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

Giulia Giupponi*
Bocconi University

Camille Landais†
London School of Economics‡

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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 aggressively used 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 on 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 the interaction of liquidity constraints with wage and hours rigidities makes labor hoarding inefficiently low without STW. Then, we show that adverse selection of low productivity firms into STW reduces the long-run insurance value of the program and creates significant negative reallocation effects when the shock is persistent.

JEL codes: H20, J20, J65.

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

*Bocconi University, via Sarfatti 25, Milan 20136 +39-02-5836-3810, giulia.giupponi@unibocconi.it

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

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1 Introduction

The economic shock created by the COVID-19 pandemic has generated a sudden revival of interest in policies aimed 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 have been 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 the working age population on STW never exceeded 4% during the Great Recession, it skyrocketed to unprecedented levels in Spring 2020. More than 11% of the German working age population and 15% of salaried employment was enrolled in a STW scheme in April 2020. The comparable figures are 14% and 31% in Italy, and 20% and 35% in 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 at stabilizing employment and at helping firms hold onto their productive workers? And do we know anything about the welfare implications of STW schemes? While almost a third of the labor force is currently on 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 has had to resort mainly to cross-country analysis (e.g. Abraham and Houseman [2009], 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

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

[2] For example, the German Federal Employment Agency (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 have been digitized for the Nuremberg metropolitan area for the years 2008 to 2010 and matched to IAB data (Tilly and Niedermayer [2017]).

sources of identification of STW treatment. In almost all countries with STW programs in place, there is no variation in firms' eligibility for 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 [2017]). 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 (Kopp and Siegenthaler [2021]).

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 create 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, those 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 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 rigidities in wages and hours – 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 sheets from CERVED, matched to our administrative data, we find that the take-up of STW strongly increases 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.

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 were 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. Moreover, 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. In contrast, 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. This indicates that STW provides short-term insurance to workers in firms exposed to shocks, but, in the context of a persistent economic shock, its insurance value partly dissipates in the medium-run and completely disappears for low-productivity matches.

Because STW subsidized low productivity matches that were unable to survive a persistent shock, STW may have inefficiently retained 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 (LLMs) and estimate how an increase in the fraction of workers treated by STW in an 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 employment growth and inflow rates in 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 points 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 firm 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). In this paper, we focus on CIGS, which is the main pillar of STW used in recessions. We start by describing its functioning and eligibility conditions, and then provide some details on how it compares with the other two pillars and with STW programs in other countries.

Cassa Integrazione Guadagni Straordinaria (CIGS) 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 an insolvency issue. 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.[3] The subsidy is available to workers in the private sector and is administered

[3]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

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.[4] Once authorized, the usage of CIGS is subject to weak conditionality requirements for both firms and workers: there are no provisions for compulsory training, no prohibition of dismissal or wage cuts 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.[5] CIGS is otherwise financed via ordinary payroll contributions, paid by eligible firms and their workers. When a firm applies to the program, it can request it for a maximum of 12 months for company crisis, and 24 months for company restructuring or reorganization, with limited possibilities of extension.[6] In practice, almost all firms use CIGS for exactly 12 months – the median and average duration 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 determined by the intersection of 5-digit industry codes and 333 different 'codice autorizzazione' (or contributory codes). These are assigned to the firm by INPS at the time of registration (which occurs when the firm is established). More specifically, once the firm submits an application for registration with INPS at a local office, INPS assigns the firm a contributory position characterized by: (i) a serial number, (ii) an industry code, and (iii) a contributory code ('codice autorizzazione'). The industry code defines the sector of activity at fine level. It serves

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

[4]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.

[5]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.

[6]Utilization of the program need not be on a continuous basis, and firms can apply more than once, but total duration cannot exceed 36 months within a 5-year period that is defined by law.

the purpose of attributing the right contribution rates to the company, based on the type of performed activity and the social insurance schemes for which it is eligible. The contributory code complements the industry code in specifying contributory obligations or exemptions for certain categories of companies. Indeed, the industry code is not always sufficient to accurately identify the contribution rate, since the company may be subject to different contributory regimes based on the activity carried out, or the presence of certain categories of employees. The authorization code has the exact purpose of identifying, within companies with the same industry code, those subject to a particular contribution or benefiting from reliefs and reductions.

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.[7] As a consequence, within fine-grained 5-digit industry codes (594 industries), there is variation in CIGS eligibility across otherwise very similar firms, due to regulations that are quite plausibly unrelated to economic conditions at such fine level today. Variation in CIGS eligibility can depend, among other things, on characteristics such as the specific activities carried out within the industry, the presence of certain categories of employees, the legal characteristics of the corporation (cooperatives, partnerships, etc), and – for some specific sectors – the direct dependence of a firm's downstream activity on that of another firm eligible for CIGS.[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

[7] The general structure of the Italian STW scheme, including firm eligibility, was legislated in a series of laws passed in the early 1970s. The pool of eligible firms has been expanded, albeit marginally, in the 1980s and 1990s. After the early 1990s, STW regulations remained substantially unchanged until the onset of the financial crisis, when CIGD was established, in 2009. CIGS has been reformed by law 92/2012 (popularly known as the 'Fornero Reform'), which abolished the possibility to use CIGS in case of bankruptcy, starting from 2016. CIGS was then reformed by D. Lgs. 148/2015 (also known as the 'Jobs Act'). The latter extended CIGS eligibility to apprentices, increased the experience rating component of CIGS costs to the firm, and redefined the set of circumstances that give access to STW (and associated maximum duration of the benefit). By focusing on the years before 2015, our analysis is not confounded by these recent legislative developments.

[8] To provide just a few concrete examples of variation in eligibility within fine grained industry codes: 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. Codice autorizzazione 3N is one of the contributory codes that indicate a firm is liable to pay the ordinary CIGS contribution and thus is eligible for CIGS treatment. Code 3A, instead, is assigned to cooperatives and consortia; jointly with specific 5-digit industry codes as specified in the INPS circular, it identifies firms that are liable to pay CIGS contributions and are eligible for STW.

(FTE) units in the six months prior to the application.[9,10] For some industries in the retail sector, the size requirement differs, and is set to 50 FTE. 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.

Comparison with Cassa Integrazione Guadagni Ordinaria (CIGO) and in Deroga (CIGD) CIGO targets small transitory shocks, including shocks in demand or production, or accidents involving forced reduction of activity (e.g. adverse weather conditions, earthquakes, power cuts). It is available to firms of any size active in the manufacturing and construction sectors and has a maximum duration of 13 weeks. CIGD is an additional program created in 2009 to provide access to STW to firms and workers not eligible for CIGS. The program was smaller in size compared to CIGS, administered at the local level and granted ad-hoc on the basis of regional decrees.

To better understand the circumstances under which CIGO and CIGS are used, we provide evidence on the main reasons for applications to STW by program type, before and after the onset of the Great Recession. Appendix Table A-1 reports the distribution of authorized hours (columns 1-3) and authorized applications (columns 4-6) across categories of reasons for application, by program type (CIGO, CIGS and CIGD) and time period, distinguishing between the pre-crisis years, 2009 and 2010-2014. Since the onset of the financial crisis, the composition of authorized applications and hours changed substantially across both CIGO and CIGS. Within CIGO, the share of applications due to slump in demand increased sharply in 2009 and was followed by an increase in applications for market crisis – a likely more severe shock – in 2010-2014. Within CIGS, the share of applications and hours due to company crisis increased substantially once the crisis hit, while those for potentially more extreme shocks such as bankruptcy, special administration and business closure either decreased or remained fairly stable. Unfortunately, we do not observe the specific reason for application for CIGD.

The evidence for CIGO and CIGS highlights that, whilst targeting relatively more severe circumstances, CIGS had a prominent role during the Great Recession and was

[9] To be precise, eligibility for 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] 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 FTE 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').

probably used by firms as either a substitute or a complement/extension to CIGO. The importance of CIGS during the Great Recession is further corroborated by evidence shown in Appendix Figure A-1, which reports the time series of authorized hours by program type from 2005 to 2014. Whilst it is indeed the case that the lion's share of STW hours was made up by CIGO in 2009, CIGS played a more important role over subsequent years.

Comparison with STW Schemes in Other Countries The rules that govern the functioning of CIGS are quite similar to those of STW programs implemented across other OECD countries. Whilst a complete comparison of STW schemes is outside the scope of this paper, we highlight here that, not only the functioning and operation, but also the type of shocks covered by CIGS during the Great Recession were close in spirit to the types of underlying shocks targeted by STW programs in other OECD countries studied in the literature.[11] According to a recent report on STW schemes in the European Union (European Commission [2020]), the main circumstances covered by the German and French schemes are not dissimilar from those covered by CIGS. The German *Kurzarbeit* covers shocks due to the economic downturn, seasonal shocks in the construction sector and worker displacement in firms undergoing restructuring. It is available to all employees covered by social security and conditional on 30% of the firm's workforce being affected by cuts in working hours (lowered to 10% during the COVID-19 crisis). The circumstances covered by the French *Chômage Partiel* are similar, and include the economic downturn, force majeure and firm restructuring.

The Italian Labor Market: Employment Protection Legislation and Duality To better contextualize this study, it is important to note that the Italian labor market is characterized by rigid employment protection legislation regulating the cost of dismissals. Over the period analyzed in the paper, protection against unfair dismissals was considerably greater for workers employed in firms with more than 15 employees within a single establishment or municipality, or 60 employees in the firm in Italy as a whole. If a dismissal was declared unfair by a judge, dismissed employees previously employed by a firm with more than 15 employees could ask to be reinstated in their job and receive earnings and social security contributions forgone over the period between the dismissal and the judgment. Alternatively, the employee could renounce the reinstatement and instead receive a severance payment equivalent to 15 months of salary. In contrast, for firms with fewer than 15 employees, the employer could choose whether to reinstate the worker (without paying any forgone wages) or make a severance payment, ranging between 2.5 and 14 months, depending on seniority (Hijzen,

[11]See Hijzen and Venn [2011] for a detailed description of STW programs in OECD countries.

Mondauto and Scarpetta [2017]).[12]

In Section 3.1, we clarify that these rules does not interfere with our identification, since we can control for non-eligible sectors – which are identically subject to employment protection legislation. Moreover, in Section 3.3, we provide 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, but become eligible for STW only once they cross the 15-FTE threshold. Hence, our identification strategy relies on the assumption that the effect of employment protection legislation did not change differentially for eligible vs non-eligible sectors before and after the onset of the Great Recession.

As a direct consequence of strong employment protection rules, the Italian labor market features, like many European countries, a strong duality between open-ended and fixed-term (or temporary) contracts (Boeri [2011], Daruich, Di Addario and Saggio [2020]). The costs of separating from workers with open-ended contracts remains significant. In contrast, the costs of separating from workers with temporary contracts when their contract comes to an end is negligible. Since 2001, the creation of temporary contracts has been almost entirely liberalized. Nevertheless, strong restrictions remain on the renewal of temporary contracts. Moreover, temporary contract workers are largely underrepresented in both unions and firm-level wage agreements (e.g. Bentolila et al. [1994], Lani [2013]). Importantly, both temporary and open-ended contract workers are eligible for STW, provided that they have more than 90 days of tenure in the firm, but because separation costs are larger for open-ended contracts, firms have higher incentives to place open-ended workers than temporary workers on CIGS.

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 link the administrative archives to firm-level balance-sheet data from CERVED via a unique identifier. CERVED is a firm register containing balance-sheet information of all limited liability companies in Italy. The

[12] The higher *de jure* costs for employers with more than 15 employees are compounded by the large *de facto* costs generated by the long average length of labor trials in Italy.

balance-sheet information covers roughly 50% of firms in the administrative records and enables us 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.

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 panel of all private sector firms that ever reached an average 6-month FTE firm size between 5 and 25 in the period 2005 to 2014.[13] Our panel is balanced in the sense that we keep all firms that ever reached a size between 5 and 25 in the sample, even when their size is no longer in that range. In particular, if firms do not survive, they are kept in the sample, with their employment, hours, etc. all set to zero. While we focus on firms in a narrow size range in terms of FTE employees for identification purposes, we note that firms in our sample account for about a fifth of all Italian establishments, for about a fourth of the Italian workforce, and a fourth of all STW spells between 2010 to 2014.

Our sample of workers is a balanced panel of all workers ever working in these firms. Appendix Table A-2 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 20.660. 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 not too dissimilar in terms of observable characteristics prior to the Great Recession. Firms in eligible INPS codes are slightly larger, but are relatively comparable in terms of hours worked per employee, wage bill per employee, revenues, investment, and liquidity. Appendix Table A-3 provides similar information for workers in our main sample of analysis. Workers in eligible INPS codes are more likely to be male and blue collar, and they are also slightly older than workers in non-eligible INPS codes, which reflect the fact that

[13] 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.

manufacturing is more represented in eligible INPS codes than in non-eligible INPS codes.

Appendix Figure A-2 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%.[14]

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-3 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 FTE employees in year $t-1$ and for firms with a maximum 6-month average size of 5 to 15 FTE 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-3 replicates the same exercise for firms in non-eligible INPS codes. For firms both 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:

[14] Appendix Figure A-2 therefore provides evidence that STW does not work like temporary layoffs, but effectively like hour reductions spread across all workers in the firm.

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

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 $t-1$. Throughout the specification, j indicates calendar years and k industries. Our coefficients of interest are γ_1^j, which trace out the dynamics of the effect of eligibility on the outcome of interest over time. The γ_2^{jk} coefficients trace out differences in the evolution of the outcome variable between firms with and without eligible codes, among firms with size below the 15-FTE threshold, over time j and in each industry k. The γ_3^{jk} coefficients trace out differences in the evolution of the outcome variable between firms with size above and below the threshold, among firms with non-eligible codes, over time j and in each industry k. The γ_4^{jk} coefficients trace out the evolution of the outcome variable in firms with non-eligible codes and size below the threshold, over time j and in each industry k.

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'.[15] 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. It should be clarified that our baseline specification does not suffer from survival bias, since for each calendar year t we look at the effect of CIGS take-up in t on

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

contemporaneous outcomes in t.

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:

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

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 and 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 should stress that because our identification relies on the interaction between INPS code eligibility and size, conditional on industry and time, we do not require that INPS code eligibility be exogenous to current time shocks. In fact, our fully flexible INPS code time shocks absorb any unobserved heterogeneous effects of the recession between firms in eligible and non-eligible INPS codes.

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.

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 outcome the probability that a firm receives CIGS treatment. It confirms the evidence from Appendix Figure A-3 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 in 2009, 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. We should stress that this effect is relative to non-treated firms. The *level* of headcount employment did decrease for all firms after 2009, but our results show that this level decrease was significantly less pronounced for firms treated by CIGS. 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 fewer hours for the same wage rate cost to the firm.

Targeting An interesting question to ask is whether firms that have a higher likelihood of separating from their workers are more likely to take up STW – i.e., whether STW is well targeted. To investigate this question, we start by building a prediction model of the probability of mass layoffs during the recession using a rich set of regressors including balance-sheet information and Bartik-style instruments.[16] We then use the model to predict the incidence of mass layoffs during the recession among eligible firms, and rank firms in quartiles of the distribution of the prediction score. In Appendix Figure A-4 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. The results in Panel A show that firms that would have been highly likely to lay off 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 are at risk of large reductions in employment.[17]

Dual Labor Markets As explained in Section 2 above, the Italian labor market is characterized by a strong duality between open-ended contracts, which are costly to terminate, but can accommodate long term job matches, and temporary contracts, which are cheap to terminate but cannot be renewed more than a few times. The use of one type of contract versus the other depends on the expected productive length of a job match.

[16]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 (see footnote 22), 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. We estimate this model using LASSO on the sample of non-eligible firms with more than 15 FTE.

[17]Interestingly, though, 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.

This dichotomy between temporary and open-ended contracts is likely to affect the impact of STW policies on firms' outcomes (Osuna and García-Pérez [2015], Daruich, Di Addario and Saggio [2020]). Indeed, STW reduces the adjustment costs to firms in case of the realization of a 'bad shock'. This, in turn, increases incentives for firms to hire open-ended contracts or transform temporary contracts into open-ended ones, leading to a change in the structure of employment within firms.

We investigate the presence of such reallocation effects on the employment structure of treated firms in Table 1. Panel I.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 an open-ended contract is $\widehat{\beta}_{IV} = .43$ (.05). In contrast, the number of employees in fixed-term contracts is negatively impacted by CIGS treatment ($\widehat{\beta}_{IV} = -.37$ (.13)). This confirms that STW treatment interacts with the duality of the labor market and tilts the structure of employment towards open-ended contracts.

Other Firms' Outcomes In Table 1, we provide additional results of the effects of STW treatment on various firms' outcomes. We decompose the total change in employment between inflows and outflows, and report in Panel I.B of Table 1 the separate effects of STW on the inflow and outflow rates. The 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 I.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 I.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.[18] 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

[18] Appendix Table A-4 reports baseline results (corresponding to those in Panel I.A and I.B of Table 1) estimated on the sample of firms that were matched to their balance-sheet data, and confirm that the effects are similar to those estimated in our baseline sample. Note that, even though 57% of firm-year observations in the main sample are matched to the CERVED data, the number of firm-year observations used in this analysis is lower since we condition the estimation sample on all balance-sheet outcomes being non-missing.

cost of goods and services used in production.[19] 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, while value added per hour worked does not change significantly. 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 fewer hours to INPS so that workers may benefit from the STW subsidy, while real working hours remain unchanged. If this 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 reacts to STW treatment, suggests that internal fund constraints may play a role in amplifying employment responses to negative productivity shocks, as suggested by Schoefer [2021]. We provide additional evidence on the role of liquidity constraints in the next section.

3.3 Robustness

Validity of Identifying Assumptions 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 to biasing our results, we first display in Appendix Figure A-5 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.

To provide more formal testing for size manipulation, we report in Appendix Figure

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

A-6 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. The 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-7 shows that in our sample less than .6% of firms change eligibility status due to a change in their INPS code every year, 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 the 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. We start by decomposing our triple difference into its sub-components. There are basically two ways in which we can think of the triple difference: (i) the first is to compare firms with eligible and non-eligible codes among those exceeding the 15-FTE size threshold (DiD1) and subtract the corresponding 'placebo' difference among those with FTE size below the 15 threshold (DiD2); (ii) the second is to compare firms above and below the 15-FTE size threshold within eligible codes (DiD3) and subtract the corresponding 'placebo' difference within ineligible codes (DiD4). The results reported in columns 2 to 5 of Table 2 are reassuring. They show that – in both approaches – the result of our main triple difference is entirely driven by the first DiD, rather than by the placebo DiD.

Indeed, we see in columns 2 and 4 that DiD1 and DiD3 yield very similar results, which are also extremely similar to our baseline triple-difference results. And we see that both placebo DiD2 and DiD4 are quantitatively small and statistically insignificant: column 3 shows that for placebo DiD2, 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. Column 5 shows that for placebo DiD4, there is little evidence of significant differential time shocks between firms below and above the 15 FTE threshold in non-eligible INPS codes within the same industry. As a consequence, this means that our baseline results do not rely much on correcting for differential trends, either across firm size, or across INPS codes.

While the previous evidence is reassuring, it cannot rule out the presence of time shocks that are both specific to eligible INPS codes and firms above 15 FTE. For instance, 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 across INPS codes may then create differential trends across INPS codes for firms with size above 15 employees. We can nevertheless assess the robustness of our results to this potential threat. 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 6 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 in column 6 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 7 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 $t-1$. The results reported in column 7 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.

