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

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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 high damage blocks with access to cash grants have 17% less credit card debt following the disaster than those without access to cash grants. Grants do not reduce negative financial outcomes, but do decrease migration. The grants play a role in mitigating the effects of the shock to businesses; resulting in 18% more establishments and 29% more employees post-disaster in disaster-affected neighborhoods 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 hurricanes in the US 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

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

was greater still for individuals who received lump sum flood insurance cash 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 to disaster-affected neighborhoods where individuals do not receive cash grants.

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 the age and size of an establishment. Establishments that rely on a local customer base (e.g. retail) may benefit more from the cash stimulus than establishments that have a non-local customer base (e.g. manufacturing). Furthermore, recent studies find that smaller businesses appear to be more vulnerable to economic shocks (e.g. Cole and Sokolyk [2016]; Greenstone et al. [2015]). Finally, knowledge of whether a local cash stimulus has a similar impact on existing establishments (i.e. increasing the survival probability) as on new establishments (i.e. growth in entrepreneurship) could help to inform public policy.

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 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 block-level EF ratings.

However, precisely controlling for the level of damage does not alleviate the 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 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 control 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 just 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.

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. There is little evidence that the cash grants diminish negative financial outcomes. There is no change in overall individual credit scores, the proportion of individuals that have at least one account that is 90 days delinquent, or mortgage foreclosure rates.

Overall, there is a 15% increase in temporary (one quarter) out-migration from the block for residents hit by a tornado who have access to cash assistance. At the same time, access to cash grants reduces more permanent (three year) migration for those residents in the most-damaged blocks by roughly 100%.

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. There are 18% more establishments and 29%

more employees relative to similarly affected neighborhoods where residents do not 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 neighborhoods where cash grants are provided.

We find that the increase in the number of establishments is due to a higher survival rate for existing establishments. There is no evidence that the cash grants affect the formation of new establishments. Moreover, the increase in the number of establishments is explained by a higher survival rate for non-manufacturing establishments. This suggests a mechanism whereby the cash stimulus to the local population most benefits businesses that rely on local demand. Finally, the increase in the survival rate is greater for smaller establishments and nearly all of the employment effects are concentrated among smaller establishments.

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 $74 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 $57 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.[3]

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]The official tornado rating scale 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 as compared to the EF scale. For simplicity, in the paper we sometimes refer to the tornadoes in our sample using only the Enhanced Fujita scale.

[3]To our knowledge, there is no single location that maintains a collection of all the tornado maps that include sub-tornado path F and EF ratings. We collected the tornado maps used in this study over the time period June 2013-August 2014, by conducting archival and internet searches based on the list of 87 tornadoes from the Tornado History Project that satisfy the first two criteria.

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) 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 disaster assistance. The largest component of disaster assistance is Public Assistance. Public Assistance is available to local and state governments as well as non-profit organizations located in the impacted area. These groups can access grant money to remove debris, repair infrastructure, and to aid in the reconstruction of public buildings. A second form of assistance comes in the form of subsidized lending. Disaster-affected individuals and businesses can request subsidized Small Business Administration (SBA) disaster loans. The third type of disaster assistance is Individual Assistance (cash grants). Residents in disaster areas can receive cash grants of up to approximately $30,000.[4] The level of assistance is linked to incurred damage (e.g. structural damage to the home) and expenses (e.g. temporary housing and relocation) caused by the disaster. Disaster-affected individuals and businesses in counties that receive either Public Assistance or Individual Assistance are also typically able to apply for SBA disaster loans.

There is no single minimum eligibility threshold or guideline that must be met in order for FEMA to approve Individual Assistance. Instead, FEMA is required to consider six criteria when deciding whether to recommend Individual Assistance for a disaster (GAO [2018]). The criteria are: concentration

[4] The maximum amount is indexed to inflation and equal to $30,200 in 2010 (Fed [2010]).

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).

Table 1 provides summary information for the tornadoes in our sample. Twenty-five tornadoes are part of disaster declarations where individuals received cash grants.[5] Twenty-two of these 25 tornadoes are also areas that received Public Assistance. Panel B provides a comparison between tornadoes where residents received cash grants and tornadoes where residents did not receive cash grants. Tornadoes with cash assistance are part of larger state-level disasters as measured by either the percent of state counties included in the PDD or Public Assistance money distributed.[6] 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 do not receive cash assistance.[7]

Cash assistance tornadoes 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. The difference in the overall damage and number of persons injured between cash and no-cash tornadoes motivates our preferred

[5] The Ferguson, MO tornado crosses state lines (see Appendix Table 1). Approximately two-thirds of the tornado is in Missouri and one-third in Illinois. Cash assistance is provided to victims living in Missouri. In the analysis, we classify the Missouri tornado victims as receiving cash assistance and the Illinois tornado victims as not receiving cash assistance.

[6] Three No Cash Assistance tornadoes receive public assistance.

[7] We calculate the average share of the two party (Democratic and Republican) vote that the losing party receives across the 1996, 2000, and 2004 presidential elections for each PDD county (e.g. Reeves [2011]).

econometric model. In our preferred model we separately compare the effect of the tornado on individuals and businesses living in low, medium, and high damage blocks.

We obtained information on all cash grants distributed under the Individual Assistance program via a Freedom of Information Act (FOIA) request. The information includes the total amount of cash grant assistance, the number of applicants receiving assistance, and the number of days between the Presidential Disaster Declaration and the approval of the first grant. Due to privacy considerations, we are only able to access summary cash grant information at the 5 digit ZIP Code level.

Appendix Table 2 shows cash grant summary statistics for the 160 ZIP Codes in our sample that are hit by (any portion of) a tornado where residents receive cash grants and 55 nearby ZIP Codes for these same tornadoes. The nearby ZIP Codes overlap with our 0.5 to 1.5 mile tornado buffer area and do not overlap with any part of the tornado path. The median time between the Presidential Disaster Declaration and the approval of the first cash grant applications in a hit ZIP Code is just 6 days. On average, a ZIP Code hit by a tornado receives $522 thousand in cash assistance. The average amount of assistance per grant is $5,863, while the mean per capita grant assistance across the hit ZIP Codes is $62. These statistics reflect the fact that that, even for the largest tornadoes, only a small fraction of a ZIP Code is directly hit. As such, the per capita ZIP Code-level summary statistics dramatically understate the average grant amount for individuals hit by the tornado.[8]

Appendix Table 2 also shows summary statistics for the level of SBA disas-

[8]Appendix Figure 1 illustrates these data limitations using the same 2011 Joplin, MO tornado shown in Figure 1. The majority of the tornado path and nearly all of the most highly damaged areas occur in a single ZIP Code (64804). More than $12 million is provided to residents in this ZIP Code. Nevertheless, the tornado only hits approximately 9.95% of the land area of the ZIP Code. Some residents in portions of the ZIP Code farther away from the tornado path likely experienced minor storm-related damage. These residents are eligible for cash assistance. As evidence for this, all of the ZIP Codes surrounding the tornado path have non-zero levels of cash assistance. The majority of these ZIP Codes (colored light blue in the figure) receive much smaller levels of total cash grants, ranging from $408 to $301,382.

ter loans awarded to the hit and nearby ZIP Codes following a tornado.[9] SBA disaster loans are available to both individuals (households) and businesses. Individuals can apply for up to $240 thousand, while businesses can apply for up to $2 million (SBA [2018]). Loan amounts are based on verified losses (i.e. building damage, personal property, business property). Small businesses can also receive loans based on "economic injury" (e.g. documented income loss). Loan applicants do not need collateral, but must demonstrate credit worthiness. Not all applications are approved.

