NBER WORKING PAPER SERIES

AFTER THE STORM:
HOW EMERGENCY LIQUIDITY HELPS SMALL BUSINESSES FOLLOWING NATURAL DISASTERS

Benjamin L. Collier
Sabrina T. Howell
Lea Rendell

Working Paper 32326
http://www.nber.org/papers/w32326

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2024

We are grateful to those who made this research possible, including Shawn Klimek and others at the U.S. Census Bureau, and Anna Calcagno and others at the SBA Office of Program Performance, Analysis, & Evaluation (OPPAE) and Office of Disaster Recovery & Resilience (ODRR). We thank the staff at the SBA for their assistance in understanding the setting. We also thank Bronson Argyle, Alexander Borisov, Tetiana Davydiuk, William Kerr, Raymond Kluender, Christopher Palmer, Benjamin Roth, Ali Sanati, and Mohammad Soltani-Nejad for assistance and comments. Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau or the Small Business Administration (SBA). The Census Bureau has ensured appropriate access and use of confidential data and has reviewed these results for disclosure avoidance protection (Project 7513031: CBDRB-FY23-CED006-0008, CBDRB-FY24-CED006-0008). Any remaining errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

After the Storm: How Emergency Liquidity Helps Small Businesses Following Natural Disasters
Benjamin L. Collier, Sabrina T. Howell, and Lea Rendell
NBER Working Paper No. 32326
April 2024
JEL No. G21,G32,H81,Q54,R33

ABSTRACT

Does emergency credit prevent long-term financial distress? We study the causal effects of government-provided recovery loans to small businesses following natural disasters. The rapid financial injection might enable viable firms to survive and grow or might hobble precarious firms with more risk and interest obligations. We show that the loans reduce exit and bankruptcy, increase employment and revenue, unlock private credit, and reduce delinquency. These effects, especially the crowding-in of private credit, appear to reflect resolving uncertainty about repair. We do not find capital reallocation away from neighboring firms and see some evidence of positive spillovers on local entry.

Benjamin L. Collier
Department of Risk, Insurance,
and Healthcare Management
Fox School of Business
1801 Liacouras Walk
Philadelphia, PA 19122
collier@temple.edu

Sabrina T. Howell
NYU Stern School of Business
KMC 9-93
44 West 4th Street
New York, NY 10012
and NBER
showell@stern.nyu.edu

Lea Rendell
University of Maryland, College Park
lrendell@umd.edu

1 Introduction

When a crisis such as a bank failure, pandemic, or hurricane occurs, firms unexpectedly need liquidity. Governments often wish to step in to aid recovery. One policy tool is subsidized credit, which has the advantage of being potentially much less costly to the taxpayer than grants to firms or other direct stimulus. Despite this benefit, recovery loans are criticized from across the political spectrum. Some argue that markets can adequately manage lending to businesses: government loans will only crowd out private activity at the taxpayer's expense or go to unviable firms that would fail regardless.[1] Others view loans as inappropriate because they add too much risk to already distressed businesses, who may be unable to service the new debt. Instead, this perspective favors grants. It won the day during the U.S. Paycheck Protection Program, where small business loans to preserve jobs during the COVID-19 pandemic were converted to grants.[2]

In this paper, we shed light on this debate by studying firms that apply for a government-provided recovery loan following a natural disaster. Natural disasters represent an increasingly widespread and costly risk. Since the early 1980s, the number of natural disasters with more than $1 billion in inflation-adjusted damages increased from two to three per year to more than 25 today (NOAA, 2023). These events create cash-flow crunches for firms by disrupting revenues or damaging productive assets (Simon and McWhirter, 2017; Ivanova, 2023; Crowley, 2023). We assess the causal effect of receiving a disaster loan on applicant firms and on their neighbors.

We study the Small Business Administration's (SBA) disaster recovery loan program, which has disbursed $60 billion in loans from its inception in 1953 to 2019.[3] These loans are primarily intended to repair damaged property, and they are underwritten, originated, and serviced by the SBA. They are the only form of U.S. federal disaster assistance provided directly to firms (besides farms). The loans have relatively low interest rates (median 4%), long maturities (median 17 years), and are usually collateralized with real estate. Our data include comprehensive information on applications, approval processes, and loans from 2005 to 2017. During the period of study, 167,000 unique firms applied for a disaster loan, and ultimately, the SBA lent to 54,500 businesses. With data from the U.S. Census Bureau and business credit reports from Experian, we observe both real and financial outcomes, which is rare in analyses of private firms.

To identify a causal effect, we exploit discontinuities in the loan approval process, where the likelihood of approval jumps discretely in the business owner's FICO score. The program has experimented with vari-

[1] For example, see Muhlhausen (2013). Also, a Heritage Foundation proposal to eliminate the U.S. federal disaster loan program argues that "Taxpayers shouldn't be on the hook for insuring business against disaster" (Heritage Foundation, 2023).

[2] From the left, the Center for American Progress argues that faced with a liquidity crunch, small businesses "need cash grants immediately in order to remain in business and retain their employees" (Thornton, 2020).

[3] Outside the U.S., subsidized disaster recovery loans for businesses are also used by governments in Australia, Canada, and Japan, among other countries (NSW, 2022; CMHC, 2022; Public Safety Canada, 2022; Japanese Finance Corporation, 2022). Countries that began or expanded business recovery lending during the COVID-19 pandemic include Germany, Italy, the Netherlands, the U.K., and the U.S. (Kfw, 2020; MEF, 2020; Government of the Netherlands, 2020; gov.uk, 2020; SBA, 2022).

ous credit score thresholds such that the thresholds span a wide range of FICO scores, time, and geography, leading to estimates that are relevant for a much broader group of businesses than a single discontinuity would allow. We use the credit score thresholds as an instrument for loan approval in a difference-in-differences design, following firms from five years before to seven years after the disaster.

Do recovery loans persistently reduce distress? We estimate that disaster recovery loans reduce the likelihood of firm exit by 13 percentage points (pp). Mean exit for declined firms during the post-disaster period is 21%, so the 13 pp treatment effect represents a large reduction in firm closures. Bankruptcy is a form of exit that imposes large social costs (Bernstein et al., 2019; Dou et al., 2021; Antill, 2022). Recovery loans reduce the chance of bankruptcy by 3.8 pp, an 86% reduction relative to the post-disaster bankruptcy rate of declined firms. Examining the combination of exits and bankruptcies, we find that among employer firms, the results are strongest for bankruptcy-driven exits. The loans also reduce the share of firm debt that is delinquent and the duration of delinquent debt, which can be precursors to bankruptcy.

In event study models, we show that the effects on these distress outcomes appear within a year of the disaster and persist over time. This persistence—even after borrowers have been servicing their disaster recovery loans for several years—implies that the effects do not represent temporary delays in exit for ultimately unviable firms. Instead, recovery loans seem to set firms on a different path, permanently reducing the likelihood of distress and exit from the disaster.

Do SBA recovery loans crowd-out private lending? Surprisingly, SBA recovery loans have the opposite effect. They increase non-SBA private debt balances by $18,000 on average, which is 200% of the mean. Therefore, disaster loans *crowd-in* private credit. The effect begins during the year of the disaster and persists for at least seven years. Recovery loans also increase the firm's total number of contracts, including loans, leases, utilities and telecom contracts. Thus, recovery loans not only appear to help firms survive but also foster supply relationships and create new investment opportunities for private lenders.

Do recovery loans increase firms' employment and revenues? Employment and revenue are important outcomes because they reflect the health of the firm and have knock-on effects for local economic recovery. We show that disaster loans have unambiguously positive effects on firm employment. In the full sample, they increase employment by 18%, which represents about half of a worker. For employer firms, the effect is larger at 45%, representing about five workers. The loans also reduce the chance that a firm transitions from being an employer to a non-employer firm. Regarding revenue, we find a significant increase only among employer firms, where the effect is around 100% of the mean.

In sum, recovery loans have strong positive effects on real and financial outcomes. For the purposes of this paper, we assume that governments seek to facilitate local rebuilding, and we find that recovery loans contribute to that goal. As disaster risks increase, relocating will be optimal in some locations (Hino et al., 2017; Deryugina et al., 2018).

Our findings are surprising and illustrate a sizable market friction. It is *not* surprising that a firm who applies for a loan benefits from receiving it, perhaps especially when the loan is subsidized. Instead, the magnitude of the effects are noteworthy because they illustrate the dire consequences, in terms of exit and bankruptcy, for control firms who do not receive this emergency liquidity. Moreover, the large crowd-in effects are difficult to explain by the subsidy alone, which is around $2,700 for the median firm. Rather than funding new projects, recovery loans repair property damages; treated firms have similar productive assets as before the disaster but more leverage.

What market frictions do recovery loans address? We consider two potential barriers to financing emergency credit demand. One is pre-existing financial constraints. There is a large body of evidence showing that financial constraints are more severe among firms that are relatively younger, have lower credit scores, lack existing relationships with lenders, operate in areas with fewer banks, or are located in Black communities (e.g., Berger and Udell, 1995; Bhutta and Hizmo, 2021; Howell, 2017; Fairlie et al., 2022). Perhaps surprisingly, we do not find significantly stronger effects on exit or private debt among any of these groups, suggesting that this channel is not the main driver of the large average effects.

The second potential friction is private lenders' uncertainty regarding repairs.[4] One reason for this channel rather than the subsidy embedded in the loan stems from the fact that subsequent private loans are junior to the government loan. In other words, the government must be repaid before the private lenders. We document increased private lending following the SBA loan to treated firms relative to control firms. Thus, these lenders view a junior loan to a treated firm as less risky than a potential senior loan to a control firm. This precludes a channel in which better capitalization alone explains the effect on subsequent private debt.

Uncertainty would arise if lenders lack the expertise to underwrite repairs following a natural disaster. Amid local economic disruption that could have indirect negative effects, they must gauge whether the firm can absorb the costs of repairs. A growing literature documents that uncertainty stifles investment (e.g., Baker et al., 2024; Campello et al., 2024). Consistent with this channel, we find larger effects of the loans among more capital-intensive firms, which we expect to experience greater frictions related to repair costs because they rely on equipment that could be damaged in a disaster. Also, the timing of the increase in private debt, which increases around six months after disaster loan approval, roughly corresponds to the repair period. If the crowding-in reflected only the certification signal of loan approval, we would expect an immediate result, while if it reflected an investment and growth channel, we would expect it to take more than a few months. Overall, it appears the SBA adds value in our setting at least in part by resolving

[4] Note that in our causal design, recovery loans are effectively randomly assigned around a credit score threshold, making approved and denied firms otherwise identical. The program is not leveraging private information regarding repair viability in this design; instead, it is (arbitrarily) funding some firms and not others near a threshold.

uncertainty about repair costs, facilitating private investment in the post-disaster period.

The loans could have positive or negative local spillovers. On the one hand, negative spillovers could occur if lenders ration and reallocate capital away from neighboring businesses who did not apply for a recovery loan. On the other hand, the loans could benefit nearby businesses, for example through more foot traffic or better neighborhood amenities. We find marginally significant effects. Recovery loans appear to create both a positive spillover in increasing firm entry in the census tract and a negative spillover in reducing revenues for existing neighbors. We do not find significant effects on other outcomes among nearby firms. For example, the crowding-in of private loans does not seem to reflect a reallocation of private lending away from neighbors. Relatedly, Cortés and Strahan (2017) show that financially integrated banks shift mortgage lending to areas with recent natural disasters, lending less to other areas.

Finally, in a back-of-the-envelope calculation, we assess the costs to the government of providing recovery loans and compare them to our estimated effects. The program enjoys the government's low cost of funds and benefits from loan repayment, which does not occur in grant programs. Combining administrative costs, all-in subsidy costs, and dead-weight losses from taxation, we estimate that the program costs 41 cents per dollar loaned. This implies that it costs about $53,000 to create one job in our overall sample. This cost per job is much lower than existing estimates from grant programs, even though they do not include administrative or taxation costs (CEA, 2011; Autor et al., 2022). Similarly, it takes about $87,000 in government costs to generate $1 million in new annual revenues, based on our estimated effect among the employer firms.

This paper contributes to three strands of literature. The first addresses consequences of credit frictions, considering both the supply side, especially the role of relationships when it comes to small business lending (Petersen and Rajan, 1994; Rice and Strahan, 2010; Berger and Black, 2011; Chodorow-Reich, 2014; Midrigan and Xu, 2014; Gilje et al., 2016; Rampini, 2019; Darmouni, 2020; Howell and Brown, 2023) as well as variation in financial constraints among firms on the demand side (Kaplan and Zingales, 1997; Rauh, 2006; Campello et al., 2010; Rampini and Viswanathan, 2010; Hoberg and Maksimovic, 2015). A subset of this work focuses on how financing frictions affect entrepreneurship and growth (Black and Strahan, 2002; Nanda and Rhodes-Kropf, 2016; Schmalz et al., 2017; Howell, 2020; Howell and Brown, 2023). This paper contributes by evaluating the provision of subsidized credit after an unexpected negative shock. While disasters are exogenously timed and observable, the large average increases in exit and delinquency that we see among rejected firms suggests important frictions leading otherwise viable businesses to fail.

Second, we add to work on government lending and loan guarantee programs (e.g., Lelarge et al., 2010; Banerjee and Duflo, 2014; Brown and Earle, 2017; Mullins and Toro, 2017; Barrot et al., 2019; Bachas et al., 2021). Small business disaster loans have received little attention, with analyses to our knowledge restricted to survey or county-level data (e.g., Josephson and Marshall, 2016; Davlasheridze and Geylani, 2017; Collier et al., 2024). Several studies examine the impacts of forgivable SBA loans and grants during the COVID-19 pandemic (Humphries et al., 2020; Balyuk et al., 2020; Howell et al., 2024; Granja et al., 2022). Evaluating government-provided credit is challenging because many programs use private intermediaries who may channel credit endogenously to more creditworthy firms, and because data is often only available for approved applicants, making a counterfactual difficult to estimate. By studying a direct lending program and exploiting quasi-experimental variation in approval, we overcome these empirical challenges to identify causal effects of government lending.

Last, a growing literature studies implications of the weather and climate risks for economic activity; examples include Giroud et al. (2012), Weitzman (2014), Baez et al. (2017), Baldauf et al. (2020), Alok et al. (2020), Brown et al. (2021), Giglio et al. (2021), and Deryugina and Marx (2021). One strand of the literature studies government programs and the incentives that they create, especially the National Flood Insurance Program (e.g., Kousky and Michel-Kerjan, 2017; Collier and Ragin, 2020; Mulder, 2021; Sastry, 2022). There is work on disaster recovery lending, showing that underwriting standards may limit access to recovery loans for credit constrained households and that applicants are sensitive to loan terms such as interest rates and collateral requirements (Begley et al., 2020; Collier et al., 2021; Billings et al., 2022; Pan et al., 2023; Collier and Ellis, 2024). More broadly, researchers have studied how disasters affect businesses and the local economy (Leiter et al., 2009; Hornbeck, 2012; Hornbeck and Naidu, 2014; Miao and Popp, 2014; Basker and Miranda, 2018; Gallagher et al., 2023). This paper is the first causal assessment of how disaster loans affect businesses and their neighbors. Our results point to an important role for government intervention in business credit markets.

2 Institutional Context

This section explains the SBA Disaster Loan Program and describes the application and underwriting process, which underlies our estimation design.

Background on SBA Disaster Loans. Through its Office of Disaster Assistance (ODA), the SBA offers recovery loans following a declared disaster to businesses of all sizes. Between its inception in 1953 and 2019, the SBA provided approximately $60 billion in disaster recovery loans. (The program was greatly and temporarily expanded during the COVID-19 pandemic in 2020 to provide pandemic-related economic injury loans.) These recovery loans are the only form of U.S. federal disaster assistance provided directly to (non-farm) businesses. They are underwritten, originated, and serviced by the SBA directly, unlike other SBA programs that also target firms but do so via bank intermediaries. Firms can learn about the program through various channels, one of which is through the temporary Federal Emergency Management Agency

(FEMA) offices that are established in affected neighborhoods.

Our SBA disaster loan data include all business applicants from 2005 to 2017.[5] Table 1 reports aggregate statistics. During this time, 167,202 unique businesses applied and 54,532 borrowed from the program. Borrowing businesses collectively had 236,571 employees at the time of application and received $4.4 billion in loans.[6] The rate of approval conditional on applying is 44%.[7] Figure 1 shows the dispersion of applicants across U.S. counties in our data. The shading reflects the log number of applicants in each county. Disaster loan applicants come from throughout the U.S. with a higher concentration in hurricane-prone areas such as along the Gulf Coast. Hurricanes account for 73% of disasters (Table 1). The next largest category is storm or flood, at 14%. We have small numbers of applications following tornadoes, wildfires, oil spills, and droughts. About one-quarter of the applicant firms are incorporated, while almost 60% are sole proprietorships or self-employed. This is roughly representative of U.S. small businesses, where 18% are corporations and 78% have no employees (NSBA, 2016; SBA, 2014, 2020).

Loan Eligibility and Terms. The primary use of recovery loans is for businesses to repair or replace damaged property after a natural disaster. Damaged property must be owned by the business and can include buildings, leasehold improvements, inventories, supplies, machinery and equipment.[8] Most loans are secured with collateral, usually real estate. Collateral is required for all loans above a certain amount (e.g., $25,000 as of 2018), but the SBA will not decline a loan for lack of collateral. The SBA's claim on collateral is subordinate to any existing secured debt (e.g., a first mortgage). Secured debt taken after the SBA recovery loan is subordinate to that recovery loan.[9]

Summary statistics about applicants are in Table 2. The median applying firm is eight years old and has just one employee. The distribution is, as expected, skewed right for both variables, with the averages being 11.8 years and 4.7 employees. This compares to about two employees for the average small business

[5] These data are proprietary to the SBA and are non-public. They are employed here via special contracts between the researchers, the U.S. Census Bureau, and the SBA. Researchers may request these data through the Census Bureau's FSRDC external research program.

[6] Almost all (94%) applications are associated with a Presidential disaster declaration. Most loans (93%) are accompanied by an additional Economic Injury (EI) loan to offset income losses. A small share, 5.7% of applicants have EI loans only. As we find no heterogeneous effects depending on whether a loan has an EI component, we do not address it further. A small share of applicants (less than 2% in our data) experienced events that are not "natural" disasters (e.g., the Deepwater Horizon oil spill). We use the term "natural disasters" throughout to reflect that the program's loans primarily address hurricanes, floods, tornadoes, wildfires, etc.

[7] The rate of borrowing conditional on applying is 33%, because some approved firms never take up a loan. Survey research suggests that approved applicants do not to take the loan because they receive sufficient insurance claims payments, relied on the owner or the owner's friends and family for funds instead, or changed their mind about taking on more debt, among other reasons (Collier et al., 2024). Take-up decisions are orthogonal to our research design since firms' outside options should be smooth through the threshold we use for identification.

[8] These repairs can include improvements that reduce future disaster damages (e.g., repairs may require updating older structures to current building codes, SBA, 2018).

[9] The SBA allows for exceptions. For example, a business can refinance its first mortgage with the new mortgage maintaining its senior position.

in the U.S. overall (SBA, 2014, 2020). The loans have a median maturity of 16.8 years, with a maximum term of 30 years. The maturity is based on the applicant's ability to repay. Interest rates are fixed for the duration of the loan with an average (median) of 3.8% (4%) during our sample period. All firms receive the same interest rate for a given disaster.[10] During our period of study, recovery loans have a subsidy rate of 15%, meaning that the program receives 85 cents in discounted present value for every dollar loaned. This implies that the median firm, who borrows about $18,000, receives a subsidy of $2,700. We discuss the subsidy calculation further in Section 8.

Figure 2 compares disaster loan interest rates with the commercial prime rate over time. The interest rate is fixed at 4% for much of our sample period. Like disaster loans, commercial property loans are typically collateralized and can have long maturities. Disaster loan interest rates were below the prime rate from 2005 to 2009 and 2017 to 2018 and were above it by about 1 pp from 2009 to 2017. While some disaster loan applicants might qualify for commercial property loans, many may not, for example, because they do not own real estate, their real estate is already committed for other loans, or the owner's credit score is below prime. Overall, disaster recovery loan terms are similar to those of some secured credit products available in commercial credit markets, but are not risk-adjusted, as most borrowers have a 4% interest rate.

Loan Process and Underwriting. The SBA typically requires firms seeking disaster recovery loans to apply within 60 days of the disaster declaration, either online or at an SBA field office. The application permits the SBA to verify property damage through an onsite loss inspection of the business and, for both the business and the owners, to collect income information from the IRS and credit bureau reports. Similar to an insurance claims adjuster, SBA loss inspectors have expertise in estimating damages and repair costs. They provide reports to loan officers based on their onsite inspection. The average (median) amount lost by applicants is $156,442 ($46,772).[11]

While recovery loans are subsidized and the SBA does not seek to make a profit, the program tries to minimize taxpayer costs by screening on creditworthiness. It relies primarily on two types of information. The first is the owners' credit scores, specifically their FICO scores. The SBA evaluates whether the highest credit score among the owners exceeds a threshold credit score, which varies over time. As we explain below, we build our identification strategy on the basis of the discontinuities in approval probability created by these FICO thresholds. The second type of information is business cash-flows prior to the disaster, which

[10] There is a small fraction of loans (about 3%) that are at a higher "market rate" of about 8%, for businesses deemed to have access to credit elsewhere.

[11] SBA (2018) reports that "Loss Verifiers conducting verifications have specific responsibilities that include, but are not limited to: 1) Determining estimated cost of repair or replacement of real, personal, and business property; 2) Providing information gathered during the verification of damages to guide you [loan officers] in establishing eligibility within program guidelines; and 3) Estimating replacement and pre-disaster FMV [fair market value] of damaged property" (p. 78).

the loan officer uses to evaluate whether the business can service additional debt.[12]

After calculating the cost to repair damages, the loan officer subtracts any proceeds from insurance to repair these damages to determine the eligible borrowing amount. The maximum possible loan size is $2 million. The average (median) loan amount is $72,667 ($18,389). Loans are disbursed in stages to ensure funds are spent according to the agreement, and based on actual physical repairs. Repayment is deferred for four months, though there are options to defer longer. After this deferment period, the firm repays in monthly installment payments of principal and interest, which fully amortize the loan amount and the interest accrued during the deferment period. The average (median) monthly payment is $685 ($274).

Insurance and Moral Hazard. As noted above, the SBA can only lend to repair damages that have not already been funded through insurance or other proceeds (e.g., through charitable donations). The program collects information on insurance claims payments for approved firms to comply with this requirement. Among these firms, 41% had a relevant form of property insurance, such as flood insurance following a hurricane, and 24% received a payment. Similarly, Collier et al. (2024) find that, among firms in Southeast Texas one year after Hurricane Harvey, around 40% of firms who experienced flood damages had flood insurance. The average (median) approved firm in our data receives $28,827 ($0) in claims payments (Table 2). In sum, most firms do not have insurance and when they do, it is often insufficient to repair damages and thus the firm requires additional funds.

Low levels of insurance coverage seem in part to reflect inefficiencies in flood and other catastrophe insurance markets. Flood (and sometimes wind) insurance coverage must be purchased through a standalone policy, which many businesses and households do not buy, especially if they are outside the highest risk areas (Billings et al., 2022). Flood maps are frequently outdated and crudely designed (Mulder, 2021). Coverage limits in the National Flood Insurance Program (NFIP) are capped at levels that may be insufficient (Collier et al., 2020, 2024) and cover a small share of commercial damages (CBO, 2019). Finally, limited trust in insurers to pay claims may also contribute to low take-up (Gennaioli et al., 2022).

A concern about government assistance, including through subsidized credit, is that it may induce moral hazard by reducing ex-ante demand for insurance. While an assessment of moral hazard is outside the focus of this paper, we anticipate that it is not first order because a firm's access to disaster loans is highly uncertain. First, it requires a federal disaster declaration for their area. Second, conditional on such a declaration, less than half of applicants are approved.

[12]The SBA's cash-flow measures are immaterial to our analyses, but are reported in SBA (2018).

3 Estimation Approach

Our goal is to estimate the causal effect of disaster loan approval on firm outcomes. We observe firms annually, beginning five years before they experience a disaster and ending seven years after the disaster. To identify the causal effects of loan approval, we leverage the credit score discontinuities used in the program's loan underwriting as instruments for loan approval. Below, we describe these discontinuities and our resulting instrumented difference-in-differences design.