Program Substitution As mentioned before, firms eligible for CIGS are also eligible for CIGO, and, since 2009, firms ineligible for CIGS could access CIGD. Panel A of Appendix Figure A-8 reports the effect of our instrument on the probability of receiving either CIGS or CIGO. The chart shows that there is indeed some substitution between CIGO and CIGS over time, in line with what is documented in Appendix Figure A-1, but our instrument remains a strong predictor of STW take-up. Panel B shows the effect of our instrument on the probability of receiving CIGS or CIGD. The chart shows that, even though non-eligible firms did indeed take up CIGD, the size of CIGS treatment is not substantially reduced once we account for it. Finally, even once we account for substitution across all programs (Panel C), our instrument retains strong predictive power for STW take-up. Point estimates of the effect of the interaction between FTE size and INPS code on the take-up of the various STW schemes are reported in Panel I.A of Appendix Table A-5.

In light of these results, we have included estimates of the effect of overall STW treatment (CIGO, CIGS and CIGD) on firm-level outcomes in Panel I.B of Appendix Table A-5. The estimated effects are both qualitatively and qualitatively similar to those we estimate for CIGS treatment in Table 1.

Alternative Specification Our baseline specification illustrated in model (3) identifies the combination of contemporaneous and past STW treatment effects. Indeed, as we show in Appendix Figure C-3, our instrument in any given year t not only predicts the probability of being treated in t, but also the probability of having been treated in $t' < t$. Importantly, our baseline specification identifies the past effect of being treated in $t' < t$ only conditional on having survived in t, as our instrument is only defined if the firm exists in t. In Section 5.2, we will propose a methodology to identify the dynamic effect of being treated at a given point in time on contemporaneous and future outcomes, and also unconditional on survival.

Here we propose an alternative specification that captures an average of contemporaneous and medium-to-long-run effects of STW treatment over the period of the Great Recession, and as such can be viewed as an intermediate step between our baseline specification and the identification of dynamic effects. In this alternative model, we consider the cohort of firms active in 2009 and define their eligibility based on FTE size in 2008 and INPS code in 2009. We estimate the effect of STW take-up over the 2010-2014 period on hours, employment and survival over the same period, measuring both treatment and employment unconditional on survival. Hours per employee are instead measured conditional on survival. We identify the effect of CIGS take-up instrumenting it with the interaction between FTE size in 2008 and INPS code in 2009. More formally, for each outcome Y, our reduced-form specification is

$$\begin{aligned} \Delta Y_{igs}^{2010-2014,2009} &= \delta_1 \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,2008} > 15] \Big\} \\ &+ \sum_k \delta_2^k \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[k = s] \Big\} \\ &+ \delta_3 \cdot \Big\{ \mathbb{1}[N_{i,2008} > 15] \Big\} + v_{igs} \end{aligned} \tag{4}$$

where $\Delta Y_{igs}^{2010-2014,2009}$ denotes the change in outcome Y for firm i, belonging to INPS code group g, in 5-digit industry s between 2009 and the 2010-2014 period, where we use an average of the (unconditional) outcome over those years. 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 ($g \in \mathcal{E}^{\complement}$). Eligibility based on INPS code is measured in 2009. $N_{i,2008}$ is firm i's FTE size in calendar year 2008. 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 $t-1$.

Estimates of the effect of STW treatment are obtained by instrumenting the probability of STW treatment T in 2010-2014 with the interaction of being in an eligible INPS code in 2009 and having more than 15 FTE employees in 2008. Specification (5) illustrates the IV model, with specification (6) being the corresponding first stage.

$$\Delta Y_{igs}^{2010-2014,2009} = \theta_{IV} \cdot T_{igs}^{2014-2010} + \sum_k \theta_2^k \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[k = s] \right\} + \theta_3 \cdot \left\{ \mathbb{1}[N_{i,2008} > 15] \right\} + \mu_{igst} \tag{5}$$

$$T_{igs}^{2010-2014} = \lambda_1 \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,2008} > 15] \right\} + \sum_k \lambda_2^k \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[k = s] \right\} + \lambda_3 \cdot \left\{ \mathbb{1}[N_{i,2008} > 15] \right\} + \nu_{igs} \tag{6}$$

This alternative model cannot identify the full dynamics of treatment since it conflates contemporaneous and medium-to-long-run treatment effects. The reason for this is that being eligible in 2009 is not only an instrument for the probability of being treated in 2009, but also for the probability of being treated in 2010, 2011, and so on. Nonetheless, this alternative model has two interesting features compared to our baseline specification: first, it is quite simple and transparent; and second, it allows us to identify an average of contemporaneous and medium-to-long-run effects of past treatment unconditional on survival. The results are reported in Panels II.A and II.B of Table 1 and are in line with the estimates from our baseline specification.[20]

From Employment Effects to Welfare: A Roadmap The results presented in this 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 those jobs alive? The answer to these questions will critically depend on whether the shock that triggers STW usage is temporary or permanent in nature. If the shock is temporary, STW usage can be welfare enhancing if it prevents inefficient layoffs, i.e., the termination of viable jobs. If the shock is permanent, keeping certain jobs alive may just delay reallocation that will be otherwise necessary. In that sense, STW can be welfare decreasing if it prevents reallocation.

In practice, when a shock hits, it is always hard to know whether it will be permanent or transitory. Interestingly, in Italy, the initial shock of the financial crisis of 2008-2009 was perceived as transitory, as can be seen in Appendix Figure C-1, based on longitudinal data from the survey on firms' expectations by the Bank of Italy. But it ended up being quite persistent, as shown in Appendix Figure C-2, because of the

[20] In light of the discussion in the previous paragraph, we also report estimates of the alternative specification using any CIG as treatment in Panels II.A and II.B of Appendix Table A-5.

European Debt Crisis that immediately followed.[21]

To understand the welfare effects of STW, we start by exploring in Section 4 the conditions under which layoffs might be inefficient in the face of a temporary shock. To this effect, we determine what moments in the data are relevant to assess whether jobs initially saved by STW might be inefficiently terminated in the absence of STW. Importantly, these empirical moments relate to the presence of frictions (liquidity constraints and bargaining frictions) that we can document irrespective of the subsequent nature of the shock. In Section 5, we then use the fact that the shock ended up being persistent in our context to investigate the impact of STW on efficient labor market reallocation.

4 Does STW Prevent Inefficient Layoffs?

When a temporary negative shock hits, many reasons make it valuable for firms and 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 on 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. Hence, STW policies can reduce inefficient labor hoarding by relaxing the liquidity constraint of firms.

[21] Appendix Figure C-2 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, in contrast to other European countries and the US.

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, doing it separately for firms in each quartile. The 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 2008 and we normalize the index so that it is increasing in financial health.[22] We then explore heterogeneity in the probability of take-up, 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)).[23] 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'

[22] The Whited-Wu index of financial constraints – proposed by Whited and Wu [2006] – is a linear combination of six empirical factors: cash flow (CF), a dividend payer dummy ($DIVPOS$), leverage (the ratio of long-term debt to total assets, $TLTD$), firm size (the natural log of total assets, $LNTA$), industry sales growth (the firm's 3-digit industry sales growth ISG), and firm sales growth (SG). The index is based on the Euler equation of an intertemporal investment model augmented to account for financial frictions. Whited and Wu [2006] estimate the Euler equation using firm-level data from the quarterly, 2002 Standard and Poor's COMPUSTAT industrial files. The estimated coefficients are used as weights in the linear combination, such that the Whited Wu index in firm i at time t is equivalent to $WW = -.091 \cdot CF_{it} - .062 \cdot DIVPOS_{it} + .021 \cdot TLTD_{it} - .044 \cdot LNTA_{it} + .102 \cdot ISG_{it} - .035 \cdot SG_{it}$. Since we do not observe $DIVPOS$, we proxy it with a dummy variable taking value one if the firm's earnings before taxes (EBT) are above median. We normalize the index by -1, so that the index ranges between 0 and 1 and is increasing in financial health.

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

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 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 without 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 employer organizations. Collective agreements are renewed on average every two years and close to 100% of private-sector employees are covered by such agreements.[24] 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 used in a limited manner (Matano, Naticchioni and Vona [2019]). These provisions clearly limit the downward flexibility of wages in the Italian setting.

[24] 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".

The evidence reported in Panel A of Appendix Figure B-1 further corroborates this notion. The figure shows the empirical distribution of the year-on-year change in log hourly wages among employees who are employed in the same firm over two consecutive years, in occupations eligible for STW. The sample is restricted to non-eligible firms. Hourly wages are obtained by dividing contractual monthly earnings by contractual hours, which can be observed in the INPS data from 2009 (as a consequence, the figure covers the years 2010-2014). The figure shows that the distribution of hourly wage changes is strongly skewed, with very little mass just below zero.[25] We view this evidence as supportive of the presence of significant downward wage rigidities. We should stress that firm-level negotiation can still be important in the Italian labor market. The presence of significant downward wage rigidity is compatible with evidence of the existence of rent-sharing (Card, Devicienti and Maida [2014], Casarico and Lattanzio [2019], Daruich, Di Addario and Saggio [2020]), to the extent that the latter is asymmetric: workers' wages respond to positive productivity shocks, but are rigid downwards.

Similarly, Panel B of Appendix Figure B-1 provides evidence of the presence of strong hour rigidities in the absence of STW. The figure plots the empirical distribution of year-on-year changes in contractual weekly hours – which can be directly observed in the INPS data – and is constructed in the same way as Panel A. The graph shows that hours are remarkably rigid within the firm: close to 100% of workers do not see any change in their contractual weekly hours across consecutive years. Admittedly, though, if labor contracts are not systematically adjusted to account for fluctuations in hours, this approach will tend to overstate hours rigidity. We therefore turn to an additional source of information on hours: the Italian Labor Force Survey (LFS), which has a short panel dimension. The information on hours worked in the LFS is self-reported and corresponds to the number of hours worked in the week preceding the interview conditional on being employed in that week. In order to select workers who are likely to have stayed in the same job over two consecutive years, we restrict the sample to workers who were in the same occupation and sector in t and $t-1$, and who were employed under a permanent contract in both periods. Since we want to assess the presence of hours rigidities in the absence of STW, we restrict the sample to workers employed in occupations eligible for STW over two consecutive years and working in firms with fewer than 15 employees, i.e., not eligible for STW. Appendix Figure B-2 reports the results of the empirical distribution of the year-on-year change in weekly hours worked for the years 2005-2014 by sector using this LFS data. In line with the above evidence, hours turn out to be extremely rigid: close to 70% of workers do not experience any change in weekly hours over consecutive years. It is worth

[25]This strong asymmetry in fact holds for both eligible and non-eligible firms.

noting, though, that the self-reported nature of hours in the LFS is likely to introduce measurement error in our measure of rigidity. In that sense, the LFS evidence can be viewed as a lower bound on the extent of hour rigidities compared to the evidence from contractual hours.

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 a 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 [forthcoming]). Firms may therefore terminate matches that exhibit significant value to workers. 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. Programs that subsidize hour reductions are prone to generating a moral hazard from firms: these, when granted access to STW, may end up reducing hours of work more than otherwise necessary, increasing the cost to the government of providing STW insurance. In Appendix B.2, 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 1.38. This means that, for the marginal Euro spent on STW to be efficient, society should be willing to pay 1.38 Euros – or a mark-up of 38% – to provide the benefit. 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 in the range of 50-150%.

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.[26] In other words, the larger the elasticity

[26] In Switzerland, Kopp and Siegenthaler [2021] 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.

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?

We study the reallocation effects of STW taking advantage of the persistence of the Italian double-dip recession of 2009. 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, except for firms in local labor markets or in industries where the shock of the recession was less persistent. Third, and finally, labor reallocation and productivity growth is significantly lower in local labor markets that receive exogenously larger levels of STW treatment during the recession.

5.1 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., $\beta_j = 1 - \alpha_j$).[27] 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 labor productivity and of TFP in 2008.

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

To investigate how pre-recession productivity affects STW take-up, we run our first-stage regression (3) separately for firms in each quartile of the distribution, using as outcome T the probability of ever taking up STW during the 2009-2014 period. The results of the estimated coefficients $\widehat{\kappa}_1$, reported in Panel A of Figure 6, 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.[28]

Do lower productivity firms also benefit more from this larger take-up of STW? In Panels B and C of Figure 6, we report estimates of $\widehat{\beta}_{IV}$ from 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. In contrast, 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. The 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.

5.2 Dynamic Effects

The evidence from Figure 6 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 relevance 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 apply for STW for a maximum of 12 months and, in practice, both average and median duration are very close to 52 weeks. Our baseline estimates $\widehat{\beta}_{IV}$ in

[28]This negative selection of firms into program take-up may be partly due to CIGS targeting relatively severe shocks.

specification (2), 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.[29] In other words, they capture both the contemporaneous effects and the past dynamic effects of STW treatment.

To identify the sequence of dynamic Treatment-On-the-Treated 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-3. 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, in each year, by the coefficients ($\beta_{2009}^{RF}, \beta_{2010}^{RF}$, 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:

$$ITT_0 = \hat{\beta}_{2009}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{7}$$

$$ITT_1 = \hat{\beta}_{2010}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}, \quad etc. \tag{8}$$

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 $\{\hat{\beta}_0^{TOT}, \hat{\beta}_1^{TOT}, ..., \hat{\beta}_4^{TOT}\}$.

[29] This is because INPS codes and firm size, which determine access to STW, are persistent over time. 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-3 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 of having received treatment in the past 5 years.

Figure 7 reports the dynamic effects of STW treatment on hours per employee. The 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-4 shows similar patterns for other employment outcomes. Upon treatment, log head-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.[30] Yet point estimates are consistently small, and close to zero, indicating no significant long-term effects of treatment on employment outcomes. 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 8 estimates for three outcomes: the probability of being employed, the total number of hours, and total earnings plus all social insurance transfers observable in the INPS data, including STW.[31] 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 8, 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 for 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 -1, employment status, annual weeks worked, earnings and firm size at -1, -2,

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

[31]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.

-3 and -4, and main industry at -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.[32]

The results of the event study estimates for all three groups and all three outcomes are reported in Figure 8 and reveal interesting dynamic patterns. First – and implicitly due to how the comparison groups have been defined – there are no differential pre-event trends across 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 (≈ 20 percentage points) than workers in non-eligible firms, and only 15 percentage points higher 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, its beneficial effects 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.[33] There is also a significant drop in total earnings and transfers of treated workers, which, two years after

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

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

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

As we discussed at the end of Section 2.1, firms have greater incentives to put open-ended-contract workers rather than temporary workers on STW. This notion, which – as we will see shortly – is indeed supported by the data, offers a way to improve on our identification of the dynamic effects of STW on workers' careers, by comparing workers employed on open-ended vs temporary contracts within the same firm. As we explain in more detail in Appendix C.3, this allows us to control for the correlation between STW treatment and persistent firm-level shocks. In Appendix Figure C-6, we start by showing that the probability of STW receipt around the time when a firm experiences a STW event is indeed larger for workers on open-ended contracts than on fixed-term ones. In Panel A we focus on workers who are on open-ended contracts, while in Panel B on workers who are on temporary contracts in the year before the event. In both panels, we also report the evolution of the probability of STW receipt among a control group of workers who have similar observable characteristics but work in firms that are ineligible for STW at event time -1. The figure shows very clearly that the probability of STW take-up is much larger among workers on open-ended contracts than among workers on temporary contracts, conditional on the firm going into STW.

We then report in Appendix Figure C-7 the evolution of the differential probability of employment of workers employed in open-ended vs fixed-term contracts in event time -1 in firms experiencing a STW event for the first time at event time 0, relative to similar workers in non-eligible firms. The figure shows clear positive effects of STW on employment in the short run, but these effects dissipate entirely after STW exhaustion. These results provide transparent and complementary evidence on the dynamic effects of STW, confirming that STW had positive effects in the short run, but that these effects did not last.

In Figure 9, we explore how the dynamics of outcomes for workers treated by STW differs by a 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 5.1. For each subsample of STW treated workers, we define two new control groups, drawn from workers in non-eligible firms with a similar level of labor productivity, and following the same methodology as in Figure 8. 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. In contrast, 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.[34]

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, indicating that – for such matches – STW clearly provided only a short-term fix, but was not better than layoff in the medium run. The targeting of relatively severe shocks by CIGS likely implies that the long-run effects we estimate are a lower bound of those that would be estimated in the presence of less severe and less persistent shocks.

Heterogeneous Treatment Effects by Temporariness of the Shock We have shown that, in the face of a persistent shock, STW has no significant effects on employment in the long run. Even though in the Italian context the shock was on average persistent, we can nonetheless exploit some variation in the degree of persistence of the economic shock across industries and local labor markets (LLMs) to probe heterogeneity in STW treatment effects by the temporariness of the shock. Our empirical implementation proceeds in two steps. First, we derive a data-driven characterization of industries and LLMs that have experienced more or less permanent shocks. We describe in detail our empirical approach for classifying industries and LLMs by type of shock in Appendix C.4. The results of this classification are displayed in Appendix Figure C-8, which shows the evolution of total employment across more and less persistently affected LLMs (Panel A) and industries (Panel B). The graphs provide support for our proposed classification. It shows that LLMs (industries) that we classify as subject to more transitory shocks experienced a similar decline at the onset of the Great Recession compared to LLMs (industries) that we classify as subject to more persistent shocks; but the former LLMs (industries) recovered, starting in 2010, while the latter remained persistently affected.

We then use the above dichotomization to investigate whether STW take-up and treatment effects are heterogeneous with respect to the temporariness of the shock. To this

[34] Figure 9 is constructed using the sample of workers belonging to firms matched with their balance-sheet data. Figure 8, instead, is based on the main sample of workers, but remains identical when estimated on the sample used in Figure 9. Results are available upon request.

effect, we run models based on specifications (5) and (6). The results are reported in Appendix Table C-1, where we consider any CIG take-up as treatment. Panel A shows heterogeneity with respect to the temporariness of the shock at the LLM level, Panel B at the industry level. Estimates in both panels indicate that, when the shock is more temporary, firms take up STW more and the employment effects of STW are larger. The magnitude of the effects is qualitatively important and similar across the two estimations, but estimated with insufficient precision to be significant at conventional levels. The effect on hours per employee (conditional on employment) does not appear to be heterogeneous by type of shock.

5.3 Reallocation Effects

STW take-up is high among low productivity matches that do not seem to survive a persistent shock after STW treatment ends. By keeping workers in these low productivity firms, STW is therefore susceptible to inefficiently delaying the efficient reallocation of workers towards more productive employment relationships. Recessions are typically believed to accelerate productivity-enhancing reallocation, since they are times in which it is less costly to reallocate factors of production. In fact, the cleansing role of recessions has been debated for a long time, for recessions could also distort the reallocation process, for example, due to credit constraints. Evidence for the US suggests that recessions are usually times of productivity-enhancing reallocation, but to an extent which can be heterogeneous across different types of shocks (Foster, Grim and Haltiwanger [2016], Barrero et al. [2021]).

