# Let the Rich Be Flooded:
# The Unequal Impact of Hurricane Harvey on Household Debt

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October 21, 2019

**Abstract**

Hurricane Harvey submerged over 25% of Houston in August 2017. Using a treatment intensity difference-in-difference design on administrative credit data, we find that average treatment effects of flooding on household debt outcomes mask substantial underlying heterogeneity based on initial financial well-being, homeownership, and floodplain (i.e., flood insurance) status. Negative financial outcomes are concentrated among residents who entered the hurricane in a weaker financial position. For example, the average bankruptcy rate in high ownership areas that heavily flooded and have many financially constrained residents rose by 1.1 percentage points (or 30%) after Harvey relative to similar areas that did not flood. Being in a floodplain and, hence, having a higher likelihood of flood insurance, largely mitigates these negative effects. Using individual FEMA registrant and SBA loan data, we present evidence that our results may be explained, in part, by inequalities in access to federal disaster assistance and loans.

JEL: Q54; H84; D0; D1; R2
Keywords: inequality, financial constraint, bankruptcy, student debt, natural disaster, FEMA, SBA

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# 1 Introduction

Of the five worst hurricane seasons to affect the United States since the 1960s, three have occurred since 2004. As catastrophic flooding events along coastal communities happen with greater frequency, it is important to understand how households cope financially with this form of wealth shock. Several studies emerged from the catastrophe of Hurricane Katrina which struck New Orleans in 2005. The consensus of this literature is that victims of flooding experience only temporary and mild effects on measures of financial distress, from credit scores to income. This conclusion is puzzling, however, when juxtaposed with numerous anecdotal news reports detailing the financial devastation of some flood victims. Our paper attempts to reconcile these two narratives by testing for heterogeneity in in the individual financial impacts of flooding on households in the path of Hurricane Harvey, which submerged 25–30% of the Houston metropolitan area (444 sq miles) in August-September of 2017 and is arguably the most generalizable large, urban natural disaster.

Our study is most closely related to Gallagher and 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. In a summation of the extant literature, Gallagher et al. [2018] 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." Contrast this statement with, for example, a recent National Public Radio report that follows two households living near creeks on opposite ends of Houston. A year and half later the author finds that "Both families had to start over from nothing. But today, one family is financially stable. The other is facing bankruptcy."[1] The report puts forth that initial financial condition, compacted by inequalities in access to federal assistance, may drive a wedge in the financial path of flood victims. This hypothesis is consistent with a widening of wealth inequality in counties affected by disasters.[2]

We apply administrative credit data to the question of whether averages mask important heterogeneity in the financial effects of flooding. Our setting is Hurricane Harvey, which, unlike Hurricane Katrina, affected a wide variety of income and racial groups, both inside and outside of the designated 100-year floodplain, across an economically vibrant city. While Harvey generated massive flooding, it did not trigger a mass diaspora, which allows us to more plausibly hold constant the economic environment of flooded versus not flooded households.[3] We apply a treatment intensity difference-in-difference design, comparing

[1] The NPR report is available at https://www.npr.org/2019/03/05/688786177/how-federal-disaster-money-favors-the-rich. Also see https://www.nytimes.com/2018/09/03/us/hurricane-harvey-houston.html.

[2] In a sociological study, Howell and Elliott [2018] follow a sample of survey respondents over time as disasters of varying scales hit their counties. They find that more advantaged respondents – along lines of race, education, and homeownership – recover wealth more quickly than less advantaged respondents after disasters and some actually benefit financially from disasters, even controlling for differences in insurance levels. They find that the more FEMA aid a county receives, the more unequal wealth becomes after the disaster. Also see a report by the Urban Institute (Ratcliffe et al., 2019).

[3] We see an increase in annual out-migration from about 0.2% to 1% in our data after Harvey but this is no where near the

the credit outcomes of Houston residents that lived in blocks that were heavily flooded to those that lived in blocks that experienced little-to-no flooding.[4] Importantly, our analysis focuses on heterogeneity in treatment effects along two dimensions – initial financial well-being and the likelihood of having flood insurance. Separating our sample along these dimensions allows us to evaluate the effect of flooding on those who have more or less resources to absorb a wealth shock. To generate variation in flood insurance coverage, we exploit the fact that if an individual owns a home in a 100-year floodplain, that individual is required by her mortgage lender to have flood insurance.[5]

Our primary dataset is the Federal Reserve Bank of New York Consumer Credit Panel/Equifax Consumer Credit Panel (CCP). The CCP contains the credit scores, outstanding debts, and delinquencies of a random 5% sample of Houston residents with credit reports. From this data, we study the quarterly credit information on over 112 thousand individuals in the Houston metropolitan area over Q2 2015 to Q2 2019. We join this dataset to measures of flooding intensity under Hurricane Harvey at the census block-level gathered from FEMA. We differentiate individuals (and blocks) based on extent of financial constraint with an index constructed using four indicators of limited access to credit and resources. We split this index at the median into *Constrained* and *Unconstrained* individuals and blocks.

Consistent with prior research, we observe no average association between flooding and changes in the share of a block's residents with a bankruptcy flag on their account. This is true irrespective of the degree of financial constraint in the block. However, when we further condition the sample on blocks with an above median owner-occupied housing share, differential effects by initial financial constraint become apparent, even more so within blocks located outside the floodplain. Indeed, in high owner-occupied areas outside of the floodplain, heavy flooding combined with financial constraint is associated with an overall 1.1 percentage point (or 30%) increase in the bankruptcy rate relative to no-flood blocks. In contrast, we see a small downward trend in bankruptcy rates for some types of blocks – namely *Unconstrained* blocks; and *Constrained* blocks in low-owner occupied areas or inside the floodplain.

The share of an individual's total debt in severe delinquency follows a similar pattern, with *Constrained* individuals experiencing significant jumps in delinquency while the *Unconstrained* appear to be unaffected (or, sometimes, benefited) by flooding. This treatment effect for *Constrained* individuals grows larger in areas outside of the floodplain and in blocks with the most extreme cases of flooding (i.e., in the top decile of flooded blocks). For example, among mortgage-holders with little equity in their homes (having

26-54% out-migration from New Orleans after Katrina (Deryugina et al., 2018, Paxson and Rouse, 2008, Sastry, 2009).

[4]To affirm that our results stem primarily from a wealth shock, we verify that flooding in Houston under Hurricane Harvey was quasi-random and had minimal impacts on migration, employment, and wages. We show that, incredibly, only about 6% of the variation in flooding across Houston census blocks can be explained by cubic polynomials of geo-spatial attributes – like 100-year floodplain status, elevation, and distance to streams. Pre-determined socioeconomic variables explain at most 1% of the variation in flooding across Houston census blocks. Any changes in employment and wages returned to pre-existing trends within six months.

[5]As many Houston blocks in the 100-year floodplain did not flood, and vice versa, we can then compare treatment effects within the floodplain versus outside of the floodplain. By using the floodplain as a proxy for flood insurance, we remove concerns related to non-mortgaged households opting out of flood insurance. We also indirectly control for expectations or, equivalently, for the extent to which a high-risk of flooding has been priced into the collateral value of the home by focusing on variation in flooding separately for blocks within and outside the 100-year floodplain (Dixon et al., 2013, Zhang, 2016). Vossler and Holladay [2018] survey residents in New York and find that for those in a 100 year floodplain, 90% of households with a mortgage have flood insurance while 60% of households without a mortgage had flood insurance.

opened a mortgage within the 5-years proceeding the storm), *Constrained* borrowers in the top tercile of flooded areas see a 35% relative increase in their pre-Harvey delinquent debt share. Beyond bankruptcy and delinquency, we find that *Constrained* mortgage holders located outside the floodplain who experience extreme flooding (top-decile) see a 24 point drop in credit scores (measured by the Equifax Risk Score) relative to no-flood blocks. Moreover, these treatment effects appear to persist through much of the post period. Overall the effect sizes on bankruptcy, delinquency and credit scores are substantial and offer a first indication of the unequal effects of flooding on individuals according to initial financial condition.

Although debt balances are not necessarily indicative of financial stress, they are informative about consumer allocation decisions after experiencing flooding. Therefore, we study the intensive and extensive margin of mortgage, home equity, student, auto, and consumer financing debt. Consistent with evidence from Katrina Gallagher and Hartley [2017], we observe a significant reduction in mortgage debt in flooded areas two-qarters after Harvey. Though, unlike in New Orleans (where insurance was common and home values were below replacement costs even before the hurricane), we see little evidence of mortgage eliminations in parts of Houston where residents were more likely to have flood insurance. Instead, mortgage debt was primarily being eliminated by *Constrained* households outside of the floodplain – indicating that these homeowners may have felt compelled to sell rather than to make repairs due to a lack of the resources needed to rebuild. A possible lack of resources among *Constrained* mortgage-holders in flooded areas is consistent with observed changes in the use of home equity loans, auto debt, and consumer financing loans.

An important contribution of this study is an examination of student debt outcomes following a natural disaster. Outside of the Great Recession (Mueller and Yannelis, 2018), we know little about how wealth shocks influence student debt repayment behavior. Our findings are surprising and may reflect the fact that student debt is nondischargeable in bankruptcy. We find that flooding is associated with a temporary *reduction* in pre-existing student debt balances among *Constrained* mortgage-holders. The timing of the reduction and the affected subsample coincides with the elimination of mortgage debt discussed above, indicating that *Constrained* borrowers may be using the funds released from a home sale to pay down student debt. It is also possible that *Constrained* mortgage-holders are simply delaying education or using disaster assistance to reduce their nondischargeable debt in advance of a possible bankruptcy. We cannot differentiate these possible explanations. Interestingly, results also suggest that *Constrained* mortgage-holders over age-50 without pre-existing student debt become much less likely to take out (co-sign on) a new student loan (likely on behalf of their children) after experiencing local flooding, which may be a reaction to their unexpected loss of housing wealth.

Overall, unlike prior research in this area, we find that flooding does impose persistent financial stress, but negative outcomes are concentrated among homeowners that enter the hurricane in a weak financial position and live in areas with little expectation of flooding. On top of higher rates of bankruptcy and delinquency, these homeowners are also more likely to eliminate their mortgage after flooding (possibly, through forced sales) and become less likely to take out student loans. As we now explain, inequalities in access to disaster assistance may help account for some of the heterogeneity in treatment effects documented above.

Beyond flood insurance, there are additional sources of federal assistance – including cash grants from the Federal Emergency Management Agency (FEMA), loans from the Small Business Administration (SBA), and tax refunds from the Internal Revenue Service (IRS). The recent work of Begley et al. [2018], using a wide array of country-level disasters, provides a first indication of substantial inequalities (according to factors like credit score) in access to individual disaster loans from the SBA. Their finding is made more important by the work of Gallagher et al. [2018], who show that access to federal disaster assistance is a key determinant of financial recovery after tornadoes.[6] Put together, these studies highlight the importance of differentiating treatment effects based on initial financial well-being and, relatedly, on differential access to assistance. We expect that inequality in assistance may result from program eligibility criteria (e.g., credit scores, income, and citizen status), the subjectivity of the disaster inspection process, as well as legal difficulty in navigating FEMA's processes.

To formally document these inequalities in our setting, we turn to public FEMA data on individual Harvey-registrants from the Individual Household Program (IHP) as well as to SBA loan approval and denial data on Harvey applicants obtained by a FOIA request. We merge these data with block-level socio-demographic information from the Census. Using simple regressions that control for the extent of flooding, flood insurance, owner-occupied housing, and/or FEMA's own assessment of property damage, we present evidence that minority and lower-income census blocks face hurdles in obtaining assistance. Although these disadvantaged blocks were more likely to register with FEMA, they are less likely to be granted assistance. Conditional on being granted some assistance, the data shows that people from higher-income areas receive the most in FEMA assistance dollars. Furthermore, using FEMA's determination of whether the registrant is eligible for an SBA disaster loan (which is based on an income test), we observe that households from high-minority and lower-income areas are dramatically less likely to be determined eligible for SBA loans. Data from the SBA also tells us that, controlling for the extent of FEMA-assessed property damage, higher income blocks receive more in SBA disaster loan dollars. For example, a one standard deviation higher block income is associated with an additional $4,729 in SBA loans approved per housing unit, corresponding to 25% of the average amount in SBA approved loans per housing unit.

Results for financial outcomes coupled with disaster assistance highlight that current policy responses are not effectively mitigating the burden of natural disasters on financially fragile households. Although wealth is, itself, a form of insurance against flood damage, the evidence suggests that inequalities in access to federal disaster assistance may be exacerbating rather than counteracting pre-existing wealth differences. Consistent with this conclusion, we should expect to find greater wealth inequality in areas affected by natural disasters going forward. Beyond changes in how disaster assistance is allocated, our results also signal a need for more wide-spread flood insurance mandates in flood-prone areas like Houston. Given the degree of flooding that occurred outside of the floodplain, mortgage-holder insurance mandates that are based on position within a 100-year floodplain seem arbitrary and out of date with current flood risk.

[6] Gallagher et al. [2018] study the effect of variation in access to cash assistance following 32 tornadoes on the financial outcomes. They exploit the fact that some the tornadoes trigger a presidential disaster designation, qualifying victims for federal assistance programs, while others do not. They find that disaster-affected individuals who receive access to federal assistance have $260–$1,400 less in credit card debt than their counterparts – an effect that persists for 3 years after the disaster.

## 2 Background

This section describes Hurricane Harvey and what makes it unique from an identification standpoint. Then, we explain how flood insurance and Federal disaster assistance function and affect our identification strategy. In particular, we discuss why the largest source of disaster assistance by total dollars, SBA loans, tend to be less accessible to borrowers that enter the hurricane in a weaker financial position.

### 2.1 Hurricane Harvey

Hurricane 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). Harvey submerged 25–30% of the Houston metro area, 444 sq miles and caused $125 billion in damage, second in cost only to Hurricane Katrina (which hit New Orleans, Louisiana in 2005). Its associated flooding damaged as many as 135,000 homes and nearly 1 million cars.[7]

Figure 1 presents a map of the flooding, overlapped with the 100-year floodplain. As is evident, the flooding was widespread and somewhat random looking, particularly when compared with the floodplain. To illustrate the unique empirical identification qualities of Hurricane Harvey, it is helpful to compare it with Hurricane Katrina, which caused only slightly more in damage than Harvey.

First, a large share of the flooding in Houston was unanticipated. As is documented in Table 1, only about 6% of the variation in our measure of flooding across Houston census blocks can be explained by predetermined socioeconomic variables and geo-spatial attributes like 100-year floodplain status, elevation, and distance to streams.[8] Gallagher and Hartley [2017] estimate this same figure to be around 40% for Hurricane Katrina. Among the most flooded tercile of blocks under Harvey, only 24% of the developed block area was in a designated floodplain, on average. By comparison, under Katrina, over 90% of the most flooded blocks were in a designated floodplain (Gallagher and 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 (although, this rule is not perfectly enforced after mortgage origination). In Houston, only 17% of homes had flood insurance (versus 66% of New Orleans).[9]

Second, Harvey was fairly indiscriminate along lines of race, wealth, and education. If anything, the higher socio-demographic classes were slightly more affected by the disaster – which is unusual to the extent that wealth helps to insulate households from risk (e.g., by purchasing homes at higher elevations).

[7] These figures were taken from newspaper coverage based on a FEMA press release available at: https://www.fema.gov/news-release/2017/09/22/historic-disaster-response-hurricane-harvey-texas. Also see: https://www.worldvision.org/disaster-relief-news-stories/hurricane-harvey-facts.

[8] We later discuss precisely how we measure flooding and provide some scope to how sensitive results are to other mesures of flooding.

[9] These numbers are pulled from Hartley et al. [2019] and from https://www.nytimes.com/2017/08/28/business/dealbook/flood-insurance-harvey.html?mcubz=3.

Figure 1: Flooding under Harvey relative to 100 year floodplain

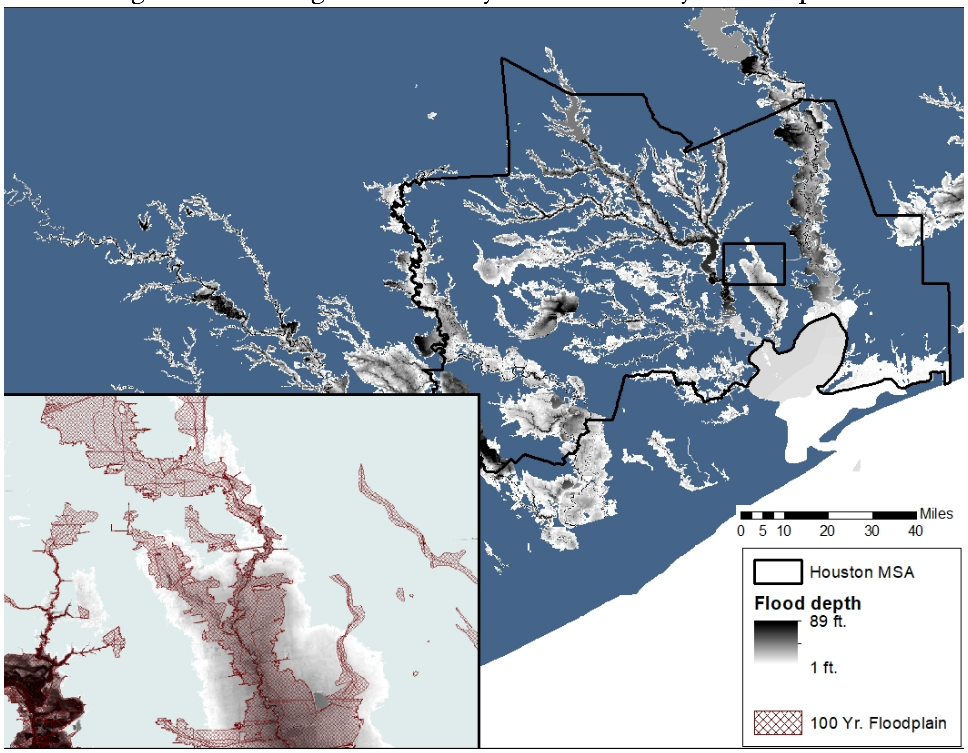



Table 1: Pre-hurricane correlates of treatment

| 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 median household economics | | | | 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 | 32937 | 32937 | 32937 | 32359 | 32359 | 32937 | 32359 |

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 continous 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 total block that is in the 100-year floodplain, share of total block 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 in poverty); block group-level socio-demographic characteristics (share with a college degree, 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.

This fact is apparent from the descriptive statistics, captured before the hurricane hit, in Tables 2 and 3. Tables 2 shows that median-income was $74,409 in the most flooded blocks versus $65,862 in the no-flood blocks. Median home values were also $23,146 higher in the most flooded blocks. Higher incomes and higher home values portend greater access to credit and, in turn, higher debt balances. Table 3 shows that average credit scores were 13 points higher in the most flooded blocks. Relative to individuals in no-flood blocks, the most flooded blocks had $16,560 more in mortgage debt, a higher credit card balance by $825, and $656 more in auto debt. Student debt is fairly similar across treatment intensities. Despite their higher balances, the most treated blocks were slightly less likely to be delinquent on their accounts pre-hurricane. For example, student debt delinquency – which is notoriously high and is, therefore, measured using 120-days past due (rather than 90-days) – was 20% in the no-flood blocks versus 17% in the most flooded blocks. Despite these facts, Table 1 also 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.[10]

Table 2: Pre-hurricane block-level characteristics by treatment intensity

| | | | | | | Mean of flooding treatment bin | | | |
|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | S.D. | p10 | p50 | p90 | No | T1 | T2 | T3 |
| WAvg. Flood Depth (ft.) | 0.27 | 0.82 | 0.00 | 0.00 | 0.85 | 0.00 | 0.03 | 0.29 | 1.87 |
| Avg flood depth of flooded (ft.) | 2.82 | 5.71 | 0.00 | 0.00 | 9.90 | 0.14 | 3.47 | 6.54 | 11.46 |
| Flooded share of dev. area | 0.10 | 0.15 | 0.00 | 0.00 | 0.10 | 0.00 | 0.01 | 0.04 | 0.16 |
| Floodplain share of dev. area | 0.11 | 0.28 | 0.00 | 0.00 | 0.50 | 0.05 | 0.16 | 0.23 | 0.23 |
| Elevation (ft) | 82.54 | 50.71 | 30.00 | 70.00 | 140.00 | 80.90 | 87.18 | 82.33 | 85.87 |
| Distance to stream (ft) | 4,244 | 4,329 | 285 | 3,194 | 8,954 | 4,554 | 4,082 | 3,669 | 3,481 |
| Med. household income ($) | 68,675 | 37,959 | 29,259 | 59,482 | 123,077 | 65,862 | 71,824 | 73,294 | 74,409 |
| Med. home value ($) | 162,911 | 121,518 | 70,900 | 122,500 | 290,400 | 156,113 | 167,248 | 175,012 | 179,259 |
| Share owner occupied | 0.72 | 0.25 | 0.33 | 0.80 | 0.96 | 0.72 | 0.74 | 0.74 | 0.73 |
| Share below poverty | 0.14 | 0.13 | 0.01 | 0.10 | 0.32 | 0.15 | 0.13 | 0.12 | 0.12 |
| Share college degree | 0.35 | 0.26 | 0.05 | 0.31 | 0.73 | 0.34 | 0.38 | 0.38 | 0.39 |
| Share minority | 0.39 | 0.27 | 0.08 | 0.33 | 0.83 | 0.41 | 0.36 | 0.36 | 0.33 |
| Out-migration rate (%) | 0.04 | 1.93 | 0.00 | 0.00 | 0.00 | 0.04 | 0.05 | 0.01 | 0.05 |
| Rate of bankruptcy (%) | 2.29 | 10.57 | 0.00 | 0.00 | 0.00 | 2.39 | 2.30 | 2.39 | 2.22 |
| Constrained rate (%) | 50.96 | 44.41 | 0.00 | 50.00 | 100.00 | 54.37 | 46.93 | 45.95 | 43.62 |
| Observations | 32,937 | | | | | 20,221 | 4,373 | 4,238 | 4,105 |

Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Third, Harvey did not alter the underlying economic rational for living in Houston, allowing us to more plausibly hold constant income and isolate the effect of the wealth shock. The Houston economy is diversified and was booming in the years leading up to Harvey – in terms of job, house prices, and population growth. Even in the year after Hurricane Harvey, Houston experienced net in-migration of

[10]To visualize the first two points above, Appendix Figure A3 plots the distribution of flooding under Harvey by floodplain status and income bin. The black bar on the graph shows that a larger number of blocks flooded within the higher deciles of block median-income relative to the lower-deciles. The association between flooding and median-income is driven primarily by blocks that were not in the floodplain. This fact may explain why, after controlling for floodplain status, block average economic and credit characteristics explain almost none of the variation in flooding across blocks (Table 1). Surprisingly, the green bar in Figure A3 shows that a substantial number of blocks that were in the floodplain did not flood during Hurricane Harvey. These blocks will act as controls when we later assess the effect of flood intensity on credit outcomes within the subsample of blocks located in the floodplain.

