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Abstract

We estimate the causal effect of emergency credit on households' finances after a negative shock. To do so, we link application data from the U.S. Federal Disaster Loan program, which provides loans to households that have uninsured damages from a federally-declared natural disaster, to a panel of credit records before and after the shock. We exploit a discontinuity in the loan approval rules that led applicants with debt-to-income ratios below 40% to be differentially likely to be approved. Using an instrumented difference-in-differences research design, we find that credit provision at the time of a shock significantly reduces severe financial distress, decreasing the likelihood of filing for bankruptcy by 61% in the three years following the disaster. We explore mechanisms using additional quasi-experimental variation in interest rates, finding support for a liquidity-based explanation. Credit provision in a time of crisis has real consumption effects in the form of additional car purchases even 3 years after loan receipt. Our findings suggest that well-timed liquidity provided to households in acute need can have substantial and persistent positive effects.

JEL Codes: D14, G23, G28, H81

Keywords: Consumer credit, disaster loan, personal bankruptcy, delinquency, auto loans, climate risk

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1 Introduction

Credit allows households to smooth shocks over time that might otherwise lead to financial distress. In particular, credit provides liquidity, and many households are illiquid. Only 40% of U.S. households report that they have enough liquid savings to cover 3 months of expenses (Bhutta and Dettling, 2018). Some shocks are much larger: A major natural disaster, health expense, or liability suit, for instance, can create a sudden and substantial uninsured financial obligation. In the absence of credit, this large need represents a financing problem that most families would be unable to solve. Unaddressed needs can fester, creating a liquidity crisis. Growing unpaid bills can lead to mounting fees and penalties, account delinquencies, and even personal bankruptcy. By providing immediate liquidity that can be repaid gradually, credit may stave off a financial cascade.

While borrowing to smooth large financial obligations is theoretically straightforward (e.g., Friedman, 1957), empirical research highlights adjustment frictions and other barriers that may preclude households' ability to do so. Existing consumption commitments, such as mortgage and auto loan payments and other household expenses, can leave little room to service additional debt (Chetty and Szeidl, 2007). Recent studies find that adjusting such commitments is even more difficult than previously appreciated (Ganong and Noel, 2020; Boar et al., 2022). Thus, attempting to borrow one's way out of a crisis might delay but ultimately exacerbate financial distress. Can households reduce the consequences of severe events through borrowing?

Empirically assessing the impact of emergency credit on households' financial health is difficult for at least three reasons. First, credit provision is inherently endogenous because lenders underwrite on financial health (Agarwal et al., 2018). Second, households may differ in the likelihood of experiencing a shock on both observable and unobservable dimensions. Finally, consumers who turn to credit in an emergency may differ (e.g., in their attitudes toward debt) from those who do not. Assessing the impact of credit, therefore, requires a setting with comparable consumers facing exogenous liquidity needs and credit provision that is plausibly randomly assigned.

In this paper, we examine the effects of credit supplied by the U.S. Federal Disaster Loan program, which provides loans to households with uninsured damages from a federally-declared natural disaster. This program is one of the primary sources of federal disaster assistance to households, lending over $60 billion since its inception. Our data include 315,000 households who applied for disaster recovery loans, spanning 614 distinct disasters from 2005 to 2013. Their loan applications are underwritten by the program and include information on credit scores, other debts, and household income. We merge these applications with credit record data from Experian, one of the nation's three large credit bureaus. Through this merge, we can observe a balanced panel of applicants' credit records from 18 months before to 3 years after the disaster.

These households have experienced large shocks to their balance sheets. The average applying household had over $80,000 in damages, an amount representing more than 110% of its annual income. During hurricanes in particular, households commonly incur large, sudden, uninsured losses due to gaps in flood insurance coverage (Michel-Kerjan, 2010; Billings et al., 2022). The average loan received is for $42,000, with a term of 20 years, at an interest rate of 2.7%, yielding average monthly payments of $230. Through this loan, households can spread the immediate cost of repairs over the next two decades.

To identify the causal effect of credit provision, we instrument for loan approval using a discontinuity in the likelihood of being approved around a debt-service-to-income (DTI) ratio of 0.4, which is codified into the program's loan underwriting handbook.[1] We find that applicants with a DTI just below this threshold are 20 percentage points (pp) more likely to be approved than applicants with a DTI just above it (80% vs. 60% approved). In our analyses described below, we restrict data to a bandwidth sample of applicants with a DTI that is within 10 pp of the 0.4 threshold. The instrument yields a strong first stage and, combined with no evidence of DTI manipulation or other applicant attributes varying discontinuously around the threshold, suggests that we can make causal inferences in this quasi-experimental setting.

Using this instrument for approval, we employ a difference-in-differences design to examine how approval for a disaster loan affects private credit outcomes such as debt balances, delinquencies, and bankruptcy in the years following a natural disaster. Each disaster in our analysis includes unique treatment and control groups; we stack the datasets to estimate and report an average response across all disasters.

We pursue three questions regarding emergency credit and households' finances: First, does emergency credit reduce long-term financial distress after the event? Second, does emergency credit affect household borrowing in the years after the disaster, including new outlays for durable goods? Third, what mechanisms explain these results?

First, we find that receiving credit after a disaster persistently reduces financial distress. Our estimates indicate that applicants who receive the loan due to the discontinuity in approval are 61% less likely to have filed for bankruptcy three years after the disaster. With rich credit report data, we trace out the path of a disaster-induced "debt spiral" and show that emergency credit

[1] DTI describes the share of applicants' monthly income that is committed to payments on fixed financial obligations (e.g., mortgages, auto loans) when the application is filed.

reduces delinquencies that precede bankruptcy. We find that the recovery loan reduces the likelihood of delinquencies by 33% around one year after the disaster. These delinquency effects predate the effects on bankruptcy, illustrating that as consumers fall behind on payments, their debts can become insurmountable. Reductions in delinquencies follow a "pecking order" across different types of debt (Andersson et al., 2013), with disaster loan approval creating the largest reductions in late payments on credit cards, followed by mortgages, and then auto loans.

It is worth noting that these treatment effects of loan receipt are measured against a control group of households experiencing mounting distress. All households in our sample survived a natural disaster, and we show that treated households were also more frequently delinquent after the disaster than before it. The treatment effects thus reflect that disaster loans ease but do not eliminate the financial hardship of approved applicants. A key insight from our study is how unmet liquidity needs exacerbate and extend the consequences of the negative shock.

These long-lasting effects on reducing delinquencies and bankruptcy filings are robust to a range of specification choices and the inclusion of a variety of observable covariates and fixed effects, consistent with the quasi-experimental design. Applicants had no differential levels or trends in delinquency prior to the disaster. We are further able to rule out that delinquency is simply being shifted from private to public markets, because total charge-offs, which include debts from the program itself, remain persistently lower for (instrumented) approved disaster loan households. These findings suggest that credit provision at the time of a shock can mitigate severe financial distress.

Our second main finding is that credit provision at the time of a disaster subsequently increases other household borrowing. A longstanding concern is that government lending may "crowd out" credit markets at the expense of taxpayers. Crowd-out would lead to an empirical pattern in which households who would have taken private loans instead substitute with lower-interest disaster loans. We find no evidence of this substitution effect in the balances of any type of private credit such as credit cards, mortgage debt, or personal loans.

Instead, we find that Federal Disaster Loans act like complements, "crowding in" private credit. We estimate that the total private debt balances of applicants who receive the loan due to the approval discontinuity are $5,000 larger in the years after the disaster. These approved applicants are also 5 percentage points more likely to take out a new auto loan, which is usually associated with purchasing a car and a commonly used proxy for household financial well-being (e.g., Di Maggio et al., 2017; Beraja et al., 2019; Berger et al., 2021). Relative to the baseline rate of roughly 8 percent per year, the response is large, and the positive effect on new auto purchases persists for 3 years. This pattern is consistent with disaster loans helping households recover financially, facilitating investment opportunities for private credit lenders.

Third, we explore the potential mechanisms generating these meaningful effects of emergency credit on financial distress and household consumption. Disaster recovery loans bundle two potentially beneficial features that might explain the results. The first is that they may allow households to solve a substantial liquidity problem. The second is that these subsidized, low-interest loans represent a wealth transfer to consumers.

To disentangle these mechanisms, we leverage additional exogenous variation in the program's interest rates. Approved applicants with credit scores above 700 are typically offered an interest rate that is roughly equal to the private market's concurrent 30-year, fixed-rate mortgage interest rate. Approved applicants with credit scores below 700 are offered an interest rate of about half that rate. Examining outcomes around this price discontinuity, we find that disaster loans reduce delinquencies similarly regardless of whether households receive the lower or higher interest rate. This result suggests that the loan's benefits arise through the provision of emergency liquidity rather than a wealth transfer, aligning with recent findings that emphasize the role of liquidity in understanding household financial distress (Ganong and Noel, 2020; Boar et al., 2022).

Our results offer new insights on households' paths to persistent financial distress. Recent research on this topic includes Gross et al. (2021), which examines the effects of changing incentives around personal bankruptcy reform, and Hsu et al. (2018), which finds that expanded unemployment insurance substantially reduces mortgage default. Keys et al. (2023) use a movers design to explore the drivers of household financial distress and bankruptcy. Indarte (2023) and Ganong and Noel (2020) highlight the importance of existing monthly budget commitments in examining whether a negative shock leads to financial distress in the form of bankruptcy and mortgage foreclosure, respectively. A challenge in this literature is identifying quasi-experimental variation in negative shocks that may precipitate distress. We address this challenge by leveraging exogenously-timed severe climate events resulting in precisely measured uninsured losses to some households. We show that, if unmet, sudden liquidity needs can have long-term financial implications including increased likelihood of bankruptcy three years after a negative shock.

Our findings advance the literature on the importance of credit as a tool to alleviate household financial distress. Classical models illustrate that households can smooth unanticipated shocks through borrowing (e.g., Friedman, 1957), which may help explain why many households, even relatively well-off ones, appear to live hand-to-mouth (Kaplan et al., 2014). However, in practice, adjustment frictions may limit households' ability to service additional debt. For example, some households turn to payday loans to manage emergency needs (Bhutta et al., 2015), yet these loans greatly increase borrower defaults on other loans and bank overdrafts in the ensuing months (Gathergood et al., 2019). Hurst and Stafford (2004) and Amromin et al. (2020) study the role of liquidity from home equity in smoothing household shocks. While the literature has explored households' tendency to pursue credit to manage adverse events, it is generally difficult to find a setting such as ours that delivers exogenous variation in credit provision to examine its effects. Our work provides some of the first causal evidence that a well-timed infusion of credit can reduce the likelihood of a household entering a debt spiral.

Our findings also offer novel evidence in the debate over whether public programs crowd out private investment (Aschauer, 1989). Research on place-based policies (Kline and Moretti, 2014) and industrial policy (Juhász et al., 2023) is generating new interest in this old debate. By allowing consumers to address their emergency expenses, disaster loans appear to affect household consumption and ultimately the real economy through the purchase of durable goods (e.g., an automobile) in the years following the disaster. This result parallels those regarding microcredit provision in developing countries, which also leads to modest credit crowd-in (Angelucci et al., 2015; Karlan and Zinman, 2019). We are thus able to conclude that government interventions to alleviate liquidity constraints can in some cases expand, rather than hinder, private credit markets.

Finally, our analysis extends the growing body of research examining the economic consequences of climate risk (e.g., Bernstein et al., 2019; Addoum et al., 2020; Keys and Mulder, 2020; Krueger et al., 2020; Lane, 2024). Forty-three percent of all U.S. homes – almost 36 million homes with a $6.6 trillion combined market value – are exposed to natural disaster risk (RealtyTrac, 2015). As the frequency and severity of disasters are increasing (Hsiang and Kopp, 2018), resulting in 28 billion-dollar disaster events in 2023 alone (NOAA, 2023), furthering our understanding of how households are affected by climate is a growing priority. Gallagher and Hartley (2017) investigate the consequences of Hurricane Katrina on household balance sheets. Gallagher et al. (2023) study the financial impact of living in the path of a tornado, and the extent to which disaster aid programs can mitigate harmful effects. We assess a large and important federal disaster lending program (Collier and Ellis, 2024; Collier et al., 2021; Billings et al., 2022; Begley et al., 2024). Collier et al. (2024) examine the effect of disaster loans on businesses: A key distinction between households and firms is that the latter are expendable, creating different policy priorities. By matching detailed disaster loan applications with consumer credit reports for the first time, we provide novel estimates of the causal effects of receiving an emergency loan in the aftermath of a disaster. We show the benefits of liquidity provision precisely when it is most needed, a situation which is increasingly relevant as natural disasters become more frequent and severe.

2 Data and Setting

This section describes the Federal Disaster Loan (FDL) program and our data, using material from FEMA (2019) and the program's Office of Disaster Assistance (2018).

2.1 Federal Disaster Loan Program Overview

Since its inception in 1953 through 2019, the FDL program has made roughly $60 billion in recovery loans. Administered by the Small Business Administration (SBA), the program is authorized to lend to households for the repair of uninsured damages to their primary residence, its contents (e.g., appliances, furniture), and their automobiles. Although it predominantly lends to households, the program also lends to businesses and non-profits. In 2017, households comprised 80% of applicants and 70% of the total loan volume. We limit our analysis to household lending.

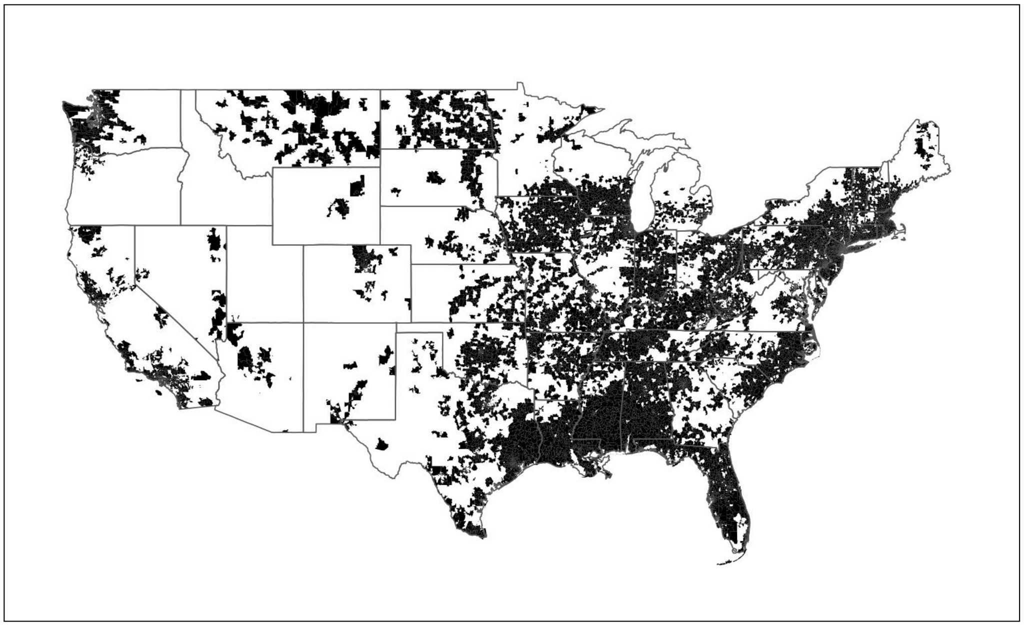
Effectively all (98%) household FDL applications are associated with a presidential disaster declaration. For these declarations, FEMA coordinates the local response, establishing temporary offices in affected neighborhoods. Households harmed by the disaster are encouraged to register with these FEMA offices. Households with incomes below a certain threshold (typically 125% of the federal poverty line) are referred to a FEMA grant program, which pays to repair or replace their lost property. FEMA refers households above the income threshold to the FDL program to apply for a loan. These households are then automatically contacted (via email, robocalls, and letters) by the FDL program.

A household's eligibility depends on the issuance of a disaster declaration for its county, incurring a loss from the disaster, and some portion of the loss being uninsured. Figure 1 shows the geographic distribution of the program, and illustrates its broad use across the contiguous U.S. with an emphasis on the Gulf and South Atlantic coasts. The black areas in the figure denote zipcodes that have at least one applicant in the program during our period of study between 2005 and 2013.

2.2 Data and Summary Statistics

Our data include all household FDL applications from January 1, 2005 to December 31, 2013 in the 50 U.S. states and the District of Columbia. During that time, the program received 314,560 completed applications and lent to 154,614 households. The program collects information on an applicant's income from the IRS, outstanding debts from credit reports, and property damages

Figure 1: Zipcodes with FDL Borrowers



Note: The figure shows zipcodes (in black) that have at least one applicant in our sample.

from an onsite loss inspection. Table 1 describes the income, credit scores, and existing debt-service-to-income (DTI) ratios of applicants and borrowers. The average credit scores of borrowers is 691, below that of Government-Sponsored Enterprise (GSE) mortgage borrowers, but around the national average. The average borrower has a DTI of 30%, around the same as GSE mortgage borrowers during this time (Fannie Mae, 2019; Freddie Mac, 2019).

