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Effectiveness of Government Support for the Private Sector during the COVID-19 Crisis

Evidence from El Salvador and Georgia

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Abstract

This paper estimates the effectiveness of government support to the private sector during the COVID-19 pandemic in El Salvador and Georgia using firm-level data collected before and during the pandemic. The two countries are selected because eligibility criteria for support involved pre-pandemic features of firms, as opposed to more prevalent criteria directly linked to firms' experiences during the pandemic and that greatly exacerbate concerns about selection bias in estimation. Four outcome variables are studied relating to firms' workforce, hours of operations, and expectations. Matching and panel estimation techniques are used on full and restricted samples, with the latter aimed at reducing selection bias. Government support appears to have helped firms avoid a reduction in operations in El Salvador, mainly through cash transfers, which also helped in terms of permanent workers, with the latter effect counteracted by wage subsidies. Smaller firms in Georgia appear to have benefited more from government support, mostly through fiscal relief, which was partially counteracted by wage subsidies that benefited larger firms more. The finding that smaller firms have benefited more helps raise confidence in the analysis as strong negative selection bias is expected in this context. Manufacturers of textiles and garments in El Salvador and hotels and restaurants in Georgia appear to have benefited from government support, but the patterns in other sectors are mixed and country-specific, highlighting potential complexities of attempting to target sectors.

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1. Introduction

COVID-19 has spread relentlessly across the world, considerably affecting nearly all aspects of livelihood nearly everywhere. The health crisis quickly led to an economic crisis, forcing governments to issue support measures for households and the private sector to help cope. This paper complements the growing research into whether these support measures were useful. In this paper, we study whether the support measures provided during the COVID-19 pandemic by the governments of two rather distinct countries, El Salvador and Georgia, were effective in terms of some measures of the private sector performance. We use firm-level data from the World Bank's Enterprise Surveys (ES) that were completed pre-pandemic, and two rounds of COVID Follow-up Surveys (COV-ES) which recontacted all the respondents of the ES to collect data about firms' sales, employment, finances, and government support.[2] The ES are representative samples of each country's private sector enabling analysis of patterns affecting a large part of each country's economy.

We use data from only two countries, El Salvador and Georgia, to help alleviate concerns about selection bias, which are central to the estimation of effectiveness. Unlike in many others, the support measures in these two countries were not conditional on a specific set of firm outcomes during the pandemic. Given that fiscal capacity is not unlimited, governments across the world tried to target the neediest and often created eligibility criteria for support measures. These criteria often included specific parameters from the firms' experience during the pandemic, such as a drop in revenue, or workforce, etc. While this may be a good way to target the neediest, it greatly complicates estimation of the effectiveness of the support as it becomes nearly impossible to form appropriate groups of firms to compare their performance. The Governments of El Salvador and Georgia also tried to target the neediest, but they used pre-pandemic fixed characteristics such as firm size and sector. In selecting the set of countries, we were also mindful of the number of firms that received government support in the COV-ES data. A lower bound on the number of observations and the nature of eligibility criteria for government support narrowed down the set of countries we could study to just El Salvador and Georgia.

[2] Both ES and COV-ES data sets are publicly available on data portal: https://login.enterprisesurveys.org/. As of June 2021, COV-ES has covered 44 countries, some of which have three rounds of surveys available. For further information on COV-ES, please visit https://www.enterprisesurveys.org/en/covid-19.

We focus on the effect of government support on the following main aspects of firms' operations: workforce, hours of operations, and expectations. In particular, we use the following four outcome variables available in COV-ES: (i) percentage change of permanent full-time workers since December 2019, and three binary outcome variables, (ii) whether the firm decreased its total number of temporary workers since the onset of the pandemic, (iii) whether the firm decreased its total hours of operation per week compared with the same month one year ago, and (iv) whether the respondent anticipates falling in arrears on outstanding liabilities within the next six months of the date of the COV-ES interview.

To alleviate further concerns about selection bias and endogeneity, we use two methods, matching, and panel estimation. For matching estimation, we use a rich set of information about each firm from the baseline ES to match firms that received government support in COV-ES with the ones that did not. In this process, we take into account details of support measures of each country. For example, when matching firms in the Georgia data, we forced the matching algorithm to perform exact matching within the sectors of hotels and restaurants because the Government of Georgia provided a specific set of support to firms in these sectors. For panel estimation, we use both rounds of COV-ES in fixed effects regression analysis which addresses selection bias stemming from the time-invariant characteristics of firms. In these specifications, we also include a long list of controls from COV-ES to account for time-varying aspects. Panel regressions also enable us to use COV-ES questions that break down the government support that firms received into separate categories, such as cash transfers, fiscal relief, and wage subsidies (with many firms receiving combinations of support types).

We re-estimate models using a restricted sample as an additional check on selection bias. In particular, we try to exclude from the analysis the firms that were not eligible to receive the support and are likely not appropriate comparators to those that did. In forming this restricted sample, we also exclude firms that report no need for such support or lack of awareness about its existence. Even after these steps, we consider that concerns about selection bias probably remain to some extent. We thus avoid commenting on the estimated size of the effectiveness of government support and instead focus on whether it is above zero, validating this focus through discussion about the expected directionality of these biases across our estimated models.

Our matching estimation does not suggest any statistically significant effect of government support on any of our four outcome variables in either country. Panel estimation mostly confirms this with just one exception: government support in El Salvador seems to have enabled firms to avoid a reduction in their total hours of operation per week. This positive relation seems to be fully attributable to one particular type of government support – cash transfers. Such breakdown by types of support, done through panel regressions, suggests that, in El Salvador, cash transfers had a positive effect on firms' permanent full-time workforce as well, but this is fully counteracted by the negative effect of wage subsidies.

We find that smaller firms benefited more across some measures and types of government support. In particular, in Georgia, small firms (size measured in the baseline ES) that received government support went through more favorable changes in the permanent full-time workforce than those that did not receive support. However, this positive effect fizzles for firms employing roughly 25 or more workers. This relation seems to be attributable to fiscal relief, with partial counteraction from wage subsidies (which appear to have benefited larger firms more). We did not find discernable and consistent patterns across size in the data from El Salvador, which could be due to the baseline ES being implemented much earlier there, in 2016 (as opposed to 2019 in Georgia). Nevertheless, the finding that smaller firms may have benefited more from government support helps validate our overall analysis. This is not only because the pattern is intuitive but because in the context of potentially strong negative selection bias, this pattern is hard to find since smaller firms are also likely to be needier.

Sectoral breakdown of the effectiveness of support policies obtained through panel regressions confirms some of our expectations but also offers mixed findings. In particular, the manufacturers of textiles and garments in El Salvador seem to have benefited from government support in terms of permanent and temporary workers; hotels and restaurants in Georgia seem to have benefited from government support in terms of permanent workers. However, patterns within the retail sector, which we broke down into food retailers or pharmacies and others, are inconsistent across the two countries. Similarly, we find a beneficial effect of government support on food manufacturers in El Salvador in terms of permanent workers, but counter-productive though not statistically significant effect in Georgia. This discrepancy could be due to the composition of food manufacturers within each country, with supply chains leading towards hotels or restaurants as

opposed to grocery stores (this information is not available in the ES or COV-ES). We think these mixed results across sectors and countries point to the importance of nuances beyond the firms' aggregate sector in understanding the effectiveness of government support, at the same time highlighting potential complexities of attempting to target sectors.

While most breakdowns of the effectiveness of government support to the private sector by type, by size, and by sector are intuitive, it is still possible that we are under-estimating the full effect. We focus on just four outcome variables, and the effect may have worked through different outcomes. For example, we do not examine attrition in the context of panel regressions, i.e., firms leaving our sample due to non-response or firms' exit from business (though the rates of the latter in both countries are slim). We also do not consider the size of the government support that firms received as this information is not available in the data. Nevertheless, we consider our findings worthy of consideration in assessing policies aimed at supporting the private sector in a massive crisis.

This paper contributes to two rapidly growing and related strands of literature. First, in trying to measure the effectiveness of government support in the context of the COVID-19 pandemic, this paper complements the existing work such as Chetty et al. (2020) and Bennedsen et al. (2020) that study the effects of government support on the firm performance in the US and in Denmark respectively. Among this type of analysis, perhaps the closest paper to ours are Cirera et al. (2021) and Janzen and Radulescu (2021). The former paper documents the cross-country relationship between government support and firm performance, whereas the latter focuses on the effectiveness of government support in European countries. Unlike these two papers, we narrow our analysis to just two countries but try to get closer to estimating the causal link between government support and firm performance. Second, in analyzing firm performance during the pandemic, this paper complements the existing work such as Aga and Maemir (2021) that provides a comparative analysis of the effects of COVID-19 on the private sector in Sub-Saharan African countries and elsewhere; Muzi et al. (2021) that investigates potential determinants of permanent closure of firms during the pandemic; and Amin and Viganola (2021) that explore the role of the pre-pandemic access to finance for firms' performance during the pandemic. This paper focuses on employment, operations, and expectations about finances as they relate to the government support measures.

The rest of the paper is organized as follows. In Section 2, we discuss details behind our focus on just two countries, El Salvador and Georgia, and provide information about government support measures in these countries. Section 3 provides a general overview of the methods that we use for analysis. Section 4 describes data and main variables. Sections 5 and 6 report results from matching and panel estimation, respectively. Section 7 briefly concludes.

2. Selection of Countries and Government Support Measures

In this section, we explain the reasoning behind focusing on El Salvador and Georgia to study the effect of government support during the COVID-19 crisis. We also provide a brief summary of support measures taken by the governments in these two countries.

As of June 2021, the World Bank Enterprise Surveys (WBES) team has made the COV-ES data available across 44 countries.[3] Several research papers have already analyzed this rich data in a cross-country context to study the effects of the pandemic (e.g., Muzi et al. 2021), including exploration of the effectiveness of government policies (e.g., Cirera et al. 2021). We started with the understanding that details of government policy, which varied tremendously across countries, would make cross-country analysis extremely complicated. This is not least because the types or sizes of government support varied across countries (as some of these variations can be accounted for through country fixed effects). More importantly, the targeting of policies varied drastically across countries and often used the severity of the firms' experience with the pandemic as part of eligibility criteria. This has direct implications on the identification of the causal link between government policy and firms' performance. In particular, the specificity of eligibility criteria to time-varying features of firms, coupled with the absence of that information in our database at the point of firms' application for the policy, makes it nearly impossible to form a proper control group to consistently estimate the effectiveness of government support. Consequently, our first criterion for selecting the country for analysis is to ensure that government support was not directly conditional on firms' experience of the pandemic and instead was general, perhaps with some targeting across firm sizes or sectors (typically defined at some pre-pandemic point in time as governments tried to avoid fraud). Our second condition is about the number of

[3] Please see https://www.enterprisesurveys.org/en/covid-19 for more details on coverage and ongoing data collection projects.

firms in the COV-ES database that received at least some government support. While there is no general rule about the minimum number of observations necessary, we restrict our focus to the countries with at least around 150 firms that have received government support by the second round of COV-ES.

To apply our first criterion, we used the Map of SME-Support Measures in Response to COVID-19 created and made publicly available by the World Bank, retrieved in early March of 2021, and covering information as of late October 2020.[4] Our first criterion ruled out the overwhelming majority of countries covered in the Map. Additional countries were ruled out after supplementing information given in the map through country-specific searches of information about policies that we conducted. The vast majority of governments tried to increase the reach of their fiscal capacity by concentrating resources on the firms that were hit the most, often using revenue as a measure of this hit and using drops in employment and other characteristics. Our second criterion, a minimum of 150 firms receiving support by Round-2 of COV-ES, also eliminated several countries, mostly in Africa, where government policy coverage by October/November was limited. To the best of our knowledge, these two conditions combined ruled out all the countries covered in COV-ES except for El Salvador and Georgia.

Governments in El Salvador and Georgia announced a set of support measures to businesses as well as to individuals and families to help cope with the pandemic and with the strict lockdowns that were introduced to limit the spread of the virus. Support measures for private businesses in El Salvador mainly targeted small, micro, and medium-sized companies (MSMEs) as well as informal businesses, and included access to new credit; wage subsidies; fiscal incentives for investment in new productive projects; deferrals of utility payments; and other supports for simplification of procedures for MSMEs. The Georgian government also implemented support policies, with some targeting of the tourism sector (loosely defined as hotels and restaurants) and small firms. The support measures in Georgia included fiscal relief; subsidies, guarantees, or other forms of assistance for loans; deferral of payments; and micro-grant programs for new business projects. Appendixes A.1. and A.2. provide details of government support policies during the

[4] The Map is available here: https://dataviz.worldbank.org/views/SME-COVID19/Overview?%3Aembed=y&%3AisGuestRedirectFromVizportal=y&%3Adisplay_count=n&%3AshowAppBanner=false&%3Aorigin=viz_share_link&%3AshowVizHome=n. See also the IMF policy tracker, https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19.

COVID-19 crisis for El Salvador and Georgia, respectively. Details about the information contained in COV-ES data about government support are given in Section 4.

3. Methods

To estimate the effect of the government support, consider the following specification:

$$Y_{o,i} = \theta S_i + \beta \boldsymbol{X}_i' + \varepsilon_i$$

where Y_o is an outcome variable of firm i, S is a dummy variable indicating whether the firm i received any local or national government support, and X is the set of observed characteristics of firm i. This specification is not suitable for consistent estimation of θ, for two main reasons. First, for most of the support measures, firms need to go through an application process to get it or take some action for this purpose. This creates selection bias. The selection bias will be positive if going through the application process to obtain the support is complex, and only better-performing firms can accomplish it. In this case, the set of firms that obtained support may have inherent features that both made them obtain support and perform better in terms of outcome variable of interest. Consequently, the estimate of θ would be higher than the true effect of policy S_i (positive selection bias).

In the context of the effectiveness of government support during the crisis, the selection bias can also be negative. It is expected that the firms that were more adversely affected by the pandemic, e.g., lost a larger share of their revenues or experienced deeper drop in liquidity, would try harder to obtain support from the government by working harder to make themselves aware of its existence and trying their best to go through the steps needed to obtain it. While support itself may have eligibility criteria such as firm size, even within the same sized firms, those hit harder are perhaps more likely to obtain it. There is also an element of solidarity contributing to this selection bias. In particular, the firms that are eligible for support may abstain from obtaining it if they do not need it as much, understanding that governments may have limited capacity to reach those that truly need this support. We anticipate a rather strong negative selection bias.

The second main source of our concerns about the above simple specification is omitted variable bias. There may be some unobserved variables that are correlated with S_i and also determine $Y_{o,i}$. For example, political connections can help some firms get government support

and also outperform others in terms of their business operations, perhaps through avoiding some of the restrictions imposed by the government to try to curb the spread of the virus.

To try to address these selection and endogeneity concerns, we apply two distinct methods, matching, and panel estimation, and implement each in two samples, full and restricted. In particular, we use propensity score and mahalanobis matching methods to estimate the average treatment effect on the treated (ATET) of the government support, using an important assumption. Let $Y_{o,1}$ denote the outcome with treatment (i.e., government support), and $Y_{o,0}$ the outcome without treatment. We cannot observe firms in both states, only in one. Assuming ignorability of treatment (Rosenbaum and Rubin 1983) or as this assumption is more often known – selection on observables – we can estimate the ATET defined as follows:

$$ATET = \mathbb{E}\left[Y_{o,1} - Y_{o,0} \,\middle|\, S = 1\right] \text{ assuming } \left(Y_{o,1}, Y_{o,0}\right) \perp S \mid \boldsymbol{X}$$

The assumption of selection on observables states that conditional on the firm characteristics that are observable in the data ($\boldsymbol{X}$), the counterfactual outcomes with and without support are independent of the realization of the government support. Restated loosely, this assumption means that if we can observe enough information through $\boldsymbol{X}$ that also determines treatment, then $\left(Y_{o,1}, Y_{o,0}\right)$ is independent of the realized S.

