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Natural Hazards, Disasters, and Demographic Change: The Case of Severe Tornadoes in the United States, 1980–2010

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Abstract
Natural hazards and disasters distress populations and inflict damage on the built environment, but existing studies yielded mixed results regarding their lasting demographic implications. I leverage variation across three decades of block group exposure to an exogenous and acute natural hazard—severe tornadoes—to focus conceptually on social vulnerability and to empirically assess local net demographic change. Using matching techniques and a difference-in-difference estimator, I find that severe tornadoes result in no net change in local population size but lead to compositional changes, whereby affected neighborhoods become more White and socioeconomically advantaged. Moderation models show that the effects are exacerbated for wealthier communities and that a federal disaster declaration does not mitigate the effects. I interpret the empirical findings as evidence of a displacement process by which economically disadvantaged residents are forcibly mobile, and economically advantaged and White locals rebuild rather than relocate. To make sense of demographic change after natural hazards, I advance an unequal replacement of social vulnerability framework that considers hazard attributes, geographic scale, and impacted local context. I conclude that the natural environment is consequential for the sociospatial organization of communities and that a disaster declaration has little impact on mitigating this driver of neighborhood inequality.

Keywords Natural disasters · Hazards · Neighborhoods · Environment · Vulnerability

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Introduction

Given the mounting evidence that links anthropogenic climate change to the increasing severity of extreme weather (Diffenbaugh et al. 2013, 2017), a pressing empirical question is how these environmental shocks affect American communities. One potential impact is on the demographic profile of neighborhoods. A multidisciplinary literature on the demographic consequences of natural hazards has developed, recently advancing beyond case studies to test generalizable hypotheses and refine theoretical concepts of vulnerability (e.g., Elliott and Pais 2010; Fussell et al. 2017; Logan et al. 2016). A primary conclusion of this literature is that vulnerability to hazards differs across demographic groups and is a driver of population change by making groups differentially likely to stay in place or move away in response to both direct impacts (e.g., housing damage) and indirect impacts (e.g., housing costs, availability, and labor markets) (Blaikie et al. 1994; Fothergill and Peek 2004; Fothergill et al. 1999; Pais and Elliott 2008). Despite this consensus and recent methodological advancements, contradictory evidence remains regarding both the direction and magnitude of the effect of natural hazards on local demographic change.

In terms of total population, some studies have found that the typical disaster leads to net population gain or an acceleration in total growth (Pais and Elliott 2008; Schultz and Elliott 2013). Other studies have documented a net out-migration (Boustan et al. 2017), a reduction in population growth (Logan et al. 2016), or even no change at all (Deryugina 2017). Many studies also have highlighted considerable effect heterogeneity (Elliott and Pais 2010; Fussell et al. 2017; Logan et al. 2016). Beyond the effects on total population, sociologists and demographers also model changes among various demographic subgroups. Although some have found a net out-migration of disadvantaged groups, such as racial minorities and impoverished individuals (Boustan et al. 2017; Elliott 2015), other studies have suggested that wealthy and White people move away and that lower-income and racial minorities are stuck in place (Logan et al. 2016). These disparate findings signal a need for further research with rigorous empirical strategies and refined theoretical models. In this study, I use a novel research design that assesses net demographic change after the apparent population of a particularly exogenous and understudied type of natural hazard—severe tornadoes (n = 1,016)—in 25 U.S. states from 1980 to 2009. In doing so, this research advances the literature in three ways.

First, given tornadoes' exogeneity and locals' inability to make residential place decisions based on tornado risk, I argue that my empirical case allows me to isolate the constitutive demographic elements of *social* vulnerability by controlling for *locational* vulnerability to natural hazards. These are two distinct concepts that scholars have theorized to drive demographic change but have yet to disaggregate empirically because patterned residential sorting around flood plains occurs in cases of hurricanes or floods, which is the focus of most previous research on post-disaster demographic change.

Second, my empirical analyses deploy causal inference methods and use the smallest geographic unit possible: census block groups. This choice represents an improvement on prior studies conducted using change score ordinary least squares (OLS) regression at the county level.

Third, I match the National Oceanic and Atmospheric Administration (NOAA) Severe Weather GIS database to the Federal Emergency Management Agency (FEMA) disaster database to identify the severe tornadic events declared by the U.S. President as a disaster, which releases federal aid that may moderate post-disaster change. Thus, to my knowledge this is the first study to empirically test how a presidential disaster declaration mitigates or exacerbates demographic change, net of magnitude.

Using a counterfactual causal inference framework, I find evidence that severe tornadic activity within a decade leads to a net increase in the White population with no observed effect on net changes in racial minority populations. Severe tornadoes also result in a net elevation in the socioeconomic status of impacted neighborhoods' populations in terms of both median income and families in poverty—a finding that is robust across models. The second stage of the analysis tests for effect heterogeneity along the attributes of both the impacted neighborhoods and the severe tornadoes themselves. Moderation models reveal that the net patterns of demographic change are most pronounced in socioeconomically advantaged places: already advantaged places become even more wealthy and White. Controlling for the magnitude of the tornado, I find no differences in the demographic implications for block groups that experienced a tornado declared as a disaster and those without a disaster declaration. I also find some evidence of dosage effects: the most severe tornadoes drive the observed changes. Finally, by exploring within-decade temporal variation, I provide some evidence that several years pass before effects become legible in local-level demographic estimates.

I interpret these findings as aligning with the displacement hypothesis, driven by a combination of an out-migration of socially vulnerable residents from affected block groups and a maintenance or in-migration of relatively more advantaged residents. In light of these findings and those from previous research, I advance a hybrid framework of unequal replacement of local residents after natural hazards, which suggests that critical variation in this process will be a function of hazard attributes, local context, and the geographic scale under consideration. In what follows, I first review the relevant literature on the demographic consequences of natural hazards and disasters in the United States. I then describe the research design, in which I use NOAA and FEMA data to plot the exact coordinates of severe tornado paths onto standardized block group boundaries to estimate net population change from four censuses: 1980, 1990, 2000, and 2010.

Natural Hazards and Demographic Change

Influenced by the findings of earlier case studies of extreme disasters (e.g., Bolin and Stanford 1998; Elliott and Pais 2006; Smith and McCarty 1996), the modal mechanism by which scholars hypothesize local post-hazard demographic change is migration: a combination of out-migration of exposed residents and in-migration of new residents. The literature relies heavily on the theoretical concept of vulnerability to make sense of heterogeneity in both baseline exposure to natural hazards and the post-event capacity to recover (Logan et al. 2016). A conceptual emphasis on vulnerability leads to three distinct expectations regarding the processes driving observed demographic change.

