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

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Abstract

We estimate the causal effect of cash grants on household finance and business survival following a natural disaster. Disaster-affected individuals in severely damaged blocks with access to cash grants have 17% less credit card debt following the disaster than those without access to cash grants. Grants reduce bill delinquency for credit-constrained victims, and overall migration. The grants play a role in mitigating the effects of the shock to businesses, resulting in 9% more establishments and 12% more employees post-disaster in the average-damaged neighborhood where residents receive grants. These effects are concentrated among small non-manufacturing establishments that rely on local demand.

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1 Introduction

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

The US government has a long history of federal assistance following natural disasters. Cash assistance has been distributed to disaster victims immediately following natural disasters via a codified legal process since at least 1953. The implicit assumption is that savings, credit markets, and existing insurance (e.g. homeowners, unemployment, health) are insufficient to smooth the negative financial consequences of the natural disaster. In other words, the aim is to assist with "acts of God" that are of "such severity and magnitude that effective response is beyond the capacities of the state and the affected local governments and that the federal assistance is necessary" (Daniels and Trebilcock [2006]; Disaster Relief Act [1974]).

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

There are two goals of this study. First, we estimate the causal effect of federal cash grants on post-disaster financial and migration outcomes using credit bureau data. The credit bureau data are from the Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP) (Lee and van der Klaauw [2010]). The panel is a random 5% sample of US residents with a Social Security number conditional on having a credit history. The panel is quarterly, and importantly, we are able to follow individuals over time who move. We test whether cash grants substitute for personal debt and lead to a decrease in the level of debt incurred by disaster victims. We also test whether the cash grants reduce the likelihood of negative financial outcomes (e.g. debt delinquency) or affect migration from the disaster area.

In our setting, the natural disasters that cause uninsured property damage act as unexpected, one-time shocks to wealth. The disasters may also lead to job loss, but the lost income is temporary for most disaster victims, as the disasters rarely cause disabling injuries that impact human capital. The life cycle/permanent income hypothesis, for example, predicts that some disaster victims will borrow to smooth the wealth shock (e.g. Meghir and Pistaferri [2011]; Parker et al. [2013]).[1] Victims who receive cash grants are likely to borrow less, as the grants substitute for borrowing. However, for credit-constrained individuals, the receipt of a cash grant would not be expected to reduce debt by as much, if at all, since in the absence of the grant these individuals would have been less able to borrow. An advantage of our data is that we have good measures of whether an individual is likely to be credit constrained (Equifax Risk Score (TM) and available credit on revolving accounts). We test whether the cash grants lead to a differential effect on debt and overall financial wellbeing for credit constrained individuals.

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

The second goal of this study is to measure the effect of the cash grants on local businesses. We test whether cash grants act as a targeted stimulus to local businesses that are directly impacted by a natural disaster. Specifically, we examine whether, following the disaster, there is less of a reduction in the number of business establishments and employees in disaster-affected neighborhoods where individuals receive cash grants, as compared

[1]Disaster victims also have other potential ways to smooth the shock including reducing consumption and using savings.

to disaster-affected neighborhoods where individuals do not receive cash grants. The cash grants to individuals could lead to improved local establishment survival rates if there is an increase in spending on local goods and services. The cash grants that go to individuals who happen to be small business owners could also help keep businesses open.

The business data are from Infogroup's Historic Business Database, a proprietary database which seeks to include every US business establishment. We use the Infogroup Database to build an annual block-level enumeration of establishments in disaster-affected communities. The establishment panel includes yearly information on the age of the establishment, the number of employees, and an (6-digit) industry code. The data are well-suited to investigate how the potential effect of a cash stimulus varies by type of industry, and by establishment age and size.

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

The main identification challenge is that the decision of whether to provide cash grants is made following a disaster. Individual Assistance is only provided for a subset of Presidential Disaster Declarations. We deal with this endogeneity problem in two ways.

First, since we are concerned that cash grants may be more likely following larger, more damaging disasters, we limit our analysis of natural disasters to very large tornadoes that hit the US between 2002-2013. There are 34 tornadoes in our sample. All have Fujita (F) or Enhanced Fujita (EF) ratings of a 4 or 5. In our analysis we are able to precisely control for heterogeneity in block-level damage intensity. Detailed damage maps delineate the within path damage variation. Figure 1 shows the damage map for an EF5 tornado that hit Joplin, Missouri on May 22, 2011. The EF ratings are determined by National Weather Service (NWS) employees who conduct on the ground damage inspections. The NWS employees survey post-tornado damage and use an engineering model to relate the observed damage to estimated tornado wind speeds. We assign each damaged block a damage intensity equal to the area-weighted average of the EF ratings measured inside the block boundary.

Our preferred econometric models use the continuous damage variable to precisely control for the level of damage, or bin affected (or "hit") blocks into low, medium, and high damage groups. The binned model allows for non-linearities in how individuals and businesses respond to the disaster damage and cash assistance. The high damage group is for blocks with F/EF damage greater than 3 and corresponds to the NWS threshold for "severe damage" (National Weather Service [2014]). A damage level of 3 typically implies significant roof damage to buildings and is often associated with collapsed outer walls of built structures. It is important to emphasize that the NWS damage analysis carefully considers the type of structure and building materials, and the local building codes (Edwards et al. [2013]). This guards against the possibility of the tornado damage being overrated due to, for example, damage to pre-fabricated homes.

Still, there is a concern that cash assistance may be made available only when areas with more vulnerable populations are affected. According to the Federal Emergency Management Agency (FEMA), decision criteria for whether cash grants are provided include whether the affected individuals involve "special populations" such as the economically disadvantaged (McCarthy [2011]). In fact, we show that victims of tornadoes where cash grants are available ("cash tornadoes") are of lower socio-economic status than victims of tornadoes where cash assistance is not available ("no-cash tornadoes").

We address this second concern through a triple difference econometric model. Since tornado damage is very localized and the exact path of a tornado is not predictable, the geographic area in close proximity to the tornado should provide a good control group. Figure 1 shows our baseline comparison group in blue, those living 0.5 to 1.5 miles from the edge of the tornado damage path. We examine the pre- to post-tornado difference in financial outcomes for hit and nearby populations who are affected by tornadoes with and without federal cash assistance. The within tornado difference between the hit and nearby populations controls for selection differences for victims of tornadoes with and without cash assistance.

Figure 2 provides an illustrative example. Figure 2 plots mean credit card debt levels separately for the hit and nearby individuals for cash and no-cash tornadoes. The means are plotted with respect to the number of quarters since the tornado. The vertical line drawn at -1, indicates the last quarter before a tornado. It would be wrong to simply compare the hit areas for the tornadoes where victims did and did not receive cash assistance. Doing so would lead to a biased estimate for the causal effect of cash assistance due to the downward trend in credit card balances in hit areas that receive cash assistance. Differencing with the nearby groups provides a way of controlling for divergent pre-existing trends among the two groups hit by a tornado.

A potential concern with our estimation approach is that there are still differences in the levels of some variables at the time of the tornado. For example, Figure 2 shows that hit individuals without access to cash assistance have higher levels of credit card debt than do hit individuals with access to cash assistance. Existing debt balances could, for example, impact the ability to adjust to the financial shock of a tornado. If this were the case, then it would not be appropriate to assume that the credit card debt trends would evolve the same between the cash and no-cash groups following the tornado. We address this concern by analyzing a robustness subsample of tornadoes that more closely matches the average levels of key CCP variables just prior to a tornado. The estimation results are similar across the two samples.

We find that disaster-affected individuals in high damage blocks with access to cash grants have \$647 (17%) less in average quarterly credit card debt after the disaster relative to disaster-affected individuals without access to cash grants. The effect lasts for at least three years and is consistent with evidence on the persistence of revolving credit card debt (Telyukova [2013]). Access to credit markets impacts how tornado victims substitute cash grants for credit card debt. Nearly all of the reduction in credit card debt is attributable to less credit-constrained individuals. Consistent with the life cycle/permanent income hypothesis, credit-constrained individuals who have access to cash grants have lower rates of bill delinquency and are more likely to increase consumption on new vehicle purchases.

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

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

We find that the increase in the number of establishments is due to a higher survival rate for existing non-manufacturing establishments. There is no evidence that the cash grants affect the formation of new establishments. Overall, our findings indicate a mechanism whereby the cash stimulus to the local population most benefits businesses that rely on local demand.

We use our establishment employment results to estimate the cost per job retained or created in the disaster areas from the distribution of the cash grants. Our baseline estimate of $108 thousand per job considers the total dollar amount of the dispersed grants and the associated administrative costs (Brown and Earle [2017]). Our more comprehensive estimate of $75 thousand per job follows in the spirit of Bastian and Jones [2019] and Hendren [2016] and is inclusive of other program costs and fiscal externalities.

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

2 Background and Data

2.1 Tornado Data

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

Three criteria determine whether a tornado is included in our sample. First, the tornado occurs from 2002-2013 so as to match the period covered by our individual and business financial data. Second, the tornado must have a Fujita (F) or Enhanced Fujita (EF) rating of either a 4 or 5.[2] Third, the tornado must have a high quality damage path map, generally created by the National Weather Service (NWS), that demarcates areas of the tornado path that suffered different levels of damage. Appendix Section 1.1 provides details on how the NWS creates the damage maps using on the ground observations and a detailed engineering model that takes into account the strength of the damaged materials and local building codes.

Thirty-five tornadoes satisfy the three criteria. Our sample includes 34 tornadoes, as one tornado violates the pre-trend assumption of our sample design. We provide more details when we discuss the econometric model in Section 3. Appendix Table 1 lists all 35 tornadoes.

