The Local Economic Impact of Natural Disasters

Brigitte Roth Tran
Daniel J. Wilson
Federal Reserve Bank of San Francisco

June 2022

Working Paper 2020-34

https://www.frbsf.org/economic-research/publications/working-papers/2020/34/

Suggested citation:

The views in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Federal Reserve Bank of San Francisco or the Board of Governors of the Federal Reserve System.
The Local Economic Impact of Natural Disasters

Roth Tran, Brigitte and Wilson, Daniel J.

June 2022

Abstract
We use county data from 1980 to 2017 to study the dynamic responses of local economies after natural disasters in the U.S. Specifically, we estimate disaster impulse response functions for personal income per capita and a broad range of other economic outcomes, using a panel version of the local projections estimator. In contrast to some recent cross-country studies, we find that disasters increase total and per capita personal income over the longer run (as of 8 years out). The effect is driven initially largely by a temporary employment boost and in the longer run by an increase in average weekly wages. We then assess the heterogeneity of disaster impacts across several dimensions. We find that the longer-run increase in income per capita is largest for the most damaging disasters. Hurricanes and tornados yield longer run increases in income, while floods do not. The longer run increase in income, which on average has fallen over time, is larger for counties with recent disaster experience. Finally, state-level analyses and estimates of spatial spillovers suggest that, unlike the positive effect for counties directly affected by disasters, the longer run effect of disasters for broad regions as a whole may be negative.

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

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

---

1 We thank seminar participants of the Cornell University Applied Economics & Policy Seminar, Richmond Fed Climate Change Economics Workshop, IMF 21st Jacques Polak Annual Research Conference, UC San Diego Environmental Economics Seminar, Federal Reserve System Regional Conference, the Federal Reserve Board Applied Micro Lunch series, the 2020 AEA meetings, the 2020 IPF Annual Congress, the 2021 NBER Summer Institute Urban Economics Session, and the 2021 Stanford SITE Housing and Urban Economics session. We also thank Laura Bakkensen, Yuriy Gorodnichenko, and numerous colleagues at the Federal Reserve Board and Federal Reserve Bank of San Francisco for their helpful feedback. Nathan Ausubel, Olivia Lofton, Tessa Maddy, and Simon Zhu provided excellent research assistance. The analysis and conclusions set forth are those of the authors and do not reflect the views of the Federal Reserve Bank of San Francisco, the Federal Reserve System or its staff.

2 Roth Tran: The Board of Governors of the Federal Reserve System. Email: brigitte.rothtran@frb.gov. Wilson: The Federal Reserve Bank of San Francisco. Email: daniel.wilson@sf.frb.org.
I. Introduction

Natural disasters have become more frequent and costly in recent decades. Figure 1(a) shows the number of counties in the U.S. with a Federal Emergency Management Agency (FEMA)-declared disasters and the associated inflation-adjusted disaster damages for each year from 1980 to 2017. Both counts and damages have trended up over the past four decades, as has government aid spending on recovery. While increased development and population growth in disaster-prone areas has played a role (Rappaport and Sachs 2003), climate change is often cited as an important driver of these trends (USGCRP 2017). Consensus climate change projections indicate that the frequency and severity of many types of disasters are likely to rise even further in the decades ahead.

Given these trends, understanding the impact that natural disasters have on affected local economies is critically important. Economic policymakers need to estimate and forecast the impacts of disasters, differentiating disaster-driven economic fluctuations from other sources. Changes in local employment, earnings, population, and property values after a disaster directly impact local tax revenues. Furthermore, natural disaster impulse responses can serve as an important input for macroeconomic climate change model calibrations.

Despite the importance for policymakers, there is little consensus among researchers on what the dynamic impacts of natural disasters are for local economic outcomes. In addition to having devastating effects on mortality and well-being—important impacts that are outside the scope of this paper—natural disasters can cause destruction to local wealth and capital stocks. That destruction could lead to business closures and the relocation of firms and businesses to other places, resulting in long-run declines in local income. Alternatively, the

---

3 These counts exclude disasters without reported damages in the SHELUS data as described in Section 4.
4 These trends are not unique to FEMA and/or SHELUS data on disasters. For instance, similar trends based on other measures of disasters have been noted in the recent Fourth National Climate Assessment (NCA4) as part of the U.S. Global Change Research Program (USGCRP 2017) and by the U.S. National Climatic Data Center (see https://www.ncdc.noaa.gov/billions/time-series).
5 Splitting damages by disaster type shows that the upward trend is driven by hurricanes and floods, which together account for 75% of county disaster observations.
6 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.
7 See, for example, “Harvey-struck Texas counties face blow to property tax revenues” (Reuters 2017).
8 See Bakkensen and Barrage (2020) for an analysis of the welfare effects of cyclones.
inflow of aid and insurance payouts to affected areas, combined with the pull of attractive locational fundamentals, may encourage reconstruction and other investments that could boost local income in the longer run. Given the potential for either of these scenarios to play out, as Botzen, Deschenes, and Sanders (2019) put it in a recent review of the literature, “more research is needed on long-term impacts (e.g., beyond 5 years) of natural disasters.”

An important recent paper by Hsiang and Jina (2014) presents a schematic (which we reproduce in Figure 2) depicting four commonly posited hypotheses for how economic activity might evolve following natural disasters. Using cross-country panel data, they find that the impulse response function (IRF) of national GDP per capita with regard to cyclones is consistent with the “no recovery” hypothesis. However, in reviewing studies examining responses of GDP to disasters in a variety of contexts one can find estimates consistent with each of the four paths depicted in the schematic (see von Peter, von Dahlen and Saxena (2012), Cavallo, Galiani, Noy, and Pantano (2013), Lackner (2019), and Sawada and Sachs (2019).) Hence, the true dynamic response is very much an open question and likely varies by context.

In this paper, we use U.S. county data to study the dynamic response of local economies following disasters. We focus on personal income, which is very highly correlated with GDP and, unlike GDP, is available at the county level back to 1980. To better understand the mechanisms by which disasters affect local income, we consider a broad range of other economic outcomes using a common methodology and data sample. We then examine heterogeneity, adaptation, and spatial spillover effects.

We start by asking what is the average response of local income to natural disasters? In contrast to the Hsiang and Jina (2014) cross-country findings discussed above of long-lasting declines in national income per capita following cyclones, we find robust evidence of long-lasting increases in local personal income per capita following natural disasters in the U.S. Indeed, of the four hypotheses depicted in Figure 2, the estimated response is somewhere between the “build back better” and “creative destruction” patterns. Specifically, our results

---

9 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, Krutti, et al (2020) have found that firms affected by hurricanes experience significant uncertainty, with significant outperformance and underperformance in returns for affected firms several months after landfall.
point to an economically and statistically insignificant initial decline, followed by a recovery to a level of income per capita that is 0.6% above the baseline trend eight years after the disaster.

We then ask what economic channels underlie this income response? We find that the recovery in personal income is initially explained by a temporary boost in employment (especially in construction), insurance payouts, and federal government aid. The boost in aid is shown to derive from both direct disaster aid and automatic stabilizers like unemployment insurance and income maintenance programs. However, over the longer run, the increase in personal income can be largely traced to average weekly wages. We find that at least part of the increase in wages is due to a compositional shift of the local economy toward higher-wage industries.

We also find a long-lasting increase in local house prices and roughly no change in population. In the context of standard spatial equilibrium models such as Hsieh and Moretti (2019), these results suggest that housing supply is relatively inelastic for the average county, so that increases in local demand after disasters push up house prices more so than population. Indeed, when we estimate the disaster IRF separately for high and low housing supply elasticity counties, using Saiz’s (2010) estimates, we find house prices rise only in low-elasticity counties while population rises in (and only in) high-elasticity counties.

Next, we consider the extent to which the average post-disaster response of local income applies across different contexts. We uncover considerable heterogeneity in this response along several key dimensions. First, given projections that some disasters will become more severe with climate change and concerns that our average results may not apply to the most severe disasters, we examine heterogeneity by disaster severity. We find that the rise in income per capita actually increases with disaster severity. Second, we examine whether our average findings apply across disaster types – an important question as many research papers focus only on one type of disaster like floods, cyclones, or fires. We find that while hurricanes and tornados result in longer-run increases in income per capita, the effects of floods, severe storms, extreme winter weather, and fires are small or statistically insignificant.

10 In our preferred specification, we measure severity using monetary damages per capita, data on which are comparable and available for all types of disasters. We also report results, just for hurricanes, using wind speed as an alternative measure. Those results also show larger income p.c. increases for the most severe disasters in the short-to medium run, though smaller increases over the longer run. We discuss the relative merits of measuring severity based on damages versus physical metrics like wind speed in Section IV.
insignificant. Third, given the findings of heterogeneity by income in the cross-country literature as well as anecdotal reports that lower income households have less access to aid, we segment counties by initial income. We find that while the income boost in the first few years after a disaster appears to be concentrated in the richest quartile of counties, by the end of eight years, per capita income generally is higher regardless of initial income.

We next turn to the issue of adaptation. Economic agents – households, firms, and policymakers – may make adaptive investments and behavior changes if they anticipate an increase in disaster frequency or severity going forward. Agents in places with many recent disasters may be more likely to anticipate future disasters and thus make more of those investments and see better outcomes after future disasters. Even just learning from prior disasters may make their responses to future disasters more effective. However, the occurrence of multiple recent disasters can also overwhelm disaster response and recovery capacity, making it more difficult to rebuild quickly and effectively.