SBA loans were made available to residents and businesses for all but one Presidential Disaster Declaration, and in four of the six tornadoes where there was no Presidential Disaster Declaration. Overall, SBA loans are available in 99% of the sample blocks hit by a tornado.[10] The total verified losses are higher for loan applicants in areas hit by cash assistance tornadoes. However, the average amount of approved loans is lower for cash assistance tornadoes (e.g. $1.32 million vs. $1.41 million for home loans). One explanation is that, by law, the amount of SBA disaster loans allocated are reduced dollar for dollar based on the receipt of IA cash grants (SBA [2011]). By contrast, the total verified business loss and total approved business loans are both higher for establishments hit by tornadoes with cash assistance.

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.

[9]The disaster loans are often available for disasters not covered by Presidential Disaster Declarations. There are several alternative mechanisms, including: a Governor Certification Declaration for businesses and an Administrative Declaration for individuals (SBA [2015])

[10]These statistics are based on merging the SBA ZIP -by-year data with our block-level tornado panel. It is possible that we understate SBA availability as not all of the SBA observations are merged with our panel.

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.

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 (TM) for each individual.[11]

To form our sample, we take the set of individuals living in the treatment and control blocks at the end of the quarter before the tornado and form a balanced panel that runs from 12 quarters prior to the quarter of the tornado through 12 quarters after the quarter of the tornado. Since individuals do not 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, our sample will consist only of individuals that are 21 and older in the quarter of the tornado. Using the CCP's individual identifiers, we can track all individuals even if they move

[11] 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 variables in each quarter so that our estimates are not driven by the presence of extremely large debt balances or credit limits. Unfortunately, SBA loans are not reported to Equifax. Thus, we are not able to observe if an individual in our sample received a SBA loan. 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.

away from the tornado-affected area or were living elsewhere for some portion of the pre-tornado period.

Table 2 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 2 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.[12] The database includes 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 hit and

[12] 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]).

buffer (control) blocks.[13]

Table 2 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 main goal is to estimate the causal effect of federal disaster cash grants on household finance and migration, and business establishment survival and employment. We use a triple difference (DDD) empirical strategy to do this. 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 damage path and taking the set of Census blocks that are more than 50% contained in the band between the buffer lines.[14] We exclude the half mile closest to the edge of the tornado damage path in case there is measurement error in the tornado map boundaries.

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,

[13]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 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.

[14]Results are similar if we define the buffer area as 0.5-1.0, 0.5-2.0, or 0.5-2.5 miles away from the tornado path.

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

Our baseline empirical specification is a regression-based implementation of a triple difference estimator. We first describe the specification that we use to examine individual financial outcomes. We then describe the difference between the individual and business models.

We estimate the following equation,

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

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). 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.[15]

[15] The binary variables $Cash_i$, Hit_i, $Post_{i,t}$, and the interaction $Cash_i * Hit_i$ are included in the triple difference model, but "drop out" since the model includes individual and time fixed effects. There are a small number of specifications for (mostly) binary, low instance outcomes whereby (terms in) the asymptotic clustering standard error formula does not invert when we cluster at the tornado level. In these specifications we cluster at a different

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. Our identifying assumption is that in the absence of cash assistance, the pre- to post-tornado difference in financial outcomes between individuals hit by a devastating tornado and those who narrowly miss being hit, would be the same for both cash and non-cash tornadoes.

We also estimate an event study version of the 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 quarters the observation is either before or after the tornado. We exclude the last quarter before the tornado which serves as the reference time period.

Since the tornado maps show heterogeneity in damage intensity, we are able to estimate a model specification that allows for the effect of cash assistance to vary with the level of damage. The idea in running this specification is that the level of cash assistance is higher for individuals living in the most damaged parts of the tornado path. The specification also allows for a more direct comparison between individuals living in blocks that sustain the same level of damage. Recall that Table 1 shows that, on average, individuals hit by cash tornadoes suffer slightly more damage.

The tornado damage paths are classified according to the Enhanced Fujita (EF) scale (integer values from 0 to 5 corresponding to six bins of estimated wind speeds). We calculate the area-weighted mean EF value for each Census block and 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. We refer to this specification as our binned damage level specification. The equation for this specification replaces Hit_i with a vector of three binary variables indicating low, medium, or high treatment.

unit (mostly census tract). We are careful to note when this occurs in the table footnotes.

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 able to confirm whether the tornado-year business data are collected before or after the tornado for all businesses. 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 2). 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, in robustness specifications we estimate our triple difference model with inverse probability weighting (IPW) (Hirano et al. [2003]). We estimate the propensity score using a logit model (Rosenbaum and Rubin [1983]).[16]

[16]Specifically, we estimate a separate logit (with the same specification) for the cash grant and no cash grant tornadoes. In our household finance analysis the outcome is a binary variable indicating whether individual i was living in a Census block that was hit by a tornado in the quarter before the tornado. The explanatory variables include the poverty rate and log of median household income in the Census block group in which the individual lived at the end of the quarter before the tornado, and the following credit variables 1, 4, 8, and 12 quarters before the tornado: an indicator for any 90+ day delinquent accounts, an indicator for the presence of a home loan, the net number of new accounts opened in the past quarter, the Equifax Risk Score (TM), and the total credit card, home loan, auto loan, and consumer finance loan balances. The explanatory variables for the business sample are the following variables for each block for each of the three years before a tornado: total, new (in service for one year or less), and existing (in service for four years or more) establishments, and total, new, and existing employees at these establishments. We also include the manufacturing share three years before the tornado. We trim the sample at the maximum control propensity score and minimum treated propensity score so as to have "common support" between individuals in the hit and control blocks.

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 (or income) 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 grants will have only a limited effect on measures of financial wellbeing.

The household finance results presented in Table 3 and Figures 3 and 4 largely confirm these predictions. Table 3 shows triple difference estimates of the effect of cash grants on debt levels and indicators of financial health. Panel A reports our baseline triple difference specification estimates. These estimates consider whether an individual lived within the tornado path, but do not take into account the level of damage. Panel B reports triple difference results which allow for variation in the degree of treatment based on the severity of block-level damage. The table presents only the triple difference coefficient(s), although the specifications also contain the other variables shown in Equation 1.

Figures 3 and 4 plot quarterly triple difference (event study) estimates for the pre- and post-tornado periods, using the quarter before a tornado as the reference period. The estimates are for the pooled damage group of Census blocks (comparable to the estimates in Panel A of Table 3). The quarterly point estimates for each outcome are marked by the dark squares, while the hollow boxes and dotted lines show the upper and lower bounds of the 95% confidence interval.

