Weathering an Unexpected Financial Shock: The Role of Cash Grants on Household Finance and Business Survival

Justin Gallagher, Daniel Hartley, and Shawn Rohlin*

March 13, 2021

Abstract

We estimate the causal effect of cash grants on household finance and business survival following a natural disaster. Disaster-affected individuals in severely damaged blocks with access to cash grants have 30% less credit card debt following the disaster than those without access to cash grants. Grants reduce bill delinquency for credit-constrained victims, and overall migration. The grants play a role in mitigating the effects of the shock to businesses, resulting in 9% more establishments and 12% more employees post-disaster in the average-damaged neighborhood where residents receive grants. These effects are concentrated among small non-manufacturing establishments that rely on local demand.

JEL Classification: D14, Q54, R11

*E-mail: *justin.gallagher1@montana.edu*, *daniel.a.hartley@chi.frb.org*, *srohlin@kent.edu* The authors would like to thank seminar participants at Case Western Reserve University, Federal Reserve Bank of Chicago, Federal Reserve System Committee on Regional Analysis, Georgia State University, London School of Economics, Louisiana State University, Ohio State University, UNLV, University of Wisconsin, Wharton and at the American Economic Association, Association of Environmental and Resource Economists, and the Urban Economics Association meetings. A special thanks to Benjamin Collier, Scott Shane, John Stevens, and Alejandro Del Valle for feedback. Jacqueline Blair, Simin Gao, Benjamin Marks, Sarah Mattson, Kyle Musser, and Jacob Rabin provided outstanding research assistance. Gallagher thanks the Freedman Center for Digital Scholarship and the Initiative for Regulation and Applied Economic Analysis for research support. The opinions expressed are those of the authors and do not necessarily represent the views of the Federal Reserve Bank of Chicago or the Board of Governors of the Federal Reserve System.

1 Introduction

Natural disasters in the US are shocks to income, wealth, and capital. In 2017, natural disasters caused at least $100 billion in insured damage losses (Munich Re [2017]). Average yearly economic losses from natural disasters in the US more than doubled in real terms from 1981 to 2010, while loss of life from natural disasters remained relatively constant (Munich Re [2013]).

The US government has a long history of federal assistance following natural disasters. Cash assistance has been distributed to disaster victims immediately following natural disasters via a codified legal process since at least 1953. The implicit assumption is that savings, credit markets, and existing insurance (e.g. homeowners, unemployment) are insufficient to smooth the negative financial consequences of the natural disaster.

Several recent studies have, for the first time, estimated individual-level financial outcomes following natural disasters in the US using large administrative datasets (Deryugina et al. [2018]; Gallagher and Hartley [2017]; Groen et al. [Forthcoming]). These studies all conclude that the average net financial impact of a large natural disaster is modest and short-lived. However, none of these papers are able to isolate the role that cash assistance has on post-disaster outcomes.

There are two goals of this study. First, we estimate the causal effect of federal cash grants on post-disaster financial and migration outcomes using credit bureau data. We test whether cash grants substitute for personal debt and lead to a decrease in the level of debt incurred by disaster victims. We also test whether the cash grants reduce the likelihood of negative financial outcomes (e.g. debt delinquency) or affect migration from the disaster area.

In our setting, the natural disasters that cause uninsured property damage act as unexpected, one-time shocks to wealth. The disasters may also lead to job loss, but the lost income is temporary for most disaster victims, as the disasters rarely cause disabling injuries that impact human capital. The life cycle/permanent income hypothesis, for example, predicts that some disaster victims will borrow to smooth the wealth shock (e.g. Meghir and Pistaferri [2011]; Parker et al. [2013]). Victims who receive cash grants are likely to borrow less, as the grants substitute for borrowing. However, for credit-constrained individuals, the receipt of a cash grant would not be expected to reduce debt by as much, if at all, since in the absence of the grant these individuals would have been less able to borrow. We test whether the cash grants lead to a differential effect on debt and overall financial wellbeing for credit constrained-individuals.

The second goal of this study is to measure the effect of the cash grants on local businesses. The business data are from a proprietary database which seeks to include every US business establishment, and includes precise, establishment-level location information on the age of the establishment, the number of employees, and an (6-digit) industry code. We test whether cash grants act as a targeted stimulus to local businesses. Specifically, we examine whether, following the disaster, there is less of a reduction in the number of business establishments and employees in disaster-affected neighborhoods where individuals receive cash grants. The cash grants to individuals could lead to improved local establishment survival rates if there is an increase in spending on local goods and services. The cash grants that go to individuals who happen to be small business owners could also help keep businesses open.

The Presidential Disaster Declaration process is the main mechanism for federal assistance following a natural disaster. The program we study is called Individual Assistance. Under Individual Assistance, residents in disaster areas can receive cash grants up to approximately $30,000 (Fed [2010]). The cash grants are linked to incurred damage and expenses caused by the disaster. Unlike most cash transfer programs, Individual Assistance is a one time grant and not limited to low socioeconomic residents (e.g. Baird et al. [2011]).

The main identification challenge is that the decision of whether to provide cash grants is made following a disaster. First, cash grants may be more likely following more damaging disasters. To address this we limit our analysis to 34 very large US tornadoes (2002-2013). In our analysis we are able to precisely control for heterogeneity in block-level damage intensity using detailed damage maps. Figure 1 shows the damage map for an Enhanced Fujita 5 (EF5) tornado that hit Joplin, Missouri on May 22, 2011. The EF ratings are determined by National Weather Service (NWS) employees who conduct on the ground damage inspections. The NWS damage analysis carefully considers the type of structure and building materials, and the local building codes (Edwards et al. [2013]). This guards against the possibility of the tornado damage being overrated due to, for example, damage to pre-fabricated homes.

Our preferred econometric models use either a continuous damage variable to precisely control for the level of damage, or bin affected blocks into low, medium, and high damage groups. The binned model allows for non-linearities in how individuals and businesses respond to the disaster damage and cash assistance. The high damage group is for blocks that correspond to the NWS threshold for “severe damage” (National Weather Service [2014]).

There is still a concern that cash assistance may be made available only when areas with more vulnerable populations are affected. We address this through a triple difference econometric model. Since tornado damage is very localized and the exact path of a tornado is not predictable, the geographic area in close proximity to the tornado should provide a good control group. Figure 1 shows our baseline comparison group in blue, those living 0.5 to 1.5 miles from the edge of the tornado damage path. We examine the pre- to post-tornado difference in financial outcomes for hit and nearby populations who are affected by tornadoes with and without federal cash assistance (“cash tornadoes” and “no-cash tornadoes”, respectively).

Figure 2 provides an illustrative example plotting the mean credit card debt levels separately for the hit and nearby individuals for cash and no-cash tornadoes. The means are plotted with respect to the number of quarters since the tornado. It would be wrong to simply compare the hit areas for the tornadoes where victims did and did not receive cash assistance. Doing so would lead to a biased estimate for the causal effect of cash assistance due to the downward trend in credit card balances in hit areas that receive cash assistance. Differencing with the nearby groups provides a way of controlling for divergent pre-existing trends among the two groups hit by a tornado.

Finally, we also analyze a robustness subsample of tornadoes that more closely matches the average *levels* of key financial and business variables just prior to a tornado. The estimation results are similar across the two samples.

We find that disaster-affected individuals in high damage blocks with access to cash grants have $774 (30%) less in average quarterly credit card debt after the disaster relative to disaster-affected individuals without access to cash grants. The effect lasts for at least three years and is consistent with evidence on the persistence of revolving credit card debt (Telyukova [2013]). Access to credit markets impacts how tornado victims substitute cash grants for credit card debt. Nearly all of the reduction in credit card debt is attributable to less credit-constrained individuals. Credit-constrained individuals who have access to cash grants have lower rates of bill delinquency and are more likely to increase consumption on new vehicle purchases.

Previous research is mixed on whether experiencing a natural disaster leads to increased migration. For example, Hornbeck [2012] and Boustan et al. [2012] show that net out-migration increases following natural disasters in the US during the first half of the 20th century. Deryugina [2017], however, finds no net population change in response to US hurricanes during the 1980's and 1990's. The expansion of both formal (disaster) and informal (social safety net) federal transfers to disaster victims in the second half of the 20th century may help to explain the lower effect on migration. At the same time, cash assistance following a disaster could increase out-migration if there are fixed costs to moving. Gallagher and Hartley [2017] show that migration from New Orleans after Hurricane Katrina is highest for those who experienced the worst flooding, and present suggestive evidence that the propensity to migrate was greater still for individuals who received lump sum flood insurance payments.

We find an increase in temporary (one quarter) out-migration from the block for residents hit by the average block-level tornado damage in our sample (F/EF 1.8) and who have access to cash assistance. At the same time, residents in the most-damaged blocks who have access to cash assistance are much less likely to permanently move from the block. One explanation is that the cash grants help facilitate a temporary move while the home is repaired, but lead residents to remain in the same neighborhood after the tornado.

Our triple difference estimate of the effect of cash grants on businesses indicates that the grants ameliorate the negative effects of tornadoes in the worst-affected neighborhoods. We estimate that there are approximately 9% more establishments and 13% more employees in blocks that sustain the average level of tornado damage in our sample when residents in the neighborhood have access to cash assistance. Separate difference-in-differences estimates for cash and no-cash tornadoes show that there are fewer surviving establishments in hit blocks regardless of whether cash grants are allocated, but that the survival rate is higher for cash grant tornadoes.

The increase in the number of establishments is due to a higher survival rate for existing non-manufacturing establishments. There is no evidence that the cash grants affect the formation of new establishments. Overall, our findings indicate a mechanism whereby the cash stimulus to the local population most benefits businesses that rely on local demand. We estimate that the cost per job retained or created in the disaster areas from the distribution of the cash grants to be approximately $75 thousand.

Our study adds to a growing literature on how cash transfers affect household finance and employment (e.g. Brudevold-Newman et al. [2017]). Studies in this literature usually examine cash transfers that occur over multiple, scheduled installments (e.g. Skoufias and Parker [2001]), and tend to focus on transfers to poor residents in developing countries (e.g. Fiszbien and Schady [2009]). Moreover, most of these studies examine cash transfer programs where the receipt of the cash is linked to socioeconomic status such as income or disability (e.g. Aizer et al. [2016]). We are not aware of another study that examines the role of a one time cash grant following a financial shock in a developed country. Thus, the household finance and migration results of our study are likely to be of interest to policymakers in the US and other developed countries considering cash grant policies in a variety of settings.

2 Background and Data

This section describes our main data sources and summarizes the institutional background. Additional details are available in Appendix Section 1.3.

