Let the Rich Be Flooded: The Distribution of Financial Aid and Distress after Hurricane Harvey*

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Abstract

Outside of flood hazard zones, households must decide whether to insure or rely on disaster assistance to manage flood risk. We use the quasi-random flooding generated by Hurricane Harvey, which hit Houston in August 2017, to understand the implications of flood losses for households at different points in the wealth distribution. We begin by characterizing the allocation of SBA disaster loans and FEMA grants as regressive. For example, per dollar of damage, 28% less in SBA loan dollars flowed into neighborhoods where residents had a more limited ability to repay (and, hence, qualify for) an SBA loan. In turn, heavy flooding increased the bankruptcy rate in these same areas by 1.4 percentage points (or 39%) relative to similar areas that did not flood. Delinquency follows a similar pattern. In contrast, flood victims with the highest likelihood of being approved for an SBA loan see a small, relative decrease in their delinquent debt after flooding – consistent with SBA loans acting as a liquidity infusion. Flood insurance, unlike disaster assistance, mitigates the credit impact of flooding across the wealth distribution. Our results highlight that averages mask important heterogeneity after disasters, which challenges existing narratives of how effectively Federal disaster programs absorb financial shocks.

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1 Introduction

Climate change has brought more frequent mass flooding events (Kossin, 2018) as well as added uncertainty over who is actually at risk of incurring disaster-related losses (Kousky, 2018). To manage the tail probability of losses outside of flood hazard zones, homeowners face a choice: buy insurance or rely on disaster assistance. The former requires paying upfront (ex-ante) premiums for a certain payout if a disaster occurs. The latter involves a stream of (ex-post) disaster loan payments on an uncertain payout. Whether households can rely on disaster assistance as a substitute for insurance depends on the generosity and allocation of disaster assistance – for which there are two competing narratives.

Disaster assistance programs in the U.S. are traditionally thought of as a social safety net – after a household loses real assets, these programs provide housing assistance as well as grants and loans to help those without insurance or other resources recover. For example, Federal Emergency Management Agency (FEMA) states that its grant program "provides financial help...to those who have necessary expenses and serious needs if they are unable to meet the needs through other means."[1] However, an alternative way to think about disaster assistance is as a reinvestment stimulus program. Rather than act primarily as a safety net for the poor, disaster assistance may prioritize the rebuilding of upper middle-class homes, thus investing in the local area and retaining tax-paying workers who might otherwise move away. Indeed, the Federal Disaster Loan Program, implemented by the Small Business Administration (SBA), describes itself in a way that emphasizes its role as a form of economic stimulus (Collier & Ellis, 2019).[2]

These two visions of disaster assistance imply vastly different assistance allocations across households that vary in pre-disaster resources and credit access. Although it is rather foreseeable that a large negative wealth shock might lead to negative credit outcomes, neither the magnitude nor the allocation of those negative outcomes across households is obvious in the context of flood insurance and federal disaster assistance – both of which aim to smooth the negative wealth shock. For example, if disaster assistance were a safety net program, offering more assistance to those with fewer resources, we might expect similar and fairly mild effects on flood victims across the wealth distribution. If disaster assistance were, foremost, a reinvestment stimulus program, we should expect large negative financial outcomes only among those with weak initial financial conditions. We might even see some improvements in the credit outcomes of those "wealthy-hand-to-mouth" (Kaplan et al., 2014) borrowers for whom disaster aid acts as a liquidity infusion. And, to the extent that flood insurance obviates demand for disaster assistance, we could expect a tighter distribution of financial outcomes within the parts of Houston where flood insurance is obligatory.

Prior research into the financial outcomes of disaster victims would, on its face, suggest that disaster assistance is well-allocated. Several studies emerged from the catastrophe of Hurricane Katrina which struck New Orleans in 2005 (McIntosh, 2008; Sacerdote, 2012; Gallagher & Hartley, 2017; Deryugina et al.,

[1] This statement was taken from `https://www.fema.gov/media-library/assets/documents/24945`; accessed on June 15, 2020. This website has since been removed.

[2] The SBA describes its program in the following terms: "disaster loans are a critical source of economic stimulation in communities hit by a disaster, spurring job retention and creation, revitalizing business health and stabilizing tax bases" (SBA, 2020). In a 2011 press release, the SBA Associate Administrator, James Rivera, explains the "program has made it possible for small towns and large cities to rebuild, saving jobs and supporting the long-term economic recovery of areas that would have otherwise failed without the help" (SBA, 2011).

2018).[3] In a summation of the extant literature, Gallagher et al. (2020b) write: "These studies all conclude that the average net financial impact of a large natural disaster is modest and short-lived, even for the most severely impacted victims." Indeed, Gallagher & Hartley (2017) describe their findings as "empirical evidence as to [disaster aid programs'] effectiveness." These authors do not, however, explore how disaster assistance is allocated in their context nor do they test whether their treatment effects represent a netting of heterogeneous outcomes. Our paper is further motivated by a recent sociology study, Howell & Elliott (2018), which finds that the more disaster assistance a county receives, the more wealth inequality rises in that county after a disaster. Our study contributes by exploring whether disaster assistance more likely counteracts or exacerbates pre-existing inequalities.

This paper uses Hurricane Harvey, which submerged 25–30% of the Houston metropolitan area in August-September of 2017, as a lens through which to understand the revealed, rather than stated, goals of current disaster assistance programs. Then, we turn to credit bureau data to understand the consequences of current assistance allocations, testing for heterogeneity in the bankruptcy and delinquency rates of disaster victims according to the same dimensions that are associated with more or less disaster assistance. Due to weak zoning, fast development, and inaccuracies in 100-year floodplain maps, Hurricane Harvey's flooding was quasi-random. We show that geospatial attributes (e.g., elevation, floodplain coverage, etc...), socio-economic factors (e.g., minority share, poverty rate, etc...), and credit variables (e.g., credit score, delinquency, etc...), explain no more than 7% of the variation in flooding across Houston-area Census blocks. Also, we show that there was little change in employment/wages and no restriction in the supply of credit after Harvey, either overall or according to flood exposure. These unique features, allow us to apply a transparent, treatment intensity difference-in-difference design comparing the credit outcomes of Houston residents that lived in blocks that were heavily flooded (in the top-quartile of flood depth) to those that lived in blocks that experienced no flooding. Our goal is to understand the financial implications of flood losses for households with differing access to credit and resources.

Our analysis begins by characterizing the revealed allocation of disaster assistance after Hurricane Harvey, incorporating data obtained from FEMA and the SBA on individuals impacted by Hurricane Harvey. The most dominant form of disaster assistance for individuals is low-interest loans from the SBA. SBA loans can reach $240,000 ($200,000 for a primary residence and $40,000 for personal property) and have extremely attractive terms, including a 1.75% interest rate with up to 30-years to repay. To limit taxpayer losses while offering one, low interest rate to most borrowers (i.e., without price-discriminating), SBA program rules explicitly limit loan eligibility to higher credit-quality applicants, thus creating a friction: applicants with the greatest need for credit may be least likely to get it. Indeed, Begley et al. (2018), use a wide array of disasters to offer the first indication of substantial inequalities (according to factors like the minority share of a county) in access to SBA loans. As further evidence, Collier & Ellis (2019) show that take-up of these loans is highly sensitive to small changes in their interest rates – implying that

[3]Our study is most closely related to Gallagher & Hartley (2017), who study the credit and debt outcomes of individuals affected by Katrina and find only modest and temporary jumps in overall delinquency rates for the most flooded residents. They also observe a pay-down of mortgage debt using flood insurance payouts. Other studies (McIntosh, 2008; Sacerdote, 2012; Deryugina et al., 2018) analyze the effect of the hurricane on local economic conditions as well as on an array of individual outcomes, including income, migration, education, and employment.

many of the borrowers who take SBA loans do so, not for financial survival, but because they believe the cost of such capital is below their expected payoff from reinvesting the funds. It follows that we should expect to find inequalities in access to SBA loans after Hurricane Harvey. What is ambiguous, however, is whether households with greater access to SBA loans – given their subsidized interest rates, scale, and flexible terms – might see *improvements* in their credit outcomes relative to counterparts who do not flood and, therefore, cannot access these loans.

We also provide novel evidence on the revealed allocation of aid through FEMA's Individuals and Households Program (IHP), which offers cash grants of up to $33,000 to help absorb immediate housing needs and pay for limited repairs. To our knowledge, this is the first paper to explore the allocation of FEMA grants. A finding of similar inequalities in FEMA grants would be surprising given that FEMA does not tie its aid to factors like credit scores. Instead, per FEMA's stated goal above, IHP grants are intended to be allocated according to unmet needs. It follows that we should expect FEMA to help fill the gap when SBA loans are insufficient or denied (like a typical safety net program).

To study heterogeneity in post-disaster financial outcomes, we differentiate individuals and blocks along wealth (meant generally as "access to credit and resources") and insurance dimensions. First, we classify individuals and blocks according to their ability-to-repay and, hence, be approved for an SBA loan. In the absence of risk-based pricing, the SBA limits losses by screening applicants on ability-to-repay. Since the SBA considers multiple correlated factors in screening applicants for ability-to-repay – e.g., FICO score and debt-to-income ratio – we identify several proxies for these screening factors in our data and take the first principal component to form an index.[4] Since ability-to-repay is necessarily correlated wealth, it can be interpreted more broadly as a measure of initial financial condition. Second, to generate variation in exposure to disaster assistance programs, we exploit two facts: (a) many Houston blocks inside the 100-year floodplain did not flood, while many blocks outside the floodplain did flood; and (b) mortgage-holders inside the 100-year floodplain are generally required by their lenders to hold flood insurance, while take-up of flood insurance outside of the floodplain is rare. As a result, we can compare the treatment effect of flooding on two groups that differ in their likelihood of having flood insurance (ex-ante coverage) versus relying on disaster assistance (ex-post coverage).

Our results confirm, as expected, that low ability-to-repay homeowners face hurdles in obtaining SBA loans and, more surprisingly, FEMA IHP grants. From regression analyses – which control for potentially confounding factors like the flood insurance and flood damage – we find that registrant homeowners with damage are 60% less likely to be approved for an SBA loan when they live in blocks where residents are less likely to be able to repay an SBA loan. On the intensive margin, per dollar of damage, 28% less in SBA loan dollars flowed into the pockets of residents in these same blocks after Harvey. Curiously, FEMA assistance does not appear to counteract this funding disparity for homeowners. In low ability-to-repay blocks, homeowners have a 7% lower probability of receiving a FEMA IHP grant and, conditional on receiving a FEMA grant, they receive $921 less in aid, which is about 8.3% of the average dollar amount of

[4] We split this index at the median into low and high ability-to-repay individuals and blocks. A low value of this index is highly predictive of SBA loan denials. See Table 4 and Appendix Table A1, where *t-statistics* on the ability-to-repay coefficient estimate range from 12.61 to 18.96, depending on specification. Note that, since all inputs are captured pre-treatment, they cannot be endogenously affected by Harvey's flooding.

assistance to homeowners ($11,085). We conclude that disaster assistance is regressive in allocation and, therefore, not neatly characterized as a safety net program. Hence, we should expect flooding to generate a wide treatment effect distribution on credit outcomes outside the floodplain and a tighter distribution inside the floodplain.

We evaluate this expectation using difference-in-difference regressions, where treatment is the flood intensity of the block and outcomes are bankruptcy and delinquency rates. We find a 1.4 percentage point or 39% increase in the bankruptcy rate in heavily flooded areas outside the floodplain (relative to similar not-flooded areas) after Harvey. Importantly, this treatment effect is present only in blocks where there reside a large share of homeowners who are unlikely to be approved for an SBA loan due to their low ability-to-repay. Based solely on the Q4 2017 bankruptcy hazard for this subset of homeowners, we can account for 28% of the additional bankruptcies that occurred in the region during the year following Harvey. The share of outstanding debt in severe delinquency follows a similar pattern. Outside the floodplain, heavy flooding and limited access to SBA loans (as instrumented for by initial ability-to-repay) are associated with a 1.95 percentage point or 10% relative increase in the share of debt in severe delinquency.

When we do not distinguish treatment effects by proxies for the SBA's ability-to-repay criteria, we observe no significant change in the overall bankruptcy hazard and only a temporary increase in the delinquent debt share according to flood intensity. This result is consistent with prior research pointing to a limited effect of natural disasters on the financial distress of the average household (e.g., Gallagher & Hartley, 2017). And, in keeping with the idea that flood insurance reduces reliance on disaster assistance, we find an insignificant treatment effect of flooding inside the floodplain that does not vary by ability-to-repay. Put together, our results signal a netting of the financial effects of flooding across households with a different initial financial condition, producing mild, temporary effects, on average. Importantly, this netting may lead to an overestimation of how effectively federal disaster programs mitigate the burden of natural disasters on households in the lower quantiles of the wealth distribution.

A confounding problem in interpreting the mechanism behind these results is that eligibility for SBA loans is regressive along dimensions, like income and credit score, which are also correlated with wealth. How, then, can we determine whether the regressive allocation of disaster assistance is a contributing factor when initial wealth disparities, alone, might be sufficient to produce heterogeneous treatment effects of flooding on credit outcomes? We point to three results. First, borrowers with a high ability-to-repay who live outside the floodplain (i.e., those with the most access to disaster assistance) become significantly less delinquent after experiencing flooding relative to counterparts in areas that did not flood. Second, relative to their not-flooded counterparts, homeowners in flooded areas with a high ability-to-repay become less likely to take out sales financing and do not increase their use of home equity loans. These loan types finance large purchases and construction and, therefore, are the most likely substitutes for SBA loans. If disaster assistance acted primarily as a safety net program, we would not expect to find relative reductions in both delinquency shares and the use of sales financing among more well-off borrowers in flooded areas relative to their counterparts in not-flooded areas. Third, the greater the share of registrants in an individual's block that receive SBA loans the less that individual's delinquent debt share rises after flooding. Put together, these findings suggest that the regressive allocation of disaster assistance is contributing to the wide treatment effect distribution outside the floodplain.

It is reasonable to ask whether these results may generalize to other disasters. Our tests suggest that Hurricane Harvey is an excellent laboratory to understand the disparate impact of hurricanes on financial outcomes more broadly. Relative to all U.S. hurricanes that have hit large urban areas (>1 million people) between 2000 and 2017, we find Hurricane Harvey near the median in terms of median income, college-educated share, the construction share of employment, as well as other factors that might correlate with both financial outcomes and access to aid after a disaster. By contrast, Hurricane Katrina is an outlier on most of the dimensions tested. Moreover, the SBA's use of a risk-insensitive interest rate is common across disasters. It is, therefore, reasonable to expect this financial friction to play a role in separating the financial recoveries of different types of households after other disasters as well. Still, we caution against extrapolating our precise estimates to other disasters because the generosity and criteria for approval of disaster assistance are not held constant over time.

2 Data

This section begins with an overview of the disaster assistance landscape and the data used to evaluate the allocation of disaster assistance after Harvey. Next, we describe the credit bureau data that is later used in a difference-in-difference design to estimate the impact of flooding on the bankruptcy and delinquency rates.

2.1 Disaster Assistance Overview

A Presidential Disaster Declaration was announced for the Houston area due to Hurricane Harvey, which opened up access to several forms of government assistance, as described here:

***Flood insurance*:** The National Flood Insurance Program (NFIP), which is managed by FEMA and delivers subsidized flood insurance to the public through a network of approximately 60 insurance companies, covers up to $250,000 for the structure of a home and $100,000 for personal property. The average NFIP payout in Texas due to Harvey was roughly $121,000, according to the Texas Department of Insurance. If an individual owns a home with a mortgage that is located in a 100-year floodplain, that person is required to have flood insurance. The low levels of insurance across Houston suggest that few individuals purchase coverage when they are located outside of the floodplain. Flood insurance gives an initial payout (approx $7,000) to deal with immediate expenses. If under a mortgage, the rest of the insurance payout is typically held in escrow by the lender and released in disbursements to complete repair work or to pay down the mortgage (Gallagher & Hartley, 2017). Importantly, since Federal disaster assistance covers only *uninsured* disaster losses, households located inside the floodplain are less likely to be reliant on FEMA and SBA assistance to finance their recovery. We exploit this feature of disaster assistance in our research design.

SBA disaster loans: By dollar volume, SBA loans (also known as the Federal Disaster Loan Program) are a dominant form of assistance to disaster-affected individuals. Loans reach $200,000 for a primary residence and $40,000 for personal property. The program is partially subsidized, such that loan terms are extremely attractive. In the context of consumers affected by Harvey, most approved applicants (86%) received an interest rate of just 1.75% and were given 30 years for repayment. SBA loans can be used to relocate and, under specific conditions, additional funds are available to refinance a mortgage, such that households may choose to switch from a traditional mortgage to an SBA loan.[5]

Eligibility is based on (1) disaster-related losses; (2) satisfactory credit; and (3) repayment ability, which is based on an income floor and a debt-to-income ceiling. Restricting eligibility allows the SBA to achieve its goal of making loans that are limited in their extent of government subsidy (for every dollar the program lends it expects to receive 87 cents) using a single interest rate for most borrowers (Collier & Ellis, 2019). The SBA's goal of limited tax-payer subsidy introduces a friction into the disaster recovery process since those with the fewest resources to recover may also be the least likely to be approved for a loan. Moreover, there is an idiosyncratic component to both SBA and FEMA assistance which leads to otherwise similar people just barely qualifying or being denied assistance. We will use this feature of disaster assistance later in this paper to test whether heterogeneity in credit outcomes after flooding can, at least in part, be attributed to disparities in access to disaster assistance.

FEMA grants: The application process for any federal assistance begins with FEMA registration. FEMA will, then, examine the damage and validate registrant identity and occupancy status. Households may receive cash assistance through FEMA's Individuals and Households Program (IHP) up to the difference between expenses incurred from the storm and other forms of assistance provided (including insurance payouts and SBA loans). At the high-end, a homeowner who can prove her home is unlivable can be given up to $33,000 and most recipients can obtain housing assistance of $2,000 a month for up to 2 months. However, the data show that the typical IHP recipient from Harvey received just $7,300.

A recent U.S. Government Accountability Office (GAO, 2020) report identifies several features of the FEMA approval process that may make grants less accessible, particularly to low-income Americans. First, the report cites the FEMA appeals process as an impediment. Following Hurricane Katrina in 2005, FEMA became more restrictive to protect against fraudulent claims. As a result, approval rates have fallen dramatically over time, from 63% in 2010 to around 13% in 2021.[6] The appeal process is one way of screening out potentially fraudulent initial claims. Although a quarter of appeals are approved, less than 5% of originally denied applicants appeal according to the GAO. A failure to appeal may be explained by FEMA's determination letters, which the GAO describes "as unclear and incomplete...requiring a reading level of a high school senior." The GAO concludes that "survivors have trouble understanding the letters and how to respond, and may stop pursuing assistance after receiving a FEMA determination letter because they believe the letter represents a final denial."[7]

[5]For more information on SBA loans, see Section 4.3 of this paper and page 51 of `https://www.sba.gov/document/sop-50-30-9-disaster-assistance-program-posted-05-31` as well as `https://fas.org/sgp/crs/homesec/R45238.pdf`.

[6]See Dreier, Hannah (April 25, 2021), "Assistance not Approved," *The Washington Post*. Available at `https://www.washingtonpost.com/nation/2021/04/25/fema-disaster-assistance-denied/?itid=hp-top-table-main`

[7]Those disaster victims who are savvy enough to appeal often lack the assistance they need to complete their appeal. The GAO writes: "FEMA staff that we interviewed in all four of the IHP's call centers noted that they could not maintain awareness

Another impediment is the requirement that some disaster victims (those with income just above a certain cut-off or who report being self-employed) must first be denied an SBA loan before they can be eligible for a FEMA IHP grant. Specifically, the GAO report described this process as "a barrier that prevented many potentially low-income IHP applicants with FEMA-verified personal property losses from being considered for personal property assistance."

Finally, on-the-ground investigative reports have cited other reasons for denials.[8] In one case, FEMA's automated scan of public records databases did not return proof that an applicant owned her home. The greater levels of housing instability in low-income communities also create paperwork issues that require legal aid. An example of this is an unregistered title or heirs' conflict on a property that was inherited (common in low-income and minority communities). In another report, a FEMA applicant was denied further assistance because she "misused funds" when she boarded with a friend while using the rental assistance to replace a damaged car instead of pay rent.[9]

There are two other forms of federal disaster assistance, ***IRS disaster refunds*** and ***forbearance***, that we discuss only briefly in this paper. First, households can file an amended tax form with the Internal Revenue Service (IRS) based on the uninsured loss of property incurred in the storm. This process is relatively quick and can lead to tax refunds for individuals with higher incomes and, thus, with greater tax liabilities. Second, after Hurricane Harvey, Housing & Urban Development (HUD) issued a 90-day moratorium on foreclosures and forbearance on mortgage payments throughout the Houston area. Fannie Mae and Freddie Mac offered forbearance on mortgage payments for three-month intervals (up to 12 months). Interest on balances continues to accrue during forbearance.[10] Since delinquencies are not reported to credit bureaus on debt that is under forbearance, our measures of delinquency will necessarily be an underestimate of the true rate of delinquency after Harvey.

2.2 Data: Disaster Assistance

Our primary disaster assistance data comes from FEMA and contains the individual records for each household that registered with FEMA in the months following Harvey.[11] This data provides details on

of IHP guidance because of its large volume and frequent changes to it, which affected the quality of their customer interactions and the consistency of their casework supporting award determinations."

[8] For the report of a denial based on public deeds information, see the source in Footnote 6. For anecdotal reports regarding the unapproved use of FEMA assistance and paperwork issues, see `https://www.npr.org/2019/03/05/688786177/how-federal-disaster-money-favors-the-rich` and `https://www.theatlantic.com/politics/archive/2017/08/seeking-legal-help-in-the-middle-of-hurricane-harvey/538488/`.

[9] Although FEMA grants are fungible, the money comes with a letter explaining acceptable uses for the payment. An applicant that misuses the assistance may be denied future assistance or be asked to return all funds. Those receiving assistance are urged to keep receipts and random audits are conducted to confirm funds were spent properly. See `https://www.fema.gov/news-release/2016/07/08/fema-those-who-receive-assistance-use-funds-its-intended-purpose`.

[10] Nonparametric means of delinquency by debt type, presented in Appendix Figure A1, signal that forbearance was indeed granted on mortgages, auto loans, and, possibly, on student loans in Q4 2017.

[11] The data was downloaded directly from FEMA's public data website ("OpenFEMA Dataset: Individual Assistance Housing Registrants Large Disasters - V1") and includes all Harvey-related registrations in the year following Harvey. To ensure a degree of consistency between the disaster assistance analysis and the credit analysis, we restrict the sample of Census blocks in the disaster assistance data to those in which we draw at least one credit file – i.e., blocks that are represented within the 5% random

individual-level FEMA assistance, estimated damage determined by property inspection, flood insurance status, as well as information on Census block of residence.[12] We find that, on average, about 33% of housing units located in a block with any flooding registered with FEMA and, of those registered with FEMA, about 80% were deemed either *not* eligible or unable to demonstrate the need for FEMA assistance. This high unconditional denial rate, in part, reflects the fact that many registrants were determined by FEMA inspection to not have qualifying damage. We, therefore, control for and subsample on homeownership, property damage, and insurance status in regressions.

