# 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 simplified information about a program, or a combination information and step-by-step application support. We find a significant treatment effect on take up of the wage support program. 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 small and medium sized firms struggling with the consequences of economic and financial crises.[1] For example, nearly 800 billion dollars in loans have been approved through the US's Paycheck Protection Program in the wake of the COVID-19 pandemic.[2] Despite the scale of these policies, there is substantial concern over their effectiveness in reaching targeted firms (Granja et al., 2020, 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 accessing government support measures using an encouragement based randomized controlled trial. We consider the impact of providing detailed information on two COVID-19 assistance measures—a layoff support program and a guaranteed credit line scheme—on firm take-up using a sample of over 170,000 Portuguese firms. We randomly assigned firms to a treatment providing either simplified information regarding one of the assistance measures (including a short program description, eligibility criteria, and a link to official government resources), or a combination of information and step-by-step application support.

We find evidence that our low-cost intervention, which consisted of targeted emails to firm representatives, had a meaningful impact on program application. However, our results also suggest that this effect is not homogeneous across the measures we study. Firms supplied with information about the layoff support program, which had a simple application process operating through a widely used social security website, were significantly more likely to apply when compared to a control group. Firms supplied with equivalent information about the credit guarantee program, a more complex scheme that required borrowers to individually arrange terms with a bank (and was subject to an array of sector specific features and formal requirements), were no more likely to apply.

Specifically, we estimate that firms provided with information about the layoff support measure were 2-3 percentage points more likely to submit applications to the program (relative to a mean of just over 30 percent). This increase in applications coincided with the timing of our treatment: we observe a sharp

[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).

increase in applications in April, the month of our intervention, and do not find significant treatment effects in subsequent months. In addition, 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. We also see no incremental effect of providing supplemental step-by-step application support on top of the informational treatment. We find consistent effects on program receipt in our sample and when using an independent survey conducted by the World Bank (The World Bank, 2020).

We complement our experimental design with survey evidence on firm awareness of government support measures and perceived application costs. Our survey allows us to gauge baseline knowledge of the measures we study and provides insight into the mechanisms through which our treatment operates. 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 support measures, but that most lacked a comprehensive understanding of the programs.

Our results indicate that information frictions act as a meaningful barrier to distribution of firm-level government support measures. This echos a long 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 also in line with recent survey evidence that suggests that small businesses identify bureaucratic hassle and uncertainty about eligibility as potential barriers to accessing COVID-19 aid (Bartik et al., 2020, Humphries 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 behavioral frictions or heterogeneity in access to information. 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.

Heterogeneity analysis indicates that our intervention had a greater impact on the types of firms that we expect either faced greater information frictions or stood to benefit most from enrolling. In particular, the effect of information provision on application to the layoff support measure is concentrated in (i) smaller firms, (ii) those with less cash, (iii) those with more leverage, and (iv) those with a large number of employees relative to assets. Small firms are plausibly the ones for which information frictions are the highest because

their managers may be less sophisticated or because they have fewer resources to devote to learning about or applying to programs. Firms with lower cash holdings, high leverage, and a high labor share are most likely to benefit from the program because it provides direct access to cash and is explicitly aimed at subsidizing labor costs. We complement this analysis by exploiting additional firm information from the World Bank Enterprise Survey Follow-up on COVID-19. Using this data we find more pronounced treatment effects in family firms and firms that are not affiliated to business organizations such as trade associations or a chamber of commerce. Arguably these are firms with less sophisticated management practices. In addition, these business organisation often provide information on programs that may be of interest of its members and for this reason unaffiliated firms are plausibly less informed. Overall, these heterogeneous treatment effects support the interpretation that information frictions act as an impediment to the efficient targeting of COVID-19 relief.

There are several mechanisms through which the estimated effect may operate. It may be that any information provision has a marginal impact on the application probability, and that our intervention had an effect—despite the fact that the Portuguese COVID-19 relief measures were covered in the media—simply because it provided an additional piece of information. Alternatively, it may be the case that specific features of our intervention, e.g. the targeted nature of the emails or the fact that they were provided by an academic source, are the source of the impact. For example, if there is a lack of trust in government sources or some stigma discouraging take-up that is alleviated through information provided by an (arguably) objective body. While we remain agnostic about the specific mechanism, the basic takeaway is that a low-cost email based information campaign can have non-trivial impacts on firm take-up. Moreover, the heterogeneous effects we estimate and our survey evidence are consistent with meaningful new information being delivered by the treatment. This suggests that procrastination and limited attention (Banerjee and Mullainathan, 2008) are not the sole mechanisms preventing effective targeting of firms.

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 our experimental design and implementation. In section 4 we describe our data and provide summary statistics. In section 5 we describe our results. In section 6 we present a discussion of our results, 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 *Banco de Portugal* in the second week of April (roughly the time of our intervention) showed that 80% of the respondent firms had experienced

a decrease in revenues and that 18% had permanently or temporarily shut down (Banco de Portugal, 2020).

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.[4] 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.[5]

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%. Any firm was eligible (regardless of size), so long as it (i) had experienced a reduction in revenue of 40% or more,[6] (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 benefits or during the subsequent two months. The program is 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 program provided lenders with a minimum government-backed guarantee of 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% for loans with maturity of over 3-years. Capital and interest payments did not start until 1 year after the 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 Social Security. 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 accepted automatically through an online portal, the credit

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

[5] 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, and a moratorium on existing bank-credit liabilities.

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

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. To apply, firms were required to contact a Portuguese commercial bank directly. 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 intervention provided simplified information about the governmental policies implemented during the COVID-19 crisis to firms.[7] We randomly exposed managers of potentially eligible firms to information about either the layoff support program or the guaranteed credit line scheme. To receive benefits 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).

Our experimental design contained two treatment tiers. The first tier was a summary of the policy, including a simplified description of benefits and eligibility conditions, and links to official government resources. The second tier consisted of a step-by-step application guide, walking potential applicants through the key features of the process. Appendix Figure A1 shows the information presented for each tier of the intervention for both the layoff support program and the credit line scheme.[8]

One third of the sample was randomly assigned to receive information about the layoff program, one third was assigned to receive information about the credit line scheme, and the remaining one third was assigned to a control group. Among those assigned to each policy, one half received only the first tier of our treatment (summary information) while one half received both the first tier and the second tier (a step-by-step application guide). Hence, the experimental design consists of four treatment arms (with one sixth of the sample each) and one control group (one third of the total sample). We performed a stratified randomization at the three-digit industry level of the Portuguese *Código de Actividade Económica*.

Alongside the informational treatment, one-fourth of all firms in each arm were sent a baseline survey. 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. In the case of

[7] 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.

[8] The information was prepared by the researchers using official government sources and was presented in Portuguese.

treatment groups, the survey was presented to the respondents before the intervention.

The information was distributed 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 assigned to receive the 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 information on the layoff policy was delivered on April 8th, 2020, while the intervention for recipients of the credit line policy 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 with all the firms in our sample in the last two weeks of September 2020, approximately 6 months after the initial intervention. We sent a single reminder to firms that did not respond to the follow-up in the first two weeks of October. The survey was also delivered via email using Qualtrics. The primary aim of the follow-up was to collect information on the outcome of interest: whether and in which month firms applied to the layoff or credit line government measures.[9] The follow-up survey made no reference to the treatment.

Given our experimental design, our primary specification is a linear probability model for each of the two measures $m$:

$$Y_i^m = \alpha + \beta_1 \times \text{Treatment}_i^m + \beta_2 \times \text{Step by Step}_i^m + \varepsilon_i^m. \tag{1}$$

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 $\text{Treatment}_i^m$ takes the value of 1 if the firm was assigned to receive information about measure $m$. The term $\text{Step by Step}_i^m$ takes the value of 1 if the firm was assigned to receive the step-by-step application guide in addition to the basic simplified information.

[9] The list of applicants and recipients of the government measures has not been publicly disclosed.

