Large indirect economic impacts of tropical cyclones shaped by disaster response

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

[1]Doerr School of Sustainability, Stanford University
[2]Department of Earth System Science, University of California, Irvine
[3]Lamont-Doherty Earth Observatory, Columbia University
[4]Department of Medicine, Stanford University
[5]Center on Food Security and the Environment, Stanford University
[6]National Bureau of Economic Research
*Corresponding author: christophercallahan@stanford.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 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 TCs, where damages initially increase with storm size but diminish for the largest storms. We show that this is likely due to the compensating effect of disaster aid following strong storms. We find that TCs have reduced U.S. income by $33 trillion over 1980-2019, >25 times their direct losses, but estimate that losses would have been nearly 70% 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 costliest and most dangerous 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 while new research has made these comparisons in the context of mortality (*16, 17*), quantitative comparisons between indirect and direct economic impacts remain lacking (*13*).

Further, 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*)), and because 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 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, reducing individual debt (*23*), avoiding negative credit card outcomes (*24*), and stabilizing small business survival and employment (*25*), with potentially long-run benefits for overall income (*20*). However, the benefits of disaster response, and its potential to facilitate climate adaptation, have not yet been 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 (*26*). 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 exposure on county-level per capita income growth in the U.S. over 1970-2019. We represent TCs using two recently developed, spatially explicit wind field models from Baldwin et al. (*27*) and Jing et al. (*28*), allowing us to assess exposure of each county to TC winds even if a storm track did not directly cross that county. Winds are just one component of the hazard posed by TCs and are only partially related to other subperils such as storm surge and rain (*17, 29*); however, modeling wind field spatial structure across many TCs is computationally tractable, and wind speed serves as a useful first-order proxy for overall TC exposure and risk that has been used in prior studies (Methods). We summarize county-level exposure as the spatially averaged maximum TC wind speed experienced across the county in each year, 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 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 winds on personal income growth. We use county and year fixed effects and county-specific trends to separate idiosyncratic local variation in TC winds from spatial and temporal confounding factors. This method has been used to study the economic growth impacts of other climate hazards (*30–33*), and is an established technique to credibly isolate the impact of climate from other confounding factors influencing societal outcomes (*34, 35*). 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. 1C, 1D), 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 ∼30 m/s provide net benefits to income (Fig. 1C), whereas in the case of the Jing et al. model, these winds merely result in reduced losses (Fig. 1D). While our primary models are quadratic, we also show results using cubic models in Fig. 1C and 1D to illustrate that our results are not solely due to an overly restrictive functional form.

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 storm in a year at each grid point (*7*). 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*).

Disaster response contributes to nonlinearity

What explains the nonlinear effect of TCs on income growth? One hypothesis is that direct transfers through safety net programs such as unemployment insurance could make up for lost income, with the benefits of strong storms thus reflecting increased income from social insurance payouts (*36*). 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 nonlinearity (Fig. S2). This finding differs slightly from that of Deryugina (*37*), though there are several reasons we might find distinct results (Supplementary Text).

An additional hypothesis relates to disaster response: Stronger TCs prompt discretionary responses by the local, state, or federal government that could help maintain incomes among those living in affected areas. Indeed, we find that the probability that a county receives a TC-related Presidential disaster declaration in a given year rises strongly with wind exposure in that year (Fig. S3), with stronger effects in the Baldwin et al. model.

Motivated by this pattern, we study whether receiving an official disaster declaration moderates the impact of a given-sized storm on subsequent income growth (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. 2A, 2B). This pattern may explain the changing shape of the income growth response over time: We find significantly greater nonlinearity in the second half of the analysis sample (1995-2019) relative to the first half (1970-1994), with benefits from the strongest TCs emerging later in both models (Fig. 2C, 2D). At the same time, presidential disaster declarations have become substantially more common over time, with the percent of counties in our sample receiving TC-related declarations rising from an average of $\sim$0.9% per year in 1970-1994 to an average of 15% per year in 1995-2019 (Fig. 2F). We also observe that country-wide trends in TC winds are mild by comparison and vary between the two wind models, suggesting that increasing declarations are not merely a result of strengthening storms (Fig. S4). These results suggest that the increasing use of disaster declarations—and the resulting aid—may have produced greater benefits from strong storms over time.

One concern is that these results might not actually reflect the causal effect of Presidential 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 (*38–40*) (Methods), 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 yield similar benefits as we find in our main analysis (Fig. S5), supporting the conclusion of a causal effect of declarations on income growth (Supplementary Text).

Collectively, these findings suggest that a greater probability of beneficial disaster declarations at higher wind speeds (Fig. S3, Fig. 2E) combined with an increase in the use of disaster declarations over time (Fig. 2F) have together produced an increasing nonlinearity in the response of income growth to TCs. These results may also explain the differing degrees of nonlinearity in the two different wind models (Fig. 1C, 1D). In the Baldwin et al. model, the probability of declaration rises strongly as a function of wind speed (Fig. S3) and the distributions of winds with and without declarations is clearly separated (Fig. 2E). As a result, the highest wind observations are very likely to receive declarations and thus produce benefits (Fig. 1C). By contrast, in the Jing et al. model, the distributions of winds with and without declarations overlap much more (Fig. 2E), meaning the benefits of declarations do not emerge as clearly at high wind speeds.

Persistent impacts of TCs

The indirect income impacts of TCs raise the question of the magnitude of personal income growth that has been foregone due to TCs or saved by disaster relief over the past several decades. Answering this question requires understanding not only the short-term impacts of TCs, but also whether those effects persist through time. We use a distributed lag (DL) model to assess the long-term effects of TC winds with and without disaster declarations (Methods). We again find a clear difference between counties that received Presidential disaster declarations and those that did not (Fig. 3). When counties are not declared disasters, their income impacts are persistently negative, with losses that are not recovered even ten years later. By contrast, when counties receive TC-related disaster declarations, they experience income growth benefits that are similarly maintained for at least a decade.

Several additional lines of evidence provide confidence in the large lagged indirect impacts of TCs. First, the persistence of TC impacts with and without declarations is consistent when using 5 or 15 lags in the DL model instead of 10 (Fig. S6). Second, we use randomization tests to calculate non-parametric p-values for the cumulative impacts of TCs, where we reshuffle TC wind exposure and disaster declarations within a county but across years, within a year but across counties, or across the full sample (*7, 17*). These "null" distributions of coefficients do not include the estimates from our original model (Fig. S7; $p < 0.01$ in all cases). Third, 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. S8). Finally, the negative impacts of TCs without declarations are robust to several alternative choices of sample restriction (Fig. S9, S10). Specifically, we exclude from the sample a unique set of disaster declarations associated with Hurricane Katrina evacuations that do not appear to be representative of the broader effects of disaster aid (*40*) (Methods). Including these observations alters the effect of TCs with declarations, but in all cases, the persistent negative impacts of TCs not receiving declarations remains robust (Methods, Fig. S9, S10).

