Large indirect economic impacts of tropical cyclones shaped by disaster response

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Tropical cyclones (TCs) have direct economic impacts, destroying property, crops, 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 indirect impacts of TCs on incomes in the years following a TC. We find a nonlinear response of income growth to TCs: most TCs persistently depress income, but the strongest TCs do not appear to affect income, likely due to the compensating effect of federal disaster aid following strong storms. We find that TCs have collectively reduced U.S. income by $30 trillion over 1980–2019, >20 times their direct losses. These findings highlight that disaster response can ameliorate indirect disaster impacts, but that to date such responses have not 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, 13*), or changes to longer-term health outcomes such as excess mortality in the months following TCs (*14*). It has been suggested that indirect impacts may substantially exceed direct impacts (*12*), but this proposition is rarely empirically tested.

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 (*15, 16*). 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.) (*17*). This debate is complicated by potentially heterogeneous effects in different sectors or regions (such as losses in agriculture and benefits in construction (*18, 19*)), 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 (*20*) and stabilizing small business survival and employment (*21*), with potentially long-run benefits for overall income (*17*). 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. At the same time, climate change is likely to accelerate the costs of extreme climate events and strain adaptation resources not originally designed to accommodate warming (*22*). Greater understanding of the interactions between physical climate hazards, their economic impacts, and the effects of disaster response policy 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 spatially explicit wind field models (*23–27*), summarizing county-level exposure as the spatially averaged maximum TC wind speed experienced across the county in each year (Fig. S1). Winds are just one component of the hazard posed by TCs and are only partially related to other subperils such as rain and storm surge; however, modeling wind field spatial structure 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 measure indirect impacts by examining the immediate and lagged effects of TCs on per capita income, using data from individual year-end tax returns. This measure of indirect impacts captures economy-wide impacts that alter people's overall income both in the year of the TC and the following years, even if they were not directly affected by the storm. That said, because our analysis does not capture changes to outcomes such as mortality risk that are not directly reflected in income, it is a conservative accounting of these impacts.

We fit a panel regression model that estimates the effect of county-level wind exposure on personal income growth. We use county and year fixed effects, along with 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 growth impacts of other climate hazards (*28–31*), and has been established as a robust technique to credibly isolate the impact of climate from other confounding factors influencing societal outcomes (*32, 33*). 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 the long-term accumulated income impacts of TCs across the U.S.

Nonlinear effect of TCs on income growth

Our analysis shows that increases in county-level TC winds are monotonically and exponentially associated with greater direct property and crop damages (Fig. 1A), consistent with prior work (*5, 6, 9, 10*). Both linear and quadratic specifications yield very similar responses, indicating little curvature in the relationship. These data are drawn from SHELDUS (*34*) and only account for immediate destruction during the storm, not its potential long-term disruption.

By contrast, per capita income growth responds nonlinearly to TC winds, in both the year of the TC (Fig. 1B) and subsequent years (Fig. S2). As wind exposure increases between 0 and 21 m/s (approximately the 96$^{\text{th}}$ percentile), personal income growth declines. However, starting at ∼21 m/s, the curve slopes upward, meaning that increases in the intensity of the top 4% of county-wind observations produce economic benefits (Fig. 1B). These benefits of the strongest wind observations also appear in more flexible cubic or binned models (Fig. S3). Additionally, the overall relationship remains statistically significant and nonlinear when more restrictive standard error clustering is used, when the county-specific trends are omitted, or when population weights are not used in the regression (Table S1).

The nonlinear response of income growth to TCs is maintained in the year after the storm, with benefits at high winds that are even greater than in the contemporaneous response (Fig. S2). These benefits appear to decay somewhat by the fifth year after the TC, at which point losses from TCs have grown relative to the contemporaneous response (Fig. S2). These results emphasize that the nonlinear indirect impact of TCs is not merely transitory, but manifests in personal income for at least half a decade following storms.

These wind speed values are averaged across counties, and thus the instantaneous wind speed at a particular location may be higher than the spatially averaged wind. This may partially explain why we observe income losses at wind speeds considered too low to cause damage by some previous literature (*6*). Indeed, we also observe direct damages at these relatively low wind speeds (Fig. S4), implying that our county-level average values may be capturing more damaging local gusts. Additionally, even areas exposed to relatively low wind speeds may experience significant rainfall from the TC (*35*) and therefore potentially damaging floods. Nevertheless, because wind fields alone are often used as a simple metric of TC hazard (*6, 23, 24, 36, 37*), we continue to focus on wind in this analysis.

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 (UI) could make up for lost income, with the benefits of strong storms thus reflecting increased income from social insurance payouts (*38*). We do find larger effects when we exclude transfers from our measure of income, implying that transfers such as UI 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. S5). This finding differs slightly from that of Deryugina (*39*), 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, conditional on the declaration of a disaster, the amount of FEMA disaster aid spent on TCs in affected states rises strongly and exponentially with the worst county-level wind exposure in that state (Fig. 2A). The most affected states may receive billions of dollars in aid following a cyclone and subsequent disaster declaration. (We use state-level aggregation for Fig. 2A because different types of FEMA spending flow to different political units or locations; Methods).

Because data on disaster spending is incomplete (*40, 41*), we extend this analysis by focusing on the binary metric of whether a given county received a TC-related disaster declaration in each year (Methods). 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, with losses in counties that do not receive disaster declarations and benefits in counties that do (Fig. 2B). Importantly, there is little evidence of nonlinearity in these distinct responses: Unlike the aggregate response (Fig. 1B), the without-declaration sample yields no benefits at high wind speeds (Fig. 2B), suggesting that federal disaster aid may be responsible for these benefits.

