contributions for affected employees during the scheme's duration but were prohibited from terminating employment contracts during the support period and for 60 days thereafter.

The most reliable information on the scope and cost of the *layoff simplificado* policy can be found in the reports produced by the *Tribunal de Contas* — the supreme audit institution responsible for overseeing public expenditure — and the Ministry of Finance's budget execution reports – see, for example, Tribunal de Contas (2022) or Direção-Geral do Orçamento (2021). According to data from these institutions, *layoff simplificado* was the single most expensive policy measure implemented by the Portuguese government in 2020 to mitigate the economic consequences of the COVID-19 pandemic. It entailed a direct fiscal cost of 817 million euros, alongside an estimated 470 million euros in foregone Social Security contributions. In 2021, an additional 366 million euros were allocated to the program due to a new general lockdown in January.

In total, nearly 850,000 workers across 104,000 businesses received support under this measure. As expected, given the timeline of the pandemic, the vast majority of support was disbursed in April and May 2020. Notably, 69% of cases involved a complete suspension of employment contracts, as opposed to partial reductions in working hours. The distribution of the policy across sectors and regions showed notable variation, with particularly pronounced differences by sector of economic activity. Some businesses – such as those with direct costumer interaction - were forced to close, while others were allowed to continue operating. Regional variation was also evident, but this largely reflects the natural geographic distribution of economic activity and employment – for example, Lisbon concentrated the highest number of workers enrolled in STW.

Preliminary models using survey-based evidence have pointed to *layoff simplificado* preventing an estimated 4.4 percentage point rise in unemployment, at a cost of 0,4% of GDP in the first semester of 2020 (Banco de Portugal, 2021). Likewise, in early models of the impact of the economic shock on business activity, Banco de Portugal (2020) reported that the percentage of Portuguese firms without sufficient liquidity to meet fixed costs increases non-linearly with the number of days of reduced activity. This percentage is more pronounced among larger firms and especially for those operating in the accommodation and food services sector. However, when *layoff simplificado* is adopted, the share of firms at risk of

liquidity shortfalls drops substantially - especially in the hardest-hit sector, where it falls by as much as 12 percentage points, from 31% to 19%.

3.2. Concurrent Policies in Portugal

The Portuguese government's response to the economic downturn was not limited to *layoff simplificado*. In this subsection, I briefly explore other concurrent policies enacted by the authorities to support firms and jobs. An exhaustive overview of the policies enacted during this period, along with detailed information on associated expenditures and the number of workers and firms covered, can be found in the reports produced by the Portuguese authorities, namely in reports by *Tribunal de Contas* and the Ministry of Finance's budget execution office – see Tribunal de Contas (2022) and Direção-Geral do Orçamento (2021).

Starting in August, with Decree-Law No. 46-A/2020 of July 30, *layoff simplificado* was complemented by *Apoio à Retoma* – officially *Apoio Extraordinário À Retoma Progressiva De Atividade* - a temporary wage subsidy scheme to support the progressive resumption of activity by afflicted firms. Although having a smaller reach, it supported 133,000 workers at a cost of 163 million euros from august to December 2020. In 2021, the program's expenditure of 500 million euros, surpassed the cost of *layoff simplificado*.

This diploma differed from *layoff simplificado* in several regards, notably: (i) restricting access to the support to a single parameter: a drop in revenues of at least 40% - which would later be revised to 25% in October; (ii) no longer allowing for a full suspension of the employment contract; (iii) by adopting a more flexible and generous approach to monetary support, progressively raising wage compensation for workers from two-thirds to four-fifths of their regular earnings.

Another key concern of the government was to ease the severe liquidity pressures faced by firms. To this end, Decree-Law No. 10-J/2020 of 26 March established the legal framework for two major financial support instruments: a public loan moratorium and state-guaranteed credit lines. These measures aimed to provide low-cost liquidity at a time when firms' revenues had collapsed due to lockdowns, and credit risk had sharply increased.

The public moratorium regime allowed the suspension of payments on existing loans for eligible firms and households. By December 2020, approximately one-third of corporate loans in Portugal were under moratorium, highlighting the substantial impact of this

mechanism on securing the business sector's liquidity. Complementing the moratorium, the government's state-guaranteed credit lines were aimed at supporting specific sectors and regions that had been most impacted by the pandemic. Notably, approximately 40% of the total value of new loans granted during this period carried public guarantees (Banco de Portugal, 2021).

Using firm and loan-level data to evaluate the impact of these policies, Mateus and Neugebauer (2025) concluded that guaranteed loans were predominantly extended to firms operating in sectors most adversely affected by the pandemic, such as accommodation, restaurants, and transport. Additionally, the data also revealed that firms with better pre-crisis credit profiles were more likely to receive guaranteed loans, whereas higher-risk firms were more inclined to rely on the moratorium.

A further significant policy measure implemented by the Portuguese government during the COVID-19 pandemic was the program *APOIAR*, designed to provide urgent liquidity support to firms operating in the sectors most severely affected by the crisis. The legal framework for the program was established through Ministerial Implementing Order n.º 271-A/2020, which approved the regulatory conditions and eligibility criteria under the newly created *Sistema de Incentivos à Liquidez*.

In 2021 alone, over 1 billion euros were distributed through this scheme, reaching approximately 100,000 businesses, making *APOIAR* the single most costly direct support measure implemented that year to mitigate the economic impact of the pandemic. Funded with European structural funds, the program aimed primarily at covering non-wage fixed costs for businesses facing sharp revenue losses due to public health restrictions. The support took the form of non-repayable grants, offering immediate treasury relief to eligible firms.

4. Building the Empirical Strategy

4.1. The Data

To answer the proposed research questions, this dissertation has leveraged novel firm-level administrative Social Security (SS) data generously provided by the *Banco de Portugal's* Microdata Research Laboratory (BPLIM, 2025). This dataset permits identifying and tracking – on a month-by-month basis - the companies that took-up *layoff simplificado* during the pandemic by verifying those that benefited from a special temporary discharge from Social Security payments to be made on behalf of their employees. This information was merged with detailed annual firm-level financial data from the *Banco de Portugal*'s Central Balance Sheet Database (CB) - including balance sheet, income statement and sector of economic activity information (BPLIM, 2024). Both datasets contain anonymized firm-level information, and access was granted exclusively through on-site use at the Porto branch of the *Banco de Portugal*.

The Social Security data used to identify firm-level policy uptake was provided by the *Banco de Portugal* in monthly observations covering the years 2020 and 2021. For each firm and month, the dataset included a variable indicating the share of the workforce under *layoff simplificado*, grouped by the data provider into six discrete categorical intervals:

1. Between 1 and 19% of the firm's workforce under *layoff simplificado*
2. Between 20 and 39% of the firm's workforce under *layoff simplificado*
3. Between 40 and 59% of the firm's workforce under *layoff simplificado*
4. Between 60 and 79% of the firm's workforce under *layoff simplificado*
5. Between 80 and 99% of the firm's workforce under *layoff simplificado*
6. 100% of the firm's workforce under *layoff simplificado*

Firms may differ not only in whether or not they accessed the policy, but also in the intensity and duration of their uptake. A single firm may receive the policy intermittently and at varying intensity levels throughout the observation period, while two otherwise similar firms may differ substantially in the number of months they benefited from the policy. This heterogeneity posed important challenges for treatment definition and causal identification.

A key identifying assumption in the structure of the data is that any worker with at least one day under *layoff simplificado* within a given month is considered as having been under *layoff simplificado* for the entire month. Additionally, the data only considers firms with employees,

thus excluding from the analysis firms that reported having zero employees. In all, 94,938 individual firms were present in the SS dataset.

Information on policy uptake was merged with detailed annual firm-level economic data, using yearly observations from 2016 to 2022, as provided by the Central Balance Sheet Database dataset. This time frame was chosen to ensure a balanced observation window that captures dynamics both before and after the COVID-19 shock, while minimizing the risk of contamination from unrelated shocks further back in time.

Some fundamental procedures for cleaning the data are identified here. For one, all firms with zero employees at any point in the observation window were removed from the data, to maintain coherence with the identified policy-using firms in the SS data. Secondly, all firms that registered null revenue figures for the years before 2020 (the start of the pandemic and thus the STW program) were removed from the sample, given that (the variation of) revenue figures were instrumental in choosing which firms received benefits. Third, the sample is restricted to firms that remained continuously active from 2016 to 2022. This selection necessarily excludes both treated and untreated firms that exited the market during the pandemic period. As a result, the estimated effects of *layoff simplificado* are identified only among surviving firms, introducing the possibility of survivorship bias.

The data was also removed of firms that presented less plausible characteristics, such as negative assets or negative sales revenue. Several relevant variables were trimmed at the 1st and 99th percentile to minimize the influence of extreme outliers, namely assets, cash, number of employees, salaries, gross value added and revenue. Importantly, firms were also trimmed at the 1st and 99th percentile based on their revenue variation in 2020, given that this criterion would be instrumental for the empirical strategy. All variables were transformed from nominal to real terms using 2012 prices (INE, 2025). Sector of economic activity (CAE) information - presented by the data provider at five-digit level - was aggregated to two digits.

In total, after all data cleaning operations, the baseline sample included 35,673 firms in the treated group and 120,830 firms in the untreated group, structured in approximately 1.1 million observations, homogeneously split across 7 years (2016-2022).

4.2. The Model

The challenge of developing a clear and convincing causal identification strategy to meet the research objectives proposed at the onset of the dissertation became evident upon reviewing the relevant research on STW schemes using microeconomic data.

Considering the existing credible micro-level literature, a few alternative strategies were identified, and ruled out, as candidates. The lack of a clear instrument, such as a variation in the eligibility criteria - e.g. Giupponi and Landais (2023) -, precluded the use of an instrumental variable (IV) approach in this dissertation. A regression discontinuity design (RDD) based on the 40% revenue drop threshold could, in principle, provide credible causality. However, this approach was not feasible because the available revenue data are annual, whereas eligibility for *layoff simplificado* was determined using monthly revenue changes. Furthermore, an approach similar to Kopp and Siegenthaler (2021)'s, comparing firms that applied for STW and were approved with those whose applications were rejected, was not attainable as the available data does not allow identification of rejected applicants.

Ultimately, an event-study differences-in-differences (DiD) model approach leveraging the possibility of direct administrative identification from the Social Security dataset was considered the most likely strategy to produce compelling results. A DiD is a quasi-experimental technique used to measure the causal effects of an intervention by comparing changes in outcomes between a treated and a control group before and after the intervention. An event-study approach simply refers to trying to estimate these causal effects across a range of time periods before and after the intervention.