The program can only lend to repair uninsured damages and monitors insurance claims for approved applicants. Insurance settlements cover 10% of the loss for the median borrower (Table 1). Gaps in flood insurance coverage are especially common: The CBO (2019) estimates that 84% of residential flood losses are uninsured. A number of frictions in the National Flood Insurance Program (NFIP) appear to contribute to low take-up (e.g., outdated maps, inaccurate pricing, low offered coverage limits, see e.g., Kousky, 2011; Billings et al., 2022; Mulder, 2021). For example, the NFIP has a maximum coverage limit of $250,000 on the home structure and does not tend to cover basements.[2]

[2]Whether government assistance, including through recovery loans, contributes to low insurance take-up is an important question that our paper does not address. However, we expect that any distortion created by recovery loans is small. Access to a recovery loan is uncertain: they require a federal disaster declaration so are only available for large floods and around half of loan applicants are denied. Ultimately, federal assistance only addresses 17% of flood losses (CBO, 2019). Given that 16% of flood losses are insured, the CBO (2019) estimates that two-thirds of residential flood losses are uncompensated.

Table 1: Summary Statistics of Federal Disaster Loan Borrowers

	Mean	SD	Percentiles p10	p50	p90
All Applicants					
Income	71999	49588	30744	57509	127147
Credit Score	693	84	575	696	801
DTI	0.33	0.28	0.05	0.30	0.59
Loss Amount	80765	102338	9701	44500	197990
Indicator for Any Delinquent Loan	0.14	0.35	0.00	0.00	1.00
Amount Past Due on Delinquent Loans	351	5900	0	0	0
New Bankruptcy in Last 6 Months	0.00	0.07	0.00	0.00	0.00
Number of New Auto Loans	0.09	0.30	0.00	0.00	0.00
Borrowers					
Income	73761	47570	32172	60831	127416
Credit Score	691	77	586	690	795
DTI	0.30	0.24	0.04	0.30	0.51
Loss Amount	86283	97633	11114	47679	213259
Indicator for Any Delinquent Loan	0.12	0.33	0.00	0.00	1.00
Amount Past Due on Delinquent Loans	198	3134	0	0	0
New Bankruptcy in Last 6 Months	0.00	0.05	0.00	0.00	0.00
Number of New Auto Loans	0.10	0.31	0.00	0.00	0.00
Interest Rate	2.72	0.75	1.69	2.69	2.94
Loan Duration (Months)	236	127	60	280	360
Monthly Payment	229	230	57	148	526
Loan Amount	42121	55777	8100	14000	120364
SBA Loan Charge-off	0.03	0.18	0.00	0.00	0.00
Insurance Settlement Amount	37247	71885	0	4518	130600

Note: Table includes data on 314,560 applicants and 154,614 borrowers for whom data on all variables listed are available. Income, credit score, DTI, and loss amount are collected at the time of loan application. Credit variables for delinquency, bankruptcy, and new auto loans are measured using the Experian data in the period immediately prior to disaster declaration. Loan duration, monthly payment, loan amount, and charge-offs are characteristics of the SBA disaster loan and thus only observed for borrowers. Appendix A provides variable definitions.

2.3 Lending Decisions

The program is "a good faith lender and will only make a disaster loan if there is reasonable expectation that the loan can be repaid" (SBA, 2020). Lending decisions largely depend on the interaction of the applicant's credit score and *existing* DTI ratio (that excludes the new disaster loan). While the rules vary over time, the program generally approves applicants with a credit score of at least 620 and an existing DTI below 40. We discuss the role of the DTI threshold in detail below. Approximately 50% of all applicants are approved.

The program can lend up to $200,000 for damages to the residence and up to a combined total of $40,000 for damages to their contents and automobiles. The average loan amount is $42,121 (median of $14,000) with a 2.72% interest rate, 20 year maturity, and $229 monthly payment (Table 1). While the program does not make lending decisions based on borrower collateral, it requires borrowers to secure their loans with available collateralizable assets if the loan is above a certain amount (e.g., $25,000 as of 2024).[3] During our period of study, borrowers almost always used loan funds to repair the property to its pre-disaster state; however, disaster loans also can be used for relocation or to mitigate property risk.[4]

The program allows for loans to be adjusted in cases of hardship by suspending payments and/or extending the loan's maturity, though interest on the loan continues to accrue during a deferment (Federal Register, 1997). For example, all disaster loans were granted an automatic deferment during COVID-19 (SBA, 2021). The program takes the following actions if the borrower defaults: First, the program transfers the delinquent debt to the Treasury Offset Program, which garnishes a portion of funds (e.g., tax refunds and social security payments) typically paid to an individual to pay down the loan balance (Treasury Offset Program, 2021). Second, the program reports the default to the credit bureaus, who register it as charged-off federal debt. Third, if the loan is collateralized, the program "may liquidate collateral securing a loan" (Federal Register, 2014). The disaster loan charge-off rate within three years of the event is 3% (Table 1). The charge-off rate over the life of the disaster loan is 14% in our sample.

2.4 Consumer Credit Reports

To conduct our analysis, we merge the application-level data from the FDL program to credit report data provided by Experian, a national credit bureau and global information services company. Our merge was conducted using loan application information such as name, address at time of disaster, and Social Security number, and thus yielded a 98.8% match rate. Individual records are linked over time through a unique, anonymous consumer identification number assigned by Experian. To form our analysis sample, we take the applicants' records in June and December of each year from 2003 to 2015, seeking a balanced panel that runs approximately 18 months prior to the disaster to 3 years after.

[3] Collier, Ellis, and Keys (2021) examine how the program's discontinuous collateral requirements affect consumers' borrowing and repayment decisions.

[4] In our sample, 4% of borrowers use the disaster loan to relocate by purchasing a different home and 1.5% take additional funds to mitigate property risk. Since 2022, the program has piloted initiatives to increase take-up of mitigation funding (SBA, 2022).

The credit report data includes information by broad loan category (e.g., mortgages, autos, credit cards) on the number of loans, total loan balances, total credit limits, and delinquency status. Delinquency indicators separately report the number of loans that have payments past due by more than 30, 60, 90, or 180 days or that have been sent to collections agencies. We also observe the total dollar amount of balances that are past due. Each applicant's zipcode is provided with every record update, allowing us to track whether households move in response to a disaster. Finally, we observe any new bankruptcy filings in the previous six months and a flag for prior bankruptcies in the last seven years. Table 1 provides summary statistics for the main credit report variables that we use in our analysis.

3 Estimation Strategy

3.1 Identification and Empirical Specification

Our goal is to estimate the causal effect of access to low-interest credit on financial health during a time of need. We focus on disaster loan applicants, which ensures that these households have suffered a verified and uninsured loss from the disaster. To motivate our empirical approach, we consider the following event study difference-in-differences (DiD) specification for the evolution of household financial outcomes:

$$y_{it} = \sum_{h=-a}^{b} \beta_h(1[t = h] \times approved_i) + \psi_{zd} + \tau_{td} + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome for applicant i at time t who was living in zipcode z when disaster d struck. Time t is in event time and β_h are the event time coefficients of interest. We observe applicants' credit records every 6 months, starting approximately 1.5 years before the disaster (a) to 3 years after the disaster (b). $approved_i$ is a binary variable for whether the loan was approved, a and b are the number of leads and lags included, $1[\cdot]$ is the indicator function, and ϵ_{it} is an error term.[5]

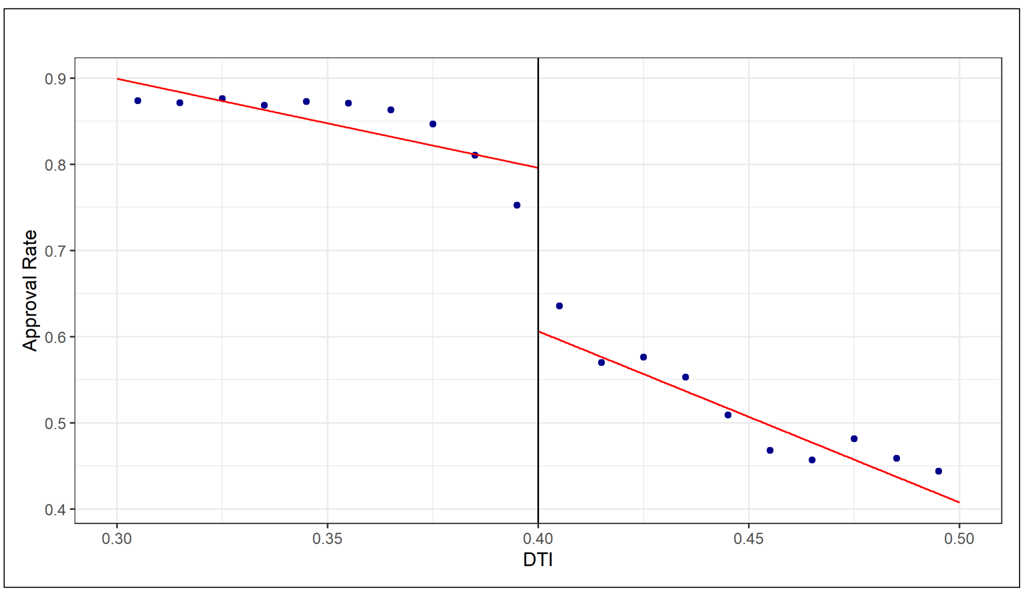
The ψ_{zd} and τ_{td} represent zipcode by disaster and calendar time by disaster fixed effects, respectively. Fitting zipcode and time fixed effects separately for each disaster creates clean controls for each event and is sometimes described as a "stacked" difference-in-differences regression (Cengiz et al., 2019; Baker et al., 2022).

[5] In practice, we use the period prior to the disaster ($t = -1$) as a reference period in β_h and include an un-interacted $approved_i$. With this structure, the β_h coefficients reflect deviations from the reference period. Details are shown in Appendix B.

The challenge with estimating this event study specification with ordinary least squares (OLS) is that even after controlling for the information included in the loan application (which we can observe), loan approval is likely still correlated with soft information that may be observable to the loan officer but not to the econometrician.

To overcome this endogeneity problem, we exploit a discontinuity in the probability of having an SBA disaster loan application accepted. The probability of loan acceptance is discontinuously higher for applicants whose DTI ratio is just below 0.4 compared to applicants whose DTI ratio is just above. Figure 2 shows a clear discontinuity of nearly -20 percentage points crossing from below to above the DTI threshold: Those applicants a few DTI points below 0.4 have an 80% chance of approval, while those a few DTI points above have a 60% chance of approval that steadily declines as DTI increases above 0.4. This threshold was codified into the SBA's lending handbook from 2005 through 2011 and applies to all households with pre-disaster incomes above $25,000.

Figure 2: Change in Approval at the Debt-to-Income Threshold, 2005–2011



Note: The figure shows the discontinuous change in the likelihood of loan approval for households below and above the DTI threshold of 40 percentage points. Each point represents one percentage point of the DTI distribution.

The DTI threshold serves as an instrument for approval, which we implement in a two-stage least squares (2SLS) model. The second stage is

$$y_{it} = \sum_{h=-a}^{b} \beta_h \widehat{(1[t = h] \times approved_i)} + \gamma_0 DTI_i + \gamma_1 (DTI_i \times below_i) + \psi_{zd} + \tau_{td} + \epsilon_{it} \tag{2}$$

The DTI_i terms control for the running variable, which is the DTI ratio on the application. $below_i$ is an indicator for whether $DTI_i < 40\%$. These DTI controls are linear; we explore other specifications for robustness in Section 4.1.

For each value of event time h in the summation above, the first stage equation is

$$\begin{aligned}(1[t = h] \times approved_i) = \sum_{h=-a}^{b} \alpha_h (1[t = h] \times below_i) \\ + \delta_0 DTI_i + \delta_1 (DTI_i \times below_i) + \phi_{zd} + \kappa_{td} + \mu_{it}\end{aligned} \tag{3}$$

resulting in h first stage equations (h endogenous variables, and h instruments), yielding a just-identified system of equations. Standard errors are robust and clustered at the zipcode-by-disaster level. See Appendix B for additional details.[6]

Our approach thus combines a fuzzy regression discontinuity with difference-in-differences, and must satisfy the identification assumptions associated with each design. The 2SLS estimation using DTI as an instrument provides the local average treatment effect (LATE) of loan approval under the assumption of monotonicity: Having a DTI below the threshold weakly improves the likelihood of approval across all applicants (i.e., no defiers). This assumption is supported by the threshold being codified in the program's underwriting rules (Office of Disaster Assistance, 2018). A test for potential manipulation of DTI (the running variable) is discussed and provided below. Our strategy further requires the exclusion restriction to hold, namely that conditional on model controls, including for the running variable, the 40% DTI *threshold* does not directly and discontinuously influence private access to credit. The DiD design offers an additional advantage in that it relies on the identifying assumption of parallel trends between those applicants just above and below the DTI threshold in the absence of treatment. The presence of pre-trends may signal a violation of this assumption.

[6] Cellini et al. (2010) employ a somewhat similar empirical strategy combining RD and difference-in-differences.

3.2 Sample Restrictions

We restrict our sample of loan applicants in two ways. First, we limit our sample to applicants who have a DTI near the 0.4 threshold, specifically, between 0.3 and 0.5.[7] Second, we keep only applicants for whom the DTI threshold affects approval decisions: applicants experiencing disasters during the years 2005–2012 and whose pre-disaster incomes exceeded $25,000.

Figure 3 shows applicants' characteristics (e.g., credit score, age, amount requested, delinquencies, etc.) across levels of DTI in the analysis sample. Many of these characteristics appear unrelated to DTI such as the applicants' age and history of bankruptcy. Other characteristics vary smoothly with DTI. For example, applicants with lower DTIs request larger disaster loans, which may result from better off households having both lower DTIs and more valuable property that was damaged. Importantly, none of the characteristics appear to change discontinuously at the DTI threshold. Combined with Figure 2, these plots show that the underwriting process used this threshold to discontinuously and differentially evaluate otherwise similar applicants.

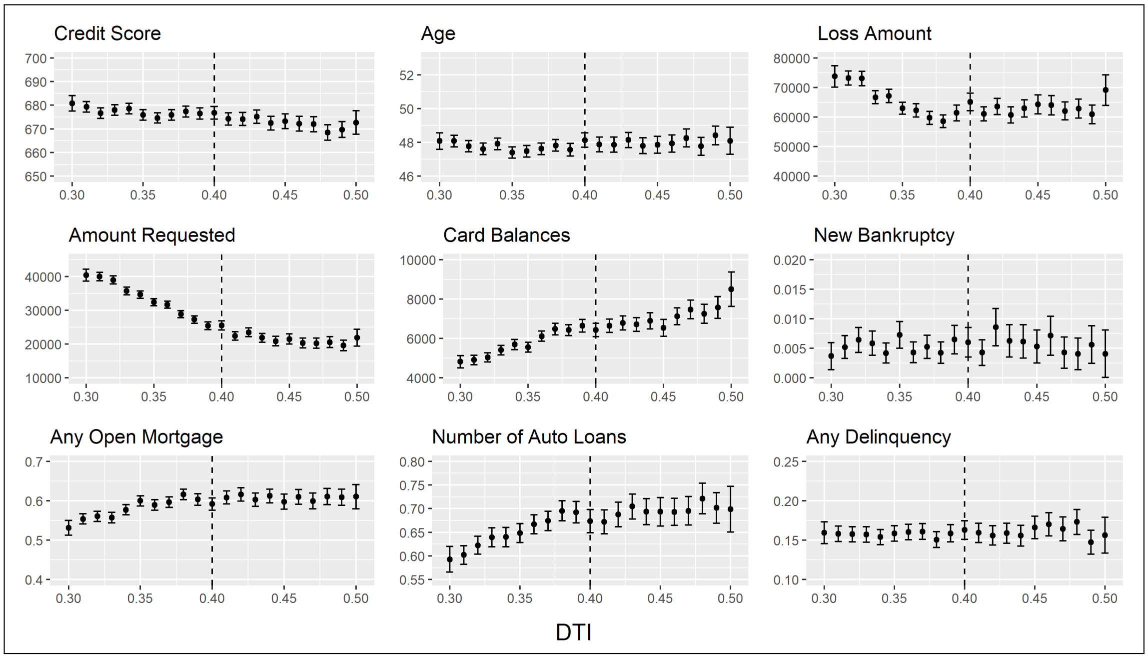
In Appendix Figure C1, we also show that the distribution of DTI on applications is smooth through the 40% cutoff. We test for bunching in the running variable (McCrary, 2008) using the procedure in Cattaneo et al. (2020) and find no evidence of manipulation, with a p-value of 0.14.[8] This pattern is to be expected, as applicants were unaware of the use of the 40% DTI threshold as a rule of thumb in guiding loan approval decisions, and the DTI ratio is calculated by the loan officer with administrative data on incomes from the IRS and debts from the credit record. In other words, at no time do applicants calculate their own DTI ratios or know that a particular value of the ratio is meaningful, ruling out many potential ways in which the exclusion restriction could be violated (e.g., financially sophisticated applicants boosting their chances for loan approval by manipulating their DTI). The lack of bunching in the running variable also helps to rule out the possibility that loan officers manipulate applications around the threshold.

