

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

Justin Gallagher, Daniel Hartley, and Shawn Rohlin*

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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 damaged Census blocks have a small *reduction* in debt and no change in bill delinquency. The business establishment survival rate declines by 9%. 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 14% more employees post-disaster in the average-damaged neighborhood. These effects are concentrated among small non-manufacturing establishments that rely on local demand.

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1 Introduction

Natural disasters in the United States are shocks to income, wealth, and capital. The US government has a long history of federal assistance following natural disasters. \$2.2 billion in cash grants was allocated to disaster victims as part of 124 Presidential Disaster Declarations in 2021 (FEMA [2021]). The implicit assumption is that savings, credit markets, and insurance (e.g. homeowners, unemployment) are insufficient to smooth the negative financial consequences of these natural disasters. Scant evidence exists on the role that disaster assistance has in preventing negative financial outcomes. Our aim is to help fill this gap.

We study large tornadoes. Tornado damage is idiosyncratic. Buildings in one block can be destroyed, while those just a few blocks away are left unscathed. Tornadoes are also unique in that they cause tremendous damage, but the spatial scale of the damage is much smaller than other types of natural disasters, such as hurricanes. How individuals and businesses manage the financial shock of a localized disaster is largely unknown, and may differ from situations when an entire region is directly impacted.

This paper has two goals. First, we document the medium-term impact of being directly hit by a devastating tornado on household finance, business survival, and migration using event study and difference-in-differences (DiD) models. We use credit bureau data to estimate the causal effect of residing in a block with a specific level of damage on post-disaster financial outcomes and migration. We examine the impact on business survival and employment using a proprietary establishment-level database.

Most of the existing literature on the financial impact of natural disasters examines single events. Our sample of nearly three dozen tornadoes improves, we believe, the generalizability of our findings. We are not aware of another study on the detailed financial impact of being hit by a tornado.

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]). 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]).

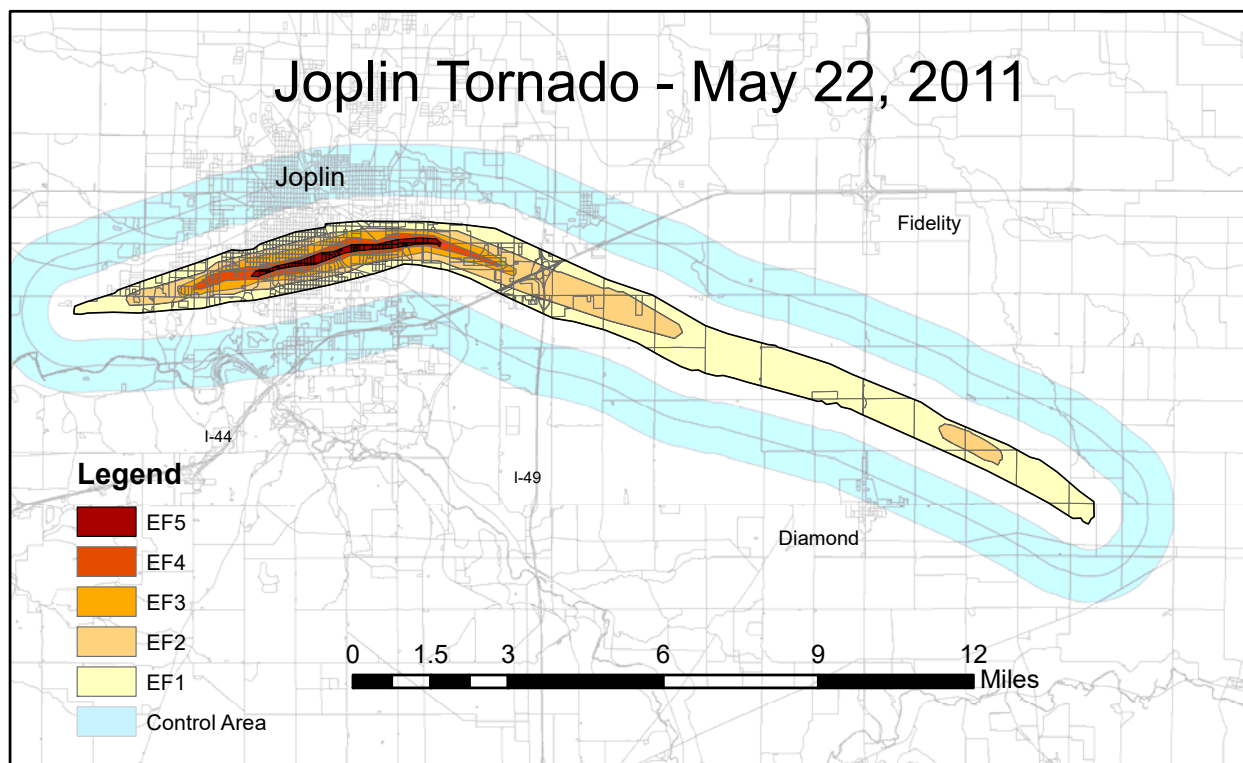


Figure 1 shows the Joplin, MO 2011 tornado damage path outlined in black. The control area in blue is located between 0.5 and 1.5 miles from the edge of the path. The grey background lines are US Census blocks. Sources: National Weather Service, US Census.

The second goal of this study is to provide insight into how federal disaster assistance affects post-disaster outcomes. Federal disaster assistance is not always distributed to tornado victims. A key reason is that tornadoes tend to have a small spatial scale. The brunt of the Joplin tornado, one of the largest tornadoes in our sample period, hit just 10% of a single ZIP Code (see Appendix Figure 1).

We estimate the impact of disaster aid using a triple difference econometric model. Our focus is on cash assistance. 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. 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.

We find that individuals hit by a tornado have a modest overall post-disaster *reduction* in personal debt and no change in measures of financial distress. 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 5% in the first year following a tornado. Disaster assistance dramatically reduces credit card debt and leads to higher migration rates. Credit-constrained individuals have lower rates of bill delinquency when disaster aid is available, and purchase (replacement) vehicles at higher rates.

Disaster assistance is a place-based policy. Cash grants are provided to individuals, but the distribution of the grants is concentrated in the most-damaged neighborhoods. 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.

