Large indirect income impacts of tropical cyclones shaped by disaster response

Christopher W. Callahan[1,2,*], Jane W. Baldwin[3,4], Renzhi Jing[1,5], Marshall Burke[1,6,7], Noah S. Diffenbaugh[1]

[1]Doerr School of Sustainability, Stanford University
[2]O'Neill School of Public and Environmental Affairs, Indiana University
[3]Department of Earth System Science, University of California, Irvine
[4]Lamont-Doherty Earth Observatory, Columbia University
[5]Department of Health Policy, Stanford University
[6]Center on Food Security and the Environment, Stanford University
[7]National Bureau of Economic Research
∗Corresponding author: ccallah@iu.edu

Tropical cyclones (TCs) have direct economic impacts, destroying property and infrastructure. However, the sign and magnitude of their indirect impacts via longer-term changes in economic output remain unclear. Here we use data on TC winds and rainfall combined with county-level income in the U.S. to quantify the long-term indirect impacts of TCs. We find a nonlinear response of income growth to TC winds, where damages initially increase with wind speed but diminish for the strongest winds. In contrast, we find a simpler linear response of reduced income growth with TC rainfall. We show that this discrepancy is likely due to the compensating effect of disaster aid following high winds, which appears to respond more strongly to TC winds than rainfall. Aggregating over recent decades, we find that TCs have reduced U.S. income by $34 trillion over 1980-2019, >25 times their direct losses, but estimate that losses would have been more than 50% larger absent disaster aid. These findings highlight that disaster response can ameliorate indirect disaster impacts, but that to date such responses have only partially avoided large accumulating losses from TCs.

Tropical cyclones are among the most damaging natural hazards, responsible for billions of dollars in direct economic impacts annually[1]. Global warming is expected to increase the impacts of TCs in several ways, including increases in the intensity of the strongest storms[2–4] and potentially both their direct[5,6] and indirect[7,8] impacts.

The direct impacts associated with TC strikes include structural losses to homes, buildings, infrastructure, and crops, as well as immediate human injury and mortality. Increases in TC intensity have been shown to drive exponential increases in these direct impacts[5,6,9,10]. On the other hand, indirect economic impacts from TCs are more difficult to quantify[11]. Disasters such as TCs may cause broader disruption of economic activity from destroyed homes, businesses, or infrastructure[12–14], or changes to longer-term health outcomes such as excess mortality in the months following TCs[15]. It has been suggested that indirect impacts may substantially exceed direct impacts, but although new research has made these comparisons in the context of mortality[16,17], quantitative comparisons between indirect and direct economic impacts remain lacking[13].

Notably, even the sign of these indirect impacts remains uncertain. It is often hypothesized that disasters such as TCs may stimulate economic growth through reconstruction investment or the replacement of destroyed capital with more productive technology[18,19]. The empirical record on this question is mixed, with some studies showing persistent negative impacts[7,8] but others showing long-term benefits for income in the United States (U.S.)[20]. This debate is complicated by potentially heterogeneous effects in different sectors or regions (such as losses in agriculture and benefits in construction[21,22]). In addition, government disaster response is not always triggered uniformly across locations for a given storm and across storms of similar intensity through time.

In the U.S., federal disaster response through the Federal Emergency Management Agency (FEMA) is usually triggered by a formal Presidential disaster declaration in response to an event such as a TC, enabling resources and money to flow to affected areas. There is evidence that disaster aid can have important economic benefits, such as reducing individual debt[23], avoiding negative credit card outcomes[24], stabilizing small business survival and employment[25], increasing job creation[26], and leading firms to upgrade physical capital[27]. Together, these effects may potentially yield long-run benefits for overall income[20]. However, the benefits of disaster response, and its potential to facilitate climate adaptation, are rarely connected to the growing literature on the macroeconomic impacts of climate variability and change. Making such connections is critical because climate change is likely to accelerate the costs of extreme climate events and strain adaptation resources not originally designed to accommodate warming[28]. Greater understanding of the interactions between physical climate hazards, their economic impacts, and the effects of disaster response is therefore essential to designing effective climate adaptation policy[11].

To quantify indirect impacts from TCs, we analyze the effect of TC wind and rainfall exposure on county-level per capita income growth in the U.S. over 1970-2019. We represent TC winds using two spatially explicit wind field models (from Baldwin et al.[29] and Jing et al.[30]), allowing us to assess exposure of each county to TC winds even if a storm track did not directly cross that county. We summarize county-level wind exposure as the spatially averaged maximum TC wind speed experienced across the county in each year[7], noting that instantaneous wind speed at a particular location within a county may be higher than the spatially averaged wind[17]. Structural differences between the two models yield different spatial patterns of wind exposure (Methods), but in both cases the highest wind exposures are felt in coastal counties (Fig. 1A, 1B, Fig. S1).

We use TC rainfall data from the National Oceanic and Atmospheric Administration (Methods). Rather than parametric models, this dataset relies on expert judgment regarding when rainfall intersects with tropical cyclone pressure and wind patterns on high-resolution weather monitoring (Methods). This dataset yields spatially coherent regions of extreme rainfall along the Gulf Coast and Eastern seaboard (Fig. S2, Fig. 1C), consistent with the locations of TC landfall.

We do not include data on local storm surge or compound flooding from TCs. To our knowledge, there is no long-term, country-wide dataset of storm surge to be included in our analytical framework. Wind speed does serve as a useful first-order proxy for overall TC exposure that has been used in previous studies (e.g.,[17,29,31,32]), and we make an additional advance beyond most studies by including data on TC rainfall, a hazard which is not usually included in large-scale econometric analyses of TC impacts (e.g.,[7,8,14]).

We measure indirect impacts by examining the immediate and lagged effects of TCs on per capita income, using data from individual year-end tax returns. To do so, we fit a panel regression model that estimates the effect of county-level wind and rainfall on personal income growth. We estimate models that include wind and rainfall simultaneously, ensuring that we control for the covariance of these two hazards. We use county and year fixed effects and county-specific trends to separate idiosyncratic local variation in TCs from spatial and temporal confounding factors. This method has been used to study the economic growth impacts of other climate hazards[33–36], and is an established technique to credibly isolate the impact of climate from other confounding factors influencing societal outcomes[37,38]. In essence, rather than comparing high-exposure coastal counties to low-exposure inland counties, we compare each county to itself in years of high versus low TC exposure, after accounting for trends in both income and TCs.

The result is a plausibly causal estimate of the effect of TC exposure on income growth across the U.S. We then assess how these effects are moderated by disaster response and quantify long-term accumulated income impacts of TCs across the U.S. By using income as our measure of indirect impacts, our analysis captures economy-wide impacts that alter people's income both in the year of the TC and the following years, even if they were not directly affected by the storm. However, because our analysis does not include changes to outcomes such as mortality risk that are not directly reflected in income, it is a conservative accounting of these impacts.

Nonlinear effect of TCs on income growth

We find a nonlinear response of per capita income growth to TC winds (Fig. 1D, 1E), though the degree of nonlinearity differs across the two wind models. In both cases, income growth declines as wind speeds grow to between 15 and 25 m/s, at which point marginal increases in TC winds become beneficial. In the case of the Baldwin et al. model, county-wide TC winds above ~23 m/s

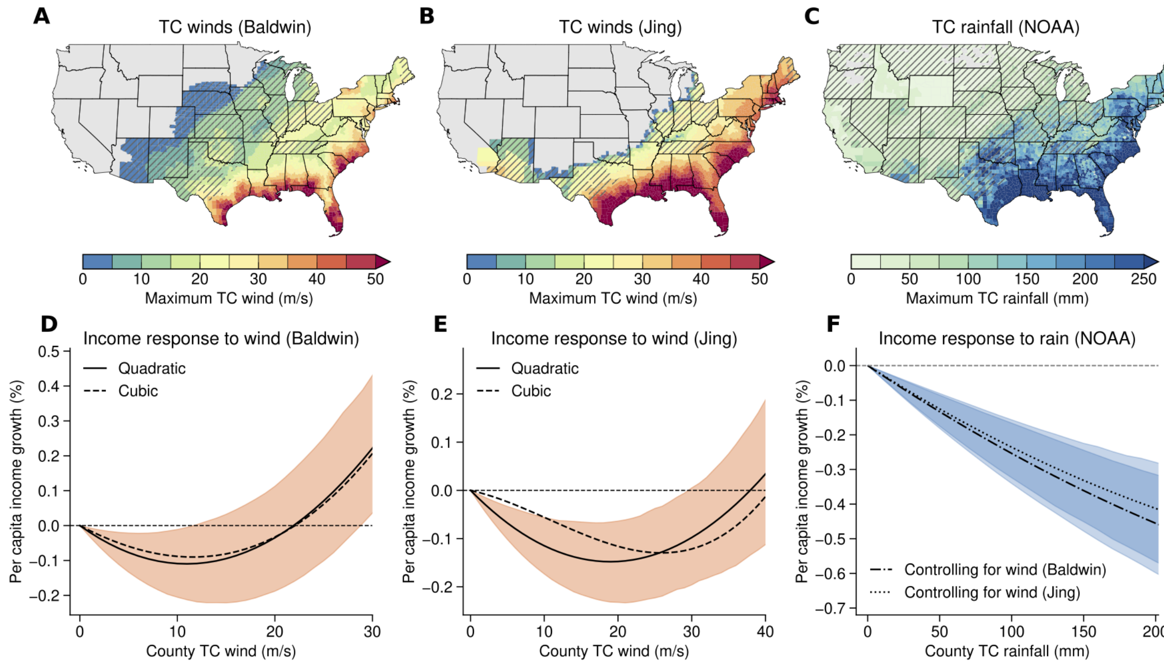

Figure 1: Income growth impacts of tropical cyclones. A, B) Long-term maximum county-level TC winds derived from wind fields produced by Baldwin et al. (A) and Jing et al. (B). C) Long-term maximum TC rainfall derived from NOAA reports. Hatching denotes counties which are excluded from the estimation sample since they have only received Katrina-evacuation-related disaster declarations (Methods). D, E, F) Nonlinear effects of TCs on county-level per capita income growth based on the wind or rain data above. In D and E, we show both quadratic (solid) and cubic (dashed) specifications. In F, we show quadratic models that estimate the response of rainfall while controlling for the Baldwin et al. wind model (dash-dotted) or Jing et al. wind model (dotted). Shading shows 95% confidence intervals produced by bootstrap resampling by county with 1,000 iterations. Note that the y-axes differ based on the different relative effects of the wind models and rain data. X-axes span the 0th to 99th percentiles of each variable.

provide net benefits to income (Fig. 1D), whereas in the case of the Jing et al. model, these winds merely result in reduced losses (Fig. 1E). While our primary models are quadratic, we also show results using cubic models (Fig. 1D, 1E) to illustrate that our results are not solely due to an overly restrictive functional form.

By contrast, the income response to rainfall is much more linear, with very little rebound at high storm rainfall rates (Fig. 1F). A county exposed to 100 mm of TC rainfall in a given storm experiences approximately a 0.25-percentage-point reduction in income growth relative to experiencing no TC rainfall. This response is very similar when controlling for either of the two wind models.

