# The Labor Market Effects of Loan Guarantee Programs[†]

Jean-Noël Barrot, HEC Paris, France
Thorsten Martin, Bocconi, Italy
Julien Sauvagnat, Bocconi, Italy
Boris Vallée, HBS, USA

*Review of Financial Studies, Forthcoming*

**Abstract**

We investigate the labor market effects of a loan guarantee program targeting French SMEs during the financial crisis. Exploiting differences in regional treatment intensity in a border discontinuity design, we uncover a central trade-off for such interventions. While the program has a positive impact on workers' employment and earnings trajectories that translates into positive aggregate employment effects, it dampens the worker reallocation toward more productive firms that happens following recessions, and particularly so for high-skill workers. This labor allocation effect is economically significant and translates into a reduction in aggregate productivity. (*JEL* G28, G33, H81, J23, J31, J65)

[†]We thank Laurent Bach, Natalie Bachas (Cavalcade discussant), Diana Bonfim (FIRS discussant), Claire Celerier, Ettore Croci (AFFI discussant), John Earle, Xavier Giroud (WFA discussant), Jessica Jeffers (EFA discussant), Debbie Lucas, Will Mullins, Jacopo Ponticelli (NBER discussant), Antoinette Schoar, and David Thesmar; conference participants at the NBER Corporate Finance meetings, WFA meetings, Cavalcade, Labor and Finance group workshop, AFSE conference, AFFI Paris Finance conference, and pre-MoFiR Workshop on SME Financing; and seminar participants at Bank of Italy, Bocconi, Carnegie Mellon, Drexel University, Edhec, Frankfurt School of Finance & Management, Harvard Business School, HEC Montreal, HEC Paris, Laval University, Mannheim University, McGill, MIT Sloan, BI Oslo, Sciences Po, UCLA, UC San Diego, University of Bristol, University of Sydney, University of Toronto, and University of Venice for comments and suggestions. Errors are ours only. We thank Bpifrance Le Lab for granting us access to their data. A previous version of this paper was circulated under the title "Employment Effects of Alleviating Financing Frictions: Worker-level Evidence from a Loan Guarantee Program." Send correspondence to Boris Vallee, bvallee@hbs.edu.

Numerous countries facilitate bank lending to small businesses through loan guarantee programs, whereby a government agency underwrites a share of the notional of loans issued by banks to qualifying borrowers (such as the Small Business Agency [SBA] programs in the United States).[1] As banks retain skin in the game, loan guarantees are designed to address the mistargeting and rent-seeking that plague direct public lending (see, for instance, Khwaja and Mian 2005). Policy makers' interest in these programs increased in the wake of the 2008 financial crisis because of concerns that small businesses might be prevented from accessing capital sufficient for them to be resilient, to grow, and to create jobs (Chen, Hanson, and Stein 2017; Bord, Ivashina, and Taliaferro 2021).[2] At the same time, changes in credit supply might lead to labor misallocation and ultimately hurt aggregate productivity (see, e.g., Bai, Carvalho, and Phillips 2018; Blattner, Farinha, and Rebelo 2023). These issues are particularly acute as SMEs represent 70% of employment in OECD countries.[3] The question of the design and efficiency of these programs has become even more important to policy makers since the COVID-19 outbreak as a large number of governments, including the United States and the majority of European countries, have massively turned to this tool to address the sharp recession resulting from the pandemic.[4,5] Despite their large and growing implementation, we know surprisingly little about the long-term effects of these programs on

[1]See Beck, Klapper, and Mendoza (2010) for a summary of these programs around the world.

[2]In the United States, the main SBA 7(a) loan guarantee programs have significantly expanded with the financial crisis. The stock of SBA 7(a) loans has increased from $46 billion in 2007 to $92 billion in 2018. See CRS (2019) for more details.

[3]See https://www.oecd.org/mcm/documents/C-MIN-2017-8-EN.pdf.

[4]The COVID-19 outbreak created a sudden revenue shortfall, accompanied with increased financial frictions, particularly for small firms that rely mostly on bank lending. Blanchard, Philippon, and Pisani-Ferry (2020) argue that banks are reluctant to lend even to viable firms that may be short on liquidity, as diversifying away the COVID-19 risk is difficult, and they are facing compressed capital ratios due to losses on their loan portfolios.

[5]COVID-19-related loan guarantee programs vary in design across countries, with, for instance, the Paycheck Protection Program (PPP) in the United States being economically closer to a short-time work program.

workers' employment and mobility across firms, and on their aggregate implications. While these programs have been shown to foster job growth at beneficiary firms (Brown and Earle 2017), assessing their effectiveness at mitigating net employment effects of financing frictions, as well as the labor allocation effects of such programs, calls for measuring the impact of these programs on both workers' transitions in and out of unemployment and their job-to-job mobility following a downturn. If these programs prevent workers from experiencing lengthy periods of unemployment, and/or impairing their human capital, the benefit of these programs can be large. However, these programs might also create a barrier to the beneficial reallocation of workers after a recession by keeping workers in less productive firms. Such a concern is, for instance, motivated by the fact that during a major countercyclical loan guarantee program in France, firms taking up the program were 24% less productive than the average firm population, per Figure 1.[6]

[INSERT FIGURE 1]

In this paper, we use novel administrative micro data combined with geographic variation in program design to uncover and assess this trade-off. We estimate the long-term impact of a countercyclical loan guarantee program in France on workers' employment and earnings trajectories, both at the initial firm and at subsequent employers. Our data tracks a representative sample of individual workers across firms over time, as well as their transitions between employment and unemployment and the associated welfare benefits they receive. Matched with firms' balance-sheet information, these data allow us to study how such programs affect workers' reallocation following a recession by observing counterfactual worker job-to-job transitions, how the productivity of their new employer differs from their initial

[6]Internet Appendix Table A.1 shows that this pattern also holds after controlling for two-digit industry fixed effects.

employer, and which type of workers are particularly affected by the changes in mobility resulting from the program. Implemented in the midst of the financial crisis, the Recovery Loan Guarantee Program offers a public guarantee for French small and medium-sized enterprises (SMEs) to rollover and extend their short-term debt. This new program, administered by Bpifrance, the French equivalent to the SBA, was announced in the last quarter of 2008 and extended until the end of 2010.[7] As French regions differently augment the funding of the national program, the treatment intensity varies geographically in a significant and plausibly exogenous manner. We exploit this heterogeneity and integrate it with a regional border discontinuity approach in order to estimate the causal impact of the program on workers at firms benefiting from a loan guarantee. The identifying assumption in our setting is that workers in firms located on each side of a regional border would have experienced similar labor market outcomes in the absence of the loan guarantee program. We first provide evidence that the regional intensity of the loan guarantee program translates into a higher take-up of loan guarantees at the firm level within the regional border area. Furthermore, higher treatment intensity is associated with both an increase in the quantity of bank debt on firms' balance sheets and a decrease in their cost of borrowing, which supports that our measure of treatment intensity captures heterogeneity in guarantee supply and not in firms' demand for loans across regional borders. We then leverage our longitudinal worker-level data to evaluate how this program affects worker employment, earnings, mobility, and matching with firms until 2015. We find that the program has a significant and persistent positive impact on workers' employment and earnings trajectories. Quantitatively, when extrapolating our estimates to the average treatment at the worker level, we obtain that individual workers initially employed by a treated firm receive earnings that are 26% higher

[7] A similar yet significantly larger loan guarantee program was launched in the second quarter of 2020, in response to the COVID-19 crisis.

on average over the 2009–2015 period, when compared to a counterfactual set of workers initially employed by nontreated SMEs. This finding mostly reflects an employment margin: workers more exposed to the program are significantly less likely to separate from their initial employer, and in turn to be unemployed over the sample period. Their unemployment benefits are consequently lower, representing a significant reduction in the cost of the government intervention. Overall, the program preserved 487,000 job(-years) at a gross cost per job(-year) of €1,400 and €425 when accounting for the ex ante or ex post cost. Since the loan guarantee program reduced workers' unemployment spells and the associated savings for the unemployment national fund amount to around 2.1 billion euros, the program actually exhibits a negative net cost. However, these employment gains need to be contrasted with labor allocation effects. The relative gain in employment and earnings from higher retention at initial employers is half offset by a reduction in worker mobility toward other firms in the economy. In tight labor markets worker mobility fully offsets the employment gains at initial employers, canceling the net employment benefits of the program in this context. Further, the reduced mobility resulting from the loan guarantee program dampens the reallocation of workers toward more productive firms, and particularly so for workers with skills in high demand. Such a result is consistent with loan guarantees supporting skilled labor hoarding, that is, higher worker retention for firms with high-skill workforce. By comparing the productivity of both the initial and new employers of the workers in our sample, we first show that workers more exposed to the program are significantly less likely to move to firms with higher productivity than workers from the counterfactual. Turning to the cross-section of workers and occupations, we find that this dampening effect is particularly pronounced for workers with high earnings capacity, for occupations for which firms report hiring difficulties, and for nonroutine/cognitive-analytical occupations. Taken together, these findings highlight an

important counterpart to the employment benefit of countercyclical loan guarantee programs we previously document: by keeping workers in their current firms and thereby creating a barrier to beneficial worker reallocation, these programs might affect the trajectory of the economy following recessions. We conduct a battery of tests to ensure that these results are not driven by alternative mechanisms, such as confounding local shocks, spillovers, or other policies. Crucially, we conduct a placebo analysis on firms that have a low propensity to obtain a guarantee, and find no effects of the program on the set of workers initially employed by these firms, which confirms that our baseline estimates are caused by the loan guarantee program rather than other policies or demand-side factors that could confound our results. To conclude our study, we develop a simple theoretical framework that helps us interpret our empirical results and provides an estimate for the effect of the program on aggregate productivity. In the model, firms need to finance labor in advance, and we interpret the guarantee program as providing treated firms with a subsidy to their cost of financing. We show that the program leads to an increase in labor demand for treated firms, which in turn depresses labor demand for untreated firms through crowding out effects on the labor market. We then build on the general insight from the misallocation literature that the program would have negative effects on aggregate productivity if the marginal revenue products on labor are lower for treated than for untreated firms. Consistent with our evidence that treated firms have lower labor productivity than other firms preprogram, we find that the program had a negative impact on aggregate productivity, of around $-1\%$. Such a net negative effect is likely to hold in economies where SMEs exhibit lower labor productivity than larger firms, as is the case in the European Union at large, for instance.[8] Our research contributes to the burgeoning literature on loan guarantees (de Andrade and Lucas 2009; Beck, Klapper, and

[8] *Source*: `https://ec.europa.eu/docsroom/documents/32581/attachments/11/translations/en/renditions/native`.

Mendoza 2010; Lelarge, Sraer, and Thesmar 2010; Mullins and Toro 2018; Brown and Earle 2017; D'Acunto, Tate, and Yang 2017; de Blasio, Mitri, D'Ignazio, Russo, and Stoppani 2018; Bachas, Kim, and Yannelis 2021).[9] Two contemporaneous papers expand on Brown and Earle (2017) and measure the real effects of loan guarantee programs at the firm level: Bonfim, Custodio, and Raposo (2023) do so in Portugal, and Gonzalez-Uribe and Wang (2022) in the United Kingdom. The settings of these two papers differ in terms of scheme design and economic contexts, resulting in distinct channels dominating: lower cost of debt for the former, and mitigation of credit rationing for the latter. Both studies however document positive firm-level effects in terms of employment and firm growth, and cost-effective job preservation when contrasting eligible firms' employment growth to the program direct costs. Our study, which centers on a worker-level analysis, complements their findings by estimating net employment effects as well as labor reallocation effects. Our analysis requires tracking workers when they transition in and out of firms or employment, and sheds a new light on the implications of loan guarantee programs for aggregate productivity. Relatedly, we add to the empirical debate on the effectiveness of public policies aiming to protect or stimulate employment in downturns, such as hiring credits (Cahuc, Carcillo, and Le Barbanchon 2019; Neumark and Grijalva 2017), and subsidies for short-time work (Cahuc, Kramarz, and Nevoux 2018; Giupponi and Landais 2022). We show that loan guarantees have a positive and persistent impact on workers' employment and earning trajectories obtained at a

[9] A burgeoning literature studies the (short-term) effects of loan guarantees implemented during the COVID-19 outbreak (see, e.g., Granja et al. (2022); Core and De Marco (2023); Bartik, Cullen, Glaeser, Luca, Stanton, and Sunderam (2020); Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar, and Yildirmaz (2022); Chetty, Friedman, Hendren, Stepner, and Team (2023); Li and Strahan (2021); Hubbard and Strain (Forth.)). Many countries have indeed used loan guarantees as one of their key measures to support the economy during the pandemic. Using data from the OECD and the IMF, Benmelech and Tzur-Ilan (2020) report that government loan guarantees amount to an average of 2.73% of gross domestic product (GDP) in the year 2020 across 85 countries, while total fiscal spending (excluding these guarantees) averages 4.97% of GDP.

relatively low cost, likely because of effective targeting resulting from the loan guarantee design (Philippon 2021), but also significantly dampen the reallocation of the workforce toward more productive firms. In doing so, we relate more broadly to the literature studying the consequences for the economy of size-dependent policies that frequently favor smaller firms (see, e.g., Restuccia and Rogerson 2008; Hsieh and Klenow 2009; Bartelsman, Haltiwanger, and Scarpetta 2013; Garicano, Lelarge, and Van Reenen 2016). Lastly, our work assesses a possible remedy to the significant employment effects of financing frictions documented by a large body of empirical studies, both at the firm level (Chodorow-Reich 2014; Duygan-Bump, Levkov, and Montoriol-Garriga 2015; Greenstone, Mas, and Nguyen 2020; Giroud and Mueller 2017; Bentolila, Jansen, and Jiménez 2018) and at the worker level (Berton, Mocetti, Presbitero, and Richiardi 2018; Caggese, Cuñat, and Metzger 2019; Baghai, Silva, Thell, and Vig 2021; Babina 2020; Acabbi, Panetti, and Sforza 2020; Gortmaker, Jeffers, and Lee 2020). Our study also contributes to the literature studying labor misallocation effects resulting from financial policies (e.g., Bai, Carvalho, and Phillips 2018; Barbosa, Bilan, and Celerier 2019; Fonseca and Van Doornik 2022; Blattner, Farinha, and Rebelo 2023).

# 1 Background

Numerous governments, including the United States, provide loan guarantees to small firms. These programs are usually implemented through a specialized entity, such as the Small Business Administration (SBA) in the United States, or Bpifrance in France, which partners with banks.