Overall, there are fewer business establishments in hit blocks in the immediate years following the disaster. However, we estimate that there are approximately 9% more establishments and 14% more employees in blocks that sustain the average level of tornado damage following disaster aid tornadoes, as compared to no-aid tornadoes. The increase in the number of establishments is due to a higher survival rate for existing non-manufacturing establishments.

Ideally, we would be able to completely isolate the role of cash grants on post-disaster household finance, migration, and business survival. 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 confounder 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 to aid in the reconstruction of public buildings. Individual and Public Assistance grants following a tornado are highly correlated.¹ 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

¹We 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.

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 are the same for victims of disaster aid and no-aid tornadoes. We discuss reasons why our disaster aid estimates might best be interpreted as a lower (or upper) bound in Section 4 when we describe our econometric model in detail.

The disaster aid tornado estimates in the second half of the paper should be interpreted as the combined impact of direct cash assistance and federal public infrastructure grants. In the Discussion Section, we argue that the spatial pattern of disaster assistance, combined with our estimation results, is most consistent with cash assistance as the primary underlying mechanism.

Our paper contributes to the growing literature that uses natural disasters as a wealth (or income) shock to study household finance and business outcomes. Most of the papers in this literature examine the impact of wind and flooding from hurricanes, and typically focus on a single event. Household finance studies include: Billings et al. [2021]; Del Valle et al. [2019]; Deryugina et al. [2018]; Gallagher and Hartley [2017]; Groen et al. [2020]. These papers all conclude that the average negative financial impact is modest and short-lived.

Basker and Miranda [2017] examine business survival and estimate a 30 percentage point decrease in the survival rate of businesses damaged by a severe hurricane. Collier et al. [2021] use credit reports and survey data to evaluate business finances following Hurricane Harvey. They find that short-term credit delinquencies increased overall, and that borrowing increased for firms without existing debt.

Our paper also contributes to the literature on how natural disasters affect migration. Previous research is mixed on whether experiencing a natural disaster leads to increased migration. Hornbeck [2012] and Boustan et al. [2012] show that net out-migration increases following natural disasters in the US during the first half of the 20th century. Deryugina [2017], however, finds no net population change in response to US hurricanes during the 1980's and 1990's. The expansion of both explicit (disaster) and implicit (social safety net) federal transfers to disaster victims in the second half of the 20th century may help to explain the lower effect on migration. The migration findings in our paper are consistent with Gallagher and Hartley [2017] who present suggestive evidence that the propensity to migrate from New Orleans after Hurricane Katrina was greater for individuals who received lump sum (flood insurance) cash payments.

Gregory [2017] and Roth Tran and Sheldon [2017] also explore how federal disaster assistance affects financial and migration outcomes. Gregory [2017] 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. The study concludes that the grant program increased rebuilding in New Orleans. Roth Tran and Sheldon [2017] find a limited financial impact on credit outcomes for individuals in Presidential Disaster Declaration counties where Individual Assistance was made available.

Our results show that federal disaster assistance most benefits credit-constrained tornado victims. Moreover, disaster assistance provided to tornado victims appears to dramatically improve the survival rate of businesses in these same neighborhoods. Together, our findings imply that disaster assistance policies will have the largest impact in preventing both individual financial distress and business closure when aid is targeted towards more vulnerable, credit-constrained disaster victims.

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. Not all of the damage is insured. The typical insurance policy has 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 because of 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 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 expect there to be a limited

debt response following a tornado when disaster assistance is *unavailable*. These individuals may be forced to sharply reduce consumption. Moreover, credit-constrained individuals may suffer from greater negative financial outcomes such as bill delinquency.

We make the following predictions when cash grants are *available* for credit-constrained individuals. First, credit-constrained individuals will reduce their post-disaster debt by less than those who are not 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, cash grants are more likely to be pivotal in preventing financial distress for victims with a limited ability to borrow. Third, 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.

A limitation of our paper is that savings are not observable in our data. The typical adult, however, 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]). Further, the exact tornado path is completely random. This randomness minimizes the scope for individuals to use disaster risk information to differentially invest in precautionary savings. Thus, it is unlikely that the average level of savings differs for individuals hit by a tornado and those just outside of the tornado path.

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 and increases 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. Second, cash assistance to individuals that happen to be small business owners may positively affect business survival. 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]).²

²The 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

Public Assistance grants to repair infrastructure could also aid business establishments if the grants increase local spending. For example, local construction companies may be contracted for repair work. Public Assistance grants could indirectly spur local growth by facilitating commerce (e.g. road reconstruction), or via economic spillovers such as through the repair of public buildings.

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 (NWS) 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 5.³ Third, the tornado must have a high quality damage path map, generally created by the NWS, that demarcates areas of the tornado path that suffered different levels of damage. Appendix Section 1.1 provides details on how the NWS creates the damage maps using on the ground observations and a detailed engineering model that takes into account the strength of the damaged materials and local building codes. Thirty-five tornadoes satisfy the three criteria. Our sample includes 34 tornadoes, as one tornado violates the pre-trend assumption of our sample design. We provide more details when we discuss our results. 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 two major types of assistance: Individual Assistance (cash grants) and Public Assistance.

employees (US Census [2018]).

³Tornado 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.

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 January 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 for 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 an aid tornado and \$344 for a no-aid tornado (see Appendix Table 5).⁴

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. FEMA’s trauma criteria appears to influence whether cash assistance is made available. 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. Tornadoes with cash assistance are part of larger state-level disasters as measured by the percent of

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

the state’s counties included in the PDD. Tornadoes with cash assistance also tend to cause more block-level damage. The difference in the overall damage between tornadoes with and without cash assistance motivates our econometric models that account for the heterogeneity in block-level damage.

Private disaster relief is a potential confounder. 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).⁵

We obtained information on all cash grants distributed under the Individual Assistance program via a Freedom of Information Act 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 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

⁵Deryugina and Marx [2021] 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 for federal disaster aid versus no-aid tornadoes.

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. Anonymous identifiers allow us to track all individuals, even if they move away from the tornado-affected area, or if they were living elsewhere for some portion of the pre-tornado period.