Importantly, the FEMA data also tells us whether or not a FEMA registrant was eventually determined eligible by the SBA for a loan. We use this field in the FEMA data as our extensive margin measure of SBA approval (i.e., the probability of a FEMA registrant being approved for an SBA loan). Note, however, that the unconditional probability of SBA approval in the FEMA data (4%) will be much lower than the same probability within the subsample of registrants that applies for an SBA loan (43%). This is because many FEMA registrants are denied an SBA loan before they apply because either (a) they do not have qualified damage or (b) they fail an income test. We account for (a) in regressions by controlling for or subsampling on homeownership, property damage, and insurance status. Indeed, if we condition the FEMA data on having uninsured damage of greater than $10,000, the SBA approval rate in the FEMA data rises substantially to 24%. Concerning (b), including registrants screened out of the SBA process (or essentially, pre-denied) due to an initial income test is an important advantage of using the FEMA data to capture SBA approval rates.[13] By contrast, using the SBA data, described below, would likely understate the true SBA denial rate in low-income samples.

From a FOIA request of the SBA, we obtained individual loan-level information on approved and denied loans, including their timing and value. We use the SBA data to study the intensive margin (dollar value) of SBA loans. In addition to the issues raised in the previous paragraph, we do not explore the extensive margin (approval rate) in the SBA data because, unfortunately, for denied loans, we only have zip code level residence. Zip code is too coarse to find substantial variation in Harvey's flooding pattern. Indeed, there are 247 zip codes versus 32,072 Census blocks in our study. For approved loans, we have detailed address information with which to determine the Census block of residence. The SBA data also does not contain verified property damage amounts. Therefore, we merge the SBA data with the FEMA data to apply block-level measures of FEMA-assessed property damage and flooding.

From the SBA data, we learn that over $2.9 billion in individual home loans were approved for Harvey victims by the SBA. This is almost double the amount given out in the form of FEMA grants ($1.6 billion).

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sample of credit files present in the CCP data described in Section 2.3. This reduces the number of blocks in our disaster data by about a quarter. However, since omitted blocks tend to be smaller in population, this restriction has no meaningful effect on our disaster assistance estimates.

[12]Although the FEMA data contains a field for "self-reported gross income" we find this field to be missing for 18% of Harvey registrants. Not only might missing values be non-random, but applicants may try to under or overstate their incomes to qualify for various programs. We, therefore, rely on Census data to infer socioeconomic information about applicants based on their Census block of residence.

[13]Specifically, FEMA only refers registrants to the SBA Disaster Loan Program if a registrant's income meets the SBA minimum guideline, which is around 1.5 times the poverty line, or if the registrant is self-employed (Congressional Research Service, 2019; GAO, 2020). If a FEMA registrant is referred to the SBA, he must first apply to the SBA before he can receive a FEMA grant. This rule provides a strong incentive for those FEMA registrants who are referred to the SBA to apply for an SBA loan. The SBA, then, communicates with FEMA about whether a formal SBA loan application was eventually approved or not.

Conditional on approval, the average approved SBA loan amount was $74,549, which would fully cover the average amount of property damage incurred due to 1 foot of flooding in a home ($72,162) and is more than 10 times the average FEMA IHP grant ($7,329). Hence, being denied an SBA loan would have a major impact on a household's prospect of recovering financially.

2.3 Data: Consumer Credit

To identify the impact of flooding on credit outcomes, we gather credit information on individual Houston residents from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP), as detailed in Lee & van der Klaauw (2010). The CCP consists of Equifax credit report data for a longitudinal quarterly panel of individuals. The panel is a nationally representative 5% random sample of all individuals with a Social Security number and a credit report. Our final sample contains the quarterly credit information over Q2 2015 to Q4 2019 on 108,707 individuals living in 32,072 Census blocks (i.e., the level of treatment) in the Houston metropolitan area as of Q2 2017 (the last quarterly snapshot before Harvey).

The CCP data include variables that can proxy for "financial distress," our primary outcome of interest in this paper. We focus on indicators of bankruptcy (both chapter 7 and chapter 13) and measures of severe delinquency (90 or more days past due). Since consumers with more debt have more opportunity to be classified as delinquent, we normalize severe delinquencies by the consumer's total outstanding debt and call this variable: *Share of debt in delinquency (%)*.[14] Additional variables used from the CCP include Census block of residence, year of birth, the number of credit inquiries initiated by the consumer in the past 3 months, and the number of new accounts opened.

This data is joined, at the Census block, with the geospatial flooding and socio-economic data, gathered primarily from FEMA and Census, as described in Appendix B. This appendix details how we create a block-level measure of floodplain coverage to explore whether household debt responses vary according to the likelihood of flood insurance coverage. Appendix B also explains how we construct our treatment intensity measure, *WAvg. Flood Depth*, which captures the weighted average flood depth across the developed area of a Census block. This treatment variable is computed by multiplying the depth of flooding (the average flood depth in the flooded developed land area within a block) with the breadth of flooding (the share of the developed land area flooded) at the block level. We confirm that this composite measure of flooding is more predictive of both FEMA registration and FEMA-determined property damage in a block than available alternative measures of flooding.

Key to our study is an exploration of heterogeneity in treatment effects according to initial (pre-treatment) financial condition. Recall, an important friction motivating this paper is that the SBA ap-

[14] The CCP also offers information on balances for major consumer debt categories. We focus on the combination of installment and revolving home equity loans, which may be used to finance home repairs, as well as sales financing, which is typically used to purchase large items like refrigerators (which can be damaged in floods). A smaller share of this latter category comes from personal loans from banks. The CCP also contains information on student, auto, mortgage, and credit card balances. We explore student debt outcomes in detail in our companion working paper (Billings et al., 2020). Comprehensive auto insurance, required by auto lenders in Texas, typically covers flooding. Auto debt is, therefore, not of primary interest. For analysis of mortgage debt after flooding, see Du & Zhao (2020). We do not explore credit card debt in this paper because the CCP does not provide information on the portion of outstanding credit card balances that are revolving. del Valle et al. (2019) apply CCAR Y-14M data to the question and find little association between flooding and revolving balances for the average account after Harvey; however, these authors do not explore the role of initial financial condition.

proves only those borrowers who they believe will be able to repay the loans. We identify four variables that proxy for the factors the SBA uses when making an ability-to-repay decision. The variables include the individual's *Equifax Risk Score*[15]; the individual's *credit card utilization rate*[16]; as well as the *median income* and *minority share* (from the 2010 Census) of the block where the individual lived as of Q2 2017. The first two variables are also closely related to the two primary screening factors used by the SBA when determining loan eligibility: FICO score and debt-to-income ratio. *Equifax Risk Score* scores are a close proxy for FICO scores. And, although we do not observe individual income, credit card balance limits are tied to income. So, if an individual has high *credit card utilization* as of Q2 2017, it would imply that she had a high debt-to-income ratio, making her less likely to qualify for an SBA loan. The median income of the consumer's Census block should also be correlated with debt-to-income ratios. Finally, minority status is correlated with income and credit score (Beer et al., 2018; Kabler, 2004) as well as with any subsequent asset tests (Sullivan et al., 2015).[17]

The fact that the SBA uses multiple highly correlated factors to determine ability-to-repay an SBA loan, we believe, leads naturally to the use of a principal components analysis (PCA).[18] As documented in Table 1, the first principal component explains 44% of the variation in these variables and the loadings have expected signs. We build an index of "ability-to-repay" an SBA loan by taking the sum of individual observations of these variables multiplied by their factor loadings. We, then, discretize the index by dividing it at the median, such that *low ability-to-repay* individuals have a value of one for $\mathbb{1}(\mathtt{Low\_Ab2Repay}_i)$. For block-level analyses, we split blocks at the median based on the block's share of sample residents classified as *low ability-to-repay*. In this case, blocks with an above-median share of sample residents with *low ability-to-repay* will have a value of one for $\mathbb{1}(\mathtt{Low\_Ab2Repay\_Share}_b)$. For transparency, we also show results using just the *minority share* of the individual's Census block.

In Appendix Table A1, we test the PCA approach against an alternative approach of applying fitted values from a cross-sectional regression of block-level SBA loan approval rates on block-level medians of the four input variables.[19] The two approaches result is nearly identical explanatory power. All individual input variables have the expected sign and large t-statistics – indicating they are each predictive of SBA loan approval. Of each variable tested, the PCA index has the highest t-statistic (12.61) and the largest (in

[15]The *Equifax Risk Score* is a trademarked measure of the likelihood that a consumer becomes seriously delinquent (90 days past due). We exclude from our sample individuals who are not scored due to a lack of credit history. We do not study the *Equifax Risk Score* as an outcome variable in this paper because it is both bounded and a key source of heterogeneity, thus complicating the interpretation of differential treatment effects on credit scores.

[16]An individual's credit card utilization rate is estimated as the sum of balances across all revolving credit card accounts, divided by the total high credit summed across those accounts. High credit is defined as the credit limit associated with an account or the highest recorded credit balance if the credit limit is not reported. For the individuals who do not have a credit card, we give them the value of the standardized mean (zero), effectively giving the utilization loading in the PCA a weight of zero.

[17]According to Sullivan et al. (2015), the typical black household has just 6% of the wealth of the typical white household. The typical Latino household has just 8%. Hence, individuals in higher-minority share areas are likely to be more credit and resource-constrained. Moreover, minority share is the key measure of a county's "need for price discrimination" employed by Begley et al. (2018) in their study of SBA loan access.

[18]All inputs into the PCA are standardized continuous variables captured before Hurricane Harvey such that they cannot be endogenously affected by Harvey's flooding. This method has elements in common with Gallagher et al. (2020a), who classify individuals as more or less likely to be in financial hardship through an index constructed using a PCA.

[19]Note that when selecting the best measure of SBA loan access for an individual or block, we face an important limitation: any merge between our anonymous credit data and the FEMA data (which contains the SBA approval indicator) must be done at the block-level. This is why, in Appendix Table A1, we test block-level measures of SBA loan approval and credit variables.

absolute) standardized coefficient estimate. Importantly, the fitted value method, due to multicollinearity between these variables in multivariate regressions, would give little-to-no weight to *credit card utilization* or *minority share*, despite independent t-statistics of 6.3 and 7.7, respectively. We, therefore, opt for the PCA since it is likely to maximize variation across both blocks and individuals within a block.

Table 1: Principal components analysis of variables related to SBA ability-to-repay decisions

Panel A. Eigenvalues of the correlation matrix				
	Eigenvalue	Difference	Proportion	Cum.
Comp1	1.75	0.70	44%	44%
Comp2	1.05	0.43	26%	70%
Comp3	0.62	0.05	16%	86%
Comp4	0.57		14%	100%

Panel B. Corresponding eigenvectors				
	Comp1	Comp2	Comp3	Comp4
Equifax Risk Score	0.55	-0.31	0.74	-0.25
Credit card utilization rate	-0.32	0.77	0.52	0.14
Block-group median income	0.56	0.37	-0.004	0.75
Block minority share	-0.54	-0.41	-0.43	0.60

This table describes the principal components of standardized variables that proxy ability-to-repay (and, hence, approval for) an SBA loan: *Equifax Risk Score, credit card utilization, block-group median income, block minority share*. These variables are captured pre-treatment. Credit variables are captured as of Q2 2017. Socioeconomic variables come from the 2010 Census measure for the Census block where the individual lived as of Q2 2017. In Panel A, the eigenvalues for different components and a variance decomposition are reported. In Panel B, the factor loadings used to construct our index of *ability-to-repay* are reported.

Table 2 presents descriptive statistics using block-level variables (Panel A) and individual-level credit information (Panel B). In general, flooding impacted all income groups with a slight preference for wealthier parts of Houston. Median income was $74,448 in the most flooded quartile of blocks versus $65,905 in the no-flood blocks. We find slightly higher debt balances in more flooded areas, consistent with higher socio-demographic classes having higher incomes, higher home values, and greater access to credit. Average credit scores were 14 points higher in the most flooded blocks. Relative to individuals in no-flood blocks, the most flooded blocks had $13,025 more in mortgage debt. Despite their higher balances, the most treated blocks were slightly less likely to be delinquent on their accounts pre-hurricane.

Table 2: Descriptive statistics

Panel A. Block-level variables, pre-Harvey

						Mean of flooding treatment bin			Mean by median split of: $\mathtt{Ab2Repay\_Share}_b$	
Variable	Mean	S.D.	p10	p50	p90	No flood	Q1-Q3	Q4	High	Low
WAvg. Flood Depth (ft.)	0.27	0.81	0.00	0.00	0.84	0.00	0.21	2.18	0.31	0.23
Avg flood depth of flooded (ft.)	2.82	5.73	0.00	0.00	9.95	0.14	5.32	12.41	3.23	2.38
Flooded share of dev. area	0.10	0.05	0.00	0.00	0.05	0.00	0.04	0.18	0.10	0.10
Floodplain share of dev. area	0.11	0.27	0.00	0.00	0.44	0.05	0.20	0.23	0.10	0.11
Elevation (ft)	82.44	50.45	30.00	70.00	140.00	80.76	84.64	86.46	91.30	72.65
Distance to stream (ft)	4,236	4,317	283	3,188	8,954	4,541	3,829	3,515	4,298	4,167
Median income ($)	68,694	37,893	29,417	59,538	123,077	65,905	72,689	74,448	90,062	45,080
Median home value ($)	162,622	121,035	71,500	122,500	290,400	155,753	171,677	179,100	215,451	103,182
Share owner-occupied	0.72	0.25	0.34	0.80	0.96	0.72	0.74	0.74	0.82	0.62
Share below poverty	0.14	0.12	0.01	0.10	0.32	0.15	0.13	0.12	0.08	0.21
Minority share	0.39	0.27	0.08	0.33	0.83	0.41	0.36	0.33	0.22	0.57
Rate of bankruptcy (%)	2.25	10.67	0.00	0.00	0.00	2.31	2.12	2.26	1.76	2.80
$\mathbb{1}(\mathtt{Low\_Ab2Repay\_Share}_b)$ (%)	51.30	44.64	0.00	50.00	100.00	54.63	46.59	44.19	11.44	95.34
Observations	32,072					19,691	9,278	3,103	16,036	16,036

Panel B. Individual-level variables, Q2 2017

						Mean by flooding treatment bin			Mean by median split of: $\mathtt{Ab2Repay}_i$	
Variable	Mean	S.D.	p10	p50	p90	No flood	Q1-Q3	Q4	High	Low
Equifax Risk Score	682.39	113.27	520.00	703.00	817.00	677.93	686.27	691.66	755.28	612.56
Share of debt in delinquency (%)	10.76	28.25	0.00	0.00	51.19	11.55	9.93	9.55	1.46	20.07
New accounts per inquiry (#)	0.28	0.54	0.00	0.00	1.00	0.28	0.29	0.28	0.33	0.25
Mortgage balance ($)	43,623	82,113	0	0	164,180	39,382	47,336	52,407	68,862	19,446
Home equity loan balance ($)	796	3,753	0	0	0	779	801	856	1,120	484
Credit card utilization rate (%)	38.59	104.18	0.17	24.45	97.69	39.62	37.57	36.92	24.02	59.08
Sales financing balance ($)	576	1,463	0	0	2,235	585	565	566	402	742
$\mathbb{1}$(Mortgage>0)	30.79	46.16	0.00	0.00	100.00	28.95	32.47	34.35	42.50	19.57
$\mathbb{1}$(Home equity loan>0)	4.87	21.53	0.00	0.00	0.00	4.82	4.82	5.26	6.67	3.15
$\mathbb{1}$(Sales financing>0)	22.96	42.06	0.00	0.00	100.00	23.57	22.55	21.29	15.84	29.78
$\mathbb{1}$(Bankrupt)	0.02	0.14	0.00	0.00	0.00	0.02	0.02	0.02	0.01	0.03
$\mathbb{1}(\mathtt{Low\_Ab2Repay}_i)$	0.50	0.50	0.00	1.00	1.00	0.54	0.48	0.44	0.00	1.00
Observations	108,707					58,700	37,525	12,482	54,353	54,354

All credit variables are captured as of Q2 2017. Socio-demographic variables (e.g. Minority share) are captured as of Census 2010. Median household income is measured at the Census block-group geography. Geospatial (e.g. Elevation) and flood variables are according to the U.S. Geological Survey (USGS), encompassing the most up-to-date information as of October 2018 (before new flood zones were created in response to Harvey). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel, USGS/FEMA, Census.

3 Empirical Strategy

This section describes the two different empirical strategies used in this paper – the first is applied to the disaster assistance data and the second is applied to the consumer credit data.

3.1 Disaster Assistance: Empirical Strategy

We begin with simple OLS regressions to test for differential access to SBA loans after Harvey:

$$\mathbb{1}(SBA_i > 0) = \alpha + \beta_1 \mathbb{1}(Low\_Ab2Repay\_Share_b) + \beta_2 \mathbb{1}(Insurance_i) + \beta_3 Damage_i + B_b \Phi + \varepsilon_i \tag{1}$$

$$Avg\ SBA\$_b = \alpha + \beta_1 \mathbb{1}(Low\_Ab2Repay\_Share_b) + B_b \Phi + \varepsilon_b \tag{2}$$

In Equation 1, the unit of observation is the individual FEMA registrant i, who lives in block b. Recall that registering with FEMA is the first step in obtaining an SBA loan and FEMA tracks whether an SBA loan is eventually approved for each of their registrants. The dependent variable is, therefore, FEMA's indicator for SBA approval, $\mathbb{1}(SBA_i > 0)$, and captures the extensive margin effect. To test for intensive margin effects, in Equation 2, the dependent variable comes from the SBA data and it is not directly linkable to individual FEMA registrants. It is the average SBA loan amount (in dollars) per FEMA registrant for a given Census block, $Avg\ SBA\$_b$.[20] In all equations, the key explanatory variable, $\mathbb{1}(Low\_Ab2Repay\_Share_b)$, is the discretized block-level measure of *ability-to-repay,* where blocks with an above-median share of sample residents with low ability-to-repay get a value of one. We also test for heterogeneity along the more transparent dimension of the *minority share* of the Census block.

Importantly, Equation 1 controls for factors – namely, the flood insurance status and property damage of an individual FEMA registrant – that, by design, affect the allocation of disaster assistance. Therefore, our findings cannot be explained, for example, by a tendency for lower-income individuals to experience less uninsured flood damage. Both equations include block-level controls, B_b – including flood depth (*WAvg. flood depth*), population density, owner-occupied housing share, the share of registrants with flood insurance, the share of registrants with damage, and the share of housing units registered with FEMA. Finally, to absorb any differential selection into the FEMA registrant pool, we include an interaction between these latter two variables, which produces a measure of the share of housing units with damage.

Thus, while previous literature on SBA loans exploits county-level variation across many disasters, we compare outcomes across Census blocks within one city and one disaster. We, therefore, confirm that the SBA lending disparities according to minority share, first documented in Begley et al. (2018), are unlikely to be driven by any disaster-related changes in, for example, the local labor market that might independently

[20] Specifically, $Avg\ SBA\$_b$ is calculated as the total dollar amount of SBA loans distributed to all individuals (not businesses) in a block normalized by the number of FEMA registrants in the block. We include in this analysis only Census blocks with at least one SBA loan. We weight the block-level regressions by the number of applicants in a block to avoid the undue influence of a large number of blocks with only a handful of FEMA registrants.

affect access to disaster aid and credit outcomes.

Next, since FEMA grants may help fill a household's funding gap when an SBA loan is denied, we evaluate whether registrants from low ability-to-repay areas are more or less likely to receive any FEMA IHP aid and the amount of that aid. We apply OLS regressions of the following form:

$$y_i = \alpha + \beta_1 \mathbb{1}(Low\_Ab2Repay\_Share_b) + \beta_2 \mathbb{1}(Insurance_i) + \beta_3 Damage_i + B_b \Phi + \varepsilon_i \quad (3)$$

In Equation, we evaluate two dependent variables (y_i). The first is an indicator for whether a FEMA registrant received any FEMA IHP aid, $\mathbb{1}(FEMA_i > 0)$. The second is the amount of that aid granted to individual i in dollars, $FEMA\$_i$. To isolate intensive margin effects from extensive margin effects, we limit the sample used with the second dependent variable to households that received some non-zero amount of assistance.

3.2 Consumer Credit: Empirical Strategy

After we characterize the allocation of disaster assistance, we explore how factors correlated with this allocation might mediate the effect of flooding on consumer credit outcomes. Our empirical strategy involves a difference-in-difference (DiD) design of the form:

$$y_{it} = \beta\,(T_b \times P_t) + \alpha_i + D_t + \kappa A_{it}^2 + (X_b \times P_t)\,\eta + X_b \phi + \varepsilon_{it} \quad (4)$$

where y_{it} is a quarterly credit outcome for individual i living in Census 2010 Block b in quarter-year t. Our primary outcomes of interest are delinquency and bankruptcy. P_t is the post-hurricane dummy, which gets the value of one during all periods after Q2 2017.[21] T_b is *WAvg. Flood Depth* – the treatment intensity associated with the block b where individual i resided as of Q2 2017 (the last quarterly observation before the hurricane). Therefore, the coefficient of interest, β, captures the effect of living in a block of a particular flooding intensity relative to the outcomes of the same set of blocks during the pre-hurricane period and relative to the post-hurricane outcomes of blocks that did not flood. Through triple interactions and subsample tests, we explore how β varies by initial financial condition (ability-to-repay and minority share) as defined in Section 2.3.

Since an individual is assigned to a treatment intensity according to the block where that individual lived in as of Q2 2017, we allow people to move around Houston and the rest of the country before and after the storm, holding their treatment intensity constant throughout time. Individuals that did not live in a Houston Census block at the dawn of the hurricane (Q2 2017) are excluded from the sample. Bear in mind, we must contend with an unknown amount of measurement error in assigning individuals to treatment since our credit data only provides geographic information at the level of a Census block. Therefore, we do not observe the exact degree of flooding experienced by individuals in our sample, rather we observe a proxy for their probability of having been flooded. This type of measurement error will attenuate estimates.

[21]Note that the hurricane hit during Q3 2017, with the Q3 2017 snapshot occurring less than a month after the storm passed. Appendix Figure A2 (Panel A) shows that the out-migration captured by Equifax is immediate, peaking in the Q3 2017 snapshot – suggesting to us that it is appropriate to include Q3 2017 in the "post" period.

Thus, our estimates should be viewed as intent-to-treat (ITT) effects. The effect of being flooded on credit outcomes may be larger in magnitude than the effects identified in this paper. Moreover, our proxy for flooding is imperfect. The exact depth and breadth of flooding merely represent FEMA's best guess based on hydrological modeling.

The treatment variable, *WAvg. Flood Depth*, is discretized and included in regressions in two forms. First, in event studies, we test the easily interpretable treatment effect of any flooding (i.e., $T_b = 1$ when *WAvg. Flood Depth*>0) relative to no flooding. Second, in regression tables, we present the treatment effect for different quartiles of flood depth. In particular, we bin *WAvg. Flood Depth* into three groups of blocks: no flood blocks (control), the bottom three quartiles of flood depth among flooded blocks (T_b^{Q1-Q3}), and the most flooded quartile of flooded blocks (T_b^{Q4}). We highlight the top quartile because the most flooded quartile (T_b^{Q4}) tends to display disproportionately larger debt responses relative to less flooded quartiles, where the majority of homes may have escaped the nearby flooding. Non-linearities in treatment might be expected given the measurement error mentioned in the previous paragraph as well as unobserved threshold effects in flooding (e.g., a car is undamaged by 6 inches of flooding but destroyed by 12 inches of flooding).