2.2 Public Disaster Assistance

The Presidential Disaster Declaration (PDD) system is a formalized process to request and receive federal assistance following large natural disasters. A governor of a US state that experiences a natural disaster must request a PDD in a written letter to their FEMA regional office. Disaster declarations occur at the county-level. The letter must contain a list of proposed counties and preliminary damage estimates. The regional office forwards a recommendation for whether to grant the request and the type of federal assistance (if any)

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

that should be offered to FEMA headquarters. FEMA headquarters then makes an official recommendation to the US president, who decides whether or not to grant the request.

A PDD opens the door to three major types of assistance. The first type is Individual Assistance and is the source of the cash grants we study in this paper. There are two steps to qualify for cash grants. First, Individual Assistance must be available to disaster-affected residents in the county. Second, the exact level of assistance is determined via an application that documents incurred damage (e.g. structural damage to the home) and expenses (e.g. temporary housing and relocation) caused by the disaster. The maximum amount of cash assistance was $30,200 in 2010 and is indexed to inflation (Fed [2010]).

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

Appendix Table 2 provides summary information for the tornadoes in our sample. Twenty-five tornadoes are part of disaster declarations where individuals received cash grants.[3] Tornadoes with cash assistance are part of larger state-level disasters as measured by the percent of the state's counties included in the PDD. Cash assistance tornadoes also impact a larger number of blocks and cause more block-level damage. The average number of damaged blocks per tornado for cash assistance tornadoes is 381, while it is 58 for tornadoes without cash assistance. The average tornado damage per block is estimated to be $1.39 and $0.84 million for cash and no-cash tornadoes, respectively. Moreover, 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. The two-way presidential vote share of the losing party is 1.1 percentage points higher for tornadoes that

[3]The Ferguson, MO tornado crosses state lines. We classify the Missouri tornado victims as receiving cash assistance and the Illinois tornado victims as not receiving cash assistance.

do not receive cash assistance.[4] The difference in the overall damage and number of persons injured between cash and no-cash tornadoes motivates our preferred econometric models that control for block-level damage.

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

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

Small Business Administration (SBA) disaster loans are the second type of disaster assistance affiliated with a PDD. Disaster-affected individuals and businesses can request subsidized SBA disaster loans. However, it is important to emphasize that SBA disaster loans are routinely available to residents in counties that are not part of a PDD. There are several ways to make SBA disaster loans available, including: a Governor Certification Declaration for businesses and an Administrative Declaration for individuals (SBA [2015]). Overall, SBA loans are available in 99% of the hit blocks in our sample (regardless of PDD designation). Appendix Table 3 shows summary statistics for the level of SBA disaster loans awarded to the hit and nearby ZIP Codes following a tornado.

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

[4]We calculate the average share of the Democratic and Republican vote the losing party receives in the 1996-2004 presidential elections for each PDD county (e.g. Reeves [2011]).

[5]FEMA typically discloses either disaster-level or county-level information. It took over four years to receive the ZIP-level data after submitting our FOIA request. One reason is that we appealed FEMA's decision not to release the block-level information.

of public buildings. A cost sharing rule requires that state and local authorities pay at least 25% of the damage costs. Public Assistance is provided for 22 of the 25 cash tornadoes and three of nine no-cash tornadoes in our main sample.

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

First, Public Assistance targets the repair of transportation infrastructure. If Public Assistance facilitates the opening of damaged roadways then this could contribute to greater economic activity in the damaged neighborhood. However, even the most destructive tornadoes tend to only directly hit a small fraction of a community. For example, the 2011 EF5 Joplin, MO tornado is the deadliest US tornado since reliable record keeping began in 1950 (National Weather Service [2018]). Yet the brunt of the tornado hit just 10% of a single ZIP Code (see Appendix Figure 1). Moreover, area roads were serviceable following the tornado so that residents and emergency personnel could immediately access the hit blocks. 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. town hall). If this occurs, then these workers may not be laid off or relocated to a different block. However, our employment findings are insensitive to the inclusion of public sector employees. Model estimates that exclude public sector employees are very similar to our main estimates that encompass all sectors and industries. Moreover, when we limit the data to only public sector employees, we estimate a very small change in employment that is not statistically different from zero. Finally, the disaggregated industry results support an economic channel whereby cash provided to the local population improves business outcomes for those establishments most reliant on local demand.

2.3 Credit and Debt Information

We use individual-level credit and debt information from the Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP) (Lee and van der Klaauw [2010]). Equifax, one of several large consumer credit repository and credit scoring companies in the US, is the source of the credit and debt data in the CCP. The panel is 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. Thus, the sample consists of a random sample of the population that has a social security number conditional on having a credit history. The CCP has quarterly observations and runs from 1999Q1 to the present. Individuals in the sample are anonymous, but can be linked from quarter to quarter with an internal ID.

Consumer credit account information is divided into five main types: home loans, auto loans, credit card accounts, consumer finance loans, and student loans. Home loan information separately tracks first mortgages, home equity loans, and home equity lines of credit. Bank and retail card accounts (i.e. credit cards) cover all types of issuers: banks, bankcard companies, national credit card companies, credit unions, and savings & loan associations, as well as department store and other retail credit cards. Consumer finance loans are a type of subprime loan typically used by borrowers with lower credit scores. We do not consider student loan debt in the paper because the way in which the data are recorded changed during our study period (Brown et al. [2014]).

The CCP includes the number of accounts for each type of debt, the total balance and indicators for whether the individual is behind on payment for each type of debt, and indicators for foreclosure and bankruptcy. The panel also includes the age, Census block of residence, and Equifax Risk Score for each individual.[6] 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. Our sample consists only of individuals that are 21 and older in the quarter of the tornado, since individuals do not

[6] We use the "primary" CCP sample that does not include other linked household members. All dollar denominated variables are in real 2010 dollars. We winsorize the 99th percentile of all dollar denominated stock variables (balances and credit limits) in each quarter so that our estimates are not driven by the presence of extremely large debt balances or credit limits. SBA loans are not reported to Equifax. The Equifax Risk Score is a trademarked measure of consumer credit risk and ranges from 280-850. A higher score indicates a higher measure of creditworthiness.

typically enter the CCP until they are 18 years old and we require them to be in the sample for 12 quarters prior to the tornado. Using the CCP's individual identifiers, we can track all individuals even if they move away from the tornado-affected area or were living elsewhere for some portion of the pre-tornado period.

Table 1 shows financial and socioeconomic information for individuals in our sample. Individuals hit by tornadoes where cash assistance is available are economically worse off than individuals hit by tornadoes where cash assistance is not available. A comparison between columns (2) and (5) shows that individuals hit by a tornado with cash assistance have lower median income, a higher poverty rate, lower home ownership, and own less valuable homes (conditional on owning a home). Overall home debt is also much lower ($54,104 vs. $77,889). The lower total debt is due largely to lower home debt.

The economic information in Table 1 is consistent with FEMA using the economic status of residents hit by the tornado as part of the calculation when deciding to award cash assistance (McCarthy [2011]). The share of the population that is African American, and the share that is at least 65 years of age are also higher in areas hit by tornadoes that receive cash grants.

2.4 Business Data

We use business establishment data from the Infogroup's Historic Business Database (Serrato and Zidar [2016]). The Infogroup database aims to include longitudinal establishment-level data on all business establishments in the US. The database covers approximately 35 million establishments each year for the years 1997 to 2017.[7] 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. Our unit of analysis is the census block. We aggregate Infogroup establishment-level data to the census block, and match the block-level establishment data with the tornado blocks.[8]

[7]The Infogroup compiles this information by first identifying business establishments through numerous sources, including: county-level public sources, utility connects and disconnects, real estate tax assessor data, yellow and white pages, and web research. Infogroup then calls every establishment in the US every year. An independent audit found the database similar to, and on many dimensions, of higher quality than other private establishment-level datasets such as the National Establishment Time-Series dataset (College of Information Science & Technology at the University of Nebraska [2017]).

[8]The database contains establishment identifiers that would allow us to estimate an establishment-level (rather than block-level) econometric model. We prefer the block-level analysis because it matches the level

Table 1 panel C shows block-level summary statistics for business establishments the year before a block is affected by a tornado. 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. The percent of employment at manufacturing establishments is similar in areas hit by cash and no-cash tornadoes (5% and 4%, respectively).

3 Empirical Specification

Our goal is to estimate the causal effect of disaster cash grants on household finance and migration, and business establishment survival and employment. We use a triple difference (DDD) empirical strategy. The triple difference estimates can be thought of as taking the difference between two difference-in-differences (DD) estimates, where we separately estimate the effect of being hit by a tornado that does and does not result in post-disaster cash assistance.

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

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

We use a triple difference model to isolate the role of cash grants on post-disaster outcomes. We compare the pre- to post-tornado difference in outcomes (e.g. credit card debt) for hit and nearby populations who are affected by cash and no-cash tornadoes. The within tornado difference between the hit and nearby populations controls for selection differences

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.

between victims of cash and no-cash tornadoes. Figure 2, discussed in the Introduction, highlights the likely bias due to differing pre-trends that would result if we did not use the nearby population as a second control group.

Our preferred sample mostly satisfies the triple-difference criteria of common pre-trends for our key dependent variables (see next section). The underlying assumption is that the dependent variable trends (difference in hit minus nearby means) for the cash tornado group would have continued to evolve similarly to those for the no-cash group had individuals hit by a cash tornado not received cash assistance.

One potential concern is that differences in the levels of the dependent variables between individuals hit by cash and no-cash tornadoes could bias our estimate if there is heterogeneity in the cash grant treatment effect. For example, individuals hit by a cash tornado have, on average, lower Equifax Risk Scores than do individuals hit by no-cash tornadoes (see Table 1). This implies that cash tornado victims are in a slightly worse overall financial position at the time of the shock. As such, cash tornado victims may be less able to cope with the financial shock in the absence of cash grants. We address this concern by analyzing a robustness sample (described at the end of this section) that more closely matches the average levels of key CCP variables just prior to a tornado.

