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The Local Economic Impact of Natural Disasters¹

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Abstract

We use nearly four decades of U.S. county data to study dynamic local economic impacts of natural disasters that trigger federal aid. We find these disasters on average raise personal income per capita in the longer run (8 years out). We also find that, in the longer run, wages and home prices are higher, while employment and population are unaffected, suggesting the income boost may reflect productivity increases and greater demand for housing in supply-constrained areas or compositional shifts. Allowing for heterogeneity across disaster types, we find the longer-run income boost is driven primarily by hurricanes and tornadoes. We also find the longer-run boost increases with damages, suggestive of an important role for insurance and government aid—which are highly correlated with damages—in fueling recovery. A spatial spillover analysis suggests the longer-run net effects of local aid-inducing disasters for wider regions are near-zero.

JEL Codes: R11, R15, Q54, Q58, H84

Keywords: natural disasters, local economic impacts of disasters, panel local projections

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I. Introduction

Natural disasters in the United States have become more frequent and costly in recent decades (see **Figure 1(a)**). While increased development and population growth in disaster-prone areas have played a role (Rappaport and Sachs 2003), climate change is often cited as an important driver (USGCRP 2017, 2023); and consensus climate change projections suggest these trends are likely to continue in the decades ahead.¹ Facing rising risks, households and communities may retreat from areas that have suffered extensive damages from disasters. However, aid and insurance payouts—and the expectation of such recompense in the future—can spur investment in disaster-hit areas even beyond what is required to restore them, raising moral hazard concerns.

Thus, it has become increasingly important for policymakers to understand the economic impacts of natural disasters, particularly those that trigger government aid. Local policymakers, whose programs depend on local tax revenues, need to anticipate how disasters will affect local economic outcomes in both the immediate aftermath of disasters and over the longer run. National policymakers, who oversee funding of federal aid programs, need to understand the impacts of aid-inducing disasters to make effective investments in resilience and support better budgeting (CEA 2022). Furthermore, the economic impacts of severe weather events tied to climate change are an important input into estimates of the social cost of carbon (Carleton and Greenstone (2021)).

Despite the topic's importance to policymakers and an abundance of studies, there is little consensus on what the dynamic impacts of natural disasters are for local economic outcomes. Hsiang and Jina (2014) provide a useful framework for thinking about the potential impacts. They present a schematic, which we reproduce in **Figure 2**, that depicts four potential paths illustrating that long-run economic activity may end up below, at, or even above pre-disaster trends. Any one of these paths can be rationalized by economic theory (Botzen, Deschenes,

¹ For instance, the recent Climate Science Special Report (Fourth National Climate Assessment: Volume 1) from the Congressionally-mandated U.S. Global Change Research Program (USGCRP 2017) concludes that “*the frequency and intensity of extreme high temperature events are virtually certain to increase in the future as global temperature increases (high confidence). Extreme precipitation events will very likely continue to increase in frequency and intensity throughout most of the world (high confidence).*” The report goes on to note that these trends will result in increased frequency and severity of disaster types such as droughts, fires, and floods that are associated with high temperatures and swings in precipitation.

and Sanders (2019)). Moreover, the empirical literature estimating these impacts has yielded wide-ranging results that include support for each path.² Hence, questions about the economic impacts of natural disasters are far from settled.

In this paper, we aim to advance the empirical literature on disaster effects by filling three key gaps that have limited its potential usefulness for policymakers. First, many prior papers focus on a single type of disaster (often hurricanes) in isolation.³ Yet whether hurricanes—or any single disaster type—are representative of disasters more broadly remains an open question. In addition, omitting information on other disaster strikes within the event window of interest can lead to biased results. Second, macroeconomic analyses covering multiple disaster types have primarily relied on cross-country data. Because availability of aid varies substantially across countries, their findings may not apply to local outcomes within a country, especially in the U.S. which has had a robust federal aid system and insurance market.⁴ Third, examinations of impacts of disasters on economic growth in the U.S. have often been performed as extensions in papers focused on other questions (e.g., Deryugina (2017) and Jerch et al. (2023)). As a result, the mechanisms, heterogeneities, and spatial dynamics underlying average responses have been largely unexplored, despite the fact that understanding them could be important to determining appropriate aid responses or whether average responses apply in particular situations.

We address these concerns using nearly four decades (1980-2017) of data on U.S. counties to flexibly estimate the short- and longer-run effects of natural disasters on local income per capita (p.c.) as well as key underlying components. We measure natural disasters using official disaster

² While Hsiang and Jina (2014) find that cyclones generally cause long-lasting declines in national GDP p.c. for a cross-country panel, other cross-country studies find less negative, or even positive, effects of disasters on national income for high-income countries (e.g., von Peter et al. (2012), Cavallo et al. (2013), Felbermayr and Gröschl's (2014), Lackner (2019), and Sawada and Sachs (2019)). Even among studies focusing on U.S. hurricanes, the empirical findings are mixed. For example, Deryugina (2017) and Jerch et al. (2023) find that hurricanes have generally negative longer-run economic effects at the county level. (We provide a detailed comparison of our results to those of Deryugina (2017) and Jerch et al. (2023) in **Appendix D**, which is available online.) By contrast, Deryugina et al. (2018) and Groen et al. (2020), both of which follow individual workers, find that Hurricanes Katrina (and Rita) resulted in substantial longer-term gains in earnings, labor income, and wages.

³ Belasen and Polacheck (2008) and Strobl (2011) examine the short-run impacts of hurricanes on labor market outcomes and income per capita, respectively, in coastal U.S. counties. Prominent examples of detailed case studies of specific disasters include Vigdor (2008), Hornbeck (2012), Gallagher and Hartley (2017), Kirchberger (2017), Deryugina et al. (2018), and Groen et al. (2020).

⁴ Even within the U.S. the aid system has changed over time. An important difference between Boustan et al. (2020)—who use county-by-decade panel data from 1940 to 2010 to estimate the contemporaneous effects of severe disasters, measured using FEMA declarations—and our paper is that most of their sample period predates the 1988 Stafford Act, which formalized and expanded the U.S. federal disaster aid system. They find more negative outcomes than we do, including higher net out-migration, higher poverty rates, and lower home prices.

declarations from the Federal Emergency Management Agency (FEMA).⁵ Thus, by construction, we are focusing on disasters that trigger federal disaster aid. However, we also report results for hurricanes where we base disaster treatment on wind speeds, which may or may not trigger federal disaster aid. We use a version of the panel local projections (LP) methodology that has been shown by Dube et al. (2023) to be an alternative to the standard event study approach that addresses concerns related to repeated treatments.

This paper's main contribution to the literature is to estimate the dynamic response of local economies to disasters by applying a unified empirical framework across a wide range of outcomes and disaster types. This approach yields several novel insights. First, on average, personal income p.c. increases over the longer run in U.S. counties after they receive FEMA disaster declarations.⁶ Second, we show the responses of key underlying components are consistent with predictions of several prominent models on the impacts of local shocks. Third, impacts vary importantly across disaster types and by severity. Fourth, we examine whether positive or negative spatial spillovers could cause the effects of a local disaster on a wider region to differ from those on the local area. We find that, while a local aid-inducing disaster boosts longer-run income per capita in that local area, it has a near-zero effect for the wider region.

Returning to the hypotheses depicted in **Figure 2**, our evidence on the dynamic impact of the average disaster on income p.c. is closest to the “build back better” scenario. Specifically, our results point to an initial decline, albeit economically and statistically insignificant, followed by a recovery to a level of income p.c. that is 0.5% *above* the baseline trend eight years after the disaster. While this finding could reflect compositional shifts, it is also consistent with the hypothesis that post-disaster aid and insurance payouts can generate improvements to local capital and consequent increases in income p.c. Both the slightly negative short-run impact and the positive longer-run impact can be explained by the fact that, while some aid is dispersed quickly after disasters, some large components of aid (for example, Community Development Block Grants) can take years to reach affected areas.

⁵ Hereafter, unless otherwise indicated, we generally use the term “disaster” in the context of our study to mean a FEMA-declared disaster.

⁶ We focus on personal income because it has long historical data available at the county level and is very highly correlated with GDP.

The result of a longer-run increase in income p.c. is robust to alternative specifications, sample restrictions, and disaster measurements. For instance, in assessing whether results are sensitive to using FEMA's classification of disasters as opposed to meteorological and geophysical definitions, we obtain similar results on the average income effects of hurricanes using either FEMA hurricane declarations, a wind speed-based hurricane indicator, or using wind speed to instrument for FEMA hurricane declarations.

We also examine the key components driving this increase in income p.c. and what they reveal about the underlying mechanisms. We find that employment losses likely reduce income in the month of the disaster and then boost income for about a year thereafter, with this positive effect driven at least in part by a jump in construction employment. The longer run increase in personal income, on the other hand, can be largely traced to higher average weekly wages.

We then examine theories in the literature that might explain these key findings and offer testable predictions. For example, the increase in wages could reflect (a) more productive capital in a setting with inelastic local labor supply or (b) a compositional shift toward higher wage workers. We find evidence in support of both explanations, which are not mutually exclusive. Indeed, to the extent that higher wage workers have better access to aid and insurance funds after experiencing damages and are better able to afford to remain in an area after it has been rebuilt, a recovery process that boosts local productivity could simultaneously drive compositional changes. By examining related theory and performing analyses involving additional outcomes (like construction employment, home prices, and population) and different splices of the data (like elastic vs inelastic housing supply, and growing vs declining home price counties), we find support for the notion that FEMA-declared disasters on average lead to increases in local capital, resulting in higher productivity. An analysis of wages implied by changes in the local industry employment mix indicates that a compositional shift in the local workforce toward higher income individuals likely explains some, but not all, of the longer-run increase in average wages.

Next, we uncover considerable heterogeneity in the average post-disaster response of local income. First, although hurricanes and tornadoes result in longer-run increases in income p.c., the effects of floods, severe storms, extreme winter weather, and fires are small, zero, or statistically insignificant. This could be explained by differences across disaster types in extent of damages, insurance coverage, or expectations of future recurrence. Regardless of the source of heterogeneity, these results indicate that economic responses to one disaster type may not be

externally valid for other disaster types. Second, we find that the rise in income p.c. actually *increases* with disaster severity, as measured in terms of p.c. monetary damages. This could reflect that more destructive events are followed by more substantial aid and insurance payouts, leading to more extensive rebuilding rather than piecemeal repairs. This interpretation is supported by an analysis we perform that shows that the longer-run income p.c. effect increases with damages but not with wind speed. Lastly, we investigate two issues related to adaptation: whether the average disaster effects vary with local disaster experience, focusing here on hurricanes and floods, or over time. We find no significant evidence of either.

Concerned that boosts to income in areas hit by disasters may be coming at the expense of other areas, in which case positive local effects would be offset by negative regional effects, we examine spatial spillovers to see how disaster effects propagate to other counties of varying distances away. Adding spatial lags of disasters to our baseline specification, and aggregating the own-county effects with the spatial lag effects, we find the longer-run income effect for a hit county's *region*—defined by all counties within 600 miles of it—is close to zero and statistically insignificant (though with a relatively large confidence interval).

Our findings have several important policy implications. First, for local policymakers, the finding that employment, and perhaps personal income (for which we don't have high-frequency data), temporarily drops immediately after a disaster suggests they may need to plan ahead—for example, with larger rainy-day funds—to better manage temporary post-disaster spending needs and tax base declines. Second, the heterogeneity in outcomes suggests that one must exercise caution in extrapolating from results based on specific events, contexts, or time frames. Finally, if local areas really do “build back better” after disasters, an important question for national policymakers is whether federal disaster aid has been “overshooting.” This is of particular concern for several reasons. First, allocating resources to rebuilding disaster-prone areas may encourage additional economic development in those areas. Second, our spatial lag findings suggest the rebuilding may come at the expense of other nearby areas. Third, the rise in income may reflect rising inequality. And, fourth, an overshooting of aid could be particularly costly if the frequency and severity

of disasters continue to increase. Because we do not perform a welfare analysis in this paper, our analysis does not speak to the efficiency of the income responses.

The remainder of the paper is organized as follows. In the next section, we describe the data we use for disasters and to measure economic activity. We follow this with a discussion of our methodology in Section III. In Section IV, we present our baseline results for personal income p.c. and the key components underlying this variable. We then discuss the economic channels that could explain our baseline findings and test related theoretical predictions in Section V. Section VI examines heterogeneity, adaptation, and spatial spillover effects. Section VII concludes.

II. Data

We use data on disasters and a variety of economic outcomes, which we describe below. **Table 1** summarizes the sources, frequency, and treatment of the dependent variables. Unless otherwise noted, we use data from 1980-2017, when most of the economic data are available. Summary statistics are provided in **Table A1** (apps. A-D are available online). All nominal monetary variables are deflated by the BLS Current Price Index – All Urban Areas (CPI-U) in constant 2017 dollars.

A. Natural Disasters

Our primary data source for disasters is FEMA’s real-time administrative Disaster Declarations Summary dataset. We focus on natural disasters that received a “Major Disaster” Presidential declaration according to the FEMA data.⁷ A FEMA disaster declaration is generally initiated when a state government issues a request to FEMA. FEMA sends a team to the disaster area to perform a Preliminary Damage Assessment, using drone, satellite, and civil air imagery as well as site visits to determine, for each affected county, whether the damage is extensive enough to warrant a major disaster designation and, if so, for what types of federal government assistance the county is eligible.^{8,9} Some types of assistance go to individuals and households

⁷ Given our focus on *natural* disasters, we exclude declarations due to terrorism or toxic substances.

⁸ Source: author conversations with FEMA staff.

⁹ As detailed in Lindsay and Reese (2018) from the Congressional Research Service, “[e]ach presidential major disaster declaration includes a ‘designation’ listing the counties eligible for assistance as well as the types of assistance FEMA is to provide under the declaration.”

such as FEMA Individual Assistance (IA) aid and Small Business Administration (SBA) loans. Other types go to state and local governments such as FEMA Public Assistance (PA) grants for infrastructure repair, FEMA Hazard Mitigation Grant Program (HMGP) funds to lessen the effects of future disaster incidents, and Community Development Block Grant Disaster Recovery (CDBG-DR) funds from the U.S. Department of Housing and Urban Development that are meant for long-term economic recovery and development.

The resulting sample of natural disasters covers nearly the entire country. **Figure 1(b)** shows for each county the number of years from 1980 to 2017 in which it had at least one disaster. Very few counties had no disasters and the modal county experienced disasters in eight of the 38 years.

We classified FEMA disaster declarations into major type categories using FEMA’s classifications augmented in some cases with FEMA’s description of the event.^{10,11}

In additional analyses, we use wind speed data from the U.S. National Hurricane Center’s Best Track Atlantic hurricane database (HURDAT2) to examine robustness of hurricane responses to using alternative treatment measures. These data are described in **Appendix B.4**.

B. Economic Data

Our primary outcome of interest is personal income p.c. We focus on personal income because it has long historical data available at the county level and is very highly correlated with GDP.¹² We use county level data on personal income and its components from the

¹⁰ We noticed a small number of instances in which FEMA classified a disaster as being a “severe storm” while their description (title) of the event noted that the underlying disaster was also a hurricane, flood, or both. For these compound events, we chose to reclassify them as hurricanes if “hurricane” was in the description—because the hurricane could be considered the primary cause of the severe storm—and otherwise as a floods—because the flooding seemed to be a key contributing factor to why FEMA declared the event to be a disaster.

¹¹ The geographic exposure to disasters varies significantly by disaster type (see **Figure A1**).

¹² There are three main conceptual differences between GDP and personal income at the county level as defined by the U.S. Bureau of Economic Analysis: (1) personal income includes government transfers, while GDP does not; (2) GDP includes corporate income while personal income does not (though it does include corporate income distributed to shareholders via dividends and interest); and (3) GDP is based on place of work, while personal income is based on place of residence. One implication is that our results on personal income generally will not reflect any post-disaster losses (or gains) to corporate profits. For example, Kruttli et al (2023) have found that firms affected by hurricanes experience significant uncertainty, with affected firms showing significant outperformance and underperformance in returns after landfall.

Regional Economic Information System (REIS) of the Bureau of Economic Analysis (see BEA 2017).¹³

Our data on employment and average weekly wages (AWW) by county come from the Quarterly Census of Employment and Wages (QCEW). The QCEW is compiled by the Bureau of Labor Statistics (BLS) based on state Unemployment Insurance (UI) administrative records. Nearly all private nonfarm employers report to their state UI agencies the number of employees they have on payroll each month and their total wages each quarter: “Monthly employment is based on the number of workers who worked during or received pay for the pay period including the 12th of the month.... Average weekly wage values are calculated by dividing quarterly total wages by the average of the three monthly employment levels (all employees, as described above) and dividing the result by 13, for the 13 weeks in the quarter.”¹⁴ AWW reflect both hourly wages and the number of hours worked per week. We separately examine employment effects for total private nonfarm employment and private construction employment (NAICS code 1012).¹⁵ The data on construction employment start in January 1990.

We use the CoreLogic Home Price Index (HPI), available by county from 1980Q1 to 2017Q1, to measure county level home prices. The index is based on transaction prices of repeated home sales, and as such is only available for counties with sufficient sales activity. Repeated-sales price indices have the advantage of reflecting price changes of individual homes holding fixed the permanent characteristics of the homes and are therefore independent of changes in the composition of homes in an area. However, a natural disaster can seriously affect the characteristics of a given home. For instance, unrepaired damage will negatively impact a home’s value, while improvements made through repairs and rebuilding may increase its quality. This potential for changing home characteristics should be kept in mind when interpreting our home price results. In addition, we use the housing supply elasticity data from Saiz (2010), which is based on topographical characteristics of metro areas.

Lastly, we use Census Bureau county population data, which reflect county populations as of July 1 of each year.

¹³ In appendix materials, we also examine wage and salary income, government transfers (and loans), and poverty rates. We describe these data in **Appendix B**.

¹⁴ <https://www.bls.gov/cew/news-release-technical-note.htm>

¹⁵ Due to concerns about data quality, when estimating IRFs for construction employment, we drop counties with more than 5 months of missing or zero construction employment.

III. Methodology

To see how local economies are affected by natural disasters, we estimate Impulse Response Functions (IRFs) of economic outcomes with respect to disaster shocks. We use a panel data version of the Jordà (2005) LP estimator for our baseline specification. We then build on that specification to explore heterogeneity in disaster effects. Dube et al. (2023) show formally that the panel LP method (which they call “LP-DiD”) generalizes the standard two-way fixed effect event study design but is more robust to time-varying and heterogeneous treatment effects, the focus of many recent difference-in-difference studies.

In our baseline specification, we estimate the following equation for a series of horizons $h \geq 0$:

$$y_{c,t+h} - y_{c,t-1} = \beta^h D_{c,t} + \mathbf{X}'_{ct} \boldsymbol{\gamma}^h + \alpha_{r(c),t}^h + \alpha_{c,m(t)}^h + \epsilon_{c,t+h}. \quad (1)$$

$y_{c,t}$ is an economic outcome of interest in county c in period t , measured in logs. Our dependent variable is thus the change in the log outcome (approximately equal to the percentage change in the outcome) from the period before the disaster to h periods after the disaster. **Table 1** outlines how the outcome variables are measured in our analyses. $D_{c,t}$ is the key treatment variable, equaling one if the county experienced a disaster in period t and zero otherwise. The series of β^h are the IRF coefficients of interest. \mathbf{X}_{ct} is a vector of control variables with parameters $\boldsymbol{\gamma}^h$. Specifically, $\mathbf{X}'_{ct} \boldsymbol{\gamma}^h$ is defined as

$$\mathbf{X}'_{ct} \boldsymbol{\gamma}^h \equiv \sum_{\substack{\tau=-p, \\ \tau \neq 0}}^h \delta^{\tau h} D_{c,t+\tau} + \sum_{\tau=-p}^{-1} \rho^{\tau h} y_{c,t+\tau}. \quad (2)$$

The first term in equation (2) controls for other disasters that may have hit the same county either before the current disaster (up to p periods prior) or between the current disaster and horizon h .^{16,17} This ensures that the estimated IRF from a disaster is not contaminated by either lingering effects of past disasters or effects of other disasters

¹⁶ Though not shown in equation (2) for tractability, when $h < 0$ we still include the p lags indicating whether disasters occurred before period 0.

¹⁷ For monthly outcomes, due to computational demands, we control for 12-month aggregate indicators for the leads and lags of disasters.

(“compound events”) that happen to occur between the current disaster and horizon h .¹⁸ The second term on the right-hand side of equation (2) explicitly accounts for pre-trends in the outcome variable. We set p , the number of lags of the treatment and outcome variables, equal to the same value.¹⁹ In **Appendix C.1** we examine how sensitive our results are to alternative approaches to controlling for the counterfactual no-disaster trend.

We include region-specific time fixed effects $\alpha_{r(c),t}^h$ to absorb any regional or national shocks that may have coincided with disasters. To control for county-level heterogeneity and seasonality, we also include county-by-calendar month (quarter for quarterly frequency data) fixed effects $\alpha_{c,m(t+h)}$. For annual outcomes, this amounts to a simple county fixed effect. We report standard errors that allow for two-way clustering at the county and state-by-time levels. Clustering by county allows for arbitrary serial correlation. State-by-time clustering allows for cross-sectional/spatial correlation of errors within each state such as those due to state government policies.²⁰ Also, because measurement errors in our outcomes are likely inversely related to county size, we weight observations by log employment as of the beginning of the sample period, January 1980.

The key identifying assumption in equation (1) is that the disaster indicator is orthogonal to the error term, $\epsilon_{c,t+h}$. In other words, we assume that FEMA disaster declarations are exogenous with respect to any factors that affect the trajectories of our outcomes that are unobserved or omitted from equation (1).

In Section VII, we extend this baseline specification in several directions, exploring heterogeneous disaster effects, adaptation, and spatial spillovers.

IV. Baseline Results

We now present our baseline IRF estimates, which come from estimating β^h in equation (1) above. The results are shown in **Figure 3**. The shaded areas around the coefficient estimates

¹⁸ In practice, in our sample the inclusion/exclusion of these intervening disaster dummies has virtually no effect on our results. Furthermore, estimates of the impacts of contemporaneous compound disasters (when more than one disaster strikes a county in the same year) on personal income are qualitatively similar to the average results (though noisier, as they are rare events.)

¹⁹ **Figure A2** shows that the disaster treatment variable has low autocorrelation as is and essentially zero autocorrelation conditional on the other variables included in our baseline regression.

²⁰ As a robustness check, in **Appendix C** we also report results allowing for distance-based spatial autocorrelation of errors following Conley (1999).

represent 90 and 95 percent confidence intervals. Recall that these IRFs should be interpreted—in line with an average treatment effect interpretation—as estimates of the average cumulative difference between the actual outcome for a county hit by a disaster and the counterfactual outcome for that county had it not been hit by a disaster. In other words, a point estimate on the horizontal zero line in the IRF graphs does *not* mean that the level of the outcome variable is equal to its pre-disaster ($t - 1$) level, but rather that it is equal to what it would have been in a no-disaster counterfactual. This no-disaster counterfactual reflects region-specific time fixed effects and (season-specific) county fixed effects as well as the pre-trend and intervening disaster controls in equation (2).

