Review of Economic Studies (2023) **90**, 1963–2005 https://doi.org/10.1093/restud/rdac069

Advance access publication 21 October 2022

Subsidizing Labour Hoarding in Recessions: The Employment and Welfare Effects of Short-time Work

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First version received February 2021*; Editorial decision June* 2022*; Accepted October* 2022 (*Eds.*)

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 labour hoarding during downturns and were aggressively used during the coronavirus disease 2019 (COVID-19) pandemic. Yet, very little is known about their employment and welfare consequences. This article leverages unique administrative social security data from Italy and quasi-experimental variation in STW policy rules to offer evidence on the effects of STW on firms' and workers' outcomes during the Great Recession. Our results show large and significant negative effects of STW treatment on hours, but large and positive effects on headcount employment. We then analyse whether these positive employment effects are welfare enhancing, distinguishing between temporary and more persistent shocks. We first provide evidence that liquidity constraints and rigidities in wages and hours may make labour 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.

Key words: Short-time work, Employment, Reallocation, Social insurance.

JEL Codes: H20, J20, J65

1. INTRODUCTION

The economic shock created by the coronavirus disease 2019 (COVID-19) pandemic generated a sudden revival of interest in policies aimed at encouraging labour 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 were aggressively used during the COVID-19 crisis, especially in European countries. Figure 1 reveals how swift and massive the

The editor in charge of this paper was Uta Schoenberg.

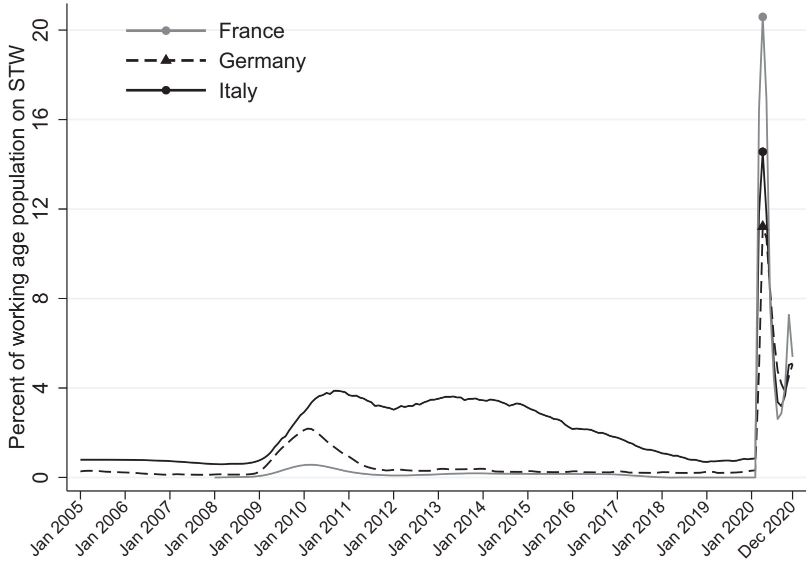

FIGURE 1

Labour 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, 2022). 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 article—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.

take-up of STW schemes was 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 was very different in the US. There, as evidenced by Figure 2, subsidized labour hoarding was almost non-existent, and most of the shock was 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 labour force was on STW programs in Europe in 2020, 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;

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

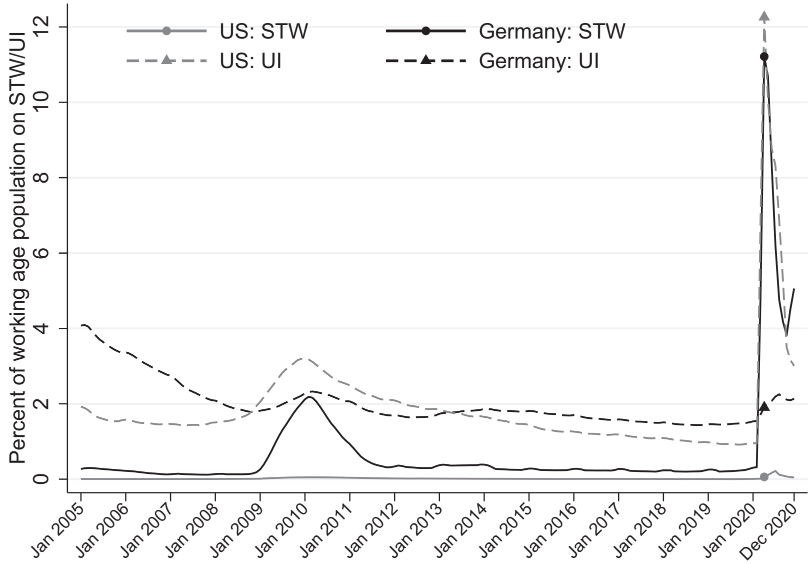

FIGURE 2

Labour 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 2022). 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.

Marinescu, 2017; Landais, Michaillat and Saez, 2018a; Landais, Michaillat and Saez, 2018b) 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, 1994; Van Audenrode, 1994; Boeri and Bruecker, 2011; Cahuc and Carcillo, 2011).

Even in the presence of firm-level data, the second issue lies in the lack of credible sources of identification of STW treatment. In almost all countries with STW programs in place, there is no variation in firms' eligibility for STW. The issue will be even more acute for the COVID-19 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 analyse 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),

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).

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 (2021) offer a credible IV strategy in the French context. They instrument STW take-up using the interaction between the approval rate of STW applications by the local authority—an indicator of the local administration efficiency—and a measure of the shock hitting each firm at the local level. 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 article contributes to our understanding of STW by addressing these limitations. It relies on 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 the optimal unemployment insurance trade-off, we show that STW must balance two additional, and empirically relevant, forces: layoff inefficiencies and reallocation inefficiencies.

Our data come from the Italian social security administration (INPS) and cover 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. The Italian STW policy, known as *Cassa Integrazione Guadagni* (CIG), comprises three schemes. We focus on *Cassa Integrazione Guadagni Straordinaria* (CIGS), which targets firms undergoing company crisis, restructuring, reorganization or severe demand shocks, and which has important similarities with STW programs used in other countries. CIGS has also the advantage of exhibiting variation in eligibility across firms, allowing 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 labour 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 liquidity constraints 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 3 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 labour market. To investigate this, we leverage the rich spatial variation available in Italy across more than 600 local labour 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 instrumental-variable strategy. Our results provide compelling evidence of the presence of equilibrium effects of STW within labour 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 labour 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 article 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 labour 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 AND DATA

2.1. *The Italian Cassa Integrazione Guadagni*

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 Great Recession: in 2013, almost 5% of the Italian workforce was on STW, for a cost of roughly 0.5% of Italian GDP. This massive expansion of STW take-up makes Italy the perfect laboratory to analyse 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 article, 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.