Credit Score Discontinuities The owner's FICO score is a key factor determining loan approval (SBA, 2018, p. 101). Like many private lenders and other government agencies (e.g., the GSEs, FHA), the SBA employs FICO score thresholds. These thresholds have changed over time. We present examples of *internal* SBA memos describing the thresholds and how they change in Appendix A. One from 2018 indicates that loan officers should typically automatically decline applicants with scores below 570, approve those with scores of at least 670, and rely more heavily on inputs besides FICO score, such as business cash flows, for intermediate scores (Figure A1). Another memo describes changes to the thresholds (Figure A2). We understand from program managers that they revise the thresholds as they learn from the performance of existing loans and adjust underwriting thresholds based on balancing the program's goals of expanding post-disaster credit provision and minimizing costs to taxpayers.

The credit score thresholds provide a set of quasi-experiments. Whether an applying firm participates in an experiment depends on two sources of plausibly exogenous variation: the timing of the natural disaster and the owner's credit score being near a discontinuity. The specific thresholds allow for causal assessments as long as the standard identifying assumptions (e.g., continuity) hold, which we consider below.

We observe the discontinuities in the data because the approval likelihood jumps discretely at certain credit score thresholds. For example, Panel A of Figure 3 shows the relationship between credit score and loan approval for disasters that occurred in 2017: loan approval increases by 20 pp at a credit score of 570. To locate the credit score discontinuities over time, we use an algorithm adapted from Argyle et al. (2020). We partition the data by years, assigning applicants to the year of their disaster declaration date. For each year, we regress the approval probability on bins of the owner's credit score (each bin includes 10 FICO points). We define a credit score discontinuity using three criteria. First, the change must be economically meaningful: the approval probability must change by at least 5 pp at credit score bin c relative to the previous bin. Second, the change must be statistically significant (at the 5% level). Third, approval must be smooth near the threshold: the change in approval probability from the nearest adjacent credit score bins (the bins just above and below bin c) must be statistically insignificant (at the 10% level).

The algorithm identifies 22 threshold-by-year discontinuities. We normalize applicants' FICO scores by subtracting the relevant threshold value and combining data from all 22 discontinuities. We include applicants within a bandwidth of 29 FICO points, which reduces overlap between possible discontinuities.[13] Panel B of Figure 3 shows the combined discontinuity. Businesses are 12 pp more likely to be approved if their owner's FICO is above the threshold.

The discontinuities range from FICO scores of 540 to 780 over the years 2005 to 2017. The 22 natural experiments represent 13 FICO thresholds, 12 years, and 916 natural disasters.[14] By leveraging these natural experiments, we can examine the effects of loan approval for a population with a broad range of credit scores. This approach stands in contrast to many regression discontinuity designs (RDDs), which rely on a single threshold for identifying variation and can speak only to the treatment effects of individuals near that threshold. Panel C of Figure 3 shows the discontinuities; the vertical axis reports the change in approval probability at the threshold. Panel D shows the number of applicants for each discontinuity.

Appendix B provides four analyses confirming the validity of the discontinuity design. Here we briefly summarize them. First, to test for manipulation near the cutoff, we conduct a McCrary test and do not find evidence of bunching near the threshold. Second, we show that firm characteristics and the financial outcome variables are continuous around the cutoff. A possible concern is that the SBA might adopt FICO thresholds used by private lenders, but we do not find any evidence of this—the appendix shows that private debt balances are smooth through the SBA's FICO thresholds in the period just before the disaster. Note that the thresholds are independent of SBA expertise in assessing disaster repairs, so expertise should also be smooth across the FICO thresholds, along with other dimensions of the SBA process such as loan officer training. Third, we estimate how the threshold affects approval likelihood in models controlling for the running variable and fixed effects for ZIP code and disaster year by FICO threshold. Across specifications, firms just above the threshold are 10 to 15 pp more likely to be approved for a loan; in all cases, the threshold is a highly significant predictor of approval. Last, we show that applicants within the FICO score bandwidth used in analysis are qualitatively similar to the full sample.

Estimating Equations. We leverage the credit score discontinuities to approximate the ideal experiment of randomly allocating loans to a subset of firms. First, consider a naïve estimation in which we regress a firm outcome y_{it} on whether the firm was approved. We observe applying firms before and after the disaster, permitting the following difference-in-differences model for firm i and event time t:

$$y_{it} = \beta Approved_i \times Post_t + \gamma_i + \delta_t + \varepsilon_{it}. \tag{1}$$

[13] Our findings using data from businesses' credit reports (e.g., on private loan balances, bankruptcy filings, credit delinquencies) are robust to alternative bandwidths, which we show below. We are prohibited by disclosure rules from indicating whether the results using data from Census are robust to alternative bandwidths. Please contact the authors if interested in disclosure of alternative bandwidths.

[14] We find discontinuities in each year except 2013, which was a relatively mild year in terms of natural disasters.

$Approved_i$ is an indicator for loan application approval and $Post_t$ is an indicator for observations that occurred after the disaster. Each firm is associated with a single disaster. γ_i is a firm fixed effect and δ_t is a time-based fixed effect.

Equation (1) does not offer a causal assessment because loan approval co-varies with firm characteristics. The effect of potential endogeneity in this estimation is ambiguous. On the one hand, lower credit risk firms are more likely to be approved, which could bias naïve estimates to overstate the benefits of recovery loans. On the other hand, loan officers have some underwriting discretion and may direct loans toward the neediest firms based on damages, which could bias naïve estimates in the opposite direction.

Instead of relying on Equation (1), we use the credit score discontinuities as an instrument for approval of firm i. Specifically, we estimate the 2SLS model

$$\begin{aligned} Approved_i \times Post_t &= \lambda \mathbb{1}(FICO_i \geq 0) \times Post_t + \theta_i + \kappa_{jt} + u_{it} \\ y_{it} &= \beta \widehat{Approved_i} \times Post_t + \gamma_i + \delta_{jt} + \varepsilon_{it} \end{aligned} \tag{2}$$

The second stage uses predicted approval $\widehat{Approved_i} \times Post_t$ in an instrumented difference-in-differences estimation. Thus, the first stage effectively randomizes treatment in the limit of the discontinuity, and the second stage leverages both pre- and post-disaster observations to estimate treatment effects. The model also includes fixed effect δ_{jt}, which interacts each natural experiment j with each event time t. The 22 natural experiments described above represent disaster-declaration-year by credit-score-threshold combinations. For example for applicants affected by disasters occurring in 2017, the program used a credit score threshold of 570. Each experiment gets its own event-time indicators, taking values such as -2 to represent the second year before the disaster year:

$$\delta_{jt} = \sum_{k=-5}^{7} \mathbb{1}(t = k) \times d_j,$$

where d_j is an indicator for each natural experiment j. The first stage includes the same fixed effects as the second stage: firm fixed effects θ_i and experiment-specific time fixed effects κ_{jt}. The experiment-specific time fixed effects help generate clean control-group estimates for each experiment.[15] This stacked regression design addresses concerns that in a setting with staggered treatments, fixed effects that are shared across treatments can bias estimates of the average treatment effect (Baker et al., 2022; Cengiz et al., 2019).

Building on the continuity assumptions of the RDD, the instrument provides a local average treatment effect (LATE) interpretation under the additional assumption that the instrument has the same directional

[15] Because each natural experiment occurs in a specific year, these event-time fixed effects are equivalent to including year fixed effects for each experiment. For example for the experiment regarding disasters in 2017 described above, an experiment-specific fixed effect for event year $t - 2$ is equivalent to an experiment-specific fixed effect for year 2015.

effect on all applicants (i.e., the setting includes no defiers). This additional assumption seems likely to hold because the program documentation codifies basing loan approval on credit score (SBA, 2018). Also, approval likelihood increases at the credit score threshold for each of the 22 identified discontinuities, not just on average across the experiments (Figure 3, Panel C).

We use several variations of this baseline model. First, we estimate an event study version, which adds event time indicators to assess treatment effects over time. These 2SLS models use a set of instruments, one for each of the approval-by-event-time interaction terms:

$$\begin{aligned} Approved_i \times \mathbb{1}(t=k) &= \lambda \mathbb{1}(FICO_i \geq 0) \times \mathbb{1}(t=k) + \theta_i + \kappa_{jt} + u_{it} \\ y_{it} &= \sum_{k=-5, k\neq -1}^{7} \beta_k \widehat{Approved_i \times \mathbb{1}}(t=k) + \gamma_i + \delta_{jt} + \varepsilon_{it}. \end{aligned} \tag{3}$$

That is, we examine loan approval on firm outcomes starting 5 years before and ending 7 years after the disaster, using the last observation of the firm before the disaster $t-1$ as the reference period. Each event-time period k receives its own first-stage equation (modeling $Approved_i \times \mathbb{1}(t=k)$), except for the reference period.[16] An additional benefit of this event study model is that it allows for examination of pre-trends. While we expect the discontinuity design to randomize treatment, pre-event observations facilitate an assessment of parallel trends before the disaster.

As an alternative specification for robustness, we replace firm fixed effects with ZIP fixed effects in the baseline model. Those models include quadratic controls for the running variable $f(FICO_i)$ and an additional first stage equation (instrumenting for $Approved_i$ with $\mathbb{1}(FICO_i \geq 0)$). Our baseline model omits these terms as they are collinear with firm fixed effects.

To assess spillovers, we apply the baseline model to consider the effect of approving firm i on neighboring, non-applicant firm m. Specifically, we estimate the 2SLS model

$$\begin{aligned} Approved_i \times Post_t &= \lambda \mathbb{1}(FICO_i \geq 0) \times Post_t + \theta_m + \kappa_{jt} + u_{mt} \\ y_{mt} &= \beta \widehat{Approved_i \times} Post_t + \gamma_m + \delta_{jt} + \varepsilon_{mt} \end{aligned} \tag{4}$$

Each neighbor is associated with a single focal firm. Thus, this estimation is like Equation (2) but replaces outcomes and firm fixed effects for focal firm i with those of neighboring firm m.

[16] For the reference period, we would normally incorporate an additional first-stage modeling the direct effect of being approved $Approved_i$ (without event-time interactions); however, approval is determined once for each applicant, making it time-invariant. Thus, $Approved_i$ is collinear with firm fixed effects and that first stage cannot be estimated.

4 Real and Financial Outcomes Data

We draw outcomes from two panel datasets, one from the U.S. Census Bureau and one from Experian, a leading credit bureau.

U.S. Census Bureau Data. The SBA applicant firms were matched by U.S. Census Bureau staff to the U.S. Census Bureau's Business Register, which contains all business establishments in the U.S. private non-farm sector. They successfully matched 82% percent of the firms.[17] We use data from 2000 (which is five years before the first disaster application in 2005) through 2019.[18]

Summary statistics at the firm-year level for the analysis sample (i.e., within the RDD bandwidth) are in Table 3.[19] Panel A describes all SBA applicants in the Census sample. Panel B describes the subset of applicants matched to both Census and Experian, which represents 87% of the firms in the Census sample. They are the same age but were somewhat more likely to be approved than the full Census sample (39% approval rate versus 34% for the full Census sample). In our heterogeneity analyses below, we examine if the treatment effects vary with whether the ZIP code has an above-median share of Black residents. The table shows that the Census sample and the subset matched to Experian have a similar share of firms located in Black communities (23% vs. 22%).

The real outcomes are summarized in Panel C of Table 3. Exit occurs when a firm is no longer found in the Census databases for the rest of our sample period. It is a binary variable that switches from zero to one in the year that the firm exits and remains one thereafter. Since only operating firms apply for SBA disaster loans, all exits occur after the disaster by construction. The mean is 12%. The remaining outcomes are only populated for operating firms. For example, when a firm exits, its employees are coded as missing so the number of employees only represents operating firms. The second outcome is deformalization, when a firm permanently transitions from an employer to a non-employer. The mean is 0.06, indicating that transition to a non-employer represents 6% of the firm-year observations for operational firms.

The third outcome is annual employment. On average, firms have about three employees, though this includes many zeros for the nonemployers; the average is just over five among employer firms. These figures are smaller than the SBA application figures, which report employees at the time of application (Table 2),

[17]The Census matched using both applicant individuals (i.e., firm owners) and the firm EIN, when available. When not available, they used probabilistic and then clerical matching using firm names, addresses, and ZIP codes.

[18]We draw outcome variables from two datasets. The first is the Longitudinal Business Database (LBD), which is the universe of non-farm, non-public administration business establishments with paid employees (Chow et al., 2021). The second is the Integrated LBD (ILBD), which includes nonemployer firms (Davis et al., 2007; Goetz et al., 2021). The share of nonemployers in our data is similar to the overall share of nonemployers among U.S. firms. This is about 75% during our sample period. See e.g. Census Nonemployer Statistics.

[19]Total observations are rounded to the nearest thousand due to Census privacy restrictions. This sample includes 24,000 unique firms. The count of 291,000 results from observing these firms annually over a 12 year period.

reflecting the inclusion of years before the application in the U.S. Census Bureau data, when firms were on average younger and smaller. The last outcome is revenue, which is observed annually. For nonemployer firms, it is calculated from reported receipts. The average is \$351,000. The sample is smaller because revenue is not available for all firms due to the limitations of the data.

Credit Bureau Data. To understand the financial implications of disaster loans, we acquire data from Experian on firm credit histories. Credit reports represent a snapshot of the firm; our records were drawn on June 30 of each year. Lenders report to Experian on firms' credit balances and repayment behavior (e.g., the duration of credit delinquencies). Lessors and utilities also report firms' repayment behavior on their contracts. Last, Experian collects bankruptcy filings from county records. The SBA disaster loan program does not report to Experian. The one exception is loan charge-offs: the federal government reports non-performing loans to Experian during the collections process.[20] Experian matched 68% percent of SBA applicant firms to their database.

Table 4 reports summary statistics of the outcome variables used in the analysis. "Outstanding debt" is in thousands of dollars and is the firm's total credit balances. The average applying firm has around \$8,000 in outstanding debt. Because of the SBA reporting process, SBA disaster loans would contribute to this debt balance only in the case that they are charged off. The second row reports "outstanding debt, paid on-time" which is the total balance on the firm's debt that is paid in accordance with the agreed contract terms. Thus, this balance excludes any delinquent loans (including any charged-off SBA disaster loans). "Number of contracts" is a count of the time-sensitive financial obligations that the firm has to lenders, lessors, telecom providers, and others. While the debt balance measures described above are specific to credit contracts, "number of contracts" comprises a broader set of commitments. "Delinquent share of debt" describes the share of total outstanding debt that is held in loans that are at least 90 days delinquent. Any charged-off SBA disaster loans should be reflected in these severe delinquencies. "Days Late on all Contracts" is a weighted average of the duration of a firm's delinquent obligations. It is calculated on a $+30$ day basis; the mean of 16.7 indicates that on average, firms pay 46.7 days after being invoiced for payment. "Bankruptcy" reports whether the firm files for bankruptcy in the current year.

To assess spillovers, we make use of data on non-applicant firms from both the Census data and from Dunn & Bradstreet. For each focal firm, which is an applicant for a SBA disaster loan, we examine how the receipt of a disaster loan affects entry of new firms nearby and how it affects local, incumbent neighbor

[20]The SBA transfers non-performing loans to the Department of Treasury, which pursues collection of the remaining loan balance and reports the delinquent debt to the credit bureaus, in accordance with the Debt Collection Improvement Act of 1996 (SBA, 2015; U.S. Congress, 1996). Managers at both the SBA and Experian confirm that SBA disaster loans are not otherwise reported to Experian.

firms. We provide more details regarding how we identify firm entry and incumbent neighbor firms when we examine spillovers below.

5 Effects of the Recovery Loans

This section uses the empirical design from Section 3 to assess direct effects of recovery loans on firms' real and financial outcomes. We organize the results around three themes. First, we examine whether recovery loans persistently affect firm distress as measured by exit, bankruptcy, and credit delinquencies. Second, we explore the effects on firms' private debt balances and contractual commitments with an interest in whether recovery loans crowd-out or crowd-in private investment in the firm. Finally, we examine the effects on firm employment and revenues. We end the section with robustness tests.

5.1 Do Recovery Loans Persistently Reduce Distress?

Exit. From a theoretical perspective, how recovery loans will affect the chance of firm failure (i.e., exit) is ambiguous. There are several possible outcomes, which guide the policy debate about recovery loans. First, the private sector may adequately provide emergency liquidity. If so, we expect a null effect: firms who do not receive a recovery loan but are potentially viable would attract funding from private lenders, enabling them to survive. Second, recovery loans might expand lending to firms that are fundamentally unviable. In this case, we expect recovery loans to delay but not to prevent exit; they might reduce short-term exit rates by providing an inflow of cash, but would increase exits in later periods when the cash is exhausted. Finally, recovery loans might reduce exits persistently if they help firms survive who are otherwise unable to obtain emergency liquidity in the private market.

Column 1 in Panel A of Table 5 provides a causal estimate of the effect of recovery loans on firm exit, following the IV estimation strategy from Equation 2.[21] We find that receiving a loan reduces the probability of exit by 13 percentage points (pp). For reference, mean exit is about 21% for declined firms in the post-disaster period, reflecting the precarious position of many firms following the disaster. Panel A in Figure 4 presents the dynamic effects of recovery loans on firm exit. The disaster occurs sometime during year zero. Note that since the firms are, by construction, alive before the disaster, we cannot estimate pre-event coefficients for exit. Therefore, these plots are informative about the dynamics of effects but do not shed light on pretrends. The effect on firm exit grows over time. The treatment effect measures the change in the loan on the likelihood of exiting by a certain year. On average, the figure shows that approved firms

[21]The table includes the Kleibergen-Paap first-stage F-statistic of 157, indicating that the credit score threshold is a strong instrument for approval. Total observations are rounded to the nearest thousand due to Census privacy restrictions. The table also reports the mean of the outcome variable for each regression to facilitate interpretation.

are around 20 pp less likely to have exited seven years after the disaster. Thus, recovery loans appear to meaningfully mitigate the distress experienced by declined firms, enabling approved firms to permanently survive that otherwise would not.

Credit Delinquencies. We next examine credit delinquencies, which can be a precursor to bankruptcy and exit. While a recovery loan provides liquidity to help rebuild, it also increases the firm's debt burden. More leverage mechanically increases credit risks. Firms may be unable to sustain this debt, especially those with pre-existing obligations. Alternatively, the SBA loan, which is specifically designated for rebuilding, may enable a firm to recover its productivity, allowing it to service its existing debt while also taking on the new financial commitments imposed by the disaster loan.

We find that receiving a loan reduces the share of debt that is delinquent by 34 pp, which is more than twice the mean (Column 1, Panel A, Table 6). We also find that a loan reduces the number of days that the firm is late in paying on all contracts—which includes leases, utilities, and other regular obligations—by 35 days (200% of the mean, Column 2). Figure 5 Panels A and B show the dynamic IV model results (Equation 3) for the share of debt that is delinquent and days late on contracts. Neither outcome shows evidence of a pre-trend in the years before the disaster. Both subsequently decline before stabilizing in the fourth year after the disaster year. As we describe more fully below, this pattern contrasts with the outstanding private debt balances (Figure 7, Panel A). Below, we show that outstanding debt increases following the disaster, so changes in total balances do not explain the continued fall in the share delinquent. In other words, the decline in delinquencies is not due to paying off old debt with new debt or to a larger denominator of balances in the share delinquent. Instead, the recovery loan seems to improve the firm's health, allowing it to fulfill its financial commitments.

To interpret these large magnitudes, it is useful to consider how the raw means change for accepted and rejected loan applicants. Figure 6 Panel A shows that after the disaster, both approved and rejected applicants experience large increases in the delinquent share of debt, consistent with a large negative shock to the firm and the local economy. However, the increases are much larger among the rejected group; for example, the delinquent share roughly triples for the declined applicants and doubles for the approved applicants. This dramatic widening helps explain the large effect and it sheds light on the counterfactual for the quasi-experiment, showing that control firms experience mounting distress following the disaster. The estimated treatment effect reflects deviations from this high-distress counterfactual.

Bankruptcy. Bankruptcy is a key outcome for our study. The real outcome of exit (examined above) is not necessarily bad from a social perspective; to the contrary, it may be desirable for a firm to exit if it is unproductive. In contrast, it is well-established that bankruptcy is socially costly, transferring a portion of the costs of exit to the firm's lenders and other liability holders, and also incurring transaction costs of filing and proceedings (Bernstein et al., 2019; Dou et al., 2021; Antill, 2022).

A loan reduces the chance of bankruptcy by 3.8 pp (Table 6 Column 3). Figure 5 Panel C reports the dynamic IV model results, showing that recovery loans significantly reduce the likelihood of bankruptcy in the first three years after the disaster. Figure 6 Panel B shows the raw mean plots, indicating that the probability of bankruptcy increases for rejected firms from below 2% before the disaster to over 6% afterward; bankruptcy rates also increased for approved firms, but to a lesser degree, up to 4%. Thus, our estimated treatment effect represents a reduction in bankruptcies of 86% relative to that benchmark.

To better understand the connection between adverse financial outcomes and exit, we restrict the sample to ever-survivors in the Census data in Panel B of Table 6.[22] The effects on the adverse financial outcomes become insignificant in this sample. The coefficient on the share of delinquent debt is still reasonably large (-0.17), but the effects on days late and bankruptcies become much smaller, with the bankruptcy coefficient close to zero. This suggests that adverse credit outcomes are harbingers of exit, helping to explain the real effect.

We can continue to connect the financial and real outcomes by exploring how firm exit is related to adverse credit events, and whether this differs across firm type. In Table 7, we use the Experian-Census matched sample to separate the exit events into those that occur in firms with bankruptcy, delinquencies (but no bankruptcy filing), or neither of these (all other).[23] In the full sample, exit is dominated by the "other" category (Panel A), which we expect given the large share of nonemployers, who are unlikely to file for bankruptcy. This "other" exit describes firms without debt closing or firms paying off all of their debts and then closing. However, when we restrict to employer firms in Panel B, we see that the decline in exit with bankruptcy is by far the strongest effect relative to the mean (Column 2). Overall, this table suggests that among employer firms, the SBA disaster loans play an important role in deterring exit via bankruptcy in particular, shedding light on the social costs of the exit effect.

5.2 Do Recovery Loans Crowd Out Private Credit?

Government loans may crowd out (i.e., substitute for) private lending. This predicts a negative treatment effect on a firm's private debt balances: approved firms would use an SBA recovery loan to conduct repairs while the control firms would use a private loan. However, the evidence above that the government loans

[22]We use the Experian sample matched to Census data, where we observe survival, and restrict the sample to firms that survived at least until the end of the Census sample, which is 2019. In unreported analyses, we replicated our main Experian results in the Census matched sample (without limiting to survivors) and find very similar results to the ones reported in Panel A.

[23]We identify a firm as having a bankruptcy or delinquency with exit if the financial outcome is positive in the year of the exit or the preceding year. Appendix Table C1 provide exit summary statistics for all firms, declined firms, and approved firms, respectively.

reduce exit suggests that the private sector may be unwilling to make emergency loans to firms for disaster repairs. Instead, the government loan might create future private lending opportunities by funding repairs. In this *crowd-in* case, government and private loans are complements and would result in a positive treatment effect on a firm's private debt balances.

Private Debt Balances. We find that an SBA recovery loan dramatically increases non-SBA private credit, with overall outstanding balances rising by nearly \$18,000 (Column 1 of Panel A in Table 8). Column 2 shows outstanding balances that are paid on time, which omits the balances of any delinquent debt. Recovery loans increase paid-on-time balances by \$16,000, indicating that not only are SBA recovery loans unlocking private credit but also that firms are able largely to sustain this additional private debt. In both cases, the effects represent a bit more than a doubling of the sample mean. The event study coefficients are plotted in Figure 7. We see no evidence of pretrends, again consistent with a valid quasi-experimental design, and then a discontinuous increase in private debt after the disaster. The effect is persistent for at least seven years.

Figure 6 Panel C presents the raw means plot for Outstanding Debt (Outstanding Debt Paid on Time looks very similar). The divergence in raw means after the disaster are in the same direction as the IV results albeit with different magnitudes. Outstanding debt tracks closely across the two groups until the first and second years after the disaster, when the approved group raises much more private debt while the rejected group raises somewhat less. The graph suggests that the effect on balances emerges from increasing the private debt of approved firms, rather than mitigating a big falloff in balances for rejected firms.