Our primary metric of TC wind exposure is the maximum wind speed experienced at each grid point from a given storm. In the main analysis, we aggregate across storms each year by taking the maximum of these maximum wind speeds, yielding the highest wind speed experienced across any

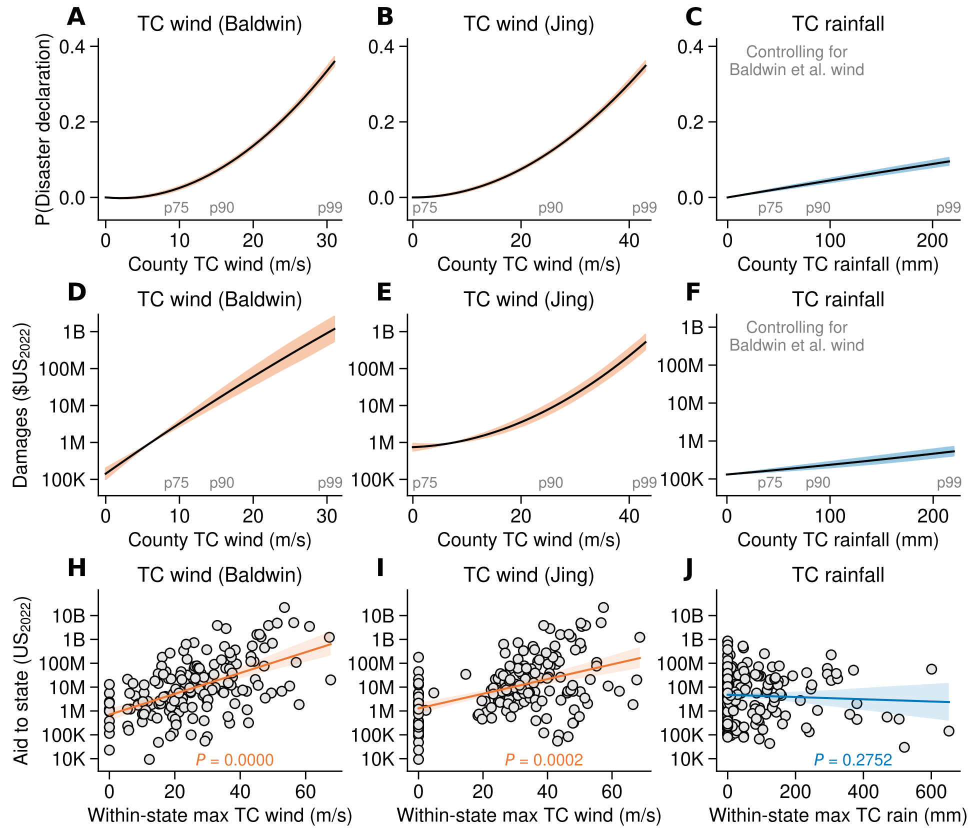

Figure 2: Effects of TCs on disaster response. A, B) Effect of TC winds on county-level disaster declaration probability, using the Baldwin et al. (A) and Jing et al. (B). wind models. C) Effect of TC rainfall on declaration probability. D, E, F) Effects of winds and rainfall on direct damages from SHELDUS (Methods). Note the log y-axis. In A through F, lower text marks the 75th, 90th, and 99th percentiles for each TC variable. H, I, J) Effects of within-state winds and rainfall on FEMA aid received by that state. Note the log y-axis. In all cases, solid line shows mean regression estimate and shading shows 95% confidence interval.

storm in a year at each grid point[7], which is then averaged across each county. Taking the sum or mean across storms yields more muted and non-statistically-significant responses (Table S1). We therefore infer that indirect impacts are driven primarily by the strongest storms in a given year rather than the accumulation of many less severe storms. This conclusion is consistent with findings of exponential increases in direct structural damages with wind speeds[6,39].

TCs prompt disaster response

One potential explanation for the nonlinearity in the income response to winds is disaster response: Stronger TCs might prompt discretionary responses by the local, state, or federal government that could help maintain or improve incomes among those living in affected areas. We analyze this issue in several steps.

First, we ask whether stronger TC exposure is associated with a greater probability of receiving a county-level disaster declaration. Indeed, we find that higher TC winds strongly and nonlinearly increase the probability of declarations, from nearly 0 at the low end to more than 40% probability at the high end (Fig. 2A, 2B). Strikingly, we find a much more muted response to rainfall, with 99th-percentile TC rainfall only being associated with a ∼10% probability of receiving a declaration (Fig. 2C).

One potential explanation for this pattern may be the high visibility and salience of direct wind-related damages. When we analyze the effect of TC winds and rainfall on direct infrastructure and property losses using the SHELDUS dataset (Methods), we find a strong and log-linear relationship between wind speed and direct damages (Fig. 2D, 2E). By contrast, higher rainfall is only weakly associated with greater direct damages (Fig. 2F).

Going beyond the presence or absence of a declaration, we also analyze the actual dollar values of disaster aid flowing to states. We aggregate across states for this analysis because aid is not always dispersed at the county scale, often going to state governments, municipalities, or individuals. We find that greater TC winds are strongly associated with greater quantities of disaster aid flowing to a state (Fig. 2H, 2I). This log-linear relationship means that the highest wind values generate exponentially greater aid than mild winds. Again, the rainfall response is different, with no strong correlation between within-state rainfall and aid going to that state. (Fig. 2J shows this for within-state maximum rainfall (i.e., the single highest of the county-level maxima). Fig. S3 shows that the same result holds when taking the state-wide average or sum of the county-level maxima.)

Disaster response affects income

The preceding results suggest that higher TC winds are more likely to generate disaster aid to compensate for the impacts of the TC. Next, we study whether receiving a disaster declaration affects the income of a county struck by a TC. We estimate additional regression models for income in which winds and rainfall are interacted with declarations (Methods). We find distinct responses in the presence or absence of a declaration in both wind models, with losses in counties that do not receive disaster declarations and benefits in counties that do (Fig. 3A, 3B). In the case of rainfall, declarations do not appear to produce net benefits, but do appear to erase the losses associated with rainfall exposure (Fig. 3C). This finding is consistent with the weaker and noisier response of aid dollars to rainfall (Fig. 2J); receiving a disaster declaration is still a prerequisite to receiving FEMA aid, but conditional on those declarations, the highest rainfall exposures are not systematically receiving more aid than lower rainfall.

Further, we find that the differentiation between the with- and without-declaration responses persists over time. Using a distributed lag (DL) model to assess the long-term effects of TCs with and without declarations (Methods), we again find a clear difference between counties that received Presidential disaster declarations and those that did not (Fig. 3D, E, F). When counties are not declared disasters, their income impacts are persistently negative. In the case of winds, these losses are not recovered even ten years later, while in the case of rainfall, the losses persist but appear to be slowly recovered after ten years. By contrast, when counties receive TC-related disaster declarations, they experience income benefits that are similarly maintained for at least a decade. It is notable that in the case of rainfall, there are long-term net benefits from declarations after several years (Fig. 3F), despite the response in the year of the storm only being to zero out the damages (Fig. 3C). This finding suggests that the long-run benefits of disaster aid may emerge subtly and slowly. In any case, the consistency in the with-declaration responses across both wind models and rainfall data suggest a robust finding of benefits from disaster aid.

These results are similar when using 5 or 15 lags instead of 10 lags in the DL model (Fig. S4). Our main results use bootstrapping by county to calculate confidence intervals (equivalent to clustering standard errors by county). Estimating the DL model with bootstrapping by state instead of county, to account for both spatial and temporal autocorrelation in growth, substantially expands the confidence intervals, but the Jing et al. wind model continues to yield negative impacts of TCs without declarations that are statistically distinguishable from zero after 10 and 15 years (Fig. S5).

One concern is that these results might not actually reflect the causal effect of disaster declarations on the income response to TCs, but rather the fact that different types of TCs or different regions could preferentially receive declarations. To examine this possibility, we require a source of variation in disaster declarations that is plausibly exogenous from the characteristics of a particular storm. We therefore leverage previous findings that disaster declarations are more likely when incumbent Presidents are running for reelection and in locations where the current President is politically aligned with the affected area[40–42] (Supplementary Text), factors that are plausibly unrelated to storm-specific factors that could trigger both declarations and affect recovery. We first predict declarations for each county and year as a function of these characteristics (Table S2). We then use these predicted declarations instead of observed declarations in the same regression model to assess how they moderate the impacts of TCs. We find that TCs with declarations predicted solely by political factors even larger benefits than our main results (Fig. S6), supporting the conclusion of a causal effect of declarations on income growth (Supplementary Text).

The role of social insurance

There are other potential hypotheses for the nonlinear response of income to TC winds. One is that direct monetary transfers through safety net programs such as unemployment insurance (UI) could compensate for lost income, with the benefits of strong storms thus reflecting increased income from social insurance payouts[43].

We do find larger effects when we exclude transfers from our measure of income, implying that transfers mitigate the negative income impacts of TCs. However, pre-transfer income is nonlinear in TC winds with a similar shape to post-transfer income, so direct transfers do not explain the overall

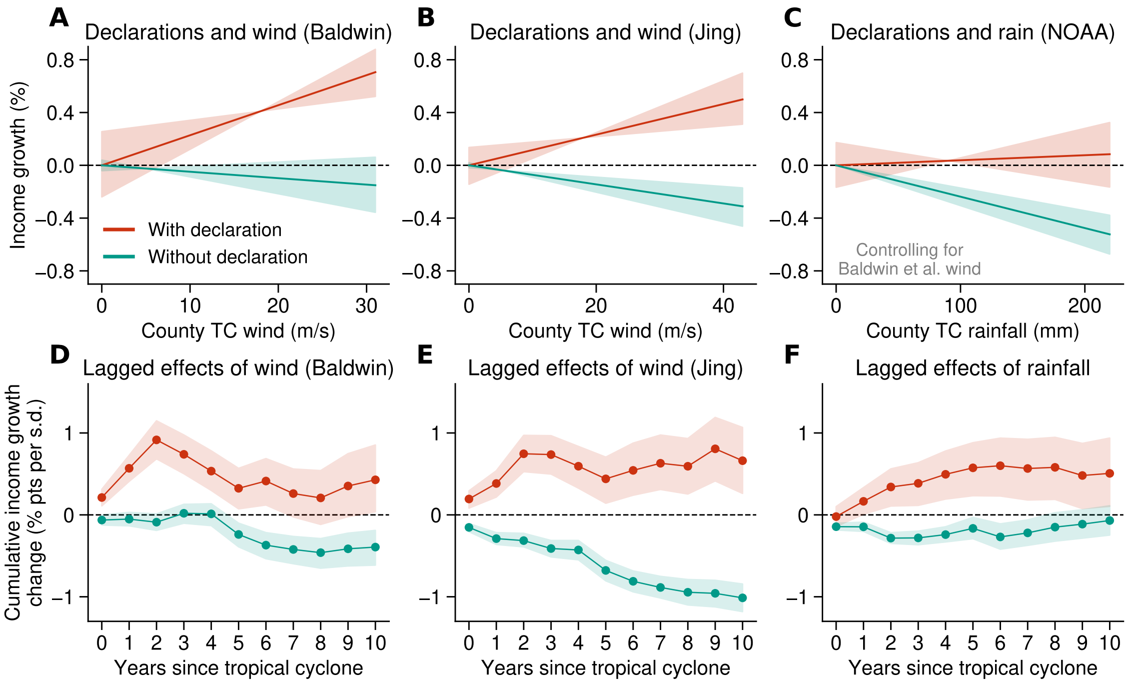

Figure 3: Moderating effects of disaster response on TC impacts. A, B, C) Linear impacts of TCs on county-level income following a storm in which a county received a Presidential disaster declarations (red) versus did not receive a declaration (green), based on wind field models from Baldwin et al. (A) and Jing et al. (B), or rainfall data (C). Confidence intervals are centered on the means of the distributions of county-level TCs with and without declarations. D, E, F) Cumulative effects of TCs on growth in the year of the cyclone and the ten following years, using a distributed lag regression model (see Methods). Shading shows 95% confidence intervals based on bootstrap resampling by county. Cumulative coefficients are presented in standardized units to compare across wind and rainfall.

nonlinearity (Fig. S7). Additionally, non-FEMA safety net transfers such as UI are not directly activated by Presidential disaster declarations in the same way that FEMA aid is. Thus, non-FEMA transfers are unlikely to explain the strong differentiation between the with- and without-declaration responses (Fig. 3).

Together, these results suggest that while social safety net transfers likely also play a role in the income impacts of disasters, they do not entirely our finding of the nonlinear response of income growth to TC winds or the differentiated effects with and without declarations. This finding differs slightly from that of Deryugina[43], though there are several reasons we might find distinct results (Supplementary Text).

The role of migration

An entirely distinct mechanism for our results would be differential migration: If TCs cause people to move in or out of affected counties, the average income of the county could be affected. Importantly, average income would only be altered if migration was differential according to income status (e.g., higher outmigration by lower-income people would increase the average income per person of the remaining people).

Testing this mechanism in our setting is difficult because there is virtually no publicly available source of fine-grained migration data that is also disaggregated by income. Instead, we take advantage of the recently developed MIGRATE dataset[44], which provides annual migration to and from each Census Block Group (CBG) within the U.S. over 2011–2019. A single county can have hundreds of CBGs within it, providing a way to estimate differential migration to and from a single county. We bin CBGs within counties by income using data from the American Community Survey, and compare the effects of TCs on migration in lower-income vs. higher-income CBGs within each county. We use the same DL model as in our earlier analysis, allowing us to examine both immediate and longer-term migration responses to TCs across income categories. The assumption in this analysis is that all income variation is across CBGs rather than within them; there is clearly some within-CBG income variation that is not captured here, but this approach provides a useful initial test of migration effects on income.