While we use a long list of variables as part of $\boldsymbol{X}$, both from the pre-pandemic surveys as well as the surveys conducted during the pandemic, the assumption of selection on observables is rather difficult to accept. In order to estimate θ, we also apply the panel estimation method. In particular, we use the following specification:

$$Y_{i,r} = \theta S_{i,r} + \beta \boldsymbol{X}'_{i,r} + \alpha_i + u_{i,r}$$

where r stands for the round of the surveys conducted during the pandemic (we have two rounds of the data available for both El Salvador and Georgia), and α_i captures firm fixed effects. Notably, the set of variables included in $\boldsymbol{X}'_{i,r}$ is rather different than those included in the matching estimation since the panel estimation includes only round-varying characteristics of firms, with α_i capturing all round-invariant characteristics (which do enter in the matching estimation).

Panel estimation with firm fixed effects only alleviates round-invariant sources of selection and endogeneity, however. Recall that conditionality of S on time-varying aspects of firms

(unobservable in the data) was one of our two criteria of selecting countries for this study. The selection and endogeneity concerns remain even without this conditionality. We think it is logical to expect that the positive selection bias is low in the fixed effects specification since inherent features of firms that determine their ability to go through some application processes is likely not strongly time-variant (and are thus mostly picked up by firm fixed effects). On the other hand, we think that negative selection bias is likely present, given the discussion above. We thus consider our estimates from the panel regression to likely be lower than the true effect of the government support. For this reason, we avoid commenting on the estimated size of the effect, and we instead focus on whether or not the data is suggestive of the effect that is above zero.

To further address the selection bias, we re-estimate both methods with a sample that is restricted in a specific way. A perennial and important question in the context of assessing the effectiveness of non-random policy is as follows: why did not all the eligible firms get the support? In other words, why did only some firms that otherwise look similar to the rest receive the support and others did not? The answer to this question has a direct implication on whether the control group is appropriate and thus whether the effectiveness can be estimated without bias. In an ideal environment for estimating the effectiveness of support, one would randomize this support, so the answer to the above question would be "pure chance". To approximate this ideal control group as much as possible with available data, we restrict the sample as follows. We exclude firms that seem to be outside of the process of government support, perhaps through their ineligibility, or lack of need, or unawareness, or some other reason. The exact variables used for this purpose are described in detail in Section 4.6, along with a brief discussion of the corresponding caveats.

4. Data and Main Variables

This section describes the data sources, discusses the variables we use, and provides summary statistics for both El Salvador and Georgia.

4.1. Data

As a baseline pre-pandemic data source, we use the most recent Enterprise Surveys (ES) completed in El Salvador in 2016 and Georgia in 2019. The ES are firm-level surveys with a representative sample of a country's private sector, implemented through a standard methodology with

stratification by firms' location, sector, and size.[5,6] Since the onset of the pandemic, two rounds of the COVID-19 Follow-up Surveys (COV-ES) were conducted in both countries, re-contacting all of the ES respondent firms and asking questions on a wide range of topics, including whether they have received any government support during the pandemic. We use these three sets of data in both countries, and we merge them through the unique firm identifier.

Table 1 shows the timing of all three surveys. The latest ES in El Salvador was conducted between March and August 2016, interviewing 719 firms, whereas the Georgia ES was implemented between March 2019 and January 2020, covering 581 firms. The first round of the COVID-19 Follow-up Surveys (COV-ES, R1) started in June 2020 in both countries. It took almost two months to complete it in El Salvador, but Georgia COV-ES, R1 was completed within the same month. The second round of the COVID-19 Follow-up Surveys (COV-ES, R2) was completed in October 2020 through January 2021 in El Salvador and October-November in Georgia. Importantly, the reference period for many questions appearing in ES is the last completed fiscal year prior to the survey implementation. This means that the reference year in ES is 2015 and 2018 for El Salvador and Georgia, respectively. The reference period for many questions appearing in COV-ES is the last completed month.

Both ES and COV-ES contain a rich set of information on firm characteristics, covering topics of sales, employment, finance, among others. As per the standard methodology, the ES data is obtained through face-to-face interviews with firm owners or top managers, lasting on average one hour, while COV-ES interviews were conducted by phone through much shorter interviews, lasting on average 25 minutes. All three datasets of each country contain sampling weights. COV-ES data combines the information contained in the ES sampling weights with the information about firm closures obtained during the implementation of COV-ES survey and provides updated sampling weights. Since the panel estimation method requires time-invariant sampling weights,

[5] See details of the survey methodology here: www.enterprisesurveys.org/en/methodology.

[6] Although the unit of observation is an establishment in ES, we refer the establishments in ES as firms following the common convention in literature.

we use the ES weights throughout this paper, including in the matching method and descriptive statistics.[7]

4.2. Dependent Variables

We focus on four outcome variables measured in COV-ES. These are, (i) the percentage change of permanent full-time workers since December 2019, (ii) whether the firm decreased its total number of temporary workers since the onset of the pandemic, (iii) whether the firm decreased its total hours of operation since the onset of the pandemic, and finally (iv) whether the firm anticipated falling in arrears on outstanding liabilities within the next six months of the date of the COV-ES interview.

Percentage change of permanent full-time workers is calculated using two separate questions of COV-ES. Each respondent is asked to provide the total number of their permanent full-time workers at the end of December 2019. This question was included in the first round of the survey in El Salvador but only implemented in the second round in Georgia. Each respondent is also asked to provide the total number of their permanent full-time workers at the end of the last completed month as of the interview date.

The second and third outcome variables of our interest, i.e., on temporary workers and on total hours of operations, are based on the respective questions included in COV-ES, asking respondents to report whether the respective item increased, remained the same, or decreased. Notably, for the question on temporary workers, the reference period in the first round of the survey is the onset of the pandemic, and in the second round – the previous round. In the question about the total hours of operations, the respondents are asked to compare the last completed month at the time of the interview with the same month one year prior. We create dummy variables taking the value of 1 for firms that report a decrease in the respective question and 0 otherwise. The fourth outcome variable of our interest, i.e., anticipation on arrears, is measured through a Yes/No question included in COV-ES.

Table 3 provides summary statistics of the four outcome variables separately by country and round of COV-ES. Firms in both countries, on average, mostly show statistically significant

[7] Our results from matching estimation are similar when we use COV-ES weights instead.

increases across rounds in our outcome variables of interest, with some exceptions. The fraction of firms that reduced the total number of temporary workers is unchanged across the two rounds in El Salvador. In Georgia, the average percentage change of permanent full-time workers increases almost four percentage points between two rounds, but this change is not statistically significant. The fraction of firms that anticipate falling in arrears on outstanding liabilities in the near future decreases considerably across the survey rounds in El Salvador but increases slightly in Georgia.

4.3. Main Explanatory Variable

COV-ES includes a question on whether firms received any national or local government support in response to the COVID-19 crisis, with three response categories: Yes, No but expect to receive it in the next three months, and No. In the first round, this question is asked with the reference period of "since the outbreak of COVID-19". In the second round, the firms that participated in the previous round were asked the same question but with the reference period "since the previous round month". We create a dummy variable taking value 1 for firms that responded Yes, and 0 otherwise to create our main explanatory variable. Importantly, for the second round, this variable is created cumulatively, meaning that if the firms responded Yes, either in the first or the second round are counted as having received government support.

Table 3 reports summary statistics of this variable too, for each country and round. The reach of government support increases considerably across rounds in both countries. While only 1.3% of the firms in El Salvador received government support in R1 (June-August 2020), 42.1% of them received some type of government support by R2 (October 2020 through January 2021). Government support in Georgia reached firms earlier than in El Salvador. By the time of R1 interviews (June 2020), the fraction of firms that received government support was 31.5%, and it increased to 48% in R2.

COV-ES also collects information about the types of government support. Firms that received or expect government support are further asked a battery of Yes/No questions on the type of support received. In particular, each firm was asked whether or not they received support in the form of (i) cash transfers, (ii) deferral of payments, (iii) access to new credit, (iv) fiscal relief, (v) wage subsidies, or (vi) some other form. Importantly, many firms in both countries received more than one type of government support, with a wide variety of combinations of the support types

appearing in the data in each country. It is important to note that the respondents are categorizing the support they received or expected into these types, creating room for several complications. First, one support measure may be characterized as taking more than one form. For example, some respondents could potentially consider a grant to be access to new credit as well as a cash transfer; or wage subsidy could also be interpreted as cash transfer depending on the details of its implementation. A further complication arises from the potential miscategorization of support measures. For example, we know that the Government of Georgia did not provide wage subsidies as an explicit form of support to businesses. Still, many firms in the Georgia survey reported receiving such support. We think that cash assistance provided to newly or temporarily unemployed was interpreted this way, but it is impossible to know exactly which support measure did each respondent mean when providing Yes/No responses to this battery. We thus urge readers to interpret our findings into the effectiveness of these types with additional caution.

4.4. Firm Characteristics Used for Matching Estimation

Table 2 presents the summary statistics for a long list of variables from the baseline ES datasets that we use in matching estimation. For each country, means are reported separately for two subgroups of respondents: those that received government support as of the second round (as defined above), the Support group, and those that did not, the No-Support group. The table also reports t-tests comparing means across the two sub-groups. In this subsection, we briefly describe each of these variables.

The firm size is determined based on the total number of full-time equivalent permanent workers of firms as of the reference period used in the baseline ES (i.e., December 2015 in El Salvador and December 2018 in Georgia). Following the standard ES stratification categories, small firms are those employing 5-19 workers. Medium-sized firms employ 20-99 workers, and large firms employ 100 or more workers. The firm age shows the number of years the firm has been operating, which we break into three categories, 0-10 years, 11-20 years, and 20+ years. The variables on sectors, namely, manufacturing, construction, transportation, hotels and restaurants, and retail, are dummy variables indicating the firms' sector of operation.

We use a long list of dummy variables, in particular, whether the firm is exporting at least 10% of their sales, whether the firm is at least 10% foreign-owned, whether the firm introduced new or significantly improved product or service or process innovation over the last three years

(prior to the ES), whether the firms is part of a larger firm whether the firm is majority female-owned, whether the firms has bank loan or line of credit, whether the firms have experienced electrical outages, and whether the firm offered formal training to its workers. We also include several continuous variables, namely the number of years that the top manager worked in the firm's sector and the natural logarithm of sales per worker to proxy for labor productivity. Furthermore, we use a composite index measuring firms' access to credit using multiple questions from the ES following Kuntchev et al. (2013), in the form of a dummy variable capturing whether the firm is fully credit-constrained.

Table 2 reports t-tests comparing means across the groups with and without government support (Support and No-Support groups, respectively) using asterisks: ***, ** and * stand for 1%, 5%, and 10% significance levels, respectively. As of the second round of COV-ES, 42.1% of the firms in El Salvador and 48.0% of the firms in Georgia have received some support from the government. Table 2 illustrates many differences between Support and No-Support groups in both countries. While the fraction of large firms in the Support group is smaller than the fraction of large firms in the No-Support group in El Salvador, it is the opposite in Georgia. Moreover, while the fraction of the medium-sized firms is significantly higher in El Salvador's Support group compared to its No-Support group, we do not see such a difference in Georgia.

Firm age and sectoral composition are similar in Support and No-Support groups in both countries, except for hotels and restaurants in Georgia. As discussed in Section 2 and described in detail in Appendix A.2., some of the government support in Georgia was specifically targeted towards hotels and restaurants. We see this targeting materialized in the data, with a higher fraction of hotels and restaurants appearing in the Support column than the No-Support group.

In terms of the ownership structure, while there is no difference between Support and No-Support groups in Georgia, in El Salvador, the fraction of foreign-owned firms and the fraction of firms that are part of a larger firm are higher in the No-Support group than in the Support group.

Another characteristic that the Support and No-Support groups differ significantly is access to finance. In El Salvador, 60% of the Support group have a bank loan or a line of credit, compared with 34.3% in the No-Support group. And in Georgia, a considerably higher fraction of firms in the No-Support group than in the Support group fall under the fully credit-constrained category.

Other firm characteristics such as top manager's experience, being an exporter, labor productivity, innovation, female ownership, electricity outages, and formal training appear to be on average similar across the Support and No-Support groups within each country.

4.5 Control Variables Used in Panel Estimation

To avoid omitted variables bias in the panel estimation, we include a number of time-varying characteristics and experiences of firms that can potentially be related to both obtaining government support and our outcome variables of interest. In particular, in these regressions, we include the average change in monthly sales compared to one year ago (i.e., comparing the last completed month as of the COV-ES interview with the same month one year ago). We also include a long list of dummy variables, in particular, whether the firm experienced decreased demand for products or services compared to one year ago; whether the firms experienced decreased supply of inputs compared to one year ago; whether the firm experienced a decrease in liquidity or cash flow availability (defined cumulatively since the onset of the COVID-19); whether the firms was overdue on obligations to financial institutions; whether the firm temporarily closed during the COVID-19 outbreak; whether the firm adjusted or converted their production or services; whether the firm started or increased online business activity; whether the firm started or increased non-contact delivery; whether the firm started or increased remote work (the latter five dummies are defined cumulatively across the rounds); and whether the firm is directly exporting at least 10% of its sales (as of the last completed month).

Table 3 shows descriptive statistics of all these variables within each country for both R1 and R2 separately. It also reports t-tests for changes across the rounds using asterisks similarly to Table 2. This table thus provides an illustration of changes in firms' performance and experiences as the pandemic unfolded between June 2020 and January 2021. In both countries, firms considerably changed several aspects of their operations over time. The fraction of firms that adjusted or converted their production or services, or the fraction of firms that started or increased their online business activity, or started or increased no-contact delivery, or started or increased remote work all increased significantly in both countries across the two rounds.[8]

[8] Since these dummy variables are defined cumulatively, these changes across rounds mean that more firms engaged in these activities between the first and the second rounds.

Table 4 and Table 5 break down Table 3 across Support and No-Support groups in El Salvador and Georgia, respectively, again reporting t-tests for comparisons of means across the subgroups using asterisks. In El Salvador, the firms in the Support group in the first round (1.3% of the sample) seems to perform better than the firms in the No-Support group in terms of two out of our four outcome variables of interest, namely percentage change in full-time permanent workers, and expectation of falling in arrears in the near future. This essentially means that in a simple OLS regression with no controls, the dummy variable capturing government support would have a positive coefficient in the regressions on these two outcome variables in El Salvador. Furthermore, a lower share of firms in the Support group adjusted or converted their production or services; or started or increased online business activity than in the No-Support group in R1. These unconditional differences across the subgroups disappear by the time of the second round.

In contrast, in Georgia, the firms in the Support subgroup underperform those in the No-Support subgroup as of R1 (Table 5) in two out of our four outcome variables, namely percentage change in full-time permanent workers and incidence of reduction of total hours of operation per week. In addition, a lower share of firms in the No-Support group than in the Support group experienced a decrease in liquidity or cash flow availability since the onset of the COVID-19 pandemic. In Georgia too, these unconditional differences across the subgroups disappear by the time of the second round.

4.6. Variables Used to Restrict the Sample

As discussed in Section 3, estimation of the effectiveness rests on the assumption that the firms that did not receive the support form a valid control group compared to the firms that did receive this support. In an environment that is ideal for such estimation, the support is randomly assigned (and fully enforced), so the effectiveness can be measured by comparing means across the treatment and the control group. In the absence of such an experiment, steps should be taken to ensure that the firms that are in the control group are appropriately comparable with the firms in the treatment group. In the context of the government policy during a crisis, this means that the control group should at least exclude the firms that do not need the government support, so they would not even try to obtain it. If these firms are included in the control group, then the firms in this group likely look better than the firms that are in the treatment group for reasons unrelated to the government support. Consequently, the effectiveness would be under-estimated.

To exclude such firms from the sample, we use the question implemented in both rounds of the COV-ES in El Salvador, asking firms to state why the firms did not receive any support. The question included the following response categories: (i) not aware (21% as of R2), (ii) not eligible (26%), (iii) no need for support (only available as a separate category in R2, 28%), (iv) applied but did not receive it (8%), (v) too difficult to apply (8%), (vi) did not expect to get it (only available as a separate category in R2, 1%), and (vii) other (with the ability to specify given in R2, 6%).[9] We use this variable to exclude the firms that respond with category (i), (ii), (iii), or (iv), i.e., the firms that are either not aware, or not eligible, or in no need of support, or have been rejected to receive the support. These categories cover the majority of the control group, so the restricted sample is much smaller. However, we think this restriction alleviates the concerns about the negative selection bias, at least to some extent.

