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Estimating the Impact of Digitalization on Firm Performance During COVID-19

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Abstract Using the coronavirus pandemic (COVID-19) as a natural experiment, recent studies have shown that digitalization has been key for improved firm performance. While earlier studies mostly use the canonical binary difference-in-difference design, recent advances have shown that this may lead to biased results when the digitalization treatment variable is continuous (treatment heterogeneity). In this paper we show that digitalization, measured in terms of online sales, has indeed been a significant driver of firm performance during the coronavirus pandemic (COVID-19) also when considering treatment heterogeneity. Using World Bank Enterprise Surveys data for ten Western European countries shows that initially fully digitalized firms with 100% of sales online experienced 10%-point higher firm-level sales growth during the pandemic when average firm sales contracted by 8.7%. Our results are robust to controlling for unobserved and observed firm heterogeneity, treatment heterogeneity and sample selectivity, while the common trend assumption is validated and further robustness is confirmed using a placebo test. This study contributes to the literature by providing a rigorous estimate of the importance of digitalization for firm performance within the context of a natural experiment.

Keywords Firm Behavior: Empirical Analysis (D22) · Business Fluctuations (E32) · Firm Performance (L25) · Digitalization · E-commerce (L81) · COVID-19

The data used in this paper are publicly accessible at the World Bank Enterprise Surveys website (<https://www.enterprisesurveys.org/>).

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1 Introduction

Digitalization is increasingly recognized to be a key driver of firm performance if not a *sine qua non* for firm growth and survival in an uncertain business environment. The outbreak of the COVID-19 pandemic is a clear case in point—with supply chain disruptions, lockdowns and social distancing measures imposed by governments across the world, businesses had to find ways to cope and survive in this new environment and cater to consumers who were limited in the ways they could access products and services. Whether this was through implementing remote work-policies or increasing online services, or both, there was a rush to digitalize businesses inside and out: the pandemic acted as an accelerator of digitalization (de Vet et al. 2021) and expedited digitalized customer and supply-chain interactions by three to four years according to a global survey of executives (LaBerge et al. 2020).

While earlier studies have tried to estimate the impact of various firm characteristics on firm performance under crises, the role of digitalization has only recently come to the forefront, particularly following the coronavirus pandemic (Abidi et al. 2023; Bai et al. 2021; Cong et al. 2024; Copestake et al. 2022; Muzi et al. 2021; Nose and Honda 2023). Most of these studies use a difference-in-difference design exploiting the unexpected nature of the COVID-19 pandemic shock as a credible natural experiment to evaluate the impact of digitalization on firm performance under crises. Despite this common approach, they differ widely with respect to the digitalization measure used (e.g., website, share of online sales, foreign technology, work-from-home feasibility, shares of inputs from ICT industries) as well as the measure of firm performance (such as change in sales, firm survival, stock returns, digital technology investment). Likewise, some studies rely on sector-level (Cong et al. 2024; Copestake et al. 2022) rather than firm-level measures of digitalization (e.g., Abidi et al. 2023; Muzi et al. 2021), or focus on a specific sample of publicly listed firms (Bai et al. 2021; Copestake et al. 2022) ignoring unlisted firms (e.g., Abidi et al. 2023; Nose and Honda 2023).¹ Such choices matter for the robustness and interpretation of the results. Nevertheless, the studies typically find positive impacts from digitalization on firm performance across various digitalization and outcome measures regardless of the choice of countries or regions.

Earlier studies have typically assumed that a standard difference-in-difference specification controls adequately for any confounding factors related to digitalization. Specifically, it has been assumed that pre-COVID-19 trends in firm performance are equal between more and less digitalized firms (after the inclusion of control variables), allowing for a causal interpretation of the difference-in-difference estimates (common trend assumption). Also, studies have commonly not taken into account recent advances in the study of causal identification using the difference-in-difference framework. Specifically, it has been shown that using a continuous treatment vari-

¹ Using sector-level measures is especially problematic when the difference-in-difference design covers two time periods (before-after design), leading to possible biased estimates in the presence of sector-specific trends. Sector-specific measures are also more noisy measures as they do not capture within-sector variation in firm-level digitalization intensity leading to possible attenuation bias (due to measurement error).

able instead of comparing treatments that are either “off” or “on” as in the canonical (i.e., binary) difference-in-differences design may not identify causal effects without allowing for differences in level and slope effects (Callaway et al. 2021). Because the proxies used for digitalization are typically continuous, the possibility of treatment heterogeneity should not be dismissed when aiming for a causal interpretation of estimated difference-in-difference digitalization effects.

In this paper we estimate the impact of digitalization on firm performance using a continuous difference-in-difference strategy but testing for treatment heterogeneity as well as validating the common trends assumption, and checking robustness with a placebo test. The analysis draws on a sample of ten European countries surveyed in the World Bank’s 2020 and 2021 Enterprise Surveys.² As measure of digitalization, the (pre-pandemic) share of online sales as percent of total sales in 2018/19 is used, capturing the treatment intensity by which the pandemic has affected the different firms.³ While there are many different aspects of digitalization, which may also vary in importance across sectors, online sales are particularly relevant in the COVID-19 context; they directly reflect a firm’s ability to sustain or expand business activity when physical restrictions are in place, thereby serving as an appropriate proxy of digitalization. Unobserved firm-level differences in sale levels are controlled for by using the change in firm sales as dependent variable, while country-location, sector and year-month fixed effects and a range of firm characteristics are included to control for possible firm-level differences in sale trends.

We find that initially fully digitalized firms with 100% of sales online experienced 10%-point higher firm-level sales growth during the pandemic, *ceteris paribus*, when average firm sales contracted by 8.7% on average. Validation of the common trend assumption, together with a placebo (permutation) test, further reinforces the causal interpretation of the result. Also, we do not find evidence for impact heterogeneity when using interaction terms of pre-pandemic online sales with a wide variety of firm characteristics, with one exception namely a more negative impact for large firms.⁴ The results are robust to the inclusion of a Heckman sample selectivity correction term to control for the fact that only firms that report sales during COVID-19 compared to pre-COVID-19 period and online sales in the pre-COVID-19 period can be included in the analysis.

Our findings contribute to the current literature on firm behavior and firm performance during COVID-19 by focusing on digitalization and its causal impact. We confirm that earlier findings on the positive impacts of digitalization on firm performance are not confounded by uncommon trends and/or treatment heterogeneity. We also note that this paper speaks to the larger literature on drivers of firm performance during crises. This literature has clearly shown that firms’ resilience

² Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Luxembourg, the Netherlands, and Sweden.

³ While other digitalization measures in our data are limited, we also test whether the estimated effects from online sales are robust to controlling for whether the firm has a website or uses foreign technology as additional proxies for digitalization (cf. Abidi et al. 2023).

⁴ We also test for using a sector-level (Cong et al. 2024; Copestake et al. 2022) rather than our preferred firm-level measure of digitalization and find that this biases the estimated impact in our study downward. These results are available upon request.

is enhanced with better access to finance (e.g. Correa and Iootty 2010; Guimbert and Oostendorp 2016; Amin and Viganola 2021), while the impact of being part of a Global Value Chain (GVC) remains somewhat ambiguous as GVCs can both propagate and mitigate external shocks depending on a company's position within a GVC (Meliciani and Tchorek 2017; Brenton et al. 2022). Also, while the role of other firm characteristics such as firm productivity, firm age, firm size, human capital, foreign ownership, export orientation and innovation have been studied, the role of digitalization has only recently gained more traction.