We find generally small effects of TCs on migration to and from CBGs (Fig. S8, S9). There is some evidence that TC winds increase outmigration from the poorest CBGs by 1-2 percentage points, but little difference across income groups for inmigration (Fig. S8). Results are more noisy for TC rainfall (Fig. S9), with virtually no effect when declarations are not issued and variable but very uncertain effects when declarations are issued. This result is consistent with other recent work finding small or moderate migration responses to storms, with Katrina being a striking outlier rather than an indication of a general pattern[45].

To understand how these effects might propagate into county-level income, we use data on the income of each CBG bin and calculate how the average county's income would change if TCs altered migration patterns based on the above results (Methods). Comparing these inferred income effects to our original results, we find that our wind results generally lie outside the bounds of the income changes that would be expected from migration alone (Fig. S10). On the other hand, the long-term rainfall responses are sufficiently small—and the effect of rainfall on migration is sufficiently noisy—that we cannot rule out that the rainfall effects are driven by differential migration (Fig. S10).

Long-term indirect costs exceed direct costs

The presence of persistent income losses from TC winds suggests that the long-term costs of TCs may substantially exceed their immediate direct costs. On the other hand, given the potential for the effects of rainfall to be explained by differential migration rather than actual income losses (Fig. S10), we refrain from calculating long-term costs of TC rainfall. (We emphasize that rainfall is still included as a control in the regression models that measure the effects of winds.)

We use the effects of TC winds shown in Fig. 3D and 3E to calculate long-term income losses due to all TCs between 1980 and 2019 (relative to a counterfactual in which those TCs did not occur), and accumulate their costs over that forty-year period. Using the Jing et al. wind model, the total indirect costs of TC wind from this calculation are approximately \$34 trillion (\$US$_{2022}$), with a 95% range of \$26-\$42 trillion due to uncertainty in the regression estimates (Fig. 4A, 4B). However, if no disaster declarations had been issued, income losses due to TCs would have been \$20 trillion greater since 1980, or ~58% greater than our main damage estimate (Fig. 4A). Observed losses are smaller, though still sizable, when using the Baldwin et al. wind model, with losses totaling \$14 trillion and an additional \$13 trillion saved by disaster declarations (Fig. 4B).

These indirect costs have accrued primarily to coastal counties, with incomes in 2019 reduced by 20-30% along the Gulf and Atlantic coasts due to TCs over the previous 40 years (Fig. 4C). The benefits of disaster declarations via indirect avoided income losses have been particularly large in coastal cities such as New Orleans, LA, and Mobile, AL, strongly reducing the harm to these cities relative to surrounding areas (Fig. 4C, 4D).

Based on data from the OpenFEMA database (Methods), we estimate that FEMA has spent \$153 billion on declared TC disasters since 1989, or nearly 150X less than the \$20 trillion in avoided income losses that we estimate occurred due to this aid. Given an average income tax rate of 14.9%[46], a back-of-the-envelope calculation yields ~\$2.9 trillion in tax revenue cumulatively gained from these income savings. While this calculation is simplistic (Supplementary Text), it suggests that the tax revenue gained from declared TC disasters substantially exceeds the total amount spent on those declarations. This calculation does not include spending through non-FEMA agencies such as Housing and Urban Development (HUD) or the Small Business Administration (SBA), but these sources of spending are likely small relative to income gains: all-time HUD disaster spending totals ~\$100 billion[47], and SBA disaster loans total ~\$60 billion[25], with TCs representing only one component of these totals. Adding these spending sources would not alter the core conclusion that the personal income saved from disaster declarations exceeds the money spent on those declarations.

These indirect costs of TCs are also much larger than total direct costs as tallied by disaster databases such as the NOAA billion-dollar-disasters database[1], EM-DAT[48], or SHELDUS[49], which put the cumulative 1980-2019 costs of all hurricanes at \$1.3 trillion, \$1 trillion, and \$270 billion, respectively (Fig. 4B). This result arises primarily because indirect losses appear to persist over time rather than recovering immediately after the storm, consistent with other analyses of TC impacts[7,8]. We emphasize that databases of direct disaster costs are often incomplete and subject to reporting biases[50,51], and extensive missing data has been documented in SHELDUS in particular[52]. However, this is unlikely to entirely explain our results; for example, the billion-dollar-disasters database is estimated to only underestimate TC-related losses by about 10%[1], which would not explain the magnitude of the difference between indirect and direct costs. Additionally, because these data sources are extensively used in academic and public discussions, they

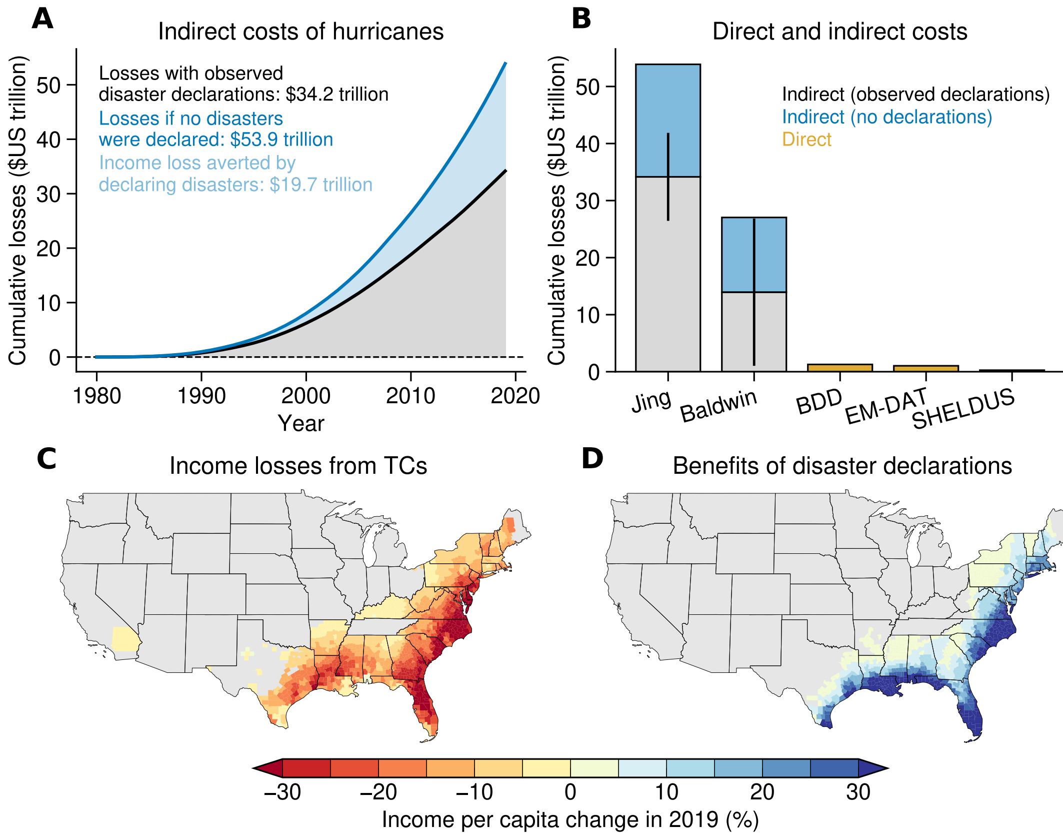

Figure 4: Long-term indirect and direct impacts of tropical cyclones. Cumulative income losses from TCs relative to a counterfactual without TCs over 1980–2019, based on the wind field data from Jing et al. The black line shows losses given observed disaster declarations, the blue line shows losses if no disasters had been declared, and the blue shading shows the difference between the two. B) Cumulative indirect and direct losses from TCs. Gray bars show losses with observed disaster declarations and blue caps show additional losses if no disasters had been declared, for the two TC wind fields. Yellow bars show cumulative direct costs from three disaster loss datasets. Error bars show the 95% range for estimates of losses using observed disaster declarations. C) Income change in 2019 relative to a counterfactual with no TCs. This calculation includes the benefits of declarations, so it corresponds to the black line in panel A. D) Income change in 2019 from disaster declarations, relative to a counterfactual with no declarations. This calculation corresponds to the light blue wedge in panel A. Maps in D and E use wind field data from Jing et al. Tennessee and other inland counties do not have damages data since they are excluded from the sample; see hatching in Fig. 1A and 1B and further discussion in Methods.

serve as an informative baseline for comparison with our results.

Discussion and conclusions

Our analysis has revealed several new facts about the impacts of tropical cyclones in the United States. We have illustrated a nonlinear response of county-level income growth to TC winds, with reduced income growth losses for TCs with the highest winds, a nonlinearity that has not been shown in previous studies on indirect TC impacts in the U.S.[14,53]. We show that beneficial disaster response following the strongest winds contributes to this nonlinearity and potentially explains the more linear response to rainfall. Our results are consistent with previous work showing micro-level benefits from disaster aid on individual debt[23], credit card outcomes[24], and long-run small business survival[25], but add to this literature by showing that disaster response can moderate long-run macroeconomic damages from TCs in the U.S. However, despite such benefits, our results also show that the long-run damages to personal income from TC exposure appear to far exceed previously-quantified direct damages to capital and infrastructure. While previous studies have shown globally persistent impacts of TCs on output[7,8], our results enable us to specifically compare indirect and direct impacts within the same region, filling an important gap outlined by previous synthesis reports[13].

Our results have implications for disaster policy and public finance, a topic of increasing importance given increases in extreme weather driven by global warming. We show that disaster declarations in response to TC winds generate avoided income losses that are much greater than the total outlays associated with those declarations. Indeed, the avoided income losses are so large that tax revenue from the income that otherwise would have been lost may compensate in full for those outlays. However, this calculation abstracts away from factors such as changes in tax incidence over time, varying tax burdens across the income distribution, and the potentially unequal distributional effects of TCs, so we leave a more detailed investigation of these tax implications for future work. Regardless, our results do suggest that expanding the scope of Presidential disaster declarations to less severe TCs and other hazards might avert additional losses that may be suffered in the future.

Our results also help reconcile previously disparate findings about the economic impacts of TCs by revealing that both losses and gains are possible given the response of decision-makers. Previous analyses of the macroeconomic impacts of natural disasters have generally not explicitly distinguished between situations with and without disaster response. However, our results show that this response has a strong influence on how local economies respond to TCs. This finding bears on other research measuring the economic response to disasters by using declaration data[20], illustrating that declarations do not necessarily represent the effects of disasters themselves, many of which do not receive declarations.

Overall, our finding of substantially greater indirect impacts from TCs relative to direct impacts adds to a growing literature highlighting the persistent and accumulating economy-wide costs of extreme weather events (e.g.,[7,33,35]). Given the potential for climate warming to increase the intensity of the strongest tropical cyclones[2–4], alongside their rainfall[54,55] and storm surge[56], our results suggest that without further investments in disaster response, the personal income impacts of these extreme events may be increasingly consequential to the U.S. economy writ large.

Methods

Tropical cyclone data

We measure the effects of two TC hazards on income: winds and rainfall. We choose these two because of the availability of long-term data on each one, allowing us to measure the effect of these hazards on income in U.S. counties.

For TC winds, we use parametric wind field models applied to TC tracks from the International Best Track Archive for Climate Stewardship[57] (IBTrACS). These wind field models allow us to quantify spatially explicit variation in wind exposure over time, including areas that were not directly struck by the TC track but still may have experienced damaging winds. Each model parameterizes the two-dimensional radial wind field using data on the central intensity of the cyclone (e.g., minimum central pressure or maximum wind speed) and the radius of maximum wind speed or outermost extent of wind. We use two different wind field models: One produced by Baldwin et al.[29], based on Willoughby et al.[58], and one by Jing et al.[30], which used the wind field models of Chavas et al.[59] and Chen et al.[60]. There are structural differences between these two models. Jing et al.[30] include a correction for the role of surface roughness in shaping the asymmetry of TC winds after landfall, meaning that winds from this model do not penetrate inland to the same degree as winds estimated by Baldwin et al.[29]. Examples of the 2005 and 2017 hurricane seasons illustrate that both models produce strong winds in coastal regions, but those winds decay more quickly inland in the Jing et al.[30] model (Fig. S1). Another difference is that in the Jing et al. model, when a storm's maximum wind intensity at the storm center drops below 34 knots, the storm is removed from the dataset. This choice does not significantly affect estimates of population exposure[30], but it does mean that wind speeds in the Jing et al. model might generally be higher than the Baldwin et al. model.