One concern is that these results might not actually reflect the causal effect of the disaster declaration 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 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 (*42–44*) (Methods), factors that are plausibly unrelated to storm-specific factors that could trigger both declarations and affect recovery. We find that using these factors to predict the likelihood of a declaration yields similar results as our main analysis on the moderating effect of declarations on TC recovery (Fig. S6), supporting the conclusion of a causal effect of declarations on income growth (Supplementary Text).

Could the nonlinear aggregate response of income to TCs that we estimate (i.e., Fig. 1B) be produced by two distinct underlying linear responses? To test this, we combine the two responses in Fig. 2B with the probability of disaster declarations across the wind distribution (Fig. 2C), and calculate the aggregate effect on income growth by multiplying by the probability of a declaration at each wind speed (Methods). Figure 2C illustrates that the probability of a declaration rises strongly with wind speed, although even at high wind speeds there are some counties that do not receive disaster declarations, and at low wind speeds some that do (perhaps in part due to political-related factors described above and because non-wind perils such as rainfall can be substantial at relatively low wind speeds). Using these predicted probabilities to combine the two linear responses yields a strongly nonlinear response similar to – if somewhat stronger than – our original aggregate income effect (Fig. 2D, red).

These results illustrate that the nonlinear response of income growth to TCs is plausibly the product of differing disaster response decisions. At low-to-moderate wind speeds, disaster declarations are rare, but people still suffer losses from these storms that are not recovered. At higher wind speeds, the probability of disaster declarations rises, and the benefits from the resulting aid begins to dominate the response.

Long-term indirect costs exceed direct costs

The indirect income impacts of TCs raise the question of the magnitude of total personal income growth that has been foregone due to TCs 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. Using a distributed lag model to assess the long-term effects of TC winds with and without disaster declarations (Methods) yields two key results (Fig. 3A): First, when counties are not declared disasters, their income impacts are persistently negative, with losses that are not recovered even five years after the storm. Second, counties that receive declarations experience benefits that manifest immediately and accumulate for an additional two years, before income returns to its original trend, yielding no significant long-run effect (Fig. S7).

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. 3A 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. The total indirect costs of TCs from this calculation are approximately \$30 trillion (\$US$_{2022}$), with a 95% range of \$15-\$45 trillion due to uncertainty in the regression estimates (Fig. 3B, 3C). These indirect costs have accrued primarily to coastal counties, with incomes in 2019 reduced by >15% along the Gulf Coast, Florida, and the Carolinas due to the accumulation of TCs over the previous 40 years (Fig. 3D).

If no disaster declarations had been issued, income losses due to TCs would have been >\$8 trillion greater since 1980 (Fig. 3B). The benefits of disaster declarations via indirect avoided income losses have been particularly large in coastal cities such as New Orleans, Mobile, and Wilmington (Fig. 3E), reducing the harm to these cities relative to surrounding areas (Fig. 3D).

We estimate that FEMA has spent \$153 billion on declared TC disasters since 1989, or more than 50x less than the >\$8 trillion in avoided income losses that we estimate occurred due to this aid. Given an average income tax rate of 14.9% (*45*), a back-of-the-envelope calculation yields \$1.2 trillion in tax revenue 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 (*46*), and SBA disaster loans total ∼\$60 billion (*21*), with TCs only one component of these. Adding these spending sources would not alter the core conclusion that the personal income saved from disaster declarations far 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 (*47*), or SHELDUS (*34*), which put the cumulative 1980-2019 costs of all hurricanes at \$1.3 trillion, \$1 trillion, and \$270 billion, respectively (Fig. 3C). This result arises primarily because indirect losses appear to persist over time rather than being recovered immediately after the storm, which is both robust across multiple TC wind models (Fig. 3C) and consistent with other analyses of the long-run impacts of TCs (*7, 8*).

Discussion and Conclusions

Our analysis has revealed a novel nonlinear response of income growth in U.S. counties to tropical cyclone exposure. This nonlinearity appears to be driven in large part by disaster response to the strongest cyclones. But despite the benefits from such disaster response, the long-run damages to personal income from TC exposure appear to far exceed previously-quantified direct damages to capital and infrastructure.

These results 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 economic impacts of natural disasters have not explicitly distinguished between situations with and without disaster response, and 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 (*17*) does not represent the effects of disasters themselves, many of which do not receive declarations.

Our results also 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. 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.

The large income losses avoided by TC disaster declarations arise primarily because of the persistent nature of TC impacts: disaster relief not only ameliorates losses at the time of the disaster, but also prevents long-run reductions in income for years following the TC. Therefore, while our results contribute to an emerging literature highlighting the persistent and accumulating growth impacts of climate change, they also reveal that the benefit-cost ratio associated with disaster response interventions can be quite high. As a result, our results illustrate the potential for climate adaptation by showing that prompt and comprehensive policy responses to climate hazards can break the link between those hazards and losses to people in harm's way.