In terms of control variables, α_i is an individual fixed effect and D_t is a year-quarter fixed effect, which functions to demean the outcome within an individual and across individuals within a quarter. Because an individual's treatment intensity is time-invariant, we cannot include a block-fixed effect as it would be collinear with α_i. We control for the square of age, A_{it}^2, since this value increases each year at an increasing rate (making its marginal effect discernible from α_i and D_t) and consumer finances are subject to strong life cycle effects (Low et al., 2010; Iacoviello & Pavan, 2013; Fulford & Schuh, 2017). Finally, with X_b, we control for several characteristics of the Census block where the individual lived as of Q2 2017 (median income, owner-occupied share, population density, median home value, and floodplain share of developed block area). These control variables are captured pre-treatment and interacted with a post-period dummy such that, $X_b \times P_t$ absorbs any debt behaviors after the hurricane that are common to individuals from certain types of blocks irrespective of flooding. Note that outside of the post-period interaction X_b drops out of the regression since it is collinear with the individual fixed effects. Standard errors are clustered at the Census block where the individual lived as of Q2 2017.[22]

We employ two variations of the above model. First, we run event studies of the form shown in Equation 5, plotting the β_τ coefficients at each date in our sample period. With this setup, we can study how long it takes for credit outcomes to respond to flooding and how long treatment effects last. This model also helps establish that the treatment and control groups are subject to similar pre-trends. The β_τ coefficients can be interpreted as the quarterly change in the outcome variable for residents living in flooded blocks, as compared with this change for residents in non-flooded blocks, relative to any difference in that outcome that existed in the quarter before Hurricane Harvey ($\tau = 0$ in Q2 2017).

$$y_{it} = \sum_{\tau=-8}^{10} \beta_\tau \left(T_b \times D_\tau\right) + \alpha_i + D_\tau + \kappa A_{it}^2 + \left(X_b \times P_t\right)\eta + e_{it} \tag{5}$$

[22]When we two-way cluster on Census block and time, standard errors are less conservative. This may indicate that we have too few time periods to cluster on the time dimension (Angrist & Pischke, 2008).

Second, we evaluate *Bankruptcy* at the block-level as well as at the individual-level. Bankruptcy is a rare event, meaning that, in any given quarter, an individual's likelihood of entering bankruptcy is very small. At the individual-level, linear probability models, as well as discrete choice models (i.e., Probit), are not well-suited to evaluating very rare events (King & Zeng, 2001). Moreover, bankruptcy is an absorbing state since the flag remains on accounts for 7 years, which removes the chance of moving in or out of bankruptcy during that period. To reduce bias from these issues, we calculate the share of each block's residents who have a bankruptcy flag on their credit reports, then we evaluate how that share changes over time according to treatment.[23] To test the robustness of our block-level *Bankruptcy* findings, we also perform individual-level regressions using a discrete-time hazard model version of Equation 5.

3.3 Identification features of Hurricane Harvey

To illustrate the unique identification qualities of Hurricane Harvey, we compare it to Hurricane Katrina, which hit New Orleans, Louisiana in 2005. Harvey made landfall as a Category 4 hurricane on August 25, 2017. It stalled over the Houston, Texas area, dumping 27 trillion gallons of rain (up to 50 inches of rainfall) before finally dissipating on September 2, 2017. This was the largest amount of rainwater ever recorded in the continental United States from a single storm (51.88 inches). Frame et al., 2020 attribute one-third of Harvey's total precipitation and $67 billion of direct economic damages to climate change. In total, Harvey damaged as many as 135,000 homes and caused $125 billion in damage, second in cost only to Katrina (FEMA, 2020).

A large share of the flooding in Houston was unanticipated. The regressions in Table 3 indicate that only about 6% of the variation in our measure of flooding across Houston Census blocks can be explained by pre-determined socioeconomic variables and geospatial attributes like 100-year floodplain status, elevation, and distance to streams. By comparison, Gallagher & Hartley (2017) estimate this same figure to be around 40% for Hurricane Katrina.[24]

Among the most flooded quartile of blocks under Harvey, an average of only 23% of the developed block area was in a designated floodplain (Table 2, Panel A). Under Katrina, over 90% of the most flooded quartile of blocks were in a designated floodplain (Gallagher & Hartley, 2017). In other words, most individuals living in New Orleans that were affected by Katrina were living in a floodplain and, hence, may reasonably have expected flooding. They may also have been insured. Flood insurance is obligatory for federally guaranteed mortgages in the 100-year floodplain. Harvey, therefore, offers a unique opportunity to study heterogeneity in credit outcomes both within and across subsamples that differ in their likelihood of being insured versus relying on disaster assistance.

Harvey was fairly indiscriminate along lines of race, wealth, and education. Table 3 shows that these pre-determined socioeconomic variables explain at most an additional 1% of the variation in flooding across

[23] In block-level regressions, we replace the individual fixed effects in Equation 4 with block fixed effects, α_b, and exclude all individual-level controls. We weight each block aggregate by the number of observations in the block – which is essentially a heteroskedasticity correction. Since blocks with more individuals should have smaller error term variances, weighting by the block sample size improves precision. As before, standard errors are clustered on Census block.

[24] Appendix Figure A3 presents a map of the flooding, further highlighting the imperfect correlation between flooding and the 100-year floodplain.

Table 3: Pre-hurricane correlates of treatment

Dependent variable: *WAvg. flood depth (ft)*							
Floodplain share of developed area	X	X	X	X	X	X	X
Other geospatial variables		X	X	X	X	X	X
Cubics of geospatial variables			X	X	X	X	X
Block group economics variables				X	X		X
Block socio-demographic shares					X		X
Credit variables (block-level means)						X	X
R-squared	0.02	0.03	0.06	0.06	0.07	0.06	0.07
$N = 32,072$							

This table presents the coefficients of determination (R-squared) from cross-sectional OLS regressions at the block-level as of Q2 2017, the last quarter before the arrival of Hurricane Harvey. The dependent variable is our continuous treatment measure (weighted average flood depth across the developed block area). Explanatory variables include the share of the developed block area that is in the 100-year floodplain; other geospatial variables (share of block area that is in the 100-year floodplain, share of the block area that is developed, central elevation of the block, central distance to stream); cubic polynomials of all geospatial variables; block group-level economic characteristics (median income, median home value, share owner-occupied, share with a college degree, share in poverty); block-level demographic characteristics (share black, share white); and block-level averages of credit information (Equifax Risk Scores, total debt, delinquent share of total debt, mortgage balance, credit card balance, auto loan balance, student loan balance). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Houston Census blocks. If anything, the higher socio-demographic classes were slightly more affected by the disaster (Table 2) – which is unusual to the extent that wealth helps to insulate households from risk (e.g., by purchasing homes at higher elevations). Nonetheless, Table 3 indicates that a block's intensity of flooding is mostly exogenous to its average economic and credit characteristics. For example, after controlling for geospatial characteristics, credit variables explain virtually none of the variation in treatment across blocks.

Harvey did not alter the underlying economic rationale for living in Houston, allowing us to more plausibly hold constant the credit and labor market faced by flooded and not-flooded individuals. The Houston economy is diversified and was booming in the years leading up to Harvey. Even in the year after Hurricane Harvey, Houston experienced a net in-migration of 1.3% (US Census, 2020). These facts stand in sharp contrast to the profound economic decay occurring in New Orleans at the time of Katrina (Vigdor, 2008). Indeed, the quarterly out-migration rate of pre-existing Houston residents increases by just 0.3 percentage points after Harvey, according to our credit bureau data, and it varies almost imperceptibly by flood intensity (see Appendix Figure A2). By contrast, Sastry & Gregory (2014) report that 47% of pre-Hurricane Katrina adults from New Orleans no longer resided in the New Orleans area one year later. Appendix Figures A4 and A5 document that any effects of Harvey on employment and wages are short-lived, returning to pre-Harvey trends six-months after the disaster.[25]

A final important feature of Harvey is that we see no evidence of a retraction in the supply of credit (number of new accounts) relative to the demand for credit (number of credit inquiries) after the hurricane, either overall or according to flood intensity (Appendix Figure A6). Hence, any changes in outstanding

[25] As shown in Appendix Figures A4 and A5, we see no changes in total employment and a modest increase in construction employment and wages after the arrival of Hurricane Harvey. These results are consistent with the evidence in Farrell & Greig (2018), who analyze the bank accounts of Chase Bank customers and find that labor income into accounts dropped by 5% the week of Hurricane Harvey, but returned to normal within just 10 days.

debt after flooding are more likely to be demand, rather than supply, driven. Given these unique features of Hurricane Harvey, we can interpret any changes in financial outcomes in affected areas as resulting primarily from the wealth shock of flooding, rather than from a shock to labor markets or credit supply factors.

A downside, however, of focusing on a single natural disaster and geographic area is that our conclusions may not generalize to other areas and natural disasters. To address this question, we present boxplots comparing pre- and post-Harvey economic measures in Houston with those before and after other hurricanes that have hit large urban areas (>1 million people) between 2000 and 2017. The evidence, shown in Appendix Figures A7 and A8, suggests that Houston/Harvey is a representative urban disaster. It exists near the median (or within the interquartile range) along most measures, including its pre-disaster unemployment rate, median income, college-educated share as well as its post-hurricane change in the population, unemployment, and construction share of employment. By comparison, the New Orleans/Katrina disaster is a frequent outlier on these dimensions.

4 Results

This section begins by characterizing the revealed distribution of disaster assistance – SBA loans and FEMA grants – after Harvey and, then, turns to credit outcomes. Finally, we explore the link between the two sets of results.

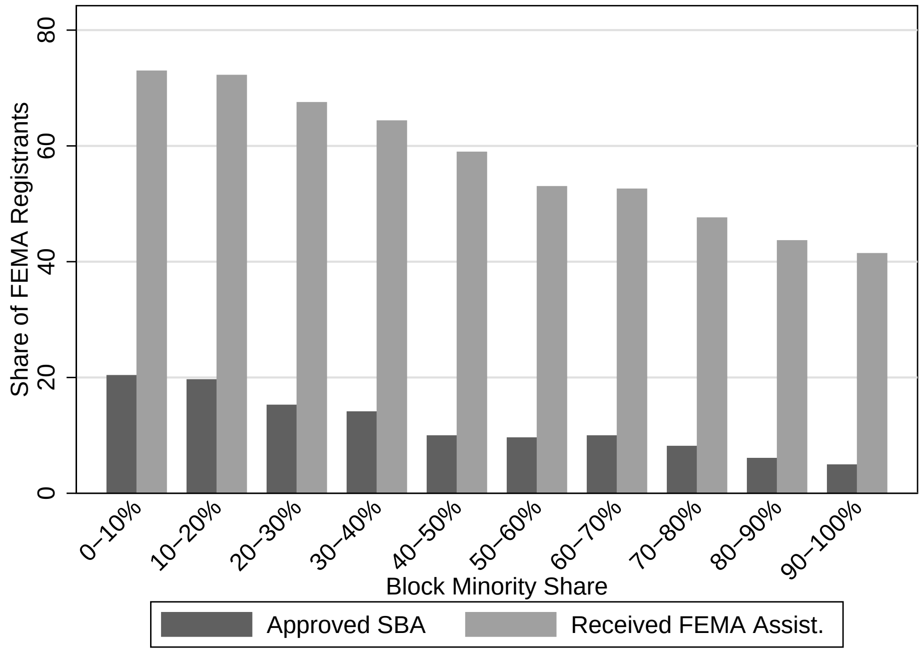
4.1 Disaster Assistance: Results

We begin with a simple nonparametric assessment of how disaster assistance was allocated across Houston neighborhoods after Hurricane Harvey. Since FEMA will not duplicate the benefits of SBA loans and most loan denials are due to "Unsatisfactory credit history" and "Lack of repayment ability" (Appendix Table A2), we should expect to find more SBA loan denials in higher minority areas and, if FEMA IHP acts as a safety net program, more FEMA aid approved in these same areas.

Figure 1 plots the share of FEMA registrants in a Census block that are approved for an SBA loan (black bar) as well as the share that are granted FEMA IHP assistance (gray bar) by the minority share of the Census block (x-axis). The sample is restricted to only FEMA registrants that own a home and had property damage. Presumably, those who are not approved for an SBA loan should be more likely to receive a FEMA grant, such that the descending pattern in SBA lending rates at increasing levels of minority share should be partially offset by an ascending pattern in FEMA aid rates. Instead, the pattern is consistent with disaster assistance acting less like a safety net and more like a reinvestment stimulus program. Relative to blocks with less than 10% minorities, homeowners with property damage in blocks with over 90% minorities have a 15 percentage point lower probability of being approved for an SBA loan and a 32 percentage point lower probability of receiving a FEMA IHP grant.

Despite the clear pattern in Figure 1, we must remember that these simple statistics do not control for flood insurance, flood damage amounts, or the possibility that households in certain types of blocks

Figure 1: Share of FEMA registrants approved by SBA and received assistance from FEMA, by block minority share



This figure plots the share of a FEMA registrants in a Census block that are approved for a SBA loan (black bar) as well as the share that are granted FEMA IHP assistance (gray bar) according to the minority share of the Census block (x-axis). The figure restricts the data to only FEMA registrants with any assessed property damage who owned their home. Data comes from FEMA, which tracks whether its registrants are approved for an SBA loan or not, and includes all FEMA registrants from the Houston metro area related to Hurricane Harvey.

might be more likely to register for assistance per dollar of damage (thus, inflating the y-axis denominator). Moreover, we are yet to relate approval rates to our ability-to-repay index. To address these concerns, we use the regressions specified in Section 3.1.

Table 4 tests for inequalities in access to SBA loans using the specifications in Equations 1 and 2. As expected, Column 1 shows that registrants from *low ability-to-repay share*, $\mathbb{1}(\mathsf{Low\_Ab2Repay\_Share}_b)$, blocks are less likely to be eligible for SBA loans, even conditioning on the dollar value of property damage as well as on flood insurance status. These findings hold using the minority share of the block in continuous form (Column 2) and after limiting our sample to FEMA registrants with non-zero assessed property damage (Column 3) as well as to only homeowners (Column 4). For example, the coefficient in Column 4 suggests that homeowners with confirmed damage in blocks with a low ability-to-repay share are 60% (-8.14/13.5) less likely to be approved for an SBA loan.

To evaluate SBA loan sizes, Column 5 of Table 4 uses a dataset of approved SBA loans that are aggregated to the Census block-level to merge in the key explanatory and control variables. Average loan amounts are positively correlated with block-level damage and negatively correlated with flood insurance penetration, as would be expected. Controlling for these factors in Column 5, we find that coming from a block with a low ability-to-repay share is associated with \$13,183 less in average SBA loans per FEMA registrant, equal to 84% of the average approved SBA loan amount per registrant (\$15,652). Put differently, 84% less in SBA loan dollars flowed into neighborhoods with a below-median ability-to-repay share after Harvey. To better manage the possibility that assessed damage amounts might also be lower in these same neighborhoods (thus, contributing to lower aggregate SBA generosity), in Column 6, we scale SBA loan amounts by property damage. Specifically, we estimate the total dollar amount of SBA loans per $1,000 of total property damage in the block, normalized by the number of registrants. We find that, per dollar of damage, 28% (-34.9/125) less in SBA loan dollars were allocated to the average FEMA registrant in blocks with a low ability-to-repay share. It is important to note that, since data limitations prevent us from isolating the intensive from the extensive margin in these regressions, part of these SBA dollar effects may stem from loan denials rather than smaller approved loan amounts.

To the extent that FEMA IHP grants act as a safety net program, we should expect FEMA grants to be more accessible to registrants who are denied SBA loans. Challenging this expectation, however, are the programmatic factors, discussed in Section 2.1, that may create hurdles for disadvantaged groups. We explore the allocation of FEMA IHP assistance in Table 5, using the regression specification in Equation 3.

First, in Columns 1–4, we ask whether FEMA registrants from certain areas are more or less likely to receive any FEMA IHP assistance (extensive margin effect), all else equal. Across all specifications, individuals in blocks with a low ability-to-repay share or a larger share of minorities have a decreased likelihood of receiving FEMA IHP assistance. This finding holds after controlling for individual flood insurance status and property damage amounts as well as for factors that may be correlated with the probability of registering with FEMA. In Column 1, individuals in low ability-to-repay blocks are 16% (-3.039/18.97) less likely to receive FEMA assistance, with a larger effect on renters (Column 4). In low ability-to-repay blocks, homeowners have a 7% (-2.034/28.97) lower probability of receiving a FEMA IHP grant. Overall, it appears that FEMA registrants in areas where the probability of receiving an SBA loan is diminished, face hurdles in receiving FEMA grants that cannot be fully explained by differential tendencies to have insurance, have experienced property damage, or to have registered with FEMA.[26]

In Columns 5-8, we examine the dollar amount given in the form of FEMA IHP grants (intensive margin effect). Consistent with program design, those with flood insurance and/or less property damage receive less in FEMA IHP assistance dollars. Interestingly, however, registrants in low ability-to-repay areas also receive less in FEMA assistance dollars (Column 5). This latter effect comes entirely from homeowners (Column 7). After qualifying for FEMA assistance, coming from a block with a low ability-to-repay share implies $921 less in aid, which is about 8.3% of the average dollar amount of assistance to homeowners ($11,085). Renters receive $186 more IHP assistance when they come from these same areas (Column 8). Part of FEMA IHP aid comes in the form of rental assistance to address immediate housing needs. Still, overall, these results offer little support for the view that FEMA assistance alleviates the funding gap when an SBA loan is not available.[27]

[26]The fact that FEMA takes into account SBA eligibility in its grant decisions, could bias the estimated relationship between ability-to-repay and FEMA approval. Appendix Table A3 presents two robustness checks: first, we set the dependent variable to be an indicator of approval for FEMA and/or SBA assistance and, second, we drop from the sample individuals who receive SBA loans. In both cases, the estimated relationship between low ability-to-repay and approval remains similarly negative and statistically significant.

[27]We sent a copy of this paper to FEMA and spoke to several FEMA officials about these results over the phone on December 4, 2019. Data analysts at FEMA confirmed our findings, noting that they also see evidence of inequalities along these same dimensions in their larger data set. However, the FEMA officials we spoke to were still investigating the drivers and were unable

Table 4: Correlates of SBA loan approval and amounts

	$\mathbb{1}(SBA_i > 0)$ Probability that a FEMA registrant is approved for an SBA loan				$Avg\,SBA\$_b$ Avg. SBA loan amount per FEMA registrant ($)	$Avg\,SBAProp\$_b$ Avg. SBA loan amount per $1,000 of property damage
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(Low\_Ab2Repay\_Share_b)$	-2.472***		-7.093***	-8.138***	-13183.466***	-34.897**
	(-17.18)		(-18.96)	(-18.85)	(-13.57)	(-2.24)
$Minority\ share_b$ *(p.p.)*		-0.026***				
		(-14.41)				
$\mathbb{1}(Insurance_i)$	-1.103***	-1.126***	-3.323***	-4.967***	n/a	n/a
	(-5.72)	(-5.81)	(-9.28)	(-12.02)		
$Damage_i$ *($)*	0.635***	0.642***	0.471***	0.355***	n/a	n/a
	(33.88)	(33.61)	(24.43)	(17.55)		
Block WAvg. flood depth (ft)	0.036	0.054	0.197	0.230	379.475	9.820*
	(0.39)	(0.57)	(0.98)	(1.07)	(0.83)	(1.85)
Block share of registrants w insurance	2.025***	3.002***	3.196***	3.057***	-1701.868	-93.490***
	(4.01)	(6.11)	(3.18)	(3.31)	(-0.86)	(-3.00)
Block share of registrants w damage	5.058***	4.550***	3.139***	-0.260	27638.789***	-434.426***
	(15.04)	(13.29)	(3.58)	(-0.26)	(7.67)	(-2.98)
Block share of housing units registered	0.927***	0.961***	4.331***	0.391	-15349.724***	-392.421**
	(4.11)	(3.96)	(4.63)	(0.18)	(-2.76)	(-2.04)
Block share of housing units w damage	-0.834	-0.933*	-2.083	9.721***	30162.254***	513.307**
	(-1.63)	(-1.72)	(-1.60)	(3.34)	(4.00)	(2.34)
Block population density (per acre)	0.079***	0.075***	0.202***	0.769***	-691.328	-5.265
	(3.52)	(3.32)	(2.76)	(2.90)	(-0.87)	(-0.55)
Block owner-occupied share	0.017***	0.023***	0.063***	0.073***	56.338	0.414
	(10.47)	(12.67)	(9.78)	(6.08)	(1.37)	(0.95)
Sample	*All FEMA*		*Damage > 0*	*Damage > 0* & Owners	*Blocks with at least one SBA approved loan*	
Y-mean	4.12	4.12	10.61	13.52	15651.64	124.96
N	419,360	419,360	135,706	96,572	4,569	4,569

Table presents cross-sectional OLS regressions of SBA loan access on individual- and block-level characteristics using the specifications in Equations 1 and 2. The key explanatory variable, $\mathbb{1}(Low\_Ab2Repay\_Share_b)$, takes the value of one when an above-median share of a block's sampled residents are classified as having a low ability-to-repay index value. The dependent variable in Columns 1–6, $\mathbb{1}(SBA_i > 0)$, comes from FEMA data and is an indicator of whether an individual FEMA registrant was approved for an SBA loan. The unit of observation in Columns 1–4 is the individual such that coefficients can be interpreted as the percentage point effect on the probability of SBA loan approval. In Columns 3-4, we limit the sample to registrants with non-zero inspected damage and to homeowners. In Columns 1–4, we control for whether an individual has flood insurance and the amount of FEMA-assessed property damage (measured in thousands of dollars). The dependent variable in Column 5, $Avg\,SBA\$_b$, is calculated as the total dollar amount of SBA loans distributed to individuals (not businesses) normalized by the number of FEMA registrants in the block. In Column 6, the dependent variable, $Avg\,SBAProp\$_b$, is calculated as the total dollar amount of SBA loans distributed to individuals (not businesses) per $1,000 of property damage in the block, normalized by the number of FEMA registrants in the block. In Columns 5 and 6, the unit of observation is the Census block and the data includes only Census blocks with at least one SBA loan. In Columns 5 and 6, we weight by the number of FEMA registrants in a block. All columns include controls for the block-level factors (B_b) described in Section 3.1. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table 5: Correlates of FEMA IHP grant approval and amounts