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

We estimate the following equation,

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

where $y_{i,t}$ is a credit outcome for individual i in quarter t. $Cash_i$ is a binary variable indicating whether individual i lived in an area either hit by or nearby to a tornado that received cash assistance. Hit_i is a binary variable indicating whether individual i lived in a tornado-damaged block at the time of the tornado. $Post_{i,t}$ is a binary variable indicating the post-tornado period (any of the 12 quarters following the quarter of the tornado). δ is our coefficient of interest and represents the effect on credit outcomes for individuals living in hit blocks where cash assistance is available following a tornado, relative to those individuals who just missed being hit by the tornado, and as compared to individuals living in hit blocks with no cash assistance. α_i is an individual fixed effect, γ_t is a quarter-by-year fixed effect, and $\epsilon_{i,t}$ is an error term. We cluster the standard errors by tornado when estimating the model.[9]

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

Our third triple difference model is a binned damage model that replaces Hit_i with a vector of three binary variables indicating low, medium, or high damage. The advantage of this model over the continuous damage model is that it allows for non-linearities in how individuals respond to disaster damage and cash assistance. We classify the block as *low* damage if the mean EF is less than 1, *medium* damage if the mean EF is greater than or equal to 1 but less than 3, and *high* damage if the mean EF is 3 or higher. A damage level of 3 on the original Fujita scale corresponds to "severe damage" (National Weather Service [2014]). A damage level of 3 on the Enhanced Fujita scale implies that the roofs for most types of buildings will be severely damaged and the outer walls of the building may have collapsed. However, it is important to emphasize that the EF damage analysis is comprehensive and carefully considers the type of structure, the type of building materials, and the local building codes (Edwards et al. [2013]).

We also estimate an event study version of the continuous damage model which allows us to observe temporal dynamics and to examine differences in pre-existing time trends. When we estimate the event study model we replace the $Post_{i,t}$ variable with a set of binary variables that indicate the number of years the observation is either before or after the tornado. The year before the tornado is excluded from the model and serves as the reference time period. We drop the quarter of the tornado from the panel before running the model.[10]

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

[10] 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

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

The three tornado and tornado mapping criteria discussed in Section 2.1 give us a sample of 35 tornadoes. Our preferred sample includes 34 tornadoes. One of the tornadoes, the Wayne, NE tornado (see Appendix Table 1), exhibits pretrends for our business outcomes (see Appendix Figure 3). We drop this tornado from our preferred sample. We show estimation results for the 35 tornado sample in the appendix. There is little difference between the two samples for the individual financial and migration outcomes. Not surprisingly, there are some differences in the business results. We highlight these differences in the discussion of the results.

Finally, we test the sensitivity of our main estimates by estimating a robustness subsample that more closely matches the average levels of the eight debt and financial health CCP variables in Table 1 for the hit cash and hit no-cash groups. We set the robustness sample using two steps. First, the robustness sample includes all individuals from the nine no-cash tornadoes in our main sample. Second, the robustness sample includes observations from nine of the 25 cash tornadoes. The underlying logic is that we could consider our main sample as a collection of random tornado events. We select a robustness sample, balanced in the number of tornadoes, that best matches the levels of the key CCP variables at the time of the tornado between hit individuals in the cash and no-cash tornado groups. Specifically, we minimize the sum of the absolute deviations in z-scores for the eight debt and financial health variables in Table 1. There are 2,042,975 possible subsamples.

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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.

4 Results

4.1 Household Finance and Migration

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

4.1.1 Debt

Table 2 presents results from our three triple difference models. Panel A displays the pooled model estimates. Credit card debt is a common type of short-term debt that victims of tornadoes could use to smooth a financial shock. Victims of cash tornadoes have a statistically insignificant \$375 (10%) reduction in their average quarterly credit card balance (column 1) over the three years following a tornado (probability value 0.286). Panel B shows results from the continuous damage model that precisely controls for block-level tornado damage. We again estimate an imprecise, negative relationship between cash assistance and credit card debt. Panel C estimates the binned damage model that allows for a non-linear effect on cash assistance. There is a statistically significant \$647 (17%) reduction in high damage blocks (probability value 0.084). Recall that the high damage blocks are those where the tornado causes "severe damage" (National Weather Service [2014]).

Total home debt, including both mortgages and home equity, decreases by \$7,713 (5%) in the continuous damage model (probability value 0.002) for the typical victim who has a home loan continuously in the 12 quarters before the tornado.[11] The reduction is much larger for individuals in severely damaged blocks who, on average, reduce their mortgage debt by about \$40,000 (probability value 0.000).

Difference-in-differences (DD) results for the debt outcomes are presented in Appendix

[11] Calculated as -\$4,285 (panel B column 2) multiplied by 1.8 (average block damage).

Table 4. There is a reduction in home debt for homeowners hit by cash tornadoes in the high damage blocks (-\$6,225, probability value 0.029). However, an increase in home debt for homeowners hit by tornadoes without cash assistance (\$37,798, probability value 0.060) is driving the triple difference result. The DD model estimates help to highlight that the impact of the cash grants estimated in our triple difference model is coming from *a drop in* debt for those disaster victims with access to cash grants, and *an increase in* debt for those disaster victims without access to grants. For example, the *difference* in cash assistance for an individual living in a severely damaged block could be as large as \$60,000. Recall that the maximum amount of assistance was \$30,200 in 2010 (Fed [2010]).

We also estimate two additional DD models so as to better understand what explains the reduction in mortgage debt for hit residents with access to cash grants (Appendix Table 7). First, we estimate the same DD model as above, except that we separately consider hit residents who either move from or stay in the same block following the tornado. The increase in home debt is larger for residents without access to cash grants who move rather than stay (\$14,839 versus -\$2,081 in the continuous damage model for the block with average damage, and \$73,971 versus \$14,134 in the severely damaged blocks using the binned model). The reduction in home debt is similar for victims of cash tornadoes regardless of whether they move. Second, we estimate a model that separately considers mortgage and home equity debt. The increase in home debt for victims of no-cash tornadoes is due to an increase in first mortgage debt. These results suggest that when residents hit by no-cash tornadoes move, that they purchase new homes and dramatically increase their mortgage debt (relative to residents hit by cash tornadoes). However, we are cautious in our interpretation because we do not have an economic model that links migration and home debt. Debt levels and migration are different outcomes that are both affected by cash grants. The point estimates in these descriptive regressions are also imprecise.

The estimated change in auto debt in Table 2 is imprecise. The DD model results in Appendix Table 4 show that auto debt increases in more damaged blocks regardless of cash assistance.

Figure 3 plots yearly event study estimates for six outcomes using the continuous model. The yearly point estimates for each outcome are marked by squares. The vertical lines with the hash marks represent the 95% confidence interval. The estimates are relative to the year before the tornado. The vertical red line shows the quarter of the tornado (which is dropped from the sample). There is no evidence of any pre-tornado trend for credit card, home, and auto debt (panels A-C). The immediate and persistent reduction in home debt is consistent with the triple difference result in Table 2 column 2 panel B.

Consumer finance loans is the other type of short-term debt, in addition to credit cards, that we can measure in the CCP data. We do not emphasize these results because this is the one outcome where there is clear evidence for pre-existing trends (panel D). Individuals living in areas that are later hit by a cash tornado are increasing their consumer finance loan debt relative to those who are later hit by a no-cash tornado.

4.1.2 Financial Wellbeing

Table 2 columns 4-5 show model estimates for the Equifax Risk Score and 90 day delinquency, two measures of financial health. Overall, the effect on credit score and 90-day delinquency are economically small and statistically insignificant. The exception is for victims in medium damage blocks. Access to cash assistance leads to a 25% decrease (probability value 0.006) in the likelihood of having at least one credit account that is 90 or more days delinquent, and an 1.2% (probability value 0.041) increase in the Equifax Risk Score.

Figure 3 shows the event study analysis for the financial health measures in panels E and F. There is no evidence of any difference in the pre-tornado trends between individuals hit by cash and no-cash tornadoes. The most striking finding is the reduction in the likelihood of having an account that is 90 days delinquent beginning one year after the tornado. The continuous damage triple difference model masked this delayed effect. The delay could be partially mechanical. 90 day delinquency is an outcome measured with a time lag. The delay may also reflect the fact that the worst financial impacts accumulate over time. In Section 4.1.4 we show that the decrease in delinquency rates is completely attributable to more credit constrained individuals.[12]

[12]The CCP data include a quarterly foreclosure variable that indicates whether an individual had a foreclosure in the past seven years. Unfortunately, the fact that new quarterly foreclosures are very uncommon and that we need to use an inexact proxy to identify changes in foreclosure, combined with our sample size, together prevent us from examining how cash assistance affects foreclosure rates. Appendix Section 1.3.2 provides more details.

4.1.3 Migration

Table 3 shows triple difference linear probability model estimates for whether an individual moves out of the Census block or county of residence. Columns (1) and (2) define migration as leaving the block (or county) of residence for at least one quarter. Columns (3) and (4) define migration as leaving the block (or county) of residence for at least three years.

We construct the migration panel differently than the main household finance panel. Our goal is to estimate changes in out-migration rates from the block and county. As such, for each quarter we estimate the fraction of individuals who no longer live in the same block or county in the following quarter (and in column (3) and (4) who do not return for three years). This is different from our main household finance panel because the composition of the sample differs from quarter to quarter. Overall, the average block out-migration rate in the quarter before a tornado is 5.7% for cash assistance tornadoes and 7.5% for no-cash tornadoes (Table 1 panel A).

