Weathering an Unexpected Financial Shock: The Role of Federal Disaster Assistance on Household Finance and Business Survival

Justin Gallagher, Daniel Hartley, and Shawn Rohlin*

December 21, 2021

Abstract

First, we document the impact of being hit by a devastating tornado on household finance and business survival. The tornado paths are random and cannot be predicted using risk information. Individuals in severely Census damaged blocks have a small *reduction* in debt and no change in bill delinquency. The business establishment survival rate declines by 10%. Second, we provide insight on the role of federal disaster assistance, which includes direct cash assistance to disaster victims and grants to repair public infrastructure, in mitigating the shock. Individuals in severely damaged blocks have 30% less credit card debt post-disaster when disaster aid is available. Migration from damaged blocks increases. Credit-constrained victims have lower bill delinquency and increase consumption. Disaster assistance is a place-based policy and results in 9% more establishments and 12% more employees post-disaster in the average-damaged neighborhood. 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* A previous version of this paper was circulated under the title "Weathering an Unexpected Financial Shock: The Role of Cash Grants on Household Finance and Business Survival". 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. 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 grants are a key component of disaster assistance and have been distributed to individuals 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 focus on flooding and all conclude that the average net financial impact of a large natural disaster is modest and short-lived. Overall, the vast majority of the nascent literature that uses natural disasters as a wealth (or income) shock to study household finance and business outcomes examines the impact from hurricanes and flooding.[1] We are not aware of another study that examines the detailed household finance, migration, or business survival impact of being hit by a tornado.

There are two goals of this study. First, we document the impact of being directly hit by a devastating tornado on household finance and business survival. There are several advantages of using tornadoes as a source of exogenous damage. First, the exact tornado path is completely random. There are areas of the US where tornadoes are prevalent, but it is not possible to predict the exact path of a tornado (e.g. FEMA [2007]). The randomness in the location of a tornado is in sharp contrast to flooding, where flood maps and land characteristics, such as proximity to the ocean, are predictors of property-level flooding (e.g. First Street Foundation [2021]). Second, the tornado location randomness effectively eliminates the ability for individuals and businesses to sort locally based on disaster risk. Disaster risk sorting complicates analyses in other settings such as flooding (e.g. Bakkensen and Barrage [2021]). Third, the tornado location randomness also minimizes the scope for individuals and businesses to use disaster risk information to differentially invest in protective actions such as more durable housing designs and precautionary savings. Measuring the

[1] Hurricane and flooding household finance studies include: Billings et al. [2019]; Del Valle et al. [2019]; Deryugina et al. [2018]; Gallagher and Hartley [2017]; Groen et al. [Forthcoming]. Hurricane and flooding business survival studies include: Basker and Miranda [2017]; Collier et al. [2020].

causal impact of a natural disaster is more challenging when victims and non-victims are likely to have different levels of (difficult to observe) protective investments (e.g. Barreca et al. [2016]).

We study 34 very large US tornadoes that occurred from 2002-2013. The tornadoes cause uninsured property damage and act as unexpected, one-time shocks to wealth and capital. The tornadoes may also lead to lost income if business operations are interrupted or if individuals are displaced. We use credit bureau data to estimate the causal effect of residing in a block with a specific level of tornado damage on post-disaster financial outcomes. We examine the impact on business survival and employment using a proprietary establishment-level database.

The geographic randomness in tornado damage provides identification in our event study and difference-in-differences (DiD) econometric models. We precisely control for block-level damage intensity using detailed damage maps and estimate how outcomes differ for individuals and business in hit blocks to those located in nearby blocks just outside the tornado path.

Figure 1 shows the damage map for an Enhanced Fujita 5 (EF5) tornado that hit Joplin, Missouri on May 22, 2011. The overall tornado damage rating is determined by the greatest damage level in the tornado path. Damage ratings at different locations in a tornado path can range from EF0 to EF5. The EF ratings are calculated 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. To get a sense of the damage classifications, examples of EF1 damage include exterior doors ripped off a building and shattered windows. The destruction of entire stories of well-constructed homes constitutes EF3 damage, while the leveling of well-constructed houses to the foundation is EF5 damage (Lukasik [2020]).

We find that individuals hit by a tornado have a modest overall post-disaster *reduction* in personal debt. A resident located in a hit block that sustains the average level of damage in our sample (EF 1.8) has approximately 4% lower credit card balances and a 2% reduction in home debt. The estimated impact on the Equifax Risk Score and bill delinquency are economically small in magnitude and statistically insignificant. The block-level migration rate, defined as the proportion of existing residents who move away from the block for at least three years, increases by 140% in the first year following a tornado.

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. FEMA states that almost 40% of small businesses close after a flood-related natural disaster (FEMA [2019]). We provide, to our knowledge, the first causal evidence on establishment survival from being in the damage path of a very large tornado. Overall, there are fewer business establishments in hit blocks in the immediate years following the disaster. Establishment closings are highest in severely damaged blocks, where the business survival rate declines by 10% and employment by 17%.

The second goal of this study is to provide insight into how federal disaster assistance affects post-disaster outcomes. Our estimates using credit bureau measures for the financial impact of tornadoes imply that individual-level financial outcomes slightly *improve* when a tornado damages your residence. This is consistent with the recent research on flooding (Deryugina et al. [2018]; Gallagher and Hartley [2017]; Groen et al. [Forthcoming]), and underscores the possibility that federal disaster assistance may play an important role in mitigating the negative impact of a natural disaster.[2]

The federal Individual Assistance Program provides cash grants directly to disaster victims. Residents in disaster areas can receive cash grants up to approximately $30,000 (Federal Register [2010]). The cash grants are linked to incurred damage and expenses caused by the disaster. Cash assistance is not provided to individuals in disaster areas following all tornadoes. We divide our tornado sample into two groups. Cash grants are made available to residents following each tornado in the first group, which we refer to as "disaster aid" (or sometimes "aid") tornadoes. No cash grants are made available to residents following any of the tornadoes in the second group ("no disaster aid" or "no aid" tornadoes). This classification facilitates a comparison between individuals (and businesses) located in blocks that suffer the same level of damage, but where cash assistance is provided only to victims of some tornadoes and not to victims of other tornadoes.

Ideally, we would be able to completely isolate the role of cash grants on post-disaster household finance, migration, and business survival. This would allow for a direct test, for example, for whether cash grants substitute for personal debt and lead to a decrease in the level of debt incurred by disaster victims. The ideal approach is complicated by the existence of multiple federal disaster assistance programs, social safety net transfers, and non-profit disaster assistance (e.g. Red Cross).

The most significant of the other disaster assistance programs in our setting is Public Assistance. Public Assistance is made available to local and state governments as well as non-profit organizations. These groups can access grant money to repair infrastructure and

[2]Gregory [2017], in related work, evaluates the impact of a congressionally approved, supplemental disaster housing grant program on the rebuilding and migration decisions of New Orleans residents following Hurricane Katrina.

to aid in the reconstruction of public buildings. Individual and Public Assistance grants following a tornado are highly correlated. Public Assistance grants are distributed after nearly all of the tornadoes where direct cash grants are available, and after very few of tornadoes without cash grants.[3] Public Assistance grants could indirectly benefit disaster victims by increasing overall economic activity.

Social safety net transfers following a natural disaster can be substantial. Deryugina [2017] finds that there is a decade-long increase in (non-disaster) government transfers in disaster counties following large hurricanes. The transfers include unemployment insurance, income maintenance payments, disability insurance, and public medical payments. An important assumption in our setting is that, conditional on block-level damage, direct social safety net transfers based on injury or job loss are the same for victims of disaster aid and no-aid tornadoes. We discuss reasons why our federal disaster assistance estimates might best be interpreted as a lower (or upper) bound in Section 4 when we describe our econometric model in detail.

The disaster-level endogeneity of whether cash grants are made available provides two related identification challenges. First, cash grants are more likely to be made available when areas with more vulnerable populations are affected. We approach this identification challenge by estimating a triple difference econometric model. We examine the pre- to post-tornado difference in financial outcomes for hit and nearby populations who are affected by disaster aid and no disaster aid tornadoes, respectively. Differencing with the nearby non-hit groups leverages the randomness of the tornado path, and provides a way of controlling for both divergent pre-existing trends and differing levels in key financial variables among the two groups hit by a tornado.

The second identification challenge is the geographic correlation between Individual Assistance and Public Assistance. This challenge is difficult to definitively address given the available public assistance data. As such, the disaster aid tornado estimates in the second half of the paper should be interpreted as the combined impact of direct cash assistance to tornado victims, along with the role of federal public infrastructure grants, on household and business establishment financial outcomes.

We find that individuals in high damage blocks have \$773 (30%) less in average quarterly credit card debt following a disaster aid tornado, relative to individuals hit by a no aid tornado.[4] Access to credit markets impacts how tornado victims substitute disaster assistance

[3] We label the tornado groups as "disaster aid" and "no disaster aid", rather than "cash assistance" and "no cash assistance", as a means to emphasize that the receipt of cash assistance is highly correlated with Public Assistance. A drawback of this nomenclature is that Public Assistance does go to some neighborhoods impacted by "no aid" tornadoes.

[4] All cost figures are in 2010 \$ throughout the paper.

for credit card debt. Consistent with the life cycle/permanent income hypothesis, nearly all of the reduction in credit card debt is attributable to less credit-constrained individuals. Disaster assistance, including immediate cash grants, reduces the financial hardship for more vulnerable individuals who are less able to smooth the financial shock. Credit-constrained individuals have lower rates of bill delinquency and are more likely to increase consumption on new vehicle purchases. The increase in overall block-level migration following a tornado is attributable to tornado victims of disaster aid tornadoes.

Disaster assistance is a place-based policy. Cash assistance is provided to individuals, but the distribution of cash assistance is concentrated in the most-damaged neighborhoods by design. Public Assistance is also provided to these same neighborhoods to repair damaged infrastructure. We examine business outcomes because we hypothesize that the disaster assistance acts as a targeted stimulus to local businesses. Cash grants to individuals could lead to improved local establishment survival rates if there is an increase in spending on local goods and services. Cash grants that go to individuals who happen to be small business owners could also help keep businesses open. Likewise, Public Assistance grants could increase establishment survival by spurring local demand for goods and services.

We estimate that there are approximately 9% more establishments and 12% more employees in blocks that sustain the average level of tornado damage following disaster aid 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 disaster assistance affects the formation of new establishments. Survival rates are greater for businesses that rely on local demand.

We conclude the paper by arguing that the spatial pattern of disaster assistance, combined with our estimation results, is most consistent with cash assistance as the primary underlying mechanism leading to the observed improvements in household finance and business survival following a disaster aid tornado.

2 Theoretical Framework

We use block-level tornado damage as a proxy for the financial shock experienced by an individual (or business) at the time of a tornado. More destructive tornadoes cause greater damage to homes and property. Much of the damage is uninsured. The typical insurance policy will not cover all damage, as there is typically both a deductible and an insurance limit. Moreover, not everyone has home, property, and auto insurance policies. Destructive tornadoes are also likely to result in lost income. Disaster victims may miss work due to an injury, time spent on disaster cleanup, or temporary housing relocation. Local employers, including home-based businesses, that close following the tornado will also cause income loss for some residents.

2.1 Household Finance

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]). Key to this prediction is that individuals are not credit-constrained, and thus able to borrow.