The question on the reason behind not receiving government support was not implemented in either round of the COV-ES in Georgia. In the absence of other alternatives, to supplement the analysis of the Georgia data performed on the full sample, we restrict the sample using the question about the government support itself. Recall that whether the firm received any government support has three response categories: Yes, No but expect to receive it in the next three months, and No. As an alternative control group, we use only the firms that did not yet receive the government support but expect it in the next three months. While this category may well include the firms that do not need this support or are not even eligible for it (since it is firms' expectations that may not materialize), at least it excludes firms that seem to be outside of the government support process as of the interview, including perhaps due to lack of their awareness, and would at least to some extent help alleviate the negative selection bias.

5. Matching Estimation

This section discusses the matching methods estimation that we used to overcome the selection and endogeneity issues discussed in Section 3.

The goal of matching methods is to create two samples out of Support and No-Support groups that look alike in terms of firm characteristics. The only difference between the two samples

[9] The remaining 2% said that they do not know the reason.

would ideally be the government support. Thus, we can test the effect of government support on our outcome variables based on the difference between these two samples. Our matching estimation involves four steps.[10]

5.1. Selection of Covariates

The first step of the matching estimation methods is to select variables related to both outcome variables and government support. We use a long list of firm characteristics from the ES, as discussed in Section 4. In particular, we use: firm size, age, sector dummies, exporting, foreign and female ownership status, top managers' experience, whether the firm introduced any innovation, labor productivity, being part of a larger firm, financial status, experiencing electricity outages, and whether the firm offered formal training.

For matching analysis, we restrict our sample to the firms for which we have observations on firm characteristics as well as four outcome variables within each country. We further restrict the No-Support group in El Salvador by dropping the firms that are not aware of the support, or are not eligible, or do not need the support, or do not know why they have not received any government support.

5.2 Matching Metrics

The second step of the matching estimation is selecting a metric for matching observations in Support and No-Support groups. We use propensity score and mahalanobis distance metrics for this purpose. The propensity score is the probability of receiving government support given firm characteristics. We estimated propensity scores by logistic regression using survey weights. The mahalanobis distance is a scaled distance between observations as follows:

$$MD(X_i, X_j) = \sqrt{(X_i - X_j)' C\ (X_i - X_j)}$$

[10] Although there is no guideline for how to apply the matching technique, many papers follow these steps, explicitly or implicitly. Please see Abadie & Cattaneo (2018), Rathi & Vermaak (2018) Garido et al (2014), Dugoff et al (2014) among many others.

where X_i and X_j are the covariates of two observations and C is the scaling parameter which is equal to $diag\left(\frac{1}{W}\right)\sum diag\left(\frac{1}{W}\right)$, where $diag(.)$ is a diagonal matrix and $\sum$ is the variance-covariance matrix of covariates, and W stands for survey weights.

5.3 Matching and Balancing

The third step involves matching observations based on the metric calculated at the second step and checking the balance of the matched sample. We use two different matching algorithms: kernel and 5 to 1 nearest neighbor (5:1 NN) algorithms. The kernel matching algorithm matches Support observations with the No-Support observations by giving closer observations more weight. 5:1 NN matching matches five closest No-Support observations based on the selected metric with each Support observations. We perform matching with a replacement for both algorithms. Subsequently, an observation in the No-Support group can match with multiple observations from the Support group. For example, if the propensity score is the selected metric, 5:1 NN matching algorithm finds five firms in the No-Support group for each firm in the Support group such that the selected five firms have the closest propensity score with the firm in the Support group. Moreover, we restrict the matching sample to the common support in the case of propensity score metric. In other words, we drop the firms in the No-Support group whose propensity score is lower than the minimum propensity score of the Support group and whose propensity score is higher than the highest propensity score in the Support group. In addition, we perform an exact match of hotels and restaurants sectors for Georgia in light of the design of the government support discussed in Section 2.

Table 6 through Table 9 show the balancing performance of the matching algorithms. Raw samples, i.e., before the matching is performed, are given in columns 2 and 3. Since we are restricting our sample to the firms that have answered all outcome questions as well as firm characteristics and to the firms in the common support, the number of observation in matching exercises is lower than in Section 4. Moreover, because of this reason, the sample sizes in both propensity score and mahalanobis matching tables is identical within each country. We test whether a firm characteristic of the Support group is different from that of the No-Support group and report with asterisks in the No-Support group. Finally, we take matching into consideration when we compare Support and No-Support in Kernel and 5:1 Matching samples by combining survey weights and the matching weights.

The raw sample, i.e., before the matching is performed, of El Salvador is overall well balanced except for the fraction of medium-sized firms, being an exporter, labor productivity, experiencing electricity outages, and offering formal training. In other words, firm characteristics in the unmatched sample's Support and No-Support groups are statistically similar other than the characteristics mentioned here. Table 6 and Table 7 present balancing of covariates before and after matching when using propensity score and mahalanobis distance as a matching metric, respectively, in El Salvador. While both kernel and 5:1 NN matching seems more balanced in propensity score matching compared to mahalanobis distance matching, the balance of firm characteristics in both matchings is worse than the balance of the unmatched sample in El Salvador. In other words, fewer firm characteristics in Support and No-Support groups of the unmatched sample are statistically different from each other compared to the kernel and the 5:1 NN matchings in both propensity and mahalanobis distance cases.

Table 8 and Table 9 summarize the balancing of the firm characteristics before and after propensity score and mahalanobis distance matchings in Georgia, respectively. The propensity score matching yields more balanced covariates compared to the mahalanobis matching. Both kernel and 5:1 NN propensity score matching have fewer covariates statistically different in Support and No-Support groups than the unmatched sample. While the covariates in the raw sample of Georgia are balanced except the fraction of large firms and the fraction of firms with a bank loan or line of credit, this imbalance disappears in kernel matching of the propensity score. After 5:1 NN propensity score matching, the imbalance between Support and No-Support groups that the raw sample has, disappears but the fraction of the construction sector is slightly higher in the Support group compared to the No-Support group.

5.4 Estimating ATET

The final step of matching estimation is calculating the ATET of government support. Results of ATET estimations for all outcome variables under all matching algorithms metrics are presented in Table 10. The matching weights and survey weights are combined for the estimation, similar to balancing results. Since all the outcome variables except the percentage change of the permanent full-time employees are dummy variables, these estimates should be interpreted as probabilities for dummy outcome variables.

One of the results we find out of the matching estimation method is that almost all of the estimated ATET are not statistically significantly different from zero. The only exception is the effect of government support on the percentage change of full-time employees estimated by kernel matching based on mahalanobis distance. While this estimation is significant at the 10% level, we note that balancing of covariates in El Salvador's matched sample was not successful compared to that of the unmatched sample.

Another result of the matching estimation is the sign of the ATET. Although almost all of them are not statistically significantly different from zero, all of them have positive signs except for two: the effect of government support on the total number of temporary workers in matching with mahalanobis distance in El Salvador and on anticipating falling in arrears on outstanding liabilities within next six months in matching with propensity score in Georgia.

To sum up, while the balancing of covariates in Georgia's matched samples seem better than that of its raw sample, the ATET is not significantly different from zero, neither in Georgia nor in El Salvador. One possible reason for the El Salvador case is the long gap between ES and COV-ES. The latest El Salvador ES was conducting in 2016, referencing the 2015 fiscal year. This may also partly explain the poor performance of balancing of covariates in El Salvador. Another possible reason why we have insignificant coefficients varying is a small sample size. While ES and COV-ES contain 719 and 581 observations in El Salvador and Georgia, respectively, the samples we use for matching estimation consist of only 169 and 235 observations in El Salvador and Georgia, respectively, because of the restrictions mentioned at the beginning of this section.

6. Panel Estimation

This section uses both rounds of the COV-ES implemented in El Salvador and Georgia, along with the baseline ES. Consequently, the sample within each country is different from the samples we used in Section 5. We use the fixed effect panel regression method to help identify the causal link between the government support policies and the outcome variables of interest.

Recall that, in Round-2 data, the firms that received any government support since the onset of the pandemic, whether before Round-1 or after, are counted as having received government support. This effectively divides the firms that enter in our analysis into three sub-groups: (i) those that did not receive any support in either round of the survey, (ii) those that received support

(irrespective of when), and (iii) those that received support only in Round-2, and not in Round-1. In the context of fixed effects regression, the third group – i.e., those that switched government support status across rounds – serves as the treatment group.

While firm characteristics are captured by the firm fixed effects, the endogeneity and selection concerns remain, as discussed above, which we try to address by adding a wide range of time-varying controls from the COV-ES data (see Section 4.5). To further address the selection issue, we estimate the same model for restricted samples. As discussed in Section 4.6, this is done differently for El Salvador and for Georgia. In particular, for El Salvador, we use firms' responses to the question on reasons that they did not receive government support and drop firms saying that either that (i) they are not eligible for the support, or (ii) they do not need it, or (iii) they applied but did not get it, or (iv) they were not aware of any such support. The firms that provided one of these responses are unlikely to be a good comparison for the firms that received government support because of the likely discrepancy in the way the pandemic shaped firms' demand and supply. The question on reasons for not receiving support was not asked in Georgia. There, the firms that did not receive government support but also do not expect to receive it in the next three months are excluded from the sample.

Table 11 and Table 12 report results from our main panel fixed effects regressions for El Salvador and Georgia, respectively. We use linear estimation when the outcome variable is the percentage change of permanent full-time workers and logistic regressions for the remaining three outcome variables, which are binary. For each outcome variable, we estimate fixed effect regressions first using the full sample and next using the restricted sample, hence two columns for each outcome variable. Similar to matching estimation results, the effect of government support on the percentage change of permanent full-time workers is positive but not statistically significantly different from zero in both countries. Most of the other coefficients of interest are also not statistically significantly different from zero, apart from the incidence of decreased total hours of operation per week in El Salvador, where firms that received government support were less likely to have done so.

It is important to note the direction of changes in coefficients of interest from full sample to restricted sample. Recall that the samples are restricted to exclude firms that perhaps were less negatively hit by the pandemic, making the control group of firms look better than a purely random

control group. It is thus expected that the coefficients on policy are further away from zero for the restricted samples. There is only one possible exception from this pattern, namely the variable on whether the firm anticipates falling in arrears on outstanding liabilities in the context of Georgia. There, the firms that expect but have not yet received government support probably also expect to avoid falling in arrears once the support arrives, so the policy may look less effective when using the restricted sample of expectant firms. In the remaining seven specifications, the coefficient on the government support is expected to move further away from zero for the restricted samples. Instead, the coefficients in Table 11 and Table 12 move in the opposite direction for two out of the three outcome variables in Georgia and two of the four variables in El Salvador (percentage change of full-time permanent workers since December 2019). These are concerning patterns that are worth keeping in mind when interpreting our results.

Table 13 and Table 14 show the results of the panel estimations when the different types of government support are added as additional explanatory variables in El Salvador and Georgia, respectively. As in Tables 11 and 12, two columns are reported for each outcome variable; the first reports estimates using unrestricted sample, and the second reports estimates using the restricted sample. Tables 13 and 14 aim to understand the distinct effects of different types of government support. The surveys in El Salvador and Georgia contained questions about the following types of government support: cash transfers, deferral of payments, access to new credit, fiscal relief, and wage subsidies (with the additional general "other" category also asked). The regressions in Table 13 and Table 14 use the types of support that were received by at least 50 firms in the sample of the respective country. The sample size restriction gives us two variables in El Salvador for cash transfers and wage subsidies; and three variables in Georgia, deferral of payments, fiscal relief, and wage subsidies. Notably, the addition of these variables in the regressions changes the interpretation of the coefficient on the general support variable: it now estimates the average effect of policy for the forms that received support types other than those explicitly included in the regressions.

Table 13 suggests that cash transfers may have had a positive and statistically significant effect on the percentage change of permanent full-time workers in El Salvador, while wage subsidies had a negative and statistically significant effect on the same outcome variable (column 2). Furthermore, cash transfers appear to have decreased the incidence of reductions in total hours

of operation per week (column 6). In contrast, Table 14 does not reveal any new results for Georgia, with little change in the coefficients on the general support, suggesting an equal absence of effectiveness across types of government support there.

In the following two sub-sections, we break down the effectiveness of government support in general and its types by firm size and sector. For this, we use the size and sector information of the firms as obtained during the baseline ES.

6.1. By Firm Size

To test whether the effect of government support differs by the firms' size, we add to our models an additional control variable: interaction term of the government support with the log of the total number of full-time equivalent workers in the baseline ES as an independent variable. First, we do this for the main specifications (without the types of government support, Tables 11 and 12). Table 15 and Table 16 report these results for El Salvador and Georgia, respectively. While none of the coefficients on the interaction term are significant for El Salvador, nearly all have expected signs, with effect decreasing by size, except for the variable on expectation about falling in arrears. In contrast, Table 16 suggests a strong pattern in Georgia of positive and statistically significant effect of the government support on two outcome variables with considerable drop-off by firm size. In particular, the firms that received government support experienced a more positive change in the number of permanent full-time workers and were less likely to have decreased the total number of temporary workers, but this effect decreases with firm size, effectively disappearing for firms with roughly 30 workers or more. From the perspective of concerns about endogeneity and selection, this finding that smaller firms may have benefited more from government support is somewhat encouraging. This is because the logic behind the positive bias is that firms have some time-varying omitted characteristics that make them more adept in obtaining government support and performing better in terms of our outcome variables. Smaller firms are generally not considered to have such features. Consequently, finding more positive results in an environment where concerns about positive bias are lower provides some validation.

We repeat this exercise of adding the interaction term of government support with the baseline ES size of firms for the specifications that include types of government support. Table 17 and Table 18 show the results for El Salvador and Georgia, respectively. Cash transfers appear to have decreased the probability of firms reducing the total number of temporary workers for small

firms, with the exact opposite pattern for wage subsidies. Furthermore, wage subsidies appear to have decreased the anticipation of falling in arrears on outstanding liabilities for small firms.

Table 18 suggests that fiscal relief had a beneficial effect on the percentage change of permanent workers for small firms (fewer than approximately 20 workers), with a similar pattern for anticipation of falling in arrears. In contrast, wage subsidies have benefited larger firms more in terms of the percentage change of permanent workers. Unexpectedly, deferral of payments increases the probability of reducing the number of temporary workers for smaller firms.

Rather distinct patterns of the effectiveness of different support policies in different countries point to the difficulty of general cross-country analysis and suggest the importance of taking into account nuances of each policy and context.

6.2. By Sector

Finally, we look at the breakdown of the effectiveness of government support policies by sector. We use the sectors of stratification used in the baseline ES. We also divide the retail sector into two groups, namely food retailers or pharmacies and the rest, anticipating that the pandemic has been experienced by firms in these sub-groups rather differently. For brevity, we only report panel estimation results across sectors for the restricted samples (i.e., corresponding to even-numbered columns in Tables 11 and 12). Table 19 and Table 20 report estimates across sectors in El Salvador and Georgia, respectively.

Sectoral breakdown in El Salvador provides a number of unexpected patterns, perhaps attributed to a rather small sample (Table 19). Among the food manufacturers, those that received government support appear more likely to have decreased total hours of operations per week, suggesting a counter-productive effect of the policy. Receiving government support is also associated with more anticipation of falling in arrears among food manufacturers, another counter-productive effect. It is hard to find much logic in these rather mixed results apart from noting that the sample of food manufacturers is likely a mix of two rather distinct groups. Perhaps one set of food manufacturers mostly supplied to hotels or restaurants and were negatively hit by the pandemic, while another set of food manufacturers mostly supplied to grocery stores with likely positive hit by the pandemic. Government support likely had a distinct impact on these subsets of food manufacturers across our outcome variables of interest. It is also conceivable that the food

manufacturers that received government support used that support to afford themselves better-perceived health safety by operating at lower capacity while at the same time retaining a higher share of permanent workers, a combination that perhaps increased anxiety regarding the arrears in the near future.

Yet another counter-productive effect of government appears to be among non-food retailers or pharmacies where government support appears to have increased the incidence of reduction in the total hours of operations per week, with the opposite, more intuitive effect for retailers of food or pharmacies. We do also see two additional intuitive patterns. In particular, government support appears to positively affect manufacturers of textiles and garments in terms of both percentage change of permanent full-time workers and incidence of reduction in temporary workers.