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.[35] For this analysis, we use data on the universe of firms and workers in the Italian labor market, with no restrictions on firm size. For 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.[36] 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 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

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

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

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:

$$\underset{t,t'}{\Delta} Y_{ij} \quad = \quad \alpha^R + \beta^R_{IV} \underset{t,t'}{\Delta} T_j + X'_j \gamma^R_0 + W'_i \gamma^R_1 + \varepsilon_{ij} \tag{9}$$

The model is estimated on the sample of all firms i that are non-eligible for 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. In our baseline estimation of model (9), we compare the recession years 2010-2013 to the pre-recession years 2005-2008. $\underset{t,t'}{\Delta} T_j$ 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 are 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 10 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 (9), that is, the relationship between the instrument Z_j (the fraction of 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 jobs. 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 (9) with long differences in LLM-level TFP as outcome.[37] 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 (9) 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 2005-2008 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 model for employment growth of non-eligible firms in the LLM is reported in Panel B of Figure 10. We clearly see no significant relationship between the placebo instru-

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

ment 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 clearly identify the presence of reallocation effects of STW. But they cannot tell us what labor allocation and TFP would look like without 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 – without STW – the level of unemployment would have been 1.8 percentage points 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 to our understanding of STW programs, 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 from being 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 lasted 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. The results also suggest that – to maximize program effectiveness – STW should be targeted on high-productivity firms facing liquidity constraints.

How much can these results teach us about the welfare effects of STW in the COVID-19 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 a 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 would 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.

We follow up on the above question in Giupponi, Landais and Lapeyre [forthcoming], where we provide a conceptual framework to analyze the welfare consequences of labor hoarding subsidies vis-à-vis unemployment insurance, and map it onto the existing empirical evidence on these programs. While progress has been made in understanding the functioning of labor hoarding policies, more research is necessary to fully establish the welfare consequences of the massive subsidization of labor hoarding during the COVID-19 crisis, in particular to assess the aggregate demand effects of STW through firm survival and employment expectations (Guerrieri et al. [forthcoming]).

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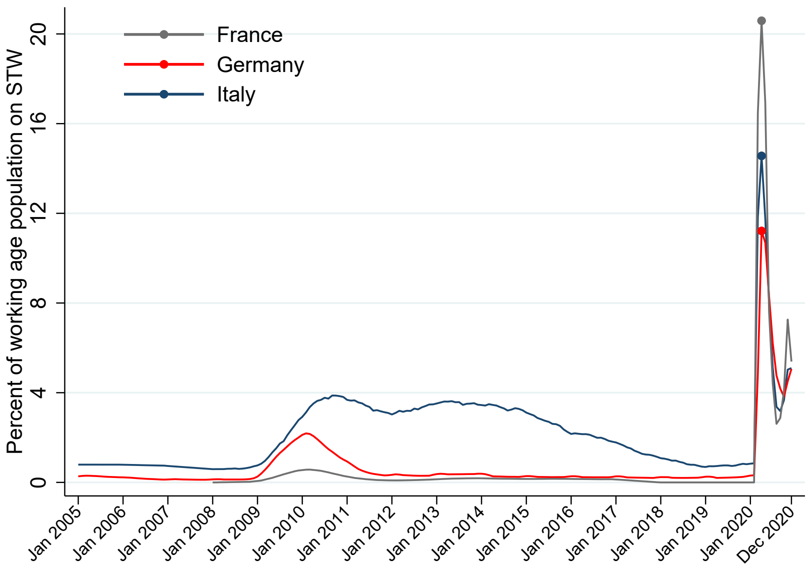
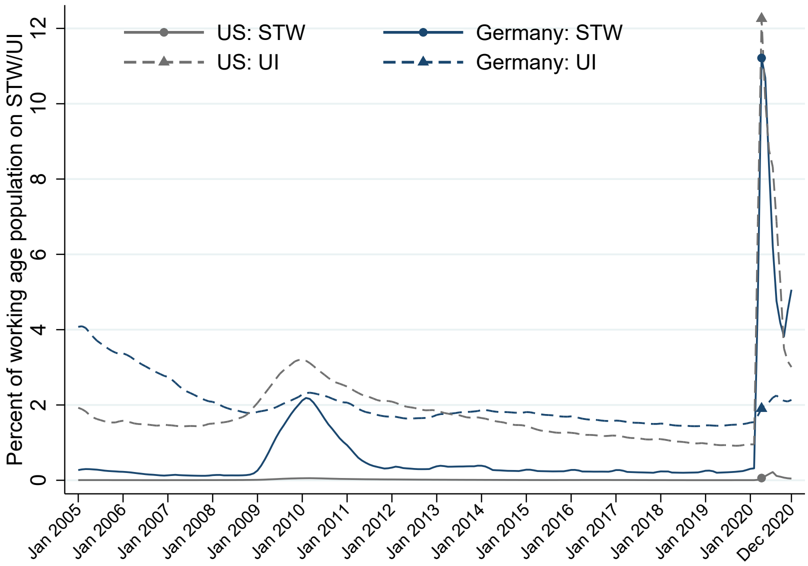
Figure 1: LABOR MARKET POLICY RESPONSES IN EUROPE IN THE COVID-19 CRISIS: THE RISE OF SHORT TIME WORK



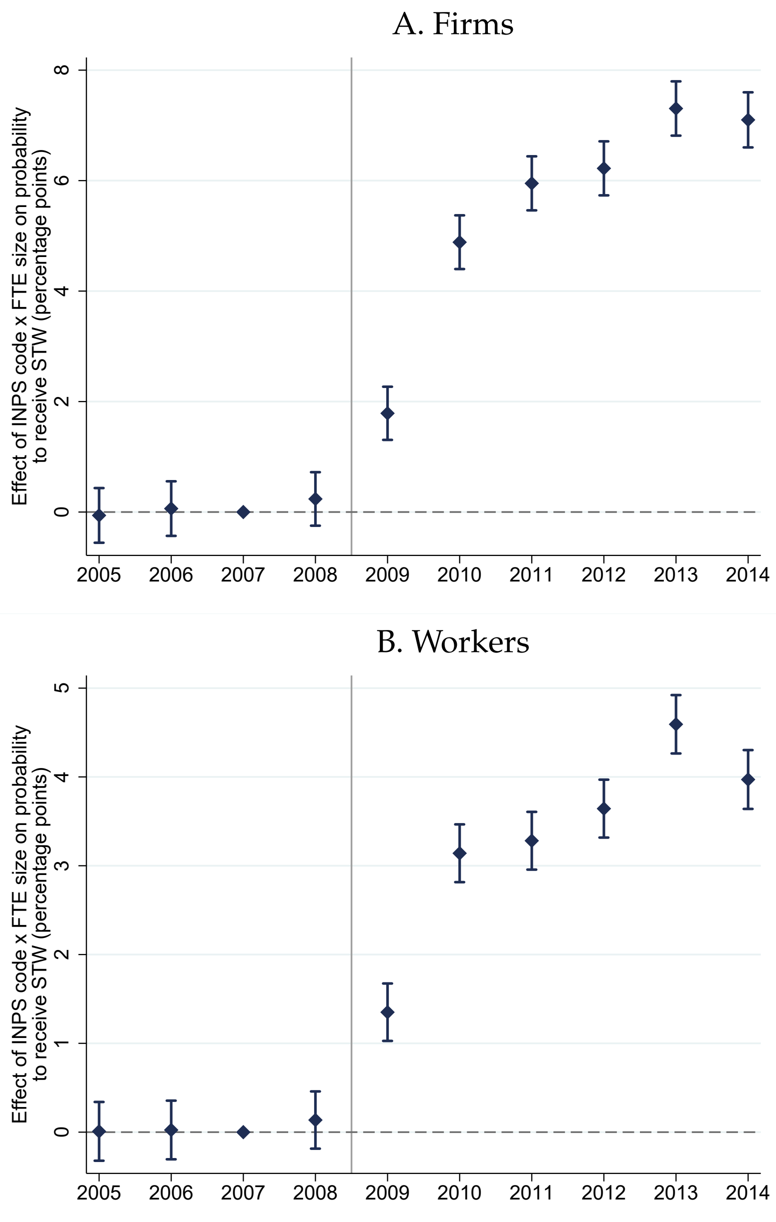
Notes: The graph reports the share of the working age population on STW in France, Germany and Italy, at monthly frequency from January 2005 to December 2020. Data for the period from January 2005 to December 2019 are sourced from national administrative authorities and statistical agencies. Data for France come from the French Ministry of Labor (`https://dares.travail-emploi.gouv.fr`), for Germany from the German Federal Employment Agency (`https://statistik.arbeitsagentur.de`), and for Italy from the Social Security Administration (`https://www.inps.it`). For the period from January to December 2020, monthly data on STW have been provided by the OECD Directorate for Employment, Labour and Social Affairs (OECD [forthcoming]). For France, data on STW start from January 2008, when the program was introduced, and are not available between January 2017 and February 2020, due to a break in the series. We assume take-up to be zero over those periods. Prior to 2020, Italian data on STW usage are recorded in terms of authorized hours of STW rather than employees on STW. In order to obtain an estimate of the number of individuals on STW, we assume – based on estimates in this paper – that 90% of authorized hours are used and that, while on STW, work hours are 35% of usual hours (assumed to be 40 per week). The series are rescaled by the quarterly working age population, taken from OECD.

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



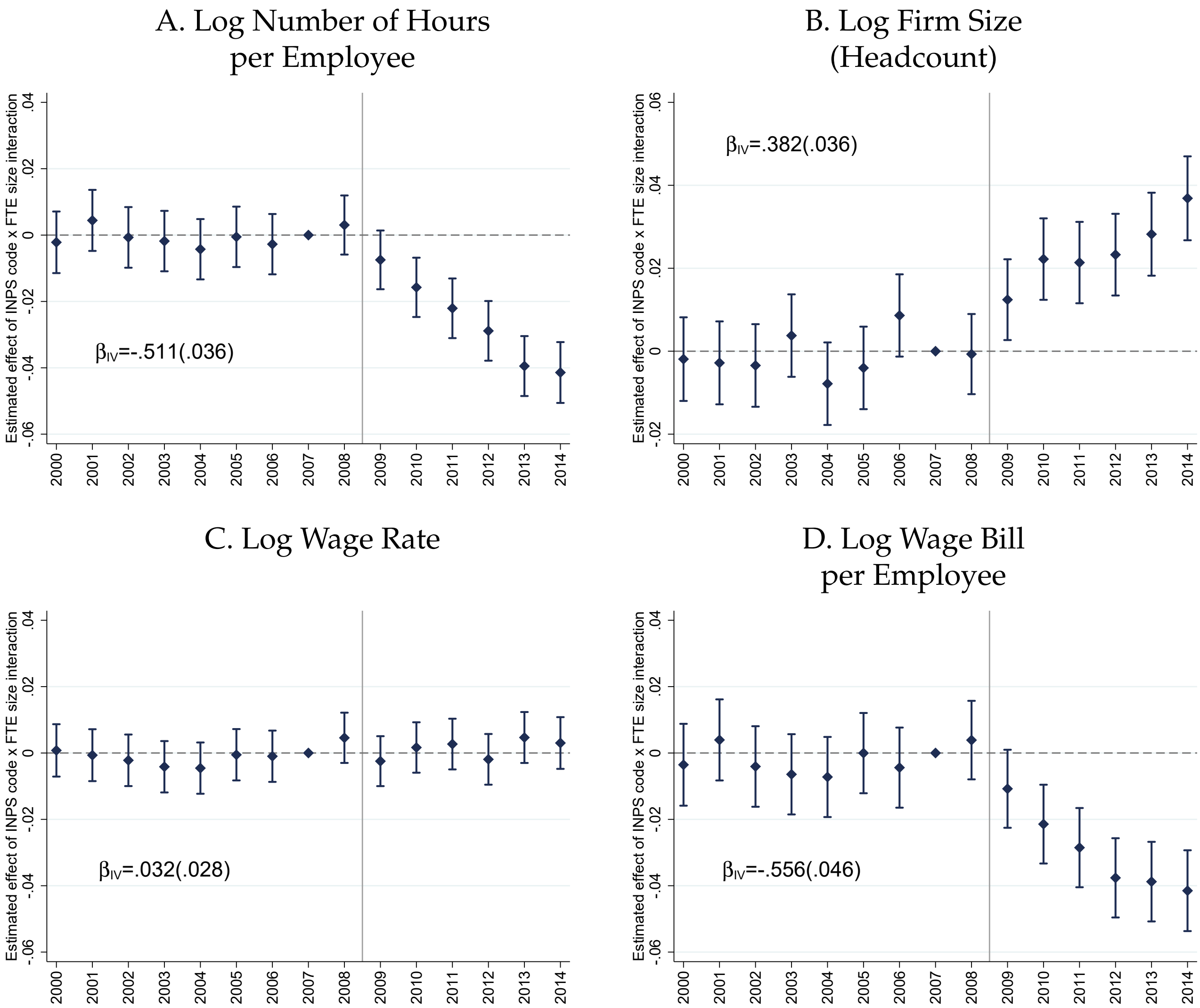
Notes: The graph reports the share of the working age population on STW and on UI in Germany and the US, at monthly frequency from January 2005 to December 2020. Data on STW for the period from January 2005 to December 2019 are sourced from national administrative authorities and statistical agencies. Data for Germany come from the German Federal Employment Agency (https://statistik.arbeitsagentur.de), and for the US from the Department of Labor (https://oui.doleta.gov). For the period from January to December 2020, monthly data on STW have been provided by the OECD Directorate for Employment, Labour and Social Affairs (OECD [forthcoming]). Data on UI for the period from January 2005 to December 2019 come from the German Federal Employment Agency (https://statistik.arbeitsagentur.de) and the US Department of Labor (https://oui.doleta.gov). For the period from January to December 2020, monthly data on UI are sourced from the OECD Social Benefit Recipients Database (https://www.oecd.org/social/social-benefit-recipients-database.htm). The series are rescaled by the quarterly working age population, taken from OECD.

Figure 3: Firms' & Workers' Probability of Receiving Short Time Work Treatment by Firm Size and Sector



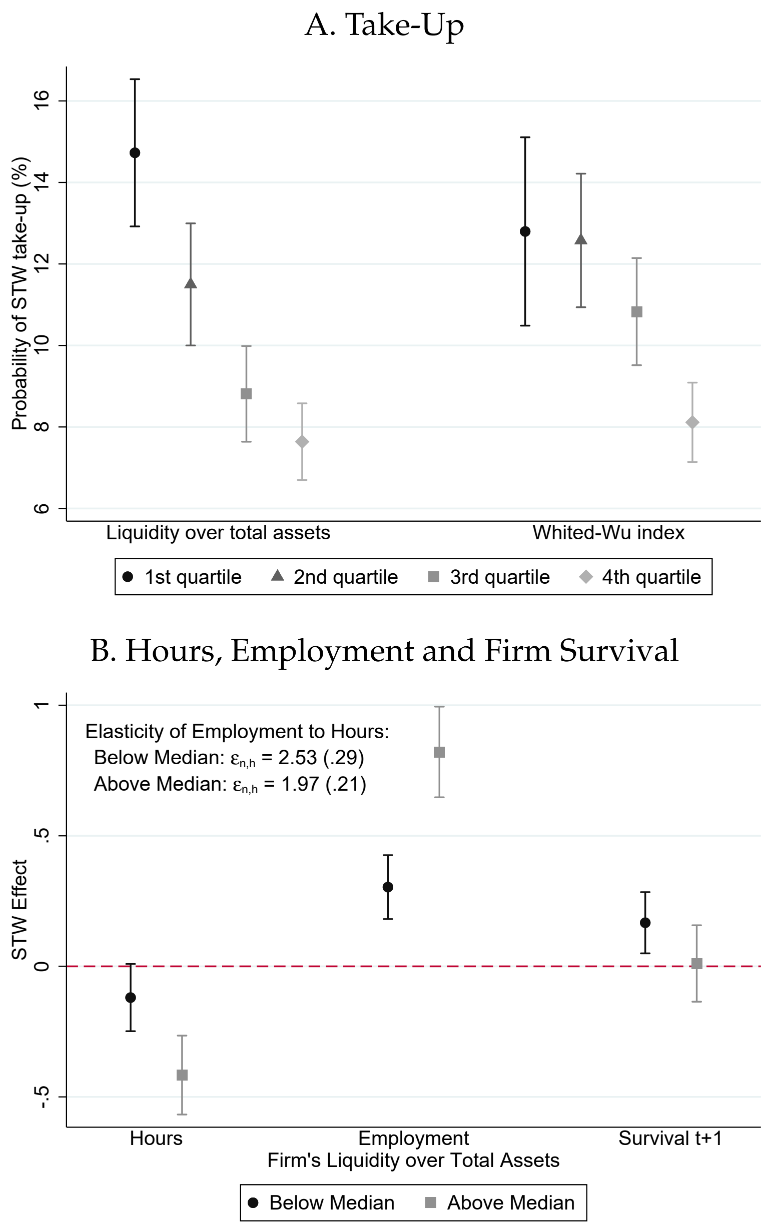
Notes: The graphs report the coefficients $\widehat{\gamma}_1^t$ estimated from equation (1) for all years $t \in [2005, 2014]$ using the probability of STW receipt as outcome. The omitted year is 2007, so all results are relative to 2007. Panels A and B plot the estimated coefficients for the probability 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



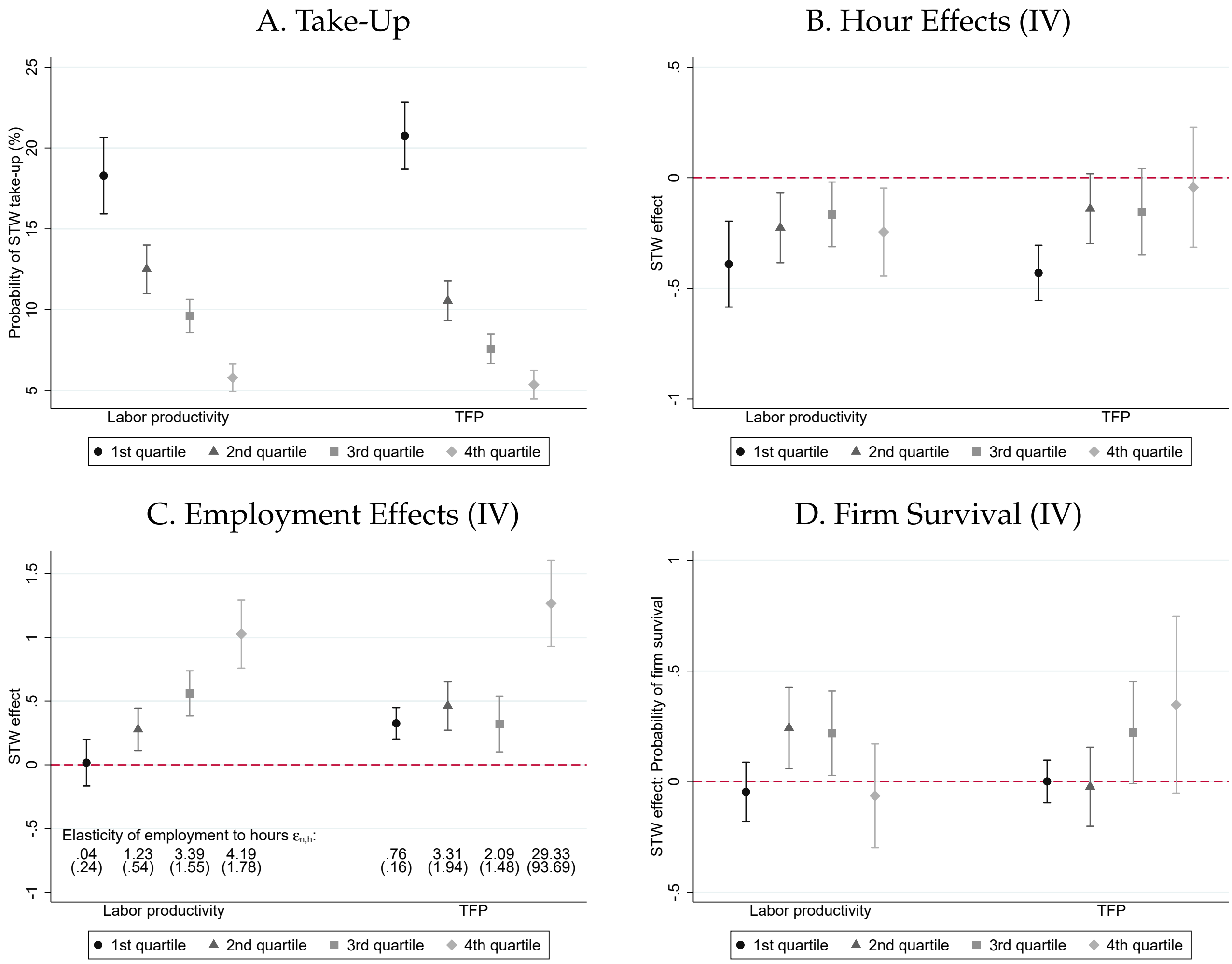
Notes: The graphs show the coefficients $\widehat{\gamma}_1^t$ estimated from equation (1) for all years $t \in [2000, 2014]$ for different firm-level outcomes. The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on standard errors 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