## 1.1 Economic rationale of loan guarantees

Loan guarantee programs allow small businesses to mitigate their financing frictions, which are particularly pronounced during recessions. Access to credit for small firms might be limited in general by adverse selection (Stiglitz and Weiss 1981), moral hazard (Holmstrom and Tirole 1997), and transaction costs. Such financing frictions are typically amplified during recessions, since revenue shortfall worsens the pool of borrowers and increases debt overhang (Brunnermeier and Krishnamurthy 2020). While also acting as a subsidy to the cost of capital, loan guarantees by a government-backed entity have several advantages over direct subsidized public lending. First, this public intervention design typically delegates screening and monitoring to private banks. Relying on banks' expertise and infrastructure mitigates the risk that political considerations drive the allocation of credit. Second, as the guarantees are partial, banks retain skin in the game when screening loans. Lastly, guarantees do not require the guarantor institution to disburse cash and raise capital at the time of their implementation, although they do create regulatory capital requirements. Theoretically, the employment effects of loan guarantee schemes are ambiguous, although prior work documents positive effects of relaxing financial constraints on employment growth at the firm level (see Chodorow-Reich 2014; Brown and Earle 2017), among others. During downturns, the main focus of our study, one important economic rationale for loan guarantee schemes is to support labor hoarding, as put forward during both the financial crisis and the COVID-19 pandemic. Financial constraints might prevent firms facing a temporary shock from optimally maintaining employment relationships with their workers, as argued in Giroud and Mueller (2017), creating an excess sensitivity of separations to business cycle

fluctuations.[10] While loan guarantees might allow recipient firms to retain their workforce, this does not necessarily translate into net aggregate positive effects on employment. In particular, in areas or periods with low unemployment rates, displaced workers might easily find a job in other firms of the economy, in which case the positive effect of guarantees on the employment of recipient firms does not translate into net employment gains in the aggregate. Moreover, worker retention, while usually beneficial to a given employer, can be detrimental to the economy at large. Downturns are indeed typically associated with significant reallocation of workers from low to high-productivity firms as less productive firms reduce the scale of their operations or exit, while more productive firms might better resist or even grow. In this context, loan guarantees might reduce efficiency on the labor market – that is, the aggregate quality of the matches between workers and firms – if they allow recipient firms with structurally low labor productivity to retain their employees, and thus dampen the reallocation of the workforce toward firms with higher labor productivity. We formalize the trade-off between the employment and productivity effect in the last section of the paper.

## 1.2 The French public guarantor and the post-GFC French recovery plan

Bpifrance[11] is the entity managing public loan guarantee programs in France and was created in 1982 as a French equivalent of the SBA. Bpifrance activities are mostly targeted toward SMEs and encompass, in addition to loan guarantees, direct lending, providing grants, and

[10] Firms may find it optimal to maintain employment relationships in downturns, if, for instance, hiring and training workers is costly to do and/or because worker-firm relationships involve firm-specific human capital that is lost during layoffs.

[11] Previously named Sofaris and then Oseo-Garantie.

investing in equity. Bpifrance does not collect deposits, but funds itself in the wholesale market. Bpifrance works with a network of partner banks that include all major French banks and relies on them to source loan applications. As of 2017, Bpifrance possesses 48 local branches that process the loan guarantee applications provided by banks. Starting in the second half of the 2000s, French regions have been partnering with Bpifrance. This partnership takes the form of complementing Bpifrance intervention by guaranteeing an additional fraction of the loans that Bpifrance underwrites. This additional guaranteed fraction is capped and varies across regions in accordance with bilateral agreements between French regions and Bpifrance. The partnership is based on top-up financing independently provided by the regions through dedicated entities, the *Fonds Regional de Garantie*. The existence, timing, and generosity of such partnerships result from an idiosyncratic local political process conducted in the regional parliaments. For the purpose of our empirical analysis, we focus on a new loan guarantee program created at the end of 2008, which specifically aimed at allowing firms to access short- and medium-term debt in the wake of the financial crisis. The French recovery plan of 2009–2010 led to the creation of a large short-term credit guarantee program managed by Bpifrance (under the Oseo-Garantie name at that time). As illustrated in Figure 2, the plan guaranteed €5.3bn of new bank debt between 2008Q4 and 2010Q4, which represented 0.2% of GDP. The plan targeted new lines of credit with a term between 12 and 18 months, as well as the restructuring of existing short-term debt into new loans with maturity between 2 and 7 years. Of firms that received guarantees on their new lines of credit, 4,000 received them for an amount of €1.8 bn, and 17,000 firms received guarantees on their medium-term new loans for €3.5 bn. Bpifrance charges an average insurance premium of around 1% per annum of the loan notional in exchange for such a guarantee. Ex post, the premiums represented a total of €126M, while

banks claimed guarantees for €333M, which illustrates that the guarantee was subsidized on average.

[INSERT FIGURE 2]

# 2 Data

We use three complementary sources of administrative micro data, which we obtained from Bpifrance and the French Statistical Office (INSEE): an exhaustive file of individual loan guarantees, the exhaustive firm registry, and a worker panel covering 1/12th of the French workforce that tracks workers across jobs and employers, as well as in and out of unemployment. First, we use proprietary data provided by Bpifrance on the universe of firms benefiting from loan guarantee programs since 2002.[12] These data provide a unique firm identifier (SIREN), and information on the guarantee characteristics, including the date and amount of the loan, whether the guarantee was part of the recovery plan, the type of loan underlying the guarantee, and the fraction of the loan covered by the guarantee. Bpifrance data do not include information on interest rates but include information on default: whether the loan benefiting from the guarantee defaults over its life, and the loss amount. The data set does not include unsuccessful application data, as Bpifrance did not collect such data.

We also use administrative micro data extracted from tax files available until 2015. The data includes balance sheets as well as profit and loss statements for the universe of French firms.[13] We track firms through time using their unique identifying number ascribed by

[12]The data sharing agreement does not grant Bpifrance any form of control over the findings of this study or their publication.

[13]These data exhibit a discontinuity in the number of firm-level variables available for researchers from 2010, meaning that we observe the breakdown of firm debt between bank debt and other debt until 2009 only.

INSEE. Lastly, we rely on worker longitudinal data ("DADS Panel"), built by INSEE from social security contribution declarations of firms and from unemployment benefits. The sample covers all individuals born in October of each year, that is, 1/12th of the French workforce. Each year firms declare the employment spells, the number of hours worked, and the associated wages for each worker. For workers who have multiple jobs in a given year, we aggregate earnings across all jobs and retain the identifier of the employer that accounted for the largest share of the worker's earnings. Data on unemployment benefits is available since 2008.

## 2.1 Data filtering and summary statistics

For the purpose of our identification strategy, we restrict the sample to firms with all employees in the same region and located within 10 miles of a regional border. Given that SMEs (defined as firms with less than 250 employees) represent virtually all the beneficiaries from the recovery plan, we also restrict the sample to SMEs. We then follow the literature and exclude firms from the financial and real estate sectors, as well as utilities, nonprofit, and regulated sectors. This filtering leaves us with 31,949 firms in our central sample. At the worker level, we restrict the sample to workers with high labor force attachment (as in, e.g., Autor et al. 2014, Yagan 2019), namely, workers with annual earnings above €10,000 in 2006, 2007, and 2008. To avoid measurement errors due to initial entry and exit from the workforce, we focus on workers that were at least 24 years old in 2008 and at most 58 years old in 2015, that is workers who were born between 1957 and 1984. We also restrict our analysis to French citizens in order to alleviate concerns over unobserved employment in foreign countries. Lastly, we only keep workers initially employed by establishments located within the region border zone. This filtering leaves 38,568 workers in the sample, which

are, by construction, representative of 12 times more workers, or 462,816. For each of these workers, we then track their employment status (employed or unemployed), the source and magnitude of their earnings, labor earnings or unemployment benefits, as well as the economic performance (e.g., productivity and growth) of their new employers when they change jobs, over the sample period. Table 1 presents descriptive statistics for our filtered sample. Panel A provides information on the exposure to the loan guarantee program, both at the regional and at the firm level. $Raw\ Guarantee_{region,2009-2010}$ corresponds to the average ratio of loan guarantees to firm assets, computed across all eligible firms in a given region, excluding firms within 10 miles of a regional border. The generosity of the program varies significantly across the 21 regions, with firms from the least generous region receiving on average 0.1% of their total assets in guarantees, while firms from the most generous region receive 7.3 times more. Panels B and C present descriptive statistics at the worker level. The average worker has worked for 6.5 years during the 2009–2015 period, received earnings equal to 6.5 times their initial annual earnings (average annual earnings over the 2006–2008 period), and received 0.2 times their initial annual earnings in unemployment benefits. The average worker is 38 years old, works 1,872 hours, and earns €23,836 per year.[14] Table A.2 in the Internet Appendix presents the same characteristics separately by firm-level guarantee take-up. We note that 5.1% of workers in our sample are initially employed in a firm receiving a loan guarantee. Finally, in panel D, we present a number of firm characteristics measured in 2008. The average firm in our sample has 20 employees in 2008, is 18 years old, has assets of €3.04 million, and return on assets of 10%.

[INSERT TABLE 1]

[14]Earnings include all wages earned during the year net of social contributions and exclude unemployment benefits. Variables are expressed in €2015.

By construction, our main sample includes only SMEs and their associated workers initially located within a 10-mile distance to a regional border. One possible concern is that this sample is not representative of the whole universe of SMEs. To shed light on this potential issue, Internet Appendix Table A.3 displays firm and worker characteristics for our sample of SMEs located within a 10 mile distance of a regional border, and for the whole universe of French SMEs for which we observe worker-level data. We also present the distribution of firms in both groups across a list of 18 industries in Internet Appendix Table A.4. Overall, the characteristics of the two groups, and their distributions across industries, are comparable. Taken together, these statistics are supportive of our estimates being informative for the whole population of SMEs.

# 3 Measuring The Labor Market Effects of Loan Guarantee Programs

## 3.1 Empirical strategy

Studying the effects of a loan guarantee program requires overcoming a major empirical challenge: receiving a loan guarantee is most likely correlated with firm characteristics, either observables or unobservables. A naive ordinary least squares (OLS) regression of worker outcomes on firm-level guarantee take-up is therefore prone to endogeneity, for instance, because of the selection of firms taking up loan guarantees on distress. For the purpose of causal identification, we thus rely on a border discontinuity design to estimate the treatment effects of the loan guarantee program. Border discontinuities have been used in a number of studies to estimate program effects in a variety of economic contexts (see, e.g., Holmes 1998; Black

1999; Dube, Lester, and Reich 2010; Huang 2008). In our setting, we rely on discontinuities in the intensity of the loan guarantee program at regional borders. Importantly, as in the studies mentioned above, the discontinuity in exposure to the program that we exploit is sharp: the location of the firm (and not of the lenders) determines the Bpifrance regional office in charge of processing the loan guarantee application. If the firm's headquarters are located in region A, the Bpifrance regional office in region A is in charge of processing the loan guarantee application. Exposure to the program therefore cannot be "manipulated" by borrowing from banks outside the region in which the firm is located.[15] We focus on firms and workers located along regional borders in order to absorb the effect of local economic conditions. The gray area in Figure 3 represents the set of municipalities whose centroid lies within 10 miles of a regional border. Our baseline sample includes workers initially employed by establishments located in one of these border municipalities. The main identifying assumption is that workers on each side of the border would have experienced similar labor market outcomes in the absence of treatment. We note that if labor markets are frictionless and workers can easily move to another region and obtain identical compensation in alternative firms, there will be no earnings or employment impact at the worker level resulting from differences in their regional exposure to the French loan guarantee program in the period 2009–2010. To filter out demand factors, such as firm composition or other regional public policies, we construct our main measure of regional exposure to the 2009–2010 loan guarantee program, $\text{Guarantee}_{region,09-10}$, by computing the regional average residualized guarantee exposure controlling for an extensive set of firm and regional characteristics, thereby focusing on idiosyncratic program variation at the regional level. Specifically, we estimate the

[15] As noted before, we keep only firms with all establishments in the same region in the sample. This ensures that firms in our sample do not have access to guaranteed loans in several regions through the different location of their establishments.

following specification across eligible firms outside the border area:

$$Guarantee_{firm,2009-2010} = \delta_1.X_f + \delta_2.X_r + \epsilon_f, \tag{3.1}$$

where $Guarantee_{firm,2009-2010}$ is the ratio of the loan guarantee amount received by firm $f$ from Bpifrance through the recovery plan over the firm total assets in 2008. $X_f$ is a vector of firm characteristics including the logarithm of firms' total assets, the logarithm of firm age, credit risk, return on assets, the ratio of dividends over sales, property, plant, and equipment (PPE) over assets, and debt over assets, as well as industry fixed effects (for 56 two-digit industries), all measured in 2008. $X_r$ is a vector of regional characteristics including the regional 2008-10 per-capita change in public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and lending by local banks. We then compute $\text{Guarantee}_{region,09-10}$ by averaging the residual $\epsilon_f$ by region and use this residualized treatment as our main explanatory variable in the border area.[16] Figure 3 displays a map of our main measure of treatment intensity, $\text{Guarantee}_{region,09-10}$. Our empirical strategy exploits this regional variation in treatment intensity as a source of identification.[17] The thin gray lines within each region separate departments, a finer geographical level, which we rely on to absorb local economic conditions in a granular manner.

[16]By doing so, we reduce the likelihood that unobservable characteristics of firms in the nonborder subsample are correlated with the error terms in our main specification implemented in the border area sample. Yet our analysis is robust to using the nonresidualized measure of treatment intensity, that is, the regional average of the ratio of the amount of loan guarantee received by a firm through the recovery plan over the firm total assets in 2008, computed across eligible firms outside the border area.

[17]Regional treatment intensity tightly relates to the level of funding of the regional companion fund in 2008, as illustrated in column 1 of Internet Appendix Table A.5 that shows that each additional euro in the regional guarantee fund leads to an additional 62 cents of loans guaranteed under the program. The large $R^2$ of this bivariate regression illustrates that the generosity of the regional companion fund is the main driver of the heterogeneity in regional treatment intensity. Column 2 of Internet Appendix Table A.5 confirms that an increase in size of the regional fund results in an increase in the guaranteed fraction of loans. In turn, since banks' skin in the game is lower in these regions, they tend to extend more guaranteed loans, as evidenced in column 3 of Internet Appendix Table A.5.

[INSERT FIGURE 3]

Our empirical strategy is akin to a difference-in-differences setting with continuous treatment, as areas are differentially exposed to the short-term loan guarantee program. The exclusion restriction relies on the regional loan guarantee exposure only affecting workers' outcomes through the subsidized access to new lines of credit and bank loans offered by the program to their employers in 2009 and 2010. In particular, regional exposure to the program needs to be orthogonal to other unobserved regional shocks that could otherwise affect workers' outcomes over the sample period. In Section 7, we conduct a placebo-like analysis on firms that have a low propensity to obtain a guarantee, and find no effects of the program on the set of workers initially employed by these firms, which mitigates the concern that other unobserved economic or policy shocks could confound our results.