We hypothesize that someone hit by a tornado who is not credit-constrained and who does not receive direct federal cash assistance will, on average, incur more debt. The event study and DiD models in Section 5 provide estimates for the overall impact of a devastating tornado that are inclusive of government assistance. As we describe in the next section, we are able to examine four categories of debt: credit card, home, car, and other (mainly consumer finance loans). Savings is not observable in our data. However, the typical adult has very little cash savings and is likely to rely on new debt. Only 46% of US adults could afford an unexpected $400 expense without borrowing or selling an asset (Federal Reserve Bank [2016]). Fifty-five percent of households do not have enough savings to cover a month of lost income (Pew Charitable Trusts [2015]).

In our triple difference model, we compare individuals living in blocks that incur the same level of damage, but where federal disaster assistance is provided to one group and not the other. The federal assistance includes Individual Assistance cash grants paid directly to disaster victims. As we discuss in the next section, the decision to offer Individual Assistance grants is not random. Tornadoes that are part of larger state-level storm systems, have a larger damage area, and hit lower socio-economic areas are all more likely to lead to Individual Assistance. A key assumption for our estimation approach is that Individual Assistance grants (when available) and direct social safety net transfers depend only on the amount of block-level damage. A comparison of papers that study Hurricane Katrina, the most-destructive hurricane to ever hit the US, provides support for this assumption (Deryugina et al. [2018]; Gallagher and Hartley [2017]). Gallagher and Hartley [2017] use a within New Orleans control group of residents at non-flooded blocks, whereas Deryugina et al. [2018] use a control group outside the city. Both studies find similar results.[5]

[5] Gallagher and Hartley [2017] write: "Our estimates, by construction, net out any common shock to New Orleans. We isolate differences in personal finance attributable to Hurricane Katrina that are based on the severity of flooding in each resident's block. One interpretation of the fact that both papers find modest and temporary negative effects on household finances is that the impact of Katrina on non-flooded residents is small." (p202)

We hypothesize that the cash grants will substitute for additional debt, provided disaster victims have access to credit markets. Overall, we expect that disaster victims who have access to cash grants will have less post-disaster debt, relative to disaster victims without access to cash grants. The LCPIH predicts that cash grants will have only a limited effect on measures of financial wellbeing when individuals are not credit-constrained. For example, tornado victims could still increase new borrowing to avoid bill delinquency if cash grants are unavailable.

Predictions differ for credit-constrained individuals. We would expect there to be a limited debt response following a tornado. These individuals may be forced to sharply reduce consumption. At the same time, credit-constrained individuals may suffer from greater negative financial outcomes, such as bill delinquency, as compared to less credit-constrained tornado victims.

Predictions also differ for credit-constrained individuals when cash grants are made available to tornado victims. First, less credit-constrained individuals will reduce their post-disaster debt by more than those who are credit-constrained. The reason is that credit-constrained individuals are largely shut out of credit markets, and there is little opportunity to substitute the grants for forgone borrowing. Second, the impact that cash grants have on preventing financial distress will be greater for credit-constrained victims. The grants are more likely to be pivotal in preventing financial distress for victims with a limited ability to borrow. Third, the cash grants will lead to higher (immediate) post-disaster consumption for credit-constrained individuals. Victims with limited access to credit markets will rely more on reduced spending as a means to manage the financial shock when cash grants are not available. When cash grants are available, these victims will not need to reduce consumption by as much, and may be more able to replace damaged property.

2.2 Business Survival

Federal disaster assistance can aid local businesses in several 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.[6] Second, cash assistance to individuals that happen

[6]The 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

to be small business owners may positively affect establishment outcomes. Many businesses are small. In 2016, 47 percent of establishments employed four or fewer people (US Census [2018]). The median establishment size in our sample is four. Around half of all establishments are operated out of a home (SBA [2012]).[7]

Public Assistance grants to repair infrastructure could also aid business establishments if the grants increase local spending. Expenditures could directly increase, for example, if local construction companies are contracted. Public Assistance grants could indirectly spur local growth by facilitating commerce (e.g. road reconstruction), or via local economic spillovers, for example, through the repair of a municipal office building.

3 Background and Data

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

3.1 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.[8] 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

establishments, the increased business revenue is likely to be more important for damaged establishments (i.e. higher marginal impact) to, for example, prevent closure.

[7]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 (US Census [2018]).

[8]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.

details when we discuss the econometric model in Section 4. Appendix Table 1 lists all 35 tornadoes.

3.2 Federal 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: Individual Assistance (cash grants), SBA disaster loans, and Public Assistance.

Individual Assistance provides cash grants to disaster victims. 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 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 (Federal Register [2010]).

In order to receive cash assistance, an individual may either apply to FEMA directly or be referred to FEMA after applying for a Small Business Administration (SBA) disaster loan (FEMA [2021]). SBA loan approval is largely based on credit score and debt-to-income ratio. Collier and Ellis [2021] examine all individual SBA disaster loan applications from 2005 - May 2018 and find that less than half of SBA loan applications are approved. Cash grants are provided based on disaster-related costs not covered by existing insurance.

SBA disaster loans are available in 99% of the hit blocks in our sample (regardless of PDD designation). Overall, the amount of SBA loan assistance provided to individuals located in hit blocks in our sample is very similar between disaster aid tornadoes where cash assistance is available, and tornadoes without disaster aid. The reason is that a PDD is not necessary to trigger SBA loan assistance. $1.3 million in SBA home loans, on average, are approved for a disaster aid tornado, while $1.4 million are approved for a no aid tornado. The average per-capita amount of approved loans is $297 for a aid tornado and $344 for a no aid tornado (see Appendix Table 5).

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. Tornadoes with cash assistance 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 tornadoes with and without cash assistance motivates our econometric models that account for heterogeneity in block-level damage.

One potential confounder that we are unable to examine is how private disaster relief varies between tornadoes where federal disaster assistance does and does not include cash assistance. Private disaster relief from individuals, the Red Cross, or civic and religious organizations more broadly, may be unequally distributed. We might expect private disaster relief to be greater for larger tornadoes (e.g. disaster aid tornadoes), or when there is a higher perceived need due to the absence of government assistance (e.g. no-aid tornadoes).[9]

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. 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). 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.

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 disaster aid tornadoes and 3 of 9 no-aid tornadoes in our main sample.

The total county-level amount of Public Assistance and Individual Assistance provides a crude measure for the level of disaster aid provided to the blocks impacted by a tornado. County-level disaster aid information is available via FEMA administrative documents for

[9] Deryugina and Marx [Forthcoming] find that local private charitable donations increase following a tornado, and are correlated with the number of tornado-related fatalities. It is unclear how to interpret this finding in our setting. Tornadoes with greater fatalities also tend to be larger in size. The per-victim amount of charitable donations could be larger or smaller between federal disaster aid an no-aid tornadoes.

around two-thirds of the tornadoes in our sample. On average, \$5.1 million in cash grants is distributed to individuals in a tornado-affected county, while \$17.0 million in Public Assistance is allocated to remove debris and repair public infrastructure.

3.3 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 card debt is a common type of uncollateralized, short-term debt that includes both bank and retail cards (Federal Reserve Bank [2021]). 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. In our sample, consumer finance loans make up just over one percent of total pre-tornado debt. 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 an indicator for foreclosure. 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. Overall, individuals hit by a tornado are similar to those in nearby neighborhoods outside the tornado path (columns 1 and 2). The CCP financial health measures (Equifax Risk Score, fraction with an account that is 90 days past due, and an indicator for a recent foreclosure) are nearly identical in the quarter before a tornado. However, individuals hit by tornadoes where cash assistance is available are economically worse off (lower home ownership, lower Equifax Risk Score, and higher 90 day delinquency) than individuals hit by tornadoes where cash assistance is not available (columns 4 and 7). 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]).

3.4 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.[10] 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 aid and no-aid tornadoes (5% and 4%, respectively).

4 Empirical Specifications

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. No portion of any control block is hit by a tornado. The average hit block sustains EF 1.8 of damage. 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, 1.0-2.0, and 0.5-2.0 buffer areas, or if we use a propensity score model to select non-hit individuals from control blocks anywhere within the same county as the tornado.

[10]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.

Our baseline empirical specification, Equation 1, is a heterogeneous damage event study model that uses block-level tornado damage as a proxy for the amount of damage experienced by individuals and businesses (e.g. Gallagher and Hartley [2017]). The exact path and timing of a tornado is not predictable and this randomness provides a source of identification. We first describe the specification that we use to examine individual financial outcomes.

$$Y_{it} = \sum_{h=-a}^{b} \tau_h * Hit_i * 1[K_{it} = h] + \alpha_i + \gamma_t + \varepsilon_{it} \quad (1)$$

Y_{it} is a credit outcome for individual i in quarter t.[11] Hit_i is a continuous damage variable measuring the average EF damage in the block. τ_h are the event time coefficients of interest, where $K_{it} = t - E_i$ is event time, E_i is the date of the tornado, a and b are the number of leads and lags included, and $1[\,]$ is an indicator function. The panel is balanced in event time and each τ_h is estimated using information from all tornadoes. The year before the tornado serves as the reference time period for the post-tornado event time coefficients. Period a is the reference time period for the pre-tornado event time coefficients (see below). α_i is an individual fixed effect, γ_t is a quarter-by-year fixed effect, and ϵ_{it} is an error term.

The recent methodological literature on event studies has shown several potential limitations with the event study model (e.g. Borusyak et al. [2021]; de Chaisemartin and D'Haultfoeuille [2020]; Callaway et al. [2021a]; Callaway et al. [2021b]; Sun and Abraham [2021]). These limitations include a strong assumption regarding the homogeneity of the event time treatment effects across calendar time and individuals (conditional on the fixed effects), and that OLS estimation may not lead to sensibly weighted treatment parameters (τ_h). Borusyak et al. [2021] propose an imputation-based estimation approach that allows for unrestricted treatment effect heterogeneity, while avoiding the OLS parameter weighting problem. We follow Borusyak et al. [2021] in estimating Equation 1, while noting that our estimation results–including the pre-tornado event study coefficients–are similar if we estimate Equation 1 using OLS. We cluster the standard errors by tornado.[12]

The imputation approach can be summarized as follows. First, estimate a version of Equation 1 that includes only the unit and calendar time fixed effects (omitting the event study coefficients), and using only those observations that have not yet been hit by a tornado

[11] We prefer the yearly event study model to the quarterly model. We include the quarter of the tornado, but the remaining quarters are pooled by event year. 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.

[12] We estimate the model using the Stata software packages provided by the authors. Finkelstein et al. [2021] use a similar imputation approach to estimate an event study model for prescription opioid abuse.

during our panel. Second, estimate a counterfactual outcome, $\hat{Y}_{it}(0)$, for each post-tornado, hit observation using the estimated unit and calendar time fixed effects from the first step. Third, calculate the effect of being hit by a tornado for each hit observation as: $\hat{\tau_{it}} = Y_{it} - \hat{Y}_{it}(0)$. The estimated event study coefficients, $\hat{\tau_h}$ (for $h \geq 0$), are calculated as a weighted sum of individual treatment effects for period h, with weights equal to the number of individual treatment effects. Our panel is balanced in event time, so each $\hat{\tau_{it}}$ is weighted equally when calculating $\hat{\tau_h}$.

