Information Frictions and Firm Take-Up of Government Support: A Randomised Controlled Experiment

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Abstract

This paper studies whether informational frictions prevent firms from accessing government support using a randomised controlled trial. We focus on two Portuguese COVID-19 relief programs, providing (i) wage support for workers who are kept on payroll and (ii) credit lines backed by government guarantees. We randomly assign firms to a treatment providing either information about a program, or a combination of information and step-by-step application guidance. We find a significant treatment effect on take-up of the wage support program, and a higher probability of survival in the years following the intervention. Our results constitute direct evidence that information frictions act as a barrier to comprehensive distribution of firm-level support measures.

Keywords: Take-up of government programs, COVID-19, SMEs, Information

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1 Introduction

Governments around the world devote substantial resources to support small and medium sized firms struggling with the consequences of economic and financial crises.[1] For example, nearly 800 billion dollars in loans were approved through the US's Paycheck Protection Program in the year following the COVID-19 pandemic.[2] In the UK, post-pandemic, there were more than 60 different programs designed to provide funding to small and medium sized firms. Despite the scale of these programs, there is substantial concern over their effectiveness in reaching targeted firms (Granja et al., 2022, The Economist, 2020).[3] A key question is whether the firms that stand to benefit most from government programs—for example, smaller firms or those with limited access to traditional financing—face information frictions that hamper access to aid.

In this paper, we test whether informational frictions prevent firms from receiving government support using an encouragement-based randomised controlled trial. We study the impact of providing information on two COVID-19 assistance measures—a layoff support program and a guaranteed credit line scheme—using a sample of over 100,000 Portuguese firms. We randomly assigned firms to a treatment presenting simplified information on one of the two assistance measures. This treatment notified (or reminded) firms about the existence of the measure, and provided detailed information on program benefits, eligibility criteria, and a link to official government resources. We view Portugal as a suitable laboratory in which to consider the policy challenges faced by developed economies with well-established and formal small business sectors. Data availability for private firms, including contact and financial information, made it possible to implement our experiment and conduct a detailed analysis of treatment effects.

We find evidence that our low-cost intervention, which consisted of targeted emails to firm representatives, had a meaningful impact on program application and subsequent firm survival. However, this effect is not homogeneous across the assistance measures we study. Firms supplied with information about the layoff support program, which had a simple and direct application process operating through a widely used social security website, were significantly more likely to apply to the program and to survive for at least 2 years when compared to a control group. Conversely, firms supplied with equivalent information about the credit guarantee scheme, a more complex measure that required and incentivised banks to act as intermediaries

[1] Government support programs for firms take multiple forms, including subsidized loans, outright grants, faster payments on public procurement projects, loan guarantees, and debt restructuring. See Beck et al. (2010), Lelarge et al. (2010), Banerjee and Duflo (2014), Gozzi and Schmukler (2016), Barrot and Nanda (2016) among others.

[2] These figures are as of May 2021. See `www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program/ppp-data`.

[3] For example, in the US context during the COVID-19 pandemic, one report wrote: *"While the SBA has approved nearly 4 million loans since it was launched April 3, businesses point to a myriad of challenges in the PPP's rollout: technical glitches, an avalanche of requests, confusing guidance and a temporary exhaustion of money. The program also has been criticized for enabling scores of publicly traded companies, such as restaurant chains and hotel groups, to receive loans thanks to a controversial provision benefiting the hospitality industry."* (USA Today, May 7 2020). This concern is also supported by the earliest academic research on the crisis. For example, Humphries et al. (2020) shows that smaller businesses were less aware of and less likely to apply for the Paycheck Protection Program, a key aspect of the US response. See also Zia (2008) and Lelarge et al. (2010).

(and was subject to an array of sector-specific features and requirements), did not apply at higher rates.

Specifically, we estimate that firms provided with information about the layoff support measure were 2-4 percentage points more likely to report applying to the program (relative to a mean of just over 32 percent in the control group). This increase in applications peaked in the month immediately following our intervention. We see no evidence of cross-program effects: firms provided information on the layoff support measure were not more likely to apply to the credit guarantee program, and vice versa, suggesting that the intervention did not simply act as a reminder that government support programs were available in general. Similarly, there is no evidence of an incremental effect of providing supplemental step-by-step application guidance, which was designed to help firms overcome bureaucratic or administrative barriers, on top of the informational treatment.

To address potential concerns about misreporting and attrition, we verify our main results using data from an independent survey conducted by the World Bank – the World Bank Enterprise Survey Follow-Up on COVID-19 (The World Bank, 2020). This survey includes questions about receipt of various support programs. Using this independent data source, we find consistent results: a positive treatment effect on program receipt for the layoff support measure but not for the credit guarantee.

The increase in program take-up appears to have helped firms survive the pandemic. Treated firms (those exposed to information on the layoff support program) were 1 percentage point more likely to survive for at least 2 years post-intervention. The effect is more pronounced in the subsample of firms that reported applying to the layoff measure, indicating that the layoff support program itself increased survivorship. This is consistent with the primary goal of the assistance measures, which was to support the business sector through the pandemic outbreak and to provide firms with enough liquidity to withstand any associated negative shocks. The impact on firm survival that we estimate suggests that simple informational interventions can have real economic consequences.

Overall, our results indicate that information frictions act as a meaningful barrier to the distribution of firm-level government support measures. This echoes a deep literature highlighting the consequences of complexity and informational barriers for take-up of individual-level social programs (see, e.g. Currie et al., 2001, Heckman and Smith, 2004, Currie, 2006, Bettinger et al., 2012, Finkelstein and Notowidigdo, 2019). While research on the role of information in the take-up of firm-level programs is less established, our findings are in line with recent survey evidence suggesting that small businesses identify information frictions and uncertainty about eligibility as potential barriers to accessing government aid (Humphries et al., 2020, Bartik et al., 2020).

The presence of information frictions is not inherently at odds with optimal policy. In principle, complexity and informational barriers may act as useful screens to improve targeting efficiency (Besley and Coate, 1992, Kleven and Kopczuk, 2011). However, allocative benefits are less likely in a world with heterogeneity in access to information or additional behavioral frictions. Indeed, when this is the case, informational barriers may wind up discouraging exactly those who would benefit most (Mullainathan and Shafir, 2013). Past work on the distribution of firm subsidies bears this out. For example, Zia (2008) shows that nearly half of loans in a subsidized export credit scheme in Pakistan went to financially unconstrained firms. Humphries et al. (2020) show that the forgivable loans granted through the Paycheck Protection Program in the US were skewed towards larger firms due to information frictions.

Heterogeneity analysis indicates that information frictions inhibited efficient targeting of aid in the Portuguese context. Our intervention had a greater impact on the types of firms that we expect either faced greater frictions or stood to benefit most from enrolling. In particular, our analysis suggests that the effects of information provision were concentrated in smaller firms, firms located in non-urban areas, and those with a large number of employees relative to assets. Complementary evidence using World Bank survey data further supports this conclusion, showing that family firms, firms that had to furlough employees, and firms facing major obstacles to accessing finance experienced more pronounced treatment effects. Smaller, non-urban, and family-owned firms are likely to face greater information frictions because, for example, they may be less sophisticated, lack resources to devote to researching support programs, or have a higher cost of information acquisition. Firms that furloughed employees, had a high labor share, or had difficulty accessing finance were most likely to benefit from the layoff support program, which provided direct access to cash and was explicitly aimed at subsidizing labor costs.

To further assist in the interpretation of our main results, we supplement our experimental design with survey evidence on firms' awareness of government support measures and perceived application costs. Our survey allows us to gauge baseline knowledge of the measures we study. While most respondents reported some level of awareness of the two measures, few rated their knowledge level as high or very high. Furthermore, the majority of respondents classified the application process as moderate to very difficult and rated the information provided by the government as not informative to only somewhat informative. This suggests that our treatment was likely not firms' first exposure to the assistance measures, but that most lacked a comprehensive understanding of the programs.

There are several mechanisms through which the estimated effect may operate, and we are largely agnostic about the specific channel. Perhaps the simplest possibility is that our treatment provided a reminder or nudge to apply without conveying any deeper understanding of the program itself. An alternative is that treated firms received meaningful additional information about program benefits or eligibility requirements and that this encouraged application. While our experiment cannot cleanly separate these two, heterogeneity in treatment effects and our survey evidence are both consistent with new information being delivered to firms by the treatment. This suggests that procrastination and inertia (Banerjee and Mullainathan, 2008, DellaVigna and Gentzkow, 2019) are not the sole mechanisms preventing effective targeting. Finally, it is possible that specific features of our intervention, e.g., the targeted nature of the emails or the fact that they were provided by an academic institution, are the source of the impact. For example, it could be that there is a lack of trust in government communications or some stigma discouraging take-up that is alleviated through information provided by an (arguably) objective body. Ultimately, regardless of the mechanism, the basic takeaway is that a low-cost email-based information campaign can have non-trivial impacts on firm take-up and real outcomes.

An important feature of our results is the fact that we find differential effects across the two measures we study. Despite the significant positive effect of information provision for the layoff support program, we find no effect for the credit guarantee scheme. While our experiment was not designed to shed light on the mechanisms explaining this difference, we hypothesize that a key aspect of the implementation of the credit guarantee scheme may be responsible. Specifically, the fact that banks were required to act as intermediaries between firms and the relevant government agency (and stood to benefit from intermediating). This provided banks an incentive to target and alleviate information frictions for eligible firms. Indeed, anecdotal evidence suggests that banks actively contacted and encouraged qualifying firms to apply. Furthermore, using data from the World Bank Enterprise Survey, we show that, on average, firms with existing bank relationships were more likely to apply to the credit line program but not more likely to apply to the layoff program, consistent with banks playing an active role. If our hypothesis is true, the experimental treatment may have been redundant to the information provided by banks about the credit line program. This suggests that providing market incentives to relatively sophisticated intermediaries may also help overcome information barriers and improve the allocation of aid.

We contribute to the growing experimental literature on information provision (see Capozza et al. (2021) and Haaland et al. (2023) for an extensive review). Existing studies have primarily focused on the decisions of individuals, such as employees or customers, across different domains.[4] Fewer papers focus on firms and the impact of information provision on firm choices, and many of those that have report limited or mixed effects on firm outcomes. Duflo et al. (2011) find no effect of reminding farmers to use fertilizer in their crops, while Hanna et al. (2014) find that exposing farmers to experimental data does not induce learning (although the latter document behavioral changes once farmers are presented with summaries that

[4]Some of these domains include public economics (e.g., Chetty and Saez (2013), Bott et al. (2020)), political economy (e.g., Lergetporer et al. (2018), Acemoglu et al. (2020), Carvalho et al. (2023)), macroeconomics (e.g., Coibion et al. (2020), Roth and Wohlfart (2020)), development economics (e.g., Afridi et al. (2020), Armand et al. (2020)), household finance (e.g., Beshears et al. (2015), Laudenbach et al. (2021), Berwart et al. (2024)), labor (e.g., Altmann et al. (2018), Abebe et al. (2021)), education (e.g., Lergetporer et al. (2018), Greaves et al. (2023)), and health (e.g., Carneiro et al. (2021), Barari et al. (2020)). Other papers such as Binder (2020), Akesson et al. (2022), Settele and Shupe (2022), and Bursztyn et al. (2023) conducted information experiments to study households' expectations, beliefs and behaviors in the context of the COVID-19 pandemic.

highlight relationships in the data). Breinlich et al. (2017) find negligible effects of providing information about the benefits and costs of exporting on export behaviour in a sample of UK exporters. In ongoing work, Bernstein et al. (2023) randomly provide US small businesses with short educational videos about the bankruptcy process and find impacts on stigma and knowledge of the bankruptcy process, but no impact on bankruptcy filings. Alternatively, Gertler et al. (2022) find that reminders significantly increase take-up of an experimentally induced low-fee option on a payment processing platform. Similarly, Coibion et al. (2020) find that information on inflation causes firms to adjust prices, employment, and capital, and Bergolo et al. (2023) and Doerrenberg and Schmitz (2015) find impacts on tax compliance. In line with our work, Gupta et al. (2023) use a quasi-experimental approach to show that access to mobile phone coverage reduces information frictions and increases take-up of government-subsidized agricultural credit. Our main contribution is to demonstrate, using experimental evidence, that information provision alleviates frictions constraining firms from accessing government assistance programs.

Our work also relates to the literature on firm responses to economic shocks, particularly the shock induced by the COVID-19 pandemic. Barrero et al. (2021) document stylized facts about the impact of the pandemic on reallocation across firms and on employment dynamics in the US. Building on a private sector database for the US economy, Chetty et al. (2024) report high-frequency activity indicators and evaluate the effects of fiscal stimulus policies. Gourinchas et al. (2024) find that absent government support, SME failure rates would have increased by 6 percentage points in a sample of 11 European countries. Core and De Marco (2023) find that Italian firms with less cash on hand and with higher leverage exhibit higher take-up rates in the context of COVID-19 government measures. It has been shown that firms with managers of diverse cultural backgrounds (Bedendo et al., 2023) and different levels of debt aversion resort to different forms of government aid (Paaso et al., 2022). Our research provides evidence that information frictions can be a barrier to the effective allocation of government aid.

The remainder of the paper is structured as follows. In section 2 we provide more detail about Portuguese COVID-19 relief programs for firms. In section 3 we describe the experimental design and implementation. In section 4 we describe our data and provide summary statistics. In section 5 we present our results. In section 6 we discuss our findings, and in section 7 we conclude.

2 The Portuguese COVID-19 Program for Businesses

The COVID-19 pandemic suddenly and severely impacted the business sector worldwide, and the experience in Portugal mirrored other developed economies. A survey carried out by Bank of Portugal in the second week of April 2020 (roughly the time of our intervention) showed that 80% of respondent firms had experienced a decrease in revenues and that 18% had permanently or temporarily shut down (Banco de Portugal, 2020b).

On March 26th, 2020 the Portuguese government approved a set of policies aimed at supporting the business sector in anticipation of the negative economic consequences of the COVID-19 outbreak.[5] According to the government, the primary goal of these policies was to save jobs and alleviate negative liquidity shocks (Comunicado do Conselho de Ministros, 2020). Our analysis focuses on two of the most prominent policies, a temporary layoff support program and a government guaranteed credit line scheme.[6]

The layoff support program allowed firms facing full or partial shutdown or a significant decrease in revenues to temporarily dismiss employees. Under the program, each employee received two-thirds of their salary, with a minimum of EUR 635 and a maximum of EUR 1,905. The employer was responsible for paying 30% of that amount while the national social security system covered the remaining 70%. A firm was eligible (regardless of size), as long as it (i) had experienced a reduction in revenue of 40% or more,[7] (ii) belonged to a non-priority sector that received a mandatory cessation order from the government, or (iii) was subject to supply chain disruptions or order cancellations (Decree Law 10-G/2020, 2020). Applications were submitted via an online social security portal and automatically accepted for qualifying firms. Benefits began the day following application and firms were not permitted to fire any employees while receiving support or in the subsequent two months. The program was similar in spirit to the *Paycheck Protection Program* in the US, the *Coronavirus Job Retention Scheme* in the UK, the *Kurzarbeit* in Germany, and related policies around the world.