First, viewing communities as ecologically resilient, an *equilibrium* hypothesis predicts that communities will eventually return to a previously achieved equilibrium and experience no net change in demographic characteristics. The earliest sociological research on population effects of natural disasters found support for this thesis. Researchers have argued that places remained largely unchanged and rebounded within a few years to functional recovery (Cochrane 1975; Friesema et al. 1977). For example, Wright et al. (1979) found no difference in changes in the population composition between counties that experienced natural hazards in the 1960s and those that did not. Recent research has updated these analyses with newer data and more sophisticated methodologies, advancing two additional hypotheses.

The *concentration hypothesis* predicts that vulnerable (or disadvantaged) groups will be stuck in place and unable to deploy resources to move, whereas socially advantaged groups will relocate and even upgrade their residential circumstances. In a rigorous county-level analysis of demographic change after 32 hurricanes from 1970 to 2005, Logan et al. (2016) developed the concept of *segmented resilience*. They found that advantaged groups—specifically, Whites and young adults—are more likely to move away from counties where a hurricane struck. Disadvantaged groups—defined as Blacks and elderly residents—are more likely to be stuck in place. Similarly, consistent with a concentration hypothesis, other studies documented an increase in poverty rates in counties after natural disasters (Boustan et al. 2017; Smiley et al. 2018).

Finally, the *displacement hypothesis* suggests that environmental hardship displaces socially vulnerable people, whereas advantaged groups rely on savings, insurance, or wealth to remain in place and rebuild. The displacement hypothesis has also received empirical support and important refinements from other studies. For example, although Elliott (2015) found no racial differences in post-disaster migration, given preexisting inequalities in residential mobility, racial minorities become more mobile in the post-disaster period. In another study, Pais and Elliott (2008) studied three, billion-dollar hurricanes and developed the concept of places as *recovery machines*, which posits that post-disaster resources and power increase local populations and housing units but in unequal ways. They found an overall increase in total population in affected regions, but the neighborhoods that experienced the most damage became smaller and more White. Schultz and Elliott (2013) further supported the recovery machine hypothesis, showing that damage from natural disasters is associated with an increase in county-level median income but not with changes in poverty, which they interpreted as increasing socioeconomic polarization among residents.

Some studies have offered support for the three hypotheses under various circumstances, namely along dimensions of urbanicity (or population density) and past population trends. Elliott and Pais (2010) exploited the fact that Hurricane Andrew hit both urban Miami and more rural parts of Louisiana. They found that in more rural areas, long-term recovery concentrates disadvantaged residents, whereas recovery processes in urban areas tend to displace disadvantaged residents. Alternatively, Fussell et al. (2017) concluded that trends in past population and past hazards matter significantly for the magnitude and direction of hurricane and tropical storm hazards' impacts on population change. Finally, Logan et al. (2016) furthered their segmented resilience hypothesis by showing that the negative effects are exacerbated in more affluent counties. This heterogeneity suggests that further research must strategically address these factors, empirically and conceptually. In this article, I carefully tackle these prior issues by first addressing the theoretical drivers of change: vulnerabilities to hazards.

Social and Locational Vulnerability

In developing the three expected ways that community demographics change after natural hazards or disasters, scholars typically differentiate between two types of vulnerability:

locational and social (Logan et al. 2016). On the one hand, *locational vulnerability*—a concept developed by geographers that captures the "hazardousness of a specific place"—is the physical risk of experiencing an environmental hazard based solely on residential location, such as in a flood plain or directly on the coast (Cutter et al. 2000:731). On the other hand, *social vulnerability* refers to the sociodemographic characteristics (e.g., income, wealth, and social capital) of locals that render them differentially likely to respond to or to cope with an event (i.e., to display resilience) (Cutter et al. 2003; Finch et al. 2010).

Locational vulnerability is especially important in empirical analyses of hydrological hazards, such as floods, hurricanes, tropical storms, and some types of industrial hazards (Crowder and Downey 2010), given the well-documented processes sorting marginalized families into flood plains and environmentally hazardous areas and also sorting advantaged families to coastal properties. Prior research has documented that individuals, especially in flood-prone areas, make residential decisions based on flood data and risk perceptions (Kousky 2010). Thus, the demographic implications of such events will partially be endogenous to this type of patterned residential sorting along dimensions of race and socioeconomic status.

Disentangling social and locational vulnerability is therefore empirically difficult. Compounding this difficulty is the fact that many studies have used aggregated measures of damage from all forms of natural hazards, treating their damage as quantitatively *and* qualitatively equivalent (e.g., Schultz and Elliott 2013; Smiley et al. 2018). This conceals variation along hazard attributes, such as timing and scale, that may influence demographic response. For example, a localized hailstorm with little warning to residents will likely have different demographic implications than a tropical storm with four days' warning that extends across a large geographic region, even if both of these hypothetical hazards caused $40 million in property damage. Among recent studies acknowledging this heterogeneity and focusing on a specific hazard type, almost all analyzed hurricanes or tropical storms (e.g., Elliott and Pais 2010; Fussell et al. 2017; Logan et al. 2016; Pais and Elliott 2008). Studies that focus on hurricanes and tropical storms may also suffer from problems of selective sampling of counties, given that hurricane damage is concentrated along the coast. This is a context that lacks exploitable variation in several of the dependent variables of interest. For example, in terms of total population change, only about 10% of the 254 U.S. coastal counties experienced a net decline between 1960 and 2008 (Wilson and Fischetti 2010).

The empirical case of severe tornadoes is advantageous for several reasons. The primary reason relates to tornadoes' exogeneity, which addresses the challenge of isolating a conceptual driver. Tornadoes are spatially and temporally unpredictable, and they occur indiscriminately across space in the tornado-prone part of the United States. Tornadoes' exogeneity means that residents (1) cannot sort around where they expect a tornado may occur unless they move entirely out of the region, and (2) are not differentially likely to experience a tornado based on their spatial location.[1] Taken together, this means that the driver of the observed demographic change is social vulnerability because locational vulnerability is held constant. In my empirical analysis

[1] Although spatial location does not differentiate vulnerability in this case, residents' dwelling type will determine their level of vulnerability. This is a constitutive factor of social vulnerability here. Also, tornadoes occur less frequently in urban areas because cities occupy relatively less land area compared with suburbs and rural areas (Hall and Ashely 2008; Wurman 2008).

of block groups, tornadoes occur randomly, conditional on neighborhood size, given that larger block groups will be more likely to contain a tornado's path.

