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Household Financial Decision-Making After Natural Disasters: Evidence from Hurricane Harvey

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Abstract

We study household credit responses to Hurricane Harvey using new, geographically granular data on credit cards, mortgages, and flooding. Estimates from a differences-in-differences design that exploits the flooding gradient show that affected households only borrow at low-interest rates, often using promotional (zero interest) cards and that they quickly pay down balances. We also document that take-up of forbearance (borrowing by missing mortgage payments without penalty) increases with flooding. These results are attenuated in floodplains, particularly in structures subject by code to physical hardening. Our results indicate that credit acts as a substitute for the lack of physical hardening.

I. Introduction

How do households use credit markets to manage financial shocks from natural disasters? Despite the standard economic prediction of increased borrowing in response to a short-term liquidity shock, much of the previous literature on the *quantity* of the credit response to natural disasters has found that household borrowing is limited (Aladangady, Aron-Dine, Dunn, Feiveson, Lengermann, and Sahm (2016), Edmiston (2017), Gallagher and Hartley (2017), Deryugina, Kawano, and Levitt (2018), and Groen, Kutzbach, and Polivka (2020)). In this article, we study the *nature* of households' credit response in the aftermath of a

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disaster. We find that the borrowing that occurs is strategic, time-limited, and price-sensitive. We also show that households use borrowing as an ex post substitute for the lack of ex ante "hardening" of their homes.[1] To gather this new evidence, we draw on a unique weather event, a unique data source, and a unique source of variation in the "hardening" of homes created by a change in building codes.

We exploit the shock created by Hurricane Harvey, which flooded coastal Texas with more than 1 trillion gallons of water, over just 4 days, in late Aug. 2017. Parts of Houston, including those that do not typically flood, received more than 4 feet of rain (HCFCD (2018)). Harvey ranks as the largest rainfall event in U.S. history and, by 20th century climate standards, is considered a 1-in-2,000 year event (Emanuel (2017)). Harvey provides an ideal quasi-experiment because the confluence of lax zoning regulations, inaccurate flood risk mapping, and Harvey's unique hydrometeorological characteristics made it difficult for households to anticipate being affected and, hence, to sort into areas that ex post were more and less flooded. This lack of sorting allows us to recover causal estimates of Harvey's impact by exploiting the gradient in flooding. Specifically, we use a difference-in-differences design that compares the credit outcomes of households that were more and less flooded but otherwise similar.

To identify the effect of flooding on credit use, we merge a very spatially disaggregated data set containing detailed information on credit cards and mortgages with geographically granular maps of Harvey's flooding. Specifically, we use data on credit card and mortgage use, terms, and performance from the monthly Comprehensive Capital Analysis and Review FR Y-14M regulatory filings (hereafter Y-14), merged at the ZIP+4 level with 3-m resolution flood depth data. To the best of our knowledge, this is the first article to study the response of households to natural disasters using credit data of such granularity, frequency, and level of detail.

To study the effect of physical hardening, we exploit a revision to Houston's building code in 1985, which required elevated foundations for any new structures built inside designated floodplains. We use deed records to identify buildings built after 1985, and we use this information to document, for the first time, how this type of building code reform altered the relationship between disasters and credit use.

Our qualitative findings are drawn from a number of disparate but consistent pieces of evidence, which we summarize here. For simplicity, we compare the outcomes of "flooded" borrowers (those who experienced more than 1 foot of flooding) with "unflooded" borrowers (those who experienced less than 1 foot of flooding or no flooding).

Like previous research, we find that the hurricane had little impact on total credit card balances. But card *use* changed markedly following the storm. Among flooded borrowers, both charges and payments jumped following the storm and continued to rise for 4 months, peaking at about 100 in additional monthly expenditures/payments (about a 25% increase on a roughly 400 baseline for both). This increase in payments and charges was limited to the one-quarter of cards that were

[1]There is also an active literature focused on firms. This literature finds that disaster-related business disruptions are short-lived but costly (Agarwal, Fan, Klapper, and Lee (2021)). These losses tend to be uninsured, and firms rely heavily on credit to finance their recovery efforts (Collier, Powell, Ragin, and You (2020a), Collier, Haughwout, Kunreuther, and Michel-Kerjan (2020b)).

"convenience cards" (i.e., did not carry a revolving balance). Balances on revolving cards fell somewhat in flooded areas relative to nonflooded areas (by 50 on a pre-storm baseline of about 1,600), as payments on these cards increased more than charges.

Instead of borrowing at standard credit card rates, storm-impacted credit card borrowers turned to promotional cards, using them intensively but paying them down rapidly. On net, most incremental credit card borrowing induced by the storm was time-limited, on new promotional cards at lower interest rates. Specifically, in flooded ZIP+4s, the probability of a promotional card origination increased by about 40% from a baseline of 6.3% per month, and the probability of a nonpromotional card origination increased by about 20% from a baseline of 8% per month. Before Harvey, balances on new promotional cards peaked, on average, at $1,600 5 months after origination and fell to $1,400 12 months after origination. After Harvey, new promotional cards built up unusually large revolving balances. Incremental borrowing on post-Harvey cards peaked at about 800 above pre-Harvey card levels 2 months after origination but fell to pre-Harvey card levels by 12 months after origination (when most promotional cards reset to standard interest rates). Despite elevated storm-induced borrowing, rapid repayment before teaser rates reset implied that post-Harvey borrowing on new cards in flooded areas occurred at lower interest rates than analogous pre-Harvey borrowing. We document that credit card offers were constant in this period, implying that the increase in originations and borrowing likely reflected increased demand for cards and not increased supply.

Households also turned to mortgage forbearance offers, made available as part of the disaster response, as a form of low-cost borrowing, possibly as a bridge to insurance or government relief payments.[2] (Forbearance takeup appears as mortgage nonpayment in our data.[3]) Forbearance-based borrowing is particularly apparent in heavily flooded areas, where mortgage nonpayment rates increased by about 15 percentage points from pre-storm levels of roughly 7%. Because scheduled mortgage payments averaged about $1,100 per month, and households missed, on average, two payments, forbearance-based borrowing was modest in aggregate but substantial for the households that used it.

Even outside flooded areas, mortgage nonpayment rates nearly doubled, increasing by about 5 percentage points. Since we saw no increased credit card borrowing activity in nonflooded areas, we infer that these borrowers did not have the same type of liquidity need as borrowers in flooded areas, and they may have used forbearance "strategically" (i.e., they likely would not have skipped payments absent the forbearance offers). Even so, these borrowers may have experienced income interruptions from the storm, which we cannot see.

Next, we explore whether borrowing acted as a complement or a substitute with other risk-management tools. If credit is a substitute for ex ante risk

[2]This finding is in line with those of Kousky, Palim, and Pan (2020), who use data matching ex post flood *damages* to mortgage performance—a different exercise than looking at forbearance conditional on *flooding*.

[3]In Section IV.C, we present evidence that forbearance was widely offered and automatic for most borrowers, making increases in nonpayment a good proxy. We also show that borrowers did not experience declines in their credit scores following missed payments in the post-Harvey period.

management, the credit response may be muted in the floodplain, where borrowers are more likely to undertake self-protection actions and may be required to purchase insurance. Indeed, borrowers outside the floodplain drove the credit response. Increases in credit card originations among flooded borrowers were concentrated outside the floodplain, and borrowing on these new cards was higher, peaking at about $250 per foot of flooding 2 months after origination among cards originated 1–3 months after Harvey – an increase that was about twice as large as for cards originated in the floodplain. Mortgage nonpayment showed a similar pattern, with flooded households outside the floodplain driving the increase in nonpayment. For mortgage borrowers, being in a floodplain roughly halved the incremental nonpayment rate associated with an additional foot of flooding.

While the previous literature (like Kousky et al. (2020), Billings, Gallagher, and Ricketts (2022)) attributes lower distress in floodplains to insurance requirements, we show that physical hardening explains much of the reduction in credit use. Forbearance take-up in the floodplain is much lower for the post-1985 structures subject to the foundation elevation requirement. (This evidence is available only for forbearance takeup and not credit card use because we can match mortgages but not credit cards to deeds.) The effect of updated building codes nearly offsets the effect of flooding: flooded borrowers in houses subject to the enhanced building codes missed payments at about the same rate as nonflooded borrowers. Outside the floodplain, among flooded borrowers, we find no relationship between the year a house was built and the mortgage's forbearance propensity, ruling out generic improvements in construction as an explanation.

Finally, we find that direct (though imperfect) measures of flood insurance coverage are not associated with lower (or higher) credit use following the storm once the role of income is accounted for. However, higher rates of insurance seem to facilitate some paydown of expensive credit card debt – behavior that is consistent with the general pattern of price-sensitive debt use and repayment we find across credit types.