The credit line scheme provided lenders with a government-backed guarantee of (at minimum) 80% of the value of each loan contracted through the program. Firms could borrow a maximum of EUR 1.5 million, remaining liable for 100% of loan balances. The stated purpose of the program was to finance working capital and the general liquidity needs of firms. Loans to finance working capital had a maximum maturity of 4 years and loans to finance liquidity needs had a maximum maturity of 3 years. Interest rates were contracted with the bank and could be fixed or variable, with a maximum spread of 1.5% over the reference rate for loans with a maturity of over 3 years. Capital and interest payments did not start until 1 year after loan origination. The program targeted both large and small firms and required participants to have a minimum credit rating of B-, positive book equity, and no unresolved irregularities with Portuguese Banks, the Portuguese Tax Authority, or the Social Security system. This policy contained sector-specific endowments and features, such as the size of the guarantee, which could go up to 90% in some cases.

Unlike the layoff measure, which could be submitted and accepted automatically through an online portal,

[5] At the time, Portugal had around 3,500 confirmed cases of COVID-19 and had imposed strong lockdown measures.

[6] There were two other support measures for firms and households announced contemporaneously: a tax and social security contributions deferral, which postponed the payment of taxes and contributions by firms and self-employed workers, and a moratorium on existing bank-credit liabilities.

[7] Validated by the firm's certified accountant.

the credit line measure required prospective applicants to individually arrange terms with a Portuguese bank and was ultimately subject to banks' screening and approval. Banks thus had substantial control over the application process. The bank performed any credit screening and monitoring functions and negotiated interest rates and other commercial fees. Upon review of all conditions, the bank then submitted paperwork to the government society in charge of the guarantees ("Sociedade de Garantia Mútua") for approval on behalf of the firm. The Portuguese government-backed guarantee was similar to those implemented in other European countries (for example, Germany, Spain, Sweden, and the UK).

3 Research Design

Our encouragement-based intervention provided information about firm-level government policies implemented during the COVID-19 crisis.[8] We randomly exposed managers of potentially eligible firms to information about either the layoff support program or the guaranteed credit line scheme. To benefit from these two measures, firms were required to formally apply either through a government social security website (layoff) or directly with commercial banks (credit line).

One-third of the sample was randomly assigned to receive information about the layoff program (the *layoff treatment*) and one-third was assigned to receive information about the credit line scheme (the *credit line treatment*). The remaining one-third was assigned to a control group. Within each treatment arm, we offered two tiers, with half of the firms assigned to each.[9] The first tier was an informational summary of the policy, including a simplified description of benefits, eligibility conditions, and links to official government resources. The second tier additionally provided a step-by-step application guide, walking potential applicants through the key steps of the process to test whether bureaucratic hurdles were at play. Appendix Figure A1 shows the content presented for each tier of the intervention for both the layoff support program and the credit line scheme. This information was prepared by the researchers based on official government sources and presented in Portuguese. We performed a stratified randomization at the three-digit industry level of the Portuguese *Código de Actividade Económica.*

Alongside the informational treatment, one-fifth of all firms were randomly sent a baseline survey.[10] The survey collected data on awareness about government policies, as well as on current and intended application status, i.e., whether firms had already applied to any measure or planned to do so. For the treatment group, the survey was presented before the intervention.

[8] We registered the experiment at the AEA RCT Registry (`https://www.socialscienceregistry.org/trials/5647`). The experiment was submitted to and approved by the ethics committee at the Nova School of Business and Economics.

[9] As a consequence, one-sixth of the sample was assigned to each assistance program $\times$ tier, with one-third of the sample assigned to the control group.

[10] We did not send the survey to the whole sample to avoid a potential priming effect.

We implemented our experiment via email using Qualtrics. The emails were addressed to the owner or the business manager and were sent with the subject "Simplified Information about Government Measures - COVID-19." The body of the email explained that a team of academic researchers had compiled simplified information about one of the government measures that might be of interest to the firm. The information treatment (or treatment and survey) was accessible through an individual traceable url link embedded in the body of the email. As the email did not mention a specific measure (e.g., layoff), we expect opening the link to be orthogonal to the initial treatment assignment. The control group (except the sub-sample randomly assigned to receive the baseline survey) did not receive any communication.

The intervention took place on the same day of the week (Wednesday) and at the same time of the day (10 a.m.) on two consecutive weeks due to system capacity constraints. The intervention for recipients of the layoff treatment was delivered on April 8th, 2020, while the intervention for recipients of the credit line treatment was sent a week later, on April 15th, 2020. The surveys targeting a sub-sample of the control group were equally spread across the two dates.

We conducted a follow-up survey of all firms in our sample in the last two weeks of September 2020, approximately 6 months after the initial intervention. The primary aim of the follow-up survey was to collect information on the outcome of interest: whether and in which month firms applied to the layoff support program or credit line scheme.[11] The survey made no reference to the intervention conducted in April and was also delivered via email using Qualtrics. We sent a single reminder to firms that did not respond to the follow-up survey in the first two weeks of October.

Given our experimental design, our most basic specification is a linear probability model that tests for an effect of either the layoff treatment or the credit line treatment on take-up of support measure m:

$$Y_i^m = \alpha + \beta_{\text{Layoff}} \times \text{Treatment}_i^{\text{Layoff}} + \beta_{\text{Credit Line}} \times \text{Treatment}_i^{\text{Credit Line}} + \varepsilon_i^m. \quad (1)$$

Here, Y_i^m is a binary variable equal to one if firm i reported applying to measure $m \in \{\text{layoff}, \text{credit line}\}$ in the follow-up survey. The variable Treatment_i^m takes the value of one if the firm was assigned to receive information about measure m. For robustness, we also show specifications in which we include pre-treatment firm characteristics (the logarithm of total assets, firm age and leverage as of 2019) and one-digit industry fixed effects. The key coefficient of interest when considering outcome Y_i^m is β_m, the impact of information about m on the probability of applying for assistance measure m.[12] We cluster standard errors at the

[11] Whether the government would disclose this information was not clear ex-ante. The Portuguese government often provides information on recipients of specific government programs (e.g., the PME Lider program which gives recipients access to government loan guarantees (Bonfim et al., 2023)) or on entities that are awarded public contracts. However, this was not the case for these specific programs.

[12] We use an identical specification when examining the treatment effect on firm survival.

three-digit industry level to match the level of stratification.

To test for any incremental impact of the step-by-step application guide, we also examine a supplemental version of Equation 1 that allows for differential effects by treatment tier. In particular, we consider regressions of the form:

$$\begin{aligned} Y_i^m = \alpha &+ \beta_1 \times \text{Treatment}_i^{\text{Layoff}} + \beta_2 \times \text{Step-by-Step}_i^{\text{Layoff}} \\ &+ \beta_3 \times \text{Treatment}_i^{\text{Credit Line}} + \beta_4 \times \text{Step-by-Step}_i^{\text{Credit Line}} + \varepsilon_i^m. \end{aligned} \tag{2}$$

In this regression, β_1 captures the impact of summary information about the layoff support program, while β_2 captures the incremental impact of the step-by-step application guide. β_3 and β_4 capture analogous impacts with respect to the credit line scheme.

4 Data

We focus on firms headquartered in Portugal with a publicly available email address in the ORBIS database.[13] We exclude listed companies, financial or public administration entities (including education, social care, and arts and sport activities), non-domestic firms, and companies with missing industry information. We do not consider firm eligibility for government support measures when selecting our sample. This filtering process yielded a list of 172,892 firms, which we then randomly assigned into the different treatment arms.[14] In column 1 of Table 1 we display the number of firms assigned to each treatment arm. There are 57,623 firms in the layoff treatment arm, 57,629 in the credit line arm, and 57,640 in the control group.

Our initial database of emails included a significant number of firms that were no longer active during our intervention. This occurred both because there is a reporting delay, and because ORBIS often retains data on firms for several years after they cease activity. In our analysis sample, we condition on firms that were active at least through 2019. Our final sample consists of 107,549 firms, of which 35,966 were assigned to the layoff treatment arm, 35,649 were assigned to the credit line treatment arm, and 35,934 were assigned to the control group (see Table 1, column 2).

In the last columns of Table 1, we report the response rate to the follow-up survey. We received 7,740 responses (representing 7.2% of our final sample). While small, the response rate is nearly identical across the

[13]The coverage of Portuguese firms in ORBIS is particularly good, as has been noted in previous studies. For example, Kalemlİ-Özcan et al. (2024) focus on the manufacturing sector and report that Portuguese firms on ORBIS represent over 90% of total gross output. Similarly, Bajgar et al. (2020) show that the share of employment and output is close to 100% when compared to the OECD Structural Analysis Database. The set of firms with available email addresses is slightly smaller. For reference, the number of firms in 2018 according to Statistics Portugal (Sistema de Contas Integradas das Empresas) was 418,767. Our sample contains 172,892 firms, which corresponds to approximately 41%.

[14]In the intervention stage, we sent 126,792 emails (with content and/or a survey) of which 20,133 bounced back. In the follow-up stage, we sent 172,892 emails, of which 29,314 bounced back. This corresponds to 84% and 83% of emails being delivered successfully in each stage, respectively.

three main treatment arms. We show in Panel B of Table 1 that the decision to answer the follow-up survey is not correlated with treatment status. In section 5, we provide a more thorough discussion of selection into the follow-up survey.

In Table 2 we present descriptive statistics as of 2019. The average firm in our sample had roughly 2 million EUR in total assets, 14 employees, and was 17 years old. Around 37% were located in urban areas. The different treatment arms are balanced in terms of observable characteristics. In the last two columns of Table 2, we show that the layoff and credit line treatment groups do not differ in a statistically significant way from the control group along observable dimensions. Appendix Table A1 shows that there is similarly balance on observable characteristics between treatment and control groups among respondents to the follow-up survey.[15]

In Appendix Table A2 we show summary statistics for the baseline and follow-up surveys. The baseline survey was sent to 21,473 of our sample firms (one-fifth), with a 3.9% response rate. Respondents, who were typically the manager, CEO, or director of the firm, reported a basic awareness of government support programs. 89% of respondents reported that they are aware of the layoff support program, compared to 71% for the credit line scheme and 74% for a program that allowed deferrals of taxes and contributions. However, only around 27% of respondents reported that their knowledge of the layoff program was "high" or "very high." Furthermore, only 17% (8%) classified the application process for the layoff (credit line) measure as easy or very easy. Among respondents to the baseline survey, less than one-third had already applied to the layoff or the credit line program.

The follow-up survey, conducted in September 2020, shows that approximately 33% of the respondent firms applied to the layoff program, out of which 97% received program support (this measure was automatically approved upon application for qualifying firms). Around 27% applied to the credit line scheme, and 87% of those firms received it. The bulk of the applications to the layoff program were in April 2020 (61%), while applications to the credit line program were spread over the months following the initial roll-out of COVID-19 support measures. The timing and rate of applications reported in our survey align with other data sources, as illustrated in Appendix Figure A2 and the accompanying explanation.

In Appendix Figure A3 we show the geographic distribution of firms in our sample, as well as the distribution of the sub-sample of firms that answered the follow-up survey. Our sample firms are spread across the country and the geographic distribution of follow-up survey respondents is not qualitatively different from the full sample.

We supplement our data with information from the first round of the World Bank Enterprise Surveys Follow-Up on COVID-19 in Portugal that was conducted in September and October 2020, which includes

[15] The number of observations is also balanced at the industry level.

data on program receipt. The correspondence identifiers used to merge the datasets were provided by the European Bank for Reconstruction and Development (EBRD) through a confidentiality agreement. Out of 1062 firms surveyed by the World Bank, 483 are part of our experimental sample.[16]

5 Results

In this section, we present the results of our intervention.

Layoff Support

Panel A of Table 3 shows our main result, i.e., the impact of our intervention on application to the layoff support program. The outcome variable is an indicator for an affirmative response to the question *"Have you applied to the layoff support measure?"* in the follow-up survey. Our estimation is therefore limited to those who answered the follow-up survey.

We find a consistently positive and significant impact of the *layoff treatment* on take-up of the layoff support program. In column (1), we report results from the specification shown in Equation 1. The coefficient of interest is 0.026, indicating that firms receiving the *layoff treatment* were 2.6 percentage points more likely to report applying to the layoff support measure (relative to a control group mean of around 32 percent). The result is statistically significant at a 95% confidence level. We do not find a cross-program effect of the *credit line treatment* on application to the layoff support program.

In column (2), we consider the incremental impact of providing step-by-step instructions on the application process. In this specification, we include additional indicators equal to one for the subset of the treatment groups that received step-by-step application instructions following Equation 2. The coefficient on the basic layoff informational treatment increases slightly and remains statistically significant. The incremental effect of step-by-step support is negative but is not statistically significant. This suggests that providing step-by-step instructions did not increase take-up further, and if anything may have been counterproductive.

In the remaining columns, we include industry fixed effects and control for firms' pre-treatment financial characteristics. Our estimates of interest remain practically unchanged (the effect of the *layoff treatment* on take-up varies between 2.3 and 3.5 percentage points). We find that larger firms are on average more likely to apply to the layoff program, while age and leverage do not correlate with take-up rates. Overall, these results indicate that our simple information treatment had an economically and statistically significant impact on application to the layoff support program.

[16] This corresponds to 45%, in line with the fraction of firms with an email contact available relative to the universe of Portuguese firms.

Credit Line

Panel B of Table 3 shows the impact of our intervention on application to the credit line scheme. The outcome variable is an indicator for an affirmative response to the question *"Have you applied to the credit line guarantee scheme?"* in the follow-up survey.

Across our specifications, we find no evidence of a significant impact of either of the two treatments on application to the credit line guarantee scheme. In columns (1), (2), (3), and (5), the effect of the *credit line treatment* is positive but small and not statistically significant. The coefficient is negative in columns (4) and (6), but remains small and is not statistically significant. We also find no cross program effect of the *layoff treatment* on take-up of the credit line program. Receiving information about the layoff program did not significantly increase the probability of applying to the guarantee. Finally, we find no evidence of an impact of step-by-step application guidance. We provide a discussion on the asymmetry of treatment effects across programs in section 6.

The Follow-Up Survey and Selection

In the ideal scenario, we would observe information on program take-up from external sources (e.g., the government) and for all firms. Several potential issues arise because our measure of program application is collected from a follow-up survey and thus self-reported. A first concern relates to misreporting, as firms may not accurately reveal whether they applied to the programs. For example, it could be the case that our treatment impacts how firms respond to the follow-up survey, without an effect on take-up itself (although it is unclear why those in the layoff treatment arm would be impacted more than those in the credit line arm). A second concern relates to the fact that we do not have information for all firms. Attrition is high in our sample, which could be problematic if the firms that answered the follow-up survey are different along observable or unobservable dimensions from the ones that did not. Below, we present and discuss evidence that our survey-based measure is not cause for concern.

To address concerns about misreporting, we verify our results using data from an independent survey conducted by the World Bank – the World Bank Enterprise Survey Follow-Up on COVID-19. This survey includes questions about receipt of various COVID-19 support programs. We present our estimates in Table 4. In this sample, the estimated coefficients for the layoff support measure are positive, statistically significant, and larger than those in our survey. Layoff treatment firms are 10-12 percentage points more likely to receive layoff support (compared to a 15% rate in the control group). We again find no significant results for the credit line scheme. These magnitudes should be interpreted with caution as this survey is small and different from our experimental sample. However, the sign and significance of the results are reassuring because the sampling process was independent of our research project, and it is unlikely that our treatment influenced participation in the World Bank Survey or the probability of misreporting. The comparison with data from the Bank of Portugal in Appendix Figure A2 further reinforces the accuracy of our survey measure.