Table 3: Pre-Huricane individual-level credit variables by treatment intensity, Q2 2017

| | | | | | | Mean of flooding treatment bin | | | |
|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | S.D. | p10 | p50 | p90 | No | T1 | T2 | T3 |
| Equifax Risk Score | 682.28 | 112.96 | 521.00 | 703.00 | 817.00 | 677.86 | 684.40 | 687.12 | 691.06 |
| Share of debt in delinquency (%) | 11.12 | 28.76 | 0.00 | 0.00 | 56.50 | 11.94 | 10.64 | 10.07 | 9.78 |
| Credit Inquiries in 3 months (#) | 0.50 | 1.07 | 0.00 | 0.00 | 2.00 | 0.51 | 0.48 | 0.50 | 0.49 |
| New accounts per inquiry (#) | 0.28 | 0.54 | 0.00 | 0.00 | 1.00 | 0.28 | 0.29 | 0.29 | 0.27 |
| Total debt balance ($) | 75,059 | 141,389 | 22 | 23,948 | 206,178 | 69,747 | 73,821 | 82,065 | 88,385 |
| Mortgage balance ($) | 49,629 | 127,786 | 0 | 0 | 162,104 | 44,923 | 48,546 | 55,770 | 61,484 |
| Home equity loan balance ($) | 2,108 | 18,701 | 0 | 0 | 0 | 2,038 | 1,918 | 2,464 | 2,212 |
| Credit card balance ($) | 4,982 | 12,620 | 0 | 1,199 | 13,791 | 4,741 | 5,189 | 5,036 | 5,566 |
| Auto loan balance ($) | 10,261 | 17,227 | 0 | 0 | 31,747 | 10,075 | 10,303 | 10,413 | 10,731 |
| Student loan balance ($) | 6,148 | 23,924 | 0 | 0 | 16,478 | 6,182 | 5,978 | 6,131 | 6,220 |
| Consumer financing balance ($) | 702 | 2,292 | 0 | 0 | 2,175 | 711 | 685 | 691 | 696 |
| I(Mortgage 90dpd) | 1.41 | 11.80 | 0.00 | 0.00 | 0.00 | 1.48 | 1.28 | 1.36 | 1.38 |
| I(Credit card 90dpd) | 13.11 | 33.75 | 0.00 | 0.00 | 100.00 | 13.61 | 13.08 | 12.66 | 11.85 |
| I(Auto 90dpd) | 11.22 | 31.57 | 0.00 | 0.00 | 100.00 | 11.82 | 11.78 | 10.06 | 9.70 |
| I(Student 90dpd) | 18.72 | 39.01 | 0.00 | 0.00 | 100.00 | 19.98 | 17.89 | 17.02 | 16.66 |
| I(Home equity 90dpd) | 3.06 | 17.22 | 0.00 | 0.00 | 0.00 | 3.33 | 2.51 | 2.72 | 3.00 |
| I(Consumer financing 90dpd) | 19.08 | 39.29 | 0.00 | 0.00 | 100.00 | 19.80 | 19.48 | 18.09 | 16.88 |
| I(Mortgage>0) | 30.09 | 45.87 | 0.00 | 0.00 | 100.00 | 28.27 | 30.85 | 32.34 | 33.58 |
| I(Home equity loan>0) | 4.78 | 21.33 | 0.00 | 0.00 | 0.00 | 4.72 | 4.66 | 4.85 | 5.03 |
| I(Credit card>0) | 72.69 | 44.56 | 0.00 | 100.00 | 100.00 | 71.59 | 74.11 | 72.99 | 74.81 |
| I(Auto>0) | 44.67 | 49.72 | 0.00 | 0.00 | 100.00 | 44.49 | 44.50 | 44.58 | 45.62 |
| I(Student>0) | 19.21 | 39.40 | 0.00 | 0.00 | 100.00 | 19.46 | 18.71 | 18.91 | 19.14 |
| I(Consumer financing>0) | 22.53 | 41.78 | 0.00 | 0.00 | 100.00 | 23.13 | 22.10 | 22.21 | 21.16 |
| I(Bankrupt) | 0.02 | 0.15 | 0.00 | 0.00 | 0.00 | 0.02 | 0.02 | 0.02 | 0.02 |
| I(Constrained) | 0.50 | 0.50 | 0.00 | 1.00 | 1.00 | 0.54 | 0.49 | 0.47 | 0.43 |
| Observations | 112,927 | | | | | 61,063 | 17,959 | 16,655 | 17,250 |

Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

1.3%.[11] These facts stand in sharp contrast to the profound economic decay occurring in New Orleans at the time of Katrina (Vigdor, 2008).[12] Indeed, as documented in Appendix Table A4, migration out of Houston by those that lived through the storm appears to be around 1.2% in the year after Harvey, at least according to our credit bureau data (which includes only people with credit scores).[13] Compare this figure to estimates

[11]See: https://www.houston.org/download_manager/2444/1994. Also, Houston had very low rate of negative equity (just 1.45%) when Harvey hit, see: https://www.urban.org/urban-wire/how-will-hurricanes-harvey-and-irma-affect-housing-markets-texas-and-florida. For migration rates, see: https://www.houston.org/houston-data/annual-update-population.

[12]For example, Vigdor [2008] writes "Hurricane Katrina appears destined to have caused greater population loss after the damage of a single day than the city of New Orleans had witnessed over 45 years of slow decline...."

[13]This rate of out-migration is less than 1/5th the rate estimated by Gallagher and Hartley [2017] following Hurricane Katrina, which hit New Orleans in 2005. A high out-migration rate could pose identification concerns, as it becomes harder to isolate the wealth shock from other economic shocks associated with moving to a new place. Appendix Table A4 also presents difference-in-difference event study estimates showing that migration out of Houston after Harvey appears to be only slightly (~0.1% per

in Sastry and Gregory [2014], who report that, in the year after Hurricane Katrina, 47% of pre–Hurricane Katrina adults from New Orleans no longer resided in the New Orleans area. Appendix Figures A1 and A2 document that any effects of Harvey on employment and wages are short-lived returning to pre-Harvey trends six month after the disaster.[14]

The three factors discussed above allow for interpretation of any changes in financial outcomes that we document as resulting primarily from a wealth shock, rather than a shock to labor markets or other economic factors.[15] Also, the above discussion highlights the fact that we can differentiate households according to floodplain status – as a number of blocks in the floodplain did not flood and vice versa. Thus, we use the floodplain status of the block as a proxy for the likelihood of having held flood insurance. Of interest is whether floodplain status mitigates any of the changes in financial outcomes that we uncover after the storm.

## 2.2 Disaster assistance

Beyond flood insurance, there are three additional sources of assistance – Federal Emergency Management Agency (FEMA) grants, Small Business Administration (SBA) disaster loans and Internal Revenue Service (IRS) disaster refunds. These programs are all designed so as not to duplicate the benefits of flood insurance. As we dicuss below (and later support with empirical evidence), some forms of assistance are, by design, more accessible to victims in a better financial position.

### 2.2.1 Flood insurance

The first source of potential funding is for homeowners that have flood insurance, which is administered by the National Flood Insurance If an individual owns a home with a mortgage that is located in a 100 year floodplain, they are required to have flood insurance.[16] Also, some non-mortgaged homeowners as well as renters obtain flood insurance. In the floodplain, the average premium is usually several thousand dollars and varies across houses based on risk. Outside the floodplain, coverage is typically optional, but can be purchased at relatively low cost (e.g., around $600 annually), depending on position along a risk gradient. Nonetheless, very few individuals purchase coverage when they are located outside of the floodplain.

quarter, on average) higher in the most flooded areas relative to areas that did not flood. This effect is concentrated among homeowners (not shown) and among households that entered the storm in a stronger financial position, indicative of the costs associated with moving to a new city.

[14] As shown in Appendix Figure A1, we see no changes in total employment and a modest increase in construction employment from before to after the arrival of Hurricane Harvey. Correspondingly, Appendix Figure A2 provides evidence on an increase in wages right after Hurricane Harvey largely driven by the construction industry consistent with the cleanup efforts following Harvey. These results are consistent with evidence in Farrell and 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.

[15] Appendix B evaluates the credit market effects of the storm (Mian and Sufi, 2009), finding no clear evidence of a retraction in the supply of credit relative to the demand for credit.

[16] This requirement is part of the design of the National Flood Insurance Program [Gallagher, 2014]. Survey results presented in Vossler and Holladay [2018] show that 90% of homeowners living in a floodplain with a mortgage have flood insurance. We cannot directly verify this relationship in our dataset, but for a selective group of homeowners (registered with FEMA), we find that the share of FEMA registered homeowners with flood insurance has a significantly positive correlation with the share of the developed block area that is in a floodplain.

The NFIP covers up to $250,000 for the structure of your home and $100,000 for personal property. The policies typically include replacement cost coverage for the structure and cash value for the possessions. Flood insurance typically gives an initial payout (approx $7,000), which is not the full claim but allows households to deal with immediate expenses.[17] Within 1 month of Harvey making landfall, 87,000 Texans made flood insurance claims. The average NFIP payout in Texas due to Harvey was roughly $117,000.[18] Importantly, if under mortgage, the rest of the insurance payout is typically held in escrow by the lender and released in disbursements as repair work is completed. Flood insurance can also be used to pay down the mortgage. In other words, flood insurance payouts, while the largest form of assistance in average magnitude, may be the least fungible form of disaster assistance – meaning that a household with a mortgage cannot easily use their flood insurance payout to, for example, pay down student debt.

### 2.2.2 FEMA grants and data

The first step in a household obtaining disaster assistance is to register online or via phone with the Federal Emergency Management Association (FEMA).[19] FEMA approved about 370,000 applications in Texas, issuing roughly $1.6 billion in grants through FEMA's Individuals and Households Program (IHP).[20] Although FEMA grants are distributed via check or direct deposit (and money is fungible), the money comes with a letter explaining what the payment is to be used for. [21]

We obtained data from FEMA that provides individual records for each household that registered with FEMA in the months following Harvey.[22] This data does not provide identifying information, but does provide details on individual-level FEMA assistance, estimated damage determined by property inspection, as well as information on census block of residence and home ownership status. In our data and study area, we find that on average about 29% of housing units located in a block with any flooding registered with FEMA and, of those registered with FEMA, 80% were deemed *not* eligible for financial assistance. The typical IHP recipient received $7,300. At the high-end, a homeowner who can prove a 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

[17] These figures were taken from newspaper coverage in the weeks following the storm as well as from a FEMA press release, available at: https://www.fema.gov/news-release/2017/09/22/historic-disaster-response-hurricane-harvey-texas

[18] NFIP estimated that it paid out $8.9 billion in claims associated with Harvey. See: `https://www.fema.gov/significant-flood-events`.

[19] The application process for any federal assistance begins with FEMA registration. Affected residents must have a valid registration before they can apply for disaster assistance. A valid registration requires primary residence or business in a disaster area as well as registration within a given time period of the disaster. To qualify for Individuals and Households Program (IHP), you must be a US citizen, non-citizen national, or qualified alien. Based on the description of damages, the application is reviewed to determine whether it should be referred to IHP or another FEMA program. After verification of applicant is completed, an inspector is sent to examine the damage. At the inspection, residents are required to show documentation to validate their (1) identity, (2) occupancy at the damaged address, and (3) (if applicable) ownership of the damaged address. Finally, the inspector's report is considered, and FEMA may or may not approve IHP assistance. If FEMA does not approve the application for assistance, the applicant may appeal the decision.

[20] For sources, see footnote 17.

[21] Those receiving assistance are urged to keep receipts of their disaster spending for three years and random audits are conducted to confirm funds were spent properly. According to FEMA, an applicant that misuses the assistance may be denied future assistance or lead to a request to return all funds. See `https://www.fema.gov/news-release/2016/07/08/fema-those-who-receive-assistance-use-funds-its-intended-purpose`.

[22] 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.

2 months. When flood insurance plus FEMA grants do not cover all damage incurred, the household may receive assistance through a number of other programs (discussed below) up to the difference between expenses incurred after the storm and other forms of assistance granted.

In Section 6, we use FEMA individual registrant data to examine how block-level socioeconomic factors (like median income and minority share) correlate with FEMA assistance, while controlling for flooding and flood insurance status.

### 2.2.3 SBA disaster loans and data

A dominant form of assistance by dollar volume comes from low-interest loans backed by the Small Business Administration (SBA). The SBA offers disaster loans which can be used to repair and replace property, homes, and businesses. The loan terms are extremely attractive. In the context of consumers affected by Harvey, most borrowers received a low interest rate of just 1.75% (while a subset with outside credit options received the higher rate of 3.50%) and were given 30 years for repayment (Begley et al., 2018). Beginning 5 months from the date of the loan, borrowers pay equal monthly installments of principal and interest. Loans reach $200,000 for a primary residence and $40,000 for personal property. Although there are some stated rules about how the loan is intended to be used, the money is fungible, such that households may choose to switch from a traditional mortgage or some other form of debt to an SBA loan (Hartley et al., 2019).[23]

We expect financially constrained households to have more difficulty obtaining an SBA loan, however, since eligibility is based on: (1) applicant's disaster-related losses; (2) satisfactory credit; and (3) repayment ability, including debt-to-income levels. Importantly, the presence of credit and income as part of the eligibility criteria, as well as the fixed nature of interest rate, has the potential to exclude financially strained borrowers, as first documented in Begley et al. [2018]. These eligibility rules are there so that the SBA can achieve its goal of making revenue neutral loans using a single interest rate for all borrowers – i.e., the SBA does not price-discriminate according to the differential credit quality of borrowers.

From FOIA request of the SBA, we obtain individual loan-level information on approved and denied loans, including their timing and value. For approved loans, we have exact addresses that can be merged with block-level measures of FEMA-assessed property damage, flooding, and socio-demographic factors (e.g., median income and minority share). 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). The average approved loan amount was $74,549 and nearly 40 thousand loans were approved. Appendix Figure A6 shows the timing of these loans, the bulk of which were approved in October and November of 2017. Given the magnitude of SBA approved loans and variation in the type of people approved for loans, we expect they play a key role in explaining the heterogeneity in the recovery process according to initial financial condition that we document later in this paper.[24]

[23]For more information on SBA loans, see `https://fas.org/sgp/crs/homesec/R45238.pdf`.

[24]Appendix Table A1 tabulates the frequency of denial reasons given to us by the SBA. The two most frequent denial reason are for "Unsatisfactory credit history" and for "Lack of repayment ability." We should, therefore, expect to find inequalities in eligibility and approved loan sizes according to initial financial condition.

### 2.2.4 IRS disaster refunds

A more immediate form of assistance is to file an amended tax form with the IRS based on the loss of property incurred in the storm. This process is relatively quick and can lead to substantial payouts for individuals with higher incomes and, thus, greater tax liabilities. Losses have to exceed 10% of household income to be eligible for any refund from property loss.

### 2.2.5 Forbearance

Finally, a number of lenders – particularly those with links to the federal government – offer loan forbearance in federally designated disaster zones. Housing & Urban Development (HUD) issued a 90-day moratorium on foreclosures and forbearance on mortgage payments across the Houston area, with some lenders allowing for up to one year. Fannie Mae and Freddie Mac offered forbearance on mortgage payments for three-month intervals (up to 12 months).[25] Interest on mortgage balances continues to accrue during forbearance, which may offset some of the effect of home sales on mortgage balances in flooded areas. Federal student loan borrowers in designated disaster areas are given automatic 90-day forbearance.[26] There is no automatic 90-day forbearance on private student loans.

Since delinquencies are not reported to credit bureaus during forbearance, a simple time-series of delinquency rates on various debt types in credit bureau data offers a sense of the extent of forbearance. Appendix Figure A9 shows that forbearance was, indeed, given on mortgages and auto loans, at least until the last quarter of 2017. Evidence of forbearance on student loans is less clear and we observe no evidence of forbearance on consumer financing or credit cards.

# 3 Data and Variables

## 3.1 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. Additional detail on the precise maps used and the mapping process is available in Appendix C. To determine the developed portions of our study area, we incorporate high resolution (30m) geospatial data from the National Land Cover Database (NLCD).[27] This data is overlaid into Census 2010 TIGER files for Census Blocks in order to compute the portion of the

[25]No late fees are charged and no delinquencies are reported to the credit bureaus during forbearance. In some cases, lenders request immediate catch-up on payments after the forbearance period ends, but, in most cases, lenders allow a return to scheduled repayment and simply extended the maturity.

[26]Interest accrues during the administrative forbearance (but is not capitalized into the principal balance). Once the initial forbearance period related to the disaster is over, the borrower may request additional forbearance, in 30-day increments (not to exceed 12 months), for reasonable cause. For a description of "disaster relief administrative forbearance" from the student loan provider, Navient, see: https://www.navient.com/disaster-relief

[27]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%).

block that is developed. We also incorporate a couple engineering variables based on elevation and distance to hydrology (e.g. streams, rivers, lakes) in our study area.[28] In order to assign elevation to a specific census block, we average the elevation values within the block. Distance to waterways are calculated based on the distance of the centroid of a block to nearest waterway with a value of zero for blocks that contain waterways.

To determine floodplain status, we obtained data from The National Flood Hazard Layer (NFHL) which incorporates all Flood Insurance Rate Map (FIRM) databases published by the Federal Emergency Management Agency (FEMA). We create block level measures of 100 year (1% annual flood risk) floodplains based on a 2015 extract of the FEMA National Flood Hazard Layer. To explore whether household debt responses vary according to the degree to which the flooding was anticipated (insured), 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 ($\text{FlP} \geqslant 50\%$).[29]

We combine the spatial data on flood depth and developed land to generate our main independent variable: the weighted average flood depth across the developed area of a block (*WAvg. Flood Depth*, hereafter). Figure 2 provides an illustration of how we calculate our measure of flooding. 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 and undeveloped 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.[30]

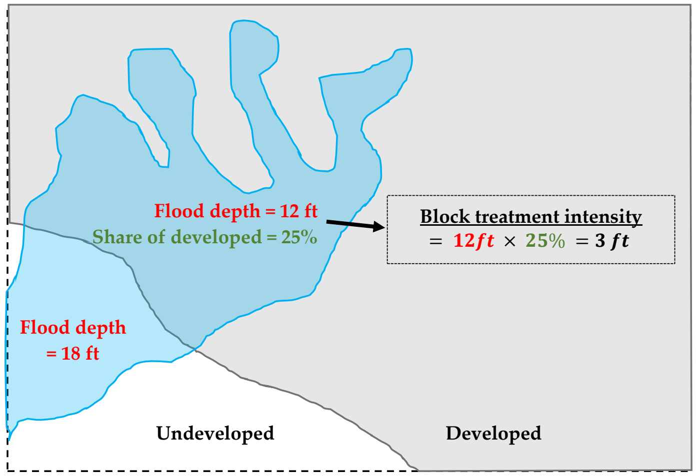
In assigning individuals to treatment, we must contend with measurement error. First, since our credit data only provides geographic information at the level of census block, we do not observe the exact degree of flooding experienced by individuals in our sample, rather we observe a proxy for their probability of having been flooded. This type of measurement error will attenuate estimates. Our estimates should be viewed as intent-to-treat (ITT) effects rather than average treatment on treated (ATT) effects. Thus, the effect of being flooded on debt outcomes may be considerably larger in magnitude than the effects identified in this paper and our results are thus conservative in magnitude. In order to get a sense of the

[28] Source: https://tnris.org/data-catalog/entry/national-elevation-dataset-ned-2013/ https://data.tnris.org/collection/af1ca25e-b38b-4203-90b8-d90f881963ae ; Elevation and hydrology are sourced from the US Geological Survey with data created using satellite imagery and lidar calculations.

[29] Note that, in very restrictive subsample specifications (e.g., conditioning the sample on being *Unconstrained*, having a mortgage, having an outstanding home equity loan as of Q2 2017, and simultaneously being outside the floodplain), we sometimes lack enough degrees of freedom in each flood intensity group to maintain this definition of floodplain status. In these cases, we use a binary inside vs. outside of floodplain split according to (a) whether or not at least 50% of the developed block area is in the floodplain or, if sample size is still insufficient, (b) whether or not *any* share of the developed block area is in the floodplain. Regardless of how one defines floodplain status, the maintained assumption is that individuals living in areas with lower floodplain coverage will necessarily have a lower likelihood of flood insurance relative to similar individuals from areas with higher floodplain coverage.

[30] Later analysis in Table 8 highlights a strong correlation between our measure of flood depth (*WAvg. Flood Depth*) and FEMA registration as well as financial assistance. Notably, the t-statistic on our measure of flooding, in Column 1, is nearly 10. Furthermore, Table 8 explores the two alternative measures of flooding that form our composite measure. These are *Avg. Flood Depth* and *Flooded Share of Developed Area.* Column 2 documents that both measures, independently, help explain FEMA registrations and assistance. Using only one of these two measures, instead of their composite, would, therefore, discard meaningful information.

Figure 2: Construction of flood intensity measure (*WAvg. Flood Depth*) within a census block




ATT relative to the ITT effect, in some specifications, we explore treatment effects in "extreme" blocks, defined as blocks with top decile flooding. Second, the proxy for flooding is, itself, imperfect. The exact depth and breadth of flooding merely represents FEMA's best guess based on hydrological modeling.

### 3.2 Consumer credit panel (CCP)

We use the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) for information on individual's credit characteristics [Lee and van der Klaauw, 2010]. The CCP consists of Equifax credit report data for a longitudinal quarterly panel of individuals. Our data spans Q2 2015 to Q4 2018. The panel is a nationally representative 5% random sample of all individuals with a Social Security number and a credit report.

