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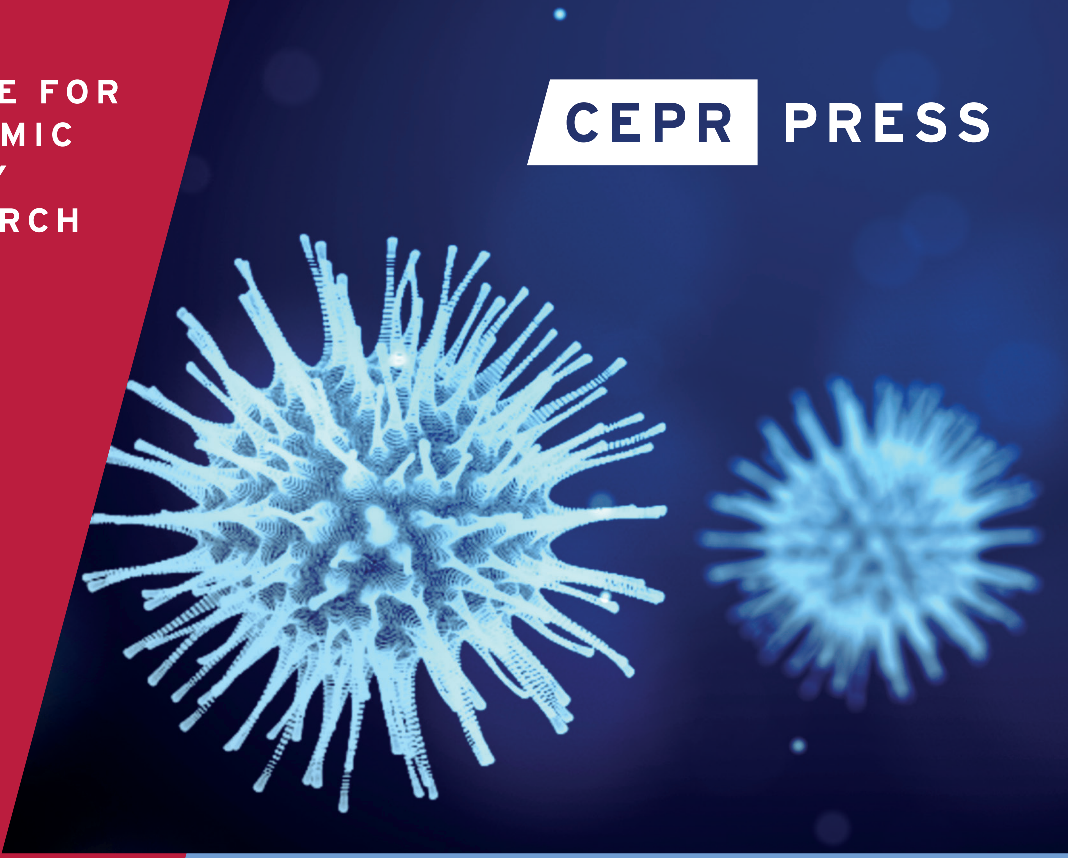

# COVID ECONOMICS

## VETTED AND REAL-TIME PAPERS

ISSUE 78
14 MAY 2021

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# Covid Economics
# Vetted and Real-Time Papers

*Covid Economics, Vetted and Real-Time Papers*, from CEPR, brings together formal investigations on the economic issues emanating from the Covid outbreak, based on explicit theory and/or empirical evidence, to improve the knowledge base.

**Founder**: Beatrice Weder di Mauro, President of CEPR
**Editor**: Charles Wyplosz, Graduate Institute Geneva and CEPR

**Contact**: Submissions should be made at https://portal.cepr.org/call-papers-covid-economics. Other queries should be sent to covidecon@cepr.org.



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*Covid Economics* will feature high quality analyses of economic aspects of the health crisis. However, the pandemic also raises a number of complex ethical issues. Economists tend to think about trade-offs, in this case lives vs. costs, patient selection at a time of scarcity, and more. In the spirit of academic freedom, neither the Editors of *Covid Economics* nor CEPR take a stand on these issues and therefore do not bear any responsibility for views expressed in the articles.

# Submission to professional journals

The following journals have indicated that they will accept submissions of papers featured in *Covid Economics* because they are working papers. Most expect revised versions. This list will be updated regularly.

*American Economic Journal, Applied Economics*
*American Economic Journal, Economic Policy*
*American Economic Journal, Macroeconomics*
*American Economic Journal, Microeconomics*
*American Economic Review*
*American Economic Review, Insights*
*American Journal of Health Economics*
*Canadian Journal of Economics*
*Econometrica**
*Economic Journal*
*Economics Letters*
*Economics of Disasters and Climate Change*
*International Economic Review*
*Journal of Development Economics*
*Journal of Econometrics**
*Journal of Economic Growth*
*Journal of Economic Theory*
*Journal of the European Economic Association**
*Journal of Finance*
*Journal of Financial Economics*
*Journal of Health Economics*
*Journal of International Economics*
*Journal of Labor Economics**
*Journal of Monetary Economics*
*Journal of Public Economics*
*Journal of Public Finance and Public Choice*
*Journal of Political Economy*
*Journal of Population Economics*
*Quarterly Journal of Economics*
*Review of Corporate Finance Studies**
*Review of Economics and Statistics*
*Review of Economic Studies**
*Review of Financial Studies*

(*) Must be a significantly revised and extended version of the paper featured in *Covid Economics*.

# Covid Economics
Vetted and Real-Time Papers

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Covid Economics Issue 78, 14 May 2021

# Why did firms draw down their credit lines during the COVID-19 shutdown?[1]

Joshua Bosshardt[2] and Ali Kakhbod[3]

Date submitted: 27 April 2021; Date accepted: 28 April 2021

*The economic shutdown associated with the COVID-19 pandemic witnessed a surge in drawdowns on pre-existing credit lines. This paper examines how this liquidity was used by firms. Drawdowns were associated with an immediate accumulation of liquid assets followed by a depletion of this liquidity as the U.S. economy stabilized after the spring of 2020. Drawdowns were generally not associated with greater levels of physical investment or employment either immediately after the drawdowns or several months later. Rather, the depletion of liquidity is simultaneous with an increase in the equity to assets ratio, consistent with repayments of the drawdowns. These facts are consistent with the idea that firms drew down their credit lines due to a precautionary motive to mitigate future liquidity risk until the economy started to stabilize. However, we find evidence that firms in industries that were less affected by the shutdown, such as professional services that can be performed remotely, were relatively more likely to use drawdowns to maintain investment rather than accumulate liquidity. On the intensive margin, this is especially true for firms in such industries that drew a relatively small amount of funds.*

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# 1 Introduction

Credit lines, or contracts that allow firms to draw funds from their bank, comprise a substantial fraction of bank lending to businesses (Shockley and Thakor (1997)). The literature has generally established that firms are more likely draw down their credit lines when liquidity is scarce, but there are conflicting perspectives about why. On the one hand, Holmstrom and Tirole (1998) argue that credit lines help firms to manage their liquidity so that they can maintain investment.[1] On the other hand, Ivashina and Scharfstein (2010) find that firms drew down their credit lines during the global financial crisis as a precaution against the possibility that their lenders could become unable to provide liquidity in the future.

This paper investigates this question within the context of the shutdown associated with the COVID-19 pandemic. During March of 2020, the U.S. introduced social distancing restrictions in response to the COVID-19 pandemic, resulting in the suspension of non-essential economic activities involving in-person interactions. Businesses faced sharply declining profits, especially in industries with less flexibility for working at home. In particular, Figure 1a shows that the median net income decreased by around 1% in 2020Q1, with an approximately 50% greater decline for firms in industries that were more disrupted by the shutdown.[2] During this time, firms drew a substantial amount of cash from pre-existing credit lines with their banks (Acharya and Steffen (2020), Li, Strahan and Zhang (2020)). In particular, Figure 1b shows that, during March 2020, approximately 8% of firms included in Compustat drew from a credit line and that the rate of drawdowns was greater in industries that were relatively more disrupted by the shutdown. To understand why firms drew down their credit lines during the COVID-19 shutdown, it is important to consider how they used the resulting liquidity.

This paper investigates two main hypotheses to explain the increase in credit line drawdowns during the COVID-19 shutdown:

H1: Firms accumulated liquidity as a precaution against future liquidity risk.

[1] Campello et al. (2012) and Berrospide and Meisenzahl (2015) find empirical evidence for this use during the global financial crisis.

[2] Papanikolaou and Schmidt (2020) find that such industries also experienced greater declines in employment, expected revenue growth, stock market performance, and creditworthiness.

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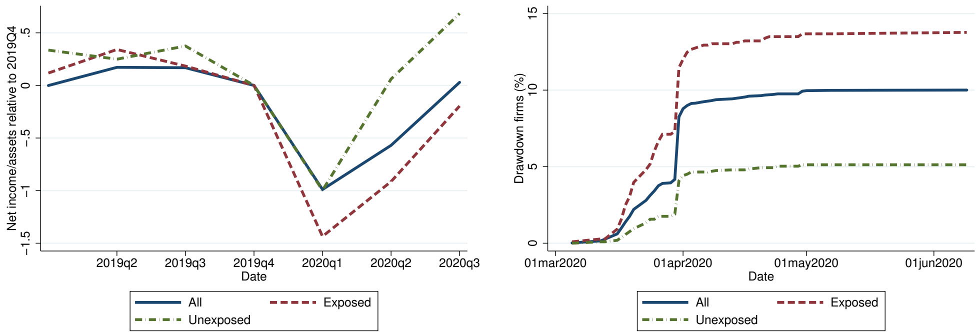
Figure 1: The top panel shows the median net income to assets ratio relative to the 2019Q4 level for three industry groups: all firms, firms in industries that were relatively exposed to the COVID-19 shutdown, and firms in industries that were relatively unexposed to the COVID-19 shutdown. The bottom panel shows the fraction of firms that have drawn down a credit line for the three industry groups. See Section 3 for a more detailed description of the industry groups.




(a) (b)

H2: Firms used the drawdowns to pay for current expenses, such as physical investment and employees.

We address this question using data on credit line drawdowns from S&P's Leveraged Commentary & Data and balance sheet data from Compustat. We compare firms that drew from a credit line in March 2020 relative to firms that did not. Although drawdowns are not randomly distributed across firms, we mitigate endogeneity by controlling for observable factors that could be jointly correlated with a firm's motivation to draw from a credit line and other adjustment strategies affecting the outcomes, such as a firm's liquidity stress and industry. Our results are robust to instrumenting credit line drawdowns with a firm's predetermined level of undrawn credit commitments. Although a firm's level of undrawn commitments could be chosen partly in anticipation of adverse shocks in general, it is arguably less likely to have been chosen in anticipation of a shock like the COVID-19 shutdown, which was unique in its abruptness and magnitude.

A unique feature of the COVID-19 shutdown was its heterogeneous effect across industries based on their ability to be performed remotely and whether they were designated as essential. Motivated by this observation, we further examine how the use of funds from credit line drawdowns varied with industry-level exposure to the shutdown. We also examine the intensive margin to see how the motivation behind credit line drawdowns was correlated with the size of the drawdowns.

We find that credit drawdowns at the start of the pandemic in March 2020 were strongly associated with an immediate increase in liquidity from 2019Q4 to 2020Q1, consistent with a precautionary motive. We do not find evidence that drawdowns were on average associated with greater levels of physical investment or employment. However, we find some evidence that, within industries that were less affected by the shutdown, firms that drew modest amounts of funds were relatively more likely maintain investment.

We also investigate how the liquidity from credit lines was used after the start of the pandemic from 2020Q1 to 2020Q3. During this period, especially from 2020Q2 to 2020Q3, macroeconomic indicators such as GDP and unemployment partially recovered following a series of stabilizing interventions in late March, including interest rate reductions, asset purchases, and the establishment of funding facilities by the Federal Reserve as well as stimulus payments, unemployment benefits, and small business lending support associated with the Coronavirus Aid, Relief, and Economic Security (CARES) Act passed by Congress. Liquidity strains eased as profitability also recovered (Figure 1a). During this time, we find that firms that initially drew down their credit lines at the start of the pandemic decreased their liquidity relative to other firms. There is little evidence that firms eventually used the liquidity to support physical investment or employment. Rather, these firms simultaneously exhibited a relative increase in the equity to total assets ratio, which is consistent with repayments of their credit lines. This further supports the view that firms initially drew down their credit lines due to a precautionary motive to accumulate liquidity at the start of the pandemic. As the economy stabilized later on in the year, this precautionary motive dissipated, and firms did not appear to have a compelling alternative use for the cash.

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## 2 Literature Review

This paper contributes to two strands of recent work examining the economic effects of the COVID-19 pandemic. First, it relates to papers showing a significant increase in firm debt issues during the COVID-19 shutdown. Acharya and Steffen (2020) find evidence that credit line drawdowns may have been driven by a precautionary motive by firms trying to avoid credit rating downgrades. Li, Strahan and Zhang (2020) and Federal Reserve (2020) remark that banks have managed to accommodate these drawdowns thanks to inflows of deposits as well recent regulations that have strengthened their balance sheets compared to the global financial crisis. Darmouni and Siani (2020) find evidence that bond issues during this time were more strongly associated with liquidity accumulation than real investment. This paper contributes to this literature by comparing the degree to which firms used their credit line drawdowns to accumulate liquidity as opposed to paying current expenses associated with real investment or employees. We compare these responses both immediately after the drawdowns and several months later. We also examine the degree to which the uses of credit line drawdowns were associated with exposure to the shutdown and the size of the drawdowns.[3]

Second, this work relates to papers illustrating the heterogeneous effects of the COVID-19 shutdown across industries. Papanikolaou and Schmidt (2020) finds that industries with fewer opportunities to work from home performed worse as measured by declines in employment, expected revenue growth, stock market performance, and expected likelihood of default. By contrast, Barrero, Bloom and Davis (2020) and Hassan et al. (2020) show that the shutdown provided expansion opportunities for some firms, such as those specialized in essential services.

Finally, this paper is also related to a more general literature on credit lines. Firms apply for credit lines to mitigate liquidity risk (e.g., Holmstrom and Tirole (1998), Acharya et al. (2014)). During the global financial crisis, firms used their credit lines to maintain investment (e.g., Campello, Graham and Harvey (2010), Campello et al. (2011), Campello et al. (2012), Berrospide and Meisenzahl (2015)). There is also evidence that firms drew

[3]Other important recent empirical works studying draw-downs include Greenwald, Krainer and Paul (2020), Chodorow-Reich and Falato (2020) and Fahlenbrach, Rageth and Stulz (2020). For theoretical works on banks' advantage in liquidity provision over capital markets see Kashyap, Rajan and Stein (2002), Gatev and Strahan (2006) and Acharya and Plantin (2020).

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down their credit lines during the crisis as a precautionary measure due to fears that their lenders would be unable to provide liquidity in the future (e.g., Ivashina and Scharfstein (2010), Montoriol-Garriga and Sekeris (2009), Huang (2010)).

## 3 Data

We obtain data on credit line drawdowns from Leveraged Commentary & Data, a subsidiary of S&P Global Market Intelligence. We construct a firm-level cross-section by computing the sum of drawdowns in March 2020 for each firm. We merge this with quarterly balance sheet data from Compustat, which includes the percentage of liquid assets (which consists of cash and short-term investments) to total assets, capital expenditure to total assets, book equity to total assets, and a measure of liquidity stress, which is defined as

$$stress = 100 * \frac{\text{lag short-term debt - lag liquid assets - net income}}{\text{lag total assets}} \tag{1}$$

In particular, liquidity stress is intended to measure a firm's short-term obligations relative to existing resources that can be used to meet those obligations, which includes the stock of liquid assets as of the last filing date as well as the flow of net income in the current period. We also examine the logarithm of the number of employees, which is only available at annual frequency. A 95% winsorization is applied variables to mitigate the effect of outliers. Following the literature, we omit firms from the agriculture, utilities, and finance sectors. Summary statistics are included in Table 1.

We investigate potential differences in the use of credit line drawdowns across industries that were differentially affected by the shutdown. To determine exposure, we consider the fact that the shutdown restricted non-essential economic activities involving in-person interactions. Specifically, we classify an industry as relatively exposed to the crisis if it is not deemed essential and a large fraction of jobs cannot be done at home, and we classify an industry as relatively unexposed to the crisis if it is essential or if a large fraction of jobs can be done at home.

We determine essential industries at the 4-digit NAICS code level based on the classification in Papanikolaou and Schmidt (2020), which is a modified version of the guid-

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Table 1: Summary statistics. Drawdown is a dummy indicating whether a firm drew funds from a credit line in March 2020, Drawdown size is the sum of drawdowns during March 2020 in millions of USD for firms that drew funds from a credit line, Exposed is a dummy indicating whether a firm was in an industry that was relatively exposed to the COVID-19 shutdown, Unexposed is a dummy indicating wheth a firm was in an industry that relatively unexposed to the shutdown (see Section 3 for a more detailed description of the industry groups), Log(assets) is the logarithm of total assets in millions of USD in 2019Q4, Stress is liquidity stress as defined in equation (1) in 2020Q1, Liquid assets/assets is the percentage of cash and short-term investments to total assets in 2020Q4, Capex/assets is the percentage of capital expenditure to total assets in 2020Q4, Log(employees) is the logarithm of the number of employees in thousands in 2019, and Equity/assets is the percentage of book equity to assets in 2019Q4.

| | N | Mean | SD | P25 | P75 |
|---|---|---|---|---|---|
| Drawdown | 5312 | 0.08 | 0.27 | 0.00 | 0.00 |
| Drawdown size ($ m) | 424 | 401.23 | 546.60 | 75.00 | 500.00 |
| Exposed | 5312 | 0.21 | 0.40 | 0.00 | 0.00 |
| Unexposed | 5312 | 0.40 | 0.49 | 0.00 | 1.00 |
| Log(assets) | 5240 | 5.63 | 3.00 | 3.64 | 7.89 |
| Stress (%) | 5097 | 1.20 | 69.84 | -23.73 | 3.67 |
| Liquid assets/assets (%) | 5239 | 22.95 | 27.45 | 3.01 | 33.34 |
| Capex/assets (%) | 5086 | 0.91 | 1.29 | 0.03 | 1.18 |
| Log(employees) | 1561 | 0.86 | 2.69 | -0.89 | 2.84 |
| Equity/assets (%) | 4508 | 43.37 | 49.79 | 29.23 | 67.88 |

ance provided by the Cybersecurity and Infrastructure Security Agency (CISA). Some essential industries include food and beverage production, utilities, transportation, and medical services.

We determine the degree to which work in an industry can be done at home at the 2-digit NAICS code level based on the classification in Dingel and Neiman (2020). Specifically, we classify an industry as having a low fraction of jobs that can be done at home if no more than 25% of jobs can be done at home, which includes accommodation and food services; agriculture, forestry, fishing, and hunting; retail trade; construction; transportation and warehousing; manufacturing; health care and social assistance; and mining, quarrying, and oil and gas extraction. We classify an industry as having a high fraction of jobs that can be done at home if at least 75% of jobs can be done at home, which includes education services; professional, scientific, and technical services; management of companies and enterprises; and finance and insurance.

## 4 Descriptive analysis

This section examines the correlates of credit line drawdowns at the start of the COVID-19 shutdown. Table 2 compares firms that drew funds from a credit line in March 2020 to firms that did not.[4] We first compare these firms upon the impact of the pandemic. This is based on the 2019Q4 values for most of the characteristics except for liquidity stress, which corresponds to the 2020Q1 value, and the number of employees, which is from the last reported date in 2019. Firms that drew down from a credit line were more likely than average to be in a relatively exposed industry and less likely to be in a relatively unexposed industry. They were also relatively large, illiquid, and levered, but they were not generally more liquidity stressed.

To compare the initial response to the pandemic, we consider the change in liquidity and investment from 2019Q4 to 2020Q1. Liquidity increased for both groups of firms, but it grew by greater than 10 times more for firms that drew from a credit line. The equity to total assets ratio decreased for both groups, possibly reflecting losses of retained

[4]Note that the number of firms with a drawdown is greater than the number of firms with data on drawdown volume in Table 1 because a small number of observations report that there was a drawdown but do not report the amount.

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earnings due to lower profitability. The decrease in the equity to total assets ratio is larger for firms that drew down their credit lines, which may reflect the mechanical effect of the increased debt on their balance sheets. Investment decreased for both groups of firms, but it decreased by more for the firms that drew from a credit line.

To compare the later response to the pandemic, we look at the change in liquidity and investment from 2020Q1 to 2020Q3. In contrast to the change from 2019Q4 to 2020Q1, liquidity declines for firms that drew from a credit line, whereas it increases for firms that did not draw from a credit line. One interpretation of this finding is that firms that initially drew their credit lines eventually used this liquidity to pay expenses or pay back the credit lines, whereas firms that did not draw down a credit line at the start of the crisis were more likely to eventually obtain liquidity through other means such as the bond market. The equity to total assets ratio for the firms that drew from a credit line increased relatively more compared to other firms, which would be consistent with a repayment of credit lines. Both groups of firms exhibit a continued decline in investment, which is relatively greater for firms that drew down their credit lines. Finally, firms that drew from a credit line also exhibit a significant relative loss in employment from 2019 to 2020 compared to other firms.

Table 3 shows a similar set of statistics for the subsample of firms in industries that were relatively exposed to the COVID-19 shutdown. Many of the patterns are generally similar to the full sample. Finally, Table 4 shows a similar set of statistics for the subsample of firms in industries that were relatively unexposed to the shutdown. Many of the patterns are generally similar to the full sample except that the negative correlation of drawdowns with the change in capital expenditure is not as pronounced.

## 5 Methodology

This section describes a panel model and two cross-sectional models to examine the immediate and medium-term effects of credit line drawdowns on liquidity, capital investment, employment, and equity.

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Table 2: This table compares firms that drew funds from a credit line to firms with no recorded drawdowns. The first row presents the number of observations in each group, and the remaining rows present the respective means as well as the t-statistic from a difference in means test.

| | Drawdown | No drawdown | T-statistic |
|---|---|---|---|
| N | 428 | 4,651 | |
| *Upon impact* | | | |
| Exposed (2019Q4) | 0.297 | 0.198 | 4.375 |
| Unexposed (2019Q4) | 0.194 | 0.420 | -11.17 |
| Log(assets) (2019Q4) | 7.895 | 5.431 | 31.52 |
| Stress (2020Q1) | −3.515 | 1.613 | -4.246 |
| Liquid assets/assets (2019Q4) | 8.560 | 24.25 | -26.991 |
| Capex/assets (2019Q4) | 0.949 | 0.907 | .853 |
| Log(employees) (2019) | 2.296 | 0.630 | 11.977 |
| Equity/assets (2019Q4) | 35.70 | 44.18 | -6.19 |
| *Initial response (2019Q4–2020Q1)* | | | |
| Δ Liquid assets/assets | 5.671 | 0.449 | 20.594 |
| Δ Capex/assets | −0.212 | −0.144 | -2.548 |
| Δ Equity/assets | −4.261 | −1.590 | -8.607 |
| *Later response (2020Q1–2020Q3)* | | | |
| Δ Liquid assets/assets | −0.876 | 2.754 | -10.307 |
| Δ Capex/assets | −0.165 | −0.106 | -1.932 |
| Δ Equity/assets | 1.689 | 0.798 | 2.494 |
| *Annual response (2019–2020)* | | | |
| Δ Log(employees) | −0.0535 | −0.0152 | -3.629 |

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Table 3: This table compares firms that drew funds from a credit line to firms with no recorded drawdowns for firms in industries that were relatively exposed to the COVID-19 shutdown. The first row presents the number of observations in each group, and the remaining rows present the respective means as well as the t-statistic from a difference in means test.

| | Drawdown | No drawdown | T-statistic |
|---|---|---|---|
| N | 125 | 937 | |
| *Upon impact* | | | |
| Exposed (2019Q4) | 1 | 1 | |
| Unexposed (2019Q4) | 0 | 0 | |
| Log(assets) (2019Q4) | 8.197 | 5.452 | 16.739 |
| Stress (2020Q1) | 0.0943 | 13.55 | -5.277 |
| Liquid assets/assets (2019Q4) | 7.798 | 13.86 | -6.767 |
| Capex/assets (2019Q4) | 1.167 | 1.486 | -3.267 |
| Log(employees) (2019) | 2.817 | 0.671 | 8.877 |
| Equity/assets (2019Q4) | 32.01 | 40.29 | -2.716 |
| *Initial response (2019Q4–2020Q1)* | | | |
| Δ Liquid assets/assets | 5.596 | 0.353 | 10.709 |
| Δ Capex/assets | −0.318 | −0.210 | -1.808 |
| Δ Equity/assets | −4.912 | −3.237 | -2.729 |
| *Later response (2020Q1–2020Q3)* | | | |
| Δ Liquid assets/assets | −0.0544 | 2.599 | -3.996 |
| Δ Capex/assets | −0.331 | −0.285 | -.65 |
| Δ Equity/assets | 1.321 | −0.250 | 2.053 |
| *Annual response (2019–2020)* | | | |
| Δ Log(employees) | −0.0583 | −0.0556 | -.148 |

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Table 4: This table compares firms that drew funds from a credit line to firms with no recorded drawdowns for firms in industries that were relatively unexposed to the COVID-19 shutdown. The first row presents the number of observations in each group, and the remaining rows present the respective means as well as the t-statistic from a difference in means test.

| | Drawdown | No drawdown | T-statistic |
|---|---|---|---|
| N | 84 | 1,961 | |
| *Upon impact* | | | |
| Exposed (2019Q4) | 0 | 0 | |
| Unexposed (2019Q4) | 1 | 1 | |
| Log(assets) (2019Q4) | 7.862 | 4.994 | 17.219 |
| Stress (2020Q1) | −4.692 | −2.974 | -.821 |
| Liquid assets/assets (2019Q4) | 8.342 | 31.76 | -19.81 |
| Capex/assets (2019Q4) | 1.031 | 0.849 | 1.739 |
| Log(employees) (2019) | 2.811 | 0.409 | 7.96 |
| Equity/assets (2019Q4) | 36.98 | 45.70 | -2.953 |
| *Initial response (2019Q4–2020Q1)* | | | |
| Δ Liquid assets/assets | 5.816 | 0.405 | 11.17 |
| Δ Capex/assets | −0.176 | −0.134 | -.954 |
| Δ Equity/assets | −3.479 | −0.746 | -5.59 |
| *Later response (2020Q1–2020Q3)* | | | |
| Δ Liquid assets/assets | −0.858 | 3.042 | -4.796 |
| Δ Capex/assets | −0.0854 | −0.0696 | -.267 |
| Δ Equity/assets | 1.915 | 1.102 | 1.138 |
| *Annual response (2019–2020)* | | | |
| Δ Log(employees) | −0.0446 | 0.000589 | -1.968 |

We estimate the panel model

$$Y_{it} = \alpha_i + \psi_t + \sum_{t \neq 2019Q4} \beta_t Drawdown_i \times \psi_t + \gamma X_{it} + \epsilon_{it} \tag{2}$$

where $Y_{it}$ is the value in quarter $t$ for firm $i$ of one of the dependent variables (liquid assets to total assets, capital expenditure to total assets, or book equity to total assets), $\alpha_i$ represents firm fixed effects, $\psi_t$ represents quarter fixed effects, $Drawdown_i$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, and $X_{it}$ is a set of controls that includes current liquidity stress and the lag of total assets. T-statistics are computed using firm-clustered standard errors. We also estimate a corresponding specification using the annual data where the dependent variable is the logarithm of the number of employees.

We include firm fixed effects and other control variables to help uniquely identify the effect of credit line drawdowns on the set of dependent variables. These other regressors address the concern that credit line drawdowns could have been correlated with other static or time-varying firm characteristics that could also affect the dependent variables. As an example in the case where the dependent variable is the ratio of liquid assets to total assets, firms that were in industries with greater exposure to the shutdown may have both drawn funds from a credit line and faced greater cash outflows, resulting in a downward bias of the coefficient $\beta$ without controlling for this exposure. Another possibility is that firms that had weaker liquidity positions at the start of the crisis could have had a greater tendency to both draw funds from a credit line and reduce cash outflows in order to increase their liquidity, resulting in an upward bias of the coefficient $\beta$ in the absence of controlling for this characteristic.

We also estimate the intensive margin of the effect of credit line drawdowns by estimating a similar set of regressions except restricting to the subsample of firms that drew funds from a credit line and using the logarithm of total credit line drawdowns in March 2020, denoted by $DrawdownSize_{ij}$, as the treatment variable. It is useful to also consider the intensive margin because the amount that a firm draws from a credit line could depend on the intended use of the funds. For example, a firm seeking to accumulate a precautionary buffer of liquidity due to anticipation of losses for a long period of time may be more likely to draw a larger volume of funds compared to a firm

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seeking to finance current investment opportunities.

It is possible that credit line drawdowns could have also been correlated with unobserved factors affecting the response variables. To sharpen the identification, the regression in equation (2) can be interpreted like a difference-in-differences design. The difference-in-differences design identifies the causal effect of drawdowns on liquidity under the assumption that firms that drew funds from a credit line and firms that had no drawdowns would have experienced parallel trends in the liquid assets to total assets ratio in the absence of the drawdown. In the respective results sections, we assess the plausibility of this assumption for each variable by checking the trends of the two groups. Note that we also compare firms with or without a credit line drawdown with respect to pre-existing characteristics in Table 2 for the full sample, Table 3 for the subset of relatively exposed firms, and Table 4 for the subset of relatively unexposed firms. As described in Section 4, drawdowns are consistently associated with size, illiquidity, and leverage.

We also estimate a corresponding cross-sectional specification

$$\Delta Y_{ij} = \beta Drawdown_{ij} + \gamma X_{ij} + \alpha_j + \epsilon_{ij} \tag{3}$$

where $\Delta Y_{ij}$ is the difference from 2019Q4 to 2020Q1 or from 2020Q1 to 2020Q3 of one of the dependent variables (liquid assets to total assets, capital expenditure to total assets, or equity to total assets) for firm $i$ in 2-digit NAICS industry $j$, $X_{ij}$ is a set of controls, $\alpha_j$ represents industry fixed effects, and $Drawdown_{ij}$ is a dummy indicating whether a firm drew funds from a credit line in March 2020. The control variables include the logarithm of total assets in 2019Q4 and liquidity stress in 2020Q1. T-statistics are computed using heteroskedasticity-robust standard errors. We also estimate a corresponding specification using the annual data for the number of employees.

We specifically focus on two variations of the cross-sectional specification. First, we include dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification to assess the relative response associated with these industry groups. Second, as a robustness exercise, we also estimate a corresponding cross-sectional specification where drawdowns are instrumented by the logarithm of a firm's pre-existing level of undrawn revolving credit commitments from

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Capital IQ as of the last reported date in 2019. This can be interpreted as a limit on the amount of credit a firm can draw during the COVID-19 shutdown. A more detailed description of the instrumental variables specification and the results can be accessed via an Online Appendix. The results are generally consistent with the OLS estimates.

# 6 Results

## 6.1 Liquidity

The results in this section provide evidence that firms drew down their credit lines to accumulate liquidity at the start of the pandemic, which is consistent with a precautionary measure to safeguard against future liquidity risk.

Figure 2 shows the results from estimating equation (2) with the percentage of liquid assets to total assets as the dependent variable. The left panel shows this comparison for all industries, the middle panel restricts to industries that were relatively exposed to the shutdown, and the right panel restricts to industries that were relatively unexposed to the shutdown. In each case, fluctuations in the relative trend between the two groups of firms before 2019Q4 are small compared to the sharp relative increase in liquidity for the firms that drew from a credit line in 2020Q1. After 2020Q1, the firms that drew from a credit line exhibit a relative decline in liquidity.

Table 5 shows the results from estimating the cross-sectional specification given by equation (3) and also includes interactions of drawdowns and the control variables with the industry exposure subsets. The initial effect of drawdowns on liquidity from 2019Q4 to 2020Q1 is positive but not significantly different across the industry groups. The effect from 2020Q1 to 2020Q3 is negative but weaker in magnitude for firms in relatively exposed industries.

Table 6 shows the results from estimating the cross-sectional specification given by equation (3) except restricting to firms that had a drawdown and using the magnitude of drawdowns in place of an indicator. Similar to the extensive margin, drawdown size is positively associated with an increase in liquidity during 2019Q4 to 2020Q1 and a decrease from 2020Q1 to 2020Q3.

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Figure 2: The left panel shows the estimates $\beta_t$ from estimating the model $Y_{it} = \alpha_i + \psi_t + \sum_{t \neq 2019Q4} \beta_t Drawdown_i \times \psi_t + \gamma X_{it} + \epsilon_{it}$, where $Y_{it}$ is the percentage of liquid assets (cash and short-term investments) to total assets for firm $i$ in quarter $t$, $\alpha_i$ represents firm fixed effects, $\psi_t$ represents quarter fixed effects, $Drawdown_i$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, and $X_{it}$ is a set of controls that includes current liquidity stress and the lag of total assets. Liquidity stress is defined as the lag of short-term debt minus the lag of liquid assets minus net income as a percentage of lag total assets. 95% confidence intervals are computed using firm-clustered standard errors. The middle panel shows the same within the subset of industries that were relatively exposed to the COVID-19 shutdown. The right panel shows the same within the subset of industries that were relatively unexposed to the COVID-19 shutdown. See Section 3 for a more detailed description of the industry groups.

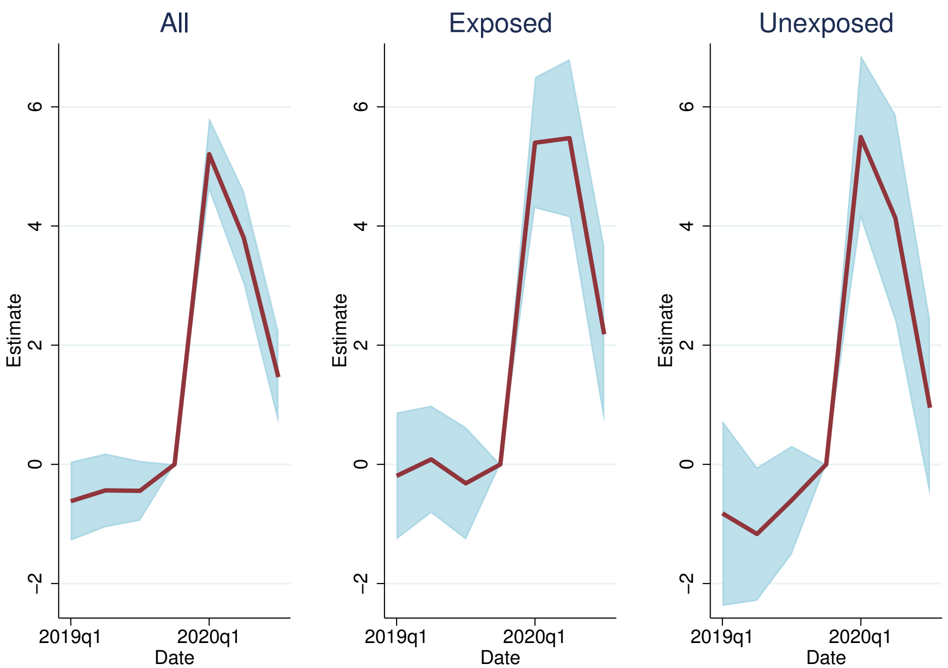


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Table 5: Effect of credit line drawdowns on liquid assets to total assets (extensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta Drawdown_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in liquid assets to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 19Q4-20Q1 | Interactions | 20Q1-20Q3 | Interactions |
| Drawdown | 4.705*** | 4.680*** | -2.514*** | -2.866*** |
| | (18.07) | (12.62) | (-6.85) | (-5.73) |
| Drawdown x Exposed | | -0.186 | | 1.688** |
| | | (-0.29) | | (1.99) |
| Drawdown x Unexposed | | 0.349 | | -0.456 |
| | | (0.57) | | (-0.46) |
| Observations | 5092 | 5092 | 5090 | 5090 |
| $R^2$ | 0.061 | 0.063 | 0.042 | 0.044 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

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Table 6: Effect of credit line drawdowns on liquid assets to total assets (intensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta DrawdownSize_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in liquid assets to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is the logarithm of total credit line drawdowns during March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1)<br>19Q4-20Q1 | (2)<br>Interactions | (3)<br>20Q1-20Q3 | (4)<br>Interactions |
|---|---|---|---|---|
| Drawdown size | 4.103*** | 4.132*** | -2.509*** | -2.648*** |
| | (13.01) | (9.37) | (-5.10) | (-4.21) |
| Drawdown size x Exposed | | 0.643 | | 0.512 |
| | | (0.86) | | (0.47) |
| Drawdown size x Unexposed | | -0.298 | | -0.253 |
| | | (-0.36) | | (-0.16) |
| Observations | 397 | 397 | 397 | 397 |
| $R^2$ | 0.443 | 0.492 | 0.192 | 0.225 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

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## 6.2 Capital investment

The results in this section do not provide robust evidence that drawdowns were used to maintain investment during the shutdown on average. However, firms in relatively unexposed industries that drew a modest amount of funds were relatively more likely to use the liquidity to finance investment.

Figure 3 shows the results from estimating equation (2) with the percentage of capital expenditure to total assets as the dependent variable.[5] Fluctuations in the relative trend between the two groups of firms before 2019Q4 are small for the full sample and the subsample of exposed firms, although there appear to be more preceding fluctuations in the subsample of unexposed firms. The firms that drew funds from a credit line generally appear to exhibit a slightly more severe decline in investment compared to the other firms, both initially in 2020Q1 and over time during 2020Q2 to 2020Q3. For the subsets of exposed and unexposed firms, the results are qualitatively similar but not statistically significant.

Table 7 shows the results from estimating the cross-sectional specification given by equation (3) and also includes interactions of drawdowns and the control variables with the industry exposure subsets. Drawdowns are generally associated with decreased investment but to a weaker extent for firms in relatively unexposed industries, although none of these result is statistically significant in this exercise.

Table 8 shows the results from estimating the cross-sectional specification given by equation (3) except restricting to firms that had a drawdown and using the magnitude of drawdowns in place of an indicator. Firms that were relatively unexposed to the crisis exhibited a relatively more negative relationship between drawdown size and investment, indicating that smaller drawdowns more strongly associated with investment within this subsample.

To illustrate this graphically, the left panel of Figure 4 shows the difference in the capital expenditure to total assets ratio from 2019Q4 to 2020Q1 for firms that drew down their credit lines relative to the magnitude of the drawdown after partialling out the control variables specified in equation (3). The left panel does not indicate a strong rela-

[5]As in Figure 2, the left panel shows this comparison for all industries, the middle panel restricts to industries that were relatively exposed to the shutdown, and the right panel restricts to industries that were relatively unexposed to the shutdown.

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Figure 3: The left panel shows the estimates $\beta_t$ from estimating the model $Y_{it} = \alpha_i + \psi_t + \sum_{t \neq 2019Q4} \beta_t Drawdown_i \times \psi_t + \gamma X_{it} + \epsilon_{it}$, where $Y_{it}$ is the percentage of capital expenditure to total assets for firm $i$ in quarter $t$, $\alpha_i$ represents firm fixed effects, $\psi_t$ represents quarter fixed effects, $Drawdown_i$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, and $X_{it}$ is a set of controls that includes current liquidity stress and the lag of total assets. Liquidity stress is defined as the lag of short-term debt minus the lag of liquid assets minus net income as a percentage of lag total assets. 95% confidence intervals are computed using firm-clustered standard errors. The middle panel shows the same within the subset of industries that were relatively exposed to the COVID-19 shutdown. The right panel shows the same within the subset of industries that were relatively unexposed to the COVID-19 shutdown. See Section 3 for a more detailed description of the industry groups.

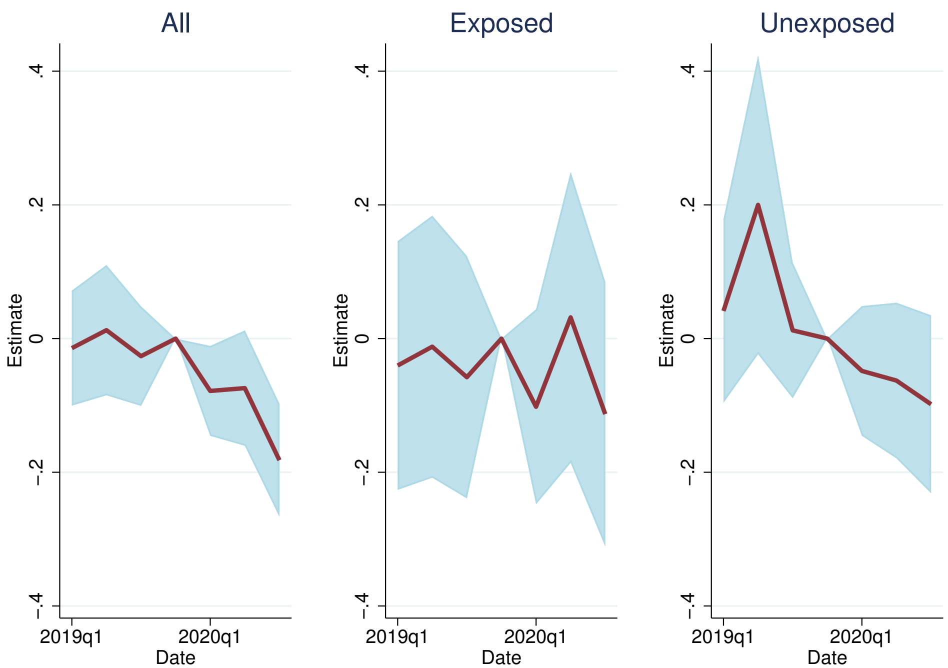


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Table 7: Effect of credit line drawdowns on capital expenditure to total assets (extensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta Drawdown_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in capital expenditure to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 19Q4-20Q1 | Interactions | 20Q1-20Q3 | Interactions |
| Drawdown | -0.035 | -0.027 | -0.000 | -0.028 |
| | (-1.20) | (-0.67) | (-0.00) | (-0.64) |
| Drawdown x Exposed | | -0.050 | | 0.099 |
| | | (-0.67) | | (1.15) |
| Drawdown x Unexposed | | 0.022 | | 0.074 |
| | | (0.35) | | (0.94) |
| Observations | 4941 | 4941 | 4942 | 4942 |
| $R^2$ | 0.010 | 0.011 | 0.038 | 0.058 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

Table 8: Effect of credit line drawdowns on capital expenditure to total assets (intensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta DrawdownSize_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in capital expenditure to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is the logarithm of total credit line drawdowns during March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1)<br>19Q4-20Q1 | (2)<br>Interactions | (3)<br>20Q1-20Q3 | (4)<br>Interactions |
|---|---|---|---|---|
| Drawdown size | -0.027 | 0.034 | -0.054 | -0.006 |
| | (-0.71) | (0.65) | (-1.13) | (-0.13) |
| Drawdown size x Exposed | | -0.120 | | -0.148 |
| | | (-1.39) | | (-1.16) |
| Drawdown size x Unexposed | | -0.181** | | 0.012 |
| | | (-2.09) | | (0.10) |
| Observations | 395 | 395 | 395 | 395 |
| $R^2$ | 0.090 | 0.114 | 0.130 | 0.170 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

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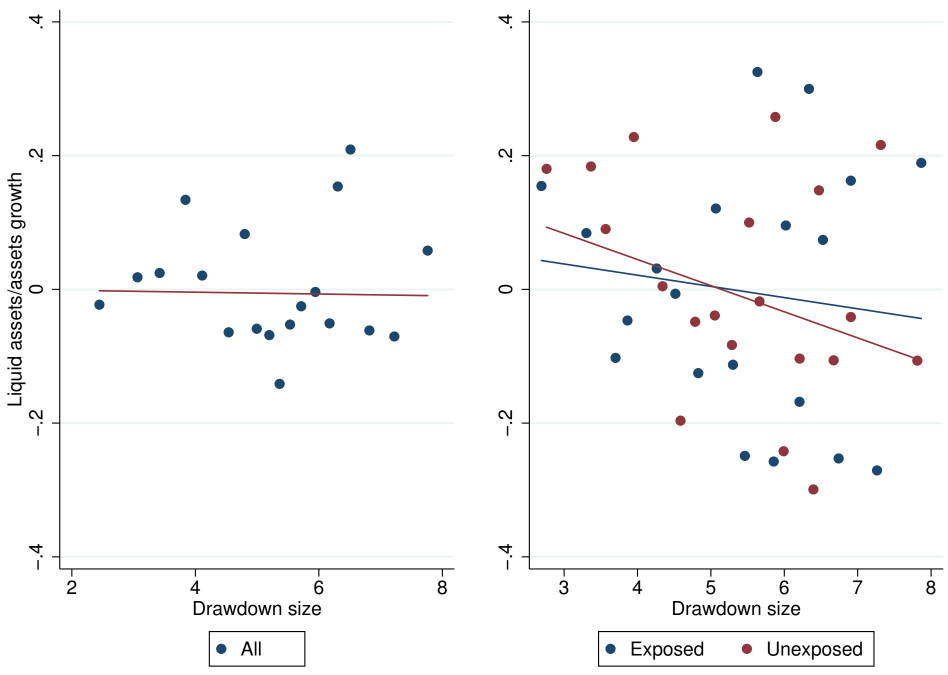
Figure 4: The left panel shows a binned scatterplot of the difference in the ratio of capital expenditure to assets from 2019Q4 to 2020Q1 for firms that drew from a credit line on the y-axis and the logarithm of total funds acquired through a credit line drawdown during March 2020 on the x-axis. The right panel shows the same within the subset of industries that were relatively exposed to the COVID-19 shutdown and industries that were relatively unexposed to the COVID-19 shutdown. Section 3 for a more detailed description of the industry groups.

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tionship between the magnitude of credit line drawdowns and physical investment in general. However, the right panel shows that, for firms in industries that were relatively unexposed to the COVID-19 shock, smaller credit line drawdowns were relatively more strongly associated with greater investment.

### 6.3 Employment

The results in this section do not provide strong evidence that the credit line drawdowns were used to support employment.

Note that the analysis for employment is conducted at annual frequency because that is the frequency at which this data is reported. Figure 5 shows the results from estimating equation (2) with the logarithm of the number of employees as the dependent variable.[6] The relative trend is approximately parallel before the pandemic in the full sample. During the pandemic, firms with drawdowns exhibited a greater loss in employees compared to other firms. Drawdowns are also associated with a reduction in employees for the subsamples of relatively exposed or relatively unexposed firms, but those associations are not statistically significant.

Table 9 shows the results from estimating the cross-sectional specification given by equation (3) and also includes interactions of drawdowns and the control variables with the industry exposure subsets. Drawdowns were associated with decreased employment, with no significant interactions with industry exposure to the pandemic.

Table 10 shows the results from estimating the cross-sectional specification given by equation (3) except restricting to firms that had a drawdown and using the magnitude of drawdowns in place of an indicator. Drawdown size was not significantly associated with the change in employees, although the association is relatively more positive on the subsample of exposed firms.

[6] As in Figure 2, the left panel shows this comparison for all industries, the middle panel restricts to industries that were relatively exposed to the shutdown, and the right panel restricts to industries that were relatively unexposed to the shutdown.

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Figure 5: The left panel shows the estimates $\beta_t$ from estimating the model $Y_{it} = \alpha_i + \psi_t + \sum_{t \neq 2019Q4} \beta_t Drawdown_i \times \psi_t + \gamma X_{it} + \epsilon_{it}$, where $Y_{it}$ is the logarithm of the number of employees for firm $i$ in quarter $t$, $\alpha_i$ represents firm fixed effects, $\psi_t$ represents quarter fixed effects, $Drawdown_i$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, and $X_{it}$ is a set of controls that includes current liquidity stress and the lag of total assets. Liquidity stress is defined as the lag of short-term debt minus the lag of liquid assets minus net income as a percentage of lag total assets. 95% confidence intervals are computed using firm-clustered standard errors. The middle panel shows the same within the subset of industries that were relatively exposed to the COVID-19 shutdown. The right panel shows the same within the subset of industries that were relatively unexposed to the COVID-19 shutdown. See Section 3 for a more detailed description of the industry groups.

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Table 9: Effect of credit line drawdowns on the logarithm of the number of employees (extensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta Drawdown_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in logarithm of the number of employees for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, $X_{ij}$ includes the logarithm of assets as of 2019 and liquidity stress as of 2020, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019 to 2020. Column (2) shows the results when the difference in the dependent variable is taken from 2019 to 2020 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1)<br>19-20 | (2)<br>Interactions |
|---|---|---|
| Drawdown | -0.041*** | -0.033** |
| | (-4.08) | (-2.11) |
| Drawdown x Exposed | | 0.001 |
| | | (0.06) |
| Drawdown x Unexposed | | -0.027 |
| | | (-1.00) |
| Observations | 1561 | 1561 |
| $R^2$ | 0.097 | 0.106 |
| Controls | Yes | Yes |
| Industry FE | Yes | Yes |

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Table 10: Effect of credit line drawdowns on the logarithm of the number of employees (intensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta DrawdownSize_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in the logarithm of the number of employees for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is the logarithm of total credit line drawdowns during March 2020, $X_{ij}$ includes the logarithm of assets as of 2019 and liquidity stress as of 2020, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019 to 2020. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1) | (2) |
|---|---|---|
| | 19-20 | Interactions |
| Drawdown size | -0.009 | -0.030 |
| | (-0.62) | (-1.36) |
| Drawdown size x Exposed | | 0.064* |
| | | (1.84) |
| Drawdown size x Unexposed | | 0.018 |
| | | (0.54) |
| Observations | 208 | 208 |
| $R^2$ | 0.264 | 0.309 |
| Controls | Yes | Yes |
| Industry FE | Yes | Yes |

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## 6.4 Equity

We consider the ratio of book equity to total assets as a means to assess whether firms engaged in share buybacks, which would decrease the equity to assets ratio, or repaid their credit lines, which would increase the equity to assets ratio. Note that this is only an indirect means to assess these actions since other events can also affect the equity to assets ratio.