We take two approaches to examine the extent of adaptation. First, consistent with local adaptation, we find that counties with more historical experience with disasters see larger increases in personal income over the longer run. Second, we test whether the average dynamic response of income per capita to disasters has changed over time. While the short-run response appears stable over time, we find the longer-run increase has fallen roughly in half over the course of our sample period.

Finally, we use two methods to examine spatial spillovers to see how disaster effects propagate to other counties of varying distances away. Migration and recovery efforts could potentially boost nearby economies or strain them if there is competition over finite local resources. First, using a spatial lag estimation methodology, we show that nearby counties (up to 199 miles away from disaster-hit county) experience a medium-run boost to personal income but are largely unaffected over the longer-run, consistent with residents of nearby counties participating in recovery efforts. Counties that are 200-399 miles away, on the other hand, see a decline in personal income over all horizons, which could be explained by resources being redirected to counties directly affected by disasters. Aggregating the own-county effects with the spatial lag effects, we find that the longer-run income effect for a region – i.e., all counties within 600 miles of a disaster’s epicenter – is modestly negative.
In our second analysis of spatial propagation, we examine state level outcomes using a continuous treatment measure that aggregates county disaster indicators. While the dynamic pattern is qualitatively similar to our baseline per capita income result at the county level, the state level impacts are small and statistically insignificant. This evidence that the longer run income per capita (p.c.) response to disasters becomes more negative at coarser levels of aggregation may help explain the negative longer-run effects of disasters found in some prior studies based on country-level data.

Our findings have several important policy implications. For local policymakers, the finding that employment, and perhaps personal income, fall sharply immediately after a disaster, before eventually recovering, suggests they may need to plan ahead—for example, with larger rainy-day funds—in order to better deal with post-disaster declines in tax bases, which can be recouped after the recovery period. Finally, the heterogeneity in outcomes suggests that we must exercise caution in extrapolating from results based on specific events, contexts, or time frames, which is how much of the literature studying natural disaster effects has been focused thus far.

The remainder of the paper is organized as follows. In the next section, we discuss key findings from prior research. In section III, we discuss the economic channels by which disasters could impact local economies. Then in section IV, we describe the data we use both for disasters and to measure economic activity. We follow this with a discussion of our methodology in section V. In section VI, we present our baseline results. Section VII examines heterogeneity, adaptation, and spatial spillover effects. Section VIII concludes.

II. Literature

Previous research on the dynamic economic effects of disasters generally has focused on national aggregate outcomes, on narrow local outcomes, or on case studies of particular disasters. As mentioned above, Hsiang and Jina (2014) use cross-country panel data to study the dynamic impact of cyclones on national GDP per capita, finding a long-lasting decline. Yet, a number of other cross-country studies find less negative, or even positive,

11 See Botzen, Deschenes, and Sanders (2019) for a recent literature review.
12 See also Noy (2009), who shows that natural disasters contemporaneously reduce national GDP on average and more so in countries that are poorer, less open, or less educated.
effects of disasters on national income for high-income countries like the U.S. Lackner (2019) shows that eight years after impact, earthquakes reduce per capita GDP for low- and middle-income countries, but may boost it for high-income countries. Similarly, von Peter, von Dahlen and Saxena (2012) find that while the average response of national GDP per capita to natural disasters is negative, the response to well-insured disasters (which are predominately in high-income countries) “can be inconsequential or positive for growth over the medium term as insurance payouts help fund reconstruction efforts.” In another cross-country analysis, Cavallo, Galiani, Noy, and Pantano (2013) find that when they include controls for political revolutions that occurred after natural disasters, even the most severe disasters appear to have no significant effect on economic growth. Another recent cross-country study, Sawada and Sachs (2019), find that natural disasters and wars have positive long-run effects on per capita GDP growth.

There have also been a number of studies of disasters’ impacts on local economies in the U.S., though these studies generally do not explore the full dynamics of the impacts. Strobl (2011) focuses on coastal U.S. counties and finds that annual per capita income growth falls significantly in the year of the hurricane but returns to the pre-hurricane growth rate in the following year. This growth pattern implies an initial reduction in the level of income, relative to the no-disaster counterfactual, that is not fully recovered in the subsequent year, consistent with the “no recovery” scenario depicted in Figure 2. By contrast, Groen, Kutzbach, and Polivka (2020) perform a careful longitudinal study of workers affected by Hurricanes Katrina and Rita in 2005, finding substantial long-term gains in earnings, driven largely by higher wages. Similarly, Deryugina, Kawano, and Levitt (2018) follow over time the income tax returns of individuals that were in areas hit by Hurricane Katrina and find that by 3 years after the hurricane, average labor income for these individuals is higher than that of a control group of unaffected individuals.

Another within-U.S. study that is closely related to ours is Boustan, Kahn, Rhode, and Yanguas (2020). Using county-by-decade panel data from 1940 to 2010, they estimate the contemporaneous (i.e., within same decade) effects of severe disasters on several economic outcomes. They find that counties affected by the most severe disasters experience higher net out-migration, higher poverty rates, and lower house prices. In this paper, we similarly find significant medium- to longer-run (up to 8 years out) increases in net out-migration and declines in population and house prices after very severe disasters. We also uncover two related results.
First, there is actually a strong positive response of house prices after severe disasters in the shorter-run, lasting about 2-3 years. This temporary boost in prices could be due to a temporary drop in housing supply caused by disaster destruction that combines with stable or increasing demand for workers in the area during recovery and rebuilding efforts. Second, we find a different pattern for less severe disasters – i.e., those with damages per capita below the 90th percentile for all disasters. For these disasters, the longer-run response of both population and house prices is slightly positive, yielding an average home price response that is positive in our 8-year horizon.

There is a broad literature examining effects of specific types of natural disasters or even specific events on particular sets of outcomes. For example, examining the first three years thereafter, McCoy and Walsh (2017) find that wildfires in Colorado yield short-lived declines in house prices, while Bin and Landry (2013) find that hurricane flooding causes temporary declines in house prices in affected areas. Separately, there are a fairly large number of detailed case studies of specific disasters. Prominent examples include Vigdor (2008), Hornbeck (2012), Gallagher and Hartley (2017), Deryugina, Kawano, and Levitt (2018), and Groen, Kutzbach, and Polivka (2020).

III. Economic Channels

The dynamic effects of natural disasters on local economic outcomes is far from clear a priori. Disasters can be thought of as negative shocks to public and private capital stocks and to household wealth. Indeed, in our empirical analysis we exclude from our definition of disasters any events for which there are no documented monetary damages. The literatures on economic growth and spatial equilibria provide useful insights as to the potential local economic effects of such damage shocks in the short- and longer-run.

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 one-time negative wealth shock increases the marginal utility of consumption, inducing households to increase labor supply, increasing employment and income all else equal. Thus, these models suggest disaster shocks could

---

13 This result is consistent with Graff Zivin, Liao, and Panassié (2020)’s finding that home prices are elevated for up to 3 years after hurricanes in Florida, though they do not look beyond 3 years.
potentially increase employment, income, and related measures of local economic activity in the short- to medium-run.

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 pressure on population and house prices. The extent to which shocks to local housing demand manifest in higher population versus higher house 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) house 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. In Section VI.C., we test and find strong empirical support for both of these hypotheses.

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 microfoundation 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 and find support for this hypothesis for the disasters in our sample in Section VI.C.

Finally, natural disasters in the U.S. typically trigger large amounts of insurance payouts and government aid. These external fiscal injections 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). See, for example, Shoag (2013), Wilson (2012), Chodorow-Reich, Feiveson, Liscow, and Woolston (2012), and Chodorow-Reich (2019) for state-level evidence and Suárez Serrato and Wingender (2016) for county-level evidence. On the other hand, aid received by households displaced from their housing—especially aid that is not required to be used for rebuilding—may facilitate household relocation away from affected areas. In particular, while Small Business Administration (SBA) disaster loans need to be repaid and rely on the homes that the funds are intended to repair as collateral, monies

---

14 The National Flood Insurance Program (NFIP) payouts averaged $2.2 billion a year from 2006-2016. FEMA’s Public Assistance (PA) program, which issues grants to state and local governments to repair or rebuild public infrastructure, averaged over $3.3 billion a year in grants over the same period. In terms of individual aid, in the U.S. FEMA provides grants to individuals for temporary housing and other needs through its Individual Assistance programs, which includes the Individual and Household Program (IHP). The Small Business Administration (SBA) makes loans to qualified individuals, households, and businesses to help cover uninsured or underinsured property losses. These two individualtransfer and loan programs aaveraged about $370 million per year from 2006-2016. Disasters may also trigger significant transfer payments from non-disaster safety-net programs such as Unemployment Insurance, Temporary Assistance for Needy Families, Medicaid, and the Earned Income Tax Credit. Transfers from these programs increase after a disaster as more households in the affected area qualify, as found in Deryugina (2017) for hurricanes.
received by households from FEMA Individual Assistance aid and NFIP payouts have fewer strings attached.

Given the theoretical ambiguity of the local economic effects of disasters, we are agnostic \textit{a priori} as to the sign and magnitude of disasters’ effects on local income, employment, wages, and other outcomes. We empirically trace out the dynamic effect of disasters on local economic activity—that is, the net effect from all potential channels—over time. We study a broad range of economic outcomes that should capture the effects of the shocks to local labor demand and supply and to locational fundamentals like productivity and amenities.