The CCP data include a number of variables that can proxy for "overall financial stress," our primary outcome of interest in this paper. In particular, the data include indicators for either a chapter 7 or chapter 13 bankruptcy, measures of delinquency (30, 60, 90, 120 days past due, severely derogatory), and the Equifax Risk Score. We focus on severe delinquency – 90-days past due ("90dpd") for all debt types except for student debt, for which we use 120dpd due to the high frequency of student debt delinquencies. Also, 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 (%)*. The Equifax Risk Score is a trademarked measure of the likelihood that a consumer becomes seriously delinquent (90-plus days past due on debt). The score ranges from 280 to 850, with someone with a higher score being viewed as a better risk than someone with a lower score. In order to ensure proper measurement of pre-trends, we restrict the sample to only those individuals that had a score as of Q2 2015. In other words, we exclude from our sample individuals who are not scored due to lack of credit history.[31]

[31]We also use two variables which flag deceased consumers to filter out those records.

The CCP offers a variety of variables for several important consumer debt categories. For housing-related debt we focus on first mortgages and separately analyze a combination of installment and revolving home equity loans. Student loans include all loans used to finance education expenses; the loans are provided by banks, credit unions and other financial institutions, as well as by federal and state governments.[32] Note that student debt could not be examined using the CCP in the context of Hurricane Katrina in 2005 due to a classification change within the CCP in 2005 [Gallagher and Hartley, 2017]. Student debt in our 2015-2018 sample has a stable classification and, therefore, can be studied for the first time in the context of a natural disaster. Given the number of cars destroyed by Harvey, we also examine auto debt, which is the sum of all loans taken out to buy an automobile; these loans include those financed by a banking institution, as well as those financed by dealerships or auto financing companies. The consumer finance loan category includes sales financing (e.g., financing for large items like refrigerators) and personal loans. Additional variables used from the CCP include census block of residence, birth year of the consumer, the number of credit inquiries initiated by the consumer in the past 3 months, and the number of new accounts opened.

We create a "credit card debt" category that combines credit card debt balances for all revolving accounts at banks, bankcard companies, national credit card companies, credit unions, and savings and loan associations; along with, revolving retail loans for clothing, groceries, department stores, home furnishings, gas, etc. We focus our attention on credit card delinquencies rather than balances in this paper because (a) credit card utilization is an input into our index of financial constraint (as discussed in the next section), complicating the interpretation of differential treatment effects by constraint, and (b) the CCP does not provide information on the portion of outstanding credit card balances that are revolving. Hence, any change in credit card balances could merely reflect spending behavior and/or a movement from other forms of payment (e.g., checks) to credit cards.[33]

This data is joined, at the census block-level, with the geospatial flooding data described above. In the end, we have the quarterly credit information on 112,927 individuals living in 32,937 Census blocks (i.e., the level of treatment) in the Houston metropolitan area over Q2 2015 to Q2 2019.

## 4 Empirical Design

Our empirical strategy involves a treatment intensity difference-in-difference (DiD) design of the form:

$$y_{ibt} = \beta\left(T_b \times P_t\right) + \alpha_i + D_t + \kappa f(A_{it}) + \eta\left(C_b \times P_t\right) + \phi C_b + \varepsilon_{ibt} \tag{1}$$

[32] We use loan-level disaggregated student loan data for each consumer (up to a maximum of 20 loans per consumer, per quarter) and aggregate after the fact as opposed to importing aggregated data from the main CCP dataset. This offers a more accurate accounting because the student loan data in the main dataset undercounts a specific category of student loans. However, using this approach obscures some information for individuals with greater than 20 student loan accounts. The incidence of these individuals is lower than that of the undercounting within the main dataset.

[33] For information on how the hurricane affected credit card usage, see del Valle et al. [2019]. These authors use monthly loan-level credit card data from CCAR Y-14M data to better isolate the effect of Hurricane Harvey on various aspects of credit card usage. They find little association between flooding and revolving balances for the average account. However, they find large increases in new card originations, particularly on "teaser" cards with short-term promotional rates. These authors do not explore the role of financial constraint nor do they evaluate other forms of debt outcomes.

where $y_{ibt}$ is a quarterly credit outcome for individual $i$ living in Census 2010 Block $b$ in year by quarter $t$.[34] $T_b$ is the treatment intensity, *WAvg. Flood Depth*, defined as in Section 3.1. An individual is assigned to a treatment intensity according to the block where that individual lived in as of Q2 2017 (the last quarterly observation before the hurricane). We, therefore, 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. The treatment variable, *WAvg. Flood Depth*, is included in regressions as a binned variable: (0) a no flood group (control); (1) the least flooded tercile of the flooded blocks ($T_b^1$); (2) the middle flooded tercile of the flooded blocks ($T_b^2$); (3) the most flooded tercile of the flooded blocks ($T_b^3$). As we will see, it appears that the response to flooding is non-linear according to our treatment variable, with the most flooded tercile ($T_b^3$) tending to display disproportionately larger debt responses relative to less flooded terciles. $P_t$ is the post-hurricane dummy, which gets the value of one during all periods after Q2 2017.[35] The coefficient of interest is $\beta$, which 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.

Additionally, $\alpha_i$ is an individual fixed effect and $D_t$ is a year-quarter fixed effect. Because an individual's treatment intensity is time-invariant, we cannot include a block-fixed effect as it would be collinear with both $T_b$ and $\alpha_i$. As age varies with time, we include a quadratic polynomial of the individual's age in each quarter, $f(A_{i,t})$. Finally, with $C_b$, we control for several characteristics of the census block where the individual lived as of Q2 2017 (median income, owner-occupied share, minority share, median home value, and floodplain share of developed block area). These controls are interacted with a post-period dummy such that, $C_b \times P_t$ 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 $C_b$ 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 at the dawn of the hurricane.

We employ two variations of the above model. First, we run event studies of the form shown in Equation 2, plotting the $\beta_\tau$ 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 trends pre-hurricane. Thus, the $\beta_\tau$ 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).

[34] Since we evaluate debt balances in dollars, we must adjust for the influence of outliers. Indeed, most debt types have outlying observations of well-over $1 million. A fast paydown of extremely large balances after the storm, for example, could skew our estimates. Therefore, we winsorize all non-zero balances at the 97th percentile by quarter. Depending on the debt type, this is roughly equivalent to a 99th percentile winsorization overall. To provide a sense of relative scale of each regression estimate, the mean of the dependent variable is shown at the bottom of each regression table.

[35] Note that the hurricane hit during Q3 2017, with the Q3 2017 snapshot occurring less than a month after the storm passed. Appendix Figure A4 (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.

$$y_{ibt} = \sum_{\tau=-8}^{8} \beta_\tau \left(T_b \times D_\tau\right) + \alpha_i + D_\tau + \kappa f(A_{it}) + \eta \left(C_b \times P_\tau\right) + \varepsilon_{ibt} \tag{2}$$

Two outcome variables (*Out-migration* and *Bankruptcy*) are better suited to analysis at the block-level (i.e., the level of treatment). 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. So, instead, we evaluate migration rates across Houston-only blocks according to the block's intensity of treatment. At the block-level, the variable *Out-migration* 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. We also evaluate *Bankruptcy* at the block-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.[36] Instead, we calculate the share of each block's residents that have a bankruptcy flag on their credit reports, then we evaluate how that share changes over time according to treatment.

In these two block-level regressions, we replace the individual fixed effects in Equation 1 with block fixed effects, $\alpha_b$, 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 size may improve precision. As before, standard errors are clustered on block.

Key to our study is an exploration of heterogeneity in treatment effects according to initial financial constraint ("*Constrained*"). There is no standard way of measuring financial constraint in household finance. Following Gallagher et al. [2019], we classify individuals according to degree of financial constraint through an index constructed using a principal component analysis (PCA) that combines a set of standardized continuous variables that proxy for financial stress. The variables include the individual's *Equifax Risk Score*; the individual's *Credit Card Utilization*; as well as the *Median Income* and *Minority Share* of the block where the individual lived as of Q2 2017.[37] The first two variables are intended to measure the individual's access to credit whereas the latter two variables are intended to capture access to outside resources.[38] As documented in Table A2, the first principal component explains 44% of the variation in these variables and the loadings have expected signs. Using only the first component, we employ a discrete measure of this index, dividing it into above- an below-median levels of constraint ($\mathsf{Unconstrained}$ and $\mathsf{Constrained}$). For block-level analysis, we split blocks at the median based on share of individual residents classified as $\mathsf{Constrained}$.

Finally, we develop a proxy for the amount of equity an individual has in her home based on the date

[36]As King and Zeng [2001] explain, statistical procedures, such as logistic regression, can sharply underestimate the probability of rare events. Nonetheless, Appendix Figure A7 shows bankruptcy results at the individual level. Patterns are similar albeit estimates are smaller in magnitude and noisier.

[37]The credit card utilization rate is estimated as the sum of balances across bankcard and retail charge 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 27% of individuals who do not have a credit card, we give them the value of the standardized mean (zero), effectively giving the utilization loading a weight of zero.

[38]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%.

in which she opened her most recent first mortgage account. Due to compounding interest, home equity grows exponentially with time. Moreover, the Houston area saw a boom in a house prices over the 5 years pre-Harvey.[39] As such, we classify individuals that opened a first mortgage more than 5 years ago as having *high equity*. All other individuals with a mortgage open date are classified as *low equity*.

# 5 Results

This section presents the main results, beginning with an analysis of the effect of flood exposure on bankruptcy and delinquency rates and, then, exploring the effect on debt balances. Before evaluating these credit outcomes, it is helpful to first establish that there is no evidence of a retraction in the supply of credit in Houston after the hurricane. We validate this assumption in Appendix B.

## 5.1 Bankruptcy

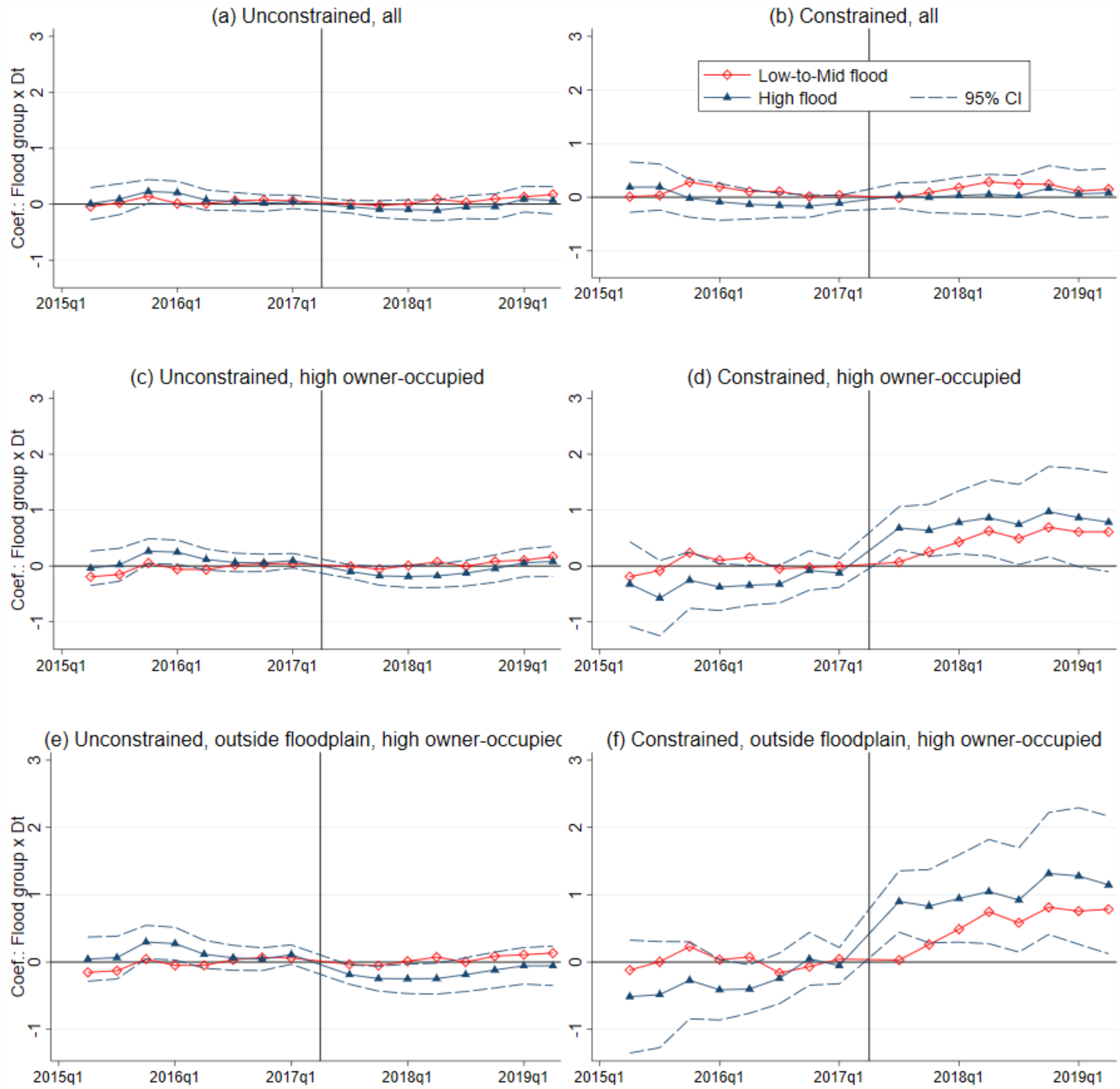
One of our main outcomes of interest in this paper is bankruptcy. In Presidential Disaster Declaration areas, some types of creditors offer temporary forbearance to all local borrowers (i.e., a pause in loan payments, see Section 2.2.5). Forbearance necessarily means that the effect of flooding on delinquency indicators will be muted. However, to the extent that forbearance is only temporary and flooding pushes borrowers on the cusp of bankruptcy into bankruptcy, we would expect to observe an immediate and lasting jump in the share of a block's residents with a bankruptcy flag on their account if flooding increases financial stress.

Figure 3 plots the event study coefficients from Equation 2 at the block-level. The coefficients on the "High flood" group capture the differential effect of being in the top tercile of flooded blocks relative to blocks that did not flood and relative to the last quarterly observation before the hurricane (Q2 2017). To explore monotonicity, we also plot the average coefficient on the bottom and middle terciles of flooded blocks ("Low-to-Mid flood"). For visual ease, we plot only the confidence interval around the "High flood" coefficients.

In Panels (a) and (b), the sample is split according to whether the block has an above (*Constrained*) or below (*Unconstrained*) median share of residents classified as "constrained" as of Q2 2017. Consistent with prior research suggesting a limited effect of natural disasters on financial distress, we observe no overall upward trend in the bankruptcy rate in the average Houston block. This result, however, masks substantial heterogeneity across block types. When we further condition the sample on blocks with an above-median share of owner-occupied housing, differential effects by initial financial constraint become very apparent. Panel (d) shows a stark increase in the bankruptcy rate in constrained blocks with substantial owner-occupied housing. The magnitude of this effect, which peaks at 1 percentage point, is large relative to the

[39] Houston home prices grew by 45% from Q2 2012 to Q2 2017, according to FRED data. Of course, a portion of this appreciation would likely be lost for houses affected by Hurricane Harvey, particularly for those outside the floodplain. According to Dixon et al. [2013], after flooding a rational buyer would offer a price that is reduced by the discounted sum of an infinite series of insurance payments into the future. Assuming that flood insurance costs $4,000 per year for a house that recently flooded, discounted at 5%, the home price discount would be $60,000, or 43% of the median Houston home price − thus, erasing all appreciation over the prior 5-years but not necessarily putting a homeowner under water on their mortgage.

Figure 3: Difference-in-difference event study estimates of *Bankruptcy*, block-level




Figures plot event study coefficients from DiD regressions using the block-level panel over Q2 2015–Q2 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. The sample is split according to whether the block has an above (*Constrained*) or below (*Unconstrained*) median share of residents classified as "constrained" according to 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. For visual ease, the low (T1) and mid (T2) terciles are combined in the regressions (red, diamond line). Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. All regressions include the full array of fixed effects and controls described in Section 4. Regressions are weighted by the number of observations within each census block in the CCP data, effectively giving more weight to more precise observations. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

average pre-Harvey block bankruptcy rate in *Constrained*, high owner-occupied areas (3.7%). In contrast, in unconstrained blocks (Panel c), the bankruptcy rate edges downward slightly.

Next, Panels (e) and (f) further restrict the sample to blocks located outside of the floodplain (i.e., where residents may have been less insured against flooding). These blocks see an even larger relative increase in their bankruptcy rate.[40] The effect peaks at about 1.3 percentage points and persists throughout the post period. To understand how this large of an effect could wash out in aggregate, see Appendix Figure A5. Among the *Constrained* from either low-owner occupied areas or areas inside the floodplain, we see a downward trend in block bankruptcy rates in the most flooded areas relative to areas that did not flood. A small downward trend can also be observed among the *Unconstrained* from high-owner occupied areas in Figure 3, Panels (c) and (e).

Table 4 tests the statistical significance of the block's residents being initially *Constrained* through an interaction term with the treatment effect in the DiD regression model in Equation 1. Column 1 indicates that there is no average effect on bankruptcy of being in the most flooded tercile ($T_b^3$) relative to the no-flood group after the storm. This is true even among *Constrained* blocks (Column 2). However, consistent with the event study plots, Column 5 shows that in high owner-occupied areas outside of the floodplain, the bankruptcy rate is 1.48 percentage points higher in *Constrained* blocks with heavy flooding. Summing the two coefficients on the *High flood* tercile ($T_b^3$) and comparing this to the average bankruptcy rate within the subsample of *Constrained,* high owner-occupied blocks outside of the floodplain (3.7%), we find a 1.1 percentage point or 30% relative increase in the bankruptcy rate after the hurricane in heavily flooded areas when the area is also *Constrained.* Column 6 indicates that similar blocks inside the floodplain (where insurance was more prevalent) do not see a significant change in bankruptcy rates, even when the block's residents are financially constrained.

In sum, the results in this section point to a large and persistant (30%) relative increase in the bankruptcy rate in areas of Houston that unexpectedly flooded and where homeowners entered the storm in a worse financial position. Interestingly, the bankruptcy rate does not rise (relative to no-flood blocks) and, in some cases, even decreases slightly, in areas with more renters, where the flooding was expected, and/or where homeowners are more financially secure. Overall, these results offer a first indication of the importance of considering underlying heterogeneity in treatment effects following natural disasters by financial constrain and floodplain status. It must be emphasized, however, that our results must be interpreted as relative effects, not absolute effects, of treatment on the bankruptcy rate. The average bankruptcy rate, even in treated areas, continued its downard trend after Harvey (see Appendix Figure A8), consistent with an improving local and national economy.

[40] Bankruptcy is a rare event in any given quarter for any given individual, making it difficult to model at the individual-level. Nonetheless, Appendix Figure A7 plots the effect of flooding on a binary indicator of bankruptcy at the individual-level using a linear probability DiD model. The same patterns, albeit noisier, can be observed.

Table 4: Difference-in-difference estimates of *Bankruptcy*, block-level

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | 0.09 | 0.13 | 0.17 | 0.01 | 0.02 | 0.21 |
| | (1.03) | (1.25) | (1.42) | (0.12) | (0.17) | (0.46) |
| $T_b^2 \times P_t$ | -0.05 | -0.01 | 0.04 | -0.09 | -0.03 | 0.38 |
| | (-0.57) | (-0.13) | (0.35) | (-0.56) | (-0.18) | (0.86) |
| $T_b^3 \times P_t$ | -0.02 | 0.01 | -0.01 | -0.35 | -0.37 | 0.07 |
| | (-0.18) | (0.14) | (-0.06) | (-1.63) | (-1.46) | (0.17) |
| $T_b^1 \times P_t \times \mathrm{Constrained}_b$ | | -0.10 | 0.13 | 0.27 | 0.62** | -0.88 |
| | | (-0.57) | (0.50) | (1.35) | (2.17) | (-1.00) |
| $T_b^2 \times P_t \times \mathrm{Constrained}_b$ | | -0.09 | 0.11 | 0.27 | 0.15 | 0.01 |
| | | (-0.49) | (0.36) | (1.04) | (0.38) | (0.02) |
| $T_b^3 \times P_t \times \mathrm{Constrained}_b$ | | -0.07 | 0.66** | 0.60* | 1.48*** | 0.08 |
| | | (-0.37) | (2.28) | (1.76) | (2.97) | (0.10) |
| N | 518,428 | 518,428 | 289,883 | 388,372 | 215,824 | 25,283 |
| AdjR2 | 0.74 | 0.74 | 0.75 | 0.75 | 0.76 | 0.73 |
| Y-mean | 2.25 | 2.25 | 2.32 | 2.31 | 2.44 | 1.79 |
| *Sample* | *All* | *All* | *Own⩾p50* | *FlP=0%* | *FlP=0% & Own⩾p50* | $\mathrm{FlP} \geqslant 50\%$ *& Own⩾p50* |

Table presents DiD estimates using the block-level panel over Q2 2015–Q2 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 tercile bins of *WAvg. Flood Depth* in the post period. The coefficient on $T_b^3 \times P_t$, for example, can be interpreted as the effect of top tercile flooding intensity on a block's post-hurricane bankruptcy rate relative to its pre-period rate and relative to the post-hurricane bankruptcy rate of blocks that did not flood. In some specifications, treatment is interacted with a dummy indicating that the block has an above-median share that entered the hurricane in a state of financial constraint (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). Columns 3–5 further restrict the sample to blocks outside of the floodplain (*FlP=0%*) while Column 6 restricts the sample to blocks where the floodplain covers at least 50% of the developed land. All regressions include the full array of fixed effects and controls described in Section 4. Regressions are weighted by the number of observations within each census block in the CCP data, effectively giving more weight to more precise observations. 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.