## 3.2 First-stage evidence

Our first stage boils down to the following cross-sectional regression on the set of eligible firms located within 10 miles of a regional border:

$$Guarantee_{firm,2009-2010} = \beta.Guarantee_{region,2009-2010} + \delta_1.X_f + \delta_2.X_r + \gamma_s + \epsilon_f, \qquad (3.2)$$

where $Guarantee_{firm,2009-2010}$ is the ratio of the amount of loan guarantee received by firm $f$ from Bpifrance through the recovery plan over the firm total assets in 2008, and $Guarantee_{region,2009-2010}$ is the residualized regional treatment estimated from Equation (3.1). $\gamma_s$ are department-pair fixed effects (a finer geographic division than regions) that allow us to absorb local shocks. Our identification therefore comes from within (short) sections of the border band we study. We further include $X_f$, a vector of firm characteristics

including the logarithm of firms' total assets, the logarithm of firm age, credit risk, return on assets, the ratio of dividends over sales, property, plant, and equipment (PPE) over assets, and debt over assets, as well as industry fixed effects (for 56 two-digit industries), all measured in 2008, and $X_r$, a vector of regional characteristics including the regional 2008–2010 per-capita change in public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and lending by local banks. We cluster the error term, $\epsilon_f$, at the treatment level: regions.[18] We start by establishing the internal validity of our empirical setting. Column 1 of panel A in Table 2 displays the regression coefficients of the first stage as described in Equation (3.2) at the firm level. The coefficient for $Guarantee_{region,2009-2010}$ is positive and strongly statistically significant, with a $t$-stat of 5.6, which confirms that higher treatment intensity at the regional level (excluding border areas) translates into higher loan guarantee take-up at firms located close to regional borders. Column 2, where the dependent variable is an indicator variable for receiving a guarantee, illustrates that the regional intensity is associated with a significantly higher likelihood of receiving a guarantee.

[INSERT TABLE 2]

To ensure that our first stage is driven by firms targeted by the program, we regress an indicator variable for guarantee take-up, Guarantee (1/0), on $\text{Guarantee}_{region,09-10}$, interacted with the firm-level predicted take-up propensity. We predict take-up propensity by first estimating a linear probability model on the whole firm population (excluding firms in the border area), and then using observable firm characteristics (logarithm of assets, ROA, logarithm of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit in-

[18] There were 21 regions in France as of the sample period. All results are robust to clustering at the department-pair level instead, to mitigate concerns over the issue raised by Moulton (1990).

dustry fixed effects) measured in 2008. The regression coefficients of the interaction terms are displayed in Figure 4. The figure illustrates that a higher regional treatment intensity translates into a higher likelihood to obtain a guarantee for the firms that exhibit the characteristics generally targeted by the program. In contrast, there is no effect for firms that do not have such characteristics, which we later exploit to conduct a placebo analysis.

[INSERT FIGURE 4]

In addition, we also check that our first stage is driven by firms facing high financial constraints, consistent with the program target. To do so, we split our sample along proxies for firm financial constraints widely used in the literature: credit risk, measured as the inverse of the interest coverage ratio, dividend payout, and cash flows. We run our first-stage specification on each of these subsamples and present the regression results in Internet Appendix Table A.6. We confirm that the relationship between $Guarantee_{region,2009-2010}$ and $Guarantee_{firm,2009-2010}$ is indeed driven by firms with above-median credit risk, not paying dividends, and below-median cash flows, as of 2008.

## 3.3 Effects on financial constraint

To further strengthen the validity of our first stage, we test whether a higher regional treatment intensity is associated with an increase in the quantity of bank debt combined with a similar or lower cost of debt, as predicted by a relaxation of the credit constraint for treated firms. We first run a specification similar to our first stage where the dependent variable is the firm-level growth rate of bank debt over 2008–2009.[19] As shown in column 3 of Table

[19]Because of data limitations, we can only observe the debt composition of firms until the end of 2009 and therefore can only measure the effect on bank debt in the first year of the program. This result is robust to using total debt growth rate over 2008–2010 as a dependent variable, which covers the whole treatment period, but does not zoom in on the part of debt directly affected by the program.

2, higher exposure to the loan guarantee program is indeed associated with a significantly higher growth in bank debt, which is consistent with a relaxation of financial constraints for the treated group. Second, we run a similar specification where the dependent variable is the change in the average interest rate paid on outstanding debt between 2008 and 2010.[20] Column 4 of Table 2 displays the regression coefficient. Treated firms exhibit a significantly lower interest rate, even when controlling for firm characteristics, which is consistent with the program driving the increase in debt, rather than higher local demand for credit.

# 4 Impact of Countercyclical Loan Guarantees on Employment

We now turn to analyzing the impact of exposure to the loan guarantee program during a downturn on workers' employment and earnings.

## 4.1 Firm-level effects

We first study firm-level effects on employment and productivity and keep the same specification as our first stage. We start with the short-term effects on firm employment and labor productivity: in column 1 of panel B of Table 2, we use the logarithm of the change of firm employment over 2008–2010, $\Delta_{08-10}Emp$, as dependent variable and find that being more exposed to the loan guarantee program results in significantly higher firm employment in the short run, consistent with the literature. Column 2 shows that this employment effect coincides with a decrease in labor productivity, measured by the change in value-added per

[20] We calculate the average interest rate from the yearly interest payments divided by the beginning of year amount of outstanding debt. Because of data constraints, we cannot restrict our analysis to newly issued debt.

employee, $\Delta_{08-10}VA/Emp$. In the last section of the paper we formally show that such a decrease in labor productivity is consistent with the program relaxing financial constraints for treated firms. Turning to the long-term effects, we observe in column 3 that the positive employment effect weakens over time, while column 4 shows that loan guarantees do not translate into increased total factor productivity for treated firms in the long run, as measured by the change in TFP over 2008–2015, $\Delta_{08-15}TFP$.[21]

## 4.2 Effects on worker employment and earnings

We then exploit the unique characteristics of our data to estimate worker-level effects on employment and earnings, including when workers change employers, using the following specification:

$$y_{w,2009-2015} = \beta.Guarantee_{region,2009-2010} + \delta_1.X_f + \delta_2.X_r + \delta_3.X_w + \gamma_s + \epsilon_w, \qquad (4.1)$$

where $y$ denotes years employed or cumulative earnings over our sample period (2009-2015) for worker $w$ employed as of 2008 in an establishment located within 10 miles of a regional border.[22] $\beta$, the main coefficient of interest, measures the causal effect of initial regional exposure to the loan guarantee program on workers' outcomes. We also include $X_w$, a vector of worker characteristics including worker age, gender, and occupation fixed effects all measured in 2008. Table 3 displays the coefficients. Panel A studies the cumulative effects of the loan guarantee program on years employed and earnings over the period 2009–2015. All

[21] $TFP_{f,j} = VA_{f,j}/L^{\alpha_j} \times K^{(1-\alpha_j)}$, where $f$ indexes firm, $j$ two-digit industry. *VA* is value-added, $L$ is number of employees, and $K$ is property, plant, and equipment. We compute the labor share $\alpha_j$ as the average ratio of salaries and social contributions scaled by value-added across all firms in each two-digit industry.

[22] Following Autor et al. (2014) and Yagan (2019), we normalize cumulative earnings by workers' initial earnings, that is, over the period 2006–2008.

specifications include department-pair fixed effects, regional controls, and firm-level controls. We add worker-level controls in columns 2 and 4. We observe a statistically significant and robust relationship between regional variation in program intensity and the number of years employed and cumulative earnings of workers over the 2009–2015 period. The effects are economically sizable, and the point estimate is left virtually unchanged when worker-level controls are introduced in the specification. Relative to the precrisis period, workers in a region with the average treatment experience a total gain in years employed of 0.07 years when compared to a hypothetical region with no exposure to the program.[23] A similar calculation for earnings yields an increase by 8.6% in cumulative earnings for workers in a region with the average treatment. This effect is large: given that 5.1% of the workers in our data are initially employed at firms taking up the program, the estimates imply that workers employed in firms receiving a guarantee during the financial crisis experience a total increase of 1.4 additional years in employment over the sample period, and of 1.7 times their initial annual income in cumulative earnings, or 26% in annualized terms.[24] In panel B, we run a 2SLS specification, and instrument $Guarantee_{firm,2009-2010}$ with $Guarantee_{region,2009-2010}$. The results confirm that worker exposure to the treatment has a significant effect on their labor outcomes.[25] In panel C, we implement a similar specification as in panel A, using the raw measure of regional treatment intensity. This exercise leads to similar point estimates than in panel A.

[INSERT TABLE 3]

[23]The average (raw) regional treatment is equal to 0.29 (%) of total firm assets, which we multiply by the most conservative point estimate of our regression, 0.240.

[24]These numbers are obtained by multiplying our estimates in columns 2 and 4 of panel A of Table 3 with the average regional treatment of 0.29 and then dividing by 0.051.

[25]Note that the coefficients in the 2SLS specification are larger than in the reduced form since the first-stage coefficients are less than one.

In Internet Appendix Table A.7 we show that the worker-level employment effects are driven by firms facing high financial constraints, that is, are above-median credit risk, not paying dividends, and below-median cash flows, as of 2008. Together with Table A.6, these results are reassuring, as we find employment effects only for workers in firms targeted by the program, a point we will return to later when conducting a placebo analysis. Lastly, we study the year-to-year impact of the loan guarantee program on worker outcomes. We plot the estimated effect of exposure to the loan guarantee program for each year from 2004 to 2015 on annual worker earnings in any firm in panel A of Figure 5.

Exposure to the loan guarantee program is associated with a large and statistically significant effect on annual earnings for the whole sample period following the treatment. The point estimates for 2004 to 2008 are all insignificant, which supports the absence of pre-trends and our interpretation of a causal impact of the guarantees on workers' earnings trajectories. As annual earnings are higher post treatment for the treated group, the cumulative effect on earnings keeps growing over that period. Overall, the effects of the policy on earnings are immediate and strikingly persistent until the end of the sample period.[26]

[INSERT FIG 5]

## 4.3 Effect on unemployment insurance

In developed economies, earning losses due to involuntary unemployment are partly mitigated by unemployment insurance. In France, unemployment benefits cover a fraction of the initial wage, are subject to eligibility criteria, and are earned for up to 2 years. In our data set, we can isolate earnings coming from unemployment benefits, which allows us to

[26]Internet Appendix Table A.8 shows our baseline results from Table 3 using employment and earnings in 2015 as outcomes and confirms the persistence of the worker-level employment effects.

estimate the fraction of earning losses in the counterfactual offset by unemployment insurance. We use the cumulated amount of unemployment benefits (scaled by initial earnings) during 2009–2015 as the dependent variables in our baseline specification. Table A.9 of the Internet Appendix displays the results. We find that treated workers collect significantly lower amounts of unemployment benefits over the study period. In economic terms, this point estimate indicates that workers from the average treatment region receive lower unemployment benefits over the 2009–2015 period, representing 1.3% of their initial annual income. This magnitude indicates that unemployment insurance offsets around 15% of the gap in earnings between the treated group and the counterfactual documented in Table 3.

## 4.4 Cost-per-job estimate

To conclude the analysis on employment effects, we calculate the cost-per-job(-year) resulting from the policy. We start with estimating the total number of job-years preserved by the policy. As our empirical analysis is conducted at the worker level, we multiply the average treatment of 0.29 (% of total assets) by the coefficient estimated in our baseline specification (0.240, see column 2 of Table 3) to calculate the average effect by worker. This calculation corresponds to an average gain of 0.07 years of employment for the average worker in our sample. As the full-time employee equivalent employment at SMEs in 2008 in France was 7.0 million, we obtain an estimate of around 487,000 job(-years) preserved over the period 2009–2015 ($7.0m \times 0.29 \times 0.240$).[27] The ex ante gross cost to the French government was the provision of a €683M fund, which translates into an estimate for the gross cost-per-job(-

[27]This extrapolation exercise is motivated by the comparability of SMEs in the border area with the general pool of such firms, as documented in Internet Appendix Table A.3. See `https://www.vie-publique.fr/rapport/34055-pme-2010-rapport-sur-levolution-des-pme` for the data on aggregate employment at SMEs.

year) of around €1,400.[28] The ex post gross cost of the guarantee program can be estimated as the difference between the amount of Bpifrance payments to the banks of defaulting firms less the premiums paid to Bpifrance. Banks have claimed guarantee payments for an aggregate amount of €333M, and Bpifrance has received premiums for an aggregate amount of €126M. The ex post cost is therefore €207M, which translates into an estimate for the gross cost-per-job(-year) around €425.[29] This gross cost-per-job(-year) ignores savings in unemployment and social benefits resulting from the loan guarantee program. We can easily adjust for the savings in unemployment benefits that we estimate in the previous subsection, which correspond to around €300 per worker, or €2.1 bn in aggregate. This calculation therefore yields a negative net cost for the program and the jobs it helps preserve, which would be even more pronounced if accounting for avoided social contributions. The positive employment effects need however to be contrasted with the effect of the program on labor allocation and aggregate productivity.

[28] Following Lucas and McDonald (2010), one can alternatively value the ex ante cost of the program as a put option using derivative pricing methods. Assuming a risk-free rate of 3.5%, time to maturity of 2 years, and volatility of 40%, the Black-Scholes value of a 70% guarantee on €5.3 bn loans is €640M.

[29] This gross cost-per-job is significantly smaller than estimates from the literature on fiscal multipliers in the United States (Suárez Serrato and Wingender 2016; Chodorow-Reich et al. 2012) that are closer to $30,000 per job. It is also smaller than estimates from the U.S. loan guarantee program 7(a) in Brown and Earle (2017), who find a cost-per-job of around $25,000 (over 3 years). Finally, it is of the same order of magnitude as the gross cost-per-job estimated for other employment policies implemented in France in 2009: €2,619 for short-time work subsidies (Cahuc, Kramarz, and Nevoux 2018), and €8,000 for hiring credits (Cahuc, Carcillo, and Le Barbanchon 2019), which are primarily targeted at low-skill workers.