# 4 Data

Our initial sample consists of firms headquartered in Portugal with a publicly available email address in the ORBIS database.[10] We exclude listed companies, companies in the financial sector or under public administration (including education, social care, and arts and sport activities), non-domestic firms, and companies with a missing industry identifier. Our final sample consists of 172,890 firms.[11]

In column 1 of Table 1 we display the number of firms assigned to each treatment group. There are 57,623 firms in the layoff group, 57,628 in the credit line group and 57,639 in the control group. In the following columns, we report the response rate in the follow-up survey. The response rate, shown in the columns labeled "Follow-up Respondents" is nearly identical across the three main treatment arms at roughly 5.8%. Given this similarity, there is no statistically significant difference in participation across initial treatment assignments (see the columns labeled "Follow-up Orthogonality").

The different treatment arms are also balanced in terms of observable characteristics. In Table 2, we show that the layoff and credit line treatment groups do not differ in a statistically significant way from the control group in terms of size (total assets or number of employees), cash holdings, operating revenue, net income or leverage. The average firm across the three groups has around 2.5 million EUR in total assets and 15 employees. Appendix table A1 shows that there is similarly balance on observable characteristics between treatment and control groups among respondents to the follow-up survey.[12] Table A2 shows summary statistics for all the questions in the baseline and follow-up surveys. The survey was mostly answered by a manager, CEO or director of the firm (77%).

In Figure A2 we show the geographic distribution of firms in the full sample as well of those that responded to the follow-up survey. Participating firms are spread across the country and the geographical distribution of firms that answered the follow-up survey 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. The link to merge the data was provided by the European Bank for Reconstruction and Development (EBRD) through a confidentiality agreement.

[10] We also rely on ORBIS for data on fundamentals. All financial data presented corresponds to 2019 year end.

[11] In the intervention stage, we sent 126,792 emails of which 20,133 bounced back. In the follow-up stage we sent 172,890 emails, of which 29,314 bounced back. This corresponds to 84% and 83% of emails being delivered successfully in each stage, respectively.

[12] The number of observations is also balanced within the 225 three-digit industries.

# 5 Results

In this section we present the results from our intervention: the estimated impact of our information based treatment on reported program take-up.

## Layoff Support

Panel A of Table 3 shows the impact of our intervention on application to the layoff support program. In columns (1)-(5) the outcome variable is an indicator for an affirmative response to the question *Have you applied to the layoff support measure?* in our follow-up survey. Our sample in these specifications is limited to those who responded to the follow-up.

We find a consistently positive and significant impact of the treatment on take-up. Column (1) compares the sample that received information about the layoff measure against the control group (who received no treatment). The estimated coefficient is 0.023, indicating that treated firms are 2.3 percentage points more likely to report applying to the layoff support measure (relative to a control group mean of just under 32 percent). In column (2), we compare the sample that received information about the layoff support measure to all other follow-up survey respondents (including the control group and those who received information about the credit line measure). The estimated coefficient is 0.031, indicating that treated firms are 3.1 percentage points more likely to apply to the layoff support measure. This coefficient is significant at the 1 percent level, while the coefficient in column (1) is significant at the 10 percent level.

In columns (3) and (4) we consider the additional effect of providing more detailed information on the application process. In these specifications we repeat the analysis in columns (1) and (2) but include an additional indicator (*Step by Step*) equal to one for the subset of the treated group that received step-by-step application instructions. In both columns, the coefficient on this indicator is small and not statistically significantly different from 0. This suggests that providing step-by-step instructions did not increase take-up further. The coefficients on our basic treatment are effectively unchanged relative to columns (1) and (2).

Column (5) shows a placebo test comparing those who received information about the credit line measure to the control group. The coefficient is negative and insignificant. This suggests that receiving information about credit lines did not increase the probability of applying to the layoff support program.

## Credit Lines

Panel B of Table 3 shows the impact of our intervention on application to the credit line measure. In columns (1)-(5) the outcome variable is an indicator for an affirmative response to the question *Have you applied to the credit line guarantee scheme?* in our follow-up survey. Our sample in these specifications is again limited

to those who responded to the follow-up.

We see no evidence of a significant impact of the treatment on application to the credit line guarantee scheme. There is no significant difference in reported application rates when comparing those who received information about the credit line guarantee to the control group (column (1)), or when comparing them to all other respondents in the follow up survey (column (2)). In columns (3) and (4) we repeat the analysis in columns (1) and (2) but include an additional indicator for the subset of the treated group that received step-by-step application instructions. The estimated coefficients on this indicator are positive but small and not statistically significant. We also find no significant relationship in column (5), which shows a placebo test comparing those who received information about the layoff measure to the control group. Receiving information about the layoff measure did not significantly increase the probability of applying to the credit line guarantee scheme.

## Selection into the Follow-Up Survey

One potential issue is that our measure of program application is self reported and based on a follow-up survey with imperfect compliance. While administrative data on program application would be ideal, we present three pieces of evidence suggesting our survey based measure is not a cause for concern.

First, we are able to verify our results in an independent survey conducted by the World Bank: the World Bank Enterprise Survey Follow-up on COVID-19. This survey, which we were able to merge to our sample, includes questions about receipt of various COVID-19 support programs. Because receipt may differ slightly from program application, columns (1) and (2) of Table 4 repeat the analysis in Table 3, but consider the outcome of program receipt as reported in our survey. The results are virtually identical to those for applications. This is perhaps unsurprising given that layoff support was processed automatically for eligible firms and the success rate for applications was reported to be 96% (see Table A2). Columns (3) and (4) confirm that results are consistent in the World Bank survey data. For the layoff support measure, the estimated coefficients are positive, statistically significant, and somewhat larger than those in our survey, at 0.092 and 0.111, respectively. We again see no significant results for the credit line measure. Because this survey had no relationship with our research project, we believe it is unlikely that our treatment and subsequent take-up of the layoff program influenced participation in the World Bank Survey.

Second, there is no significant difference in follow-up survey participation across our treatment and control groups. As Table 1 shows, effectively identical fractions of the control group, the layoff support group, and the credit line group responded to the survey (5.82%, 5.74% and 5.79%, respectively). There is similarly no statistically significant difference in baseline observable characteristics between treatment and

control follow-up respondents (see appendix Table A1).

Third, there is evidence of a significant treatment effect even in a very general intent to treat framework. In the final column of Table 3, we consider an indicator equal to one for those who answered our survey question on application affirmatively, and 0 for all others (including those who did not respond to the treatment). This represents the joint outcome of (i) responding to the survey and (ii) responding affirmatively. By design, this variable is observed for all individuals, not just those who responded to the follow-up.

In panel (A) we see a positive and significant (at the 10 percent level) coefficient on this indicator. This mirrors the finding of a positive impact among our follow-up respondents. Our estimate suggests that receiving our layoff information increased the probability that participants reported applying to the layoff measure by more than 0.1 percentage points (versus reporting that they had not applied or not responding whatsoever). While this approach is very conservative, and almost certainly under-counts the number of actual applications, it confirms that our intervention had an impact on treated firms. The last column of Panel (B) repeats this analysis for the outcome of application to the credit line guarantee scheme. We find no evidence that treatment increased the probability that a firm reported applying to the measure, again mirroring the results when considering only the sample of follow-up respondents.

## Heterogeneity Over Time and Across Firms

We next turn to exploring heterogeneity in our results over time and across firm characteristics. These analyses serve two purposes. First, the time and firm level heterogeneity both fortify our baseline results by showing that the impact of treatment is concentrated in the time periods and set of firms we would expect ex-ante. Second, firm level heterogeneity supports the assertion that information frictions hinder access to aid for the those that plausibly benefit most.