The pre-tornado event study coefficients in Equation 1 are estimated using OLS. We estimate a version of Equation 1 that includes only the pre-tornado event study coefficients, where the earliest event time indicator is omitted as a reference period (Borusyak et al. [2021]). The model is run only for observations that have not yet been hit by a tornado (including control observations from the buffer areas). An important advantage of estimating the pre-tornado coefficients using non-hit observations only, is that this avoids estimation bias that could occur if we were to use a sample that includes hit observations when there is treatment effect heterogeneity (e.g. Sun and Abraham [2021]).

Pre-treatment event study coefficients are often used as a way to evaluate the standard difference-in-differences (DiD) parallel trends identification assumption, which assumes that control observations represent a valid counterfactual time trend for what would have occurred in the absence of treatment. Callaway et al. [2021b] show that the standard parallel trends assumption is typically not sufficient for continuous treatment event study (and DiD) models. In our setting, we are interested in the causal parameter $ATT(d|d)$–the average treatment effect of tornado damage level d for individuals living in blocks that sustain damage level d. This parameter is analogous to the average treatment on the treated when the treatment is binary. $ATT(d|d)$ is completely identified off of potential outcomes for those individuals not hit by a tornado. As such, the standard parallel trends assumption is sufficient to identify *each* of the many $ATT(d|d)$. The challenge comes when we want to compare the magnitudes of the $ATT(d|d)$ across different levels of damage (e.g. EF3 versus EF4). To do so, we need a stronger parallel trends assumption that restricts the potential outcomes of individuals in damaged blocks. Specifically, we must assume that the average potential outcomes for individuals within the tornado path are the same at each level of damage, or that (on average) there is no selection into a particular level of damage.

The stronger parallel trends assumption is likely to be satisfied in our setting. First, the pre-tornado event study coefficients can still be used to test the standard (weaker) assumption. We show that there is support for the weaker parallel trends assumption for our financial, migration, and business outcomes. Second, the random timing and location of a tornado implies that no one could pro-actively select the level of damage. For example, we may be more concerned about the stronger parallel trends assumption in a medical setting where patients have influence over the level of (or adherence to) a particular dosage of a medicine, and can therefore select-on-gains. Third, Appendix Table 4 shows that observable characteristics are very similar between individuals (and businesses) located in low, medium, and high damage blocks. The *realized* levels of damage for individuals in the tornado paths appears random. Fourth, individuals (and businesses) are unable to use tornado risk to sort locally before a tornado into blocks that are likely to sustain less tornado damage. Thus, individuals that are affected by differing levels of damage are unlikely to have different levels of important, unobservable variables (such as savings) that could lead potential outcomes to differ based on the observed level of damage.

$$Y_{it} = \delta(Hit_i * Post_{it}) + \beta_1(Post_{it}) + \alpha_i + \gamma_t + \varepsilon_{it} \tag{2}$$

Equation 2 is a difference-in-differences model that also includes individual and calendar time fixed effects. $Post_{it}$ is a binary variable equal to one if the observation is the hit quarter, or any quarter following the tornado. Note that the non-interacted Hit_i variable is subsumed by the individual fixed effects. δ is a convenient way to summarize the average effect of the post-tornado event study coefficients in Equation 1, and represents the effect on credit and migration outcomes for individuals living in hit blocks relative to those individuals who just missed being hit by the tornado. We use the wild cluster bootstrap when calculating standard errors (Cameron and Miller [2015]). In some specifications of the DiD model we only include a subset of the 34 tornadoes. The wild cluster bootstrap is robust to estimating the model with a smaller number of clusters, whereas clustered standard errors may overstate the model's precision due to asymptotic reliance on the number of tornadoes.[13]

We also estimate a binned damage specification for both Equation 1 and Equation 2. In the binned damage specifications we replace 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. EF1 damage includes loss of exterior doors and roofs that are "severely stripped" of shingles (Lukasik [2020]). EF3 implies that the roofs

[13] It turns out that the standard errors are very similar in all DiD and triple difference models regardless of whether we cluster the standard errors by tornado, or use the wild cluster bootstrap. We conduct bootstap inference using Stata's *boottest* package (Roodman et al. [2019]). We opt for (non-bootstrapped) clustered standard errors in the event study models for two reasons. First, we run these models using our complete sample (so smaller clusters are less of a concern). Second, (non-bootstrapped) clustered standard errors are more compatible with the software package provided by Borusyak et al. [2021].

for most types of buildings will be severely damaged and the outer walls of the building may have collapsed. A damage level of 3 on the original Fujita scale corresponds to "severe damage" (National Weather Service [2014]).

$$y_{it} = \rho(Aid_i * Hit_i * Post_{it}) + \beta_1(Aid_i * Post_{it}) + \beta_2(Hit_i * Post_{it}) + \beta_3(Post_{it}) + \alpha_i + \gamma_t + \epsilon_{it} \quad (3)$$

We use a triple difference model, Equation 3, to examine the role of federal disaster assistance on post-disaster outcomes. The triple difference model augments Equation 2 by including Aid_i, a binary variable indicating whether individual i lived in an area either hit by or near a tornado that received cash assistance under the Individual Assistance Program.[14] Recall that federal assistance to repair public infrastructure is also available for the majority of these same tornadoes. ρ is our coefficient of interest and represents the effect on credit and migration outcomes for individuals living in hit blocks where cash assistance and public grants are 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 access to federal cash assistance. When we estimate the triple-difference event study model, we replace $Post_{it}$ with the binary event time variables K_{it}, and again use an imputation-based approach that follows Borusyak et al. [2021].

The triple difference estimate can be thought of as taking the difference between two DiD estimates, where we separately estimate the effect of being hit by a tornado that does and does not result in federal post-disaster cash assistance. The within tornado difference between the hit and nearby populations helps to control for selection differences between victims of tornadoes where disaster aid is and is not available. Our strategy is similar to Deschenes et al. [2017] who use a triple difference model to control for state-level selection into a voluntary air pollution program.

Figure 2 provides an illustrative example for how the triple difference model makes identification of disaster assistance more robust. The figure plots the mean credit card debt levels separately for the hit and nearby individuals for aid and no-aid 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 disaster assistance. Doing so would lead to a biased causal estimate due to the downward trend in credit card balances in hit areas that receive assistance. Differencing with the nearby groups provides a way of controlling for divergent pre-existing trends among the two groups hit by a tornado.

[14] The variables Aid_i, Hit_i, and the interaction $Aid_i * Hit_i$ are excluded from the model since they are subsumed by the individual fixed effects.

The triple difference model estimates are likely an underestimate of the causal effect of federal disaster assistance. The reason is that Individual Assistance is more likely to be available following tornadoes that hit lower socio-economic populations.[15] Differencing with the nearby residents reduces, but does not eliminate this concern. On the other hand, it is possible that our estimates are an overestimate if, for example, direct social safety net transfers are larger (conditional on block-level damage) when there is greater total damage. This could occur if the duration of unemployment is longer, and leads to an increase in unemployment insurance or income maintenance payments.

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 disaster aid and hit no-aid groups. 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-aid tornadoes in our main sample. Second, the balanced sample includes observations from nine of the 25 disaster aid 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 aid and no-aid tornado groups.[16] Overall, the main and balanced sample estimates are similar. This suggests limited heterogeneity in the treatment effects, and that the no-aid hit group is an appropriate control group in our main sample.

Finally, 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 the 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 3.1 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 outcomes. Not surprisingly, there are some differences in the business results. We highlight these differences in our discussion of the results.

[15] The counterfactual of being hit by a tornado and not having access to cash grants is likely to slightly overstate the financial resiliency for how hit victims with access to grants would have done. Any improvement in financial outcomes due to cash assistance is measured relative to this counterfactual, which may slightly attenuate the measured effect.

[16] Specifically, we minimize the sum of the absolute deviations in z-scores for eight debt and financial health variables (credit card, auto, home, other, and total debt, and Equifax risk score, 90 day delinquency, and foreclosure). There are 2,042,975 possible subsamples.

5 Overall Tornado Results

In this section, we present evidence on the impact of being hit by a tornado relative to being located in a nearby undamaged block.

5.1 Household Finance and Migration

Figure 3 plots difference-in-differences event study estimates for our main household finance and migration outcomes using the continuous damage model. The dashed vertical line indicates the *quarter* of the tornado. The estimate plotted at zero is for the quarter of the tornado only. The circles to the right of the dashed line are the estimated post-tornado yearly event time coefficients. Recall that the post-tornado coefficients are estimated using an imputation-based method, and are relative to the year before a tornado. The pre-tornado yearly event time coefficients (squares) are estimated using OLS with three years before the tornado as the reference period. The shaded regions represent the 95% confidence intervals.

The four debt categories (credit card, auto, home, and other) are shown in Panels A-D.[17] There is a modest, statistically significant decline in credit card debt during the first two post-tornado years. The reduction is equal to approximately 8% of credit card balances for an individual located in a hit block that sustains the average level of damage (EF 1.8).[18] Home debt also decreases in the year following a tornado by approximately 3% for an individual located in the average-damaged block. Auto debt increases in the first post-tornado year. The increase in auto debt is attributable to an increase in new auto purchases for credit-constrained individuals who have access to federal disaster assistance. We discuss new auto purchases in detail in Section 6. There is no change in other debt.

Table 1 shows the DiD model (Equation 2) estimates for the same debt outcomes as in Figure 3. The model summarizes the average medium-run tornado impact for the quarter of the tornado and the three immediate post-tornado years. Table 1 panel A shows DiD coefficient estimates from our continuous damage model. Individuals located in a hit block that sustains the average level of damage have approximately 4% lower ($104) in average quarterly credit card balances (p-value 0.089) for the three years following the disaster. Total home debt decreases by $1,213 (2%, p-value 0.022) for a homeowner in a block with average

[17] Student debt is the only major CCP debt category we don't evaluate. This is due to a change in how these data are recorded during our study period (Brown et al. [2014]). 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.

[18] Throughout the text, we calculate the estimated effect from the continuous damage model for an individual (or business establishment) in the average hit block as the product of the point estimate and 1.8. We divide this product by the pre-tornado mean of the dependent variable for the hit group when calculating the percent change (or in the case of the triple-difference model the mean of the hit-aid group).

damage who has a home loan continuously during the 12 quarters before the tornado. Auto debt increases by $122 (4%, p-value 0.030). The binned damage model estimates in panel B are less precise, but show the same pattern of results.

There is no evidence that being hit by a tornado affects overall financial health. Figure 3 estimates the change in the Equifax Risk Score (panel E), and the change in the number of individuals with a 90 day delinquent account (panel F). The post-tornado coefficients are economically small and statistically insignificant. For example, we can rule out an effect of more than 4 points (0.6%) in the Equifax Risk Score for an individual in an average-damaged block. The DiD estimates in Table 1 (columns 5 and 6) are all close to zero and statistically insignificant.[19]

The final two panels in Figure 3 show estimates for whether an individual moves out of their Census block for at least one quarter (panel G), or for at least three years (panel H). We refer to these as our migration estimates. 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. As such, for each quarter we estimate the fraction of individuals who no longer live in the same block in the following quarter (and in Panel H 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. Overall, the average block out-migration rate in the quarter before a tornado is 6.0% for the more temporary migration and 1.6% for the more permanent migration (Table 1 panel A columns 7 and 8).