IV. Data

We use data on disasters and a variety of economic indicators, which we describe below. Appendix Table A1 summarizes the sources and treatment of the dependent variables, while Appendix Table A2 shows summary statistics. Note that 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. Although FEMA disaster declarations go back to 1953, we only estimate IRFs for disasters that occurred between 1980 and 2017 based on the availability of our outcome data. We focus on natural disasters that received a “Major Disaster” Presidential declaration according to the FEMA data.\textsuperscript{15} 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 assistance the county is eligible.\textsuperscript{16} Potential types of assistance include (1) Public Assistance (PA) for infrastructure repair; (2) Hazard Mitigation Grant Program (HMGP) grants to lessen the effects of future disaster incidents; and (3) Individual Assistance (IA) for aid to individuals and households.

We exclude FEMA-declared disasters with zero reported damages because we observe in the data many instances of FEMA declarations covering all counties in an affected state even when it

\textsuperscript{15} Given our focus on natural disasters, we exclude declarations due to terrorism or toxic substances.

\textsuperscript{16} Source: author conversations with FEMA staff.
is clear that only a portion of counties were physically affected.\(^{17}\) We use data on damages from the Spatial Hazard Events and Losses Database for the United States (SHELDUS).\(^{18}\)

The resulting sample of natural disasters covers much of the country, with 95 percent of counties experiencing at least one FEMA disaster declaration with positive damages between 1980 and 2017. Figure 1(b) maps the number of years, within 1980 to 2017, with a positive-damages disaster by county. The modal county experienced disasters in eight of the years of this period.

To examine heterogeneity of effects by disaster type, we use FEMA disaster type categories with some modifications. We designate as a hurricane any FEMA disaster declaration containing “hurricane” in the declaration title or categorized by FEMA as a hurricane. We then designate as a flood any remaining disaster classified by FEMA as a flood or containing “flood” (but not “hurricane”) in its title.\(^{19}\) Note that disaster types differ significantly in terms of damages distributions (see Appendix Figure A2), with hurricanes and tornadoes tending to have the highest per capita damages in affected counties.

B. Disaster Severity

We use the SHELDUS database of monetary damages mentioned above for an extension examining the heterogeneity of disasters’ income effects across disasters of varying severity. The SHELDUS database is based on data from the NOAA Storm Database, which in turn are based on reports from insurance companies, media, and other sources. SHELDUS separately reports county-level crop and property damages for a wide range of event types, such as floods, tornados, thunderstorms from 1990 onward. We aggregate damages within the county over all events occurring during a month to estimate total disaster damages by county and month. We then use Census population data to estimate per capita damages.

To our knowledge, SHELDUS is the most comprehensive source of monetary damages for natural disasters in the U.S., covering all types of natural disasters and the entire country.

\(^{17}\) 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.

\(^{18}\) We consider disasters where SHELDUS shows positive damages for the month of the incident begin date according to FEMA or the month thereafter if FEMA shows the incident end date in a month after the incident begin date.

\(^{19}\) The geographic exposure to disasters varies significantly by disaster type (see Appendix Figure A1).
at the county level. However, SHELDUS likely contains non-trivial measurement error. A primary source of measurement error appears to stem from the fact that, when only total damages are known for a given disaster, SHELDUS splits the total among all disaster-declared counties equally. Given that more populous counties are likely to have more property at risk of damage, we redistribute county damages to equate the per capita damages across affected counties.

A potential concern with measuring disaster severity using monetary damages is endogeneity. Specifically, as noted above, damages are measured ex-post by insurance companies, news reports, and other sources. Though these damages are intended to reflect physical damages rather than lost income, one potential source of measurement error in damages is that they could partially reflect immediate losses to local income from the disaster. We note two important considerations regarding this potential endogeneity. First, if reductions in income lead to higher measured damages, any resulting bias for the estimated income impulse response function should be negative. Second, because damages measure initial economic losses, any bias is unlikely to be a concern for horizons beyond the initial time period of the disaster.

Alternative approaches to measuring damages include modeling damages, in a first-stage regression, as a function of physical disaster characteristics or simply using physical characteristics as a reduced-form measure of damages. We use disaster declaration and pecuniary damage data for our treatment measure for several key reasons. First, data on physical characteristics are not readily available at the U.S. county level for a broad set of disaster types. Second, even when these data are available, they are not comparable across disaster types. Finally, we are inherently interested in natural hazards that result in disastrous outcomes, which are a function of the built environment that those hazards occur in. The monetary damages caused by a disaster – i.e., the magnitude of the “treatment” represented by the disaster shock – of a given physical strength can vary greatly from place to place depending on the quantity and market value of local property as well as construction quality, building codes, and other differences in local resilience. For example, Bakkensen and Mendelsohn (2016) show that

---

20 For example, Deryugina (2017) used FEMA’s HAZUS-MH simulation model to estimate damages for major hurricanes in the U.S. as a function of wind speed and other storm characteristics. Hsiang and Jina (2014) use a reduced form approach to estimate the economic impact of major hurricanes around the world as a function of the wind speeds associated with each hurricane. Felbermayr and Groschl (2014) expand that approach to cover other types of disasters, using international geophysical and meteorological data. Similarly, Lackner (2019) estimates the impact of earthquakes, measuring their severity using spatially disaggregated data on ground shaking.
hurricane damages tend to be higher in the U.S. than in other OECD countries when examining responses to physical storm characteristics.

Although our preferred treatment measure is based on disaster declarations and monetary damages, as an extension we also use wind speed data to examine the heterogeneous income effects of hurricanes by disaster severity. We describe the county level hurricane wind speed data for this analysis in Appendix B4.

C. Income

We use annual county level data on personal income and its components from the Regional Economic Information System (REIS) of the Bureau of Economic Analysis for 1980 through 2016 (see BEA 2017). In addition to total personal income, we examine wage and salary income, government transfers (and loans), and poverty rates. We describe these data in Appendix B.

D. Employment and Average Weekly Wages

Our data on employment and average weekly wages by county come from the Quarterly Census of Employment and Wages (QCEW), as used and described in detail in Wilson (2017). The QCEW is compiled by the Bureau of Labor Statistics (BLS) based on state Unemployment Insurance administrative records. Nearly all private nonfarm employers in the U.S. are required to report monthly employment counts and quarterly wages of their employees to their state Unemployment Insurance agencies. Employment covers “all full- and part-time workers who worked during or received pay (subject to Unemployment Insurance wages) for the pay period which includes the 12th day of the month.” We separately examine effects for total nonfarm employment and construction employment (category 1012). Our data on total nonfarm employment and wages covers the period January 1980 to December 2016, while the construction employment data start in January 1990.

The BLS calculates average weekly wages (AWW) “by dividing quarterly total wages by the average of the three monthly employment levels (all employees, as described above) and

---

21 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.

22 Our employment and wages data cover “nonfarm” employment and so exclude any employment in agriculture, ranching, fishing, and hunting.
dividing the result by 13, for the 13 weeks in the quarter.” Note that AWW reflect both hourly wages and the number of hours worked per week.

E. **House prices**

We use the CoreLogic Home Price Index (HPI), available by county at a quarterly frequency from 1980Q1 to 2016Q4, to measure house prices. The index is based on transaction prices of repeated home sales. Repeated-sales price indices have the advantage of reflecting price changes of individual houses holding fixed the permanent characteristics of the houses and are therefore independent of changes in the composition of houses in an area. However, a natural disaster can seriously affect the characteristics of a given house. For instance, unrepaired damage will negatively impact a house’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 house price results.

In addition, we use the housing supply elasticity data from Saiz (2010), which is based on topographical characteristics of metro areas.

F. **Population**

Estimates of annual population by county were obtained from the Census Bureau for 1980 through 2017 and reflect the population in each county as of July 1 of each year.

V. **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 variant of the Jordà (2005) local projection method, modified for panel data, as our baseline specification. We then build on that specification to explore heterogeneity in disaster effects.

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

\[
y_{c,t+h} - y_{c,t-1} = \beta^h D_{c,t} + X_{ct}' \gamma^h + \alpha^h_{r(c),t} + \alpha^h_{c,m(t)} + \epsilon_{c,t+h}.
\]

\( y_{c,t} \) 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. Appendix Table A1 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, as described in Section IV.A. The series of \( \beta^h \) are the IRF coefficients of interest. \( X_{c,t} \) is a vector of control variables with parameters \( \gamma^h \). Specifically, \( X'_{c,t} \gamma^h \) is defined as

\[
X'_{c,t} \gamma^h \equiv \sum_{\tau=-p}^{h} \delta^\tau D_{c,t+\tau} + \sum_{\tau=-p}^{h} \theta^\tau D^0_{c,t+\tau} + \rho^h \Delta y_{c,t-1,t-k}.
\]  

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 the horizon of interest (\( h \)).\(^{24,25}\) This ensures that the estimated IRF from a disaster is not contaminated by either lingering effects of past disasters or effects of other disasters that happen to occur between the current disaster and horizon \( h \).\(^{26}\) The second term controls for other minor disasters (i.e., without reported damages) occurring in the same county within the window from \( p \) periods before to \( h \) periods after period \( t \).\(^{27}\)

The third term in equation (2) \((y_{c,t-1} - y_{c,t-k})\) explicitly accounts for the potential of a pre-trend in the outcome variable. Because the dependent variable is the sum of period-by-period changes in the (log) outcome over the post-disaster timeframe up until horizon \( h \)—that is, \( y_{c,t+h} - y_{c,t-1} = \sum_{i=0}^{h} \Delta y_{c,t-i} \)—this pre-trend term is a lag of the dependent variable with a different time horizon. We measure this pre-trend over the prior three years, so \( k \) equals 3, 12, or 36 depending on whether the outcome variable is annual, quarterly, or monthly.\(^{28,29}\)

\(^{24}\) Though not shown in equation (2) for tractability, when \( h < 0 \) we still include the \( p \) lags indicating whether disasters occurred before period 0.