2.1 Tornado Data

There are 34 tornadoes in our sample. To form our sample we start with the list of tornadoes compiled by the Tornado History Project. The main source of the Tornado History Project information is the Storm Prediction Center's historical tornado data file. The Storm Prediction Center is part of the National Weather Service and the National Centers for Environmental Prediction. We use tornado cost, casualty, and maximum intensity information from the Tornado History Project.

Three criteria determine whether a tornado is included in our sample. First, the tornado occurs from 2002-2013 so as to match the period covered by our individual and business financial data. Second, the tornado must have a Fujita (F) or Enhanced Fujita (EF) rating of either a 4 or 5.[1] Third, the tornado must have a high quality damage path map, generally created by the National Weather Service (NWS), that demarcates areas of the tornado path that suffered different levels of damage. Appendix Section 1.1 provides details on how the NWS creates the damage maps using on the ground observations and a detailed engineering model that takes into account the strength of the damaged materials and local building codes. Thirty-five tornadoes satisfy the three criteria. Our sample includes 34 tornadoes, as one tornado violates the pre-trend assumption of our sample design. We provide more details when we discuss the econometric model in Section 3. Appendix Table 1 lists all 35 tornadoes.

2.2 Public Disaster Assistance

The Presidential Disaster Declaration (PDD) system is a formalized process to request and receive federal assistance following large natural disasters. A PDD opens the door to three major types of assistance. The first type is Individual Assistance and is the source of the cash grants we study in this paper. There are two steps to qualify for cash grants. First, Individual Assistance must be available to disaster-affected residents in the county. Second, the exact level of assistance is determined via an application that documents incurred

[1]Tornado classification switched from the Fujita scale to the Enhanced Fujita scale in 2007. The Fujita scale estimated wind speeds are a bit higher for the same numerical rating.

damage (e.g. structural damage to the home) and expenses (e.g. temporary housing and relocation) caused by the disaster. The maximum amount of cash assistance was $30,200 in 2010 and is indexed to inflation (Fed [2010]).

Individual Assistance is not declared for all disasters. There is no single minimum eligibility threshold or guideline that must be met in order for FEMA to approve Individual Assistance as part of a PDD. Instead, FEMA is required to consider six criteria (GAO [2018]). The criteria are: concentration of damages, trauma (e.g. casualties and deaths), special populations (e.g. low-income and elderly), voluntary agency assistance (e.g. non-profit, local, and state capacity), access to insurance for the affected population, and the average past amount of Individual Assistance by state. The multiple criteria and lack of numerical thresholds have contributed to the "subjective nature" of Individual Assistance approval following a disaster (GAO [2018], p22).

Appendix Table 2 provides summary information for the tornadoes in our sample. Twenty-five tornadoes are part of disaster declarations where individuals received cash grants. Tornadoes with cash assistance are part of larger state-level disasters as measured by the percent of the state's counties included in the PDD. Cash assistance tornadoes also tend to cause more block-level damage. FEMA's trauma criteria appears to influence whether cash assistance is made available, as the average number of fatalities and casualties are both larger for cash tornadoes. There is no evidence that tornadoes with cash assistance occur in more electorally competitive states. The difference in the overall damage between cash and no-cash tornadoes motivates our preferred econometric models that control for block-level damage.

We obtained information on all cash grants distributed under the Individual Assistance program via a Freedom of Information Act (FOIA) request. Due to privacy considerations, we are only able to access summary cash grant information at the 5 digit ZIP Code level. For this reason, we do not estimate a model that uses the block-level magnitude of cash assistance. Rather, we use these data to confirm that the distribution of cash grants paid out coincides with the tornado path, and as part of a cost per job calculation in Section 5.

ZIP Codes are spatially much larger than blocks in our sample. Even for the largest tornadoes, only a small fraction of a ZIP Code is directly hit (see Appendix Figure 1). At the same time, individuals living outside the tornado path are eligible for cash assistance to cover less severe damage from the storm system that spawned the tornado. As such, the ZIP Code-level cash assistance data obscure the fact that individuals hit by the tornado, and especially those living in highly damaged blocks, receive much higher levels of cash assistance than the average grant recipient.

Small Business Administration (SBA) disaster loans are the second type of assistance affiliated with a PDD. Disaster-affected individuals and businesses can request subsidized SBA disaster loans. SBA disaster loans are routinely available to residents in counties that are not part of a PDD. SBA loans are available in 99% of the hit blocks in our sample, regardless of PDD designation.

Public Assistance is the third type of disaster assistance. Public Assistance is available to local and state governments as well as non-profit organizations located in a PDD county. These groups can access grant money to repair infrastructure and to aid in the reconstruction of public buildings. Public

Assistance is provided for 22 of the 25 cash tornadoes and 3 of 9 no-cash tornadoes in our main sample.

We interpret the estimated impact of being hit by a cash tornado on household finance and business establishments as due to the cash grants and not from the unequal dissemination of Public Assistance. While we can not rule out the possibility that Public Assistance contributes to our findings, several factors shape our view that cash assistance is driving our results.

First, Public Assistance targets the repair of transportation infrastructure. If Public Assistance facilitates the opening of damaged roadways then this could contribute to greater economic activity in the damaged neighborhood. However, even the most destructive tornadoes tend to only directly hit a small fraction of a community. For example, the 2011 EF5 Joplin, MO tornado is the deadliest US tornado since reliable record keeping began in 1950 (National Weather Service [2018]). Yet the brunt of the tornado hit just 10% of a single ZIP Code (see Appendix Figure 1). Moreover, area roads were immediately serviceable following the tornado. By contrast, severe winds and flooding from Hurricane Katrina in 2005 (the most costly US hurricane) impacted parts of four states, and flooded more than 80% of New Orleans, a city of 450 thousand people (Sills et al. [2008]). Portions of the city were underwater for five weeks. More than $2.4 billion was spent in the six years following Katrina to repair the transportation infrastructure around New Orleans (Lee and Hall [2011]).

Second, Public Assistance can offset the reconstruction cost of public buildings. Public Assistance could lead to higher public sector employment following a tornado if, for example, buildings that employ public sector workers are repaired faster. If this occurs, then these workers may not be laid off or relocated to a different block. However, our employment findings are insensitive to the inclusion of public sector employees. Moreover, when we limit the data to only public sector employees, we estimate a very small, statistically imprecise change in employment. Finally, the disaggregated industry results support an economic channel whereby cash provided to the local population improves business outcomes for those establishments most reliant on local demand.

2.3 Credit and Debt Information

We use individual-level credit and debt information from the Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP) (Lee and van der Klaauw [2010]). Equifax, one of several large consumer credit repository and credit scoring companies in the US, is the source of the credit and debt data. The panel is quarterly and built using a 5% sample of the US population that is selected based on the last two digits of an individual's social security number. To form our sample, we take the individuals living in the treatment and control blocks at the end of the quarter before the tornado and set a balanced panel that runs from 12 quarters prior to the quarter of the tornado through 12 quarters after the quarter of the tornado. We can track all individuals even if they move away from the tornado-affected area or were living elsewhere for some portion of the pre-tornado period using anonymous individual identifiers.

Consumer credit account information is divided into five main types: home loans, auto loans, credit card accounts, student loans, and other debt. Home loan information separately tracks first mortgages, home equity loans, and home equity lines of credit. Credit cards include both bank and retail cards.

We do not consider student loan debt because the way in which the data are recorded changed during our study period (Brown et al. [2014]). A significant component of other debt (81% of other debt accounts) are consumer finance loans, which are a type of subprime loan typically used by borrowers with lower credit scores. We follow Lee and van der Klaauw [2010] and group consumer finance loans as part of other debt, in part, due to the relatively low consumer finance loan balances. The CCP includes the number of accounts for each debt, the total balance, indicators for whether the individual is behind on payment for each type of debt, and indicators for foreclosure and bankruptcy. The panel also includes the age, Census block of residence, and Equifax Risk Score (TM) for each individual. The Equifax Risk Score is a composite score that represents overall financial risk.

Appendix Table 3 shows financial and socioeconomic information for individuals in our sample. Individuals hit by tornadoes where cash assistance is available are economically worse off than individuals hit by tornadoes where cash assistance is not available. The economic information in the table is consistent with FEMA using the economic status as part of the calculation when deciding to award cash assistance (McCarthy [2011]).

2.4 Business Data

We use business establishment data from the Infogroup's Historic Business Database (Serrato and Zidar [2016]). The Infogroup database aims to include longitudinal establishment-level data on all business establishments in the US. The database covers approximately 35 million establishments each year for the years 1997 to 2017. The database includes each establishment's exact location (latitude/longitude or address), start date, number of employees, sales volume in dollars, detailed six-digit industry code, and corporate linkages. The appendix contains details on how Infogroup compiles this information.

Our unit of analysis is the census block. We aggregate the establishment-level data to the census block, and match the block-level establishment data with the tornado blocks.[2] On average, there are 2.1 establishments in hit blocks where individuals receive cash assistance, and 3.0 establishments in hit blocks where individuals do not receive cash assistance (see Appendix Table 3). The percent of employment at manufacturing establishments is similar in areas hit by cash and no-cash tornadoes (5% and 4%, respectively).

3 Empirical Specification

The sample of hit Census blocks includes all Census blocks that are more than 50% contained in a tornado damage path. The control blocks are selected drawing a 0.5 mile buffer and a 1.5 mile buffer around each tornado path and taking the set of Census blocks that are more than 50% contained in the band between the buffer lines. We exclude the half mile closest to the edge of the tornado path in case there is measurement error in the tornado map boundaries. Results are similar if we use 0.5-1.0 and 0.5-2.0 buffer areas.[3]

While there are areas of the US where tornadoes are prevalent such as the

[2]The database contains identifiers that would allow us to estimate an establishment-level model. We prefer the block-level analysis because it matches the level of treatment variation (tornado damage), and because it allows us to fix the geography and focus on the neighborhood economic recovery within a small geographic unit. The block-level analysis also allows us to look at entry and exit of establishments in a natural way.

[3]Results are also similar if we use a propensity score model to select non-hit individuals from control blocks anywhere within the same county as the tornado.

Great Plains, it is not possible to predict the exact path of a tornado. Thus, whether an individual or business in our sample is hit by a tornado, or located just outside the tornado path, is as good as random. This randomness provides a source of identification for the DD models.

We use a triple difference model to isolate the role of cash grants on post-disaster outcomes. The triple difference estimates can be thought of as taking the difference between two difference-in-differences (DD) estimates, where we separately estimate the effect of being hit by a tornado that does and does not result in post-disaster cash assistance. The within tornado difference between the hit and nearby populations controls for selection differences between victims of cash and no-cash tornadoes.