We provide additional detailed summary statistics on this analysis sample in Appendix C. Compared to the full sample of applicants, the analysis sample is qualitatively similar but has slightly lower incomes (median $56,000 versus $58,000) and credit scores (median 676 versus 696) and higher rates of credit delinquency (16% of applicants versus 14%).

[7]Section 4.1 presents evidence showing that our results are robust to modifying the DTI bandwidth.

[8]Depending on the degree of polynomial used, p-values for the test of manipulation of the running variable range from 0.14 to 0.66.

Figure 3: Applicant Characteristics by DTI



Note: Figure includes data on 137,334 applicants with debt-to-income ratios between 0.3 and 0.5. Each panel shows the average value of various applicant characteristics by bins of debt-to-income at loan application. All outcomes are measured in the period before disaster declaration. See Table 1 note for details on variable definitions.

4 Results

4.1 Do Recovery Loans Reduce Household Financial Distress?

We begin by examining whether the provision of an emergency loan affects the likelihood that a household experiences financial distress. Our measures of distress include bankruptcy filings and credit delinquencies on the disaster loan applicant's credit report. *A priori*, the direction of the effect is unclear. By increasing balance sheet leverage and committing more of the household's cash flows to servicing debt, emergency borrowing unambiguously increases standard metrics of the household's credit risk. Thus, by providing a large lump sum, disaster loans may reduce distress immediately following the disaster; however, borrowing households might be unable to sustain their additional debt burden over time, potentially increasing distress in the years after the event. On the other hand, emergency liquidity may allow a household to meet its immediate consumption needs—to repair or replace its home and other key durable goods. The benefits of doing so may outweigh the additional risks normally associated with increasing household debt.

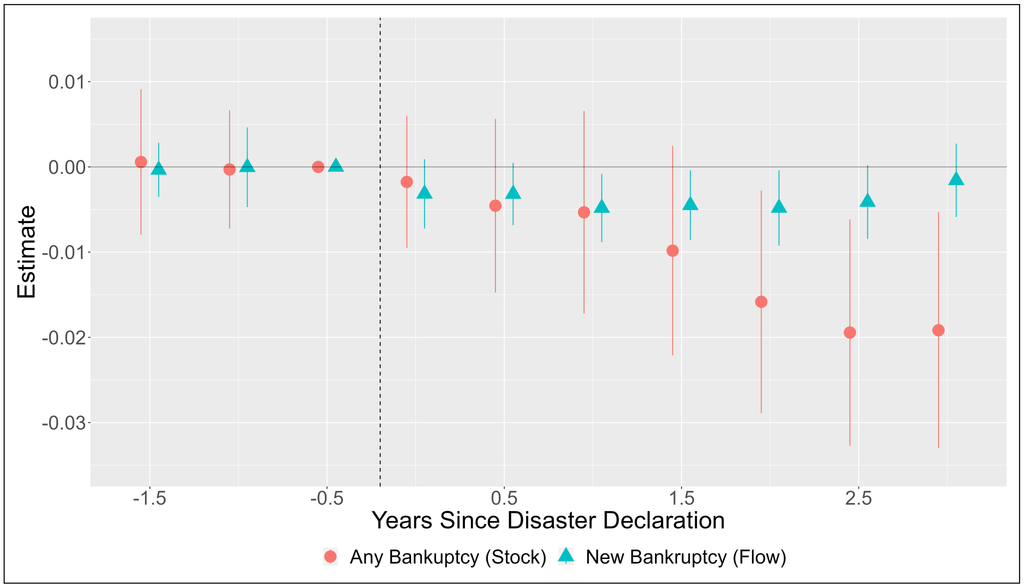
Bankruptcy. We use two measures of bankruptcy in the analyses. The first is a "flow" variable, indicating whether the applicant filed for bankruptcy in the last 6 months. The second is a "stock" variable, which is cumulative, describing whether the applicant had filed for bankruptcy by a given date. The stock version of bankruptcy takes a value of 0 and then permanently switches to a value of 1 if the household files for bankruptcy in a given period.

Figure 4 shows 2SLS estimates of Equation (2) for the effect of loan approval on the likelihood of filing for bankruptcy. The first stage Cragg-Donald F-statistic in the estimation is 962, indicating that the DTI threshold is a strong instrument for loan approval. The periods in the figure are 6-month intervals, starting 1.5 years prior and ending 3 years after the disaster. The last observation before the disaster, $t = -0.5$ years, serves as the reference period in the regressions. For example, Hurricane Irene made landfall in August 2011 so $t = -0.5$ represents the June 30, 2011 credit reports of households applying because of that event, while $t = 0$ represents the December 31, 2011 credit report immediately after the event. There is no evidence of a pre-trend for either the flow or stock bankruptcy measure, consistent with an interpretation of our experimental design creating exogenous variation in loan approval near the DTI threshold.

We find that loan approval significantly reduces bankruptcies. The flow measure, shown with blue triangles, indicates that disaster loans significantly reduce the likelihood of filing, beginning one year and continuing until three years after the disaster. In each of these periods, the loan reduces the likelihood of filing by 0.5 pp. The stock measure, shown with red circles, indicates that these effects accumulate over time. In the three years after the disaster, 3.3% of declined applicants file for bankruptcy. Loan approval reduces the likelihood of having a bankruptcy by 2 pp, which translates to a 61% reduction in bankruptcy likelihood after the disaster. The sustained reduction in bankruptcies indicates that the benefits of disaster loans continue even after households have been servicing the additional debt for many months. Thus, recovery loans appear to persistently reduce the likelihood that a household files for bankruptcy due to the disaster.

How large is a 2 percentage point decline in bankruptcy over three years? We compare this estimate to other papers using bankruptcy as an outcome when studying the costs of various shocks. For instance, Dobkin et al. (2018) examine bankruptcy filing rates after unanticipated hospital admissions and estimate that uninsured non-elderly hospital admissions are associated with an increased bankruptcy probability of 1.4 pp, while insured hospital admissions increase the probability by 0.4 pp. Billings et al. (2022) find an 0.8 pp increase in bankruptcy filing after Hurricane Harvey for low-income residents outside of the flood plain, a proxy for being uninsured against flood risk. Keys (2018) estimates that job displacement for males increases bankruptcy likelihoods by 1.4 pp in the subsequent three years. These comparisons to the literature, while limited, suggest that the reduction in bankruptcy filing due to loan receipt is substantial.

Figure 4: Effect of Loan Approval on Likelihood of Bankruptcy



Note: The figure shows the local average treatment effect (LATE) of receiving a disaster loan on the likelihood of having a bankruptcy (stock) and on the likelihood of having a new bankruptcy in the past 6 months (flow) on the credit report using the DTI threshold as an instrument for approval. The estimation follows Equation (2).

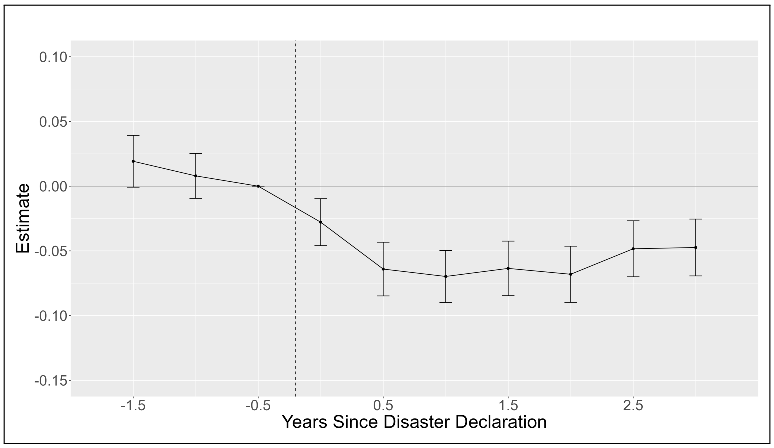
Credit Delinquencies. How does loan approval lead to such large reductions in bankruptcy? It is possible that on experiencing a natural disaster and being rejected for a recovery loan, households might immediately appreciate that their existing debt is unsustainable and requires restructuring through bankruptcy. Indeed, the bankruptcy results suggest that some households file almost immediately: While not statistically significant at the 5% level, the treatment effect estimates are negative beginning in the first period after the disaster (Figure 4).

For other households, distress may build over time. Households struggling to make ends meet while repairing their property may fall behind on payment obligations, leading to mounting delinquencies and their associated penalties. The largest bankruptcy effects in Figure 4 occur 2 years after the disaster, suggesting that they reflect mounting distress.

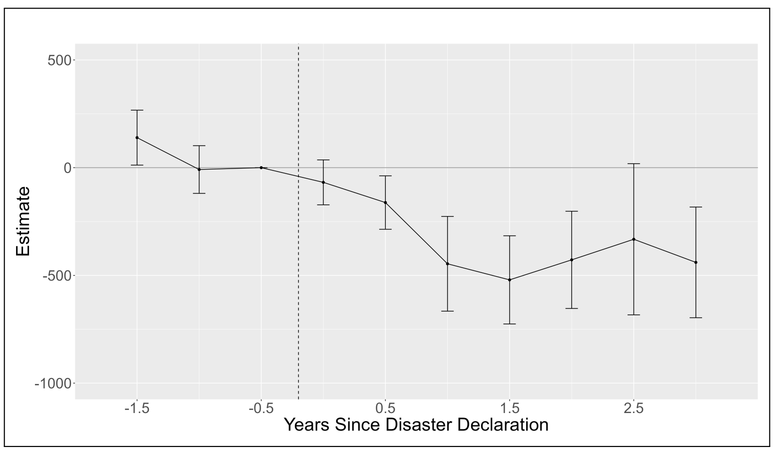
Delinquencies on consumers' credit reports offer insights regarding this potential path to bankruptcy. Figure 5 presents the effect of loan approval on private credit delinquencies. Panel (a) shows the likelihood of having any delinquent or derogatory debt, while Panel (b) shows the effect of loan approval on the total dollar amount of delinquent or derogatory debt. These measures reflect loan payments that are 30 or more days late.

Figure 5: Effect of Loan Approval on Delinquency

(a) Likelihood of Delinquency



(b) Amount Past Due on Delinquent Loans



Notes: The figure shows the treatment effect of loan approval on (a) the likelihood of having any loan 30 or more days late and (b) the amount past due. Both estimates use the DTI threshold as an instrument for approval. The amount past due is the balance that households owe in late loan payments. The estimation follows Equation (2).

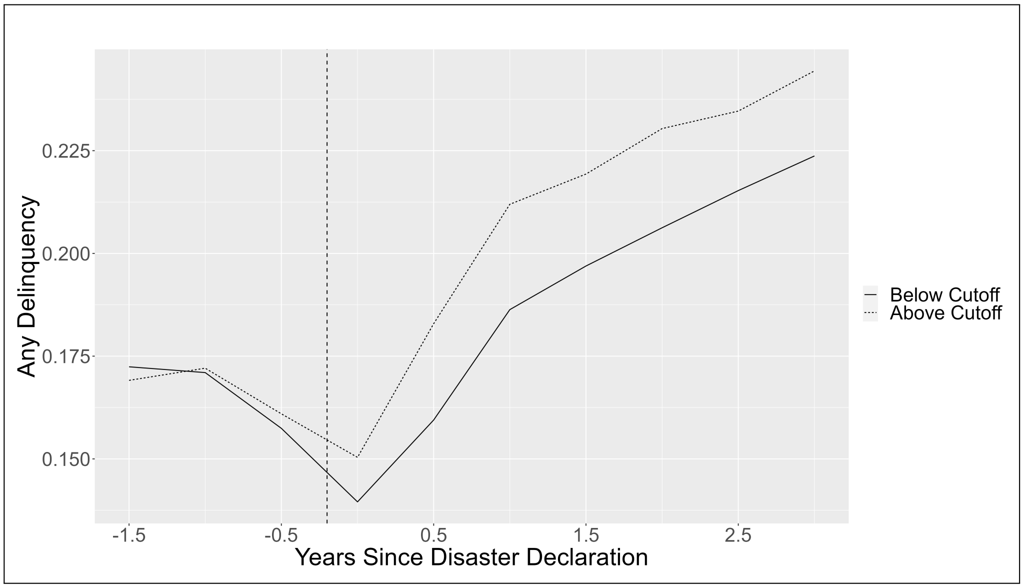
Loan approval has a large and persistent effect on the likelihood of delinquency beginning just after the disaster and continuing for the full estimation period, which ends 3 years later (Panel (a)). For example, 1.5 years after the disaster, approval reduces the likelihood of delinquency by 7 pp. Panel (b) similarly shows that, on average, loan approval reduces the amount delinquent on private balances by roughly \$500 as of 1.5 years after the disaster. For reference, at this time 21% of the control group had at least one delinquency and the average delinquent balance was \$977.

To facilitate interpreting these treatment effects, Figure 6 illustrates that both treated and control populations more frequently experienced distress following the disaster. The figure shows the share of consumers with credit delinquencies over time, using the analysis sample (applicants whose DTIs are within 10 pp of the threshold). The solid line shows applicants with DTIs between 30 and 40 (the intent-to-treat population); the dotted line shows applicants with DTIs between 40 and 50. Immediately following the disaster ($t = 0$), delinquencies decline. Some lenders offer loan forbearance following natural disasters, which may explain this temporary decline (Overby, 2007; New Jersey Department of Banking and Insurance, 2012). Delinquencies then increase monotonically for both groups, illustrating the financial challenges that many households experience during the post-disaster recovery period. For example, around 17% of control households had credit delinquencies just prior to the disaster, while 24% were delinquent 3 years after the event.[9] Appendix D shows similar conditional means plots for the other distress outcomes. Thus, the local average treatment effects (LATEs) in Figure 5 reflect that loan approval reduces, but does not fully eliminate, the mounting distress experienced by households who do not receive a disaster loan.

Delinquencies by Loan Type. For what loan types do disaster loans change repayment behavior? We next examine the effects of recovery loans on delinquencies across different credit products: mortgages, auto loans, and credit cards. This analysis offers insights into how households prioritize different payments, the so called "pecking order" of defaults, given that the consequences of delinquency vary by credit product (Andersson et al., 2013). Specifically, falling behind on credit cards results in high interest rates and penalty charges but, since they are unsecured debt, no repossession of collateral. Mortgage defaults can have large financial implications because households' wealth is concentrated in the home, but repossession of the home through the foreclosure process is slow. Auto loans are marked by much quicker repossession in the event of default.

[9]The figure plots raw means and thus does not incorporate the rich set of fixed effects and model controls in our regressions. However, the difference between the two groups approximates the effect in an intent-to-treat regression design, regressing the delinquency indicator (y) on an indicator for the DTI threshold (z). The intent-to-treat effect is the numerator in the instrumental variables Wald estimator, where the denominator is the effect of the DTI indicator on loan approval (x, i.e., the first-stage regression in our 2SLS). Thus, the intent-to-treat effect shown here, divided by the first-stage effect of the threshold on approval, approximates the LATEs shown in Figure 5, $\theta_{\text{Wald}} = (dy/dz)/(dx/dz) \approx 0.02/0.2 = 0.10$.

Figure 6: Conditional Means, Delinquencies



Note: The figure shows the share of households with any loan delinquent by 30 or more days, for two groups based on debt-to-income at loan application. The data are restricted to the analysis sample, only households with DTIs between 0.3 and 0.5 are included. The solid line represents households with DTI below the threshold of 0.4, who are significantly more likely to be approved for a disaster loan compared to those above the DTI threshold (dashed line).

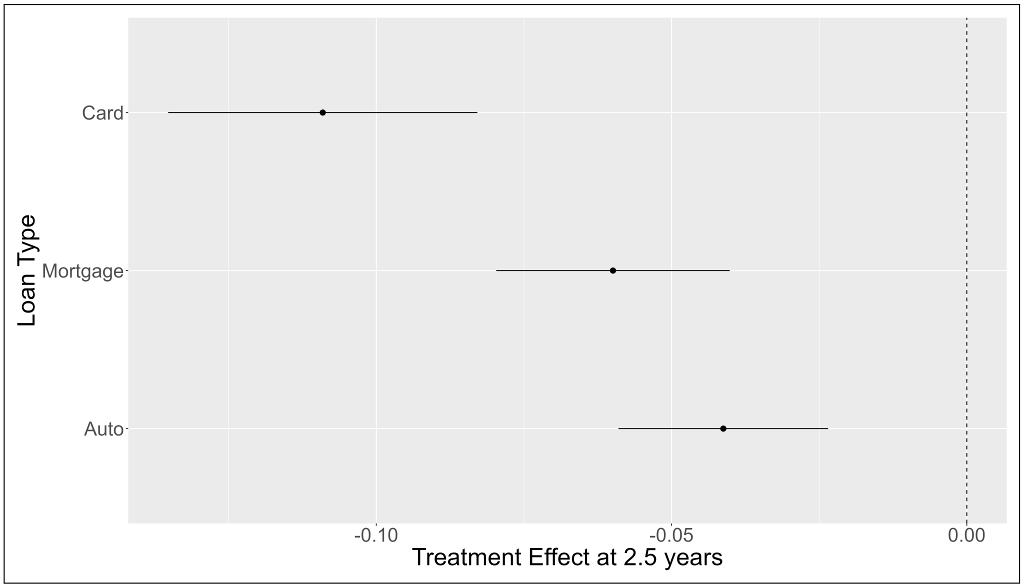
Figure 7 shows the effect of loan approval on delinquency as of 2.5 years after a disaster declaration. Delinquency is measured using an indicator for ever having a delinquency of that loan type in the last 24 months. The sample is restricted to households with at least one mortgage, auto, and credit card loan prior to disaster declaration. This sample restriction helps clarify questions around pecking order: Households in this sample must prioritize which payments to make if they are unable to meet all of their credit obligations.[10]

Since disaster loans help stave off delinquency, one would expect the largest beneficial effect for the product that people tend to stop paying first and the smallest effect for the product that people continue to make payments on no matter the circumstances. We find that the largest effect of loan approval is on credit card delinquencies, resulting in an 11 pp decline in the likelihood of having at least one delinquency, followed by mortgages (6 pp decline), and finally auto loans (4 pp decline). Our finding of smaller treatment effects for auto loans suggests that consumers prioritize making payments on these loans, potentially because the threat of repossession is most pressing for these credit products (Assunção et al., 2014; Adams et al., 2009). Consumers appear

[10]This restricted sample represents 29% of the analysis sample. That said, removing this sample restriction so that we do not require households to have all three credit products (mortgages, auto loans, and cards) yields qualitatively similar results.

to prioritize mortgage repayment next, which may reflect concerns about losing home equity, maintaining a place to live, and the psychological attachment to a home (Collier et al., 2021). Credit cards are the lowest priority, resulting in the largest treatment effects for these products.