\(^{25}\) For monthly outcomes, due to computational demands, we control for 12-month aggregate indicators for the leads and lags of disasters.

\(^{26}\) We note that in practice in our sample, the inclusion/exclusion of these intervening disaster dummies has virtually no effect on our results.

\(^{27}\) \( D^0_{c,t} \) can only equal one if \( D_{c,t} \) is zero in a period.

\(^{28}\) The choice of \( k \) involves a trade-off: higher values of \( k \) may provide a better forecast of the counterfactual no-disaster trend in the outcome between time \( \theta \) and \( h \) but will also reduce the sample size available for any given \( h \) regression.

\(^{29}\) We could alternatively control for the counterfactual no-disaster trend by including a county-specific time trend, which would entail no loss of regression observations from the beginning of the sample. The downside of this approach is that a county’s post-disaster time trend could itself be impacted by the disaster, making it a “bad control” (Angrist and Pischke 2009, Goodman-Bacon 2021). Nonetheless, to assess robustness, in Appendix Figure
We include region-specific time fixed effects $\alpha_{r(c),t}$ 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. In addition, to account for serial and spatial correlation, we cluster the standard errors at the county and Census region-by-time levels.

Note that 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, and the reporting of non-zero damages in SHELDUS, 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.

VI. Baseline Results

We now present our baseline IRF estimates, which come from estimating $\beta^h$ in equation (1) above. The results are shown in Figure 3. The shaded areas around the coefficient estimates represent 90 and 95 percent confidence intervals, calculated based on errors that are robust to heteroskedasticity and clustering by county and year-by-Census region (to account for serial and spatial correlation). 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 our estimate of what it would have been in a no-disaster counterfactual. This no-disaster counterfactual reflects region-by-time and county-by-calendar month (or quarter) fixed effects as well as the controls in equation (2).

A3. We provide alternative IRF results for each of our main outcome variables whereby we replace the pre-trend term in equation (2) with a county-specific time trend (i.e., an interaction between county fixed effects and the time variable).
A. Personal Income Per Capita

Figure 3 panel (a) shows the estimated IRF for personal income per capita (p.c.). The estimated contemporaneous response is negative but economically and statistically insignificant. As of one year after a disaster, income p.c. is nearly 0.2% higher than it would have been otherwise. Income p.c. remains about that much higher for the next several years and then increases more around 6 to 7 years out. As of 8 years out, income p.c. is estimated to be a little over 0.6% above where it otherwise would have been. Recalling the hypothetical scenarios in Figure 2, these baseline results on income p.c. lie somewhere between the “build back better” and “creative destruction” scenarios.

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 or whether these average effects are instead driven, for example, by the top end of the distribution which would imply a rise in inequality. Thus, before turning to the key components driving the average effects on personal income, we briefly report results on the response of the poverty rate to natural disasters. Shown in Appendix Figure A4, we find a positive but statistically insignificant longer-run effect of average disasters on the poverty rate. If the personal income increase were experienced equally by all parts of the income distribution, we would expect the poverty rate to decline as some see their income rise above the poverty threshold. Thus, these results suggest that despite an increase in average personal income per capita, local inequality may rise after natural disasters.

B. Key Components

We next estimate the disaster IRFs for outcomes that are key components driving the longer-run positive response of income per capita to disasters. We start with the estimated IRF for total nonfarm employment, which is estimated at a monthly frequency. The results are shown in panel (b) of Figure 3. Consistent with an initial disruption in activity, employment falls sharply, by about 0.08%, 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 nearly 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, with employment peaking around one year out. After this recovery period, employment gradually returns to the no-disaster counterfactual. As of eight years out, the
point estimate suggests modestly higher employment of about 0.2% but it is not statistically significant.

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, when construction employment is estimated to be roughly 1.1% higher than in the no-disaster counterfactual. This is about six times larger than the total employment response 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 is, on average, quite long-lasting.

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 about 0.4% higher than they would have been in 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 house prices, based on the CoreLogic repeat-sales house price index (HPI). We find that the HPI increases very modestly in the near-term and then more substantially over the longer term. As of eight years after a disaster, the local HPI is estimated to be about 1.5% higher than it would have been otherwise. While the initial modest increase likely reflects a reduction in housing supply due to disaster damage, the longer run positive price effect could be explained by a steady or increasing demand for housing—consistent with the wage and employment responses—combined with a persistently reduced supply. It is also possible that the higher house price path reflects a higher quality of homes in the rebuilding process or a shift in the composition of houses being resold for the CoreLogic repeat sales index. That is, it is possible that homes are being rebuilt in more resilient locations (perhaps through managed retreat) or using better methods and materials, reflecting a shift in quality that won’t be controlled for in a repeat-sales index.
We show the 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.\textsuperscript{30} Thus we find that the average response is neither one of significant net out-migration (in response to the negative effects of disasters) or significant net in-migration (in response to recovery efforts). However, these averages mask modest longer-run declines in both in-migration and out-migration that roughly cancel each other out (see Appendix Figure A6).\textsuperscript{31} Thus the near-zero population response is not due to a lack of migration responses. Taken together, the positive response of house prices and the relative non-response of population over the longer-run is consistent with a standard spatial equilibrium model with the average county having a relatively inelastic housing supply. A low elasticity of housing supply constrains population growth and stimulates house prices in response to a positive shock to local amenities and/or local productivity. Below we further explore this hypothesis using data on local housing supply elasticities.

Another component that could potentially explain our finding of higher personal income p.c. is government transfers. In Appendix Figure A7, we examine the impact of disasters on government transfers, including disaster aid and loans. Although federal disaster aid and insurance payouts increase substantially after a natural disaster as one would expect, we find that over the longer run overall government transfers are lower than they would have been in the absence of a disaster.\textsuperscript{32} This latter phenomenon 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 average weekly wages are higher.

\textsuperscript{30} As shown in Appendix Figure A5, we obtain a somewhat different result if we use an alternative specification that replaces the county-specific pre-disaster linear time trend variable with a county-specific full-sample linear time trend variable (i.e., an interaction between the county fixed effect and year). As mentioned earlier, our preferred specification does not include the latter because it is potentially endogenous with respect to the disaster treatment. Nonetheless, using that specification yields results that are broadly similar to the baseline results for all outcomes except population. This specification yields an IRF for population that is steadily declining over time. As of eight years out, population is estimated to be a little over –0.1% below the no-disaster counterfactual.

\textsuperscript{31} Appendix Figure A6 shows results from estimating the IRFs separately for in-migration and out-migration, where each outcome is measured as the number of migrants divided by pre-disaster \((t-1)\) population. See Appendix B5 for details on the migration data.

\textsuperscript{32} In results not shown, we find that aid increases with disaster severity.
C. Economic Mechanisms

Our baseline results point to a longer-run increase in local personal income after a natural disaster. This increase owes to higher labor income—which appears to stem from a longer-run increase in average weekly wages—and not government transfers, which actually decline over the longer-run.\(^{33}\) The increase in earnings is 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 house prices over the longer run.\(^{34}\)

These results are 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. In such models, increases in productivity and amenities result in higher demand for both labor and housing. Hornbeck and Keniston (2017) suggest a specific channel for how disasters might lead to improvements in local public and private capital, pushing up local land values. In their model, property owners do not internalize the fact that improvements to their own properties have a positive externality on the values of other properties in the local area. This leads to a suboptimal level of property quality. A disaster compels and through insurance and aid provides funds for many local property owners to simultaneously rebuild or renovate properties up to current codes and standards, which raises the value of all local properties, even those not renovated.

A prediction of their model is that this channel is only operative in areas in which property values were growing; in declining areas, owners have no incentive to invest in their properties regardless of the externality. We thus test this prediction in our sample by estimating the house price disaster IRF separately for growing vs. declining counties, based on a county’s 3-year pre-disaster growth rate of the house price index (deflated by the CPI). The results are shown in Figure 4. Consistent with the Hornbeck and Keniston (2017) model, we find that the positive response of house prices to a disaster shock applies only to growing counties.

\(^{33}\) See Appendix Figure A8 for IRF estimates for the wage and salary component of personal income.

\(^{34}\) Note that if the increase in the house 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.
As mentioned earlier, the fact that we see longer-run increases in house prices, but not in population, also suggests that housing supply is relatively inelastic on average, at least within our eight-year horizon. Indeed, two predictions of standard spatial equilibrium models, such as that of Hsieh and Moretti (2019), are (i) that house prices should respond more to disasters in areas with relatively inelastic housing supply, while (ii) population should respond more to disasters in areas with relatively elastic housing supply. We test these predictions by estimating the house 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 are shown in Figure 5. We find strong evidence supporting both predictions. Namely, the increase in house prices after a disaster occurs only in local 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 places where housing supply can easily expand in response to higher demand (driven by higher amenities and/or higher productivity) can accommodate a higher population, whereas in places where housing supply is closer to fixed, the higher demand drives up prices.