2.1.1. Cassa Integrazione Guadagni Straordinaria. 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 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

3. Hours not worked are computed against the regular hours stipulated in the labour contract. Normal weekly working hours are 40 in Italy. The benefit schedule applies homogeneously across worker types, with an 80% replacement rate up to a cap. The cap is established by law each year. In 2009, for example, the monthly cap was Euro 1,065.26. If a firm is eligible, all workers with at least 90 days of tenure are eligible to be put on CIGS, except for apprentices and managers. 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.

CIGS is minimal: they pay a fee to INPS equal to 3–4.5% 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 *et al.*, 2021). 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 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

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.

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 legislative developments.

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 versus non-eligible INPS codes. The main size requirement is that a firm must have employed on average more than 15 employees in full-time equivalent (FTE) units in the 6 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.

2.1.2. 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, and 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. Supplementary 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 2010–14. 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 2010–14. 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.

8. To provide just a few concrete examples of variation in eligibility within fine grained industry codes: within the 5-digit industry codes 11,306, 11,307, and 11,308, 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.

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 article, 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 6 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").

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 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 Supplementary Appendix Figure A-1, which reports the time series of authorized hours by program type from 2005 to 2014. Whilst it is 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.

2.1.3. 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 article, 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.

2.1.4. The Italian labour market: employment protection legislation and duality. To better contextualize this study, it is important to note that the Italian labour market is characterized by rigid employment protection legislation regulating the cost of dismissals. Over the period analysed in the article, 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, Mondauto and Scarpetta, 2017).[12]

In Section 3.1, we clarify that these rules do 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 subject to dismissal cost regulation irrespective of firm size 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.

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

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 labour trials in Italy.

As a direct consequence of strong employment protection rules, the Italian labour market features, like many European countries, a strong duality between open-ended and fixed-term (or temporary) contracts (Boeri, 2011; Daruich *et al.*, 2020). The costs of separating from workers with open-ended contracts is 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 their renewal. Moreover, temporary-contract workers are largely underrepresented in both unions and firm-level wage agreements (e.g. Bentolila, Dolado, Franz and Pissarides, 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 include detailed information on workers' demographics, working histories, participation in social assistance, and social insurance programs. It also provides detailed information on firms' characteristics such as employment, labour-force composition, and industry. Most importantly, starting from 2005, the data provide 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 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 from 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, a fourth of the Italian workforce, and a fourth of all STW spells between 2010 and 2014. Our sample of workers is a balanced panel of all workers ever working in these firms.

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

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

Supplementary Appendix Figure A-2 reports additional information on the distribution of treatment across workers in firms experiencing STW. Supplementary Appendix Figure A-2A 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. Supplementary Appendix Figure A-2B shows 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 AND 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.

Supplementary Appendix Figure A-3 provides direct evidence of this variation in access to CIGS by INPS codes and firm size. Supplementary Appendix Figure A-3A 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 15–25 FTE employees in year $t-1$ and for firms with a maximum 6-month average size of 5–15 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. Supplementary Appendix Figure A-3B 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:

$$Y_{igst} = \sum_j \gamma_1^j \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \right\}$$

14. Supplementary 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.

$$+\sum_j \sum_k \gamma_2^{jk} \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s]$$

$$+\sum_j \sum_k \gamma_3^{jk} \cdot \left\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s]$$

$$+\sum_j \sum_k \gamma_4^{jk} \cdot \left\{ \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s] + v_{igst}, \tag{1}$$

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 neighbourhood around the 15 FTE cutoff, 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 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:

$$Y_{igst} = \beta_{IV} \cdot T_{igst}$$

$$+\sum_j \sum_k \eta_2^{jk} \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s]$$

$$+\sum_j \sum_k \eta_3^{jk} \cdot \left\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s]$$

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

$$+\sum_j \sum_k \eta_4^{jk} \cdot \left\{ \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s] + \mu_{igst} \tag{2}$$

$$\begin{aligned}T_{igst} = &\kappa_1 \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t > 2008] \right\} \\ &+ \sum_j \sum_k \kappa_2^{jk} \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s] \\ &+ \sum_j \sum_k \kappa_3^{jk} \cdot \left\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s] \\ &+ \sum_j \sum_k \kappa_4^{jk} \cdot \left\{ \mathbb{1}[j=t] \right\} \cdot \mathbb{1}[k=s] + \nu_{igst}.\end{aligned} \tag{3}$$

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 labour 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*

Figure 3A 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 Supplementary 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.

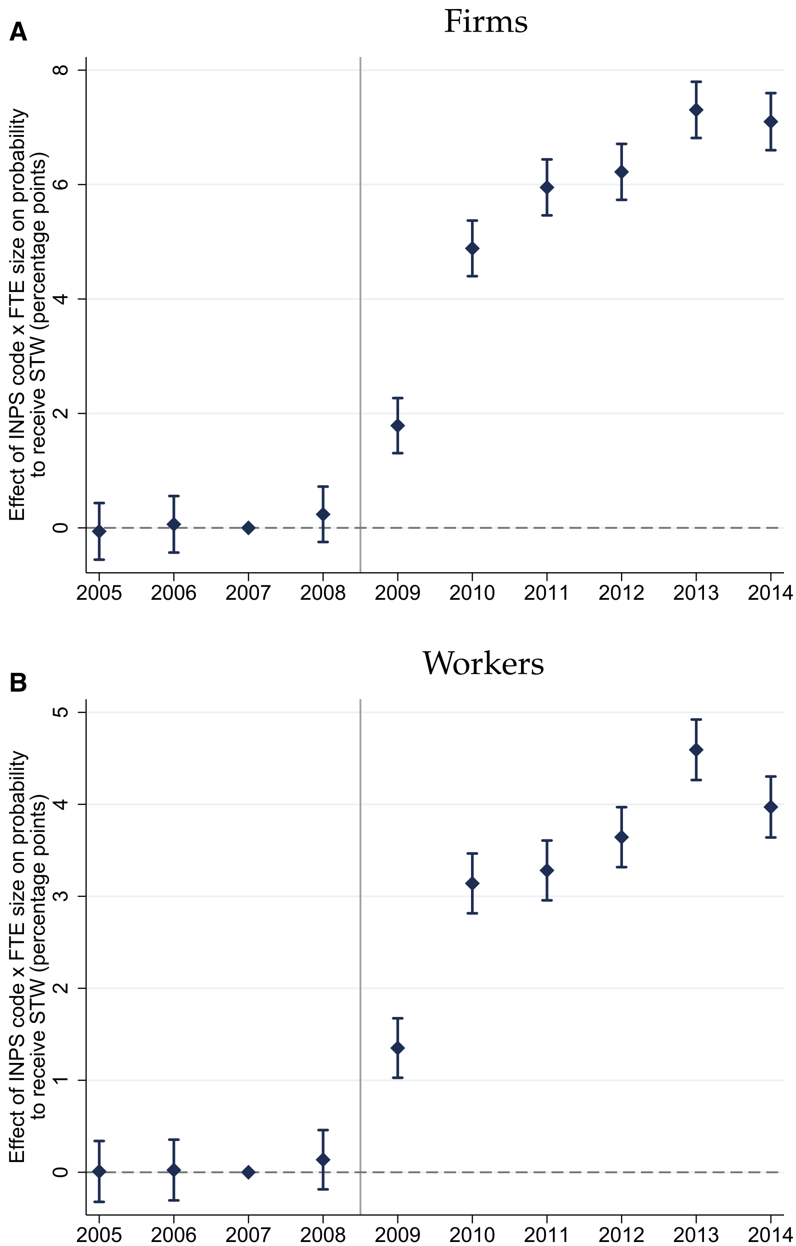

FIGURE 3

Firms' and 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 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).