Our baseline empirical specification is a regression-based implementation of a triple difference estimator that uses a binary variable to classify whether an individual is hit by a tornado. We refer to this specification as our pooled model. We first describe the specification that we use to examine individual financial outcomes. We estimate the following equation,

$$y_{i,t} = \delta(Cash_i * Hit_i * Post_{i,t}) + \beta_1(Cash_i * Post_{i,t}) + \beta_2(Hit_i * Post_{i,t}) + \alpha_i + \gamma_t + \epsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is a credit outcome for individual i in quarter t. $Cash_i$ is a binary variable indicating whether individual i lived in an area either hit by or near a tornado that received cash assistance. Hit_i is a binary variable indicating whether individual i lived in a tornado-damaged block at the time of the tornado. $Post_{i,t}$ is a binary variable indicating the post-tornado period (any of the 12 quarters following the quarter of the tornado). δ is our coefficient of interest and represents the effect on credit outcomes for individuals living in hit blocks where cash assistance is available following a tornado, relative to those individuals who just missed being hit by the tornado, and as compared to individuals living in hit blocks with no cash assistance. α_i is an individual fixed effect, γ_t is a quarter-by-year fixed effect, and $\epsilon_{i,t}$ is an error term. We cluster the standard errors by tornado when estimating the model.[4]

We estimate a second triple difference model that replaces the binary hit variable in Equation 1 with a continuous damage variable measuring the average EF damage in the block. The continuous damage model provides a direct comparison between individuals living in blocks that sustain the same level of damage.

Our third triple difference model is a binned damage specification that replaces Hit_i with a vector of three binary variables indicating low, medium, or high damage. The advantage of this model over the continuous damage model is that it allows for non-linearities in how individuals respond to disaster damage and cash assistance. We classify the block as *low* damage if the mean EF is less than 1, *medium* damage if the mean EF is greater than or equal to 1 but less than 3, and *high* damage if the mean EF is 3 or higher. A damage level of 3 on the original Fujita scale corresponds to "severe damage" (National Weather Service [2014]). A damage level of 3 on the Enhanced Fujita scale implies that the roofs for most types of buildings will be severely damaged and the outer walls of the building may have collapsed. The EF damage analysis

[4] The binary variables $Cash_i$, Hit_i, $Post_{i,t}$, and the interaction $Cash_i * Hit_i$ are included, but "drop out" since the model includes individual and time fixed effects.

is comprehensive and carefully considers the type of structure, the type of building materials, and the local building codes (Edwards et al. [2013]).

We also estimate an event study version of the continuous damage model which allows us to observe temporal dynamics and to examine differences in pre-existing time trends. When we estimate the event study model we replace the $Post_{i,t}$ variable with a set of binary variables that indicate the number of years the observation is either before or after the tornado. The year before the tornado is excluded from the model and serves as the reference time period. We drop the quarter of the tornado from the panel before running the model.[5]

When we consider business outcomes we use a block-by-year panel. The panel is balanced in event time with four years before and after the year of a tornado. We drop the year of a tornado from our panel, since we are not always able to confirm whether the tornado-year business data are collected before or after the tornado. In place of individual and quarter-by-year fixed effects we use block and year fixed effects.

The three tornado and tornado mapping criteria discussed in Section 2.1 give us a sample of 35 tornadoes. Our preferred sample includes 34 tornadoes. One of the tornadoes, the Wayne, NE tornado, exhibits pretrends for our business outcomes (see Appendix Figure 3). We drop this tornado from our preferred sample. There is little difference between the two samples for the individual financial and migration outcomes. Not surprisingly, there are

[5]We prefer the yearly event study model to the quarterly model. The estimated pattern of the yearly coefficients is more informative. First, pooling the quarterly data increases the statistical precision. Second, our financial distress outcomes are low incidence outcomes, and estimating yearly coefficients smooths out the high quarter-to-quarter variance. Finally, the yearly dynamics match the business establishment panel and help to facilitate an easier comparison between individual and establishment-level outcomes.

some differences in the business results. We highlight these differences in the discussion of the results.

Finally, we test the sensitivity of our main estimates by estimating a robustness subsample that more closely matches the average *levels* of key debt and financial health variables for the hit cash and hit no-cash groups. A potential concern is that differences in the levels of the dependent variables between individuals hit by cash and no-cash tornadoes could bias our estimate if there is heterogeneity in the cash grant treatment effect. We set the robustness sample, which we refer to as the "balanced sample," using two steps. First, the balanced sample includes all individuals from the nine no-cash tornadoes in our main sample. Second, the balanced sample includes observations from nine of the 25 cash tornadoes. The underlying logic is that we could consider our main sample as a collection of random tornado events. We select a robustness sample, balanced in the number of tornadoes, that best matches the levels of the key CCP variables at the time of the tornado between hit individuals in the cash and no-cash tornado groups. Specifically, we minimize the sum of the absolute deviations in z-scores for the eight debt and financial health variables in Appendix Table 3. There are 2,042,975 possible subsamples.

4 Results

4.1 Household Finance and Migration

Most economic theories of consumption, including the life cycle/permanent income hypothesis (LCPIH), predict that disaster victims will borrow (or use savings) to intertemporally smooth the effect of a temporary, unexpected wealth shock (e.g. Meghir and Pistaferri [2011]). There will be a relatively small reduction in consumption. Disaster victims who receive cash grants will borrow less and consume (slightly) more than victims who do not receive cash grants. Provided individuals are not credit constrained, the LCPIH predicts that the receipt of the cash will have only a limited effect on measures of financial wellbeing. The household finance results largely confirm these predictions.

4.1.1 Debt

Panels A-C display the pooled, continuous, and binned model estimates, respectively. Credit card debt (column 1) is a common type of short-term debt that victims of tornadoes could use to smooth a financial shock.[6] Overall, there is a statistically insignificant reduction in the average quarterly credit card balances for victims of cash tornadoes. However, residents in high damage blocks show an economically large and statistically significant reduction in credit card debt (probability value 0.017). For people facing credit constraints, receiving a cash grant will likely need to be used to smooth the shock of the disaster, possibly helping to avoid financial distress. We find that the reduction in credit card debt is due to less credit-constrained individuals reducing their debt levels. More credit-constrained individuals hit by cash tornadoes do not reduce credit card balances, however become less likely to be delinquent (see Section 4.1.4).

Total home debt decreases by $3,922 (6%) in the continuous damage model

[6] Credit card debt in the CCP is measured at a point in time which means that we cannot distinguish individuals that rollover credit card debt from one month to the next and incur interest charges from those that pay their balance in full each month and do not incur interest charges.

(probability value 0.001) for the typical victim who has a home loan continuously in the 12 quarters before the tornado.[7] The reduction is much larger for individuals in severely damaged blocks who, on average, reduce their mortgage debt by about $19,000 (probability value 0.000).

Difference-in-differences (DD) results for the debt outcomes are presented in Appendix Table 5. There is a reduction in home debt for homeowners hit by tornadoes with grants in the high damage blocks (-$2,697, probability value 0.021). However, an increase in home debt for homeowners hit by tornadoes without cash assistance ($19,308, probability value 0.000) is driving the triple difference result. The DD model estimates help to highlight that the impact of the cash grants estimated in our triple difference model is coming from *a drop in* debt for those disaster victims with access to cash grants, and *an increase in* debt for those disaster victims without access to grants. Recall, the maximum assistance was $30,200 in 2010 (Fed [2010]).

We also estimate two additional descriptive DD models so as to better understand what explains the reduction in mortgage debt for hit residents with access to cash grants (Appendix Table 8). First, we estimate the same DD model as above, except that we separately consider hit residents who either move from or stay in the same block following the tornado. The increase in home debt is larger for residents without access to cash grants who move rather than stay. The reduction in home debt is similar for victims of cash tornadoes regardless of whether they move. Second, we estimate a model that separately considers mortgage and home equity debt. The increase in home debt for

[7]Calculated as -$2,179 (panel B column 2) multiplied by 1.8 (average block damage).

victims of no-cash tornadoes is due to an increase in first mortgage debt. These results suggest that when residents hit by no-cash tornadoes move, that they purchase new homes and dramatically increase their mortgage debt (relative to cash tornado victims). However, we are cautious in our interpretation as we do not have an economic model that links migration and home debt.

Figure 3 plots yearly event study estimates for six outcomes using the continuous model. The yearly point estimates for each outcome are marked by squares. The vertical lines with the hash marks represent the 95% confidence interval. The estimates are relative to the year before the tornado. The vertical red line shows the quarter of the tornado (which is dropped from the sample). There is no evidence of any pre-tornado trend for credit card, home, and auto debt. The immediate and persistent reduction in home debt is consistent with the triple difference result in Table 1 column 2 panel B. While the pre-period triple difference estimates for other debt are not statistically distinguishable from zero, there is a hint of a pre-trend in the point estimates. We do not regard this as evidence against our empirical design, since it can be attributed to testing multiple outcomes and sample variation in the hit group for the no cash tornadoes.

4.1.2 Financial Wellbeing

Table 1 columns 4-5 show model estimates for the Equifax Risk Score and 90 day delinquency, two measures of financial health. Overall, the effect on credit score and 90-day delinquency are economically small and statistically insignificant. The exception is for victims in medium damage blocks. Access to cash assistance leads to a 25% decrease (probability value 0.006) in the likelihood of having at least one credit account that is 90 or more days delinquent, and an 1.2% (probability value 0.041) increase in the Equifax Risk Score.

Figure 3 shows the event study analysis for the financial health measures in panels E and F. There is no evidence of any difference in the pre-tornado trends between individuals hit by cash and no-cash tornadoes. The most striking finding is the reduction in the likelihood of having an account that is 90 days delinquent beginning one year after the tornado. The continuous damage triple difference model masked this delayed effect. The delay could be partially mechanical. 90 day delinquency is an outcome measured with a time lag. The delay may also reflect the fact that the worst financial impacts accumulate over time. In Section 4.1.4 we show that the decrease in delinquency rates is completely attributable to more credit constrained individuals.[8]

4.1.3 Migration

Table 2 shows triple difference linear probability model estimates for whether an individual moves out of the Census block or county of residence. Columns (1) and (2) define migration as leaving the block (or county) of residence for at least one quarter. Columns (3) and (4) define migration as leaving the block (or county) of residence for at least three years. We construct the migration panel differently than the main household finance panel. Our goal is to estimate changes in out-migration rates from the block and county. As such, for each quarter we estimate the fraction of individuals who no longer live in the same

[8]The CCP data include a quarterly foreclosure variable that indicates whether an individual had a foreclosure in the past seven years. The fact that new quarterly foreclosures are not very common prevents us from examining how cash assistance affects foreclosure rates. The Appendix provides a detailed discussion.

block or county in the following quarter (and in column (3) and (4) who do not return for three years). This is different from our main household finance panel because the composition of the sample differs from quarter to quarter.