It is possible that the effects on private debt balances are demand-driven; that is, firms who are rejected for a recovery loan may not apply for a private loan. To assess this, we examine the firm's number of credit inquiries, which occurs when a potential lenders checks the firm's credit record due to a loan application. We find no significant effect, suggesting that treated and control firms apply for private credit at similar rates after the disaster (Table C2).

Number of Private Contracts. The number of contracts on a firm's credit report is a holistic measure of its time-sensitive obligations, including private credit, leases and agreements with utilities and telecoms, and others. Recovery loans increase these private contracts by 0.6, which is 116% of the mean (Column 3 of Panel A in Table 8). This suggests that recovery loans not only improve access to private lending but may also foster a supply network. The raw means are plotted in Figure 6 Panel D.

We explore whether these credit effects mechanically result from survival. That is, the estimated differences might emerge from the effects of SBA loans on firm exit: while surviving firms may take on debt, exited firms cannot. We restrict the sample to ever-survivors in the Census data in Panel B of Table 8.

The results are very similar to Panel A, indicating that the additional credit is not closely tied to survival.[24] Instead, overall these results indicate that recovery loans help firms attract additional investment from the private sector and maintain activity with vendors, consistent with a positive outlook for firm growth.

5.3 Do Recovery Loans Affect Employment and Revenues?

Finally, we examine the effects of recovery loans on employment and revenues. These measures speak to the health of the firm and are likely especially relevant to broader local economic recovery.

Deformalization. We first consider transitions from an employer firm to a nonemployer firm, which we term “deformalization." This is an adverse outcome from a growth perspective, pointing to fewer benefits for the community through employing workers. Recovery loans reduce the likelihood of deformalization by five percentage points, or about 85% of the mean, though this result is noisy (Column 2 in Panel A of Table 5). The sample size is smaller in Column 2 relative to Column 1 because it only includes firms that were still operating in a given year. Figure 4 provides the dynamic IV model results, showing that recovery loans significantly reduce the likelihood of deformalization even six years after the disaster.

Employment and revenues. We find that the loans increase employment by 18% among survivor firms, which implies about half a worker at the sample mean (Column 3 of Table 5). Here and subsequently, we exponentiate coefficients for logged outcome variables to interpret the results relative to the mean. Note also that the employment measure is the log of one plus employment, so it considers the primary owner an employee in addition to any official employees, and is zero when firms have no official employees in a given year. We find a large but insignificant effect on revenue, in Column 4. Recall that the sample is smaller for revenue because of limitations in Census's data; revenue is missing for many firms.[25] Figure 4 Panels C and D contain the event studies for these outcomes. Employment increases significantly starting in the year after the disaster year and remains elevated for at least five years.

Employer Firms. Employer firms are more important for growth than nonemployers, even though they are the minority among firms in the overall economy (Haltiwanger, 2022). In Panel B of Table 5, we restrict the sample to firms that were employers in the year before the disaster. Here, we find more pronounced effects. An SBA recovery loan reduces the chances of exit by 17 percentage points for an employer firm. The effect

[24] In unreported analyses, we replicated our main Experian results in the Census matched sample (without limiting to survivors). Results are available upon request.

[25] Revenue data comes from tax forms that do not always merge with the core databases behind the Business Register, in part because it is only reported at the firm rather than establishment level, and sometimes under a different EIN than the employment and payroll data. Revenue is more often missing for nonemployers and in the initial years after firm entry.

on deformalization is larger, at 12 percentage points or about twice the mean. This is not surprising since only firms with employees can deformalize. Employment increases by about 45%, which in this subsample represents 2.25 workers. Finally, revenue increases significantly in this sample, by about 100% of the mean. Appendix Figure C1 presents the dynamic IV model results for employer firms, showing statistically significant effects that persist and even grow stronger toward the end of the time series.

Overall, we find that recovery loans substantially reduce the likelihood of adverse firm outcomes in the form of exit, bankruptcy, and credit delinquencies. They have a powerful crowding-in effect on private debt, and appear to increase firm growth, especially among employer firms. The effects are persistent for at least seven years after the disaster, indicating that borrowers typically are able to sustainably service these loans. These results point to a positive impact on local economic recovery.

5.4 Robustness Tests & OLS Results

We conduct a range of robustness tests in addition to the validation tests of the empirical strategy described in Section 3. First, we re-estimate our models using ZIP code rather than firm fixed effects in Appendix C.1. Here we incorporate the additional first stage for $Approved_i$ as well as quadratic controls for FICO score (the running variable), which is not possible in our main specification because the controls are collinear with firm fixed effects. For rare outcomes (e.g., firm exit, bankruptcy), firm-level fixed effects soak up very little variation and models with ZIP code fixed effects may be more effective. These alternative specifications provide qualitatively similar results.

Second, we re-estimate our models using other credit score bandwidths in Appendix C.2. The primary analyses use a bandwidth of 29 (i.e., the owner's credit score is within 29 FICO points of a credit score threshold for all applicants in the regression). The appendix provides the credit report outcomes using samples with bandwidths of 19 and 9. The results using these others samples are similar in sign, magnitude, and statistical significance. One exception is for bankruptcy: while the magnitude of the treatment effect is similar, it is not statistically significant, which seems due to the loss of statistical power in the samples using smaller bandwidths.

Third, we examine how the results differ across the FICO distribution. To do this, we split the sample into three parts using the FICO thresholds and estimate treatment effects within each subsample. The first group includes FICO thresholds below 600 (42% of applicants in the regression sample), the second includes thresholds between 600 and 700 (31% of applicants), and the third includes thresholds of at least 700 (27% of applicants). The results for private credit balances and bankruptcy are in Appendix C.3. (We do not report similar figures for the real outcomes due to Census disclosure restrictions.) For private credit balances, the effect is large and significant throughout the FICO distribution. For bankruptcy, the effect is driven by FICO scores in the middle of the distribution, suggesting that these are the more marginal firms where the loan has the largest effect on distress.

The fourth robustness test examines recovery loan charge-offs in our assessment of credit delinquencies in Appendix C.4. While Experian and the SBA indicate that recovery loan charge-offs are reported to the credit bureaus, it is possible that gaps or delays in recording these charge-offs prevent them from being incorporated into credit delinquencies. If so, our finding that recovery loans reduce credit delinquencies could instead reflect shifting delinquencies off-credit-report to the SBA. We observe recovery loan charge-offs in the SBA data and find that adding them directly to our delinquency measures yields qualitatively similar results (a LATE of -0.32 versus -0.34 in the main analyses). Thus, the decline in credit delinquencies does not appear to result from gaps in reporting recovery loan charge-offs.

As a final robustness test, we examine whether the crowd-in results are affected by the program's collateral rules in Appendix C.5. Small recovery loans (e.g., below $10,000 in 2005) do not need to be secured by collateral (Section 2). It is possible that the private debt effects might only occur for uncollateralized recovery loans because collateral availability can affect access to private credit (Rampini, 2019; Rampini and Viswanathan, 2020). We find that private debt balances increase both for firms with collateralized and firms with uncollateralized recovery loans.[26]

We present OLS results for all outcomes in Appendix C.6. The OLS results are a useful reference but should be interpreted with caution due to endogeneity concerns. Overall, they are consistent with the 2SLS results: uniformly in the same direction and highly statistically significant. The estimates are generally larger for 2SLS than OLS, indicating that the effect for compliers (firms who are approved because of the credit score threshold) is larger than for the average approved firm. A likely explanation is that SBA loan officer discretion introduces endogeneity, biasing the OLS results downward. Loan officers may use their discretion to direct credit toward more damaged firms. Consistent with this explanation, Table 2 shows that approved firms have larger damages than declined ones. In contrast, the 2SLS results reflect an effectively random assignment of loan approval and so more accurately capture causal effects.

6 What Frictions do SBA Recovery Loans Address?

We consider two barriers to emergency liquidity financing that might explain our results. The first, which our data best supports, is uncertainty about disaster repairs and recovery. This friction could also exist after

[26]Consistent with this finding, Pan et al. (2023) examine the recovery loan program's collateral rules. They conclude that businesses' decisions regarding whether to take a collateralized recovery loan do not relate to accessing private credit, but instead to maintaining flexibility in selling and buying real assets (e.g., machinery) that are often used as collateral.

other types of shocks, such as pandemics, financial crises, technology disruptions, or trade wars. The second is pre-existing financial constraints. Some firms may be more difficult to underwrite because they are riskier or have less information available to lenders.

Repair Cost Uncertainty. Uncertainty stifles investment, especially when the investment is difficult to reverse (Julio and Yook, 2012; Baker et al., 2016; Gulen and Ion, 2016; Baker et al., 2024; Campello et al., 2024). This could help explain the recovery loan's effect on private lenders. Lenders may lack the expertise to assess whether the firm can absorb the costs of repairs, especially in light of indirect negative effects from local economic disruption. By funding repairs, the SBA resolves this uncertainty, facilitating private sector investment. Note that in our instrumented setting, treated and control firms are effectively randomly assigned around a FICO threshold, making loan approval a lottery that contains no information about firm quality. Instead, it is the SBA's act of funding repairs that changes the firm in the eyes of the private sector.

The seniority of the government loan helps to narrow down the channel by implying that the loan must reduce risk for the private lender, rather than act only through the subsidy value of the loan amount. The government loan is senior, so the private lender gets nothing before the SBA is fully repaid. The crowd-in results reveal lenders' preferences. They do not lend as much to control firms as to treated firms—despite control firms applying at similar levels (see Section 5.2)—and when they lend to treated firms they do so after the SBA in a junior position. This points to high uncertainty in the absence of the SBA loan. To see this, suppose that the SBA loan amount is X_1, and the private (crowd-in) loan amount is X_2. Without uncertainty, the private lender should be willing to also lend X_1. In that scenario, control firms would receive $X_1 + X_2$ from private lenders, and we should observe no causal crowding-in. Instead, the positive effect on private debt shows that control firms do not receive X_1 or X_2, pointing to a role for uncertainty resolution in explaining the government loan's impact.[27] The government loan's seniority is crucial to this argument, precluding a story in which the recovery loan's effects solely reflect the firm being somehow in a better financial position.

We expect that resolving this uncertainty should be more valuable for more capital-intensive firms, since they rely on machinery, equipment, and other physical assets that may be damaged by the disaster. Uncertainty appears to be especially important in limiting investments in capital-intensive firms because they are often irreversible (Campello et al., 2024). We examine firms based on their capital intensity using the firm's NAICs code.[28] To test for this and other relationships between firm characteristics and treatment effects, we repeat our analysis on a sub-sample representing one half of the data, on one side of the median

[27] The authors are grateful to Ben Roth for this suggestion.

[28] The Bureau of Labor Statistics (BLS) provides statistics on labor share (labor compensation relative to sectoral output) at the NAICS 3-digit level for each year. We classify a business's industry as capitally intensive if its labor share is below the median labor share of all industries in a given year.

of the relevant characteristic.[29] We present the results in Figure 8, focusing on the two key outcomes of exit and private debt balances. The figure displays treatment effects as a percent of the mean for each sub-sample. The dashed black line in the figure represents zero. The dotted red line marks the treatment effect for the full sample for comparison with the sub-samples. The coefficients for capital-intensive firms (Figure 8, second row) indicate that the effects of the recovery loan on exit and private debt are statistically significantly larger than the full-sample effect. Although this does not rule out other mechanisms, it is consistent with the repair uncertainty channel.

Finally, the timing of the positive effect on private debt effect is consistent with treated firms completing disaster repairs. We present both raw mean and instrumented effects by month around the disaster in Appendix Figure D1.[30] The increase in private debt occurs around six months after recovery loan approval, which corresponds to the period that repairs are completed. We would expect the effect on private debt to occur more immediately under a pure certification effect. Alternatively, if the channel were investment and growth, we would expect the crowding-in effect to take longer to manifest.

Financial Constraints. There is a large body of evidence finding that financial constraints are more severe among firms that are relatively younger, have lower credit scores, lack existing relationships with lenders, operate in areas with fewer banks, or are located in Black communities (e.g., Berger and Udell, 1995; Bhutta and Hizmo, 2021; Howell, 2017; Fairlie et al., 2022). Lenders in general have less information about these firms and they are often riskier, making them difficult to underwrite. In an information asymmetry channel, we expect to see larger effects among firms that are likely to be more constrained.

We examine treatment heterogeneity using proxies for ex-ante financial constraints and summarize the findings in Figure 8. "Low intelliscore" restricts the sample to firms with below-median scores on Experian's proprietary measure of the firm's credit risk. "Young firms" includes those below the median age of eight years. "No lending relationship" includes firms who have no positive debt balances prior to the disaster. "Num bank low" includes ZIP codes with a below-median number of banks.[31] "Black ZIPs" restricts the sample to ZIP codes with an above-median share of residents who identify as Black. Figure 8 shows that we do not find meaningful differences in the effects of disaster loans along any of the measures of preexisting credit constraints, suggesting that this channel does not explain the large effects of recovery loans.

These results suggest that disaster recovery loans benefit many types of firms and provides additional support that the treatment effects are not driven by the subsidy alone. Since almost all recovery borrowers

[29] Because of confidentiality protections that limit our ability to export data from Census, we show only one side of each sample split.Appendix D provides additional results for each sub-sample split.

[30] While our main dataset tracks firms annually, we acquired additional credit report data to assess private debt balances on a monthly basis in the period following the disaster.

[31] This is from the FDIC Summary of Deposits (SOD) data on bank locations, publicly available here: https://www.fdic.gov/resources/bankers/call-reports/call-summary-of-deposits.html.

receive the same interest rate (typically 4%), the implicit subsidy varies by firm risk. For example, younger firms typically pose greater credit risk so receive a larger implicit subsidy than more established firms, but both appear to benefit from recovery loans.

7 Spillover Effects

We are interested in the effects of disaster loans on businesses in the local community. Above we show that a disaster loan increases the recipient's chances of survival, growth, and access to private credit. Here, we examine how this impacts neighbors of that "focal" recipient firm. On the one hand, neighbors may benefit if foot traffic increases and the focal firms serve as amenities. For example, at a strip mall a nail salon may benefit from the presence of a grocery store. On the other hand, SBA recovery loans may reduce activity at other firms. For example, a focal firm may draw traffic from competitors because it is able to make repairs more quickly and effectively. Similarly, private lenders might choose to reallocate funds away from neighbors and towards the focal firm.

To identify neighbor firms, we use the complete Census Business Register for analysis with the Census sample, and Dunn & Bradstreet for analysis in the Experian sample. We construct two measures: one of local entry and one of local incumbent neighbor firms. Both begin by identifying all other firms that did not apply for an SBA loan in the focal firm's Census tract. Entry is measured as the difference between the number of firms in the tract in year t and year $t-1$.[32] To construct the incumbent neighbors dataset, we then identify those firms in the focal firm's ZIP code that are also in the retail industry, since we expect that retail will be most sensitive to changes in traffic and neighborhood amenities.[33] Among this group, we retain the five nearest neighbors using an algorithm with the following preference step structure. First, the algorithm identifies best matches that are in the same census block. Second-best matches are on the same street and in the same tract. Finally, third-best matches are on the same street.[34]

To assess financial spillovers, we develop a set of incumbent neighbors for the focal firms using Dunn & Bradstreet (D&B) and then acquire Experian credit bureau panel data for them. This allows us to make use of the full Experian sample rather than only the subset matched to Census. Note that it is impossible to assess financial outcomes of the Census neighbors, because data on individual businesses (which Experian would require to match) cannot be taken out of Census due to disclosure restrictions. Therefore, we draw a set of neighbors from the complete D&B database from 2000 to 2019. We first geolocate each focal and

[32]We truncate this variable at 100 entrants, there are some outlier tracts with many more entrants.

[33]Retail includes the following three-digit NAICS groups: 442, 443, 444, 445, 447, 448, 451, 452, 453 and 722.

[34]The algorithm stops when it reaches five firms at any given step. If there are more than five firms that are matched in a step then we randomly choose five. If there is no match for three measures, then we take a random sample of five from the ZIP code. To match over street, we clean street names and then use a string distance measure.

D&B firm in ArcGIS, and then calculate shortest driving distance using an Open Source Routing Machine (OSRM).[35] As within Census, we then identified the closest five firms in the retail industries. We describe the key findings here; the full results for the neighbors analysis are in Appendix E.

We find two marginally significant effects of disaster loans on neighbors. First, we consider firm entry into the neighborhood. We focus on the sample of focal firms with at least three employees, which could plausibly affect local amenities and neighborhood quality. We find that receiving a loan increases local firm entry by about eight firms, which is large relative to mean entry of just 1.3 firms (Table E2 Panel A, Column 1).[36] We find insignificant effects using all focal firms (not reported), that is, including firms with fewer than three employees. Second, among incumbent firms, we find a marginally significant reduction in revenues, suggesting that focal firms may draw customers away from nearby competitors. One interpretation of these results is that they reflect a type of creative destruction: recovery loans may help focal firms cater to the rebuilding neighborhood (e.g., replacing damaged furniture and fixtures with pieces that better match the tastes of current residents). This revitalization might entice entrants to locate nearby but also reduce the competitiveness of incumbent firms who had not made such updates.[37]

8 Program Costs and Benefits

While our main results document real and financial benefits to borrowers, whether the program is socially useful depends also on costs to the government (and therefore taxpayers). Relative to grants, a potential benefit of subsidized credit for governments is that loans require repayment; however, credit programs add an administrative burden in that loans must be underwritten and serviced, potentially attenuating their advantages. We can begin to shed light on this question by calculating the costs of the programs and comparing it to certain dimensions of the benefits that our causal estimates capture. It is important to emphasize that particularly on the benefits side, we can offer only a very rough, back-of-the-envelope calculation.

Regarding costs, the SBA reports disaster loan program administrative expenses for each fiscal year (e.g., SBA, 2017).[38] The federal fiscal year begins on October 1, and we limit our analyses to the fiscal years for which we have complete records in our loan application data, 2006 to 2017. The Office of Management and Budget (OMB, 2023) provides subsidy rates for the program, treating loans made in each fiscal year as its own cohort. The subsidy is calculated following the Federal Credit Reform Act of 1990 (FCRA), which

[35] We geolocate using an existing address book available through NYU. OSRM is available here.

[36] The result is similar using the log outcome or not truncating the entry variable at 100 firms.

[37] We do not find significant effects on exit, deformalization or employment of nearby incumbent firms. We also do not find any significant effects on credit report outcomes (e.g., private debt, delinquency, or bankruptcy) using the D&B neighbors (see Appendix E for results).

[38] Disaster loan program costs are not differentiated between loans made to firms versus households. Based on the assumption that underwriting entails fixed costs per loan, we allocate administrative costs based on the number of loans to firms versus households.

standardizes subsidy calculations across government lending programs (GAO, 2016). The FCRA subsidy is an expected-net-present-value calculation: the difference between the loan disbursement (i.e., current cash outflows) and the discounted value of expected payments on that loan (i.e., future cash inflows). Thus, both low interest rates and expected default rates contribute to disaster loan subsidies. The FCRA calculates present-value discounting using U.S. Treasury rates on loans with similar durations.[39] The OMB subsidy rates are retrospective in that they revise costs based on realized loan performance (e.g., based on loan repayment history) up to the time of the report. We add an additional expense to reflect the deadweight loss of funding government programs through taxes, following Poterba (1996) who estimates that taxation adds 30% to program costs.[40]

Based on these calculations, we estimate that on average, each dollar loaned to businesses costs $0.41. This cost comprises administrative expenses of $0.17, subsidies of $0.15, and a taxation cost of $0.09. Table 9 reports program costs by fiscal year. It shows that program costs per dollar loaned are lower during years with large disasters, suggesting economies of scale. For example, the cost per dollar loaned was $0.22 in fiscal year 2013, when the program provided recovery loans for Hurricane Sandy. Also, the program appears to have reduced costs over time. The average cost per dollar loaned was $0.45 from 2006 to 2011 and $0.31 from 2012 to 2017.

On the benefits side, we first examine how additional revenue generated by the loans compares to the cost of the loans. Revenue represents a positive benefit for the economy, and likely would have multiplier effects. We focus on the result for the employer firms, where we have more precision. The coefficient of 0.719 from Column 4 in Panel B of Table 5 implies an annual average revenue increase of 105%, or $434,323 based on the sample mean shown in the bottom of the table. The average loan among firms that are employers as of the time of the loan application is $92,000 in nominal dollars (the revenue data in Census is also nominal). Therefore, an average annual revenue increase of $434,323 in the post-disaster years costs the government $37,720 ($92,000 times 0.41). Equivalently, it takes about $87,000 in government costs to generate $1 million in new annual revenue.

Next, we calculate the cost per job created because increasing employment is an important policy agenda for these types of programs. We use the estimated effect on employment from the overall borrower population. Here, the average loan is $72,000. The coefficient from Column 3 in Panel A of Table 5 is .17, or 18.5%. The average employment in this sample is 3, so the coefficient implies an increase of 0.56, or about

[39]Some academics argue that the government's projected cash flow analyses might better reflect true societal costs if they were based on taxpayers' discount rates instead of Treasury borrowing costs (e.g., see Lucas, 2012, for a review). We are sympathetic to this argument but rely on the FCRA calculations because they are the only reported subsidy estimates.

[40]This expense is often incorporated in government program analyses to reflect that taxes impose costs on consumers and producers that distort markets and reduce total welfare (García and Heckman, 2022). Some academics argue against incorporating this expense since it adds assumptions to the analysis and can make cross-program comparisons more complicated (Hendren and Sprung-Keyser, 2022). We have incorporated this 30% taxation cost in the interest of being conservative but have also itemized program costs to make its contribution to total costs transparent.

a half a job. The cost per loan is $29,520 ($72,000 times 0.41). Therefore, the cost per job is $53,000.

Other government assistance programs for firms offer a benchmark for interpreting the $53,000 cost per job. However, it is important to note that because each program is tied to a specific setting, whether the benefits of one program would extend to other contexts is unclear. The fiscal stimulus provided after the financial crisis by the 2008 American Recovery and Reinvesment Act (ARRA) created jobs at an estimated cost of $185,000 to $278,000 each (CEA, 2011). The forgiveable loans in the Paycheck Protection Program during the COVID-19 pandemic resulted in an estimated cost of $170,000 to $257,000 (not including other costs such as administration or taxation costs) to preserve just one job-year (Autor et al., 2022).

9 Conclusion

Does government-provided emergency credit facilitate business recovery? While subsidized credit is a potentially attractive policy tool in that it is less costly to taxpayers than grants, its benefits are unclear. Debate regarding recovery loans focuses on two concerns. First, any reductions in firm distress may be transient: emergency credit might keep struggling firms alive but saddle them with new obligations that they cannot sustain. Second, government lending may crowd out private lending that would otherwise have met firms' liquidity needs.

We assess this question using administrative data from the SBA disaster recovery loan program between 2005 and 2017. One advantage of our setting relative to existing work is that application data are available, which is not true for other SBA programs that are intermediated by private lenders. Moreover, we link the applicants to both financial and real economic outcomes, which are rare to observe simultaneously, especially outside of publicly traded firms. Our estimation design relies on discontinuities in approval rates at thresholds of the owner's FICO score. The thresholds permit an instrument for loan approval, creating a series of natural experiments that effectively randomly assign credit to businesses around the threshold. The annual, panel structure of our data allows us to study the dynamics of firm outcomes over time.

We find that disaster recovery loans have strong, positive effects on both real and financial outcomes. First, recovery loans reduce firm distress. They decrease the chances of firm exit and transition from an employer to a nonemployer firm, which has implications for future hiring and firm growth. They also reduce the likelihood that a firm is delinquent on its debts or files for bankruptcy. These effects grow over time and are strongly persistent, indicating that emergency credit places firms on a more secure path after the disaster. Second, we find that recovery loans *crowd-in* private investment. The private debt balances of treated firms increase in the months following receipt of a recovery loan and continue to grow over time, peaking five years after the disaster. In conjunction with these private investments, we also see positive effects on employment and revenue, especially among employer firms.

The meaningful and persistent effects of disaster recovery loans suggest that viable firms may be unable to address their liquidity needs in private credit markets. Thus, mitigating credit frictions may not only benefit markets by facilitating entrepreneurship and growth but also by reducing liquidity-induced distress and exit. We find evidence of market frictions consistent with uncertainty about the cost or feasibility of repairs. By agreeing to fund repairs, the SBA resolves this uncertainty, facilitating private investment in the post-disaster recovery period. This market friction is distinct from standard asymmetric information channels that constrain credit for firms, such as being young. It connects to a growing literature on how uncertainty stifles investment (e.g. Baker et al., 2024; Campello et al., 2024).