A. *Personal Income Per Capita*

Figure 3 panel (a) shows the estimated IRF for personal income p.c. The estimated contemporaneous response is negative but economically and statistically insignificant. As of one year after a disaster, income p.c. is about 0.2% higher than it would have been otherwise (not quite significant at the 5% level). Income p.c. remains about that much higher for the next several years and then increases more around 6 years out. As of 8 years out, income p.c. is estimated to be approximately 0.5% above where it otherwise would have been (significant at the 5% level). Recalling the hypothetical scenarios in **Figure 2**, this baseline result on income p.c. is closest to the “build back better” scenario.

Although these results establish that the longer run *average* income increases in counties directly affected by natural disasters, they do not tell us whether income increases across the income distribution. In **Figure A3**, we find no evidence of changes in the poverty rate.²¹ If the personal income increase were experienced equally across the income distribution, we would expect the poverty rate to decline as some see their income rise above the poverty threshold. The absence of a decline in the poverty rate suggests that despite an increase in average personal income p.c., local inequality may rise after natural disasters.

B. *Robustness*

Before investigating impacts of disasters on other outcomes, we highlight that the key qualitative finding of a positive and significant medium- to longer-run effect of FEMA-declared

²¹ The poverty rate is measured in percentage points. Thus, the estimated 95% confidence intervals rule out impacts on the poverty rate of plus or minus 0.14 percentage points at any horizon.

disasters on local income per capita is robust to a range of alternative specifications, sample restrictions, and disaster treatment definitions.

One particular *a priori* concern is that natural disasters measured by FEMA declarations may differ systematically from disasters measured by meteorological and/or geophysical data. We first address this concern by estimating our baseline specification only for hurricane disasters. We compare results based on measuring hurricane treatment using FEMA declarations (while controlling for occurrences of non-hurricane FEMA declarations) versus maximum sustainable wind speed (irrespective of whether FEMA declares the event a disaster).²² The results are shown in panels (a) and (b) of **Figure 4** and are remarkably similar. Panel (c) reports the results from estimating the same specification as in panel (a) but using cubic wind speed to instrument for FEMA declarations. Again we find an economically and statistically significant longer-run effect on income per capita. Thus, at least for hurricanes, our baseline results do not appear to be driven by potential endogeneity of FEMA declarations.

We present a variety of other sensitivity analyses for our baseline results in **Appendix C**. First, we find similar results excluding FEMA disasters with zero observed monetary damages, controlling for whether a county's state governor is of the same party as the president, or whether the disaster occurs in a presidential election year or not. These results, along with the wind speed findings, suggest that our results are not driven by endogeneity of FEMA declarations. Second, we find similar results for alternative specifications, namely, whether we drop lags of income per capita growth, add a county-specific linear time trend, or control for cumulative income per capita growth over the prior three years. Lastly, with regard to sample restrictions, the baseline results are robust to using a common sample for all horizons in the local projections regressions, extending the sample back to 1969 (which is only possible for income per capita), and excluding counties that are hit by disasters either rarely or very frequently.

C. Key Components Underlying Income Per Capita

We next estimate the disaster IRFs for additional outcomes to determine what the key components driving the longer-run positive response of income p.c. to disasters are. In panel (b)

²² The wind speed data are described in **Appendix B.4**. For this exercise, we follow Jerch et al. (2023) in designating a county as treated if it has wind speeds of at least 50 knots. We control for counties that received FEMA disaster declarations (for hurricanes or other disaster types) but did not have wind speeds of at least 50 knots.

of **Figure 3**, we show the estimated IRF for total nonfarm employment, which is estimated at a monthly frequency. Consistent with an initial disruption in activity, employment falls sharply, by about 0.10%, in the month of the disaster. Average monthly employment growth in our sample is approximately 0.09%, so this initial impact amounts to wiping out all of that month's employment growth. The initial decline carries over into the next month, but then rises significantly over subsequent months for an extended recovery period, peaking around one year out. After this recovery period, employment gradually returns to the no-disaster counterfactual.

To get a better sense of the extent to which the overall employment response is driven by recovery and rebuilding efforts, we look at the response of construction employment in Panel (c). As with total employment, there is a sharp decline in the month of the disaster, followed by a recovery period with local construction employment peaking about a year out, at roughly 0.9% above the no-disaster counterfactual. This is about six times larger than the percentage response of total employment at that horizon. The IRF of construction employment beyond one year flattens out somewhat but then, unlike total employment, steadily rises over the medium to longer run. As of eight years out, construction employment is estimated to be nearly 3% higher than it would have been in absence of the disaster. This suggests that the process of repairing and rebuilding public and private structures typically lasts quite long.

Panel (d) shows the quarterly IRF for average weekly wages (AWW) of local workers. AWW rise steadily after a disaster; by the end of the 8-year horizon, we estimate that AWW are nearly 0.6% higher than they would have been in the absence of the disaster. Because AWW reflect the product of weekly hours and the hourly wage, this rise could be driven by an increase in hours worked per week, the hourly wage, or a combination of the two.

Panel (e) displays the estimated IRF for quarterly home prices, which is essentially flat in the near term before increasing a good amount over the longer term. As of eight years after a disaster, the local home price index is estimated to be about 1.3% higher than it would have been otherwise. The lack of an increase in home prices in the first four to five years after the disaster suggests a limited role for damage-induced reductions in supply driving up prices. Rather, the longer-run positive price effect appears to be in line with a steady or increasing demand for housing, which would in turn be consistent with the positive wage and income

responses we observe. In addition, the rise in home prices could directly boost incomes via higher rents for local property owners. The longer-run increase in housing demand could push up home prices through either increased land valuations or improvements in the quality of the pre-existing housing stock. Improvements may be made in the process of rebuilding or repairing damaged homes or in the composition of homes resold for the CoreLogic repeat sales index.

We show results for population in panel (f). We find that, on average, the response of population to a disaster is small and generally statistically insignificant up to at least eight years out. Thus, we find that the *average* response is neither one of significant net out-migration (in response to the adverse consequences of disasters) nor one of significant net in-migration (in response to recovery efforts). However, this average masks modest longer-run declines in both in-migration and out-migration that roughly cancel each other out.²³ Thus the near-zero population response is not due to a lack of migration responses.

Another factor that could explain our finding of higher personal income p.c. is government transfers, a component of personal income. In **Figure A4**, we examine the impact of disasters on government transfers, including disaster aid and loans.²⁴ Although federal disaster aid and insurance payouts increase substantially in response to a natural disasters, we find no statistically significant longer-run effect on total government transfers.²⁵ The lack of a positive longer-run effect on total government transfers despite the increase in disaster aid appears to be driven by lower transfers for “income maintenance,” as unemployment insurance transfers are essentially unchanged over the longer run. The lack of any longer run increase in these safety-net transfers is consistent with our findings for key components in **Figure 3**. In particular, one might expect fewer local households to qualify for safety-net programs over the longer run given that total employment is unchanged while wages are higher.

²³ **Figure A5** shows results from estimating the IRFs separately for in- and out-migration, where each outcome is measured as the number of migrants divided by pre-disaster ($t - 1$) population. (See **Appendix B.5** for details.)

²⁴ Because we observe the timing of the events that trigger aid spending but not when the funds are distributed, the aid panels (a-c) should not be interpreted as indicating that the aid is distributed immediately after a disaster.

²⁵ By contrast, Deryugina (2017) finds a positive impact of hurricanes on government transfers 5 to 10 years later (see **Appendix D** for details.)

V. Economic Channels

Our baseline results point to a longer-run increase in local personal income p.c. after a FEMA-declared natural disaster. We now explore what theories can explain our findings, running tests of some specific theoretical predictions.

Disasters are often modeled as negative shocks to public and private capital stocks (e.g., Bilal and Rossi-Hansberg (2023)) and to household wealth. Indeed, our empirical analysis excludes any events for which FEMA has determined there to be insufficient damages to warrant a major disaster declaration. In a standard Solow growth model, a one-time capital depreciation shock leads to higher investment and output growth as the economy transitions back to steady state. Similarly, in a standard Neoclassical model of labor supply, a negative wealth shock raises the marginal utility of consumption, inducing households to increase labor supply, driving up employment and income all else equal. Thus, these models suggest disaster shocks could increase employment, income, and related measures of local economic activity in the short- to medium-run. This is consistent with the boost to personal income that we observe, which appears to stem from a longer-run increase in wages.²⁶ This increase in income is also consistent with a long-lasting process of recovery and rebuilding—as reflected by the longer-run increase in construction employment—along with, potentially, productivity gains from improved local public and private capital stock. The hypothesis that the local capital stock is substantially improved is supported by our finding of higher home prices over the longer run.²⁷

Our results are also consistent with spatial equilibrium models to the extent that disasters result eventually in improvements to local capital, which in turn improve local productivity and amenities. The classic local labor markets model of Rosen (1979) and Roback (1982) (and its more recent articulations such as Hsieh and Moretti (2019)) points to the central roles of productivity and amenities, and their effects on population movements, in determining the longer-run impact of local shocks on the spatial equilibrium of economic activity. Shocks that increase local amenities increase local housing demand, which in turn puts upward

²⁶ We also estimate a positive longer-run effect for the wage and salary component of personal income, though the estimate is noisy and statistically insignificant (see **Figure A6**).

²⁷ If the increase in the home price index reflects an increase in the *quality-adjusted* cost of housing services, then it implies an increase in local consumer prices (cost of living), which could partially offset the benefits of higher income for local residents.

pressure on population and home prices. The extent to which shocks to local housing demand manifest in higher population versus higher home prices depends on the elasticity of housing supply. Shocks that increase local productivity increase local labor demand, which in turn puts upward pressure on employment and wages. The extent to which shocks to local labor demand manifest in higher employment versus higher wages depends on the elasticity of labor supply, including the migration margin. In particular, two hypotheses of the local labor markets model are that (1) home prices should respond more to disasters in areas with relatively inelastic housing supply and (2) population (assumed to move with housing quantity) should respond more to disasters in areas with relatively elastic housing supply.

We test these predictions by estimating the home price and population disaster IRFs separately for counties with a Saiz (2010) housing supply elasticity above the median (“elastic”) vs. below the median (“inelastic”). The results in **Figure 5** show strong evidence supporting both predictions. Namely, the post-disaster increase in home prices occurs only in areas where housing supply is relatively inelastic, while a post-disaster increase in population occurs only in areas where housing supply is relatively elastic. The intuition is that higher demand (driven by higher amenities and/or higher productivity) expands housing supply to accommodate a higher population where it can, but drives up prices in places where housing supply is closer to fixed.

Similarly, that wages increase over the longer-run while employment returns to pre-disaster levels, might suggest that external labor supply—that is, net in-migration of workers—is relatively inelastic on average. However, an alternative interpretation is that the industry composition of employment shifts after disasters toward higher-wage industries, thereby pulling up the average wage. To test this, we calculate the predicted wage for a county based on its industry employment shares and assuming local wages within each industry equal the national average.²⁸ Estimating its IRF, we find in **Figure 6** that these local composition-implied wages generally increase over time after a disaster, though the statistical significance varies by horizon. As of 8 years out, the point estimate is about 0.1%—about one-fifth the total increase in AWW found in **Figure 3**. However, the coefficient is not statistically significant at horizon 8. This

²⁸ See **Appendix B.2** for a discussion of how we construct this measure using CEPR yearly extracts of the CPS Outgoing Rotation Group micro-data and Eckert et al. (2021)’s version of the Census Bureau’s County Business Patterns (CBP) data.

suggests that a shift in the composition of the local workforce toward higher income individuals likely explains some, but not all, of the longer-run increase in average wages.²⁹

One might expect natural disasters to negatively affect both productivity and amenities in the short run. However, after that immediate negative impact, productivity and amenities may recover to their pre-disaster levels over the medium to longer run. For example, Davis and Weinstein (2002) found that within 15 years of the extensive bombing of some Japanese cities in World War II, the spatial population distribution had fully recovered. Yet, natural disasters can differ from war destruction shocks in at least one important way. A natural disaster may increase the probability of future disasters perceived by local producers and residents, reducing the attractiveness (anticipated productivities and amenities) of the area. If so, the disaster may lead to (a) permanently lower economic activity in the area and (b) a more rapid transition (e.g., investment, employment, and construction) to the new lower steady state.

In contrast, in some settings one might expect economic activity after damaging shocks to more than fully recover. In particular, shocks could improve productivity and amenities over the medium to longer run if capital and housing stocks are “built back better.” Hornbeck and Keniston (2017) develop a theoretical micro-foundation for the build back better scenario and provide supporting empirical evidence from the 1872 Great Fire of Boston. According to their model, underinvestment in local buildings can occur in growing urban areas because an individual landowner does not internalize the increase in property values of other local buildings that occurs when the landowner improves their own property. A disaster, such as the Great Fire, causes many landowners to simultaneously improve/modernize their properties, increasing the values of all local properties, even those undamaged by the disaster. Such a process may be facilitated by insurance and government aid, especially in advanced economies like the U.S. An interesting, testable implication of the Hornbeck and Keniston (2017) model is that property values should increase after a disaster only in growing localities because those are the localities subject to underinvestment.

We test this prediction in our sample by estimating the home price disaster IRF separately for growing vs. declining counties, based on a county’s 3-year pre-disaster HPI growth rate

²⁹ We assume here that changes in industry mix affect AWW by affecting average hourly wages. It is also possible that changes in industry mix affect AWW by affecting average hours per week if hours per week vary by industry, in which case our results here understate the role of industry mix.

(deflated by the CPI). The results are shown in **Figure 7**. Consistent with the Hornbeck and Keniston (2017) model, the positive response of home prices to a disaster shock appears to be driven by growing counties.

Finally, large amounts of insurance payouts and government aid triggered by natural disasters in the U.S. could have positive or negative net effects on local economic activity. On the one hand, aid may spur economic activity through a government spending multiplier. While estimates of the size of the government spending multiplier vary widely, the literature generally has found large multipliers on employment and income in local areas from federal spending that is not financed by local taxation (i.e., local windfall spending).³⁰ On the other hand, aid received by displaced households—especially aid that is not required to be spent on rebuilding—may facilitate household relocation away from affected areas. In particular, while Small Business Administration (SBA) disaster loans need to be repaid and generally rely on the homes that the funds are intended to repair as collateral, monies received by households from FEMA Individual Assistance aid and NFIP payouts have fewer strings attached. Although we are not able to test for the role of aid due to lack of exogenous variation in aid conditional on our disaster measures, aid is likely playing a major role in determining how local economic activity evolves after disasters in our setting.

VI. Heterogeneity, Adaptation, and Spatial Spillovers

We now explore three sets of extensions relating to heterogeneity, adaptation, and spatial spillovers.

A. Heterogeneity

Although informative for many policy questions, the *average* dynamic response of local economic activity to natural disasters presented above may mask important heterogeneities. In this subsection, we focus on heterogeneity in terms of disaster type (i.e., floods, hurricanes, etc.) and disaster severity.

³⁰ See, for example, Shoag (2013), Wilson (2012), Chodorow-Reich et al. (2012), and Chodorow-Reich (2019) for state-level evidence and Suárez Serrato and Wingender (2016) for county-level evidence.

1. Heterogeneity by type of disaster

In our first heterogeneity analysis, we explore how economic responses to natural disasters vary by disaster type. Specifically, we estimate the following joint regression:

$$y_{c,t+h} - y_{c,t-1} = \sum_{d \in \mathcal{D}} \beta_d^h D_{c,t}^d + \mathbf{X}'_{ct} \boldsymbol{\gamma}^h + \alpha_{r(c),t}^h + \alpha_{c,m(t)}^h + \epsilon_{c,t+h} \quad (3)$$

where $D_{c,t}^d$ is an indicator for a disaster of type d . The set of disaster types, \mathcal{D} , consists of hurricanes, floods, severe storms, extreme winter weather, fires, tornadoes, and other. For each type d , the estimates of β_d^h trace out the IRF of the outcome variable with respect to a disaster of that type. For these regressions, we modify the first term of the control vector so that the leads and lags of disasters are differentiated by type:

$$\mathbf{X}'_{ct} \boldsymbol{\gamma}^h \equiv \sum_{d \in \mathcal{D}} \sum_{\substack{\tau=-p, \\ \tau \neq 0}}^h D_{c,t+\tau}^d + \sum_{\tau=-p}^{-1} \rho^{\tau h} y_{c,t+\tau}. \quad (4)$$

Shown in **Figure 8**, the results display significant heterogeneity in how personal income p.c. responds to different types of natural disasters. Hurricanes, tornadoes, and fires yield substantial medium- to longer-run increases in personal income p.c.³¹ However, fires are quite rare in our sample, reflecting just 2 percent of the disasters, and thus their IRFs are imprecisely estimated. In contrast, non-hurricane floods account for 60% of the disasters in our sample. For these floods, we find no statistically significant effects, with a long-run point estimate that is close to zero. Severe storms and extreme winter weather also do not appear to have significant impacts.³²

There are several potential explanations for this set of findings. First, disaster types differ in terms of damages, with hurricanes and tornadoes tending to have the highest estimated p.c. damages in affected counties (see **Figure A7**.) To the extent that hurricanes and tornadoes tend to more fully destroy structures such that they must be completely rebuilt, this could yield more aid and more productivity-enhancing improvements in the capital stock. Second,

³¹ These results on hurricanes contrast somewhat with prior findings by Strobl (2011) and Deryugina (2017). We examine these differences in results in Section VIII.

³² Coronese et al. (2023) also estimate the dynamic effects of severe storms on income p.c. using U.S. county panel data. Consistent with our results, they find no significant short-run or long-run effect of severe storms that receive FEMA disaster declarations. For more minor storms, which do not receive FEMA declarations, they find a modest short-run decline but a near-zero long-run impact.

wind damage has generally been covered by standard homeowner insurance plans while flood damage has not. Greater insurance coverage of hurricane and tornado damages could lead to greater insurance payouts which could help finance recovery efforts. Third, floods may disproportionately increase the perceived risk of future flooding in the same location, reducing incentives to make rebuilding and recovery investments.³³ In contrast, hurricanes and tornadoes may be perceived to be less likely to strike the same location again soon. An intensification analysis that interacts disaster treatment with indicators for how many of the previous ten years also had FEMA declarations in the county does not show meaningful differences in longer-run effects based on recent experience (see **Figure A8.**) However, this backward-looking intensification analysis may not fully capture differences in forward-looking expectations.

2. *Heterogeneity by disaster severity*

We next examine disaster severity, which is important given projections that some types of disasters may become more severe with climate change (USGCRP 2017). We seek to understand both whether the average effects apply to the most severe disasters and whether the most severe disasters are driving the estimated average effects. To explore how the economic response to a disaster varies with damages, we estimate the impulse response at horizon h by estimating

$$y_{c,t+h} - y_{c,t-1} = \beta_0^h D_{c,t} + \beta_1^h D_{c,t} s_{c,t} + \beta_2^h D_{c,t} s_{c,t}^2 + \mathbf{X}'_{ct} \boldsymbol{\gamma}^h + \alpha_{r(c),t}^h + \alpha_{c,m(t)}^h + \epsilon_{c,t+h} \quad (5)$$

where $s_{c,t}$ denotes p.c. damages for county c in period t .³⁴ The estimated coefficients, $\hat{\beta}_p^h$ for $p = \{0,1,2\}$, from this regression allow us to compute the impulse response, $\hat{\beta}^h(s)$, for any given level of damages (s). We measure damages using SHEL DUS data aggregated to the county-month level and converted to a per capita basis using Census population data.³⁵ These data are based on the NOAA Storm Database (see **Appendix C.3** for details).

³³ Giglio et al. (2021) find that increases in perceived risk of future flooding in a local area (ZIP code) are associated with declines in local real estate prices, which would reduce the incentive to invest in new real estate construction projects.

³⁴ We obtain very similar results using a third-order polynomial. The third-order terms generally are found to be near zero and statistically insignificant.

³⁵ When only total damages are known for a given disaster, SHEL DUS splits the total among all disaster-declared counties equally. This can lead to outsize per capita damages for small counties. Given that more populous counties are likely to have more property at risk of damage, we adjust the data as follows. When we observe identical damages across counties for a given disaster, we redistribute total damages for that disaster to equate the

Some prior studies have measured damages or disaster severity using data on meteorological or geophysical characteristics, such as wind speed for hurricanes.³⁶ We focus here on pecuniary damages data for two reasons. First, historical data on physical characteristics are not readily available at the U.S. county level for a broad set of disaster types and are not comparable across disaster types when available. Second, and more importantly, we are interested in the effects not of natural hazards per se, but of disasters that result in economic destruction or disruption sufficient to warrant a FEMA declaration. Such destruction or disruption is inevitably a function of the built environment that those hazards occur in. For instance, the monetary damages caused by a physical event depend greatly on the quantity and market value of local property as well as building codes, construction quality, infrastructure, and other factors that affect resilience.³⁷ Furthermore, because FEMA disaster aid and other government aid are largely functions of damages, our results on disaster effects by damage level also will reflect the role of government aid. That said, for comparison, below we also present results specifically for hurricanes using wind speed to measure disaster severity.

For the same six economic outcomes included in **Figure 3**, **Figure 9** displays the estimated IRFs for different percentiles of (non-zero) p.c. damages in our sample, with details available in **Tables A2-A7**. In each panel, the solid thick (dark) blue line depicts the IRF corresponding to a disaster with p.c. damages equal to the 50th percentile of all disasters (with positive damages), while the thick solid (light) orange line depicts the IRF for a disaster with p.c. damages equal to the 99th percentile. The thin solid, dashed, and dash-dotted lines show the IRFs for other percentile damages.

Panel (a) shows the results for personal income p.c. The IRFs for disasters with damages up to the 90th percentile are very similar to the baseline IRF shown in **Figure 3**, panel (a). In particular, for 25th to 90th percentile damage disasters the longer-run ($h = 8$) effect estimates range from about 0.4 to 0.6%, which is very similar to the baseline effect of roughly 0.5%.

per capita damages across affected counties. Also, we winsorize damages p.c. at the 99.9th percentile to minimize the sensitivity of the results to extreme outliers.

³⁶ See, for example, Hsiang and Jina (2014), Felbermayr and Gröschl (2014), Deryugina (2017), Lackner (2019), and Jerch et al. (2023).

³⁷ For example, Bakkensen and Mendelsohn (2016) show that hurricane damages tend to be higher in the U.S. than in other OECD countries when examining responses to physical storm characteristics.

These results, which are statistically significant at the 90% level or above (see **Table A2**)³⁸, indicate that the average findings in the baseline are not simply driven by the most severe events.

