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Short-Time Work, Employment and Digitalization During COVID-19

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Abstract

Short-time work (STW) schemes were widely deployed during COVID-19 to stabilize employment, while the same crisis also accelerated firm digitalization, potentially reshaping firm-level labor demand. We examine how these responses relate to firms' employment and productivity trajectories during and beyond the shock. Using the German Innovation Survey (2012-2024) and a dynamic difference-in-differences approach, we find that STW is associated with lower employment through 2024, alongside a productivity recovery from 2022 onward. Firms with STW shrank relative to comparable firms that did not use the scheme and became, at most, slightly leaner. Digitalization in response to the crisis is associated with sustained employment growth, but this advantage weakens with STW intensity and is not matched by productivity gains. We highlight a tension between protecting existing jobs and adapting to a crisis that rewarded changes in how firms operate.

Keywords: COVID-19 crisis, short-time work, labor market policy, digitalization

JEL Classification: J23, J68, O33, D22

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1 Introduction

In spring 2020, COVID-19 brought large parts of economic activity to a sudden halt, and governments across Europe turned to short-time work (STW) as a tool to prevent mass layoffs. Such schemes serve a straightforward purpose by shifting part of employees' wage costs to the state for hours not worked, encouraging firms under pressure to cut hours rather than jobs. The goal is to preserve employer-employee matches, protect aggregate demand, and prevent mass unemployment during what is expected to be a temporary disruption. Germany's STW scheme *Kurzarbeitergeld* covered approximately six million workers at its April 2020 peak and had an estimated total cost of €42 billion in 2020–2021 (Kagerl & Kruppe, 2024). Four years later, we find that firms that relied on the scheme shed more of their workforce than comparable firms that did not. In this paper, we document this gap and relate it to another dominant firm response to the global pandemic: digitalization. By doing so, we examine how employment protection interacts with firms' digital adaptation to the crisis.

Macroeconomic evidence from earlier recessions shows that STW mitigates the immediate employment impact of the crisis, at the cost of reallocation frictions across firms and sectors (Boeri & Bruecker, 2011; Cahuc, 2019, 2024; Cahuc & Carcillo, 2011; Dengler et al., 2025; Giupponi et al., 2022; Giupponi & Landais, 2023; Hijzen & Martin, 2013; Hijzen & Venn, 2011). Germany's STW scheme in particular is credited with avoiding mass unemployment during the 2008/09 Global Financial Crisis (GFC) (Balleer et al., 2016; Bellmann et al., 2012; Brenke et al., 2013). Firm-level evidence on whether job protection worked and outlasts the subsidy is thinner and mixed (Giupponi & Landais, 2023; Kopp & Siegenthaler, 2021), and for COVID-19 it remains scarce and short-run (Albertini et al., 2022; Kagerl, 2026; Merikül & Paulus, 2024). Two fundamental features of the COVID-19 crisis make it unclear whether the existing evidence carries over, and warrant revisiting the scheme during the most recent crisis. First, COVID-era STW eligibility rules were broad, and firm take-up was nearly ten-fold

compared to the 2008/09 GFC (German Federal Employment Agency). Second, the GFC and the pandemic were very different disruptions. The COVID-19 crisis not only suppressed demand but also pushed firms to change how they operate to comply with lockdowns and social-distancing rules, and to manage disrupted global supply chains. Digital adoption accelerated sharply, with two in three German firms investing in digital technologies in response to the crisis (Gathmann et al., 2024). Such adaptation, and digitalization in particular was often central to how well firms manage and recover from the crisis (Calza et al., 2023; Copestake et al., 2024; Doerr et al., 2021; Lien & Timmermans, 2024; Trunschke et al., 2024; Wenzel et al., 2021), but requires complementary investment in human capital and labor reallocation (Bresnahan et al., 2002; Moncada et al., 2025; Pedota et al., 2023). There is hence a tension between a scheme designed to protect existing business models and employer-employee relationships, and a shock situation that rewarded firms that changed business activities and labor relations. The objective of the paper is to investigate this tension.

We use the Mannheim Innovation Panel (MIP), Germany's contribution to the EU Community and Innovation Surveys (CIS). Its 2021 wave provides information on STW uptake, COVID-induced digitalization, and shock severity, and we link this to firm outcomes from 2012-2024. In this setting, we estimate a dynamic difference-in-differences model with two-way fixed effects, using STW intensity as the treatment. Under broad allocation rules, firms self-selected into the program and chose whether to digitalize. The design limits selection through entropy balancing on pre-crisis characteristics, conditioning on shock severity and flat pre-trends, but we read the estimates as conditional associations, and the STW × digitalization interaction as heterogeneity rather than causal moderation.

Three results emerge from our analysis. First, at the mean treatment intensity (23.5% of total working hours omitted using the scheme), STW firms' employment is 4-8% below that of comparable non-STW users, bottoming out in 2022 when the crisis ends and remaining well

below comparable non-users through 2024. Second, labor productivity drops sharply in 2020-2021 and converges to non-users' levels by 2022, slightly exceeding them. Users emerge from the crisis clearly smaller, and at most, slightly leaner. Third, digitalization is associated with sustained employment growth, but this growth shrinks with higher STW intensity. Among firms that did not digitalize, we find no employment differential for scheme use, consistent with the scheme working as intended. Digitalizing STW users show no productivity advantage over non-digitalizing STW users, so our findings do not support firms increasing productivity through digitalization by replacing employees under the scheme.

We contribute to two streams of literature and policy debate. For the microeconomic STW literature (Cahuc, 2024; Giupponi et al., 2022; Kagerl, 2026; Kopp & Siegenthaler, 2021), we extend beyond the crisis's acute phase and provide intensity-based evidence on the largest STW deployment to date, tracing trajectories of scheme-using firms within a STW-using economy. We find that STW-using firms don't return to their pre-crisis state; instead, their productivity converges as they adjust employment downward. Our findings on crisis-induced digitalization speak to the literature on crisis-induced innovation and digital resilience (Calza et al., 2023; Copestake et al., 2024; Doerr et al., 2021; Lien & Timmermans, 2024). We find that employment returns to adaptation depend on the policy context they occur in. For policy, we do not claim STW failed. Our design does not capture the scheme's role in protecting aggregate demand, and we cannot estimate job loss in a non-STW-using labor market. Our results show that, under broad eligibility and take-up, the scheme's premise of simply bridging a temporary shock did not hold for its most intensive users; these firms do not return to their pre-crisis state, and STW coincides with smaller returns to digital adaptation.

The remaining sections are structured as follows. Section 2 describes the institutional setting, and Section 3 develops our hypotheses. Sections 4 and 5 present the data and empirical strategy; Section 6 reports the results; Section 7 discusses them; and Section 8 concludes.

2 Institutional setting: short-time work in Germany

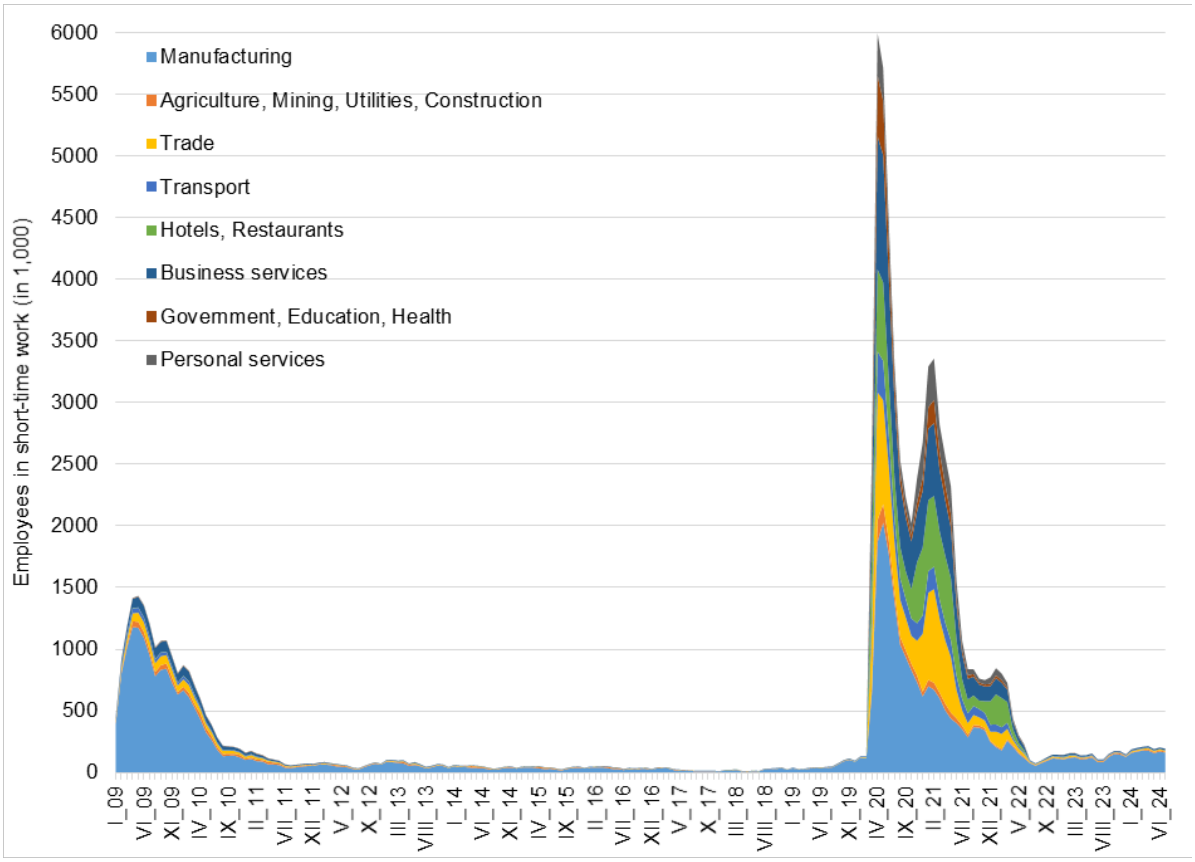
Germany's short-time work scheme, or *Kurzarbeitergeld*, in its current form introduced in 1957, is widely regarded as the gold standard because it is the most institutionalized and best-studied STW scheme (IMF, 2020). Firms apply to the Federal Employment Agency and provide information on the reasons for the exceptional economic situation requiring a reduction in working hours, the number of reduced working hours and affected employees, and the length of the reduction period. When approved, employers must document the amount of reduced work for each employee in short-time work and report it to the Agency. The employer continues to pay the affected employees the normal wage for hours worked plus compensation for hours not worked. The Agency refunds the compensation to the employer. Eligible employees must have employment that involves social insurance contributions. During the pandemic, the scheme was expanded on three margins:

