DISCUSSION PAPER SERIES

DP13310
(v. 2)

SUBSIDIZING LABOR HOARDING IN RECESSIONS: THE EMPLOYMENT & WELFARE EFFECTS OF SHORT TIME WORK

Camille Landais and Giulia Giupponi

LABOUR ECONOMICS

PUBLIC ECONOMICS

ISSN 0265-8003

SUBSIDIZING LABOR HOARDING IN RECESSIONS: THE EMPLOYMENT & WELFARE EFFECTS OF SHORT TIME WORK

Camille Landais and Giulia Giupponi

Discussion Paper DP13310
First Published 12 November 2018
This Revision 26 May 2020

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

SUBSIDIZING LABOR HOARDING IN RECESSIONS: THE EMPLOYMENT & WELFARE EFFECTS OF SHORT TIME WORK

Abstract

Short time work (STW) policies provide subsidies for hour reductions to work- ers in firms experiencing temporary shocks. They are the main policy tool used to support labor hoarding during downturns, and have been used aggressively since the start of the COVID-19 pandemic. Yet, very little is known about their employment and welfare consequences. This paper leverages unique administrative social security data from Italy during the Great Recession and quasi-experimental variation in STW policy rules to offer evidence of the effects of STW on firms' and workers' outcomes. Our results show large and significant negative effects of STW treatment on hours, but large and positive effects on headcount employment. We then analyze whether these positive employment effects are welfare enhancing, distinguishing between temporary and more persistent shocks. We first provide evidence that liquidity constraints and bargaining frictions make labor hoarding inefficiently low absent STW. Then, we show that adverse selection of low productivity firms into STW creates significant negative reallocation effects when the shock is persistent.

JEL Classification: N/A

Keywords: N/A

Camille Landais - c.landais@lse.ac.uk
LSE and CEPR

Giulia Giupponi - giulia.giupponi@ifs.org.uk
LSE and CEPR

Acknowledgements
We thank Richard Blundell, Tito Boeri, Pierre Cahuc, Henrik Kleven, Pascal Michaillat, Magne Mogstad, Barbara Petrongolo, Thomas Piketty, Steve Pischke, Emmanuel Saez, Benjamin Schoefer, Johannes Spinnewijn and participants at the NBER Public Economics 2018 Fall Meeting, CEPR 2018 Public Symposium, the Annual Research Conference of Banco de Espana in Madrid, University of Chicago, LMU, Bocconi, Mannheim, Kings College, Manchester, LSE and the HELP! (HEaLth and Pandemics) Econ Working Group for helpful discussions and suggestions. We also thank Miguel Fajardo Steinhäuser, Jack Fisher, Tom Monk, and Mathilde Munoz for outstanding research assistance. We are greatly indebted to all the staff of Direzione Centrale Studi e Ricerche at INPS for their invaluable support with the data and the institutional setting, and to the sponsors of the VisitINPS program for financial support. We gratefully acknowledge support from ERC grant #679704 -DYNAMICSS, STICERD, ESRC, and the INPS "Valeria Solesin" Fellowship. The findings and conclusions expressed are solely those of the authors and do not represent the views of INPS.

Subsidizing Labor Hoarding in Recessions: The Employment & Welfare Effects of Short Time Work

Giulia Giupponi[*] Institute for Fiscal Studies

Camille Landais[†] London School of Economics[‡]

May 26, 2020

Abstract

Short time work (STW) policies provide subsidies for hour reductions to workers in firms experiencing temporary shocks. They are the main policy tool used to support labor hoarding during downturns, and have been used aggressively since the start of the COVID-19 pandemic. Yet, very little is known about their employment and welfare consequences. This paper leverages unique administrative social security data from Italy during the Great Recession and quasi-experimental variation in STW policy rules to offer evidence of the effects of STW on firms' and workers' outcomes. Our results show large and significant negative effects of STW treatment on hours, but large and positive effects on headcount employment. We then analyze whether these positive employment effects are welfare enhancing, distinguishing between temporary and more persistent shocks. We first provide evidence that liquidity constraints and bargaining frictions make labor hoarding inefficiently low absent STW. Then, we show that adverse selection of low productivity firms into STW creates significant negative reallocation effects when the shock is persistent.

JEL codes: H20, J20, J65.

Keywords: Short time work, employment, reallocation, social insurance.

[*]Institute for Fiscal Studies, 7 Ridgmount Street, London WC1E 7AE, giulia.giupponi@ifs.org.uk

[†]Department of Economics, Houghton Street, London WC2A 2AE +44(0)20-7955-7864, c.landais@lse.ac.uk

[‡]We thank Richard Blundell, Tito Boeri, Pierre Cahuc, Henrik Kleven, Pascal Michaillat, Magne Mogstad, Barbara Petrongolo, Thomas Piketty, Steve Pischke, Emmanuel Saez, Benjamin Schoefer, Johannes Spinnewijn and participants at the NBER Public Economics 2018 Fall Meeting, CEPR 2018 Public Symposium, the Annual Research Conference of Banco de Espana in Madrid, University of Chicago, LMU, Bocconi, Mannheim, Kings College, Manchester, LSE and the HELP! (HEaLth and Pandemics) Econ Working Group for helpful discussions and suggestions. We also thank Miguel Fajardo Steinhäuser, Jack Fisher, Tom Monk, and Mathilde Munoz for outstanding research assistance. We are greatly indebted to all the staff of Direzione Centrale Studi e Ricerche at INPS for their invaluable support with the data and the institutional setting, and to the sponsors of the VisitINPS program for financial support. We gratefully acknowledge support from ERC grant #679704 -DYNAMICSS, STICERD, ESRC, and the INPS "Valeria Solesin" Fellowship. The findings and conclusions expressed are solely those of the authors and do not represent the views of INPS.

1 Introduction

The economic shock created by the COVID-19 pandemic has generated a sudden revival of interest in policies destined at encouraging labor hoarding during downturns. Short time work programs (STW), which are subsidies for temporary reductions in the number of hours worked, are the most emblematic of such policies, and are being aggressively used during the COVID-19 crisis, especially in European countries. Figure 1 reveals how swift and massive the take-up of STW schemes has been in the pandemic. While the fraction of employees on STW never exceeded 5% during the Great Recession, it has skyrocketed to unprecedented levels in Spring 2020. More than 20% of German workers were enrolled in a STW scheme in April 2020. The same fraction is larger than 30% in Italy and France. Interestingly, despite the existence of similar schemes in a majority of US states, the policy response has been very different in the US. There, as evidenced by Figure 2, subsidized labor hoarding is almost non-existent and most of the shock is cushioned by unemployment insurance.[1]

But what do we know about the effects of STW schemes? Are they effective in stabilizing employment and in helping firms hold onto their productive workers? Is it a more effective way to provide insurance to workers than unemployment insurance (UI)? And do we know anything about the welfare implications of STW schemes? While almost a third of the labor force is currently in STW programs in Europe, we do not have answers to these fundamental questions: we know close to nothing about the effects of STW and about its welfare consequences. This is all the more surprising given the large literature devoted to the use of other insurance programs over the business cycle, such as UI (e.g. Schmieder, von Wachter and Bender [2012], Marinescu [2017], Landais, Michaillat and Saez [2018*a*], Landais, Michaillat and Saez [2018*b*]) or "partial unemployment benefits" (Le Barbanchon [2020]).

There are however three simple reasons that explain the very limited knowledge that we have of the effects and desirability of STW. The first reason is a critical lack of firm- or individual-level administrative data on STW.[2] The literature on STW had to mainly resort to cross-country analysis (e.g. Van Audenrode [1994], Boeri and Bruecker [2011], Cahuc and Carcillo [2011]). Even in the presence of firm-level data, the second issue lies in the lack of credible sources of identification of STW treatment. In almost all

[1] State STW programs have been actively promoted by the Job Creation Act of 2012. In 2020, 27 U.S. states have STW programs established in law and 26 have operational programs (U.S. Department of Labor, Employment and Training Administration [2020]).

[2] As a matter of example, the German social security administration (*IAB*) did not collect data on STW in the Great Recession. Most STW applications and reports were sent in paper format to the Federal Employment Agency and were not digitized. Only a sample of these reports has been digitized for the Nuremberg metropolitan area for the years 2008 to 2010 and matched to *IAB* data (Tilly and Niedermayer [2016]).

countries with STW programs in place, there is no variation in firms' eligibility to STW. The issue will be even more acute for the current recession, as most countries have purposefully extended STW access to every single firm. This severely complicates identification, with no obvious method to control for the selection of firms into STW take-up. Most papers therefore rely on the structure of calibrated models to analyze the effects of STW on workers and firms (e.g. Tilly and Niedermayer [2016]). Alternatively, a few studies have tried to find instruments for the take-up of STW. Boeri and Bruecker [2011], Cahuc and Carcillo [2011] and Hijzen and Martin [2013] instrument STW take-up during the Great Recession with firms' prior experience with the program and find competing results. More recently, Cahuc, Kramarz and Nevoux [2018] offer a credible IV strategy in the French context. They instrument STW take-up using the proximity of a firm to other firms that used STW before the recession. As an alternative instrument, they use response-time variation in the administrative treatment of STW applications across French departments. They find, similar to our results, large and significant employment effects of STW treatment. Another recent study also finds significant positive employment effects of STW in Switzerland during the Great Recession, comparing firms in the program to firms whose STW application was rejected (Siegenthaler and Kopp [2019]).

The third issue behind our limited knowledge of STW is the lack of a framework to evaluate the inefficiencies that STW wishes to correct. STW may preserve employment, but how can we assess whether keeping such matches is welfare improving? While a small theoretical literature shows that STW may distort both hours and the allocation of workers across firms, thus reducing output (Burdett and Wright [1989]), there is no clear view of the conditions under which STW programs might be socially desirable and improve welfare.

This paper contributes to our understanding of STW by addressing these limitations. It relies on uniquely rich administrative data on STW from Italy during the Great Recession. It uses the presence of variation in eligibility rules across firms to provide compelling evidence of the causal impact of STW on firms' and workers' outcomes. And it explores empirically the forces underlying the welfare trade-offs implied by STW programs. Beyond the canonical moral hazard and insurance effects at the heart of optimal unemployment insurance trade-off, we show that STW must balance two additional, and empirically relevant forces: layoff inefficiencies, and reallocation inefficiencies.

Our data comes from the Italian social security administration (INPS) and covers the universe of Italian employer-employee matches in the private sector, and the universe of all social security and transfer payments in Italy, from 1983 to 2015. Besides granular information on firms' and workers' histories, it provides detailed information on eligibility, applications and authorizations of the universe of STW episodes at both the firm and individual level from 2005 to 2015. This data, combined with the specificities of the Italian STW program, which creates variation in eligibility across firms, allows us to provide causal evidence of the effects of STW. Identification stems from the interaction between two sources of variation in eligibility: INPS codes and firm size. First, we exploit the fact that within 5-digit industries, certain firms – as defined by particular INPS codes – are eligible while others are not. This occurs because of the particular interpretation of the law regulating STW that was given by INPS, in a circular for the implementation of STW rules dating back to the 1970s. While this variation in STW access across otherwise very similar firms appears exogenous to economic conditions at such fine level today, we use the additional requirement that firms must be above a certain full-time-equivalent size threshold to be eligible for the program. This enables us to test and control for the possibility that differential time shocks affected eligible and non-eligible INPS codes within 5-digit industries during the recession. We further provide multiple robustness checks for the validity of our approach. In particular, we show that our approach is not confounded by manipulation of size or INPS codes, nor by any other change in regulations at the main eligibility size threshold.

Our results demonstrate that STW has large and significant effects on firms' employment at both the intensive and extensive margin. Compared to counterfactual firms, firms treated by STW experience a 40% reduction in hours worked per employee, and an increase of similar magnitude in the number of employees in the firm, with no discernible effect on wage rates. We further find that the employment effects are driven by a small positive effect on inflows and a large negative effect on outflows, and that most of the effects are concentrated on open-ended contracts (as opposed to fixed-term contracts). STW is finally shown to have a positive effect on firms' survival probability.

After having established in the first part of our empirical analysis that STW has a positive effect on employment, we ask in a second part whether this is actually socially efficient. To assess the welfare effects of STW, it is key to separate shocks according to their persistence. We first focus on the welfare trade-off when the shock is temporary. We show that two sources of frictions – liquidity constraints and bargaining frictions – may make the level of labor hoarding by firms inefficiently low in response to the shock. We provide evidence of the presence of such frictions and show that the take-up and employment effects of STW are larger when these frictions are more prevalent. Using data on firms' balance sheet from CERVED, matched to our administrative data, we find that the take-up of STW is strongly increasing in measures of financial constraints of firms, and that the positive effects of STW on firms' survival are concentrated at the bottom of the distribution of firms' pre-crisis liquidity. Exploiting a strong discontinuity in the outside option of workers created by the UI system, we then provide evidence of bargaining frictions suggesting that firms do not internalize the surplus of individual workers when making labor hoarding decisions.

While this set of results offers a strong case for the desirability of STW in the presence of a temporary shock, we then show that the welfare trade-off will be different in the presence of persistent shocks. If shocks are persistent, as was the case in our context due to the Italian double-dip recession following the financial crisis, STW may create reallocation issues, the extent of which will depend on the selection of firms into the program. Using various measures of firms' pre-crisis productivity, we find that firms in the bottom quartile of pre-crisis productivity where almost four times more likely to take up STW during the crisis than firms in the top quartile. Looking at dynamic effects, we find that the long run effects of STW were null for the low productivity firms. We find that the employment and earnings of workers from low productivity firms treated by STW were the same as those of laid-off workers in similarly low productivity firms three years after treatment. To the contrary, workers in high productivity firms pre-crisis, had long run outcomes after STW treatment that were significantly better than those of laid-off workers in similarly high productivity firms.

Because STW subsidized low productivity matches that were unable to survive a persistent shock, STW may have inefficiently kept workers in low productivity firms, keeping alive inefficient matches that had negative surplus and generating negative reallocation effects in the labor market. To investigate this, we leverage the rich spatial variation available in Italy across more than 600 local labor markets (LLM) and estimate how an increase in the fraction of workers treated by STW in a LLM affects employment outcomes of non-treated firms. We instrument variation in the intensity of STW treatment across LLMs by the average yearly fraction of eligible workers in the LLM based on the interaction between firm size and INPS codes in the pre-recession period, controlling for a rich set of firm and LLM characteristics. We provide various placebo tests confirming the validity of our IV strategy. Our results provide compelling evidence of the presence of equilibrium effects of STW within labor markets. We show that STW significantly decreases the employment growth and inflow rates of non-treated firms, and has a significant negative impact on TFP growth in the labor market. While informative, these reduced-form estimates do not offer by themselves a sense of the magnitude of the reallocation effects that would arise if we were to shut down STW programs. For this purpose, we use a matching model calibrated to our reduced-form empirical evidence to run counterfactual analysis and quantify the reallocation effects of STW. This analysis suggests that – in the absence of any STW subsidy – the level of unemployment would have been almost 2 percentage point higher during the recession in Italy, and aggregate TFP about 2% higher.

We conclude by drawing lessons from our context to understand the likely welfare effects of the massive use of STW schemes during the COVID-19 crisis, depending on the temporary or persistent nature of the pandemic shock.

The remainder of the paper is organized as follows. Section 2 describes the Italian STW institutions and the data. Section 3 presents the identification strategy and our estimates of the effects of STW on employment outcomes and firms' survival. We explore in Section 4 the presence of frictions preventing efficient labor hoarding in the context of temporary shocks. Section 5 investigates reallocation issues created by STW in the presence of persistent shocks. Section 6 concludes.

2 Institutional Background & Data

2.1 The Italian *Cassa Integrazione Guadagni* (CIG)

The Italian Cassa Integrazione Guadagni (CIG) was created in 1941. It represents, with the German *Kurzarbeit*, one of the oldest, largest and most comprehensive STW programs in the world. It was heavily used during the latest recession: in 2013, almost 5% of the Italian workforce was on STW, for a cost of roughly .5% of Italian GDP. This massive expansion of STW take-up makes Italy the perfect laboratory to analyze the employment and welfare consequences of STW during the Great Recession.

CIG is composed of three programs: Cassa Integrazione Guadagni Ordinaria (CIGO), Cassa Integrazione Guadagni Straordinaria (CIGS) and Cassa Integrazione Guadagni in Deroga (CIGD). We focus throughout the paper on the second program, CIGS, which is the main pillar of STW used in recessions.[3]

CIGS rules are quite standard among STW programs, and make it a good example of most of the programs implemented across OECD countries. CIGS targets firms experiencing economic shocks, broadly defined: it can be a demand or revenue shock, a company crisis, a need for restructuring or reorganization, a liquidity or insolvency issue, etc. CIGS is a subsidy for partial or full-time hour reductions, replacing approximately 80% of the earnings forgone by the worker due to hours not worked, up to a cap.[4] The subsidy is available to workers in the private sector and is administered

[3]CIGO is restricted to small transitory shocks or accidents involving forced reduction of activity (e.g. adverse weather conditions, earthquakes, power cuts). It is restricted to the manufacturing and construction sectors, and has a maximum duration of 13 weeks. CIGD is a smaller additional program created in 2009, administered at the local level and granted ad-hoc on the basis of regional decrees.

[4]Hours not worked are computed against the regular hours stipulated in the labor contract. The normal weekly working hours are 40 in Italy. The benefit schedule applies homogeneously across worker types, with an 80% replacement rate up to a cap. The cap is established by law each year. In 2009, for example, the monthly cap was Euro 1.065,26. If a firm is eligible, all workers with at least 90 days of tenure are eligible to be put on CIGS, except for apprentices and managers in the firm. Firms are free to

by the Italian Social Security (INPS). The subsidy is remitted directly to the workers. Firms intending to use the program must file an application to the Social Security or the Ministry of Labor, providing a justification of economic need and a recovery plan.[5] Once authorized, the usage of CIG is subject to weak conditionality requirements for both firms and workers: there are no provisions for compulsory training nor prohibitions of dismissal by firms, and no job-search requirements for employees. The cost to firms of putting workers on CIGS is minimal: they pay a fee to INPS equal to 3 to 4.5% of the total amount of the subsidy to workers.[6] CIGS is otherwise financed via ordinary payroll contributions, paid by eligible firms and their workers. The program has a maximum duration of 12 months, with limited possibilities of extension. Utilization of the program need not be on a continuous basis, but cannot exceed a maximum duration of 36 months – including extensions – over 5-year periods that are fixed and defined by the law. In practice, almost all firms use CIGS for exactly 12 months – the median and average durations of CIGS take-up being approximately equal to 52 weeks.

One of the specificities of CIGS is the presence of various provisions of the law that create quasi-exogenous variation in eligibility across firms, offering the unique possibility of identifying the causal effect of STW programs on firm and individual outcomes. This is remarkable as most STW programs like the German *Kurzarbeit* or the French STW, provide little to no variation in eligibility across firms, making it complicated to identify the causal effect of STW in these contexts (Cahuc, Kramarz and Nevoux [2018]). We exploit the fact that a firm's eligibility for CIGS depends in particular on two dimensions: an INPS specific code called "contributory regime" and the size of the firm prior to filing an application.

Contributory regimes (or INPS codes) are created by combining 5-digit industry codes and 333 different "codice autorizzazione".[7] Eligibility of each INPS code to CIGS is assigned on the basis of an INPS circular that regulates the implementation of the STW law. STW legislation by the Ministry of Labor, and the rules that determine its application as made operational by INPS, date back to the 1970s. As a consequence, within fine-grained 5-digit industry codes (594 industries), there is variation in CIGS

decide the amount of hour reductions they request, i.e. there is no minimum or maximum amount of reduced hours in the CIGS program.

[5] Using data on CIGS applications and authorizations, we found that in practice, applications are never rejected: 99.99% of applications are authorized by the Ministry of Labor.

[6] The fee is 3% for firms with up to 50 employees and 4.5% for larger firms. In 2015, a reform introduced an experience rating component to the costs of CIGS to the employer by making the fee an increasing function of the amount of subsidized hours.

[7] The "codice autorizzazione" is an administrative code used by INPS that, in combination with the 5-digit industry code, defines the various programs and contributions a firm is eligible to or subject to. The combination of 5-digit industry codes and "codice autorizzazione" creates an INPS code that allows to univocally identify the contributory regime and CIGS eligibility of any given firm.

eligibility across otherwise very similar firms, due to regulations that are quite plausibly exogenous to economic conditions at such fine level today. To provide just a few concrete examples: within the 5-digit industry codes 11306, 11307 and 11308, which are firms in construction specialized in the installation of electrical machinery, only those with codice autorizzazione 3N are eligible; within the 5-digit code 10106, which are firms that produce seeds and beans, only firms with codice autorizzazione 3A are eligible.[8]

Besides INPS codes, a firm's eligibility to CIGS depends on its size being above a certain threshold. This variation in eligibility across firms of different sizes allows to use non-eligible firms within INPS codes to test and control for differential time shocks across eligible vs non-eligible INPS codes. The main size requirement is that a firm must have employed on average more than 15 employees in full-time equivalent (FTE) units in the six months prior to the application.[9] For some industries in the retail sector, the size requirement differs, and is set to 50 FTE. Note that employment protection legislation regulating dismissals also apply in Italy when a firm reaches 15 employees within a single establishment or municipality, or 60 employees in the firm in Italy as a whole.[10]

We explain in Section 3.1 how these sources of variation in eligibility across INPS codes and firm size can be combined to identify the effects of CIGS on firms and workers.

2.2 Data

We use administrative data from INPS on the universe of employer-employee matches and social security payments in the private sector in Italy from 1983 to 2015. The data includes detailed information on workers' demographics, working histories, participation in all social assistance and social insurance programs. It also provides detailed information on firms' characteristics such as employment, labor-force composition and industry. Most importantly, starting from 2005, the data provides information on eligibility, applications, authorizations, duration and payments of the Italian STW program at the individual and firm level. We linked the administrative archives to firm-level balance-sheet data from CERVED via a unique identifier. CERVED is a

[8]Codice autorizzazione 3N is one of the contributory codes that indicate a firm is liable to pay the ordinary CIG contribution and thus is eligible for CIG treatment. Code 3A, instead, is assigned to cooperatives and consortia; joint with specific 5-digit industry codes as specified in the INPS circular, it identifies firm that are liable to pay CIG contributions and are eligible for STW.

[9]To be precise, eligibility to CIGS, and therefore eligibility requirements, all apply at the establishment level. INPS codes are also establishment specific. When we refer to firms throughout the paper, we mean "establishments". We restrict our baseline sample to single-establishment firms.

[10]In Section 3.3, we explain and provide multiple pieces of evidence that our approach is robust to variation in dismissal costs at the 15-FTE threshold. To show this, we look at multi-establishment firms that are always subject to the dismissal cost regulation.

firm register containing balance-sheet information of all limited liability companies in Italy. The balance-sheet information covers roughly 50% of firms in the administrative records and enables to create various measures of productivity and credit constraints.

We define STW events at the firm level as any month in which a STW episode is reported in the INPS records, which is also authorized according to the authorization data. When aggregating at the annual level, an event is defined as having at least one STW episode during the year. Eligibility status is defined dynamically using INPS codes and based on the maximum 6-month average FTE firm size in each year.[11]

To define intensive measures of employment, we leverage detailed weekly level information on whether a worker was working full-time or part-time. When working part-time, we have information on the percentage of part-time work. We use this information to create a measure of hours worked for each worker. We assign 40 hours per week to full-time workers, and weight hours for part-time work using the percentage of part-time work, assuming a corresponding full-time contract of 40 hours.

Our main sample of analysis is a balanced panel of all ever-active private sector firms that ever reach an average 6-month full-time equivalent firm size between 5 and 25 in the period 2005 to 2014. Our sample of workers is a balanced panel of all workers ever working in these firms.[12] Appendix Table A-1 provides descriptive statistics on our main sample of firms in 2008, prior to the start of the Great Recession. The average firm size in our sample is close to 9 employees, with an average of 38.7 weekly hours worked per employee. The average wage bill per employee is Euro 20.6k. The table also breaks down firms between eligible and non-eligible INPS codes. Despite being unequally distributed across industries, firms in eligible and non-eligible INPS codes are quite similar in terms of observable characteristics prior to the Great Recession. Firms in eligible INPS codes are slightly larger, but are quite comparable in terms of hours worked per employee, wage bill per employee, revenues, investment and liquidity. Appendix Table A-2 provides similar information for workers in our main sample of analysis. Workers in eligible INPS codes are more likely to be male and blue collars, and they are also slightly older than workers in non-eligible INPS codes, which

[11] The FTE size measure relevant for establishing CIGS eligibility is computed considering all employees, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units. Eligible firms must have employed on average more than 15 employees in FTE in the 6 months prior to their application. Firms that have less than six months of activity should consider the average number of employees (in FTE) in the month or months of activity. In order to determine whether a firm meets the size requirement, we use the exact FTE firm size measure that determines CIGS eligibility as provided by INPS (the variable is called "forza aziendale").

[12] We restrict the main analysis to the period up to 2014, as an important reform of Italian labor market regulations started being implemented in 2015, which may have interfered with the effects of STW programs.

reflect the fact that manufacturing is more represented in eligible INPS codes than in non-eligible INPS codes.

Appendix Figure A-1 reports additional information on the distribution of treatment across workers in firms experiencing STW. Panel A plots the distribution of the ratio of treated workers to eligible workers in firms currently under STW treatment, and shows that most firms choose to put all their eligible workers in the program and therefore spread hour reductions across all eligible workers. Panel B reports the distribution of reported weekly hour reductions for workers currently experiencing STW. The graph shows a smooth distribution of hour reductions, with a mode around 25%, and an average weekly hour reduction of a little more than 35%.[13]

3 Effects of STW on Employment & Firm Outcomes

3.1 Identification

The eligibility requirements of the Italian CIGS create sharp variation in a firm's probability to use STW based on INPS codes and firm size.

Appendix Figure A-2 provides direct evidence of this variation in access to CIGS by INPS codes and firm size. Panel A plots, among firms with eligible INPS codes in our sample, the evolution of the fraction of firms receiving CIGS in each calendar year t from 2005 to 2014, for firms with a maximum 6-month average size of 15 to 25 full-time-equivalent employees in year $t-1$ and for firms with a maximum 6-month average size of 5 to 15 full-time-equivalent employees in year $t-1$. For firms with more than 15 FTE employees, CIGS take-up rose sharply from less than 1% before the onset of the recession, to roughly 8% throughout the recession. While for firms with less than 15 employees, take-up was essentially zero throughout the period. Panel B of Appendix Figure A-2 replicates the same exercise for firms in non-eligible INPS codes. For both firms below and above the 15 FTE threshold, the take-up is null throughout the entire period.

Our main identification strategy relies on using the interaction of being in an eligible INPS code and having size above the 15 FTE threshold as a source of quasi-experimental variation in CIGS treatment after the onset of the recession in 2008. For each outcome Y, the baseline specification underlying our reduced-form graphical evidence is:

[13] Appendix Figure A-1 therefore provides evidence that STW does not work like temporary layoffs, but effectively like hour reductions spread across all workers in the firm.

$$
\begin{aligned}
Y_{igst} = & \sum_j \gamma_1^j \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \right\} \\
& + \sum_j \sum_k \gamma_2^{jk} \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \gamma_3^{jk} \cdot \left\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \gamma_4^{jk} \cdot \left\{ \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] + v_{igst}
\end{aligned}
\tag{1}
$$

where Y_{igst} denotes outcome Y for firm i, belonging to INPS code group g, in 5-digit industry s in year t. A firm can either be in the group of INPS codes eligible to receive CIGS ($g \in \mathcal{E}$) or in the group of non-eligible firms ($g \in \mathcal{E}^{\complement}$). $N_{i,t-1}$ is firm i's full time equivalent size in calendar year $t-1$. Note that by systematically controlling for 5-digit industry fixed effects and their interactions with time and firm size, we only exploit variation in eligibility of INPS codes across firms within the same fine-level industry code. This variation stems from the interaction between industry codes and "codice autorizzazione".[14] To restrict our attention to comparable firms in a narrow neighborhood around the 15 FTE cut-off, we estimate the above model on firms who reach a size between 5 and 25 FTE in $t-1$. Our graphical evidence consists in plotting the estimated coefficients $\widehat{\gamma}_1^t$ for all years t. These coefficients capture the evolution over time of the relative outcomes of firms that are just above and just below the 15 FTE employee threshold in eligible INPS codes, compared to firms that are just above and below the same 15 FTE employee threshold in non-eligible INPS codes, but within the same 5-digit industry. The omitted year in specification (1) is 2007, so results are expressed relative to levels in year 2007.