Figure 7: Effect of Loan Approval on Likelihood of Delinquency, by Loan Type



Note: The figure shows the treatment effect of loan approval on the likelihood of having a delinquency for various loan types. The data are restricted to individuals with at least one of each type of loan prior to the disaster. The outcome variables are indicator variables for having had a delinquency of a given type in the last 24 months. Estimations follow Equation 2 and report the treatment effect for period $t = 2.5$ (i.e., 2.5 years after the disaster).

Delinquencies by Borrower Type. Next, we consider whether the delinquency results exhibit heterogeneity by borrower credit score (prior to the disaster), a proxy for the household's access to liquidity. Our estimates for the full sample may mask important variation in response to loan receipt, especially if the primary mechanism through which loan receipt is beneficial is a liquidity channel. In this analysis, we split the sample by credit score at the time of loan application within the same disaster and focus on comparing outcomes for the top and bottom quartiles.

The top two panels of Figure 8 plot the mean propensity of having any type of loan delinquency in the lowest credit score quartile (top-left panel) and the highest credit score quartile (top-right) for the below cutoff, 30-40 DTI sample (solid lines) and the above cutoff, 40-50 DTI sample (dashed lines). As applicants with DTI below 40 are more likely to be approved, these plots provide a type of intent-to-treat comparison.

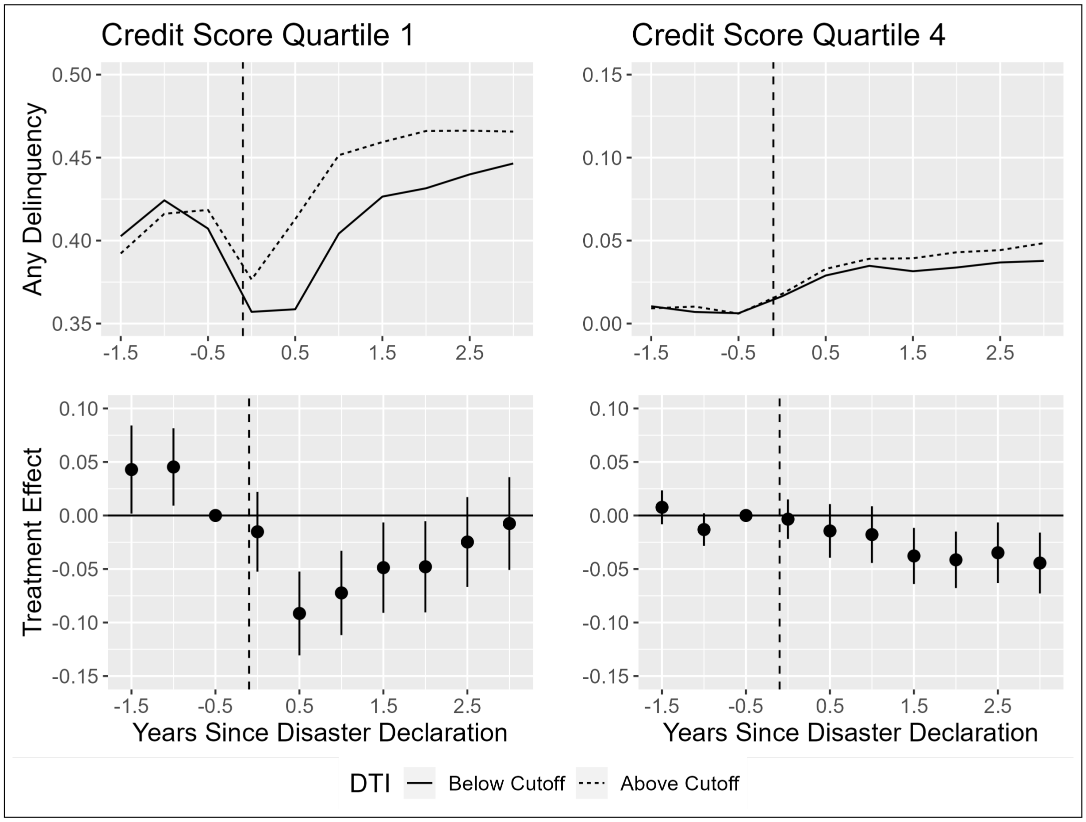
In the top-left panel of Figure 8, the lowest credit score quartile household delinquency rates are almost exactly equal in the year and a half before the disaster, but diverge sharply after the disaster. In contrast, for households with high credit scores (top-right panel), we see a more gradual and smaller separation in delinquency outcomes. For both DTI samples, delinquency outcomes are substantially higher two years after the disaster relative to their pre-disaster delinquency rates.

The bottom two panels of Figure 8 plot our estimates of the causal effects of loan receipt on the probability of having a delinquent loan separately for low and high credit score households. These LATE coefficients incorporate fixed effects and other model controls, scaling by the discontinuity in loan approval. The event study figure in the bottom-left reveals a large and persistent effect of loan approval on delinquency rates for the low credit score group, a reduction by 10 pp 6 months after the disaster. The response for the high credit score group (bottom-right) is more muted, but is starting from a base of nearly zero delinquency. The effect of loan receipt builds over time for this highest score quartile.

This heterogeneity across credit score quartiles supports the hypothesis that the loans are most beneficial to those consumers with heightened liquidity needs. That said, even the highest credit score quartile borrowers see clear benefits to loan receipt, with lower delinquency rates even three years after the disaster.

Robustness. To check the robustness of our findings, we examine the results under a variety of alternative specifications of the baseline estimating equation. Figure 9 shows how the effect of loan approval on the likelihood of bankruptcy varies across models. These specifications use the stock variable, which indicates whether a household has filed for bankruptcy by a given period, as the dependent variable. The first plot repeats our preferred estimation strategy for reference. The next plots adjust the model fixed effects and controls with individual fixed effects; no fixed effects; and controls for size of disaster loss, applicant income, and credit score. Next, we vary the baseline DTI bandwidth (10 pp) by expanding it to 20 pp and reducing it to 5 pp. The next plot omits the largest event in the data, Hurricane Katrina. The "donut" plot omits observations 2 DTI percentage points just above and below the threshold out of concern that some incentive to manipulate the data may exist at the threshold. The preferred model includes linear controls for the running variable (DTI); in the final plot we additionally examine quadratic controls for the DTI ratio. Across specifications, the results appear quite robust. Loan approval reduces the likelihood of bankruptcy by around 2 pp, although the exact timing and magnitude are influenced by the specification.

Figure 8: Heterogeneity by Credit Score



Note: The first row of the figure shows the average likelihood of having any loan 30 or more days late for the lowest within-disaster credit score quartile (left panel) and the highest credit score quartile (right panel), by DTI instrument status. The second row of the figure shows the LATE of loan approval on the likelihood of having any loan 30 or more days late, estimated separately for individuals with credit scores in the bottom (left panel) or top (right panel) within-disaster credit score quartile.

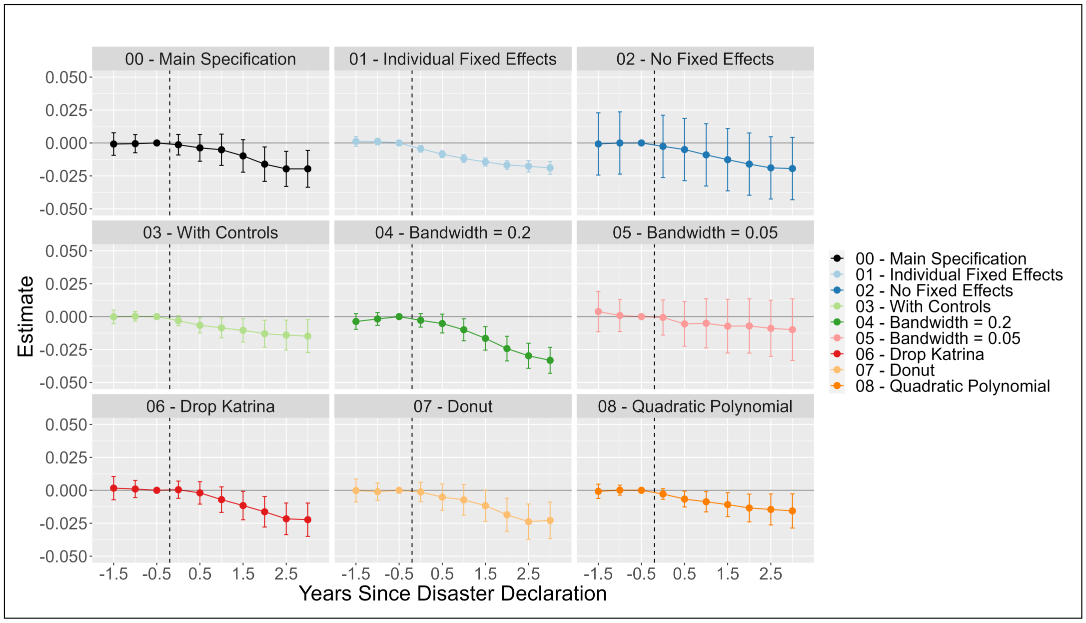
In Appendix E, we similarly examine alternative specifications for modeling the effect of loan approval on households' delinquency rates and amount past due on delinquent loans. In those cases as well, our main findings appear consistent across a variety of specifications.

We additionally show the effect of disaster loans on the likelihood of having a charge-off in the last two years (Appendix E). Charge-offs occur when a lender initiates a process to recoup funds through collections, repossession, or foreclosure on severely delinquent debt (e.g., loans that are in arrears more than 120 days). For this measure, we include both charge-offs of private loans from consumers' credit reports and disaster loan charge-offs.[11] By including disaster loan charge-offs, we address the possibility that approved applicants are shifting delinquencies from private loans

[11]The Treasury Offset Program (2021) manages collections for delinquent disaster loans. Federal loan programs are required to send delinquent debts to Treasury when they exceed 120 days past due. At this stage, the FDL program reports unpaid disaster loans to the credit bureaus as charged-off federal debt. The Treasury engages in a collections

to disaster loans. Similar to our results regarding delinquencies and bankruptcy, we find that disaster loans dramatically reduce the likelihood that households experience a charge-off during the recovery period: loan receipt reduces the likelihood of having any type of loan charge-off by 6 pp as of 1.5 years after the disaster.

Figure 9: Effects of Loan Approval on Bankruptcy, Alternative Specifications



Note: The figure shows the LATE of loan approval on the likelihood of filing for bankruptcy. The series of plots show how alternative specifications of the estimating equation affect the results.

4.2 How Do Recovery Loans Affect Households' Balance Sheets?

Emergency credit may have broader effects on households' balance sheets. On one hand, recovery loans might increase private debt balances. By helping consumers avoid costly coping strategies (e.g., bankruptcy or credit card delinquencies), disaster recovery loans could improve their ability to own a home, replace a vehicle, and remain in their community. For example, if disaster loans preserve home ownership, we would expect a positive effect of disaster loans on mortgage balances: Treated households would maintain a mortgage, but control households would not.

process, including possibly garnishing tax refunds and social security payments and seizing collateral used to secure the loan.

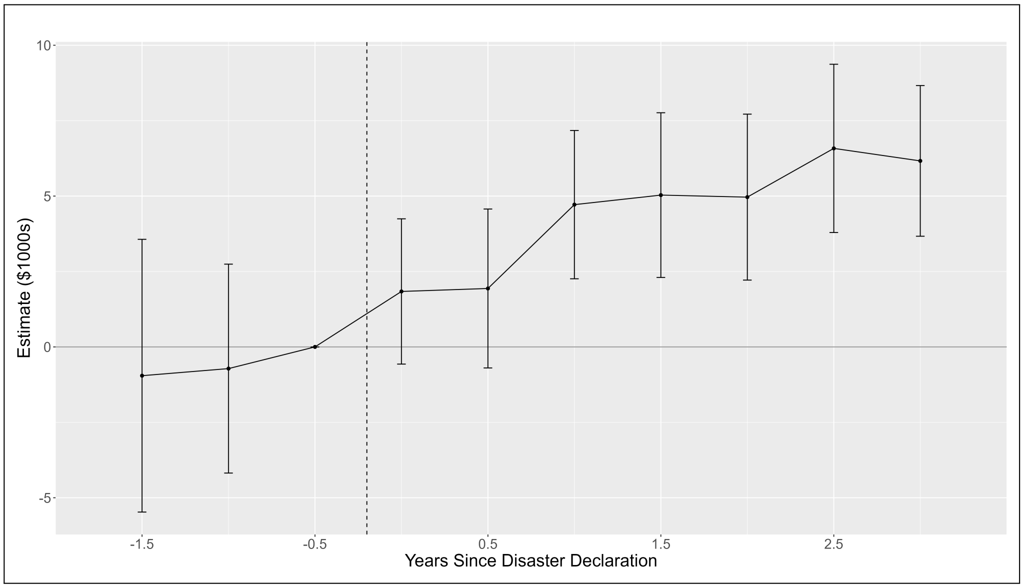
On the other hand, government-provided recovery loans might reduce households' private debt balances. A common critique of government lending is that it could crowd out private lending, encouraging households to substitute a (cheaper) government loan for the private loan they would have otherwise taken. If so, we would expect a negative effect of emergency credit on private debt balances: Control households would use private-sector loans to fund repairs while treated households would use a recovery loan.

Debt Balances. We first examine total debt balances, which includes all of the consumer's debts *except* for the disaster recovery loan, in Figure 10. Because balances contain a large amount of individual-specific heterogeneity that is captured by our panel data, we replace disaster-by-zipcode fixed effects and linear controls for DTI with individual fixed effects when estimating the response of balances. The figure shows no significant differences in balances prior to the disaster.

We find that receiving a disaster loan causally increases private debt balances. Starting around one year after the disaster, private balances are around $5,000 larger for treated households. Three years after the disaster, treated households have around $6,000 more in private debt. Below, we examine the effects of disaster loans on specific types of borrowing. Panel A of Figure 11 decomposes balances by loan type, reporting the event study treatment effect 2.5 years after the disaster. Auto, mortgage, and card balances are all significantly larger for treated households.

Automobile Purchases. Opening an auto loan is a common proxy for household consumption and well-being (e.g., Di Maggio et al., 2017; Beraja et al., 2019; Abel and Fuster, 2021; Berger et al., 2021; Scharlemann and van Straelen, 2024). Moreover, the ability to replace an aging or damaged automobile in the years after the disaster may be especially important since many individuals rely on a vehicle to travel to work. We examine the number of new auto loans opened by households in the last six months using the same event study 2SLS design from Equation 2. Figure 12 shows that disaster recovery loan approval causes a 5 pp increase in the share of households that took out a new auto loan in the year following the disaster. The effect is largest around a half-year after the disaster, likely because households are replacing vehicles damaged by the event. However, the effect remains significant even two to three years after the disaster, which likely reflects replacing an aged vehicle instead of disaster damage. This pattern is consistent with loan approval loosening credit constraints and stimulating private borrowing, rather than crowding out private borrowing.

Figure 10: Debt Balances (Excluding Disaster Loan)



Note: The figure shows the LATE of receiving a disaster loan on non-disaster-loan debt balances using the DTI threshold as an instrument for approval.