# 5 Impact of Loan Guarantees on Labor Allocation

## 5.1 Worker retention and adjustment margin

Given our ability to follow workers over time, and observe their job-to-job transitions across firms, we turn to precisely measuring both the impact of the loan guarantee program on employment in initial firms, and how much of this effect is offset by the adjustment margin at other firms. We therefore follow prior work (e.g., Autor et al. (2014)) to decompose the overall effect on years employed and cumulated earnings in Table 4. Column 1 displays the net effect, which corresponds to the results in column 2 of Table 3. Column 2 presents the share coming from the firm at which the worker is initially employed as of 2008 and column 3 the share coming from their subsequent employment at other firms. The point estimate of column 2 captures the differences in employment and earnings obtained by workers at their initial employer.[30] The baseline coefficients of column 1 represent less than half of these effects at the initial firm, and reflect the fact that the relative employment and earning gains at the initial firm for treated workers are partially offset by counterfactual workers' mobility to other firms. Indeed, as shown in column 3, workers less exposed to the loan guarantee program are more likely to subsequently work and receive earnings from other employers over the sample period.

[INSERT TABLE 4]

This exercise highlights the benefit of using worker-level panel data to accurately assess the net employment and earnings effects of loan guarantee programs, and evidences the significant dampening effect of the program on worker mobility following a downturn. It

[30] In Internet Appendix Table A.10 we use a dummy variable equal to one if a worker separates from her initial employer instead, and we find similar results.

raises the question whether loan guarantee programs might be preventing the beneficial reallocation of the workforce toward more productive firms. This concern is particularly relevant given the persistence of the effects on both worker retention and mobility across firms. Panels B and C of Figure 5 displays in a longitudinal setting the breakdown between the effects on worker earnings at the initial firm and at other firms, and evidences significant effects 5 years after the end of the program.

## 5.2 Low versus high unemployment areas

To explore the heterogeneity of the effects of loan guarantee programs according to labor market conditions, we split our sample between low and high unemployment municipalities and run our central specification to measure the net effect and the retention effect.[31] Theoretically, the program should have less impact on net employment in tight labor markets, as workers that are displaced in the absence of support to their employers can more easily find a new job in another firm. Regression coefficients are displayed in Table 5, separately for high unemployment areas (columns 1 and 2) and low unemployment areas (columns 3 and 4). In low unemployment areas, although the retention effect is large and significant (column 4), the net effect of the loan guarantee program on workers' cumulative employment over the sample period is instead low and statistically insignificant. However, in high unemployment areas, while exposure to the program is associated with a large and significant retention effect by initial employers as in low unemployment areas (as shown in column 6 comparing the coefficients presented in columns 2 and 4), the net effect on workersâ€™ cumulative employment remains large, and of similar magnitude compared to the effect on retention. In high unemployment areas, higher retention rates at recipient firms thus benefit the employment

[31] We use municipality-level unemployment data from INSEE and define high unemployment as above 10%.

trajectories of individual workers, as it prevents them from experiencing lengthy periods of unemployment.

[INSERT TABLE 5]

## 5.3 Labor hoarding

We further investigate retention policies of treated firms by exploring heterogeneity across workforce and individual worker skill level. We first classify workers in the highest tertile of the overall worker distribution as high-skill for three different skill measures: earnings within age cohorts, hiring difficulty for a given occupation, and cognitive-analytical task content. Based on this classification, we compute the firm-level fraction of high-skill workers for each measure.[32] We then split the sample of firms at the median and implement our baseline firm-level specification to measure firm employment effects on these split samples. Regression coefficients are displayed in panel A of Table 6. For each measure of skill, we observe that the positive firm employment effect previously documented in Table 2 results from firms with a high-skill workforce. Such heterogeneity is consistent with higher incentives to hoard workers for such firms, as high-skill workers might be costlier to rehire in the future. We then split the worker sample at the median of the respective skill measure, and implement our baseline worker-level specification, using years employed and cumulative earnings at the initial firm as the dependent variable. Such analysis yields consistent results with the firm-level one. Worker retention resulting from the program is higher for high-skill workers, although retention is also increased for lower-skill workers. This analysis suggests

[32]We use the universe of workers from DADS Postes to define high-skill workers and to compute the firm-level skill intensity measures. The hiring difficulty data are from a survey on the personnel needs of firms, the Enquête *Besoins en Main d'Oeuvre* (BMO). The task content data of French occupations is described in Le Barbanchon and Rizzotti (2020). We thank the authors for sharing the data.

that firms prioritize retaining high-skill workers as they might be particularly costly to replace. While beneficial to the firm, such retention policies might however have negative aggregate implications, as the economy could benefit from a reallocation of these workers to more productive firms, an issue that we explore in the following section.[33]

[INSERT TABLE 6]

## 5.4 Dampened worker reallocation to more productive firms

Dampening the reallocation happening following downturns is detrimental to the economy if workers would have moved to more productive firms absent the public intervention. Since the richness of our matched employer-employee data allows us to track both the employment history of individual workers over time and the identity of their new employers, we can observe which type of firms workers reallocate to in our counterfactual. Specifically, we focus on the effects of treatment on employment and earnings at other firms, which we report in column 1 of Table 7. To study whether counterfactual workers tend to move to more or less productive firms than the firm they worked at as of 2008, we implement a similar specification, splitting the set of other firms between high and low productivity and growth per the following measures: labor productivity, measured by value-added per employee in columns 2 and 3, total factor productivity for columns 4 and 5, return on assets for columns 6 and 7, and sales growth for columns 8 and 9. The coefficients in columns 2, 4, 6 and 8, are significantly negative, while the ones of columns 3, 5, 7, and 9, are not statistically different from zero. Treated workers are therefore less likely to work and earn wages from more productive firms than their initial firm in 2008 during the sample period, relative to

[33]We study differential effects of the loan guarantee program on firms' long-term (over 2008–2015) performance and survival in Internet Appendix Table A.11, but do not find significant differences across our measures of workforce skill intensity.

counterfactual workers. This analysis evidences that, absent the loan guarantee program, a significant share of workers from treated firms would have moved to more productive firms following the downturn.[34]

[INSERT TABLE 7]

## 5.5 Heterogeneity in reallocation dampening by worker characteristics

Next, we explore whether this dampening of worker reallocation toward more productive firms is more pronounced for certain types of workers. We reproduce the specification of columns 2 and 3 of Table 7, while doing a second split of the worker sample across our previously used measures of skills: earnings, hiring difficulty, and cognitive-analytical skills. Regression coefficients are provided in Table 8. We observe that coefficients between columns 1 and 2, corresponding to high-skill workers, are significantly different from each other, while this is not the case for columns 3 and 4, which cover low-skill workers. This illustrates that the reallocation toward more productive firms is particularly dampened by the loan guarantee program for high-skill workers. While retention of such workers is likely beneficial to their initial employers, their reallocation toward more productive firms would be particularly beneficial to the overall economy. This evidence further highlights the hidden cost of loan guarantee programs resulting from reducing worker mobility.

[INSERT TABLE 8]

[34] In Internet Appendix Table A.12 we use a dummy variable equal to one if a worker moves to a more/less productive firm as an outcome instead and find similar results. In Figure 6 we study the year-by-year dynamics and confirm the absence of pre-trends for the dampened worker reallocation to more productive firms.

# 6 Alternative Explanations

In this section, we address alternative mechanisms that could explain our central results.

## 6.1 Confounding local shocks and pre-trends

A legitimate empirical concern is that our treatment variable is correlated with other local shocks, potentially unobserved. We first check that initial worker and firm characteristics are not correlated with the treatment variable. For this, we run a similar cross-sectional specification as (3.2) with workers' and firms' characterstics as dependent variables, all measured in 2008. We present the results in Internet Appendix Table A.13. The differences across low and high exposure regions in workers' earnings, hours worked, unemployment benefits, as well as firm age, size, return on assets, credit risk, payout ratio, tangibility, and leverage, all measured in 2008, are all small and statistically insignificant. We also test for the presence of pre-trends in economic activity correlated with our treatment variable. In panel A of Table A.14, we proxy for economic activity by summing the value-added of firms located in the border area of each region, scaled by the corresponding population. In panels B and C we study economic activity pre-trends for our sample firms, measured by value-added and employment. Figure A.1 confirms the absence of pre-trends in terms of economic activity and credit outcomes for our sample firms. Taken together, these tests mitigate concerns over potential diverging pre-trends in economic activity before the program. Further, we directly control for other public policies which may confound our estimates as well as for the political preferences of the region. Specifically, we control for changes in EU funds the region received from 2008–2010, the size of a short-time work program implemented in 2009 in the region, and the vote share of the left party in regional elections in 2004, the last election before

the start of the program.[35] Reassuringly, these regional policies and political preferences are not correlated with our treatment as shown in Table A.15, and we find similar results when controlling for these regional confounders shown in Table A.16. To address concerns over unobserved shocks, we also conduct a sample split allowing a placebo analysis: we separate firms with a high propensity to take-up guarantees from firms with a low propensity. Per Figure 4, the regional abnormal treatment intensity only affects firms from the top-three quintiles of take-up propensity. We therefore verify that our results do not hold when restricting to workers of firms from the bottom-two quintiles. Reassuringly, the coefficients of the treatment variable on the employment and earnings of workers of low take-up propensity firms are all small and statistically insignificant in each specification of Table 9.[36] Such a test allows us to reject that other local economic or policy shocks affect differentially the outcomes of workers on each side of regional borders, in a way that could have biased the treatment effects of the loan guarantee program.

[INSERT TABLE 9]

## 6.2 Spillovers

One may be concerned that the program distorts competition in product markets in favor of firms located in regions that are more exposed to the guarantee program. Under this hypothesis, our coefficients would also reflect business-stealing effects between more and less exposed firms on each side of the regional borders. We address this concern by removing nontradable industries from our sample (e.g., restaurants), where local demand spillovers

[35] Cahuc, Kramarz, and Nevoux (2018) describe the data on short-term work. We thank the authors for sharing the data.

[36] In Internet Appendix Table A.17, we present the same results in a specification in which we interact the regional treatment intensity with a dummy variable equal to one for firms with high take-up propensity. Panel B shows that the results are robust to the inclusion of region fixed effects.

could bias our estimates upward, and present the results in panel A of Internet Appendix Table A.18. Reassuringly, our baseline results are quantitatively comparable when we restrict the sample to tradable industries only. A related concern pertains to local labor market effects. Workers from low treatment regions might benefit from the proximity to nearby firms headquartered in high treatment regions when losing their jobs. Such a phenomenon would however induce a downward bias in our estimates. As shown in Internet Appendix Table A.19, we fail to find evidence that workers move from low to high treatment regions over the sample period.

### 6.3 Robustness to border area definition

Finally, we ensure that our results are robust to the definition of regional border areas. In panels B and C of Internet Appendix Table A.18, we use 5 and 15 instead of 10 miles from the regional border as a cutoff to define the border area. The results are consistent with our baseline estimates and remain highly statistically significant, despite the substantially lower sample size when we restrict to 5 miles.

## 7 Aggregate Implications

In this section, we first highlight the role of the program targeting small firms in the labor allocation result, then propose a formal micro-foundation for our empirical results that builds on this observation, and finally leverage this framework to examine the implications of the program for aggregate productivity. Our theoretical framework is derived from a simplified version of Jermann and Quadrini (2012), in which firms need to finance labor in advance, that we connect to work on the link between resource misallocation and aggregate productivity

(Hsieh and Klenow 2009). We model a loan guarantee as a subsidy to the cost of financing of the firm. The loan guarantee program leads to an increase in labor demand for treated firms, which, to a lower extent, depresses labor demand for untreated firms through crowding-out effects on the labor market. We quantify the impact of the program on aggregate productivity based on either employment or labor wedge micro-estimates and find a reduction of around -1% with both approaches.

## 7.1 Program target as the source of labor misallocation

As previously documented, the loan guarantee program limits the reallocation of workers to high productivity firm. This phenomenon results from the program targeting small firms, which typically exhibit lower marginal labor productivity compared to larger firms. To support this claim, we first confirm in our data the evidence in Bartelsman, Haltiwanger, and Scarpetta (2013) and Garicano, Lelarge, and Van Reenen (2016) showing a positive correlation between labor productivity and firm size in a large set of developed countries, including France. In Figure A.2 in the Internet Appendix, we document a positive correlation in the cross-section of French firms in 2008, the announcement year of the program, between labor productivity, measured as value-added over employment, and firm size, measured as the logarithm of firm employment.[37] Furthermore, in Internet Appendix Table A.20, we reproduce the results on worker reallocation presented in Table 7 distinguishing between small and large firms, instead of low versus high labor productivity. We find robust evidence of a reduction in worker reallocation toward large firms due to the program, consistent with

[37]Regressing value-added over employees ($VA/Emp$) on logarithm of the employment yields a regression coefficient of 1.4 significant at the 1% level. For confirming this relationship in recent years, see the 2019 SBA Fact Sheet produced by the European Commission for statistics on France and the EU as a whole. In 2018, French SMEs accounted for 64.1% of total employment but only 55.8% of total value-added (against 35.9% of employment and 44.2% of value-added for large firms). These statistics are similar for the EU as a whole where SMEs represent 66.6% of employment, but only 56.4% of aggregate value-added.

the difference in size between treated and untreated firms being the underlying driver of the dampening of worker reallocation toward more productive firms we document in Table 7. Specifically, during our sample period, treated workers are less likely to work and earn wages from firms larger than their initial firm, relative to counterfactual workers. These results corroborate that, in the absence of the loan guarantee program, a significant share of workers from treated firms would have moved toward larger firms, which on average have higher labor productivity.