The paper is organized as follows: Section 2 describes the data and methodology used in this study; Section 3 outlines the results; Section 4 discusses robustness and validity checks; and Section 5 concludes with a discussion, policy implications and directions for future research.

2 Data and Methodology

2.1 Data Description

The data used in this research come from The World Bank's Enterprise Surveys (ES). The ES are firm-level surveys conducted by private contractors on behalf of The World Bank across the world, with the goal of providing indicators of the business environment that can be compared across countries and time (The World Bank Enterprise Surveys 2022). This is achieved by using stratified random sampling based on region, sector and firm size to obtain a representative sample of an economy's private sector at the country and sector level, and by using standardized questionnaires for international and longitudinal comparability.⁵ The data include probability weights to allow for statistical inference for the population of non-agricultural private firms.

Respondents of these surveys are business owners, top managers, as well as HR-managers and accountants of registered companies with five or more employees in the manufacturing and services sectors (corresponding ISIC Rev. 3.1 codes: 15–37, 45, 50–52, 55, 60–64, 72). Information is collected on firm characteristics, annual sales, costs, access to finance, gender participation, workforce composition, and innovation and technology among others. Unique to the 2020/21 ES is the inclusion of additional questions about specific COVID-19 related obstacles potentially faced by a firm, such as how many weeks a firm was closed due to the pandemic. Normally ES interviews are conducted face-to-face, however during the pandemic the interviews were conducted through a mixture of face-to-face and phone interviews.

The ES included in this research were carried out in 2020 and 2021 and cover ten European countries: Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Luxembourg, The Netherlands, and Sweden.⁶ The total number of firm-level

⁵ For the full explanation of the ES Methodology, see <https://www.enterprisesurveys.org/en/methodology>.

⁶ The ES are not always annual surveys. The inclusion of both years has to do with data availability: for some countries only data for 2020 is available, for others only 2021. Inclusion of both years allows us to extend the analysis for more countries and increase the sample size.

observations for these ten countries in the ES is 8403. After dropping observations because of missing values of (online) sales and reports outside the COVID-19 period,⁷ the baseline sample for this paper's analysis consists of the 3153 observations that include information on the sales during COVID-19 compared to the pre-COVID-19 period and online sales in the pre-COVID-19 period. The Appendix includes a sample description of the countries and sectors studied in Tables A1 and A2 respectively, and a formal definition of all variables included in the empirical analysis as well as descriptive statistics are provided in Table A3.

2.2 Change in Firm Sales Under COVID-19

In this paper we focus on the change in firm sales under COVID-19 as our main measure of firm performance, measured as the firm's percentage change in the last reported monthly sales ($\Delta Sales$) in 2020 or 2021 compared to the same month in 2019. In the baseline sample, 28% of firms reported an increase in sales, 21% no change, and 51% a decrease, with a mean change in overall sales of -8.7% (all figures weighted by probability weights). Figures 1 and 2 provide further insights into the change in sales by country and sector. Both graphs include 95% confidence intervals. Figure 1 maps the average change in sales from 2019 to 2020/21 per country. On average, firms in the majority of countries experienced a drop in sales compared to 2019. Only firms in Denmark experienced on average a slight increase in sales, however this is less than a 1% increase with the 95% confidence interval revealing high variance across firms. Finland also experienced a relatively small average decrease of sales of under 2%, while the highest decrease in sales on average of close to 25% was experienced by firms in Luxembourg, although for a relatively small sample.⁸

The differences between countries could be partly related to the number of COVID-19 cases and severity of lockdowns. Denmark and Finland adopted partial lockdowns in the spring of 2020, and Denmark again a more stringent one for the winter months of 2020–2021, while the remaining countries adopted more stringent lockdowns multiple times throughout 2020 and 2021, including stay at home orders at least once (ECDC 2022). Other factors, such as population density and degree of societal trust also play a role in differences between pandemic cases and resulting severity of lockdowns, with governments in Nordic countries, for example, being able to rely much more heavily on recommendations than formal restrictions compared to other EU-countries (Saunes et al. 2021). Noticeable, however, are the

⁷ Also, 16 firm-level observations for France with non-annual fiscal years are excluded. The other firms are dropped from the baseline sample because of the timing of the interview, leading to reports of changes in sales before the onset of COVID-19 and/or reports of online sales after the onset of COVID-19. To the extent that the timing of interviewing is correlated with unobserved factors, this may create a bias in the estimated results. Therefore, it is verified whether the results are robust to controlling for sample selectivity (see section 4).

⁸ The baseline sample includes 92 firms for Luxembourg (refer to Table A1 in the Appendix for breakdown of firms per country).

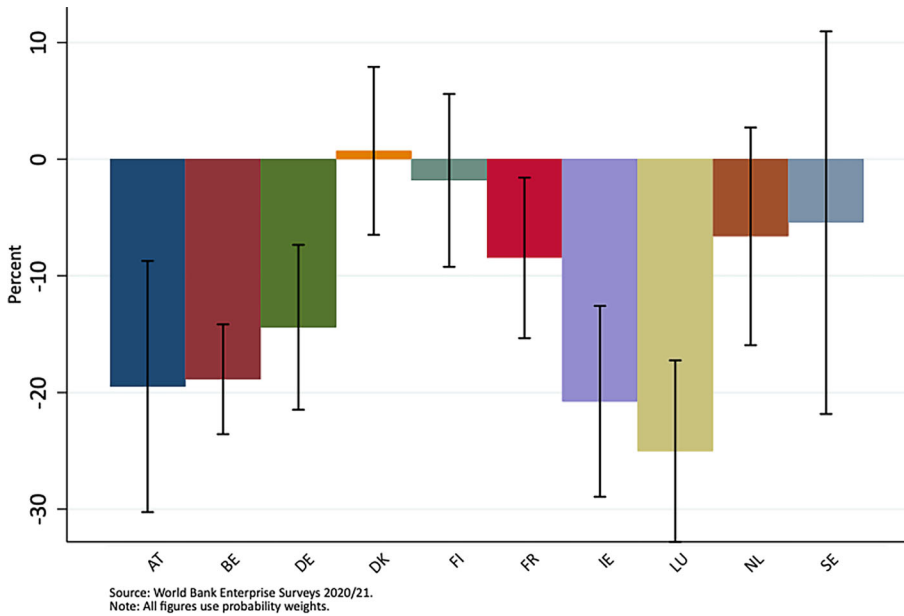


Fig. 1 Average change in sales from 2019 by country

large confidence intervals around the means reflecting large within-country firm-level variation in performance under COVID-19.⁹

Figure 2 shows that the impact of the pandemic on the average change in sales from 2019 to 2020/21 has been very uneven across 2-digit ISIC (Rev. 3.1) sectors.¹⁰ On average, firms in most sectors experienced a drop in sales in 2020/21 compared to the same month in 2019, with the most notable exception being IT showing a positive growth of 25%. This finding is in line with the European Parliament's research on the impact of COVID-19 on sectors at EU-level (de Vet et al. 2021). Conversely, firms belonging to the leather and hotel/restaurants sectors saw approximately a halving of their sales. This is also unsurprising, as lockdowns disrupted especially the demand for luxury goods and primarily targeted the hospitality sector, with either partial or full closures of restaurants and bars across the countries, limiting tourism. Although the small number of observations in some sectors may account for some of the large 95% confidence intervals, the latter are also large in sectors with large numbers of observations reflecting large within-sector firm-level variation in performance under COVID-19.¹¹

⁹ The average country-level coefficient of variation of the firm-level changes in sales is 11.3 for the base-line sample.