Panel (a) also shows that the most severe disasters—those with damages at or above the 95th percentile—result in marked immediate increases in personal income p.c., which remains substantially higher than the counterfactual through the longer run. For instance, we estimate that a 99th percentile disaster causes personal income p.c. to increase by about 0.6% the year of the disaster (albeit not significantly) and then by about 2.5% after one year and over 3% in the longer run. These results indicate that the positive response of income p.c. to disasters increases with disaster severity.³⁹

To unpack the income p.c. finding, we next examine how other outcomes respond to different disaster severities. The results are shown in **Figure 9**, panels (b)-(f). They indicate more severe disasters cause large and persistent increases in construction employment and AWW (panels (b) and (d)), though statistical power declines at the highest percentiles.^{40,41} For population and home prices, the longer-run effects of the most severe disasters are not statistically significant. However, the point estimates are suggestive of a longer-run decline in population and no longer-run change in home prices for the most severe disasters.⁴²

On the whole, these results suggest that the magnitude of our baseline responses increases with severity, as measured by monetary damages p.c. However, we note that unlike for our baseline estimates of average disaster effects—which Figure 4 panels (a) and (b) showed to be robust to using either FEMA declaration or wind speed to measure hurricane exposure—the personal income by disaster severity results *are* sensitive to the damages measure. The estimated longer run response of personal income to the most extreme hurricane exposures is positive when using SHELDUS (like the **Figure 9** (a) result for all disaster types) but zero when using wind

³⁸ Though there are some negative point estimates for some of the short- and medium-run horizons, these are not economically or statistically significant.

³⁹ It is worth noting that our finding of a significant longer-run increase in income p.c. after very severe disasters is consistent with the observed pattern of income p.c. following the most severe disaster in our sample, Hurricane Katrina in 2005 (see **Figure A9**.)

⁴⁰ The longer-run increases in total private nonfarm employment shown in **Figure 9**, panel (b), are not statistically significant at any percentile (see **Online Table A3**).

⁴¹ The large increase in AWW over the longer run is consistent with worker-level evidence of higher long-term wages following the major 2005 hurricanes, Katrina and Rita (see Groen et al. (2020)).

⁴² This could reflect a persistent decline in attractiveness of locations hit by the most severe disasters, resulting in lower population, combined with fairly elastic long-run housing supply preventing a drop in home prices.

speed (see **Figure A10**). As discussed above, this could reflect that wind speed does not account for other physical aspects of hurricanes such as rainfall and flooding or the value or quality of property at risk, leading to attenuation bias toward zero.⁴³

The results above suggest that the post-disaster boost to income p.c. increases with severity as measured by monetary damages. What can explain this phenomenon? One possibility is that very damaging disasters trigger large investment in new, modern public and private infrastructure, funded by government aid as well as private insurance, which spurs local economic development, consistent with the “build back better” scenario from **Figure 2** and the theory and evidence discussed in Section V. Another possibility, consistent with the large decline in population, is that the composition of households in affected counties is changed by the most severe natural disasters, with lower-income households more likely to move away. This possibility is consistent with Sheldon and Zhan (2021), who find that post-disaster out-migration increases with disaster severity and more so the lower the income of the population, and with Graff Zivin et al. (2020), who find that incoming homebuyers have higher incomes after hurricanes have hit an area in Florida.

B. Adaptation and Changes Over Time

Greater experience with disasters may lead to learning, while expectations of more future disasters can spur adaptative investments in resilience. These phenomena could cause average responses to disasters to vary with experience and over time. We now perform two analyses to explore these dynamics.

First, we investigate whether disaster IRFs vary with a county’s historical experience with disasters. To do so, we estimate the following specification:

$$y_{c,t+h} - y_{c,t-1} = \sum_{e \in E} \beta_e^h M_c^e D_{c,t} + \mathbf{X}'_{ct} \boldsymbol{\gamma}^h + \alpha_{r(c),t}^h + \alpha_{c,m(t)}^h + \epsilon_{c,t+h} \quad (6)$$

where M_c^e is a pair of experience indicators ($e = \text{high or low}$) and $D_{c,t}$ is a disaster indicator.

High experience is measured by being above median for the number of disasters of that type hit

⁴³ To assess this concern, we examined the wind speeds for some of the most destructive hurricane disasters in the U.S. over the 1988–2017 sample period covered by the wind speed data. Online **Table A8** shows the top 25 highest p.c. damages county-month FEMA hurricane disaster declarations, of which only 6 had wind speeds at or above the 98th percentile of wind speeds, only 9 had wind speeds above NOAA’s threshold for a “major” hurricane, and 9 had wind speeds below NOAA’s minor hurricane threshold. These results highlight that wind speed alone can understate the monetary destruction caused by a local area’s hurricane exposure.

by the county over the full sample period. We estimate equation (6) separately for two types of disasters: hurricanes and floods.⁴⁴ When estimating the effects of hurricanes (floods), we add to $\mathbf{X}'_{ct}\boldsymbol{\gamma}^h$ a set of controls indicating whether another type of disaster hit the county between $t - p$ and $t + h$. The estimates of β_e^h trace out the IRFs for low- and high-experience counties.

Figure 10 shows results for hurricanes in panel (a) and floods in panel (b). For both hurricanes and floods, the longer-run impact of the disaster is similar for high- and low-experience counties. The differential effect of experience on the short-run disaster impact varies by type of disaster. The short-run impact is more positive for high-hurricane-experience counties than low-hurricane-experience counties. In contrast, the short-run impacts for high- and low-flood-experience counties are not statistically distinguishable, though the latter is statistically significantly positive, whereas the former is not. Overall, these results suggest experience plays no important role in the long run, though it may impact short-run outcomes, depending on the context.

Our second analysis addresses a related question: how the average disaster response across *all* counties—regardless of their individual disaster experiences—has changed over time. There have been important federal policy changes in the U.S. during our sample. Most notably, the 1988 Stafford Act formalized and streamlined the FEMA declaration and aid processes, which among other things generally increased the amount of aid received by local governments, households, and businesses after a disaster declaration. At the same time, expectations regarding the likelihood of disasters may be increasing, which could lead to adaptive investments. Such adaptations could mitigate the initial negative impacts of disasters on income as well as wealth, property, and health. Furthermore, a more resilient infrastructure could yield smaller boosts to income that result from rebuilding and recovery efforts.

To test whether the average response to disasters changed over time, and especially after the Stafford Act, we estimate a variant of specification given by equations (1) and (2) where we interact $D_{c,t+\tau}$, for $\tau = -3, \dots, 8$, with a post-1988 dummy variable. This allows for the joint estimation of pre- and post-Stafford Act IRFs. For this exercise, we extend our sample back to 1969, which is possible for income p.c. but not for our other outcomes (given data limitations). The results are shown in **Figure 10**, with the estimated pre-Stafford Act IRF in panel (c) and the

⁴⁴ As shown in **Figure A1**, other types of disasters rarely hit the same county in our sample period more than two or three times, leaving very few counties that could be considered “high experience.”

post-Stafford Act IRF in panel (d). We find the post-Stafford Act effect of disasters on income has been similar to our baseline estimates, with a positive longer-run effect that is statistically significant at the 90% level. In contrast, for pre-Stafford disasters, we estimate zero longer-run effect, though with fairly large confidence intervals. Because of the wide confidence intervals, we cannot statistically reject the null hypothesis that the longer-run effect before and after Stafford are equal. Nonetheless, these results are suggestive that the Stafford Act may have played an important role in our baseline results, reinforcing the idea that federal aid is a key explanation for the post-disaster boost we observe in personal income p.c.

C. Spatial Spillovers

We have thus far focused on estimating the economic impacts of natural disasters at the county level. The impacts at higher levels of aggregation could be different as important spatial spillovers could propagate the effects of a disaster in one county to other counties of varying distances away. However, it is also possible that post-disaster responses divert resources (such as funds and skilled labor) from areas that were not directly affected by disasters, raising concerns about the efficiency of disaster aid.

We investigate spillovers by adding spatial lags of the disaster indicator and its time lags to our baseline regression specification. The spatial lags are weighted averages of the disaster indicator in other counties of a given distance band away from the focal county.

Specifically, we build on our baseline specification in equation (1) by adding continuous treatment variables $D_{c,t}^b$ measuring the occurrence of disasters in other counties of varying distances away from county c :

$$y_{c,t+h} - y_{c,t-1} = \sum_{b \in B} \pi^{h,b} D_{c,t}^b + \beta^h D_{c,t} + \mathbf{X}'_{ct} \boldsymbol{\gamma}^h + \alpha_{r(c),t}^h + \alpha_{c,m(t)}^h + \epsilon_{c,t+h} \quad (7)$$

For any given focal county, c , we split all other counties into B separate distance bands indexed by b . The spatial lag variable $D_{c,t}^b$ is a population weighted-average of the disaster indicator over counties whose population centroids are between b and b' miles away from the population centroid of county c :

$$D_{ct}^b \equiv \sum_{i \in S_c^b} \omega_{ci}^b D_{it}, \quad (8)$$

where

$$\omega_{ci}^b \equiv \frac{pop_i}{\sum_i pop_i}. \quad (9)$$

S_c^b denotes the set of counties whose population centroids are between b and b' miles away from the population centroid of county c and pop_i denotes population of county i . For example, suppose county c has 10 million people living in a band ranging from 200 to 399 miles away. If in year t the counties within that band that were hit by disasters collectively had a population of 2 million, then $D_{c,t}^{200}$ would be 0.2.

In **Figure 11**, we show the results of estimating equation (7) for personal income for bands of counties that are up to 199, 200-399, and 400-599 miles away from a county affected by a disaster.^{45,46} The IRF for the directly-hit counties are shown in panel (a), while the spatial lag IRFs are shown in panels (b)-(d).^{47,48} Controlling for spatial lags, the long-run own-county effect remains positive but has wider confidence intervals than the baseline specification. It is statistically significant at the 90% level. The IRF point estimates for nearby counties (within 199 miles) tend to be positive while those for counties a bit further away (200-399 miles) tend to be negative, but the statistical significance of both varies by horizon. The IRF for counties further away (400 to 599 miles) is around zero and statistically insignificant at all horizons.

Panel (e) provides an estimate of the net effect on a county's personal income p.c. of disasters anywhere within 600 miles of the county, including disasters hitting the county itself. The net effect is the sum of the four separate effects (panels (a) – (d)). To ease interpretation, we rescale each of the four components of this sum by multiplying by the sample mean of the

⁴⁵ See **Figure A11** for a visual illustration of the spatial lags for a single year of disasters.

⁴⁶ We also estimated a version of equation (7) (a Spatial Durbin model) where we added three spatial lags—constructed analogously to those in equations (8)-(9)—of the dependent variable and obtained IRFs for the disaster treatments very similar to those in **Figure 11**.

⁴⁷ This own-county effect estimated here differs from our baseline estimate shown in **Figure 3** because many disasters affect multiple neighboring counties, in which case we would have to add to the panel (a) curve the effect captured in the panel (b) curve.

⁴⁸ In panels (b)-(d), the spatial lag coefficients have been normalized by dividing $D_{c,t}^b$ by its mean, conditional on $D_{c,t}^b > 0$. Thus, a one-unit change in each spatial lag variable represents the average population share in that distance band of disaster-hit counties in the event of at least one disaster. These conditional means vary slightly across horizons. The coefficients $\pi^{h,b}$ can then be interpreted as the impact in county c from the average disaster event hitting at least one county b to b' miles away.

corresponding variable. The sample means are roughly 0.2 for each of four variables, meaning that roughly 20% of counties are hit by a FEMA disaster in an average year. This rescaling allows us to interpret the estimated net effect as the percentage change in personal income per capita within a 600-mile radius impacted by a treatment equal to the average population exposure to disasters within a 600-mile radius. We find a negative but not quite statistically significant (p-value equal to 0.105) net effect in the initial year followed by economically and statistically insignificant net effects over all subsequent horizons. In particular, the longer-run ($h = 8$) net effect has a point estimate very close to zero and is far from statistically significant, albeit with a fairly large confidence interval. These results suggest that while the average longer run impact of a disaster on income p.c. in a county directly hit by a disaster appears to be positive, the longer-run impact for the broader region appears to be near zero.⁴⁹

VII. Conclusion

By applying a unified framework to a broad range of disasters, we have found that, on average, counties hit by FEMA-declared natural disasters experience a longer-run boost in income p.c. While a rise in employment contributes to the initial boost, the longer-run increase in income p.c. is driven largely by an increase in average weekly wages. Construction employment and home prices also increase over the longer run while population is roughly unchanged.

We further find that there is significant heterogeneity in income responses with hurricanes and tornadoes yielding positive longer run effects, but other types of disasters yielding zero or statistically insignificant effects. Furthermore, the most severe disasters (measured by p.c. monetary damages) yield the largest increases to income, while there is no evidence of a longer-run income response for disasters that occurred before the Stafford Act formalized and streamlined the FEMA aid process.

These results can be explained in the context of standard spatial equilibrium models whereby disasters result eventually in improvements to local capital, which in turn improve local productivity and amenities. Increases in productivity and amenities increase demand for

⁴⁹ We also estimated state-level disaster effects by aggregating the county data to the state level for each period. Here the disaster treatment becomes the share of state population in counties with disasters during the year. The results in **Figure A12** show small and statistically insignificant effects at all horizons, suggesting that positive own-county effects and negative other-county (within-state) effects roughly offset at the state level.

both labor and housing. Our findings that, on average, disasters lead to longer-run increases in wages and home prices, rather than employment and population, suggest relatively inelastic supply of both labor and housing on average. Furthermore, we find that composition shifts may explain some of the increases in income, which could reflect lower-wage workers finding it more difficult to afford housing in these areas.

Standard spatial equilibrium theory can also explain some of the heterogeneity we observe. In particular, we observe increases in personal income after hurricanes and tornadoes but not after floods. If the perceived probability of repeat incidents in a location is high after floods but low after hurricanes and tornadoes, the willingness to make the recovery investments that improve local capital stock may be much greater after hurricanes and tornadoes than floods. Indeed, under the Hazard Mitigation Grant Program, FEMA has bought out tens of thousands of flood-prone homes in recent decades to mitigate National Flood Insurance Costs through managed retreat (Kousky 2014, Mach et al. 2019).

Our findings are also consistent with the Hornbeck and Keniston (2017) framework in which disasters can lead to significant improvements in capital where the ex-ante quality is inefficiently low due to externalities. Thus, the boost to income that we observe could reflect a correction from a socially suboptimal level of local investment.

However, the boost to income could instead signal an overshooting of aid to counties directly impacted by disasters or to particular populations within those counties. While the main focus of this paper has been on the *local* impact of natural disasters, our spatial analyses do not show significant net effects when accounting for potential spillovers in broader regions. One implication is that the post-disaster reinvestment and redevelopment in an affected county may be financed in part by governmental resources diverted from other areas in the region. If so, government aid—both allocated and expected—may pose moral hazard concerns by encouraging development in high-risk areas. Furthermore, the lack of evidence that the poverty rate declines as average total personal income increases, suggests that the economic benefits of post-disaster aid may disproportionately support higher income individuals, leading to compositional shifts in affected counties.

Taken together, our findings suggest that despite the immense toll that disasters take, local economies in the U.S. generally follow the “build back better” path, with a longer-run boost in incomes p.c. These findings—which could reflect investment or compositional shifts or both—

pertain to the U.S. in recent decades, a country and time period with a well-developed private insurance market and in which disasters generally triggered substantial government aid to local affected areas. Disasters in other contexts may well have more negative economic effects. Even in the U.S., natural disasters, which are projected to increase in frequency and severity going forward due to climate change, pose substantial economic risks. To the extent that the recovery of local economies relies on public and private insurance, more numerous and damaging disasters will put increasing demands on insurance markets and government budgets. An important question for future research in light of the issues of moral hazard, increasing strain, and potentially inefficiently low pre-disaster investment is what kind of post-disaster path would be socially optimal.

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Table 1: Variable Descriptions

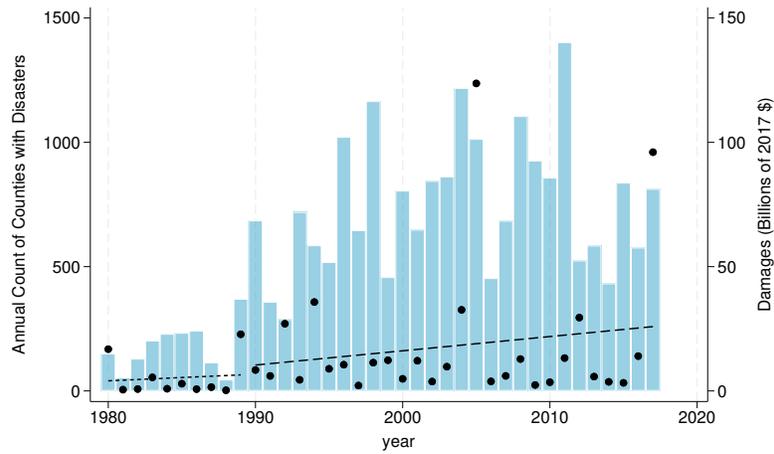
Variable	Frequency	Form	Winsorized	Per capita	Source
Personal income	annual	log	no	yes	BEA
Total nonfarm employment	monthly	log	0.5, 99.5	no	QCEW
Construction employment	monthly	log	0.5, 99.5	no	QCEW
Average weekly wages	quarterly	log	0.5, 99.5	no	QCEW
Home price index	quarterly	log	no	no	CoreLogic
Population	annual	log	no	no	Census
Government transfers	annual	log	no	yes	BEA
Income maintenance transfers	annual	log	no	yes	BEA
UI transfers	annual	log	no	yes	BEA
FEMA IHP aid	annual	$\log(1 + \cdot)$	0.5, 99.5	yes	FEMA
SBA disaster loans	annual	$\log(1 + \cdot)$	0.5, 99.5	yes	SBA
NFIP payouts	annual	$\log(1 + \cdot)$	0.5, 99.5	yes	FEMA
Wage & salary income	annual	log	no	yes	BEA
Poverty rate	annual	log	no	yes	SAIPE
Migration	annual	percent	no	yes	IRS SOI
Industry-based hourly wages	annual	log	yes	no	CBP, CEPR
Damages	monthly	linear	99.9	yes	SHELDUS
FEMA disasters	monthly	indicator	N/A	N/A	FEMA

Note: UI stands for unemployment insurance, FEMA for Federal Emergency Management Agency, IHP for Individual and Household Program (aid), SBA for the Small Business Administration, NFIP for the National Flood Insurance Program, BEA for the Bureau of Economic Analysis, QCEW for the Quarterly Census of Employment and Wages, SAIPE for the Small Area Income and Poverty Estimates (at the Census Bureau), IRS SOI for the Internal Revenue Service Statistics on Income, CBP for the Census Bureau's County Business Pattern data, CEPR for the Center for Economic Policy Research, and SHELDUS for the Spatial Hazard Events and Losses Database for the United States.

Data on IHP aid and NFIP payments are aggregated to annual frequency to be comparable to data on other government transfer income. Also, IHP aid and NFIP payments data reflect the time period of the disaster with which they are associated; data on when they are paid out is not available. The annual SBA Loan data are based on the fiscal year ending September 30 of the indicated calendar year. We examine $\log(1 + \cdot)$ form for the aid variables to address the very high share of observations with zero aid.

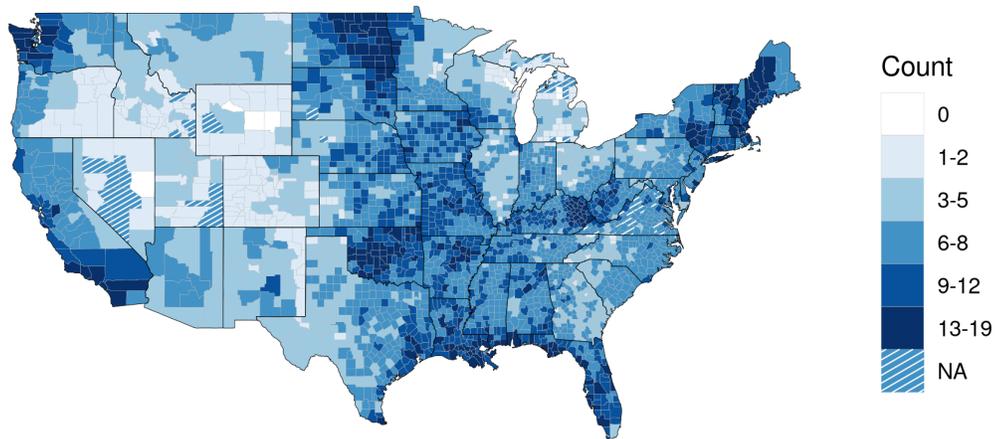
Figure 1: Natural Disaster Trends and Distribution, 1980 - 2017

(a) Disaster Frequency and Damages



(b) Geographic Distribution of Disasters

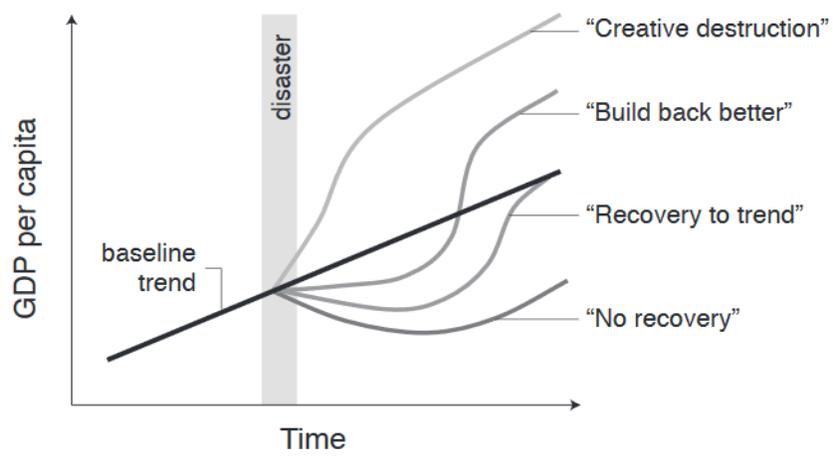
All Disaster Types



Source: Federal Emergency Management Agency (FEMA) and SHELDUS.