Estimates of the effect of STW treatment are obtained from running IV models where we instrument the probability of STW treatment T by the triple interaction of being after the onset of the recession, being in an eligible INPS code and having more than 15 FTE employees. Specification (2) illustrates the IV model, with specification (3) being the corresponding first stage:

[14] This approach therefore fully controls for the fact that eligible firms are not evenly distributed across 5-digit industries nor across "codice autorizzazione".

$$
\begin{aligned}
Y_{igst} = \quad & \beta_{IV} \cdot T_{igst} && (2)\\
& + \sum_j \sum_k \eta_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \eta_3^{jk} \cdot \Big\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \eta_4^{jk} \cdot \Big\{ \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] + \mu_{igst} \\
T_{igst} = \quad & \kappa_1 \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t > 2008] \Big\} && (3)\\
& + \sum_j \sum_k \kappa_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \Big\} * \mathbb{1}[k = s] \\
& + \sum_j \sum_k \kappa_3^{jk} \cdot \Big\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \kappa_4^{jk} \cdot \Big\{ \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] + \nu_{igst}
\end{aligned}
$$

Note that our approach allows for fully flexible 5-digit industry specific time shocks, so that our identification is not confounded by differences in the way various industries responded to the recession. Furthermore, within industry, we allow for fully flexible INPS code time shocks. In other words, we allow for the fact that within industry, firms in eligible and non-eligible INPS codes might have fared differently during the recession. Finally, within industry, we also allow for fully flexible time shocks interacted with firm size. This controls for the fact that, in the Italian Labor Law, firms are exposed to different employment protection legislation regimes when smaller or larger than 15 employees. Our strategy therefore allows for these differential regimes to impact differently over time firms just below 15 employees and firms just above 15 employees, within each industry.

Given this rich set of flexible controls, our identification rests on the assumption that there are no unobservable time shocks that would be, within each industry, specific to firms that are in the set of INPS codes eligible to CIGS *and* whose size is just above the 15 FTE threshold. Or equivalently, we rely on the parallel trend assumption that size specific time shocks are common across eligible and non-eligible INPS codes within the same industry, and that "INPS code"-specific time shocks within a given industry are common across firms just above and below 15.

We explore the credibility and validity of these assumptions in a series of robustness tests in Section 3.3. In terms of inference, we define two groups of firm sizes: a group with FTE above 15 in $t-1$ and a group with FTE below 15 in $t-1$, and we cluster all our standard errors at the INPS code times firm size group level. We explore additional inference approaches such as permutation tests (see footnote 17).

3.2 Results

Panel A of Figure 3 starts by providing a graphical representation of the variation used to identify the causal effects of STW. It plots the coefficients $\widehat{\gamma}_1^t$ for all years t from a regression following specification (1), using as an outcome the probability that a firm receives CIGS treatment. It confirms the evidence from Appendix Figure A-2 discussed above, that our instrument generates a sharp and significant first stage. Our instrument accounts for a 5 percentage point increase in the probability of CIGS take-up by firms during the 2008 recession, starting from a baseline very close to zero for all firms prior to the onset of the crisis. Regarding the timing, the graph also shows that CIGS take-up quickly increased after the onset of the recession, and was high throughout the recession, with a peak in 2013.

Figure 4 displays estimates of the effect of STW on employment outcomes and wages. For each panel, we plot the coefficients $\widehat{\gamma}_1^t$ for all years from 2000 to 2014, based on a regression following specification (1), and we also report on the graph the estimated IV coefficient $\widehat{\beta}_{IV}$ of the effect of CIGS treatment following the IV model in specification (2).

First, the figure provides supporting evidence for our identifying assumption, by confirming, for each outcome, the absence of differential pre-trends between firms just below and just above the 15 FTE threshold in eligible and non-eligible INPS codes within the same industry. The figure also suggests that STW has had large employment effects at both the intensive and extensive margin but insignificant effects on wage rates. Panel A shows that CIGS reaches its primary intent, by allowing firms to reduce employment at the intensive margin. Our estimates suggest that access to CIGS enables firms to significantly reduce the number of hours worked per employee by $e^{-.51} - 1 = 40\%$ on average. While reducing employment at the intensive margin, CIGS treatment significantly increases employment at the extensive margin, as shown in Panel B. Firms experience a large and highly significant increase in headcount employment of $e^{.38} - 1 \approx 45\%$ due to CIGS treatment. Importantly, Panel C suggests that CIGS has no statistically significant effect on wage rates, defined here as earnings per hour worked per worker. This rigidity of wages means that the wage bill per employee decreases significantly with CIGS, by about 45% as shown in Panel D, since workers work less hours for the same wage rate cost to the firm.

In Table 1, we provide additional results of the effects of STW treatment on various firms' outcomes. Panel B shows that the positive employment effects are driven by an increase in the relative number of employees in open-ended contracts. The estimated

IV coefficient for the effect of CIGS treatment on the log number (headcount) of employees in open-ended contract is $\widehat{\beta}_{IV} = .61$ (.043), but the number of employees in fixed-term contracts is negatively impacted by CIGS treatment ($\widehat{\beta}_{IV} = -.40$ (.11)). This reallocation of employment between open-ended and fixed-term contracts reflects the duality of the Italian labor market (Boeri [2011]) and shows that STW is mostly protective of the "insiders" of the labor market. We then decompose the total change in employment between inflows and outflows, and report in Panel B of Table 1 the separate effects of STW on the inflow and outflow rates. Results show that STW has a small, positive effect (although very imprecisely estimated) on the inflow rate. In fact, most of the effect is concentrated on the outflow rate: STW decreases firms' outflow rate by 34%. Panel B of Table 1 also reports the effect of STW on the probability of firm survival one year after treatment. The coefficient estimate is rescaled by the average survival probability in $t+1$. Results show that STW significantly increases survival probability by approximately 10%.

Panel C of Table 1 presents results on the effect of STW on balance-sheet and productivity outcomes. These results are estimated on the sample of firms that were matched to their balance-sheet data from CERVED. To get a better idea of the magnitude of the effects, we report the estimated IV coefficient $\widehat{\beta}_{IV}$ scaled by the average value of the outcome for non-eligible firms in the post-2008 period. Our results suggest that there is a small positive (yet not significant) effect of STW on firms' total output. We measure total output by firm value added, that is, total revenues plus unsold stocks minus cost of goods and services used in production.[15] We find a small positive insignificant effect of STW of .09 (.16). Value added per worker goes down significantly by roughly 50% (12%) in response to STW treatment. Interestingly, this result of a negative effect on value added per worker provides evidence that the hours and employment responses to STW are real responses, and are not simply driven by reporting behavior. One may indeed worry that collusive avoidance behavior may occur within the firm, by which firms report less hours to INPS so that workers may benefit from the STW subsidy, while real working hours remain unchanged. If it were the case though, value-added per worker would remain unchanged when measured in the CERVED data. The significant decline in value-added per worker indicates that our estimates of hour responses to STW capture real behavior rather than avoidance.

Finally we investigate the effect of STW on firms' investment and liquidity, defined as cash and cash equivalents. We do not find any effect on investment and find a positive effect (although very imprecisely estimated) on liquidity. Combined with the large employment effect of STW and with wage rigidity, the fact that a firm's liquidity

[15] In effect, this is equivalent to defining firm output as total profits plus total capital depreciation plus total wage cost.

reacts to STW treatment, suggests that internal funds constraints may play a role in amplifying employment responses to negative productivity shocks, as suggested by Schoefer [2015]. We provide additional evidence on the role of liquidity constraints in the next section.

3.3 Robustness

The first potential concern with our identification strategy is that firms may endogenously select into either firm size or eligible INPS codes in order to benefit from STW.

In terms of firm size, treatment eligibility is defined by a firm's six-month FTE size *prior* to STW application. While this may limit manipulation opportunities in practice, firms with private information about future shocks may still have the possibility to endogenously adjust their FTE size ex ante. To assess to what extent size manipulation creates significant selection susceptible of biasing our results, we first display in Appendix Figure A-3 the probability density function of FTE size over our entire sample period. Size manipulation to benefit from STW treatment in response to the 15 FTE threshold should result in "bunching from below", with missing mass just below the threshold, and excess mass above. The figure displays little signs of bunching from below. To provide more formal testing for size manipulation, we report in Appendix Figure A-4 results from McCrary tests of the presence of a discontinuity in the probability density function of FTE size. We report the statistic from the test and its confidence interval for each year, and separately for eligible and non-eligible INPS codes in Panel A and Panel B, respectively. In the presence of manipulation, we would expect a significant discontinuity in the probability density function for eligible INPS codes, which would be more pronounced during the Recession, if access to STW is indeed valuable during a recession. The figure shows that, for both eligible and non-eligible INPS codes, no statistically significant discontinuity in the probability density function of FTE firm size can be found, and that this holds for each year from 2000 to 2014. As a final exercise to assess the robustness of our results to size manipulation, we run a "doughnut" regression, where we exclude all firms with FTE between 12 and 18. Results, displayed in column 1 of Table 2 are almost identical to our baseline results, confirming that our estimated effects are not driven by selection due to size manipulation by firms.

Beyond their FTE size, firms may be willing to manipulate their INPS code, either through their codice autorizzazione or their industry code, in order to gain eligibility to STW. In practice, while not impossible, such manipulation is complicated, and extremely rare. Appendix Figure A-5 shows that less than .6% of firms change eligibility status due to a change in their INPS code every year in our sample, with the same fraction ($\approx$ .3%) of firms moving from being eligible to non-eligible and moving from being non-eligible to being eligible. Furthermore, these fractions are extremely stable over time. These results suggest that it is highly unlikely that firms endogenously self-select into INPS codes in order to get access to CIGS.

The identifying assumption underlying our strategy is that there is no time shock that would be specific to firms just above 15 FTE size threshold *and* in eligible INPS codes within 5-digit industry codes. To assess the credibility of this assumption and the robustness of our approach, we proceed in several steps. First, we show that there is little evidence of significant differential time shocks between eligible and non-eligible INPS codes within the same industry for firms just below the 15 FTE size threshold. To this end, we directly estimate differential trends across INPS codes within 5-digit industry codes using only firms with FTE size below 15 and therefore not eligible to receive STW. We estimate a model of the following form on a sample restricted to firms with size between 5 and 15 FTE in year $t-1$:

$$Y_{igst} = \alpha_1 \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[t \geq 2009] \right\} + \sum_k \alpha_2^k \cdot \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[k = s] + \sum_j \sum_k \alpha_3^{jk} \cdot \mathbb{1}[j = t] \cdot \mathbb{1}[k = s] + v_{igst} \tag{4}$$

We report in column 2 of Table 2, the estimated coefficient $\widehat{\alpha}_1$ of the interaction for being in eligible INPS codes after the start of the Great Recession. Results for all outcomes of interest show that differential effects of the Great Recession for eligible vs non-eligible INPS codes within the same industry are either not statistically significant or of very limited magnitude for firms with size below 15 FTE. These results confirm that within 5-digit industry, variation in CIGS eligibility across INPS codes, which is mostly a product of regulations from the Ministry of Labor in the 1970s, is quite plausibly exogenous to economic conditions today.[16]

The previous evidence suggests that, for firms with size below 15 FTE, there is no evidence of time shocks that would be, within 5-digit industries, specific to eligible INPS codes. But of course finding no differential trends across eligible and non-eligible INPS codes for firms below 15 employees does not preclude the possibility that such differential trends exist for firms above 15 employees. Indeed, firms below and above the 15 FTE threshold differ in terms of the employment protection legislation they are subject to. Heterogeneity in the treatment effects of employment protection legislation

[16] As a consequence, this means that our baseline results do not rely much on correcting for differential trends across eligible and non-eligible INPS codes within industry, using firms with less than 15 FTE. This can be clearly seen from results in column 3 of Table 2 which reports estimates from a specification where we focus on firms with size between 15 and 25 FTE only, and therefore only identify the effects of STW by comparing firms in eligible vs non-eligible INPS codes, before vs after the onset of the Great Recession. Results are indeed extremely similar to our baseline results.

across INPS codes may then create differential trends across INPS codes for firms with size above 15 employees. We assess the robustness of our results to this potential threat in two simple ways.

First we can directly assess the extent of heterogeneity in the treatment effects of employment protection legislation across INPS codes by running placebo specifications across non-eligible INPS codes. We restrict the sample to non-eligible INPS codes only. Among these non-eligible INPS codes, we randomly select a series of INPS codes, to which we attribute a placebo "eligible" status and then run the reduced-form of our baseline IV specification (2). We replicate this procedure 100 times and obtain bootstrapped estimates of the placebo reduced-form coefficient for the triple interaction of being a firm above the 15 FTE threshold in (placebo) eligible INPS codes after 2008. We report the set of estimated $\widehat{\gamma}_1^t$ coefficient from our placebo reduced-form regressions in Appendix Figure A-6. We also report the mean and standard error of the distribution of these 100 bootstrapped estimates in column 4 of Table 2. All estimates are statistically insignificant, very close to zero, with tight standard errors, showing no evidence of heterogeneous responses to the Recession across INPS codes by firms just above the 15-FTE threshold. This evidence clearly alleviates the concern that our baseline estimates may be picking up some idiosyncratic time shocks at the INPS code level for firms above the 15-FTE threshold during the Great Recession.[17]

Second, we use the fact that for some firms, the size thresholds that determine CIGS eligibility and employment protection legislation do not coincide. One reason for the two thresholds not to coincide is that employment legislation regulating dismissals apply in Italy when a firm reaches 15 employees within a single establishment, *or 60 employees in the firm in Italy as a whole*. But, as explained in footnote 9 above, eligibility to CIGS, and therefore eligibility requirements, all apply at the establishment level. We take the set of multi-establishment firms that have more than 60 employees across Italy, and select – within those firms – establishments with FTE size around the 15-threshold. In column 5 of Table 2, we run our baseline IV specification (2) on this sample. Because all these establishments are already subject to dismissal regulation, the identifying variation in CIGS eligibility cannot be confounded by potential heterogeneity in the treatment effect of employment protection laws. Results reported

[17]This placebo procedure naturally lends itself to a simple permutation test for the estimates obtained from our baseline specification. In other words, we can use the bootstrapped placebo estimates to determine what the likelihood would be of getting our baseline estimates if "treated" INPS codes were actually allocated randomly. We report in Appendix Figure A-7 the p-value from such tests for the baseline estimate of each coefficient $\widehat{\gamma}_1^t$ in specification (1), each panel corresponding to a different firm outcome. Results show that for the outcomes (intensive and extensive employment margin, and wage bill) where we find large statistically significant effects in our baseline specification, the probability of finding such effects "at random" is extremely small, and always below 5%. To the contrary, for the wage rate, where we find no statistically significant effect in our baseline specification, the p-value is large, which further suggests that wage rates seem to be totally unresponsive to STW.

in column 5 of Table 2 are qualitatively similar to our baseline estimates, with large negative effects on employment at the intensive margin and large positive effects on employment at the extensive margin, although much less precise due to the small size of this sample. In column 6 of Table 2, we provide additional evidence of the robustness of our results by focusing on another small group of firms in the retail sector for which the size threshold that determines CIGS eligibility is set at 50 FTE, and therefore does not coincide with the 15 FTE size threshold for employment protection legislation. We create a sample of single-establishment firms in the wholesale and retail sectors that ever reach a maximum 6-month FTE size between 25 and 75. We estimate our baseline model specification (2), on this sample, by replacing the dummy variable $\mathbb{1}[N_{i,t-1} > 15]$ with a dummy for reaching a maximum 6-month firm size above 50 FTE in year $t-1$. Results reported in column 6 are again very comparable to our baseline estimates, with negative effects on hours and large positive effects on headcount employment. Although point estimates are similar to our baseline estimates, standard errors are much larger due to the small size of this sample.

Taken together, this set of results provides evidence of the credibility of our identifying assumption, and of the robustness of our baseline results.

3.4 Targeting

Turning to the targeting of STW, we investigate whether firms that have a higher likelihood to separate from their workers are more likely to take up STW. To investigate this effect, we start by building a prediction model of the probability of mass layoff during the recession using a rich set of regressors including balance-sheet information and Bartik-style instruments.[18] We estimate this model using LASSO on the sample of non-eligible firms with more than 15 FTE. We then use the model to predict the incidence of mass layoff during the recession among eligible firms, and rank firms in quartiles of the distribution of the prediction score. In Appendix Figure A-8 we report the first stage estimate $\widehat{\kappa}_1$ from specification (3) in Panel A, and the IV estimates $\widehat{\beta}_{IV}$ from specification (2) in Panel B, splitting the sample by quartiles of the predicted score of mass layoff. Results in Panel A show that firms that would have been highly likely to layoff workers in the absence of STW are 80% more likely to select into treatment, conditional on eligibility. In that sense, STW is well-targeted towards firms that

[18] A mass layoff is a layoff of at least 5 workers over a time period of 120 days. We define an indicator for mass layoff taking value 1 in each year in which we observe at least 5 layoffs occurring over a 4-month period. The regressors included in the prediction model are: a Bartik-style index for employment shocks at the 2-digit industry level and provincial level, labor productivity, a Whited-Wu index of credit constraints, net revenues per employee, profits per employee, liquidity over total assets, cash flows over total assets, tangible and intangible assets over total assets. All regressors enter the model in levels, one-year lags and first differences.

are at risk of large reductions in employment. But interestingly, Panel B indicates that, conditional on STW take-up, there is no significant heterogeneity in hour reductions nor employment effects across different levels of mass-layoff risk.

4 Does STW Prevent Inefficient Layoffs?

Results from the previous section indicate that STW does increase employment. But is this necessarily efficient? In other words, if STW saves jobs, is it welfare enhancing to keep these jobs alive? To assess the welfare effects of STW programs, it is critical to know whether, absent STW, the level of employment would be inefficiently low, and the level of layoffs inefficiently large.

When a temporary negative shock hits, many reasons make it valuable for the firms and the workers to keep their match alive. First, there are frictions in the labor market, and the hiring and training of workers is a costly process. Furthermore, workers can develop human capital that is specific to the firm they work for. On the workers' side, a large body evidence shows that layoffs can have long-run scarring effects (e.g. Von Wachter, Song and Manchester [2009]). So if workers and firms know that their match is valuable, why would firms not hoard labor optimally? Two main mechanisms could actually make layoffs inefficiently high and labor hoarding too low. The first mechanism is the presence of liquidity constraints or, more generally, constraints to the ability to transfer resources across time. The second mechanism is inefficient bargaining, or the inability to transfer surplus between workers and firms. We explore both mechanisms.

4.1 Liquidity Constraints

The simplest way to think about labor hoarding is that it represents a transfer of resources across time. The firm pays a cost today for keeping its workers when productivity is down; the return of this investment is that these workers will generate surplus tomorrow when productivity is up again. Liquidity constraints, by limiting the ability to transfer resources across time, may prevent efficient labor hoarding. In Appendix B.1, we develop a simple model to illustrate this logic. The model also explains how STW policies can reduce inefficient labor hoarding by relaxing the liquidity constraint of firms. This will happen when the marginal hour worked in the firm has a marginal product below its cost. In that sense, the model illustrates the fact that STW can act as a good tag, channeling liquidity precisely towards firms experiencing drops in (unobserved) productivity.

We investigate empirically the role of liquidity constraints by using the subsample of firms for which we were able to match balance-sheet data from CERVED to our INPS records. We first analyze how liquidity affects the take-up of STW. To this end, we start by ranking firms by their level of liquidity – defined as cash and cash equivalents – divided by the total value of assets in 2008, just prior to the onset of the great Recession. We then split the sample into the four quartiles of the distribution of liquidity. We then run specification (3) using CIGS take-up as the outcome, and doing it separately for firms in each quartile. Results, reported in Panel A of Figure 5, show that firms with lower liquidity are significantly more likely to take up STW. We explore in the same panel the sensitivity of STW take-up to alternative measures of financial constraints. We compute for each firm its Whited-Wu index of financial constraint (Whited and Wu [2006]) in the period prior to 2008, using a method similar to Altomonte, Favoino and Sonno [2018], and we normalize the index by -1, so that the index ranges between 0 and 1 and is increasing in financial health. We then explore the probability of take-up running specification (3) splitting the sample into the four quartiles of the distribution of the normalized Whited-Wu index – lower quartiles corresponding to lower financial health. The results confirm that the take-up of STW is strongly increasing in measures of financial constraints of firms.

We then investigate how the hours, employment and survival responses to STW differ according to a firm's exposure to liquidity constraints. In Panel B of Figure 5, we report the IV estimates $\widehat{\beta}_{IV}$ from specification (2) splitting the sample between firms with below vs above median level of liquidity over total assets in 2008. Interestingly, the panel shows that the reduction in hours worked is significantly smaller in lower liquidity firms taking-up STW compared to firms with higher level of liquidity. As lower liquidity firms request a lower amount of STW hours, this also translates mechanically into a lower increase in employment than in high liquidity firms. But interestingly, we also compute and report in Panel B the elasticity of employment with respect to the hour reduction $\varepsilon_{n,h} = -\frac{d \log n / d\text{STW}}{d \log h / d\text{STW}}$. We find that this elasticity is greater for low liquidity firms (2.53 (.29)) than for high liquidity firms (1.97 (.21)).[19] In other words, the increase in employment per STW hour used is significantly stronger among low liquidity firms. We finally investigate heterogeneity in the effect of STW on firms' survival by degree of liquidity. We find significant positive effects of STW on firms' survival in $t + 1$ for low liquidity firms. These effects are quantitatively large: the probability of survival increases by 16.69% (5.98%) upon STW take-up for firms with below median liquidity pre-crisis. We do not find any such significant effect for firms with higher liquidity pre-crisis (1.09% (7.47%)).

The above evidence reveals a very strong sensitivity of STW take-up, as well as of

[19] Standard errors on the elasticity are computed using the Delta-method.

STW effects on employment and survival, to the level of firms' liquidity at the onset of the crisis. This suggests that liquidity constraints do play a critical role in explaining patterns of labor hoarding, as also evidenced by Giroud and Mueller [2017], and that STW can increase welfare by pushing firms' labor hoarding towards its efficient level. While we note that other policy instruments may help reduce firms' liquidity constraints, our results also show that STW is particularly effective at targeting firms with liquidity constraints, which might be more complicated to achieve with other policy instruments.

4.2 Inefficient Bargaining

The second reason why labor hoarding may not be optimal absent STW is the lack of efficient bargaining within the firm. If a match is valuable to both the worker and the firm, and if they can bargain efficiently, they should find ways to keep it alive. However, commitment issues and asymmetric information can make it complicated to find and enforce an efficient labor hoarding contract within the firm (Acemoglu [1995]). Second, the presence of bargaining frictions or institutional constraints, may create significant rigidities in wages and hours, which are the main channels to split the match surplus between the worker and the firm. In our context, there is substantial evidence of such rigidities.

In terms of wages, wage floors are fixed at the industry level via collective bargaining agreements between trade unions and employers organizations. Collective agreements are renewed on average every two years and close to 100% of private-sector employees are covered by such agreements.[20] Importantly, wage floors are set for all occupations, from blue collars to managers. Decentralized bargaining is subordinated to national-level bargaining (i.e. it only works "in melius") and has traditionally been limitedly used (Matano, Naticchioni and Vona [2019]). These provisions clearly limit the downward flexibility of wages in the Italian setting.

Similarly, we provide evidence of the presence of strong hour rigidities absent STW in Appendix Figure B-1, where we focus on firms that are not eligible to STW. For each worker i, who is present in firm j in two consecutive years over the period 2010-2014, we compute her annual change in average weekly hours worked $\Delta h_{i,j,t} = h_{i,j,t+1} - h_{i,j,t}$. We then plot the distribution $\Delta h_{i,j,t}$ using bins of size 1. The graph shows that

[20] Even though formally a collective agreement is only binding for workers who are members of the signatory union(s), in practice wage floors set in collective agreements are extended to all workers because they may be used by labor courts as a reference to determine compliance with Art. 36 of the Italian Constitution, stating that "workers have the right to a remuneration commensurate to the quantity and quality of their work, and in any case such as to ensure them and their families a free and dignified existence".

hours are remarkably rigid within the firm: 85% of workers do not see any change in their weekly hours worked between two consecutive years.

This combination of wage and hour rigidities can make it impossible to transfer surplus across parties in the employment relationship. At an extreme, if the productivity of a match falls below its wage cost, and this wage cost is rigid because either the wage rate or hours cannot be adjusted downwards, the firm may terminate a match that still bears positive surplus to the worker. Rigidities, in other words, may make the firm incapable of internalizing the workers' part of the employment surplus (Hall and Lazear [1984], Jäger, Schoefer and Zweimüller [2019]).

We provide evidence suggesting that firms, indeed, do not internalize workers' surplus when making labor hoarding decisions. For this, we take advantage of the presence of strong exogenous variation in the outside option of Italian workers generated by the Italian unemployment insurance system. Workers who meet the eligibility requirement for UI and are separated before the age of 50 are entitled to 8 months of benefits, while they are entitled to 12 months of benefits if they are separated after their 50th birthday. Because the surplus of a match for a worker is the difference between the value of the present labor contract and the value of her outside option (i.e. unemployment), this 50% increase in the potential duration of benefits generates a stark increase in the outside option of workers at age 50 which in turn should significantly reduce the size of the worker's surplus from employment. We now show that this large variation in workers' surplus does not correlate with the probability of being "hoarded" by the firm. We focus on firms who experience an episode of STW and restrict our sample to workers who meet the UI eligibility requirements. Using the age-50 cutoff in a regression-discontinuity design, in Figure 6 we plot the probability that a worker is "hoarded" and receives STW as a function of age. We find no evidence of a significant discontinuity at age 50. The corresponding RD estimate, using a quadratic polynomial, shows that the effect of the increase in the outside option at age 50 on the probability of receiving STW is very small (-.08 percentage points from a baseline of 65 percentage points) and insignificant. In Appendix Figure B-2, we explore the validity of our RD design: using standard McCrary tests, we show that there is no discontinuity in the density of workers at age 50, and that other observable characteristics of workers such as experience or tenure do not exhibit any sign of discontinuity at the age cutoff. Panel F also shows that the stark increase in UI generosity does not affect the hourly wage rates of workers.

This body of evidence suggests that the presence of various rigidities prevents firms from fully internalizing individual workers' surplus when making labor hoarding decisions. Firms may therefore terminate matches that exhibit significant value to workers. This evidence is in line with results from Jäger, Schoefer and Zweimüller [2019], who – in the context of Austria – identify the presence of "non-Coasean" bargaining, leading to inefficient separations. By increasing labor hoarding, STW may thus be welfare enhancing by preserving workers' surplus.

4.3 Trading-Off Inefficiency Correction vs Moral Hazard

Overall, both liquidity constraints and rigidities preventing efficient bargaining suggest that subsidizing labor hoarding can be desirable in the face of large temporary shocks. The efficient level of the STW subsidy will then have to trade-off the welfare gains from the positive efficiency correction on employment with the fiscal externality generated by moral hazard responses to the program. In Appendix B.3, we derive and provide an estimate of the total fiscal externality from the Italian STW program, based on our estimated elasticities of hours and employment to STW treatment. Our results suggest that for every Euro transferred to a worker on STW, the total cost to the government, due to behavioral responses, is around Euro 1.07. This means that, for the marginal Euro spent on STW to be efficient, society should be willing to pay a mark-up of about 7% on that Euro. The first thing to note about this number is that it is relatively low, especially when compared to UI, where the mark-up is typically estimated to be close to 50%. The reason why the fiscal externality is limited is that the cost created by the behavioral responses in hours is partially compensated by the positive employment effect, which reduces the cost to the UI system.[21] In other words, the larger the elasticity of employment with respect to hours, the lower the overall fiscal externality created by the program. Finally, we note that if the value of transferring one Euro to a STW worker is close to the estimated value of transferring a Euro to individuals on UI, then, the inefficiency correction does not have to be very large to make a marginal Euro spent on STW more efficient than a Euro spent on UI in response to temporary shocks.

5 Does STW Prevent Efficient Reallocation?

5.1 Temporary vs Permanent Shocks

The arguments laid above in favor of STW rely on the premise that the productivity shock faced by firms is temporary. But what if the shock becomes persistent? Would

[21] In Switzerland, Siegenthaler and Kopp [2019] find that the positive effect on UI costs due to labor hoarding is large enough to fully offset the cost of the program, suggesting that the total fiscal externality is lower than 1, and the program pays for itself.

STW, by subsidizing employment in persistently lower productivity firms, now hinder efficient reallocation in the labor market?