Migration increases from hit blocks in the first year following a tornado. We estimate that short-term migration from an average-damaged block increases by approximately 25% (p-value 0.056). The three year migration rate increases by approximately 10% (p-value 0.010). The migration rates for a hit block return to the pre-tornado rates beginning in the second post-tornado year. The binned damage migration estimates in Table 1, while imprecise, show a pattern whereby average post-tornado migration is considerably higher in severely damaged blocks.

There is no evidence of different event study pre-trends for any of our outcomes. Individuals residing in nearby blocks at the time of a tornado appear to offer a valid counterfactual for what would have occurred if individuals were not hit by the tornado. The evidence for pre-trends is also similar using a conventional event study approach that estimates Equation 1 for the entire panel using OLS (not shown).

There are three important caveats worth highlighting. First, the credit bureau data show

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

a comprehensive picture of debt, but do not include direct measures of savings. A reduction in debt could be more than offset by a drawdown in savings, or the destruction of physical property. Still, one advantage of our setting is that individuals are unable to use disaster risk information to differentially invest in protective actions. Thus, there is no reason to expect that individuals inside the tornado path would have preemptively invested in more durable housing or precautionary savings. Second, the credit bureau data do not cover the entire population. Lower income individuals who are more vulnerable to the disaster shock are less likely to have a credit history (e.g. Jacob and Schneider [2006]). Our analysis in Section 6 partially addresses this shortcoming by separately examining the disaster impact on individuals who have lower and higher Equifax Credit Scores at the time of a tornado.

Finally, it is important to note that the recent methodological literature shows that the standard DiD model is susceptible to the same type of shortcomings as the event study model, when both models are estimated using OLS (e.g. Callaway et al. [2021a]; Goodman-Bacon [2021]). We present the DiD results since the model remains a popular way to summarize treatment effects across multiple periods. The overall similarity between the DiD and (imputation-based) event study results give us confidence that the DiD model is an appropriate summary in our setting. We also note that in our sample we use a balanced event time panel and a large never treated control group. The balanced event time sample helps to ensure that any temporary tornado impact is not masked because we observe a long post-tornado period for early calendar time tornadoes. The large never-treated group minimizes the role that problematic counterfactual comparisons (i.e. using the already treated units as controls) have in identifying the DiD estimates (Goodman-Bacon [2021]).

5.2 Local Businesses

Figure 4 plots event study estimates for the log number of establishments (panel A), log employment (panel B), and log sales (panel C). The top, middle, and bottom rows of the figure show the plots for the low, medium, and high damage groups, respectively. We emphasize the establishment survival results as these data do not rely on a survey response. The employment and sales data are self-reported and (may be) more likely to involve measurement error. Nevertheless, the three sets of results tell a similar story.[20]

There is no evidence of any pre-tornado trends. The 95% confidence interval for the pre-tornado coefficients always contains zero. The post-disaster coefficients show that the

[20] We have no reason to expect that there is mis-measurement in the business employment and sales information, apart from the fact that the information is survey-based. To the contrary, as we write in the data appendix, an academic-led study on the reliability of the Infogroup business database concludes that the data are either similar to, or of higher quality, than other private establishment-level datasets such as the National Establishment Time-Series dataset (University of Nebraska [2017]).

decrease in establishment survival and employment begin in the first year following a tornado. Some panels show an immediate decrease in the year of the tornado. However, it is difficult to interpret the effect in the tornado year, as we do not always know whether the establishment data were collected before or after the tornado. The coefficients are negative, statistically significant, and fairly stable in second and third years following a tornado.

Table 2 shows the DiD model estimates. Overall, there are fewer business establishments in hit blocks in the four years following the disaster. The continuous damage model implies that the number of establishments decreases by around 3% (p-value <0.001) in blocks that sustain the average (EF 1.8) amount of damage. Establishment closings are highest in severely damaged blocks, where the business survival rate declines by 10% (p-value <0.001). To our surprise, the business survival rate is 4% higher (p-value 0.004) in low damaged blocks. One potential explanation is that businesses located in blocks that sustain relatively minor damage receive more post-tornado business, since nearby establishments in more severely damaged blocks are less likely to be open. The triple difference disaster assistance tornado results, discussed in the next section, show that only establishments located in low damage blocks where federal disaster assistance is allocated have a positive survival rate.

The employment and sales findings mirror the survival results. Our continuous model estimates imply that employment and sales decrease by 6% (p-value <0.001) and 19% (p-value <0.001), respectively. The binned model estimates are positive in the low damage blocks, negative in the medium damage blocks, and most negative in the high damage blocks. All coefficients are statistically significant at conventional significance levels.

6 Disaster Assistance Results

In this section, we separately estimate the effect of being hit by aid and no-aid tornadoes (Equation 3) on household finance, migration, and business survival. Recall that we divide the tornadoes in our sample into two groups based on whether cash assistance was made available following the tornado. However, it is important to remember that this classification also reflects an uneven distribution of federal aid to repair public infrastructure. The disaster aid treatment coefficients should be interpreted as the combined effect of cash assistance together with the unequal distribution of grants to repair local public infrastructure.

6.1 Household Finance

Figure 5 plots yearly triple difference event study estimates for the four debt outcomes in Panels A-D using the continuous model. There is no evidence of a pre-tornado trend for any of the debt outcomes. There is a small decrease in credit card debt that is statistically significant in the second post-tornado year. Home debt decreases by about \$3,000 during the first post-tornado year for residents of blocks with average tornado damage who have a home loan continuously in the 12 quarters before the tornado.

Table 3 shows the triple difference model estimates. 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 ($773) and statistically significant reduction in credit card debt (p-value 0.017). The reduction in credit card debt for residents in high damage blocks is consistent with evidence on the persistence of revolving credit card debt (Telyukova [2013]). Total home debt decreases by $3,922 (6%) in the continuous damage model (p-value 0.001) for a resident in an average-damaged block who has a home loan continuously in the 12 quarters before the tornado. The reduction is four to five times larger for individuals in severely damaged blocks.

Difference-in-differences (DiD) results for the debt outcomes are presented separately for disaster aid and no-aid tornadoes in Appendix Table 6. There is a reduction in home debt for homeowners hit by tornadoes with disaster aid in the high damage blocks (-$2,697, p-value 0.021). However, an increase in home debt for homeowners hit by tornadoes without aid ($19,308, p-value <0.001) is driving the triple difference result. The DiD model estimates help to highlight that the impact of the disaster assistance estimated in our triple difference model is coming from *a drop in* debt for those disaster victims with access to aid, and *an increase in* debt for those disaster victims in areas without disaster aid.

We also estimate two additional descriptive DiD models so as to better understand what explains the reduction in mortgage debt for hit residents with access to disaster aid (Appendix Table 8). First, we estimate the same DiD 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 aid who move rather than stay. The reduction in home debt is similar for victims of tornadoes when aid is available regardless of whether they move. Second, we estimate a model that separately considers mortgage and home equity debt. The increase in home debt for victims of tornadoes without aid is due to an increase in first mortgage debt. These results suggest that when residents hit by no-aid tornadoes move, they purchase new homes and dramatically increase their mortgage debt (relative to victims of tornadoes with disaster aid). However, we are cautious in our interpretation as we do not have an economic model that links migration and home debt.

There is suggestive evidence for an increase in auto debt in the year following the tornado (Figure 5 panel C, p-value 0.057). In the next section, we show that the increase in auto debt is completely driven by credit-constrained individuals, while the decrease in credit card debt is primarily from victims of cash tornadoes who are *not* credit-constrained. Taken together, we interpret this as evidence consistent with intertemporal models of consumption. Credit-constrained victims have a difficult time borrowing. Disaster assistance does not offset borrowing for credit-constrained tornado victims because, absent the government assistance, these individuals were (mostly) unable to increase borrowing. Instead, (some) credit-constrained individuals are forced to reduce consumption, for example, by not replacing damaged vehicles. When government assistance is made available, it is less necessary for credit-constrained individuals to reduce consumption.

The availability of disaster assistance has no overall impact on Equifax Risk Score or 90 Day Delinquency (Figure 5 panels E and F). The estimated coefficients are close to zero. Using the 95% confidence intervals we can rule out an effect of more than 3.5 points in the Equifax Risk Score and a two percentage point change in the number of individuals with a 90 day delinquent account. Disaster assistance does appear to prevent increased bill delinquency for tornado victims who are credit-constrained. We present these results in the next section.

The triple difference migration event studies again show an increase in block migration during the first post-tornado year. The similarity to the results in the last section implies that the increase in migration immediately following the tornado is driven by individuals living in blocks hit by a disaster aid tornado. There is some evidence that the temporary increase in migration for the complete sample masks a decrease in migration from the most-damaged blocks hit by a disaster aid tornado (Table 3 column 8).

6.1.1 Heterogeneity by Access to Credit Markets

Table 4 explores how cash grants impact debt and financial health based on the likelihood a victim is credit constrained. 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 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]).

Disaster assistance leads 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 <0.001 probability level.

Credit constrained individuals in blocks hit by a disaster aid tornado are less likely to forgo paying a bill. A hit resident with a low credit score is 14% less likely (p-value <0.001) to have a 90 day delinquency. This segment of the population is not likely to be eligible for a SBA disaster loan based on their low credit score. For this reason, Individual Assistance cash grants could be particularly helpful in preventing adverse financial outcomes. 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.[21]

6.1.2 Robustness

Appendix Tables 14 and 15 show robustness results using the continuous damage specification for our overall DiD tornado (Table 1, panel A) and disaster assistance triple difference models (Table 3, panel A), respectively. We estimate four robustness models. First, we consider a model that uses non-hit control individuals from blocks between 1-2 miles away from the tornado (rather than 0.5-1.5 miles). Second, we estimate fully interacted ("stacked") DiD and triple difference models (e.g. Cengiz et al. [2019]). Third, we provide estimates from our balanced sample that more closely matches the pre-tornado levels of key financial variables. Fourth, we estimate a model 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 triple difference balanced tornado findings.

The mean levels of the CCP and Census variables for the hit groups in the balanced tornado sample are much closer, as compared to the main sample (see Appendix Table 13). Individuals hit by aid and no-aid 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 disaster assistance. The most striking differences in the balanced sample are that the estimates for the reduction in home debt and the increase in the propensity to migrate are both larger, while the reduction in credit card debt is smaller. The larger reduction in home debt in the balanced sample is partially due to larger reductions for low credit score individuals (not shown). Still, all of the point estimates from our main sample are within the balanced sample confidence intervals.

[21] 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. Tran and Sheldon [2017] examine credit outcomes for individuals in declared disaster counties and find that those individuals residing in counties where Individual Assistance (cash grants) was available show few negative impacts.

6.2 Local Businesses

6.2.1 Business Growth and Employment

We explore the effect of cash assistance on the number of establishments and the level of employment. Figure 6 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 6 plots the trends separately for disaster aid tornadoes (circles) and no-aid tornadoes (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 no-aid tornado (dashed lines). The same is true for hit and nearby establishments of aid tornadoes (solid lines). Second, the trends in establishment outcomes are increasing slightly in areas that are later hit by a disaster aid tornado. The trends are flatter for establishments which are later affected by a no-aid 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 disaster aid. 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 disaster aid. The reduction is greatest in neighborhoods hit by no-aid tornadoes.