We estimate a 0.65 percentage point or 12% increase (probability value 0.033) in the one quarter block migration rate in the continuous damage model for individuals who have access to cash grants. The percentage point effect is calculated as 0.0035 (panel B column 1) times the average tornado block-level damage of 1.8. The point estimates for the binned damage model are all positive, but imprecisely estimated. Overall, cash grants do not impact more permanent (three year) migration. However, residents of high damage blocks who have access to cash grants are much less likely to permanently migrate following the tornado. One explanation for these findings is that access to cash grants allows individuals to temporarily move residences while major structural repairs to their homes are completed. At the same time, cash grants reduce the likelihood of a permanent move from the neighborhood.

Figure 4 shows the block migration event study analysis for a possibly temporary (one quarter) and a more permanent (three years) move. The figure shows that access to cash grants increases the one quarter block migration rate during the first post-tornado year. The overall effect on the temporary out-migration rate observed in the continuous damage triple difference model is completely due to the impact during the first post-tornado year.

4.1.4 Heterogeneity by Access to Credit Markets

Credit-constrained individuals who are affected by the tornado and who receive cash grants will likely reduce their debt by less than tornado victims who are not credit-constrained. Credit-constrained individuals may also be more likely to become financially distressed. The reason is that credit-constrained individuals are largely shut out of credit markets. In the absence of the cash grants, these individuals would not have been able to borrow and may be less able to manage the financial shock.

The effect of the cash grants may also differ by the age of the tornado victim. Younger tornado victims are likely to have less accumulated savings (e.g. Attanasio [1998]) and may need to rely more on new debt. At the same time, younger residents may be more willing to borrow, since the cost of borrowing can be smoothed over a greater number of future time periods.

Table 3 explores how cash grants impact debt, financial health, and migration based on the likelihood a victim is credit constrained, and by age. We consider two proxies for whether an individual is credit constrained: Equifax Risk Score and available credit. We define available credit as the difference between total credit card debt and the total credit card debt limit. We separately divide our sample into thirds based on age, Equifax Risk Score, and available credit, and compare outcomes for the lowest third to the highest third using the continuous damage model (Gelman and Park [2008]; Parker et al. [2013]).

Cash grants lead to a larger reduction in credit card debt for less credit-constrained tornado victims. Individuals in severely damaged blocks who have high available credit at the time of the tornado reduce their quarterly credit card debt by $904 (calculated as the product of -$226 and EF damage level 4). We estimate an economically small decrease of $8 for those with low available credit. The coefficients are statistically different at the 0.000 probability level.

Cash grants lead younger individuals to reduce their credit card debt by more than older individuals, but the point estimates are too imprecise to reject the null hypothesis that the estimates are equivalent.

Credit constrained individuals with access to cash grants are less likely to forgo paying a bill. A hit resident with a low credit score is 14% less likely (probability value 0.000) to have a 90 day delinquency (calculated as -0.042 times 1.8 divided by 0.525). There is no effect on tornado victims who are less credit constrained. The difference between the estimated effects on delinquency rates for low and high credit constrained tornado victims is statistically significant using both our available credit and credit score proxies.[9]

4.1.5 Robustness

We show robustness analysis for our triple difference model for each of our household finance and migration outcomes in the appendix (Appendix Tables 15 - 20 and Appendix Figures 7 - 10). We estimate two alternative samples for each outcome. First, we show results from the balanced tornado sample. Second, we estimate the model on the full sample that includes the Wayne, NE tornado that is dropped from our preferred sample due to differing pre-trends. Overall, the estimates are qualitatively similar to our main sample. We limit our discussion in the text to a short summary of the balanced tornado sample.

The mean levels of the CCP and Census variables for the hit groups in the

[9] Our results are supported by Del Valle et al. [2019] who find that high-quality borrowers are more likely to have new credit card originations after flooding from Hurricane Harvey. Billings et al. [2019] find that financially constrained flooded residents have higher personal bankruptcy rates following Hurricane Harvey. Collier et al. [2020] examine how businesses cope with flooding from Hurricane Harvey and find that business delinquencies are largest for those businesses already showing signs of financial distress before the flood.

balanced tornado sample are much more similar, as compared to in the main sample (see Appendix Table 14). Individuals hit by cash and no-cash tornadoes have identical pre-tornado Equifax Risk Scores. The balanced sample model estimates are less precise, but suggest that there is limited heterogeneity in the response to cash assistance. The most striking differences between the balanced sample and our preferred model are that the estimates for the reduction in home debt and the increase in the propensity to migrate are both larger in the balanced sample, while the reduction in credit card debt is smaller. Our heterogeneity estimates show that the larger reduction in home debt is partially due to larger reductions for older and low credit score individuals. Still, all of the point estimates from our main sample are within the balanced sample confidence intervals.

4.2 Local Businesses

Business establishments are vulnerable to natural disasters. Basker and Miranda [2017] estimate a 30 percentage point decrease in the survival rate of businesses damaged by a severe hurricane, relative to those not damaged. Smaller-sized establishments are at a greater risk of closing (Basker and Miranda [2017]). The FEMA claims that almost 40% of small businesses close after a flood-related natural disaster (FEMA [2019]).

Federal cash grant assistance to individuals can aid local businesses in two important ways. First, when tornado-affected individuals receive cash assistance a portion is spent locally increasing revenues for local establishments. Damaged business establishments may disproportionately benefit from the increased demand for their goods and services following a disaster, relative to nearby undamaged establishments.[10] Second, cash assistance to individuals that happen to be small business owners may positively affect establishment outcomes. In 2016, 47 percent of establishments employed four or fewer people (SUSB [2018]). The median establishment size in our sample is four. Around half of all establishments are operated out of a home.[11]

4.2.1 Business Growth and Employment

We explore the effect of cash assistance on the number of establishments and the level of employment. Figure 5 shows the trends in the number of establishments and employees for establishments located in a hit Census block at the time of a tornado, and for establishments nearby, but outside the tornado path. The figure plots residual means from a regression of block-level establishment outcomes on year dummy variables. The horizontal axis shows tornado event time. The trends are plotted relative to when the tornado occurred. The vertical line at -1 indicates the last year before the tornado, while points to the right of the vertical line are years after the tornado.

The left side of Figure 5 plots the trends separately for tornadoes where affected residents were able to access cash grants (circles) and where no cash

[10] Recall that identification in our model comes from taking the difference between establishment outcomes in damaged blocks inside the tornado path and in the undamaged buffer blocks. There are at least two underlying mechanisms consistent with the higher business demand channel. First, our model will estimate (a lower bound) on the role of the cash grants provided that individuals spend a higher fraction of the grant money at locally damaged businesses as compared to establishments farther away. Second, even if residents who receive cash grants spend the money equally on locally damaged establishments and buffer region establishments, the increased business revenue is likely to be more important for damaged establishments (i.e. higher marginal impact) to, for example, prevent closure.

[11] The SBA reports that 52 percent of all small businesses are home-based (SBA [2012]). The SBA defines a small business as one with fewer than 500 employees. Over 99 percent of businesses have fewer than 500 employees (SUSB [2018]).

grants were distributed (triangles). Three facts emerge. First, trends for the two outcomes in the years leading up to a tornado are roughly parallel for the hit and nearby establishments affected by a tornado where no cash grants were distributed (dashed lines). The same is true for hit and nearby establishments of cash tornadoes (solid lines). Second, the trends in establishment outcomes are increasing slightly in areas that are later hit by a cash tornado. The trends are flatter for establishments which are later affected by a no-cash tornado. Third, in the four years after a tornado, the trend in the number of establishments and employees is flat for establishments affected by tornadoes where residents received cash grants. During the same post-tornado period there is a reduction in the number of establishments and employees in areas hit by tornadoes where residents did not receive cash assistance. The reduction is greatest in neighborhoods hit by no-cash tornadoes.

The right side of Figure 5 plots the difference in establishment outcomes between blocks hit by and nearby a tornado. This difference is plotted separately for tornadoes where residents did and did not receive cash grants. The triple difference model assumes that in the absence of the grants the difference in outcomes after a tornado would be the same for the two groups. The trends to the left of the vertical line are roughly parallel, providing evidence for the validity of the key triple difference model identifying assumption.

Table 4 columns (1) and (2) show triple difference estimates of the effect of cash grants on the number of establishments and employment. Establishments in damaged blocks where residents have access to cash grants benefit economically. We estimate that there are 9% more establishments (probability value

0.047) and 12% more employees (probability value 0.095) in blocks with average tornado damage in our sample when cash grants are available to residents. The business survival estimate for medium damage blocks in our binned damage model is 13% (probability value 0.039). This suggests the possibility of a nonlinear response in how cash assistance impacts business survival based on the underlying block damage. However, the three estimates are too imprecise to reject equality in the binned model.

Table 4 columns (3)-(6) show DD model estimates separately for cash and no-cash tornadoes. There are two key patterns in the binned damage level DD model estimates. First, the greater the block-level damage, the worse the establishment outcomes post-tornado. For example, column (3) shows that there is a slight increase (4.7%, probability value 0.002) in the number of establishments located in low damage blocks where there is cash assistance, relative to establishments in the nearby neighborhoods that are not hit by the tornado. In medium damage blocks there is a 6.0% decrease (probability value 0.015) in the number of establishments. The decrease is largest in the most-damaged blocks (-12.7%, probability value 0.000). Second, the reduction in the number of establishments and the level of employment is consistently greater at each damage level for establishments hit by no-cash tornadoes, as compared to establishments and employment in blocks hit by a cash tornado.

4.2.2 Heterogeneity by Industry, Age, and Size

Table 5 presents estimation results from our triple difference continuous damage model that examine how the treatment effects vary by establishment industry, age, and size. The goal is to shed light on how cash assistance to the local population affects business survival and growth.

Panel A of Table 5 estimates the model separately for manufacturing and non-manufacturing establishments. We classify each establishment as manufacturing or non-manufacturing using its two digit SIC. We view manufacturing as a proxy for whether an establishment is likely to rely on a local or non-local consumer base. Manufacturing establishments are more likely to produce goods for consumers outside the local economy. By contrast, non-manufacturing establishments, which include the retail, service, and construction industries, are more likely to rely on local demand.

The positive effect that the cash grants have on the number of establishments and employees is completely attributable to non-manufacturing, local service-driven establishments. The estimated effect on manufacturing establishments and manufacturing employment is close to zero and not statistically significant. The estimates for non-manufacturing establishments are more than an order of magnitude larger than the manufacturing estimates, and are nearly identical to full sample estimates in Table 4. Appendix Tables 9 and 10 show triple difference estimates for establishments in each of the "1 digit" industries that make up the non-manufacturing category. The largest impacts are observed in the service, construction, and retail sectors. The estimate for public sector employment is small and not statistically different from zero.

