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ECONOMIC DEVELOPMENT

The Unintended Consequences of Post-Disaster Policies for Spatial Sorting

CRED Research Paper No. 37

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Posted: June 2022

Last revised: March 2023

Abstract

We provide new empirical and theoretical evidence on the spatial consequences of public policies driven by electoral motives. Using exogenous variation in the timing of natural disasters, we show that hurricanes occurring close to Election Day in the United States lead to increased local post-disaster efforts. These electorally motivated measures lead populations to sort into hazard-prone areas. To comprehend the aggregate implications of this sorting pattern, we introduce the relationship between electoral cycles and public policies in a spatial equilibrium model. These electorally motivated policies generate considerable productivity and output losses without being compensated by aggregate welfare gains.

Key words: Natural Disasters, Electoral Cycles, Fiscal Policies, Spatial Sorting

JEL classification: H7, H84, P48, R12, R13

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1 Introduction

Electoral motives often affect the allocation of resources for public policies. In targeting specific groups or places, incumbents may strategically try to maximize their reelection prospects. At the same time, shifting fiscal resources across the economy alters the relative attractiveness of locations. The resulting spatial sorting may lead to inefficient equilibrium outcomes with sizable aggregate implications since households usually neglect the impact of their location decisions on other agents. Consequently, when public policies are at the center of voters' attention, electoral motives may alter the spatial distribution of fiscal resources with significant spillovers on the rest of the economy.

This paper focuses on post-disaster measures, a particularly salient policy matter for many political candidates when large disasters occur amid an electoral campaign. We document a trade-off between providing legitimate aid to affected populations and encouraging more people and capital to stay in hazard-prone areas. Indeed, transfers to regions affected by natural disasters can revitalize the local economy and deter workers and businesses from relocating at the cost of shifting the labor force away from the economy's most productive regions. In the United States, counties that received federal post-disaster assistance following a hurricane grew, on average, 4.97% faster than the rest of the country between 2001 and 2019 (i.e., $5,900$ more individuals per county).[1] We claim that electoral motives cause a misallocation of post-disaster resources large enough to distort the spatial distribution of economic activity in the United States.

To identify the impact of the electoral cycle on local post-disaster efforts in the United States, we exploit the timing of hurricane landings relative to Election Day as an exogenous variation of natural disasters with greater electoral importance.[2] Election Day occurs on the first Tuesday of November in even years; because the hurricane season occurs between June and November, it is a salient electoral matter every other year. Additionally, whether hurricanes' timing is concurrent with Election Day is as-if random, giving us an adequate empirical setting to leverage variation in post-disaster policy efforts. Conditional on location and time fixed effects, the quasi-random trajectory of hurricanes implies that we can take the exact place and landfall time of a hurricane as exogenous.

[1] We identify counties obtaining post-disaster assistance as those receiving a disaster declaration from the President of the United States after a hurricane, excluding declarations related to the hurricane Katrina evacuation plan, which incorporated far-away, unaffected counties. For comparison, we focus on the continental United States as, between 2001 and 2019, 99% of all counties within coastal states received either an emergency or major disaster declaration from federal authorities. We extract disaster declaration information from the FEMA Open Database and population data from the SEER database.

[2] Election Day happens every other year and directly affects the composition of Congress, which votes on different budgetary accounts such as the Disaster Relief Fund. Indeed, there is a general election every two years to replace the entire House of Representatives and a third of the Senate. Voters generally decide on many other federal offices on that same day, such as the presidential office (every four years) and, depending on the location, gubernatorial and local offices.

Using the timing of hurricanes as an exogenous shock, we study local budget responses to electorally driven post-disaster efforts and the subsequent population sorting at the county level between 2001 and 2019. To achieve this analysis, we employ an event study approach (Callaway and Sant'Anna, 2021) to estimate the dynamic consequences of hurricanes occurring less than a year before Election Day (i.e., "on-cycle" hurricanes) and hurricanes occurring more than a year before Election Day (i.e., "off-cycle" hurricanes).