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. Consumer finance loans make up just over one percent of total pre-tornado debt in our sample. 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 type, 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 are nearly identical in the quarter before a tornado. However, individuals hit by tornadoes where cash assistance is available are economically worse off (lower Equifax Risk Score, higher 90 day delinquency rate, and live in Census blocks with a lower home ownership rate) than individuals hit by tornadoes where cash assistance is not available (columns 4 and 7).

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.⁶ 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, or 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.

There are several advantages of using tornadoes as a source of exogenous disaster 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 [Forthcoming]). 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 precau-

⁶The 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.

tionary savings. Measuring the 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]).

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]). We first describe the specification that we use to examine individual financial outcomes. At the end of this section we document the differences in the empirical specifications for the business 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 time period t . 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. Note that τ_0 is the *quarter* of the tornado, while the other τ_h coefficients are yearly.⁷ 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. α_i is an individual fixed effect, γ_t is a time-period-by-year fixed effect, and ε_{it} is an error term. We cluster the standard errors by tornado to account for the spatial correlation in tornado damage and disaster assistance.

We estimate Equation 1 using OLS. The recent methodological literature on event studies has shown several potential limitations with the OLS event study model (e.g. Borusyak et al. [2021]; de Chaisemartin and D’Haultfoeuille [2020]; Sun and Abraham [2021]). These limitations include a strong assumption regarding the homogeneity of the event time treatment effects across calendar time and individuals, 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 parameter weighting problem. One drawback is that this imputation approach does not allow for a continuous treatment. We estimate a binary damage event study model (hit versus not hit) following Borusyak et al. [2021] as a robustness model. The pattern of results are similar to our OLS estimates from Equation 1.

⁷We consider the quarter of the tornado separately, but the remaining quarters are pooled by event year. For example, τ_1 includes the first four quarters following the quarter of the tornado. We prefer the yearly event study model to the quarterly model. The estimated pattern of the yearly coefficients is more informative. First, pooling the quarterly data increases the statistical precision. Second, our financial distress outcomes are low incidence outcomes, and estimating yearly coefficients smooths out the high quarter-to-quarter variance. Finally, the yearly dynamics match the business establishment panel and help to facilitate an easier comparison between individual and establishment-level outcomes.

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.

This 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.

Equation 2 is a DiD model that 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. 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. Note that the non-interacted Hit_i variable is subsumed by the individual fixed effects. We again cluster the standard errors by tornado.

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

In some specifications of Equation 2 we only include a subset of the 34 tornadoes. A potential concern is that clustered standard errors may overstate the model’s precision. The standard errors are very similar in all our specifications regardless of whether we cluster the standard errors by tornado, or use the wild cluster bootstrap (Cameron and Miller [2015]).⁸

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 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]).

We examine the role of federal disaster assistance on post-disaster outcomes using a triple difference model (Equation 3). 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.⁹ 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} .

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

⁸We conduct bootstrap inference using Stata’s *boottest* package (Roodman et al. [2019]). The wild cluster bootstrap is robust to estimating the model with a smaller number of clusters.

⁹The 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 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 (the solid triangles and solid circles). Doing so would lead to a biased causal estimate due to the downward pre-tornado 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.

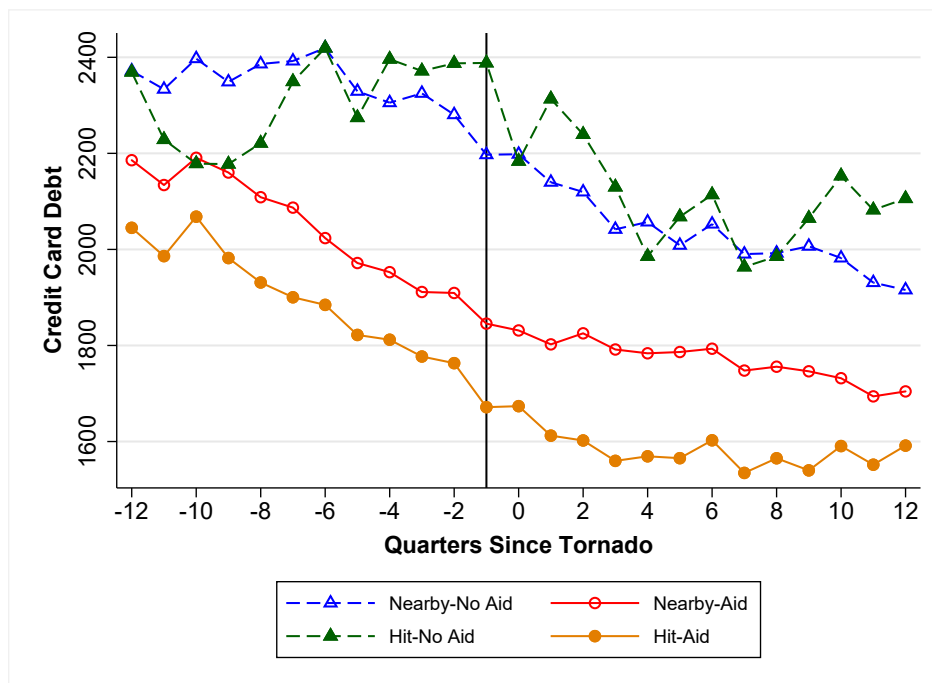


Figure 2 shows trends in dollars of credit card debt. The vertical line indicates the last quarter before a tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

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.¹⁰ Differencing with the nearby residents reduces, but does not eliminate this concern. It is also 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.¹¹

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 define 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.¹² 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 include the year of the tornado, but 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.

¹⁰The counterfactual of being hit by a tornado and not having access to cash grants is likely to slightly overstate the financial resiliency of hit victims with access to grants. Any improvement in financial outcomes due to cash assistance is measured relative to this counterfactual, which may attenuate the measured effect. We also note that if individuals who receive disaster aid have unobservably worse damage on average, then this would also likely underestimate the causal effect of disaster assistance.

¹¹A 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 of non-flooded blocks, whereas Deryugina et al. [2018] use a control group outside the city. Both studies find similar results. 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).