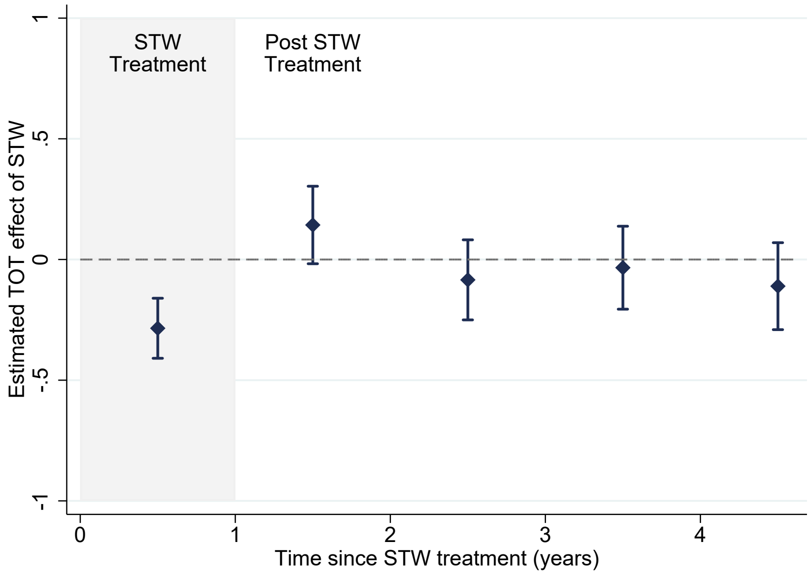
Notes: The graphs show heterogeneity in STW take-up and treatment effects by measures of liquidity constraints. Panel A displays the estimated coefficient $\widehat{\kappa}_1$ from specification (3) for the probability of STW take-up for firms at different quartiles of the distribution of liquidity – 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 $\varepsilon_{n,h} = -\frac{d \log n / d\text{STW}}{d \log h / d\text{STW}}$, with standard errors computed using the Delta-method.

Figure 6: 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 5.1. 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.

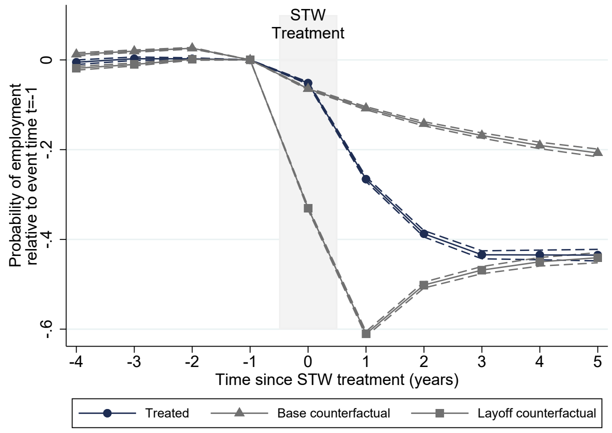
Figure 7: TOT ESTIMATES OF THE DYNAMIC EFFECT OF SHORT TIME WORK ON LOG NUMBER OF HOURS PER EMPLOYEE



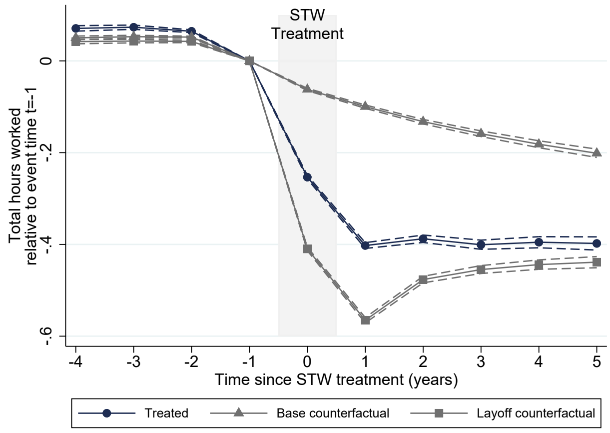
Notes: The graph reports the coefficients $\widehat{\beta}_k^{TOT}$ for $k \in [0, ..., 4]$ for the dynamic effects of STW treatment on hours worked per employee. These effects are estimated recursively as illustrated in Appendix C.2. The β_k^{TOT} coefficients identify the dynamic treatment effects 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 8: DYNAMIC EFFECTS OF SHORT TIME WORK ON WORKERS' OUTCOMES

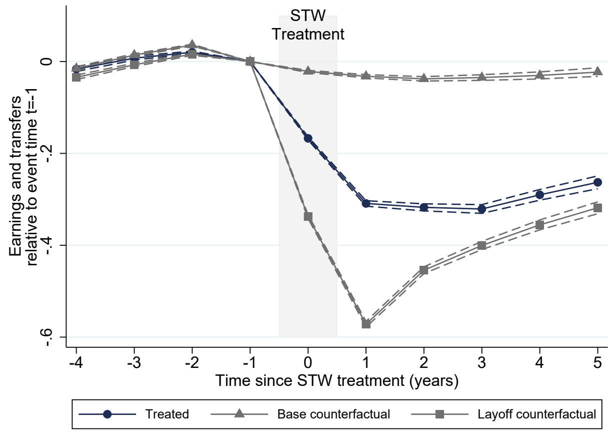
A. Probability of Employment



B. Number of Hours Worked



C. Earnings + CIGS/Transfers

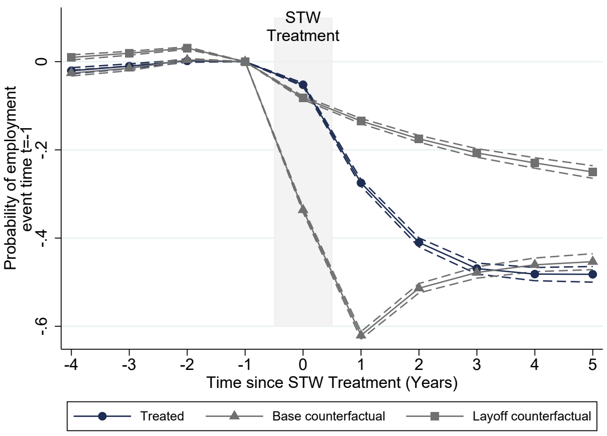


Notes: The graphs report the estimated coefficients of event study regressions for different outcomes and different event-year definitions at the worker level. All estimates are relative to event-year -1 and are scaled by the average level of the outcome in that year. Individual and calendar-year fixed 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 and any other social insurance program available in the INPS data).

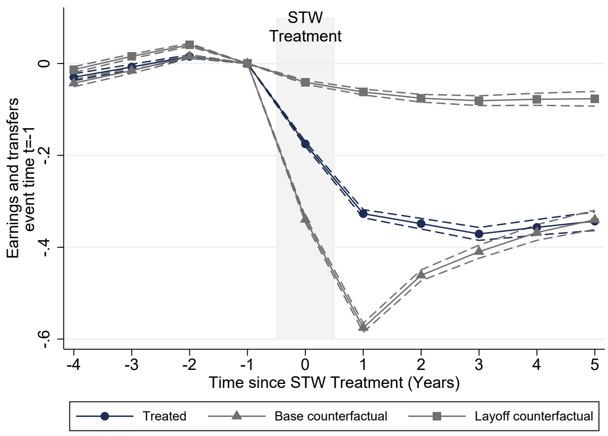
Figure 9: DYNAMIC EFFECTS OF SHORT TIME WORK ON WORKERS' OUTCOMES BY FIRMS' PRE-CRISIS LEVEL OF LABOR PRODUCTIVITY

A. Low Labor Productivity Firms

Employment

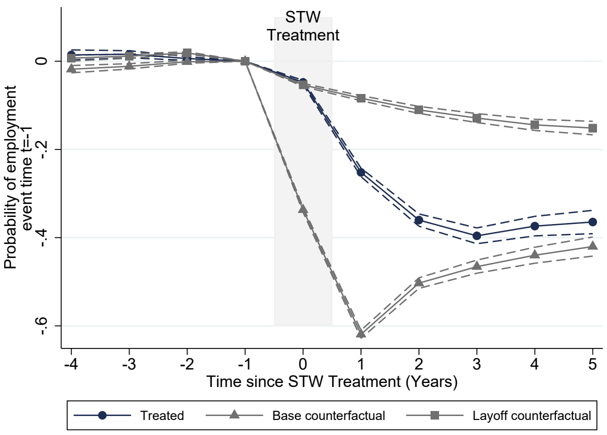


Earnings + Transfers

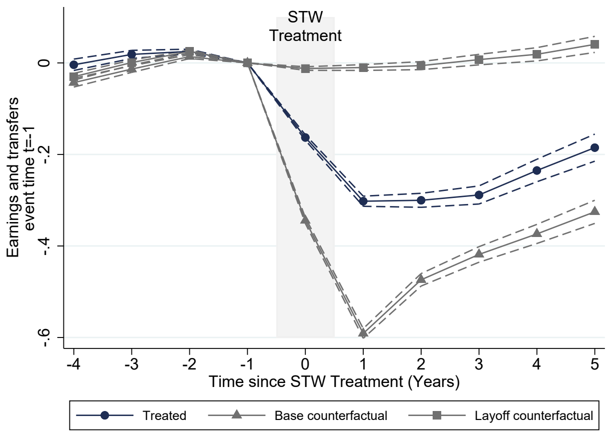


B. High Labor Productivity Firms

Employment

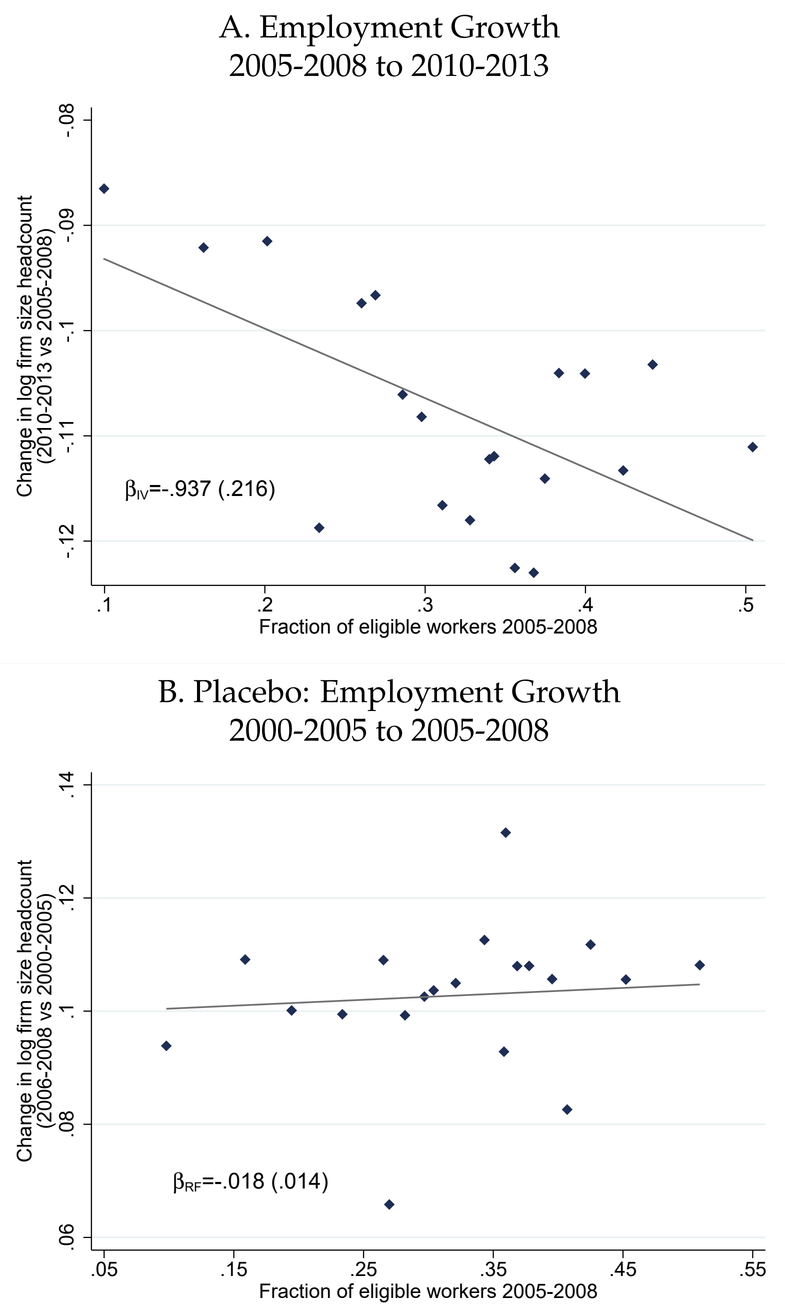


Earnings + Transfers



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 8. 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 10: 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 (9). 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 $\hat{\beta}^{R}_{IV}$ coefficient from equation (9) 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 an LLM between 2000-2005 and 2005-2008, and the fraction of eligible workers in 2005-2008 in the LLM. Panel B also reports the reduced-form $\hat{\beta}^{R}_{RF}$ coefficient from equation (9) and its associated robust standard error clustered at the LLM level.

Table 1: EFFECTS OF STW TREATMENT ON FIRMS' OUTCOMES: BASELINE SPECIFICATION

	Estimate (1)	Std Error (2)	N (3)
Panel I. Baseline specification			
	A. First Stage		
Probability of CIGS take-up	.054	(.001)	2843205
	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	(.128)	2843205
Rate of inflows	.081	(.599)	2843205
Rate of outflows	-.337	(.027)	2843205
Firm survival probability (in $t+1$)	.104	(.038)	2843205
	C. Balance-Sheet & Productivity Outcomes (IV)		
Firm value added	.095	(.159)	873839
Value added per worker	-.508	(.120)	873839
Value added per hour worked	-.057	(.101)	873839
Tangible investment	-.003	(.672)	873839
Liquidity	.939	(.461)	873839
Panel II. Alternative specification			
	A. First Stage		
Probability of CIGS take-up	.091	(.004)	300795
	B. Employment Outcomes (IV)		
Hours per employee (inverse hyperbolic sine)	-.258	(.066)	300795
Firm size headcount (inverse hyperbolic sine)	.261	(.131)	300795
Firm survival probability	.248	(.037)	300795

Notes: Panel I.A reports the estimates of the coefficient $\widehat{\kappa}_1$ from specification (3) and its associated cluster-robust standard error in parenthesis. Panels I.B and I.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. Panel II.A reports the estimates of the coefficient $\widehat{\lambda}_1$ from specification (6) and its associated cluster-robust standard error in parenthesis. Panel II.B reports the $\widehat{\theta}_{IV}$ coefficients (and cluster-robust standard errors) estimated from equation (5).

Table 2: ROBUSTNESS OF BASELINE EFFECTS

	"Doughnut" Regression	Only >15 FTE [DiD 1]	Only $\leq$ 15 FTE [DiD 2] (Placebo)	Only Eligible [DiD 3]	Only Non-Eligible [DiD 4] (Placebo)	No Dismissal Rule Change >60FTE Across Italy	No Dismissal Rule Change 50FTE Threshold
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				A. First Stage			
Probability of CIGS take-up	.053	.051	.002	.058	.000	.055	.041
	(.002)	(.002)	(.000)	(.001)	(.000)	(.005)	(.004)
				B. Outcomes			
	IV	**IV**	**RF**	**IV**	**RF**	**IV**	**IV**
Log hours per employee	-.449	-.602	-.011	-.540	.018	-.670	-.156
	(.037)	(.081)	(.020)	(.045)	(.030)	(.230)	(.132)
Log employment	.284	.306	-.020	.383	.000	.848	.338
	(.032)	(.099)	(.030)	(.048)	(.003)	(.297)	(.258)
Log wage bill per employee	-.544	-.498	-.026	-0.592	.015	-.568	-.390
	(.049)	(.155)	(.030)	(.072)	(.005)	(.297)	(.709)
N	2686140	429490	2608383	59634	2978239	152753	44793

Notes: Panel A reports the first stage coefficients for different samples and specifications. 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 IV coefficients for specification (DiD1) 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 3 reports the reduced-form coefficient $\widehat{\alpha}_1$ for placebo specification (DiD2), restricting the sample to firms with 6-month average FTE size $\in (5, 15]$. Column 4 reports the IV coefficients for specification (DiD3) restricting the sample to firms with eligible INPS codes and instrumenting STW take-up with $\mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t \geq 2009]$. Column 5 reports reports the reduced-form coefficient $\widehat{\alpha}_1$ for placebo specification (DiD4) restricting the sample to firms non-eligible INPS codes. Column 6 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 7 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

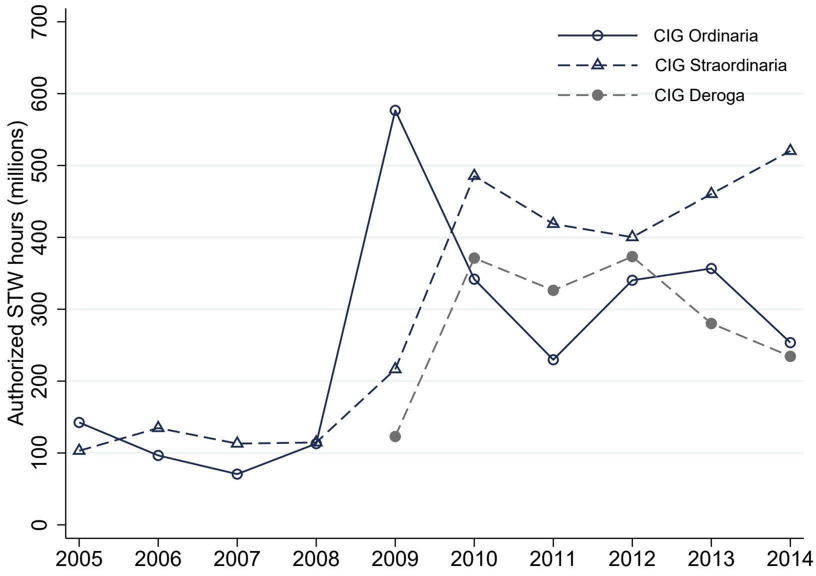
	Reallocation Effects			Placebo Estimates		
	IV	IV	IV	RF	RF	RF
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Employment Spillovers on Non-Eligible Firms					
Log employment	-0.492	-0.918	-0.937	-0.018	-0.018	-0.018
	(0.137)	(0.216)	(0.216)	(0.014)	(0.014)	(0.014)
Log inflows	-3.594	-4.406	-3.176	0.029	0.029	0.047
	(1.947)	(2.380)	(1.440)	(0.153)	(0.153)	(0.147)
LLM controls		×	×		×	×
Firm-level controls			×			×
N		3023166			2784567	
	B. Effects on Labor Market Productivity					
Log TFP	-2.307	-2.093		-0.003	-0.003	
	(0.593)	(0.606)		(0.062)	(0.062)	
LLM controls		×			×	
N		1222			1222	