Our findings indicate that disaster loans are an important policy tool for resolving liquidity crises that might otherwise lead to financial distress and limit local reinvestment. The direct lending approach of the SBA contrasts with other disaster relief policy options, such as direct transfers to affected households, grants or forgiveable loans to firms, or paying for intermediation. In back-of-the-envelope calculations, we estimate that disaster recovery loans create employment at a cost of $53,000 per job.

Our findings are relevant to markets and policy, especially as climate-related disasters increase. Businesses' needs will vary as risks grow. More businesses will need to rebuild than in the past. In the highest risk areas, it may be optimal for communities and their businesses to relocate instead of rebuild (e.g., Hino et al., 2017). This will require community-level coordination and larger adaptation initiatives. While not the focus of our study, such initiatives could incorporate low-cost credit for business relocation. To our knowledge, community relocation has occurred too rarely thus far to quantify business outcomes, leaving this an important topic for future work.

References

Alok, Shashwat, Nitin Kumar, and Russ Wermers, "Do fund managers misestimate climatic disaster risk," *The Review of Financial Studies*, 2020, *33* (3), 1146–1183.

Antill, Samuel, "Do the right firms survive bankruptcy?," *Journal of Financial Economics*, 2022, *144* (2), 523–546.

Argyle, Bronson S, Taylor D Nadauld, and Christopher J Palmer, "Monthly payment targeting and the demand for maturity," *The Review of Financial Studies*, 2020, *33* (11), 5416–5462.

Autor, David, David Cho, Leland D Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz, "The $800 billion Paycheck Protection Program: where did the money go and why did it go there?," *Journal of Economic Perspectives*, 2022, *36* (2), 55–80.

Bachas, Natalie, Olivia S Kim, and Constantine Yannelis, "Loan guarantees and credit supply," *Journal of Financial Economics*, 2021, *139* (3), 872–894.

Baez, Javier, German Caruso, Valerie Mueller, and Chiyu Niu, "Heat exposure and youth migration in Central America and the Caribbean," *American Economic Review*, 2017, *107* (5), 446–450.

Baker, Andrew C, David F Larcker, and Charles CY Wang, "How much should we trust staggered difference-in-differences estimates?," *Journal of Financial Economics*, 2022, *144* (2), 370–395.

Baker, Scott R, Nicholas Bloom, and Stephen J Terry, "Using disasters to estimate the impact of uncertainty," *Review of Economic Studies*, 2024, *91* (2), 720–747.

—, —, and Steven J Davis, "Measuring economic policy uncertainty," *The quarterly journal of economics*, 2016, *131* (4), 1593–1636.

Baldauf, Markus, Lorenzo Garlappi, and Constantine Yannelis, "Does climate change affect real estate prices? Only if you believe in it," *The Review of Financial Studies*, 2020, *33* (3), 1256–1295.

Balyuk, Tetyana, Nagpurnanand R Prabhala, and Manju Puri, "Indirect costs of government aid and intermediary supply effects: Lessons from the paycheck protection program," Technical Report, National Bureau of Economic Research 2020.

Banerjee, Abhijit V and Esther Duflo, "Do firms want to borrow more? Testing credit constraints using a directed lending program," *Review of Economic Studies*, 2014, *81* (2), 572–607.

Barrot, Jean-Noel, Thorsten Martin, Julien Sauvagnat, and Boris Vallee, "Employment Effects of Alleviating Financing Frictions: Worker-level Evidence from a Loan Guarantee Program," *Available at SSRN 3409349*, 2019.

Basker, Emek and Javier Miranda, "Taken by storm: business financing and survival in the aftermath of Hurricane Katrina," *Journal of Economic Geography*, 2018, *18* (6), 1285–1313.

Begley, Taylor A, Umit Gurun, Amiyatosh Purnanandam, and Daniel Weagley, "Disaster Lending:'Fair'Prices, but'Unfair'Access," 2020.

Berger, Allen N and Gregory F Udell, "Relationship lending and lines of credit in small firm finance," *Journal of business*, 1995, pp. 351–381.

— and Lamont K Black, "Bank size, lending technologies, and small business finance," *Journal of Banking & Finance*, 2011, *35* (3), 724–735.

Bernstein, Shai, Emanuele Colonnelli, Xavier Giroud, and Benjamin Iverson, "Bankruptcy spillovers," *Journal of Financial Economics*, 2019, *133* (3), 608–633.

Bhutta, Neil and Aurel Hizmo, "Do minorities pay more for mortgages?," *The Review of Financial Studies*, 2021, *34.*

Billings, Stephen B, Emily A Gallagher, and Lowell Ricketts, "Let the rich be flooded: the distribution of financial aid and distress after Hurricane Harvey," *Journal of Financial Economics*, 2022, *146* (2), 797–819.

Black, Sandra E and Philip E Strahan, "Entrepreneurship and bank credit availability," *The Journal of Finance*, 2002, *57* (6), 2807–2833.

Brown, J David and John S Earle, "Finance and growth at the firm level: Evidence from SBA loans," *The Journal of Finance*, 2017, *72* (3), 1039–1080.

Brown, James R, Matthew T Gustafson, and Ivan T Ivanov, "Weathering cash flow shocks," *The Journal of Finance*, 2021, *76* (4), 1731–1772.

Campello, Murillo, Gaurav Kankanhalli, and Hyunseob Kim, "Delayed creative destruction: How uncertainty shapes corporate assets," *Journal of Financial Economics*, 2024, *153*, 103786.

__ , John R Graham, and Campbell R Harvey, "The real effects of financial constraints: Evidence from a financial crisis," *Journal of Financial Economics*, 2010, *97* (3), 470–487.

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma, "Simple local polynomial density estimators," *Journal of the American Statistical Association*, 2020, *115* (531), 1449–1455.

__ , __ , and __ , "Local regression distribution estimators," *Journal of Econometrics*, 2021.

CBO, "Expected Costs of Damage From Hurricane Winds and Storm-Related Flooding," 2019. Congressional Budget Office, https://www.cbo.gov/system/files/2019-04/55019-ExpectedCostsFromWindStorm.pdf.

CEA, "The Economic Impact of the American Recovery and Reinvestment Act of 2009, Seventh Quarterly Report," *Council of Economic Advisors,*, 2011.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 2019, *134* (3), 1405–1454.

Chodorow-Reich, Gabriel, "The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis," *The Quarterly Journal of Economics*, 2014, *129* (1), 1–59.

Chow, Melissa C, Teresa C Fort, Christopher Goetz, Nathan Goldschlag, James Lawrence, Elisabeth Ruth Perlman, Martha Stinson, and T Kirk White, "Redesigning the longitudinal business database," Technical Report, National Bureau of Economic Research 2021.

CMHC, "Residential Rehabilitation Assistance Program," 2022. Canadian Mortgage and Housing Corporation, *https://www.cmhc-schl.gc.ca/en/professionals/project-funding-and-mortgage-financing/funding-programs/all-funding-programs/residential-rehabilitation-assistance-program*.

Collier, Benjamin and Cameron Ellis, "A Demand Curve for Disaster Recovery Loans," 2024. Forthcoming, *Econometrica*, https://www.econometricsociety.org/publications/econometrica/forthcoming-papers/0000/00/00/A-Demand-Curve-for-Disaster-Recovery-Loans/file/20417-3.pdf.

Collier, Benjamin L and Marc A Ragin, "The influence of sellers on contract choice: Evidence from flood insurance," *Journal of Risk and Insurance*, 2020, *87* (2), 523–557.

__ , Andrew F Haughwout, Howard C Kunreuther, and Erwann O Michel-Kerjan, "Firms' management of infrequent shocks," *Journal of Money, Credit and Banking*, 2020, *52* (6), 1329–1359.

__ , Cameron Ellis, and Benjamin J Keys, "The cost of consumer collateral: Evidence from bunching," Technical Report, National Bureau of Economic Research 2021.

Collier, Benjamin, Lawrence Powell, Marc A Ragin, and Xuesong You, "Financing Negative Shocks: Evidence from Hurricane Harvey," 2024. Forthcoming, *Journal of Financial and Quantitive Analysis*, https://jfqa.org/2024/02/03/financing-negative-shocks-evidence-from-hurricane-harvey/.

Cortés, Kristle Romero and Philip E Strahan, "Tracing out capital flows: How financially integrated banks respond to natural disasters," *Journal of Financial Economics*, 2017, *125* (1), 182–199.

Crowley, Chris, "Restaurants Are Shutting Down Until the Smoke Clears," *New York Magazine*, 2023.

Darmouni, Olivier, "Informational frictions and the credit crunch," *The Journal of Finance*, 2020, *75* (4), 2055–2094.

Davis, Steven J, John C Haltiwanger, Ron S Jarmin, Cornell J Krizan, Javier Miranda, Alfred Nucci, and Kristin Sandusky, "Measuring the dynamics of young and small businesses: Integrating the employer and nonemployer universes," 2007.

Davlasheridze, Meri and Pinar C Geylani, "Small Business vulnerability to floods and the effects of disaster loans," *Small Business Economics*, 2017, *49* (4), 865–888.

Deryugina, Tatyana and Benjamin M Marx, "Is the supply of charitable donations fixed? evidence from deadly tornadoes," *American Economic Review: Insights*, 2021, *3* (3), 383–398.

__ , Laura Kawano, and Steven Levitt, "The economic impact of Hurricane Katrina on its victims: Evidence from individual tax returns," *American Economic Journal: Applied Economics*, 2018, *10* (2), 202–233.

Dou, Winston Wei, Lucian A Taylor, Wei Wang, and Wenyu Wang, "Dissecting bankruptcy frictions," *Journal of Financial Economics*, 2021, *142* (3), 975–1000.

Fairlie, Robert, Alicia Robb, and David T Robinson, "Black and white: Access to capital among minority-owned start-ups," *Management Science*, 2022, *68* (4), 2377–2400.

Gallagher, Justin, Daniel A Hartley, and Shawn M Rohlin, "Weathering an Unexpected Financial Shock: The Role of Federal Disaster Assistance on Household Finance and Business Survival," *Journal of the Association of Environmental and Resource Economists*, 2023.

GAO, "Credit Reform: Current Method to Estimate Credit Subsidy Costs Is More Appropriate for Budget Estimates Than a Fair Value Approach," 2016. U.S. Government Accountability Office, GAO-16-41.

García, Jorge Luis and James J Heckman, "On criteria for evaluating social programs," Technical Report, National Bureau of Economic Research 2022.

Gennaioli, Nicola, Rafael La Porta, Florencio Lopez de Silanes, and Andrei Shleifer, "Trust and insurance contracts," *The Review of Financial Studies*, 2022, *35* (12), 5287–5333.

Giglio, Stefano, Matteo Maggiori, Krishna Rao, Johannes Stroebel, and Andreas Weber, "Climate change and long-run discount rates: Evidence from real estate," *The Review of Financial Studies*, 2021, *34* (8), 3527–3571.

Gilje, Erik P, Elena Loutskina, and Philip E Strahan, "Exporting liquidity: Branch banking and financial integration," *The Journal of Finance*, 2016, *71* (3), 1159–1184.

Giroud, Xavier, Holger M Mueller, Alex Stomper, and Arne Westerkamp, "Snow and leverage," *The Review of Financial Studies*, 2012, *25* (3), 680–710.

Goetz, Christopher, Zachary Kroff et al., "Recent Improvements to the Integrated Longitudinal Business Database (ILBD)," Technical Report, Center for Economic Studies, US Census Bureau 2021.

Government of the Netherlands, "Support and recovery package substantially expanded," 2020. *https://www.government.nl/latest/news/2021/01/21/support-and-recovery-package-substantially-expanded*.

gov.uk, "Recovery Loan Scheme," 2020. *https://www.gov.uk/guidance/recovery-loan-scheme*.

Granja, João, Christos Makridis, Constantine Yannelis, and Eric Zwick, "Did the paycheck protection program hit the target?," *Journal of financial economics*, 2022, *145* (3), 725–761.

Gulen, Huseyin and Mihai Ion, "Policy uncertainty and corporate investment," *The Review of financial studies*, 2016, *29* (3), 523–564.

Haltiwanger, John, "Entrepreneurship in the twenty-first century," *Small Business Economics*, 2022, pp. 1–14.

Hendren, Nathaniel and Ben Sprung-Keyser, "The Case for Using the MVPF in Empirical Welfare Analysis," Technical Report, National Bureau of Economic Research 2022.

Heritage Foundation, "Eliminate the Small Business Administration Disaster Loans Program," *Heritage Foundation The Budget Book*, 2023.

Hino, Miyuki, Christopher B Field, and Katharine J Mach, "Managed retreat as a response to natural hazard risk," *Nature*

Climate Change, 2017, *7* (5), 364–370.

Hoberg, Gerard and Vojislav Maksimovic, "Redefining financial constraints: A text-based analysis," *The Review of Financial Studies*, 2015, *28* (5), 1312–1352.

Hornbeck, Richard, "The enduring impact of the American Dust Bowl: Short-and long-run adjustments to environmental catastrophe," *American Economic Review*, 2012, *102* (4), 1477–1507.

— **and Suresh Naidu**, "When the levee breaks: Black migration and economic development in the American South," *American Economic Review*, 2014, *104* (3), 963–90.

Howell, Sabrina T, "Financing innovation: evidence from R&D grants," *American Economic Review*, 2017, *107* (4), 1136–64.

— , "Reducing information frictions in venture capital: The role of new venture competitions," *Journal of Financial Economics*, 2020, *136* (3), 676–694.

— **and J David Brown**, "Do cash windfalls affect wages? Evidence from R&D grants to small firms," *The Review of Financial Studies*, 2023, *36* (5), 1889–1929.

— **, Theresa Kuchler, David Snitkof, Johannes Stroebel, and Jun Wong**, "Lender automation and racial disparities in credit access," *The Journal of Finance*, 2024, *79* (2), 1457–1512.

Humphries, John Eric, Christopher A Neilson, and Gabriel Ulyssea, "Information frictions and access to the Paycheck Protection Program," *Journal of public economics*, 2020, *190*, 104244.

Ivanova, Irina, "Wildfire smoke impacts more than our health – it also costs workers over $100B a year. Here's why.," *CBS News*, 2023.

Japanese Finance Corporation, "Special Purpose Loans," 2022. *https://www.jfc.go.jp/n/english/sme/loans.html*.

Josephson, Anna and Maria I Marshall, "The demand for post-Katrina disaster aid: SBA disaster loans and small businesses in Mississippi," *Journal of Contingencies and Crisis Management*, 2016, *24* (4), 264–274.

Julio, Brandon and Youngsuk Yook, "Political uncertainty and corporate investment cycles," *The Journal of Finance*, 2012, *67* (1), 45–83.

Kaplan, Steven N and Luigi Zingales, "Do investment-cash flow sensitivities provide useful measures of financing constraints?," *The Quarterly Journal of Economics*, 1997, *112* (1), 169–215.

Kfw, "Kfw coronavirus aid," 2020. *https://www.kfw.de/About-KfW/Reporting-Portal/KfW-coronavirus-aid/*.

Kousky, Carolyn and Erwann Michel-Kerjan, "Examining flood insurance claims in the United States: Six key findings," *Journal of Risk and Insurance*, 2017, *84* (3), 819–850.

Leiter, Andrea M, Harald Oberhofer, and Paul A Raschky, "Creative disasters? Flooding effects on capital, labour and productivity within European firms," *Environmental and Resource Economics*, 2009, *43* (3), 333–350.

Lelarge, Claire, David Sraer, and David Thesmar, "Entrepreneurship and credit constraints: Evidence from a French loan guarantee program," in "International differences in entrepreneurship," University of Chicago Press, 2010, pp. 243–273.

Lucas, Deborah, "Valuation of government policies and projects," *Annu. Rev. Financ. Econ.*, 2012, *4* (1), 39–58.

MEF, "Liquidity support for households, businesses and local authorities," 2020. Ministero dell'Economia e delle Finance (MEF), *https://www.mef.gov.it/en/covid-19/Liquidity-support-for-households-businesses-and-local-authorities/*.

Miao, Qing and David Popp, "Necessity as the mother of invention: Innovative responses to natural disasters," *Journal of Environmental Economics and Management*, 2014, *68* (2), 280–295.

Midrigan, Virgiliu and Daniel Yi Xu, "Finance and misallocation: Evidence from plant-level data," *American economic review*, 2014, *104* (2), 422–458.

Muhlhausen, David, "Business Disaster Reform Act of 2013: Review of Impact and Effectiveness," *Testimony Before the Committee on Small Business and Entrepreneurship, United States Senate on March 14, 2013*, 2013.

Mulder, Philip, "Mismeasuring risk: The welfare effects of flood risk information," Technical Report, Working Paper 2021.

Mullins, William and Patricio Toro, "Credit guarantees and new bank relationships," *Policy Research Working Paper No*, 2017, *8241*.

Nanda, Ramana and Matthew Rhodes-Kropf, "Financing entrepreneurial experimentation," *Innovation Policy and the Economy*, 2016, *16* (1), 1–23.

NOAA, "U.S. Billion-Dollar Weather and Climate Disasters," 2023. U.S. National Oceanic and Atmospheric Administration, DOI: 10.25921/stkw-7w73.

NSBA, "2016 Year-End Economic Report," Technical Report, National Small Business Association 2016.

NSW, "Disaster Recovery Loans," 2022. New South Wales Government, *https://www.raa.nsw.gov.au/disaster-assistance/disaster-recovery-loans*.

OMB, "Credit Supplement, Budget of the U.S. Government, fiscal year 2024," 2023. Office of Management and Budget, https://www.whitehouse.gov/wp-content/uploads/2023/03/cr_supp_fy2024.pdf.

Pan, Guangqian, Zheyao Pan, and Kairong Xiao, "The shadow cost of collateral," *Available at SSRN 3984585*, 2023.

Petersen, Mitchell A and Raghuram G Rajan, "The benefits of lending relationships: Evidence from small business data," *The journal of finance*, 1994, *49* (1), 3–37.

Poterba, James M, "Government intervention in the markets for education and health care: how and why?," in "Individual and social responsibility: Child care, education, medical care, and long-term care in America," University of Chicago Press, 1996, pp. 277–308.

Public Safety Canada, "Disaster Assistance Programs," 2022. *https://www.publicsafety.gc.ca/cnt/mrgnc-mngmnt/rcvr-dsstrs/dsstr-ssstnc-prgrms/dsstr-ssstnc-prgrms-ll-en.aspx*.

Rampini, Adriano A, "Financing durable assets," *American Economic Review*, 2019, *109* (2), 664–701.

— **and S Viswanathan**, "Collateral, risk management, and the distribution of debt capacity," *The Journal of Finance*, 2010, *65* (6), 2293–2322.

— **and** — , "Collateral and secured debt," *Unpublished working paper, Duke University*, 2020.

Rauh, Joshua D, "Investment and financing constraints: Evidence from the funding of corporate pension plans," *the Journal of Finance*, 2006, *61* (1), 33–71.

Rice, Tara and Philip E Strahan, "Does credit competition affect small-firm finance?," *The Journal of Finance*, 2010, *65* (3), 861–889.

Sastry, Parinitha, "Who bears flood risk? evidence from mortgage markets in florida," *Evidence from Mortgage Markets in Florida (December 18, 2022)*, 2022.

SBA, "SBA 2014 Small Business Profile," Technical Report, Small Business Administration 2014.

— , "Disaster Loan Servicing and Liquidation," 2015. Small Business Administration, SOP 50 52 2, https://www.sba.gov/sites/default/files/files/SOP_50_52_2_1.pdf.

— , "Agency Financial Report, Fiscal Year 2017," 2017. Small Business Administration, https://www.sba.gov/sites/default/files/2018-06/SBA_FY_2017_AFR_.pdf.

SBA, "Standard Operating Procedure, Disaster Assistance Program," 2018. Small Business Administration, Office of Disaster Assistance, 31 May 2018.

SBA, "SBA 2020 Small Business Profile," Technical Report, Small Business Administration 2020.

SBA, "COVID-19 Economic Injury Disaster Loan," 2022. Small Business Administration, *https://www.sba.gov/funding-programs/loans/covid-19-relief-options/eidl*.

Schmalz, Martin C, David A Sraer, and David Thesmar, "Housing collateral and entrepreneurship," *The Journal of Finance*, 2017, *72* (1), 99–132.

Simon, Ruth and Cameron McWhirter, "Harvey's test: businesses struggle with flawed insurance as floods multiply," *Wall Street Journal*, 2017.

Thornton, Alexandra, "How Congress Can Help Small Businesses Weather the Coronavirus Pandemic," *Center for American Progress*, 2020.

U.S. Congress, "Debt Collection Improvement Act of 1996," 1996. Public Law 104-134, https://www.fiscal.treasury.gov/files/dms/dmdcia.pdf.

Weitzman, Martin L, "Fat tails and the social cost of carbon," *American Economic Review*, 2014, *104* (5), 544–546.

Table 1: Summary Statistics of SBA Applicants

Panel A: Applicant Information (Counts)

Applications	167,202
Borrowers	54,532
Total Employees of Borrowers	236,570
Amount Disbursed ($)	5,330,940

Panel B: Approval and Borrowing Frequencies (%)

P(Approved | Applied)	43.9
P(Borrowed | Applied)	32.6

Panel C: Applications by Disaster Type (%)

Hurricane	73.0
Storm / Flood	14.3
Tornado	7.3
Fire	1.4
Drought	1.0
BP Oil Spill	0.9
Other	2.2

Panel D: Applications by Organizational Type (%)

Sole Proprietor/Individual	59
Corporation	23
LLC, LLP, Or LLE	12
NonProfit Organization	4
Partnership	1
Other	1

Notes: All dollar amounts are in thousands of $2018. Panel A describes the number of applicants and dollars disbursed. The remaining panels describe respectively the share of applicants by lending and borrowing decisions, disaster type, and organizational type.

Table 2: Summary Statistics for SBA Applicants by Approval Status

Panel A: All Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	11.8	13.0	1.0	8.0	28.0	137,581
Employees	4.7	11.1	0.0	1.0	10.0	120,403
FICO Score	655.2	101.3	513.0	662.0	790.0	133,273
Loss Amount ($, 000s)	156.4	833.9	6.9	46.8	308.4	102,995

Panel B: Declined Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	10.4	12.1	1.0	6.0	26.0	75,936
Employees	3.8	10.0	0.0	1.0	8.0	65,302
FICO Score	608.8	97.7	489.0	599.0	754.0	74,674
Loss Amount ($, 000s)	124.6	855.3	4.8	33.8	236.3	42,577

Panel C: Approved Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	13.7	13.7	2.0	9.0	31.0	61,645
Employees	5.7	12.1	0.0	2.0	13.0	55,101
FICO Score	714.4	70.1	620.0	719.0	802.0	58,599
Loss Amount ($, 000s)	178.9	817.8	9.2	58.1	355.9	60,418
Insurance Payments ($, 000s)	28.3	116.0	0.0	0.0	57.7	60,055
Loan Amount ($, 000s)	72.7	209.4	0.0	18.4	179.9	73,361
Interest Rate	3.8	0.7	2.9	4.0	4.0	73,361
Maturity (Years)	18.9	10.1	5.0	16.8	30.0	73,361
Monthly Payments ($, 000s)	0.7	2.3	0.1	0.3	1.4	73,361

Notes: All dollar amounts are in thousands of $2018. Firm age is in years.

Table 3: Sample Characteristics and Census Outcome Summary Statistics

Panel A: Sample Characteristics (Census-Matched Sample)

	Mean (1)	Std. Dev (2)	Quasimedian (3)	Observations (4)
Post Disaster	0.51			291,000
Approved	0.34			291,000
Post Disaster x Approved	0.17			291,000
Firm Age	11.3	29.3	7.00	291,000
Black Population Share	0.23			291,000

Panel B: Sample Characteristics (Experian-Matched Sample)

	Mean (1)	Std. Dev (2)	Median (3)	Observations (4)
Post Disaster	0.62	0.49	1.00	262,847
Approved	0.39	0.49	0.00	262,847
Post Disaster x Approved	0.24	0.43	0.00	262,847
Firm Age	11.4	60.4	7.00	255,710
Black Population Share	0.22	0.26	0.10	239,634

Panel C: Real Outcomes (Census-Matched Sample)

	Mean (1)	Std. Dev (2)	Quasimedian (3)	Observations (4)
Exit	0.12			291,000
Deformalization	0.06			170,000
Number of Employees	2.99	4.37	1.00	170,000
Revenue ($, 000s)	351	592	72.9	128,000

Note: "Exit" is a binary variable equal to 1 after a firm has permanently exited from the sample. "Deformalization" marks a transition from an employer firm to a non-employer firm. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008.