The right side of Figure 6 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 disaster aid. The triple difference model assumes that in the absence of disaster aid 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 5 columns (1) and (2) show triple difference estimates of the effect of disaster assistance on the number of establishments and employment. Establishments in damaged blocks where residents have access to disaster assistance benefit economically. We estimate that there are 9% more establishments (p-value 0.047) and 12% more employees (p-value 0.095) in blocks with average tornado damage in our sample when disaster assistance is available. The business survival estimate for medium damage blocks in our binned damage model is 13% (p-value 0.039). This suggests the possibility of a nonlinear response in how federal disaster aid impacts business survival based on the underlying block damage. However, the three estimates are too imprecise to reject equality in the binned model.

Table 5 columns (3)-(6) show DiD model estimates separately for aid and no-aid tornadoes. There are three key patterns in the binned damage level DiD 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%, p-value 0.002) in the number of establishments located in low damage blocks where there is disaster 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 (p-value 0.015) in the number of establishments. The decrease is largest in the most-damaged blocks (-12.7%, p-value <0.001). 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-aid tornadoes, as compared to establishments and employment in blocks hit by a disaster aid tornado. Third, the estimated difference in the business survival rate between aid and no-aid tornadoes is greatest in low damage blocks and smallest in high damage blocks (panel B, columns 3 and 4). This pattern is consistent with cash assistance to individuals providing the greatest demand stimulus in locations where establishments are damaged, but still likely to be open.

6.2.2 Heterogeneity by Industry, Age, and Size

Table 6 presents estimation results from our triple difference continuous damage model that examine how the treatment effects vary by establishment industry, age, and size. Panel A of Table 6 estimates the model separately for manufacturing and non-manufacturing establishments. We classify each establishment as manufacturing or non-manufacturing using its two digit SIC code. 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 disaster assistance has 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 the full sample estimates in Table 5. 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 6 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 (p-value 0.059).

Panel C of Table 6 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% (p-value 0.005) and employees of 8.6% (p-value 0.019) at very small establishments for the average-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 federal disaster assistance, including the provision of cash grants, is distributed locally. This finding is consistent with other recent research on the vulnerability of small businesses (e.g. Cole and Sokolyk [2016]; Greenstone et al. [2015]).[22]

6.2.3 New Purchases and Sales

Improvements in establishment survival rates following disaster aid tornadoes 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. Doing so would provide direct evidence for how cash grants improve establishment survival. We are 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 disaster aid 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]).

[22] 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]).

New quarterly auto purchases and balances both increase by more than 50% (p-values <0.001) 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 disaster aid, and follow the same pattern as the establishment survival estimates in Table 5.[23] Low available credit and low credit score individuals increase their purchases when they have access to disaster assistance (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 disaster assistance.

6.2.4 Robustness

Appendix Table 13 shows business survival robustness results using the continuous damage specification for our overall DiD tornado (Table 2, panel A, column 1) and disaster assistance triple difference models (Table 5, panel A, column 1), respectively. We estimate the same four robustness models as referenced in Section 6.1.2. The DiD model results are all similar to our main model. The triple difference results are similar to our main model with one exception. The business survival coefficient estimate for the 35 tornado sample that includes the Wayne, NE tornado is approximately one estimated standard deviation smaller in magnitude. In our view, the differing business pre-tornado trends for the Wayne, NE tornado can explain the attenuated point estimate (Appendix Figures 3 and 4).

7 Discussion

In our view, the geography of Individual Assistance cash grants, together with the pattern of our results, support our interpretation that cash grants are the primary mechanism driving the disaster aid findings in the previous section.

The spatial scale of the tornadoes in our sample is important in interpreting our results. First, even the most destructive tornadoes tend to only directly hit a small fraction of a community. As such, the tornado is likely to have only a limited financial impact on the overall region. 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). Second, tornadoes cause severe damage to structures within the tornado path, and the allocation of cash grants matches the concentration of tornado damage. More than $12 million in cash grants are distributed to the primary ZIP Code hit by the Joplin, MO tornado. The cash assistance to

[23]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.

the primary Zip Code is an order of magnitude larger than the total amount dispersed to an adjacent ZIP Code that was also hit, and several orders of magnitude more than all the other surrounding ZIP Codes.[24]

The post-tornado household finance results are consistent with cash assistance as the underlying mechanism (see Table 4 and Appendix Table 12). As we outline in Section 2, economic theories of consumption predict that the availability of cash grants will lead to less post-tornado debt, provided individuals have access to additional credit. However, we would not expect credit-constrained individuals to reduce debt. Instead, access to cash grants may lead credit-constrained tornado victims to increase consumption, or incur fewer negative financial outcomes.

The post-tornado business establishment survival results are also consistent with cash assistance as the underlying mechanism (see Table 6). Cash assistance to local residents could benefit businesses by increasing demand. Business establishments that rely on local demand would benefit most. Cash assistance could also improve establishment survival when the recipient is a business owner. Smaller (e.g. home-based) establishments would likely benefit most.

Public Assistance grants are a key component of federal disaster assistance. Still, we do not view Public Assistance grants as the primary mechanism that explains our results. First, Public Assistance targets the repair of transportation infrastructure. Public Assistance could contribute to greater economic activity in a tornado-damaged neighborhood, for example, if the grants facilitate the opening of damaged roadways. However, given the limited geographic size of the tornado damage, Public Assistance grants are less critical (e.g. area roads following the Joplin tornado were immediately serviceable). 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 (e.g. 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. When we limit the data to only public sector

[24] 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.

employees, we estimate a very small, statistically imprecise change in employment.

Third, it is not clear how Public Assistance grants would disproportionately aid credit-constrained individuals. By contrast, economic theories of consumption provide clear predictions that match our household finance results.

8 Conclusion

We compile a new database that combines individual-level credit bureau data, establishment-level business information, ZIP Code-level disaster assistance, and block-level tornado damage. The 34 tornadoes in our sample cause substantial property damage and act as shocks to wealth and capital. There are several advantages of using tornadoes as a source of exogenous damage. The damage location is completely random within a community and effectively eliminates the ability for individuals and businesses to sort locally, or to differentially invest in protective actions based on disaster risk. This is in sharp contrast to flooding where local geography can help predict property-level flood risk.

We are among the first to document the impact of being directly hit by a devastating tornado on household finance and business survival. Nearly all of the nascent literature on the household and business financial impacts of natural disasters examine flooding. We find that tornadoes lead to a small reduction in personal debt. The estimated impact on the Equifax Risk Score and bill delinquency, two key indicators of financial distress, are economically small in magnitude and statistically insignificant. Overall, we estimate that there are fewer business establishments in hit blocks in the four years following the disaster. Establishment closings are highest in severely damaged blocks, where the business survival rate declines by 10% and employment by 17%.

The second half of the paper examines how post-tornado financial outcomes differ based on whether individuals and businesses are located in neighborhoods where federal disaster assistance is made available following the tornado. The disaster assistance includes grants to repair public infrastructure and cash grants directly to residents. Business establishments could benefit if there is increased spending on local goods and services. There are four main household finance conclusions. First, we find that disaster-affected individuals in high damage blocks with access to federal disaster assistance have less credit card debt following a disaster, relative to disaster victims without access to disaster assistance. Second, there is a dramatic reduction in home debt for residents in high damage blocks where disaster assistance is available. Third, consistent with the life cycle/permanent income hypothesis, credit-constrained individuals who have access to cash grants have lower rates of bill delinquency and increase their spending. Fourth, migration away from the damaged blocks increases in the year following a tornado when there is access to federal disaster assistance.

Disaster assistance to impacted neighborhoods increases the survival rate of business establishments in these neighborhoods. The establishments most reliant on local demand benefit the most. Business survival and employment are consistently lower in low, medium, and high damage blocks when local residents do not have access to cash grants. In our view, the spatial pattern of disaster assistance, combined with our estimation results, is most consistent with cash assistance as the main underlying mechanism.

9 References

Tornado risks and hazards in the midwest united states. Technical Report DR-1699-RA1, FEMA, August 2007.

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.

The 2020 survey of consumer payment choice: Summary results. Technical report, Federal Reserve Bank of Atlanta, 2021. URL https://fred.stlouisfed.org/series/UEMPMEAN.

Setting the record straight on sba loans and fema assistance. Technical report, FEMA, September 2021. URL https://www.fema.gov/fact-sheet/true-or-false-setting-record-straight-sba-loans-and-fema-assistance.

The cost of climate. Technical report, First Street Foundation, February 2021.

Laura Bakkensen and Lint Barrage. Flood risk belief heterogeneity and coastal home price dynamics: Going under water? *NBER Working Paper*, (23854), February 2021.

Alan Barreca, Karen Clay, Olivier Deschenes, Michael Greenstone, and Joseph S. Shapiro. Adapting to climate change: The remarkable decline in the us temperature-mortality relationship over the twentieth century. *Journal of Political Economy*, 124(1), 2016.

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

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.

Board of Governors of the Federal Reserve System. Report on the economic well-being of u.s. households in 2015. Technical report, May 2016. URL https://www.federalreserve.gov/2015-report-economic-well-being-us-households-201605.pdf.

Kirill Borusyak, Xavier Jaravel, and Jann Spiess. Revisiting event study designs: Robust and efficient estimation. *Working Paper*, 2021.

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.

Brantly Callaway, , and Pedro H.C. Sant'Anna. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 2021a.

Brantly Callaway, Andrew Goodman-Bacon, and Pedro H.C. Sant'Anna. Difference-in-differences with a continuous treatment. *Working Paper*, 2021b.

A. Colin Cameron and Douglas L. Miller. A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, 50(2), 2015.

Doruk Cengiz, Arindrajit Dube, Attila Lindner, and Ben Zippere. The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, 134(3), 2019.

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.

Omaha College of Information Science & Technology at the University of Nebraska. Business database competitive audit report. 2017.

Benjamin L. Collier and Cameron Ellis. Credit demand in a crisis. *Working Paper*, May 2021.

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.

Clement de Chaisemartin and Xavier D'Haultfoeuille. Two-way fixed effects estimators with heterogenous treatment effects. *American Economic Review*, 110, 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 and Benjamin M. Marx. Is the supply of charitable donations fixed? evidence from deadly toarndoes. *American Economic Review: Insights*, Forthcoming.

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.

Olivier Deschenes, Michael Greenstone, and Joseph S. Shapiro. Defensive investments and the demand for air quality: Evidence from the nox budget program. *American Economic Review*, 107(10), October 2017.

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.

Amy Finkelstein, Matthew Gentzkow, Dean Li, and Heidi Williams. What drives prescription opiod abuse? evidence from migration. *NBER Working Paper*, June 2021.

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.

Andrew Goodman-Bacon. Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, March 2021.

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.

Jesse M. Gregory. The impact of post-katrina rebuilding grants on the resettlement choices of new orleans homeowners. *Working Paper*, 2017.

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.

Katy Jacob and Rachel Schneider. Market interest in alternative data sources and credit scoring. *The Center for Financial Services Innovation*, December 2006.

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

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

Tara Lukasik. How damage determines a tornado's rating: From fujita to enhanced fujita. *Building Safety Journal*, March 2020.

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.

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.

The Pew Charitable Trusts. The precarious state of family balance sheets. Technical report, January 2015. URL https://www.pewtrusts.org/~/media/Assets/2015/01/FSM_Balance_Sheet_Report.pdf.