Similarly, that average weekly 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, results in Appendix Figure A7 on composition-adjusted wages point to an alternative interpretation.\textsuperscript{35} There we find natural disasters likely lead to a shift in the composition of the local workforce toward higher income individuals, explaining at least part of the longer-run increase in (unadjusted) average weekly wages.\textsuperscript{36} As such, quality/composition-adjusted employment could be said to have increased while composition-adjusted wages were closer to flat, consistent with a relatively elastic labor supply margin via migration.

\textsuperscript{35} See Appendix B2 for a discussion of how we construct a measure of composition-adjusted wages using CEPR yearly extracts of the CPS Outgoing Rotation Group micro-data and Eckert, Fort, Schott, and Yang (2021)’s version of the Census Bureau’s County Business Patterns (CBP) data.

\textsuperscript{36} We assume here that changes in industry mix affect a average weekly wages (AWW) by affecting a average hourly wages. It is also possible that changes in industry mix affect AWW by affecting a average hours per week if hours per week vary by industry, in which case our results here understatement the role of industry mix.
VII. Heterogeneity, Adaptation, and Spatial Spillovers

We now explore three sets of extensions relating to heterogeneity of disaster responses, adaptation to disasters over time and based on county-specific prior experience, and spatial spillover effects.

A. Heterogeneity

Although the average dynamic response of local economic activity to natural disasters presented above is informative, this average response may well mask heterogeneity along several important dimensions. Average effects may be of limited use to policymakers and analysts seeking to predict the responses to specific disasters. In this section, we consider heterogeneity in terms of disaster severity, disaster type (i.e., floods, hurricanes, etc.), and county income prior to the disaster. The subsections below present the methodology and results for each of these exercises in turn.

1. Heterogeneity by disaster severity

In our first heterogeneity analysis, we examine the role of disaster severity, which is important in light of projections that some disasters will become more severe with climate change (USGCRP 2017). We seek to understand both whether the estimated 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 the extent of its damages, we estimate an outcome’s impulse response at a given horizon $h$ to a third-order polynomial function of the damages caused by the disaster:

$$\beta^h(s) = \sum_{p=0}^{3} \beta^h_p s_{ct}^p$$

where $s_{ct}$ denotes the per capita damages for county $c$ in period $t$, as measured in SHELDUS. Substituting equation (3) into equation (1) yields the following specification:

$$y_{ct+h} - y_{ct-1} = \beta^h_0 D_{ct} + \beta^h_1 D_{ct} s_{ct} + \beta^h_2 D_{ct} s_{ct}^2 + \beta^h_3 D_{ct} s_{ct}^3$$

$$+ \mathbf{X}'_ct \gamma^h + \alpha^h_{r(c),t} + \alpha^h_{c,m(t)} + \epsilon_{ct+h}$$

(4)
The estimated coefficients, $\hat{\beta}_p^h$ for $p = 0, \ldots, 3$, from this regression allow us to compute the impulse response, $\hat{\beta}^h(s)$, for any given level of damages ($s$) according to equation (3). In Section VI, we report the full impulse response function (from $h = 0$ to $H$) for selected percentiles of the distribution of $s_{c,t}$ across disasters in our sample.

Examining the same six economic outcomes as in Figure 3, Figure 6 displays the estimated IRFs for different levels of damages corresponding to various percentiles of the distribution of (non-zero) damages p.c. in our sample. In particular, in each panel the solid thick blue line depicts the IRF corresponding to a disaster with per capita damages equal to the 50th percentile of all disasters (with positive damages), while the thick solid orange line depicts the IRF for a disaster with per capita 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 IRF for a median-damages disaster is quite similar to the IRF for the mean disaster shown in Figure 3, panel (a), with a modest initial drop followed by a modest positive response over the medium run and a larger positive response over the longer run. In particular, personal income p.c. following a median-damages disaster is estimated to be around 0.6% higher after eight years. Note also that the 25th percentile IRF is visually indistinguishable from the 50th. In fact, notable differences in either the level or shape of the IRF do not really emerge until damages rise above the 95th percentile. For the most severe disasters – those with damages above the 95th percentile – personal income p.c. increases substantially both in the short-run and over the longer-run. For instance, we estimate that a disaster in the top 1% of damages causes personal income p.c. to increase by over 1% in the initial year and by over 1.5% after eight years.

Looking at the analogous results for the other outcomes in panels (b)-(f) allows us to unpack this result. First, we find that the most severe disasters cause large and persistent increases in both total employment (panel b) and average weekly wages (panel d), with employment and AWW up over 1% after eight years. 37 Within overall employment, construction employment increases by nearly 6% by 1-2 years after the disaster but then comes down to a level similar to that after a more typical disaster. In other words, the short-

---

37 The large increase in average weekly wages over the longer run is consistent with the worker-level evidence of higher long term wages following the major 2005 hurricanes, Katrina and Rita, provided by Groen, Kutzbach, and Polivka (2020).
to medium-run increase in construction activity after a disaster is much higher for very severe disasters, but the longer-run increase is roughly independent of the disaster’s severity.

We uncover an interesting pattern for house prices, in that the medium-run (1-3 years out) response to very severe disasters is strongly positive while the longer-run response is strongly negative. This longer-run decline in house prices after very severe disasters may be partially explained by the population responses shown in panel (f). While population responds very little to disasters with damages up to the 90th percentile, population falls substantially after the most severe disasters. This longer-run drop in population should reduce demand for housing, putting downward pressure on home prices.

In addition, the longer-run population response helps explain the magnitude of the longer-run increase in personal income p.c. In particular, we find that a disaster with damages p.c. equal to the 99th percentile causes population to decline by roughly 0.75% after eight years. This result in turn suggests that the roughly 1.5% longer-run increase in personal income p.c. after very severe disasters (panel (a)) is about half due to higher total county personal income and half due to reduced population.

On the whole, these results suggest that the magnitude of our baseline responses increase with severity. Here we have measured severity with monetary damages per capita, which should reflect the magnitude of the destruction of public and private capital (including residential capital) in the immediate aftermath of the disaster. As we noted in Section IV, a concern might be that measured damages could be endogenous, at least in the short run, in that they could pick up not only losses of capital/wealth but also initial income losses. If reductions in initial income lead to higher measured damages, any resulting bias for the estimated income impulse response function should be negative and primarily apply to our coefficient estimates for the initial horizons. Thus, to the extent this is a concern, the larger income per capita response we find for the most severe disasters should be considered a lower bound on the true role of severity, at least for the short-run response for which the endogeneity concern is most relevant.

\[\text{Appendix A8}\] provides results separately for in-migration and out-migration. In-migration is measured as the total flow of in-migrants to the county from period \(t\) to \(t + h\) as a share of pre-disaster population. Out-migration is defined analogously. While both increase substantially in the first 3-4 years after the most severe disasters, the out-migration effect is much larger. Over the longer-run (up to 8 years out), both the in- and out-migration responses to severe disasters become somewhat negative, though out-migration continues to be higher (less negative). The higher out-migration response is consistent with the estimated decline in population over the medium and longer runs for the most severe disasters. These results points to considerable population “churn” after a disaster.
In addition to our preferred specification, we examine two alternative versions of the severity analysis. First, we use the original SHELDUS data rather than our adjusted version that spreads duplicate damage observations evenly across counties on a per capita basis. As Figure A10 shows, all of the severity results are qualitatively unchanged except for the population result, which shows that the population increase by the end of 8 years is unaffected by severity.  

Second, to examine whether physical measures of disaster severity yield different results than monetary damages, we re-estimate equation (4) just for hurricane disasters, once using damages p.c. to measure severity, \( s_{c,t} \), and once using maximum sustainable wind speed. In each case, the percentiles of the severity distribution are calculated over only hurricane disasters. The results are shown in Figure A11. Panel (a) shows that the IRF by damages for hurricanes is qualitatively similar to the IRF by damages for all disasters – namely that the income p.c. response is positive and larger for more damaging hurricanes at all horizons. Panel (b) shows the IRF by wind speed is positive and increases with wind speed in the short to medium run, but it is close to zero and decreases with wind speed in the longer run. Thus, damages and wind speed yield qualitatively similar results in the short to medium run, but different results at longer horizons. This difference is unlikely to be attributable to any endogeneity of damages given that any such endogeneity should primarily be a short-run phenomenon. The difference may stem from wind speed not fully capturing the true economic severity of hurricanes – i.e., measurement error leading to attenuation bias toward zero.

In sum, we find that increases in personal income are generally larger for more severe disasters. What can explain this phenomenon? One possibility is that very severe disasters trigger investment in new, modern public and private infrastructure, funded perhaps by government aid as well as private insurance, which spurs local economic development,

---

39 In our view the original SHELDUS data incorrectly assign larger damages on a per capita basis to counties with smaller populations, which are going to be disproportionately represented among the top percentile damages in the original SHELDUS data. Panel (f) in this figure shows that those counties eventually experience the same larger growth as the median counties, and thus the original SHELDUS data hide the negative population effects of the most severe disasters that we find in our baseline outcome.

40 For example, wind speed may not reflect the flooding damage associated with hurricanes, which could have long-term effects on income per capita. In addition, high wind speed hurricanes could have a very different longer run effect in a county with significant property value at risk than in a county with little value at risk or in a county with significant investments in resilient infrastructure than one without.
consistent with the “build back better” scenario from Figure 2. This is not unlike the theory and evidence on war destruction of capital, and subsequent investment-led growth, discussed in Section III. 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 out of the county. This possibility is consistent with Sheldon and Zhan (2021), who find that post-disaster out-migration increases with the severity of the disaster and more so the lower the income of the population.