- Access. The minimum share of affected employees was reduced from 33.3% to 10%.
- Generosity. Employees usually receive compensation equal to 60 percent of the wage loss; compensation rates were increased to 70 percent and 80 percent for employees who remained in short-time work for more than 3 or 6 months, respectively. Higher rates apply for employees with children.
- Duration. The scheme's maximum benefit period was extended from 12 months to 28 months.

The number of workers under the scheme was 0.13 million in February 2020. This figure rose to 2.58 million in March 2020 and peaked at 6.00 million in April 2020 after the first lockdown was announced on March 22nd. The April 2020 peak was four times higher than the short-time work peak during the severe financial and economic crisis in 2008/09. The number of workers under the scheme fell to 2.02 million in October 2020 and increased again to 3.36 million during

the second lockdown (February 2021). The scheme's usage gradually declined and fell below pre-pandemic levels in July 2022. At the firm level, the April peak reached 610 thousand firms, or 28 percent of all firms with eligible employees, and was ten times higher than the peak usage during the 2008/09 crisis.

Figure 1: Number of people in short-time work scheme in Germany 2009 to 2024, by industry



Source: German Federal Employment Agency (https://statistik.arbeitsagentur.de/DE/Navigation/Statistiken/Interaktive-Statistiken/Kurzarbeitergeld/Kurzarbeitergeld-Nav.html?Thema%3Dzr%26DR_Gebietsstruktur%3Dd%26Gebiete_Region%3DDeutschland%26DR_Region%3Dd%26DR_Region_d%3Dd%26DR_WZ%3Dinsgesamt%26mapHadSelection%3Dfalse%26toggleswitch%3D1)

3 Literature review and hypotheses

3.1 Short-time work as a policy instrument

Short-time work schemes (STW) are designed to protect employment during temporary disruptions in labor demand. The scheme lets firms reduce employees' hours while the state

compensates workers for lost earnings, discouraging mass layoffs. The economic rationale for such schemes rests on preserving worker-employer matches and protecting aggregate demand. When shocks are temporary, dismissing workers destroys match-specific human capital and creates separation, search, and rehiring costs that could otherwise be avoided. From the government's point of view, the program is effective if it prevents layoffs and keeps the unemployment rate low during a recession, thereby avoiding cuts in effective demand and excessive consumer saving due to fear of job losses (Dengler et al., 2025). In addition to this macroeconomic objective, the program also has a microeconomic aim of maintaining the competitiveness of firms using the scheme, enabling them to return to the same (or higher) level of economic activity after the end of the crisis. However, meeting this objective cannot be taken for granted for two reasons. First, firms with low competitiveness at the start of the recession may use the scheme to cut costs and remain in the market for some time despite deteriorating prospects. Secondly, firms using the scheme may reduce their efforts to maintain competitiveness during the recession, as STW allows them to save costs. In both cases, the policy ties workers to unproductive firms (Merikül & Paulus, 2024).

Most existing evidence on STW comes from the 2008/09 Global Financial Crisis (GFC), where studies across different countries and institutions generally point to positive employment effects of STW at the macro-economic level, with some variance in magnitude and persistence (Balleer et al., 2016; Boeri & Bruecker, 2011; Brey & Hertweck, 2020; Cahuc & Carcillo, 2011; Hijzen & Martin, 2013; Hijzen & Venn, 2011). Firm-level evidence from the same period points in the same direction. When faced with severe negative shocks, firms dismiss fewer employees than comparable non-users (Cahuc et al., 2018; Lydon et al., 2019). The conventional rationale for the scheme rests on the disruption being temporary. The employer-employee match should survive beyond the subsidy period; otherwise, dismissals are simply delayed rather than avoided (in this reasoning we temporarily disregard the scheme's role in protecting aggregate demand

and avoiding mass unemployment in the acute phase of the crisis). STW is most beneficial when the protected match becomes productive again once economic activity resumes. This makes the longer-term employment trajectory of STW users an important empirical question. The best-identified firm-level research from the GFC period is mixed, depending on the shock's magnitude (Cahuc et al., 2021) and firm characteristics. Kopp & Siegenthaler (2021), exploiting variation in Swiss STW approvals, find that STW prevents dismissals rather than merely delaying them. In contrast, Giupponi & Landais (2023), using detailed administrative data from the same crisis, find that it depends on whether the shock is persistent or temporary, and on firm quality: STW's effectiveness in protecting employment is durable for high-productivity firms but dissipates entirely for low-productivity firms.

A firm-level finding of delayed rather than prevented dismissal does not imply scheme failure. STW's aggregate-demand-stabilizing role operates independently of whether those jobs remain protected beyond the crisis, a benefit a firm-level analysis cannot capture. Nor would subsequent deployment decline necessarily be inefficient. Recessions typically push out weaker firms and reallocate resources to stronger ones, although frictions can hamper this effect (Caballero & Hammour, 1996; Osotimehin & Pappadà, 2017). In that sense, STW schemes can slow labor reallocation toward more productive firms. Giupponi & Landais (2023), find that greater STW intensity causally reduces employment growth and hiring at non-treated firms in the same labor market, alongside a corresponding negative effect on local TFP growth. Therefore, if these frictions lift at the end of the crisis, a decline in employment among STW users may merely be the resumption of an efficient labor reallocation process rather than a policy failure. This highlights the trade-off between short-term stabilization and longer-term reallocation frictions and inefficiencies, which may outweigh one another depending on the shock's persistence.

Given the unprecedented scale of STW take-up (Adams-Prassl et al., 2020) and eligibility differences during the COVID-era, which we documented in Section 2, it is not obvious whether those (generally) positive employment effects translated to the COVID setting – Giupponi et al., (2022), note that inflated STW take-up may impose reallocation costs that outweigh the benefit of employment protection. Consistent with this concern, Meriküll and Paulus (2024), show that within-industry reallocation of labor towards more productive firms accelerated during the GFC but not in the COVID-19 crisis, whilst positive employment effects only partly offset this, leaving limited net gains. The pandemic combined a demand drop the scheme was designed to address (Albertini et al., 2022) with severe supply-side disruptions and public health measures that restricted how firms could operate at all. These unique conditions motivate revisiting the effectiveness of STW, and whether digitalization, playing a very different role than during the GFC era, altered how the employment-preservation mechanism played out. Direct COVID-era evidence is limited. Kagerl (2026) reports employment gains for STW users in Germany over a shorter post-crisis period, with a sample limited to low-intensity users. Aiyar & Dao (2021) find that in the absence of STW, unemployment in Germany would have increased by 3%, while Brinkmann et al. (2025), who investigate the extension of STW duration, find no evidence that unemployment outcomes improved while reallocation to other firms decreased. Considering the existing work, we follow the dominant priors that STW has an employment-protecting effect.

H1: STW use during the COVID-19 pandemic is associated with equal or higher employment during and following the COVID shock, relative to comparable firms.