We study the reallocation effects of STW taking advantage of the specificities of the Italian double-dip recession of 2009. The initial shock of the financial crisis of 2008-2009 ended up being quite persistent in Italy, as shown in Figure C-1, because of the European Debt Crisis that immediately followed.[22] In this context, we show three pieces of evidence that highlight the impact of STW on efficient labor market reallocation. First, STW subsidizes matches that exhibit permanently lower levels of productivity. Second, the effects of STW are temporary and disappear quickly when the program lapses. Finally, labor reallocation and productivity growth is significantly lower in local labor markets that receive exogenously larger levels of STW treatment during the recession.

5.2 STW Subsidizes Low Productivity Matches

We start by documenting patterns of selection into STW take-up and heterogeneity in the treatment effects of STW according to pre-crisis levels of productivity. We use the sample of firms for which we have matched balance-sheet data from CERVED, and focus on two measures of productivity: labor productivity and total factor productivity (TFP). Labor productivity is defined as firm value-added in calendar year t divided by the total number of hours worked in the firm in year t. We compute the TFP of firm i in industry j in year t as $\text{TFP}_{ijt} = \text{VA}_{ijt}/(L_{ijt}^{\alpha_j} K_{ijt}^{\beta_j})$ where VA is total value added in year t, L_{ijt} is total wage bill, and K_{ijt} is fixed capital net of depreciation. The parameters α_j and β_j correspond to the labor share and the capital share respectively. We compute the labor share at the 2-digit industry level. It is the mean ratio of labor expenditure to value added for all firms in industry j. We then set the capital share as one minus the labor share, assuming a constant returns to scale production function (i.e. $\beta_j = 1 - \alpha_j$).[23] Our measure of TFP therefore captures the residual variation in value-added across firms within 2-digit industry codes, once controlling for employment and capital levels. We then rank firms in quartiles of the distribution of average yearly labor productivity, and of average yearly TFP, over the 2007-2008 period.

To investigate how pre-recession productivity affects STW take-up, we run the first-stage regression (3) separately for firms in each quartile of the distribution, taking as the outcome T the probability of ever taking up STW during the 2009-2014 period.

[22] Figure C-1 reports the evolution of real GDP per capita for Italy, France, Germany and the US. Each series is normalized to 100 in 2007. The graph illustrates quite strikingly how the initial shock due to the 2008-2009 financial crisis became a protracted double-dip recession in Italy, contrary to other European countries and the US.

[23] See Calligaris et al. [2016] for a similar implementation in the Italian context using CERVED data.

Results of the estimated coefficients $\widehat{\kappa}_1$, reported in Panel A of Figure 7, indicate that firms that had very low productivity prior to the recession are substantially more likely to take up STW conditional on eligibility. The fraction of firms using STW was four times larger in the bottom quartile of the pre-crisis TFP distribution than in the top quartile.

Do lower productivity firms also benefit more from this larger take-up of STW? In Panels B and C of Figure 7, we report estimates of $\widehat{\beta}_{IV}$, from the IV model (2), again estimated separately for each quartile of the pre-recession productivity distribution. Panel B focuses on hour effects and shows that low productivity firms tend to reduce hours significantly more when using STW. Panel C shows that this comes with limited total effects on employment. To the contrary, firms that were experiencing high productivity levels pre-recession seem to exhibit a much larger positive effect of STW on employment. As a result, the elasticity of employment to hour reductions increases sharply with pre-crisis productivity levels. For the bottom quartile of labor productivity for instance, the elasticity is small and insignificant, but it is as large as 4.19 (1.78) for the top quartile. In Panel D, we also report the estimated effects of STW on firms' survival by productivity level. Results indicate that firms at the bottom of the pre-crisis productivity distribution do not exhibit any positive effect of receiving STW on their probability of surviving through the crisis.

5.3 Dynamic Effects

The evidence from Figure 7 suggests that STW subsidizes mostly matches in low productivity firms. One concern is that such matches may not be able to survive a persistent negative shock. In that case, STW may only be a temporary fix. To investigate the validity of this concern in the context of the Great Recession in Italy, we explore the dynamics of STW treatment effects to investigate the longer-run impact of STW on firms and workers.

Dynamic Effects at the Firm Level. We start by looking at the dynamic effects of STW treatment at the firm level. As explained in Section 2, CIGS treatment is temporary. Firms can receive STW for a maximum of 12 months over a fixed 5-year period and, in practice, both average and median duration are very close to 52 weeks.

Our baseline estimates $\widehat{\beta}_{IV}$, which use the triple interaction $\mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t > 2008]$ as an instrument, are identifying the total effect of exposure to STW during the Great Recession.[24] In other words, they capture both contemporaneous

[24]This is because INPS codes and firm size, which determine access to STW, are persistent over time.

effects of STW treatment and past dynamic effects of STW treatment. To identify the sequence of dynamic treatment effects of STW $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$, we develop a methodology similar in spirit to the recursive identification of dynamic treatment effects in Cellini Riegg, Ferreira and Rothstein [2010]. All the details of the procedure are given in Appendix C.2. The main intuition is straightforward. Take all firms that are active in 2009, and define our instrument for STW access in 2009 – Z_{2009} – as the interaction between firm size and INPS code in 2009. The difference in outcome in 2009 of eligible firms in 2009 ($Z_{2009} = 1$) versus non-eligible firms ($Z_{2009} = 0$) only reflects the contemporaneous effect of treatment (β_0^{TOT}) in 2009. This is because there is no difference in 2009 in the probability of past treatment between eligible and non-eligible firms in 2009 as clearly shown in Appendix Figure C-2. Because eligible firms in 2009 are not only more likely to be treated in 2009, but also to be treated in 2010, the difference in their outcome in 2010 will reflect both the 1-year lagged effect of treatment in 2009 (β_1^{TOT}) and the contemporaneous effect of treatment (β_0^{TOT}) in 2010. And so on and so forth. That is, in any year $k \geq 2009$, the difference in outcome between firms that are eligible versus non-eligible in 2009 captures the dynamic Intention-To-Treat (ITT) effect from treatment in 2009 after k years, allowing for potential future treatment.

Exploiting this intuition, we show in Appendix C.2 that the sequence of ITT effects are identified by the coefficients for each year ($\beta_{2009}^{RF}, \beta_{2010}^{RF}$, etc.) of the reduced form relationship between the outcome and Z_{2009}. We also show that ITT effects have the following recursive structure as a function of TOT effects:

$$ITT_0 = \hat{\beta}_{2009}^{RF} \quad = \quad \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{5}$$

$$ITT_1 = \hat{\beta}_{2010}^{RF} \quad = \quad \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}, \qquad etc. \tag{6}$$

Using estimates of $\hat{\beta}_{2009}^{RF}, \hat{\beta}_{2010}^{RF}$, etc., and of the first stages $\widehat{\frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}}, \widehat{\frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}}}, etc.$, we can identify the sequence of dynamic TOT effects $\{\hat{\beta}_0^{TOT}, \hat{\beta}_1^{TOT}, ..., \hat{\beta}_4^{TOT}\}$.

Figure 8 reports the dynamic effects of STW treatment on hours per employee. Results suggest that the entire employment effects of STW are on impact. At the time of treatment, log hours per employee decrease by .3, but this effect disappears immediately after treatment, with no significant long-term impact. Appendix Figure C-3 shows similar patterns for other employment outcomes. Upon treatment, log head-

As a result, a firm that is eligible based on firm size and INPS code in year t is not only more likely to receive treatment in t, but also more likely to have received treatment in $t-1$, $t-2$, etc. Appendix Figure C-2 provides direct evidence of the correlation between current eligibility and past treatment by plotting the effect of the triple interaction $\mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[N_{i,t-1} > 15] * \mathbb{1}[j = t]$ on the probability to have been receiving treatment in the past 5 years.

count employment increases by .2 and the log wage bill decreases by .2, but both these effects dissipate instantly as treatment disappears. In the long run, the recursive identification lacks precision, as it makes standard errors become somewhat large.[25] Yet point estimates are consistently small, and close to zero, indicating no significant long term effects of treatment. This dynamic pattern of results, with short-run employment effects that quickly dissipate after treatment, is confirmed by our analysis of the dynamics of outcomes at the worker level, which we now turn to.

Worker-Level Event Studies. We document the dynamics of labor market outcomes of workers following STW treatment using event studies. We create a panel of the labor market histories of all employees of firms active and with FTE firm size $\in (5;25]$ at any point between 2000 and 2015. An event year is defined as the first year in which a worker experiences a STW spell. Treated individuals are individuals who experienced at least one STW spell. We run event study regressions on this sample of treated individuals, controlling for individual and calendar-year fixed effects and report in Figure 9 estimates for three outcomes, the probability of being employed, the total number of hours and total earnings plus all the social insurance transfers observable in the INPS data including STW.[26] Both hours and earnings are unconditional on employment. All estimates are relative to event year -1, and scaled by the average level of the outcome among the treated in year -1.

In Figure 9, we also report results for two comparison groups of similar workers not treated by STW. The first comparison group consists of workers with similar characteristics as treated workers pre-treatment, but who cannot access STW since they work in firms that are not eligible to CIGS based on their FTE size or INPS codes. To create this group, we match each treated worker, using Mahalanobis nearest-neighbour matching without replacement, with a worker from the sample of firms with FTE size $\in (15;25]$ and non-eligible INPS code, and with FTE size $\in (5;15]$ and eligible INPS code, in event year -1. Matching is based on gender, age, job characteristics at event time *t-1*, employment status, annual weeks worked, earnings and firm size at *t-1*, *t-2*, *t-3* and *t-4*, and main industry at *t-1*. For this control group, event year 0 is defined as the event year of their matched nearest-neighbor in the STW treatment group. The second comparison group consists of workers in non-eligible firms who experience a layoff, and is created following a similar nearest-neighbor-matching strategy using the same variables. For this group, event year 0 is defined as the year of the layoff.[27]

[25] We report bootstrapped standard errors for the TOT effects. Because of the recursive nature of identification, standard errors using the Delta-method equally suffer from this lack of precision.

[26] Social insurance transfers include transfers for all events that are covered by social insurance during an employment spell, e.g. paid sick leave, paid family leave, etc.

[27] We note that the event study estimates on workers treated by STW describe the dynamics of their

Results of the event study estimates for all three groups and all three outcomes are reported in Figure 9 and reveal interesting dynamic patterns. First, there seems to be no differential pre-event trends across the treated workers and our comparison groups, signaling little anticipation of STW treatment in terms of labor market trajectories. Second, treated STW workers experience, on impact, a sharp reduction of roughly 25% of their worked hours, a reduction close to our IV estimate of the effects of STW on hours using firm-level outcomes. This sharp drop in hours translates into a milder drop of 18% in total earnings and transfers, because of the high replacement of the STW subsidy.

When comparing the labor market outcomes of treated workers to our comparison groups during the treatment period, it is interesting to note that workers experiencing STW treatment maintain a probability of being employed similar to workers in non-eligible firms, and much larger than workers in the layoff comparison group. This is indicative that STW has indeed a positive effect on employment in the short run. However, despite having a similar probability of being employed, treated workers experience a reduction in hours that make their total employment, measured by total annual hours worked, much lower ($\approx$ 20 percentage point) than workers in non-eligible firms, and only 15 percentage point larger than laid-off workers. The high replacement rate of STW makes their total income from earnings and transfers significantly larger ($\approx$ 18%) than that of laid-off workers.

After STW is over, the beneficial effects of STW seem to dissipate quickly. Treated workers experience a sharp drop in labor market outcomes, confirming the reversal also observed for firms' outcomes. First, there is a sharp drop in the probability of employment and in total hours worked in the two years following treatment.[28] There is also a significant drop in total earnings and transfers of treated workers, which, 2 years after treatment, amount to only 65-70% of their pre-treatment level. In comparison to non-eligible workers, treated workers fare much worse in terms of all labor market outcomes in the medium and long run. But even more strikingly, two to three years after treatment, labor market outcomes of treated workers are only marginally better than those of non-eligible workers who were laid-off at time 0. This suggests that, while STW offers some short-run insurance, in the medium run, being laid-off or

labor market outcomes, but cannot be interpreted as the causal dynamic impact of STW. This is because the incidence and timing of CIGS treatment across firms are indeed not random and workers within these firms may differ from other workers along various characteristics affecting their labor market dynamics. We nevertheless show in Appendix C.3 under what assumptions the comparison of event study estimates for the treated group and for our two comparison groups can provide bounds on the dynamic treatment effects of STW. All details and results are reported in Appendix C.3.

[28]The decrease in total hours worked between event year 0 and 1 is a little less severe (15 percentage point) than that of the probability of employment (around 20 percentage point), and reflects the fact that hours conditional on employment increase post treatment, a result similar to what was observed in firm-level outcomes.

being put on STW are almost equivalent in terms of labor market outcomes.

In Figure 10, we explore how the dynamics of outcomes for workers treated by STW differs by firm's labor productivity level. We split the sample according to the average level of labor productivity of the firm in event-time years $t = -4$ to $t = -1$, using the same definition of labor productivity as in Section 5.2. For each subsample of STW treated workers, we define two new control groups, drawn from workers in non-eligible firms with similar level of labor productivity, and following the same methodology as in Figure 9. Panel A shows the results for workers in low productivity firms: when treated by STW, they do not fare better than laid-off workers in similarly low productivity firms 3 years after treatment, neither in terms of employment, nor in terms of earnings. To the contrary, Panel B demonstrates that for workers in high productivity firms, the long-run outcomes after STW treatment are significantly better than those of laid-off workers in similar high productivity firms.

Overall, these event studies confirm that STW has a positive effect on workers' outcomes during treatment and therefore provides short-term insurance to workers in firms exposed to shocks. However, in the context of a persistent economic shock such as the Great Recession in Italy, these effects partly disappeared after treatment. For low productivity matches, they entirely dissipated. For such matches, STW clearly provided only a short-term fix, but was not better than layoff in the medium run.

5.4 Reallocation Effects

STW take-up is high among low productivity matches that do not seem to survive a persistent shock after STW treatment stops. By keeping workers in these low productivity firms, STW is therefore susceptible of inefficiently delaying the efficient reallocation of workers towards more productive employment relationships. To empirically investigate the importance of reallocation effects, we leverage the rich spatial variation available in Italy across more than 600 local labor markets (LLM) defined by the Italian statistical agency (ISTAT), and estimate how an increase in the fraction of workers treated by STW in an LLM affects employment outcomes of non-treated firms.[29] In each LLM, we define the fraction of treated workers as the total number of workers on STW divided by the total number of employed workers observed from INPS records.[30] Appendix Figure D-1 shows the large amount of variation in the intensity of STW treatment across LLMs during the recession. Importantly, this spatial variation arises mostly within rather than between Italian regions. Yet, variation in the intensity

[29] We use the ISTAT 2011 classification of municipalities into 611 local labor markets.

[30] For employed workers, we use information about the address of the place of work available in the INPS individual records.

of STW treatment across LLMs will be of course endogenous to local economic and labor market conditions during the Great Recession, which might affect employment outcomes of non-treated firms. To account for this threat, we instrument the fraction of workers treated by STW during the recession by the average yearly fraction of eligible workers in the LLM in the pre-recession period, based on the interaction between firm size and INPS codes in the years 2005 to 2008. We identify the reallocation effects of STW on non-treated firms at the LLM level based on the following model:

$$\underset{t,t'}{\Delta} Y_{ij} \quad = \quad \alpha + \beta^R_{IV} \underset{t,t'}{\Delta} T_j + X'_j \gamma_0 + W'_i \gamma_1 + \varepsilon_{ij} \tag{7}$$

The model is estimated on the sample of all firms i that are non-eligible to STW based on their characteristics in 2008. $\underset{t,t'}{\Delta} Y_{ij}$ are long differences in average yearly employment outcomes of firm i in LLM j between the recession period t' and the pre-recession period t.[31] $\underset{t,t'}{\Delta} T_j$ is the long difference in the average yearly fraction of workers treated by STW in LLM j between period t and t'. The long difference in the fraction of workers treated by STW in LLM j is instrumented by the average yearly fraction Z_j of workers of LLM j that are eligible for STW during the pre-recession period based on the interaction between their firm size and INPS code in the pre-recession period. We control for a rich vector W_i of firm characteristics, correlated with CIGS take-up, and likely to affect firm employment outcomes during the recession. The vector is composed of 5-digit industry fixed effects, a dummy for eligible codice autorizzazione, as well as firm size in 2008 and a dummy for STW treatment. We also control for LLM characteristics that could be correlated with the fraction of treated workers and likely to affect employment outcomes during the recession, such as the industry composition of the LLM and the initial unemployment rate in the LLM prior to the recession. Identification therefore comes from comparing LLMs with similar characteristics, but with different allocations of workers within firm size times INPS code bins during the pre-recession period. We propose various tests for the validity of our exclusion restriction below. Standard errors are clustered at the LLM level. Appendix Figure D-2 provides evidence of the strong first-stage relationship between the fraction of eligible workers in an LLM during the pre-recession years 2005-2008 and the fraction of workers on STW during the recession conditional on controls for firm and LLM characteristics.

Panel A of Figure 11 provides striking evidence of the presence of significant reallocation effects of STW within LLMs. The graph is a binned scatter plot of the reduced-form of IV model (7), that is, the relationship between the instrument Z (the fraction of

[31] In our baseline estimation of model (7), we compare the recession years 2010-2013 to the pre-recession years 2005 to 2008.

eligible workers in the pre-recession period in an LLM based on the interaction of firm size and INPS codes) and the long difference in log employment of non-eligible firms. The reduced-form relationship is strongly negative, indicating that in LLMs with a larger fraction of eligible workers in the pre-recession period, employment growth of non-eligible firms was significantly worse during the recession. The corresponding IV estimate is β^R_{IV}=-.94 (.22), which means that a 1 percentage point increase in the fraction of treated workers in an LLM reduces employment of non-eligible firms by .94%. Another way of assessing the magnitude of these spillover effects on non-treated firms is to ask the following question: what is the impact of preserving one employment relationship in a firm treated by STW on the number of jobs in non-treated firms? Given our estimates of the effect of STW treatment on employment in treated firms, our $\widehat{\beta}^R_{IV}$ estimates imply that for one job "saved" by STW in a treated firm, employment in non-treated firms decreases by .03 job. Table 3 summarizes the results, and also shows that the employment effects are driven by a significant decline in inflows in non-eligible firms (measured as the number of new hires) as the fraction of workers treated by STW increases in the LLM.

By keeping more workers in low productivity firms, and by reducing the number of workers reallocating to non-treated firms, which have higher productivity than treated firms on average, STW is likely to affect overall productivity within the LLM. We explore this possibility by computing an LLM-level measure of TFP and running an IV model similar to (7) with long differences in LLM-level TFP as outcome.[32] The IV results, displayed in Table 3, confirm that STW has a significant negative impact on overall TFP within LLM, with a one percentage point increase in the fraction of workers treated by STW translating into a roughly 2% decrease in TFP growth.

One may worry about the validity of the exclusion restriction underpinning the IV estimates. This restriction may be violated if the fraction of workers eligible to CIGS in the pre-recession period based on the interaction of firm size and INPS code is correlated with other unobserved characteristics of the LLM affecting employment and TFP growth. To assess the credibility of our strategy we run placebo models similar to (7) where we now compare long differences between 2000-2005 and 2005-2008, and use as a placebo instrument the fraction of eligible workers in the LLM based on the interaction between firm size and INPS codes in the 2000-2005 period. Because there is no take-up of CIGS during the 2005-2008 period, there is no first stage in this model, so that our placebo instrument will only pick up an effect if the exclusion restriction does not hold, and the instrument is correlated with other determinants of employment and TFP growth within an LLM. The reduced-form relationship of the placebo

[32] We define TFP as TFP $=$ VA$/(L^{\alpha}K^{\beta})$, but we now aggregate all variables (VA, L and K) at the LLM level.

model for employment growth of non-eligible firms in the LLM are reported in Panel B of Figure 11. We clearly see no significant relationship between the placebo instrument and the outcomes, which provides comforting evidence for the validity of our exclusion restriction. We report similar placebo models for TFP growth in Table 3 and find no significant relationship between our instrument and TFP growth in the LLM in the pre-recession period.

Overall, by leveraging the rich spatial variation across LLMs in Italy, and the variation in STW treatment created by the interaction of firm size and INPS codes, these results provide compelling evidence that STW has significant equilibrium effects within labor markets. STW creates significant spillover effects on non-treated firms by limiting the reallocation of workers. Non-treated firms are less able to grow and hire new workers as a result. Moreover, by tilting the allocation of workers towards less productive firms, STW has a significant negative impact on TFP growth in the labor market.

These reduced-form estimates identify clearly the presence of reallocation effects of STW. But they cannot tell us what labor allocation and TFP would look like absent STW. To get a sense of the magnitude of the reallocation effects of STW implied by this reduced-form evidence, we turn in Appendix E.1 to a calibrated matching model of the Italian labor market during the Great Recession. The model incorporates two types of firms that differ by their productivity level, and adds the possibility for low productivity firms to use a STW subsidy for reducing hours. The contribution of the model is to calibrate key parameters of the structure of the model – such as parameters of the matching function and of the firm's production function – based on our reduced-form quasi-experimental evidence. We use the model to quantify how the presence of STW affected the equilibrium allocation of employment and total factor productivity of the Italian economy. Results of our counterfactual analysis, reported in Appendix Figure E-2, suggest that – absent STW – the level of unemployment would have been 1.8 percentage point higher in Italy during the recession. The presence of STW reduced the level of employment in high productivity firms by about 10%, and increased the amount of employment in low productivity firms by a little less than 50%. Overall, the model suggests that STW, by tilting the allocation of workers towards low productivity firms, reduced the total factor productivity of the Italian economy by about 2% during the Great Recession.

6 Concluding Remarks

STW programs have attracted a lot of attention as a tool to subsidize labor hoarding, and have been aggressively used during the current COVID-19 crisis. Yet, very little is known about their effects and welfare consequences. This paper contributes by providing new high-quality administrative data and a compelling quasi-experimental setting to investigate the employment and welfare consequences of STW.

The first important takeaway from our analysis is that STW has positive and significant effects on employment. The second takeaway is that, to assess the welfare consequences of this increase in employment, the degree of persistence of the shock is key. The welfare effects of STW differ markedly if the shock is temporary or if it persists over time.

In the presence of temporary shocks, our paper confirms that substantial frictions prevent efficient labor hoarding by firms. We provide evidence of the presence of two types of frictions that make employment inefficiently low in response to temporary shocks: first, frictions such as liquidity constraints that prevent firms from transferring resources across time; second, frictions, such as wage and hour rigidities that prevent surplus to be transferred between workers and firms. Our results show that the positive employment effects of STW are significantly larger when these frictions are more prevalent.

When the shock becomes persistent, our paper highlights that the benefits of STW must be traded-off against the potential reallocation effects of the program. The severity of the reallocation problem depends on the characteristics of the employer-employee matches that are hit by the shock. In the context of the Great Recession in Italy, we show that the shock was quite persistent and hit firms that had low productivity prior to the crisis. These employment matches were unable to survive a persistent shock; as a consequence, STW was a temporary fix for the majority of them. The positive effects of STW did not on average survive the end of the program. The positive effects of STW did last longer only for firms that had higher productivity prior to the recession. Overall, our paper shows that, by keeping workers in low productivity firms, STW had negative effects on reallocation and productivity, although the magnitude of these effects remains limited.

How much can these results teach us about the welfare effects of STW in the COVID crisis? On the one hand, one needs to assess external validity carefully and account for the difference in the nature of the shocks. On the other hand, it is likely that, due to a lack of identification opportunities, it will be difficult to identify the causal effects of STW in the current recession. With this in mind, we believe our results do provide some useful guidance for understanding the consequences of STW schemes in the current COVID-19 crisis. They suggest that STW probably prevented a large and inefficient surge in unemployment. If the overall fiscal externality generated by moral hazard was on par with the relatively limited level observed in Italy during the Great Recession, the welfare benefits of STW may have been large. Our results also emphasize that the magnitude of the reallocation issue will depend on the characteristics of the firms that will be more affected if the shock were to persist, as this will determine how employment matches can survive in the medium run. Interestingly, the nature of the pandemic suggests that, contrary to the financial crisis of 2008, the shock may be orthogonal to firms' productivity prior to the crisis. To fully establish the welfare consequences of the massive subsidization of labor hoarding during the COVID-19 crisis, we finally note that more research is necessary, in particular to assess the aggregate demand effects of STW through firm survival and employment expectations (Guerrieri et al. [2020]).

References

Acemoglu, Daron. 1995. "Asymmetric Information, Bargaining, and Unemployment Fluctuations." *International Economic Review*, 36(4): 1003–24.

Altomonte, Carlo, Domenico Favoino, and Tommaso Sonno. 2018. "Markups and Productivity under Heterogeneous Financial Frictions." BAFFI CAREFIN Centre Research Paper 100.

Boeri, Tito. 2011. "Institutional Reforms and Dualism in European Labor Markets." *Handbook of Labor Economics*, 4(Part B): 1173–1236.

Boeri, Tito, and Herbert Bruecker. 2011. "Short-Time Work Benefits Revisited: Some Lessons from the Great Recession." *Economic Policy*, 26(68): 697–765.

Burdett, Kenneth, and Randall Wright. 1989. "Unemployment Insurance and Short-Time Compensation: The Effects on Layoffs, Hours per Worker, and Wages." *Journal of Political Economy*, 97(6): 1479–1496.

Cahuc, Pierre, and Stéphane Carcillo. 2011. "Is Short-Time Work a Good Method to Keep Unemployment Down?" Institute for the Study of Labor (IZA) Discussion Papers 5430.

Cahuc, Pierre, Francis Kramarz, and Sandra Nevoux. 2018. "When Short-Time Work Works." Centre for Economic Policy Research (CEPR) Discussion Paper 13041.

Calligaris, Sara, Massimo Del Gatto, Fadi Hassan, Gianmarco Ottaviano, and Fabiano Schivardi. 2016. "Italy's Productivity Conundrum. A Study on Resource Misallocation in Italy." Directorate General Economic and Financial Affairs (DG ECFIN), European Commission.

Cellini Riegg, Stephanie, Fernando Ferreira, and Jesse Rothstein. 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics*, 125(1): 215–261.

Galí, Jordi. 2011. "The Return of the Wage Phillips Curve." *Journal of the European Economic Association*, 9(3): 436–461.

Giroud, Xavier, and Holger M. Mueller. 2017. "Firm Leverage, Consumer Demand, and Employment Losses during the Great Recession." *Quarterly Journal of Economics*, 132(1): 271–316.

Guerrieri, Veronica, Guido Lorenzoni, Ludwig Straub, and Iván Werning. 2020. "Macroeconomic Implications of COVID-19: Can Negative Supply Shocks Cause Demand Shortages?" National Bureau of Economic Research Working Paper 26918.

Hall, Robert, and Edward Lazear. 1984. "The Excess Sensitivity of Layoffs and Quits to Demand." *Journal of Labor Economics*, 2(2): 233–57.

Hijzen, Alexander, and Sébastien Martin. 2013. "The Role of Short-Time Work

Schemes during the Global Financial Crisis and Early Recovery: A Cross-Country Analysis." Institute for the Study of Labor (IZA) Discussion Papers 7291.

Jäger, Simon, Benjamin Schoefer, and Josef Zweimüller. 2019. "Marginal Jobs and Job Surplus: A Test of the Efficiency of Separations." National Bureau of Economic Research Working Paper 25492.

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018*a*. "A Macroeconomic Approach to Optimal Unemployment Insurance: Theory." *American Economic Journal: Economic Policy*, 10(2): 152–81.

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018*b*. "A Macroeconomic Approach to Optimal Unemployment Insurance: Applications." *American Economic Journal: Economic Policy*, 10(2): 182–216.

Landais, Camille, and Johannes Spinnewijn. 2019. "The Value of Unemployment Insurance." Centre for Economic Policy Research (CEPR) Discussion Paper 13624.

Le Barbanchon, Thomas. 2020. "Taxes Today, Benefits Tomorrow." Working Paper.

Marinescu, Ioana. 2017. "The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board." *Journal of Public Economics*, 150: 14–29.

Matano, Alessia, Paolo Naticchioni, and Francesco Vona. 2019. "The Institutional Adjustment Margin to Import Competition: Evidence from Italian Minimum Wages." Institute for the Study of Labor (IZA) Discussion Paper 12714.

Michaillat, Pascal. 2012. "Do Matching Frictions Explain Unemployment? Not in Bad Times." *American Economic Review*, 102(4): 1721–1750.