¹⁰ The sample includes a total of 27 sectors. For some countries, more than 27 sectors were reported due to subdivisions in certain sectors (e.g., "Transport machines"), however these have been merged according to the larger sub-categories (e.g., sector 35 merged into sector 34 as they both fall under "Transport Machines") to increase sample sizes and create more precise sector-level estimates.

¹¹ See Table A2 in the Appendix for the full break-down by sector.

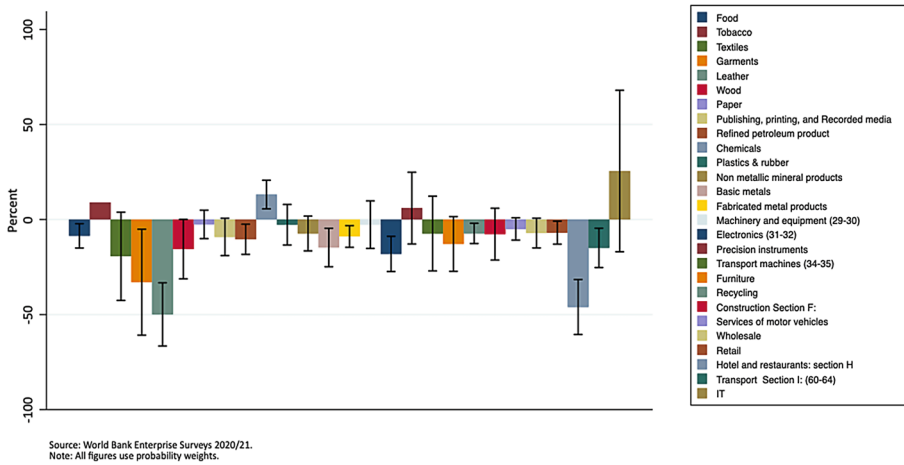


Fig. 2 Average change in sales from 2019 per sector

2.3 Online Sales

Our measure of digitalization, *Online sales*, is reported directly in the ES by the share of total sales through online channels as per last fiscal year (%). In the baseline sample, only firms are included for which the last fiscal year was 2019 or 2018, reflecting pre-COVID-19 online sales. Online channels include web-based platforms, social media platforms, establishment's website, and smartphone app. *Online sales* thus measures the share of online sales pre-pandemic and provides a direct measure of the degree of digitalization.

Figure 3 shows large country-level variation in digitalization, with Austria, Denmark, and The Netherlands being most digitalized and generating on average 10–11% of their sales through online channels. Firms in Luxembourg, on the other hand, generate only 3% of their sales through online channels on average, making it the least digitalized country (in terms of online sales) in our sample and, interestingly, also the country with the largest decline in subsequent sales (Fig. 1).

Given that we also observe a large variation in the share of online sales across sectors (see Figure A1), we can also consider the relationship between (pre-COVID) online sales and subsequent sales performance during COVID at the sector level across countries. Figure 4 shows that this relationship is positive and statistically significant, suggesting that sectors with an additional 10% of pre-COVID-19 online sales experienced a 3.5%-point higher sales growth subsequently. Of course, this bivariate sector-level relationship may be confounded by other factors that are not included such as the contact-intensity of the production process and firm-consumer relationship, and therefore we now turn to a firm-level analysis to control for such omitted factors.

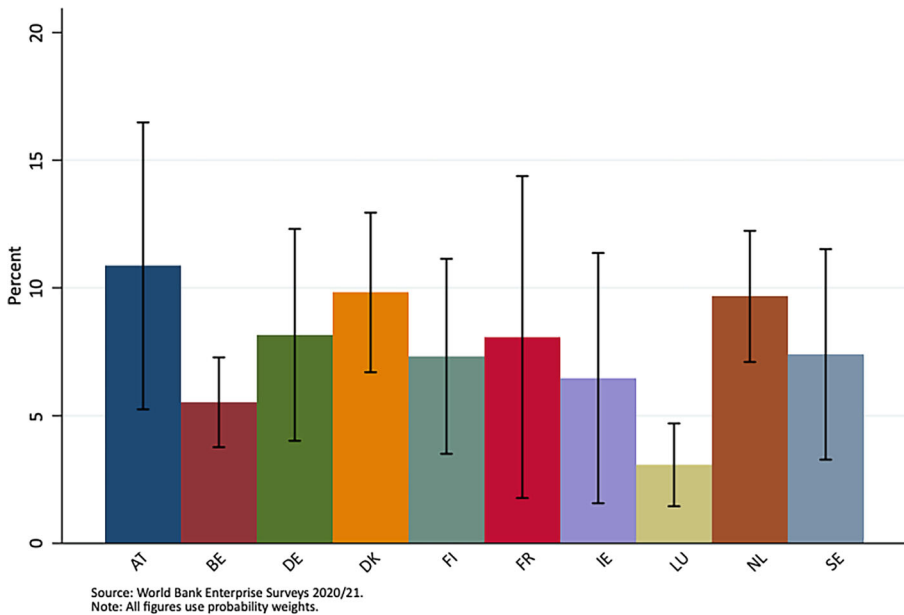


Fig. 3 Average share of pre-COVID-19 online sales by country

2.4 Model Specification

To investigate the causal effects of the share of online sales on the change in sales of firm i , we first estimate the following version of a difference-in-difference equation:

$$\Delta Sales_{i,COVID-19} = \alpha + \beta Online\ sales_{i,pre-COVID-19} + \theta Z_i + D_c + D_s + D_t + \varepsilon_i \quad (1)$$

Where $\Delta Sales_{i,COVID-19}$ denotes the percentage change of sales of firm i in 2020/21 (COVID-19) compared to the same month in 2019 (pre-COVID-19). The variable $Online\ sales_{i,pre-COVID-19}$ captures the share of online sales of firm i in 2018/19 (pre-pandemic), β is the coefficient of interest capturing the effect of the level of online sales on the change of firm sales from 2019 to 2020/21, Z_i refers to a vector of firm controls, D_c , D_s and D_t denote a set of dummies that account for country-location, sector and year-month fixed effects respectively and ε_i is the error term.¹² Eq. 1 can be interpreted as a first-difference representation of a two-way fixed effects (TWFE) model allowing for differential trends across Z_i , D_c , D_s and D_t . Within this interpretation, the variable $Online\ sales_{i,pre-COVID-19}$ indicates the treatment intensity, or “dose”, by which the pandemic has affected the different firms. Hence, instead of having the canonical difference-in-differences design comparing treatments that are either “off” or “on” (binary treatment), we follow a continuous treatment difference-

¹² The year-month fixed effects are included to control for the fact that firms have been interviewed at different points in time and hence different periods for reported changes in sales.