Table 4: Summary Statistics for Experian Financial Outcomes

Panel A: Private Credit Outcomes (Experian Sample)

	Mean (1)	Std. Dev. (2)	Median (3)	Observations (4)
Outstanding Debt ($, 000s)	7.93	20.9	0.00	142,199
Outstanding Debt Paid On Time ($, 000s)	6.77	18.68	0.00	142,199
Number of Contracts	0.49	0.82	0.00	142,199
Delinquent Share of Debt	0.13	0.32	0.00	53,959
Days Late on all Contracts	16.7	34.5	0.00	53,959
Bankruptcy	0.028	0.16	0.00	142,876

Panel B: Private Credit Outcomes (Experian Sample, Conditional on Matching to Census)

	Mean (1)	Std. Dev. (2)	Quasimedian (3)	Observations (4)
Outstanding Debt ($, 000s)	10.00	24.50	0.00	62,000
Outstanding Debt Paid On Time ($, 000s)	9.10	22.00	0.00	62,000
Number of Contracts	0.56	0.91	0.00	62,000
Delinquent Share of Debt	0.08	0.26	0.00	28,000
Days Late on all Contracts	5.08	19.6	0.00	62,000
Bankruptcy	0.01			62,000

Note: Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008.

Table 5: Effect of Disaster Loans on Real Outcomes (2SLS)

Panel A: Real Outcomes (Census Sample, All Firms)

Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.132***	-0.0515*	0.170**	0.196
	(0.0413)	(0.0281)	(0.0831)	(0.143)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.115	0.059	1.042	4.384
Observations	291,000	170,000	170,000	128,000
KP F-Stat	157.2	81.3	81.3	115.5

Panel B: Real Outcomes among Employers (Census Sample, Employer Firms Only)

Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.169***	-0.121**	0.376**	0.719***
	(0.0641)	(0.0503)	(0.183)	(0.254)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.084	0.056	1.413	6.025
Observations	107,000	85,000	85,000	51,000
KP F-Stat	45.28	27.96	27.96	27.56

Note: This table contains estimates of Equation 2. "Exit" is a binary variable equal to 1 after a firm has permanently exited from the sample. "Deformalization" marks a permanent transition from an employer firm to a non-employer firm. Panel B is restricted to firms with employees in the year prior to the disaster. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Effect of Disaster Loans on Adverse Financial Outcomes (2SLS)

Panel A: Credit Outcomes (Experian Sample)

Dependent Variable:	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)
Post Disaster x Approved (IV)	-0.339***	-34.996***	-0.038*
	(0.086)	(9.676)	(0.022)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	0.134	16.675	0.028
Observations	53,959	53,959	142,876
KP F-Stat	14.835	14.835	61.142

Panel B: Survivor Credit Outcomes
(Experian Sample Conditional on Matching to Census and Never Exiting as of 2019)

Dependent Variable:	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)
Post Disaster x Approved (IV)	-0.165	-7.082	-0.00994
	(0.111)	(4.459)	(0.0222)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	0.083	5.085	0.014
Observations	28,000	62,000	62,000
KP F-Stat	13.60	47.01	47.01

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Effect of Disaster Loans on Form of Firm Exit (2SLS)

Panel A: Exit Outcomes (Experian Sample Matched to Census)

	Form of Exit			
Dependent Variable:	Overall	Bankruptcy	Delinquency	Other
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.145***	-0.0178	-0.0150	-0.110**
	(0.0544)	(0.0128)	(0.0246)	(0.0468)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.149	0.007	0.030	0.115
Observations	112,000	112,000	112,000	112,000
KP F-Stat	67.25	67.25	67.25	67.25

Panel B: Employer Exit Outcomes (Experian Sample Matched to Census, Employer Firms Only)

	Form of Exit			
Dependent Variable:	Overall	Bankruptcy	Delinquency	Other
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.0969*	-0.0407**	0.00632	-0.0725
	(0.0562)	(0.0182)	(0.0365)	(0.0445)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.112	0.009	0.032	0.074
Observations	60,000	60,000	60,000	60,000
KP F-Stat	39.75	39.75	39.75	39.75

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Effect of Disaster Loans on Private Credit (2SLS)

Panel A: Credit Outcomes (Experian Sample)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts
	(1)	(2)	(3)
Post Disaster x Approved (IV)	17.712***	16.369***	0.568***
	(3.432)	(3.350)	(0.089)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	7.934	6.769	0.488
Observations	142,199	142,199	142,199
KP F-Stat	61.46	61.46	61.46

Panel B: Survivor Credit Outcomes
(Experian Sample, Conditional on Match to Census and Never Exiting as of 2019)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts
	(1)	(2)	(3)
Post Disaster x Approved (IV)	17.220***	15.860***	0.523***
	(5.366)	(5.126)	(0.154)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	10.00	9.10	0.556
Observations	62,000	62,000	62,000
KP F-Stat	47.01	47.01	47.01

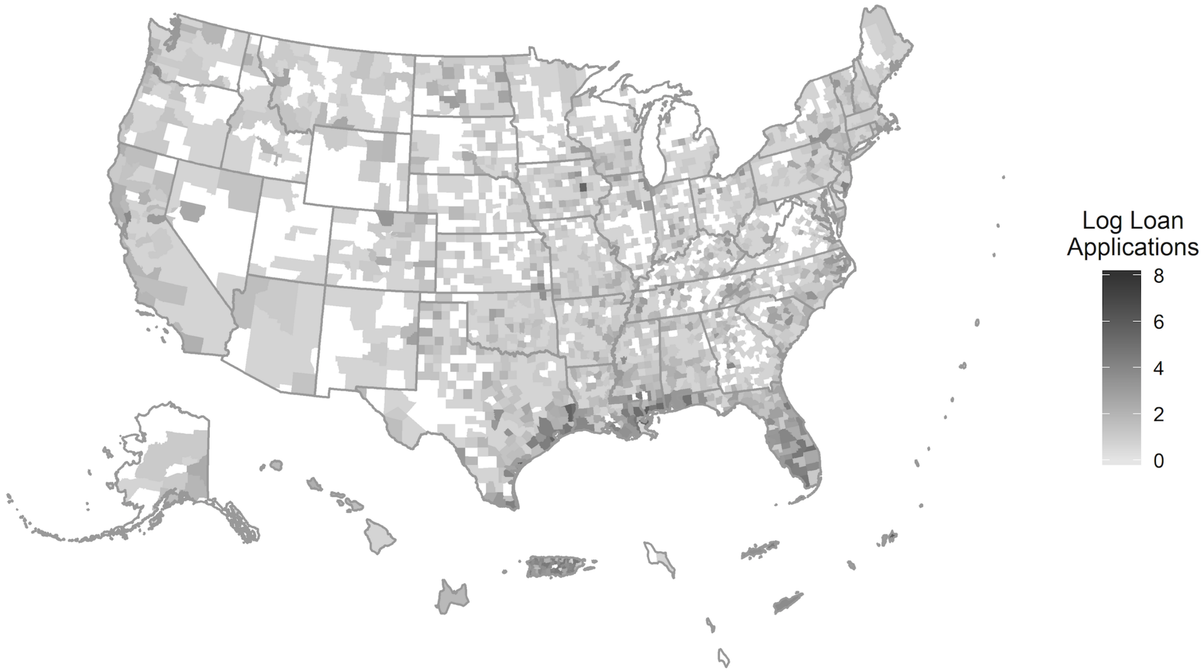
Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. "Outstanding Private Debt" and "Outstanding Private Debt, Paid on Time" are total private credit balances (and do not include the SBA loan). They are in thousands of $2018."Number of Contracts" is a holistic measure of the firm's time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Program Costs by Year

Fiscal Year	Total Loans ($, mill)	Costs ($, mill) Administrative	Subsidy	MC of Taxation	Total	Total Cost per $1 Loaned
2006	1,577	217	333	165	716	0.45
2007	251	55	49	31	135	0.54
2008	202	40	27	20	87	0.43
2009	220	56	17	22	95	0.43
2010	157	30	13	13	55	0.35
2011	134	26	6	10	42	0.31
2012	122	25	2	8	36	0.29
2013	308	32	19	15	66	0.22
2014	59	29	5	10	43	0.74
2015	50	16	3	6	25	0.50
2016	142	21	12	10	42	0.30
2017	197	21	26	14	61	0.31

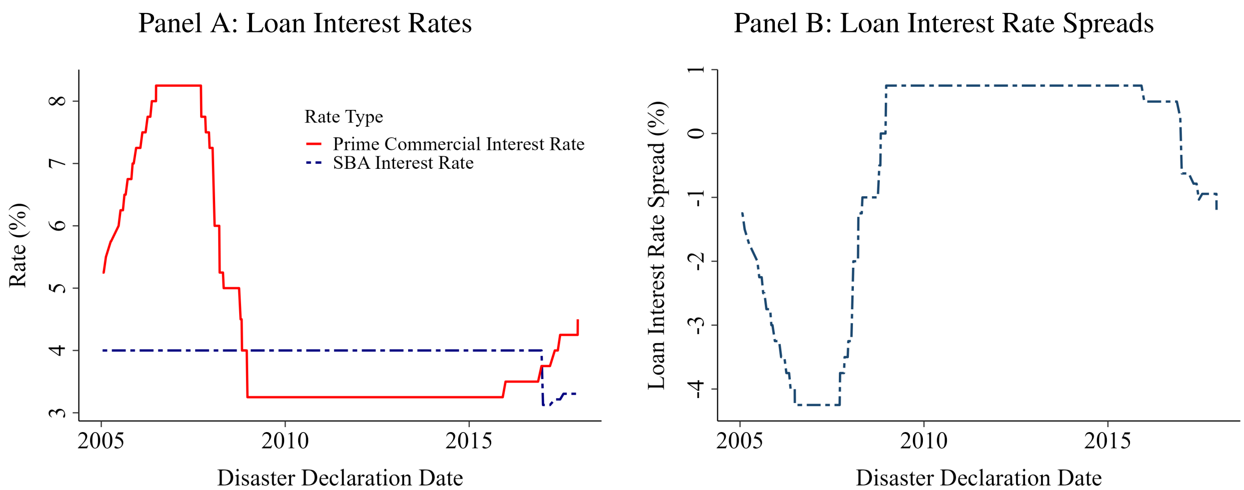
Note: This table shows the costs of recovery loans by fiscal year. The SBA reports administrative and subsidy costs annually (e.g., SBA, 2017). We incorporate an additional cost, calculated as 30% of administrative and subsidy costs, to account for the deadweight loss of funding government programs through taxes, following Poterba (1996).

Figure 1: Geographic Dispersion of Applications



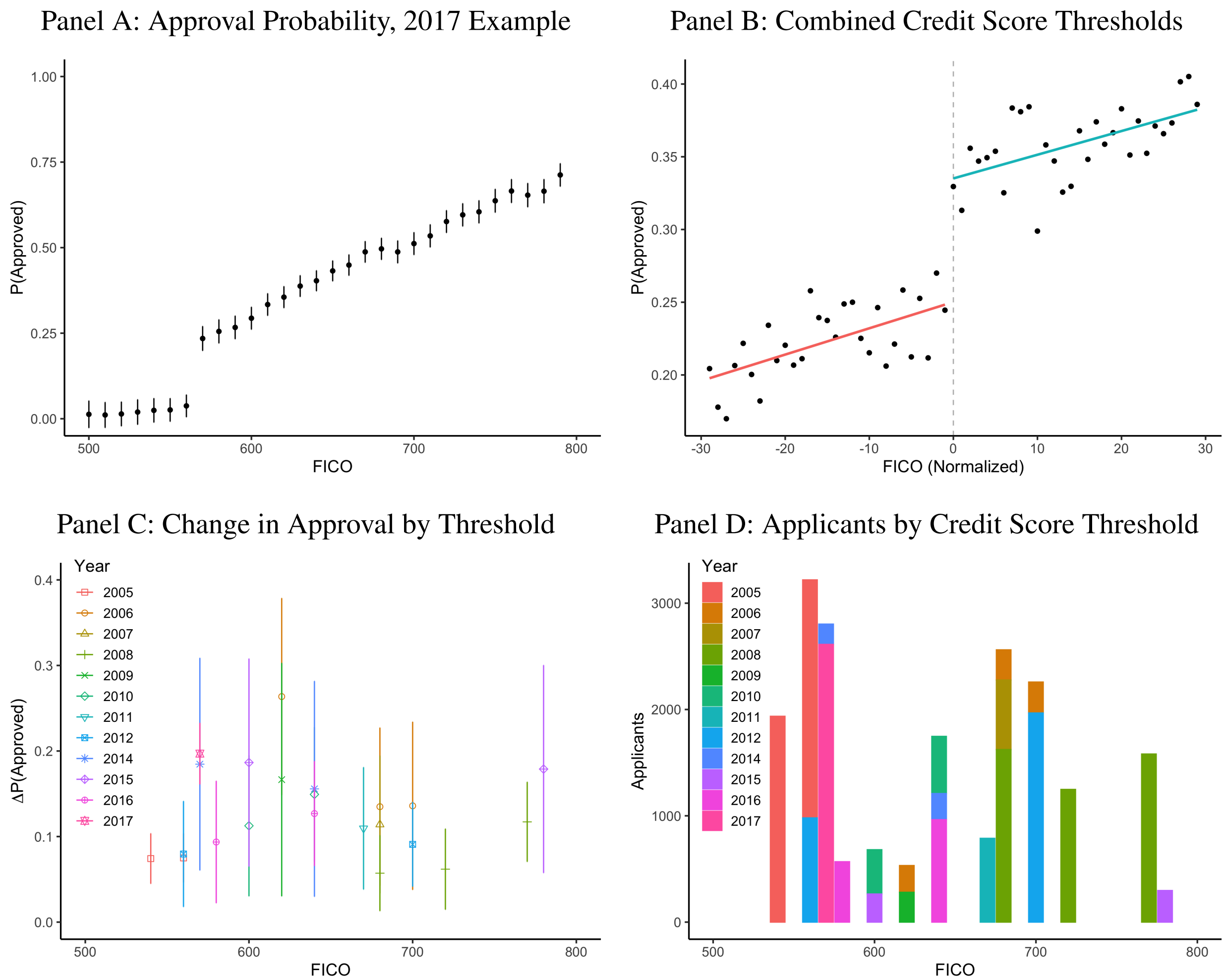
Note: This map shows the log number of applications by county for the U.S. and the territories.

Figure 2: Interest Rates



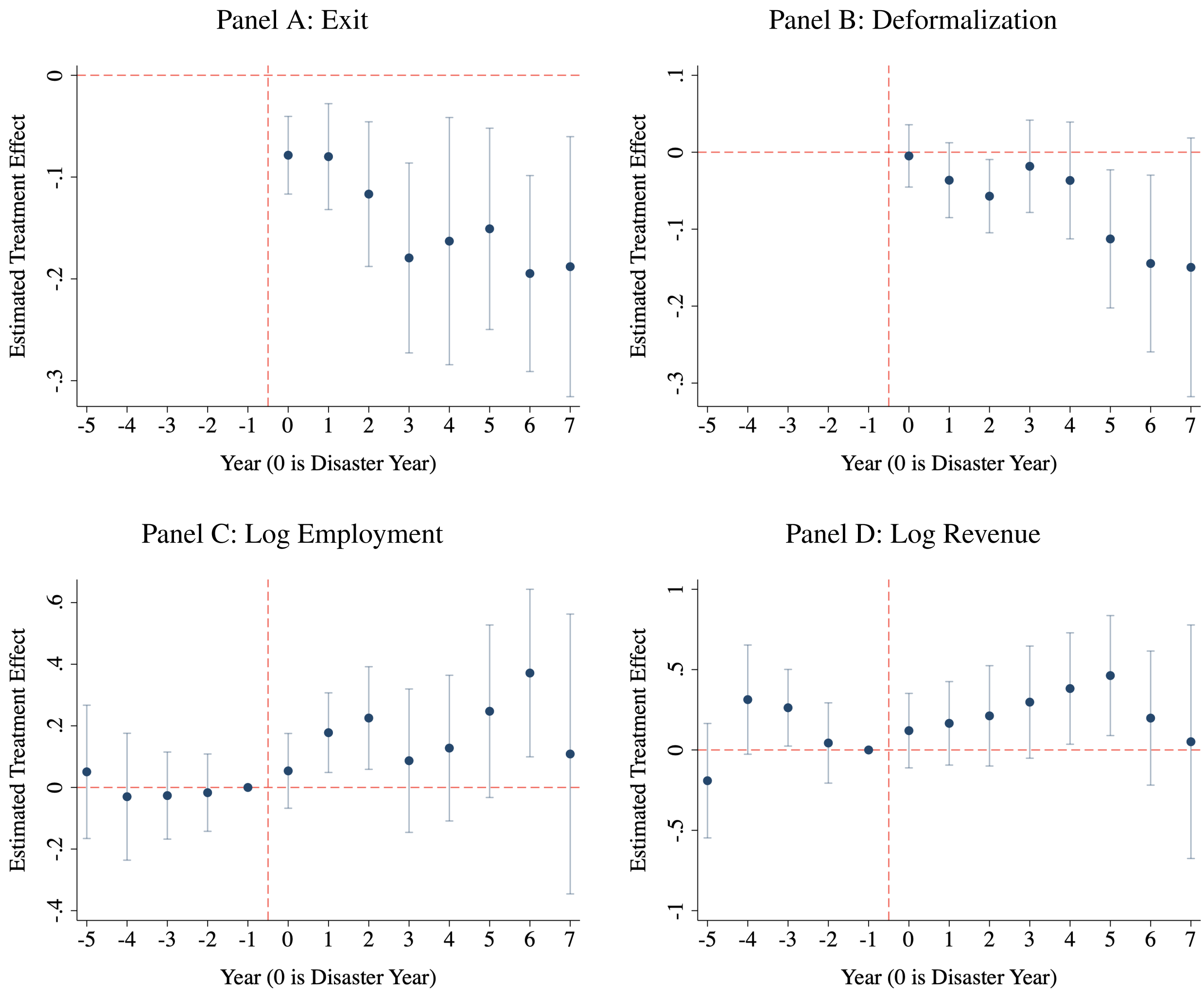
Note: Panel A plots the interest rate SBA assigned to applications and prime commercial property loan interest rate at the same date. We dropped 3% of SBA loans that received a higher market-based interest rate. Panel B plots the spread of loan interest rates, that is, the difference between the SBA interest rate and Prime Commercial interest rate.

Figure 3: Loan Approval and Credit Score



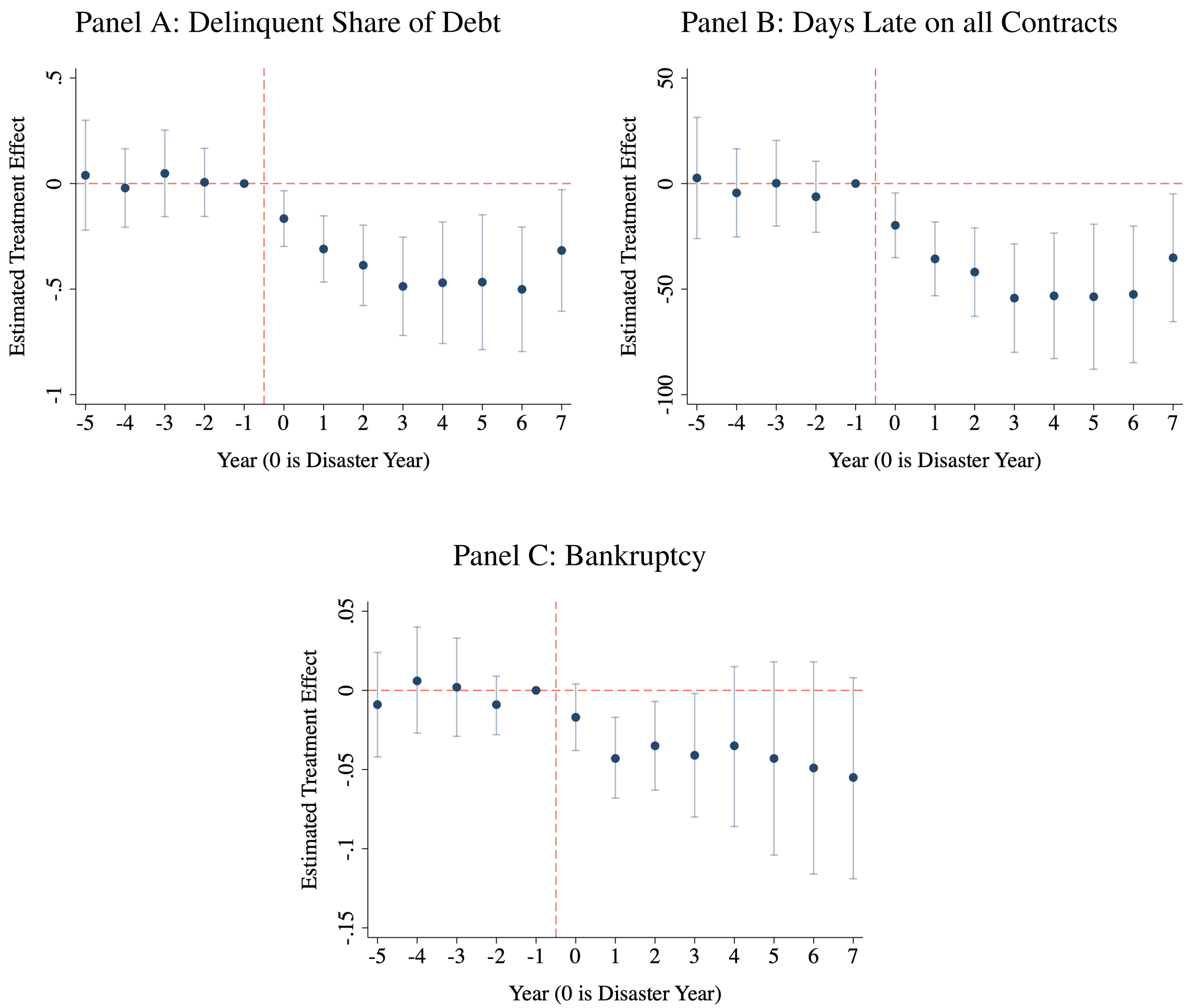
Note: The figure shows the relationship between credit score and approval probability. Panel (a) is an illustration, showing that having a FICO score of at least 570 is an important predictor of approval for applicants who experienced a disaster in 2017. The plot reports the coefficients from a regression of whether an applicant was approved on binned FICO scores. Figure B3 in the Online Appendix shows a similar plot for each year. Panel (b) combines data from 22 identified credit score thresholds between 2005 and 2018, normalizing FICO scores so that FICO = 0 at the threshold. Panel (c) shows the 22 credit score discontinuities used in the analyses. The vertical axis reports the change in approval probability at the threshold. Panel (d) shows the number of applicants by credit score discontinuity.

Figure 4: Effect of Disaster Loans on Real Outcomes (2SLS)



Note: These figures contain estimates of Equation 3. "Exit" is a binary variable equal to 1 after a firm has permanently exited from the sample. "Deformalization" marks a permanent transition from an employer firm to a non-employer firm. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this figure were disclosed under DRB CBDRB-FY23-CED006-0008.