It is worth emphasizing that other studies of large-scale TC impacts almost exclusively focus on winds alone rather than other hazards such as rainfall and storm surge[6–8,12,17,29,31,61]. This is generally because winds are computationally tractable to model, with multiple wind field models incorporating some degree of model structural uncertainty[29,30,58–60]. In contrast, regionally or globally comprehensive data on TC rainfall and storm surge are much more difficult to assemble.

Here we advance on this previous work by incorporating data on TC rainfall from the U.S. National Oceanic and Atmospheric Administration (NOAA). These data are produced by meteorologists examining rainfall observations from weather stations and other distributed sources (e.g., CoCoRaHS), and overlaying these observations on weather maps of tropical cyclone pressure and wind fields. Some of the rainfall observations are then classified as TC-related rain, with some expert judgment used to exclude, for example, predecessor rainfall events not directly related to the storm footprint.

The rainfall data covers 1950–2020, across the full range of our analysis period, and includes

323 named storms over that time period along with 84 tropical or subtropical storms. Further information is available at:
`https://www.wpc.ncep.noaa.gov/tropical/rain/tcrainfall.html`.

TCs also generate coastal flooding from storm surge alongside winds and inland rainfall. Unfortunately, data limitations prevent us from explicitly incorporating storm surge into our analysis. To some degree, storm surge is correlated with wind speed, which has led other studies to use wind speeds as a proxy for these other impacts[6]. As large-scale data on storm surge exposure become available, future versions of our analysis could incorporate that information.

Our primary metric of TC exposure is the maximum wind speed or rainfall experienced at each location from a given storm. For winds, we follow other work[7] in aggregating across storms each year by taking the maximum of each storm's maximum wind, yielding the highest wind speed experienced across any storm in a year at each location. We calculate county-level winds by projecting each gridded wind field onto a shapefile of U.S. counties from the U.S. Census Bureau and calculating the average within each county. The rainfall data denote the total accumulated rainfall from each storm in a given year at individual weather station locations. We first average the stations within each county's borders for each year, and then take the maximum value across all storms to denote annual rainfall exposure. Most counties have between 2 and 9 stations within them reporting precipitation to this dataset in a given year.

Other work has found that minimum central pressure is a better predictor of TC damages than maximum sustained wind speed[62]. However, here we use wind as a spatially explicit representation of the entire field of TC exposure rather than simply a representation of the central intensity of the storm, allowing us to account for impacts across the footprint of each storm.

Economic and disaster data

We draw data on county-level personal income from the U.S. Bureau of Economic Analysis, based on administrative records of tax returns filed in each county[63]. Income is primarily composed of wages, but also includes income from owning a property or business, as well as government transfers such as social insurance. These data are available at an annual resolution from 1969 onwards, though we limit the period of analysis to end in 2019 to avoid the complexities associated with COVID-19. Growth in each year is calculated as the fractional difference in income relative to the previous year (which results in dropping 1969). We use growth instead of the level of income because income levels are highly autocorrelated through time, which may induce spurious regression results.

Data on presidential disaster declarations at the county level are taken from OpenFEMA (https://www.fema.gov/about/openfema/data-sets). Per the OpenFEMA terms and conditions, we note that our work is not endorsed by FEMA and the Federal Government and FEMA cannot vouch for the data or analyses derived from these data after they have been retrieved from the Agency's website.

We compare our analysis of income impacts with previous, independent estimates of direct damages. Our data on direct damages are drawn from EM-DAT[48], the NOAA Billion-Dollar-Disasters database[1], and SHELDUS version 22.0[49], and we examine each dataset separately in case they report different damages for the same storm. In SHELDUS, we use all county-level property and crop damages, adjusted to 2022 dollars, where the hazard is listed as "Hurricane/Tropical Storm." All three of these databases focus on direct damages at the time of the storms, and none account for longer-term income disruptions. SHELDUS damage data are drawn from the National Center for Environmental Information (NCEI) Storm Data reports, which in turn are drawn from the National Weather Service (NWS). The NWS gathers damage data from a variety of sources, such as the insurance industry, on-the-ground assessments made by emergency management agencies, and power utility companies. EM-DAT damage data are also drawn from NCEI. The Billion-Dollar-Disasters database gathers much of the same information, from sources including FEMA damage assessments, the National Flood Insurance Program, and the Insurance Services Office[1]. Despite these similar data sources, these databases can differ on the total losses attributable to TCs. One reason may be that SHELDUS lists "Property" and "Crop" damages specifically, whereas sources such as the Billion-Dollar-Disaster database may include losses due to short-term business interruptions and other potentially non-property-related damages.

Empirical strategy

We use a panel regression model with fixed effects to model income growth in county i and year t as a function of TC wind and rainfall, county-specific average characteristics, and country-wide and local trends:

$$g_{it} = \beta_1 W_{it} + \beta_2 W_{it}^2 + \theta_1 R_{it} + \theta_2 R_{it}^2 + \mu_i + \gamma_t + \tau_i t + \epsilon_{it} \quad (1)$$

Here g denotes growth, W denotes county TC winds, R denotes county TC rainfall, μ is a county fixed effect that removes all time-invariant county characteristics, γ is a year fixed effect that removes all country-wide shocks in each year, and τ is a county-specific linear time trend. Standard errors are clustered at the county level to adjust for autocorrelation within counties. The regression is estimated over 1970–2019, the period where our TC and economic data overlap.

The identifying assumption of Eqn. 1 is that TCs are as-if randomly assigned with respect to income, after accounting for time-invariant state characteristics, country-wide shocks, and county-level long-term trends. TCs are clearly not random in space, as states such as Florida are consistently exposed to a greater degree than states such as Minnesota. However, using fixed effects allows us to remove time-invariant average county characteristics and use only idiosyncratic within-county variation to identify the effects.

For the analysis in Fig. 2, we replace g in Eqn. 1 with either a binary indicator of a county-level disaster declarations (Fig. 2A–C) or the log of county-level direct damages from SHELDUS (Fig. 2D–F).

When we interact TCs with declarations (Fig. 3), we run the following linear model with an interaction between wind/rainfall and a dummy variable for a TC-specific disaster declaration (D):

$$g_{it} = \beta_1 W_{it} + \beta_2 W_{it} * D_{it} + \theta_1 R_{it} + \theta_2 R_{it} * D_{it} + \mu_i + \gamma_t + \tau_i t + \epsilon_{it} \tag{2}$$

In this case, β_1 and θ_1 describe the effect of TC winds and rainfall when D is zero, meaning when a disaster is not declared. β_2 and θ_2 describe the change in the effect of TCs when disasters are declared, meaning the actual marginal effects of TCs when disasters are declared is given by $\beta_1 + \beta_2$ for winds and $\theta_1 + \theta_2$ for rainfall.

Finally, to assess the long-term impacts of TCs with and without declarations, we modify the linear interacted model (Eqn. 2) to add lags of winds and declarations. Following previous climate-economy work[7,33,35,64], this approach allows us to track the effects of TCs both in the year of occurrence and the following years, allowing us to distinguish between transient and persistent impacts:

$$g_{it} = \sum_{L=0}^{j} \left[\beta_{1L} W_{i(t-L)} + \beta_{2L} W_{i(t-L)} * D_{i(t-L)} + \theta_{1L} R_{i(t-L)} + \theta_{2L} R_{i(t-L)} * D_{i(t-L)}\right] + \mu_i + \gamma_t + \tau_i t + \epsilon_{it} \tag{3}$$

In Fig. 3, we present the sum across the lags of the coefficients (β and θ). A sum of marginal effects that is significantly different from zero implies persistent growth effects, where a sum that cannot be distinguished from zero implies that we cannot reject a hypothesis of only transient and not persistent effects.

The FEMA disaster declaration data contain a description of the "Hazard" for each declaration. When we interact declarations with TC winds, we use the disasters listed as for "Tropical storms," "Typhoons," or "Hurricanes." On the other hand, for the interaction with TC rainfall, we use declarations listed as for "Flood" or "Dam/Levee Break." Fig. S11 shows a version of Fig. 3 where we test this choice, using TC-related declarations for the entire analysis instead of using flood-related declarations for rainfall. We find that the interaction between rainfall and TC-related declarations is much weaker than the interaction between rainfall and flood-related declarations, suggesting that our main analysis is capturing the most important signal of declarations in response to TC rainfall.

Migration analysis

Our migration data come from MIGRATE[44], a new dataset providing fine-grained annual migration estimates between Census Block Groups (CBGs) over 2010–2019. MIGRATE is produced by combining proprietary individual-level observations from the data aggregator Infutor with coarser but publicly available Census data. The resulting dataset has both in- and out-migration estimates, and we maintain the separation between in- and out-migration throughout the analysis.

This data is not disaggregated by income, making it imperfect for our application, but it is spatially fine: CBGs are one level more disaggregated than census tracts (and much finer than counties), and we have data on 217,289 unique CBGs across the U.S. Additionally, the 5-year American Community Survey (ACS) provides data on the income per capita in each CBG.

In an ideal world, we would observe the effect of TCs on the propensity of each individual to migrate into or out of a county, along with the income of that individual. In the absence of such ideal information, our approach is to proxy for income-heterogeneity in migration by using heterogeneity across CBGs with different incomes. Within each county, we group CBGs into five bins by their income per capita (bottom 20% poorest CBGs, 20th-40th percentile, etc.) and calculate population-weighted in- and out-migration rates for each bin. We then use these migration rates as the dependent variable in a distributed lag regression model exactly equivalent to Eqn. 3. The results of this analysis are shown in Figs. S8 and S9. We drop 2005 from these regressions, given the strong evidence for Katrina being an outlier from other TCs in this regard[45].

To assess how the migration responses to TCs affect income, we take the average income per capita and population in each of the five bins of CBGs, and combine them with the bin-specific migration responses shown in Figs. S8 and S9. This approach approximates the income distribution across CBGs within the average county. By applying the in- and out-migration responses to the population in each bin, we calculate how each bin's population would change in response to a TC. We then calculate the overall income per capita in the average county by taking the population-weighted average of the income per capita in each of the five CBGs within the county. The result of this calculation is a change in county-level income that results only from changing the population of the CBG categories within the county via migration. This result is shown in Fig. S10.

Calculating long-run damages

We calculate long-run indirect losses from TCs by comparing observed TCs with a counterfactual scenario in which all county-level TC winds were set to zero. For each county, we apply the lagged response function shown in Fig. 3 to observed and counterfactual TC winds and difference them to calculate the change in growth due to observed TCs. We add this change back to observed growth to calculate counterfactual growth in the absence of TCs, and we re-integrate county-level income from growth in this counterfactual scenario. Further details on this integration procedure can be found in Diffenbaugh and Burke[65] and Callahan and Mankin[34].

We calculate damages over 1980-2019, rather than the initial analysis period of 1970-2019, since several of the direct damages data sources are only available starting in 1980.

In the main version of this analysis, we use observed disaster declarations, so county-year TC observations with declarations yield benefits instead of costs. We conduct an additional version of this analysis where we set all declarations to zero, and re-calculate long-term cumulative losses. The additional losses if no disasters were declared represent the income losses avoided by observed disaster declarations.

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Competing interests

The authors declare no competing interests.

Data and code availability

Data and code that support the findings of this study will be made available upon publication at [xxx].

Supplementary Materials

Supplementary Text

Explaining declarations with political factors

Our main results show that TCs that receive disaster declarations yield economic benefits, whereas those that do not yield losses. However, it is possible that this result does not reflect the causal effect of the disaster declaration, but instead that different types of TCs or different regions preferentially receive declarations. To examine this possibility, we leverage previous findings that disaster declarations are shaped by political factors: Declarations are more likely when incumbent Presidents are running for reelection, and Presidents are more likely to issue declarations to areas that are politically aligned with their party[40–42]. We use these factors to predict the probability of declarations as a function of political factors that are plausibly exogenous from the characteristics of individual TCs. Specifically, we estimate the following logit model:

$$D_{it} = \beta_1 reelection\_yr + \beta_2 stafford + \beta_3 dem\_president * dem\_share + \mu_i + \epsilon_{it} \tag{4}$$

Here, "reelection_yr" is 1 if the incumbent president is up for reelection in a given year, "stafford" is a dummy variable for whether the year is after 1988, "dem_president" is 1 if the President is a Democrat, and "dem_share" is the state-level share of votes for the Democratic president in the most recent Presidential election. μ is a county fixed effect. We include the "stafford" variable because the Stafford Act of 1988 gave the President much greater unilateral power to declare disasters. We only use state-level vote share data, so while we predict declarations in each county, we cluster standard errors at the state level since that is the level of treatment assignment. Results from this regression model are shown in Table S2. (We run it separately for TC-related and flood-related declarations, and show both in Table S2).