Notes: Columns 1-3 of the table report the $\widehat{\beta}^R_{IV}$ estimated from equation (9) and its associated robust standard errors clustered at the LLM level in parenthesis. Columns 4-6 report reduced-form placebo estimates of equation (9) comparing outcome growth during placebo pre-recession periods (2000-2005) vs (2005-2008), and using the fraction of eligible workers in 2005-2008 as instrument. 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 an IV model similar to (9) but where the outcome is the long difference of TFP, at the LLM level. We define TFP as 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

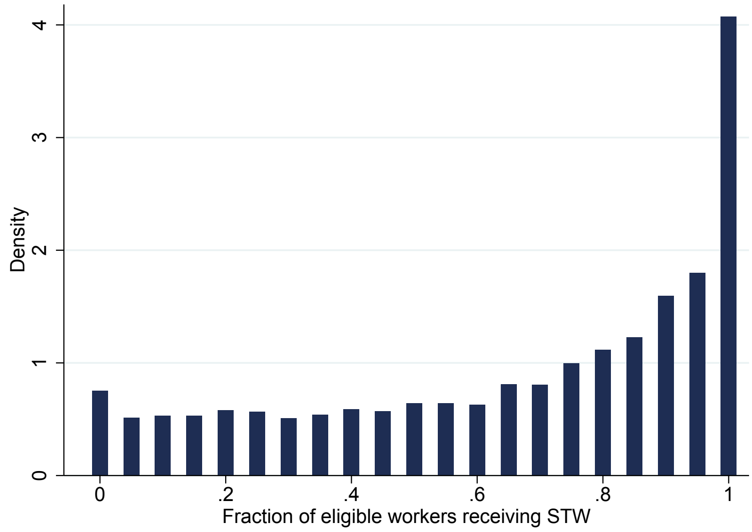
Figure A-1: TIME SERIES OF AUTHORIZED SHORT TIME WORK HOURS BY PROGRAM TYPE



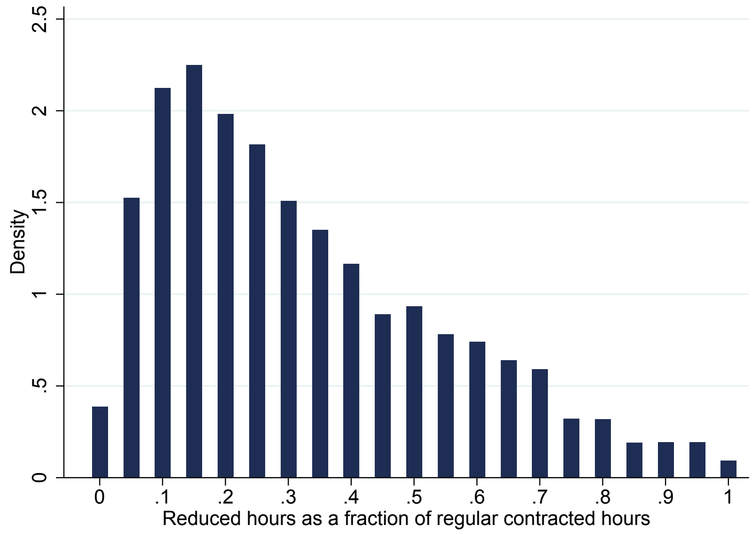
Notes: The graph reports the time series of authorized hours (in millions) by program type from 2005 to 2014.

Figure A-2: 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 contractual 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 AUTHORIZED HOURS AND APPLICATIONS, BY SHORT TIME WORK SCHEME AND REASON FOR APPLICATION

	Share of Authorized Hours			Share of Authorized Applications		
	2005-2008	2009	2010-2014	2005-2008	2009	2010-2014
Reason for application	(1)	(2)	(3)	(4)	(5)	(6)
CIGO						
Adverse weather conditions	.35	.07	.13	.93	.71	.72
Market crisis	.03	.02	.16	.00	.01	.05
Slump in demand	.59	.89	.68	.06	.27	.21
Other	.03	.01	.03	.01	.01	.02
CIGS						
Company crisis	.38	.65	.46	.46	.69	.57
Restructuring/Reorganization	.25	.09	.18	.14	.06	.08
Bankruptcy	.16	.09	.16	.21	.13	.15
Special administration	.09	.04	.02	.05	.03	.01
Business closure	.00	.00	.03	.00	.00	.02
Other	.12	.13	.15	.14	.09	.17
CIGD						
Total	-	1.0	1.0	-	1.0	1.0

Notes: The table reports the distribution of authorized hours (columns 1-3) and authorized applications (columns 4-6) across categories of reasons for application, by program type (CIGO, CIGS and CIGD) and time period, distinguishing between the pre-crisis years 2005-2008, the year 2009 and the crisis years 2010-2014. The INPS data do not report the specific reason for application for CIGD.

Table A-2: 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.

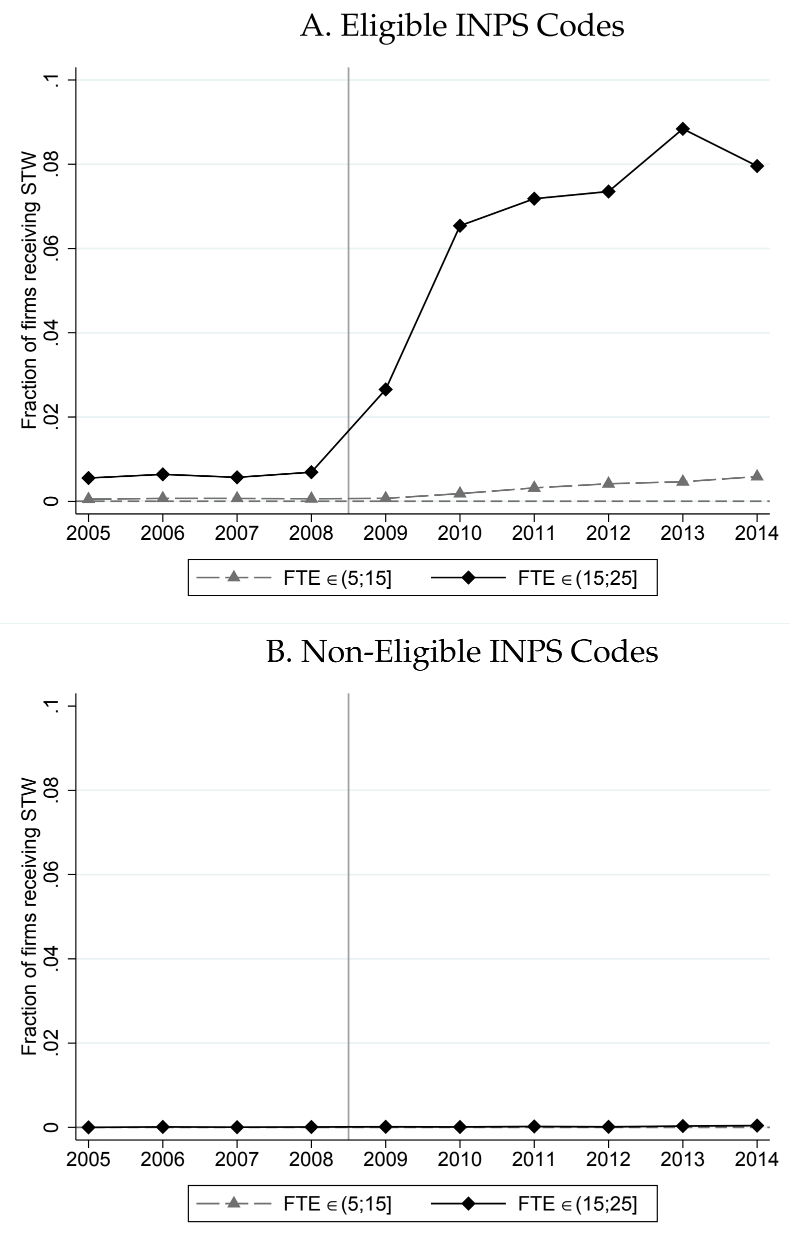
Table A-3: DISTRIBUTION OF WORKERS' CHARACTERISTICS IN THE MAIN SAMPLE BY ELIGIBLE AND NON-ELIGIBLE INPS CODES (2008)

	(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.

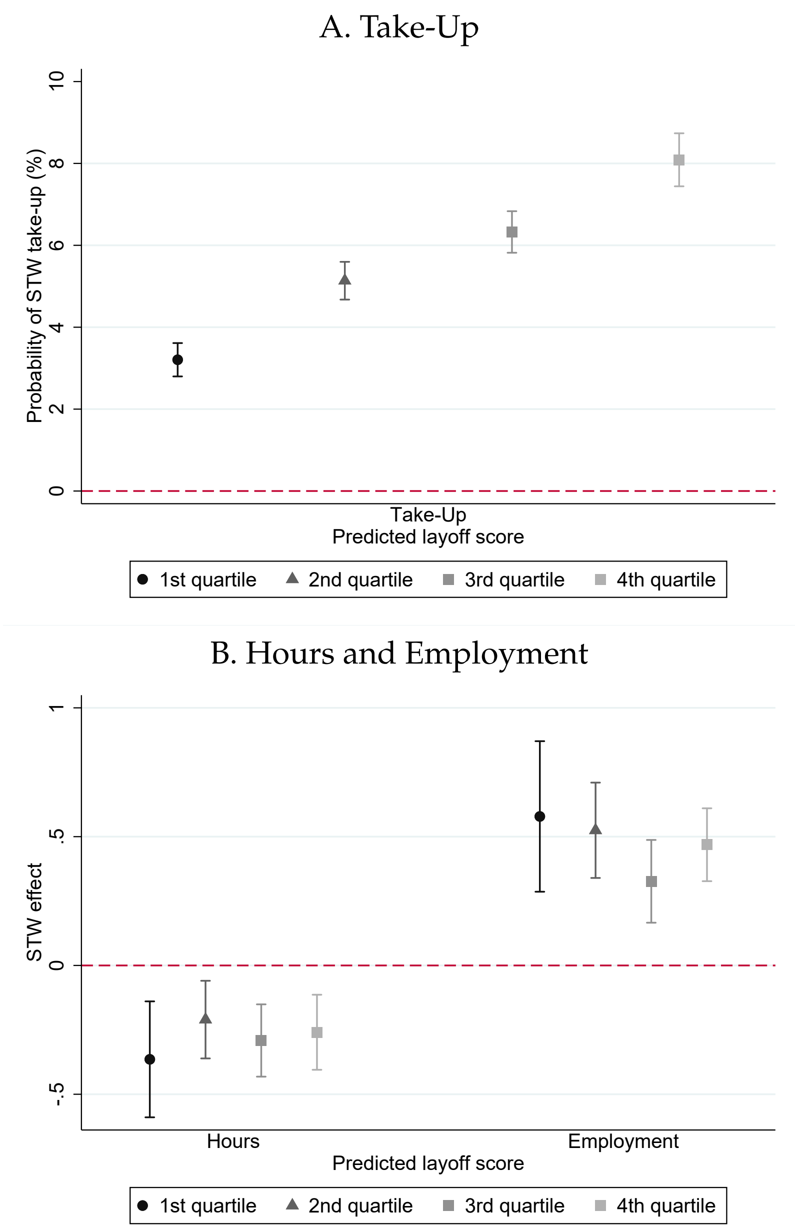
A.2 Identification & Robustness: Additional Evidence

Figure A-3: FRACTION OF FIRMS RECEIVING SHORT TIME WORK BY FIRM SIZE & INPS CODE



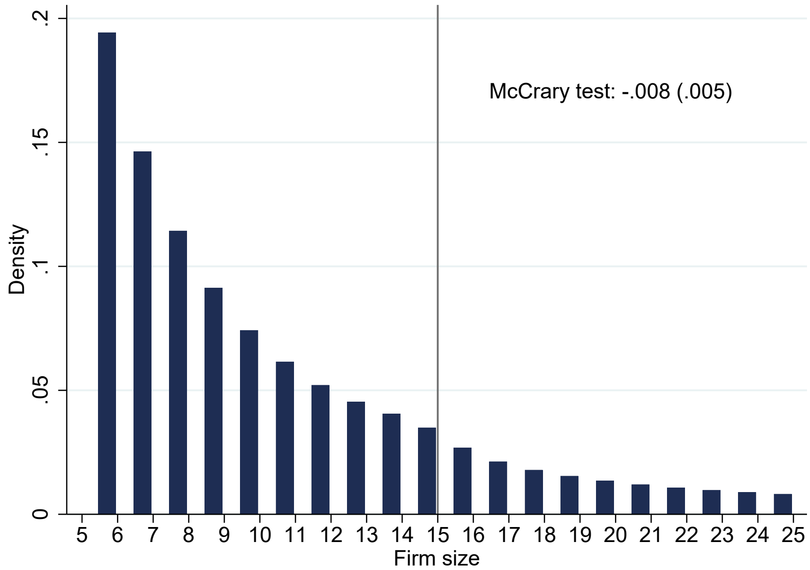
Notes: The graphs show the fraction of firms receiving STW in each calendar year $t \in [2005, 2014]$ by eligibility status and maximum 6-month average FTE firm size in year $t-1$. Panel A plots, among firms with eligible INPS codes in our sample, the evolution of the fraction of firms receiving STW 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-4: EFFECTS OF SHORT TIME WORK BY PREDICTED LAYOFF-RISK SCORE



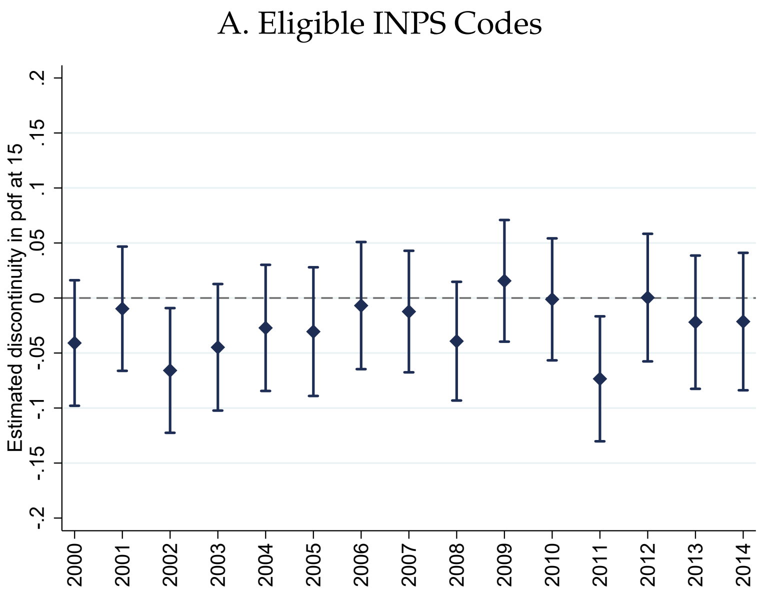
Notes: The graphs show heterogeneity in STW take-up and treatment effects by a score of the predicted probability that a firm experiences a mass layoff. The prediction model for the probability of mass layoff is described in Section 3.2. We rank firms into the four quartiles of the distribution of this score, and estimate specification (3) on the sample of firms in each quartile. Panel 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. 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.

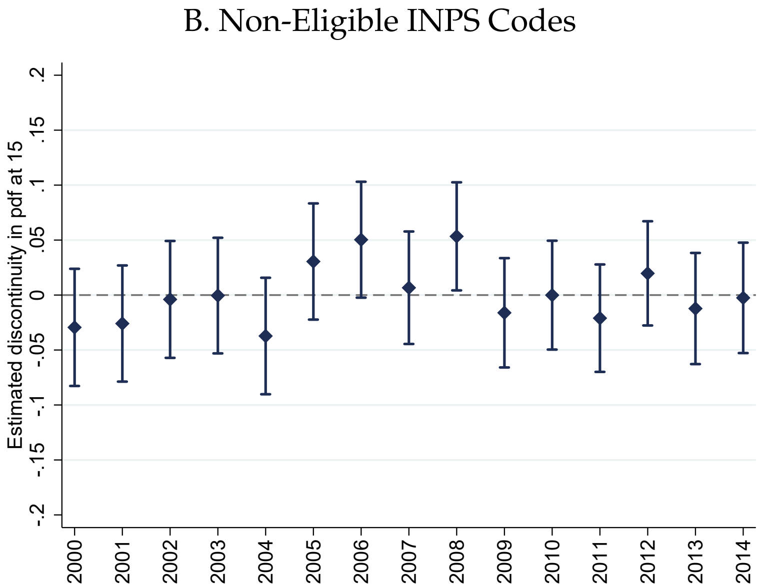
Figure A-5: Distribution of Firms' FTE Size (2000-2014)



Notes: The graph shows the probability density function of FTE firm size by 1-unit bins for the years 2000-2014. 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 FTE units.