## 5.2 Severe delinquencies and credit scores

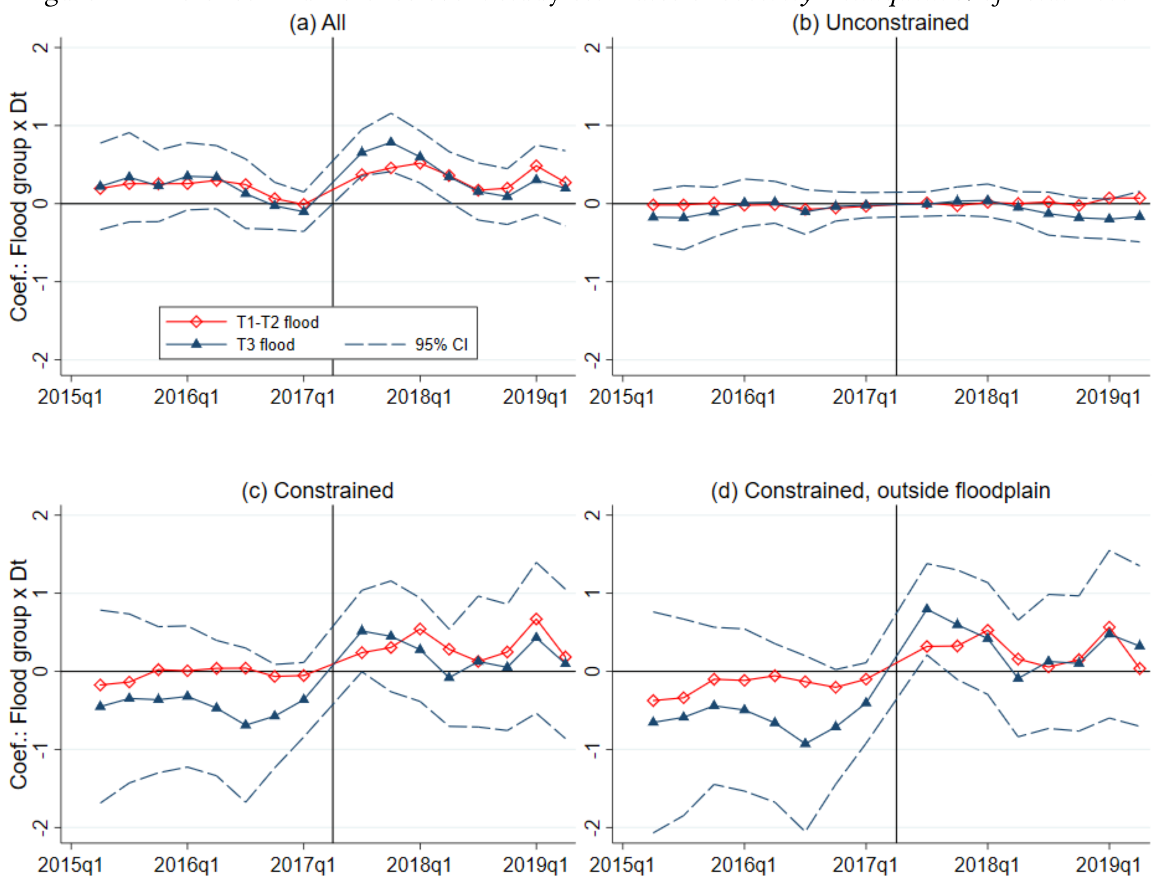
In this section, we evaluate the effect of flooding on delinquencies and credit scores. In both cases, the predicted effect of flooding is ambiguous due to the provision of (a) temporary forbearance by some lenders, meaning that new delinquencies are not reported to credit bureaus for a period of time after the hurricane, and (b) disaster assistance, which could be used to absorb expenses as well as to restructure debt (see Section 2.2). Some forms of debt have a greater likelihood of forbearance.[41] Therefore, to simplify matters,

[41]This fact is apparent in Appendix Figure A9, which plots the rate of severe delinquency by block flooding intensity and by date. The figure shows that many new delinquencies on mortgage and auto debt were likely not reported until after Q4 2017. Meanwhile, we see no evidence of a drop of in reported delinquencies for credit card and consumer financing debt. With student debt the extent of forbearance is ambiguous. In untabulated results, we observe no change in the delinquency rate on mortgage debt due to flooding; however, there is evidence of a relative increase in the delinquency rate on outstanding auto, student, and, especially, consumer financing debt among *Constrained* borrowers. For *Unconstrained* borrowers, delinquency rates on these same forms of debt either decline or stay constant.

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.

Figure 4 presents the event study coefficients. Among all individuals in our sample of Houston residents during Hurricane Harvey, there is a temporary (lasting four quarters post-Harvey) spike in the share of debt that is severely delinquent in the three-quarters post-hurricane (Panel a). This result is constistent with the findings from Hurricane Katrina, where delinquency effects were found to be mild and temporary. We observe no such spike among individuals classified as *Unconstrained* (Panel b). Although noisy, the effects for individuals classified as *Constrained* (Panel c) mirror those of bankruptcy in that, there is a spike in delinquency for these individuals after the storm, particularly for those located outside of the floodplain (Panel d).

Figure 4: Difference-in-difference event study estimates of *Severely Delinquent % of Total Debt*




Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. In Panels (b) and (c), the sample is split according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017. In Panel (d), the sample is further restricted to blocks with no floodplain coverage. Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. For visual ease, in both panels, the low (T1) and mid (T2) terciles are combined into one bin. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

We formalize these results in Table 5 by running DiD regressions of the type in Equation 1. Column 1 shows a significant but non-monotonic effect of treatment on delinquent debt. Interacting treatment with an indicator of individual constraint in Column 2, we observe a monotonically increasing relationship between flooding intensity and the individual's delinquency share. The coefficient estimate on $T_b^3 \times P_t \times \mathit{Constrained}_i$ of 0.79 equates to a 4.4% relative increase in the average pre-Harvey share of debt that is delinquent for the *Constrained* sample (17.73). Estimates are only slightly larger when the post-period is restricted to just the first four quarters after the hurricane (Column 3), indicating a degree of persistence in the effect of flooding on the delinquent debt share of *Constrained* individuals.

Since we do not observe the exact degree of flooding experienced by individuals in our sample (rather we only observe the degree of flooding in their Census block), the effect of being flooded on debt outcomes may be considerably larger in magnitude than the ITT effects identified thus far. To bring us closer to an ATT effect, Columns 4–6 restrict the sample to include only no-flood blocks and blocks in the top decile of flood depth among flooded blocks. Put differently, we hone in on the treatment effect by comparing blocks that did not flood to blocks where the likelihood of homes flooding is very high. When we do this, we observe a clear separation according to financial constraint (Column 4) that is actually more dramatic over the eight quarters post-Harvey than over the four quarters post-Harvey (Column 5). Whereas treatment is associated with a 0.78 percentage point (or relative 44%) *reduction* in the delinquency share among *Unconstrained* individuals, it is associated with a 2.70 percentage point (or relative 15%) *increase* in the delinquency share among *Constrained* individuals. As expected, the delinquency effect of extreme flooding expands when the sample is conditioned on mortgage-holders living outside the floodplain (Column 6). These effect sizes are large and offer further indication of the unequal effects of flooding on individuals according to initial financial condition. Moreover, they point to the possibility that financially well-off households may benefit in some ways from these storms.

Returning to the full sample, Columns 7–11 investigate other sources of heterogeneity in treatment effects on delinquency. The results suggest that, within the pool of mortgage holders, those with little equity in their homes (having opened a mortgage within the 5-years proceeding the storm) see a 1.49 (=1.38+0.11) percentage point increase in their delinquency share when they living in the top-tercile of flooded areas and are *Constrained*. This effect size is large; it represents 35% of the pre-Harvey delinquency rate of all *Constrained* mortgage-holders (4.3%). Columns 10 and 11 suggest that being outside the floodplain also has an amplifying effect on delinquency.

Next, in Table 6, we investigate the effect of flooding on *Equifax Risk Score*s during the first four and eight quarters post-Harvey, seperately. Note that credit scores rose across the Houston area after Hurricane Harvey, perhaps due to broad-brush forbearance (provided to all borrowers in the disaster zone irrespective of flooding).[42] However, the evidence in Table 6, Column 1, suggests that *Equifax Risk Scores* rose marginally *less* (-0.91 points) for individuals in the top-tercile of flooded areas relative to individuals in no-flood areas in the first four quarters after the storm. Columns 2 and 4 indicate that the effect of top-tercile flooding on credit scores is temporary (i.e., does not endure for a full eight quarters post-Harvey) for the average individual in our sample but is persistent for mortgage-holders.

[42]For example the average pre-period credit score was 681 versus 688 in the post-period (see also Appendix Figure A10).

Table 5: Difference-in-difference estimates of *Severely Delinquent % of Total Debt*, individual-level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | -0.08 | -0.09 | -0.06 | | | | 0.11 | -0.01 | -0.11 | 0.05 | -0.28 |
| | (-0.41) | (-0.81) | (-0.85) | | | | (1.30) | (-0.04) | (-0.99) | (0.42) | (-1.38) |
| $T_b^2 \times P_t$ | 0.45** | 0.14 | 0.15 | | | | 0.09 | -0.07 | 0.15 | 0.17 | -0.50 |
| | (2.54) | (1.11) | (1.51) | | | | (0.63) | (-0.36) | (0.88) | (0.95) | (-1.33) |
| $T_b^3 \times P_t$ | 0.23* | -0.01 | 0.12 | -0.78*** | -0.46** | -0.46*** | 0.11 | 0.04 | 0.07 | -0.15 | -0.30 |
| | (1.73) | (-0.09) | (1.29) | (-3.27) | (-2.22) | (-4.06) | (1.10) | (0.19) | (0.40) | (-0.64) | (-1.44) |
| $T_b^1 \times P_t \times \text{Constrained}_i$ | | 0.04 | 0.09 | | | | 0.56 | 0.68 | 0.19 | 0.59 | 2.81 |
| | | (0.11) | (0.29) | | | | (1.07) | (1.59) | (0.41) | (1.06) | (1.41) |
| $T_b^2 \times P_t \times \text{Constrained}_i$ | | 0.68** | 0.63* | | | | 0.96* | 0.61 | 0.70* | 1.12* | 1.54 |
| | | (2.23) | (1.82) | | | | (1.98) | (1.03) | (1.87) | (1.90) | (1.68) |
| $T_b^3 \times P_t \times \text{Constrained}_i$ | | 0.79** | 0.85** | 2.70*** | 1.92*** | -5.57** | 1.38*** | -0.36 | 2.91*** | 1.75** | 0.84 |
| | | (2.28) | (2.67) | (4.26) | (3.57) | (-2.50) | (2.91) | (-0.64) | (3.07) | (2.16) | (0.71) |
| N | 1,605,162 | 1,605,162 | 1141739 | 973,905 | 688946 | 244465 | 297,002 | 288,532 | 1,070,314 | 191,942 | 20,828 |
| AdjR2 | 0.68 | 0.68 | 0.72 | 0.68 | 0.72 | 0.39 | 0.43 | 0.55 | 0.69 | 0.42 | 0.32 |
| Y-mean | 9.89 | 9.89 | 9.44 | 10.52 | 10.06 | 2.00 | 1.21 | 4.91 | 10.23 | 1.19 | 0.85 |
| Last quarter | *Q2 2019* | *Q2 2019* | *Q2 2018* | *Q2 2019* | *Q2 2018* | *Q2 2019* | *Q2 2019* | *Q2 2019* | *Q2 2019* | *Q2 2019* | *Q2 2019* |
| Sample | *All* | *All* | *All* | *Extremes* | *Extremes* | *Extremes & Mtg>0 & FlP=0%* | *LoEquity* | *HiEquity* | *FlP=0%* | *LoEquity & FlP=0%* | *LoEquity & FlP⩾50%* |

Table presents DiD estimates using the individual-level panel over Q2 2015–Q2 2019. In specified regressions, the post period is shortened such that the data span Q2 2015–Q2 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. Treatment intensity is defined according to tercile bins of *WAvg. Flood Depth* in the post period. In some specifications, treatment is interacted with a dummy indicating that the individual had an above-median index level of "Constraint" as of Q2 2017 (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). To provide a rough sense of the treatment-on-treated effect, Columns 3 and 4 restrict the sample to only blocks that either had no flooding or were in the top decile of flooded blocks. Columns 5 and 6 restrict the sample to mortgage holders who have had their mortgage for less than (*LoEquity*) or more (*HiEquity*) than 5 years. The three two columns further restrict the sample to individuals from blocks outside of the floodplain (*FlP=0%*) or inside the floodplain (*FlP⩾50%*). All regressions include the full array of fixed effects and controls described in Section 4. 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.

Columns 1–4 also indicate that constraint plays no mediating role in explaining the effect of flooding on credit scores. Note, however, that credit scores take into account, not just delinquencies, but factors such as the number of open accounts, loan balances relative to original loan amounts, and credit card utilization. Indeed, del Valle et al. [2019] find large increases in new card originations and balances, particularly on "teaser" cards and among borrowers with higher credit scores and incomes (i.e., the *Unconstrained*). A proliferation of new credit card accounts could lower the credit scores of *Unconstrained* borrowers after flooding and explain the why *Constraint* does not differentially affect credit scores in the full sample.

It is also possible that our estimates are simply attenuated. Recall that credit scores are one of the inputs into the PCA that defines constraint. To the extent that constrained individuals already have low scores due to delinquent accounts, a small average increase in delinquency might have limited effect on average credit scores. To address the issue of attenuated estimates, the remainder of the table restricts the sample to include only no-flood blocks and blocks in the top decile of flood depth among flooded blocks. The goal is to provide a rough sense of the ATT effect on credit scores. Consistent with the results for delinquency, we now observe a clear separation in treatment effects according to constraint. Most notably, estimates in Column 7 indicate that mortgage-holders living in a block with top decile flooding located outside the floodplain experience a 24 (=20.06+3.56) point relative *decrease* in their credit scores when they entered the storm *Constrained*. This effect size is substantial. To put it in context, Dobbie et al. [2017] estimate that having a bankruptcy flag removed from one's credit report results in a 14 point (positive) change in credit scores.[43] Consistent with results for bankruptcy and delinquency, estimates in Column 8 suggest that the effect on credit scores does not translate to mortgage-holders living inside the floodplain.

In summary, despite a degree of forbearance offered to consumers, people in the top tercile of flooded blocks are relatively more likely to fall delinquent. The increase in delinquency comes primarily from treated individuals who entered the storm in a state of financial constraint and is amplified by being a mortgage holder living outside of the floodplain. Our results also point to large effects on credit scores in the top decile of flooded blocks for *Constrained* mortgage-holders living outside the floodplain. Overall, these results suggest that the length of forbearance and the uniformity of its generosity across lenders and debt types is insufficient to fully negate the effect of flooding on the delinquency rates of *Constrained* individuals after the disaster. Meanwhile, living inside a floodplain (i.e., having flood insurance) *or* having a better initial financial condition both appear to insulate a person from the negative credit consequences of flooding.

[43]For further context, Brevoort et al. [2018] document a 0.5 point (positive) intent-to-treat effect on state-wide credit scores in the first year of Medicaid expansion under the Affordable Care Act. For lower-income communities, the effect is 1.04 points. In other words, experiencing heavy flooding in one's census block due to a hurricane has about the same absolute reduced form effect on credit cores as gaining access to subsidized health insurance.

Table 6: Difference-in-difference estimates of *Equifax Risk Score*, individual-level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | -0.21 | -0.38 | 0.25 | -0.16 | | | | |
| | (-0.63) | (-0.94) | (0.44) | (-0.22) | | | | |
| $T_b^2 \times P_t$ | -0.16 | -0.03 | -0.84 | -0.78 | | | | |
| | (-0.34) | (-0.06) | (-1.31) | (-1.14) | | | | |
| $T_b^3 \times P_t$ | -0.94** | -0.56 | -1.45** | -1.50** | -0.45 | -0.39 | -3.56 | -1.50 |
| | (-2.37) | (-1.48) | (-2.61) | (-2.24) | (-0.67) | (-0.36) | (-0.64) | (-0.57) |
| $T_b^1 \times P_t \times Constrained_i$ | 0.25 | 1.23 | -1.30 | -0.50 | | | | |
| | (0.29) | (1.48) | (-0.85) | (-0.38) | | | | |
| $T_b^2 \times P_t \times Constrained_i$ | 0.23 | 0.14 | 1.03 | 1.09 | | | | |
| | (0.36) | (0.21) | (0.95) | (0.90) | | | | |
| $T_b^3 \times P_t \times Constrained_i$ | -0.04 | 0.37 | 0.03 | 1.41 | -1.54 | -5.37** | -20.06*** | 5.43 |
| | (-0.05) | (0.53) | (0.02) | (0.78) | (-1.17) | (-2.68) | (-3.34) | (0.74) |
| N | 1,253,347 | 1,766,849 | 394,904 | 557,428 | 1073788 | 319,226 | 246,483 | 15,130 |
| AdjR2 | 0.91 | 0.89 | 0.90 | 0.87 | 0.89 | 0.87 | 0.87 | 0.87 |
| Y-mean | 684.52 | 686.49 | 723.30 | 725.32 | 681.86 | 722.23 | 721.44 | 727.80 |
| Last quarter | *Q2 2018* | *Q2 2019* | *Q2 2018* | *Q2 2019* | *Q2 2019* | *Q2 2019* | *Q2 2019* | *Q2 2019* |
| Sample | *All* | *All* | *Mtg>0* | *Mtg>0* | *Extremes* | *Extremes & Mtg>0* | *Extremes & Mtg>0 & FlP=0%* | *Extremes & Mtg>0 & FlP⩾50%* |

Table presents DiD estimates using the individual-level panel over Q2 2015–Q2 2019. In specified regressions, the post period is shortened such that the data span Q2 2015–Q2 2018. The dependent variable is the individual's *Equifax Risk Score*. Treatment intensity is defined according to tercile bins of *WAvg. Flood Depth* in the post period. In some specifications, treatment is interacted with a dummy indicating that the individual had an above-median index level of "Constraint" as of Q2 2017 (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). To provide a rough sense of the treatment-on-treated effect, Columns 3–9 restrict the sample to only blocks that either had no flooding or were in the top decile of flooded blocks. Columns 5–9 further restrict the sample to mortgage holders. Columns 6–9 restrict the sample to individuals from blocks outside of the floodplain (*FlP=0%*) or from blocks where the floodplain covers at least 50% of the developed land ($FlP \geqslant 50\%$). Finally, Columns 8 and 9 restrict the post period to the first 3 quarters after the hurricane ($D_\tau$<Q12018). All regressions include the full array of fixed effects and controls described in Section 4. 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.

## 5.3 The intensive and extensive margin of debt balances

In this section, we explore how local flooding affected balance size (intensive margin) and the probability of having a balance (extensive margin). Although high debt balances do not necessarily signal financial distress, changes in balances are informative on how consumers allocate their resources in response to flooding. We evaluate mortgage, home equity, student, auto, and consumer financing debt balances.[44] For brevity, we focus our attention on the treatment effects of being in the top tercile of flooded blocks ($T_b^3$) going forward.

[44]Note that we do not examine credit card debt balances in this paper for the reasons discussed in Section 3.2.

#### 5.3.1 Mortgage and home equity loan debt balances

In this section, we evaluate changes in mortgage loans and home equity loans following Hurricane Harvey. It is important to first note that Houston, in 2017, had a strong housing market. By comparison, the New Orleans housing market at the time of Hurricane Katrina was in an extended period of downturn – with home values below the replacement cost of new construction [Vigdor, 2008]. Indeed, Gallagher and Hartley [2017] present evidence that lenders encouraged Katrina victims to use their flood insurance payouts to pay-down their mortgages rather than rebuild. This was possible in New Orleans, given that over 90% of the flooding under Katrina occurred in the floodplain and flood insurance was much more common there than under Harvey. Given these stark differences in local conditions, we should expect different effects from Hurricane Harvey.

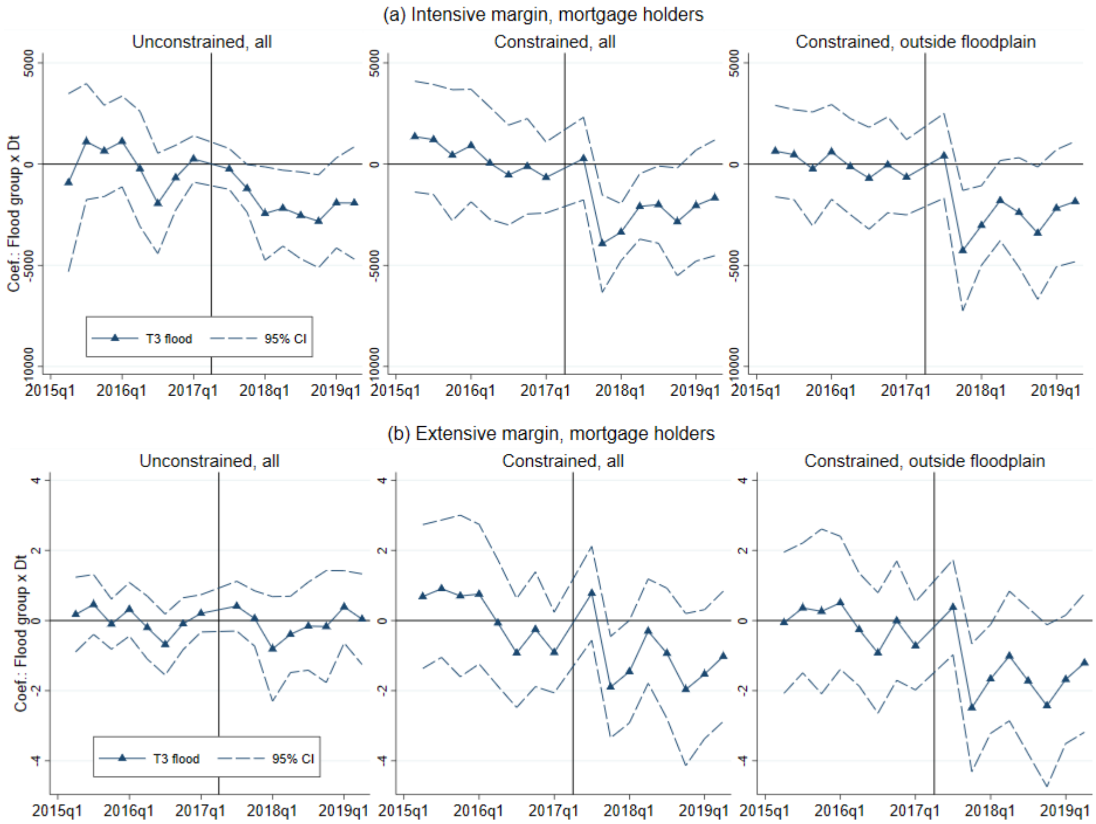
Figure 5 presents intensive and extensive margin effects of flooding on mortgage debt, where the sample is split according to financial constraint and floodplain status. Panel (a) offers clear evidence of a reduction in mortgage debt balances across all types of mortgage borrowers. The effect is larger in magnitude, however, for *Constrained* borrowers outside of the floodplain. For these individuals, mortgage balances decline by approximately $3,000, on average, after the hurricane. There is also a corresponding extensive margin effect in Q4 2017 (Panel b), indicating that *Constrained* households were more likely than *Unconstrained* households to eliminate their mortgage – i.e., by selling their property and/or by using federal disaster loans and assistance to restructure their debt. We do not observe a clear extensive margin effect inside the floodplain or among *Unconstrained* households outside the floodplain (not shown).