Schmieder, Johannes F., Till von Wachter, and Stefan Bender. 2012. "The Effects of Extended Unemployment Insurance over the Business Cycle: Evidence from Regression Discontinuity Estimates over Twenty Years." *Quarterly Journal of Economics*, 127(2): 701–752.

Schoefer, Benjamin. 2015. "The Financial Channel of Wage Rigidity." Working Paper.

Siegenthaler, Michael, and Daniel Kopp. 2019. "Short-Time Work and Unemployment in and after the Great Recession." KOF Swiss Economic Institute Working Paper 19-462.

Tilly, Jan, and Kilian Niedermayer. 2016. "Employment and Welfare Effects of Short-Time Work." Working Paper.

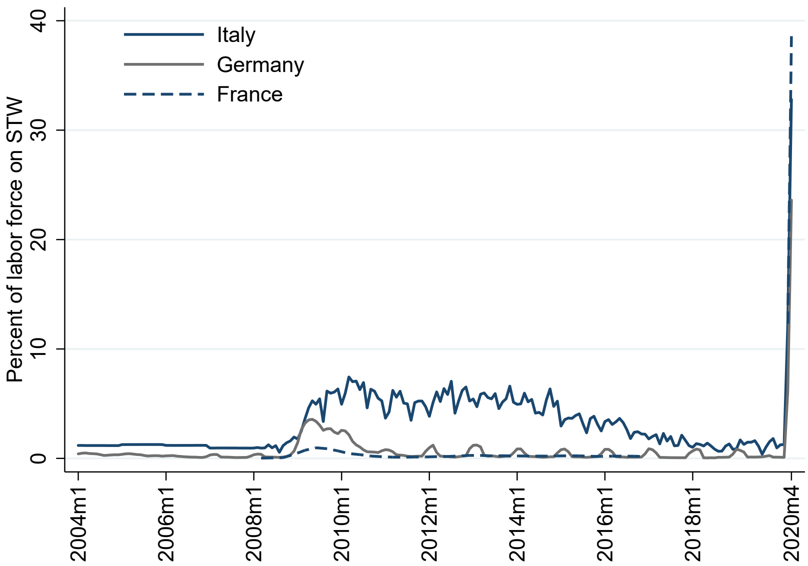
U.S. Department of Labor, Employment and Training Administration. 2020. "Short-Time Compensation Fact Sheet." https://stc.workforcegps.org/resources/2016/03/15/00/46/STC_Fact_Sheet.

Van Audenrode, Marc A. 1994. "Short-Time Compensation, Job Security, and Employment Contracts: Evidence from Selected OECD Countries." *Journal of Political Economy*, 102(1): 76–102.

Von Wachter, Till, Jae Song, and Joyce Manchester. 2009. "Long-Term Earnings Losses Due to Mass Layoffs During the 1982 Recession: An Analysis Using U.S. Administrative Data from 1974 to 2004." Unpublished Paper.

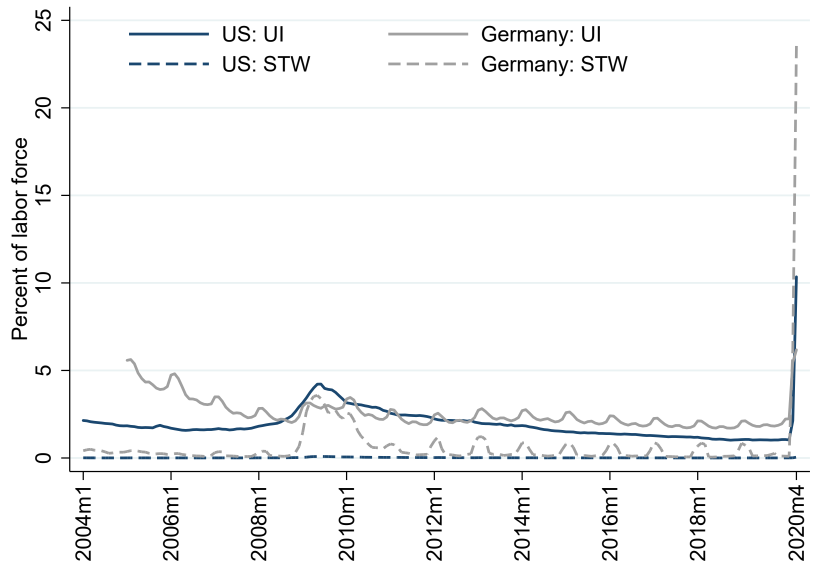
Whited, Toni M., and Guojun Wu. 2006. "Financial Constraints Risk." *Review of Financial Studies*, 19(2): 531–559.

Figure 1: LABOR MARKET POLICY RESPONSES IN EUROPE IN THE COVID-19 CRISIS AND THE RISE OF SHORT TIME WORK



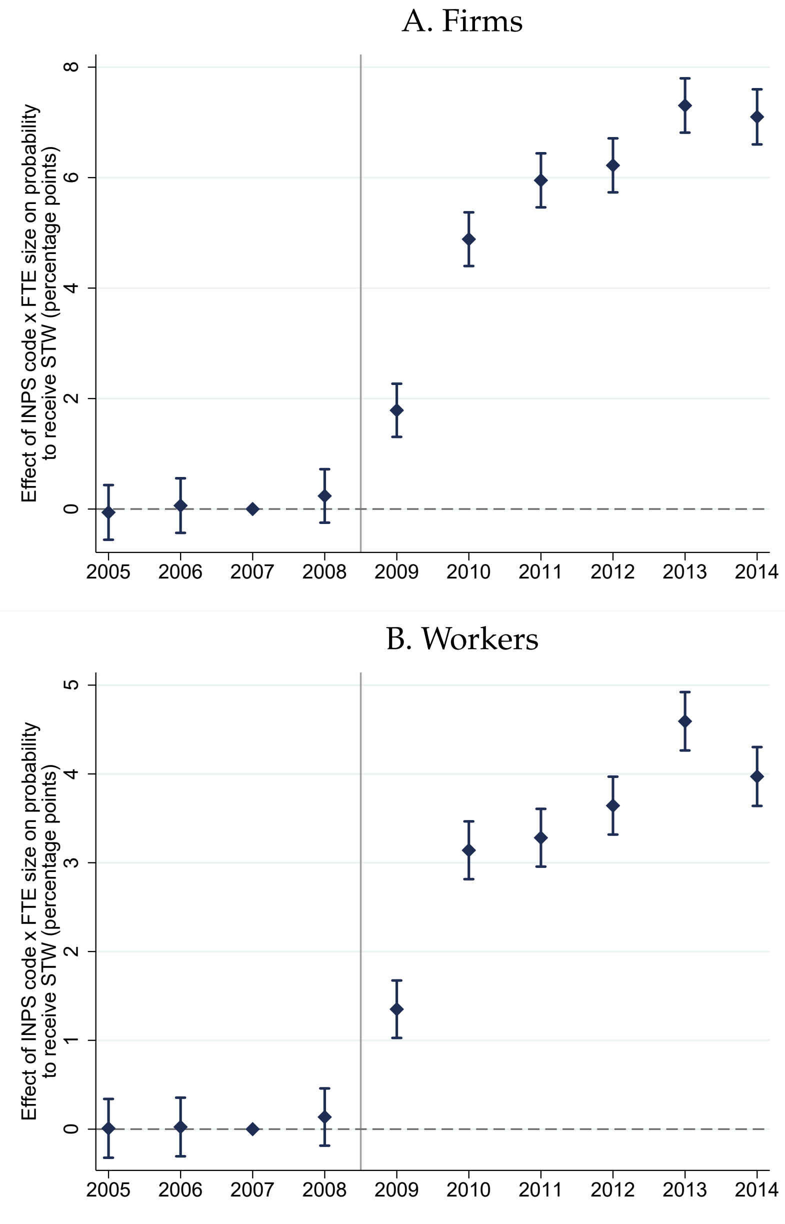
Notes: Panel A reports the percent of the labor force on STW in France, Germany and Italy, at monthly frequency. For Italy, we use data on monthly authorized hours of CIG, as provided by INPS. Before March 2020, authorized hours are converted into headcounts assuming a ratio of used hours to authorized hours of 90% (as per our calculations on INPS data), a work week of 40 hours and an average of 35% of STW hours per week. Data for March and April 2020 are headcounts of CIG beneficiaries as per INPS' statements. For France, we use data from the French Ministry of Labor on the average monthly number of workers on STW in each quarter until December 2016. There is no comparable data between December 2016 and February 2020. Data for March and April 2020 are the cumulated number of workers on STW at the end of each month, from the Ministry's latest releases. For Germany, we use data from the German Employment Agency on the monthly number of workers on STW until October 2019. For the period November 2019-March 2020, we use the number of workers that have been notified to be on STW. For April 2020, we use data on STW notifications from https://de.statista.com/statistik/daten/studie/2603/umfrage/entwicklung-des-bestands-an-kurzarbeitern/. The series are rescaled by the monthly labor force taken from Eurostat. We use the labor force in December 2019 to rescale quantities in 2020.

Figure 2: LABOR MARKET POLICY CHOICES IN EUROPE AND THE US IN THE COVID-19 CRISIS



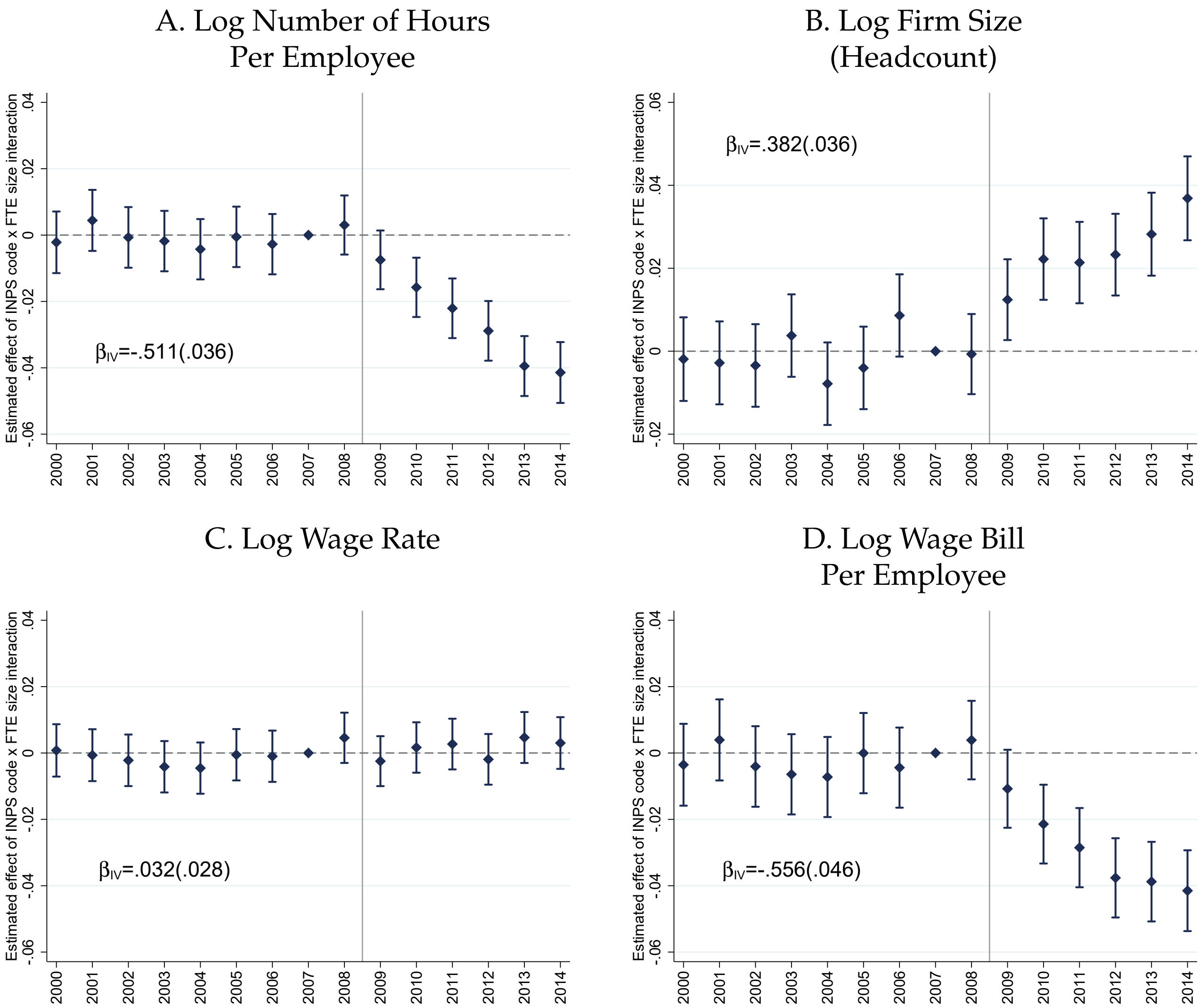
Notes: The graph reports the percent of the labor force involved in STW or UI in Germany and the US, at monthly frequency. For Germany, STW data is the same as for Figure 1. Data on UI is also taken from the German Employment Agency and corresponds to the number of individuals receiving UI in a given month. Data for March and April 2020 is taken from the Agency's statements. For the US, STW and UI data are, respectively, the number of workers on STW and continued UI claims from the Department of Labor. Originally at weekly level, the data is averaged over each month to obtain monthly figures. The series are rescaled by the monthly labor force taken from Eurostat for Germany and from BLS for the US. We use the labor force in December 2019 to rescale quantities in 2020.

Figure 3: FIRMS' & WORKERS' PROBABILITY OF RECEIVING SHORT TIME WORK TREATMENT BY FIRM SIZE AND SECTOR



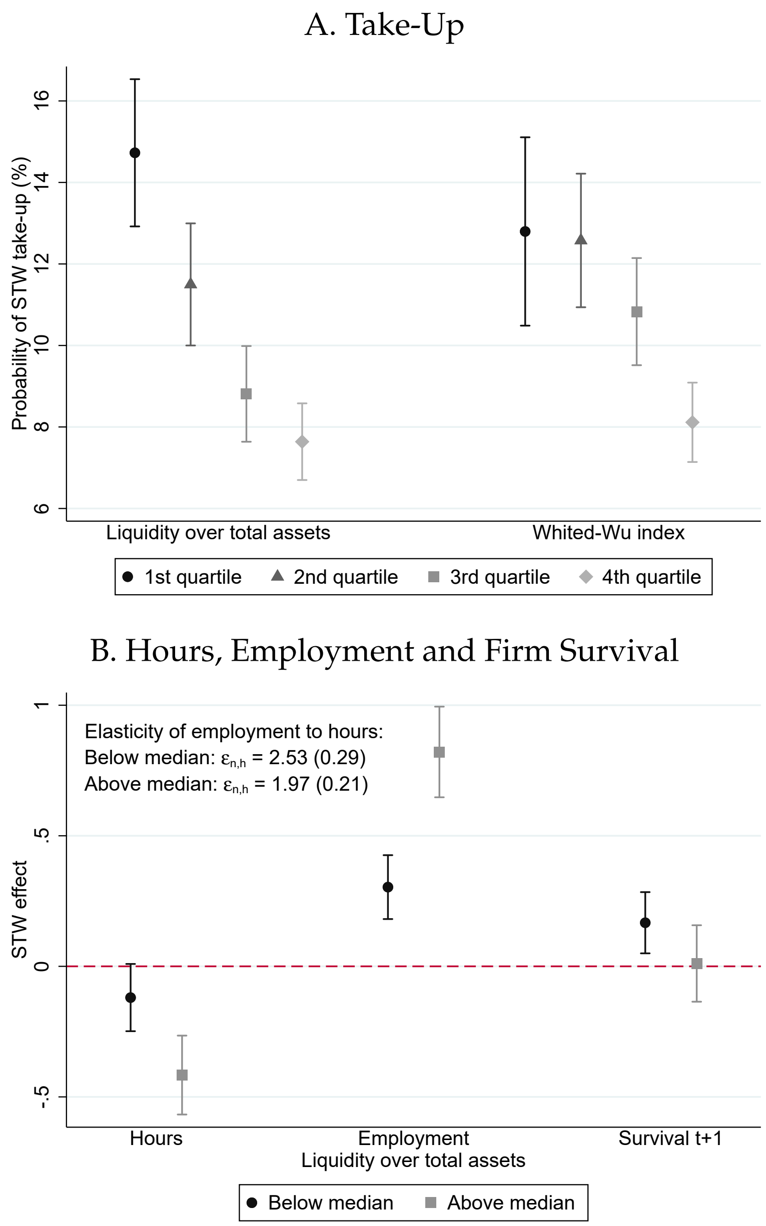
Notes: The graphs report the coefficients $\widehat{\gamma}_1^t$ estimated from equation (1) for all years $t \in [2005, 2014]$ using the probability of STW receipt as outcome. The omitted year is 2007, so all results are relative to 2007. Panels A and B plot the estimated coefficients for the probability of STW receipt at the firm level and at the worker level respectively. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level.

Figure 4: ESTIMATES OF THE EFFECTS OF SHORT TIME WORK ON FIRMS' OUTCOMES



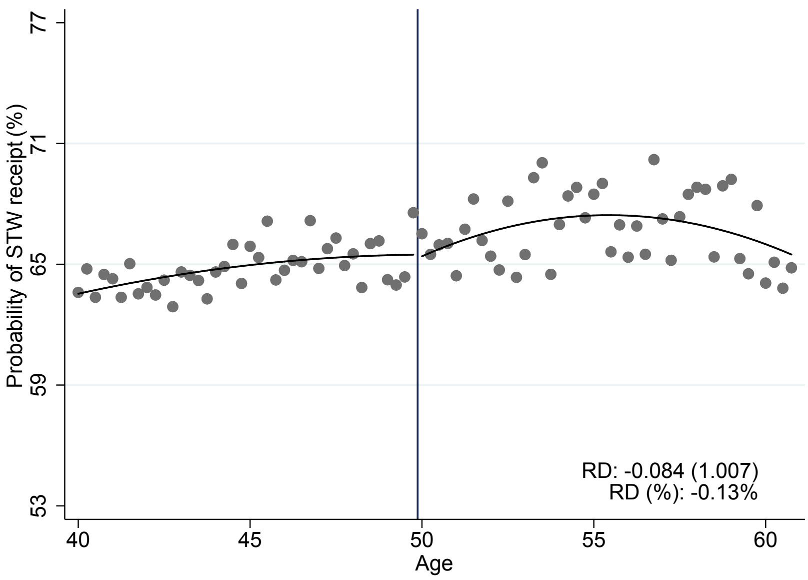
Notes: The graphs show the coefficients $\widehat{\gamma}_1^t$ estimated from equation (1) for all years $t \in [2000, 2014]$ for different firm-level outcomes. The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level. Each graph also reports the coefficient $\widehat{\beta}_{IV}$ estimated from equation (2) and its associated standard error. The wage rate is defined as earnings per hour worked per employee.

Figure 5: EFFECTS OF SHORT TIME WORK BY MEASURES OF LIQUIDITY CONSTRAINTS



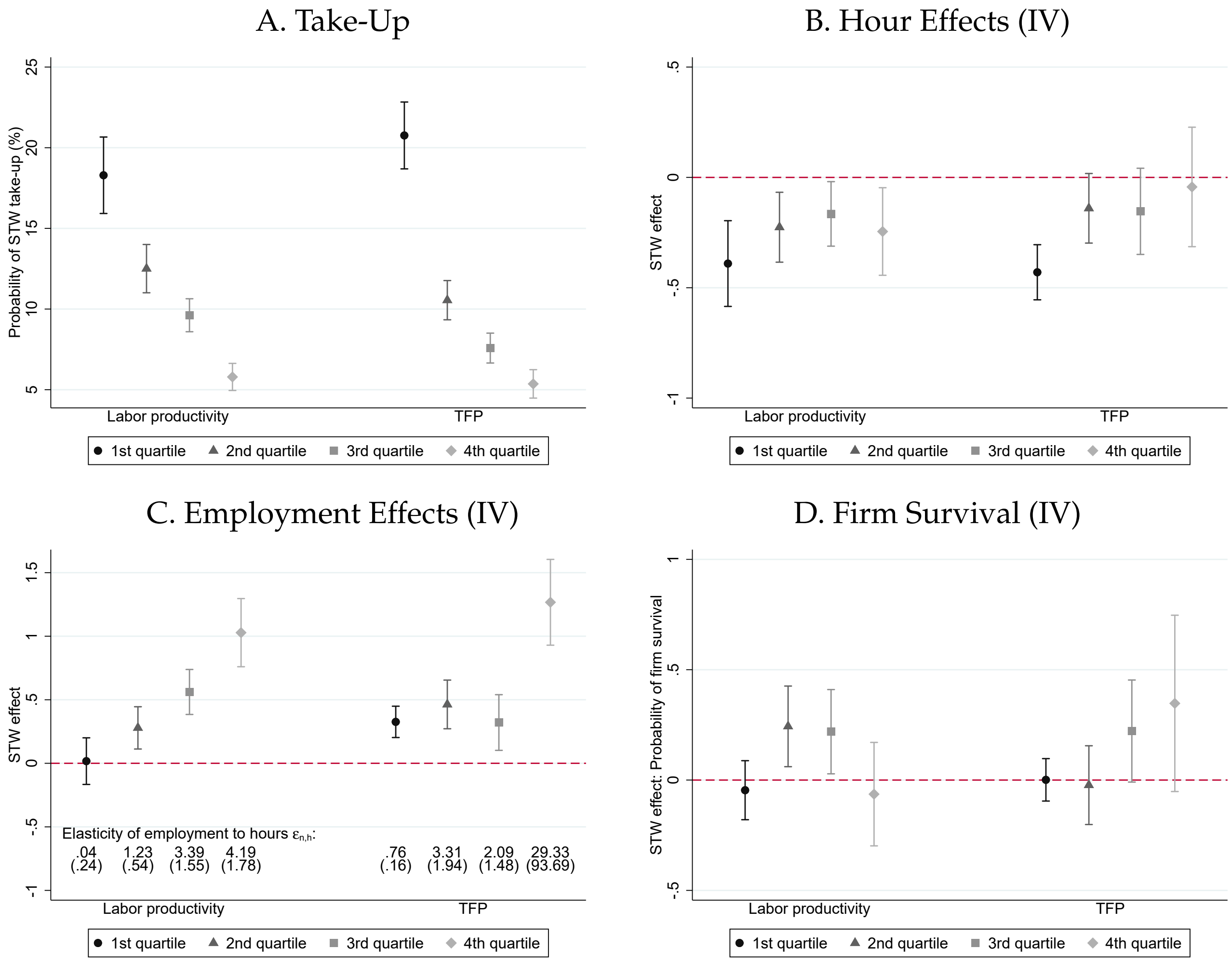
Notes: The graphs show heterogeneity in STW take-up and treatment effects by measures of liquidity constraints. Panel A displays the estimated coefficient $\widehat{\kappa}_1$ from specification (3) for the probability of STW take-up for firms at different quartiles of the distribution of liquidity – defined as cash and cash equivalents – over total assets, and of the Whited-Wu index of financial health (Whited and Wu [2006]). The Whited-Wu index is normalized so that it is increasing in financial health. We rank firms into the four quartiles of the distribution of each of these measures in 2008, and estimate specification (3) on the sample of firms in each quartile. Panel B reports the IV estimates $\widehat{\beta}_{IV}$ from specification (2) for different outcomes, splitting the sample between firms with below vs above median level of liquidity over total assets in 2008. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level. In Panel B, we also report the elasticity of employment with respect to the hour reduction $\varepsilon_{n,h} = -\frac{d\log n/d\mathrm{STW}}{d\log h/d\mathrm{STW}}$, with standard errors computed using the Delta-method.

Figure 6: PROBABILITY OF SHORT TIME WORK RECEIPT AS FUNCTION OF AGE



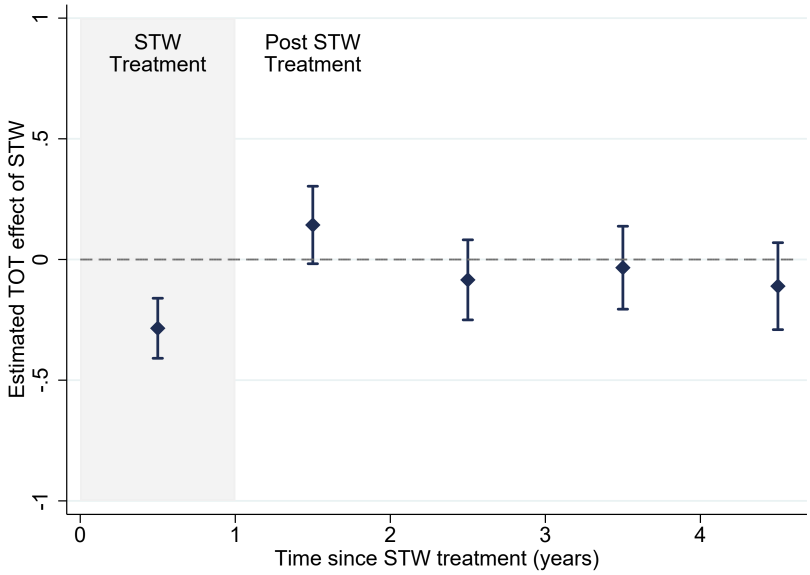
Notes: The graph shows the average probability that a worker receives STW as a function of her age. The sample is restricted to workers who meet the eligibility requirements for STW and for UI, and who are in a firm that experiences an episode of STW. The grey dots indicate the average probability of receiving STW in each age bin, conditional on firm, year and firm by year fixed effects. Age is measured in months in July of each year and is binned into bins of 3-month width. The solid dark lines display predicted values from a quadratic polynomial fit, conditional on firm, year and firm by year fixed effects. The graph reports the RD estimate at the 50-age cutoff and associated robust standard error, and the RD coefficient rescaled by the mean of the outcome variable in the four quarters to the right of the age-50 cutoff. The RD coefficient is estimated using a quadratic polynomial, conditional on firm, year and firm by year fixed effects.

Figure 7: SELECTION OF FIRMS INTO SHORT TIME WORK AND HETEROGENEOUS TREATMENT EFFECTS BY LEVEL OF PRE-RECESSION PRODUCTIVITY



Notes: The graphs show heterogeneity in STW take-up and treatment effects by measures of firm productivity. Panel A displays the estimated coefficient $\widehat{\kappa}_1$ from specification (3) for the probability of STW take-up for firms at different quartiles of the distribution of labor productivity – defined as value added per hour worked – and of total factor productivity (TFP) – defined in Section 5.2. We rank firms into the four quartiles of the distribution of each of these measures in 2008, and estimate specification (3) on the sample of firms in each quartile. Panels B, C and D report the IV estimates $\widehat{\beta}_{IV}$ from specification (2) for different outcomes. The three panels are otherwise constructed in the same way as Panel A. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level. In Panel C, we also report the elasticity of employment with respect to the hour reduction $\varepsilon_{n,h} = -\frac{d \log n / d\text{STW}}{d \log h / d\text{STW}}$, for each quartile and with standard errors computed using the Delta-method.

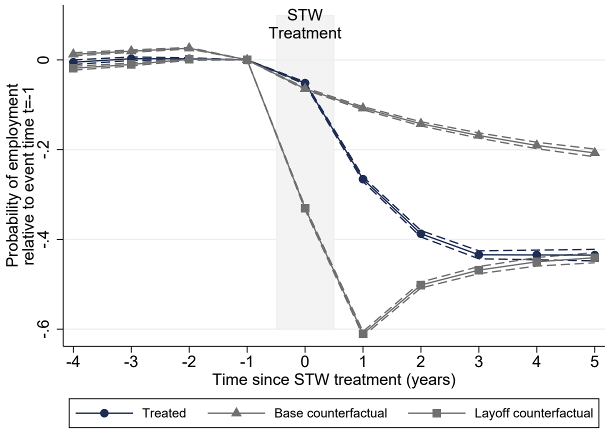
Figure 8: TOT ESTIMATES OF THE DYNAMIC EFFECT OF SHORT TIME WORK ON LOG NUMBER OF HOURS



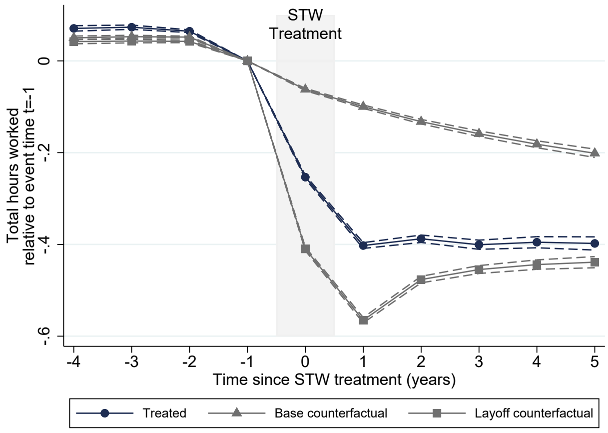
Notes: The graph reports the coefficients $\widehat{\beta}_k^{TOT}$ for $k \in [0, ..., 4]$ for the dynamic effects of STW treatment on hours worked per employee. These effects are estimated recursively as illustrated in Appendix C.2. The β_k^{TOT} coefficients identify the dynamic treatment effects of STW receipt in year $k = 0$ on outcomes in years $k \in [0, ..., 4]$. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors.