	$\mathbb{1}(\mathrm{FEMA}_i > 0)$ *FEMA registrants granted FEMA IHP aid*				$\mathrm{FEMA\$}_i$ *Amount of FEMA IHP assistance granted to an individual ($)*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}(\mathrm{Low\_Ab2Repay\_Share}_b)$	-3.039***		-2.034***	-3.275***	-632.310***		-921.271***	185.942***
	(-10.447)		(-7.776)	(-7.347)	(-5.570)		(-12.090)	(4.061)
$\mathrm{Minority\ share}_b$ *(p.p.)*		-0.061***				-1.171		
		(-13.258)				(-0.498)		
$\mathbb{1}(\mathrm{Insurance}_i)$	-4.899***	-4.905***	-5.523***	3.157***	-10830.251***	-10839.088***	-10424.129***	0.838
	(-23.921)	(-23.951)	(-23.785)	(7.036)	(-54.980)	(-54.922)	(-28.210)	(0.017)
Damage_i *($)*	1.903***	1.906***	1.911***	7.759***	437.355***	440.002***	573.937***	93.385***
	(41.393)	(41.520)	(38.910)	(27.097)	(39.095)	(39.386)	(42.259)	(14.384)
$\mathbb{1}(\mathrm{Damage}_i > 0)$	32.384***	32.369***	30.208***	21.898***	48.459	-47.242	-4072.939***	42.169
	(63.763)	(63.845)	(65.362)	(23.252)	(0.337)	(-0.321)	(-3.931)	(1.020)
Block WAvg. flood depth (ft)	0.269*	0.231	-0.189	0.509***	-97.245*	-83.845	38.444	-0.450
	(1.716)	(1.496)	(-0.924)	(2.601)	(-1.883)	(-1.572)	(0.920)	(-0.021)
Block share of registrants w insurance	-0.516	-0.311	1.936***	7.092***	-1568.313***	-1209.838***	1194.363***	-78.247
	(-0.707)	(-0.437)	(3.166)	(5.288)	(-4.896)	(-3.731)	(4.544)	(-0.379)
Block share of registrants w damage	7.646***	7.025***	6.165***	7.747***	2666.160***	2698.733***	246.209	-59.215
	(7.852)	(6.965)	(9.114)	(7.191)	(5.407)	(5.517)	(1.194)	(-0.656)
Block share of housing units registered	-0.249	1.144**	1.664	-1.102*	1219.633***	1096.765***	330.216	-79.975
	(-0.464)	(2.122)	(1.582)	(-1.839)	(3.331)	(3.238)	(0.736)	(-0.519)
Block share of housing units w damage	18.038***	16.894***	20.153***	17.942***	-1405.576**	-1410.238***	569.406	21.156
	(12.702)	(11.853)	(10.880)	(7.578)	(-2.440)	(-2.636)	(1.055)	(0.117)
Block population density (per acre)	0.086	0.113*	0.330**	0.021	3.868	-0.354	-6.423	-9.944*
	(1.381)	(1.865)	(2.108)	(0.418)	(0.204)	(-0.020)	(-0.138)	(-1.943)
Block owner-occupied share	-0.064***	-0.062***	-0.075***	-0.025***	38.621***	41.545***	11.114***	4.532***
	(-12.419)	(-12.375)	(-11.143)	(-4.664)	(20.464)	(21.825)	(4.964)	(6.434)
Sample	*All*		Owners	Renters	*Assistance>0*			
					All		Owners	Renters
Y-mean	18.97	18.97	28.12	10.43	7445.69	7445.69	11084.54	2377.50
N	419,360	419,360	203,372	214,137	79,555	79,555	38,121	22,337

Table presents cross-sectional OLS regressions using individual FEMA registrant data and the specifications in Equations 3. The key explanatory variable, $\mathbb{1}(\mathrm{Low\_Ab2Repay\_Share}_b)$, takes the value of one when an above-median share of a block's sampled residents are classified as having a low ability-to-repay index value. In Columns 1–4, the dependent variable, $\mathbb{1}(\mathrm{FEMA}_i > 0)$, is defined as an indicator equal to one if an individual registrant was approved for FEMA IHP assistance. In Columns 5-8, the unit of observation is the individual FEMA registrant and the dependent variable, $\mathrm{FEMA\$}_i$, is the amount of *FEMA IHP assistance* granted to an individual (in dollars). To isolate intensive margin effects from extensive margin effects, we limit the sample to households that received some non-zero amount of assistance in Columns 5-8. We also further limit our sample to only repair assistance in Column 7 and only rental assistance in Column 8. In all models, we control for whether an individual has flood insurance and the amount of property damage as well as an indicator for any property damage. All columns include controls for the block-level factors (B_b) described in Section 3.1. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Taken together, the results in this section indicate that disaster assistance is being allocated in a way that may be exacerbating, rather than that counteracting, pre-existing wealth inequalities. Controlling for differences in property damage and insurance, we find that where residents were less likely to be approved for an SBA loan (based on ability-to-repay), homeowners took in 28% less in SBA loan dollars per dollar of damage and had a 7% lower probability of receiving a FEMA grant. Given the presence of both extensively and intensively less aid for homeowners, we should expect worse credit outcomes in areas where homeowners are unlikely to be approved for an SBA loan based on ability-to-repay criteria. We explore this hypothesis next.

4.2 Consumer Credit: Results

This section tests for heterogeneity in credit outcomes after flooding using the consumer credit data, described in Section 2.3, and the difference-in-difference regression specifications, presented in Section 3.2.

4.2.1 Bankruptcy

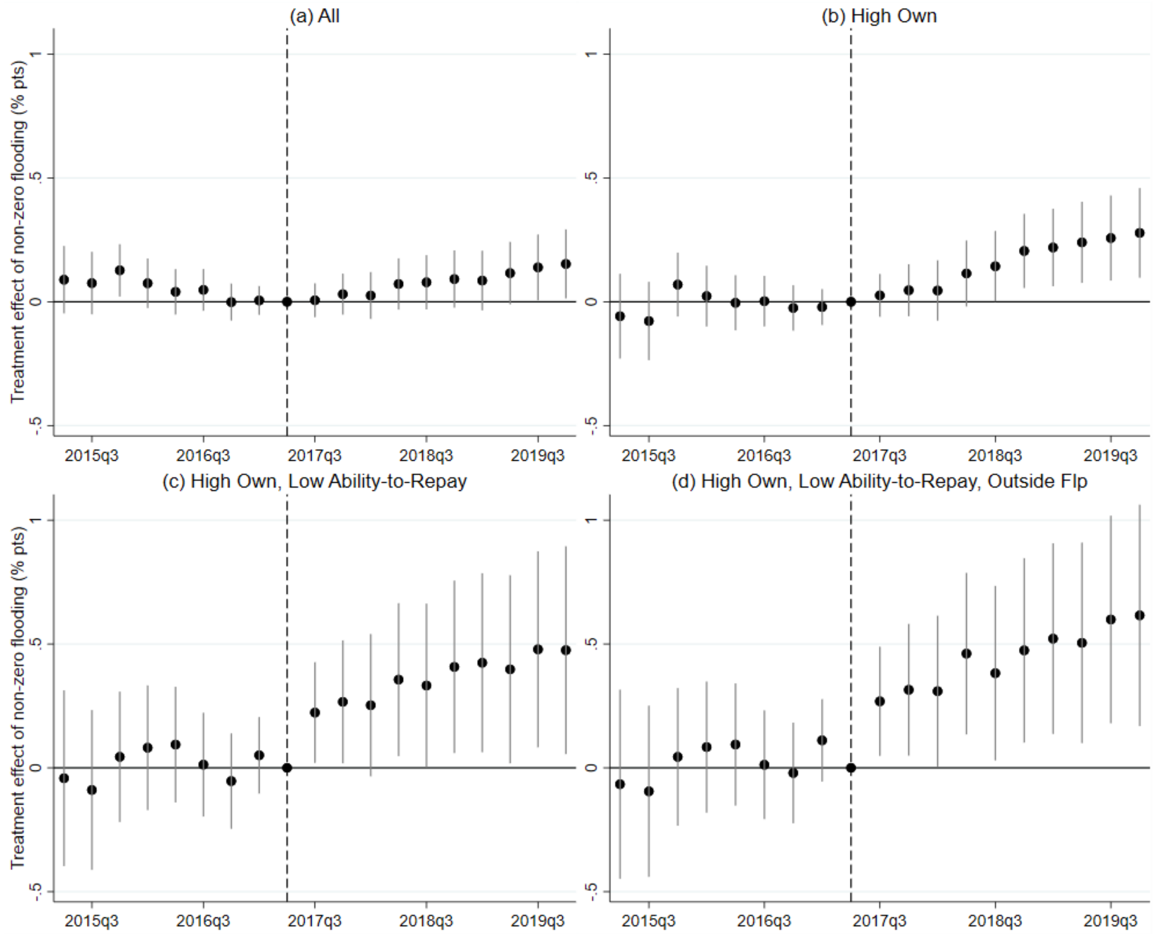
To the extent that flooding pushes borrowers on the cusp of bankruptcy into bankruptcy, we would expect to observe an immediate jump in the share of a block's residents with a bankruptcy flag on their account if flooding increases financial stress. Figure 2 plots the event study coefficients from Equation 5 at the block-level. The coefficients capture the effect of being in a block with any (non-zero) flooding relative to blocks that did not flood and relative to the last quarterly observation for that block before the hurricane (Q2 2017).

Panel (a) shows no clear change in the overall bankruptcy rate associated with flooding in the average Houston block after Harvey. This result is consistent with prior research suggesting a limited effect of natural disasters on financial distress (e.g., Gallagher & Hartley, 2017). This plot, however, masks substantial heterogeneity across block types. There is a clear positive effect of flooding on bankruptcy rates in Panel (b), where the sample is restricted to blocks with an above-median share of owner-occupied housing. In Panel (c), differential effects by ability-to-repay share become apparent. Finally, Panel (d) shows a marked increase in the bankruptcy rate in low ability-to-repay blocks with high owner-occupied housing outside of the floodplain (where homeowners are less likely to be insured against flooding). The magnitude of this effect grows over time and peaks at about 0.60 percentage points at the end of our sample period. This effect size is economically significant – it represents 17% of the average pre-Harvey block bankruptcy rate in these same areas (3.6%). Moreover, since this is an intent-to-treat specification, in which some residents in blocks with flooding did not flood, we are likely underestimating the true effect of flooding on bankruptcy rates.

Table 6 tests the effect of being in the top quartile of flooding as well as the statistical significance of the *low ability-to-repay share* indicator variable, $\mathbb{1}(\mathsf{Low\_Ab2Repay\_Share}_b)$, through an interaction term. Column 1 indicates that bankruptcy rates are unaffected, on average, by being in the most flooded quartile (T_b^{Q4}) relative to the no-flood group after the storm. This is true even within low ability-to-repay blocks

to formally comment on why these inequalities exist.

Figure 2: Effect of flooding on the block *Bankruptcy Rate* (% pts)



Figures plot event study coefficients from DiD regressions using the block-level panel over Q2 2015–Q4 2019. The dependent variable, *Bankruptcy*, is the percentage of a block's residents that have a bankruptcy flag on their credit report during the quarter. Coefficients can be interpreted as the percentage point effect on the block bankruptcy rate of non-zero flooding relative to no flooding and relative to Q2 2017. The sample is split according to whether the block has an above-median share of residents classified as "low ability-to-repay" as of the last quarterly observation before the hurricane (Q2 2017). The sample is further split at the median according to the owner-occupied share and according to floodplain status (*Outside Flp*). All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

(Column 2). However, in predominately owner-occupied areas (Column 3), the treatment effect of being in the most flooded quartile of blocks is significantly positive in low ability-to-repay blocks. Specifically, in high owner-occupied areas, heavy flooding combined with low ability-to-repay is associated with a 0.81 percentage point relative increase in the block bankruptcy rate.

Next, we explore the role of floodplain status. Column 4 shows that in high owner-occupied areas outside of the floodplain with heavy flooding, the bankruptcy rate is 1.40 percentage points higher when the blocks' residents have a low ability-to-repay and, hence, are unlikely to qualify for an SBA loan. This treatment effect represents a 39% increase in the bankruptcy rate relative to the average pre-hurricane bankruptcy rate within the subsample of low ability-to-repay, high owner-occupied blocks outside of the floodplain (3.6%). As expected, the treatment effect of top-quartile flooding (T_b^{Q4}) is much greater than the average effect in the lower quartiles of flooding (T_b^{Q1-Q3}). A similar pattern holds when using the indicator of above-median block *minority share*, $\mathbb{1}(\mathsf{High\_Mrty\_Share_b})$, in the triple interaction in Column 6.

Finally, Columns 5 and 7 document that blocks inside the floodplain do not see a significant change in bankruptcy rates after flooding, even when the block is classified as having a low ability-to-repay share or high minority share. This result is consistent with flood insurance mitigating the impact of heavy flooding, combined with a weak initial financial condition, on bankruptcy.

Table 6: Effect flooding on the block *Bankruptcy Rate* (% pts)

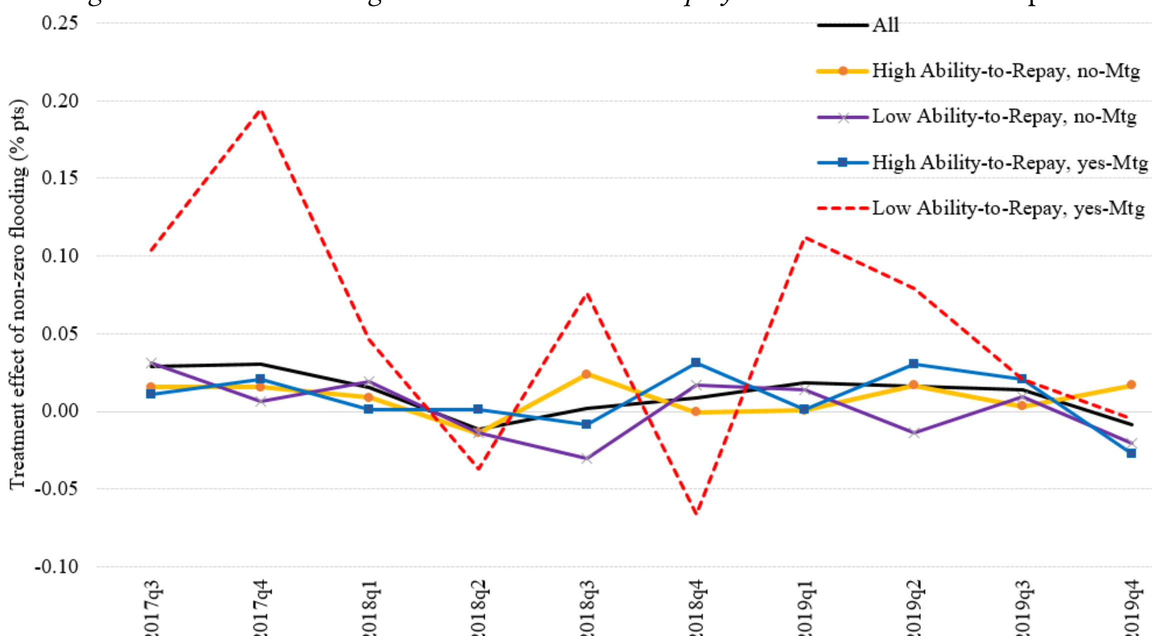
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$T_b^{Q1-Q3} \times P_t$	0.049	0.116	0.158	0.003	0.332	0.127	0.124
	(0.71)	(1.41)	(1.64)	(0.02)	(0.81)	(0.92)	(0.30)
$\mathbf{T_b^{Q4}} \times P_t$	-0.037	-0.011	-0.083	-0.491	0.405	-0.621*	0.127
	(-0.35)	(-0.10)	(-0.65)	(-1.53)	(0.89)	(-1.70)	(0.24)
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(\mathsf{Low\_Ab2Repay\_Share_b})$		-0.167	0.068	0.596**	-0.684		
		(-1.21)	(0.32)	(2.29)	(-0.88)		
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(\mathsf{Low\_Ab2Repay\_Share_b})$		-0.066	0.805**	1.395**	0.094		
		(-0.29)	(2.22)	(2.17)	(0.10)		
$P_t \times \mathbb{1}(\mathsf{Low\_Ab2Repay\_Share_b})$		-0.066	-0.297**	-0.406**	0.232		
		(-0.66)	(-2.06)	(-2.56)	(0.32)		
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(\mathsf{High\_Mrty\_Share_b})$						0.123	-0.166
						(0.54)	(-0.22)
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(\mathsf{High\_Mrty\_Share_b})$						1.483***	0.860
						(2.73)	(1.00)
$P_t \times \mathbb{1}(\mathsf{High\_Mrty\_Share_b})$						0.019	0.163
						(0.14)	(0.26)
N	574,766	574,766	322,127	239,782	28,115	239,782	28,115
AdjR2	0.72	0.72	0.73	0.73	0.70	0.73	0.70
Y-mean	2.21	2.21	2.26	2.37	1.78	2.37	1.78
Sample	*All*	*All*	*High Own*				
				out-Flp	*in-Flp*	*out-Flp*	*in-Flp*

Table presents DiD estimates using the block-level panel over Q2 2015–Q4 2019. The dependent variable, *Bankruptcy*, is the share of a block's residents that have a bankruptcy flag on their credit report during the quarter. Treatment intensity is defined according to quartile bins of *WAvg. Flood Depth* in the post period. The coefficient on $T_b^{Q4} \times P_t$, for example, can be interpreted as the percentage point effect of top quartile flood intensity on a block's post-hurricane bankruptcy rate relative to its pre-hurricane rate and relative to the post-hurricane bankruptcy rate of blocks that did not flood. In some specifications, treatment is interacted with dummies indicating that the block has an above-median share that entered the hurricane with a low ability-to-repay index value, $\mathbb{1}(\mathsf{Low\_Ab2Repay\_Share_b})$, or has an above-median minority share, $\mathbb{1}(\mathsf{High\_Mrty\_Share_b})$. All associated secondary interactions (that are not perfectly collinear with the fixed effects) are included. Columns 3–7 restrict the sample to blocks with above-median owner-occupied share of housing (*High Own)* as well as blocks that are completely outside of the floodplain (*out-Flp*) or where the floodplain covers more than 50% of the developed land (*in-Flp*). All regressions include the full array of fixed effects and controls described in Section 3.2. Regressions are weighted by the number of observations within each Census block in the CCP data. Standard errors are clustered on Census block. Parentheses contain t-statistics: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure 3 validates these findings at the individual-level. Here, we plot the coefficients from a discrete-time hazard model. As discussed in Section 2.3, we use a hazard model to account for the absorbing nature of bankruptcy (flags are active for 7 years after entering bankruptcy). A discrete-time hazard model removes individuals from the sample after a bankruptcy flag appears in the post-period. Note that one cannot observe pre-trends in a hazard model because the sample is restricted to individuals without a bankruptcy flag during the pre-period. Since we observe little bankruptcy inside the floodplain, even among low ability-to-repay individuals, we restrict the sample to individuals living outside the floodplain at the time of Harvey.

The hazard estimates follow the same basic patterns observed at the block-level. For example, when a low ability-to-repay mortgage-holder from outside the floodplain is in a flooded block, his relative probability of entering bankruptcy increases immediately after the storm. Specifically, during Q4 2017 (i.e., the first full quarter after Harvey), such individuals enter bankruptcy at a rate that is 0.19 percentage points higher than their counterparts in blocks that did not flood.

Figure 3: Effect of flooding on the individual *Bankruptcy* hazard outside the floodplain



Figures plot discrete-time hazard model estimates for each of the 10 post-Hurricane quarters using the individual-level panel. The dependent variable, *Bankruptcy*, is a dummy that takes the value of 100 when a bankruptcy flag appears on the individual's credit report and 0 otherwise. Coefficients can be interpreted as the percentage point effect on the probability of entering bankruptcy in a given quarter associated with being in a block with non-zero flooding relative to being in a block that did not flood. The sample is restricted to only blocks outside of the floodplain (i.e., where there is a non-trivial bankruptcy hazard). The sample is split according to whether the individual has an above- or below- median *ability-to-repay index* value as of the last quarterly observation before the hurricane (Q2 2017). The sample is further split based on whether the individual has an outstanding mortgage ("Mtg") balance as of Q2 2017. Regressions include time fixed effects, as well as the individual and block-level controls described in Section 3.2. For visual ease, confidence intervals are not shown. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

The estimate for low ability-to-repay mortgage-holders is statistically significant at the 5% level, as documented in Table 7, which presents the hazard estimates and their confidence intervals for each subsample as of Q4 2017. The estimate is also economically important. A 0.19 percentage point increase in the bankruptcy probability of this subsample, when scaled to match their share of Houston residents, would imply that flooding caused 157 additional bankruptcies among low ability-to-repay mortgage-holders outside the floodplain in Q4 2017 alone.[28] Although this number might sound small, personal bankruptcy is a rare event, such that 157 additional bankruptcies is roughly 6% of Houston's pro-rata share of all Texas personal bankruptcies in 2016 (the year before Harvey). More remarkably, these 157 bankruptcies can account for 28% of the additional bankruptcies that occurred in Texas during the year following Harvey.[29] Simply put, while Harvey's flooding caused a substantial increase in personal bankruptcy, the pain was not born equally – it was highly concentrated in a specific subpopulation.

It is reasonable to ask how such large bankruptcy effects for one subsample could wash out in aggregate? Indeed, as documented in Figure 3 and Table 7, when we consider all sample individuals living outside the floodplain, there is no significant effect of flooding on the overall bankruptcy hazard. We address this puzzle in the last column of Table 7, which shows that there are comparatively few low ability-to-repay mortgage-holders; they represent just 9.9% of the sample living outside the floodplain. Yet, the flooding experienced by this small subpopulation accounts for a disproportionate share (over a quarter) of the estimated number of additional bankruptcies in the immediate aftermath of Harvey. If one takes the sum-product of the hazard estimates and sample proportions in Table 7, the result will be an overall hazard estimate of 0.031, equal to that of the full sample outside the floodplain (last row). These results offer a first indication of the importance of considering underlying heterogeneity in treatment effects following natural disasters.

Table 7: The effect of flooding on the Q4 2017 individual *Bankruptcy* hazard outside the floodplain

Sample	Beta	LB 95% CI	UB 95% CI	% of N
Low Ability-to-Repay, yes-Mtg.	0.194	0.032	0.356	9.9%
High Ability-to-Repay, yes-Mtg.	0.021	-0.002	0.043	21.4%
High Ability-to-Repay, no-Mtg.	0.016	-0.007	0.038	28.4%
Low Ability-to-Repay, no-Mtg.	0.006	-0.039	0.051	40.3%
All (outside floodplain)	0.031	0.005	0.056	100.0%

The table presents discrete-time hazard model estimates for Q4 2017. The dependent variable, *Bankruptcy*, is a dummy such that a value of 100 indicates a bankruptcy flag appears on the individual's credit report during the quarter and 0 otherwise. Coefficients can be interpreted as the percentage point effect on the probability of entering bankruptcy in a given quarter associated with being in a block with non-zero flooding relative to being in a block that did not flood. The sample is restricted to only blocks outside of the floodplain (i.e., where there is a non-trivial bankruptcy hazard). The sample is split according to whether the individual has an above- or below- median *ability-to-repay index* value as of the last quarterly observation before the hurricane (Q2 2017). The sample is further split based on whether the individual has an outstanding mortgage ("Mtg") balance as of Q2 2017. Regressions include time fixed effects, as well as the individual and block-level controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

[28] If, instead of focusing only on Q4 2017, we take the integral of the bankruptcy hazard rate over the 8-quarters post-Harvey, we estimate 325 additional bankruptcies due to flooding within this small subset of the Houston population. For perspective, 325 bankruptcies equate to 12% of Houston's pro-rata share of all Texas personal bankruptcies in 2016 and 58% of Houston's share of all additional Texas bankruptcies that occurred during the year following Harvey.