We estimate a 0.65 percentage point or 12% increase (probability value 0.033) in the one quarter block migration rate in the continuous damage model for individuals who have access to cash grants. The percentage point effect is calculated as 0.0036 (panel B column 1) times the average tornado block-level damage of 1.8. The point estimates for the binned damage model are all positive, but imprecisely estimated. Cash grants do not impact more permanent (three year) migration in the pooled or continuous models. However, there is a reduction in more permanent out-migration among residents in the high damage blocks who have access to cash grants. Residents in high damage blocks are more than twice as likely to stay in the neighborhood when they have access to cash grants, as compared to the pre-tornado migration rate (probability values 0.000 and 0.040, respectively). One explanation for these findings is that access to cash grants allows individuals to temporarily move residences while repairs to their homes are completed. At the same time, cash grants reduces the likelihood of a permanent move from the neighborhood.

Figure 4 shows the block migration event study analysis for a temporary and permanent move. The figure shows that access to cash grants increases the one quarter block migration rate during the first post-tornado year. The overall effect on the temporary out-migration rate observed in the continuous damage triple difference model is completely due to the impact during the first post-tornado year. There is no impact on permanent migration when we examine the entire sample.

4.1.4 Heterogeneity by Access to Credit Markets

The LCPIH predicts that the tornado will have a relatively small effect on current consumption, given that the wealth and income shocks are temporary for most tornado victims. Tornado victims can smooth the shock by borrowing from past time periods (e.g. withdrawing savings), or from future time periods (e.g. new debt). However for those without sufficient savings, the predictions of the LCPIH, or any intertemporal model, hinge on access to credit markets.

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

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

Table 4 explores how cash grants impact debt, financial health, and migration based on the likelihood a victim is credit constrained, and by age. We consider two proxies for whether an individual is credit constrained: Equifax Risk Score and available credit. We define available credit as the difference between total credit card debt and the total credit card debt limit. We separately divide our sample into thirds based on age, Equifax Risk Score, and available credit, and compare outcomes for the lowest third to the highest third using the continuous damage model (Gelman and Park [2008]; Parker et al. [2013]). The lower and upper tercile cutoffs for each of the variables are as follows: 40 and 58 for age, \$149 and \$11,364 for available credit, and 618 and 759 for Equifax Risk Score. The credit card and credit score cutoffs are based on averages across the 12 pre-tornado quarters, while the age is from the quarter before the tornado.

Cash grants lead to a larger reduction in credit card debt for less credit-constrained tornado victims. Individuals in severely damaged blocks who have high available credit at the time of the tornado reduce their quarterly credit card debt by \$1220 (calculated as the product of -$305 and EF damage level 4). We estimate an economically small increase of $104 for those with low available credit. The coefficients are statistically different at the 0.000 probability level. Cash grants lead younger individuals to reduce their credit card debt by more than older individuals, but the point estimates are too imprecise to reject the null hypothesis that the estimates are equivalent.

Credit constrained individuals with access to cash grants are less likely to forgo paying a bill. A hit resident with a low credit score is 14% less likely (probability value 0.000) to have a 90 day delinquency (calculated as -0.042 times 1.8 divided by 0.525). There is no effect on tornado victims who are less credit constrained. The difference between the estimated delinquency rates for low and high credit constrained tornado victims is statistically significant using both our available credit and credit score proxies.[13]

4.1.5 Robustness

Appendix Tables 12 - 17 and Appendix Figures 6 - 9 show robustness analysis for our triple difference model for each of our household finance and migration outcomes. We estimate two alternative samples for each outcome. First, we show results from the balanced tornado sample, selected to minimize the difference in the CCP debt and financial variables for individuals hit by cash and no-cash tornadoes. Second, we estimate the model on the full sample that includes the Wayne, NE tornado that is dropped from our preferred sample due to differing pre-trends. Overall, the estimates are qualitatively similar to our main sample. We limit our discussion in the text to a short summary of the balanced tornado sample.

Appendix Table 11 shows that the mean levels of the CCP and Census variables for the hit groups in the balanced tornado sample are much more similar, as compared to in the main sample. The Equifax Risk Score is a composite score that represents overall financial risk. Individuals hit by cash and no-cash tornadoes have nearly identical pre-tornado Equifax Risk Scores. The balanced sample model estimates are less precise, but suggest that there is limited heterogeneity in the response to cash assistance. The most striking differences between the balanced sample and our preferred model are that the estimates for the reduction

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

in home debt and the increase in the propensity to migrate are both larger in the balanced sample. Our heterogeneity estimates show that the larger reduction in home debt is partially due to larger reductions for older and low credit score individuals. Still, all of the point estimates from our main sample are within the balanced sample confidence intervals.

4.2 Local Businesses

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

Federal cash grant assistance to individuals can aid local businesses in two important ways. First, when tornado-affected individuals receive cash assistance a portion is spent locally increasing revenues for local establishments. Damaged business establishments may disproportionately benefit from the increased demand for their goods and services following a disaster, relative to nearby undamaged establishments.[14] Second, cash assistance to individuals that happen to be small business owners may positively affect establishment outcomes. In 2016, 47 percent of establishments employed four or fewer people (SUSB [2018]). The median establishment size in our sample is four. Around half of all establishments are operated out of a home.[15]

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

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

4.2.1 Business Growth and Employment

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

The left side of Figure 5 plots the trends separately for tornadoes where affected residents were able to access cash grants (circles) and where no cash grants were distributed (triangles). Three facts emerge. First, trends for the two outcomes in the years leading up to a tornado are roughly parallel for the hit and nearby establishments affected by a tornado where no cash grants were distributed (dashed lines). The same is true for hit and nearby establishments of cash tornadoes (solid lines). Second, the trends in establishment outcomes are increasing slightly in areas that are later hit by a cash tornado. The trends are flatter for establishments which are later affected by a no-cash tornado. Third, in the four years after a tornado, the trend in the number of establishments and employees is flat for establishments affected by tornadoes where residents received cash grants. During the same post-tornado period there is a reduction in the number of establishments and employees in areas hit by tornadoes where residents did not receive cash assistance. The reduction is greatest in neighborhoods hit by no-cash tornadoes.

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

Table 5 columns (1) and (2) show triple difference estimates of the effect of cash grants on the number of establishments and employment. Establishments in damaged blocks where residents have access to cash grants benefit economically. We estimate that there are 9% more establishments (probability value 0.047) and 12% more employees (probability value

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

Table 5 columns (3)-(6) show DD model estimates separately for cash and no-cash tornadoes. There are two key patterns in the binned damage level DD model estimates. First, the greater the block-level damage, the worse the establishment outcomes post-tornado. For example, column (3) shows that there is a slight increase (4.7%, probability value 0.002) in the number of establishments located in low damage blocks where there is cash assistance, relative to establishments in the nearby neighborhoods that are not hit by the tornado. In medium damage blocks there is a 6.0% decrease (probability value 0.015) in the number of establishments. The decrease is largest in the most-damaged blocks (-12.7%, probability value 0.000). Second, the reduction in the number of establishments and the level of employment is consistently greater at each damage level for establishments hit by no-cash tornadoes, as compared to establishments and employment in blocks hit by a cash tornado.

4.2.2 Heterogeneity by Industry, Age, and Size

Table 6 presents estimation results from our triple difference continuous damage model that examine how the treatment effects vary by establishment industry, age, and size. The goal is to shed light on how cash assistance to the local population affects business survival and growth.

Panel A of Table 6 estimates the model separately for manufacturing and non-manufacturing establishments. We classify each establishment as manufacturing or non-manufacturing using the two digit SIC. The manufacturing employment share is approximately 5% in our sample and is similar in blocks hit by cash and no-cash tornadoes (Table 1 panel C). We view manufacturing as a proxy for whether an establishment is likely to rely on a local or non-local consumer base. Manufacturing establishments are more likely to produce goods for consumers outside the local economy. By contrast, non-manufacturing establishments, which include the retail, service, and construction industries, are more likely to rely on local demand.

The positive effect that the cash grants have on the number of establishments and employees is completely attributable to non-manufacturing, local service-driven establishments. The estimated effect on manufacturing establishments and manufacturing employment is close to zero and not statistically significant. The estimates for non-manufacturing establishments are more than an order of magnitude larger than the manufacturing estimates, and are nearly identical to full sample estimates in Table 5. Appendix Tables 8 and 9 show triple difference estimates for establishments in each of the "1 digit" industries that we pool together in the non-manufacturing category. The largest impacts are observed in the service, construction, and retail sectors. The estimate for public sector employment is small and not statistically different from zero.

Panel B of Table 6 provides evidence that the positive effects on business establishments are due to an improvement in the survival rate of existing businesses, and are not driven by growth in entrepreneurship (new business establishments). We estimate our model separately for establishments in operation for one year or less and for establishments that have been open for at least four years. We estimate a fairly precise zero for new establishments. The effect on existing establishments is positive, an order of magnitude larger, and statistically different from zero (probability value 0.059).

Panel C of Table 6 divides establishments into small and large-sized establishments based on the size distribution in our sample (Appendix Figure 5). Roughly one-third of the establishments employ three or fewer employees, while one-third employ greater than seven employees. We estimate an increase in the number of establishments of 7.7% (probability value 0.005) and employees of 8.6% (probability value 0.019) at very small establishments at the mean damaged block. The estimates for larger establishments are an order of magnitude smaller, close to zero, and not statistically significant. We interpret the size of establishment results as evidence that smaller establishments are more vulnerable to the economic shock caused by the tornado, and thus benefit more when cash grants are provided to the local population. This finding is consistent with other recent research on the vulnerability of small businesses (e.g. Cole and Sokolyk [2016]; Greenstone et al. [2015]).[16]

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

4.2.3 New Purchases and Sales

We argue that the cash grants to individuals after a natural disaster act as a local stimulus and improves business survival. The previous section shows evidence that improvements in establishment survival rates in cash grant neighborhoods are driven by small, existing, local service and sales-oriented establishments. Ideally, we would be able to link individual-level purchases (including the home address) with the establishment location of the purchase. We are not aware of any publicly available data that provides this linkage. Instead, we separately show that new vehicle purchases are greater for individuals hit by cash tornadoes, and that sales are larger for local service and sales-oriented establishments in these same neighborhoods. Large tornadoes often destroy motor vehicles. Auto purchase is a consumption response we can measure using the CCP data. We follow Ganong and Noel [Forthcoming] and use new auto loans as a proxy for new auto purchases. Approximately, 80% of new vehicle purchases use auto loans (Di Maggio et al. [2017]). The sales data are collected when Infogroup contacts each establishment.