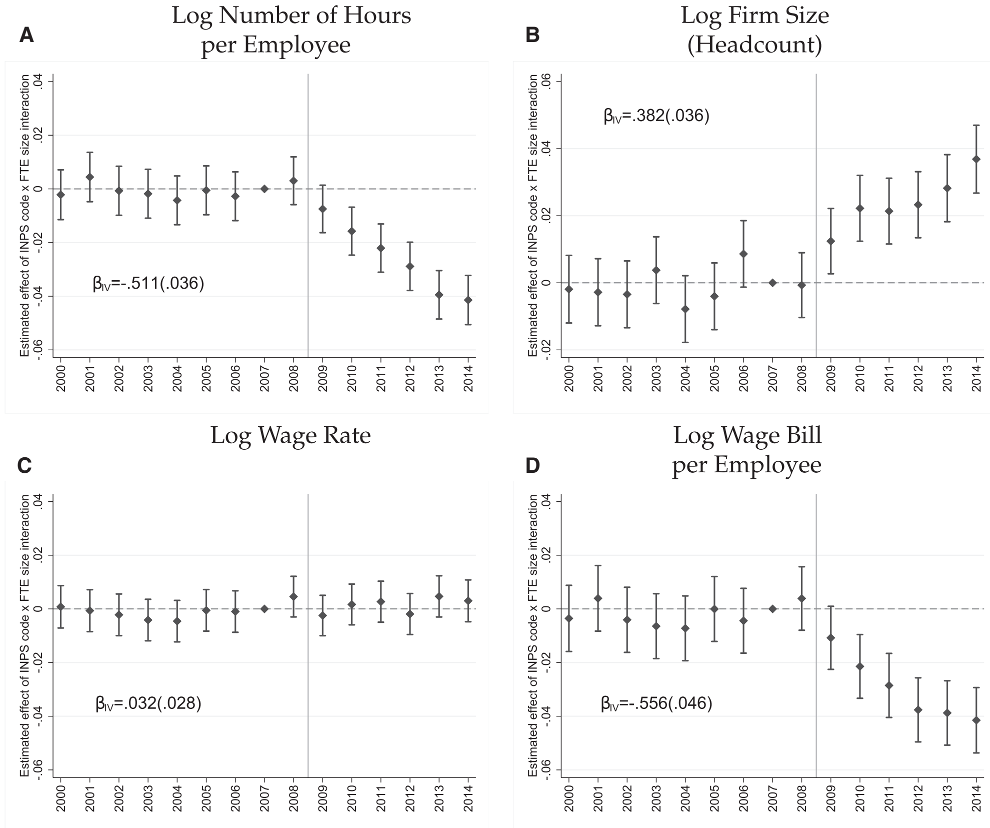

FIGURE 4

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.

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 of 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 $e^{-0.51}-1=40\%$ 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 $e^{0.38}-1 \approx 45\%$ 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.

3.2.1. 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 Supplementary Appendix Figure A-4, we report the first-stage estimate $\widehat{\kappa}_1$ from specification (3) in Supplementary Appendix Figure A-4A, and the IV estimates $\widehat{\beta}_{IV}$ from specification (2) in Supplementary Appendix Figure A-4B, splitting the sample by quartiles of the predicted probability 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 almost 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]

3.2.2. Dual labour markets. As explained in Section 2 above, the Italian labour 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 vs. the other depends on the expected productive length of a job match.

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 *et al.*, 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. 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 $\widehat{\beta}_{IV}=0.43$ (0.05). In contrast, the number of employees in fixed-term contracts is negatively impacted by CIGS treatment ($\widehat{\beta}_{IV}=-0.37$ (0.13)). This confirms that STW treatment interacts with the duality of the labour market and tilts the structure of employment towards open-ended contracts.

3.2.3. 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

16. A mass layoff is a layoff of at least five 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 five 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, labour productivity, a Whited-Wu index of credit constraints (see footnote 23), 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, 1-year lags and first differences. We estimate this model using LASSO on the sample of non-eligible firms with FTE firm size above 15.

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.

TABLE 1
Effects of STW treatment on firms' outcomes

	Estimate (1)	Std error (2)	N (3)
Panel I. Baseline specification			
	A. First stage		
Probability of CIGS take-up	0.054	(0.001)	2,851,216
	B. Employment outcomes (IV)		
Log number of hours per employee	−0.511	(0.036)	2,851,216
Log number of full-time weeks per employee	−0.461	(0.034)	2,851,216
Log firm size (headcount)	0.382	(0.036)	2,851,216
Log wage rate	0.032	(0.028)	2,851,216
Log wage bill per employee	−0.556	(0.046)	2,851,216
Log number of open-ended contracts	0.432	(0.047)	2,851,216
Log number of fixed-term contracts	−0.367	(0.128)	2,851,216
Rate of inflows	0.081	(0.599)	2,851,216
Rate of outflows	−0.337	(0.027)	2,851,216
Firm survival probability (in $t+1$)	0.104	(0.038)	2,851,216
	C. Balance-sheet and productivity outcomes (IV)		
Firm value added	0.095	(0.159)	881,276
Value added per worker	−0.508	(0.120)	881,276
Value added per hour worked	−0.057	(0.101)	881,276
Investment per hour worked	−0.677	(0.487)	881,276
Liquidity	0.939	(0.461)	881,276
Panel II. Alternative specification			
	A. First stage		
Probability of CIGS take-up	0.091	(0.004)	300,795
	B. Employment outcomes (IV)		
Hours per employee (inverse hyperbolic sine)	−0.258	(0.066)	300,795
Firm size headcount (inverse hyperbolic sine)	0.261	(0.131)	300,795
Firm survival probability	0.248	(0.037)	300,795

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).

between inflows and outflows, and report in Table 1 Panel I.B the separate effects of STW on the inflow and outflow rates.[18] 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

18. To look at the dynamics of flows, we employ the sample definition used for specification (5), to allow for following the same firm over time.

rate: STW decreases firms' outflow rate by 34%. Table 1 Panel I.B also reports the effect of STW on the probability of firm survival 1 year after treatment. The coefficient estimate is rescaled by the average survival probability in $t+1$. Results show that STW significantly increases the probability of survival by approximately 10%.