4.1.1 Debt

Credit cards and consumer finance loans are two common types of short-term debt that victims of tornadoes could use to smooth a financial shock. Table 3 column 1 panel A shows that victims of cash tornadoes have a statistically insignificant \$375 (10%) reduction in their average quarterly credit card balance over the three years following a tornado (probability value 0.286). Panel B shows a larger, statistically significant \$647 (17%) reduction in high damage blocks (probability value 0.084). The estimated impact on the use of consumer finance loans is economically small and not statistically significant in either the pooled model or in the high damage blocks. However, as we discuss below, the muted response for consumer finance loans is at least partially due to differences in pre-tornado debt levels between individuals affected by the cash and no-cash tornadoes.

Total home debt, including both mortgages and home equity, decreases by \$7,738 (5%) in the pooled sample (probability value 0.136) for the sub-sample of individuals that have home loans continuously in the 12 quarters before the tornado. The reduction is much larger for individuals in the most damaged blocks who, on average, reduce their mortgage debt by about \$40,000 (probability value 0.000). Difference-in-differences results for the debt outcomes are presented in Appendix Table 3. 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.

We also estimate two additional difference-in-differences models so as to better understand what explains the reduction in mortgage debt for hit residents with access to cash grants (Appendix Table 6). First, we estimate the same difference-in-differences 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 (\$14,536 versus \$6,655 in the pooled model, and \$73,971 versus \$14,134 in the high hit blocks). The reduction in home debt is similar for victims of cash tornadoes regardless of whether they move. Second, we estimate a difference-in-differences 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 both affected by cash grants. The point estimates in these descriptive regressions are also imprecise.

The estimated change in auto debt in Table 3 is imprecise and varies between the pooled and binned specifications. Nevertheless, the DD model results in Appendix Table 3 show that auto debt increases more damaged blocks regardless of cash assistance.

Figure 3 plots quarterly event study debt estimates for the pooled sample. The pre-tornado trends for credit card, home, and auto debt are roughly flat in the pre-tornado period. None of the quarterly coefficients are statistically significant. There is no evidence for differing pre-tornado trends for the victims of cash and no-cash tornadoes for these outcomes.

However, there is a clear upward trend in the level of consumer finance loan debt in the three years before a tornado (Figure 3 panel B). 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. As a robustness specification, we estimate the same event study model except that we weight and trim by the propensity score. There is no tornado pretrend in the inverse propensity score weighted (IPW) event study specification (Appendix Figure 5 panel B). The IPW event study figure and the IPW triple difference model (Appendix Table 8 column 2) both show a drop in consumer finance loan debt following a cash tornado. After correcting for pre-tornado trends, there is a $279 (30%) reduction in consumer finance loan debt for individuals in the high damage blocks (probability value 0.027).

4.1.2 Financial Wellbeing

Overall, Table 3 columns 5-7 show that there is little evidence that cash grants improve financial wellbeing following a devastating tornado. There is a large, but statistically insignificant decrease in the foreclosure rate in the pooled model (-35%, probability value 0.508) for the sub-sample of people with a home loan in all 12 quarters before the tornado. The effect on credit score and 90-day delinquency are economically small and statistically insignificant. The one 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 risk score.

Figure 4 shows the quarterly event study analysis for the financial health measures. The quarterly estimates are noisier than those for debt. Nevertheless, there is no evidence of any difference in the pre-tornado trends between individuals hit by cash and no-cash tornadoes.

4.1.3 Migration

Table 4 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 2 panel A).

We estimate a 0.8 percentage point or 15% increase (probability value 0.033) in the one quarter block migration rate for the pooled sample of individuals who have access to cash grants. The point estimates for the binned damage model are all positive, but imprecisely estimated. Cash grants do not impact more permanent (3 year) migration in the pooled sample. However, there is a reduction in more permanent out-migration among residents in the high damage blocks who have access to cash grants. These residents are more than 100% less likely to move from their block and county of residence and to remain away for (at least) three consecutive years (probability values 0.000 and 0.040, respectively).

Figure 4 panel D shows the quarterly event study block migration analysis. Prior to a tornado, there is some differential migration between residents in blocks that will later be hit by cash and no-cash tornadoes. **IPW findings?**

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 receive cash grants will likely reduce their debt by less than tornado victims who are not credit-constrained. The reason is that credit-constrained individuals are largely shut out of credit markets, and in the absence of the cash grants, these individuals would not have been able to borrow. 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 5 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: risk score and available credit. We define available credit as the difference between total credit card debt and the total credit card debt limit (across all credit cards). We separately divide our sample into thirds based on age, Equifax Risk Score (TM), and available credit, and compare outcomes for the lowest third to the highest third (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 (TM).[17]

Cash grants lead to a larger reduction in credit card debt for less credit-constrained tornado victims. Individuals with high available credit at the time of the tornado reduce their quarterly credit card debt by $836 (12%) (probability value 0.168). There is no change in credit card debt for those individuals with low available credit ($205, probability value 0.227).

Cash grants lead younger individuals to reduce their credit card debt by more than older individuals (-25% versus 3%), but the point estimates are too imprecise to reject the null hypothesis that the estimates are equivalent. At the same time, older tornado victims who have access to cash grants reduce their home and auto debt more than younger victims. As with our main results, there is little evidence that cash grants lead to changes in 90-day delinquency or foreclosure.

Access to cash grants leads less credit-constrained residents who are hit by a tornado to move from the block and county. For example, hit residents with high available credit increase their block out-migration rate by 2.5 percentage points (56%) (probability value 0.004). The cash grants do not lead to changes in migration rates for hit residents who have low available credit. We estimate an economically small and statistically insignificant change in block-migration of -0.3 percentage points (-6%) (probability value 0.828). There is no evidence that cash grants differentially affect migration rates for the young and old.

[17] 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.

4.1.5 Robustness

Appendix Tables 8-13 show robustness analysis for our triple difference model for each of our nine household finance and migration outcomes. We estimate two alternative specifications for each outcome. First, we use our main 34 tornado sample, except that we weight by the propensity score. Second, we estimate the same model as in the paper, except we use all 35 tornadoes. Overall, the estimates are qualitatively similar, particularly in the 35 tornado sample. The estimates tend to be somewhat smaller in magnitude when we do not weight with the propensity score.

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 following a natural disaster 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 that incur capital losses (e.g. infrastructure, machinery, and inventory) may disproportionately benefit from the increased demand for their goods and services relative to nearby undamaged establishments, given the evidence that natural disasters have a large negative effect on establishment survival.[18]

[18] 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 (and a larger amount of grant money goes to individuals in the tornado-damaged blocks rather than the buffer blocks). Second, even if residents who receive cash grants spend the money equally on locally

Second, cash assistance to individuals that happen to be small business owners may positively affect establishment outcomes. The Bureau of Labor Statistics calculates that the average establishment size in the US during our sample period is 16 employees (Choi and Spletzer [2012]). However, the average size masks the fact that there are a large number of establishments with very few employees. In our sample, the median business establishment is small and has just four employees. Moreover, survey evidence finds that the majority of US business establishments are operated out of a home. Approximately three quarters of these establishments employ workers other than the business owner (Kelley et al. [2012]).

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, which we label as year zero. 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

damaged establishments and buffer region establishments, the increased business revenue is likely to be more important for damaged establishments to, for example, prevent closure.

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 cash grants the difference in establishment outcomes after a tornado would be the same for the two groups. The trends to the left of the vertical line are roughly parallel for both the number of establishments and the number of employees, providing evidence for the validity of the key identifying assumption in the triple difference model.

