Ethnic Diversity and Cooperation After Disaster: Evidence from Hurricane Harvey

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Abstract

Under what conditions does disaster exposure shape cooperation? A large literature examines the link between disasters and cooperation. We study how a key feature of the social environment—ethnic diversity—moderates the relationship between disaster damage and cooperation. We argue that diversity shapes different forms of cooperation depending on the feasibility of monitoring. When monitoring is feasible, as in face-to-face interactions, we do not predict a systematic relationship between diversity and cooperation following shocks. By contrast, when monitoring is more difficult, as in the provision of public goods such as disaster recovery and adaptation policies, we predict a diversity penalty: following shocks, cooperation is lower at higher levels of diversity. We examine these predictions using observational and experimental data from Hurricane Harvey, which generated substantial variation in local exposure to disaster damage. Consistent with prior work, diversity is associated with weaker pre-disaster cooperation with neighbors. Following the storm, we find no diversity penalty in interpersonal face-to-face helping behavior, but document a diversity penalty in support for disaster recovery and adaptation policies.

Keywords: natural disasters; ethnic diversity; cooperation; public goods

Introduction

Under what conditions does disaster exposure shape cooperation? A growing literature documents that disasters tend to increase prosocial behavior and that social capital facilitates recovery. However, the evidence remains mixed: some studies document positive effects, where exposure to natural disasters is associated with higher cooperation (Calo-Blanco et al., 2017; Cassar et al., 2017; Dacy and Kunreuther, 1969; Douty, 1972; Dynes, 1994; Méon and Verwimp, 2022; Toya and Skidmore, 2014; Schilpzand, 2023; Rao et al., 2011). Others report mixed or negative effects (Albrecht, 2018; Berrebi et al., 2021; Fleming et al., 2014; Rayamajhee et al., 2024). Much of this research treats disasters as uniform shocks, but whether shocks foster cooperation may depend on the social environment in which they occur.

We study how a key feature of the social environment—ethnic diversity—moderates the relationship between disaster damage and cooperation. Prior research suggests that shocks such as natural disasters foster cooperation and increase compliance with social norms (Gelfand et al., 2011). Diversity, in contrast, tends to undermine cooperation and the provision of public goods (Alesina et al., 1999; Alesina and Ferrara, 2000; Algan et al., 2016; Costa and Kahn, 2003; Habyarimana et al., 2007; Luttmer, 2001; Miguel and Gugerty, 2005). Yet little attention has been given to how these two features interact in shaping cooperation.

Hurricane Harvey, the second-costliest disaster in U.S. history, generated substantial tract-level variation in the stakes of cooperation. Harvey was highly destructive—causing $125 billion in damages, 68 direct deaths, and over 100 indirect deaths (Blake and Zelinsky, 2018)—and struck one of the largest and most ethnically diverse metropolitan areas in the United States. Because both flood exposure and ethnic diversity vary across neighborhoods, the setting allows us to examine whether the diversity penalty increases with damage exposure, as our argument predicts.

We argue that diversity shapes different forms of cooperation in distinct ways, depending on the extent to which contributions can be monitored. Monitoring is a central mechanism for mitigating asymmetric information and detecting free-riding, enabling groups to sanction defectors and sustain cooperation (Greif, 1993; Habyarimana et al., 2007; Miguel and Gugerty, 2005; Ostrom, 1990; Williamson, 1985). In face-to-face interactions—such as helping a neighbor—where actions are directly observable and the act of helping itself serves as a costly signal, we do not expect diversity to impose a penalty on cooperation. By contrast, when monitoring is difficult and sanctioning is limited—as in contributions to tax-funded public goods—we expect diversity to be associated with lower levels of cooperation. Such contributions are not directly observable and their benefits are diffuse, creating scope for free-riding; in such settings, the diversity penalty is more likely to emerge. Climate recovery and adaptation policies—such as flood control infrastructure or costly recovery programs—fit this logic, as they are tax-funded and non-excludable, making them particularly exposed to out-group free-riding.

We study patterns of cooperation following Harvey using observational and experimental data. We draw on three waves of an original panel survey fielded by the Hobby School of Public Affairs at the University of Houston between 2017 and 2020, covering four counties in the Houston area; the 2020 wave included two survey experiments. We combine these survey data with detailed administrative records to construct measures of ethnic diversity and damage at the census tract level.

Our main finding is that the relationship between diversity and cooperation depends on the form of cooperation. We do not find a relationship between diversity and interpersonal, face-to-face helping behavior—giving or receiving assistance. By contrast, for impersonal cooperation in the provision of public goods, tract-level storm damage is associated with greater support for climate recovery and adaptation policies in more homogeneous areas; among affected areas, support declines as diversity increases. We find a similar pattern in a conjoint survey experiment. Support for costlier policies—those requiring higher tax increases—is modestly higher among respondents personally affected by the storm. However, among affected respondents, support for costly policies declines in more diverse areas.

We also assess two alternative explanations. First, the diversity penalty may reflect visible fiscal costs rather than monitoring problems: policies that require taxes or public spending may elicit lower support in diverse areas. Second, the penalty may reflect policy time horizon: forward-looking mitigation policies, whose benefits are uncertain and delayed, may elicit lower support than policies addressing damage already incurred. Neither alternative accounts for the diversity penalty we document. Overall, the patterns are more consistent with our argument that monitoring capacity structures how diversity shapes different forms of cooperation.

Our findings contribute to a growing literature on how natural disasters affect cooperation and prosocial behavior. While much work documents positive effects (Calo-Blanco et al., 2017; Cassar et al., 2017; Dacy and Kunreuther, 1969; Douty, 1972; Toya and Skidmore, 2014; Schilpzand, 2023), findings remain mixed (Berrebi et al., 2021; Fleming et al., 2014). We contribute to this literature in three ways. First, we move beyond proxies such as trust and cohesion (Cassar et al., 2017; Toya and Skidmore, 2014; Calo-Blanco et al., 2017; Schilpzand, 2023) and measure cooperation directly, distinguishing between interpersonal and impersonal cooperation. This focus also distinguishes our contribution from recent work on disasters and politics that examines political attitudes (Arin et al., 2025; Frost et al., 2025; Visconti, 2022a) and electoral outcomes (Heersink et al., 2022; Masiero and Santarossa, 2021). Second, we show that disaster exposure is associated with support for climate recovery and adaptation policies in ways that depend on local diversity.

This adds to recent work linking disaster exposure to policy support (Arias and Blair, 2024; Bechtel and Mannino, 2023; Bergquist and Warshaw, 2019; Visconti, 2022b) and to work showing that local disaster risk and cross-cutting distributive interests can soften partisan divides over climate policy (Cayton and Crisher, 2026). We identify a different local moderator: ethnic diversity. Third, we connect disaster politics to research on ethnic diversity and public-goods provision. Our findings complement evidence that racial attitudes predict support for government disaster spending (Gilens et al., 2025) by shifting attention from individual racial attitudes to contextual ethnic diversity. They also extend work showing that diversity can undermine public-goods provision by weakening shared norms, monitoring, and sanctioning (Habyarimana et al., 2007; Miguel and Gugerty, 2005). We show that this logic applies to post-disaster cooperation: the diversity penalty is concentrated in costly and impersonal forms of cooperation, where monitoring is more difficult.

Disasters, ethnic diversity, and cooperation

Cooperation occurs when an individual incurs a cost to provide a benefit to others (Henrich and Henrich, 2007, p.37). Cooperative interactions can occur interpersonally—helping a friend or a stranger—or at larger scales—such as voting, recycling, or paying taxes. While cooperation in small-scale societies can be explained by kinship (McNamara and Henrich, 2017), large-scale cooperation in diverse societies remains a puzzle that motivates substantial theoretical and empirical work (Bowles and Gintis, 2011; Henrich and Henrich, 2007; Seabright, 2010).

Convergent lines of research show that common shocks, such as natural disasters, are associated with increases in prosocial behavior, including trust, social cohesion, and helping behavior (Calo-Blanco et al., 2017; Cassar et al., 2017; Dacy and Kunreuther, 1969; Douty, 1972; Méon and Verwimp, 2022; Rao et al., 2011; Schilpzand, 2023). Ethnic diversity is another feature of the social environment associated with individuals' propensity to cooperate. A large literature shows that diversity hinders cooperation and the provision of public goods (Algan et al., 2016; Alesina and Ferrara, 2000; Costa and Kahn, 2003; Luttmer, 2001; Miguel and Gugerty, 2005). These two forces are typically studied as separate main effects. Yet shocks may condition the relationship between diversity and cooperation. Natural disasters raise the stakes of cooperation by increasing demand for help, recovery, and public goods, making them an important context in which to study how diversity shapes cooperation. In particular, post-disaster recovery may be more difficult in heterogeneous, loosely connected communities, where it is harder to form expectations about the behavior of others (Storr and Haeffele-Balch, 2012).

We propose that diversity shapes different forms of cooperation, depending on the scope for free-riding and the feasibility of monitoring. Monitoring is a key feature facilitating cooperation across domains: it makes behavior observable so that deviations can be detected and punished, whether in common-pool resources (Ostrom, 1990), public goods provision (Habyarimana et al., 2007; Miguel and Gugerty, 2005), contractual exchange (Williamson, 1985), or long-distance trade (Greif, 1993). We argue that diversity moderates the relationship between shocks and cooperation when the cost of monitoring contributions is high and sanctioning is limited. In such situations, diversity dampens impersonal cooperation. In contrast, in interpersonal, face-to-face cooperative interactions, such as receiving assistance from a neighbor, each action is a costly signal that others can observe directly. Because monitoring occurs during the interaction itself, the scope for free-riding is limited. In such interactions, we do not expect a diversity penalty.

Cooperation aimed at providing public goods follows a different logic. These contributions are generally not directly observable, are tax-funded, and generate diffuse benefits. Consequently, they are susceptible to concerns about out-group free-riding. Climate recovery and adaptation policies, such as flood-control infrastructure or costly recovery programs, are examples of such public goods. We therefore predict that, for cooperation aimed at providing public goods, the diversity penalty increases with damage.

Note that our argument differs from accounts that attribute the negative correlation between diversity and public-goods provision to divergent preferences across ethnic groups (Alesina et al., 1999; Alesina and Ferrara, 2000), as well as from accounts that emphasize individual racial attitudes in disaster-spending preferences (Gilens et al., 2025). Our argument instead predicts that local ethnic diversity should matter most when cooperation depends on contributions that are difficult to observe and sanction, and that the diversity penalty should increase with damage exposure.

In sum, our argument is that the association between diversity and cooperation depends on the feasibility of monitoring. When monitoring is difficult, as in costly and impersonal contributions to public goods, we expect diversity to impose a larger penalty. We therefore predict that support for costly recovery policies with public-goods characteristics will decline with diversity, especially in more heavily damaged areas. By contrast, when monitoring is feasible, as in face-to-face helping, we expect disaster exposure to be associated with cooperation regardless of diversity. We examine these predictions using survey data and a policy conjoint experiment. A partner-choice experiment, reported in the Online Appendix, provides supplementary evidence related to the proposed monitoring logic.

Hurricane Harvey: A costly disaster in a diverse metropolitan area

The Houston metropolitan area is one of the largest and most ethnically diverse metropolitan areas in the United States, with more than 7 million residents. It has grown by 25% since 2010, an increase of 1.5 million people. Its population is 44.5% Hispanic or Latino, 24.1% White, 22.1%

Black or African American, and 6.8% Asian. (U.S. Census Bureau, 2023). More than 28% of Houston's residents were born outside the United States. Diversity at the metropolitan level coexists with segregation at the neighborhood level: pooling race counts across the 971 census tracts in the study area gives a metro-wide diversity index of 0.69, whereas the tract in which the average resident lives has an index of 0.53. Tract-level diversity ranges from 0 to 0.76, with an interquartile range of 0.41 to 0.63 (Figure 1). These features make the Houston metropolitan area an important setting for studying how local diversity shapes cooperation and support for disaster recovery policy

In the United States, disasters are increasing in frequency and cost. Between 1980 and 2023, the country experienced 403 weather and climate disasters that exceeded $1 billion each in damages, with total costs exceeding $2.9 trillion[1]. Hurricane Harvey, a Category 4 hurricane, was the second-costliest disaster in U.S. history, with damages of $125 billion, 68 direct deaths, and over 100 indirect deaths (Blake and Zelinsky, 2018). It caused widespread damage due to extreme rainfall—the heaviest in U.S. history—totaling 27 trillion gallons, with some areas receiving over 50 inches (Blake and Zelinsky, 2018). It displaced 30,000 people and destroyed more than 200,000 homes and businesses.[2]

The response to Harvey spanned federal, state, and local actors, alongside large civilian efforts. Federal and state responses included the U.S. Coast Guard, the Federal Emergency Management Agency (FEMA), and the Texas National Guard. Local responders included the Houston Fire Department, the Houston Police Department, and Harris County Sheriff's marine teams. Congress initially approved $15.25 billion in federal disaster relief, followed by additional appropriations. Over 890,000 households registered for FEMA aid. The National Flood Insurance Program (NFIP) paid out more than $8.92 billion in claims. Beyond the governmental response, Harvey triggered grassroots efforts and volunteer operations. Civil society organizations spearheaded emergent re-

[1] U.S. Billion-Dollar Disasters: 1980–2024
[2] National Centers for Environmental Information (2025).

sponse efforts (Kendra and Wachtendorf, 2016; Wachtendorf et al., 2017). Churches and faith communities played a prominent role, providing services including financial support and housing assistance (Westfall et al., 2019). Local volunteers and groups—such as the civilian "Cajun Navy"—conducted boat rescues and assisted with evacuations.[3]

Data

Survey data. The Hobby School of Public Affairs at the University of Houston conducted three survey waves in Harris, Fort Bend, Brazoria, and Montgomery counties following Hurricane Harvey. The first wave, fielded by telephone in November–December 2017, interviewed 2,002 adults and oversampled areas expected to have flooded; weights were applied only to correct for the owner–renter imbalance in Harris County. The second wave, conducted by telephone in June–July 2018, included 1,073 respondents, of whom 572 had participated in the 2017 survey and 501 were drawn from a fresh sample; weights corrected for the owner–renter balance and racial distribution across all four counties. The third wave, conducted online in May–June 2020, surveyed 1,065 respondents with demographic quotas for county, age, race, and gender, and applied weights for race, sex, age, and party identification.[4] Across the three surveys, the 2017 and 2018 waves shared a phone-based design and an emphasis on flood-exposed areas, while the 2020 wave shifted to an online mode with quota sampling. Online Appendix Section A2 provides details on sampling and survey logistics, and Table A1 provides descriptive statistics for the survey variables used in the analysis.

[3] "In all-hands-on-deck response to Harvey, lessons learned from earlier storms," *The Christian Science Monitor*, August 28, 2017.