### Heterogeneity Over Time

Figure 1 shows that the effect of our treatment is concentrated in April 2020, the month of our intervention. This figure plots coefficients on the layoff treatment variable ($\text{Treatment}_i^{\text{layoff}}$) from versions of the specification in Equation 1 in which the outcome is equal to one if the firm reported applying to the layoff measure in a specific month (e.g. April, May, or June, 2020) and 0 otherwise. This specification is estimated separately for each month. Firm responses come from our follow-up survey, which was conducted in September and October of 2020. The sample is restricted to follow-up survey respondents and excludes those who reported applying prior to our intervention. We find a point estimate of 2.5 percentage points in April, significant at 95% confidence level. We do not find meaningful effects in the subsequent months. We present these results in table form in appendix Table A3.

**Heterogeneity Across Firms**

In Table 5 we exploit information on firm characteristics to study heterogeneity in our estimated treatment effects. We consider four dimensions of heterogeneity: (1) firm size (total assets) (2) labor intensity (employees/total assets) (3) cash holdings (cash/total assets) and (4) leverage (debt/total assets). Each of these variables represents a dimension that we expect might make firms particularly sensitive to our intervention. Specifically, we hypothesize that small firms might face greater information frictions (and so may be particularly impacted by our intervention). Alternatively, we expect that firms with high labor intensity, with low levels of cash, and with high leverage stand to benefit most from the measures, and hence might be most likely to respond to our interventions by applying.

Panel A shows results for the layoff program. As hypothesized, we find larger coefficients for small firms, firms with high labor intensity, firms with low levels of cash and firms with high leverage (although the point estimates above and below the median are not, in general, statistically different). Small firms treated with information about the layoff program were 5 percentage points more likely to apply than other small firms. Similarly, we see treatment effects of 5.6 percentage points for labor intensive firms, 4.8 percentage points for low-cash firms, and 5.5 percentage points for high leverage firms. Our results are consistent with Core and De Marco (2021) who find that firms with less cash on hand and with higher leverage exhibit higher take-up rates in the context of Covid-19 government measures. Panel B 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 in any of the sub-samples we consider.[13]

In Table 6 we use data from the World Bank Enterprise Survey Follow-up on COVID-19 to estimate heterogeneous effects on layoff program receipt using additional firm characteristics. Columns (1) and (2) show a significant treatment effect for firms that report having closed temporarily during the COVID-19 outbreak but not for those that remained opened. This is expected, as the program specifically targeted firms that had to close temporarily by law or that had a significant reduction in revenue due to lockdowns. Columns (3) and (4) show a significant treatment effect for firms that reported at least one furloughed worked, but not for other firms. This is also expected as the furlough of workers was one of the main aims of the layoff program. These results, although unsurprising, offer consistent evidence of a meaningful treatment effect of our intervention. In columns (5) and (6) we see heterogeneous treatment effects for family and non-family firms. We define family firms as those in which the same family owns more that 75% of the firm. The point estimates are marginally larger for family firms sub-samples (0.123 and 0.100, respectively), and the

[13] 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.

treatment effect is only significant for family firms (although the two effects are not statistically different). We conjecture that family firms are smaller, and presumably less sophisticated in terms of management practices, and thus we may expect more pronounced informational frictions.

In columns (7) and (8), we consider effects for firms that are part of a business membership organization, trade association, guild, or chamber of commerce, who we expect to be more informed about relief measures, compared to those without any affiliation. We find significant and more pronounced treatment effects for non-affiliated firms. Non-affiliated firms treated with information about the layoff program were 16.3 percentage points more likely to receive the layoff support compared with untreated non-affiliated firms. In columns (9) and (10) we see a significant treatment effect of our intervention for firms that have performance bonuses for managers, but not for other firms. Managers that receive performance contingent bonuses have arguably greater incentives to take action and apply to government support under the assumption that this will likely benefit firm performance.

Our final tests using World Bank data focus on measures of financial constraints and access to external financing. Column (11) shows a sizeable and significant treatment effect for firms that report having major obstacles in access to financing. We see a smaller and insignificant effect for firms that report only minor obstacles. Similarly, columns (13) and (14) show no significant effect for firms with access to overdraft facilities, but large and statistically significant effects for those without such access. Columns (15) and (16) show a more pronounced treatment effect for firms without a line of credit. These results are consistent with our previous evidence that firms with liquidity needs—who likely benefit most from layoff support—were more likely to apply to the layoff program as a result of our intervention.

# 6 Discussion

In this section we discuss the magnitude and interpretation of our results as well as potential channels through which our estimated effects may 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 3.3 percentage points more likely to report applying to the layoff support measure. There is little experimental evidence on information interventions targeting firms that provide comparable estimates to allow us to benchmark our main effects. One exception is Duflo et al. (2011), who find no effect of reminding farmers to use fertilizer in their crops when this usage is considered

advantageous for the farmer. Similarly, Breinlich et al. (2017) find no effects of information provision about the costs and benefits of exporting on firms' decision to export, despite significant changes in their perceptions about such costs and benefits. These results stand in contrast to similar information interventions targeting individual level financial decision-making. For instance, Hotard et al. (2019) find that a simple and low cost information nudge increased the rate of citizenship applications by 8.6 percentage points 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. In other words, our estimates are larger than the null effects found in some firm-level interventions, but the magnitudes are plausible when compared to broader informational interventions.

## Mechanisms

There are several mechanisms through which our main effect might operate. In general, it may be that any information provision has a marginal impact on application probability. In other words, that our intervention had an effect despite the wide coverage of COVID-19 relief measures in the Portuguese media simply because it provided an additional unit of information. This includes the possibility that due to limited attention or procrastination (see Banerjee and Mullainathan, 2008, Duflo et al., 2011, Ponce et al., 2017) our intervention might provide a reminder of the program without conveying new information. Figure 2a shows that, according to our baseline survey, a very small fraction of managers were totally unaware of the layoff program, with only 11.3% reporting no knowledge of its existence at the time of the intervention. Figure 2b also shows that 55% of survey respondents actively searched for information regarding the layoff program. At the same time, few respondents report having high or very high knowledge of the layoff program prior to our intervention (see Figure 3a). This is consistent with the possibility that there was meaningful information delivered by the treatment, and that procrastination and limited attention were not the sole mechanisms in place. Moreover, more pronounced treatment effects for small firms, family firms, and firms that are not affiliated to a business organization are consistent with these firms having more severe information frictions, which is highly plausible.

It maybe also be that specific features of our intervention are 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 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. A third option is that the time and mode of information provision was 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 10am), which might induce responsiveness. A fourth possibility is that the directed nature of our intervention was crucial. The fact that our intervention 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.[14]

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

## Heterogeneous 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. However, firms supplied with equivalent information about the credit guarantee program were no more likely to apply.

Our baseline tests cannot reject the hypothesis that the credit line program was 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. In addition, Figure A3 shows that less than 40% of firms perceive the information provided by the government to be "Very Informative" or "Totally Informative". It may also be that banks, having an active role in the selection and application processes, act as information providers. In this case, our information could be redundant for the firms that are more likely to apply.

An alternative explanation is that other frictions or binding constraints prevent the information treatment from effectively increasing applications to the credit line program.[15] One reason for this could be the complexity of the application process, including the role of additional parties (in this case, banks). While the treatment provided across the two programs was very similar, and both programs targeted the short term liquidity needs of firms, the application process was very different. The layoff program had a simple

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

[15] A survey conducted by *Banco de Portugal* in the second week of April shows that only 2% of the respondent firms had applied to the credit line scheme at that time, and around 60% did not expect to apply at all (Banco de Portugal, 2020).

application process operating through a widely used website. The credit line program involved a more complex process requiring borrowers to arrange terms with a partner commercial 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.

Debt aversion might have also played 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. (2020) 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 financial equivalent alternative not labeled as such.

A final explanation for the heterogeneous 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 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.

Overall, the difference in treatment effects seems to be associated with greater application complexity for the credit line program, which may be a binding constraint.