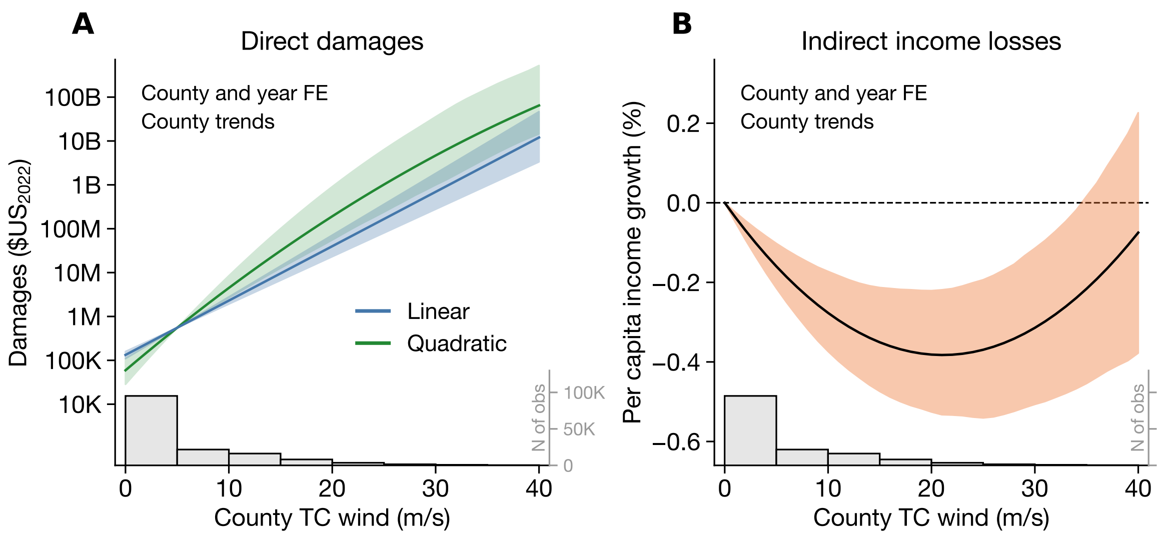

Figure 1: Direct and indirect impacts of TC winds. A) Effect of TC winds on county-level TC-driven property and crop damages, with both linear (blue) and quadratic (green) specifications. Note logarithmic y-axis. Response functions are centered on the average wind speed and average damages level. B) Effect of TC winds on changes in county-level personal income growth using a quadratic specification. In both plots, regressions include county and year fixed effects and county-specific linear trends, and shading shows 95% confidence intervals from bootstrapping by county. Lower histograms show the distribution of county-year wind observations. We bound the histogram at 40 m/s, but note that ∼0.26% of observations are above 40 m/s.

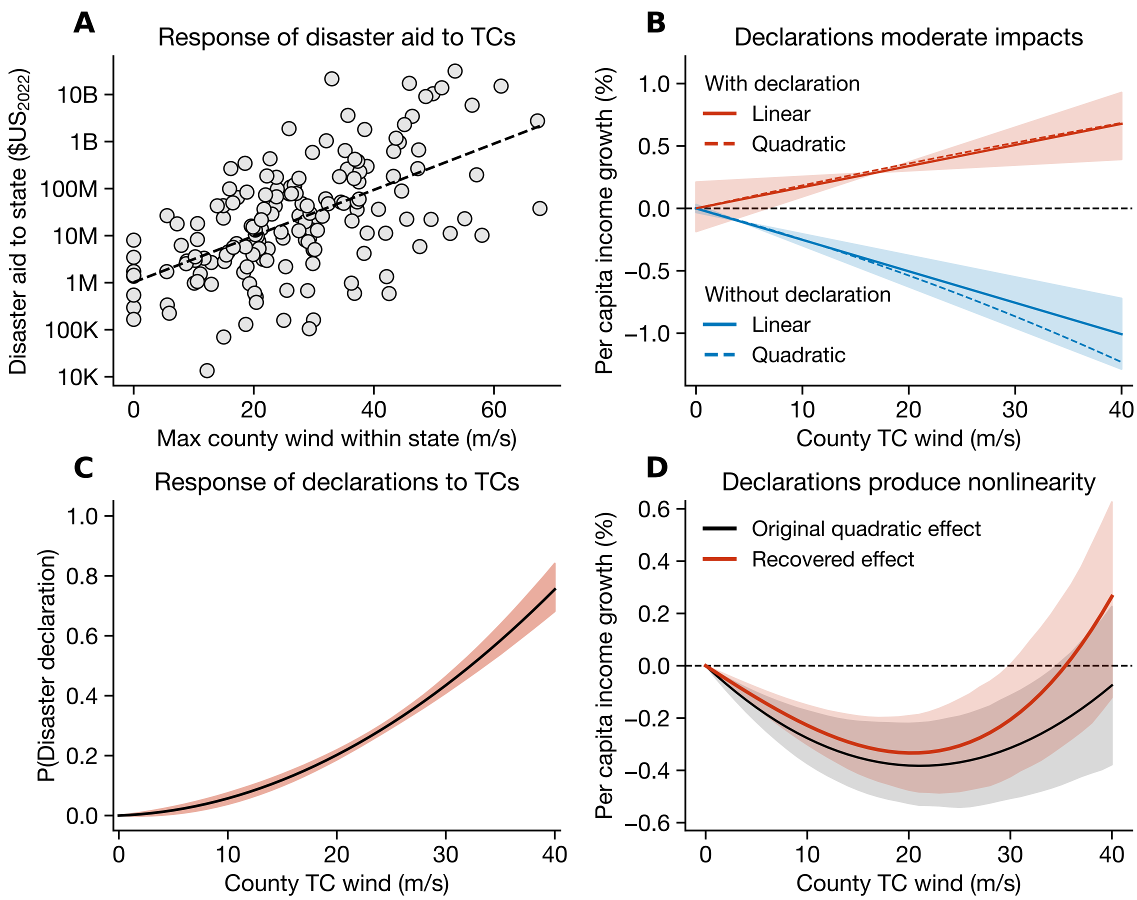

Figure 2: Disaster declarations shape nonlinear response of income to TCs. A) State-level disaster aid increases in response to within-state TC winds. Note logarithmic y-axis. Dashed line is linear regression fit. B) Linear effects of TCs on county-level income growth without (blue) and with (red) Presidential disaster declarations. Confidence intervals are centered on the means of the distributions of county-level winds with and without declarations. C) The probability of a county-level disaster declaration increases in response to TC winds, using a panel regression model with county and year fixed effects and county-level trends. D) Original quadratic response of income growth is shown in black and recovered response is shown in red. Recovered response is calculated by adding the without-declaration response (blue line in B) to the with-declaration response (red line in B), after multiplying the with-declaration response by the probability of declarations as estimated in C (Methods). In B, C, and D, shading denotes 95% confidence intervals calculated by bootstrapping by county.