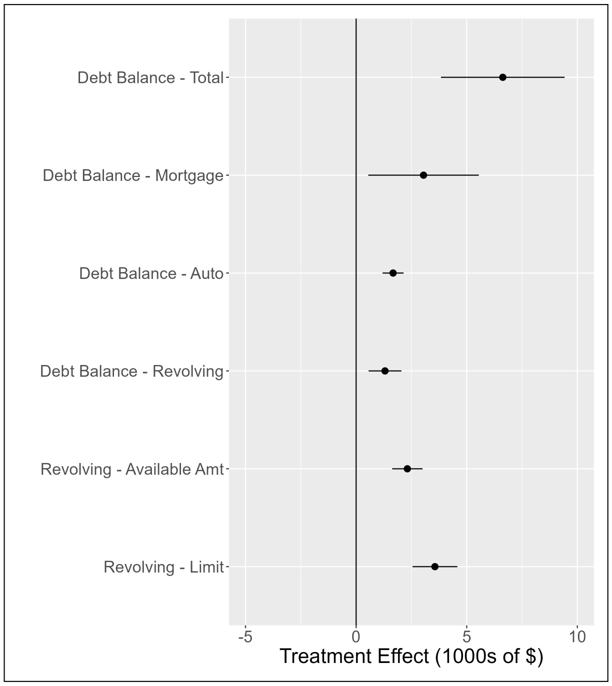
Revolving Balances. Disaster loans also increase consumers' revolving debt balances, which combines credit cards and HELOCs (Figure 11). The revolving balances of treated households are $1,200 larger than control households two-and-a-half years after the disaster. While this type of revolving debt would be concerning if consumers spend beyond their means, we have already shown that treated households have fewer card delinquencies, suggesting that these larger balances do not reflect distress spending (Figure 7).

We find a significant increase in revolving credit limits. Disaster loan receipt increases credit limits across all revolving loans by $3,800 as of 2.5 years after the disaster, while the amount of available revolving credit (i.e., limits minus balances) increases by $2,400. Similar to our findings regarding auto loans, these results suggest that disaster loans facilitate recovery and "crowd in" private credit supply in the form of higher revolving credit limits. Moreover, the increase in the amount of available credit suggests that disaster loans reduce the likelihood that households are liquidity constrained in the years after the event.

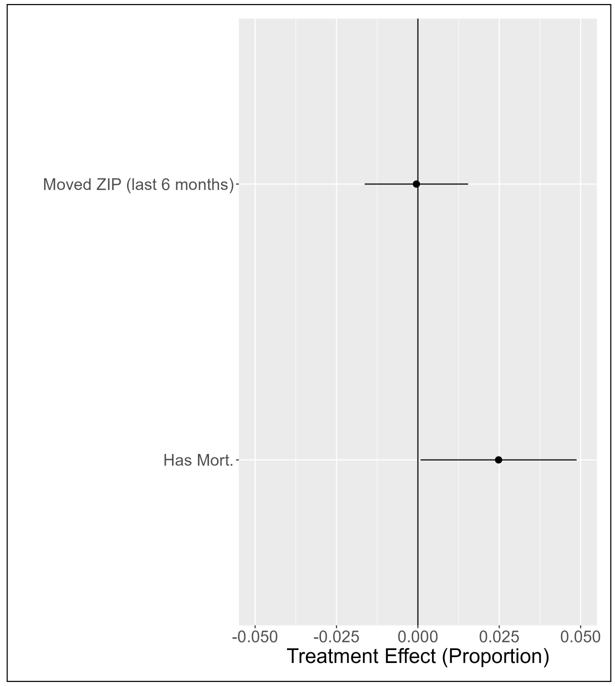
Mortgages and Homeownership. Household debt is typically concentrated in mortgages, and we find the largest treatment effects of emergency credit on mortgage balances, an average increase of $3,000 as of two-and-a-half years after the disaster (Figure 11). This difference in mortgage balances may reflect that households who cannot fund repairs to their homes may not be

Figure 11: Other Outcomes

(a) Balances by Loan Type



(b) Homeownership Proxies

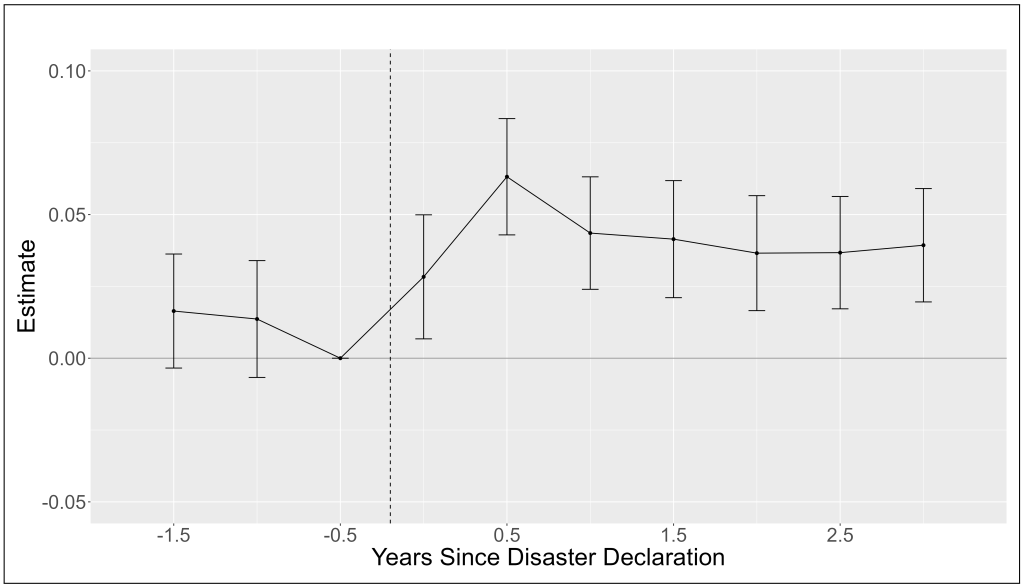


Note: The figure shows the treatment effect of loan approval on additional outcomes as of 2.5 years after the disaster declaration. For each outcome variable y, the figure displays the $\beta_{2.5\text{years}}$ coefficient from the second stage regression.

able to continue living there, perhaps requiring them to downsize or to become renters. We find a positive effect of disaster loan approval on an indicator for having a mortgage, a proxy for homeownership, of 2.5 pp (Panel b), or a 4% increase relative to the baseline that 56% of households in our sample had a mortgage prior to the disaster.

We find no evidence that receiving a disaster loan affects whether households remain in the same zipcode. Panel (b) of Figure 11 shows that disaster loans do not affect the likelihood that households have changed zipcodes in the last six months. We additionally find a null effect on whether households have relocated from the zipcode in which they lived as of 6 months prior to the disaster (i.e., their location at $t = -0.5$). This finding is somewhat surprising in that we might expect that disaster loans would help households remain in their community, especially given prior research that certain disasters, such as Hurricane Katrina, caused permanent outward migration (e.g., Deryugina et al., 2018). A potential explanation is that post-disaster moving decisions may be motivated by factors other than loan approval. For example, a subset of applicants might decide to relocate because of a job search, and this propensity to relocate may be continuous through the DTI threshold.

Figure 12: New Auto Loans



Note: The figure shows the LATE of receiving a disaster loan on new auto loans using the DTI threshold as an instrument for approval. The outcome is the number of new auto loans that households have taken out in the last 6 months. The estimation follows Equation (2).

These effects illustrate the importance of addressing households' emergency liquidity needs. By enabling repairs to home damages, the effects of disaster loans permeate households' balance sheets. Instead of reducing the ability to borrow for other consumption needs, disaster loans appear to increase households' debt capacity.

The results are also surprising in that we might expect negative effects on mortgage and revolving credit balances since these are partial substitutes for disaster loans. That is, denied disaster loan applicants might extract equity from their homes (e.g., through refinancing or a second mortgage) or turn to higher-cost products such as credit cards to fund repairs. However, we do not find evidence of this type of substitution effect, suggesting that denied disaster loan applicants are largely unable or unwilling to fund disaster repairs through private borrowing.

Benchmarking Disaster Loan Applicants. How should we interpret our causal effects of loan receipt relative to the broader consequences of living through a disaster? To further examine these balance sheet effects, we compare the credit market outcomes of all disaster loan applicants with a random sample of U.S. credit records, drawn outside of affected communities, at the same point in time.[12] In contrast to our causal estimates above, this comparison is descriptive because disaster loan applicants may differ from the general population.

We examine the evolution of credit record outcomes in an event study framework, where the event is the date of the disaster, the treated group is all disaster loan applicants (both approved and denied), and the control group is the random sample drawn from communities outside the affected area. Figure 13 plots event study coefficients on time relative to experiencing a disaster; the effect at each time period is estimated for each disaster relative to the non-disaster sample, and aggregated across disasters using the method in Sun and Abraham (2021).[13] The figure follows consumers from two years before to seven years after the disaster, with each point representing six months.

The figure shows long-term effects of the disaster on households' balance sheets and their probability of moving. The top left panel traces the total debt balances (which do not include Federal Disaster Loans) of disaster-affected households, showing a sharp drop in total reported debts. This result can largely be attributed to the closing of mortgage accounts, suggesting that many of these households are now renters (top right). These disaster affected households have lower debt balances and are less likely to have a mortgage seven years after the disaster.

We also find that affected households are more likely to move to a new zipcode immediately after the event (bottom left). The disaster increases the likelihood of moving zipcodes by about 6 pp one year after the event. This effect is consistent with prior research on migration following a disaster (e.g., Deryugina et al., 2018).

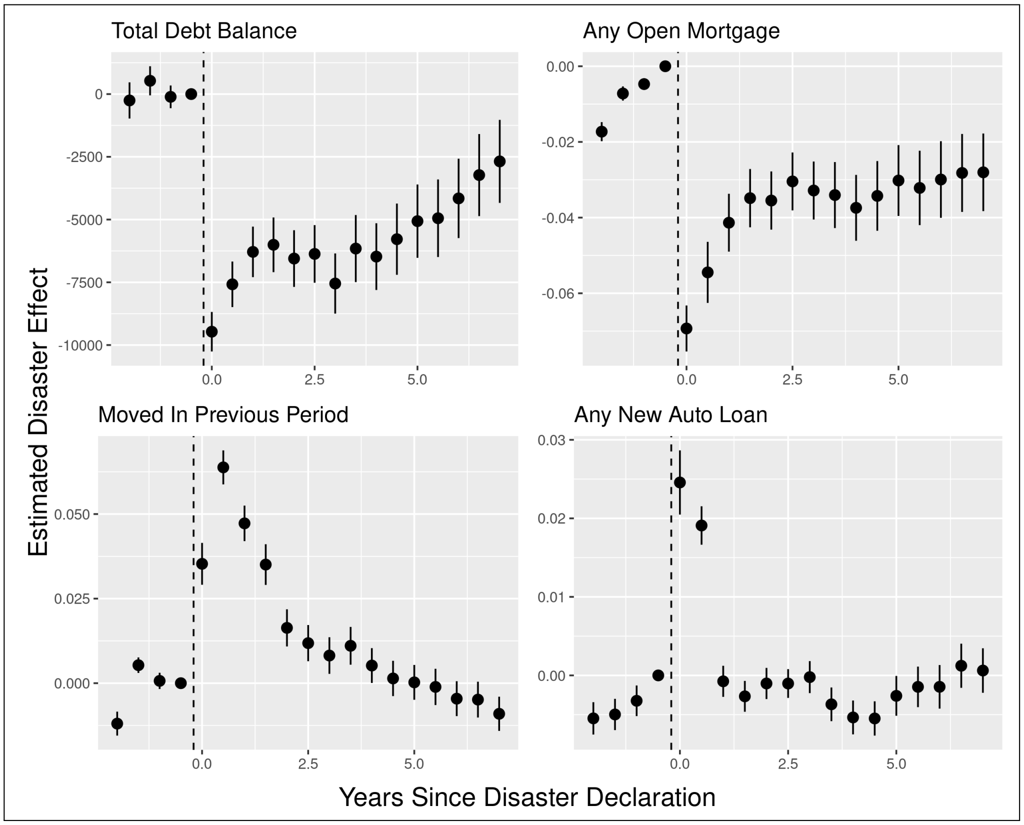
Finally, we observe in the bottom right panel of Figure 13 that households who are affected by disasters are more likely to take a new auto loan in the year after the disaster, presumably because of disaster damages, but not later. This pattern is in contrast to the disaster loan treatment effect that we describe above (shown in Figure 12), which remains significant in the second and third years after the disaster. This figure provides additional support that the disaster loan effect is driven by replacing aging vehicles and thus is a signal of the household's financial health.

Thus, these descriptive results provide context for interpreting our treatment effects. For example, approved and declined disaster loan applicants alike experience a decline in private debt

[12] We construct the dataset as follows: 1) restrict the data to a six-month interval (e.g., January 1 to June 30, 2011), 2) exclude any zipcodes with a disaster loan applicant during that period, 3) randomly sample consumers with a credit report from the remaining zipcodes, and 4) repeat this procedure for every six-month interval in our period of study (2005 to 2013). We observe each of these consumers in a balanced panel for our full time series.

[13] In the terminology of Sun and Abraham (2021), we treat each disaster as a cohort, and the non-disaster control group as a single cohort. The estimation includes calendar and cohort-by-zipcode fixed effects, and standard errors are clustered at the cohort-by-zipcode level.

Figure 13: Credit Market Consequences of a Disaster



Note: The figure shows point estimates of coefficients on event time indicators comparing disaster-affected households to a nationally representative sample of people drawn from outside of the disaster-affected area at the same point in time. Coefficients are estimated and aggregated using the method in Sun and Abraham (2021).

balances relative to the national sample. The positive treatment effects, identified by our causal design and shown earlier in this section, indicate that disaster loan receipt attenuates this decline in private debt balances.

Disaster survey. Given the sizable effects of disaster loan provision, we also examine a survey of households managing disaster repairs. The survey sheds light on a broader set of recovery strategies than can be observed on consumers' credit reports. It was conducted by the Wharton Risk Center and includes 474 respondents who incurred damage to their home from one of four major hurricanes between 2017 and 2021 (additional details in Appendix F).

We find that respondents often cobbled together repair funds, using a combination of draining savings, insurance payments, credit, grants, assistance from friends and family, and other means.

Households also reported coping by spending less on medical care and consumer goods; taking on additional work; and falling behind on obligations including utility bills, credit cards, and housing (consistent with our main findings). Taken together, the survey results illustrate the difficulty that households face in organizing enough repair funds and how these challenges may exacerbate financial distress.

In sum, we find that households with direct damages from disasters have persistent negative shocks to their balance sheets. Our identification strategy around the DTI threshold isolates the benefits of emergency loan receipt, finding large improvements to the balance sheet and the crowding-in of private credit. Our results thus far show the beneficial impacts of loans following a disaster, but the question remains of whether that benefit is due to a relaxation of liquidity constraints or wealth effects from the subsidy implicit in the large share of disaster loans which have interest rates set below the market rate.

4.3 Disentangling Liquidity Versus Wealth Mechanisms: Evidence from the Credit Score Discontinuity

Disaster loans bundle two benefits to recipients that might explain their positive effects: a lump sum loan amount that expands household liquidity and a subsidized interest rate, which represents a wealth transfer. Regarding liquidity, the disaster loan amount is large relative to household cash flows. The average disaster loan of $42,000 represents more than half of the annual income of the average borrower ($74,000, Table 1). The loan amount is modest relative to lifetime earnings, however, suggesting that the liquidity effects of a disaster loan would only benefit borrowers in the presence of market frictions that prevent disaster-affected households from borrowing against their lifetime earnings.

Regarding wealth effects, disaster loan borrowers receive a transfer via the loan's low interest rate. The Office of Management and Budget (OMB, 2023) reports that the interest rate subsidy has a net-present-value (NPV) of 15 cents for every dollar loaned during our period of study. Thus, the average borrower, whose loan is $42,000, receives a subsidy with an NPV of roughly $6,300. This subsidy accrues to the borrower over the life of the loan: A back-of-the-envelope calculation suggests that the average households' monthly loan payment of $229 would be $40 larger without the subsidy. The wealth transfer appears small relative to the sizable treatment effects on bankruptcy and delinquencies that we document in Section 4.1.

To shed light empirically on the question of liquidity versus wealth effects, we leverage plausibly exogenous variation in the program's subsidy. The program offers two interest rates, a "market rate," which is roughly equal to the prevailing interest rate for a 30 year fixed-rate mortgage, and a "subsidized rate," which is typically half of the program's market rate and intended for lower credit quality applicants (Collier and Ellis, 2024). The probability of receiving a subsidized-rate loan jumps discretely as the applicant's credit score falls below 700 points. This credit score discontinuity creates a situation in which borrowers receive similar liquidity benefits on either side of the threshold, but the wealth transfer is positive for lower credit score borrowers, and approximately zero for higher credit score borrowers.[14] Furthermore, it provides a unique setting in which loan pricing is higher for lower risk borrowers, contrary to standard practices of risk-based pricing.

Identification and Estimation. Figure 14 presents the relationship between whether households receive a market-rate disaster loan offer and their credit score at the time of application. The sample comprises all disasters from 2006–2013, when this discontinuous pricing policy was in use. The effect of having a credit score over the 700 threshold sharply increases the probability of receiving a market interest rate by roughly 14 percentage points (from 13 to 27 percent).