Next, we study home equity loans – meaning home equity installment loans (second mortgages) and revolving lines of credit (HELOCs). We focus our attention on mortgage-holders who did not have a home equity loan at the dawn of the hurricane. Event study coefficients, in Figure 6, indicate that these loans become significantly more popular among flooded residents *inside* the floodplain after the storm. This effect is once again confined to *Constrained* individuals (Panel f). The effect size is large: a roughly 3 percentage point increase in take up of home equity loans equates to a 41% relative increase in the pre-Harvey use of these loans among *Constrained* mortgage-holders inside the floodplain. Outside the floodplain, *Unconstrained* households actually appear to reduce their use of home equity loans by about 1 percentage point at the end of the post period.

One possibile explanation is that home equity loans serve as a substitute to SBA disaster loans. As we will later document, these loans are more difficult for *Constrained* borrowers to access due to their strict income and credit requirements. As such, *Constrained* borrowers inside the floodplain may be using home equity loans to top-off flood insurance as they repair their homes. Moreover, the risk of flooding may have already been capitalized into the house prices inside the floodplain (MacDonald, 1990, Zhang, 2016), preserving borrower's equity after flooding such that it can be drawn down through home equity loans.

In summary, the mortgage debt response of Harvey victims in Houston differs from the response of Katrina victims in New Orleans as outlined in Gallagher and Hartley [2017]. Unlike in New Orleans, we see little evidence of mortgage eliminations in parts of Houston where residents were more likely to have flood insurance. Instead, we find that mortgage debt was primarily being eliminated by *Constrained* households outside of the floodplain. One interpretation is that flooded homeowners inside the floodplain

Figure 5: Difference-in-difference event study estimates of *Mortgage Loans*, individual-level

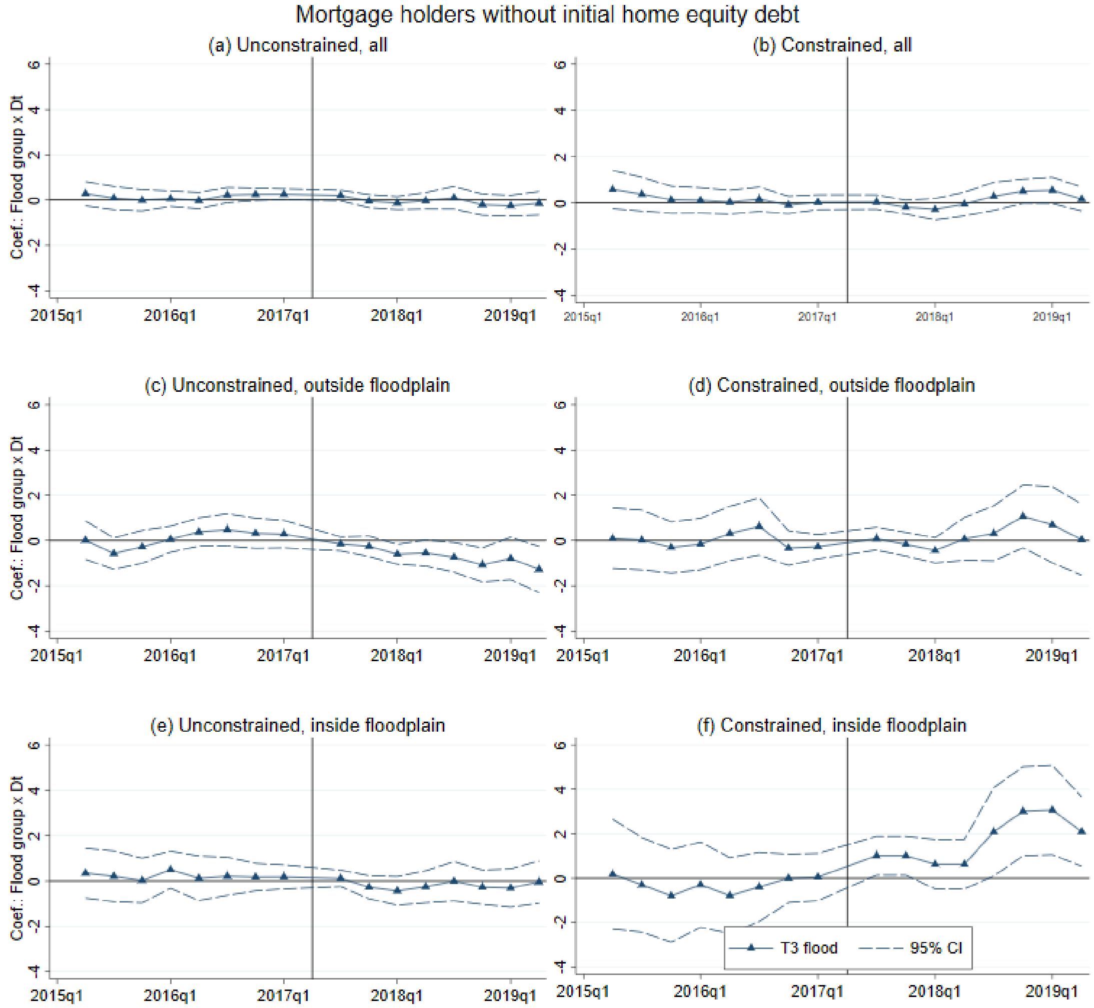



Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. In Panel (a), the dependent variable measures first mortgage balances in dollars (intensive margin effects) conditional on having a mortgage balance as of Q2 2017. In Panel (b), the dependent variable measures the extensive margin probability (in percentage terms) of having an outstanding first mortgage balance conditional on having a non-zero mortgage balance as of Q2 2017. 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. Different columns correspond to different sample splits: first, according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017, respectively, and second, according to whether the individual lives in a block that is outside of the floodplain. For visual ease, in both panels, the low (T1) and mid (T2) terciles are dropped from the graph. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

are comparatively more likely than their uninsured counterparts to make repairs rather than sell. In other words, *Constrained* households living outside of the floodplain may have been forced to sell their home due to a lack of the resources (home equity loans and SBA loans) needed to rebuild. This story is consistent with an increase in use of home equity loans inside the floodplain only and with the fact that SBA loan eligibility is contingent on the ability to repay (i.e., being *Unconstrained*).

Put together, these findings suggests that the degree of financial constraint, the extent of flood insurance, as well as the state of the local housing market when a disaster strikes (e.g., whether home values are above the replacement cost of construction) may lead to dramatically different conclusions about how disasters affect mortgage debt.

Figure 6: Difference-in-difference event study estimates of the probability of a *Home Equity Loan*, individual-level




Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. The dependent variable captures the extensive margin probability (in percentage terms) of having an outstanding home equity loan balance (including both second mortgage installment loans and revolving HELOCs). 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. Different graphs correspond to different sample splits: first, according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017, respectively, and second, according to whether the individual lives in a block that is outside or inside the floodplain. For visual ease, in both panels, the low (T1) and mid (T2) terciles are dropped from the graph. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

### 5.3.2 Student debt balances

Research points to a "student debt overhang" problem, in which high levels of student debt deter investments in human capital (Fos et al., 2017, Maggio et al., 2019) and homeownership (Mezza et al., 2016). There is also evidence that student loan borrowers repay debt strategically, according to variation in bankruptcy and wage garnishment rules (Yannelis, 2017). Since student debt is nondischargeable in bankruptcy and limits productive investments, we might therefore expect homeowners fearing a future bankruptcy to prioritize student debt over other debts.[45] Nevertheless, we know little about how isolated wealth shocks influence student debt repayment behavior. To our knowledge, this is the first study to analyze student debt in the context of a natural disaster.

Figure 7 displays the student debt DiD event study coefficients. Surprisingly, relative to blocks that did not flood, top-tercile flooding is associated with a temporary *decline* in student debt balances among constrained individuals (Panel a). These effects are larger in magnitude among mortgage-holders outside of the floodplain (Panel b). At its peak in Q4 2017, there is $2,000 relative decline in student debt balances. These findings coincide with the timing of the elimination of mortgage debt by this same subset of borrowers identified in Figure 5 as well as with the arrival of SBA loan assistance in Figure A6. Unlike with mortgage debt, results in Panel (c) indicate that these pay-downs did not perfectly translate into an elimination of the full student debt balance. Also note that, in all cases, student loan balances creep back up, returning to pre-trend levels by the start of the next school year.

Panel (d) shows that constrained, mortgage-holders *without* student debt at the dawn of the hurricane become relatively less likely to take out a student loan when located in an area with heavy flooding, particularly when that area is outside of the floodplain. In Panel (e) we divide the sample from areas outside of the floodplain by age, figuring that individuals under age-40 are potential students themselves while individuals over age-50 are potential co-signers on the loans of their dependents. The statistically strongest effects come from constrained mortgage-holders over age-50. These borrowers reduce their relative likelihood of co-signing on a student loan by roughly 1 percentage point (or 5.5% relative the mean probability of having student debt among *Constrained*, mortgage-holders outside of the floodplain). We dig deeper in Appendix Table A4. We find that the extensive margin reduction in the likelihood of opening a student debt account is concentrated among *Constrained*, mortgage-borrowers who likely had substantial equity in their homes before the hurricane and lived outside of the floodplain. For these mortgage-holders there is a 2.7 percentage point (representing 15% of their pre-Harvey probability of student debt) reduction in the likelihood of taking out (co-signing on) a student loan. We see no significant effect inside the floodplain or among borrowers with little equity.

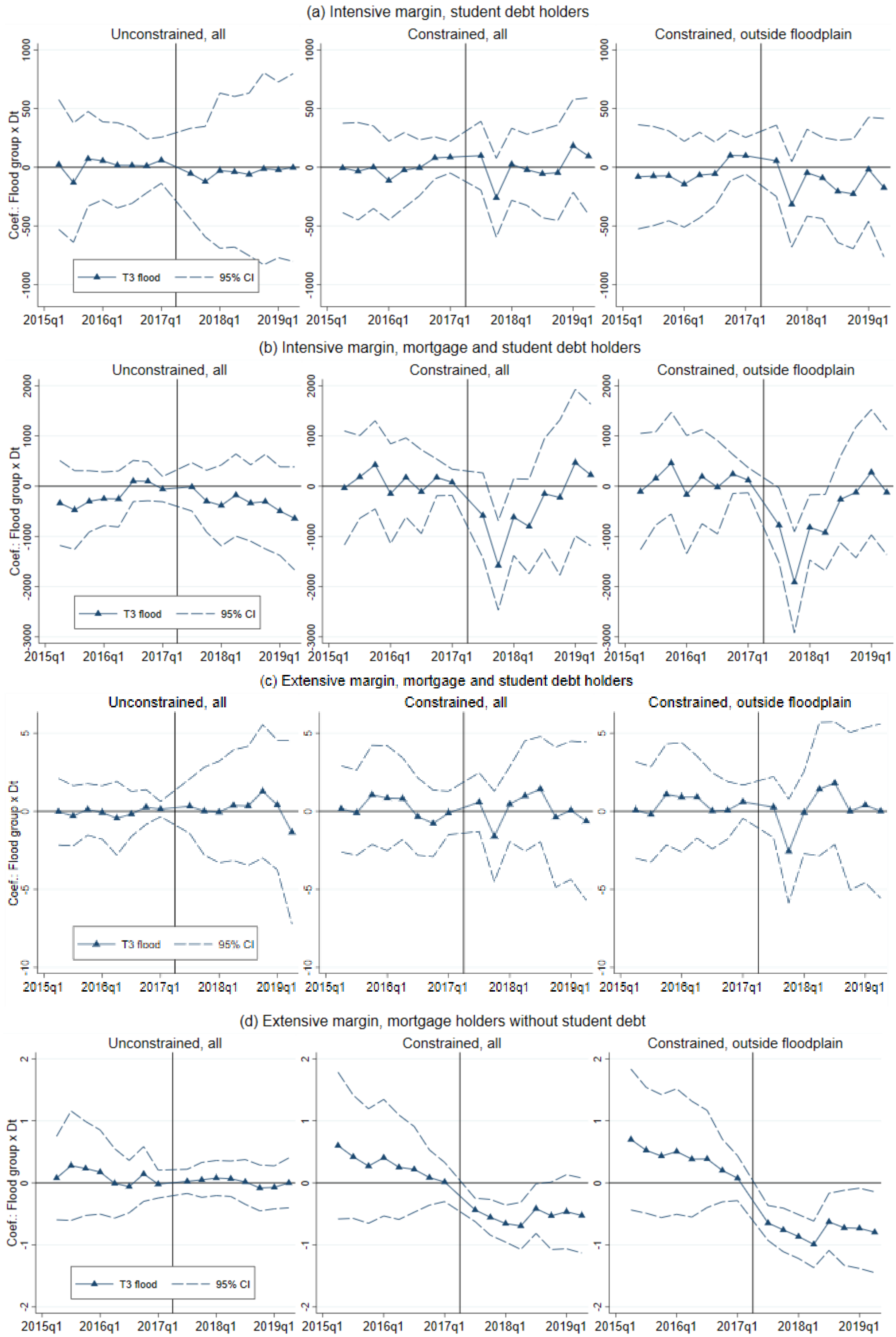
In summary, the student debt effects of Hurricane Harvey are unexpected and raise a number of questions. Most surprisingly, there is clear evidence that flooding is associated with a temporary reduction in pre-existing student debt balances among constrained mortgage-holders. We also find that constrained mortgage-holders over age-50 without student debt as well as those with more equity in their homes prior to the flooding to become much less likely to take out (co-sign on) a new student loan after experiencing local flooding – an effect which persists throughout the post period.

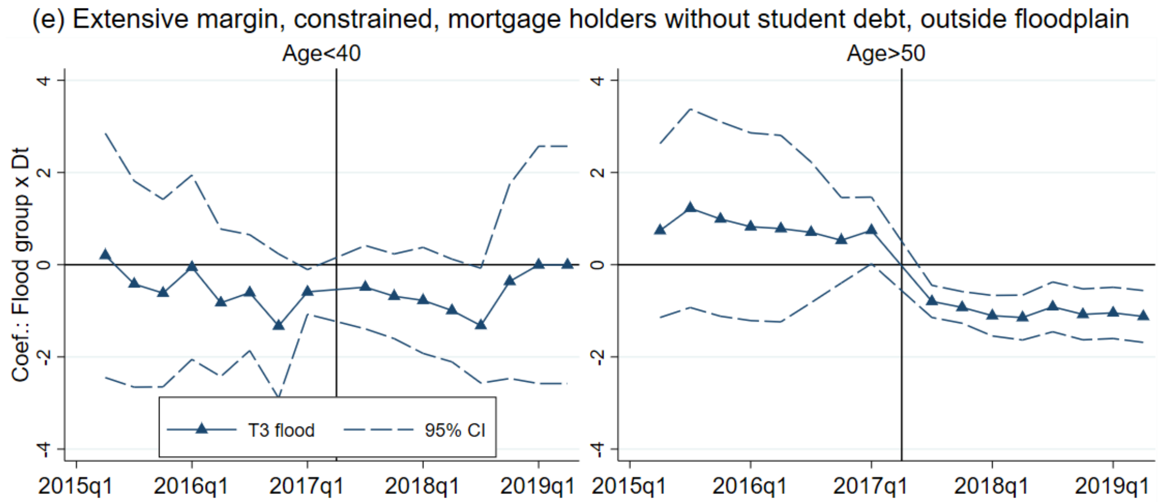
A number of possible explanations come to mind. Mortgage-holders may elect to reduce their non-dischargeable student debt by using the funds released from a home sale or from disaster assistance. The timing of the pay-downs is consistent with both of these stories; however, as we will later document, *Constrained* households face hurdles in obtaining disaster assistance, casting doubt on this latter explanation. Alternatively, these results for pre-existing borrowers might reflect a decision by constrained mortgage-

[45] Yannelis [2017] uses policy-induced variation in non-repayment costs (associated with bankruptcy and wage garnishment rules) to show that strategic behavior on the part of borrowers plays an important role in student loan repayment.

holders to delay education for the school-year after Harvey, thereby temporarily reducing student debt balances relative to balances in areas that did not flood. For homeowners without a pre-existing student loan, an unexpected loss of housing wealth may make *Constrained* individuals more reluctant to ever co-sign on student loans, explaining the lasting relative reduction in their probability of having student debt. Unfortunately, we cannot disentangle these possible mechanisms without detailed data on college enrollment by Harvey victims. We leave this task to future research.

Figure 7: Difference-in-difference estimates of *Student Debt*, individual-level






Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. In Panels (a) and (b), the dependent variable measures student loan balances in dollars (intensive margin effects) conditional on having student debt (Panel a) and on having a mortgage balance (Panel b) as of Q2 2017. Panel (c)–(e) measure the extensive margin probability (in percentage terms) of having an outstanding student loan balance conditional the factors noted above each panel. Panel (e) studies variation in effects according to the age of the borrower. We examine the below age-40 sample, since these individuals are potential students, and the above age-50 sample, since these individuals are potential co-signers on the loans of their children. 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. Different columns correspond to different sample splits: first, according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017, respectively, and second, according to whether the individual lives in a block that is outside of the floodplain. For visual ease, in both panels, the low (T1) and mid (T2) terciles are dropped from the graph. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

### 5.3.3 Auto loan balances

Up to half a million cars were flooded during Harvey.[46] However, as noted in Hartley et al. [2019], auto lenders typically require that borrowers have comprehensive auto insurance policies (which typically cover flooding) for the life of the loans. Hence, most borrowers with outstanding auto loans likely received insurance payments to replace their vehicles. To the extent that comprehensive auto insurance is common, we would expect to find muted changes in auto debt related to the storm.

Table 7 shows the DiD estimates according to flooding intensity for various subsamples.[47] Panel A documents the extensive margin likelihood of continuing to have auto debt. Among *Constrained* auto-borrowers there is a roughly 1 percentage point per foot of flooding relative increase in the probability of continuing to have an auto loan (Column 1). The differential effect of constraint widens for mortgage holders (Column 2), even more so for those with low equity in their homes (Column 3) and those outside the floodplain (Column 5). The estimate on $T_b \times P_t \times \mathtt{Constrained}_i$ in Column 5 implies a 3.6 percentage point per foot of block flooding relative increase in the likelihood of continuing to have auto debt when

[46]See `https://www.houstonpublicmedia.org/articles/news/2017/09/12/236782/half-a-million-cars-could-be-lost-to-harveys-waters/`

[47]For brevity, flood intensity is modeled in Table 7 as a continuous variable (rather than in terciles). Event study plots in Appendix Figure A11 present the top-tercile estimates, which follow the same patterns.

Table 7: Difference-in-difference estimates of *Auto Debt Balances*, individual-level

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *A. Extensive margin of auto debt; pre-existing auto debt holders* | | | | | | |
| $T_b \times P_t$ | -0.99*** | -1.11** | -1.41*** | -0.79 | -0.43 | -1.91** |
| | (-3.50) | (-2.62) | (-3.18) | (-1.15) | (-0.78) | (-2.45) |
| $T_b \times P_t \times \text{Constrained}_i$ | 0.95** | 2.49*** | 2.89** | 1.31 | 3.59*** | 3.24 |
| | (2.09) | (3.20) | (2.40) | (1.28) | (3.85) | (1.44) |
| N | 825401 | 318784 | 172924 | 149600 | 207485 | 20842 |
| Y-mean | 86.67 | 88.30 | 88.23 | 87.61 | 88.24 | 86.99 |
| *B. Intensive margin of auto debt; pre-existing auto debt holders* | | | | | | |
| $T_b \times P_t$ | -222.53** | -360.49** | -515.83*** | -200.99 | -498.25 | -587.73* |
| | (-2.21) | (-2.19) | (-2.79) | (-0.70) | (-1.67) | (-1.72) |
| $T_b \times P_t \times \text{Constrained}_i$ | 226.43 | 486.67* | 182.61 | 424.47 | 369.83 | 1290.01* |
| | (1.27) | (1.81) | (0.39) | (1.17) | (0.90) | (1.72) |
| N | 798108 | 311911 | 168115.00 | 147352.00 | 203049 | 20409 |
| Y-mean | 19449.11 | 22100.56 | 22129.66 | 20742.46 | 21734.86 | 21672.28 |
| *C. Extensive margin of auto debt; without pre-existing auto debt* | | | | | | |
| $T_b \times P_t$ | 0.10 | -0.19 | -0.46 | 0.6 | -0.64 | 0.28 |
| | (0.46) | (-0.43) | (-0.94) | (1.26) | (-0.89) | (0.34) |
| $T_b \times P_t \times \text{Constrained}_i$ | -0.14 | -0.39 | -2.99** | -0.37 | -0.58 | 0.43 |
| | (-0.48) | (-0.73) | (-2.55) | (-0.57) | (-0.35) | (0.32) |
| N | 994483 | 250206 | 136306 | 154921 | 164611 | 20026 |
| Y-mean | 9.86 | 13.58 | 14.69 | 11.08 | 13.55 | 12.54 |
| Sample | *All* | *Mtg>0* | *LoEquity* | *HiEquity* | *Mtg>0 & FlP=0%* | *Mtg>0 & FlP⩾50%* |

Table presents DiD estimates using the individual-level panel over Q2 2015–Q2 2019. The dependent variables capture the intensive and extensive margin of auto debt. Treatment intensity is defined according to a continuous measure of *WAvg. Flood Depth* in the post period. Treatment is interacted with a dummy indicating that the individual had an above-median index level of "Constraint" as of Q2 2017 (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). Specified columns restrict the sample to only mortgage holders as of Q2 2017. Columns 3 and 4 further restrict the sample to mortgage holders who have had their mortgage for less than (*LoEquity*) or more (*HiEquity*) than 5 years. The three two columns further restrict the sample to individuals from blocks outside of the floodplain (*FlP=0%*) or inside the floodplain (*FlP⩾50%*). All regressions include the full array of fixed effects and controls described in Section 4. 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.