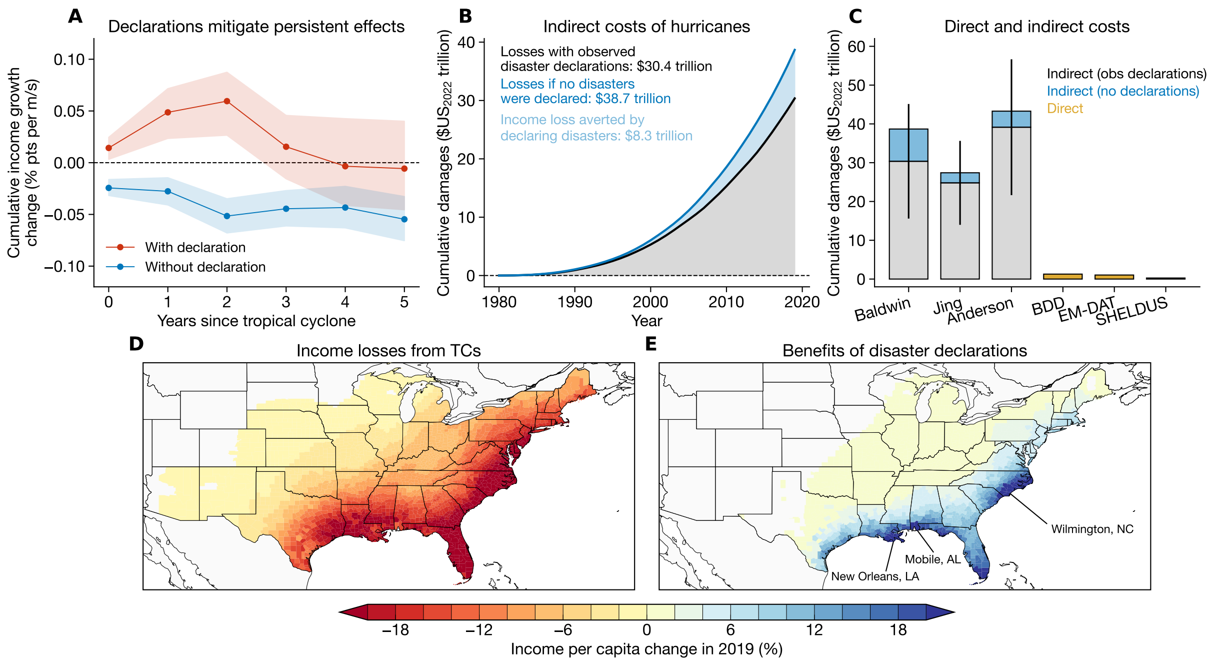

Figure 3: Long-term direct and indirect costs of tropical cyclones. A) Marginal effects of TCs on income in the year of the cyclone and the five following years, with and without disaster declarations, using a distributed lag regression model (Methods). B) Cumulative income losses from TCs relative to a counterfactual without TCs over 1980–2019. 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. C) 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 three TC wind models (Methods). 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. D) 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 B. E) Income change in 2019 from disaster declarations, relative to a counterfactual with no declarations. This calculation corresponds to the light blue wedge in panel B. Maps in D and E use wind field data from Baldwin et al. (*23*).

Supplementary Materials

Materials and Methods

Supplementary Text

Figs. S1 to S6

Tables S1 to S7

References *(48–56)*

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 (*48*) (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 we use parameterizes the two-dimensional radial wind field of the cyclone using data on the central intensity of the cyclone (e.g., minimum central pressure) and its radius of maximum wind speed or outermost extent of wind. Our main analysis uses the wind field model from Baldwin et al. (*23*), based on Willoughby et al. (*26*). We test an alternative TC dataset developed by Jing et al. (*24*), which used the wind field models of Chavas et al. (*25*) and Chen et al. (*27*). Finally, we also test county-level winds estimated from the stormwindmodel R package developed by G. B. Anderson (https://github.com/geanders/stormwindmodel) (*49*), which again uses the model of Willoughby et al. (*26*). One difference between these models is that the model of Jing et al. (*24*) incorporates a correction for the asymmetry of wind structures over land, whereas Baldwin et al. (*23*) do not. That said, because our results are qualitatively similar across these models (Fig. 3C), this choice does not broadly alter our conclusions.

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, and the development of several wind field models allows us to systematically compare TC impacts considering some degree of model structural uncertainty (*23–27*). Second, winds have been used in several previous studies that assess the income impacts of TCs (*7, 8*), as well as other studies of TC exposure and risk (*6, 23, 24, 36, 37*), allowing our results to be more directly comparable to previous work. Finally, the strong relationship between TC winds and direct damages (Fig. 1A) provides reassurance that we are measuring damaging characteristics of landfalling storms.

Previous work has found that minimum central pressure is a better predictor of TC damages than wind speed (*50*). 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.

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 across storms yields a more muted but still nonlinear response, while taking the mean across storms yields no significant effect (Table S2). We therefore infer that indirect damages are driven primarily by the worst storms in a given year rather than the accumulation of many less severe storms, which is also consistent with the strong nonlinearity of vulnerability curves found in engineering-based studies of TC hazards (*51*).

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. The long-term maximum county-level wind speed from all three wind models is shown in Fig. S1. If we repeat our analysis after taking the within-county spatial maximum rather than spatially averaging, we again find very similar effects (Table S2).