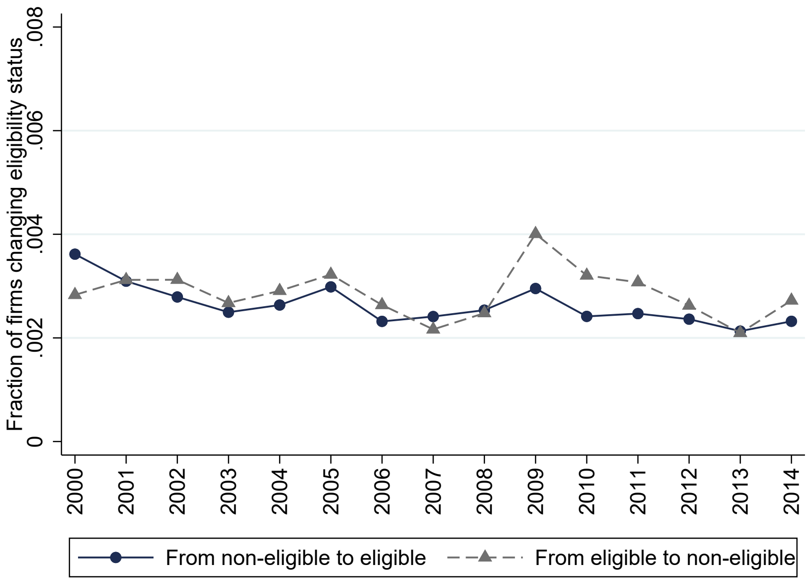
Figure A-6: McCrary Test Statistic of Discontinuity in Firm Size Distribution





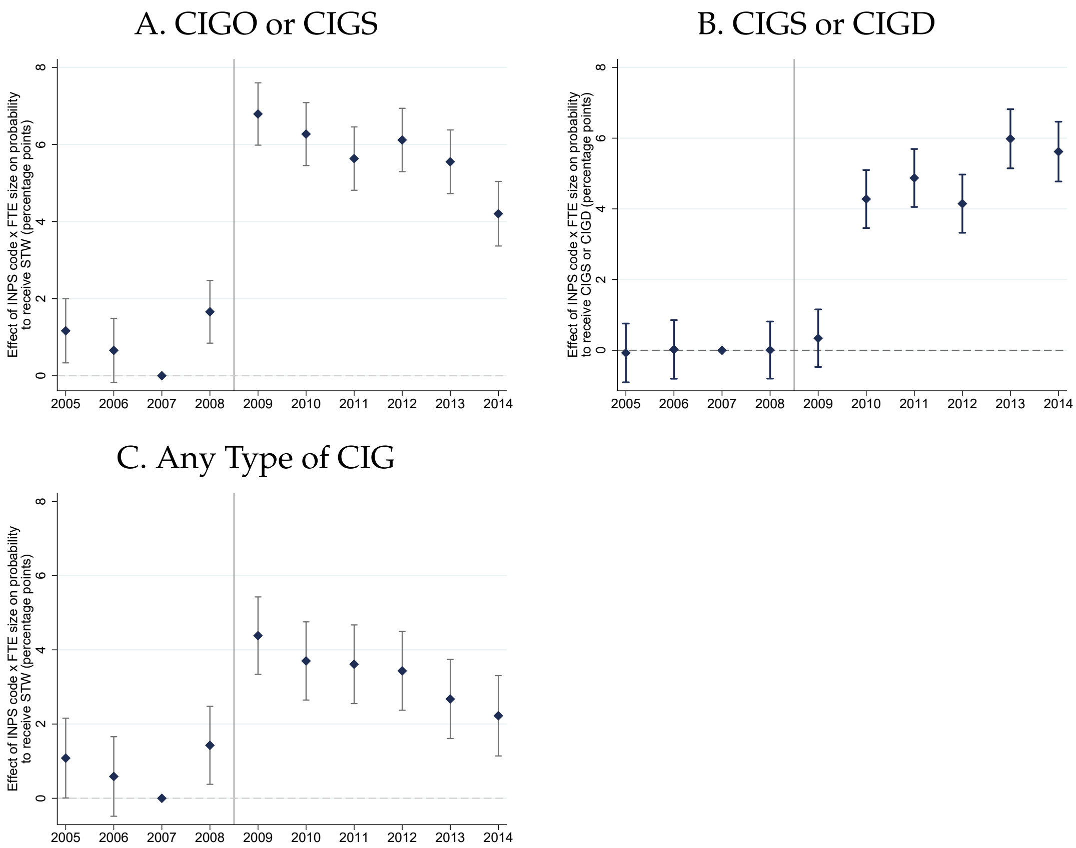
Notes: The graphs report the McCrary test statistic for the presence of a discontinuity in the probability density function of FTE size at 15 and its confidence interval for each year $t \in [2000, 2014]$, and for eligible and non-eligible INPS codes separately. The vertical bars indicate 95% confidence 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 FTE units.

Figure A-7: 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-8: Firms' Probability of Receiving CIGO, CIGS or CIGD by Firm Size & INPS Code



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 (i) CIGO or CIGS receipt in Panel A, (ii) CIGS or CIGD receipt in Panel B and (iii) any CIG (CIGO, CIGS or CIGD) receipt in Panel C at the firm level as outcome. 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.

Table A-4: EFFECTS OF 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. TREATMENT ON FIRMS' AND WORKERS' OUTCOMES: SAMPLE OF FIRMS MATCHED TO BALANCE-SHEET DATA

	Estimate (1)	Std Error (2)	N (3)
	A. First Stage		
Probability of CIGS take-up	.042	(.002)	873839
	B. Employment Outcomes (IV)		
Log number of hours per employee	-.374	(.054)	873839
Log number of full-time weeks per employee	-.278	(.051)	873839
Log firm size (headcount)	.421	(.072)	873839
Log wage rate	.094	(.051)	873839
Log wage bill per employee	-.305	(.074)	873839
Log number of open-ended contracts	.597	(.087)	873839
Log number of fixed-term contracts	-.830	(.240)	873839
Rate of inflows	-.143	(.063)	873839
Rate of outflows	-.016	(.093)	873839
Firm survival probability (in $t+1$)	.032	(.020)	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 $t+1$: $\widehat{\beta}_{IV}/\bar{Y}$. The sample includes firms that have been linked to their balance-sheet data from CERVED.

Table A-5: EFFECTS OF OVERALL SHORT TIME WORK (CIG) TREATMENT ON FIRMS' OUTCOMES

	Estimate (1)	Std Error (2)	N (3)
Panel I. Baseline specification			
	A. First Stage		
Probability of any CIG take-up	.026	(.002)	2843205
Probability of CIGO take-up	.023	(.002)	2843205
Probability of CIGD take-up	-.023	(.002)	2843205
Probability of CIGO or CIGS take-up	.049	(.002)	2843205
	B. Employment Outcomes of CIG Treatment (IV)		
Log number of hours per employee	-.534	(.086)	2843205
Log number of full-time weeks per employee	-.553	(.083)	2843205
Log firm size (headcount)	.377	(.101)	2843205
Log wage rate	-.015	(.059)	2843205
Log wage bill per employee	-.693	(.107)	2843205
Log number of open-ended contracts	.441	(.106)	2843205
Log number of fixed-term contracts	-.557	(.276)	2843205
Firm survival probability (in $t+1$)	.069	(.023)	2843205
Panel II. Alternative specification			
	A. First Stage		
Probability of any CIG take-up	.078	(.006)	300795
	B. Employment Outcomes (IV)		
Hours per employee (inverse hyperbolic sine)	-.302	(.080)	300795
Firm size headcount (inverse hyperbolic sine)	.306	(.154)	300795
Firm survival probability	.291	(.045)	300795

Notes: Panel I.A reports the estimates of the coefficient $\widehat{\kappa}_1$ from our baseline first-stage specification (3) using various definitions of CIG treatment as outcome. Associated cluster-robust standard errors are reported in parenthesis. Panel I.B reports the $\widehat{\beta}_{IV}$ coefficients estimated from our baseline IV specification (2) using the take-up of any CIG program as treatment, for a set of different firm-level outcomes. Panels II.A and II.B report coefficient estimates (and associated cluster-robust standard errors) from our alternative specification illustrated in equations (6) and (5). Treatment is defined as take-up of any CIG program.

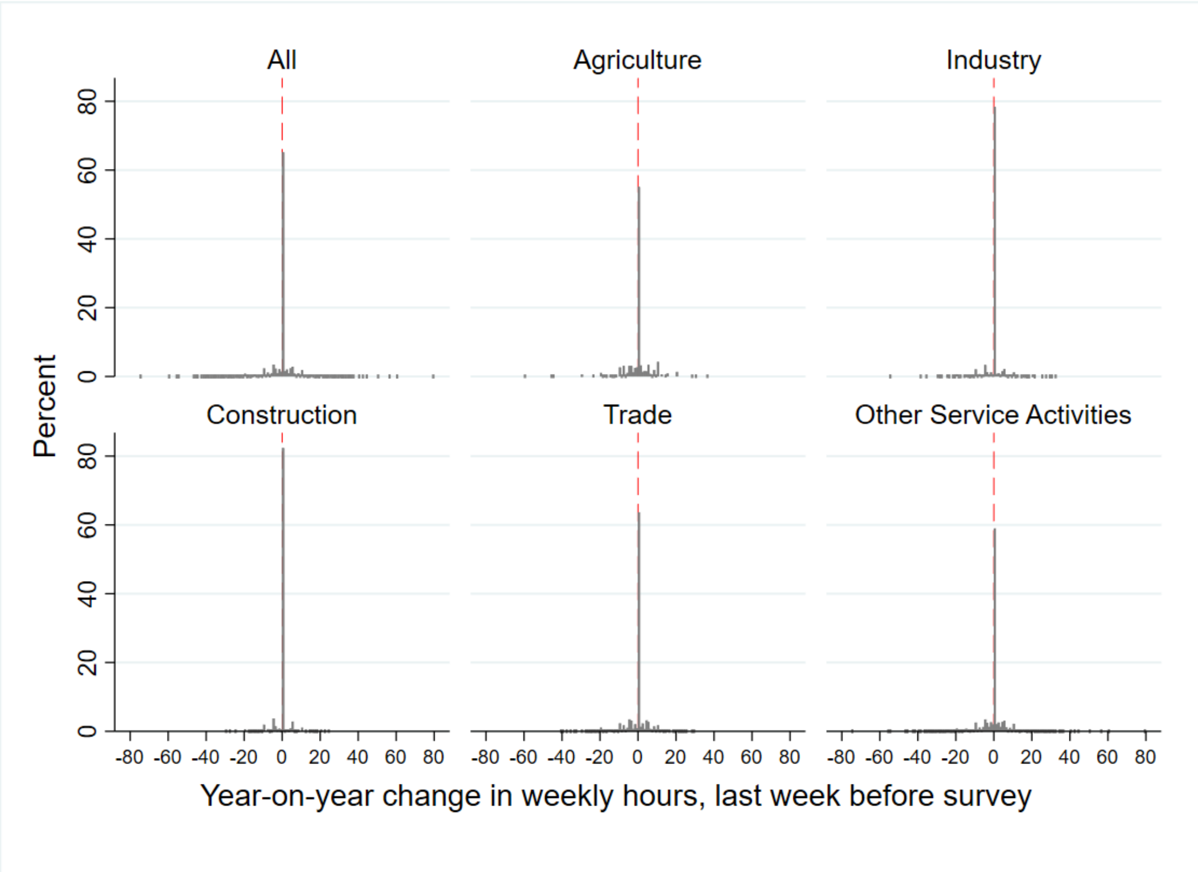
Appendix B: Sources of Layoff Inefficiencies - Additional Details

B.1 Bargaining Efficiency

Figure B-1: HOUR AND WAGE RIGIDITIES

Notes: Panels A and B report the empirical distribution of the year-on-year change in log hourly wages and contractual weekly hours for the years 2010-2014. Year-on-year changes are binned into bins of 1-unit width. Contractual weekly hours can be directly observed in the INPS data starting from 2009 and correspond to the number of hours of work specified in the contract. Hourly wages are computed dividing contractual monthly earnings by contractual weekly hours (assuming 4.3 weeks per month). Both contractual monthly earnings and contractual weekly hours can be observed in the INPS data from 2009. Year-on-year changes are based on the values observed in March of each year. The sample is restricted to workers employed in occupations eligible for STW in non-eligible firms, and who are employed in the same firm over two consecutive years.

Figure B-2: Hour Rigidities by Sector (Labor Force Survey Data)



Notes: The figure reports the empirical distribution of the year-on-year change in weekly hours worked for the years 2005-2014 by sector using data from the Italian Labor Force Survey. Year-on-year changes are binned into bins of 1-unit width. Weekly hours are self-reported actual hours worked in the week before the survey, conditional on having worked in that week. The sample is restricted to workers employed in occupations eligible for STW over two consecutive years and working at firms with less than 15 employees, i.e. that are not eligible for STW. In order to select workers who likely stayed in the same job over two consecutive years, we restrict the sample to workers who were in the same occupation and sector in t and $t-1$, and who were employed under a permanent contract in both periods.

B.2 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:

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

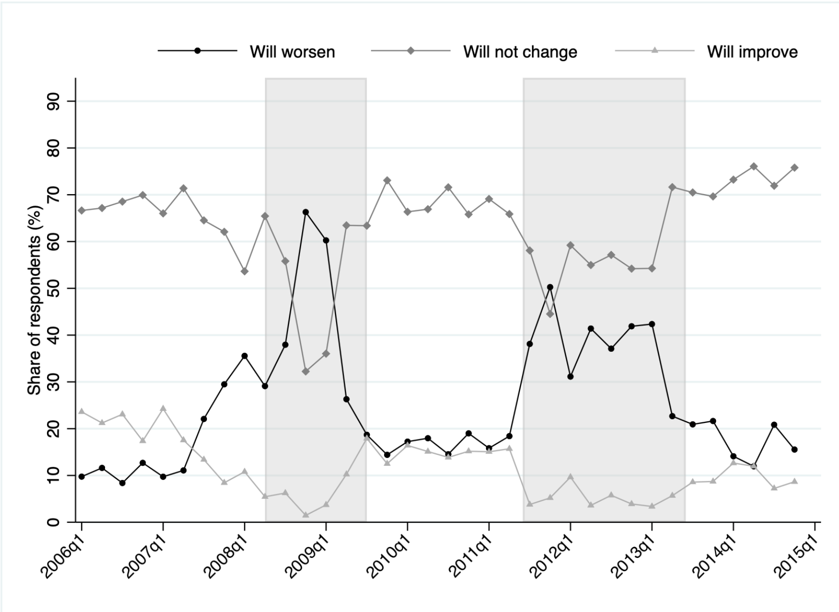
where $\varepsilon_{n,\tau}$ is the elasticity of employment to the STW subsidy, and $\varepsilon_{h,\tau}$ is the elasticity of hours to the STW subsidy. Calibrating the value of the fiscal externality using our estimates of the elasticity, a UI replacement rate of 70%, an STW replacement rate of 80% and a ratio of STW hours to full-time hours of 35% as per our results in Panel B of Figure A-2, we obtain a value of the fiscal externality of 1.38.

Appendix C: Dynamic Treatment Effects

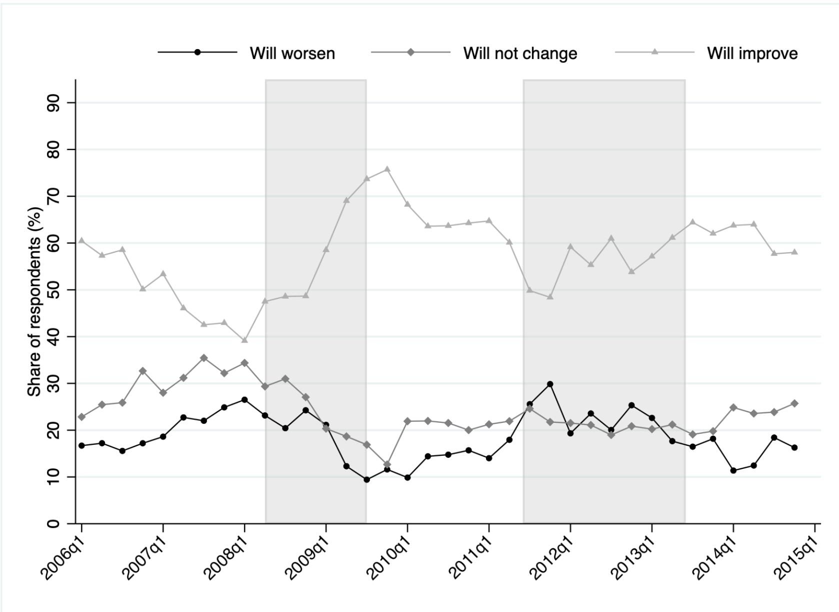
C.1 Persistence of the Recessionary Shock

Figure C-1: FIRMS' EXPECTATIONS ABOUT BUSINESS CONDITIONS

A. Over the Next Three Months

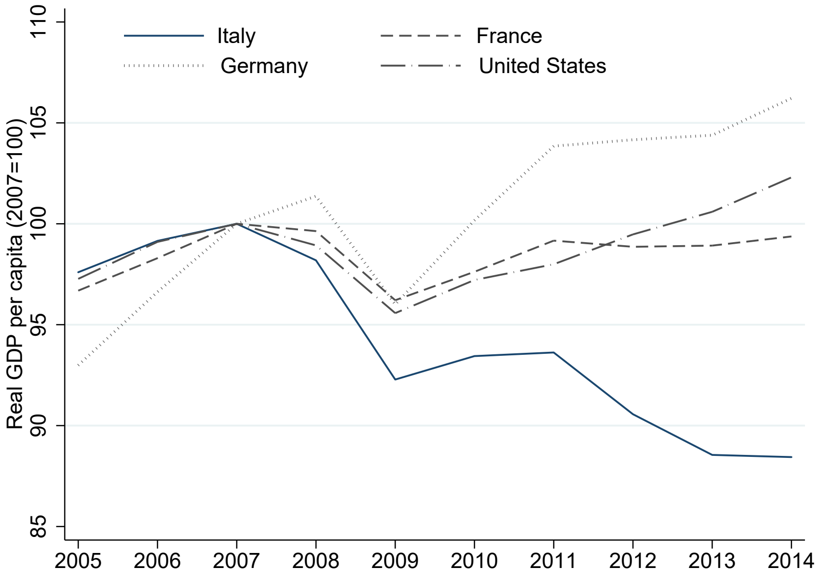


B. Over the Next Three Years



Notes: The graphs report evidence on answers to the question "How do you think business conditions for your company will be in the next 3 months?" (Panel A) and "in the next three years?" (Panel B). The shaded areas indicate recessionary periods, as identified by the FRED's OECD based Recession Indicators. Data come from the Bank of Italy Survey on Inflation and Growth Expectations.

Figure C-2: 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-On-the-Treated 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-3 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:

$$\begin{aligned}
Y_{igst} = \; & \sum_j \beta_j^{RF} \cdot Z_{2009} \cdot \mathbb{1}[j=t] \\
& + \sum_j \sum_k \gamma_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j=t] \Big\} \cdot \mathbb{1}[k=s] \\
& + \sum_j \sum_k \gamma_3^{jk} \cdot \Big\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j=t] \Big\} \cdot \mathbb{1}[k=s] \\
& + \sum_j \sum_k \gamma_4^{jk} \cdot \Big\{ \mathbb{1}[j=t] \Big\} \cdot \mathbb{1}[k=s] + v_{igst}
\end{aligned} \tag{10}$$

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

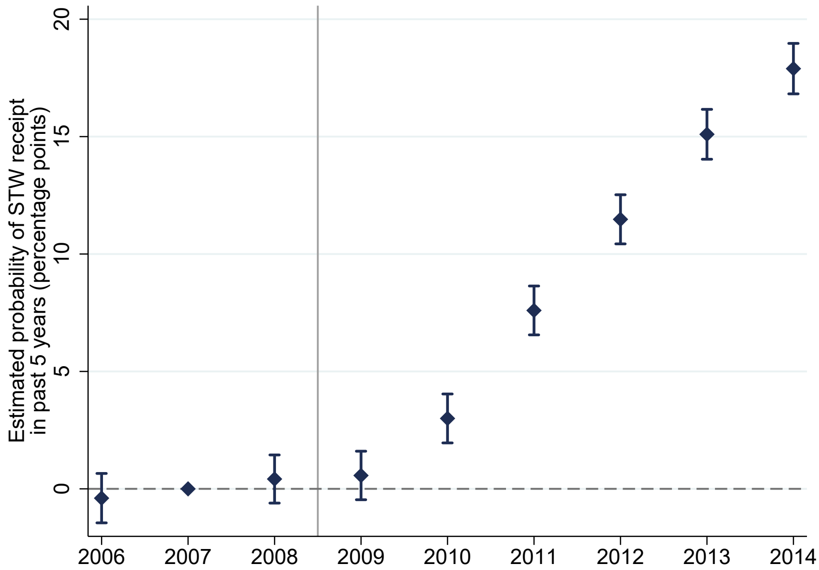
$$\beta_{2009}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{11}$$

$$\beta_{2010}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{12}$$

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 $\widehat{\beta}_t^{RF}$ and the recursive structure of equations (11), (12), etc., we can identify the sequence of dynamic treatment effects $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$.