[4] Wave 3 was fielded during the COVID-19 pandemic. The observational analyses use Waves 1 and 2 only, and the policy conjoint reproduces their pattern with a different sample and survey mode, which is reassuring.

Administrative data. To measure ethnic diversity and storm damage, we employ two administrative datasets: the U.S. Census 2015 American Community Survey (ACS) and the City of Houston Open Data website.

1. ***Ethnic diversity***. We calculate measures of ethnic diversity using data from the 2015 American Community Survey (ACS). The ACS provides social and demographic data at the census tract level. We use ACS variables that capture the number of individuals in each ethnic group in each tract: White or Anglo, Black or African American, Hispanic or Latino, Asian, and Other. We construct a standard measure of ethnic fractionalization, defined as the probability that two randomly selected individuals in a given tract belong to different ethnic groups.[5]

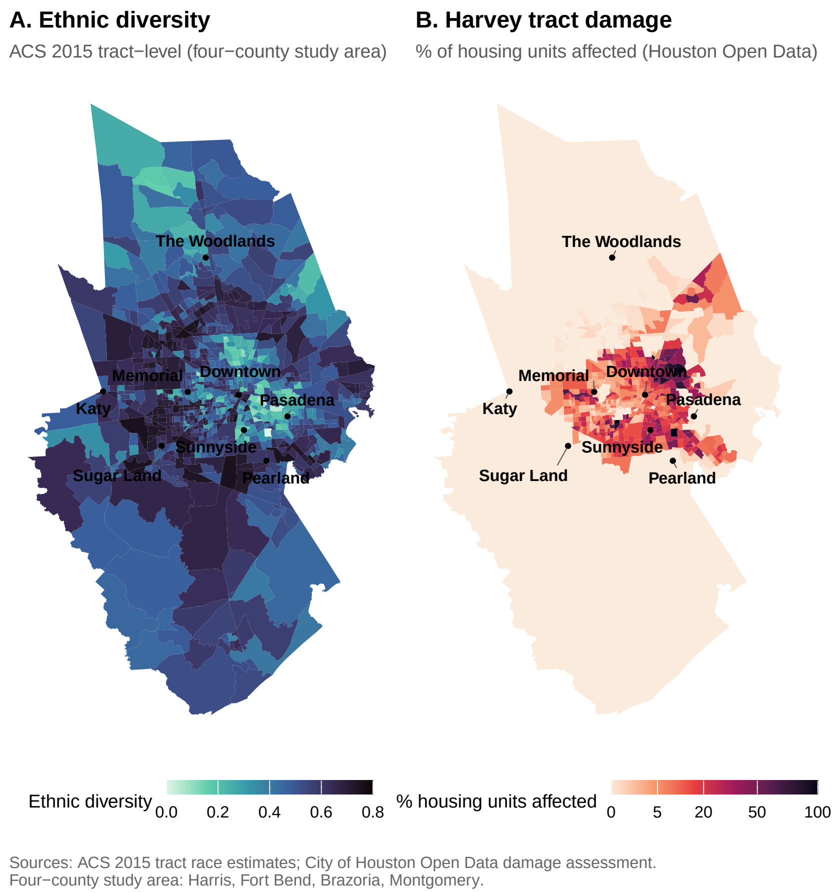
2. ***Storm damage***. We obtained census tract-level measures of damage from the City of Houston Open Data website. The data contain count variables for housing units affected by Harvey, based on a range of assessment sources, including 311 service request calls, 911 incident calls, Solid Waste Management Department debris pickup locations, high water mark data obtained by the Public Works Department Flood Plain Management Office, FEMA individual assistance requests, and data from the National Flood Insurance Program. We construct a continuous tract-level measure, Damage$_j$, defined as the share of housing units in tract j recorded as affected. The damage records cover Harris, Fort Bend, and Montgomery counties but not Brazoria County; the 141 Wave 1 respondents in Brazoria enter only the specifications that use self-reported damage. The distribution is heavily right-skewed: most tracts have zero or near-zero damage, while a small number of flood-prone tracts reach much higher shares (Online Appendix Figure A1).[6]

[5] Formally, ethnic fractionalization is defined as $1 - \sum_{g=1}^{G} \pi_{gt}^2$, where π_{gt} denotes the share of residents in tract t who belong to ethnic group g, and G is the total number of ethnic groups.

[6] Online Appendix Section A2 provides a detailed analysis of missingness in each variable used in the analysis in the following section.

3. ***Tract-level controls.*** All observational models include six tract-level controls drawn from the ACS 2015 five-year estimates: median household income, share of renter-occupied units, share of foreign-born residents, share of residents below the poverty line, share of residents who moved in the past year, and population density. These controls address the concern that diversity may proxy for potential confounders, such as socioeconomic conditions, rather than ethnic heterogeneity per se. Importantly, diversity is uncorrelated with most of these characteristics: correlations with income, density, and the foreign-born share are all below 0.10, and more diverse tracts are, on average, *less* poor ($r = -0.26$) (Online Appendix Table A5).

Figure 1: Study area: census tracts, ethnic diversity, and Harvey damage.



Notes: Left panel: ethnic diversity (fractionalization index from ACS 2015) at the census-tract level across Harris, Fort Bend, Brazoria, and Montgomery counties. Right panel: share of housing units affected by Harvey, from the City of Houston Open Data. Darker shading indicates higher values.

Results

Baseline patterns: diversity and pre-disaster coordination

Table 1 reports OLS estimates of the relationship between ethnic diversity and reported cooperation prior to Harvey with different groups in the community. The outcome is whether respondents coordinated preparedness actions with immediate family, extended family, neighbors, or friends before the hurricane made landfall in the region. The exact survey question is: "Before Harvey reached Houston, did you coordinate plans for the approaching hurricane with others? With whom did you coordinate?" Although the wording emphasizes coordination, we interpret this behavior as a form of cooperation. These results show whether the diversity penalty found in prior work (Algan et al., 2016; Alesina and Ferrara, 2000; Costa and Kahn, 2003; Luttmer, 2001; Miguel and Gugerty, 2005) appears in our data before the storm caused any damage. We estimate the following equation:

$$\text{Coordination}_{ij}^{k} = \alpha + \beta\,\text{Diversity}_{j} + \mathbf{X}_{ij}\boldsymbol{\gamma} + \varepsilon_{ij}^{k}, \tag{1}$$

where i denotes a respondent in census tract j, $\text{Coordination}_{ij}^{k}$ is a pre–Harvey binary measure of coordination, and k indexes social tie categories (immediate family, extended family, neighbors, or friends). All outcome variables are coded 1 when the response is affirmative and 0 otherwise. The key explanatory variable, Diversity_{j}, is the tract-level index of ethnic fractionalization. The coefficient of interest, β, captures the association between local ethnic heterogeneity and each form of coordination. $\mathbf{X}_{ij}$ is a vector of individual-level controls. Controls include indicators for partisanship, race/ethnicity, age group, educational attainment, household-income bracket, ZIP-code fixed effects, a categorical years-in-town indicator, and a survey-wave indicator. All models are estimated by weighted least squares using survey weights, with standard errors clustered at the census-tract level. The baseline model includes ZIP-code fixed effects and individual-level controls (Party ID, age, education, race, income, years in town, and survey wave). The extended models include six tract-level controls drawn from the ACS 2015 five-year estimates: median household income, percent of renter-occupied units, percent of foreign-born residents, population density, percent of residents below the poverty line, and percent of residents who moved in the past year.

Ethnic diversity is not correlated with coordination with family or friends, but respondents in more diverse tracts were less likely to coordinate with neighbors before Harvey (Table 1). A one-unit increase in ethnic diversity is associated with a 20.9 percentage-point (pp) decrease in the likelihood of coordinating with neighbors conditional on individual controls ($p < 0.05$); the estimate shrinks slightly to 18.7 pp once tract-level characteristics are added ($p < 0.10$). This is a large association relative to the baseline probability of coordinating with neighbors (13.3%; Online Appendix Table A2). The coefficient is positive for immediate family ($+0.08$) and most negative for neighbors (-0.19), a monotone gradient across tie strength; the sharpest contrast, family versus neighbors, is significant at $p = 0.07$, while the intermediate ties fall in between (Online Appendix Table A7).

Table 1: Likelihood of coordinating prior to Hurricane Harvey.

	Immediate Family		Extended Family		Neighbors		Friends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ind. Controls	+ Tract Controls	Ind. Controls	+ Tract Controls	Ind. Controls	+ Tract Controls	Ind. Controls	+ Tract Controls
Diversity	-0.031	0.084	-0.163	-0.079	-0.209**	-0.187*	-0.051	-0.162
	(0.104)	(0.130)	(0.124)	(0.154)	(0.089)	(0.113)	(0.130)	(0.159)
Observations	1904	1904	1904	1904	1904	1904	1904	1904
Adj. R-squared	0.078	0.081	0.084	0.084	0.089	0.087	0.074	0.076

Notes: Results from OLS models. Each outcome is estimated twice: with individual controls only (Ind. Controls) and with the same individual controls plus six tract-level controls (+ Tract Controls): median household income, % renter-occupied, % foreign-born, population density, % below poverty, and % moved in the past year. All models include ZIP-code fixed effects, individual controls (party ID, age, education, race, income, years in town, survey wave), and survey weights. Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

No diversity penalty in interpersonal cooperation

Here, we examine whether ethnic diversity shaped patterns of interpersonal cooperation around Hurricane Harvey. We consider four behavioral measures of cooperation: (i) whether the respondent received help in the days immediately before or after Harvey; (ii) whether the respondent provided help to others over the same period; (iii) the (log) number of volunteers known to the respondent; and (iv) a composite cooperation index aggregating the three preceding items.[7] We estimate the following equation:

$$\text{Cooperation}_{ij}^{k} \;=\; \alpha + \beta_1\,\text{Diversity}_j + \beta_2\,\text{Damage}_j + \beta_3\left(\text{Diversity}_j \times \text{Damage}_j\right) + \mathbf{X}_{ij}\boldsymbol{\gamma} + \varepsilon_{ij}^{k}, \tag{2}$$

where i denotes a respondent in census tract j, and $\text{Cooperation}_{ij}^{k}$ is one of the four cooperation measures listed above, indexed by k—that is, received help, provided help, the log of one plus volunteers known, or the cooperation index. The first three are estimated as separate regressions; the cooperation index is a composite of the underlying items. Diversity_j denotes the ethnic fractionalization index, and Damage_j is a measure of tract-level storm damage; we report results under three operationalizations—a self-reported binary indicator, the continuous share of housing units affected, and an above-median binary indicator (see Table 2). The interaction term $\text{Diversity}_j \times \text{Damage}_j$ allows the coefficient on diversity to differ across levels of damage. $\mathbf{X}_{ij}$ is a vector of controls as in Equation 1, with the addition of an indicator for whether the respondent evacuated before landfall. All models are estimated by weighted least squares using survey weights, with standard errors clustered at the census tract level.

[7] The exact question wording from the 2017 Wave 1 instrument is as follows. Received help: "In the days immediately before or after Hurricane Harvey, did you receive any help from family members, friends, or other people who you might know through clubs, churches or organizations you belong to?". Provided help: "In the days immediately before or after Hurricane Harvey, did you provide any help to family members, friends, or other people who you might know through clubs, churches or organizations you belong to?". Volunteers known: "How many volunteers do you know who provided help in the days immediately before or after Hurricane Harvey?". Cooperation index: rowwise mean of the standardized (z-scored) values of received help, provided help, and $\log(1 + \text{volunteers known})$, computed for respondents with non-missing values on at least two of the three items. Note that the Wave 1 question asks about help "in the days immediately before or after Hurricane Harvey," and thus bundles preparedness assistance with post-storm recovery. This does not threaten the interpretation of the null interaction. The question covers help from family, friends, and association members—categories for which Table 1 shows no diversity penalty in pre-storm coordination—so the pre-Harvey component is approximately flat in diversity and does not introduce a diversity-correlated confound. The Diversity $\times$ Damage interaction is, if anything, attenuated toward zero by the pre-Harvey component (which is uncorrelated with damage by construction). Online Appendix Table A9 re-estimates the model on the subsample of respondents reporting any administrative damage (where the help variable mechanically loads on the post-storm component); the Diversity $\times$ Damage interaction remains null across all four outcomes.

Table 2 reports results for four interpersonal-cooperation outcomes—help received, help provided, number of volunteers known, and a composite index—each estimated using three damage measures. In general, damage does not predict cooperation. The interaction between diversity and damage is statistically indistinguishable from zero under most outcomes and damage specifications.[8][9]

Table 2: Interpersonal cooperation following Hurricane Harvey.

	Received help			Provided help			log(1+Vol. known)			Coop. index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Self-rep	Continuous	Above med	Self-rep	Continuous	Above med	Self-rep	Continuous	Above med	Self-rep	Continuous	Above med
Diversity	-0.379*	-0.063	-0.252	0.103	-0.021	0.009	0.675	0.314	0.106	-0.168	-0.025	-0.186
	(0.214)	(0.191)	(0.187)	(0.258)	(0.217)	(0.211)	(0.655)	(0.623)	(0.564)	(0.377)	(0.298)	(0.295)
Damage	0.116	0.791**	0.126	0.152	-0.443	-0.108	0.608*	0.078	-0.198	0.286	0.324	0.003
	(0.124)	(0.357)	(0.133)	(0.143)	(0.384)	(0.167)	(0.314)	(1.001)	(0.332)	(0.224)	(0.498)	(0.199)
Diversity × Damage	0.104	-1.127	-0.065	-0.081	-0.117	0.029	-0.751	-2.235	-0.228	-0.061	-1.468	-0.131
	(0.222)	(0.713)	(0.257)	(0.259)	(0.812)	(0.321)	(0.589)	(2.008)	(0.645)	(0.398)	(1.019)	(0.378)
Observations	2099	1949	1949	1906	1770	1770	1470	1359	1359	1905	1770	1770
Adj. R-squared	0.196	0.180	0.177	0.163	0.175	0.166	0.224	0.236	0.233	0.188	0.174	0.170

Notes: Results from OLS models. Each outcome is estimated under three damage measures: self-reported damage (Self-rep), administrative tract-level damage share (Continuous), and above-median administrative damage (Above med). *Received help* (cols 1–3) is observed in both the 2017 (Wave 1) and 2018 (Wave 2) survey waves and pooled across them; the remaining three outcomes—*Provided help* (cols 4–6), *log(1+Vol. known)* (cols 7–9), and the cooperation index (cols 10–12)—are 2017 (Wave 1) items only. All specifications include individual controls (party ID, age, education, race, income, evacuation, years in town, survey wave), ZIP-code fixed effects, and six tract-level controls (median household income, % renter, % foreign-born, population density, % below poverty, % moved in the past year). Survey weights included. Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

A diversity penalty in costly public goods provision

Here, we examine a different set of measures of costly cooperation: support for recovery and adaptation policies. We construct an index of policy support equal to the average of ten binary

[8] The estimates are more precise for help provided than for help received: the 95% confidence interval for the former excludes penalties of the size we find for policy support below, whereas the interval for the latter does not (Online Appendix Table A10).