## External Validity

A natural question is the extent to which our findings may generalize to other programs or contexts. There are at least three relevant margins to consider. First, it may be the case that follow-up respondents (for which we observe the key dependent variable) were differentially impacted by our treatment relative to the full sample. However, recall that there is no statistically significant difference in follow-up response rates between treatment and control groups (see Table 1), despite the fact that the majority of firms in the control group received no email whatsoever at the treatment stage. Furthermore, we see no meaningful difference in observable characteristics between the full sample (shown in Table 2) and follow-up respondents (shown in Appendix Table A1). Given this, we see no evidence to suggest that our sample of respondents is selected in a material way.

A second concern is the representativeness of our full sample relative to all firms in Portugal. In Appendix Table A4 we present a direct comparison of the firms in our sample to all remaining Portuguese firms in the

ORBIS data (consisting primarily of firms for which ORBIS does not provide an email address). Excluded firms are notably smaller on all dimensions: the average value of assets and number of employees are both less than 50 percent of the corresponding averages in our sample. This suggests that our sample does not perfectly capture the Portuguese economy as a whole, but likely captures a large 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 in the excluded sample.

A third a final consideration is a comparison of the Portuguese context to other countries around the world. Portugal is a high income country, a member of the EU and the Eurozone, and has a robust small business sector.[16] However, it lags behind top OECD countries in terms of productivity and per-capita GDP.[17] 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 not perfectly representative of the US context.

# 7 Conclusion

This paper tests whether informational frictions prevent firms from accessing government stimulus programs such as the ones launched during the COVID-19 pandemic. Using a randomized 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.

The effect on take-up for the layoff program is economically relevant. Upon receiving information, treated firms were 2.3 to 3.3 percentage points more likely to apply. Our results are more pronounced for smaller firms, and for other types of firms that we would expect to benefit from the program: labor intensive firms, those with limited cash holdings, and those with high leverage. Despite reasonable public awareness and media coverage of government stimulus programs, our results suggest that low cost interventions, such as targeted emails to firms, can improve take-up rates for the firms that arguably benefit most.

Our results clearly show a causal effect of providing firms with information on the take up of government support programs, further research is needed to understand the real effects of such treatments. The variation created by our intervention will provide a baseline to investigate important real outcomes, such as employment and firm survival, once appropriate longer run data are available.

[16] 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.

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

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# 8 Tables

Table 1: Randomization Counts

| | Treatment Arm | Follow-up Respondents | | Follow-up Orthogonality | |
|---|---|---|---|---|---|
| | Obs. | Obs. | % | Coef. | P-value |
| Layoff | 57623 | 3307 | 5.74% | -0.001 | 0.552 |
| Credit Line | 57628 | 3334 | 5.79% | -0.000 | 0.797 |
| Control | 57639 | 3355 | 5.82% | - | |
| Total | 172890 | 9996 | 5.78% | - | |

This table displays the number of observations assigned to each treatment arm as well as the number of respondents in the follow-up stage. In the last two columns we test orthogonality between participation in the follow-up survey and assignment to treatment. We present the coefficient and p-value of a regression of a binary variable equal to one if the firm participated in the follow-up on each treatment binary variables. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 2: Descriptive Statistics

| | Layoff | | Credit Line | | Control | | Treat. vs. Control | P-value |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | | |
| Total Assets | 2.44 | 49.11 | 2.52 | 56.16 | 2.53 | 48.90 | -0.05 | 0.89 |
| Nr. Employees | 14.98 | 115.68 | 15.79 | 123.95 | 15.14 | 113.29 | 0.24 | 0.76 |
| Cash | 0.22 | 3.40 | 0.21 | 1.72 | 0.21 | 2.88 | 0.00 | 0.90 |
| Operating Revenue | 2.24 | 29.66 | 2.24 | 23.79 | 2.32 | 30.18 | -0.08 | 0.65 |
| Net Income | 0.07 | 5.15 | 0.08 | 1.69 | 0.00 | 13.17 | 0.07 | 0.19 |
| Long-term Debt | 0.82 | 37.56 | 0.56 | 12.85 | 0.74 | 22.69 | -0.05 | 0.81 |

This table displays descriptive statistics. 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. All financial variables are reported as of 2019 (year end) and are available from ORBIS. Variables are reported in millions of Euros (except the number of employees). *, **, *** Significance at 10, 5 and 1%, respectively.

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

| **Panel A: Applied to Layoff** | Follow-Up Respondents | | | | | All |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Layoff vs. Control | 0.023* | | 0.025* | | | |
| | [0.012] | | [0.015] | | | |
| Layoff vs. (Credit + Control) | | 0.031*** | | 0.033** | | 0.001* |
| | | [0.010] | | [0.013] | | [0.001] |
| Layoff step-by-step | | | -0.004 | -0.004 | | |
| | | | [0.017] | [0.017] | | |
| Credit vs. Control | | | | | -0.015 | |
| | | | | | [0.012] | |
| Constant | 0.317*** | 0.309*** | 0.317*** | 0.309*** | 0.317*** | 0.017*** |
| | [0.008] | [0.006] | [0.008] | [0.006] | [0.008] | [0.000] |
| Observations | 6149 | 9239 | 6149 | 9239 | 6205 | 172890 |
| Adjusted R-squared | 0.000 | 0.001 | 0.000 | 0.001 | 0.000 | 0.000 |

| **Panel B: Applied to Credit Line** | Follow-Up Respondents | | | | | All |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Credit vs. Control | 0.003 | | -0.004 | | | |
| | [0.011] | | [0.014] | | | |
| Credit vs. (Layoff + Control) | | -0.002 | | -0.009 | | -0.000 |
| | | [0.010] | | [0.013] | | [0.001] |
| Credit Step-by-step | | | 0.015 | 0.015 | | |
| | | | [0.016] | [0.016] | | |
| Layoff vs. Control | | | | | 0.009 | |
| | | | | | [0.011] | |
| Constant | 0.252*** | 0.256*** | 0.252*** | 0.256*** | 0.252*** | 0.013*** |
| | [0.008] | [0.006] | [0.008] | [0.006] | [0.008] | [0.000] |
| Observations | 5836 | 8696 | 5836 | 8696 | 5792 | 172890 |
| Adjusted R-squared | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |

This table shows the effect of the intervention on program take-up (layoff in panel A and credit line in panel B). From column 1 to 5, we present regression results for the participants who answered the follow-up (those that we have self-reported information on applications). In column 6 we extend the analysis to all firms, coding those that did not reply to follow-up survey as 0. *, **, *** Significance at 10, 5 and 1%, respectively.

Table 4: Effect of the Intervention on Program Receipt

| **Panel A: Layoff** | Follow-up | | World Bank | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Layoff vs. (Credit + Control) | 0.028*** | 0.030** | 0.092** | 0.111** |
| | [0.010] | [0.013] | [0.039] | [0.052] |
| Layoff step-by-step | | -0.004 | | -0.036 |
| | | [0.017] | | [0.066] |
| Constant | 0.294*** | 0.294*** | 0.161*** | 0.161*** |
| | [0.006] | [0.006] | [0.021] | [0.021] |
| Observations | 9182 | 9182 | 483 | 483 |

| **Panel B: Credit Line** | Follow-up | | World Bank | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Credit vs. (Layoff + Control) | -0.000 | -0.004 | 0.026 | 0.010 |
| | [0.009] | [0.012] | [0.028] | [0.036] |
| Credit Step-by-step | | 0.008 | | 0.030 |
| | | [0.015] | | [0.045] |
| Constant | 0.208*** | 0.208*** | 0.035* | 0.086*** |
| | [0.005] | [0.005] | [0.018] | [0.017] |
| Observations | 8567 | 8567 | 483 | 483 |

This table shows the effect of the intervention on program receipt (layoff in panel A and credit line in panel B). The first two columns are based on the firms that answered our follow-up survey, specifically the questions related to program approval. The outcome variable is one when a firm reports to have benefited from the program, and zero when it did not apply or it was not approved. In the last two columns, we show results using the World Bank Enterprise Surveys Follow-Up on COVID-19: Portugal, Round 1 conducted in September and October 2020. We merge the individual answers to this survey with our sample. 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 variables COVf2e (wage subsidies) in panel A and COVf2c (access to new credit) in panel B. *, **, *** Significance at 10, 5 and 1%, respectively.