The results in this section generally indicate that credit line drawdowns were associated with an immediate reduction in the equity to total assets ratio from 2019Q4 to 2020Q1, which likely reflects the mechanical effect of drawing down debt on their balance sheets, followed by a recovery during 2020Q1 to 2020Q3, which is consistent with firms paying back their credit lines. This is generally consistent with the hypothesis that firms drew down their credit lines as a precautionary measure at the start of the crisis and then repaid them later as the economic repercussions of the pandemic stabilized.

Note that the analysis for equity restricts to firms with at least $10 million assets to mitigate the effect of outlier firms whose total equity is negative and several times the magnitude of total assets. Figure 6 shows the results from estimating equation (2) with the logarithm of the number of employees as the dependent variable.[7] The equity to assets ratio exhibits an increasing relative trend before the pandemic, but this difference is relatively small compared to the large relative decline at the start of the pandemic in 2020Q1. This likely reflects the fact that drawing down a credit line directly increases a firm's indebtedness. However, from 2020Q1 to 2020Q3 the equity to assets ratio begins to recover in the full sample, which is consistent with repayment of the drawdowns.[8]

Table 11 shows the results from estimating the cross-sectional specification given by equation (3) and also includes interactions of drawdowns and the control variables with the industry exposure subsets. The results indicate that drawdowns were associated with a relative decline in the equity to assets ratio from 2019Q4 to 2020Q1 and a relative increase from 2020Q1 to 2020Q3.

Table 12 shows the results from estimating the cross-sectional specification given

[7]As in Figure 2, the left panel shows this comparison for all industries, the middle panel restricts to industries that were relatively exposed to the shutdown, and the right panel restricts to industries that were relatively unexposed to the shutdown.

[8]Note that, in this exercise, the relative trend for the subsample of firms in exposed industries from 2020Q1 to 2020Q3 qualitatively depends on whether liquidity stress is included as a control variable.

Figure 6: The left panel shows the estimates $\beta_t$ from estimating the model $Y_{it} = \alpha_i + \psi_t + \sum_{t \neq 2019Q4} \beta_t Drawdown_i \times \psi_t + \gamma X_{it} + \epsilon_{it}$, where $Y_{it}$ is the percentage of equity to total assets for firm $i$ in quarter $t$, $\alpha_i$ represents firm fixed effects, $\psi_t$ represents quarter fixed effects, $Drawdown_i$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, and $X_{it}$ is a set of controls that includes current liquidity stress and the lag of total assets. Liquidity stress is defined as the lag of short-term debt minus the lag of liquid assets minus net income as a percentage of lag total assets. 95% confidence intervals are computed using firm-clustered standard errors. The middle panel shows the same within the subset of industries that were relatively exposed to the COVID-19 shutdown. The right panel shows the same within the subset of industries that were relatively unexposed to the COVID-19 shutdown. See Section 3 for a more detailed description of the industry groups.

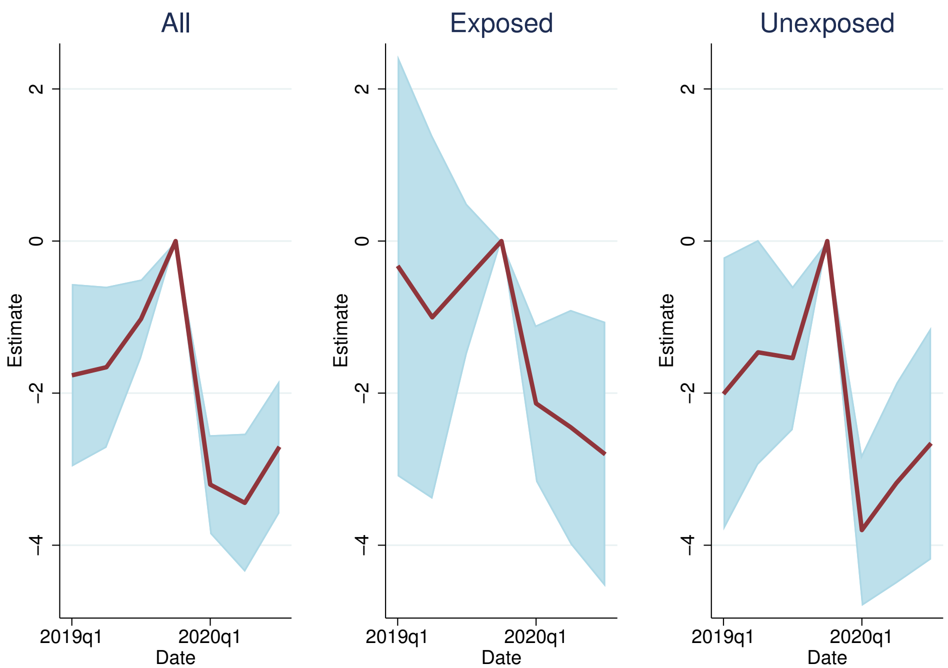


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Table 11: Effect of credit line drawdowns on equity to total assets (extensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta Drawdown_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in equity to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is a dummy indicating whether a firm drew funds from a credit line in March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1)<br>19Q4-20Q1 | (2)<br>Interactions | (3)<br>20Q1-20Q3 | (4)<br>Interactions |
|---|---|---|---|---|
| Drawdown | -2.671*** | -2.285*** | 1.333*** | 0.708 |
| | (-7.73) | (-4.42) | (3.17) | (1.31) |
| Drawdown x Exposed | | -0.429 | | 1.893* |
| | | (-0.52) | | (1.74) |
| Drawdown x Unexposed | | -0.657 | | 0.430 |
| | | (-0.86) | | (0.44) |
| Observations | 5094 | 5094 | 5092 | 5092 |
| $R^2$ | 0.138 | 0.151 | 0.011 | 0.017 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

Table 12: Effect of credit line drawdowns on equity to total assets (intensive margin, cross-section, interactions). This table presents results from estimating variations of the equation $\Delta Y_{ij} = \beta DrawdownSize_i + \gamma X_{ij} + \alpha_j + \epsilon_{ij}$, where $\Delta Y_{ij}$ is the difference in equity to total assets for firm $i$ in 2-digit NAICS industry $j$, $Drawdown_{ij}$ is the logarithm of total credit line drawdowns during March 2020, $X_{ij}$ includes the logarithm of assets as of 2019Q4 and liquidity stress as of 2020Q1, and $\alpha_j$ represents 2-digit NAICS industry fixed effects. T-statistics computed using heteroskedasticity-robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. Column (1) shows the results from the baseline specification when the difference in the dependent variable is taken from 2019Q4 to 2020Q1. Column (2) shows the results when the difference in the dependent variable is taken from 2019Q4 to 2020Q1 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification. Column (3) shows the results from the baseline specification when the difference in the dependent variable is taken from 2020Q1 to 2020Q3. Column (4) shows the results when the difference in the dependent variable is taken from 2020Q1 to 2020Q3 and adding dummies for relatively exposed and unexposed industries as well as their interactions with the regressors in the baseline specification.

| | (1)<br>19Q4-20Q1 | (2)<br>Interactions | (3)<br>20Q1-20Q3 | (4)<br>Interactions |
|---|---|---|---|---|
| Drawdown size | -1.732*** | -2.006*** | 0.800* | 0.685 |
| | (-4.31) | (-3.74) | (1.88) | (1.24) |
| Drawdown size x Exposed | | 0.938 | | -0.284 |
| | | (1.09) | | (-0.35) |
| Drawdown size x Unexposed | | 1.356 | | 0.197 |
| | | (1.02) | | (0.14) |
| Observations | 397 | 397 | 397 | 397 |
| $R^2$ | 0.193 | 0.268 | 0.142 | 0.181 |
| Controls | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |

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by equation (3) except restricting to firms that had a drawdown and using the magnitude of drawdowns in place of an indicator. Drawdown size is associated with an immediate reduction in the equity to assets ratio and a subsequent partial recovery.

## 7 Conclusion

This paper examines how firms used credit line drawdowns during the COVID-19 shutdown. Drawdowns were strongly associated with increased cash holdings in the short-run, consistent with the interpretation that firms sought to reduce future liquidity risk. We do not find strong evidence that firms used credit line drawdowns to maintain physical investment or employment during the pandemic. Instead, drawdowns were associated with an increase in the equity to assets ratio as the economy stabilized from 2020Q1 to 2020Q3, which suggests firms were repaying their drawdowns. However, there is some evidence that firms in industries that were relatively unexposed to the economic restrictions associated with the shutdown were relatively more likely to use their credit lines to maintain investment during the shutdown, especially if they drew a modest amount of funds.

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*Covid Economics* **Issue 78, 14 May 2021**

# Outlasting the pandemic: Corporate payout and financing decisions during Covid-19[1]

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*The outbreak of the Covid-19 pandemic massively increased uncertainty about firms' cash flows and access to financial markets. We examine its effect on firms' strategies for preserving cash by suspending dividends and share repurchase programs and raising new funds through bond and equity issues. Our estimates suggest that between March and December 2020 US firms saved a combined $86bn by suspending or reducing dividend payments and another $140bn from suspending buybacks. We identify a short list of firm and stock characteristics that explain most of the cross-sectional variation in firms' payout and financing decisions. We show that the expansive monetary policies pursued by the Federal Reserve in the early phase of the pandemic crucially affected the timing and sequencing of firms' decisions. Announcement effects on stock returns were highly unusual during the pandemic as dividend and buyback suspensions were associated with a more rapid recovery in firms' stock prices, consistent with investors interpreting them as prudent actions that helped reduce risks.*

1 We are grateful to Bo Becker, Ralph S.J. Koijen and Per Strömberg for valuable suggestions on our paper. We also received helpful comments from seminar participants at the Singapore Management University. We thank Youssef Aljabi and Mark McAvoy, Ping Gu, and Yiyang Yin for excellent research assistance with preparing the data.
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*"Whatever it takes." — July 26, 2012*

*President of the European Central Bank Mario Draghi, expressing the ECB's commitment to the Euro.*

*"As long as it takes." — March 22, 2021*

*Federal Reserve Chairman Jay Powell, pledging continued support*

# 1 Introduction

The outbreak of the Covid-19 pandemic in the Spring of 2020 caused economic disruption on a scale and at a speed that were unprecedented in modern history. Throughout the ensuing months, firms were scrambling to keep up with the impact the lockdown of the economy had on their cash flows, cash reserves, and balance sheets. Uncertainty about the economy's trajectory remained extremely high throughout 2020 depending, as it did, on an unusual array of factors such as medical progress in developing a vaccine, fiscal stimulus programs, aggressive monetary policy measures, and shifts in household and corporate behavior. Absent any direct historical precedents, attempts at forecasting the magnitude and duration of the pandemic's impact on corporate earnings and growth prospects posed unique and unparalleled challenges.[1]

Uncertainty about the speed and length of the path towards recovery accompanied by sharp reductions in cash flows, forced many firms to preserve short-term capital and raise new funds to ensure their survival. Rather than taking a single action to restore their balance sheet and liquidity position, many firms engaged in a string of decisions meant to reduce cash outflows (suspensions of dividends and share buybacks) or tap into capital markets in sometimes desperate attempts at outlasting the pandemic and dealing with duration risk.

The intricate chain of actions taken by many companies in their quest to survive the pandemic is vividly illustrated through the example of Carnival, a major US-based cruise line operator. Against the background of Carnival's share price along with major events affecting the cruise line industry, Figure 1 shows a timeline for the corporate

[1] Studies examining how the pandemic affected growth prospects and economic uncertainty include Gourinchas (2020), Eichenbaum et al. (2020), Atkeson (2020), and Ludvigson et al. (2020).

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actions taken by Carnival during 2020. On March 31, Carnival announced it had suspended dividends and share buybacks. This was immediately followed the next day (April 1) by simultaneous equity and bond issues. A chain of actions ensued with bond issues on July 15 (Moody's rating: Ba1), August 14 (Ba1), and November 20 (B2), along with equity issues on August 05, September 15, and November 17. In total, Carnival raised \$8.2bn in bonds and another \$4.1bn in equity; the company also drew down a revolving credit line of \$2.8bn. Using these numbers, our estimate of the quarterly cash burn (shown in grey shades) is \$1.7bn in Q2, \$3.3bn in Q3, and \$1.9bn in Q4 of 2020.[2]

This example illustrates several important points that, as we shall see, hold more broadly in our sample. First, companies reacted with extraordinary speed to the pandemic. The US declared a state of national emergency on March 13 and Carnival suspended dividends and share repurchases a little more than two weeks later. Second, corporate actions that normally are distantly spaced in time frequently got compressed over very short periods.[3] Third, as firms updated their expectations about the duration of the pandemic, they dynamically adjusted their actions, suspending payouts and raising capital as they thought prudent and deemed necessary.

In this paper we provide a detailed look into how US firms managed their payout policies and capital structure during 2020, notably their decisions to suspend dividend payment and share repurchase programs as well as their financing activity in the stock and bond markets. We begin by providing a comprehensive analysis of the timing and magnitude of these decisions during the Covid pandemic. The three weeks following the US declaration on March 13th of a national emergency witnessed a sharp uptick in the number of firms suspending their dividends and share repurchase programs. Payout suspensions remained elevated until mid-May before quickly receding to more normal levels.

[2]The cash burn rate is defined as the change in cash and cash equivalents between two consecutive quarters, accounting for new bond and equity issues. In Q1 2021, Carnival raised \$3.5bn in debt and \$1bn in equity and had an average monthly cash burn rate of \$785mn.

[3]Having suspended dividends and share buybacks on March 31st, Carnival promptly issued stocks and bonds the following day (April 1st). Four corporate actions announced over the course of two business days would be unheard of in normal times. Consistent with the pecking order theory, Carnival's first actions were to preserve internal sources of capital and, only then, raise funds by tapping into external capital markets.

Overall, between March and December of 2020, we estimate that US firms saved \$29bn through dividend suspensions and another \$56.5bn by reducing (but not suspending) dividends. Estimates of firm savings from buyback suspensions are more uncertain. Under the assumption that the firms that announced a suspension of their buyback programs in 2020 would have continued with the same amount of share repurchases during 2020 as they did in 2019, savings from this source amount to an additional \$140bn in 2020.

The corporate bond market came to a near-standstill in late February and the first two weeks of March before bond issues came roaring back to \$60bn during the week of March 15, increasing further to exceed \$80bn per week in late March and early April. This followed a sequence of massive policy interventions by the Federal Reserve system and the US government. From mid-March to mid- April, firms continued to issue large amounts of bonds, but only for issues rated at or above upper medium grade whereas the market for non-investment grade bonds largely disappeared.

The market for equity issues experienced even stronger disruption during the pandemic as very few companies issued stocks between the first reported Covid-related death in the US in mid-February and mid-April. Equity issuing recovered somewhat in mid-May with almost \$20bn raised during the week of May 10 and elevated activity lasting for another six weeks.

The bond market played a far more important role than the stock market for firms' ability to raise capital during the pandemic. The total dollar amount raised by bond issues went up sharply after the pandemic outbreak, peaking at \$230bn in March and April of 2020 and exceeding \$200bn in May. These are by far the largest monthly bond issues by US corporations in recent decades and are consistent with the Federal Reserve's massive purchase of investment-grade corporate bonds.

While the dollar amount raised by equity issuers also increased sharply in March and April of 2020, their peak is not nearly as large in absolute terms (less than \$50bn each month) or relative to earlier periods. This is consistent with the sharp fall in equity prices during the early phase of the pandemic which made it more difficult–and costlier–for firms to tap into this source of financing.

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Next, we explore which firm and stock characteristics help explain cross-sectional variation in firms' decisions on suspending payouts or raising new funds in the bond and equity markets. We find that a short list of accounting measures capturing firm size, leverage, cash holdings, profitability and revenue growth along with two measures of return performance in the stock market in the month leading up to an announcement are strongly associated with firms' likelihood of announcing any of these actions.

Characteristics such as firm size, leverage and cash holdings played a surprisingly small role in explaining cross-sectional variation in firms' decision to suspend dividends during the pandemic. Conversely, profitability and, in particular, revenue growth were significant drivers of dividend suspensions. Negative revenue growth also correlates strongly with an increased propensity for firms to suspend buyback programs during the pandemic.

Firms' propensity to issue bonds was far less sensitive to firm and stock characteristics during the pandemic than during the Great Recession in 2008/09. Bearing this in mind, large firms with high leverage, low profitability and negative revenue growth were significantly more likely to have issued bonds during the pandemic. Large and less profitable firms were more likely than their peers to have issued equity during the pandemic while leverage and revenue growth did not play a role in this decision. This is in sharp contrast to what we found for bond issues and suggests that firms that were highly levered and experienced low (negative) revenue growth were relatively more likely to issue bonds than equity. Another contrasting finding is that firms with the largest cash holdings were more likely to issue equity but less likely to issue bonds compared to firms with smaller cash holdings.

Firms' short-term return performance in the stock market is a strong predictor of all the corporate actions that we analyze. Firms with highly volatile and large negative idiosyncratic stock returns in the 30-day period leading up to an announcement were far more likely to have suspended dividends or buybacks during the pandemic and to have issued stocks or bonds than firms with less volatile and larger stock returns.

Having inspected drivers of corporate actions, we consider the sequencing of actions undertaken by firms to outlast the pandemic. Here we exploit that the pandemic

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triggered a substantial increase in the number of firms that undertook a chain of consecutive payout suspension or financing decisions. This makes the period ideal for studying some of the implications of the Myers and Majluf (1984) pecking order theory which holds that firms will first seek to use internal funds before issuing debt and, finally, issue equity as the least-preferred option. The pecking order theory implies testable hypotheses on how firms sequence a chain of payout and financing decisions, ruling out that firms issue equity prior to suspending dividends and share buybacks to preserve internal funds. We inspect chains of corporate actions during the pandemic as well as during the Great Recession and find fewer violations and more instances that are fully consistent with the pecking order theory during the pandemic than in 2008-09.

Finally, our paper uses event study methodology to inspect cumulative abnormal returns (CARs) and analyse how the stock market reacted to announcements of corporate actions during the pandemic. We find that firms suspended dividends and buybacks after a string of large negative returns (close to -8% in the week preceding the announcement). After the announcement, stock prices tend to bounce back, suggesting that payout suspensions were seen by markets as prudent actions that helped reduce firms' cash flow risk.

In normal times, announcements of bond issues are associated with a small and significant positive effect on CARs. We find no such announcement effect on CARs in 2020, consistent with investors not attributing any substantive information content to a bond issue. A possible explanation is the Federal Reserve's massive intervention in the bond markets which greatly reduced any information signal from successful bond issues. Interestingly, firms tended to issue bonds and shares on the back of a string of days with positive CARs and total return performance.

Our analysis is related to recent studies that examine the impact of the COVID pandemic on corporate financing decisions and financial markets. Hotchkiss et al. (2020) show that firms raised large amounts of capital in bond and equity markets during the first and second quarter of 2020. They find that smaller and riskier firms tended to raise funds in the equity market although they do not find evidence that financially constrained firms raised less capital than other firms. Acharya and Steffen (2020) show

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that firms raised cash levels during the first quarter of 2020 by initially drawing down their bank credit lines. Following the policy interventions of the Federal Reserve and central government, the highest-rated firms switched to external capital markets to raise cash. Halling et al. (2020) show that bond issues increased substantially after the outbreak of the COVID-19 pandemic both for high- and low-rated bonds. They also find that the average maturity of the newly issued bonds exceeds that of bonds issued by the same firms prior to the pandemic. Becker and Benmelech (2021) document that activity in the syndicated loan market was low during the Covid crisis and show that the Federal Reserve's interventions supported the bond market, especially the investment-grade segment, more than the loan market. Compared to these papers, ours is the first analysis that looks at how firms' decisions on dividend payouts and share buybacks were affected by the Covid crisis and how firms managed their joint payout and financing decisions to preserve short-term capital.[4]

The remainder of the paper is organized as follows. Section 2 introduces the data used in the paper and provides new evidence on the dividend and buyback suspensions announced during the early stage of the pandemic. Section 3 explores which firm-specific characteristics help explain firms' decision to suspend dividend payments and buybacks, while Section 4 discusses the sequencing of firms' payout suspension and financing decisions during the Covid-19 pandemic. Section 5 examines the reaction of stock markets to corporate announcements and Section 6 concludes.

## 2 Data

We begin our analysis by introducing our data sources and providing initial evidence and historical context on firms' payout policies and bond and equity issues during the Covid-19 pandemic. The speed with which economic events unfolded and firms responded during the pandemic makes it crucial to conduct our analysis at a much higher frequency

[4] Campello et al. (2010) survey CFOs from the U.S., Europe, and Asia to assess whether their firms were credit constrained during the global financial crisis of 2008. They find that constrained firms (i) planned larger cuts in tech spending, employment, and capital spending; (ii) burned more cash, drawing more heavily on lines of credit; and (iii) sold more assets to fund their operations.

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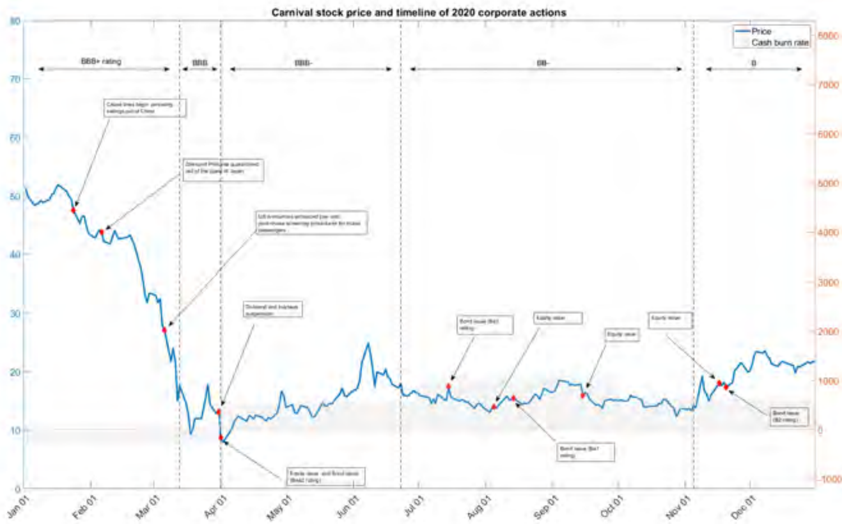


**Figure 1: Timeline of corporate actions and stock price for Carnival.** This figure plots the timeline of corporate actions taken by Carnival (CCL.N), together with its stock price and S&P bond issuer rating (line at the top) and Moody's bond ratings, in 2020.

than is common in the literature.[5] It is equally important to use announcement dates rather than, say, payout dates in order to accurately capture the timing of the information content in firms' actions. We accomplish this by using daily data as the basis for our empirical analysis.

## 2.1 Data on Dividends and Share Repurchases

We begin our analysis by explaining how we collect *daily* data on dividend and share repurchase announcements, including those made by firms that suspended dividends and buybacks. Our analysis starts in 2005 in order to include the Great Recession period in 2008-09 and a few years preceding it. The Great Recession was the last major crisis prior to the pandemic and so provides a useful comparison that helps benchmark many of our results.

Our analysis merges data from a variety of sources. First, we obtain data from the Center for Research in Security Prices (CRSP) from January 2005 through December 2020

[5] An exception is Pettenuzzo et al. (2020) who develop a model for dynamics in daily dividend data.

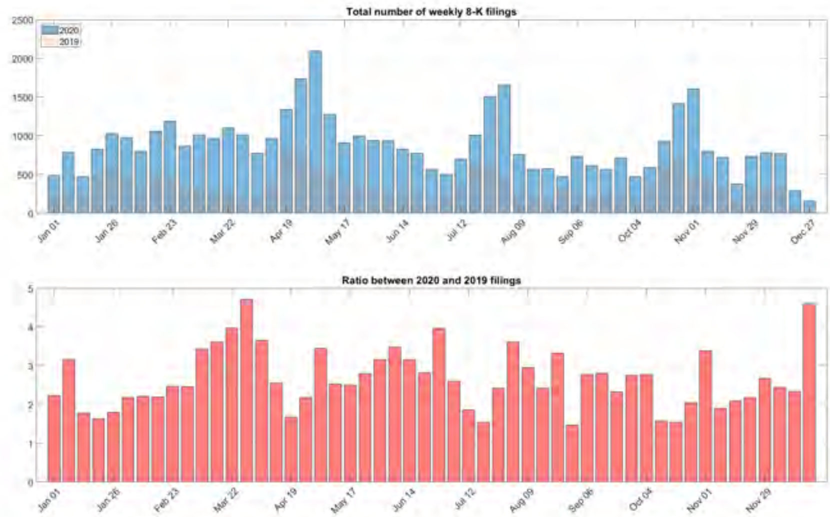


**Figure 2: Total number of weekly 8-K filings in 2019 and 2020.** The top panel plots the total number of 8-K filings by all public firms in our list for each week of 2020 (blue bars) and 2019 (grey bars), while the bottom panel shows the ratio between the 2020 and 2019 filings.

to extract daily stock prices, shares outstanding, and dividend announcements for individual firms. This sample includes all ordinary cash dividends declared by US firms with common stocks (share codes 10 and 11) listed on the NYSE, NASDAQ, or AMEX exchanges.[6] To be included, firms are required to have valid stock prices and shares outstanding when dividends are announced.

CRSP provides detailed information on dividend announcements which allows us to compute year-on-year changes in dividend distributions but does *not* include information on dividend suspension dates. Historically, this has not mattered a great deal since dividend suspensions have been rare except for during the 2008-2009 Great Recession. As we shall see, the early stage of the Covid-19 pandemic witnessed a significant change in this pattern with many firms suspending dividends as they adapted to unprecedented economic circumstances.

To obtain information on dividend suspensions, we rely on two other data sources. First, for each of the public companies in CRSP, we use the EDGAR database to download

[6]Ordinary cash dividends have CRSP distribution codes below 2000.

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all 8-K forms that companies filed to the SEC between January 2005 and December 2020. The top panel in Figure 2 compares the total number of weekly 8-K filings reported by firms in our data set in 2020 versus the corresponding number in 2019. Comparing the same weeks across the two years is a simple way to account for the pronounced seasonal cycle in quarterly filings which tend to be higher in late April, July, and October. We see a clear spike in filings from late April to early May, from late June to early July, and from late September to early October of 2020. During these weeks, 8-K filings mostly exceed 1,500, peaking at more than 2,000 in early May.

Controlling more explicitly for the seasonality effect, the bottom panel in Figure 2 shows the ratio between weekly filings in 2020 and 2019. Relative to 2019, the number of filings in 2020 is higher in every single week with increases exceeding 100% for most weeks. The largest proportional increase in filings between the two years - ranging from 300% to nearly 500% - happens in March. Clearly the Covid-19 pandemic led to a sharp rise in the arrival rate of information deemed to be "materially important" to firms and, thus, triggering an 8-K filing.[7]

While EDGAR keeps an up-to-date list of all public companies' 8-K filings, the most recent events may not yet be included. To address this concern, we complement the information extracted from EDGAR by using as our second data source the NASDAQ news platform to download recent press releases on companies in our sample.[8] Between January 1 and December 31, 2020, we identify a total of 122,706 press releases with a clear spike around late February (after the lockdown in Northern Italy) and late April. Combining the textual data from EDGAR and the NASDAQ news platform, we next identify the 8-K filings and press releases that mention dividend suspensions in either the text or title and extract the date of the suspension and the associated ticker using an automated text scraper. This process yields an initial list of 1,765 dividend suspensions. After manually reviewing each case to remove false positives, we identify a total of 498 suspensions from 2005 through 2020.

[7] A total of 46,771 8-K filings were reported between January and December of 2020.

[8] NASDAQ offers a platform for news and financial articles written by professional reporters and analysts from selected contributors that include leading media such as Reuters, MT Newswires, RTT news, or investment research firms such as Motley Fool, Zacks or GuraFocus.

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Similarly, we collect buyback suspension data from a variety of sources. First, we obtain data on suspended and cancelled buybacks from Capital IQ. In addition to this, we scrape the 8-K forms and data on company press releases as we did for the dividend suspensions. Lastly, we manually check every single buyback suspension date to obtain a final sample of 497 buyback suspensions from 2005 through 2020.[9] Finally, we merge the dividend and buyback suspensions with price and accounting data from COMPUSTAT.

Detailed collection and meticulous cleaning of data on suspensions of dividends and buyback programs is a key step in our analysis. It is also necessary; commonly used data providers either do not collect this data at all (e.g., on dividend suspensions) or only provide partial and incomplete data. For example, Capital IQ has partial data on buyback suspensions, but large and important firms such as Home Depot and Kohl's are missing from their data while we include them in our final data set.[10]

## 2.2 Timeline of the Pandemic

Before presenting our analysis of corporate actions during the pandemic, for context it is worthwhile briefly recalling just how rapidly economic and political events moved after the outbreak of the pandemic. The US declared a national emergency on March 13. This was followed on March 17 by the Federal Reserve Board announcing that it had established a commercial paper funding facility (CPFF) and a primary dealer credit facility (PDCF) to ensure flows of credit to households and businesses and help support their credit needs. The following day, on March 18, the Fed announced it had established a money market mutual fund liquidity facility (MMLF), followed by an enhancement of liquidity flowing to state and municipal money markets (March 20) and other extensive support measures announced on March 23. On March 27, the CARES Act was signed into law. Finally, on April 9, the Federal Reserve announced the provision of up to $2.3 trillion

[9] Company executives will occasionally release statements such as "the buyback program has been suspended in Q1 of 2020" without providing a precise date. We exclude such suspensions from our analysis because we cannot map them to a precise date and so are slightly under-estimating the actual number of buyback suspensions. Moreover, several buyback programs do not commit the company to repurchase a certain number of shares of its common stock on a fixed schedule so a firm could have an active buyback program that in practice is suspended without a formal announcement.

[10] Home Depot's suspension of its share repurchase program can be found in footnote (3) on page 25 of its 2020 annual report.

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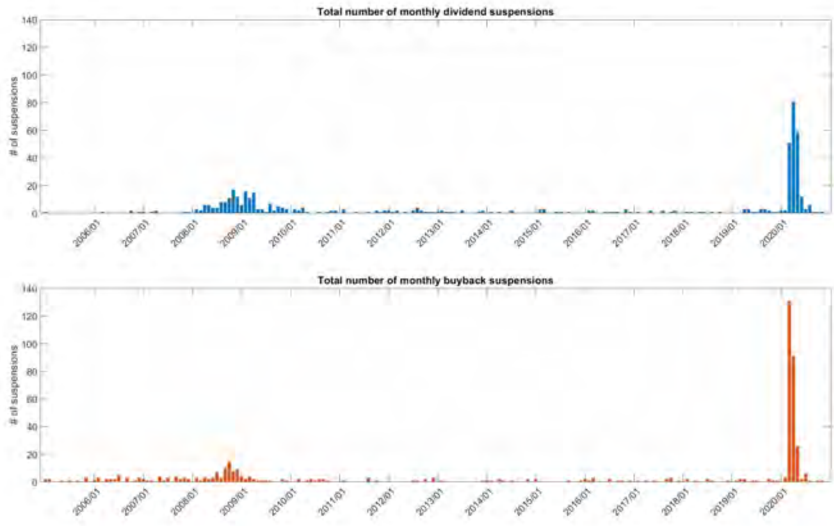


**Figure 3: Total number of monthly dividend and buyback suspensions.** The top panel plots the total number of dividend suspensions by all public firms in our list for each month between January 2005 and December 2020, while the bottom panel plots the number of buyback suspensions over the same period.

in loans aimed at supporting the economy. These were major policy actions that could be expected to significantly impact firms' access to liquidity and opportunities for raising funds in the capital markets.

## 2.3 Dividend Suspensions

The top panel in Figure 3 shows the total number of announced suspensions of dividend and share repurchase programs, aggregated by month between January 2005 and December 2020. First consider the dividend suspensions (top panel). Typically less than two or three firms (and often none) announce a suspension of dividends in any given month. There are two notable exceptions to this, namely the Great Recession (2008:01-2009:06) and the Covid pandemic in 2020. During the Great Recession, dividend suspensions peaked with 17 firms suspending dividends in November 2008, two months after the default of Lehman Brothers. Still, dividend suspension activity built up gradually, rising markedly in March and April of 2008 following J.P. Morgan's acquisition of Bear Stearns on March 16. A total of 135 dividend suspensions (81 in 2008

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and 54 in 2009) got reported during this 18-month period.

Dividend suspension numbers during the Great Recession are dwarfed by events during the pandemic. In March 2020, 51 firms announced they had suspended their dividends, followed by another 81 in April and nearly 60 in May before suspensions tapered back to 12 in June and returned to normal levels after August 2020. In total, 219 dividend suspensions were announced in 2020.

How much money did firms actually save by suspending or reducing their dividend payments? To address this question, Figure 4 plots the actual dividend cuts – along with dividend increases – announced by individual firms and summed, each month, across all firms in our sample. We also show the imputed dollar value of dividend suspensions, computed by assuming that the dividend-suspending firms, had they not announced a dividend stop, would have paid the same dividends in a given month as they did in 2019.

This imputed figure for firms' savings on dividend payments is, as we would expect, zero or extremely small in January and February 2020 and remains quite small in March and April. From May onward, the value rises to a level between \$3bn and \$4bn in most months. Moreover, from January through March, the dollar value of dividend rises far outpaces the value of any dividend cuts. In May, July, August, and October the two roughly balance out, whereas the dividend cuts are at least twice as large as dividend increases in June and December and much larger in November of 2020.

Overall, between March and December of 2020, firms saved around \$29bn through dividend suspensions. Firms saved substantially more - with the bulk concentrated in November and December of 2020 - by cutting dividends by approximately \$56.5bn between March and December. This figure exceeded the dollar value of dividend rises over the same period (\$36.5bn) by \$20bn.

The first two columns of Table 1 lists the industry composition of dividend suspensions during the two crises using the 17-industry classification scheme from Ken French's website. As expected, we observe clear differences in industry composition. During 2008-09, Banks, Insurance Companies and Other Financials counted for nearly half of all dividend suspensions (63 of 135), with Other and Automobiles counting for

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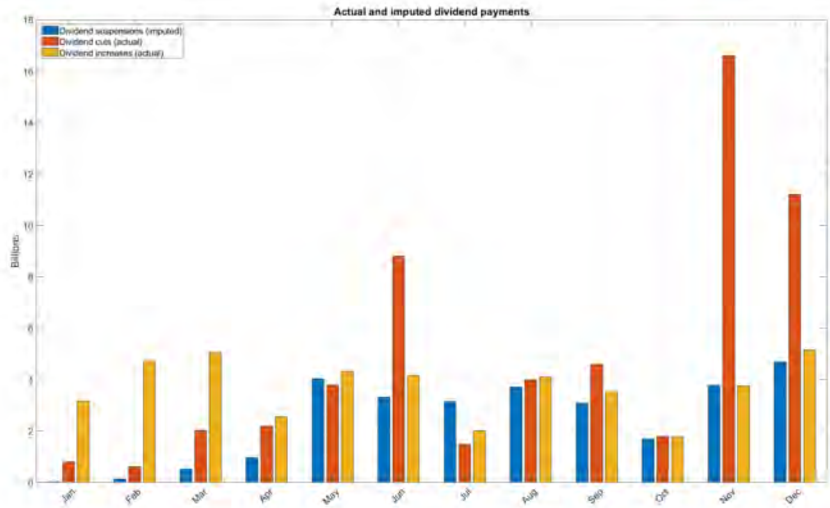


**Figure 4: Actual and "imputed" dividends in 2020.** This figure plots the total monthly dollar values of dividend increases and dividend cuts, summed across firms. It also shows the dollar values of "imputed" dividend suspensions, calculated as the sum of the dividend amounts that suspenders paid during the respective same-quarter periods in 2019.

another 21 and 13 suspensions, respectively.[11] Conversely, during the Covid-19 pandemic, the Other sector counted for 30% of dividend suspensions (66 of 219) while Retail Stores (31) and Textiles, Apparel and Footwear (11) took up another 20% combined. Conversely, Financial firms (29) counted for less than 15% - a sharp reduction from the Great Recession. Oil and Petroleum Products, Machinery and Business Equipment, and Automobiles each counted for at least 10 dividend suspensions during the Covid pandemic.

[11]The "Other" category includes many service firms, e.g., hotels such as Hilton, Marriott, and Choice Hotels and gambling/entertainment/casinos such as Las Vegas Sands and Boyd Gaming Corporation.

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**Table 1: Number of dividend/buyback suspensions and bonds/equity issues by year and industry.** This table reports the total number of dividend and buyback suspensions, together with the number of bond and equity issues during the Global Financial Crisis (2008-2009) and the Covid-19 crisis (2020), broken down by industry. We use the SIC codes and the Fama-French 17 industry definitions to classify companies into the various industries.

| Dividend and Buyback Suspensions, Bonds and Equity Issues | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Dividends | | Buybacks | | Bonds | | Equity | |
| Industry | 2008-09 | 2020 | 2008-09 | 2020 | 2008-09 | 2020 | 2008-09 | 2020 |
| Food | 2 | 3 | 3 | 3 | 47 | 32 | 8 | 11 |
| Mining and Minerals | 1 | 5 | 0 | 0 | 18 | 21 | 45 | 17 |
| Oil and Petroleum Products | 1 | 14 | 3 | 12 | 134 | 39 | 74 | 16 |
| Textiles, Apparel and Footwear | 1 | 11 | 0 | 12 | 5 | 11 | 5 | 0 |
| Consumer Durables | 8 | 5 | 3 | 5 | 15 | 4 | 6 | 10 |
| Chemicals | 1 | 5 | 1 | 0 | 28 | 14 | 21 | 14 |
| Drugs, Soap, Perfumes, Tobacco | 0 | 3 | 1 | 3 | 102 | 45 | 152 | 74 |
| Construction and Construction Materials | 5 | 2 | 0 | 6 | 36 | 27 | 16 | 7 |
| Steel Works Etc | 1 | 2 | 1 | 0 | 14 | 9 | 8 | 1 |
| Fabricated Products | 0 | 2 | 0 | 3 | 8 | 4 | 2 | 1 |
| Machinery and Business Equipment | 5 | 15 | 13 | 27 | 99 | 67 | 77 | 39 |
| Automobiles | 13 | 11 | 2 | 9 | 19 | 29 | 9 | 6 |
| Transportation | 4 | 14 | 2 | 11 | 67 | 55 | 27 | 25 |
| Utilities | 0 | 1 | 1 | 0 | 140 | 65 | 63 | 26 |
| Retail Stores | 9 | 31 | 6 | 33 | 59 | 33 | 12 | 21 |
| Banks, Insurance Companies, and Other Financials | 63 | 29 | 24 | 64 | 588 | 366 | 429 | 118 |
| Other | 21 | 66 | 18 | 74 | 473 | 386 | 1,072 | 751 |
| Total | 135 | 219 | 78 | 262 | 1,852 | 1,207 | 2,026 | 1,137 |

## 2.4 Buyback Suspensions

Buyback suspensions (Figure 3, lower panel) follow a similar pattern to dividend stops with few cases – typically less than three – in any given month. During the Great Recession, buyback suspensions peak at 15 in October of 2008 before gradually tapering off and reaching normal levels in early 2009. Compared with the Great Recession, buyback suspensions picked up far more rapidly and were more concentrated in time during the Covid pandemic: suspensions peak at over 130 in March 2020, followed by 91 in April and another 26 in May. Hence, more firms (248) suspended buybacks than suspended dividends (191) during the turbulent first three months of the pandemic (March-May, 2020). By August, buyback suspension numbers were back to normal.

In total, buyback suspensions tripled in numbers during the Covid pandemic relative to the Great Recession (262 versus 78). Banks, Insurance Companies and Other Financials, Other, and Machinery and Business Equipment lead the industries with the highest number of buyback suspensions during both crises (columns 3 and 4 in Table 1). In addition, 33 firms in the Retail Stores industry suspended buybacks during the pandemic - far more than during the Great Recession (6).

Reliably estimating how much money firms saved by suspending their share repurchase programs during the pandemic is difficult. Many programs do not commit firms to buy back shares on a particular schedule and some firms might simply have chosen to let their share repurchase programs lapse without formally announcing their suspension. Bearing this caveat in mind, if we assume that the buyback suspenders would have carried out the same amount of share repurchases during 2020 as they did in 2019, we would have expected an additional $140bn of net buybacks in 2020, including $16bn in April, $22.5bn in July, and $33bn in October.

## 2.5 Bond and Equity Issues

We collect data on bond and seasoned equity issued by U.S. domiciled firms from SDC Platinum. Our bond data includes convertible and non-convertible bonds, and MTN programs. We also collect information on the specific bond rating from Moody's, and

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global USD proceeds from the bond sales. Our equity dataset includes new issues of common/ordinary and preferred shares, and equity rights.[12] We require firms to have valid tickers in order to match them with CRSP/COMPUSTAT data.[13]

We begin by inspecting bond and equity issues by month. The top panels in Figure 5 show the total dollar amount raised from bond (top left panel) and equity (top right panel) issues between January 2005 and December 2020. In the aftermath of the pandemic outbreak, the total dollar value of bond issues rose sharply, peaking at $230bn in March and April of 2020 and exceeding $200bn in May. These values are, by some distance, the largest monthly dollar values raised in the bond market during our entire 16-year sample. While the dollar value of equity issues also rose significantly in May and June of 2020, their peak is not nearly as large in absolute terms (less than $50bn each month) or relative to earlier months in our sample.

Zooming in on the events during 2020, the plots of weekly bond and equity issues shown in the bottom panels of Figure 5 reveal that the corporate bond market came to a near-standstill in late February and the first two weeks of March before bond issues came roaring back to $60bn during the week of March 15, increasing further to more than $80bn per week in the last two weeks of March and early April.

US equity markets experienced even stronger disruption during the pandemic as very few companies issued stocks between the first reported Covid-related death in the US in mid-February and mid-April. In mid-May the equity market began to thaw with almost $20bn raised during the week of May 10. Elevated activity in the market for equity issues lasted for another six weeks until the end of June before falling back to its pre-Covid level.

Corporate bond markets recovered faster than equity markets following the decisive interventions of the Federal Reserve. Still, the pandemic had a longer-lasting impact on the ratings composition of bond issues. To see this, Figure 6 plots, for each week in 2020 and, for comparison, 2019, the fraction of bond issues using four categories of Moody's ratings, namely Prime and high grade, upper medium grade, lower medium grade, and non-investment grade. During the three weeks starting on March 8, 2020, bond issues

[12] We exclude IPOs from our sample.

[13] We exclude Freddie Mac and Fannie Mae from the bond issuers as they are outliers.

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rated at or above upper medium grade accounted for 60-80% of all bond issues. Conversely, non-investment grade issues accounted for less than 10% and, during the last two weeks of March, zero, and remained low until mid-April. For the remainder of the year, non-investment grade issues picked up in volume, averaging roughly 30% of all bond issues compared to 25% during 2019. Lower medium grade issues also accounted for a larger fraction of corporate bond issues: from March through December, 2020, these bonds accounted for 40% compared to 30% during the same period in 2019.

These figures show that during the period from mid-March to mid-April, the market for corporate bond issues was almost entirely limited to bonds rated at or above upper medium grade. Conversely, the market for non-investment grade bonds froze during the early stage of the pandemic outbreak (March - mid April). In common with the market for lower medium grade bonds, the non-investment grade bond segment bounced back markedly from mid-April onward. This followed a sequence of massive policy interventions taken by the Federal Reserve system and the US government.

Table 1 show that the Other sector accounted for an outsized proportion of bond and equity issues - nearly 70% of all equity issues - in 2020, reflecting the need for new capital for firms in the service sector. Conversely, Banks, Insurance Companies and Other Financials accessed equity market far less in 2020 than during the Great Recession.

Overall, our findings show that the bond market played a far more important role than the stock market for firms' ability to raise cash from capital markets during the pandemic. This is consistent with the sharp fall in equity prices during the early phase of the pandemic which made it more difficult–and costlier–for firms to tap into this source of financing. It is also consistent with the Federal Reserve's efforts targeting the purchase of investment-grade corporate bonds.[14]

[14]For 2020 as a whole, the companies in our sample issued $1.996tn of new bonds compared to $1.049tn in 2019 (a 90% increase). They raised another $245bn of equity in the form of ordinary and preferred shares and right issues compared to $125bn in 2019 (a 95% increase).

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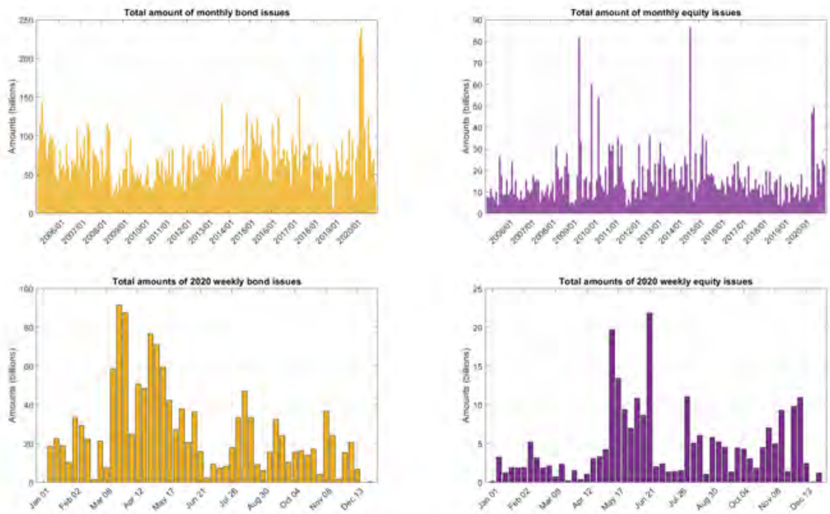


**Figure 5: Total amounts of bonds and equity issues.** The top panels plot the nominal dollar amounts of bond and equity issues summed across all firms in our final data for each month between January 2005 to December 2020. The bottom panels show the weekly breakdown of the total dollar amounts of bonds and equity issues during 2020.

## 2.6 Summary of Corporate Actions During the Pandemic

To summarize our findings in this section, Figure 7 plots the total number of weekly corporate announcements by type along with markers for some of the main events related to the pandemic and the policy responses it triggered as noted earlier. We see a sharp uptick in the number of buyback and dividend suspensions during the three weeks that include the initial round of policy interventions by the Federal Reserve and Congress.

Accounting for all four corporate actions, activity levels remain elevated well into mid-June. The unusually large number of bond and equity issues between August 3 and 16 followed the announcement by the Federal Reserve Board on July 28 that it had extended its lending facilities through December 31st.[15]

[15] These facilities had previously been scheduled to expire on September 30.

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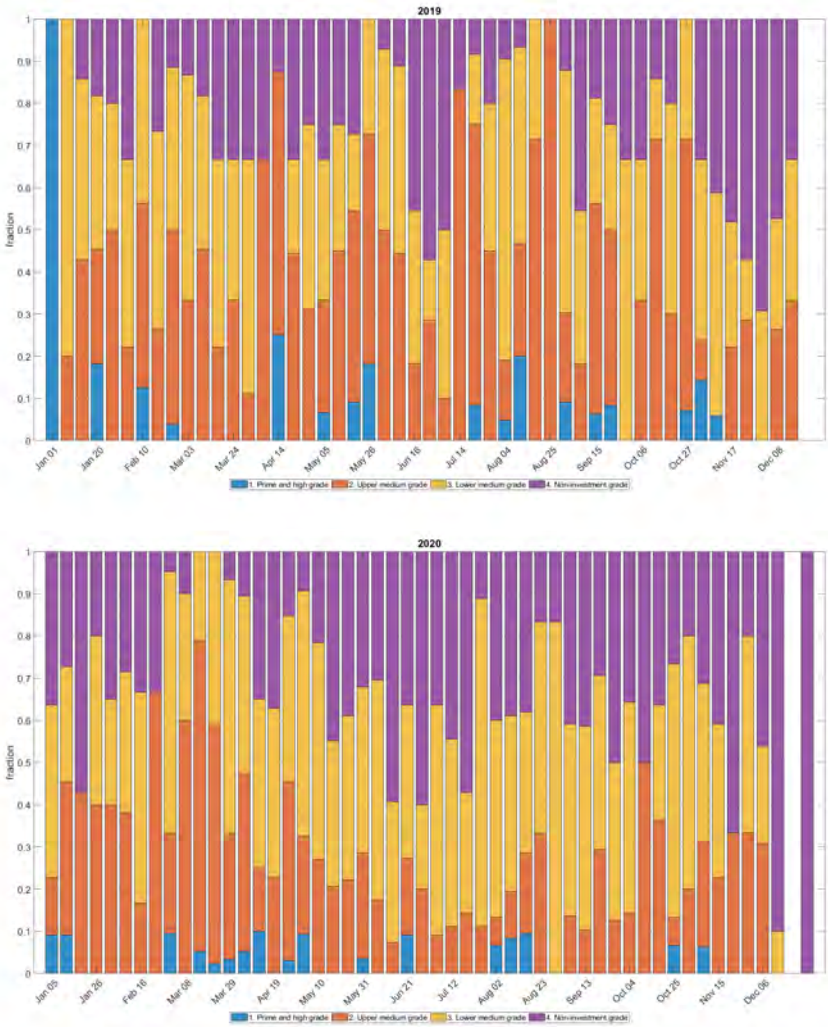


**Figure 6: Weekly bond issues by rating.** This figure shows the rating breakdown of all bond issues for all weeks of 2019 (top panel) and 2020 (bottom panel). Rating categories are based on Moody's Investors Service's ratings system and are defined as follows: (1) Prime and high grade bonds include bond issues with ratings Aa3 and above; (2) Upper medium grade includes all bond issues with ratings between A1 and A3; (3) Lower medium grade includes all issues with ratings between Baa3 and Baa1; (4) Non-investment grade includes all bond issues rated Ba1 or below.