[9] Columns 1–3 use data from waves 1 and 2. We analyze attrition and find that tract damage share predicts inclusion in the help-received sample ($p = 0.02$): respondents in more-damaged tracts are less likely to be missing, while diversity does not predict missingness. Online Appendix Table A4 reports the full attrition analysis.

survey questions on support for specific policy proposals to protect the Houston area from severe weather events:[10]

$$\text{Policy Support}_{ij} = \frac{1}{10}\big(\text{policy}_{ij}^{(\mathrm{i})} + \text{policy}_{ij}^{(\mathrm{ii})} + \cdots + \text{policy}_{ij}^{(\mathrm{x})}\big), \quad \text{with policy}_{ij}^{(\cdot)} \in \{0,1\}.$$

We estimate the following equation:

$$\text{Policy support}_{ij} \;=\; \alpha + \beta_1\,\text{Diversity}_j + \beta_2\,\text{Damage}_j + \beta_3\left(\text{Diversity}_j{\times}\text{Damage}_j\right) + \mathbf{X}_{ij}\boldsymbol{\gamma} + \varepsilon_{ij}, \tag{3}$$

where i indexes respondents in census tract j, the dependent variable Policy Support_{ij} is the index described above, Diversity_j is the ethnic fractionalization index, Damage_j is an indicator of whether tract j experienced storm damage, and the interaction term $\text{Diversity}_j \times \text{Damage}_j$ allows the association between diversity and policy support to differ between damaged and undamaged tracts, and $\mathbf{X}_{ij}$ is a vector of controls as in the earlier models, including an indicator for evacuation before landfall. All models are estimated by weighted least squares using survey weights, with standard errors clustered at the census tract level.

Greater damage is correlated with higher policy support in the self-reported and continuous damage specifications (Table 3). This result is consistent with work on how disaster exposure

[10]The items are: (i) a program to buy homes in areas that have repeatedly flooded with local, state, and federal moneys; (ii) construction of a new reservoir to protect the western portion of the Houston area; (iii) greater restrictions on construction in flood plains; (iv) establishment of a regional flood agency with taxing authority to plan for the prevention of regional flooding; (v) denying federally financed flood insurance to homeowners whose homes have flooded three or more times since 2001; (vi) not allowing homes that have flooded three or more times since 2001 to be rebuilt by buying out these homeowners with local and federal moneys; (vii) requiring sellers of homes to fully disclose prior flood damage to their homes and prior flooding in the surrounding neighborhood; (viii) preventing development/construction on native prairies and wetlands in western and northwestern portions of Harris County; (ix) requiring government compensation for homes that are flooded due to the release of water from local reservoirs; and (x) new building codes that require homes built in flood-prone areas to be elevated/raised to avoid flooding.

affects support for climate policies (Arias and Blair, 2024; Bechtel and Mannino, 2023; Bergquist and Warshaw, 2019). The interaction between damage and diversity is negative and significant ($p < 0.05$) in all three columns. Under the self-reported specification, the marginal effect of damage on policy support is approximately $+7$ pp in lower-diversity tracts ($Diversity_j = 0.25$) and -3 pp in higher-diversity tracts ($Diversity_j = 0.75$)—a difference of roughly 10 pp, about 13% of the baseline mean of 78.4%. The continuous damage specification yields a larger interaction: each additional percentage point of tract-level damage reduces policy support by an additional 0.80 pp in the most diverse tracts relative to the least diverse ($p < 0.05$).

Across the ten items, the interaction is negative for most, and the items that reach statistical significance vary with the damage measure: under the continuous measure they are buyouts in place of rebuilding, restrictions on flood-plain construction, and the denial of federal flood insurance to repeat claimants (all $p < 0.05$), whereas under self-reported damage they are the buyout program and the new reservoir (Online Appendix Table A13). No consistent split between spending and regulatory items emerges; the "Alternative explanations" subsection below tests this and the time-horizon account directly.[11]

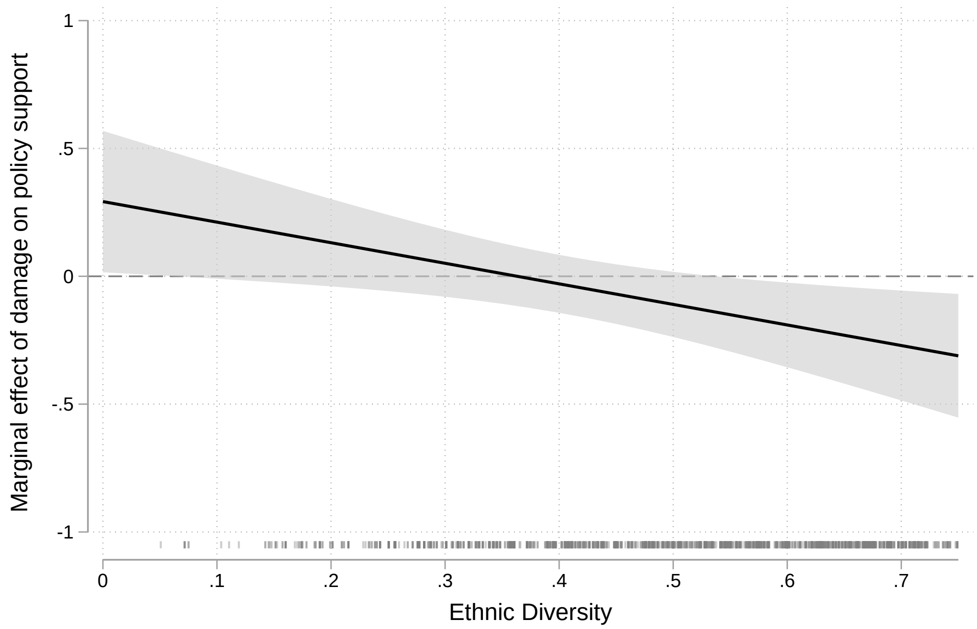
[11] A potential alternative explanation is that damaged residents prioritize private goods over costly public goods due to shortsightedness (Visconti, 2022b). However, note that this would imply a diversity penalty that is constant in damage. Instead, the monitoring argument predicts that the penalty increases with damage—the pattern we observe.

Table 3: Support for climate recovery and adaptation policies following Hurricane Harvey.

	Support for Policies (Index)		
	(1)	(2)	(3)
	Self-reported	Continuous	Above median
Diversity	0.083	0.038	-0.004
	(0.084)	(0.086)	(0.078)
Damage	0.118***	0.292**	0.084
	(0.044)	(0.141)	(0.055)
Diversity $\times$ Damage	-0.200**	-0.804**	-0.249**
	(0.080)	(0.316)	(0.107)
Observations	1905	1770	1770
Adj. R-squared	0.104	0.105	0.105

Notes: Results from OLS models. Three damage specifications: self-reported (Self-reported), administrative tract damage share (Continuous), and above-median administrative damage (Above median). All specifications include individual controls (party ID, age, education, race, income, evacuation, years in town, survey wave), ZIP-code fixed effects, and six tract-level controls (median household income, % renter, % foreign-born, population density, % below poverty, % moved in the past year). Survey weights included. Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Figure 2: Marginal effect of damage on policy support across tract diversity.



Notes: Marginal effect of tract damage share on the policy-support index as a function of tract ethnic diversity, estimated from the continuous-damage specification in Table 3. The black line plots the marginal effect of damage at each level of diversity; the shaded area denotes the 95% confidence interval (clustered at the tract). Tick marks at the bottom show the distribution of tract diversity in the estimation sample. Damage is positively associated with policy support in homogeneous tracts (about $+0.29$ at Diversity $= 0$), the marginal effect declines with diversity, crosses zero near Diversity ≈ 0.36, and becomes significantly negative past Diversity ≈ 0.55, reaching about -0.31 at Diversity $= 0.75$. $N = 1{,}770$.

Figure 2 plots how the association between damage and policy support varies with tract diversity. In the most homogeneous tracts, an additional unit of tract damage share is associated with a 0.29 increase in the policy-support index ($p < 0.05$). The marginal effect declines monotonically as diversity increases, crosses zero around $Diversity_j \approx 0.36$, and becomes significantly negative beyond $Diversity_j \approx 0.55$, reaching -0.31 at the upper end of the observed diversity range.[12]

To test whether the diversity penalty grows with the size of the required contribution, we use a conjoint experiment from the third survey wave that randomizes the tax cost of recovery pro-

[12]The pattern does not depend on the linear form of the interaction: estimating the damage slope separately within each quartile of diversity yields a marginal effect indistinguishable from zero in the bottom three quartiles and -0.48 in the top quartile (Online Appendix Table A12).

posals. Respondents were presented with pairs of policy proposals that varied randomly along three dimensions: (1) project type (road infrastructure, loans to rebuild housing, or infrastructure to prevent new flooding), (2) the geographic area covered by the policy (poor residential areas, industrial areas, commercial areas, or Houston-wide), and (3) the level of tax increase. The three project types correspond to policies that Houston-area governments contested after Harvey: Harris County voters approved a $2.5 billion flood-control bond in August 2018 that funds a home buyout program, the Houston City Council tightened floodplain elevation rules that April, and Texas expanded flood-disclosure requirements for home sellers in 2019.[13] Choosing the higher-tax proposal is a costly contribution to a public good whose benefits also accrue to others, making the choice a measure of costly cooperation rather than simple policy preference. The original attribute randomized sales and property tax schedules, each displayed with its annual dollar cost: $200, $400, or $600 per year for a $225,000 house or a $60,000 household (Figure 3).

[13] *Texas Tribune*, August 25, 2018; April 4, 2018; August 22, 2019.

Figure 3: Policy conjoint experiment: sample scenario.

In each of the following pages, you will be shown profiles from **two different** proposals associated with generating funds for recovery/prevention of natural disasters and your willingness to pay for funding of those programs. For each pair, read each profile carefully and indicate your response. Please consider each pair of profiles independently of the pairs listed on other pages.

Attribute	Proposal A	Proposal B
Tax Option	1% increase in sales tax (extra $200 tax per year for a household income of $60,000)	0.2% increase in property tax (extra $400 tax per year for a $225,000 house)
To be used for	Build infrastructure to prevent new flooding	Recover road infrastructure affected by Harvey
Project	Poor residential areas	Throughout the Houston area

Which of these two proposals do you prefer?

- ○ **Proposal A**
- ○ **Proposal B**

The conjoint presented seven tax schedules, spanning from no increase to a three-percent sales-tax increase and a 0.3 percent increase in property tax. To test whether respondents are willing to endorse the more costly policy, the tax attribute—regardless of the tax base (sales or property)—was recoded as a binary variable that takes the value 1 when the profile shows the higher of the two tax levels in the pair and 0 when it shows the lower tax level; profiles in which both options display the same tax level are coded as missing. After this transformation, 3,700 profile choices are classified as Higher Tax Increase and 3,700 as Lower Tax Increase, while 1,120 tied profiles are excluded from the analysis.[14] We estimate the following linear probability mixed model:

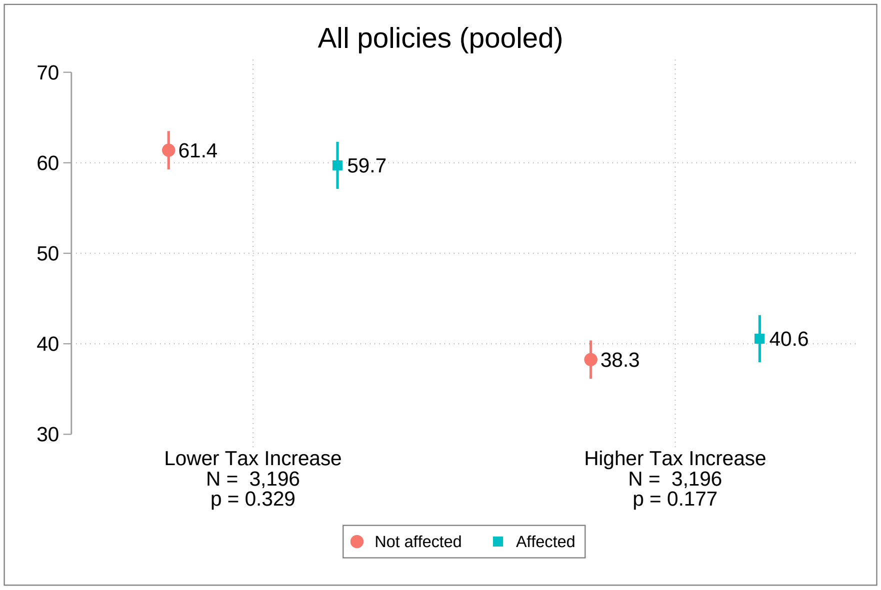
$$\begin{aligned}\text{Choice}_{itp} &= \alpha + b_1\,\text{Use}_p + b_2\,\text{Projects}_p + b_3\,\text{HighTax}_{tp} + b_4\,\text{Affected}_i + b_5\big(\text{Use}_p \times \text{Affected}_i\big)\\ &\quad + b_6\big(\text{Projects}_p \times \text{Affected}_i\big) + b_7\big(\text{HighTax}_{tp} \times \text{Affected}_i\big) + u_t + v_i + \varepsilon_{itp},\end{aligned} \tag{4}$$

where the dependent variable Choice_{itp} is a binary variable capturing whether profile p evaluated by respondent i in paired trial t was selected. The attribute Use_p denotes the policy type, and Projects_p refers to the target area of the project. The binary variable HighTax_{tp} equals 1 when the profile carries the higher of the two tax levels in the pair and 0 otherwise. We interact each attribute with Affected_i, an indicator of whether the respondent reported being personally affected by Harvey. The random intercepts u_t and v_i account for unobserved heterogeneity at the trial and respondent levels.

Figure 4 presents the results and Online Appendix Table A14 reports the full set of estimates. In general, respondents were more likely to support proposals that involved lower rather than higher tax increases. Among non-affected respondents, the high-tax penalty—the coefficient on HighTax_{tp}—is approximately -24 pp on the 0–100 choice probability scale: non-affected respondents are roughly 24 pp less likely to choose the higher-tax proposal than the lower-tax proposal. The shock attenuates this aversion: the coefficient on $\text{Affected}_i \times \text{HighTax}_{tp}$ is $+4.2$ pp ($p < 0.10$) in the Houston-only sample and $+7.8$ pp ($p < 0.01$) in the full sample as a robustness check (Online Appendix Table A14). This coefficient corresponds to the change in the vertical gap between affected and non-affected respondents across the two tax conditions in Figure 4.

[14] 304 observations were excluded due to missingness in the damage survey question. Tied pairs contain no within-pair variation in the tax attribute and therefore do not identify the tax effect. In the Houston-only sample of affected respondents used in Equation 5 below, 186 of 1,460 pairs are tied, leaving 2,548 profile evaluations.