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

| **Panel A: Applied to Layoff** | Total Assets | | Employees / T. Assets | | Cash / T. Assets | | Debt / T. Assets | |
|---|---|---|---|---|---|---|---|---|
| | High | Low | High | Low | High | Low | High | Low |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Layoff vs. (Credit + Control) | 0.033 | 0.050** | 0.056*** | 0.028 | 0.033 | 0.048** | 0.054** | 0.027 |
| | [0.022] | [0.020] | [0.021] | [0.023] | [0.021] | [0.021] | [0.025] | [0.024] |
| Observations | 3738 | 3922 | 4016 | 3261 | 3938 | 3660 | 2855 | 2739 |

| **Panel B: Applied to Credit Line** | Total Assets | | Employees / T. Assets | | Cash / T. Assets | | Debt / T. Assets | |
|---|---|---|---|---|---|---|---|---|
| | High | Low | High | Low | High | Low | High | Low |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Credit vs. (Layoff + Control) | 0.006 | -0.008 | -0.001 | -0.002 | -0.005 | 0.013 | -0.029 | 0.005 |
| | [0.021] | [0.018] | [0.019] | [0.022] | [0.017] | [0.023] | [0.025] | [0.022] |
| Observations | 3490 | 3727 | 3816 | 3053 | 3734 | 3425 | 2703 | 2573 |

This table shows heterogeneous effects of the intervention on program take-up (layoff in panel A and credit line in panel B). The regression includes the (displayed) treatment indicator as well as a constant and the step-by-step indicator (unreported), similar to column 4 of Table 3. Firms are split at the median of the distribution of each variable in the full sample. Financial data is as of 2019 year-end and available from ORBIS. *, **, *** Significance at 10, 5 and 1%, respectively.

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

| **Outcome: Layoff Program Receipt** | **Did this establishment close temporarily due to the COVID-19 outbreak?**[a] | | **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] | | **Is this firm part of a business membership organization, trade association, guild, or chamber of commerce?**[b] | |
|---|---|---|---|---|---|---|---|---|
| | Yes | No | At least one | None | $\geq 75$ | $< 75$ | Yes | No |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Layoff vs. (Credit + Control) | 0.233* | 0.065 | 0.344*** | -0.014 | 0.123** | 0.100 | 0.068 | 0.163** |
| | [0.121] | [0.053] | [0.107] | [0.033] | [0.061] | [0.101] | [0.074] | [0.073] |
| Layoff step-by-step | -0.341** | 0.067 | -0.027 | 0.032 | -0.016 | -0.083 | 0.074 | -0.146 |
| | [0.168] | [0.065] | [0.142] | [0.040] | [0.076] | [0.127] | [0.095] | [0.089] |
| Observations | 125 | 354 | 175 | 308 | 331 | 148 | 260 | 222 |

| | **Over the last year, did this establishment have performance bonuses for managers?**[b] | | **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] | | **At this time, does this establishment have a line of credit or a loan from a financial institution?**[b] | |
|---|---|---|---|---|---|---|---|---|
| | Yes | No | Major | Minor | Yes | No | Yes | No |
| | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| Layoff vs. (Credit + Control) | 0.259* | -0.015 | 0.192*** | 0.046 | 0.052 | 0.154** | 0.066 | 0.219** |
| | [0.131] | [0.088] | [0.070] | [0.080] | [0.082] | [0.069] | [0.063] | [0.095] |
| Layoff step-by-step | -0.033 | 0.073 | -0.155 | 0.058 | 0.009 | -0.062 | 0.108 | -0.261** |
| | [0.168] | [0.115] | [0.095] | [0.095] | [0.112] | [0.083] | [0.083] | [0.110] |
| Observations | 101 | 171 | 213 | 256 | 201 | 277 | 298 | 183 |

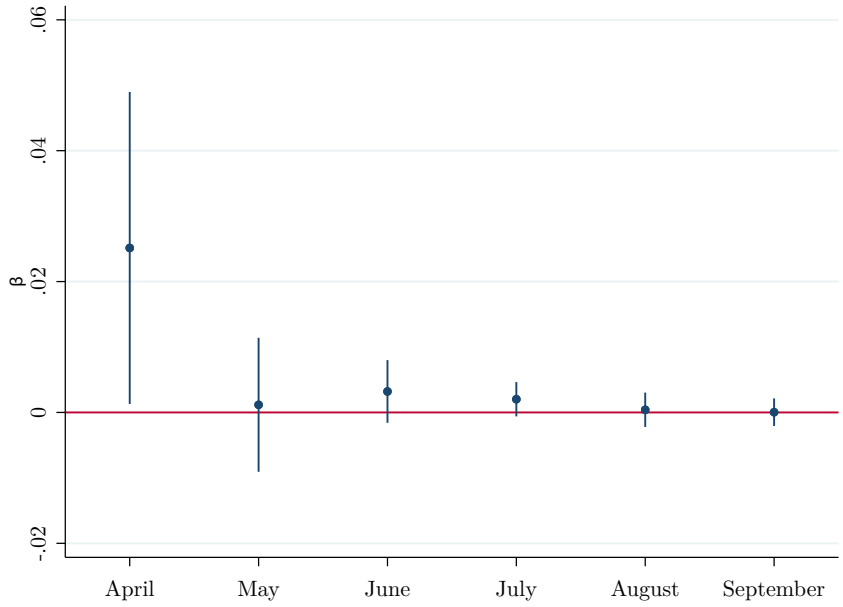
[a] Enterprise Surveys Follow-Up on COVID-19: Portugal, Round 1 [Sep – Oct 2020] (The World Bank, 2020)

[b] Enterprise Survey [Nov 2018 – Jan 2020] (The World Bank, 2019)

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 two questions are from the Follow-up on COVID-19 Survey conducted in September and October 2020. The remaining questions are taken from the standard Enterprise Survey (conducted between November 2018 and January 2020). The names of the original variables are COVb1a, COVd8, BMb1, BMb6, BMr8, k30, k7, and k8, 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). *, **, *** Significance at 10, 5 and 1%, respectively.

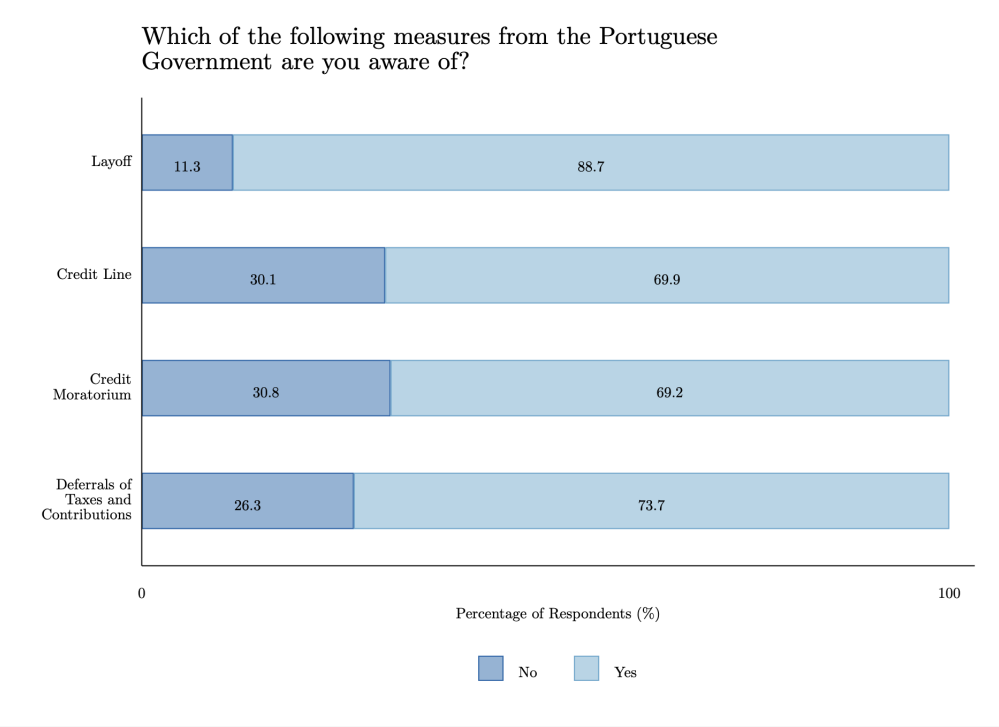
# 9 Figures

Figure 1: Timing of the Effect (Layoff Support Program)