3.2 Short-time work and productivity

The scheme deliberately reduces employment adjustment in response to a fall in activity, meaning firms retain workers whose labor they do not require and, by design, decrease the

average productivity per worker during the disruption. An initial labor productivity deficit among STW users is a mechanical side-effect of STW working as intended and does not, in itself, imply permanently unproductive employment. In Germany during the GFC, working-hours adjustment occurred primarily on the intensive margin rather than on headcount, following this pattern (Balleer et al., 2016; Cahuc et al., 2018; Giupponi & Landais, 2023).

What's most informative is what happens after the shock: if and how the productivity deficit closes. This pattern, along with the employment result, yields three potential scenarios. First, output recovers, and the retained labor force is fully utilized again. In this case, the scheme has successfully bridged the gap, preserved employment, and the firm required no reallocation. Second, firms may release labor despite support or once support ends; that would mean productivity recovers at the cost of lower employment, and the scheme merely delayed layoffs. Third, productivity does not recover after the shock subsides, in which case the scheme did not work as intended. This finding would be consistent with permanent scarring or the scheme slowing reallocation from less productive to more productive firms (Meriküll & Paulus, 2024).

H2: STW use is associated with an initial drop in labor productivity that diminishes as the crisis period ends.

3.3 Short-time work and digitalization

The conventional labor-hoarding mechanism becomes less straightforward when firms must respond to a shock in ways that fundamentally change the way they operate. Digital capacity supported firm resilience during and beyond the COVID-19 shock (Abidi et al., 2022; Bai et al., 2021; Calza et al., 2023; Copestake et al., 2024), while the pandemic itself accelerated the adoption of digital technologies across the board. In Germany, two out of three firms invested in digital solutions as a direct response to the pandemic (Gathmann et al., 2024). Such investment not only altered how firms adapted to the shock but also changed how they organize

and use labor. Combining both margins yields four groups of firms: those that did neither, those that only digitalized, those that only made use of STW, and firms that did both.

Digitalization without STW. The employment consequences under such a digital shift are theoretically ambiguous. Complementarity is possible: digital technologies may enable workers to continue activity that would otherwise have been disrupted, e.g., through working-from-home arrangements (Alipour et al., 2021; Brem et al., 2021) or directly increase the firm's demand for skilled labor to facilitate the adoption of new digital technologies (Bresnahan et al., 2002; Koch et al., 2021; Moncada et al., 2025; Pedota et al., 2023). On the other hand, labor substitution occurs when firms replace physical service points and human-to-human touchpoints with automated or digital alternatives.

STW without digitalization. These firms follow the logic of Sections 3.1 and 3.2. In case the scheme works as a bridge, it protects employment, and productivity recovers when the shock ends. It may also temporarily protect employment and delay eventual layoffs rather than protect employment sustainably. In the latter case, we expect a negative selection effect as firms fail to make adaptive investments in response to the crisis. This group includes firms that would otherwise have made digital investments but may refrain because subsidized labor hoarding allows them to cut costs, relieving pressure to make necessary investments.

Both. Gathmann et al. (2024) examine the relationship between STW and digitalization as substitutable pandemic responses; we propose that some firms did both. For these firms, STW and digitalization can interact through opposing channels. STW may create slack. It relieves firms of otherwise tighter financial constraints, giving them financial and organizational flexibility to implement necessary changes or explore solutions, including digitalization. Slack alone predicts more adaptation, not fewer jobs. Employment loss occurs only if firms use that flexibility to replace labor with digital alternatives (Acemoglu & Restrepo, 2020), which firm-level evidence says is not the typical consequence of technology adoption (Koch et al., 2021;

Peters et al., 2022). STW may also create rigidity. Subsidizing hours not worked weakens the pressure to reallocate labor (Dengler et al., 2025; Merikül & Paulus, 2024), slowing the necessary changes digitalization requires to be implemented successfully (Bresnahan et al., 2002; Moncada et al., 2025; Pedota et al., 2023). We distinguish these channels by their joint effects on employment and productivity. If STW-using firms replace labor with digital alternatives, digitalizing users should have lower employment and higher productivity. If, instead, STW use introduces rigidities, digitalizing users should have equal employment and lower productivity. Under an assumption of STW subsidizing a transition into leaner, more digitalized business models, we predict that:

H3a: The association between STW intensity and subsequent employment is more negative among firms that digitalized than among firms that did not.

H3b: The association between STW intensity and subsequent labor-productivity recovery is more positive among firms that digitalized than among firms that did not.

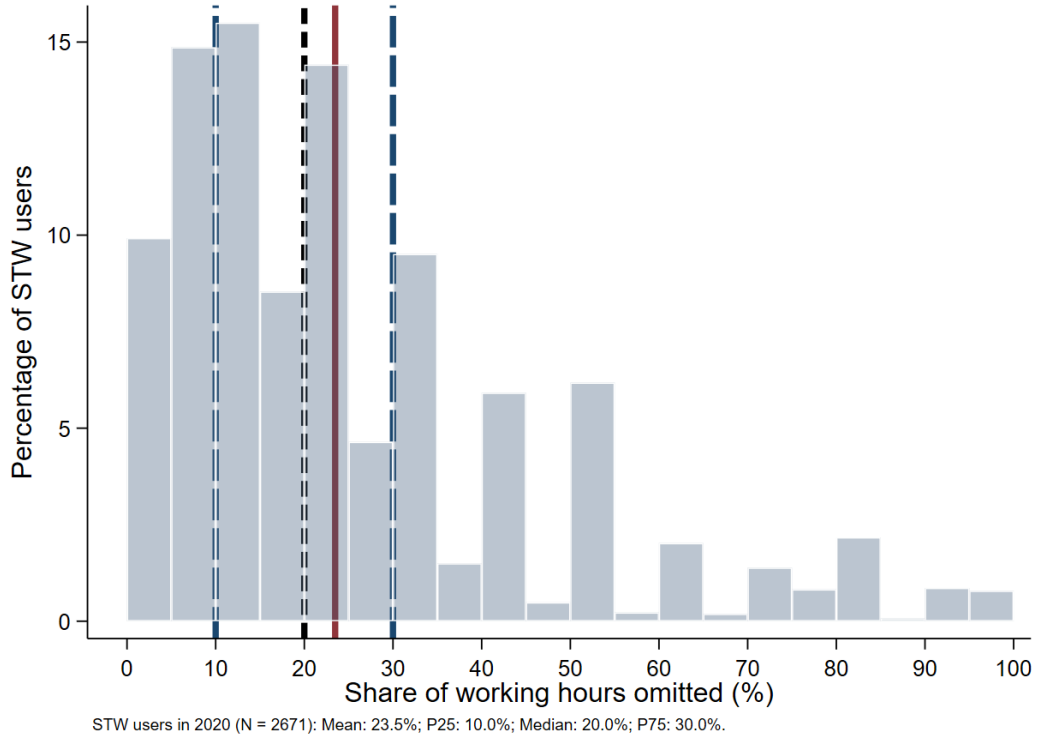
4 Data

We use the Mannheim Innovation Panel (MIP), Germany's contribution to the Community and Innovation Survey (CIS). MIP is an annual panel, allowing longitudinal analyses at the firm level (Peters and Rammer, 2023). It draws on a stratified random sample of firms with at least five employees in goods-producing sectors (such as mining, manufacturing, and utilities) and a variety of service industries (including wholesale trade, transport, and IT, financial and business services), refreshed biannually to offset panel attrition. The panel does not cover consumer-oriented sectors such as retail, hospitality, and personal services, which were most exposed to social distancing mandates and lockdowns.

The 2021 wave (reference year 2020) of the MIP added COVID-specific questions (Appendix Figures 4-6) central to our analysis. Our sample consists of the 6,512 firms that responded to these questions, from which we build an unbalanced panel over 2012–2024 (49,426 firm-year observations). Below, we describe key variables derived from this survey wave.

Short-time work. Firms report whether they made use of STW and the percentage of total working hours they omitted using the scheme, which gives us a treatment dose $STW_i \in [0,1]$. In our sample, 2,671 (41.0%) firms made use of the scheme at an average intensity of 23.5% (s.d. 21%). The share of STW users is higher than the average share across eligible firms in Germany (28%), which reflects that the MIP contains almost no firms with less than 5 employees, which rarely used the scheme. Self-reported STW use is known to be misreported. Kagerl et al. (2022), investigate STW reporting in Germany. We rely on their findings, along with two MIP features, to address this concern. MIP is retrospective (surveyed in 2021, for reference period 2020); their findings show most reporting bias disappears quickly over time, and the survey is answered at the enterprise level, avoiding the establishment-firm mismatch that drives much of the difference between reported and actual STW use. Figure 2 provides an overview of STW intensity, and we use these values to interpret our findings.

Figure 2: Share of Working Hours omitted in firms using Short-time work in 2020



Source: Mannheim Innovation Panel

Shock severity. Firms rate the impact of COVID-19 on their business activities in 2020 on a six-point Likert scale from "Extremely Negative" to "Very Positive". This deliberately generic scale ensures comparability across firms, industries, and business impacts, which can range from business closures to export restrictions, bans on certain distribution channels, and higher costs for safety and health measures, but may also include positive impacts such as higher demand for products that were particularly relevant during the pandemic situation (e.g. health products, IT services).