The key identifying assumption in this strategy is that outcome trends would be the same in both groups of firms - treated and control - in the absence of treatment - benefiting from STW. Were it not for having received treatment, firms would have maintained their common/parallel trends (Angrist & Pischke, 2009). Importantly, the exogenous nature of the COVID-19 shock reduces the likelihood that firms could have anticipated the policy rollout.

Assuming treatment occurs simultaneously for all firms, this dissertation's event-study framework is simply

$$Y_{i,t} = \sum_{2016<t\leq 2022} \beta_t \cdot Treat_i \cdot Year_t + \alpha_i + \lambda_t + \theta_s + \varepsilon_{i,t} \qquad (4.1.)$$

Where $Y_{i,t}$ denotes the outcome of interest for firm *i* in year *t*. The main outcomes of concern in this dissertation are the natural logarithm of employment – simply the number of employees in the firm - and of average firm annual wage rate – calculated as total salaries divided by the number of employees. $Treat_i$ is a binary indicator equal to one for firms that received treatment and $Year_t$ is a calendar year dummy. β_t is the parameter of interest and captures the differential outcome for treated firms in each year relative to the control group: the effect of the STW. The equation also includes a vector of unobserved firm fixed-effects α_i; time fixed-effects λ_t controlling for shocks common to all firms in each year; and sector fixed-effects θ_s accounting for time-invariant industry-specific heterogeneity. The residual term is represented by $\varepsilon_{i,t}$. The observations window is 2016-2022 and year 2016 is omitted, so all estimates are in relation to this year.[2]

In the analysis, two samples are used: one encompassing the full population of firms, and a second restricted to firms with an average pre-treatment headcount of at least 10 employees. The latter is the preferred specification, as it is the only sample for which robust causal estimates are obtained. In Portugal, firms with fewer than 10 employees are classified as micro-firms and are subject to distinct regulatory conditions, including simplified labour obligations. This mirrors the approach in Giupponi and Landais (2023), which adopts a threshold of 15 workers to account for differential regulatory treatment in Italian labour law. Furthermore, several studies of the Portuguese labour market commonly condition on firm size. For example, studies such as Martins (2011) and Lourenço et al. (2022) restrict their samples to firms with at least 10 employees, while Addison and Teixeira (2001) and Blanchard and Portugal (2001) adopt a threshold of five employees. These restrictions may be due to several reasons, namely ensuring data comparability with foreign sources or to

[2] A common choice in event-study designs is to omit the period immediately prior to treatment as the baseline. However, following Miller (2023)'s guidance that baseline periods should be selected balancing the need for proximity to the treatment with the benefits of using a stable window, I omit 2016 so that all estimates are relative to a stable pre-treatment year. Using 2019 as the baseline would violate the parallel trends assumption. Therefore, I follow the logic of Jacobson (1993)'s seminal event-study paper, which sets the baseline several years before the shock to ensure a credible counterfactual.

mitigate the skewing effect of including an overwhelming number of micro-firms in the sample (characteristic to the Portuguese labour market structure).

Importantly these restrictions may also be used to account for inconsistencies and the differing motivations that guide firm behaviour. For example, Carreira et al. (2022) exclude firms with fewer than three employees, arguing that such firms are primarily motivated by the need to generate income for the owners and their families. Thus, including micro-firms (those with fewer than 10 employees) could add substantial unobserved heterogeneity, since their employment structures are often shaped by family arrangements and they may lack the capacity to engage with formal programs like STW. This heterogeneity can bias estimates and violate the parallel trends assumption. By restricting attention to firms with 10 or more employees, I reduce this contamination and obtain a more accurate estimate of policy impact. Nonetheless, this approach entails a trade-off: losing a large share of the Portuguese firm population. In the Appendix (section Robustness A1) I assess the sensitivity of this dissertation's main results when using alternative employment thresholds. These confirm the 10-person threshold as adequate given that causal inference is not reliably obtained when including smaller firms.

4.3. Treatment Definition and Avoiding Biased Counterfactuals

Defining Treatment

The mismatch between the monthly frequency of policy uptake data and the annual measurement of firm-level outcomes - compounded by monthly eligibility criteria for receiving benefits, as discussed in Section 3.1. - posed a significant challenge for identifying credible treatment and control groups. This temporal misalignment complicates causal inference, as policy exposure varies month to month, while outcome variables are only observed annually. Moreover, policy uptake is not binary but measured as a discrete intensity variable at the monthly level. As discussed when presenting the data, firms differ not only in whether or when they accessed the policy, but also in the intensity and duration of their uptake over time.

To address these issues, I adopt a simplifying assumption: treatment status is defined based solely on whether a firm received the policy - and at what intensity - in a single reference

month. This approach mitigates the difficulty of modelling cumulative or time-varying exposure and enables a clear and consistent classification of firms into treatment and control groups.

As illustrated in Figure 1, the number of firms participating in the STW programme fluctuated considerably over time, reflecting the successive waves of COVID-19 and the associated lockdown measures. These restrictions forced many firms to close temporarily or suffer substantial declines in revenue, as consumers remained at home under government-imposed mandates. The two main waves of the pandemic that affected Portugal are clearly identifiable in the data: the first occurred in April 2020, coinciding with the initial outbreak of the global pandemic; the second took place in early 2021, following the implementation of renewed containment measures. Accordingly, the number of firms enrolled in the STW programme shows two distinct peaks, each corresponding to these periods, with the highest level of enrolment recorded in April 2020, reaching approximately 80,000 firms. Furthermore, a closer examination of policy uptake in April 2020 in Figure 2, highlights the severity of the economic downturn, as the majority of firms had 100% of their workforce enrolled in the STW programme for at least one day.

Figure 1. Number of firms enrolled in STW per period

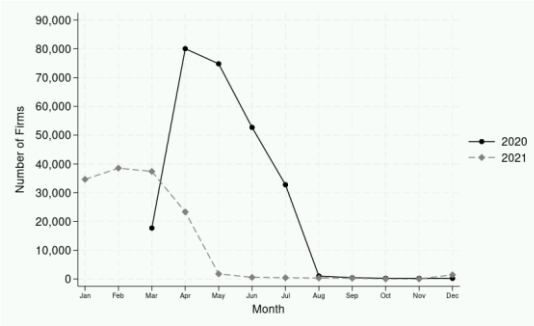
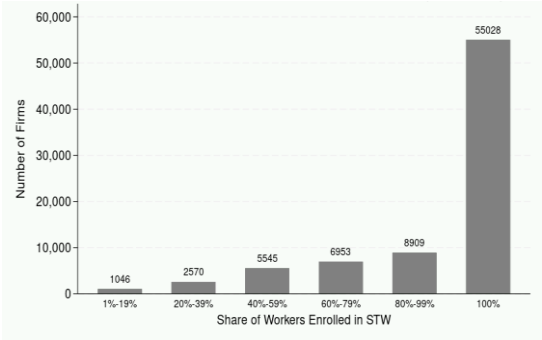


Figure 2. Distribution of firms by share of workers covered by STW (April 2020)



Notes: Author's elaboration using Social Security data for both Figure 1 and Figure 2.

Based on this information, treatment was defined solely in terms of a firm's participation in the programme during the peak of enrolment: April 2020. Specifically, the treatment group comprises firms that, in April 2020, had 100% of their workforce under the STW scheme. All other firms that received support at different intensities and/or at other points in time were excluded from both the treatment and control groups. A robustness test, presented in the Appendix, in section Robustness A2, assesses the sensitivity of this dissertation's results

by lowering the threshold for treated firms to 80% of their workforce under the STW scheme in April 2020. The obtained results are similar to the primary criteria, reinforcing the confidence in the estimates of policy effect.

Defining a *Biased* Counterfactual

The ability to identify an appropriate control group is critical to accurately evaluating the effectiveness of any policy intervention. For now, I define the control group as all firms that remained untreated.

As identified in the literature review section, the main problem in assessing the causal impact of STW using firm-level data is identifying an adequate control group: firms that participate in STW schemes are, often, less competitive than those that do not, before treatment occurs. Intuitively, firms that are "more fragile" before a crisis are likely to experience greater stress when confronted with a negative shock and are therefore more inclined to rely on government support. As a result, treated and untreated firms may differ fundamentally in ways that complicate causal inference. If not properly addressed, this can lead to biased estimates that may distort not only the magnitude but even the direction of the estimated treatment effects.

However, the specificity of the COVID-19 crisis may partially mitigate this problem, as the shock was largely exogenous and arguably unrelated to firms' pre-crisis performance (OECD, 2021). On the contrary, the widespread and deliberately loose eligibility criteria for *layoff simplificado* limits the presence of credibly exogenous variation in firms' exposure to short-time work schemes, making causal identification more challenging (Giupponi et al., 2022). Moreover, concurrent policy measures, such as the liquidity and credit relief programs identified in subsection 3.2., complicate the isolation of STW effects from other interventions.

Establishing causality requires that the parallel trends assumption holds. For a preliminary insight, I begin by visually inspecting the pre-trend *raw* behaviour of several variables of interest - i.e., without controlling for any of the factors accounted for in the formal event-study specification. To this end, Figure B1 in the Annex plots the observed evolution over time of treated and untreated firms with respect to several key variables: revenue, which was one of the primary criteria for accessing the STW scheme; employment and annual average

wage rate, which serve as the dependent variables in the event-study model; productivity, which may be a relevant factor in understanding which firms opted into the policy; and total expenditure per employee as a measure of payroll burden. Figure B1 tentatively points to these raw variables following similar pre-treatment paths, with the notable exception of employee headcount for which there is a slight pre-treatment divergence.

Additionally, one might reasonably argue that the pre-treatment level of firm's observable characteristics can help assess whether the treatment and control groups were already fundamentally different prior to the policy intervention. For instance, it may be plausible that firms with larger cash reserves in 2019 may have resorted to STW with less intensity, relying more heavily on internal funds rather than government support, relative to firms with more constrained liquidity.

Table 1 presents the distribution of several baseline characteristics for treated and untreated firms. Before the policy, treated firms were smaller in terms of assets, were less productive, and generated far lower annual revenues than their untreated counterparts. By contrast, average annual salaries and mean employee headcount were relatively similar across groups, although untreated firms had higher mean values for both variables. Importantly, the two groups exhibit a pronounced difference in revenue change from 2019 to 2020, with treated firms experiencing, on average, a substantially larger decline - losing approximately a quarter of their annual revenue - while untreated firms lost only about 2%. This disparity raises concerns regarding the direct comparability of the two groups.