Our findings contribute to several strands of literature. Most narrowly, they speak to the literature on the credit response of households to natural disasters, as we can characterize in more detail the borrowing response of households. Importantly, our findings document the amount of borrowing and the type and duration of that borrowing. The characterization of the borrowing response also informs two strands of the literature on adaptation to climate shocks and the federal government's role. First, we extend the findings of previous papers (e.g., Kousky et al. (2020), Billings et al. (2022), and Gallagher, Hartley, and Rohlin (2023)) by showing that adaptation measures like updated building codes play an important role in reducing households' reliance on credit markets after natural disasters – even if, as Wagner (2022) finds, these adaptations also reduce insurance propensities. We also show that even in cases where robust federal aid is available, private credit markets damp the financial consequences of hurricanes (e.g., through bridge-lending). Second, we extend the findings of Collier and Ellis (2021) by showing that price-sensitive borrowing is not specific to post-disaster loans from the federal government (through the Small Business Administration) but that the price-sensitive borrowing response to natural disasters is also observed in private credit markets.

More broadly, our findings help us understand the role of liquidity shocks in household borrowing. A large, nondisaster literature shows that typical U.S. borrowers often rely on expensive, repeated sources of credit and routinely fail to take advantage of arbitrage opportunities (Laibson, Repetto, and Tobacman (2000)). This behavior has been documented both in secured long-term borrowing arrangements such as mortgages (e.g., Woodward and Hall (2012), Davidoff (2015), Agarwal, Rosen, and Yao (2016), Keys, Pope, and Pope (2016), and Agarwal, Ben-David, and Yao (2017)) and in unsecured short-term borrowing arrangements such as those offered by payday lenders or credit card companies (Ausubel (1991), Agarwal, Skiba, and Tobacman (2009), Laibson, Agarwal, Gabaix, and Driscoll (2009), Agarwal, Chomsisengphet, Liu, and Souleles (2015), Lusardi and Tufano (2015), Ponce, Seira, and Zamarripa (2017), Hundtofte, Olafsson, and Pagel (2019), and Keys and Wang (2019)). Understanding whether this observed behavior is suboptimal (e.g., driven by consumers' biases, lack of information, financial literacy, or cognitive limitations) is crucial to designing consumer financial protection regulations (Campbell, Jackson, Madrian, and Tufano (2011)). Hurricane Harvey provides a reason for an unlucky but fairly typical group of U.S. residents to borrow, even if they may not have been frequent borrowers in the past. These individuals induced by storm damage to borrow do not seem to use credit in the costly, recurring, and problematic ways documented in more frequent borrowers. Instead, their borrowing appears to be price-sensitive and time-limited. One implication of these findings is that even in an area hit by a 1-in-2,000 year flood, the resultant short-term shocks to liquidity are not a significant source of long-term credit card borrowing.

Importantly, this finding is unlikely to be driven by changes in the regional or macroeconomic environment, as our unique source of variation and data set allow us to control for these factors. Note that our shock is the experience of having a flooded structure, with any accompanying costs and access to insurance and government programs. By using nonflooded structures as controls, we can account for Hurricane Harvey's regional or macroeconomic impact. This feature of our design is important because it additionally allows us to inform the debate over the response of borrowers to good or bad macroeconomic/regional conditions (e.g., Mian, Rao, and Sufi (2013), Keys, Tobacman, and Wang (2017), and Hundtofte et al. (2019)) where the effects of liquidity shocks can be difficult to disentangle from the effects of the macro environment. Specifically, we show that in the absence of these regional or macroeconomic shocks (including to expectations), the liquidity shocks induced by natural disasters are unlikely to drive substantial increases in Americans' aggregate revolving balances, even if these events become much more frequent.

II. Data

A. Flooding, Floodplain, Flood Insurance, and Demographic Data

To assess the severity of the flooding caused by Hurricane Harvey, we use the high-water-mark (HWM) depth grids created by the Federal Emergency Management Agency (FEMA). The FEMA (2017a) data set is a raster image composed of

3.2 billion grids (pixels). Each grid reports the maximum depth of Harvey flooding in feet (ft) and has an area of 3 square meters ($\approx$ 9.8 square feet). In our analysis, we use the depth grids that cover Harris County (Houston) and the coastal counties of Aransas, Nueces, and San Patricio, where Harvey made landfall. Graph A of Figure 1 presents depth grids for Harris County, which makes up the bulk of our sample.[4]

We use the FEMA depth grids to assign flood depth at the ZIP+4, the smallest geographic unit available in our credit data. First, we overlay the footprints of houses and buildings from aerial imagery (FEMA (2017b)) on the depth grid and calculate the maximum flood depth around each structure. Next, we use the ZIP+4 centroid coordinates provided by a private shipping company (Pitney Bowes (2018)) to locate all structures within 100 m ($\sim$328 feet) of a ZIP+4 centroid. We then calculate the flood depth for the structures that fall within that 100 m radius and assign that value to the ZIP+4. Graph B of Figure 1 illustrates this calculation.

Hurricane Harvey was unusual because many affected households were outside the floodplain. Floodplain designation affects insurance requirements, building codes, and, arguably, households' understanding of their vulnerability to flooding. To assign floodplain status, we use the national flood hazard zones map that was current at the time of Hurricane Harvey (FEMA (2017c)). We overlay our ZIP+4 centroids on the flood insurance risk maps for the counties that make up our sample and assign to each ZIP+4 the official FEMA flood zone designation. We define a ZIP+4 to be in the floodplain when it has a 1% chance of flooding each year or when it is at high risk from storm surge (i.e., FEMA flood zone types A and V). This is the risk threshold used for flood insurance requirements and for physical hardening standards (elevating structures) in Houston-area building codes.

While borrowers in floodplains are technically required to carry flood insurance if they have a mortgage, compliance with this rule is far from perfect (Michel-Kerjan (2010)). We measure insurance penetration directly by calculating the share of structures with an active policy at the time of Hurricane Harvey. Specifically, we divide the counts of active policies at the census tract level provided by FEMA (2017d) by the count of structures derived from aggregating FEMA (2017b) to the same level. We then assign to each ZIP+4 the calculated value of the census tract share of insured structures.

Finally, it is reasonable to expect that floodplain status and insurance take-up are correlated with income and that income also influences borrowers' behavior conditional on flooding. For that reason, when we evaluate how floodplain status and insurance affect flood-induced borrowing, we control independently for the role of median household income, which we measure at the census block group level using data from the American Community Survey (US Census Bureau (2017)).

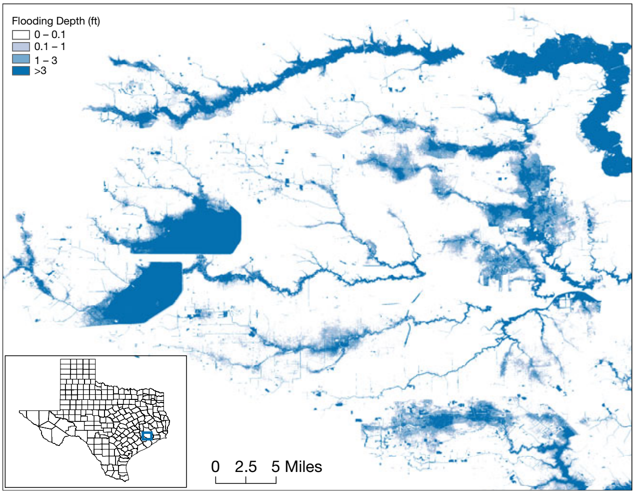
[4]To construct HWM grid depths for Harvey, hydrographers from the U.S. Geological Survey visited the affected counties between Sept. 2 and Oct. 10, 2017, and recorded the height of flooding at 2,755 points. HWM provide information on maximum flooding because hydrographers record the distance between the ground and the highest mark left on objects exposed to the water. FEMA then interpolates these points to construct a flood surface depicting the maximum water height. Flooding depth is derived, in turn, by subtracting the flooding surface from (3-m resolution) LIDAR terrain data.

FIGURE 1

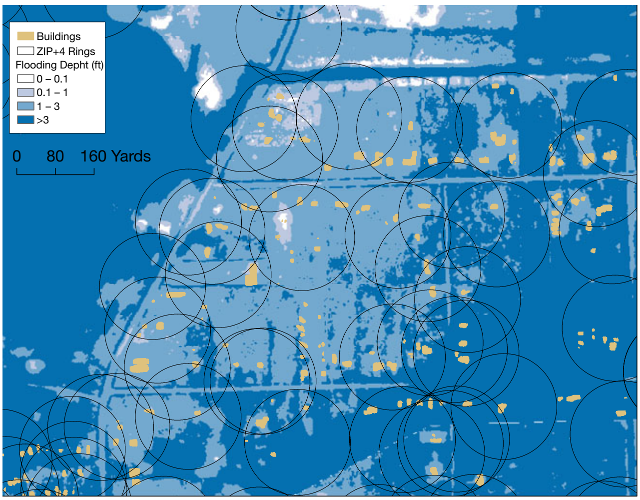
Flooding Extent and Calculation of ZIP+4 Level Flooding

Graph A of Figure 1 shows the map of the flooding created by Hurricane Harvey in Harris County, Texas. The darker the shade of blue, the greater the depth of flooding. Graph B plots flooding caused by Hurricane Harvey in Redwood Estates, Houston, TX 77044. It also plots a 100 m (328 foot) ring around each ZIP+4 centroid, and structure footprints shaded in yellow. ZIP+4 average flooding is calculated by determining the maximum flooding that each structure experienced and then averaging among all structures that are located within a ZIP+4 ring.