We use a logit model instead of ordinary least squares in Eqn. 4 because we are interested in predicting declarations, which should not be less than 0 or greater than 1. We then use the predicted values from this regression (i.e., $\hat{D_{it}}$), and input them into the distributed lag interacted model in Eqn. 3. We again find benefits with declarations (Fig. S6). The fact that declarations provide benefits even when they are solely motivated by political incentives rather than the characteristics of a TC supports our conclusion of a causal effect of declarations on income growth.

Alternative sample choices

Our main analysis uses a sample of ∼1300 counties in the eastern United States over 50 years (1970-2019). For both wind models, we define two criteria for counties to be included in the sample:

- The county must have experienced at least one TC wind observation greater than 0.

- The county must not have experienced only a TC-related disaster declaration due to Hurricane Katrina evacuees in 2005.

We include the latter criterion because Hurricane Katrina produced a unique set of declarations: thousands of counties that were not struck by the storm itself received Presidential emergency declarations if they received evacuees from New Orleans. This resulted in Hurricane Katrina receiving the largest number of disaster declarations of any natural disaster. For our purposes, these unique declarations may not be representative of the effects of other disaster declarations, which typically aim to mobilize resources directly to affected areas. Therefore, we drop counties for which the only disaster declaration was listed as "Hurricane Katrina Evacuation" or "Hurricane Katrina Evacuees" in the OpenFEMA data (we preserve declarations which are directly listed as for "Hurricane Katrina"). We note that other work has also made the same choice to limit Katrina-evacuation-related declarations out of their sample[42].

The final analysis therefore contains two samples, one for each wind model, comprising 67,100 observations for the Baldwin et al. wind model and 66,600 observations for the Jing et al. wind model. For both wind models, the sample is shown by the colored and unhatched counties in Fig. 1A and 1B. And in both cases, the panel remains balanced; that is, we either drop all observations from a county or preserve all observations from a county, rather than allowing different counties to have different numbers of observations.

In Figures S12 and S13, we show the implications of relaxing these sample restrictions. Fig. S12 shows distributed lag results if we still require counties to have experienced wind exposure, but do not drop counties which only experienced Katrina-evacuation-related declarations. Fig. S13 shows results if we simply include all 2,948 counties for which we have data in the U.S. In all cases, we see consistently negative impacts that last for at least 15 years from TCs without disaster declarations, similar to our main results. However, in both of the more expansive samples, the with-declaration response becomes unstable across lags and can vary in sign, even within the same wind model. The fact that our DL results are stable and consistent when excluding the one-time evacuation-related declarations, but inconsistent and unstable when including them (Fig. S12, S13), leads us to conclude that these Hurricane Katrina-related evacuation declarations may not be representative of the broader effects of disaster aid.

Comparison between our findings and Deryugina (2017)

Deryugina[53] found that, following hurricanes, social safety net transfers such as unemployment insurance are much greater than direct disaster aid. Our results are not necessarily inconsistent with this finding; we do find that safety net transfers mitigate the income effects of TCs (Fig. S2). (To our knowledge, our data includes generally the same set of transfer payments as Deryugina's sample, including unemployment insurance, the Earned Income Tax Credit, the Supplemental Nutrition Assistance Program, and Supplemental Security Income. The full set of payments categorized as transfers is listed in Part V of reference[63].)

However, transfers do not appear to fully explain the nonlinearity of these income effects. It is possible that the public assistance component of disaster aid creates broader spillover effects that exceed those of individual safety net transfers, such as by allowing municipalities to repair infrastructure or public buildings[23]. Additionally, there is substantial disaster-related spending outside of FEMA channels, such as through the Department of Housing and Urban Development[66] and the Small Business Administration[25]. It is likely that both our analysis and that of Deryugina underestimate the total amount of disaster aid flowing to affected counties.

Deryugina[53] also found that earnings do not change significantly following hurricanes. There are several differences in our analysis that may explain this apparent discrepancy. Deryugina used only the radius of maximum wind to measure TC exposure, which is a relatively small area around the eye of the storm. Our radial wind fields encompass a greater area of exposure[29,30]. This difference is especially important given that the areas treated as exposed in Deryugina's work are a small set of coastal counties (Fig. 1 in[53]), often the same counties that are receiving disaster declarations in our data (Fig. 4D), which may counteract the effects of TCs. By using a model that does not incorporate the offsetting effect of disaster aid, it is possible that Deryugina's empirical approach was not able to identify the income effects that we find.

Calculating tax revenue from avoided income losses

We estimate that disaster declarations have avoided \$19.7 trillion (\$US$_{2022}$) in lost income between 1989 and 2019. We begin this calculation in 1989 because that is the first year we have data on FEMA spending, to enable an appropriate comparison between money spent and income loss avoided. The nonprofit Tax Foundation estimates that the average income tax rate in 2021 was 14.9 percent[46]. Multiplying 19.7 trillion by 0.149 yields potential tax revenues of \$2.9 trillion. We emphasize that this calculation is simplistic, since it ignores changes in tax incidence over time, varying tax burdens across the income distribution, and varying impacts of TCs across the income distribution. Nevertheless, we believe it is a credible initial estimate for evaluating the magnitude of this benefit-cost ratio.

Treatment of Virginia income data

The state of Virginia has 95 official counties as well as 38 independent cities which are considered equivalent to counties. In their construction of county-level income data, the Bureau of Economic Analysis aggregates some of these smaller counties and cities into combined entities that do not match official county borders from the U.S. Census Bureau[63]. To match our county-level TC wind data to the income data for Virginia, we divide the income and population from these combined entities equally among the individual cities and counties that comprise them. Dropping these imputed counties does not substantially change our regression results (Table S3), but this analytical choice allows us to include all counties in Virginia in our analysis rather than dropping some of them due to a mismatch between the wind data and income data.

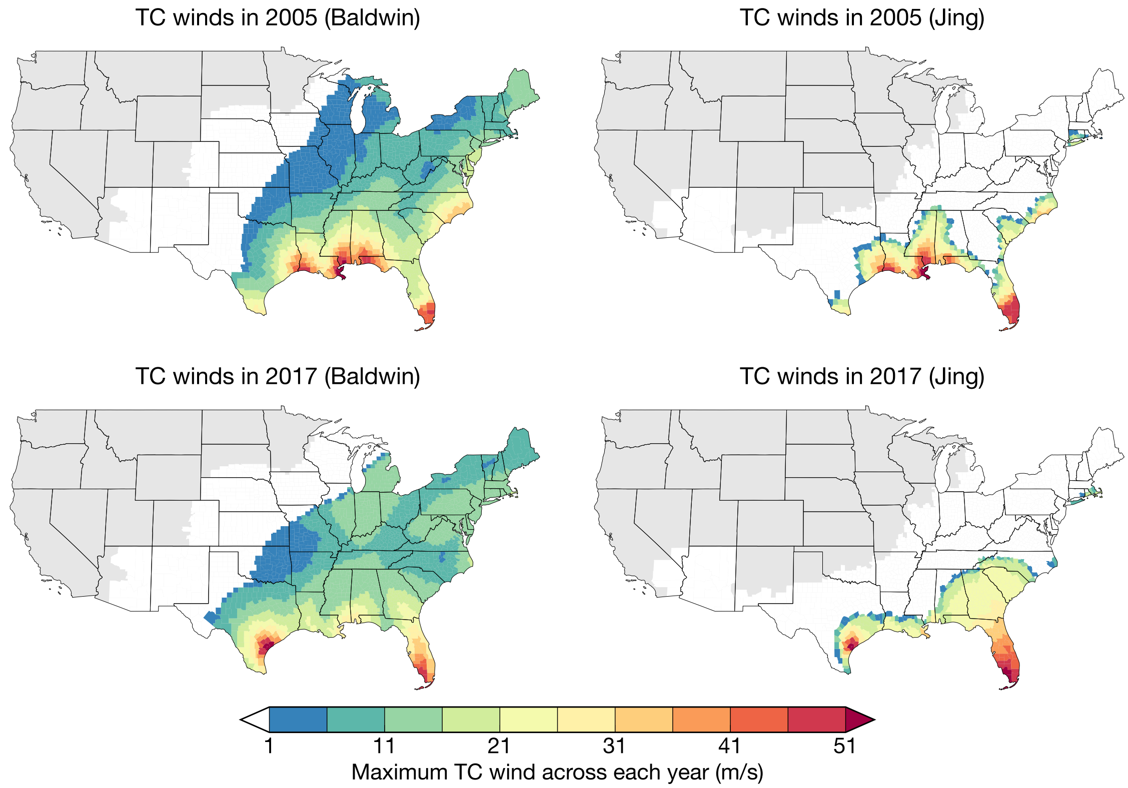

Figure S1: Example TC winds from both wind models. Each map shows maximum TC winds across all storms in a year for 2005 (top row) and 2017 (bottom row). Left column shows winds from the Baldwin et al. wind field and right column shows winds from the Jing et al. wind field. White denotes counties experiencing less than 1 m/s of wind and gray denotes counties which are not included in the sample (see Methods). 2005 was chosen for the example of Hurricane Katrina, which primarily struck Louisiana, and 2017 was chosen for the example of Hurricanes Harvey, which primarily struck Houston, and Irma, which primarily struck Florida. (Note that Puerto Rico is not included in our data, which was most directly impacted by Hurricane Maria during the 2017 Atlantic hurricane season.)

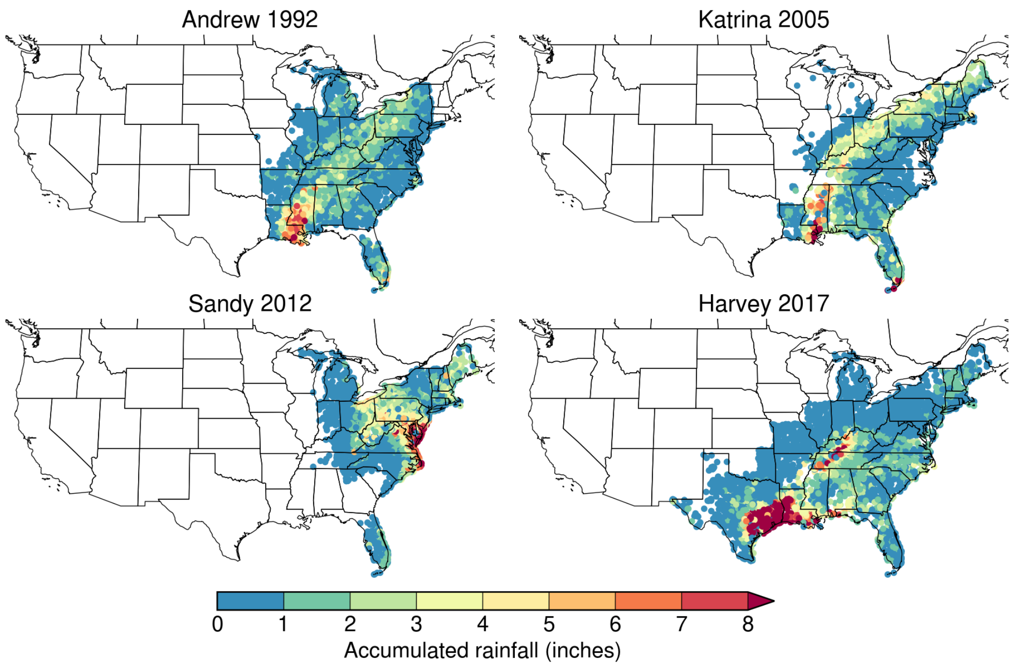

Figure S2: TC rainfall estimates for four example storms. Each map shows accumulated TC rainfall at weather stations across the United States for four example storms. Data comes from NOAA (Methods).