We assess the causal effect of the loan subsidy on household distress by extending our main estimating equation (Equation 2) to a treatment intensity design that accounts for the interest rate. This 2SLS estimation compares three groups: control households who applied for a loan but were denied, treated households who were approved for a market-rate disaster loan, and treated households who were approved for a subsidized disaster loan. We use the DTI threshold to instrument for loan approval and the FICO threshold to instrument for whether the household receives the subsidized rate or the market rate. To assess households in a narrow bandwidth around the thresholds, we restrict the sample to applicants with both a DTI $\in [30, 49]$ and a FICO $\in [680, 719]$. The second stage equation in the 2SLS estimation is

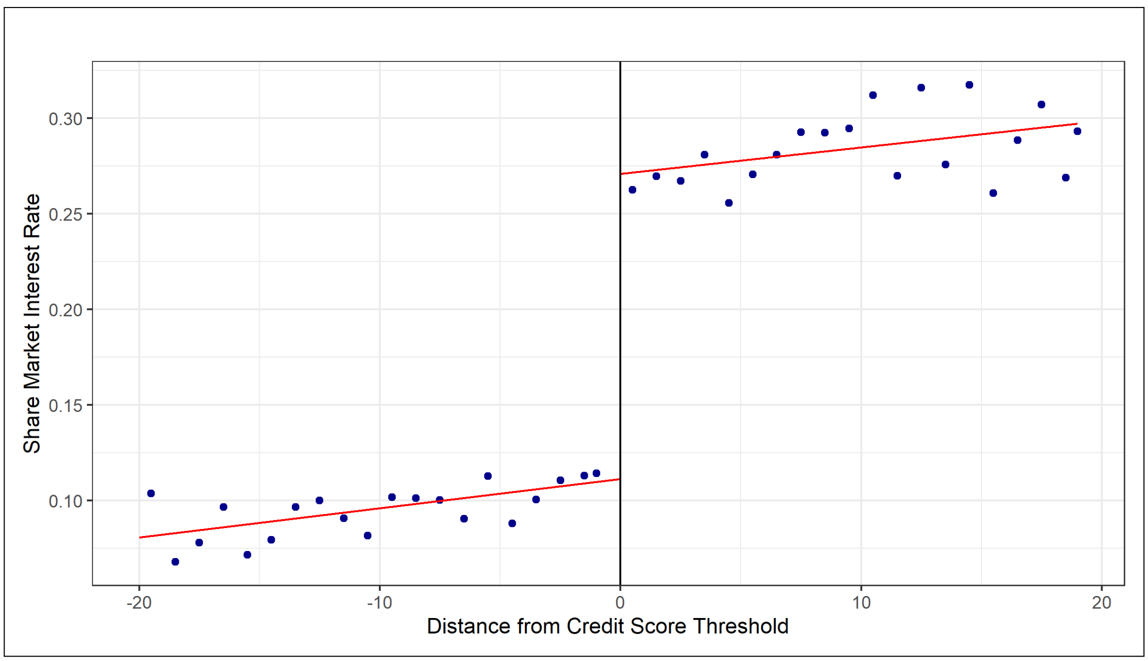
$$\begin{aligned} y_{it} = &\sum_{h=-a}^{b} \beta_h (1[t=h] \times \widehat{approved_i} \times lowrate_i) \\ &+ \sum_{h=-a}^{b} \gamma_h (1[t=h] \times \widehat{approved_i} \times highrate_i) \end{aligned} \tag{4}$$

[14]We adopt the program's language of "market" versus "subsidized" rate for convenience; however, some FDL borrowers likely would be unable to access the program's "market" rate in the private sector (e.g., an applicant with a DTI of 49 might be unable to borrow at the average prevailing mortgage rate). The key consideration for identification is that the interest rate subsidy changes *discretely* at the FICO threshold.

$$+ f(DTI_i) + g(FICO_i) + \psi_{zd} + \tau_{td} + \epsilon_{it}$$

where the instrument for the lower interest loans $lowrate_i$ includes an indicator for the credit score threshold $1[FICO_i < 700]$, and the instrument for the higher interest loans $highrate_i$ includes an indicator $1[FICO_i \geq 700]$.[15] Thus, the β coefficients estimate the LATE for households who are offered the subsidized loan, and the γ coefficients estimate the LATE for households who are offered the market-rate loan. Terms $f(DTI_i)$ and $g(FICO_i)$ are linear controls for the running variables, DTI and FICO. Following our main specification (Equation 2), terms ψ_{zd} and τ_{td} are zipcode-by-disaster and time-by-disaster fixed effects, respectively, and standard errors are robust and clustered at the zipcode-by-disaster level. The full estimation, including all first stage equations, is in Appendix B.2.

Figure 14: Change in Market Rate Likelihood at the Credit Score Threshold



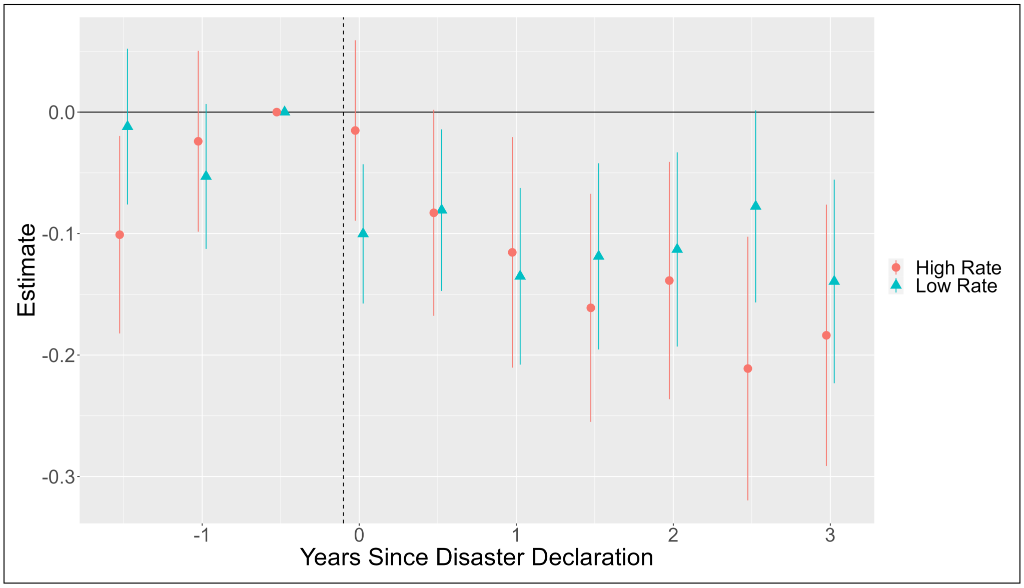
Note: The figure shows the discontinuous change in the likelihood of being offered an unsubsidized interest rate loan for households above the credit score threshold of 700 relative to households below the threshold.

Subsidy Effects. We find that disaster loans significantly reduce the likelihood of delinquency similarly for both the low and high interest rate groups. Figure 15 shows the results. The blue

[15] For example, to instrument for the treatment effect of the low-interest loan in period 2 ($1[t = 2] \times approved_i \times lowrate_i$), we use the term $1[t = 2] \times 1[DTI < 40] \times 1[FICO_i < 700]$.

triangles report the LATE of being approved for a subsidized disaster loan (the β coefficients); the red circles report the LATE for a market-rate disaster loan (the γ coefficients). In both cases, disaster loans significantly reduce the likelihood of credit delinquencies in the years following the disaster. While the point estimates observably differ between the low and high rate groups in several periods (e.g., $t = 0$), these differences are not statistically significant.

Figure 15: Likelihood of Delinquency, Subsidized vs. Unsubsidized Interest Rate



Note: The figure shows the LATEs of receiving a subsidized (low interest rate) disaster loan and of receiving a market-rate (high interest rate) disaster loan using a control group of denied applicants. The outcome is the likelihood of having a credit delinquency. The estimation follows Equation (4) and uses the credit score threshold of 700 to instrument for receiving a market rate versus a subsidized rate loan.

We conduct two additional tests as robustness in Appendix E.2. These tests use simpler estimation strategies than our preferred model in Equation (4) to assess the effects of the loan subsidy on delinquencies. First, we restrict the sample to approved applicants and use the FICO threshold as an instrument for the interest rate. By restricting to approved applicants, this approach simplifies the estimation but comes at the cost of losing the denied households as a control group. Similar to the results in Figure 15, we do not find that the subsidy reduces the delinquency rate among approved applicants. Second, we estimate our main regression (Equation 2) of delinquency on loan approval on two subsamples: those with FICO scores just below (680 to 699) versus above (700 to 719) the threshold. The former subsample is significantly more likely to receive the subsidized interest rate than the latter. As in Figure 15, we find that disaster loan approval reduces delinquencies for applicants on both sides of the FICO threshold at similar rates.

Overall, the evidence from the credit score discontinuity supports the view that disaster loans reduce households' financial distress through the liquidity provision of the loan, as opposed to the wealth effect of the subsidy. Since subsidies are costly, should this subsidy be removed? While the subsidy does not reduce delinquencies in our analyses, it might serve other objectives. First, our finding reflects a local average treatment effect for the population around both the 700 FICO score and 40 DTI cutoffs and thus cannot speak to whether the effects differ in other parts of the FICO and DTI distributions. Second, positive externalities of rebuilding might motivate loan subsidies. For example, Fu and Gregory (2019) find that home repairs following Hurricane Katrina increased the value of nearby properties. In situations with multiple equilibria such as this, loan subsidies may increase the likelihood of ending up in an equilibrium where more households rebuild.

5 Conclusion

What is the causal effect of emergency credit receipt on households' financial well-being? We answer this question in the context of credit provision after natural disasters. Using consumer credit reports, we track the borrowing and repayment behavior of a large group of applicants to a federal program that offers low-interest disaster recovery loans. The program's use of a target underwriting threshold of 40% DTI provides a source of exogenous variation for comparing similar applicants, some of whom are differentially more likely to receive the recovery loan, thus isolating the causal effect of credit provision.

We find large and persistent positive effects of emergency credit. Loan receipt reduces the likelihood of credit delinquency and bankruptcy following the disaster, even 3 years after the event. Emergency credit also appears to crowd-in private-sector borrowing for real outlays including new vehicle purchases. Using causal variation in the program's offered interest rate, we find that liquidity is the main driver of the reduction in delinquency, rather than the subsidized interest rate. In summary, our findings suggest that well-timed liquidity provision can attenuate the consequences of a negative shock, offering significant and lasting benefits to affected households.

Our results have important implications for understanding household consumption and debt usage patterns. They illustrate how liquidity frictions can exacerbate the cost of a negative shock. Households' disaster expenses on top of existing financial obligations appear to drive many who do not receive the loan into financial distress. These results suggest that adjusting consumption commitments is challenging and that the financial strain of unanticipated expenses are not easily smoothed over time.

Further research is needed to better understand the difficulties faced by those households who are denied credit and why private lenders do not appear to expand access after a disaster. Our findings are necessarily constrained by analyzing those households who are near the DTI threshold, and additional identification strategies would be needed to fully generalize our findings to all applicants. A broader set of questions regarding the general equilibrium effects of ex post disaster assistance on households' insurance take-up and adaptation efforts are beyond the scope of our setting, but remain important areas of subsequent investigation.

With climate-driven disasters becoming more common worldwide, our work also informs how best to aid households in a crisis. We find that short-term liquidity provided by the government reduces household financial distress in ways that private credit markets fail to replicate. The lack of private credit provision, due to adverse selection, market uncertainty, or other drivers, suggests an important role for public credit markets to play in times of need.

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Online Appendix A Variable Definitions

This appendix accompanies Table 1 and describes key variables in the data.

Table A1: Variable Definitions

Variable	Description
Income	Annual adjusted gross income, winsorized at the 99% level
Credit score	FICO score of the primary applicant
DTI	A household's existing total monthly debt service payments divided by its monthly income, winsorized at the 99% level
Loss Amount	The program's onsite assessment of property losses
Indicator for Any Delinquent Loan	Equals 1 if the individual has any loans reported 30 or more days delinquent on their credit file and 0 otherwise
Amount Past Due	Dollar amount past due on all loans 30 or more days delinquent
New Bankruptcy	Indicates a new public record bankruptcy on the credit report in the last 6 months
Number of New Auto Loans	Number of new auto loans in the last 6 months
SBA Loan Charge-Off	Indicates the the SBA loan is charged-off within 3 years
Insurance Settlement Amount	Amount received from insurance

Online Appendix B Estimation Details

B.1 DTI Discontinuity

In the interest of simplifying notation, the estimation details in Section 3.1 do not show explicitly that the models include a reference period. In practice, the interaction terms β_h uses $t = -1$, the last observation of an applicant before the disaster, as a reference. $t = -1$ occurs around 6 months before the event.

We provide the implemented model here. The notationally simpler version presented in Section 3.1 and the implemented one described here yield equivalent predicted values of the outcome y_{it}, goodness-of-fit statistics, and instrument diagnostics.

We can explicitly incorporate this reference period by rewriting Equation (1) as

$$y_{it} = \sum_{h=-a, h\neq -1}^{b} \beta_h (1[t = h] \times approved_i) + \gamma\, approved_i + \psi_{zd} + \tau_{td} + \epsilon_{it} \tag{B5}$$

where γ estimates the average difference in the outcome at time $t = -1$ for approved applicants (relative to declined applicants) and β_h capture deviations from this effect over time.

We can similarly rewrite the 2SLS specification in Equations (2) and (3) to show this reference period. The second stage is

$$\begin{aligned} y_{it} = &\sum_{h=-a, h\neq -1}^{b} \beta_h (1[t \widehat{= h] \times approv}ed_i) + \gamma\, \widehat{approved_i} \\ &+ \zeta_0 DTI_i + \zeta_1 (DTI_i \times below_i) + \psi_{zd} + \tau_{td} + \epsilon_{it} \end{aligned} \tag{B6}$$

The first stage equations are

$$\begin{aligned} (1[t = h] \times approved_i) = &\sum_{h=-a, h\neq -1}^{b} \alpha_h (1[t = h] \times below_i) + \eta\, below_i \\ &+ \delta_0 DTI_i + \delta_1 (DTI_i \times below_i) + \phi_{zd} + \kappa_{td} + \mu_{it} \\ approved_i = &\sum_{h=-a, h\neq -1}^{b} \theta_h (1[t = h] \times below_i) + \phi\, below_i \\ &+ \lambda_0 DTI_i + \lambda_1 (DTI_i \times below_i) + \pi_{zd} + \rho_{td} + \mu_{it} \end{aligned} \tag{B7}$$

The number of first stage equations and the number of instruments are the same as in Equations (2) and (3). Equations (B6) and (B7) are clearer in showing that all instruments (e.g., the interaction term for each event time h, $(1[t = h] \times below_i)$) is included in each of the first stage equations.

B.2 FICO Discontinuity

In Section 4.3, we use a FICO-based discontinuity to examine the effects of the program's offered interest rate and provide a parsimonious version of the estimation to provide the intuition. Below is the full set of estimating equations. The second stage equation is

$$\begin{aligned}
y_{it} = & \sum_{h=-a,h\neq-1}^{b} \beta_h(1[t=h] \times \widehat{approved_i \times lowrate_i}) \\
& + \sum_{h=-a,h\neq-1}^{b} \gamma_h(1[t=h] \times \widehat{approved_i \times highrate_i}) \\
& + \theta(\widehat{approved_i \times lowrate_i}) \\
& + \kappa(\widehat{approved_i \times highrate_i}) \\
& + f(DTI_i) + g(FICO_i) + \psi_{zd} + \tau_{td} + \epsilon_{it}
\end{aligned} \tag{B8}$$

where the $f(DTI_i)$ reflects linear controls for DTI above and below the threshold, DTI_i and $DTI_i \times 1[DTI_i < 40]$. Similarly, $g(FICO_i)$ reflects $FICO_i$ and $FICO \times 1[FICO \geq 700]$. In this equation, β estimates the instrumented effect of approval for the subsidized-rate loan for each period, γ estimates the instrument effect of approval for the market-rate loan for each period, and θ and κ respectively estimates these effects for the reference period $t = -1$. ψ are zipcode-by-disaster fixed effects and τ are event-time-by-disaster fixed-effects.