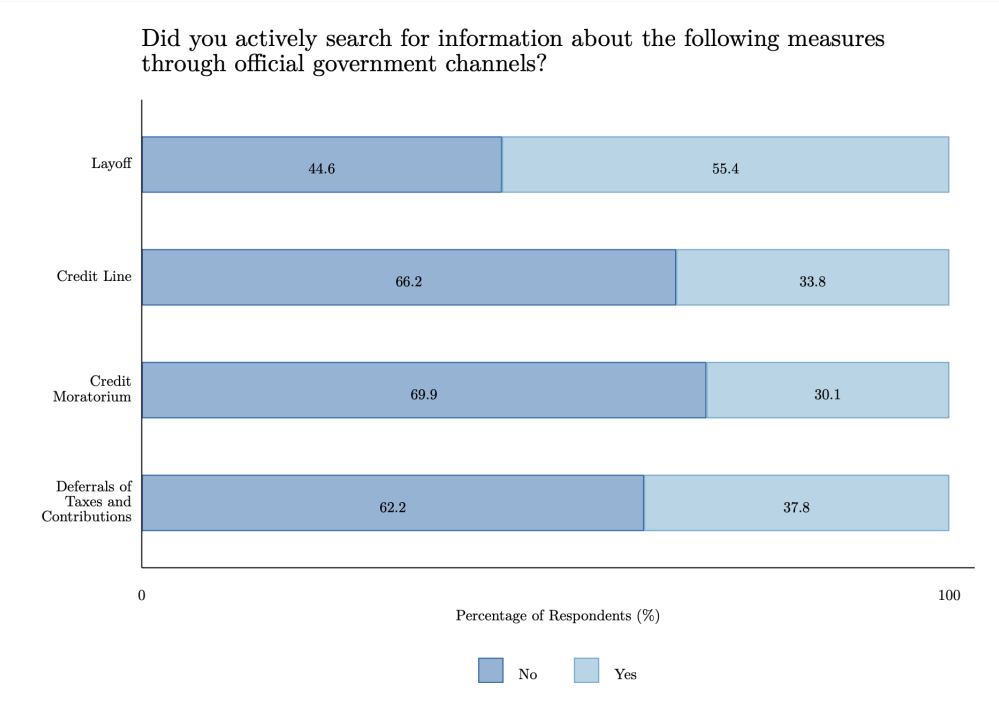



This figure plots the treatment effect coefficient on the layoff intervention (and the corresponding 95% confidence interval) estimated using the specification in column (4) of table 3 where the outcome variable is a binary variable equal to one if the firm applied to the layoff program in a given month between April and September 2020 (zero otherwise). The information on the month of the application was collected in the follow-up survey. We exclude firms that reported applying before the intervention (in March 2020). We report the full results in table A3.

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



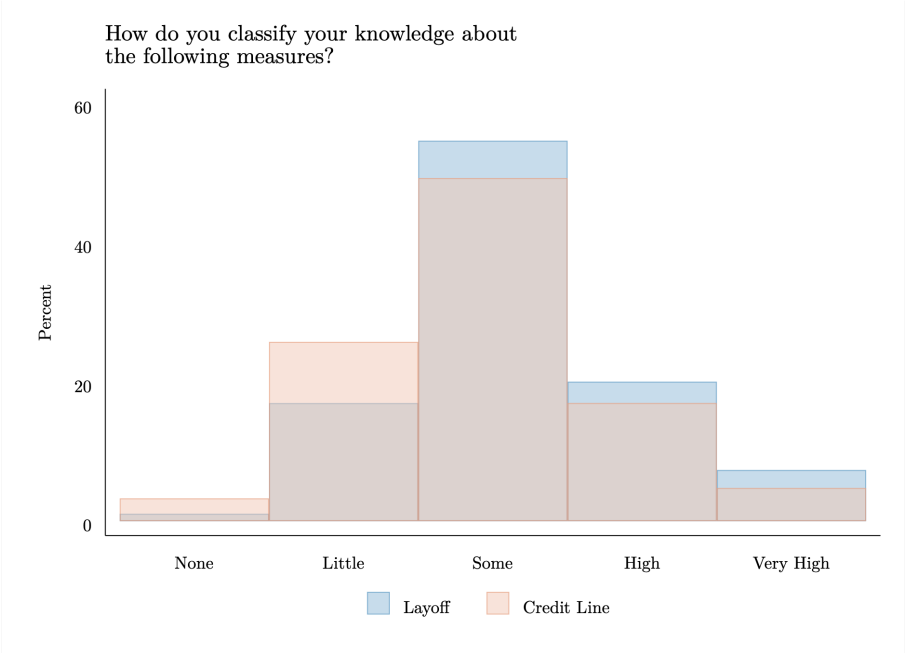

(a) Awareness



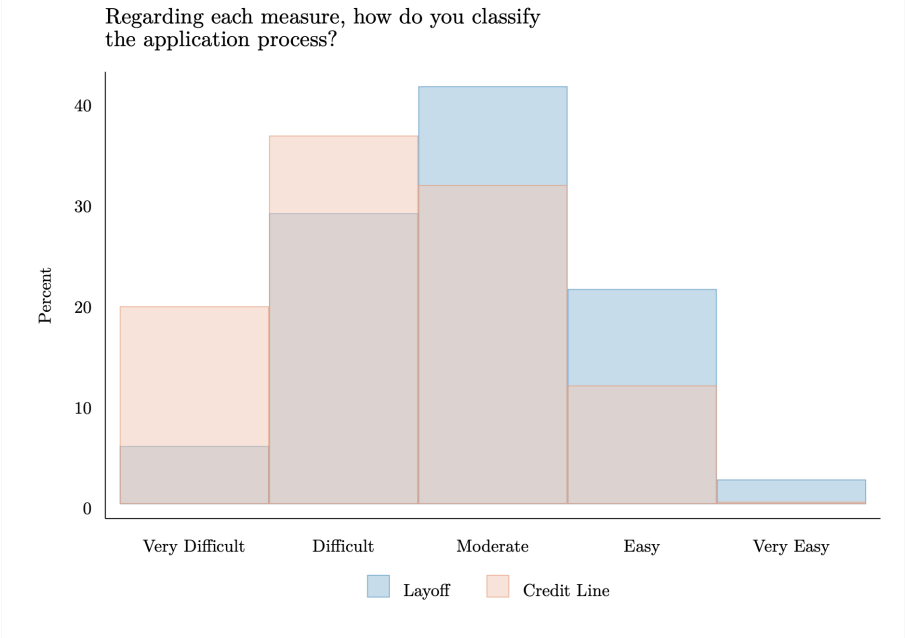

(b) Active Search

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 number of observations in this graph is 1,055. 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?*". The number of observations is 881.

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. Due to this attrition during the survey, the number of observations is 844 and 672, respectively, for each 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. This option is not reported in the graph and the observations are excluded from the percentage computation. The number of observations is 814 (layoff) and 646 (credit line).