To address concerns about selection and attrition, we start by showing that there is no significant correlation between treatment assignment and follow-up survey participation. In Appendix Table A3, we regress the probability of responding to the follow-up survey on treatment status (columns 1 and 3) and on treatment status interacted with pre-treatment financial characteristics (columns 2 and 4). We find no evidence that treatment status is a determinant of follow-up survey participation. We also show that there is balance on observable characteristics between treatment and control groups among the firms that answered the follow-up survey (see Appendix Table A1).

Because attrition could still occur along unobservable dimensions, we estimate treatment-effect bounds for nonrandom sample selection following Lee (2009). Panel A of Table A4 shows the results. Both the lower and upper bounds of the estimated treatment effect for the *layoff treatment* on layoff program application are positive and statistically significant (at least at a 90% significance level). The estimated lower bound is 0.024 while the upper bound is 0.04. Bound estimates using the credit-line information treatment are not statistically significant. In Panel B of Table A4 we show treatment effects with inverse probability weighting, which corrects for missing data on the outcome. The effect of the *layoff treatment* on layoff program take-up remains unchanged and significant at the 95% level. Together, this evidence suggests that our results are unlikely to be driven by attrition.

Heterogeneity Across Firms and Over Time

We next turn to exploring heterogeneity in our results across firm characteristics and over time. These analyses serve two purposes. First, the time and firm-level heterogeneity both reinforce our baseline results by showing that the impact of treatment is concentrated in the time period and set of firms we would expect ex-ante. Second, firm-level heterogeneity supports the assertion that information frictions hinder access to aid for those that plausibly benefit most. Because we do not find a statistically significant or economically meaningful treatment effect for the credit line scheme in our main analysis, we focus primarily on the layoff support program here and relegate our results on heterogeneity with respect to the credit line scheme to the Appendix. Similarly, we focus on the specification in Equation 1 here but repeat our analysis using the specification in Equation 2 (which incorporates step-by-step application support) in Appendix B.

Heterogeneity Across Firms

In Table 5 we exploit information on firm characteristics in the ORBIS data to study heterogeneity in our estimated treatment effects on the layoff program. We focus on program take-up and consider four dimensions of heterogeneity: firm size (total assets), labor intensity (employees/total assets), leverage (debt/total assets), and location (urban vs non-urban areas). Each of these variables represents a dimension that could make firms more sensitive to our intervention. Specifically, we hypothesize that small firms and firms located in non-urban areas might face greater information frictions (and so may be particularly impacted by our intervention, see, e.g. Bernstein et al., 2023), and that firms with higher labor intensity and with higher leverage stand to benefit most from the government support measures.

As hypothesized, we find larger coefficients for small firms, firms with high labor intensity, and firms located in non-urban areas (although the point estimates for the sub-groups are not, in general, statistically different).[17] Small firms that received the *layoff treatment* were 3.7 percentage points more likely to apply to the layoff program.[18] Firms in non-urban areas are more responsive to the *layoff treatment* (3.8 p.p.) than those in urban areas (0.001 p.p.). We also see a treatment effect of 4.5 percentage points for the most labor intensive firms (versus 0.003 for the least). We do not find a differential treatment effect along the leverage dimension.[19] Appendix Table A5 shows results for the credit line program. Unsurprisingly, given the lack of significant coefficients in our main specification, we do not find significant treatment effects for credit line take-up in the sub-samples we consider.

We then make use of the data from the World Bank Enterprise Survey Follow-up on COVID-19—which offers a richer set of firm-level observable characteristics—to estimate the effects on layoff program receipt across several additional dimensions of heterogeneity. We present these results in Table 6.

Columns (1)-(4) show a significant treatment effect for firms that reported at least one furloughed worker, but not among firms that did not. The difference is expected as the furlough of workers was one of the main aims of the layoff program, which reaffirms the credibility of the treatment effects we estimate in our baseline analysis. In columns (5)-(8) we see heterogeneous treatment effects for family versus non-family firms. We define family firms as those in which the same family owns more than 75% of the firm. The point estimate is larger and statistically significant for family firms (0.142). We conjecture that family firms are smaller

[17] We find no meaningful relationship between measures of financial literacy from the baseline survey and program take-up. However, we have a small sample—the intersection of firms that answered the baseline and follow-up surveys—and hence low power to detect any treatment effects.

[18] One concern is that differential treatment effects for small firms is the result of better email targeting. This could be the case if, for example, email addresses provided to ORBIS by small firms are more likely to be for firm decision-makers (versus generic human resources or communications representatives). To test this, we split our sample into emails that clearly represent individual names versus all others. Point estimates are nearly identical, suggesting this concern is unlikely to be driving the observed heterogeneity.

[19] Appendix Table B1 shows consistent results when examining the step-by-step treatment tier alongside summary information. As in our baseline estimates, there does not appear to be an incremental impact of step-by-step application support across specifications.

and less sophisticated in terms of management practices, and so may face more pronounced informational frictions.

Our final tests using World Bank data focus on measures of financial constraints and access to external financing. Columns (9)-(12) show a sizeable and significant treatment effect for firms that report facing major obstacles in accessing finance. We see a much smaller and insignificant effect for firms that report only minor obstacles. Similarly, columns (13)-(16) show no significant effect for firms with access to overdraft facilities, but large and statistically significant effects for those without such access. These results support the notion that firms with liquidity needs—who arguably benefit most from layoff support—were more likely to apply to the layoff program as a result of our intervention.[20]

Heterogeneity Over Time

In Figure 1 we show that the impact of our treatment peaks in the month immediately following the intervention. We plot the coefficient estimates on the layoff treatment variable (following the specification in column 5 of Table 3) with an outcome equal to one if the firm reported applying to the layoff measure in a given month and zero otherwise. The specification is estimated separately for each month between March and September. Firm responses come from our follow-up survey, which was conducted in September 2020. We find that the effect of the layoff treatment is largest (and is statistically different from zero at a 95% confidence level) in May 2020. We do not find significant effects at conventional levels in other months.

Survival

The main goal of the COVID-19 assistance measures was to support the business sector through the pandemic outbreak and to provide firms with enough liquidity to withstand the negative economic consequences of the virus. As such, firm survival is the key outcome for measuring their success. In this section, we ask whether our treatment impacted survival rates post-pandemic.[21] Using the most recent firm-level data from Statistics Portugal, which covers firms through the end of 2022, we construct a binary variable equal to one if the firm was still active in 2022.[22] We show the results in Table 7.

We find a positive and significant impact of the layoff treatment on firm survival. Columns (1) and (2) of Panel A show that firms exposed to the layoff treatment were 1 percentage point more likely to survive and remain active through 2022. The coefficient is statistically significant at a 95% confidence level. This effect is more pronounced in the subsample of firms that reported applying to the layoff support program (versus those that did not report applying), consistent with the impact on firm survival operating through

[20] Appendix Table B2 shows results when adding the step-by-step treatment indicators.

[21] Survival rate is a frequent outcome in informational intervention studies (see, for instance, Bernstein et al. (2023)).

[22] We match the data using firm financial reports following Card et al. (2016).

greater receipt of the assistance measure. Indeed, firms that reported receipt of the layoff support program recorded a lower change in the wages per worker ratio in 2020 relative to those that did not.[23] We do not find a statistically significant impact of the credit line treatment on firm survival, in line with the lack of a treatment effect on program take-up.

Because firms can also reduce scale by closing down establishments, we additionally consider the number of establishments in each firm as an outcome (see Panel B of Table 7).[24] We observe this variable only through the end of 2021. We find that firms assigned to the layoff treatment have on average 0.09 more establishments compared to the control group. Once again, the impact is concentrated in the subsample of firms that reported applying to the layoff program. Interestingly, we also find a positive and statistically significant effect of the credit line treatment on the number of establishments in this subsample (columns 3 and 4). However, we are cautious when interpreting this result, given that we find no impact of the credit line treatment in our earlier analysis (and applying to the program is itself endogenous to treatment status). Overall, these results provide evidence that small information-based interventions such as ours can have real implications for targeted companies.[25]

6 Discussion

In this section, we discuss the magnitude and interpretation of our results as well as potential channels through which our estimated effects operate. We make use of survey data to present suggestive evidence on different mechanisms.

Magnitude

Our results on the layoff program indicate that our information intervention had a positive impact on program application. Treated firms were between 2.3 to 3.7 percentage points more likely to report applying to the layoff support measure. There is little experimental evidence on information interventions targeting firms that provides comparable estimates to benchmark our estimates against. One exception is Gertler et al. (2022), who find that a reminder increases take-up of a lower-fee option on a payment processing system by 3.6 percentage points, exactly in line with our findings. This is slightly smaller than the effects found in some informational interventions that target individuals. For example, Hotard et al. (2019) find that a simple and low-cost information nudge increased the rate of citizenship applications by 8.6 percentage points

[23] This difference is statistically significant at the 1% level, while no difference between the two groups existed in the two years before 2020.

[24] We assign firms that are no longer active to have 0 establishments. We find qualitatively similar results when excluding these firms, indicating that there is a meaningful intensive margin effect in addition to the extensive margin shown in Panel A.

[25] Appendix Table B3 shows results with respect to survival and the number of establishments when examining the step-by-step treatment tier alongside summary information.

among low-income immigrants eligible for a federal fee waiver in the US, and Marx and Turner (2019) show that loan-eligible students randomly receiving a student loan offer were 40 percent more likely to borrow.

It is worth emphasizing that a fraction of treated companies were not eligible to apply (because, for example, they were not facing a full or partial shutdown). As a result, the magnitudes we estimate provide only a lower bound on the treatment effect among eligible firms. To address this issue, Appendix Table A6 shows that the treatment effect on the layoff program is significantly more pronounced in retail (6.1 p.p.), and manufacturing and utilities sectors (10.4 p.p.), two of the three sectors most harmed by COVID-19 at the time of our treatment (we show the percentage of firms that were affected in each sector in the second week of April as reported in Banco de Portugal (2020a) in Panel B). However, the treatment effect on accommodation and restaurants, potentially the most affected sector at that time, is not statistically different from zero (perhaps because the salience of the shock to this sector rendered our treatment redundant).

Mechanisms

While the experiment was designed to test the effectiveness of a simple targeted information intervention, and not to study specific underlying mechanisms, we next briefly discuss some hypotheses about why our treatment was effective (in the case of the layoff program). There are several channels through which the impact might operate.

Perhaps the simplest possible mechanism is that our treatment provided a reminder or nudged firms to apply for the program without conveying any new details about the program's benefits or application process. Such an effect could be due to inertia or procrastination (see Banerjee and Mullainathan, 2008, Duflo et al., 2011, Ponce et al., 2017, DellaVigna and Gentzkow, 2019). Consistent with this mechanism, Figure 2a shows that, according to our baseline survey, a very small fraction of managers were totally unaware of the layoff program, with only 11% reporting no knowledge of its existence at the time of the intervention. Figure 2b also shows that 43% of survey respondents had not actively searched for information regarding the layoff program, which may be consistent with procrastination.

However, other evidence suggests that the detail of our intervention meaningfully changed firms' understanding of the layoff support program. Less than 40% of firms perceived the information provided by the government to be "Very Informative" or "Totally Informative" (Figure A4). Few respondents report having high or very high knowledge of the layoff program prior to our intervention (see Figure 3a). In addition, the absence of an impact of the *credit line treatment* on take-up of the layoff support program indicates that there was some informational value in our intervention. Reminding participants about one government support measure does not increase take-up of other measures. These arguments are consistent with the possibility that there was meaningful information delivered by the treatment, and that inertia and procrastination were not the sole mechanisms in place.

It may also be that specific features of our intervention were the main source of the observed impact. One possibility is that the context and framing of the information drove the effect. The language in the email and pamphlets was not as formal as that used by the government in official communications about the programs, and only the information that the research team considered essential was provided. This included the eligibility criteria, an example of the program benefit, and a link to a website with government resources. A second possibility is that the source of the information was key. There may be a lack of trust in information provided by official or media sources, and/or intangible costs of applying (e.g., stigma towards benefiting from government support), which discourage take-up. This stigma or lack of trust may be alleviated when information is provided by a university source that is perceived to be objective and trustworthy (see Haaland et al. (2023) for a discussion on sources of information). A third option is that the time and mode of information provision were important. The intervention took place by email shortly after the government measures were announced. The communication was done during the week, and during business hours (Wednesday at 10 am), which might induce responsiveness. A fourth possibility is that the targeted nature of our intervention was crucial. The fact that our treatment was delivered directly to a firm representative as opposed to being available by search or another type of exposure (e.g., general advertisement or newspapers) might induce compliance.[26]

While we remain agnostic about the specific mechanism at work, the simple takeaway is that a low-cost email-based information campaign can have non-trivial impacts on firm take-up of government support and, subsequently, on firm survival.

Differential Treatment Effects Across Programs

We find evidence that our intervention had a meaningful impact on program application. However, our results also suggest that this effect is not homogeneous across programs. Firms supplied with information about the layoff support program were significantly more likely to apply when compared to a control group, while firms supplied with equivalent information about the credit guarantee program were no more likely to apply. While the experiment was not designed to study heterogeneous treatment effects across programs, we next discuss this finding.

We hypothesize that the absence of a treatment effect for the credit guarantee scheme may be the result of banks' role as intermediaries in the application process. Because banks have a prominent role

[26] Adams et al. (2021) shows that consumers respond differently to different forms of information disclosure while deciding to switch savings accounts.

in the process of administering guaranteed loans (and because they stand to benefit from the guarantees), they face direct incentives to alleviate information frictions. If banks acted upon this incentive by directly contacting, advertising to, or otherwise informing eligible firms, then our experimental treatment may have been redundant. Anecdotal evidence suggests that banks actively contacted and encouraged qualifying firms. The basic patterns of program receipt across firms support this notion. Appendix Table A7 displays the proportion of firms in the World Bank Enterprise survey with various characteristics that benefited from each support measure. Crucially, past contact with a commercial bank—as captured by currently having or recently applying for a loan or line of credit—is a key predictor of receiving a credit line through the guarantee scheme but does not predict receipt of the layoff support program.[27]

An alternative but not mutually exclusive explanation is that other frictions or binding constraints prevented the information treatment from effectively increasing applications to the credit guarantee program.[28] One reason for this could be the complexity of the application process. While the treatment provided across the two measures was very similar, and both targeted the short-term liquidity needs of firms, the application process was very different. The layoff program had a simple application process operating through a widely used website. The credit line program had a more involved process requiring borrowers to arrange and specify loan terms with a participating bank. It also had a more complex design, with an array of sector-specific features and formal requirements. Figure 3b shows that the credit line program was perceived by firms to have a more difficult application process than the layoff program: a larger fraction of firms reported that the credit line application process was difficult or very difficult (or that they did not know or did not have an opinion about the program).