2. Heterogeneity by type of disaster

We next explore how economic responses to natural disasters vary by disaster type. Specifically, we estimate the following joint regression:

$$y_{ct+h} - y_{ct-1} = \sum_{d \in D} \beta_{dh} D_{ct}^{d} + X_{ct}^{h} + \alpha_{\tau(c),t}^{h} + \alpha_{c,m(t)}^{h} + \epsilon_{ct+h} \quad (5)$$

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

$$X_{ct}^{h} \equiv \sum_{d \in D} \sum_{\tau=-p, \tau \neq 0}^{h} D_{ct+\tau}^{d} + \sum_{\tau=-p}^{h} \theta^{\tau h} D_{ct+\tau}^{0} + \rho^{h} \Delta y_{ct-1,t-k}. \quad (6)$$

The results are shown in Figure 7. We find significant heterogeneity in how personal income p.c. responds to different types of natural disasters. There are substantial medium- to longer-run increases in personal income p.c. for hurricanes, tornados, and fires. However, fires are quite rare in our sample, accounting for just 2 percent of the disasters, and thus their IRFs are

---

41 Indeed, as shown in Appendix Figure A6, we find that after the most severe damages both in-migration and out-migration increase over the medium term, with out-migration apparently dominating such that population falls.

42 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. Appendix Figure A12 plots actual income p.c. for Orleans Parish, Louisiana, from 1980 to 2017. Relative to the approximately linear trend up to 2005, income p.c. spikes in the first 2-3 years after the disaster before gradually returning to the pre-disaster growth trend but at a permanently higher level.

43 For monthly outcomes, due to computational demands, we control for 12-month aggregate indicators for the leads and lags of each disaster type.
imprecisely estimated. Non-hurricane floods, on the other hand, account for 60% of the disasters in our sample. For these floods, we estimate a statistically significant negative effect in the year of the disaster, followed by a modest positive effect in the medium term, and no significant effect in the longer run.

There are several potential explanations for this finding. First, it could reflect differences in destruction. To the extent that hurricanes and tornados tend to more fully destroy structures such that they must be completely rebuilt, this could yield more productivity-enhancing improvements in the capital stock. Second, floods may disproportionately increase the perceived risk of future flooding in the same exact location, reducing incentives to make rebuilding and recovery investments.\textsuperscript{44} In contrast, hurricanes and tornados may be perceived to be unlikely to strike the same exact spot again soon. Third, wind damage is generally covered by standard homeowner insurance plans while flood damage is not. Greater insurance coverage of damages from hurricanes and tornados than floods could lead to larger gains from recovery efforts.

Interestingly, our results on hurricanes here contrast somewhat with prior findings by Strobl (2011). Strobl finds that income p.c. growth falls significantly in coastal U.S. counties in the initial year of a hurricane and then returns to the pre-hurricane growth rate in the following year. This dynamic growth pattern translates into an initial year decline in the level of income p.c. that is not made up thereafter (which would require above-trend growth in the following year). That pattern contrasts with our finding of a positive impact even in the first year after a disaster. Strobl’s estimates do not speak to whether income rises or falls beyond one year out.

3. Heterogeneity by county pre-disaster income

In our third heterogeneity analysis, we examine the role of pre-disaster income in the disaster response. This analysis is prompted by the findings in the literature that lower income countries see worse outcomes in GDP after natural disasters. Furthermore, households in higher income counties may have more private insurance. This, combined with reports that lower income

\textsuperscript{44} Giglio, Maggiori, Rao, Stroebel, and Weber (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.
households tend to receive less aid, would lead to higher income counties having more resources with which to fund their recovery and rebuilding activities.

To investigate heterogeneity in treatment effects in terms of initial income, we split county*year observations into four groups based on their quartile of the distribution of prior-year \((t - 1)\) personal income per capita. We then interact the disaster indicator with the income quartile variable, estimating the following specification:

\[
y_{c,t+h} - y_{c,t-1} = \sum_{q=1}^{4} \beta^{h,q} M_{c,t-1}^q D_{c,t} + \sum_{q=1}^{4} \phi^{h,q} M_{c,t-1}^q + X'_{ct} \gamma^h + \alpha^h_{r(c),t} + \alpha^h_{c,m(t)} + \epsilon_{c,t+h} (7)
\]

where \(M_{c,t-1}^q\) is one of four income quartile indicators indexed by \(q\) and \(X'_{ct} \gamma^h\) is as defined earlier in equation (2). The \(\beta^{h,q}\) coefficients trace out a separate impulse response function for each quartile \(q\). We also include the four quartile indicators themselves as separate conditioning variables in the regression to ensure against possible “selection into treatment” – that is, the possibility that either higher or lower income counties are more likely to be hit by a disaster.

In Figure 8 we show the results of estimating equation (7), which estimates separate IRFs by quartile of pre-disaster county income. We find that the point estimates for all four quartiles are consistent with our baseline results of a longer-run (as of eight years out) increase in income p.c., although they are not all statistically significant. Furthermore, the timing of the increase differs, as only counties in the top quartile of pre-disaster income p.c., experience a statistically significant increase in income in the first four years after the disaster (relative to the no-disaster counterfactual.) Thus, if we looked only a couple of years post-disaster, our findings would be consistent with those showing increases in income or GDP only for higher income countries.

B. Adaptation

An increase in disaster frequency can spur adaptative investments in resilience, as greater experience with disasters leads to learning and expectations of more future disasters. However, intensification can also overwhelm disaster response and recovery capacity, making it more difficult to rebuild quickly and effectively. Each of these phenomena could shift average responses to disasters as they become more frequent over time. We take two approaches to examine these issues.
First, we investigate whether disaster IRFs vary with a county’s historical experience with disasters. To do so, we estimate equation (7), where \( M^q_{c,t-1} \) for each \( q \in Q \) is an indicator for whether the county experienced (a) no periods, (b) 1 period, (c) 2-3 periods, or (d) 4 or more periods with disasters in the previous 10 years.\(^{45}\) We again interact the category indicators with the disaster indicator (as well as including the category indicators as separate regressors).

The results in Figure 9, panel (a) show limited evidence of adaptation. In contrast to the notion that disaster-prone areas adapt and become more resilient to disasters, we find that only counties with disaster experience show an immediate negative hit to income p.c. (statistically significant at the 90% level for the two intermediate categories). This suggests an important role for intensification, whereby counties hit by other disasters within the previous 10 years may not have had time to fully recover and rebuild private and public capital, leaving them vulnerable to the impact of new disasters. In the medium- to longer-run, increased personal income p.c. occurs in all cases except for counties that have experienced zero disasters in the prior ten years.

Our second analysis addresses a closely related adaptation question on how the average disaster response across all counties—regardless of their individual disaster experiences—has evolved over time. Given the increased aggregate frequency of disasters that we documented in Figure 1, communities throughout the United States may be anticipating a higher likelihood of disasters going forward, which could in turn lead to adaptations to mitigate the initial negative impacts on income, not to mention the negative impacts to wealth, property, and health. Furthermore, a more resilient infrastructure could yield smaller boosts to income that result from rebuilding and recovery efforts.

We interact our disaster indicator with a continuous time variable in order to estimate how the impulse response function has evolved over time as shown below:

\[
y'_{c,t+h} - y'_{c,t-1} = \theta^h \cdot t \cdot D_{c,t} + \beta^h D_{c,t} + X'_{ct} \gamma^h + \alpha^h_{r(t),t} + \alpha^h_{c,m(t)} + \epsilon_{c,t+h}.
\]

Here \( \theta^h \) estimates how the response has evolved over time, where a positive value would reflect that the marginal effect of a disaster on personal income has increased over time. To illustrate the results, we then calculate the implied IRF for the earliest (1983) and latest

\(^{45}\) Periods are monthly, quarterly, or annual, based on the outcome of interest.
(2009) years in the sample, excluding the years in the earliest pre-trend interval (3 years) and the latest longer-run interval (8 years) using our estimate of $\theta^h$.

The results for income p.c. in Figure 8, panel (b) show that the initial impact coefficient has become less negative, shifting toward zero over time, consistent with adaptation; however, the shift is not statistically significant. The magnitude of the longer-run post-disaster increase in income has also decreased over time. In particular, in addition to dipping in the medium run, the longer-run increase as of the latest year in our analysis (2009) is about 0.4%, which is less than half the increase as of the earliest year of the analysis (1983). This suggests that going forward, as disasters become more frequent and severe, counties hit by disasters may not see such significant boosts in personal income afterward.

C. Spatial Spillovers and Wider Geographies

We have thus far focused on estimating the economic impacts of natural disasters at the local level, proxied by counties. 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. We now investigate this issue in two complementary ways. First, we construct geographic circular bands (“donuts”) of counties of given distance ranges surrounding each focal county hit by a disaster and then jointly estimate the impact of disasters on both the focal county and on the geographic areas defined by each surrounding band. Second, we aggregate to the state level our treatment and outcome variables and repeat our baseline IRF specifications at the state, rather than county, level.

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

\[
y_{c,t+h} - y_{c,t-1} = \sum_{b \in B} \pi^{h,b} D^b_{c,t} + \rho^h D_{c,t} + X'_{ct} Y^h + \alpha_{r(c),t} + \alpha_{c,m(t)} + \epsilon_{c,t+h}
\]

For any given focal county, $c$, we split all other counties into $B$ separate distance bands (“donuts”) indexed by $b$.