However, this imbalance in *levels* should be interpreted with caution, as it does not necessarily imply that the two groups would have experienced divergent *trends* in the absence of treatment. Such imbalances may pose a threat to the validity of the parallel trends assumption insofar as firms with different initial conditions might have followed different trajectories in employment and average wages, even without policy intervention (Baker et al., 2025).

Moreover, imbalances in observable covariates might also help predict selection into treatment even before it occurred. To illustrate this point, consider once again the main difficulty in finding an adequate control group in STW schemes, as identified in the literature: treated firms tend to be less competitive than untreated firms, before the shock, causing them to disproportionately self-select into treatment. To inform this hypothesis, Table 2 regresses a set of observable variables in 2019 on the treatment dummy. The results indicate

that most variables were not statistically significant predictors of treatment status at the 5% level, with the exceptions of firm assets and productivity.

Table 1. Covariate balance - treated and untreated firms

	Untreated			Treated		
	Mean	**Median**	**SD**	**Mean**	**Median**	**SD**
Assets	928,896	186,724	(2,523,440)	401,566	140,471	(1,114,963)
Liabilities	1,438,002	98,942	(52,881,780)	263,706	84,801	(1,275,781)
Equity	1,038,755	70,569	(33,983,030)	169,281	46,212	(1,156,669)
Cash	100,338	21,886	(237,369)	56,125	16,816	(131,620)
Liquidity	0.22	0.13	(0.23)	0.22	0.14	(0.23)
Leverage	1.25	0.58	(17.36)	1.45	0.65	(30.05)
Gross Value Added	240,748	70,737	(523,186)	129,316	64,716	(243,041)
Revenue	771,762	183,706	(1,741,708)	401,279	173,775	(850,209)
Personnel Expenditures	250,572	47,466	(3,131,321)	91,862	49,083	(182,758)
Productivity	30,521	19,699	(71,017)	20,489	16,066	(29,653)
Δ% Revenue (2020)	-1.94	-1.99	(35.06)	-26.15	-26.29	(29.41)
Employment	7.73	3.00	(12.76)	6.04	4.00	(7.52)
Wage Rate	9,715	8,194	(8,345)	8,028	7,224	(8,570)
N	120,830			35,673		

Notes: The table presents the covariate balance for treated firms and the full universe of untreated firms as described in the text. Values are for 2019 and are in real terms real terms based on 2012 prices. The Δ% Revenue (2020) is calculated as the percentage change in annual revenue between 2019 and 2020. Productivity is calculated as gross value added per employee. Liquidity is cash divided by assets and leverage is liabilities divided by assets. Author's elaboration.

The interpretation of these results suggest that larger firms were less likely to receive treatment, arguably because they had alternative resources to absorb the shock. Notably, neither the pre-treatment number of employees nor the wage rate – the two outcome variables of interest - appear to significantly predict treatment status, mitigating potential reverse causality concerns. Finally, productivity is a strong negative predictor of treatment: less productive firms were significantly more likely to participate in the scheme. This finding is consistent with existing literature and raises important concerns regarding the validity and potential bias of the estimated policy effects.

Table 2. Selection into treatment analysis

	Coefficient	SE
Treated		
Log Revenue	0.039	(0.214)
Log Productivity	-0.275***	(0.096)
Log Assets	-0.274**	(0.126)
Log Cash	0.015	(0.017)
Log Liabilities	0.007	(0.061)
Log Employment	0.009	(0.186)
Log Wage Rate	0.126*	(0.068)
Constant	3.160**	(1.475)
N	131,438	

Notes: The table presents the results from a logit regression with the treatment dummy as the dependent variable and the regressors are firm characteristics measured in 2019. Standard errors are clustered at the sector level. * p<0.1, ** p<0.05, *** p<0.01. Author's elaboration.

A formal test of the parallel trends assumption is implemented by estimating the event-study specification in equation (4.1.). The pre-treatment interaction coefficients (2017-2019) assess whether the policy appears to have had any significant effects prior to its implementation. To support the validity of the parallel trends assumption, these coefficients should be statistically insignificant and small in magnitude. Conversely, conditional on the parallel trends assumption holding, statistically significant post-treatment coefficients (2020–2022) are interpreted as the dynamic causal effects of the STW policy.

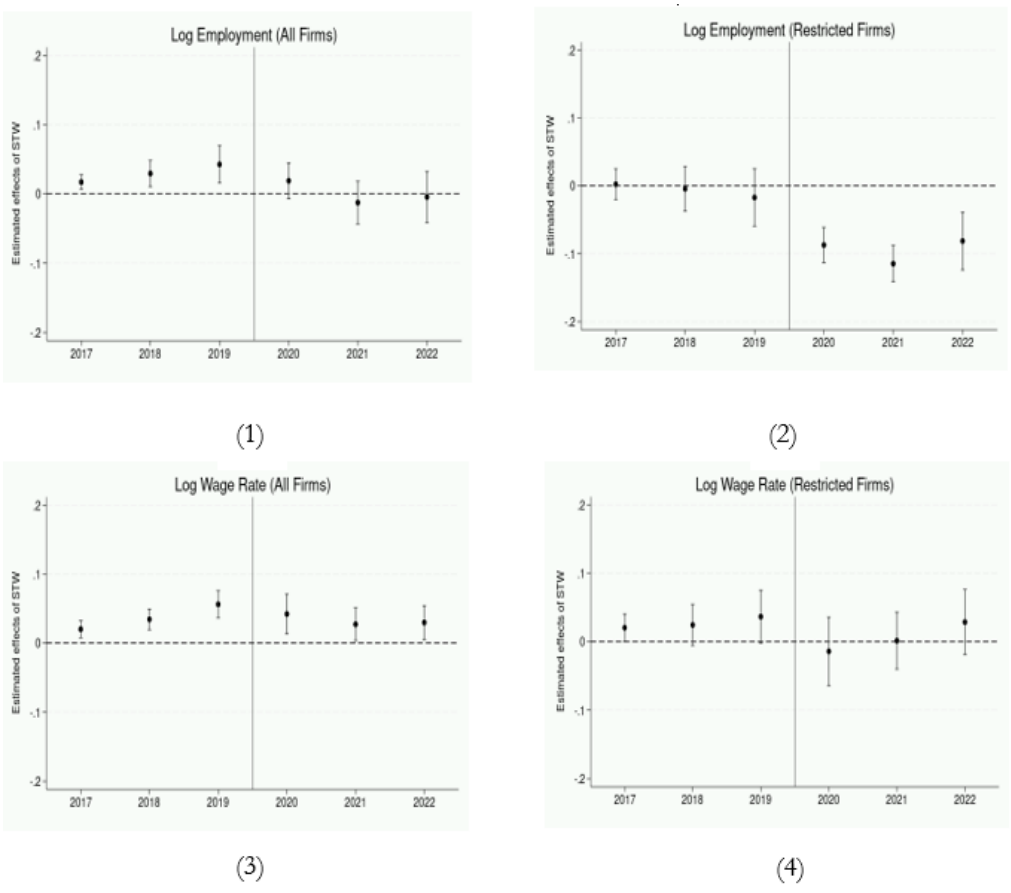
The results of this regression are summarised in Figure 3, which presents the dynamic event-study estimates of the impact of the STW policy on firms' employment (1–2) and average wage (3-4). Plots (1) and (3) report results for all firms in the sample and show a clear violation of the parallel trends assumption. Plots 2 and 4 restrict the analysis to firms with at least 10 employees, on average, before the pandemic. The employment estimates (2) satisfy the parallel trends assumption, showing no significant pre-treatment differences at the 5% level and revealing persistent employment declines between 2020 and 2022 among treated firms. The equivalent estimates for the wage rate (4) still show mild pre-trends in 2017 and insignificant treatment effects at the 5% level, suggesting no causal impact on wages.

Rather than confirming the validity of the specification, these results reinforce the concern that the analysis may still be affected by substantial selection bias. The objective of a STW policy is to preserve employment – which is consistent with the empirical evidence generally found in microeconomic and macroeconomic literature. Indeed, it is difficult to justify a

scenario in which a policy designed to reduce employers' payroll expenses - by allowing workers to be temporarily sent home with substantial government assistance - would immediately result in net job destruction in the first year of policy implementation. This lends support to the interpretation that these results likely reflect the persistent challenge found across the STW literature: treated and control firms are fundamentally different as treated firms were more prone to terminating employment contracts, even in the absence of the policy. This interpretation is also supported by the results in Table 2, which show that less productive firms were significantly more likely to receive treatment.

To address this, the next section introduces the main empirical specification: treated firms are compared to untreated firms that were the *next in line* for treatment.

Figure 3. Event-study: effect of STW on employment and wages (all untreated firms)



Notes: Event-study estimates based on equation (4.1.) of the effect of STW treatment on log employment headcount (1-2) and log firm's average annual wage rate (3-4). Firms that had their entire workforce under STW in April 2020 are considered treated. The control group is all untreated firms. (1) and (3) present estimates for all firms; (2) and (4) restrict the sample to firms with 10 or more employees. The estimates include firm, time and sector fixed effects. The year 2016 is omitted, so all estimates are in relation to this year. Standard errors are clustered at the sector level. Author's elaboration.

5. Results

In this section, I present and discuss the main findings of this dissertation. As in previous sections, treatment is defined as firms that had their entire workforce under the STW scheme during April 2020. To address the selection bias previously identified, the baseline control group consists of the 25% of untreated firms that experienced the largest revenue declines in 2020 – I refer to this as the *next-in-line* control group. Causal identification is achieved when restricting the sample to firms that employed, on average, at least 10 workers during the pre-treatment period. The estimated treatment effects are positive and statistically significant for both employment and average wages. The magnitude of these effects increases when the control group is further restricted to firms experiencing even larger revenue shocks.

5.1. Main Results

The previous subsection illustrated the risks of relying on a control group specification that fails to account for systematic differences between treated and untreated firms, potentially leading to biased treatment effect estimates. Specifically, the use of all untreated firms as a counterfactual produced negatively biased estimates, arguably driven by self-selection into treatment by less competitive firms - those that, even in the absence of the policy, would likely have terminated more employment contracts.

To address this source of bias, the main event-study specification adopts a more credible counterfactual group: firms that did not receive treatment but would plausibly have been *next in line* based on the severity of their revenue decline in 2020. As discussed in Section 3.1, eligibility for policy support was contingent on three criteria: government-mandated closures, supply chain disruptions, or a significant drop in revenue. Among these, the revenue-based criterion is the most observable and suitable for constructing a control group. As previously shown, treated firms experienced substantially larger revenue losses on average than their untreated counterparts. This motivates the use of a refined control group composed of untreated firms that nevertheless experienced severe revenue shocks.