¹²Specifically, 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 DiD event study estimates for our main household finance and migration outcomes using the continuous damage model. The four debt categories (credit card, home, auto, and other) are shown in Panels A-D.¹³ There is a modest, statistically significant decline in credit card debt during the first two post-tornado years. The reduction is equal to approximately \$135 or 6% of credit card balances for an individual located in a hit block that sustains the average level of damage (EF 1.8).¹⁴ Home debt also decreases in the year following a tornado by approximately 4% for an individual located in the average-damaged block. Auto debt increases by roughly 6% 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-term 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 damage who has a home loan continuously during the 12 quarters before the tornado. Auto debt increases by \$121 (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. The Equifax Risk Score (Figure 3, panel E) and the indicator for having a 90 day delinquent account (Figure 3, panel F) coefficients are economically small and statistically insignificant.

¹³Student 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.

¹⁴Throughout 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).

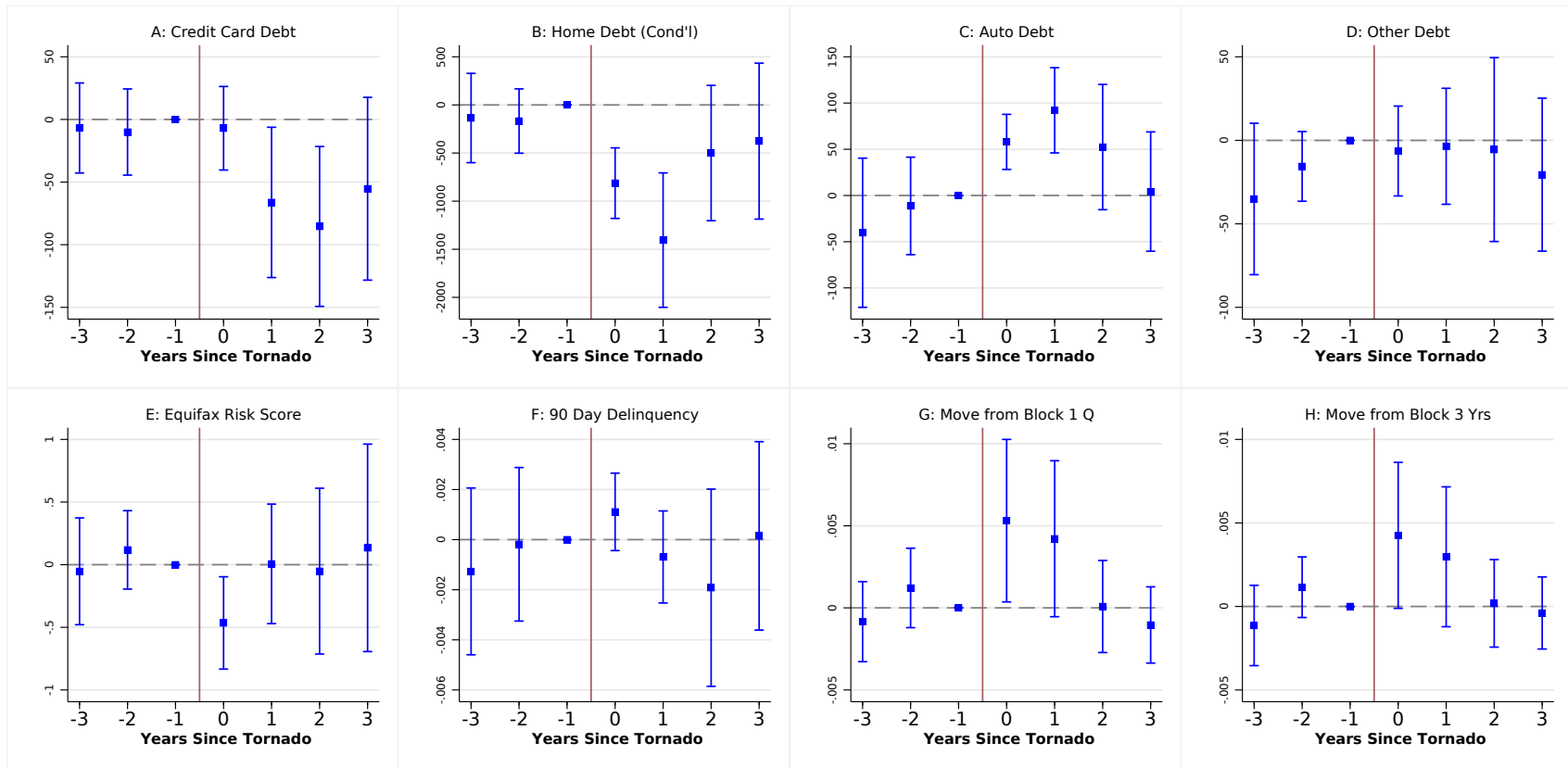


Figure 3 plots DiD debt, financial wellbeing, and migration continuous damage event study estimates and 95% confidence intervals for individuals hit by a tornado. The red vertical line splits each panel into the pre- and post-tornado periods. All plotted coefficients are yearly, except for time period 0 which represents the *quarter* of the tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 1: Household Finance and Migration Impact of being Hit by a Tornado