Figure 4: Predicted margins of choosing high versus low tax increases following Hurricane Harvey conditional on being affected by the storm.



Notes: Points plot predicted margins of choosing the proposed policy option for respondents affected by Hurricane Harvey (teal squares) and not affected (salmon circles), conditional on the size of the required tax increase (lower vs. higher). Labels below each tax condition report the number of choice tasks (N) and the associated p-value. Vertical bars denote 95% confidence intervals. Houston-only Wave 3 sample. The decomposition by policy type appears in Online Appendix Figure A3, alongside the underlying coefficients in Table A14.

Equation (5) re-estimates the conjoint model for the 365 respondents who reported being affected by Hurricane Harvey in the Houston-only Wave 3 sample (38.5 percent of the sample).

$$\begin{aligned}\text{Choice}_{itp} \;&= \alpha + \beta_1\,\text{Use}_p + \beta_2\,\text{Projects}_p + \beta_3\,\text{HighTax}_{tp} + \beta_4\,\text{HighDiversity}_i \\ &\quad + \beta_5\,\text{Use}_p \times \text{HighDiversity}_i + \beta_6\,\text{Projects}_p \times \text{HighDiversity}_i \\ &\quad + \beta_7\,\text{HighTax}_{tp} \times \text{HighDiversity}_i + u_t + v_i + \varepsilon_{itp},\end{aligned} \tag{5}$$

where Choice_{itp} equals 1 when profile p is selected in paired trial t by respondent i. The covariates Use_p, Projects_p, and HighTax_{tp} retain their earlier definitions. The binary variable HighDiversity_i

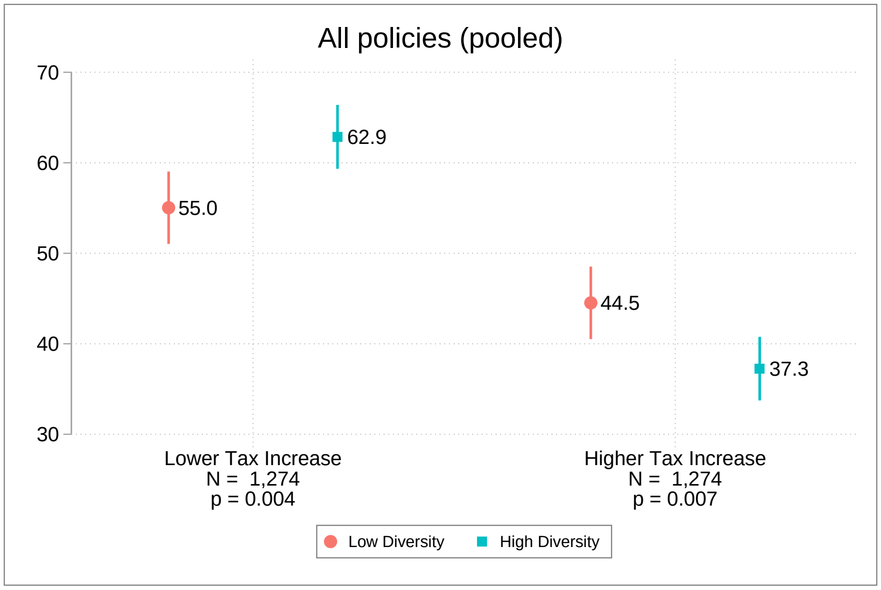
takes the value 1 when the ZIP-code ethnic fractionalization index for respondent i's ZIP code exceeds the sample mean and 0 otherwise.[15]

Across policies, affected respondents living in higher-diversity areas are less likely to choose the high-tax proposal and more likely to choose the low-tax proposal (Figure 5). Online Appendix Table A14 reports the full set of estimates. Among lower-diversity affected respondents, the high-tax penalty—the coefficient on HighTax_{tp}—is approximately -10 to -11 pp on the 0–100 choice probability scale: an affected respondent in a lower-diversity area is 10–11 pp less likely to choose the higher-tax proposal than the lower-tax proposal. The diversity tax penalty—the coefficient on $\text{HighTax}_{tp} \times \text{HighDiversity}_i$—is an additional -15.1 pp ($p < 0.01$) in the Houston-only sample and -9.4 pp ($p < 0.01$) in the full sample as a robustness check: respondents in higher-diversity areas are much more averse to higher-tax proposals. This coefficient corresponds to the change in the vertical gap between higher- and lower-diversity respondents across the two tax conditions in Figure 5.[16]

[15]Because Wave 3 collected respondents' ZIP codes rather than street addresses, the diversity index and the additional controls in the conjoint analyses are constructed at the ZIP-code level, rather than at the census-tract level used in the observational analyses (Tables 1–3). See Online Appendix Section A6.

[16]The tax attribute is randomized within respondent, so the interaction cannot reflect respondent- or area-level differences in tax aversion; it is unchanged when respondent partisanship is added as a control or interacted with the tax attribute (Online Appendix Table A15).

Figure 5: Predicted margins of choosing high versus low tax increases following Hurricane Harvey for affected respondents conditional on ethnic diversity.



Notes: Points plot predicted margins of choosing the proposed policy option for respondents living in lower-diversity areas (below-mean ethnic diversity, salmon circles) and higher-diversity areas (above-mean, teal squares), conditional on the size of the required tax increase (lower vs. higher). Labels beneath each tax condition report the number of choice tasks (N) and the associated p-value. Vertical bars denote 95% confidence intervals. Houston-only Wave 3 sample, affected respondents only. The decomposition by policy type appears in Online Appendix Figure A4, alongside the underlying coefficients in Table A14.

A second conjoint experiment in Wave 3 asked respondents to choose between two hypothetical neighbors as partners in providing information and help during future flooding. The profiles randomized gender, race, age, partisanship, religion, and civic-association membership. Shared partisanship and shared religion increase the probability of choosing a profile by about 25 and 17 percentage points, respectively, with similar effects across diversity levels. The effect of shared civic-association membership is larger in higher-diversity ZIP codes than in lower-diversity ZIP codes—9.2 versus 4.5 percentage points—a pattern consistent with associations serving as observable signals where ascriptive cues are less informative. The cross-diversity difference, however, is not statistically significant ($p = 0.34$), so we treat this result as suggestive evidence consistent with our monitoring argument. Online Appendix Section A7 reports the design and estimates.

Alternative explanations

Support for recovery policies differs from interpersonal help in ways other than the feasibility of monitoring. Two differences could in principle produce the pattern above: fiscal cost and time horizon. Tables 4 and 5 re-estimate Equation 3 on subsets of the policy index chosen to separate each alternative from the monitoring account.

First, recovery policies cost money and interpersonal help usually does not. If diversity depressed support because the cost is visible and taxed, the penalty should concentrate on the five items that require public spending or taxing authority—buyouts, a new reservoir, a regional flood agency with taxing authority, buyouts in place of rebuilding, and compensation for reservoir releases—and disappear on the five items that impose regulatory or private costs. It does not. The interaction is -0.76 ($p < 0.05$) on the regulatory items and -0.78 on the spending items, and the two estimates do not differ ($p = 0.97$; Table 4).

Table 4: Support for recovery policies by fiscal visibility of the policy items.

	Full index (10 items) (1)	Spending items (5) (2)	Regulatory items (5) (3)
Self-reported damage			
Diversity × Damage	-0.200**	-0.232**	-0.145*
	(0.080)	(0.114)	(0.084)
Observations	1,905	1,899	1,903
Wald test H_0: (2) = (3)	$\chi^2(1)$ = 0.67, p = 0.41		
Continuous damage share			
Diversity × Damage	-0.804**	-0.775**	-0.758**
	(0.316)	(0.381)	(0.362)
Observations	1,770	1,764	1,768
Wald test H_0: (2) = (3)	$\chi^2(1)$ = 0.00, p = 0.97		
Above-median damage			
Diversity × Damage	-0.249**	-0.296**	-0.173
	(0.107)	(0.139)	(0.115)
Observations	1,770	1,764	1,768
Wald test H_0: (2) = (3)	$\chi^2(1)$ = 0.85, p = 0.36		
Individual controls	Yes	Yes	Yes
Tract controls	Yes	Yes	Yes
ZIP-code fixed effects	Yes	Yes	Yes

Notes: OLS estimates. The dependent variable is the share of policy items the respondent supports, scaled 0–1. Column 1 uses all ten items; column 2 the five items that require government outlays or taxing authority (buyout program; new reservoir; regional flood agency with taxing authority; buyouts of repeatedly flooded homes in lieu of rebuilding; compensation for reservoir-release flooding); column 3 the five items that impose private or regulatory costs with no public spending (flood-plain construction restrictions; denial of federal flood insurance to repeat claimants; seller disclosure; prohibition of development on prairies and wetlands; elevation building codes). Each panel uses one of the three damage measures of Table 3: self-reported damage (binary), the share of housing units affected in the respondent's census tract (continuous), and an indicator for above-median tract damage share. All specifications include individual controls (party identification, race, age, education, income, evacuation, years in town, home ownership, insurance, survey wave), six ACS 2015 tract controls, and ZIP-code fixed effects, and are weighted by survey weights; standard errors, in parentheses, are clustered at the census-tract level. The Wald test stacks columns 2 and 3 by seemingly unrelated estimation and tests equality of the Diversity × Damage coefficients. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Second, recovery policies are forward-looking and help is immediate. If the diversity penalty reflected reluctance to sacrifice for distant benefits across group lines, it should concentrate on the eight mitigation items and be weaker on the two items that address damage already incurred.

The estimate is at least as large on the response items (-1.01, $p = 0.06$) as on the mitigation items (-0.78, $p < 0.05$; Table 5). The policy conjoint shows the same ordering: the diversity–tax interaction is -16.8 percentage points for road recovery and housing loans and -11.1 for flood-prevention infrastructure (Online Appendix Table A17). In both cases the penalty remains; neither difference in cost or time horizon accounts for the pattern we document.

Table 5: Support for recovery policies by policy time horizon.

	Full index (10 items) (1)	Response items (2) (2)	Mitigation items (8) (3)
Self-reported damage			
Diversity × Damage	-0.200**	-0.350**	-0.168**
	(0.080)	(0.143)	(0.082)
Observations	1,905	1,866	1,904
Wald test H_0: (2) = (3)	$\chi^2(1) = 2.04$, $p = 0.15$		
Continuous damage share			
Diversity × Damage	-0.804**	-1.005*	-0.784**
	(0.316)	(0.534)	(0.327)
Observations	1,770	1,733	1,769
Wald test H_0: (2) = (3)	$\chi^2(1) = 0.21$, $p = 0.64$		
Above-median damage			
Diversity × Damage	-0.249**	-0.295	-0.247**
	(0.107)	(0.203)	(0.107)
Observations	1,770	1,733	1,769
Wald test H_0: (2) = (3)	$\chi^2(1) = 0.07$, $p = 0.79$		
Individual controls	Yes	Yes	Yes
Tract controls	Yes	Yes	Yes
ZIP-code fixed effects	Yes	Yes	Yes

Notes: OLS estimates. The dependent variable is the share of policy items the respondent supports, scaled 0–1. Column 1 uses all ten items; column 2 the two backward-looking response items (buyouts of repeatedly flooded homes; compensation for homes flooded by reservoir releases); column 3 the eight forward-looking mitigation items. Each panel uses one of the three damage measures of Table 3: self-reported damage (binary), the share of housing units affected in the respondent's census tract (continuous), and an indicator for above-median tract damage share. All specifications include individual controls (party identification, race, age, education, income, evacuation, years in town, home ownership, insurance, survey wave), six ACS 2015 tract controls, and ZIP-code fixed effects, and are weighted by survey weights; standard errors, in parentheses, are clustered at the census-tract level. The Wald test stacks columns 2 and 3 by seemingly unrelated estimation and tests equality of the Diversity × Damage coefficients. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Conclusion

We examined how disaster exposure and ethnic diversity relate to different forms of cooperation. Our main finding is that cooperation does not respond uniformly to shocks: the relationship between disaster exposure and cooperation varies across domains, depending on the feasibility of monitoring.

Using observational and experimental data collected around Hurricane Harvey, we uncovered two patterns. First, we found no relationship between diversity and interpersonal, face-to-face cooperation following the disaster, where actions are directly observable. Second, for costly forms of cooperation in the provision of public goods, diversity moderates responses to shocks: while exposure to damage is associated with greater support for climate recovery and adaptation policies, this relationship weakens and reverses in more diverse areas.[17]

Together, our findings suggest that diversity is not uniformly associated with lower cooperation; rather, it shapes different forms of cooperation in distinct ways. This distinction matters for interpreting the role of race and diversity in disaster politics. While recent work shows that individual racial attitudes shape disaster-spending preferences (Gilens et al., 2025), our results point to an additional contextual mechanism: local ethnic diversity reduces support for climate recovery and adaptation policies when those policies require contributions that are difficult to observe and sanction. Where contributions are observable, we do not detect a consistent diversity penalty; where contributions are difficult to monitor and vulnerable to free-riding, cooperation declines with diversity.

[17] An alternative interpretation is that diversity penalizes weak-tie cooperation but not strong-tie cooperation. However, this account is difficult to reconcile with the finding that the diversity penalty increases with the stakes of cooperation. Under the monitoring argument, larger contributions increase incentives for free-riding, so the diversity penalty should increase with the stakes, as in the high-tax options in the conjoint analysis (Table A14). A tie-strength account, by contrast, predicts a uniform penalty regardless of the size of the contribution.

More broadly, the results suggest that institutional features which make contributions more observable may shape how diversity affects cooperation. In the context of disaster recovery and climate adaptation, policies that rely on contributions that are difficult to monitor are likely to be more sensitive to social characteristics that shape the scope for free-riding. Civic-association membership may serve as one such observable signal in more diverse settings. Future work can assess whether these patterns extend across contexts and types of shocks.

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Online Appendix

Ethnic Diversity and Cooperation After Disaster: Evidence from Hurricane Harvey

Table of Contents

A1 Descriptive statistics

Table A1 presents descriptive statistics for the full pooled three-wave dataset. Because each survey wave fielded a different subset of questions, per-variable observation counts vary across rows: the pre-Harvey coordination items and the policy-support items were asked in Wave 1 only, the help-received item was asked in Waves 1 and 2, and the ethnic diversity and tract damage measures are merged from administrative sources for every respondent with a valid census-tract identifier. Table A2 restricts the sample to the 1,904 Wave 1 respondents with non-missing ethnic diversity, self-reported damage, and individual covariates—the analytic sample for the pre-Harvey coordination regressions in Table 1. The post-Harvey help-received models (Table 2) and policy-support models (Table 3) draw on a larger Wave 1 + Wave 2 pooled sample (N ranging 1,770–2,099, reported directly in those tables); the cooperation and helping outcomes that also appear in Table A2 are tabulated there only for the Wave 1 subset and should not be read as descriptives of the post-Harvey analytic samples. The Wave 1+2 analytic samples have very similar covariate distributions and are not reproduced separately. Ethnic diversity is based on ACS 2015 five-year tract estimates; tract damage is the continuous share of housing units affected.