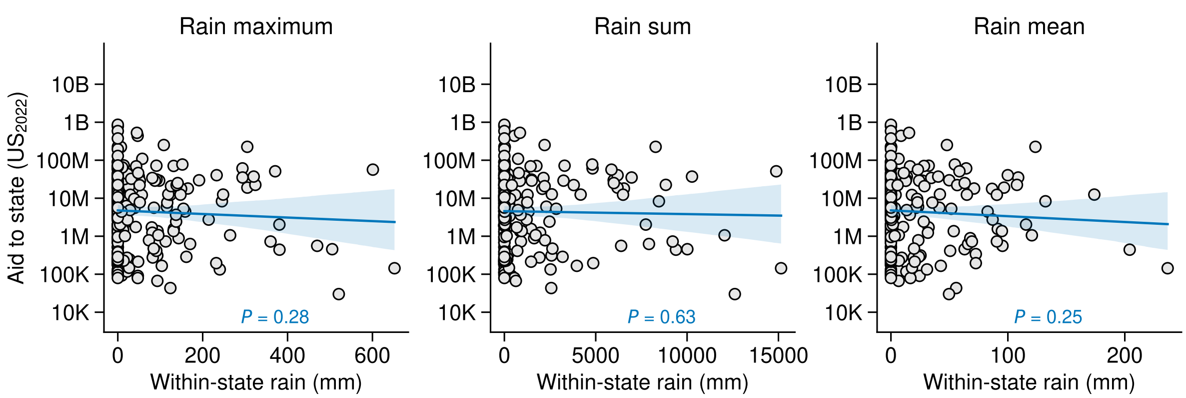

Figure S3: Effect of within-state rain on disaster aid to state. FEMA disaster aid in each state and year plotted against within-state rainfall, aggregated as the state-wide maximum of the county-level maxima (left), state-wide sum of county-level maxima (middle), and state-wide mean of county-level maxima (right).

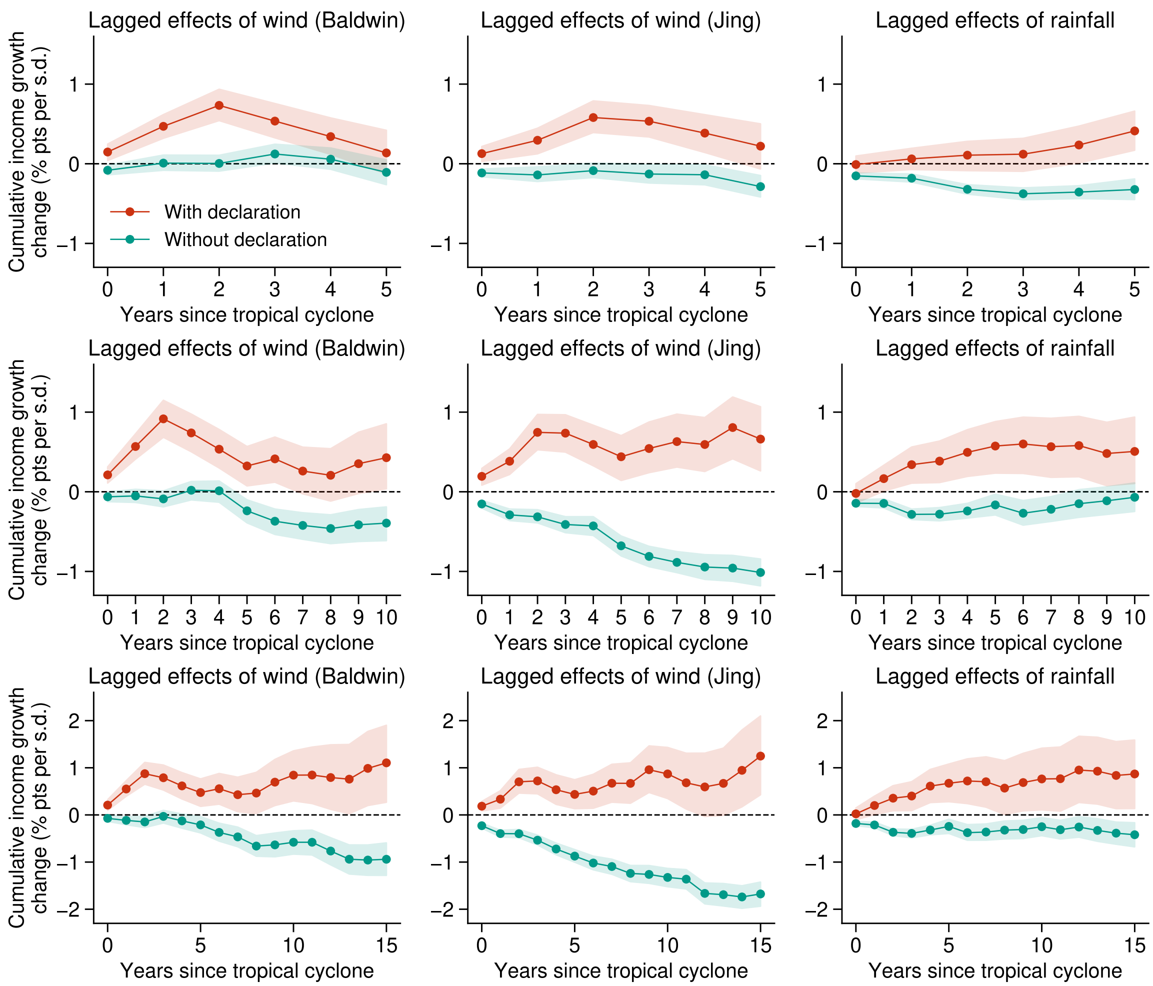

Figure S4: Persistent effects of TCs at different lag lengths. Each panel shows the effect of TCs with and without Presidential disaster declarations (red and green, respectively). Top row shows results from models with 5 lags, middle row shows results with 10 lags (as in main text Fig. 3), and bottom row shows results with 15 lags.

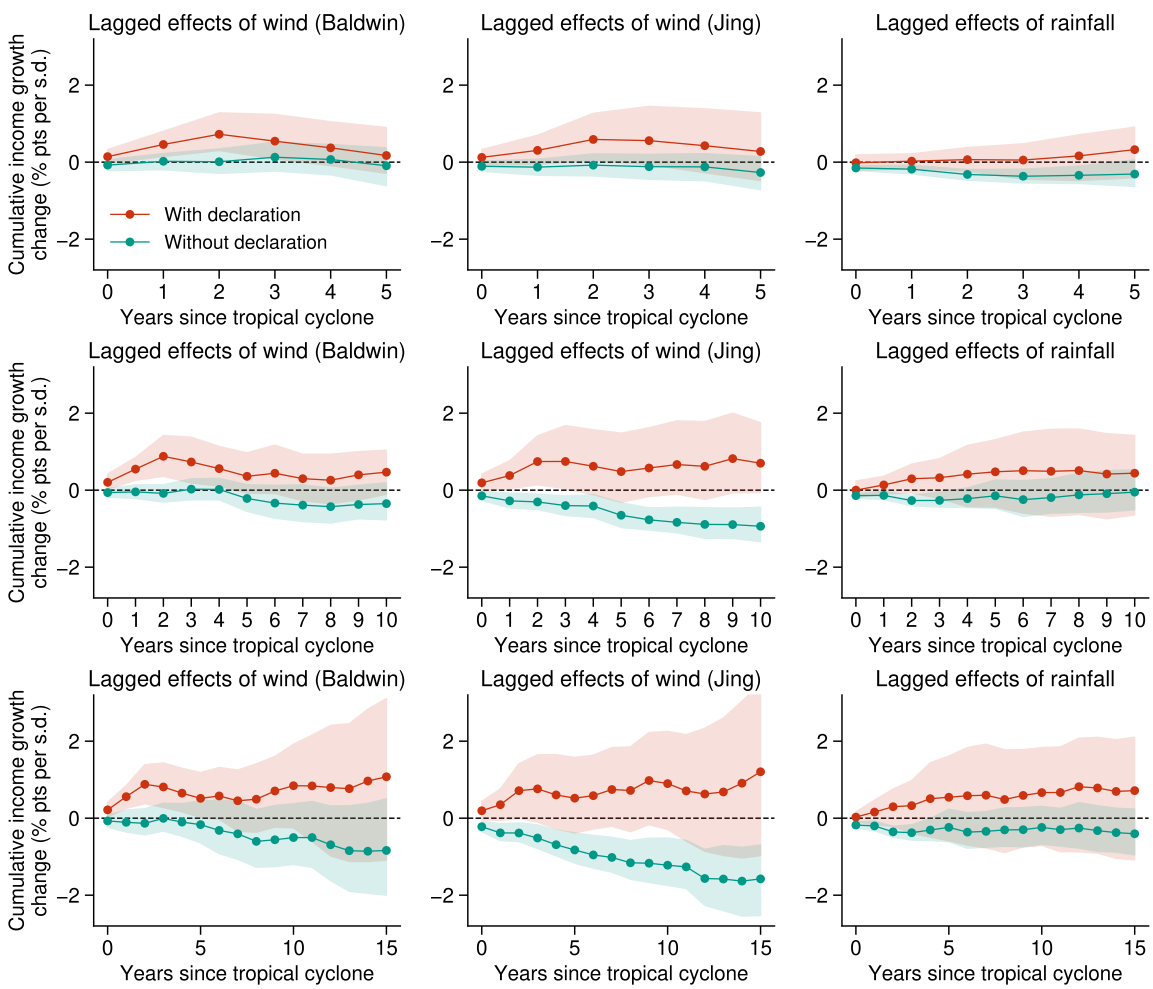

Figure S5: Persistent effects of TCs when bootstrapping by state. As in Fig. S4, but when bootstrap resampling by state and county.

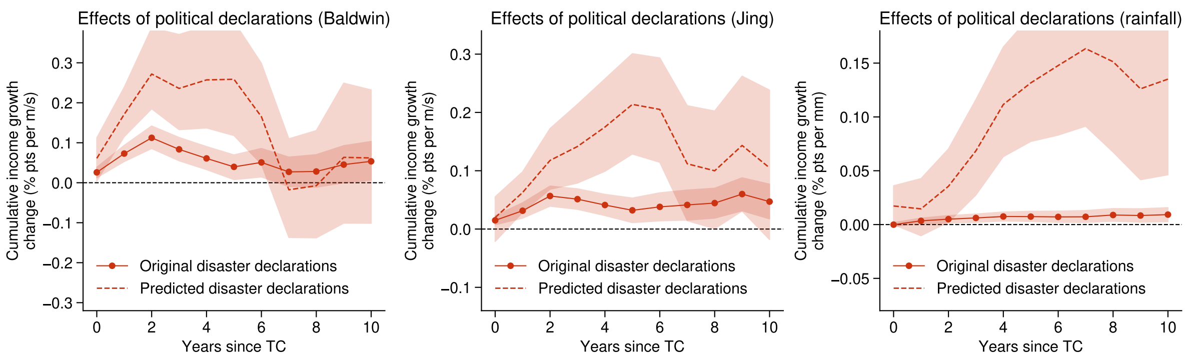

Figure S6: Original and predicted effects of disaster declarations. Panels show the effects of TCs when TC-related Presidential disaster declarations occur for the Baldwin et al. wind model (left) and Jing et al. wind model (middle), and flood-related disaster declarations for TC rainfall (right). Solid lines show results from a distributed lag model with 10 lags using observed disaster declarations, and dashed line shows the same model where declarations are predicted by solely political factors (Methods and Table S2). Shading shows 95% confidence intervals calculated by bootstrapping by county. Note the different units for the different exposures: m/s in the first two panels for wind and mm in the third panel for rainfall.

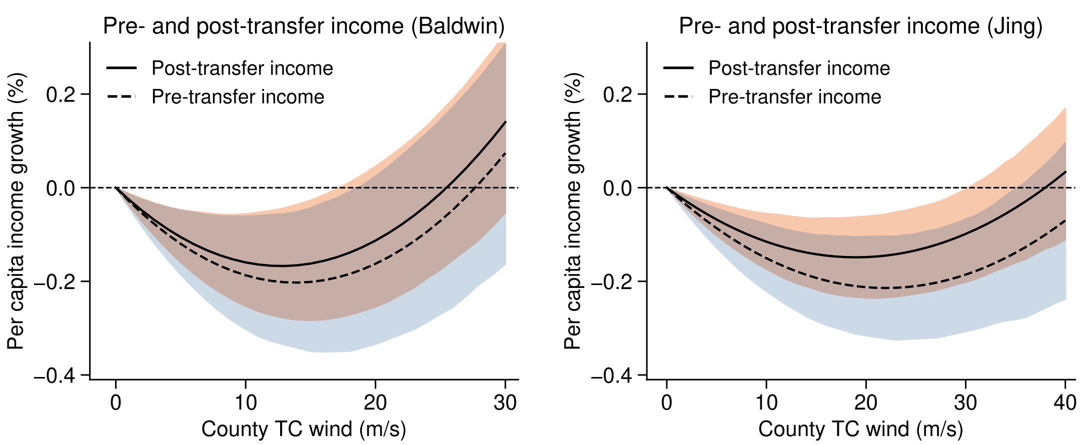

Figure S7: Effects on pre- and post-transfer income. Our result in the main analysis uses total post-transfer income, which is shown here in the solid line for both wind models. Dashed line shows the effect of TC winds on pre-transfer income, meaning income excluding unemployment insurance, Social Security benefits, medical benefits, and veterans' benefits. Shading shows 95% confidence intervals calculated by bootstrapping by county. Quadratic model is used in all cases.