| Dependent Variable: | Categories of Consumer Debt | | | | Financial Health | | Migration | |
|-----------------------------------|-----------------------------|-----------|---------|---------|--------------------|--------------------|-----------|---------|
| | 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 | | | | | | | | |
| <u>Post x Hit</u> | -58* | -674** | 67** | 7 | -0.03 | -0.0002 | 0.001 | 0.001 |
| | (33) | (281) | (30) | (21) | (0.31) | (0.0016) | (0.001) | (0.001) |
| Dep. Variable Mean | \$2,467 | \$67,404 | \$3,190 | \$1,278 | 674 | 0.20 | 0.053 | 0.041 |
| R-squared | 0.009 | 0.018 | 0.003 | 0.017 | 0.002 | 0.001 | 0.006 | 0.006 |
| Observations | 496,708 | 123,602 | 496,708 | 496,708 | 492,439 | 496,708 | 763,632 | 763,632 |
| Panel B: Binned Damage | | | | | | | | |
| <u>Post x Low</u> | -18 | -100 | 53 | -14 | 0.53 | -0.0033 | 0.002 | 0.003 |
| | (80) | (1,340) | (104) | (71) | (0.75) | (0.0069) | (0.002) | (0.002) |
| Dep. Variable Mean | \$2,348 | \$69,580 | \$3,157 | \$1,317 | 675 | 0.20 | 0.047 | 0.038 |
| <u>Post x Medium</u> | -128 | -4,076*** | 169 | -72 | 0.40 | -0.0053 | 0.000 | 0.001 |
| | (118) | (947) | (114) | (85) | (1.27) | (0.0068) | (0.003) | (0.002) |
| Dep. Variable Mean | \$2,621 | \$67,467 | \$3,506 | \$1,323 | 675 | 0.20 | 0.057 | 0.041 |
| <u>Post x High</u> | -374 | -1,064 | 360* | 126 | -1.39 | 0.0047 | 0.006 | 0.006 |
| | (265) | (1,990) | (188) | (75) | (1.79) | (0.0074) | (0.008) | (0.007) |
| Dep. Variable Mean | \$2,579 | \$59,031 | \$2,630 | \$1,037 | 670 | 0.20 | 0.063 | 0.056 |
| R-squared | 0.009 | 0.017 | 0.003 | 0.017 | 0.002 | 0.001 | 0.006 | 0.006 |
| Observations | 496,708 | 123,602 | 496,708 | 496,708 | 492,439 | 496,708 | 763,632 | 763,632 |

Notes: Dependent variable means are for the last quarter before a tornado for the hit group. The debt variables are winsorized at 99%. Standard errors (in parentheses) are clustered by tornado: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

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.¹⁵

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

¹⁵The 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.

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 4.1% for the more temporary migration and 5.3% for the more permanent migration (Table 1 panel A columns 7 and 8).

Migration increases from hit blocks during the quarter of a tornado. We estimate that short-term migration from an average-damaged block increases by approximately 17% (p-value 0.036). The three year migration rate increases by approximately 19% (p-value 0.056). The migration rates for a hit block return to the pre-tornado rates by 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 residing in damaged blocks were not hit by the tornado. There is also no evidence for pre-trends in the binary damage imputation-based model (Borusyak et al. [2021]) that is robust to the limitations of the standard OLS event study model (see Appendix Figure 8).

There are three important caveats worth highlighting. First, the credit bureau data show 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 (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. 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 continuous damage DiD event study estimates and 95% confidence intervals for the log number of establishments (panel A), log employment (panel B), and log sales (panel C). 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.¹⁶

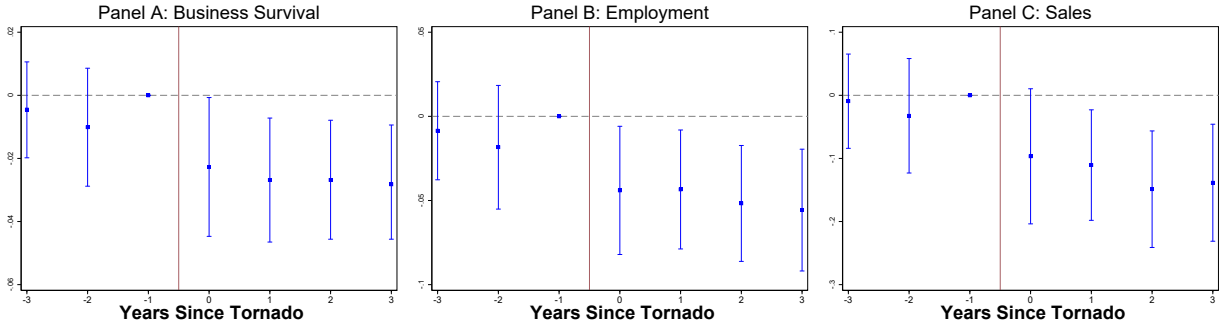


Figure 4 data sources: Infogroup Historic Business Database, National Weather Service, US Census.

There is no evidence of any pre-tornado trends. The 95% confidence interval for the pre-tornado coefficients always contains zero. Overall, the binary damage imputation-based model (Borusyak et al. [2021]) also confirms these results (see Appendix Figure 9). There is, however, suggestive evidence of a slight upward trend in low-damage blocks. The post-disaster coefficients show that the decrease in establishment survival and employment begin in the year when the tornado hits. The coefficients are negative, statistically significant, and fairly stable in the three 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 9% (p-value <0.001). To our surprise, the business survival rate is 4% higher (p-value 0.004) in low damaged blocks.

¹⁶We have no reason to expect that there is mis-measurement in employment and sales, apart from the fact that the information is survey-based. An academic-led study on the reliability of the Infogroup database concludes that the data are either similar to, or of higher quality, than other establishment-level datasets such as the National Establishment Time-Series dataset (University of Nebraska [2017]).

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 5% (p-value <0.001) and 17% (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. The low and high damage coefficients are all statistically significant at conventional significance levels.

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

| Dependent Variable: | Log(Establishments) (1) | Log(Employment) (2) | Log(Sales) (3) |
|-----------------------------------|----------------------------|------------------------|----------------------|
| Panel A: Continuous Damage | | | |
| <u>Post x Hit</u> | -0.016*** (0.005) | -0.03*** (0.009) | -0.095*** (0.023) |
| R-squared | 0.539 | 0.529 | 0.457 |
| Observations | 159,743 | 159,743 | 159,743 |
| Panel B: Binned Damage | | | |
| <u>Post x Low</u> | 0.043*** (0.015) | 0.061** (0.028) | 0.143** (0.071) |
| <u>Post x Medium</u> | -0.027 (0.022) | -0.054 (0.040) | -0.22** (0.107) |
| <u>Post x High</u> | -0.091*** (0.026) | -0.152*** (0.051) | -0.453*** (0.129) |
| R-squared | 0.539 | 0.529 | 0.457 |
| Observations | 159,743 | 159,743 | 159,743 |

Notes: Standard errors (in parentheses) are clustered by tornado: * p < 0.10, ** p < 0.05, *** p < 0.01.
Data sources: Infogroup Historic Business Database, National Weather Service, US Census.