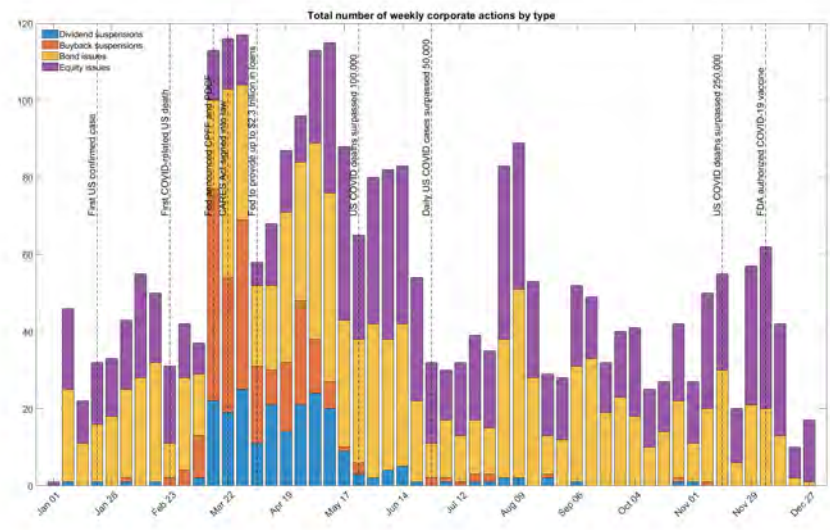


**Figure 7: Corporate actions by week in 2020.** This figure plots the total number of dividend suspensions, buyback suspensions, and equity and bond issuance by all public firms for each week between January 2020 and December 2020.

# 3 Payout Suspensions, Financing Decisions, and Firm Characteristics

To better understand what drove some firms to suspend dividends and buybacks during the pandemic - and raise funds by issuing bonds or stocks - while others continued with their payouts or chose not to tap into capital markets, we next study which firm and stock characteristics help explain corporate decisions.

Studies such as Fama and French (2001) and Hoberg and Prabhala (2008) analyze the drivers of the long-term trend away from firms paying dividends. For example, Fama and French (2001) identify variation in profitability, size, and investment opportunities as important determinants of dividend stops. Hoberg and Prabhala (2008) consider idiosyncratic and systematic risk measures estimated from stock returns, both of which are strongly correlated with firms' propensity to stop dividend payments.[16]

[16] Hoberg and Prabhala (2008) find that the two risk measures explain around 40% of the trend variation in firms' propensity to pay dividends between 1978 and 1999, a finding that is only strengthened by including a post-1983 dummy for the introduction of safe harbor provisions which increased firms' share repurchases.

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The objective of our analysis here is instead to identify which characteristics led firms to suspend their payouts in the immediate aftermath of the pandemic outbreak and, for comparison, during the 2008-09 Global Recession.

### 3.1 Regression Model

We start by listing the set of covariates used to explain variation in firms' decisions to suspend dividend payments and share repurchase programs. Following Fama and French (2001), Hoberg and Prabhala (2008), and Ding et al. (2021), we consider several variables that measure the financial conditions of firms such as firm size (market capitalization), leverage, cash holdings, profitability, changes in revenues, idiosyncratic stock returns, and idiosyncratic return volatility.

We add to this list a "prior corporate action" dummy which takes a value of unity if a firm has taken one or more corporate actions prior to taking the corporate action under examination and otherwise is zero. For example, in the case of dividend suspensions, this indicator would equal one if, prior to the announcement of its dividend suspension, a firm had previously announced it had suspended its buyback program or issued bonds or equity during the period under study (e.g., in 2020). The idea is to examine whether suspending dividends is more or less likely if a firm has preserved capital or raised new funds through its previous corporate actions.

To determine which of the variables on our list explain companies' actions, we estimate a set of cross-sectional Probit regressions. Specifically, define the indicator variable $A_{it} = 1$ if, in period $t$, company $i$ took some action $A \in \mathcal{A} = \{$dividend stop, buyback stop, bond issue, share issue$\}$; otherwise $A_{it} = 0$. Using this definition, our quarterly-frequency Probit models take the following form:

$$\begin{aligned} \Pr(A_{it}) &= \alpha + \beta_1 size_{it} + \beta_2 lev_{it} + \beta_3 cash_{it} + \beta_4 ROA_{it} + \beta_5 \Delta rev_{it} \\ &\quad + \beta_6 \widehat{\sigma}_{it} + \beta_7 \overline{ret}_{it} + \beta_8 \sum_{A' \in \mathcal{A}, A' \neq A} A'_{it'} + \sum_{j=1}^{17} \lambda_j Ind_{ijt} + \varepsilon_{it}. \end{aligned} \quad (1)$$

As in Fama and French (2001), firm size ($size_{it}$) is defined as the quintile of the natural logarithm of the market value of equity, leverage ($lev_{it}$) is computed as the long-term

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debt (DLTTQ) plus debt in current liabilities (DLCQ) divided by assets (ATQ), cash holdings ($cash_{it}$) is the sum of actual cash and short-term investments (CHEQ) divided by total assets (ATQ). Return-on-assets ($ROA_{it}$) is the ratio of net income (NIY) to total assets (ATQ). Growth in revenues, $\Delta rev_{it}$ is the year-on-year change in quarterly revenues. All quarterly accounting variables are calculated as of Q1 2020 (Q1 2008) for the non-suspenders, and as of the same calendar quarter of the suspension for the suspenders in 2020 (2008).[17]

Idiosyncratic volatility of firm-level returns, $\widehat{\sigma}_{it}$, is computed from a three-factor Fama-French model and estimated using a 30-day window preceding the announcement date for the corporate action. Similarly, $\overline{ret}_{it}$ measures the cumulative value of firm $i$'s idiosyncratic returns, again based on the three-factor model and cumulated over the preceding 30-day window prior to the announcement date. The prior corporate action dummy equals unity if firm $i$ took another corporate action $A'$ on a prior date $t_i' < t_i$ during the event window, where $t_i$ is the announcement date for $A_{it}$. Finally, $Ind_{ijt}$ is a set of 17 industry fixed effects (dummies) that control for variation in the propensity for corporate actions across industries. All continuous variables have been normalized by scaling their values by their standard deviation so coefficient estimates are comparable across covariates.

## 3.2 Dividend Suspensions

Table 2 shows empirical results for the Probit specification in equation (1) estimated with the dividend suspension indicator as the dependent variable. We estimate separate Probit models on two cross-sections of data covering the Great Recession 2008:01-2009:06 and the pandemic (2020). Industry fixed effects are always included but we do not report their estimates because they are insignificant for the vast majority of industries–the main exception being Oil and Petroleum Products which generates a significantly negative coefficient during the pandemic but not for 2008/09.

First consider the results for the Great Recession (columns 1-4). During this period,

[17]Our results are robust to lagging the accounting variables by one quarter since most of these are quite persistent.

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firm size was a highly significant negative predictor of dividend suspensions with small firms having a higher chance of suspending dividend payments than larger ones. Leverage had a significantly positive effect on firms' propensity to suspend dividends with highly levered firms more likely to suspend dividend payments. Cash holdings did not seem to matter for suspension probabilities, while profitability and revenue growth were significantly negatively correlated with the likelihood of dividend suspensions. Less profitable firms whose revenue growth were most adversely impaired were therefore more likely to have suspended their dividend payments during the Great Recession.

Turning to the two return-based variables, idiosyncratic return volatility was highly positively correlated with the likelihood of dividend suspensions with greater uncertainty about firm prospects translating into a higher chance of a dividend suspension. Similarly, 30-day cumulative idiosyncratic returns prior to the announcement date were strongly negatively correlated with the likelihood of a dividend suspension as recent underperformance in the stock market made it more likely that a firm would suspend its dividend payments.[18] Finally, the prior corporate action dummy has a highly significant and positive effect on the likelihood of a dividend suspension. Prior corporate actions would thus have raised the likelihood that a firm subsequently suspended its dividends during the Great Recession.

We conclude from these findings that small, unprofitable firms with high leverage, low or negative revenue growth, and uncertain prospects reflected in their stock market performance were more likely to have suspended their dividends during the Great Recession. Overall, our list of variables explains a sizeable part of the cross-sectional variation in firms' decisions to suspend dividends during the Great Recession with (pseudo) $R^2$-values around 60%.

Turning to the 2020 pandemic, a very different picture emerges: firm size and leverage are no longer statistically significant and cash holdings also remain insignificant. Profitability and revenue growth are the only accounting-based variables

[18] Because idiosyncratic volatility and cumulative returns are highly correlated, we include them in separate regressions instead of simultaneously.

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that retain significant (negative) associations with the likelihood of dividend suspensions. Although less profitable firms with low or negative revenue growth were more likely to suspend dividends during the pandemic, the slope coefficient on the ROA variable is only one-third of that observed for the Great Recession. In contrast, the estimated coefficient on idiosyncratic volatility nearly doubles compared to the Great Recession, and the coefficient on 30-day idiosyncratic returns is also substantially higher for the pandemic sample. The prior corporate action dummy retains its positive and significant coefficient.

The explanatory power of the Probit model that includes idiosyncratic volatility remains very high during the pandemic ($R^2$ of 63%) but is somewhat lower (54%) if we swap idiosyncratic volatility for cumulative idiosyncratic returns.

The columns labeled $\Delta Pr$, listed after the Probit estimates, provide an estimate of how much the probability of a dividend suspension changes as we move from the 10th to the 90th percentile value of each of the variables listed in the rows, keeping the remaining variables at their sample means.[19] For example, for the pandemic sample, moving from a firm with substantial negative revenue growth (10th percentile) to a firm with much larger growth (90th percentile) reduces the probability of a dividend suspension by 13.4%. This is a far bigger effect than seen for revenue growth during the Great Recession (0.7%). To put this into perspective, 19.3% of the firms in our 2020 sample suspended dividends, while only 8.1% of firms did so in 2008-09.

The $\Delta Pr$ estimates show that 30-day idiosyncratic return volatility and cumulative idiosyncratic returns were very powerful predictors of the likelihood of a dividend suspension during the pandemic: A shift from the 10th to the 90th percentile of the idiosyncratic volatility distribution is associated with a 49% increase in the dividend suspension probability, while the same shift for cumulative idiosyncratic returns (from a large negative value to a value near zero) reduces the dividend suspension probability by nearly 22% during the pandemic. The corresponding numbers for the Global Recession are only 2% and -1%, respectively. Hence stock market volatility and return

[19]The percentile distribution for each variable is generated separately for the Great Recession and pandemic periods.

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performance were far more powerful predictors of dividend suspension probabilities during the pandemic than during the Great Recession. Part of the reason for this difference is the bigger coefficient estimates on these variables during the pandemic; however, the main reason is the far greater differences in return performance experienced during the pandemic than during the Great Recession.

Firms' decisions to suspend dividends turn out to have predictive power over next-quarter revenue growth. Specifically, regressing next-quarter revenue growth during the pandemic on dividend and buyback suspension dummies along with bond and equity issues, both scaled by firm size, industry fixed effects, and time fixed effects, we find that the dividend suspension dummy obtains a highly significant, negative coefficient. Moreover, the effect is economically large as firms that suspended dividends saw their revenue growth decrease by an average of 35% the following quarter, compared to non-suspending firms. Hence, dividend suspensions appear to have been taken in correct anticipation of worsening future revenue growth.

To summarize, we find that characteristics such as firm size, leverage and cash holdings played no significant role in explaining cross-sectional differences in firms' decision to suspend dividends during the pandemic. Profitability and, in particular, revenue growth were important predictors of dividend suspensions. Short-term performance in the stock market, particularly return volatility which proxies for uncertainty about firms' future prospects, were also important predictors of which firms were more likely to suspend dividend payments during the pandemic. Revenue growth and short-term stock market performance had a much stronger ability to identify which firms suspended dividends during the pandemic compared to during the Great Recession.

### 3.3 Buyback Suspensions

Table 3 reports estimates for the Probit model fitted to buyback suspensions. For the Great Recession period (columns 1-4), firm size (positively) and profitability and revenue growth (both negatively) correlate significantly with suspension probabilities,

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**Table 2: Probit regressions of dividend suspenders on firm characteristics.** This table reports estimates of the cross-sectional probit regression $Prob(\text{dividend suspender})_{i,t} = \alpha + \beta_1 size_{i,t} + \beta_2 leverage_{i,t} + \beta_3 cash_{i,t} + \beta_4 ROA_{i,t} + \beta_5 \Delta revenues + \beta_6 30dayidiovol(cumulativeidiosyncraticret)_{i,t} + \beta_7 firstactiondummy + \varepsilon_{i,t}$ of dividend suspenders on firm characteristics. Firm size is defined as the quintile of the natural logarithm of the market value of equity as in Fama and French (2001). Leverage is calculated as Long term debt (DLTTQ) plus debt in current liabilities (DLCQ), divided by assets (ATQ). Cash is calculated as the sum of actual cash and short-term investments (CHEQ) divided by total assets (ATQ). Return-on-assets (ROA) is the ratio of net income (NIY) to total assets (ATQ). Δ revenues is the year-on-year, same quarter, change in revenues. 30-day idiosyncratic volatility and 30-day cumulative idiosyncratic return are calculated using the Fama-French three-factor models. The prior corporate action dummy is equal to one if the dividend suspension is not the first action in the year, and zero otherwise. Square brackets report $t$-statistics. ***, **, * indicate statistical significance at the 1%, 5% , 10% level, respectively. The sample period is 2008-2009 (2020), with firm characteristics as of the end of Q1 2008 (Q1 2020) for non-suspenders, and on the quarter of suspension in 2008-2009 (2020) for suspenders.

| Probit of dividend suspenders | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 2008-2009 | | | | | 2020 | | | | |
| | (1) | (2) | (3) | (4) | $\Delta Pr$ | (5) | (6) | (7) | (8) | $\Delta Pr$ |
| Firm size | -0.350** | -0.492*** | -0.397** | -0.539*** | -2.00% | 0.0174 | -0.0619 | -0.110 | -0.184* | -1.67% |
| | [-2.78] | [-3.45] | [-3.28] | [-3.93] | | [0.20] | [-0.68] | [-1.40] | [-2.25] | |
| Leverage | 0.734*** | 0.793*** | 0.748*** | 0.802*** | 1.11% | 0.113 | 0.125 | 0.134 | 0.142 | 2.16% |
| | [3.88] | [3.86] | [4.08] | [4.05] | | [0.85] | [0.93] | [1.12] | [1.18] | |
| Cash | -0.383 | -0.442 | -0.362 | -0.414 | -0.64% | 0.0923 | 0.101 | 0.0619 | 0.0685 | 1.92% |
| | [-1.85] | [-1.81] | [-1.82] | [-1.78] | | [0.83] | [0.87] | [0.60] | [0.63] | |
| ROA | -1.913*** | -1.830** | -2.063*** | -2.008*** | -0.52% | -0.609* | -0.552 | -0.788** | -0.744* | -3.04% |
| | [-3.77] | [-3.26] | [-4.11] | [-3.66] | | [-2.11] | [-1.91] | [-2.71] | [-2.53] | |
| Δ revenues | -0.555*** | -0.645*** | -0.564*** | -0.638*** | -0.72% | -0.697*** | -0.673*** | -0.798*** | -0.769*** | -13.40% |
| | [-4.15] | [-4.43] | [-4.32] | [-4.56] | | [-5.77] | [-5.58] | [-6.81] | [-6.63] | |
| 30-day idiosyncratic vol | 1.009*** | 1.081*** | | | 1.96% | 1.780*** | 1.738*** | | | 49.38% |
| | [5.18] | [4.91] | | | | [11.57] | [11.15] | | | |
| 30-day cumulative idiosyncratic returns | | | -0.576*** | -0.593*** | -0.97% | | | -0.961*** | -0.918*** | -21.89% |
| | | | [-4.61] | [-4.33] | | | | [-9.14] | [-8.72] | |
| Prior corporate action dummy | | 0.983*** | | 0.904*** | | | 0.830*** | | 0.879*** | |
| | | [3.65] | | [3.55] | | | [3.29] | | [3.73] | |
| Industry FE | Y | Y | Y | Y | | Y | Y | Y | Y | |
| Pseudo $R^2$ | 60.44% | 63.97% | 58.51% | 61.79% | | 62.14% | 63.59% | 53.56% | 55.50% | |
| Observations | 756 | 756 | 756 | 756 | | 901 | 901 | 901 | 901 | |

while leverage and cash holdings are both insignificant. The estimated coefficient on firm size has switched from negative for dividend suspensions to positive for buyback suspensions. While small firms were more likely to have suspended their dividends, the largest firms were instead more likely to have suspended their share repurchase programs.

Idiosyncratic return volatility and cumulative idiosyncratic returns are both strong predictors of buyback suspensions. As expected, firms whose returns in the 30-day period leading up to the suspension date were either highly volatile or very low had a significantly higher chance of suspending their share repurchase programs. Without the prior corporate action dummy included, our list of regressors has lower explanatory power over buyback suspensions ($R^2$ of 23-25%) than over dividend suspensions. Adding this dummy increases the explanatory power to 40%, suggesting that prior corporate actions made it far more likely that firms would suspend their buybacks during the Great Recession.

During the pandemic (columns 5-8), firm size obtains a significantly positive coefficient in explaining buyback suspensions, with larger firms again more likely to suspend buybacks than smaller ones. Leverage, cash holdings, and profitability are not significant drivers of firms' propensity to suspend buybacks. However, revenue growth is even more important in explaining buyback suspensions during the pandemic, with coefficients that are about 50% larger than for the Great Recession sample. 30-day prior idiosyncratic return volatility obtains a highly significant, positive coefficient and cumulative returns a significantly negative coefficient. The estimated coefficients of both return-based measures are at least twice as large for the pandemic sample as for the Great Recession, highlighting how return performance became an even stronger predictor of the likelihood of buyback suspensions during the pandemic.

The prior corporate action dummy obtains a very large positive and highly significant coefficient that is far greater for the 2020 pandemic sample than for the Great Recession. Without the prior corporate dummy action included, the (pseudo) $R^2$ is 43% for the model that includes idiosyncratic return volatility as a predictor and 33% for the model that instead includes cumulative idiosyncratic returns. These values rise to 81%

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and 80%, respectively, once the prior corporate action dummy is included, consistent with this variable being an important predictor of firms' decisions to suspend share repurchases.

The $\Delta Pr$ columns show that firm size was a strong differentiator of buyback suspensions, particularly during the Great Recession where a large firm ranked in the 90th size percentile was nearly 5% more likely to suspend its share repurchases than a small firm ranked in the 10th size percentile. Since 14.8% of firms suspended buybacks in our 2020 sample while only 6.2% did so in 2008-09, a 5% difference in the 2008-09 suspension probability is clearly a large effect. The second most important predictor of buyback suspension probabilities is 30-day idiosyncratic volatility, although its effect on buyback suspensions in 2020 is much smaller than that seen for dividend suspensions.

To summarize, large firms whose revenues dropped sharply and whose stock market performance indicated highly uncertain prospects were far more likely to suspend their buyback programs during the pandemic, particularly if they had previously suspended dividends or issued shares or bonds.

## 3.4 Bond and Share Issues

We finally consider Probit regressions fitted to the indicators tracking if firms issued bonds or equity at least once during a particular sample. First consider the determinants of firms' decisions to issue bonds (Table 4). During both the Great Recession (columns 1-4) and the pandemic (columns 5-8), large firms with high leverage, low profitability and negative revenue growth were significantly more likely to have issued bonds than their counterparts.

Higher idiosyncratic return volatility and lower cumulative idiosyncratic returns, both measured over the 30-day period prior to the bond issue, are highly significant predictors of firms' decision to issue bonds. Prior corporate actions also made a bond issue more likely. Interestingly, in the models that exclude the prior action dummy, the estimated coefficient on cash holdings is negative and marginally significant. This is consistent with larger cash holdings reducing the need for issuing bonds, particularly during the

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**Table 3: Probit regressions of buyback suspenders on firm characteristics.** This table reports estimates of the cross-sectional probit regression $Prob(\text{buyback suspender})_{i,t} = \alpha + \beta_1 size_{i,t} + \beta_2 leverage_{i,t} + \beta_3 cash_{i,t} + \beta_4 ROA_{i,t} + \beta_5 \Delta revenues + \beta_6 30daysidiovol(cumulativeidiosyncraticret)_{i,t} + \beta_7 firstactiondummy + \varepsilon_{i,t}$ of buyback suspenders on firm characteristics. Firm size is defined as the quintile of the natural logarithm of the market value of equity as in Fama and French (2001). Leverage is calculated as Long term debt (DLTTQ) plus debt in current liabilities (DLCQ), divided by assets (ATQ). Cash is calculated as the sum of actual cash and short-term investments (CHEQ) divided by total assets (ATQ). Return-on-assets (ROA) is the ratio of net income (NIY) to total assets (ATQ). Δ revenues is the year-on-year, same quarter, change in revenues. The 30-days idiosyncratic volatility and 30-days cumulative idiosyncratic return are calculated using the Fama-French 3 factors models. Prior corporate action dummy is equal to one if the buyback suspension is not the first action in the year, and zero otherwise. Square brackets report *t*-statistics. ***, **, * indicate statistical significance at the 1%, 5% , 10% level, respectively. The sample period is 2008-2009 (2020), with firm characteristics as of the end of Q1 2008 (Q1 2020) for non-suspenders, and on the quarter of suspension in 2008-2009 (2020) for suspenders.

| | Probit of buyback suspenders | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 2008-2009 | | | | | 2020 | | | | |
| | (1) | (2) | (3) | (4) | $\Delta Pr$ | (5) | (6) | (7) | (8) | $\Delta Pr$ |
| Firm size | 0.726*** | 0.705*** | 0.606*** | 0.631*** | 4.68% | 0.664*** | 0.504*** | 0.492*** | 0.396** | 1.60% |
| | [4.72] | [3.93] | [4.17] | [3.62] | | [8.50] | [3.61] | [6.99] | [2.98] | |
| Leverage | -0.0878 | -0.0640 | -0.0583 | -0.0403 | -0.11% | -0.150 | -0.234 | -0.0672 | -0.180 | -0.36% |
| | [3.88] | [3.86] | [4.08] | [4.05] | | [0.85] | [0.93] | [1.12] | [1.18] | |
| Cash | -0.321* | -0.154 | -0.298* | -0.138 | -0.90% | -0.0906 | 0.216 | -0.0633 | 0.216 | 0.43% |
| | [-2.27] | [-1.02] | [-2.14] | [-0.92] | | [-1.21] | [1.66] | [-0.89] | [1.69] | |
| ROA | -0.970** | -1.457*** | -0.975** | -1.486*** | -1.44% | 0.258 | -0.716* | 0.253 | -0.704* | -0.35% |
| | [-2.80] | [-3.51] | [-2.82] | [-3.57] | | [1.44] | [-2.16] | [1.44] | [-2.16] | |
| Δ revenues | -0.443*** | -0.369** | -0.471*** | -0.393** | -1.27% | -0.641*** | -0.594*** | -0.662*** | -0.598*** | -0.89% |
| | [-3.60] | [-2.63] | [-3.94] | [-2.86] | | [-6.21] | [-3.59] | [-6.68] | [-3.63] | |
| 30-days idiosyncratic vol | 0.929*** | 0.656** | | | 2.38% | 1.898*** | 1.378*** | | | 3.39% |
| | [5.04] | [3.06] | | | | [13.50] | [5.57] | | | |
| 30-days cumulative idiosyncratic returns | | | -0.484*** | -0.335* | -0.90% | | | -1.257*** | -0.868*** | -1.26% |
| | | | [-4.01] | [-2.33] | | | | [-10.91] | [-4.52] | |
| Prior corporate action dummy | | 1.410*** | | 1.473*** | | | 3.634*** | | 3.695*** | |
| | | [6.12] | | [6.50] | | | [10.49] | | [11.26] | |
| Industry FE | Y | Y | Y | Y | | Y | Y | Y | Y | |
| Pseudo $R^2$ | 27.27% | 41.45% | 23.77% | 40.10% | | 43.42% | 80.57% | 33.85% | 79.09% | |
| Observations | 657 | 657 | 657 | 657 | | 1,176 | 1,176 | 1,176 | 1,176 | |

pandemic (Fahlenbrach et al. (2020)).

The explanatory power of our list of variables is around 40% during the pandemic for the models that do not include the prior corporate action dummy. Adding this dummy increases the $R^2$ to 47%. For the Great Recession, the explanatory power is marginally lower.

The $\Delta Pr$ columns show that the probability of issuing bonds was far more sensitive to firm and stock characteristics during the Great Recession than during the pandemic. For example, in 2008-09 a firm in the 90th percentile of the idiosyncratic volatility distribution had a 22% higher chance of issuing bonds than a firm in the 10th percentile compared to an incremental effect of 7% in 2020.[20]

Turning to the Probit estimates for firms' equity issues (Table 5), we find that large firms with big cash holdings and low profitability were more likely than their peers to have issued equity during the Great Recession and Covid pandemic. Interestingly, leverage and revenue growth are not significant in any of the specifications for equity issues. This is in sharp contrast to what we found for bond issues and suggests that firms that were highly levered and experienced low (negative) revenue growth during the two crises were relatively more likely to raise funds by issuing bonds rather than equity.

The estimates for cash holdings suggest that firms with the *largest* holdings of short-term reserves were *more* likely to issue equity. A possible explanation of this somewhat counter intuitive finding is that the larger cash holdings created a short-term buffer which made it possible for such firms to tap into the equity market without seeing a strongly adverse effect on their stock market valuation. When combined with the negative estimate of cash holdings on bond issuance in the previous table, our estimates show that firms with larger cash holdings were more likely to have raised funds by issuing equity rather than bonds compared to firms with small cash holdings.

High idiosyncratic return volatility and negative cumulative idiosyncratic returns during the 30-day period preceding an equity issue are highly significant predictors of the likelihood that a firm will issue equity during the two crises. Firms with highly volatile returns (in the 90th percentile of the idiosyncratic volatility distribution) were

[20]Overall, 30.6% and 19.3% of firms in our sample issued bonds in 2008-09 and 2020, respectively.

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13% more likely to issue equity in 2008-09 than firms with less volatile returns in the 10th percentile. The corresponding figure is 6% in 2020.[21]

Without the prior corporate action dummy included, the $R^2$ for the Probit model fitted to equity issues is around 32% during our 2020 sample and about 10% lower for the Great Recession. Including this dummy increases the $R^2$ value by 10-12 percentage points indicating that, as in the earlier cases, a prior corporate action made it more likely that a firm would follow up with another action.

In summary, these results suggest both similarities and important differences in the determinants of firms' decisions to issue bonds or equity during the pandemic. Large firms with low profitability were more likely to issue either bonds or equity. Similarly, high idiosyncratic return volatility or large negative idiosyncratic returns in the preceding 30-day period significantly raised the probability that a firm would subsequently issue bonds or equity as did the existence of a prior corporate action.

Conversely, whereas highly levered firms with low (negative) revenue growth were more likely to issue bonds, these factors do not seem to have played an important role for firms' decisions to issue equity during the pandemic. Firms with large cash holdings, on the other hand, were more likely to issue equity while short term cash reserves did not correlate with the decision to issue bonds.

## 4 Sequencing of Firms' Actions

Theories of firms' optimal choice of capital structure have testable implications for the sequence in which firms use access to internal sources of capital versus tap into bond or equity markets. For example, the Myers and Majluf (1984) pecking order theory stipulates that firms' choice of which financing sources to use follows a hierarchical ordering. The theory holds that the adverse selection costs from issuing equity (net of any benefits) are sufficiently large that they dominate the costs from other funding sources. In particular,

[21]Overall, 17% and 12% of firms in our sample issued equity in 2008-09 and 2020, respectively.

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**Table 4: Probit regressions of bond issues on firm characteristics.** This table reports estimates of the cross-sectional probit regression $Prob(\text{bond issue})_{i,t} = \alpha + \beta_1 size_{i,t} + \beta_2 leverage_{i,t} + \beta_3 cash_{i,t} + \beta_4 ROA_{i,t} + \beta_5 \Delta revenues + \beta_6 30dayidiovol(cumulativeidiosyncraticret)_{i,t} + \beta_7 firstactiondummy + \varepsilon_{i,t}$ of bond issues on firm characteristics. Firm size is defined as the quintile of the natural logarithm of the market value of equity as in Fama and French (2001). Leverage is calculated as Long term debt (DLTTQ) plus debt in current liabilities (DLCQ), divided by assets (ATQ). Cash is calculated as the sum of actual cash and short-term investments (CHEQ) divided by total assets (ATQ). Return-on-assets (ROA) is the ratio of net income (NIY) to total assets (ATQ). Δ revenues is the year-on-year, same quarter, change in revenues. 30-day idiosyncratic volatility and 30-day cumulative idiosyncratic return are calculated using the Fama-French three-factor models. Prior corporate action dummy is equal to one if the bond issue is not the first action in the year, and zero otherwise. Square brackets report $t$-statistics. ***, **, * indicate statistical significance at the 1%, 5% , 10% level, respectively. The sample period is 2008-2009 (2020), with firm characteristics as of the end of Q1 2008 (Q1 2020) for non-issuers, and on the quarter of the first bond issue in 2008-2009 (2020) for bond issuers.

| | Probit of bond issue | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 2008-2009 | | | | | 2020 | | | | |
| | (1) | (2) | (3) | (4) | $\Delta Pr$ | (5) | (6) | (7) | (8) | $\Delta Pr$ |
| Firm size | 1.134*** | 1.072*** | 1.033*** | 0.971*** | 59.15% | 1.156*** | 1.052*** | 1.175*** | 1.070*** | 45.87% |
| | [22.52] | [20.34] | [21.86] | [19.67] | | [20.05] | [18.05] | [19.79] | [17.93] | |
| Leverage | 0.236*** | 0.268*** | 0.245*** | 0.277*** | 6.84% | 0.143** | 0.117* | 0.152** | 0.125* | 2.08% |
| | [5.19] | [5.59] | [5.43] | [5.83] | | [2.81] | [2.24] | [2.98] | [2.40] | |
| Cash | -0.0934* | -0.0433 | -0.0852* | -0.0371 | -2.95% | -0.156** | -0.0797 | -0.147** | -0.0762 | -2.70% |
| | [-2.36] | [-1.02] | [-2.17] | [-0.89] | | [-3.20] | [-1.54] | [-3.04] | [-1.48] | |
| ROA | -0.396*** | -0.429*** | -0.409*** | -0.441*** | -6.03% | -0.223* | -0.306** | -0.210* | -0.266** | -3.17% |
| | [-6.47] | [-6.48] | [-6.75] | [-6.76] | | [-2.41] | [-3.05] | [-2.25] | [-2.63] | |
| Δ revenues | -0.189*** | -0.177*** | -0.199*** | -0.191*** | -5.44% | -0.0843** | -0.0748* | -0.0882** | -0.0808* | -1.14% |
| | [-6.19] | [-5.47] | [-6.57] | [-5.92] | | [-2.73] | [-2.13] | [-2.90] | [-2.34] | |
| 30-day idiosyncratic vol | 0.701*** | 0.682*** | | | 21.58% | 0.404*** | 0.384*** | | | 6.55% |
| | [12.89] | [12.02] | | | | [10.14] | [9.45] | | | |
| 30-day cumulative idiosyncratic returns | | | -0.550*** | -0.526*** | -10.04% | | | -0.512*** | -0.539*** | -4.94% |
| | | | [-10.32] | [-9.58] | | | | [-10.03] | [-10.19] | |
| Prior corporate action dummy | | 1.403*** | | 1.393*** | | | 1.276*** | | 1.309*** | |
| | | [15.80] | | [15.97] | | | [10.96] | | [11.26] | |
| Industry FE | Y | Y | Y | Y | | Y | Y | Y | Y | |
| Pseudo $R^2$ | 37.12% | 46.42% | 35.34% | 44.82% | | 40.95% | 47.79% | 40.14% | 47.37% | |
| Observations | 2,730 | 2,730 | 2,730 | 2,730 | | 2,280 | 2,280 | 2,280 | 2,280 | |

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**Table 5: Probit regressions of equity issues on firm characteristics.** This table reports estimates of the cross-sectional probit regression $Prob(\text{equity issue})_{i,t} = \alpha + \beta_1 size_{i,t} + \beta_2 leverage_{i,t} + \beta_3 cash_{i,t} + \beta_4 ROA_{i,t} + \beta_5 \Delta revenues + \beta_6 30dayidiovol(cumulativeidiosyncraticret)_{i,t} + \beta_7 firstactiondummy + \varepsilon_{i,t}$ of equity issues on firm characteristics. Firm size is defined as the quintile of the natural logarithm of the market value of equity as in Fama and French (2001). Leverage is calculated as Long term debt (DLTTQ) plus debt in current liabilities (DLCQ), divided by assets (ATQ). Cash is calculated as the sum of actual cash and short-term investments (CHEQ) divided by total assets (ATQ). Return-on-assets (ROA) is the ratio of net income (NIY) to total assets (ATQ). Δ revenues is the year-on-year, same quarter, change in revenues. 30-day idiosyncratic volatility and 30-day cumulative idiosyncratic return are calculated using the Fama-French 3 factors models. Prior corporate action dummy is equal to one if the equity issue is not the first action in the year, and zero otherwise. Square brackets report $t$-statistics. ***, **, * indicate statistical significance at the 1%, 5% , 10% level, respectively. The sample period is 2008-2009 (2020), with firm characteristics as of the end of Q1 2008 (Q1 2020) for non-issuers, and on the quarter of the first equity issue in 2008-2009 (2020) for equity issuers.

| | Probit of equity issue | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 2008-2009 | | | | | 2020 | | | | |
| | (1) | (2) | (3) | (4) | $\Delta Pr$ | (5) | (6) | (7) | (8) | $\Delta Pr$ |
| Firm size | 0.341*** | 0.374*** | 0.256*** | 0.297*** | 15.75% | 0.253*** | 0.281*** | 0.220*** | 0.242*** | 7.31% |
| | [7.92] | [8.02] | [6.28] | [6.69] | | [5.07] | [5.13] | [4.48] | [4.52] | |
| Leverage | 0.0570 | 0.116* | 0.0699 | 0.126* | 2.26% | -0.121 | -0.0804 | -0.118 | -0.0776 | -1.04% |
| | [1.13] | [2.13] | [1.41] | [2.34] | | [-1.66] | [-1.00] | [-1.63] | [-0.99] | |
| Cash | 0.109** | 0.108** | 0.113*** | 0.113** | 4.79% | 0.345*** | 0.353*** | 0.348*** | 0.355*** | 11.68% |
| | [3.17] | [2.93] | [3.34] | [3.10] | | [8.48] | [8.08] | [8.62] | [8.21] | |
| ROA | -0.501*** | -0.500*** | -0.526*** | -0.522*** | -5.74% | -0.402*** | -0.405*** | -0.421*** | -0.427*** | -3.17% |
| | [-8.51] | [-7.86] | [-9.01] | [-8.26] | | [-5.52] | [-5.14] | [-5.76] | [-5.45] | |
| Δ revenues | -0.0225 | -0.00530 | -0.0232 | -0.00771 | -0.11% | 0.00287 | 0.0138 | 0.000838 | 0.0101 | 0.17% |
| | [-0.89] | [-0.19] | [-0.92] | [-0.28] | | [0.11] | [0.48] | [0.03] | [0.35] | |
| 30-day idiosyncratic vol | 0.616*** | 0.618*** | | | 12.72% | 0.412*** | 0.435*** | | | 6.25% |
| | [12.70] | [11.98] | | | | [10.22] | [10.19] | | | |
| 30-day cumulative idiosyncratic returns | | | -0.469*** | -0.495*** | -6.33% | | | -0.400*** | -0.406*** | -3.70% |
| | | | [-9.85] | [-9.60] | | | | [-9.29] | [-8.86] | |
| Prior corporate action dummy | | 1.349*** | | 1.358*** | | | 1.395*** | | 1.328*** | |
| | | [14.32] | | [14.62] | | | [9.31] | | [9.30] | |
| Industry FE | Y | Y | Y | Y | | Y | Y | Y | Y | |
| Pseudo $R^2$ | 22.23% | 34.26% | 19.41% | 32.01% | | 32.04% | 40.69% | 30.73% | 39.07% | |
| Observations | 2,521 | 2,521 | 2,521 | 2,521 | | 2,123 | 2,123 | 2,123 | 2,123 | |

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the theory holds that firms will first seek to use internal funds before issuing debt and, finally, equity as the least-preferred option.

The pecking order theory predicts how firms sequence a chain of payout and financing decisions. According to the theory, we should not expect to see instances in which firms issue equity prior to suspending dividend payments or buybacks since these actions preserve internal funds. The pandemic sample is well suited for testing this prediction because, as we saw earlier, an unusually large number of firms undertook multiple corporate actions during this period.

Before turning to our broader analysis of transitions between a chain of corporate actions, we consider a subset of multiple corporate actions deemed either to be consistent with or in violation of the pecking order theory. These chains of actions are particularly interesting because they can help shed light on the evolution during the pandemic in how firms' perceived the trade-offs between preserving cash through internal funds (suspensions) versus raising capital externally.

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## 4.1 Chains of Actions Consistent with the Pecking Order Theory

Table 6 reports a complete list of companies whose chain of actions was fully consistent with what we should expect from the pecking order theory, i.e., a buyback or dividend stop followed by a bond issue, and, finally, an equity issue.[22] The table shows the company name (first column) and industry (second column), followed by the announcement dates for the four possible actions (columns 3-6). Rows are sorted by date of first action. The list only includes firms that announced at least two actions.

First consider the list generated for the pandemic crisis (Panel A). In total, 30 firms started a chain of actions with a buyback stop with the vast majority of these suspensions occurring in March and April. Retail Stores selling clothing or furniture, fast food restaurants, airlines (Alaska Air and Hawaiian Holdings) and firms in the travel industry (Expedia, Marriott) feature prominently on the list. Another 12 firms started

[22]Because some firms either may not have a share repurchase program in place or may have suspended an existing program without a formal announcement, we allow the first action to be either a buyback stop or a dividend suspension.

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with a dividend suspension followed by a bond issue. This list includes companies like Macy's and Designer Brands and oil companies such as Continental Resources.

The corresponding list for the Great Recession (Panel B) is much shorter and only includes two firms that first stopped buybacks before suspending their dividend payments. Another 10 firms started by suspending dividends prior to issuing bonds and, in three cases, equities.

### 4.2 Chains of Actions in Violation of the Pecking Order Theory

Table 7 shows the list of firms whose chain of actions represents a strong violation of the pecking order theory defined as equity issues that occur prior to dividend or buyback suspensions. We identify a total of 30 such cases during the pandemic. Ten of these occur in the "other" industry that includes mining, construction, building material and transportation followed by seven cases in utilities and four cases among financial firms.

The list of strong violations is, however, much longer for the Great Recession (136 firms) than for the pandemic (30). The industry composition is also very different as many more Banks, Insurance Companies and Other Financials, Chemicals, and Construction firms appear on the 2008/09 list relative to the list for 2020.

These comparisons show that the list of firms that sequenced a chain of multiple corporate actions fully consistent with the pecking order theory was much longer for the 2020 pandemic than for the Great Recession. In sharp contrast, the list of firms whose actions were in strong violation of the pecking order theory was much longer for the Great Recession period than during the pandemic.

### 4.3 Transitions between Corporate Actions

Having analyzed specific instances of chains of corporate actions that were either consistent with or in violation of the pecking order theory, we next provide a broader analysis of the transitions between corporate actions.

Recall from earlier that $A \in \mathcal{A} = \{$dividend stop, buyback stop, bond issuance, equity issuance$\}$ denotes the set of corporate actions included in our analysis. Further, define

**Table 6: Pecking Order Theory: Consistent Firms** This table reports the list of firms whose chain of actions are consistent with the pecking order theory in 2020 (Panel A) and 2008-2009 (Panel B). The initial corporate action must be either a dividend or a buyback suspension and firms must have taken multiple corporate actions over a 12-month window. We use SIC codes and the Fama-French 17 industry definitions to classify companies into the various industries.

| Company | Industry | Buyback stop date | Dividend stop date | Bond issue date | Equity issue date |
|---|---|---|---|---|---|
| Panel A: 2020 | | | | | |
| Gap Inc | Retail Stores | 12-Mar-2020 | 26-Mar-2020 | 23-Apr-2020 | |
| Expedia Inc | Other | 13-Mar-2020 | 23-Apr-2020 | 23-Apr-2020 | 07-Jul-2020 |
| Alaska Air Group Inc | Transportation | 16-Mar-2020 | 25-Mar-2020 | 23-Jun-2020 | |
| Texas Roadhouse Inc | Retail Stores | 17-Mar-2020 | 24-Mar-2020 | | |
| Hawaiian Holdings Inc | Transportation | 18-Mar-2020 | 20-Apr-2020 | 07-Aug-2020 | 01-Dec-2020 |
| Ford Motor Co | Automobiles | 19-Mar-2020 | 19-Mar-2020 | 17-Apr-2020 | |
| Emerald Expositions Events Inc | Other | 20-Mar-2020 | 20-Mar-2020 | | |
| SYNNEX Corp | Other | 24-Mar-2020 | 24-Mar-2020 | | |
| Marriott Vacations Worlwide | Banks, Insurance Companies, and Other Financials | 24-Mar-2020 | 06-May-2020 | | |
| Cracker Barrel Old Country Store | Retail Stores | 25-Mar-2020 | 25-Mar-2020 | | |
| Dick's Sporting Goods | Retail Stores | 25-Mar-2020 | 14-Apr-2020 | | |
| Terex Corp | Other | 25-Mar-2020 | 23-Apr-2020 | | |
| Carter's Inc | Retail Stores | 26-Mar-2020 | 05-May-2020 | | |
| Abercrombie & Fitch Co | Retail Stores | 26-Mar-2020 | 21-May-2020 | 18-Jun-2020 | |
| La-Z-Boy Incorporated | Consumer Durables | 29-Mar-2020 | 29-Mar-2020 | | |
| Herman Miller Inc | Other | 30-Mar-2020 | 03-Apr-2020 | | |
| Kohl's Corp | Retail Stores | 30-Mar-2020 | 17-Apr-2020 | 27-Apr-2020 | |
| Polo Ralph Lauren Corp | Textiles, Apparel & Footware | 31-Mar-2020 | 27-May-2020 | 01-Jun-2020 | |
| Phillips-Van Heusen Corp | Textiles, Apparel & Footware | 01-Apr-2020 | 01-Apr-2020 | 21-Apr-2020 | 06-Jul-2020 |
| Bed Bath & Beyond Inc | Retail Stores | 02-Apr-2020 | 02-Apr-2020 | | |
| Group 1 Automotive Inc | Automobiles | 07-Apr-2020 | 07-Apr-2020 | 03-Aug-2020 | |
| National Oilwell Varco Inc | Machinery and Business Equipment | 09-Apr-2020 | 20-May-2020 | | |
| Jack In The Box | Retail Stores | 15-Apr-2020 | 13-May-2020 | | |
| DineEquity Inc | Retail Stores | 16-Apr-2020 | 29-Apr-2020 | | |
| HCA Inc | Other | 21-Apr-2020 | 21-Apr-2020 | | |
| Yum China Holdings | Retail Stores | 28-Apr-2020 | 28-Apr-2020 | | |
| Standard Motor Products Inc | Automobiles | 29-Apr-2020 | 29-Apr-2020 | | |
| Dunkin Brands Group Inc | Other | 30-Apr-2020 | 30-Apr-2020 | | |
| Foot Locker | Retail Stores | 03-May-2020 | 22-May-2020 | | |
| Marathon Oil Corp | Oil and Petroleum Products | 06-May-2020 | 06-May-2020 | | |
| Domtar Corporation | Other | 08-May-2020 | 08-May-2020 | | |
| Twin River Worldwide Holdings | Other | 11-May-2020 | 13-May-2020 | 06-Oct-2020 | |
| Viad Corp | Other | 14-May-2020 | 14-May-2020 | | |
| Maxim Integrated Products Inc | Machinery and Business Equipment | 13-Jul-2020 | 28-Jul-2020 | | |
| Park Hotels & Resorts Inc | Other | | 16-Mar-2020 | 15-Sep-2020 | |
| Triumph Group Inc | Transportation | | 19-Mar-2020 | 05-Aug-2020 | |
| Macy's Inc | Retail Stores | | 20-Mar-2020 | 27-May-2020 | |
| Boyd Gaming Corp | Other | | 25-Mar-2020 | 13-May-2020 | |
| Vail Resorts Inc | Other | | 01-Apr-2020 | 29-Apr-2020 | |
| Arconic Corporation | Steel Works Etc | | 06-Apr-2020 | 29-Apr-2020 | |
| Continental Resources Inc | Oil and Petroleum Products | | 07-Apr-2020 | 10-Nov-2020 | |
| Meredith Corp | Other | | 20-Apr-2020 | 25-Jun-2020 | |
| Designer Brands | Retail Stores | | 01-May-2020 | 08-May-2020 | 04-Sep-2020 |
| KAR Auction Services Inc | Automobiles | | 07-May-2020 | 26-May-2020 | |
| Penske Automotive Group Inc | Automobiles | | 13-May-2020 | 04-Aug-2020 | |
| Townsquare Media Inc | Other | | 15-Jun-2020 | 16-Dec-2020 | |
| Panel B: 2008-2009 | | | | | |
| Lee Enterprises Inc | Other | 28-Sep-2008 | 19-Nov-2008 | | |
| WABCO Holdings Inc | Automobiles | 29-Oct-2008 | 27-Apr-2009 | | |
| Warner Music Group Corp | Other | | 08-May-2008 | 19-May-2009 | |
| Nelnet Inc | Banks, Insurance Companies, and Other Financials | | 22-May-2008 | 25-Nov-2008 | |
| Landry's Restaurants Inc | Retail Stores | | 20-Jun-2008 | 04-Feb-2009 | |
| M/I Homes Inc | Construction and Construction Materials | | 31-Jul-2008 | 04-Aug-2008 | 19-May-2009 |
| Boyd Gaming Corp | Other | | 01-Aug-2008 | 12-Dec-2008 | |
| Centex Corp | Construction and Construction Materials | | 14-Oct-2008 | 06-Nov-2008 | |
| CNA Financial Corp | Banks, Insurance Companies, and Other Financials | | 27-Oct-2008 | 30-Apr-2009 | |
| Midwest Banc Holdings Inc | Banks, Insurance Companies, and Other Financials | | 07-Nov-2008 | 29-Dec-2008 | |
| Brookdale Senior Living Inc | Other | | 02-Mar-2009 | 12-May-2009 | 02-Jun-2009 |
| Harman Intl Industries Inc | Consumer Durables | | 29-Apr-2009 | 15-Jun-2009 | 17-Jun-2009 |

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**Table 7: Pecking Order Theory: Violations** This table reports the total number of firms whose chain of actions during the Global Financial Crisis and the Covid-19 pandemic constitute a strong violations of the pecking order theory. Strong violations happen when a dividend-paying firm raises equity without suspending their ordinary dividend payments. The last column lists the name of the companies and the date of their first equity issue in 2020. We use SIC codes and the Fama-French 17 industry definitions to classify companies into the various industries.