Table 6 columns (1) and (2) show triple difference estimates of the effect of cash grants on the number of establishments and employment. Panel A reports our baseline triple difference specification that pools all areas hit by the tornado regardless of damage intensity. Establishments in damaged blocks where residents have access to cash grants benefit economically. Overall, there are 17.7% more establishments (probability value 0.094) and 28.6% more employees (probability value 0.104). There is suggestive evidence that the effect of providing cash grants to the local population is greater for establishments that are located in less damaged blocks. We estimate a 22% (probability value 0.077) increase in the number of establishments in low damaged blocks and a 11.8% (probability value 0.263) increase in the number of establishments in high damage blocks. Nevertheless, we are unable to reject the null hypothesis that the low and high damage estimates are equal at conventional significance levels.

Table 6 columns (3)-(6) show difference-in-differences 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. In the medium and high damage blocks we estimate a reduction in the number of establishments regardless of whether cash assistance is provided to the local population. However, the reduction is largest in blocks hit by no-cash tornadoes.

4.2.2 Heterogeneity by Industry, Age, and Size

Table 7 presents estimation results from our triple difference model that examines 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. We estimate the same pooled damage level specification presented in Table 6.

Panel A of Table 7 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 similar in blocks hit by cash and no-cash tornadoes (Table 2 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 estimated effect on manufacturing establishments and manufacturing employment is close to zero and not statistically significant. We estimate a -0.6% reduction (probability value 0.529) on the number of manufacturing establishments, and a -2.7% reduction (probability value 0.490) in manufacturing employment. 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 6. Appendix Table 7 shows triple difference estimates for establishments in each of the “1 digit” industries that we pool together in the non-manufacturing category. The largest impact is observed in the service, construction, and retail sectors. 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.

Panel B of Table 7 provides suggestive 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. The effect on new establishments is close to zero: 1.1% for the number of establishments and -0.2% for employment, and not statistically significant (probability values 0.428 and 0.916, respectively). The effect on existing establishments is positive and an order of magnitude larger: 10.2% (probability value 0.193) for the number of establishments and 19.9% (probability value 0.174) for the number of employees.

Panel C of Table 7 divides establishments into small and large-sized establishments based on the empirical distribution of establishment size in our sample (Appendix Figure 4). 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 and employees at very small establishments of 13.1% (probability value 0.076) and 16.3% (probability value 0.081), respectively. We estimate a 7.7% increase (probability value 0.099) in the number of establishments and a 14.2% increase (probability value 0.156) in the number of employees at large establishments. The difference is even more stark when we run our model in levels.[19] 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]).[20]

4.2.3 Robustness

Appendix Tables 14-17 show robustness analysis for the two establishment outcomes using our triple difference and DD models. We estimate the same alternative specifications as in our household finance and migration analysis. Coefficient estimates when we weight using the propensity score are very similar. However, 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.

[19]The number of small establishments at a block increases by 0.35 (probability value 0.066) and block-level employment at small establishments by 0.62 employees (probability value 0.050) in the same triple difference model that considers the dependent variable in levels rather than logs. The increase in the number of employees is precisely estimated and implies an increase in employment at very small establishments of about 28%. By contrast, we estimate that the number of larger establishments increases by 0.12 (probability value 0.425), and that there is a small, imprecisely estimated increase in employment of 0.44 employees (probability value 0.974). The increase in the number of employees at larger establishments implies an effect on employment of about 4%, which is an order of magnitude lower than that at the smaller establishments.

[20]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]).

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.[21]

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 point estimate from 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.

Table 8 presents estimates of the cost per job created from the cash grants. We estimate that the cash grants created 1,816 jobs in the damaged cash grant blocks using our preferred model (Table 7 panel C) and 3,165 jobs when we estimate the model in levels. We calculate these job figures using employment at establishments with three or fewer employees. In panel A, we 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. In panel B, we do the same calculation, except we use the larger

[21] 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 7 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.

jobs created estimate from the levels model. We use these job estimates as the denominator in a cost per job calculation.

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.[22] We estimate a cost of $128 thousand per job. We use the total amount of cash assistance allocated throughout all of the disaster counties associated with the PDD in our cost calculations. A policy evaluation that only considered the cost of the cash grants that went to the blocks hit by the tornado would provide a lower estimate.

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 an equal amount of SBA disaster loans would have been distributed to disaster victims. 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. We estimate a cost of $93 thousand per job when we adjust our baseline estimate to include the SBA, federal tax, and unemployment cost savings (column 5). When we use the larger jobs created point estimate in Panel B the cost per job estimates are about half as large.