[29] The numbers are generated from back-of-the-envelop calculations that assume that, since we have a 5% random sample, every 1 person in our data is representative of 20 people with credit scores in Houston. Bankruptcy information comes from American Bankruptcy Institute data on the number of non-business filings in Texas. We apportion aggregate Texas numbers to Houston residents according to their pro-rata population share.

4.2.2 Severe delinquencies

An advantage of examining delinquency is that it is much more common than bankruptcy, even among high ability-to-repay individuals. The disadvantage of examining delinquency is that some types of creditors offered temporary forbearance to all borrowers in Houston, even those areas that did not flood. Forbearance necessarily means that the effect of flooding on delinquency will be muted, at least temporarily. Our estimates are, therefore, likely to be conservative. Some forms of debt have a greater likelihood of forbearance (see Section 2.1). Therefore, to simplify matters, we measure delinquency in aggregate using the *Severely Delinquent % of Total Debt*, measured as the share of an individual's total debt that is at least 90-days delinquent. Only individuals with continuously non-zero total debt (which is dominated by mortgage-holders) are included in this analysis.

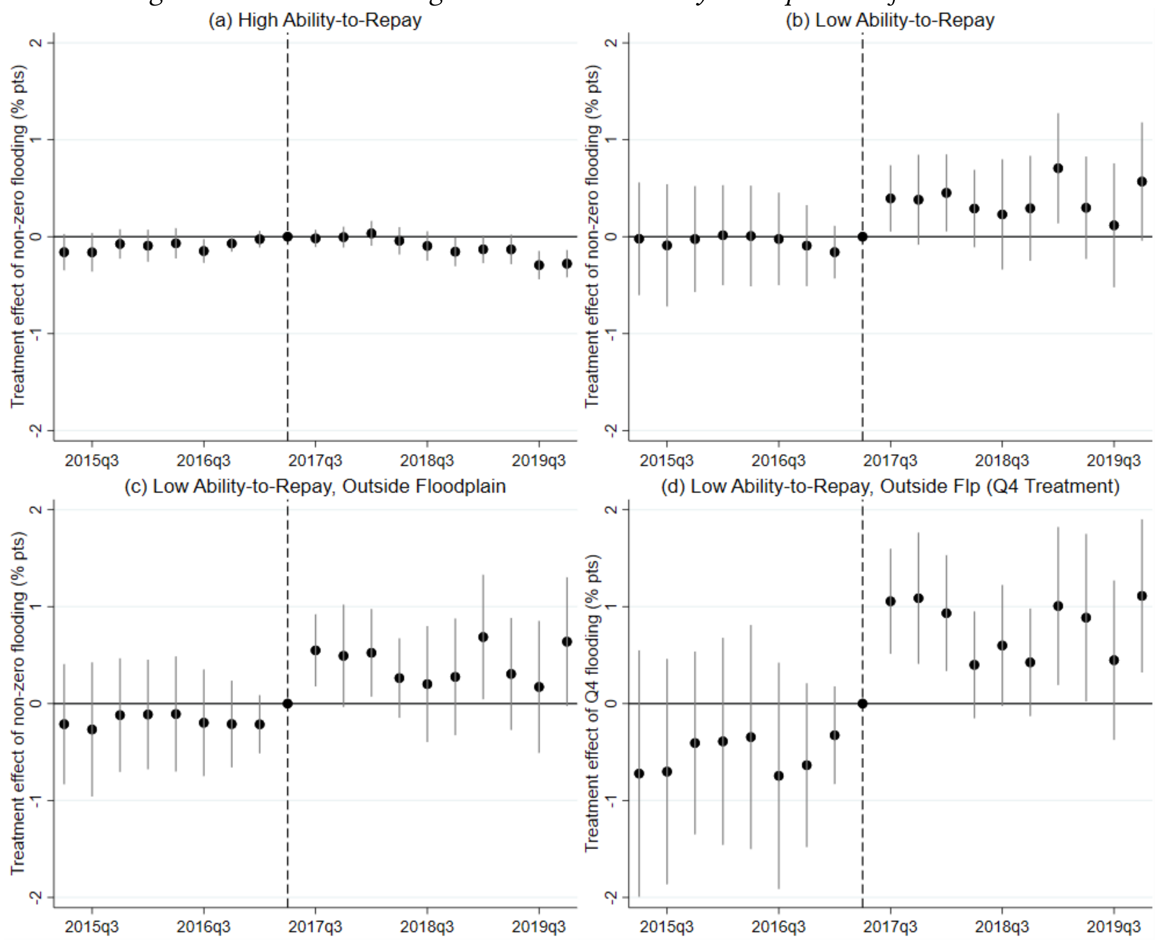
Figure 4 presents the event study coefficients. In Panel (a), flooding is associated with a slight downward trend in the delinquency share of high ability-to-repay borrowers. In contrast, in Panel (b), individuals classified as low ability-to-repay mirror the trend in bankruptcy in that, there is an increase in delinquency for these individuals immediately after the storm, particularly for those located outside of the floodplain (Panel c). As expected, the treatment effect becomes larger in magnitude in Panel (d), where treatment is defined as coming from a block with top-quartile (Q4) flooding.

We formalize these results in Table 8 by running DiD regressions of the type in Equation 4. Column 1 shows a significant, mild effect (0.55 percentage point) of top-quartile flooding on the delinquent debt share of the average borrower immediately after Harvey – i.e., when we end the post-period in Q1 2018, three quarters after Harvey. The treatment effect dissipates in Column 2 when we use the full post-period (ending in Q4 2019), indicating that the treatment effect on the average borrower is temporary. Columns 3 and 4, however, indicate that the treatment effect does not dissipate for low ability-to-repay borrowers. The coefficient associated with top-quartile flooding (T_b^{Q4}) interacted with $\mathbb{1}(Low\_Ab2Repay_i)$ suggests that these factors increase delinquency by nearly 1 percentage point relative to counterparts in blocks that did not flood.[30] This effect size is moderate, representing 5% of the pre-treatment delinquency share of low ability-to-repay borrowers (20%). Recall, however, that since some residents in blocks with top-quartile flood depth likely escaped flooding (i.e., we use an intent-to-treat estimator), we are likely underestimating the true effect of flooding on individual delinquency.

Columns 5–10 investigate heterogeneity in treatment effects according to floodplain status. Column 5 shows a positive overall treatment effect of top-quartile flooding outside the floodplain that is not present inside the floodplain (Column 6). Column 7 proves that this effect is driven by initial financial condition. Outside the floodplain, top-quartile flooding combined with low ability-to-repay is associated with a 1.95

[30] Appendix Table A4 explores how each of the four individual components of the ability-to-repay index interacts with treatment to influence delinquency rates after flooding. Outside the floodplain, the Equifax Riskscore of the individual as well as the median income and minority share of the individual's block are each marginally statistically significant when interacted with treatment. Only credit card utilization fails to be statistically significant. Importantly, t-statistics are smaller on the individual components than on the index – implying that the index better captures overall financial condition (and, hence, probability of being granted an SBA loan). Of the four individual components, credit score has the most consistent explanatory power, likely because it is, itself, a form of latent predictor of repayment probability. However, factors like income and race are not included in credit bureaus' scoring models, which may explain why our ability-to-repay index better separates the treatment effect of flooding on both SBA loan approval rates (Appendix Table A1) as well as on delinquency outcomes after flooding (Appendix Table A4).

Figure 4: Effect of flooding on individual *Severely Delinquent % of Total Debt*



Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q4 2019. The dependent variable is the percentage of total debt that is at least 90-days past due (*Severely Delinquent % of Total Debt*). An individual's treatment intensity (*WAvg. Flood Depth*) is assigned according to the Census block where the individual lived as of the last quarter before the hurricane (Q2 2017). In Panels (a)–(c), coefficients can be interpreted as the effect of being in a block with non-zero flooding relative to no-flood blocks and relative to Q2 2017. In Panel (d), coefficients can be interpreted as the effect of being in a block with top-quartile (Q4) flooding relative to no-flood blocks and relative to Q2 2017. For visual ease, we suppress the Q1-Q3 estimates, showing only the estimates for the top-quartile group. In Panels (a) and (b), the sample is split according to whether the individual has an above- or below- median *ability-to-repay index* value as of Q2 2017. In Panel (c), the sample is further restricted to blocks outside the floodplain. All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

percentage point relative increase in the delinquent share of debt, which represents nearly 10% of the pre-Harvey delinquency share of this subsample. Columns 9 and 10 document that the same patterns hold (despite being statistically weaker) using the more transparent measure of block minority share.

In summary, in flooded areas outside of the floodplain, residents with a low ability-to-repay (and, hence, a low probability of being approved for an SBA loan) see a 10% increase in their share of debt in severe delinquency and account for a disproportionate number of Houston-area bankruptcies after Harvey. Meanwhile, consumers with a high ability-to-repay appear to weather flooding with no negative credit consequences. Inside the floodplain, we observe no significant effect of flooding on credit outcomes – implying that flood insurance, unlike disaster assistance, mitigates the negative credit impact of flooding

Table 8: Effect of flooding on individual *Severely Delinquent % of Total Debt*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$T_b^{Q1-Q3} \times P_t$	0.206**	0.124	0.079	-0.038	0.186	0.002	-0.089	-0.481***	0.004	0.907
	(2.01)	(0.91)	(0.98)	(-0.46)	(0.91)	(0.00)	(-0.78)	(-3.67)	(0.02)	(1.20)
$\mathbf{T_b^{Q4}} \times P_t$	0.546***	0.320	0.243	-0.035	0.868**	-0.317	0.151	-0.163	0.229	0.738
	(3.04)	(1.68)	(1.47)	(-0.20)	(2.21)	(-0.49)	(0.74)	(-0.58)	(0.59)	(0.80)
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(Low\_Ab2Repay_i)$			0.328*	0.433*			0.651	0.827		
			(1.80)	(1.83)			(1.62)	(1.12)		
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(Low\_Ab2Repay_i)$			0.822*	0.995**			1.947**	-0.122		
			(1.79)	(2.24)			(2.09)	(-0.10)		
$P_t \times \mathbb{1}(Low\_Ab2Repay_i)$			2.263***	3.314***			3.321***	3.138***		
			(11.28)	(18.85)			(16.09)	(4.80)		
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(High\_Mrty\_Share_b)$									0.270	-1.328
									(0.87)	(-1.59)
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(High\_Mrty\_Share_b)$									1.113*	-1.570
									(1.84)	(-1.55)
$P_t \times \mathbb{1}(High\_Mrty\_Share_b)$									0.112	1.226*
									(0.59)	(1.69)
N	1,156,758	1,826,715	1,156,758	1,826,715	1,216,537	154,089	1,216,537	154,089	1,216,537	154,089
AdjR2	0.71	0.66	0.71	0.66	0.66	0.66	0.66	0.66	0.66	0.66
Y-mean	9.31	9.91	9.31	9.91	10.24	10.47	10.24	10.47	10.24	10.47
Sample	*$\leqslant$ Q1 2018*	*All*	*$\leqslant$ Q1 2018*	*All*	*out-Flp*	*in-Flp*	*out-Flp*	*in-Flp*	*out-Flp*	*in-Flp*

Table presents DiD estimates using the individual-level panel over Q2 2015–Q4 2019. In Columns 1 and 3, the post-period is shortened to just the first 3-quarters after the hurricane, such that the data span Q2 2015–Q1 2018. The dependent variable is the percentage of total debt that is at least 90-days past due (*Severely Delinquent % of Total Debt*). To be included in the sample, the individual must have non-zero outstanding debt (denominator). Treatment intensity is defined according to quartile bins of *WAvg. Flood Depth* in the post period. Treatment is interacted with a dummy indicating that the individual had a below-median index level of ability-to-repay, $\mathbb{1}(Low\_Ab2Repay_i)$, as of Q2 2017 or lives in a block with an above-median minority share, $\mathbb{1}(High\_Mrty\_Share_b)$. All associated secondary interactions (that are not perfectly collinear with the fixed effects) are included. Columns 5–10 restrict the sample to individuals from blocks completely outside of the floodplain (*out-Flp*) or majority (at least 50%) inside the floodplain (*in-Flp*). All regressions include the full array of fixed effects and controls described in Section 3.2. Standard errors are clustered on Census block. Parentheses contain t-statistics: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

across the wealth distribution.

4.3 Exploring the mechanism

Wealth is itself a form of protection against the negative effect of wealth shocks on credit outcomes. Therefore, even in the absence of disaster assistance, we should still expect low ability-to-repay disaster victims to experience slower recoveries (Lusardi et al., 2011). However, in the presence of robust disaster assistance programs allocating hundreds of billions of dollars to victims, the predicted role of initial wealth becomes less clear. Presumably, it would depend on how much assistance is given and how that assistance is allocated. Thus far, we have shown that disaster aid (particularly in the form of SBA loans) can fully absorb damage, but is regressive. In this section, we develop three tests to help us characterize the relative importance of disaster aid in contributing to heterogeneity in credit outcomes after flooding.

Test 1: Improvements in financial condition

Our first test relies on the fact that, in the absence of generous disaster aid, it is difficult to rationalize why individuals in flooded areas would see improvements in their financial condition relative to similar individuals in not-flooded areas. Recall from Figure 4, Panel (a), that any flooding has a slightly negative effect on the delinquency share of the high (above-median) ability-to-repay sample.

One possible explanation for this finding is that SBA loans act as a liquidity infusion for some, otherwise, financially well-off consumers. Kaplan et al. (2014) estimate that around 22% of U.S. households can be classified as "wealthy-hand-to-mouth" – meaning high-income but liquidity constrained because their wealth is held in illiquid assets (e.g., real estate and retirement accounts). An individual with a high credit score and income may have low levels of liquid assets if she, for example, saves little of her discretionary income. An unexpected expense or income shock could, on rare occasions, push this type of consumer into delinquency. Indeed, according to the Survey of Consumer Finances, 7% of households in the top quartile of the income distribution fell behind on a debt payment in 2016. Nonetheless, the SBA may regard this type of applicant as having a high ability-to-repay since loan payments, when spread over 30-years, form a small share of her cash inflow. If SBA loans act as a liquidity infusion for this type of high-income borrower who lives close to her budget constraint, then we would expect to see a decline in the occasional delinquency of such consumers in flooded areas relative to not-flooded areas.

To identify consumers who may fall into the "wealthy-hand-to-mouth" category, we focus on borrowers in the top quartile ("Q4") of our ability-to-repay index. These are the borrowers who are most likely to receive large SBA loans according to the estimates in Table 4. We further sort according to credit card utilization. Although high credit card utilization can imply a high debt-to-income ratio, it can also be indicative of high levels of consumption, (i.e., large non-revolving balances) and, hence, low levels of liquidity (Gross & Souleles, 2002). Therefore, we classify as "wealthy-hand-to-mouth" those sample individuals who are close to their credit limit while simultaneously (by virtue of being in the top quartile of our ability-to-repay index) having high credit scores and living in high-income neighborhoods with few minorities.

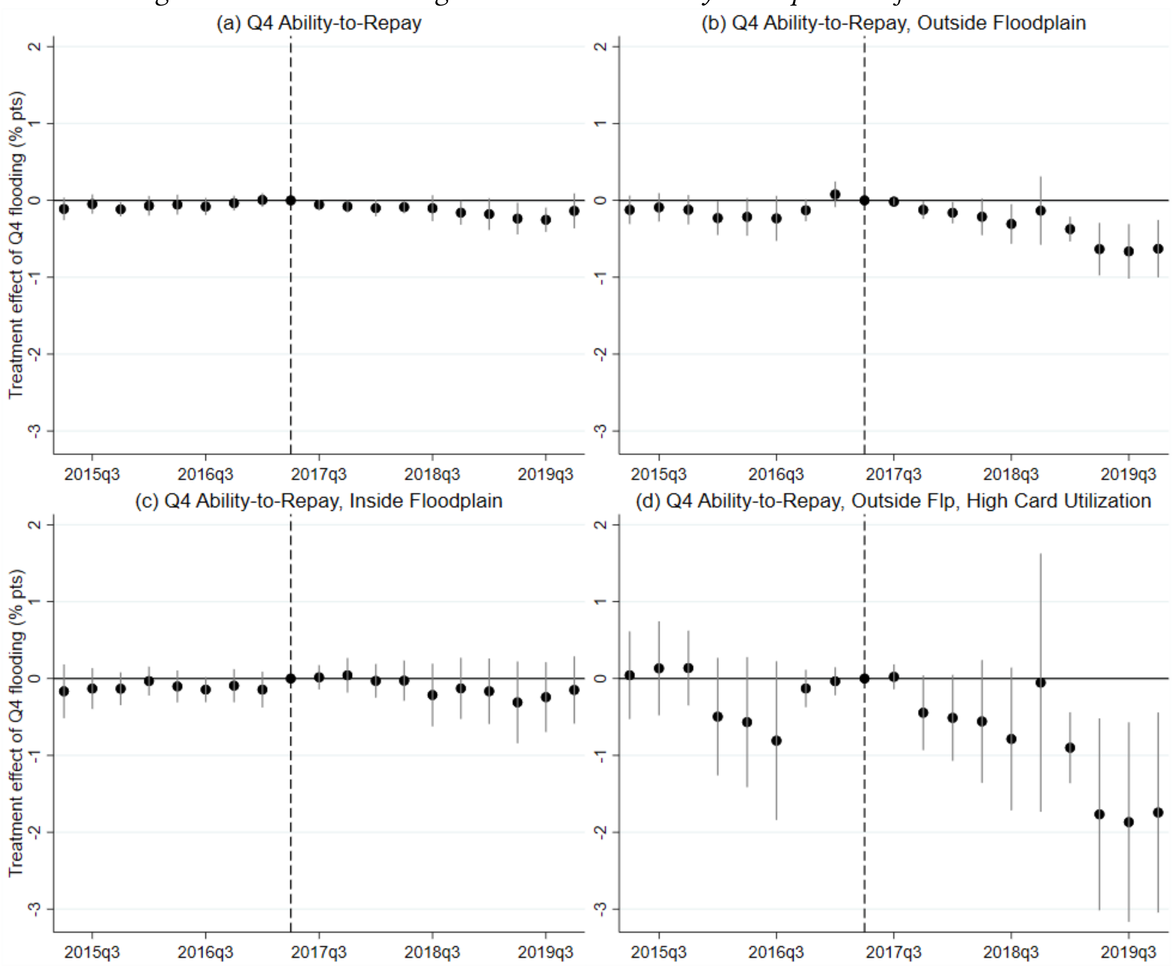
Figure 5 presents the event study estimates from difference-in-difference regressions on these subsets of borrowers. Panel (a) shows a slight downward trend in the delinquent debt share of top quartile ability-to-repay borrowers after heavy flooding. This effect comes from the sample living outside of the floodplain (Panel b), where SBA loans were most likely to substitute for flood insurance. Inside the floodplain (Panel c), the effect is statistically insignificant. Because delinquency is rare for this subsample, the magnitude of the treatment effect in Panel (b) is large: a roughly 0.50 percentage point decline in the delinquency share represents about a third of the pre-Harvey delinquency share for this borrower type. Note that SBA loans do not appear in our credit bureau dataset; therefore, their provision cannot inflate the denominator and mechanically produce this result. Instead, it appears borrowers living outside the floodplain who flood and, thus, gain access to SBA loans fall less delinquent compared to counterparts who do not flood. In Panel (d), we further restrict the sample to those with high (above-median) credit card utilization – i.e., those well-off individuals who are likely consuming a large share of their income – and find that the treatment effect on delinquency grows in magnitude. Although merely suggestive, this result is consistent with SBA loans acting as a large liquidity infusion for "wealthy-hand-to-mouth" households.

Hypothetically, SBA loans can reach $840,000 for a single disaster and household (Collier & Ellis, 2019). SBA loans, which cover uninsured damages of up to $40,000 for personal property and $200,000 for the real estate, would, for example, fully absorb FEMA's estimated repair cost of $103,355 from four feet of flooding to the average home. Substantially damaged homes are eligible for *additional* funds to, among other things, refinance a mortgage. Refinancing can represent a substantial liquidity infusion. For example, refinancing a $200,000 mortgage at 4% interest with 10-years remaining into a 30-year SBA loan at 1.75% interest would reduce mortgage payments by $1,310 per month or $15,725 per year.

The speed of disbursement may also play a role in the provision of liquidity. Appendix Figure A9 shows the timing of SBA loans, the bulk of which were approved in October and November of 2017 (within 3 months of Harvey). The first SBA loan installment (of up to $25,000) occurs within just 5-days of loan closing. Borrowers pay equal monthly installments of principal and interest starting 5 months from the date of the loan; however, payments can be deferred for up to 1 year. By comparison, flood insurance offers a much smaller liquidity infusion. Initial payouts from flood insurance are around $7,000 and the rest of the insurance payout is typically held in escrow and dispersed only as repairs occur. Put together, the comparative scale and speed of the SBA loans, in addition to the refinancing option, may explain why we do not observe a significant dip in delinquency after flooding inside the floodplain as well as why delinquency becomes even rarer for high ability-to-repay borrowers in flooded areas relative to not flooded areas.

More broadly, Figure 5 highlights that treatment effects can not only be diluted in aggregate, as in the case of bankruptcy (Section 4.2.1), but also be offset. In the case of delinquency, the treatment effect of flooding in high and low ability-to-repay subsamples has opposite signs, netting to the mild average effects seen in Table 8 (Columns 1 and 2).