Panel B of Table 5 provides evidence that the positive effects on business establishments are due to an improvement in the survival rate of existing businesses, and are not driven by growth in entrepreneurship (new business establishments). We estimate our model separately for establishments in operation for one year or less and for establishments that have been open for at least four years. We estimate a fairly precise zero for new establishments. The effect on existing establishments is positive, an order of magnitude larger, and statistically different from zero (probability value 0.059).

Panel C of Table 5 divides establishments into small and large-sized establishments based on the size distribution in our sample. Roughly one-third of the establishments employ three or fewer employees, while one-third employ greater than seven employees (see Appendix Figure 5). We estimate an increase in the number of establishments of 7.7% (probability value 0.005) and employees of 8.6% (probability value 0.019) at very small establishments at the mean damaged block. The estimates for larger establishments are an order of magnitude smaller, close to zero, and not statistically significant. We interpret the size of establishment results as evidence that smaller establishments are more vulnerable to the economic shock caused by the tornado, and thus benefit more when cash grants are provided to the local population. This finding is consistent with other recent research on the vulnerability of small businesses (e.g. Cole and Sokolyk [2016]; Greenstone et al. [2015]).[12]

4.2.3 New Purchases and Sales

Improvements in establishment survival rates in cash grant tornado-affected areas are driven by small, existing, local service and sales-oriented establishments. Ideally, we would be able to link individual-level purchases (including the home address) with the establishment location of the purchase. We are

[12] We are not able to provide any insight as to why small businesses are more vulnerable to the economic impact of tornadoes. Possible explanations include lower capital reserves and more difficulty accessing credit markets (e.g. Runyan [2006]; Basker and Miranda [2017]).

not aware of any publicly available data that provides this linkage. Instead, we separately show that new vehicle purchases are greater for individuals hit by cash tornadoes, and that sales are larger for local service and sales-oriented establishments in these same tornado-affected areas. Large tornadoes often destroy motor vehicles. Auto purchase is a consumption response we can measure using the CCP data. We follow Ganong and Noel [Forthcoming] and use new auto loans as a proxy for new auto purchases. Approximately, 80% of new vehicle purchases use auto loans (Di Maggio et al. [2017]).

New quarterly auto purchases and balances both increase by more than 50% (probability values 0.000 and 0.000, respectively) for individuals in the most-damaged blocks (see Appendix Table 11). The triple difference sales regression results are greater for retail and service establishments located in damaged neighborhoods with cash assistance, and follow the same pattern as the establishment survival estimates in Table 4.[13] Low available credit and low credit score individuals increase their purchases when they have access to cash grants (see Appendix Table 12). These groups are the most credit-constrained, and in the absence of the cash grants, are more likely to reduce consumption. Individuals who are not credit-constrained do not change their consumption based on access to cash assistance.

[13]As discussed in the appendix, the sales data are collected from survey responses at the time Infogroup contacts each establishment which are subject to measurement error that could bias estimates towards zero. However, whether an establishment exists is not based on a survey response.

4.2.4 Robustness

We show business establishment survival and employment regression estimates for the robustness samples in the appendix (Appendix Tables 21 - 24). The balanced tornado sample estimates are very similar to those in our main sample. The coefficient estimates from the 35 tornado sample are somewhat smaller in magnitude. The difference is most stark in the no-cash tornado DD model estimates. When we include the Wayne, NE tornado in the sample (a no-cash tornado) the estimates are much less precise. In our view, the reason for this result is the differing business pre-tornado trends for the Wayne, NE tornado.

5 Discussion

The goal of the IA cash grant program is to provide assistance to individuals who incur direct expenses from a natural disaster. At the same time, we show that the cash grants increase business survival and lead to greater employment in damaged tornado blocks where individuals receive cash grants. In this section, we calculate a rough measure for the cost of each job retained or created by the cash grants.[14]

We calculate the cost per job created from cash grants using two approaches. In the first approach, we use the total amount of cash assistance

[14]Figure 5 shows a dramatic decrease in the number of employees in damaged blocks that did not receive cash grants (rather than a disproportionate increase in blocks where individuals received grants). The raw trends in the figure, along with the separate model estimates for new versus existing business establishments (Table 5 panel B), suggest that the difference in jobs between cash and no-cash disaster blocks is largely due to "retained" jobs rather than "new" jobs. Unfortunately, the data do not allow us to distinguish between newly created jobs and the retention of existing jobs. We follow the literature and streamline the exposition by (hereafter) referring to our estimates as jobs created.

allocated throughout all of the disaster counties in the state. In the second approach, we only consider the cost of the cash grants that went to ZIP codes hit by (part of) a tornado. The second approach is a policy evaluation that narrows the cash assistance to where we observe job creation. Still, we view the cost per job estimates as an upper bound for two reasons. First, we limit the jobs created calculation to establishments with fewer than three employees. In all likelihood, there are jobs created at larger establishments. Second, we consider cash assistance provided to everyone hit in a ZIP code, even though a tornado typically hits only a very small fraction of the ZIP.

Our baseline calculation includes the dollar amount of the grants along with the associated IA administrative costs. The IA administrative costs are estimated from program documents. Our baseline calculation follows Brown and Earle [2017] who use administrative data to examine two large SBA (non-disaster) business loan programs. We estimate a cost of $242 thousand per job when we consider the total amount of cash assistance, and $108 thousand when we limit the evaluation to where we measure job creation (hit blocks). Our more comprehensive calculation estimates the net job cost inclusive of other program costs and fiscal externalities (e.g. Bastian and Jones [2019]; Hendren [2016]). Our preferred estimate shows a cost of $75 thousand per job, using our second approach, when we adjust our baseline estimate to include the SBA, federal tax, and unemployment cost savings. The appendix provides a detailed discussion of the cost calculation.

6 Conclusion

We provide some of the first evidence for how cash grants to residents following a natural disaster affect individual-level financial and migration outcomes. We also examine how the cash grants affect the survival rate of establishments in disaster-affected neighborhoods. We construct a new dataset that combines individual-level credit bureau data, establishment-level business information, and block-level tornado damage from 34 large, devastating tornadoes.

There are three main household finance conclusions. First, we find that disaster-affected individuals in high damage blocks with access to cash grants have less credit card debt following a disaster, relative to disaster victims without access to cash grants. Second, cash grants lead to a dramatic reduction in home debt for residents in high damage blocks. Third, consistent with the life cycle/permanent income hypothesis, credit-constrained individuals who have access to cash grants have lower rates of bill delinquency.

Cash grants increase temporary migration from the disaster-affected neighborhood. At the same time, access to cash grants dramatically reduces more permanent migration for residents in the most-damaged blocks.

Cash grants to residents in disaster impacted neighborhoods increase the survival rate of business establishments in these neighborhoods. The establishments most reliant on local demand benefit the most. A rough, upper bound estimate for the cost of each retained job is $75 thousand dollars.

7 References

Notice of maximum amount of assistance under the individuals and households program. Technical report, Federal Register, October 2010.

Federal disaster assistance. individual assistance requests often granted, but fema could better document factors considered. Technical Report GAO-18-366, Government Accountability Office (GAO), May 2018.

Protecting your business. Technical report, FEMA, 2019.

Anna Aizer, Shari Eli, Joseph Ferrie, and Adriana Lleras-Muney. The long-run impact of cash transfers to poor families. *American Economic Review*, 106(4), 2016.

Orazio P. Attanasio. Cohort analysis of savings behavior by u.s. households. *Journal of Human Resources*, 33, 1998.

Sarah Baird, Craig McIntosh, and Berk Ozler. Cash or condition? evidence from a cash transfer experiment. *Quarterly Journal of Economics*, 126(4), 2011.

Emek Basker and Javier Miranda. Taken by storm: business financing and survival in the aftermath of hurricane katrina. *Journal of Economic Geography*, 2017.

Jacob E. Bastian and Maggie R. Jones. Do eitc expansions pay for themselves? effects on tax revenue and public assistance spending. *Working Paper*, 2019.

Stephen B. Billings, Emily A. Gallagher, and Lowell Ricketts. Let the rich be flooded: The unequal impact of hurricane harvey on household debt. *Working Paper*, October 2019.

Leah Platt Boustan, Matthew E. Kahn, and Paul W. Rhode. Moving to higher ground: Migration response to natural disasters in the early twentieth century. *American Economic Review: Papers and Proceedings*, 102(3), 2012.

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

Meta Brown, Andrew Haughwout, Donghoon Lee, Jolle Scally, and Wilbert an der Klaauw. Measuring student debt and its performance. *Federal Reserve Bank of New York Staff Reports*, (668), April 2014.

Andrew Brudevold-Newman, Maddalena Honorati, Pamela Jakiela, and Owen Ozier. A firm of one's own, experimental evidence on credit constraints and occupational choice. *World Bank Working Paper*, February 2017.

Rebel Cole and Tatyana Sokolyk. Who needs credit and who gets credit? evidence from the surveys of small business finances. *Journal of Financial Stability*, 24, 2016.

Benjamin L. Collier, Lawrence S. Powell, Marc A. Ragin, and Xuesong You. Financing severe climate risk: Evidence from businesses during hurricane harvey. *Working Paper*, October 2020.

Alejandro Del Valle, Therese Scharlemann, and Stephen Shore. Household financial behavior after hurricane harvey. *Working Paper*, March 2019.

Tatyana Deryugina. The fiscal cost of hurricanes: Disaster aid versus social insurance. *American Economic Journal: Economic Policy*, 9(3), 2017.

Tatyana Deryugina, Laura Kawano, and Steven Levitt. The economic impact of hurricane katrina on its victims: Evidence from individual tax returns. *American Economic Journal: Applied Economics*, 10(2), 2018.

Marco Di Maggio, Amir Kermani, Benjamin J. Keys, Tomasz Piskorski, Rodney Ramcharan, Amit Seru, and Vincent Yao. Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging. *American Economic Review*, 107(11), 2017.

Roger Edwards, John T. Ferree James G. LaDue, Kevin Scharfengerg, Chris Maier, and William L. Coulbourne. Tornado intensity estimation. *American Meteorological Society*, May 2013.

Ariel Fiszbien and Norbert Schady. Conditional cash transfers: Reducing present and future poverty. *World Bank*, 2009.

Justin Gallagher and Daniel Hartley. Household finance after a natural disaster: The case of hurricane katrina. *American Economic Journal: Economic Policy*, 9 (3), 2017.

Peter Ganong and Pascal Noel. Liquidity vs. wealth in household debt obligations: Evidence from housing policy in the great recession. *American Economic Review*, Forthcoming.