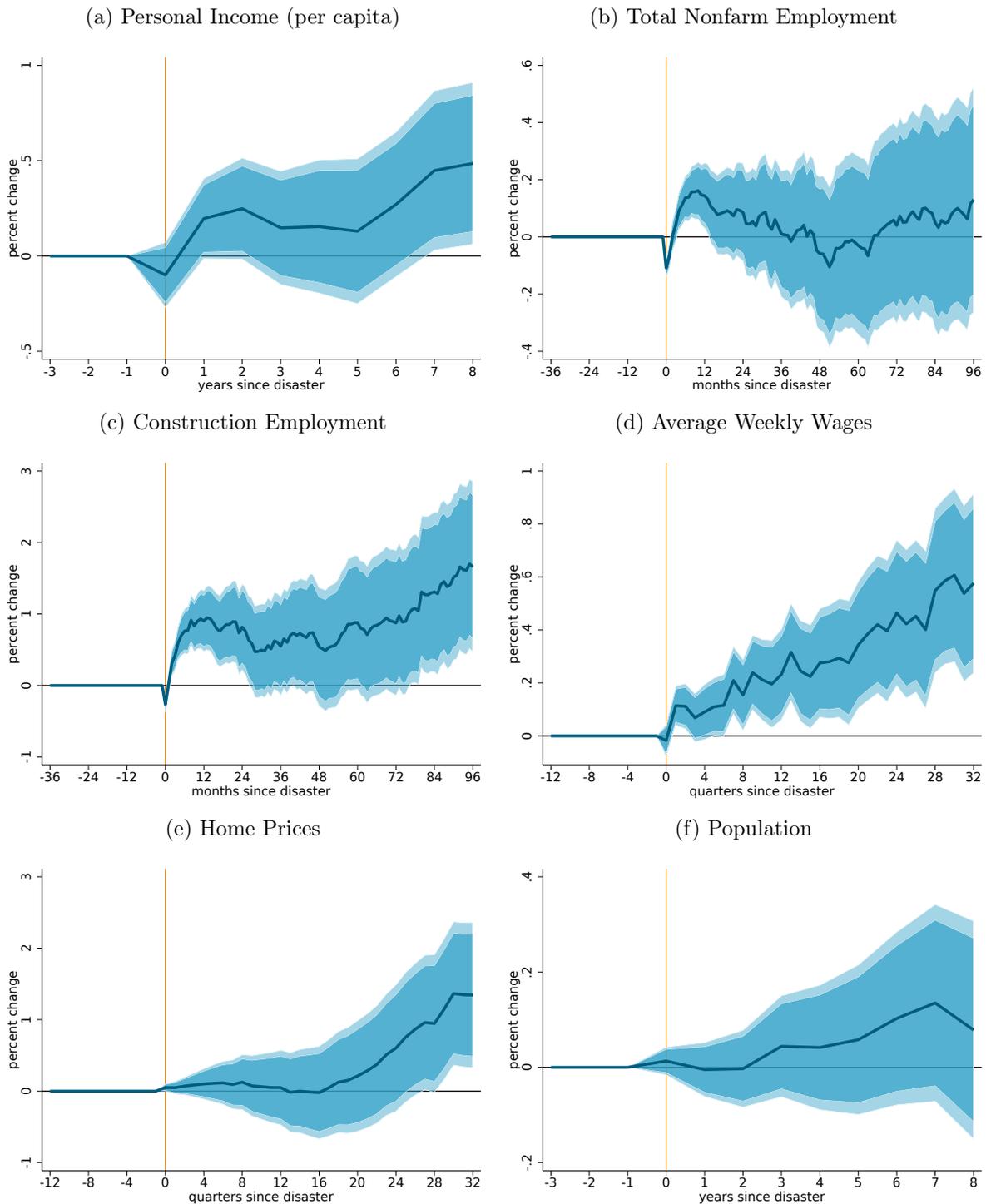
Note: Panel (a) shows for each year the number of counties with at least one disaster declaration (bars) and nationwide monetary damages (circles). The dashed lines show linear trends in damages for 1980-1989 and 1990-2017. The count in panel (b) shows the number of years with disaster declarations for each county over the period 1980-2017.

Figure 2: Theoretical paths for disaster recovery



Source: Hsiang and Jina (2014)

Figure 3: Impulse Responses of Selected Outcomes to Disaster Shocks

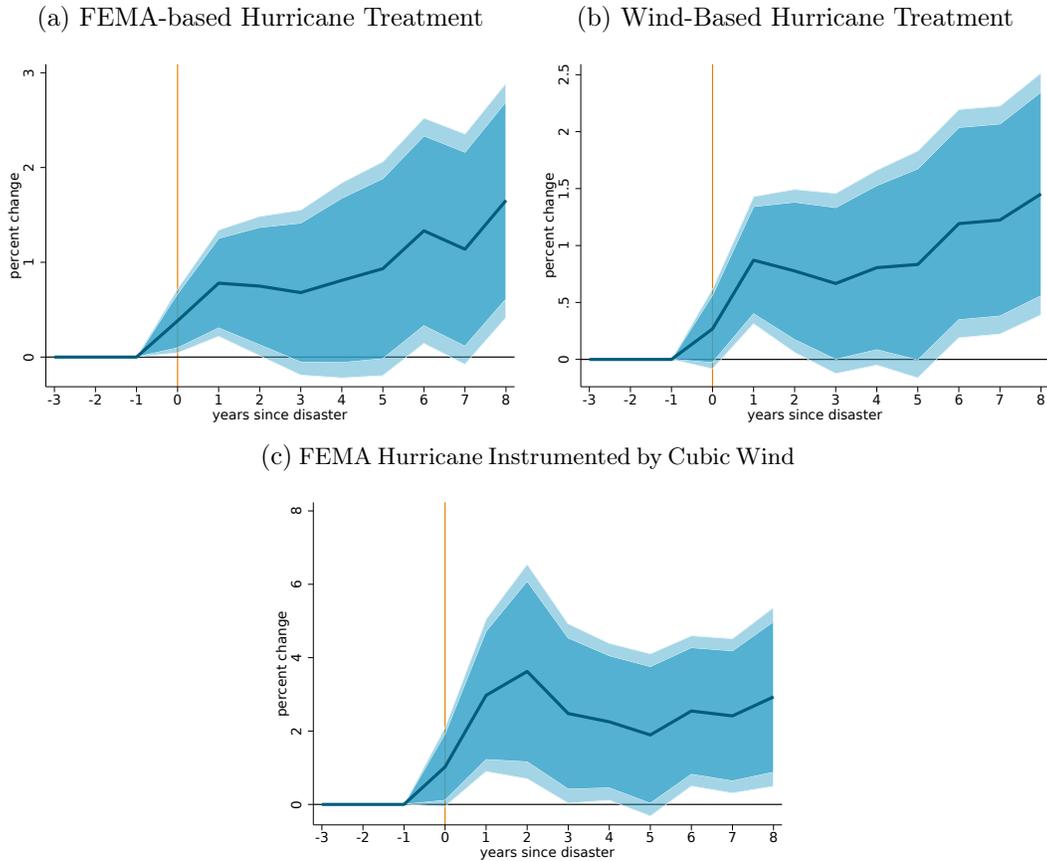


Source: Federal Emergency Management Agency (FEMA), BLS, Census, BEA, and CoreLogic.

Note: These plots show the impulse response functions from estimating equation (1), where the shaded regions indicate the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and state-by-time levels. All outcome variables are observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

Figure 4: Robustness of Treatment Measure, Based on Hurricane Disasters

Impulse Response of Personal Income (p.c.) to Disaster Shocks

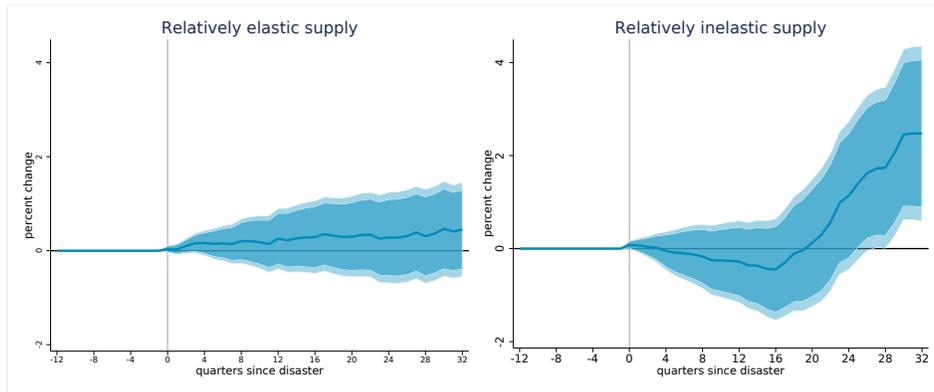


Source: FEMA, Census, BEA, SHELDUS, Anderson et al (2020).

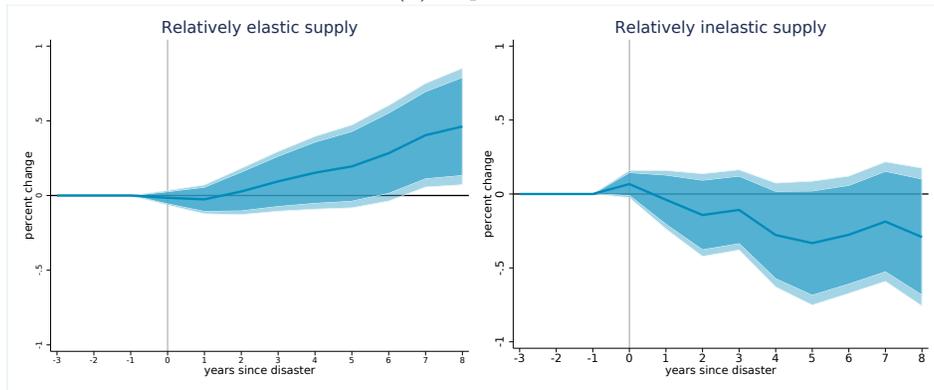
Note: This plot shows the impulse response functions from estimating equation (1). Panel (a) defines treatment as FEMA declaration for a hurricane, controlling for all other FEMA disaster declarations. Panel (b) uses a wind speed of at least 34 knots to determine county treatment by a hurricane (regardless of whether there was a FEMA declaration) and includes controls for all other FEMA disaster declarations. Panel (c) repeats the estimation in panel (a) but using a cubic of wind speed to instrument for FEMA hurricane declarations.

Figure 5: Heterogeneity by housing supply elasticity

(a) Home Prices



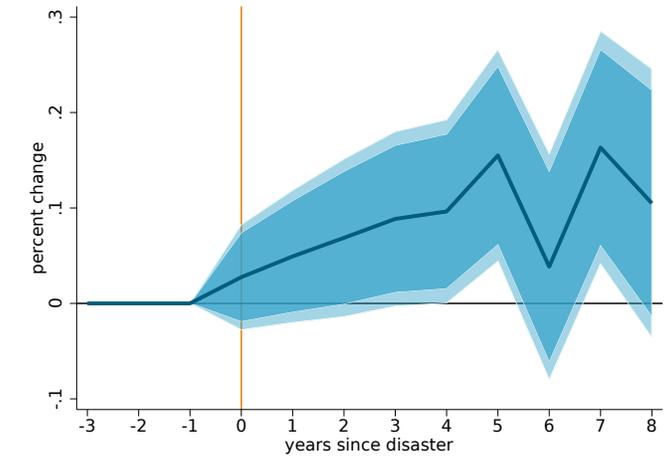
(b) Population



Source: FEMA, BEA, and CoreLogic.

Note: This figure shows the impulse response function from estimating equation (1) interacted with an indicator for whether a county's Saiz elasticity is in the top half of the sample. Standard errors are clustered at the county and at the state-by-time levels.

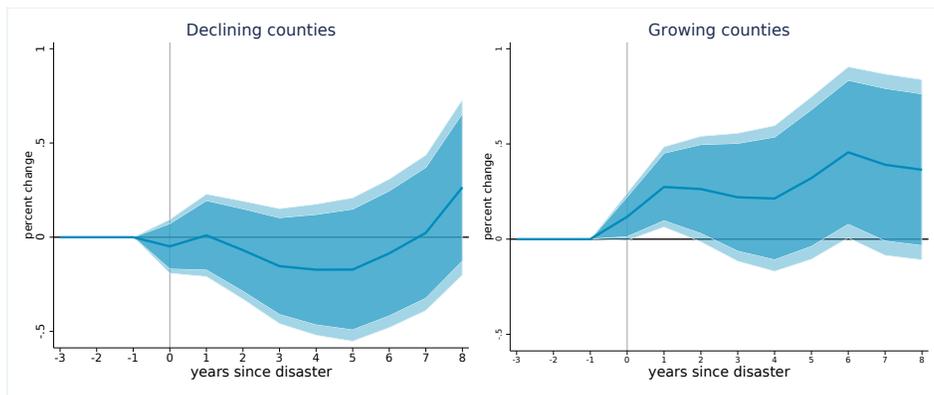
Figure 6: Wages Implied by Local Industry Mix



Source: FEMA, SHELDUS, CEPR yearly extracts of the CPS Outgoing Rotation Group micro-data, and the Census Bureau’s County Business Patterns.

Note: Figure shows the impulse response function from estimating equation (1) where the dependent variable is an estimate of what the mean wage would be if the local wage composition is applied to the national wage rates. The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels.

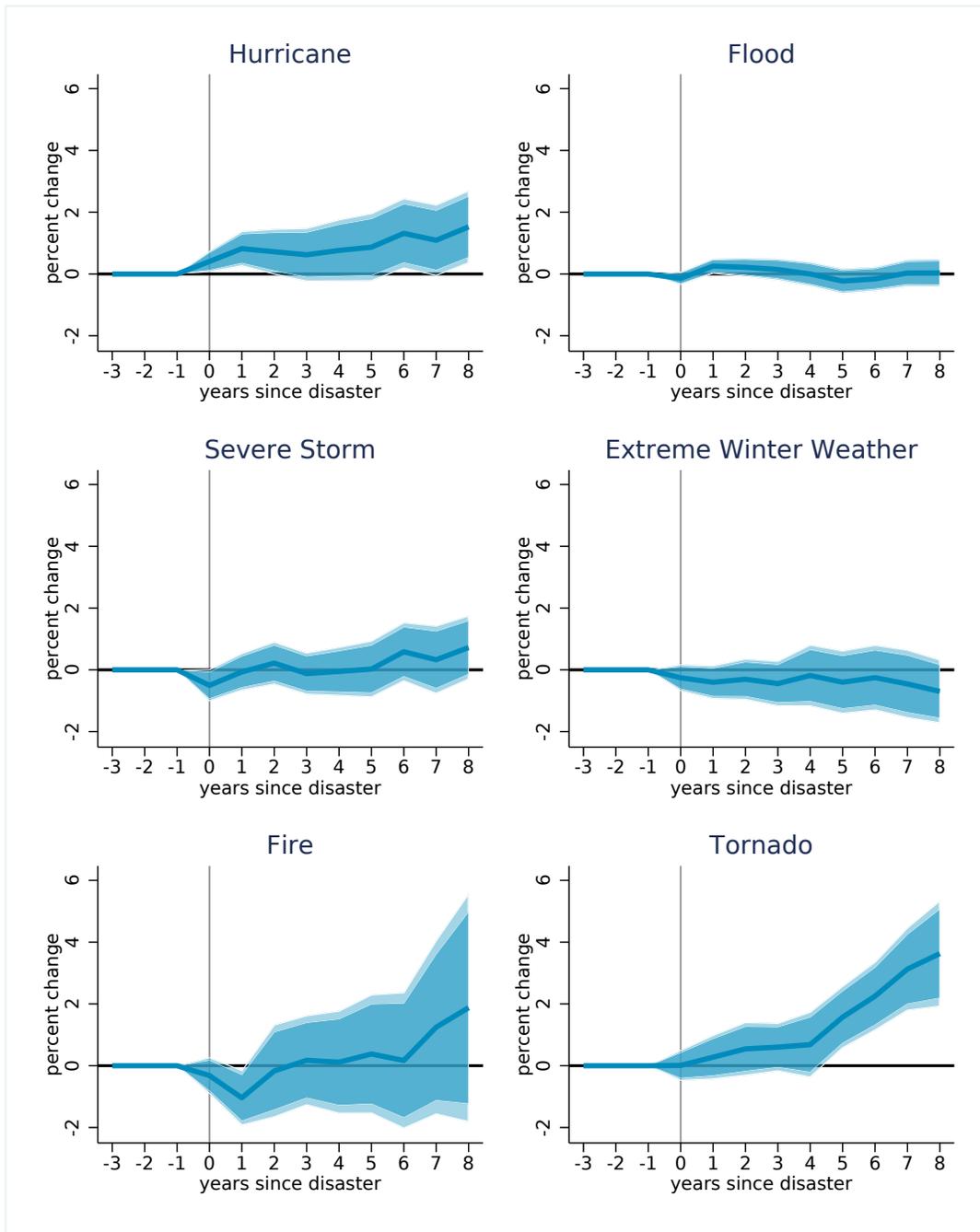
Figure 7: Growing versus declining areas (in terms of home prices)



Source: FEMA, BEA, and CoreLogic.

Note: This figure shows the impulse response function of personal income p.c. from estimating equation (1) interacted with an indicator for whether the home price index was increased (net of inflation) over the three years prior to the disaster. Standard errors are clustered at the county and at the state-by-time levels.

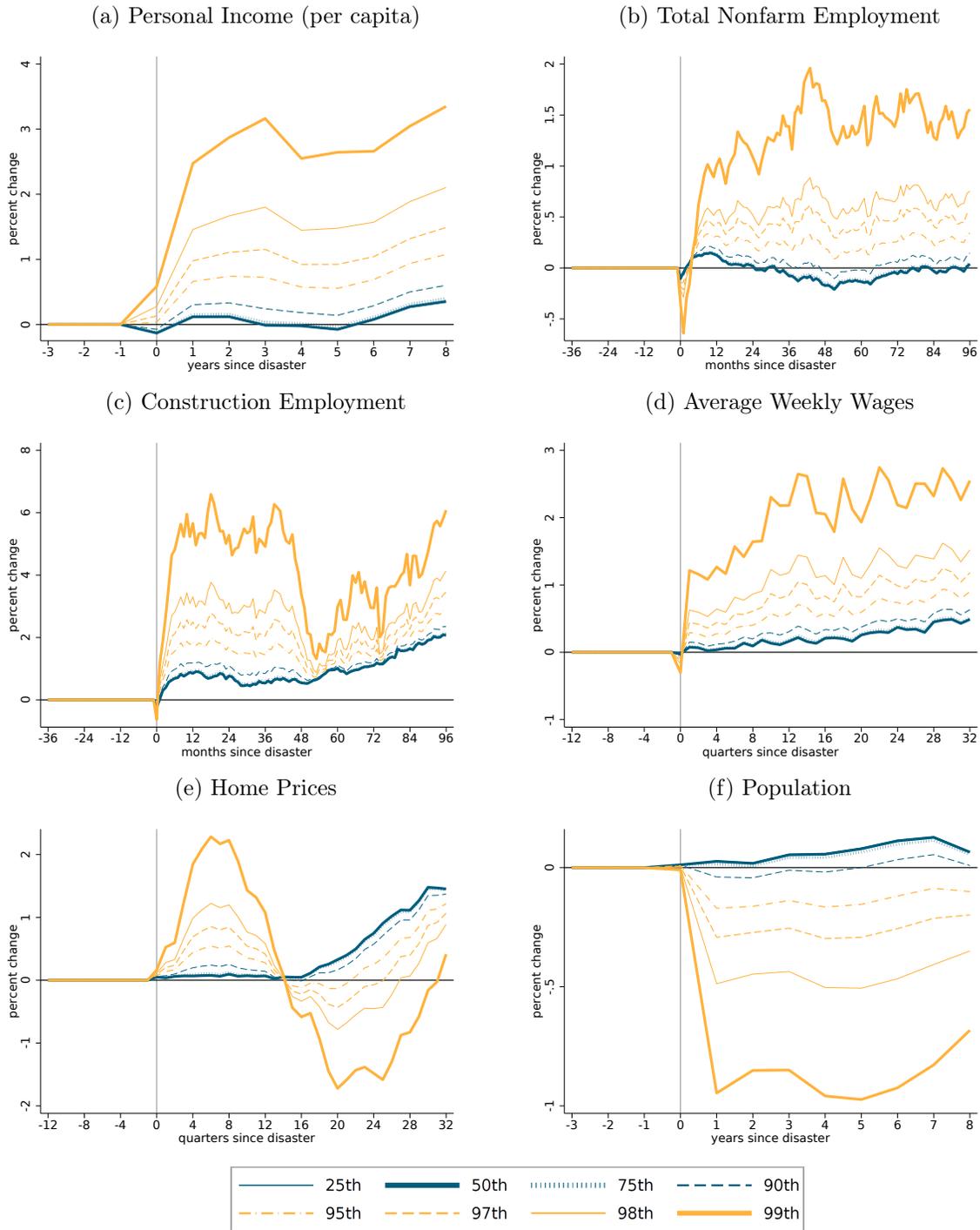
Figure 8: Heterogeneous Impulse Responses of Personal Income (p.c.) to Disaster Shocks By Type



Source: FEMA, SHELDUS, BEA, and Census.

Note: These plots show the impulse response functions from estimating equation (5), where the shaded regions show the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. The disaster type categories are based on FEMA declaration types and titles, with the flood category excluding floods associated with hurricanes. Each disaster is assigned a single type. However, within a year a county can experience multiple disasters and hence multiple types. Personal income per capita is observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

Figure 9: Heterogeneous Impulse Responses by Selected Percentiles of Damages Per Capita

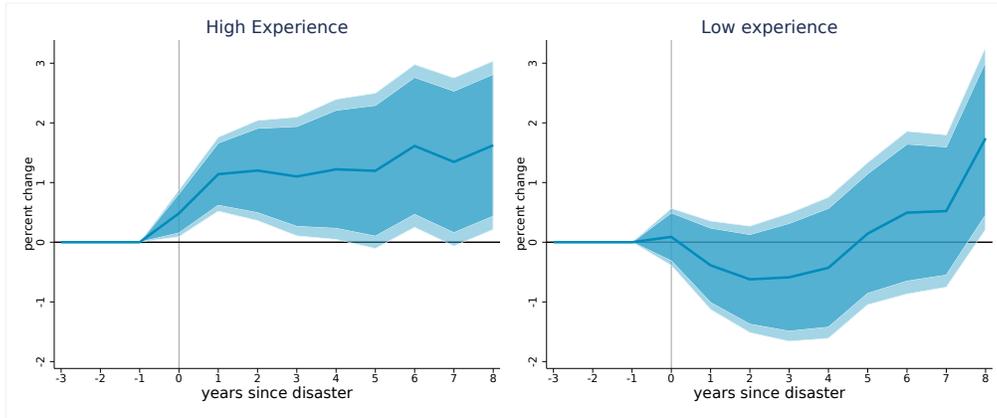


Source: FEMA, BLS, Census, BEA, CoreLogic, and SHELUDS.

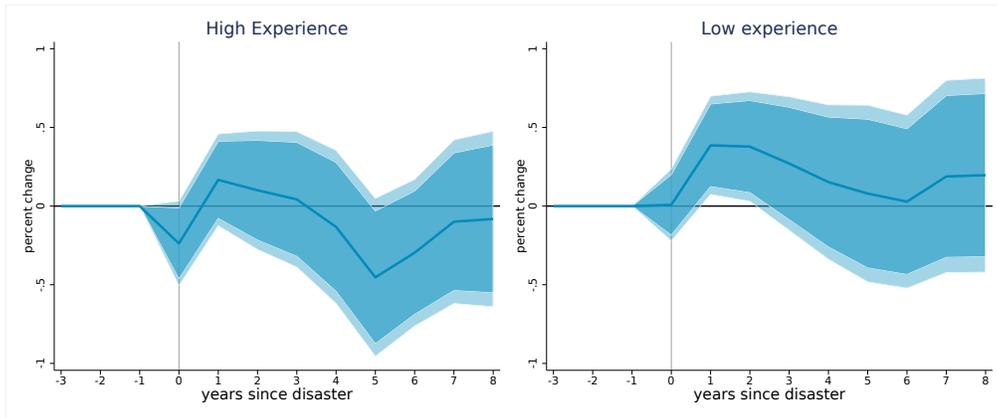
Note: These plots show the impulse response functions (IRFs) from estimating equation (4) with a second-order polynomial, where the percentile lines show the implied IRF for selected percentiles of the damages per capita distribution over all county-month observations with a positive-damages disaster. Damages per capita are winsorized at the 99.9th percentile. All variables are observed at the county level and modeled as differences in logs between the indicated horizon h and the period before the disaster (-1).