David Roodman, James G. MacKinnon, Morten Orregaard Nielsen, and Matthew D. Webb. Fast and wild: Bootstrap inference in stata using boottest. *Stata Journal*, 19, 2019.

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.

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneou treatment effects. *Journal of Econometrics*, 2021.

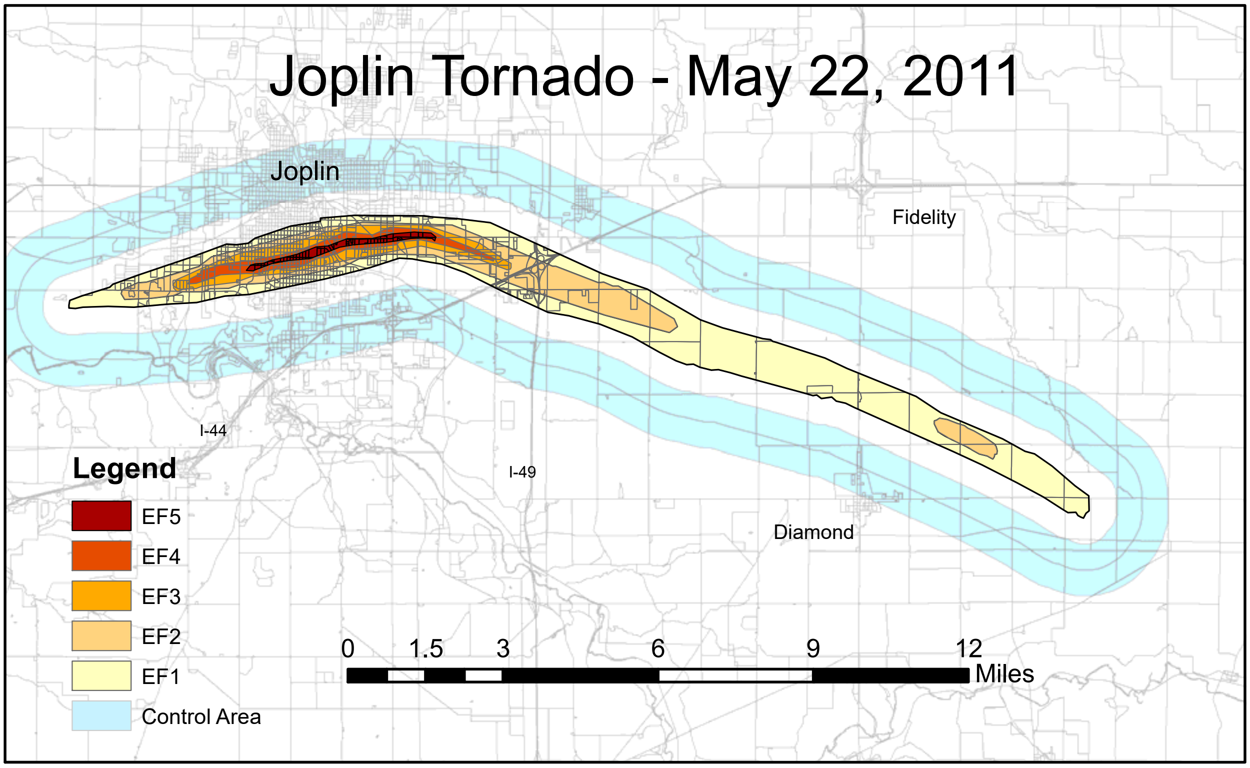
US Census Bureau SUSB. 2016 susb annual data tables by establishment industry. Technical report, December 2018.

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

Brigitte Roth Tran and Tamara L. Sheldon. Same storm, different disasters: Consumer credit access, income inequality, and natural disaster recovery. *Working Paper*, 2017.

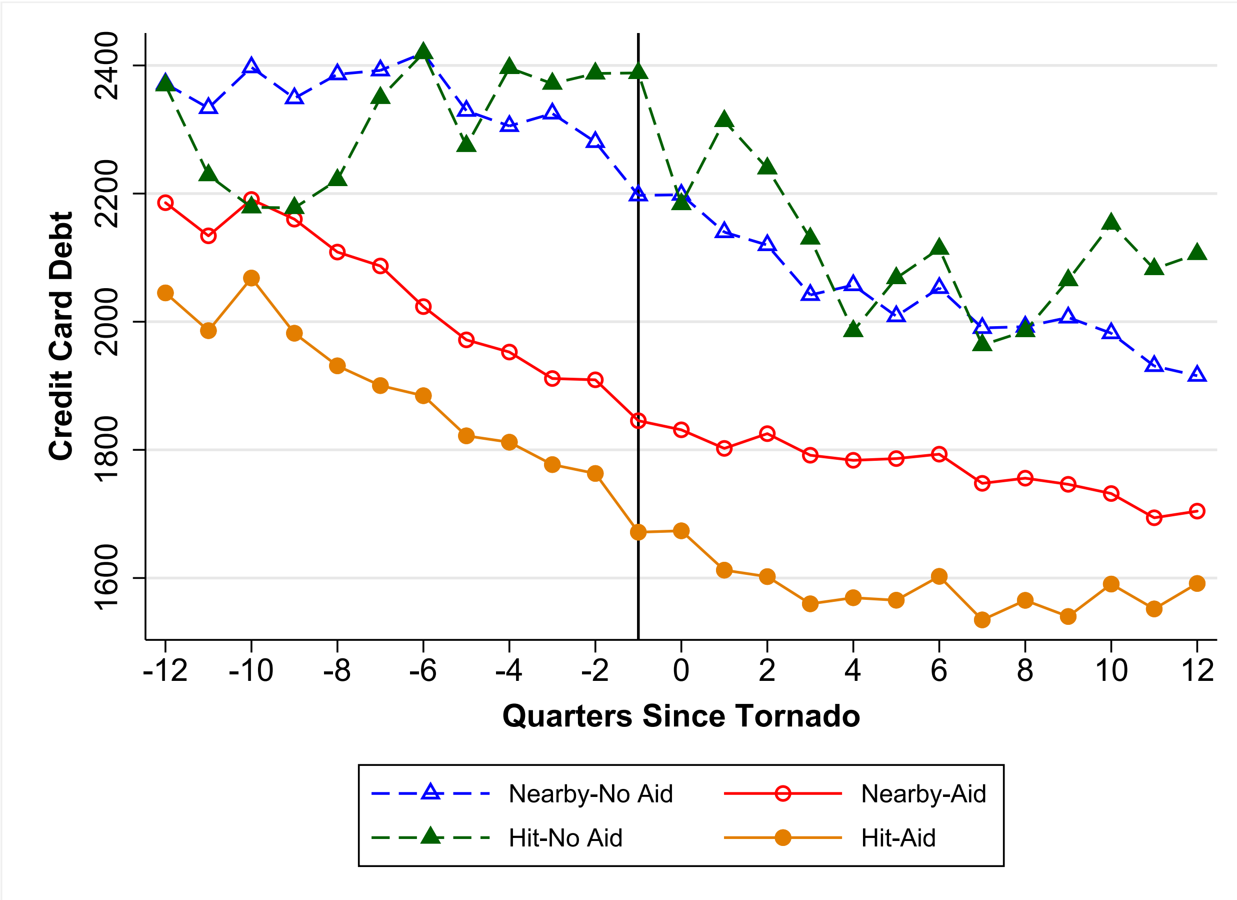
10 Figures and Tables

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



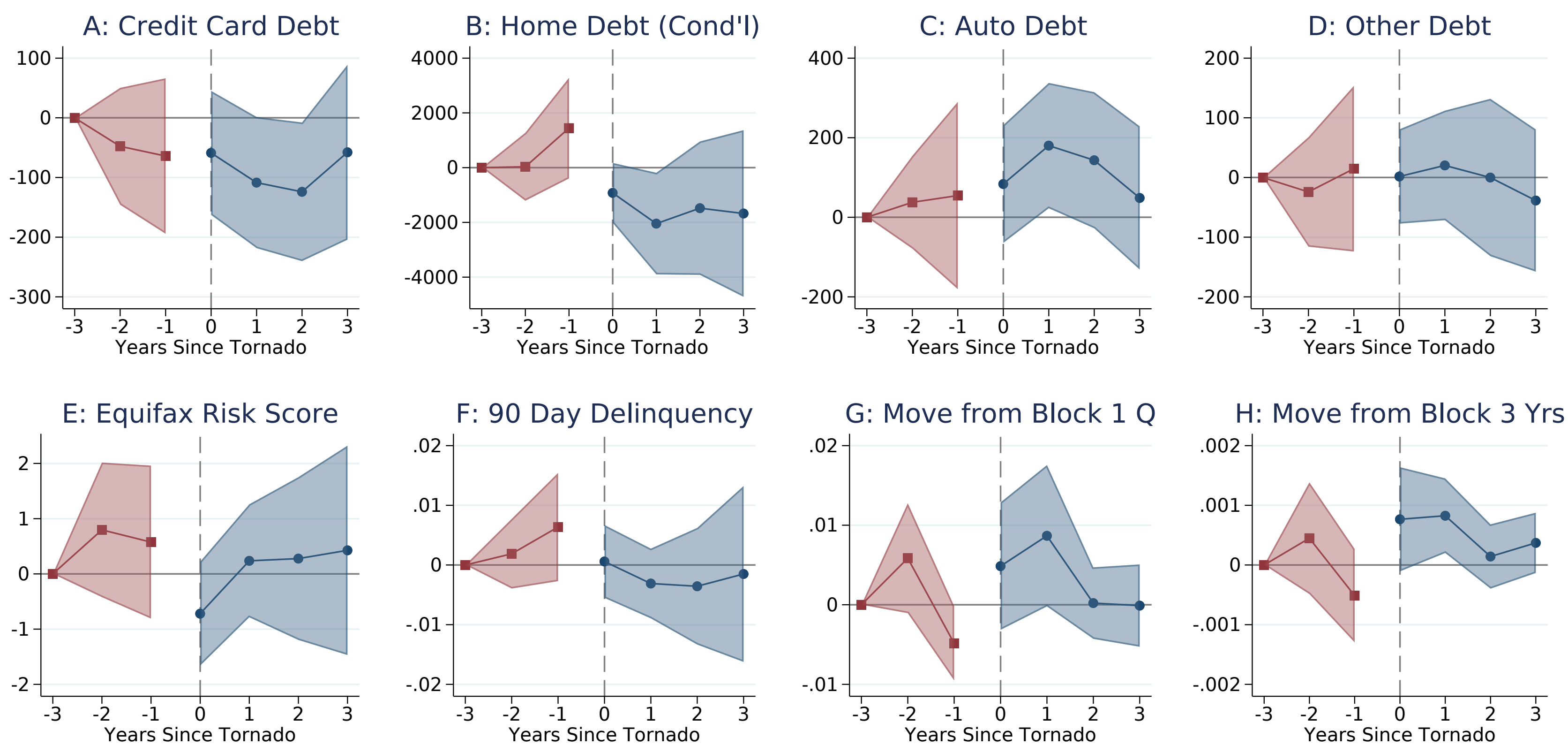
The figure shows the damage map for an EF5 tornado that hit Joplin, Missouri on May 22, 2011. The tornado path is outlined in black. The control area is in blue and located between 0.5 and 1.5 miles from the edge of the damage path. The tornado path and the control area are overlaid on a US Census block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