The treatment variable $D^b_{c,t}$ is then defined as the share of population within distance band $b$ from county $c$ that was in counties that experienced a disaster in period $t$:
\[ D_{ct}^b \equiv \sum_{i \neq c} 1[b \leq d_{ci} < b'] \omega_{ci} D_{it}, \]  

where

\[ \omega_{ci} \equiv \frac{\text{pop}_i}{\sum_i 1[b \leq d_{ci} < b'] \text{pop}_i}, \]

and \( \text{pop}_i \) denotes population of county \( i \) and \( d_{ci} \) denotes the distance between the population centroids of counties \( c \) and \( i \). For example, if county \( c \) has 10 million people living within 200-399 miles of it, and there is a disaster in year \( t \) in a county or counties in that band covering a population of 2 million, then \( D_{ct}^{200} \) would be 0.2.

In Figure 10, we show the results of estimating equation (9) 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.\(^{46}\) The thin blue curves show the IRFs for the directly-hit counties, while the orange curves show the spatial lag IRFs.\(^{47,48}\) These results show that nearby counties (within 199 miles) experience a medium-run boost to personal income, consistent with residents of nearby counties participating in recovery efforts and experiencing positive spillovers (panel (a)). However, these counties do not appear to share in any longer run boost to income per capita. Counties that are 200-399 miles away see a persistent decline in personal income, as shown in panel (b). This could be explained by regional resources being redirected to the counties directly affected by disasters. Counties in the furthest band, 400-599 miles away, experience some modest intermediate gains in income per capita followed by a longer run decline (panel (c)).

In panel (d) we show an estimate for the net effect on personal income per capita within 600 miles of disasters. Here we estimate the sum of the four curves shown in panels (a) – (c), where each IRF is rescaled by multiplying each of the \( \beta^h \) and \( \pi^{h,b} \) terms by the unconditional mean of the corresponding variable. With the estimated \( \beta^h \) and \( \pi^{h,b} \) coefficients representing the average

\(^{46}\) See Appendix Figure A13 for a visual illustration of the spatial lags for a single year of disasters.\(^{47}\) Note that the blue curve shown 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 blue curve the effect captured in the orange curve in panel (a).\(^{48}\) In panels (a)-(c), the spatial lag coefficients have been normalized such that \( D_{ct}^b \) has been divided it by its mean, conditional on \( D_{ct}^b > 0 \). Given this normalization, 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 because the regression samples are horizon-specific. For the 50-199 mile band, the conditional mean varies from 0.27 to 0.28. For the 200-399 band, it rounds to 0.24 across all horizons; and for the 400-599 band, it varies from 0.23 to 0.24. 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.
effect for a county within each category at each horizon, intuitively, we are taking an average of the contribution of these responses to estimate a net effect. This post-estimation rescaling of the IRF coefficients is equivalent to a pre-estimation mean-normalization. These results suggest that while the longer run local impact of a disaster on income per capita in the area directly hit by a disaster is positive, the longer run impact for the broader region appears to be negative.

Turning to our second analysis, we estimate the equation (1) baseline specification at an aggregated state level by replacing the $D_{c,t}$ indicator with a continuous measure of the share of the state’s population living in counties hit by disasters in period $t$.\textsuperscript{49} For this analysis, we cluster the standard errors at the state and time-by-Census Division levels.

The results in Figure 11 indicate that the dynamic pattern for this state-level analysis is qualitatively similar to our baseline per capita income result at the county level (Figure 3, panel (a)). However, the state-level disaster impacts are small and statistically insignificant, consistent with the Figure 10 results showing a statistically insignificant net effect of disasters on income per capita for broad geographic areas.\textsuperscript{50}

\textbf{VIII. Conclusion}

We have found that, on average, counties hit by natural disasters experience a medium- to longer-run boost in income per capita. While a rise in employment contributes to the initial boost, we find the longer-run increase in income per capita is driven largely by an increase in average weekly wages. The increase in average weekly wages appears to be at least partly due to a shift in the skill composition of the local workforce.

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 both labor and housing. Our findings that disaster leads to longer-run increases in wages and house prices, rather than employment and population, suggest relatively inelastic supply of both labor and housing on average. Indeed, we are able to directly examine the role of the supply elasticity

\textsuperscript{49} Using a simple indicator variable for whether there was a disaster in any county in a given state-year results in very few no-treatment observations and also lumps together disasters of widely varying geographic breadth.

\textsuperscript{50} We have also investigated whether the state-level impact varied with the scale/breadth of the disaster by interacting the disaster treatment variable with four dummy variables indicating whether the percentage of the state population in a county hit by a disaster was below 10\%, 10-50\%, 50-90\%, or above 90\%. Shown in panel (b) of \textbf{Appendix Figure A14}, the longer-run positive impacts are found to increase with the scale/breadth of the disaster, though the results are not statistically significant.
for housing by estimating disasters’ effects on house prices and population separately for high and low housing supply elasticity counties using Saiz’s (2010) elasticity estimates. Consistent with theory, we find the post-disaster increase in house prices occurs only in counties where housing supply is relatively inelastic, while the increase in population after a disaster occurs only in areas where housing supply is relatively elastic.

We have also found that there is significant heterogeneity in disaster effects. The post-disaster response of personal income per capita depends on the severity of the disaster, the type of disaster, and, to a lesser extent, the pre-disaster income level of the county. We find that the positive medium- to longer-run effect of disasters on personal income per capita is amplified for more severe disasters. For the most severe disasters, part of the effect is due to a drop in population, though most of it comes from an increase in aggregate county income. Across disaster types, we find that the longer-run increase in income per capita is true for all types except extreme winter weather and severe storms; the increase is largest for tornados, fires, and hurricanes. We find that the longer-run increase holds for both rich and poor counties, as measured by their pre-disaster levels of income per capita.

We have found mixed evidence when it comes to adaptation. On the one hand, consistent with adaptation, counties with more historical experience with disasters see larger post-disaster increases in personal income over the longer run. However, we find that the longer-run increase in personal income per capita has fallen roughly in half over the course of our sample period—a period in which disasters nationwide have become more frequent and severe—suggesting that the ability to recover and rebuild from disasters in a way that yields lasting economic benefits has declined over time, the opposite of what one might expect from adaptation.

Lastly, while the main focus of this paper has been on the local impact of natural disasters, our spatial analyses suggests that the long run increase in personal income locally may come at the cost of, and be more than offset by, a long run decline in personal income in surrounding counties. This could potentially be explained by a diversion of resources to areas affected by disasters.

Taken together, our results suggest that despite the immense toll that disasters take, local economies in the U.S. have generally recovered successfully in terms of income. Indeed, not only does local income recover fully after a few years, it ends up somewhat higher in the
longer run than what it would have been without the disaster. However, this average finding masks significant heterogeneity across contexts and a potential rise in inequality after disasters, given the lack of any decline in the poverty rate.

Our results contrast with some prior international evidence, especially research focused on developing countries, that has found long-lasting negative effects of certain disasters on national income. An important difference between developing countries and the U.S. is the wide availability in the U.S. of private and public insurance, the latter coming in the form of government aid. Going forward, climate change may lead to more frequent and more damaging disasters, which could put increasing demands on insurance markets, government budgets, and the construction sector.
References


American economic review 95.1: 161-182.


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

(a) Disaster Frequency and Damages

(b) Geographic Distribution of Disasters

Source: Federal Emergency Management Agency (FEMA) and SHELDUS.
Note: The count in panel (a) shows the number of counties each year with at least one disaster declaration. 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

(a) Personal Income (per capita)

(b) Total Nonfarm Employment

(c) Construction Employment

(d) Average Weekly Wages

(e) Home Prices

(f) Population

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

Note: These 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. 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: Growing versus declining areas (in terms of house prices)

Source: FEMA, SHELDUS, BEA, and CoreLogic.
Note: This figure shows the impulse response function from estimating equation (1) interacted with an indicator for whether the house 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 time-by-state levels.

Figure 5: Heterogeneity by housing supply elasticity

(a) House Prices

Source: FEMA, SHELDUS, 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 time-by-state levels.
Figure 6: Heterogeneous Impulse Responses by Selected Percentiles of Damages Per Capita

(a) Personal Income (per capita)  
(b) Total Nonfarm Employment  
(c) Construction Employment  
(d) Average Weekly Wages  
(e) Home Prices  
(f) Population

Source: FEMA, SHELDUS, BLS, Census, BEA, and CoreLogic.
Note: These plots show the impulse response functions (IRFs) from estimating equation (4), where the percentile lines show the implied IRF for selected percentiles of the damages per capita distribution over all county-month observations with a disaster (defined as having a FEMA disaster declaration and positive damages). 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 7: 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. 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 8: Heterogeneous Impulse Responses of Personal Income (p.c.) to Disaster Shocks

By Initial Personal Income Per Capita

Source: FEMA, SHELDUS, BEA, and Census.
Note: These plots show the impulse response functions from estimating equation (7), where the shaded regions show the 90 and 95 percent confidence intervals. The quartiles of initial personal income per capita are based on the personal income in year -1 relative to the national distribution in that year. 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 ($-1$).
Source: FEMA, SHELDUS, BEA, and Census.
Note: Panel (a) shows the impulse response functions (IRFs) from estimating equation (7), where the shaded regions show the 90 and 95 percent confidence intervals. 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 major disaster with positive damages. Panel (b) shows the implied IRFs from estimating equation (1) but with an interaction between the disaster dummy and year (i.e., a linear time trend), then calculating the implied IRF for the earliest (1983) and latest year (2009) possible given our sample period and leads and lags. 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 10: Impacts of Own-county Disasters vs. Spatially-Lagged Disasters on Personal Income (p.c.)