*Constrained.* Meanwhile, *Unconstrained* borrowers inside the floodplain become 1.9 percentage points per foot of block flooding less likely to have an auto loan (Column 6).

Estimates in Panel B point to a marginally significant relative increase in oustanding auto debt balances among *Constrained* mortgage-holders and a reduction in auto debt balances among *Unconstrained* mortgage holders. For *Constrained* mortgage holders, the differential effect size is +$487 per foot of flooding (Column 2). Again, these estimates are for borrowers with pre-existing auto debt, such that most losses would have fallen under comprehensive insurance. Taken together, Panels A and B suggest that *Constrained* auto-borrowers were more likely than *Unconstrained* auto-borrowers to replace a flooded vehicle with one under loan. Appendix Figure A11 suggests extensive and intensive margin effects for pre-existing auto borrowers are temporary, peaking in 2018 Q2 and returning back to pretends by Q1 2019. Surprisingly, in Panel C, we find no consistent evidence that flooding is associated with an increased likelihood of taking out an auto loan conditional on not having auto debt when the hurricane hit.

In Appendix Figure A12, we find similar results when we evaluate changes in consumer financing debt, which includes personal loans and sales financing, typically for large household items (like mattresses and refrigerators). Constrained mortgage-holders with outstanding consumer financing debt increase their use of consumer financing when living in the top-tercile of flooded blocks. At its peak, there is a $200 relative increase in their balances over similar individuals in no-flood blocks, which represents 15% of the average pre-Harvey balance of *Constrained*, mortgage-holders outside the floodplain ($1,326). This effect may reflect a lack of resources needed to repay pre-existing balances and/or replace items destroyed by the flood.[48]

## 6 The Role of Disaster Assistance

Why are financial outcomes so much worse for those that enter the storm in a financially precarious position? Obviously, wealth is, itself, a form of protection against the negative effects of wealth shocks on credit outcomes. In the absence of disaster assistance, we should, therefore, expect people with more limited wealth 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 to help them recover, the predicted role of initial wealth becomes less clear. Presumably, it would depend on how the assistance is allocated. It could be that disaster assistance is allocated equally, but it is simply insufficient relative to the average cost of flooding. Or, it could be that disaster assistance is allocated in a way that benefits the wealthy comparatively more. To investigate these two possibilities, we turn to FEMA and SBA disaster loan data.

The FEMA data is at the individual-registrant-level and comes from FEMA's IHP program for the Houston area after Hurricane Harvey (see Section 2.2.2). In Table 8, we aggregate the individual-level data to the block-level and run OLS regressions. Aggregating individuals to census blocks allows us to normalize dependent variables by the number of FEMA registrants from a particular Census block and compare these normalized values to Census block characteristics. The first thing to note in Table 8 is that both *Avg. flood depth of flooded area* and the *Flooded share of developed area* are independently predictive of FEMA registration, damage, as well as assistance. This finding highlights the importance of accounting for both the depth and breadth of flooding, which we do in this paper using our composite measure of treatment

[48]To understand why we might observe a reduction in student debt balances by pre-existing borrowers and a simultaneous jump in use of auto debt and consumer financing, it is helpful to consider the well-documented phenomenon that, given the chance, households tend not to reduce their highest-interest debt account first (Amar et al., 2011). Since student debt is nondischargeable, the desire to eliminate student debt may be stronger, particularly after experiencing a negative wealth shock that may lead to bankruptcy.

(*Wgt. avg. flood depth*).

Columns 1 and 2 indicate that higher minority and lower income blocks were more likely to register with FEMA, even after controlling for flooding intensity, signaling a greater demand for assistance.[49] Columns 3 and 4 also indicate that lower-income areas were also more likely (per FEMA registrant) to be determined by FEMA to have experienced property damage. Meanwhile, minority areas were less likely (per FEMA registrant) to be determined to have experienced property damage. This latter result could reflect either subjectivity in the FEMA inspection process or a greater tendency for people in minority areas to register despite not experiencing damage.[50] In case the true explanation is the latter, when evaluating the correlates of being granted FEMA assistance, we will test whether our estimates are robust to controlling for a block's share of FEMA registrants determined by FEMA to have property damage (*Share of FEMA registrants w property damage*) as well as a block's share of FEMA registrants with flood insurance (*Share of FEMA registrants w flood insurance*).

In Columns 5–8, we evaluate our main question of interest: are FEMA registrants from lower income and/or higher minority areas less likely to be receive FEMA grants per registrant, all else equal. As expected based on the results above, we find that controlling for FEMA assessed damage and insurance in Columns 7 and 8 affects the magnitude of the estimates. Notably, these controls reduce the importance of minority share. Nonetheless, both minority share and median income coefficients remain statistically and economically important. Based on our final specification (Column 8), a one standard deviation increase in the minority share of a block (27 percentage points) is associated with a 1.4 percentage point decrease in the likelihood of being granted financial assistance, holding fixed the block's median household income, flood intensity, FEMA-assessed damage, and flood insurance. This effect size is nontrivial – it represents about 5% of the overall share of FEMA registrants that were granted financial assistance (27%). The effect of median household income on receiving FEMA assistance is much larger; a one standard deviation ($37,959) decrease in median income is linked to a 11 percentage point (42%) decrease in the share of FEMA registrants granted financial assistance. In sum, Columns 7–9 suggest that registrants from lower income areas (and, to a lesser extent, higher minority areas) are substantially less likely to be granted assistance.[51] In other words, disadvantaged areas face hurdles in receiving assistance that cannot be fully explained by differential tendencies to have insurance or to register with FEMA per unit of damage.

Next, in Table 9, we look at the dollar amount given in the form of FEMA IHP grants. We study

[49] As we would expect, the other socioeconomic controls point to fewer FEMA registrants in dense areas with more registrants in owner-occupied blocks. Surprisingly, however, these trends reverse for receipt of financial assistance.

[50] For example, a recent blog by the Urban Institute highlights the subjectivity of the disaster inspection process and the legal complexity of the FEMA application process. It notes, for example, that if the FEMA inspector believes that there was a pre-existing structural problem with the house, a homeowner is ineligible for assistance despite experiencing hurricane-related damage. See: `https://www.urban.org/urban-wire/problems-damage-assessments-can-keep-disaster-victims-receiving-help-they-need`

[51] Since one may be concerned that flooding is correlated with socioeconomic status and the need for assistance, we also run some models that interact flood intensity with both minority share in the block and median household income. Results indicate some small interaction effects for the portion of housing units that register with FEMA, but no interaction effect on the share determined to have experienced damage or on the share granted assistance by FEMA. In other words, results signal a significant correlation between access to FEMA assistance and socioeconomic factors (race and income) that cannot be explained away by any tendency for flooding to be more or less pronounced in areas with particular socioeconomic characteristics. These results are available upon request.

Table 8: Block-level correlates of the FEMA registration and assistance share

| | *Share of housing units registered w FEMA* | | *Share of FEMA registrants w property damage* | | *Share of FEMA registrants granted assistance* | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Wgt. avg. flood depth* | 0.019*** | | 0.017*** | | 0.019*** | | 0.003*** | |
| | (10.79) | | (7.44) | | (8.23) | | (2.66) | |
| *Minority share* | 0.333*** | 0.340*** | -0.205*** | -0.202*** | -0.265*** | -0.261*** | -0.052*** | -0.052*** |
| | (67.49) | (69.13) | (-30.96) | (-30.39) | (-40.19) | (-39.57) | (-14.95) | (-14.69) |
| *Median income (10k)* | -0.008*** | -0.008*** | -0.006*** | -0.006*** | 0.002*** | 0.002*** | 0.004*** | 0.003*** |
| | (-22.48) | (-23.08) | (-10.68) | (-11.01) | (3.31) | (2.97) | (12.56) | (12.41) |
| *Pop density (pop per acre)* | -0.007*** | -0.006*** | 0.006*** | 0.006*** | 0.008*** | 0.008*** | 0.002*** | 0.002*** |
| | (-6.88) | (-6.89) | (8.65) | (8.68) | (8.73) | (8.74) | (8.20) | (8.20) |
| *Owner-occ share* | 0.108*** | 0.111*** | 0.026*** | 0.026*** | -0.057*** | -0.057*** | 0.004 | 0.004 |
| | (18.33) | (19.12) | (3.30) | (3.36) | (-7.19) | (-7.12) | (0.97) | (1.02) |
| *Flooded share of developed area* | | 0.075*** | | 0.303*** | | 0.332*** | | 0.053*** |
| | | (3.53) | | (10.22) | | (10.84) | | (3.40) |
| *Avg. flood depth of flooded area* | | 0.005*** | | 0.003*** | | 0.003*** | | 0.001*** |
| | | (20.55) | | (8.46) | | (9.33) | | (4.20) |
| *Share of FEMA registrants w flood insurance* | | | | | | | 0.183*** | 0.183*** |
| | | | | | | | (68.33) | (68.27) |
| *Share of FEMA registrants w property damage* | | | | | | | 0.755*** | 0.755*** |
| | | | | | | | (226.74) | (225.85) |
| Y-mean | 0.26 | 0.26 | 0.40 | 0.40 | 0.27 | 0.27 | 0.27 | 0.27 |
| N | 46,874 | 46,874 | 46,874 | 46,874 | 46,874 | 46,874 | 46,874 | 46,874 |

Table presents cross-sectional OLS regressions of aggregate FEMA registration and assistance, at the block-level, on block-level characteristics, including treatment intensity (*WAvg. Flood Depth*), and its underlying components: *Avg. Flood Depth* in the flooded area multiplied by the share of the developed block area that flooded (*Flooded Share of Developed Area*). All models use the Census Block 2010 as the unit of observations. We define *Share of Housing Units Registered with FEMA* as the share of housing units in a Census Block 2010 that were registered with FEMA following Hurricane Harvey. *Share of FEMA Registrants Granted Assistance* is defined as the share of all FEMA registered households that were determined eligible and, in most cases, received financial assistance. *Share of FEMA Registrants with Property Damage* is defined as the share of all FEMA registered households that were found to have property damage>0 based on FEMA inspections. Parentheses contain t-statistics, generated from standard errors clustered on census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

this outcome at the individual-level data in order to directly control for whether or not an individual has flood insurance as well as for the dollar value of the individual's FEMA-assessed property damage.[52] Meanwhile, due to data limitations, socioeconomic variables continue to reflect those of the Census block where the individual lives. Consistent with the block-level results, registrants in higher-income and lower-minority areas receive more in FEMA assistance dollars per dollar of damage (Column 1).[53] Conditional on achieving a non-zero amount of assistance (Column 2), the income effect on assistance dollars becomes six times larger in magnitude and the t-statistic grows. After qualifying for assistance, having a one standard deviation lower median income implies $3,927 less in assistance dollars, essentially cutting in half the average dollar amount of assistance ($7,295). However, the coefficient on minority share switches signs and the standard errors widen. One possible explanation is that minorities face hurdles to receiving assistance; however, once that hurdle is surpassed, minorities actually start to receive slightly more in assistance dollars (i.e., $102 per standard deviation increase in minority share). In summary, results point to median income (more so than minority share) as a major determinate of, not only of the likelihood of receiving a FEMA grant, but also the dollar value of that grant.

Finally, we test for inequalities in access to SBA loans. We use the fact that the FEMA registration data contains a field that indicates if a registrant is deemed eligible for an SBA loan based on an income test.[54] Using an indicator for SBA eligibility and individual registrations as the unit of analysis, we examine if block-level socioeconomic factors are predictive of SBA eligibility. Consistent with Begley et al. [2018], Table 10 shows that registrants from lower minority and higher income areas are more likely to be eligible for SBA loans, even conditioning on the dollar value of FEMA-assessed property damage as well as on flood insurance status. These findings hold after limiting our sample to FEMA registrants with non-zero assessed property damage (Columns 2–3) as well as to only homeowners (Columns 4–5). For example, the coefficients in Column 2 would suggest that a simultaneous one standard deviation reduction in block median income and increase in block minority share would reduce a registrants likelihood of SBA loan eligibility by 5.3 percentage points – essentially cutting the registrants chances of an SBA loan in half relative to the 10.1% mean eligibility rate. The interaction effects in Columns 3 and 5 indicate that the disparity is made worse by having above median property damage.

Column 6 of Table 10 uses individual-level, approved loans obtained from the SBA by FOIA request. We would expect to find inequalities in approved loan sizes as well given that "Unsatisfactory credit history" is the most frequent denial reason given by the SBA (see Appendix Table A1). Since controls for property damage and insurance are not included in the SBA data, we link the SBA data to the FEMA registrant data at the census block-level, and control for block-level aggregates of property damage and prevalence of insurance, normalized by the number of housing units in the block. The dependent variable is constructed similarly, by aggregating approved individual SBA loan amounts and dividing by the number of housing

[52]Results in Table 9 affirm that substantial flood insurance coverage relative to damage implies less in alternative assistance. In untabulated results, we find that this result holds even within a block (including block-level fixed effects). Recall that federal disaster benefits are designed not to duplicate other benefits across assistance programs, including the benefits of flood insurance (Section 2.2).

[53]For example, a simultaneous one standard deviation reduction the median income and increase in the minority share of the block where the registrant lives is associated with $116 less in FEMA assistance according to Column 1.

[54]See Table 2 on page 14 of `https://fas.org/sgp/crs/homesec/R45238.pdf`.

Table 9: Individual- and block-level correlates of FEMA assistance dollars

| | (1) | (2) |
|---|---|---|
| *Has Flood Insurance* | -4018.57*** | -10936.02*** |
| | (-45.69) | (-66.14) |
| *Property damage ($000s)* | 463.03*** | 437.18*** |
| | (67.41) | (46.81) |
| *Median Income ($10k)* | 16.76*** | 103.45*** |
| | (2.94) | (5.94) |
| *Minority Share* | -194.11*** | 378.79** |
| | (-4.60) | (2.33) |
| *Pop Density (pop per acre)* | -0.06 | -4.49 |
| | (-0.04) | (-0.33) |
| *Owner-Occ Share* | 599.22*** | 3935.21*** |
| | (19.67) | (23.45) |
| Sample | All | Assistance>0 |
| Y-mean | 1,354 | 7,295 |
| N | 509,677 | 94,626 |
| F.E. | None | None |

Table presents cross-sectional OLS regressions of the amount of *FEMA Assistance* granted to an individual (in dollars) on individual- and block-level characteristics. The unit of observation is the individual household. In Column 2, to isolate intensive margin effects from extensive margin effects, we limit the sample to households that received some non-zero amount of assistance. Although the FEMA data contain individual gross income, a nontrivial number of individual income observations are missing. Moreover, their correlation with block median income imperfect, suggesting that the missing values are non-random and, thus, may be selected on unobservables. For this reason, we use only block-level median income instead. Parentheses contain t-statistics, generated from standard errors clustered on census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

units in the block. We restrict our sample to only blocks with at least one approved SBA loan. As expected, loan amounts are positively correlated with block-level damage and negatively correlated with having flood insurance. Controlling for these factors, higher income blocks receive larger SBA loans, on average, such that a one standard deviation higher block income is associated with an additional $4,729 in SBA loans approved per housing unit, equal to 25% of the average amount in SBA approved loans per housing unit. The coefficient on minority share is negative and large in magnitude but statistically insignificant. In other words, it appears that higher income people are not only more eligible for SBA loans, but they also receive larger loans amounts all else equal.

Taken together, these results indicate that disaster assistance is being allocated in a way that is regressive. It appears that individuals from lower-income areas and (depending on the outcome measure) minority areas face hurdles in obtaining federal disaster assistance that cannot be explained by differences in property damage or insurance status. One implication is that federal disaster assistance may be exacerbating, rather than that counteracting, pre-existing wealth inequalities. This factor may partly explain why we find that flooding is associated with comparatively worse (better) credit outcomes, for individuals that enter the storm in a weaker (stronger) financial position.

Table 10: Correlates of SBA loan eligibility and amounts

| | SBA loan eligible (Individual-level) | | | | | SBA loan amount per housing unit ($) (Block-level) |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Property damage ($000s)* | 0.699*** | 0.483*** | | 0.443*** | | 603.287*** |
| | (42.18) | (30.17) | | (27.79) | | (11.37) |
| *Has Flood Insurance* | -0.236 | -2.555*** | -1.997*** | -3.698*** | -4.076*** | -2666.203*** |
| | (-1.26) | (-7.47) | (-5.88) | (-9.95) | (-10.82) | (-3.34) |
| *Median Income ($10k)* | 0.369*** | 1.165*** | 0.338*** | 1.263*** | 0.335*** | 1245.770*** |
| | (15.87) | (21.91) | (8.63) | (20.29) | (6.34) | (6.48) |
| *Minority Share* | -1.479*** | -3.156*** | -2.024*** | -4.027*** | -2.293*** | -9785.285 |
| | (-9.27) | (-7.58) | (-6.75) | (-7.55) | (-5.62) | (-1.13) |
| *Pop Density (pop per acre)* | 0.030*** | 0.109*** | 0.031 | 0.534*** | 0.064 | -1.016 |
| | (3.26) | (2.64) | (0.83) | (2.59) | (0.32) | (-1.10) |
| *Owner-Occ Share* | 1.735*** | 4.717*** | 6.307*** | 7.039*** | 7.876*** | 7744.558 |
| | (11.98) | (9.22) | (12.90) | (7.06) | (7.39) | (0.79) |
| *Median Income x Property Damage>p50* | | | 0.001*** | | 0.001*** | |
| | | | (17.11) | | (15.75) | |
| *Share Minority x Property Damage>p50* | | | -4.533*** | | -4.631*** | |
| | | | (-5.19) | | (-4.44) | |
| *Property Damage>p50* | | | 2.413*** | | 3.643*** | |
| | | | (3.11) | | (4.15) | |
| Sample | *All FEMA* | Damage > 0 | | *Damage > 0 & Owners* | | *Blocks with SBA eligible* |
| Y-mean | 3.85 | 10.12 | 10.12 | 13.03 | 13.03 | 18867.76 |
| Observations | 509,677 | 161,594 | 161,594 | 113,571 | 113,571 | 17259 |

Table presents cross-sectional OLS regressions of SBA loan access on individual- and block-level characteristics. The first dependent variable comes from FEMA data and is an indicator of an individual FEMA registrant's SBA loan eligibility as determined by FEMA. The unit of observation is the individual household such that coefficients can be interpreted as percentage point effects on the probability of SBA loan eligibility. The second dependent variable comes from the SBA (through FOIA request) and captures the average SBA loan amount (in dollars) distributed to residents of a given census block. It is calculated as the total amount of SBA loans distributed to individuals (not businesses) normalized by the number of housing units in the block. The unit of observation is the census block given we cannot link SBA loan data to FEMA registrants and we include only census blocks with at least one SBA loan and all individual-level explanatory variables are averaged across FEMA registrants in the block (e.g., property damage is totalled and then divided by the number of FEMA registrants in the block). To improve precision, in models (2)–(3), we limit the sample to households with inspected damages of greater than zero (very few households are SBA eligible yet found to not be damaged by FEMA). In models (4)–(5), we further limit the sample to homeowners since the majority of SBA loans were given to homeowners. Although the FEMA data contain individual gross income, a nontrivial number of individual income observations are missing. Moreover, their correlation with block median income imperfect, suggesting that the missing values are non-random and, thus, may be selected on unobservables. For this reason, we use only block-level median income instead. Parentheses contain t-statistics, generated from standard errors clustered on census block: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

# 7 Conclusion

This paper evaluates the effect of flooding under Hurricane Harvey on individual credit outcomes. We focus on heterogeneity in treatment effects, asking whether flooding imposes additional burden on individuals that entered the hurricane in a weaker financial position and/or live in areas with less flood insurance penetration. Our estimates point to substantial heterogeneity in financial outcomes along these dimensions. Overall, we find that flooding has comparatively more deleterious effects on the bankruptcy rates, delinquencies and credit scores of people who enter the storm in a worse financial position. Outcomes are worse among homeowners and people residing outside the floodplain, where there is a lower likelihood of having flood insurance. In addition, we present suggestive evidence that a lack of resources to put toward rebuilding likely compels some constrained homeowners to sell, thereby eliminating their mortgage. We also observe reductions in outstanding and new student debt, which could be explained by a variety of factors, including a reluctance on the part of homeowners to take on nondischargable debt after experiencing a wealth shock. Finally, we see a large increase in the consumer financing balances among constrained mortgage-holders, again indicative of a lack of resources to put towards large purchases.

We hypothesize that the allocation of disaster assistance may help explain the divergent financial paths according to initial financial condition that we document in this paper. Controlling for damages and insurance, we find strong correlations between access to disaster assistance (FEMA IHP assistance and SBA loans) and socioeconomic factors, namely the median income and minority share of an individual's block. Indeed, coming from a higher-income areas is consistently predictive, not just of eligibility, but also of dollar value of FEMA assistance and SBA loans.

These findings carry a number of policy implications. Foremost, our results suggest there is a consequence, in terms of exacerbating inequalities, to allocating disaster assistance in a way that appears to be (unintentionally) regressive. The SBA may consider, for example, using price discrimination in order to make more loans while maintaining its goal of revenue neutrality. Next, the fact that we observe better outcomes for individuals located in floodplains highlights the important of the National Flood Insurance Program in protecting households from wealth related shocks such as Hurricane Harvey. Of serious concern, then, is the current fiscal challenge faced by the NFIP (Michel-Kerjan, 2010) coupled with expectations of more flooding in the future. Furthermore, to the extent that flood insurance is considered unaffordable, induces rebuilding in flood-prone areas, and/or sends homeowners an inaccurate signal (based on a binary 100-year flood map) of flood risk, policy changes may be needed.[55]

[55]Kunreuther et al. [2018] detail a number of possible policy changes to FEMA flood mapping and the NFIP in line with those outlined here.

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# A Online Appendix: Additional Results

Table A1: 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).

Table A2: Principal components analysis of financial hardship

This table describes the principal components of standardized variables that proxy financial constraint: *Equifax Risk Score, Credit Card Utilization, Block Median Income, Block Minority Share.* 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 financial constraint are reported.