Figure 2: Trends in Credit Card Debt



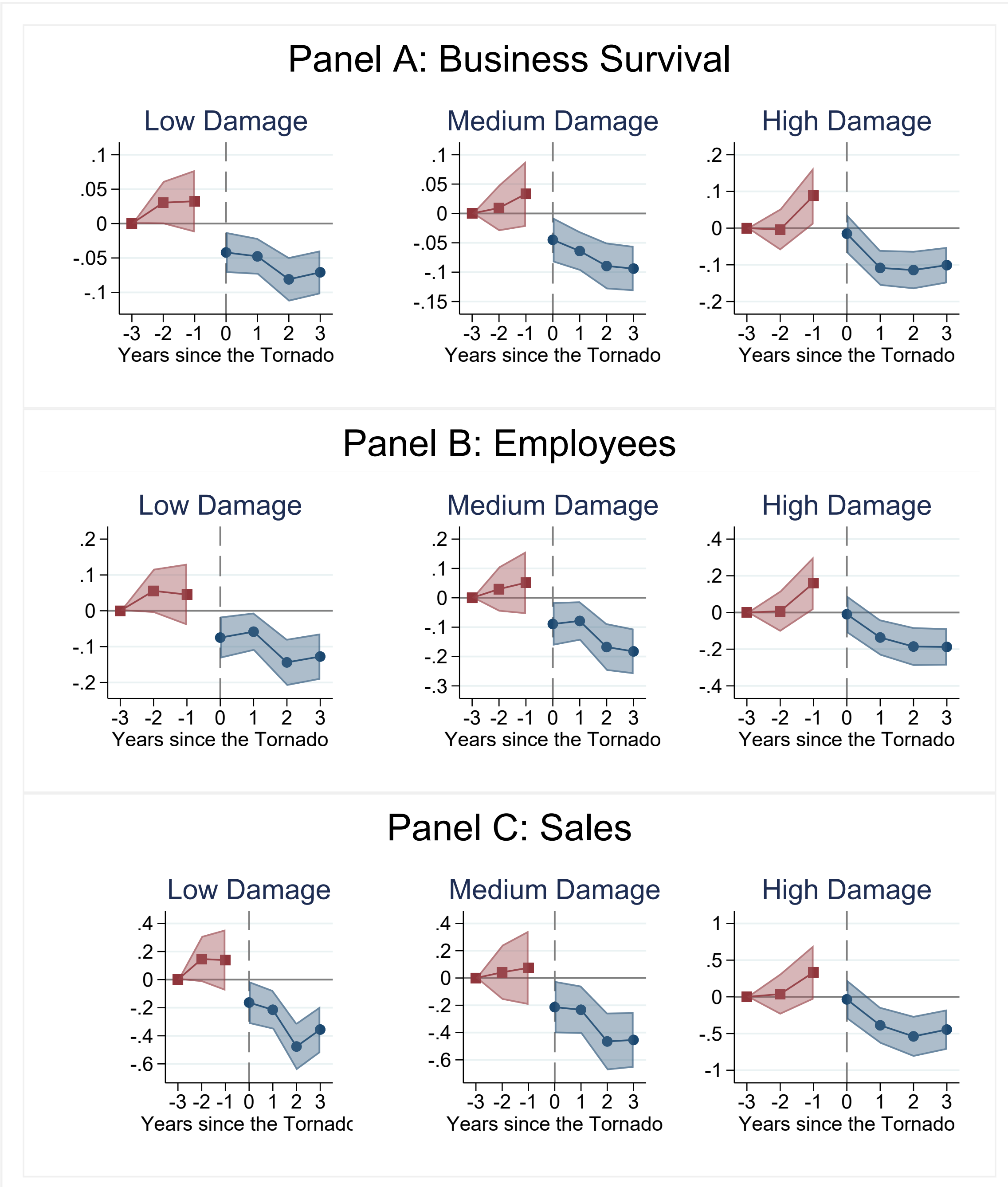
The figure plots the mean credit card balance (bank cards) of four groups of individuals: non-hit residents who lived in the 0.5 to 1.5 mile buffer area around the tornadoes that did not receive cash grants (dashed blue triangles), hit residents who lived in the damage path of tornadoes that did not 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: Debt, Financial Wellbeing, and Migration of being Hit by a Tornado



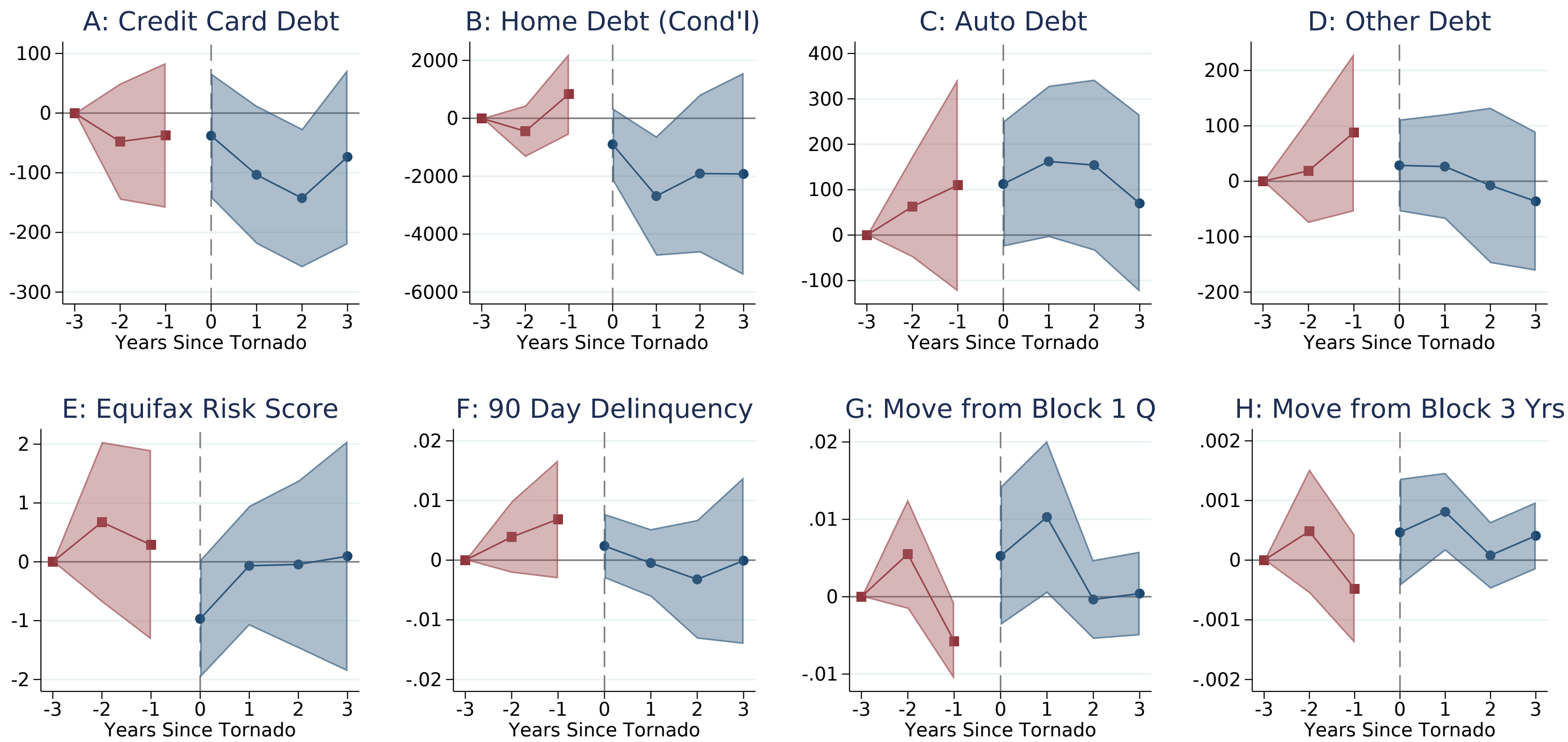
The figure plots DiD event study estimates for our main household finance and migration outcomes using the continuous damage model. The dashed vertical line indicates the *quarter* of the tornado. All other plotted coefficients are yearly. The post-tornado coefficients are estimated using an imputation-based method, and are relative to the year before a tornado. The pre-tornado yearly event time coefficients (squares) are estimated using OLS with three years before the tornado as the reference period. The shaded regions represent the 95% confidence intervals. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 4: Business Survival, Employment, and Sales of being Hit by a Tornado



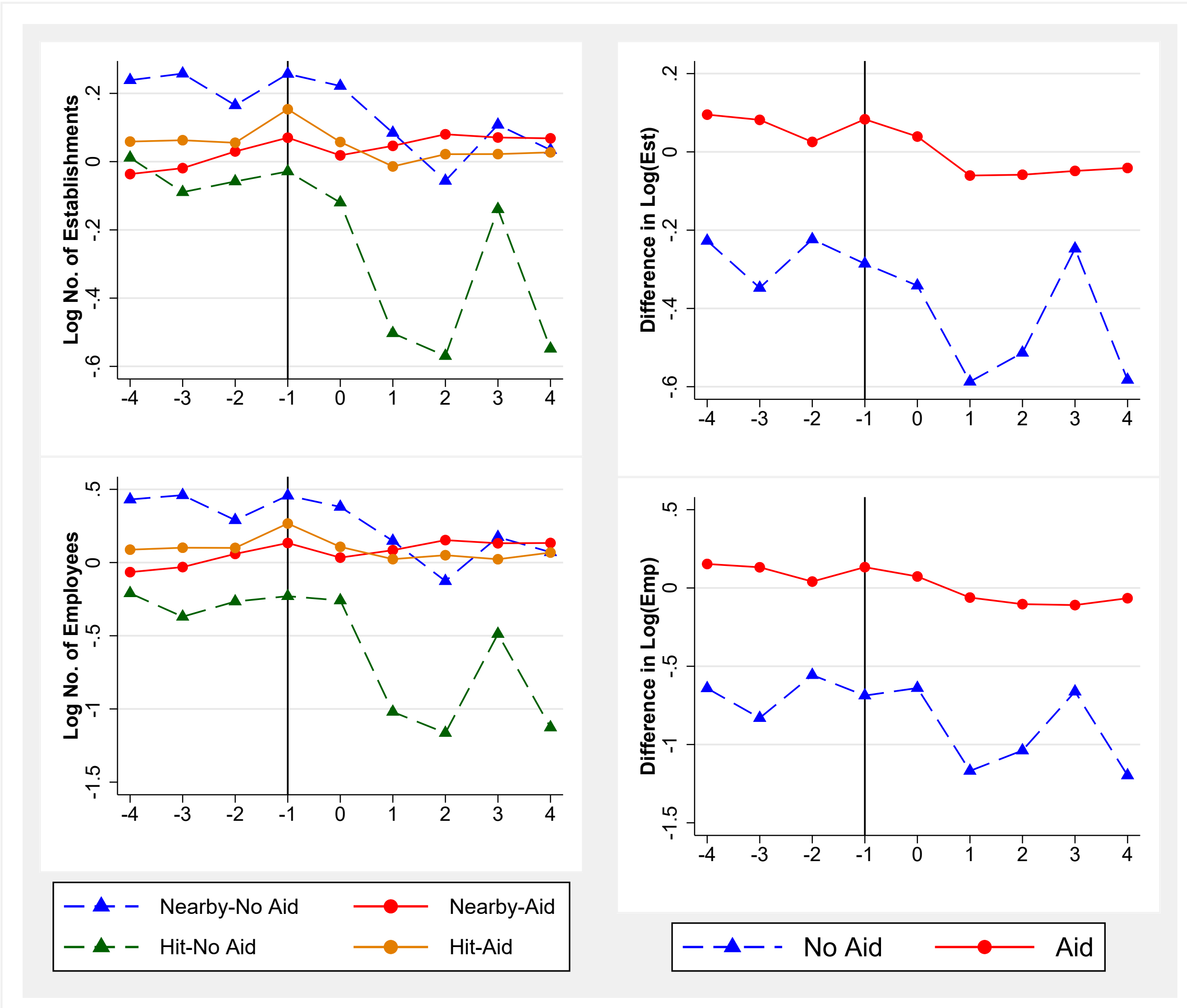
The figure shows event study estimates and 95% confidence intervals for business outcomes based on being located in a hit tornado block versus a non-hit block just outside the tornado path. The business outcomes are business survival, log number of employees, and log dollar sales, by whether a block suffers low, medium, or high tornado damage. All plotted coefficients are yearly, except for the quarter of the tornado (dashed line). The event year three years before the tornado is excluded from the model and serves as the reference time period. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Figure 5: Debt, Financial Wellbeing, and Migration of being Hit by a Disaster-Aid Tornado