Figure 6 shows the hit minus nearby difference for new quarterly auto loans and log sales for retail and service establishments. We plot the differences separately for cash and no-cash tornadoes. The data are residualized (controlling for calendar time) and plotted relative to when a tornado hits. The left panel shows that the difference in new vehicle loans for hit and nearby individuals oscillates around zero for the entire time period with two exceptions: an increase for the cash group right after the tornado, and a year-long decrease for the no cash group beginning two quarters post-tornado. The right panel shows a pattern in retail and service establishment sales that mirrors the survival and employment plots in Figure 5. Sales plummet for hit retail and service establishments in neighborhoods with no cash assistance. Appendix Table 10 shows our regression estimates for the number and dollar amount of new auto loan originations, and for retail and service establishment sales, using our triple difference models. New quarterly auto purchases and balances both increase by more than 50% (probability values 0.000 and 0.000, respectively) for individuals in the most-damaged blocks. The triple difference sales regression results are greater for retail and service establishments located in damaged neighborhoods with cash assistance, and follow the same pattern as the establishment survival estimates in Table 5.

Consistent with the LCPIH, Table 7 shows that low available credit and low credit score individuals increase their purchases when they have access to cash grants. These groups are the most credit-constrained, and in the absence of the cash grants, are more likely to reduce consumption. Individuals who are not credit-constrained do not change their consumption based on access to cash assistance.

4.2.4 Robustness

Appendix Tables 18 - 21 show business establishment survival and employment regression estimates for the balanced tornado and 35 tornado robustness samples. The establishment survival and employment estimates for the balanced tornado sample are very similar to those in our main sample. The coefficient estimates from the 35 tornado sample are somewhat smaller in magnitude. The difference is most stark in the no-cash tornado DD model estimates. When we include the Wayne, NE tornado in the sample (a no-cash tornado) the estimates are much less precise. In our view, the reason for this result is the differing business pre-tornado trends for the Wayne, NE tornado.

5 Discussion

The goal of the IA cash grant program is to provide assistance to individuals who incur direct expenses from a natural disaster. At the same time, we show that the cash grants increase business survival and lead to greater employment in damaged tornado blocks where individuals receive cash grants. In this section, we calculate a rough measure for the cost of each job retained or created by the cash grants.[17]

Our baseline calculation follows Brown and Earle [2017] who use administrative data to examine two large SBA (non-disaster) business loan programs. Brown and Earle [2017] estimate the causal effect of SBA business loans on employment and calculate the cost per created job. The authors consider two costs (loan defaults and administrative expenses) and report the cost of a job created as $25,450 (2010$) using the employment estimate from

[17] Figure 5 shows a dramatic decrease in the number of employees in damaged blocks that did not receive cash grants (rather than a disproportionate increase in blocks where individuals received grants). The raw trends in the figure, along with the separate model estimates for new versus existing business establishments (Table 6 panel B), suggest that the difference in jobs between cash and no-cash disaster blocks is largely due to "retained" jobs rather than "new" jobs. Unfortunately, the data do not allow us to distinguish between newly created jobs and the retention of existing jobs. We follow the literature and streamline the exposition by (hereafter) referring to our estimates as jobs created.

their preferred model. The authors are careful to emphasize that their calculation does not include, among other considerations, the effect of increased employment on the government budget from tax revenue and reduced unemployment benefits.

We estimate that the cash grants created 963 jobs in the damaged cash grant blocks using our preferred model (Table 6 panel C). We calculate these job figures using employment at establishments with three or fewer employees. We then multiply the jobs created point estimate in our preferred model by the number of jobs at small establishments in the damaged cash grant blocks in the year before a tornado.

Table 8 presents estimates of the cost per job created from the cash grants. In panel A, we use the total amount of cash assistance allocated throughout all of the disaster counties in the state to calculate the cost per job. In panel B, we only consider the cost of the cash grants that went to ZIP codes hit by (part of) a tornado. Panel B is a policy evaluation that narrows the cash assistance to where we observe job creation. Still, we view the cost per job estimates as an upper bound for two reasons. First, we limit the jobs created calculation to establishments with fewer than three employees. In all likelihood, there are jobs created at "medium-sized" (4-6 employees) establishments. Second, we consider cash assistance provided to everyone hit in a ZIP code, even though a tornado typically hits only a very small fraction of the ZIP.

Table 8 column (1) is our baseline calculation and includes the dollar amount of the grants along with the associated IA administrative costs. The IA administrative costs are estimated from program documents.[18] We estimate a cost of \$242 thousand per job in panel A and \$108 in panel B. Columns (2)-(5) provide rough estimates of the net job cost inclusive of other program costs and fiscal externalities (e.g. Bastian and Jones [2019]; Hendren [2016]). Column (2) adds to our baseline estimate the cost savings from FEMA allocating cash grants rather than SBA disaster loans. FEMA is prohibited from duplicating benefits between the Individual Assistance and SBA programs (SBA [2011]). We assume that in the absence of the cash grants that SBA disaster loans would have increased dollar for dollar by the amount of the reduction in cash grants. Columns (3) and (4) respectively subtract estimates of the federal tax revenue and the federal unemployment benefit savings attributable to the new jobs from our baseline calculation. When we adjust our baseline estimate to include the SBA, federal tax, and unemployment cost savings and limit the calculation to grant

[18] Appendix Section 2 provides additional details regarding the cost calculations.

dollars allocated to ZIP codes hit by a tornado (panel B column 5) we estimate a cost of $75 thousand per job.

6 Conclusion

We provide some of the first evidence for how cash grants to residents following a natural disaster affect individual-level financial and migration outcomes. We also examine how the cash grants affect the survival rate of establishments in disaster-affected neighborhoods. We construct a new dataset that combines individual-level credit bureau data, establishment-level business information, and block-level tornado damage from 34 large, devastating tornadoes. Cash assistance was made available to victims of 25 of the 34 tornadoes. We use the detailed within tornado path damage heterogeneity, along with a triple difference identification strategy, to account for the federal government's endogenous decision to provide cash assistance.

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

Cash grants increase temporary migration from the disaster-affected neighborhood. At the same time, access to cash grants dramatically reduces more permanent migration for residents in the most-damaged blocks. One explanation is that the cash grants provide resources to both pay for home reconstruction and to cover a temporary move while home repairs are completed.