Table 1 Panel I.C 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.[19] 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 of 0.09 (0.16). We measure total output by firm value added, that is, total revenues plus unsold stocks minus cost of goods and services used in production.[20] 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 behaviour. One may indeed worry that collusive avoidance behaviour 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 behaviour 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*

3.3.1. 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 6-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 Supplementary 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.

19. Supplementary Appendix Table A-4 reports baseline results (corresponding to those in Table 1 Panels I.A and I.B) estimated on the sample of firms that were matched to their balance-sheet data and confirms 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.

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

TABLE 2
Robustness of baseline effects

	"Doughnut" regression	Only >15 FTE	Only ≤ 15 FTE	Only eligible	Only non-eligible	No dismissal rule change	
		[DiD 1]	[DiD 2] (placebo)	[DiD 3]	[DiD 4] (placebo)	>60FTE across Italy	50FTE threshold
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				A. First stage			
Probability of CIGS take-up	0.070	0.056	0.001	0.054	0.000	0.055	0.041
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.005)	(0.004)
				B. Employment outcomes			
	IV	IV	RF	IV	RF	IV	IV
Log hours per employee	−0.442	−0.431	−0.004	−0.191	0.018	−0.670	−0.104
	(0.038)	(0.037)	(0.002)	(0.033)	(0.003)	(0.230)	(0.160)
Log employment	0.214	−0.127	−0.032	0.428	0.000	0.848	0.149
	(0.046)	(0.042)	(0.002)	(0.050)	(0.003)	(0.297)	(0.202)
Log wage bill per employee	−0.503	−0.132	0.014	−0.225	0.015	−0.568	−0.237
	(0.049)	(0.052)	(0.003)	(0.044)	(0.005)	(0.297)	(0.597)
N	2,317,705	407,143	2,444,073	891,118	1,960,098	156,213	44,798

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 coefficients 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 the reduced-form coefficients for placebo specification DiD4 restricting the sample to firms with 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.

To provide more formal testing for size manipulation, we report in Supplementary Appendix Figure 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 Supplementary Appendix Figure A-6A and 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 size 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. Supplementary Appendix Figure A-7 shows that in our sample less than 0.6% of firms change eligibility status due to a change in their INPS code every year, with the same fraction ($\approx 0.3\%$) 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 2–5 of Table 2 report this decomposition.

In Column 3, we can see that, absent STW, the employment shock is larger for eligible than non-eligible firms (DiD2), leading to a negative effect in the simple comparison of eligible and non-eligible firms with size above the 15-FTE threshold (DiD1). Hence our triple difference relies on correcting for differential trends across INPS codes. The DiD3 estimated in Column 4, instead, yields results extremely similar to our baseline triple-difference results. 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 by firm size.

While the previous evidence of no differential trends by size in non-eligible sectors is reassuring, it cannot rule out the presence of time shocks that are both specific to eligible INPS codes and firm size above 15 FTE. For instance, finding no differential trends across firms below and above the 15 FTE threshold in non-eligible INPS codes does not preclude the possibility that such differential trends exist for eligible firms. 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 applies 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.

3.3.2. Program substitution. As mentioned before, firms eligible for CIGS are also eligible for CIGO, and, since 2009, firms ineligible for CIGS could access CIGD. Supplementary Appendix Figure A-8A 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 Supplementary Appendix Figure A-1, but our instrument remains a strong predictor of STW take-up. Supplementary Appendix Figure A-8B 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 (Supplementary Appendix Figure A-8C), 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 Supplementary Appendix Table A-5 Panel I.A.

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 Supplementary Appendix Table A-5 Panel I.B. The estimated effects are both qualitatively and quantitatively similar to those we estimate for CIGS treatment in Table 1.

3.3.3. Alternative specification. Our baseline specification illustrated in model (3) identifies the combination of contemporaneous and past STW treatment effects. Indeed, as we show in Supplementary 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 to t, as our instrument is only defined if the firm exists in t. In Section 5.2.1, 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 2010–14 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

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

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 2010–14 period, where we use an average of the outcome unconditional on survival 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 neighbourhood around the 15 FTE size cutoff, we estimate the above model on firms who reach an FTE size between 5 and 25 in $t-1$.

Estimates of the effect of STW treatment are obtained by instrumenting the probability of STW treatment T in 2010–14 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.

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

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

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 Table 1 Panels II.A and II.B and are in line with the estimates from our baseline specification.[21]

3.4. *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

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

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, that is, 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 2008–9 was perceived as transitory, as can be seen in Supplementary Appendix Figure C-1, based on longitudinal data from a survey on firms' expectations by the Bank of Italy. But it ended up being quite persistent, as shown in Supplementary Appendix Figure C-2, because of the European Debt Crisis that immediately followed.[22]

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 labour 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 labour 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 of 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 labour optimally? Two main factors could actually make layoffs inefficiently high and labour hoarding too low. The first is the presence of liquidity constraints or, more generally, constraints on the ability to transfer resources across time. The second is inefficient bargaining, or the inability to transfer surplus between workers and firms. We explore both factors.

4.1. *Liquidity constraints*

The simplest way to think about labour 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 labour hoarding. Hence, STW policies can reduce inefficient labour hoarding by relaxing the liquidity constraint of firms.

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 analyse how liquidity affects the take-up of STW. To this end, we start by ranking firms

22. Supplementary 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 2008–9 financial crisis became a protracted double-dip recession in Italy, in contrast to other European countries and the US.

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, and run specification (3) using CIGS take-up as outcome, separately for firms in each quartile. The results, reported in Figure 5A, 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 constraints in 2008 and we normalize the index so that it is increasing in financial health (Whited and Wu, 2006).[23] 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 Figure 5B, we report the IV estimates $\widehat{\beta}_{IV}$ from specification (2) splitting the sample between firms with below versus 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 levels 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 Figure 5B the elasticity of employment with respect to the hour reduction $\varepsilon_{n,h} = -\frac{d\log n/d\mathrm{STW}}{d\log h/d\mathrm{STW}}$. We find that this elasticity is greater for low liquidity firms (2.53 (0.29)) than for high liquidity firms (1.97 (0.21)).[24] In other words, the increase in employment per STW hour used is significantly stronger among low liquidity firms. We finally investigate heterogeneity in the effect of STW on firms' survival by degree of liquidity. We find significant positive effects of STW on firms' survival in $t+1$ for low liquidity firms. These effects are quantitatively large: the probability of survival increases by 16.69% (5.98%) upon STW take-up for firms with below median liquidity pre-crisis. We do not find any such significant effect for firms with higher liquidity pre-crisis (1.09% (7.47%)).

The above evidence reveals a very strong sensitivity of STW take-up, as well as of 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 labour hoarding, as also evidenced by Giroud and Mueller (2017), and that STW can increase welfare by pushing firms' labour 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.

23. 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 2002 quarterly 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 $WW_{it} = -0.091 \cdot CF_{it} - 0.062 \cdot DIVPOS_{it} + 0.021 \cdot TLTD_{it} - 0.044 \cdot LNTA_{it} + 0.102 \cdot ISG_{it} - 0.035 \cdot SG_{it}$. 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.