Figure 5: Effect of flooding on individual *Severely Delinquent % of Total Debt*



Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q4 2019. The dependent variable is the percentage of total debt that is at least 90-days past due (*Severely Delinquent % of Total Debt*). An individual's treatment intensity (*WAvg. Flood Depth*) is assigned according to the Census block where the individual lived as of the last quarter before the hurricane (Q2 2017). Coefficients can be interpreted as the effect of being in a block with top-quartile (Q4) flooding relative to no-flood blocks and relative to Q2 2017. For visual ease, we suppress the Q1-Q3 estimates, showing only the estimates for the top-quartile group. In all panels, the sample is restricted to borrowers in the top quartile (Q4) of our *ability-to-repay index*. In Panels (b) and (c), the sample is further restricted to blocks outside and inside the floodplain, respectively. Panel (d) further restricts the sample to individuals with above-median credit card utilization – which can, for some individuals, indicate high levels of consumption. All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Test 2: Substitution into other loans to finance repairs

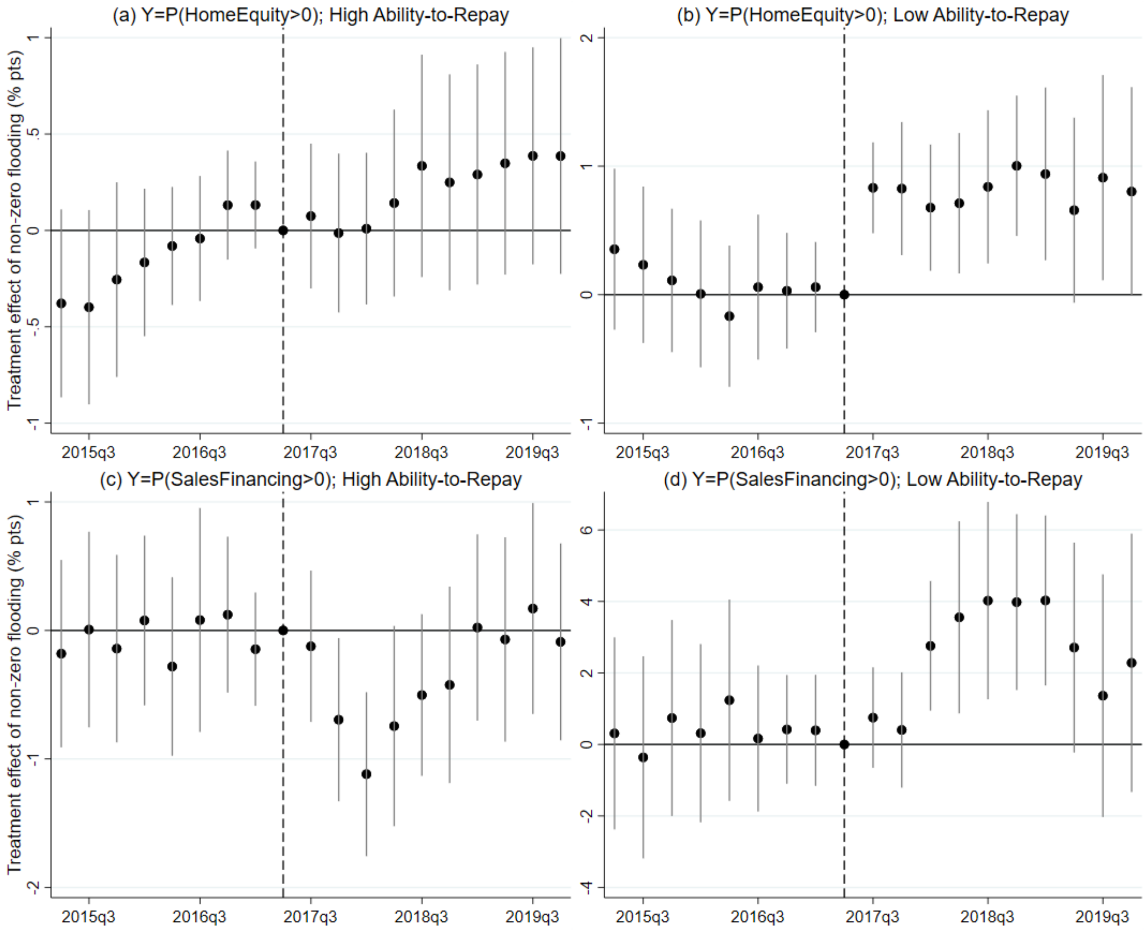
Next, we look at what happens to the take-up of the two types of debt that are most likely to serve as substitutes for SBA loans. These are home equity loans (including home equity lines of credit and home improvement loans), which can be used to finance reconstruction, and sales financing, which is commonly used to pay for expensive household items like furniture and appliances. Presumably, if SBA loans are fully funding the reconstruction needs of the high ability-to-repay sample, then this sample would have no extra need for substitute loan types in flooded areas relative to not-flooded areas.

We explore this hypothesis in Figure 6 for a sample of mortgage-holders living outside of the floodplain.

As expected, home equity loans (Panel b) and sales financing (Panel d) are used by low ability-to-repay borrowers with significantly greater frequency after their block floods. In stark contrast, there is no significant increase in the use of home equity loans (Panel a) among high ability-to-repay borrowers when their block floods and there is a temporary dip in the use of sales financing (Panel c). This latter result would be consistent with the hypothesis that disaster assistance acts as a liquidity infusion for some wealthy but illiquid (high consumption) households, resulting in a temporary substitution away from sales financing after flooding. We do not observe any significant effects inside the floodplain (not shown).

Another way to interpret Figure 6 is as confirmation that alternative forms of financing are available to borrowers who have a low probability of being approved for an SBA loan. It is important to emphasize, however, that there are disadvantages – in terms of scale, flexibility, and price – to using home equity loans and other risk-sensitive forms of debt to finance recovery. First, the funds advanced under a home equity loan (plus the outstanding principal balance on the mortgage) may not exceed 80% of the home's fair market value. Since flooded homes in Houston were trading at discounts of at least 12% in the months following Harvey (Billings et al., 2020, Appendix), this rule limits the scale of home equity loans for heavily mortgaged homeowners. Moreover, due to risk-based pricing, interest rates on home equity loans are typically around 2-3 percentage points higher than the private sector's 30-year mortgage rate. In contrast, for 86% of approved borrowers, the SBA offers a subsidized interest rate, which is set to half that of the 30-year mortgage rate. These stark differences in financing terms may help explain earlier evidence (Figure 4) that, within flooded areas outside the floodplain, those who are unlikely to be approved for an SBA loan experience a significant relative increase in delinquency.

Figure 6: Effect of flooding on the probability of using *Sales Financing* and *Home Equity Loans*



Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q4 2019. The dependent variable is the extensive margin probability of having a *Home Equity Loan* (Panels a and b) or *Sales Financing* (Panels c and d). To hone in on the sample that is most likely to need these forms of debt after Harvey, the sample is restricted to mortgage-holders living outside of the floodplain. An individual's treatment intensity (*WAvg. Flood Depth*) is assigned according to the Census block where the individual lived as of the last quarter before the hurricane (Q2 2017). Individuals with a below-median *ability-to-repay index* value are classified as "low ability-to-repay." All coefficients can be interpreted as the effect of being in a block with non-zero flooding relative to no-flood blocks and relative to Q2 2017. All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Test 3: Isolating the independent explanatory power of disaster aid

Our final test relies on the fact that approval for disaster assistance (both FEMA grants and SBA loans) has a component that is quasi-random. Notably, according to Collier & Ellis (2019), 35% of applicants to the SBA disaster loan program are automatically declined due to low credit scores (below 550) or high debt-to-income (above 75%), with additional screens thereafter. It follows that there exists a degree of idiosyncratic variation in who is approved for assistance – since otherwise similar applicants will be approved or denied based on their position relative to an arbitrary cut-off. We can exploit this fact to test whether disaster assistance, on its own, can explain any of the jump in delinquency observed in flooded areas after Harvey.

We face several challenges in isolating the quasi-random component of disaster aid. First, we cannot merge our anonymous credit data at the individual level with FEMA and SBA data. We must, instead, use block-level measures of FEMA and SBA approval rates (approvals normalized by the number of applicants). Second, our credit data differs in small ways from that used by the SBA to screen applicants, prohibiting a regression discontinuity design. Namely, we observe an individual's Equifax Riskscore, rather than her FICO score, and we observe an individual's debt but not her income.

Our approach, therefore, involves using continuous proxies for SBA and FEMA screening factors (our *ability-to-repay index* and its inputs) as control variables and testing for any residual explanatory power in disaster aid flowing into an individual's neighborhood. Additionally, we can control for a variety of other factors, including block median home values and average damage amounts. In the end, we should have absorbed the trend line (but not the arbitrary discontinuities) in SBA and FEMA approval screens. By including these variables as controls, any explanatory power remaining in the block's SBA and FEMA assistance rates should primarily capture the idiosyncratic portion of disaster aid – the portion that is not directly attributable to an individual's pre-Harvey financial condition. With this approach, we can provide a suggestive estimate of how much of our treatment effect is attributable to variation in disaster assistance as opposed to initial financial condition.

Table 9 presents the summarized results and specifies the regression equation. Our goal is to explain the post-hurricane increase in individual delinquency in flooded areas. Our outcome variable is individual delinquency and our key explanatory variables are extensive margin measures of disaster assistance – namely, the share of registrant homeowners approved for an SBA loan or a FEMA grant. We begin in Columns 1, 4, and 7 by controlling only for factors that should, by design, affect the allocation of disaster assistance – property damage average amounts and rates, flood insurance rates, flood depth, and floodplain coverage. These estimates are suppressed in Table 9 for brevity but are available in Appendix Table A5 along with several other variations on this specification. Next, we control for the effect of having below-median ability-to-repay, $\mathbb{1}(Low\_Ab2Repay_i)$, and, then, for the index in continuous form, $Ab2Repay_i$.[31] All explanatory variables must be interacted with the post-period dummy since they are time-invariant and, therefore, would otherwise be collinear with the individual fixed effects. Comparing the disaster assistance effect before and after including controls for initial financial condition offers a sense of the portion of the increase in delinquency in flooded areas that derives from the provision of disaster assistance as opposed to initial credit and resources.

Results suggest that the post-Hurricane rise in delinquency in particular flooded areas of Houston is unlikely to be fully explained by initial financial condition alone. Even after controlling for our *ability-to-repay index* (as well as several related factors shown in Appendix Table A5), the rate of SBA assistance offered to registrants in an individual's block, $\mathbb{1}(High\_SBA)$, remains significantly predictive of the post-Harvey change in her delinquent debt share. In contrast, a higher probability of receiving FEMA aid, $\mathbb{1}(High\_FEMA)$, does not significantly reduce delinquency. This difference in the explanatory power of the two aid programs likely reflects the fact that FEMA grants are on average, just one-tenth of the dollar value of SBA loans.

[31] As documented in Appendix Table A5, results are robust to using alternative measures of individual financial condition, including separately estimating the underlying continuous inputs into our *ability-to-repay index* as well controls for median home value, density, and owner-occupied share.

According to Column 3, coming from a flooded block with an above-median share of applicants approved for SBA loans implies a 0.38 percentage point lower delinquent debt share compared to before Harvey and compared to other flooded blocks with a lower SBA approval rate. A comparison of Columns 1 and 3 offers suggestive evidence that differences in individual initial financial condition may explain as little of 33% – i.e., (0.574-0.382)/0.574 – of the relationship between greater access to SBA loans and lower delinquency.

Of course, it is possible that, despite our best attempt to account for the confounding role of initial financial condition, there exist unobserved factors that affect both an individual's probability of receiving an SBA loan and her ability to avoid post-Hurricane delinquency. We, therefore, interpret these results cautiously. They are merely suggestive evidence that greater SBA (but not FEMA) assistance plays a role in reducing post-hurricane delinquency in flooded areas.

Table 9: Relationship between disaster aid and the post-hurricane change in *Severely Delinquent % of Total Debt*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$P_t \times \mathbb{1}(High\_SBA_b)$	-0.574***	-0.442**	-0.382**				-0.577***	-0.430**	-0.357**
	(-3.53)	(-2.63)	(-2.14)				(-3.69)	(-2.64)	(-2.06)
$P_t \times \mathbb{1}(High\_FEMA_b)$				-0.042	-0.188	-0.306	0.032	-0.132	-0.260
				(-0.16)	(-0.72)	(-1.18)	(0.12)	(-0.52)	(-1.02)
$P_t \times \mathbb{1}(Low\_Ab2Repay_i)$		3.333***			3.357***			3.338***	
		(15.10)			(14.94)			(14.86)	
$P_t \times Ab2Repay_i$			1.792***			1.804***			1.797***
			(13.20)			(13.30)			(13.21)
N	604,725	604,725	604,725	604,725	604,725	604,725	604,725	604,725	604,725
AdjR2	0.67	0.67	0.67	0.67	0.67	0.67	0.67	0.67	0.67
Y-mean	9.87	9.87	9.87	9.87	9.87	9.87	9.87	9.87	9.87
Controls (I_t^b)	Y	Y	Y	Y	Y	Y	Y	Y	Y

This table presents OLS estimates from the following specification: $Del\_Share_{it} = \beta\,(P_t \times Aid_b) + \alpha_i + D_t + \kappa A_{it}^2 + (P_t \times I_b)\,\rho + (P_t \times C_i)\,\vartheta + \varepsilon_{it}$. The sample is restricted to only blocks with non-zero flooding, damage, and FEMA registrants. The dependent variable, Del_Share_{it}, is the individual's share of debt that is in severe delinquency. The key explanatory variables (Aid_b) are indicators for whether individual *i*'s block *b* had an above-median share of homeowner applicants approved for an SBA loan, $\mathbb{1}(High\_SBA_b)$, or a FEMA IHP grant, $\mathbb{1}(High\_FEMA_b)$. All regressions include (but do not show) individual, α_i, and time, D_t, fixed effects as well as the square of individual age, A_{it}^2. All regressions also control for (but do not show) factors (I_b) that should, by design, lead to more or less disaster assistance flowing into a block and might also be correlated with delinquency: the average amount of property damage per registrant, the share of registrants with property damage, the share with flood insurance, *WAvg. Flood Depth*, and the floodplain share of the developed block area. Specified columns control for individual financial condition, C_i, using the *ability-to-repay index* in dummy (below-median) or continuous form. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Put together, all three tests provide suggestive evidence in support of a partial disaster assistance mechanism. If disaster assistance acted primarily as a safety net program for poor flood victims, we would not expect to find reductions in both delinquency shares and the use of sales financing among the high ability-to-repay sample in flooded areas relative to counterparts in areas that did not flood. We also would not expect the prevalence of disaster assistance in one's neighborhood to remain so predictive of individual delinquency, even after controlling for one's initial financial condition. While initial credit and resources, no doubt, plays an important role in recovery, such inequality appears unlikely to entirely explain the heterogeneity in credit outcomes documented in this paper.

5 Discussion

In the face of climate change, U.S. households must decide how to optimally manage disaster-related expected losses. This paper uses Hurricane Harvey, which hit Houston in August of 2017, as a lens through which to explore the current distribution of disaster assistance as well as on the credit consequences of having insufficient resources to recover. We present evidence that disaster assistance programs – both SBA loans and FEMA IHP grants – are regressive in allocation. While this finding might be consistent with the stated goals of the SBA's loan program, it is unexpected in the context of the FEMA IHP program. In turn, using a difference-in-difference regression design, we document a wide distribution in the treatment effect of flooding on credit outcomes outside of the floodplain. Broadly speaking, our findings imply that, for flooded households outside of mandated flood hazard zones, losses depend on whether the household chooses to purchase flood insurance or, instead, rely on disaster assistance.

While a comprehensive value calculation comparing flood insurance and SBA loans is beyond the scope of this paper, a back-of-the-envelope exercise can demonstrate the trade-offs for different types of homeowners. Let us first consider a homeowner who buys flood insurance and lives outside of the 100-year floodplain in the moderate flood risk Zone B (defined as between 0.2%–1% annual flood risk). Collier et al. (2020) and GAO (2013) show that consumers tend to over-insure, selecting flood insurance premiums well above the expected value of their contracts. We confirm this fact using OpenFEMA data on single-family NFIP policies opened in Houston in 2016 and 2017. A high coverage limit of $250,000 and a $1,250 deductible is, by far, the most common selection even though the average paid flood insurance claim from Harvey was $121,000. According to the OpenFEMA data, the average annual flood insurance premium for this policy type in Zone B of Houston is $663. Therefore, for an insured homeowner with a 0.7% annual flood risk, the prospect of flooding generates an expected annual cost of $672 (the premium plus the expected deductible), which reflects a $167 NFIP subsidy.

Now, take the case of a homeowner with the same flood risk who is likely to be approved for an SBA loan. Amortizing a $121,000 SBA loan with monthly payments at the subsidized 1.75% annual interest over 30 years would cost $432 per month. Discounting those future payments to the present at 2.8% (the 30-year Treasury rate at the time) results in a present value of $105,201, which, due to the subsidized below-market SBA loan rate, is less than the amount of damage. In expectation, the annual cost of flooding without insurance is then $736, or $64 more than with insurance. Depending on risk aversion, discounting, the prospect of flooding more than once, and whether there is asymmetric information about true flood risk, $64 might or might not be sufficient to incent a homeowner to pay the upfront insurance premium rather than take the chance on having an ex-post loan payment.

As a final example, assume the homeowner is unlikely to qualify for an SBA loan and, instead, must use a price-discriminating loan vehicle, such as a home equity loan, to finance reconstruction. If we assume an interest rate of 6.5% on a 15-year home equity loan. Discounting at the 10-year Treasury rate of 2.1%, the present value of the monthly ex-post loan payments now amounts to $162,629 – substantially more than the $121,000 in damage. Again, assuming 0.7% annual flood risk, the expected annual cost of flooding without insurance is $1,138, or $466 more than with insurance. Hence, without access to an SBA loan, flood insurance is much more likely to be optimal, even when the homeowner over-insures.

Considering the long-term costs of higher bankruptcy and delinquency rates, this simple exercise probably understates the benefits of insurance for homeowners who cannot access SBA loans. Miller & Soo (2020) show that the removal of a bankruptcy flag on a credit report results in a sharp increase in access to traditional credit and raises credit scores, credit card limits, and approval rates. Brevoort et al. (2020) show that reduced medical collections following an expansion of health insurance lead to significant declines in the offered interest rates on credit cards and personal loans. It follows that flood insurance may result in substantial long-term savings through greater access to price-discriminating forms of credit with lower associated interest payments.

Our findings carry several policy implications. First, our results highlight a strange interplay between Federal Disaster Loan Program and the National Flood Insurance Program (NFIP). Moral hazard in flood insurance is controlled, to an extent, by mandatory coverage requirements for mortgaged homes in 100-year floodplains. Outside of these areas, however, our results signal that flood insurance is less valuable to high-credit quality households relative to low-credit quality households since the former gains access to generous ex-post disaster loans and, as we show, experience no negative credit impacts from flooding. As homeowners learn from disasters, an open question is whether disincentives to purchase insurance due to the presence of SBA loans may lead to less pooling of risk and higher insurance premiums.

Second, our results highlight that averages mask important nuance after disasters, which can bias our understanding of how effectively federal disaster programs mitigate financial burden. Because negative treatment effects are highly concentrated in a relatively small subset of the population, they can be diluted in aggregate, producing insignificant or mild average effects. Evidence in this paper points to SBA disaster loans as a particularly important factor in explaining the disparities in post-disaster credit outcomes. However, any democratization of the SBA disaster loan program – e.g., through boosting taxpayer subsidies, extending maturities, and/or using price discrimination – should be carefully weighed against associated disincentives to insure.

Lastly, the fact that we observe better outcomes inside the floodplain highlights the importance of the NFIP in protecting households across the wealth distribution from shocks. Given the degree of flooding that occurred outside of the floodplain in Houston, however, mortgage-holder insurance mandates that are based on position within a 100-year floodplain seem arbitrary and out of date with current flood risk. Moreover, existing mandates may send homeowners an inaccurate signal of flood risk (Kunreuther et al., 2018). To encourage broader take-up, particularly within lower-income populations, FEMA may consider tying flood insurance premiums to income.[32] Complicating any expansion of funding for the NFIP, however, are criticisms that insurance subsidies induce rebuilding in flood-prone areas – an issue that could be rectified by expanding FEMA and state government buyout programs.

[32]An income-based premium sharing program was proposed by FEMA in a 2018 policy report on flood insurance affordability issues. See https://www.fema.gov/media-library-data/1524056945852-e8db76c696cf3b7f6209e1adc4211af4/Affordability.pdf.

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A Online Appendix

A.1 Additional Tables and Figures

Table A1: Tests relating SBA loan approval rates to proxies for ability-to-repay an SBA loan, block-level

Dependent variable: $\mathbb{P}(SBA_b > 0)$

	(1)	(2)	(3)	(4)	(5)	(6)
$Median\ Riskscore_b$	0.100***				0.038**	
	(9.44)				(2.10)	
$Median\ CardUtilization_b$		-0.068***			-0.006	
		(-6.32)			(-0.39)	
$Median\ Income_b$			0.108***		0.078***	
			(11.42)		(6.64)	
$Minority\ Share_b$				-0.075***	-0.021*	
				(-7.66)	(-1.86)	
$Low\_Ab2Repay\_Share_b$						-0.125***
						(-12.61)
N	7429	6970	7429	7429	6970	7429
R2	0.25	0.25	0.25	0.24	0.26	0.25

The table presents block-level, cross-sectional tests relating the share of a block's FEMA registrants that are approved for an SBA loan, $\mathbb{P}(SBA_b > 0)$, with four proxies for the factors the SBA considers in its ability-to-repay decision as well as a measure formed from the first principal component of those proxies. Specifically, $Low\_Ab2Repay\_Share_b$ is equal to the portion of a block's sampled residents that have a below-median individual ability-to-repay index value. All variables are standardized continuous variables. All regressions include controls for the average amount of property damage per registrant, the share of registrants with property damage, the share with flood insurance, *WAvg. Flood Depth*, and the floodplain share of the developed block area (*Flp*). The sample is restricted to only blocks with non-zero flooding, damage, and FEMA registrants. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A2: SBA loan denial reasons

Denial Reason	2017-2018 Frequency (%)
Unsatisfactory credit history	57.45
Lack of repayment ability	23.79
Unsatisfactory history on a Federal obligation	6.52
Ineligible real property	2.82
Lack of repayment ability - Applicant's income below minimum income level for the family size	1.47
Lack of ability to repay a disaster loan based upon the applicant's income alone	1.27
Unsatisfactory history on an existing or previous SBA loan	0.95
Other	0.88
Not eligible due to policy (non-citizen, NOT a qualified alien)	0.87
Not eligible due to failure to maintain required flood insurance as directed by FEMA	0.75
Not eligible due to recoveries from other sources	0.74
Not eligible due to delinquent child support payments	0.68
N	51,513

The table provides the frequency of various reasons given by the SBA to explain denying a loan to applicants in the area of Hurricane Harvey. These are the authors' tabulations of data from the Small Business Administration (SBA). Note that we tally only the first reason given. Second reasons are given very rarely (<1% of the time). Data includes all counties impacted by Harvey.