Figure 9: DYNAMIC EFFECTS OF SHORT TIME WORK ON WORKERS' OUTCOMES

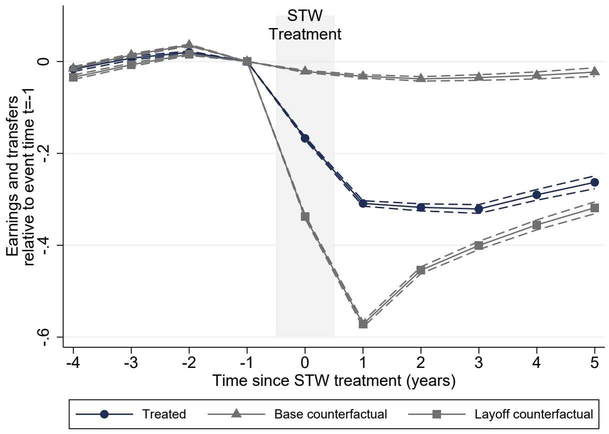
A. Probability of Employment



B. Number of Hours Worked



C. Earnings + CIGS/Transfers

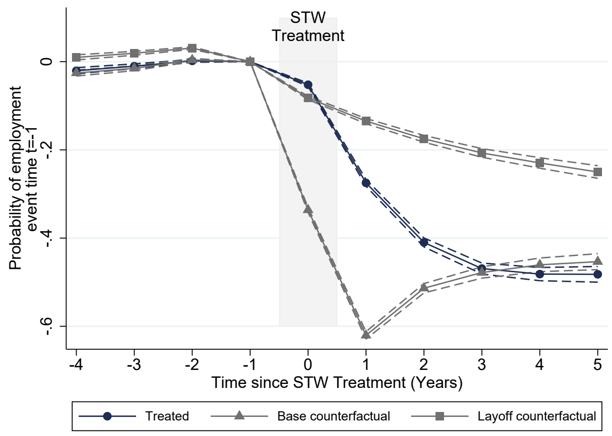


Notes: The graphs report the estimated coefficients of event study regressions for different outcomes and different event-year definitions at the worker level. All estimates are relative to event-year -1 and are scaled by the average level of the outcome in that year. Individual and calendar-year fixed effects are included in the event-time specification. The dashed lines around the estimates indicate 95% confidence intervals based on robust standard errors clustered at the individual level. For the treatment group (indicated by solid circles), an event year is defined as the first year in which the worker experiences a STW event, conditional on the worker being in an eligible firm (according to the FTE size and INPS code eligibility requirements) at event time -1. The first comparison group (indicated by solid triangles) consists of workers employed at firms with 6-month average FTE size $\in (5;25]$ at event time -1, which are not eligible for STW due to either their INPS code or FTE size. The second comparison group (indicated by solid squares) consists of workers employed at non-eligible firms with 6-month average FTE size $\in (5;25]$ at event time -1 and who experience a layoff at event time 0. Note that – for both counterfactuals – we consider as non-eligible, firms with non-eligible INPS code and size $\in (15;25]$, and firms with eligible INPS codes and size $\in (5;15]$. Individuals in the two comparison groups are matched to individuals in the treatment group using Mahalanobis nearest-neighbor matching without replacement based on gender, age, job characteristics at event time -1, employment status, annual weeks worked, earnings and firm size at event times -1, -2, -3 and -4, and main industry at event time -1. Total hours worked and total earnings are unconditional on employment. In Panel C, we report the evolution of all earnings, and all transfers received (including STW or any other social insurance program available in the INPS data).

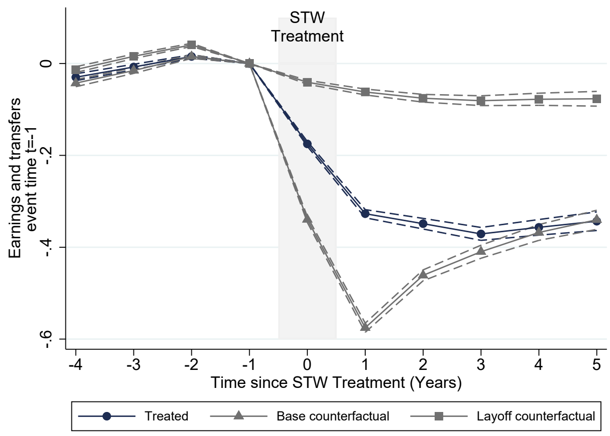
Figure 10: DYNAMIC EFFECTS OF SHORT TIME WORK ON WORKERS' OUTCOMES BY FIRMS' PRE-CRISIS LEVEL OF LABOR PRODUCTIVITY

A. Low Labor Productivity Firms

Employment

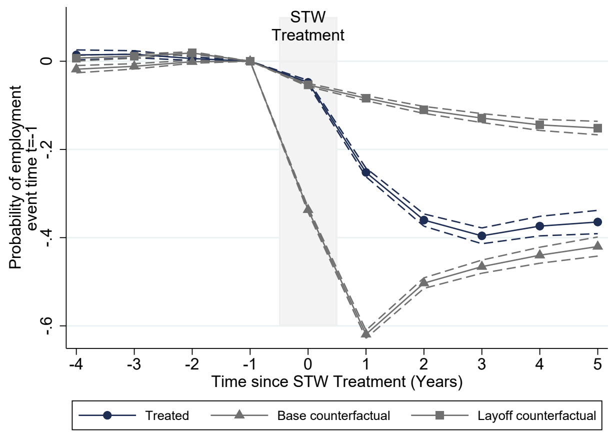


Earnings + Transfers

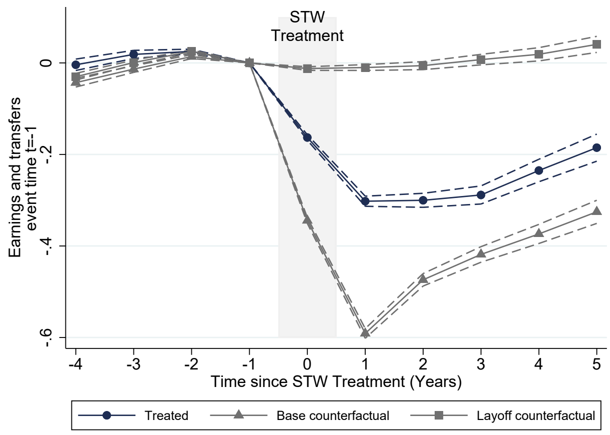


B. High Labor Productivity Firms

Employment

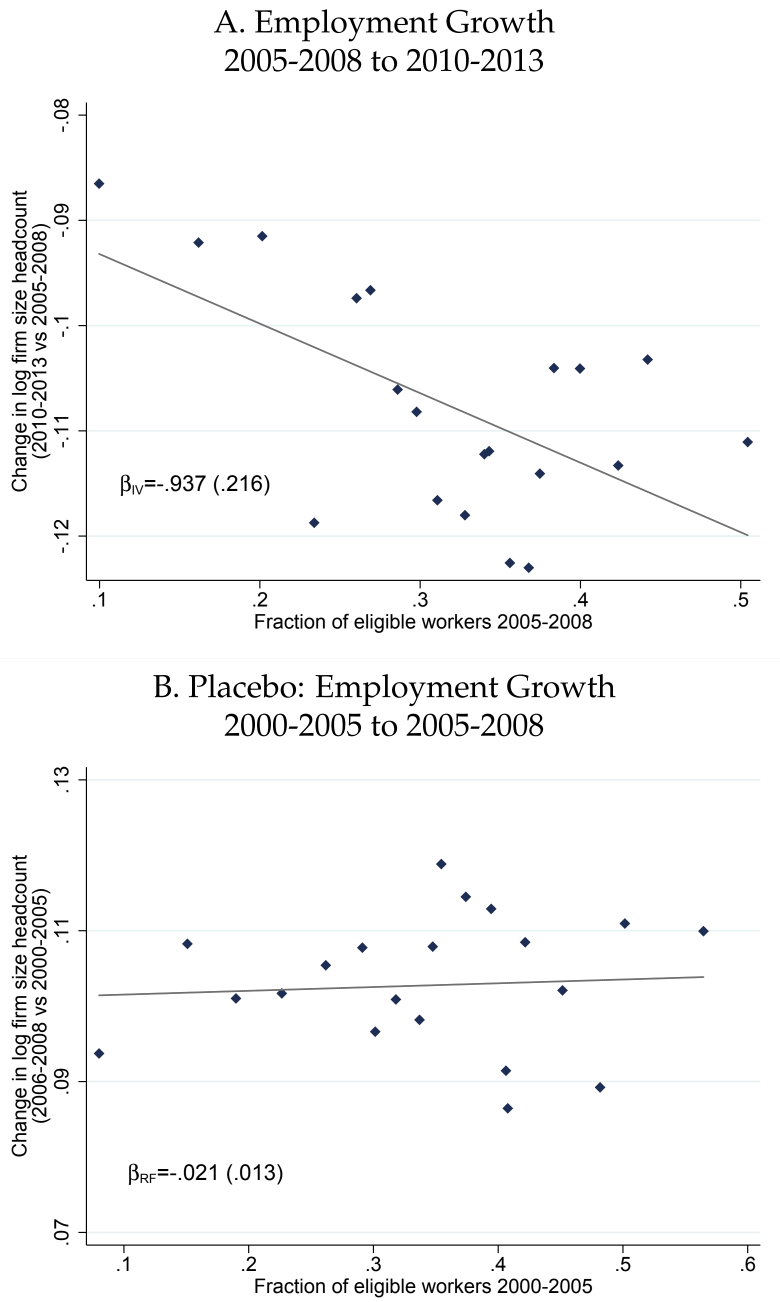


Earnings + Transfers



Notes: The graphs report the estimated coefficients of event study regressions for different outcomes and different event-year definitions at the worker level. The estimation and event-year definitions (STW treatment, base counterfactual and layoff counterfactual) are constructed in the same way as those in Figure 9. In these graphs, we split the sample of workers according to the average level of labor productivity of the firm that the worker is in event year $t = -1$ – the average being taken over event-time years $t = -4, ..., -1$. Panel A shows results for workers, who, at event time $t = -1$, were employed by firms in the bottom half of the distribution of labor productivity. Panel B instead shows results for workers, who, at event time $t = -1$, were employed by firms in the top half of the distribution of labor productivity. Labor productivity is defined as value added per hour worked.

Figure 11: REALLOCATION EFFECTS: EMPLOYMENT GROWTH IN NON-ELIGIBLE FIRMS AS A FUNCTION OF SHORT TIME WORK ELIGIBILITY IN THE LOCAL LABOR MARKET



Notes: The graphs show binned scatterplots of the reduced form of equation (7). Panel A plots the reduced form relationship between the change in average log firm size headcount of firms non-eligible to STW in a local labor market (LLM) between 2005-2008 and 2010-2013, and the fraction of eligible workers in 2005-2008 in the LLM based on the interaction between firm size and INPS codes. Both variables are residualized on firm-level and LLM-level controls. Panel A also reports the $\hat{\beta}_{IV}$ coefficient from equation (7) and its associated robust standard error clustered at the LLM level. Panel B is constructed in the same way as Panel A and shows the placebo relationship between the change in average log firm size headcount of firms non-eligible to STW in a LLM between 2000-2005 and 2005-2008, and the fraction of eligible workers in 2000-2005 in the LLM. Panel B also reports the reduced-form $\hat{\beta}_{RF}$ coefficient from equation (7) and its associated robust standard error clustered at the LLM level.

Table 1: EFFECTS OF STW TREATMENT ON FIRMS' AND WORKERS' OUTCOMES

	Estimate (1)	Std Error (2)	N (3)
	A. First Stage		
Probability of CIGS take-up	.05	(.002)	3029855
	B. Employment Outcomes (IV)		
Log number of hours per employee	-.511	(.036)	2843205
Log number of full-time weeks per employee	-.461	(.034)	2843205
Log firm size (headcount)	.382	(.036)	2843205
Log wage rate	.032	(.028)	2843205
Log wage bill per employee	-.556	(.046)	2843205
Log number of open-ended contracts	.432	(.047)	2843205
Log number of fixed-term contracts	-.367	(0.128)	2843205
Rate of inflows	0.081	(0.599)	2843205
Rate of outflows	-0.337	(0.027)	2843205
Firm survival probability (in $t+1$)	0.104	(0.038)	2843205
	C. Balance-Sheet & Productivity Outcomes (IV)		
Firm value added	.095	(.159)	873839
Value added per worker	-.508	(.120)	873839
Tangible investment	-.003	(.672)	873839
Liquidity	.939	(.461)	873839

Notes: Panel A reports the estimates of the coefficient $\widehat{\kappa}_1$ from specification (3) and its associated cluster-robust standard error in parenthesis. Panels B and C report the $\widehat{\beta}_{IV}$ coefficients estimated from equation (2) and their associated cluster-robust standard errors in parenthesis for a set of different firm-level outcomes. The wage rate is defined as total earnings per hours worked per employee. For survival probability, the reported coefficient is the IV estimate scaled by average survival probability in $t+1$: $\widehat{\beta}_{IV}/\bar{Y}$. Value added is defined as total revenues plus unsold stocks minus cost of goods and services used in production, or equivalently total profits plus total capital depreciation and total wage costs. Liquidity is defined as cash and cash equivalents.

Table 2: ROBUSTNESS OF BASELINE EFFECTS

	"Doughnut" Regression	Only $\leq$ 15 FTE	Only >15 FTE	Permutation Test	No Dismissal Rule Change	
					>60FTE	50FTE
		(Placebo)		(Placebo)	Across Italy	Threshold
	(1)	(2)	(3)	(4)	(5)	(6)
			A. First Stage			
Probability of CIGS take-up	.053	.002	.051	.000	.055	.041
	(.002)	(.000)	(.002)	(.000)	(.005)	(.004)
			B. Outcomes			
	IV	**RF**	**IV**	**RF**	**IV**	**IV**
Log hours per worker	-.449	-.011	-.602	.000	-.670	-.156
	(.037)	(.020)	(.081)	(.010)	(.230)	(.132)
Log employment	.284	-.020	.306	-.001	.848	.338
	(.032)	(.030)	(.099)	(.009)	(.297)	(.258)
Log wage bill	-.544	-.026	-.498	.000	-.568	-.390
	(.049)	(.030)	(.155)	(.013)	(.297)	(.709)
N	2686140	2608383	429490	2978239	152753	44793

Notes: The upper panel of the table reports the estimated coefficient $\widehat{\kappa}_1$ from specification (3). Cluster-robust standard errors are reported in parenthesis below each coefficient. The lower panel reports either reduced-form or IV coefficients for different firm-level outcomes. Column 1 reports the coefficients of a doughnut version of specification (2) excluding firms with 6-month average FTE size $\in (12, 18]$. Column 2 reports the reduced-form coefficient $\widehat{\alpha}_1$ for specification (4) restricting the sample to firms with 6-month average FTE size $\in (5, 15]$. Column 3 reports the IV coefficients for specification (4) restricting the sample to firms with 6-month average FTE size $\in (15, 25]$ and instrumenting STW take-up with $\mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[t \geq 2009]$. Column 4 reports reduced-form coefficients for a placebo-version of specification (2) in which the sample is restricted to firms with non-eligible INPS codes and placebo "eligibility" status is assigned to a randomly chosen subgroup of INPS codes. Column 5 reports the estimated IV coefficients for specification (2) for a sample of establishments with 6-month FTE size $\in (0, 40]$ that belong to multi-establishment firms with FTE size > 60. For this group of firms, employment protection legislation does not apply differentially for firms above and below the 15 size threshold. Column 6 reports the estimated IV coefficients for specification (2) for a sample of firms with INPS codes in the retail sectors and with 6-month FTE size $\in (25, 75]$. For this small group of firms, the size threshold that determines eligibility is set at 50 and employment protection legislation does not apply differentially above and below the threshold.

Table 3: EQUILIBRIUM EFFECTS OF SHORT TIME WORK ON NON-TREATED FIRMS' OUTCOMES

	Reallocation Effects			Placebo Estimates		
	IV	IV	IV	RF	RF	RF
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Employment Spillovers on Non-Eligible Firms					
Log employment	-0.492	-0.918	-0.937	-0.021	-0.021	-0.021
	(0.137)	(0.216)	(0.216)	(0.013)	(0.013)	(0.013)
Log inflows	-3.594	-4.406	-3.176	-0.047	-0.046	-0.030
	(1.947)	(2.380)	(1.440)	(0.112)	(0.113)	(0.107)
LLM controls		×	×		×	×
Firm-level controls			×			×
N		3023166			2784567	
	B. Labor Market Effects on Productivity					
Log TFP	-2.307	-2.093		-0.161	-0.161	
	(0.593)	(0.606)		(0.129)	(0.129)	
LLM controls		×			×	
N		1222			1222	

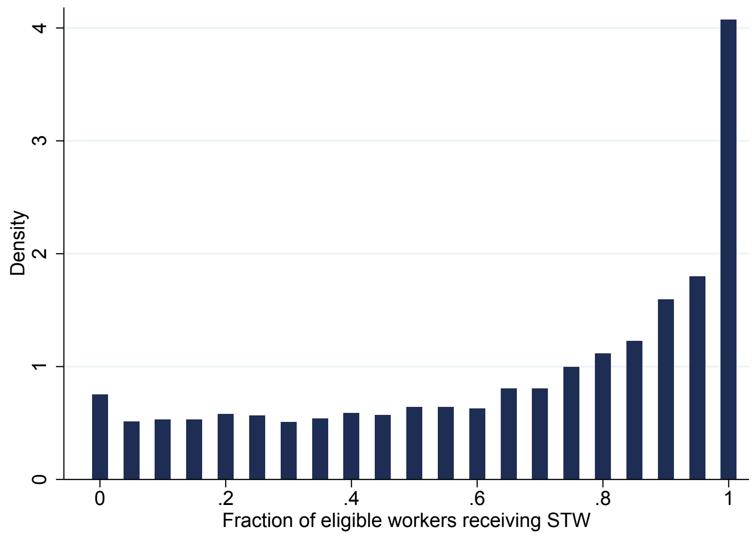
Notes: Columns 1-3 of the table report the $\widehat{\beta}^{R}_{IV}$ estimated from equation (7) and its associated robust standard errors clustered at the LLM level in parenthesis. Columns 4-6 report reduced-form placebo estimates of equation (7) comparing outcome growth during a placebo pre-recession periods (2000-2005) vs (2005-2008). LLM controls include the unemployment rate and the industrial composition of employment (employment shares by industry) in the LLM in the pre-recession period. Firm-level controls are a dummy for STW take-up, firm size in 2008 (2005 for columns 4-6), a dummy for whether the firm ever has an eligible codice autorizzazione and 5-digit industry dummies. In Panel B, we estimate IV model similar to (7) but where the outcome is long differences of TFP, at the LLM level. We define TFP as $\text{TFP} = \text{VA}/(L^{\alpha}K^{\beta})$, where we aggregate all variables (VA, L and K) at the LLM level.

Appendix A: Additional Figures & Tables

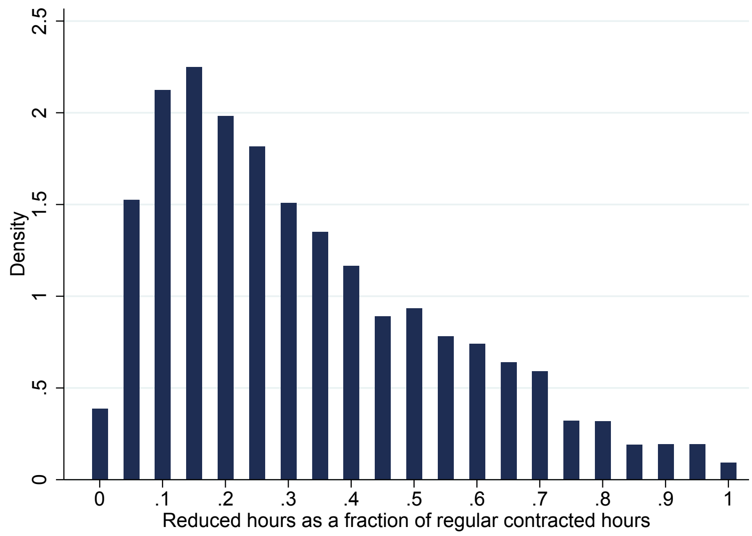
A.1 Descriptive Statistics

Figure A-1: DISTRIBUTION OF SHORT TIME WORK TREATMENT ACROSS WORKERS IN FIRMS EXPERIENCING SHORT TIME WORK

A. Distribution of Fraction of Eligible Workers on STW in Treated Firms



B. Distribution of Reported Weekly Hour Reductions across Treated Workers



Notes: The figure reports descriptive statistics on the distribution of treatment across workers in firms experiencing STW. Panel A plots the distribution of the ratio of treated workers to eligible workers in firms currently using STW. Panel B reports the distribution of reported weekly hour reductions for workers on STW, that is hours on STW out of regular contacted weekly hours. The latter are assumed to be 40 for full-time workers, and 40 times the share of part-time for part-time workers (as reported in the INPS data). The mode is around .25 and the average around .35.

Table A-1: DISTRIBUTION OF FIRMS' CHARACTERISTICS IN THE MAIN SAMPLE BY ELIGIBLE AND NON-ELIGIBLE INPS CODES (2008)

	(1)		(2)		(3)	
	All INPS Codes		Eligible INPS Codes		Non-Eligible INPS Codes	
	Mean	SD	Mean	SD	Mean	SD
Employees (headcount)	8.72	5.16	9.78	5.55	8.22	4.90
Employees (FTE)	8.04	4.78	9.35	5.38	7.42	4.33
Employees on open-ended contracts	7.80	4.91	8.96	5.35	7.25	4.60
Employees on fixed-term contracts	0.92	2.11	0.81	1.78	0.98	2.25
Annual hours worked per employee	2015.26	1008.70	2043.69	980.97	2001.86	1021.24
Annual wage bill per employee (000)	20.66	12.38	22.49	13.22	19.80	11.86
Net revenue per week worked (000)	6.22	49.55	5.94	52.77	6.48	46.31
Value added per week worked (000)	1.11	11.36	1.22	14.41	1.01	7.42
Liquidity	0.11	0.14	0.09	0.13	0.12	0.15
Investment in tangibles	0.07	0.11	0.07	0.10	0.07	0.11
Investment in intangibles	0.02	0.05	0.01	0.04	0.02	0.06
North-West	0.29	0.46	0.30	0.46	0.29	0.46
North-East	0.25	0.43	0.20	0.40	0.27	0.44
Center	0.21	0.40	0.20	0.40	0.21	0.41
South	0.25	0.43	0.30	0.46	0.23	0.42
Observations	321580		102757		218823	

Notes: The table reports the mean and standard deviation of a set of firm-level variables for firms in our sample as of 2008. The summary statistics refer to year 2008. Column 1 refers to both firms with eligible and non-eligible INPS codes. Column 2 restricts the sample to firms with eligible codes and column 3 to firms with non-eligible codes. Revenue, value-added, liquidity and investments come from the CERVED data which covers approximately 50% of firms in our sample. Value added is defined as total revenues plus unsold stocks minus cost of goods and services used in production, or equivalently total profits plus total capital depreciation and total wage costs. Liquidity is defined as cash and cash equivalents. All monetary figures are expressed in 2008 Euros. North-West, North-East, Center and South are dummies for the geographic region of location of the firm within Italy.

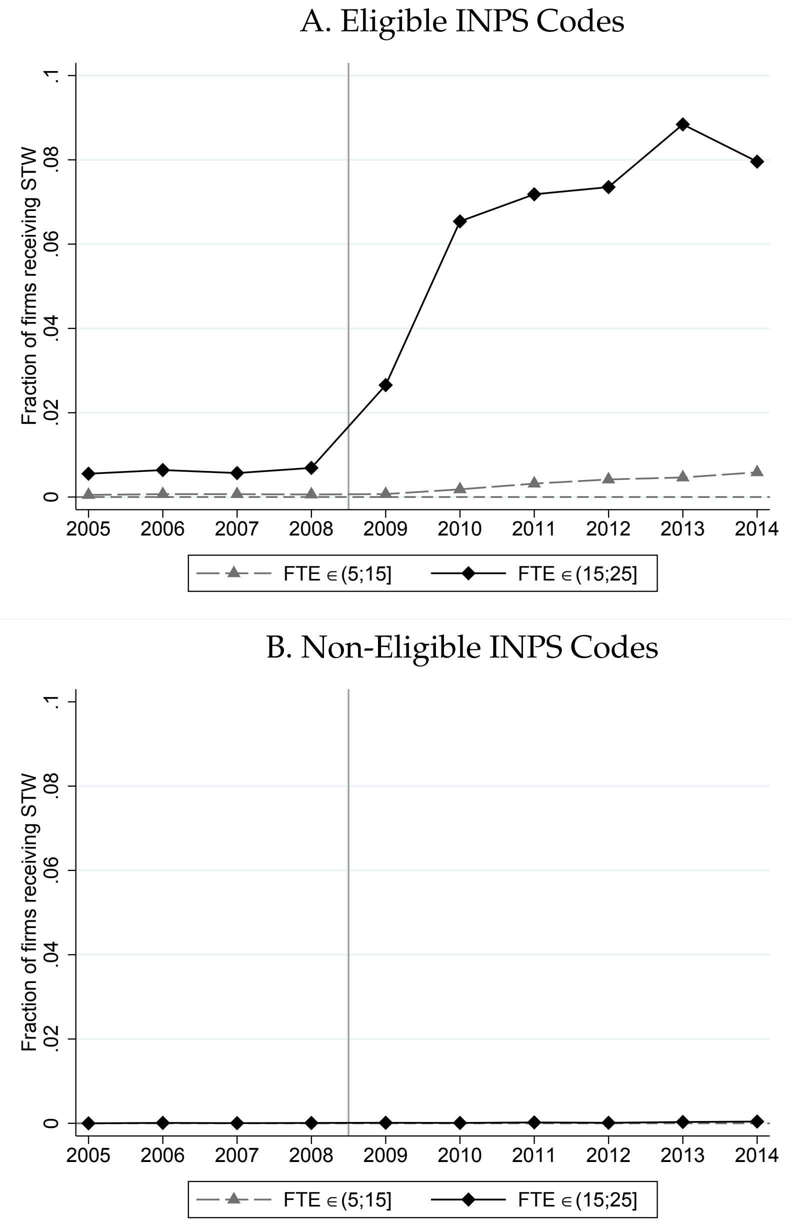
Table A-2: DISTRIBUTION OF WORKERS' CHARACTERISTICS IN THE MAIN SAMPLE BY ELIGIBLE AND NON-ELIGIBLE INPS CODES (2008)

	(1)		(2)		(3)	
	All INPS Codes		Eligible INPS Codes		Non-Eligible INPS Codes	
	Mean	SD	Mean	SD	Mean	SD
Proportion female	0.38	0.48	0.24	0.43	0.45	0.50
Age	36.89	10.72	38.53	10.51	36.04	10.72
Proportion aged <40	0.57	0.49	0.51	0.50	0.60	0.49
Proportion aged 40-54	0.35	0.48	0.40	0.49	0.33	0.47
Proportion aged 55+	0.08	0.26	0.09	0.29	0.07	0.25
Experience (years)	14.23	10.58	16.04	10.81	13.30	10.34
Tenure (months)	59.49	71.52	66.72	76.83	55.75	68.31
Prop. on full-time contract	0.82	0.38	0.90	0.30	0.78	0.42
Prop. on open-ended contract	0.83	0.37	0.88	0.32	0.81	0.40
Prop. on fixed-term contract	0.15	0.36	0.12	0.32	0.17	0.38
Prop. on seasonal contract	0.02	0.13	0.00	0.05	0.02	0.15
Proportion blue collar	0.64	0.48	0.69	0.46	0.61	0.49
Proportion white collar	0.27	0.44	0.24	0.43	0.28	0.45
Proportion manager	0.00	0.05	0.00	0.06	0.00	0.05
Proportion apprentice	0.07	0.26	0.05	0.22	0.09	0.28
Proportion native born	0.84	0.36	0.85	0.36	0.84	0.37
Observations	3350203		1140981		2209222	

Notes: The table reports the mean and standard deviation of a set of worker-level variables for workers who are employed at firms in our sample at some point during year 2008. The summary statistics refer to year 2008. Column 1 refers to workers in both firms with eligible and non-eligible INPS codes. Column 2 restricts the sample to workers in firms with eligible codes and column 3 to workers in firms with non-eligible codes.