24. Standard errors on the elasticity are computed using the Delta-method.

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 reports 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 versus 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.

4.2. *Inefficient bargaining*

The second reason why labour 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 labour 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.[25] 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, 2022). These provisions clearly limit the downward flexibility of wages in the Italian setting.

The evidence reported in Supplementary Appendix Figure B-1A 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 2010–14). The figure shows that the distribution of hourly wage changes is strongly skewed, with very little mass just below zero.[26] We view this evidence as supportive of the presence of significant downward wage rigidities. However, we should stress that firm-level negotiation can still be important in the Italian labour 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, Supplementary Appendix Figure B-1B 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 Supplementary Appendix Figure B-1A. 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 labour contracts are not systematically adjusted to account for fluctuations in hours, this approach will tend to overstate hour rigidities. We therefore turn to an additional source of information on hours: the Italian labour 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

25. 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 labour 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".

26. This strong asymmetry in fact holds for both eligible and non-eligible firms.

under a permanent contract in both periods. Since we want to assess the presence of hour 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, that is, not eligible for STW. Supplementary Appendix Figure B-2 reports the results of the empirical distribution of the year-on-year change in weekly hours worked for the years 2005–14 by sector using the 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 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. In an extreme case, 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, 2022). Firms may therefore terminate matches that exhibit significant value to workers. By increasing labour 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 labour 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 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 Supplementary 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 behavioural 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 behavioural response in hours is partially compensated by the positive employment effect, which reduces the cost to the UI system.[27] In other words, the larger the elasticity of employment with respect to hours, the lower the overall fiscal externality created by the program. Finally, we note that if the value of transferring one Euro to a STW worker is close to the estimated value of transferring a Euro to individuals on UI, then the inefficiency correction does not have to be very large to make a marginal Euro spent on STW more efficient than a Euro spent on UI in response to temporary shocks.

27. In Switzerland, Kopp and Siegenthaler (2021) find that the negative effect of STW on UI costs is large enough to fully offset the cost of the STW program, suggesting that the total fiscal externality is lower than 1 and that the program pays for itself.

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 labour 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 labour markets or in industries where the shock of the recession was less persistent. Third, and finally, labour reallocation and productivity growth is significantly lower in local labour 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: labour productivity and total factor productivity (TFP). Labour 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 $\text{TFP}_{ijt} = VA_{ijt}/(L_{ijt}^{\alpha_j} K_{ijt}^{\beta_j})$ where VA_{ijt} 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 labour share and the capital share, respectively. We compute the labour share at the 2-digit industry level. It is the mean ratio of labour expenditure to value added for all firms in industry j. We then set the capital share as one minus the labour share, assuming a constant returns to scale production function (i.e. $\beta_j = 1 - \alpha_j$).[28] 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 labour productivity and of TFP in 2008.

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 the probability of ever taking up STW during the 2009–14 period (T). The estimated coefficients $\widehat{\kappa}_1$ from model 3, reported in Figure 6A, 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.[29]

Do lower productivity firms also benefit more from this larger take-up of STW? In Figure 6B and C, we report estimates of $\widehat{\beta}_{IV}$ from IV model (2), again estimated separately for each quartile of the pre-recession productivity distribution. Figure 6B focuses on hour effects and shows that low productivity firms tend to reduce hours significantly more when using STW. Figure 6C shows that this comes with limited total effects on employment. In contrast, firms that were experiencing high productivity levels before the 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 labour productivity, for instance, the elasticity is small and insignificant, but it is as large as 4.19 (1.78) for the top quartile. In Figure 6D, we also report the estimated effects of STW on firms' survival by productivity level.

28. See Calligaris, Del Gatto, Hassan, Ottaviano and Schivardi (2016) for a similar implementation in the Italian context using CERVED data.

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

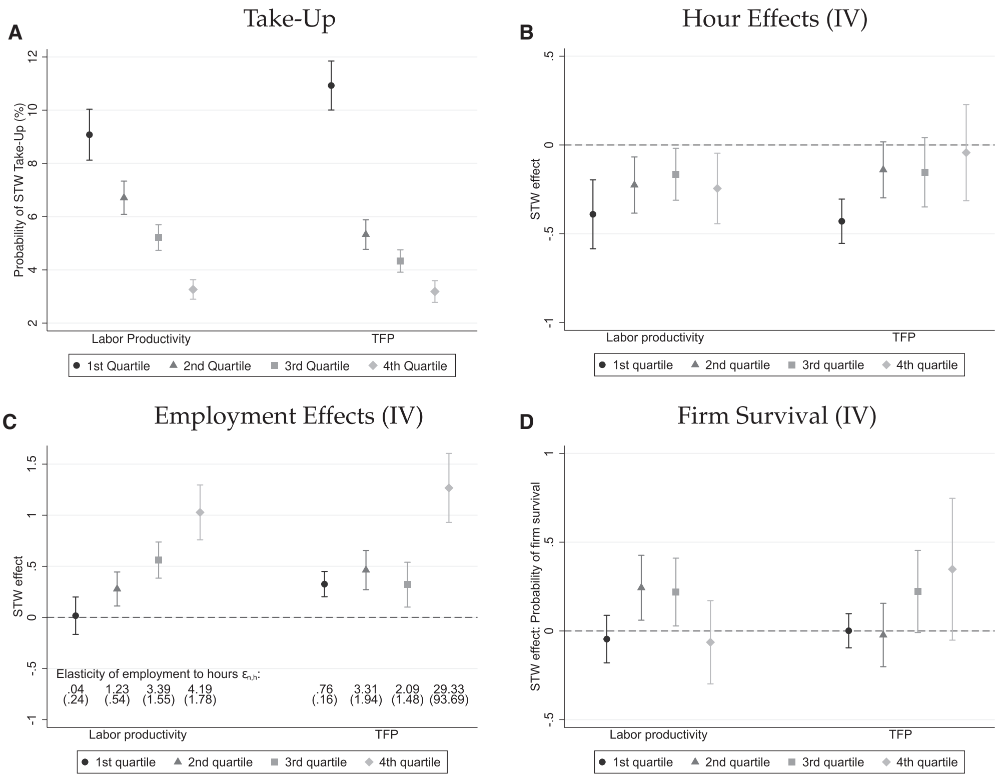

FIGURE 6

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 reports 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 labour 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-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 (A). The vertical bars indicate 95% confidence intervals based on standard errors clustered at the INPS code times firm size group level. In (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.

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 assess 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.

5.2.1. 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 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 instrument, are identifying the total effect of exposure to STW during the Great Recession.[30] 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, Ferreira and Rothstein (2010). All the details of the procedure are given in Supplementary 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 in 2008 and INPS code in 2009. The difference in outcome in 2009 of eligible firms in 2009 ($Z_{2009}=1$) vs. 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 as clearly shown in Supplementary 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 Supplementary 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 $\{\hat{\beta}_0^{TOT}, \hat{\beta}_1^{TOT},...,\hat{\beta}_4^{TOT}\}$.