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

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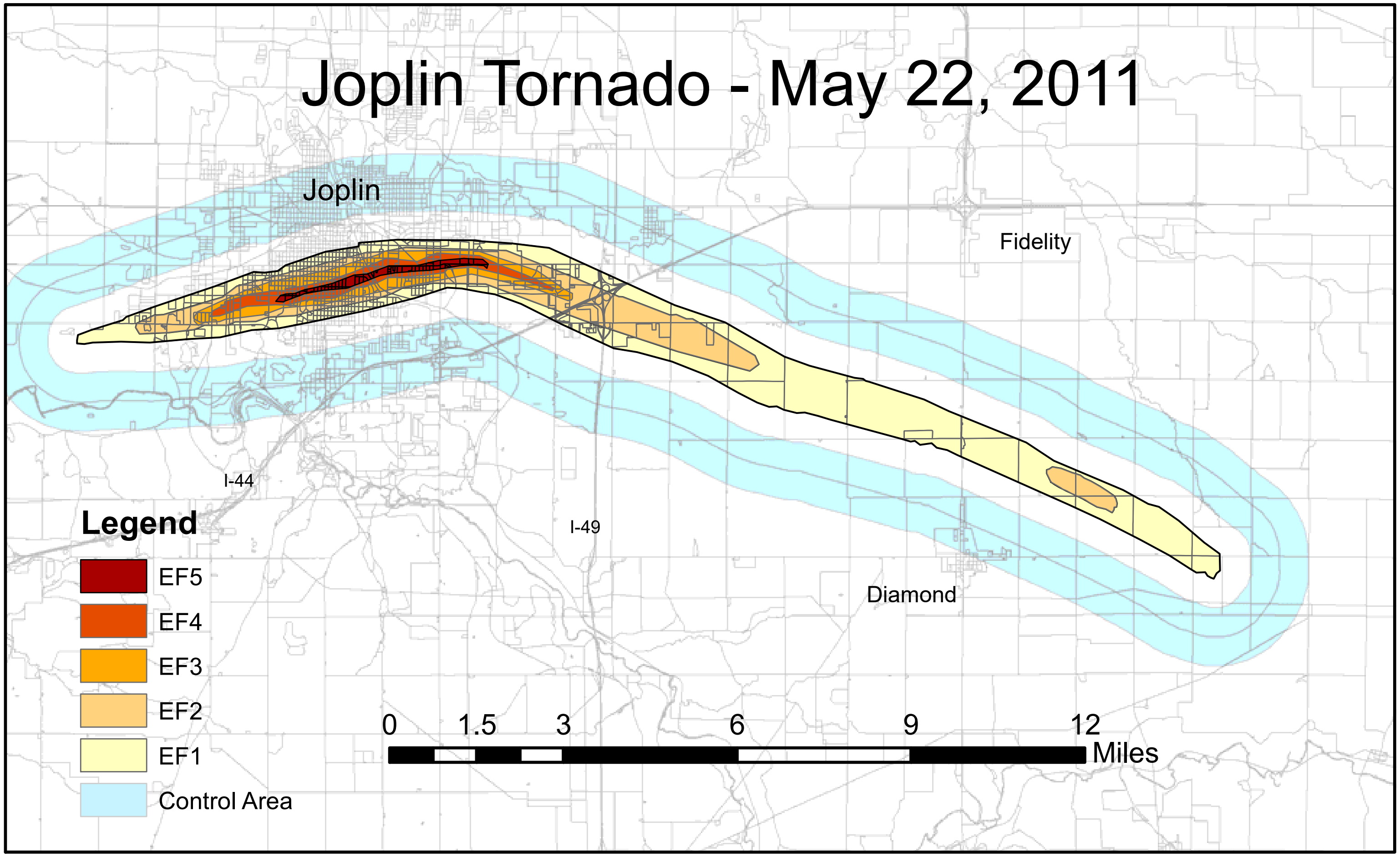
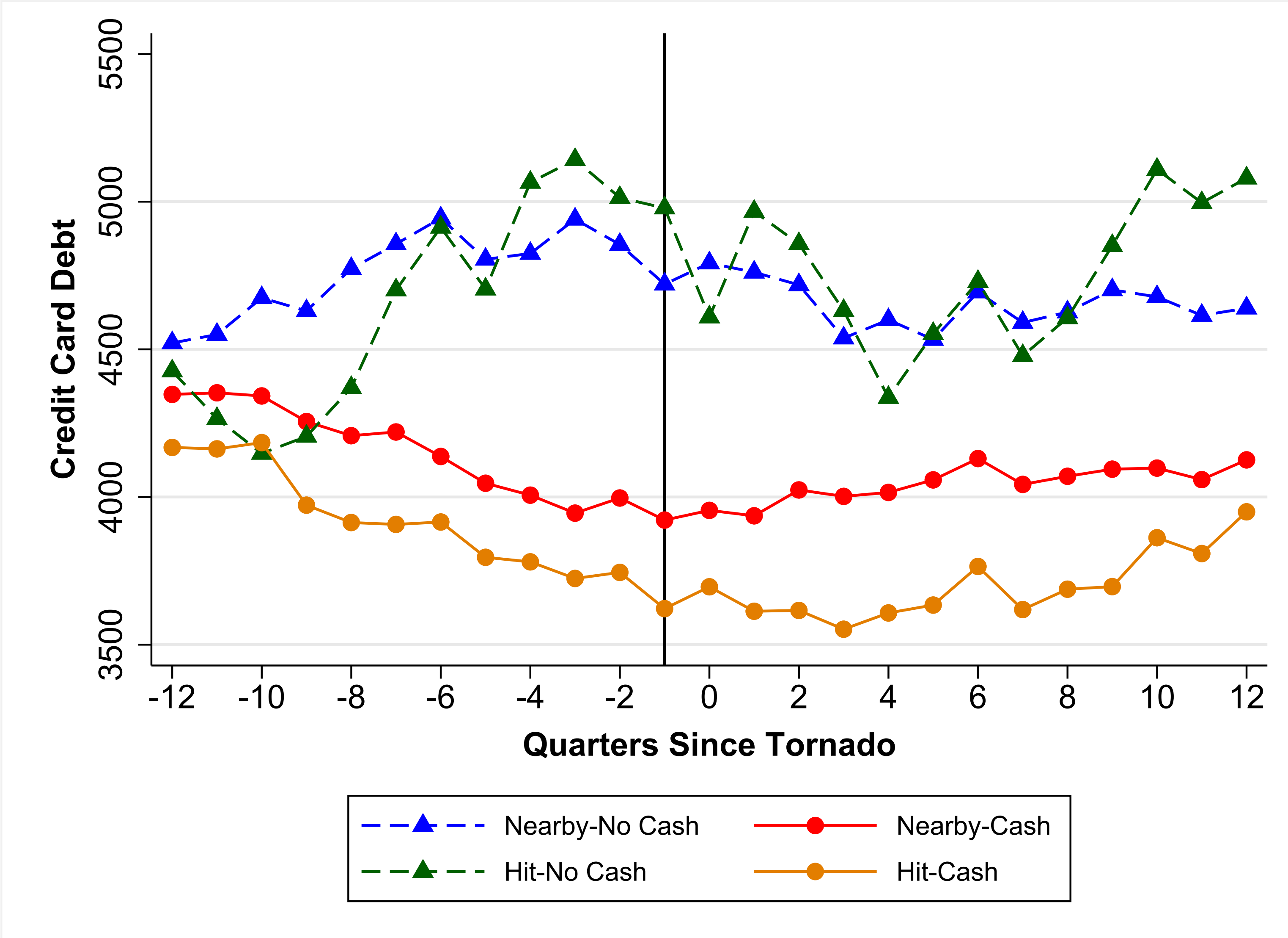
7 Figures and Tables

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



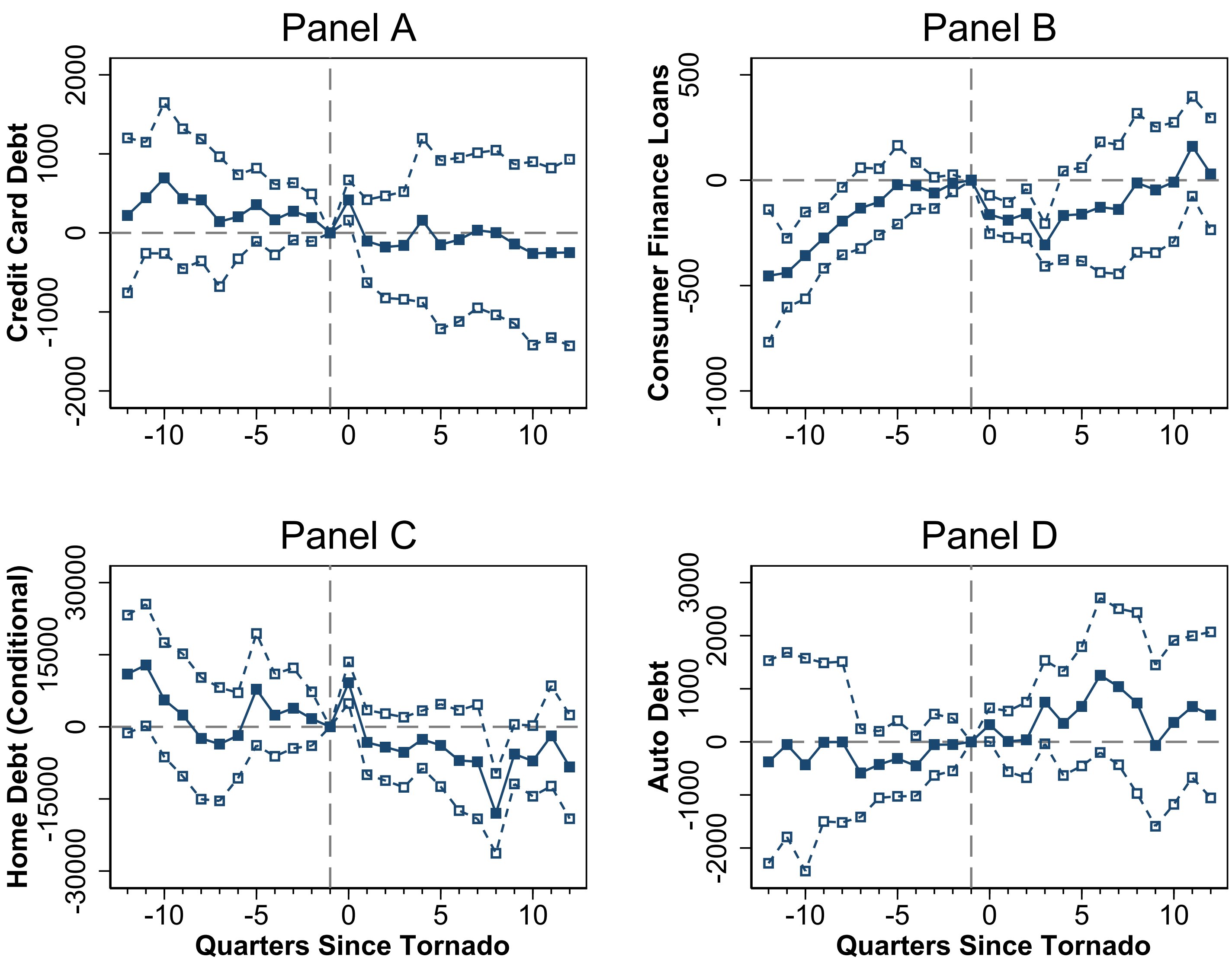
The figure shows the damage map for an EF5 tornado that hit Joplin, Missouri on May 22, 2011. The tornado path is outlined in black. The control area is in blue and located between 0.5 and 1.5 miles from the edge of the damage path. The tornado path and the control area are overlaid on a US Census block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