Panel A: Violations

| Industry | 2008-2009 | 2020 | List in 2020 |
|---|---|---|---|
| Food | 1 | 0 | |
| Mining and Minerals | 5 | 1 | Gold Resource Corp: 15-Jun |
| Oil and Petroleum Products | 8 | 2 | Brigham Minerals: 09-Jun; Panhandle Oil & Gas: 28-Aug |
| Consumer Durables | 1 | 0 | |
| Chemicals | 5 | 0 | |
| Drugs, Soap, Perfumes, Tobacco | 2 | 3 | Owens & Minor: 01-Oct; Turning Point Brands: 08-Jul<br>Vector Group: 13-May |
| Construction and Construction Materials | 6 | 0 | |
| Steel Works Etc | 1 | 0 | |
| Machinery and Business Equipment | 5 | 2 | GrafTech International: 14-Dec; Vertiv Holdings: 12-Aug |
| Automobiles | 1 | 0 | |
| Transportation | 2 | 2 | Heartland Express: 21-Jul; Werner Enterprises: 03-Jun |
| Utilities | 12 | 7 | Avista: 15-May; Chesapeake Utilities: 30-Jun; MGE Energy: 12-May<br>Dominion Resources: 17-Mar; Consolidated Edison: 01-Dec<br>Ormat Technologies: 18-Nov; South Jersey Industries: 06-Apr |
| Retail Stores | 5 | 0 | |
| Banks, Insurance Companies, and Other Financials | 58 | 4 | Bain Capital Specialty Finance: 30-Mar; Flagstar Bancorp: 10-Aug<br>Houlihan Lokey: 18-May; Stewart Information Services: 12-Aug |
| Other | 24 | 10 | The ADT Corp: 15-Sep; Bentley Systems Inc: 12-Nov; Cable One: 19-May;<br>Hamilton Lane: 02-Jun; Kinsale Capital Group: 04-Aug<br>Mesa Laboratories: 09-Jun; Simulations Plus: 05-Aug; Shutterstock: 11-Aug-2020<br>Strategic Education: 05-Aug; Towers Watson: 22-Apr |
| Total | 136 | 31 | |

an indicator variable $z_{A,i,t}$ such that $z_{A,i,t} = 1$ if company $i$ announces action $A$ on day $t$, while otherwise $z_{A,i,t} = 0$. We can then study the chain of corporate actions, focusing on whether some action $A$ by firm $i$ ($z_{A,i,t}$) precedes another action $A'$ ($z_{A',i,t'}$) by the same firm, i.e., if $t < t'$. Chains of corporate actions during some window can be measured through the number and proportion of transitions from action $A$ to action $A'$.

During normal times, corporate actions are often distantly separated in time, making it important to clearly define transitions between corporate actions. Because pairs of corporate actions may not be linked if they occur far apart, we only count as transitions those instances in which the two corporate actions are separated by at most one year.

Table 8 shows the number and proportion of transitions computed for a baseline period (2009:07-2019:12, Panel A) and the Covid pandemic (2020, Panel B). With four types of corporate actions, this yields a $4 \times 4$ transition table. Each entry (cell) shows the number of times a given row action preceded a column action. For example, during the baseline period (Panel A) an equity issue preceded a bond issue within one year on 1,975 occasions. The bottom row labeled "total" shows the number of times the action listed in the corresponding column was preceded by an earlier action while the "total" column shows the number of times the row actions preceded other actions.[23] Finally, the right-most column labeled "single actions" counts the number of instances in which the action listed in the corresponding row was not followed by another action within a year.

The roughly 10-year baseline period saw a total of 15,789 transitions between corporate actions. In the vast majority of these instances, bond or equity issuance either precede another action (7,672 and 8,058 cases, respectively) or follow it (7,488 and 8,266 cases, respectively). Conversely, there are only 33 and 26 cases in which buyback or dividend suspensions preceded other actions and even fewer cases (19 and 16, respectively) where they followed another action.[24]

[23] Because the actions listed in the rows could themselves have been preceded by other actions, the "total" column does not equal the number of times the row action was the first to occur. For example, a chain consisting of a bond issue → buyback stop → equity issue and a shorter chain consisting of a buyback stop → equity issue would both add one to the count of buyback stop → equity issue transitions. However, the buyback stop is the first action only for the second chain.

[24] Consistent with the pecking order theory, we see very few (two and three) instances in which a buyback or dividend stop is preceded by an equity issue.

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Converting these numbers into transition probabilities, in normal times bond and equity issuance account for about 48% and 51% of all transitions, respectively. By far the most common chain is equity issuance→equity issuance (38.5%), followed by bond issuance→bond issuance (34.7%) and bond→equity issuance or equity→bond issuance, both of which account for roughly 13% of the transitions between actions. All other pairs of actions account for a tiny fraction of overall transitions.

Among the list of single actions that were not followed by another action within a year (final column), buyback stops account for a disproportionately large part, namely 450 out of 1,901 single actions compared to 33 of 15,789 of the transitions. In many cases, a buyback stop was thus the only action taken by firms, at least within a one-year window.

Turning to the pandemic period (Panel B), out of a total of 1,069 transitions the preceding action was a bond issue in 532 cases, an equity issue in 382 cases, with buyback and dividend suspensions accounting for 93 and 62 cases, respectively. Thus, while buyback and dividend suspensions remained less common than bond and equity issuance during the pandemic–in part because the latter can occur multiple times–they account for a nontrivial proportion of corporate actions and a much larger share than during the baseline period.

During the pandemic, bond and equity issues accounted for 52% and 37% of transitions between corporate actions with buyback and dividend suspensions accounting for 6% and 5% of transitions, respectively. The most common transitions are bond→bond issuance (35%) and equity→equity issuance (27%) followed by bond→equity issuance (11%) and equity→bond issuance (9%).

In marked contrast with the baseline period, 10% of transitions during the pandemic come from buyback or dividend suspensions preceding a bond issue. This chain of actions is consistent with internal funds being the least costly way of accessing capital and also fully consistent with the pecking order theory. Equally consistent with this theory, we only see a single case in which an equity issue precedes either a buyback stop or a dividend suspension.

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**Table 8: Transitions between corporate actions.** This table reports the total number (N) and percentages (%) of transitions between dividend and buyback suspensions, bond and equity issues during the benchmark period (July 2009 – December 2019, Panel A) and the Covid-19 crisis (2020, Panel B). Rows and columns labeled "Total" sum up the underlying numbers of transitions, while the final column (Single Actions) shows the number of cases in which an initial corporate action was not followed by a second action within the listed period.

Panel A: July 2009 – December 2019

| From/To | Bond issue | | Buyback stop | | Dividend stop | | Equity issue | | Total | | Single actions |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | % | N | % | N | % | N | % | N | % | N |
| Bond issue | 5,483 | 0.347 | 15 | 0.001 | 11 | 0.001 | 2,163 | 0.137 | 7,672 | 0.486 | 1,372 |
| Buyback stop | 18 | 0.001 | 1 | 0.000 | 2 | 0.000 | 12 | 0.001 | 33 | 0.002 | 450 |
| Dividend stop | 12 | 0.001 | 1 | 0.000 | 0 | 0.000 | 13 | 0.001 | 26 | 0.002 | 25 |
| Equity issue | 1,975 | 0.125 | 2 | 0.000 | 3 | 0.000 | 6,078 | 0.385 | 8,058 | 0.510 | 54 |
| Total | 7,488 | 0.474 | 19 | 0.001 | 16 | 0.001 | 8,266 | 0.524 | 15,789 | 1 | 1,901 |

Panel B: 2020

| From/To | Bond issue | | Buyback stop | | Dividend stop | | Equity issue | | Total | | Single actions |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | % | N | % | N | % | N | % | N | % | N |
| Bond issue | 370 | 0.346 | 33 | 0.031 | 9 | 0.008 | 120 | 0.112 | 532 | 0.498 | 441 |
| Buyback stop | 68 | 0.064 | 0 | 0.000 | 20 | 0.019 | 5 | 0.005 | 93 | 0.087 | 349 |
| Dividend stop | 45 | 0.042 | 9 | 0.008 | 0 | 0.000 | 8 | 0.008 | 62 | 0.058 | 118 |
| Equity issue | 91 | 0.085 | 1 | 0.001 | 1 | 0.001 | 289 | 0.270 | 382 | 0.357 | 115 |
| Total | 574 | 0.537 | 43 | 0.040 | 30 | 0.028 | 422 | 0.395 | 1,069 | 1 | 1,023 |

## 4.4 Multiple Simultaneous Corporate Actions

On rare occasions, a firm announces multiple corporate actions on the same day. Such instances are of particular interest because they often indicate that a firm faces very high levels of financial distress as reflected in the fact that (a) a single corporate action was deemed insufficient; or (b) the firm did not have the time to separate the two actions and see if a single action would suffice. To examine these events during the pandemic, Figure 8 plots a weekly count of the number of times a firm announced multiple corporate actions on the same day in 2020. With four different types of actions, there is a total of six possible combinations; only five of these occur during our pandemic sample.

The most common pairs of actions announced simultaneously are bond and equity issues and suspensions of dividends and share repurchases. The time profile of these paired actions is very different, however. Whereas simultaneous bond and equity issues are fairly evenly spread out across the pandemic and never exceed three in any one week, same-day suspensions of buybacks and dividends are entirely concentrated between March 22 and May 27. During this spell, there were up to nine weekly same-day announcements of a dividend and buyback suspension. Days on which the

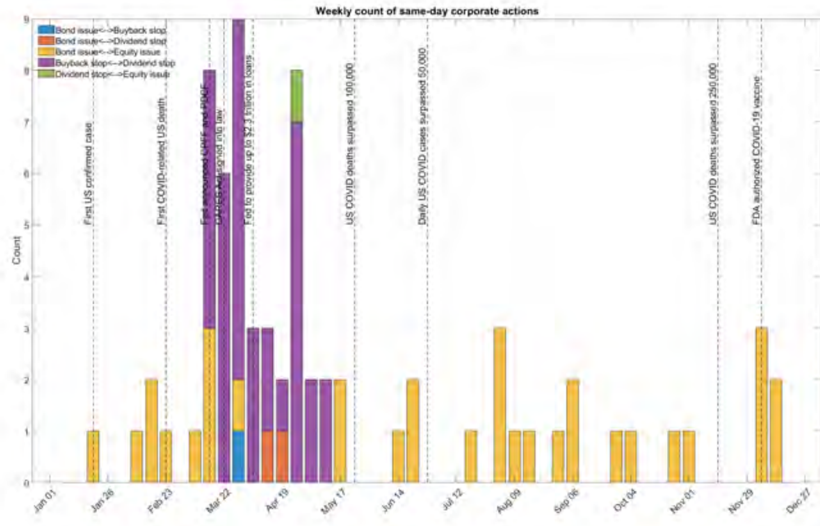


**Figure 8: Weekly count of the number of cases for which a firm announced multiple corporate actions on the same day.** This figure plots the weekly count of the number of instances in which a firm announced multiple corporate actions on the same day during 2020.

same firm announces either a bond issue and a buyback, a bond issue and a dividend stop, or a dividend stop and an equity issue occur only once or twice in our 2020 sample.[25]

## 5 Stock Market's Reaction to Corporate Announcements

During normal times, corporate actions such as suspensions of dividends or share repurchase programs are likely to be interpreted by financial markets as strong signals about firm-specific growth prospects. The Covid-19 pandemic clearly does not fit this mold - the ensuing lockdown was an economy-wide, common shock that fundamentally altered the information content investors could infer from firms' payout or financing decisions. Stated differently, the first order effect of companies like Hilton or Marriott suspending their dividends after the pandemic outbreak, could plausibly have been for

[25]Readers may wonder whether there are any cases in which a company announced more than two corporate actions on the same day. We have found only one such instance: On April 28, 2020, Southwest Airlines announced that they had suspended dividends, stopped share buybacks, and also issued equity.

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investors to infer that these firms wanted to preserve capital in a situation with uncertain revenue prospects. It is less likely that such announcements caused investors to fundamentally revise their views on Hilton and Marriott's firm-specific prospects because data on sharp declines in hotel occupancy rates and business travel was already publicly available.

Before presenting our analysis, we note that other papers have studied the stock market's reaction to the COVID-19 shock. Ramelli and Wagner (2020) conduct a cross-sectional analysis of how stock prices responded to the emergence of the COVID-19 pandemic. Albuquerque et al. (2020) and Pagano et al. (2020) find evidence that firms with high environmental and social ratings and firms from industries that were less affected by social distancing outperformed the market. Fahlenbrach et al. (2020) document that firms with greater financial flexibility and larger cash holdings were better able to withstand the COVID-19 revenue shock, as evidenced by a drop in their stock price that was 9.7 percentage points lower on average than for firms with more limited financial flexibility.

As will become clear below, our focus is very different from these papers as we analyze the impact of dividend and buyback policy announcements on asset prices. Specifically, to explore whether the stock market reacted differently to announcements of corporate actions during the pandemic compared to during the baseline period (2009:07-2019:12), we study how firms' stock prices evolved during a short event window surrounding the announcement dates.

## 5.1 Methodology

Our analysis uses tools from standard event study methodology. Specifically, using a three-factor Fama-French model we first regress each firm's excess returns on market, SMB, and HML factors. These regressions use daily data during a 100-day window stretching back from 115 days to 15 days prior to each firm's announcement date. Using the estimated coefficients from this regression, we next compute abnormal returns from ten days before each firm's announcement date to ten days after. For each firm we accumulate these residuals to obtain cumulative abnormal returns (CARs). Finally, we

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compute simple cross-sectional averages of the CARs.

### 5.2 Dividend Suspensions

First consider dividend suspensions (left panels in Figure 9). During the benchmark sample (2009:07-2019:12), firms that suspend dividends on average earn CARs around -2% in the period from 10 days to 3 days prior to the announcement - values that are borderline significant on most days. CAR values then start rising and actually turn slightly positive on the announcement date (day 0), though this value is not significant. For the remainder of the event window, CAR values are essentially zero. During the pandemic, the pattern and magnitude of movements in CAR values is very similar to that seen for the benchmark period: small negative values in the period leading up to the announcement date, followed by a slight increase on the announcement date with CAR values that remain insignificantly different from zero thereafter.

On a cumulative basis, CAR values during the pandemic rose by 4% in the period preceding the suspension announcement by a few days and ending 10 days after. A plausible explanation for this reaction is that dividend suspensions did not come as a big surprise to markets and, when announced, were seen as a prudent action that helped reduce risk in a situation with extreme uncertainty surrounding firms' future cash flows.

### 5.3 Buyback Suspensions

During the baseline period (top right panel in Figure 9), CAR values are essentially zero prior to the announcement of a buyback suspension. The announcement date sees a sharp negative effect of about -2% with CAR values remaining quite stable and borderline significant for up to 10 days afterwards. This pattern is consistent with no leakage of news about the buyback suspension prior to its announcement and a clear, if economically modest, negative short-term announcement effect.

Buyback suspensions announced during the pandemic (second row, right panel) were associated with a very different pattern in CAR values. Between five and ten days prior to the suspension announcement, CAR values are significantly negative and trend

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downward from zero to -2%. They then reverse course and begin to trend upwards, peaking around 2-3% (which is significant) towards the end of the post-announcement window. Moreover, there is a modest positive announcement effect - the opposite of what we find for the baseline period.

During the pandemic, buyback suspensions were, thus, both preceded and followed by a sequence of positive abnormal returns, consistent with the action being seen as prudent and precautionary by the markets.[26] The fact that the CAR curve begins to trend upward five days prior to the announcement also suggests that markets were expecting buybacks to be suspended ahead of time.

### 5.4 Total Stock Returns Around Suspension Dates

Our estimates in Table 2 and Table 3 suggest that cumulative return performance in the stock market are strongly predictive of firms' decision to suspend dividends and buybacks. Ultimately it is difficult to separate a "causal" effect from stock prices to suspension decisions (lower stock prices making suspensions more attractive) from a more traditional information channel (markets anticipating a suspension announcement and reacting accordingly) and the two mechanisms need not be mutually exclusive. However, it is certainly plausible that negative return performance triggered suspension decisions. First, large negative returns could reflect the stock market's pessimism on the economic impact of the pandemic. This, in turn, could have caused firms to revise downward their expectations of future revenues. Second, large negative stock returns and a reduced stock market valuation would have made it more attractive for firms to save on internal sources of capital as it made it harder for firms to tap into equity markets. Lower valuations may also have triggered more stringent loan conditions through bond covenants, making it more difficult to access external capital markets.

To the extent that poor stock market performance played a role in triggering suspensions, we would expect companies' total returns, rather than the abnormal return component alone, to matter most during the even window. We pursue this idea by

[26]This is also very different from a sharply negative association between buyback suspensions and CAR values during the Great Recession.

plotting in the bottom four panels of Figure 9 the cumulative *total* returns during the 21-day event window surrounding the dividend and buyback suspensions. During the benchmark period (third row), cumulative total returns around dividend suspension announcements are borderline flat between -1% and -2% before increasing to a level near zero where they remain from the event date and onward with none of these values being statistically significant. A very different pattern emerges during the pandemic (bottom left panel): cumulative total returns decline from about -1% ten days prior to the announcement to a highly significant level of -6% two days prior to the announcement date before sharply reversing the direction of the trend and finishing above 5% at the end of the event window.

Similar differences in the total return patterns are seen for buyback suspensions: during the benchmark period, cumulative total returns are negative on most days with borderline significant values mostly in the range of -1% to -3%. Conversely, in 2020, cumulative total returns drop sharply from zero to -8% two days prior to the announcement. From this point onward, cumulative total returns start rising, reaching a level near zero by the end of the event window.

These plots show that firms announced the suspensions of their dividend and share repurchase programs during the pandemic following large drops in their total returns. The subsequent recovery in cumulative total returns of 8-10% from two days prior to the announcement day to ten days after is more difficult to explain. One possibility is that markets anticipated the suspension decision two days prior to the announcement and rewarded firms for taking what was seen as a "prudent" action. This does not explain why cumulative returns continued to rise even after the announcement. This rise could possibly be due, instead, to firms being perceived as "lower risk" as a result of their decision to suspend payouts and preserve capital. For this mechanism to have played out over several days - as opposed to on a single (announcement) day - investor expectations would need to display some degree of stickiness, however.

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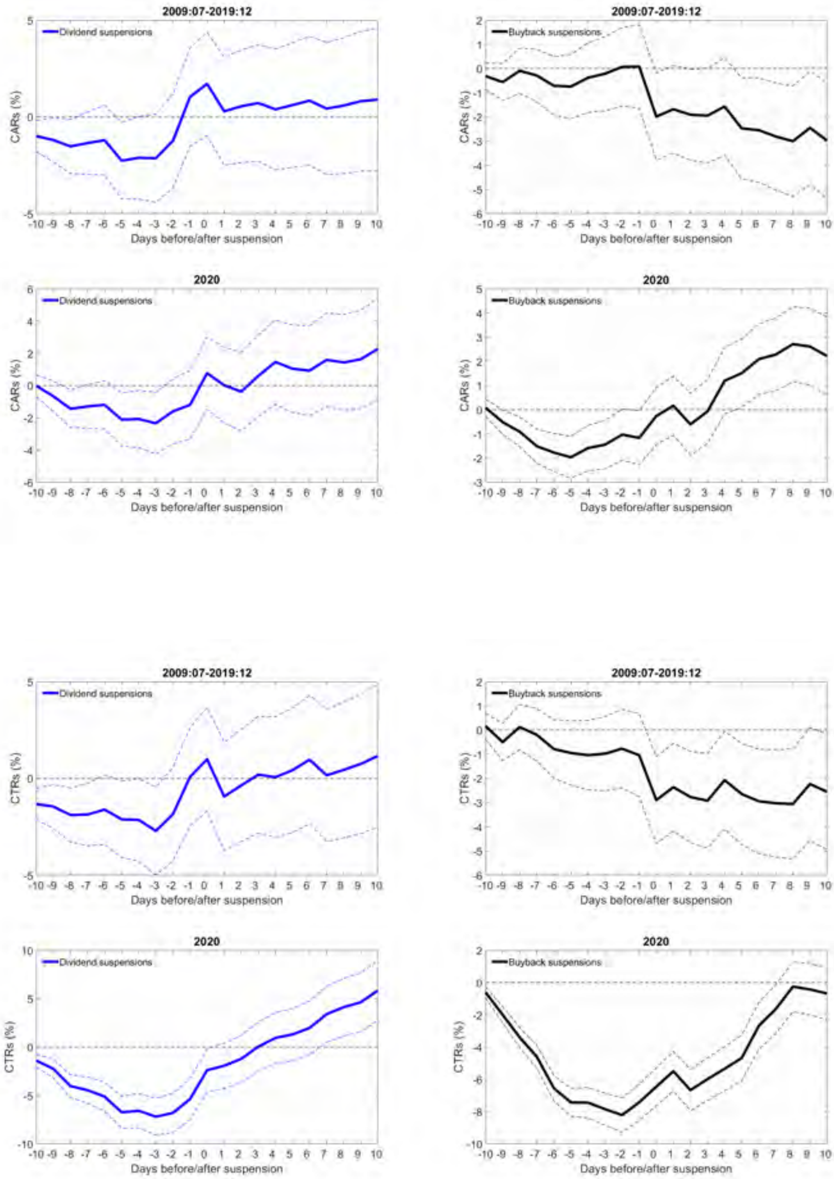


**Figure 9: Stock market's reaction to announcements of dividend and buyback suspensions**. This figure plots the cumulative abnormal returns (CARs, top two rows) and the cumulative total returns (CTR, bottom two rows), averaged across firms, during a window of twenty days around individual firms' announcements of dividend (left quadrants) and buyback (right quadrants) suspension (day 0). Results are shown separately for the Covid-19 pandemic and the 2009:07–2019:12 period.

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### 5.5 Equity and Bond Issues

Figure 10 shows that movements in CAR values associated with news of equity and bond issues in general were smaller than what we saw for payout suspensions. During the baseline period, announcements of equity issues (top left panel) were associated with positive and mostly significant CAR values that rose from zero ten days prior to the announcement to 0.7% one week later where it plateaued until the announcement date. CAR values then dropped sharply the following day and stayed near 0.4% for the remainder of the event window. A few days after the announcement, CAR values were no longer statistically significant, suggesting that announcements of equity issues were associated with an economically small and short-lived effect on stock prices.

During the pandemic, equity issues (second row, left panel) were associated with significantly positive and economically large CAR values that steadily rose from zero 10 days prior to the announcement and peaked above 3% one day prior to the announcement. The announcement is associated with a reversal in the trend in CAR values which start a systematic decline and turn negative and insignificantly different from zero after a few days. While the pattern in CAR values during the pandemic is, thus, broadly similar to what we see in the benchmark period, the magnitude of movements is much greater during 2020.

Turning to the bond issues (top right panels in Figure 10), CAR values during the baseline period hover around zero until four days prior to the announcement date. They then climb to reach a statistically significant level of 0.4% on the announcement date and remain constant thereafter, consistent with a small positive, medium-term effect of bond issues on stock prices.

Conversely, during the pandemic, the estimated effect of bond issue announcements on CAR values is small and statistically insignificant throughout the entire event window. A possible explanation of this is that the Federal Reserve's intervention in the bond markets made it easy for the majority of firms to tap into this source of capital and suspended the usual price discovery and screening process associated with raising external capital. This easy access to raise money by issuing bonds essentially muted the signaling value of bond issues which is seen during more normal times.

The bottom four panels of Figure 10 display results using cumulative total returns. During both the baseline and pandemic periods, stock prices rose near-monotonically both before and after the announcement date, with a small reversal seen on the announcement date itself. No reversal effect on the announcement date is seen for bond issues: In both samples, cumulative total returns rise near-monotonically from near-zero, ten days prior to the announcement to 2.5% during the benchmark period or 4.5% during 2020.

Assuming that movements in total returns prior to the issue announcements were not driven by leaked information, these plots suggest that companies tend to issue equity and bonds after a run of significantly positive (total) stock returns. A string of positive returns enables firms to raise new funds from external markets at a better price. The continued rise in total returns after the announcement of an issue could again be related to a lower perceived risk after a firm has managed to successfully raise capital.

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### 5.6 Market Reaction for Firms that did not Suspend Dividends

In a separate analysis we consider the stock market's reaction to news about firms that chose not to suspend their dividend payments. Our analysis categorizes non-dividend suspending firms into three groups, namely (i) firms announcing no changes or small reductions (less than 30% year-on-year decreases) in their dividends; (ii) firms announcing increases to their dividends; and (iii) firms with large dividend cuts. For all three groups, CAR values are economically small (typically below 1%) and insignificantly different from zero throughout the 21-day event window.

## 6 Conclusion

US firms suspended their dividend and share repurchase programs in unprecedented numbers and at unparalleled speed after the outbreak of the Covid-19 pandemic; they

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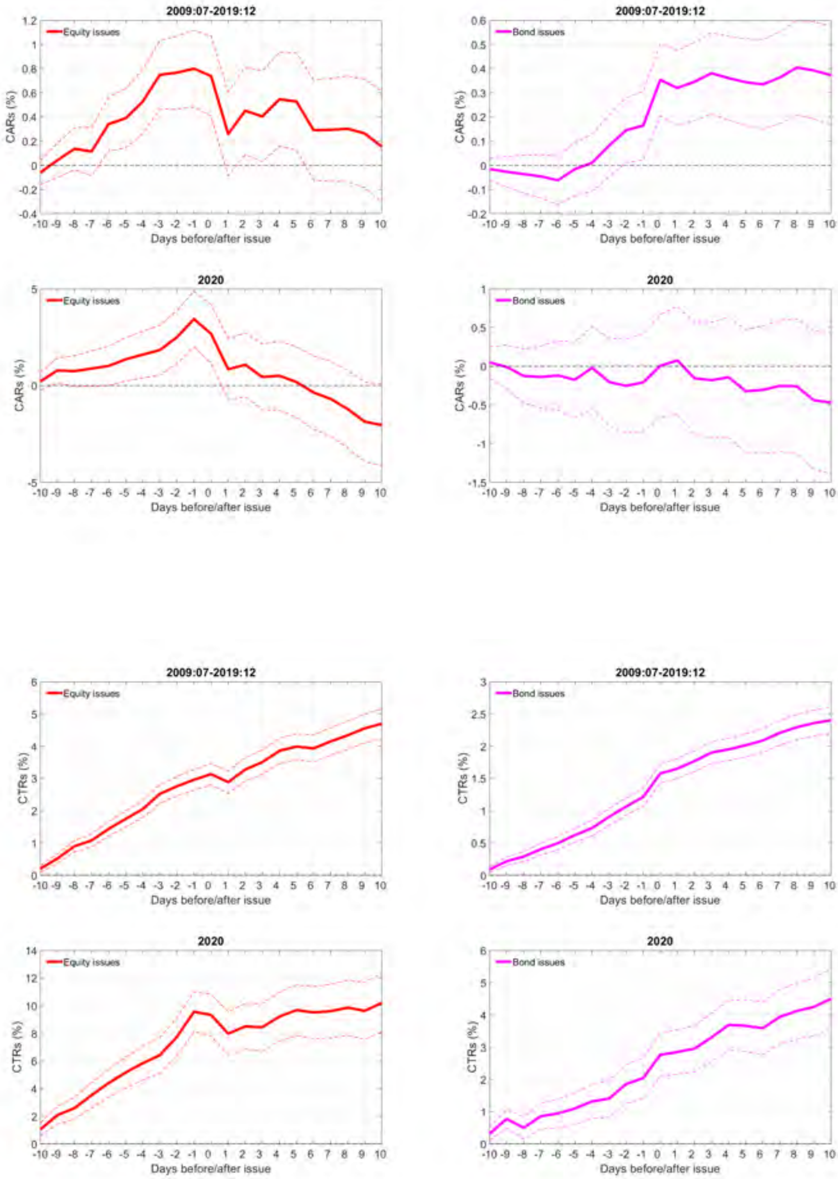


**Figure 10: Stock market's reaction to announcements of equity and bond issues**. This figure plots the cumulative abnormal returns (CARs, top two rows) and the cumulative total returns (CTR, bottom two rows), averaged across firms, during a window of twenty days around individual firms' announcements of equity (left quadrants) and bond (right quadrants) issues. Results are shown separately for the Covid-19 pandemic and the 2009:07–2019:12 period.

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also raised large sums of money by issuing bonds and stocks. We provide a detailed analysis of the timing and importance of these decisions, quantifying how much money US firms preserved by suspending or reducing dividends, stopping share buybacks, and issuing bonds and equity. We also examine the determinants of firms' decisions to suspend payouts and issue debt or equity. Firm characteristics such as size, leverage, profitability, cash holdings, and revenue growth were important predictors of many of these decisions, with revenue growth playing a particularly important role. In addition, firms with highly volatile and large negative idiosyncratic stock returns in the 30-day period leading up to an announcement were far more likely to have suspended dividends or buybacks and to have issued stocks or bonds than firms with less volatile and larger returns.

The stock market's reaction to corporate announcements during the pandemic shows that investors were aware of the highly unusual circumstances that led to the flurry of payout suspensions and financing decisions. For example, payout suspensions that normally would contain bad news about firm prospects tended to be associated with higher stock returns, possibly because they reduced firm risks.

As the stock and bond markets bounced back from the initial pandemic shock, companies dynamically adjusted their payout and financing decisions, in many cases raising new capital multiple times. For the most part, the sequence of corporate decisions during the pandemic was consistent with that predicted by the pecking order theory, with firms initially preserving internal capital by suspending dividends or buybacks, followed by bond issues and, finally, equity issues.

Our analysis demonstrates the crucial role played by the Federal Reserve's massive interventions which helped firms with below-investment grade ratings regain market access after the market for their bond issues came to a standstill in March. The continued supply of liquidity kept the financial markets functioning smoothly after the initial pandemic shock. The many firms in our sample that raised capital over multiple rounds throughout 2020 demonstrates how the continued access to deep and liquid capital markets proved pivotal to firms' ability to outlast a pandemic whose adverse impact on revenues turned out to be severe and long-lived.

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# Do remittances mitigate the COVID-19 employment shock on food insecurity? Evidence from Nigeria

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*This paper assesses the mitigating role of remittances during the adverse COVID-19 employment shock on Nigeria's food insecurity. Based on pre-COVID-19 and post- COVID-19 surveys, we use a difference-in-difference approach while controlling for time and household fixed effects. Results indicate that remittances are mitigating the negative consequences of COVID-19 employment shocks, especially in the short run. We find that 100% of the deterioration in food insecurity, owing to the shock, is offset by the remittances received. While the adverse effects of the shock persist over time, the mitigation effects of remittances appears to be effective only at the early stages of the pandemic. Furthermore, the mitigation effect of remittances is heterogeneous regarding the origin of remittances, residence area, and poverty status. The mitigation effect of remittances is higher for remittances from abroad than for domestic ones. We also find a higher mitigating effect of remittances in rural areas and for non-poor households. Finally, our results shed light on the capital channel as a crucial mechanism explaining the mitigation effect of remittances. Notably, our findings suggest that formal financial inclusion, capital ownership like livestock or rental earnings, amplifies the attenuating effect of remittances.*

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## 1 Introduction

The recent COVID-19 pandemic poses unprecedented challenges for governments around the world. The rapid spread of the virus and the subsequent death toll have led the policymakers to enact unprecedented strict movement restriction measures to slow down its transmission. Although these measures are intended to prevent the loss of human life, their economic cost is worth questioning. The COVID-19 outbreak is affecting economic activity and, consequently the household economies through two main channels representing supply and demand adverse shocks. The first channel is health (hereafter, the "health channel"), as workers getting sick results in lower productivity. At the same time, consumers may react to the outbreak by significantly reducing their demand for goods/services requiring interpersonal contact[1] (Wren-Lewis, 2020). The second channel is the government's movement restriction measures, especially the lockdown (hereafter, the "lockdown channel") to contain the pandemic. These measures are causing sizeable economic disruptions simultaneously. Businesses across most industries have been constricted because of stay-at-home orders and other movement restrictions measures. For instance, most countries have closed their borders, and entire industries (restaurants, stores, etc.) have been shut-down for a certain length of time.

The immediate consequences of such major disruptions have been a significant drop in income and high unemployment, or job losses, which have affected the household welfare. It has been reported that in 2020, the COVID-19 shock could translate into a decrease of 2.1 % of economic activity in the African region alone (Arezki Rabah and Koffi, 2021). This represents the continent's first recession in half a century and could push about 39 million people into extreme poverty[2] in 2021 (Arezki Rabah and Koffi, 2021). The pandemic may also dramatically worsen food insecurity in this region (Pereira and Oliveira, 2020). In parallel to prediction-based studies, the rapidly growing literature on real-time household surveys supports these forecasts. Using online survey data, Kansiime et al. (2020) find that, during the pandemic, the proportion of food insecure respondents has increased by 38% and 44% in Kenya and Uganda,respectively, compared with the period before the pandemic. However, the approach and the data used in their study may be questioned for several reasons. First, their data may suffer from a lack of representativeness. Second, their approach, based on a before-after period comparison of food insecurity, may be biased. For instance, their approach does not control for biases in time-trend-related factors such as seasonality of harvest or planting period. Despite these limitations, the findings provide suggestive evidence of the tension caused by the COVID-19 shock and are supported by other studies investigating the Nigeria setting. Based on a high-frequency household survey, Amare et al. (2020) account for some of the limitations in Kansiime et al. (2020)'s study. They use a difference-in-difference (DiD) methodology to assess the impact of the COVID-19 shock on food insecurity. Using a similar approach, Adjognon, Bloem, and Sanoh (2020)'s results confirm the adverse effect of COVID-19 on household food insecurity in Mali, particularly in urban areas. Amare et al. (2020) find similar results in Nigeria settings and provide evidence of two potential channels affecting household economics: labor market participation and

[1] restaurants, travel, haircuts, etc.
[2] In terms of proportion, extreme poverty could increase by 2.9% points in 2021.

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food prices.

In Sub-Saharan Africa, several factors may be exacerbating the COVID-19 shock on food insecurity. Households from this region are particularly exposed to adverse shocks, where nearly 81% of the population is not covered by social protection compared with 38% in Latin America and the Caribbean, and 40% in South Asia in 2016[3]. Balde, Boly, and Avenyo (2020) find that the COVID-19 impact is higher among informal workers. These workers are more likely to experience job loss and hardship in trying to meet their basic needs. The lack of access to social protection is even more concerning, given the imperfections in markets and credit constraints. Financial inclusion in Sub-Saharan Africa region is the lowest in the world (Demirgüç-Kunt, 2014). The evidence indicates that households with no access to social protection or financial services, such as poor households and informal workers, are likely to experience considerably greater food insecurity (Amare et al., 2020; Balde, Boly, and Avenyo, 2020).

Against a backdrop of market failures and weak social protection, households' alternatives to mitigating COVID-19 adverse shocks are limited. Households tend to rely on private insurance for risk-sharing based on informal mechanisms, including remittances from migrants or relatives living within the same country or abroad. Evidence from the insurance-related migration literature suggests that remittances can function as an insurance mechanism to smooth household consumption. Based on a panel dataset of developing countries, Combes and Ebeke (2011); Mondal and Khanam (2018); Beaton, Cevik, and Yousefi (2018) find that remittances significantly decrease consumption volatility, highlighting hence their smoothing-consumption role. From a micro perspective, using a household survey, Amuedo-Dorantes and Pozo (2011)' find similar results, indicating a decrease in income volatility in the Mexican setting. Meanwhile, Yang and Choi (2007) find that remittances increase in response to a rainfall shock, partially offsetting the resulting decline in income in the Philippines. Instead of aggregate shocks, such as rainfall shocks, Beuermann, Ruprah, and Sierra (2016); Akim (2018) investigate the remittance insurance function against idiosyncratic shocks such as health or death shocks, in Jamaica and Mali, respectively. Their results indicate that remittances can help households to smooth their consumption when facing idiosyncratic shocks.

Although the insurance-related migration literature provides evidence of the insurance function of remittances, the findings are not necessarily generalizable to the context of COVID-19 shock and the disruptions related to government measures. The COVID-19 shock is of a different nature in many aspects, including its magnitude and mechanisms. The mitigating role of remittances on adverse COVID-19 shock has not been explored sufficiently in the rapidly growing COVID-19-related literature. Balde, Boly, and Avenyo (2020) analyze how remittances can cushion the adverse effects of COVID-19 on the ability of informal worker in meeting basic needs. They find that informal workers in Senegal who receive remittances face fewer challenges in meeting their basic needs in Senegal, whereas this is not the case in Mali or Burkina Faso. Nonetheless, they use an online survey that is subject to a serious bias as it lacks representativeness, similar to that used by Kansiime et al. (2020). Besides the representativeness bias, their estimates may suffer from

[3]World Bank, ASPIRE: THE ATLAS OF SOCIAL PROTECTION - INDICATORS OF RESILIENCE AND EQUITY.

endogeneity bias as cross-sectional probit regressions are used without controlling for selection bias related to unobservables.

This paper aims to assess the mitigating role of remittances during the adverse COVID-19 employment shock on Nigeria's food insecurity. Using pre-COVID-19 face-to-face surveys and post-COVID-19 phone surveys, we exploit temporal changes in food insecurity and COVID-19 employment shocks measured at the household level, to design a difference-in-difference (DiD) strategy. Our findings contribute to expanding the rapidly growing COVID-19 literature. Given the data at hand and the methodology, our paper expands on the works of Amare et al. (2020) and Balde, Boly, and Avenyo (2020). However, we distinguish from these paper in following respects. The primary focus of our paper is on the role of remittances in mitigating the COVID-19 adverse shocks, while that of Amare et al. (2020)'s is on an assessment of the actual magnitude of the shock and its potential impact pathways. In contrast to Balde, Boly, and Avenyo (2020), we use a more robust approach that addresses the potential endogeneity arising from selection bias. Furthermore, we add to the literature by exploiting our panel data's length to investigate the response to the shock over time and the persistence of the remittances' mitigating effect.

Our paper likewise contributes to the nascent insurance migration literature. Although the role of remittances in smoothing consumption has been highlighted in the literature, insufficient attention has been given to the underlying mechanisms. We expand the scope of shocks considered thus far in the insurance-migration literature and shed light on one of the two potential mechanisms through which remittances may protect households against adverse shocks. First, remittances can function as an **ex-post** shock-mitigating mechanism. Households may receive remittances immediately following the shock, subsequently increasing their income. There is evidence of an increase in remittances following shocks such as natural disasters or weather shocks (Gubert, 2002; Yang and Choi, 2007; David, 2011; Lara, 2016). However, this ex-post mechanism is unlikely to operate in the case of shocks from a global pandemic such as COVID-19, as remittances are expected to decrease sharply (Ratha et al., 2020). Second, remittances can function as an **ex-ante** mitigating shock mechanism. By releasing budgetary constraints, remittances may allow the households to increase savings and subsequently cope with the shock. There is evidence of remittances stimulating financial services, such as savings and credit, (Anzoategui, Demirgüç-Kunt, and Pería, 2014; Ambrosius and Cuecuecha, 2016) and even substituting for credits in the case of a health shock (Ambrosius and Cuecuecha, 2013).

We focus on the **ex-ante** mechanism by testing whether household capital ownership amplifies the mitigating effect of remittances. Specifically, we test whether the remittances' attenuating effect is higher for households with capital. We adopt a broad definition of capital that includes savings/credit, livestock, and rental earnings to account for Sub-Saharan setting. In that perspective, households capital ownership refers to two situations in our paper. The first is households that have an account in a financial institutions. We reasonably assume that these households are likely to have access to savings or credit, consistent with the evidence that remittances stimulate financial services that help households cope with the shock. The second situation is households that own livestock or receive rental earnings. Evidence that poor and rural households rely more on such

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assets as a coping mechanism instead of savings (Nikoloski, Christiaensen, and Hill, 2018), motivates our decision to include livestock and rental earnings in the capital mechanism test. Livestock can attenuate deterioration in household food security through their sale (Fafchamps, Udry, and Czukas, 1998). Some types of livestock can also provide food for households, especially during hard times. For instance, poultry and cattle can provide meat, milk, and eggs. As remittances ease budgetary constraints, some households might, theoretically, acquire more goods, including livestock. We believe assets such as livestock, or those generating rental earnings, are worth considering as a potential consumption smoothing mechanisms.

Nigeria arguably offers an appealing context to investigate the remittances-mitigating role of COVID-19 shock. On the one hand, the Nigerian economy is expected to be hardly affected due to economic vulnerabilities that prevailed even before the shock. In 2018, the country included the largest share of the extreme poor population in the Sub-Saharan Africa region, with 20% of this population living in Nigeria [4]. The country also faces critical challenges in terms of food security, as illustrated by its low food consumption score and high-calorie deficiency[5]. At the same time, COVID-19 has had a huge impact on Nigeria, with 161,737 confirmed cases[6] (38% of the total cases in West Africa) as of March 21, 2021. Forecasts suggest that the COVID-19 pandemic and the related disruptions may result in 5 million additional poor people [7] and also put more pressure on food systems that are already vulnerable. On the other hand, Nigeria ranks among the top 10 remittance-recipient countries in Sub-Saharan Africa[8]. Remittances represent considerable financial flow to beneficiary households, which may reduce poverty and inequality (Odozi, Awoyemi, and Omonona, 2010). Notably, they may also stimulate financial inclusion, which constitutes a potential mechanism for consumption smoothing. In Nigeria, there is evidence that remittances increase the likelihood of using formal financial services, such as deposit accounts and Internet/mobile banking (Ajefu and Ogebe, 2019).

We find that remittances can mitigate the negative consequences of the COVID-19 employment shock on Nigeria's food insecurity. Households receiving remittances seem to experience a lower deterioration of their food security compared with non-beneficiary households in the short run. The dramatic rise in food insecurity associated with the shock appears to be 100% offset by the remittances received. The mitigating effects of remittances decline over time, while the adverse impact of the shock persists. Interestingly, our results indicate that this mitigating effect may operate through the capital mechanism, notably financial inclusion, rental earnings, or livestock ownership. We find that the mitigating effect of remittances is significantly amplified when households have access to or hold capital. The heterogeneity of the remittance mitigating effect by remittance origin, residence area, and household poverty status is worth highlighting as well. The remittance cushion effect appears to have a greater impact for remittances from abroad than for domestic ones, as those from

[4] $https://openknowledge.worldbank.org/bitstream/handle/10986/34496/9781464816024_Ch1.pdf$

[5] https://ebrary.ifpri.org/digital/collection/p15738coll16/id/1248

[6] African Development Bank (March 2021), Weekly Data flach on COVID-19 in Africa: the situation as of Sunday, March 21, 2021.

[7] https://blogs.worldbank.org/opendata/impact-covid-19-coronavirus-global-poverty-why-sub-saharan-africa-might-be-region-hardest

[8] https://www.knomad.org/sites/default/files/2019-04/Migrationanddevelopmentbrief31.pdf

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abroad are considerably larger. Our results also suggest that remittances mitigate adverse shocks, mainly in rural areas and for non-poor households. Concerning poor households, there is evidence of a mitigating effect of remittances for those receiving international remittances. In the urban areas, our findings also indicate remittance mitigation effects only for households in capital cities (Lagos/Abuja) receiving international remittances.

The remainder of this paper is organized into four sections. Section 2 presents our data sources and variables. Section 3 describes our methodology, and Section 4 discusses our results and robustness tests. Section 5 provides conclusions arising from our findings.

## 2 Data sources and variables

### 2.1 Data and representativeness

This paper combines data from a pre-COVID-19 face-to-face survey and a post-COVID-19 phone survey to assess the mitigating role of remittances during adverse COVID-19 employment shock on Nigeria's food insecurity. The surveys are part of the World Bank's Living Standards Measurement Study - Integrated Surveys on Agriculture (LSMS-ISA). The LSMS-ISA data for Nigeria include the General Household Survey (GHS) conducted 2018-19. The GHS panel sample used in our study includes 4,976 households interviewed in two waves: during the post-planting period from July to September 2018 and during the post-harvest period in January/February 2019. This sample of households is nationally representative and spans the six geopolitical zones that divide up the country.

Additionally, to track the impact of the pandemic, the National Bureau of Statistics implemented the Nigeria COVID-19 National Longitudinal Phone Survey (COVID-19 NLPS-2020) on a nationally representative sample of households drawn from those interviewed in the 2018/2019 GHS wave 4. The extensive information collected in the GHS panel just over a year prior to the pandemic provides abundant background information on COVID-19 NLPS households, which can be leveraged to assess the differential impacts of the pandemic in the country.

Among the 4,976 households interviewed in the 2018 post-harvest timeframe, 4,934 (99.2%) provided at least one telephone number. Out of the full sample of households with phone numbers, 3,000 households were randomly selected for the NLPS. Of those contacted, 94% (1,950) completed phone interviews. These 1,950 households constitute the final successful sample and will be contacted in subsequent rounds of the survey. To create a balanced panel across rounds, we merged these households with the GHS panel 2019 data and retained those households with complete information in both rounds (N = 1,950).

To manage selection bias associated with nonresponse and potential attrition in a phone survey and to construct nationally representative statistics, appropriate sampling weights must be built and applied. The LSMS-ISA team constructed the sampling weights using the weights for the GHS panel as the basis, with further adjustment for the attrition issue in the phone survey. The weights

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for the final sample of households from the phone survey were calculated in several stages (see NBS and WB (2020) for details). These weights are shown to ensure the comparable distribution of observable characteristics (state, urban/rural, household size, sex of the household head, age, asset ownership, education, etc.) from the GHS panel and the COVID-19 phone survey.

Table 1 presents the weighted and unweighted summary statistics of selected variables and observable household characteristics in both rounds (pre-COVID and post-COVID). Analysis of the unweighted GHS Panel and NLPS-2020 columns shows how attrition or nonresponse can affect the statistics of household characteristics. The unweighed column of NLPS-2020 suggests that more households with a higher standard of living responded to the phone survey. These households are more likely to own certain goods such as regular mobile phones, smartphones, televisions, cars, and generators. Following the weighting adjustments and calibration, the weighted estimates obtained from the GHS panel and NLPS samples match very closely across all dimensions. The use of these weights reduces the unweighted differences markedly in the observable characteristics of the GHS panel sample and phone survey samples. This is encouraging, as most of these household characteristics are not expected to change significantly in such a short period. Consistent with Wooldridge (2007) and Korinek, Mistiaen, and Ravallion (2007), using these corrected sampling weights reduces attrition bias and provides appropriate and representative statistics.

Table 1: Sample composition : Pre-COVID-19 vs Post-COVID-19

| **Characteristic** | **Pre-COVID-19 (GHS-2019)** | | **Post-COVID-19 (NLPS 2020)** | |
|---|---|---|---|---|
| | Unweighted | Weighted | Unweighted | Weighted |
| Sample size (successful interviews) | 4976.0 | - | 1950.0 | - |
| Average household size (family size) | 5.3 | 5.5 | 5.5 | 5.5 |
| **Household head characteristics** | | | | |
| Female head (%) | 20.1 | 18.6 | 19.1 | 18.6 |
| Age of head (years) | 49.8 | 48.8 | 49.4 | 49.2 |
| Literate (%) | 72.8 | 74.4 | 79.4 | 74.4 |
| **Education level of head** | | | | |
| None (on no school) | 22.2 | 20.5 | 15.8 | 20.6 |
| Primary | 24.6 | 24.1 | 24.6 | 24.1 |
| Junior secondary | 4.3 | 4.0 | 4.4 | 4.0 |
| Senior secondary | 23.3 | 23.9 | 26.7 | 23.9 |
| Tertiary | 16.7 | 16.0 | 21.7 | 16.0 |
| Religious/other | 8.9 | 11.5 | 6.8 | 11.4 |
| **Asset ownership** | | | | |
| Regular mobile phone | 66.1 | 65.4 | 71.1 | 66.0 |
| Smart phone | 26.5 | 26.7 | 32.9 | 26.8 |
| Television | 45.5 | 45.1 | 55.3 | 48.1 |
| Refrigerator | 18.0 | 17.3 | 23.4 | 18.7 |
| Car | 9.8 | 9.6 | 12.5 | 9.4 |
| Generator | 26.3 | 24.6 | 32.4 | 24.4 |

Source : GHS-Panel wave 4 (2018/2019), COVID-19 NLPS 2020, Authors' calculations.

## 2.2 Variable definition and descriptive statistics

### COVID-19 employment shock

The variable used to measure COVID-19 employment shock is extracted from the section on employment in the COVID-19 NLPS 2020 baseline household questionnaire. In particular, we focus

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on: (1) if the respondent has been working before mid-March and, if not, (2) the main reason why the respondent stopped working. For all individuals responding "yes" to the first question, that is, they had been working before mid-March, we consider the following two reasons as representing employment shock: (1) Business/office closed due to coronavirus legal restrictions; and (2) not able to go to farm due to movement restrictions. This approach allows us to account for differences in the way households are affected by employment shocks due to COVID-19. Accordingly, our COVID-19 employment shock variable takes the value of 1 if any household member stopped working because his/her business/office was closed due to legal restrictions or he/she was unable go to the farm due to movement restrictions (Shocked household); it takes the value 0 in any other case (Not-shocked household).

Table 2 presents the characteristics of the two group of households. Unsurprisingly, Shocked households are more likely to live in urban areas (Lagos/FCT or other urban) compared with Not-shocked households. This result is expected as the COVID-19 pandemic and the movement-restrictions measures has started in urban areas. In line with the literature, we find that households engaged in non-farm family firms or wage work would experience more shocks than those working in agricultural activities. Moreover, results indicate that Shocked households are better endowed in terms of living standards and education than the Not-shockeded ones. The proportion of Shocked households in the top consumption quintile (23.8%) is significantly higher than Not-shockeded households (18.4%). Shocked households own on average more assets, particularly refrigerators and cars. Literacy rates and household heads with secondary and tertiary education are proportionally higher within Shocked households than Not-shockeded households. These findings are in overall consistent with the new profile of the poor population induced by COVID-19 (Freije-Rodriguez and Woolcock, 2020).