Other studies do not use purely physical metrics but instead incorporate socioeconomic damages into the TC metric in order to estimate the growth effects of these TCs (*13*). This approach is potentially problematic due to endogeneity between overall income impacts and direct economic losses (*11,15*), as well as non-classical measurement error in direct losses (*52*). We believe our approach, by using an entirely physical metric of TC exposure and relating it to both direct and indirect losses in the same regression framework, provides a more accurate assessment of TC impacts.

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 (*53*). 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.

Our primary metric of direct damages is data from SHELDUS, version 22.0 (*34*). We use all county-level property and crop damages, adjusted to 2022 dollars, where the hazard is listed as "Hurricane/Tropical Storm." Alternatively, we use data from EM-DAT (*47*) or the NOAA Billion-Dollar-Disasters database (*1*) (Fig. 3). 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, including the insurance industry, on-the-ground assessments made by emergency management agencies, power utility companies, and more. 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 and spending 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." Expanding the selected data to include other hazard types such as floods yields noisier but qualitatively similar results.

Data on the occurrence of disaster declaration are available at the county level; we match these data to our other county-year observations. The spending data covers three main FEMA disaster relief programs: the Individual Assistance program, the Public Assistance program, and the Hazard Mitigation program. Each of these programs is activated by a presidential disaster declaration. However, data on each of these programs is aggregated to different scales in FEMA data sources; for example, the Individual Assistance data may be listed by the ZIP code of the homeowner to whom the assistance is provided, whereas the Public Assistance data may include funds disbursed to city governments. To simplify our analysis of disaster spending (Fig. 2A), we aggregate these data to state-year totals.

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. The regression is weighted by county population and standard errors are clustered at the county level to adjust for autocorrelation within counties. In Fig. 1A, we use the same model, but we replace income growth with the natural log of direct TC damages from SHELDUS. In both cases we drop counties that never experience non-zero tropical cyclone winds in any year in the dataset. This regression model is therefore estimated on 132,997 observations, across 50 years and 2,661 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.

To quantify the effects of TCs in the years after they occur, we add lags of wind speed to the right-hand side. Following previous climate-economy work (*7, 28, 30, 54*), this approaches allows us to track the effects of TCs both in the year of occurrence and the following years and distinguish between transient and persistent impacts:

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

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):

$$g_{it} = \beta_1 W_{it} + \beta_2 W_{it} * D_{it} + \mu_i + \gamma_t + \theta_i t + \epsilon_{it} \tag{3}$$

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 $\beta_1 + \beta_2$.

When we recover a nonlinear response from the linear interacted model (Fig. 2D), we first predict the probability of a declaration as a function of wind speed, by re-running the main regression in Eqn. 1 with D_{it} as the dependent variable. The response of declaration probability to wind speed is shown in Fig. 2C. We then take this predicted probability of declarations at each wind speed ($P(D)_w$) and combine it with the interaction coefficients estimated from Eqn. 3 across a range of wind speeds W:

$$g_w = \beta_1 * W + \beta_2 * P(D)_w * W \tag{4}$$

We plot the resulting change in growth across this distribution of wind speeds in Fig. 2D.

Finally, to assess the long-term impacts of TCs with and without declarations, we modify the linear interacted model (Eqn. 3) to add lags of winds and declarations, similar to Eqn. 2:

$$g_{it} = \sum_{L=0}^{j} \left[\beta_{1L} W_{i(t-L)} + \beta_{2L} W_{i(t-L)} * D_{i(t-L)} \right] + \mu_i + \gamma_t + \theta_i t + \epsilon_{it} \tag{5}$$

In Fig. 3A, we present the sum of the β_{1L} and β_{2L} terms across the lags. 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. 3A 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 (*55*) and Callahan and Mankin (*29*).

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.

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 (*42–44*). 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 (Methods and Table S3). Specifically, we estimate the following logit model:

$$D_{it} = \beta_1 reelection\_yr + \beta_2 stafford + \beta_3 dem\_president * dem\_share + \mu_i + \epsilon_{it} \quad (6)$$

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.

We use a logit model instead of ordinary least squares in Eqn. 6 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 plug them into the linear interacted model in Eqn. 3. We again find losses without declarations and 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.

Comparison between our findings and Deryugina (2017)

Deryugina (*39*) 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. S5). That said, transfers do not appear to 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 (*20*). Additionally, there is substantial disaster-related spending outside of FEMA channels, such as through the Department of Housing and Urban Development (*40*) and the Small Business Administration (*21*). It is likely that both our analysis and that of Deryugina underestimate the total amount of disaster aid flowing to affected counties.

Deryugina (*39*) also found that earnings do not change significantly following hurricanes. There are several differences in our analysis that may explain this apparent discrepency. 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 much greater area of exposure (*23*). 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 (*39*)), often the same counties that are receiving disaster declarations in our data (Fig. 3E), which may counteract the effects of TCs. By using a linear model that does not either allow nonlinearity or 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 \$8.2 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 (*45*). Multiplying 8.2 trillion by 0.149 yields potential tax revenues of \$1.23 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 usefully highlights that the cost-benefit ratio associated with TC-focused disaster aid has the potentially to be extremely favorable.

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 (*53*). 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 S4), 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.

Treatment of SHELDUS damages data

County-level damages in SHELDUS are sometimes produced by allocating observed state-level observations equally across counties within a state, making them an imperfect representation of local damages (*56*). In Fig. 1A, we limit the SHELDUS data to exclude observations before 1997—when NWS storm data transitioned to an electronic database system—and exclude observations where multiple counties have the exact same values in a given month. Both of these choices increase our confidence that the filtered SHELDUS data are representative of local county-level damages (*56*).

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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].