Debt aversion might have also had a deterrent effect on take-up that our intervention was not able to mitigate. Using a sample of small and medium enterprises in Finland, Paaso et al. (2022) find that entrepreneurs' debt aversion explains the low take-up of government guaranteed loans, and conclude that, in the context of COVID-19, entrepreneurs are less interested in a hypothetical rescue package if it is labeled as debt compared to a financially equivalent alternative not labeled as such. Supporting this view, in Panel A of Table A8 we show that access to new credit is amongst the support programs with the lowest reported demand by firms across different European countries, including Portugal.

One further potential explanation for the differential effects across programs has to do with our intervention being conducted at slightly different times. Because of limited server capacity, the credit line intervention occurred one week after the layoff intervention. During this time period firms could have acquired more

[27] This evidence is consistent with Beck et al. (2018) showing that relationship banking is particularly important in periods of economic downturns.

[28] A survey conducted by Bank of Portugal in the second week of April shows that only 2% of the eligible respondent firms had received the credit line scheme at that time, and around 57% did not expect to apply at all (Banco de Portugal, 2020a).

information through their own means, rendering the information treatment redundant. However, given that the baseline level of awareness was larger for the layoff program this seems unlikely to be the primary explanation. Moreover, Figure 2b shows that a larger fraction of survey respondents reported having actively searched for the layoff program than for the credit line scheme.

Of course, our basic results are also consistent with the possibility that the credit guarantee scheme was simply free of information frictions. However, our survey evidence suggests that this was not the case. Figure 3a shows that a larger fraction of firms report having no or little knowledge about the credit line program when compared to the layoff support measure. Given this, we suspect the null effect is the result of three complementary forces. First, the inclusion of commercial banks as intermediaries gave them incentives to provide detailed information and encouragement to firms at the margin of applying. Second, the application process was sufficiently daunting that our straightforward information treatment was not comprehensive enough to push firms far from the margin to apply. Finally, any potential effect of the information provided about the credit line was not sufficiently strong to counteract cognitive heuristics associated with the use of debt.

External Validity

A natural question is the extent to which our findings may generalize to other contexts. There are at least three relevant margins to consider. First, at the highest level, one might ask about the relevance of the Portuguese setting and this specific set of programs. Portugal is a high-income country, a member of the EU and the Eurozone, and has a robust small business sector.[29] However, it lags behind top OECD countries in terms of productivity and per-capita GDP.[30] While Panel B of Table A8 shows substantial heterogeneity in access to government support across European countries, Portugal is not an outlier. In Portugal, 35% of firms received government assistance, mostly in the form of wage subsidies and cash transfers. This is comparable to other Southern European Countries such as Italy (28%) and Spain (36%). Panel A in Table A8 shows similar heterogeneity in the demand for different support programs across European countries. Importantly, these tables show that Portugal is by no means an exception, lying in the middle of the distribution when considering government support in various forms. Given this, we view Portugal as a reasonable laboratory in which to consider the sorts of policy challenges faced by developed economies with well-established and formal small business sectors, even if it is not perfectly representative of other contexts, such as the US.

A second concern is the representativeness of our main sample relative to all firms in Portugal. In Appendix Table A9 we present a direct comparison of the firms in our sample (consisting primarily of firms

[29] Small and medium enterprises employ more than 70 percent of the labor force. See `https://www.oecd-ilibrary.org/sites/6707606e-en/index.html?itemId=/content/component/6707606e-en`.

[30] `https://www.oecd.org/economy/growth/Portugal-country-note-going-for-growth-2021.pdf`.

for which ORBIS provides an email address) to all remaining Portuguese firms in the ORBIS data. Non-sample firms are significantly smaller (based on the average value of assets and the number of employees). Moreover, they have less cash, are more levered, and were less profitable in 2019. While this comparison suggests that our sample does not perfectly capture the Portuguese economy as a whole, it likely captures a significant fraction of the larger and more consequential firms. Furthermore, an extrapolation from our findings regarding small firms in our sample would indicate that we would expect treatment effects to be even larger among excluded firms.

Third and finally, it may be the case that the follow-up respondents to our survey (for which we observe the key dependent variable) are selected in some way and are differentially impacted by our treatment relative to the full population. However, we find consistent results when analyzing a World Bank survey conducted independently from our study. Given this, we see no evidence to suggest that our sample of respondents is selected in a material way.

7 Conclusion

This paper tests whether informational frictions prevent firms from accessing government assistance programs. Using a randomised controlled trial, we find that providing targeted and simplified information on a layoff support program had a positive effect on program application. We do not find an impact on application to a more complex program that provided government guarantees for credit lines using banks as intermediaries.

The effect on take-up for the layoff program is economically relevant. Upon receiving information, treated firms were 2.3 to 3.7 percentage points more likely to apply. Our results are more pronounced for smaller, non-urban, and labor-intensive firms, the types of firms that we would expect to benefit from this program. Despite reasonable public awareness and media coverage of government stimulus programs, our findings suggest that low-cost interventions, such as targeted emails to firms, can improve take-up rates for the firms that arguably benefit most.

Furthermore, we find evidence that our intervention had important real outcomes. Treated firms were more likely to survive in the years post-treatment, and to maintain a greater number of active establishments when compared to the control group.

While previous research on the take-up of government programs provides suggestive evidence that assistance often does not reach targeted firms, we provide causal evidence of the existence of frictions in the allocation of government support. Our low-cost and easy-to-replicate informational intervention helped vulnerable firms access government aid during a major economic downturn.

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8 Tables

Table 1: Randomization Counts and Follow-Up Orthogonality

Panel A: Randomization Counts

	Randomization	Sample	Follow-up Survey Respondent	
	Obs.	Obs.	Obs.	%
Layoff	57623	35966	2561	7.1
Credit Line	57629	35649	2570	7.2
Control	57640	35934	2609	7.3
Total	172892	107549	7740	7.2

Panel B: Follow-up Survey Respondent

Outcome: Follow-up Respondent	(1)	(2)
Layoff	-0.001	-0.001
	[0.002]	[0.002]
Credit Line	-0.001	-0.001
	[0.002]	[0.002]
Constant	0.073***	0.073***
	[0.001]	[0.002]
Industry FE	No	Yes
Observations	107549	107549
Adjusted R-squared	-0.000	0.001

This table displays the number of observations assigned to each treatment arm, the number of observations assigned to each treatment arm conditional on being active in 2019, and the number of respondents in the follow-up survey (we report the number of firms that answered the follow-up survey and were active in 2019). We define firms as active when we observe non-missing financial reporting in 2019. In the bottom table we test the orthogonality between participation in the follow-up survey and assignment to treatment. We regress a binary variable equal to one when the firm answered the follow-up survey on layoff and credit line treatment indicators. We include 1-digit industry dummies in column 2. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 2: Descriptive Statistics and Balance Tests

	Treatment				Control		Treat. vs. Control	
	Layoff		Credit Line					
	Mean	SD	Mean	SD	Mean	SD	Diff.	P-value
Firm Age	16.60	13.22	16.51	13.19	16.59	13.28	-0.04	0.64
Nr. Employees	13.72	77.55	15.18	113.55	14.20	92.16	0.25	0.70
Total Assets	2111.48	45724.34	1977.31	18667.73	1994.16	19150.08	50.58	0.80
Cash	192.29	1591.42	196.35	1231.68	195.73	1298.81	-1.41	0.88
Operating Revenue	1731.80	12400.68	1972.79	14464.73	1988.54	15697.91	-136.95	0.15
Net Income	46.67	4645.40	61.33	1109.94	68.09	1192.35	-14.13	0.45
Long-term Debt	714.14	36298.64	450.51	7982.05	487.25	6542.24	95.39	0.57
Tangibility	0.28	0.28	0.27	0.27	0.28	0.27	0.00	0.85
Leverage	0.31	0.54	0.31	0.54	0.31	0.56	0.00	0.24
ROA	2.59	18.25	2.57	18.04	2.63	18.17	-0.05	0.68
Urban	0.37	0.48	0.37	0.48	0.37	0.48	0.00	0.63

This table displays descriptive statistics and balance tests. We present means and standard deviations of each variable for each of the three treatment arms. In the last two columns, we present t-tests and corresponding p-values for the mean difference between the treatment (layoff and credit line treatment arms are pooled) and control groups. We similarly do not find statistically significant differences when we make pairwise comparisons between the treatment arms and the control group. All financial variables are reported as of 2019 (year end) and are available from ORBIS. Balance sheet and income statement variables are reported in thousands of Euros. Tangibility is the ratio of fixed assets over total assets, leverage is the total debt over total assets, and ROA is net income over total assets. Urban is equal to 1 when a firm is located in a council with more than 150,000 inhabitants. Out of the 308 municipalities, 14 (4.6%) are classified as urban and these correspond to 31% of the total population. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 3: Effect of the Intervention on Program Take-Up

Panel A: Layoff	Applied to Layoff					
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.026**	0.037***	0.023*	0.035**	0.023*	0.035**
	[0.012]	[0.013]	[0.012]	[0.014]	[0.012]	[0.014]
Layoff Step-by-step		-0.021		-0.024		-0.023
		[0.015]		[0.015]		[0.015]
Credit Line	-0.007	0.000	-0.007	-0.002	-0.007	-0.002
	[0.013]	[0.014]	[0.013]	[0.013]	[0.013]	[0.013]
Credit Line Step-by-step		-0.015		-0.011		-0.010
		[0.016]		[0.016]		[0.016]
Log(Assets)					0.020***	0.020***
					[0.005]	[0.005]
Age					0.001	0.001
					[0.001]	[0.001]
Leverage					0.003	0.003
					[0.003]	[0.003]
Constant	0.324***	0.324***	0.326***	0.326***	0.068	0.068
	[0.030]	[0.030]	[0.021]	[0.021]	[0.066]	[0.066]
Industry FE	No	No	Yes	Yes	Yes	Yes
Observations	7740	7740	7740	7740	7734	7734
Adjusted R-squared	0.001	0.001	0.071	0.071	0.077	0.077

Panel B: Credit Line	Applied to Credit Line					
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.003	-0.009	0.001	-0.012	0.001	-0.011
	[0.012]	[0.015]	[0.011]	[0.015]	[0.011]	[0.014]
Layoff Step-by-step		0.024		0.025		0.024
		[0.018]		[0.018]		[0.017]
Credit Line	0.005	0.002	0.003	-0.001	0.003	-0.001
	[0.010]	[0.014]	[0.010]	[0.014]	[0.010]	[0.014]
Credit Line Step-by-step		0.006		0.007		0.008
		[0.021]		[0.021]		[0.021]
Log(Assets)					0.030***	0.030***
					[0.005]	[0.005]
Age					-0.001	-0.001
					[0.000]	[0.000]
Leverage					0.000	0.000
					[0.004]	[0.004]
Constant	0.263***	0.263***	0.265***	0.265***	-0.092	-0.092
	[0.015]	[0.015]	[0.012]	[0.012]	[0.061]	[0.061]
Industry FE	No	No	Yes	Yes	Yes	Yes
Observations	7296	7296	7296	7296	7290	7290
Adjusted R-squared	-0.000	-0.000	0.017	0.017	0.027	0.027

This table shows the effect of the intervention on program take-up (layoff in Panel A and credit line in Panel B). We present regression results for firms that were active in 2019 and answered the follow-up survey (i.e., for those for which we have self-reported information on applications). We add pre-treatment financial controls (logarithm of total assets, firm age and leverage as of 2019) in some specifications. In columns 3 to 6, we include 1-digit industry dummies. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 4: Effect of the Intervention on Program Receipt (World Bank Sample)

Panel A: Layoff	Layoff Program Receipt					
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.101**	0.121**	0.103**	0.120**	0.110**	0.122**
	[0.041]	[0.052]	[0.041]	[0.053]	[0.043]	[0.055]
Layoff Step-by-step		-0.036		-0.032		-0.022
		[0.075]		[0.074]		[0.078]
Credit Line	0.019	-0.032	0.025	-0.022	0.005	-0.046
	[0.037]	[0.040]	[0.037]	[0.041]	[0.038]	[0.042]
Credit Line Step-by-step		0.098*		0.090*		0.097**
		[0.050]		[0.051]		[0.048]
Log(Assets)					0.044***	0.044***
					[0.016]	[0.016]
Age					0.000	0.000
					[0.001]	[0.001]
Leverage					-0.009	-0.007
					[0.027]	[0.028]
Constant	0.152***	0.152***	0.150***	0.150***	-0.490**	-0.482**
	[0.029]	[0.029]	[0.029]	[0.029]	[0.203]	[0.207]
Industry FE	No	No	Yes	Yes	Yes	Yes
Observations	483	483	482	482	471	471
Adjusted R-squared	0.008	0.010	0.008	0.009	0.040	0.041

Panel B: Credit Line	Credit Line Program Receipt					
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	-0.016	-0.018	-0.015	-0.015	-0.009	-0.011
	[0.025]	[0.037]	[0.025]	[0.038]	[0.025]	[0.039]
Layoff Step-by-step		0.003		0.001		0.003
		[0.047]		[0.047]		[0.050]
Credit Line	0.018	0.003	0.014	-0.003	0.015	-0.002
	[0.025]	[0.037]	[0.025]	[0.038]	[0.024]	[0.040]
Credit Line Step-by-step		0.030		0.034		0.033
		[0.048]		[0.049]		[0.049]
Log(Assets)					0.013	0.013
					[0.009]	[0.009]
Age					-0.000	-0.000
					[0.001]	[0.001]
Leverage					-0.001	-0.000
					[0.015]	[0.016]
Constant	0.094***	0.094***	0.095***	0.095***	-0.091	-0.089
	[0.019]	[0.019]	[0.019]	[0.019]	[0.129]	[0.129]
Industry FE	No	No	Yes	Yes	Yes	Yes
Observations	483	483	482	482	471	471
Adjusted R-squared	-0.002	-0.005	-0.000	-0.003	-0.000	-0.004

This table shows the effect of the intervention on program receipt (layoff in Panel A and credit line in Panel B). We show the effect of our intervention on the sample of firms that participated in the World Bank Enterprise Surveys Follow-Up on COVID-19: Portugal, Round 1 conducted in September and October 2020. We merged the individual answers to this survey with our sample, and constructed the outcome variable based on a question on receipt of national and local government support, specifically the variables COVf2e (wage subsidies) in Panel A and COVf2c (access to new credit) in Panel B. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 5: Heterogeneous Effects of the Intervention on Layoff Program Take-Up

	Total Assets				Employees / T. Assets			
	High		Low		High		Low	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.016	0.015	0.037**	0.032**	0.045***	0.040***	0.003	0.004
	[0.021]	[0.019]	[0.014]	[0.014]	[0.014]	[0.014]	[0.019]	[0.019]
Credit Line	-0.019	-0.015	0.003	-0.003	-0.000	-0.003	-0.031	-0.027
	[0.021]	[0.019]	[0.018]	[0.017]	[0.018]	[0.017]	[0.019]	[0.018]
Constant	0.360***	0.358***	0.291***	-0.234**	0.331***	-0.073	0.337***	0.003
	[0.031]	[0.123]	[0.034]	[0.108]	[0.036]	[0.082]	[0.028]	[0.112]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3713	3713	3883	3881	4009	4009	3240	3240
Adjusted R-squared	0.000	0.075	0.001	0.081	0.001	0.113	0.000	0.045