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**Remittances**

To create the remittance measurement variable, we use the GHS panel wave four and consider the post-harvest data from January and February 2019. The survey section on remittances is intended to capture remittances to household members aged ten years and older. We focus on the questions asking whether the individual received the following types of assistance from a non-household member in the past 12 months: monetary assistance and/or in-kind assistance. It should be noted that these two types of assistance are further grouped based on their origin in the questionnaire: "from abroad" and "from within Nigeria." Therefore, the remittance variable takes the value 1 if the individual received any assistance in the past 12 months, from abroad (international remittances) or from within Nigeria (domestic remittances), and 0 otherwise. Based on this individual-level information, we aggregate at the household level and define three groups. First, the "non-beneficiary remittance " group includes households without any remittance recipient member. Second, the "international remittance" group includes households with at least one international remittance beneficiary. Third, the "domestic remittance" group includes members receiving remittances originating solely within the country. Households with members receiving remittances from abroad and domestically are included in the "international remittances" group.

Table 2: Households characteristics at baseline (Post-harvest wave - 2018/2019)

| | Shocked (1) | Not-shocked (2) | Difference (1) - (2) | T-test |
|---|---|---|---|---|
| **Residence area** | | | | |
| Lagos/FCT | 3.9 | 2.4 | 1.5 | 2.0** |
| Other urban | 35.7 | 24.4 | 11.3 | 5.4*** |
| Rural | 60.4 | 73.4 | -13.0 | -6.1*** |
| **Socio-demographic characteristics** | | | | |
| Average household size | 5.6 | 5.5 | 0.1 | 1.2 |
| Female head (%) | 14.8 | 20.9 | -6.1 | -3.3*** |
| Age of head (years) | 46.5 | 50.7 | -4.2 | -6.1*** |
| Literate (%) | 80.5 | 77.3 | 3.2 | 4.8*** |
| **Education level of head (%)** | | | | |
| None (on no school) | 31.2 | 40.3 | -9.1 | -4.0*** |
| Primary | 20.2 | 26.3 | -6.1 | -3.1*** |
| Secondary | 29.8 | 19.1 | 10.7 | 5.5*** |
| Tertiary | 18.7 | 14.3 | 4.4 | 2.6*** |
| **Asset ownership (%)** | | | | |
| Regular mobile phone | 77.1 | 75.3 | 1.8 | 0.9 |
| Television | 48.6 | 47.8 | 0.8 | 0.4 |
| Refrigerator | 23.3 | 16.1 | 7.2 | 4.0*** |
| Car | 11.2 | 8.3 | 2.9 | 2.1** |
| Generator | 23.9 | 24.6 | -0.7 | -0.3 |
| **Working status (% Adults)** | | | | |
| Agricultural activities | 20.5 | 32.5 | -12.0 | -7.1*** |
| Non-farm family enterprise | 36.2 | 31.1 | 5.1 | 3.0*** |
| Wage work | 14.7 | 12.0 | 2.7 | 2.2** |
| **Consumption quintile (%)** | | | | |
| Q1 | 19.6 | 19.9 | -0.3 | -0.2 |
| Q2 | 20.4 | 19.7 | 0.7 | 0.4 |
| Q3 | 16.7 | 21.7 | -5.0 | -2.7*** |
| Q4 | 19.5 | 20.2 | -0.7 | -0.4 |
| Q5 | 23.8 | 18.4 | 5.4 | 2.8*** |
| **Observations** | 725 | 1225 | 1950 | |

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$

Source : GHS-Panel wave 4 (2018/2019), Authors' calculations.

Note: Estimates are adjusted by the weights accounting for non-contact and non-response.

Figure 1 presents remittance distribution by sending origin consistent with other data sources and literature. This gives us confidence that our data are reliable, despite the attrition and non-response issues highlighted previously. The results indicate that most of the households do not receive any remittances (68%[9]). This percentage is similar to the proportion of households reporting never receiving remittances provided in the Afrobarometer survey[10]. Furthermore, our findings show that the likelihood of households receiving domestic remittances (27.9%) is significantly higher than international remittances (4%). However, average international remittances are overwhelmingly higher than domestic ones. The average remittances from abroad are roughly 2.5 times those from within the country. The likelihood of international remittances is conditional based on International migration rates, which is relatively low (0.6% in 2013 [11]). We find a similar proportion if we switch

[9] of the finale sample (1,950 households)
[10] Afrobarometer survey based on the online data analysis tool.
[11] World Bank (2016), Migration and Remittances Factbook 2016

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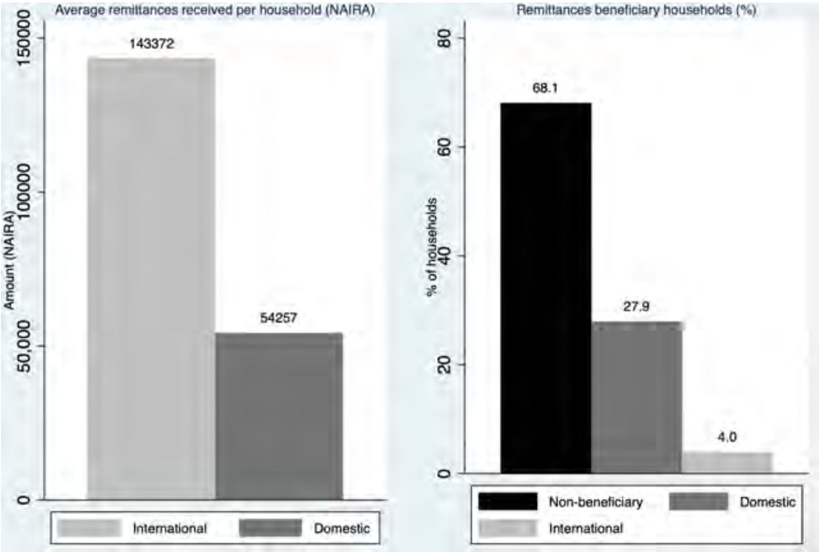
from household to individual scales when computing the likelihood of receiving remittances. The ratio between the number of international beneficiaries and the whole population, based on the data at hand, is estimated at 0.7%.

Figure 1: Remittances distribution by sending origin (2018/2019)




Source: GHS-Panel wave 4 (2018/2019), Authors' calculations.
Note: Estimates are adjusted by the weights accounting for non-contact and non-response.

**Food insecurity**

The food insecurity variable is constructed from data on food security captured consistently across rounds. This variable reflects household food shortage situations based on three yes/no questions related to the participants' last 30 days. These situations are: (1) skipping a meal because there was not enough money or other resources to get food; (2) ran out of food because of a lack of money or other resources; and (3) went without eating for a whole day because of a lack of money or other resources. To construct the variable, we took the following two steps. Step 1: transforming each of these situations into dummy variables; and Step 2: for each household, calculating the sum of the values of the three dummy variables constructed in Step 1. This procedure yields a food insecurity variable score that takes the following values: 0, 1, 2, and 3. In the case where the household replies no to all three situations, 0 is assigned. In the case where the household responds yes to only one of the situations, 1 is assigned; 2 is assigned if the households responds yes to two of the situations; and 3 is assigned if the household responds yes to all three situations. Consequently, the higher the score, the higher the food shortage that household faces.

Figure 2 presents the food insecurity score over the survey waves covering the period before the shock (2018 plantation and 2019 harvest) and after (May 2020). Overall, the food insecurity score increased sharply during COVID-19 for both household samples, those receiving remittances and

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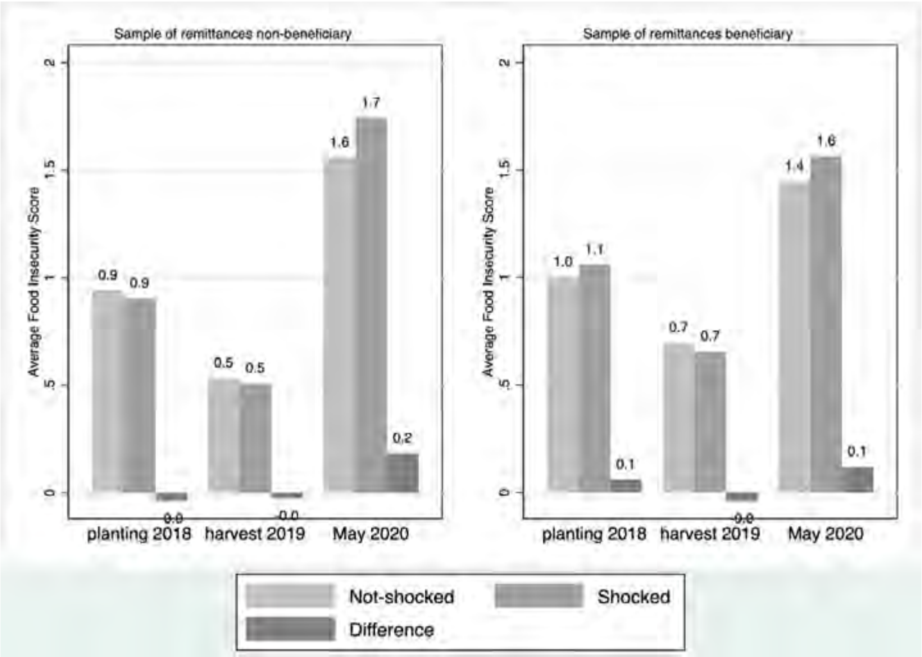
those that did not. The increase is even higher for households not receiving remittances, especially those who are shocked. While the food insecurity score of non-beneficiary shocked households is lower than that of Not-shocked households in the period before the shock, the reverse is observed after the shock. If both groups experience a significant increase in food insecurity, food insecurity is even higher for the Shocked households than their Not-shocked counterparts. Nonetheless, the difference in food insecurity between Shocked and Not-shocked households seems lower within the recipient subgroup, suggesting that remittances may cushion the negative consequences of this subgroup's shock.

Figure 2: Average Food Insecurity Score over time




Source: GHS-Panel wave 4 (2018/2019), COVID-19 NLPS 2020, Authors' calculations.
Note: Estimates are adjusted by the weights accounting for non-contact and non-response.

## 3 Methodology

To examine the mitigating role of remittances during COVID-19 employment shock on food insecurity, we use a **DiD** model specified in equation (1). All estimates are adjusted by the weights accounting for noncontact and nonresponse. Consistent with Wooldridge (2007) and Korinek, Mistiaen, and Ravallion (2007), using this corrected sampling weight will allow limiting attrition bias based on the assumption that data are randomly missing conditional on the observables used to compute the weights:

$$y_{ht} = \alpha + \beta_0 \ shock_h \times post_t + \beta_1 \ shock_h \times post_t \times \mathbb{1}_{remittances_h} + \delta_h + \mu_t + \epsilon_{ht} \tag{1}$$

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Where $y_{ht}$ represents the food insecurity outcome of the household h in period t. $\alpha$ is the constant term. $\delta_h$ and $\mu_t$ are household and time-fixed effects to control for time-invariant and trend-associated omitted unobservable heterogeneity, respectively. $Shock_h$ is a dummy variable indicating whether any member of the household stopped working due to coronavirus legal restrictions or was not able to farm due to movement restrictions. $post_t$ is a dummy variable taking the value 1 for the post-COVID-19 rounds and 0 for the pre-COVID-19 rounds. The coefficient $\boldsymbol{\beta_0}$, associated with the shock variable, is expected to be positive $(\boldsymbol{\beta_0 > 0})$, as it captures the adverse impact of the shock on food insecurity. The household-level defintion of the shock is more precise than the exposure to COVID-19[12] or lockdown measured at the state level, as used by Amare et al. (2020). All households in a given area are not exposed to the COVID-19 shock in the same way, as they do not necessarily comply with lockdown measures. Compliance with lockdown measures depends on poverty, trust in government, and economic/fiscal support measures (Bargain and Aminjonov, 2020a,b; Akim and Ayivodji, 2020).

$\mathbb{1}_{remittances_h}$ is a dummy indicating whether the household received remittances during the 12 last months. The coefficient $\boldsymbol{\beta_1}$, associated with the interaction term, is our parameter of interest. This coefficient measures the mitigating effect of remittances on adverse shocks on food insecurity. Therefore, the hypothesis of the remittances' mitigating role is whether $\boldsymbol{\beta_1}$ is negative, that is, $(\boldsymbol{\beta_1 < 0})$:

$$\beta_1 = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 0\right] \quad (2)$$

We focus primarily on remittances before the COVID-19 shock occurs for identification purposes. The DiD method requires avoiding a variable affected by the shock among the explanatory ones. Current remittances are likely to be affected by the COVID-19 shock and subsequent government lockdown measures. Remittance inflows in Sub-Saharan Africa are projected to decline by 8.8% in comparison with 2019, mainly due to COVID-19 shocks (Ratha et al., 2020). As a consequence of the COVID-19 shock, migrants are likely to experience earning losses in the destination location, which may negatively affect their ability to send money back home. Government measures enacted in both destination and origin locations, such as the business shutdown[13]/travel bans, are also likely to affect remittances. Evidence from High-Frequency Household surveys supports these forecasts. Of Nigerian households, 72% receiving remittances report experiencing a decrease in remittances in 2020[14]. Similarly, Ratha et al. (2020) find a decline in Nigerian remittance inflows by more than 45% in comparison with 2019.

The DiD identification relies on the fundamental assumption of a parallel trend. In our case, this assumption means that food insecurity in a household shocked and not-shocked would have evolved in tandem in the absence of the shock. As this counterfactual situation is unobservable, we conduct a validity check that compares the food insecurity trend among both the shocked and

[12] Measured as the number of cases
[13] Including remittances service providers
[14] World Bank. "COVID-19 High-Frequency Monitoring Dashboard".The World Bank Group. Washington, DC.

not-shocked groups prior to the shock. We reinforce the identification of the remittance-mitigating effect by conducting a placebo test. We run a placebo regression to ensure that no spurious effect drives the remittance-mitigating effect. This test consists of re-estimating the regression as specified in equation 1, but over the period preceding the shock, meaning in planting 2018 and harvest 2019.

We hypothesize that the capital channel, including savings/credit, livestock, and rental earnings, is an essential mechanism through which the mitigating role of remittances may operate. Relying on savings represents the second most reported coping mechanism [15] (29% of households), highlighting the importance of savings as a coping strategy. Given the data evidence and literature findings suggesting that remittances can stimulate financial services (savings or credits) by relaxing household budgetary constraints (Anzoategui, Demirgüç-Kunt, and Pería, 2014; Ambrosius and Cuecuecha, 2016; Ajefu and Ogebe, 2019), we can reasonably expect that households leveraging remittances to access such financial capital are more able to smooth their consumption. Instead of using savings or asking for credit, rural households may rely on their assets as a coping mechanism (Nikoloski, Christiaensen, and Hill, 2018). As remittances release budgetary constraints, households are likely to buy more assets, such as livestock or equipment/land, generating rental earnings. Consequently, households with more capital are less likely to suffer from food insecurity during the COVID-19 shock. While $\beta_1$ captures the overall remittance mitigating effect, we propose to decompose this effect based on the access to capital to shed light on the capital mechanism. We investigate the capital mechanism formally using the following equations:

$$y_{ht} = \tilde{\alpha} + \tilde{\beta}_0 \ shock_h \times post_t + \sum_{j=1}^{3} \tilde{\beta}_j \ shock_h \times post_t \times \mathbb{1}_{group\ =\ j} + \tilde{\delta}_h + \tilde{\mu}_t + \epsilon_{ht} \tag{3}$$

Where $j = \ 0, \ 1, \ 2, \ 3$ represents four subgroups of households. The **first group** represents the reference group and comprises households with no capital or remittances ($j = 0$). The coefficient $\boldsymbol{\tilde{\beta}_0}$ is expected to be positive $\boldsymbol{(\tilde{\beta}_0 > 0)}$, as it captures the impact of the shock on households with no capital or remittances. This group is supposed to be the most vulnerable to the shock. The **second group**, which is our primary interest group, comprises households that simultaneously hold or access capital and receive remittances ($j = 1$). The coefficient associated with this latter group, i.e. $\boldsymbol{\tilde{\beta}_1}$, is the parameter that tests the capital mechanism hypothesis of the remittance mitigation effect:

$$\tilde{\beta}_1 = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=1\}} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right] \tag{4}$$

The capital mechanism relies on the following hypothesis test: $\boldsymbol{\tilde{\beta}_1 \ < 0}$. The intuition is that the attenuating role of the remittances operates through the capital if accessing or holding capital amplifies its mitigating effect. In other words, the remittance mitigation effect is even higher when the household owns capital.

[15] Nigeria National Bureau of Statistics, The World Bank. 2020. COVID-19 impact monitoring, baseline report. https://microdata.worldbank.org/index.php/catalog/3712/download/48362

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The two following groups account for potential confounding mechanisms of the mitigating effect of remittances, different from the capital mechanism. The **third group** comprises households not-receiving remittances with capital ($j = 2$). The coefficient $\tilde{\boldsymbol{\beta}}_{\mathbf{2}}$ captures potential mitigating effects related solely to the capital that are not related to remittances. Finally, the **fourth group** comprises households receiving remittances but with no capital ($j = 3$), to rule out other mechanisms contributing to the mitigation effect of remittances not operating through the capital. The coefficient associated with this group is $\tilde{\boldsymbol{\beta}}_{\mathbf{3}}$, assumed to be negative ($\tilde{\boldsymbol{\beta}}_{\mathbf{3}} < \mathbf{0}$). $\tilde{\boldsymbol{\beta}}_{\mathbf{3}}$ captures the presence of other mechanisms driving the mitigation effect of remittances. For instance, households may use part of the remittances to buy inputs instead of investing in physical capital such as machinery. Household productivity may then increase so that when a shock occurs, they may be more able to better cope with the shock. The relationship between the parameters $\beta_1$ and $\tilde{\beta}_i, i = 0, 1, 2, 3$ is provided in Appendix A.

# 4 Results

## 4.1 Overall mitigating effect of remittances during COVID-19 employment shock

Table 3 shows the mitigating effect of remittances during COVID-19 employment shock on food insecurity. The results indicate that households that receive remittances experience less food insecurity. While the COVID-19 shock tends to increase the food insecurity score, remittances of any origin mitigate the shock's adverse effects (column 2). Food insecurity increases during the shock by 0.29, for not-receiving households. However, the shock appears to be offset entirely or absorbed when the households receive remittances, as the food insecurity score is roughly zero for remittance beneficiaries (0.29-0.33). The literature on migration insurance tends to support significant remittance mitigation of this magnitude. For instance, Beuermann, Ruprah, and Sierra (2016) find similar magnitudes in Jamaica. Although interested in an entirely different shock, they indicate that remittances absorb a 100% of an adverse health shock on household consumption. The remittance mitigation effect is also relatively sizable in the Philipines. Yang and Choi (2007) find that international remittances replace 60% of the decline in household income resulting from rainfall shock. Furthermore, our findings highlight the heterogeneity of remittance mitigation effects regarding their origin. While domestic remittances allow households to completely cover the adverse shock effect (-0.29 + 0.29; column 5), the mitigating effect of International remittances absorbs the adverse shock effect and significantly exceeds it. The mitigating effect of international remittances is double that of domestic remittances (columns 3, 4, and 5). The high average amount of remittances from overseas compared to those within-country might explain this.

Our results are in line with the global literature, especially regarding adverse shocks to food insecurity. There is evidence of an increase in food insecurity due to the COVID-19 shock in Kenya and Uganda, as well as in Nigeria. We find that overall, the shock increases the food insecurity score by 0.19 (column 1). This represents an increase of 25% in comparison with the baseline level (0.76),

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which is comparable to what has been found in the literature. Using the same data from Nigeria settings, Amare et al. (2020) findings show that lockdown increases the likelihood of running out of food by 26.8%. Adjognon, Bloem, and Sanoh (2020) find that in instances of shock, food insecurity increases in the Mali urban area by approximately 20% compared with the baseline mean.

Although not studying the actual impact of the COVID-19 shock, Balde, Boly, and Avenyo (2020) investigate factors associated with the difficulty of meeting basic needs during the COVID-19 pandemic. Their results suggest that informal workers tend to experience more challenges in meeting their basic needs. However, their results suggest also informal workers receiving remittances are less likely to experience challenges in meeting their basic needs in Senegal, but not in Mali and Burkina Faso. Even though their results may suffer from sample representativeness issues, they provide suggestive evidence of the mitigating effect of remittances on the COVID-19 shock in Senegal. In the Nigeria setting, Amare et al. (2020) study the differential impact of lockdown measures on various livelihoods, including remittances and assistance receipt. Their findings suggest a lower lockdown adverse effect on food insecurity in households that rely on remittances and government assistance. However, actual remittance mitigation effects cannot be disentangled because they pool remittances and government assistance.

Table 3: Remittances' mitigating effect

| Dependent variable<br>Food insecurity score | (1) | (2) | (3) |
|---|---|---|---|
| Lockdown-due business closure | 0.19** | 0.29*** | 0.29*** |
| | (0.08) | (0.10) | (0.10) |
| All remittances 2018/2019<br>× Lockdown-due business closure | – | -0.33*** | – |
| | | (0.12) | |
| International remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.69*** |
| | | | (0.26) |
| Domestic remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.29** |
| | | | (0.13) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.96*** | 0.96*** | 0.96*** |
| | (0.02) | (0.02) | (0.02) |
| Observations | 5850 | 5850 | 5850 |
| Adjusted $R^2$ | 0.243 | 0.245 | 0.245 |
| Food Insecurity Score Baseline Mean | | 0.76 | |

Robust Standard errors in parentheses; $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$;
Note: Food Insecurity Score Baseline Mean corresponds to the weighted average over planting 2018 and harvest 2019 periods.

## 4.2 Heterogeneous effects

The heterogeneous impact of the COVID-19 shock documented in the literature raises the question of whether the mitigation effect of remittances is also heterogeneous. The effect of the COVID-19 shock on food insecurity is notably higher among poor populations (Amare et al., 2020). In urban

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areas, the results depend on the context. Adjognon, Bloem, and Sanoh (2020) find a sharp increase in food insecurity in Bamako (Mali), while Amare et al. (2020)) suggest no differential effect of the shock between urban and rural areas on food security. In other words, the impact of the shock on food insecurity is similar in rural and urban areas[16], unlike in Mali. To examine this in Nigeria, we investigate the heterogeneity of remittance mitigation effects in both residential areas and across poverty dimensions.

Table 4 presents the heterogeneity of the mitigating effect of remittances on households by residence area. We operationalize the heterogeneity analysis with a triple interaction among the shock variables, remittance receipt, and household residence area. We consider households living in rural areas with no remittances as the reference group. Our results suggest a strong remittance-mitigating effect in rural areas. Indeed, we find a significant overall increase in food insecurity among households in rural areas with no remittances (0.28; column 1). However, this adverse shock seems to be considerably attenuated by remittances in those areas (-0.39; column 1). The cushioning effect of international remittances (-1.26; column 2), unsurprisingly, is higher than that of remittances originating within the country (-0.32; column 3). Estimates fail to validate the mitigating effect of remittances in urban areas, except in Lagos, where we see a mitigating effect from international remittances.

The weak mitigating effect in urban areas is probably due to better underlying resilience or better access to other coping mechanisms that make these residents less reliant on remittances. For instance, market imperfections, such as credit constraints, are likely to be more pronounced in rural areas than in urban areas. Consequently, we can reasonably expect remittances to mitigate the shock impact in more financially constrained environments such as rural areas than in urban areas. Urban households are likely to access financial services, such as credit and savings, independent of remittances. They are then better able to smooth their consumption without relying on remittances. In contrast, in rural areas, credit constraints are pronounced, and households are expected to rely on remittances. Another reason for this weak mitigating effect may be the more stringent lockdown measures compared with rural areas, as the pandemic first started in large towns. Adjognon, Bloem, and Sanoh (2020) provides evidence of a significant decrease in human mobility in Bamako, Mali's capital, relative to rural areas following the lockdown. Given the intensity of mobility restrictions, households in urban areas may be exposed more to significant income losses, which may increase the likelihood of suffering from food insecurity.

Table 5 presents the heterogeneity results based on the poverty status measured in the 2018/2019 wave. We use the triple interaction among the shock, remittance status, and poverty status[17] to investigate the poverty differential effects of the mitigating role of remittances. Our reference group comprises poor households with no remittances. The results indicate that remittances can mitigate the negative effects of the shock, mainly for non-poor households. The mitigating effect of pooled remittances is estimated at -0.46 (column 1). Consistent with the previous results, we find a larger

[16] We find similar results that we can provide upon request

[17] All the households in the two first consumption quintiles, which represent the 40% bottom consumption distribution, are considered as poor.

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Table 4: Remittances' mitigating effect : heterogeneity regarding residence area

| | Definition of remittances | | |
|---|---|---|---|
| | Pooled remittances (1) | International remittances (2) | Domestic remittances (3) |
| Lockdown-due business closure | 0.28** | 0.23* | 0.26** |
| | (0.12) | (0.12) | (0.12) |
| Lockdown-due business closure x Residence area x Remittances (Ref: remittances = No, Rural = Yes) | | | |
| Closure = Yes × (remittances = Yes, Lagos/FCT = Yes) | -0.30 | -0.55* | -0.19 |
| | (0.24) | (0.30) | (0.30) |
| Closure = Yes × (remittances = No, Lagos/FCT = Yes) | -0.05 | -0.05 | -0.05 |
| | (0.36) | (0.36) | (0.36) |
| Closure = Yes × (remittances = Yes, Other urban = Yes) | -0.21 | -0.23 | -0.21 |
| | (0.18) | (0.41) | (0.19) |
| Closure = Yes × (remittances = No, Other urban = Yes) | 0.02 | 0.02 | 0.02 |
| | (0.17) | (0.17) | (0.17) |
| Closure = Yes × (remittances = Yes, Rural = Yes) | -0.39** | -1.26*** | -0.32* |
| | (0.16) | (0.28) | (0.17) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.96*** | 0.93*** | 0.95*** |
| | (0.02) | (0.03) | (0.02) |
| Observations | 5850 | 4200 | 5559 |
| Adjusted $R^2$ | 0.245 | 0.273 | 0.252 |
| Sample | 1950 households | 1303 Non-Benef + 97 Int. Remit. | 1303 Non-Benef + 550 Dom. Remit. |

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

+ FCT stands for Federal Capital Territory

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mitigating effect of international remittances than domestic ones. Concerning poor households, we find evidence of mitigating effects only for remittances coming from abroad (-0.93; column 2). The domestic remittance mitigation is almost zero. This indicates that domestic remittances are likely to mitigate shocks only for better-off households. In contrast, international remittances can mitigate the negative consequences of a shock on food insecurity for the entire population.

Table 5: Remittances' mitigating effect: heterogeneity regarding poverty status in 2018/2019

| | Definition of remittances | | |
|---|---|---|---|
| | Pooled remittances (1) | International remittances (2) | Domestic remittances (3) |
| Lockdown-due business closure | 0.36** (0.14) | 0.31** (0.14) | 0.34** (0.14) |
| Lockdown-due business closure x Poor status (2018/2019) x Remittances (Ref: remittances = No, = Yes) | | | |
| Closure = Yes × (remittances = Yes, Poor = Yes) | -0.14 (0.23) | -0.93*** (0.24) | -0.08 (0.24) |
| Closure = Yes × (remittances = No, Poor = Yes) | -0.14 (0.17) | -0.14 (0.17) | -0.14 (0.17) |
| Closure = Yes × (remittances = Yes, Poor = No) | -0.46*** (0.17) | -0.73** (0.31) | -0.43** (0.17) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.96*** (0.02) | 0.93*** (0.03) | 0.95*** (0.02) |
| Observations | 5850 | 4200 | 5559 |
| Adjusted $R^2$ | 0.246 | 0.273 | 0.253 |
| Sample | 1950 households | 1303 Non-Benef + 97 Int. Remit. | 1303 Non-Benef + 550 Dom. Remit. |

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

+ Households in the 1st and 2nd quintile of consumption are considered as poor.

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### 4.3 Robustness checks

### 4.4 Sensitivity of the estimates to both Shock and Food Insecurity definitions

The primary shock definition used in this paper, lockdown-due business closure, is likely to capture a limited scope of the COVID-19 employment shock. It only captures the COVID-19 employment shock created by stringent restriction movement measures. However, COVID-19 may affect employment through the aforementioned "health channel" as well. A household member may get sick from COVID-19, causing him/her to stop working. To prevent themselves from getting sick, households may intentionally reduce their demand for goods/services requiring interpersonal contacts, resulting in business closures due to low demand. We test the robustness of our results by considering an alternative measure of the shock, that is, the number of confirmed COVID-19 cases by state. This measure of the shock is expected to capture the broad channels of the COVID-19 shock on employment. It measures household exposure to the pandemic and is generally used in the literature, for instance, by Amare et al. (2020).

Table 6 presents the the mitigation effect of remittances during COVID-19 exposure on food insecurity. We adopt two definitions of COVID-19 exposure. First, COVID-19 exposure is measured by the number of confirmed cases (log scale) at the state level. Second, we measure the shock based on a dummy variable distinguishing high[18] exposure to COVID-19 versus low exposure using the number of confirmed cases. Our results are consistent with previous estimates. Remittances can cushion the negative effects of COVID-19 shock. The mitigating effect is relatively higher for remittances coming from abroad than for those originating domestically.

Table 6: Remittances' mitigating effect: robustness to shock definition

| Dependent variable<br>Food insecurity score | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Confirmed cases (log scale) | 0.04<br>(0.03) | 0.06***<br>(0.02) | 0.06***<br>(0.02) | – | – | – |
| COVID-19-exposure (**Ref** = **Low**) | | | | | | |
| High | – | – | – | 0.15*<br>(0.09) | 0.26***<br>(0.10) | 0.26***<br>(0.10) |
| Remittances × Confirmed cases | | | | | | |
| All remittances 2018/2019<br>× **Confirmed cases (log scale)** | – | -0.07***<br>(0.02) | – | – | – | – |
| International remittances 2018/2019<br>× **Confirmed cases (log scale)** | – | – | -0.10***<br>(0.03) | – | – | – |
| Domestic remittances 2018/2019<br>× **Confirmed cases (log scale)** | – | – | -0.06***<br>(0.02) | – | – | – |
| Remittances × COVID-19 exposure (Ref = Low) | | | | | | |
| All remittances 2018/2019<br>× **COVID-19-exposure = High** | – | – | – | – | -0.33***<br>(0.10) | – |
| International remittances 2018/2019<br>× **COVID-19-exposure = High** | – | – | – | – | – | -0.46***<br>(0.16) |
| Domestic remittances 2018/2019<br>× **COVID-19-exposure = High** | – | – | – | – | – | -0.30***<br>0.10 |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Constant | 0.96***<br>(0.02) | 0.96***<br>(0.02) | 0.93***<br>(0.03) | 0.95***<br>(0.02) | 0.96***<br>(0.02) | 0.96***<br>(0.02) |
| Observations | 5850 | 5850 | 5850 | 5850 | 5850 | 5850 |
| Adjusted $R^2$ | 0.241 | 0.248 | 0.248 | 0.241 | 0.246 | 0.246 |

Robust Standard errors in parentheses; $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$

We complete our robustness checks using two sensitivity tests of our results. First, to ensure that our results are robust to the outcome definition (food insecurity), we proxy the food insecurity situation with the likelihood of skipping a meal, running out of food, or going the whole day without eating. Each of these three outcomes relates to the questions used to compute the food insecurity score. We estimate the mitigation effect of remittances on each of the three questions used to compute the food insecurity score (table B.6). Overall, our results remain qualitatively the same,

[18] High COVID-19 exposure are states with a number of confirmed cases exceeding 62 cases (the median value).

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suggesting the remittances' mitigating effect during the shock.

Second, we test the sensitivity of our results to the correction of attrition using sampling weights. We estimate the remittances' mitigating effect without adjusting with the sampling weights (table 7). The coefficients are roughly the same, although small differences in the expected direction are notable. We find that the magnitude of the remittance mitigation effect is slightly lower, in absolute value, than the weight-adjusted estimates (table 3). For instance, we find that the mitigating effect of international remittances is -0.60 (table 7, column 5), when sampling weights are ignored, versus -0.69, in the case where weights are accounted for (table 3, column 5). This result suggests that the mitigating effect of remittances is likely to be downward-biased when attrition is not corrected, which is expected. Indeed, positive selection is likely to drive attrition, as previously highlighted. Table 1 indicates that higher-educated and wealthier households are more likely to be contacted and included in the post-COVID-19 survey sample. The mitigating effect is likely to be underestimated based on a sample of wealthier households because they are expected to be better able to cope with adverse shocks. Although our results seem robust regarding the correction of attrition, we could worry about potential unobservables. However, we are confident that the results remain unchanged given that our estimates represent a lower bound.

Table 7: Remittances' mitigating effect: Robustness to sampling weights

| Dependent variable<br>Food insecurity score | (1) | (2) | (3) |
|---|---|---|---|
| Lockdown-due business closure | 0.20*** | 0.30*** | 0.30*** |
| | (0.06) | (0.06) | (0.06) |
| All remittances 2018/2019<br>× Lockdown-due business closure | – | -0.30*** | |
| | | (0.09) | |
| International remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.60*** |
| | | | (0.19) |
| Domestic remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.25** |
| | | | (0.10) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.89*** | 0.89*** | 0.89*** |
| | (0.02) | (0.02) | (0.02) |
| Observations | 5889 | 5889 | 5889 |
| Adjusted $R^2$ | 0.238 | 0.240 | 0.241 |

Robust Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$;
Note: Estimates are not adjusted to corrected weights in order to gauge the robustness of the results to the sampling weights.

## 4.5 Parallel trend and Placebo tests

To confirm the mitigation effect of remittances, we test the plausibility of a parallel trend assumption and conduct a placebo test. The figure B.1 provides visual evidence of the plausibility of the common trend hypothesis. Before the COVID-19 shock occurred in May 2020, food insecurity in

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the two groups of households seems to have evolved in tandem. We complete this visual comparison with regression analysis, including the impact of the shock on food insecurity in the period before May 2020. A statistically zero effect of the shock treatment (-0.02) is observed in this period, suggesting that the parallel trend assumption is plausible (table B.2). In addition to the parallel trend hypothesis, the placebo test tends to validate our estimations. The results of the placebo test are presented in Table 8. As expected, the shock has zero effect on food insecurity, and there is no evidence of remittance-mitigating effect across the diverse specifications.

Table 8: Placebo test of the remittances' mitigating effect

| Dependent variable<br>Food insecurity score | (1) | (2) | (3) |
|---|---|---|---|
| Lockdown-due business closure | -0.02<br>(0.08) | -0.02<br>(0.09) | -0.02<br>(0.09) |
| All remittances 2018/2019<br>× Lockdown-due business closure | – | -0.01<br>(0.13) | – |
| International remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.56<br>(0.34) |
| Domestic remittances 2018/2019<br>× Lockdown-due business closure | – | – | 0.06<br>(0.14) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.96***<br>(0.02) | 0.96***<br>(0.02) | 0.96***<br>(0.02) |
| Observations | 3926 | 3900 | 3900 |
| Adjusted $R^2$ | 0.108 | 0.097 | 0.100 |

Robust Standard errors in parentheses; $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$;
Note: Food Insecurity Score Baseline Mean corresponds to the weighted average over planting 2018 and harvest 2019 periods.

### 4.6 Persistence of the shock and remittances' mitigating effect

Governments worldwide, including the Nigerian government, have enacted many social safety net programs to help households cope with COVID-19 negative consequences. This support may help households recover their businesses. They also have eased movement restriction measures over time[19]. Table B.5 shows a decrease in the incidence of shock over the period May to November 2020 from 37% to only 2%. These subsequent measures may raise some identification issues. For instance, our estimates may be downward-biased because the impact of the shock could be more critical in the absence of these support programs. To avoid this potential identification problem, we primarily exploit the sample covering the period from planting 2018 to the early period (April–May 2020) of the COVID-19 shock (hereafter "short panel"). We assume that the relief program effects are limited to this period. For the first time, the government announced the delivery of up to 70,000 tons of grain on May 12, 2020. Furthermore, this period coincided with the highest level of

[19] Based on Government announcements of early May, 2020: https://nairametrics.com/2020/04/27/fill-speech-of-president-buhari-on-covid-19-pandemic/

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movement restriction measures.[20].

We conduct a robustness check by expanding our sample (hereafter "extended panel") and include additional waves from June, August, and November 2020 (table 9). The results remain qualitatively consistent with previous findings based on the short panel (table 3). Employment shocks significantly increase household food insecurity, and remittances can mitigate this adverse effect. However, the magnitudes of the coefficients are lower in the extended panel. The impact of the shock on households with no remittances is lower, ranging from 0.19 to 0.26, in comparison with the short panel estimates (coefficients vary from 0.20 to 0.30). The remittance mitigation effect is also lower in the extended panel. For instance, the magnitude of the mitigation of the pooled remittances is estimated at -0.22 in the extended panel (table 9) versus -0.30 in the short panel (table 3). While these results may suggest a potential downward bias in our estimates due to government relief programs, the results remain unchanged overall[21]. Notably, the results ensure our strategy that focuses on the impact in the early period of the shock, which is the first round of the COVID-19 survey in May 2020.

Table 9: Remittances mitigating effect (extended panel)

| Dependent variable<br>Food insecurity score | (1) | (2) | (3) |
|---|---|---|---|
| Lockdown-due business closure | 0.19*** | 0.26*** | 0.26*** |
| | (0.05) | (0.06) | (0.06) |
| All remittances 2018/2019<br>× Lockdown-due business closure | – | -0.22*** | – |
| | – | (0.08) | – |
| International remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.42** |
| | – | – | (0.17) |
| Domestic remittances 2018/2019<br>× Lockdown-due business closure | – | – | -0.19** |
| | | | (0.09) |
| Time fixed effects | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes |
| Constant | 0.88*** | 0.88*** | 0.88*** |
| | (0.02) | (0.02) | (0.02) |
| Observations | 11213 | 11213 | 11213 |
| Adjusted $R^2$ | 0.185 | 0.186 | 0.186 |

Robust Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$

* The sample includes the additional waves of June, August and November 2020 rounds

** Estimates are unweighted.

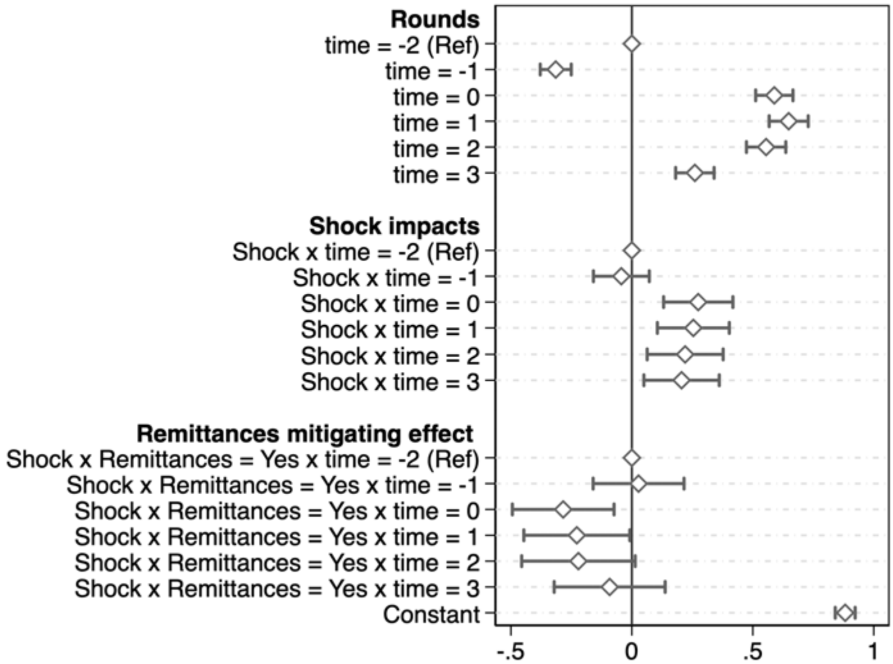
The extended panel sample offers the opportunity to investigate the persistence of the COVID-19 employment shock over time and the lasting mitigation effect of remittances. Figure 3 shows the regression coefficients estimating the impacts of the shock and remittance mitigation effect over time. The results show that the negative shock effects are likely to persist over the considered period, while the mitigating role of the remittances seems effective in the early stages of the shock.

[20]https://www.ifpri.org/project/covid-19-policy-response-cpr-portal

[21]All estimates with the extended panel are unweighted and are more likely to be subject to attrition bias. Hence, results must be taken with caution and can provide only suggestive evidence

We find that the COVID-19 employment shock increases food insecurity in May 2020 (time = 0) and remains significantly high over the following periods from June 2020 (time =1) to November 2020 (time = 3). The remittances appear to significantly cushion the shock's adverse effect during the three rounds from May 2020 (time = 0) to August 2020 (time = 2). In November 2020, the remittance mitigation effect became insignificant, while the shock's adverse effect persisted over the entire period (from time = 0 to time = 3). The downward pattern of remittance mitigation is expected because household capital, especially savings, may be insufficient to hold out in the long run. Indeed, household savings are likely to decline over time because of the employment shock, preventing the renewal of savings stock.

Figure 3: Lasting effects of shock and remittances' mitigating role over time




Source: GHS wave 4 (2018/2019), COVID-19 NLPS 2020, Authors' calculations.
* The sample includes the additional waves of June, August and November 2020 rounds.
** Confident Intervals are estimated at 95% level

## 4.7 Capital mechanism test

The capital mechanism is tested by considering the broad definitions of household capital, including three dimensions. The **first dimension** is the ownership of an account in a financial institution. We assume that households holding an account in a financial institution [22] are likely to access formal financial services such as savings or financial credits that make them less capital-constrained. Subsequently, they are likely to smooth their consumption through access to such services compared with households with no formal financial services. The **second dimension** is informal financial

[22] Commercial bank, micro-finance institution, cooperative society

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services through participation in rotating savings and credit association. The **third dimension** of capital is ownership of livestock or rental earnings. Indeed, households may hold capital in forms other than money or may even receive non-labor income, which may prevent them from facing food insecurity. For instance, households may hold assets such as livestock that they can sell or consume at the time of the COVID-19 shock. Some households may earn non-labor income from land or other productive assets (tractors, trailers, etc.) that they rent out.

Table 10 presents the capital mechanism hypothesis test results based on Equation 3. The findings support the assumption that capital represents a channel through which the mitigating effect of the remittances can operate. The remittance mitigation effect appears to be amplified when households have access to any form of the considered capital (-0.44; column 1). The capital mechanism seems to be driven mainly by formal financial inclusion, defined as having an account in a financial institution, livestock ownership, or rental earnings. The interaction effect of informal capital and remittances is insignificant, failing to validate the capital hypothesis mechanism for this type of capital. The capital mechanism hypothesis remains consistent when considering the remittances' origins (tables B.3). Overall, the results indicate that the cushioning effects of both international and domestic remittances are higher for households with access to or those holding, capital.

The other interaction coefficients consistently support the savings amplifying effect of the mitigating role of remittances. The coefficients associated with the group of remittance recipient households with no capital or those with a negative expected sign are not significant. In other words, recipient households with no capital appear to be unable to cope with the shock. This result may suggest that households fail to properly smooth their consumption when remittances do not contribute to reinforcing household capital. Similarly, households with capital, but not receiving remittances, seem unable to significantly mitigate the shock's negative consequences. This may indicate that the capital held by these households is insufficient to mitigate the shock.

The literature points out the differential impact of the shock according to the activity sector, which may raise some identification concerns. Wage workers seem to be less affected by the COVID-19 shock and lockdown measures (Adams-Prassl et al., 2020; Balde, Boly, and Avenyo, 2020; Amare et al., 2020). One potential explanation is that wage workers, especially those working in the formal sector, may continue to receive their salary even during the pandemic when businesses are shut down. Wage related activities are also likely to be operated remotely. In Nigeria, Amare et al. (2020) find that most wage workers are employed in the public sector and non-governmental organizations. Such individuals tend to have long-run contracts, which enables easier access to financial services, such as savings or credit, and subsequently, more capital. Evidence in the literature also indicates that farmers are less likely to experience deterioration in food security in comparison with other sectors(Kansiime et al., 2020), mostly because farmers rely less on market sources for food. The correlation between these underlying factors of employment and capital may represent confounding factors for the capital mechanism test.

We test the robustness of the capital mechanism by controlling for employment activity heterogeneity prior to COVID-19 (table 11). We revisit the capital mechanism test by interacting the

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Table 10: Capital channel hypothesis test of pooled remittances' mitigating effect

| | Definition of the capital | | | |
|---|---|---|---|---|
| | Pooled capital | Formal Financial Services | Informal Financial Services | Livestock ownership, Rental earnings |
| | (1) | (2) | (3) | (4) |
| Lockdown-due business closure | 0.40* | 0.48** | 0.44* | 0.49** |
| | (0.22) | (0.22) | (0.22) | (0.23) |
| **Lockdown-due business closure x Capital group (Ref: Capital = No, remittances = No)** | | | | |
| Closure = Yes × (Capital = Yes, remittances = Yes) | -0.44* | -0.54** | -0.10 | -0.91*** |
| | (0.23) | (0.24) | (0.26) | (0.27) |
| Closure = Yes × (Capital = Yes, remittances = No) | -0.15 | -0.26 | 0.01 | -0.03 |
| | (0.23) | (0.24) | (0.24) | (0.29) |
| Closure = Yes × (Capital = No, remittances = Yes) | -0.39 | -0.39 | -0.39 | -0.39 |
| | (0.38) | (0.38) | (0.38) | (0.38) |
| Time fixed effects | Yes | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes | Yes |
| Constant | 0.96*** | 0.97*** | 0.92*** | 1.02*** |
| | (0.02) | (0.03) | (0.03) | (0.04) |
| Observations | 5850 | 4671 | 3132 | 1995 |
| Adjusted $R^2$ | 0.245 | 0.213 | 0.253 | 0.217 |

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

time variable with three dummies capturing household employment activities during the harvest 2019 period. The coefficients associated with the capital mechanism test decrease when accounting for employment heterogeneity. The interaction effect between capital and remittances declines by approximately 0.05. This may suggest a potential upward bias in the capital mechanism test if the employment heterogeneity trend is not accounted for. As anticipated, this decline in the coefficients seems to be driven by wage employment. However, the findings remain robust, validating the capital mechanism test.

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Table 11: Capital channel hypothesis test of pooled remittances' mitigating effect: Robustness to control for employment activities

| | Definition of the capital | | | |
|---|---|---|---|---|
| | Pooled capital | Formal Financial Services | Informal Financial Services | Livestock ownership, Rental earnings |
| | (1) | (2) | (3) | (4) |
| Lockdown-due business closure | 0.37* | 0.44** | 0.41* | 0.49** |
| | (0.22) | (0.22) | (0.22) | (0.23) |
| **Lockdown-due business closure x Capital group (Ref: Capital = No, remittances = No)** | | | | |
| Closure = Yes × (Capital = Yes, remittances = Yes) | -0.39* | -0.48** | -0.12 | -0.86*** |
| | (0.23) | (0.24) | (0.26) | (0.27) |
| Closure = Yes × (Capital = Yes, remittances = No) | -0.09 | -0.19 | 0.03 | 0.01 |
| | (0.23) | (0.24) | (0.24) | (0.29) |
| Closure = Yes × (Capital = No, remittances = Yes) | -0.41 | -0.40 | -0.40 | -0.42 |
| | (0.38) | (0.38) | (0.37) | (0.38) |
| **Round x Employment activities in 2019 (Ref: Round = Planting 2018, Farm Activities = No)** | | | | |
| Round = Harvest 2019 × Farm Activities = Yes | -0.02 | -0.09 | 0.01 | -0.07 |
| | (0.07) | (0.09) | (0.10) | (0.13) |
| Round = May 2020 × Farm Activities = Yes | -0.00 | -0.03 | -0.06 | 0.02 |
| | (0.10) | (0.11) | (0.13) | (0.16) |
| Round = Harvest 2019 × Non-farm Business = Yes | 0.06 | -0.01 | 0.01 | -0.05 |
| | (0.07) | (0.08) | (0.10) | (0.12) |
| Round = May 2020 × Non-farm Business = Yes | 0.08 | 0.02 | 0.02 | -0.00 |
| | (0.09) | (0.10) | (0.13) | (0.16) |
| Round = Harvest 2019 × Wage employment = Yes | -0.14 | -0.18* | -0.01 | -0.17 |
| | (0.09) | (0.09) | (0.15) | (0.17) |
| Round = May 2020 × Wage employment = Yes | -0.37*** | -0.29** | -0.49*** | -0.45** |
| | (0.11) | (0.11) | (0.17) | (0.23) |
| Time fixed effects | Yes | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes | Yes |
| Constant | 0.96*** | 0.97*** | 0.92*** | 1.02*** |
| | (0.02) | (0.03) | (0.03) | (0.04) |
| Observations | 5850 | 4671 | 3132 | 1995 |
| Adjusted $R^2$ | 0.251 | 0.216 | 0.260 | 0.221 |

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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## 5 Conclusion

This study assesses the mitigation effect of remittances during the COVID-19 employment shock on Nigeria's food insecurity. Combining pre-COVID-19 and post-COVID-19 data, we implement the DiD approach to assess the cushioning impact of remittances. The results indicate that remittances are mitigating adverse COVID-19 employment shocks, especially in the short run. Remittances beneficiary households appear to experience significantly lower deterioration in their food security in the early stages of the shock. The findings also highlight some heterogeneity regarding the origin of remittances. Overall, the mitigating effect is higher for remittances coming from abroad than for those originating domestically. Furthermore, the mitigating effect of remittances appears to have the greatest impact on rural households and those that are non-poor. Concerning urban and poor populations, we find that only international remittances are cushioning the adverse shock to food insecurity. Interestingly, we find evidence that the mitigating effect is likely to operate through the capital mechanism. The remittance mitigation effect seems amplified when the household holds a bank account in a financial institution and capital in the form of livestock or rental earnings.