Concretely, the baseline control group consists of firms that remained untreated but fell within the first quartile of the 2020 revenue variation distribution - that is, the 25% of untreated firms with the largest decline in annual revenue relative to 2019.

Table 3 presents the covariate balance for the revised control group, comprising 35,673 treated firms and 30,208 control firms. Notably, the revenue variation between 2019 and 2020 now appears more comparable across the two groups, as both experienced substantial declines - with the control group, in fact, exhibiting a larger average decrease.

Table 3. Covariate balance - treated and next-in-line firms

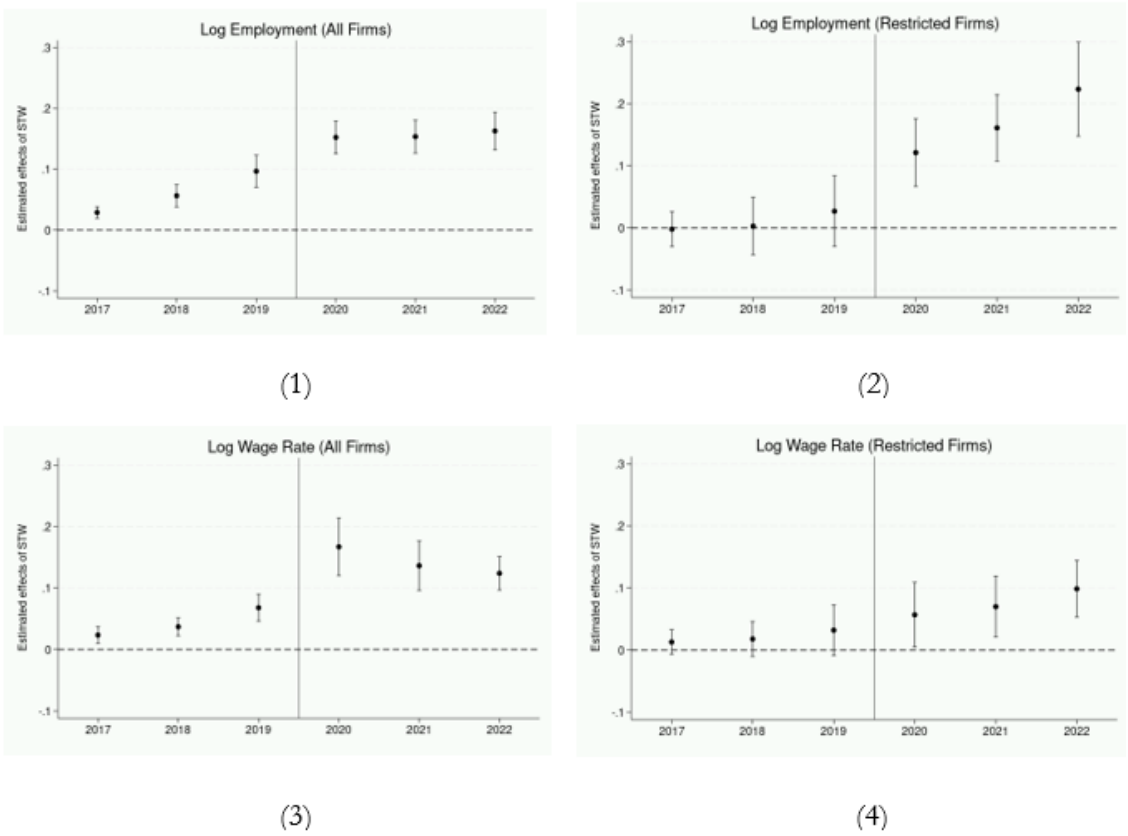
	Control			Treated		
	Mean	**Median**	**SD**	**Mean**	**Median**	**SD**
Assets	602,620	134,289	(1,727,208)	401,565	140,471	(1,114,962)
Liabilities	448,480	76,942	(5,417,494)	263,706	84,800	(1,275,781)
Equity	395,286	46,554	(8,644,928)	169,280	46,211	(1,156,669)
Cash	74,996	15,766	(191,979)	56,125	16,815	(131,620)
Liquidity	0.23	0.13	(0.25)	0.22	0.14	(0.23)
Leverage	1.81	0.61	(22.43)	1.45	0.65	(30.05)
Gross Value Added	149,874	47,151	(354,026)	129,315	64,716	(243,041)
Revenue	441,300	121,909	(1,048,562)	401,278	173,775	(850,209)
Personnel Expenditures	96,212	29,493	(303,234)	91,861	49,083	(182,758)
Productivity	31,222	18,531	(74,130)	20,488	16,067	(29,652)
Δ% Revenue (2020)	-41.59	-36.11	(18.92)	-26.15	-26.29	(29.41)
Employment	5.01	2.00	(8.81)	6.04	4.00	(7.52)
Wage Rate	8,850	7,484	(8,389)	8,028	7,223	(8,570)
N		30,208			35,673	

Notes: The table presents the covariate balance for treated firms and the baseline 25th percentile next-in-line control group as described in the text. All values are for 2019 and are in real terms based on 2012 prices. The Δ% Revenue (2020) is calculated as the percentage change in annual revenue between 2019 and 2020. Productivity is calculated as gross value added per employee. Liquidity is cash divided by assets and leverage is liabilities divided by assets. Author's elaboration.

This dissertation's main results are found in Figure 4 which plots the dynamic treatment effects from the event-study specification from equation (4.1). Plots (1) and (3) correspond to the full sample and reveal a violation of the parallel trends' assumption, undermining their credibility as causal estimates. In contrast, in the restricted sample of firms with at least 10 employees - (2) and (4) -, the estimated pre-treatment coefficients are statistically indistinguishable from zero, indicating that there is no evidence of pre-treatment differences, at the 5% significance level. This reinforces that the restricted sample provides a more credible specification and validates the identification strategy. The estimates for wages, however, even though none of the 2017–2019 coefficients are statistically different from

zero at the 5% level (supporting parallel trends), their positive signs and gradually increasing magnitudes suggest that average wages in treated firms were already on a modest upward trajectory before the treatment in April 2020. Notably - and in contrast to the earlier specification which used the full universe of untreated firms as the control group (see Figure 3), the results are now consistently positive and statistically significant across both employment and wages.

Figure 4: Event-study: effect of STW on employment and wages (next-in-line control)



Notes: Event-study estimates based on equation (4.1.) of the effect of STW treatment on log employment headcount (1-2) and log firm's average annual wage rate (3-4). Firms that had their entire workforce under STW in April 2020 are considered treated. The control group consists of the 25% untreated firms that sustained the largest revenue declines. (1) and (3) present estimates for all firms; (2) and (4) restrict the sample to firms that had 10 or more employees on average in the pre-treatment period. The estimates include firm, time and sector fixed effects. The year 2016 is omitted, so all estimates are in relation to this year. Standard errors are clustered at the sector level. Author's elaboration.

The main regression results are also summarized in Table 4 which presents the estimates of the impact of the STW policy on firms' employment (Columns 1–2) and average wage

(Columns 3–4). Columns 1 and 3 report results for all firms in the sample and show a clear violation of the parallel trends assumption at the 5% level. Columns 2 and 4 restrict the analysis to firms based on the 10-employee threshold and recover parallel trends.

Focusing on the preferred specification in Columns 2 and 4, the causal effects are both statistically meaningful and quantitatively large. Access to the policy led to a substantial and persistent increase in firm-level employment. In 2020, treated firms employed 12.91% more workers than comparable untreated firms. [3] This indicates that the average treated firm had 2.32 more employees in 2020 than they would have had in the absence of STW.[4] In fact, the effect was large enough to reverse the overall trend in the sample: while control firms reduced employment in 2020, the average treated firm expanded its workforce. The positive treatment effects on employment persisted beyond the duration of the STW benefits, rising to 25.04% by 2022. This pattern suggests that the policy not only prevented temporary layoffs but also contributed to the permanent preservation of jobs.

A similar pattern is observed for wages. In 2020, treated firms on average, paid annual wages 5.86% higher than the counterfactual. As with employment estimates, the positive impact on wages is persistent after STW implementation ends, with the effect increasing to 10.38% by 2022. In the context of this event-study specification, the results suggest that firms enrolled in *layoff simplificado* increased their average annual wages in 2020 by 556€ relative to a counterfactual scenario without the STW program. By 2022, this wage differential had risen to 984€.

[3] Since the event-specification is a log-level model, β is a semi-elasticity, therefore the percentage change in the outcome is calculated as $100(\exp(\beta) - 1)$.

[4] Given that the baseline is 2016 and I am restricting the sample to firms that had 10 or more employees, on average, in the pre-treatment period, I consider the mean annual wage rate per employee (9479€) and mean employee headcount (18 employees) for firms with these characteristics.

Table 4. Effect of STW on employment and wages (next-in-line control)

	Employment		Annual Wage Rate	
	All Firms	**Restricted Firms**	**All Firms**	**Restricted Firms**
	(1)	**(2)**	**(3)**	**(4)**
2017	0.029***	0.079***	0.064***	0.055***
	(0.005)	(0.011)	(0.005)	(0.007)
2018	0.040***	0.111***	0.106***	0.084***
	(0.009)	(0.017)	(0.007)	(0.008)
2019	0.025**	0.092***	0.138***	0.117***
	(0.012)	(0.019)	(0.010)	(0.012)
2020	-0.070***	-0.096***	0.045**	0.073***
	(0.015)	(0.028)	(0.019)	(0.011)
2021	-0.098***	-0.171***	0.107***	0.106***
	(0.017)	(0.031)	(0.018)	(0.012)
2022	-0.095***	-0.208***	0.106***	0.080***
	(0.015)	(0.032)	(0.010)	(0.012)
Treated x 2017	0.029***	-0.002	0.024***	0.013
	(0.005)	(0.014)	(0.007)	(0.010)
Treated x 2018	0.057***	0.003	0.037***	0.018
	(0.009)	(0.023)	(0.007)	(0.014)
Treated x 2019	0.097***	0.027	0.068***	0.032
	(0.013)	(0.029)	(0.011)	(0.020)
Treated x 2020	0.152***	0.121***	0.167***	0.057**
	(0.013)	(0.027)	(0.023)	(0.026)
Treated x 2021	0.154***	0.161***	0.136***	0.070***
	(0.014)	(0.027)	(0.020)	(0.025)
Treated x 2022	0.163***	0.224***	0.124***	0.099***
	(0.015)	(0.038)	(0.014)	(0.023)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.886	0.707	0.648	0.712
N	460,964	52,561	394,239	52,226

Notes: The table presents event-study estimates of the effect of STW treatment on log employment headcount (columns 1-2) and log firm's average annual wage rate (columns 3-4). The control group consists of the 25% untreated firms that sustained the largest revenue declines. Columns 1 and 3 present estimates for all firms; columns 2 and 4 restrict the sample to firms that had 10 or more employees on average in the pre-treatment period. The estimates include firm, time and sector fixed effects. The year 2016 is omitted, so all estimates are in relation to this year. Standard errors are clustered at the sector level. * p<0.1, ** p<0.05, *** p<0.01. Author's elaboration.