## 7.2 Theoretical framework

Our setting is a one-period general equilibrium model in which firms use labor, $L$, to produce, but only receive payments after selling their output, $Y$, so that they have to finance labor costs in advance. The economy consists of two sets of firms: $t$ (for treated) and $u$ (for untreated), with respective mass $\mu$ and $1-\mu$, which differ in whether they are eligible or not to the program subsidy. We allow these two types of firms to have different levels of productivity, and we represent them below as $A_t$ and $A_u$. Both sets of firms $i \in (t, u)$ have decreasing returns-to-scale technology in labor:

$$Y_i = A_i L_i^{\alpha},$$

where $\alpha < 1$ captures the decreasing returns to scale. Firms maximize profit $\Pi_i$:

$$\max_{L_i} \Pi_i(L_i) = pY_i - w(1+\tau_i)L - R_i(wL_i), \tag{7.1}$$

where $w$ is the competitive wage and $R_i$ is the cost of financing for the firm of type $i$. $\tau_i$ are nonfinancial distortions, and capture, for instance, frictions associated to labor regulation,

which may vary depending on the type of the firm. In our empirical setting, to the extent that labor regulation is stronger on large firms than on small firms, we expect $\tau_u > \tau_t$.[38] We take output as the numeraire such that $p = 1$. Using the first-order condition for the maximization of profit with respect to labor, we get:

$$MRPL_i = \frac{\alpha Y_i}{L_i} = w(1 + R_i + \tau_i), \tag{7.2}$$

where $MRPL_i$ is the marginal revenue product of labor, and $(1 + R_i + \tau_i)$ is the wedge on labor driven by the sum of financial and nonfinancial distortions. The marginal product of labor is larger for untreated firms than for treated firms if the gap in nonfinancial labor distortions $\tau_u - \tau_t$ is large enough, namely, larger than the differences in the cost of financing between treated and untreated firms $(R_t - R_u)$. Under the assumption that treated and untreated firms share the same $\alpha$, the difference in $MRPL$ between untreated and treated firms is proportional to the observed difference in labor productivity, defined as value-added over employees, between the two sets of firms. In the data, we do find that labor productivity is larger for untreated firms before the program. Using Equation (7.2), we get that labor demand is:

$$L_i^* = \left( \frac{\alpha A_i}{w(1 + R_i + \tau_i)} \right)^{\frac{1}{1-\alpha}}. \tag{7.3}$$

Labor demand is decreasing in the wage $w$, the cost of financing $R_i$, nonfinancial distortions $\tau_i$, and increasing in productivity, $A_i$. The household maximizes:

[38] For evidence of higher labor regulation on large firms in France and India, see Garicano, Lelarge, and Van Reenen (2016) and Amirapu and Gechter (2020). Employment protection legislation in several developed countries contains provisions that depend on the size of firms and/or establishments. This is present in many aspects of the prevailing provisions (e.g., rules regarding fixed-term contracts, redundancy procedures, prenotification periods, severance payments and requirements for collective dismissals) for countries like Italy, Germany, France, and Spain (Guner, Ventura, and Xu 2008).

$$U(C, L) = C - \zeta \frac{L^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}},$$

where $C$ is the numeraire, $L$ is labor supply, and $\zeta$ captures the disutility from working, subject to the budget constraint:

$$C \leq wL + \Pi - T,$$

where $\Pi$ is the aggregate profits of firms, which are owned by the household, and $T$ is a lump-sum tax financing the program, which can be negative. The first-order conditions allow us to express labor supply as:

$$L_s^* = \left(\frac{w}{\zeta}\right)^{\epsilon}, \tag{7.4}$$

where $\epsilon$ is the labor supply elasticity. The equilibrium wage $w^*$ is obtained from the market clearing condition, by equating demand and supply on the labor market. We model the guarantee program as providing treated firms with a subsidy to their cost of financing. We write the guaranteed cost of financing in the post treatment period $R_{t,1} < R_{t,0}$, and derive employment at both treated, $L_{t,1}^*$, and untreated firms, $L_{u,1}^*$, when the loan guarantee program is implemented, which we compare to their counterfactual levels represented by $L_{t,0}^*$ and $L_{u,0}^*$. We can thus assess the effect of the program on aggregate employment and aggregate productivity. See the Internet Appendix for the proofs. Employment growth at treated firms is obtained from Equation (7.3) and is equal to:

$$\frac{L_{t,1}^*}{L_{t,0}^*} = \left(\frac{1+R_{t,0}+\tau_t}{1+R_{t,1}+\tau_t}\right)^{\frac{1}{1-\alpha}} \times \left(\frac{w_0^*}{w_1^*}\right)^{\frac{1}{1-\alpha}}. \tag{7.5}$$

The first term on the right-hand side of Equation (7.5) captures the effect of higher labor demand triggered by the program subsidy (as $R_{t,1} < R_{t,0}$). The second term captures the crowding-out of labor demand through wage increases. Untreated firms do not receive the subsidy, and their employment growth is given by:

$$\frac{L^*_{u,1}}{L^*_{u,0}} = \left(\frac{w^*_0}{w^*_1}\right)^{\frac{1}{1-\alpha}}, \tag{7.6}$$

which is negative because untreated firms are negatively affected by the increase in wage triggered by the higher labor demand from treated firms. Next, we can derive the effect of the program on aggregate employment as:

$$\frac{\mu L^*_{t,1} + (1-\mu)L^*_{u,1}}{\mu L^*_{t,0} + (1-\mu)L^*_{u,0}} = \left(\frac{w^*_1}{w^*_0}\right)^{\epsilon}. \tag{7.7}$$

As expected, the positive effect of the subsidy on aggregate employment is lower in markets with lower labor supply elasticity $\epsilon$, in which case employment growth at treated firms is largely offset by employment declines at untreated firms.Finally, we show in the Internet Appendix, as expected, that the program leads to a decline in aggregate productivity if $MRPL_{t,0} < MRPL_{u,0}$ (in which case a subsidy to the cost of financing of treated firms increases the gap in $MRPL$ between the two groups of firms, and in turn increases labor misallocation).[39] Last, we derive the effect of the program on aggregate productivity as a

[39]For the sake of simplicity, we abstract from capital in the model and focus, as for our empirical analysis, on the role of labor reallocation in explaining the effect of the loan guarantee program on aggregate productivity. That said, in our data, the marginal revenue product of capital $MRPK$, measured as value-added over capital stock, is as $MRPL$, larger for untreated firms than for treated firms before the program, suggesting that potential capital reallocation effects would reinforce the ones from labor allocation we focus on.

function of employment at treated and untreated firms pre- versus post-program, and obtain:

$$\frac{A_{agg,1}}{A_{agg,0}} = \frac{\mu + (1-\mu)\left(\frac{A_u}{A_t}\right)\left(\frac{L^*_{u,1}}{L^*_{t,1}}\right)^{\alpha}}{\mu + (1-\mu)\left(\frac{A_u}{A_t}\right)\left(\frac{L^*_{u,0}}{L^*_{t,0}}\right)^{\alpha}} \left(\frac{L_{s,0}}{L_{s,1}}\right)^{\alpha}. \tag{7.8}$$

## 7.3 Estimating the impact of the program on aggregate productivity

Equation (7.8) allows us to obtain an estimate for the effect of the program on aggregate productivity. It depends on employment at untreated and treated firms pre- versus post-program, on the ratio of productivity $A_u/A_t$ between these two groups, which we both observe empirically, and the $\alpha$ parameter for firms' production functions that we can calibrate with standard values used in the literature. We set $\alpha = 2/3$. We exploit our empirical estimates to calibrate employment growth at treated firms and untreated firms, and obtain that employment at treated firms has increased by 2.08%, and employment at untreated firms has decreased by 0.98%.[40] In the aggregate, using the employment shares of SMEs versus non-SMEs in 2008, we find that the program has a positive effect on employment of +0.47%.[41] We then use the relative number of SMEs in the economy and their employment in 2008 to calibrate $\mu = 0.996$, $L^*_{t,0}$, and $L^*_{u,0}$, and the ratio of average productivity between untreated and treated firms in 2008 to calibrate $\frac{A_u}{A_t} = 1.2$.[42] Plugging these values in Equation (7.8), we

[40]For the estimate on employment growth at treated firms, we multiply the average treatment of 0.29 by the coefficient estimated in column 2 of Table 4 (initial firm, 0.503) divided by seven (the number of years in our sample), and get 0.29×0.503/7=2.08%. For employment growth at untreated firms, we multiply the average treatment of 0.29 by the coefficient estimated in column 3 of Table 4 (other firm, -0.264), and the ratio of employment at SMEs over employment at non-SMEs (7.0/7.8), divided by seven, and get -0.98%.

[41]This number is consistent with the estimate of 487,000 job-years preserved over the period 2009–2015 discussed in Section 5.4, once extrapolated over the 7 years of the sample period and applied to aggregate employment in France in 2008 of 14.8 million: $0.47\% \times 7 \times 14.8 = 0.487$.

[42]This ratio is based on a comparison of TFP between firms not receiving a guarantee and those receiving a guarantee under the recovery plan, using the whole sample of French firms.

infer that the program had a negative impact on aggregate productivity, of around $-0.65\%$. For robustness, we reevaluate the impact of the program on aggregate productivity using empirical estimates for the effect of the program on the labor wedge of treated firms, rather than the effect of the program on their employment, and find consistent results. As indicated in Equation (7.2), the labor wedge $(1+R+\tau)$ at equilibrium is equal to value-added over the wage bill, $\frac{Y}{wL}$, times $\alpha$, a parameter which is assumed constant across firms.[43] We present in panel B of Internet Appendix Table A.21 the results of the program's impact on labor wedges, by substituting value-added over the wage bill as dependent variable in the firm-level specification used in Table 2. We find a significant decline in labor wedges for treated firms by around 3.2%, relative to the average labor wedge in our sample.[44] At equilibrium, the ratio of employment growth at treated firms to that at untreated firms equals the change in the labor wedge raised to the power $\frac{1}{1-\alpha}$: $\left(\frac{1+R_{t,0}+\tau_t}{1+R_{t,1}+\tau_t}\right)^{\frac{1}{1-\alpha}}$.[45] Keeping the same calibrated value for $\alpha = 0.66$, and maintaining the impact of the program on aggregate employment at 0.47%, we recompute the drop in aggregate productivity using the estimate of the labor wedge for treated firms. This calculation yields a 1.38% decline in aggregate productivity, in the same order of magnitude as the decline obtained using the employment estimates. This alternative calculation confirms that the negative impact on aggregate productivity is economically significant when compared to the employment gains, and needs to be weighed in by policymakers when deciding on implementing loan guarantee programs.

[43] We show in Internet Appendix Table A.21, panel A, that the results regarding treated firms having lower $MRPL$ than untreated firms (measured using labor productivity $\frac{Y}{L}$, see Figure 1, also holds for labor wedges.)

[44] This number is obtained by multiplying the estimate in panel B of Internet Appendix Table A.21 with the average regional treatment of 0.29 and then dividing by 1.997, the sample mean of value-added over wage bill in 2008. The magnitude of the treatment effect on $VA/Emp$ presented in column 2 of Table 2 is in the same ballpark, a drop of around 2.4%, relative to the average labor productivity in our sample.

[45] This is a direct implication of Equations (7.5) and (7.6).

# 8 Conclusion

In this paper, we use administrative micro data to examine how exposure to a loan guarantee program implemented in France during the 2008–2009 financial crisis affects the employment and earnings trajectories of workers over the medium run. We find that exposure to the program results in a significantly higher likelihood of being employed over the next 7 years, which translates into significantly higher cumulated earnings, and lower unemployment benefits. Our findings have important implications for the targeting of loan guarantee programs, which appears more effective at sustaining aggregate employment in periods or areas characterized by slack labor markets. In tight labor markets, an unintended effect of the policy is to dampen the reallocation of workers toward more productive firms. This is especially true for workers with high earnings, in high demand, and with high cognitive-analytical task content. Based on a parsimonious theoretical framework, we quantify the impact of the program on aggregate productivity, and find a reduction of around 1%, which is economically significant when compared to the employment gains.

**Code Availability**: The replication code is available in the Harvard Dataverse at `https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi%3A10.7910%2FDVN%2FOCQAGA&version=DRAFT`

# References


Acabbi, E., E. Panetti, and A. Sforza. 2020. The financial channels of labor rigidities: Evidence from portugal. *Working Paper, Universidad Carlos III de Madrid* .

Amirapu, A., and M. Gechter. 2020. Labor Regulations and the Cost of Corruption: Evidence from the Indian Firm Size Distribution. *The Review of Economics and Statistics* 102:34–48. ISSN 0034-6535. doi:10.1162/rest_a_00837.

Autor, D., D. Cho, L. D. Crane, M. Goldar, B. Lutz, J. Montes, W. B. Peterman, D. Ratner, D. Villar, and A. Yildirmaz. 2022. An evaluation of the paycheck protection program using administrative payroll microdata. *Journal of Public Economics* 211:104664–. ISSN 0047-2727. doi:https://doi.org/10.1016/j.jpubeco.2022.104664.

Autor, D. H., D. Dorn, G. H. Hanson, and J. Song. 2014. Trade adjustment: Worker level evidence. *Quarterly Journal of Economics* 129:1799–860. doi:10.1093/qje/qju026.

Babina, T. 2020. Destructive creation at work: How financial distress spurs entrepreneurship. *The Review of Financial Studies* 33:4061–101.

Bachas, N., O. S. Kim, and C. Yannelis. 2021. Loan guarantees and credit supply. *Journal of Financial Economics* 139:872–94.

Baghai, R., R. Silva, V. Thell, and V. Vig. 2021. Talent in distressed firms: Investigating the labor costs of financial distress. *Journal of Finance* 76(6):2907–61.

Bai, J., D. Carvalho, and G. M. Phillips. 2018. The impact of bank credit on labor reallocation and aggregate industry productivity. *The Journal of Finance* 73:2787–836. doi:https://doi.org/10.1111/jofi.12726.

Barbosa, L., A. Bilan, and C. Celerier. 2019. Credit supply and human capital: Evidence from bank pension liabilities. *Working Paper, Bank of Portugal* .

Bartelsman, E., J. Haltiwanger, and S. Scarpetta. 2013. Cross-country differences in productivity: The role of allocation and selection. *American Economic Review* 103:305–34. doi:10.1257/aer.103.1.305.

Bartik, A. W., Z. Cullen, E. L. Glaeser, M. Luca, C. Stanton, and A. Sunderam. 2020. The targeting and impact of paycheck protection program loans to small businesses. *Working Paper, University of Illinois at Urbana-Champaign* .

Beck, T., L. F. Klapper, and J. C. Mendoza. 2010. The typology of partial credit guarantee funds around the world. *Journal of Financial Stability* 6:10–25. ISSN 1572-3089. doi:https://doi.org/10.1016/j.jfs.2008.12.003.

Benmelech, E., and N. Tzur-Ilan. 2020. The determinants of fiscal and monetary policies during the covid-19 crisis. *Working Paper, Northwestern University* .

Bentolila, S., M. Jansen, and G. Jiménez. 2018. When credit dries up: Job losses in the great recession. *Journal of the European Economic Association* 16:650–95.

Berton, F., S. Mocetti, A. F. Presbitero, and M. Richiardi. 2018. Banks, firms, and jobs. *The Review of Financial Studies* 31:2113–56.

Black, S. E. 1999. Do Better Schools Matter? Parental Valuation of Elementary Education. *The Quarterly Journal of Economics* 114:577–99. ISSN 0033-5533. doi: 10.1162/003355399556070.

Blanchard, O., T. Philippon, and J. Pisani-Ferry. 2020. A new policy toolkit is needed as countries exit covid-19 lockdowns. *Peterson Institute for International Economics Policy Brief* 20–8.

Blattner, L., L. Farinha, and F. Rebelo. 2023. When losses turn into loans: The cost of weak banks. *American Economic Review* .

Bonfim, D., C. Custodio, and C. Raposo. 2023. Supporting small firms through recessions and recoveries. *Journal of Financial Economics* 147:658–88.

Bord, V. M., V. Ivashina, and R. D. Taliaferro. 2021. Large banks and small firm lending. *Journal of Financial Intermediation* 48:100924–.