Table 20 reports the same estimates for Georgia. Similar to El Salvador, we see a counter-productive effect of government policy for food manufacturers. The firms in this sector that received government support are more likely to have decreased total hours of operation in Georgia, too, again perhaps to achieve a better-perceived health safety.

It is widely anticipated that the pandemic has hit the hotels and restaurants hard, which makes it encouraging to see that the government support seems to have had a positive effect on the permanent workforce in this sector. However, in the same sector, government support seems to have contributed to increased anxiety about falling in arrears in the near future. Another example of the counter-productive effect is for the permanent workforce in food retailers and pharmacies, where it appears that the firms that received government support have experienced more negative changes compared with the firms that have not yet received such support (but are expecting in the next three months).

Interestingly, the effects of government support on the probability of reducing total hours of operation for non-food/pharmacy and food/pharmacy retail sectors are opposite in Georgia compared to that in El Salvador. In Georgia, government support decreases the incidence of reduction in total hours of operation in non-food/pharmacy retail sectors but increases for food retailers and pharmacies.

To summarize the sectoral breakdown of the effectiveness of government policy, our analysis confirms the intuition that government support has helped manufacturers of textiles and garments (in El Salvador) and hotels and restaurants (in Georgia). Less intuitively, food manufacturers in El Salvador appear to have benefited from government support through avoiding changes in permanent full-time workforce, though the same effect in Georgia is opposite but not statistically significant (perhaps due to the composition of food manufacturers within each country, with supply chains leading towards hotels or restaurants as opposed to grocery stores). We also find evidence of counter-productive effects of government policy in some sectors. In particular, government support was found counter-productive in Georgia for food retailers and pharmacies across two of our four outcome variables. Overall, the outcome variables on the incidence of decreased total hours of operations and expectations of falling in arrears offer a number of counter-intuitive findings. The former is likely attributed to the conflict between perceived health safety, which likely forced firms to reduce hours of operations, and the business objective of staying open to keep earning revenue. As for the expectations of falling in arrears, we conclude that this variable is rather noisy in general, perhaps pointing to an unprecedented uncertainty that firms were facing during the pandemic, complicating the formation of logical and grounded expectations about the near future.

7. Summary and Conclusions

In this paper, we used firm-level data collected before and during the COVID-19 pandemic (ES and COV-ES) in El Salvador and Georgia to estimate the effectiveness of government support to the private sector in terms of firms' workforce, operations, and expectations. While ES and COV-ES data cover 44 countries, we focus on just two because, in most of the other countries, the government support was available only to the firms whose experience during the pandemic was at or worse than the set thresholds, such as a drop in revenue or workforce, etc. Such selection criteria, coupled with the fact that this information is not normally contained in data, make a valid estimation of the effectiveness of support nearly impossible. We also excluded countries that had fewer than around 150 firms reporting having received government support in COV-ES data.

We apply matching and panel estimation techniques. To match firms that received support with those that did not, we use a long list of firm characteristics from the pre-pandemic ES and some variables measuring firms' experience during the pandemic. For panel analysis, we use two

rounds of COV-ES and include individual firm fixed effects to eliminate selection bias stemming from time-invariant characteristics of firms. We also add a set of COV-ES variables to capture time-varying aspects of firms' operations, reducing endogeneity concerns. To alleviate concerns about selection bias, we re-estimate panel regressions on a restricted sample. For El Salvador, this means that we drop firms that state the following reasons for not receiving government support: not aware, not eligible, no need, or applied but did not receive it. In the context of the pandemic, these firms likely faced fewer hardships than others, so we expect to reduce negative selection bias by excluding them from the control group. This same information is not available in the Georgia surveys, so to restrict the sample, we include only the firms that have not yet received support but are expecting in the next three months.

We think that some concerns about selection bias – which can be positive as well as negative, with positive selection bias likely reduced in the context of panel regressions – and endogeneity likely remain, so we refrain from commenting on the estimated size of the effectiveness of government support and only focus on whether it is estimated to be above zero.

We find nearly no statistically significant positive effect of government support on any of our four outcome variables in either country, with only one exception. Our panel regressions suggest that government support helped firms avoid reducing their total hours of operations per week in El Salvador (Table 11). COV-ES data includes questions asking firms to specify whether they received the following types of support: cash transfers, deferral of payments, access to new credit, fiscal relief, and wage subsidies (with the additional general "other" category also asked), with multiple types possible at the same time. Among these questions, we added to the respective panel regression the ones with at least 50 Yes responses. This exercise suggests that the link between government support and total hours of operations in El Salvador is through cash transfers (Table 13). We also find that cash transfers had a positive effect on firms' permanent workforce in El Salvador, but this effect was fully counteracted by wage subsidies (Table 13).

Breakdown of the effectiveness of government support by size reveals that smaller firms, namely those employing roughly 25 or fewer workers pre-pandemic (in the baseline ES), benefited more in Georgia (Table 16) in terms of changes to the permanent workforce. This link seems to be through fiscal relief (Table 18), which appears to have benefited smaller firms more. This positive effect of fiscal relief was partially counteracted with effects of wage subsidies that appear to have

benefited larger firms more (Table 18). The finding that, in Georgia, smaller firms benefited more, at least in terms of one outcome variable, increases our confidence in the overall analysis. This is because, in the context of panel regressions, we expect selection bias to be negative. Since smaller firms are likely to be needier, the negative selection bias would work against finding the pattern that we do find. While we find similar size dependency in El Salvador, the coefficients are mostly not statistically different from zero there (Table 15 and Table 17). We think this may be due to the size information being more outdated for El Salvador, where the baseline ES was collected in 2016, compared with Georgia's baseline ES of 2019.

The breakdown of the effectiveness of government support by sector confirms some of our expectations but is mixed otherwise. The baseline ES in El Salvador had manufacturers of textiles and garments stratified separately, while the ES in Georgia had hotels and restaurants stratified separately. Both ES had food manufacturers as separate stratification sectors, as well as retail, which in our analysis we broke down into food retailers or pharmacies and the rest of the retailers. As expected, manufacturers of textiles and garments in El Salvador show signs of benefitting from government support in terms of permanent and temporary workers (Table 19), while hotels and restaurants in Georgia seem to have benefitted in terms of permanent workers. Effects on food manufacturers are very different in the two countries, perhaps related to the structure of the supply chain leading towards hotels or restaurants as opposed to grocery stores (this information is not available in the ES or COV-ES). Effects on subgroups of retailers are also mixed, including several counter-productive effects of government support, indirectly suggesting that perhaps reduction of total hours of operation was perceived desirable from the health and safety perspective.

In terms of the outcome variables of interest, we find most effects on the percentage change in the total number of permanent full-time workers since December 2019, with the dummy variables on temporary workers and hours of operations also showing important reactions to the government support. However, the variable capturing firms' expectations about falling in arrears on outstanding liabilities in the six months following the interview appears the hardest to predict or make sense of, perhaps pointing to considerable uncertainties that firms faced during the pandemic, complicating the formation of consistent expectations.

While we take multiple steps to alleviate the selection and endogeneity concerns, it is still possible that we are under-estimating the full effect of the government support. This could be due to several reasons. For example, we focus on just four outcome variables, and the effect may have worked through different outcomes. Furthermore, we do not examine attrition, and the support may have helped the firms that otherwise would have closed to continue operations. Moreover, we do not have information about the size of government support, which is likely an important determinant of its effectiveness. We also study the short-term effects of government support, while its full effect may take a longer time to materialize. Nevertheless, we hope our findings are useful in informing the policy, perhaps through highlighting potential complexities.

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Tables

Table 1: Timing of the Surveys

	ES	N	COV-ES, R1	COV-ES, R2
El Salvador	03/2016-08/2016	719	06/2020 - 08/2020	11/2020 - 01/2021
Georgia	03/2019-01/2020	581	6/2020	10/2020 - 11/2020

Note: This table shows dates of implementation of ES and two rounds of COV-ES. ES stands for Enterprise Surveys, and COV-ES, R1, and COV-ES, R2 stand for COVID follow-up Enterprise Surveys Round 1 and COVID follow-up Enterprise Surveys Round 2, respectively. See Section 4 for more details.

Table 2: Summary Statistics of Firm Characteristics

	El Salvador			Georgia		
	Support	No-Support	N	Support	No-Support	N
Firm Size						
Small (5-19)	0.646	0.737	407	0.592	0.628	472
Medium (20-99)	0.325	0.164**	407	0.325	0.347	472
Large (100+)	0.029	0.099**	407	0.084	0.025**	472
Firm Age						
0-10 years	0.124	0.186	408	0.544	0.584	476
11-20 years	0.455	0.435	408	0.309	0.318	476
20+ years	0.421	0.379	408	0.147	0.098	476
Manufacturing	0.212	0.217	408	0.232	0.215	476
Construction	0.046	0.105	408	0.154	0.113	476
Transportation	0.095	0.053	408	0.084	0.025	476
Hotels & restaurants	0.013	0.058	408	0.175	0.073*	476
Retail	0.417	0.317	408	0.227	0.204	476
Years of the top manager's experience	22.422	22.091	408	16.991	15.932	476
Exporting directly 10% of sales	0.110	0.121	408	0.150	0.112	476
At least 10% of foreign owned	0.065	0.191**	408	0.057	0.086	476
Product/service or process innovation	0.454	0.475	408	0.492	0.485	476
Part of a larger firm	0.218	0.368*	408	0.039	0.030	476
Log sales per worker	9.947	9.965	408	9.817	9.826	476
Majority female owned	0.289	0.175	408	0.097	0.138	476
Fully credit constrained	0.021	0.087	408	0.013	0.158***	476
Bank loan/line of credit	0.600	0.343***	408	0.469	0.409	476
Experiencing electrical outages	0.495	0.462	408	0.555	0.506	476
Offering formal training	0.638	0.528	408	0.365	0.294	476

Note: This table shows firm characteristics in El Salvador and Georgia ES datasets. Support columns show the respective statistics of firms that have received government support as reported in COV-ES R2. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the number of observations. Means are weighted using the sampling weights from the ES. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. Standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 4 for more details.

Table 3: Firm Performance and Government Support during COVID-19

	El Salvador			Georgia		
	Round 1	Round 2	N	Round 1	Round 2	N
Percentage change of permanent full-time employees since December 2019	-0.161	-0.088*	785	-0.142	-0.103	853
Firms that the total number of temporary workers decreased	0.332	0.328	776	0.283	0.447***	845
Firms that total hours of operation per week decreased	0.905	0.509***	800	0.633	0.384***	952
Firms that anticipate falling in arrears on outstanding liabilities within next 6 months	0.515	0.198***	783	0.285	0.349*	794
Received national or local government support	0.013	0.421***	797	0.315	0.480***	960
Cash transfers	0.001	0.113***	791	0.027	0.059**	904
Deferral of payments	0.003	0.100**	792	0.160	0.210***	936
Access to new credit	0.002	0.140***	791	0.005	0.025	899
Fiscal relief	0.007	0.080***	791	0.072	0.273***	907
Wage subsidies	0.000	0.360***	789	0.120	0.194***	916
Other support	0.005	0.021	706	0.006	0.027*	910
Average change monthly sales compared to one year ago	-0.588	-0.249***	792	-0.484	-0.297***	885
Overdue on obligations to financial institutions	0.270	0.376***	736	0.107	0.139*	910
Adjusting or converting their production or services	0.224	0.768***	802	0.269	0.404***	957
Decreased demand for products or services compar to one year ago	0.751	0.668	802	0.689	0.554***	951
Decreased liqdt or cash flow avail. since COVID-19 began	0.835	0.832	803	0.706	0.829***	958
Started or increased online business activity	0.326	0.565***	803	0.251	0.347***	964
Started or increased delivery of goods, services or carryout	0.366	0.567***	803	0.234	0.329***	965
Started or increased remote work	0.472	0.538	801	0.433	0.509***	988
Exporting directly (at least 10% of sales)	0.052	0.107	734	0.093	0.073	878

Note: This table shows descriptive statistics from El Salvador and Georgia COV-ES data. Round 1 and Round 2 columns show the respective statistics of firms in the respective round. N denotes the number of observations. Means are weighted using the sampling weights from the ES. Asterisks reported in the columns for Round-2 are for unconditional t-tests comparing means across the two rounds. For these t-tests, standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 4 for more details.

Table 4: Firms in El Salvador during COVID

	Round 1			Round 2		
	Support	No-Support	N	Support	No-Support	N
Percentage change of permanent full-time employees since December 2019	-0.053	-0.161***	377	-0.022	-0.133	401
Firms that the total number of temporary workers decreased	0.210	0.335	372	0.366	0.302	395
Firms that total hours of operation per week decreased	0.850	0.906	383	0.476	0.520	410
Firms that anticipate falling in arrears on outstanding liabilities within next 6 months	0.041	0.524***	369	0.219	0.181	407
Cash transfers	0.118		387	0.270		404
Deferral of payments	0.265		387	0.238		405
Access to new credit	0.179		387	0.334		404
Fiscal relief	0.541		387	0.190		404
Wage subsidies	0.013		387	0.861		402
Other support	0.378		387	0.087		319
Average change monthly sales compared to one year ago	-0.476	-0.589	387	-0.269	-0.229	406
Overdue on obligations to financial institutions	0.111	0.272*	379	0.535	0.269*	373
Adjusting or converting their production or services	0.034	0.228***	359	0.756	0.788	410
Decreased demand for products or services compar to one year ago	0.882	0.749	385	0.731	0.613	410
Decreased liqdt or cash flow avail. since COVID-19 began	0.832	0.835	385	0.848	0.811	408
Started or increased online business activity	0.055	0.327***	385	0.540	0.573	409
Started or increased delivery of goods, services or carryout	0.562	0.362	387	0.527	0.604	409
Started or increased remote work	0.631	0.472	387	0.638	0.473	404
Exporting directly (at least 10% of sales)	0.111	0.051	387	0.179	0.058	409

Note: This table shows the firm performance and government support types in El Salvador in Round 1 and Round 2 surveys. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the observation number. Means are weighted using the sampling weights from the ES. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 4 for more details.

Table 5: Firms in Georgia during COVID

	Round 1			Round 2		
	Support	No-Support	N	Support	No-Support	N
Percentage change of permanent full-time employees since December 2019	-0.300	-0.069*	403	-0.078	-0.129	431
Firms that the total number of temporary workers decreased	0.296	0.272	427	0.465	0.419	404
Firms that total hours of operation per week decreased	0.702	0.606*	471	0.420	0.360	457
Firms that anticipate falling in arrears on outstanding liabilities within next 6 months	0.223	0.306	412	0.330	0.355	363
Cash transfers	0.087		497	0.151		463
Deferral of payments	0.507		497	0.467		407
Access to new credit	0.017		497	0.066		439
Fiscal relief	0.229		496	0.695		402
Wage subsidies	0.385		496	0.475		411
Other support	0.018		497	0.074		420
Average change monthly sales compared to one year ago	-0.530	-0.458	433	-0.292	-0.303	396
Overdue on obligations to financial institutions	0.112	0.105	481	0.138	0.126	432
Adjusting or converting their production or services	0.271	0.271	497	0.429	0.356	416
Decreased demand for products or services compar to one year ago	0.714	0.683	469	0.590	0.527	442
Decreased liqdt or cash flow avail. since COVID-19 began	0.776	0.673**	486	0.847	0.806	459
Started or increased online business activity	0.284	0.238	496	0.337	0.347	456
Started or increased delivery of goods, services or carryout	0.249	0.229	496	0.376	0.278	455
Started or increased remote work	0.426	0.440	497	0.522	0.476	455
Exporting directly (at least 10% of sales)	0.106	0.088	394	0.080	0.069	458

Note: This table shows the firm performance and government support types in Georgia in Round 1 and Round 2 surveys. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the observation number. Means are weighted using the sampling weights from the ES. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 4 for more details.