Our results highlight the crucial role that remittances play in mitigating the adverse consequences of a shock of magnitude such as that of the COVID-19 pandemic, especially in the early stages. Before the government enacts relief measures, remittances help households cope with the shock through the capital mechanism. This result is striking because remittances have not been predicted to play a role during the COVID-19 pandemic, as they are expected to decrease sharply owing to the pandemic outbreak in migration destination countries or locations. Consequently, our findings have the important policy implication that remittances may still represent a vital insurance source worth considering, especially in the post-pandemic context. Governments worldwide, along with the international community, are likely to rethink and revise national social protection strategies to provide more support to households and increase their resilience to adverse shocks. These strategies should include measures that incentivize remittance recipient households to channel them toward increasing household capital. Furthermore, remittance-provided protection should be considered complementary to existing social protection systems. Our findings support that remittances are likely to protect only a part of the population, mainly rural and non-poor households.

This paper has some limitations that pave the way for further investigations. We focus mainly on past remittances because our primary aim is to shed light on the ex-ante mechanism through the capital channel. However, remittances received during the shock, although lower than usual circumstances, may potentially help households cope with shock and contribute to the recovery and rebuilding required following the COVID-19 pandemic. For identification purposes, this raises challenges beyond this paper's scope. We leave this issue for future research.

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# Appendix

## A. Identification of the interest parameter $\beta_1$

From the equation 1, we have the following expressions :

$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 1\right] = \alpha + \beta_0 + \beta_1 + \delta_h + \mu_t$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 0\right] = \alpha + \beta_0 + \delta_h + \mu_t$$

$\hat{\beta}_1$ is the estimator of:

$$\beta_1 = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 0\right] \quad (5)$$

Using equation 3, we get the following decomposition :

$$E\left[y_{ht} \mid shock_h = 0, post_t = 0, \mathbb{1}_{\{group=0\}} = 1\right] = \tilde{\alpha} + \tilde{\delta_h} + \tilde{\mu_t}$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right] = \tilde{\alpha} + \tilde{\beta_0} + \tilde{\delta_h} + \tilde{\mu_t}$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=1\}} = 1\right] = \tilde{\alpha} + \tilde{\beta_0} + \tilde{\beta_1} + \tilde{\delta_h} + \tilde{\mu_t}$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=2\}} = 1\right] = \tilde{\alpha} + \tilde{\beta_0} + \tilde{\beta_2} + \tilde{\delta_h} + \tilde{\mu_t}$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=3\}} = 1\right] = \tilde{\alpha} + \tilde{\beta_0} + \tilde{\beta_3} + \tilde{\delta_h} + \tilde{\mu_t}$$

The following decomposition gives the expressions of parameters $\tilde{\beta_0}, \tilde{\beta_1}, \tilde{\beta_2}$, and $\tilde{\beta_3}$ in population:

$$\tilde{\beta_0} = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right] - E\left[y_{ht} \mid shock_h = 0, post_t = 0, \mathbb{1}_{\{group=0\}} = 1\right]$$
$$\tilde{\beta_1} = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=1\}} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right]$$
$$\tilde{\beta_2} = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=2\}} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right]$$
$$\tilde{\beta_3} = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=3\}} = 1\right] - E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right]$$

Suppose that:

$$E_1 = (shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 1)$$
$$E_0 = (shock_h = 1, post_t = 1, \mathbb{1}_{remittances_h} = 0)$$

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Using the below equations for event $E_1$n we have :

$$E[y_{ht} \mid E_1] = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=1\}} = 1\right] \times P(Capital = Yes \mid E_1) +$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=3\}} = 1\right] \times P(Capital = No \mid E_1)$$
$$E[y_{ht} \mid E_1] = (\tilde{\alpha} + \tilde{\beta}_0 + \tilde{\beta}_1 + \tilde{\delta}_h + \tilde{\mu}_t).P(Capital = Yes \mid E_1) + (\tilde{\alpha} + \tilde{\beta}_0 + \tilde{\beta}_3 + \tilde{\delta}_h + \tilde{\mu}_t).P(Capital = No \mid E_1)$$

In case of event $E_0$, we have:

$$E[y_{ht} \mid E_0] = E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=2\}} = 1\right] \times P(Capital = Yes \mid E_0) +$$
$$E\left[y_{ht} \mid shock_h = 1, post_t = 1, \mathbb{1}_{\{group=0\}} = 1\right] \times P(Capital = No \mid E_0)$$
$$E[y_{ht} \mid E_0] = (\tilde{\alpha} + \tilde{\beta}_0 + \tilde{\beta}_2 + \tilde{\delta}_h + \tilde{\mu}_t).P(Capital = Yes \mid E_0) + (\tilde{\alpha} + \tilde{\beta}_0 + \tilde{\delta}_h + \tilde{\mu}_t).P(Capital = No \mid E_0)$$

Combining these equations, our interest parameter $\beta_1$ in equation 1 is obtained by :

$$\begin{aligned}\beta_1 &= E[y_{ht} \mid E_1] - E[y_{ht} \mid E_0] \\ &= \tilde{\beta}_1.P(Capital = Yes \mid E_1) + \tilde{\beta}_3.P(Capital = No \mid E_1) - \tilde{\beta}_2.P(Capital = Yes \mid E_0)\end{aligned}$$

Finally, $\hat{\beta}_1$ is the estimator of $\tilde{\beta}_3 + \left(\tilde{\beta}_1 - \tilde{\beta}_3\right).P(Capital = Yes \mid E_1) - \tilde{\beta}_2.P(Capital = Yes \mid E_0)$ in sample.

## B. Supplemental tables and graphs

Table B.1: Sector of activity by shock statue (% Adults)

| Sector | Shocked (1) | Unshocked (2) | Difference (1) - (2) | T-stat |
|---|---|---|---|---|
| Agriculture | 0.31 | 0.68 | -0.37 | -1.32 |
| Mining | 0.22 | 0.01 | 0.21 | 2.58*** |
| Manufacturing | 0.84 | 0.64 | 0.20 | 0.75 |
| Professional/scientific/Technical | 0.30 | 0.52 | -0.22 | -0.95 |
| Electricity/water/gas/waste | 0.81 | 0.06 | 0.75 | 2.92*** |
| Construction | 0.88 | 0.75 | 0.13 | 0.43 |
| Transportation | 0.40 | 0.72 | -0.32 | -1.34 |
| Buying and selling | 0.54 | 0.67 | -0.13 | -0.58 |
| Financial/insurance/reast est. | 0.23 | 0.29 | -0.05 | -0.27 |
| Personal services | 2.43 | 1.00 | 1.43 | 3.30 *** |
| Education | 4.55 | 2.31 | 2.25 | 3.61*** |
| Health | 1.29 | 0.64 | 0.65 | 2.05*** |
| Public Administration | 1.90 | 2.27 | -0.36 | -0.77 |
| Other | 0.27 | 0.41 | -0.14 | -0.71 |
| **Observations** | 725 | 1225 | 1950 | |

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$

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Table B.2: Parallel trend hypothesis

| Dependent variable: Food insecurity score | |
|---|---|
| Time (Ref = planting 2018) | |
| harvest 2019 | -0.37*** |
| | (0.05) |
| May 2020 | 0.56*** |
| | (0.06) |
| Business closure (Yes/No) × Round | |
| (ref: Business closure = No; Round = Planting 2018) | |
| Yes × Harvest 2019 | -0.02 |
| | (0.08) |
| Yes × May 2020 | 0.18* |
| | (0.10) |
| Constant | 0.96*** |
| | (0.02) |
| Observations | 5850 |
| Adjusted $R^2$ | 0.242 |

Robust Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$

Table B.3: Capital channel hypothesis test by origin of remittances

| | Definition of the capital | | | |
|---|---|---|---|---|
| | Pooled capital | Formal Financial Services | Informal Financial Services | Livestock ownership, Rental earnings |
| | (1) | (2) | (3) | (4) |
| Lockdown-due business closure | 0.40* | 0.48** | 0.44* | 0.49** |
| | (0.22) | (0.22) | (0.22) | (0.23) |
| **Lockdown-due business closure x Savings group (Ref: Capital = No, remittance = No)** | | | | |
| Closure = Yes × (Capital = Yes, Int. remit. = Yes) | -0.80** | -1.01*** | 0.23 | -1.08*** |
| | (0.35) | (0.29) | (0.45) | (0.34) |
| Closure = Yes × (Capital = Yes, Dom. remit. = Yes) | -0.42* | -0.46* | -0.12 | -0.90*** |
| | (0.24) | (0.24) | (0.27) | (0.28) |
| Closure = Yes × (Capital = Yes, Int. & Dom remit. = No) | -0.12 | -0.26 | 0.01 | -0.03 |
| | (0.23) | (0.24) | (0.24) | (0.29) |
| Closure = Yes × (Capital = No, Int. remit. = Yes) | -0.15 | -0.15 | -0.15 | -0.15 |
| | (0.21) | (0.21) | (0.21) | (0.21) |
| Closure = Yes × (Capital = No, Dom. remit. = Yes) | -0.39 | -0.39 | -0.39 | -0.39 |
| | (0.39) | (0.39) | (0.39) | (0.39) |
| Time fixed effects | Yes | Yes | Yes | Yes |
| Household fixed effects | Yes | Yes | Yes | Yes |
| Constant | 0.96*** | 0.97*** | 0.92*** | 1.02*** |
| | (0.03) | (0.03) | (0.03) | (0.04) |
| Observations | 5433 | 4671 | 3132 | 1995 |
| Adjusted $R^2$ | 0.243 | 0.213 | 0.253 | 0.216 |

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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Table B.5: Sample distribution over rounds

| Round | Not-shocked | Shocked | Total |
|---|---|---|---|
| May-20 | 1233 | 730 | 1963 |
| % | 63 | 37 | 100 |
| Jun-20 | 1656 | 174 | 1830 |
| % | 90 | 10 | 100 |
| Jul-20 | 1728 | 66 | 1794 |
| % | 96 | 4 | 100 |
| Aug-20 | 1762 | 36 | 1798 |
| % | 98 | 2 | 100 |

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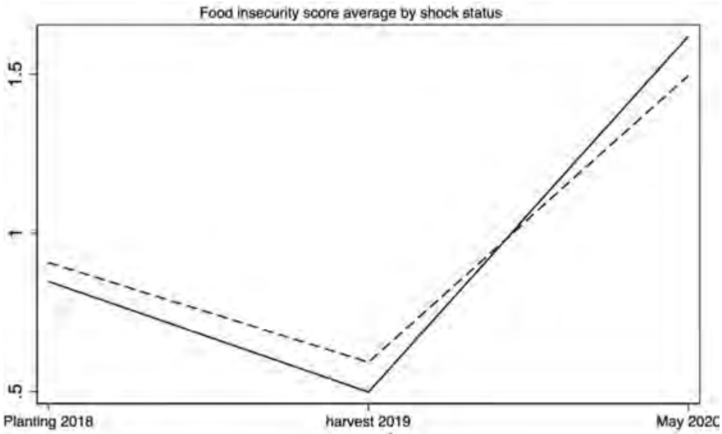
Table B.6: Remittances' mitigating effect: robustness to Food Insecurity definition

| Dependent variable | Likelihood to skip a meal | | Likelihood to run out of food | | Likelihood to not eat for a whole day | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Lockdown-due business closure | 0.11*** | 0.11*** | 0.07* | 0.07* | 0.11** | 0.11** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| All remittances 2018/2019 × Lockdown-due business closure | -0.10* | – | -0.11* | – | -0.13** | – |
| | (0.05) | | (0.06) | | (0.06) | |
| International remittances 2018/2019 × Lockdown-due business closure | – | -0.24* | – | -0.29 | – | -0.16 |
| | | (0.12) | | (0.20) | | (0.15) |
| Domestic remittances 2018/2019 × Lockdown-due business closure | – | -0.08 | – | -0.09 | – | -0.12** |
| | | (0.06) | | (0.06) | | (0.06) |
| Household fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Constant | 0.43*** | 0.43*** | 0.39*** | 0.39*** | 0.14*** | 0.14*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 5850 | 5850 | 5850 | 5850 | 5850 | 5850 |
| Adjusted $R^2$ | 0.255 | 0.256 | 0.136 | 0.137 | 0.090 | 0.090 |

Robust Standard errors in parentheses; $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$

Figure B.1: Parallel trend hypothesis test




Source: GHS wave 4 (2018/2019), COVID-19 NLPS 2020, Authors' calculations.

Table B.4: Dictionary of variables used

| Variables | Questionnaire used | Questions considered |
|---|---|---|
| COVID-19 employment shock | *COVID-19 NLPS 2020 baseline household questionnaire* | **1. Were you working before mid-March?** (Yes/No)<br><br>**2. What was the main reason you stopped working?**<br>• Business/Office closed due to coronavirus legal restrictions<br><br>• Not able to go to farm due to movement restrictions |
| Remittances | *Nigeria General Household Survey - Panel Wave 4, 2018-2019, Post-Harvest Community Questionnaire* | **1. In the past 12 months, did [NAME] receive any of the following assistance from a non-household member?** (Yes/No)<br><br>• FROM ABROAD<br>A. Monetary assistance<br>B. In-kind assistance<br><br>• FROM WITHIN NIGERIA<br>A. Monetary assistance<br>B. In-kind assistance |
| Food insecurity | • *COVID-19 NLPS 2020 baseline household questionnaire*<br><br>• *Nigeria General Household Survey - Panel Wave 4, 2018-2019, Post-Harvest Community Questionnaire*. | **1. You, or any other adult in your household, had to skip a meal because there was not enough money or other resources to get food?** (Yes/No)<br><br>**2. Your household ran out of food because of a lack of money or other resources?** (Yes/No)<br><br>**3. You, or any other adult in your household, went without eating for a whole day because of a lack of money or other resources?** (Yes/No) |

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# The virus that devastated tourism: The impact of Covid-19 on the housing market[1]

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*This paper estimates the differential impact of the Covid-19 pandemic on the housing market of different neigbourhoods within the city boundaries of a tourist-intensive capital with a high density of short-term rental properties. This is the first paper to analyse the consequences of a shifting of dwellings from the short-term into the long-term rental market. We use a panel that spans the 24 civil parishes of Lisbon between the third quarter of 2018 and the third quarter of 2020. Our identification combines the sudden and sharp decrease in tourism caused by the Covid-19 pandemic with a parish-level treatment relying on the pre-pandemic intensity of short-term rentals. We use difference-in-differences specifications, and an instrumental variable based on the density of museums. We show that in the long-term rental market, prices decrease 4.1%, while quantities increase 20% in the treated civil parishes vis-a-vis comparison ones. We also find evidence of an incremental negative impact on sale prices of 4.8% in treated civil parishes, with no effect on quantities. The results are robust to the inclusion of the second largest city of the country.*

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# 1 Introduction

In an attempt to control the spread of the SARS-CoV-2 virus, governments around the world have imposed lockdowns and travel restrictions, starting in January 2020, which have ravaged the global tourism and hospitality markets.[1] This almost sudden stop is particularly striking in economies that rely a lot on tourism. According to the OECD, in 2018, Portugal ranked first in the contribution of tourism to the country's economy, with 12.5% of its GDP directly or indirectly linked to this sector. The hotel and short-term rental sector in Portugal hosted 10.5 million guests in 2020, down from 26 million in 2019. The number of overnight stays went down to its 1993 level, mostly driven by a 75% contraction in the stays of foreign tourists, according to Statistics Portugal.

Following the outbreak of the pandemic, rents in Lisbon have contracted 11.1% in the third quarter of 2020, when compared to the same period of 2019. The most central parts of the city might be less attractive in covid-19 times, because working from home implies lower commuting costs, and a preference for larger housing units and for balconies or gardens. In addition, covid-19 makes it very hard to enjoy city center amenities such as bars, restaurants, and cultural events. This effect may also be explained by a decision from landlords to rent their properties in the long-term rental market or, possibly, sell the property as a response to the sharp decrease in the demand for short-term rental from tourists.[2]

In this paper, we use data from the parishes of the municipality of Lisbon and Porto, which are small relative to their metropolitan area.[3] The urban neighborhoods in our treatment and control groups are similar in terms of residents' socioeconomic characteristics, population density, and amenities such as retail. More importantly, they are geographically close within the city boundaries, and it is unlikely that the residents who move because of the changed trade-off between space and commuting costs will do so within these urban areas. The

[1] For the negative effects of the pandemic on travelling see, e.g., Lee and Chen (2020) and Coibion et al. (2020).

[2] According to Turner et al. (2014), the short-term rental market can decrease the real estate market through *negative externalities* such as increased congestion, and increase it through a *demand side efficient use* and *housing supply effects*.

[3] Lisbon (Porto) is just one municipality out of 18 (17) in the respective metropolitan area. The population of the municipalities of Lisbon (Porto) in 2019 was 508.368 (215.945) inhabitants.

neighboorhoods do differ in the intensity of short-term rental properties. We use the pre-pandemic intensity of short-term rentals in different civil parishes as a measure of the intensity of the shock. Therefore, our setting is particularly suited to disentangle the effect of the shock on the short-term rental market from the above mentioned trend of urban exodus on the housing market.

We address two research questions. Firstly, we provide an estimate of the impact of the covid-19 crisis on the housing market in a capital city with a high density of short-term rental properties. Secondly, we identify the impact of a sudden and sharp negative demand shock on the short-term rental market, contrary to the existing literature that looks either at the introduction and growth of the short-term rental platforms.

We combine administrative data on short-term rental registries, together with quarterly data for Lisbon' and Porto's housing markets, namely, rental and sale prices, as well as on the number of dwellings for rental and for sale, to analyze the short-term impact of the pandemic. We then implement a difference-in-differences specification with a binary treatment specification that uses the civil parishes targeted by the partial bans implemented by the municipality of Lisbon in 2018 and 2019 as the treated units. We complement this analysis with a continuous treatment intensity. For robustness, we include the civil parishes of Porto (and exploit the respective 2019 ban) in some specifications. Finally, in order to address possible endogeneity concerns of the intensity of short-term rentals, we provide an instrumental variable specification that uses the intensity of museums to instrument the short-term rental intensity in each parish.

We provide the following estimates for the very short-run impact of the pandemic. Firstly, we estimate a decrease in rental prices in Lisbon's most touristic civic parishes of 4.1% and an increase of around 20% in the number of apartments for rental, *vis-à-vis* comparison parishes. The preferred estimate for the impact of high density of short-term rentals on the rental price represents more than one third of the overall impact on rents observed in the period. These magnitudes are significant and robust across all estimations and suggest a strong supply side

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effect of landlords reallocating their properties to the long-term rental market. Secondly, we find a statistically significant decrease in sale prices of between 4.8% and 7.6% in treated civil parishes, but no statistical significant effect on quantities, when compared to the remaining civil parishes. This suggests a demand shift that decreased the negotiated prices. Fourthly, we analyze of heterogeneous effects, that are particularly concentrated in one- and two-bedroom apartments in the rental market, suggesting a strong preference for this type of dwellings in the short-term rental market.

This is one of the first papers about the effect of the pandemic on real estate, and the only one to focus on the consequences of the collapse in the short-term rental market.[4] Liu and Su (2020) finds that covid-19 reduced demand for housing in neighborhoods with high population density in the US, with previous high home prices experiencing a larger decline. Gupta et al. (2021) show that the pandemic flattened the bid-rent curve in the U.S. as house price and rent declines in city centres where counteracted by price and rent increases away from the center.[5] Contrary to the US case, we do not focus on the divide between the central city and the suburbs, but analyse instead the differential impacts within the city.

We also contribute to the growing literature on the effects of short-term rentals on the housing market.[6] A causal impact of the short-term rental market on housing prices is obtained by Sheppard et al. (2016), with matched difference-in-differences in New York city, Barron et al. (2018) with an instrumental variable based on google trends, and Garcia-López et al. (2019), using an interaction between space-invariant proximity to Barcelona's touristic amenities and time-variant google searches of Airbnb Barcelona as an instrument. Almagro

[4]The focus of this paper is on residential real estate. Wang and Zhou (2020) and Garcia et al. (2021) study the impact of the pandemic on asset-level commercial real estate.

[5]Bloom and Ramani (2021) coined the term *donut effect* to refer to this reallocation of demand away from city centers toward city suburbs in the US. Homeworking creates migration away from city centers, with fading local commerce and restaurants, eventually leading to a sale price decrease. See, e.g., Althoff et al. (2020), Delventhal and Parkhomenko (2020) and Delventhal et al. (2021). The role of mobility restrictions in limiting covid-19 spread in New York, as well as the determinants of the differential exposure to the disease, are studied by Glaeser et al. (2020) and Almagro and Orane-Hutchinson (2020), respectively.

[6]There is a short literature about the effects of the pandemic in Portugal. Carvalho et al. (2020) design a difference-in-differences event study to evaluate changes consumer behavior, documenting a contraction on overall consumption levels, particularly affecting urban and central municipalities, as well as leisure and tourism activities.

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and Domínguez-Iino (2019) use a structural model to show that short-term rental platforms had a significant impact on rents, amenities, and within-city migration in Amsterdam. Effects on the short-term rental supply have been documented by Koster et al. (2018), using quasi-experimental evidence from the Los Angeles' Home Sharing Ordinances market, combining a spatial regression discontinuity design with difference-in-differences and Duso et al. (2020) who exploit new restrictions on the registration of short-term rentals in Berlin. Finally, Gonçalves et al. (2020) exploit partial bans on new short-term rental registries in Lisbon through a difference-in-differences specification, and estimate the consequences on registries, Airbnb prices and quantities, number of transactions and housing prices, at the neighborhood level.[7]

Our main contributions are as follows. Firstly, we analyse the impact of the pandemic on the urban landscape within the city boundaries, providing credible evidence that the reversal of the short-term rental market improves the affordability of the historical city centres in high-density cities. Secondly, given the small geographical scale of our analysis, most of the confounders that could potentially contaminate the analysis affect similarly the treatment and comparison areas. This is in contrast with the studies of city centres vs suburbs impacts. Finally, we complement the existing literature in that, for the first time, we focus on the impact on the residential market of a demand-driven sudden stop of the short-term rental market, while the existing literature focuses on the impact of demand growth or on partial supply-side restrictions.

This paper is organized as follows. Section 2 presents a brief institutional background, whilst Section 3 describes the empirical strategy and presents descriptive statistics. Finally, Section 4 reports our results, and Section 5 provides a summary of the main conclusions and policy implications of the paper.

[7] Franco and Santos (2021) analyse the impact of short-term rental density on the earlier period of 2011–2016 and document significant price increases in the historical areas of Lisbon and Porto. The distributional impact of these platforms on the housing market is studied by Calder-Wang (2019) who that the gains from the host channel do not compensate the losses from renters in New York City, especially for those who prefer housing and location amenities that are most desirable to tourists.

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## 2 Institutional Background

The decree-law 128/2014 created a straightforward online registration process for short-term rental properties. The license is available immediately and is a necessary step for the landlord to post her property on Airbnb and other home sharing platforms. Non-compliance entails a fine; moreover, platforms cooperate with the government by actively checking the licence number. Safety regulations are verified through random checks by the competent authority, *Turismo de Portugal*. Importantly, the license belongs to the individual, i.e., it expires when the property is sold. Moreover, the licence is free to acquire and hold. Therefore, one can own a property that is registered as short-term rental, while not actually renting it in any sharing platform, and retaining the licence for its positive option value. Moving back and forth between the residential rental market and the short-term one is costless.

Following this new regulation, the country witnessed an almost three-fold increase in the number of overnight stays in short-term rental properties, from 3.6 to 10.2 million between 2013 and 2019. Overnight stays by foreign tourists in all the hospitality sector almost doubled in the same period (from 8.6 to 16.4 million). To counteract rapid increases in real estate prices, the municipality of Lisbon introduced a ban on new short-term rental registries in some neighborhoods, in 2018, which it extended in 2019. The municipality of Porto followed suit in 2019.

The districts of Lisbon and Porto are responsible for 40% of the stock of 92 thousand short-term rental registries reached in 2019. Most of them are located in Lisbon, designated by the World Travel Awards as the world's leading city break in four consecutive years between 2017 and 2020. In 2019, Lisbon had 19,479 apartments registered as short-term rental properties, corresponding to almost 6% of the total dwellings.[8] The short-term rental boom coincided with a rapid increase in real estate prices, pricing out locals and pushing some of Lisbon's residents to the outskirts of the city.[9] Between 2017 and 2019, median

[8] See `https://travelbi.turismodeportugal.pt/pt-pt/Paginas/PowerBI/rnal-registo-nacional-de-alojamento-local.aspx`

[9] See `https://www.nytimes.com/2018/05/23/world/europe/lisbon-portugal-revival.html`

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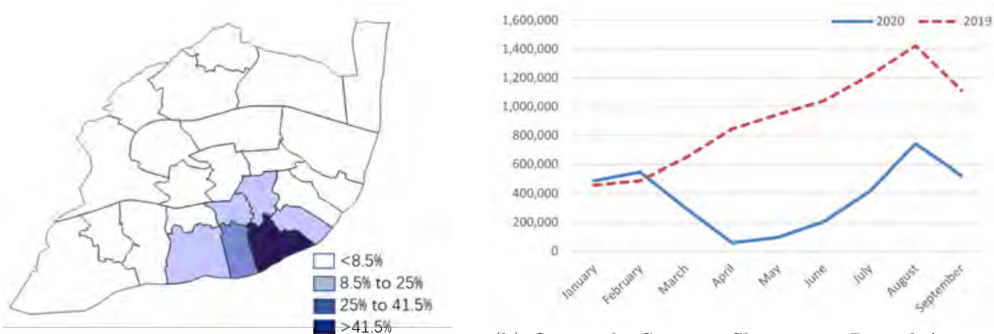
rental prices grew 21.2% in Portugal, while Lisbon and Porto saw rents rising 23.9% and 31.9%, respectively. Moreover, as presented in Figure A1 in the Appendix, Lisbon's rental prices are well above the median of mainland Portugal.[10]

Short-term rental density is not homogeneous even within the city boundaries that we are analysing. This is shown on the city map in panel (a) of Figure 1, which depicts the city of Lisbon partitioned into its 24 civil parishes (*freguesias*), the lowest political unit in Portugal.[11] The density of short-term rentals is the highest in historic downtown areas.

Figure 1: Short-term Rental Accomodations




(a) Density of Short-term Rental Accommodations

Source: RNAL

(b) Overnight Stays in Short-term Rental Accommodations in Portugal

Source: Statistics Portugal. This figure only includes short-term accommodations for more than 10 people.

In 2018, faced with the rising concerns over housing affordability, the municipality of Lisbon restricted new registries in areas with a ratio of short-term rentals to total property above 25%, named *Zonas Turísticas Homogénas*, which were then updated in 2019 to include additional neighborhoods (Proposal 204/CM/2019).[12] In 2019, Porto's municipality approved a similar legislation encompassing two civil parishes. We exploit these legislative changes in the identification strategy below (Edital NUD/260310/2019/CMP).

[10] During the same period, median sale prices in mainland Portugal grew 23.2%, 32.4% in Lisbon and 33.2% in Porto.

[11] The area of the municipality of Lisbon is 100 km2 while the area of the municipality of Porto is less than 42 km2.

[12] These include *Baixa, Eixos Avenida da Liberdade, Avenida da República, Avenida Almirante Reis, Bairro Alto, Madragoa, Castelo, Alfama, Mouraria, Colina de Santana* (areas of absolute contention), and *Graça and Bairro das Colónias* (areas of relative contention).

The first case of coronavirus in Portugal was reported on the 2nd of March, and, one week later, the WHO declared covid-19 as a pandemic. By the 18th of March, Portuguese authorities had declared the entire territory to be in a State of Emergency, imposing strict restrictions on the movement of people and closing schools and non-essential economic activities until May. As the number of new infections grew exponentially, and with countries adopting travel bans that restricted the inflow of foreigners, Portugal saw the number of foreign visitors decrease by 75%. The number of overnight stays in short-term rental accommodations also decreased considerably as can be seen in the panel (b) of Figure 1, where we contrast the number for 2019 (in red, dashed) and 2020 (in blue).

In a survey conducted by ISCTE, a Lisbon-based university, between July and October, 40% of the 868 owners of short-term rentals report that revenues from renting these units represent over half of their income streams, and 17% were considering moving to the long-term rental market. Additionally, urban areas were the most affected by the covid-19 disruption of leisure activities, with Lisbon seeing revenue breaks of 93%, and Porto of 87%.[13] This disruption is also confirmed by Carvalho et al. (2020).

The Portuguese government has also implemented measures of financial relief to mitigate the economic fallout. Among these measures was the creation of a temporary moratorium on the repayments of capital and interest of rents, mortgages and commercial loans (at least) until September 2021. The suspension of residential and non-residential rental payments as well as of mortgage expenditures for households facing difficulties was approved in April and extended in the second semester (Decree-laws 4-C/2020 and 78-A/2020). This policy is likely to delay any impact of the crisis on the availability of housing units in for sale in the market.

[13] See `https://expresso.pt/economia/2020-11-27-Covid-19.-Alojamento-local-com-quebras-de-faturacao-superiores-a-75-no-2.-trimestre` (in Portuguese)

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# 3 Empirical Strategy

## 3.1 Data Sources

We combine data on short-term rental registries, rental prices and quantities, and sale prices and quantities. For simplicity, we refer to the residential rental market as "long-term", although we have no information on the duration of the contracts. Our unit of analysis is the civil parish, of which there are 24 in Lisbon and 7 in Porto. Civil parishes are fairly small units, with an average surface of 4.2 and 6 square kilometers, and had an average number of residents of 21 thousand and 31 thousand in 2019 in Lisbon and Porto, respectively.

The first data source comes from the SIR platform, collected by *Confidencial Imobiliário* following a protocol established with the Municipalities of Lisbon and Porto. *Confidencial Imobiliário* is a Portuguese databank specialized in the real estate market whose data is used by private and public institutions such as the European Central Bank. The platform collects rent and transaction prices and quantities from more than 700 real estate agents operating in the country. We collected quarterly data on the number of apartments for rent and for sale, the average price, and the first and third quartile of the price distribution. The data is disaggregated by type of dwelling, from one or less to three or more bedrooms. The data covers the 31 civil parishes in Lisbon and Porto, between the third quarter of 2018 and the third quarter of 2020, i.e., a total of 9 quarters.

For the short-term rental density per civil parish, we proceed as follows. We use the publicly available data on the National Short-Term Rental Registry (RNAL) to obtain the number of registered short-term rental properties in each civil parish in the fourth quarter of 2019, i.e., pre-treatment. In order to compute a measure of density, we need an estimate of the total number of dwellings per civil parish. The only parish level data on the number of dwellings is from census data, and the last available one is from 2011. We update it using yearly figures of construction and demolition of buildings in each civil parish, available from Statistics Portugal. Finally, we have to deal with the 2013 reorganization of civil parishes

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which, through a sequence of mergers and splits, transformed the city from its original map of 53 parishes into the current one with 24. We deal with the merged civil parishes by simply adding the dwellings.[14]

Lastly, we collect a number of socio-economic and political indicators as pre-treatment characteristics from Marktest, a Portuguese subscription databank specialized in software development, research drafting and public data collection, and from publicly available data on the Lisbon's municipality website.

## 3.2 Methodology

We seek to test if civil parishes with a higher density of short-term rental properties are more hit by the pandemic than the remaining ones. In the rental market, this may happen via a supply side effect if the landlords relocate their properties to the long-term rental market. A potential demand side effect occurs if the residents seek to abandon the city centre because of changing preferences due to homeworking. Given the geographic proximity of the comparison areas and the similar urban density, it is unlikely that homeworking patterns are different in treatment and comparison civil parishes. Therefore, our empirical strategy rules out this effect. In the real estate market, a supply side effect may occur if short-term rental landlords decide to sell their dwellings. As already discussed, it is unlikely that the crisis provoked by the pandemic will have effects on the real estate market through household defaults, given the moratorium policy. As regards demand side effects, there are several possible channels. The changing preferences argument that applies for the rental market applies to the sale one as well. On the other hand, the foregone expected profit from renting to tourists may decrease the demand for apartments.

We employ a difference-in-differences approach based on the exposure of each civil parish to short-term rentals to evaluate four outcome variables: *(i)* average rental price per square

[14] For the one that was split (*Sta Maria dos Olivais*), we use the number of registered voters as a proxy for civil parishes' residents. We then apportion the number of dwellings in the original parish to the new, smaller ones, using the number of voters (Reorganização administrativa de Lisboa; Law 56/2012).

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meter, *(ii)* average sale prices per square meter, *(iii)* number of dwellings available in the rental market and *(iv)* number of apartments for sale. Our baseline regressions only include civil parishes in Lisbon but we add civil parishes in Porto for robustness. We use two treatment definitions. The first assigns all the civil parishes that contain neighborhoods that are covered by the two (2018 and 2019) Lisbon bans and the 2019 Porto ban on new short-term rental licences to the treatment group. We also consider a continuous treatment alternative, where the treatment intensity is the density of short-term rentals in the last quarter of 2019. Panel (b) of Figure 1 displays a sharp drop in the number of tourists in March, coinciding with the onset of the pandemic in Portugal. Therefore, the treatment period begins in the first quarter of 2020.[15]

To construct the difference-in-differences estimator, the treated civil parishes are the *high short-term rental density* ones, and the comparison civil parishes are *low short-term rental density*. We assign civil parishes to the treatment group when they contain neighborhoods that are subject to the bans on short-term rental registries implemented in Lisbon and Porto in 2018 and 2019. Importantly, these restrictions were imposed at a smaller geographical scale than that of the civil parish (Gonçalves et al., 2020). We assume that a civil parish is treated if it contains at least one restricted area. Overall, this includes 7 civil parishes in Lisbon – *Misericórdia, Santa Maria Maior, Santo António, São Vicente, Arroios, Avenidas Novas* and *Estrela* – and 2 in Porto – *Bonfim* and *UF Centro Histórico do Porto.*

Two assumptions are necessary for inferring causality using difference-in-differences models: firstly, the absence of contemporary events that differently affected civil parishes with higher short-term rental density; secondly, the presence of parallel trends in the outcome variables prior the treatment period.

Regarding the former, it is safe to assume that there were no policies with differential impacts on civil parishes. Note that we rely on the large, world-wide, unexpected shock caused by the onset of the pandemic. Other contemporaneous events that potentially affect

[15]The first case in Europe was reported in France, on the 23rd of January.

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civil parishes differently would be second order in face of this large shock. Moreover, we are dealing with the very short-run effects: our analysis starts in the third quarter of 2018 and goes until the third quarter of 2020. In 2020, the municipal and the central governments have been occupied with policies to mitigate the effects of the pandemic. There were no urban or zoning policies implemented during this period that may have impacted civil parishes differently. Moreover, as already explained, the treatment and control areas in our study are all high-density parishes within the city boundaries, and residents who flee in face of the pandemic to less central or more rural areas where space constraints and congestion are less binding would be equally put off by these areas. Nevertheless, to mitigate further concerns, we instrument the density of short-term rentals with the density of museums in each civil parish. Additionally, quarter and civil parishes fixed effects are included in the regressions. This also accounts for potential differences in the time-invariant socioeconomic and labour market characteristics of the residents, which may have been hit differently by the pandemic crisis.

We also provide a formal test of the parallel trends assumption. We carry out event study exercises to verify that, before the pandemic, the outcome variables followed parallel trends. As previously mentioned, the treatment period starts in the first quarter of 2020.[16] Hence, the omitted quarter is the one immediately before. The event-study exercise is carried out using the following specification for civil parish $p$ and quarter $q$:

$$\ln(y_{pq}) = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \sum_{2018Q3 \leq q \leq 2019Q3} \delta_q \times hdens_p \times \mathbb{1}_q + \sum_{2020Q1 \leq q \leq 2020Q3} \delta_q \times hdens_p \times \mathbb{1}_q + \epsilon_{pq} \tag{1}$$

where $y_{pq}$ is the outcome variable for civil parish p in quarter q, $\alpha_p$ and $\lambda_q$ are civil parishes

[16] Although the last quarter in 2019 is clearly the last pre-pandemic period, Portuguese people were not anticipating the shock for most of the first quarter in 2020. We relax this assumption by removing the first quarter of 2020 from our sample as a robustness test.

and quarter fixed effects, $\mathbb{1}_p$ and $\mathbb{1}_q$ are indicator variables of civil parish and quarter, and $\epsilon_{pq}$ is the error term. Finally, $hdens_p$ is the treatment indicator, i.e., it is equal to 1 for the civil parishes that contain areas that were covered by the bans on new short-term rental registries by the municipalities of Lisbon and Porto.

Our baseline difference-in-differences specification is given by the following equation:

$$\ln(y_{pq}) = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \beta\, Post_q \times hdens_p + \epsilon_{pq} \tag{2}$$

where all variables are defined as in Equation (1) and $Post_q$ is equal to 1 in the treatment period, i.e., starting in the first quarter of 2020. The coefficient of interest, $\beta$, measures the differential impact of the pandemic on high versus low density areas, where high density areas are defined by the bans on short-term rentals implemented by the municipalities of Lisbon and Porto. The comparison group of civil parishes that do not include areas covered by the bans is not expected to suffer the effects of the pandemic on the short-term rental market.

We also implement an intensity of treatment specification, as follows:

$$\ln(y_{pq}) = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \beta Post_q \times STRdensity_p + \epsilon_{pq} \tag{3}$$

where all variables are defined as in Equation (2) , and $STRdensity_p$ is equal to the density of short-term rentals, given by the ratio of short-term rental units to total number of dwellings in civil parish $p$ in the last quarter of 2019. Thus, the coefficient $\beta$ gives us an estimate of the impact of the pandemic when the intensity of treatment with short-term rental intensity increases by one per 100 dwellings.

On a last note, in all regressions, logarithms are used to ensure proper distribution of the dependent variable and standard errors are robust to account for heteroskedasticity.

Finally, we deal with concerns of endogenous intensity of treatment. As already discussed, the pandemic may drive housing prices in treated areas differently from comparison ones for reasons unrelated to the short-term rental market, if residents want to flee more central and

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congestioned areas. We address this issue with an instrumental variable approach. The instrument is computed as the number of museums per squared kilometer of each civil parish $p$, $museumdens_p$. Therefore, our IV analysis estimates the following equations:

$$STRdensity_p = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \beta_1 museumdens_p + \epsilon_{pq} \tag{4}$$

$$\ln(y_{pq}) = \alpha_p \times \mathbb{1}_p + \lambda_q \times \mathbb{1}_q + \beta_1 Post_q \times \widehat{STRdensity}_p + \epsilon_{pq} \tag{5}$$

where all variables are defined as in Equation (2). Instrument validity relies the exclusion restriction, i.e., the density of museums can only affect the housing market through the impact on short-term rentals and not through a direct impact. This assumption would be challenged if residents are also attracted by the presence of museums in the surroundings of their residence. We provide two arguments to sustain the validity of our instrument.

The first is based on survey data from Statistics Portugal. There are 64 museums in Lisbon in the period covered by our analysis. All of them were built well before our sample period, with just 4 inaugurated between 2013 and 2016. Data from Statistics Portugal shows that, in 2019, 53.6% of resident adults had not visited any museum during the previous year and that 52.3% of museums' visitors were foreigners.[17] Hence, it is unlikely that proximity to museums is an important factor when deciding residential location.[18]

The second is a more formal exercise, reminiscent of Garcia-López et al. (2019), and it consists in a Placebo event study where we compare the parishes with the highest museum density with the remaining ones to show that there were no differences in trends for sale prices before 2014, i.e., before the onset of the short-term rental market in Lisbon. It is shown in Figure A3 in the Appendix. Due to data availability, we use yearly data from 2009 to 2014 to compare the seven civil parishes with high density museums *vis-à-vis* the

[17]Source: Inquérito à Educação e Formação de Adultos (IEFA).
[18]Results are robust when using the density of monuments as an instrument and are available upon request.

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remaining areas in Lisbon. We do not find any difference in real estate prices for the two groups, both before and after 2012. This constitutes strong evidence that museum density does not predict different trajectories in sale prices in the period before short-term rentals.

### 3.3 Descriptive Statistics

Before proceeding to the regression results, the trends for the four outcome variables are presented in Figure A2 in the Appendix, where pre- and post-treatment periods are separated by the grey vertical line. In panels (a) and (b), one can immediately see a decrease in rental prices, especially in the second quarter of 2020, together with a spike in the number of apartments for rental in the long-term rental market. Panels (c) and (d) suggest a price decrease and quantity increase of much smaller magnitude in the sales market.

In Table 1, we report descriptive statistics of all the variables used in the the event studies, baseline regressions, and instrumental variable specification, that focus on the city of Lisbon.[19]

Table A1 in the Appendix presents comparative statistics of the treatment and control groups in terms of several characteristics, namely: real estate, rental, and short-term rental markets, political preferences, socioeconomic and demographic characteristics, and amenities. Naturally, the short-term rental density differs between the two groups. The rental and sale markets also differ, both in terms of prices and quantities, with higher prices and more transactions and rentals in the treated parishes.[20] The other significant difference is the number of monuments and museums, i.e., tourist amenities, which we exploit in our instrumental variable strategy. In terms of political variables, the two groups are similar, safe for a small difference of four percentage points in the vote for the incumbent party of the mayor. Importantly, the population density and the share of highly educated residents are not statistically different across the two groups. Neither are proxies of economic activity such

[19] We only use Porto in some robustness specifications, which is why the data for Porto is not included in Table 1.

[20] The parallel trends assumption tested with the event study regressions defined in Equation (1) tests for pre-shock parallel growth rates, not levels.

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as the number of retailers and bank agencies. Moreover, bear in mind that all our regressions include civil-parish fixed effects which control for time invariant, unobserved factors.

Table 1: Descriptive Statistics on Sample Characteristics (for Lisbon)

| | N | Mean | Stand Dev | Min | Max |
|---|---|---|---|---|---|
| Number of Civil Parishes | 24 | - | - | - | - |
| % Alignment | 212 | 0.484 | (0.064) | 0.330 | 0.622 |
| % Turnout | 212 | 0.532 | (0.045) | 0.434 | 0.626 |
| *A.Rental Market* | | | | | |
| Average Rental Price (€/m2) | 212 | 13.31 | (2.532) | 5.2 | 19.2 |
| Number of Apartments for Rental | 216 | 70 | (45.617) | 9 | 204 |
| *B.Sales Market* | | | | | |
| Average Sale Price (€/m2) | 213 | 3640.59 | (907.02) | 1733 | 6368 |
| Number of Apartments for Sale | 216 | 373 | (224.58) | 58 | 910 |
| *C.Short-Term Rentals* | | | | | |
| Density of Short-term Rental Accommodations | 24 | 0.071 | (0.111) | 0.001 | 0.440 |
| *D. Museums* | | | | | |
| Density of Museums | 24 | 0.872 | (1.312) | 0 | 5.648 |

Notes: The upper panel refers to characteristics of political variables from the 2013 and 2017 elections, where % Alignment is the share of voters that voted on the Mayor's party and % Turnout is the percentage of people that did not vote, given by 1 minus the turnout rate. % Higher Education is the percentage of residents who have a higher educational degree. Panel *A. Rental Market* and Panel *B. Sale Market* present descriptive statistics for selected variables on the rental and housing markets, respectively, while panel *C. Short-term rentals* describes statistics for the data set from RNAL with information on short-term rental registries in Lisbon in 2019. Finally, Panel *D. Museums* shows statistics for Lisbon's museums in 2017.

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# 4 Results

The main results of our empirical approach are presented in this section. Additionally, we exploit possible heterogeneous effects across samples.

## 4.1 Rental Market

The first set of results assesses the impact of the Covid-19 pandemic on median rental prices per square meter and quantities for rental in each civil parish.

To verify the parallel trends assumption, we conduct event studies for Lisbon's civil parishes. Figure 2 plots the values of the binary coefficient $Post_q \times hdens_p$ and highlights that both rental prices in panel (a) and quantities in panel (b) followed parallel trends before the treatment. Moreover, in the third quarter of 2020, rental prices plummeted in high density civil parishes, although the spike in the number of apartments for rental is shortly below 20% but marginally not significant at the 5% level in the second quarter of 2020.

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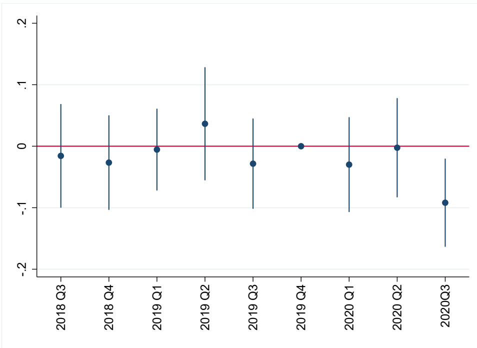
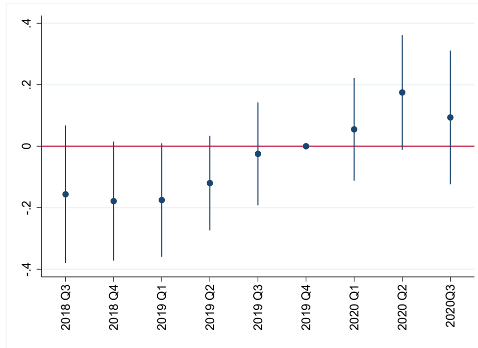
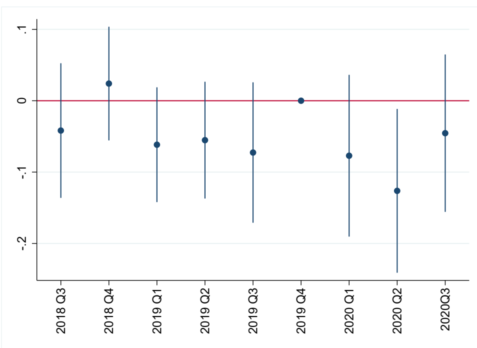
Figure 2: Event Studies



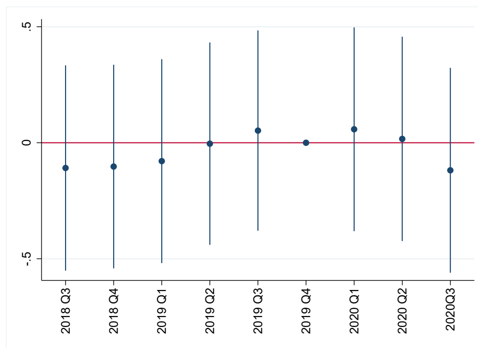

(a) ln(Rental Prices)




(b) ln(Quantities for Rental)




(c) ln(Sale Prices)




(d) ln(Quantities for Sale)

Notes: In Panel (a) N=216, in Panel (b) N=212, in panel (c) N=213 and in panel (d) N=212. The comparison group consists of the 17 low short-term rental density civil parishes. The regression includes quarter and civil parishes fixed effects. The treatment period starts in 2020Q1. 95% confidence intervals.

We present our baseline results, obtained from estimating Equation (2), Equation (3), and the difference-in-differences instrumental variable specification spelled out in Equation (4) and Equation (5), in Table 2.[21] For the sake of space, the first-stage regressions are reported in Table A3 in the Appendix.

Columns 1 to 5 show the results for the rental price. In column 1, the coefficient of $Post_q \times hdens_p$ indicates that rental prices in Lisbon's civil parishes included in suspension

[21] We show that our findings remain very similar if we exclude the first quarter of 2020 in Table A2 in the Appendix. This accommodates the fact that the first case was reported in Portugal in March 2[nd], and the residents were not anticipating the strong effect of the pandemic long before that.

areas fell around 3.5% *vis-à-vis* the remaining civil parishes. This result is not robust when including Porto's civil parishes in Column 4. Column 2 reports the intensity of treatment coefficient. In order to compare it to the binary treatment one, we compute the average treatment effect as follows: given that average short-term rental density in treated (resp., comparison) areas is 19% (resp., 2%), as indicated in Table A1, column 2 suggests that rental prices in Lisbon decreased 3.6% ($-0.214 \times (0.19 - 0.02)$) after the pandemic. This result is in line with the one obtained for the binary treatment. It is robust to the inclusion of Porto's civil parishes (in Column 5), although smaller in magnitude in this case. The instrumental variable estimate, presented in Column 3, is similar to the OLS one, although of a slightly higher magnitude (4.1% applied to the sample means). This a fairly sizeable result, recalling that the average rental price fall in Lisbon was 11.1%. The differential impact in treated areas, i.e., those with a high density of short-term rentals, represents more than one third of the overall impact.