Tables A1 and A2 in the Appendix test the robustness of the "10 or more employees" criterion by re-estimating the event-study specification using alternative thresholds for both employment and wages. The results show that when the analysis is restricted to firms with fewer than 10 employees, or thresholds of more than 3 or more than 5 employees, the parallel trends assumption is violated. In contrast, estimates for firms with an average pre-treatment size above 20 or above 50 employees satisfy the parallel trends assumption. These findings reinforce the credibility of the empirical strategy as outlined when discussing the model: micro firms may have underlying heterogeneity that prevents credible causal inference.

To further strengthen the identification strategy, Table 5 summarizes the event-study coefficients for two alternative outcomes - sales per employee and productivity - whose relationship to STW should be limited if the control group is a suitable counterfactual. These variables are important for two reasons. First, sales are directly tied to the policy's eligibility criteria and are therefore a potential source of selection bias. Second, productivity could reflect unobserved firm quality that might confound estimates.

Columns 1 and 2 correspond to the full (biased) universe of untreated firms, while columns 3 and 4 use the baseline 25th percentile next-in-line control group. Reassuringly, the results support the idea that the negative selection bias that contaminated the initial policy effect estimates has been mitigated. Specifically, when using the full (biased) control group (columns 1 and 2), there is a strong and significant negative effect of treatment on both productivity and average revenue. Moreover, when considering the baseline 25th percentile next-in-line control group (columns 3 and 4), I find no statistically significant differences between treated firms and next-in-line controls in either variable, during the year of treatment or in the year following. That is, treatment does not appear to have affected either productivity or sales while the pandemic was ongoing (in 2020 and in 2021). Taken together, these results suggest that the refined control group does not differ systematically from the treated group in the observable dimensions most likely to influence both treatment assignment and outcome trajectories.

Table 5. The effect of STW on Sales and Productivity

	Full Biased Control		Next-in-line Control	
	Sales per Worker	**Productivity**	**Sales per Worker**	**Productivity**
	(1)	**(2)**	**(3)**	**(4)**
Treated x 2017	0.012	0.030	0.012	0.017
	(0.012)	(0.020)	(0.013)	(0.018)
Treated x 2018	-0.002	0.024	-0.002	0.004
	(0.016)	(0.025)	(0.017)	(0.021)
Treated x 2019	-0.008	0.021	-0.047	-0.025
	(0.021)	(0.029)	(0.035)	(0.029)
Treated x 2020	-0.373***	-0.258***	-0.085	-0.057
	(0.045)	(0.034)	(0.053)	(0.039)
Treated x 2021	-0.245***	-0.067***	-0.068	0.015
	(0.036)	(0.025)	(0.045)	(0.030)
Treated x 2022	-0.037	0.020	0.093***	0.063***
	(0.023)	(0.024)	(0.025)	(0.018)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.890	0.7628	0.835	0.655
N	177,221	176,453	52,456	52,073

Notes: The table presents event-study estimates based on equation (4.1.) of the effect of STW treatment on log sales per worker (columns 1 and 3) and log productivity (columns 2 and 4). I use two alternative control group specifications. In Columns 1 and 2, the control group includes all firms that did not receive treatment (consistent with Section 4.3). I refer to this as the "Full Biased Control". In Columns 3 and 4, I use the baseline control group which comprises the 25% of untreated firms that experienced the largest revenue declines. I term this the "Next-in-line Control". All estimates restrict the sample to firms that had 10 or more employees on average in the pre-treatment period. The estimates include firm, time and sector fixed effects. The year 2016 is omitted, so all estimates are in relation to this year. Standard errors are clustered at the sector level. * p<0.1, ** p<0.05, *** p<0.01. Author's elaboration.

Robustness of the control group design is further reinforced by varying the revenue decline threshold used to define the counterfactual. To this end, Equation (5.1.) estimates a standard two-group, two-period difference-in-differences (DiD) model, under the assumption of parallel trends. *Yi,t* denotes the outcome of interest - the natural logarithm of employment

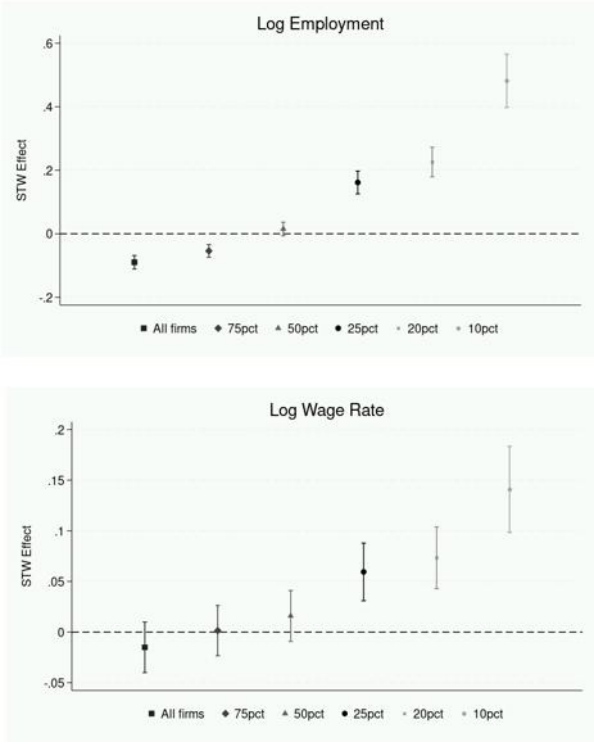
or average annual wage rate - for firm *i* in year *t*. As in the event-study specification, the regression includes unit, time and sector fixed effects. $D_{i,t}$ represents a dummy which takes the value of 1 if the firm is treated and is in the post-treatment period and 0 if not. β_{treat} is interpreted as the parameter of interest. The residual term is represented by $\varepsilon_{i,t}$. Only firms with, on average, 10 or more employees in the pre-treatment period are included.

$$Y_{i,t} = \alpha_i + \lambda_t + \beta_{treat} \cdot D_{i,t} + \theta_s + \varepsilon_{i,t} \qquad (5.1.)$$

Figure 5 presents the average treatment effect on the treated (ATET) coefficients for the entire post-period (2020-2022) based on equation (5.1.). The key conclusion from these results is that the negative selection bias is mitigated as the threshold for control group inclusion becomes more stringent - that is, as the control group is restricted to firms with increasingly severe revenue losses, the estimated policy effects shift from negative to positive. Moreover, the magnitude of the treatment effect increases. This pattern suggests that when treated firms are compared to more severely affected - but untreated - firms, the latter face greater financial strain due to the lack of policy support. As a result, the positive effects of STW on both employment and wages among treated firms become more pronounced.

These results suggest that the STW policy prevented the destruction of a socially significant number of jobs and effectively supported workers' incomes. Considering the baseline 25th revenue-drop percentile and the estimates from equation (5.1.), the Portuguese STW policy prevented the destruction of approximately 15,000 jobs, on average between 2020 and 2022, that would have otherwise been lost. This figure increases to over 50,000 jobs when considering only the 10% most severely afflicted untreated firms. Based on an average of 352,000 unemployed individuals during this period (INE 2024a), the absence of the policy would have resulted in a 4% to 15% increase in unemployed individuals.

Figure 5. Effect of STW by varying control group definitions



Notes: Impact of STW on employment and average wage rates based on equation (5.1.). Standard errors are clustered at the sector level. Author's elaboration.

Table 6. Estimates of saved jobs and wage effects under alternative control groups

Pre-treatment Mean Employees	19.35
Pre-treatment Mean Annual Wages	10,215.24
Number of Treated Firms	4,482

A - Employment			
Control Group	**Coefficient**	**Jobs Saved per Firm**	**Total Jobs Saved**
25th Percentile	0.162	3.39	15,203
20th Percentile	0.226	4.91	21,990
10th Percentile	0.482	11.98	53,687
B - Annual Wage Rate			
Control Group	**Coefficient**	**Annual Wage Impact**	**Monthly Wage Impact**
25th Percentile	0.059	625.53€	52.13€
20th Percentile	0.073	777.93€	64.83€
10th Percentile	0.141	1,546.26€	128.85€

Notes: Impact of STW on employment and average wage rates based on equation (5.1.). All estimates are relative to the baseline pre-period (2016-2019) for treated firms that had 10 or more employees. For employment (Panel A) the number of jobs saved for individual firms and for all treated firms, compared to a counterfactual scenario without STW, is estimated. Likewise, the annual and monthly wage effects are presented in Panel B. Wage values are in real terms using 2012 prices Author's elaboration.

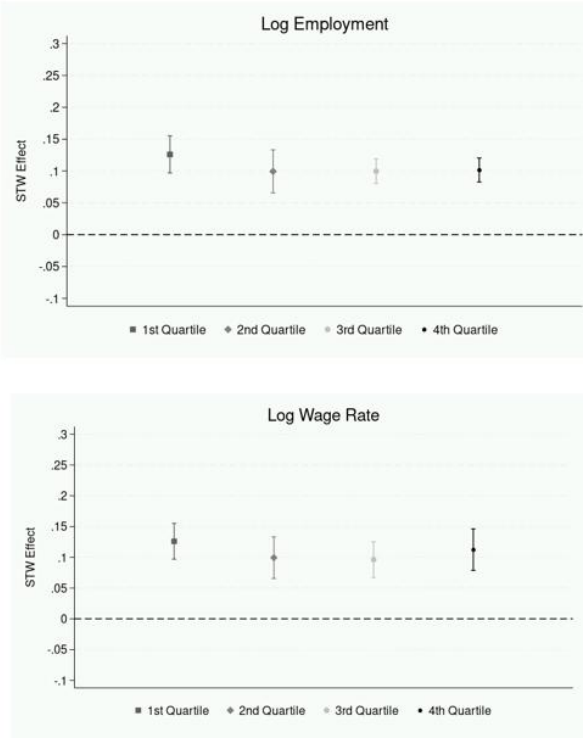
5.2. Discussing the Effectiveness of STW

Does STW affect all firms equally? To explore potential heterogeneity in policy impact, I re-estimate the specification from equation (5.1.) on subsamples partitioned by firms' pre-treatment liquidity and productivity. The motivation for this analysis is twofold.