Table 6: Balancing of Covariates after Propensity Score Matching, El Salvador

	Raw		Kernel Matching		5:1 NN Matching	
	Support	No-Support	N	Support	No-Support	N
Firm Size						
Small (0-19)	0.576	0.717	0.585	0.665	0.591	0.681
Medium (20-99)	0.411	0.174*	0.402	0.247*	0.395	0.255*
Large (100+)	0.014	0.109	0.013	0.088**	0.014	0.064*
Firm Age						
0-10 years	0.108	0.054	0.111	0.045	0.110	0.055
11-20 years	0.437	0.358	0.457	0.388	0.448	0.337
20+ years	0.456	0.587	0.433	0.567	0.442	0.607**
Manufacturing	0.251	0.493	0.252	0.444**	0.253	0.409**
Construction	0.068	0.023	0.071	0.029	0.070	0.051
Retail	0.497	0.315	0.492	0.274**	0.496	0.280***
Year of the top manager's experience	22.655	20.442	21.677	22.959	21.819	22.139
Exporting directly 10% of sales	0.133	0.027*	0.129	0.060	0.128	0.046*
At least 10% of foreign owned	0.095	0.032	0.099	0.069	0.097	0.061
Product/service or process innovation	0.441	0.572	0.424	0.541	0.431	0.580*
Part of a larger firm	0.235	0.306	0.241	0.333	0.236	0.299
Log sales per worker	10.163	9.614**	10.161	9.688***	10.143	9.746***
Majority female owned	0.320	0.553	0.321	0.434	0.328	0.452
Fully credit constrained	0.004	0.079	0.005	0.042*	0.005	0.030
Bank loan/line of credit	0.702	0.660	0.706	0.642	0.708	0.609
Experiencing electrical outages	0.353	0.595*	0.367	0.556**	0.361	0.561**
Offering formal training	0.730	0.306***	0.737	0.412***	0.723	0.426***
N	169		158		162	

Note: This table shows the balancing of firm characteristics in El Salvador when propensity score is used as a matching metric. Raw columns describe firms before matching happens. Kernel matching columns are for the firms that are matched according to the kernel matching algorithm. 5:1 NN Matching columns are for the firms that are matched according to the 5 to 1 nearest neighbor matching algorithm. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the number of observations. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 5 for more details.

Table 7: Balancing of Covariates after Mahalanobis Distance Matching, El Salvador

	Raw		Kernel Matching		5:1 NN Matching	
	Support	No-Support	Support	No-Support	Support	No-Support
Firm Size						
Small (0-19)	0.576	0.717	0.489	0.871***	0.489	0.859***
Medium (20-99)	0.411	0.174*	0.510	0.116***	0.510	0.137***
Large (100+)	0.014	0.109	0.002	0.013	0.002	0.004
Firm Age						
0-10 years	0.108	0.054	0.054	0.015	0.054	0.036
11-20 years	0.437	0.358	0.581	0.383**	0.581	0.367**
20+ years	0.456	0.587	0.365	0.602***	0.365	0.596***
Manufacturing	0.251	0.493	0.094	0.525***	0.094	0.537***
Construction	0.068	0.023	0.049	0.004*	0.049	0.010
Retail	0.497	0.315	0.771	0.343***	0.771	0.289***
Year of the top manager's experience	22.655	20.442	21.273	21.930	21.273	22.442
Exporting directly 10% of sales	0.133	0.027*	0.085	0.000**	0.085	0.001**
At least 10% of foreign owned	0.095	0.032	0.043	0.004	0.043	0.006
Product/service or process innovation	0.441	0.572	0.215	0.793***	0.215	0.683***
Part of a larger firm	0.235	0.306	0.119	0.436***	0.119	0.263**
Log sales per worker	10.163	9.614**	10.356	9.589***	10.356	9.622***
Majority female owned	0.320	0.553	0.514	0.803***	0.514	0.609
Fully credit constrained	0.004	0.079	0.000	0.003	0.000	0.003
Bank loan/line of credit	0.702	0.660	0.847	0.781	0.847	0.679**
Experiencing electrical outages	0.353	0.595*	0.190	0.768***	0.190	0.499***
Offering formal training	0.730	0.306***	0.882	0.137***	0.882	0.163***
N	169		167		164	

Note: This table shows the balancing of firm characteristics in El Salvador when mahalanobis distance is used as a matching metric. Raw columns describe firms before matching happens. Kernel matching columns are for the firms that are matched according to the kernel matching algorithm. 5:1 NN Matching columns are for the firms that are matched according to the 5 to 1 nearest neighbor matching algorithm. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the number of observations. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 5 for more details.

Table 8: Balancing of Covariates after Propensity Score Matching, Georgia

	Raw		Kernel Matching		5:1 NN Matching	
	Support	No-Support	Support	No-Support	Support	No-Support
Firm Size						
Small (0-19)	0.669	0.642	0.685	0.658	0.679	0.636
Medium (20-99)	0.249	0.333	0.262	0.303	0.263	0.335
Large (100+)	0.082	0.025**	0.053	0.039	0.058	0.029
Firm Age						
0-10 years	0.572	0.580	0.562	0.546	0.558	0.520
11-20 years	0.319	0.329	0.322	0.344	0.327	0.351
20+ years	0.109	0.091	0.117	0.111	0.115	0.130
Manufacturing	0.207	0.152	0.173	0.165	0.170	0.174
Construction	0.172	0.170	0.186	0.148	0.183	0.104*
Retail	0.045	0.037	0.048	0.037	0.048	0.051
Year of the top manager's experience	0.292	0.225	0.301	0.297	0.296	0.313
Exporting directly 10% of sales	15.972	15.589	15.876	15.799	15.747	16.422
At least 10% of foreign owned	0.128	0.080	0.125	0.113	0.125	0.108
Product/service or process innovation	0.059	0.052	0.046	0.061	0.054	0.070
Part of a larger firm	0.537	0.426	0.512	0.428	0.506	0.434
Log sales per worker	0.049	0.032	0.048	0.049	0.047	0.056
Majority female owned	9.939	9.591	9.939	9.744	9.932	9.691
Fully credit constrained	0.095	0.180	0.095	0.132	0.102	0.118
Bank loan/line of credit	0.009	0.168***	0.008	0.037	0.010	0.040
Experiencing electrical outages	0.536	0.406	0.546	0.481	0.545	0.515
Offering formal training	0.460	0.501	0.464	0.467	0.469	0.497
N	235		219		217	

Note: This table shows the balancing of firm characteristics in Georgia when propensity score is used as matching metric. Raw columns describe firms before matching happens. Kernel matching columns are for the firms that are matched according to the kernel matching algorithm. 5:1 NN Matching columns are for the firms that are matched according to the 5 to 1 nearest neighbor matching algorithm. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support. N denotes the number of observations. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 5 for more details.

Table 9: Balancing of Covariates after Mahalanobis Distance Matching, Georgia

	Raw		Kernel Matching		5:1 NN Matching	
	Support	No-Support	Support	No-Support	Support	No-Support
Firm Size						
Small (0-19)	0.669	0.642	0.832	0.672***	0.832	0.631***
Medium (20-99)	0.249	0.333	0.141	0.324***	0.141	0.355***
Large (100+)	0.082	0.025**	0.027	0.004	0.027	0.014
Firm Age						
0-10 years	0.572	0.580	0.613	0.651	0.613	0.718*
11-20 years	0.319	0.329	0.318	0.305	0.317	0.210*
20+ years	0.109	0.091	0.069	0.044	0.069	0.072
Manufacturing	0.207	0.152	0.131	0.049**	0.131	0.093
Construction	0.172	0.170	0.246	0.175	0.246	0.246
Retail	0.045	0.037	0.041	0.049	0.041	0.053
Year of the top manager's experience	0.292	0.225	0.281	0.177*	0.281	0.202
Exporting directly 10% of sales	15.972	15.589	16.485	13.960**	16.482	12.984***
At least 10% of foreign owned	0.128	0.080	0.126	0.050**	0.126	0.037**
Product/service or process innovation	0.059	0.052	0.014	0.031	0.015	0.025
Part of a larger firm	0.537	0.426	0.543	0.400**	0.543	0.342***
Log sales per worker	0.049	0.032	0.038	0.009	0.038	0.020
Majority female owned	9.939	9.591	10.151	9.652**	10.150	9.894
Fully credit constrained	0.095	0.180	0.088	0.116	0.088	0.063
Bank loan/line of credit	0.009	0.168***	0.002	0.163***	0.002	0.016
Experiencing electrical outages	0.536	0.406	0.559	0.366***	0.559	0.397**
Offering formal training	0.460	0.501	0.389	0.503*	0.390	0.437
N	235		234		229	

Note: This table shows the balancing of firm characteristics in Georgia when mahalanobis distance is used as a matching metric. Raw columns describe firms before matching happens. Kernel matching columns are for the firms that are matched according to the kernel matching algorithm. 5:1 NN Matching columns are for the firms that are matched according to the 5 to 1 nearest neighbor matching algorithm. Support columns show the respective statistics of firms that have received government support. No-Support columns show the respective statistics of firms that have not received any government support yet. N denotes the number of observations. Asterisks in the No-Support columns are for unconditional t-tests comparing the respective statistic with the same statistic for the firms that received support. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 5 for more details.

Table 10: Average Treatment Effect on the Treated of the Government Support

		El Salvador		Georgia	
		Kernel Matching	5:1 NN Matching	Kernel Matching	5:1 NN Matching
Percentage change of permanent full-time employees since December 2019	Propensity Score	0.094	0.095	0.087	0.041
		(0.128)	(0.127)	(0.068)	(0.059)
	Mahalanobis Distance	0.179*	0.120	0.099	0.141
		(0.095)	(0.089)	(0.074)	(0.104)
Probability of decreasing total number of temporary workers	Propensity Score	0.150	0.107	0.034	0.065
		(0.136)	(0.138)	(0.101)	(0.102)
	Mahalanobis Distance	-0.252	-0.164	0.122	0.070
		(0.267)	(0.295)	(0.110)	(0.118)
Probability of decreasing the total hours of operation per week	Propensity Score	0.069	0.030	0.020	0.057
		(0.158)	(0.160)	(0.092)	(0.103)
	Mahalanobis Distance	0.043	0.268	0.012	0.096
		(0.206)	(0.192)	(0.137)	(0.079)
Probability of anticipating falling in arrears on outstanding liabilities within next 6 months	Propensity Score	0.079	0.038	-0.079	-0.060
		(0.113)	(0.132)	(0.085)	(0.094)
	Mahalanobis Distance	0.246	0.246	0.070	0.047
		(0.198)	(0.194)	(0.084)	(0.094)

Note: This table reports the average treatment effect on the treated of the government support for all outcome variables in both countries. Kernel matching columns are for the firms that are matched according to the kernel matching algorithm. 5:1 NN Matching columns are for the firms that are matched according to the 5 to 1 nearest neighbor matching algorithm. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 5 for more details.

Table 11: Main Panel Regressions, El Salvador

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.148	0.006	-0.174	-0.759	-0.919*	-1.652***	-0.798*	-0.797
	(0.113)	(0.064)	(0.564)	(0.787)	(0.526)	(0.540)	(0.431)	(0.530)
Average change in monthly sales compared to one year ago	-0.160	0.255*	-2.842***	-5.754***	-2.223**	-1.091	-2.591***	-3.228**
	(0.130)	(0.138)	(0.909)	(1.625)	(0.867)	(1.377)	(0.807)	(1.388)
Decreased demand for products or services compar to one year ago	-0.059	0.473***	-0.264	-0.054	1.391***	1.018	-0.606	-0.547
	(0.077)	(0.154)	(0.695)	(1.755)	(0.493)	(0.853)	(0.784)	(0.671)
Decreased supply of inputs compared to one year ago	0.118	-0.326**	2.492**	4.555**	0.414	-0.442	-0.680*	0.240
	(0.090)	(0.151)	(1.063)	(2.114)	(0.440)	(1.027)	(0.359)	(0.493)
Decreased liqdt or cash flow avail. since COVID-19 began	1.156	0.077	-1.487**	-4.727**	-1.178	1.290**	1.050**	0.900
	(0.771)	(0.128)	(0.721)	(1.911)	(0.816)	(0.611)	(0.468)	(0.979)
Overdue on obligations to financial institutions	-0.190	-0.716	-0.183	-1.756**	-0.440	-0.404	1.203*	0.164
	(0.173)	(0.522)	(0.467)	(0.749)	(0.545)	(0.939)	(0.651)	(0.525)
Ever temporarily closed during the COVID-19 outbreak	-0.089	0.069	-0.657	0.011	0.124	-0.508	-0.817***	-0.061
	(0.127)	(0.082)	(0.510)	(0.656)	(0.553)	(0.610)	(0.261)	(0.589)
Adjusting or converting their production or services	0.120	-0.032	0.334	2.015***	-0.121	0.894	-0.192	0.584
	(0.107)	(0.072)	(0.467)	(0.589)	(0.326)	(0.662)	(0.303)	(0.578)
Started or increased online business activity	-0.079	-0.278***	0.222	-1.027*	-0.542*	-0.820**	-0.602	-1.132
	(0.141)	(0.091)	(0.444)	(0.585)	(0.315)	(0.376)	(0.396)	(0.731)
Started or increased delivery of goods, services or carryout	-0.057	0.142**	0.080	0.521	0.343	0.733	-0.219	0.270
	(0.062)	(0.070)	(0.374)	(0.478)	(0.278)	(0.487)	(0.520)	(0.672)

Started or increased remote work	0.208*	0.001	-0.816*	-1.284*	-0.004	-0.053	-0.011	0.016
	(0.114)	(0.096)	(0.444)	(0.750)	(0.588)	(0.620)	(0.279)	(0.444)
Exporting directly (at least 10% of sales)	-0.065	-0.092	-0.286	-0.001	-0.983	-0.552	0.186	1.602**
	(0.080)	(0.099)	(0.541)	(0.782)	(0.637)	(0.786)	(0.716)	(0.628)
Constant	-1.196*	0.139	-2.337***	-2.607	0.624	-0.118	-0.939	-2.493***
	(0.654)	(0.262)	(0.555)	(1.642)	(0.385)	(0.952)	(0.873)	(0.947)
Number of observations	632	262	553	223	643	264	632	261
r2_w	0.328	0.376						
r2_b	0.032	0.007						
r2_o	0.011	0.025						
rho	0.760	0.835						

Notes: This table reports panel regressions with individual firm fixed effects for the El Salvador COV-ES data. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 12: Main Panel Regressions, Georgia

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.034	0.008	-0.090	0.049	0.054	-0.449	-0.473**	-0.197
	(0.147)	(0.111)	(0.309)	(0.416)	(0.364)	(0.338)	(0.235)	(0.248)
Average change in monthly sales compared to one year ago	0.111	0.080	-1.181*	-1.602**	-2.556***	-2.857***	-2.972***	-2.574***
	(0.176)	(0.206)	(0.653)	(0.719)	(0.652)	(0.749)	(0.621)	(0.731)
Decreased demand for products or services compar to one year ago	0.018	0.170*	0.710**	0.739	1.459**	0.924*	1.241*	1.545*
	(0.071)	(0.087)	(0.357)	(0.628)	(0.580)	(0.544)	(0.662)	(0.916)
Decreased supply of inputs compared to one year ago	-0.241**	-0.448***	0.052	0.350	1.135***	1.245***	-0.712	-0.693
	(0.114)	(0.167)	(0.338)	(0.390)	(0.358)	(0.345)	(0.453)	(0.543)
Decreased liqdt or cash flow avail. since COVID-19 began	-0.062	-0.258	-0.170	0.292	-0.598*	-0.525	0.850	0.459
	(0.086)	(0.174)	(0.555)	(0.606)	(0.348)	(0.423)	(0.739)	(0.893)
Overdue on obligations to financial institutions	-0.034	0.118	0.696	0.632	0.134	0.499	1.535***	1.044*
	(0.058)	(0.101)	(0.488)	(0.510)	(0.507)	(0.357)	(0.455)	(0.633)
Ever temporarily closed during the COVID-19 outbreak	-0.567***	-0.410	0.104	-0.302	0.354	0.450	-0.131	-0.097
	(0.086)	(0.251)	(0.359)	(0.344)	(0.306)	(0.274)	(0.440)	(0.558)
Adjusting or converting their production or services	0.119	0.325**	-0.137	-0.600**	0.736**	1.149**	0.335	0.422
	(0.113)	(0.144)	(0.280)	(0.258)	(0.314)	(0.480)	(0.309)	(0.407)
Started or increased online business activity	-0.402	-0.596**	0.202	0.439	-0.629	-0.496	-0.776*	-0.810