	Debt / T. Assets				Urban			
	High		Low		Yes		No	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Layoff	0.018	0.009	0.011	0.014	0.001	-0.002	0.038**	0.036**
	[0.020]	[0.020]	[0.022]	[0.022]	[0.022]	[0.022]	[0.015]	[0.015]
Credit Line	-0.025	-0.023	-0.012	-0.011	0.015	0.019	-0.028*	-0.030*
	[0.021]	[0.018]	[0.023]	[0.021]	[0.025]	[0.023]	[0.017]	[0.016]
Constant	0.363***	0.160*	0.317***	0.070	0.351***	0.127*	0.319***	0.048
	[0.030]	[0.092]	[0.036]	[0.094]	[0.034]	[0.076]	[0.029]	[0.089]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2840	2840	2717	2717	2973	2970	4516	4514
Adjusted R-squared	0.001	0.084	-0.000	0.084	-0.000	0.064	0.003	0.086

This table shows heterogeneous effects of the intervention on the Layoff program take-up. Regarding the first 12 columns, firms are split at the median of the distribution of each variable. Financial data is as of 2019 year-end and available from ORBIS. In the last 4 columns, we classify the municipality where the firm operates based on total population. A council is classified as 'Urban' if the total population is higher than 150,000. Out of 308 municipalities, 14 (4.6%) are classified as urban and these correspond to 31% of the total population. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 6: Heterogeneous Effects of the Intervention on Layoff Program Receipt (World Bank Sample)

	Since the outbreak of COVID-19, how many workers have been furloughed?[a]				**What percentage of the firm is owned by the same family?**[b]			
	At least one		None		≥ 75		< 75	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.289***	0.323***	0.024	0.028	0.139***	0.142***	0.048	0.056
	[0.094]	[0.097]	[0.023]	[0.026]	[0.052]	[0.053]	[0.074]	[0.084]
Credit Line	-0.078	-0.117	0.042	0.048	0.049	0.026	-0.015	-0.017
	[0.086]	[0.094]	[0.028]	[0.031]	[0.045]	[0.046]	[0.069]	[0.068]
Constant	0.407***	-0.200	0.018	-0.101	0.113***	-0.438*	0.208***	-0.425
	[0.063]	[0.363]	[0.013]	[0.129]	[0.029]	[0.239]	[0.059]	[0.321]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	175	172	308	298	331	328	148	139
Adjusted R-squared	0.080	0.083	0.002	0.008	0.016	0.038	-0.010	-0.009

	To what degree is Access to Finance an obstacle to the current operations of this establishment?[b]				**At this time, does this establishment have an overdraft facility?**[b]			
	Major		Minor		Yes		No	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Layoff	0.146**	0.164**	0.078	0.082	0.027	0.005	0.160***	0.148***
	[0.060]	[0.063]	[0.060]	[0.063]	[0.074]	[0.069]	[0.053]	[0.055]
Credit Line	0.042	0.041	-0.007	-0.025	-0.061	-0.056	0.079	0.050
	[0.042]	[0.039]	[0.056]	[0.056]	[0.058]	[0.056]	[0.050]	[0.048]
Constant	0.120***	-0.725***	0.172***	-0.451*	0.218***	-0.550	0.101***	-0.517**
	[0.040]	[0.243]	[0.040]	[0.239]	[0.045]	[0.368]	[0.032]	[0.221]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	213	209	256	248	201	193	277	272
Adjusted R-squared	0.015	0.077	0.001	0.030	-0.002	0.108	0.021	0.060

This table shows heterogeneous effects of the intervention on the Layoff Program receipt for firms that answer the World Bank Enterprise Surveys. The outcome variable is constructed based on a question that inquires firms about receipt of national and local government support in response to the crisis, specifically the variable COVf2e (wage subsidies). We split the sample according to the answers to different questions. The first question (COVd8) is from the Follow-up on COVID-19 Survey conducted in September and October 2020. The remaining variables are taken from the standard Enterprise Survey (conducted between November 2018 and January 2020). The original variables are BMb1, k30, and k7, respectively. The question "To what degree is Access to Finance an obstacle to the current operations of this establishment?" is a 5-point scale ranging from "No Obstacle" (0) to "Very Severe Obstacle" (4). We split answers into major (2, 3 or 4) or minor obstacle (0 or 1). Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively. Source: [a] Enterprise Surveys Follow-Up on COVID-19: Portugal, Round 1 [Sep – Oct 2020] (The World Bank, 2020); [b] Enterprise Survey [Nov 2018 – Jan 2020] (The World Bank, 2019)

Table 7: Effect on Survival and Number of Establishments

Panel A: Survival (2022)

	Survival					
	All		Applied to Layoff		Did not Apply to Layoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.010**	0.010**	0.014*	0.016**	0.007	0.007
	[0.004]	[0.004]	[0.008]	[0.008]	[0.005]	[0.005]
Credit Line	0.005	0.005	0.008	0.010	0.003	0.003
	[0.005]	[0.005]	[0.007]	[0.007]	[0.005]	[0.005]
Constant	0.969***	0.869***	0.966***	0.865***	0.971***	0.868***
	[0.003]	[0.018]	[0.006]	[0.037]	[0.004]	[0.021]
Controls	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	7,720	7,716	2,552	2,550	5,168	5,166
R-squared	0.001	0.031	0.001	0.034	0.000	0.032

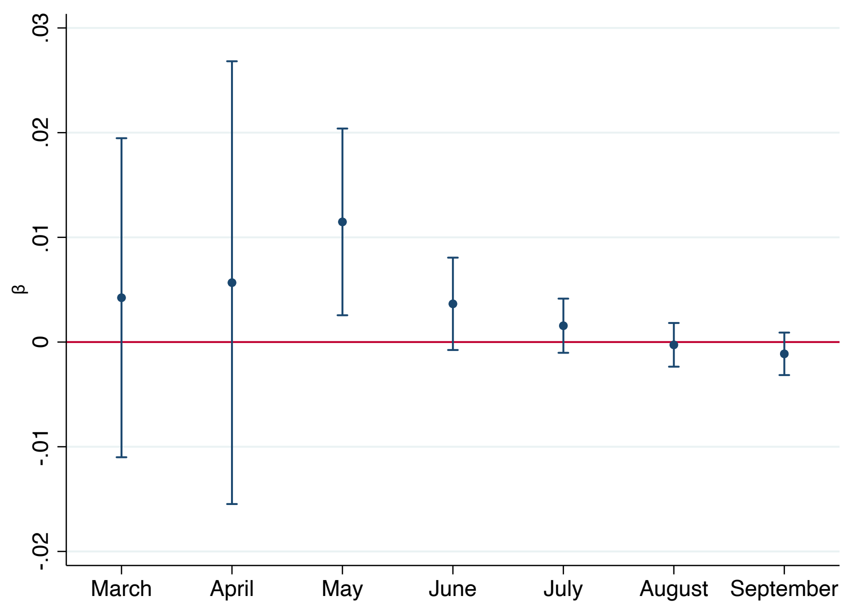
Panel B: Establishments (2021)

	Establishments					
	All		Applied to Layoff		Did not Apply to Layoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.089*	0.087*	0.211**	0.222***	0.013	0.006
	[0.046]	[0.045]	[0.083]	[0.080]	[0.045]	[0.043]
Credit Line	0.052	0.050	0.189**	0.205**	-0.018	-0.035
	[0.036]	[0.037]	[0.085]	[0.082]	[0.040]	[0.047]
Constant	1.131***	-1.914***	1.163***	-2.889***	1.114***	-1.366*
	[0.031]	[0.589]	[0.034]	[0.778]	[0.038]	[0.731]
Controls	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	5,772	5,770	2,070	2,069	3,702	3,701
R-squared	0.001	0.072	0.003	0.078	0.000	0.086

This table shows the effect of the intervention on survival (until 2022) and the number of establishments (in 2021) on the sample of firms that answered our follow-up survey. Survival data is available from Statistics Portugal (INE) until 2022 and the number of establishments until 2021. In the first two columns we present regression results for firms that were active in 2019 and answered the follow-up survey (i.e., those for which we have self-reported information on applications). From those, we condition on firms that reported to have applied to the layoff measure in the following two columns. In the last two columns, we condition on those that did not apply to the layoff measure. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

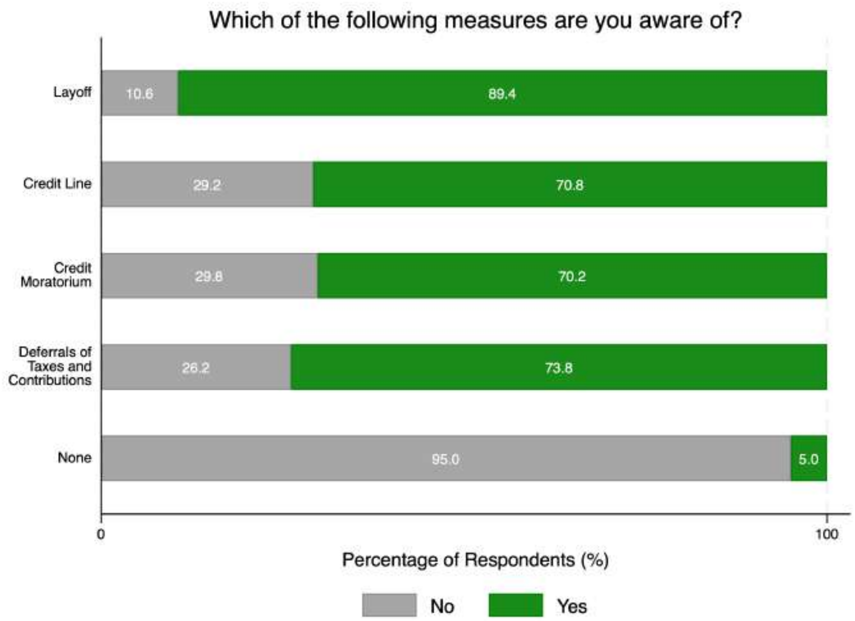
9 Figures

Figure 1: Timing of the Effect (Layoff Program Take-Up)

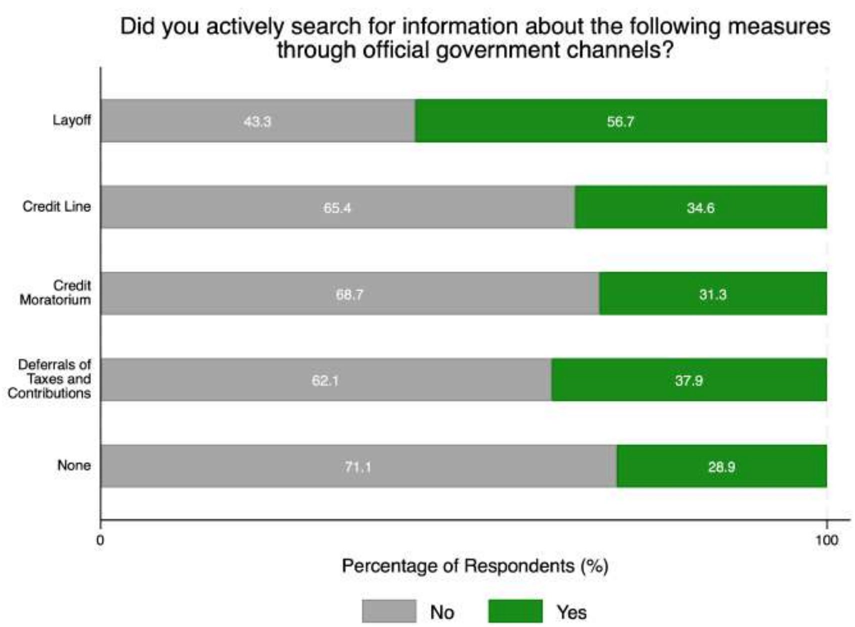


This figure plots the treatment effect coefficient on the layoff intervention (and the corresponding 95% confidence interval) estimated following the specification as in column 5 of Table 3 where the outcome variable is a binary variable equal to one if the firm applied to the layoff program in a given month between March and September 2020, and zero otherwise. The information on the month of the application was collected in the follow-up survey.

Figure 2: Baseline Survey: Awareness and Active Search for Information



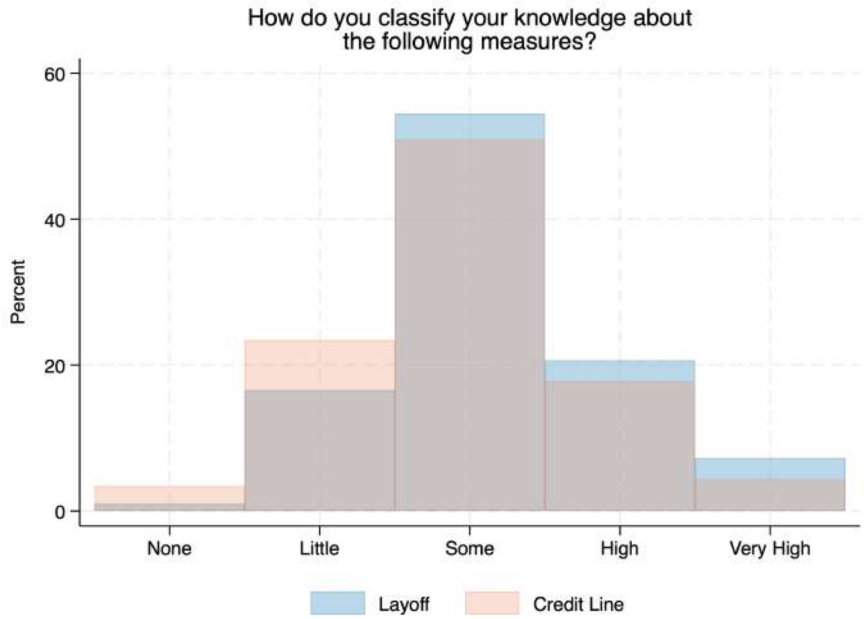
(a) Awareness



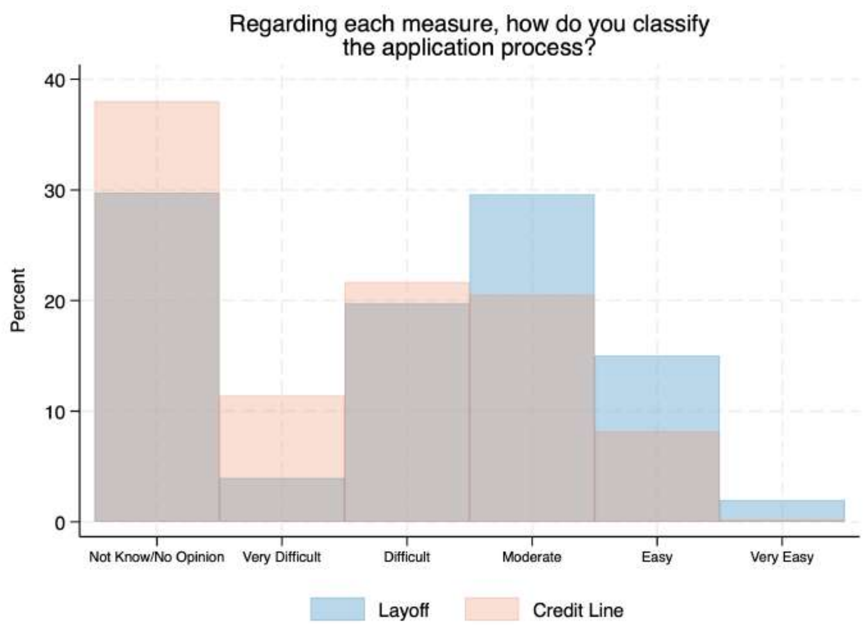
(b) Active Search

The top figure shows the percentage of respondents in the baseline survey that are aware of each of the four government measures: layoff, credit line, credit moratorium, and deferrals of taxes and contributions. The survey question is "*Which of the following measures of the Portuguese Government are you aware of?*". The bottom figure shows the percentage of respondents who actively searched for information about the measures through official government channels. The survey question is "*Did you actively search for information in the government official channels about the following measures?*". See Table A2 for the corresponding number of observations.