Andrew Gelman and David K. Park. Splitting a predictor at the upper quarter or third and the lower quarter or third. *The American Statistician*, 62(4), November 2008.

Michael Greenstone, Alexandre Mas, and Hoai-Luu Nguyen. Do credit market shocks affect the real economy? quasi-experimental evidence from the great recession and 'normal' economic times. *Working Paper*, September 2015.

Jeffrey A. Groen, Mark J. Kutzbach, and Anne E. Polivka. Storms and jobs: The effect of hurricanes on individuals' employment and earnings over the long term. *Journal of Labor Economics*, Forthcoming.

Nathaniel Hendren. The policy elasticity. *Tax Policy and the Economy*, 30(1), 2016.

Richard Hornbeck. The enduring impact of the american dust bowl: Short- and long-run adjustments to environmental catastrophe. *American Economic Review*, 102 (4), 2012.

Donghoon Lee and Wilbert van der Klaauw. An introduction to the frbny consumer credit panel. Technical Report 479, Federal Reserve Bank of New York, November 2010.

Lauren J. Lee and Bambi Hall. Louisiana's recovery. Technical report, Federal Highway Administration, 2011.

Francis X. McCarthy. Fema's disaster declaration process: A primer. Technical Report 18, Congressional Research Service, May 2011.

Costas Meghir and Luigi Pistaferri. Earnings, consumption and life cycle choices. *Handbook of Labor Economics*, 4b, 2011.

Munich Re. Topics geo natural catastrophes 2012, report. Technical report, Munich Re, 2013.

Munich Re. Preliminary sigma estimates for 2017: global insured losses of usd 136 billion are third highest on sigma records. Technical report, Munich Re, 2017.

National Weather Service. The enhanced fujita scale (ef scale). Technical report, National Weather Service, 2014. URL https://www.spc.noaa.gov/efscale/.

National Weather Service. 7th anniversary of the joplin tornado - may 22nd, 2011. Technical report, National Weather Service, 2018.

Jonathan A. Parker, Nicholas S. Souleles, David S. Johnson, and Robert McClelland. Consumer spending and the economic stimulus payments of 2008. *American Economic Review*, 103(6), 2013.

Rodney C. Runyan. Small business in the face of crisis: Identifying barriers to recovery from a natural disaster. *Journal of Contingencies and Crisis Management*, 14(1), 2006.

Small Business Administration SBA. Frequently asked questions. Technical report, September 2012.

Juan Carlos Serrato and Owen Zidar. Who benefits from state corporate tax cuts? a local labor markets approach with heterogeneous firms. *American Economic Review*, 106(9), 2016.

G.L. Sills, N.D. Vroman, R. E. Wahl, and N. T. Schwanz. Overview of new orleans levee failures. *Journal of Geotechnical and Geoenvironmental Engineering*, 134 (5), May 2008.

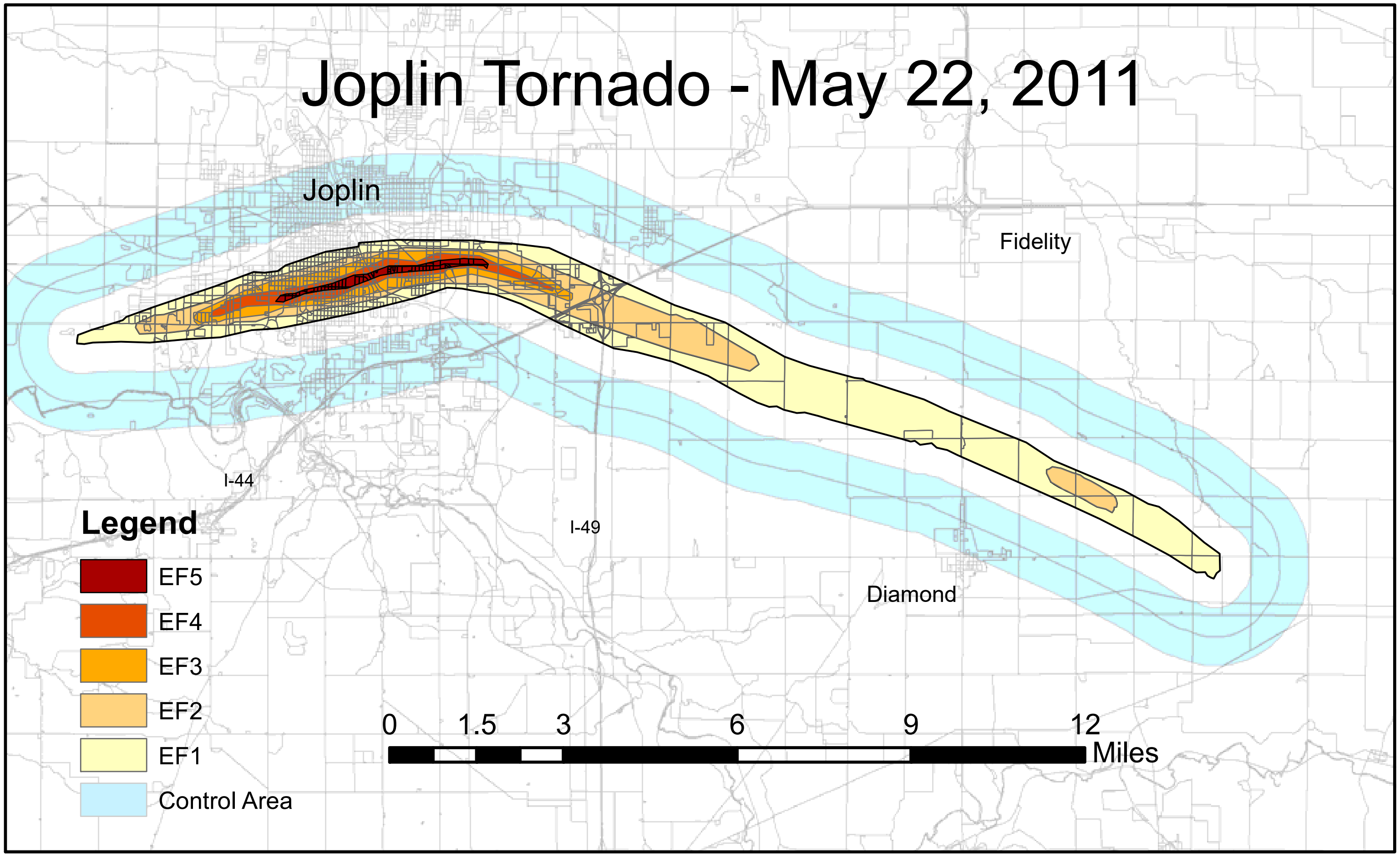
Emmanuel Skoufias and Susan W. Parker. Conditional cash transfers and their impact on child work and schooling: Evidence from the progresa program in mexico. *Economia*, 2(1), 2001.

US Census Bureau SUSB. 2016 susb annual data tables by establishment industry. Technical report, December 2018.

Irina A. Telyukova. Household need for liquidity and the credit card debt puzzle. *The Review of Economic Studies*, 80, July 2013.