Digitalization. As part of a broader set of strategic responses to the COVID-19 pandemic, firms were surveyed on whether they temporarily or permanently increased digitalization within the enterprise to respond to the crisis. We define a digitalizing firm as one that responds positively, whether permanently or temporarily; this applies to 3,749 firms (57.6%).

Table 1: Mean values of firm characteristics, STW users and non-users, pre- and post-pandemic

	STW Users (2,671 firms)				Non-STW Users (3,841 firms)			
	2012-2019		2020-2024		2012-2019		2020-2024	
	mean	sd	mean	sd	mean	sd	mean	sd
Turnover (m€)	15.93	62.45	16.30	61.56	14.98	120.48	17.95	83.91
Employment (FTE)	69.42	146.86	71.73	162.34	51.96	137.15	56.48	164.58
Labor productivity (deflated turnover per FTE, €)	164,424	152,398	151,733	142,645	178,329	177,500	174,880	175,883
Firm Size								
Micro (<10)	0.19	0.40	0.21	0.41	0.31	0.46	0.28	0.45
Small (10-49)	0.48	0.50	0.46	0.50	0.46	0.50	0.47	0.50
Medium (50-249)	0.27	0.44	0.26	0.44	0.18	0.39	0.19	0.39
Large (250+)	0.06	0.24	0.07	0.25	0.05	0.21	0.05	0.23
STW: share of omitted hours (0-1)			0.23	0.21				
Introduced innovation	0.54	0.50	0.61	0.49	0.44	0.50	0.54	0.50
Increased digitalization			0.61	0.49			0.54	0.50
COVID Impact								
Extremely negative			0.13	0.33			0.01	0.12
Very negative			0.25	0.43			0.06	0.23
Negative			0.42	0.49			0.26	0.44
No impact			0.17	0.37			0.53	0.50
Positive			0.03	0.16			0.11	0.31
Very positive			0.00	0.07			0.03	0.16
# Observations	10,909		9,022		16,174		13,321	

Outcomes. We measure employment in full-time equivalents (FTE), crucially including the hours of employees under STW. Labor productivity is measured as deflated turnover per FTE. We deflate turnover relative to 2020, such that results more accurately reflect actual output changes per employee rather than post-shock inflation, e.g., effects of the energy crisis.

5 Empirical strategy

5.1 Selection and identification

Since basically all firms with employees were eligible for STW under COVID-era eligibility rules, we treat the decision to use STW as self-selected[1]. Similarly, firms choose whether or not to increase digitalization in response to the pandemic. For both, selection likely correlates with firm-specific characteristics prior to the shock and the severity of the crisis's impact. We describe this selection using probit models estimating STW take-up and crisis-induced digitalization, where one set of estimations uses 2019 firm characteristics as regressors, and the other uses reported shock severity (Section 6.1). Both models include industry dummies, absorbing average differences between industries.

5.2 Baseline specification

We estimate how STW use relates to employment and productivity using a dynamic conditional differences-in-differences framework covering pre-shock and post-shock periods. Let $STW_i \in [0,1]$ be the share of total working hours omitted through STW (zero for non-users). We formally estimate:

[1] An eligibility threshold of 10% affected employees applied to the majority of firm population and was difficult to monitor; leading to nearly all applications being granted.

$$lnY_{it} = \alpha_i + \lambda_t + \sum_{k=2020}^{2024} \beta_k \ (STW_i \times 1[t = k]) + Shock_i \times Post_t + \epsilon_{it} \quad (1)$$

where Y_{it} is full-time equivalent (FTE) employment (H1) or labor productivity measured as revenue per FTE (H2), and α_i and λ_t are firm and year fixed effects. The reference year is 2019. $Shock_i$ is the firm's self-reported COVID impact, measured on a 6-point Likert scale from 1 "Extremely negative" to 6 "Very positive", with category 4 "No impact" as the reference, interacted with a post dummy for years 2020-2024. Standard errors are robust. As we measure outcomes in logs and $STW_i \in [0,1]$, β_k is the difference, in log points, of moving from no STW to full STW. We report this alongside effects at mean treatment intensity among users to give more representative estimates.

We account for selection by re-weighting non-STW users through entropy balancing (Hainmueller, 2012). We balance on the first and second moments of pre-pandemic characteristics measured in 2019: log(employment), log(employment growth over 2016-2019), log(labor productivity), the working-from-home share in total working hours, whether the firm introduced any product/process innovation, and industry. Selection may also occur on shock severity, as firms making use of STW will have been more severely affected by the pandemic than non-users. We account for shock severity using a self-reported measure of the initial pandemic shock. Because it is measured at the same time as STW take-up, it may reflect the firm receiving support. We therefore report estimates with and without this contemporaneous measure as complementary specifications resting on different conditioning assumptions.

5.3 Heterogeneity: digitalization as a moderator

We investigate heterogeneity in employment and productivity outcomes by digital response using a triple-difference design and estimate it year by year, anchoring all interactions in 2019

such that changes capture changes relative to the immediate pre-pandemic baseline. Let Dig_i = 1 if the firm increased digitalization in response to the pandemic. We estimate

$$lnY_{it} = \alpha_i + \lambda_t + \sum_{k \neq 2019} [\beta_k (\boldsymbol{STW_i} \times 1[t = k]) + \phi_k (\boldsymbol{Dig_i} \times 1[t = k]) + \theta_k (\boldsymbol{STW_i} \times \boldsymbol{Dig_i} \times 1[t = k])] + Shock_i \times Post_t + \epsilon_{it} \quad (2)$$

The estimation traces four groups of firms: those that used neither STW nor digitalized (reference group), those that used only STW, those that digitalized only, and those that did both, while controlling for the severity of the shock. This allows us to identify differences across firm archetypes. Each coefficient is interpreted as follows:

- β_k: STW dose-outcome; users vs non-users, among non-digitalizers
- ϕ_k: Employment premium of digitalizers vs. non-digitalizers, in the absence of STW
- θ_k: the triple difference, the additional effect (slope) in the relationship between STW and employment for firms that digitalized

We report linear combinations of coefficients, with their respective standard errors, as they address the digitalization hypotheses directly:

- $\beta_k + \theta_k$: the STW dose-outcome among digitalizing firms;
- $\phi_k + \theta_k \times STW_i$: digitalization premium among STW users at intensity STW_i

Crucially, STW use and digitalization are chosen jointly by the firm in 2020, which separates this estimation from a conventional triple difference that requires the third dimension to be predetermined; therefore, we interpret θ as heterogeneity in the STW effect by digitalization, but we cannot read it as a causal moderation effect in this design.

6 Results

6.1 Selection into STW and predictors of digitalization

Before presenting our main results, we examine selection into the program and predictors of digitalization (Appendix Table 5). STW take-up and crisis-induced digitalization are systematically associated with both pre-shock firm characteristics and shock exposure.

For STW take-up, we uncover the following patterns. Relative to micro-firms, small, medium, and large firms are 7.6, 14.1, and 14.9 percentage points more likely to take up STW, respectively. Higher pre-pandemic labor productivity is associated with a lower probability of eventual participation, suggesting negative selection on firm quality into the scheme prior to any shock. This suggests that firms with already deteriorating prospects self-select into the program. Neither internal R&D nor existing working-from-home arrangements significantly predict eventual take-up. By comparison, shock severity is a far stronger predictor; relative to firms reporting no impact, take-up rises monotonically from -6.5 percentage points less likely among firms reporting very positive impact to 63.4 percentage points more likely among firms reporting extremely negative impacts, indicating a strong concentration of STW use among adversely affected firms once the shock materialized. This underscores the importance of accounting for heterogeneous shock exposure between scheme users and non-users.

Digitalization follows a similar pattern to STW take-up, but firm size effects are substantially larger: large firms were 48.7 percentage points more likely to digitalize than micro-firms, with small and medium firms in the middle, at 12.2 and 35.2 percentage points higher likelihood. Other correlates differ from STW take-up. Higher labor productivity, existing home-office arrangements, and prior internal innovative activity all positively predict digitalization, so productivity shows the opposite relationship to digitalization than it did to STW take-up. Digitalization is also associated with shock severity, though less strongly. Firms reporting

negative impacts are 6.3 to 8.9 percentage points more likely to digitalize than firms reporting no impact, while firms reporting positive impacts are also 5.2 percentage points more likely to do so.

Neither can therefore be treated as randomly assigned. This motivates our baseline design (Section 5), where entropy balancing, shock exposure controls and pre-trends checks address patterns above, but selection on firms' prospects remains. Therefore, we read the estimates as conditional associations.