Graph A. Flooding from Hurricane Harvey in Harris County, Texas



Graph B. Illustration of Flooding Calculations for a ZIP+4



B. Credit Data

We draw data on the terms and performance of mortgages and credit cards from the Comprehensive Capital Analysis and Review FR Y-14M filing (hereafter Y-14).[5]

[5]All Y-14 data were pulled in Nov. 2020.

The Y-14 filing collects monthly loan-level credit card, first mortgage lien, and home equity loan data from large bank holding companies and intermediate holding companies subject to capital assessments and stress testing.

Y-14 filers covered roughly 90% of credit cards in the marketplace at the time of the storm (though coverage has since declined) and about half of the mortgages.[6] The credit card and mortgage data schedules are separate and do not accommodate merges at the household level. We evaluate mortgage and credit card outcomes independently of one another.

Hurricane Harvey's warning was issued on Aug. 23, 2017. Harvey made landfall on Aug. 25, with substantial rainfall continuing in the Houston area until Aug. 29. Accordingly, we consider September the first treatment month. We monitor credit card and mortgage borrowers for 24 months before Hurricane Harvey and 12 months after so that our data span Sept. 2015 to Aug. 2019.

1. Credit Card Data

We use two credit card data sets, which we assemble from the Y-14 credit card schedules. First, we use a sample of outstanding cards to evaluate credit card use on the intensive margin. Second, we use data on newly issued cards to evaluate the extensive margin of credit card use.

We discuss data quality filters and sample composition in Section A of the Supplementary Material. Because of the large number of outstanding cards, we sample for computational feasibility. We use a 5% subsample of existing cards in nonflooded areas and 100% of existing cards in areas that experienced any measurable flooding. We retain all-new card originations for our analysis of the extensive margin.

We track four key measures of credit card use on the intensive margin: charges, payments, revolving balances, and new 30-day delinquencies. Charges refer to the total purchase volume on that card in a given month. Payments refer to the total actual payment amount received for that card in a given month. The revolving balance is the cycle-ending balance from the previous month minus payments in the current month; for interest-bearing cards, the revolving balance reflects the amount on which any interest is owed.

For analysis using existing cards, we exclude cards that originated after Jan. 2017. We also limit our sample to cards that we define as "active": cards that carry balances or have been used for purchases (charges) within the previous 6 months. Our results are robust to these filters, but point estimates are smaller because the response is concentrated on actively used cards.

For analysis of the extensive margin of credit use, we additionally track whether the card is under promotion at origination (i.e., whether purchase and transfer balances are carried at market or discounted interest rates). The card's promotional status is a calculated field, described in more detail in Section A of the Supplementary Material.

In our sample, about 13% of the unique, active credit cards experienced more than 1 foot of flooding. (An additional 5% had nonzero flooding of less than 1 foot.)

[6]Because the Y-14 filers are large bank servicers, the mortgage data include fewer Federal Housing Administration loans and more portfolio-held loans than the market as a whole.

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Flood depth among these flooded borrowers averaged 2.75 feet, with roughly 60% of these flooded borrowers residing outside the floodplain.

Table A1 in the Supplementary Material shows origination characteristics for cards issued during our observation period from June 2016 to Aug. 2018. Table A2 in the Supplementary Material shows the same origination characteristics for cards issued in 3 months before the hurricane separately for each category of observed flooding intensity. No clear differences emerge in the origination characteristics of affected and unaffected borrowers.

2. Mortgage and Property Data

The mortgage data include granular geographical information (ZIP+4), monthly performance (delinquency status, prepayment, servicing transfer, modification status), updated credit score, and detailed origination information.

We merge information about the build year of the structure at the property level using CoreLogic data drawn from deed records. We are able to merge property information for mortgages but not for credit cards because the Y-14 filings include the property address for each mortgage, whereas the credit card schedule includes only the ZIP+4. While these data offer a very high match rate with the mortgage data, they ended in 2014, and records for substantially renovated properties in the intervening 3 years will be stale.

Table A3 in the Supplementary Material shows the distribution of these characteristics during our sample period. Table A4 in the Supplementary Material shows how these characteristics vary by flood depth in the 3 months before the storm hits.

III. Empirical Strategy

We estimate a two-way fixed effects model that relates credit market outcomes to the level of flooding created by Hurricane Harvey. Our preferred specification is the following:

$$(1) \qquad Y_{czt} = \sum_{\tau=-24}^{12} \beta_\tau \cdot D_{ct}^{\tau} \times F_z + \alpha_c + \alpha_t + \varepsilon_{czt},$$

where Y_{czt} denotes the outcome of credit line c in ZIP+4 z and month t, α_c is a credit-line fixed effect, α_t is a year-month fixed effect, and $D_{ct}^{\tau} = 1\{t - \tau^* = \tau\}$ is an indicator variable for being τ months away from Aug. 23, 2017 (τ^*) when the National Weather Service issued the first hurricane watch for Texas. The variable F_z measures the depth of flooding in feet created by Harvey at the ZIP+4 level.[7]

For continuous outcomes (charges, payments, balances), we estimate equation (1) using ordinary least squares. For binary outcomes (mortgage nonpayment and credit card delinquencies), we use a linear probability model. In cases with predicted probabilities outside the unit interval, such as the indicator of observing at least one credit card origination, we use a logit model.

[7]Note that $\sum_{\tau=-24}^{12} \beta_\tau \cdot D_{ct}^{\tau}$ is subsumed by the time-fixed effects, and F_z is subsumed by the credit-line fixed effects.

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We cluster standard errors at the ZIP+4 level. All regressions using credit card data are weighted to account for the sampling framework described in Section II.B.1. Because the hurricane watch occurred at the end of August ($\tau = 0$), we interpret the β_τ coefficients for $\tau \leq 0$ as corresponding to the pre-Harvey period (leads of treatment). Accordingly, the β_τ coefficients for $\tau > 0$ correspond to the post-Harvey period (lags of treatment). We normalize the coefficient on $(\tau = 0)$ to be equal to 0.

We argue that the β_τ coefficients for $\tau > 0$ have a causal interpretation and that they describe the evolution over time of the average causal response to flooding under the assumptions of no anticipation effects and parallel trends. In our application, both assumptions are likely to hold.

We argue that the assumption of no anticipation is well-founded because affected borrowers could not have foreseen the exact timing or distribution of the flooding created by Harvey. Specifically, with regards to the timing of the hurricane, forecasts including long-range hurricane tracks (which are the earliest indicators of a possible strike) and Harvey's landfall occurred in the same calendar month as the storm hit. This circumstance implies that our treatment encompasses both the hurricane's announcement and landfall. Since households are unlikely to anticipate the forecast, anticipatory behavior should not affect our results.

Similarly, several pieces of evidence suggest that the spatial distribution of flooding created by Harvey was largely unanticipated. First, the commonly used marker for flood risk, the FEMA 100-year floodplain boundaries, are very inaccurate in areas with the topographic characteristics of Harris. For example, Blessing, Sebastian, and Brody (2017) show that in Harris, up to 80% of losses from flooding created by smaller pre-Harvey events occurred outside of the 100-year floodplain. Second, developers have taken advantage of loopholes in the redesignation process to sell properties nearly at the level of the 100-year floodplain but without having to disclose to buyers that these are high-risk properties (Schwartz, Glanz, and Lehren (2017)). Third, even in areas with accurate maps and informed buyers, residents with correct priors about the flooding distribution created by 1-in-100 year events could not have anticipated the flooding distribution of a 1-in-2,000 years event like Harvey. Consistent with these ideas, Billings et al. (2022) show that a wide range of geographic and socio-economic characteristics explains less than 7% of the spatial variation in Harvey flooding. Taking these reasons together, we conclude that it is unlikely that households could have accurately anticipated the spatial distribution of flooding.

The parallel trends assumption in our application requires that the average change in outcomes across ZIP+4s would have been invariant to the flooding exposure from Hurricane Harvey *had the storm not happened.* Two pieces of supporting evidence indicate that this is a reasonable assumption. First, we test and are unable to reject, for almost every outcome, that treatment leads $\tau \leq 0$ are statistically different from 0. Note that this test is valid because our event date is common (no differential timing) and because we can rule out anticipation effects. Accordingly, the contamination effects described in Sun and Abraham (2020) for this type of test are not present in our application. Second, we show in Tables A2 and A4 in the Supplementary Material that in terms of pre-Harvey credit outcomes, households are similar across levels of flooding.