Figure 3: Baseline Survey: Knowledge about the Measures and Difficulty of the Application Process



(a) Knowledge about the Measures



(b) Difficulty of the Application Process

The top figure shows the baseline self-reported knowledge level about the layoff and credit line measures. The survey question is "*How do you classify your knowledge about the following measures?*". A separate question for each measure was displayed, and was asked only when the respondent reported awareness of the corresponding measure in a previous question. Thus, this figure reports the knowledge level among those who are aware of a given measure. The bottom figure shows a histogram of self-reported perception of the difficulty of the layoff and credit line application process on a scale from very difficult to very easy. The survey question is "*Regarding the [measure], how do you classify the application process?*". An option "I don't know/I have no opinion" was also presented. See Table A2 for the corresponding number of observations.

Information Frictions and Firm Take-Up of Government Support: A Randomised Controlled Experiment

For Online Publication

Cláudia Custódio, Christopher Hansman, and Diogo Mendes

Appendix Tables A

Table A1: Descriptive Statistics and Balance Tests among Follow-Up Survey Respondents

	Treatment				Control		Treat. vs. Control	
	Layoff		Credit Line					
	Mean	SD	Mean	SD	Mean	SD	Diff.	P-value
Firm Age	14.49	12.95	14.71	12.68	14.74	13.31	-0.14	0.66
No. Employees	14.41	120.89	17.50	160.30	16.13	132.27	-0.18	0.96
Total Assets	2420.27	27746.25	2201.92	24980.90	2091.83	25419.68	219.46	0.73
Cash	247.67	2601.22	225.05	2436.26	204.78	1490.68	31.59	0.56
Operating Revenue	1541.73	8497.35	1821.46	14515.29	1687.87	9558.16	-6.83	0.98
Net Income	89.51	1499.25	72.54	757.79	59.10	1388.83	21.93	0.47
Long-Term Debt	675.37	10528.95	503.54	6495.91	569.74	8684.51	19.41	0.94
Tangibility	0.28	0.27	0.29	0.27	0.28	0.28	0.00	0.98
Leverage	0.30	0.49	0.29	0.48	0.31	0.54	-0.02	0.16
ROA	3.52	18.69	3.28	17.97	3.86	17.89	-0.46	0.30
Urban	0.40	0.49	0.39	0.49	0.41	0.49	-0.01	0.28

This table displays descriptive statistics and balance tests among follow-up survey respondents. We present means and standard deviations of each variable for each of the three treatment arms. In the last two columns, we present t-tests and corresponding p-values for the mean difference between the treatment (layoff and credit line treatment arms are pooled) and control groups. We similarly do not find statistically significant differences when we make pairwise comparisons between the treatment arms and the control group. All financial variables are reported as of 2019 (year end) and are available from ORBIS. Balance sheet and income statement variables are reported in thousands of Euros. Tangibility is the ratio of fixed assets over total assets, leverage is the total debt over total assets, and ROA is net income over total assets. Urban is equal to 1 when a firm is located in a council with more than 150,000 inhabitants. Out of the 308 municipalities, 14 (4.6%) are classified as urban and these correspond to 31% of the total population. *, **, *** Significance at 10, 5 and 1%, respectively.

Table A2: Descriptive Statistics of Baseline and Follow-up Surveys

Panel A: Baseline Survey (April 2020)	Obs.	Mean	SD
Which of the following measures are you aware of?			
Layoff	848	0.894	0.308
Credit line	848	0.708	0.455
Credit moratorium	848	0.702	0.458
Deferrals of taxes and contributions	848	0.738	0.440
None	848	0.050	0.217
How do you classify your knowledge about the following measures? (1-Very High, 5-None)			
Layoff	687	2.834	0.823
Credit line	550	3.038	0.852
Credit moratorium	550	2.855	0.824
Deferrals of taxes and contributions	572	2.657	0.885
Did you receive any simplified information about the measures without asking for it?			
Yes, from traditional and digital media (newspapers, television, etc.)	713	0.487	0.500
Yes, from accountants and lawyers	713	0.626	0.484
Yes, from other sources	713	0.146	0.353
No	713	0.111	0.314
Did you actively search for information about the following measures through official gov. channels?			
Layoff	713	0.567	0.496
Credit line	713	0.346	0.476
Credit moratorium	713	0.313	0.464
Deferrals of taxes and contributions	713	0.379	0.485
No	713	0.289	0.454
How do you classify the information provided by the official government channels? (1 - Totally informative, 5 - Not informative)	491	2.829	0.774
Classifies Layoff application process as easy or very easy	659	0.170	0.376
Classifies Credit Line application process as easy or very easy	526	0.084	0.277
Did your firm apply to the Layoff program already?	685	0.317	0.466
Did your firm apply to the Credit Line scheme already?	685	0.207	0.406
Do you have a main bank that you turn to for financing and other fin. transactions?	682	0.837	0.369
How do you classify the relat. with your main bank? (1 - Very close, 4 - Very distant)	570	1.777	0.767
What is your role in the firm?			
CEO/ Director/ Manager	670	0.773	0.419
Founder	670	0.258	0.438
Business Owner	670	0.409	0.492
Accountant	670	0.066	0.248
Other employee	670	0.134	0.341
What is the highest educational level that you attained?			
No high education	668	0.371	0.484
Undergraduate	668	0.407	0.492
Masters	668	0.210	0.407
PhD	668	0.012	0.109
Do you have academic background in Finance or related areas?	670	0.322	0.468
How do you classify your level of financial literacy? (1 - Very high, 5 - Very low)	670	2.816	0.748

Panel B: Follow-Up Survey (September 2020)	Obs.	Mean	SD
Have you applied to the Layoff program?	7740	0.331	0.470
When did you apply?			
March	2542	0.251	0.433
April	2542	0.612	0.487
May	2542	0.093	0.290
June	2542	0.020	0.139
July	2542	0.006	0.079
August	2542	0.006	0.079
September	2542	0.004	0.063
Don't recall	2542	0.008	0.091
Was your application to the Layoff program approved?	2513	0.969	0.173
Have you applied to the Credit Line scheme?	7296	0.266	0.442
When did you apply?			
March	1929	0.147	0.354
April	1929	0.382	0.486
May	1929	0.175	0.380
June	1929	0.078	0.268
July	1929	0.064	0.244
August	1929	0.064	0.245
September	1929	0.065	0.246
Don't recall	1929	0.025	0.157
Was your application to the Credit Line scheme approved?	1840	0.861	0.346

This table presents descriptive statistics on the baseline (Panel A) and follow-up (Panel B) survey responses.

Table A3: Correlation of Treatment and Follow-up Survey Response

	Answered Follow-up Survey			
	(1)	(2)	(3)	(4)
Layoff	-0.001	-0.007	-0.001	-0.007
	[0.002]	[0.015]	[0.002]	[0.016]
Credit Line	-0.001	-0.002	-0.001	-0.002
	[0.002]	[0.015]	[0.002]	[0.015]
Layoff x Log(Assets)		0.001		0.001
		[0.001]		[0.001]
Layoff x Age		-0.000		-0.000
		[0.000]		[0.000]
Layoff x Leverage		0.000		0.001
		[0.001]		[0.001]
Credit Line x Log(Assets)		0.000		0.000
		[0.001]		[0.001]
Credit Line x Age		0.000		0.000
		[0.000]		[0.000]
Credit Line x Leverage		0.001		0.001
		[0.001]		[0.001]
Log(Assets)		0.000		0.001
		[0.001]		[0.001]
Age		-0.001***		-0.001***
		[0.000]		[0.000]
Leverage		-0.002***		-0.002***
		[0.001]		[0.001]
Constant	0.073***	0.088***	0.073***	0.080***
	[0.001]	[0.011]	[0.002]	[0.013]
Industry FE	No	No	Yes	Yes
Observations	107549	107328	107549	107328
Adjusted R-squared	-0.000	0.002	0.001	0.002

This table reports the correlation of treatment with follow-up survey response. The outcome variable is one when the firm answered the follow-up survey. We present regression results for firms that were active in 2019. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table A4: Effect of the Intervention on Program Take-Up: Lee Bounds and Inverse Probability Weighting

Panel A: Lee Bounds				
	Applied to Layoff		Applied to Credit Line	
Treatment Variable	Layoff	Credit Line	Layoff	Credit Line
	(1)	(2)	(3)	(4)
Lower	0.024*	-0.021	-0.003	0.003
	[0.014]	[0.020]	[0.013]	[0.021]
Upper	0.040**	-0.019	0.010	0.003
	[0.020]	[0.014]	[0.021]	[0.013]
CI Left Bound	0.001	-0.058	-0.025	-0.038
CI Right Bound	0.074	0.007	0.049	0.028
Observations	107549	107549	107549	107549

Panel B: Inverse Probability Weighting				
	Applied to Layoff		Applied to Credit Line	
	(1)	(2)	(3)	(4)
Layoff	0.026**	0.026**		
	[0.013]	[0.013]		
Credit Line			0.005	0.005
			[0.013]	[0.013]
Controls	No	Yes	No	Yes
Observations	5170	5166	4877	4873

Panel A reports Lee Bound estimates following Lee (2009) for the effect of the intervention on program take-up. We implement this test with the Stata command *leebounds*, which computes treatment effect bounds for samples with non-random sample selection. Column 1 shows the treatment effect of the Layoff Treatment on Application to Layoff, column 2 the treatment effect of the Credit Line Treatment on Application to Layoff, and so forth (this estimator does not allow more than one treatment indicator at a time). The top rows report the lower and upper bounds that correspond to extreme assumptions about the missing information that are consistent with the observed data. We also report the confidence interval for the treatment effect (option *cie*). Panel B reports the average treatment effects by inverse-probability weighting (IPW). IPW estimators use estimated probability weights to correct for missing data on the potential outcomes. *, **, *** Significance at 10, 5 and 1%, respectively.

Table A5: Heterogeneous Effects of the Intervention on Credit Line Program Take-Up

	Total Assets				Employees / T. Assets			
	High		Low		High		Low	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.015	0.016	-0.007	-0.010	-0.008	-0.011	0.019	0.019
	[0.017]	[0.016]	[0.014]	[0.014]	[0.013]	[0.013]	[0.019]	[0.018]
Credit Line	0.014	0.016	-0.003	-0.008	-0.001	-0.004	0.008	0.007
	[0.015]	[0.015]	[0.015]	[0.014]	[0.014]	[0.014]	[0.017]	[0.017]
Constant	0.302***	0.279**	0.226***	-0.392***	0.259***	-0.340***	0.275***	-0.004
	[0.015]	[0.107]	[0.019]	[0.078]	[0.021]	[0.091]	[0.013]	[0.099]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3470	3470	3690	3688	3809	3809	3036	3036
Adjusted R-squared	-0.000	0.026	-0.000	0.034	-0.000	0.044	-0.000	0.017

	Debt / T. Assets				Urban			
	Yes		No		Yes		No	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Layoff	0.008	0.004	0.012	0.011	-0.018	-0.019	0.013	0.011
	[0.020]	[0.020]	[0.024]	[0.023]	[0.016]	[0.016]	[0.017]	[0.017]
Credit Line	-0.026	-0.026	0.026	0.026	-0.007	-0.005	0.010	0.007
	[0.018]	[0.018]	[0.018]	[0.018]	[0.014]	[0.014]	[0.017]	[0.017]
Constant	0.353***	-0.132	0.219***	-0.061	0.274***	-0.037	0.261***	-0.160**
	[0.021]	[0.099]	[0.014]	[0.072]	[0.021]	[0.073]	[0.013]	[0.077]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2690	2690	2552	2552	2810	2807	4243	4241
Adjusted R-squared	0.000	0.037	-0.000	0.015	-0.000	0.043	-0.000	0.020

This table shows heterogeneous effects of the intervention on the Credit Line program take-up. Regarding the first 12 columns, firms are split at the median of the distribution of each variable. Financial data is as of 2019 year-end and available from ORBIS. In the last 4 columns, we classify the municipality where the firm operates based on total population. A council is classified as 'Urban' if the total population is higher than 150,000. Out of 308 municipalities, 14 (4.6%) are classified as urban and these correspond to 31% of the total population. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table A6: Effect of the Intervention on Layoff Program Take-Up, by Industry

Panel A: Applied to Layoff	Applied to Layoff						
	Accomm. and Restaurants	Retail	Manuf. And Utilities	Construct.	Transport. and Storage	Services	Agriculture
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Layoff	-0.029	0.061**	0.104**	0.056	-0.003	0.001	-0.063
	[0.063]	[0.022]	[0.052]	[0.034]	[0.023]	[0.023]	[0.075]
Layoff Step-by-step	-0.065	-0.029	-0.095*	-0.002	0.074	0.006	-0.020
	[0.051]	[0.031]	[0.054]	[0.044]	[0.055]	[0.023]	[0.103]
Credit Line	-0.012	0.014	-0.069*	-0.001	-0.007	0.009	0.027
	[0.044]	[0.031]	[0.039]	[0.013]	[0.094]	[0.026]	[0.060]
Credit Line Step-by-step	-0.080	-0.027	0.021	-0.005	0.088	0.006	-0.056
	[0.046]	[0.027]	[0.041]	[0.023]	[0.149]	[0.026]	[0.167]
Log(Assets)	0.055***	0.017	-0.002	0.016	0.017	0.027*	0.000
	[0.011]	[0.010]	[0.014]	[0.011]	[0.015]	[0.015]	[0.024]
Age	0.005***	0.001	0.000	-0.001	0.002	-0.002	0.004
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]
Leverage	0.000	0.000	-0.011	0.005	0.124*	0.011	-0.008
	[0.007]	[0.007]	[0.011]	[0.006]	[0.065]	[0.008]	[0.013]
Constant	0.028	0.106	0.384*	-0.037	0.032	0.009	0.185
	[0.174]	[0.138]	[0.206]	[0.123]	[0.239]	[0.167]	[0.312]
Observations	522	1,917	969	1,292	288	2,602	144
R-squared	0.098	0.007	0.014	0.008	0.021	0.008	0.033

Panel B: Impact by Industry	Accomm. and Restaurants	Retail	Manuf. And Utilities	Construct.	Transport. and Storage	Services	Agriculture
Temporarily or permanently closed.	61.7	16.3	13.9	11.3	10.1	9.6	-
Negative impact on the company's turnover.	90.6	73.1	72.2	63.3	79.9	70.0	-

This table shows the effect of the intervention on layoff program take-up, in different industries. In Panel A, we present regression results for firms that were active in 2019 and answered the follow-up survey (i.e., for those for which we have self-reported information on applications). Standard errors clustered at 3-digit industry level are presented in brackets. For reference, we report in Panel B the percentage of firms that reported to have temporarily or permanently closed, or for which the COVID-19 crisis was having a negative impact on turnover, by industry, as reported in Banco de Portugal (2020a). Specifically, we report the percentage of firms that answered "Temporarily or permanently closed" to the question "What is the situation that best describes your company this week?" and that answered "Yes, a reduction" to the question "Is the COVID-19 pandemic impacting your company's turnover this week?". We sort industries by the percentage of firms that have temporarily closed. The agriculture sector is not surveyed by the Bank of Portugal. Services are relative to Information and Communication Services in the survey.