6.2 STW, employment and labor productivity

Table 2 presents the baseline estimates of STW use in log(FTE) and log(labor productivity). We start with a baseline estimation (column 1a for employment, column 1b for productivity) and successively account for the severity of a firm's exposure to the shock (2a and 2b), then allow the coefficient of STW use to vary year by year (3a and 3b), and finally apply entropy-balanced weights constructed from 2019 firm characteristics as discussed in Section 5.2 (columns 4a and 4b). STW intensity is measured continuously as the share of working hours omitted (0 to 1), with non-users coded at zero; coefficients therefore reflect the log-point difference of a full-scale increase in this share, relative to non-use. The average treatment intensity among STW users is 0.235, or 23.5% of total working hours omitted through the scheme, which we use to interpret coefficients throughout. We convert the log coefficients to percentage terms using the exact transformation $\exp(0.235\,\beta_k) - 1$.

The pooled specification associates STW intensity with a large employment reduction on the log scale (-0.305^{***}). After accounting for shock severity, this falls to -0.150^{***}, indicating that shock severity explains part, but not all, of the reduction. At mean intensity, these correspond to employment reductions of approximately -6.9% and -3.5%. The dynamic entropy-balanced year-by-year estimation is significant at the 1% level in every year: At mean intensity among

STW users, this corresponds to a reduction of approximately -3.7% in 2020, widening to -7.7% in 2021 and -8.3% in 2022, -7.9% in 2023 and -6.1% by 2024. The employment divergence persists well beyond the crisis's acute phase. Labor productivity patterns look very different. At mean intensity, the entropy-balanced estimate shows that STW use is associated with -11.6% lower labor productivity in 2020 (-0.526***) and -7.9% in 2021 (-0.349***). From 2022 onwards, which also marks the end of the acute COVID crisis, the coefficient turns positive but largely insignificant (+0.6%, +2.8% and +3.8%*), The productivity differential relative to non-users disappears post-crisis. Figure 3 reports corresponding placebo tests. Pre-treatment coefficients are small and statistically insignificant; a joint test fails to reject that the 2016–2018 coefficients are jointly equal to zero for both employment (F(3,6518) = 0.35, p = 0.791) and productivity (F(3,6409) = 1.25, p = 0.288).

Figure 3: Placebo test - STW Intensity x Year, Employment and Productivity (Dynamic/Unbalanced)

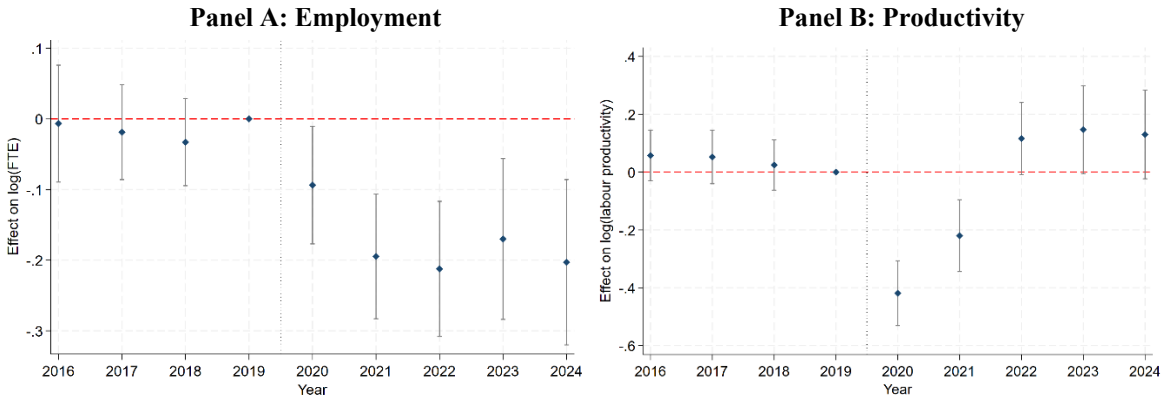


Table 2 (H1 & H2): Short-Time Work, Employment and Labor Productivity, with COVID-19 impact coefficients

	Employment (log FTE)				Productivity (log(revenue/FTE))			
	Baseline	+ COVID controls	Dynamic	Dynamic (EB)	Baseline	+ COVID controls	Dynamic	Dynamic (EB)
	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
STW intensity (share of hours omitted, 0-1)	-0.305***	-0.150***			-0.322***	-0.139***		
	(0.030)	(0.042)			(0.038)	(0.047)		
STW intensity x 2020			-0.079*	-0.162***			-0.438***	-0.526***
			(0.041)	(0.060)			(0.051)	(0.074)
STW intensity x 2021			-0.180***	-0.340***			-0.236***	-0.349***
			(0.043)	(0.064)			(0.055)	(0.080)
STW intensity x 2022			-0.203***	-0.367***			0.120**	0.027
			(0.047)	(0.070)			(0.058)	(0.082)
STW intensity x 2023			-0.161***	-0.349***			0.138**	0.116
			(0.056)	(0.082)			(0.065)	(0.092)
STW intensity x 2024			-0.184***	-0.269***			0.126**	0.158*
			(0.058)	(0.088)			(0.064)	(0.094)
COVID Impact (Ref.: neutral)								
Extremely negative		-0.138***	-0.138***	-0.066		-0.167***	-0.167***	-0.230***
		(0.031)	(0.031)	(0.046)		(0.028)	(0.028)	(0.041)
Very negative		-0.090***	-0.089***	-0.061***		-0.102***	-0.102***	-0.123***
		(0.016)	(0.016)	(0.023)		(0.018)	(0.017)	(0.026)
Negative		-0.030***	-0.030**	-0.011		-0.040***	-0.039***	-0.048***
		(0.012)	(0.012)	(0.017)		(0.011)	(0.011)	(0.018)
Positive		0.028	0.028	0.025		0.032*	0.033**	-0.007
		(0.018)	(0.018)	(0.022)		(0.017)	(0.017)	(0.020)
Very positive		0.119***	0.119***	0.087**		0.084**	0.084**	0.029
		(0.033)	(0.033)	(0.042)		(0.039)	(0.038)	(0.059)
# Observations	49,426	49,426	49,426	26,624	41,292	41,292	41,292	23,636
Within R-squared	0.010	0.015	0.015	0.026	0.015	0.020	0.030	0.046

Standard errors in parentheses. Firm and year fixed effects included in all columns; robust standard errors in parentheses.
*Columns 3-4 and 7-8 replace pooled STW intensity with year-specific (2020-2024) interactions; columns 4 and 8 apply entropy-balancing weights using pre-shock firm characteristics; any firm missing data to balance on is dropped. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

We interpret both results together. Productivity among STW users dips during the crisis and recovers afterward, while employment falls relative to non-users and does not return to their path. STW-using firms emerged from the crisis period clearly smaller. Evidence that they also became leaner (more productive) is weak: productivity estimates turn positive from 2022 but are significant only in 2024, at the 10% level, in the entropy-balanced specification (and at 5% in 2022–2024 without weights). H1, which predicts equal or higher employment than comparable non-users, is not supported, while H2, which predicts a rebound in productivity, is supported.

6.3 Robustness: STW intensity and a discretized dose-response estimation

Dose-specific estimates indicate negative associations that grow with STW intensity (Appendix Table 6). They rise from zero in the lowest bin to -0.12 for employment and to -0.18 for productivity in the highest bin (Appendix Table 6). However, a specification that allows for deviation from linear dose response does not reject linearity (Appendix Table 7: $p = 0.41$ for employment and $p = 0.37$ for productivity).

Our main specification assumes a linear dose-response, imposing a constant marginal effect irrespective of whether a firm omits 10% or 80% of total working hours. To analyze whether our findings depend on this restriction, we re-estimate the effects using the Callaway, Goodman-Bacon, and Sant'Anna (2024) approach. They propose two methods for our STW regressor: a binary estimation, which follows our main analysis results (Appendix Figure 7), and one that treats STW intensity as a multi-valued discrete variable, since reported intensity is concentrated at round values. We group users into four dose bins by intensity (up to 10%, 10-20%, 20-30%, above 30%, roughly equating to quartiles) and estimate, for each bin and year, the change in the outcome from 2019 relative to never-users. We aggregate the differences between adjacent bin coefficients into an average response, weighted by the share of users in

each bin. Unlike our base specification, this approach does not impose a constant marginal effect across treatment intensities, allowing a non-linear dose-response. Because the estimation is the average effect of a step up in bins and not a per-unit slope, we compare timing and shape, not size, with our estimates from Table 2. The results are consistent with the main specification (Appendix Table 7-9 and Figure 7-8). Employment shows no pre-trend and a persistent decline; productivity dips in 2020-2021, but this deficit disappears by 2022 and has a small positive pre-period drift (+0.01 to +0.02), but much smaller than the 2020 dip (−0.07). Overall, our results are robust to allowing a more flexible relationship with STW intensity, and we do not reject linearity.

6.4 Digitalization and the employment association of STW

For reference, we report the four different combinations of STW take-up and digitalization for the full sample of firms in Table 3.