Figure 10: Adaptation and Changes Over Time

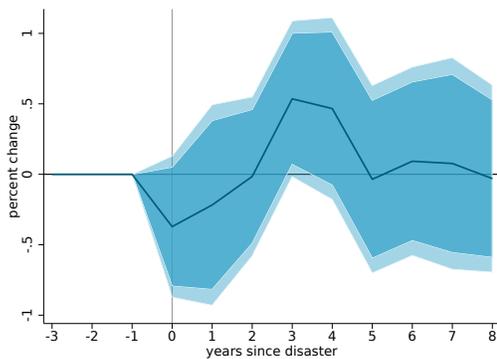
(a) Heterogeneous Impulse Responses By County Experience: Hurricanes



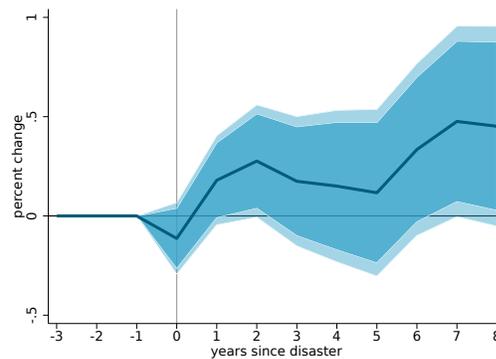
(b) Heterogeneous Impulse Responses By County Experience: Floods



(c) Impulse Responses, Pre-Stafford Act



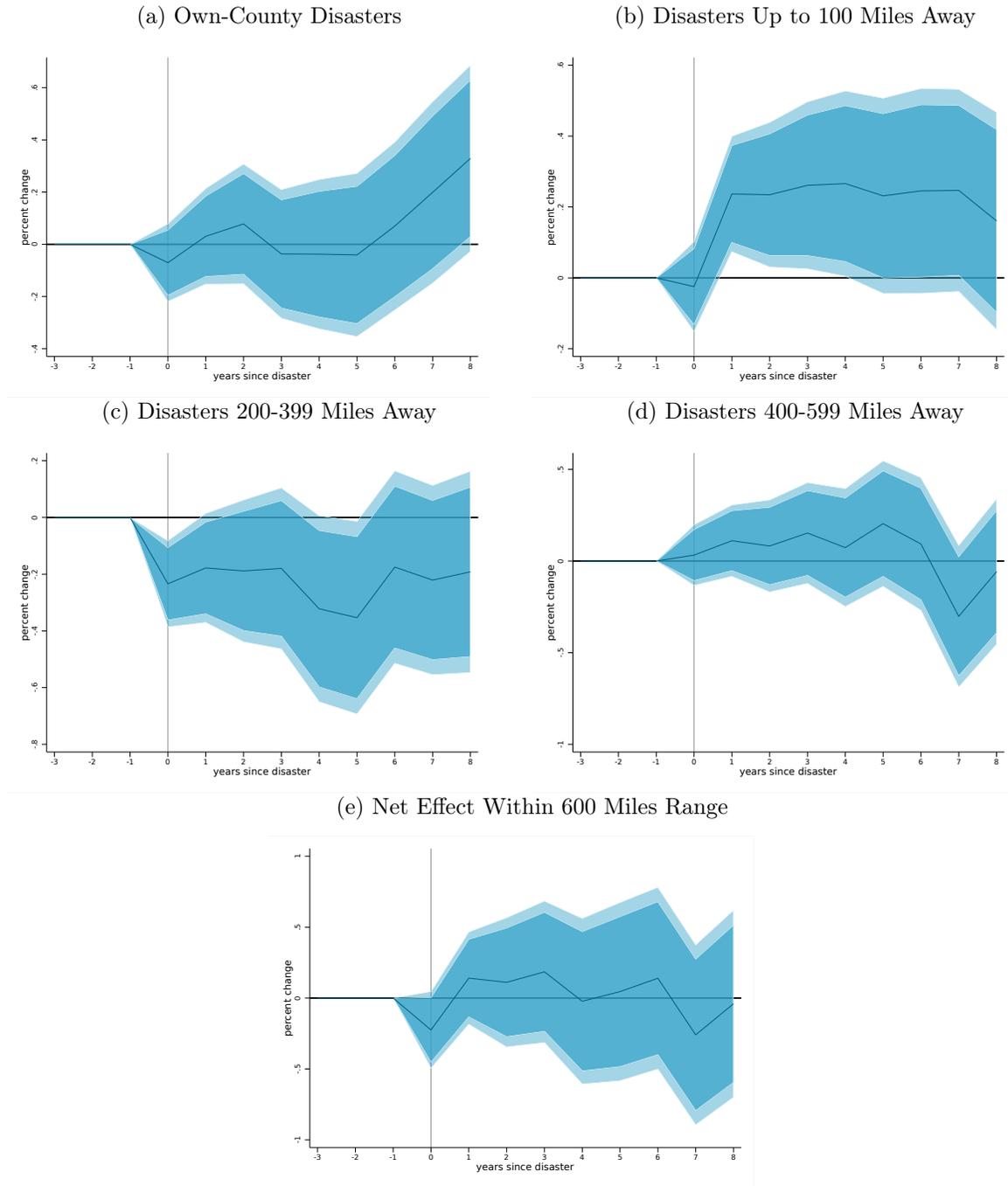
(d) Impulse Responses, Post-Stafford Act



Source: FEMA, SHELDUS, BEA, and Census.

Note: Panels (a) and (b) show the personal income (p.c.) impulse response functions (IRFs) from estimating equation (7), where hurricane or flood indicators are interacted with experience indicators. High experience counties are those that experience a number of hurricanes or floods above the 50th percentile frequency (for counties that have ever experienced such a disaster type). Panels (c) and (d) show the pre- vs. post-Stafford Act IRFs obtained from estimating equation (7) with an interaction between the disaster dummy and an indicator for whether the disaster occurred after the Stafford Act (1988). For this analysis, the sample is extended back to 1969. For all panels, the shaded regions show the 90 and 95 percent confidence intervals based on standard errors clustered at the county and state-by-time levels. Personal income per capita is observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

Figure 11: Impacts of Own-County and Spatially-Lagged Disasters on Personal Income (p.c.)



Source: FEMA, Census, and BEA.

Note: Panels (a)-(d) show the impulse response functions (IRFs) from estimating equation (8), where the shaded regions show the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. There is a separate IRF for the own-county disaster treatment and each of the three spatial lags. The intensity of treatment for the spatial lags is the share of population within each band that has experienced a disaster in period 0. Each of the spatial lag IRFs has been rescaled by the mean population share for positive observations within the band. Thus the IRFs represent the average effect on counties having at least one county within the given range experience a disaster in period 0. Panel (e) shows the net effect on personal income within these bands, where each coefficient has been rescaled by the variable's unconditional mean. Personal income per capita is observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

Online Appendix A – Not For Publication

Table A1: Summary Statistics

	Mean	SD	Min	Median	Max	N
Levels						
Personal income (per capita)				27,640		115,383
Private nonfarm employment				5,697		1,380,648
Construction employment				368		945,759
Average weekly wages				572		460,216
Home price index				95.4		183,568
Population				24.2		115,434
FEMA disaster declaration				0		1,385,328
Per capita damages				1.87		25,145
First differences of logs						
Personal income per capita				1.47		112,345
Private nonfarm employment				.209		1,377,395
Construction employment				.159		872,810
Average weekly wages				.331		456,963
Home price index				.884		183,568
Population				.351		115,430

Source: QCEW, Census, CoreLogic, BEA, FEMA, and SHELDUS.

Note: See Table A1 and its notes for description of variable names and further details. Summary statistics reflect observations between 1980 and 2017. The first differences of log variables have been rescaled by 100 so that they show percentage point values. The per capita damages are shown only for observations with FEMA disaster declarations. Income and wages are reported in 2010 US dollars. Damages are reported in 2017 US dollars.

Table A2: Severity Percentile Regression Details - Personal Income Per Capita

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	-0.125 (-1.41)	0.119 (1.09)	0.120 (0.88)	-0.009 (-0.06)	-0.019 (-0.11)	-0.074 (-0.40)	0.091 (0.49)	0.288 (1.43)	0.375* (1.78)
Disaster \times Damages	0.000 (1.62)	0.000** (2.16)	0.000* (1.79)	0.001* (1.79)	0.000 (1.59)	0.001* (1.74)	0.001* (1.75)	0.001* (1.77)	0.001** (2.02)
Disaster \times Damages ²	-0.000 (-0.78)	-0.000* (-1.87)	-0.000 (-0.93)	-0.000 (-1.33)	-0.000 (-1.10)	-0.000 (-1.20)	-0.000 (-1.44)	-0.000 (-1.62)	-0.000* (-1.93)
<i>Net Effects</i>									
25th Percentile	-0.124 (-1.41)	0.120 (1.10)	0.122 (0.89)	-0.007 (-0.05)	-0.018 (-0.10)	-0.073 (-0.39)	0.092 (0.49)	0.289 (1.44)	0.376* (1.79)
50th Percentile	-0.122 (-1.38)	0.127 (1.18)	0.130 (0.95)	0.002 (0.02)	-0.010 (-0.06)	-0.064 (-0.35)	0.100 (0.54)	0.298 (1.49)	0.386* (1.84)
75th Percentile	-0.112 (-1.28)	0.160 (1.50)	0.168 (1.25)	0.049 (0.33)	0.028 (0.16)	-0.024 (-0.13)	0.140 (0.76)	0.340* (1.72)	0.431** (2.07)
90th Percentile	-0.066 (-0.73)	0.310** (2.48)	0.341** (2.09)	0.257 (1.35)	0.200 (0.98)	0.159 (0.75)	0.320 (1.53)	0.531** (2.37)	0.636*** (2.78)
95th Percentile	0.041 (0.33)	0.664*** (2.62)	0.752** (2.16)	0.749* (1.79)	0.605 (1.51)	0.591 (1.50)	0.742* (1.92)	0.975** (2.39)	1.113*** (2.84)
97th Percentile	0.137 (0.80)	0.977** (2.51)	1.118** (2.08)	1.183* (1.82)	0.963 (1.59)	0.971 (1.64)	1.112* (1.92)	1.364** (2.24)	1.529*** (2.65)
98th Percentile	0.281 (1.14)	1.452** (2.40)	1.678** (2.02)	1.840* (1.84)	1.503 (1.62)	1.547* (1.72)	1.666* (1.90)	1.944** (2.11)	2.149** (2.47)
99th Percentile	0.586 (1.45)	2.455** (2.31)	2.882** (1.98)	3.220* (1.85)	2.637* (1.66)	2.754* (1.79)	2.808* (1.87)	3.128** (1.99)	3.405** (2.30)
R-squared	0.28	0.33	0.34	0.36	0.40	0.44	0.47	0.50	0.54
Observations	106,047	103,017	99,989	96,959	93,929	90,899	87,871	84,841	81,813

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: BEA, Census, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A3: Severity Percentile Regression Details - Total Private Nonfarm Employment

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	-0.100*** (-5.42)	0.128** (2.10)	0.021 (0.24)	-0.075 (-0.65)	-0.177 (-1.28)	-0.141 (-0.87)	-0.064 (-0.36)	-0.037 (-0.18)	0.033 (0.15)
Disaster \times Damages	-0.000** (-2.49)	0.000*** (3.17)	0.000** (1.99)	0.000* (1.84)	0.000* (1.73)	0.000 (1.51)	0.000 (1.42)	0.000 (0.94)	0.000 (1.02)
Disaster \times Damages ²	0.000 (1.63)	-0.000*** (-2.75)	-0.000 (-1.35)	-0.000 (-1.38)	-0.000 (-1.41)	-0.000 (-1.36)	-0.000 (-1.43)	-0.000 (-0.68)	-0.000 (-0.84)
<i>Net Effects</i>									
25th Percentile	-0.100*** (-5.42)	0.128** (2.11)	0.022 (0.24)	-0.075 (-0.65)	-0.177 (-1.28)	-0.141 (-0.87)	-0.064 (-0.36)	-0.036 (-0.18)	0.033 (0.16)
50th Percentile	-0.101*** (-5.46)	0.130** (2.15)	0.025 (0.28)	-0.071 (-0.62)	-0.172 (-1.24)	-0.136 (-0.84)	-0.060 (-0.33)	-0.033 (-0.17)	0.037 (0.17)
75th Percentile	-0.104*** (-5.62)	0.142** (2.36)	0.038 (0.43)	-0.053 (-0.46)	-0.149 (-1.08)	-0.116 (-0.72)	-0.039 (-0.22)	-0.017 (-0.09)	0.055 (0.26)
90th Percentile	-0.118*** (-6.07)	0.199*** (3.19)	0.103 (1.05)	0.035 (0.27)	-0.042 (-0.26)	-0.018 (-0.10)	0.057 (0.29)	0.059 (0.27)	0.141 (0.62)
95th Percentile	-0.149*** (-5.73)	0.324*** (3.94)	0.245* (1.73)	0.226 (1.16)	0.196 (0.77)	0.199 (0.73)	0.267 (0.91)	0.228 (0.68)	0.332 (0.96)
97th Percentile	-0.182*** (-5.01)	0.456*** (3.97)	0.398* (1.94)	0.434 (1.48)	0.452 (1.17)	0.431 (1.06)	0.492 (1.14)	0.410 (0.82)	0.537 (1.04)
98th Percentile	-0.214*** (-4.51)	0.579*** (3.88)	0.544** (2.00)	0.630 (1.61)	0.694 (1.34)	0.650 (1.20)	0.704 (1.23)	0.584 (0.87)	0.732 (1.06)
99th Percentile	-0.342*** (-3.66)	1.039*** (3.60)	1.127** (2.09)	1.416* (1.78)	1.656 (1.56)	1.509 (1.36)	1.521 (1.31)	1.286 (0.94)	1.510 (1.07)
R-squared	0.57	0.30	0.34	0.39	0.43	0.47	0.51	0.55	0.59
Observations	1,267,362	1,230,219	1,193,196	1,156,218	1,119,309	1,082,478	1,045,710	1,008,996	972,369

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: QCEW, Census, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A4: Severity Percentile Regression Details - Construction Employment

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	-0.245*** (-3.83)	0.828*** (3.78)	0.747** (2.39)	0.507 (1.40)	0.525 (1.21)	1.010** (2.06)	1.116** (2.07)	1.649*** (2.77)	2.091*** (3.30)
Disaster \times Damages	-0.000 (-1.05)	0.001*** (2.64)	0.001*** (2.59)	0.001*** (2.61)	0.001 (1.02)	0.000 (0.56)	0.000 (0.45)	0.000 (0.47)	0.001 (0.98)
Disaster \times Damages ²	0.000 (1.45)	-0.000 (-1.46)	-0.000* (-1.84)	-0.000 (-1.43)	-0.000 (-0.15)	0.000 (0.45)	0.000 (0.45)	0.000 (0.34)	-0.000 (-0.69)
<i>Net Effects</i>									
25th Percentile	-0.245*** (-3.83)	0.830*** (3.79)	0.748** (2.40)	0.509 (1.40)	0.526 (1.21)	1.011** (2.07)	1.117** (2.07)	1.650*** (2.77)	2.093*** (3.30)
50th Percentile	-0.246*** (-3.85)	0.842*** (3.84)	0.761** (2.44)	0.520 (1.43)	0.533 (1.23)	1.015** (2.07)	1.120** (2.07)	1.653*** (2.77)	2.102*** (3.32)
75th Percentile	-0.251*** (-3.95)	0.899*** (4.08)	0.817*** (2.61)	0.572 (1.58)	0.565 (1.30)	1.033** (2.10)	1.136** (2.10)	1.671*** (2.80)	2.145*** (3.40)
90th Percentile	-0.275*** (-4.14)	1.187*** (4.66)	1.101*** (3.26)	0.832** (2.21)	0.726 (1.56)	1.125** (2.11)	1.218** (2.08)	1.762*** (2.78)	2.362*** (3.54)
95th Percentile	-0.330*** (-3.42)	1.866*** (4.24)	1.767*** (3.58)	1.446*** (2.94)	1.114 (1.61)	1.353* (1.77)	1.420* (1.70)	1.984** (2.23)	2.873*** (2.96)
97th Percentile	-0.382*** (-2.74)	2.539*** (3.83)	2.423*** (3.45)	2.056*** (3.09)	1.505 (1.52)	1.592 (1.48)	1.634 (1.39)	2.216* (1.78)	3.380** (2.42)
98th Percentile	-0.443** (-2.25)	3.367*** (3.56)	3.222*** (3.30)	2.805*** (3.10)	1.999 (1.44)	1.907 (1.29)	1.917 (1.18)	2.520 (1.46)	4.003** (2.03)
99th Percentile	-0.599 (-1.64)	5.785*** (3.34)	5.498*** (3.12)	4.997*** (3.09)	3.531 (1.39)	2.983 (1.11)	2.896 (0.99)	3.546 (1.12)	5.823 (1.59)
R-squared	0.49	0.27	0.33	0.39	0.43	0.47	0.51	0.54	0.58
Observations	633,984	607,902	581,841	555,792	529,851	503,925	478,065	452,229	426,420

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: QCEW, Census, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A5: Severity Percentile Regression Details - Average Weekly Wages

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	-0.024 (-0.73)	0.041 (0.78)	0.089 (1.29)	0.152* (1.80)	0.196* (1.92)	0.243** (2.06)	0.358*** (2.64)	0.439*** (2.80)	0.484*** (2.84)
Disaster \times Damages	-0.000 (-1.40)	0.000** (2.37)	0.000* (1.92)	0.000** (1.98)	0.000* (1.87)	0.000 (1.55)	0.000* (1.67)	0.000 (1.39)	0.000 (1.33)
Disaster \times Damages ²	0.000 (1.28)	-0.000 (-0.67)	-0.000 (-1.01)	-0.000 (-1.35)	-0.000 (-1.27)	-0.000 (-0.92)	-0.000 (-1.09)	-0.000 (-0.76)	-0.000 (-0.66)
<i>Net Effects</i>									
25th Percentile	-0.024 (-0.73)	0.041 (0.78)	0.090 (1.30)	0.153* (1.81)	0.196* (1.93)	0.244** (2.06)	0.358*** (2.64)	0.439*** (2.81)	0.485*** (2.85)
50th Percentile	-0.025 (-0.76)	0.044 (0.84)	0.094 (1.37)	0.158* (1.88)	0.201** (1.98)	0.248** (2.10)	0.363*** (2.68)	0.444*** (2.85)	0.490*** (2.89)
75th Percentile	-0.029 (-0.89)	0.059 (1.12)	0.112* (1.66)	0.183** (2.19)	0.224** (2.22)	0.269** (2.29)	0.386*** (2.86)	0.467*** (3.01)	0.515*** (3.06)
90th Percentile	-0.047 (-1.37)	0.126** (2.11)	0.200** (2.49)	0.301*** (2.87)	0.331*** (2.84)	0.367*** (2.72)	0.494*** (3.25)	0.576*** (3.34)	0.634*** (3.33)
95th Percentile	-0.088* (-1.69)	0.281*** (2.63)	0.401** (2.48)	0.569*** (2.66)	0.576*** (2.69)	0.591** (2.46)	0.738*** (2.92)	0.824*** (2.78)	0.905*** (2.71)
97th Percentile	-0.130* (-1.67)	0.444*** (2.64)	0.610** (2.33)	0.846** (2.45)	0.828** (2.46)	0.822** (2.19)	0.991** (2.56)	1.081** (2.35)	1.187** (2.28)
98th Percentile	-0.180 (-1.62)	0.643*** (2.62)	0.864** (2.23)	1.180** (2.34)	1.134** (2.32)	1.102** (2.03)	1.297** (2.34)	1.393** (2.09)	1.529** (2.03)
99th Percentile	-0.319 (-1.53)	1.261*** (2.64)	1.637** (2.16)	2.182** (2.23)	2.047** (2.17)	1.947* (1.87)	2.210** (2.09)	2.335* (1.83)	2.564* (1.78)
R-squared	0.77	0.65	0.59	0.55	0.53	0.53	0.53	0.53	0.54
Observations	422,454	410,073	397,732	385,406	373,103	360,826	348,570	336,332	324,123

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: QCEW, Census, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A6: Severity Percentile Regression Details - Home Prices

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	0.050* (1.91)	0.061 (0.57)	0.087 (0.45)	0.058 (0.22)	0.037 (0.11)	0.331 (0.83)	0.760* (1.66)	1.128** (2.30)	1.480*** (2.85)
Disaster × Damages	0.000 (0.64)	0.000 (1.49)	0.000 (1.41)	0.000 (0.48)	-0.000 (-0.31)	-0.000 (-0.63)	-0.000 (-0.55)	-0.000 (-0.48)	-0.000 (-0.30)
Disaster × Damages ²	-0.000 (-0.70)	-0.000 (-1.14)	-0.000 (-1.12)	-0.000 (-0.34)	0.000 (0.71)	0.000 (0.87)	0.000 (0.74)	0.000 (0.64)	0.000 (0.41)
<i>Net Effects</i>									
25th Percentile	0.050* (1.91)	0.061 (0.58)	0.088 (0.45)	0.058 (0.22)	0.037 (0.11)	0.330 (0.83)	0.759* (1.66)	1.128** (2.30)	1.479*** (2.85)
50th Percentile	0.051* (1.92)	0.066 (0.62)	0.094 (0.48)	0.061 (0.23)	0.035 (0.11)	0.323 (0.82)	0.752* (1.65)	1.122** (2.30)	1.476*** (2.85)
75th Percentile	0.052** (1.98)	0.090 (0.83)	0.123 (0.62)	0.074 (0.28)	0.023 (0.07)	0.293 (0.75)	0.720 (1.60)	1.094** (2.26)	1.460*** (2.85)
90th Percentile	0.060*** (1.99)	0.203 (1.38)	0.257 (1.11)	0.135 (0.45)	-0.031 (-0.08)	0.153 (0.34)	0.570 (1.09)	0.964* (1.75)	1.387** (2.50)
95th Percentile	0.078 (1.55)	0.457 (1.57)	0.561 (1.43)	0.275 (0.55)	-0.150 (-0.22)	-0.160 (-0.19)	0.234 (0.24)	0.674 (0.68)	1.223 (1.33)
97th Percentile	0.096 (1.27)	0.718 (1.58)	0.871 (1.48)	0.418 (0.55)	-0.263 (-0.25)	-0.473 (-0.36)	-0.103 (-0.07)	0.382 (0.24)	1.059 (0.75)
98th Percentile	0.117 (1.09)	1.028 (1.58)	1.241 (1.49)	0.590 (0.54)	-0.389 (-0.26)	-0.839 (-0.44)	-0.497 (-0.22)	0.043 (0.02)	0.869 (0.42)
99th Percentile	0.173 (0.87)	1.926 (1.59)	2.309 (1.50)	1.098 (0.53)	-0.673 (-0.23)	-1.828 (-0.51)	-1.572 (-0.35)	-0.880 (-0.20)	0.353 (0.09)
R-squared	0.47	0.64	0.67	0.68	0.69	0.70	0.72	0.73	0.74
Observations	168,610	163,694	158,778	153,859	148,940	144,021	139,101	134,181	129,261