Figure 2: Trends in Credit Card Debt



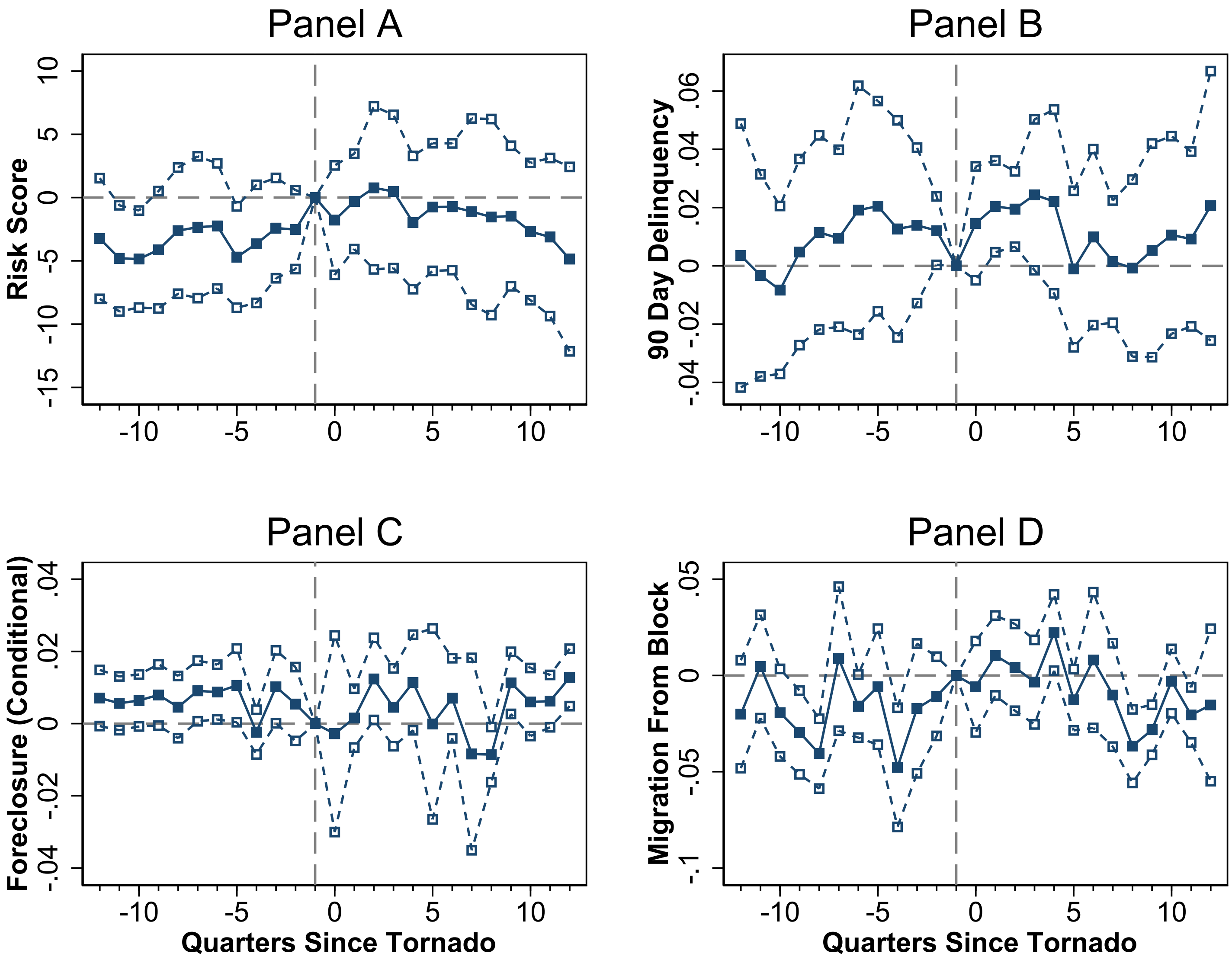
The figure plots the mean credit card balance of four groups of individuals: non-hit residents who lived in the 0.5 to 1.5 mile buffer area around the tornadoes that did not receive cash grants (dashed blue triangles), hit residents who lived in the damage path of tornadoes that did not receive cash 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: Quarterly Analysis of Debt