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While we conduct our initial analysis using specification (1), this specification reports 36 β_τ coefficients of interest. To provide a concise summary of the impact of Harvey, we report results in table format using a modified version of specification (1), where we bin the D^{τ}_{ct} indicator variable. The combined lags are τ = 1–3, 4–6, 7–9, and 10–12. The combined leads are $\tau = -24$ to -3, -2 to 0. We normalize the last bin before Harvey ($\tau = -2$ to 0) to be 0.

Lastly, our preferred specification (equation (1)) may not always provide a good summary of the impact of Harvey. For example, we find substantial non-linearity in the flood response at around 1 foot of flooding. For this reason, we also present coefficient plots derived from a modified version of specification (1) where we discretize F_z into two groups (more and less than 1 foot of flooding, where the group with "less than 1 foot of flooding" includes ZIP+4s that experienced no flooding). Similarly, when there is substantial heterogeneity in an outcome in response to flooding intensity, we will report coefficient plots for four groups: those in ZIP+4s with 0–0.1 feet of flooding, 0.1–1 feet, 1–3 feet, and over 3 feet of flooding.

IV. Results

A. Charges, Payments, Balances, and Delinquency on Existing Cards

We begin by studying the impact of Hurricane Harvey on the use of existing credit lines. Figure 2 plots the differential impact of Harvey over time on charges, payments, and revolving balances between those exposed to more and less than 1 foot of flooding. The figure reveals that consumers used existing market-rate cards to spend but not to borrow. Specifically, we observe that while Harvey leads to an increase in credit card charges, consumers can avoid expensive borrowing by immediately matching the increase in charges with credit card payments.[8] Figure 2 additionally provides strong supporting evidence for the causal interpretation of these coefficients because it shows that pre-trends for charges and payments are very similar between more and less flooded areas. Consistent with the previous results, we also find small negative effects on revolving balances. One important caveat is that the figure reveals a downward pre-trend for revolving balances.[9]

To better understand the magnitude of Harvey's impact, Table 1 summarizes the previous results and investigates whether these effects are driven by credit lines that were frequently used to borrow. Specifically, we extend specification (1) and include an interaction term with an indicator variable that takes the value of 1 for credit lines that carry a revolving balance before Harvey. The table also presents results for the likelihood of delinquency on existing cards.

The coefficients on the main effects in columns 1 and 2 show that charges and payments move in lockstep among cards without a pre-Harvey revolving balance. The most significant increase in charges and payments (roughly \$70 per foot of flooding) is observed 4–6 months after Harvey's landfall. Because average flooding

[8]Payments lag charges by one period. The initial jump in payments observed in period 2 more than offsets the increase in charges observed in period 1, reducing revolving balances.

[9]The revolving balance is calculated as the cycle-ending balance of the previous period minus payments. If the indicated balance is negative, the revolving balance is 0.

FIGURE 2

Harvey's Impact on Charges, Payments, and Revolving Balances

Figure 2 plots point estimates and 95% confidence intervals of the differential impact of Harvey between those exposed to more and less than 1 foot of flooding. Specifically, the coefficients are derived from three separate OLS regressions of specification (1) (for revolving balances, charges, and payments as dependent variables, respectively) where we discretize $F_{z\tau}$ into two groups (more and less than 1 foot of flooding). Displayed coefficients show increases in balances (or charges, or payments) in the high-flood area relative to the low-/no-flood area (in a given month for a given ZIP+4), relative to the most immediate pre-storm benchmark month. All regressions include credit-line and month-year fixed effects. Confidence intervals are derived from robust standard errors clustered at the ZIP+4 level. Specification (1) reports 36 coefficients (i.e., β_{-24} to β_{12}). To avoid multi-collinearity, we normalize β_{-24} and β_0 to be equal to 0. Unless it is informative, we provide more concise results by only plotting coefficients for β_{-12} to β_{12}. Section II.B.1 provides definitions of all variables and describes the weights used. The sample includes all active cards.

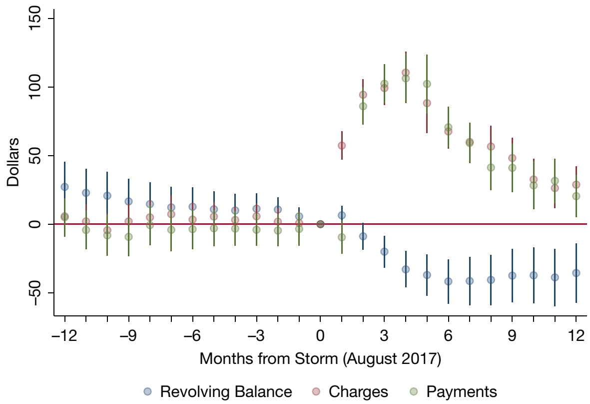

TABLE 1

Storm-Induced Response on Active Cards

Table 1 presents estimates from four separate OLS regressions (for charges, payments, revolving balances, and delinquency, respectively) that interact the specification described in equation (1) with the indicator variable equal to 1 if the borrower had a revolving balance in July 2017. The depth variable measures the average ZIP+4 level of flooding created by Harvey in feet. All regressions include credit line and month-year fixed effects. Robust standard errors clustered at the ZIP +4 are presented in parentheses. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively. Section II.B.1 provides definitions of all variables and describes the weights used.

	Charges	Payments	Revolving Balance	New 30+ Day Delinquency (ppt)
	1	2	3	4
1–3 MTHS_POST × DEPTH	58.151***	38.578***	4.148*	0.006
	(4.824)	(4.302)	(1.763)	(0.004)
4–6 MTHS_POST × DEPTH	70.198***	73.438***	0.942	−0.001
	(11.347)	(11.774)	(2.591)	(0.004)
7–9 MTHS_POST × DEPTH	40.346***	40.142***	1.549	−0.006
	(7.384)	(7.276)	(3.111)	(0.005)
10–12 MTHS_POST × DEPTH	19.932**	25.870***	−0.617	−0.007
	(7.492)	(7.747)	(3.378)	(0.006)
1–3 MTHS_POST × DEPTH × REVOLVES	−47.988***	−28.902***	−12.097***	−0.021
	(4.858)	(4.381)	(2.604)	(0.014)
4–6 MTHS_POST × DEPTH × REVOLVES	−59.597***	−59.607***	−22.397***	−0.046**
	(11.377)	(11.786)	(3.948)	(0.014)
7–9 MTHS_POST × DEPTH × REVOLVES	−33.423***	−33.712***	−28.268***	−0.021
	(7.424)	(7.346)	(4.900)	(0.014)
10–12 MTHS_POST × DEPTH × REVOLVES	−15.987*	−22.363**	−28.422***	−0.027
	(7.600)	(7.888)	(5.393)	(0.016)
No. of obs.	15,501,407	15,501,407	15,501,407	14,350,490
R^2	0.638	0.569	0.811	0.119

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in areas with nonzero flooding is about 1.5 feet, these coefficients imply that Harvey increased charges and payments by roughly \$100. Consistent with these results, column 3 reports the effects of Harvey on revolving balances that are small and statistically indistinguishable from 0.[10]

The coefficients on the interaction with the indicator variable *revolves*, in columns 1–3, show that the response in charges, payments, and balances is largely limited to cards without revolving balances before the storm. We see very little change in charging activity on cards with revolving balances and a slight net paydown in revolving balances, suggesting that Harvey led to increased use of credit cards for purchases but not borrowing. These results also indicate that households use different cards for borrowing and purchasing or that Harvey increased the use of cards for purchases among households that seldom use their cards for borrowing.

We find very little change in the probability that a card will become newly delinquent. This finding is unsurprising on nonrevolving cards, where delinquency rates are extremely low. For cards with revolving balances, we see a transient, substantial reduction in new delinquency rates. This finding echoes the slight paydown in revolving balances on these cards, suggesting some excess insurance or government assistance was directed toward payments on revolving cards – borrowers' most expensive form of credit.

On the whole, our findings extend those of Gallagher and Hartley (2017), who find small, transient impacts of Hurricane Katrina on credit card balances but who cannot distinguish between spending and borrowing. Specifically, we show that the muted effect in balances is the result of charges and payments surging together in affected areas. Additionally, we show that these effects are driven by the use of existing credit lines that are infrequently used for borrowing.

B. Origination and Subsequent Use of New Cards

Although the previous section showed no average increase in intensive borrowing (carrying additional balances on existing credit cards), borrowers may have increased borrowing on the extensive margin. That is, affected borrowers may have originated new cards and carried balances on those cards.

To study the impact of Harvey on the number of credit card originations and their use, we use a panel of credit card originations at the ZIP+4 month level consisting of the count of new promotional cards (cards with temporary, low-interest rates), the count of new standard cards, and the total count of both promotional and standard originations. Because less than 5% of ZIP+4s report more than one card origination in any given month, we code every ZIP+4-month unit as either reporting 0 or at least one origination. This choice implies that the card origination outcome is a binary variable.