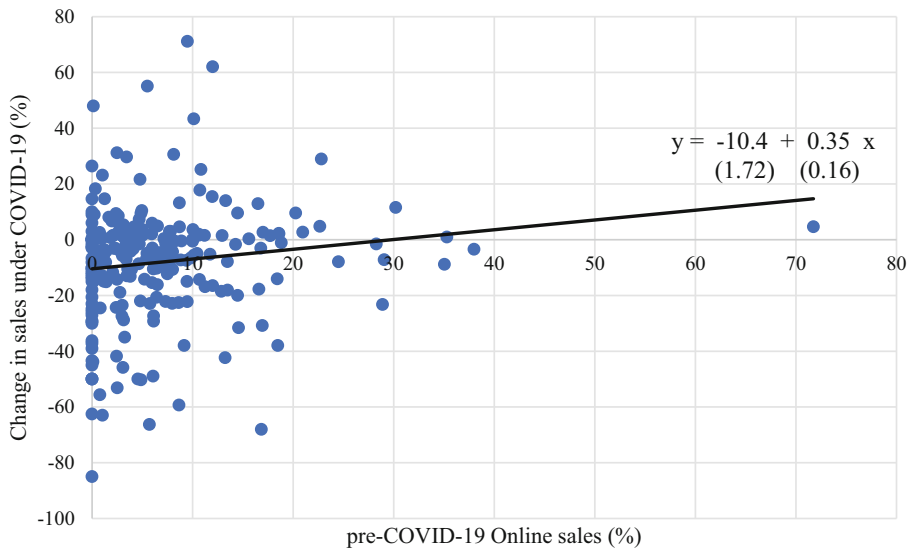


Fig. 4 Change in sales under COVID-19 versus pre-COVID-19 online sales (sector-level)

in-differences model and compare outcomes across firms that have experienced different levels of exposure to the pandemic shock (Callaway et al. 2021).¹³

Recent advances in difference-in-difference methods have shown that the coefficient β in Eq. 1 can be interpreted as the causal treatment effect if there are two time periods and the usual common trend assumption on untreated potential outcomes holds. However, this is no longer the case in the case of treatment heterogeneity with more time periods and/or a continuous treatment because the comparisons between already-treated units or between higher- and lower-dose units may not yield a proper causal estimate. While additional parallel assumptions have been introduced in the literature to allow for a causal interpretation of the estimated coefficient β , these are much stronger than the standard common trend assumption between (binary) treated and untreated units (Roth et al. 2023; Callaway et al. 2021).

We therefore follow the recommendation by Roth et al. (2023) to explicitly specify the comparisons to be made between treatment and control groups. Specifically, we estimate the following equation for each k^{th} bracket (percentage range) of positive pre-pandemic online sales as treatment group, defined as $Online_{k,i,pre-COVID-19} = 1$ if $25(k-1) < Online\ sales_{i,pre-COVID-19} \leq 25k$ and zero otherwise ($k =$

¹³ Instead of a discrete policy intervention that affects some firms but not others, we exploit the COVID-19 pandemic as an unexpected and exogenous shock that affected all firms but differentially reflecting their differences in *exposure* to the common shock. The exposure level forms a *continuous treatment variable* captured by the pre-pandemic intensity of online sales or “dose” of digitalization that conditions how the COVID-19 shock affects firm outcomes. The DiD dimension arises from comparing each firm’s performance *before and after the pandemic* across firms that experienced *different treatment intensity*, or “doses” of online sales shares. This continuous-treatment DiD framework follows previous empirical work to understand how the *intensity* of exposure to an exogenous shock or policy intervention affects outcomes, for example: Callaway et al. (2021); Topalova (2010); Wilson (2022).

1,2,3,4), and all observations with $Online\ sales_{i,pre-COVID-19} = 0$ as control group to estimate the impact of different treatment intensities:

$$\Delta Sales_{i,COVID-19} = \alpha + \beta_k Online_{k,i,pre-COVID-19} + \theta Z_i + D_c + D_s + D_t + \varepsilon_i \quad (2)$$

The specified model now follows a canonical two-period difference-in-differences strategy for binary treatments, where we group the continuous treatment intensity in four binary treatment groups. We only include one bracket (k) at a time to compare each treatment bracket to the control group of firms with no pre-pandemic online sales and to avoid mixing the level and slope effects. By comparing each treatment bracket only with the no treatment group, the level and slope effects are the same and the estimated effect has a clear causal interpretation (Callaway et al. 2021). Ideally a higher number of treatment groups is preferred to avoid any remaining inappropriate comparisons between higher- and lower-dose units within the different treatment brackets, but this is not feasible because of the relatively small sample sizes for, especially, the higher brackets.¹⁴ Still, by estimating Eq. 2 for each treatment bracket separately, the number of inappropriate comparisons is vastly reduced and treatment heterogeneity should be of much less concern when focusing on treatment firms falling within one bracket rather than across all brackets. For the estimated parameter β_k to have a causal interpretation we need to assume that the common trend assumption now holds for $k = 1, 2, 3, 4$. In order to make the estimated β_k for the dummy variable $Online_k$ in columns (2)–(5) comparable with the estimated β for the percentage variable $Online\ sales$ in column (1), we normalize (divide) β_k with the mean value of $Online\ sales$ among firms with $Online_k = 1$.

Apart from the common trend assumption, we need to assume that our measures of treatment, i.e. $Online\ sales_{i,pre-COVID-19}$ and $Online_{k,i,pre-COVID-19}$, $k = 1, 2, 3, 4$, in Eqs. 1 and 2 respectively, are exogenous. Threats to internal validity (endogeneity issues) may arise here from reverse causality and omitted variable bias. Reverse causality in this instance is not a concern, however, due to the unexpected nature of COVID-19: firms could not predict COVID-19 was going to happen and thus any investments in digitalization pre-pandemic happened outside expectations of the pandemic occurring. Therefore, the main threat to validity remains potential confounding factors that impact both the dependent and independent variable. For the regression to be valid, there should be no omitted variables that are correlated with both the treatment intensity ($Online\ sales$ and $Online_k$, $k = 1, 2, 3, 4$) and the dependent variable ($\Delta Sales$). For example, it could be that younger firms have a higher portion of online sales than older firms, or more innovative firms have invested more in digitalization, and both these types of firms are already on a higher growth trajectory. Alternatively, it could be that certain managers are more forward-looking, better anticipating digital to become more important in the future and investing more in online sales but also being better at crisis management. In all of these cases, there are possible confounding factors influencing both the share of online sales in 2018/19 and overall change in sales in 2020/21. In attempting to deal with endogeneity, four methods have commonly been used in the related firm-level

¹⁴ The number of firms falling into the brackets one to four is 651, 133, 57, and 82 respectively.

literature, namely lagged explanatory variables, instrumental variables, the ‘leave-one-out’-mean (i.e., ‘cell average’) approach, and inclusion of additional controls.