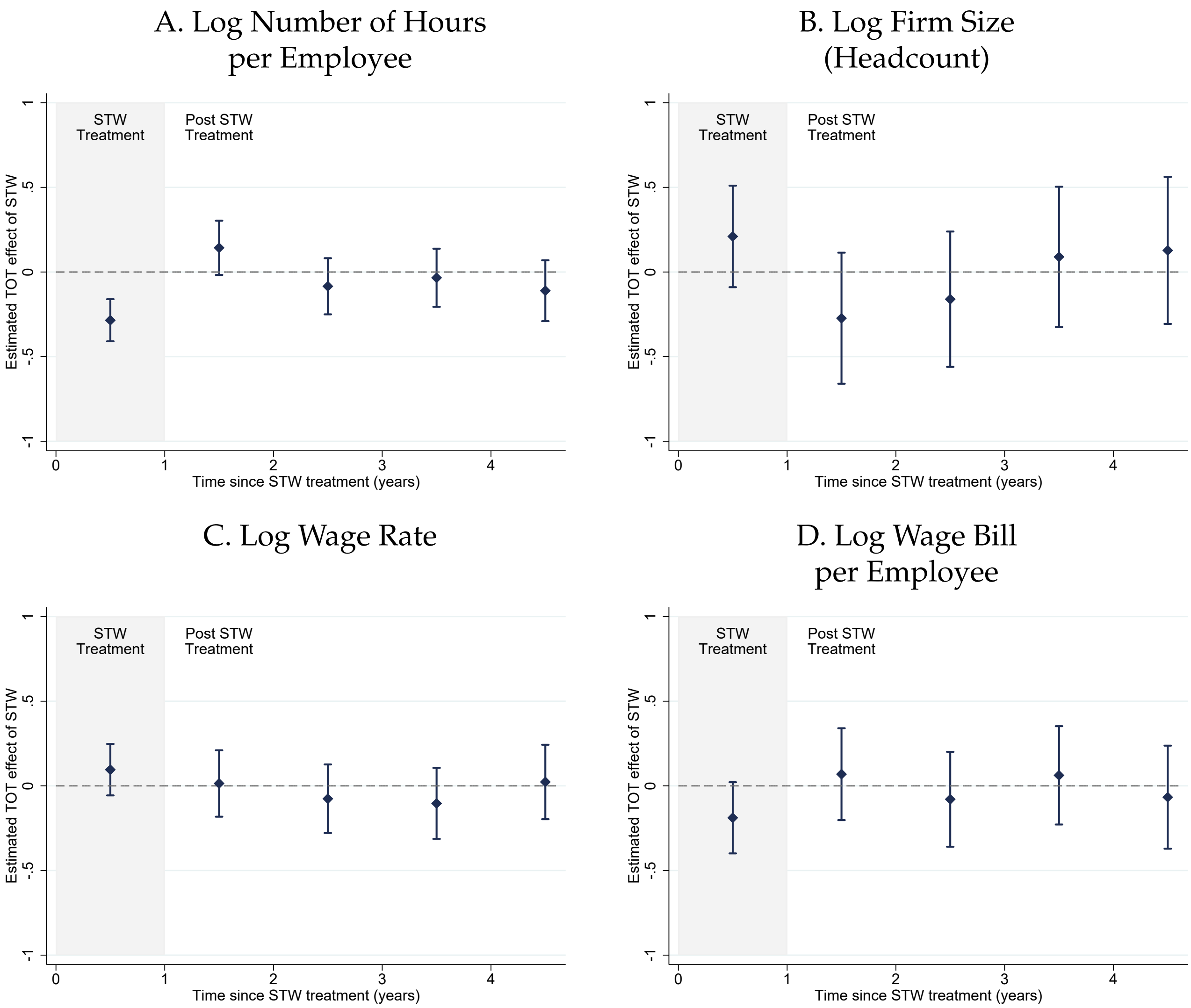
We display in Appendix Figure C-4 the results of these dynamic TOT effects, for various outcomes. The results suggest that the effects are large on impact, but disappear immediately once treatment stops.

Figure C-3: 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-4: 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.

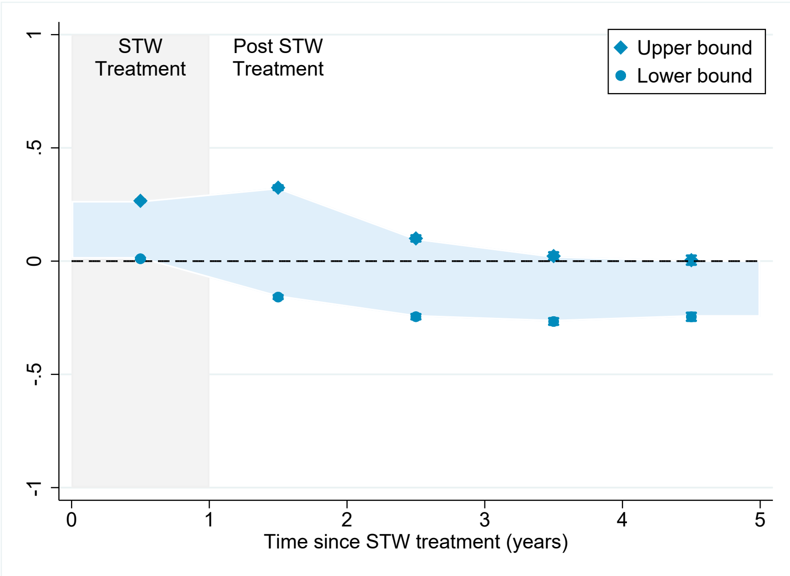
Counterfactual 1: A similar worker at time $t-1$ from any non-eligible firm due to firm size and INPS code. Under the assumption that only the worse shocks select into STW, that is $\mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k} | \text{Counterfact 1}] \geq \mathbf{E}[\varepsilon_{j,t+k} + \mu_{i,t+k} | \text{STW Treated}]$, 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 3.2, 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.

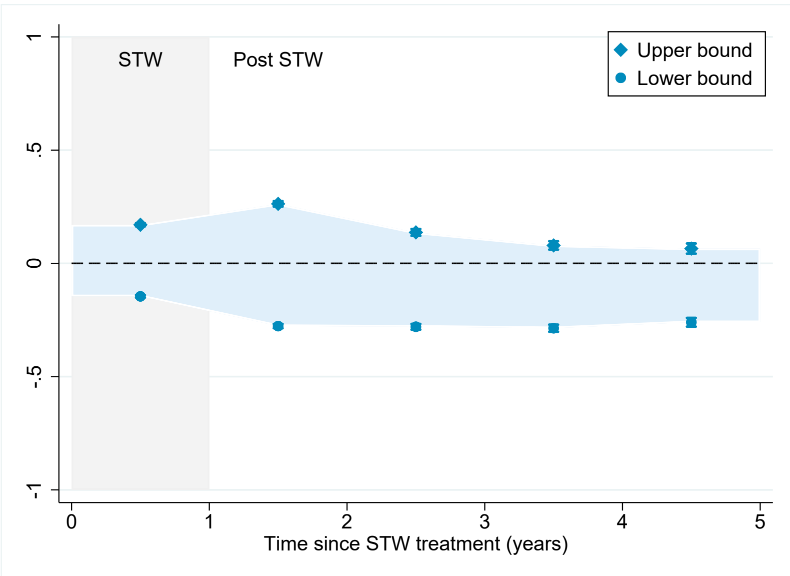
In Appendix Figure C-5, we overlay the upper bound and lower bound estimates from the event study approach. In Panel A, we show the effect for employment, and in Panel B the effect on worker's total gross earnings plus transfers. The graphs show that, in both cases, the upper bound estimate – which compares 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 8.

Figure C-5: DYNAMIC EFFECTS OF SHORT TIME WORK 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 8. 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.

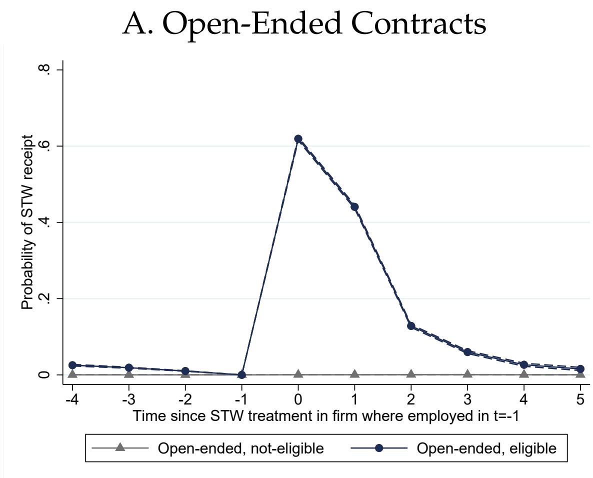
Accounting for Firm-level Shocks Using Variation in Treatment by Contract Type A second way in which we can address concerns related to the correlation between STW treatment and persistent firm-level shocks is by using variation in STW exposure between workers on open-ended and fixed-term contracts within the same firm. This allows us to control for firm fixed effects and thus improve on our identification of the dynamic effects of STW on workers' careers.

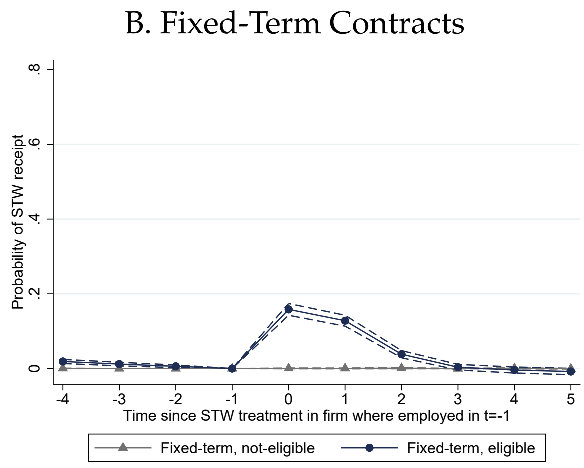
Appendix Figure C-6 shows the probability of STW receipt for workers around the time when a firm experiences a STW event. In Panel A we focus on workers who are on open-ended contracts, while in Panel B on workers who are on temporary contracts in the year before the event. In both panels, we also report the evolution of the probability of STW receipt among a control group of workers who have similar observable characteristics but work in firms that are not eligible for STW at time $t = -1$. The figure shows very clearly that the probability of STW take-up is much larger among workers on open-ended contracts than among workers on temporary contracts, conditional on the firm going into STW. This is in line with the theoretical predictions – discussed at the end of Section 2.1 – whereby firms have incentives to put open-ended-contract workers (but not temporary workers) on STW.

The main advantage of using variation in access to STW between temporary vs open-ended contracts within the same firm is that it allows to fully control for firm-level shocks $\varepsilon_{j,t+k}$. Yet, variation in $T_{jt} = 1$ will now be driven by the nature of the contract individuals had at time t, which may be correlated with $\mu_{i,t+k}$ as individuals on temporary vs open-ended contracts have different labor market dynamics in general. We can nevertheless control for these differences in workers' dynamics absent STW by comparing workers in open-ended contracts vs workers in temporary contracts in non-eligible firms that did not experience STW in time t.

We report in Appendix Figure C-7 below the evolution of the differential probability of employment of workers employed in open-ended vs fixed-term contracts in event time -1 in firms experiencing a STW event for the first time at event time 0, relative to similar workers in non-eligible firms. The figure shows clear positive effects of STW on worker's employment in the short run, but these effects dissipate entirely after STW exhaustion. These results provide strong, transparent and complementary evidence on the dynamic effects of STW, confirming that STW had positive effects in the short run, but that these effects did not last.

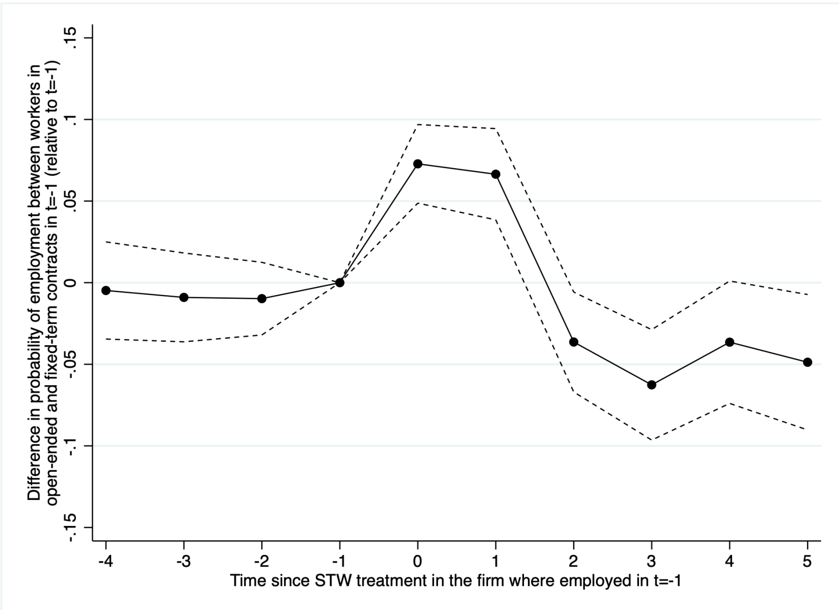
Figure C-6: PROBABILITY OF SHORT TIME WORK RECEIPT BY CONTRACT TYPE





Notes: The graphs report the estimated coefficients of event study regressions for the probability of STW receipt at the worker level, for workers on different contract types and different event-year definitions. All estimates are relative to event-year -1. 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. The treatment group (indicated by solid circles) consists of workers who, at event time $t = -1$, were employed in eligible firms that experienced their first STW spell in $t = 0$. In other words, for the treatment group, an event year is defined as the first year in which the firm where the worker was employed in $t = -1$ experiences a STW event, conditional on said firm to be eligible for STW at event time -1. We consider eligible firms with FTE size $\in (15; 25]$ in $t = -1$ in our sample. We distinguish two groups of treated workers: those employed under an open-ended contract in -1 (Panel A), and those employed under a fixed-term contract in -1 (Panel B). For each of these two treatment groups, we define a comparison group. The comparison group (indicated by solid triangles) consists of workers employed at non-eligible firms with 6-month average FTE size $\in (15; 25]$ at event time -1. Individuals in the comparison group are matched to individuals in the treatment group using Mahalanobis nearest-neighbor matching without replacement based on gender, age, job characteristics (including contract type) 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. The probability of STW received is measured unconditional on employment in the same firm as $t = -1$ and unconditional on employment.

Figure C-7: DYNAMIC EFFECT OF SHORT TIME WORK RECEIPT ON THE PROBABILITY OF EMPLOYMENT: USING CONTRACT TYPE AS SOURCE OF WITHIN-FIRM VARIATION IN EXPOSURE TO STW



Notes: The graph reports the evolution of the differential probability of employment of workers employed in open-ended vs fixed-term contracts in $t = -1$ in firms experiencing a STW event for the first time in $t = 0$. More precisely, each dot reports the coefficient estimates of a regression of the employment probability on the full set of interactions between contract type, treatment/comparison status (as defined in the notes to Figure C-6) and event time, conditional on individual fixed effects. Estimates are relative to $t = -1$. The dashed lines around the estimates indicate 95% confidence intervals based on robust standard errors clustered at the individual level.

C.4 Heterogeneous Treatment Effects by Temporariness of the Shock

We are interested in understanding how the treatment effects of STW depend on the persistence of the shock experienced by firms. To this end, we start by deriving a data-driven characterization of industries and local labor markets (LLM) that have experienced more or less permanent shocks. We then document heterogeneity in the long-run effects of STW by the temporariness of the shock.

We start by constructing panels of total employment counts in each year at the LLM or 3-digit industry level, using data on non-eligible firms, irrespective of firm size. For each panel separately, we estimate regressions of the following form

$$\Delta \log e_{j,2007-2014} = \alpha_S + \beta_S \Delta \log e_{j,2007-2009} + \varepsilon_j \tag{13}$$

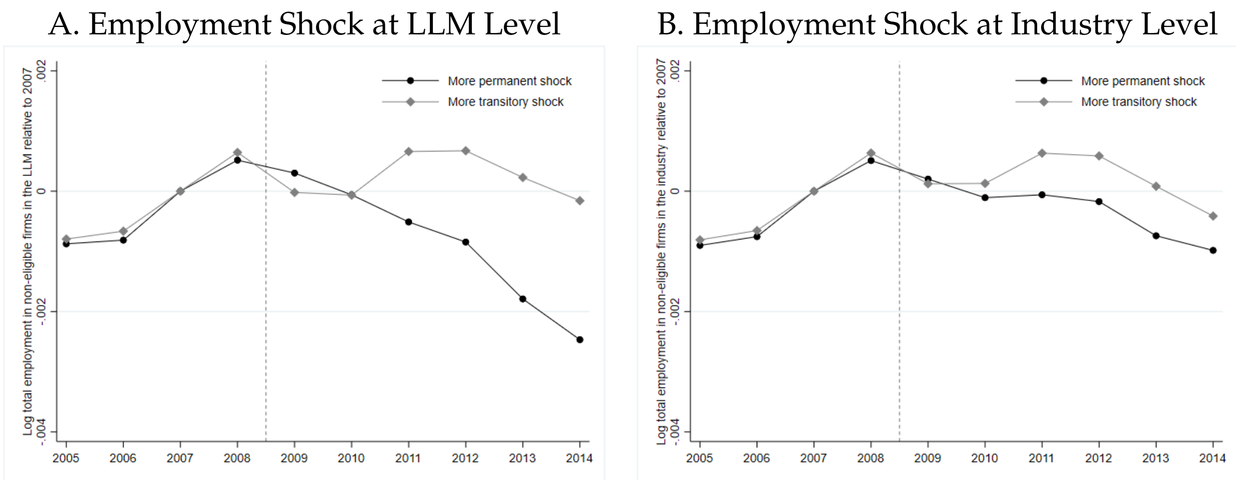
where $\Delta \log e_{j,2007-2014}$ is the change in the logarithm of total employment in LLM or industry j between 2007 and 2014, $\Delta \log e_{j,2007-2009}$ is a similar change between 2007 and 2009, and ε_j is an error term. The coefficient β_S captures the average correlation between short-run and long-run employment growth: in other words it measures the average magnitude of the shock in employment due to the double-dip recession between 2009 and 2014, expressed as the extent of the deviation from pre-crisis employment growth. Having estimated model (13), we rank LLMs/industries into quantiles of the distribution of the residuals, with more negative values of the residual term indicating more persistent shocks. Here we exploit the notion that the residual term should be positive for LLMs/industries in which the shock is relatively more temporary/less permanent, and negative for LLMs/industries in which it is more persistent.

Note that we estimate model (13) and rank LLMs/industries using the sample of non-eligible firms (but then extend the ranking to both eligible and non-eligible firms). Also, when running the regression at the LLM level, we control for the fraction of workers eligible for STW in the LLM in pre-recession years to account for any spillover effect of STW take-up between eligible and non-eligible firms. This ensures that we measure the transitoriness of the shock on sectoral or LLM employment in the absence of STW.

Appendix Figure C-8 shows the evolution of the logarithm of total employment in non-eligible firms at the LLM (Panel A) and industry (Panel B) level relative to 2007, distinguishing between LLMs/industries with predicted residuals above and below the median level, i.e. with a more transitory and permanent employment shock respectively. The graphs provide supporting evidence for our proposed approach to identify employment shocks of different persistence. It shows that LLMs (industries) that we classify as subject to more transitory shocks experienced a similar decline at the onset of the Great Recession compared to LLMs (industries) that we classify as subject to more persistent shocks; but the former LLMs (industries) recovered starting in 2010, while the latter remained persistently affected.