Long-term indirect costs exceed direct costs

The presence of persistent and accumulating income losses suggests that the long-term costs of TCs may substantially exceed their immediate direct costs. We use the effects shown in Fig. 3 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 TCs from this calculation are approximately \$33 trillion (\$US$_{2022}$), with a 95% range of \$26-\$42 trillion due to uncertainty in the regression estimates (Fig. 4A, 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 the accumulation of TCs over the previous 40 years (Fig. 4C).

However, if no disaster declarations had been issued, income losses due to TCs would have been a striking \$23 trillion greater since 1980, or ∼68% greater than our main damage estimate (Fig. 4A). 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 or even entirely compensating for the harm to these cities relative to surrounding areas (Fig. 4C, 4D). Observed losses are smaller, though still sizable, when using the Baldwin et al. wind model, with losses totaling \$14 trillion and an additional \$18 trillion saved by disaster declarations (Fig. 4B).

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 \$23 trillion in avoided income losses that we estimate occurred due to this aid. Given an average income tax rate of 14.9% (*41*), a back-of-the-envelope calculation yields ∼\$3.4 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 (*42*), and SBA disaster loans total ∼\$60 billion (*25*), with TCs 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 (*43*), or SHELDUS (*44*), 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 (*45, 46*), and extensive missing data has been documented in SHELDUS in particular (*47*). 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 serve as an informative baseline for comparison with our results.

Discussion and Conclusions

Our analysis has revealed several new facts about the impacts of TCs in the United States. We have illustrated a nonlinear response of county-level income growth to TC exposure, a nonlinearity that has not been shown in previous studies on indirect TC impacts in the U.S. (*14, 37*). We show that beneficial disaster response following the strongest cyclones contributes to this nonlinearity. 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 TCs 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. In more practical terms, our analysis shows that measuring the economic response to disaster declarations (*20*) does not represent the effects of disasters themselves, many of which do not receive declarations (Fig. 2E, 2F).

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 climate events (e.g., (*7, 30, 32*)). Given the potential for climate warming to increase the intensity of the strongest tropical cyclones (*2–4*), alongside their rainfall (*48*) and storm surge (*49*), 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.

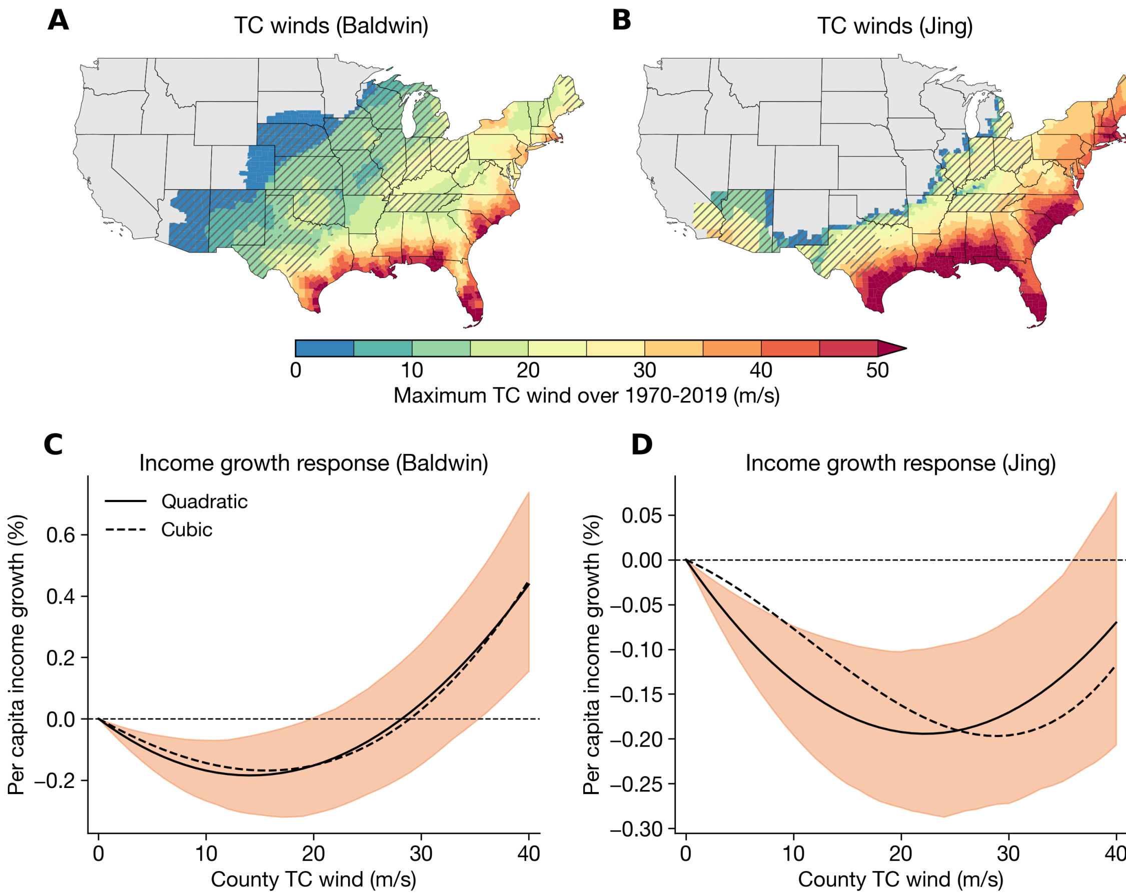

Figure 1: Indirect impacts of TC winds. A, B) Long-term maximum county-level TC winds derived from wind fields produced by Baldwin et al (A) and Jing et al (B). Hatching denotes counties which are excluded from the estimation sample since they have only received Katrina-evacuation-related disaster declarations (Methods). C, D) Nonlinear effects of TC winds on county-level per capita income growth based on the wind fields above, using both quadratic (solid) and cubic (dashed) specifications. Shading shows 95% confidence intervals produced by bootstrap resampling by county with 1,000 iterations. Note that the y-axes of C and D differ based on the different relative effects of the two wind models.