Figure A1 plots the distribution of the continuous tract-level damage measure used throughout the observational analyses.

Table A1: Descriptive statistics for all observations.

Variable	Obs	Mean	Std. Dev.	Min	Max
Cooperation and helping outcomes					
Coordinated with immediate family (pre-Harvey)	1995	0.230	0.421	0.00	1.00
Coordinated with extended family (pre-Harvey)	1995	0.278	0.448	0.00	1.00
Coordinated with neighbors (pre-Harvey)	1995	0.127	0.333	0.00	1.00
Coordinated with friends (pre-Harvey)	1995	0.140	0.347	0.00	1.00
Received help after Harvey	2495	0.319	0.466	0.00	1.00
Provided help after Harvey	1998	0.623	0.485	0.00	1.00
Policy support index (0–1)	1993	0.783	0.188	0.00	1.00
Key predictors					
Ethnic diversity (fractionalization index)	2582	0.507	0.156	0.000	0.763
Tract damage share (prop. of housing units)	2414	0.118	0.188	0.000	1.000
Self-reported damage (indicator)	2503	0.602	0.490	0	1
Individual characteristics					
Party identification (1–9)	2503	3.069	2.359	1	9
Race/ethnicity (1–9)	2503	2.191	1.889	1	9
Age category (1–4)	2503	3.089	0.941	1	4
Education (1–9)	2503	4.306	1.709	1	9
Household income (1–9)	2503	4.436	2.710	1	9
Residential characteristics					
Years in town (1–9 brackets)	2446	3.092	2.536	1	9
Homeowner	2423	0.808	0.394	0	1
Insurance coverage	2471	0.579	0.494	0	1
Survey design					
Wave 1 respondent	2503	0.800	0.400	0	1
Survey weight	2503	0.608	0.720	0.001	2.616
Tract-level ACS 2015 five-year estimates					
Median household income ($10k)	2578	7.38	4.01	0.88	25.00
Share renter-occupied (%)	2580	0.36	0.23	0.01	1.00
Share foreign-born (%)	2582	0.22	0.12	0.00	0.73
Share below poverty line (%)	2580	0.14	0.12	0.00	0.79
Share moved in last year (%)	2582	0.16	0.08	0.00	0.80
Population density (per km^2)	2582	1648.15	1287.08	1.43	24099.97

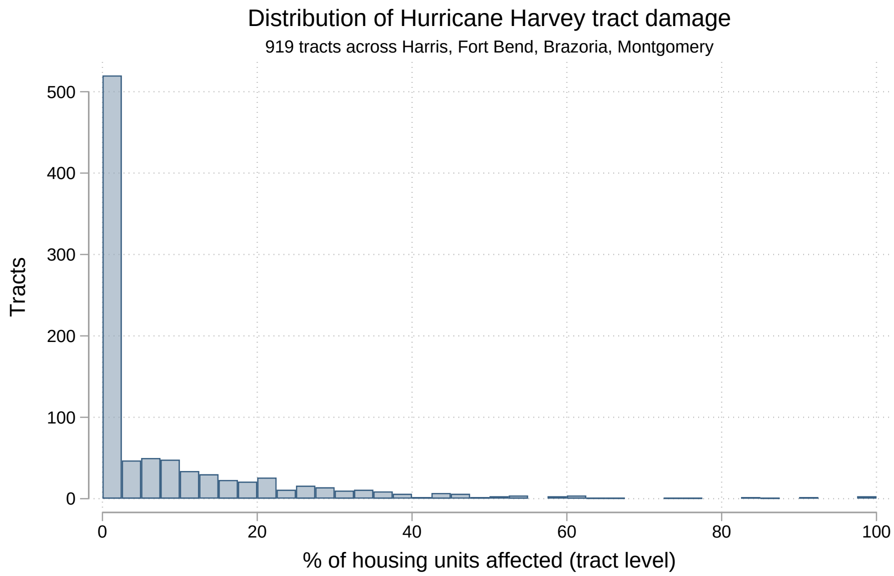
Notes: Ethnic diversity is the fractionalization index constructed from ACS 2015 five-year race estimates (`elf_acs2015`). Tract damage share is the proportion of housing units with recorded damage (City of Houston Open Data); range is 0–1 (1 = all units affected). Tract-level ACS 2015 controls use the five-year estimates merged by census tract GEOID.

Table A2: Descriptive statistics for the Table 1 analytic sample ($N = 1{,}904$).

Variable	Obs	Mean	Std. Dev.	Min	Max
Cooperation and helping outcomes					
Coordinated with immediate family (pre-Harvey)	1904	0.234	0.423	0.00	1.00
Coordinated with extended family (pre-Harvey)	1904	0.284	0.451	0.00	1.00
Coordinated with neighbors (pre-Harvey)	1904	0.133	0.340	0.00	1.00
Coordinated with friends (pre-Harvey)	1904	0.144	0.352	0.00	1.00
Received help after Harvey	1896	0.329	0.470	0.00	1.00
Provided help after Harvey	1901	0.624	0.485	0.00	1.00
Policy support index (0–1)	1900	0.784	0.188	0.00	1.00
Key predictors					
Ethnic diversity (fractionalization index)	1904	0.509	0.153	0.051	0.760
Tract damage share (% housing units)	1768	0.1	0.2	0	1
Self-reported damage (indicator)	1904	0.665	0.472	0	1
Individual characteristics					
Party identification (1–9)	1904	3.040	2.368	1	9
Race/ethnicity (1–9)	1904	2.180	1.928	1	9
Age category (1–4)	1904	3.175	0.902	1	4
Education (1–9)	1904	4.337	1.679	1	9
Household income (1–9)	1904	4.465	2.739	1	9
Residential characteristics					
Years in town (1–9 brackets)	1904	3.116	2.578	1	9
Homeowner	1904	0.834	0.373	0	1
Insurance coverage	1904	0.520	0.500	0	1
Survey design					
Wave 1 respondent	1904	1.000	0.000	1	1
Survey weight	1904	0.755	0.740	0.001	2.616
Tract-level ACS 2015 five-year estimates					
Median household income ($10k)	1904	7.65	4.08	1.41	25.00
Share renter-occupied (%)	1904	0.34	0.22	0.01	1.00
Share foreign-born (%)	1904	0.21	0.12	0.01	0.73
Share below poverty line (%)	1904	0.13	0.11	0.00	0.63
Share moved in last year (%)	1904	0.15	0.08	0.01	0.53
Population density (per km^2)	1904	1614.32	1128.92	5.72	12765.08

Notes: Restricted to the 1,904 Wave 1 respondents with non-missing ethnic diversity, self-reported damage, and individual covariates—the analytic sample for the pre-Harvey coordination regressions (Table 1). Cooperation and helping outcomes (received help, provided help, policy-support index) are tabulated for this same Wave 1 subsample; the post-Harvey help-received and policy-support regressions (Tables 2 and 3) use a larger Wave 1 + Wave 2 pooled sample whose analytic Ns are reported in the corresponding result tables. Wave 1 respondent = 1.000 (Table 1 analysis is restricted to the 2017 survey wave).

Figure A1: Distribution of Hurricane Harvey tract-level damage.



Notes: Distribution of the continuous damage measure (share of housing units affected) across census tracts in Harris, Fort Bend, Brazoria, and Montgomery counties with non-missing damage data. Source: City of Houston Open Data, tract-level administrative damage counts.

A2 Survey and sampling details

2017 survey wave. A university-affiliated public affairs research institute conducted a baseline telephone survey in November and December 2017 to understand the experiences of people impacted by Hurricane Harvey and to gauge their support for flood prevention policies. A total of 2,002 respondents aged 18 and older and residing in Harris, Fort Bend, Brazoria, and Montgomery counties were interviewed. The survey was conducted by telephone (50 percent landline, 50 percent cell phone) between November 20 and December 20, 2017. The four counties were divided into eight sampling areas based on whether structures in a given area had a high or low probability of significant flooding. Areas in each county that received two or more feet of rainwater between August 26 and 30 were identified as having the greater potential for flooding to residential and commercial property and used to define the high-risk strata. A sample of active landline and cell phone numbers matched to residential household street addresses was purchased from Marketing Systems Group in Horsham, PA.[1] Interviews were conducted in English and Spanish by Customer Research International of San Marcos, Texas, and sampling was based on the proportion of households in each county and in areas where flooding was expected to have occurred.

Oversampling of areas where flooding was expected to have occurred produced an imbalance in the share of owner- and renter-occupied households in Harris County. We constructed post-stratification weights to address this imbalance and to align the sample with U.S. Census Bureau benchmarks; details of the weight construction are summarized in the "Sample weights" section below. Demographic measures for the combined four-county sample fall within ACS ranges, so no additional adjustment was implemented. Our observational models also include tract-level measures of storm damage, so differences in flood risk are accounted for directly as covariates rather than through additional sampling weights. The margin of error for the full sample of 2,002 respondents is ± 2.2 percentage points; margins of error for subgroups (e.g., by county or race/ethnicity) are larger. These data, merged with tract-level administrative measures of ethnic diversity and storm damage, form part of the pooled dataset used for the observational analyses of pre-Harvey coordination, mutual help, and support for recovery policies reported in Tables 1–3 of the main text.

2018 survey wave. A second telephone survey was conducted between June 25 and July 31, 2018 with 1,073 respondents in Brazoria, Fort Bend, Harris, and Montgomery counties. Slightly more than half of these respondents ($N = 572$) had participated in the 2017 wave, while the remaining 501 respondents were drawn from a fresh sample in the same counties. The margin of error for the

[1] Street addresses were purchased from Marketing Systems Group in Horsham, PA.

full 2018 sample is ± 3 percentage points; for the Harris County subsample it is ± 3.4 percentage points, with larger margins of error for smaller subgroups.

As in 2017, interviews were conducted by telephone (50 percent landline, 50 percent cell phone). The panel component of the sample—those first interviewed in 2017—was drawn from areas expected to have experienced household flooding (areas receiving two or more feet of rainwater between August 26 and 30, 2017). The fresh cross-sectional sample of residential households was randomly selected in each of the four counties. As in 2017, active landline and cell phone numbers matched to residential street addresses were purchased from the same sample vendor, and interviews were administered in English and Spanish by the same field firm, with only one phone number per household selected at each address. Because oversampling of flood-prone areas generated an imbalance in the share of owner- and renter-occupied households in Harris County, survey weights were constructed to align the distribution of tenure and race across Brazoria, Fort Bend, Harris, and Montgomery counties with the 2016 ACS. Together with the 2017 baseline, the 2018 wave extends the panel and supplies additional observations for the pooled observational models of coordination before Harvey, help received, and support for recovery policies (Tables 1–3).

2020 survey wave. A third survey wave was fielded online between May 20 and June 23, 2020, using a representative sample of adults (18+) residing in Brazoria, Fort Bend, Harris, and Montgomery counties. In total, 1,065 individuals completed the questionnaire. Data collection was conducted in English and Spanish by Customer Research International (CRI), a survey research firm headquartered in San Marcos, Texas.

Sampling for the 2020 wave relied on demographic quotas for county, age, race, and gender based on 2019 American Community Survey benchmarks. Post-stratification weights were then applied to match the joint distribution of race, sex, age, and party identification in the target population. The margin of error for the full 2020 sample is approximately ± 3 percentage points, and

± 4 percentage points for the Harris County subsample; margins of error for smaller subgroups are larger. This online wave included the two conjoint experiments analyzed in the main text: the policy conjoint on willingness to support costlier recovery measures and the partner-choice conjoint on cooperation across partisan, religious, racial, gender, and associational identities.

Sample weights. For the pooled observational analyses in Tables 1–3, we use a single survey-weight variable that stacks the wave-specific design weights we constructed for each wave. The weights consist of wave-specific post-stratification adjustments designed to align the survey distribution of demographic and tenure variables with the corresponding ACS five-year estimates for the four-county study area; the precise margins targeted differ by wave (the 2017 weight primarily corrects the tenure imbalance in Harris County induced by flood-area oversampling, while the 2018 and 2020 weights align demographic and tenure margins across the four-county area). We apply these weights as provided in all observational analyses; we do not construct or modify the weight variable. Models include an indicator for survey wave.

Missing data in Tables 1, 2, and 3. The observational analyses (Tables 1–3) pool Wave 1 and Wave 2 and use tract-level ethnic diversity. Wave 3, the 2020 online wave, is used only for the conjoint analyses, where diversity is constructed at the ZIP-code level (matched to ZIP Code Tabulation Areas) because Wave 3 collected ZIP codes rather than street addresses; see Online Appendix Section A6. The ZIP-to-ZCTA match is essentially complete for any ZIP code in the Census ZCTA frame, so missingness in the conjoint sample is driven by item-level non-response rather than geographic matching.

For the Wave 1 + Wave 2 observational frame, the ethnic diversity index has essentially complete coverage of Wave 1 respondents: all 2,002 Wave 1 observations have a non-missing tract-level diversity value. Wave 2 respondents lack tract-level diversity only when their address could not be geocoded to a census tract. The remaining missingness in Tables 1–3 is driven by item-level non-response and a small number of address-geocoding failures. Self-reported damage (`damage_sub`) is complete for the Wave 1 frame (0 missing), because it is constructed entirely from survey items asked of all 2017 respondents. The main source of sample loss for the continuous-damage specification is tract-level administrative damage (`damage_share`): 141 Wave 1 respondents (7.0%) could not be matched to a tract with damage records; all 141 reside in Brazoria County, which the City of Houston damage records do not cover. Outcome item non-response is minimal—fewer than 10 Wave 1 observations are missing on any of the three main outcomes. Individual covariates (homeownership, insurance, in-town status) account for most of the remaining exclusions. Table A3 summarizes missingness for each key variable in the pooled three-wave file and the Wave 1 frame; the bottom panel reports the analytic sample sizes for each observational table.

Table A3: Missingness summary for key variables.

	Full file (N = 2724) N missing (%)	Wave 1 frame (N = 2002) N missing (%)
Key predictors		
Diversity	142 (5.2%)	0 (0.0%)
Tract damage share	310 (11.4%)	141 (7.0%)
Self-reported damage	221 (8.1%)	0 (0.0%)
Outcomes		
Coord. with neighbors (pre-Harvey)	729 (26.8%)	7 (0.3%)
Received help after Harvey	229 (8.4%)	8 (0.4%)
Policy support index	731 (26.8%)	9 (0.4%)
Individual controls with missing values		
Homeowner	301 (11.0%)	75 (3.7%)
Insurance coverage	253 (9.3%)	25 (1.2%)
In town during Harvey	278 (10.2%)	0 (0.0%)
Evacuated	224 (8.2%)	2 (0.1%)
Analytic sample sizes (self-reported damage specification)		
Table 1 (pre-Harvey coordination)	$N = 1904$	
Table 2 (interpersonal help)	$N = 2099$	
Table 3 (policy support)	$N = 1905$	

Notes: "Full file" includes all three survey waves (N = 2,724). "Wave 1 frame" is restricted to 2017 respondents (N = 2,002). N *missing* counts observations with a missing value for each variable; the percentage is relative to the corresponding frame. Ethnic diversity (`elf_acs2015`) achieves complete coverage of the Wave 1 frame. Analytic sample sizes in the bottom panel reflect the self-reported damage specification in Tables 1, 2, and 3.