Figure 3 plots point estimates and 95% confidence intervals derived from a logit specification where the outcome is the ZIP+4-month level card origination

[10]We also tested—and failed to find evidence—that our results could be driven by a reduction in charges and payments among the nonflooded group instead of an increase among the flooded group. Specifically, we find no substantial deviation from trend for the nonflooded group after Harvey.

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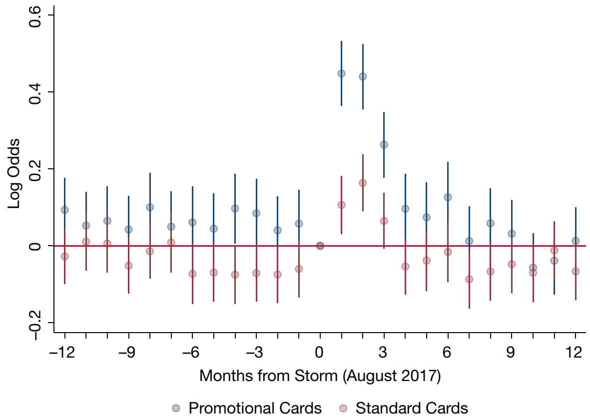
FIGURE 3

Harvey's Impact on Card Originations

Figure 3 plots point estimates and 95% confidence intervals of the differential impact of Harvey between those exposed to more and less than 1 foot of flooding. Specifically, the coefficients are derived from two separate logit estimations of specification (1) (for promotional cards and standard cards, respectively) where we discretize $F_{z\tau}$ into two groups (more and less than 1 foot of flooding). Displayed coefficients show increases in log odds of a new origination in the high-flood area relative to the low-/no-flood area (in a given month for a given ZIP+4), relative to the most immediate pre-storm benchmark month. All regressions include month-year fixed effects. Confidence intervals are derived from robust standard errors clustered at the ZIP+4 level. Specification (1) reports 36 coefficients (i.e., β_{-24} to β_{12}). To avoid multi-collinearity, we normalize β_{-24} and β_0 to be equal to 0. Unless it is informative, we provide more concise results by only plotting coefficients for β_{-12} to β_{12}. Definitions of all dependent variables can be found in Section II.B.1.



indicator.[11] In the months after Harvey in affected areas, the log odds of a new card origination increase. This increase is particularly notable among promotional cards, where the log odds increase by about 0.3–0.4 for borrowers who experience at least 1 foot of flooding (or roughly 30%).[12] The figure also shows no evidence of a pre-trend in originations for either card type, supporting our parallel trends assumption.

Table 2 presents results for total card originations and separate results for promotional and standard cards. As before, we estimate a logit model and summarize the results into 3-month bins. Column 1 shows that Harvey leads to an overall 6% per foot of flooding temporary increase in the odds of observing a card origination. Columns 2 and 3 highlight that, consistent with the result of Figure 3, the temporary increase is driven by a large spike in promotional card originations, which experience an increase of 9% per foot of flooding. By comparison, we find that standard card originations experience a smaller 4% increase per foot of flooding.

While these results could be consistent with either an increase in the demand or the supply of credit, two pieces of evidence suggest that an increase in demand best explains the findings. First, consistent with the idea that issuers did not increase credit supply following Harvey, previous work using data on consumer credit files

[11]This specification does not include ZIP+4-level fixed effects. We have also tested a linear probability model that includes ZIP+4-level fixed effects and produces nearly identical results.

[12]The average probability of card origination is relatively low, at 15% per month—8% for nonteasers and 6% for teasers. These low probabilities allow us to approximate a change in log odds as roughly equivalent to the same percent change.

TABLE 2

Storm-Induced Card Originations

Table 2 presents three separate logit estimates from specification (1). The depth variable measures the average ZIP+4 level of flooding created by Harvey in feet. All regressions include month-year fixed effects. Robust standard errors clustered at the ZIP+4 are presented in parentheses. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively. The sample includes all originations between Jan. 2016 and Aug. 2018 for borrowers with mailing addresses in Harris, Aransas, Nueces, and San Patricio counties in Texas at the time of Hurricane Harvey. Definitions of all dependent variables can be found in Section II.B.1.

	All Cards	Promotional Cards	Standard Cards
	1	2	3
1–3 MTHS_POST × DEPTH	0.0585***	0.0853***	0.0415***
	(0.00515)	(0.00730)	(0.00651)
4–6 MTHS_POST × DEPTH	0.00543	0.0151	−0.00101
	(0.00578)	(0.00867)	(0.00737)
7–9 MTHS_POST × DEPTH	−0.00774	−0.00738	−0.00547
	(0.00574)	(0.00837)	(0.00727)
10–12 MTHS_POST × DEPTH	−0.0147**	−0.0219*	−0.00665
	(0.00568)	(0.00856)	(0.00706)
No. of obs.	12,096,717	12,096,717	12,096,717
Pseudo R^2	0.003	0.003	0.003

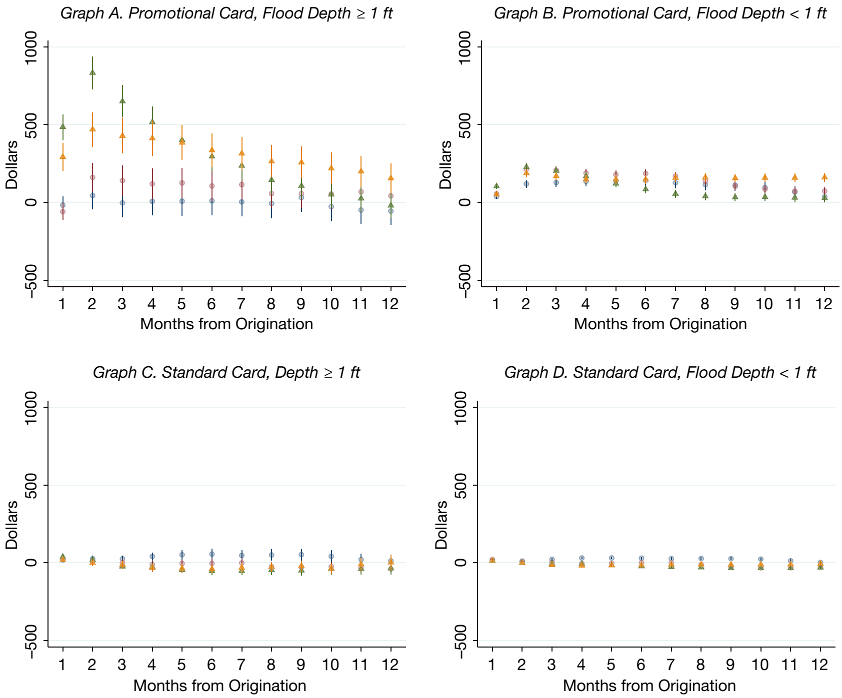
shows no change in the ratio of new accounts to credit inquiries, both overall and by the level of flooding before and after Harvey (Billings et al. (2022)). Second, we use Compremedia's Mintel (2022) data set to test whether credit offers increased after Harvey. This monthly survey allows us to track the number of credit card offers mailed. While the data are not granular enough to allow us to distinguish between flooded and nonflooded households, it is unlikely that issuers would have sufficient data on flooding immediately following the storm to tailor offers specifically to affected households. Therefore, we speculate that the entire Houston area may have been targeted. Figure A1 in the Supplementary Material plots the average number of offers per person in the Houston area – calculated per ZIP code and averaged across all Houston area ZIP codes– in the months before and after Harvey. For comparison, we plot the analogous number of credit offers mailed in the USA and in the Atlanta area. The figure reveals that the number of offers made in Houston does not change materially after Hurricane Harvey and that the temporal patterns for Houston, Atlanta, and the nation are all similar. Note that the small number of households in the sample in any region leads to noisy monthly averages in Houston and Atlanta. Consistent with the figure, a formal test shows no statistical difference between the pattern of mailings between these groups before and after Hurricane Harvey. We conclude therefore that issuers are unlikely to have increased the credit supply in response to Harvey and that the increase in card origination is likely driven by increased demand for credit.