Brown, J. D., and J. S. Earle. 2017. Finance and growth at the firm level: Evidence from sba loans. *The Journal of Finance* 72:1039–80.

Brunnermeier, M., and A. Krishnamurthy. 2020. Corporate debt overhang and credit policy. *Brookings Papers on Economic Activity* 2020:447–502.

Caggese, A., V. Cuñat, and D. Metzger. 2019. Firing the wrong workers: Financing constraints and labor misallocation. *Journal of Financial Economics* 133:589–607.

Cahuc, P., S. Carcillo, and T. Le Barbanchon. 2019. The effectiveness of hiring credits. *Review of Economic Studies* 86:593–626. doi:10.1093/restud/rdy011.

Cahuc, P., F. Kramarz, and S. Nevoux. 2018. When short-time work works. *Working Paper, IZA Institute of Labor Economics* .

Chen, B. S., S. G. Hanson, and J. C. Stein. 2017. The decline of big-bank lending to small business: Dynamic impacts on local credit and labor markets. *NBER Working Paper* .

Chetty, R., J. N. Friedman, N. Hendren, M. Stepner, and T. O. I. Team. 2023. The economic impacts of covid-19: Evidence from a new public database built using private sector data. *Quarterly Journal of Economics* Advance Access published October 4, 2023, 10.1093/qje/qjad048.–.

Chodorow-Reich, G. 2014. The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics* 129:1–59. ISSN 0033-5533. doi:10.1093/qje/qjt031.

Chodorow-Reich, G., L. Feiveson, Z. Liscow, and W. G. Woolston. 2012. Does state fiscal relief during recessions increase employment? evidence from the american recovery and reinvestment act. *American Economic Journal: Economic Policy* 4:118–45. doi:10.1257/pol.4.3.118.

Core, F., and F. De Marco. 2023. Information technology and credit: Evidence from public guarantees. *Management Science* Advance Access October 31, 2023, 10.1287/mnsc.2023.4957.–.

CRS. 2019. Small business administration 7(a) loan guaranty program. Report, Congressional Research Service.

D'Acunto, F., G. Tate, and L. Yang. 2017. Correcting market failures in entrepreneurial finance. *Working Paper, Georgetown University* .

de Andrade, F., and D. Lucas. 2009. Why do guaranteed sba loans cost borrowers so much? *Working Paper* .

de Blasio, G., S. D. Mitri, A. D'Ignazio, P. F. Russo, and L. Stoppani. 2018. Public guarantees to sme borrowing. a rdd evaluation. *Journal of Banking and Finance* 96:73 – 86. ISSN 0378-4266. doi:https://doi.org/10.1016/j.jbankfin.2018.08.003.

Dube, A., T. W. Lester, and M. Reich. 2010. Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties. *The Review of Economics and Statistics* 92:945–64. ISSN 0034-6535. doi:10.1162/REST_a_00039.

Duygan-Bump, B., A. Levkov, and J. Montoriol-Garriga. 2015. Financing constraints and unemployment: Evidence from the great recession. *Journal of Monetary Economics* 75:89 – 105. ISSN 0304-3932. doi:https://doi.org/10.1016/j.jmoneco.2014.12.011.

Fonseca, J., and B. Van Doornik. 2022. Financial development and labor market outcomes: Evidence from brazil. *Journal of Financial Economics* 143(1):550–68.

Garicano, L., C. Lelarge, and J. Van Reenen. 2016. Firm size distortions and the productivity distribution: Evidence from france. *American Economic Review* 106:3439–79. doi:10.1257/aer.20130232.

Giroud, X., and H. M. Mueller. 2017. Firm leverage, consumer demand, and employment losses during the great recession. *The Quarterly Journal of Economics* 132:271–316.

Giupponi, G., and C. Landais. 2022. Subsidizing Labour Hoarding in Recessions: The Employment and Welfare Effects of Short-time Work. *The Review of Economic Studies* ISSN 0034-6527. doi:10.1093/restud/rdac069. Rdac069.

Gonzalez-Uribe, J., and S. Wang. 2022. The real effects of small-firm credit guarantees during recessions. *Working Paper, London School of Economics and Political Science* .

Gortmaker, J., J. Jeffers, and M. Lee. 2020. Labor reactions to credit deterioration: Evidence from linkedin activity. *Working Paper* .

Granja, J., C. Makridis, C. Yannelis, and E. Zwick. 2022. Did the paycheck protection program hit the target? *Journal of Financial Economics* 145:725–61. ISSN 0304-405X. doi:https://doi.org/10.1016/j.jfineco.2022.05.006.

Greenstone, M., A. Mas, and H.-L. Nguyen. 2020. Do credit market shocks affect the real economy? quasi-experimental evidence from the great recession and "normal" economic times. *American Economic Journal: Economic Policy* 12:200–25.

Guner, N., G. Ventura, and Y. Xu. 2008. Macroeconomic implications of size-dependent policies. *Review of Economic Dynamics* 11:721–44. ISSN 1094-2025. doi:https://doi.org/10.1016/j.red.2008.01.005.

Holmes, T. 1998. The effect of state policies on the location of manufacturing: Evidence from state borders. *Journal of Political Economy* 106:667–705.

Holmstrom, B., and J. Tirole. 1997. Financial intermediation, loanable funds, and the real sector. *The Quarterly Journal of Economics* 112:663–91.

Hsieh, C.-T., and P. J. Klenow. 2009. Misallocation and manufacturing tfp in china and india. *The Quarterly Journal of Economics* 124:1403–48. ISSN 00335533, 15314650.

Huang, R. R. 2008. Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across us state borders. *Journal of Financial Economics* 87:678–705. ISSN 0304-405X. doi:https://doi.org/10.1016/j.jfineco.2007.01.004.

Hubbard, G. R., and M. Strain. Forth. Has the paycheck protection program succeeded? *Brookings Papers of Economic Activity* .

Jermann, U., and V. Quadrini. 2012. Macroeconomic effects of financial shocks. *American Economic Review* 102:238–71. doi:10.1257/aer.102.1.238.

Khwaja, A. I., and A. Mian. 2005. Do Lenders Favor Politically Connected Firms? Rent Provision in an Emerging Financial Market. *The Quarterly Journal of Economics* 120:1371–411. ISSN 0033-5533. doi:10.1162/003355305775097524.

Le Barbanchon, T., and N. Rizzotti. 2020. The task content of french jobs. *Working Paper, Bocconi University* .

Lelarge, C., D. Sraer, and D. Thesmar. 2010. *Entrepreneurship and credit constraints: Evidence from a french loan guarantee program.* in Josh Lerner and Antoinette Schoar, eds.: International Differences in Entrepreneurship.

Li, L., and P. E. Strahan. 2021. Who supplies ppp loans (and does it matter)? banks, relationships, and the covid crisis. *Journal of Financial and Quantitative Analysis* 56:2411–38.

Lucas, D., and R. McDonald. 2010. Valuing government guarantees: Fannie and freddie revisited. In *Measuring and managing federal financial risk*, 131–54. University of Chicago Press.

Moulton, B. R. 1990. An illustration of a pitfall in estimating the effects of aggregate variables on micro units. *The review of Economics and Statistics* 334–8.

Mullins, W., and P. Toro. 2018. Credit guarantees and new bank relationships. *Working Paper, Banco de España* .

Neumark, D., and D. Grijalva. 2017. The employment effects of state hiring credits. *ILR Review* 70:1111–45. doi:10.1177/0019793916683930.

Philippon, T. 2021. Efficient programs to support businesses during and after lockdowns. *The Review of Corporate Finance Studies* 10:188–203.

Restuccia, D., and R. Rogerson. 2008. Policy distortions and aggregate productivity with heterogeneous establishments. *Review of Economic Dynamics* 11:707–20. ISSN 1094-2025. doi:https://doi.org/10.1016/j.red.2008.05.002.

Stiglitz, J. E., and A. Weiss. 1981. Credit rationing in markets with imperfect information. *The American Economic Review* 71:393–410.

Suárez Serrato, J. C., and P. Wingender. 2016. Estimating local fiscal multipliers. *Working Paper, Stanford University* .

Yagan, D. 2019. Employment hysteresis from the great recession. *Journal of Political Economy* 127:2505–58.

# 9 Figures and Tables

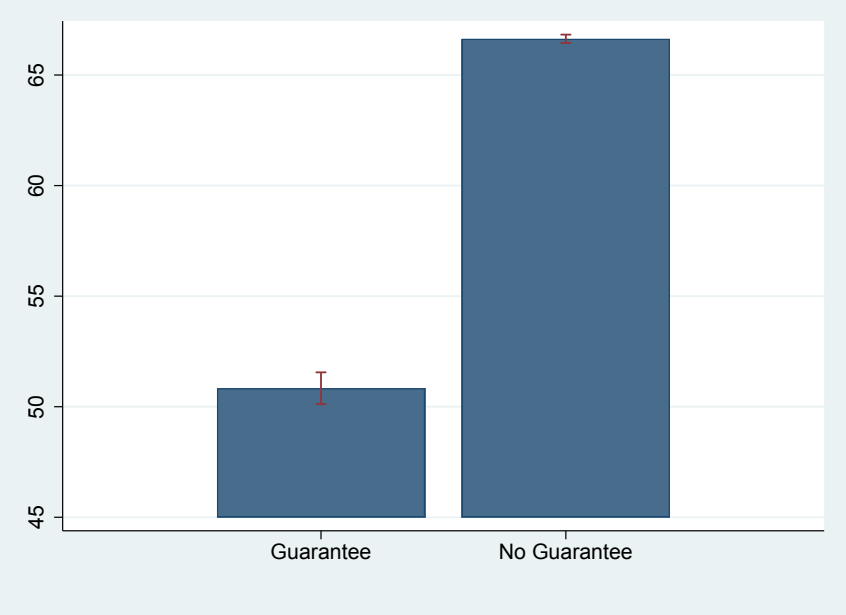



**Figure 1**
Labor productivity by guarantee status

This figure displays average labor productivity measured by value-added (in thousand euros) over employees (VA/Emp) in 2008 for the population of French firms. The first bar represents labor productivity of firms receiving a guarantee under the recovery plan, while the second bar represents the labor productivity of firms not receiving a guarantee. Confidence intervals at the 99.99% level are represented in red. Table A.1 in the Internet Appendix shows that the pattern is robust to controlling for industry fixed effects.

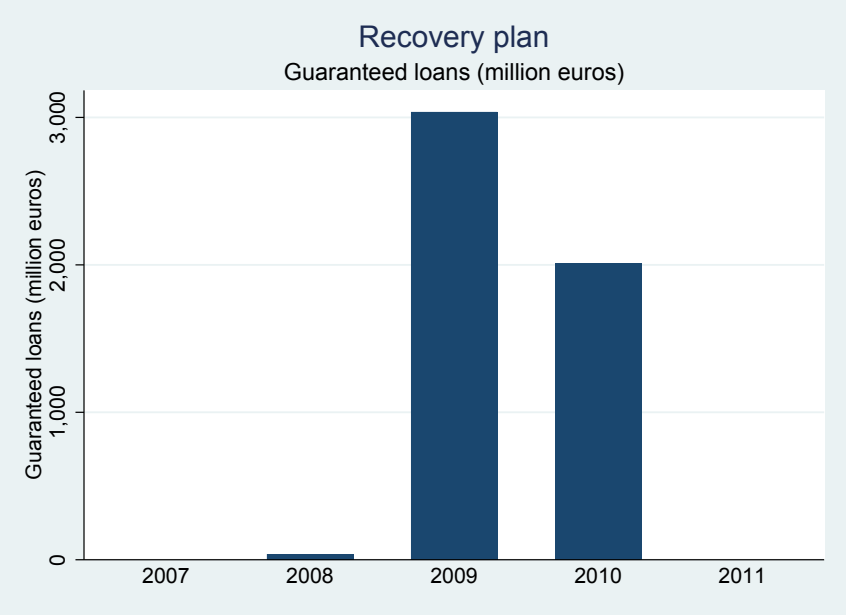



**Figure 2**
Yearly volume of guarantees of the Recovery Plan

This figure displays the total volume of guarantees by Bpifrance as part of the recovery plan.

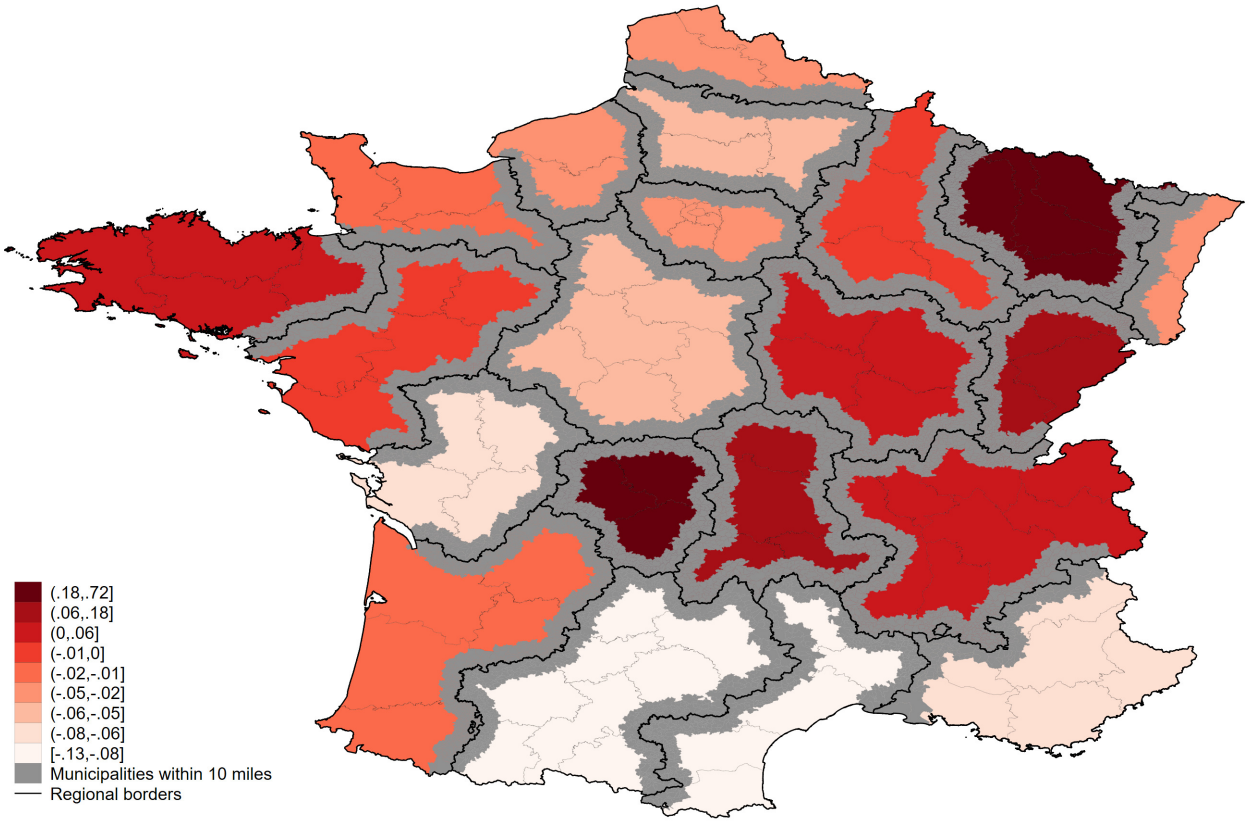



**Figure 3**
Regional intensity of loan guarantee intervention

This figure displays the regional intensity of intervention by Bpifrance, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area, see Table 1. Darker colors represent regions with higher treatment intensity. The gray area corresponds to municipalities within 10 miles of a regional border. Thin lines in gray represent department boundaries within regions.