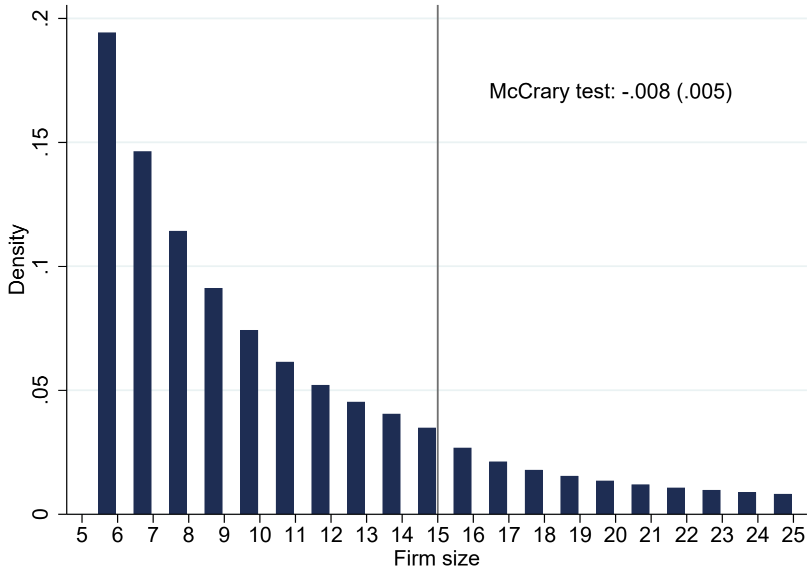
A.2 Identification & Robustness: Additional Evidence

Figure A-2: FRACTION OF FIRMS RECEIVING SHORT TIME WORK BY FIRM SIZE & INPS CODE



Notes: The graphs show the fraction of firms receiving STW in each calendar year $t \in [2005, 2014]$ by eligibility status and maximum 6-month average FTE firm size in year $t-1$. Panel A plots, among firms with eligible INPS codes in our sample, the evolution of the fraction of firms receiving STW in each calendar year t from 2005 to 2014, for firms with a maximum 6-month average FTE size $\in (15, 25]$ in year $t-1$ and for firms with a maximum 6-month average FTE size $\in (5, 15]$ in year $t-1$. Panel B replicates Panel A for firms in non-eligible INPS codes.

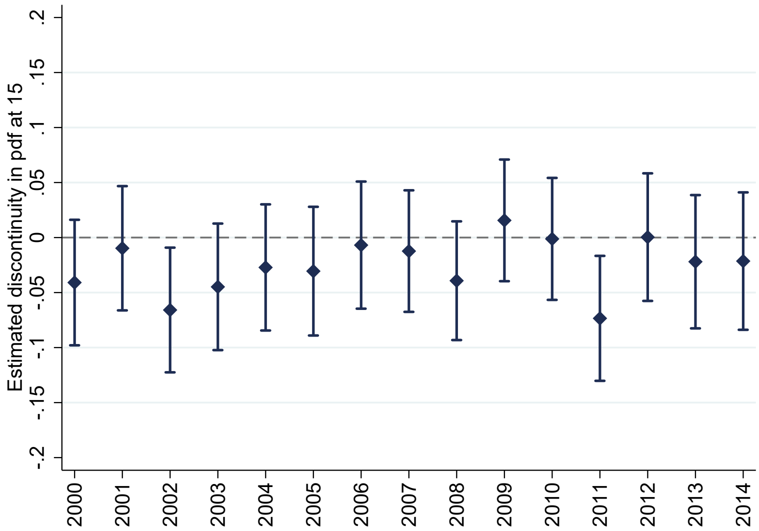
Figure A-3: DISTRIBUTION OF FIRMS' FTE SIZE (2000-2015)



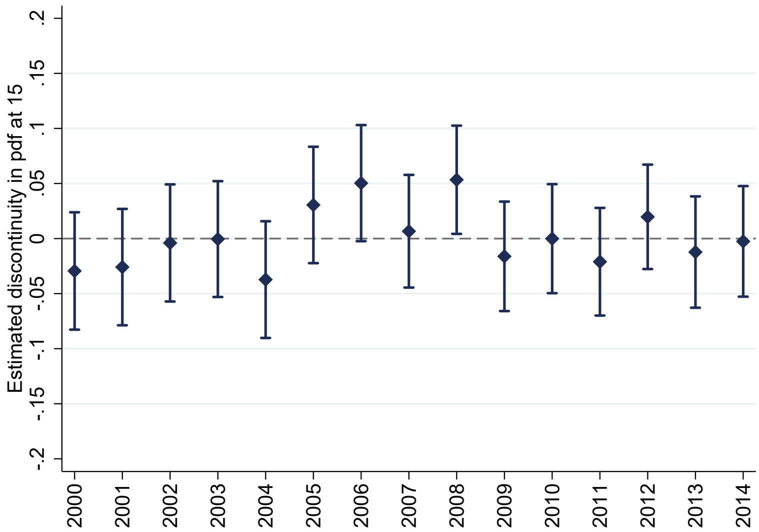
Notes: The graph shows the probability density function of FTE firm size by 1-unit bins for the years 2000-2015. The graph also reports the McCrary test statistic for the presence of a discontinuity in the probability density function of FTE size at 15 and its standard error. FTE firm size is defined as the full-time equivalent of all employees in the firm, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units.

Figure A-4: McCrary Test Statistic of Discontinuity in Firm Size Distribution

A. Eligible INPS Codes

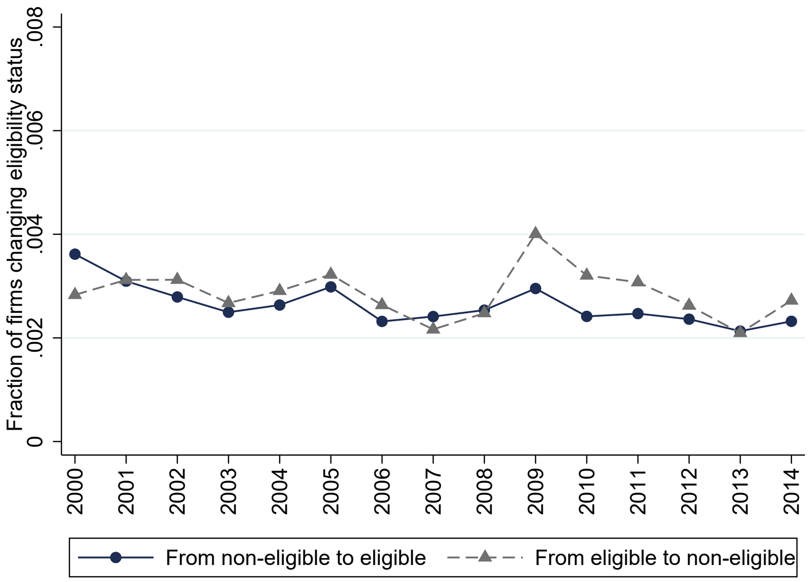


B. Non-Eligible INPS Codes



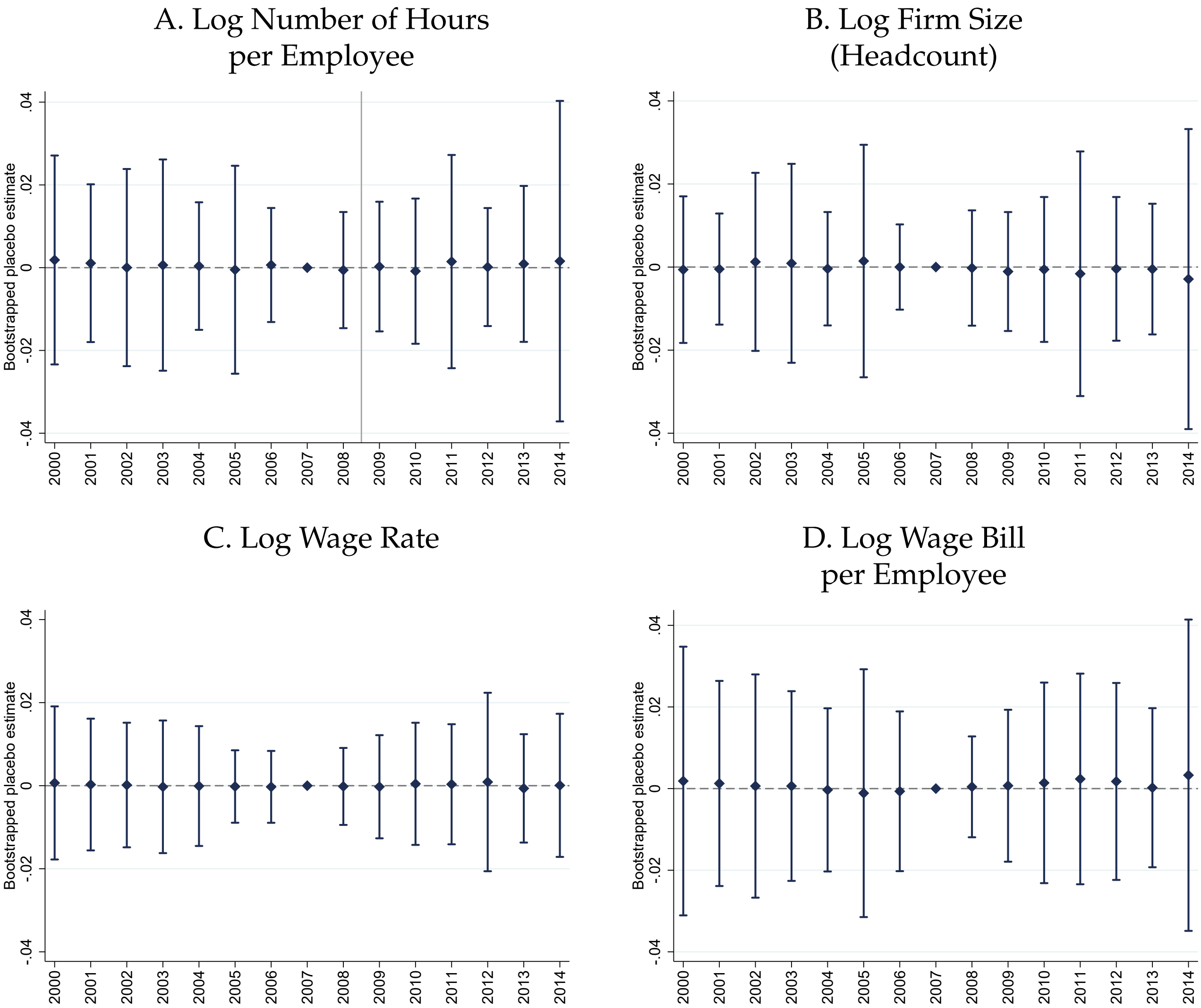
Notes: The graphs report the McCrary test statistic for the presence of a discontinuity in the probability density function of FTE size at 15 and its confidence interval for each year $t \in [2000, 2014]$, and for eligible and non-eligible INPS codes separately. The vertical bars indicate 95% confidence intervals. FTE firm size is defined as the full-time equivalent of all employees in the firm, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units.

Figure A-5: FRACTION OF FIRMS CHANGING ELIGIBILITY STATUS DUE TO CHANGES IN INPS CODE (2000-2014)



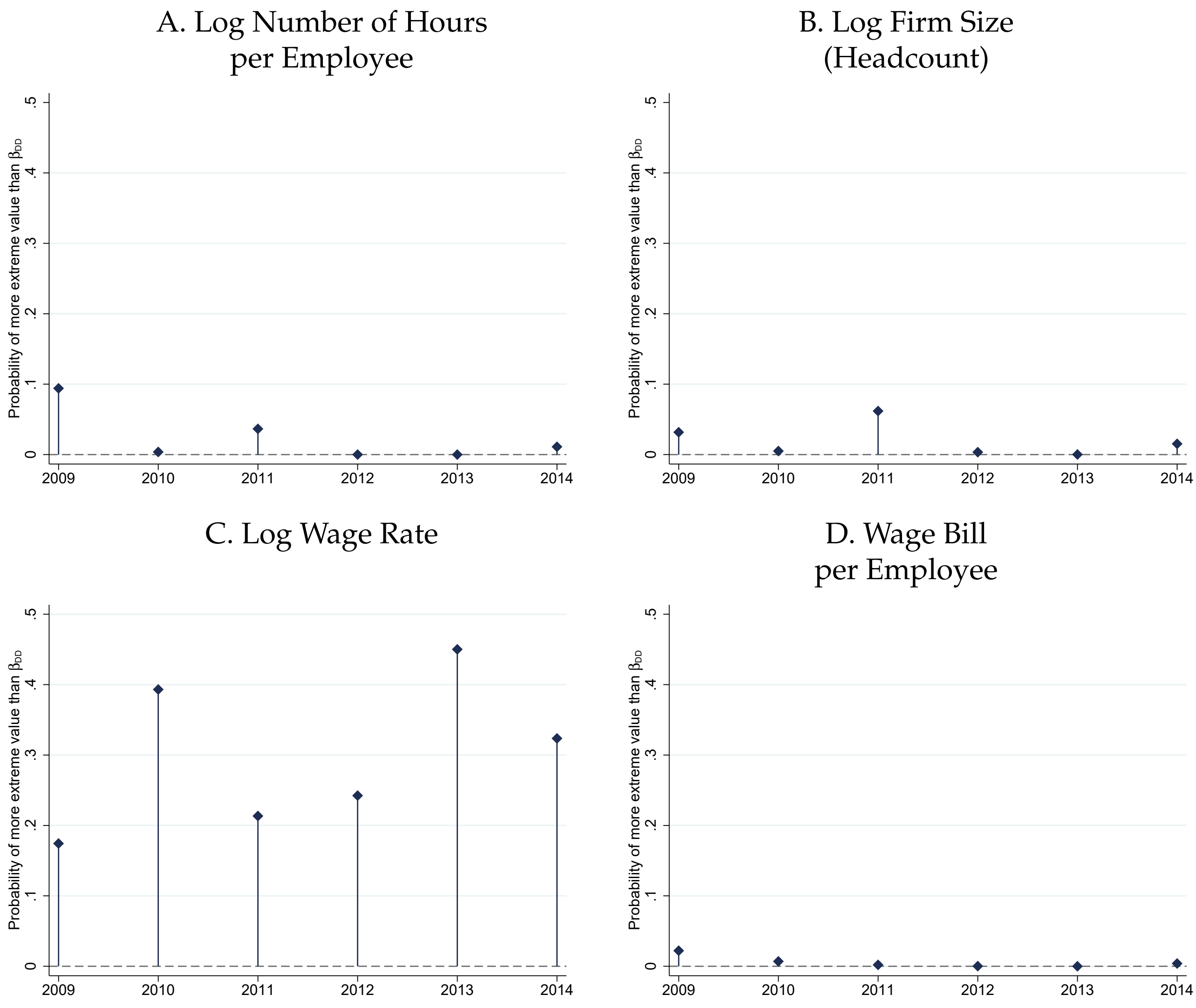
Notes: The graph shows the fraction of firms that change eligibility status due to a change in their INPS code for each year $t \in [2000, 2014]$, and separately for firms changing their status from eligible to non-eligible and vice versa.

Figure A-6: PLACEBO ESTIMATES OF THE EFFECTS OF SHORT TIME WORK ON FIRMS' OUTCOMES



Notes: These graphs show the coefficients $\widehat{\gamma}_1^t$ estimated from a placebo version of equation (1) for all years $t \in [2000, 2014]$ for different firm-level outcomes. Restricting the sample to non-eligible INPS codes, we select a random series of INPS codes to which we assign a placebo "eligible" status. On this sample we run specification (1). The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors from 100 replications of the placebo estimation. The wage rate is defined as total earnings per week worked per employee.

Figure A-7: P-VALUES OF PERMUTATION TEST ON BASELINE ESTIMATES USING BOOTSTRAPPED PLACEBO ESTIMATES

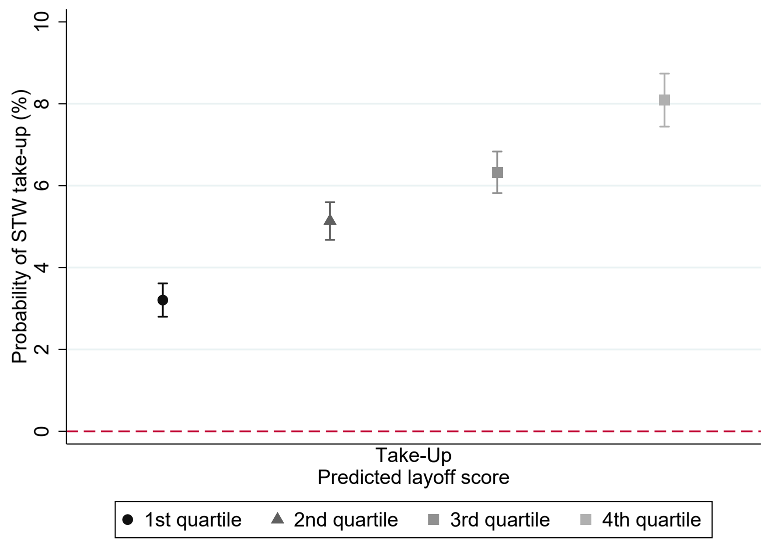


Notes: These graphs report the p-values of a test of equality of the baseline reduced-form estimates of model (1) reported in Figure 4 and the bootstrapped placebo estimates reported in Appendix Figure A-6 for the years 2009 to 2014. The p-values indicate the probability of randomly estimating an effect at least as large as our baseline estimates. The wage rate is defined as total earnings per week worked per employee.

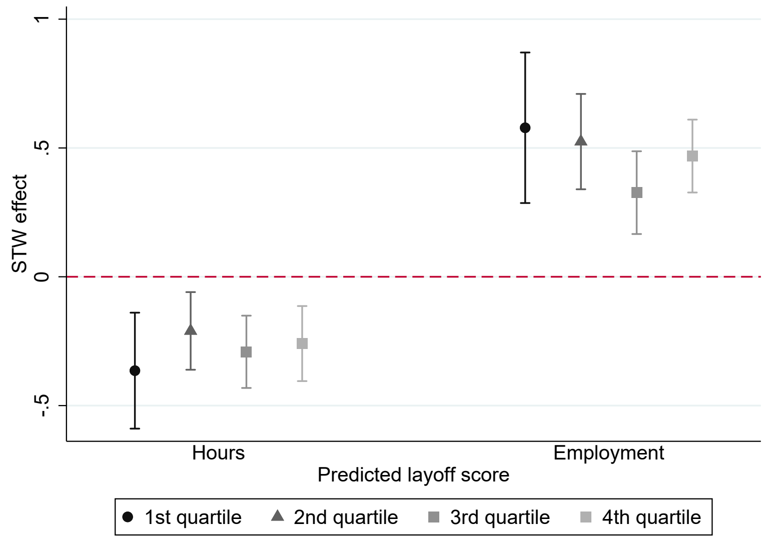
A.3 Targeting: Additional Evidence

Figure A-8: EFFECTS OF STW BY PREDICTED LAYOFF-RISK SCORE

A. Take-Up



B. Hours, Employment and Firm Survival



Notes: The graphs show heterogeneity in STW take-up and treatment effects by a score of the predicted probability that a firm experiences a mass layoff. The prediction model for the probability of mass layoff is described in Section 3.4. Panel A displays the estimated coefficient $\widehat{\kappa}_1$ from specification (3) for the probability of STW take-up for firms at different quartiles of the distribution of the mass-layoff score. We rank firms into the four quartiles of the distribution of this score, and estimate specification (3) on the sample of firms in each quartile. Panel B reports the IV estimates $\widehat{\beta}_{IV}$ from specification (2) for different outcomes, again splitting the sample in the four quartiles of the distribution and estimating the regression separately for each quartile. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level.

Appendix B: Sources of Layoff Inefficiencies - Additional Details

B.1 A Simple Illustrative Model of Labor Hoarding with Liquidity Constraints

A simple way to capture labor hoarding behavior is to have firms choose employment before the realization of productivity shocks. Furthermore, to capture the fact that hoarding labor is valuable, we introduce an upfront cost that the firm has to pay so that its labor is productive at the start of the next period. This cost can be thought of as representing hiring costs in labor markets with frictions, or training costs and specific human capital.

A representative firm enters each period with a level of employment n_t. At the start of each period, the firm draws a productivity level ϵ_t, and chooses its level of employment for the next period n_{t+1} to maximize profits:

$$\max_{n_{t+1}} \Pi_t = S(\epsilon_t, n_t) - C(n_{t+1}) + \mathbb{E}[\Pi_{t+1}|\epsilon_{t+1}, n_{t+1}]$$

$$\text{s.t.} \quad (\lambda) \quad S(\epsilon_t, n_t) - C(n_{t+1}) \geq -\bar{A}$$

where $S(\epsilon_t, n_t)$ is the flow surplus produced by employment n_t for the firm in period t. $C(n_{t+1})$ ($C'(\cdot) \geq 0$, $C''(\cdot) \geq 0$) is the upfront cost that firms have to pay so that its n_{t+1} workers are productive at the beginning of period $t+1$. The firm is subject to a liquidity constraint. The first order conditions of the firm are:

$$(1+\lambda)C'(n_{t+1}) = \frac{d\mathbb{E}[\Pi_{t+1}|\epsilon_{t+1}, n_{t+1}]}{dn_{t+1}}$$

$$S(\epsilon_t, n_t) - C(n_{t+1}) = -\bar{A}$$

We see immediately that when the liquidity constraint binds ($\lambda \neq 0$), the level of employment in $t+1$ is too low compared to the unconstrained setting. The presence of liquidity constraints makes the "labor hoarding" behavior of firms deviate from its optimal level n^*_{t+1}, defined by $C'(n^*_{t+1}) = \frac{d\mathbb{E}[\Pi_{t+1}|\epsilon_{t+1}, n^*_{t+1}]}{dn_{t+1}}$.

Can STW policy reduce the amount of inefficient labor hoarding in this setting? We model the STW policy τ as a policy that decreases hours worked per worker h_t: $dh_t/d\tau < 0$. And we assume that the flow surplus is $S(\epsilon_t, h_t, n_t) = F(\epsilon_t, h_t) \cdot n_t - \omega_t(h_t) \cdot n_t$, where $F(\epsilon_t, h_t)$ is output per worker, which depends on hours worked, and $\omega_t(h_t)$ is labor cost per worker, which also depends on hours worked. When the liquidity constraint binds, the effect of the STW policy on employment n_{t+1} is implicitly defined by:

$$\frac{dn_{t+1}}{d\tau} = -\frac{\frac{dS(\epsilon_t, h_t, n_t)}{d\tau}}{C'(n_{t+1})} \tag{8}$$

In Section 4.1, we focus on the empirical construct $\varepsilon_{n,h} = -\frac{\frac{dn_{t+1}}{d\tau}\frac{1}{n_t}}{\frac{dh_t}{d\tau}\frac{1}{h_t}}$, which is the ratio of the percentage change in employment to the percentage decline in hours in response to the STW policy.

From equation (8) above, we have that:

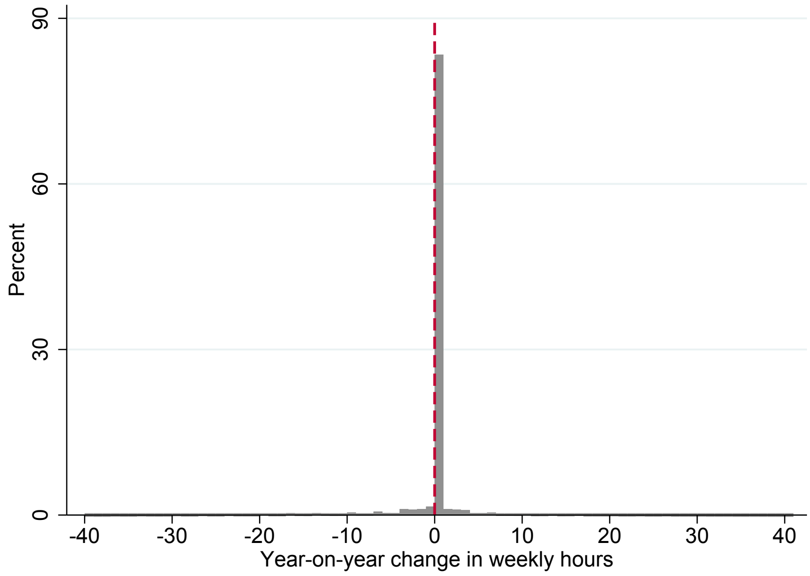
$$\varepsilon_{n,h} = h_t . \frac{d\omega_t(h_t)/dh_t - dF/dh_t}{C'(n_{t+1})}$$

$\varepsilon_{n,h}$ will be positive when a reduction in hours relaxes the liquidity constraint of the firm, which occurs when the marginal cost of an extra hour per worker is larger than its marginal product.

This highlights an important targeting property of STW. As productivity is difficult to observe for the government, policy tools offering liquidity to firms may have trouble screening firms experiencing negative productivity shocks. To the contrary, STW only relaxes the liquidity constraint of firms whose marginal-worker productivity drops below the wage. Firms in which the productivity of the marginal hour worked is higher than its cost have no incentives to reduce hours, and will therefore have no incentives to apply for STW.

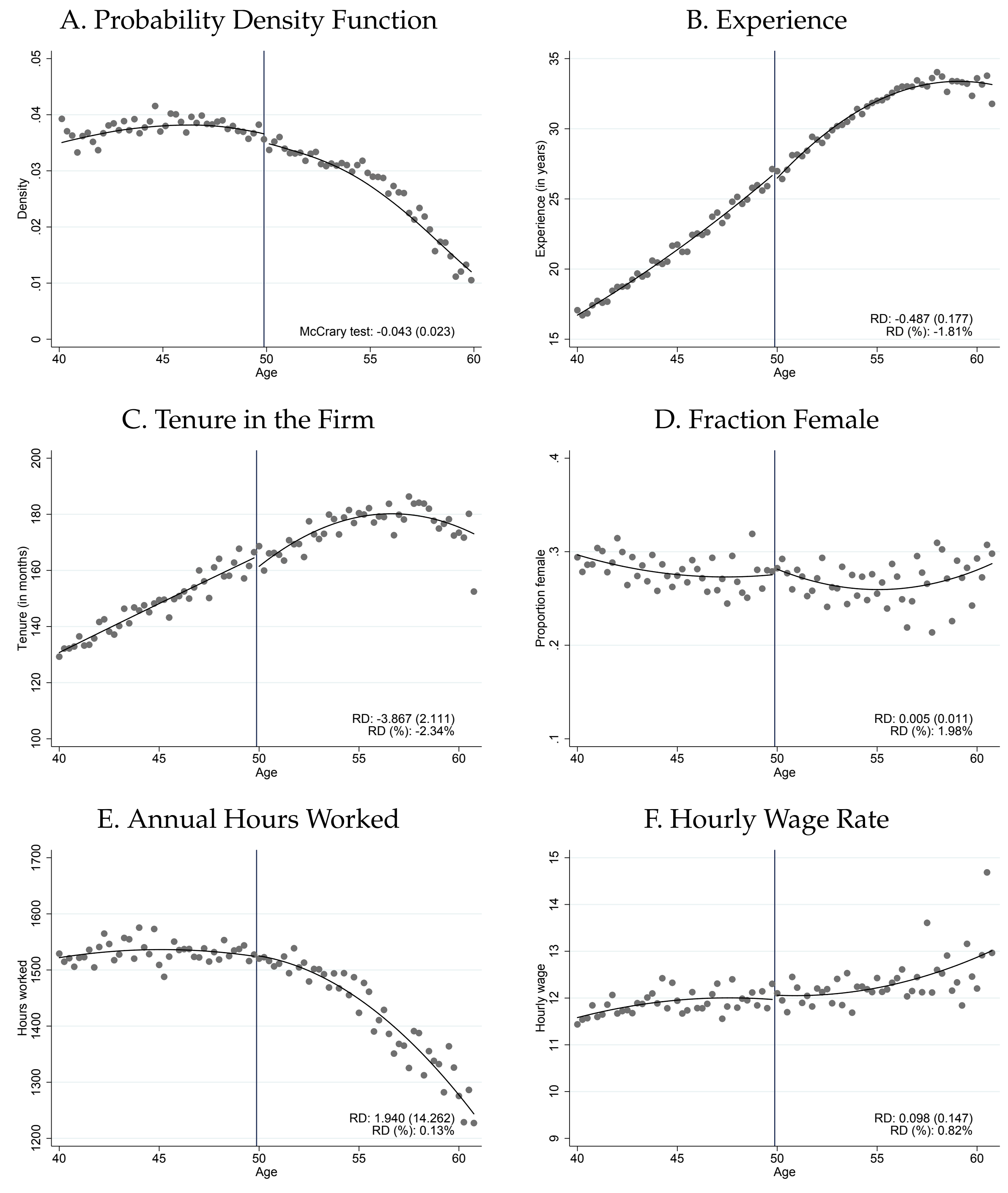
B.2 Bargaining Efficiency

Figure B-1: HOURS RIGIDITY



Notes: The graph reports the empirical density of the distribution of the year-on-year change in average weekly hours worked for the years 2010-2014. Year-on-year changes are binned into bins of 1-unit width. The sample is restricted to workers in firms that are not eligible for STW and who are employed in the same firm over two consecutive years. Weekly hours worked are defined as follows. For full-time workers we divide total annual weeks worked by 52 and assume a 40-hour work week. For part-time workers, we divide total annual weeks worked by 52 and assume a work week of 40 hours times the share of part-time as reported in the administrative data. The red vertical line indicates the lower-bound of bin [0,1).