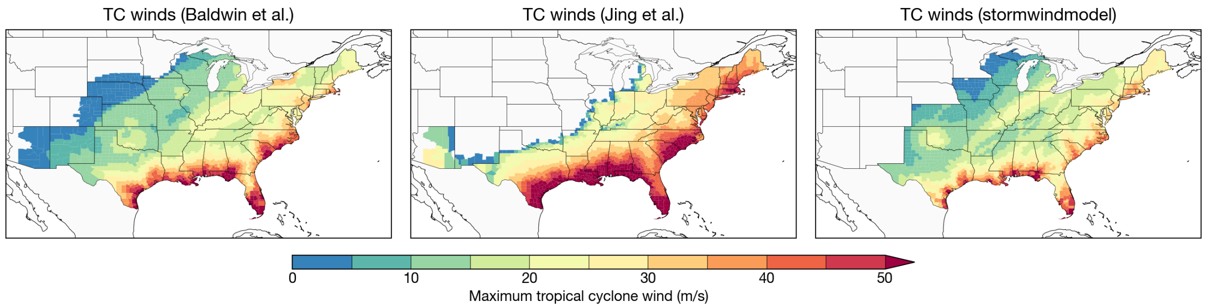

Figure S1: TC wind speeds. Long-term maximum TC wind speeds in each county over 1970–2019 using wind field models from three distinct sources: Baldwin et al. (*23*) (left), Jing et al. (*24*) (middle), and the `stormwindmodel` R package (*49*) (right).

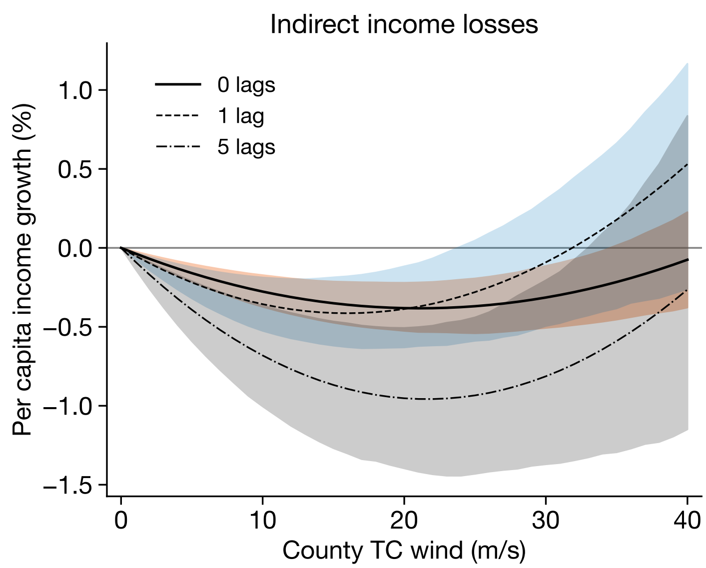

Figure S2: Contemporaneous and lagged nonlinear effects. Solid black line and orange shading show the quadratic effect of TCs on income growth in the year of their occurrence (same as Fig. 1B). Dashed black line and blue shading show the cumulative quadratic effect after an additional year, and dashed-dotted line and gray shading show the cumulative quadratic effects after five additional years. Shading denotes 95% confidence intervals calculated by bootstrapping by county.

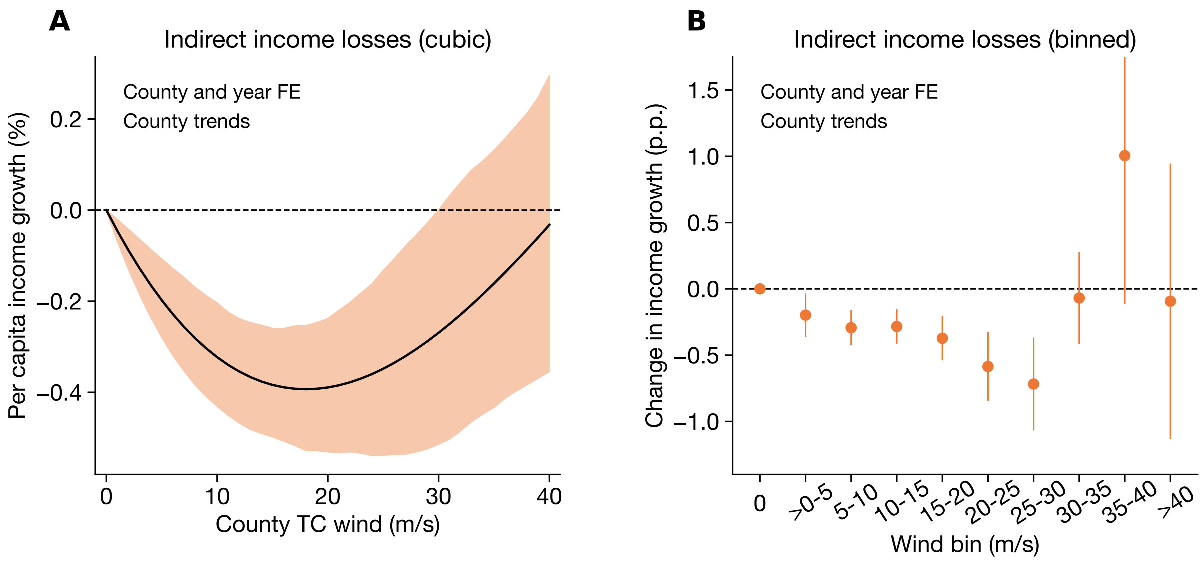

Figure S3: Cubic and binned models. Regression of county per capita personal income growth on county TC winds, using a cubic instead of quadratic specification (A) or a binned specification (B). 95% confidence intervals are shown with shading in A or lines in B. Coefficients in B are referenced to the 0-m/s bin, so they denote the change in growth associated with moving from 0 m/s to each bin.