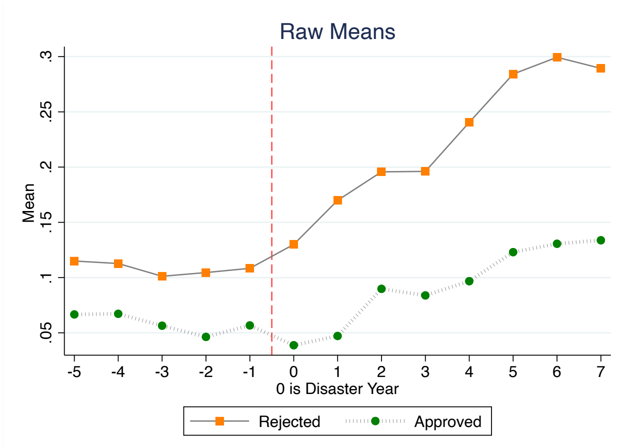
Figure 5: Event Study Effect of Disaster Loans on Adverse Financial Outcomes (2SLS)



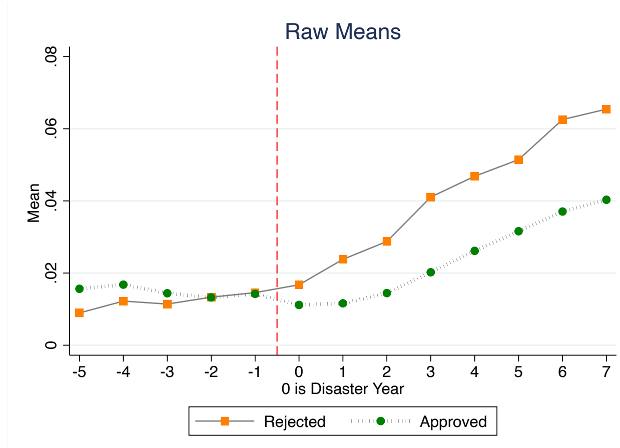
Note: These figures contain estimates of Equation 3. Experian variables are drawn from a June 30 snapshot. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID.

Figure 6: Raw Means For Financial Outcomes

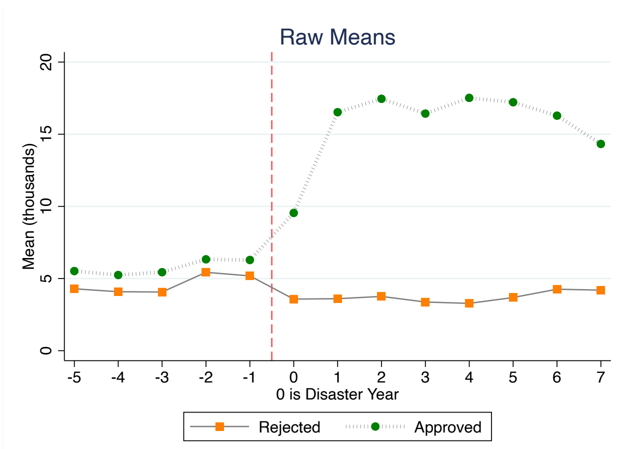
Panel A: Delinquent Share of Debt



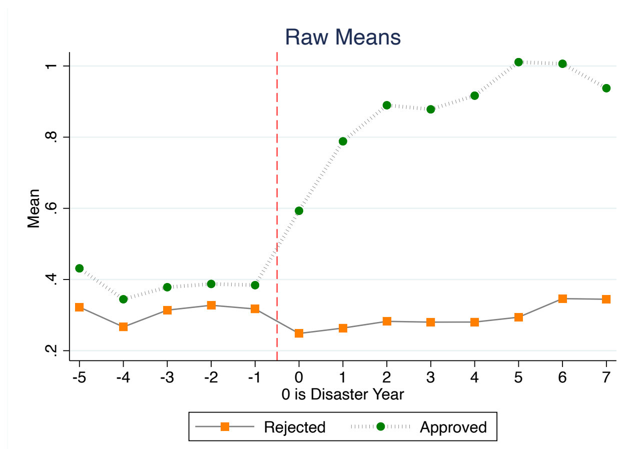
Panel B: Bankruptcy Filings



Panel C: Outstanding Debt

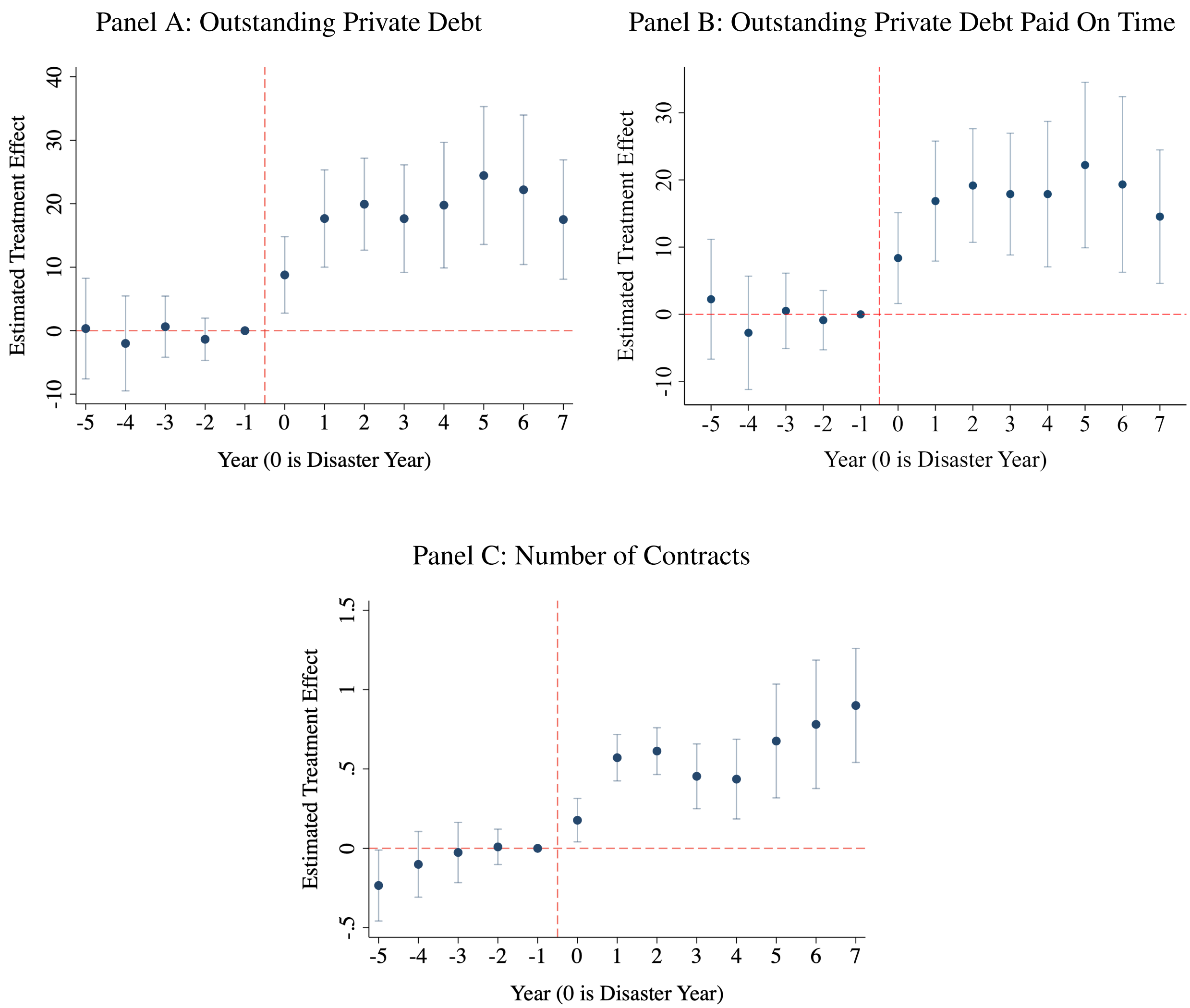


Panel D: Number of Contracts



Note: These figures show average values for approved and rejected applicants by year. Experian variables are drawn from a June 30 snapshot. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days. Bankruptcy includes any type of bankruptcy filing. “Outstanding Private Debt” is total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. We are not permitted to disclose the raw means for the real outcomes studied in the Census Bureau data.

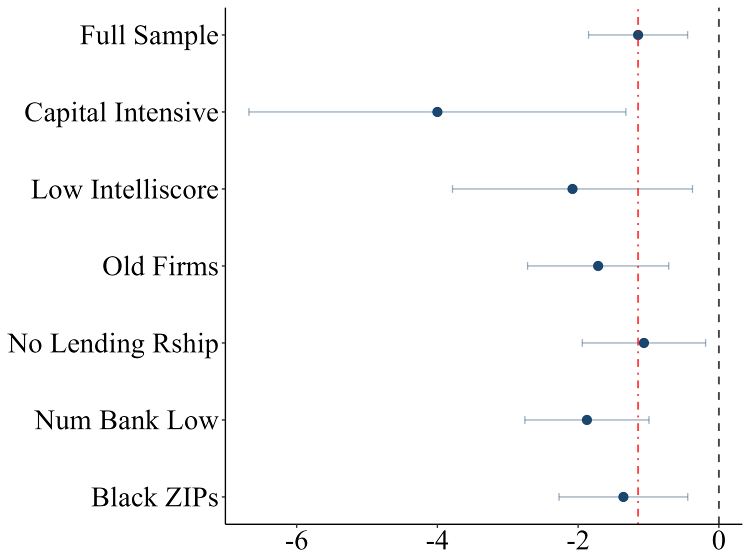
Figure 7: Event Study Effect of Disaster Loans on Private Credit (2SLS)



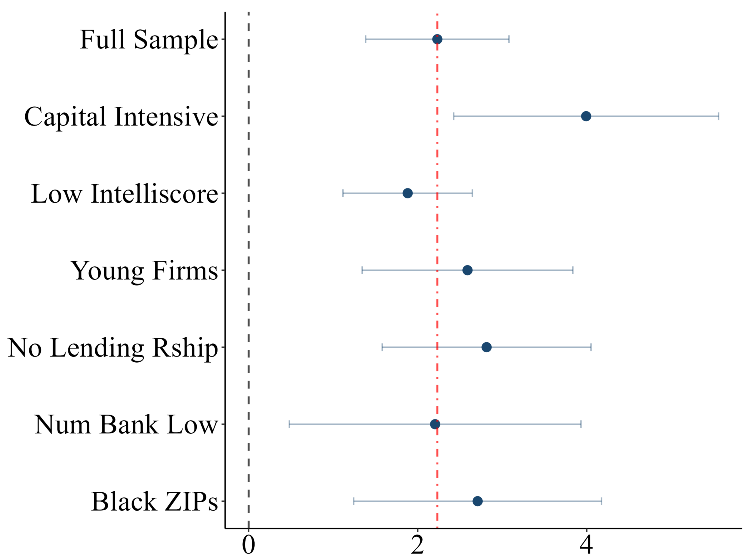
Note: These figures contain estimates of Equation 3. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time’ are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.

Figure 8: Outcomes by Subsample

Panel A: Exit



Panel B: Outstanding Private Debt



Note: The figure shows the effects of recovery loans on exit and private debt balances in subsample analyses. The reported effects divide the estimated treatment effect by the mean for each subsample. The dotted red line indicates the effect for the full regression sample for comparison. Appendix D reports the full results.

APPENDICES FOR ONLINE PUBLICATION

Appendix A Small Business Administration Underwriting Documentation

Figure A1: 2018 Memo on Loan Approval and Credit Score Targets

U.S. SMALL BUSINESS ADMINISTRATION
WASHINGTON, D.C. 20416

Date: June 14, 2018

To: All Office Directors
All ODA HQ Staff

From: Associate Administrator
for Disaster Assistance

Subject: #18-17, SOP 50 30 9 Credit Model

This memo addresses the changes to the current Credit Model as a result of SOP 50 30 9. The Credit Model is effective May 31, 2018.

Credit Score Range	MAFD%	Income	Result
1-569	N/A		Auto Decline 28 (loan type H, SP)*
1-669	> 75%		Auto Decline 21 (Repayment: H)
≥ 670 **(Strong Credit)**	N/A	≥ $50,000	Non-Exception Processing
0, Null, ≥ 570	≤ 75%	> Minimum Income**	Non-Exception Processing
All other scenarios default to Exception Processing which must be addressed by loan officer.			

* H= Home SP= Sole Proprietorship 100% Owner **e.g. Appendix 4

DCMS 2.0 (original and reacceptances) will filter the files to the appropriate Result Queue based on the chart above. All approved loans are required to have a minimum payment amount of $50.

All loan recommendations requiring the applicant to carry more than 75 percent MAFD (excluding those with strong credit and income of $50,000 or greater) must be adequately justified as a part of the approval process and only the Assistant Center Director for Application Processing or higher has final approval authority.

Further clarification has been added to Paragraph 6.5 F as follows:

2. When a recommended loan requires the applicant to carry more than 75 percent of MAFD it must be justified in the file prior to final signoff by ACDAP or higher (excluding those with strong credit and income of $50,000 or greater).

Reconsiderations and Appeals will require a full eligibility, credit, and repayment analysis performed based on the criteria in the Credit Model noted above. Application Processing training will provide complete details on processing reconsiderations and appeals.

for JWR

James E. Rivera

Federal Recycling Program Printed on Recycled Paper

Note: This Figure shows a revision to the program's credit score targets in 2018, recommending "auto decline" for applicants with credit scores below 570 and classifying applicants as having "strong credit" if their scores are at least 670.

Figure A2: 2021 Memo on Loan Approval and Credit Score Targets

U.S. SMALL BUSINESS ADMINISTRATION
WASHINGTON, D.C. 20416

Date: September 23, 2021

To: All Center Directors
All ODA HQ Staff

From: Associate Administrator
for Disaster Assistance

Subject: #21-32, Policy Changes: Applicant Contact and Strong Credit

This memo addresses changes to SBA policy that will enable SBA to provide much needed emergency assistance in an expedited manner to small businesses and residences experiencing hardship as a direct result of disaster events.

Effective immediately, SOP 50 30 9 Paragraph 4.3 C 6 (a) will be modified to remove the requirement to reach out to the applicant/borrowers to obtain insurance recovery information prior to loan approval. SBA's accounts department will provide a blank Assignment of Insurance Proceeds (AIP) form and the applicant/borrower will submit the documentation to the insurance provider(s) to complete. The applicant/borrower will return the completed form to SBA with loan closing documents.

Effective immediately, SOP 50 30 9 Paragraph 7.4 will be modified to remove the requirement to reach out to the applicant/borrower to discuss the terms and conditions of a loan approval recommendations. Loan officers must continue to reach out to applicant/borrower to discuss decline and withdrawal recommendations. Applicant/borrowers should be notified by the case manager of the applicable loan terms and conditions.

Effective immediately, SOP 50 30 9 Paragraph 3.31 C 5 (e) and (f) will be modified to increase the maximum Phase I eligible loan amount from $300,000 to $500,000. Additionally, the number of months used to calculate Phase I will be increased from 4 months to 6 months.

Finally, effective immediately, SBA's determination of "strong credit" will be modified from 670 to 625.

This policy will remain in effect until December 31, 2021. SBA will evaluate these changes by January 31, 2022, to determine overall effectiveness of each policy change.

If you have any questions, please contact Program Policy & Evaluation.

James E. Rivera

Note: This Figure shows a change to the program's credit score targets in 2021, revising "strong credit" so that it includes applicants with scores of at least 625.

Appendix B Credit Score Discontinuity, Supplementary Material

Figure B1 plots a McCrary sorting-style test for manipulation of the running variable (Cattaneo et al., 2020, 2021). A density discontinuity test results in a t-statistic of -1.58 (p = 0.11).

Figure B2 examines the continuity of Experian outcomes and firm characteristics through the threshold.

Table B1 shows regression estimates of how the threshold affects approval likelihood in models controlling for the running variable and fixed effects for ZIP code and disaster year by FICO threshold.

Table B2 reports the FICO thresholds used in the analyses. In a couple of cases, two thresholds are nearby in the same year (e.g., FICO 540 and 560 in 2005), resulting in around 1% of observations that could be assigned to either threshold. In these cases, we associate the firm with the closest threshold.

Figure B3 reports the results of regressing the likelihood of approval on FICO score for each year. The panels show the change in approval likelihood relative to a reference group of applicants with FICO scores of at least 800.

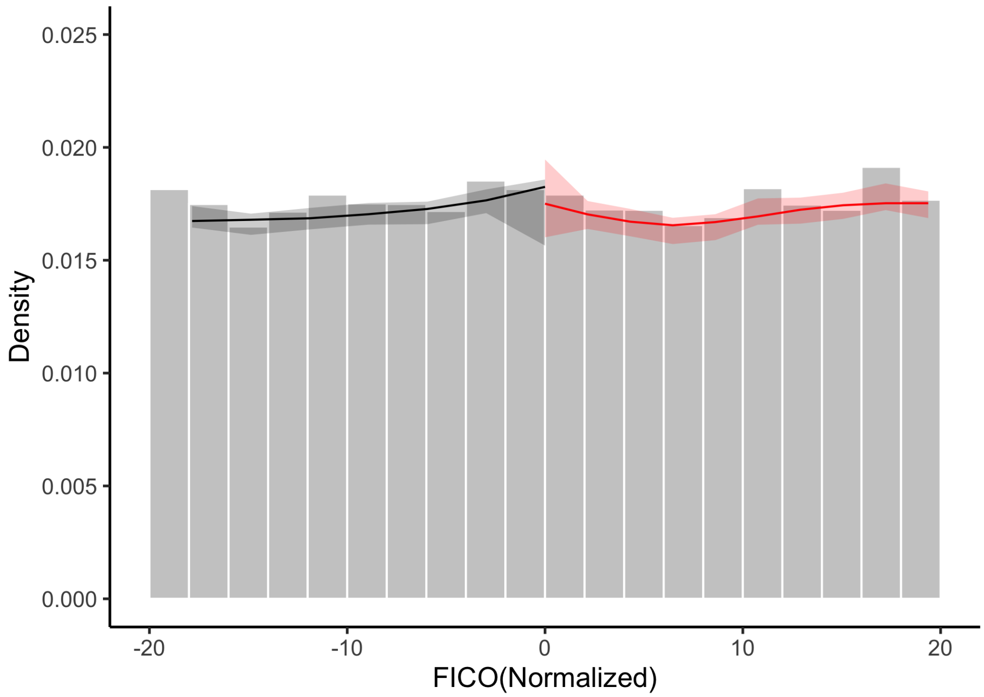
Table B3 provides summary statistics for firms in the analysis sample, which includes all applicants whose owners have a credit score within 29 points of a credit score discontinuity. This sample is qualitatively similar to the full sample of applicants (Table 2), though slightly younger (with an average age of 10.9 years vs. 11.8) and larger (6.5 employees vs. 4.7).

Table B1: Regression of Approval Likelihood on Threshold, Bandwidth 29

Dependent Variable: $\mathbb{1}(Approved)$

	(1)	(2)	(3)	(4)
$\mathbb{1}(FICO \geq 0)$	0.149***	0.096***	0.128***	0.120***
	(0.017)	(0.016)	(0.018)	(0.021)
$\mathbb{1}(FICO < 0) \times FICO$		0.002***	-0.005*	-0.002
		(0.001)	(0.003)	(0.002)
$\mathbb{1}(FICO < 0) \times FICO^2$			-0.000***	-0.000
			(0.000)	(0.000)
$\mathbb{1}(FICO \geq 0) \times FICO$		0.002***	0.002	-0.001
		(0.001)	(0.002)	(0.002)
$\mathbb{1}(FICO \geq 0) \times FICO^2$			0.000	0.000**
			(0.000)	(0.000)
Disaster Year $\times$ Threshold FE	No	No	No	Yes
Zip FE	No	No	No	Yes
Clustered SE	Disaster ID	Disaster ID	Disaster ID	Disaster ID
R^2	0.023	0.024	0.025	0.303
Within R^2	0.023	0.024	0.025	0.019
N	20,219	20,219	20,219	20,219

Figure B1: Density Plot for Manipulation Testing

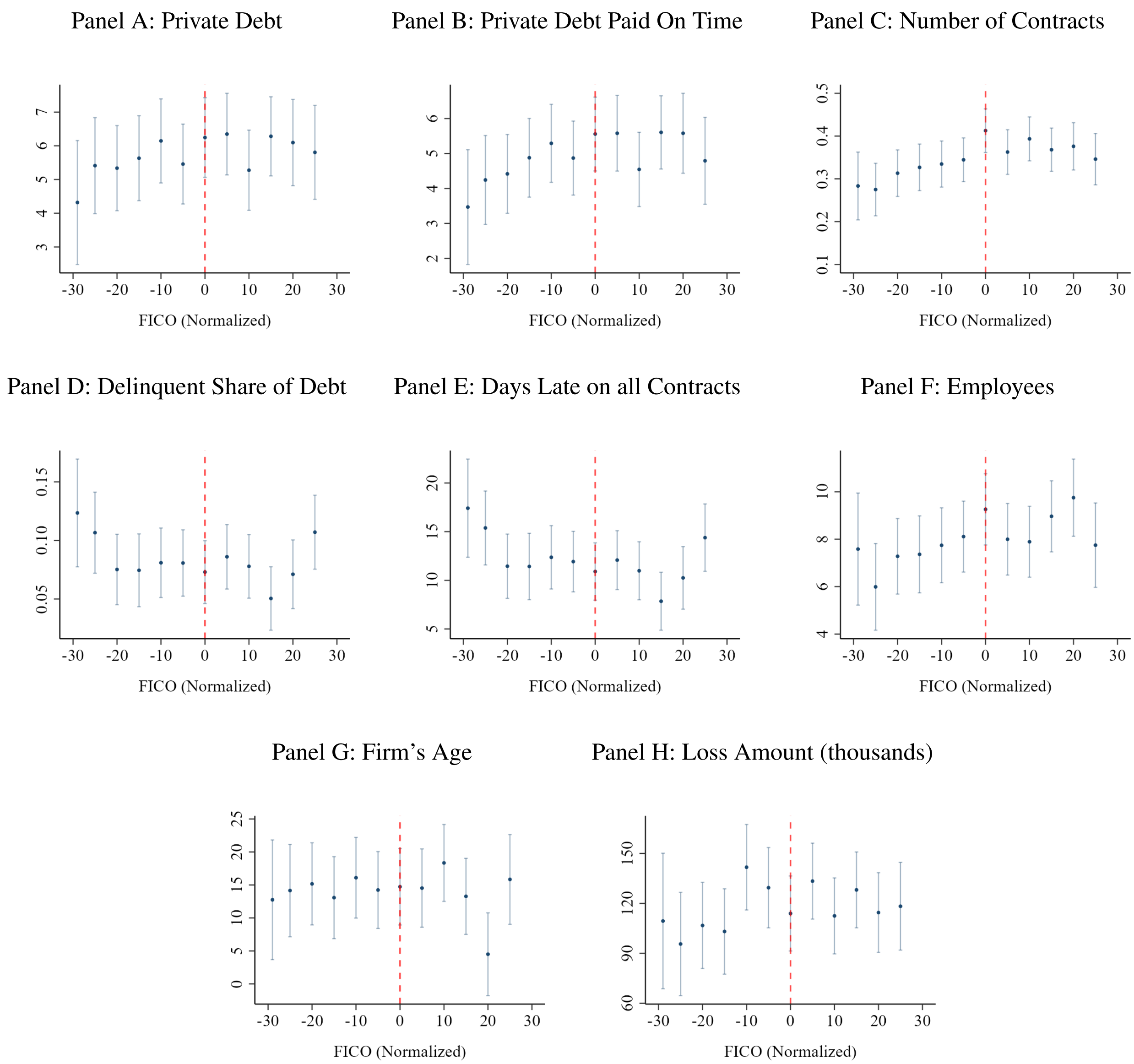


Note: Figure plots the local polynomial density estimator of Cattaneo et al. (2020, 2021), implemented using R package “rddensity." The shaded bars plot the histogram of the running variable (the owner’s FICO score). The estimates use a triangular kernel. The lines plot local, quadratic polynomial point estimates with bias-corrected confidence intervals.

Table B2: Credit Score Thresholds Over Time

FICO	Years		
540	2005		
560	2005	2012	
570	2014	2017	
580	2016		
600	2010	2015	
620	2006	2009	
640	2010	2014	2016
670	2011		
680	2006	2007	2008
700	2006	2012	
720	2008		
770	2008		
780	2015		

Figure B2: Threshold Continuity



Note: The figure shows the outstanding private debt, outstanding private debt paid on time, number of contracts, delinquent share of debt, days late on all contracts, number of employees, firm age, and disaster-related loss amount across values of normalized credit score.