First, firms facing tight liquidity constraints may lack the financial capacity to continue paying their workers during an economic downturn, leading to inefficient layoffs that STW schemes are designed to prevent. As such, STW may be particularly important for liquidity-constrained firms. Empirically, Giupponi and Landais (2023) find that more liquidity-constrained firms were not only more likely to participate in STW programs, but also experienced stronger positive employment effects from the policy.

To evaluate these patterns, I rank firms by their 2019 liquidity - measured as the ratio of cash and cash-equivalents to total assets - and estimate the 2x2 DiD specification separately for firms in each quartile of the liquidity distribution. Figure 6 displays the treatment effects of the STW policy on employment and average wages.

Figure 6. Effect of STW by liquidity quartiles



Notes: Impact of STW on employment and average wage rates based on equation (5.1.). Firms are ranked into quartiles based on 2019 liquidity. Standard errors are clustered at the sector level. Author's elaboration.

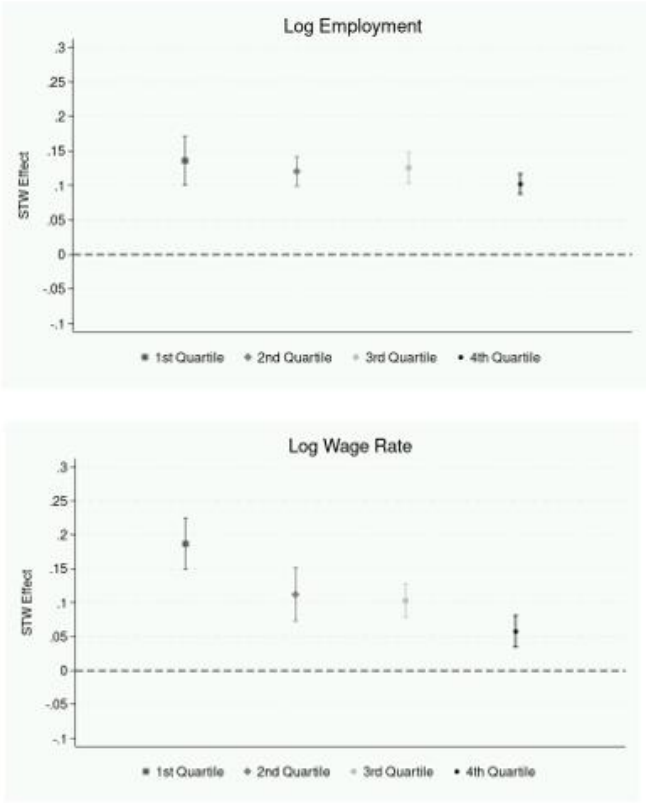
While all quartiles experienced positive wage and employment effects, the estimates are slightly larger for firms in the first quartile of the liquidity distribution, particularly in the case of employment. This finding is consistent with the idea that liquidity-constrained firms are more reliant on policy support to preserve jobs during the crisis. However, it is important to note that the difference between estimated effects across quartiles is small. Additionally, confidence intervals overlap across groups, suggesting that the observed differences may not be statistically significant. Nonetheless, the pattern offers suggestive evidence that STW may have been more effective in preserving employment among more financially vulnerable firms.

Second, STW schemes may generate displacement effects by slowing down job reallocation toward more productive matches. This risk is especially relevant when the negative shock is persistent: in such cases, STW may end up subsidizing low-productivity firms that would otherwise exit, thereby locking workers into unproductive matches and delaying necessary labour market adjustment. Empirically, Giupponi and Landais (2023) find that firms at the bottom of the productivity distribution were more likely to enter STW, but the policy had no statistically significant effect on employment among this group. To the contrary, high-productivity firms saw more significant positive increases to headcount employment. This latter conclusion is also supported by Kopp and Siegenthaler (2021)'s study.

To assess whether similar dynamics are present in my setting, I partition firms into quartiles based on their 2019 productivity levels - measured as value added per worker - and run the 2x2 DiD specification separately for each group. Figure 7 summarizes the STW treatment effects for both employment and wages.

As had happened with the liquidity quartiles, I find that STW had a positive effect for all quartiles of the productivity distribution, for both outcomes. Moreover, the point estimate of the effect on employment is larger for the least productive firms, but, as with the liquidity estimates, confidence intervals overlap indicating statistically insignificant differences. Conversely, the effect on average wages is much more pronounced and significant: the STW policy supported the wages of workers in the least productive firms significantly more than workers in the most productive firms. This result goes against what was found when reviewing the relevant literature and indicates that workers operating in less-competitive businesses were disproportionately benefited.

Figure 7. Effect of STW by productivity quartiles



Notes: Impact of STW on employment and average wage rates based on equation (5.1.). Firms are ranked into quartiles based on 2019 productivity. Standard errors are clustered at the sector level. Author's elaboration.

6. Policy Analysis

Short-time work programs help firms retain workers during downturns by subsidizing reduced hours, while unemployment insurance (UI) provides temporary income support to individuals who have lost their jobs. Although STW can generate deadweight losses by subsidizing jobs that would have survived in the absence of the policy, it is often more cost-effective than UI and can even offset its own costs by preventing layoffs and reducing UI expenditures (Cahuc et al., 2021; Kopp & Siegenthaler, 2021). For a broader comparison of STW in Europe and UI in the United States during COVID-19, see Giupponi et al. (2022).

In this section, I discuss the deadweight losses of the Portuguese STW policy and evaluate whether its cost was offset by the reduction in unemployment benefits. I use official data for information on unemployment duration (INE, 2023) and unemployment subsidies paid by Social Security (INE, 2024b), along the CB and SS data used throughout the dissertation.

I focus on the restricted sample of firms with at least 10 employees in the pre-treatment period. Using the full sample of treated firms to estimate total jobs saved would compromise the validity of the analysis: when smaller firms are included, it is not possible to credibly establish whether the policy had a causal impact on employment. However, as a result, the number of jobs saved can only be precisely estimated for this more limited subset of firms. In contrast, the total administrative cost of the policy - as reported by the Portuguese authorities - reflects support extended to a much broader universe of firms, making reported costs inadequate for this analysis.

Additionally, although this study has produced estimates on the number of jobs preserved due to *layoff simplificado*, the available data and the scope of the dissertation do not allow for characterization of the *profile* of the jobs saved – although the results from Figure 7 suggest that the strongest employment effects may have been felt in less productive firms. Therefore, it is not possible to predict the characteristics of the workers that would have been dismissed, neither in terms of how long they would stay unemployed, their tenure in the firm, nor what their wages were – and therefore, more importantly, their prospective unemployment subsidy paid by Social Security.

To address these issues, I estimate the deadweight loss and cost-effectiveness of *layoff simplificado* using a set of simplifying assumptions. First, the analysis is restricted to firms with 10 or more employees in the pre-treatment period. Second, in line with the legislative

framework, the total cost of the policy is the sum of its direct and indirect components. The direct cost corresponds to income support provided to workers, assumed to be 70% of two-thirds of the gross monthly wage, based on the pre-treatment average wage of treated workers. The indirect cost reflects the exemption from employer social security contributions, taken as 23.75% of the average wage. To estimate the fiscal savings from the policy, I consider the average annual unemployment benefit between 2020-2022 (INE,2024b)[5] and weight this by the average unemployment duration for the same period.

Table 7 summarizes the number of jobs saved by STW using the treatment effect estimates for the entire post-period from equation (5.1.). The figure includes estimates for 3 next-in-line control group definitions based on the severity of revenue-decline. The estimated deadweight effects vary substantially due to the increasing magnitude of policy effect coefficients. Considering this dissertation's baseline specification – 25th percentile of the 2020 revenue-drop distribution -, the results indicate that STW saved approximately 1 job for every 5 jobs that received the policy. These results indicate that, despite saving a non-negligible number of jobs, the policy entailed significant deadweight costs. Moreover, the estimates are compatible with those found by Boeri and Bruecker (2011) that estimate deadweight losses for STW policies during the Great Recession of 66% on average for several OECD countries, but as high as 99% for Japan, 97% for Norway or 80% for Germany.[6] The fact that the estimated deadweight losses during the COVID-19 shock are similar to those observed during the Great Recession is particularly relevant, as the deliberately broad eligibility criteria for STW schemes during the pandemic could have led to excessively large and poorly targeted benefits, resulting in substantial deadweight effects.

[5] This benchmark has the limitation of including all different types of unemployment insurance payments, and beneficiaries are counted as many times as the number of subsidies they receive.

[6] Interestingly, the authors report a deadweight loss of 94% for Portugal's STW program. However, this estimate is not directly comparable to the results obtained during the COVID-19 shock, as the program operated under different conditions. Notably, *layoff simplificado* - literally "simplified layoff" - was introduced with streamlined eligibility criteria and was applied on an unprecedented scale during the pandemic.

Table 7. Deadweight Losses

Control Group	Total Jobs Saved	Treated Jobs	Deadweight Loss
25th Percentile	15,203	86,709	82%
20th Percentile	21,990	86,709	75%
10th Percentile	53,687	86,709	38%

Notes: The table presents the deadweight losses of STW using estimates from equation (5.1.). All estimates are relative to the baseline pre-period (2016-2019) for treated firms that had 10 or more employees. Baseline firm characteristics are the same as in Table 6, as are the total number of jobs saved. The total number of treated jobs is calculated by multiplying the number of treated firms (4,482) by the pre-treatment mean number of employees (19.35) for firms with these characteristics. Author's elaboration.

Table 8 summarizes the cost-benefit analysis of the STW policy, based on one month of policy expenditure (April 2020), consistent with the treatment definition.[7] For the 25th and 20th percentile specification, the fiscal savings from avoided unemployment benefits do not fully offset the program's cost. However, under the alternative 10th percentile threshold, the policy yields a net positive outcome for taxpayers. The estimated cost-benefit ratio, which measures the fiscal savings generated per euro spent, illustrates this point: using as a counterfactual the most severely affected but untreated firms - arguably those that needed the policy most - the estimates indicate that each euro spent on the STW program saved taxpayers 1.58€ in avoided unemployment benefit payments.