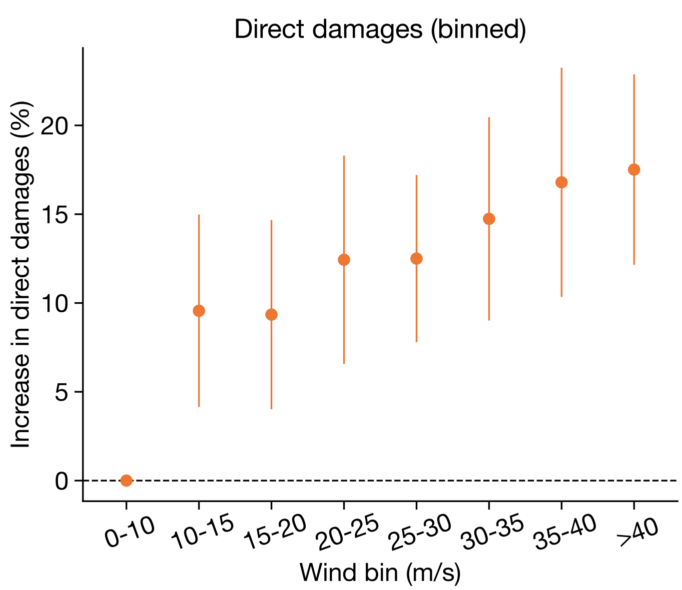

Figure S4: Direct TC damages as a function of binned wind speeds. Each coefficient shows the percent change in direct crop and property damages when a county falls into the specified bin, relative to the bin of 0-10 m/s. Dots show point estimate and bars show 95% CIs, clustered by county.

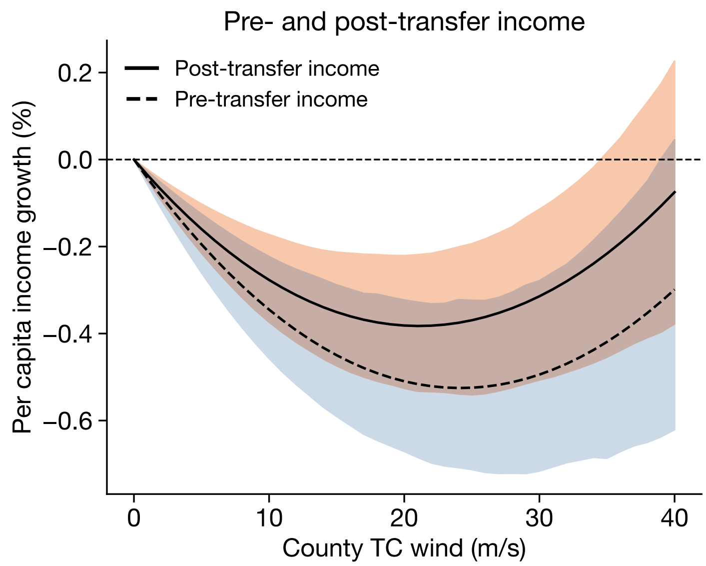

Figure S5: Effects on pre- and post-transfer income. Our result in the main analysis uses total post-transfer income, which is shown here in the solid line. Dashed line shows the effect of TC winds on pre-transfer income, meaning income excluding unemployment insurance, Social Security benefits, medical benefits, and veterans' benefits. Shading shows 95% confidence intervals calculated by bootstrapping by county.

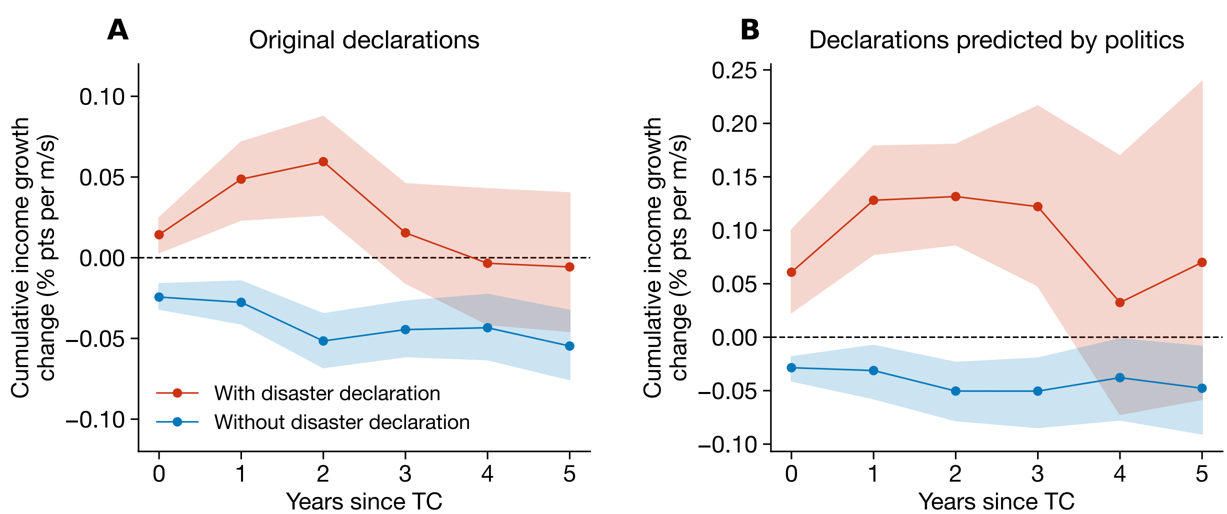

Figure S6: Effect of declarations predicted using political factors. A) Original results for the lagged effects of TCs with and without disaster declarations, using a distributed lag model (same as Fig. 3A). B) Results for the same analysis applied to declarations when predicted using political factors (Methods and Table S3) rather than observed declarations. In both panels, shading shows 95% confidence intervals calculated by bootstrapping by county.