Tornadoes are also extremely difficult to forecast. The average *lead time*—the amount of time between a tornado warning and touchdown—is only 14 min (Zhang et al. 2018). Therefore, families and communities have little time to prepare or evacuate, effectively netting out the documented socioeconomic variation in evacuation timing related to other types of hazards (Elliott and Pais 2006; Groen and Polivka 2010). Those pre-event evacuations may be consequential for patterns of demographic change in other hazard cases if evacuees are differentially likely to return (e.g., Fussell et al. 2010). Finally, tornadoes are an extremely acute form of natural hazard. Whereas the spatial boundaries of other natural hazards are difficult to measure, tornadoes are relatively specific in a locale, and such precision aids in identifying the impacted places. I estimate change at the block group level, which more closely maps onto the scale of the hazard and therefore reduces the statistical noise involved by aggregating to higher geographic levels.

Given these theoretically and empirically useful attributes of severe tornadoes, it is surprising that very few contemporary demographic studies have analyzed them. The few studies that did are more than 10 years old and are associational. Rather than focusing on how places change in response to tornadoes, these studies analyzed the relationship between community-level attributes and tornado damage outcomes, such as fatalities and injuries (Aguirre et al. 1993; Ashley 2007; Donner 2007). A classic scientific study of tornadoes is Moore's (1958) *Tornadoes Over Texas: A Study of Waco and San Angelo in Disaster*. The study made visible the various difficulties that cities face in rebuilding and described how the communities came together in the aftermath of the tornadoes. Stallings (2002) revisited Moore's original findings and suggested that greater attention can be paid to the social inequalities that produced recovery trajectories. My analysis adopts a similar emphasis on inequality (also see Tierney 2006) and applies it specifically to demographic change. Importantly, since the Moore (1958) study, disaster aid policies in the United States have changed considerably.

Presidential Disaster Declarations and FEMA

Under the Stafford Disaster Relief and Emergency Assistance Act (1988), upon the request from a state's governor, the President of the United States can issue a major disaster declaration, which releases federal funds from FEMA in the aftermath of an event. Political scientists have analyzed the political conditions under which presidents declare a disaster and found that concerns for electorate and political partisanship have a direct effect on whether a president grants a governor's request for FEMA aid (Garrett and Sobel 2003; Reeves 2011). Because declarations can release several streams of aid, including in the form of grants from FEMA's Individual Assistance (IA) Program and low-interest loans from the U.S. Small Business Administration's disaster loan program, individuals and families who may have otherwise been forced to relocate can apply for aid to repair their homes.[2] FEMA assistance cannot be received on top of

[2] A disaster declaration may include provisions for public assistance and/or hazard mitigation. I use a parsimonious indicator for declarations and include any of the provisions. This captures aid for direct housing damage and indirect damage that assists public works or local businesses but not with respect to aid from community organizations and churches.

insurance payouts, though. Homeowners insurance policies often include damage from tornadoes, so FEMA assistance in these cases will likely be collected by uninsured or underinsured locals. Thus, one might expect that the demographic implications differ for the natural hazards that are declared disasters.

Disaster researchers have traditionally differentiated *hazards*—dangerous risks that have the potential to wreak havoc—from *disasters*—nonroutine physical events that overwhelm a social system (Kreps and Drabek 1996; for a thorough discussion, see Tierney 2019). Recent research, however, has also referred to extreme weather events as *natural hazards* because they have become more commonplace and may not necessarily entirely disrupt social systems (e.g., Elliott 2015; Fussell et al. 2017; see also, Clark and Knightley 2013; Hazards and Vulnerability Research Institute 2015; Intergovernmental Panel on Climate Change (IPCC) 2012). Operationalization of the two concepts in empirical models can be especially difficult and is subject to researcher discretion. For clarity, I use the terms in a nested way. The events studied here are all tornadic *hazards* because they are weather-related events with disaster potential but may not necessarily disrupt local social systems (although they may feel like disasters to those directly impacted). A subset of those hazards may then be considered *disasters*, which I designate as those of sufficient scale to invoke a Presidential disaster declaration. This empirical operationalization is ideal for the purpose of this analysis given that it is the first study to investigate how a disaster declaration mitigates or exacerbates the effects of natural hazards on demographic change. The trade-off of using a more bureaucratic definition of a disaster is that some tornadic hazards that do overwhelm a social system will not be operationally defined as such.

Data and Methods

To examine the effects of severe tornadoes on local demographic change, I use data from three sources: (1) the NOAA Severe Weather GIS database (SVRGIS), (2) the FEMA Presidential Disaster Declaration Database (PDD), and (3) the U.S. Census from GeoLytics CDs (GeoLytics, Inc. 2010).

The SVRGIS contains the exact geographic location of the population of tornado paths in the United States, linking the latitude and longitude coordinates for the touchdown and dissipation points with the width in yards (Smith 2006). The full SVRGIS data set includes information on tornadoes from 1950 to 2017. Studies have documented that even though the quality of the reported data has improved over time, the data set likely suffers from underreporting of less severe tornadoes and those in very rural areas (Verbout et al. 2006).[3] A tornado's magnitude is estimated at several points along its path, and the entire path is assigned the highest damage rating.[4] The climatology of severe tornadoes means that this measurement error is random; the

[3] For more information on the construction of SVRGIS, see Murphy (2018) and Edwards (2018). Kurdzo et al. (2017) compared tornado data from SVRGIS with data from Atmospheric Imaging Radar for several outbreak cases, and they found that the data sets closely align in space and time but that the SVRGIS data miss several short-lived, smaller tornadoes.

[4] For example, if the tornado path had F4 wind speed for only one-half mile along the path and F3 wind speed for three miles, it would be assigned an F4. For a discussion of tornado intensity assessment, see Strader et al. (2015).

gradient of magnitude along a tornado path will be orthogonal to local characteristics and will not be systematic across tornadoes. Despite these important caveats, the SVRGIS is still superior to other data sets of natural hazards (Clark and Knightley 2013), but the results should be interpreted with these limitations in mind (e.g., Simmons and Sutter 2011).