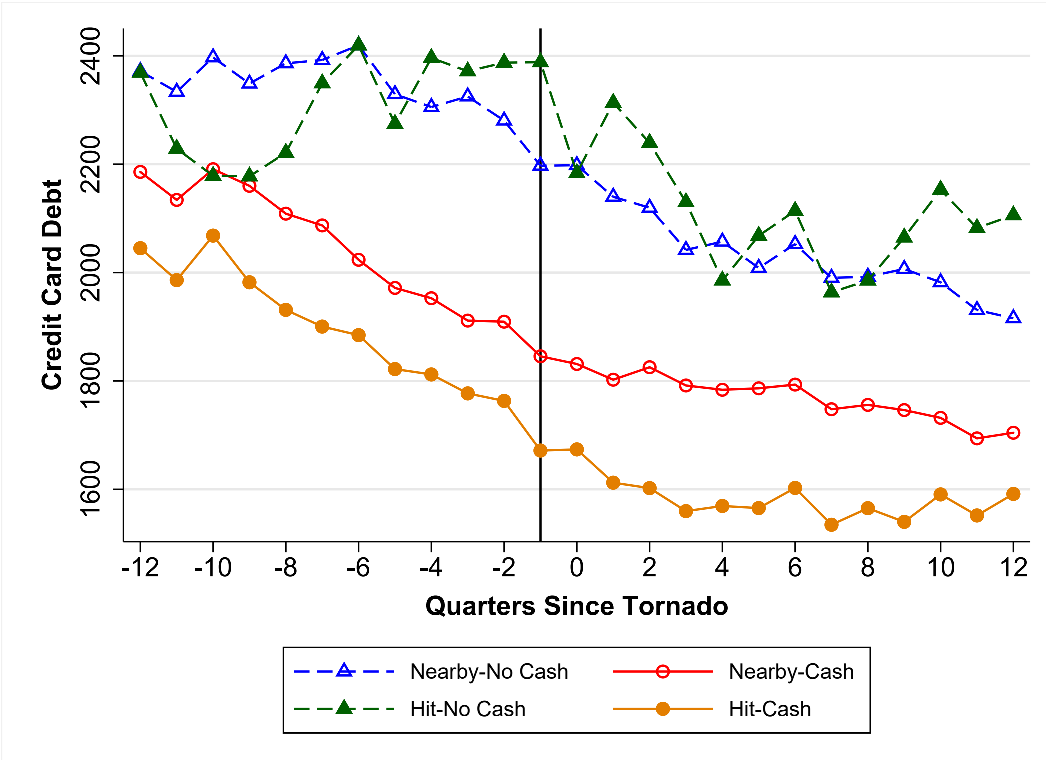
8 Figures and Tables

Figure 1: Tornado Damage Map for Joplin, MO 2011 Tornado



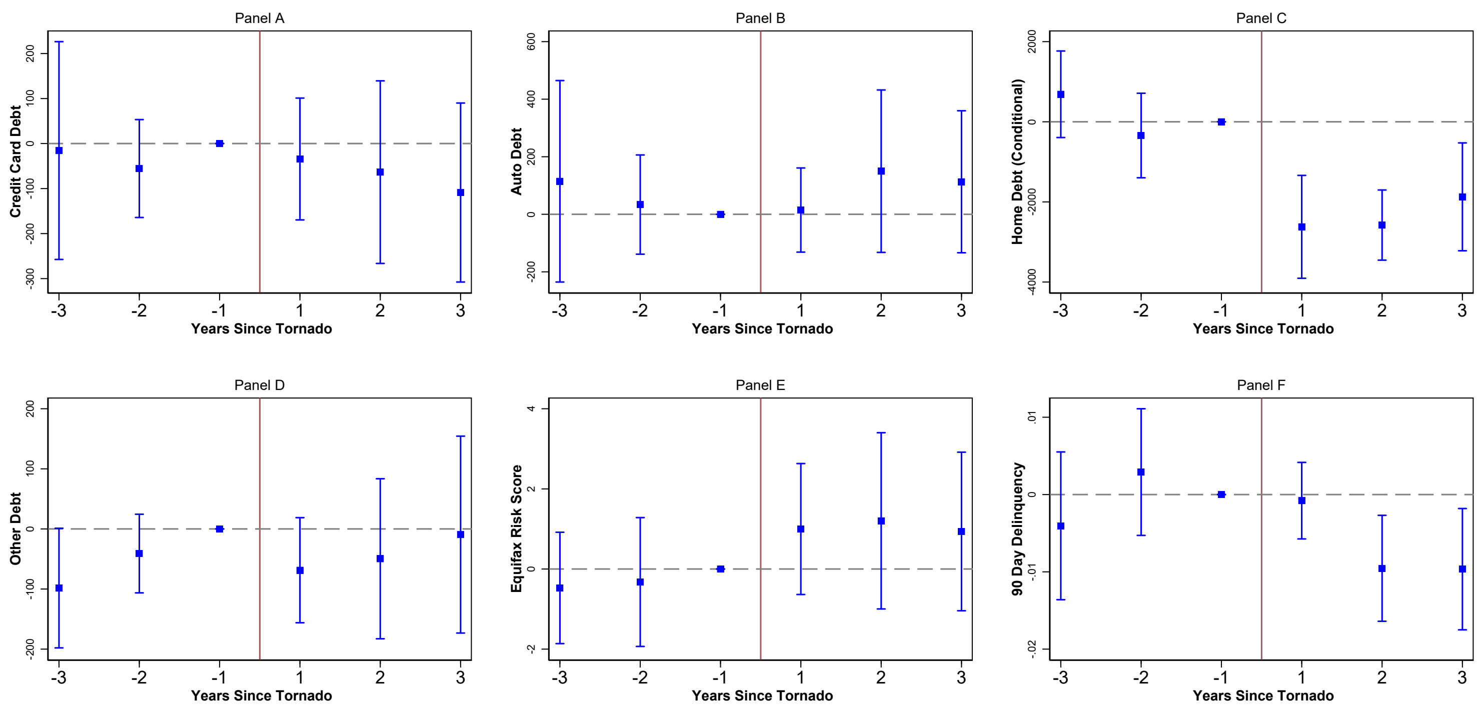
The figure shows the damage map for an EF5 tornado that hit Joplin, Missouri on May 22, 2011. The tornado path is outlined in black. The control area is in blue and located between 0.5 and 1.5 miles from the edge of the damage path. The tornado path and the control area are overlaid on a US Census block map (background grey lines). Sources: National Weather Service, US Census.

Figure 2: Trends in Credit Card Debt



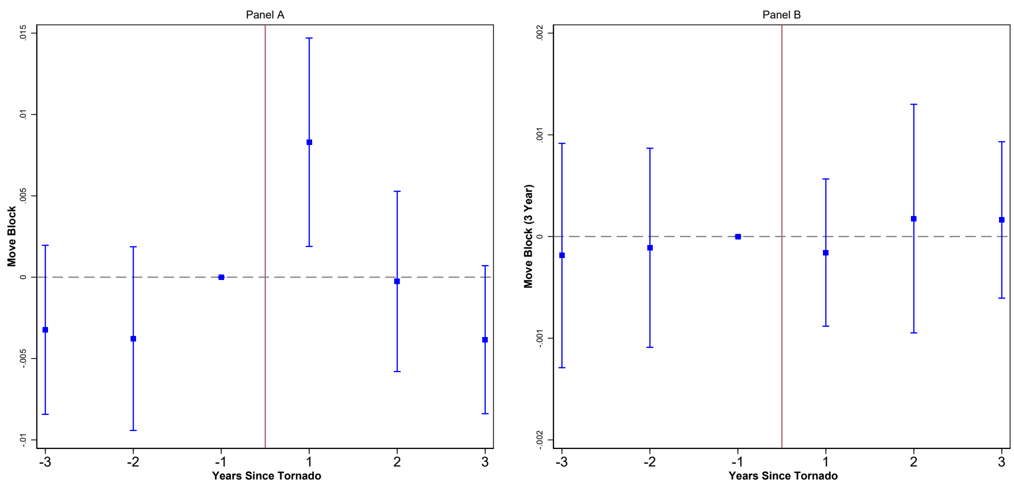
The figure plots the mean credit card balance (bank cards) of four groups of individuals: non-hit residents who lived in the 0.5 to 1.5 mile buffer area around the tornadoes that did not receive cash grants (dashed blue triangles), hit residents who lived in the damage path of tornadoes that did not receive cash grants (dashed green triangles), non-hit residents who lived in in the buffer areas of the tornadoes that did receive cash grants (solid red circles), and hit residents from tornadoes that received cash grants (solid orange circles). All dollar denominated variables are expressed in real terms in 2010 dollars. The vertical line indicates the last quarter before a tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 3: Yearly Event Study Analysis of Debt and Financial Wellbeing



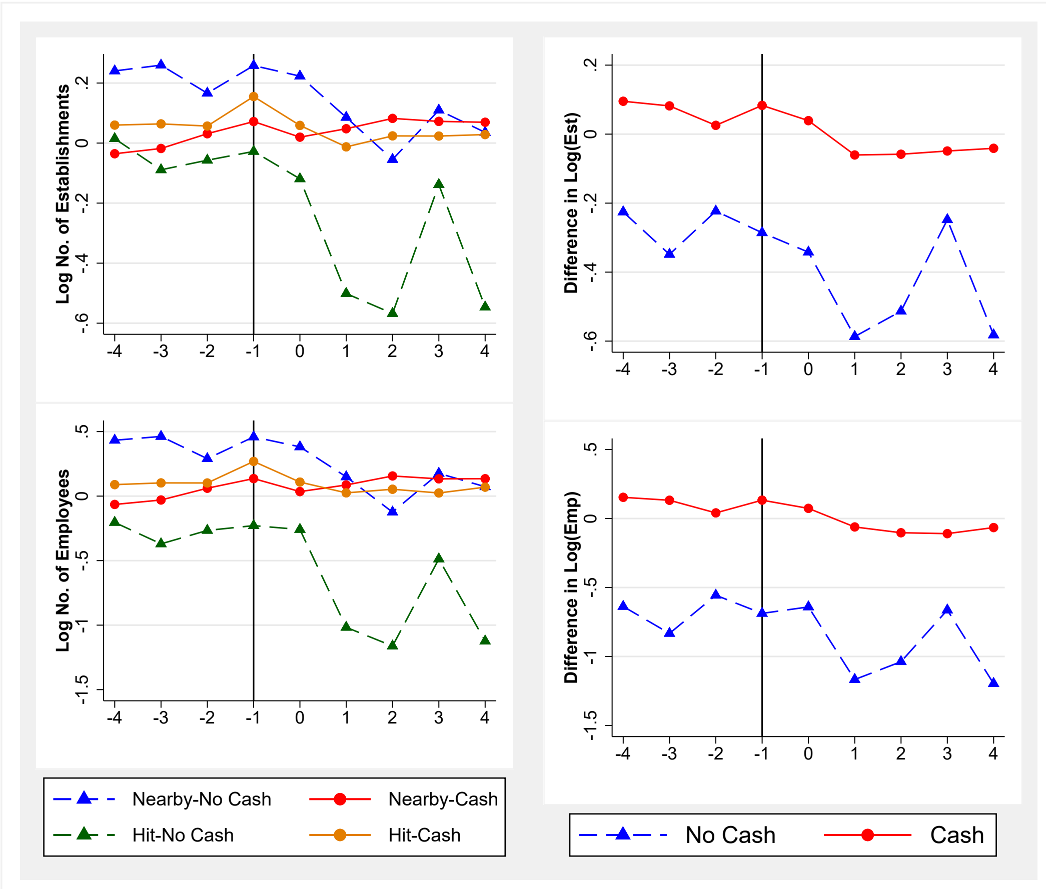
The figure shows yearly event study estimates and 95% confidence intervals for the outcomes in Table 1. The event study model replace the $Post_{i,t}$ variable in the continuous damage model with a set of binary variables that indicate the number of years the observation is either before or after the tornado. The year before the tornado is excluded from the model and serves as the reference time period. We drop the quarter of the tornado (red vertical line in each panel) from the panel before running the model. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 4: Yearly Event Study Analysis of Block Migration



The figure shows yearly event study estimates and 95% confidence intervals for temporary (one quarter) and more permanent (three year) block migration (Table 2, columns 1 and 3). The event study model replace the $Post_{i,t}$ variable in the continuous damage model with a set of binary variables that indicate the number of years the observation is either before or after the tornado. The year before the tornado is excluded from the model and serves as the reference time period. We drop the quarter of the tornado (red vertical line in each panel) from the panel before running the model. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 5: Trends in Business Outcomes



The figure shows the trends in the number of establishments and employees for establishments located in hit Census blocks at the time of a tornado, and for establishments near to the tornado path. The figure plots means of the residuals from a regression of block-level establishment outcomes on year dummy variables. The left side of the figure plots the trends separately for tornadoes where affected residents were able to access cash grants (circles) and where no cash grants were distributed (triangles). The right side of figure plots the difference in establishment outcomes between blocks hit by and nearby to a tornado. This difference is plotted separately for tornadoes where residents did and did not receive cash grants. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 1: **Household Finance Triple Difference Estimates**

Dependent Variable:	Credit Card (1)	Home (Conditional) (2)	Auto (3)	Other (4)	Equifax Risk Score (5)	90 Day Delinquency (6)
Panel A: Pooled						
Cash Tornado x Post x Hit	-164	-4,065	339	-46	1.7	0.0047
	(191)	(1,869)	(241)	(149)	(3.4)	(0.0166)
Dependent Variable Mean	$2,411	$66,371	$3,143	$1,300	671.7	0.2073
R-squared	0.013	0.079	0.003	0.007	0.018	0.001
Observations	496,708	123,602	496,708	496,708	492,439	496,708
Panel B: Continuous Damage						
Cash Tornado x Post x Hit	-39	-2,179	42	7	1.3	-0.0051
	(81)	(574)	(71)	(52)	(0.9)	(0.0035)
Dependent Variable Mean	$2,411	$66,371	$3,143	$1,300	671.7	0.2073
R-squared	0.013	0.079	0.003	0.007	0.018	0.001
Observations	496,708	123,602	496,708	496,708	492,439	496,708
Panel C: Binned						
Cash Tornado x Post x Low	-405	-3,827	440	-227	-2.3	0.0405
	(239)	(2,721)	(386)	(148)	(2.9)	(0.0215)
Dependent Variable Mean	$2,287	$68,614	$3,148	$1,362	672.6	0.2085
Cash Tornado x Post x Medium	425	598	346	240	8.3	-0.0509
	(327)	(2,353)	(385)	(228)	(3.8)	(0.0170)
Dependent Variable Mean	$2,532	$65,659	$3,429	$1,320	672.7	0.2058
Cash Tornado x Post x High	-774	-19,479	-289	-153	1.3	0.0001
	(307)	(2,414)	(295)	(94)	(2.3)	(0.0109)
Dependent Variable Mean	$2,611	$59,365	$2,527	$1,033	666.4	0.2059
R-squared	0.013	0.079	0.003	0.007	0.018	0.001
Observations	496,708	123,602	496,708	496,708	492,439	496,708