**Firm Take-up of COVID-19 Government Measures – Evidence from Portugal**

**For Online Publication**

Cláudia Custódio, Christopher Hansman, and Diogo Mendes

# A1 Tables

Table A1: Balance Test among Follow-Up Survey Respondents

| | Layoff | | Credit Line | | Control | | Treat. vs. Control | P-value |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | | |
| Total Assets | 2.78 | 29.54 | 2.74 | 27.23 | 3.43 | 50.51 | -0.67 | 0.44 |
| Nr. Employees | 17.36 | 136.19 | 22.88 | 239.86 | 16.80 | 130.73 | 3.29 | 0.43 |
| Cash | 0.26 | 2.55 | 0.26 | 2.70 | 0.22 | 1.57 | 0.04 | 0.45 |
| Operating Revenue | 2.21 | 22.48 | 2.19 | 17.03 | 1.89 | 10.63 | 0.31 | 0.45 |
| Net Income | 0.20 | 4.83 | 0.07 | 2.07 | 0.09 | 1.89 | 0.05 | 0.55 |
| Long-term Debt | 0.68 | 10.10 | 0.65 | 9.35 | 0.62 | 8.57 | 0.04 | 0.87 |

This table displays balance tests based on follow-up survey respondents. We present the mean and standard deviation for 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 treatment (layoff and credit line treatment arms are pooled) and control groups. All financial variables are reported as of 2019 (year end) and are available from ORBIS. Variables are reported in millions of Euros (except the number of employees).. *, **, *** 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 | 1055 | 0.887 | 0.316 |
| Credit line | 1055 | 0.699 | 0.459 |
| Credit moratorium | 1055 | 0.692 | 0.462 |
| Deferrals of taxes and contributions | 1055 | 0.737 | 0.44 |
| None | 1055 | 0.049 | 0.217 |
| How do you classify your knowledge about the following measures? | | | |
| Layoff | 844 | 2.844 | 0.826 |
| Credit line | 672 | 3.058 | 0.864 |
| Credit moratorium | 673 | 2.863 | 0.825 |
| Deferrals of taxes and contributions | 706 | 2.694 | 0.885 |
| Did you receive any simplified information about the measures without asking for it? | | | |
| Yes, from traditional and digital media (newspapers, television, etc.) | 881 | 0.49 | 0.5 |
| Yes, from accountants and lawyers | 881 | 0.612 | 0.488 |
| Yes, from other sources | 881 | 0.142 | 0.349 |
| No | 881 | 0.109 | 0.312 |
| Did you actively search for information about the following measures through official gov. channels? | | | |
| Layoff | 881 | 0.554 | 0.497 |
| Credit line | 881 | 0.338 | 0.473 |
| Credit moratorium | 881 | 0.301 | 0.459 |
| Deferrals of taxes and contributions | 881 | 0.378 | 0.485 |
| No | 881 | 0.297 | 0.457 |
| How do you classify the information provided by the official government channels? (1 - Totally informative, 5 - Not informative) | 601 | 2.81 | 0.753 |
| Classifies Layoff application process as easy or very easy | 814 | 0.167 | 0.373 |
| Classifies Credit Line application process as easy or very easy | 646 | 0.076 | 0.265 |
| Did your firm apply to the Layoff program already? | 851 | 0.309 | 0.462 |
| Did your firm apply to the Credit Line scheme already? | 851 | 0.206 | 0.404 |
| Do you have a main bank that you turn to for financing and other fin. transactions? | 846 | 0.823 | 0.382 |
| How do you classify the relat. with your main bank? (1 - Very close, 4 - Very distant) | 694 | 1.81 | 0.787 |
| What is your role in the firm? | | | |
| CEO/ Director/ Manager | 828 | 0.767 | 0.423 |
| Founder | 828 | 0.249 | 0.433 |
| Business Owner | 828 | 0.405 | 0.491 |
| Accountant | 828 | 0.058 | 0.234 |
| Other employee | 828 | 0.134 | 0.341 |
| What is the highest educational level that you attained? | | | |
| No high education | 826 | 0.361 | 0.481 |
| Undergraduate | 826 | 0.416 | 0.493 |
| Masters | 826 | 0.212 | 0.409 |
| PhD | 826 | 0.011 | 0.104 |
| Do you have academic background in Finance or related areas? | 828 | 0.324 | 0.468 |
| How do you classify your level of financial literacy? (1 - Very high, 5 - Very low) | 828 | 2.813 | 0.733 |

| **Panel B: Follow-Up Survey (September 2020)** | Obs. | Mean | SD |
|---|---|---|---|
| Have you applied to the Layoff program? | 9238 | 0.319 | 0.466 |
| When did you apply? | | | |
| March | 2928 | 0.26 | 0.438 |
| April | 2928 | 0.606 | 0.489 |
| May | 2928 | 0.091 | 0.288 |
| June | 2928 | 0.019 | 0.138 |
| July | 2928 | 0.006 | 0.076 |
| August | 2928 | 0.006 | 0.076 |
| September | 2928 | 0.004 | 0.061 |
| Don't recall | 2928 | 0.009 | 0.094 |
| Was your application to the Layoff program approved? | 2890 | 0.964 | 0.187 |
| Have you applied to the Credit Line scheme? | 8695 | 0.256 | 0.436 |
| When did you apply? | | | |
| March | 2212 | 0.145 | 0.352 |
| April | 2212 | 0.386 | 0.487 |
| May | 2212 | 0.175 | 0.38 |
| June | 2212 | 0.078 | 0.268 |
| July | 2212 | 0.062 | 0.241 |
| August | 2212 | 0.064 | 0.245 |
| September | 2212 | 0.065 | 0.247 |
| Don't recall | 2212 | 0.025 | 0.156 |
| Was your application to the Credit Line scheme approved? | 2094 | 0.851 | 0.356 |

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

Table A3: Timing of the Effect (Layoff Support Program)

| | April | May | June | July | August | September |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Layoff vs. (Credit + Control) | 0.025** | 0.001 | 0.003 | 0.002 | 0.000 | 0.000 |
| | [0.012] | [0.005] | [0.002] | [0.001] | [0.001] | [0.001] |
| Layoff step-by-step | -0.016 | 0.008 | 0.002 | -0.001 | 0.001 | -0.001 |
| | [0.015] | [0.007] | [0.003] | [0.002] | [0.002] | [0.001] |
| Constant | 0.204*** | 0.030*** | 0.005*** | 0.002*** | 0.002*** | 0.001*** |
| | [0.005] | [0.002] | [0.001] | [0.001] | [0.001] | [0.000] |
| Observations | 8479 | 8479 | 8479 | 8479 | 8479 | 8479 |
| Adjusted R-squared | 0.000 | 0.000 | 0.000 | 0.000 | -0.000 | -0.000 |

This table shows the timing of the effect of the layoff intervention on take-up of the layoff program. In each column we report the results of a regression where the outcome variable is a binary variable equal to one if the firm applied to the layoff program in a given month between April and September 2020 (zero otherwise). The information on the month of the application was collected in the follow-up survey. We exclude firms that reported applying before the intervention (in March 2020). *, **, *** Significance at 10, 5 and 1%, respectively.