Table A7: Firm Characteristics and Program Receipt (World Bank Data)

	Yes			No			Diff. Layoff	Diff. Credit Line
	Obs.	% Received Layoff	% Received Credit Line	Obs.	% Received Layoff	% Received Credit Line		
Establishment closed temporarily due to the COVID-19 outbreak.	187	34.8	12.3	518	13.9	7.9	20.9***	4.4*
Since the outbreak of COVID-19, at least one employee has been furloughed.	254	46.5	13.0	458	4.4	6.8	42.1***	6.2***
The firm has a line of credit or a loan from a financial institution.	426	18.8	12.2	282	20.2	4.3	-1.4	8.0***
In the last fiscal year, the firm applied for lines of credit or loans.	190	17.9	15.8	522	19.9	6.5	-2.0	9.3***
Sales of at least 5 million EUR	163	33.7	14.1	497	14.9	7.6	18.9***	6.5**
Number of Employees higher than 100	133	36.1	14.3	579	15.5	7.8	20.5***	6.5**
Ownership by the same family above 75%	509	18.5	9.0	198	21.2	9.1	-2.7	-0.1
The firm has a board of directors or a supervisory board.	284	24.6	12.7	428	15.9	6.5	8.8***	6.1***
In the last fiscal year, the firm had its annual financial statements certified by an external auditor.	302	22.8	12.9	410	16.8	6.1	6.0**	6.8***
Over the last year, the firm has performance bonuses for managers.	135	26.7	11.1	230	23.9	10.4	2.8	0.7
The firm is part of a business membership organization, trade association, guild, chamber of commerce, or other business support group.	354	21.8	9.3	357	17.1	8.7	4.7	0.6
Establishment began operations before 2000.	454	21.4	10.6	258	15.9	6.2	5.5*	4.4*
Exports at least 25% of the sales.	205	21.0	11.2	507	18.7	8.1	2.2	3.1
Access to Finance is a major obstacle to the current operations of the firm.	300	19.7	10.3	390	19.2	8.2	0.4	2.1
The firm has an overdraft facility.	283	20.1	8.8	422	19.0	9.0	1.2	-0.2

This table displays the proportion of firms that received the layoff or credit line program support according to several firm characteristics in the World Bank sample. The first two rows refer to the Enterprise Surveys Follow-up on COVID-19: Round 1 conducted in September and October 2020. The remaining rows relate to the (pre-COVID-19) Enterprise Survey conducted between November 2018 and January 2020. In the last two columns we present t-test for differences in proportions. *, **, *** Significance at 10, 5 and 1%, respectively.

Table A8: Demand for Government Support and Government Support Receipt across European Countries (World Bank Data)

Panel A: What would be the most needed government support?

	Obs.	Cash transfers	Deferral of credit payments, interest suspension, or debt rollover	Access to new credit	Fiscal exemptions or reductions	Wage Subsidies
Czech Republic	502	14.4	3.5	0.8	26.8	44.4
Estonia	360	0.7	0.0	1.7	18.3	43.6
Greece	600	31.7	29.3	8.1	18.6	4.4
Hungary	805	11.8	1.6	1.4	32.8	49.3
Italy	760	54.7	1.2	2.4	33.3	7.1
Latvia	359	30.6	9.8	6.0	18.1	15.1
Lithuania	358	5.0	2.7	5.0	16.2	39.2
Poland	1369	32.7	14.6	4.8	22.3	16.1
Portugal	1062	30.8	1.8	2.7	53.0	9.7
Slovenia	409	2.0	2.8	3.2	15.5	48.6

Panel B: Which government support did the firm receive?

	Obs.	Received any national or local govt. support	Cash transfers	Deferral of credit payments, interest suspension, or debt rollover	Access to new credit	Fiscal exemptions or reductions	Wage Subsidies
Austria	600	72.0	60.9	15.2	20.1	23.3	80.1
Belgium	614	51.8	54.0	38.3	8.3	17.7	44.0
Czech Republic	502	59.8	62.8	11.2	4.1	4.1	33.1
Denmark	995	40.3	58.7	25.7	10.3	10.3	77.2
Estonia	360	43.2	0.0	16.2	2.6	11.1	94.9
Finland	759	48.9	80.9	21.7	10.7	7.1	17.3
France	1566	68.5	30.0	49.6	61.1	15.7	92.7
Greece	600	53.2	38.8	46.2	30.2	56.2	73.1
Hungary	805	28.6	9.5	6.3	6.3	2.1	92.1
Ireland	606	67.3	16.3	24.8	8.8	35.7	91.0
Italy	760	27.7	44.9	41.7	32.9	25.5	49.5
Latvia	359	2.9	0.0	12.5	12.5	50.0	50.0
Lithuania	358	59.6	64.6	14.6	8.5	0.8	84.6
Luxembourg	170	89.7	48.3	30.3	13.5	5.6	69.7
Netherlands	808	50.9	43.7	28.1	10.0	29.4	82.1
Poland	1369	60.2	59.0	31.4	25.0	42.1	53.3
Portugal	1062	34.6	44.6	24.9	26.8	19.0	53.5
Slovenia	409	74.3	1.6	8.9	3.7	15.7	94.2
Spain	1051	36.3	11.8	6.0	78.0	5.8	22.8
Sweden	591	97.9	1.4	7.2	0.9	79.9	44.9

Panel A shows firm's reported most needed government measure since the outbreak of COVID-19 (question COV2f3), and Panel B shows the percentage of firms that received any national or local government support, and which support measures the firms received (questions COVf1 and COVf2a-COVf2e) since the outbreak of COVID-19 for the European, OECD-member countries, for which the World Bank Enterprise Surveys (COVID-19 Series) are available. The timing of the surveys varies from country to country.

Table A9: Descriptive Comparison of Our Sample and the Remaining Firms in ORBIS

	Our Sample		Remaining Firms	
	Mean	SD	Mean	SD
Firm Age	16.57	13.23	11.33	13.02
No. Employees	14.36	95.49	5.68	47.14
Total Assets	2027.84	30619.96	1144.92	22389.30
Cash	194.78	1383.29	93.50	1405.68
Operating Revenue	1897.38	14251.81	574.67	6835.88
Net Income	58.68	2847.02	34.70	1113.62
Long-Term Debt	550.62	21775.65	431.82	9017.81
Tangibility	0.28	0.28	0.29	0.32
Leverage	0.31	0.55	0.40	0.79
ROA	2.60	18.16	1.25	24.74
Urban	0.37	0.48	0.40	0.49

This table displays a comparison between the firms in our sample and the remaining Portuguese firms in ORBIS. We present the mean and standard deviation for each variable for each group of firms. All financial variables are reported as of 2019 (year end) and are available from ORBIS.

Appendix Tables B

Table B1: Heterogeneous Effects of the Intervention on Layoff Program Take-Up

	Total Assets				Employees / T. Assets			
	High		Low		High		Low	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.024	0.024	0.050***	0.046***	0.058***	0.047**	0.011	0.017
	[0.022]	[0.021]	[0.017]	[0.017]	[0.018]	[0.019]	[0.021]	[0.021]
Layoff Step-by-step	-0.015	-0.019	-0.025	-0.026	-0.026	-0.014	-0.017	-0.025
	[0.024]	[0.024]	[0.018]	[0.019]	[0.021]	[0.020]	[0.023]	[0.023]
Credit Line	-0.008	-0.003	0.005	-0.003	0.013	0.006	-0.028	-0.024
	[0.024]	[0.022]	[0.019]	[0.017]	[0.018]	[0.017]	[0.023]	[0.022]
Credit Line Step-by-step	-0.023	-0.025	-0.004	0.001	-0.027	-0.018	-0.007	-0.007
	[0.023]	[0.022]	[0.025]	[0.024]	[0.021]	[0.021]	[0.025]	[0.024]
Constant	0.360***	0.357***	0.291***	-0.233**	0.331***	-0.072	0.337***	0.002
	[0.031]	[0.122]	[0.034]	[0.107]	[0.036]	[0.082]	[0.028]	[0.112]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3713	3713	3883	3881	4009	4009	3240	3240
Adjusted R-squared	0.000	0.075	0.001	0.081	0.001	0.113	-0.000	0.045

	Debt / T. Assets				Urban			
	Yes		No		Yes		No	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Layoff	0.039*	0.030	0.022	0.027	0.028	0.020	0.036**	0.041**
	[0.023]	[0.024]	[0.024]	[0.024]	[0.027]	[0.028]	[0.017]	[0.018]
Layoff Step-by-step	-0.042	-0.040	-0.022	-0.026	-0.053**	-0.043*	0.003	-0.010
	[0.031]	[0.030]	[0.030]	[0.032]	[0.026]	[0.026]	[0.019]	[0.018]
Credit Line	-0.012	-0.016	0.017	0.013	0.011	0.013	-0.013	-0.017
	[0.024]	[0.021]	[0.028]	[0.026]	[0.026]	[0.025]	[0.019]	[0.017]
Credit Line Step-by-step	-0.026	-0.015	-0.059**	-0.049**	0.008	0.012	-0.031	-0.026
	[0.029]	[0.027]	[0.024]	[0.025]	[0.023]	[0.025]	[0.021]	[0.020]
Constant	0.363***	0.161*	0.317***	0.071	0.351***	0.129*	0.319***	0.049
	[0.030]	[0.092]	[0.036]	[0.094]	[0.034]	[0.076]	[0.029]	[0.088]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2840	2840	2717	2717	2973	2970	4516	4514
Adjusted R-squared	0.001	0.084	0.000	0.084	-0.000	0.064	0.003	0.086

This table shows heterogeneous effects of the intervention on the Layoff program take-up. Regarding the first 12 columns, firms are split at the median of the distribution of each variable. Financial data is as of 2019 year-end and available from ORBIS. In the last 4 columns, we classify the municipality where the firm operates based on total population. A council is classified as 'Urban' if the total population is higher than 150,000. Out of 308 municipalities, 14 (4.6%) are classified as urban and these correspond to 31% of the total population. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Table B2: Heterogeneous Effects of the Intervention on Layoff Program Receipt (World Bank Sample)

	Since the outbreak of COVID-19, how many workers have been furloughed?[a]				**What percentage of the firm is owned by the same family?**[b]			
	At least one		None		≥ 75		< 75	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.302***	0.298***	0.006	0.013	0.148***	0.152***	0.092	0.082
	[0.099]	[0.106]	[0.027]	[0.028]	[0.056]	[0.057]	[0.121]	[0.137]
Layoff Step-by-step	-0.027	0.054	0.032	0.027	-0.016	-0.019	-0.083	-0.051
	[0.148]	[0.146]	[0.040]	[0.039]	[0.074]	[0.074]	[0.146]	[0.163]
Credit Line	-0.157*	-0.229**	0.003	0.011	0.027	0.005	-0.128*	-0.137*
	[0.093]	[0.100]	[0.025]	[0.030]	[0.056]	[0.057]	[0.068]	[0.070]
Credit Line Step-by-step	0.162	0.221**	0.073*	0.069*	0.043	0.042	0.216**	0.231**
	[0.107]	[0.106]	[0.037]	[0.040]	[0.063]	[0.063]	[0.097]	[0.100]
Constant	0.407***	-0.162	0.018	-0.104	0.113***	-0.437*	0.208***	-0.365
	[0.063]	[0.356]	[0.013]	[0.130]	[0.029]	[0.240]	[0.060]	[0.352]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	175	172	308	298	331	328	148	139
Adjusted R-squared	0.080	0.092	0.009	0.013	0.012	0.033	0.004	0.005

	To what degree is Access to Finance an obstacle to the current operations of this establishment?[b]				**At this time, does this establishment have an overdraft facility?**[b]			
	Major		Minor		Yes		No	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Layoff	0.213***	0.216***	0.042	0.033	0.023	-0.030	0.196**	0.191**
	[0.122]	[0.119]	[0.063]	[0.069]	[0.070]	[0.082]	[0.085]	[0.089]
Layoff Step-by-step	-0.155	-0.119	0.058	0.082	0.009	0.078	-0.062	-0.073
	[0.095]	[0.092]	[0.114]	[0.119]	[0.119]	[0.112]	[0.107]	[0.109]
Credit Line	-0.039	-0.042	-0.039	-0.064	-0.132**	-0.122**	0.045	0.026
	[0.042]	[0.045]	[0.063]	[0.063]	[0.055]	[0.054]	[0.061]	[0.060]
Credit Line Step-by-step	0.162*	0.163*	0.062	0.075	0.143*	0.129*	0.066	0.047
	[0.082]	[0.083]	[0.065]	[0.060]	[0.077]	[0.069]	[0.072]	[0.069]
Constant	0.120***	-0.716***	0.172***	-0.437*	0.218***	-0.533	0.101***	-0.498**
	[0.041]	[0.248]	[0.040]	[0.242]	[0.045]	[0.371]	[0.032]	[0.227]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	213	209	256	248	201	193	277	272
Adjusted R-squared	0.034	0.091	-0.003	0.029	-0.001	0.111	0.018	0.057

This table shows heterogeneous effects of the intervention on the Layoff Program receipt for firms that answer the World Bank Enterprise Surveys. The outcome variable is constructed based on a question that inquires firms about receipt of national and local government support in response to the crisis, specifically the variable COVf2e (wage subsidies). We split the sample according to the answers to different questions. The first question (COVd8) is from the Follow-up on COVID-19 Survey conducted in September and October 2020. The remaining variables are taken from the standard Enterprise Survey (conducted between November 2018 and January 2020). The original variables are BMb1, k30, and k7, respectively. The question "To what degree is Access to Finance an obstacle to the current operations of this establishment?" is a 5-point scale ranging from "No Obstacle" (0) to "Very Severe Obstacle" (4). We split answers into major (2, 3 or 4) or minor obstacle (0 or 1). Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively. Source: [a] Enterprise Surveys Follow-Up on COVID-19: Portugal, Round 1 [Sep – Oct 2020] (The World Bank, 2020); [b] Enterprise Survey [Nov 2018 – Jan 2020] (The World Bank, 2019)

Table B3: Effect on Survival and Number of Establishments

Panel A: Survival (2022)