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.

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 increase in credit card debt in the quarter of a tornado. Home debt decreases during the first three post-tornado years.

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.

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 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

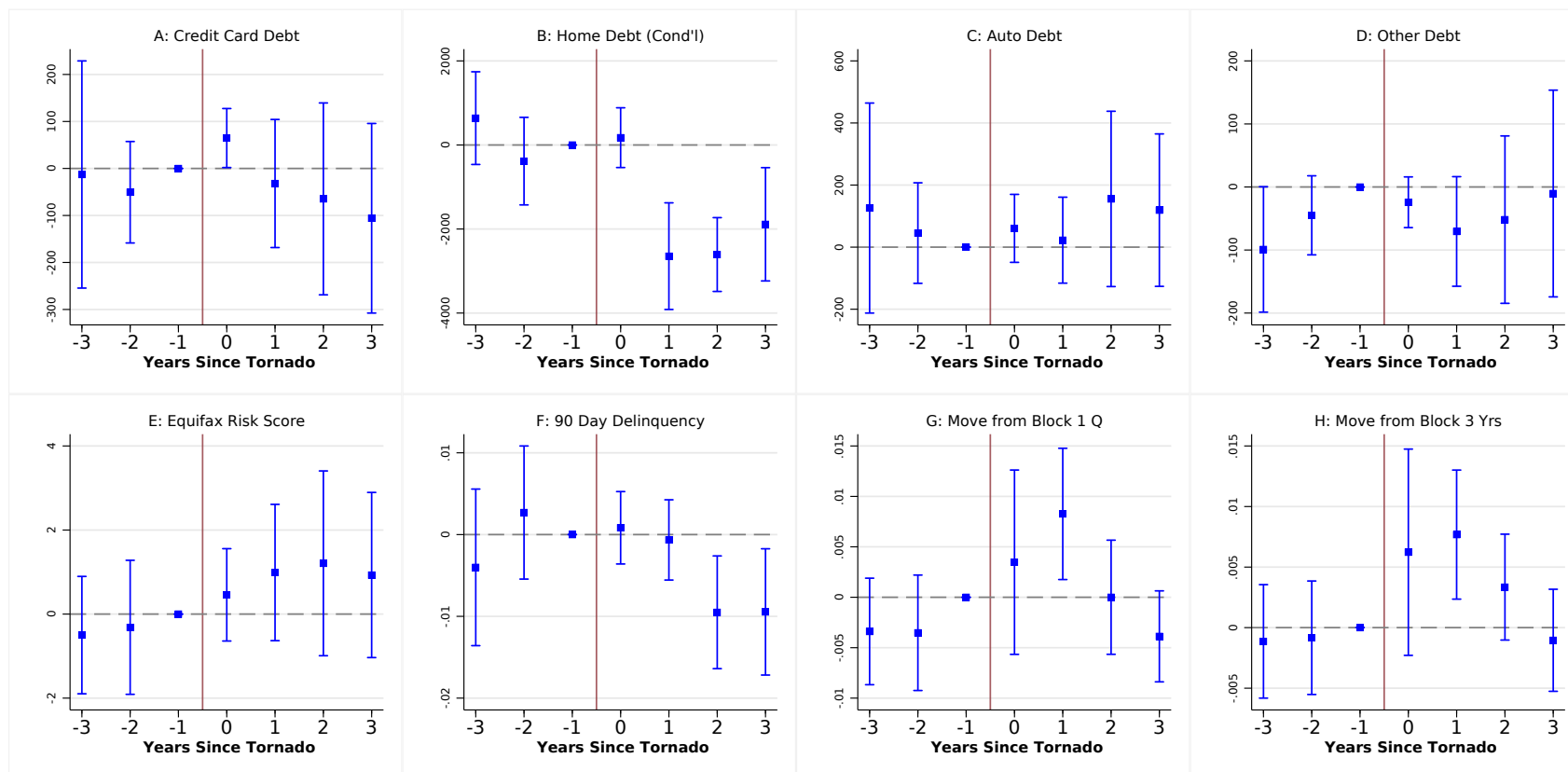


Figure 5 plots continuous damage triple difference debt, financial wellbeing, and migration estimates and 95% confidence intervals of being hit by a disaster-aid tornado. The red vertical line splits each panel into the pre- and post-tornado periods. All plotted coefficients are yearly, except for time period 0 which represents the *quarter* of the tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

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

| Dependent Variable: | Categories of Consumer Debt | | | | Financial Health | | Migration | |
|-------------------------------------|-----------------------------|-----------------------|---------------|---------------|--------------------|------------------------|--------------------|--------------------|
| | 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 | | | | | | | | |
| <u>Disaster Aid x Post x Hit</u> | -39 (81) | -2,179*** (574) | 42 (71) | 7 (52) | 1.3 (0.9) | -0.0051 (0.0035) | 0.004** (0.002) | 0.004** (0.001) |
| Dep. Variable Mean | \$2,411 | \$66,371 | \$3,143 | \$1,300 | 672 | 0.2073 | 0.054 | 0.042 |
| R-squared | 0.009 | 0.017 | 0.003 | 0.017 | 0.001 | 0.000 | 0.005 | 0.002 |
| Observations | 496,708 | 123,602 | 496,708 | 496,708 | 492,439 | 496,708 | 763,632 | 763,632 |
| Panel B: Binned Damage | | | | | | | | |
| <u>Disaster Aid x Post x Low</u> | -405* (239) | -3,827 (2,721) | 440 (386) | -227 (148) | -2.3 (2.9) | 0.0405* (0.0215) | 0.008 (0.006) | 0.004 (0.008) |
| Dep. Variable Mean | \$2,287 | \$68,614 | \$3,148 | \$1,362 | 673 | 0.2085 | 0.048 | 0.038 |
| <u>Disaster Aid x Post x Medium</u> | 425 (327) | 598 (2,353) | 346 (385) | 240 (228) | 8.3** (3.8) | -0.0509*** (0.0170) | 0.009* (0.005) | 0.009** (0.004) |
| Dep. Variable Mean | \$2,532 | \$65,659 | \$3,429 | \$1,320 | 673 | 0.2058 | 0.059 | 0.042 |
| <u>Disaster Aid x Post x High</u> | -773** (307) | -19,479*** (2,414) | -289 (295) | -153 (94) | 1.3 (2.3) | 0.0001 (0.0109) | 0.004 (0.009) | 0.017** (0.008) |
| Dep. Variable Mean | \$2,611 | \$59,365 | \$2,527 | \$1,033 | 666 | 0.2059 | 0.067 | 0.059 |
| R-squared | 0.009 | 0.017 | 0.003 | 0.017 | 0.001 | 0.000 | 0.005 | 0.002 |
| Observations | 496,708 | 123,602 | 496,708 | 496,708 | 492,439 | 496,708 | 763,632 | 763,632 |

Notes: The table shows triple difference estimates for eight outcomes. Dependent variable means are for the last quarter before a tornado for the hit group. The debt variables are winsorized at 99%. Standard errors (in parentheses) are clustered by tornado: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

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.