Table 3. Digitalization response and short-time work take-up, 2020

Digitalization response	Non-STW users	STW users	Total
No digitalization	1,735 (26.6%)	1,028 (15.8%)	2,763 (42.4%)
Digitalization	2,106 (32.3%)	1,643 (25.2%)	3,749 (57.6%)
Total	3,841 (59.0%)	2,671 (41.0%)	6,512 (100.0%)

Notes: Entries are firm counts, with percentages of the full sample (N = 6,512) in parentheses. Percentages may not sum exactly due to rounding.

Table 4 reports the estimates of equation (2), allowing the STW association to differ by firms' digital response to the pandemic relative to 2019, the year before firms respond to the crisis through STW and/or digitalization. The triple difference coefficient (θ) captures the difference in the STW dose–outcome association between digitalizing and non-digitalizing firms.

Table 4 (H3): STW intensity, employment and productivity by digitalization status: Year-by-year.

	Log(FTE)	Log(Productivity)
Digitalized, no STW		
Digitalized x 2020	0.039***	-0.000
	(0.010)	(0.012)
Digitalized x 2021	0.043***	0.001
	(0.012)	(0.014)
Digitalized x 2022	0.049***	0.020
	(0.014)	(0.015)
Digitalized x 2023	0.059***	0.052***
	(0.016)	(0.017)
Digitalized x 2024	0.081***	0.031
	(0.018)	(0.019)
STW intensity (non-digitalizers)		
STW intensity x 2020	0.102*	-0.450***
	(0.056)	(0.068)
STW intensity x 2021	-0.026	-0.233***
	(0.059)	(0.073)
STW intensity x 2022	-0.042	0.103
	(0.065)	(0.072)
STW intensity x 2023	-0.026	0.160**
	(0.085)	(0.079)
STW intensity x 2024	-0.038	0.162*
	(0.091)	(0.092)
STW intensity x Digitalized (differential)		
STW intensity x 2020 x Digitalized	-0.161***	0.077
	(0.061)	(0.086)
STW intensity x 2021 x Digitalized	-0.113*	0.053
	(0.068)	(0.100)
STW intensity x 2022 x Digitalized	-0.129*	0.087
	(0.077)	(0.100)
STW intensity x 2023 x Digitalized	-0.085	0.012
	(0.097)	(0.116)
STW intensity x 2024 x Digitalized	-0.105	-0.008
	(0.106)	(0.121)
# Observations	49,426	41,292
Shock Control	Yes	Yes
Within R-squared	0.023	0.032

Standard errors in parentheses. Firm and year fixed effects included in all columns; robust (cluster-by-firm) standard errors in parentheses. Reference year is 2019. Estimated on the full sample without entropy weights.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Two main effects underlie the interaction. The STW association for non-digitalizing firms (β_k) is no longer robustly associated with negative employment change and even shows a positive coefficient (+0.102* in 2020) at the onset of the crisis. This is consistent with labor hoarding during the crisis's acute phase, and the coefficient is small and insignificant thereafter; however,

estimates are imprecise, and the 95% confidence intervals do not rule out 3–5% shortfalls in 2021–2024. Meanwhile, firms enjoy a digitalization premium (ϕ_k) independent of STW use, which is significant and growing each year, from +0.039*** in 2020 to +0.081*** in 2024.

The triple-difference coefficient (θk) is negative every year. It is significant in 2020-2022 (-0.161***, -0.113*, -0.129*), then loses significance in 2023-2024 (-0.085, -0.105). We compute the STW dose–outcome slope among digitalizing firms (through linear combination of $\beta_k + \theta_k$), and find −0.058, −0.138***, −0.171***, −0.111*, −0.143** from 2020 to 2024, which is approximately -1.4%, -3.2%, -3.9%, -2.6%, and -3.3% at the average STW intensity. Digitalizing STW users show a steeper dose-employment slope than non-digitalizing STW users. This does not imply that combining STW and digitalizing is always associated with negative employment trajectories, as those firms still enjoy the employment premium (ϕ_k). At average intensity, digitalizing users show more favorable employment trajectories than non-digitalizing STW users (+0.1%, +1.7%, +1.8%, +3.9% and +5.6%, estimated through linear combinations of $\phi_k + \theta_k \times STW_i$) but remain 2–4 percentage points lower than firms that digitalized without using STW. We interpret the negative θ_k as an erosion of digitalization's employment premium as STW intensity increases. The negative STW–employment association among digitalizing firms takes the form of a smaller share of digitalization-related growth. STW users digitalize more often (61.5%) than non-users (54.8%), so the smaller digitalization premium among users contributes to the aggregate employment gap reported in Table 2. Together, these results partially support H3A. Among STW users, the STW–employment association is more negative among digitalizing firms than among non-digitalizing firms. The STW-employment slope ($\beta_k + \theta_k$) for digitalizing firms remains statistically significant and negative from 2021 to 2024, but the differential (θ_k) is statistically significant only for 2020–2022. However, because digitalization generally generates growth, STW use attenuates rather than reverses the digitalization premium for firms that combine both.

Productivity follows a different pattern from employment. The non-digitalization STW-dose coefficient is significantly negative in 2020-2021 (-0.450***, -0.233***) before turning positive in 2022-2024. STW intensity alone therefore shows the same crisis-dip-then-recovery pattern as the aggregate sample. The productivity differential of digitalization, at zero STW intensity, is small and insignificant during the acute phase of the crisis (-0.000, 0.001, 0.020), becoming significant only in 2023 (0.052***), with 2024 (0.031) not significant. Digitalization alone has no clear productivity advantage. The differential (θ_k) for STW use and digitalization is insignificant throughout (0.077, 0.053, 0.087, 0.012, −0.008). The implied trajectory by combining the STW base with the differential ($\beta_k + \theta_k$) declines significantly in 2020–2021 (−0.373***, −0.180**), and recovers in 2022-2024 (0.190**, 0.172*, 0.154*). Therefore, among STW users, digitalizing and non-digitalizing firms follow nearly identical productivity trajectories, driven primarily by the STW-dose main effect. H3b is not supported.

7 Discussion

7.1 Smaller, possibly leaner

The microeconomic rationale for STW is to bridge temporary shocks: firms retain workers through the disruption, and those workers become productive again once activity recovers. Our results fit this scenario only partially. After the crisis's acute phase, STW users' productivity recovered and converged to that of non-users, but their employment remained below that of the comparison group. This aligns most closely with firms recovering productivity at the cost of employment, and with existing evidence from the GFC that labor-hoarding firms grow more slowly afterward (Greenhalgh-Stanley et al., 2026). Evidence that users became leaner is weak: productivity point estimates rise from 2022 and exceed those of non-users significantly only in 2024, at the 10% level. STW users emerged from the crisis clearly smaller and, at most, slightly leaner, contrary to the bridging rationale.

7.2 Short-time work and digitalization

Digitalization as a response to the crisis was associated with sustained employment growth, consistent with complementarities between digitalization and labor demand (Bresnahan et al., 2002; Moncada et al., 2025; Pedota et al., 2023) by allowing firms to sustain or expand activities under crisis conditions. Non-digitalizing users are informative in their own right. Among firms that did not digitalize, STW users kept pace with comparable non-users. They show no employment differences, and both follow the same-dip-then recovery productivity pattern. The bridging-logic schemes seem to hold for firms where the shock was a pause, and weaken where firms adapted.

Firms that combined STW with digitalization only partially enjoyed a digitalization-induced growth: the employment premium from digitalization shrinks as firms increasingly rely on STW. Two interpretations are consistent with our findings:

Idle capacity. Firms that did not make use of STW were presumably operating at (near) full capacity and had no reduced working hours to restore. For them, expanding activity after digitalization required hiring, whereas digitalizing STW users could restore employees' working hours before expanding their workforce.

Rigidity. Digitalization requires organizational readjustment, while subsidizing existing employment weakens firms' incentives to reorganize and redeploy labor effectively, limiting the employment gains from digital adoption.

While we cannot distinguish these two channels, we find that digitalization alone enables employment growth. STW acts as a defensive survival mechanism, and when firms do both, STW attenuates the growth associated with digitalization. The two responses are not incompatible, since many firms used both, but they do not reinforce each other.

Importantly, STW users who digitalize do not show a stronger productivity recovery than STW users who do not; both follow the dip-then-recover productivity path of the aggregate estimate, providing little evidence that digitalization was the path through which firms restored productivity beyond the pick-up of economic activity as the crisis ended.

7.3 Policy implications

Our findings do not imply that the scheme failed. A firm-level analysis of STW use does not capture its effectiveness in protecting aggregate demand (economy-wide counterfactual) or rule out that scheme-using firms would have seen even larger employment decreases without the scheme in place (firm-level counterfactual) (Aiyar & Dao, 2021). For the COVID-19 crisis in particular, the scheme also played an important role in facilitating mandatory lockdowns and preventing the spread of the virus. However, they raise important questions about how job retention schemes affect employment beyond their immediate stabilization effects in settings that require structural change.