We now turn to columns 6 to 10, i.e., the results on the quantity of dwellings for rental. The results show a consistent increase in the number of apartments available for rental in the traditional rental market, which ranges from a magnitude of 19.4% in the continuous treatment specification ($1.141 \times (19\% - 2\%)$) to 21.7% in the binary treatment one. The IV estimate lies between the two. Including Porto decreases the magnitude of the effect in the binary treatment case and increases it in the continuous treatment one, albeit slightly, and it does not change the significance.

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Table 2: Difference in Differences - Rental Market

| | Average Rental Prices | | | | | Quantities for Rental | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Lisbon | | | +Porto | | Lisbon | | | +Porto | |
| | DiD (1) | DiD (2) | DiD-IV (3) | DiD (4) | DiD (5) | DiD (6) | DiD (7) | DiD-IV (8) | DiD (9) | DiD (10) |
| $Post \cdot hdens$ | -0.035* | | | -0.013 | | 0.217*** | | | 0.186*** | |
| | (0.020) | | | (0.021) | | (0.048) | | | (0.056) | |
| $Post \cdot STRDensity$ | | -0.214** | -0.239** | | -0.169* | | 1.141*** | 1.222*** | | 1.175*** |
| | | (0.090) | (0.101) | | (0.089) | | (0.164) | (0.173) | | (0.178) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 212 | 212 | 212 | 266 | 266 | 216 | 216 | 216 | 279 | 279 |
| R-squared | 0.780 | 0.783 | 0.783 | 0.864 | 0.866 | 0.962 | 0.965 | 0.965 | 0.948 | 0.951 |
| KP 1st Stage F-stat | | | 461.84 | | | | | 460.36 | | |

Notes: The treated areas are *Avenidas Novas, Arroios, Estrela,Misericórdia, Santa Maria Maior, Santo António, São Vicente*, as well as *Bonfim and UF Centro Histórico do Porto* when including Porto's civil parishes in the regressions. Robust standard errors are depicted in parenthesis. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01. For more information on the DID-IV regressions see Table A3 in the Appendix.

To verify the existence of heterogenous effects, we re-estimate the difference-in-differences model for subsamples of dwellings according to their respective number of rooms. These regressions are estimated with a lower number of observations, because the data is censored when the number of observations in a triplet (dwelling type, civil parish, quarter) is below three. Therefore, one has to be cautious when interpreting the following results, which can be seen in Table A4 and Table A5 in the Appendix. The binary treatment specification suggests that the price effect was stronger and significant for two-room apartments (-6.2%). This is consistent with the findings by Gonçalves et al. (2020) who also show that these are the most attractive dwellings to be used as short-term rental properties. With the intensity of treatment specification, the effect is similar for three bedroom apartments. These effects are robust to the inclusion of Porto's parishes.

Turning to quantities, we find a higher increase in the smaller, one-bedroom apartments in treated areas *vis-à-vis* the comparison ones, consistent with the negative, albeit non-significant, price effect. We also find a positive impact on sales of two-bedroom apartments, which is significant in the intensity of treatment specification. There is no effect on bigger apartments.

All in all, our results lend strong support for a sizeable impact of the pandemic on the

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rental market of the most touristic areas of Lisbon. There is a strong supply side effect, with landlords relocating their properties to the long-term rental market. The pandemic has created incentives to dislodge apartments to traditional rental markets, curbing trends of rising rental prices. This is consistent with fact that it is costless for property owners to relocate their property to the long-term rental market, since the licence one obtains when registering an apartment in a peer-to-peer platform can be kept for free. As regards the demand side, the price decrease is consistent both with an increase or a decrease in the demand for long-term rental contracts. Our results reinforce the existing evidence about the impact of short-term rentals on the rental market for long-term residents.

## 4.2 Sales Market

In this section, we perform the same regressions as in Section 4.1 to evaluate the impact of the pandemic on the sales market. From the event study plots presented in Figure 2, we confirm that civil parishes were on the same trends for both sale prices (in panel c) and quantities (in panel d) before the pandemic, and we find a significant reduction in prices for 2020 Q2. For quantities, the market remains quite stable, at least in the short-run.

We now turn to the results of the regressions Equation (2), Equation (3), and the instrumental variable specification in (4) and (5) applied to house transactions. They are shown in Table 3.[22]

Columns 1 to 5 present the results for prices. We find a statistically significant decrease in transaction prices in treated parishes *vis à vis* the comparison ones of 4.8%. The intensity of treatment applied to sample average yields a higher negative impact of 6.1% ($-0,356 \times (19\% - 2\%)$), and the IV estimate is even higher (7.6%), consistent with the result for rents. When we include Porto, the coefficients remain negative and statistically significant, with a stronger and more precise magnitude of the effect.

Turning to quantities, in columns 6 to 10, we find no statistically significant effects, except

[22] We show that our findings remain very similar if we exclude the first quarter of 2020 in Table A6 in the Appendix.

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in the continuous treatment case when we include Porto civil parishes, albeit this result is only significant at the 10% level.

Table 3: Difference-in-Differences - Sales Market

| | Average Sale Prices | | | | | Quantities for Sale | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Lisbon | | | +Porto | | Lisbon | | | +Porto | |
| | DiD (1) | DiD (2) | DiD-IV (3) | DiD (4) | DiD (5) | DiD (6) | DiD (7) | DiD-IV (8) | DiD (9) | DiD (10) |
| *Post · hdens* | -0.048* | | | -0.082*** | | 0.024 | | | -0.009 | |
| | (0.0288) | | | (0.0247) | | (0.0514) | | | (0.0500) | |
| *Post · STRDensity* | | -0.356** | -0.444*** | | -0.432*** | | -0.123 | -0.253 | | -0.292* |
| | | (0.145) | (0.121) | | (0.131) | | (0.171) | (0.174) | | (0.175) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 213 | 213 | 213 | 276 | 276 | 216 | 216 | 216 | 279 | 279 |
| R-squared | 0.925 | 0.928 | 0.928 | 0.948 | 0.949 | 0.935 | 0.935 | 0.935 | 0.922 | 0.923 |
| KP 1st Stage F-stat | | | 467.71 | | | | | 460.36 | | |

Notes: The treated areas are *Avenidas Novas, Arroios, Estrela,Misericórdia, Santa Maria Maior, Santo António, São Vicente*, as well as *Bonfim and UF Centro Histórico do Porto* when including Porto's civil parishes in the regressions. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For more information on the DID-IV regressions see Table A7 in the Appendix.

We now report the heterogeneous effects, presented in Table A8 and Table A9 in the Appendix. The pricing effects are concentrated in two- and three-bedroom apartments. Regarding quantities for sale, we find modest evidence for increased sales of the same types of dwellings, with low statistical significance and not robust across specifications.

Despite a decrease in sale prices, the magnitude of changes in quantities for sale is low, suggesting that the sales market was less affected by the pandemic than the rental market.

This is consistent with a slight decrease in demand and no change in supply of houses for sale. Low interest rates and moratoriums have brought some financial relief and allowed property owners to hold on to their dwellings, which may explain the non-existent spike in units for sale.[23] Moreover, as we are estimating short run effects, the low magnitude of outcomes in the sale market is not surprising. These results reinforce the hypothesis that the most significant changes occurred in the rental market, due to a reallocation of dwellings back to the traditional and more long-term market. Data from RNAL reports listings in Lisbon

[23] According to Bank of Portugal, interest rates on mortgage payments were 0.87% in October, a year on year fall of 0.15%, and there were 751725 moratoriums granted by the end of September, of which 42% were related to credit from housing contracts.

fell from 19477 to 19356[24] , however, as hosts can keep the short-term licence while in the long-term rental market, these values underestimate the full extent of housing reallocation.

## 5 Conclusions

In this paper, we analyze the effect of the sudden and large shock on the inflow of tourists caused by the unexpected onset of the SARS-CoV-2 pandemic in the city of Lisbon, the capital of Portugal. This is a natural setup to analyze the question for several reasons. Firstly, according to the OECD, in 2018, Portugal ranked first in terms of the contribution of tourism to the country's economy. Secondly, the tourism shock was overwhelming: the number of overnight stays went down to its 1993 level, mostly driven by a 75% contraction in the stays of foreign tourists when compared to 2019. Thirdly, Portugal, and Lisbon in particular, witnessed a rapid tourism boom, accompanied by a three-fold growth in short-term rental overnight stays, in the six years prior to the pandemic. Finally, the treatment and comparison areas are very similar in terms of population density, political preferences and socioeconomic composition, and therefore we provide the ideal setup to analyse the impact of the pandemic within the city boundaries.

Our empirical strategy exploits the sudden and sharp decrease in tourism caused by the pandemic. We provide an estimate of the impact of the covid-19 crisis on the housing market in a capital city with high density of short-term rental properties. We thus have a natural identification strategy to obtain causal estimates of the impact of the short-term rental market on the rental and sale markets in a country capital very demanded by tourists. In our setting, other potential mechanisms capable of influencing housing prices, such as homeworking and amenities, are unlikely to bias our findings as we rely on a within urban areas comparison taking advantage of small geographic units of analysis.

We use data on rents and transactions for the 24 civil parishes of the city of Lisbon

[24]See https://travelbi.turismodeportugal.pt/pt-pt/Paginas/PowerBI/rnal-registo-nacional-de-alojamento-local.aspx

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between the third quarter of 2018 and the third quarter of 2020. We sometimes add data for the 7 civil parishes of Porto, the second largest city in the country, for robustness estimates. We use three difference-in-differences specifications. The first is a binary treatment where civil parishes are assigned to the treatment group when they contain areas that were subject to bans on new registries imposed by the municipalities of Lisbon and Porto in 2018 and 2019. The second uses the share of short-term rental properties in the total number of dwellings in each civil parish as a measure of the intensity of treatment. Finally, we complement our analysis using the density of museums in each parish to instrument the intensity of short-term rental dwellings.

Our results are consistent in the three specifications. We find a consistent and sizeable impact on the rental market in Lisbon's most touristic areas. Rents decrease by 4.1% more in treated areas *vis-à-vis* comparison ones. We also document an increase of around 20% in the number of houses in the long-term rental market. This is convincing evidence that landlords reallocated their dwellings to the long-term rental market, which they can do at no cost given the Portuguese institutional setup. Regarding properties for sale, we find no statistical significant impact on the quantity of dwellings being offered in the market. Prices decrease between 4.8% and 7.6%, depending on the specifications. This is consistent with a demand side effect, with a shift in the demand for housing, possibly due to the loss in the potential income stream due to the shrinking tourism market.

The almost sudden stop of tourists creates heterogeneous wealth effects depending on one's position in the housing market. It is good news for tenants, particularly if the market is flexible enough to allow incumbents to change to new contracts. It also improves the housing affordability of non-incumbent house owners. However, it represents a negative shock for incumbents. One should bear in mind that we provide a very short-run analysis, just three quarters into the shock, and we do not observe important components of the rental contracts such as their duration. It may well happen that landlords are negotiating not too long contracts with tenants in the hope of reallocating the unit to the short-term market if

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and when tourists come back. This is an interesting avenue for future research.

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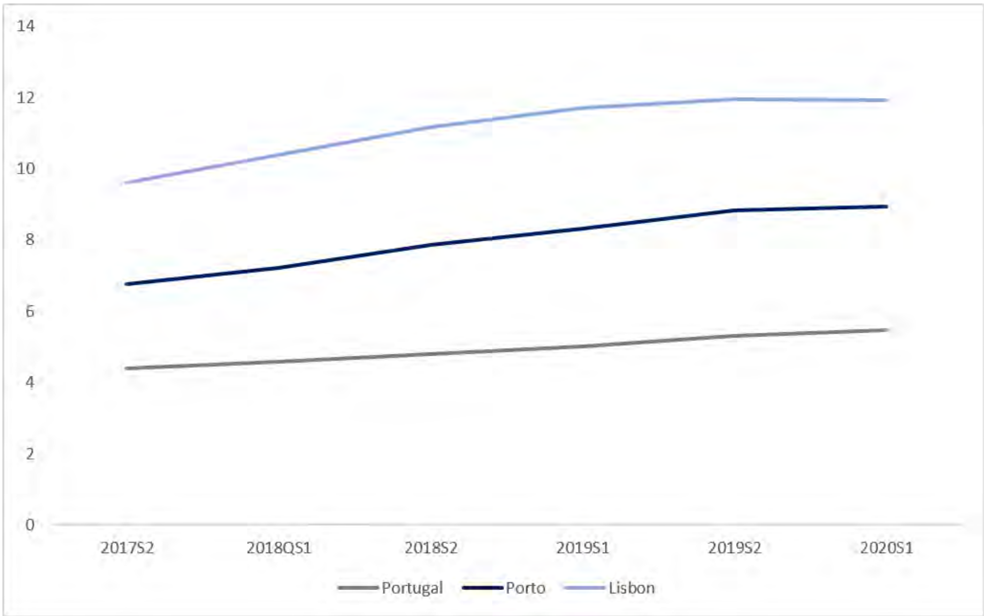
# A Figures

Figure A1: Median Rental Price (€) per Square Meter




Source: National Statistics Institute

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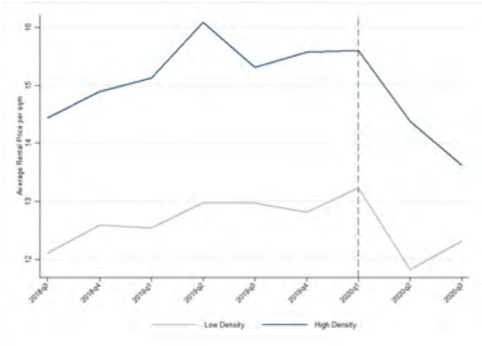
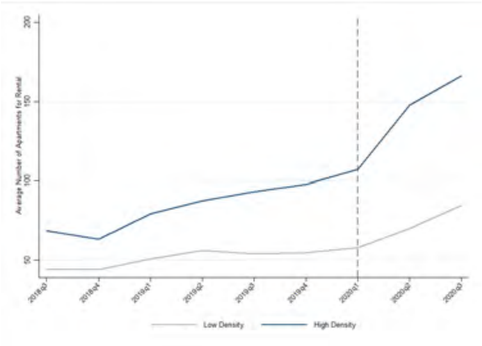
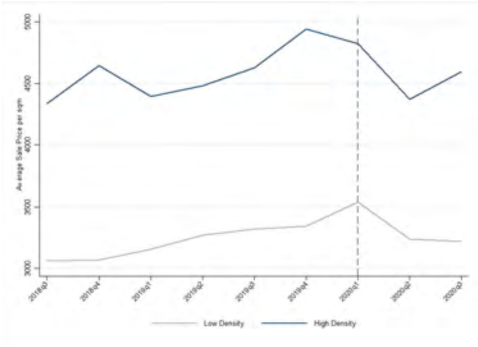
Figure A2: Trends for Outcome Variables



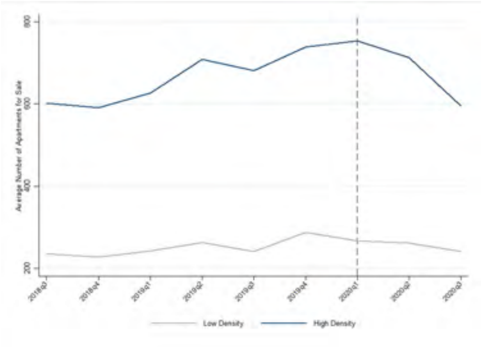

(a) Rental Prices




(b) Quantities for Rental



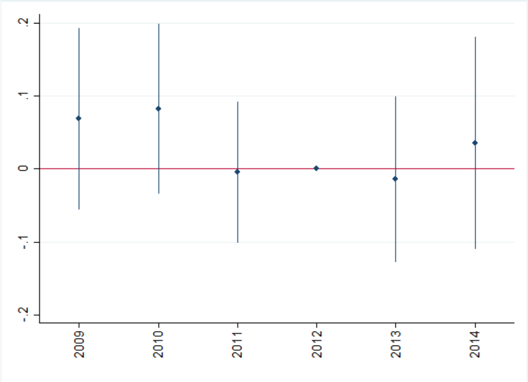

(c) Sale Prices




(d) Quantities for Sale

N= 212. High Density: *Avenidas Novas, Arroios, Estrela, Misericórdia, Santa Maria Maior, Santo António, São Vicente.*

Figure A3: Event study for the Density of Museums




Notes: N= 144. The treatment group consists of the 7 civil parishes with the highest density of museums - *Alcântara, Avenidas Novas, Estrela, Misericórdia, Santa Maria Maior, Santo António, and S.Vicente*. The regression includes year and civil parishes fixed effects. The "placebo" treatment period starts in 2013. We remove from this sample the two museums built after 2014. 95% confidence intervals computed with standard errors clustered by civil parish.

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# B Tables

Table A1: Balance Tests

| | Pre-Treatment | | |
|---|---|---|---|
| | High Density | Low Density | Difference |
| Number of Civil Parishes | 7 | 17 | - |
| % Alignment | 0.46 | 0.50 | -0.04** |
| | | | (0.017) |
| % Turnout | 0.55 | 0.53 | 0.02 |
| | | | (0.012) |
| % Higher Education | 0.04 | 0.05 | -0.01 |
| | | | (0.01) |
| Population Density (Nº/km2) | 7476.612 | 6231.389 | 1245.223 |
| | | | (1493.976) |
| *A.Rental Market* | | | |
| Average Rental Price (€/m2) | 15.24 | 12.66 | 2.576*** |
| | | | (0.500) |
| Number of Apartments for Rental (per civil parish) | 82 | 51 | 31** |
| | | | (13.380) |
| *B.Sales Market* | | | |
| Average Sale Price (€/m2) | 4570.93 | 3202.44 | 1368.49*** |
| | | | (305.353) |
| Number of Apartments for Sale (per civil parish) | 658 | 250 | 408*** |
| | | | (50.495) |
| *C.Short-term Rentals* | | | |
| Density of Short-term Rental Accommodations | 0.19 | 0.02 | 0.176*** |
| | | | (0.05) |
| *D.Amenities* | | | |
| Monuments | 6 | 1 | 5** |
| | | | (2.184) |
| Museums | 5 | 1 | 4* |
| | | | (1.919) |
| Prémio Valmor | 14 | 10 | 4 |
| | | | (4.836) |
| Retailers | 9 | 8 | 1 |
| | | | (3.101) |
| Banks | 33 | 18 | 15 |
| | | | (11.739) |

Notes: The control group is composed by civil parishes in low density areas. % High Education is the percentage of citizens with higher education as given in the 2011 Census. *Retailers* includes stores as given by the law nº12/2004, such as supermarkets. *Prémio Valmor* is a Portuguese architectural award granted to buildings.

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Table A2: Difference in Differences - Rental Market

| | Average Rental Prices | | | | Quantities for Rental | | | |
|---|---|---|---|---|---|---|---|---|
| | Lisbon | | +Porto | | Lisbon | | +Porto | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Post \cdot hdens$ | -0.0413* | | -0.0338 | | 0.243*** | | 0.259*** | |
| | (0.0240) | | (0.0221) | | (0.0613) | | (0.0575) | |
| $Post \cdot STRDensity$ | | -0.215* | | -0.203* | | 1.323*** | | 1.437*** |
| | | (0.110) | | (0.107) | | (0.146) | | (0.174) |
| Observations | 188 | 188 | 236 | 236 | 192 | 192 | 248 | 248 |
| R-squared | 0.799 | 0.801 | 0.877 | 0.878 | 0.961 | 0.965 | 0.950 | 0.952 |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The treated areas are *Avenidas Novas, Arroios, Estrela,Misericórdia, Santa Maria Maior, Santo António, São Vicente*, as well as *Bonfim and UF Centro Histórico do Porto* when including Porto's civil parishes in the regressions. Robust standard errors are depicted in parenthesis. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A3: Instrumental Variables Estimates - Rental Market

| | Average Rental Prices | | Quantities for Rental | |
|---|---|---|---|---|
| | First Stage | Second Stage | First Stage | Second Stage |
| museum dens | 0.0751*** | | 0.0749*** | |
| | (0.00351) | | (0.00351) | |
| $Post \cdot STRDensity$ | | -0.239** | | 1.222*** |
| | | (0.101) | | (0.173) |
| Civil Parish FE | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES |
| Observations | 212 | 212 | 216 | 216 |
| R-squared | 0.894 | 0.783 | 0.892 | 0.965 |
| KP 1st Stage F-Stat | 461.84 | | 460.36 | |

Notes: Treated and control groups are defined as for the difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

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Table A4: Heterogeneous Effects - ln(Rental Prices)

| | Lisbon | | | | | | +Porto | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 Room or Less | | 2 Rooms | | 3 Rooms | | 1 Room or Less | | 2 Rooms | | 3 Rooms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *Posthdens* | -0.0411 | | -0.0617** | | -0.0137 | | -0.0259 | | -0.0668** | | -0.0150 | |
| | (0.0328) | | (0.0304) | | (0.0534) | | (0.0346) | | (0.0310) | | (0.0509) | |
| $Post \cdot STRDensity$ | | -0.216 | | -0.369** | | -0.437* | | -0.216 | | -0.357** | | -0.415* |
| | | (0.174) | | (0.176) | | (0.197) | | (0.172) | | (0.169) | | (0.206) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 142 | 142 | 163 | 163 | 86 | 86 | 174 | 174 | 189 | 189 | 95 | 95 |
| R-squared | 0.592 | 0.594 | 0.741 | 0.748 | 0.747 | 0.766 | 0.794 | 0.796 | 0.836 | 0.838 | 0.851 | 0.779 |

Notes: Treated and control groups are defined as for the difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Heterogeneous Effects - ln(Quantities for Rental)

| | Lisbon | | | | | | +Porto | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 Room or Less | | 2 Rooms | | 3 Rooms | | 1 Room or Less | | 2 Rooms | | 3 Rooms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| $Post \cdot hdens$ | 0.221** | | 0.043 | | 0.056 | | 0.228** | | 0.015 | | 0.0306 | |
| | (0.013) | | (0.0777) | | (0.09) | | (0.085) | | (0.0747) | | (0.0924) | |
| $Post \cdot STRDensity$ | | 1.344*** | | 0.885*** | | 0.0281 | | 1.452*** | | 0.684** | | 0.331 |
| | | (0.365) | | (0.224) | | (0.365) | | (0.336) | | (0.266) | | (0.362) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 142 | 142 | 163 | 163 | 86 | 86 | 174 | 174 | 189 | 189 | 95 | 95 |
| R-squared | 0.874 | 0.870 | 0.888 | 0.894 | 0.891 | 0.891 | 0.885 | 0.878 | 0.901 | 0.903 | 0.896 | 0.896 |

Notes: Treated and control groups are defined as for the baseline difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A6: Difference in Differences - Sales Market

| | Average Sale Prices | | | | Quantities for Sale | | | |
|---|---|---|---|---|---|---|---|---|
| | Lisbon | | +Porto | | Lisbon | | +Porto | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Post \cdot hdens$ | -0.0505 | | -0.0928*** | | -0.0156 | | -0.0416 | |
| | (0.0343) | | (0.0297) | | (0.0642) | | (0.0637) | |
| $Post \cdot STRDensity$ | | -0.345* | | -0.450*** | | -0.236 | | -0.426* |
| | | (0.191) | | (0.169) | | (0.215) | | (0.216) |
| Observations | 189 | 189 | 245 | 245 | 192 | 192 | 248 | 248 |
| R-squared | 0.930 | 0.933 | 0.949 | 0.949 | 0.937 | 0.937 | 0.922 | 0.923 |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Treated and control groups are defined as for the baseline difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Instrumental Variables Estimates - Sales Market

| | Average Sale Prices | | Quantities for Sale | |
|---|---|---|---|---|
| | First Stage | Second Stage | First Stage | Second Stage |
| museum dens | 0.0756*** | | 0.0749*** | |
| | (0.00351) | | (0.00351) | |
| $Post \cdot STRDensity$ | | -0.444*** | | -0.253 |
| | | (0.121) | | (0.174) |
| Civil Parish FE | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES |
| Observations | 213 | 213 | 216 | 216 |
| R-squared | 0.897 | 0.928 | 0.892 | 0.935 |
| KP 1st Stage F-Stat | 467.71 | | 460.36 | |

Notes: Treatment and control groups are defined as in the difference-in-differences specifications. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A8: Heterogeneous Effects - ln(Sale Prices)

| | Lisbon | | | | | | +Porto | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 Room or Less | | 2 Rooms | | 3 Rooms | | 1 Room or Less | | 2 Rooms | | 3 Rooms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *Post · hdens* | -0.0160 | | -0.0905** | | -0.111** | | -0.00723 | | -0.104*** | | -0.137*** | |
| | (0.0465) | | (0.0514) | | (0.0461) | | (0.0436) | | (0.0399) | | (0.05) | |
| *Post · STRDensity* | | -0.236 | | -0.622*** | | -0.853*** | | -0.167 | | -0.684*** | | -0.968*** |
| | | (0.221) | | (0.182) | | (0.202) | | (0.203) | | (0.170) | | (0.197) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations Observations | 125 | 125 | 173 | 173 | 132 | 132 | 156 | 156 | 213 | 213 | 168 | 168 |
| R-squared | 0.860 | 0.863 | 0.872 | 0.883 | 0.882 | 0.892 | 0.900 | 0.901 | 0.924 | 0.929 | 0.923 | 0.928 |

Notes: Treated and control groups are defined as for the difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Heterogeneous Effects - ln(Quantities for Sale)

| | Lisbon | | | | | | +Porto | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 Room or Less | | 2 Rooms | | 3 Rooms | | 1 Room or Less | | 2 Rooms | | 3 Rooms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *Post · hdens* | -0.022 | | 0.159* | | 0.208* | | -0.011 | | 0.101 | | 0.060 | |
| | (0.0946) | | (0.081) | | (0.092) | | (0.086) | | (0.082) | | (0.096) | |
| *Post · STRDensity* | | -0.329 | | 0.315 | | 0.433 | | -0.418 | | 0.023** | | -0.0847 |
| | | (0.266) | | (0.243) | | (0.249) | | (0.265) | | (0.210) | | (0.403) |
| Civil Parish FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter FE | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 125 | 125 | 173 | 173 | 132 | 132 | 156 | 156 | 213 | 213 | 168 | 168 |
| R-squared | 0.948 | 0.930 | 0.888 | 0.892 | 0.830 | 0.864 | 0.955 | 0.955 | 0.872 | 0.871 | 0.828 | 0.828 |

Notes: Treated and control groups are defined as for the baseline difference-in-differences specifications before. Robust standard errors are depicted in parenthesis. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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# COVID-era trade policy passthrough to trade flows: Idiosyncratic or not?

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Date submitted: 6 May 2021; Date accepted: 11 May 2021

*In an original contribution to the COVID-19 and trade literatures, we examine the trade policy passthrough to trade flows of restrictive and liberalizing measures imposed on exports and imports of food and medical products (Evenett et al. 2021) during the first nine months of 2020 for a sample of 142 countries. We find that where the imposition of trade policy measures is more consistent with theoretical/conceptual predictions, such measures are also found to be associated with trade flow changes that are less idiosyncratic. This is found to be true in general for trade policy measures imposed on exports and imports of medical products; for food products, stylized facts suggest that the trade policy activism may have been more idiosyncratic. In some cases, however, accounting for sample heterogeneities renders the results less idiosyncratic. On the whole, our results suggest that richer, more globally-integrated economies with high levels of government effectiveness may have exhibited higher trade policy effectiveness during the first nine months of the pandemic.*

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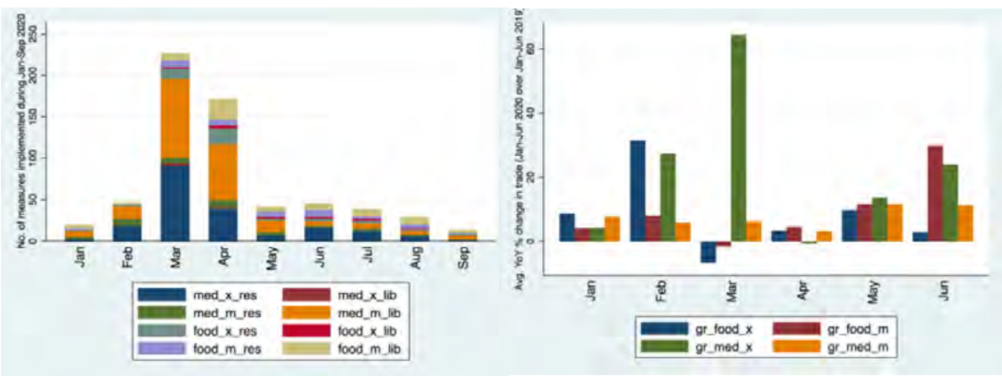
## 1. Introduction

COVID-19 has been an unprecedented health and economic shock, resulting in lockdowns and stalling of economic activity in countries across the world. An immediate casualty of the pandemic has been disruption of GVCs along with a sharp fall in international trade, both merchandise and services, emanating from the concomitant supply and demand shock, GVC contagion effect (Baldwin and Freeman, 2020; Friedt and Zhang, 2020) as well as social distancing-related practices.

Significantly, despite the general decline in trade across countries and sectors and the imposition of restrictive measures, exports and imports of food and medical products did not report declines on a YoY basis (relative to the same period in 2019), with the exception of trade in food products during May and that of medical exports in April 2020 (Figure 1, right panel). Not surprisingly, exports of medical goods alone witnessed a 65 percent increase in March 2020 relative to their value in 2019. This trend was visible across most countries including Pakistan, the Philippines, Serbia, Slovakia and Slovenia that experienced an average growth rate of over a 100 percent in the first six months of 2020 compared to 2019. Similarly, June 2020 witnessed the highest growth rate in food imports, averaging 29.8 percent relative to 2019, followed by the month of May when food imports rose by 11.6 percent compared to the previous year. The highest positive growth rates were seen for Australia, Netherlands, Philippines, Switzerland, and the UK (see Figure 2).

**Figure 1: Count of trade policy measures imposed on food and medical products (left panel) and average YoY change (%) in the trade of these products (right panel)**




*Source: Covid-19 Trade Policy database; own calculations* *Source: UNCTAD; own calculations*

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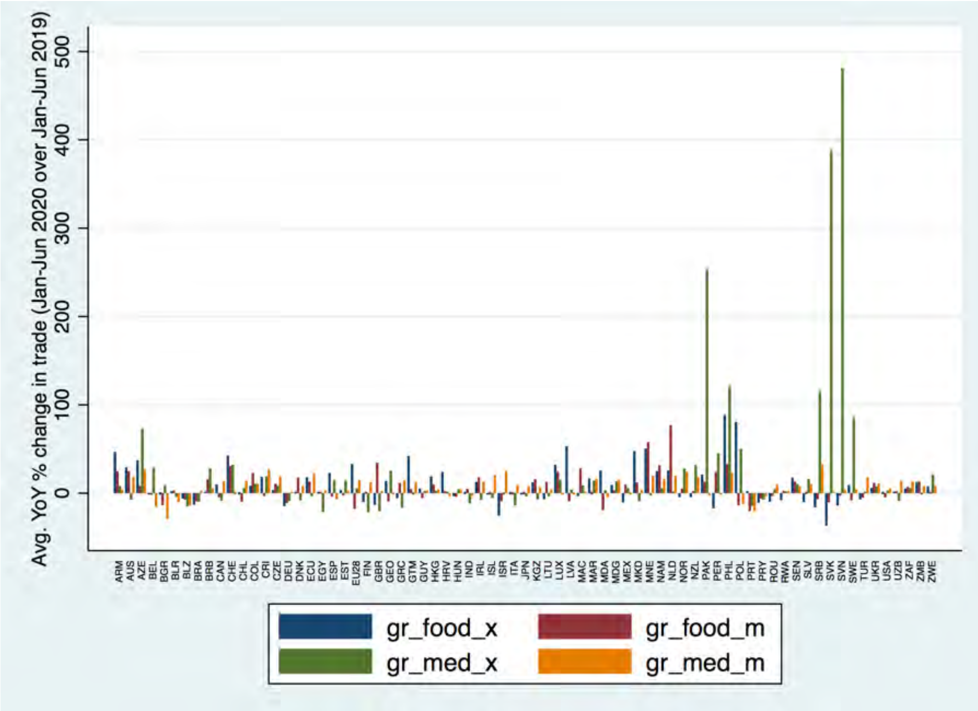
**Figure 2: Average YoY change (%) in trade of food and medical products by country**




*Source: UNCTAD; own calculations*

One element of the policy response to the pandemic has been the active use of trade policy instruments by governments to promote access to essential supplies. A joint project of the European University Institute (EUI), the Global Trade Alert (GTA) and the World Bank (WB) has compiled a database on trade policy measures affecting trade in food and medical goods put in place since the beginning of 2020 (Evenett et al. 2021).

These data show that, as of September 2020, 179 and 458 trade policy measures were imposed on food and medical products, respectively. These measures, that largely comprised import liberalization of and export restrictions on medical goods[1], were mostly imposed during March and April of 2020 and have witnessed a consistent decline since June 2020 (Figure 1, left panel). While high and upper middle-income countries have enacted more trade policy measures in the medical sector, lower-middle and low-income countries seem to have displayed an equal preference for the two sectors (see Figure 3).

[1] Measures on medical goods include 221 import policy reforms implemented in 59 jurisdictions and 192 export controls imposed by 64 countries. In contrast, measures on food products include 78 import policy reforms implemented in 27 jurisdictions and 46 export controls imposed by 27 countries. For more stylized facts on the Covid-19 Trade Policy database, see Evenett et al. (2021).

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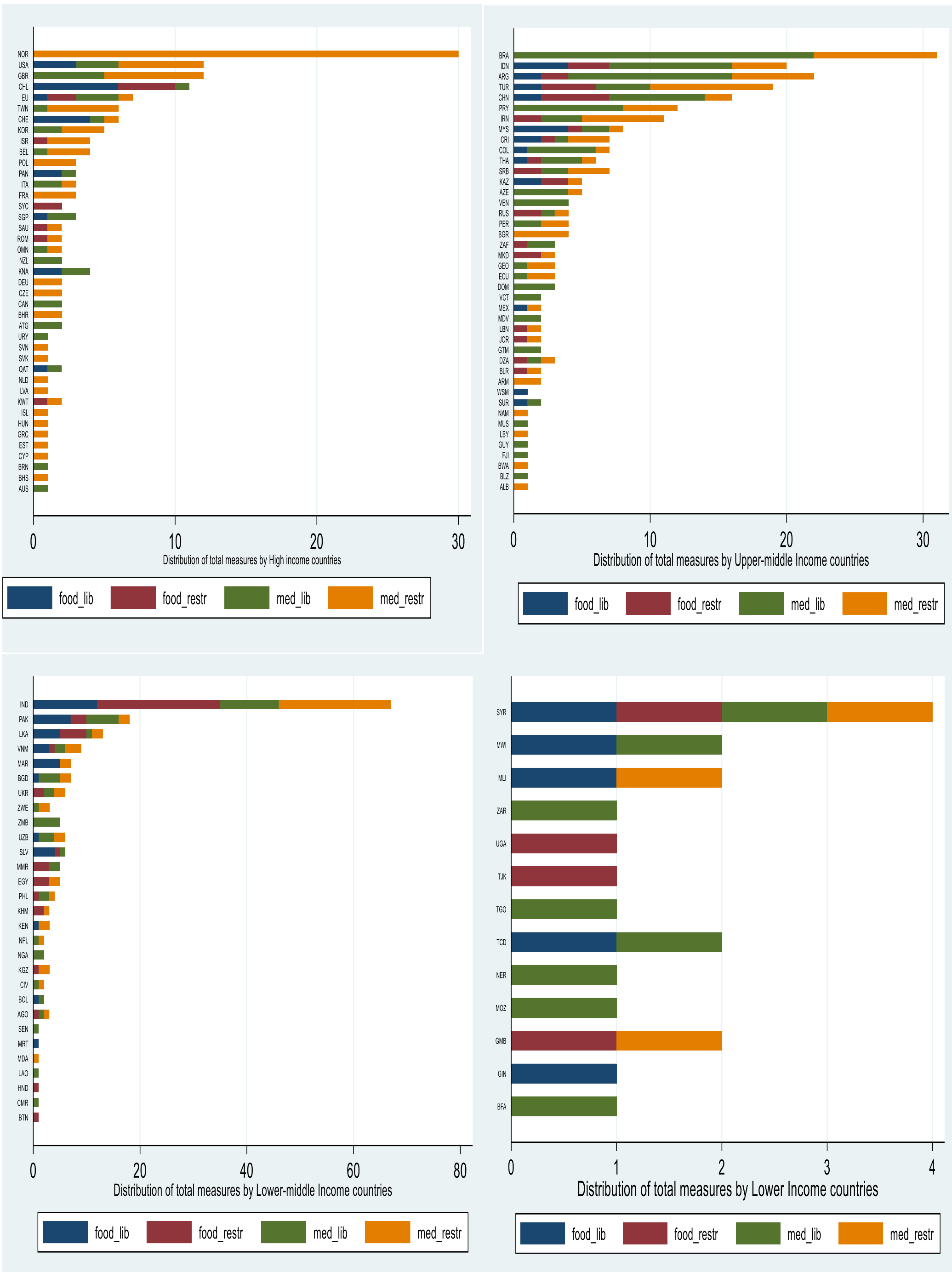
**Figure 3: Distribution of trade policy measures by country across income levels**




*Source: COVID-19 Trade Policy database; own calculations*

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Trade can play a pivotal role in emergencies to ensure that the supply of essential goods such as medical supplies gets to where it is needed (Gereffi, 2020; OECD, 2020). Instead, governments have imposed export control measures and requisitioned domestic supplies of essential goods during the current pandemic. Such reactions tend to exacerbate rather than facilitate provision of vital equipment to healthcare workers by increasing prices, market volatility and distorting investment decisions (Fiorini et al. 2020). The associated disruption to crisis health planning in trading partners makes net importers of medical products particularly vulnerable (Baldwin and Evenett, 2020). While WTO rules permit use of trade restrictions in public emergencies, they require these to be temporary (lasting only for the crisis duration). This is because the use of export controls can lead to negative spillovers, including by constraining the ability of firms to ramp up production, resulting in increased prices, and impeding the ability of other countries to import supplies (Atkinson et al. 2020; Evenett, 2020; Gereffi, 2020; Hoekman et al. 2020).

A rise in imports of essential commodities is an obvious response to excess demand at home or backed by the need to ensure sufficient supply in the home country during a crisis, but there may be other factors at play governing trade policy activism.

Leibovici and Santacreu (2020) suggest that the heterogeneous response of trade policy across countries may be systematically related to the extent to which countries are net importers or net exporters of essential goods. According to their analysis, net importers of essential goods are not competitive in their production and any rise in trade barriers following a shock like this pandemic only makes it more difficult for these countries to import the essential goods, besides raising their prices and leading to an overall loss in welfare; this induces these countries to lower trade barriers. In contrast, while net exporters of essential goods may ex-ante prefer to live in a world with low trade costs, when a global pandemic hits, they may be tempted to renege on their commitments and increase trade barriers.

In other work, Hoekman et al. (2021) examine whether trade policy activism on medical products during the pandemic was associated with attributes of public procurement regimes of the reporting countries. They find the imposition of export restrictions on medical products during the first seven months of 2020 to be strongly positively correlated with the average time and number of steps required to complete public procurement processes in the pre-crisis period.

In this paper, we consider other economic, institutional and market size determinants of countries' trade policy activism during the pandemic and study the relationship between the measures imposed and both levels of and YoY changes (relative to the corresponding month in 2019) in export and import of food and medical products. One objective of the analysis is to examine if the policy response and the trade outcomes were idiosyncratic or consistent with theoretical/conceptual predictions. A testable proposition in this regard is that imposition of trade policy measures that are more consistent with theoretical/conceptual predictions are also more likely to be associated with trade flow changes that are less idiosyncratic.

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Our results suggest that richer, more globally-integrated economies with high levels of government effectiveness may have exhibited higher trade policy effectiveness during the first nine months of the pandemic. For this set of countries, trade (both exports and imports of food and medical products) largely responded in line with the liberalizing and restrictive measure implemented during the pandemic, the imposition of these measures also being consistent with theoretical/conceptual predictions. The absence of idiosyncratic behavior, especially during a crisis period, suggests that these countries may have had the right incentives and mechanisms in place even in times preceding the pandemic.

The rest of the paper is structured as follows. Section 2 discusses the likely economic and institutional determinants of trade policy imposition and the resultant trade flows during crises to provide a conceptual framework motivating the empirical analysis in the paper. Section 3 presents stylized facts to examine the propositions outlined in Section 2. Section 4 discusses the empirical methodology and data sources while Section 5 discusses results from estimation. Section 6 concludes.

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## 2. Likely determinants of trade policy activism and trade flows during the pandemic

The imposition of trade policy measures on essential goods during a crisis can be governed by economic and institutional factors (as observed in the analysis in Leibovici and Santacreu, 2020 and Hoekman et al. 2021, respectively) or be completely idiosyncratic. In this section, we discuss some of the economic, institutional and demand-related factors that may determine both trade policy during a crisis and the response of trade flows to the imposition of such measures. This discussion serves to provide the conceptual framework on the basis of which we group countries in the empirical analysis that follows to examine if the trade policy response to COVID-19 and its relationship with actual trade flows was consistent with theoretical/conceptual predictions or idiosyncratic.

To begin with, large, populous countries are likely to have market power, which in turn may induce governments to use trade policy to affect their terms of trade. At the same time, such countries are likely to liberalize imports and restrict exports of essential products to meet domestic demand, especially if they lack domestic capacity, and observe a commensurate response in trade flows.

Government effectiveness is directly correlated with per capita income as well as openness to trade (Han et al. 2014; Hoekman et al. 2020). A more effective government is more likely to adapt procurement processes to source needed medical supplies, thereby reducing the incentive to change trade policy or be restrictive. Such countries are also likely to witness trade respond to their policies along expected lines.

Import shares, revealed comparative advantages and import tariffs proxy the political economy forces that shape both trade policy and trade outcomes. Higher import dependence is suggestive of a pro-liberalization domestic political economy. At the same time, high levels of import restrictions suggest that a country has industrial development goals. Such countries may relax

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import barriers temporarily to improve access to essential commodities but are more likely to reimpose import barriers more rapidly to support local production, reflecting prevailing industrial policy objectives and/or a desire to facilitate domestic capacity for exports. At the same time, such import restrictions can also hurt exports by Lerner's symmetry, leading to reduced trade flows along both dimensions.

Meanwhile, a revealed comparative advantage in exports of essential products implies supply-side capacity that may induce governments to remove export controls more rapidly if a decision is taken to temporarily restrict exports in response to the pandemic spike in demand. On the whole, however, such countries are more likely to liberalize exports and also observe a consequent increase in trade flows.

Openness to trade, membership of trade agreements and participation and position in GVCs are also likely to be significant determinants of both trade policy and trade outcomes during the pandemic. More open economies, well integrated into the multilateral and preferential trading system and GVCs, and countries more upstream in value-chains are also likely to be less restrictive/more liberalizing and witness an increase in their trade of essential products.

Finally, imposition of trade policy measures is also likely governed by the level of economic development and the productive capacity of its domestic industry, and by geography and distance from and to its most important markets, as these have a direct bearing on trade flows. At the same time, the number of cases and casualties related to COVID-19 is likely to be positively correlated with trade-policy activism by countries across measures and also at least be directly related to their imports of medical products.

Against this background, we provide first "estimates" of the impact of COVID-era trade policy measures on exports and imports of food and medical products to which these measures are applicable. In doing so, we also examine if the imposition of measures underlying the hypotheses in Leibovici and Santacreu (2020) delivered the desired outcome. We further motivate our empirical analysis by providing stylized facts in the following section to examine the different propositions outlined in this section.

## 3. Stylized facts

We begin by examining the hypotheses in Leibovici and Santacreu (2020) and find these to be largely inconsistent with the stylized facts on trade measures reported in the EUI-GTA-WB COVID-19 Trade Policy database (Evenett et al. 2021). For instance, net food importers accounted for 70% of total liberalization measures imposed on food imports between January and September 2020 but they also imposed 76% of total restrictive measures on food imports. Meanwhile, net food exporters accounted for three-fifths of total restrictive measures imposed on food exports but they also imposed 10 out of the 13 liberalization measures on food exports (see Table 1).

Similarly, while net importers of medical goods accounted for 86% of total liberalization measures imposed on these products over this period they also imposed 76% of total restrictive measures on these imports. Meanwhile, net exporters[2] of medical goods accounted for only a fifth of total restrictive measures imposed on these exports besides imposing one of three liberalization measures on exports of these products.[3]

Interestingly, net exporters of food and medical products exceeded net importing countries in the average imports of these products over this period; moreover, net exporters of medical products exported US$50 million less in value on average over this period compared to net importers of these products (the full list of these countries is reported in Annex table 1). This suggests that demand for these essential goods has been uniformly high, irrespective of countries being net exporters or importers of these products.

Meanwhile, there seems to be suggestive evidence in the stylized facts for the role of population, per capita income, government effectiveness, GVC-integration and political economy factors driving the imposition of liberalizing and restrictive trade policy measures on both imports and exports, especially in the case of medical products. Moreover, as expected, higher COVID-19-incidence is positively correlated with trade policy activism across measures.[4]

The stylized facts reported in Table 1 suggest that more heavily populated countries liberalized a larger share of food exports and imposed a larger share of import restrictions on medical products, both of which run counter to expectations, though, as expected, they also liberalized a much smaller share of medical exports than less populated countries. The relationship of these measures with trade flows of this country sample is thus likely to be mixed.

Countries with higher levels of per capita income than the sample median liberalized a larger share of medical exports than the rest; they also liberalized a larger share of food exports during the first six months of the year than the rest of the country sample. These stylized facts are consistent with their likely domestic capacity in producing and comparative advantage in exporting these products, which is also likely to be reflected in an expected relationship with trade flows.

More effective governments have liberalized a larger share of medical exports and imposed a lower share of restrictions on medical imports than countries with less-effective governance indicators. Both these stylized facts are consistent with expectations and also suggest that the relationship of the imposed measures on trade flows is likely to be as expected.

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[2] In each case, trade balance was calculated as the difference between a country's total exports and imports of these products based on trade values averaged over 2017-18.
[3] These stylized facts remain qualitatively similar if we break down the sample period into two to examine the immediate and short-run response to the pandemic.
[4] The COVID-19 incidence measures are based on data during January-September 2020. Data on all other variables (except for GVC-participation and GVC-position that pertain to 2015), on the basis of which the total sample is "split" above and below median to present stylized facts in this section, pertain to average values over 2017-2018. All data sources are discussed in Section 4.