Figure B-2: VALIDITY OF REGRESSION DISCONTINUITY DESIGN



Notes: The figure reports a set of RD graphs to assess the validity of the RD design in Figure 6. The sample is restricted to workers who meet the eligibility requirements for STW and for UI, and who are in a firm that experiences an episode of STW. Panel A reports the empirical density function of age, measured in months in July of each year and binned into bins of 3-month width. The graph also reports the McCrary test statistic and associated standard error. Panels B-F report a set of RD graphs for various outcomes. For each outcome, the grey dots indicate the average value of the outcome in each age bin. Each graph reports the RD estimate and associated robust standard error, and the RD coefficient rescaled by the mean of the outcome variable in the four quarters to the right of the age-50 cutoff. The solid dark lines display predicted values from a quadratic polynomial fit. All estimates are based on a quadratic polynomial fit conditional on firm, year and firm by year fixed effects. The wage rate is defined as total earnings per hour worked.

B.3 Moral Hazard & Fiscal Externality

In this subsection, we derive the total fiscal externality created by behavioral responses to STW, and provide an estimate of the mark-up that society should be willing to pay on STW expenditures to make the current level of STW subsidy optimal.

There is a unit mass of identical workers in the economy. Workers can be either employed or unemployed. When employed, workers can either work full time or be on STW. Employed workers pay a tax t on their labor income. The government budget constraint can be written as:

$$t \cdot w \cdot h \cdot n + t \cdot w \cdot \bar{h} \cdot (1 - n - u) = b \cdot w \cdot \bar{h} \cdot u + \tau \cdot w \cdot (\bar{h} - h) \cdot n$$

where u is the share of unemployed workers, and b is the replacement rate of the UI system. n is the share of employment on STW and h is the number of hours worked per worker in STW. The level of full-time hours is given by $\bar{h}$. Hours not worked below the full time level in STW firms ($(\bar{h} - h)$) are subsidized at replacement rate τ. The hourly wage rate is w.

Differentiating the government budget constraint with respect to τ, assuming $du/d\tau = -dn/d\tau$, and rescaling by $n \cdot (\bar{h} - h)$, we obtain the fiscal externality for each unit of subsidy:

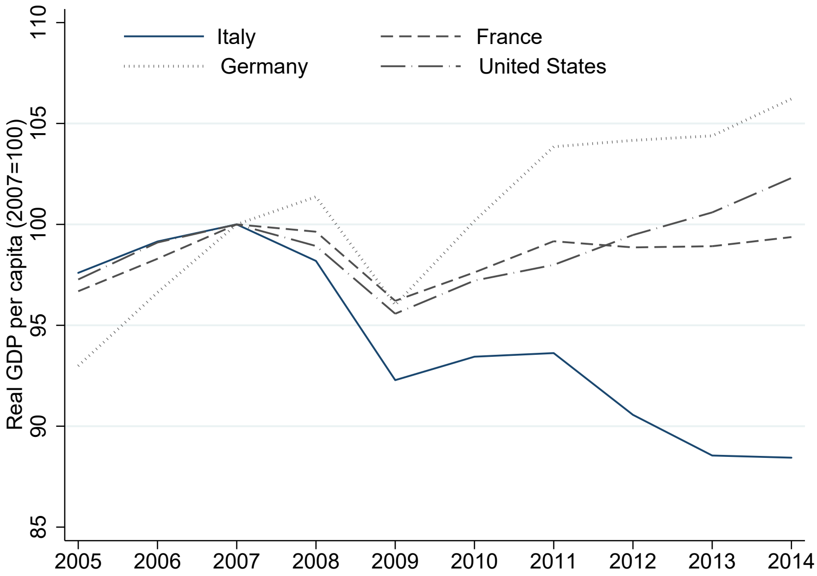
$$FE = 1 + \varepsilon_{n,\tau} \left(1 - \frac{b \cdot \bar{h}}{\tau \cdot (\bar{h} - h)}\right) - \varepsilon_{h,\tau} \cdot \frac{h}{(\bar{h} - h)}$$

where $\varepsilon_{n,\tau}$ is the elasticity of employment to the STW subsidy, and $\varepsilon_{h,\tau}$ is the elasticity of hours to the STW subsidy. Calibrating the value of the fiscal externality using our estimates of the elasticity, a UI replacement rate of 70%, an STW replacement rate of 80% and a ratio of STW hours to full-time hours of 35% as per our results in Panel B of Figure A-1, we obtain a value of the fiscal externality of 1.07.

Appendix C: Dynamic Treatment Effects

C.1 Persistence of the Recessionary Shock

Figure C-1: Evolution of Real GDP per Capita in the Aftermath of the Financial Crisis in Europe and the US



Notes: The graph reports the evolution of real GDP per capita in Italy, France, Germany and the United States. Each series is normalized to 100 in 2007. The data is taken from OECD.

C.2 Recursive Identification of Dynamic Treatment Effects for Firms' Outcomes

To identify the full sequence of dynamic effects of STW treatment, we develop a methodology similar in spirit to the recursive identification of dynamic treatment effects in Cellini Riegg, Ferreira and Rothstein [2010]. We would like to identify the sequence of dynamic treatment effects $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$, which capture the effect of STW treatment on a given outcome in the year of treatment (β_0^{TOT}), one year after treatment (β_1^{TOT}), etc., up to k years after treatment (β_k^{TOT}). We restrict our sample to firms that are active in 2009, and with FTE firm size between 5 and 25 workers in 2008. We create the instrumental variable Z_{2009}, equal to one if a firm is eligible to STW in 2009, that is equal to the triple interaction of being above the 15 FTE firm size threshold in 2008 and being in an eligible INPS code in 2009. We know that this variable will be correlated with the probability of STW treatment in 2009 (T_{2009}), but also with the probability of treatment in 2010 (T_{2010}), in 2011 (T_{2011}), etc. We also know from Appendix Figure C-2 that Z_{2009} is not correlated with treatment in the past (T_{2008}, T_{2007}, etc.). If, on this sample, we now run the following reduced-form of the baseline IV model (2) using Z_{2009} as an instrument:

$$\begin{aligned}
Y_{igst} = \; & \sum_j \beta_j^{RF} \cdot Z_{2009} \cdot \mathbb{1}[j=t] \\
& + \sum_j \sum_k \gamma_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] * \mathbb{1}[j=t] \Big\} \\
& + \sum_j \sum_k \gamma_3^{jk} \cdot \Big\{ \mathbb{1}[k=s] * \mathbb{1}[N_{i,t-1} > 15] * \mathbb{1}[j=t] \Big\} \\
& + \sum_j \sum_k \gamma_4^{jk} \cdot \Big\{ \mathbb{1}[k=s] * \mathbb{1}[j=t] \Big\} \\
& + \sum_k \gamma_5^{k} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] * \mathbb{1}[N_{i,t-1} > 15] \Big\} \\
& + \sum_k \gamma_6^{k} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] \Big\} + v_{igst}
\end{aligned} \tag{9}$$

the estimated reduced-form coefficients for each year 2009, 2010, etc. (β^{RF}_{2009}, β^{RF}_{2010}, etc.) capture the dynamic Intention-To-Treat (ITT) effects from 2009, letting potential future treatment occur. That is:

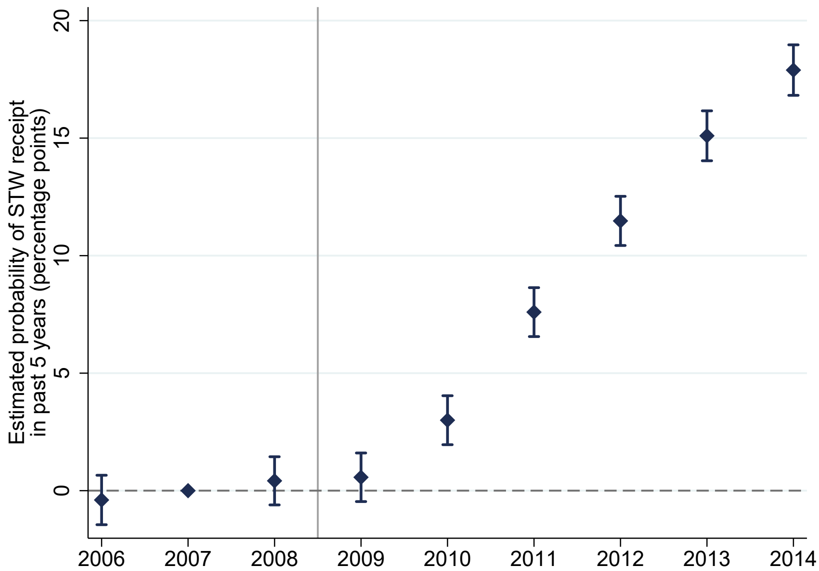
$$\beta^{RF}_{2009} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{10}$$

$$\beta^{RF}_{2010} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{11}$$

The first-stage regressions of T_{igst} on Z_{2009} enable us to identify $\frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}$, $\frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}}$, etc. Using these estimates, the estimates of the ITT effects $\widehat{\beta}^{RF}_t$ and the recursive structure of equations (10), (11), etc., we can identify the sequence of dynamic treatment effects $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$.

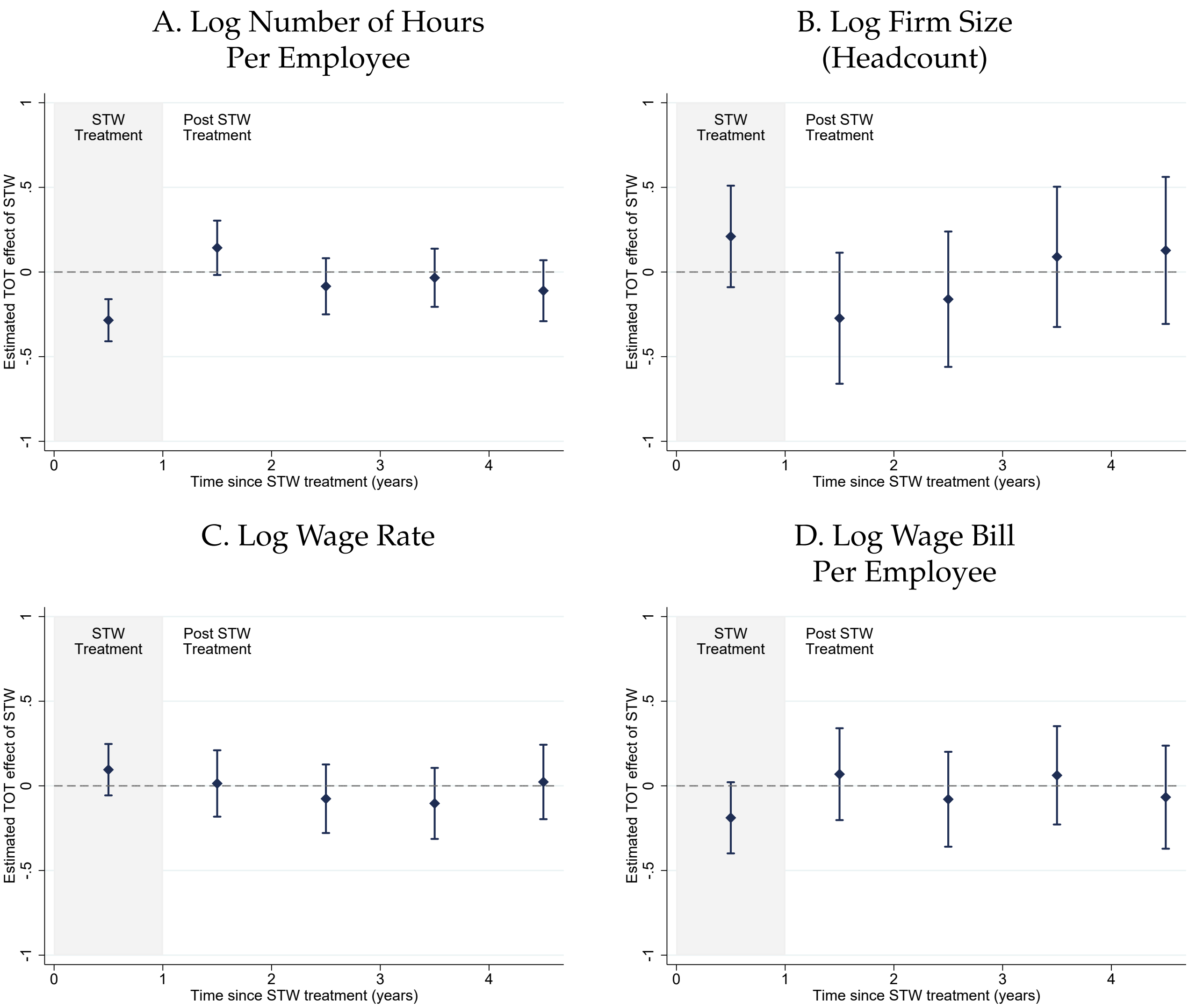
We display in Appendix Figure C-3 the results of these dynamic TOT effects, for various outcomes. The results suggest that the effects are large on impact, but disappear immediately once treatment stops.

Figure C-2: EFFECT OF INPS CODE AND FIRM SIZE INTERACTION ON THE PROBABILITY OF HAVING RECEIVED SHORT TIME WORK IN THE PAST 5 YEARS



Notes: The graph reports the coefficients $\widehat{\gamma}_1^t$ estimated from equation (1) for all years $t \in [2006, 2014]$ using as outcome the firm-level probability of having received STW in the previous five years. The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level.

Figure C-3: TOT ESTIMATES OF THE DYNAMIC EFFECTS OF SHORT TIME WORK



Notes: The graphs report the coefficients $\widehat{\beta}_k^{TOT}$ for $k \in [0, ..., 4]$ for the dynamic effects of STW treatment on various outcomes. These effects are estimated recursively as illustrated in Appendix C.2. The β_k^{TOT} coefficients identify the dynamic treatment effects of STW receipt in year $k = 0$ on outcomes in years $k \in [0, ..., 4]$. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. The wage rate is defined as total earnings per hour worked per employee.

C.3 Event Studies for Worker-Level Outcomes

Identification of Dynamic Treatment Effects. We want to understand to what extent the dynamic patterns from the event studies reveal the causal dynamic impact of STW treatment. Endogeneity concerns prevent interpreting the event study estimates on the treated as the causal dynamic impact of STW. The incidence and timing of CIGS treatment across firms are indeed not random, and workers within these firms may differ from other workers along various characteristics affecting their labor market dynamics. We start by explaining these issues and show two things that can be done to tackle them.

Model. We start by formulating a general statistical model of the dynamics of workers' outcomes:

$$Y_{i,j,t+k} = \eta_i + X_{it}'\alpha_k + \beta_k \mathbb{1}[T_{jt} = 1] + \varepsilon_{j,t+k} + \mu_{i,t+k}$$

where $Y_{i,j,t+k}$ is the outcome of worker i in year $t+k$, given the worker was in firm j at time t. This outcome depends on some observed and unobserved individual characteristics η_i and X_{it}, and on having received STW treatment or not at time t. This outcome also depends on the dynamics of two types of unobserved shocks: firm-level shocks $\varepsilon_{j,t+k}$ and individual level shocks $\mu_{i,t+k}$.

To identify the sequence of dynamic effects of STW β_k, we first need to control for individual fixed effects η_i: this is easily done using individual fixed effect panel models. Second, we need to control for individual level characteristics of workers X, as they may affect dynamics of labor market: this is done creating proper control groups using nearest-neighbor matching.

The next important concern is that firms who select into STW in t are subject to (unobservable) bad shocks in t ($\varepsilon_{j,t}$). Such shocks are possibly quite time persistent, creating a correlation between STW treatment and $\varepsilon_{j,t+k}$. In other words, workers treated by STW will do badly because the firms that trigger STW experience bad shocks. A final issue is the potential correlation between $\mathbb{1}[T_{jt} = 1]$ and $\mu_{i,t+k}$.

A way to address these two concerns is to create counterfactual event studies that put bounds on the values of these firm and individual shocks, and therefore bounds on the treatment effects of STW.

Bounds on Dynamic Treatment Effects Using Counterfactual Event Studies. The idea is to use comparison groups as bounds on the distribution of the unobserved shocks, to bound the causal effect of STW.

Intuitively, treated workers at time t are selected on the basis that the firm in which they are employed experiences a negative (unobservable) shock in t.

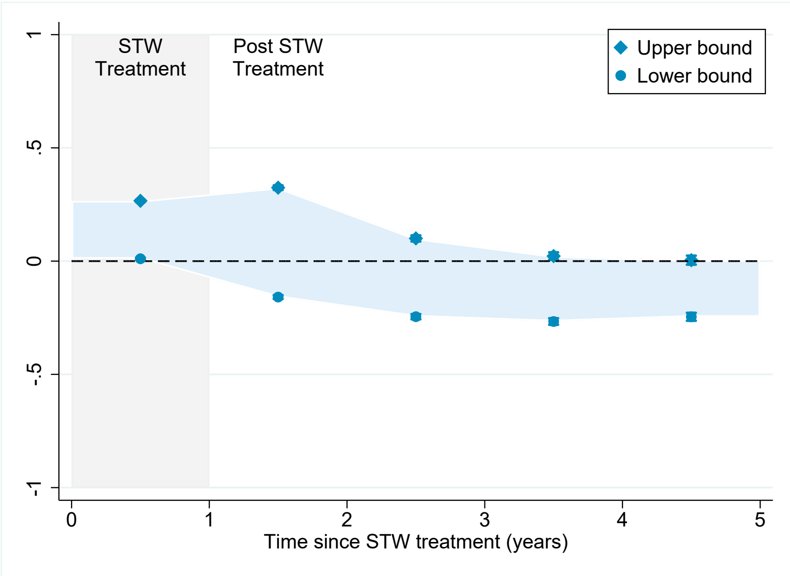
Counterfactual 1: A similar worker at time $t-1$ from any non-eligible firm due to firm size and INPS code. Under the assumption that only the worse shocks select into STW, that is $\mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k}|\text{Counterfact 1}] \geq \mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k}|\text{STW Treated}]$, the outcomes for workers in this first comparison group can be thought of as an upper bound counterfactual for what would have happened to treated workers in the absence of the program. And the difference $\beta_k^T - \beta_k^{C1}$ between the event study estimates for treated workers and workers of this first comparison group provide therefore a lower bound estimate on the dynamic treatment effect of STW.

Counterfactual 2: A similar worker at time $t-1$ from non-eligible firms due to firm size and INPS code, who experiences a layoff in t. If we assume that the shock triggering a layoff is at least as bad as a STW shock and that the firms would have used STW instead if they were eligible, that is $\mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k}|\text{Counterfact 1}] \leq \mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k}|\text{STW Treated}]$, then workers in this layoff comparison group can be thought as a lower bound counterfactual for what would have happened to treated workers absent STW. As we show in Section 3.4, this assumption is credible as not all firms who take up STW would have been laying off workers. In that sense, the layoff comparison group is clearly more negatively selected than our treated group. Under the previous assumption, the difference $\beta_k^T - \beta_k^{C2}$ between the event study estimates for treated workers and workers of this second comparison group provides an upper bound estimate of the effect of STW.

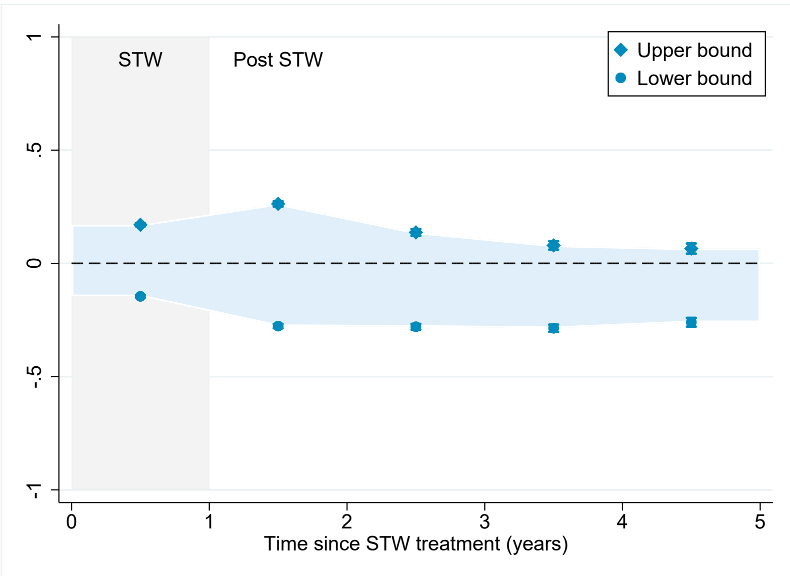
In Appendix Figure C-4, we overlay the upper bound and lower bound estimates from the event study approach. In Panel A, we show the effect for employment, and in Panel B the effect on worker's total gross earnings plus transfers. The graphs show that, in both cases, the upper bound estimate – which compares treated workers to their layoff counterfactual – is positive at the time of treatment (event year 0), but quickly converges to being close to zero, as suggested by the event studies in Figure 9.

Figure C-4: DYNAMIC EFFECTS OF STW ON WORKERS' OUTCOMES

A. Probability of Employment



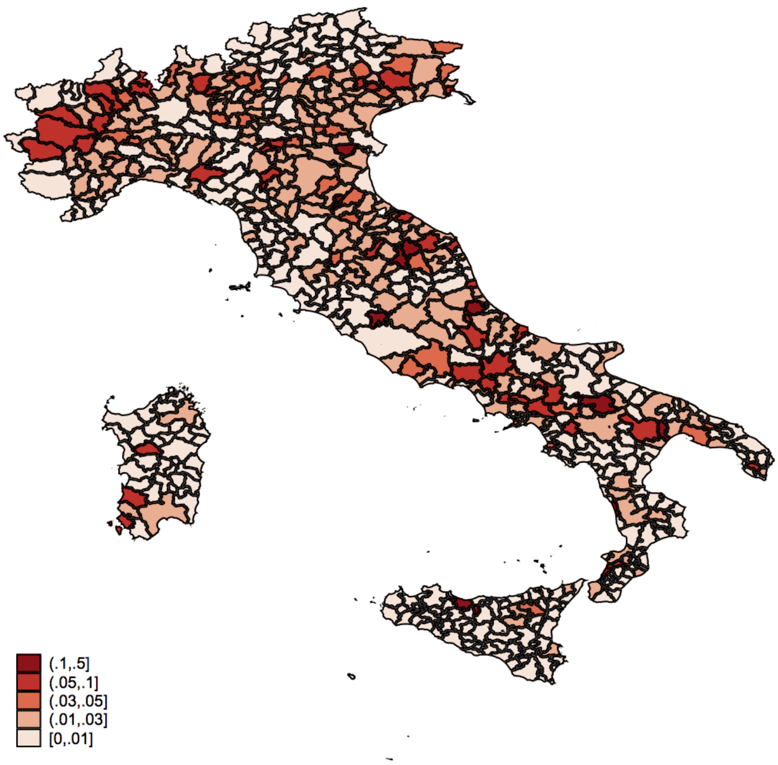
B. Earnings + CIGS/Transfers



Notes: The graphs report bounds on the dynamic treatment effect of STW receipt on workers' employment probability and total earnings including social insurance transfers and STW. The shaded area shows upper- and lower-bound estimates of the dynamic effect, using the event study estimates reported Panel A and C of Figure 9. The upper bound (indicated by diamonds) compares treated individuals with the layoff counterfactual. The lower bound (indicated by circles) compares treated workers with workers in non-eligible firms.

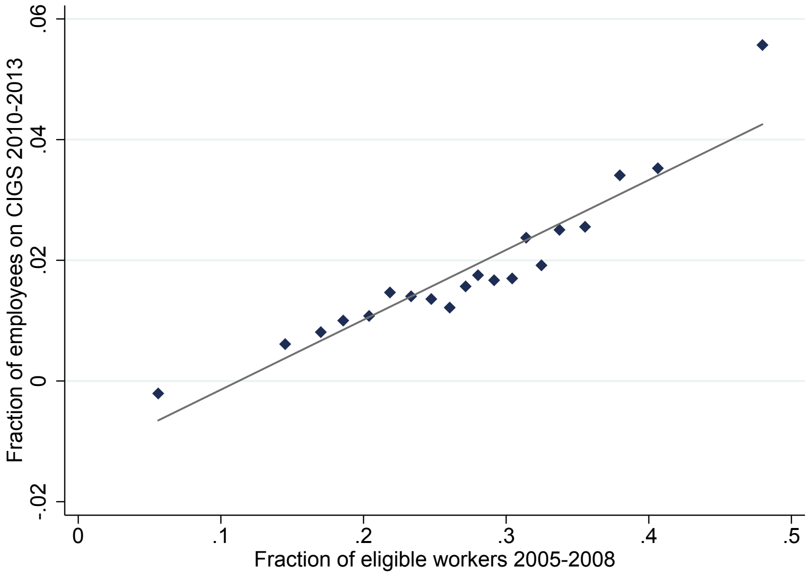
Appendix D: Selection & Spillover Effects - Additional Evidence

Figure D-1: Fraction of Workers Treated by CIGS across Italian Local Labor Markets (2010-2013)



Notes: The graph shows a map of the Italian territory subdivided into 611 local labor markets (LLM), as defined by the Italian Statistical Institute (ISTAT). The graph reports the fraction of workers treated by CIGS in the years 2010 to 2013 in each LLM. The fraction of treated workers is defined as the number of workers with at least one STW spell divided by the total number of employees in the LLM.

Figure D-2: Fraction of Workers Eligible to CIGS in an LLM based on Firm Size and INPS Codes during the Pre-Recession Period vs Fraction of Workers on CIGS during the Recession



Notes: The graph reports a binned scatter plot of the relationship between the fraction of employees on STW in 2010-2013 (y-axis) and the fraction of workers eligible to STW in 2005-2008, based on the interaction between firm size and INPS codes in the LLM (x-axis). Both variables are measured at the LLM level, and are residualized on firm-level and LLM-level controls (see Section 5.4 for details). This relationship corresponds to the first stage of IV model (7).

Appendix E: Model Calibration & Counterfactual Analysis

We develop a matching model of the Italian labor market to calibrate the reallocation effects of STW during the Great Recession, using our reduced-form evidence. There are two types of firms in the model, that differ by their level of productivity. We model the Italian economy in the period 2009-2014 as being in a steady-state. This captures the fact that the recession in Italy was persistent. In this context, we wish to ask quantitatively how the presence of STW for low productivity firms affected equilibrium TFP and the allocation of employment in that steady-state.

The way STW enters the model is that workers in low productivity firms can get a subsidy for hours not worked below a threshold. This endogenously reduces equilibrium hours per worker in low productivity firms, and increases the employment level of these firms. By increasing labor market tightness, this reduces the equilibrium employment of high productivity firms. This captures in a nutshell the logic of the reallocation effects of STW.

The contribution of this calibration is to use our reduced-form evidence to identify the key parameters of the model, and therefore provide a quantitative exploration of the effects of STW. We identify for instance key parameters of the matching function from our quasi-experimental evidence on reallocation. We also identify key parameters of firms' production function from our reduced-form evidence on the causal effects of STW.