To study how new cards are used, we return to the Y-14 data. Figure 4 plots average revolving balances over time by 3-month cohort of origination, type of card, and level of flooding. Graphs A and B present results for promotional cards in areas affected by more and less than 1 foot of flooding, respectively. Graphs C and D present analogous results for standard cards. In the figures, triangle markers correspond to origination cohorts after Harvey, and circle markers correspond to origination cohorts before Harvey. The figures show that balances

FIGURE 4

Average Revolving Balance by Origination Cohort, Card Type, and Flood Level

Graphs A–D of Figure 4 plot coefficients representing the difference in monthly average revolving balance relative to a pre-period spanning June 2015 to Feb. 2017. Each series is comprised of a 3-month cohort of originations. Triangle markers correspond to origination cohorts after Harvey (green is Sept. to Nov. 2017, yellow is Dec. 2017 to Feb. 2018). Circle markers correspond to origination cohorts before Harvey (blue is Mar. to May 2017, red is June to Aug. 2017). Results are shown separately for areas that experienced over 1 foot of flooding and areas that experienced less than 1 foot of flooding or no flooding. These results are implemented in a regression, with coefficients for months-from-origination for each origination cohort, with the pre-period group used as a baseline. Robust standard errors are clustered at the ZIP+4.



on these new promotional originations in hurricane-affected areas following Harvey are much larger than is typical at other times and in other areas and are paid off faster. Specifically, Graph A shows that average revolving balances on new promotional cards in hurricane-affected areas are much higher immediately after Harvey than before it (incremental borrowing on promotional cards peaks at approximately $800 2 months after origination). We also find that revolving balances on promotional post-Harvey originations in the most affected areas fall precipitously over the first 12 months, and the incremental storm-induced balance is paid off within the first year. By comparison, cards that originated before Harvey in less affected areas (Graph B) and cards that charge standard interest rates (Graphs C and D) show effectively no storm-induced change in balances during the first year of the loan. In sum, incremental borrowing on new cards induced by the storm was overwhelmingly short-term and on promotional cards.

This card-utilization pattern is also apparent in regression form in Tables A5 and A6 in the Supplementary Material. Specifically, Table A5 in the Supplementary

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Material shows that among promotional cards originated after Harvey, each additional foot of flooding leads to revolving balances that are \$221, \$131, and \$87 higher 2, 4, and 6 months after origination, respectively. By comparison, in Table A6 in the Supplementary Material, we find no evidence of increased borrowing on standard cards. Accordingly, we conclude that this pattern of borrowing is unique to promotional cards.

We have so far documented an increase in promotional card origination, heavy use of these new cards, and rapid repayment. The rate of repayment implies that by the time most promotional cards' interest rates reset, the storm-induced excess balance is paid down. This pattern implies an increase in aggregate borrowing as a consequence of Hurricane Harvey in flooded areas without a commensurate increase in the interest and fees paid by these borrowers. To calculate total borrowing in flooded areas on cards originated just before and after Hurricane Harvey, we aggregate the total revolving balance outstanding for months 1 through 24 among cards originated in the same four 3-month cohorts as above. These results are shown in the left of Graph A of Figure A2 in the Supplementary Material. In the right graph, we aggregate cumulative total fees paid on newly originated cards over the same time period. This figure shows revolving balances roughly doubling relative to the pre-storm period immediately following the storm; by 24 months, the aggregate excess balance is repaid. Total borrowing (summing borrowing in each month over 24 months, measured in dollar-months) was about one-third higher over the 24 months after the storm (\$120 million dollar-months of pre-Hurricane-cohort borrowing versus \$170 million dollar-months post-Hurricane-cohort borrowing). However, as the right graph shows, total fees paid were about the same on pre- and post-Harvey cohorts by the end of 24 months, reflecting both the accelerated repayment speed in the promotional period for post-Harvey borrowers and faster repayment of higher-rate cards than usual after the rate reset. On net, in the first 24 months after origination, borrowers paid an average annual interest rate of 5.5% on pre-Hurricane-originated borrowing in flooded areas but only 3.8% on post-Hurricane-originated borrowing.

Overall, in this section, we showed that hurricane-induced borrowers are sensitive to the price of credit and borrow more and at lower rates than typical borrowers. They take advantage of the least expensive borrowing option available to them, and they are sophisticated in the sense that they pay down their additional balances before the promotional period expires so that they pay a substantially reduced average interest rate.

C. Borrowing by Missing Mortgage Payments

Recent work has shown that mortgages operate as lines of credit because homeowners borrow by missing payments. Specifically, Herkenhoff and Ohanian (2019) show that foreclosure delays after the Great Recession functioned as a form of unemployment insurance, and Gelman, Kariv, Shapiro, Silverman, and Tadelis (2020) show that federal workers missed mortgage payments to smooth consumption after the 2013 government shutdown.

After Harvey, households were likely to borrow using this form of credit, as lenders and servicers introduced forbearance offers that reduced the penalties

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associated with missing mortgage payments. Specifically, according to the policies of Fannie Mae, Freddie Mac, and the Federal Housing Administration, the forbearance offers provided financial relief in two ways. First, the offers suspended foreclosures. Second, they allowed homeowners to skip mortgage payments for 3 months without penalties or risk of being reported to a credit bureau. Importantly, no action was needed on the borrower's part to access this relief, as all missed payments immediately following the storm were automatically granted short-term forbearance. The rules also allowed borrowers to continue missing payments for an additional 9 months after the borrower established contact with their servicer. At the end of this period, borrowers could make up any skipped payments over a short period (a repayment plan) or modify their loan to recapitalize the missed payments. Eligibility for forbearance depended only on the borrower's home or place of employment being within the Harvey major disaster declaration area, and no evidence of damage was required. In sum, for most homeowners, forbearance offers were equivalent to an offer to borrow, at their mortgage rate, the value of any missed payments for up to 1 year.

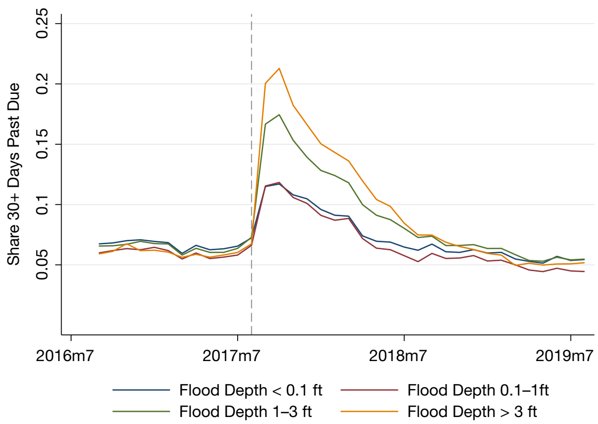
To study the degree of borrowing by missing mortgage payments after Harvey, we use the mortgage schedule from the Y-14 filing. While we cannot observe forbearance directly (forbearance is not a field reported by Y-14 filers), we observe whether a mortgage loan is past due. We interpret increases in mortgage nonpayment after Harvey as borrowing by missing mortgage payments (since there was no penalty for this borrowing).

Figure 5 presents the main results and shows that the use of these forbearance offers was widespread. Specifically, the figure plots the average mortgage nonpayment rate for four levels of flooding intensity. The figure reveals that, consistent with the idea that individuals borrowed by missing mortgage payments, there is a sharp increase in missed payments just after Harvey that returns to pre-Harvey

FIGURE 5

Mortgage Nonpayment Rate by Flood Depth

Figure 5 plots the average mortgage nonpayment rate (more than 30 days past due) by level of flooding intensity. The dashed line corresponds to Aug. 2017, when Harvey was announced and made landfall. Definition of variables can be found in Section II.B.2.



levels roughly within 1 year. The figure also reveals that the use of forbearance grew monotonically (though nonlinearly) with the level of flooding. Among those that experienced no flooding, nonpayment increased by roughly 5 percentage points from the rate observed in the pre-storm period. This increase amounts to approximately a doubling of the nonpayment rate. For those that experienced the most extreme flooding (3 feet or more), we observe that Harvey led to a further increase of roughly 10 percentage points in the nonpayment rate, or equivalently a 15 percentage point increase from the pre-storm period. This increase implies that more than one in five borrowers in this group missed payments after Harvey.

The widespread use of forbearance, including in nonflooded areas, is unsurprising, as our entire sample falls within Harvey's major disaster declaration area and was therefore eligible for forbearance. While it is generally expensive to borrow by missing payments because of the financial penalties and damage to credit scores, forbearance does not entail these downsides. Recent literature suggests similar behavior in response to Covid-19 forbearance programs (Zhao, Farrell, and Greig (2020), An, Cordell, Geng, and Lee (2021), Cherry, Jiang, Matvos, Piskorski, and Seru (2021), Kim, Lee, Scharlemann, and Vickery (2021), and Lambie-Hanson, Vickery, and Akana (2021)). To further verify that borrowers were not adversely affected by missing payments, Figure A3 in the Supplementary Material plots the 6-month change in credit score following a missed mortgage payment. The figure reveals that missing payments just after Harvey has no impact on credit scores but that missing payments outside of forbearance leads to a reduced credit score of roughly 20 points. All in all, these results highlight that missing mortgage payments in the region affected by Harvey was a low-cost (at the mortgage interest rate), low-hassle (no need to apply for a loan) way to borrow moderate sums (up to 1 year of mortgage payments).