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: CoreLogic, QCEW, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A7: Severity Percentile Regression Details - Population

	Years After Disaster								
	0	1	2	3	4	5	6	7	8
Disaster	0.010 (0.71)	0.026 (0.77)	0.018 (0.43)	0.052 (0.91)	0.058 (0.83)	0.083 (1.02)	0.116 (1.23)	0.130 (1.21)	0.070 (0.59)
Disaster \times Damages	-0.000 (-0.02)	-0.000 (-1.56)	-0.000 (-1.64)	-0.000* (-1.94)	-0.000** (-2.36)	-0.000** (-2.27)	-0.000* (-1.93)	-0.000 (-1.40)	-0.000 (-0.96)
Disaster \times Damages ²	-0.000 (-0.76)	-0.000 (-0.77)	-0.000 (-0.58)	-0.000 (-0.31)	0.000 (0.05)	0.000 (0.26)	0.000 (0.35)	0.000 (0.29)	0.000 (0.11)
<i>Net Effects</i>									
25th Percentile	0.010 (0.71)	0.026 (0.77)	0.018 (0.42)	0.052 (0.90)	0.057 (0.83)	0.082 (1.01)	0.116 (1.22)	0.130 (1.21)	0.069 (0.58)
50th Percentile	0.010 (0.71)	0.024 (0.72)	0.016 (0.38)	0.050 (0.87)	0.054 (0.79)	0.079 (0.98)	0.113 (1.19)	0.127 (1.19)	0.067 (0.57)
75th Percentile	0.010 (0.71)	0.013 (0.44)	0.006 (0.15)	0.039 (0.70)	0.042 (0.61)	0.066 (0.82)	0.099 (1.06)	0.115 (1.08)	0.058 (0.49)
90th Percentile	0.010 (0.69)	-0.035 (-1.03)	-0.038 (-0.84)	-0.010 (-0.18)	-0.015 (-0.22)	0.005 (0.06)	0.038 (0.39)	0.059 (0.53)	0.015 (0.12)
95th Percentile	0.010 (0.49)	-0.153 (-1.57)	-0.146 (-1.56)	-0.128 (-1.40)	-0.153 (-1.53)	-0.141 (-1.20)	-0.107 (-0.78)	-0.073 (-0.44)	-0.086 (-0.46)
97th Percentile	0.009 (0.34)	-0.262 (-1.61)	-0.246* (-1.66)	-0.235* (-1.71)	-0.276** (-1.96)	-0.272* (-1.69)	-0.238 (-1.26)	-0.192 (-0.83)	-0.178 (-0.68)
98th Percentile	0.007 (0.19)	-0.439 (-1.64)	-0.404* (-1.71)	-0.405* (-1.87)	-0.469** (-2.22)	-0.474** (-2.03)	-0.438 (-1.60)	-0.374 (-1.10)	-0.320 (-0.84)
99th Percentile	-0.001 (-0.01)	-0.857* (-1.68)	-0.774* (-1.75)	-0.790** (-1.98)	-0.896** (-2.42)	-0.916** (-2.35)	-0.874* (-1.93)	-0.771 (-1.37)	-0.631 (-1.00)
R-squared	0.50	0.61	0.66	0.70	0.72	0.74	0.76	0.78	0.80
Observations	106,080	103,050	100,022	96,992	93,962	90,932	87,904	84,874	81,846

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: Census, QCEW, FEMA, and SHELDUS.

Note: Table shows regression result details from estimating equation (5), underlying estimates for Figure 9.

Table A8: Top 25 Most Damaging FEMA County Hurricane Disaster Declarations

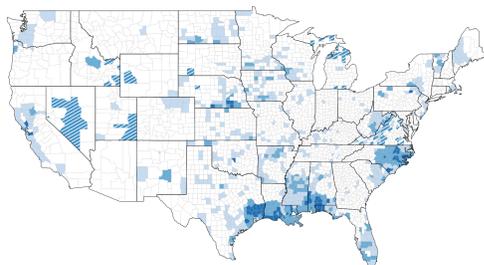
Hurricane	County	State	Year	Month	Wind Speed (knots)	Wind Speed Percentile
Harvey	Aransas	TX	2017	8	129	99.8
Katrina	Hancock	MS	2005	8	121	99.5
Katrina	Harrison	MS	2005	8	98	96.1
Katrina	Jackson	MS	2005	8	77	80.8
Katrina	Pearl River	MS	2005	8	108	98.0
Katrina	Walthall	MS	2005	8	75	76.5
Katrina	Pike	MS	2005	8	63	54.6
Katrina	Amite	MS	2005	8	56	28.6
Katrina	Wilkinson	MS	2005	8	*	
Harvey	Galveston	TX	2017	8	*	
Katrina	Jefferson	LA	2005	8	89	92.2
Katrina	Lafourche	LA	2005	8	69	67.1
Katrina	Plaquemines	LA	2005	8	114	99.0
Katrina	Saint Bernard	LA	2005	8	108	98.2
Katrina	Saint Tammany	LA	2005	8	101	96.8
Katrina	Orleans	LA	2005	8	96	95.3
Harvey	Montgomery	TX	2017	8	*	
Harvey	San Jacinto	TX	2017	8	*	
Sandy	Monmouth	NJ	2012	10	70	70.9
Harvey	Fort Bend	TX	2017	8	*	
Sandy	Ocean	NJ	2012	10	81	85.7
Harvey	Orange	TX	2017	8	*	
Harvey	Walker	TX	2017	8	*	
Hugo	Williamsburg	SC	1989	9	94	94.4
Harvey	Refugio	TX	2017	8	128	99.7

Source: FEMA, SHELDUS, and Anderson, et al. (2020).

Note: County-Month observations are sorted in descending order based on damages per capita according to SHELDUS. Wind speed values above 96 knots, which is the NOAA wind threshold for defining a “major” hurricane, are shown in bold text. NOAA defines storms with wind speed between 64 and 95 knots as “minor” hurricanes and storms with wind speed between 34 and 63 knots as tropical storms. Asterisks denote wind speed values below 50 knots.

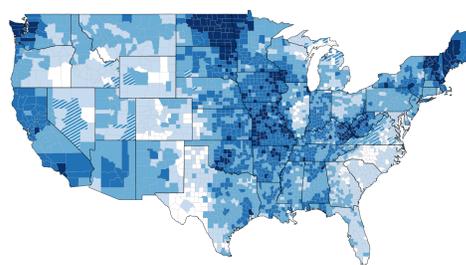
Figure A1: Distribution of Disaster Declarations

Per Capita Damages at or Above 99th Percentile



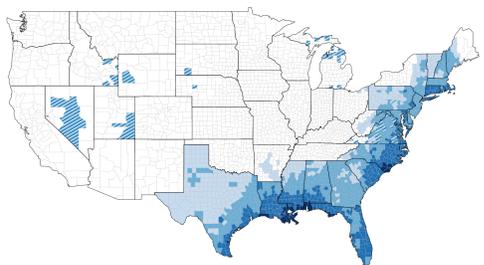
Count
0
1
2
3-4
NA

Floods (61%)



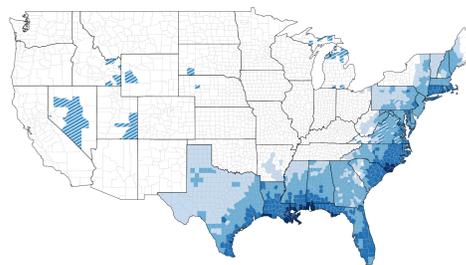
Count
0
1-2
3-5
6-9
10-19
NA

Hurricanes (14%)



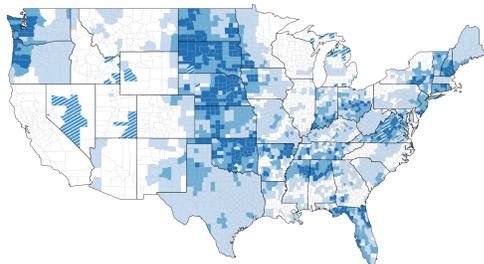
Count
0
1
2-3
4-7
8-10
NA

Hurricanes with Associated SHELDS Damages (14%)



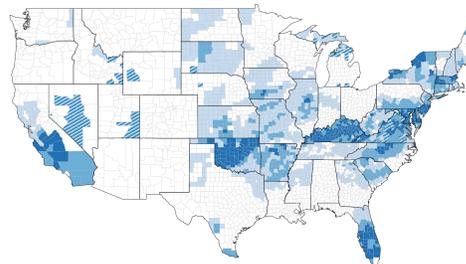
Count
0
1
2-3
4-7
8-10
NA

Severe Storms (15%)



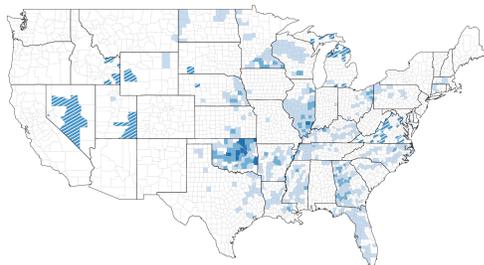
Count
0
1
2
3-6
NA

Severe Winter Weather (11%)



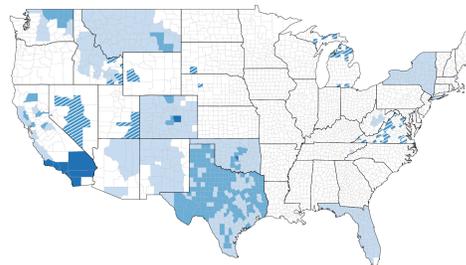
Count
0
1
2
3-5
NA

Tornadoes (3%)



Count
0
1
2
3-4
NA

Fires (4%)

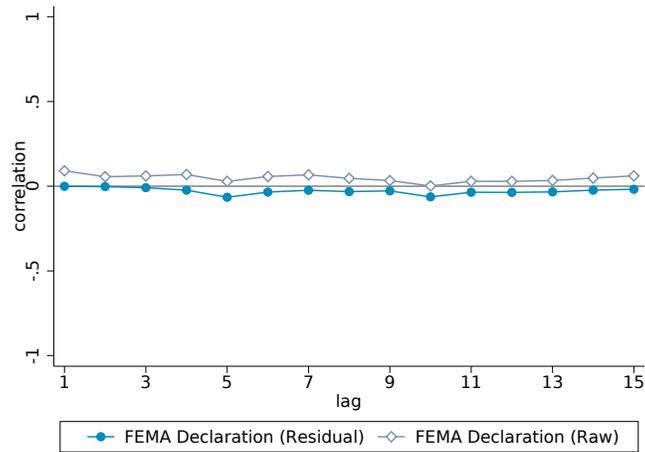


Count
0
1
2
3-8
NA

Source: FEMA, SHELDS.

Note: The Per Capita Damages at or Above 99th Percentile map shows the number of months a county's disasters had SHELDS per capita damages in the 99th percentile of those with FEMA disaster declarations from 1980 to 2017. The remaining maps show the counts of months in which the disaster type was declared in a given county with some hierarchical ordering. If a county experienced flooding due to a hurricane, that will show up only on the hurricane map. If a county receives two separate disaster declarations in a month, one for a hurricane and one for a flood not caused by the hurricane, this will also only show up on the hurricane map.

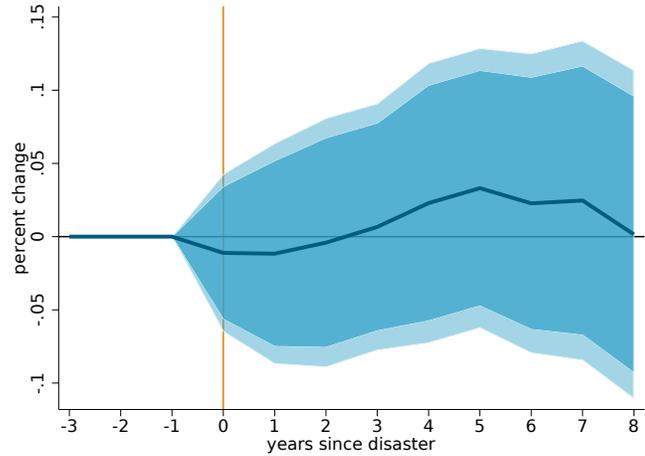
Figure A2: Auto-correlation of FEMA disaster declarations



Source: FEMA, SHELDUS.

Note: This figure shows the correlation between contemporaneous and a series of lagged disaster treatments based on FEMA declarations for the baseline regression sample for personal income (where observations are annual). The autocorrelations are shown for both the raw data as well as residuals of declarations after regressing declarations on the fixed effects included in the baseline regression.

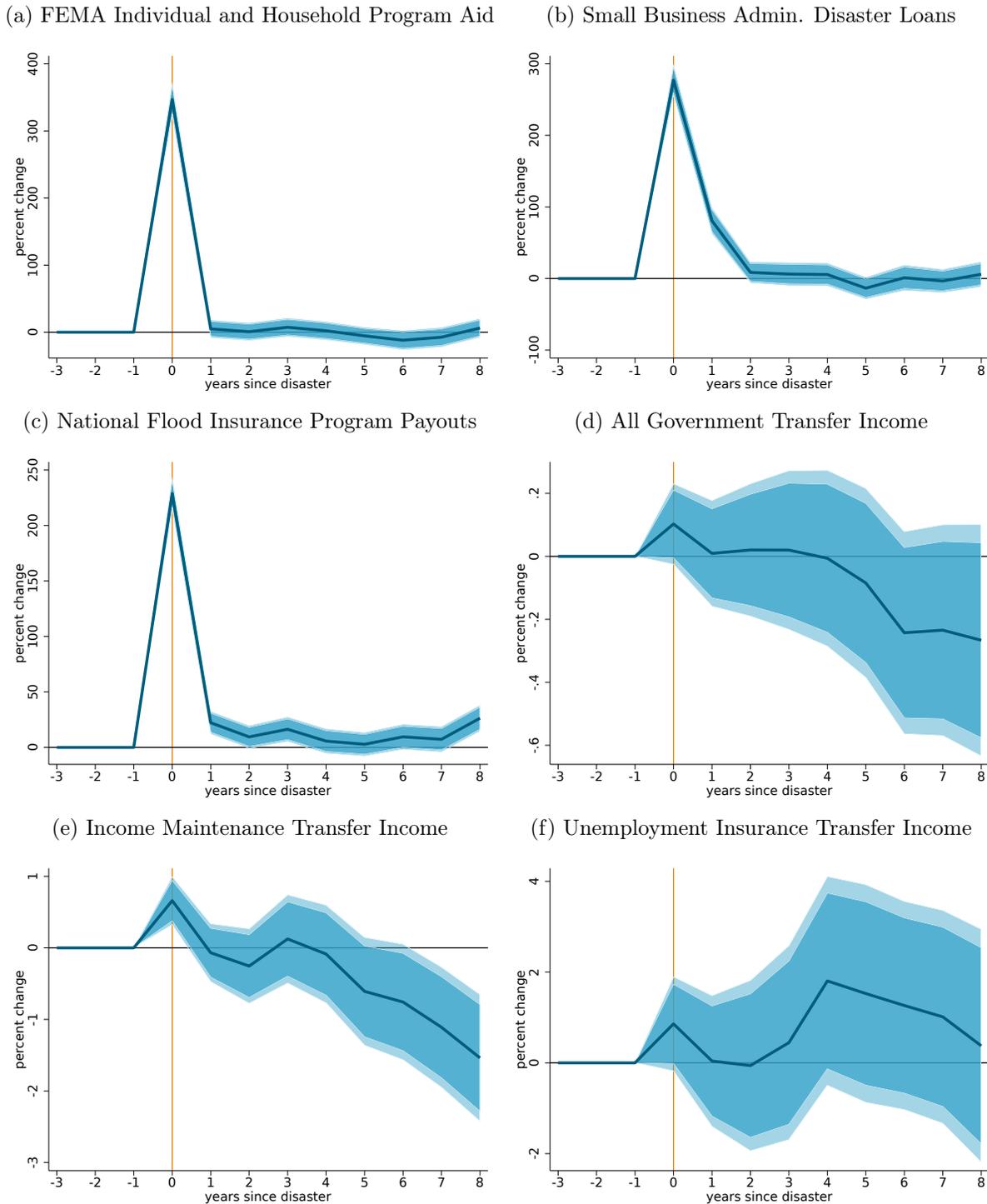
Figure A3: Impulse Response of Poverty Rate to Disaster Shocks



Source: FEMA, SHELDUS, and the Census Bureau’s Small Area Income and Poverty Estimates (SAIPE) program.

Note: Figure shows the impulse response function from estimating equation (1) where the dependent variable is the poverty rate (measured in percentage points). The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels.

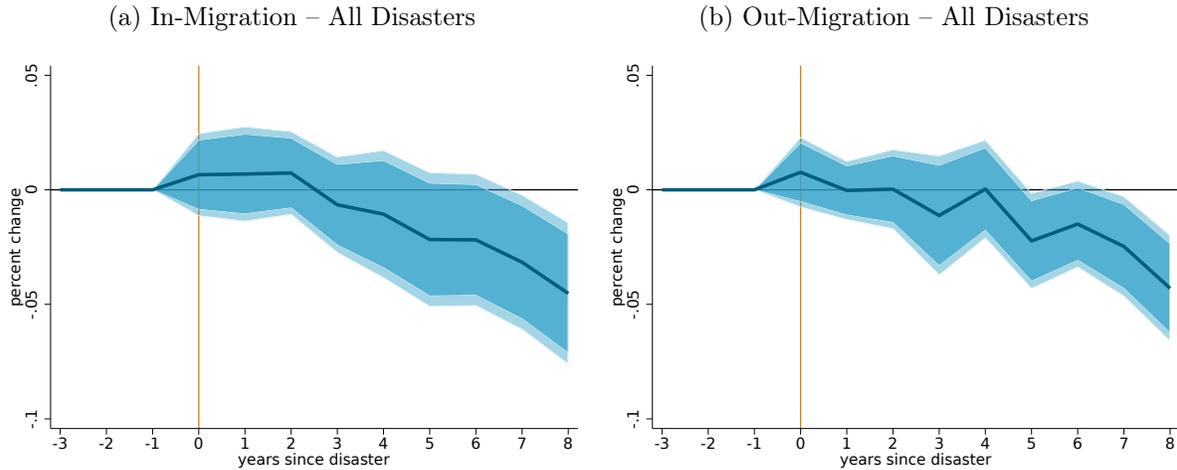
Figure A4: Impulse Responses of Government Transfers and Insurance Payouts to Disaster Shocks



Source: Federal Emergency Management Agency (FEMA), SHELDUS, Small Business Administration (SBA), FEMA National Flood Insurance Program (NFIP) Redacted Claims data, Census, and BEA.

Note: These plots show the impulse response functions for alternative types of government transfers and insurance payouts from estimating equation (1), where the shaded regions indicate the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. Outcomes variables are observed at the county level, measured in real (2017\$) per capita units, and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

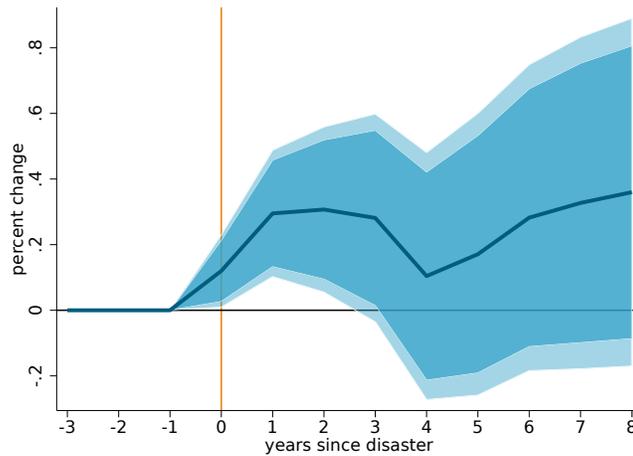
Figure A5: Impulse Responses of Migration to Disaster Shocks



Source: FEMA and Census.

Note: Plots show the impulse response functions (IRFs) from estimating equation (1), where the shaded regions indicate the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. All variables are observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

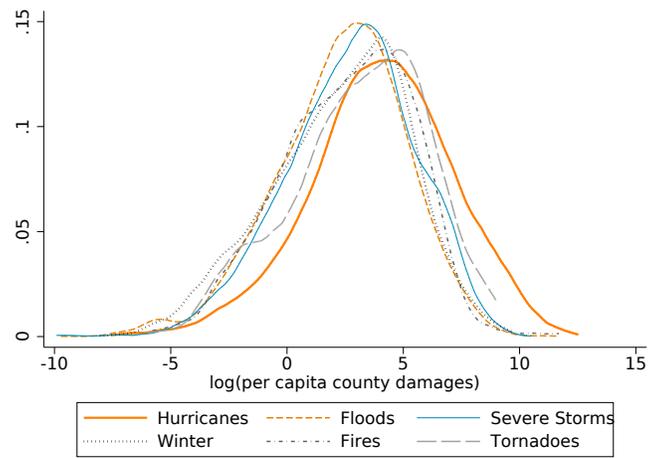
Figure A6: Wage & Salary Income (per capita)



Source: FEMA, SHELDUS, Census, and BEA.

Note: This plots show the impulse response functions from estimating equation (1), where the inner shaded regions indicate the 90 percent confidence intervals and the lighter outer shaded regions indicate the 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. Wage & salary income is observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1).