The availability of disaster assistance has no overall impact on the Equifax Risk Score (Figure 5 panel E). Using the 95% confidence intervals we can rule out an effect of more than 3.5 points. Disaster assistance does prevent bill delinquency in the second and third years following a tornado (Figure 5 panel F). In the next section we show that this is entirely due to a reduction in bill delinquency for tornado victims who are credit constrained.

The triple difference migration event studies show an increase in block migration during

the first post-tornado year. More permanent migration is greatest in the most-damaged blocks (Table 3 column 8). The point estimate implies that average quarterly 3 year migration increased by 29% (p-value 0.034).

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 and home 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. There is an increase in auto debt only for credit-constrained individuals.

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, 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.

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 unlikely 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.¹⁷

¹⁷Our 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. [2021] find that financially constrained flooded residents have higher personal bankruptcy rates following Hurricane Harvey.

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 (1) | Home (Conditional) (2) | Auto (3) | 90 Day Delinquency (4) | 1 Quarter Move (5) | 3 Year Move (6) |
|----------------------------------|--------------------|------------------------------|---------------|------------------------------|--------------------------|-----------------------|
| Panel A: Available Credit | | | | | | |
| Low Available Credit | | | | | | |
| Disaster Aid x Post x Hit | -2 (53) | -180 (943) | 161** (59) | -0.0102 (0.0080) | 0.003* (0.002) | 0.002 (0.001) |
| Dependent Variable Mean | \$238 | \$55,950 | \$1,526 | 0.3150 | 0.052 | 0.044 |
| Observations | 152,278 | 13,439 | 152,278 | 152,278 | 246,950 | 246,950 |
| High Available Credit | | | | | | |
| Disaster Aid x Post x Hit | -226* (116) | -5,037*** (946) | 76 (90) | 0.0037 (0.0039) | 0.009*** (0.003) | 0.006*** (0.002) |
| Dependent Variable Mean | \$4,523 | \$67,655 | \$3,797 | 0.0391 | 0.045 | 0.032 |
| 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 (72) | 1,388 (1,444) | 256 (179) | -0.0416*** (0.0094) | -0.001 (0.002) | 0.001 (0.002) |
| Dependent Variable Mean | \$1,556 | \$57,003 | \$2,497 | 0.5249 | 0.071 | 0.061 |
| Observations | 161,520 | 21,380 | 161,520 | 161,520 | 246,339 | 246,339 |
| High Equifax Credit Score | | | | | | |
| Disaster Aid x Post x Hit | -17 (90) | -4,262*** (871) | 9 (77) | 0.0004 (0.0004) | 0.007** (0.003) | 0.006*** (0.002) |
| Dependent Variable Mean | \$2,090 | \$72,028 | \$2,747 | 0.0000 | 0.031 | 0.024 |
| Observations | 165,527 | 55,420 | 165,527 | 165,527 | 245,138 | 245,138 |

Notes: The table estimates the triple difference model 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 determined from 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. Standard errors (in parentheses) are clustered by tornado: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

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 (see Appendix Figure 3). 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). For example, 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.

6.2 Local Businesses

6.2.1 Business Growth and Employment

Figure 6 plots continuous damage triple difference point estimates and 95% confidence intervals for business survival, employment (ln number of employees), and sales (ln dollar sales). The red vertical line splits each panel into the pre- and post-tornado periods. There is no evidence of a pre-tornado trend for business survival or employment. The estimate three years before a tornado for sales is marginally statistically significant. Recall that the sales data are self-reported and likely to include the most measurement error. There is an increase in business survival for the first two post-tornado years for those establishments located in damaged blocks where residents have access to cash grants. The survival rate is lower in the third year. However, this reduction is temporary (see Appendix Figure 7).

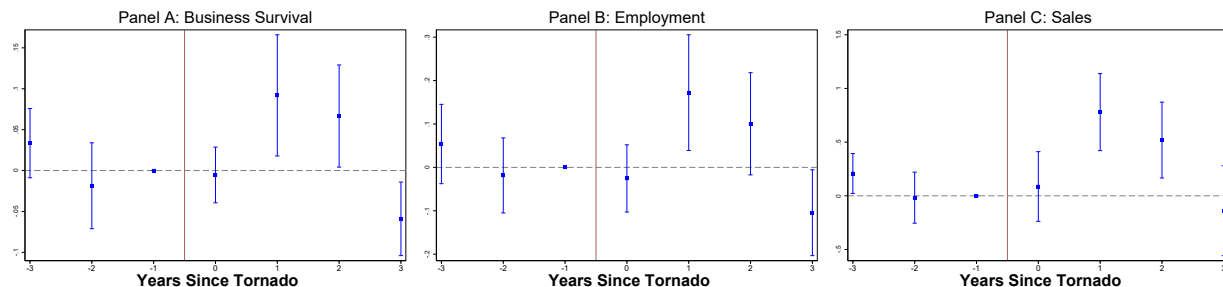


Figure 6 data sources: Infogroup Historic Business Database, National Weather Service, US Census.