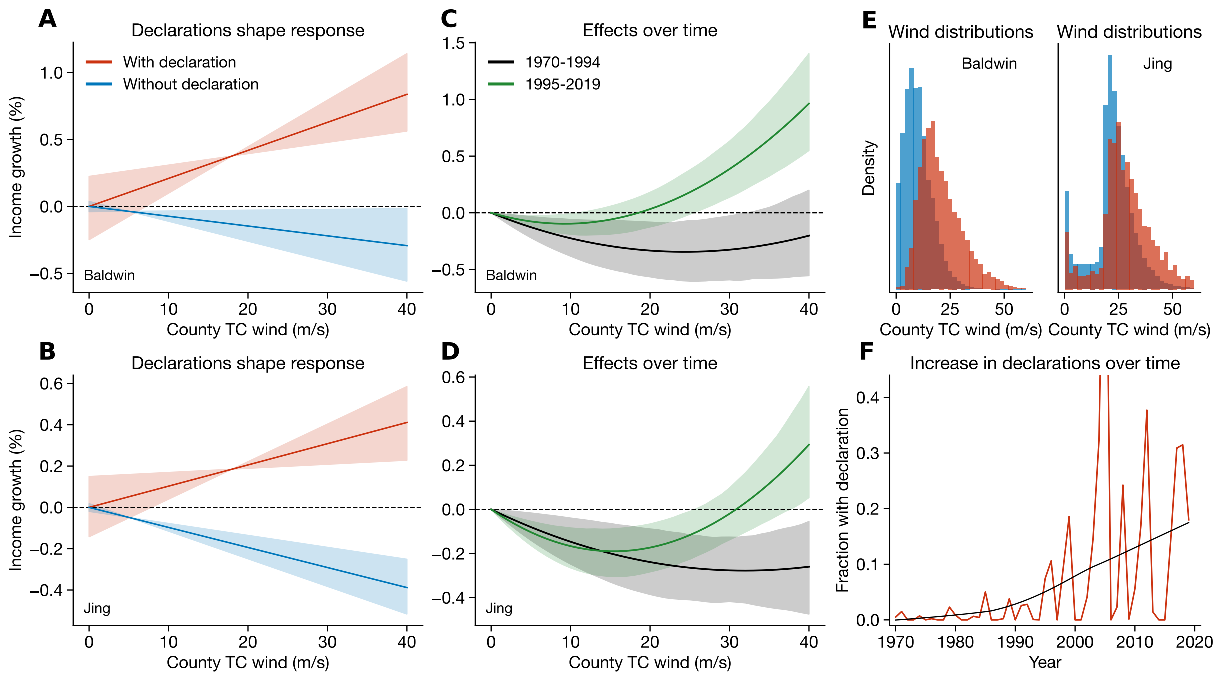

Figure 2: Disaster declarations contribute to nonlinear indirect impacts of TCs. A, B) Linear impacts of TCs on county-level income following a storm in which a county received a Presidential disaster declarations (red) versus did not receive a declaration (blue), based on wind field models from Baldwin et al. (A) and Jing et al. (B). Confidence intervals are centered on the means of the distributions of county-level winds with and without declarations. C, D) Nonlinear income impacts of TCs in the first 25 years (black) of the sample, during which there were few disaster declarations, and the second 25 years (green) of the sample time period, during which there were frequent declarations, based on wind field models from Baldwin et al. (C) and Jing et al. (D). E) Distributions of non-zero TC wind observations, separated by observations with Presidential disaster declarations (red) and without Presidential disaster declarations (blue), based on wind field models from Baldwin et al. (left) and Jing et al. (right). Plots show the density of observations within each distribution, not the total number of observations, to aid visualization. The absolute number of observations with declarations is <10% of the full sample. F) Fraction of counties in each year with a TC-related disaster declaration. Black line shows a locally-weighted (lowess) smoothing. The data for 2005 are truncated since nearly every county (>90%) received declarations due to abnormal circumstances associated with Hurricane Katrina (see Methods). In A-D, shading shows 95% confidence intervals based on bootstrap resampling by county.

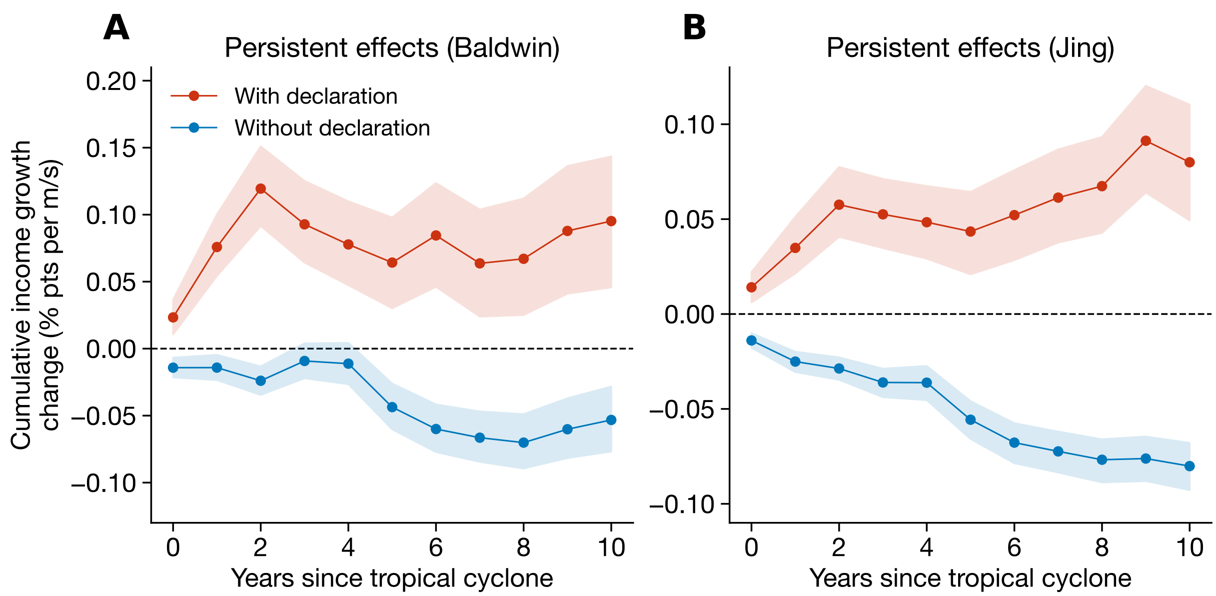

Figure 3: Persistent economic impacts of TCs. Panels show cumulative effects of TCs on growth in the year of the cyclone and the ten following years, using a distributed lag regression model (see Methods). Panel A shows results using the wind field of Baldwin et al. and panel B shows results using the wind field of Jing et al. Shading shows 95% confidence intervals based on bootstrap resampling by county.