	(0.250)	(0.275)	(0.548)	(0.491)	(0.394)	(0.402)	(0.415)	(0.528)
Started or increased delivery of goods, services or carryout	0.260*	0.447	-0.429	-0.417	0.518	0.080	1.410**	1.369**
	(0.146)	(0.281)	(0.335)	(0.424)	(0.483)	(0.490)	(0.587)	(0.628)
Started or increased remote work	0.164**	0.091	0.309	0.312	-0.977***	-1.002***	-0.260	-0.228
	(0.078)	(0.164)	(0.257)	(0.323)	(0.336)	(0.362)	(0.323)	(0.346)
Exporting directly (at least 10% of sales)	0.142	0.250	0.162	0.497	0.274	0.152	0.826	1.079
	(0.101)	(0.164)	(0.443)	(0.523)	(0.577)	(0.662)	(0.750)	(0.791)
Constant	0.279***	0.286	-1.942***	-2.562***	-1.966***	-1.427***	-3.278***	-3.284***
	(0.097)	(0.232)	(0.535)	(0.459)	(0.357)	(0.354)	(0.479)	(0.500)
Number of observations	619	469	630	473	679	522	554	427
r2_w	0.215	0.334						
r2_b	0.111	0.079						
r2_o	0.097	0.077						
rho	0.620	0.677						

Notes: This table reports panel regressions with individual firm fixed effects for the Georgia COV-ES data. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 13: Breakdown by Types of Government Support, El Salvador

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.572	0.192**	-0.189	-0.466	0.472	-1.225	-0.536	-1.259
	(0.415)	(0.088)	(1.216)	(1.004)	(1.089)	(1.076)	(0.860)	(0.959)
Cash transfers	-0.016	0.331***	0.665	0.372	-1.352**	-2.232***	0.165	0.662
	(0.180)	(0.076)	(0.813)	(1.259)	(0.612)	(0.799)	(0.525)	(0.721)
Wage subsidies	-0.504	-0.455***	-0.273	-0.487	-1.297	0.178	-0.397	0.304
	(0.390)	(0.112)	(1.174)	(1.176)	(1.446)	(1.592)	(0.841)	(0.882)
Average change in monthly sales compared to one year ago	-0.079	0.442***	-2.800***	-5.714***	-2.032**	-1.044	-2.586***	-3.274**
	(0.117)	(0.144)	(0.889)	(1.649)	(0.915)	(1.621)	(0.813)	(1.373)
Decreased demand for products or services compar to one year ago	-0.014	0.453***	-0.075	-0.049	1.383***	1.329	-0.596	-0.551
	(0.066)	(0.136)	(0.658)	(1.765)	(0.384)	(1.009)	(0.800)	(0.626)
Decreased supply of inputs compared to one year ago	0.091	-0.325***	2.343**	4.534**	0.588	-0.420	-0.684*	0.197
	(0.093)	(0.117)	(0.990)	(2.102)	(0.397)	(1.012)	(0.365)	(0.499)
Decreased liqdt or cash flow avail. since COVID-19 began	1.021	0.075	-1.494**	-4.635**	-1.030	1.181	1.060**	0.887
	(0.623)	(0.075)	(0.686)	(1.973)	(0.860)	(0.736)	(0.473)	(0.996)
Overdue on obligations to financial institutions	-0.106	-0.704	-0.135	-1.722**	-0.500	-0.765	1.233*	0.218
	(0.138)	(0.493)	(0.463)	(0.776)	(0.532)	(1.066)	(0.659)	(0.506)

Ever temporarily closed during the COVID-19 outbreak	0.026	0.056	-0.636	0.002	0.175	-0.223	-0.824***	-0.151
	(0.113)	(0.080)	(0.485)	(0.658)	(0.609)	(0.647)	(0.259)	(0.633)
Adjusting or converting their production or services	0.092	-0.002	0.329	2.049***	-0.191	0.800	-0.192	0.607
	(0.104)	(0.060)	(0.465)	(0.593)	(0.333)	(0.691)	(0.304)	(0.585)
Started or increased online business activity	-0.093	-0.228**	0.234	-0.947*	-0.532	-1.069***	-0.600	-1.072
	(0.141)	(0.088)	(0.452)	(0.544)	(0.347)	(0.391)	(0.396)	(0.742)
Started or increased delivery of goods, services or carryout	-0.075	0.273***	0.083	0.515	0.209	0.506	-0.221	0.263
	(0.079)	(0.068)	(0.391)	(0.477)	(0.267)	(0.355)	(0.514)	(0.679)
Started or increased remote work	0.219**	-0.041	-0.789*	-1.305*	-0.074	-0.211	-0.003	0.078
	(0.105)	(0.080)	(0.448)	(0.746)	(0.571)	(0.652)	(0.277)	(0.451)
Exporting directly (at least 10% of sales)	-0.087	-0.158***	-0.395	-0.145	-1.025*	-0.234	0.132	1.503**
	(0.108)	(0.049)	(0.576)	(0.606)	(0.608)	(0.792)	(0.728)	(0.594)
Constant	-1.140**	0.224	-2.380***	-2.685	0.614	0.027	-0.954	-2.486**
	(0.552)	(0.236)	(0.540)	(1.745)	(0.401)	(0.820)	(0.887)	(0.980)
Number of observations	632	262	553	223	643	264	632	261
r2_w	0.373	0.491						
r2_b	0.031	0.012						
r2_o	0.009	0.033						
rho	0.751	0.873						

Notes: This table reports panel regressions with individual firm fixed effects for the El Salvador COV-ES data. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 14: Breakdown by Types of Government Support, Georgia

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.045	0.010	-0.208	-0.044	0.230	-0.200	-0.766	-0.559
	(0.174)	(0.134)	(0.422)	(0.501)	(0.596)	(0.553)	(0.621)	(0.616)
Deferral of payments	0.199	0.184	0.656*	0.747*	-0.126	-0.143	0.038	0.126
	(0.185)	(0.163)	(0.373)	(0.391)	(0.532)	(0.499)	(0.781)	(0.728)
Fiscal relief	-0.103	-0.053	-0.532	-0.662	-0.228	-0.324	0.251	0.268
	(0.137)	(0.119)	(0.372)	(0.417)	(0.464)	(0.446)	(0.576)	(0.524)
Wage subsidies	0.070	-0.025	-0.132	-0.255	-0.049	-0.148	0.452	0.538
	(0.177)	(0.136)	(0.438)	(0.513)	(0.430)	(0.404)	(0.496)	(0.517)
Average change in monthly sales compared to one year ago	0.125	0.093	-1.175*	-1.612**	-2.557***	-2.873***	-2.993***	-2.600***
	(0.176)	(0.204)	(0.678)	(0.728)	(0.654)	(0.744)	(0.656)	(0.759)
Decreased demand for products or services compar to one year ago	0.025	0.174**	0.738**	0.740	1.457**	0.905*	1.256*	1.574*
	(0.076)	(0.087)	(0.370)	(0.666)	(0.576)	(0.532)	(0.647)	(0.880)
Decreased supply of inputs compared to one year ago	-0.240**	-0.441***	-0.023	0.213	1.126***	1.235***	-0.735*	-0.733
	(0.110)	(0.162)	(0.352)	(0.415)	(0.338)	(0.325)	(0.409)	(0.482)
Decreased liqdt or cash flow avail. since COVID-19 began	-0.066	-0.272	-0.124	0.485	-0.587*	-0.498	0.820	0.407
	(0.084)	(0.171)	(0.500)	(0.499)	(0.352)	(0.423)	(0.778)	(0.932)
Overdue on obligations to financial institutions	-0.026	0.121	0.600	0.507	0.153	0.563	1.522***	0.993
	(0.066)	(0.111)	(0.450)	(0.444)	(0.540)	(0.368)	(0.460)	(0.606)

Ever temporarily closed during the COVID-19 outbreak	-0.551***	-0.372	0.155	-0.262	0.339	0.430	-0.132	-0.099
	(0.104)	(0.260)	(0.342)	(0.327)	(0.302)	(0.269)	(0.443)	(0.584)
Adjusting or converting their production or services	0.110	0.339**	-0.147	-0.613**	0.739**	1.166**	0.326	0.408
	(0.114)	(0.145)	(0.285)	(0.268)	(0.318)	(0.494)	(0.309)	(0.407)
Started or increased online business activity	-0.393*	-0.590**	0.413	0.849*	-0.611	-0.450	-0.839**	-0.896*
	(0.231)	(0.258)	(0.535)	(0.481)	(0.423)	(0.438)	(0.426)	(0.504)
Started or increased delivery of goods, services or carryout	0.235*	0.425*	-0.578*	-0.691	0.513	0.066	1.425**	1.386**
	(0.127)	(0.248)	(0.347)	(0.479)	(0.493)	(0.509)	(0.598)	(0.649)
Started or increased remote work	0.176**	0.100	0.271	0.212	-0.980***	-1.012***	-0.254	-0.231
	(0.085)	(0.165)	(0.268)	(0.329)	(0.344)	(0.361)	(0.325)	(0.357)
Exporting directly (at least 10% of sales)	0.142	0.246	0.144	0.461	0.267	0.141	0.823	1.080
	(0.097)	(0.159)	(0.434)	(0.515)	(0.573)	(0.661)	(0.743)	(0.750)
Constant	0.234**	0.233	-1.970***	-2.636***	-1.965***	-1.433***	-3.242***	-3.222***
	(0.092)	(0.237)	(0.487)	(0.435)	(0.363)	(0.369)	(0.483)	(0.535)
Number of observations	619	469	630	473	679	522	554	427
r2_w	0.224	0.342						
r2_b	0.091	0.069						
r2_o	0.079	0.065						
rho	0.629	0.679						

Notes: This table reports panel regressions with individual firm fixed effects for the Georgia COV-ES data. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 15: Interaction with Firm Size, El Salvador

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.634	0.187	-0.949	-2.015	-1.636	-2.890	-0.051	1.230
	(0.458)	(0.163)	(1.477)	(1.990)	(1.724)	(2.363)	(1.385)	(1.404)
Support X ln of employment size at ES	-0.180	-0.069	0.291	0.425	0.244	0.407	-0.272	-0.704
	(0.138)	(0.059)	(0.535)	(0.591)	(0.504)	(0.686)	(0.439)	(0.473)
Average change in monthly sales compared to one year ago	-0.156	0.265*	-2.837***	-5.765***	-2.229**	-1.005	-2.616***	-3.342***
	(0.131)	(0.140)	(0.910)	(1.634)	(0.869)	(1.404)	(0.801)	(1.276)
Decreased demand for products or services compar to one year ago	-0.038	0.506***	-0.290	-0.126	1.370***	0.965	-0.586	-0.384
	(0.073)	(0.169)	(0.710)	(1.787)	(0.491)	(0.831)	(0.757)	(0.614)
Decreased supply of inputs compared to one year ago	0.090	-0.354**	2.505**	4.525**	0.455	-0.254	-0.723*	-0.011
	(0.091)	(0.165)	(1.068)	(2.115)	(0.430)	(0.896)	(0.383)	(0.554)
Decreased liqdt or cash flow avail. since COVID-19 began	1.119	0.062	-1.471**	-4.727**	-1.218	1.096*	1.081**	1.091
	(0.736)	(0.130)	(0.729)	(1.923)	(0.789)	(0.561)	(0.487)	(1.071)
Overdue on obligations to financial institutions	-0.194	-0.631	-0.154	-1.672**	-0.372	-0.178	1.169*	-0.050
	(0.172)	(0.473)	(0.470)	(0.761)	(0.590)	(1.160)	(0.668)	(0.620)
Ever temporarily closed during the COVID-19 outbreak	-0.091	0.085	-0.653	-0.036	0.128	-0.471	-0.839***	-0.129
	(0.120)	(0.080)	(0.501)	(0.637)	(0.556)	(0.597)	(0.274)	(0.601)

Adjusting or converting their production or services	0.108	-0.035	0.330	2.029***	-0.111	0.967	-0.196	0.528
	(0.104)	(0.078)	(0.472)	(0.583)	(0.333)	(0.737)	(0.304)	(0.539)
Started or increased online business activity	-0.050	-0.269***	0.231	-0.927*	-0.540*	-0.832**	-0.603	-1.157
	(0.131)	(0.086)	(0.441)	(0.515)	(0.316)	(0.383)	(0.397)	(0.757)
Started or increased delivery of goods, services or carryout	-0.053	0.147**	0.065	0.483	0.338	0.712	-0.208	0.309
	(0.065)	(0.071)	(0.367)	(0.485)	(0.280)	(0.483)	(0.516)	(0.688)
Started or increased remote work	0.228*	-0.031	-0.843*	-1.351*	-0.030	-0.077	0.012	0.050
	(0.120)	(0.103)	(0.453)	(0.734)	(0.616)	(0.654)	(0.291)	(0.445)
Exporting directly (at least 10% of sales)	-0.063	-0.096	-0.326	-0.132	-1.044	-0.718	0.275	1.905***
	(0.075)	(0.100)	(0.517)	(0.667)	(0.671)	(0.850)	(0.734)	(0.711)
Constant	-1.171*	0.127	-2.333***	-2.514	0.642	-0.119	-0.958	-2.544***
	(0.642)	(0.251)	(0.558)	(1.623)	(0.398)	(0.951)	(0.854)	(0.968)
Number of observations	630	262	551	223	641	264	630	261
r2_w	0.349	0.386						
r2_b	0.036	0.003						
r2_o	0.012	0.022						
rho	0.763	0.823						

Notes: This table reports panel regressions with individual firm fixed effects for the El Salvador COV-ES data. Size information used as the interaction terms is taken from the total number of full-time equivalent workers in the baseline ES. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 16: Interaction with Firm Size, Georgia

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.665**	0.504**	-1.037*	-0.934	-0.451	-0.911	-0.633	-0.258
	(0.275)	(0.223)	(0.543)	(0.676)	(1.080)	(1.050)	(0.798)	(0.804)
Support X ln of employment size at ES	-0.196*	-0.153**	0.296**	0.311**	0.175	0.166	0.051	0.018
	(0.100)	(0.066)	(0.132)	(0.138)	(0.272)	(0.269)	(0.253)	(0.253)
Average change in monthly sales compared to one year ago	0.117	0.093	-1.169*	-1.579**	-2.652***	-2.973***	-2.972***	-2.570***
	(0.169)	(0.193)	(0.638)	(0.707)	(0.630)	(0.717)	(0.620)	(0.733)
Decreased demand for products or services compar to one year ago	-0.028	0.090	0.775**	0.855	1.493**	0.961	1.248*	1.546*
	(0.078)	(0.088)	(0.352)	(0.611)	(0.638)	(0.627)	(0.645)	(0.877)
Decreased supply of inputs compared to one year ago	-0.197**	-0.396***	0.018	0.295	1.118***	1.223***	-0.717	-0.697
	(0.094)	(0.150)	(0.334)	(0.378)	(0.378)	(0.377)	(0.441)	(0.528)
Decreased liqdt or cash flow avail. since COVID-19 began	-0.104	-0.307*	-0.142	0.352	-0.504	-0.451	0.847	0.453
	(0.086)	(0.171)	(0.568)	(0.607)	(0.350)	(0.423)	(0.750)	(0.902)
Overdue on obligations to financial institutions	-0.075	0.052	0.713	0.661	0.171	0.525	1.534***	1.044
	(0.052)	(0.100)	(0.506)	(0.550)	(0.514)	(0.357)	(0.455)	(0.636)
Ever temporarily closed during the COVID-19 outbreak	-0.597***	-0.388	0.103	-0.306	0.379	0.467*	-0.132	-0.098
	(0.093)	(0.259)	(0.361)	(0.349)	(0.305)	(0.271)	(0.437)	(0.551)