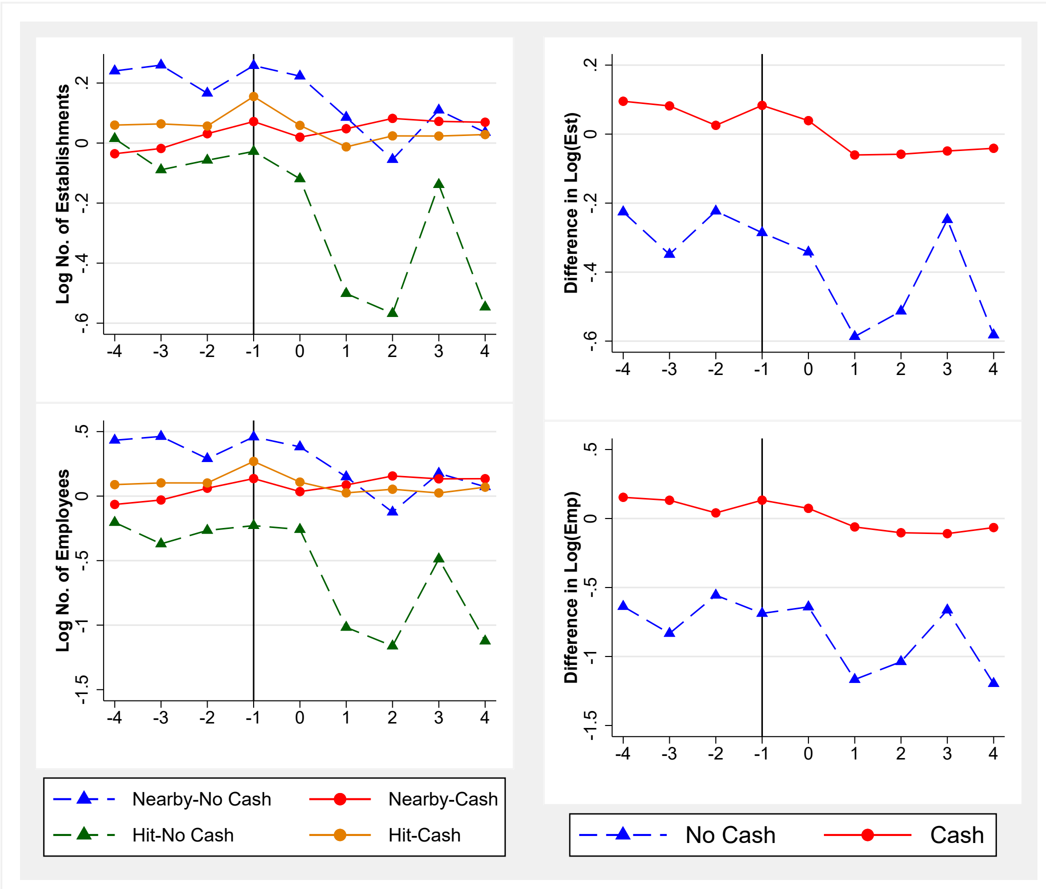
The figure shows quarterly event study estimates and 95% confidence intervals for the four debt outcomes in Table 3. The estimates are on the the same pooled triple difference model as in Table 3 panel A, except that the post tornado indicator variable is replaced with a set of binary variables that indicate the number of quarters the observation is either before or after the tornado. The last quarter before the tornado (dashed vertical line) is excluded and serves as the reference time period. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 4: Quarterly Analysis of Financial Outcomes



The figure shows quarterly event study estimates and 95% confidence intervals for the financial health and block migration outcomes in Table 3. The estimates are on the same pooled triple difference model as in Table 3 panel A, except that the post tornado indicator variable is replaced with a set of binary variables that indicate the number of quarters the observation is either before or after the tornado. The last quarter before the tornado (dashed vertical line) is excluded and serves as the reference time period. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Figure 5: Trends in Business Outcomes



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

Table 1: **Tornado Damage Characteristics**

Panel A: Overall Sample Characteristics

Total Number of Tornadoes	34
Presidential Disaster Declaration Tornadoes	
Public Assistance	25
Individual Assistance (Cash Grants)	25
Public Assistance and Individual Assistance	22
Tornado Damage Severity	
F5/EF5 Tornadoes	7
F4/EF4 Tornadoes	27
States hit by Tornado	15

Panel B: Characteristics by Assistance Status

	Cash Assistance	No Cash Assistance
	Mean (Median)	Mean (Median)
Disaster-Level		
Number of Counties in Disaster Declaration	34.8 (23)	7.1 (0)
Percent State Counties in Disaster Declaration	42.8 (29)	6.8 (0)
Public Assistance (Millions $)	114.6 (23.1)	7.8 (0.0)
Electoral Competitiveness of State	42.8 (41.9)	43.9 (44.1)
Tornado-Level		
Tornado F/EF Rating	4.3 (4)	4.0 (4)
Number of Damaged Blocks	381 (233)	58 (45)
Estimated Tornado Damage (Millions $)	513 (150)	53 (40)
Fatalities	19 (8)	2 (1)
Casualties	178 (59)	23 (13)
Block-Level		
Average Block F/EF Rating	1.39 (1.44)	0.84 (0.70)
Average Tornado Damage per Block (Millions $)	1.43 (0.60)	1.25 (0.48)

Tornadoes occur from 2002-2013. A Presidential Disaster Declaration event can include either Public Assistance and/or Individual Assistance. Public Assistance is allocated to communities to repair public infrastructure. Individual Assistance provides cash grants directly to residents. *Cash Assistance* includes information from the 25 Individual Assistance tornadoes (22 were also allocated Public Assistance). *No Cash Assistance* includes 3 tornadoes where Public Assistance was allocated and 9 tornadoes that were not part of a Presidential Disaster Declaration. Damages in 2010$. *Electoral Competitiveness* follows Reeves [2011] and measures the 2-way voteshare of the losing political party at the midpoint of our sample (2007) averaged over 3 presidential elections (2004, 2000, and 1996). Sources: Federal Emergency Management Agency, Tornado History Project, US Census, uselectionatlas.org

Table 2: **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 Bachelor's or Above	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 3: **Household Finance Triple Difference Estimates**

Dependent Variable:	Debt Level				Financial Health		
	Credit Card	Consumer Finance Loans	Home (Conditional)	Auto	Risk Score	90 Day Delinquency	Foreclosure (Conditional)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Pooled							
Cash Tornado x Post x Hit	-375	71	-7,738	775	1.7	0.005	-0.0017
	(346)	(107)	(5,058)	(607)	(3.5)	(0.017)	(0.0024)
Dependent Variable Mean	$3,622	$884	$149,584	$6,996	671.7	0.207	0.0049
R-squared	0.753	0.623	0.806	0.626	0.882	0.605	0.055
Observations	496,664	496,664	123,577	496,664	492,394	496,664	123,577
Panel B: Binned							
Cash Tornado x Post x Low	-638	-112	-7,168	1,151	-2.3	0.041*	-0.0053*
	(578)	(113)	(5,452)	(943)	(3.0)	(0.022)	(0.0032)
Dependent Variable Mean	$3,472	$887	$154,202	$7,018	672.6	0.209	0.0059
Cash Tornado x Post x Medium	144	380***	1,514	528	8.3**	-0.051***	0.0008
	(282)	(102)	(6,001)	(941)	(3.9)	(0.017)	(0.0045)
Dependent Variable Mean	$3,846	$898	$148,588	$7,690	672.7	0.206	0.0052
Cash Tornado x Post x High	-647*	-63	-39,550***	-409	1.3	0.000	0.0042***
	(363)	(78)	(4,450)	(642)	(2.4)	(0.011)	(0.0015)
Dependent Variable Mean	$3,700	$845	$134,135	$5,471	666.4	0.206	0.0000
R-squared	0.753	0.623	0.806	0.626	0.882	0.605	0.055
Observations	496,664	496,664	123,577	496,664	492,394	496,664	123,577