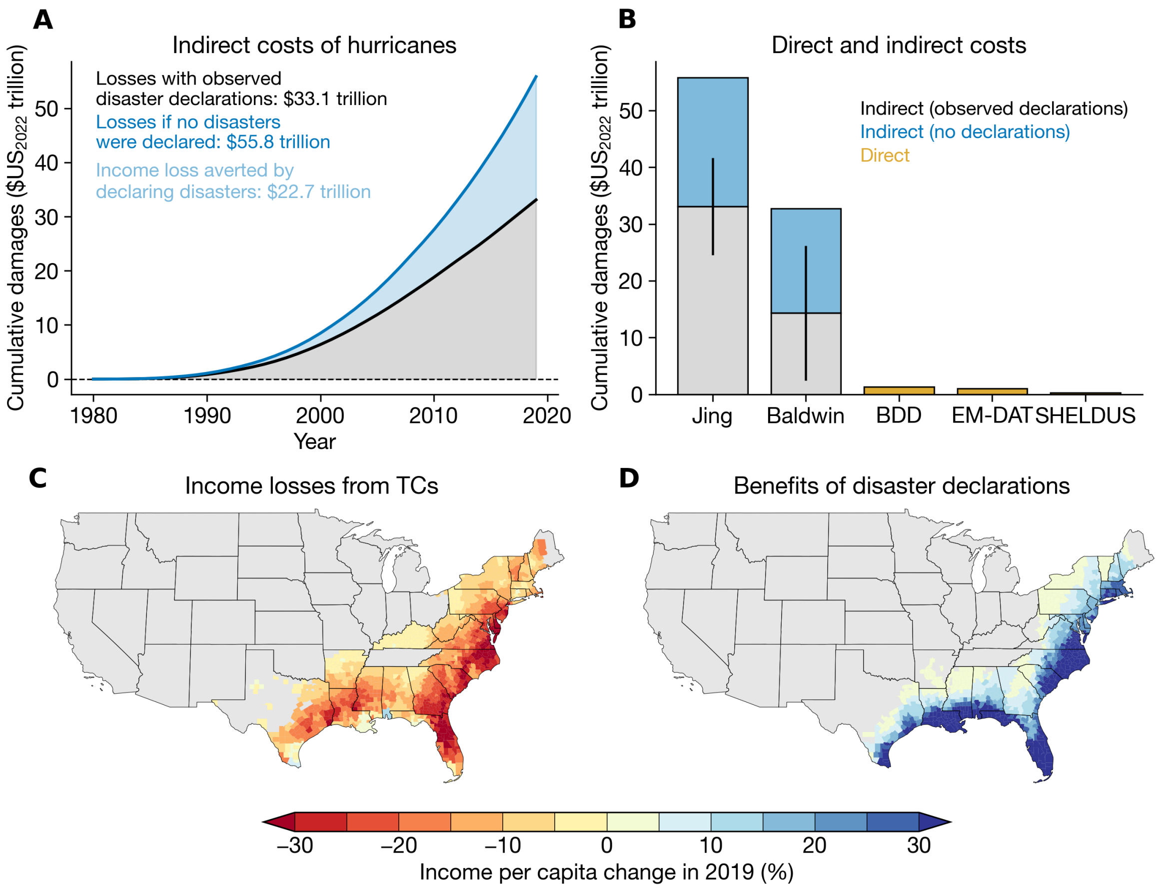

Figure 4: Long-term indirect and direct impacts of tropical cyclones. Cumulative income losses from TCs relative to a counterfactual without TCs over 1980–2019, based on the wind field data from Jing et al. The black line shows losses given observed disaster declarations, the blue line shows losses if 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.

Supplementary Materials

Materials and Methods

Tropical cyclone data

To represent exogeneous physical exposure to TCs, we use parametric wind field models applied to Atlantic-basin TC tracks from the International Best Track Archive for Climate Stewardship (*50*) (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. (*27*), based on Willoughby et al. (*51*), and one by Jing et al. (*28*), which used the wind field models of Chavas et al. (*52*) and Chen et al. (*53*). There are structural differences between these two models. Jing et al. (*28*) 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. (*27*). 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. (*28*) 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 (*28*), but it does mean that wind speeds in the Jing et al. model might generally be higher than the Baldwin et al. model.

Winds are only one component of tropical cyclones, which can also generate inland flooding via rainfall and coastal flooding via storm surges. That being said, there are several reasons why we focus on wind speeds as our metric of TC exposure. First, they are computationally tractable to model as functions of storm intensity and size, as compared to other hazards such as rainfall and storm surge. Second, the development of several wind field models allows us to systematically compare TC impacts considering some degree of model structural uncertainty (*27, 28, 51–53*). Third, winds have been used in several previous studies that assess the income impacts of TCs (*7, 8, 12*), as well as other studies of TC exposure and risk (*6, 27, 28, 54, 55*), allowing our results to be more directly comparable to previous work.

Other work has found that minimum central pressure is a better predictor of TC damages than maximum sustained wind speed (*56*). 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. 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 storm in a year at each grid point (*7*). We calculate county-level values 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.

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 (*57*). 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.

We compare our analysis of income impacts with previous, independent estimates of direct damages. Our data on direct damages are drawn from EM-DAT (*43*), the NOAA Billion-Dollar-Disasters database (*1*), and SHELDUS version 22.0 (*44*). 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.

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. Within the FEMA data, we limit our analysis to disasters whose "Hazard" is listed as "Tropical storms," "Typhoons," or "Hurricanes."

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, county-specific average characteristics, and country-wide and local trends:

$$g_{it} = \beta_1 W_{it} + \beta_2 W_{it}^2 + \mu_i + \gamma_t + \theta_i t + \epsilon_{it} \tag{1}$$

Here g denotes growth, W denotes county TC winds, 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 θ is a county-specific linear time trend. Standard errors are clustered at the county level to adjust for autocorrelation within counties.

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.

When we interact wind with declarations (Fig. 2), we run the following linear model with an interaction between wind (W) and a dummy variable for a TC-specific disaster declaration (D):

$$g_{it} = \beta_1 W_{it} + \beta_2 W_{it} * D_{it} + \mu_i + \gamma_t + \theta_i t + \epsilon_{it} \tag{2}$$

In this case, β_1 describes the effect of TC winds when D is zero, meaning when a disaster is not declared. β_2 describes the change in the effect of TCs when disasters are declared, meaning the actual marginal effect of TCs when disasters are declared is given by $\beta_1 + \beta_2$.

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, 30, 32, 58*), this approaches 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:

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

In Fig. 3, we present the sum across the lags of the β_{1L} and β_{2L} terms. 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.

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 (*59*) and Callahan and Mankin (*31*).

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.

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 (*40*).

The final analysis therefore contains two samples, one for each wind model, comprising 69,750 observations for the Baldwin et al. wind model and 66,500 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 S9 and S10, we show the implications of relaxing these sample restrictions. Fig. S9 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. S10 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 (Fig. S6), but inconsistent and unstable when including them (Fig. S9, S10), leads us to conclude that these Hurricane Katrina-related evacuation declarations may not be representative of the broader effects of disaster aid.

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 (*38–40*). 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 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. S5). 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.

Comparison between our findings and Deryugina (2017)

Deryugina (*37*) 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 (*57*).)

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 (*60*) 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 (*37*) 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 (*27, 28*). 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 (*37*)), 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 \$22.6 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 (*41*). Multiplying 22.6 trillion by 0.149 yields potential tax revenues of \$3.4 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 (*57*). 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.

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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].