The indicator of tornado magnitude is based on the Fujita scale used during 1980–2006 and the Enhanced Fujita (EF) scale used from 2007 (Fujita 1971; McDonald and Mehta 2004). F3 tornadoes are considered *severe*, with wind speeds between 158 to 206 mph; F4 are considered *devastating*, with wind speeds from 207 to 260 mph; and F5 are considered *incredible*, with wind speeds exceeding 261 mph (Fujita 1971; McDonald and Mehta 2004). I restrict the analysis to only those tornadoes with a magnitude of F3, F4, or F5 because less extreme tornadoes do not have the destructive capabilities theorized to effect demographic change.

The FEMA PDD data are at the county level and include information on the disaster type and date. Therefore, to identify the severe tornadoes federally declared as a disaster, I match SVRGIS and FEMA data using three pieces of information: (1) hazard type (i.e., tornado), (2) location, and (3) date. For more information on the matching procedure, see the online appendix, section A.

For each of the three decades, I plot the tornado paths onto standardized block group boundaries, projected on the same coordinate reference system (WGS84), to construct a block group × decade data set. Block groups are nested within census tracts and contain between 600 and 3,000 people. Demographic data at the block group level come from 1980, 1990, and 2000 decennial censuses, acquired from GeoLytics CDs, and 2010 data come from the American Community Surveys (ACS) (2008–2012, five-year estimates). I follow the existing literature regarding the relevant sociodemographic variables (Logan et al. 2016). I collect demographic data on seven dependent variables: (1) total population, (2) young adult population, aged 20–35, (3) non-Hispanic Whites, (4) non-Hispanic Blacks, (5) Hispanics of any race, (6) families living in poverty, and (7) median family income. For each block group × decade, I log the outcome count variables; for block groups with 0 on any outcome, I take the log of 0.01.

In the 25 U.S. states, there are 116,420 populated block groups with housing units, and I identify the 6,018 block groups that are *treated*—that is, those that experienced a severe tornado—across the 30-year period.[5] I correct for baseline differences between the treated and control block groups using coarsened exact matching (CEM), a preprocessing, nonparametric method for correcting for imbalance on observable pretreatment characteristics between the treated and control block groups (Imbens 2000). The procedure matches each treated unit with a control unit that seeks balance on an *ex ante* defined set of covariates (Blackwell et al. 2009; Iacus et al. 2012). I match the treated and control block groups at their 1980 levels on 14 variables.

[5] I restrict my analysis to the 25 states in which at least one severe tornado occurred in each of the three decades. Twelve other states (AZ, CT, CO, FL, MA, MD, MT, NJ, NY, UT, WV, and WY) experienced a severe tornado in the 30-year period, but in these states, severe tornadoes were extremely rare (five or fewer severe tornadoes) and were only F3 in magnitude.

The variables include the seven demographic outcomes (listed earlier) and seven additional variables demonstrated by prior research to be important for population change: (1) total number of occupied housing units, (2) median housing age, (3) rented units, (4) land area, (5) rural population, (6) number of manufactured homes or trailer units, and (7) population trend from 1970 to 1980.[6] All variables are matched at the block group level, but supplemental analyses show that the matching strategy also significantly corrects for imbalance in county-level demographics. Thus, matching allows for regional-level comparability. The matched columns in Table 1 demonstrate that my matching strategy significantly improves balance on the observable baseline covariates and leads to only slight loss of data due to pruning (loss of 14.7% treatment block groups).[7]

The columns labeled Unmatched in Table 1 present the descriptive statistics of the unweighted demographic characteristics of the block groups based on treatment. The descriptive statistics show considerable baseline differences in the treated and control block groups, such that treated block groups generally are more populated, have fewer young people, are more White, are less racially diverse, are more rural, are poorer, are larger, and have more manufactured homes. This imbalance relates mostly to the fact that block groups are differentially likely to be treated because of their size. The matched sample is balanced considerably on observable characteristics, so I use the weights provided by CEM to estimate my models for a counterfactual framework.

Using the matched sample, I estimate a series of difference in difference models using unit and time fixed effects, as well as lead and lag treatment terms, to isolate within–block group change. Formally, I estimate the model as follows:

$$y_{jt} = \beta_1 T_{jt} + \beta_2 T_{j,t-1} + \beta_3 T_{j,t+1} + X_t + D_j + \varepsilon_{jt}, \tag{1}$$

where j and t are indices of block groups and decade, respectively; y_{jt} is one of seven population outcomes of interest (e.g., log of total population); and β_1 is the estimate of the treatment effect of a severe tornado. T_{jt} is a dummy variable coded as 1 if the block group experienced a tornado during the decade, and 0 otherwise. X_t and D_j are time- and block group–level fixed effects that capture aggregate time trends and time-invariant, block group–level differences. $T_{j,\,t-1}$ and $T_{j,\,t+1}$ are lead and lag treatment (dummy) variables, respectively, which identify changes before (lead) a tornado occurs and after (lag) a tornado occurs (Shadish et al. 2008).[8] The main assumption of my model holds that in the absence of a severe tornado, the observed demographic change for treated block groups would have been the same as the change in the unaffected

[6] The 1970 to 1980 population trend is calculated at the census tract level and interpolated for the block groups within the census tracts. Missingness is a value to be matched on.

[7] I also exclude the block groups that experienced a tornado in the 1970s (n = 3,226) to ensure a clear temporal pattern for the matching and later treatment.

[8] The lead term equals 1 for a block group the decade before it experiences a severe tornado, and the lag term equals 1 for the decade after (Angrist and Pishke 2008:237). For example, for a block group that experienced a tornado in the 1990s, the treatment term is coded 1 for the 1990s, the lead term is coded 1 for the 1980s, and the lag term is coded 1 for the 2000s. Lag term for the 1980s identifies treated block groups from 1970 to 1979. Lead for the 2000s identifies treated block groups from 2010 to 2016.