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

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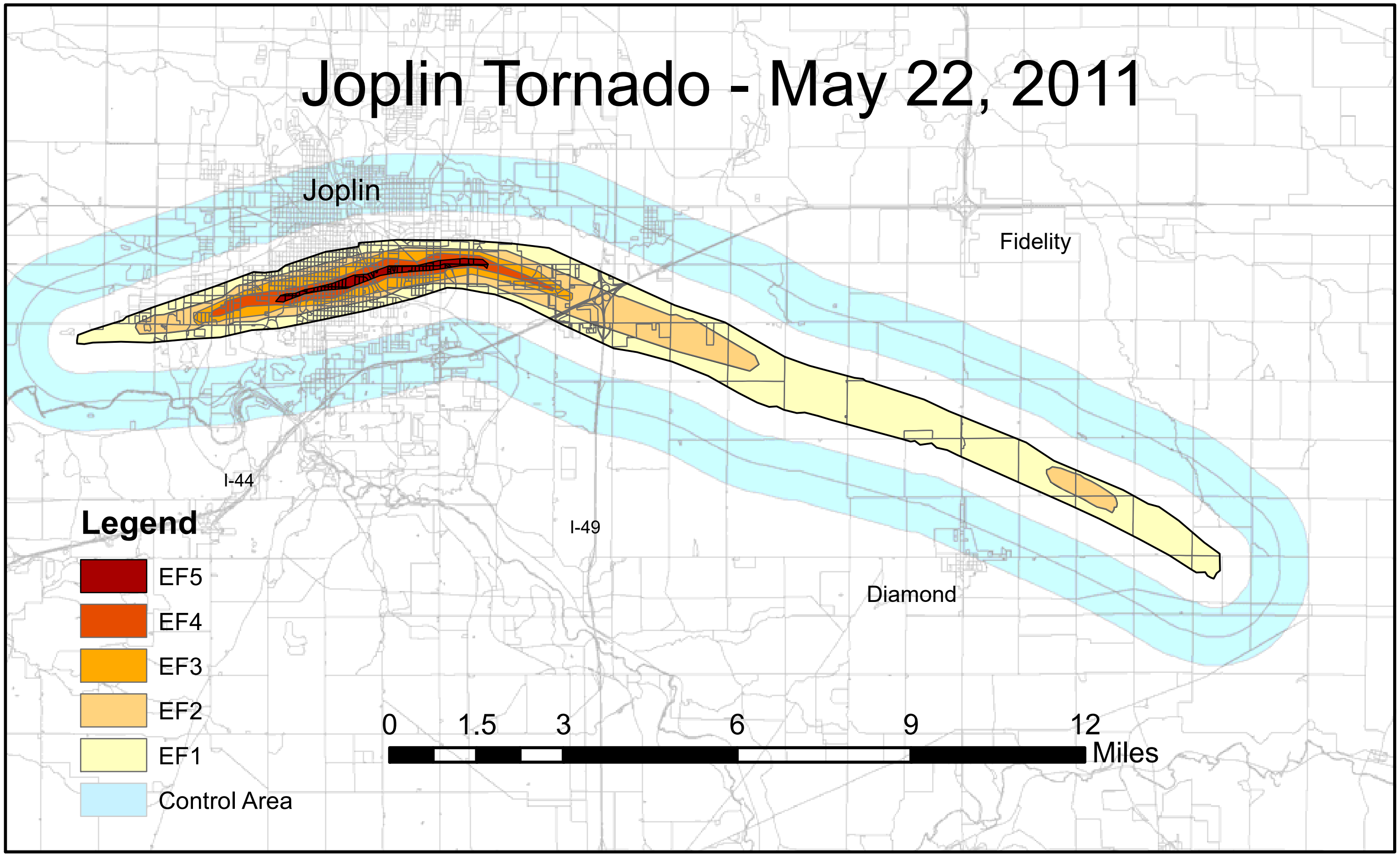
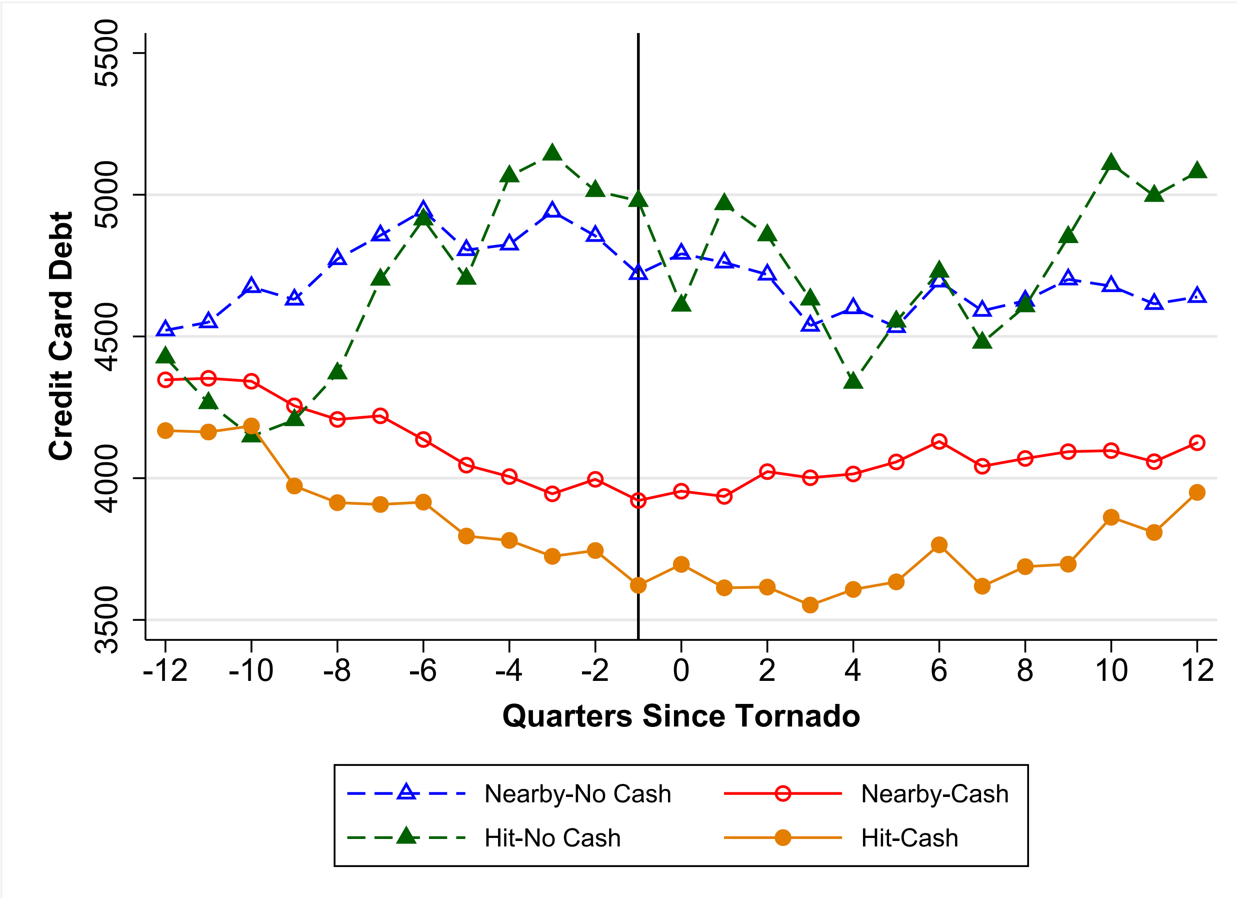
8 Figures and Tables

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



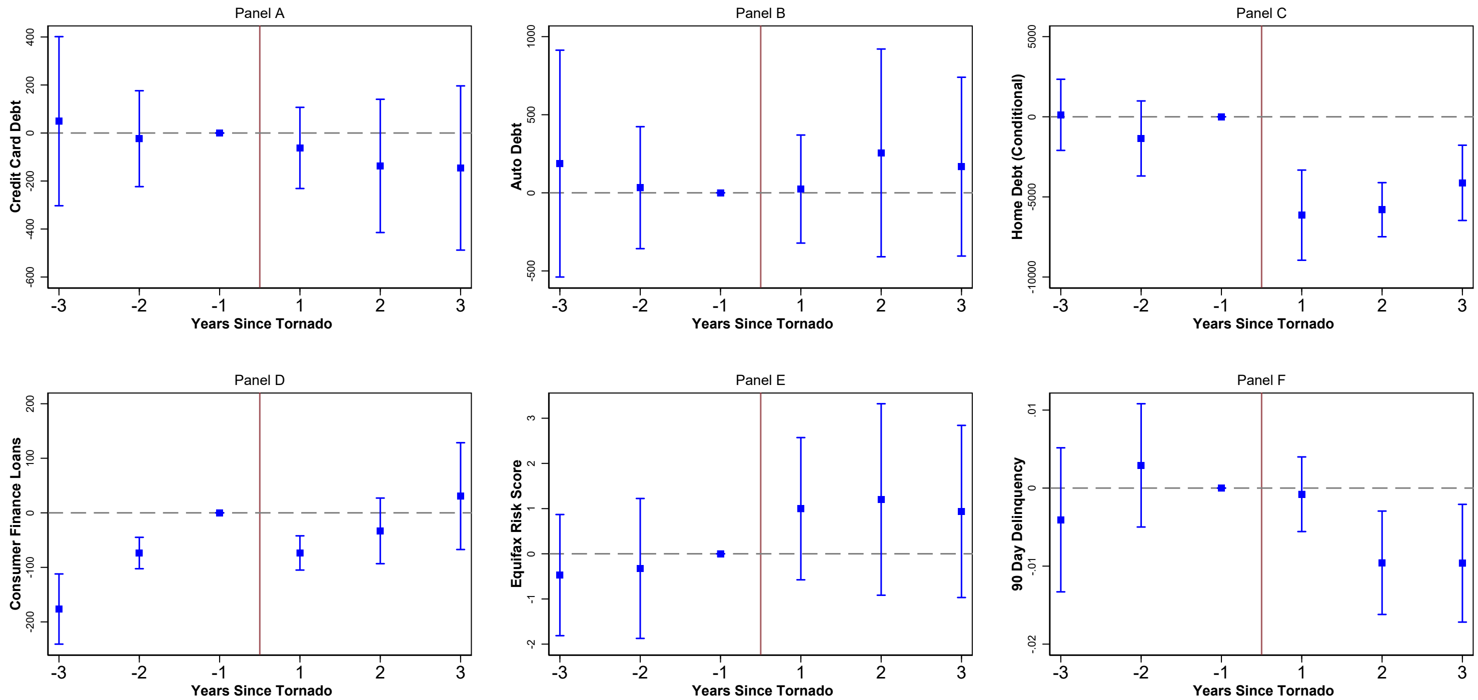
The figure shows the damage map for an EF5 tornado that hit Joplin, Missouri on May 22, 2011. The tornado path is outlined in black. The control area is in blue and located between 0.5 and 1.5 miles from the edge of the damage path. The tornado path and the control area are overlaid on a US Census block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

Figure 2: Trends in Credit Card Debt



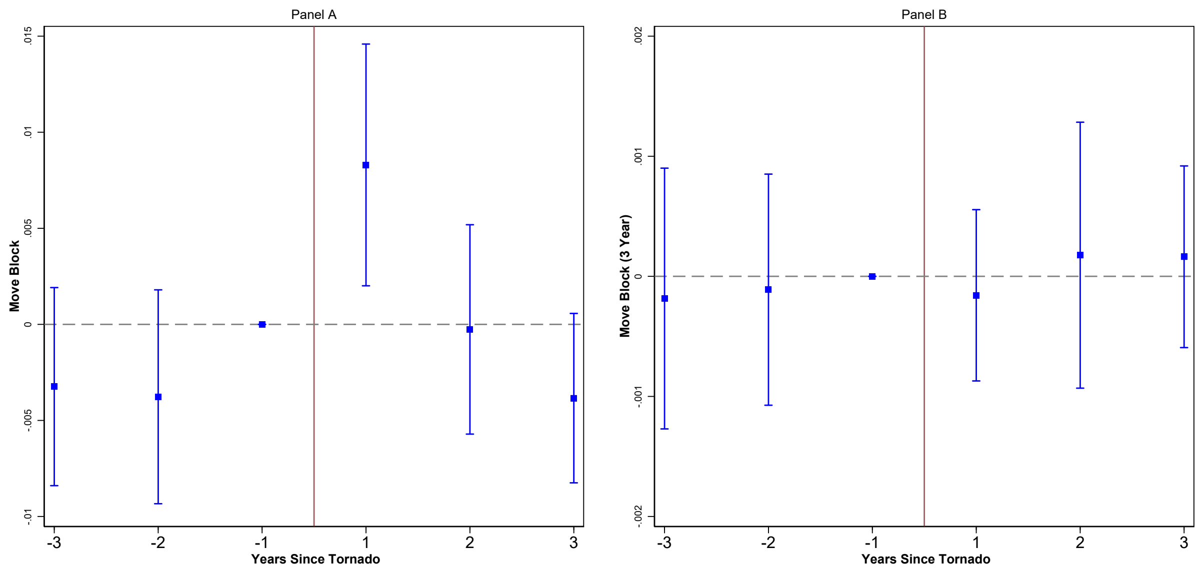
The figure plots the mean credit card balance of four groups of individuals: non-hit residents who lived in the 0.5 to 1.5 mile buffer area around the tornadoes that did not receive cash grants (dashed blue triangles), hit residents who lived in the damage path of tornadoes that did not receive cash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