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 0.3, but this effect disappears immediately after treatment, with no significant long-term impact. Supplementary Appendix Figure C-4 shows similar patterns for other employment outcomes. Upon treatment, log headcount employment increases by 0.2 and the log wage bill decreases by 0.2, 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.[31] 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.

30. 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. Supplementary Appendix Figure C-3 provides direct evidence of the correlation between current eligibility and past treatment by plotting the effect of the triple interaction $\mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j=t]$ on the probability of having received treatment in the previous 5 years.

31. 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.

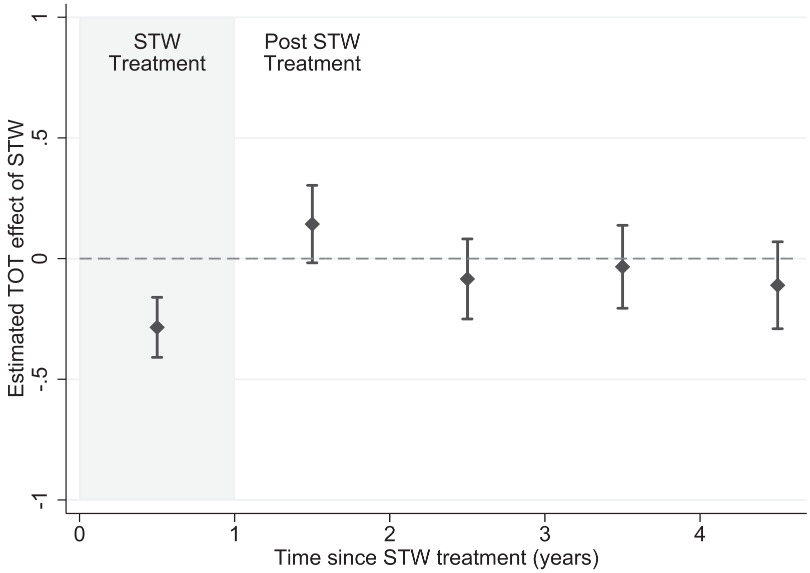

FIGURE 7

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 Supplementary 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.

5.2.2. Worker-level event studies. We document the dynamics of labour market outcomes of workers following STW treatment using event studies. We create a panel of the labour 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.[32] 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, -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-neighbour in the STW treatment group. The

32. 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.

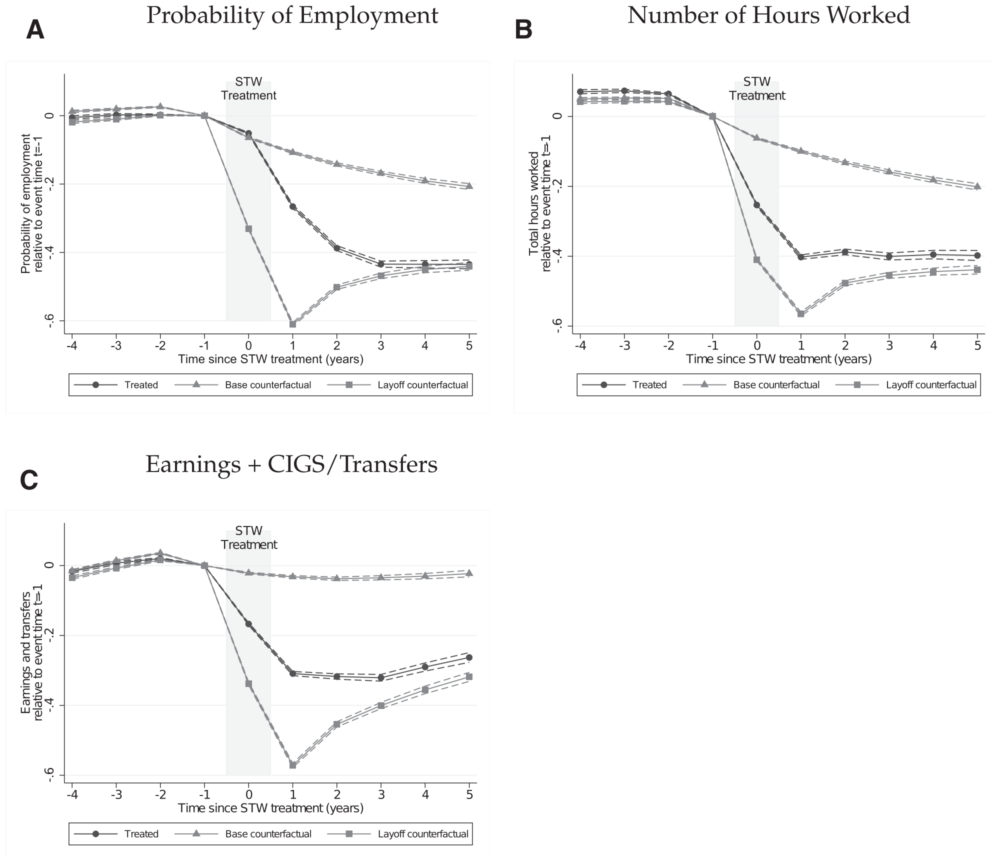

FIGURE 8

Dynamic effects of short-time work on workers' outcomes

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-neighbour 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 (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).

second comparison group consists of workers in non-eligible firms who experience a layoff and is created following a similar nearest-neighbour-matching strategy using the same variables. For this group, event year 0 is defined as the year of the layoff.[33]

33. We note that the event study estimates on workers treated by STW describe the dynamics of their labour 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

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, signalling little anticipation of STW treatment in terms of labour 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 labour 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—by approximately 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—by approximately 18 percentage points—than that of laid-off workers.

After STW is over, its beneficial effects seem to dissipate quickly. Treated workers experience a sharp drop in labour market outcomes, confirming the reversal that we 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.[34] There is also a significant drop in total earnings and transfers of treated workers, which, 2 years after treatment, amount to only 65–70% of their pre-treatment level. In comparison to non-eligible workers, treated workers fare much worse in terms of all labour market outcomes in the medium and long run. But even more strikingly, 2–3 years after treatment, the labour 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 labour 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 versus temporary contracts within the same firm. As we explain in more detail in Supplementary Appendix C.3, this allows us to control for the correlation between STW treatment and persistent firm-level shocks. In Supplementary 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 Supplementary Appendix Figure C-6A, we focus on workers who are on open-ended contracts, while in Supplementary Appendix Figure C-6B on workers who are on temporary contracts in

various characteristics affecting their labour market dynamics. We nevertheless show in Supplementary 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 Supplementary Appendix C.3.

34. 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.

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 Supplementary Appendix Figure C-7 the evolution of the differential probability of employment of workers employed in open-ended versus 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 labour productivity level. We split the sample according to the average level of labour productivity of the firm in event-time years $t=-4$ to $t=-1$, using the same definition of labour 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 labour productivity, and following the same methodology as in Figure 8, 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 three years after treatment, neither in terms of employment, nor in terms of earnings. In contrast, 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.[35]

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 layoffs 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.