Table 1 Descriptive statistics for the unmatched and matched samples

	Unmatched			Matched		
1980 Variable	Control	Treatment	Difference	Control	Treatment	Difference
Total Population	1,074	1,120	46***	1,027	1,033	6
	(556)	(512)	(7)	(399)	(391)	(6)
Young Adult	277	263	–13***	243	240	–3*
	(188)	(163)	(2)	(107)	(104)	(2)
White	871	955	84***	903	907	5
	(509)	(463)	(7)	(399)	(390)	(6)
Black	146	134	–12***	100	101	2
	(354)	(283)	(5)	(211)	(209)	(3)
Hispanic	43	17	–26***	14	12	–2*
	(153)	(71)	(2)	(38)	(35)	(1)
Median Family Income	20,238	17,673	–2,565***	18,174	18,079	–95
	(6,556)	(5,126)	(86)	(5,094)	(5,050)	(70)
Family Poverty	28	37	9***	30	31	1*
	(35)	(32)	(0)	(24)	(24)	(0)
Rural Population	322	691	369***	631	644	13*
	(467)	(540)	(6)	(505)	(497)	(7)
Housing Units	413	436	23***	395	397	2
	(207)	(200)	(3)	(155)	(153)	(2)
Rented Units	122	99	–23***	88	86	–2
	(125)	(92)	(2)	(64)	(62)	(1)
Median Housing Age	1957	1957	0	1957	1957	0
	(0)	(0)	(0)	(12)	(12)	(0)
Manufactured Homes	20	35	14***	31	31	0
	(31)	(33)	(0)	(28)	(27)	0
Land Area	11	45	34***	21	32	11**
	(51)	(97)	(1)	(44)	(49)	(1)
1970–1980 Population Change	0.151	0.152	0.001	0.153	0.153	0.00
	(0.225)	(0.170)	(0.000)	(0.163)	(0.162)	(0.000)
N (block groups)	110,402	6,018	116,420	59,098	5,135	64,233

Note: Values in parentheses are standard deviations.

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$ (two-sample t test)

block groups. I address this assumption by including 1970–1980 population change in my matching technique. I extend this model in several ways, using a different variable for T_{jt}, to test for differences based on disaster declaration, dosage effects, and timing effects. A moderation model of effect heterogeneity extends Eq. (1) by interacting T_{jt} with a z score for median income. All models are estimated on samples that use multiple imputation to address the 2% missing data for family poverty and median income from the ACS.

Table 2 Tornado-level descriptive statistics

	1980s	1990s	2000s
Magnitude			
F3	282	320	257
F4	61	82	44
F5	3	10	2
Fatalities, Mean	1.14	1.00	1.28
(SD)	(3.13)	(3.64)	(3.19)
[Range]	[0, 30]	[0, 36]	[0, 24]
Injuries, Mean	21.13	16.21	16.40
(SD)	(50.65)	(45.98)	(38.23)
[Range]	[0, 463]	[0, 583]	[0, 350]
Length in Miles, Mean	16.20	17.48	18.41
(SD)	(17.19)	(18.08)	(16.86)
[Range]	[0.10, 134]	[0.20, 160]	[0.70, 121.84]
Width in Yards, Mean	412.71	529.72	647.42
(SD)	(451.45)	(499.81)	(550.01)
[Range]	[10, 3,330]	[10, 2,640]	[50, 4,400]
Property Damage (1–9)	5.82	5.64	6.31
(SD)	(1.27)	(1.50)	(0.95)
Federally Declared Disasters	81	158	128
(%)	(23)	(38)	(42)
Total Affected Counties	582	656	582
Total Affected Block Groups	2,128	2,443	1,785
N	346	412	303

Results

Descriptive Statistics

Table 2 presents the descriptive statistics of the severe tornadic activity grouped into the three decades in my study: 1980–1989, 1990–1999, and 2000–2009. There were 346 tornadoes in the 1980s, 412 tornadoes in the 1990s, and 303 tornadoes in the 2000s.

On average, severe tornadoes each killed just over 1 person and injured approximately 18 people. With each successive decade, there is an increase in the average length and average width of severe tornadoes. In the 1980s, the average severe tornado was just over 15 miles long and 413 yards wide; comparatively, in the 2000s, the average severe tornado was more than 18 miles long and 647 yards wide. In terms of property damage, severe tornadoes cause, on average, between \$500,000 and \$5,000,000 in estimated property damage.[9] On average, 35% (n = 367) of severe

[9] From 1980 to 1996, property loss information is in nine categories: <\$50; \$50–\$500; \$500–\$5,000; \$5,000–\$50,000; \$50,000–\$500,000; \$500,000–\$5,000,000; \$5,000,000–\$50,000,000; \$50,000,000–\$500,000,000; and >\$500,000,000. For comparability, I collapse post-1996 data into the same categories.

tornadoes were defined as a disaster as part of a presidential declaration. In each successive decade, an increasing fraction of severe tornadoes were declared a disaster, activating federal resources for recovery efforts. This likely reflects two trends: (1) an increase in the intensity of tornadic activity, and (2) variation and an overall increase in the use of the Stafford Act, releasing FEMA aid.

In the tornado-prone part of the United States, severe tornadoes occurred in 582 U.S. counties in the 1980s, 656 counties in the 1990s, and 582 counties in the 2000s. Within these counties, approximately 2,000 block groups were affected by a severe tornado in a decade, on average. The greatest number of block groups affected in a decade was 2,443 in the 1990s. The geographic location of the population of severe tornado paths is provided in Fig. 1, with panels a–c corresponding to each of the three decades. See the online appendix, section D, for a version with color-coded tornado tracks.

Contrary to popular opinion, consistently severe tornadic activity does not occur exclusively within the four or five states commonly associated with so-called Tornado Alley, but instead extends across a large swath of the United States, bound by the Rocky Mountains in the west to the Atlantic coast to the east. It includes areas in the Great Plains, the Midwest, the Sunbelt, the Ozarks, the Mississippi Delta, the Black Belt, and the Rust Belt. Within this tornado-prone area, I calculate that 1 in every 50 people will experience a severe tornado in the specific block group of their neighborhood by the end of a decade. In other words, on average, roughly 2.4 million Americans live in a block group that will experience a severe tornado in a given decade.

Severe Tornadoes and Demographic Change

The primary estimates of the effect of a severe tornado on demographic change are presented in Table 3. Results suggest no statistically meaningful effect of a severe tornado on change in the total population size or in young adults. In terms of race, I also detect null effects for average changes in Black and Hispanic populations. However, I find that severe tornadoes lead to an average net increase in the White population by 4%, but the lead term suggests that localities may be undergoing increased change prior to the tornado. The median income of post-disaster residents increases by 1.6%, and the number of families living in poverty decreases by 8.8%. These findings provide first evidence that in net demographic terms, block groups become, on average, more White and more socioeconomically advantaged due to a severe tornado.