Lags of explanatory variables are commonly used as identification strategies to alleviate endogeneity concerns. The main idea behind lag identification is that albeit a causal relationship exists between a lagged explanatory variable (X_{t-1}) and the dependent variable (Y_t) (through the causal path of X_{t-1} with X_t), no direct causal link exists between the error term (U_t) and the lagged value (X_{t-1}), thereby eliminating any potential confounding factors running from the error term (U_t) to the lagged value (X_{t-1}). However, it can be argued that lagged explanatory variables almost never entirely solve issues of endogeneity, but rather only move the channel through which the endogeneity occurs (Bellemare et al. 2017); if X_{t-1} influences X_t , then most likely U_{t-1} also influences U_t , this way U_{t-1} also influencing X_{t-1} . In other words, while the error term does not directly impact the lagged variable, lagged variables of the error term may impact it, and thereby, endogeneity issues are not fully eliminated. Lag identification only works in case there are no temporal dynamics among the unobservables (ibidem). Hence, although we use lagged online sales as our measure of treatment intensity, this is unlikely to be sufficient given the possibility that confounding factors impacting the share of online sales during COVID-19 also correlate with the share of pre-pandemic online sales.

An alternative, or complementary, approach would be the use of an instrumental variable that is correlated with the main explanatory variable (*Online sales*) but uncorrelated with the error term, i.e., satisfying the instrument relevance and exogeneity conditions respectively. The question is, therefore, what exogenous variable impacts the share of online sales directly but not the change in sales (directly)? One possible candidate is access to internet. Without sufficient access to internet or established networks, it is difficult for a firm to set up and operate web-based platforms, social media platforms, a website, or smartphone apps. Access to internet therefore directly impacts online sales, making it a relevant instrument. Access to internet has indeed been used in earlier literature investigating the role of ICT on firm performance. For example, Timmis (2013) studies the impact of internet on firms’ direct and indirect exports by using density of IP-addresses within a country as instrument for internet. This approach is not appropriate for this paper’s research for two reasons, however. First, the ten countries in this study have all high levels of household internet access, ranging from 90%–98% in 2019, with Austria and Belgium having the lowest at 90% (Eurostat 2022). Consequently, access to internet is not a deciding factor for firms whether to adopt online sales channels creating a weak instrument problem. Second, one can question the exogeneity of the instrument in our study context: access to internet may also impact firm sales through other channels than online sales. For example, access to internet affects the company’s ability to attract more or better talent by allowing hiring of people through online job adverts, or alternatively allowing them to reach a wider network of suppliers online and find better inputs.

In this study we therefore include a rich set of controls together with lagged online sales to minimize the impact of any remaining possible confounders.¹⁵ Specifically, we control for country-location, sector, period of sales, firm size, firm age, labor productivity, foreign ownership, access to finance, innovation, export orientation, use of foreign inputs, GVC involvement, share of female workers, whether company has a website, use technology licensed from a foreign-owned company¹⁶, workers' level of technical and IT skills, managerial skills, share of temporary and seasonal workers, presence of electricity issues, and presence of transport issues. The online appendix provides a justification for the selection of control variables and their exact definitions.

All control variables are measured pre-COVID-19 in 2018 or 2019 or are (time-invariant) fixed effects, except those for workers' level of skill, presence of website and whether the firm encountered electricity or transport obstacles which are measured at the time of the interview, i.e., after the onset of COVID-19 in 2020 or 2021. While including these latter variables may control for possible confounding factors, they might also introduce new endogeneity concerns. Therefore, we perform a test for whether the results are robust to the inclusion of these variables as well. Reassuringly, the key results are robust to the inclusion to both types of control variables. Also, a common trend test and a placebo (permutation) test are performed to support a causal interpretation of the results.

3 Results

Table 1 shows the baseline regression results for the effect of pre-pandemic online sales on pandemic sales due to differential exposure (treatment intensity) to the pandemic. The dependent variable is $\Delta Sales$, indicating the firm's percentage change in the last reported monthly sales in 2020 or 2021 compared to the same month in 2019 (pre-COVID-19). Column (1) gives the estimates for Eqs. 1 and where the key independent variable *Online sales* is given by the share of total sales through

¹⁵ We have also considered the 'leave-one-out' mean or 'cell-average'-approach, taking the mean of the endogenous variable across all firms in the same 'cell' while leaving out the firm in question. The 'cell' can for instance be defined as a country, sector, or country-sector group. The leave-out-mean can then be used as an instrumental variable or simply substituted for the endogenous variable. This approach works by making the studied firm-level characteristic a function of a (presumably) exogenous 'cell' environment. Borowiecki et al. (2021) use sector-wide digital adoption as an instrument for firm-level digital adoption to study the firm-level impact of digital technologies and digital skill use on productivity growth. Amin and Viganola (2021), on the other hand, replace a firm-level dummy variable for whether a firm is financially unconstrained by its country-sector average to study the impact of access-to-finance on the probability of firm sales growth during COVID-19. There are two major drawbacks using the cell-average approach in our study. First, the approach works well in so far as the studied characteristic of a firm is purely environmental or at least strongly affected by the firm's environment. If not, then the cell-average is likely to be a noisy proxy of the endogenous variable leading to a weak instrument problem or attenuation bias. Second, online sales could well be correlated with other unobserved locational factors not captured by the country-sector cell-average (or non-locational factors correlated with country-sector cell-averages), such as good transport networks or access to internet in that specific location.

¹⁶ Having a website and using technology licensed from a foreign-owned company are used by Abidi et al. (2023) to construct a measure for 'digital connectivity'.

Table 1 Base regression results

VARIABLES	(1) Δ Sales	(2) Δ Sales	(3) Δ Sales	(4) Δ Sales	(5) Δ Sales
<i>Panel A: Only fixed effects</i>					
Online sales	0.111** (0.0487)				
Online ₁		0.240 (0.174)			
Online ₂			0.128 (0.0900)		
Online ₃				0.410** (0.184)	
Online ₄					0.0534 (0.0486)
Observations	2,492	2,278	1,832	1,766	1,788
R-squared	0.104	0.105	0.116	0.113	0.114
<i>Panel B: Firm-level controls and fixed effects</i>					
Online sales	0.102** (0.0479)				
Online ₁		0.225 (0.173)			
Online ₂			0.108 (0.0895)		
Online ₃				0.403** (0.184)	
Online ₄					0.0480 (0.0490)
Small	-8.473 (5.845)	-8.703 (6.478)	-9.136 (7.719)	-7.138 (8.364)	-10.06 (8.283)
Medium	-7.839 (5.995)	-8.758 (6.568)	-9.554 (7.824)	-6.129 (8.594)	-9.958 (8.420)
Large	-3.713 (6.014)	-3.038 (6.661)	-3.556 (7.934)	-1.455 (8.636)	-5.553 (8.521)
Firm age	-2.747*** (1.037)	-2.058* (1.093)	-2.228* (1.317)	-2.986** (1.414)	-2.634* (1.364)
Productivity	-0.839 (1.074)	-0.327 (1.006)	-0.652 (1.092)	-1.219 (1.385)	-0.621 (1.115)
Foreign ownership	0.0164 (0.0208)	0.0263 (0.0215)	0.0172 (0.0243)	0.0221 (0.0252)	0.0291 (0.0246)
Access to finance	10.67 (17.01)	7.884 (17.45)	18.33 (19.17)	14.19 (19.07)	10.87 (18.86)
Innovation	0.131 (1.475)	0.358 (1.508)	1.446 (1.757)	0.960 (1.820)	0.980 (1.791)
Exports	-0.0436 (0.0341)	-0.0634** (0.0310)	-0.0696** (0.0340)	-0.0455 (0.0418)	-0.0656* (0.0346)