We find that local public goods provision increases significantly by, on average, 4.1% in counties hit by on-cycle hurricanes compared to the rest of the U.S. counties. At the same time, the population rises significantly and permanently by an average of 4.7%. In contrast, we do not find statistically significant responses when off-cycle hurricanes strike. Importantly, these results remain qualitatively similar when using alternative estimators or control groups, controlling for storm intensity, or considering more extended periods or other extreme natural disasters such as floods or wildfires. In line with the literature (e.g., Besley and Case, 1995; Besley and Burgess, 2002; Eisensee and Strömberg, 2007; Healy and Malhotra, 2009), using alternative political dimensions (e.g., political alignment) instead of electoral cycles supports the hypothesis that electoral motives powerfully drive post-disaster efforts.

We also find that the increased local public goods provision is not financed by local authorities but rather by intergovernmental transfers that shift other regions' fiscal resources to the impacted areas. This electorally motivated intervention leads to a temporary contraction of the local economy by diverting private labor demand to non-tradable, government-sponsored activity. Nevertheless, such electoral-motivated efforts also improve local amenities by upgrading roads, energy grids, and waste management beyond pre-disaster standards. This latter amenity channel is critical to rationalizing population sorting patterns after an on-cycle hurricane.

To quantify and comprehend the implications of these distortions for the aggregate economy, we embed electoral-cycle-driven public policies in a dynamic spatial equilibrium model (Desmet and Rossi-Hansberg, 2015; Desmet et al., 2018, 2021; Cruz and Rossi-Hansberg, 2021). We interpret the public policy component of the post-disaster policies as a fiscal transfer scheme that reallocates resources across space. Moreover, we allow both the amenity and the production functions to be shocked by the government's intervention in local quality of life and productivity.

We choose parameter values that reflect the U.S. economy. In particular, we identify the causal effects of major hurricanes on amenities, productivity, tax rates, and transfer rates across the electoral cycle at the county level. Our calibrated model economy generates quantitatively equivalent population and wage responses in reaction to post-disaster policies along the electoral cycle, as observed in the data. We then use our quantified model to evaluate the implications of alternative versions of post-disaster policies for the aggregate economy. We simulate the model forward for 80 years under two scenarios: a baseline scenario that is simulated under the current electoral-cycle-driven post-disaster policy, and a counterfactual scenario where we remove the electoral cycle's impact.

By simulating our dynamic spatial model under plausible parameter constellations, we find that these distortions are large enough to affect the aggregate economy. Continuing the current electoral-cycle-driven post-disaster policy lowers aggregate productivity and output after 80 years and leads populations to sort to hazard-prone coastal areas. The spatial reallocation of labor into exposed regions decongests highly productive areas such as New York, Chicago, and the San Francisco Bay Area. Fewer workers live in dense, productive regions, while less crowded coastal regions around the Gulf of Mexico, Southeast Florida, and the Atlantic Ocean become endogenously more productive. Most of this population sorting pattern is due to the local quality of life improvements in on-cycle counties. The excessive redistribution of public resources and subsequent redevelopment after on-cycle events impact substantially large productive areas, causing sizable aggregate productivity and output losses. The latter productivity effect cancels amenities' positive impact, leaving social welfare unaffected.

In particular, we calculate that the current post-disaster policies increase on-cycle county populations by up to 13.06% in 80 years at the expense of high-productivity cities. As a result, the current electoral-cycle-driven post-disaster policy decreases the present discounted value of real GDP by 1.17% relative to our counterfactual scenario without the impact of the electoral cycle on the post-disaster economy. The resulting welfare change is only +0.17%. Furthermore, we compute the efficiency cost of maintaining the current policies (Mayshar, 1990; Hendren and Sprung-Keyser, 2020) and show that for every dollar worth of electorally motivated post-disaster transfer, U.S. residents would be willing to pay only $0.43.

We conclude that politically motivated public policies that shift fiscal resources across space have significant spillovers on the rest of the economy. Incumbent governments are incentivized to deliver more post-disaster aid when electoral accountability is high. By doing so, these electoral-cycle-driven post-disaster policies shift labor into natural hazard-prone coastal areas away from the most productive American cities. Social welfare will presumably decline if electoral motives extend post-disaster measures further across the electoral cycle.