Table A3: Correlates of FEMA IHP or SBA approval

	$\mathbb{1}(FEMA_i + SBA_i > 0)$ FEMA registrants approved for FEMA IHP aid and/or SBA loan				$\mathbb{1}(FEMA_i > 0 \mid SBA_i = 0)$ FEMA registrants approved for FEMA IHP aid but not SBA loan			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}(Low\_Ab2Repay\_Share_b)$	-2.382***		-7.644***	-4.528***	-1.675***		-6.466***	-2.798***
	(-7.811)		(-11.821)	(-8.538)	(-5.808)		(-9.426)	(-5.139)
$Minority\ share_b$ *(p.p.)*		-0.052***				-0.043***		
		(-11.027)				(-9.520)		
$\mathbb{1}(Flood\ insurance_i)$	-2.509***	-2.510***	-7.737***	-8.486***	-4.069***	-4.064***	-10.173***	-10.459***
	(-10.369)	(-10.378)	(-20.507)	(-22.303)	(-16.792)	(-16.775)	(-24.154)	(-24.314)
$Property\ damage_i$ *($)*	2.574***	2.575***	1.559***	1.576***	2.773***	2.772***	1.757***	1.793***
	(47.322)	(47.378)	(33.395)	(32.338)	(45.595)	(45.603)	(32.473)	(30.668)
Block WAvg. flood depth (ft)	0.019	-0.020	0.697**	-0.201	0.028	-0.014	0.761**	-0.229
	(0.102)	(-0.112)	(2.004)	(-0.652)	(0.169)	(-0.084)	(2.161)	(-0.701)
Block share of registrants w flood insurance	-8.294***	-8.296***	-2.461*	4.392***	-8.039***	-8.253***	-2.523*	4.127***
	(-9.182)	(-9.376)	(-1.805)	(4.196)	(-9.143)	(-9.579)	(-1.718)	(3.638)
Block share of registrants w property damage	37.250***	36.752***	37.350***	33.930***	32.610***	32.270***	37.668***	33.570***
	(35.005)	(34.145)	(16.053)	(21.577)	(30.782)	(30.181)	(15.446)	(21.630)
Block share of housing units registered	-0.366	0.951	12.545***	33.748***	-0.446	0.794	12.793***	30.573***
	(-0.625)	(1.613)	(5.586)	(8.219)	(-0.791)	(1.409)	(5.627)	(7.285)
Block share of housing units w damage	12.124***	11.058***	10.422***	-11.816**	13.408***	12.403***	12.165***	-6.266
	(7.602)	(7.035)	(3.129)	(-2.420)	(8.373)	(7.905)	(3.471)	(-1.245)
Block population density (per acre)	0.067	0.094	0.086	-0.491	0.079	0.104	0.120	-0.691*
	(0.969)	(1.390)	(0.340)	(-1.367)	(1.160)	(1.568)	(0.487)	(-1.923)
Block owner-occupied share	-0.046***	-0.045***	-0.191***	-0.157***	-0.059***	-0.060***	-0.226***	-0.189***
	(-8.449)	(-8.583)	(-12.898)	(-11.743)	(-11.698)	(-12.300)	(-15.586)	(-14.097)
Sample	*All*		*Damage > 0*	*Damage > 0 & Owners*	*All*		*Damage > 0*	*Damage > 0 & Owners*
Y-mean	20.34	20.34	58.53	61.43	16.91	16.91	53.60	55.40
N	419,360	419,360	135,706	96,572	402,085	402,085	121,305	83,513

Table presents cross-sectional OLS regressions using individual FEMA registrant data and the specifications in Equations 3. The key explanatory variable, $\mathbb{1}(Low\_Ab2Repay\_Share_b)$, takes the value of one when an above-median share of a block's sampled residents are classified as having a low ability-to-repay index value. In Columns 1–4, the dependent variable, $\mathbb{1}(FEMA_i + SBA_i > 0)$, is defined as an indicator equal to one if an individual registrant is approved for FEMA IHP and/or for an SBA loan. In Columns 5–8, we drop observations for individuals who are eligible for SBA loans and the dependent variable, $\mathbb{1}(FEMA_i > 0 \mid SBA_i = 0)$, is defined as an indicator equal to one if an individual registrant is approved for FEMA IHP assistance but not for an SBA loan. In all models, we control for whether an individual has flood insurance and the amount of assessed property damage by FEMA. All columns include controls for the block-level factors (B_b) described in Section 3.1. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A4: Effect of flooding on individual *Severely Delinquent % of Total Debt*, by PCA input

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
$T_b^{Q1-Q3} \times P_t$	-0.038	-0.089	-0.481***	-0.006	-0.014	-0.093	0.118	0.222	0.209	0.016	0.094	-0.093	0.194	0.004	0.907
	(-0.46)	(-0.78)	(-3.67)	(-0.12)	(-0.25)	(-0.47)	(0.63)	(0.95)	(0.38)	(0.18)	(0.70)	(-0.24)	(1.49)	(0.02)	(1.20)
$\mathbf{T_b^{Q4}} \times P_t$	-0.035	0.151	-0.163	-0.088	0.213	-0.605**	0.248	1.300***	-0.387	0.086	0.299	-0.377	0.100	0.229	0.738
	(-0.20)	(0.74)	(-0.58)	(-1.04)	(1.50)	(-2.41)	(1.15)	(3.12)	(-0.61)	(0.46)	(0.76)	(-0.59)	(0.39)	(0.59)	(0.80)
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(Low\_Ab2Repay_i)$	0.433*	0.651	0.827												
	(1.83)	(1.62)	(1.12)												
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(Low\_Ab2Repay_i)$	0.995**	1.947**	-0.122												
	(2.24)	(2.09)	(-0.10)												
PCA INPUTS:															
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(Low\_Riskscore_i)$				0.295	0.446	0.229									
				(1.21)	(1.23)	(0.23)									
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(Low\_Riskscore_i)$				0.864**	1.518*	0.612									
				(2.16)	(1.81)	(0.52)									
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(High\_CardUtil_i)$							-0.024	-0.105	-0.455						
							(-0.08)	(-0.33)	(-0.36)						
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(High\_CardUtil_i)$							0.065	-1.027	0.100						
							(0.17)	(-1.18)	(0.07)						
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(Low\_MedIncome_b)$										0.276	0.240	0.210			
										(1.01)	(0.64)	(0.17)			
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(Low\_MedIncome_b)$										0.661	1.899*	0.081			
										(1.65)	(1.82)	(0.05)			
$T_b^{Q1-Q3} \times P_t \times \mathbb{1}(High\_MrtyShare_b)$													-0.119	0.270	-1.328
													(-0.60)	(0.87)	(-1.59)
$\mathbf{T_b^{Q4}} \times P_t \times \mathbb{1}(High\_MrtyShare_b)$													0.348	1.113*	-1.570
													(1.00)	(1.84)	(-1.55)
N	1,826,715	1,216,537	154,089	1,826,715	1,216,537	154,089	1,826,715	1,216,537	154,089	1,826,715	1,216,537	154,089	1,826,715	1,216,537	154,089
AdjR2	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66
Y-mean	9.91	10.24	10.47	9.91	10.24	10.47	9.91	10.24	10.47	9.91	10.24	10.47	9.91	10.24	10.47
Sample	*All*	*out-Flp*	*in-Flp*	*All*	*out-Flp*	*in-Flp*	*All*	*out-Flp*	*in-Flp*	*All*	*out-Flp*	*in-Flp*	*All*	*out-Flp*	*in-Flp*

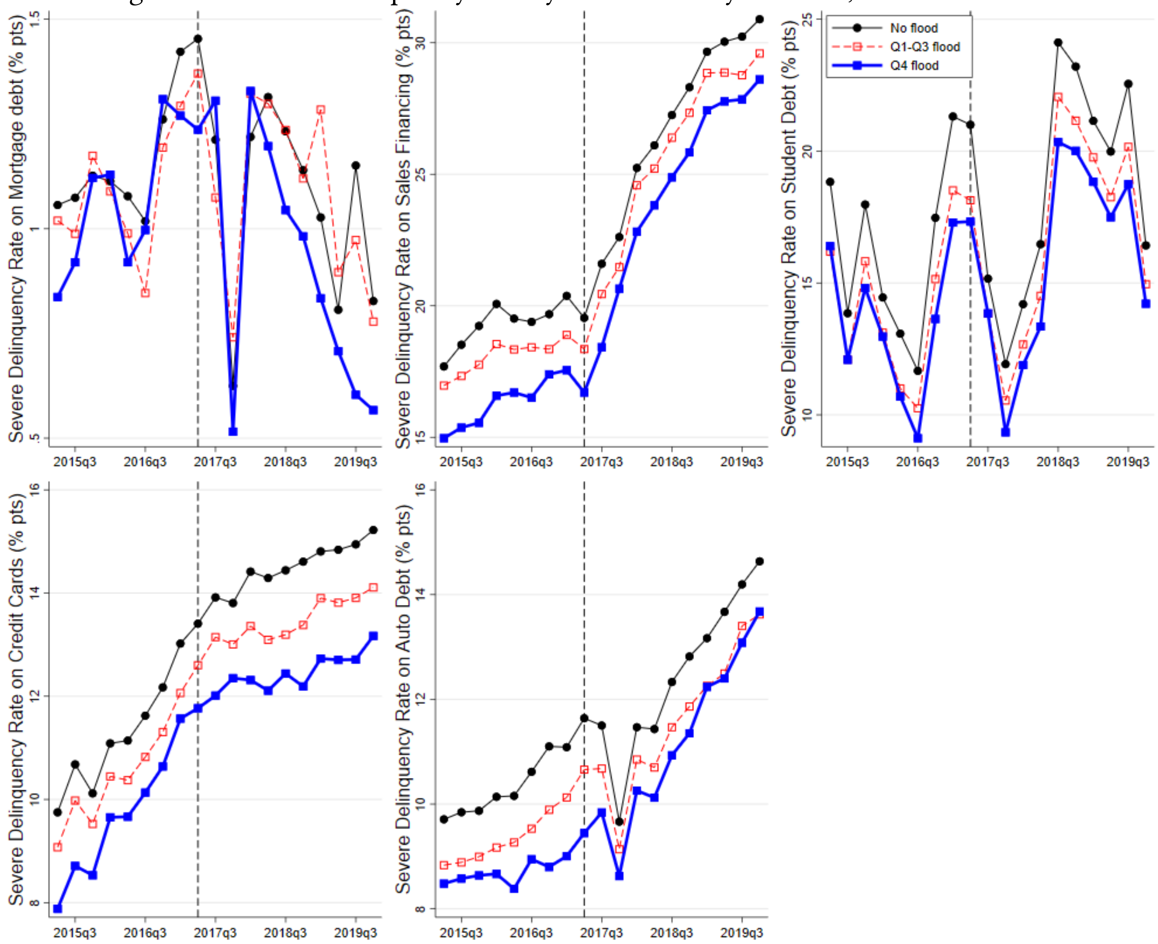
Table presents DiD estimates using the individual-level panel over Q2 2015–Q4 2019. The dependent variable is the percentage of total debt that is at least 90-days past due (*Severely Delinquent % of Total Debt*). To be included in the sample, the individual must have non-zero outstanding debt (denominator). Treatment intensity is defined according to quartile bins of *WAvg. Flood Depth* in the post period. In Columns 3 and 4, treatment is interacted with a dummy indicating that the individual had a below-median *ability-to-repay index* value as of Q2 2017 (*Low_Ab2Repay*). In the remaining columns, treatment is interacted with dummy versions (using median splits) of each of the four inputs into the the *ability-to-repay index*. All associated secondary interactions (that are not perfectly collinear with the fixed effects) are included but are not shown for brevity. Specified columns restrict the sample to individuals from blocks completely outside of the floodplain (*out-Flp*) or majority (at least 50%) inside the floodplain (*in-Flp*). All regressions include the full array of fixed effects and controls described in Section 3.2. Standard errors are clustered on Census block. Parentheses contain t-statistics: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Table A5: Relationship between disaster aid and the post-hurricane change in *Severely Delinquent % of Total Debt*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$P_t \times \mathbb{1}(High\_SBA_b)$	-0.574***	-0.442**	-0.382**	-0.522***	-0.343**	-0.577***	-0.430**	-0.357**	-0.504***	-0.319**
	(-3.53)	(-2.63)	(-2.14)	(-2.94)	(-2.37)	(-3.69)	(-2.64)	(-2.06)	(-2.88)	(-2.41)
$P_t \times \mathbb{1}(High\_FEMA_b)$						0.032	-0.132	-0.260	-0.200	-0.267
						(0.12)	(-0.52)	(-1.02)	(-0.78)	(-1.20)
$P_t \times Avg.Damage_b$ ($)	-0.000	0.000	0.000	-0.000	0.000	-0.000	0.000	0.000*	0.000	0.000
	(-1.07)	(0.27)	(1.37)	(-0.17)	(0.20)	(-1.08)	(0.46)	(1.90)	(0.11)	(0.54)
$P_t \times P(Damage_b)$	1.464***	1.101**	0.946**	1.369***	0.661	1.444***	1.186***	1.114**	1.495***	0.822**
	(3.15)	(2.59)	(2.28)	(3.23)	(1.64)	(2.98)	(2.72)	(2.62)	(3.44)	(2.06)
$P_t \times P(Insurance_b)$	-1.979***	-0.480	0.627	0.077	0.258	-1.978***	-0.481	0.628	0.085	0.271
	(-4.05)	(-1.15)	(1.53)	(0.19)	(0.64)	(-4.05)	(-1.15)	(1.53)	(0.22)	(0.67)
$P_t \times WAvg.Flood\,Depth_b$	-0.005	0.061	0.103	0.073	0.010	-0.005	0.060	0.102	0.072	0.009
	(-0.06)	(0.71)	(1.19)	(0.81)	(0.14)	(-0.05)	(0.70)	(1.16)	(0.79)	(0.12)
$P_t \times Flp_b$	0.855*	-0.181	-0.633	-0.535	-0.234	0.854*	-0.175	-0.624	-0.529	-0.226
	(1.97)	(-0.40)	(-1.38)	(-1.16)	(-0.63)	(1.98)	(-0.39)	(-1.37)	(-1.15)	(-0.61)
$P_t \times \mathbb{1}(Low\_Ab2Repay_i)$		3.333***					3.338***			
		(15.10)					(14.86)			
$P_t \times Ab2Repay_i$			1.792***	2.421***				1.797***	2.422***	
			(13.20)	(13.69)				(13.21)	(13.70)	
$P_t \times Riskscore_i$					-0.038***					-0.038***
					(-20.94)					(-20.97)
$P_t \times CardUtilization_i$					-0.013**					-0.013**
					(-2.29)					(-2.28)
$P_t \times Mrty\_Share_b$					-0.014**					-0.015**
					(-2.63)					(-2.65)
$P_t \times Median\_Income_b$					-0.000					-0.000
					(-0.23)					(-0.30)
$P_t \times OwnerOcc\_Share_b$				1.163**	-0.894*				1.136**	-0.931*
				(2.58)	(-1.86)				(2.56)	(-1.94)
$P_t \times Density_b$				-2414.040***	-912.811				-2460.524***	-973.183*
				(-3.69)	(-1.60)				(-3.79)	(-1.71)
$P_t \times Median\_HomeValue_b$				0.000***	0.000***				0.000***	0.000***
				(6.46)	(2.81)				(6.51)	(2.77)
N	604,725	604,725	604,725	594,477	507,510	604,725	604,725	604,725	594,477	507,510
AdjR2	0.67	0.67	0.67	0.67	0.64	0.67	0.67	0.67	0.67	0.64
Y-mean	9.87	9.87	9.87	9.64	6.44	9.87	9.87	9.87	9.64	6.44

This table documents the robustness of results in Table 9 to additional specifications and presents the suppressed control estimates. These are difference-in-difference estimates from the following specification: $Del\_Share_{it} = \beta\,(P_t \times Aid_b) + \alpha_i + D_t + \kappa A_{it}^2 + (P_t \times I_b)\,\rho + (P_t \times C_i)\,\vartheta + (P_t \times X_b)\,\eta + \varepsilon_{it}$. The sample is restricted to only blocks with non-zero flooding, damage, and FEMA registrants. The dependent variable, Del_Share_{it}, is the individual's share of debt that is in severe delinquency. The key explanatory variables (Aid_b) are indicators for whether individual *i*'s block *b* had an above-median share of homeowner applicants approved for an SBA loan, $\mathbb{1}(High\_SBA_b)$, or a FEMA IHP grant, $\mathbb{1}(High\_FEMA_b)$. All regressions include (but do not show) individual, α_i, and time, D_t, fixed effects as well as the square of individual age, A_{it}^2. All regressions also control for factors (I_b) that should, by design, lead to more or less disaster assistance flowing into a block and might also be correlated with delinquency: the average amount of property damage per registrant, the share of registrants with property damage, the share with flood insurance, *WAvg. Flood Depth*, and the floodplain share of the developed block area (*Flp*). Specified columns control for individual financial condition, C_i, using the *ability-to-repay index* in dummy (below-median) or continuous form, as well as using the four underlying inputs (in continuous form) into the index. Finally, we add additional block-level controls (X_b) for median home value, density, and owner-occupied share. Parentheses contain t-statistics, generated from standard errors clustered on Census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

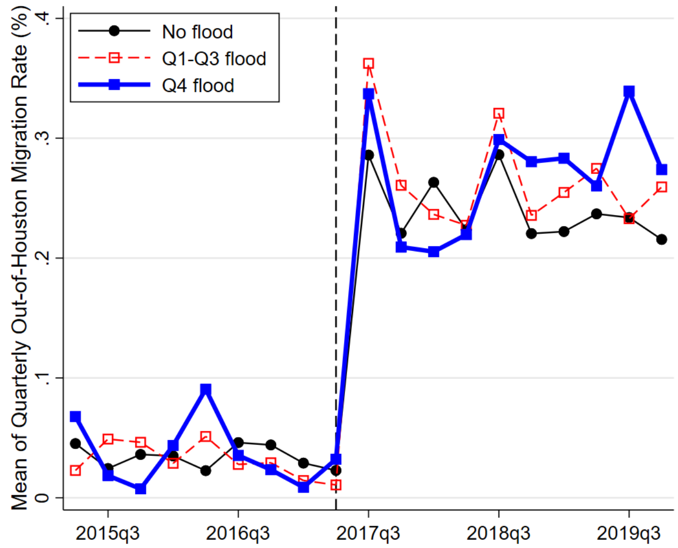
Figure A1: Severe delinquency rate by flood intensity and date, individual-level



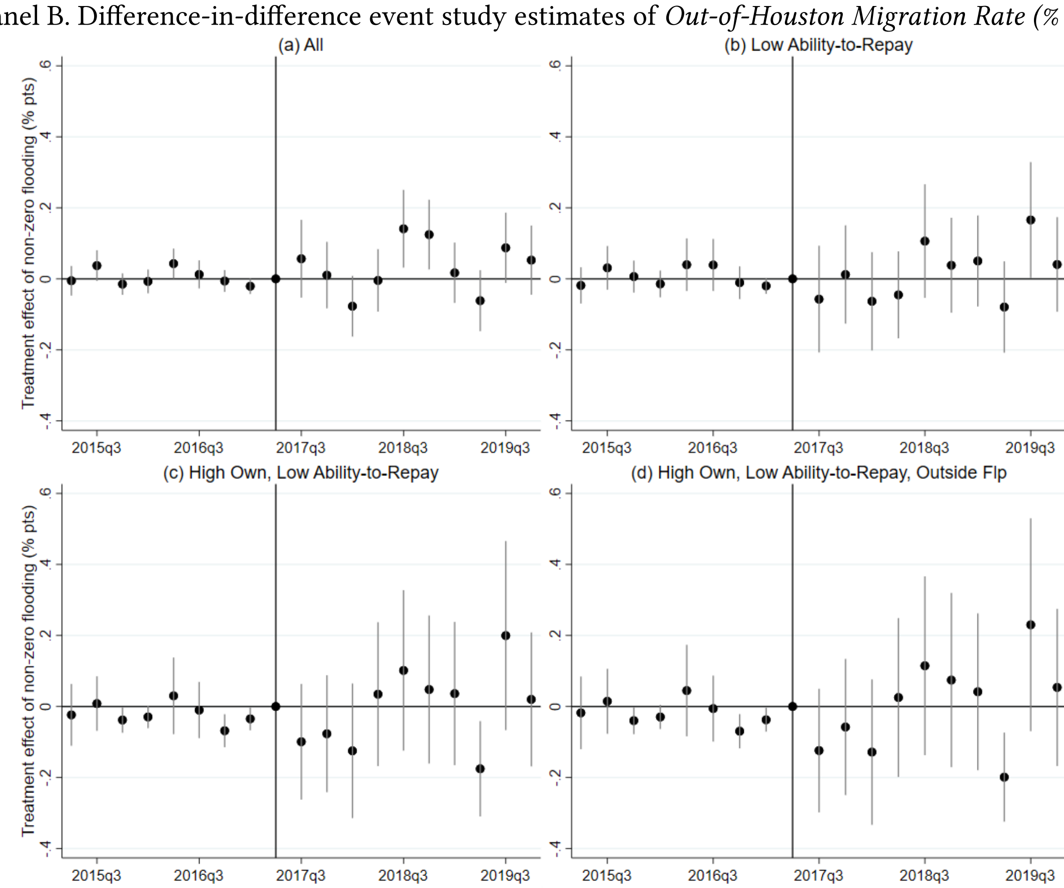
The figure plots the rate of severe delinquency (90 days past due) across individuals within blocks by flooding intensity of the block where the individual lived as of Q2 2017 and by date. Only individuals with non-zero balances as of Q2 2017 are included in the sample for each graph. For visual ease, blocks in the bottom three quartiles of flood depth are combined (red, squares). For student debt (which generally carries a high delinquency rate), we use 120 days past due. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A2: Nonparametric and difference-in-difference estimates of *Out-of-Houston Migration Rate*

Panel A. Mean of *Out-of-Houston Migration Rate*, quarterly block-level (%)



Panel B. Difference-in-difference event study estimates of *Out-of-Houston Migration Rate (% pts)*



The outcome variable is the block rate of migration out of Houston. All graphs use the block-level panel over Q2 2015–Q4 2019. We measure migration out of Houston at the block-level since our individual-level results condition on living in Houston in Q2 2017, we cannot establish a pre-trend for out-migration using the individual-level data. The outcome variable is measured as the share of a Houston block's residents who were living in that block as of the last quarter but who are no longer living in Houston this quarter. Panel A plots the nonparametric mean. Panel B plots the event study coefficients from DiD regressions. In Panel B, coefficients can be interpreted as the percentage point effect on the block out-migration rate of non-zero flooding relative to no flooding and relative to the rate in Q2 2017. The sample is split according to whether the block has an below-median share of residents classified as "low ability-to-repay" as of the last quarterly observation before the hurricane (Q2 2017). The sample is further split at the median according to the owner-occupied share and according to floodplain status (*Outside Flp*). All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A3: Flooding under Harvey relative to 100 year floodplain