To what degree did borrowers take advantage of this straightforward, low-cost credit form? To gauge the extent to which individuals borrowed using forbearance offers, Figure A4 in the Supplementary Material plots the average number of missed payments by the level of flooding among homeowners using forbearance. While the use of credit increases monotonically with the level of flooding, the amount borrowed is relatively limited for all levels. Specifically, we find that even those most affected by Harvey miss only 2 of 12 possible payments on average, while those least affected miss only 1.5 payments on average. These results suggest that these homeowners borrowed between \$1,650 and \$2,200 by missing payments. The fact that most borrowers did not miss three or more payments also suggests that there may have been a substantial transaction cost in extending the forbearance offer by establishing contact with the servicer. Also consistent with the idea that transaction costs are important, we observe that most borrowers (about 95%) repay their debt within a year, either by selling their house or becoming current. By comparison, only about 5% of borrowers record mortgage modifications, and this fraction is roughly invariant to flooding intensity.

D. Substitution and Complementarity with Other Risk-Management Tools

Households have several tools at their disposal to mitigate the risk of flooding. These tools include taking self-protection measures to reduce the probability

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of damage from flooding (e.g., sandbags, water pumps, or structure elevation) and using flood insurance to reduce the value of the losses. This section investigates the relationship between household borrowing and these risk-management tools.

We measure the extent to which households engaged in risk management against flooding before Harvey in several ways. First, we use an indicator variable for location in the floodplain (FEMA zones A and V). Because the risk from flooding is more salient, we hypothesize that households in these areas are more likely to undertake self-protection and purchase insurance. Additionally, mortgage lenders require mortgages in flood zones to carry flood insurance. Second, we construct an elevated structure indicator taking advantage of Houston's building code (City of Houston (1985)), which mandated elevating new structures at least 1 foot above the 100-year floodplain. From an engineering perspective, elevating a structure is one of the most effective, albeit expensive, ways to harden a structure against flooding. Accordingly, we expect those residing in an elevated structure to experience much lower direct damage. Third, we proxy insurance penetration by computing the share of insured structures at the census tract level using data from the National Flood Insurance Program (NFIP).

To study how the borrowing response varies with the level of self-protection and insurance, we begin by augmenting our baseline econometric specification (equation (1)) with an interaction term between the flood measure variable F_z and an indicator variable for being in the floodplain. Table 3 presents the result from this exercise for credit card borrowing, in columns 1–3, and for borrowing by missing mortgage payments, in column 4. The table shows that borrowing using credit cards and missing mortgage payments (using forbearance) is substantially attenuated among those in the floodplain. Specifically, column 1 presents results using promotional card originations as the outcome variable.[13] We focus on promotional cards because, as discussed in Section II.B, credit card borrowing after Harvey takes place primarily through this type of card. The coefficients on the interaction reveal that the odds of observing a promotional card origination are more sensitive to flooding outside than inside the floodplain. This result likely reflects that outside the floodplain, fewer households used insurance or undertook self-protection actions. Columns 2 and 3 report results where the outcome is the revolving balance on these promotional cards at 2 and 4 months after origination, respectively. The columns show that short-term borrowing on these cards is concentrated among those outside the floodplain. Specifically, the coefficients for the main effect reveal that outside the floodplain, balances on these cards are quickly built by 2 months after origination before starting to decline. We observe the largest impact on balances 2 months after origination for cards originated 4–6 months after Harvey, with the coefficient indicating an increase in balances of roughly $250 per foot of flooding. By comparison, for those in the floodplain, the marginal effects (main effect plus interaction) indicate a much more muted increase in balances, with the impact of flooding peaking in the same time frame but at a much lower level of borrowing, roughly $125 per foot of flooding. In sum, these results highlight that the use of promotional

[13] As a reminder, this is a binary variable equal to 1 if at least one promotional card originated in a ZIP+4 month unit.

TABLE 3

Storm-Induced Borrowing and Risk Management

Table 3 presents the results from five separate regressions of augmented versions of specification (1). The dependent variable is given by the column title. In columns 1–4 specification (1) is augmented by including an interaction with an indicator variable for being inside of the floodplain (FP). In column 5, we include an additional interaction for residing in an elevated structure. The depth variable measures the average ZIP+4 level of flooding created by Harvey in feet. All regressions include month-year fixed effects and interactions of median household income with flood depth and post-period × flood depth. Columns 4 and 5 additionally include credit line fixed effects. Robust standard errors clustered at the ZIP+4 are presented in parentheses. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively. The sample for credit card originations and revolving balances after origination (columns 1–3) includes all originations between Jan. 2016 and Aug. 2018 for borrowers with mailing addresses in Harris, Aransas, Nueces, and San Patricio counties in Texas at the time of Hurricane Harvey. Definitions of dependent variables can be found in Sections II.B.1 and II.B.2.

	Promotional Card Originations (logit)	Revolving Balance 2 Months	Revolving Balance 4 Months	Mortgage Nonpayment	Mortgage Nonpayment
	1	2	3	4	5
1–3 MTHS_POST × DEPTH	0.13***	154.9*	124.8	0.027***	0.027***
	(0.012)	(72.5)	(65.2)	(0.0013)	(0.0017)
4–6 MTHS_POST × DEPTH	0.035**	246.9***	211.0***	0.018***	0.018***
	(0.013)	(60.9)	(61.8)	(0.0012)	(0.0017)
7–9 MTHS_POST × DEPTH	0.011	67.4	12.8	0.013***	0.012***
	(0.012)	(60.4)	(61.0)	(0.0011)	(0.0015)
10–12 MTHS_POST × DEPTH	−0.021	103.7	99.3	0.0060***	0.0059***
	(0.013)	(56.9)	(57.9)	(0.00091)	(0.0012)
1–3 MTHS_POST × DEPTH × FP	−0.097***	−168.6**	−74.8	−0.013***	−0.0083**
	(0.016)	(57.6)	(56.0)	(0.0020)	(0.0027)
4–6 MTHS_POST × DEPTH × FP	−0.039*	−125.9*	−105.7	−0.0100***	−0.0061*
	(0.019)	(52.3)	(56.4)	(0.0018)	(0.0024)
7–9 MTHS_POST × DEPTH × FP	−0.045*	−114.6*	−35.7	−0.0082***	−0.0048*
	(0.018)	(50.1)	(54.5)	(0.0016)	(0.0023)
10–12 MTHS_POST × DEPTH × FP	−0.0019	−47.4	−17.2	−0.0042**	−0.0021
	(0.018)	(47.5)	(51.7)	(0.0013)	(0.0019)
1–3 MTHS_POST × DEPTH × FP × ELEVATED					−0.014***
					(0.0037)
4–6 MTHS_POST × DEPTH × FP × ELEVATED					−0.011**
					(0.0035)
7–9 MTHS_POST × DEPTH × FP × ELEVATED					−0.0098**
					(0.0032)
10–12 MTHS_POST × DEPTH × FP × ELEVATED					−0.0056*
					(0.0026)
No. of obs.	9,213,624	861,654	861,845	16,067,039	15,723,040
R^2		0.018	0.017	0.515	0.516
Pseudo R^2	0.003				

cards in both the extensive and the intensive margin was driven by households outside the floodplain.[14]

Next, in column 4, we present results when the outcome is a binary variable for mortgage nonpayment – which, during the post-Harvey period, is equivalent to taking advantage of forbearance. Consistent with our previous results, we find that borrowing by missing mortgage payments occurs at a much higher rate among those outside of the floodplain. At its peak immediately after Harvey, we find an increase in nonpayment of about 2.7 percentage points per foot of flooding for this group. By comparison, being inside the floodplain roughly halves the increase in

[14]Table A7 in the Supplementary Material further shows that among existing credit lines, charges and payments are also muted for those in the floodplain, but as in Section A of the Supplementary Material, these lines of credit are not used to borrow in the aftermath of Harvey.

mortgage nonpayment associated with an additional foot of flooding to 1.4 percentage points.

For those in the floodplain, there are two potential explanations for their more limited borrowing: structure hardening or insurance requirements. To isolate the role of structure hardening, we further augment our econometric specification by including an additional interaction with an indicator variable for residing in an elevated structure (equivalent to residing in a structure built or substantially renovated after 1985). The results of this specification are reported in column 5. We find that those subject to the updated building code use substantially less forbearance. Specifically, we observe that immediately after Harvey, those residing in post-1985 homes in the floodplain are 1.4 percentage points per foot of flooding less likely to miss payments than those in the floodplain in pre-1985 homes. We also find that those in pre-1985 homes in the floodplain are themselves 0.8 percentage point per foot less likely to miss payments than those outside of the floodplain. This finding suggests that in the floodplain, other forms of voluntary self-protection still play a role in reducing damage. To put these numbers in perspective, consider that the combined effect of hardening and other self-protection measures is a reduction of 2.2 percentage points per foot of flooding. This reduction implies that borrowers in the floodplain in post-1985 homes experiencing, for example, 1 foot of flooding missed payments at about a 5% rate, which is roughly the same rate observed among borrowers who did not experience flooding. Importantly, our findings also highlight the bulk of this protection benefits comes from structure hardening (about two-thirds of the reduction in nonpayment observed in the floodplain, 1.4/2.2). Last, we can also rule out generic improvements in construction as an explanation for the observed protection provided by structure hardening, as we fail to find differential nonpayment rates between pre- and post-1985 structures outside the floodplain.[15]

Next, we explore the relationship between extensive credit use and flood insurance penetration. Specifically, Table A8 in the Supplementary Material presents results from repeating the previous exercise, this time augmenting our baseline econometric specification (equation (1)) by interacting an indicator variable for above-median insurance penetration in place of the floodplain dummy. Unlike physical hardening or preventive actions, flood insurance does not reduce storm damage, so it may not reduce liquidity needs in the short run. We find no evidence that high insurance penetration altered originations, credit card borrowing, or mortgage nonpayment conditional on flooding.