The table shows triple difference estimates for seven 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. The binned coefficients in panel B 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, except for those standard errors marked with a 1 which are clustered by census tract, and those marked with a 2 which are not clustered, * < 0.10, ** < 0.05, *** < 0.01. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 4: **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	0.008**	0.005	-0.001	-0.001
	(0.004)	(0.004)	(0.001)	(0.001)
Dependent Variable Mean	0.054	0.025	0.001	0.001
R-squared	0.104	0.099	0.039	0.039
Observations	763,557	763,557	763,557	763,557
Panel B: Binned Damage Levels				
Cash Tornado x Post x Low	0.008	0.009	-0.001	-0.001
	(0.006)	(0.008)	(0.002)	(0.000)
Dependent Variable Mean	0.048	0.022	0.000	0.000
Cash Tornado x Post x Medium	0.009*	0.001	0.001	-0.000
	(0.005)	(0.004)	(0.001)	(0.001)
Dependent Variable Mean	0.059	0.026	0.003	0.002
Cash Tornado x Post x High	0.004	-0.001	-0.002***	-0.001**
	(0.009)	(0.004)	(0.000)	(0.001)
Dependent Variable Mean	0.067	0.033	0.001	0.001
R-Squared	0.104	0.099	0.039	0.039
Observations	763,557	763,557	763,557	763,557

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 model includes individual and quarter fixed effects. Only the 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. The binned coefficients in panel B 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, * < 0.10, ** < 0.05, *** < 0.01. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 5: **Household Finance Triple Difference Estimates - Heterogeneity**

Dependent Variable:	Debt Level				Financial Health		Migration	
	Credit Card	Consumer Finance Loans	Home (Conditional)	Auto	90 Day Delinquency	Foreclosure (Conditional)	Move from Block	Move from County
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Available Credit								
Low Available Credit								
Cash Tornado x Post x Hit	205	124	-7,328	553	-0.001	-0.0078	-0.003	-0.002
	(166)	(130)	(6,698)	(518)	(0.056)	(0.0161)	(0.012)	(0.007)
Dependent Variable Mean	$392	$598	$126,903	$3,435	0.316	0.0000	0.052	0.022
Observations	152,282	152,282	13,439	152,282	152,282	13,439	246,992	246,992
High Available Credit								
Cash Tornado x Post x Hit	-836	-181	-12,494**	1,391	0.010	-0.0001	0.025***	0.020***
	(593)	(171)	(5,544)	(943)	(0.012)	(0.0014)	(0.008)	(0.003)
Dependent Variable Mean	$7,101	$558	$153,092	$8,447	0.039	0.0031	0.045	0.018
Observations	170,306	170,306	68,576	170,306	170,306	68,576	262,536	262,536
Panel B: Credit Score								
Low Credit Score								
Cash Tornado x Post x Hit	88	534**	13,984	1,152	-0.026	0.0024	-0.003	-0.007
	(272)	(237)	(8,539)	(811)	(0.053)	(0.0121)	(0.009)	(0.006)
Dependent Variable Mean	$1,900	$1,307	$127,929	$5,466	0.525	0.0179	0.071	0.032
Observations	161,501	161,501	21,380	161,501	161,501	21,380	246,314	246,314
High Credit Score								
Cash Tornado x Post x Hit	-742	-30	-13,329*	539	0.001	0.0003	0.023***	0.017***
	(507)	(64)	(7,355)	(714)	(0.001)	(0.0002)	(0.007)	(0.003)
Dependent Variable Mean	$3,204	$319	$162,278	$6,193	0.000	0.0000	0.031	0.013
Observations	165,502	165,502	55,395	165,502	165,502	55,395	245,063	245,063
Panel C: Age								
Young								
Cash Tornado x Post x Hit	-666	298**	17,545*	2,284*	0.011	-0.0049	-0.003	0.002
	(472)	(127)	(9,717)	(1,205)	(0.039)	(0.0050)	(0.009)	(0.010)
Dependent Variable Mean	$2,684	$850	$167,478	$7,315	0.289	0.0108	0.072	0.038
Observations	168,227	168,227	27,112	168,227	168,227	27,112	254,855	254,855
Old								
Cash Tornado x Post x Hit	118	-46	-15,779**	-100	0.011	-0.0008	0.009	0.006
	(434)	(161)	(6,103)	(962)	(0.019)	(0.0014)	(0.007)	(0.004)
Dependent Variable Mean	$3,571	$618	$119,737	$5,336	0.107	0.0054	0.034	0.013
Observations	162,810	162,810	39,133	162,810	162,810	39,133	261,768	261,768

The table shows triple difference heterogeneity estimates for eight of the same nine outcomes (omitting credit card) in Tables 3 and 4 using the same model as in Table 3 panel A. The difference is that the model is estimated separately on two groups of individuals (lower and upper terciles) based on available credit (panel A), Equifax Risk Score (panel B), and age (panel C). The three variables are measured in the last quarter before a tornado. Available credit is defined as the difference between total credit card debt and the total credit debt limit across all credit cards. The lower and upper tercile cutoffs for each of the variables are as follows: 40 and 58 for age, $5 and $8,026 for available credit, and 607 and 749 for Equifax Risk Score (TM). Dependent variable means are for the last quarter before a tornado. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by tornado, except for those standard errors marked with a 1 which are clustered by census tract, and those marked with a 2 which are not clustered, * < 0.10, ** < 0.05, *** < 0.01. Sources: Federal Reserve Bank of New York Consumer Credit Panel / Equifax (CCP), National Weather Service, US Census.

Table 6: **Estimates for the Number of Businesses 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: 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 weighted triple difference estimates of the effect of cash grants on the number of establishments and employment. Columns (3)-(6) show inverse propensity score weighted difference-in-differences model estimates separately for cash and no-cash tornadoes. The pooled coefficients in panel A consider a block as hit if more than 50% of the block is inside the tornado path. The binned coefficients in panel B 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 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, * < 0.10, ** < 0.05, *** < 0.01. Sources: Infogroup Historic Business Database, National Weather Service, US Census.

Table 7: **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	0.177*	0.285
	(0.102)	(0.170)
R-squared	0.560	0.552
Manufacturing		
Cash Tornado x Post x Hit	-0.006	-0.027
	(0.009)	(0.038)
R-squared	0.513	0.519
Panel B: Establishment Age		
New (1 year or less)		
Cash Tornado x Post x Hit	0.011	-0.002
	(0.014)	(0.019)
R-squared	0.379	0.317
Existing (4 years or more)		
Cash Tornado x Post x Hit	0.102	0.199
	(0.077)	(0.143)
R-squared	0.538	0.534
Panel C: Establishment Size		
Small (≤ 3 Employees)		
Cash Tornado x Post x Hit	0.131*	0.163*
	(0.072)	(0.090)
R-squared	0.544	0.529
Large (≥ 7 Employees)		
Cash Tornado x Post x Hit	0.077*	0.142
	(0.046)	(0.098)
R-squared	0.570	0.571

The table shows triple difference estimates using the same model as in Table 6 panel A, except that we limit the sample by establishment industry (panel A), age (panel B), and size (panel C). Each point estimate in the table is from a separate regression. 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, * < 0.10, ** < 0.05, *** < 0.01. Sources: Infogroup Historic Business Database, 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: Log Regression Estimate					
Cost Per Job ($1,000)	$128.44	$118.16	$110.72	$121.13	$93.15
Panel B: Levels Regression Estimate					
Cost Per Job ($1,000)	$73.72	$67.82	$56.00	$66.41	$42.80

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 7, panel C). 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].