This section describes the details of the calibration of the model: the choice of functional form specifications, the calibration of the various parameters using quasi-experimental evidence, the GMM estimation of the parameters that could not be directly calibrated from reduced-form evidence, and the details of the counterfactual exercises.

E.1 Matching in the Labor Market

We consider a unit mass of workers in a frictional labor market. In each period t, u_t unemployed workers meet firms with a vacancy at a rate described by a constant returns to scale matching technology function $M(u_t, v_t)$, increasing and concave in both arguments. We define labor market tightness $\theta_t \equiv \frac{v_t}{u_t}$ as the ratio of vacancies to unemployment, which is, given M, a sufficient statistic for both the vacancy filling probability $q(\theta)$ and the job finding probability $\phi(\theta)$. Each period, a fraction δ of existing employment relationships is destroyed exogenously.

We assume random matching between workers and firms irrespective of their productivity, that is, search is not directed across separate search markets for high and low productivity firms.

Identifying Parameters of the Matching Function from Reduced-Form Evidence. We consider the Cobb-Douglas matching function:

$$M(u_t, v_t) = \mu u_t^{\gamma} v_t^{1-\gamma} \tag{12}$$

The vacancy filling probability $q(\theta)$ is therefore, as above:

$$q(\theta_t) = \frac{M(u_t, v_t)}{v_t} = \mu \left(\frac{u_t}{v_t}\right)^{\gamma} = \mu \theta_t^{-\gamma} \tag{13}$$

Log linearizing the above equation yields:

$$\ln(\frac{M}{v_t}) = \ln(\mu) - \gamma ln(\theta) \tag{14}$$

To obtain information on the measures of hires per vacancy, M/v_t, and labor market tightness at the local labor market level, θ, we use the RIL 2007, 2010 and 2015 surveys from INAPP. Using question C7 (and question C8 for 2015), we can compute $v_{j,t}^{RIL}$ the total number of vacancies (number of individuals the firm seeks to hire) in the RIL data at time t in labor market j.

To scale the vacancies in the RIL data to the whole local labor market level, we use the ratio of total employment of firms in the RIL data at time t in labor market j to total employment at time t in labor market j computed from the INPS administrative data, that is we have:

$$v_{j,t} = \frac{n_{j,t}}{n_{j,t}^{RIL}} \cdot v_{j,t}^{RIL} \tag{15}$$

Once a measure of vacancies $v_{j,t}$ is obtained, this is combined with measures of matches $M_{j,t}$ and of unemployment $u_{j,t}$ to create $q_{j,t}$ and $\theta_{j,t}$. For $M_{j,t}$ we compute the total number of new hires (inflows) in firms of LLM j in year t from the INPS data, and for $u_{j,t}$ we compute the total number of unemployed in LLM j at time t from ISTAT.

We therefore can run the following specification:

$$\log q_{j,t} = a + b \log(\theta_{j,t}) + c_j + \zeta_t + \nu_{j,t} \tag{16}$$

For b to identify $-\gamma$, exogenous variation in $\theta_{j,t}$ is required. We use exposure to CIG treatment as an instrument. Intuitively, the intensity of CIG treatment offers an exogenous shock to labor demand in the LLM as depicted in Panel C of Appendix Figure E-1. This shock allows us to move along the "supply curve" of steady state equality of flows in the labor market, and therefore identify the curvature of the matching function. We use again the interaction between firm size and INPS codes in the pre-recession period as an instrument for the change in the number of unemployed (and therefore for the change in tightness) during the recession. Therefore, we obtain the 2SLS model:

$$\begin{aligned} \Delta \log q_{j,t} &= b\Delta \widehat{\log(\theta_{j,t})} + W_j'\mu_1 + \zeta_t + \nu_{j,t} \\ \Delta \log(\theta_{j,t}) &= Z_j^{2005-2008} + W_j'\mu_0 + \mu_{j,t} \end{aligned} \tag{17}$$

where Δ is the difference operator between pre vs post 2008.[33] Z_j is the average yearly fraction of workers in LLM j that are eligible to STW during the pre-recession period, based on the interaction between their firm size and INPS code in the pre-recession period. W_j is a vector of LLM characteristics that could be correlated with the fraction of treated workers and likely to affect equilibrium labor market outcomes during the recession, such as the industry and firm size composition of the LLM and the initial unemployment rate in the LLM prior to the recession. Identification therefore comes from comparing LLMs with similar characteristics, including firm size composition and industry composition, but with different allocations of workers within firm size times INPS codes bins during the pre-recession period. From this specification, we obtain $\gamma = 0.53$.

E.2 Firms

Firms produce a homogeneous consumption good using labor inputs according to the technology $\epsilon_k F(h_t, n_t)$. Firms differ in terms of their productivity ϵ_k, which can take two levels: ϵ_H for high productivity firms, and ϵ_L for low productivity firms. We consider these two productivity levels as persistent characteristics of firms, to capture the issue of reallocation created by STW in an environment where a recession creates a persistent negative shock for certain firms. The production function depends on the number of employees n and the number of hours worked per employee h.

Firms determine every period the number of vacancies to be posted v_t to maximize profits:

$$\Pi(n_{t-1}) = \max_{v_t}\{\epsilon_k F(h_t, n_t) - wh_t n_t - cv_t + \beta\Pi(n_t)\} \tag{18}$$

[33] Because only three waves of the survey are available (2007, 2010 and 2015), the pre-2008 data is observations for 2007, and post-2008 data is an average of the 2010 and 2015 observations.

subject to the law of motion of employment:

$$n_t = (1-\delta)\cdot n_{t-1} + q(\theta_t)\cdot v_t \tag{19}$$

The first order condition of profit maximization implicitly determines the demand for employment $n_t = n(\theta_t, h_t, w)$ of the firm.

In a stationary equilibrium, $\theta_t = \theta_{t+1} = \theta$, so the first-order condition of the firm reduces to:

$$\epsilon_k F'_n(h_t, n_t) = wh_t + (1-\beta(1-\delta))\frac{c}{q(\theta)} \tag{20}$$

E.2.1 Identifying Production Function Parameters

We assume that the production function of the firm is of the form:

$$F(h_t, n_t) = h_t^{\alpha} n_t^{\eta} \tag{21}$$

We then use our reduced-form evidence to identify the parameters α and η of the production function. Log-linearization of the first order condition of the firm's profit maximization with respect to employment gives:

$$\log n = \frac{\alpha}{1-\eta}\log h - \frac{1}{1-\eta}\log(wh) - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{whq(\theta)} + \frac{1}{1-\eta}\log(\epsilon_k \eta) \tag{22}$$

Letting $\nu_k = \frac{1}{1-\eta}\log(\epsilon_k\eta)$, and re-arranging we obtain:

$$\log n = \frac{\alpha-1}{1-\eta}\log h - \frac{1}{1-\eta}\log w - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{whq(\theta)} + \nu_k \tag{23}$$

A third specification can be obtained through consolidating the whole wage bill as follows: $W = w\bar{h} + (h^{max} - \bar{h})\tau_f w$. Before 2015, the experience rating of the STW program was almost zero: $\tau_f \approx 0$, so $W = wh$. After 2015, the introduction of $\tau_f > 0$ for firms on CIG introduces some exogenous variation in the wage bill.[34] The new specification becomes:

$$\log n = \frac{\alpha}{1-\eta}\log h - \frac{1}{1-\eta}\log W - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{W\cdot q(\theta)} + \nu_k \tag{24}$$

[34]In September 2015, a reform of the Italian *Cassa Integrazione Guadagni* introduced a degree of progressivity in the experience-rating component of STW (D. Lgs. 148/2015). Before the reform, firms using STW had to pay a contribution equivalent to 3% (or 4.5% for firms with more than 50 employees) of the subsidy received by their workers. After the 2015 reform, these rates have been increased to 9% of the wage bill corresponding to hours not worked. The 9% rate applies to the first 52 weeks of subsidy, and is then increased to 12% for the next 52 weeks and to 15% for any additional week.

The previous log-linearization suggests the following estimation model:

$$\log n_{i,j,t} = \gamma_i + \zeta_j + \mu_t + \alpha_1 \log h_{i,j,t} + \alpha_2 \log W_{i,j,t} + \alpha_3 \underbrace{\frac{1}{W_{i,j,t} q(\theta_{j,t})}}_{X_{i,j,t}} + \nu_{i,j,t}$$

where i indexes firms, and j indexes LLMs. Structurally, the coefficients α_1, α_2 and α_3 from this regression identify the key parameters of the demand function. We estimate the previous specification instrumenting the change in hours by STW treatment and the change in the wage bill by the interaction of STW treatment and being after 2015, when the reform introduced some positive experience rating $\tau_f > 0$. Solving for these parameters gives $\alpha = 0.6, \eta = 0.7$.

E.2.2 Firm Productivity

We must define how to interpret productivity in the data. We take low productivity firms as those who are eligible for CIG and who have at least one CIG event after 2009. High productivity firms are those eligible but that do not take up CIG at any point post 2009.

We observe that 13% of firms are treated post 2009 in the baseline DD sample. We thus define the fraction of high productivity firms $\rho = 0.87$. Further, taking the mean (log) total factor productivity of these firms, and normalizing the low productivity value to 1 yields: $\epsilon_L = 1, \epsilon_H = 1.62$.

E.3 Workers

Workers are identical. They value consumption and have disutility in hours worked, according to a general utility function $u(c, h)$, $u'_c > 0, u'_h < 0$. Workers are risk-averse in consumption, $u''_c < 0$, and discount the future at the same rate β as firms do. Since there is no storage technology, agents consume all they earn every period. Workers therefore value insurance against income fluctuations provided by the government, which takes two forms. First, unemployment insurance benefits b (extensive margin insurance) are given to unemployed workers. Second, intensive margin insurance is provided in the form of a STW subsidy of rate τ given against earnings losses for hour reductions below a threshold level $\bar{h}$ for workers in low productivity firms. The total amount of STW benefits for a worker in the program is therefore $b^{STW} = \tau w(\bar{h} - h)$. Both UI and STW benefits are funded by a lump sum tax t levied on all workers.

The value function of a worker when employed by a firm of productivity $\epsilon_k \in \{\epsilon_H, \epsilon_L\}$

is W_k^e:

$$W_k^e = u(c_k, h_k) + \beta(\delta W^u + (1-\delta)W_k^e) \tag{25}$$

In the steady state, a constant proportion of workers are employed by the low vs high productivity firms and, similarly, a constant proportion of vacancies are created by the low productivity firms v_L vs the high productivity firms $1 - v_L$.

The value function of a worker when unemployed is W^u:

$$W^u = u(b, 0) + \beta(\phi(v_L W_L^e + (1-v_L)W_H^e) + (1-\phi)W^u) \tag{26}$$

The continuation value of being employed in a firm of productivity ϵ_k must be at least equal to the value of being unemployed $W_k^e - W^u \geq 0$. The zero surplus condition $W_k^e - W^u = 0$ implicitly defines the reservation values of wage and hours that a worker is willing to accept for any employment relationship. Note that these reservation values will be functions of the UI benefits and STW subsidy. In particular, the lower bound on hours that workers are willing to accept decreases with STW, *ceteris paribus*. In other words, STW relaxes the constraint on offering lower hours contracts.

Calibration of Utility Function. We use the following isoelastic, additively separable utility function:

$$u(c, h) = \frac{c^{1-\sigma_c} - 1}{1 - \sigma_c} - \varphi \frac{h^{1+\sigma_h}}{1 + \sigma_h} \tag{27}$$

where σ_c, the coefficient of risk aversion is set to 2.5. The parameter σ_h can be interpreted as the inverse of the Frisch labor supply elasticity. We set this parameter to $\sigma_h = 3.5$ in line with conventional calibrations from New Keynesian models (see Galí [2011]).

E.4 Wage and Hours Determination

We assume wages are rigid and not bargained over, to be in line with the Italian context which puts institutional constraints on the rebargaining of wages as explained in the main text. We capture the presence of wage rigidity in the data by assuming that the wage has the following form:

$$w(\epsilon) = w_s \epsilon^{w_a} \tag{28}$$

with $w_a < 1$. The wage does not respond to variation in the STW subsidy, nor to variation in hours, consistent with our empirical evidence. The wage responsiveness to firm productivity, w_a, is set to 0.2, in line with similar models in the literature (see Landais, Michaillat and Saez [2018*a*]).

Hours in low productivity firms are obtained by assuming that firms have all the bargaining power in low productivity firms, therefore leaving workers at their outside option. For high productivity firms, to make the model simple and to capture the presence of hours rigidity, we consider a simple exogenous hours schedule:

$$h(\theta, \epsilon) = h_s \epsilon^{h_a} \theta^{h_b} \tag{29}$$

To estimate the parameter h_b – the responsiveness of the hours function to a change in labor market tightness – we regress log hours among ineligible firms at LLM level against log tightness, instrumented by eligibility of CIG. This model obtains a coefficient of 0.14.

E.5 Additional Parameters

E.5.1 Transfer Generosity

The unemployment benefit, b, is set to match the net replacement rate for the average worker in Italy in 2008, which is around 70%. For our purposes, this is 70% of the wage obtained if working the full hours endowment.

The STW replacement rate, τ, is the policy parameter, which is determined by the legal implementation of CIG. This rate is defined as 80% of the total remuneration that would have been paid to the worker for the hours of work not provided, bounded between 0 and the fully contracted time.

E.5.2 Miscellaneous Parameters

The model imposes an exogenous separation rate, δ. To calibrate the separation rate we compute the probability that an individual working in a firm in year t will still be working with the same firm in $t + 1$, accounting for all types of employment contracts. We find an annual separation rate of 0.2. The model's discount factor, β, is set to 0.935, implying an annual interest rate of 7%.

E.6 Summary of Exogenous Parameters

The model is run at yearly frequency. All parameters in the following table are yearly unless otherwise specified.

Parameter	Description	Calibrated value
β	Discount factor	0.935
α	Hour share	0.6
η	Labor share	0.7
γ	Matching function curvature	0.53
w_a	Wage function curvature	0.2
$\bar{h}$	Total weekly hours endowment	40
δ	Separation rate	0.2
b	Unemployment benefit	$0.7 \cdot \bar{h} \cdot w_s$
τ	STW replacement rate	0.8
σ_c	Coefficient of risk aversion	2.5
σ_h	Inverse of Frisch elasticity of labor supply	3.5
ρ	Fraction of high productivity firms	0.87
ϵ	Productivity values	$\{1; 1.62\}$

E.7 Endogenous Parameters & Target Moments

After setting the exogenous parameters, we are left with 5 endogenous parameters:

Parameter	Description
μ	Matching function scaling
c	Vacancy cost
φ	Utility function labor scaling
h_a	Hours schedule productivity curvature
w_s	Wage function scaling

We obtain these parameters through the method of simulated moments, with five target moments:

Target Moments	Value
Unemployment rate	0.108
High productivity weekly hours level	34
Low productivity weekly hours level, without STW	39
Low productivity weekly hours level, with STW	20
Proportion of total employment that is high productivity	0.9

The target unemployment rate is the Italian unemployment rate computed from the ISTAT data. We target the average unemployment rate in the period 2008-2014: 0.108. Low productivity firms are defined as:

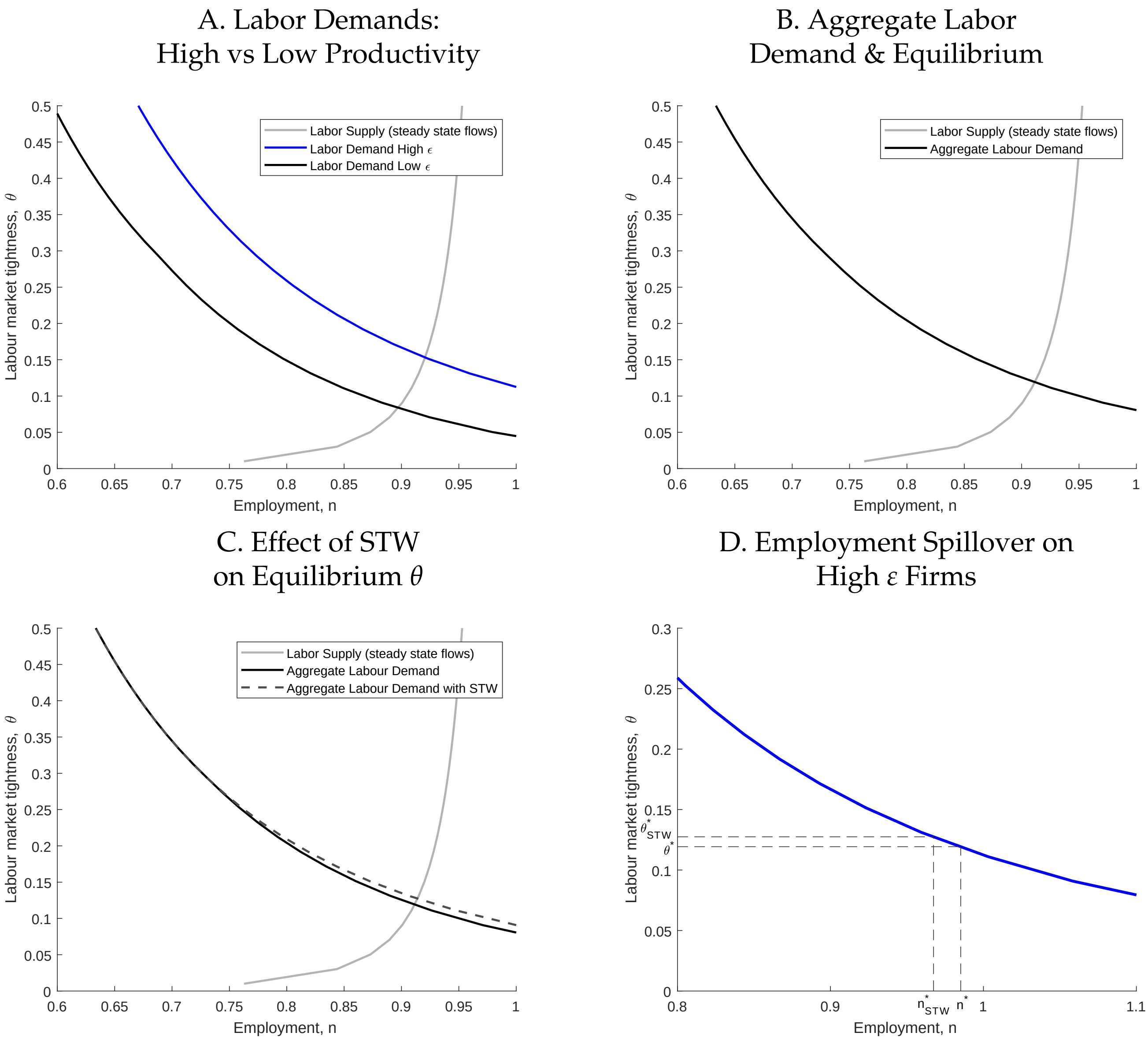
- For eligible firms, those that take up CIG
- For non-eligible firms, in eligible 5-digit industries, firms whose total factor productivity is in the bottom 12% of the distribution, post 2009

E.8 Equilibrium & Spillover Effects

A steady state equilibrium consists in a set of: (i) hours levels h and wage levels w that split the surplus in high and in low productivity firms, subject to the incentive constraint that $W_k^e - W^u \geq 0$; (ii) labor demand functions n^d in high and in low productivity firms that maximize firms' profits and (iii) a labor market tightness θ that clears the labor market, subject to the steady state equality of flows in and out of employment. We borrow the equilibrium representation of Michaillat [2012]. A graphical illustration, using the calibrated version of our model, is presented in Appendix Figure E-1 below.

In this representation, the steady state equality of flows in and out of employment characterizes a labor supply $n^s(\theta, \delta)$, which is an increasing function of θ in the $\{n, \theta\}$ space. The profit maximization of firms determines a labor demand $n^d(\theta)$, which is a decreasing function of θ as the marginal product of n is decreasing (Panel A). With random matching, aggregate labor demand is simply the weighted sum of demands of high and low productivity firms. Equilibrium tightness and equilibrium employment are determined at the intersection of aggregate demand and supply (Panel B). When STW is introduced, labor demand of low productivity firms increases, especially so when tightness is low (and hiring is therefore cheap). This in turn increases aggregate demand and equilibrium tightness (Panel C). This increase in equilibrium tightness is the force driving our observed spillover effects in the data. It makes hiring more costly for all firms and therefore reduces employment of firms not treated by STW. This equilibrium mechanism captures the negative reallocation effects of STW, which distorts employment towards low productivity firms rather than high productivity firms. Again, this effect will be stronger the more horizontal labor demands are – that is, the more linear production technology is in n (Panel D).

Figure E-1: EQUILIBRIUM REPRESENTATION & SPILLOVER EFFECTS OF SHORT TIME WORK



Notes: The figure offers a graphical illustration of labor market equilibrium using the calibrated version of our model. In this representation, the steady state equality of flows in and out of employment characterizes a labor supply $n^s(\theta, \delta)$, which is an increasing function of θ in the $\{n, \theta\}$ space. The profit maximization of firms determines a labor demand $n^d(\theta)$, which is a decreasing function of θ in the $\{n, \theta\}$ space. With random matching, aggregate labor demand is simply the weighted sum of demands of high and low productivity firms (Panel A). Equilibrium tightness and equilibrium employment are determined at the intersection of aggregate demand and supply (Panel B). When STW is introduced, labor demand of low productivity firms increases, especially so when tightness is low (and hiring is therefore cheap). This in turn increases aggregate demand, and equilibrium tightness (Panel C). This increase in equilibrium tightness is the force driving our observed spillover effects in the data. It makes hiring more costly for all firms, and therefore reduces employment of firms not treated by STW. This equilibrium mechanism captures the negative reallocation effects of STW, which distorts employment towards low productivity firms rather than high productivity firms. This effect will be stronger the more horizontal labor demands are – that is, the more linear technology is in n (Panel D).

E.9 Counterfactual Policy Analysis

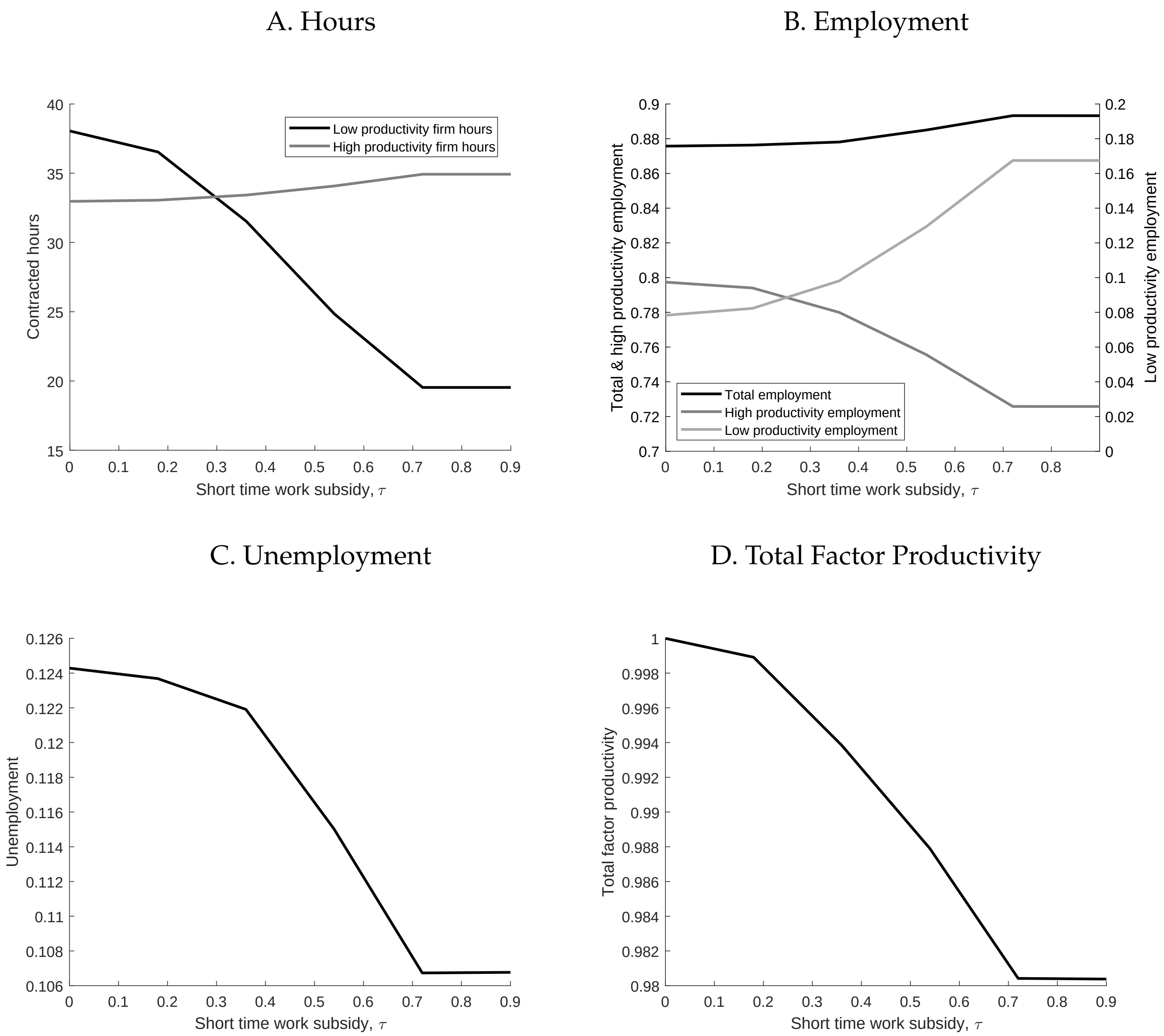
Our calibration relies on the thought experiment that we have a version of the Italian economy where all firms correspond to firms above 15 FTE and are eligible to STW. We use this model to explore how different levels of STW generosity would affect the equilibrium allocation in the labor market. In particular, this helps us to assess the counterfactual scenario of what the level of employment and productivity would have been absent STW (i.e. $\tau = 0$) during the recession.

Appendix Figure E-2 displays the results of this counterfactual analysis of the steady state equilibrium during the recession, for various levels of the STW subsidy τ. Panel A shows that STW makes low productivity firms offer lower hours to workers. The level of hours in low productivity firms, for current levels of STW generosity, is 44% lower compared to the counterfactual of no STW. This matches closely our reduced-form estimates. Panel B shows the level of employment in high productivity firms (left axis) and in low productivity firms (right axis). The higher the generosity of STW, the higher the level of employment in low productivity firms. Compared to a situation without STW, the level of employment in low productivity firms is higher by about 50%, which again closely matches our reduced form evidence. But this comes at the cost of reducing high productivity employment, from .8 to .72 of the labor force. Overall, the total effect on employment is positive, as shown by total employment in Panel B, as well as by Panel C which plots the unemployment rate as a function of the STW subsidy. In the absence of any STW subsidy ($\tau = 0$), our calibration suggests that the unemployment level would have been 1.8 percentage point higher during the recession. In Panel D, we ask how the effects of STW on the relative allocation of employment between high and low productivity firms translate into aggregate TFP in the economy. We find that – by increasing the relative employment of low productivity firms – the provision of STW does come at the cost of a decline in aggregate TFP of about 2%.

We note that results from Appendix Figure E-2 also suggest that the marginal effect of increasing or decreasing the subsidy is close to zero. The reason is that the subsidy is already large enough that workers are willing to accept extremely low hours: Panel A shows that, at $\tau = .8$, the hours constraint on low productivity firms does not bite any longer, so that any further increase in the subsidy does not affect the hours and employment allocation any more.

Finally, we note that computing the effects of STW on total welfare in this type of model is extremely sensitive to the assumptions made on entry and profits. In our model, we do not have free entry, so there are firm profits, which we rebate lump sum to workers. In this environment we find that welfare is 2% higher with the current level of STW generosity than in an economy without STW, but these results should be taken with caution.

Figure E-2: COUNTERFACTUAL SIMULATIONS: EFFECTS OF CHANGING SHORT TIME WORK GENEROSITY τ



Notes: The figure displays the results of a counterfactual analysis of steady state equilibria of the Italian economy during the Great Recession, using our calibrated model and varying the level of the STW subsidy τ. Panel A displays counterfactual values of hours per worker for low and high productivity firms. Panel B shows counterfactual values of total employment (left axis), and of employment in high productivity firms (left axis) and low productivity firms (right axis). Panel C shows counterfactual values of the equilibrium unemployment rate, and Panel D of total factor productivity. For Panel D, results are normalized to the level of TFP in the steady state equilibrium without STW (τ=0). All details of the calibration of the model are given in Appendices E.1-E.7.