In sections B and C of the online appendix, I conduct several sensitivity analyses. To ensure the aforementioned findings are not due to random chance, I conduct a placebo test by predicting past decade demographic change as a function of future tornado activity. As expected, I find all null effects (see the online appendix, section B). I also repeat all stages of the preceding analysis at the census tract level, which shows substantively similar but muted effects (see the online appendix, section C).

Effect Heterogeneity by Neighborhood and Tornado Attributes

Table 4 tests for effect heterogeneity along neighborhood socioeconomic status. The model takes the form of Eq. (1) and adds an interaction between the z score for median income with the treatment variable. Thus, the interpretation of the interaction term is the effect of a severe tornado for every 1 standard deviation change in median income.

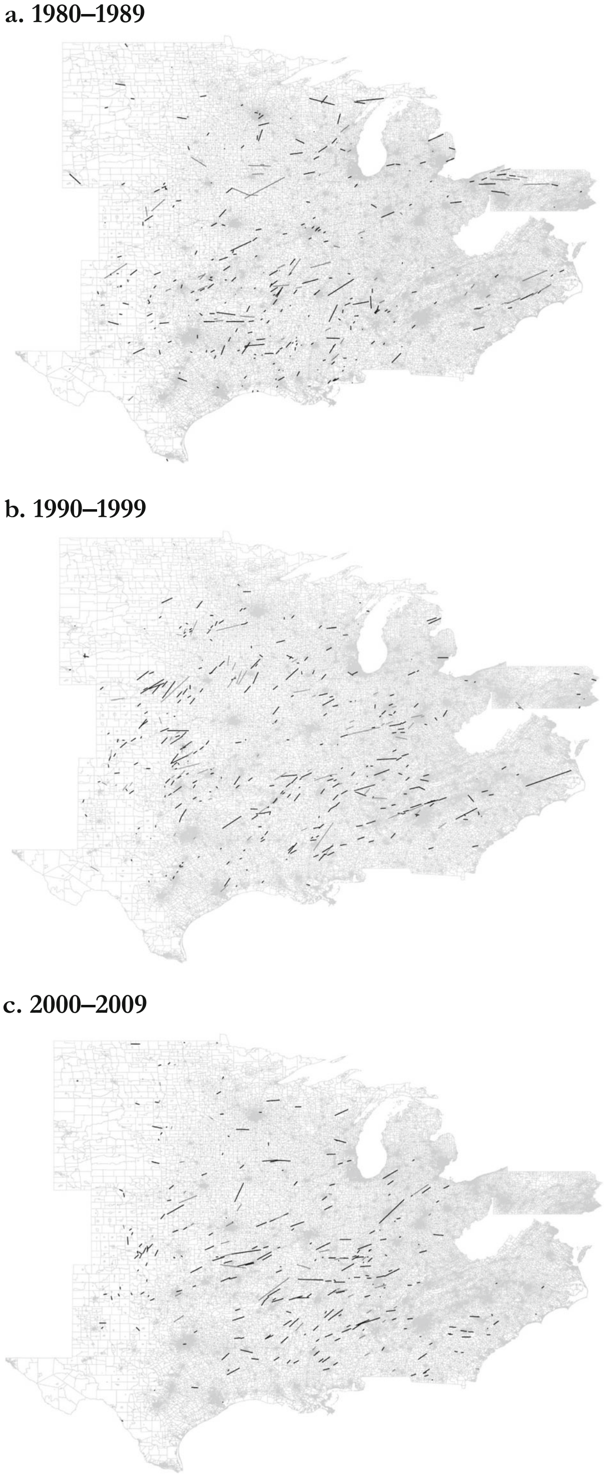

Fig. 1 Three decades of severe tornadic activity in the United States, 1980–2009

The results show that the effects on the White population and impoverished families vary significantly based on the impacted block group median income.

Specifically, a severe tornado in a block group with an average median income leads to a 4.0% increase in the White population. The effect of a severe tornado in a block group with a median income 1 standard deviation above the average is a 13.7% increase in the White population (0.040 + 0.097 = 0.137). Interestingly, the effect approaches 0 and becomes negative for a treated block group with 1 standard deviation below the average median income, a 5.7% decrease in the local White population (0.040 – 0.097 = –0.057).

The results on total population suggest no meaningful difference in change after a tornado if the impacted block group has an average median income. However, if the impacted block group is 1 standard deviation above the average median income, the model predicts an average local population increase of about 8.8% (0.087 + 0.001 = 0.088). In terms of impoverished families, the model predicts an 8.3% decrease for an impacted neighborhood with average median income but a 32.6% decrease in the impoverished families if the impacted block group is 1 standard deviation above the average median income (–0.083 – 0.243 = –0.326). The models also predict significant interactions for changes in the young adult population and Hispanic populations. In fact, the only nonsignificant interaction is for changes in the Black population. Thus, by testing for effect heterogeneity, the results show that post-hazard unequal replacement varies by socioeconomic status and that disadvantage displacement is exacerbated in neighborhoods with higher socioeconomic status. They bolster others' conclusions that show significant variation in hazards' impacts based on socioeconomic status and that local coalitions exploit hazards to promote growth in advantaged neighborhoods (Logan et al. 2016; Pais and Elliott 2008).

Table 3 Robust difference-in-difference estimates of tornadoes on block group–level net demographic change, CEM matched sample (1980–2010)

	Total Population	Young Adult	White	Black	Hispanic	Family Poverty	Median Family Income
Tornado	–0.001	–0.008	0.040***	–0.040	0.028	–0.088**	0.016***
	(0.009)	(0.009)	(0.009)	(0.042)	(0.047)	(0.035)	(0.004)
Tornado Lead	0.015†	–0.002	0.059***	0.018	0.047	–0.067	0.034***
	(0.008)	(0.010)	(0.010)	(0.048)	(0.048)	(0.043)	(0.005)
Tornado Lag	0.007	0.005	0.024**	–0.108**	–0.125**	–0.000	0.007*
	(0.007)	(0.008)	(0.008)	(0.042)	(0.045)	(0.031)	(0.004)
Block Group Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	.507	.515	.684	.701	.434	.345	.619
N (block group × decade)	256,918	256,918	256,918	256,918	256,918	256,918	256,918

Notes: All models are weighted using coarsened exact matching. Huber-White robust standard errors are shown in parentheses.