The first stage equations are

$$\begin{aligned}
(1[t=h] \times approved_i \times lowrate_i) = & \sum_{h=-a,h\neq-1}^{b} \alpha_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i < 700]) \\
& + \sum_{h=-a,h\neq-1}^{b} \delta_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
& + \eta(1[DTI_i < 40] \times 1[FICO_i < 700]) \\
& + \nu(1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
& + f(DTI_i) + g(FICO_i) + \phi_{zd} + \kappa_{td} + \mu_{it} \\
(1[t=h] \times approved_i \times highrate_i) = & \sum_{h=-a,h\neq-1}^{b} \alpha_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i < 700]) \\
& + \sum_{h=-a,h\neq-1}^{b} \delta_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
& + \eta(1[DTI_i < 40] \times 1[FICO_i < 700]) \\
& + \nu(1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
& + f(DTI_i) + g(FICO_i) + \phi_{zd} + \kappa_{td} + \mu_{it} \\
(approved_i \times lowrate_i) = & \sum_{h=-a,h\neq-1}^{b} \alpha_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i < 700]) \\
& + \sum_{h=-a,h\neq-1}^{b} \delta_h(1[t=h] \times 1[DTI_i < 40] \times 1[FICO_i \geq 700])
\end{aligned}$$

$$
\begin{aligned}
&+ \eta(1[DTI_i < 40] \times 1[FICO_i < 700]) \\
&+ \nu(1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
&+ f(DTI_i) + g(FICO_i) + \phi_{zd} + \kappa_{td} + \mu_{it}
\end{aligned}
$$

$$
\begin{aligned}
(approved_i \times highrate_i) = &\sum_{h=-a, h\neq -1}^{b} \alpha_h(1[t = h] \times 1[DTI_i < 40] \times 1[FICO_i < 700]) \\
&+ \sum_{h=-a, h\neq -1}^{b} \delta_h(1[t = h] \times 1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
&+ \eta(1[DTI_i < 40] \times 1[FICO_i < 700]) \\
&+ \nu(1[DTI_i < 40] \times 1[FICO_i \geq 700]) \\
&+ f(DTI_i) + g(FICO_i) + \phi_{zd} + \kappa_{td} + \mu_{it} \qquad \text{(B9)}
\end{aligned}
$$

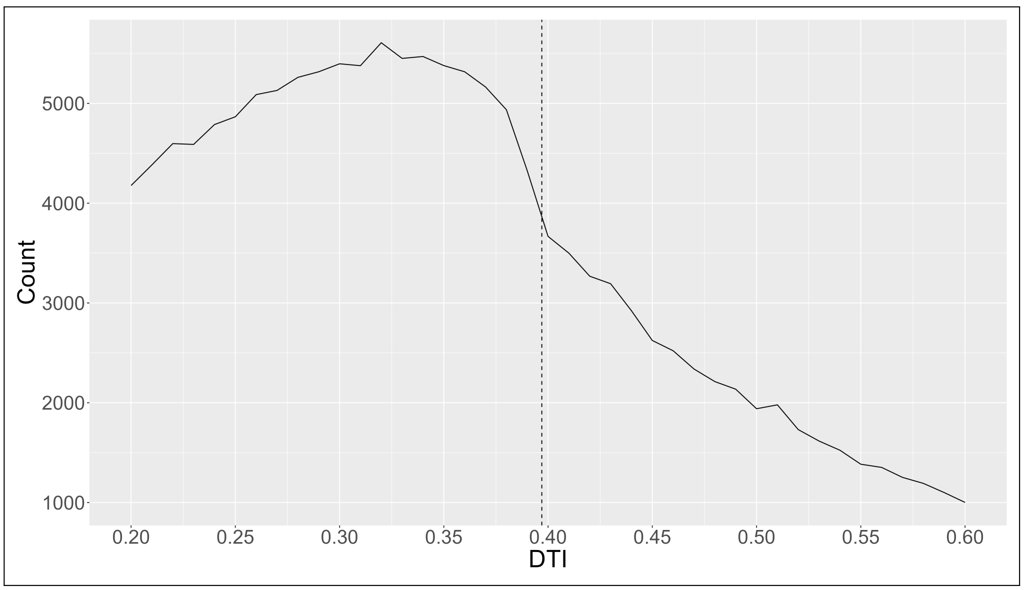
The number of first stage equations equals the number of instruments, leading to a just-identified system of equations. As these first stage equations show, all instruments are included in each first stage equation.

Online Appendix C DTI Density Plot and Summary Statistics, Analysis Sample

Figure C1 shows the distribution of applicant DTI ratios around the 40% threshold. The DTI distribution peaks near 30% and falls thereafter.

Table C1 presents summary statistics for our analysis sample for just under 80,000 applicants with DTI between 30 and 50 percent in our sample. The top panel pools all applicants while the middle and bottom panels split the sample based on the 40 percent DTI threshold. Comparing the means of all variables except income and DTI reveals economically small differences between the two groups. Of course, the differences in DTI reflect the split of the sample. The differences in income are somewhat mechanically related to this split as well since income is in the denominator of DTI.

Figure C1: Distribution of DTI Ratio Among Loan Applicants



Note: The figure shows the distribution of DTI ratios among applicants.

Table C1: Summary Statistics of Analysis Sample

	Mean	SD	Percentiles p10	p50	p90
Analysis Sample					
Income	66163	38144	30988	56117	112221
Credit Score	676	81	566	676	787
DTI	0.38	0.05	0.31	0.37	0.46
Loss Amount	64675	83274	8805	32633	167383
Indicator for Any Delinquent Loan	0.16	0.37	0.00	0.00	1.00
Amount Past Due on Delinquent Loans	245	2297	0	0	0
New Bankruptcy in Last 6 Months	0.01	0.07	0.00	0.00	0.00
Number of New Auto Loans	0.11	0.33	0.00	0.00	1.00
Analysis Sample, DTI $<$ 40					
Income	68158	39243	31500	58314	115000
Credit Score	677	82	567	677	789
DTI	0.35	0.03	0.31	0.35	0.38
Loss Amount	65668	85173	8980	32960	169268
Indicator for Any Delinquent Loan	0.16	0.36	0.00	0.00	1.00
Amount Past Due on Delinquent Loans	228	2163	0	0	0
New Bankruptcy in Last 6 Months	0.01	0.07	0.00	0.00	0.00
Number of New Auto Loans	0.11	0.33	0.00	0.00	1.00
Analysis Sample, DTI $\geq$ 40					
Income	62785	35955	30000	52792	107164
Credit Score	673	80	566	673	783
DTI	0.44	0.03	0.40	0.44	0.49
Loss Amount	62993	79926	8500	32149	163977
Indicator for Any Delinquent Loan	0.16	0.37	0.00	0.00	1.00
Amount Past Due on Delinquent Loans	273	2506	0	0	19
New Bankruptcy in Last 6 Months	0.01	0.08	0.00	0.00	0.00
Number of New Auto Loans	0.12	0.34	0.00	0.00	1.00

Note: Table includes data on 79,102 applicants with debt-to-income ratios between 0.3 and 0.5. See Table 1 note for details on variable definitions.

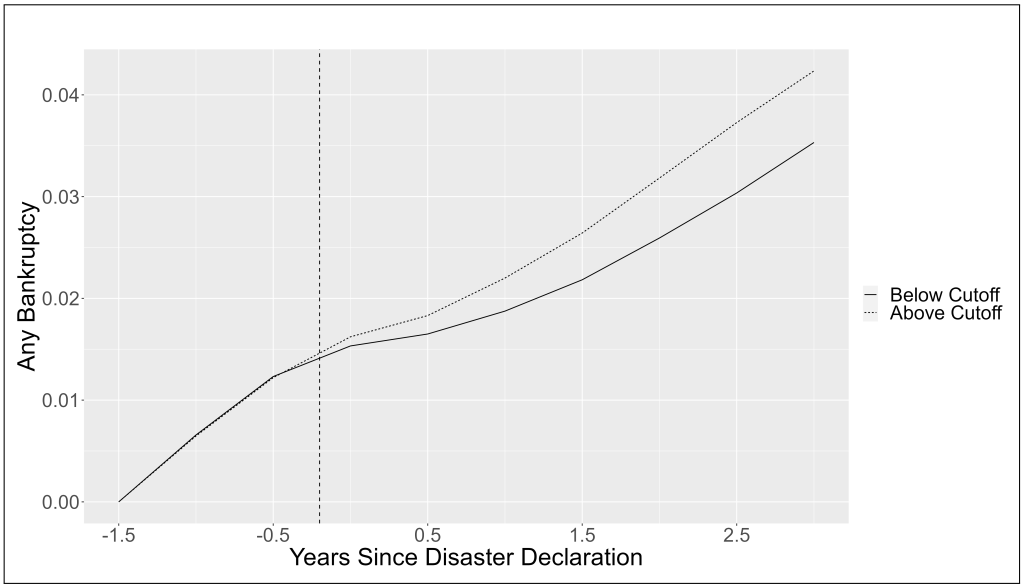
Online Appendix D Level-Plots of Key Outcomes

Figure D1 shows the share of households who have filed for bankruptcy by each period so corresponds with the stock variable in the regression. Only households who have not yet filed by period $t = -1.5$ are included. The solid line represents households who are below the DTI threshold, so are more likely to be approved for a disaster loan; the dotted line represents households above the threshold. The figure shows highly similar filing rates prior to the disaster, which then diverge in the post-disaster period.

A relatively large number of filings occur during the pre-disaster periods. This pattern emerges because Hurricane Katrina, the largest event in our sample, occurred in late summer 2005. The

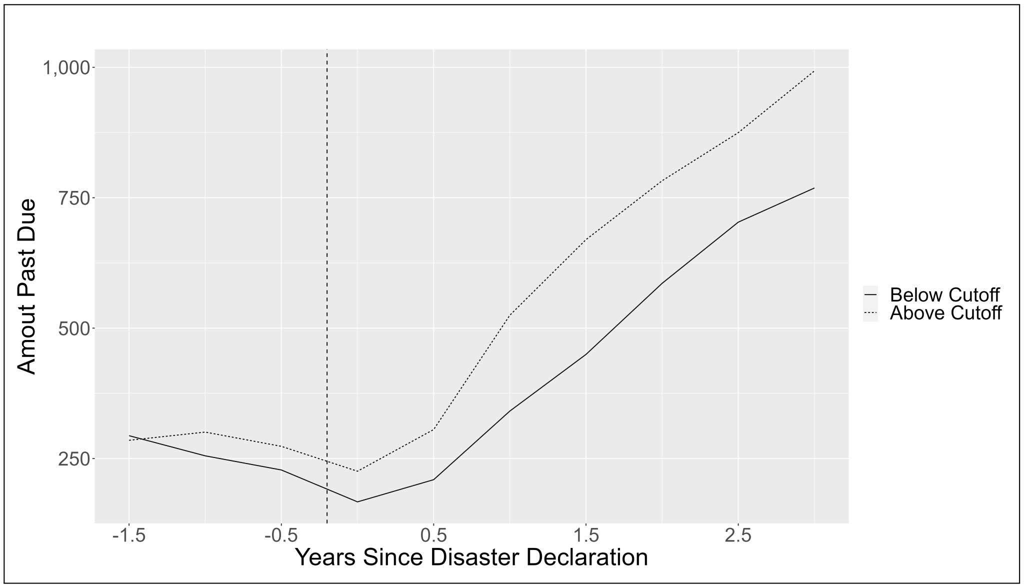
Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) was implemented in October 2005 reduced the benefits of filing, and Gross et al. (2021) document an increase in filings in summer 2005.

Figure D1: Conditional Means, Bankruptcy



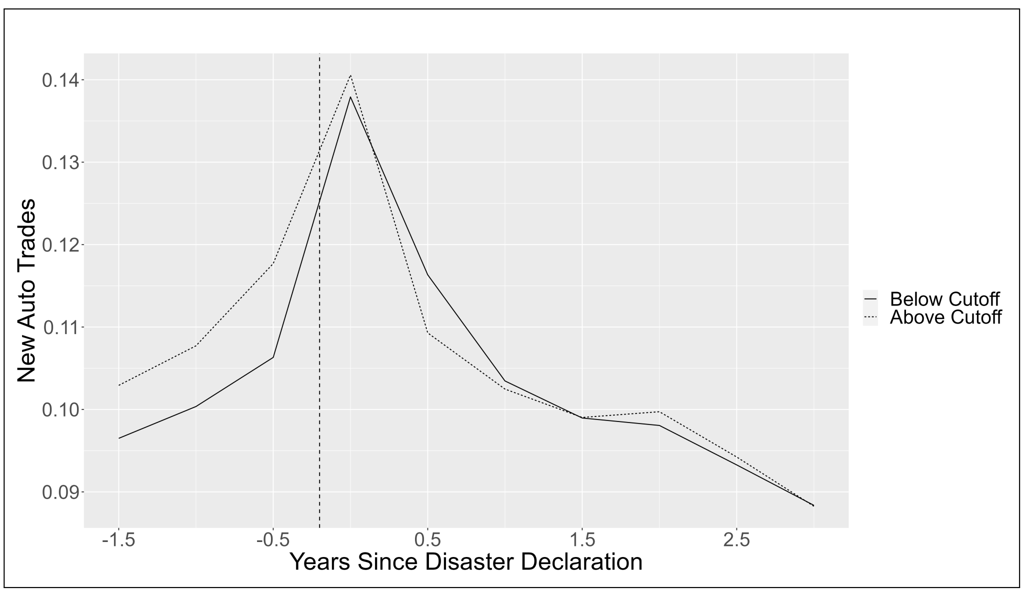
Note: The figure shows the share of households who have filed for bankruptcy by each period (the stock variable). The solid line represents households with DTI below the threshold, who are significantly more likely to be approved for a disaster loan compared to those above the DTI threshold (dashed line).

Figure D2: Amount Past Due on Delinquent Loans



Note: The figure shows the amount that households owe in late loan payments, for two groups based on debt-to-income at loan application. The solid line represents households with DTI below the threshold, who are significantly more likely to be approved for a disaster loan compared to those above the DTI threshold (dashed line).

Figure D3: New Auto Loans

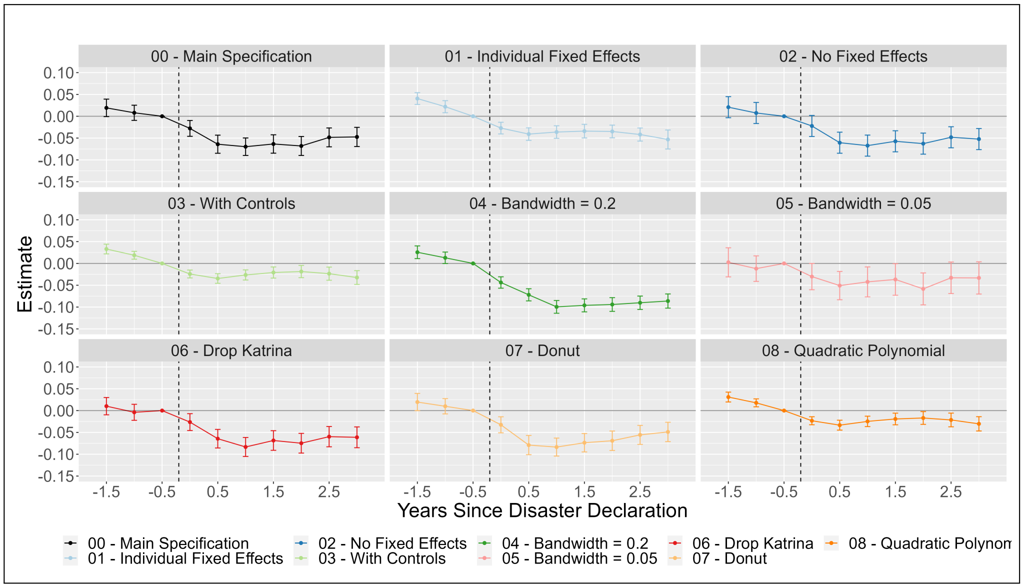


Note: The figure shows the number of new auto loans that households have taken in the last 6 months, for two groups based on debt-to-income at loan application. The solid line represents households with DTI below the threshold, who are significantly more likely to be approved for a disaster loan compared to those above the DTI threshold (dashed line).