The overall findings from both the deadweight loss and cost-effectiveness analyses highlight the importance of ensuring that STW policies are well-targeted. This supports the OECD (2021)'s recommendation that such interventions should be timely, targeted, and temporary, as discussed in the literature review. The more precisely these schemes reach firms genuinely at risk - that is, those on the margin of workforce reduction - the lower their deadweight losses and the greater their effectiveness in preserving jobs as well as fiscal efficiency. In practice, however, policymakers often face informational constraints that make perfect targeting infeasible, requiring them to extend support more widely to guarantee timely and effective implementation.

[7] It is important to note that the estimated benefits would likely increase if the policy were applied for a longer period, which helps address concerns about relying on only a single month to assess costs.

Given the limitations of this analysis, it should be viewed as a preliminary and exploratory assessment of the policy implications of *layoff simplificado*. Future research is needed to strengthen these findings and address unresolved questions.

Table 8. Cost-Effectiveness of STW

Fiscal Savings	
Avg. UI Expenditure per / Year	3,158.33 €
Avg. Unemployment Duration (in days)	176
Avg. UI Payout per Unemployed Individual	1,525.81 €
Fiscal Cost	
Pre-treatment Average Annual Wage	10,215.24 €
Average Monthly Wage	851.27 €
Direct Cost	397.26 €
Indirect Cost	202.18 €
Avg. STW Cost per Supported Individual (1 month)	599.44 €
Treated Jobs	86,709
Total STW Cost (1 month)	51,976,493€

Control Group	Total Jobs Saved	Total Savings	Total Cost	STW Net Cost	Cost-Benefit Ratio
25th Percentile	15,203	23,196,175€	51,976,493€	**- 28,780,317 €**	**0.45**
20th Percentile	21,990	33,551,848 €	51,976,493€	**- 18,424,645 €**	**0.65**
10th Percentile	53,687	81,915,543 €	51,976,493€	**29,939,050 €**	**1.58**

Notes: The table reports the estimated fiscal costs and savings of the STW policy. All estimates are relative to the baseline pre-period (2016-2019) for treated firms that had 10 or more employees. Baseline firm characteristics are the same as in Table 6, as are the total number of jobs saved.
Fiscal savings are estimated using data from INE (2024b) on the average annual expenditure on unemployment benefits for 2020–2022. This figure is adjusted by the average unemployment duration over the same period (INE 2023) to calculate the average unemployment insurance payout per unemployed worker. This amount is then multiplied by the estimated number of jobs saved to obtain total savings from STW.
Total costs are estimated for a one-month policy period, in line with the empirical strategy. Direct costs are calculated as 70% of two-thirds of the gross monthly wage, based on the pre-treatment average wage of treated workers. Indirect costs are estimated as 23.75% of the average wage. Total costs are then scaled by the total number of treated jobs. The cost-benefit ratio is simply total savings by total cost and indicates fiscal savings per euro spent. All values are in real terms (2012 prices). Author's calculations.

7. Conclusion

This dissertation has examined the effectiveness of Portugal's *layoff simplificado* short-time work scheme in mitigating job losses and supporting workers' wages during the COVID-19 crisis. Motivated by the unprecedented scale of STW adoption worldwide and the uncertainty surrounding its true impact, this study provides new micro-level evidence on whether such policies can deliver meaningful and lasting labour market benefits.

Using novel Portuguese Social Security data and firm-level financial information, an event-study difference-in-differences approach was employed to estimate the causal effects of the policy. Firms that had their entire workforce enrolled in STW in April 2020 were considered treated. A central contribution of this analysis is to show that credible identification hinges critically on the choice of an appropriate counterfactual. Relying on all untreated firms as a counterfactual produces highly biased estimates due to strong negative self-selection into treatment by less productive firms. By instead constructing a more plausible *next-in-line* control group - untreated firms that experienced similar revenue declines - this dissertation demonstrates that the estimated treatment effects shift from negative to positive and increase in magnitude as the control group is restricted to more severely affected firms.

The main results show that *layoff simplificado* had statistically significant and economically meaningful positive effects on both employment and wages. These effects are present in the year of policy implementation and persisted for at least two years, suggesting that the program helped avoid permanent job destruction rather than merely delaying inevitable layoffs. While no substantial heterogeneity was found across firms with different levels of liquidity, the wage effect was more pronounced for workers in the least productive firms.

I find that although the policy preserved a considerable number of jobs, it also may have generated significant deadweight losses, as some subsidized jobs might have been maintained even without support. Using the baseline control group specification, the analysis suggests that approximately 15,000 jobs may have been saved throughout the post-policy period, with an associated firm-level annual wage rate increase of 625€. This implies that roughly one in every five treated jobs was preserved because of the policy. Under a more restrictive control group definition, these estimates could rise to as many as 50,000 jobs saved and an annual wage increase of up to 1,500€. Furthermore, the fiscal savings from reduced unemployment benefit payments may have been sufficient to offset the overall cost of the program.

The overall findings from the policy analysis highlight that the more precisely these schemes reach firms and jobs genuinely at risk, the lower their deadweight losses and the greater their job-preserving effects and fiscal efficiency. However, there is a trade-off between targeting precision and implementation feasibility that policymakers must carefully consider.

7.1. Limitations and Future Research

One of this dissertation's key challenges was the mismatch between the monthly frequency of policy uptake data and annual firm-level outcomes. Combined with the monthly eligibility criteria for receiving benefits, this limited the ability to identify credible treatment and control groups. To address this, treatment status was defined as having the entire workforce covered by STW in April 2020. While this approach was necessary to ensure credible identification, it inevitably removed variation in treatment timing and duration that could have provided deeper insights.

Likewise, had more granular information been available, it may have been possible to determine, on a month-by-month basis, which firms experienced revenue declines sufficiently large to qualify for the STW scheme and yet did not enrol - thereby creating a much stronger counterfactual. Moreover, if revenue figures had been sufficiently detailed and reliable, a regression discontinuity design exploiting the 40% revenue drop eligibility threshold could have been used. Such refinement would be especially valuable to try to recover causal inference for micro-firms (those with fewer than ten employees), which comprise the majority of Portugal's business sector.

Additionally, the data structure did not allow for alternative identification strategies used in studies such as Kopp and Siegenthaler (2021), which compare firms that applied for STW but were rejected to those that were approved. Future research could also consider generating a synthetic control group using propensity score matching, as done by these authors. Moreover, future work could adopt a staggered difference-in-differences design to account for variation in the timing of firms' entry and exit from the program, thereby better capturing the dynamic nature of policy exposure.

As this dissertation focuses only on firms that continuously operated during the observation window, the findings are subject to survivorship bias. Future research could assess whether the STW policy had a positive effect on firm survival or not. It would also be valuable to

examine whether these effects vary with firms' pre-shock characteristics, such as liquidity or productivity levels, and whether they are temporary or persistent.

Regarding the cost-effectiveness analysis, an important limitation is the reliance on simplified calculations to approximate the fiscal costs and savings of the policy. The treatment definition in this study focuses on one month only, preventing the use of comprehensive administrative cost figures that capture the full duration and intensity of STW uptake. Likewise, the estimates of deadweight losses should be interpreted with caution as they rely on a restricted sample of larger firms.

Moreover, the analysis did not identify which types of workers would have been laid off in the absence of the policy or whether aggregate employment changes resulted primarily from hiring inflows or separations. Consequently, it remains unclear whether firms predominantly relied on dismissing lower-skilled workers - who represent a smaller payroll burden and can be more easily replaced as the economy recovers - or higher-skilled workers, who are more costly but harder to replace and retrain. In addition, it would be valuable to know whether workers who were able to transition to remote work experienced different employment or wage outcomes under the STW scheme. Similarly, sector-specific effects deserve further exploration, particularly considering industries that may have sustained permanent disruptions due to structural changes in the economy. Persistent shocks may cause STW to negatively affect labour reallocation, keeping workers in lower productivity positions unlikely to survive in the medium-run.

Consequently, this analysis does not consider possible longer-term effects on overall productivity. By helping firms keep jobs that might otherwise have been lost, STW may also have slowed down the natural reallocation of workers and resources to more productive firms or sectors. This trade-off between short-term job protection and long-term efficiency is an important area that remains outside the scope of this dissertation.

Overall, addressing these limitations with more detailed administrative data and more precise tracking of treatment intensity over time, enabling alternative identification strategies, would help verify the reliability of this dissertation's causal estimates. This would also make it possible to better understand how the policy's effects differ across firm size, worker types, and sectors. Further research on the causal effect of COVID-19 related STW policies in other countries is also needed to validate the robustness of this dissertation's conclusions.

Appendix: Robustness

Robustness A1. Sensitivity to the 10 or more employee's assumption

Table A1 and A2 report a robustness test of this dissertation's main results (Table 4) - which rely on restricting the sample to firms that had 10 or more employees on average in the pre-treatment period - by checking the sensitivity of the specification to different thresholds for this criterion. Treated firms are those that placed their entire workforce under STW in April 2020; the control group is the 25% of untreated firms with the largest revenue declines. Columns 1-3 do not satisfy the parallel trends assumption, while columns 4-5 recover parallel trends. These results reinforce the conclusion that micro firms (less than 10 employees) introduce substantial heterogeneity and do not allow credible inference. However, as the threshold increases, the number of observations decreases, reducing the precision of the estimates. All estimates include firm, time, and sector fixed effects and are relative to 2016. Standard errors are clustered at the sector level. *p<0.1, **p<0.05, ***p<0.01.