	Survival					
	All		Applied to Layoff		Did not Apply to Layoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.008	0.007	0.017*	0.019**	0.003	0.001
	[0.005]	[0.006]	[0.009]	[0.009]	[0.007]	[0.008]
Layoff Step-by-step	0.004	0.005	-0.005	-0.006	0.009	0.011
	[0.006]	[0.006]	[0.009]	[0.009]	[0.007]	[0.007]
Credit Line	0.005	0.006	0.011	0.011	0.003	0.004
	[0.005]	[0.005]	[0.008]	[0.008]	[0.006]	[0.006]
Credit Line Step-by-step	-0.002	-0.002	-0.005	-0.003	-0.000	-0.002
	[0.006]	[0.006]	[0.010]	[0.009]	[0.007]	[0.007]
Constant	0.969***	0.869***	0.966***	0.865***	0.971***	0.868***
	[0.003]	[0.018]	[0.006]	[0.037]	[0.004]	[0.021]
Controls	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	7,720	7,716	2,552	2,550	5,168	5,166
R-squared	0.001	0.031	0.001	0.035	0.001	0.032

Panel B: Establishments (2021)

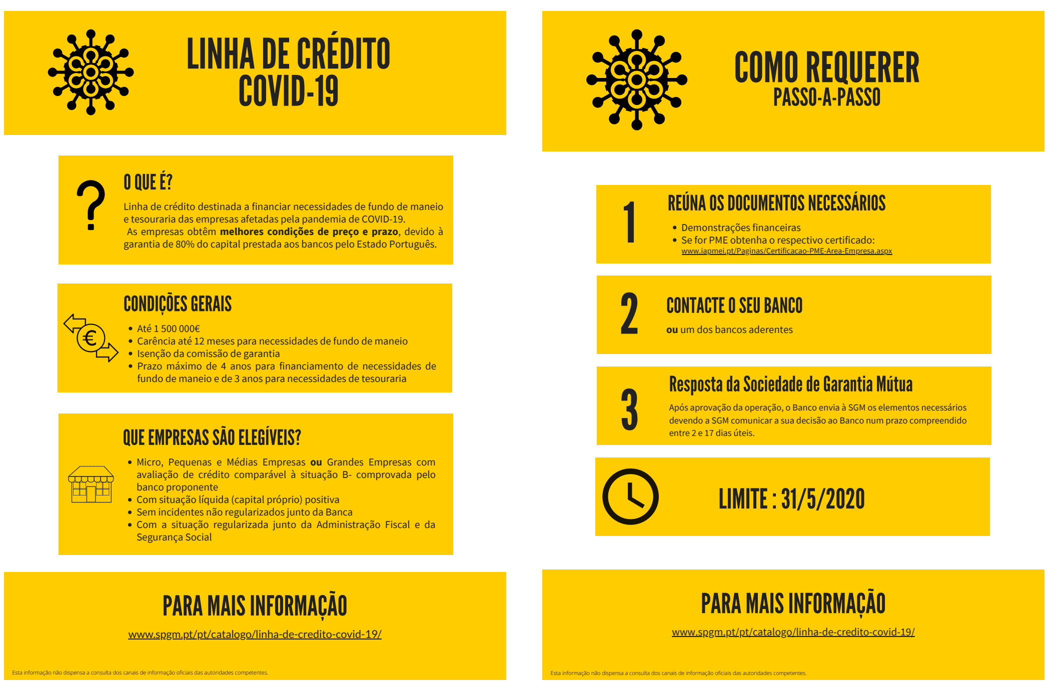
	Establishments					
	All		Applied to Layoff		Did not Apply to Layoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff	0.115*	0.105**	0.232*	0.227*	0.036	0.024
	[0.059]	[0.052]	[0.127]	[0.124]	[0.063]	[0.057]
Layoff Step-by-step	-0.050	-0.036	-0.044	-0.009	-0.044	-0.036
	[0.086]	[0.080]	[0.168]	[0.166]	[0.090]	[0.085]
Credit Line	0.038	0.035	0.199	0.195	-0.044	-0.051
	[0.052]	[0.051]	[0.134]	[0.130]	[0.042]	[0.044]
Credit Line Step-by-step	0.027	0.030	-0.021	0.021	0.054	0.032
	[0.062]	[0.063]	[0.161]	[0.154]	[0.045]	[0.048]
Constant	0.969***	0.869***	0.966***	0.865***	0.971***	0.868***
	[0.003]	[0.018]	[0.006]	[0.037]	[0.004]	[0.021]
Controls	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	5,772	5,770	2,070	2,069	3,702	3,701
R-squared	0.001	0.072	0.003	0.078	0.000	0.086

This table shows the effect of the intervention on survival (until 2022) and the number of establishments (in 2021) on the firms that answered our follow-up survey. Survival data is available from Statistics Portugal (INE) until 2022 and number of establishments until 2021. In the first two columns we present regression results for firms that were active in 2019 and answered the follow-up survey (i.e., those for which we have self-reported information on applications). In the following two columns, we condition on firms that reported to have applied to the layoff measure. In the last two columns, we condition on those that did not apply to the layoff measure. Standard errors clustered at 3-digit industry level are presented in brackets. *, **, *** Significance at 10, 5 and 1%, respectively.

Appendix Figures A

Figure A1: Information Presented to the Treatment Groups

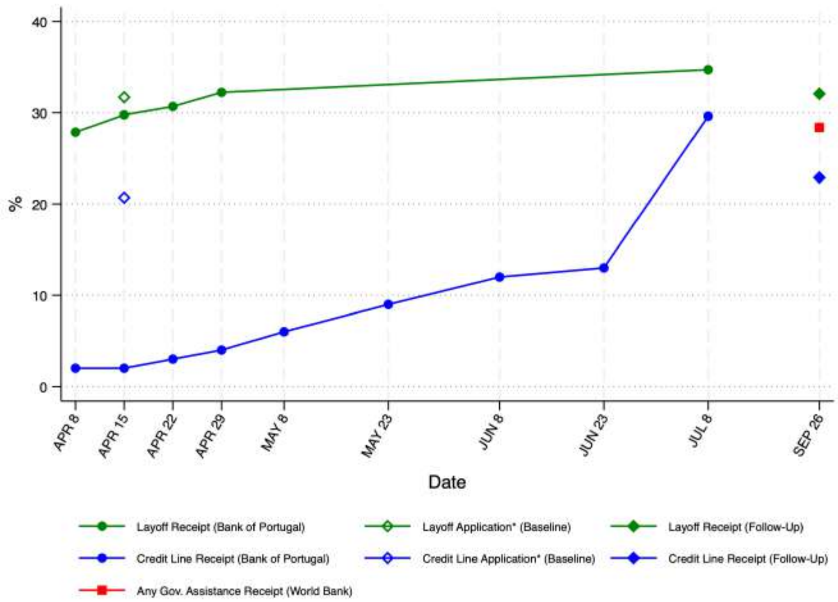
(a) Layoff Treatment



(b) Credit Line Treatment

This figure displays the information presented to the treatment groups (in Portuguese). The sub-figure on the left shows the first intervention tier composed of a brief summary of the policy, a simplified description of the benefits and eligibility conditions, and links to official government resources. The sub-figure on the right shows the step-by-step application guide (second tier).

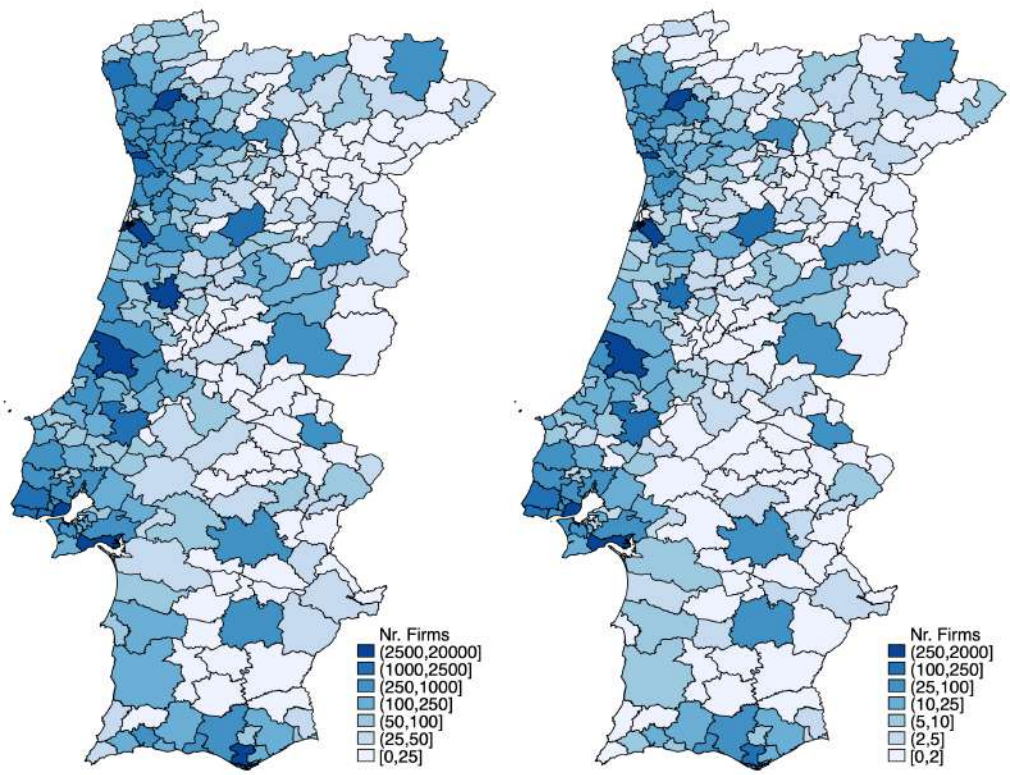
Figure A2: Program Take-up and Receipt Figures From Several Sources



This figure documents the percentage of firms that report having applied to or received the layoff and credit line support programs over time according to different sources: (i) Bank of Portugal/Statistics Portugal, (ii) our surveys, and (iii) World Bank.
(i) In solid lines, we show the percentages estimated based on the different rounds of the "Fast and Exceptional Enterprise Survey - COVID-19" survey conducted by Bank of Portugal/Statistics Portugal. This survey was conducted on a weekly basis between April 6th and May 1st, and once every two weeks between May 2nd and July 15th. Although the survey directly asks firms if they benefited from the credit line, regarding the layoff program it only asks whether layoff is one of the main reasons why firms report a lower number of employees in the weeks before. Thus, we construct this series using the percentage of firms that answer that layoff is one of the main reasons over the total number of firms that did not do so or did not report a reduction in the number of employees in the previous weeks. We cannot construct this series from May to June due to a change in the format of the question.
(ii) In hollow diamonds, we show the results of our baseline survey conducted in the first two weeks of April. Notice that in the baseline survey we asked about program application (not about program receipt). In solid diamonds, we show receipt rates based on our follow-up survey, conducted in the last two weeks of September.
(iii) We display the percentage of firms that reported having received at least one government measure in the first round of the "Enterprise Surveys Follow-up on COVID-19" by World Bank and available here (`https://www.enterprisesurveys.org/content/dam/enterprisesurveys/documents/covid/country-profile-Portugal_English.pdf`). This round took place in September and October 2020. We display the results from our follow-up survey and the World Bank survey in the same date as they broadly overlap in time.
*Program application.

Figure A2 compares application and receipt rates for the layoff and credit line assistance measures in our sample to other surveys conducted during the same period. Starting from the first week of April, the Bank of Portugal conducted weekly surveys to monitor the impact of the COVID crisis in real time. During the second week of April, when our baseline survey was conducted, the Bank of Portugal reported that 30% of firms had received the layoff support measure. Our findings are consistent with this, as 32% of firms in our baseline survey reported applying for the layoff program. Conversely, by mid-April, the Bank of Portugal noted that only 2% of firms had received benefits from the credit line scheme, whereas our survey indicated that 21% of firms had applied. This suggests that the approval process for the credit line scheme was relatively slow initially. Nevertheless, these applications seem to have eventually translated into receipt of benefits. Well over 20% of firms reported receiving benefits by the final Bank of Portugal survey rounds in July 2020. The results from our follow-up survey, conducted in September 2020, are in line with these later figures.

Figure A3: Number of Firms in Each Stage, by Municipality

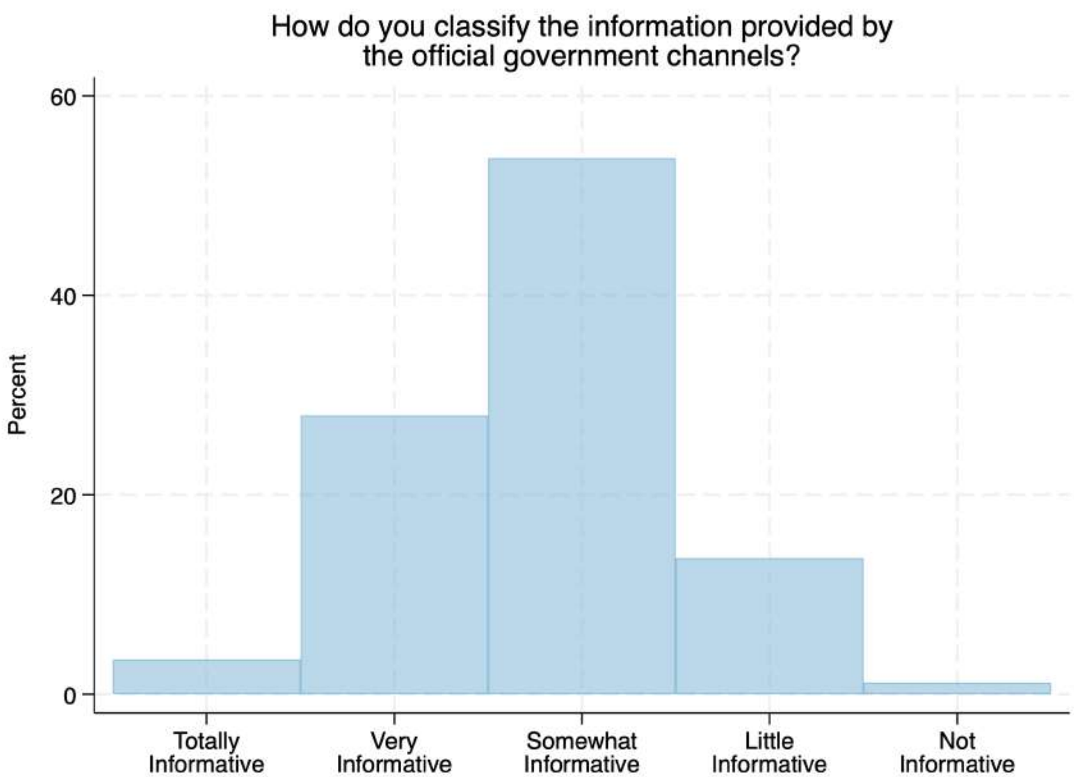


(a) Full Sample

(b) Follow-Up Respondents

This figure shows the number of firms by municipality. We show the number of firms in the full sample on the left and the number of firms that answered the follow-up survey on the right. We omit Azores and Madeira Archipelagos from the picture.

Figure A4: Baseline Survey: Quality of the Government Information



This figure shows the baseline perception of the quality of the information provided by the government regarding the measures on a scale from totally informative to not informative. The survey question is "*How do you classify the information provided by official government channels about the measures?*". This question was presented only to respondents who reported having actively searched for information in the government official channels in a previous question. See Table A2 for the corresponding number of observations.