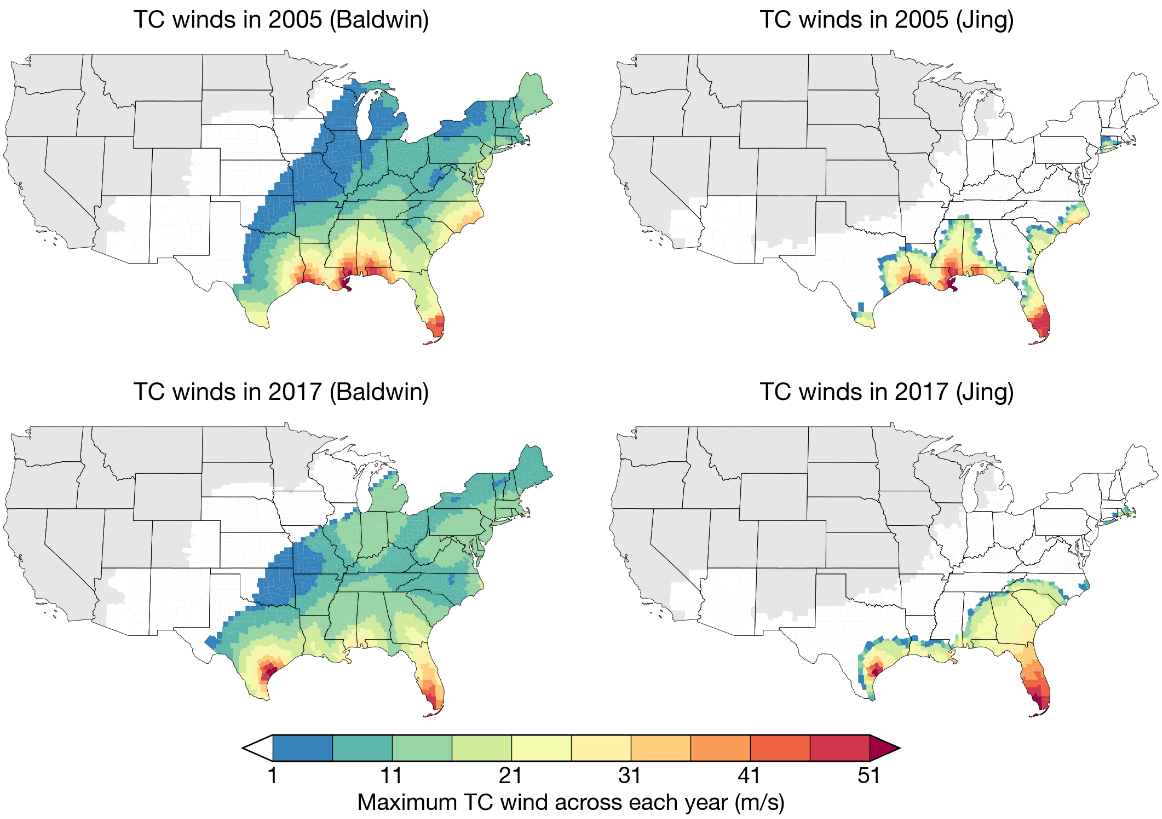

Figure S1: Example TC winds from both wind models. Each map shows maximum TC winds across all storms in a year for 2005 (top row) and 2017 (bottom row). Left column shows winds from the Baldwin et al. wind field and right column shows winds from the Jing et al. wind field. White denotes counties experiencing 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.)

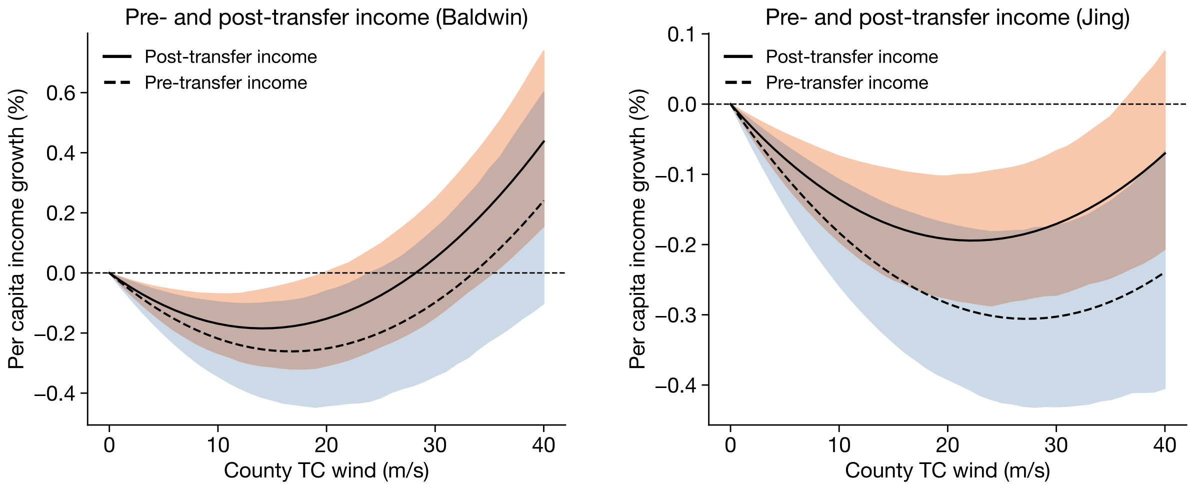

Figure S2: Effects on pre- and post-transfer income. Our result in the main analysis uses total post-transfer income, which is shown here in the solid line for both wind models. Dashed line shows the effect of TC winds on pre-transfer income, meaning income excluding unemployment insurance, Social Security benefits, medical benefits, and veterans' benefits. Shading shows 95% confidence intervals calculated by bootstrapping by county. Note differing y-axis scales.

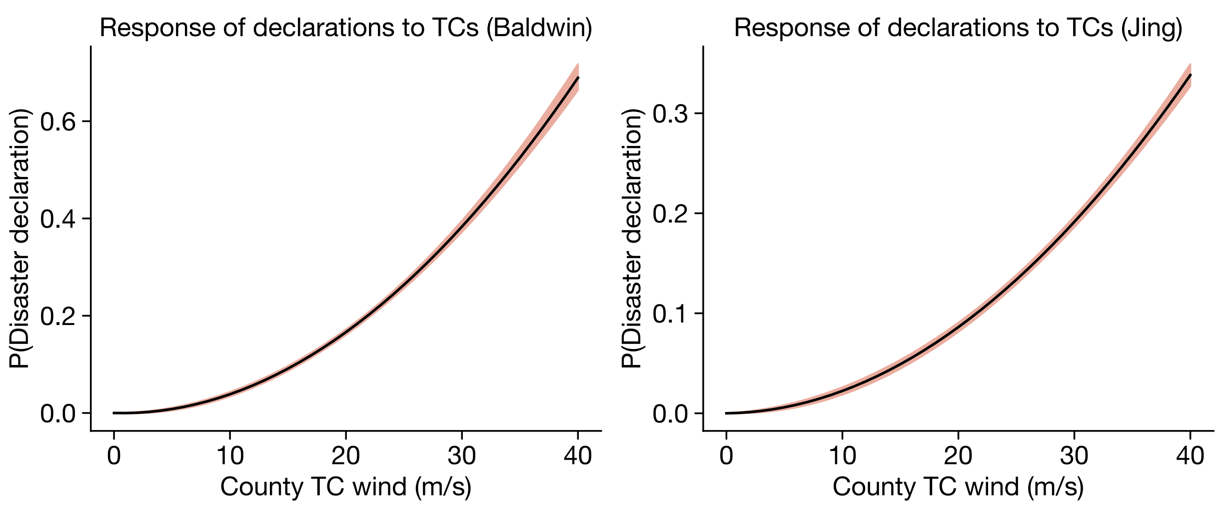

Figure S3: Higher TC winds are associated with greater probability of Presidential disaster declarations. Panels show the effect of TC winds on the probability of a TC-related Presidential disaster declaration for both wind models: Baldwin et al. (left) and Jing et al. (right). Results are derived from a panel regression model with county and year fixed effects and county-level trends, as in the main analysis, with both linear and quadratic terms for wind speed. Note differing y-axis scales.