*Panel A. Eigen values 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 eigen vectors*

| | Comp1 | Comp2 | Comp3 | Comp4 |
|---|---|---|---|---|
| *Equifax Risk Score* | -0.55 | 0.31 | -0.74 | 0.25 |
| *Credit Card Utilization* | 0.32 | -0.77 | -0.52 | -0.14 |
| *Block Median Income* | -0.56 | -0.37 | 0.004 | -0.75 |
| *Block Minority Share* | 0.54 | 0.41 | -0.43 | -0.60 |

Table A3: Difference-in-difference estimates of *Out-migration*, block-level rate

| | (1) | (2) | (3) | (4) | (45) |
|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | 0.02 | 0.02 | | | |
| | (1.11) | (0.63) | | | |
| $T_b^2 \times P_t$ | 0.03 | 0.03 | | | |
| | (1.36) | (1.04) | | | |
| $T_b^3 \times P_t$ | 0.03 | 0.05 | 0.06* | 0.13** | 0.19** |
| | (1.23) | (1.64) | (1.77) | (2.34) | (2.33) |
| $T_b^1 \times P_t \times Constrained_b$ | | 0.01 | | | |
| | | (0.13) | | | |
| $T_b^2 \times P_t \times Constrained_b$ | | -0.01 | | | |
| | | (-0.19) | | | |
| $T_b^3 \times P_t \times Constrained_b$ | | -0.06 | | -0.16* | -0.34*** |
| | | (-1.29) | | (-1.92) | (-2.70) |
| N | 518428 | 518428 | 332291 | 332291 | 52943 |
| AdjR2 | 0.04 | 0.04 | 0.04 | 0.04 | 0.05 |
| Y-mean | 0.18 | 0.18 | 0.17 | 0.17 | 0.14 |
| Sample | *All* | *All* | *Extremes* | *Extremes* | *Extremes & FlP=0%* |

Table presents DiD estimates using the block-level panel over Q2 2015–Q2 2019. The dependent variable, *Out-migration*, is the share of a block's residents that move out of houston during the quarter (in percentage points). Treatment intensity is defined according to tercile bins of *WAvg. Flood Depth* in the post period. The coefficient on $T_b^3 \times P_t$, for example, can be interpreted as the effect of top tercile flooding intensity on a block's post-hurricane out-migration rate relative to its pre-period out-migration and relative to the post-hurricane out-migration rate of blocks that did not flood. In some specifications, treatment is interacted with a dummy indicating that the block has an above-median share that entered the hurricane in a state of financial constraint (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). To provide a rough sense of the treatment-on-treated effect, Columns 3 and 4 restrict the sample to only blocks that either had no flooding or were in the top decile of flooded blocks. Column 5 further restrics the sample to blocks outside of the floodplain (*FlP=0%*). All regressions include the full array of fixed effects and controls described in Section 4. Regressions are weighted by the number of observations within each census block in the CCP data, effectively giving more weight to more precise observations. 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 A4: Difference-in-difference estimates of *Extensive Margin of Student Debt*, individual-level

| | (1) | (2) | (3) | (4) | |
|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | 0.06 | -0.20 | 1.89* | 0.26 | 0.77** |
| | (0.18) | (-0.50) | (1.94) | (0.84) | (2.48) |
| $T_b^2 \times P_t$ | -0.30 | -0.07 | 1.06 | 0.45 | 0.38 |
| | (-1.06) | (-0.16) | (1.35) | (1.01) | (1.15) |
| $T_b^3 \times P_t$ | 0.05 | -0.03 | 0.21 | 0.43 | 1.75* |
| | (0.11) | (-0.06) | (0.20) | (1.37) | (1.78) |
| $T_b^1 \times P_t \times \text{Constrained}_i$ | -0.49 | -0.08 | -2.76* | -0.99 | -0.55 |
| | (-0.94) | (-0.11) | (-1.87) | (-1.55) | (-0.78) |
| $T_b^2 \times P_t \times \text{Constrained}_i$ | 0.22 | -1.10 | -3.43 | -0.57 | 0.48 |
| | (0.33) | (-1.24) | (-1.64) | (-0.61) | (0.57) |
| $T_b^3 \times P_t \times \text{Constrained}_i$ | -1.94** | -1.04 | 1.71 | -3.17*** | -1.44 |
| | (-2.25) | (-0.88) | (0.96) | (-2.89) | (-1.17) |
| N | 310228 | 167008 | 18496 | 180302 | 20026 |
| AdjR2 (binned) | 0.34 | 0.40 | 0.52 | 0.39 | 0.33 |
| Y-mean | 1.12 | 1.22 | 1.31 | 0.95 | 0.43 |
| Sample | *Stud=0* | *Stud=0* | *Stud=0* | *Stud=0* | *Stud=0* |
| | *& Mtg>0* | *LoEquity* | *LoEquity* | *HiEquity* | *HiEquity* |
| | *& FlP=0%* | *& FlP=0%* | *& FlP⩾50%* | *& FlP=0%* | *& FlP⩾50%* |

Table presents DiD estimates using the individual-level panel over Q2 2015–Q2 2019. The dependent variable is a binary indicator that equals one if the individual has a non-zero student loan balance in a given quarter. The sample is restricted to individuals with an outstanding mortgage balance (*Mtg>0*) but with no outstanding student debt (*Stud=0*) as of Q2 2017 (the last observation before the hurricane). Treatment intensity is defined according to tercile bins of *WAvg. Flood Depth* in the post period. Treatment is interacted with a dummy indicating that the individual had an above-median index level of "Constraint" as of Q2 2017 (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). As specified at the bottom of the table, Columns 2–5 further restrict the sample to mortgage holders who have had their mortgage for less than (*LoEquity*) or more (*HiEquity*) than 5 years. Finally, the sample is also restricted according outside floodplain (*FlP=0%*) or inside the floodplain (*FlP⩾50%*). All regressions include the full array of fixed effects and controls described in Section 4. 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 A1: Employment before and After Hurricane Harvey for the Houston MSA

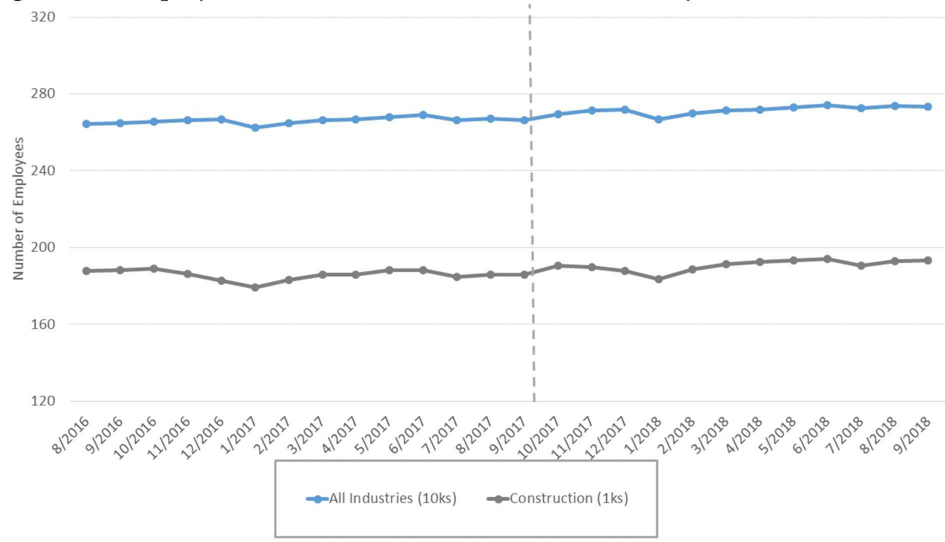



Figure A2: Average Weekly Wages before and After Hurricane Harvey for the Houston MSA

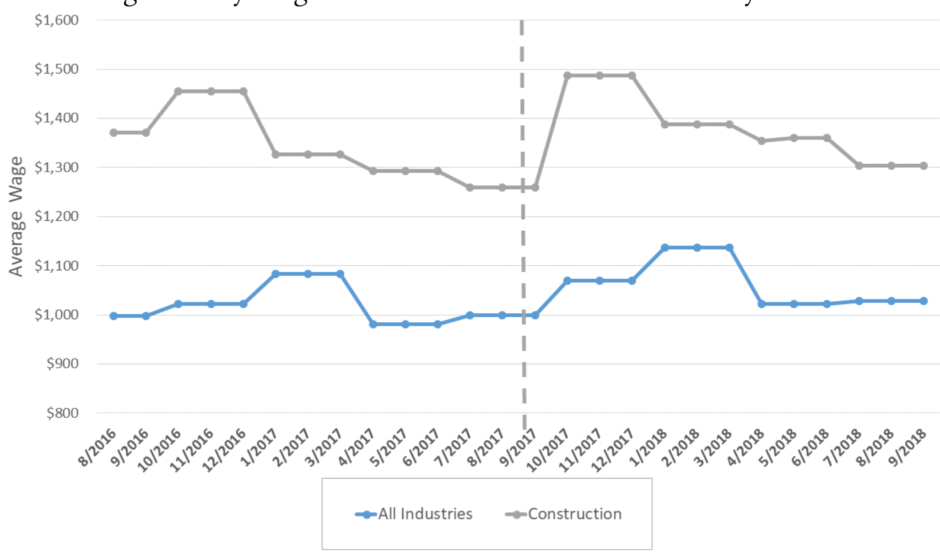


Figure A3: Distribution of flooding by income bin

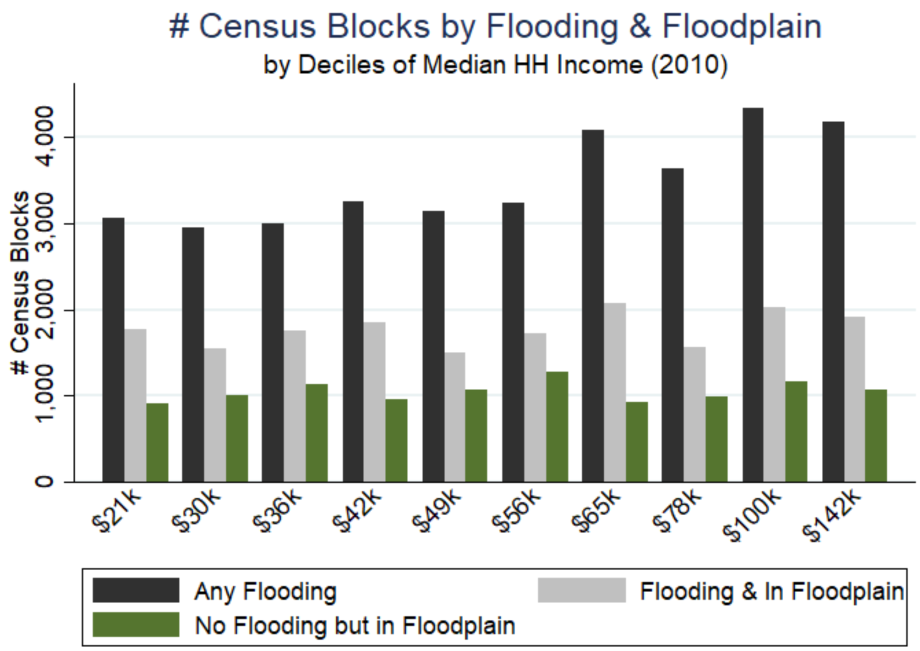
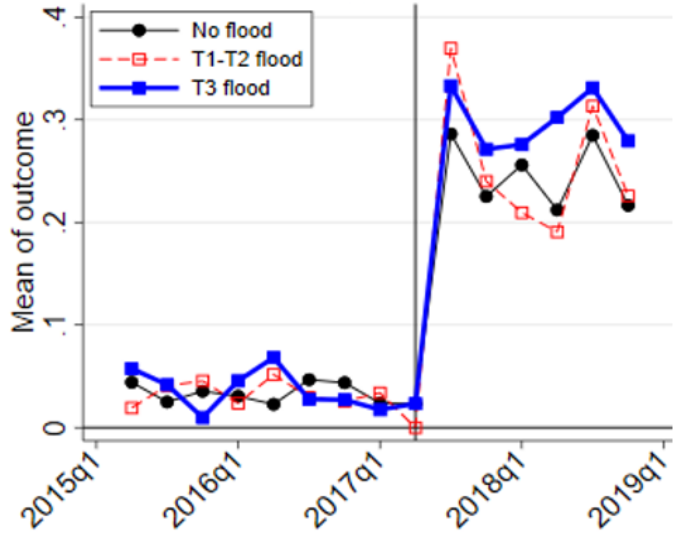
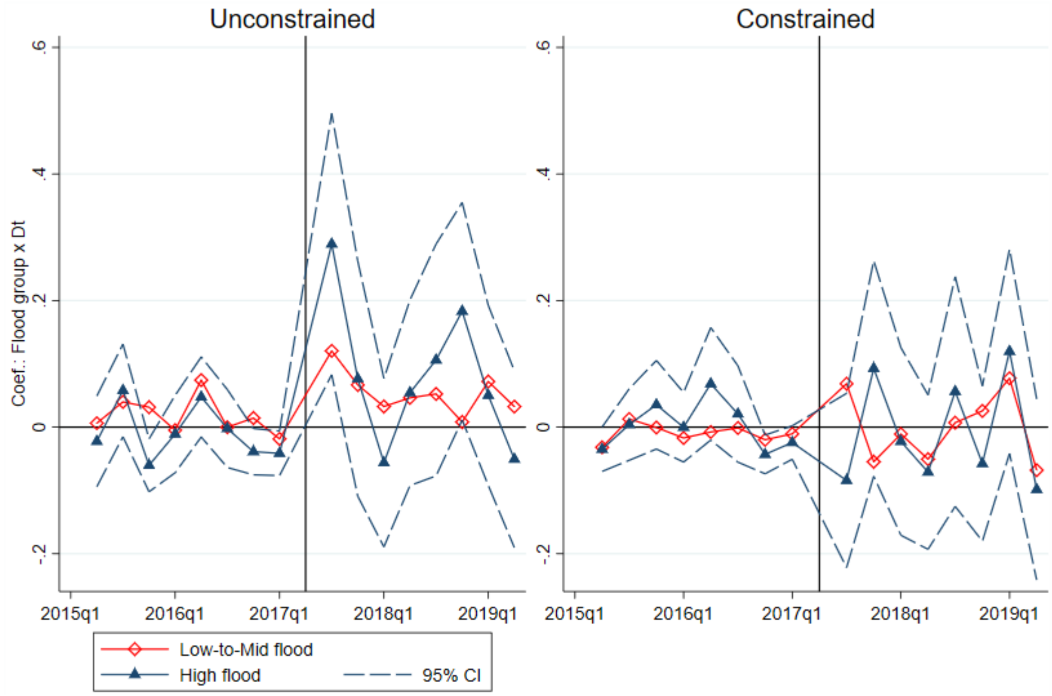


Figure A4: Nonparametric and difference-in-difference event study estimates of *Out-migration*, block-level

Panel A. Out-migration rate, block-level mean

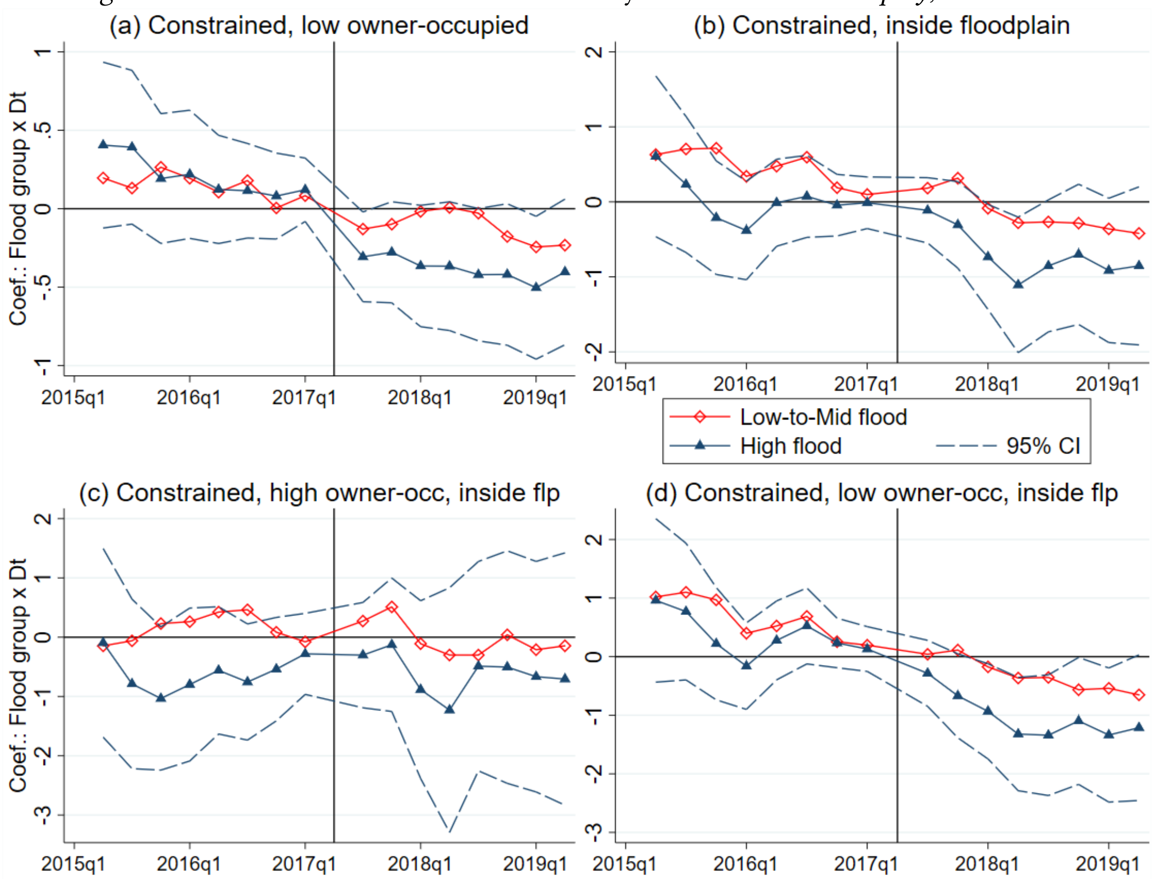



Panel B. Difference-in-difference event study estimates of *Out-migration*



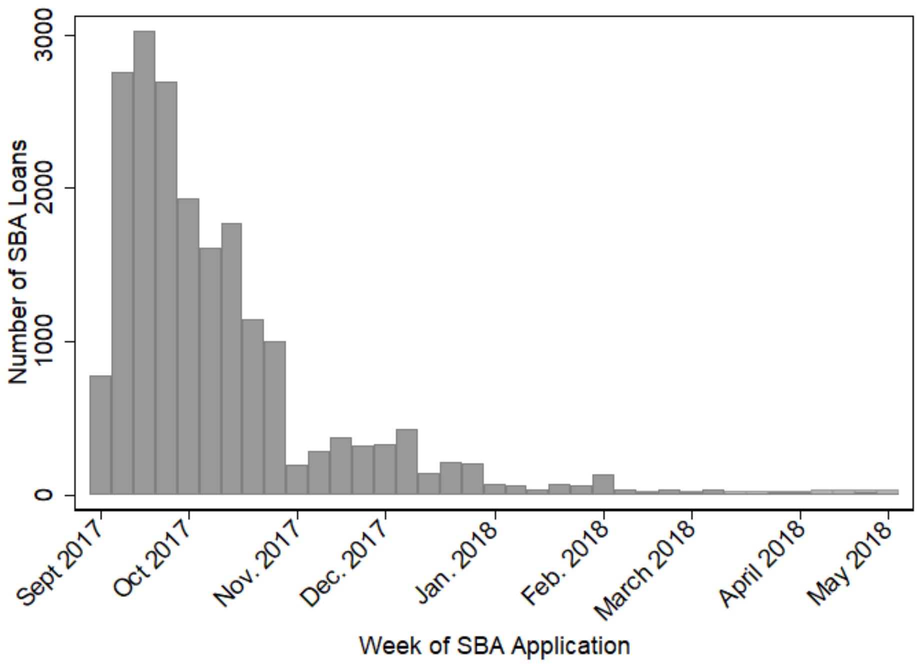

The outcome variable is the block rate of migration out of Houston (in percentage points). Panel A plots the nonparametric mean. Panel B plots the event study coefficients from DiD regressions. All graphs use the block-level panel over Q2 2015–Q2 2019. In Panel B, the sample is split according to whether the block has an above (*Constrained*) or below (*Unconstrained*) median share of residents classified as "constrained" according to the last quarterly observation before the hurricane (Q2 2017). For visual ease, the low (T1) and mid (T2) terciles are combined in the regressions (red, diamond line). Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. All regressions include the full array of fixed effects and controls described in Section 4. Regressions are weighted by the number of observations within each census block in the CCP data, effectively giving more weight to more precise observations. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A5: Difference-in-difference event study estimates of *Bankruptcy*, block-level