Figure B3: Credit Discontinuities by Year

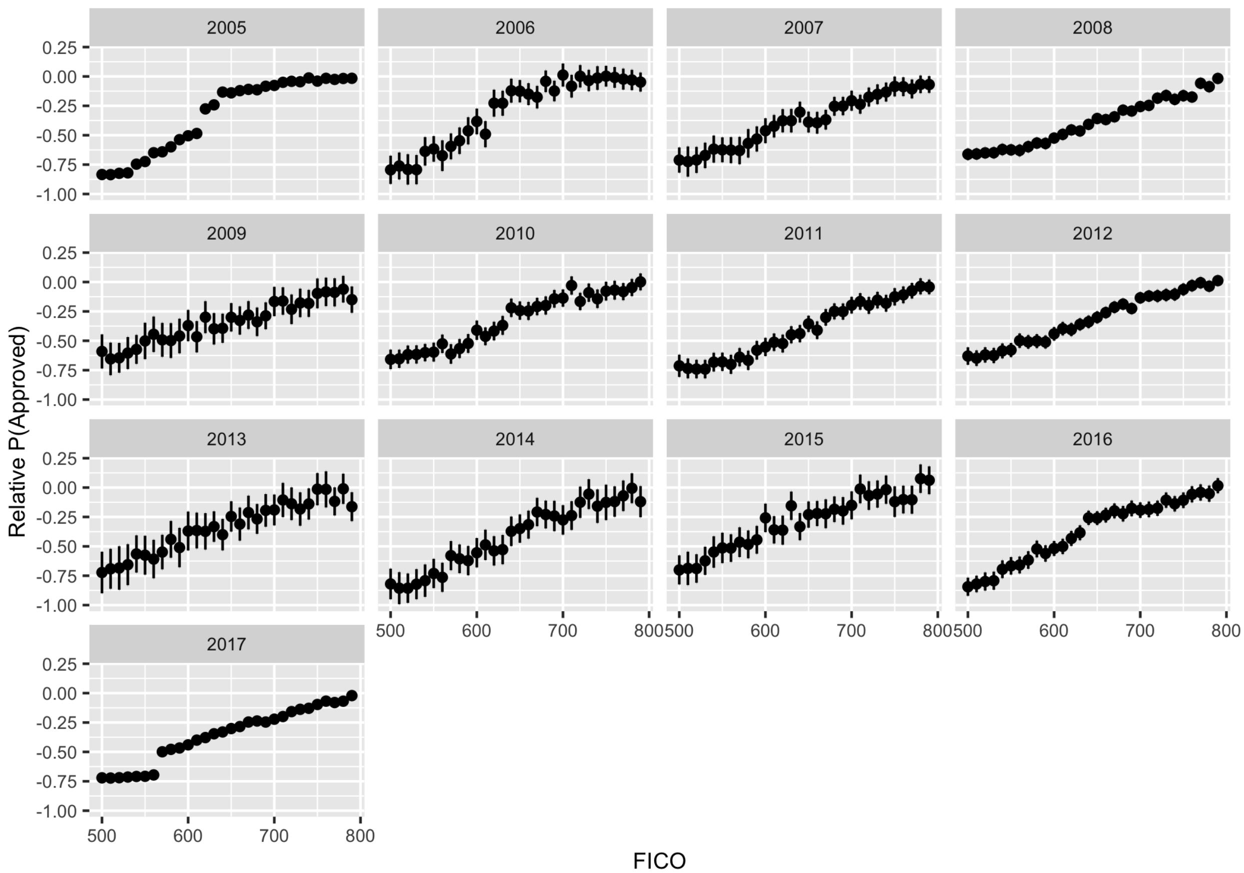


Table B3: Summary Statistics for SBA Applicants by Approval Status (Regression Sample)

Panel A: All Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	10.9	11.8	1.0	7.0	26.0	19,622
Employees	6.5	12.3	1.0	2.0	15.0	8,612
FICO Score	635.6	75.7	544.0	634.0	740.0	20,219
Loss Amount ($, 000s)	110.3	263.4	6.2	38.2	250.4	11,712

Panel B: Declined Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	10.4	11.5	1.0	7.0	25.0	11,827
Employees	6.2	11.8	1.0	3.0	14.0	4,872
FICO Score	615.2	72.6	536.0	587.0	718.0	12,302
Loss Amount ($, 000s)	96.4	223.8	4.8	33.3	222.0	5,387

Panel C: Approved Applicants

	Mean (1)	Std. Dev (2)	p10 (3)	Median (4)	p90 (5)	Observations (6)
Age	11.7	12.2	1.0	8.0	28.0	7,795
Employees	6.9	12.9	0.0	2.0	18.0	3,740
FICO Score	667.4	69.1	573.0	674.0	768.0	7,917
Loss Amount ($, 000s)	122.1	292.4	7.8	42.1	274.9	6,325
Insurance Payments ($, 000s)	21.2	94.2	0.0	0.0	35.6	7,917
Loan Amount ($, 000s)	73.8	170.1	0.0	24.2	183.1	7,917
Interest Rate	4.1	0.6	4.0	4.0	4.0	7,917
Maturity (Years)	17.5	10.3	4.0	15.0	30.0	7,917
Monthly Payments	0.6	1.4	0.1	0.3	1.2	7,917

Notes: All dollar amounts are in thousands of $2018. Firm age is in years. The regression sample includes all applicants whose owners have a credit score within 29 points of a credit score discontinuity.

Appendix C Main Results: Supplementary Material

Table C1: Summary Statistics for Form of Firm Exit in Experian Sample Matched to Census

Panel A: All Firms Post Disaster

Dependent Variable:	Mean (1)	Observations (2)
Exit, Overall	0.15	112,000
Exit, Any Bankruptcy	0.01	112,000
Exit, Any Deliquency	0.03	112,000
Exit, Other	0.11	112,000

Panel B: Declined Firms Post Disaster

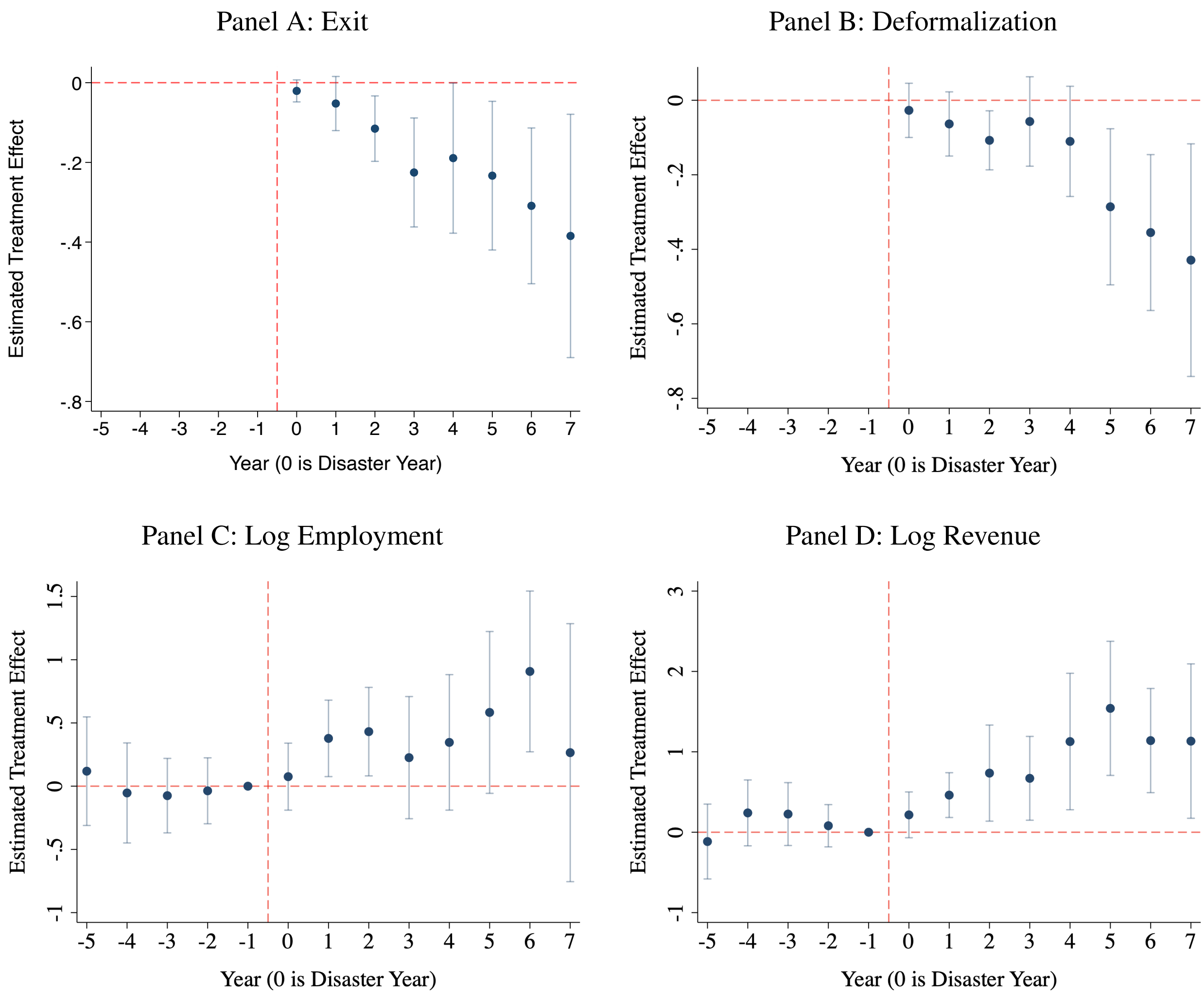
Dependent Variable:	Mean (1)	Observations (2)
Exit, Overall	0.21	78,000
Exit, Any Bankruptcy	0.01	78,000
Exit, Any Deliquency	0.04	78,000
Exit, Other	0.16	78,000

Panel C: Approved Firms Post Disaster

Dependent Variable:	Mean (1)	Observations (2)
Exit, Overall	0.17	35,000
Exit, Any Bankruptcy	0.01	35,000
Exit, Any Deliquency	0.04	35,000
Exit, Other	0.13	35,000

Note: Experian variables are drawn from a June 30 snapshot. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008.

Figure C1: Effect of Disaster Loans on Real Outcomes (Census Sample, Employer Firms Only, 2SLS)



Note: These figures contain estimates of Equation 3. "Exit" is a binary variable equal to 1 after a firm has permanently exited from the sample. "Deformalization" marks a permanent transition from an employer firm to a non-employer firm. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this figure were disclosed under DRB CBDRB-FY23-CED006-0008.

Table C2: Effect of Disaster Loans on Credit Inquiries

Financial Outcomes (Experian Sample)

Dependent Variable:	Number of Inquiries Past 9 Months	Number of Inquiries Past 6 Months	Number of Inquiries Past 3 Months
	(1)	(2)	(3)
Post-disaster x Approved (IV)	0.183	0.188	0.072
	(0.207)	(0.139)	(0.071)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	0.30	0.20	0.10
Observations	142,199	142,199	142,199
KP F-stat	61.462	61.462	61.462

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Number of Inquiries” is the number of credit inquiries made on behalf of potential lenders. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

C.1 Results Using ZIP Code Fixed Effects

Table C3: Disaster Loan Effects Estimated using ZIP code Fixed Effects (2SLS)

Panel A: Real Outcomes (Census Sample)

Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved	-0.134***	-.055	.1944	.0779
	(0.0475)	(0.0342)	(0.1189)	(0.3052)
Zipcode FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.115	0.059	1.042	4.384
Observations	291,000	170,000	170,000	128,000
KP F-Stat	163.5	97.1	97.1	128.1

Panel B: Credit and Adverse Financial Outcomes (Experian Sample)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved (IV)	14.940***	13.626***	0.461***	-0.246***	-25.933***	-0.026
	(2.833)	(2.772)	(0.096)	(0.088)	(9.341)	(0.022)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.934	6.769	0.488	0.134	16.675	0.028
Observations	142,199	142,199	142,199	53,959	53,959	142,876
KP F-Stat	11.71	11.71	11.71	2.14	2.14	12.03

Note: This table contains estimates of Equation 2, but replaces firm fixed effects with ZIP code fixed effects and includes quadratic controls for the running variable (the owner's credit score). "Exit" is a binary variable equal to 1 after a firm has permanently exited from the sample. "Deformalization" marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. "Outstanding Private Debt" and "Outstanding Private Debt Paid On Time" are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. "Number of Contracts" is a holistic measure of the firm's time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. "Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. "Bankruptcy" is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

C.2 Results with Other Bandwidths

Table C4: Different Bandwidths (2SLS)

Panel A: Experian Sample within the Bandwidth of 29

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	17.712***	16.369***	0.568***	-0.339***	-34.996***	-0.038*
	(3.432)	(3.350)	(0.089)	(0.086)	(9.675)	(0.022)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.93	6.77	0.49	0.13	16.68	0.03
Observations	142,199	142,199	142,199	53,959	53,959	142,876
KP F-stat	61.462	61.462	61.462	14.839	14.839	61.144

Panel B: Experian Sample within the Bandwidth of 19

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	18.977***	17.810***	0.531***	-0.309***	-31.049**	-0.038
	(4.172)	(4.066)	(0.133)	(0.112)	(12.064)	(0.026)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	8.11	6.91	0.50	0.13	16.63	0.03
Observations	103,245	103,245	103,245	39,403	39,403	103,751
KP F-stat	50.402	50.402	50.402	10.482	10.482	50.032

Panel C: Experian Sample within the Bandwidth of 9

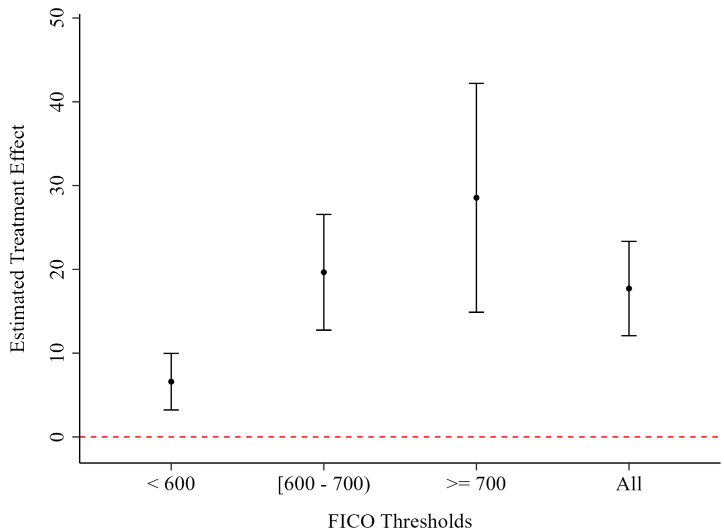
Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	13.856***	13.727***	0.546***	-0.543**	-58.984*	-0.044
	(4.687)	(4.565)	(0.209)	(0.269)	(30.931)	(0.031)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	8.14	6.94	0.50	0.14	16.88	0.03
Observations	53,569	53,569	53,569	20,404	20,404	53,805
KP F-stat	40.716	40.716	40.716	5.376	5.376	40.141

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid on Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.“Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

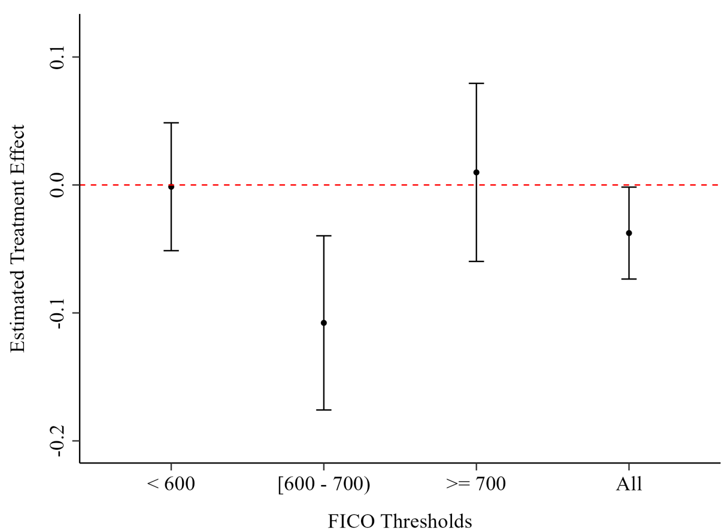
C.3 FICO Thresholds

Figure C2: Effect of Disaster Loans at Different FICO Scores

Panel A: Outstanding Debt



Panel B: Bankruptcy



Note: These figures contain estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” is the total private credit balance (and does not include the SBA loan) and is in thousands of $2018. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID.

C.4 Incorporating SBA Loan Charge Offs

We re-estimate our credit delinquency measure, explicitly adding recovery loan charge-off amounts. While Experian and the SBA confirm that recovery loan charge-offs are reported to the credit bureaus, we conduct this additional analysis out of concern that unpaid recovery loans may not be incorporated fully into reported delinquencies. The SBA transfers charge-offs to Treasury who then reports them to the credit bureaus. Given this sequence, gaps or delays in reporting seem possible.

We add recovery loans and their charge-offs to the share of debt that is delinquent for each firm with a recovery loan charge-off. Specifically, we add the recovery loan charge-off amount to the numerator and the total recovery loan amount to the denominator. Since Experian indicates that recovery loan charge-offs are included in their reports, this addition may double-count these charge-offs so should be considered a conservative estimate.

We report the results in Table C5. The number of observations in the regression grows from 54 thousand to 75 thousand. Only firms with outstanding private debt are included in our Experian measure of share delinquent (Column 1). The larger sample size in Column 2 reflects that some recovery loan borrowers do not have any other outstanding debt on their credit reports. We find that adding these recovery loan charge-offs results in a LATE of -0.32, instead of -0.34 (Column 1), so is consistent with the findings in our main analysis that recovery loans reduce credit delinquencies.

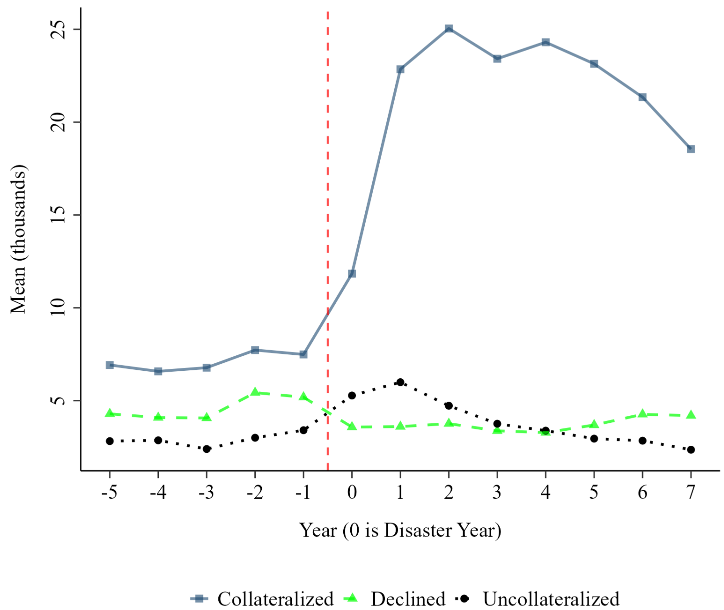
Table C5: Effect on Delinquencies (2SLS)

Dependent Variable:	Delinquent Share of Debt	Delinquent Share of Debt Including SBA Loan Charge-offs
	(1)	(2)
Post-disaster x Approved (IV)	-0.339***	-0.317***
	(0.086)	(0.078)
Application FE	Yes	Yes
Year x Experiment FE	Yes	Yes
Mean Dep Var	0.13	0.13
Observations	53,959	74,827
KP F-stat	14.839	13.031

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. "Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days. "Delinquent Share of Debt Including SBA Loan Charge-offs" is the sum of outstanding debt reported delinquent for at least 90 days and the SBA loan charge-off divided by the sum of outstanding private debt and SBA loan disbursed. Standard errors are clustered by FEMA disaster declaration ID. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

C.5 Collateralized versus Uncollateralized Recovery Loans

Figure C3: Outstanding Debt, Raw Means, Collateralized Vs. Uncollateralized Recovery Loans



Note: This figure shows raw means, comparing the outstanding private debt balances of 1) firms with a collateralized recovery loan, 2) firms with an uncollateralized recovery loan, and 3) declined firms. "Outstanding Private Debt" is the total private credit balance (and does not include the SBA loan) and is in thousands of $2018.

C.6 OLS Results

For the main real and financial outcomes, we report OLS results in Table C6, and event studies in Figures C4, C5, and C6. While the OLS results do not have a causal interpretation, they provide a useful reference by comparing outcomes for approved versus declined firms. Overall, the OLS results are consistent with the IV results in being uniformly in the same direction and highly statistically significant. For example, the IV LATE for private debt is about $17,700, while the OLS estimate is $11,900. The figures showing the OLS results also illustrate pre-trends: for example, approved firms were more likely to have lower employment and revenues and more delinquent debt before the disaster. These pre-trends, which suggests that endogeneity may affect the OLS results, provide additional motivation for the IV specification on which we rely.

Table C6: Effect of Disaster Loans Estimated using OLS

Panel A: Real Outcomes (Census Sample)

Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved	-0.0414***	-0.0116***	0.0182*	0.0471***
	(0.00379)	(0.00334)	(0.0105)	(0.0182)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.115	0.059	1.042	4.384
Observations	291,000	170,000	170,000	128,000

Panel B: Credit and Adverse Financial Outcomes (Experian Sample)

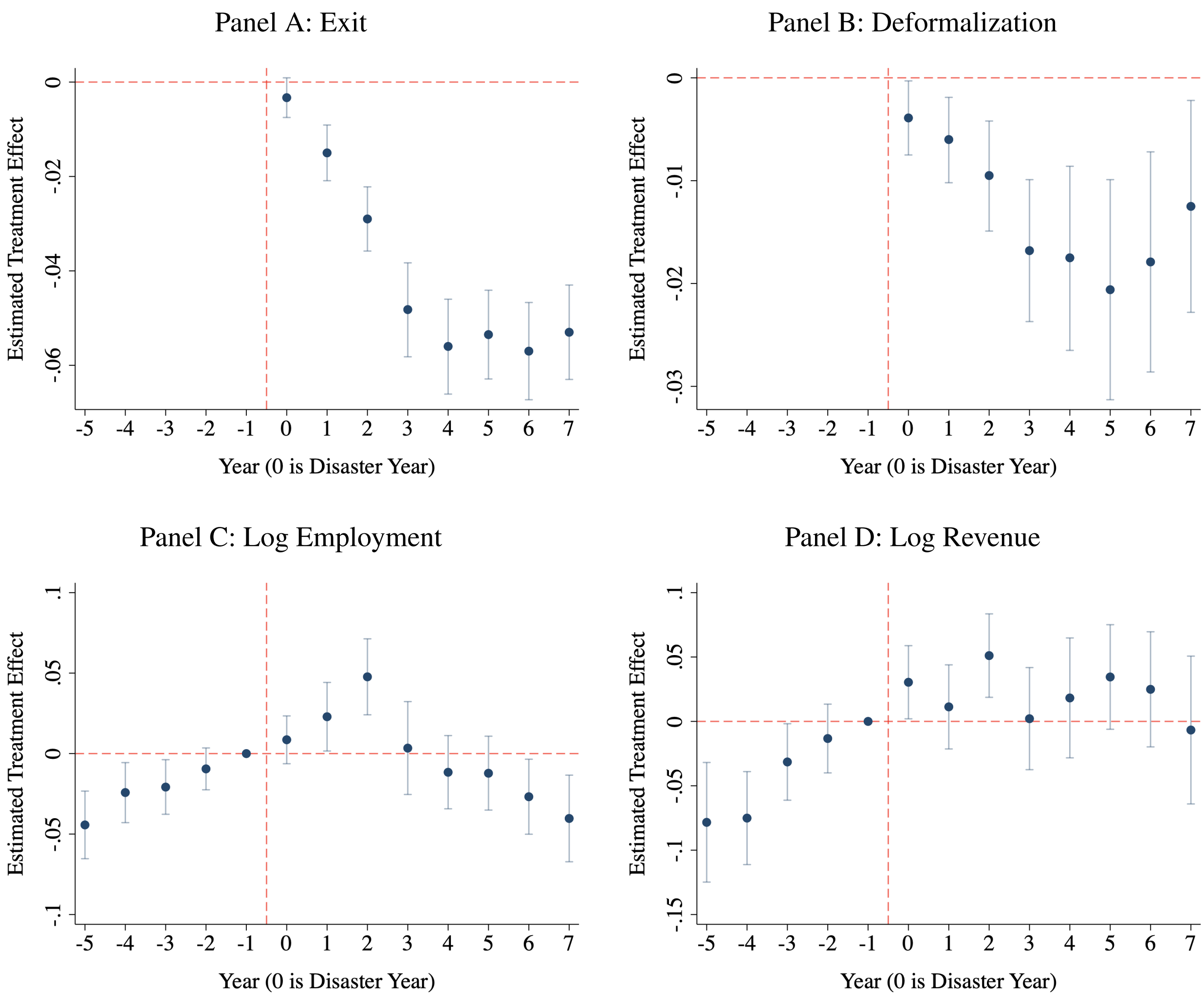
Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved	11.931***	10.732***	0.543***	-0.054***	-6.944***	-0.012***
	(0.857)	(0.755)	(0.023)	(0.011)	(1.179)	(0.003)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.934	6.769	0.488	0.134	16.675	0.028
Observations	142,199	142,199	142,199	53,959	53,959	142,876

Note: This table contains estimates of Equation 1. “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix D Frictions Analyses, Supplementary Materials

Capital Intensity. We classify firms as capitally intensive using data from the Bureau of Labor Statistics (BLS) to estimate the labor share of each industry at NAICS 3-digit level in each year. Labor share is defined as the ratio of “labor compensation" over “sectoral output". High capital intensity industries are those with labor share below the median labor share of all industries in each year, and the indicator variable is equal to one for these industries in the corresponding year and zero otherwise. BLS data is not comprehensive and does not cover some major industries in our sample. We manually assign the high capital intensity indicator for some industries (e.g., classifying businesses owning real estate as high capital

Figure C4: Effect of Disaster Loans on Real Outcomes, OLS



Note: These figures contain estimates of Equation 3. “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this figure were disclosed under DRB CBDRB-FY23-CED006-0008.