Table 1 (Continued)

VARIABLES	(1) Δ Sales	(2) Δ Sales	(3) Δ Sales	(4) Δ Sales	(5) Δ Sales
Foreign inputs	-0.00513 (0.0250)	-0.0192 (0.0261)	-0.00279 (0.0293)	-0.0122 (0.0301)	-0.0114 (0.0293)
GVC	-1.714 (1.267)	-1.842 (1.306)	-2.142 (1.487)	-2.044 (1.532)	-1.826 (1.509)
Female	-0.0621 (0.0584)	-0.0288 (0.0572)	-0.0704 (0.0722)	-0.0664 (0.0772)	-0.0434 (0.0704)
Temporary	-0.0248 (0.0551)	-0.0253 (0.0565)	-0.00624 (0.0669)	-0.00582 (0.0716)	-0.0187 (0.0689)
Observations	2,492	2,278	1,832	1,766	1,788
R-squared	0.117	0.120	0.133	0.128	0.130

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include Country-Location, Sector, and Year-Month fixed effects.

online channels in 2018/19. Columns (2)–(5) give the estimates for Eq. 2 with $Online_k$ indicating a dummy equal to one if *Online sales* fall in the k th bracket of the positive values, zero if *Online sales* equals zero. All regressions include location, sector, year-month fixed effects, where Panel A shows the results without firm-level controls and Panel B with firm-level controls. The coefficients are estimated with OLS and standard errors are robust for heteroskedasticity.

Column (1) shows a positive and statistically significant relationship between the online sales and change of sales during pandemic, and the estimated impact is virtually identical whether we control for firm-level characteristics or not (c.q. Panels A and B). The size of the estimated coefficients suggests that firms fully specialized in online sales (i.e., *Online sales* equal to 100) experienced about 10%-point higher sales growth during the pandemic.¹⁷ Columns (2)–(5) shows the estimated impacts across the different brackets of positive online sales with all firms without any online sales forming the control group. All of the bracket estimates are also positive and largest for firms with between 50 and 75% of pre-pandemic online sales. Intuitively, this may suggest that firms that were already highly digitalized, but not fully, had the most to gain. They likely had sufficient experience with e-commerce and the necessary channels in place to expand quickly, while still retaining the scope to further scale their online activities. By contrast, firms that were already (almost) fully digitalized before the pandemic may have been more dependent on external factors, such as attracting new customers, for growth. However, the coefficients for Eq. 2 are not statistically different from each other at conventional levels of significance (p -value 0.28) and also not different from the estimate for Eq. 1 (p -value 0.36).¹⁸ Hence, we conclude that treatment heterogeneity as recently discussed does not appear to

¹⁷ On average, sales decreased by 8.7%, as reported in section 2.2.

¹⁸ Note that because the coefficients are normalized, the estimates are comparable between column (1) and columns (2)–(5).

be an important issue here and therefore we will focus on Eq. 1 in the remainder of the analysis.¹⁹

Firm-level controls are included to improve the precision of the main estimates. Among these, only firm age is statistically significant at customary significance levels.²⁰ The rest of the controls are not significant, but the direction of the coefficients is broadly consistent with earlier literature (e.g., Guimbert and Oostendorp 2016; Amin and Viganola 2021; Burger et al. 2017; Brenton et al. 2022).

Table 2 tests whether the results are robust to the inclusion of additional controls. As discussed, these controls are measured at the time of the interview and hence possibly subject to endogeneity bias. At the same time, these controls may capture omitted confounding factors and if their inclusion affects the estimated coefficient for online sales then this may complicate a causal interpretation. The results should alleviate this concern—the coefficients for online sales in both equations remain virtually unaffected, with the effect size even increasing slightly. Interestingly, website and foreign technology are insignificant, and website is even negative. This contrasts Abidi et al.'s (2023) findings who use website and foreign technology to construct their 'digital index'. By including a more direct measure of digitalization, online sales, we no longer find a direct effect for having a website and/or using foreign technology.²¹ IT-skills are both positively and significantly related with change of sales, in line with earlier literature on firm-specific skills (Guimbert and Oostendorp 2016; Amin and Viganola 2021).

One would expect a complementarity between IT-skills and online sales, as better IT-skills allow for better set-up, operation and management of online channels. To further explore the relationship between IT-skills, online sales and change in firm sales, the regression is estimated for subsamples based on the level of workers' IT-skills in Table A5 in the Appendix, where the IT-skill-level is either 'below required' (column (1)), 'as required' (column (2)) or 'above required' (column (3)). The estimated coefficient for online sales increases in the level of workers' IT skills. While firms with more online sales do barely better during COVID-19 when employing workers with inadequate skills, firms employing workers with required and above required IT skills appear to be strongly benefitting from online sales. While these effects are admittedly imprecisely estimated in columns (1) and (3) due

¹⁹ Table A4 presents a variation of Table 1, using year fixed effects instead of year-month fixed effects. This adjustment allows us to take advantage of the full sample of firms, notably including those from France. The French survey did not report the "last completed month", which we use to specify the last month of activity during the pandemic year; we therefore only control for the pandemic year in Table A4. Expanding the sample has little effect on the estimates, but more of the coefficients for online sales become significant presumably because of the larger sample. However, the coefficients for Eq. 2 remain not statistically different from each other (p -value 0.18) and also not different from the estimate for Eq. 1 (p -value 0.23).

²⁰ While earlier literature finds that older firms often perform better in various crises (Correa and Iooty 2010), during the COVID-19 crisis many top-performing firms turned out to be young (see Figure A2).

²¹ Alternatively, the differences could also arise due to the varying regions studied; as previously argued, our group of European countries studied are well developed in terms of access to internet, whereas the firms in the Middle Eastern and Central Asian group have a lower share of website and foreign technology (Abidi et al. 2023).

Table 2 Regression results with additional controls

VARIABLES	(1) Δ Sales	(2) Δ Sales
Online sales	0.120** (0.0505)	0.112** (0.0492)
Small		−10.50 (6.684)
Medium		−9.554 (6.791)
Large		−8.107 (6.761)
Firm age		−2.577** (1.085)
Productivity		−0.609 (1.146)
Foreign own		0.0216 (0.0227)
Access to Finance		9.150 (17.30)
Innovation		0.543 (1.523)
Exports		−0.0432 (0.0361)
Foreign inputs		−0.0110 (0.0271)
GVC		−1.184 (1.351)
Female		−0.0990 (0.0603)
Temporary		−0.00715 (0.0599)
Website	−0.315 (3.883)	−0.314 (3.995)
Foreign technology	0.541 (1.788)	0.572 (1.793)
Skill	3.912* (2.220)	3.595 (2.253)
IT-skills	3.553** (1.678)	3.407** (1.667)
Manager-skills	−1.496 (1.028)	−1.265 (1.032)
Electricity	2.696 (1.650)	2.151 (1.672)

Table 2 (Continued)

VARIABLES	(1) Δ Sales	(2) Δ Sales
Transport	0.411 (1.592)	0.531 (1.595)
Constant	-5.732 (10.32)	10.63 (26.32)
Observations	2,260	2,260
R-squared	0.108	0.121

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include Country-Location, Sector, and Year-Month fixed effects.

to small subsample sizes, these findings suggest that complementarities between IT-skills and online sales exist.