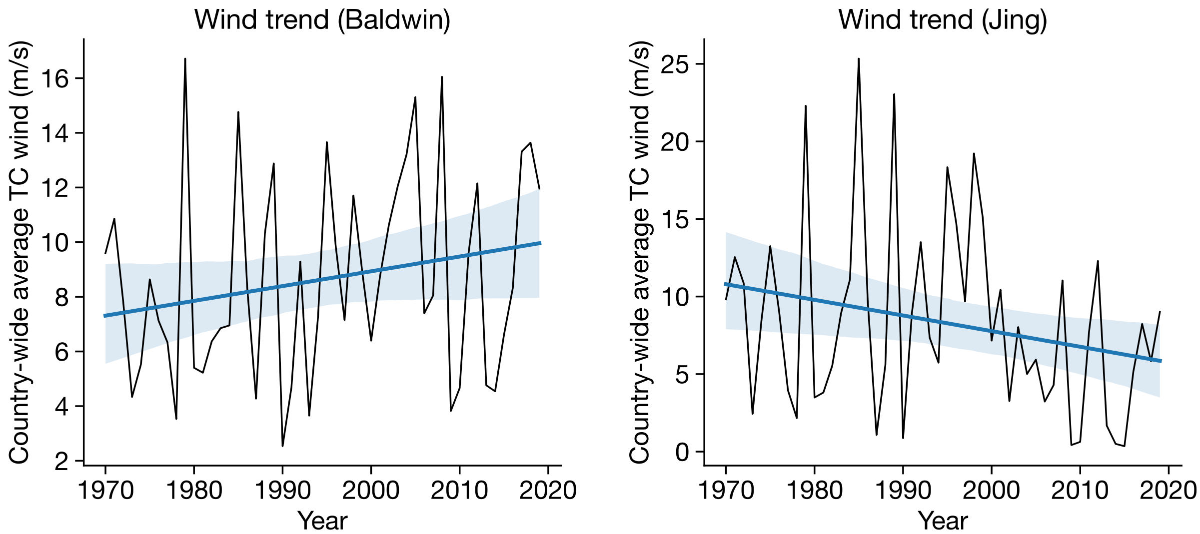

Figure S4: Trends in wind speeds. Panels show average TC winds across all counties in the sample in each year for both wind models. Left panel shows winds from the Baldwin et al. model and right panel shows winds from the Jing et al. model. Blue line shows the linear trend line with the 95% confidence interval shaded.

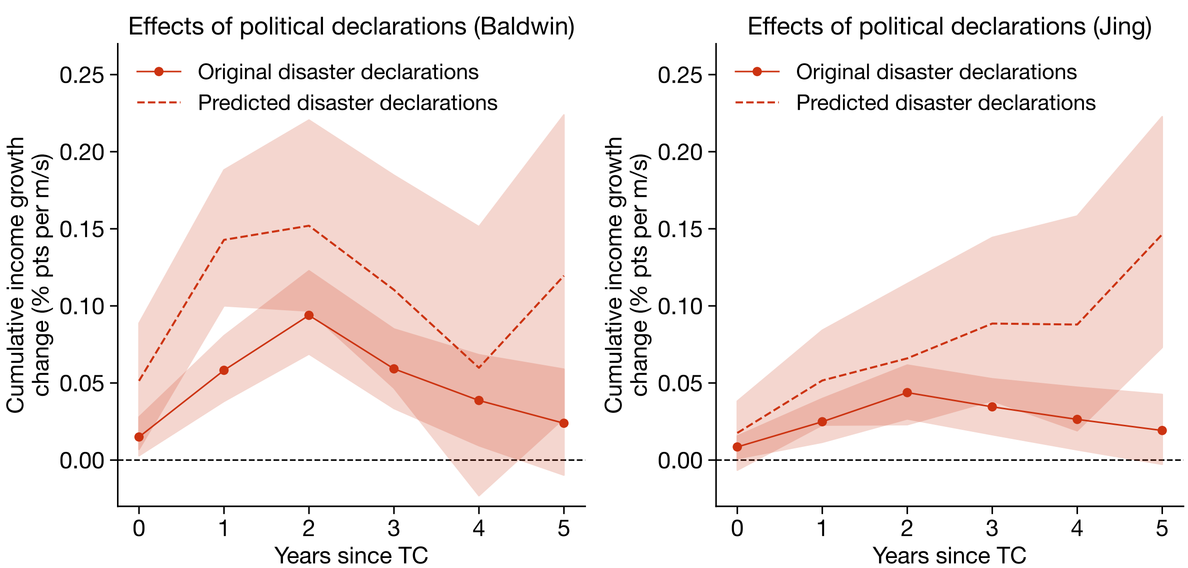

Figure S5: Original and predicted effects of disaster declarations. Panels show the effects of TCs when Presidential disaster declarations occur for the Baldwin et al. wind model (left) and Jing et al. wind model (right). In both panels, solid line shows results from a distributed lag model with 5 lags 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.