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



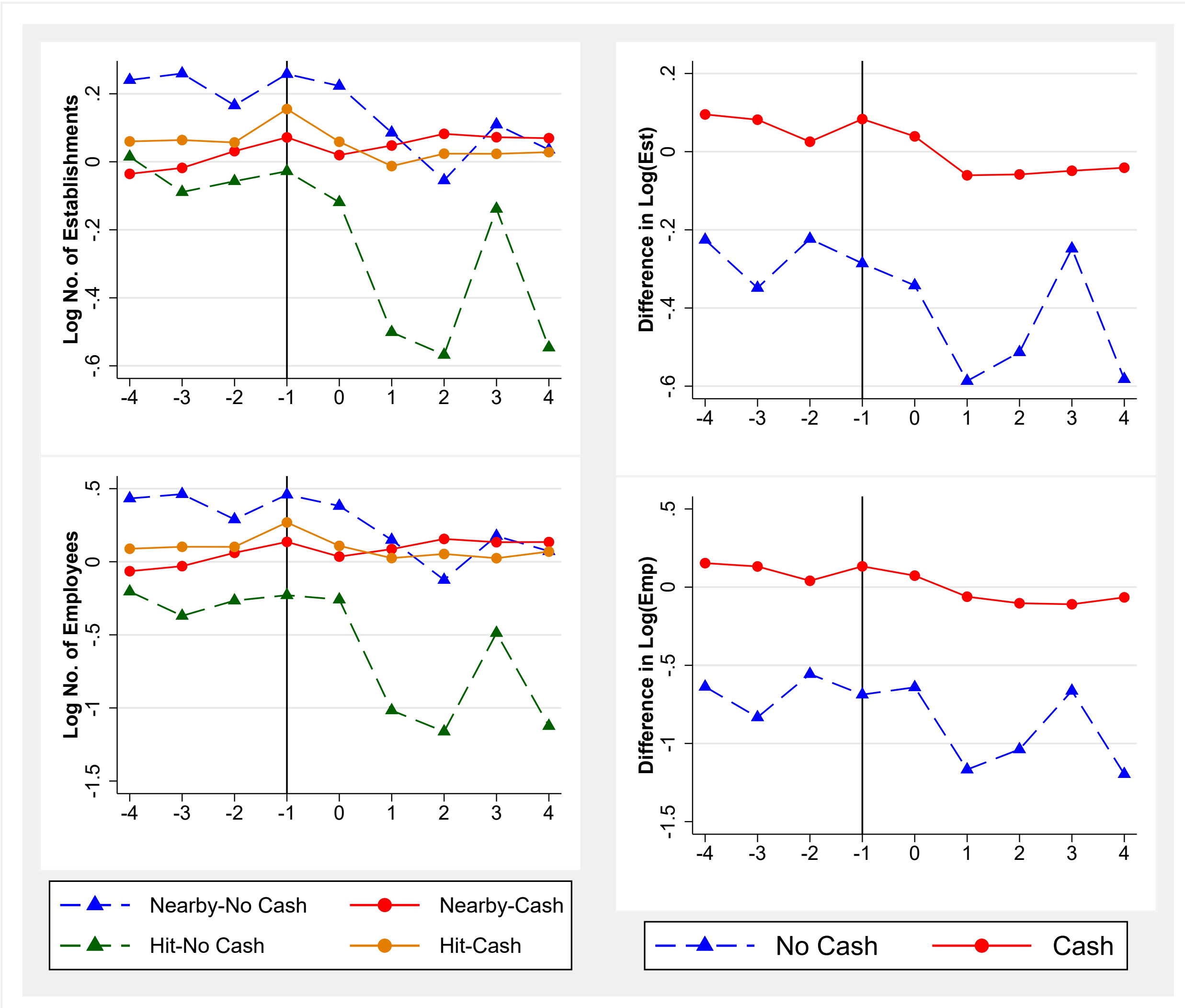
The figure shows yearly event study estimates and 95% confidence intervals for the outcomes in Table 2 and temporary block migration (Table 3, column 1). The event study model replace the $Post_{i,t}$ variable in the continuous damage model with a set of binary variables that indicate the number of years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

Figure 4: Yearly Event Study Analysis of Block Migration



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

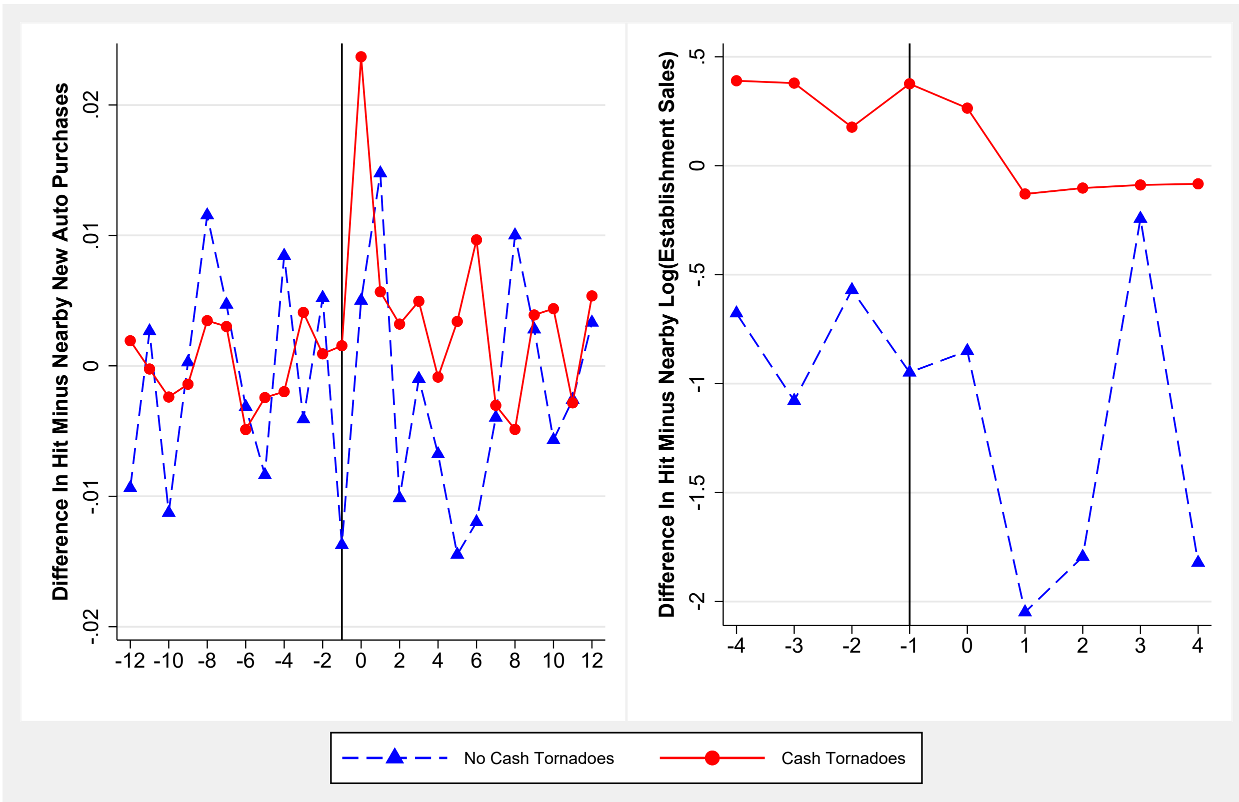
Figure 5: Trends in Business Outcomes



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

Figure 6: Trends in Motor Vehicle Purchases and Business Establishment Sales

Figure 6: Trends in Motor Vehicle Purchases and Business Establishment Sales



The figure shows trends in the hit minus nearby difference for new quarterly auto loans and establishment-level log sales. We plot the differences separately for cash and no-cash tornadoes, after first taking the mean residuals from a regression that controls for calendar time. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), Infogroup Historic Business Database, National Weather Service, US Census.