First, judging the policy's effectiveness only by its effect on unemployment may be incomplete. Under broad eligibility, the scheme reaches firms that will not return to their pre-crisis trajectories, and the scheme's use coincides with smaller gains from adapting to the changed business environment.

Second, take-up and intensity are informative measures. Firms with lower pre-pandemic productivity are more likely to take up the scheme despite deteriorating prospects. The employment gap between users and non-users is absent amongst low-intensity users and increases with intensity. Policymakers can monitor this in real time, although we concede that distinguishing temporary from persistent disruptions per firm is difficult; intensity is a predictor of eventual employment outcomes.

Third, adaptation paid off for STW users too: digitalizing users grew faster than non-digitalizing users, indicating that STW did not promote firms to use the scheme and do nothing (despite these firms keeping up with comparable non-users who did not digitalize). When a crisis also requires structural change, the scheme aims not only to protect employment but also to incentivize affected firms to adapt and grow. Our results suggest that it may be useful to pair STW with training, redeployment, or support for broader firm reorganization.

We highlight a tension between retaining employment and adapting. STW protects employment during temporary disruption. However, when crisis also requires structural change, as the surge in digital adoption during COVID-19 did, subsidizing firms to retain their existing structure may reduce subsequent growth. Therefore, the insurance-reallocation trade-off highlighted in the literature between firms may also operate within firms. Part of the cost of STW use takes the form of foregone growth rather than job destruction, to the extent that a smaller digitalization premium among users reflects weakened incentives to reorganize and organizational frictions.

7.4 Limitations

Our study uses a unique database that allows us to study STW use, digitalization, and the affectedness by the COVID-19 disruption at the firm level and its consequences on labor demand and productivity. The breadth of data comes with some limitations in terms of data depth. First, we are limited in addressing self-selection into STW since we cannot capture differences in the firm's prospects at the time of the disruption. Since firms with deteriorating prospects are more likely to self-select into the scheme, our estimations cannot be interpreted as strictly causal but should be read rather as associations. More detailed data on firm prospects would allow a much better identification of self-selection, which we have to leave to future research.

Second, we are able to observe STW use and digitalization for the year 2020 only. We lack information on the duration of STW and on digitalization efforts in later periods, which may alter our results for the years 2021 onwards. Thirdly, our shock measure may not capture the full impact of COVID-19 on firms and leaves substantial variation within an impact category. More detailed data on the consequences of COVID-19 on firms over time would be highly useful to conduct more detailed analysis on the dynamic effects of STW. Another limitation is linked to the post-shock nature of the measure on COVID-19 affectedness. Firms using STW may have felt the disruption as less severe in an ex-post perspective, probably indicating the COVID impact as "negative," but might have reported "very negative" when not having used the scheme.

Fourthly, our digitalization measure captures a broad set of potential adaptations (offensive or defensive) which we cannot distinguish from one another. This greatly limits the depth of analysis of the type of digitalization that helps firms avoid adverse impacts of COVID-19 and recover quickly. We have to leave such more nuanced analysis to future research, too.

Fifth, by measuring labor productivity as deflated turnover per employee, we capture efficiency gains only insofar as they raise revenue. Gains through non-labor cost reduction or quality improvements do not show up in this measure.

Finally, the Mannheim Innovation Panel does not cover all industries heavily affected by COVID-19 but focuses on manufacturing and business services. The role of STW and digitalization may be different in other industries, such as trade, hotels & restaurants, and personal services.

8 Conclusion

Short-time work schemes (STW) help firms weather temporary shocks by preserving employer-employee relationships and were deployed at an unprecedented scale during the COVID-19 crisis. Following German STW users from the outbreak of the pandemic until the end of 2024, we find that STW use was associated with a persistent decline in employment well beyond the shock's acute phase. At the same time, STW users recover productivity and adjust employment downward. We combine two dimensions that are often considered separately: employment outcomes following STW use, and the role of crisis-induced digitalization in shaping them. Digitalization as a crisis response was associated with sustained firm growth, but less so as firms relied more on STW, with no corresponding productivity advantage.

Our findings highlight a tension between protecting employment during an exogenous disruption and adapting to a crisis that rewarded changes in how firms operate. They do not establish that STW failed but suggest that preserving employment relationships does not necessarily restore pre-crisis employment trajectories, particularly when disruptions coincide with changes in how firms operate.

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Appendix

Table 5: Selection into treatment: average marginal effects on STW take-up and Digitalization

	Pr(Take up STW)			Pr(Digitalization)		
	Baseline	+WFH/R&D	COVID impact	Baseline	+WFH/R&D	COVID impact
Firm size$_{2019}$(ref.: micro)						
Small (10-49)	0.081***	0.076***		0.122***	0.092***	
	(0.017)	(0.018)		(0.017)	(0.018)	
Medium (50-249)	0.147***	0.141***		0.352***	0.276***	
	(0.021)	(0.023)		(0.020)	(0.021)	
Large (250+)	0.154***	0.149***		0.487***	0.413***	
	(0.032)	(0.034)		(0.024)	(0.028)	
Log(Productivity)$_{2019}$	-0.042***	-0.040***		0.095***	0.079***	
	(0.011)	(0.012)		(0.011)	(0.011)	
WFH		-0.026			0.147***	
arrangement$_{2019}$		(0.017)			(0.016)	
Internal R&D$_{2019}$		0.020			0.109***	
		(0.020)			(0.019)	
Covid Impact (ref.: neutral)						
Extremely negative			0.634***			0.074***
			(0.020)			(0.026)
Very negative			0.524***			0.063***
			(0.017)			(0.019)
Negative			0.316***			0.089***
			(0.013)			(0.014)
Positive			-0.048***			0.052**
			(0.018)			(0.024)
Very positive			-0.065**			0.058
			(0.030)			(0.044)
# Observations	4,352	3,955	6,512	4,352	3,955	6,512
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.105	0.106	0.238	0.160	0.180	0.068

*Standard errors in parentheses. Models 1-2 and 4-5: year==2019 cross-section. Models 3 and 6: year==2020 cross-section. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Table 6: Dose-response ATT(d|d) by STW-intensity dose bin (Reference: no STW use)

	Employment (log FTE)	Productivity (log)
Up to 10%	0.006	0.003
	(0.011)	(0.013)
10-20%	-0.032**	-0.058***
	(0.014)	(0.016)
20-30%	-0.063***	-0.100***
	(0.015)	(0.023)
Above 30%	-0.121***	-0.177***
	(0.018)	(0.023)
# Observations	4,604	4,315
R-squared	0.015	0.028

*Standard errors in parentheses. Dependent variable: average outcome 2020-2024 minus outcome in 2019, one observation per firm. Linearity test (dose-bin dummies jointly zero, given a linear dose term): p = 0.411 (employment), p = 0.372 (productivity). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Table 7: Linearity test regression (linear dose term plus dose bins)

	Employment (log FTE)	Productivity (log)
STW intensity (0-1, linear term)	-0.265***	-0.243
	(0.099)	(0.166)
Up to 10%	0.023*	0.018
	(0.013)	(0.017)
10-20%	0.014	-0.016
	(0.022)	(0.033)
20-30%	0.012	-0.032
	(0.032)	(0.053)
Above 30%	0.025	-0.045
	(0.053)	(0.081)
# Firms	4,604	4,315
R-squared	0.017	0.030

Standard errors in parentheses. Dependent variable: average outcome 2020-2024 minus outcome in 2019, one observation per firm. Same sample and dependent variable as Table 6, with a linear dose term added. Joint test that the four dose-bin coefficients are zero: p = 0.411 (employment), p = 0.372 (productivity)
** p < 0.10, ** p < 0.05, *** p < 0.01*

Table 8: Effect by dose bin, by year (Employment (log FTE))

	2016	2017	2018	2020	2021	2022	2023	2024
Up to 10%	-0.027	-0.001	-0.002	0.002	-0.013	-0.001	-0.006	0.015
	(0.017)	(0.014)	(0.011)	(0.010)	(0.012)	(0.014)	(0.017)	(0.022)
10-20%	0.002	0.006	0.009	-0.018	-0.035*	-0.041*	-0.054**	-0.017
	(0.022)	(0.020)	(0.016)	(0.013)	(0.018)	(0.024)	(0.025)	(0.026)
20-30%	0.011	0.031	0.014	-0.027**	-0.081***	-0.108***	-0.077***	-0.114***
	(0.024)	(0.020)	(0.015)	(0.013)	(0.020)	(0.022)	(0.022)	(0.032)
Above 30%	0.062*	0.033	0.042**	-0.076***	-0.128***	-0.139***	-0.128***	-0.156***
	(0.032)	(0.028)	(0.020)	(0.018)	(0.021)	(0.025)	(0.032)	(0.038)
Average step	0.010	0.005	0.009*	-0.017***	-0.028***	-0.026***	-0.029***	-0.025**
SE of av. step	0.009	0.008	0.005	0.005	0.006	0.007	0.009	0.010
# Firms	3,100	3,183	3,716	4,604	3,739	3,222	2,896	2,547