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

Table 4 Heterogeneous treatment effects by median income on block group–level net demographic change, CEM matched sample (1980–2010)

	Total Population	Young Adult	White	Black	Hispanic	Family Poverty
Tornado × Median Income	0.087**	0.054*	0.097***	0.051	0.147*	–0.243***
	(0.029)	(0.025)	(0.026)	(0.054)	(0.059)	(0.058)
Median Income	0.154***	0.077*	0.250***	–0.017	0.051	–0.774***
	(0.038)	(0.031)	(0.022)	(0.038)	(0.029)	(0.073)
Tornado	0.001	–0.006	0.040***	–0.036	0.038	–0.083*
	(0.008)	(0.009)	(0.009)	(0.043)	(0.044)	(0.036)
Tornado Lead	0.006	–0.007	0.044***	0.020	0.046	–0.019
	(0.008)	(0.010)	(0.010)	(0.048)	(0.048)	(0.042)
Tornado Lag	0.005	0.005	0.021*	–0.108*	–0.125**	0.009
	(0.006)	(0.007)	(0.008)	(0.042)	(0.045)	(0.030)
Block Group Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y
Adjusted R^2	.578	.549	.705	.700	.434	.371
N (block group × decade)	256,918	256,918	256,918	256,918	256,918	256,918

Notes: All models are weighted using coarsened exact matching. Huber-White robust standard errors are shown in parentheses.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

To test whether a presidential disaster declaration moderates demographic change after a severe tornado, I again extend Eq. (1). The treatment variable is categorical, coded 0 for a control block group, 1 for an impacted block group without a PDD, and 2 for an affected block group with a PDD. Because tornadoes declared as a disaster are more likely to be greater in magnitude, all models control for the magnitude of a tornado (a categorical variable coded 3, 4, or 5 corresponding to the Fujita or EF scale). I am primarily interested in comparing the outcomes for block groups that experienced a hazard compared with a disaster, and thus the reference category is the block groups that experienced a severe tornadic event without a disaster declaration.

The first row of Table 5 presents the estimates that compare the demographic change for a block group that experienced a disaster-declared tornado with the change experienced by block groups that experienced a tornadic hazard that was not declared a disaster. Both the magnitude and statistical significance of the point estimates suggest that there is no difference in the demographic implications between a disaster declaration and a nondeclared tornado, net of magnitude. Therefore, I conclude that these models provide evidence to a first approximation that FEMA aid does not have a detectable impact on mitigating processes of demographic change.

My final test for effect heterogeneity is by tornado magnitude. I estimate Eq. (1) with a treatment variable that is a three-part categorical variable corresponding to EF scale magnitude. The first row in Table 6 shows that the most severe tornadoes drive much of the observed change of the White population and that the change in median income and family poverty spans across magnitude. Comparing the effect sizes down each column

Table 5 Robust difference-in-difference estimates comparing the effects of disaster-declared and nondeclared tornadoes on block group–level demographic change, CEM matched sample (1980–2010)

	Total Population	Young Adults	White	Black	Hispanic	Family Poverty	Median Family Income
Treatment (ref. = tornado, no PDD)							
Tornado, PDD	–0.010	0.001	–0.015	0.080	0.056	0.009	0.004
	(0.012)	(0.013)	(0.014)	(0.074)	(0.082)	(0.060)	(0.006)
No tornado	–0.050*	–0.027	–0.092***	0.061	0.035	0.080	0.015
	(0.023)	(0.030)	(0.024)	(0.209)	(0.213)	(0.016)	(0.014)
Block Group Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Tornado Lead	Y	Y	Y	Y	Y	Y	Y
Tornado Lag	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	.507	.515	.671	.696	.434	.345	.645
N (block group × decade)	256,918	256,918	256,918	256,918	256,918	256,918	256,918

Notes: Models control for treatment magnitude on the Fujita scale. Huber-White robust standard errors are shown in parentheses.

*p < .05; ***p < .001 (two-tailed tests)

offers additional support for dosage effects, such that the coefficients for the F5 tornadoes are consistently the largest. However, they are estimated more imprecisely because of their rarity, as evidenced by the larger standard errors.

Temporal Effects on Demographic Change

Finally, the preceding analyses rely on tornadic activity that can occur between a few months and nine years before the observed outcomes. To interrogate the temporal nature of these processes of demographic change, I estimate models with a treatment variable coded 1–10 according to the number of years away from the measured census; for example, a block group that experienced a severe tornado in 1981 would be assigned a 9, corresponding to nine years away from the 1990 observed change between 1980 and 1990. I also include a squared term for the number of years.

Table 7 presents the results and shows evidence of a curvilinear effect of timing on demographic change. The greater number of years that transpire between a severe tornado and the next census, the model detects less of an effect on net demographic change. This finding makes sense in light of the research that documents residential instability in the immediate post-disaster stage and that several years pass before population stabilization (Curtis et al. 2015; Fussell et al. 2010). If enough time passes, other changes may occur that confound the permanent effects of the natural hazard or disaster. In other studies, for example, Logan et al. (2016) suggested that effects on counties last for three years.

Table 6 Robust difference-in-difference estimates of dosage effects on block group–level net demographic change, CEM matched sample (1980–2010)

	Total Population	Young Adults	White	Black	Hispanic	Family Poverty	Median Family Income
Tornado Magnitude							
F5	0.044†	0.028	0.083***	–0.010	0.001	–0.076	–0.012
	(0.022)	(0.029)	(0.023)	(0.204)	(0.207)	(0.155)	(0.013)
F4	0.002	–0.002	0.037*	–0.085	–0.150†	–0.116†	0.021**
	(0.016)	(0.016)	(0.017)	(0.075)	(0.085)	(0.057)	(0.006)
F3	–0.004	–0.012	0.039***	–0.027	0.088†	–0.089*	0.015**
	(0.009)	(0.010)	(0.010)	(0.047)	(0.050)	(0.039)	(0.004)
Block Group Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Tornado Lead	Y	Y	Y	Y	Y	Y	Y
Tornado Lag	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	.507	.515	.671	.701	.434	.362	.619
N (block group × decade)	256,918	256,918	256,918	256,918	256,918	256,918	256,918

Notes: Huber-White robust standard errors are shown in parentheses. All models include lead and lag terms for treatment (not shown).