Online Appendix E Additional Robustness Results

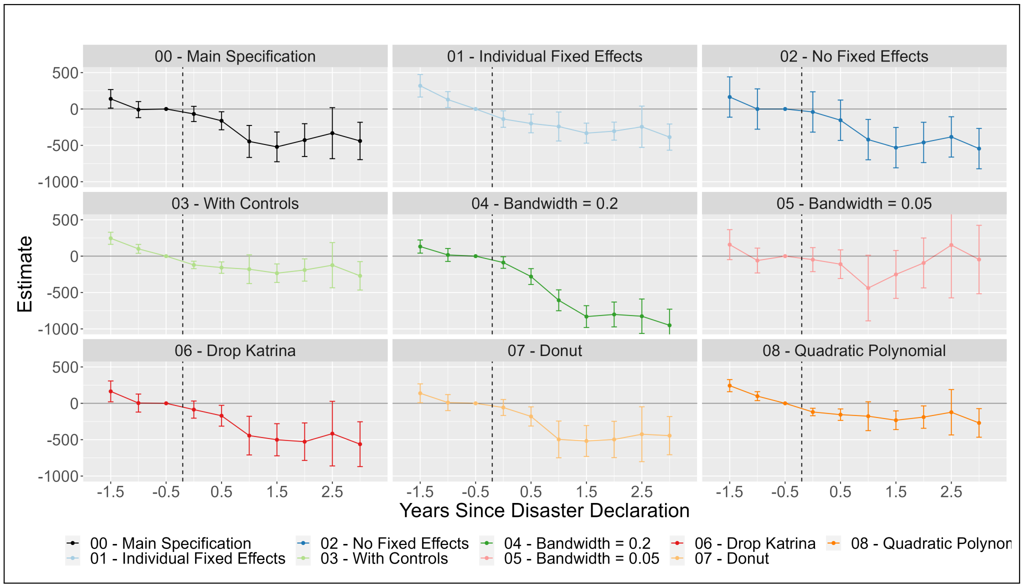
E.1 Alternative Specifications

Figure E1: Effects of Loan Approval on Delinquency, Alternative Specifications



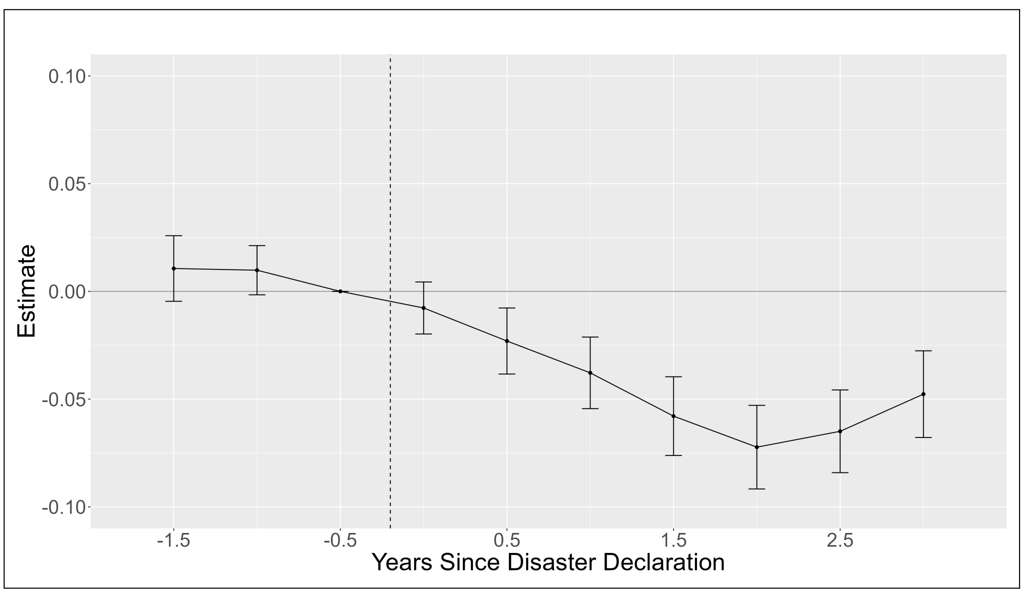
Note: The figure shows the LATE of loan approval on the likelihood of having any loan 30 or more days late. The series of plots show how alternative specifications of the estimating equation affect the results. Specification 3 uses controls for disaster loss, credit score, and income. Specifications 4 and 5 alter the DTI bandwidth for selection of the sample. Specification 6 drops Hurricane Katrina from the analysis sample. Specification 7 drops borrowers with DTI within $[0.38, 0.42]$.

Figure E2: Effects of Loan Approval on Amount Past Due, Alternative Specifications



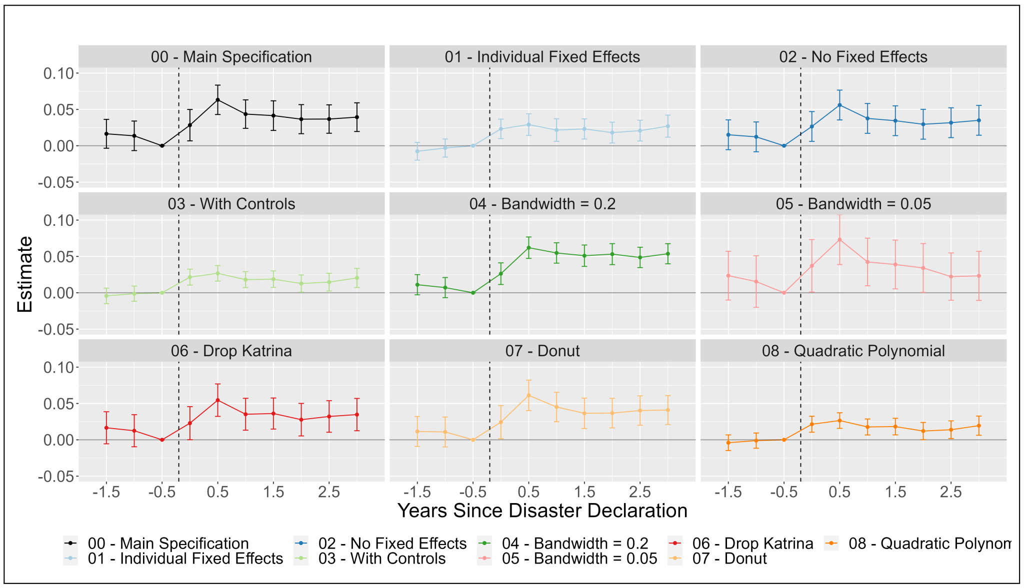
Note: The figure shows the LATE of loan approval on the amount past due on delinquent loans. The series of plots show how alternative specifications of the estimating equation affect the results. Specification 3 uses controls for disaster loss, credit score, and income. Specifications 4 and 5 alter the DTI bandwidth for selection of the sample. Specification 6 drops Hurricane Katrina from the analysis sample. Specification 7 drops borrowers with DTI within $[0.38, 0.42]$.

Figure E3: Effects of Loan Approval on Charge-Offs



Note: The figure shows the LATE of loan approval on the likelihood of having a charge-off loan. The estimate includes charge-offs on disaster loans and loans on the consumers' credit report.

Figure E4: Effects of Loan Approval on New Auto Loans, Alternative Specifications



Note: The figure shows the LATE of loan approval on the number of new auto loans that households have taken in the last 6 months. The series of plots show how alternative specifications of the estimating equation affect the results. Specification 3 uses controls for disaster loss, credit score, and income. Specifications 4 and 5 alter the DTI bandwidth for selection of the sample. Specification 6 drops Hurricane Katrina from the analysis sample. Specification 7 drops borrowers with DTI within $[0.38, 0.42]$.

E.2 Credit Score Discontinuity

We additionally estimate the causal effect of the subsidy on credit delinquency with a 2SLS specification using the credit score discontinuity as an instrument and restricting the sample to include only approved applicants. This estimation is similar to our main specification (Equation 2) except that the running variable is now the household's FICO score at the time of application rather than their DTI ratio. Specifically, we estimate a second stage

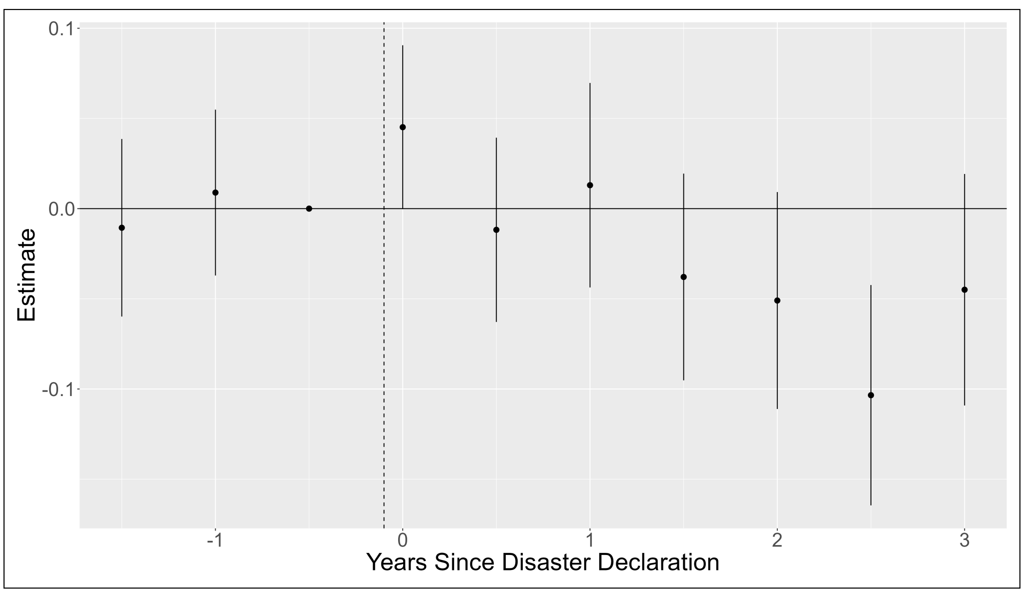
$$y_{it} = \sum_{h=-a}^{b} \beta_h \widehat{(1[t=h] \times market_i)} + \gamma_0\, CS_i + \gamma_1 (CS_i \times above_i) + \psi_{zd} + \tau_{td} + \epsilon_{it} \tag{E1}$$

and h first stages

$$\begin{aligned}(1[t=h] \times market_i) = &\sum_{h=-a}^{b} \alpha_h (1[t=h] \times above_i) \\ &+ \delta_0\, CS_i + \delta_1 (CS_i \times above_i) + \phi_{zd} + \kappa_{td} + \mu_{it}\end{aligned} \tag{E2}$$

The CS_i terms control for the running variable, which is the applicant's credit score. $above_i$ is an indicator for whether $CS_i > 700$. $market_i$ is an indicator, which equals 1 if the approved applicant is offered the market rate and 0 if offered the below-market rate. We use a bandwidth of 10 FICO points on either side of the threshold of 700.

Figure E5: Effect of Market Interest Rate Loan on Likelihood of Delinquency

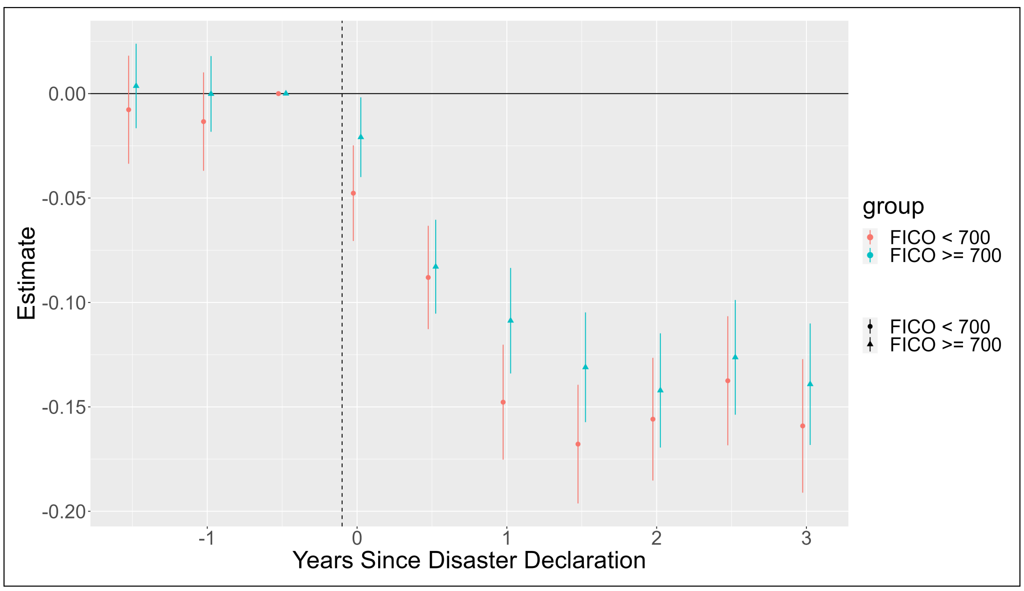


Note: The figure shows the LATE of receiving a market interest rate disaster loan on the likelihood of having an account past due. The credit score threshold of 700 is used as the instrument for receiving a market rate versus a lower interest rate loan.

Figure E5 shows the causal effect of receiving a loan offer with a market interest rate on the probability of having any type of loan delinquency, relative to receiving a loan offer with a subsidized interest rate. These results are based on a two-stage specification identical to the specification expressed in Equations (E1) and (E2) except with the offered interest rate instrumented by the FICO threshold and controlling for FICO score on either side of the threshold either linearly (in specifications 1-5) or quadratically (specification 6). In all our specifications, we find no statistically significant effect of receiving a higher interest rate loan offer on the likelihood of delinquency except in the period 2.5 years after the disaster. However, the higher rate *reduces* the probability of having a loan that is past due – the opposite direction of what one would expect from the wealth effect of the subsidy. Given that the effect is limited to a single period and inconsistent with theory, we view it as an anomaly.

In Figure E6, we estimate our main regression (Equation 2) of delinquency on loan approval on two subsamples: those with FICO scores just below (680 to 699) versus above (700 to 719) the threshold. The former subsample is significantly more likely to receive the subsidized interest rate than the latter. Consistent with the results in our preferred specification (Figure 15), we find that disaster loan approval reduces delinquencies for applicants on both sides of the FICO threshold.

Figure E6: Likelihood of Delinquency, Below vs. Above Credit Score Threshold



Note: The figure shows the LATE of receiving a disaster loan on the likelihood of having an account past due for two subsamples, following Equation (2). The first subsample has a FICO $\in [680, 700)$ at the time of application; the second has a FICO $\in [700, 720)$. Applicants in the former are significantly more likely to receive a subsidized interest rate on their disaster loan relative to the latter.

Online Appendix F Survey of Household Recovery Funding

The survey includes 474 respondents who incurred damage to their home from one of four major hurricanes between 2017 and 2021. The studied events are Hurricane Harvey (n = 136), Hurricane Florence (n = 117), Hurricane Michael (n = 96), and Hurricane Ida (n = 125). The survey was primarily distributed through Qualtrics, which randomly sampled individuals in its internet panels who lived in disaster-affected areas. The survey was additionally distributed through (1) a geographically targeted Facebook ad campaign, (2) spots on local radio stations, and (3) community group outreach. Only individuals affected by the hurricane and who are the primary decision-maker in their household are included in the table. Survey participants were entered in a lottery to win gift cards valued at $20-30. See You and Kousky (2024) for additional details regarding data collection and results.[16]

Panel A of Table F1 shows how households funded damages to their homes from these disasters. The categories are not mutually exclusive and show that consumers often fund repairs through multiple sources. Over half of respondents used homeowners or renters insurance to fund repairs. These insurance products do not cover flood risks but often address other hurricane-related damages (e.g., from wind). Only 21% of respondents received flood insurance claims payments. Half of respondents also reported drawing down savings to fund repairs. This savings drawdown aligns with the results of Deryugina et al. (2018), who find that Hurricane Katrina significantly increased early withdrawals from individual retirement accounts.

Among credit products, consumers most frequently used credit cards, with a fifth of respondents saying they turned to revolving credit. Note that some credit card use may represent short-term, low-cost borrowing: Del Valle et al. (2022) find that households affected by Harvey open new credit cards at promotional rates and then pay off these balances before the promotion expires. The median credit card in their analysis has a $3,000 limit, suggesting an inability to fund large repairs in this way. However, for other households, credit card usage may reflect borrowing at a high cost to fund repairs. For example, Morse (2011) and Dobridge (2018) find that disasters also increase payday loan borrowing. Regarding long-term loans, 8% of respondents funded repairs using a loan from a private lender. Around 7% of respondents received a Federal Disaster Loan. The share of respondents using a disaster loan appears relatively small but may result from several factors. First, half of disaster loan applicants are rejected, suggesting that the share of respondents applying for a disaster loan may be closer to 14%. Second, low income respondents may have been referred to apply for a FEMA grant instead of a disaster loan. Finally, consistent with the focus of our paper, disaster loans are intended to cover large uninsured damages, which might be difficult for households to repair without a loan. Respondents who incurred small damages may have preferred to pay for them out-of-pocket (e.g., with savings or a credit card) to avoid the process of applying for a long-term loan.

Households also received transfers from family and friends, governments, non-profits, or employers. Twenty-nine percent of respondents obtained assistance from family and friends, while 19% percent of respondents received a FEMA grant. These grants are typically small, averaging $4,500 during our period of study (Collier and Ellis, 2024). Ten percent or fewer of respondents received assistance from a charitable organization or an employer or a local government.

[16]We thank these authors and the Wharton Risk Center for use of the data.

Table F1: Funding Disaster Repairs

Panel A	
Funding source	%
Savings	51
Insurance	
Homeowners/Renters Insurance	54
Flood Insurance	21
Credit	
Credit cards	19
Formal loan from private bank or lender	8
Federal disaster loan	7
Transfers	
Family & friends	29
FEMA grant	19
Charity, nonprofit, or community group	10
Employer	8
Local government	5

Panel B	
Action taken	%
Took on extra work	29
Delayed payments	
Fell behind on utility bills	27
Fell behind on credit card bills	24
Fell behind on rent/mortgage	18
Reduced consumption	
Spent less on consumer goods	40
Spent less on medical care	16
Spent less on education	6
Moved in with family or friends	20
Sold personal belongings	19
None of the above	28

Note: Table presents survey responses from the following questions, (1) "Which of the following sources provided funds to help pay for the costs of repairing/rebuilding your home or for the costs of replacing items inside your home? (check all that apply)" and (2) "Did you or anyone in your household do any of the following to pay for costs incurred as a result of [Event Name]? (check all that apply)."

As a result of the disaster, households adjusted not only their balance sheet through drawing down savings and increasing their debt, but also shifted their consumption and work behavior. Panel B of Table F1 shows how survey respondents adjusted to address the costs of repairs: 40% reduced their spending on consumer goods, 16% spent less on medical care, and 20% report (likely temporarily) moving in with family or friends. Twenty-nine percent of respondents took on additional work to cover incurred costs. Consistent with our findings regarding credit delinquencies, a common reaction to disaster-imposed costs is to delay payments on existing obligations, with 27% falling behind on utility bills, 24% on credit card bills, and 18% on housing (rent or mortgage) expenses.