Table A 1. Employment Effect – Sensitivity to the 10 or more employee's assumption

	Employment (Headcount)				
	<10	**>3**	**>5**	**>20**	**>50**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Treated x 2017	0.032***	0.005	0.010	0.000	0.007
	(0.004)	(0.006)	(0.008)	(0.022)	(0.028)
Treated x 2018	0.062***	0.021*	0.025	-0.021	0.014
	(0.007)	(0.012)	(0.015)	(0.028)	(0.034)
Treated x 2019	0.104***	0.053***	0.047**	-0.004	0.019
	(0.011)	(0.018)	(0.022)	(0.030)	(0.049)
Treated x 2020	0.157***	0.156***	0.142***	0.090***	0.166**
	(0.013)	(0.025)	(0.025)	(0.032)	(0.065)
Treated x 2021	0.155***	0.186***	0.178***	0.137***	0.245***
	(0.014)	(0.026)	(0.026)	(0.034)	(0.083)
Treated x 2022	0.158***	0.218***	0.225***	0.229***	0.316***
	(0.015)	(0.028)	(0.030)	(0.045)	(0.106)
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.816	0.820	0.778	0.601	0.395
N	408,403	252,082	141,809	17,459	3,440

Table A 2. Wage Rate Effect – Sensitivity to the 10 or more employee's assumption

	Annual Wage Rate				
	<10	**>3**	**>5**	**>20**	**>50**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Treated x 2017	0.025***	0.008	0.008	0.004	0.003
	(0.007)	(0.008)	(0.009)	(0.010)	(0.015)
Treated x 2018	0.040***	0.021**	0.017	0.028	0.004
	(0.008)	(0.009)	(0.010)	(0.017)	(0.024)
Treated x 2019	0.073***	0.048***	0.034**	0.032	0.035
	(0.011)	(0.015)	(0.017)	(0.022)	(0.022)
Treated x 2020	0.184***	0.103***	0.070***	0.061**	0.081**
	(0.025)	(0.024)	(0.026)	(0.028)	(0.034)
Treated x 2021	0.147***	0.110***	0.082***	0.051**	0.023
	(0.021)	(0.021)	(0.024)	(0.024)	(0.028)
Treated x 2022	0.128***	0.115***	0.106***	0.087***	0.037
	(0.014)	(0.016)	(0.019)	(0.027)	(0.058)
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.617	0.689	0.692	0.737	0.648
N	342,013	248,036	140,774	17,358	3,415

Robustness A2. Alternative treatment threshold

In figure A1, I estimate the event-study specification analogous to Figure 3 using the alternative treatment criteria: firms with 80% or more of its workforce under STW in April 2020. The control group is all firms that remained untreated. These results are consistent with those in Figure 3 and reinforce the main conclusions. Causal inference is only achieved when restricting the sample to firms with 10 or more employees. Specifically, the estimates show a negative effect of STW on employment and a visible downward trend for wages, although these estimates are not statistically significant at the 5% level.

In Figure A2, I estimate the event-study specification analogous to this dissertation's main results from Figure 4, using the alternative treatment criteria - firms with 80% or more of its workforce under STW in April 2020 - and the baseline next-in-line 25th percentile control group. These results are consistent with those presented earlier and reinforce the main conclusions. Causal inference is only achieved when restricting the sample to firms with 10 or more employees. Specifically, the estimates now show a positive effect of STW on both

employment and wages. However, there is a mild violation of the parallel trends assumption for employment.

Figure A 1. All untreated firms' event-study – alternative treatment criteria

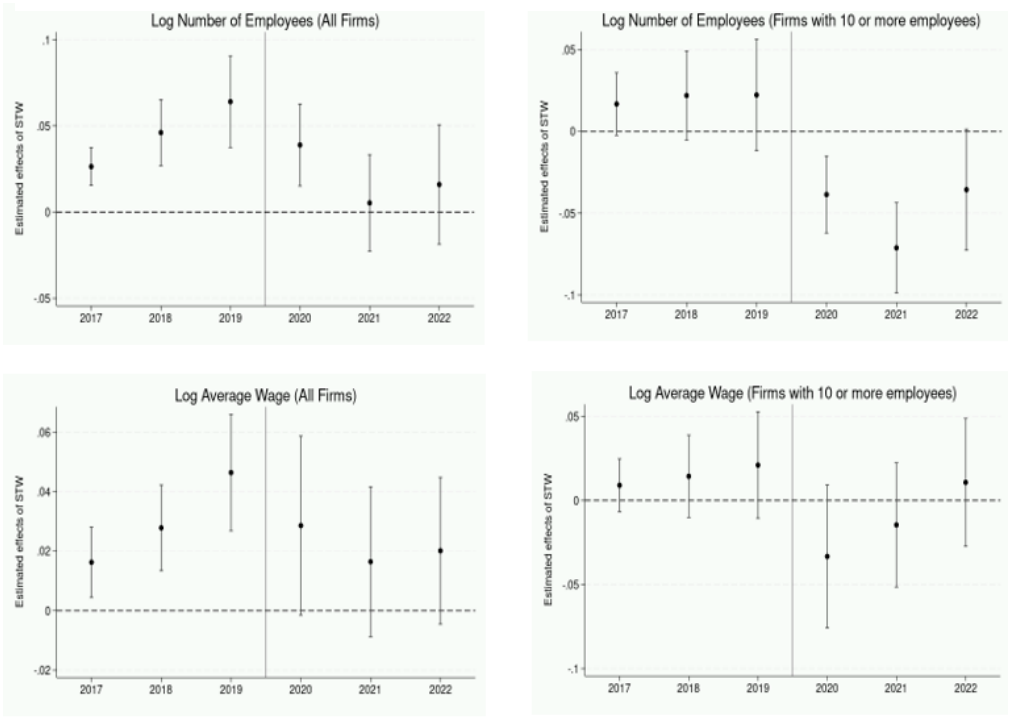
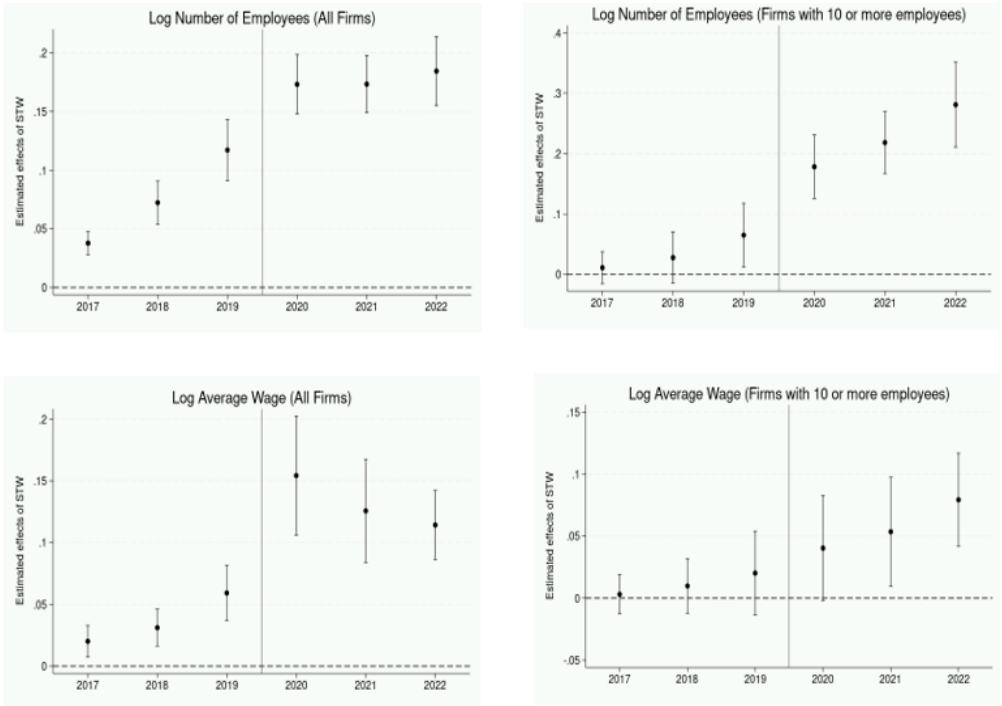


Figure A 2. Next-in-line control event-study – alternative treatment criteria



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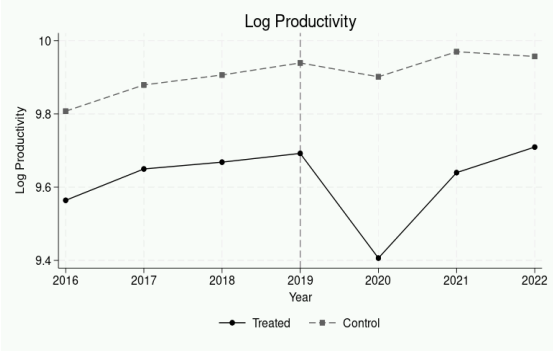
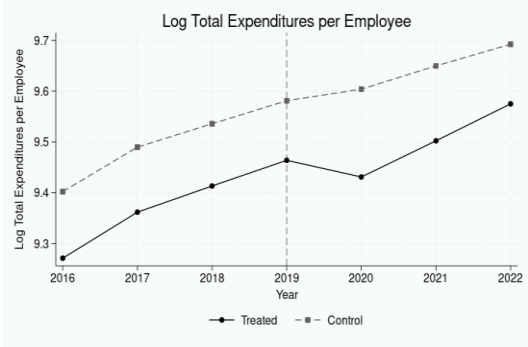
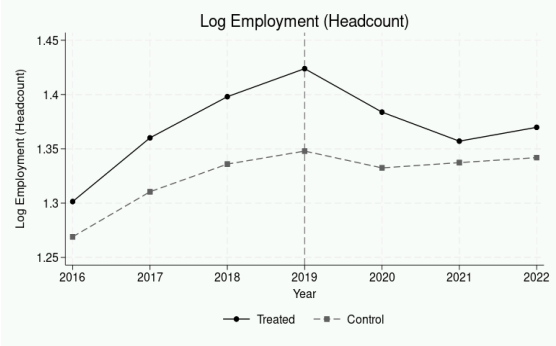
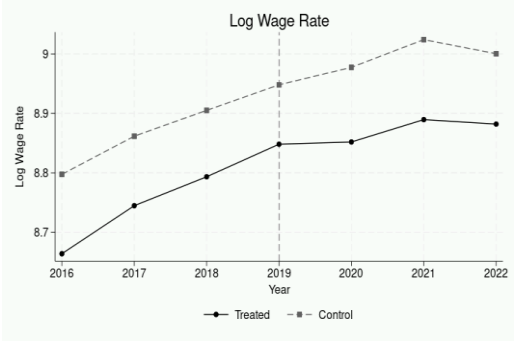
Annex

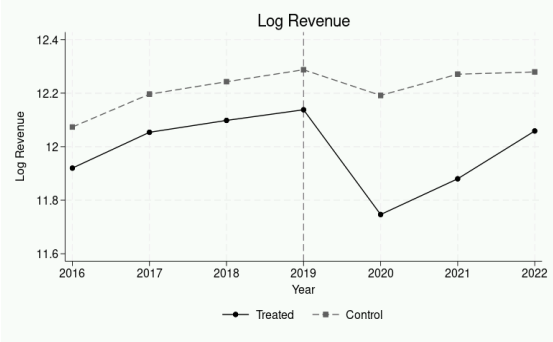
Figure B 1. Evolution of key variables for all untreated firms











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