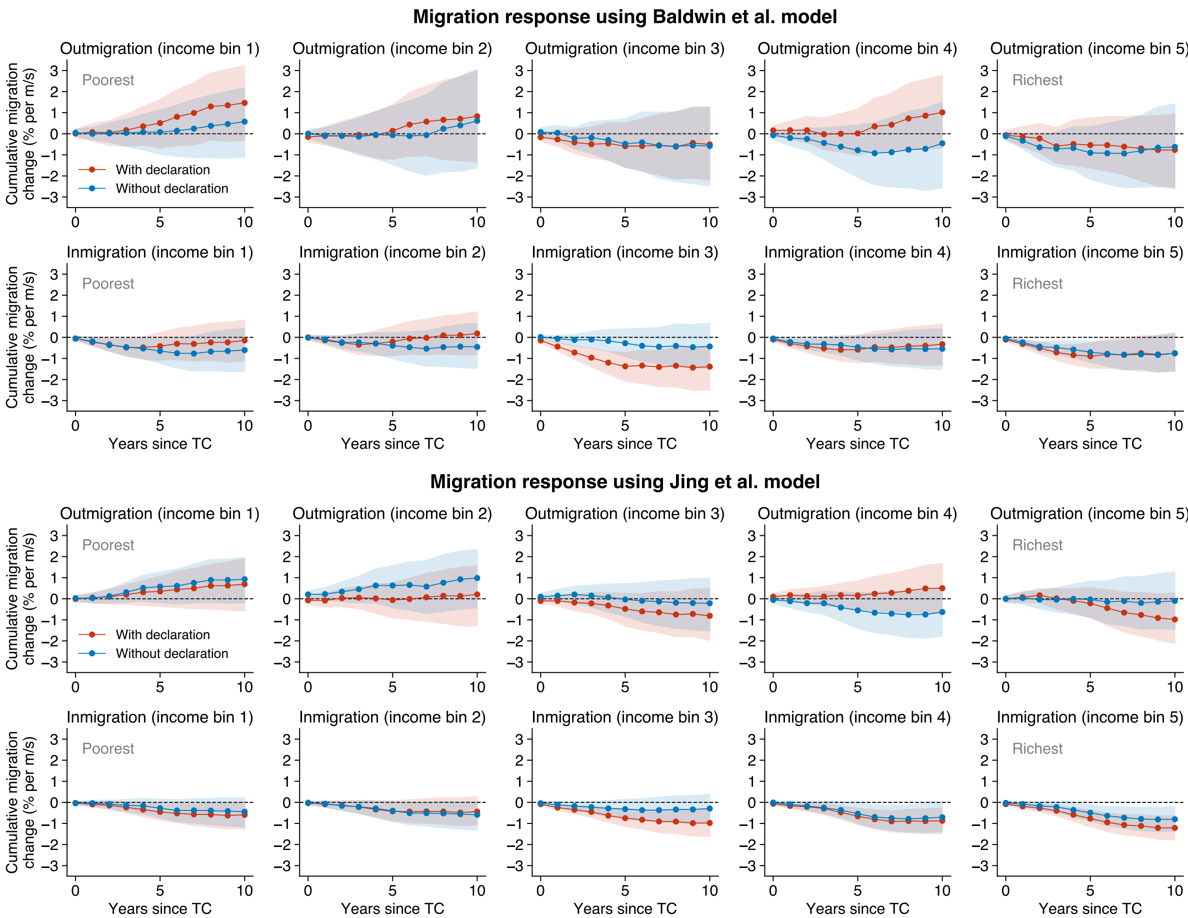

Figure S8: Effects of TC winds on migration across census block group bins. Each panel shows the effect of TC winds on migration with (red) and without (blue) disaster declarations for 10 years after a TC using a distributed lag model (Methods). Each panel shows these effects separately for within-county income bins, from the poorest 20% (left) to the richest 20% (right). The top two rows show out- and in-migration using the Baldwin et al. model, and the bottom two rows show out- and in-migration using the Jing et al. model.

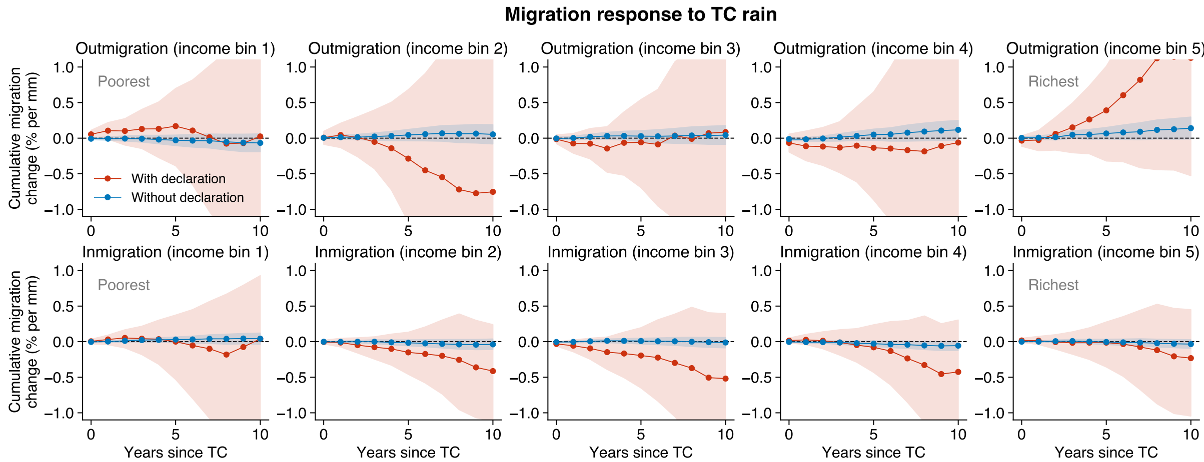

Figure S9: Effects of TC rainfall on migration across census block group bins. As in Fig. S8, but for TC rainfall.

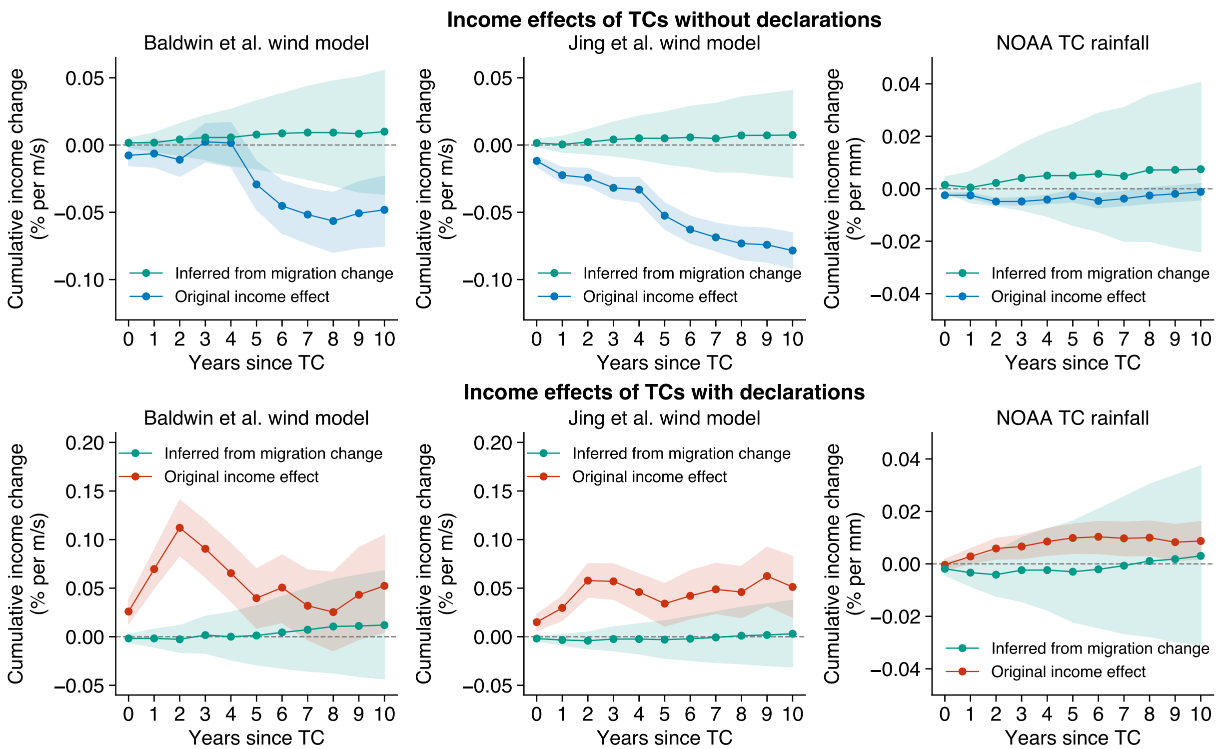

Figure S10: Original vs. migration-driven effects on county-level income. Each panel shows the effect of TCs on county-level income from our original results (blue/red) and inferred from migration change (green). Top row shows effects without declarations, for the Baldwin et al. wind model (left), Jing et al. wind model (center), and rainfall (right). Bottom row shows the same effects with declarations. See Methods for details on inferring income change from migration. Note the different units for the different exposures: m/s in the first two columns for wind and mm in the third column for rainfall.

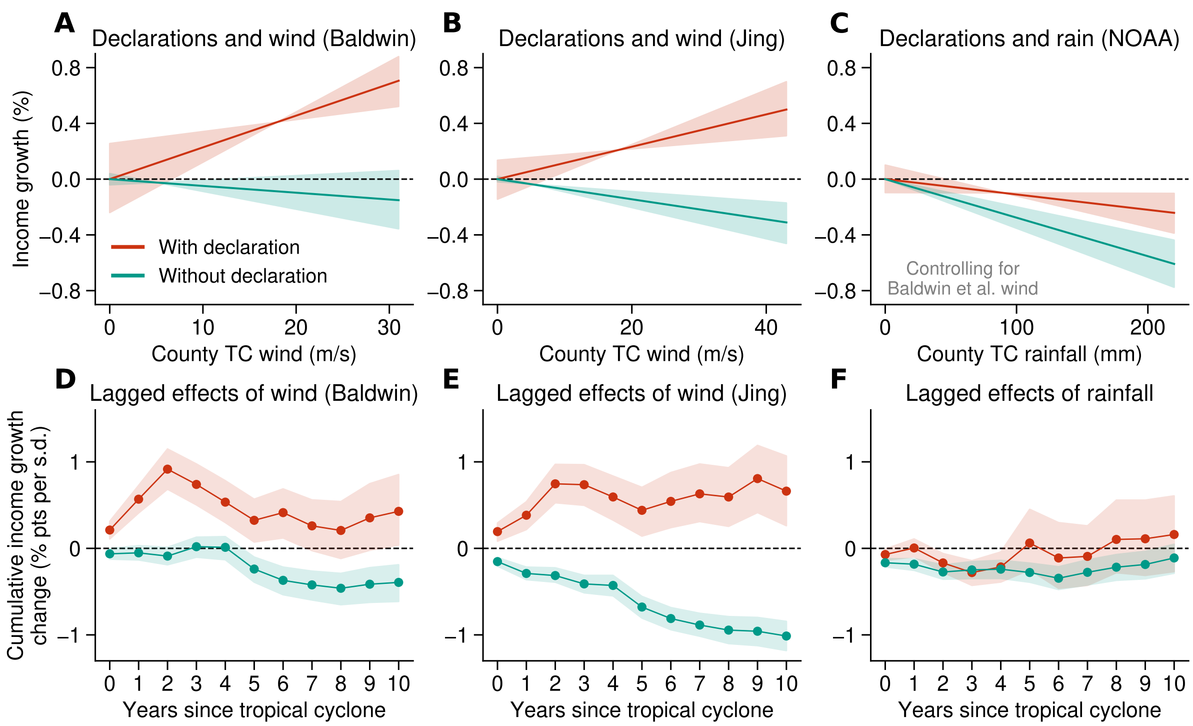

Figure S11: Results from interacting TC rainfall with TC-related declarations instead of flood-related declarations. As in Fig. 3, but the third column (for TC rainfall) contains an interaction between rainfall and declarations listed as for TCs instead of for floods in the Open-FEMA database.