Table A4: Descriptive Comparison of Our Sample and the Remaining Firms in ORBIS

| | Our Sample | | Remaining ORBIS | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| Total Assets | 2.49 | 51.49 | 1.16 | 22.75 |
| Nr. Employees | 15.30 | 117.70 | 6.37 | 105.79 |
| Cash | 0.21 | 2.76 | 0.09 | 1.18 |
| Operating Revenue | 2.27 | 28.04 | 0.83 | 32.41 |
| Net Income | 0.05 | 8.23 | 0.04 | 1.13 |
| Long-term Debt | 0.70 | 26.40 | 0.49 | 10.75 |

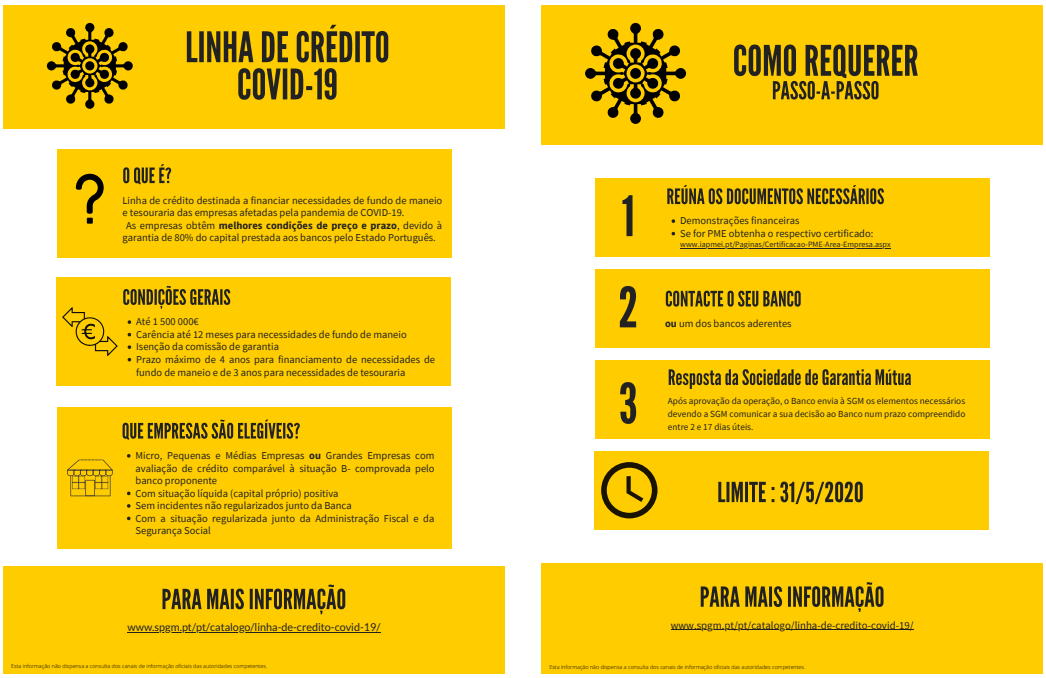
This table displays a comparison between the firms in our sample and the remaining Portuguese firms in ORBIS. We apply the same cleaning as for the sample firms (i.e., we exclude listed companies, companies in the financial sector or under public administration (including education, social care, and arts and sport activities), non-domestic firms, and companies with a missing industry identifier), except the requirement of a valid email address 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. Variables are reported in millions of Euros (except the number of employees).

# A2 Figures

Figure A1: Information presented to the Treatment Groups

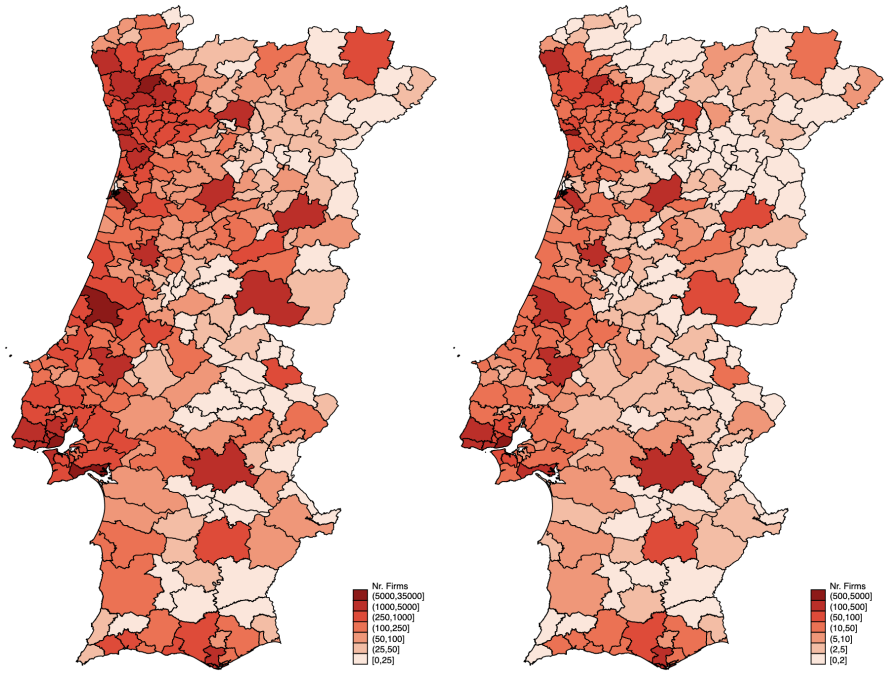
This figure displays the information presented to the treatment groups (in Portuguese).

(a) Layoff Treatment




(b) Credit Line Treatment

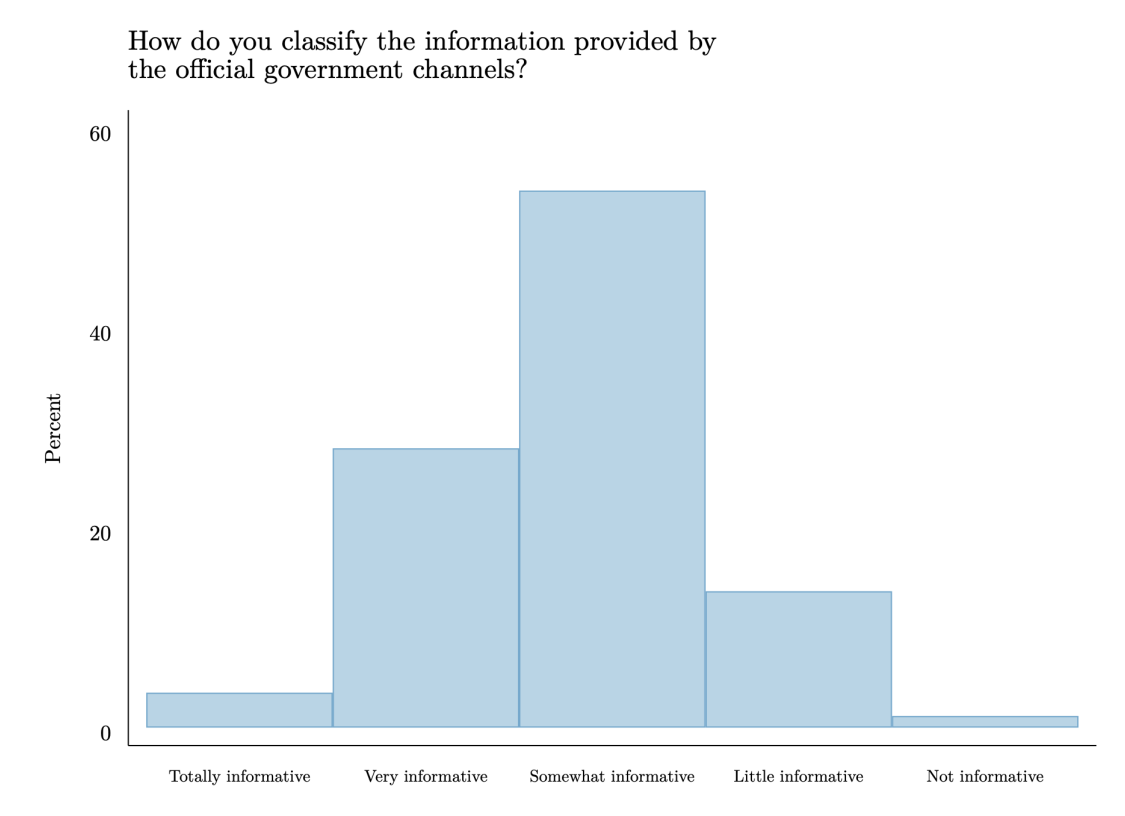
Figure A2: Number of firms in each stage, by municipality




(a) Full Sample (b) Follow-Up

This figure shows the number of firms by municipality. In subfigure a), we show the number of observations in the full sample. In subfigure b), we show the number of respondent firms in the follow-up stage. We omit Azores and Madeira Archipelagos.

Figure A3: 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. The number of observations in this graph is 601.