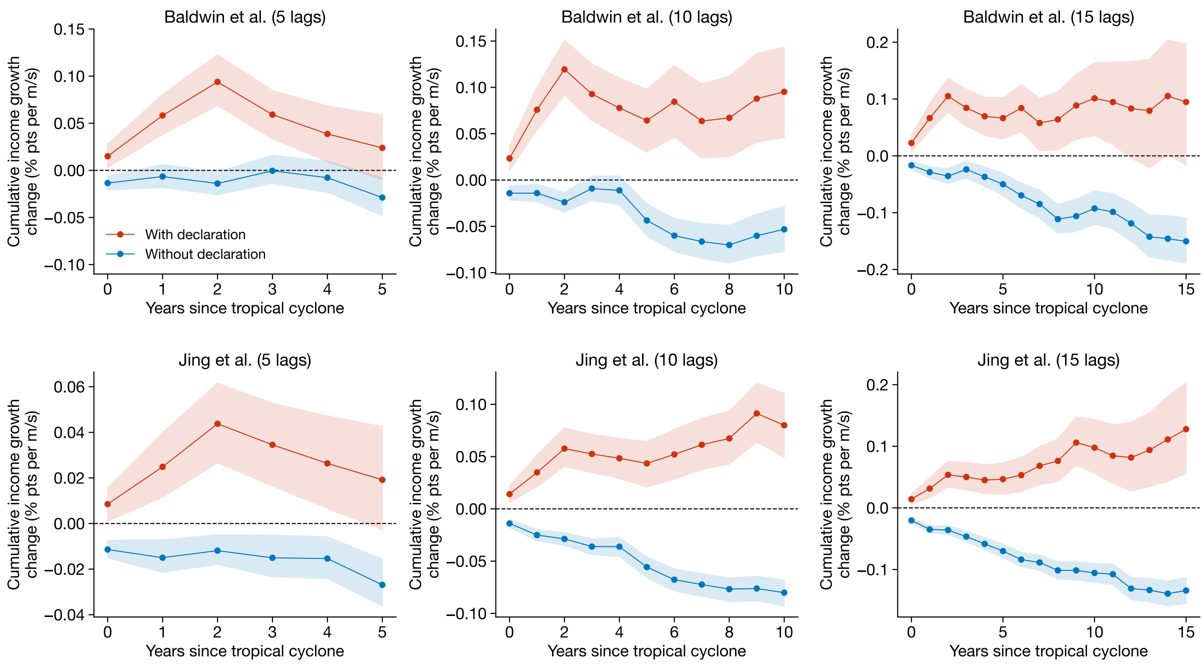

Figure S6: Persistent effects of TCs at different lag lengths. Each panel shows the effect of TCs with and without Presidential disaster declarations (red and blue, respectively), using two different wind models: Baldwin et al. (top row) and Jing et al. (bottom row). Left column shows results from models with 5 lags of TC winds, middle column shows results with 10 lags of TC winds (as in main text Fig. 3), and right column shows results with 15 lags of TC winds.

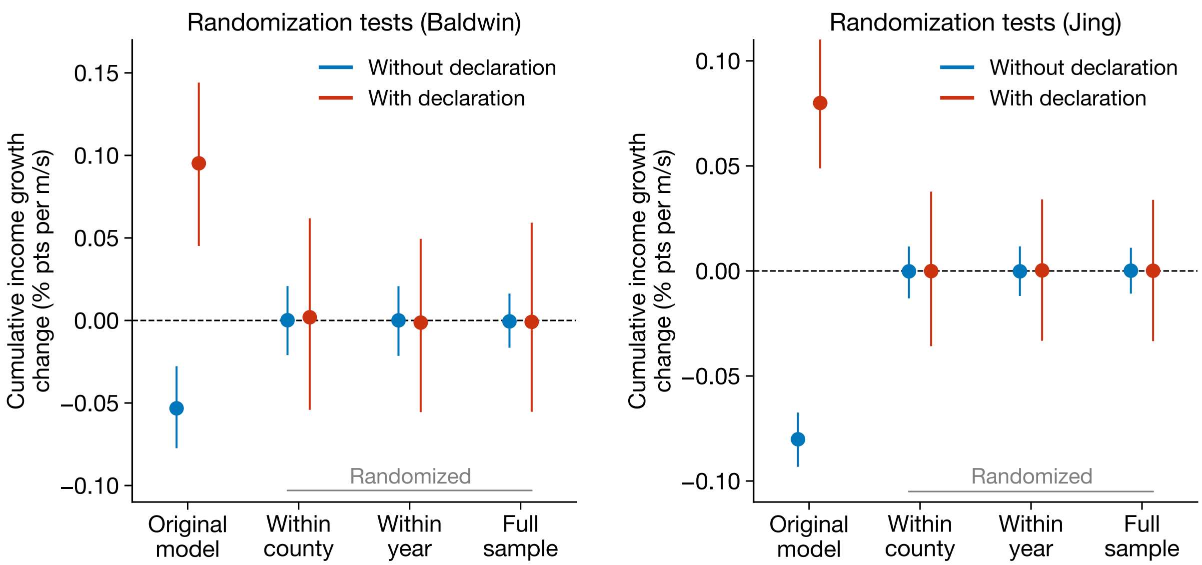

Figure S7: Randomization tests for distributed lag results. Effects of TCs on economic growth, calculated as cumulative impacts from a distributed lag model with 10 lags, compared to effects from randomized samples of TC winds and disaster declarations. The first two coefficients in each panel show the effects with and without declarations from the original model, the second set of coefficients shows effects when winds and declarations are randomized within counties across years, the third set shows effects when randomized within years across counties, and the last set shows effects when randomized across the full sample. Distributions of randomized coefficients are performed by sampling 1,000 permutations. Note differing y-axis scales.

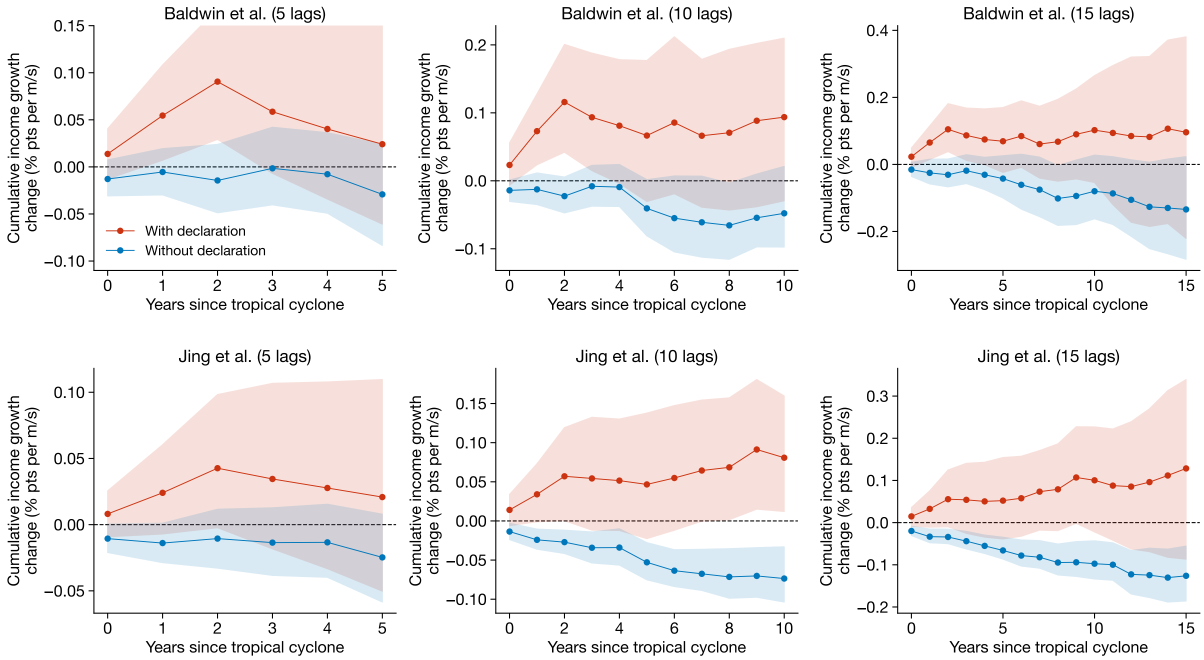

Figure S8: Persistent effects of TCs when bootstrapping by state. As in Fig. S6, but when using bootstrap resampling by state instead of county, which accounts for both spatial and temporal autocorrelation in growth.