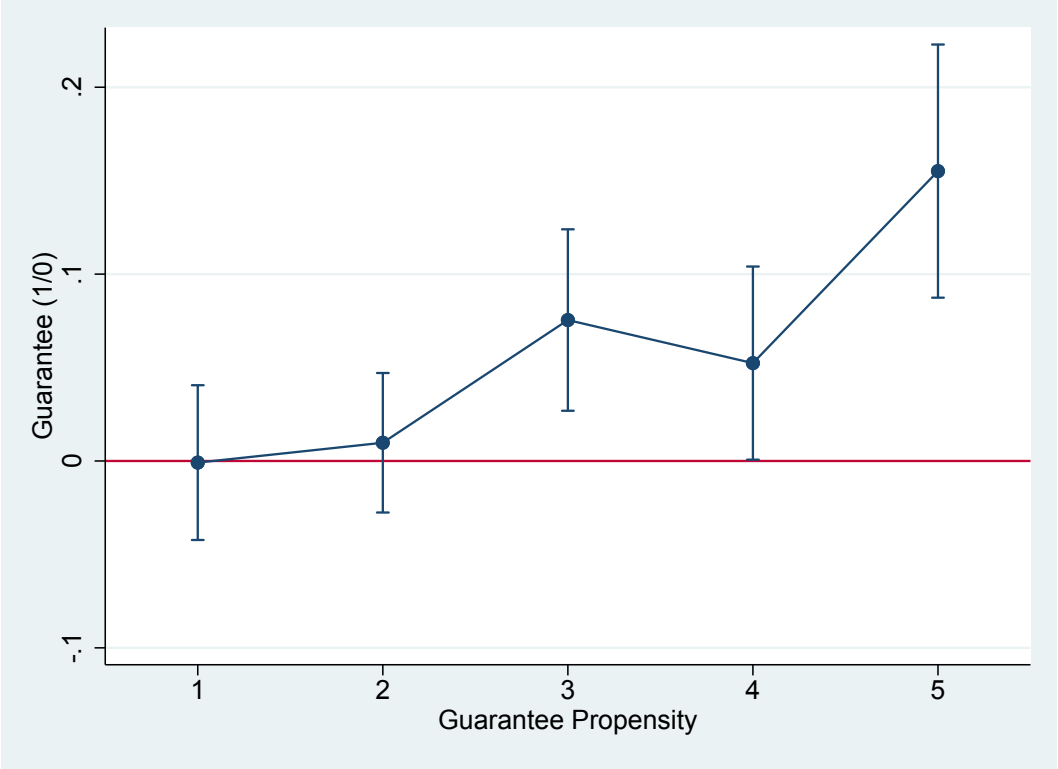
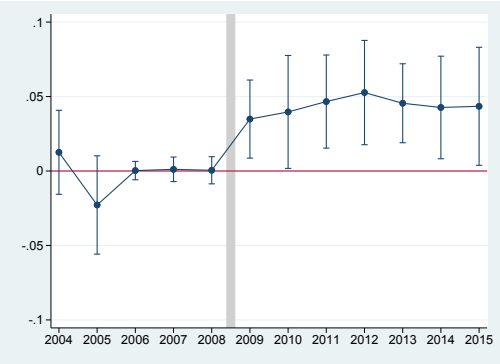


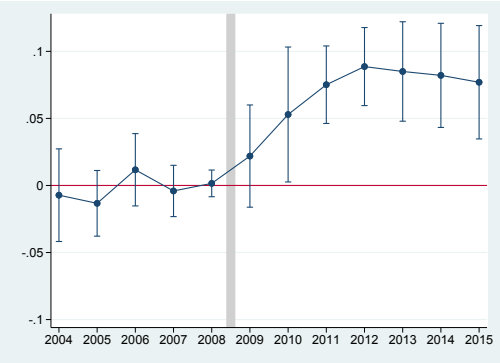

**Figure 4**
Regional treatment intensity, firm-level take-up propensity, and actual take-up

This figure plots regression coefficients and 95% confidence intervals of regressing actual guarantee take-up, Guarantee (1/0), on the regional exposure to the 2009–2010 loan guarantee program, $\text{Guarantee}_{region,09-10}$, interacted with quintiles of firm-level predicted take-up propensity. We estimate take-up propensity using observable firm characteristics (logarithm of assets, ROA, logarithm of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects) measured in 2008.

(A) All earnings




(B) Earnings at same firm




(C) Earnings at other firms

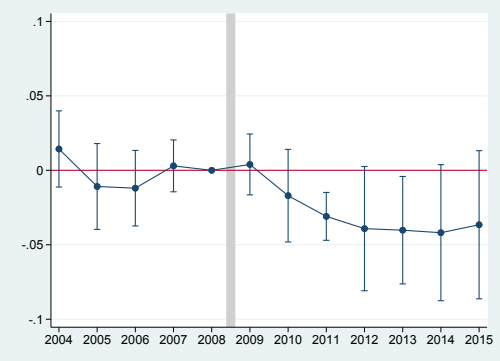



**Figure 5**
Dynamics: Effect on earnings

This figure plots regression coefficients and 95% confidence intervals from 12 regressions of earnings that a worker obtains in the year indicated on the $x$-axis, normalized by average annual earnings in 2006–2008, on our measure of regional exposure to the 2009–2010 loan guarantee program, $\text{Guarantee}_{region,09-10}$. All regressions include department-pair fixed effects, the distance from the regional border, changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), as well as firm- and worker-level controls measured in 2008.

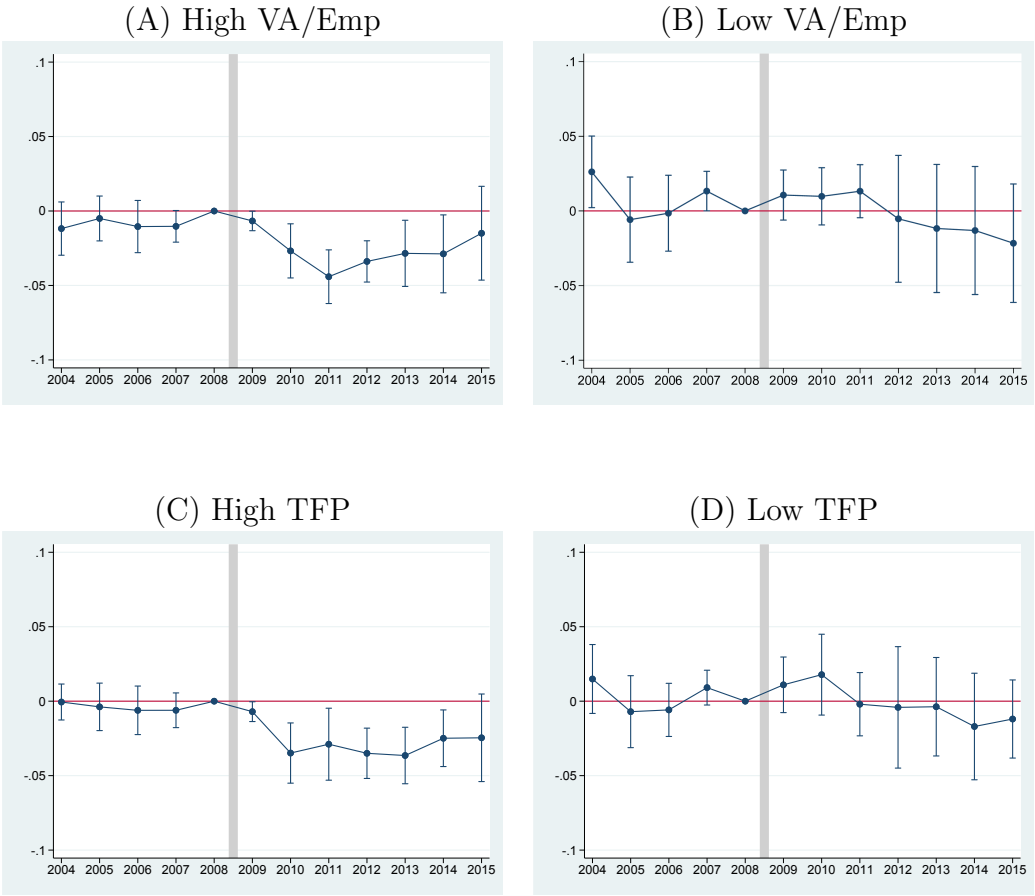



**Figure 6**
Effect on earnings at other firms: Split by productivity measures

This figure plots regression coefficients and 95% confidence intervals from 12 regressions of earnings that a worker obtains in the year indicated on the $x$-axis, normalized by average annual earnings in 2006–2008, on our measure of regional exposure to the 2009–2010 loan guarantee program, $\text{Guarantee}_{region,09-10}$. All regressions include department-pair fixed effects, the distance from the regional border, changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), as well as firm- and worker-level controls measured in 2008.

# Table 1
Summary statistics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Obs. | Mean | SD | p1 | p50 | p99 |
| *A. Loan guarantee exposure* | | | | | | |
| Raw guarantee$_{region,09-10}$ (over assets in %) | 21 | 0.290 | 0.156 | 0.105 | 0.256 | 0.769 |
| Guarantee$_{region,09-10}$ | 21 | 0.040 | 0.185 | -0.140 | -0.018 | 0.726 |
| Guarantee$_{firm,09-10}$ (over assets in %) | 31,949 | 0.307 | 1.666 | 0.000 | 0.000 | 11.876 |
| Guarantee (1/0) | 31,949 | 0.040 | 0.196 | 0.000 | 0.000 | 1.000 |
| Default amount$_{firm}$ (over assets in %) | 31,949 | 0.030 | 0.381 | 0.000 | 0.000 | 0.000 |
| Default on guaranteed loan (1/0) | 31,949 | 0.010 | 0.098 | 0.000 | 0.000 | 0.000 |
| *B. Main outcome variables, 2009–2015* | | | | | | |
| Years employed$_{2009,2015}$ | 38,568 | 6.512 | 1.295 | 1.000 | 7.000 | 7.000 |
| Earnings$_{2009,2015}$ | 38,568 | 6.498 | 2.167 | 0.140 | 7.085 | 11.022 |
| Separation$_{2009,2015}$ (1/0) | 38,568 | 0.488 | 0.500 | 0.000 | 0.000 | 1.000 |
| Unemployment benefits$_{2009,2015}$ | 38,568 | 0.218 | 0.478 | 0.000 | 0.000 | 2.154 |
| *C. Worker characteristics in 2008* | | | | | | |
| Earnings | 38,568 | 23,836 | 13,435 | 12,112 | 20,755 | 74,275 |
| Hours | 38,568 | 1,872.131 | 215.916 | 1,152.000 | 1844.000 | 2,479.000 |
| Age | 38,568 | 38.320 | 7.762 | 24.000 | 39.000 | 51.000 |
| Male | 38,568 | 0.740 | 0.439 | 0.000 | 1.000 | 1.000 |
| *D. Firm characteristics in 2008 and outcomes* | | | | | | |
| $\Delta_{08-09}\frac{Bankdebt}{Debt}$ | 23,238 | -0.043 | 0.258 | -0.957 | 0.000 | 0.827 |
| $\frac{Bankdebt}{Debt}_{08}$ | 25,487 | 0.652 | 0.373 | 0.000 | 0.810 | 1.000 |
| $\Delta_{08-10}$Interest rate | 24,176 | -0.013 | 0.048 | -0.209 | -0.005 | 0.147 |
| Interest rate$_{08}$ | 26,579 | 0.065 | 0.064 | 0.000 | 0.048 | 0.334 |
| Nb employees | 31,949 | 19.948 | 27.932 | 0.000 | 10.750 | 155.250 |
| Assets (€'000$s$) | 31949 | 3037 | 75255 | 48 | 754 | 26,908 |
| ROA | 31,949 | 0.104 | 0.187 | -0.619 | 0.101 | 0.703 |
| Firm age | 31,949 | 17.987 | 12.888 | 1.000 | 16.000 | 54.000 |
| Dividend/Sales | 31,949 | 0.016 | 0.037 | 0.000 | 0.000 | 0.218 |
| PPE/Assets | 31,949 | 0.453 | 0.331 | 0.000 | 0.376 | 1.000 |
| Debt/Assets | 31,949 | 0.150 | 0.193 | 0.000 | 0.070 | 0.856 |
| Credit risk | 31,949 | 5.977 | 2.953 | 1.000 | 6.000 | 10.000 |
| VA/Emp (€'000$s$) | 31949 | 52.1 | 37.8 | 3.6 | 44.1 | 293.5 |
| TFP | 31949 | 2.055 | 0.997 | 0.222 | 1.843 | 6.866 |

This table presents summary statistics at the regional and firm levels (panel A), worker level (panels B and C), and firm level (panel D). The sample includes 1/12th of employees who were working in SMEs located within a 10-mile distance to a regional border in 2008.