In quantifying the spatial consequences of public policies along the electoral cycle, we intend to combine the lessons of the economic geography and political economy literature in a unified, consistent framework. In general, electoral motives, embedded in institutional design, play a significant role in resource allocation in the economy (Sieg and Yoon, 2017; Finan and Mazzocco, 2021). The political economy literature has long demonstrated the importance of the electoral cycle for redistributing fiscal resources as policymakers tend to increase expenditures and delay tax increases close to an election (Rogoff, 1990; Rogoff and Sibert, 1988; Besley and Case, 1995). Post-disaster measures often belong to this class of electorally motivated redistribution. Adverse shocks, such as hurricanes, generally reduce local governments' budget capacity to recover (Jerch et al., 2023), justifying higher-governments' fiscal support. However, the importance and location of these policies appear to be driven by electoral accountability (Besley and Burgess, 2002; Strömberg, 2004; Eisensee and Strömberg, 2007; Healy and Malhotra, 2009; Cole et al., 2012; Gagliarducci et al., 2019).

To our knowledge, this literature has never studied the impact of the subsequent location decision distortions on aggregate output and welfare. Indeed, the economic geography literature has long emphasized the equity efficiency trade-off brought by resources spatial redistribution (Glaeser and Gottlieb, 2008; Kline and Moretti, 2014) and recently challenged by the introduction of heterogenous workers (Gaubert et al., 2021). This fundamental trade-off eventually led researchers to focus on the optimal redistribution design that would minimize efficiency costs (Albouy et al., 2019; Fajgelbaum et al., 2019; Fajgelbaum and Gaubert, 2020). However, this class of models assumes the existence of a benevolent planner acting on behalf of the median voter and, respectively, abstracts from any political economy mechanism. This paper further underscores the crucial role of government intervention in the mechanisms driving population spatial sorting after a natural disaster. An extensive strand of the literature is interested in documenting the impact of unexpected adverse shocks, particularly natural disasters, on economic activity and spatial sorting (Davis and Weinstein, 2002; Hornbeck, 2012; Boustan et al., 2012; Hauer, 2017; Spitzer et al., 2020; Mahajan and Yang, 2020; Kocornik-Mina et al., 2020). These shocks directly affect location decisions through productivity and output (Strobl, 2011; Dell et al., 2012, 2014; Boustan et al., 2020) or changes in local amenities, such as the perception of future risks (Deryugina, 2013; Gallagher, 2014; Howe et al., 2015; Bakkensen and Barrage, 2021; Gibson and Mullins, 2020; Bakkensen and Ma, 2020; Bernstein et al., 2021). Local quality of life is a crucial determinant of population sorting (Rosen, 1979; Roback, 1982; Albouy and Lue, 2015; Albouy, 2016; Diamond, 2016; Ahlfeldt et al., 2020) with potential long-lasting implications (Lee and Lin, 2018; Heblich et al., 2021).

This literature, however, often abstracts from the effect of endogenous public policy responses on spatial sorting. Consistent with the Dutch disease hypothesis, post-disaster intervention might cause a temporary contraction of the local economy (Corden and Neary, 1982; Rajan and Subramanian, 2011; Allcott and Keniston, 2017; Bulte et al., 2018), and at the same time, transfers to households and local authorities—e.g., as targeted revitalization programs—might translate into positive amenity spillovers (Rossi-Hansberg et al., 2010; Diamond and McQuade, 2019; Fu and Gregory, 2019). The initial effect of the government intervention on local amenities might also later drive population sorting via local multipliers (Chodorow-Reich, 2019; Corbi et al., 2019). We simultaneously consider government interventions' impact on amenity and productivity channels and document how each might influence population sorting across space.