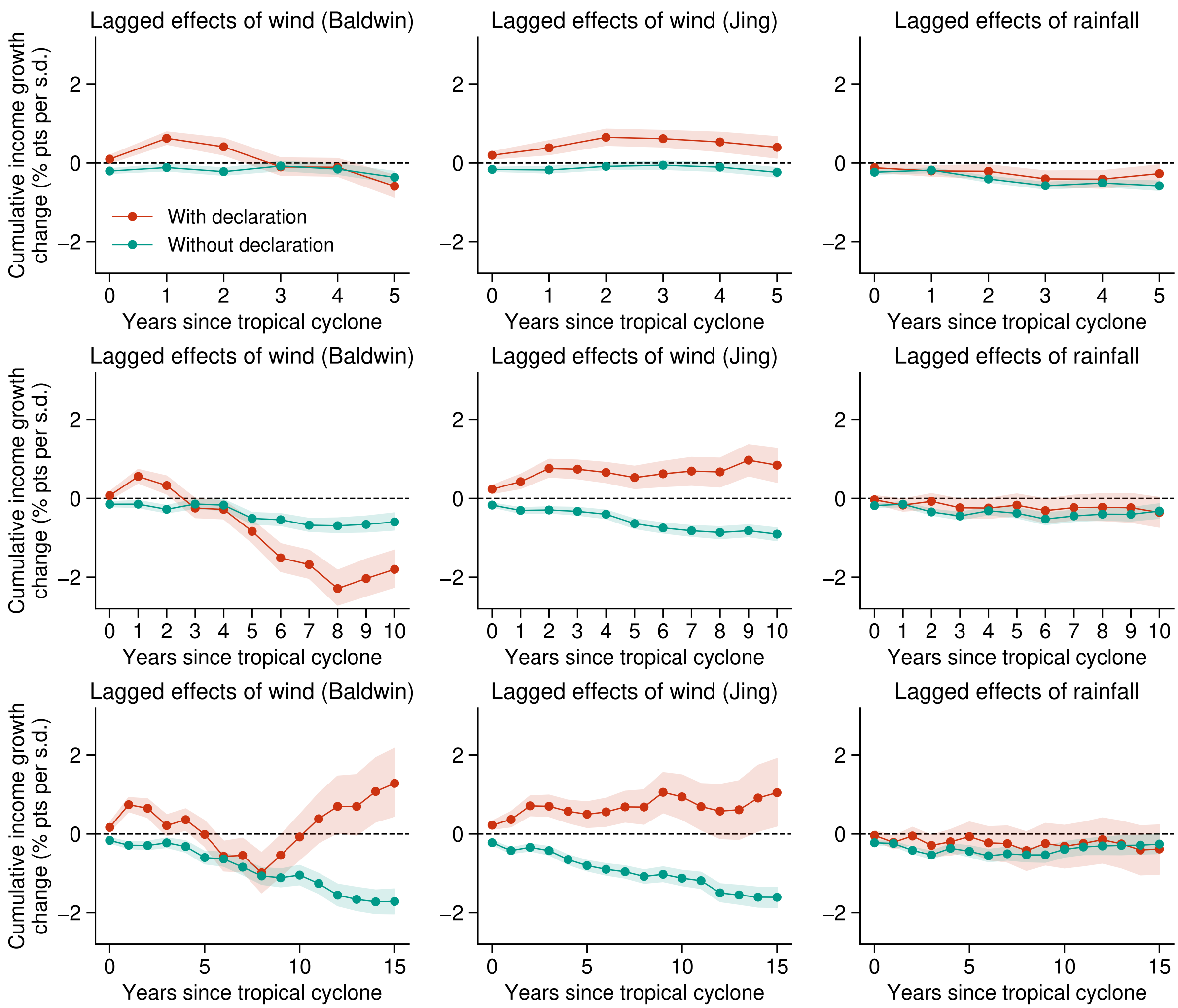

Figure S12: Effects of TCs including Katrina-evacuation-related declarations. As in Fig. S6, but including counties whose only disaster declaration occurred in 2005 and was listed as for "Hurricane Katrina Evacuation" or "Hurricane Katrina Evacuees." See Methods for details.

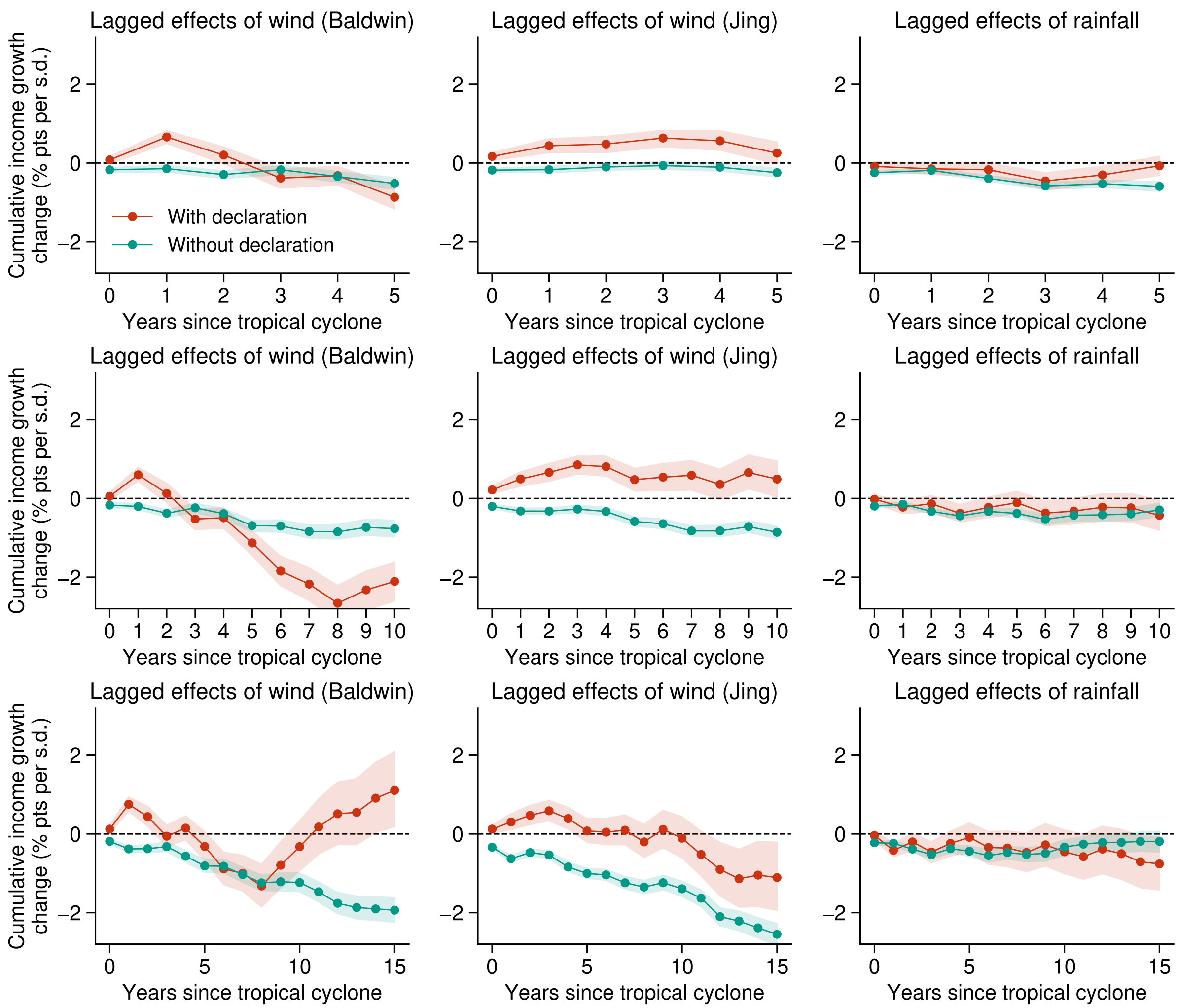

Figure S13: Effects of TCs including all counties. As in Fig. S6, but including all 2,948 counties for which we have data regardless of wind exposure or experience with disaster declarations. See Methods for details.

	(1)	(2)	(3)
Wind max	-0.02640^{***}		
	(0.00633)		
Wind max^2	0.00103^{***}		
	(0.00017)		
Wind mean		0.03261	
		(0.02094)	
Wind mean^2		-0.00031	
		(0.00167)	
Wind sum			-0.00015
			(0.00283)
Wind sum^2			0.00012^{***}
			(0.00003)
Rain max	-2.79929^{***}	-3.22989^{***}	-3.49912^{***}
	(0.55368)	(0.55847)	(0.55076)
Rain max^2	0.00256	0.00367^{**}	0.00364^{**}
	(0.00137)	(0.00141)	(0.00141)
Observations	67100	67100	67100

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table S1: Effects of TC winds on per capita personal income growth using three different annual wind aggregations and the wind field from Baldwin et al. We take the maximum wind speed from each storm, and aggregate to the annual level by either taking the maximum across storms (column 1), average across storms (column 2), or sum across storms (column 3). County and year fixed effects and county trends are included in all models and standard errors are clustered by county. Regression coefficients are multiplied by 100 so they are directly interpretable as percentage points. Rain is in units of meters rather than mm.

	(1)	(2)	(3)	(4)
Reelection year	0.7311^{**}	0.7350^{**}	0.4549	0.4619
	(0.2480)	(0.2458)	(0.4008)	(0.3973)
Post-1988	3.4923^{***}	3.4732^{***}	0.1287	0.1092
	(0.3243)	(0.3216)	(0.2171)	(0.2141)
Dem. president	-5.4354^{***}	-5.4324^{***}	-2.7099	-2.6258
	(1.2972)	(1.2998)	(1.3856)	(1.3685)
State Democratic presidential vote share	1.1906	1.1287	-6.3835^{**}	-6.3451^{**}
	(2.6363)	(2.6411)	(2.1362)	(2.0999)
Dem. president $\times$ Dem. vote share	9.3229^{***}	9.3172^{***}	7.1832^{*}	7.0248^{*}
	(2.6749)	(2.6776)	(2.8974)	(2.8666)
Observations	58960	58564	38896	38764
Wind model	Baldwin	Jing	Baldwin	Jing
Hazard type	TC	TC	Flood	Flood

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table S2: Effect of political factors on the probability of a county-level disaster declaration, calculated using a logit model. Column (1) uses the sample associated with the Baldwin et al. wind model and column (2) uses the sample associated with the Jing et al. wind model (see difference in number of observations). The models are otherwise identical. Columns (1) and (2) assess the effect on declarations listed as for hurricanes or tropical cyclones, while columns (3) and (4) assess the effect on declarations listed as for floods. Standard errors are clustered by state.

	(1)	(2)	(3)	(4)
Wind max (B)	-0.0264^{***}	-0.0283^{***}		
	(0.0063)	(0.0064)		
Wind max^2 (B)	0.0010^{***}	0.0011^{***}		
	(0.0002)	(0.0002)		
Wind max (J)			-0.0158^{***}	-0.0168^{***}
			(0.0040)	(0.0040)
Wind max^2 (J)			0.0004^{***}	0.0004^{***}
			(0.0001)	(0.0001)
Rain max	-2.7993^{***}	-2.8119^{***}	-2.6400^{***}	-2.6645^{***}
	(0.5537)	(0.5664)	(0.5529)	(0.5660)
Rain max^2	0.0026	0.0026	0.0029^{*}	0.0029^{*}
	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Observations	67100	64550	66600	64050
Wind model	Baldwin	Baldwin	Jing	Jing
51 VA counties dropped	No	Yes	No	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table S3: Effect of TCs on per capita personal income growth when excluding Virginia counties whose incomes were imputed, across wind models from Baldwin et al. and Jing et al. Columns (1) and (3) show our main models, and columns (2) and (4) show models where 51 of Virginia's counties are excluded since they were grouped with other independent cities by the Bureau of Economic Analysis. In the main models, we divide the income and population of these combined groups equally among the counties that comprise them (see Supplementary Text). Rain is in units of meters rather than mm.

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