**Table 2**
First stage: Firm-level exposure to the loan guarantee program

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A. Credit* | $\text{Guarantee}_{firm,09-10}$ | Guarantee (1/0) | $\Delta_{08-09}\frac{Bankdebt}{Debt}$ | $\Delta_{08-10}$Interest |
| $\text{Guarantee}_{region,09-10}$ | 0.501*** | 0.051*** | 0.043*** | -0.005* |
| | (0.089) | (0.010) | (0.013) | (0.002) |
| Distance to border | 0.004 | 0.001 | 0.000 | 0.000 |
| | (0.003) | (0.000) | (0.000) | (0.000) |
| Department-pair FE | Y | Y | Y | Y |
| Regional controls | Y | Y | Y | Y |
| Firm-level controls | Y | Y | Y | Y |
| $F$-statistic | 18.222 | 14.267 | - | - |
| Observations | 31,949 | 31,949 | 23,238 | 24,176 |
| $R^2$ | .009 | .009 | .060 | .090 |
| *B. Labor & productivity* | $\Delta_{08-10}$Emp | $\Delta_{08-10}$VA/Emp | $\Delta_{08-15}$Emp | $\Delta_{08-15}$TFP |
| $\text{Guarantee}_{region,09-10}$ | 0.214*** | -4.334* | 0.127 | -0.035 |
| | (0.071) | (2.372) | (0.117) | (0.105) |
| Department-pair FE | Y | Y | Y | Y |
| Regional controls | Y | Y | Y | Y |
| Firm-level controls | Y | Y | Y | Y |
| Observations | 31,949 | 31,949 | 31,949 | 31,949 |
| $R^2$ | .105 | .080 | .110 | .079 |

This table reports results at the firm level. Panel A presents the first-stage OLS regressions along with effects on credit outcomes. The dependent variable is the amount of guaranteed loans the firm received due to the 2009–2010 recovery plan scaled by 2008 firm assets in column 1, a dummy variable equal to one if the firm received any loan guarantee from the recovery plan in 2009–2010 in column 2, the change in bank debt/debt from 2008 to 2009 in column 3, and the change in interest rate expenses/debt from 2008 to 2009/2010 in column 4. Panel B presents effects on firm-level employment and productivity. The dependent variable is the logarithm of the change in employment from 2008 to 2010 in column 1, the change in labor productivity from 2008 to 2010 in column 2, the logarithm of the change in employment from 2008 to 2015 in column 3, and the change in total factor productivity (TFP) from 2008 to 2015 in column 4. Regressions in panel B are weighted by 2008 employment. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), and firm-level controls including the logarithm of assets, ROA, the logarithm of firm age, dividend/sales, PPE/assets, debt/assets, credit risk, and two-digit industry fixed effects. Firm controls are measured in 2008. Standard errors clustered by region are reported in parentheses. *$p$ <.1; **$p$ <.05; ***$p$ <.01.

**Table 3**
Worker-level employment effects

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A. Baseline* | Years employed $_{09,15}$ | | Earnings $_{09,15}$ | |
| Guarantee$_{region,09-10}$ | 0.246*** | 0.240*** | 0.333*** | 0.298*** |
| | (0.049) | (0.051) | (0.061) | (0.066) |
| Observations | 38,568 | 38,568 | 38,568 | 38,568 |
| $R^2$ | .031 | .039 | .053 | .064 |
| *B. 2SLS* | Years employed $_{09,15}$ | | Earnings $_{09,15}$ | |
| $\widehat{\text{Guarantee}}_{firm,09-10}$ | 0.433*** | 0.420*** | 0.586*** | 0.522*** |
| | (0.142) | (0.141) | (0.172) | (0.177) |
| Observations | 38,568 | 38,568 | 38,568 | 38,568 |
| *C. Raw treatment* | Years employed $_{09,15}$ | | Earnings $_{09,15}$ | |
| Raw guarantee$_{region,09-10}$ | 0.267*** | 0.258*** | 0.367*** | 0.328*** |
| | (0.062) | (0.063) | (0.076) | (0.082) |
| Observations | 38,568 | 38,568 | 38,568 | 38,568 |
| $R^2$ | .030 | .038 | .051 | .062 |
| Department-pair FE | Y | Y | Y | Y |
| Regional controls | Y | Y | Y | Y |
| Firm-level controls | Y | Y | Y | Y |
| Worker-level controls | | Y | | Y |

This table reports regression results of the effect of loan guarantees on worker-level outcomes. Panel A presents the baseline reduced-form results with the regional intensity of the recovery plan, Guarantee$_{region,09-10}$ as main explanatory variable. Panel B presents the corresponding 2SLS estimates, and panel C presents reduced-form results using the raw treatment variable, Raw guarantee$_{region,09-10}$, defined as the average regional ratio of loans guaranteed under the recovery plan in 2009–2010 scaled by assets in 2008, computed across SMEs outside the border area. Earnings are the sum of earnings 2009–2015 scaled by average annual earnings 2006–2008. All regressions include department pair fixed effects, distance to the border, as well as changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending) and firm-level controls (logarithm of assets, ROA, logarithm of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects). Worker-level controls added in columns 2 and 4 include worker age, gender, and occupation fixed effects. Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. *$p$ <.1; **$p$ <.05; ***$p$ <.01.

**Table 4**
Adjustment margins and worker reallocation

| | (1) | (2) | (3) |
|---|---|---|---|
| (N=38,568) | All firms | Initial firm | Other firm |
| Years employed | 0.240*** | 0.503*** | -0.264** |
| | (0.051) | (0.084) | (0.100) |
| Cumulative earnings | 0.298*** | 0.523*** | -0.225** |
| | (0.066) | (0.099) | (0.101) |

This table reports reduced-form OLS regression results of the effect of loan guarantees on employment and earnings at the initial firm and at other firms. Column 1 shows the effect across all firms. Column 2 measures employment and earnings at the initial firm (in 2008). Column 3 measures employment and earnings at other firms. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), firm (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects), and worker-level controls (worker age, gender, and occupation fixed effects). Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

**Table 5**
Employment effects and local labor market conditions

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Local unemployment rate: | High | | Low | | High-Low | |
| | All firms | Initial firm | All firms | Initial firm | All firms | Initial firm |
| Years employed | 0.385*** | 0.386** | 0.105 | 0.495*** | 0.280*** | -0.109 |
| | (0.037) | (0.160) | (0.074) | (0.146) | (0.077) | (0.221) |
| Cumulative earnings | 0.529*** | 0.504*** | 0.108 | 0.472** | 0.421*** | 0.031 |
| | (0.082) | (0.169) | (0.101) | (0.168) | (0.113) | (0.233) |

This table reports the effect of loan guarantees on worker employment and earnings at all firms and at the initial firm separately for municipalities with unemployment rates above and below 10%. Columns 1 and 3 show the effect across all firms. Columns 2 and 4 measure employment and earnings at the initial firm (in 2008). Columns 5 and 6 show the difference between high and low unemployment areas for all firms and at the initial firm, respectively. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), firm (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects) and worker-level controls (worker age, gender, and occupation fixed effects). Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

**Table 6**
Heterogenous effects: Labor hoarding

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *A. Firm level* | Earnings | | Hiring difficulty | | Cognitive skill | |
| | High | Low | High | Low | High | Low |
| $\Delta_{08-10}$Emp | 0.351** | 0.155 | 0.412*** | -0.045 | 0.517*** | -0.115 |
| | (0.130) | (0.100) | (0.091) | (0.098) | (0.125) | (0.070) |
| *B. Worker level* | Earnings | | Hiring difficulty | | Cognitive skill | |
| | High | Low | High | Low | High | Low |
| Years employed at initial firm | 0.754*** | 0.344** | 0.631*** | 0.433*** | 0.690*** | 0.402*** |
| | (0.086) | (0.137) | (0.115) | (0.139) | (0.119) | (0.122) |
| Cumulative earnings at initial firm | 0.922*** | 0.275* | 0.673*** | 0.481*** | 0.740*** | 0.423*** |
| | (0.130) | (0.148) | (0.156) | (0.156) | (0.132) | (0.130) |

This table reports the effect of loan guarantees on firm and worker-level employment outcomes, splitting the sample by proxies for worker skill. Columns 1 and 2 split the sample by worker earnings capacity, columns 3 and 4 by hiring difficulty, and columns 5 and 6 by cognitive-analytical task content. For panel A, we first classify workers in the highest tertile of the distribution as high skill for each skill measure, utilizing the universe of all workers in 2008 from DADS Postes. Based on this classification, we compute the firm-level fraction of high-skilled workers for each measure. We then split the sample of firms at the median of the fraction of high-skilled workers. In panel B, we split the worker sample at the median of the respective skill measure. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), and firm (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects). We add worker-level controls (worker age, gender, and occupation fixed effects) in panel B. Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. *$p$ <.1; **$p$ <.05; ***$p$ <.01.

**Table 7**

Dampened worker reallocation to more productive firms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| (N=38,568) | Other firms | Other firms VA/Emp | | Other firms TFP | | Other firms ROA | | Other firms Sales growth | |
| | | Higher | Lower | Higher | Lower | Higher | Lower | Higher | Lower |
| Years employed | -0.264** | -0.255*** | -0.009 | -0.262*** | -0.001 | -0.268*** | 0.004 | -0.275*** | 0.011 |
| | (0.100) | (0.065) | (0.103) | (0.062) | (0.085) | (0.080) | (0.043) | (0.067) | (0.106) |
| Cumulative earnings | -0.225** | -0.214*** | -0.011 | -0.213*** | -0.012 | -0.218*** | -0.007 | -0.237*** | 0.012 |
| | (0.101) | (0.059) | (0.098) | (0.050) | (0.092) | (0.055) | (0.058) | (0.050) | (0.118) |

This table reports reduced-form OLS regression results of the effect of loan guarantees on worker employment and earnings at firms other than their initial employer. Column 1 measures employment and earnings at other firms. Columns 2, 4, 6, and 8 measure employment and earnings at other firms with higher labor productivity (value-added/employment), total factor productivity (TFP), return-on-assets (ROA), and sales growth compared to the initial firm. Columns 3, 5, 7, and 9 measure employment and earnings at other firms with lower labor productivity, TFP, ROA, and sales growth compared to the initial firm. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), firm (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects) and worker-level controls (worker age, gender, and occupation fixed effects). Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

**Table 8**
Heterogeneity in reallocation dampening by worker characteristics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A. Earnings capacity* | High | | Low | |
| (N=38,568) | Other firm VA/Emp | | Other firm VA/Emp | |
| | Higher | Lower | Higher | Lower |
| Years employed | -0.434*** | -0.037 | -0.116 | -0.022 |
| | (0.096) | (0.102) | (0.096) | (0.126) |
| Cumulative earnings | -0.293*** | -0.079 | -0.144 | -0.007 |
| | (0.082) | (0.107) | (0.091) | (0.117) |
| *B. Hiring difficulty* | High | | Low | |
| (N=38,568) | Other firm VA/Emp | | Other firm VA/Emp | |
| | Higher | Lower | Higher | Lower |
| Years employed | -0.523*** | 0.146 | -0.001 | -0.173 |
| | (0.151) | (0.141) | (0.134) | (0.119) |
| Cumulative earnings | -0.416*** | 0.092 | -0.026 | -0.131 |
| | (0.142) | (0.148) | (0.135) | (0.108) |
| *C. Cognitive-analytical task content* | High | | Low | |
| (N=38,568) | other firm VA/Emp | | Other firm VA/Emp | |
| | Higher | Lower | Higher | Lower |
| Years employed | -0.387*** | -0.070 | -0.137* | -0.022 |
| | (0.089) | (0.096) | (0.077) | (0.132) |
| Cumulative earnings | -0.297*** | -0.044 | -0.133** | -0.055 |
| | (0.079) | (0.082) | (0.056) | (0.132) |

This table reports the effect of loan guarantees on employment and earnings at other firms for subgroups of workers. Columns 1 and 3 show the effect across firms with higher labor productivity (value-added/employment) compared to the initial firm. Columns 2 and 4 show the effect across firms with lower labor productivity compared to the initial firm. Panel A splits the sample based on workers' earnings (within their age cohort) in 2008. Panel B splits the sample based on firms' reported difficulty to hire workers in a given occupation and department. Panel C splits the sample based on the nonroutine, cognitive-analytical task content of a workers' occupation in 2008. High (low) is a dummy variable equal to one if the respective variable is above (below) the sample median. The main explanatory variable is the regional intensity of the recovery plan, $\text{Guarantee}_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), firm (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects) and worker-level controls (worker age, gender, and occupation fixed effects). Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

**Table 9**
Placebo test using firms with low take-up propensity

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *A. Baseline* | | | | | | |
| Take-up propensity | high | low | high | low | high | low |
| | Guarantee$_{firm,09-10}$ | | Years employed $_{09,15}$ | | Earnings $_{09,15}$ | |
| Guarantee$_{region,09-10}$ | 0.901*** | 0.054 | 0.299*** | 0.112 | 0.511*** | -0.074 |
| | (0.205) | (0.055) | (0.061) | (0.067) | (0.080) | (0.111) |
| Department-pair FE | Y | Y | Y | Y | Y | Y |
| Regional controls | Y | Y | Y | Y | Y | Y |
| Firm-level controls | Y | Y | Y | Y | Y | Y |
| Worker-level controls | | | Y | Y | Y | Y |
| Observations | 23,137 | 15,421 | 23,137 | 15,421 | 23,137 | 15,421 |
| $R^2$ | .067 | .022 | .044 | .050 | .080 | .064 |
| *B. Reallocation* | | | | | | |
| Take-up propensity | | | High | Low | High | Low |
| | | | Years employed $_{09,15}$ | | Earnings $_{09,15}$ | |
| | | | Other firm Higher VA/Emp | | Other firm Higher VA/Emp | |
| Guarantee$_{region,09-10}$ | | | -0.534*** | 0.115 | -0.429*** | 0.047 |
| | | | (0.108) | (0.148) | (0.089) | (0.134) |
| Department-pair FE | | | Y | Y | Y | Y |
| Regional controls | | | Y | Y | Y | Y |
| Firm-level controls | | | Y | Y | Y | Y |
| Worker-level controls | | | Y | Y | Y | Y |
| Observations | | | 23,137 | 15,421 | 23,137 | 15,421 |
| $R^2$ | | | .097 | .053 | .084 | .051 |

This table reports the effect of loan guarantees on worker employment and earnings separately for firms with high and low guarantee take-up propensity. We first estimate take-up propensity using observable firm characteristics (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects) measured in 2008. Columns 1, 3, and 5 show the effect for firms with high take-up propensity, while columns 2, 4, and 6 show the effect for firms with low take-up propensity. Panel A shows the baseline results (first stage and worker outcomes), while panel B shows the dampening of worker reallocation to more productive firms. The main explanatory variable is the regional intensity of the recovery plan, Guarantee$_{region,09-10}$, estimated across SMEs outside the border area. All regressions include department pair fixed effects, distance to the border, and changes in regional controls from 2008 to 2010 (public spending, local taxes, public equipment expenditures, public debt, state contribution, value-added of non-SMEs, and regional bank lending), and firm controls (log of assets, ROA, log of firm age, dividend/sales, PPE/assets, debt/assets, credit risk and two-digit industry fixed effects). Columns 3 to 6 add worker-level controls (worker age, gender, and occupation fixed effects). Firm- and worker-level controls are measured in 2008. Standard errors clustered by region are reported in parentheses. *$p$ <.1; **$p$ <.05; ***$p$ <.01.