The table shows triple difference estimates for six different outcomes. The model includes individual and quarter fixed effects. Only the triple difference coefficients of interest are reported. The pooled coefficients in panel A consider a block as hit if more than 50% of the block is inside the tornado path. Panel B replaces the binary hit variable with a continuous block-level damage variable. The binned coefficients in panel C are estimated separately for individuals in blocks with low (F/EF < 1), medium (F/EF ≥ 1 & < 3), and high (F/EF ≥ 3) damage. Dependent variable means are for the last quarter before a tornado. The debt variables are winsorized at 99%. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 2: **Block and County Migration Estimates**

Dependent Variable:	Move From Block	Move From County	Move From Block	Move From County
Duration:	1 Quarter		3 Years	
	(1)	(2)	(3)	(4)
Panel A: Pooled				
Cash Tornado x Post x Hit	0.0083	0.0051	-0.0006	-0.0008
	(0.0037)	(0.0036)	(0.0010)	(0.0005)
Dependent Variable Mean	0.0537	0.0247	0.0011	0.0006
R-squared	0.006	0.003	0.000	0.000
Observations	763,632	763,632	763,632	763,632
Panel B: Continuous Damage				
Cash Tornado x Post x Hit	0.0035	0.0018	-0.0001	-0.0001
	(0.0016)	(0.0008)	(0.0002)	(0.0002)
Dependent Variable Mean	0.0537	0.0247	0.0011	0.0006
R-squared	0.006	0.003	0.000	0.000
Observations	763,632	763,632	763,632	763,632
Panel C: Binned Damage Levels				
Cash Tornado x Post x Low	0.0081	0.0086	-0.0013	-0.0008
	(0.0059)	(0.0080)	(0.0018)	(0.0005)
Dependent Variable Mean	0.0476	0.0219	0.0003	0.0000
Cash Tornado x Post x Medium	0.0092	0.0010	0.0008	-0.0005
	(0.0049)	(0.0037)	(0.0008)	(0.0010)
Dependent Variable Mean	0.0587	0.0258	0.0026	0.0015
Cash Tornado x Post x High	0.0043	-0.0009	-0.0019	-0.0014
	(0.0091)	(0.0040)	(0.0005)	(0.0006)
Dependent Variable Mean	0.0666	0.0333	0.0011	0.0011
R-Squared	0.006	0.003	0.000	0.000
Observations	763,632	763,632	763,632	763,632

The table shows triple difference estimates for whether an individual hit by a tornado moves from their census block or county of residence. Columns (1) and (2) define a move as being for (at least) one quarter, while columns (3) and (4) define a move as being for (at least) three years. The pooled coefficients in panel A consider a block as hit if more than 50% of the block is inside the tornado path. Panel B replaces the binary hit variable with a continuous block-level damage variable. The binned coefficients in panel C are estimated separately for individuals in blocks with low (F/EF < 1), medium (F/EF ≥ 1 & < 3), and high (F/EF ≥ 3) damage. Dependent variable means are for the last quarter before a tornado. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 3: **Household Finance Triple Difference Estimates - Heterogeneity**

Dependent Variable:	Credit Card	Home (Conditional)	Auto	90 Day Delinquency	Move from Block	Move from Block (3 Year)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Available Credit						
Low Available Credit						
Cash Tornado x Post x Hit	-2	-180	161	-0.0102	0.0029	0.0002
	(53)	(943)	(59)	(0.0080)	(0.0016)	(0.0007)
Dependent Variable Mean	$238	$55,950	$1,526	0.3150	0.0524	0.0004
Observations	152,278	13,439	152,278	152,278	246,950	246,950
High Available Credit						
Cash Tornado x Post x Hit	-226	-5,037	76	0.0037	0.0094	0.0002
	(116)	(946)	(90)	(0.0039)	(0.0032)	(0.0004)
Dependent Variable Mean	$4,523	$67,656	$3,797	0.0391	0.0453	0.0019
Observations	170,386	67,901	170,386	170,386	262,603	262,603
Panel B: Credit Score						
Low Equifax Credit Score						
Cash Tornado x Post x Hit	76	1,388	256	-0.0416	-0.0014	-0.0003
	(72)	(1,444)	(179)	(0.0094)	(0.0021)	(0.0002)
Dependent Variable Mean	$1,556	$57,003	$2,497	0.5249	0.0713	0.0005
Observations	161,520	21,380	161,520	161,520	246,339	246,339
High Equifax Credit Score						
Cash Tornado x Post x Hit	-17	-4,262	9	0.0004	0.0067	0.0001
	(90)	(872)	(77)	(0.0004)	(0.0025)	(0.0004)
Dependent Variable Mean	$2,090	$72,028	$2,747	0.0000	0.0313	0.0005
Observations	165,527	55,420	165,527	165,527	245,138	245,138
Panel C: Age						
Young						
Cash Tornado x Post x Hit	-161	1,138	233	-0.0182	-0.0040	-0.0006
	(130)	(1,119)	(147)	(0.0078)	(0.0021)	(0.0002)
Dependent Variable Mean	$1,697	$73,944	$3,279	0.2894	0.0723	0.0010
Observations	168,246	27,112	168,246	168,246	254,855	254,855
Old						
Cash Tornado x Post x Hit	-23	-1,516	-236	0.0022	0.0054	-0.0001
	(92)	(720)	(129)	(0.0063)	(0.0024)	(0.0004)
Dependent Variable Mean	$2,372	$53,070	$2,378	0.1066	0.0345	0.0013
Observations	162,810	39,133	162,810	162,810	261,768	261,768

The table shows triple difference heterogeneity estimates for six outcomes (omitting credit score, other debt, and county migration) in Tables 1 and 2 using the continuous damage model. The difference is that the model is estimated separately on two groups of individuals (lower and upper terciles) based on available credit (panel A), Equifax Risk Score (panel B), and age (panel C). The credit card and credit score cutoffs are based on averages across the 12 pre-tornado quarters, while the age is from the quarter before the tornado. The lower and upper tercile cutoffs for each of the variables are as follows: 40 and 58 for age, $149 and $11,364 for available credit, and 618 and 759 for Equifax Risk Score. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 4: **Estimates for the Number of Business Establishments and Employees**

Model:	Triple Difference		Difference-In-Difference			
Dependent Variable:	Log (Establishments)	Log (Employment)	Log(Establishments)		Log(Employment)	
Tornado Type:			Cash	No-Cash	Cash	No-Cash
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled						
Cash Tornado x Post x Hit	0.177	0.286	-0.002	-0.163	-0.014	-0.255
	(0.103)	(0.171)	(0.014)	(0.051)	(0.026)	(0.092)
R-squared	0.560	0.555	0.559	0.516	0.549	0.510
Observations	141,977	141,977	125,016	16,961	125,016	16,961
Panel B: Continuous Damage						
Cash Tornado x Post x Hit	0.048	0.069	-0.026	-0.067	-0.048	-0.094
	(0.023)	(0.040)	(0.005)	(0.021)	(0.009)	(0.041)
R-squared	0.561	0.555	0.559	0.516	0.549	0.510
Observations	141,977	141,977	125,016	16,961	125,016	16,961
Panel C: Binned Damage Levels						
Cash Tornado x Post x Low	0.218	0.351	0.047	-0.158	0.066	-0.249
	(0.119)	(0.204)	(0.015)	(0.058)	(0.030)	(0.107)
Cash Tornado x Post x Medium	0.133	0.151	-0.060	-0.178	-0.116	-0.213
	(0.062)	(0.118)	(0.025)	(0.076)	(0.046)	(0.140)
Cash Tornado x Post x High	0.118	0.374	-0.127	-0.208	-0.209	-0.468
	(0.104)	(0.393)	(0.027)	(0.088)	(0.052)	(0.266)
R-Squared	0.561	0.555	0.559	0.516	0.549	0.510
Observations	141,977	141,977	125,016	16,961	125,016	16,961

Columns (1) and (2) show triple difference estimates of the effect of cash grants on the number of establishments and employment. Columns (3)-(6) show difference-in-differences model estimates separately for cash and no-cash tornadoes. Dependent variable means are for the last year before a tornado. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado in the triple-difference model and by census tract in the difference-in-differences model. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 5: **Heterogeneity in Business Establishment Triple Difference Estimates by Industry, Age, and Size**

	(1)	(2)
Dependent Variable:	**Log(Establishments)**	**Log(Employment)**
	Panel A: Establishment Industry	
Non-Manufacturing		
Cash Tornado x Post x Hit	0.048	0.070
	(0.023)	(0.041)
R-squared	0.560	0.553
Manufacturing		
Cash Tornado x Post x Hit	-0.002	-0.007
	(0.003)	(0.009)
R-squared	0.513	0.519
	Panel B: Establishment Age	
New (1 year or less)		
Cash Tornado x Post x Hit	-0.005	-0.009
	(0.005)	(0.010)
R-squared	0.379	0.318
Existing (4 years or more)		
Cash Tornado x Post x Hit	0.035	0.057
	(0.018)	(0.037)
R-squared	0.538	0.534
	Panel C: Establishment Size	
Small (≤ 3 Employees)		
Cash Tornado x Post x Hit	0.043	0.048
	(0.014)	(0.020)
R-squared	0.544	0.529
Large (≥ 7 Employees)		
Cash Tornado x Post x Hit	-0.005	-0.004
	(0.014)	(0.029)
R-squared	0.570	0.571

The table shows triple difference estimates using the same model as in Table 4 panel B, except that we limit the sample by establishment industry (panel A), age (panel B), and size (panel C). Each point estimate in the table is from a separate regression. We classify each establishment as manufacturing or non-manufacturing using the two digit SIC. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Infogroup Historic Business Database, National Weather Service, US Census.