Finally, we provide new empirical and theoretical methodology contributions. Empirically, we introduce the timing of hurricanes relative to the electoral cycle as a novel source of exogenous variation in post-disaster efforts. Interested researchers may extend the electoral timing of disasters to a more extensive set of exogenous catastrophes (e.g., tornados, droughts) and elections (e.g., municipal, gubernatorial). The timing of natural disasters to an election can then help leverage important information about the role played by electoral motives on other post-disaster outcomes, such as green bill production (Gagliarducci et al., 2019) or local partisan sorting (Bernstein et al., 2021). Theoretically, we extend an economic geography dynamic spatial equilibrium framework (Desmet and Rossi-Hansberg, 2015; Desmet et al., 2018, 2021; Cruz and Rossi-Hansberg, 2021; Conte, 2022) by including a government sector (Henkel et al., 2021) influenced by a political economy feature in the form of an electoral cycle. This novel extension allows one to study the aggregate consequences of a wide range of public policies beyond post-disaster efforts and whose implementation may vary along the electoral cycle, such as corporate taxation (Foremny and Riedel, 2014), value-added taxation (Hallerberg and Scartascini, 2017; Benzarti et al., 2020), or public services efficiency (Bertoli and Grembi, 2021).

The rest of the paper is as follows. Section 2 provides information on fiscal revenues redistribution, post-disaster policies in the U.S., and hurricanes. Section 3 summarizes the data sources, and Section 4 presents our empirical strategy and results. Section 5 develops the dynamic spatial general equilibrium model we use to interpret the empirical findings. Section 6 describes the quantification of the model, while Section 7 documents the counterfactual analysis. Section 8 concludes.

2 Hurricanes and Post-Disaster Policies

In this section, we provide some background information on the spatial distribution of fiscal resources and post-disaster policies in the U.S. as well as hurricanes, our main treatment.

2.1 Fiscal Resources and Post-Disaster Policies

Fiscal revenues redistribution. We are interested in the spatial distribution of fiscal revenues in the United States, which occurs via various public policies or programs and provides macroeconomic stabilization across regions (Farhi and Werning, 2017). One crucial source of public funds for local governments is intergovernmental transfers (Agrawal et al., 2022; Bruce et al., 2019; Carlino et al., 2023). Concurrently, transfers from the federal government and states to local governments constitute around 3.43% of total GDP, and 41% of local budgets come from the federal government and states (Government Finance Database). Federal assistance expansion often happens during economic downturns and after catastrophic events like natural disasters. In particular, between 2001 and 2019, the federal Disaster Relief Fund (DRF) provided approximately $15.7 billion (in current dollars, constant (FY2012)) annually to state, local, and tribal agencies and groups through grants and cooperative agreements (Painter, 2019). The spatial redistribution of tax revenues and transfers affects the allocation of economic activity through local spending and subsequent welfare (Fajgelbaum and Gaubert, 2020; Henkel et al., 2021). Additionally, redistributing resources from high- to low-productive areas may affect aggregate output (Albouy, 2009; Colas and Hutchinson, 2021; Hsieh and Moretti, 2019; Wildasin, 1980). It is, therefore, crucial to understand the underlying institutional structure and electoral motives—beyond economic efficiency considerations—that affect this redistribution of public resources.

Post-disaster policies. In the United States, local governments' post-disaster policies are financially supported by the DRF, which is voted on and funded by Congress and is managed by the Federal Emergency Management Agency (FEMA). The DRF is the primary funding source for the federal government's domestic general post-disaster programs. Other post-disaster programs exist, such as Small Business Administration loans. However, FEMA remains historically the largest provider of post-disaster assistance through the DRF. Importantly, catastrophic events (i.e., causing over $500 million in projected costs to FEMA) are the major driver of DRF funding (Painter, 2019). These disasters are often caused by hurricanes and subsequent hazards (such as floods and storm surges). Out of the 31 disasters for which FEMA provided more than $500 million in assistance between 2001 and 2019, 28 were caused by severe storms or hurricanes.[3]

States must request a Presidential Declaration of Disaster (PDD) to access federal support for natural disasters.[4] The Stafford Act of 1988 defined a major disaster as "any natural catastrophe ... or, regardless of cause, any fire, flood, or explosion, in any part of the United States." As specified by the 1988 amendment, a PDD identifies counties eligible for federal assistance and the associated grant programs. These programs make state and local governments and individuals eligible for relief or preparation funds, most of which are financed by the DRF. As a result of the Stafford Act, the president and, in general, the executive branch has considerable discretionary powers over post-disaster intervention.