Results for spatial lags of varying distance bands

(a) Disasters up to 199 Miles Away

(b) Disasters 200-399 Miles Away

(c) Disasters 400-599 Miles Away

(d) Net Effect Within 600 mile range

Source: FEMA, SHELDUS, Census, and BEA.
Note: Panels (a)-(c) show the impulse response functions (IRFs) from estimating equation (8), where the shaded regions show the 90 and 95 percent confidence intervals. The thin blue curve (repeated in each panel) reflects the IRF estimated for counties directly experiencing a disaster. The orange curves depict the IRFs for the counties within the indicated mile ranges of counties experiencing disasters. The intensity of treatment for the orange curves is the share of population within each band that has experienced a disaster in period 0. Each of the orange curves has been rescaled by the mean population share for positive observations within the band. Thus the curves represent the average effect on counties having at least one county within the given range experience a disaster in period 0. Panel (d) 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 ($-1$).
Figure 11: State-Level Impulse Response of Personal Income (p.c.) to Disaster Shocks

Source: FEMA, SHELUS, 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 inner shaded region indicates the 90 percent confidence intervals, and the lighter outer shaded region indicates the 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 are clustered at the state and at the time-by-division levels.
Table A1: Variable Descriptions

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


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 + ✓) form for the aid variables to address the very high share of observations with zero aid.
Table A2: Summary Statistics

<table>
<thead>
<tr>
<th>Levels</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Median</th>
<th>Max</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal income (per capita)</td>
<td>28,702</td>
<td>8,631</td>
<td>6,494</td>
<td>27,328</td>
<td>183,918</td>
<td>109,486</td>
</tr>
<tr>
<td>Total nonfarm employment</td>
<td>30,830</td>
<td>117,776</td>
<td>0</td>
<td>5,612</td>
<td>3,875,009</td>
<td>1,289,249</td>
</tr>
<tr>
<td>Construction employment</td>
<td>1,878</td>
<td>6,383</td>
<td>0</td>
<td>317</td>
<td>181,710</td>
<td>932,721</td>
</tr>
<tr>
<td>Average weekly wages</td>
<td>602</td>
<td>140</td>
<td>0</td>
<td>581</td>
<td>11,996</td>
<td>430,974</td>
</tr>
<tr>
<td>Home price index</td>
<td>101</td>
<td>43.1</td>
<td>19.6</td>
<td>95.2</td>
<td>369</td>
<td>183,142</td>
</tr>
<tr>
<td>Population</td>
<td>90</td>
<td>291</td>
<td>0.055</td>
<td>24.7</td>
<td>10,164</td>
<td>1,364,091</td>
</tr>
<tr>
<td>Disaster indicator (monthly)</td>
<td>.0186</td>
<td>.135</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>1,366,020</td>
</tr>
<tr>
<td>Per capita damages</td>
<td>244</td>
<td>5,706</td>
<td>0</td>
<td>1.9</td>
<td>848,616</td>
<td>25,442</td>
</tr>
</tbody>
</table>

First differences of logs

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Median</th>
<th>Max</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal income per capita</td>
<td>1.57</td>
<td>5.81</td>
<td>-103</td>
<td>1.49</td>
<td>146</td>
<td>106,296</td>
</tr>
<tr>
<td>Total nonfarm employment</td>
<td>.0868</td>
<td>3.36</td>
<td>-16.9</td>
<td>.207</td>
<td>16.1</td>
<td>1,285,096</td>
</tr>
<tr>
<td>Construction employment</td>
<td>.182</td>
<td>9.37</td>
<td>-40.5</td>
<td>.157</td>
<td>38.8</td>
<td>796,369</td>
</tr>
<tr>
<td>Average weekly wages</td>
<td>.154</td>
<td>7.18</td>
<td>-22.8</td>
<td>.235</td>
<td>22</td>
<td>426,808</td>
</tr>
<tr>
<td>Home price index</td>
<td>.878</td>
<td>1.99</td>
<td>-23.8</td>
<td>.882</td>
<td>20.3</td>
<td>181,749</td>
</tr>
<tr>
<td>Population</td>
<td>.467</td>
<td>1.62</td>
<td>-4.56</td>
<td>.341</td>
<td>6.88</td>
<td>110,409</td>
</tr>
</tbody>
</table>

Source: QCEW, Census, CoreLogic, BEA, Federal Emergency Management Agency (FEMA), and the Spatial Hazard Events and Losses Database for the United States (SHELDUS).

Note: See Table 1 and its notes for description of variable names and further details. The first differences of log variables have been rescaled by 100 so that they show percentage point values. The contemporaneous per capita damages may equal zero if there are positive damages observed in the following month. Income and wages are reported in 2010 US dollars. Damages are reported in 2017 US dollars.
Figure A1: Distribution of Disaster Declarations

Source: FEMA, SHELDUS.
Note: The All Disaster Types map shows counts of months with at least one disaster with damages reported in SHELDUS. The Per Capita Damages at or Above 99th Percentile map shows the number of months a county’s disasters had 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: Distribution of Per Capita County Damages by Disaster Type

Source: FEMA, SHELDUS.
Note: The y-axis shows density and not frequency.
Figure A3: Alternative Specification - County-Specific Linear Time Trend
Impacts - All Disasters

(a) Personal Income (per capita)

(b) Total Nonfarm Employment

(c) Construction Employment

(d) Average Weekly Wages

(e) Home Prices

(f) Population

Source: FEMA, SHELUS, BLS, Census, BEA, and CoreLogic.
Note: These plots show the impulse response functions from estimating an alternative to equation (1), the equation (2) pre-trend term has been replaced with a county-specific time trend. The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals. 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 A4: 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. 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 time-by-state level.
Figure A5: Alternative Specifications for Trends

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

(a) Baseline – Control for Pre-trend

(b) No Pre-trend

(c) County-Specific Linear Time Trend

(d) Control for 3 lags of Y/Y Growth

Source: FEMA, SHELDUS, BLS, Census, BEA, and CoreLogic.
Note: These plots show the impulse response functions from estimating equation (1), where the equation (2) pre-trend term has been replaced with either no trend, a county-specific time trend, or three lags of annual income p.c. growth. The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 95 percent confidence intervals. 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: Impulse Responses of Migration to Disaster Shocks

(a) In-Migration – All Disasters

(b) Out-Migration – All Disasters

(c) In-Migration – By Disaster Severity

(d) Out-Migration – By Disaster Severity

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

Note: Plots (a) and (b) show the impulse response functions (IRFs) from estimating equation (1), where the shaded regions indicate the 90 and 95 percent confidence intervals. Plots (c) and (d) show the IRFs from estimating equation (4), where the percentile lines reflect the model predictions given the per capita damage distributions for all county-month observations with FEMA disaster declarations. 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 A7: Impulse Responses of Government Transfers and Insurance Payouts to Disaster Shocks

(a) FEMA Individual and Household Program Aid
(b) Small Business Admin. Disaster Loans

(c) National Flood Insurance Program Payouts
(d) All Government Transfer Income

(e) Income Maintenance Transfer Income
(f) Unemployment Insurance Transfer Income

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. 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 A8: 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. 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 A9: Impulse Response of Industry-Composition-Adjusted Wages to Disaster Shocks

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 time-by-state level.
Figure A10: Impulse Responses by Damages Per Capita Percentile – Using Original SHELDUS damages data

(a) Personal Income (per capita)

(b) Total Nonfarm Employment

(c) Construction Employment

(d) Average Weekly Wages

(e) Home Prices

(f) Population

Source: FEMA, SHELDUS, BLS, Census, BEA, and CoreLogic.
Note: These plots show the impulse response functions from estimating equation (4), where the percentile lines reflect the model predictions given the per capita damage distributions for all county-month observations with disasters (defined as having a FEMA disaster declaration with positive damages). 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 A11: Heterogeneous Impulse Responses of Personal Income Per Capita to Hurricane Disasters By Severity

Severity Alternately Measured by Damages or by Wind Speed

(a) Severity measured by Damages p.c.  (b) Severity measured by Wind Speed

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 A12: Personal Income Per Capita in New Orleans Before and After Hurricane Katrina

Source: BEA, Census.
Source: BEA and Census.
Note: Vertical red line indicates 2005, the year of Hurricane Katrina.

60
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 A14: State-Level Impacts of Disasters on Personal Income (p.c.) with Disasters Differentiated by Geographic Breadth

Source: FEMA, SHELDUS, BLS, Census, BEA, and CoreLogic.
Note: This figure shows the impulse response functions from estimating equation (1) on state-aggregated data, but allowing the coefficient on the disaster treatment variable to vary across four geographic breadth indicator variables. These variables indicate what quartile the disaster falls in of the distribution of the share of the state’s population in a county hit with a disaster (with positive damages), conditional on having a disaster. The inner shaded regions indicate the 90 percent confidence intervals, and the lighter outer shaded regions indicate the 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 are clustered at the state and at the time-by-division levels.
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

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.
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 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 hurricaneexposedata, 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. 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 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