Table 1: **Comparative Statistics for Individuals and Business Establishments Hit by and Nearby to a Tornado**

	(1)	(2)	(3)	(4)	(5)	(6)
Tornado Type:	Cash Assistance			No Cash Assistance		
Census Block:	**Overall**	**Hit**	**Nearby**	**Overall**	**Hit**	**Nearby**
Panel A: CCP Variables						
Debt Balances						
Credit Card	3,832	3,622	3,922	4,750	4,978	4,720
Auto	7,040	6,996	7,059	8,000	7,984	8,003
Home	50,057	54,104	48,311	60,336	77,889	58,028
Consumer Finance Loans	815	884	786	825	797	829
Total	64,802	68,644	63,144	76,832	94,227	74,544
Financial Health						
Equifax Risk Score	671	672	671	696	705	695
90 Day Past Due	0.21	0.21	0.22	0.16	0.15	0.16
Foreclosure Flag	0.021	0.019	0.022	0.017	0.018	0.017
Migration						
Move From Block	0.057	0.054	0.058	0.075	0.037	0.079
Move From County	0.024	0.025	0.024	0.044	0.027	0.046
Panel B: Census Variables						
Economic						
Median Income	30,249	29,601	30,528	33,886	44,104	32,555
Poverty Rate	0.13	0.13	0.12	0.11	0.06	0.11
Fraction Owner Occupied	0.73	0.74	0.73	0.71	0.85	0.69
Median Home Value	61,548	60,837	61,854	72,361	92,994	69,674
Socioeconomic						
Fraction College Degree	0.17	0.17	0.18	0.25	0.33	0.24
Fraction African American	0.22	0.19	0.23	0.06	0.05	0.06
Fraction Hispanic	0.02	0.02	0.02	0.03	0.02	0.03
Fraction Age 65+	0.13	0.13	0.13	0.12	0.10	0.12
Panel C: Business Establishments						
Number of Establishments	2.0	2.1	2.0	3.1	3.0	3.1
Number of Employees	25	23	26	33	44	31
Manufacturing Employment Share	0.05	0.05	0.05	0.04	0.04	0.04
CCP Observations	17,957	5,401	12,556	3,368	388	2,980
Number of Blocks	6,345	1,949	4,396	1,050	118	932
Number of Establishment Blocks	15,627	4,944	10,683	2,139	365	1,774

Panel A shows CCP variable means from the quarter before a tornado for individuals residing in hit or nearby (control) blocks at the time of the tornado. Panel B shows 2000 US Census block group information for the same hit and nearby blocks as in Panel A. Panel C shows block-level business establishment information for the year before a tornado for the same blocks as in Panel A. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), Infogroup Historic Business Database, National Weather Service, US Census.

Table 2: **Household Finance Triple Difference Estimates**

Dependent Variable:	Credit Card (1)	Consumer Finance Loans (2)	Home (Conditional) (3)	Auto (4)	Equifax Risk Score (5)	90 Day Delinquency (6)
Panel A: Pooled						
Cash Tornado x Post x Hit	-375	71	-7,742	775	1.7	0.0047
	(338)	(104)	(4,955)	(594)	(3.4)	(0.0166)
Dependent Variable Mean	$3,622	$884	$149,584	$6,996	671.7	0.2073
R-squared	0.004	0.006	0.021	0.008	0.018	0.001
Observations	496,708	496,708	123,602	496,708	492,439	496,708
Panel B: Continuous Damage						
Cash Tornado x Post x Hit	-108	55	-4,285	85	1.3	-0.0051
	(90)	(21)	(1,271)	(170)	(0.9)	(0.0035)
Dependent Variable Mean	$3,622	$884	$149,584	$6,996	671.7	0.2073
R-squared	0.004	0.006	0.021	0.008	0.018	0.001
Observations	496,708	496,708	123,602	496,708	492,439	496,708
Panel C: Binned						
Cash Tornado x Post x Low	-638	-112	-7,173	1,152	-2.3	0.0405
	(566)	(111)	(5,342)	(923)	(2.9)	(0.0215)
Dependent Variable Mean	$3,472	$887	$154,202	$7,018	672.6	0.2085
Cash Tornado x Post x Medium	144	380	1,509	529	8.3	-0.0509
	(276)	(100)	(5,879)	(921)	(3.8)	(0.0170)
Dependent Variable Mean	$3,846	$898	$148,588	$7,690	672.7	0.2058
Cash Tornado x Post x High	-647	-63	-39,555	-409	1.3	0.0001
	(355)	(77)	(4,360)	(628)	(2.3)	(0.0109)
Dependent Variable Mean	$3,700	$845	$134,135	$5,471	666.4	0.2059
R-squared	0.005	0.006	0.022	0.008	0.018	0.001
Observations	496,708	496,708	123,602	496,708	492,439	496,708

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

Table 3: **Block and County Migration Estimates**

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

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

Table 4: **Household Finance Triple Difference Estimates - Heterogeneity**

Dependent Variable:	Credit Card (1)	Home (Conditional) (2)	Auto (3)	90 Day Delinquency (4)	Move from Block (5)	Move from Block (3 Year) (6)
Panel A: Available Credit						
Low Available Credit						
Cash Tornado x Post x Hit	26	-942	366	-0.0110	0.0030	0.0002
	(55)	(2,216)	(136)	(0.0083)	(0.0016)	(0.0007)
Dependent Variable Mean	$392	$126,903	$3,435	0.3159	0.0520	0.0004
Observations	152,301	13,439	152,301	152,301	247,017	247,017
High Available Credit						
Cash Tornado x Post x Hit	-305	-10,039	231	0.0039	0.0091	-0.0000
	(182)	(1,874)	(236)	(0.0038)	(0.0032)	(0.0004)
Dependent Variable Mean	$7,101	$153,092	$8,447	0.0387	0.0448	0.0019
Observations	170,331	68,576	170,331	170,331	262,586	262,586
Panel B: Credit Score						
Low Equifax Credit Score						
Cash Tornado x Post x Hit	-59	2,448	380	-0.0416	-0.0014	-0.0003
	(103)	(2,960)	(406)	(0.0094)	(0.0021)	(0.0002)
Dependent Variable Mean	$1,900	$127,929	$5,466	0.5249	0.0713	0.0005
Observations	161,520	21,380	161,520	161,520	246,339	246,339
High Equifax Credit Score						
Cash Tornado x Post x Hit	-100	-7,772	52	0.0004	0.0067	0.0001
	(103)	(1,810)	(188)	(0.0004)	(0.0025)	(0.0004)
Dependent Variable Mean	$3,204	$162,278	$6,193	0.0000	0.0313	0.0005
Observations	165,527	55,420	165,527	165,527	245,138	245,138
Panel C: Age						
Young						
Cash Tornado x Post x Hit	-231	1,959	494	-0.0182	-0.0040	-0.0006
	(191)	(2,475)	(338)	(0.0078)	(0.0021)	(0.0002)
Dependent Variable Mean	$2,684	$167,478	$7,315	0.2894	0.0723	0.0010
Observations	168,246	27,112	168,246	168,246	254,855	254,855
Old						
Cash Tornado x Post x Hit	41	-2,510	-407	0.0022	0.0054	-0.0001
	(126)	(1,465)	(305)	(0.0063)	(0.0024)	(0.0004)
Dependent Variable Mean	$3,571	$119,737	$5,336	0.1066	0.0345	0.0013
Observations	162,810	39,133	162,810	162,810	261,768	261,768

The table shows triple difference heterogeneity estimates for six outcomes (omitting credit score and county migration) in Tables 2 and 3 using the continuous damage model. The difference is that the model is estimated separately on two groups of individuals (lower and upper terciles) based on available credit (panel A), Equifax Risk Score (panel B), and age (panel C). Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 5: **Estimates for the Number of Business Establishments and Employees**

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

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

Table 6: **Heterogeneity in Business Establishment Triple Difference Estimates by Industry, Age, and Size**

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

The table shows triple difference estimates using the same model as in Table 5 panel B, except that we limit the sample by establishment industry (panel A), age (panel B), and size (panel C). Each point estimate in the table is from a separate regression. We classify each establishment as manufacturing or non-manufacturing using the two digit SIC. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 7: **Triple Difference Estimates for Auto Purchase by Credit Availability**

Dependent Variable:	New Auto Purchases	New Auto Balance
	(1)	(2)
Panel A: Available Credit		
Low Available Credit		
Cash Tornado x Post x Hit	0.005	92
	(0.001)	(21)
Dependent Variable Mean	0.017	$216
Observations	171,825	171,825
High Available Credit		
Cash Tornado x Post x Hit	0.001	5
	(0.002)	(55)
Dependent Variable Mean	0.037	$630
Observations	184,225	184,225
Panel B: Credit Score		
Low Equifax Credit Score		
Cash Tornado x Post x Hit	0.006	167
	(0.003)	(78)
Dependent Variable Mean	0.025	$330
Observations	171,375	171,375
High Equifax Credit Score		
Cash Tornado x Post x Hit	0.001	-13
	(0.001)	(51)
Dependent Variable Mean	0.031	$565
Observations	170,225	170,225

The table shows triple difference heterogeneity estimates for new car loans and new car loan balances. The model is estimated separately on two groups of individuals (lower and upper terciles) based on available credit (panel A), Equifax Risk Score (panel B), and age (panel C). We do not winsorize the new auto loan variable at the 99% level. The reason is that the new dollar loan variable is a flow variable with a median of $0. A decision to winsorize would affect a large fraction of the non-zero values. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 8: **Cost per Job Retained or Created**

Model:	Baseline	Baseline + SBA Savings	Baseline + Federal Tax Revenue	Baseline + Unemployment Savings	Baseline + (2)-(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: All Zips					
Cost Per Job ($1,000)	$242	$223	$225	$235	$198
Panel B: Hit Zips					
Cost Per Job ($1,000)	$108	$100	$91	$101	$75

This table calculates the cost per job retained or created by establishments in damaged blocks where residents have access to cash grants. We use the employment estimates for small businesses (Table 6, panel C). Panel A considers the total cash assistance provided to individuals living in any disaster county in the state. Panel B considers cash assistance to individuals living in ZIP codes hit by a tornado. Typically, a tornado only hits a small fraction of the ZIP code. The baseline calculation in column (1) only includes the direct and administrative costs for the grants. Column (2) adds the estimated administrative cost savings to the SBA program to our baseline calculation. Column (3) adds the estimated tax revenue to our baseline calculation. Column (4) adds the estimated federal government unemployment insurance savings to our baseline calculation. Column (5) is our most comprehensive calculation and includes the estimated SBA and unemployment savings, as well as, the estimated tax revenue. Sources: Brown and Earle [2017], Bureau of Labor Statistics, Federal Emergency Management Agency, Federal Reserve Bank of St. Louis, Government Accountability Office, Infogroup Historic Business Database, National Bureau of Economic Research, National Weather Service, US Census, Whittaker et al. [2019].