Attrition analysis. Table A4 reports linear probability models (LPMs) predicting whether a respondent is missing from each of the three main analytic samples. Column (1) asks whether Wave 1 respondents are absent from the Table 3 (policy support) sample; column (2) asks whether any respondent in the full three-wave file is absent from the Table 2 (interpersonal help) sample; and column (3) repeats column (1) for the Table 1 (pre-Harvey coordination) sample. Predictors are ethnic diversity, tract-level damage share, and three coarsened demographic indicators (white, above-median income, college-educated).

The key finding is that diversity does not predict attrition in any of the three samples (all diversity coefficients are small and statistically indistinguishable from zero). Respondents in more-damaged tracts are, if anything, *less* likely to be missing: the damage coefficient is negative across all three columns, significant at $p \leq 0.05$ in the Table 3 and Table 2 samples and at $p \leq 0.10$ in the Table 1 sample. This pattern is consistent with Harvey-affected respondents being more engaged with the study. Some demographic predictors are significant—White respondents are marginally less likely to attrit ($p < 0.10$), college-educated respondents are significantly less likely to attrit in the Table 3 and Table 2 samples ($p < 0.05$), and above-median-income respondents are *more* likely to be missing in the Table 3 and Table 1 samples ($p < 0.05$)—but ethnic diversity is orthogonal to these demographic patterns, so the diversity coefficient in the main tables is not contaminated by differential attrition.

Table A4: Attrition: linear probability models predicting absence from each analytic sample.

	Missing from Table 3	Missing from Table 2	Missing from Table 1
	(1)	(2)	(3)
	Wave 1 frame	Full file	Wave 1 frame
Diversity	-0.033	-0.015	-0.009
	(0.036)	(0.049)	(0.038)
Tract damage share (%)	-0.050**	-0.113**	-0.042*
	(0.022)	(0.049)	(0.023)
White respondent	-0.023*	-0.032*	-0.022*
	(0.012)	(0.017)	(0.012)
Above-median income	0.033**	0.016	0.027**
	(0.014)	(0.017)	(0.013)
College-educated	-0.035**	-0.043**	-0.024*
	(0.014)	(0.018)	(0.013)
Observations	1382	1652	1382
R-squared	0.015	0.010	0.009

Notes: Dependent variable = 1 if respondent is not in the analytic sample. Column (1) and (3): estimated on Wave 1 respondents with non-missing predictors ($N = 1{,}382$). Column (2): estimated on all respondents with non-missing predictors ($N = 1{,}652$). Damage share is on a 0–1 scale. Standard errors clustered at census tract. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

A3 Correlations between ethnic diversity and tract-level controls

More diverse tracts may be lower-income, more transient, or more renter-heavy, so that the diversity coefficient could reflect a bundle of socioeconomic confounds. Table A5 shows that this is not the case. Ethnic diversity is uncorrelated with median household income ($r = 0.03$), essentially uncorrelated with density ($r = 0.01$) and the foreign-born share ($r = 0.04$), and *negatively* correlated with poverty ($r = -0.26$): the more diverse tracts are *less* poor on average. The most diverse neighborhoods are middle-class Hispanic, Asian, and mixed-ethnicity enclaves (Alief, Spring Branch, Gulfton, Sharpstown, Westchase), not distressed areas. Adding tract controls to the main regressions barely moves the diversity coefficient.

Table A5: Pairwise correlations between ethnic diversity and tract-level ACS 2015 controls.

Tract characteristic	r with ethnic diversity
Median household income	$+0.03$
% renter-occupied	$+0.09$
% foreign-born	$+0.04$
% below poverty	-0.26
% moved in last year	$+0.24$
Population density	$+0.01$
Tract-level damage share	-0.24

Notes: Pairwise Pearson correlations computed at the unique-tract level ($N = 543$ tracts with non-missing diversity). Ethnic diversity is the fractionalization index from ACS 2015 race estimates (`elf_acs2015`).

The conjoint analyses measure diversity at the ZIP-code level. Table A6 reports the corresponding correlations across the 251 ZIP codes in the Wave 3 policy-conjoint sample. Diversity is weakly positively correlated with income ($r = 0.16$) and negatively with poverty ($r = -0.17$).

Table A6: Pairwise correlations between ethnic diversity and ZIP-code-level ACS 2015 controls.

ZIP-code characteristic	r with ethnic diversity
Median household income	0.16
% renter-occupied	0.18
% foreign-born	0.29
% below poverty	-0.17
% moved in last year	0.23

Notes: Pairwise Pearson correlations across the 251 ZIP codes represented in the Wave 3 policy-conjoint sample. Diversity is the fractionalization index carried in the conjoint data.

A4 Pre-Harvey coordination: tests across tie types

Table A7 tests whether the diversity coefficients in Table 1 differ across tie types. The four equations are stacked by seemingly unrelated estimation, which accounts for the correlation of estimates across outcomes reported by the same respondents; the joint estimation uses importance weights and clusters standard errors at the census-tract level. Each coefficient is compared with the neighbors coefficient, and the final row tests equality of all four.

Table A7: Diversity coefficients by tie type and cross-equation tests.

	Point estimate on Diversity		Cross-equation Wald test vs. Neighbors	
Outcome	Coefficient	Std. error	χ^2	p-value
Immediate family	0.084	0.130	3.309	0.069
Extended family	-0.079	0.154	0.396	0.529
Neighbors	-0.187	0.113	—	—
Friends	-0.162	0.159	0.032	0.857
Joint: all four equal	—		3.386 (df = 3)	0.336

Notes: Coefficients from the specification of Table 1 with tract controls. χ^2 statistics from Wald tests on the stacked equations.

A5 Additional damage specifications

Interpersonal cooperation: additional damage specifications

Table A8 reports the interpersonal-cooperation models under the two damage specifications not shown in the main text: administrative binary (any housing units affected) and log(1+% affected).

Table A8: Interpersonal cooperation: binary and log damage specifications.

	Received help		Provided help		log(1+Vol. known)		Coop. index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Binary	log(1+%)	Binary	log(1+%)	Binary	log(1+%)	Binary	log(1+%)
Diversity	0.157	0.036	0.193	-0.069	1.032	0.168	0.401	-0.010
	(0.284)	(0.235)	(0.282)	(0.259)	(1.035)	(0.771)	(0.420)	(0.354)
Damage	0.265	0.113**	0.083	-0.069	0.829	-0.100	0.412	0.021
	(0.176)	(0.049)	(0.189)	(0.051)	(0.610)	(0.135)	(0.265)	(0.070)
Diversity × Damage	-0.585*	-0.144	-0.162	0.030	-1.008	-0.108	-0.744*	-0.118
	(0.299)	(0.091)	(0.306)	(0.102)	(1.045)	(0.260)	(0.438)	(0.127)
Observations	1949	1949	1770	1770	1359	1359	1770	1770
Adj. R-squared	0.178	0.180	0.164	0.168	0.231	0.234	0.172	0.171

Notes: Structure mirrors Table 2. Binary = administrative tract indicator (any damage); log(1+%) = log of one plus the percentage of housing units affected. All specifications include individual controls (party ID, age, education, race, income, evacuation, years in town, survey wave), ZIP-code fixed effects, and the same six ACS 2015 tract controls used in Table 2 (median household income, % renter, % foreign-born, population density, % below poverty, % moved in the past year). Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Interpersonal cooperation: damaged-only subsample

Table A9 re-estimates the interpersonal-cooperation models on the subsample of respondents whose census tract experienced any administrative damage (damage_share > 0). Within this subsample, damage continues to vary across tracts—from a small share of housing units affected to a substantial share—so the Diversity × Damage interaction is still identified by within-subset variation in damage share. The restriction matters for the survey-wording concern because it drops the zero-damage tracts, where the help-received variable defaults to its pre-Harvey component (there is no damage to recover from); for the remaining respondents, "help in the days immediately before or after Harvey" is dominated by post-storm recovery. The diversity main effect, the damage main effect, and the Diversity × Damage interaction are null across all four outcomes, confirming that the null interaction in Table 2 is not an artifact of the survey wording bundling preparedness and recovery help.

Table A9: Interpersonal cooperation: damaged-tract subsample.

	(1) Received help	(2) Provided help	(3) log(1+Vol. known)	(4) Coop. index
Diversity	-0.030	-0.222	0.055	-0.190
	(0.255)	(0.290)	(0.756)	(0.369)
Damage	0.657	-0.834*	-0.672	-0.200
	(0.429)	(0.471)	(1.033)	(0.540)
Diversity × Damage	-0.913	0.554	-0.912	-0.589
	(0.851)	(0.969)	(2.159)	(1.106)
Observations	1174	1089	840	1089
Adj. R-squared	0.218	0.205	0.287	0.240

Notes: Sample restricted to respondents in census tracts with non-zero administrative damage share (damage_share > 0). Diversity is the ACS 2015 fractionalization index. Damage is the continuous tract-level damage share. All specifications include individual controls (party ID, age, education, race, income, evacuation, years in town, survey wave), ZIP-code fixed effects, and six ACS 2015 tract controls (median household income, % renter, % foreign-born, population density, % below poverty, % moved in the past year). Survey weights included. Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Interpersonal cooperation: magnitudes

Table A10 reports, for each interpersonal-cooperation outcome, the Diversity × Damage estimate from the continuous-damage specification of Table 2 together with its 95% confidence interval, its translation into percentage points, and the mean of the outcome in the estimation sample. For a resident of a heavily damaged tract, moving from the first to the third quartile of tract diversity changes the probability of having provided help by -0.6 points and of having received help by

-5.9 points. The first interval excludes penalties of the size we find for policy support; the second does not.

Table A10: Interpersonal cooperation: Diversity × Damage estimates with confidence intervals.

Outcome	Diversity × Damage	Std. error	95% CI	IQR shift (pp)	Mean of DV	N
Received help	-1.127	0.713	[-2.526, 0.273]	-5.9 [-13.2, 1.4]	0.339	1949
Provided help	-0.117	0.812	[-1.712, 1.478]	-0.6 [-8.9, 7.7]	0.601	1770
log(1 + volunteers)	-2.235	2.008	[-6.180, 1.710]	—	1.914	1359
Cooperation index	-1.468	1.019	[-3.470, 0.534]	-7.7 [-18.1, 2.8]	-0.039	1770

Notes: Continuous-damage specification of Table 2. IQR shift is the coefficient and its confidence interval multiplied by the interquartile range of tract diversity (0.41 to 0.64) and the 75th percentile of tract damage share (0.22), in percentage points of the outcome; not defined for the log-volunteers outcome. Mean of DV is the unweighted mean of the outcome in the estimation sample.

Policy support: additional damage specifications

Table A11 reports the policy-support model under binary and log damage specifications.

Table A11: Support for recovery policies: binary and log damage specifications.

	Support for Policies (Index)	
	(1)	(2)
	Binary	log(1+%)
Diversity	-0.042	0.053
	(0.116)	(0.101)
Damage	0.063	0.023
	(0.073)	(0.019)
Diversity × Damage	-0.057	-0.076**
	(0.117)	(0.035)
Observations	1770	1770
Adj. R-squared	0.098	0.103

Notes: Binary = administrative tract indicator (any damage); log(1+%) = log of one plus the percentage of housing units affected. All specifications include individual controls (party ID, age, education, race, income, evacuation, years in town, survey wave), ZIP-code fixed effects, and the same six ACS 2015 tract controls used in Table 3 (median household income, % renter, % foreign-born, population density, % below poverty, % moved in the past year). Standard errors clustered at the census-tract level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Policy support: binned interaction

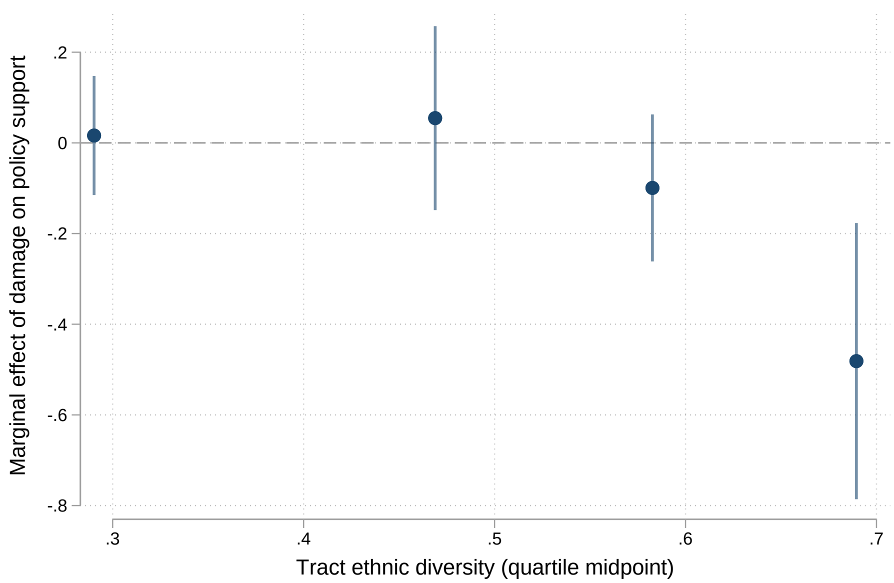
Table A12 and Figure A2 relax the linear interaction in Table 3: diversity is divided into quartiles and the damage slope is estimated separately within each, with the same controls, weights, and clustering. A joint test rejects equality of the four slopes ($F(3, 625) = 3.33$, $p = 0.019$).

Table A12: Marginal effect of damage on policy support by quartile of tract diversity.

Diversity quartile	ME(damage)	95% CI	Std. error
Q1 (least diverse)	0.016	[-0.12, 0.15]	0.067
Q2	0.055	[-0.15, 0.26]	0.104
Q3	-0.099	[-0.26, 0.06]	0.083
Q4 (most diverse)	-0.481	[-0.79, -0.18]	0.155

Notes: Quartile-specific slopes on tract damage share from the specification of Table 3. Standard errors clustered at the census-tract level.

Figure A2: Marginal effect of damage on policy support by quartile of tract diversity.



Notes: Points are the quartile-specific slopes of Table A12, plotted at the quartile midpoint; bars are 95% confidence intervals.