Figure A7: Distribution of Per Capita County Damages by Disaster Type

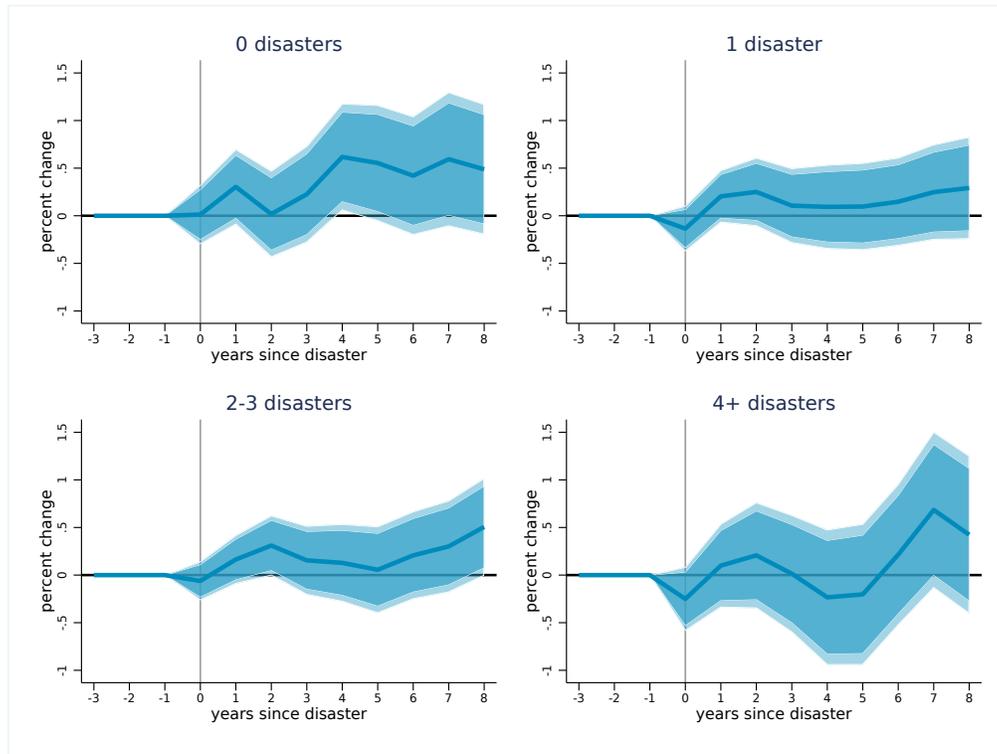


Source: FEMA, SHELDUS.

Note: The y-axis shows density and not frequency.

Figure A8: Intensification and Personal Income Per Capita

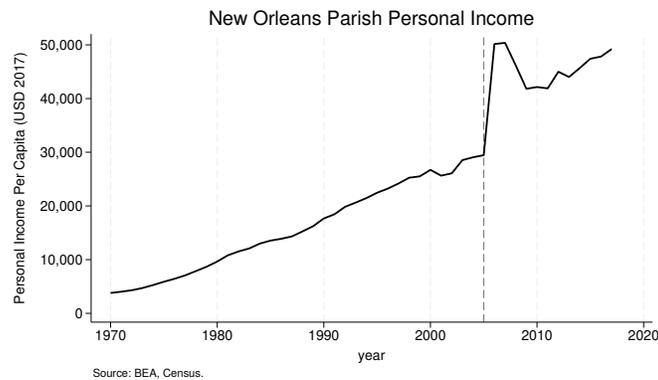
(a) Heterogeneous Impulse Responses By Local Historical Disaster Exposure



Source: FEMA, SHELDUS, BEA, and Census.

Note: This figure shows the impulse response functions (IRFs) from estimating equation (7), where the shaded regions show the 90 and 95 percent confidence intervals. Standard errors are clustered at the county and at the state-by-time levels. The four categories of historical disaster experience (0, 1, 2-3, and 4+) represent the number of years within years -10 to -1 in which a county experienced a disaster.

Figure A9: Personal Income Per Capita in New Orleans Before and After Hurricane Katrina

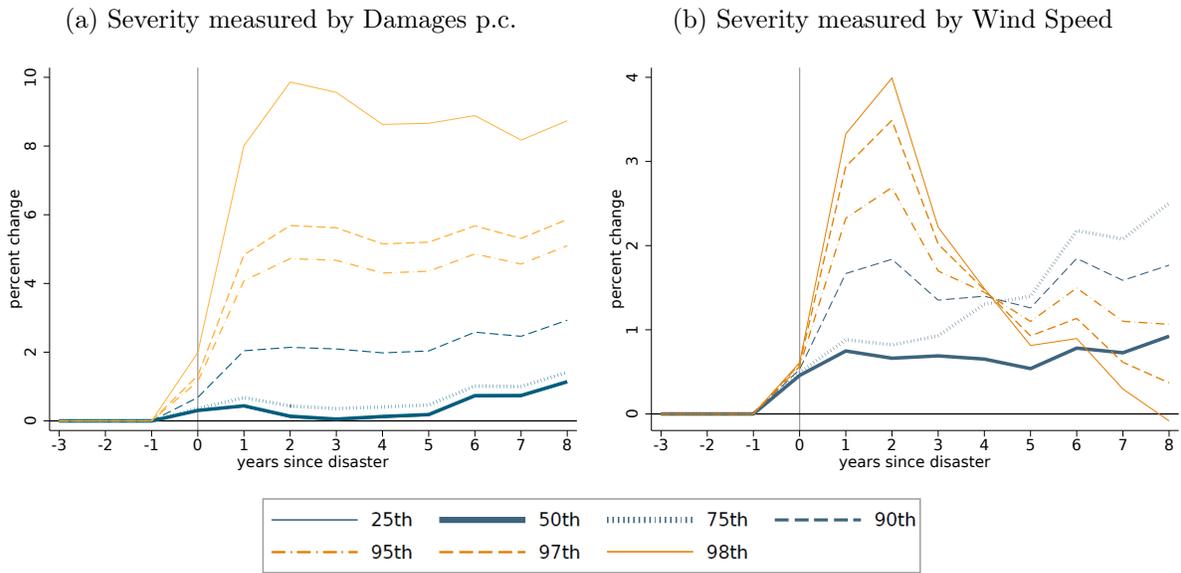


Source: BEA and Census.

Note: Vertical red line indicates 2005, the year of Hurricane Katrina.

Figure A10: Heterogeneous Impulse Responses of Personal Income Per Capita to Hurricane Disasters By Severity

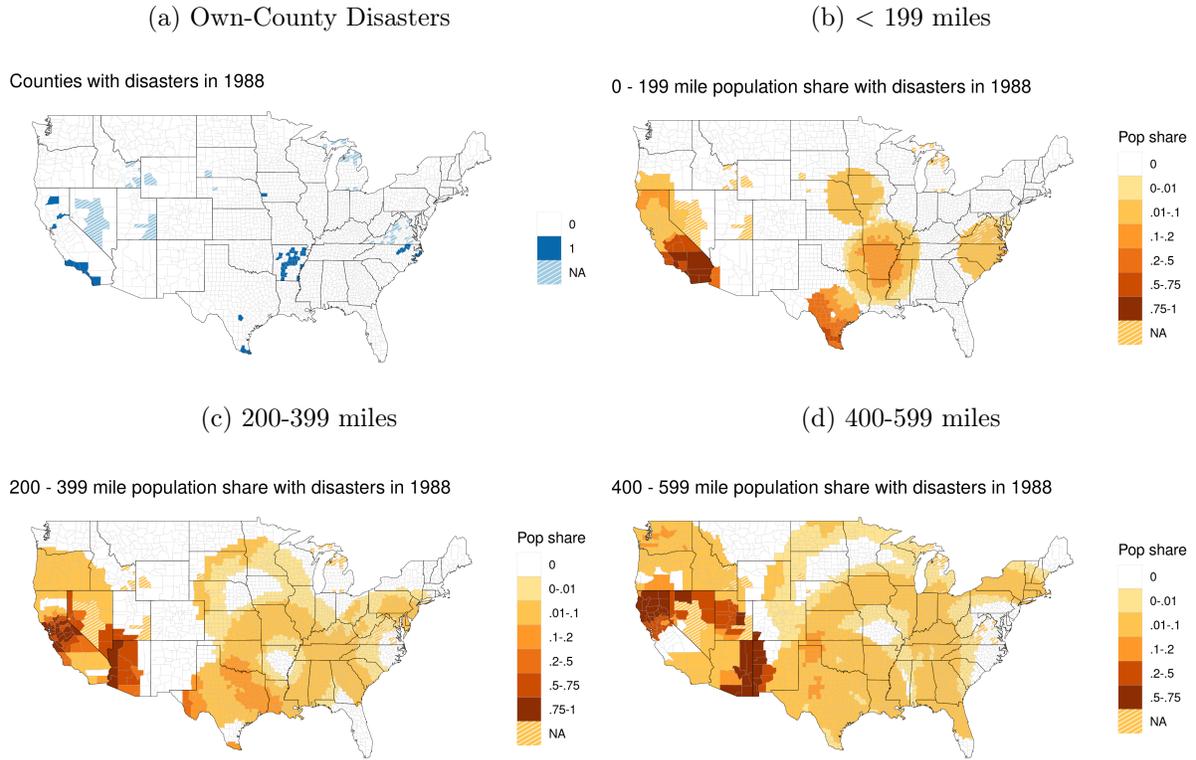
Severity Alternately Measured by Damages or by Wind Speed



Source: FEMA, SHELDUS, Census, BEA, Anderson et al (2020).

Note: This figure shows the impulse response functions from estimating equation (4) where the treatment variable is a hurricane dummy variable and an indicator variable for non-hurricane disasters is added to the set of controls. Severity (s) is measured by damages p.c. in panel (a) and by wind speed in panel (b). Percentiles are from distribution over all hurricanes in the sample with positive damages. See text for further details.

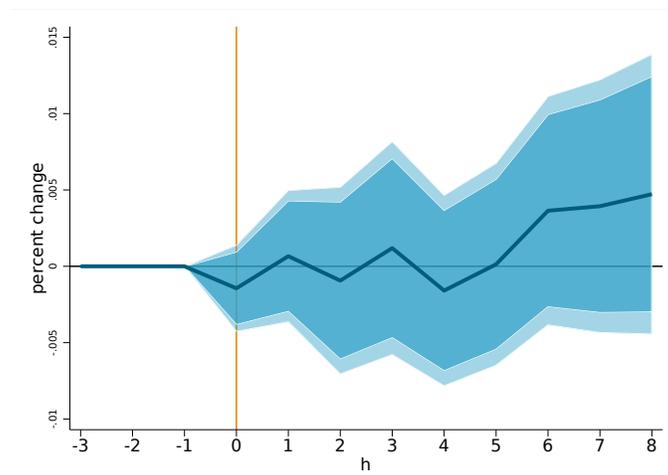
Figure A11: Spatial Lags in 1988



Source: FEMA, SHELDUS.

Note: Using 1988 as an example, panel (a) depicts the counties that received major disaster declarations from FEMA with positive damages in SHELDUS. Panels (b)-(c) depict the share of population within each band (50-199, 200-399, and 400-599 miles) of a given county that had disaster declarations with damages. Darker shading in panels (b)-(d) indicate a higher population share.

Figure A12: State-Level Impulse Response of Personal Income (p.c.) to Disaster Shocks

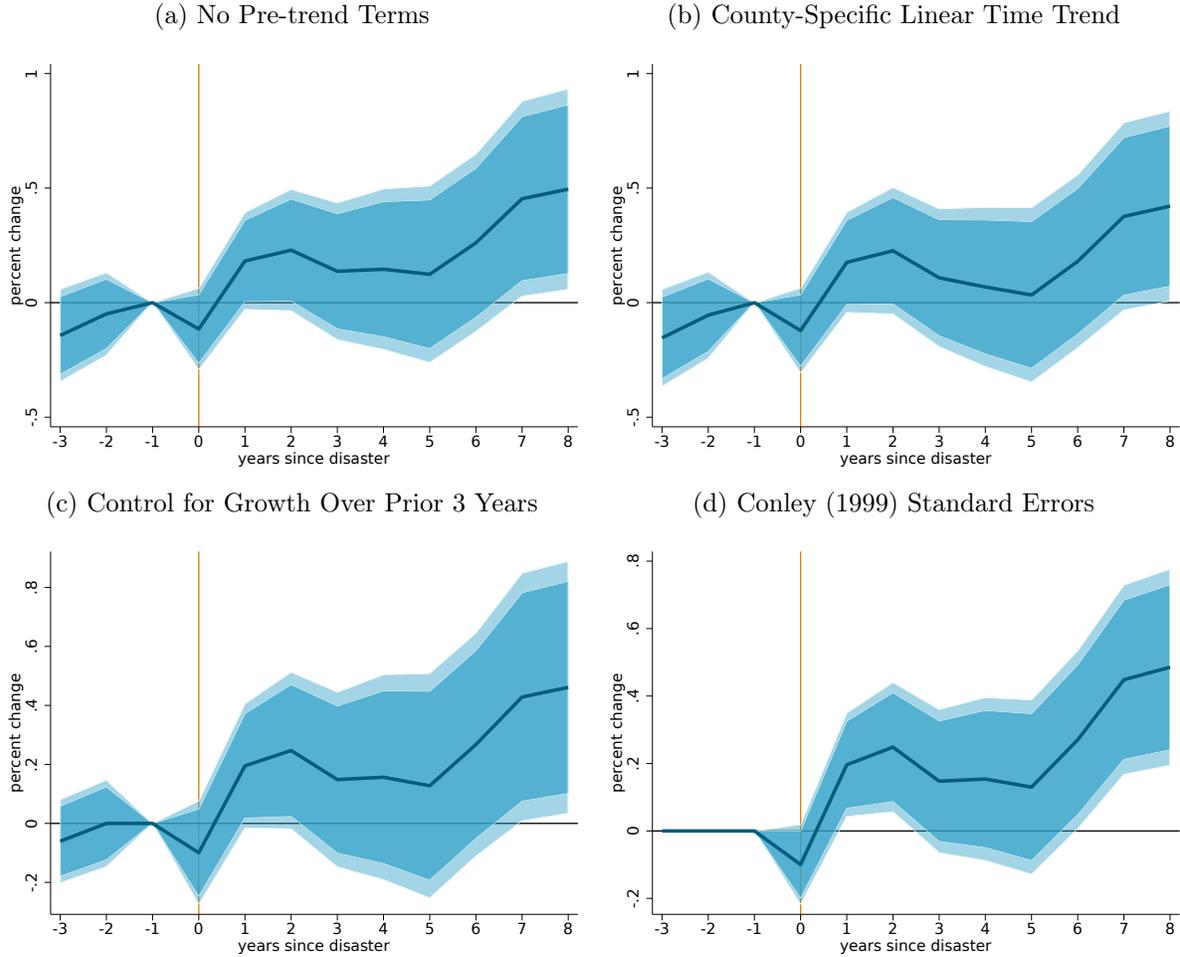


Source: FEMA, SHELDUS, BLS, Census, BEA, and CoreLogic.

Note: This figure shows the impulse response function from estimating equation (1) on state-aggregated data, where the treatment is the share of the state's population living in counties hit by disasters in a given year. The shaded regions indicate the 90 and 95 percent confidence intervals. Personal income is aggregated at the state level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1). Standard errors allow for clustering by state. The regressions include Census division-by-time fixed effects and state fixed effects.

Figure A13: Alternative Specifications

Impulse Response of Personal Income (p.c.) to Disaster Shocks

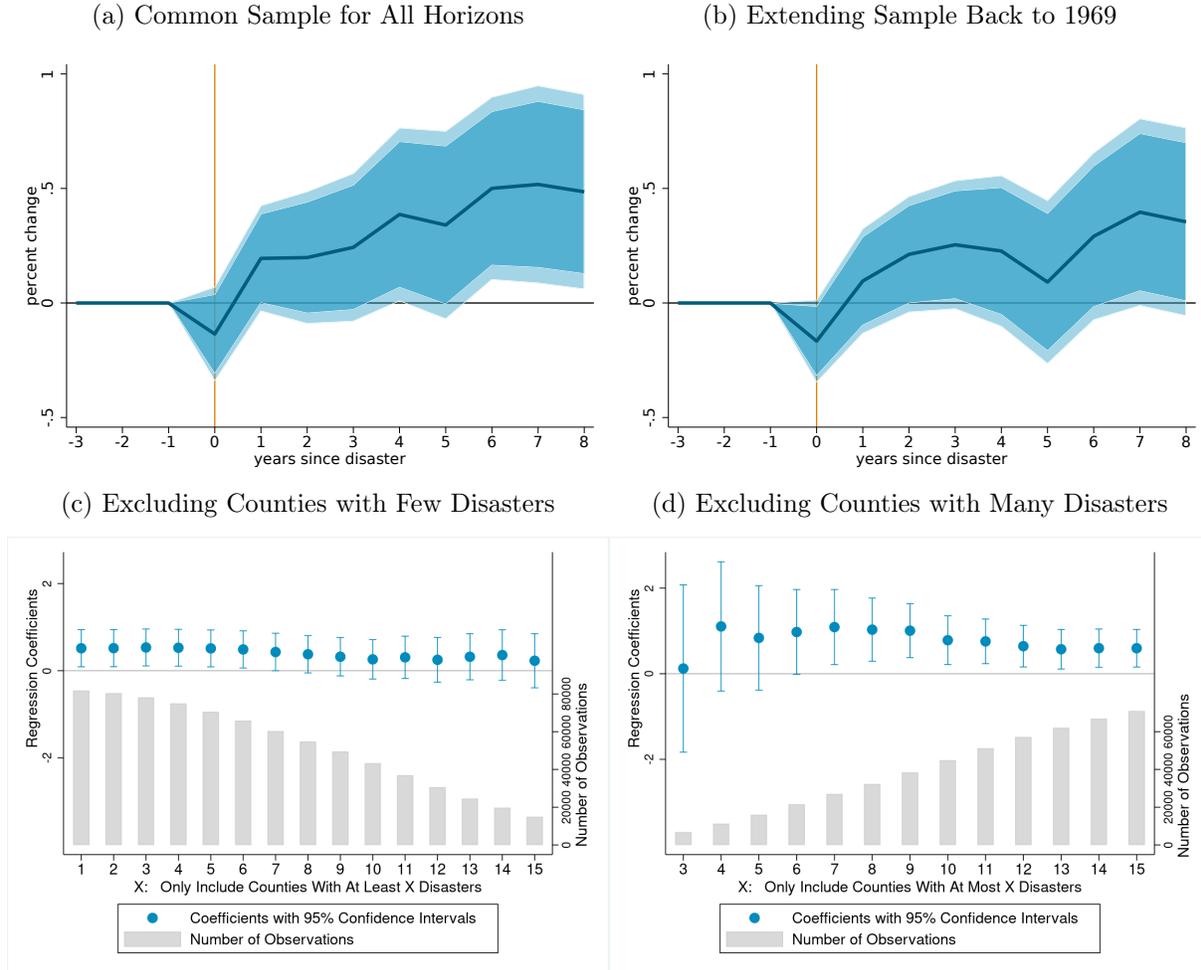


Source: FEMA, Census, and BEA.

Note: These plots show the impulse response functions from estimating equation (1) with alternative specifications. In panels (a)-(c), the equation (2) pre-trend terms (lags of p.c. growth covering prior 3 years) have been replaced with either no trend (a), the pre-trend in growth over prior three years (b), or a county-specific time trend (c). Panel (d) applies Conley (1999) standard errors that allow for spatial autocorrelation with a distance threshold of 200 kilometers and serial correlation (Newey-West) up to 5 years. All variables are observed at the county level and modeled as differences in logs between the indicated horizon (h) and the period before the disaster (-1). For all panels, the inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals.

Figure A14: Robustness to Varying Samples

Impulse Response of Personal Income (p.c.) to Disaster Shocks



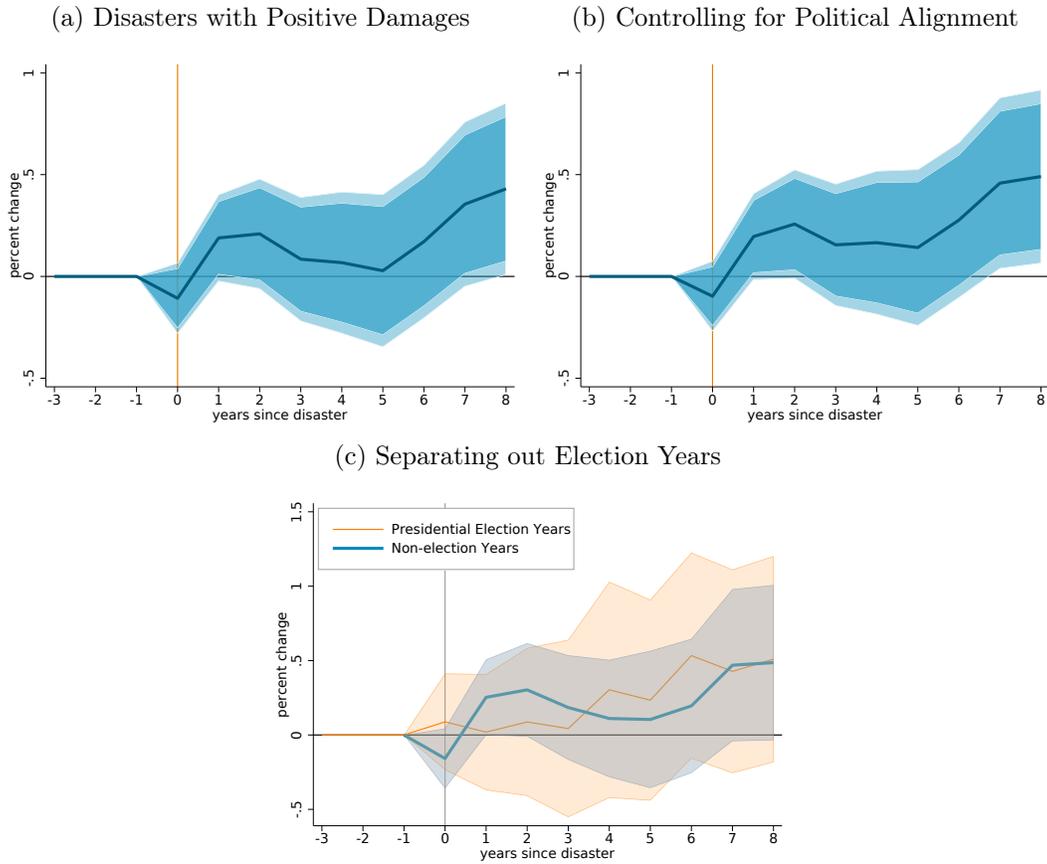
Source: FEMA, Census, BEA.

Note: This plot shows the impulse response functions from estimating equation (1). For panel (a), the sample is restricted to be the same for all horizons, which is the same as the sample for the longest horizon in the baseline IRFs (Figure 3). The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals. Standard errors for panel (a) are same as in the baseline (Figure 3). Panel (b) extends the sample back to 1969. Panels (c) and (d), the dots mark the point estimates and the lines indicate the 95 percent confidence intervals. The bars below each point estimate indicate the sample size. The results are based on estimating equation (1) for a horizon of eight years after a disaster. In panel (c), the samples drop counties that have fewer than the listed number of years with disasters. In panel (d), the samples drop counties that have more than the listed number of years with disasters.

In all cases, standard errors are clustered at the county and state-by-time levels. Personal income per capita is observed at the county level and modeled as difference in logs between eight years after the disaster and the year before the disaster (-1).

Figure A15: Robustness of Treatment Measure

Impulse Response of Personal Income (p.c.) to Disaster Shocks



Source: FEMA, Census, BEA, SHELDUS, Schneider and Kunze (2023).