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 0.038) and 14% more employees (p-value 0.063) 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 15% (p-value 0.032). 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: **Estimates for the Number of Business Establishments and Employees following a Tornado when Federal Disaster Assistance is Available**

| Model: | Triple Difference | | Difference-In-Difference | | | |
|-------------------------------------|--------------------|-------------------|--------------------------|----------------------|----------------------|----------------------|
| Dependent Variable: | Log | Log | Log(Establishments) | | Log(Employment) | |
| Tornado Type: | (Establishments) | (Employment) | Aid | No-Aid | Aid | No-Aid |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: Continuous Damage | | | | | | |
| <u>Disaster Aid x Post x Hit</u> | 0.050** (0.023) | 0.078* (0.040) | -0.023*** (0.005) | -0.063*** (0.020) | -0.043*** (0.009) | -0.090*** (0.038) |
| R-squared | 0.551 | 0.5543 | 0.542 | 0.537 | 0.531 | 0.526 |
| Observations | 159,743 | 159,743 | 140,643 | 19,100 | 140,643 | 19,100 |
| Panel B: Binned Damage | | | | | | |
| <u>Disaster Aid x Post x Low</u> | 0.228* (0.118) | 0.386* (0.199) | 0.046*** (0.015) | -0.163*** (0.056) | 0.066** (0.028) | -0.272*** (0.101) |
| <u>Disaster Aid x Post x Medium</u> | 0.150** (0.067) | 0.203 (0.134) | -0.047** (0.022) | -0.172** (0.072) | -0.091** (0.041) | -0.218* (0.132) |
| <u>Disaster Aid x Post x High</u> | 0.111 (0.094) | 0.345 (0.310) | -0.118*** (0.026) | -0.179*** (0.088) | -0.195*** (0.052) | -0.397* (0.220) |
| R-Squared | 0.551 | 0.5543 | .542 | .537 | .531 | .526 |
| Observations | 159,743 | 159,743 | 140,643 | 19,100 | 140,643 | 19,100 |

Notes: Standard errors (in parentheses) are clustered by tornado: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Data sources: Infogroup Historic Business Database, National Weather Service, US Census.

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 (4.6%, 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 4.7% decrease (p-value 0.033) in the number of establishments. The decrease is largest in the most-damaged blocks (-11.8%, 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 Table 9 shows triple difference survival 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

Table 6: Triple Difference Model Heterogeneity by Industry, Age, and Size

| | (1) | (2) |
|---|---------------------|--------------------|
| Dependent Variable: | Log(Establishments) | Log(Employment) |
| Panel A: Establishment Industry | | |
| <u>Non-Manufacturing</u> | | |
| Disaster Aid x Post x Hit | 0.050** (0.023) | 0.078* (0.041) |
| R-squared | 0.550 | 0.541 |
| <u>Manufacturing</u> | | |
| Disaster Aid x Post x Hit | -0.002 (0.003) | -0.007 (0.010) |
| R-squared | 0.551 | 0.541 |
| Panel B: Establishment Age | | |
| <u>New (1 year or less)</u> | | |
| Disaster Aid x Post x Hit | -0.003 (0.005) | -0.005 (0.008) |
| R-squared | 0.370 | 0.308 |
| <u>Existing (4 years or more)</u> | | |
| Disaster Aid x Post x Hit | 0.037** (0.018) | 0.063* (0.035) |
| R-squared | 0.534 | 0.528 |
| Panel C: Establishment Size | | |
| <u>Small (≤ 3 Employees)</u> | | |
| Disaster Aid x Post x Hit | 0.044*** (0.014) | 0.050** (0.020) |
| R-squared | 0.531 | 0.514 |
| <u>Large (≥ 7 Employees)</u> | | |
| Disaster Aid x Post x Hit | -0.005 (0.013) | -0.004 (0.028) |
| R-squared | 0.567 | 0.562 |

Notes: The table estimates the triple difference model separately based on establishment industry, age, and size. Standard errors (in parentheses) are clustered by tornado: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources: Infogroup Historic Business Database, National Weather Service, US Census.

different from zero (p-value 0.046).

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 8% (p-value 0.004)

and employees of 9% (p-value 0.016) 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. [2020]). Small businesses may be more vulnerable to the economic impact of tornadoes due to lower capital reserves and more difficulty accessing credit markets (e.g. Runyan [2006]; Basker and Miranda [2017]).

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 [2020] and use new auto loans as a proxy for new auto purchases. Approximately, 80% of new vehicle purchases use auto loans (Di Maggio et al. [2017]).

New quarterly auto purchases and balances both increase by more than 50% (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. 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 purchases (or not replace damaged vehicles). Individuals who are not credit constrained do not change their purchasing behavior based on access to disaster assistance.

6.2.4 Robustness

Appendix Table 16 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 the household finance results (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. 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. 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 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.¹⁸

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. Cash assistance to local residents could benefit businesses by increasing demand. Business establishments that rely on local demand benefit most (Table 6, panel A). Cash assistance could also improve establishment survival when the recipient is a business owner. Smaller establishments benefit most (Table 6, panel C). Smaller establishments are often home-based (SBA [2012]).

¹⁸Individuals 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.

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. For example, area roads following the Joplin tornado, the most costly 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 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 are among the first to document the impact of being directly hit by a destructive tornado on household finance and business survival. Tornadoes cause tremendous damage, but the spatial scale of a tornado is much smaller than that of a hurricane. How individuals and businesses manage the financial shock of a localized disaster is largely unknown.

We find that tornadoes, inclusive of any aid that follows, lead to a small, medium-term 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 9%. Our sample size of 34 tornadoes arguably allows for greater generalizability of our findings, as compared to studies in the literature that examine a single disaster event.

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 cash grants directly to residents, grants to repair public infrastructure, and SBA disaster loans. The availability of cash grants in a disaster region is not randomly assigned. However, conditional on regional availability, individual-level disaster expenses determine the level of cash assistance. 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.

There are four main household finance conclusions. First, we find that 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, 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.

Disaster assistance to impacted neighborhoods increases the survival rate of business establishments in these neighborhoods. The rate of establishment closure is twice as large in neighborhoods without cash assistance. The establishments most reliant on local demand benefit the most. Overall, our findings imply that disaster assistance policies will have the largest impact in preventing both individual financial distress and business closure when aid is targeted towards more vulnerable, credit-constrained disaster victims.

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