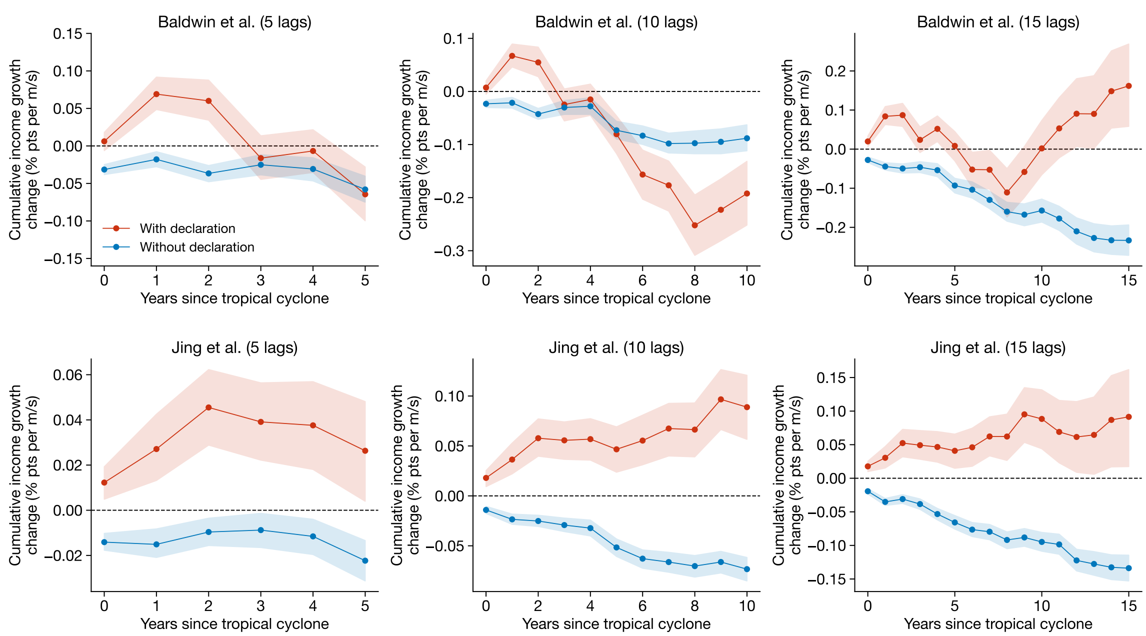

Figure S9: Effects of TCs including Katrina-evacuation-related declarations. As in Fig. S6, but including counties whose only disaster declaration occurred in 2005 and was listed as for "Hurricane Katrina Evacuation" or "Hurricane Katrina Evacuees." See Methods for details.

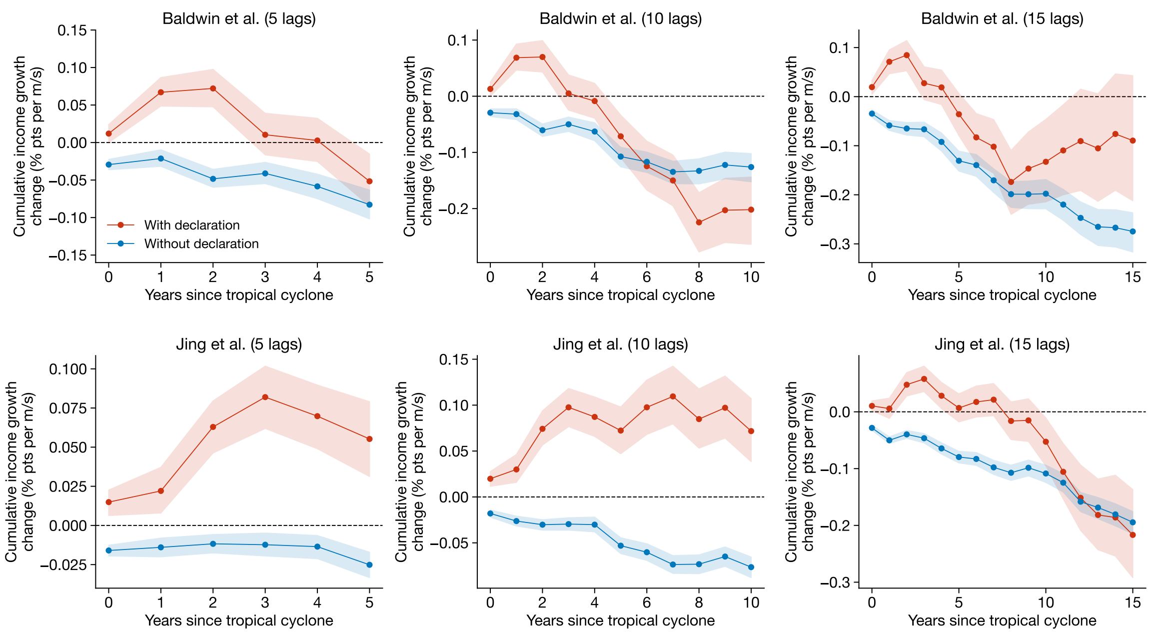

Figure S10: Effects of TCs including all counties. As in Fig. S6, but including all 2,948 counties for which we have data regardless of wind exposure or experience with disaster declarations. See Methods for details.

	(1)	(2)	(3)
Wind max	-0.0261^{***}		
	(0.0066)		
Wind max^2	0.0009^{***}		
	(0.0002)		
Wind mean		0.0197	
		(0.0215)	
Wind mean^2		-0.0009	
		(0.0016)	
Wind sum			-0.0024
			(0.0029)
Wind sum^2			0.0001^{***}
			(0.0000)
Observations	69750	69750	69750

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

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.

	(1)	(2)
Reelection year	0.7488^{**}	0.7547^{**}
	(0.2446)	(0.2429)
Post-1988	3.4712^{***}	3.4695^{***}
	(0.3378)	(0.3387)
Democratic president	-5.3514^{***}	-5.3594^{***}
	(1.2771)	(1.2828)
State Democratic presidential vote share	1.5717	1.5728
	(2.6043)	(2.6180)
Dem. president × Dem. vote share	9.1825^{***}	9.1990^{***}
	(2.6289)	(2.6385)
Observations	58960	58476
Wind model	Baldwin	Jing

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

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. Standard errors are clustered by state.

	(1)	(2)	(3)	(4)
Wind max	-0.0261^{***}	-0.0261^{***}		
	(0.0066)	(0.0065)		
Wind max^2	0.0009^{***}	0.0009^{***}		
	(0.0002)	(0.0002)		
Wind max			-0.0177^{***}	-0.0193^{***}
			(0.0040)	(0.0039)
Wind max^2			0.0004^{***}	0.0004^{***}
			(0.0001)	(0.0001)
Observations	69750	67200	66500	63950
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 TC winds 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).

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