Adjusting or converting their production or services	0.141	0.336**	-0.101	-0.552**	0.657**	1.047**	0.344	0.432
	(0.115)	(0.136)	(0.287)	(0.260)	(0.255)	(0.418)	(0.311)	(0.417)
Started or increased online business activity	-0.353*	-0.550**	0.144	0.362	-0.686	-0.568	-0.783*	-0.812
	(0.212)	(0.255)	(0.531)	(0.479)	(0.431)	(0.436)	(0.415)	(0.537)
Started or increased delivery of goods, services or carryout	0.264*	0.451*	-0.415	-0.408	0.594	0.173	1.405**	1.364**
	(0.145)	(0.271)	(0.341)	(0.434)	(0.474)	(0.490)	(0.584)	(0.629)
Started or increased remote work	0.135**	0.045	0.276	0.259	-0.958***	-0.961***	-0.265	-0.234
	(0.065)	(0.145)	(0.256)	(0.325)	(0.326)	(0.357)	(0.329)	(0.359)
Exporting directly (at least 10% of sales)	0.156	0.244	0.073	0.387	0.284	0.153	0.808	1.067
	(0.103)	(0.173)	(0.447)	(0.522)	(0.598)	(0.678)	(0.717)	(0.748)
Constant	0.329***	0.352	-1.959***	-2.623***	-2.144***	-1.605***	-3.269***	-3.266***
	(0.112)	(0.227)	(0.551)	(0.475)	(0.339)	(0.358)	(0.490)	(0.513)
Number of observations	618	468	629	472	678	521	553	426
r2_w	0.238	0.352						
r2_b	0.104	0.079						
r2_o	0.096	0.079						
rho	0.640	0.681						

Notes: This table reports panel regressions with individual firm fixed effects for the Georgia COV-ES data. Size information used as the interaction terms is taken from the total number of full-time equivalent workers in the baseline ES. Columns 3 through 8 report marginal effects from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 17: Interaction with Firm Size with by Types of Government Support, El Salvador

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	3.120*	-0.051	0.616	-13.068	1.625	-1.599	2.776	3.353*
	(1.646)	(0.284)	(3.758)	(8.925)	(2.607)	(3.552)	(1.867)	(1.916)
Cash transfers	-0.563	0.022	-3.744	-11.365**	2.333	0.307	2.739	6.171
	(0.664)	(0.395)	(3.490)	(5.619)	(2.946)	(2.675)	(3.004)	(4.099)
Wage subsidies	-2.717	0.204	-0.433	15.365*	-5.292*	-2.565	-4.561**	-4.281*
	(1.663)	(0.378)	(3.734)	(9.232)	(2.835)	(3.576)	(2.101)	(2.381)
Support X ln of employment size at ES	-0.934*	0.073	-0.229	3.399	-0.368	0.150	-1.149*	-1.308***
	(0.503)	(0.105)	(1.157)	(2.451)	(0.779)	(1.115)	(0.617)	(0.497)
Cash transfers X ln of employment size at ES	0.173	0.123	1.558	3.740**	-1.393	-0.981	-1.010	-1.845
	(0.197)	(0.147)	(1.082)	(1.879)	(0.952)	(0.822)	(1.096)	(1.304)
Wage subsidies X ln of employment size at ES	0.804	-0.229*	-0.055	-4.756*	1.430	0.979	1.511**	1.315**
	(0.526)	(0.136)	(1.187)	(2.712)	(0.911)	(1.103)	(0.732)	(0.647)
Average change in monthly sales compared to one year ago	-0.073	0.446***	-2.829***	-5.714***	-1.977**	-0.896	-2.588***	-3.310**
	(0.111)	(0.133)	(0.891)	(1.549)	(0.950)	(1.584)	(0.823)	(1.333)
Decreased demand for products or services compar to one year ago	0.035	0.494***	-0.208	0.064	1.441***	1.362	-0.530	-0.284
	(0.066)	(0.132)	(0.668)	(1.576)	(0.394)	(0.998)	(0.806)	(0.603)
Decreased supply of inputs compared to one year ago	0.043	-0.352***	2.480**	4.377**	0.585	-0.225	-0.770**	-0.031
	(0.103)	(0.119)	(1.038)	(1.983)	(0.401)	(0.873)	(0.386)	(0.595)
Decreased liqdt or cash flow avail. since COVID-19 began	0.811*	0.083	-1.528**	-4.762**	-1.011	1.005	1.161**	1.212
	(0.437)	(0.082)	(0.724)	(1.956)	(0.781)	(0.632)	(0.512)	(1.181)

Overdue on obligations to financial institutions	-0.015	-0.534	-0.089	-1.641**	-0.371	-0.436	1.211*	0.006
	(0.130)	(0.409)	(0.483)	(0.792)	(0.519)	(1.179)	(0.689)	(0.585)
Ever temporarily closed during the COVID-19 outbreak	-0.132	0.115	-0.670	-0.012	0.174	-0.118	-0.855***	-0.209
	(0.173)	(0.075)	(0.496)	(0.654)	(0.554)	(0.621)	(0.284)	(0.619)
Adjusting or converting their production or services	0.040	-0.036	0.297	2.185***	-0.211	0.917	-0.243	0.650
	(0.105)	(0.067)	(0.494)	(0.627)	(0.353)	(0.739)	(0.309)	(0.611)
Started or increased online business activity	-0.024	-0.215***	0.273	-0.961*	-0.555	-1.173***	-0.588	-1.106
	(0.110)	(0.079)	(0.446)	(0.519)	(0.359)	(0.377)	(0.403)	(0.768)
Started or increased delivery of goods, services or carryout	-0.087	0.288***	0.106	0.771*	0.113	0.356	-0.263	0.214
	(0.092)	(0.064)	(0.390)	(0.427)	(0.264)	(0.335)	(0.516)	(0.697)
Started or increased remote work	0.289***	-0.077	-0.807*	-1.281*	-0.089	-0.293	0.034	0.141
	(0.103)	(0.082)	(0.454)	(0.729)	(0.598)	(0.703)	(0.282)	(0.493)
Exporting directly (at least 10% of sales)	-0.104	-0.152***	-0.430	-0.171	-1.052*	-0.462	0.301	1.538***
	(0.152)	(0.050)	(0.493)	(0.592)	(0.613)	(0.892)	(0.726)	(0.559)
Constant	-0.901**	0.120	-2.375***	-2.710	0.647	0.007	-0.996	-2.732***
	(0.399)	(0.195)	(0.556)	(1.845)	(0.424)	(0.851)	(0.873)	(1.030)
Number of observations	630	262	551	223	641	264	630	261
r2_w	0.455	0.531						
r2_b	0.023	0.007						
r2_o	0.003	0.034						
rho	0.775	0.858						

Notes: This table reports panel regressions with individual firm fixed effects for the El Salvador COV-ES data. Size information used as the interaction terms is taken from the total number of full-time equivalent workers in the baseline ES. Columns 3 through 8 report coefficients from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 18: Interaction with Firm Size with by Types of Government Support, Georgia

	Percentage change of perm full-time workers since Dec 2019		Decreased tot number of temp workers		Decreased total hours of operations		Anticipate falling in arrears on outstanding liabilities	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Received national or local government support	0.507	0.237	-2.519**	-2.337*	-1.442	-1.748	-2.288	-2.045
	(0.320)	(0.279)	(1.149)	(1.243)	(1.688)	(1.659)	(1.881)	(1.866)
Deferral of payments	-0.526	-0.228	2.911**	3.233**	0.430	0.052	0.522	0.712
	(0.473)	(0.412)	(1.202)	(1.293)	(1.531)	(1.552)	(1.996)	(1.774)
Fiscal relief	0.828***	0.829***	-0.659	-1.123	0.695	0.751	2.921	3.037*
	(0.263)	(0.214)	(1.126)	(1.473)	(1.167)	(1.061)	(1.871)	(1.666)
Wage subsidies	-0.605*	-0.756**	-0.178	-0.472	1.206	1.098	0.131	0.219
	(0.336)	(0.300)	(1.186)	(1.448)	(1.178)	(1.198)	(1.267)	(1.198)
Support X ln of employment size at ES	-0.147	-0.064	0.756**	0.769**	0.587	0.547	0.505	0.491
	(0.120)	(0.095)	(0.354)	(0.360)	(0.494)	(0.483)	(0.585)	(0.571)
Deferral of payments X ln of employment size at ES	0.220	0.126	-0.727**	-0.806**	-0.216	-0.105	-0.175	-0.203
	(0.152)	(0.117)	(0.356)	(0.371)	(0.402)	(0.421)	(0.560)	(0.502)
Fiscal relief X ln of employment size at ES	-0.282***	-0.273***	-0.029	0.072	-0.322	-0.367	-0.860	-0.876*
	(0.093)	(0.076)	(0.341)	(0.404)	(0.313)	(0.292)	(0.554)	(0.496)
Wage subsidies X ln of employment size at ES	0.217*	0.224**	0.006	0.049	-0.420	-0.411	0.110	0.104
	(0.115)	(0.088)	(0.300)	(0.357)	(0.358)	(0.363)	(0.420)	(0.388)
Average change in monthly sales compared to one year ago	0.165	0.178	-1.164*	-1.620**	-2.674***	-3.036***	-2.990***	-2.599***
	(0.170)	(0.195)	(0.660)	(0.725)	(0.648)	(0.750)	(0.679)	(0.772)

Decreased demand for products or services compar to one year ago	-0.014	0.148	0.865**	0.991	1.516**	0.957	1.380**	1.768**
	(0.091)	(0.091)	(0.373)	(0.642)	(0.641)	(0.634)	(0.613)	(0.744)
Decreased supply of inputs compared to one year ago	-0.166**	-0.340***	-0.104	0.084	1.087***	1.203***	-0.752**	-0.740*
	(0.074)	(0.114)	(0.374)	(0.443)	(0.363)	(0.368)	(0.358)	(0.417)
Decreased liqdt or cash flow avail. since COVID-19 began	-0.072	-0.266	-0.060	0.584	-0.480	-0.434	0.710	0.238
	(0.089)	(0.171)	(0.488)	(0.480)	(0.368)	(0.455)	(0.811)	(0.898)
Overdue on obligations to financial institutions	-0.012	0.188	0.569	0.443	0.193	0.633	1.565***	1.037*
	(0.088)	(0.167)	(0.431)	(0.415)	(0.564)	(0.409)	(0.457)	(0.573)
Ever temporarily closed during the COVID-19 outbreak	-0.605***	-0.483	0.158	-0.287	0.332	0.402	-0.043	0.029
	(0.106)	(0.329)	(0.352)	(0.355)	(0.313)	(0.286)	(0.399)	(0.527)
Adjusting or converting their production or services	0.068	0.267**	-0.116	-0.592**	0.709***	1.152***	0.399	0.494
	(0.106)	(0.134)	(0.308)	(0.301)	(0.240)	(0.407)	(0.298)	(0.380)
Started or increased online business activity	-0.375**	-0.602***	0.343	0.744	-0.692	-0.535	-0.870**	-0.931*
	(0.182)	(0.215)	(0.512)	(0.475)	(0.444)	(0.456)	(0.415)	(0.503)
Started or increased delivery of goods, services or carryout	0.242**	0.418**	-0.531	-0.609	0.611	0.159	1.455**	1.423**
	(0.114)	(0.206)	(0.330)	(0.436)	(0.464)	(0.495)	(0.572)	(0.605)
Started or increased remote work	0.135**	0.047	0.246	0.163	-0.978***	-1.011***	-0.253	-0.225
	(0.062)	(0.144)	(0.265)	(0.324)	(0.337)	(0.358)	(0.322)	(0.360)
Exporting directly (at least 10% of sales)	0.156	0.259	-0.046	0.209	0.216	0.089	0.761	1.046

	(0.097)	(0.158)	(0.470)	(0.550)	(0.622)	(0.718)	(0.646)	(0.639)
Constant	0.298***	0.330	-2.032***	-2.791***	-2.144***	-1.588***	-3.303***	-3.341***
	(0.098)	(0.241)	(0.494)	(0.463)	(0.352)	(0.384)	(0.484)	(0.538)
Number of observations	618	468	629	472	678	521	553	426
r2_w	0.293	0.420						
r2_b	0.079	0.069						
r2_o	0.072	0.064						
rho	0.678	0.718						

Notes: This table reports panel regressions with individual firm fixed effects for the Georgia COV-ES data. Size information used as the interaction terms is taken from the total number of full-time equivalent workers in the baseline ES. Columns 3 through 8 report coefficients from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 19: Breakdown by Sector, El Salvador

	Percentage change of perm full-time workers since Dec 2019				Decreased tot number of temp workers				Decreased total hours of operations				Anticipate falling in arrears on outstanding liabilities			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Received national or local government support	-0.018	0.005	0.004	-0.007	-0.660	-0.529	-0.630	-0.665	-1.776***	-1.667***	-2.204***	-1.568***	-1.002*	-0.778	-0.873*	-0.772
	(0.061)	(0.065)	(0.066)	(0.076)	(0.794)	(0.781)	(0.852)	(0.821)	(0.568)	(0.550)	(0.655)	(0.538)	(0.546)	(0.532)	(0.527)	(0.539)
Support X Food manufacturing	0.328**				-2.248				2.112*				2.570***			
	(0.152)				(1.695)				(1.088)				(0.842)			
Support X Textiles and garments manufacturing		0.408*				-4.164**				0.443				-0.440		
		(0.222)				(1.733)				(0.498)				(0.554)		
Support X Retail, non-food or pharmacy			0.047				-0.635				1.835**				0.266	
			(0.101)				(1.104)				(0.782)				(0.701)	
Support X Retail, food or pharmacy				0.084								-2.734*				
				(0.075)								(1.603)				
Average change in monthly sales compared to one year ago	0.310**	0.234*	0.243*	0.266*	-5.867***	-6.062***	-5.619***	-5.774***	-0.961	-1.088	-1.185	-1.124	-3.160**	-3.234**	-3.220**	-3.218**
	(0.143)	(0.129)	(0.138)	(0.144)	(1.652)	(1.674)	(1.528)	(1.631)	(1.369)	(1.376)	(1.284)	(1.357)	(1.402)	(1.387)	(1.399)	(1.382)
Decreased demand for products or services compar to one year ago	0.459***	0.471***	0.466***	0.476***	-0.084	-0.060	0.036	-0.071	1.039	1.014	1.170	0.998	-0.530	-0.543	-0.551	-0.549
	(0.150)	(0.148)	(0.152)	(0.155)	(1.740)	(1.850)	(1.687)	(1.728)	(0.814)	(0.851)	(0.835)	(0.858)	(0.670)	(0.672)	(0.672)	(0.668)
Decreased supply of inputs compared to one year ago	-0.318**	-0.302**	-0.327**	-0.330**	4.568**	4.691**	4.531**	4.512**	-0.491	-0.439	-0.414	-0.454	0.247	0.241	0.225	0.228
	(0.146)	(0.137)	(0.151)	(0.151)	(2.098)	(2.219)	(2.064)	(2.098)	(1.016)	(1.025)	(0.847)	(1.025)	(0.537)	(0.492)	(0.480)	(0.487)
Decreased liqdt or cash flow avail. since COVID-19 began	0.121	0.079	0.083	0.070	-4.746**	-4.883**	-4.635**	-4.915**	1.467**	1.297**	0.993*	1.050*	0.964	0.893	0.881	0.810
	(0.117)	(0.130)	(0.127)	(0.129)	(1.922)	(1.918)	(1.837)	(1.954)	(0.656)	(0.613)	(0.592)	(0.602)	(1.031)	(0.976)	(0.987)	(0.963)
Overdue on obligations to financial institutions	-0.747	-0.877*	-0.710	-0.717	-1.773**	-1.858**	-1.803**	-1.753**	-0.473	-0.405	-0.132	-0.408	0.136	0.158	0.170	0.163