Policy support: individual policy items

Table A13 estimates separate models for each of the ten individual policy proposals that make up the index used in Table 3, under each of the three damage measures used there. Respondents were asked if they supported or opposed the following policies: (1) a program to buy homes in areas that have repeatedly flooded with local and federal moneys, (2) the construction of a new reservoir to protect the western portion of the Houston area, (3) greater restrictions on construction in flood plains and new building codes, (4) the establishment of a regional flood agency with taxing authority to plan for the prevention of regional flooding, (5) denying federally financed flood insurance to homeowners whose homes have flooded three or more times since 2001, (6) not allowing homes that have flooded three or more times since 2001 to be rebuilt by buying out these homeowners with local and federal moneys, (7) requiring sellers of homes to fully disclose prior flood damage to their homes and prior flooding in the surrounding neighborhood, (8) preventing development/construction on native prairies and wetlands in western and northwestern portions of Harris County, (9) requiring government compensation for homes that are flooded due to the release of water from local reservoirs, and (10) new building codes that require homes built in flood-prone areas to be elevated/raised to avoid flooding.

The items that reach statistical significance vary with the damage measure. Under self-reported damage, the buyout program (-0.41, $p < 0.05$) and the new reservoir (-0.28, $p < 0.10$) are significant; under the continuous damage share, buyouts in place of rebuilding, restrictions on floodplain construction, and the denial of federal flood insurance to repeat claimants are (all $p < 0.05$), with compensation for reservoir-release flooding at $p < 0.10$; under the above-median indicator, buyouts in place of rebuilding and the denial of flood insurance are. Only buyouts in place of rebuilding is significant under two measures. The pattern does not follow the split between spending and regulatory items, which Table 4 in the main text tests directly.

Table A13: Support for individual policies: Diversity × Damage by damage measure.

Policy item	Self-reported damage (1)	Continuous damage share (2)	Above-median damage (3)
Buyout program for repeatedly flooded homes	-0.413**	-0.916	-0.162
	(0.206)	(0.860)	(0.338)
New reservoir to protect western Houston	-0.278*	-0.280	-0.016
	(0.143)	(0.558)	(0.177)
Restrictions on flood-plain construction	-0.207	-1.568**	-0.177
	(0.162)	(0.678)	(0.222)
Regional flood agency with taxing authority	-0.022	-0.748	-0.288
	(0.217)	(0.654)	(0.273)
Deny federal flood insurance to repeat claimants	-0.201	-1.834**	-0.613**
	(0.219)	(0.823)	(0.291)
Buyouts in place of rebuilding repeatedly flooded homes	-0.031	-2.142**	-0.774**
	(0.234)	(0.873)	(0.316)
Seller disclosure of prior flooding	-0.071	0.081	0.221
	(0.104)	(0.452)	(0.144)
No development on prairies and wetlands	0.062	-1.002	-0.271
	(0.196)	(0.873)	(0.313)
Compensation for reservoir-release flooding	-0.280	-1.231*	-0.291
	(0.188)	(0.674)	(0.220)
Elevation building codes in flood-prone areas	-0.079	0.093	-0.138
	(0.146)	(0.548)	(0.179)

Notes: Each cell reports the Diversity × Damage coefficient from a separate OLS regression of support for the indicated item (0/1) on diversity, damage, and their interaction, under the damage measure in the column header: self-reported damage (binary), the share of housing units affected in the tract (continuous), and an indicator for above-median tract damage share. All specifications include individual controls (party identification, race, age, education, income, evacuation, years in town, home ownership, insurance, survey wave), six ACS 2015 tract controls, and ZIP-code fixed effects, and are weighted by survey weights; standard errors, in parentheses, are clustered at the census-tract level. N ranges from 1,500 to 1,776 across cells. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

A6 Policy conjoint

Table A14 reports the policy conjoint estimates for both the full sample and the Houston-only subsample (ZIP codes beginning with 77). In the conjoint analyses, both the diversity moderator and the contextual covariates are merged onto respondents' ZIP codes, matching each ZIP to the corresponding ZIP Code Tabulation Area (ZCTA)—the U.S. Census areal approximation of a ZIP code. The 2020 (Wave 3) survey collected respondents' ZIP codes rather than street addresses, so tract-level matches (used in the observational analyses, Tables 1–3) were unavailable. Diversity is constructed as the ethnic fractionalization index applied to ACS 2014–2018 five-year race counts aggregated within each ZCTA (the most recent estimates available when Wave 3 was fielded); the same Herfindahl-style formula used for the observational tract-level measure is applied at this coarser ZIP scale. We refer to these as ZIP-code measures throughout the body of the paper. All specifications include ZIP-code ACS 2015 covariates: median household income, % renter-occupied, % foreign-born, and % below poverty.

Table A14: Policy conjoint: affected × high tax and high diversity × high tax, by sample.

	Affected × Tax (full)		Affected × Tax (Houston)		HighDiv × Tax (full)		HighDiv × Tax (Houston)	
	(1) Baseline	(2) + ZCTA	(3) Baseline	(4) + ZCTA	(5) Baseline	(6) + ZCTA	(7) Baseline	(8) + ZCTA
High tax in pair=1	-23.860***	-24.147***	-23.135***	-23.308***	-11.084***	-11.085***	-10.530***	-10.530***
	(1.484)	(1.486)	(1.529)	(1.530)	(2.581)	(2.581)	(2.888)	(2.888)
Affected by Harvey=1 × High tax in pair=1	7.825***	8.002***	3.982	4.155*				
	(2.283)	(2.284)	(2.423)	(2.423)				
Affected by Harvey=1	-6.231**	-6.195**	-1.853	-1.829				
	(2.936)	(2.940)	(3.142)	(3.147)				
ZCTA median HH income (10k USD)		-0.140		-0.148		-0.206		-0.238
		(0.355)		(0.381)		(0.622)		(0.702)
ZCTA % renter		0.628		0.709		-0.144		-0.039
		(4.268)		(4.553)		(6.747)		(7.439)
ZCTA % foreign-born		-1.645		-1.365		-1.631		-3.519
		(5.992)		(6.628)		(9.804)		(11.302)
ZCTA % poverty		-3.869		-4.246		-3.181		-3.055
		(12.246)		(13.036)		(20.894)		(23.551)
High diversity (≥ sample mean)=1 × High tax in pair=1					-9.426***	-9.425***	-15.140***	-15.139***
					(3.553)	(3.553)	(3.847)	(3.847)
High diversity (≥ sample mean)=1					-0.222	-0.167	-1.133	-1.097
					(4.579)	(4.672)	(5.022)	(5.131)
Observations	7138	7122	6392	6386	3012	3012	2548	2548

Notes: Linear probability mixed model. Coefficients on the 0–100 choice probability scale. "Full sample" matches the original pipeline; "Houston only" restricts to ZIP codes beginning with 77. Dollar-rank ties (pairs where both profiles fall in the same dollar-equivalent tax bracket) are dropped in all columns. Models include ZIP-code ACS 2015 controls (median household income, % renter, % foreign-born, % below poverty).

Table A15 adds respondent party identification to Equation 5, first as a control and then interacted with the tax attribute, so that each party group has its own tax aversion. Table A16 re-estimates the interaction separately on pairs that compare two options with the same tax base, or a tax with no tax, and on pairs that compare a property tax with a sales tax.

Table A15: Policy conjoint: the diversity–tax interaction and respondent partisanship.

	(1) Baseline	(2) + Party ID	(3) + Party ID × Tax
Higher diversity × High tax	-15.139	-15.142	-15.214
	(3.847)	(3.847)	(3.855)
Observations	2548	2548	2548

Notes: Linear probability mixed model of Equation 5, Houston-only affected respondents, with ZIP-code ACS 2015 controls. Column (2) adds respondent party identification; column (3) adds its interaction with the high-tax indicator. Standard errors in parentheses.

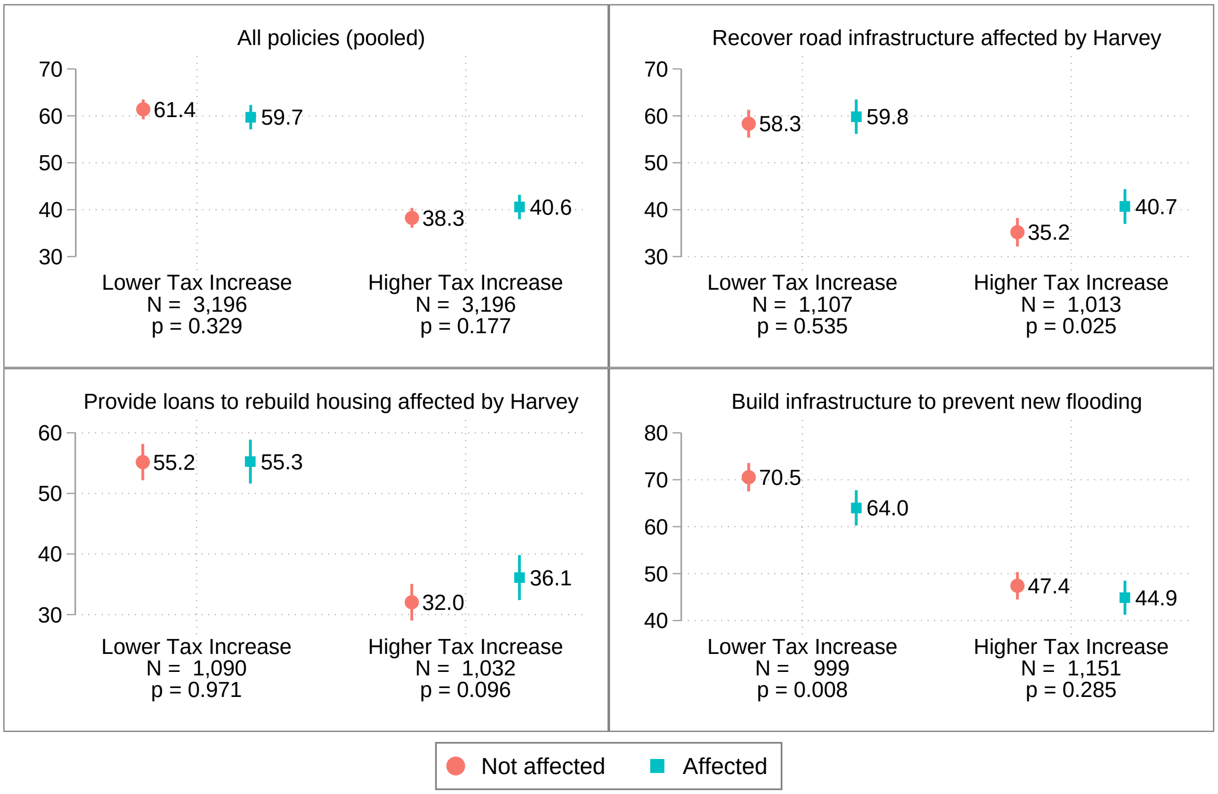
Table A16: Policy conjoint: the diversity–tax interaction by tax-base composition of the pair.

	All pairs (1)	Excluding cross-base (2)	Cross-base only (3)
Higher diversity × High tax	-15.139	-11.278	-20.857
	(3.847)	(4.727)	(6.643)
	$p = 0.000$	$p = 0.017$	$p = 0.002$
Profile evaluations	2,548	1,678	870
Difference (3) − (2)	-9.579 (SE 8.153), $p = 0.240$		
MDE at 80% power, column (1)	10.8 pp		

Notes: Linear probability mixed model of Equation 5, Houston-only affected respondents, with ZIP-code ACS 2015 controls. Each tax option displayed its annual dollar cost; the three property-tax and three sales-tax levels carry the same amounts ($200, $400, $600). The difference row treats columns (2) and (3) as independent samples. MDE is the minimum detectable effect at 80% power and a two-sided 5% test.

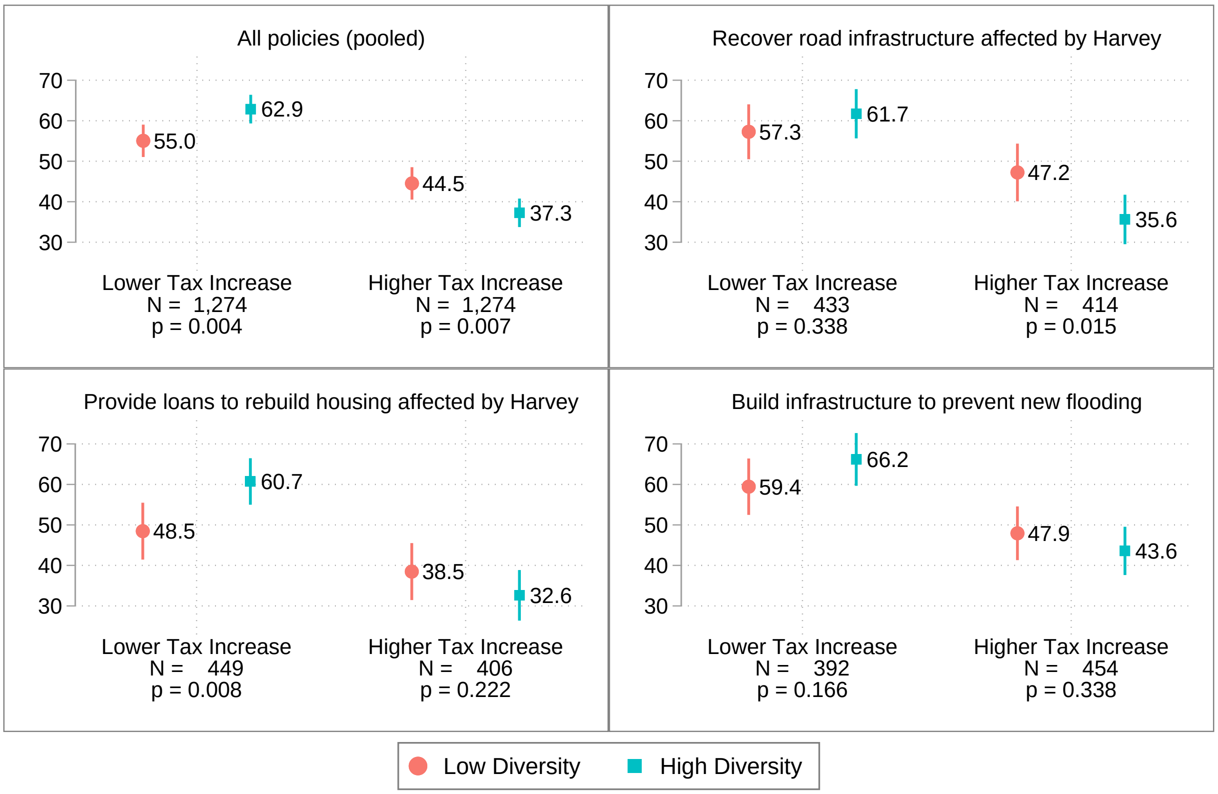
Figures A3 and A4 reproduce the policy conjoint with the full per-policy decomposition. The top-left panel of each figure (pooled across policy types) is reproduced in the main text as Figure 4 and Figure 5 respectively. The remaining three panels disaggregate the predicted margins by policy domain (road recovery, housing loans, flood prevention).