4 Validity Tests

4.1 Sample Selectivity

When it comes to the validity of the results, one may argue that the results so far may be affected by sample selectivity, given that only firms that report sales during COVID-19 compared to the pre-COVID-19 period and online sales in the pre-COVID-19 period are included, reducing the sample significantly (from 8403 to 3153 observations—see section 2). It is clear that the selection of the firms is predominantly affected by the time of interview, as firms are asked about the “last fiscal year” (for online sales) and “last completed month” (for change in sales). If the timing of the interview is random and if, conditional on the timing of interview, the last fiscal year and last completed month are random as well, then the selected sample of 3153 firms will be a random sample of the full sample of 8403 firms. The timing of the interview is unlikely to be random, however, for instance because different firm strata are interviewed sequentially (e.g., enumerators move in teams from location to location). Also, information on the last fiscal year and/or last completed month is likely to be forthcoming with different time lags across different firms, and hence even if firms are interviewed at the same point in time, they may be reporting for different “last” fiscal years and/or “last completed” month.

Therefore, the base regressions for Eq. 1 in Table 1 (column (1)) are re-estimated with also a Heckman sample selectivity correction term. In order to avoid identification on the basis of functional form only, the selection equation should include variables (‘instruments’) which affect selection but which are not having a direct impact on the change in sales (‘exclusion restriction’). For reasons discussed above, using the timing of interview as an instrument likely leads to a violation of the exclusion restriction but one can instead use the firm size, sector and location as reported in the *sampling frame*. The implementation of the ES relies on this pre-survey information to select and plan for the actual surveying process. Because this information is often outdated and/or incorrect, it is possible to use the firm size,

sector and location as reported in the sampling frame in the Heckman selectivity equation (1st stage), *as well as* the actual firm size, sector and location as reported by the firm in the OLS regression including the Heckman sample selectivity correction term (Mill's ratio) (2nd stage). Because the actual firm size, sector and location rather than those (mis)reported in the sampling frame affect the change in sales, the exclusion restriction is plausibly satisfied. It is also found that selection is strongly related with firm size, sector and location as reported in the sampling frame (instrument relevance). Table A6 in the Appendix reports the results and it is clear that controlling for sample selectivity has little effect on any of the estimated coefficients. If anything, the estimated impact of online sales becomes more statistically significant, with the effect size remaining unchanged. These findings suggest that sample selectivity is not driving the results.

4.2 Impact Heterogeneity

Another concern is that the results mask impact heterogeneity across firms. Literature has shown that certain firm characteristics such as access to finance, firm size, being a part of GVC are a driving force of firm sales (Amin and Viganola 2021; Meliciani and Tchorek 2017). Another factor is whether a firm is publicly listed: studies focusing only on public companies (cf. Bai et al. 2021; Copestake et al. 2022) may obtain different results, as these firms are typically larger, more resilient, have better access to finance, and tend to use more capital- and skill-intensive technologies. Likewise, whether the firm is a part of a multi-establishment firm or a business group can enhance resilience (Meliciani and Tchorek 2017), as larger businesses often have more funds to undertake different types of investments. State-ownership and firm's political connections can also help soften budget constraints, which in some countries have proven particularly beneficial during the pandemic with firms under state ownership demonstrating positive growth (Liu et al. 2021a). Regarding gender, the gender composition at the top can also play a role; Liu et al. (2021b) provide evidence for this during the pandemic, documenting that women-led businesses were more likely to close.

In the specification so far, we have assumed that exposure to the pandemic is fully determined by the level of pre-pandemic online sales, irrespective of other firm characteristics. We therefore re-estimate the base regressions for Eq. 1 in Table 1 (column (1), panel (B)) but with interaction terms for the variable *Online sales* with firm-level measures for access to finance, firm size (small, medium, large), GVC participation, public listing status, membership of business group, forming part of a multi-establishment firm, having a political connection, and female ownership. The results are listed in Table A7. Only the interaction term for large firms is statistically significant and negative, which could reflect that larger firms face greater inertia when adapting or scaling online sales. The results suggest that impact heterogeneity does not seem to be an issue (apart from large firms), confirming the finding that exposure to the COVID-19 pandemic is mainly through prior levels of online sales.²²

²² The absence of impact heterogeneity by (direct and indirect) access to finance may reflect the European Union's swift and large-scale pandemic response. In spring 2020, the EU, European Central Bank (ECB),

Table 3 Common trend test—with fixed effects and controls

VARIABLES	(1) ΔSales_0	(2) ΔSales_0	(3) ΔSales_0
Online sales	0.0525 (0.0464)	0.00874 (0.0472)	0.00743 (0.0473)
Small		4.634 (3.642)	4.725 (3.689)
Medium		10.78** (4.296)	11.19** (4.451)
Large		10.72* (5.733)	11.69** (5.700)
Firm age		-13.01*** (2.914)	-13.22*** (3.044)
Productivity		3.340** (1.662)	3.324** (1.669)
Foreign ownership		0.0140 (0.0625)	0.0232 (0.0642)
Access to finance		24.29 (26.62)	22.12 (27.80)
Innovation		7.162*** (2.289)	7.583*** (2.453)
Exports		-0.0315 (0.0447)	-0.0287 (0.0448)
Foreign inputs		-0.0679 (0.0652)	-0.0618 (0.0645)
GVC		4.890 (3.332)	4.920 (3.344)
Female		-0.00958 (0.0598)	-0.00348 (0.0623)
Temporary		0.196* (0.101)	0.200** (0.101)
Website			4.291 (2.697)
Foreign technology			-2.912 (2.454)
Skill			0.112 (3.786)
IT-skills			-1.748 (2.440)
Manager-skills			1.303 (1.619)
Electricity			4.464* (2.615)

Table 3 (Continued)

VARIABLES	(1) $\Delta Sales_0$	(2) $\Delta Sales_0$	(3) $\Delta Sales_0$
Transport			-2.998 (2.473)
Constant	16.87 (13.43)	-13.48 (40.84)	-7.182 (40.93)
Observations	2,181	2,181	2,181
R-squared	0.033	0.088	0.091

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include Country-Location, Sector, and Year-Month fixed effects.

4.3 Common Trend Test

The key identification assumption in a difference-in-differences strategy is the common trend assumption, i.e., that if COVID-19 would not have occurred, firm sales would have evolved in a similar way across firms with different shares of pre-COVID-19 online sales. To test for the presence of such common trend, online sales is regressed on the pre-pandemic change of sales. The change of sales before COVID-19 is constructed from two variables: total sales in the last fiscal year (2018/19) and the total sales two years before the last fiscal year (2016/17).²³ The pre-pandemic change of sales (in percentages) is represented by the variable $\Delta Sales_0$.