When a county receives a PDD, local governments and individuals can apply for FEMA support for immediate relief (e.g., debris removal) or mitigation of future risks (e.g., road elevation). While the most extensive amounts are allocated in the immediate aftermath of the disasters, DRF appropriations build on the subsequent years to address long-term recovery costs (Painter, 2019). For instance, the DRF Monthly Report of January 2023 confirms fundings for counties affected by Hurricane Sandy, which occurred in 2012. These later transfers complement the initial relief in providing or adjusting local mitigation plans and infrastructures. Finally, social insurance programs also endogenously react to natural disasters. In particular, Deryugina (2017) shows that significant transfers from traditional safety net programs (such as unemployment insurance or Medicare) occur in the years following federal disaster assistance.

2.2 Hurricanes in the United States

Tropical origins and season. Hurricanes are tropical cyclones forming in the North Atlantic basin characterized by a rotating storm system involving high winds and a low-pressure center. They typically form as moisture rises above the water, generally in areas of pre-existing low pressure. This process requires waters to reach temperatures of at least 26°C (80°F), which, in the Atlantic, happens only in tropical or subtropical regions during the summer. As moisture rises, it attracts more air above the water, causing more moisture to ascend. Large, heavy clouds form as the humid air cools off. Because of Earth's spin and the Coriolis effect, the storm starts rotating counterclockwise and generally moving west. During the late summer season, vertical wind shear is less present, thus favoring the creation of cyclones with appropriate heat and humidity conditions.

[3] See the disaster summaries provided by (FEMA).

[4] While states' governors can issue declarations of disaster, states do not generally provide substantial relief (Sylves, 2019).

According to the National Center for Atmospheric Research, hurricanes can be up to 600 miles (ca. 966 kilometers) long and have powerful gusts spiraling upward from 75 mph (ca. 121 kilometers per hour) to 200 mph (ca. 322 kilometers per hour). They usually last over a week, moving 10–20 mph over the open ocean. When the cyclone makes landfall, it loses its fuel—the ocean moisture—and quickly vanishes. Because of the conditions mentioned above, the hurricane season spans between June and November in the North Atlantic basin.

Classification and frequency. Like most natural disasters, hurricanes do not simply appear but are specific storms that have evolved from milder conditions. In particular, a hurricane is a tropical storm that is a type of tropical depression. A tropical depression is classified as a tropical storm when maximum sustained winds reach 39 mph (ca. 63 kilometers per hour).

The Saffir-Simpson scale[5] categorizes hurricanes on a 1-to-5 scale based on the maximum one-minute sustained winds (which does not account for other related hazards, such as storm surge or heavy rainfall). A tropical storm becomes a category one hurricane when winds get up to 73 mph (ca. 117 kilometers per hour). A "major" hurricane is of category 3 ($\geq$ 111 mph (ca. 179 kilometers per hour)) or above. A hurricane enters the top category—category 5—when maximum sustained winds reach 157 mph (ca. 253 kilometers per hour). Examples of category five hurricanes include Dorian, who caused significant fatalities and destruction to the Bahamas in 2019, and Andrew, who struck Louisiana and Florida in 1992.

Appendix Table A.1 summarizes the number of historical hurricanes in the North Atlantic basin from 1851 to 2019. This paper focuses on the impact of hurricanes (i.e., rotating storm systems characterized by at least 73 mph maximum winds) that made landfall on the U.S. Atlantic coast between 2001 and 2019.

3 Data

We combine data from several U.S. sources in our empirical analysis. To investigate the relationship between post-disaster policies and the sorting responses of economic activity, we collect yearly data from the International Best Track Archive for Climate Stewardship (IBTrACS), the Surveillance Epidemiology and End Results (SEER) population database, the Bureau of Economic Analysis (BEA), and the Government Finance Database, at the county level in the continental U.S. between 2001 and 2019. Our main economic variables are available across all counties and sources from 2001 on-

[5]See the Saffir-Simpson Hurricane Wind Scale Extended Table provided by the National Hurricane Center (NHC).

wards, and population data from 1969 onwards. We combine these data sources with various other information at the county level.