Note: This plot shows the impulse response functions from estimating equation (1). For panel (a), the disaster indicator equals 1 only if SHELDUS disaster damage shows positive damages at the same time as a FEMA disaster declaration, controlling for FEMA disaster declarations without damages in SHELDUS. Panel (b) controls for the Schneider and Kunze (2023) “same party” variable, which equals 1 when the state governor is of the same party as the United States president. Panel (c) separately estimates responses based on whether or not disasters occurred in presidential election years (per the Schneider and Kunze (2023) replication data).

Online Appendix B – Not For Publication

Additional Details on Data Construction

B1. Poverty Data

The county-level poverty rate data come from the Census Bureau’s Small Area Income and Poverty Estimates (SAIPE) program and cover the years 1989, 1993, 1995, and 1997-2020. We fill in the missing years for each county via linear interpolation between adjacent years. We expect the resulting measurement error to be systematically unrelated to disaster occurrence and hence expect it to inflate standard errors but not to introduce bias.¹

B2. Data for Industry Mix-Implied Wages

We construct a measure of industry mix in order to assess how disasters impact the industry composition of a county’s workforce. Specifically, we construct a variable measuring each county’s expected average wage in a given year based only on its industry composition:

$$w_{ct}^{pred} = \sum_j s_{cjt} w_{jt} ; \quad \sum_j s_{cjt} = 1$$

where s_{cjt} is the employment in industry j in county c in year t and w_{jt} is the national mean hourly wage in industry j in year t .

Data on s_{cjt} come from the Census Bureau’s County Business Patterns (CBP) data. We use Eckert, et al. (2021)’s version of the CBP data, which exploits various adding-up constraints in the raw data to fill in missing values. It imputes some missing values in the raw CBP data by exploiting cross-county (within industry) and cross-industry (within county) totals and adding up constraints. To further minimize missing values, we use the “major sector” NAICS industry level rather than a finer level of industry categorization.

We calculate w_{jt} as the mean hourly wage by NAICS major sector across individuals using the CEPR yearly extracts of the CPS Outgoing Rotation Group micro-data.²

B3. Government Transfer and Loan Data

We use data on SBA disaster loans for fiscal years 2001 through 2017 from the SBA website. Data for years from 1989 through 2000 came from Bondonio and Greenbaum (2018) and were

¹ Using only 1997-2020 results in too short a sample to estimate the full dynamic pattern from $h = -3$ to 8 with reasonable precision.

² Using the median wage yields qualitatively similar results to the mean.

generously provided by Robert Greenbaum. The data provide dollar amounts of disbursements of SBA disaster loans, separately for households and for businesses, by county and fiscal year. We use data on county level IHP payments going back to 1990 that we obtained from FEMA via FOIA request.

We use the Federal Insurance & Mitigation Administration National Flood Insurance Program (FIMA NFIP) Redacted Claims Dataset (available at <https://www.fema.gov/media-library/assets/documents/180374>) to calculate NFIP payments associated with floods occurring each month in each county. Although we are able to observe the date of the incident to associate the payment amounts with our disaster observations, we are unable to observe when the payments are actually made.

B4. Hurricane Wind Speed Data

For hurricane wind speed data, we use county level data made available via Anderson et al (2020a) and Anderson et al (2020b) using the U.S. National Hurricane Center's Best Track Atlantic hurricane database (HURDAT2.) We use versions 0.1.1 and 0.1.0 of R packages `hurricaneexposure` and `hurricaneexposuredata`, respectively. Our wind speed measure is the highest maximum sustained wind speed that has a duration of at least 10 minutes. We only include the observed wind speed for a county and event if the maximum gust is at least 64 knots, the maximum sustained wind is at least 50 knots, the daily precipitation is at least 50 mm (about 2 inches), or the total precipitation over the five-day period is at least 200 mm (about 8 inches).

B5. Migration Data

We use IRS Statistics on Income (SOI) data on gross migration flows into and out of each county from 1995 to 2017.³ In-migration to a county in a given year is defined as the number of taxpayers (roughly equivalent to households) filing taxes in that county in that year but that filed in a different county in the prior year. Out-migration from a county in a given year is defined as the number of taxpayers (roughly equivalent to households) that filed taxes in that county in the prior year but that filed taxes in a different county in the current year. The

³ <https://www.irs.gov/statistics/soi-tax-stats-migration-data>

IRS does not disclose migration flows of less than 10. In those cases, we treat the flow as a zero. These zeros are relatively rare, occurring for 0.6% of observations for in-flows and 0.5% for out-flows.

References

Anderson B, Yan M, Ferreri J, Crosson W, Al-Hamdan M, Schumacher A and Eddelbuettel D (2020). *hurricaneexposure: Explore and Map County-Level Hurricane Exposure in the United States*. R package version 0.1.1, <URL: <http://CRAN.R-project.org/package=hurricaneexposure>>.

Anderson B, Schumacher A, Crosson W, Al-Hamdan M, Yan M, Ferreri J, Chen Z, Quiring S and Guikema S (2020). *hurricaneexposuredata: Data Characterizing Exposure to Hurricanes in United States Counties*. R package version 0.1.0, <URL: <https://github.com/geanders/hurricaneexposuredata>>.

Bondonio, Daniele, and Robert T. Greenbaum (2018). “Natural Disasters and Relief Assistance: Empirical Evidence on the Resilience of U.S. Counties using Dynamic Propensity Score Matching”

Online Appendix C – Not For Publication

Details on Robustness Checks

In this appendix, we assess the robustness of our personal income results to alternative specifications, sample restrictions, and disaster measurements.

C1. Alternative Specifications

We consider alternative approaches to modeling the counterfactual county-specific time trends in absence of a disaster treatment. As shown in equation (2) in the paper, our baseline specification controls for pre-trends in the outcome variable over the prior three years, implicitly assuming that, conditional on the other controls, this trend would have continued in the absence of a disaster. Our first alternative specification drops these pre-trend variables. The impulse response function (IRF) for income per capita (p.c.) based on this specification, shown in **Figure A13**, panel (a) is nearly identical to the baseline IRF in panel (a) of **Figure 3** in the paper. Next, we replace the pre-trend variables with a county-specific linear time trend (i.e., the time variable interacted with county fixed effects). We do not use this specification as our baseline because of the *a priori* concern that the estimated full-sample time trend could absorb the treatment effect itself. That is, the time trend is endogenous with respect to the disaster treatment because a disaster may well alter a county's income p.c. trend (Goodman-Bacon, 2021). Shown in panel (b), the results of this specification are very similar to our baseline specification. Lastly, rather than controlling just for three lags in income p.c. as we do in the baseline, panel (c) shows the results where we control for the cumulative three-year change in the outcome. The resulting IRF is nearly identical to the baseline.

Another concern could be whether our baseline specification adequately handles potential spatial spillovers in the error term. Our baseline specification controls for such correlations by clustering the errors by state-time as well as by county, which allows county-level errors to be arbitrarily correlated within state. In **Figure A13** panel (d), we show an IRF with confidence intervals based on Conley standard errors (see Conley (1999)). Compared to these results, the confidence intervals in our baseline specification are relatively conservative.

C2. Sample Restrictions

We now examine three sample issues. The first relates to how our panel local projection (LP) methodology involves a separate regression and slightly different sample for each

horizon of interest. Specifically, the effective sample period for the treatment variable decreases as horizon h in equation (1) increases. For instance, in our baseline 1980-2017 sample period, the set of disasters available to estimate the regression goes through 2017 for $h = 0$ and through 2009 for $h = 8$. Our baseline estimates use the maximum sample available for each horizon to provide the most efficient estimate of the treatment effect at each horizon. However, if the treatment effect for a given horizon has changed over time, the estimated IRF could be sensitive to these changes in sample period. To assess this concern, we estimate the income p.c. IRF using a common sample for all horizons, that used for the $h = 8$ regression which covers disasters from 1983-2009.⁴ The results, shown in **Figure A14**, panel (a), yield a slightly more positive income p.c. effect in the medium term, though the confidence intervals are slightly larger. These results could suggest that some disasters near the end of our sample period (included in the full sample but not the common sample) had less positive medium-run effects compared with disasters earlier in the sample period. The 8-year post-disaster estimates are identical by construction.

We next examine our choice of baseline sample period. A key contribution of our paper is to provide estimates of disasters' effects on income p.c. and key components underlying income p.c. (see the subsection below) using the same empirical framework for the same sample period. Hence, because data for most of the other outcomes start in 1980, our baseline analysis for income p.c. also uses the sample starting in 1980. However, as data on income p.c. are available back to 1969, we assess whether our baseline results are robust to this longer sample period. **Figure A14** panel (b) shows the estimated IRF using this longer sample is very similar to the baseline IRF, though the statistical significance is more marginal.

We next explore the sensitivity of our results to outlier counties that experience disasters either very rarely or very frequently. One *a priori* concern with our analysis based on using *all* U.S. counties is that counties that never or rarely experience disasters may not represent a valid control group for counties that are treated (i.e., experience a disaster) more often. To assess this concern, we estimate the disaster effect on income p.c. repeatedly, first dropping counties that have never experienced a disaster (during 1980-2017)—that is, only including counties with at least one disaster—then only including counties that have experienced at least two disasters, and so on up to 15 disasters. In panel (c) of **Figure A14**, we plot the estimated longer-run disaster

⁴ In both cases, the set of disasters starts in 1983 due to the presence of 3 lags of the disaster and outcome variables.

impact on income p.c., $\hat{\beta}^8$ (dots), as well as the sample size (bars), for each of these restricted samples. The coefficient is generally stable across the samples. As we increasingly limit the sample, the confidence interval widens such that the estimated longer-run impact becomes statistically insignificant for samples excluding counties with fewer than 10 disasters. This is likely explained by the notable shrinking of the sample size.

A similar concern is that our estimates are biased due to outsize influence of counties that experience disasters very frequently, which may be inherently different in other unobserved ways. In panel (d) of **Figure A14**, we show a plot similar to that in panel (c), except here we sequentially drop counties that experience more than a given number of disasters. The coefficient point estimates are again generally stable. Only when we limit the sample to the set of counties that experienced at most three disasters during our sample period do we see the point estimate drop. However, these estimates are very noisy as indicated by the wide confidence intervals and are based on a very small sample of counties that are likely to be unrepresentative of the typical county.⁵

C3. Disaster Measurement

While for a given location the timing of disasters to which FEMA declarations respond is exogenous, prior work has shown that some FEMA declarations may be politically motivated and that disaster severity may be endogenous. As such, omitted variables may bias our estimates. The inclusion of outcome pre-trends and county fixed effects in our baseline estimation should control for omitted variables that could influence county-level economic outcomes and the likelihood of receiving FEMA disaster declarations conditional on a given physical event. However, we now explicitly test for three potential sources of omitted variable bias.

First, there may be disasters that led to FEMA declarations despite causing negligible damages. In a few instances, FEMA declarations cover all counties in an affected state even when only a portion of counties were physically affected. As a robustness check, we consider a county to be treated only if there are positive reported damages in the Spatial Hazard Events and Losses Database for the United States (SHELDUS) contemporaneous with the

⁵Results are not shown for samples including only counties with at most 1 or 2 disasters because these samples are extremely small.

FEMA declaration. SHELDUS is based on the NOAA Storm Database, which is in turn based on reports from insurance companies, media, and other sources. We control for the remaining roughly 6% of FEMA-declared disasters with zero damages reported in SHELDUS. The results, shown in panel (a) of **Figure A15**, are similar to the baseline results.

We next consider the concern that political favor (by the President or Congress) can make some disaster-hit counties more likely to receive both FEMA declarations *and* other benefits that improve economic outcomes. This could lead to a positive omitted variable bias for outcomes like income p.c. because political favor and associated benefits are unobserved but correlated with the probability of receiving a FEMA declaration if hit by a disaster. This concern applies to relatively minor, marginal disasters, which Schneider and Kunze (2023) show are potentially prone to political influence.⁶

As we show in Section VII, we find that the positive longer-run average effect of disasters is stronger for severe disasters than minor disasters, suggesting that our baseline positive longer-run effects are unlikely to be driven by political endogeneity bias. Nonetheless, we perform two analyses here using data from Schneider and Kunze (2023) to directly explore this concern. We first re-estimate the IRF for income p.c. explicitly controlling for political favor as measured by Schneider & Kunze’s same-party variable, which equals 1 if the state governor is of the same party as the president. As shown in **Figure A15**, panel (b), inclusion of this variable has little effect on the income p.c. IRF. Second, in panel (c) we show separate IRF estimates for presidential election years and non-presidential election years (another variable from Schneider and Kunze (2023)). The non-election year IRF is remarkably similar to our baseline result, suggesting that presidential elections are not driving our main finding.⁷ These two results suggest our main findings are not driven by political motives influencing FEMA declarations.

Lastly, Felbermayr & Gröschl (2014) point out that many existing global disaster data sets are based on insurance records, which reflect (insured) monetary damages rather than meteorological and geophysical data. Because damages are correlated with GDP, the concern

⁶ Schneider and Kunze (2023) show that, at least for hurricanes, more severe disasters are obvious and clear to the public and FEMA and are hence consistently declared disasters while very minor county exposures to hurricanes almost never result in FEMA declarations.

⁷ The presidential election year IRF has wider confidence intervals—which is to be expected given that it has one-third as many observations as non-election years—that overlap with those for non-presidential years and yields nearly identical point estimates for the longer run response.

arises that a given event is more likely to be classified as a disaster in a high-income country than in a low-income country. A similar concern could apply to FEMA declarations across U.S. counties, in that an event of given meteorological or geophysical severity could be more likely to result in a FEMA declaration in a higher income county. The county fixed effects in our specification should absorb average level and growth of income p.c. over the sample period while the pre-trend controls should capture any increased likelihood of FEMA declarations for counties that had been growing faster prior to the event.

References

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- Goodman-Bacon, Andrew. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225, no. 2 (2021): 254-277.
- Schneider, Stephan A., and Sven Kunze. "Disastrous discretion: political bias in relief allocation varies substantially with disaster severity." *Review of Economics and Statistics* (2023): 1-33.

Online Appendix D – Not For Publication

Comparison to Other Recent Studies of Local Disaster Effects in the U.S.

In the introduction to the main manuscript, we discussed findings from prior studies on the longer-run economic effects of natural disasters, which range from strongly negative to strongly positive. We mentioned two studies in particular: Jerch, Kahn, and Lin (2023)—which we will hereafter refer to as JKL—and Deryugina (2017). These studies also use county-level variation in the U.S. to estimate disaster effects—though they look only at hurricanes—and find generally more negative economic effects than we find for disasters as a whole. In this appendix, we identify where our results diverge from theirs and delve into the sources of the key differences. We also compare our short-run results to those of Strobl (2011).

Focusing on how hurricanes affect local fiscal revenues, expenditures, and borrowing, JKL also report results on the longer-run (6 to 10 years out) effects of hurricanes on employment, population, and home prices.⁸ Their findings that minor hurricanes have statistically insignificant negative effects on population and employment and insignificant positive effects on home prices in the longer run are broadly consistent with our average disaster results (**Figure 3**). They find that major hurricanes have significant negative longer-run effects on all three of these outcomes. We similarly find that the longer-run response of population to the most severe disasters is negative, though not statistically significant (**Figure 7** and **Table A7**). We find a near-zero longer-run effect of the most severe disasters on home prices. However, this result is difficult to relate to the hurricane results from JKL because our home price data -- which goes back to 1980 but covers only large counties -- is very different than the Zillow house price index they use, which covers all counties but has a shorter sample period.

Their finding that major hurricanes have negative and significant longer-run employment effects contrasts with our results—based on all disasters—in **Figure 7**, panel (b), which shows that the estimated longer-run employment effect becomes increasingly positive (though not statistically significant) with disaster severity. To investigate this difference in findings, we apply our standard LP approach to estimate the long-run effect of hurricanes by severity, measured alternately by damages and by wind speed, as in **Figure A10**, but using employment as the

⁸ We compare our results to those in JKL’s Table 5, panel B (columns 2-4), which shows the estimated effects of outcomes with respect to separate indicators for minor and major hurricanes.

outcome variable instead of personal income p.c. We use wind speed data from the same source (HURDAT-2) as JKL. We find that how one identifies severe hurricanes explains the difference between our results. In particular, county exposures to high monetary damages lead to positive and statistically significant long-run employment effects, while exposures to high wind speeds yield negative, albeit statistically insignificant, long-run employment effects, which is broadly consistent with JKL's findings.⁹ This underscores that measuring disaster severity/destructiveness using monetary damages as opposed to meteorological data like wind speed can yield different results. As discussed in more detail in Section VII.A.2, the most destructive county hurricane exposures based on monetary damages are often not those with the highest wind speeds. Other storm characteristics such as rainfall and storm surge, in addition to the value of local property at risk, often drive the economic destructiveness of a local area's exposure to a hurricane.

Deryugina (2017) focuses on the response of fiscal transfers to hurricanes, but also reports results for labor income p.c., the employment rate, and population (see her Tables 5 and 6.) Consistent with our results, she finds no significant effect of hurricanes on the employment rate or population after 7.5 years.¹⁰ However, she finds a negative effect (significant at the 10% level) on labor income p.c. after 7.5 years. In contrast, we find that all FEMA-declared disasters on average as well as specifically hurricanes (based on wind speed exposure or FEMA declarations) yield positive and significant (at the 5% level) effects on total income p.c. after 8 years (see **Figure 3**, **Figure 8**, and **Figure A10**).¹¹ Although we find that the longer-run wage and salary income response to all disasters is not statistically significant (see **Figure A6**), the response to hurricanes based on FEMA declarations is significantly positive as well. We delve here into understanding why Deryugina (2017) finds a negative impact on labor income when we find a positive one.

Dimensions along which the Deryugina (2017) analysis differs from ours that could potentially explain this divergence in results include treatment definition, sample, and

⁹ A similar exercise yields negative, though statistically insignificant, longer-run home price responses to the highest wind speeds when using our home price data.

¹⁰ We do not directly estimate the effects of disasters on the employment rate, but we find near-zero percentage effects on both employment and population, implying no effect on their ratio.

¹¹ We also find that hurricanes result in statistically significant longer-run increases in wage and salary income during our sample.

methodology. As in JKL, Deryugina (2017) uses an event-study research design and wind speed data to measure hurricane exposure. Unlike the comparison between our and JKL’s employment effects (for major hurricanes), differences in treatment definition cannot explain the difference between our findings, as we find similar results for income (both total and just the labor component) when we measure hurricane treatment using wind speed or FEMA declarations (see, for example, **Figure 4, panels (a) and (b)** and the discussion in **Section IV.B**). Similarly, sample period does not appear to explain our differences as when we run our baseline regression just for hurricanes between 1979 and 2002 (the period covered by Deryugina (2017)), we still find positive point estimates, though they are less significant (labor income is significant at the 10% level, total income is not significant.) Thus, we perform a systematic evaluation of differences in methodology.

We begin by replicating Deryugina’s results using her data, sample period, and preferred event-study specification (her equation (3)). We then examine methodological differences by using the same data and sample period to estimate an LP specification that is conceptually very similar to that event-study specification.¹² We again obtain a negative longer-run effect, though it is no longer statistically significant. Thus, using LP instead of the standard event study approach cannot explain the difference in results.¹³

Another methodological difference relates to how one handles repeat treatments. In an ideal research setting for studying dynamic treatment effects, a unit (or county) would be treated exactly once. However, in the U.S., there are many counties that are repeatedly exposed to hurricanes (see **Figure A1**). Deryugina (2017) tackles this challenge by identifying the effects of the *first* hurricane hitting a county within the event window, setting the treatment variable to zero for subsequent hurricanes. In contrast, our baseline specification considers all disasters as treatments, controlling for subsequent disasters by including leads and lags of the treatment variable. We now re-estimate the effect of hurricanes on labor income p.c. using both the data and event-study specification (which, like our LP specification, includes controls for leads of the treatment) from Deryugina (2017), but *not* setting subsequent treatments (hurricane indicator

¹² The LP specification we estimate (for $h = 8$) is:

$Y_{ct+h} - Y_{ct-1} = \beta_h \sum_{\tau=0}^h dH_{c,t+\tau} + \sum_{\tau=-3, \tau \neq 0}^h \delta_{\tau} dH_{c,t+\tau} + \alpha_{r(c),t}^h + \alpha_c^h + e_{ct}$, where $dH_{c,t}$ is an indicator for whether there was a hurricane in county c in year t and the other variables are as defined previously.

¹³ This is consistent with the formal equivalence of the panel LP approach and the panel event-study approach (under certain conditions) shown by Dube, et al. (2023).

values) to zero after the first one. Making this change flips the estimated effect to be positive and significant, indicating that the handling of subsequent hurricanes can explain the bulk of the difference in our results. This finding relates directly to the new two-way fixed effects literature that has emerged since Deryugina (2017). This literature has shown that in settings with staggered treatment effects, the dynamic average treatment effect can be biased if repeat treatments are not accounted for (see Dube et al. (2023)). This comparison exercise illustrates how the handling of subsequent disasters that occur between the initial event and the horizon of interest can affect results, especially for frequently recurring disasters like hurricanes.

Finally, although we have largely focused on *longer-run* effects in this paper, we now examine what can explain why we find a positive and significant *contemporaneous* effect of hurricanes on personal income p.c. (see **Figure 6**) while Strobl (2011) finds it to be negative and significant. To investigate this discrepancy, we first establish that we can reproduce the negative and significant contemporaneous effect found in Strobl (2011) by estimating his main specification (equation (4) in that paper) using his 1970-2005 sample period, the vintage of BEA county income p.c. data likely used in that paper (the vintage ending with 2005 data), and a wind speed-based hurricane indicator.¹⁴ We then find that by *either* switching the wind-based hurricane indicator to the FEMA-based hurricane indicator *or* by switching the vintage of BEA income p.c. data to the later vintage we use in this paper (i.e., the vintage covering our baseline sample period, 1969-2017), the sign of the effect flips to positive, though it is statistically insignificant. When we implement *both* changes—using the FEMA-based hurricane indicator and the later data vintage—the effect becomes not only positive but sizeable and statistically significant, consistent with our baseline result for the 1980-2017 period in **Figure 6**. We conclude that the seeming discrepancy between the Strobl (2011) results and ours is due to a combination of using different data vintages and different hurricane measures.

¹⁴ Strobl’s specification is equivalent to ours (equation (1) above) when $h = 0$ except that we include more than one lag of income p.c. and the hurricane treatment variable. The replication materials for Strobl (2011) do not provide county identifiers like FIPS codes, meaning that we cannot merge our FEMA indicators with the data used in his paper. For this reason, we cannot use the exact same set of 409 counties—those close to the East or Gulf coasts—that he used. Instead, we use the sample of counties in the states along those coasts. For the purposes of these comparisons, the “early vintage” we use are the data published by BEA in April 2007, which are available along with other vintages on BEA’s website. We use the wind speed-based hurricane indicator from Deryugina (2017); the wind speed data we use in other analyses (e.g., **Figure 4**) are available only as of 1988.

References

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