We also study whether insurance altered the use of existing credit cards. Specifically, in Figure 6, we plot how charges, payments, and revolving balances differ between areas with above- and below-median insurance penetration.[16] We find that

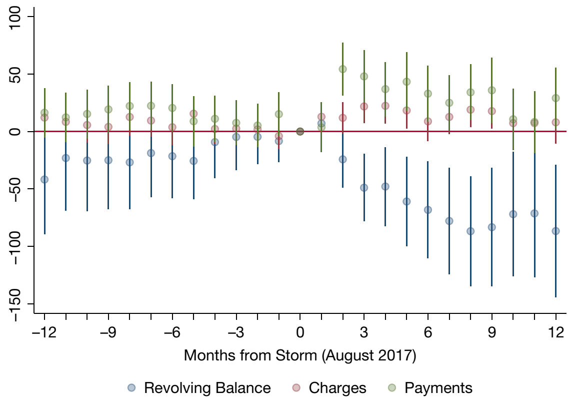
[15]To keep the table concise, we do not report these interaction coefficients. They are available from the authors.

[16]The figure plots the coefficients from three regressions with the basic structure of equation (1), with charges, payments, and revolving balances as dependent variables. The coefficients are the interaction between a dummy variable indicating the card is in a census tract with above-median insurance coverage and a dummy variable indicating an area that experienced more than 1 foot of flooding. The coefficients can be interpreted as the difference in the response among those with above- and below-median

FIGURE 6

Evolution of Charges, Payments, and Revolving Balances by Insurance Level

Figure 6 plots point estimates and 95% confidence intervals of three OLS regressions, where card-level charges, payments, and revolving balances are the respective dependent variables. Specifically, we estimate an augmented version of specification (1) where we discretize $F_{z\tau}$ into two groups (more and less than 1 foot of flooding), and we add an interaction with an indicator variable for above-median insurance penetration. The plotted coefficient can be interpreted as the difference (in revolving balances, charges, or payments) among flooded borrowers between areas with above and below insurance penetration. All regressions include credit card and month-year fixed effects. Confidence intervals are derived from robust standard errors clustered at the ZIP+4. Specification (1) reports 36 coefficients (i.e., β_{-24} to β_{12}). To avoid multi-collinearity, we normalize β_{-24} and β_0 to be equal to 0. Unless it is informative, we provide more concise results by only plotting coefficients for β_{-12} to β_{12}. Definitions of all dependent variables can be found in Section II.B.1.



households in areas with high-insurance penetration increased charges more than households in lower-insurance areas, suggesting that insurance may have supported consumption following the storm. We also see an unexpected pattern: 2 months after the hurricane, payments in high-insurance, flooded areas increased even more than charges, and revolving balances declined, suggesting insurance payments may have helped households pay down existing expensive credit card debt.

Table A9 in the Supplementary Material corroborates this story. This table shows charges, payments, revolving balances, and delinquency separately for cards with and without preexisting revolving balances at the time of the storm. Charges and payments on cards without preexisting revolving balances moved in tandem. On cards with preexisting revolving balances, in high-insurance areas, charges did not increase with flooding, but payments did, and revolving balances fell – consistent with high-insurance borrowers paying off credit card balances. Likewise, new delinquency rates on cards with preexisting revolving balances declined temporarily, further suggesting the use of flood insurance toward card payments.

These results come with some caveats. First, our measure of NFIP insurance is imprecise at the ZIP+4 level because the publicly available NFIP data are aggregated to the much coarser census tract level. Therefore, our point estimates may be attenuated. Second, we find that flood insurance at the tract level is highly colinear with household income, which we measure at the more granular census block group level. To account for this collinearity, we controlled for income in levels and interacted with time and flood depth variables. This approach generates high standard errors, as we

insurance coverage. The regression controls separately for the relationship between household income and flooding before and after Hurricane Harvey.

cannot clearly distinguish the effect of household income from the impact of insurance on household behavior. For both of these reasons, we do not place great weight on the magnitudes of our findings and instead view these results as suggestive.

V. Robustness

A plausible concern with our identification strategy is that the behavioral responses we document reflect not just the effect of flooding but also the unobservable differences between more- and less-affected areas. This concern is especially relevant if areas that experienced different levels of flooding are very far from one another, given that important unobservable characteristics or poorly measured economic factors are often geographically correlated. These unobservable factors could confound our results if, for example, areas that experienced different degrees of flooding would have responded differently to the same degree of flooding.

To address this possibility, we run a series of robustness tests in which we control for the degree of flooding experienced by neighbors. This strategy is designed to capture geographically correlated unobservable characteristics. We begin this test by identifying ZIP+4s in 250–500 m and 500–1,000 m rings around each ZIP+4 in our sample and computing the average level of flooding for near neighbors (250–500 m) and faraway neighbors (500–1,000 m). We then sequentially test whether our results are robust to controlling for near or faraway neighbors. Specifically, we estimate two augmented versions of equation (1) where we additionally control for the interaction between the D_{ct}^{τ} indicator variable for being τ months away from Harvey's landfall and the level of neighbor flooding either near or faraway. The correlation between own-flooding and neighbor flooding is 0.71 for the 250–500 m ring and 0.54 for the 500–1,000 m ring.

All our results are qualitatively robust to adding these controls, though our point estimates attenuate slightly. Tables A10 and A11 in the Supplementary Material present these results for charges, payments, revolving balances, and delinquencies on existing cards. Tables A12 and A13 in the Supplementary Material present the results for card originations. Tables A14 and A15 in the Supplementary Material present the results for mortgage nonpayment.

VI. Conclusion

In this article, we assemble a new data set that provides detailed information on credit card and mortgage borrowing for small geographic units (ZIP+4 locations) affected by varying levels of flooding from Hurricane Harvey. We use this data set to describe the financial decisions of households in the aftermath of Harvey. Our estimates rely on a difference-in-differences design that exploits the flooding gradient created by Hurricane Harvey.

We find that households drawn into borrowing following Harvey are generally sensitive to the price of credit and that they quickly repay new loans. Three pieces of evidence show that the borrowing response to Harvey is concentrated in low-cost credit options. First, we find that while households respond to the need for funds created by Harvey by increasing charges on their credit cards, they avoid this expensive form of credit by increasing payments in lockstep. Second, we find that

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originations of promotional (zero interest) cards spike in affected areas, enabling households to avoid expensive borrowing. Specifically, we find that while households generate large balances on these new cards, they largely pay down excess storm-induced balances before the end of the promotional period. Third, we find that homeowners took advantage of forbearance programs and borrowed at their mortgage interest rate by missing mortgage payments. Consistent with our previous results, we also find that most homeowners repay their debt without incurring any penalties.

We additionally exploit floodplain designations and the 1985 building code revision, which mandated the elevation of the foundation of new structures 1 foot above the floodplain, to study the degree of complementarity between borrowing and other risk-management tools. Consistent with the idea that ex post borrowing operates as a substitute for ex ante risk management, we find a muted borrowing response (across all types of credit lines) in the floodplain (where residents are more likely to self-insure and self-protect). We find that affected households residing in elevated structures behaved similarly to nonflooded households, suggesting a strong role for physical hardening in mitigating households' financial vulnerability to natural disasters. We also present suggestive evidence indicating that insurance may have facilitated the paydown of expensive credit card debt.

Our findings are important for policymakers because they routinely face a choice over how much credit to encourage or provide after natural disasters. Policymakers may be concerned that additional borrowing after storms may lead to an ongoing cycle of expensive borrowing and default or that the newly issued credit will generate large losses for banks. We show that these concerns are not well-founded, as post-storm borrowers generally use credit in a cost-conscious and time-limited manner. These findings suggest that policymakers may want to encourage the extension of credit following natural disasters, particularly to homeowners in nonhardened homes, for whom credit may be particularly necessary.

One caveat to our results is that they are derived at a time when credit was relatively abundant and when Houston was booming. Had the storm occurred at another point in the business or credit cycle, private credit might not have been available as a tool to help affected individuals manage storm damage. In such a circumstance, government provision of credit (e.g., forbearance policies) might have been even more important, but the potential for these programs to experience losses would also be higher. This highlights another important policy implication of our findings – namely, that regulations encouraging physical hardening can substantially reduce households' reliance on credit following natural disasters, potentially reducing a source of overlapping climate and macroeconomic risk.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000728.

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