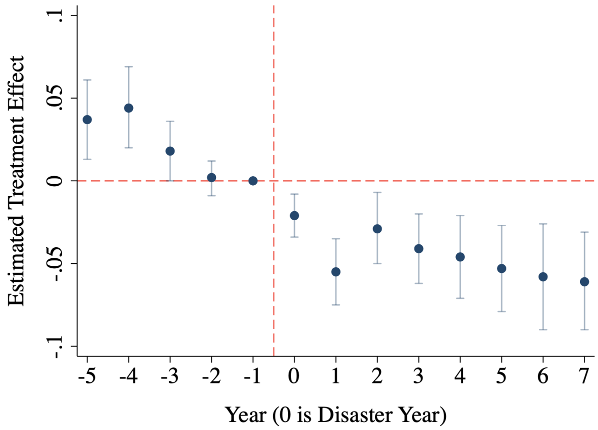
intensity and businesses in professional, scientific, and technical services as low capital intensity).

Intelliscore. Intelliscore is Experian’s proprietary measure of the firm’s credit risk. The analysis of firms with low Intelliscore in Table D4 restricts the sample to firms with scores below the median.

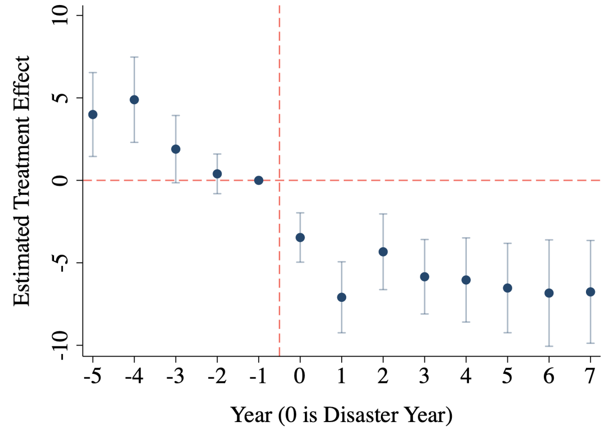
Firm Age. The analyses of firms based on age in Table D5 splits the sample at the median firm age of 8 years: firms who are 7 years old or less are considered young.

Figure C5: Effect of Disaster Loans on Adverse Financial Outcomes, OLS

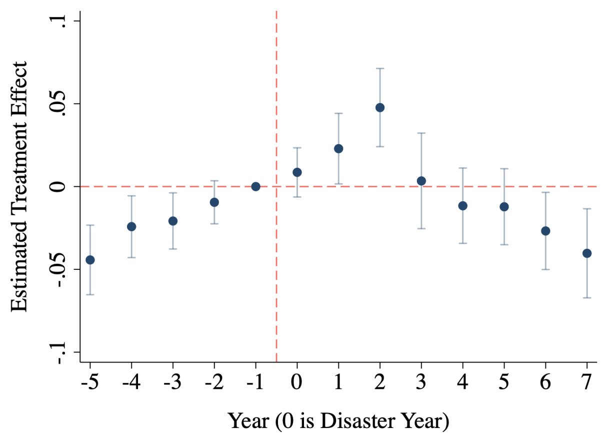
Panel A: Delinquent Share of Debt



Panel B: Days Late on all Contracts



Panel C: Bankruptcy

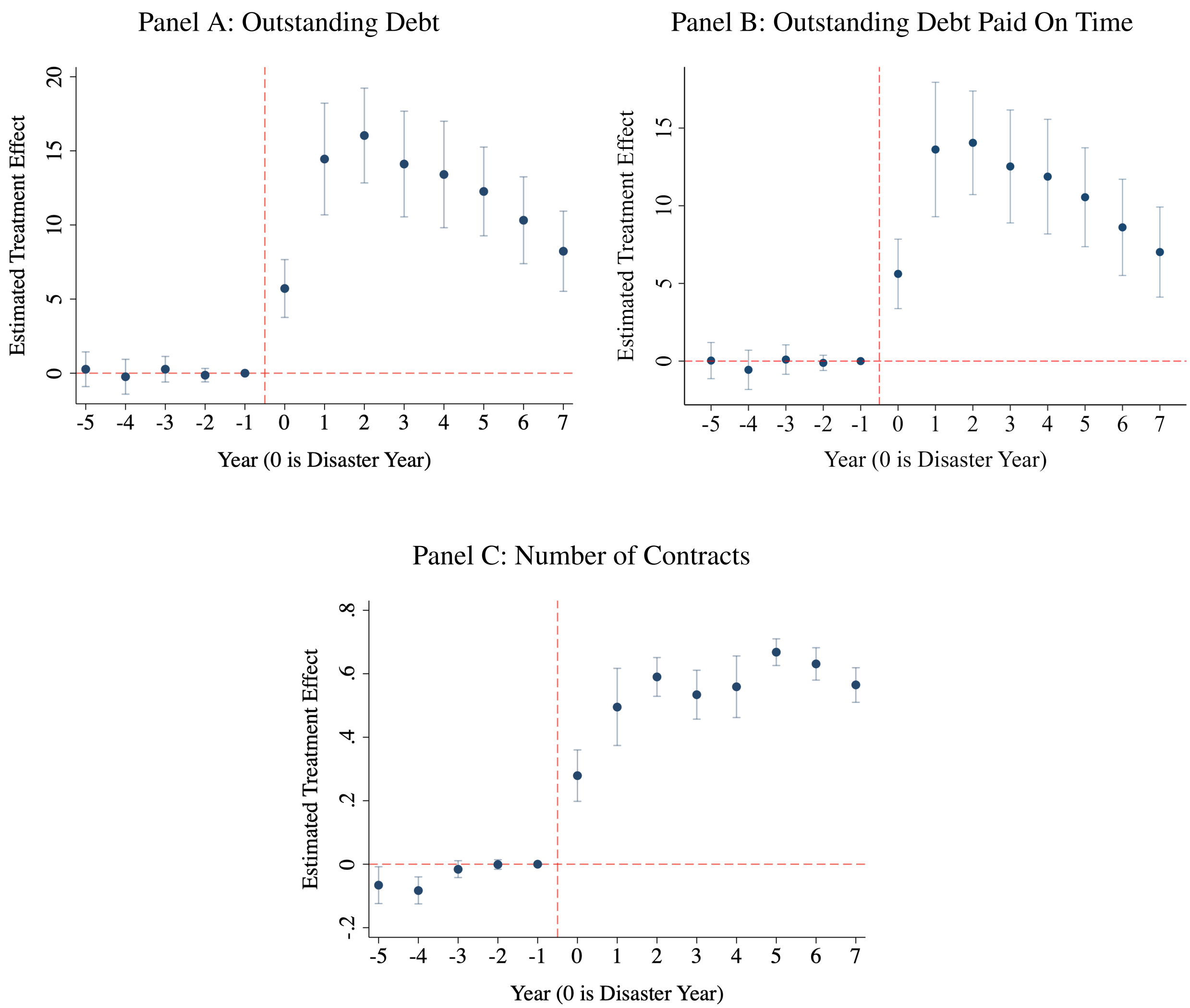


Note: These figures contain estimates of Equation 3. Experian variables are drawn from a June 30 snapshot. "Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. "Bankruptcy" is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID.

Previous Lending Relationship. For the analysis of firms with no previous lending relationship in Table D6, we restrict the sample to firms who have no positive private debt balances prior to the disaster.

Bank Density. The analyses based on bank density in Table D7 restricts the sample to ZIP codes with a below-median number of banks.

Figure C6: Effect of Disaster Loans on Private Credit, OLS



Note: These figures contain estimates of Equation 3. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. Standard errors are clustered by FEMA disaster declaration ID.

Black Communities. The analyses examining the effects in Black communities in Table D8 restricts the sample to ZIP codes with an above-median share of residents who identify as Black.

Table D1: Capital Intensive (2SLS)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy	Exit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-disaster x Approved (IV)	36.657***	38.164***	0.077	-0.535**	-62.669**	-0.082*	-0.480***
	(7.340)	(7.565)	(0.332)	(0.250)	(26.828)	(0.047)	(0.164)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	9.18	7.97	0.54	0.11	13.46	0.02	0.12
Observations	44,328	44,328	44,328	16,973	16,973	44,471	37,500
KP F-stat	36.607	36.607	36.607	9.174	9.174	36.280	21.47

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid on Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.“Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY24-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Since we are only able to classify a subset of firms in the Census data regarding whether their industry is capital versus labor intensive, we re-estimate exit likelihood for this sub-sample and report it in Table D2. The effect on exit in this subsample is negative but statistically insignificant. Given the large and statistically significant effects on exit among capitally intensive firms show in Table D1, this result suggests that recovery loans have a less meaningful effect on exit for labor intensive firms.

Table D2: Census Sample with Observed Capital Versus Labor Intensity

Dependent Variable:	Exit
	(1)
Post-disaster x Approved (IV)	-0.130
	(0.84)
Application FE	Yes
Year x Experiment FE	Yes
Mean Dep Var	0.12
Observations	75,000
KP F-stat	74.83

Note: This table contains estimates of Equation 2 using the subsample of firms in the Census data for which their labor versus capital intensity is observed. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY24-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table D3: Temporal Variation in Disaster Loan Effects (2SLS)

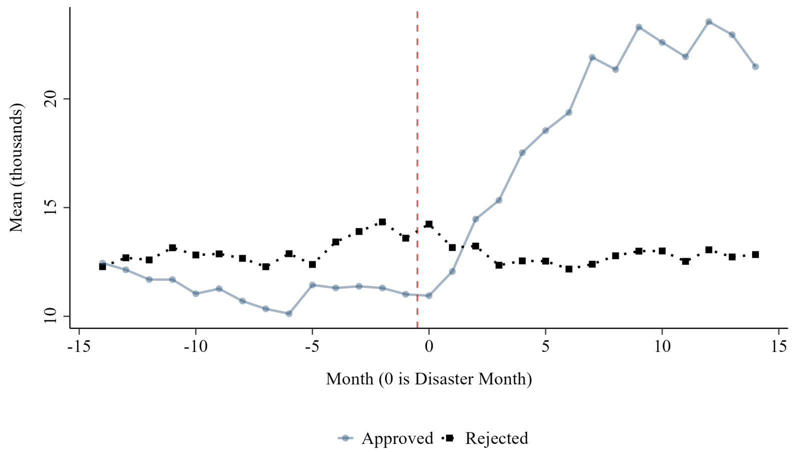
Outstanding Private Debt (Experian Sample)

Time after loan decision date:	0 to 3 Months	3 to 6 Months	6 to 9 Months	9 to 12 Months
	(1)	(2)	(3)	(4)
I(Event Year = 0) x Approved (IV)	-11.324*	7.407	1.259	0.272
	(6.742)	(5.371)	(3.306)	(4.750)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	9.03	8.11	7.28	8.62
Observations	18,781	28,653	67,644	27,111

Note: This table contains estimates of $Approved_i \times \mathbb{1}(t = 0)$ coefficient of Equation 3. Experian variables are drawn from a June 30 snapshot. "Outstanding Private Debt" is the total private credit balance (and does not include the SBA loan). It is in thousands of $2018. Standard errors are clustered by FEMA disaster declaration ID. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure D1: Outstanding Private Debt Monthly Development

Panel A: Raw Means



Panel B: Estimated Coefficients

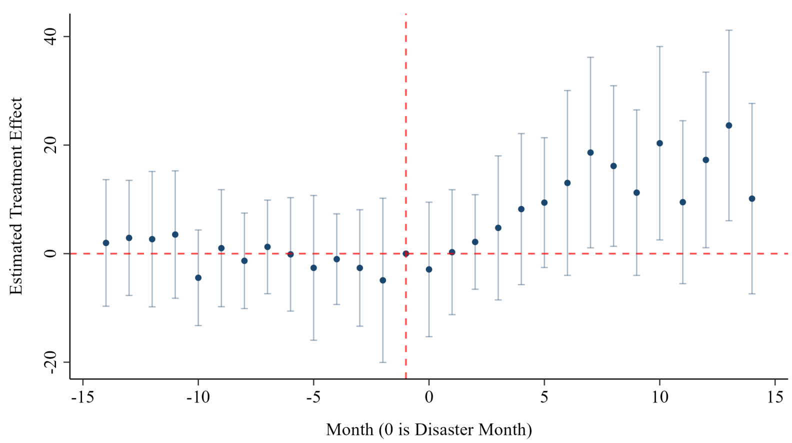


Table D4: Low Intelliscore (2SLS)

Panel A: Experian Sample with Below-Median Intelliscore

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	14.902***	13.293***	0.448***	-0.213	-21.243	-0.035
	(3.093)	(2.744)	(0.143)	(0.144)	(14.400)	(0.030)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.92	6.51	0.45	0.19	24.00	0.04
Observations	40,086	40,086	40,086	15,219	15,219	40,364
KP F-stat	84.527	84.527	84.527	8.683	8.683	84.198

Panel B: Census Sample with Below-Median Intelliscore

Dependent Variable:	Exit	Deformalization	Log Employment
	(1)	(2)	(3)
Post-disaster x Approved (IV)	-0.208**	-0.113*	0.347**
	(0.087)	(0.067)	(0.160)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	0.10	0.07	1.19
Observations	55,500	38,500	38,500
KP F-stat	89.08	59.83	59.83

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid on Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.“Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY24-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table D5: Variation in Disaster Loan Effects Around Median Firm Age at Application (2SLS)

Panel A: Real Outcomes (Census Sample)

	Older Firms		Young Firms	
Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.175***	-0.0805*	0.246**	0.624**
	(0.0522)	(0.0443)	(0.0956)	(0.262)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.102	0.070	0.935	3.990
Observations	140,000	94,000	76,000	56,000
KP F-Stat	156.5	80.3	35.4	38.0

Panel B: Credit Outcomes (Experian Sample)

	Older Firms			Young Firms		
Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved (IV)	19.322***	18.045***	0.668***	15.215***	13.707***	0.433**
	(4.234)	(4.336)	(0.130)	(3.736)	(3.319)	(0.188)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	9.711	8.407	0.570	5.876	4.873	0.393
Observations	76,308	76,308	76,308	65,891	65,891	65,891
KP F-Stat	46.41	46.41	46.41	36.59	36.59	36.59

Panel C: Adverse Financial Outcomes (Experian Sample)

	Older Firms			Young Firms		
Dependent Variable:	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved (IV)	-0.251***	-26.762***	0.009	-0.639*	-60.966*	-0.111***
	(0.087)	(9.943)	(0.024)	(0.326)	(31.013)	(0.041)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	0.116	14.756	0.029	0.163	19.796	0.026
Observations	33,410	33,410	76,652	20,549	20,549	66,224
KP F-Stat	13.76	13.76	45.79	3.62	3.62	37.34

Note: This table contains estimates of Equation 2. Young firms are those 7 years or younger. “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table D6: No Previous Borrowing (2SLS)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy	Exit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-disaster x Approved (IV)	15.259***	14.415***	0.582***	-0.428*	-39.746	-0.049**	-0.181**
	(3.415)	(3.101)	(0.107)	(0.243)	(26.270)	(0.024)	(0.076)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	5.42	4.64	0.36	0.15	17.76	0.02	0.17
Observations	91,759	91,759	91,759	23,935	23,935	92,194	90,000
KP F-stat	105.780	105.780	105.780	6.905	6.905	103.887	110.10

Note: This table is restricted to firms who have no positive private debt balances prior to the disaster. It contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid on Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.“Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY24-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table D7: Low Bank Density (2SLS)

Panel A: Experian Sample with Lower Than Median Number of Banks

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid on Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	15.951**	16.755***	0.798***	-0.555**	-61.706**	-0.104*
	(6.363)	(5.208)	(0.258)	(0.230)	(27.225)	(0.061)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.23	6.22	0.43	0.13	16.36	0.03
Observations	30,719	30,719	30,719	10,755	10,755	30,877
KP F-stat	24.027	24.027	24.027	6.797	6.797	24.138

Panel B: Census Sample with Lower than Median Number of Banks

Dependent Variable:	Exit	Deformalization	Log Employment
	(1)	(2)	(3)
Post-disaster x Approved (IV)	-0.225***	-0.132***	0.202*
	(0.054)	(0.050)	(0.113)
Application FE	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes
Mean Dep Var	0.12	0.06	1.00
Observations	141,000	90,000	90,000
KP F-stat	134.2	49.53	49.53

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid on Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others.“Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY24-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table D8: Disaster Loan Effects in Neighborhoods with Above-Median Black Populations (2SLS)

Panel A: Real Outcomes (Census Sample)

Dependent Variable:	Exit	Deformalization	Log Employment	Log Revenue
	(1)	(2)	(3)	(4)
Post Disaster x Approved (IV)	-0.175***	-0.0886	0.277**	0.122
	(0.0602)	(0.0551)	(0.127)	(0.340)
Application FE	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.129	0.057	0.989	4.080
Observations	145,000	76,000	76,000	59,500
KP F-Stat	86.66	31.95	31.95	35.41

Panel B: Credit and Adverse Financial Outcomes (Experian Sample)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved (IV)	19.006***	18.765***	0.328**	-0.473***	-49.037***	-0.058
	(5.250)	(4.964)	(0.164)	(0.115)	(12.636)	(0.036)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	7.016	5.976	0.448	0.155	18.827	0.020
Observations	68,097	68,097	68,097	24,621	24,621	68,189
KP F-Stat	30.98	30.98	30.98	10.92	10.92	30.97

Note: This table contains estimates of Equation 2. “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix E Spillover Effects, Supplementary Material

Summary statistics for nearby incumbent firms in retail are reported in Table E1.

The regression results for potential spillovers of the effects of recovery loans on nearby firms in the retail sector are in Table E2. Panel A shows a marginally significant increase in firm entry in the neighborhood and a marginally significant decrease in the revenues of nearby firms who did not apply for a recovery loan. We do not find significant effects on firm exit, deformalization or employment for these nearby firms. These estimates are relatively precise in that they have small standard errors, and the F-statistics are large, increasing confidence in these null results.

Panel B reports the credit report outcomes for nearby firms, none of which are statistically significant. In contrast to Panel A, the standard errors are large and the F-statistics are small, ranging from 3 to 7, in these regressions. One reason may be the smaller sample size, due to restricting to retail. It is also possible that firms with credit reports in the retail sector are evaluated by the SBA differently than those in other sectors, making the instrument (the FICO threshold) less important. Whatever the reason, the insignificant results in Panel B are inconclusive in that they could be due to the weakness of the instrument instead of a lack of an effect.

Table E3 includes more observations by removing the sample restriction limiting the analysis to retail firms in Table E2. These regressions include all nearby firms, regardless of industry. The F-statistics are larger in this case, ranging from 20 to 45 and the standard errors are smaller. Thus, while looking at a broader sample than retail, these results point toward a lack of effect: we do not find evidence that recovery loans affect the private debt balances, delinquencies, or bankruptcy filings of nearby firms who do not apply for a recovery loan.

Table E1: Summary Statistics for Incumbent Neighbors

Panel A: Census Incumbent Neighbors

	Mean (1)	Std. Dev (2)	Quasimedian (3)	Observations (4)
Neighborhood Entry	1.19	25.92	0.00	231,000
Exit	0.08			914,000
Deformalization	0.003			914,000
Number of Employees	9.79	9.77	5.50	691,000
Revenue ($, 000s)	625,000	1,864,000	945	543,000

Panel B: Experian Incumbent Neighbors

	Mean (1)	Std. Dev (2)	Median (3)	Observations (4)
Outstanding Private Debt ($, 000s)	5,150	17,013	0.00	89,851
Outstanding Private Debt Paid On Time ($, 000s)	4.10	13.66	0.00	89,851
Number of Contracts	0.42	1.42	0.00	89,851
Delinquent Share of Debt	0.10	0.27	0.00	27,797
Days Late on all Contracts	13.26	29.56	0.00	27,797
Bankruptcy	0.02	0.14	0.00	90,438

Note: “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt Reported Last 30 Day” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008.

Table E2: Spillovers: Disaster Loan Effects on Local Entry and Incumbent Neighbor Firms (2SLS)

Panel A: Real Outcomes (Census Sample with Census Data Neighbors)

Dependent Variable:	Neighborhood Entry (if $\geq$ 3 Employees)	Exit	Deformalization	Log Employment	Log Revenue
		Incumbent Neighbor Firms			
	(1)	(2)	(3)	(4)	(5)
Post Disaster x Approved (IV)	7.901*	-0.0102	0.00372	-0.0414	-0.227*
	(4.382)	(0.0235)	(0.00269)	(0.0494)	(0.130)
Application FE	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	1.336	0.076	0.003	1.781	8.053
Observations	130,000	914,000	914,000	691,000	543,000
KP F-Stat	97.41	181.2	181.2	158.0	155.2

Panel B: Financial Outcomes (Experian Sample with D&B Neighbors)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disaster x Approved (IV)	4.673	3.743	-0.010	0.182	12.740	-0.000
	(4.212)	(3.380)	(0.320)	(0.194)	(18.538)	(0.046)
Application FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	5.150	4.104	0.424	0.096	13.262	0.020
Observations	89,851	89,851	89,851	27,797	27,797	89,851
KP F-Stat	6.88	6.88	6.88	3.21	3.21	6.88

Note: This table contains estimates of Equation 2. “Neighborhood Entry” is measured as the difference between the number of firms in the tract in year t and year $t-1$. “Exit” is a binary variable equal to 1 after a firm has permanently exited from the sample. “Deformalization” marks a permanent transition from an employer firm to a non-employer firm. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days "Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table E3: Spillovers: Disaster Loan Effects on All Neighbor Firms (2SLS)

Financial Outcomes (Experian Sample with D&B Neighbors)

Dependent Variable:	Outstanding Private Debt	Outstanding Private Debt Paid On Time	Number of Contracts	Delinquent Share of Debt	Days Late on all Contracts	Bankruptcy
	(1)	(2)	(3)	(4)	(5)	(6)
Post-disaster x Approved (IV)	1.115	0.886	0.060	-0.057	-7.586	-0.006
	(0.792)	(0.622)	(0.053)	(0.052)	(5.242)	(0.008)
Year x Experiment FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	3.48	2.79	0.37	0.13	16.73	0.01
Observations	314,277	314,277	314,277	91,247	91,247	314,277
KP F-stat	45.409	45.409	45.409	20.189	20.189	45.409

Note: This table contains estimates of Equation 2. Experian variables are drawn from a June 30 snapshot. “Outstanding Private Debt” and “Outstanding Private Debt, Paid On Time” are total private credit balances (and do not include the SBA loan). They are in thousands of $2018. “Number of Contracts" is a holistic measure of the firm’s time-sensitive obligations including private credit contracts but also leases, utilities, and telecom, among others. “Delinquent Share of Debt" is the share of outstanding debt that has been reported delinquent for at least 90 days “Days Late on all Contracts" is the number of days late (i.e., beyond the payment deadline) for all contracts. “Bankruptcy” is identified by any type of bankruptcy filing. Standard errors are clustered by FEMA disaster declaration ID. Estimates in this table were disclosed under DRB CBDRB-FY23-CED006-0008. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.