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

Conclusion

This study conducts the first block group–level analysis of the effect of natural hazards and disasters on net demographic change in the United States. I apply

Table 7 Robust fixed-effects estimates of tornado timing on block group–level demographic change, CEM matched sample (1980–2010)

	Total Population	Young Adults	White	Black	Hispanic	Family Poverty	Median Family Income
Treatment Timing	0.002	0.007	0.018**	0.031	–0.041	–0.031	0.010***
	(0.005)	(0.005)	(0.005)	(0.025)	(0.028)	(0.021)	(0.002)
Treatment Timing, Squared	–0.000	–0.001†	–0.001*	–0.004	0.006†	0.003	–0.001***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.000)
Block Group Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Tornado Lead	Y	Y	Y	Y	Y	Y	Y
Tornado Lag	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	.507	.515	.671	.696	.434	.345	.645
N (block group × decade)	256,918	256,918	256,918	256,918	256,918	256,918	256,918

Notes: Models control for tornado magnitude on the Fujita scale. Huber-White robust standard errors are shown in parentheses.

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

causal inference methods to assess changes in seven commonly studied dimensions of local demographics, along axes of total population, age, race, and socioeconomic status. The advantage of my empirical strategy is that I compare block groups in a counterfactual world in which a tornado did not occur, by using a CEM strategy paired with a difference-in-difference estimator with block group and time fixed effects.

The existing literature posits three hypotheses regarding natural hazards' impacts on demographic change. I detect no effect on total population size, which might ostensibly support an equilibrium hypothesis. Critically, however, the results also show shifts in the demographic constitution of a block group after a severe tornado. Impacted neighborhoods become more socioeconomically advantaged in terms of median income and poverty, and they experience an increase in the White population. Therefore, my findings more closely align with the displacement hypothesis, which speculates that disadvantaged residents would move away from places after a hazard or disaster. I further show that these effects are exacerbated in socioeconomically advantaged neighborhoods and by the most severe hazards, and that a presidential disaster declaration does not mitigate the effects. Finally, I provide evidence that as a decade progresses, the detected effects become muted, which suggests temporal processes related to short-term migratory instability and post-hazard events.

I interpret my findings alongside other scientific studies of disparate hazard types and at larger geographic scales to suggest that natural hazards and disasters lead to an unequal replacement of socially vulnerable residents. I contend that the extent of uneven replacement depends critically on hazard type, impacted neighborhood characteristics, and the geographic scale under consideration. Future research on hazards and demographic change should consider these three factors. In this article, I focus on the case of severe tornadic hazards and document a displacement process, driven by social vulnerability at a highly local level.

Prior studies concluded that natural hazards lead to an increase in county-level poverty (Boustan et al. 2017; Schultz and Elliott 2013) and that natural hazards in less urban areas concentrate disadvantaged residents (Elliott and Pais 2010). Poverty may increase at the regional level, but I argue that those changes are more likely due to associated but not direct processes of local social vulnerability, such as an influx of labor migrants to adjacent towns (Pais and Elliott 2008). In terms of hazard type, missing from my analysis of tornadoes are locational vulnerability and evacuation, both patterned processes that produce inequality in hurricane and tropical storms. It is possible, therefore, that locational vulnerability induces differential patterns of local demographic change and complicates mechanisms driving uneven replacement.

My findings also reiterate others' conclusions that natural hazards can lead to a polarization of local residents, within and across neighborhoods, along important sociodemographic dimensions (Pais and Elliott 2008; Schultz and Elliott 2013). Like others, I show that the effects vary by attributes of the impacted places. Most notably, the detected moderation effect of local socioeconomic context resembles the one found by Logan et al. (2016). This makes sense in light of the recovery machine hypothesis (Pais and Elliott 2008), and it suggests that across hazard type and various geographic scales, the extent of a natural hazard's effect is moderated by preexisting local socioeconomic attributes. The only effect that is not moderated by local socioeconomic status is change in the Black population, which suggests that the mechanisms driving changes for Black residents in different types of affected neighborhoods may differ from other groups; future research should interrogate this question.

Unique to this study is the finding that the block groups that experienced a declared disaster saw similar demographic change to those that experienced a nondeclared hazard. For scholars and policymakers, this is an important finding that deserves more attention by future research to explore the constitutive processes that influence the uptake of FEMA at the individual level. Additionally, future research would benefit from unpacking and testing for variation, along axes of the aid packages provided, amount, and for other disaster settings with different insurance programs. Finally, this study advances a conceptual approach to natural hazard research that finds theoretically useful the disaggregation of social and locational vulnerability.

Several important considerations are necessary to qualify the results. First, common to other studies relying on aggregated measures of net demographic change, I cannot disentangle the changes that constituted the gross shifts. Thus, I cannot test the extent to which processes of selective in-migration may counteract out-migration. Future research would benefit from individual-level analyses of direct impacts of natural hazards (see, e.g., Howell and Elliott 2019). Second, the empirical analyses involve a trade-off because of census data limitations. I rely on a more suitable geographic scale (block group) but a longer temporal scale (decade). Future analyses would benefit from addressing this limitation by combining data from less temporally restrictive sources, including from administrative or remotely sensed data.

Finally, qualitative studies shed critical light into the mechanisms likely driving my quantitative findings. At the individual level, the displacement process is driven by sociodemographic inequality in dwelling type and household arrangements; inequality in government-funded aid accessibility; and more difficult-to-observe factors, such as social networks or the contexts to which residents are displaced (Asad 2015; Bolin and Stanford 1998; Kroll-Smith 2018; Reid 2013). At the neighborhood level, differences in investment from leaders, planners, and businesses also influence demographic change that can exacerbate inequality and produce processes that decidedly resemble gentrification (Weber and Lichtenstein 2015).

This study is important for several reasons. First, climate scientists have pointed to trends in increasing intensity and frequency of natural hazards in the United States (IPCC 2012). Recent research has shown that tornadic events track this foreboding trend, and some scholars have provided early evidence linking tornadic activity to anthropogenic climate change (Tippett et al. 2016; Trapp and Hoogewind 2016). Increasing disaster frequency and intensity will not only increase exposure risk for socially vulnerable people but also exacerbate patterns of displacement and uneven neighborhood change. Understanding these patterns of local neighborhood change after natural hazards is therefore vital to efficiently allocating resources to improve post-hazard recovery. Second, for policymakers, the findings show that FEMA aid after a tornado had no mitigating effect on demographic change. Thus, in the interest of the efficient allocation of resources, the findings suggest that social vulnerability to natural hazards likely redistributes socioeconomically vulnerable families, such that a place-based and property-based approach to aid misses demographic groups who could use assistance the most.

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