	(0.524)	(0.525)	(0.525)	(0.523)	(0.755)	(0.782)	(0.760)	(0.742)	(0.934)	(0.940)	(0.937)	(0.929)	(0.517)	(0.525)	(0.527)	(0.523)
Ever temporarily closed during the COVID-19 outbreak	0.070	0.074	0.064	0.066	-0.032	0.079	0.050	0.000	-0.531	-0.518	-0.386	-0.542	-0.063	-0.050	-0.021	-0.060
	(0.078)	(0.088)	(0.083)	(0.083)	(0.656)	(0.660)	(0.642)	(0.630)	(0.619)	(0.615)	(0.589)	(0.612)	(0.582)	(0.590)	(0.548)	(0.585)
Adjusting or converting their production or services	-0.058	-0.019	-0.027	-0.027	2.038***	2.122***	2.038***	1.993***	0.872	0.892	0.821	0.948	0.536	0.589	0.558	0.583
	(0.073)	(0.071)	(0.076)	(0.073)	(0.585)	(0.602)	(0.579)	(0.577)	(0.680)	(0.666)	(0.606)	(0.658)	(0.575)	(0.580)	(0.591)	(0.575)
Started or increased online business activity	-0.300***	-0.302***	-0.286***	-0.274***	-1.009*	-1.202**	-1.045*	-1.075*	-0.916***	-0.820**	-0.995***	-0.956**	-1.262*	-1.136	-1.122	-1.149
	(0.092)	(0.098)	(0.097)	(0.091)	(0.561)	(0.556)	(0.572)	(0.593)	(0.342)	(0.376)	(0.383)	(0.396)	(0.741)	(0.731)	(0.729)	(0.730)
Started or increased delivery of goods, services or carryout	0.167**	0.147**	0.120	0.147*	0.503	0.625	0.586	0.554	0.733	0.740	0.490	0.857*	0.230	0.272	0.259	0.288
	(0.071)	(0.066)	(0.086)	(0.074)	(0.477)	(0.460)	(0.457)	(0.476)	(0.460)	(0.485)	(0.420)	(0.494)	(0.645)	(0.672)	(0.664)	(0.670)
Started or increased remote work	0.025	-0.275	0.003	0.005	-1.328*	-1.333*	-1.252*	-1.279*	0.010	-0.052	-0.195	-0.047	0.144	0.015	-0.012	0.021
	(0.099)	(0.219)	(0.094)	(0.099)	(0.753)	(0.755)	(0.729)	(0.737)	(0.642)	(0.621)	(0.585)	(0.611)	(0.437)	(0.445)	(0.440)	(0.443)
Exporting directly (at least 10% of sales)	-0.132	-0.089	-0.088	-0.092	0.207	-0.028	-0.103	-0.020	-0.804	-0.552	-0.205	-0.556	1.487**	1.603**	1.655***	1.585**
	(0.096)	(0.102)	(0.102)	(0.104)	(0.781)	(0.760)	(0.694)	(0.767)	(0.763)	(0.784)	(0.695)	(0.801)	(0.697)	(0.624)	(0.621)	(0.630)
Constant	0.144	0.338	0.136	0.148	-2.597	-2.776	-2.719*	-2.377	-0.104	-0.115	0.033	0.102	-2.501***	-2.502***	-2.465***	-2.397**
	(0.258)	(0.327)	(0.262)	(0.265)	(1.653)	(1.689)	(1.640)	(1.625)	(0.951)	(0.953)	(0.856)	(0.915)	(0.965)	(0.948)	(0.931)	(0.940)
Number of observations	262	262	262	262	223	223	223	221	264	264	264	264	261	261	261	258
r2_w	0.398	0.405	0.377	0.378												
r2_b	0.008	0.001	0.008	0.008												
r2_o	0.023	0.010	0.026	0.025												
rho	0.850	0.884	0.833	0.835												

Notes: This table reports panel regressions with individual firm fixed effects for the El Salvador COV-ES data. Sector information used as the interaction terms is taken from the baseline ES. Columns 3 through 8 report coefficients from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Table 20: Breakdown by Sector, Georgia

	Percentage change of perm full-time workers since Dec 2019				Decreased tot number of temp workers				Decreased total hours of operations				Anticipate falling in arrears on outstanding liabilities			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Received national or local government support	0.042	-0.023	0.014	0.066	-0.022	0.106	0.080	0.045	-0.519	-0.414	-0.233	-0.593	-0.205	-0.491*	-0.087	-0.020
	(0.121)	(0.116)	(0.111)	(0.111)	(0.416)	(0.440)	(0.449)	(0.388)	(0.345)	(0.341)	(0.311)	(0.361)	(0.253)	(0.258)	(0.255)	(0.315)
Support X Food manufacturing	-0.237				0.662				0.748**				0.100			
	(0.158)				(0.602)				(0.367)				(0.523)			
Support X Hotels and restaurants		0.493**				-0.361				-0.423				2.648***		
		(0.216)				(0.435)				(0.619)				(0.743)		
Support X Retail, non-food or pharmacy			-0.087				-0.272				-1.627***				-0.860	
			(0.360)				(0.526)				(0.574)				(1.223)	
Support X Retail, food or pharmacy				-0.425***				0.028				1.227*				-1.074
				(0.131)				(1.020)				(0.681)				(1.034)
Average change in monthly sales compared to one year ago	0.071	0.083	0.079	0.066	-1.627**	-1.655**	-1.577**	-1.602**	-2.903***	-2.913***	-2.869***	-2.888***	-2.575***	-2.390***	-2.527***	-2.521***
	(0.205)	(0.215)	(0.206)	(0.207)	(0.735)	(0.719)	(0.729)	(0.721)	(0.757)	(0.775)	(0.773)	(0.757)	(0.731)	(0.759)	(0.720)	(0.721)
Decreased demand for products or services compar to one year ago	0.163*	0.227**	0.181**	0.175*	0.724	0.725	0.764	0.737	0.942*	0.919*	1.145*	0.865	1.547*	1.914**	1.673*	1.712*
	(0.085)	(0.097)	(0.091)	(0.090)	(0.638)	(0.622)	(0.624)	(0.647)	(0.549)	(0.549)	(0.588)	(0.595)	(0.918)	(0.855)	(0.970)	(0.894)
Decreased supply of inputs compared to one year ago	-0.456***	-0.456***	-0.453**	-0.454***	0.369	0.361	0.351	0.352	1.249***	1.254***	1.204***	1.317***	-0.695	-0.678	-0.738	-0.761
	(0.169)	(0.159)	(0.174)	(0.164)	(0.387)	(0.387)	(0.390)	(0.419)	(0.341)	(0.352)	(0.352)	(0.383)	(0.542)	(0.526)	(0.515)	(0.583)
Decreased liqdt or cash flow avail. since COVID-19 began	-0.285	-0.244	-0.235*	-0.286*	0.307	0.301	0.297	0.290	-0.543	-0.508	-0.537	-0.627	0.459	0.127	0.475	0.506
	(0.174)	(0.175)	(0.139)	(0.162)	(0.605)	(0.608)	(0.605)	(0.612)	(0.421)	(0.422)	(0.427)	(0.458)	(0.894)	(0.823)	(0.870)	(0.896)
Overdue on obligations to financial institutions	0.133	0.128	0.113	0.166	0.587	0.644	0.667	0.633	0.468	0.493	0.771	0.480	1.042	1.029*	1.186**	1.000
	(0.101)	(0.099)	(0.097)	(0.113)	(0.468)	(0.494)	(0.520)	(0.497)	(0.352)	(0.354)	(0.481)	(0.350)	(0.640)	(0.565)	(0.543)	(0.638)
Ever temporarily closed during the COVID-19 outbreak	-0.347	-0.408	-0.415*	-0.431*	-0.276	-0.287	-0.293	-0.299	0.482*	0.467	0.492*	0.557*	-0.095	-0.163	-0.070	-0.172
	(0.271)	(0.258)	(0.250)	(0.244)	(0.346)	(0.348)	(0.356)	(0.381)	(0.272)	(0.293)	(0.264)	(0.296)	(0.562)	(0.530)	(0.574)	(0.545)
Adjusting or converting their production or services	0.331**	0.279*	0.322**	0.335**	-0.614**	-0.607**	-0.594**	-0.599**	1.141**	1.169**	1.266**	1.176**	0.422	0.332	0.449	0.356
	(0.145)	(0.144)	(0.141)	(0.144)	(0.269)	(0.259)	(0.261)	(0.272)	(0.474)	(0.492)	(0.505)	(0.491)	(0.407)	(0.373)	(0.400)	(0.383)

Started or increased online business activity	-0.604**	-0.634**	-0.599**	-0.648**	0.529	0.408	0.441	0.441	-0.459	-0.524	-0.552	-0.433	-0.802	-0.777	-0.814	-0.889*
	(0.276)	(0.272)	(0.276)	(0.269)	(0.482)	(0.493)	(0.485)	(0.480)	(0.405)	(0.404)	(0.394)	(0.409)	(0.546)	(0.508)	(0.521)	(0.533)
Started or increased delivery of goods, services or carryout	0.435	0.371	0.454	0.523*	-0.466	-0.387	-0.402	-0.419	0.092	0.115	0.242	0.005	1.368**	1.339**	1.442**	1.449**
	(0.285)	(0.249)	(0.280)	(0.271)	(0.431)	(0.413)	(0.426)	(0.407)	(0.494)	(0.505)	(0.492)	(0.498)	(0.629)	(0.624)	(0.686)	(0.584)
Started or increased remote work	0.084	0.125	0.090	0.108	0.294	0.303	0.326	0.313	-1.034***	-1.037***	-1.009***	-0.984***	-0.229	-0.134	-0.176	-0.275
	(0.161)	(0.171)	(0.163)	(0.160)	(0.320)	(0.322)	(0.332)	(0.352)	(0.355)	(0.370)	(0.372)	(0.343)	(0.345)	(0.340)	(0.359)	(0.358)
Exporting directly (at least 10% of sales)	0.259	0.289*	0.247	0.238	0.469	0.508	0.509	0.499	0.110	0.174	0.164	0.220	1.073	0.777	1.080	1.007
	(0.160)	(0.162)	(0.162)	(0.160)	(0.525)	(0.531)	(0.522)	(0.503)	(0.659)	(0.695)	(0.670)	(0.696)	(0.804)	(0.588)	(0.743)	(0.765)
Constant	0.278	0.251	0.271	0.293	-2.598***	-2.600***	-2.596***	-2.562***	-1.444***	-1.456***	-1.641***	-1.439***	-3.285***	-3.176***	-3.436***	-3.283***
	(0.239)	(0.234)	(0.215)	(0.218)	(0.464)	(0.476)	(0.491)	(0.459)	(0.353)	(0.367)	(0.384)	(0.362)	(0.498)	(0.463)	(0.465)	(0.508)
Number of observations	469	469	469	469	473	473	473	473	522	522	522	522	427	427	427	427
r2_w	0.340	0.358	0.335	0.355												
r2_b	0.066	0.035	0.077	0.074												
r2_o	0.066	0.038	0.074	0.073												
rho	0.669	0.697	0.674	0.701												

Notes: This table reports panel regressions with individual firm fixed effects for the Georgia COV-ES data. Sector information used as the interaction terms is taken from the baseline ES. Columns 3 through 8 report coefficients from logistic regressions. Sampling weights from the ES are used. Standard errors are shown in parenthesis. All standard errors are clustered at survey strata, and *, **, *** denotes the significance level at 10%, 5%, and 1%, respectively. See Section 6 for more details.

Appendix A.1. Government Support Policies in El Salvador

This Appendix compiles the list of support that the Government of El Salvador provided to the private sector during the COVID-19 crisis as of January 15, 2021. To create this list, we used the Map of SME-Support Measures in Response to COVID-19, and supplemented through sources as provided below. Colleagues at the World Bank's office in El Salvador kindly verified and confirmed the support measures, for which we are grateful.

Salvadoran Company Recovery Trust (FIREMPRESA)

- Formed as of October 15, 2020, and administered by administered by the Development Bank of El Salvador (BANDESAL) (source). Total of 600 mln. USD
- As of March 18, 2021, the government has expanded the coverage, to include informal sectors of the economy such as tourist transport, taxi drivers, artists , artisans, fishermen, among others, and also merchants of the Santa Ana market affected by the fire. (source, source)
- **Access to new credit:**
    - For micro, small and medium-sized companies (MSMEs) affected by the COVID-19 pandemic, max. 99 employees and max. 7 million USD in profits per year
    - No collateral is needed, there is one year of full grace - no interest, no fees - and 3% annual interest
    - Total budget for this component: 360 mln. USD (source)
    - Also available for informal firms (as a "third pillar of FIREMPRESA"), loans for working capital, with total budget for informal businesses 100 mln. USD
- **Wage subsidies:**
    - Subsidize 50% of the payroll, aimed at MSMEs registered as formal companies
        - To access, the company needs to show that it has been affected by the pandemic, that it continues to operate, that it has registered with the Salvadoran Social Security Institute (ISSS)
    - Total budget for this component: 140 mln. USD

Additional wage subsidies

- Support to employers for the payment of contribution fees to SAP.

Fiscal relief

- Fiscal incentives for investment in new productive projects, creation of angel capital funds and co-investment.

Deferral of payments

- Mid-March 2020: Waiver for utility bills payments for individuals and legal entities directly affected by curfew enacted because of COVID-19 for three months. Utilities include electricity, water, telephone, cable, and internet.
- As of October 15, 2020, users of financial services can defer payments (source)

Other support

- Simplification of procedures for MSME account

Support of individuals and families

- Mid-March 2020: The government pledged to give $300 to around 1.5 million households who work in the informal economy (source).
- Mid-March 2020: Suspension of utility, phone and internet bills for three months, to be paid back over the course of the subsequent two years. (source)
- Freezing payments on items such as mortgages, cars, motorcycles and credit cards.
- Food package deliveries.
- As of January 2021, access to subsidized new mortgage loans for individuals, to support construction sector. (source)

Appendix A.2. Government Support Policies in Georgia

This Appendix compiles the list of support that the Government of Georgia provided to the private sector during the COVID-19 crisis as of January 15, 2021. Similarly to the list for El Salvador, we started with the Map of SME-Support Measures in Response to COVID-19, and supplemented through searches about the government policies through official websites of the Government of Georgia (mainly https://stopcov.ge/, sources mentioned therein, and this report prepared by the Government of Georgia).

Income tax break:

- Exemption from income tax of up to 750 GEL for persons with salaries up to 1500 GEL
- From May 1, 2020 to June 1, 2021

Income tax break for the tourism sector:

- The 4-month income tax for 2020 was first postponed from April 15 to November 1, then until December 1, and finally written off.

Property tax breaks for the tourism sector:

- Hotels and restaurants are exempt from property tax:
    - The 4-month tax for 2020 was first postponed from April 15 to November 1, then until December 1, and finally written off.
    - During 2021

Subsidizing interest on hotel bank loans:

- Subsidize 80% of bank loan interest for small hotels
    - Small defined as with annual turnover of 20 mln. GEL or less, with a minimum of 4 rooms (to restrict to hotels, and exclude those renting out parts of houses)
- Period: from May 1, 2020 to June 1, 2021

Subsidizing interest on restaurant bank loans:

- Subsidizing 80% of the bank loan interest
- Period: from December 1, 2020 to June 1, 2021

Micro Grants Program:

- Financing new business projects
- Period: from December 1, 2020 to June 1, 2021
- Source: http://www.enterprisegeorgia.gov.ge/ka

Credit-guarantee fund:

- Assistance in restructuring existing loans or raising new funds
- Period: from December 1, 2020 to June 1, 2021
- Source is here.

Possibility of deferring bank loans:

- Companies and their employees who have their activities restricted from May 1, 2020 to February 1, 2021

Subsidizing utility costs

- All those households that consume 200 kWh and up to 200 cubic m. of natural gas
- Electricity, gas, cleaning and water bills are subsidized
- Period: March, April, May, November, December, January 2021, February 2021

Assistance for the unemployed:

- Those employed in the formal sector who have temporarily lost their jobs
- 200 GEL assistance for 6 months (does not exceed 1200 GEL)

300 GEL one-time assistance:

- To the self-employed
- Persons employed in those facilities and ind. entrepreneurs whose activities will be suspended as a result of additional restrictions imposed in December and January

65,000-100,000 Social Score Families:

- 100 GEL assistance for 6 months (does not exceed 600 GEL)

0-100,000 Social Score Families with children aged 3 or over:

- 100 GEL assistance for 6 months (does not exceed 600 GEL)

Persons with disabilities under 18 and severely disabled:

- 100 GEL assistance for 6 months (does not exceed 600 GEL)

Each child under the age of 17:

- 200 GEL

Students with up to 150,000 social scores:

- Funding for tuition fees for the fall semester of the 2020 academic year

Students with up to 150,000 social scores, suspended due to non-payment of tuition fees:

- 1 year tuition fee (spring semester of 2019-2020 academic year and autumn semester of 2020 academic year)