The figure plots triple difference event study estimates for our main household finance and migration outcomes using the continuous damage model. The dashed vertical line indicates the *quarter* of the tornado. All other plotted coefficients are yearly. The post-tornado coefficients are estimated using an imputation-based method, and are relative to the year before a tornado. The pre-tornado yearly event time coefficients (squares) are estimated using OLS with three years before the tornado as the reference period. The shaded regions represent the 95% confidence intervals. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 6: 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 the 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 and Migration Impact of being Hit by a Tornado**

	Categories of Consumer Debt				Financial Health		Migration	
Dependent Variable:	Credit Card	Home	Auto	Other	Equifax Risk Score	90 Day Delinquency	1 Quarter	3 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Continuous Damage								
Post x Hit	-58	-674	68	7	-0.03	-0.0002	0.001	0.0002
	(33)	(281)	(30)	(21)	(0.31)	(0.0016)	(0.001)	(0.0001)
Dep. Variable Mean	$2,467	$67,404	$3,190	$1,278	674	0.20	0.053	0.0010
R-squared	0.009	0.018	0.003	0.017	0.002	0.001	0.006	0.000
Observations	496,708	123,602	496,708	496,708	492,439	496,708	763,632	763,632
Panel B: Binned Damage								
Post x Low	-18	-100	53	-14	0.53	-0.0033	0.002	0.0001
	(80)	(1,340)	(104)	(71)	(0.75)	(0.0069)	(0.002)	(0.0003)
Dep. Variable Mean	$2,348	$69,580	$3,157	$1,317	675	0.20	0.047	0.0003
Post x Medium	-128	-4076	169	-72	0.40	-0.0053	0.000	0.0004
	(118)	(947)	(114)	(85)	(1.27)	(0.0068)	(0.003)	(0.0005)
Dep. Variable Mean	$2,621	$67,467	$3,506	$1,323	675	0.20	0.057	0.0024
Post x High	-374	-1064	360	126	-1.39	0.0047	0.006	0.0011
	(265)	(1,990)	(188)	(75)	(1.79)	(0.0074)	(0.008)	(0.0005)
Dep. Variable Mean	$2,579	$59,031	$2,630	$1,037	670	0.20	0.063	0.0010
R-squared	0.009	0.017	0.003	0.017	0.002	0.001	0.006	0.000
Observations	496,708	123,602	496,708	496,708	492,439	496,708	763,632	763,632

The table shows difference-in-differences estimates for eight outcomes. The model includes individual and quarter fixed effects. Only the coefficients of interest are reported. The binned coefficients in panel B are estimated separately for individuals in blocks with low, medium, and high damage. Dependent variable means are for the last quarter before a tornado for the hit group. The debt variables are winsorized at 99%. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 2: **Impact of being Hit by a Tornado on Business Survival, Employment, and Sales**

Dependent Variable:	Log(Establishments)	Log(Employment)	Log(Sales)
	(1)	(2)	(3)
Panel A: Continuous Damage			
Post x Hit	-0.019	-0.036	-0.108
	(0.005)	(0.009)	(0.024)
R-squared	0.550	0.542	0.478
Observations	143,449	143,449	143,449
Panel B: Binned Damage			
Post x Low	0.044	0.063	0.148
	(0.015)	(0.029)	(0.075)
Post x Medium	-0.039	-0.116	-0.273
	(0.024)	(0.045)	(0.116)
Post x High	-0.100	-0.170	-0.497
	(0.026)	(0.051)	(0.132)
R-squared	0.550	0.542	0.478
Observations	143,449	143,449	143,449

The table shows difference-in-differences estimates for three business establishment outcomes. The model includes block and year fixed effects. Only the coefficients of interest are reported. The binned coefficients in panel B are estimated separately for individuals in blocks with low, medium, and high damage. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 3: **Household Finance and Migration Impact of being Hit by a Tornado when Federal Disaster Assistance is Available**

	Categories of Consumer Debt				Financial Health		Migration	
Dependent Variable:	Credit Card	Home (Conditional)	Auto	Other	Equifax Risk Score	90 Day Delinquency	1 Quarter	3 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Continuous Damage								
Disaster Aid x Post x Hit	-39	-2,179	42	7	1.3	-0.0051	0.004	-0.0001
	(81)	(574)	(71)	(52)	(0.9)	(0.0035)	(0.002)	(0.0002)
Dep. Variable Mean	$2,411	$66,371	$3,143	$1,300	672	0.2073	0.054	0.0011
R-squared	0.009	0.017	0.003	0.017	0.001	0.000	0.005	0.000
Observations	496,708	123,602	496,708	496,708	492,439	496,708	763,632	763,632
Panel B: Binned Damage								
Disaster Aid x Post x Low	-405	-3,827	440	-227	-2.3	0.0405	0.008	-0.0013
	(239)	(2,721)	(386)	(148)	(2.9)	(0.0215)	(0.006)	(0.0018)
Dep. Variable Mean	$2,287	$68,614	$3,148	$1,362	673	0.2085	0.048	0.0003
Disaster Aid x Post x Medium	425	598	346	240	8.3	-0.0509	0.009	0.0008
	(327)	(2,353)	(385)	(228)	(3.8)	(0.0170)	(0.005)	(0.0008)
Dep. Variable Mean	$2,532	$65,659	$3,429	$1,320	673	0.2058	0.059	0.0026
Disaster Aid x Post x High	-773	-19,479	-289	-153	1.3	0.0001	0.004	-0.0019
	(307)	(2,414)	(295)	(94)	(2.3)	(0.0109)	(0.009)	(0.0005)
Dep. Variable Mean	$2,611	$59,365	$2,527	$1,033	666	0.2059	0.067	0.0011
R-squared	0.009	0.017	0.003	0.017	0.001	0.000	0.005	0.000
Observations	496,708	123,602	496,708	496,708	492,439	496,708	763,632	763,632

The table shows triple difference estimates for eight outcomes. The model includes individual and quarter fixed effects. Only the coefficients of interest are reported. The binned coefficients in panel B are estimated separately for individuals in blocks with low, medium, and high damage. Dependent variable means are for the last quarter before a tornado for the hit group. The debt variables are winsorized at 99%. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 4: **Access to Credit and the Household Finance and Migration Impact of being Hit by a Tornado when Federal Disaster Assistance is Available**

Dependent Variable:	Credit Card	Home (Conditional)	Auto	90 Day Delinquency	1 Quarter Move	3 Year Move
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Available Credit						
Low Available Credit						
Disaster Aid x Post x Hit	-2	-180	161	-0.0102	0.003	0.0002
	(53)	(942)	(59)	(0.0080)	(0.002)	(0.0007)
Dependent Variable Mean	$238	$55,950	$1,526	0.3150	0.052	0.0004
Observations	152,278	13,439	152,278	152,278	246,950	246,950
High Available Credit						
Disaster Aid x Post x Hit	-226	-5,037	76	0.0037	0.009	0.0002
	(116)	(946)	(90)	(0.0039)	(0.003)	(0.0004)
Dependent Variable Mean	$4,523	$67,655	$3,797	0.0391	0.045	0.0019
Observations	170,386	67,901	170,386	170,386	262,603	262,603
Panel B: Credit Score						
Low Equifax Credit Score						
Disaster Aid x Post x Hit	76	1,388	256	-0.0416	-0.001	-0.0003
	(72)	(1,444)	(179)	(0.0094)	(0.002)	(0.0002)
Dependent Variable Mean	$1,556	$57,003	$2,497	0.5249	0.071	0.0005
Observations	161,520	21,380	161,520	161,520	246,339	246,339
High Equifax Credit Score						
Disaster Aid x Post x Hit	-17	-4,262	9	0.0004	0.007	0.0001
	(90)	(871)	(77)	(0.0004)	(0.003)	(0.0004)
Dependent Variable Mean	$2,090	$72,028	$2,747	0.0000	0.031	0.0005
Observations	165,527	55,420	165,527	165,527	245,138	245,138

The table shows triple difference heterogeneity estimates for six outcomes (omitting credit score and other debt) in Tables 3 using the continuous damage model. The model is estimated separately on two groups of individuals (lower and upper terciles) based on available credit (panel A) and Equifax Risk Score (panel B). The credit card and credit score cutoffs are based on averages across the 12 pre-tornado quarters. The tercile cutoffs are as follows: $149 and $11,364 for available credit, and 618 and 759 for Equifax Risk Score. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 5: **Estimates for the Number of Business Establishments and Employees following a Tornado when Federal Disaster Assistance is Available**

Model:	Triple Difference		Difference-In-Differences			
Dependent Variable:	Log (Establishments)	Log (Employment)	Log(Establishments)		Log(Employment)	
Tornado Type:			Aid	No-Aid	Aid	No-Aid
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Continuous Damage						
Disaster Aid 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 B: Binned Damage						
Disaster Aid 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)
Disaster Aid x Post x Medium	0.133	0.151	-0.060	-0.178	-0.116	-0.213
	(0.062)	(0.118)	(0.024)	(0.076)	(0.046)	(0.140)
Disaster Aid 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 disaster aid on the number of establishments and employment. Columns (3)-(6) show difference-in-differences model estimates separately for aid and no-aid tornadoes. The binned coefficients in panel B are estimated separately for individuals in blocks with low, medium, and high damage. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 6: **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		
Disaster Aid x Post x Hit	0.048	0.070
	(0.023)	(0.041)
R-squared	0.560	0.553
Manufacturing		
Disaster Aid 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)		
Disaster Aid x Post x Hit	-0.005	-0.009
	(0.005)	(0.010)
R-squared	0.379	0.318
Existing (4 years or more)		
Disaster Aid 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)		
Disaster Aid x Post x Hit	0.043	0.048
	(0.014)	(0.020)
R-squared	0.544	0.529
Large (≥ 7 Employees)		
Disaster Aid 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 5 panel A, 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. Bootstrapped standard errors (in parentheses) are robust to heteroskedasticity and tornado-level correlation. Sources: Infogroup Historic Business Database, National Weather Service, US Census.