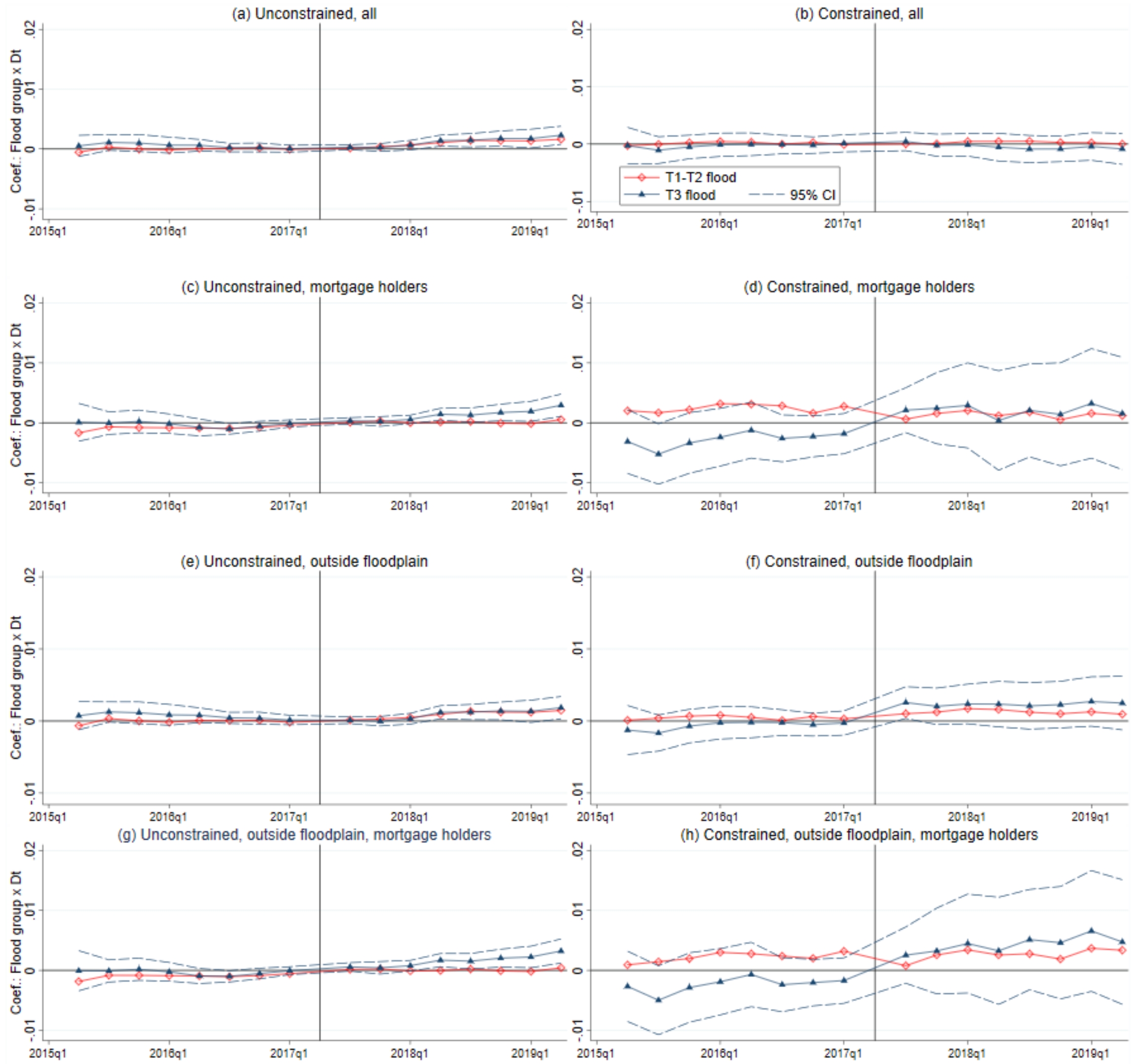

Figures plot event study coefficients from DiD regressions using the block-level panel over Q2 2015–Q2 2019. The dependent variable, *Bankruptcy*, is the share of a block's residents that have a bankruptcy flag on their credit report during the quarter. The sample is restricted to blocks with an above median share of residents classified as "constrained" according to the last quarterly observation before the hurricane (Q2 2017). The sample is further split, at the median, according to the owner-occupied share and conditioned on floodplain status (having at least 50% of the block's developed area inside the floodplain). For visual ease, the low (T1) and mid (T2) terciles are combined in the regressions (red, diamond line). Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. All regressions include the full array of fixed effects and controls described in Section 4. Regressions are weighted by the number of observations within each census block in the CCP data, effectively giving more weight to more precise observations. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A6: Timing of SBA loan issuance



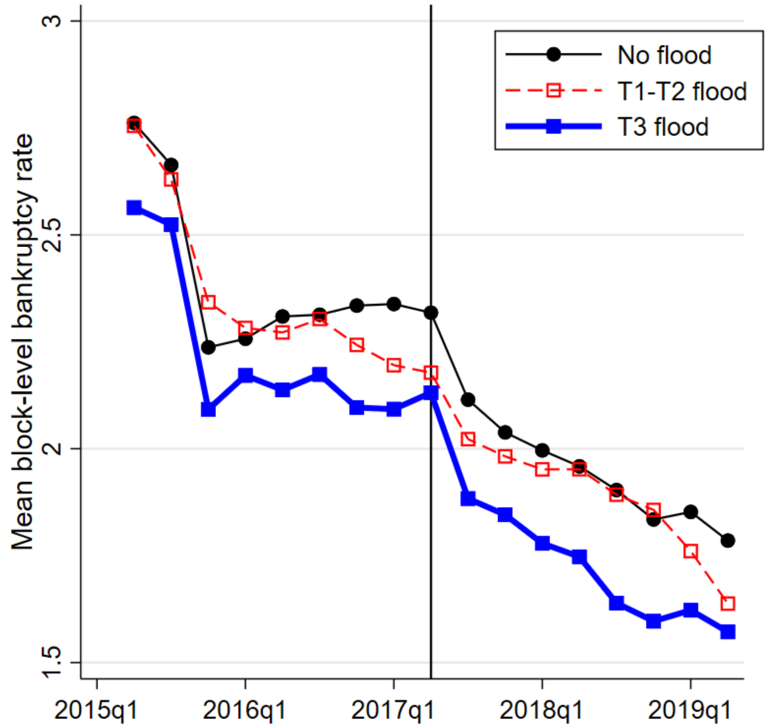

This figure shows the weeks in which SBA loan assistance was granted to individuals in Houston after Hurricane Harvey. Data come from a FOIA request of the SBA.

Figure A7: Difference-in-difference event study estimates of *Bankruptcy*, individual-level
{PENDING CHANGE TO RARE EVENTS LOGIT SPECIFICATION TABLE}



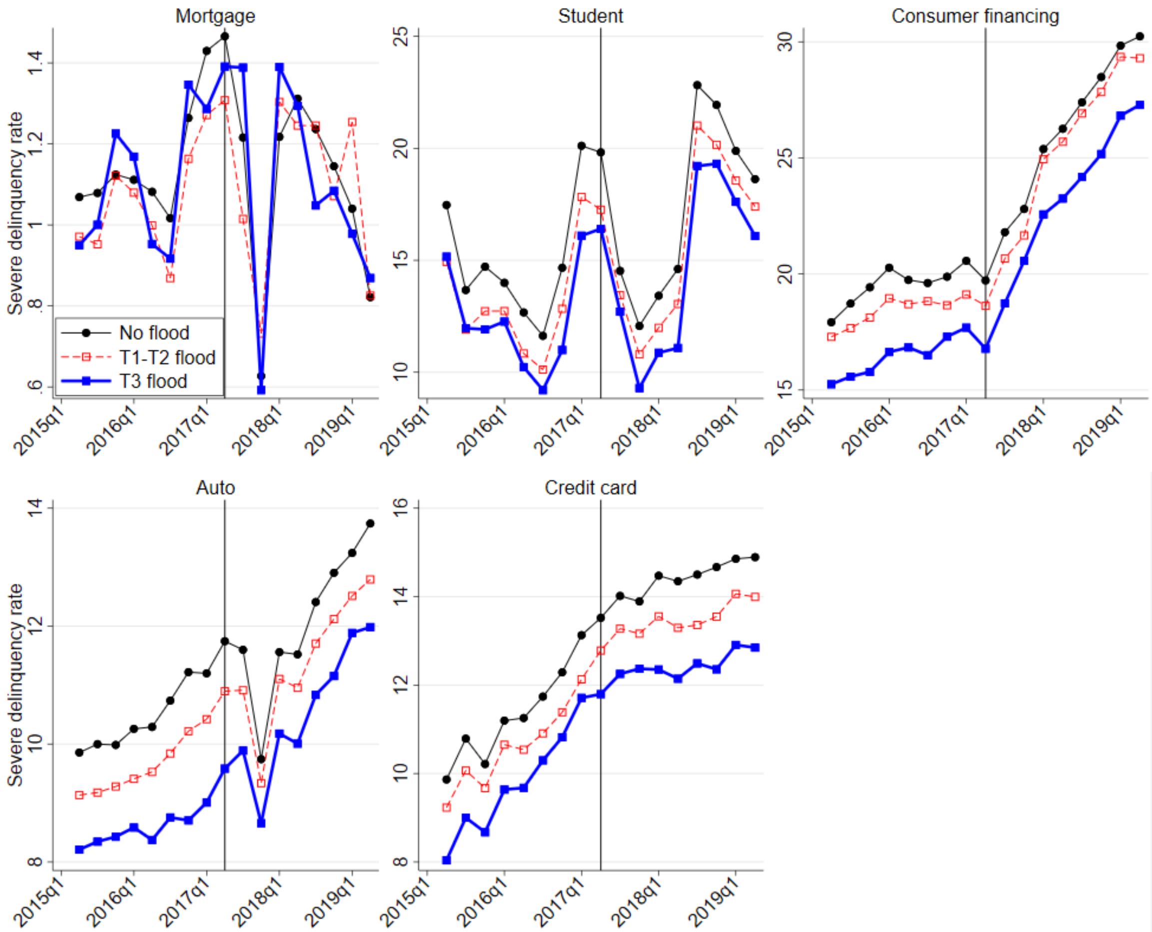

Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. The dependent variable, *Bankruptcy*, is a dummy such that a value of 1 indicates a bankruptcy flag on the individual's credit report during the quarter. The sample is split according to whether the individual has an above (*Constrained*) or below (*Unconstrained*) median index value of "constrained" 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 balance as of Q2 2017 and according to floodplain status. For visual ease, the low (T1) and mid (T2) terciles are combined in the regressions (red, diamond line). Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A8: Mean of *Bankruptcy Share* by block flood intensity and date, block-level



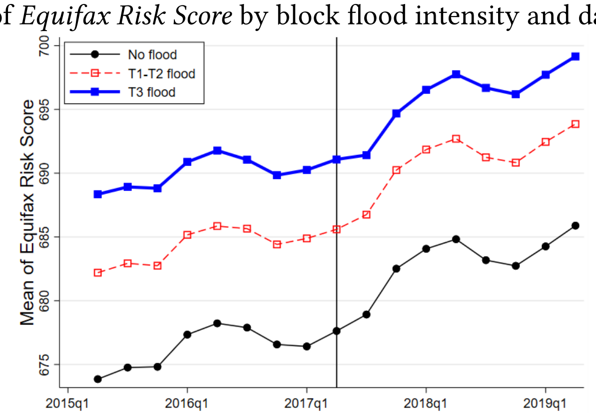

The figure plots the mean share of residents with a bankruptcy flag on their credit file across blocks by date and by the flooding intensity of the block. For visual ease, blocks in the low (T1) and mid (T2) terciles are combined (red, squares). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A9: Severe delinquency rate by block flood intensity and date, individual-level



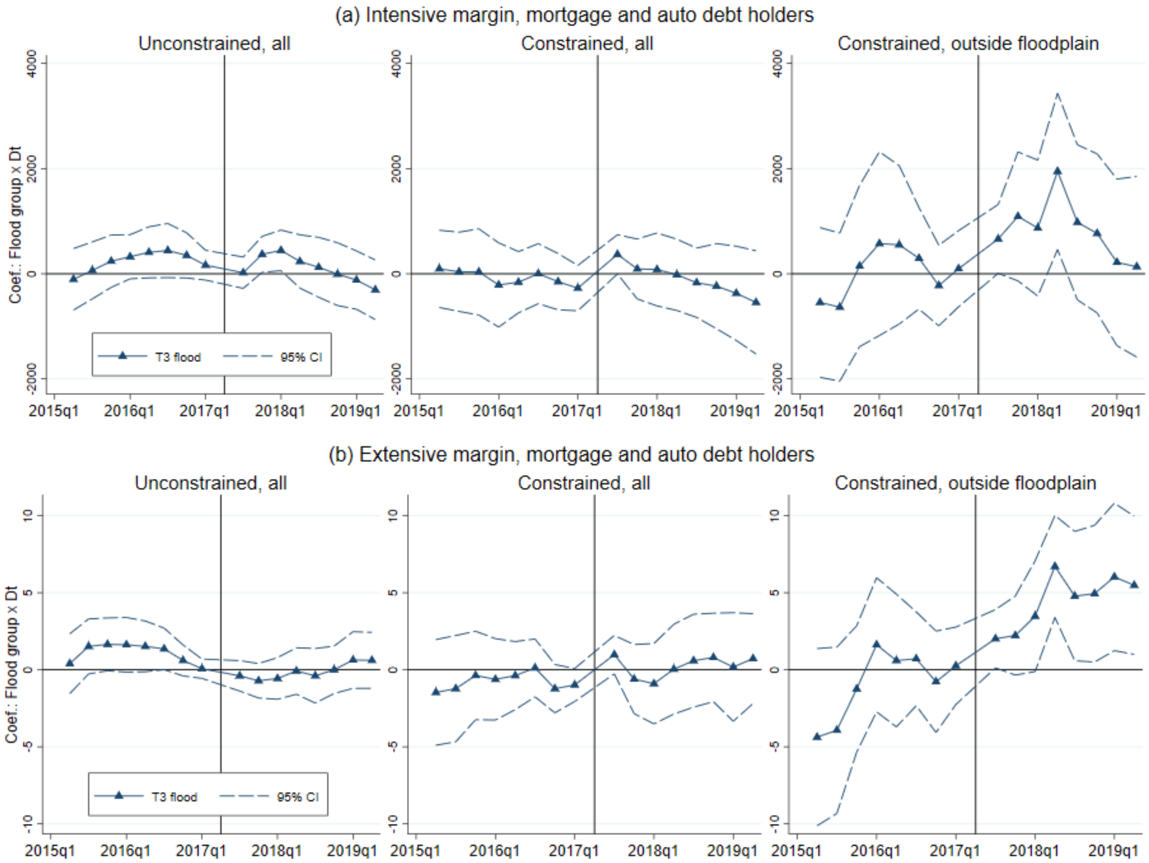

The figure plots the rate of severe delinquency (90 days past due) across individuals within blocks by flooding intensity of the block where the individual lived as of Q2 2017 and by date. For visual ease, blocks in the low (T1) and mid (T2) terciles are combined (red, squares). For student debt (which generally carries a high delinquency rate), we use 120 days past due. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A10: Mean of *Equifax Risk Score* by block flood intensity and date, individual-level




The figure plots the mean *Equifax Risk Score* across individuals by date and by the flooding intensity of the block where the individual lived as of Q2 2017. For visual ease, blocks in the low (T1) and mid (T2) terciles are combined (red, squares). Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A11: Difference-in-difference estimates of *Auto Debt*, individual-level




Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. In Panel (a), the dependent variable measures auto debt balances in dollars (intensive margin effects) conditional on having both a mortgage and auto debt balance as of Q2 2017. In Panel (b), the dependent variable measures the extensive margin probability (in percentage terms) of having an outstanding first mortgage balance conditional on the same factors. 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. Different columns correspond to different sample splits: first, according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017, respectively, and second, according to whether the individual lives in a block that is outside of the floodplain. For visual ease, in both panels, the low (T1) and mid (T2) terciles are dropped from the graph. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

Figure A12: Difference-in-difference estimates of *Consumer Financing Loans*, individual-level

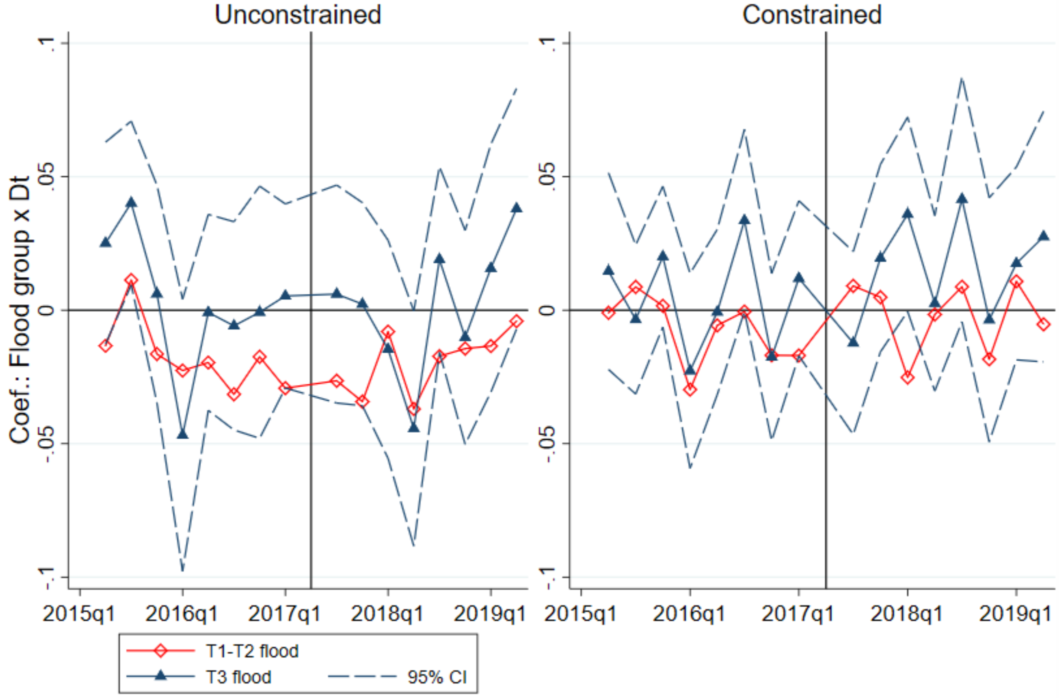

Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 2019. In Panel (a), the dependent variable measures consumer financing debt balances in dollars (intensive margin effects) conditional on having both a mortgage and consumer financing debt balance as of Q2 2017. In Panel (b), the dependent variable measures the extensive margin probability (in percentage terms) of having an outstanding consumer financing debt conditional on the same factors. 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. Different columns correspond to different sample splits: first, according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017, respectively, and second, according to whether the individual lives in a block that is outside of the floodplain. For visual ease, in both panels, the low (T1) and mid (T2) terciles are dropped from the graph. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

# B Credit supply

To accurately interpret any changes in individual credit outcomes associated with flooding, it is important to first establish that there was no obvious disequilibrium between demand for credit and supply of credit in Houston after the hurricane. A retraction in the supply of credit according to flood intensity, for example, would make it more difficult to interpret associated changes in debt outstanding and delinquencies as resulting from a wealth shock caused by flooding, as opposed to attributable to a retraction in access to credit.

Figure A13 displays the coefficients from event study regressions, where the sample is split according

Figure A13: Difference-in-difference event study estimates of *# of New Accounts per Inquiry*, individual-level




Figures plot event study coefficients from DiD regressions using the individual-level panel over Q2 2015–Q2 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). "T3 flood" signals that the individual lived in a block that was in the top tercile of flood depth among flooded blocks. The sample is split according to above (*Constrained*) and below (*Unconstrained*) median index levels of "Constraint" as of Q2 2017. Coefficients can be interpreted as the effect of being in a given tercile of flooding relative to the no flood group and relative to Q2 2017. For visual ease, in both panels, the low (T1) and mid (T2) terciles are combined into one bin. All regressions include the full array of fixed effects and controls described in Section 4. Data source: Federal Reserve Bank of NY/Equifax Consumer Credit Panel.

to financial constraint as of Q2 2017. The dependent variable is the the number of new accounts opened per credit inquiry – a measure of supply relative to demand. These results suggest that, for the most part, the supply of credit kept up with demand, even within the pool of constrained borrowers. Table A5 shows that this result holds even at the extremes of flooding (restricting the sample to only blocks that either experience no-flooding or top-decile flooding). Simply put, those individuals that were most affected by the flooding were not differentially denied credit on an ongoing basis after the hurricane.[56]

[56]Note that, although SBA disaster loans information is sometimes reported to credit bureaus like Equifax, that information is excluded from the CCP data. Therefore, access to SBA disaster loans is not part of this assessment of credit constraints.

Table A5: Difference-in-difference estimates of *# of New Accounts per Inquiry*, individual-level

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $T_b^1 \times P_t$ | 0.00 | 0.00 | | | |
| | (0.75) | (0.14) | | | |
| $T_b^2 \times P_t$ | -0.00 | -0.01 | | | |
| | (-0.52) | (-1.33) | | | |
| $T_b^3 \times P_t$ | 0.00 | -0.01 | -0.00 | -0.01 | -0.19 |
| | (0.08) | (-0.89) | (-0.13) | (-0.40) | (-1.08) |
| $T_b^1 \times P_t \times \text{Constrained}_i$ | | 0.00 | | | |
| | | (0.24) | | | |
| $T_b^2 \times P_t \times \text{Constrained}_i$ | | 0.02 | | | |
| | | (1.40) | | | |
| $T_b^3 \times P_t \times \text{Constrained}_i$ | | 0.01 | | 0.01 | 0.09 |
| | | (1.38) | | (0.43) | (0.53) |
| N | 482918 | 482918 | 210304 | 210304 | 164210 |
| AdjR2 | 0.06 | 0.06 | 0.06 | 0.06 | 0.06 |
| Y-mean | 0.30 | 0.30 | 0.31 | 0.31 | 0.31 |
| Sample | *All* | *All* | *Extremes* | *Extremes* | *Extremes & FlP=0%* |

Table presents DiD estimates using the individual-level panel over Q2 2015–Q2 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*). Treatment intensity is defined according to tercile bins of *WAvg. Flood Depth* in the post period. In some specifications, treatment is interacted with a dummy indicating that the individual had an above-median index level of "Constraint" as of Q2 2017 (*Constrained*). All associated secondary interactions (that are not perfect collinear with the fixed effects) are included, but not shown (for brevity). To provide a rough sense of the treatment-on-treated effect, Columns 3 and 4 restrict the sample to only blocks that either had no flooding or were in the top decile of flooded blocks. Column 5 further restricts the sample to blocks outside of the floodplain (*FlP=0%*). All regressions include the full array of fixed effects and controls described in Section 4. 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.

# C GIS flood mapping

The U.S. Geological Survey (USGS) worked in cooperation with the Federal Emergency Management Agency (FEMA) to document flooding from the Hurricane Harvey storm event in southeastern Texas and southwestern Louisiana. 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. The USGS is still in the process of refining flood depth maps due to the recent timing of Hurricane Harvey. The data release used in this paper is based on the most up to date information as of October 2018.[57]

In essence, the maps we use could be considered intent-to-treat from the rainfall of Harvey and the

[57] 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 into Houston and near the Cinco Ranch area.

topography of river basins in the Houston metropolitan area. Therefore, 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 encompass flooding due to the release of levees and dams during the storm as they are based on modeling actual river crests and planned water releases such as those in western Houston from the Addick and Baker Reservoirs.