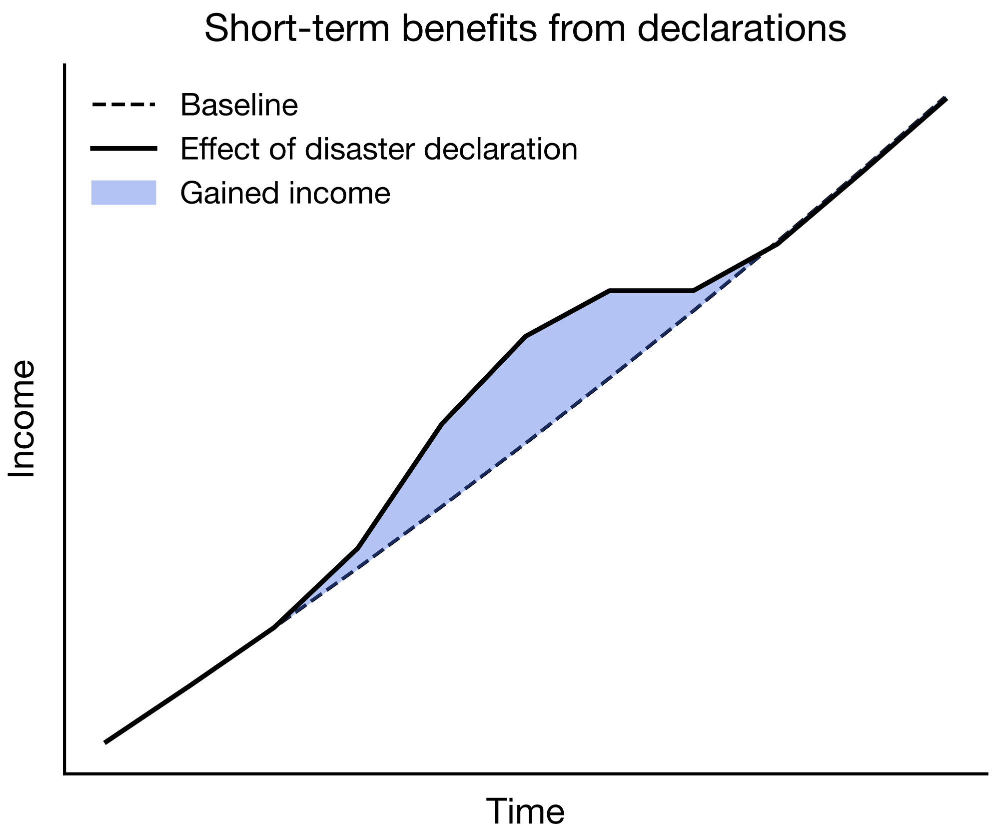

Figure S7: Schematic short-term benefits from disaster declarations. This figure shows a stylized example of the income effects of disaster declarations, similar to the cumulative growth response in Fig. 3. Disaster declarations benefit growth starting in the fourth year of this simple simulation. Income remains above the baseline trajectory for three years, after which growth declines and income returns to its original trend. In the meantime, there is substantial income gained relative to that baseline, shown in the blue shading.

	(1)	(2)	(3)	(4)
Wind	-0.0360^{***}	-0.0343^{***}	-0.0360^{***}	-0.0506^{***}
	(0.0068)	(0.0066)	(0.0065)	(0.0060)
Wind^2	0.0008^{***}	0.0008^{***}	0.0008^{***}	0.0014^{***}
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Trends	Yes	No	Yes	Yes
Clustering	County	County	State	County
Weighting	Yes	Yes	Yes	No
Observations	132997	132997	132997	132997

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

Table S1: Marginal effects of TC and its square on per capita personal income growth. Column 1 shows our baseline model, column 2 shows the model without county-specific trends, column 3 shows the model when clustering by state, and column 4 shows the model when not weighting by county population. Regression coefficients are multiplied by 100 so they are directly interpretable as percentage points.

	(1)	(2)	(3)	(4)
Wind max	-0.0360^{***}			
	(0.0068)			
Wind max^2	0.0008^{***}			
	(0.0002)			
Wind mean		-0.0129		
		(0.0240)		
Wind mean^2		-0.0017		
		(0.0020)		
Wind sum			-0.0112^{***}	
			(0.0032)	
Wind sum^2			0.0002^{***}	
			(0.0000)	
Wind spatial max				-0.0351^{***}
				(0.0061)
Wind spatial max^2				0.0008^{***}
				(0.0002)
Observations	132997	132997	132997	131997

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

Table S2: Effects of TC winds on per capita personal income growth using four different annual wind aggregations. 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). In columns (1), (2), and (3), we spatially average across the county. In column (4), we take the spatial maximum within-county wind after taking the annual maximum across storms. County and year fixed effects and county trends are included in all models, standard errors are clustered by county, and regressions are weighted by county population. Regression coefficients are multiplied by 100 so they are directly interpretable as percentage points.

	(1)
Reelection year	0.42
	(0.26)
Post-1988	3.60^{***}
	(0.30)
Democratic president	-5.39^{***}
	(1.33)
State Democratic presidential vote share	2.03
	(2.20)
Dem. president × Dem. vote share	8.65^{**}
	(2.75)
Observations	111364

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

Table S3: Effect of political factors on the probability of a county-level disaster declaration, calculated using a logit model. Standard errors are clustered by state.

	(1)	(2)
Wind max	-0.0360^{***}	-0.0349^{***}
	(0.0068)	(0.0067)
Wind max^2	0.0008^{***}	0.0008^{***}
	(0.0002)	(0.0002)
Observations	132997	130447

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

Table S4: Effect of TC winds on per capita personal income growth when excluding Virginia counties whose incomes were imputed. Column (1) shows our main model. Column (2) shows a model 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 model, we divide the income and population of these combined groups equally among the counties that comprise them (see Supplementary Text).

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