The regression results are presented in Table 3, respectively when only fixed effects are included (column (1)), pre-COVID-19 firm controls are added (column (2)), and COVID-19-era firm controls also included (column (3)). The dependent variable is the change of sales before the pandemic ($\Delta Sales_0$). Across all three specifications, the coefficient on online sales is statistically not different from zero and becoming close to zero when controls are added. These findings support the suitability of the difference-in-differences strategy.

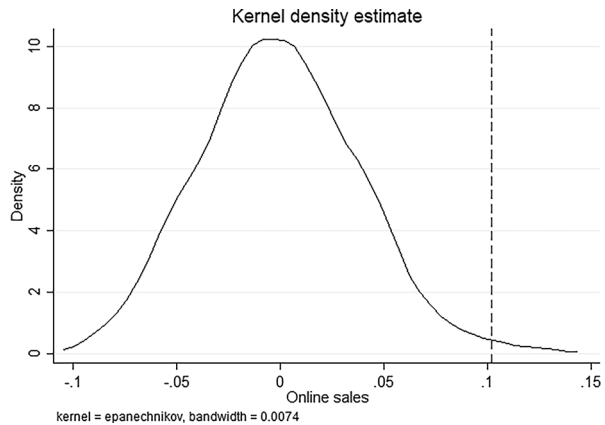
4.4 Permutation Test

To provide further validity for the causal interpretation of the model, a placebo test in the form of a permutation test is also conducted. The placebo test is an additional way to test for the assumption that pre-pandemic levels of online sales are indicative of positive change of sales during the pandemic, and that the results are not due to randomness. It is a non-parametric resampling method, often used in studies of clinical or economic policy to test the treatment effects of an independent variable (see e.g., Du et al. 2020). In a classic difference-in-differences study, it allows for testing that the effects on the treated group are actually due to the treatment and not

and European Investment Bank (EIB) introduced measures including the €750 billion Pandemic Emergency Purchase Programme (PEPP), the SURE loan scheme to support short-time work, and a €25 billion guarantee fund targeting small businesses, followed by additional funds in 2021 (Consilium.europa.eu 2022).

²³ Hence firms established after 2016/17 are not included here.

Fig. 5 Permutation distribution of estimated coefficient for Online sales



due to randomness, by assuming the null hypothesis (i.e., randomness of results) and reshuffling the actual outcomes of the treated and untreated among the studied observations. This reshuffling process, or permutation, is repeated multiple times, and for each permutation, a test-statistic is calculated. Once all the permutations have been run, the resulting information reports all the possible test-statistics under the null hypothesis. If a significant proportion of the reshuffled test-statistic of interest is greater or equal than the absolute value of the original baseline's test-statistic, then the null hypothesis cannot be rejected (MacKinnon 2007).

Here, the permutation test is used to test for the null hypothesis that there is no difference in the change of sales during the pandemic between the firms with a positive share of online sales in 2018/19 and firms with no online sales in 2018/19, i.e., change in sales is independent of share of online sales. The permutation process is as follows: each of the firms is randomly assigned a different share of online sales to estimate the “placebo” treatment effect on the change of sales from 2019 to 2020/21. For every random assignment, the estimated coefficient for online sales is saved to construct the test-statistic, here p -value, for the approximated permutation distribution. Since considering all the possible permutations is not possible given the number of observations (all possible permutations = $N!$ i.e., 2492! for the regression (1) in Table 1 Panel B), a sample of 2000 of all possible permutations is taken instead.²⁴

Figure 5 displays the distribution of the placebo estimates. The dashed line represents the observed coefficient of online sales in the preferred specification column (2) in Table 1, and where it lies relative to the permuted coefficients. Given the relatively extreme location of the dashed lines in comparison to the permutation distributions, corresponding to a low permutation p -value, it can be concluded that the actually estimated impact is non-random (i.e., that pre-pandemic online sales are indicative of change of sales during the pandemic). This is further proof that a posi-

²⁴ Increasing the number of permutations increases the precision of the p -value and narrows the confidence interval, while increasing computing time. 1000, 2000 and 3000 simulations were tested for, all of which resulted in lower simulated p -values than the actual observed p -value.

tive causal relationship exists between pre-pandemic online sales and the subsequent change of sales during COVID-19.

5 Conclusion and Discussion

COVID-19 was an unforeseen natural event that led to an abrupt standstill in societies worldwide. As governments tried to grapple with how best to approach the situation, business activity was hampered by the policy measures taken to protect the health and wellbeing of the people and to constrain the spread of the virus. From supply chain disruptions to country-wide lockdowns preventing certain sectors from remaining fully active, companies scrambled for ideas on how to survive whether through new innovative ways to cater to their customers and/or through cutting costs. As customers were restricted in accessing products and services through traditional, physical means, pivoting to e-commerce and finding alternative non-physical means to serve customers seemed better than ever. However, given that setting up and increasing online activity is a time and resource-intensive task, those firms with established pre-pandemic online channels seemed to be less exposed, and thereby advantaged. How much of an advantage, if any, did these firms have in terms of sales performance?

To answer this question, this paper exploits the unexpected and sharp disruption caused by COVID-19 through a careful application of the difference-in-differences research design. Unlike previous studies, we verify whether the results are robust to allowing for treatment heterogeneity, test for the presence of common trends, and perform a placebo (permutation) test. Using data from The World Bank's Enterprise Surveys, the cross-country analysis of ten different Western European countries reveals that pre-existing shares of online sales are indeed a driver of firm sales during COVID-19. Firms that were fully digital with 100% of sales online pre-pandemic, generated 10%-point higher firm-level sales growth during the pandemic, when average firm sales contracted by 8.7%. Overall, the results are remarkably robust when controlling for a wide range of firm characteristics, treatment heterogeneity and sample selectivity. A causal interpretation of the relationship between digitalization and firm performance is further supported by performing a common trend test and a permutation test. Although it cannot be excluded that there remain confounding factors that have not been accounted for, these findings support the conclusion that digitalization, measured as share of pre-pandemic online sales, was the key mechanism by which firms reduced their exposure to COVID-19. While not unexpected, our study is one of the few studies to provide a rigorous estimate of the importance of digitalization for firm performance using COVID-19 as a natural experiment.

Our results show that online sales provided an important source of revenue for firms during the COVID-19 crisis, sustaining firm activity when physical business was restricted. For firms, e-commerce offered a new way to reach customers (at home and abroad) and generate growth, while for governments it provided an additional channel to support economic activity alongside financial aid. Investments in digital infrastructure, skills and training of employees remain important to ensure that firms

can take advantage of these opportunities, though attention should be also paid to the environmental footprint of expanding online activity.

At the same time, these findings should be interpreted with caution. The advantage of online sales in our study reflects the unique nature of the pandemic, when businesses and consumers had to rely on e-commerce due to lockdowns and mobility constraints, leaving offline businesses at a disadvantage. It remains unclear whether online sales provide the same benefits under normal conditions. As such, broad policy conclusions cannot be drawn on the basis of this study alone. Future research should therefore examine both the continued uptake of online activity and the role of other forms of digitalization (such as AI) during and outside of the pandemic to better understand where the largest and most persistent gains arise.

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