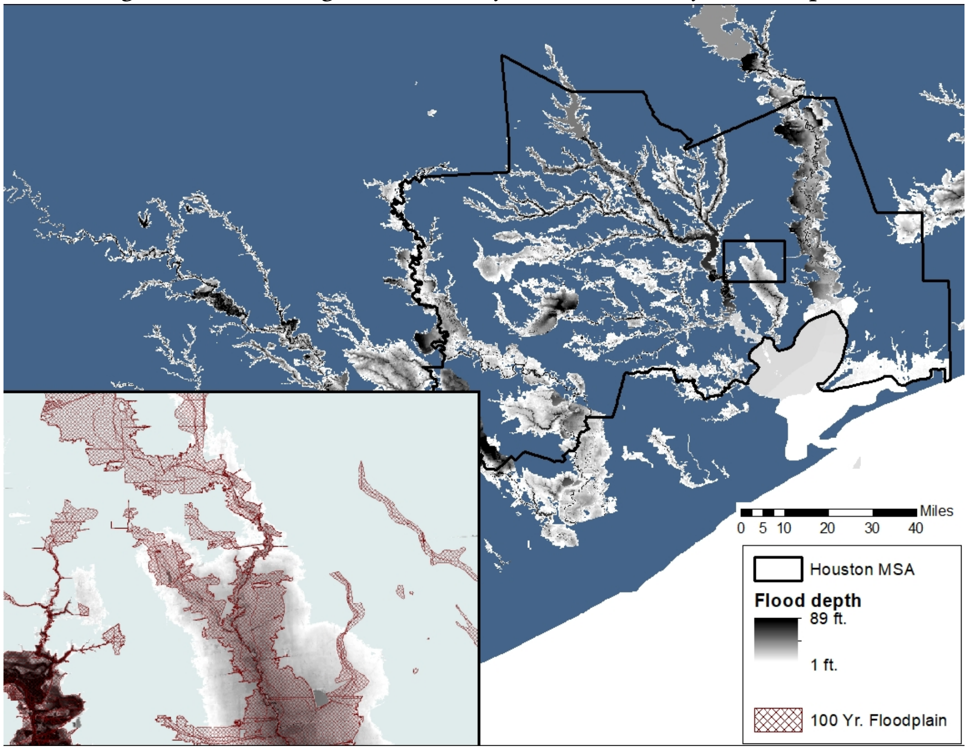
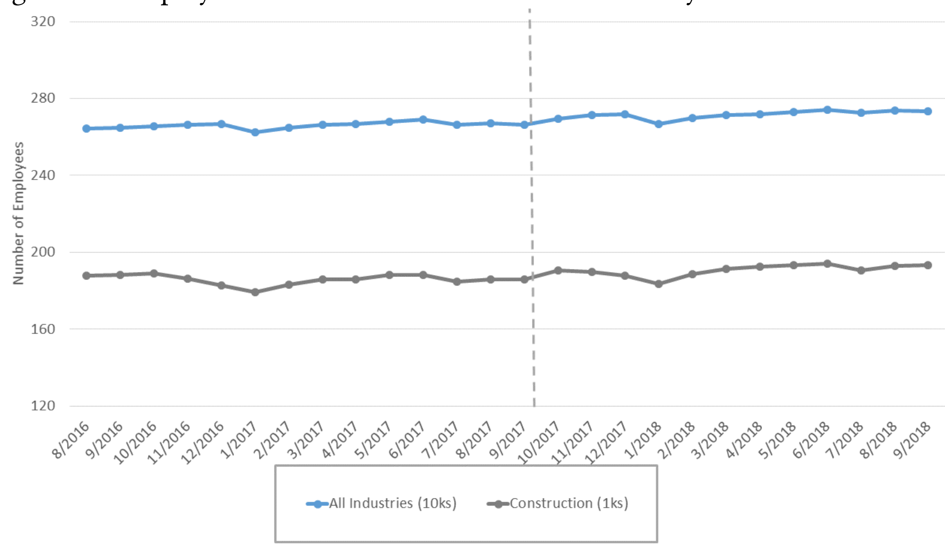
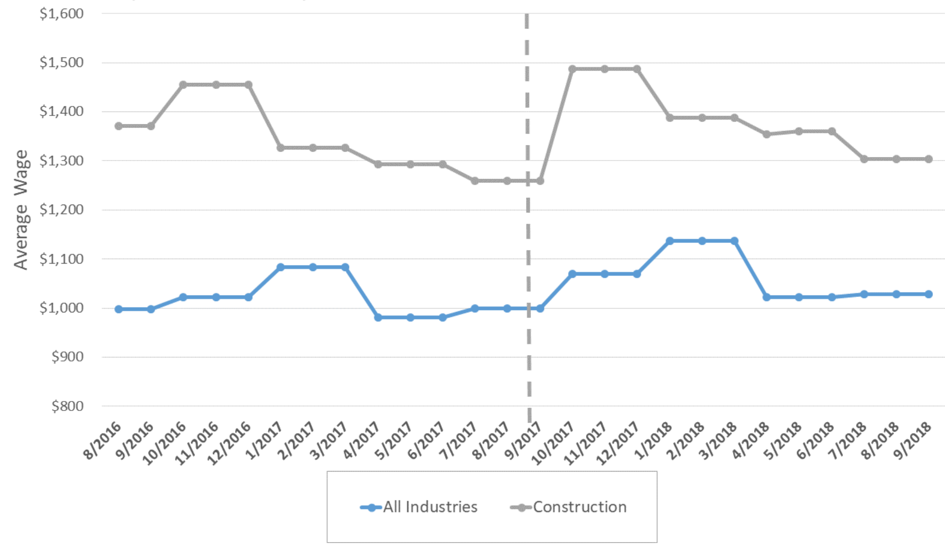


Figure A4: Employment before and After Hurricane Harvey for the Houston MSA



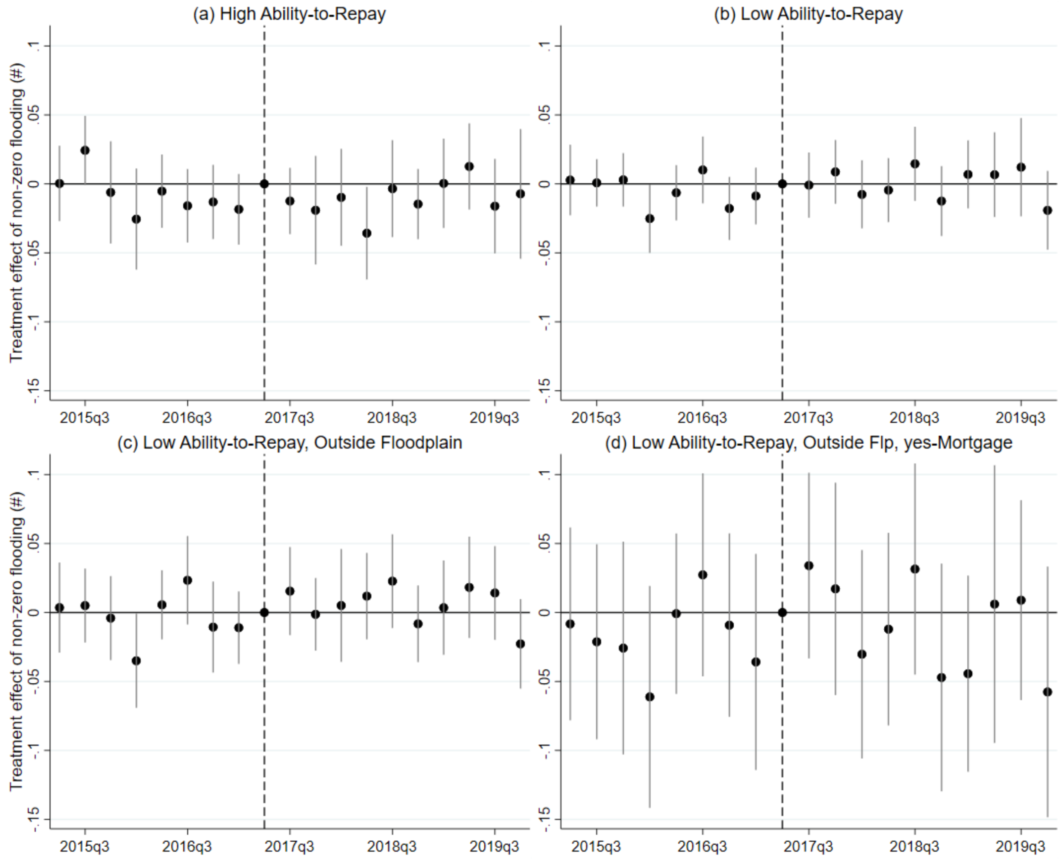
Note: Data downloaded from the US Bureau of Labor Statistics Quarterly Census of Employment and Wages (July 2019). https://data.bls.gov/cew/apps/data_views/data_views.htm#tab=Tables

Figure A5: Average Weekly Wages before and After Hurricane Harvey for the Houston MSA



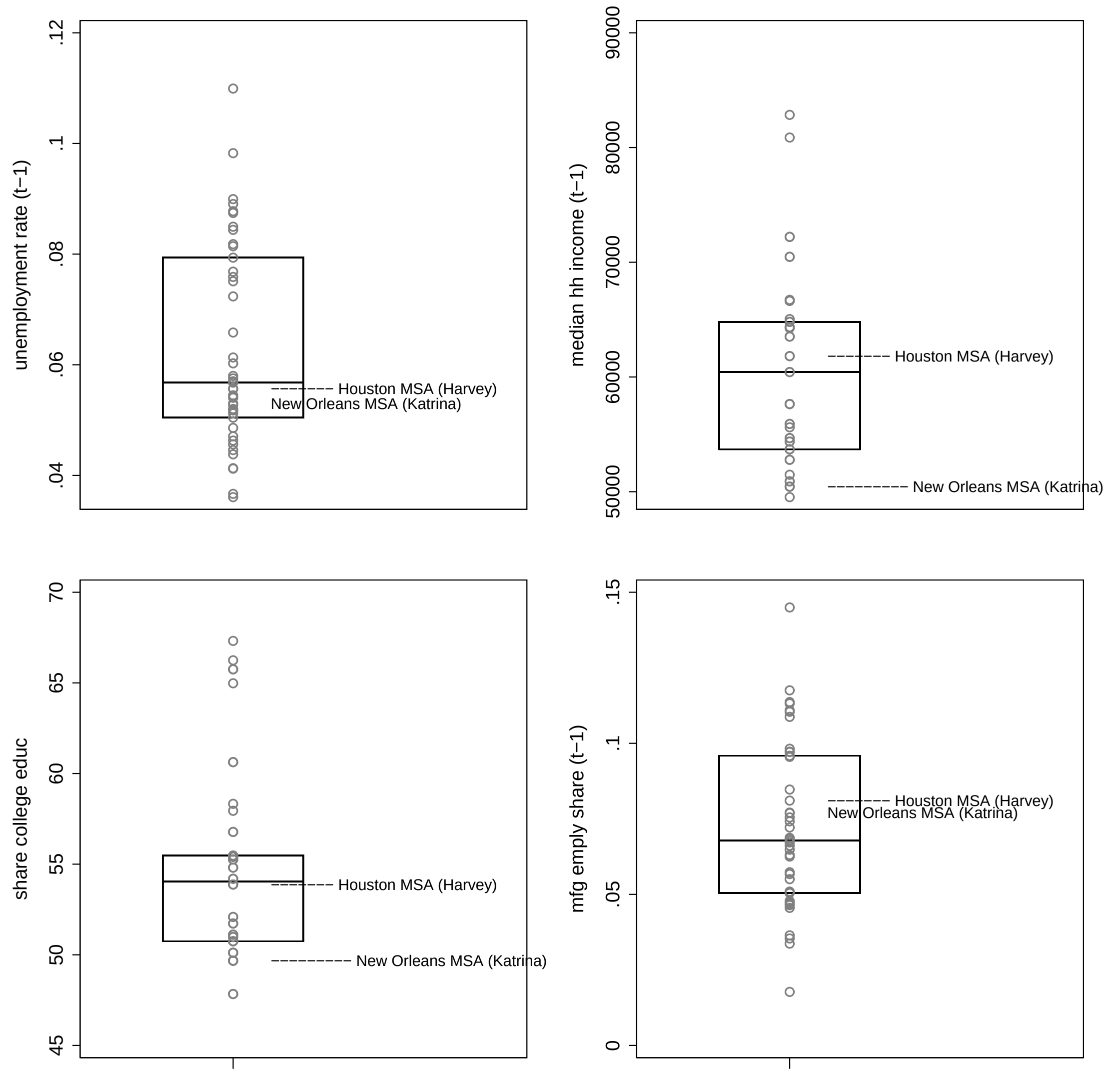
Note: Data downloaded from the US Bureau of Labor Statistics Quarterly Census of Employment and Wages (July 2019). https://data.bls.gov/cew/apps/data_views/data_views.htm#tab=Tables

Figure A6: The effect of flooding on *# of New Accounts per Inquiry*



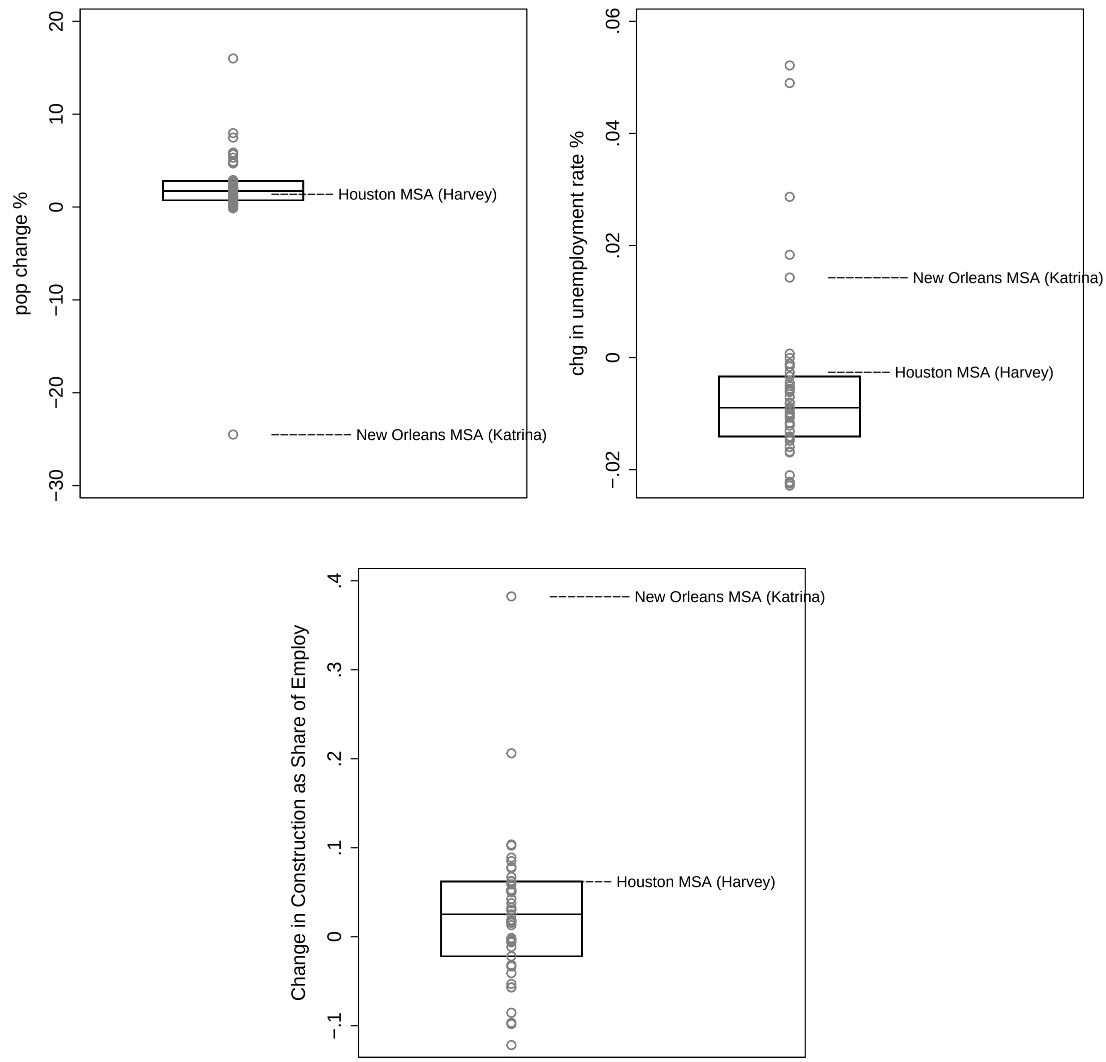
Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q4 2019. The dependent variable captures the extent to which credit supply changes relative to credit demand – measured as the number of new accounts opened during the quarter divided by the number of credit inquiries during the quarter (*# of New Accounts per Inquiry*). An individual's treatment intensity (*WAvg. Flood Depth*) is assigned according to the Census block where the individual lived as of the last quarter before the hurricane (Q2 2017). Coefficients can be interpreted as the number effect of being in a block with any (non-zero) flooding relative to being in a block with no flooding and relative to Q2 2017. Individuals with a below-median *ability-to-repay index* value are classified as "low ability-to-repay." Bottom panels restrict the sample to individuals living outside of the floodplain as well as to mortgage-holders. All regressions include the full array of fixed effects and controls described in Section 3.2. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A7: Disasters and MSA Attributes



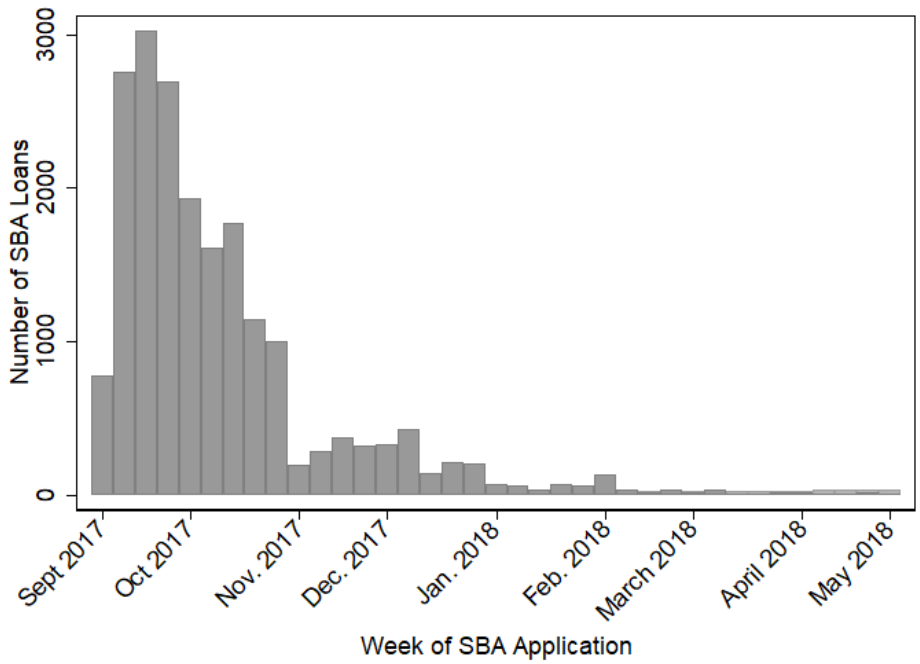
Figures provide the distribution of y-axis outcomes which are measured as annual values for all MSAs that experienced a major Hurricane (> $1 Billion in property damage and at least one fatality) in year t . We include an MSA multiple times if it experienced multiple distinct major hurricanes. Outcomes include the outcome of unemployment rate in year t-1 (top left), median household income in t-1 (top right), share with a college education in 2000 (bottom left), and share of employment in manufacturing in year t-1 (bottom right)

Figure A8: Disasters and Changes in MSA Attributes



Figures provide the distribution of changes in y-axis outcomes from year t to year t+1 for all MSAs that experienced a major Hurricane (> $1 Billion in property damage and at least one fatality) in year t . We include an MSA multiple times if it experienced multiple distinct major hurricanes. Outcomes include change in population (top left), change in unemployment rate (top right), and change in construction as a share of employment (bottom).

Figure A9: Timing of SBA loan issuance



This figure shows the weeks in which SBA loan assistance was granted to individuals in Houston after Hurricane Harvey. Data come from a FOIA request of the SBA.

B Geospatial data and the treatment variable

All flood depth mapping was done based on high water marks and hydrological modeling by the U.S. Geological Survey (USGS), working in cooperation with FEMA. The data contains the flood inundation polygons, flood-depth rasters, mapped boundaries, and high-water mark (HWM) locations for the selected river basins, coastal basins, and coastal areas in Texas and Louisiana that flooded as a consequence of Hurricane Harvey. If specific parcels or portions of rivers contain some type of barriers or embankments in anticipation of flooding from Harvey, they will not be considered in this model. These maps do, however, capture flooding due to the release of levees and dams during the storm (i.e., the releases of the Addick and Baker Reservoirs), since modeling is based on actual water crests. The flood data used in this paper encompasses the up-to-date information as of October 2018.[33]

To determine the developed portions of our study area, we incorporate high resolution (30m) geospatial data from the National Land Cover Database (NLCD).[34] This data is overlaid into Census 2010 TIGER files for Census Blocks to compute the portion of the block that is developed. To capture elevation and distance to hydrology (e.g. streams, rivers, lakes), we use USGS data, which uses satellite imagery and lidar calculations.[35] To assign elevation to a specific Census block, we average the elevation values within the block. Distance to waterways is calculated based on the distance of the centroid of a block to the nearest waterway with a value of zero for blocks that contain waterways.

To determine floodplain status, we use the 2015 extract of the FEMA National Flood Hazard Layer (NFHL), which incorporates all Flood Insurance Rate Map (FIRM) databases published by FEMA.[36] We create block-level measures of 100-year (1% annual flood risk) floodplains. To explore whether household debt responses vary according to flood insurance coverage, we calculate the 100-year floodplain share of the developed area within a Census Block (*Flp*, hereafter). Our preferred definition of "outside of the floodplain" includes those Census blocks in which the floodplain covers 0% of the block's developed area (*Flp=0%*); while Census blocks "inside the floodplain" are those where the floodplain covers at least 50% the developed block area ($\text{FlP} \geqslant 50\%$). In some restrictive subsample specifications, we lack sufficient sample size inside the floodplain within each flood intensity group to maintain our preferred definition of floodplain status. In these cases, we split floodplain status according to whether or not *any* share of the developed block area is in the floodplain. By using the floodplain as a proxy for flood insurance, we also indirectly control for expectations or, equivalently, for the extent to which a high-risk of flooding has

[33] All GIS maps for flood depth were downloaded from https://data.femadata.com/NationalDisasters/HurricaneHarvey/Data/DepthGrid/FEMA/ and in cases where maps had multiple versions, we created a composite version that assigned the highest flood depth to a given cell. This data has high spatial resolution with grid cells that are 25x25 feet. We include the following counties: Chambers, Harris, Liberty, Montgomery, Waller, and the parts of Fort Bend for which we found flood data. The maps for parts of Fort Bend County appear incomplete. We, therefore, include only parts of Fort Bend Country – in particular, areas closer to central Houston and the Cinco Ranch area.

[34] This data was downloaded at https://tnris.org/data-catalog/entry/national-land-cover-database-2011/. We include the following classifications in determining the portion of land in a 2010 Census Block that contains developed area based on NLCD classifications as low-intensity developed (imperviousness from 20 - 49%), medium intensity developed (imperviousness from 50 -79%), and high-intensity developed (imperviousness > 79%).

[35] Sources: https://tnris.org/data-catalog/entry/national-elevation-dataset-ned-2013/ and https://data.tnris.org/collection/af1ca25e-b38b-4203-90b8-d90f881963ae.

[36] Source: https://tnris.org/data-catalog/entry/fema-national-flood-hazard-layer/.

already been baked into the purchase price of the home (Dixon et al., 2013; Zhang, 2016).

We combine the spatial data on flood depth and developed land area to generate our main independent variable: the weighted average flood depth across the developed area of a block (*WAvg. Flood Depth*, hereafter). Figure B1 illustrates how we calculate our measure of flooding. In particular, we calculate the average flood depth of flooded areas within the developed portion of a Census Block (*Avg. Flood Depth*) as well as the flooded share of the developed portion of the block (*Flooded Share of Developed Area*). Our measure of flood intensity (*WAvg. Flood Depth*) is based on multiplying these two measures of flood intensity – one capturing depth and the other capturing breadth – together. In untabulated results, we find that both *Avg. Flood Depth* and the *Flooded Share of Developed Area* are independently predictive of both FEMA registration and FEMA-determined property damage. Hence, using only one of these two measures of flooding (either depth or breadth), instead of their composite, would discard meaningful information and increase measurement error.

Figure B1: Construction of flood intensity measure (*WAvg. Flood Depth*) within a Census block