5.2.3. 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 labour 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 Supplementary Appendix C-4. The results of this classification are displayed in Supplementary Appendix Figure C-8, which shows the evolution of total employment across more and less persistently affected LLMs (Supplementary Appendix Figure C-8A) and industries (Supplementary Appendix Figure C-8B). The graphs provide

35. 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.

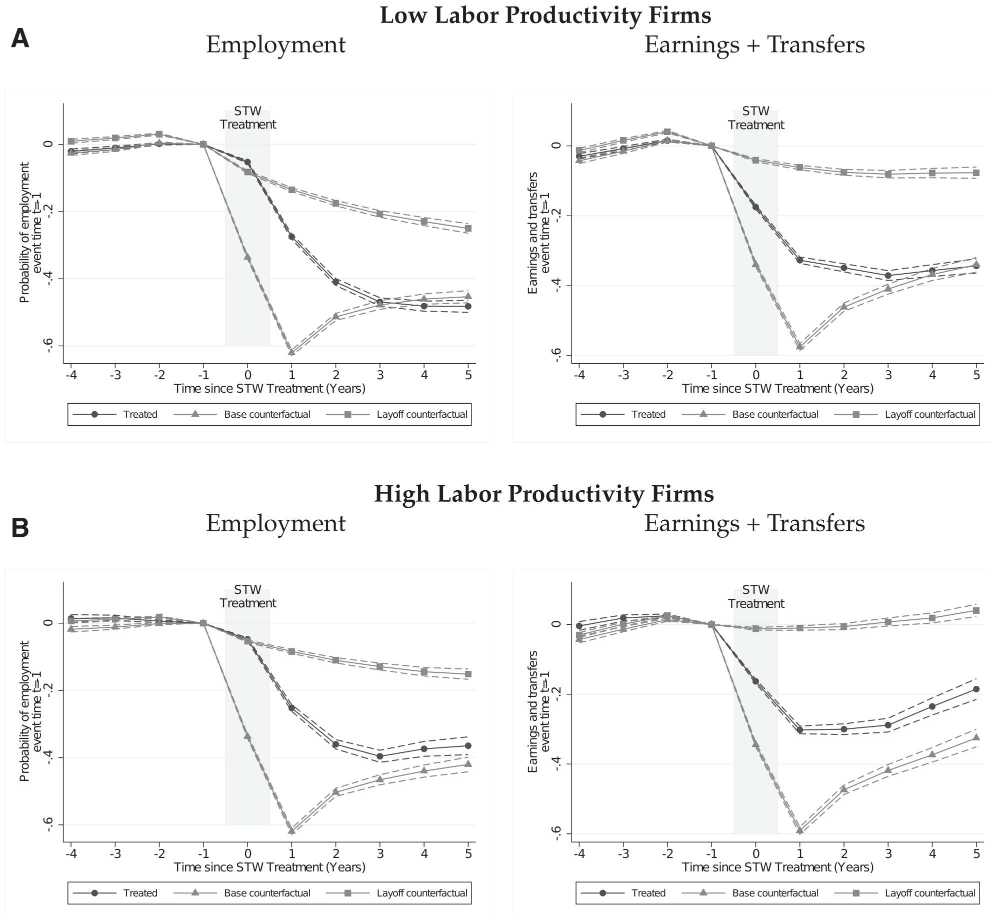

FIGURE 9

Dynamic effects of short-time work on workers' outcomes by firms' pre-crisis level of labour productivity

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 labour productivity of the firm that the worker is employed at in event year $t=-1$ – the average being taken over event-time years $t=-4,\ldots,-1$. Panel A reports 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 reports results for workers, who, at event time $t=-1$, were employed by firms in the top half of the distribution of labor productivity. Labour productivity is defined as value added per hour worked.

support for our proposed classification. They show 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 specifications (5) and (6). The results are reported in Supplementary Appendix Table C-1, where we consider any CIG take-up as treatment. Supplementary Appendix Table C-1 Panel A shows heterogeneity with respect to the temporariness of the shock at the LLM level, Supplementary Appendix Table C-1 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, Bloom, Davis and Meyer, 2021).

To empirically investigate the importance of reallocation effects, we leverage the rich spatial variation available in Italy across more than 600 local labour markets (LLMs) 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.[36] For this analysis, we use data on the universe of firms and workers in the Italian labour 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.[37] Supplementary Appendix Figure D-1 shows the large amount of variation in the intensity of STW treatment across LLMs during the Great 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 labour market conditions during the Great Recession, which might affect the employment outcomes of non-treated firms. To account for this threat, we instrument the fraction of workers treated by STW during the recession by the average yearly fraction of eligible workers in the LLM in the pre-recession period, based on the interaction between firm size and INPS codes in the years 2005–8. We identify the reallocation effects of STW on non-treated firms at the LLM level based on the following model:

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

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 2010–13 to the pre-recession years 2005–8. $\underset{t,t'}{\Delta} T_j$ 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

36. We use the ISTAT 2011 classification of municipalities into 611 local labour markets.

37. We assign workers to LLMs based on the address of the place of work, which is available in the INPS individual records.

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. Supplementary 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 2005–8 and the fraction of workers on STW during the recession, conditional on controls for firm and LLM characteristics.

Figure 10A 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_{IV}^{R}=−0.94 (0.22), which means that a 1 percentage point increase in the fraction of treated workers in an LLM reduces employment of non-eligible firms by 0.94%. 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 $\widehat{\beta}_{IV}^{R}$ estimates imply that for one job "saved' by STW in a treated firm, employment in non-treated firms decreases by 0.03 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).

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.[38] The IV results, displayed in Table 3, confirm that STW has a significant negative impact on overall TFP at the LLM level, 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 for 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 2000–5 and 2005–8, and use as placebo instrument the fraction of eligible workers in the LLM