Figure A3: Predicted margins of choosing high versus low tax increases following Hurricane Harvey conditional on being affected by the storm, by policy type.



Notes: Points plot predicted margins of choosing the proposed policy option for respondents affected by Hurricane Harvey (teal squares) and not affected (salmon circles), conditional on (i) the size of the required tax increase (lower vs. higher) and (ii) how the additional revenue would be used. The top-left panel pools all policy earmarks. The top-right panel shows results for recovering damaged road infrastructure, the bottom-left panel for providing loans to rebuild housing affected by Harvey, and the bottom-right panel for building infrastructure to prevent future flooding. Labels below each tax condition report the number of choice tasks (N) and the associated p-value. Vertical bars denote 95% confidence intervals. Houston-only Wave 3 sample.

Figure A4: Predicted margins of choosing high versus low tax increases following Hurricane Harvey for affected respondents conditional on ethnic diversity, by policy type.



Notes: Points plot predicted margins of choosing the proposed policy option for respondents living in lower-diversity areas (below-mean ethnic diversity, salmon circles) and higher-diversity areas (above-mean, teal squares), conditional on (i) the size of the required tax increase (lower vs. higher) and (ii) how the additional revenue would be used. The top-left panel pools all policy earmarks, the top-right panel shows results for recovering road infrastructure damaged by Hurricane Harvey, the bottom-left panel for providing loans to rebuild housing affected by Harvey, and the bottom-right panel for building infrastructure to prevent future flooding. Labels beneath each tax condition report the number of choice tasks (N) and the associated p-value. Vertical bars denote 95% confidence intervals. Houston-only Wave 3 sample, affected respondents only.

The tax–diversity penalty by project type

Table A17 splits the policy conjoint by project type: flood-prevention infrastructure is the forward-looking category, road recovery and housing loans the backward-looking ones, and the rows report the tax–diversity penalty within each group. A time-horizon account predicts the penalty concentrates on the mitigation profiles. It is at least as large on the response profiles, the opposite of what the account predicts.

Table A17: Policy conjoint: the tax–diversity penalty by project type.

	Coef.	Std. error	z	p
Mitigation × High tax × Higher diversity (Houston, affected)	5.746	8.167	0.70	0.482
Mitigation × High tax × Higher diversity (full sample, affected)	8.271	7.533	1.10	0.272
High tax × Higher diversity, response profiles	-16.842	4.720	-3.57	0.000
High tax × Higher diversity, mitigation profiles	-11.097	6.665	-1.66	0.096

Notes: Linear probability mixed model of Equation 5 with the project attribute recoded as mitigation (flood-prevention infrastructure) versus response (road recovery, housing loans) and fully interacted with the high-tax and higher-diversity indicators; Houston-only affected respondents except where the row indicates the full sample. The two within-group rows are linear combinations of the interaction terms.

A7 Partner-choice conjoint

The partner-choice conjoint, implemented in the 2020 (Wave 3) survey, presented respondents with paired hypothetical individuals and asked which they would prefer to collaborate with in a neighborhood association formed for future flooding. Each profile randomizes six attributes: race/ethnicity (White, Black, Hispanic), gender (male, female), a photograph and first name aligned with the race–gender cue, party identification (Democrat, Republican, Independent), religion (Christian/Protestant, Roman Catholic, nonreligious, Muslim, Jewish), and civic-association membership (ASPCA, MADD, NRA, Rotary International, YMCA). Race and gender are the only attributes presented visually, via the photographs; all other attributes are shown as text. Each race–gender combination is represented by a single photograph and first name, so estimates of race and gender ingroup effects reflect responses to these specific exemplars as well as to the broader social categories.

Figure A5 displays the six profile images used in the partner-choice conjoint. Each image represents a unique combination of race and gender, which were the only attributes presented visually; all other attributes were presented as text.

Figure A5: Artificially generated profile images used in the partner-choice conjoint. The images do not depict real individuals. Race and gender are the only attributes presented via images; all other attributes are shown as text.

Respondents were presented with the following vignette:

> *Now, let's suppose that you are creating a neighborhood association to provide information and help to people in the neighborhood during future flooding. Which of these two individuals do you prefer to collaborate with?*

Figure A6: Example of paired-choice task in the partner-choice experiment. Artificially generated profile images. The images do not depict real individuals. Race and gender are the only attributes presented via images; all other attributes are shown as text.

Notes: See Online Appendix Section A7 for the full set of race–gender profiles.

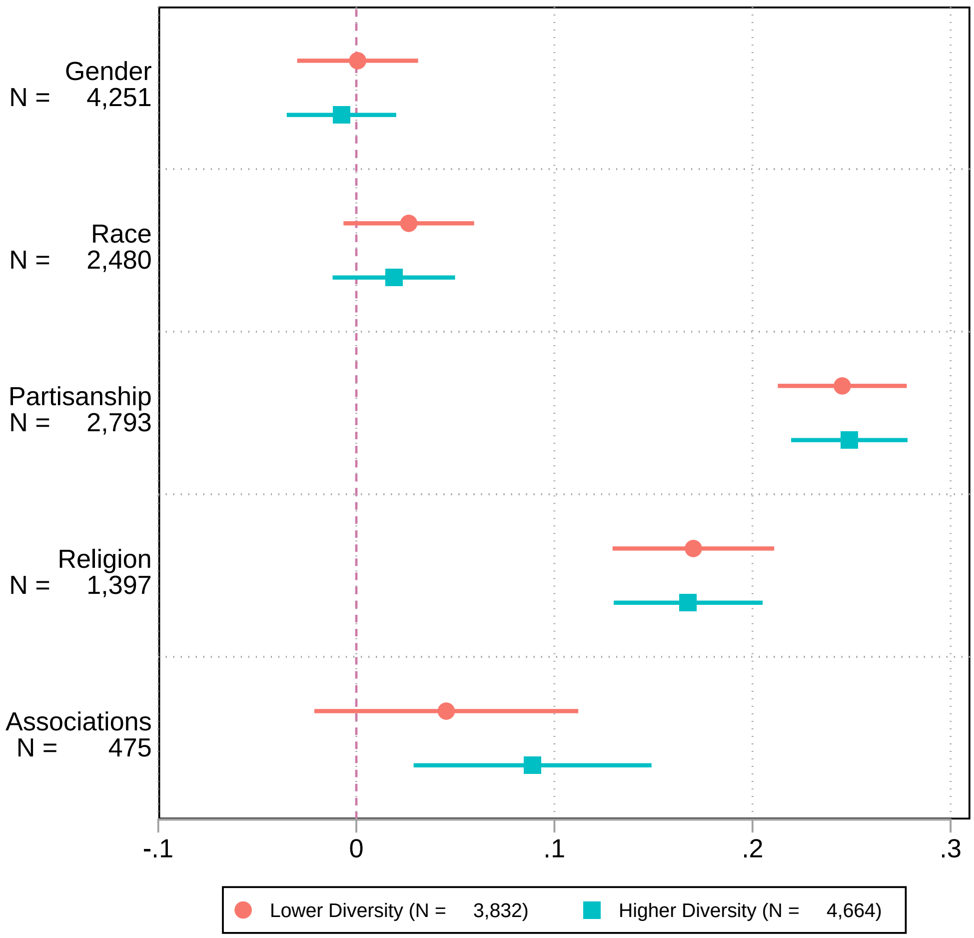
Cases in which the respondent and the profile share the same value on a given attribute are coded as "ingroup." For each respondent i, task t, and profile p, we recode five binary indicators that equal 1 when the profile matches the respondent's self-report and 0 otherwise: M_{itp}^{gender} (ingroup gender), M_{itp}^{race} (ingroup race), M_{itp}^{party} (ingroup party), M_{itp}^{relig} (ingroup religion), and M_{itp}^{assoc} (ingroup association). "Independent" and "Non-religious" are treated as valid ingroups. For respondents who selected "Other" for race, we set $M_{itp}^{\text{race}} = 0$ for all profiles; likewise, for respondents who did not report gender, we set $M_{itp}^{\text{gender}} = 0$ for all profiles. We classify each respondent's ZIP code as lower-diversity if the diversity index is below the sample mean and as higher-diversity if it is above the sample mean. Let $g_i \in \{\text{Lower Diversity}, \text{Higher Diversity}\}$ denote this status (constant across tasks for respondent i). We estimate the following equation separately for $g_i = \text{Lower Diversity}$ and $g_i = \text{Higher Diversity}$:

$$
\text{Cooperation}_{itp} = \alpha + b_1\, M^{\text{gender}}_{itp} + b_2\, M^{\text{race}}_{itp} + b_3\, M^{\text{party}}_{itp} + b_4\, M^{\text{relig}}_{itp} + b_5\, M^{\text{assoc}}_{itp} + \varepsilon_{itp} \quad \text{if diversity} = g_i. \tag{6}
$$

We compare b_1–b_5 across g_i = Lower Diversity versus g_i = Higher Diversity to assess how cooperation patterns vary with neighborhood diversity. The results show three patterns. First, respondents are more likely to cooperate with hypothetical profiles sharing the same religion (17 pp) or partisan affiliation (25 pp). These effects are stable across levels of diversity. Second, and notably, effects are indistinguishable from zero for gender and race, suggesting that ascriptive traits appear to be less important drivers of cooperation. Finally, civic associations display a different pattern: they show no detectable effect in lower-diversity areas, but become relevant drivers of cooperation in higher-diversity areas. The average marginal component effect of shared civic-association membership rises from 4.5 pp (not significant) in lower-diversity ZIP codes to 9.2 pp ($p < 0.01$) in higher-diversity ZIP codes.

Consistent with our monitoring argument, in diverse environments, ascriptive traits are not inherently informative signals for identifying reliable cooperation partners. They drive cooperation only insofar as they carry informational value about expected reciprocity and sanctioning capacity. In our setting, race and gender appear to carry little such informational value even in areas with below-mean levels of diversity. In such settings, shared membership in a civic association becomes a more informative signal. This interpretation is consistent with experimental evidence showing that ascriptive traits do not automatically boost cooperation, but matter insofar as they convey information about expected reciprocity, trust, and sanctioning capacity (Burns and Keswell, 2015). It also complements qualitative research on civic associations in disaster recovery, which emphasizes their role in information transmission and coordination in heterogeneous communities (Storr and Haeffele-Balch, 2012).

Figure A7: Probability of cooperating with ingroup profiles.



Notes: Average marginal component effects of each ingroup cue on cooperation probability. Salmon circles: respondents in lower-diversity ZIP codes (below-mean ethnic diversity). Teal squares: respondents in higher-diversity ZIP codes (above-mean ethnic diversity). Horizontal bars denote 95% confidence intervals; the vertical line at zero marks a null effect. Labels on the left report the number of profile evaluations in which each ingroup cue is present (N); the legend reports the number of profile evaluations in the lower- and higher-diversity subsamples. Full 1,065-respondent Wave 3 sample. Diversity is measured at the ZIP-code level (see Online Appendix Section A7). See Online Appendix Table A18 for the full coefficient table.

Table A18 reports the coefficients underlying Figure A7, estimated separately for respondents in ZIP codes with below-mean (Lower diversity) and above-mean (Higher diversity) ethnic fractionalization. Table A19 pools the two subsamples and interacts each ingroup cue with the higher-diversity indicator. The interaction on the association cue is 0.044 (SE 0.046, $p = 0.34$), and a joint test across the five cues gives $p = 0.95$; the split-sample contrast therefore does not establish that the association cue differs across diversity levels (Gelman and Stern, 2006). Respondents were asked whether they belong to each of the five organizations shown in the profiles (ASPCA, MADD, NRA, Rotary International, and YMCA); membership rates are 12.4 percent for the YMCA, 8.6 for the ASPCA, 4.9 for the NRA, and 4.3 each for MADD and Rotary, 26.9 percent of respondents belong to at least one, and a respondent and a profile share an organization in 5.6 percent of evaluations. Re-estimating the association effect excluding one organization at a time, dropping the NRA reduces the higher-diversity coefficient from 0.089 to 0.039; dropping any other organization leaves it between 0.075 and 0.137.

Table A18: Partner-choice conjoint: ingroup cooperation by neighborhood diversity.

	Lower Diversity		Higher Diversity	
	(1)	(2)	(3)	(4)
	Orig	+ ZCTA	Orig	+ ZCTA
Ingroup gender	0.001	0.001	-0.007	-0.007
	(0.016)	(0.016)	(0.014)	(0.014)
Ingroup race	0.026	0.027	0.018	0.019
	(0.017)	(0.017)	(0.016)	(0.016)
Ingroup party	0.245***	0.246***	0.247***	0.247***
	(0.017)	(0.017)	(0.015)	(0.015)
Ingroup religion	0.170***	0.170***	0.167***	0.167***
	(0.021)	(0.021)	(0.019)	(0.019)
Ingroup association	0.045	0.045	0.092***	0.092***
	(0.034)	(0.034)	(0.031)	(0.031)
ZCTA median HH income ($10k)		-0.002		0.002
		(0.004)		(0.005)
ZCTA % renter		0.027		-0.003
		(0.072)		(0.054)
ZCTA % foreign-born		-0.010		0.008
		(0.100)		(0.078)
ZCTA % poverty		-0.094		0.087
		(0.171)		(0.186)
Observations	3832	3832	4632	4632
Log-likelihood	-2639.661	-2639.445	-3188.450	-3188.322

Notes: OLS estimates. Each row reports the average marginal component effect of a given ingroup cue on the probability of choosing that profile. Estimated separately for respondents in lower-diversity (below-mean ethnic diversity) and higher-diversity (above-mean ethnic diversity) ZIP codes. Full 1,065-respondent sample. ZCTA controls do not move estimates (not shown). Standard errors clustered at the respondent level.

Table A19: Partner-choice conjoint: pooled model with diversity interactions.

	(1)
	Pooled with Diversity interactions
Ingroup gender	0.001
	(0.016)
Ingroup race	0.026
	(0.017)
Ingroup party	0.245***
	(0.017)
Ingroup religion	0.170***
	(0.021)
Ingroup association	0.045
	(0.034)
High Diversity	0.004
	(0.019)
$\times$ Ingroup gender	-0.008
	(0.021)
$\times$ Ingroup race	-0.008
	(0.023)
$\times$ Ingroup party	0.004
	(0.022)
$\times$ Ingroup religion	-0.003
	(0.028)
$\times$ Ingroup association	0.044
	(0.046)
Observations	8496
Log-likelihood	-5848.180

Notes: Linear probability mixed model with random intercepts at the task and respondent levels, full Wave 3 sample. Each ingroup cue is interacted with an indicator for above-mean ZIP-code ethnic diversity. Standard errors in parentheses.