*Standard errors in parentheses. One regression per year. Dependent variable: outcome in that year minus outcome in 2019 (Employment (log FTE)). Average step (ACR): bin effects differenced and weighted by the share of users in each bin. * p < 0.10, ** p < 0.05, *** p < 0.01*

Table 9: Effect by dose bin, by year (Productivity (log))

	2016	2017	2018	2020	2021	2022	2023	2024
Up to 10%	0.028	0.015	0.024	-0.026**	0.042**	0.036**	0.038*	0.011
	(0.019)	(0.018)	(0.016)	(0.013)	(0.017)	(0.018)	(0.023)	(0.025)
10-20%	0.012	0.038	0.029	-0.110***	-0.003	0.026	-0.025	-0.018
	(0.023)	(0.025)	(0.018)	(0.015)	(0.022)	(0.028)	(0.024)	(0.030)
20-30%	0.025	0.043	0.027	-0.169***	-0.099***	-0.013	-0.033	-0.044
	(0.031)	(0.027)	(0.023)	(0.021)	(0.038)	(0.038)	(0.038)	(0.037)
Above 30%	0.054	0.039	0.020	-0.317***	-0.218***	-0.050	-0.015	0.011
	(0.033)	(0.031)	(0.025)	(0.026)	(0.032)	(0.032)	(0.037)	(0.041)
Average step	0.015	0.011	0.008	-0.072***	-0.035***	-0.002	0.001	0.005
SE of av. step	0.009	0.009	0.007	0.007	0.009	0.009	0.010	0.011
# Firms	2,431	2,567	3,424	4,245	3,064	2,567	2,361	1,972

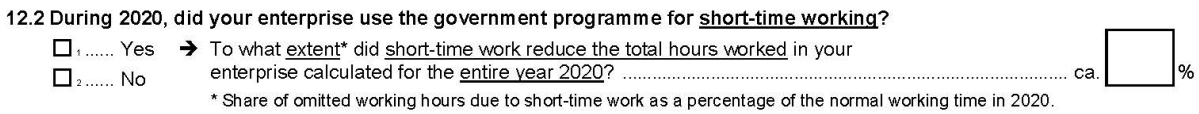
*Standard errors in parentheses. One regression per year. Dependent variable: outcome in that year minus outcome in 2019 (Productivity (log)). Average step (ACR): bin effects differenced and weighted by the share of users in each bin. * p < 0.10, ** p < 0.05, *** p < 0.01*

Table 10 (H1 & H2): Short-Time Work on Employment, with COVID-19 impact coefficients

	Employment (FTE)			
	Baseline	+ Corona	Dynamic	Dynamic (EB)
STW intensity (share of hours omitted, 0-1)	-11.585***	-7.261***		
	(1.013)	(1.327)		
STW intensity x 2020			-3.563**	-5.558***
			(1.434)	(2.016)
STW intensity x 2021			-8.177***	-12.385***
			(1.425)	(2.139)
STW intensity x 2022			-10.068***	-15.550***
			(1.690)	(2.448)
STW intensity x 2023			-7.953***	-12.143***
			(1.555)	(2.301)
STW intensity x 2024			-9.985***	-14.374***
			(1.930)	(3.063)
COVID Impact (ref: no impact)				
Extremely negative		-3.364***	-3.363***	-2.738*
		(1.036)	(1.036)	(1.582)
Very negative		-2.552***	-2.539***	-1.845
		(0.822)	(0.822)	(1.349)
Negative		-1.469***	-1.461***	-0.609
		(0.550)	(0.550)	(1.056)
Positive		1.199	1.191	-1.518
		(1.195)	(1.195)	(1.665)
Very positive		7.672***	7.675***	4.065
		(1.823)	(1.825)	(2.706)
# Observations	49,426	49,426	49,426	26,624
Within R-squared	0.009	0.011	0.012	0.024

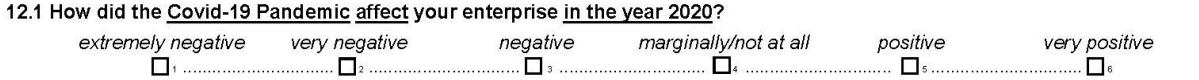
Standard errors in parentheses. Firm and year fixed effects included in all columns; robust standard errors in parentheses. * *p < 0.10,* ** *p < 0.05,* *** *p < 0.01*

Figure 4: Question on the use of short-time work



Source: German Innovation Survey (MIP 2021)

Figure 5: Question on the impact of COVID-19 on the firm's business activities



Source: German Innovation Survey (MIP 2021)

Figure 6: Question on strategic reactions to COVID-19

12.5 Did your enterprise use any of the following measures to react on the Covid-19 Pandemic, and were these measures temporary or permanent in nature?

	Yes, temporarily	*Yes, permanently*	*No*
Reducing internal cost of production/service delivery	☐ 1	☐ 2	☐ 3
Reorganising supply to increase robustness of supply chains	☐ 1	☐ 2	☐ 3
Reorganising sales towards domestic customers	☐ 1	☐ 2	☐ 3
Developing new market segments or customer groups	☐ 1	☐ 2	☐ 3
Increasing digital offers and digital distribution channels	☐ 1	☐ 2	☐ 3
Increasing digitalisation within the enterprise (e.g. telework, digital communication)	☐ 1	☐ 2	☐ 3
Introducing new products or services	☐ 1	☐ 2	☐ 3
Stopping to offer certain products or services	☐ 1	☐ 2	☐ 3

Source: German Innovation Survey (MIP 2021)

Figure 7: Any STW use vs. no STW use

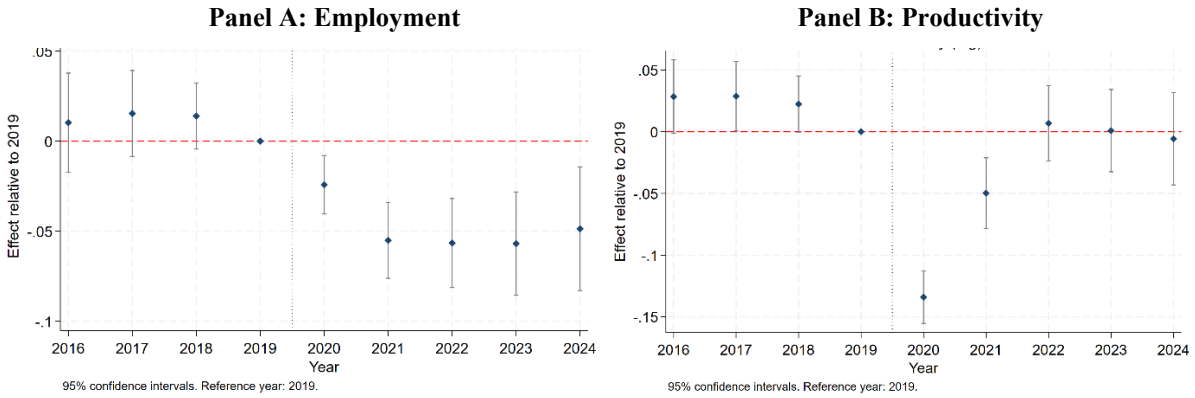
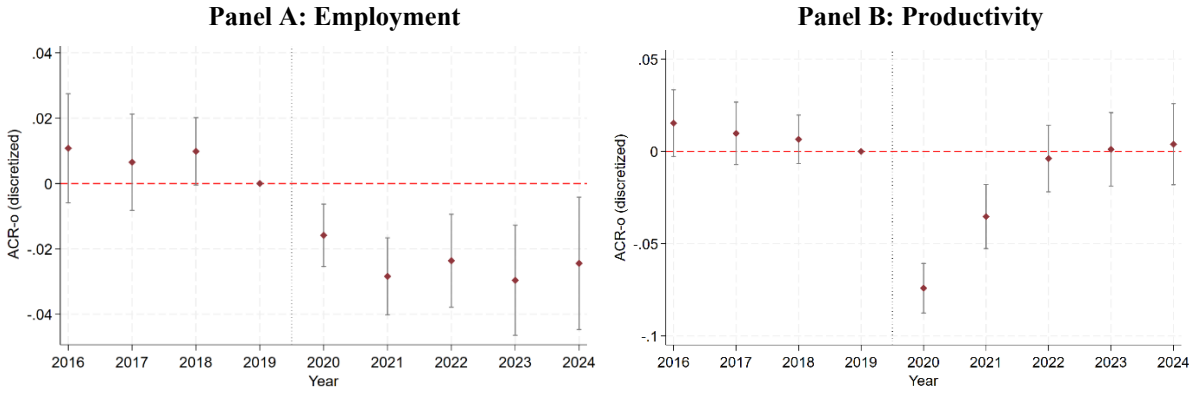


Figure 8: Discretized Average Causal Response





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