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

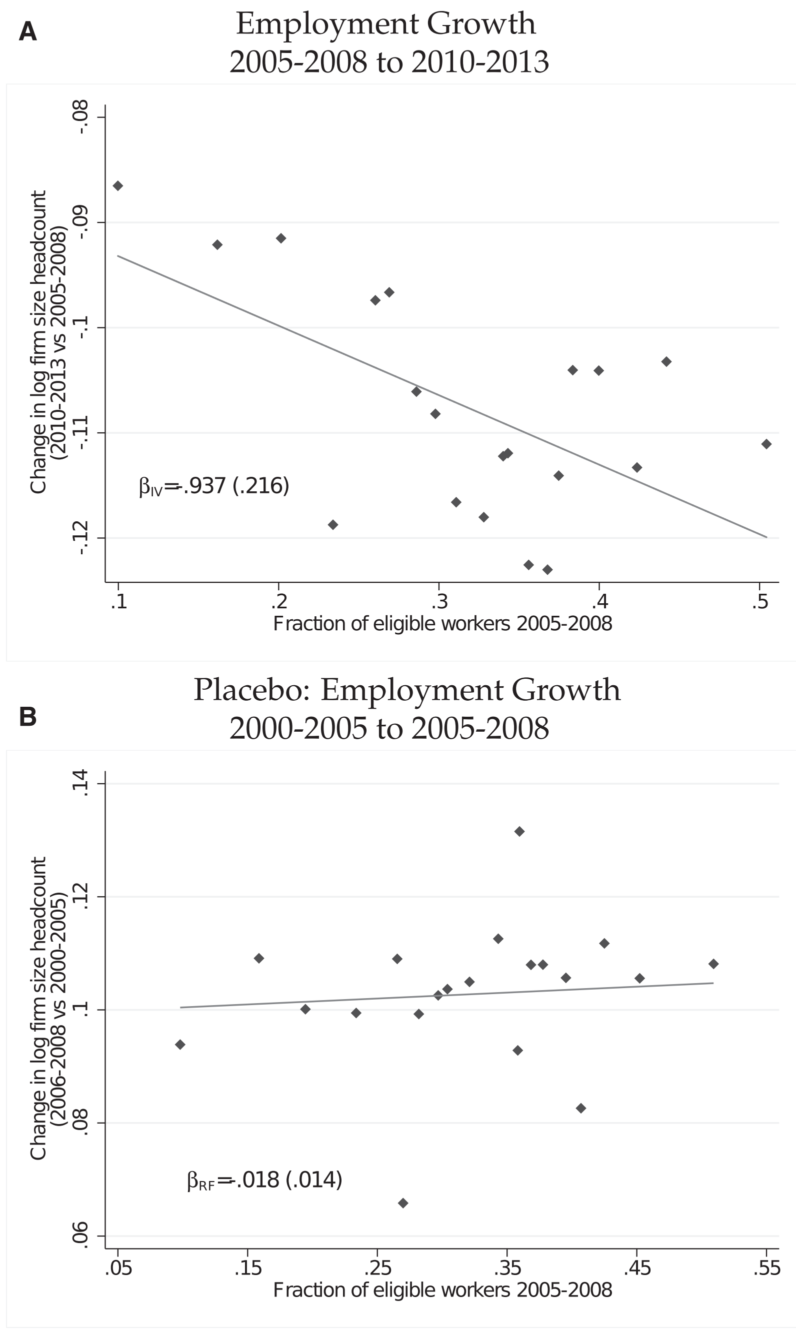

FIGURE 10

Reallocation effects: employment growth in non-eligible firms as a function of short-time work eligibility in the local labour market

Notes: The graphs show binned scatterplots of the reduced form of equation (9). Panel A shows the reduced form relationship between the change in average log firm size headcount of firms non-eligible to STW in a local labour market (LLM) between 2005–8 and 2010–13, and the fraction of eligible workers in 2005–8 in the LLM based on the interaction between firm size and INPS codes. Both variables are residualized on firm-level and LLM-level controls. The graph 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 (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 2000–5 and 2005–8, and the fraction of eligible workers in 2005–8 in the LLM. The graph 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 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)
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		3,023,166			2,784,567	
	B. Effects on labour market productivity					
Log TFP	−2.307	−2.093		−0.003	−0.003	
	(0.593)	(0.606)		(0.062)	(0.062)	
LLM controls		×			×	
N		1,222			1,222	

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 2000–5 vs. 2005–8, and using the fraction of eligible workers in 2005–8 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 $TFP = VA/(L^{\alpha}K^{\beta})$, where we aggregate all variables (VA, L, and K) at the LLM level.

based on the interaction between firm size and INPS codes in the 2005–8 period. Because there is no take-up of CIGS during the 2005–8 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 Figure 10B. We clearly see no significant relationship between the placebo instrument and the outcomes, which provides comforting evidence for the validity of our exclusion restriction. We report similar placebo models for TFP growth in Table 3 and find no significant relationship between our instrument and TFP growth in the LLM in the pre-recession period.

Overall, by leveraging the rich spatial variation across LLMs in Italy, and the variation in STW treatment created by the interaction of firm size and INPS codes, these results provide compelling evidence that STW has significant equilibrium effects within labour 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 labour market.

These reduced-form estimates clearly identify the presence of reallocation effects of STW. But they cannot tell us what labour 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, in Supplementary Appendix E.1, we turn to a calibrated matching model of the Italian labour 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 Supplementary 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 TFP in the Italian economy by about 2% during the Great Recession.

6. CONCLUDING REMARKS

During the COVID-19 crisis, STW programs attracted a lot of attention as a tool to subsidize labour hoarding and were aggressively used across OECD countries. Yet, very little is known about their effects and welfare consequences. This article 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 depending on whether the shock is temporary or persistent.

Our paper confirms that, in the presence of temporary shocks, substantial frictions prevent efficient labour hoarding by firms. We provide evidence of the presence of two types of frictions that may 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 suggest that the positive employment effects of STW are significantly larger when these frictions are more prevalent.

When the shock becomes persistent, 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 article 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 to high-productivity firms facing liquidity constraints.

In practice, though, we note that, when a shock hits, it is always hard to know whether it will be permanent or transitory. Limiting the duration of STW schemes can mitigate the issue by preventing that job matches that turn out to be persistently hit end up being persistently subsidized.

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 carefully assess the external validity of our findings, and account for differences in the nature of the Great Recession and COVID-19 shocks. On the other hand, given that STW was extended to almost all firms during the pandemic, identification opportunities in that context are scarce. With this in mind, we believe that our results do provide some useful guidance for understanding the consequences of STW schemes during the 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 issues depend on the characteristics of the firms that are more affected when the shock persists, as this determines the extent to which employment matches can survive in the medium run. Interestingly, the nature of the pandemic suggests that, contrary to the financial crisis of 2008, the COVID-19 shock may have been quite orthogonal to firms' productivity prior to the crisis.

We follow up on the above question in Giupponi, Landais and Lapeyre (2022), where we provide a conceptual framework to analyse the welfare consequences of labour 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 labour hoarding policies, more research is necessary to fully establish the welfare consequences of the massive subsidization of labour hoarding during the COVID-19 crisis, in particular to assess the aggregate demand effects of STW through firm survival and employment expectations (Guerrieri, Lorenzoni, Straub and Werning, 2022).

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

Supplementary Data

Supplementary data are available at *Review of Economic Studies* online. And the replication packages are available at https://dx.doi.org/10.5281/zenodo.6951801.

Data Availability Statement

The data underlying this article cannot be shared publicly, as the paper uses confidential administrative data from the Italian Social Security (INPS) that we are not authorized to post online. The data can be accessed by researchers who ask for an authorization, prepare an application, and get clearance from the VisitInps Scholars Program at INPS. However, all replications scripts, including detailed explanations of data construction, etc. are available at the following DOI: 10.5281/zenodo.6951801 http://doi.org/10.5281/zenodo.6951801.

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