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**Table 1: Stylized facts on likely determinants of trade policy activism**

| Variable | Status | Period | food_m_lib | food_m_res | food_x_lib | food_x_res | med_m_lib | med_m_res | med_x_lib | med_x_res |
|---|---|---|---|---|---|---|---|---|---|---|
| Population | Above sample median | Jan-Sep | 59 | 58 | 100 | 63 | 56 | 85 | 30 | 52 |
| | Below sample median | Jan-Sep | 41 | 42 | 0 | 37 | 44 | 15 | 70 | 48 |
| | Above sample median | Jan-Jun | 55 | 55 | 100 | 58 | 54 | 86 | 30 | 52 |
| | Below sample median | Jan-Jun | 45 | 45 | 0 | 42 | 46 | 14 | 70 | 48 |
| | Above sample median | Jul-Sep | 74 | 78 | 100 | 100 | 100 | 83 | | 47 |
| | Below sample median | Jul-Sep | 26 | 22 | 0 | 0 | 0 | 17 | | 53 |
| OECD member | No | Jan-Sep | 55 | 44 | 85 | 71 | 61 | 83 | 30 | 54 |
| | Yes | Jan-Sep | 45 | 56 | 15 | 29 | 39 | 17 | 70 | 46 |
| | No | Jan-Jun | 55 | 43 | 80 | 68 | 61 | 80 | 30 | 56 |
| | Yes | Jan-Jun | 45 | 57 | 20 | 32 | 39 | 20 | 70 | 44 |
| | No | Jul-Sep | 57 | 44 | 100 | 100 | 75 | 100 | | 37 |
| | Yes | Jul-Sep | 43 | 56 | 0 | 0 | 25 | 0 | | 63 |
| Government effectiveness | Above sample median | Jan-Sep | 48 | 64 | 23 | 33 | 51 | 15 | 85 | 55 |
| | Below sample median | Jan-Sep | 52 | 36 | 77 | 67 | 49 | 85 | 15 | 45 |
| | Above sample median | Jan-Jun | 53 | 64 | 30 | 35 | 52 | 14 | 85 | 55 |
| | Below sample median | Jan-Jun | 47 | 36 | 70 | 65 | 48 | 86 | 15 | 45 |
| | Above sample median | Jul-Sep | 30 | 67 | 0 | 14 | 25 | 17 | | 63 |
| | Below sample median | Jul-Sep | 70 | 33 | 100 | 86 | 75 | 83 | | 37 |
| Number of PTAs | Above sample median | Jan-Sep | 68 | 74 | 69 | 53 | 66 | 80 | 67 | 70 |
| | Below sample median | Jan-Sep | 32 | 26 | 31 | 47 | 34 | 20 | 33 | 30 |
| | Above sample median | Jan-Jun | 77 | 90 | 100 | 92 | 93 | 85 | 100 | 89 |
| | Below sample median | Jan-Jun | 23 | 10 | 0 | 8 | 7 | 15 | 0 | 11 |
| | Above sample median | Jul-Sep | 86 | 85 | 25 | 88 | 100 | 87 | 100 | 97 |
| | Below sample median | Jul-Sep | 14 | 15 | 75 | 12 | 0 | 13 | 0 | 3 |

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| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Country participation in GVCs | Above sample median | Jan-Sep | 50 | 55 | 46 | 54 | 52 | 7 | 89 | 61 |
| | Below sample median | Jan-Sep | 50 | 45 | 54 | 46 | 48 | 93 | 11 | 39 |
| | Above sample median | Jan-Jun | 55 | 58 | 60 | 57 | 54 | 9 | 89 | 61 |
| | Below sample median | Jan-Jun | 45 | 42 | 40 | 43 | 46 | 91 | 11 | 39 |
| | Above sample median | Jul-Sep | 30 | 33 | 0 | 29 | 19 | 0 | | 63 |
| | Below sample median | Jul-Sep | 70 | 67 | 100 | 71 | 81 | 100 | | 37 |
| Position of country in GVCs | Upstream | Jan-Sep | 54 | 42 | 46 | 40 | 51 | 78 | 7 | 61 |
| | Downstream | Jan-Sep | 46 | 58 | 54 | 60 | 49 | 22 | 93 | 39 |
| | Upstream | Jan-Jun | 49 | 39 | 30 | 35 | 50 | 77 | 7 | 58 |
| | Downstream | Jan-Jun | 51 | 61 | 70 | 65 | 50 | 23 | 93 | 42 |
| | Upstream | Jul-Sep | 70 | 67 | 100 | 86 | 94 | 83 | | 95 |
| | Downstream | Jul-Sep | 30 | 33 | 0 | 14 | 6 | 17 | | 5 |
| Net exporter of food products | No | Jan-Sep | 61 | 65 | 23 | 46 | | | | |
| | Yes | Jan-Sep | 39 | 35 | 77 | 54 | | | | |
| | No | Jan-Jun | 59 | 65 | 30 | 48 | | | | |
| | Yes | Jan-Jun | 41 | 35 | 70 | 52 | | | | |
| | No | Jul-Sep | 70 | 67 | 0 | 29 | | | | |
| | Yes | Jul-Sep | 30 | 33 | 100 | 71 | | | | |
| Net importer of food products | No | Jan-Sep | 39 | 35 | 77 | 54 | | | | |
| | Yes | Jan-Sep | 61 | 65 | 23 | 46 | | | | |
| | No | Jan-Jun | 41 | 35 | 70 | 52 | | | | |
| | Yes | Jan-Jun | 59 | 65 | 30 | 48 | | | | |
| | No | Jul-Sep | 30 | 33 | 100 | 71 | | | | |
| | Yes | Jul-Sep | 70 | 67 | 0 | 29 | | | | |
| Avg. applied tariffs on food products | Above sample median | Jan-Sep | 61 | 55 | 54 | 64 | | | | |
| | Below sample median | Jan-Sep | 39 | 45 | 46 | 36 | | | | |

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| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Above sample median | Jan-Jun | 57 | 57 | 70 | 62 | | | | |
| | Below sample median | Jan-Jun | 43 | 43 | 30 | 38 | | | | |
| | Above sample median | Jul-Sep | 74 | 44 | 0 | 86 | | | | |
| | Below sample median | Jul-Sep | 26 | 56 | 100 | 14 | | | | |
| Net exporter of medical products | No | Jan-Sep | | | | | 78 | 76 | 63 | 78 |
| | Yes | Jan-Sep | | | | | 22 | 24 | 37 | 22 |
| | No | Jan-Jun | | | | | 77 | 71 | 63 | 78 |
| | Yes | Jan-Jun | | | | | 23 | 29 | 37 | 22 |
| | No | Jul-Sep | | | | | 100 | 100 | | 79 |
| | Yes | Jul-Sep | | | | | 0 | 0 | | 21 |
| Net importer of medical products | No | Jan-Sep | | | | | 22 | 24 | 37 | 22 |
| | Yes | Jan-Sep | | | | | 78 | 76 | 63 | 78 |
| | No | Jan-Jun | | | | | 23 | 29 | 37 | 22 |
| | Yes | Jan-Jun | | | | | 77 | 71 | 63 | 78 |
| | No | Jul-Sep | | | | | 0 | 0 | | 21 |
| | Yes | Jul-Sep | | | | | 100 | 100 | | 79 |
| Avg. applied tariffs on medical products | Above sample median | Jan-Sep | | | | | 61 | 80 | 26 | 47 |
| | Below sample median | Jan-Sep | | | | | 39 | 20 | 74 | 53 |
| | Above sample median | Jan-Jun | | | | | 61 | 77 | 26 | 47 |
| | Below sample median | Jan-Jun | | | | | 39 | 23 | 74 | 53 |
| | Above sample median | Jul-Sep | | | | | 75 | 100 | | 37 |
| | Below sample median | Jul-Sep | | | | | 25 | 0 | | 63 |

Source: COVID-19 Trade Policy database; own calculations

Note: The numbers report the percentage share of cumulative trade policy measures in the total sample for each variable, above and below the median sample value or the non-existence and existence of each variable as applicable, for the entire sample period (Jan-Sep) and for sub-periods (Jan-Jun and Jul-Sep) reflecting the immediate- and short-run following the pandemic. Legend: m=import; x=export; lib=liberalizing; res=restrictive.

With the exception of measures liberalizing medical and food exports, respectively, Contracting Parties to the WTO's plurilateral Agreement on Government Procurement (GPA) and countries with greater involvement in PTAs have resorted to less trade policy activism than GPA non-members and countries signatories to below-sample-median number of PTAs. These findings are consistent with others in the literature associating ex-ante openness with a preference for non-restrictive trade policy.

More GVC-integrated countries imposed a lower share of import restrictions on medical products but were associated with a larger share of export policy measures, both liberal and restrictive. However, countries more upstream[5] in GVCs imposed a larger share of measures restricting medical trade (with this tendency becoming even more pronounced over time) and imposed a smaller share of medical export liberalizing measures. The latter set of stylized facts runs counter to expectations and suggests that any relationship with trade flows is likely to be mixed.

Amongst factors proxying the role of political economy factors, countries exhibiting a pre-crisis revealed comparative advantage in medical exports tended to impose a larger share of export liberalization measures on these products and also imposed a smaller share of export restrictive measures during the last three months. Similarly, as expected, countries more reliant on medical imports in the pre-crisis period imposed a relatively smaller share of total restrictions on medical imports in the immediate-run though, counter-intuitively, this trend was reversed in the short-run. However, as expected, these countries also liberalized a large share of medical imports, a tendency that became even more pronounced over time.

Meanwhile, countries with above-sample-median pre-crisis tariff levels on medical imports both liberalized and restricted a larger share of these imports and both these shares became even more pronounced over time. This is suggestive of demand-side factors necessitating import liberalization along with a domestic political economy that continues to push for import restrictions. Interestingly, this sample of countries both liberalized and restricted a smaller share of their medical exports; the latter is consistent with trade policy that looks at building domestic capacity for exports behind protectionist barriers.

Similarly, countries with above-sample-median pre-crisis tariff levels on food imports also both liberalized and restricted a larger share of these imports, though the latter pattern was reversed during the second half of the sample period. This sample of countries also both liberalized and restricted a larger share of their food exports; while the former pattern reversed completely during the second half of the sample period, the latter became even more pronounced over time. This is less suggestive of a trade policy that looks at building domestic capacity for exports behind protectionist barriers.

[5] GVC position is calculated as follows: ln(1+forward participation in GVCs)-ln(1+backward participation in GVCs). The higher the value, the more "upstream" is the country in GVCs.

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Countries with an ex-ante revealed comparative advantage in exporting food products also imposed a smaller share of export liberalization measures in the immediate run, but reversed this tendency subsequently. Again, counter-intuitively, countries ex-ante more reliant on food imports imposed a larger share of total restrictions on food imports but they also imposed a greater share of import liberalization measures (and this tendency became even more pronounced over time).

On the whole, we thus expect a more idiosyncratic association between food policy measures and trade flows; in contrast, the association between trade policy measures and trade of medical products is more likely to be along expected lines.

**4. Empirical methodology and data sources**

We assess the relationship between trade policy measures and imports and exports of food and medical products in levels and YoY change by estimating the following equations using fixed effects specifications:

$$M_{ckt} = \exp[A_1 \ln(Meas^{l/r}{}_{ckt}) + A_2 \ln(Covid_{ct}) + \delta_c + \delta_t] + \varepsilon_{ckt} \quad (1)$$

$$X_{ckt} = \exp[A_1 \ln(Meas^{l/r}{}_{ckt}) + A_2 \ln(Covid_{ct}) + \delta_c + \delta_t] + \varepsilon_{ckt} \quad (2)$$

where $M_{ckt}$ and $X_{ckt}$ are the levels of imports and exports of country c in product k (food or medical) in month t of 2020; $Meas_{ckt}$ is the count of liberalizing or restrictive measures imposed by country c on product k in month t; $Covid_{ct}$ includes the incidence of cases and deaths related to the pandemic; $\delta_c$ and $\delta_t$ are country and month fixed effects; and $\varepsilon_{ckt}$ is the error term.[6]

Equations (1) and (2) are estimated for the full sample separately using the Poisson Pseudo-Maximum Likelihood estimator (PPML; Silva and Tenreyro, 2006) due to over-dispersion in the dependent variable. Variants of equations (1) and (2) use YoY change as dependent variables (instead of levels) and these equations are estimated using OLS.

We also estimate all equations including interaction terms [$Var_{c/ck}.\ln(Meas^{l/r}{}_{ckt})$] to exploit the heterogeneity of our dataset and accommodate the various sub-samples discussed in Section 3.

$Var_c = \{POP_c, PCY_c, GE_c, GVC_c, GVC^{POS}{}_c, PTA^{NUM}{}_c, LIC_c, LMIC_c, UMIC_c, OECD_c, GPA_c, REG_c\}$. These variables denote, respectively, dummy variables that are unity when the pre-COVID-19 levels of population; per capita income; government effectiveness; GVC-participation; GVC-position; and the number of PTAs that each country is signatory to exceed

[6] Augmented versions of these equations also included (the logs of) additional control variables – the monthly index of industrial production ($IIP_{ct}$), the monthly consumer price index ($CPI_{ct}$) and the monthly nominal exchange rate ($NER_{ct}$) by country. However, the paucity of data on these variables rendered the effective sample infeasible for drawing any inference from empirical analysis. We thus report all results only on the basis of equations (1) and (2) where, under the present circumstances, the COVID-19 incidence variables are also reasonable proxies for population, economic activity and prices in the sample countries.

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the respective median values for the country sample; as well as belonging to the low-income, lower-middle-income and upper-middle-income groups according to the World Bank income classification; belonging to the group of OECD countries; being a Contracting Party to the WTO's GPA; and belonging to one of eight geographical regions[7].

$Var_{ck} = \{Msh_{ck}, RCA_{ck}, TAR_{ck}, NX_{ck}, NM_{ck}\}$. These variables denote, respectively, dummy variables that are unity when the pre-COVID-19 levels of import shares in total imports for food/medical products; RCA indices for food/medical exports; and applied tariff rates on food/medical imports exceed the respective median values for the country sample; as well as being net exporters and net importers of food and medical products, respectively.

### 4.1 Endogeneity

To account for endogeneity-related concerns in estimation and draw causal inference from analysis, we also experimented with GMM (both difference and system) specifications and used predicted values of the count trade policy measures in distinct 2SLS-IV regressions.[8] The difference GMM and 2SLS-IV regressions were diagnostically weak and we therefore focus on the system GMM estimates to draw causal inference while discussing the results in the following section.

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[7] These regions include Africa; North America; Latin America and the Caribbean; Europe including transition economies; CIS countries and West Asia; South Asia; ASEAN and East Asia; and Australia, New Zealand and the Pacific islands.

[8] The predicted values emanate from the following specifications, based on Hoekman et al. (2021):

$$Meas^{M,T}_{ckt} = \alpha + \emptyset_{\mathbf{p}} Proc_{\mathbf{pc}} + \Sigma\beta_{\mathbf{z}} z_{\mathbf{z}ck} + \varepsilon_{ckt} \quad (3)$$

$$Meas^{X,T}_{ckt} = \alpha + \emptyset_{\mathbf{p}} Proc_{\mathbf{pc}} + \Sigma\beta_{\mathbf{z}} z_{\mathbf{z}ck} + \varepsilon_{ckt} \quad (4)$$

where $Meas^{M,T}$ is the count of import ("M") policy measures imposed by type ("T" = liberalizing, restrictive); $Meas^{X,T}$ is the count of export ("X") policy measures imposed by type; $Proc_{\mathbf{pc}}$ is a vector of pre-COVID-19 public procurement variables by country; $z_{\mathbf{kj}}$ is a vector of pre-COVID-19 country- and country-sector specific control variables; $\alpha$ is the constant term and $\varepsilon_{ckt}$ is the error term.

The procurement vector comprises variables reflecting the timeliness ($Total\_time_c$), regulatory burden ($Total\_steps_c$), ease ($Eproc_c$) and openness of government procurement regimes. The first two variables denote the total time and total number of steps to complete the procurement process. Use of e-procurement is measured as the share of e-procurement in total procurement, based on the range categorization reported in the World Bank Doing Business Contracting with the Government indicator: less than 25%, 25%-50%, 50%-75% and 100%. Openness of procurement regimes is proxied by a binary variable indicating WTO GPA membership ($GPA_c$) and by the number of deep procurement agreements (DPAs) signed by each country with trading partners ($Num\_DPA_c$).

The control vector includes country size – the log of population ($POP_c$); a measure of geographic distance to global markets – the log of market penetration ($MP_c$), computed as a distance ($d_{c\check{c}}$) weighted measure of other countries' GDP ($GDP_{\check{c}}$), i.e. $MP_c = \Sigma_{\check{c}}(GDP_{\check{c}}/d_{c\check{c}})$; a measure of government effectiveness ($GE_c$) to reflect institutional strength; and participation ($GVC_c$) and position ($GVC^{pos}_c$) in GVCs as well as the number of PTAs ($Num\_PTA_c$) as proxies for openness and integration into the trading system. Both equations also include (i) the share of imports of food/medical goods in country c's total imports ($Msh_{ck}$); (ii) country c's standardized RCA index ($RCA_{ck}$) for food/medical goods; and (iii) the (log of) simple average applied tariff rate [$\ln(1+Tar_{ck})$] in country c on food/medical goods.

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### 4.2 Data sources

The control variables used to predict values of the count trade policy measures for 2SLS-IV analysis also form the basis on which the total sample is "split" above and below median to present stylized facts in the preceding section. These variables are constructed using data averaged over 2017-2018 in most cases, which are sourced as follows: population, GDP and per capita income data are from the World Bank World Development Indicators; market penetration is computed using bilateral distance data from CEPII (Head et al. 2010); government effectiveness is sourced from the Worldwide Governance Indicators (Kaufmann et al. 2011). Trade data used to construct the import share and RCA variables, as well as to calculate trade balance used to define a country's net exporter/net importer status, are taken from UN Comtrade. Import tariffs are sourced from UNCTAD TRAINS/WITS. Measures on country participation and position in GVCs are constructed using the EORA MRIO database and pertain to the year 2015. Data on coverage of government procurement provisions in PTAs emanate from Shingal and Ereshchenko (2020) which cover all PTAs in effect until March 2017[9] and the number of PTAs by country is constructed using the WTO's RTA-IS database as of December 2018. Monthly data on COVID-19 incidence are taken from the WHO (https://covid19.who.int/table).

The data are organized in a panel and the full sample for empirical analysis comprises 550 observations. Summary statistics are reported in Annex Table 2.

## 5. Results and discussion

The PPML levels and OLS YoY results are best interpreted as conditional correlations and these are reported in Table 2. For the full country sample, most results are either counter-intuitive or lack statistical significance. For instance, export liberalization of food products is found to be negatively correlated with food exports while export restrictions on food products are associated with an increase, both of which are unexpected outcomes. At the same time, import restrictions on medical products are associated with a rise in both imports and exports (while the former is again counter-intuitive, the latter suggests possible use of protectionism to facilitate domestic export capacity) while import restrictions on food products are associated with an export decline (providing suggestive evidence for Lerner's symmetry).

[9] Shingal and Ereshchenko (2020) measure the "depth" of procurement provisions in PTAs on the basis of seven broad attributes: non-discrimination; coverage in terms of goods, services (including construction) and type of procuring entity (central, sub-central government and utilities); procedural disciplines; ex-ante and ex-post transparency, dispute settlement; and new issues (e-procurement, sustainable procurement, SME participation, adoption of safety standards, and cooperation on matters of public procurement).

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**Table 2: Conditional correlations (full sample)**

| VARIABLES | (1) $med\_m_{ct}$ | (2) $med\_x_{ct}$ | (3) $med\_m\_YoY_{ct}$ | (4) $med\_x\_YoY_{ct}$ | (5) $food\_m_{ct}$ | (6) $food\_x_{ct}$ | (7) $food\_m\_YoY_{ct}$ | (8) $food\_x\_YoY_{ct}$ |
|---|---|---|---|---|---|---|---|---|
| $\ln(med\_m\_lib_{ct})$ | -0.894 | -1.022* | -4.199 | -56.100** | | | | |
| | (0.593) | (0.608) | (8.285) | (27.743) | | | | |
| $\ln(med\_m\_res_{ct})$ | 3.264** | 2.750** | -3.297 | -12.282 | | | | |
| | (1.360) | (1.213) | (6.873) | (14.228) | | | | |
| $\ln(med\_x\_lib_{ct})$ | 2.630* | 2.101 | 15.111 | 230.351** | | | | |
| | (1.402) | (1.351) | (16.576) | (96.318) | | | | |
| $\ln(med\_x\_res_{ct})$ | -0.960* | -0.503 | 4.726 | 8.464 | | | | |
| | (0.551) | (0.602) | (6.364) | (30.480) | | | | |
| $\ln(food\_m\_lib_{ct})$ | | | | | -0.110 | 0.064 | 15.692 | 24.488** |
| | | | | | (0.535) | (0.413) | (12.896) | (12.182) |
| $\ln(food\_m\_res_{ct})$ | | | | | -0.628 | -1.882** | 11.254 | 18.454 |
| | | | | | (0.926) | (0.802) | (18.951) | (17.162) |
| $\ln(food\_x\_res_{ct})$ | | | | | -0.110 | 1.053* | 10.240 | -15.019 |
| | | | | | (0.681) | (0.627) | (19.075) | (18.633) |
| $\ln(food\_x\_lib_{ct})$ | | | | | -5.459*** | -4.984*** | 10.355 | -32.660 |
| | | | | | (0.607) | (0.924) | (21.810) | (24.445) |
| $\ln(cases_{ct})$ | 0.227 | 0.411* | -5.356 | -17.105 | 0.136 | 0.257 | -3.151 | -9.609 |
| | (0.230) | (0.233) | (4.193) | (16.104) | (0.173) | (0.177) | (7.075) | (6.812) |
| $\ln(deaths_{ct})$ | -0.556** | -0.560** | 1.053 | 9.723 | -0.316* | -0.412** | 0.962 | 10.420 |
| | (0.248) | (0.218) | (3.141) | (14.261) | (0.168) | (0.170) | (9.996) | (6.810) |
| Observations | 192 | 192 | 175 | 175 | 187 | 185 | 166 | 164 |
| R-squared | 0.798 | 0.670 | 0.429 | 0.538 | 0.560 | 0.655 | 0.410 | 0.474 |

Note: Columns (1), (2), (5) and (6) report PPML estimates with the dependent variable defined in levels; columns (3), (4), (7) and (8) report OLS estimates with the dependent variable defined in YoY growth (%). All specifications include country and month fixed effects. Standard errors are clustered by country-month. Levels of significance: *10%, **5%, ***1%. Legend: m=import; x=export; lib=liberalizing; res=restrictive; YoY = year on year.

However, the full country sample masks significant heterogeneities (see Table 3). For instance, export policy measures on medical products imposed by non-OECD countries seem to have desired results - export liberalization is found to be positively correlated with exports of these products and export restrictions negatively associated; however, similar correlations are not observed in the case of food products.

Imposition of restrictions on food imports by countries more-integrated into GVCs is associated with a fall in their food imports. Meanwhile, import restrictions on medical products by countries less-integrated into GVCs is associated with a rise in exports (suggesting possible use of protectionism to facilitate domestic export capacity). At the same time, their imposition of export liberalization measures is positively correlated with exports, which is an expected outcome. Food import liberalization by countries less-integrated into GVCs is associated with a rise in imports but imposition of restrictions is associated with an export decline (providing suggestive evidence for Lerner's symmetry).

Liberalization of food imports by countries more upstream in GVCs is correlated with a decline in exports (suggesting that non-protection for domestic industry may be inhibiting export capacity). Import restrictions on medical products by countries more downstream in GVCs is correlated with an export rise (suggestive evidence for protectionism promoting export capacity) while food import liberalization by these countries is found to be correlated with a rise in both imports and exports.

Liberalization of medical imports by high-income countries (based on the World Bank income classification) is correlated with a decline in their medical exports while their import restrictions on food are associated with a decline in their food exports (providing suggestive evidence for Lerner's symmetry). Amongst other country groups, liberalization of medical imports by upper-middle-income countries is correlated with a rise in their medical exports while their restrictions on medical product exports are associated with a decline. Meanwhile, import liberalization of medical products by lower-middle-income countries is correlated with an import rise and liberalization of their food imports is associated with a rise in food exports, though the result is only weakly significant.

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**Table 3: Conditional correlations (exploiting sample heterogeneities)**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **med_m** | **med_x** | **med_m_YoY** | **med_x_YoY** | **food_m** | **food_x** | **food_m_YoY** | **food_x_YoY** |
| **GVC integration** | | | | | | | | |
| lnmed_m_lib | -1.947* | -2.061** | | | | | | |
| | (1.073) | (0.810) | | | | | | |
| lnmed_m_res | 3.430** | 3.816*** | | | | | | |
| | (1.641) | (1.312) | | | | | | |
| lnmed_x_lib | 5.558** | 6.913*** | | | | | | |
| | (2.504) | (1.990) | | | | | | |
| GVC*lnmed_x_lib | | -5.786** | | | | | | |
| | | (2.645) | | | | | | |
| lnfood_m_lib | | | | | 1.761*** | | | |
| | | | | | (0.594) | | | |
| lnfood_m_res | | | | | 1.920** | -2.524** | | |
| | | | | | (0.803) | (1.094) | | |
| lnfood_x_lib | | | | | -5.168*** | -4.963*** | | |
| | | | | | (0.776) | (0.982) | | |
| lnfood_x_res | | | | | -1.691* | 1.565** | | |
| | | | | | (0.885) | (0.769) | | |
| GVC*lnfood_m_lib | | | | | -3.109*** | | | |
| | | | | | (0.987) | | | |
| GVC*lnfood_m_res | | | | | -4.132*** | | | |
| | | | | | (1.288) | | | |
| GVC*lnfood_x_res | | | | | 1.918* | | | |
| | | | | | (1.071) | | | |
| Observations | 192 | 192 | | | 187 | 185 | | |
| R-squared | 0.806 | 0.687 | | | 0.579 | 0.653 | | |
| **GVC position** | | | | | | | | |
| lnmed_m_lib | | | | -111.840** | | | | |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | (55.796) | | | | |
| lnmed_m_res | | 4.086*** | | | | | | |
| | | (0.983) | | | | | | |
| lnmed_x_lib | | | | 255.616*** | | | | |
| | | | | (96.640) | | | | |
| lnfood_m_lib | | | | | 1.217** | 1.922*** | | 31.716** |
| | | | | | (0.573) | (0.719) | | (15.198) |
| lnfood_x_lib | | | | | -5.082*** | -4.606*** | | |
| | | | | | (0.707) | (0.998) | | |
| $GVC^{POS}$*lnfood_m_lib | | | | | -2.390*** | -3.031*** | | |
| | | | | | (0.914) | (0.775) | | |
| Observations | | 192 | | 175 | 187 | 185 | | 164 |
| R-squared | | 0.665 | | 0.545 | 0.581 | 0.663 | | 0.478 |
| **Level of development** | | | | | | | | |
| lnmed_m_lib | -1.652** | -1.490** | | | | | | |
| | (0.809) | (0.716) | | | | | | |
| lnmed_x_lib | 3.767** | | | | | | | |
| | (1.760) | | | | | | | |
| UMIC*lnmed_m_lib | | 2.499** | | | | | | |
| | | (1.082) | | | | | | |
| UMIC*lnmed_x_res | | -6.715** | | | | | | |
| | | (3.130) | | | | | | |
| UMIC*lnmed_x_lib | | | 124.624*** | 557.147*** | | | | |
| | | | (29.609) | (143.413) | | | | |
| lnfood_m_res | | | | | -1.814 | -3.275*** | | |
| | | | | | (1.479) | (1.240) | | |
| lnfood_x_lib | | | | | -4.800*** | -5.096*** | | |
| | | | | | (0.874) | (1.108) | | |
| lnfood_x_res | | | | | | 1.978*** | | |
| | | | | | | (0.695) | | |
| LMIC*lnfood_m_lib | | | | | | | | -76.026* |

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| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | | | (44.457) |
| LMIC*lnfood_m_res | | | | | 4.999*** | 2.935* | | |
| | | | | | (1.866) | (1.749) | | |
| UMIC*lnfood_m_res | | | | | | 3.127* | | |
| | | | | | | (1.623) | | |
| LMIC*lnfood_x_res | | | | | | -2.372* | | |
| | | | | | | (1.265) | | |
| Observations | 192 | 192 | 175 | 175 | 187 | 185 | | 164 |
| R-squared | 0.816 | 0.715 | 0.477 | 0.560 | 0.592 | 0.671 | | 0.494 |
| **OECD** | | | | | | | | |
| lnmed_m_res | 5.366*** | 5.253*** | | | | | | |
| | (2.027) | (1.857) | | | | | | |
| lnmed_x_lib | | 3.382* | | 301.287* | | | | |
| | | (1.990) | | (176.423) | | | | |
| lnmed_x_res | -2.734** | -3.444** | | | | | | |
| | (1.143) | (1.417) | | | | | | |
| OECD*lnmed_m_lib | -1.636* | -1.517* | | | | | | |
| | (0.944) | (0.909) | | | | | | |
| OECD*lnmed_m_res | -4.914** | | | | | | | |
| | (2.268) | | | | | | | |
| OECD*lnmed_x_lib | | | | | | | | |
| OECD*lnmed_x_res | | 3.941** | | | | | | |
| | | (1.656) | | | | | | |
| lnfood_x_lib | | | | | -5.628*** | -4.747*** | | |
| | | | | | (0.595) | (1.061) | | |
| Observations | 192 | 192 | | 175 | 187 | 185 | | |
| R-squared | 0.813 | 0.706 | | 0.540 | 0.563 | 0.656 | | |

Note: Columns (1), (2), (5) and (6) report PPML estimates with the dependent variable defined in levels; columns (3), (4), (7) and (8) report OLS estimates with the dependent variable defined in YoY growth (%). All specifications include country and month fixed effects. Standard errors are clustered by country-month. Table only reports estimates that were statistically significant. Levels of significance: *10%, **5%, ***1%.

### 5.1 System GMM results for medical products

The baseline System GMM results for medical products suggest that a 1% increase in the number of measures liberalizing medical exports led to a 3% decline in their exports. Though the effect is only weakly significant, it is counter-intuitive. System GMM results exploring sample heterogeneities for food and medical products are reported in Table 4.

For countries less reliant on medical imports, a 1% rise in medical export liberalizing measures led to a 7.2% decline in medical imports and a 12.5% decline in medical exports, both of which allude to idiosyncratic responses. Significantly and as expected, both effects ran in the opposite direction via the interaction term for the sample of more import-reliant countries - a 1% increase in export liberalizing measures raised both imports and exports of medical goods by 6.3% and 10.8%, respectively. Moreover, a 1% rise in the count of medical export restrictions led to a 2% increase in medical imports for less-import-reliant countries, suggesting that this set of countries may have been characterized by excess demand/insufficient capacity, which was addressed by imposing export restrictions that elicited the expected response.

Trade liberalizing measures imposed by countries less-integrated in global trade seem to have been effective during this pandemic. For example, for below-sample-median PTA signatories, a 1% rise in the number of import liberalizing measures led to a 1.6% increase in medical imports. Likewise, a 1% rise in the number of import liberalizing measures by below sample-median GVC-integrated countries led to a 26-percentage point increase in the growth of medical imports relative to last year (the effect is found to be significant at 5%). In contrast, the response of imports to similar measures by countries more integrated in GVCs as well as above sample median PTA signatories was found to be idiosyncratic; a 1% rise in the number of import liberalizing measures led to a -26.8 percentage point decline in their YoY change and a 1.7% decline in their levels, respectively, though both results were found to be weakly significant.

For non-OECD countries, a 1% rise in the number of import liberalizing measures led to a 31.0 percentage point increase in the growth of medical imports relative to last year (the effect is found to be significant at 5%). This sample of countries also includes below-sample-medium per capita income countries and as expected, the results were similar for the latter sample. Illustratively, a 1% rise in the number of import liberalizing measures led to a 24.8 percentage point increase in the growth of medical imports relative to last year, though the effect was found to be weakly significant. Medical imports also increased by 1.9% in response to a 1% rise in the number of export restrictions for the same sub-sample of countries. Countries in this sub-sample are also likely to display low government effectiveness. Thus, for countries with below-sample-median levels of government effectiveness, a 1% increase in the number of measures liberalizing medical exports led to a 16% decline in exports of medical products. Though the effect is only weakly significant, it is counterintuitive.

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**Table 4: System GMM results (exploiting sample heterogeneities)**

| | (1) med_m | (2) med_x | (3) med_m_YoY | (4) med_x_YoY | (5) food_m | (6) food_x | (7) food_m_YoY | (8) food_x_YoY |
|---|---|---|---|---|---|---|---|---|
| **Government effectiveness** | | | | | | | | |
| lnmed_x_lib | | -16.316* | | | | | | |
| | | (9.149) | | | | | | |
| lnfood_m_res | | | | | 2.181** | | | |
| | | | | | (0.987) | | | |
| GE*lnfood_m_res | | | | | -3.011** | | | |
| | | | | | (1.1339) | | | |
| GE*lnfood_x_res | | | | | -4.551* | | | |
| | | | | | (2.659) | | | |
| Observations | | 179 | | 176 | 172 | | | |
| R2 | | 0.008 | | 0.027 | 0.044 | | | |
| Sargan | | 34.126 | | 44.762 | 32.803 | | | |
| Prob>Chi2 | | 1.000 | | 0.524 | 0.928 | | | |
| **GVC integration** | | | | | | | | |
| lnmed_m_lib | | | 26.155** | | | | | |
| | | | (12.871) | | | | | |
| H_gvc_lnmed_m_lib | | | -26.834* | | | | | |
| | | | (14.188) | | | | | |
| lnfood_m_res | | | | | 1.826** | | | |
| | | | | | (0.916) | | | |
| lnfood_x_res | | | | | | | 80.505* | |
| | | | | | | | (45.749) | |
| GVC*lnfood_m_res | | | | | -3.079** | | | |
| | | | | | (1.226) | | | |
| GVC*lnfood_x_res | | | | | | | -84.462* | |
| | | | | | | | (51.215) | |
| Observations | | | 160 | | 176 | | 160 | |

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| | | | | | | |
|---|---|---|---|---|---|---|
| R2 | | | 0.019 | 0.019 | | 0.002 |
| Sargan | | | 39.652 | 47.256 | | 16.019 |
| Prob>Chi2 | | | 0.768 | 0.544 | | 0.996 |
| **GVC position** | | | | | | |
| lnfood_m_lib | | | | | 3.472** | |
| | | | | | (1.452) | |
| $GVC^{POS}$*lnfood_m_lib | | | | -3.855*** | -5.059** | |
| | | | | (1.449) | (2.084) | |
| $GVC^{POS}$*lnfood_x_res | | | | 6.451** | 6.293* | |
| | | | | (2.523) | (2.247) | |
| Observations | | | | 176 | 172 | |
| R2 | | | | 0.026 | 0.035 | |
| Sargan | | | | 42.429 | 31.088 | |
| Prob>Chi2 | | | | 0.700 | 0.972 | |
| **Import share** | | | | | | |
| lnmed_x_lib | -7.193** | - 12.522*** | | | | |
| | (3.352) | (4.549) | | | | |
| Msh*lnmed_x_lib | 6.298* | 10.813** | | | | |
| | (3.549) | (4.859) | | | | |
| Observations | 179 | 179 | | | | |
| R2 | 0.028 | 0.040 | | | | |
| Sargan | 62.149 | 68.699 | | | | |
| Prob>Chi2 | 0.577 | 0.353 | | | | |
| **Per capita income** | | | | | | |
| lnmed_m_lib | | | 24.761* | | | |
| | | | (13.588) | | | |
| lnmed_x_res | 1.866* | | | | | |
| | (1.111) | | | | | |

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| | | | | | | |
|---|---|---|---|---|---|---|
| lnfood_m_lib | | | | -1.947** | | |
| | | | | (0.925) | | |
| lnfood_m_res | | | | 1.859** | | |
| | | | | (0.908) | | |
| lnfood_x_res | | | | | | 101.836* |
| | | | | | | (52.099) |
| PCY*lnfood_m_lib | | | | 2.850** | 3.126* | |
| | | | | (1.268) | (1.857) | |
| PCY*lnfood_x_res | | | | | | -103.063** |
| | | | | | | (50.350) |
| Observations | 179 | 160 | | 176 | 172 | 160 |
| R2 | 0.013 | 0.012 | | 0.027 | 0.041 | 0.003 |
| Sargan | 67.321 | 31.348 | | 47.802 | 33.181 | 10.888 |
| Prob>Chi2 | 0.432 | 0.970 | | 0.360 | 0.904 | 1.000 |
| **Population** | | | | | | |
| lnmed_x_lib | | | 247.677* | | | |
| | | | (147.065) | | | |
| lnfood_m_res | | | | -2.100* | | |
| | | | | (1.147) | | |
| POP*lnfood_m_res | | | | 3.501** | | |
| | | | | (1.482) | | |
| Observations | | | 160 | 176 | | |
| R2 | | | 0.080 | 0.027 | | |
| Sargan | | | 4.096 | 48.251 | | |
| Prob>Chi2 | | | 1.000 | 0.463 | | |
| **Net importer** | | | | | | |
| lnfood_m_lib | | | | -1.945* | | |
| | | | | (1.055) | | |
| $NM^F$*lnfood_m_lib | | | | 3.017** | 4.286** | |
| | | | | (1.292) | (1.830) | |

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| | | | | |
|---|---|---|---|---|
| Observations | | | 176 | 172 |
| R2 | | | 0.036 | 0.063 |
| Sargan | | | 50.414 | 30.525 |
| Prob>Chi2 | | | 0.457 | 0.987 |
| **Net exporter** | | | | |
| $NX^M$*lnmed_x_lib | | 454.984** | | |
| | | (221.832) | | |
| lnfood_m_lib | | | | 3.232*** |
| | | | | (1.179) |
| $NX^F$*lnfood_m_lib | | | -3.017** | -4.286** |
| | | | (1.292) | (1.830) |
| Observations | | 160 | 176 | 172 |
| R2 | | 0.115 | 0.036 | 0.063 |
| Sargan | | 6.969 | 50.414 | 30.525 |
| Prob>Chi2 | | 1.000 | 0.457 | 0.987 |
| **OECD** | | | | |
| lnmed_m_lib | 31.141** | | | |
| | (12.983) | | | |
| lnfood_m_lib | | | -2.273** | |
| | | | (1.072) | |
| lnfood_x_res | | | | 3.497* |
| | | | | (1.974) |
| OECD*lnfood_m_lib | | | 3.110** | |
| | | | (1.406) | |
| OECD*lnfood_x_res | | | | -4.151** |
| | | | | (2.067) |
| Observations | 160 | | 176 | 172 |
| R2 | 0.007 | | 0.019 | 0.042 |
| Sargan | 33.604 | | 44.104 | 33.049 |
| Prob>Chi2 | 0.964 | | 0.467 | 0.887 |

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| | | | | |
|---|---|---|---|---|
| **Geographical region** | | | | |
| lnmed_x_res | 12.991* | | | |
| | (7.772) | | | |
| REG2_lnmed_x_res | -13.317* | | | |
| | (7.798) | | | |
| REG3_lnmed_x_res | -12.821* | | | |
| | (7.574) | | | |
| REG5_lnmed_x_res | -14.263* | | | |
| | (8.127) | | | |
| REG4_lnfood_m_lib | | | 7.284* | |
| | | | (4.217) | |
| REG7_lnfood_m_lib | | | 8.452* | |
| | | | (4.995) | |
| Observations | 179 | | 172 | |
| R2 | 0.016 | | 0.064 | |
| Sargan | 70.326 | | 25.876 | |
| Prob>Chi2 | 0.987 | | 1.000 | |
| **Level of development** | | | | |
| lnfood_m_lib | | | | -45.842* |
| | | | | (26.446) |
| LMIC*lnfood_m_lib | | | | 60.046* |
| | | | | (35.953) |
| LMIC*lnfood_m_res | | 2.871* | | |
| | | (1.494) | | |
| LMIC*lnfood_x_res | | | | 106.890* |
| | | | | (58.674) |
| Observations | | 176 | | 160 |
| R2 | | 0.042 | | 0.002 |
| Sargan | | 65.768 | | 35.879 |
| Prob>Chi2 | | 0.684 | | 0.973 |

| | | |
|---|---|---|
| **Tariff level** | | |
| lnfood_m_lib | | 49.480** |
| | | (22.654) |
| lnfood_m_res | -2.125* | |
| | (1.268) | |
| $TAR^F$*lnfood_m_lib | | -51.828* |
| | | (31.213) |
| Observations | 172 | 156 |
| Sargan | 35.238 | 13.807 |
| Prob>Chi2 | 0.955 | 1.000 |
| R2 | 0.033 | 0.023 |

Note: Columns (1), (2), (5) and (6) report PPML estimates with the dependent variable defined in levels; columns (3), (4), (7) and (8) report OLS estimates with the dependent variable defined in YoY growth (%). All specifications include month fixed effects. Standard errors are clustered by country-month. Table only reports estimates that were statistically significant. The reported Sargan statistic tests the null of overidentifying restrictions being valid. Levels of significance: *10%, **5%, ***1%.

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Another expected result is that less-populous countries are likely to face lower domestic demand for medical goods during a health shock and such countries would be in a favorable position to export these goods to the rest of the world in case of excess supply at home. This expectation is corroborated by our results for the sub-sample of less populous countries; a 1% rise in the number of export liberalizing measures by less populous countries led to a 248 percentage point increase in the growth of medical exports relative to last year, though the effect is only weakly significant.

Finally, according to Leibovici and Santacreu (2020), net exporters of medical goods may be tempted to increase trade barriers during a global pandemic. However, our results contradict this finding; a 1% rise in the number of export liberalizing measures led to a 455-percentage point increase in the growth of medical exports relative to last year for this sub-sample of countries (the effect is found to be significant at 5%). This supports the theory that net exporters of essential items prefer to live in a world with low-barriers to trade and this tendency may not have been reversed during the current pandemic.

### 5.2 System GMM results for food products

Baseline GMM estimates of the impact of food policy measures lack statistical significance. For countries more downstream in GVCs (see Table 4), a 1% rise in the number of food import liberalizing measures led to a 3.5% rise in food exports, which corroborates the results from Santos-Paulino and Thirlwall (2004) that import liberalization is likely to boost exports especially for developing countries that may also be positioned lower in GVCs. An opposite effect was observed for countries more upstream in GVCs perhaps due to greater domestic demand. The latter also witnessed a decline in food imports from an increase in import liberalizing measures and a rise in food exports from an increase in export restrictive measures, both of which are counter-intuitive outcomes.

Piermartini (2004) and Bouët and Debucquet (2010) provide justification and evidence from previous food crisis situations for imposition of export taxes or duties on food products as these products were deemed to be ‘essential’ at home. A similar justification can be extended for a rise in export restrictions during the current pandemic that has sparked an overall economic and food crisis. The importance of government effectiveness in getting expected response to trade policy changes is also borne out by these results – a 1% rise in the number of export-restrictive measures by countries with high government-effectiveness led to a 4.5% decline in the export of food products, while a 1% rise in import-restrictive measures led to a 3% decline in food imports. At the same time, a 1% increase in the number of measures restricting food imports by countries with low government effectiveness led to a 2.2% rise in imports of food products. Though the effect is only weakly significant, it is counter-intuitive.

A 1% rise in the number of import liberalizing measures led to a counter-intuitive 2.3% decline in food imports for non-OECD countries and an expected 0.8% rise for OECD countries. The result for OECD countries is along expected lines, perhaps due to higher government effectiveness and its ability to procure essential supplies in response to a trade policy measure

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(Han et al. 2014; Hoekman et al. 2020). Moreover, a 1% rise in the number of export restrictive measures led to a counter-intuitive 3.5% increase in food exports for non-OECD countries and an expected 0.65% decline for OECD countries.

A 1% rise in the number of import liberalizing and restrictive measures by below-sample-median per capita income countries led to a 2.0% decline and a 1.9% increase in food imports respectively, both of which are counter-intuitive outcomes. In contrast, a 1% increase in the number of import liberalizing measures led to a 2.8% increase in food imports for above-sample-median-per capita income countries that may also be traced back to better government procurement practices.

For below sample median GVC-integrated countries, a 1% rise in the number of import restrictive measures led to a counter-intuitive 1.8% rise in food imports, while a 1% rise in the number of export restrictive measures led to an 80.5 percentage point increase in the growth of food imports relative to last year perhaps due to higher domestic demand. On the contrary, in countries highly-integrated in GVCs, a 1% rise in the number of import restrictive measures reduced food imports by 3%. This result is similar to that displayed by the sample of countries with above-sample-median per capita income as high per capita income is linked to high GVC participation (Kowalski, et al., 2015).

A 1% rise in import liberalizing measures was associated with a 60 percentage point increase in the growth of food imports for lower-middle-income countries but a 45.8 percentage point decline for high-income countries, which is counter-intuitive. Meanwhile, a 1% rise in export restrictions also led to a 107 percentage point increase in the growth of food imports for lower-middle-income countries, suggesting that measures ensuring domestic availability led to a complementary response on the import side. All effects were weakly estimated at the 10% level.

For net importers of food products, a 1% rise in the number of import liberalizing measures led to a 3% rise in food imports, but a (weakly significant) 1.9% decline for net exporters, which is a perverse result. Net exporters of food products also tend be less reliant on food imports. A similar 1% increase in import liberalization measures led to a 2.9% and 39.6 percentage point increase in food exports for countries less reliant on food imports or countries that were likely to be net exporters.

Net exporters of food are also likely to feature in a sample with above-median revealed comparative advantage in such exports. However, the response of this sample to a rise in import liberalizing measures was not statistically significant. Yet, a 1% rise in import restrictions was associated with a 2.3% counter-intuitive increase in food imports for countries more competitive in exporting food products. When examined in an aggregate manner, countries with low competitiveness in exporting food products did not display a significantly different response to additional import restrictions. However, a 1% increase in import liberalizing measures was associated with a 35 percentage point rise in the growth of food exports for less-competitive food exporting countries that are likely to be net importers of food, while more

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competitive countries witnessed a 53.4 percentage point decline, suggesting that a reverse-Learner Symmetry effect was not observed for the latter. All effects were weakly significant.

A 1% rise in the number of import restrictive measures by less populous countries led to a 2.1% decline in food imports, though this effect is found to be counter-intuitive for more populous countries.

Finally, 1% increase in import liberalizing measures was associated with a (weakly significant) 49.5 percentage point rise in the growth of food exports for below-sample-median tariff countries, though higher-tariff countries witnessed a 51.8 percentage point decline, suggesting that a reverse-Learner Symmetry effect was not observed for the latter.

## 6. Conclusion

In an original contribution to the COVID-19 and trade literatures, we examine the trade policy passthrough to trade flows of restrictive and liberalizing measures imposed on exports and imports of food and medical products (Evenett et al. 2021) during the first nine months of 2020 for a sample of 142 countries. We find that where the imposition of trade policy measures is more consistent with theoretical/conceptual predictions, such measures are also found to be associated with trade flow changes that are less idiosyncratic. This is found to be true in general for trade policy measures imposed on exports and imports of medical products; for food products, stylized facts suggest that the trade policy activism may have been more idiosyncratic. In some cases, however, accounting for sample heterogeneities renders the results less idiosyncratic. On the whole, our results suggest that richer, more globally-integrated economies with high levels of government effectiveness may have also displayed higher trade policy effectiveness during the first nine months of the pandemic. The absence of idiosyncratic behavior, especially during a crisis period, suggests that these countries may have had the right incentives and mechanisms in place even during pre-pandemic times.

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**Annex Table 1: Country sample**

*Net exporters of medical products:* Bangladesh, Cambodia, China, Costa Rica, Denmark, Dominican Republic, France, Germany, India, Ireland, Israel, Italy, Luxembourg, Macedonia, Malaysia, Montserrat, Netherlands, Singapore, Slovenia, South Korea, Sweden, Switzerland.

*Net importers of medical products:* Albania, Algeria, Angola, Antigua and Barbuda, Argentina, Armenia, Australia, Azerbaijan, Bahamas, Bahrain, Barbados, Belarus, Belgium, Belize, Bermuda, Bhutan, Bolivia, Botswana, Brazil, Brunei Darussalam, Bulgaria, Burkina Faso, Cameroon, Canada, Chad, Chile, Colombia, Cote d'Ivoire, Croatia, Cyprus, Czech Republic, Democratic Republic of Congo, Ecuador, Egypt, El Salvador, Estonia, Fiji, Finland, Gambia, Georgia, Greece, Guatemala, Guyana, Honduras, Hong Kong, Hungary, Iceland, Indonesia, Iran, Iraq, Japan, Jordan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Libya, Lithuania, Macao, Madagascar, Malawi, Maldives, Mali, Mauritania, Mauritius, Mexico, Moldova, Montenegro, Morocco, Mozambique, Myanmar, Namibia, Nepal, New Caledonia, New Zealand, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russian Federation, Rwanda, Samoa, Saudi Arabia, Senegal, Serbia, Slovak Republic, South Africa, Spain, Sri Lanka, St. Kitts and Nevis, St. Vincent and the Grenadines, Sudan, Suriname, Syria, Tajikistan, Thailand, Togo, Turkey, Ukraine, United Kingdom, United States, Uruguay, Uzbekistan, Venezuela, Vietnam, Zambia, Zimbabwe.

*Net exporters of food products:* Argentina, Australia, Belarus, Belize, Bolivia, Brazil, Bulgaria, Cambodia, Cameroon, Canada, Chad, Chile, Colombia, Costa Rica, Cote d'Ivoire, Denmark, Ecuador, Estonia, France, Guatemala, Guyana, Honduras, Hungary, Iceland, Indonesia, Ireland, Kazakhstan, Laos, Latvia, Lithuania, Malawi, Malaysia, Moldova, Myanmar, Namibia, New Zealand, Niger, Norway, Panama, Paraguay, Peru, Poland, Romania, Russian Federation, Serbia, South Africa, Spain, Suriname, Thailand, Ukraine, United States, Uruguay, Vietnam, Zambia.

*Net importers of food products:* Albania, Algeria, Angola, Antigua and Barbuda, Armenia, Azerbaijan, Bahamas, Bahrain, Bangladesh, Barbados, Belgium, Bermuda, Bhutan, Botswana, Brunei Darussalam, Burkina Faso, China, Croatia, Cyprus, Czech Republic, Democratic Republic of Congo, Dominican Republic, Egypt, El Salvador, Fiji, Finland, Gambia, Georgia, Germany, Greece, Hong Kong, India, Iran, Iraq, Israel, Italy, Japan, Jordan, Kenya, Kuwait, Kyrgyzstan, Lebanon, Libya, Luxembourg, Macao, Macedonia, Madagascar, Maldives, Mali, Mauritania, Mauritius, Mexico, Montenegro, Montserrat, Morocco, Mozambique, Nepal, Netherlands, New Caledonia, Nigeria, Oman, Pakistan, Philippines, Portugal, Qatar, Rwanda, Samoa, Saudi Arabia, Senegal, Singapore, Slovak Republic, Slovenia, South Korea, Sri Lanka, St. Kitts and Nevis, St. Vincent and the Grenadines, Sudan, Sweden, Switzerland, Syria, Tajikistan, Togo, Turkey, United Kingdom, Uzbekistan, Venezuela, Zimbabwe.

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