We then use the above dichotomization to investigate whether STW take-up and treatment effects are heterogeneous with respect to the temporariness of the shock. To this effect, we run models based on specification (5) and (6). The results are reported in Appendix Table C-1, where we consider any CIG take-up as treatment. Panel A shows heterogeneity with respect to the temporariness of the shock at the LLM level, Panel B at the industry level. Estimates in both panels indicate that, when the shock is more temporary, firms take up STW more and the employment effects of STW are larger. The magnitude of the effects is qualitatively important and similar across the two estimation, but estimated with not enough precision to be significant at conventional levels. The effect on hours per employee (conditional on employment) does not appear to be heterogeneous by type of shock.

Figure C-8: Evolution of Log Employment by Temporariness of the Employment Shock at the LLM and Industry Level



Notes: Each panel reports the evolution of log employment counts at the LLM (Panel A) or 3-digit industry (Panel B) level, relative to 2007. LLMs/Industries are split into two groups depending on whether the estimated residual from the estimation of model (13) $\hat{\varepsilon}_j = \Delta \log e_{j,2007-2014} - \hat{\alpha}_S - \hat{\beta}_S \Delta \log e_{j,2007-2009}$ falls above or below the median of the distribution of residuals among non-eligible firms.

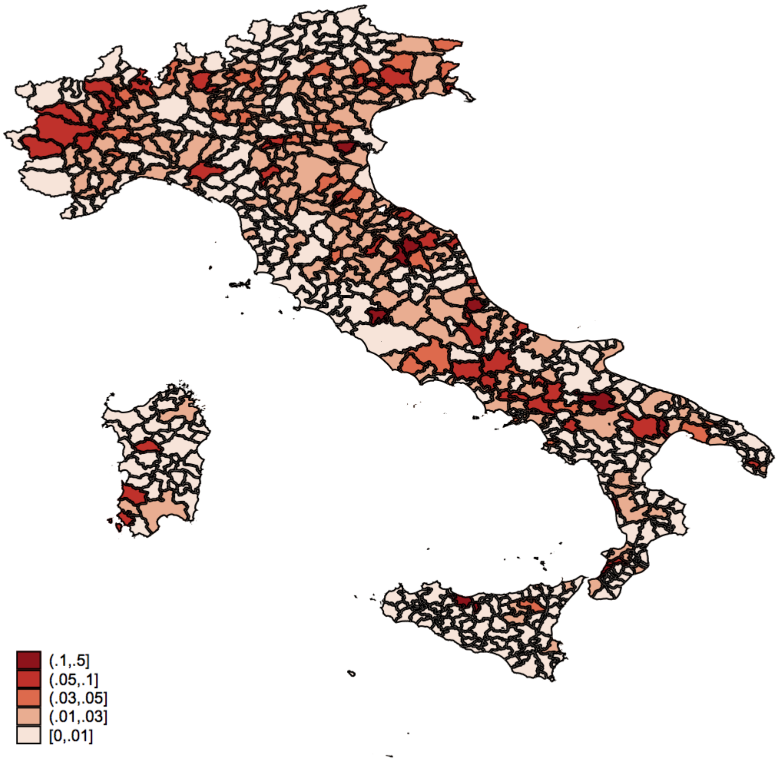
Table C-1: SELECTION OF FIRMS INTO SHORT TIME WORK (CIG) AND HETEROGENEOUS TREATMENT EFFECTS BY TEMPORARINESS OF THE EMPLOYMENT SHOCK

	Probability of CIG take-up (1)	Firm size headcount (inverse hyperbolic sine) (2)	Number of hours per employee (inverse hyperbolic sine) (3)
	A. Employment Shock at LLM Level		
$\mathbb{1}[N_{i,2008} > 15] \times \mathbb{1}[g \in \mathcal{E}]$	.061***		
	(.009)		
$\mathbb{1}[N_{i,2008} > 15] \times \mathbb{1}[g \in \mathcal{E}] \times$ Tempor.	.025**		
	(.012)		
$CIG^{2014-2010}$		.038	-.344*
		(.317)	(.176)
$CIG^{2014-2010} \times$ Tempor.		.367	.064
		(.359)	(.199)
	B. Employment Shock at Industry Level		
$\mathbb{1}[N_{i,2008} > 15] \times \mathbb{1}[g \in \mathcal{E}]$	.060***		
	(.009)		
$\mathbb{1}[N_{i,2008} > 15] \times \mathbb{1}[g \in \mathcal{E}] \times$ Tempor.	.032***		
	(.012)		
$CIG^{2014-2010}$		.062	-.315**
		(.300)	(.155)
$CIG^{2014-2010} \times$ Tempor.		.427	-.002
		(.349)	(.180)
Obs.	300795	300795	300795

Notes: Column 1 reports the estimates of the coefficient $\widehat{\lambda}_1$ and its associated cluster-robust standard error in parenthesis from an augmented version of specification (6) in which we include interaction terms with a dummy for whether the firm is in an LLM/industry in which the employment shock is estimated to be temporary. Columns 2-3 report the $\widehat{\theta}_{IV}$ coefficients estimated from a similarly augmented version of equation (5) and their associated cluster-robust standard errors in parenthesis for a set of different firm-level outcomes. Panel A shows heterogeneity with respect to the temporariness of the shock at the LLM level, Panel B at the industry level. In this table, we include any type of CIG under STW treatment.

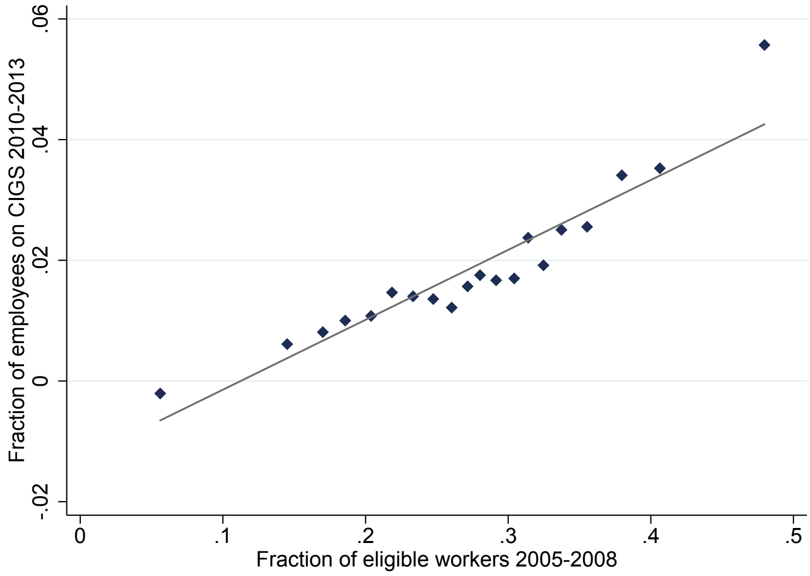
Appendix D: Selection & Spillover Effects - Additional Evidence

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 (LLMs), 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 FOR 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 for 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 5.3 for details). This relationship corresponds to the first stage of IV model (9).

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.[38]

[38] We note that this assumption, which greatly improves the tractability of the model, implies that the reallocation impact of STW will operate only through the job creation channel in the model. In practice,

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:

$$M(u_t, v_t) = \mu u_t^{\gamma} v_t^{1-\gamma} \tag{14}$$

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

$$q(\theta_t) = \frac{M(u_t, v_t)}{v_t} = \mu \left(\frac{u_t}{v_t}\right)^{\gamma} = \mu \theta_t^{-\gamma} \tag{15}$$

Log linearizing the above equation yields:

$$\ln(\frac{M}{v_t}) = \ln(\mu) - \gamma ln(\theta) \tag{16}$$

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 questions on the number of new hires that the firm would currently like to hire, 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.[39]

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:

$$v_{j,t} = \frac{n_{j,t}}{n_{j,t}^{RIL}} \cdot v_{j,t}^{RIL} \tag{17}$$

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:

our results in Table 1 shows that among firms taking up STW, headcount employment increases both through a relative increase in inflows and a relative decrease in outflows.

[39] The questions that we use are question C7 for 2010 and question C8 for 2015. Both are phrased in the same way and ask the firm how many employees it is currently trying to recruit (Quanti dipendenti sta attualmente cercando l'impresa?).

$$\log q_{j,t} = a + b \log(\theta_{j,t}) + c_j + \zeta_t + \nu_{j,t} \tag{18}$$

For b to identify $-\gamma$, exogenous variation in $\theta_{j,t}$ is required. We use exposure to CIGS treatment as an instrument. Intuitively, the intensity of CIGS 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:

$$\begin{aligned} \Delta \log q_{j,t} &= b \Delta \widehat{\log(\theta_{j,t})} + W_j' \mu_1 + \zeta_t + \nu_{j,t} \\ \Delta \log(\theta_{j,t}) &= Z_j^{2005-2008} + W_j' \mu_0 + \mu_{j,t} \end{aligned} \tag{19}$$

where Δ is the difference operator between pre vs post 2008.[40] 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 $\gamma = .53$.

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.

[40] 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.

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

$$\Pi(n_{t-1}) = \max_{v_t}\{\epsilon_k F(h_t, n_t) - wh_t n_t - cv_t + \beta\Pi(n_t)\} \tag{20}$$

subject to the law of motion of employment:

$$n_t = (1-\delta)\cdot n_{t-1} + q(\theta_t)\cdot v_t \tag{21}$$

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:

$$\epsilon_k F_n'(h_t, n_t) = wh_t + (1-\beta(1-\delta))\frac{c}{q(\theta)} \tag{22}$$

E.2.1 Identifying Production Function Parameters

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

$$F(h_t, n_t) = h_t^{\alpha} n_t^{\eta} \tag{23}$$

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:

$$\log n = \frac{\alpha}{1-\eta}\log h - \frac{1}{1-\eta}\log(wh) - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{whq(\theta)} + \frac{1}{1-\eta}\log(\epsilon_k\eta) \tag{24}$$

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

$$\log n = \frac{\alpha-1}{1-\eta}\log h - \frac{1}{1-\eta}\log w - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{whq(\theta)} + \nu_k \tag{25}$$

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 CIGS introduces some exogenous variation in the wage bill.[41] The new

[41] 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.

specification becomes:

$$\log n = \frac{\alpha}{1-\eta}\log h - \frac{1}{1-\eta}\log W - \frac{1-\beta(1-\delta)}{1-\eta}\frac{c}{W\cdot q(\theta)} + \nu_k \tag{26}$$

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 $\alpha = .6, \eta = .7$.

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 CIGS and who have at least one CIGS event after 2009. High productivity firms are those eligible but that do not take up CIGS 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 $\rho = .87$. 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:

$$W_k^e = u(c_k, h_k) + \beta(\delta W^u + (1-\delta)W_k^e) \tag{27}$$

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:

$$W^u = u(b, 0) + \beta(\phi(v_L W_L^e + (1 - v_L)W_H^e) + (1-\phi)W^u) \tag{28}$$

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:

$$u(c, h) = \frac{c^{1-\sigma_c} - 1}{1 - \sigma_c} - \varphi \frac{h^{1+\sigma_h}}{1 + \sigma_h} \tag{29}$$

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:

$$w(\epsilon) = w_s \epsilon^{w_a} \tag{30}$$

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 .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:

$$h(\theta, \epsilon) = h_s \epsilon^{h_a} \theta^{h_b} \tag{31}$$

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 CIGS. This model obtains a coefficient of .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 CIGS. 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 .2. The model's discount factor, β, is set to .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	.935
α	Hour share	.6
η	Labor share	.7
γ	Matching function curvature	.53
w_a	Wage function curvature	.2
$\bar{h}$	Total weekly hours endowment	40
δ	Separation rate	.2
b	Unemployment benefit	$.7 \cdot \bar{h} \cdot w_s$
τ	STW replacement rate	.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	.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	.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	.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: .108. Low productivity firms are defined as:

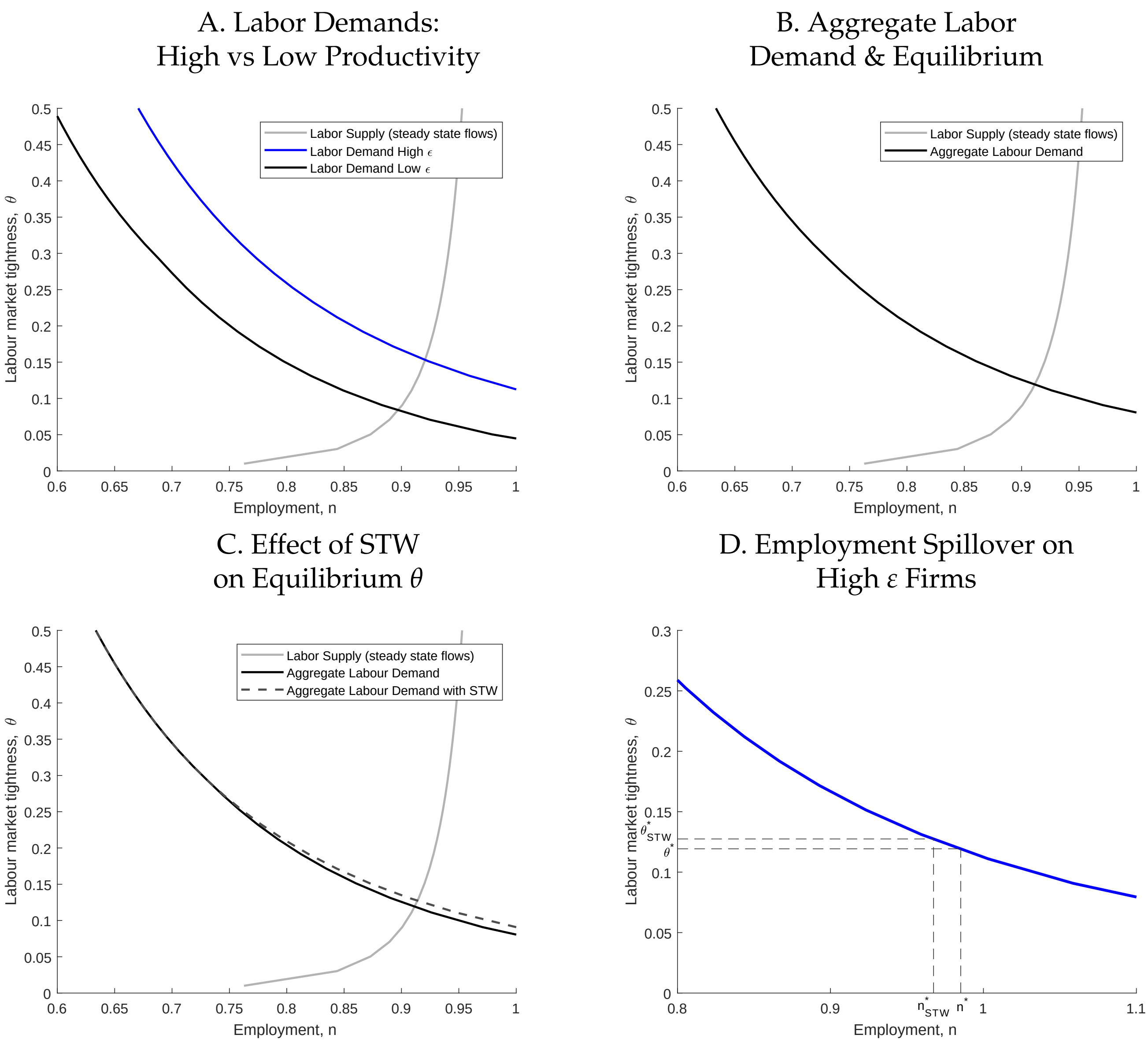
- For eligible firms, those that take up CIGS
- For non-eligible firms, in eligible 5-digit industries, firms whose total factor productivity is in the bottom 12% of the distribution, post 2009

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

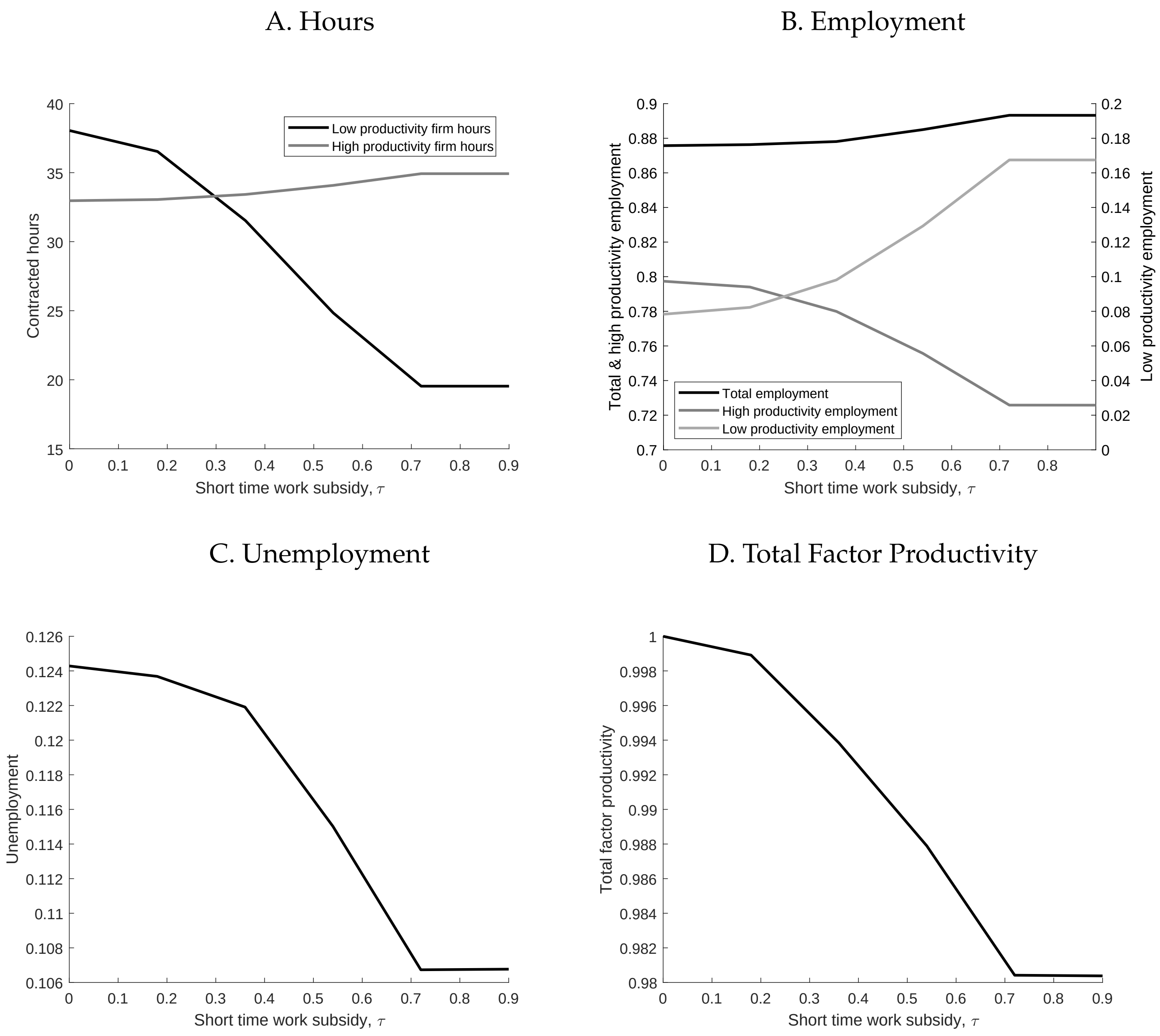
Our calibration relies on the thought experiment that we have a version of the Italian economy where all firms correspond to firms above 15 FTE and are eligible to STW. We use this model to explore how different levels of STW generosity would affect the equilibrium allocation in the labor market. In 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 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.

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.