3.1 Data Sources

IBTrACS. The IBTrACS dataset, provided by the National Oceanic and Atmospheric Administration (NOAA), includes the spatial and temporal distribution of tropical cyclones worldwide. It is one of the complete global sets of historical tropical cyclones available. We use the reported timing—every six hours—and the extent of hurricanes. The list of the U.S. hurricanes included in our sample is summarized in Table A.2. Over our sample periods, 33 hurricanes occurred, all during the hurricane season, between July and November. The intensity and the wind speed of tropical cyclones vary across hurricanes. We match the date of hurricanes (Column (1)) to the closest Election Day date (Column (9)) and calculate how many days are left until the next Election Day when the hurricanes hit. We define a hurricane as an on (off) cycle event if the next election is set less (more) than 365 days after the hurricane hits.

To extract local wind speeds, we use the CLIMADA wind field model, supported by the ETH Zürich. The algorithm essentially computes the one-minute sustained peak gusts in each cell of a gridded map as the sum of a circular wind field (Holland, 2008) and the translational wind speed that arises from the storm movement (Aznar-Siguan and Bresch, 2019). We bring the IBTrACS hurricane data to this model and define a grid cell resolution of 0.12 degrees (approximately 12 km).

SEER population database. The SEER population database provides detailed population counts by age, gender, and race since 1969. This database, already used by Deryugina (2017), provides practical, clean, and consistent intercensal estimates. The original population source is taken from the U.S. Census.

BEA. The BEA stores useful county-level information spanning back from at least 2001 (and up to 1969) to nowadays. We collect information on GDP, employment, personal wages, incomes, and non-disaster transfers (e.g., Medicare and unemployment benefits). We also download this information by NAICS industry when available. Combined, these data provide detailed and precise information on the counties' labor market and production profiles between 2001 and 2019.

Government Finance Database. The Government Finance Database (Pierson et al., 2015) collects, cleans, and classifies in a standard way the information provided by the Census of Governments.

We compile local public finance information for spending and revenue categories aggregated at the county level. Because this information is only available every five years (years ending with "2" or "7"), we infer intercensal data points from the corresponding yearly series available at the state level at the Tax Policy Center.

Miscellaneous sources. We further collect and aggregate at the county level information from different secondary sources, including FEMA post-disaster grants and PDDs freely accessible on the FEMA webpage, County Business Patterns, county-to-county migration data from the Internal Revenue Service's (IRS) SOI Tax Stats, and electoral data, collected at the county and state levels from the MIT Election Lab.

3.2 Defining the Treatment

In our preferred specification, we define counties hit by hurricanes as those within a hurricane's radius of maximum winds. This definition presents the advantage of only considering the same six-hour (maximum) winds within a storm. Nonetheless, these wind intensities might differ across different categories of hurricanes (i.e., two counties hit simultaneously by other hurricanes might be differently affected). Later, we show that our results remain unaffected when defining treated areas as hit by major hurricane winds (i.e., ≥ 50 m/s). This second definition has the advantage of considering specific wind categories (i.e., category 3 and above) based on a hurricane's absolute wind field distribution. However, wind velocities within a storm might not be similar (i.e., two counties hit simultaneously by the same hurricane might be differently affected). We will show that the results are qualitatively robust to alternative specifications and controlling for wind speeds, regardless of whether the threshold is determined by the hurricane's radius of maximum winds or by the Saffir-Simpson expression.

We define hurricanes occurring less than a year before Election Day (i.e., "on-cycle" hurricanes) and hurricanes occurring more than a year before Election Day (i.e., "off-cycle" hurricanes). The regular hurricane season lasts from June to November in the North Atlantic basin, but most hurricanes occur during the fall. This timing, which takes its roots in the tropics' weather conditions, is orthogonal to Election Day that Congress set, in 1845, on the first Tuesday of November to allow farmers to travel to the polling station after the fall harvest. Between 2001 and 2019, 33 hurricanes hit the U.S. Atlantic coast, with 17 occurring less than 365 days before Election Day (on-cycle disaster) and 16 happening more than 365 days after (off-cycle disaster). On average, a cyclone in our sample occurred 268 days before Election Day. However, this number varies greatly due to the timing specific to the

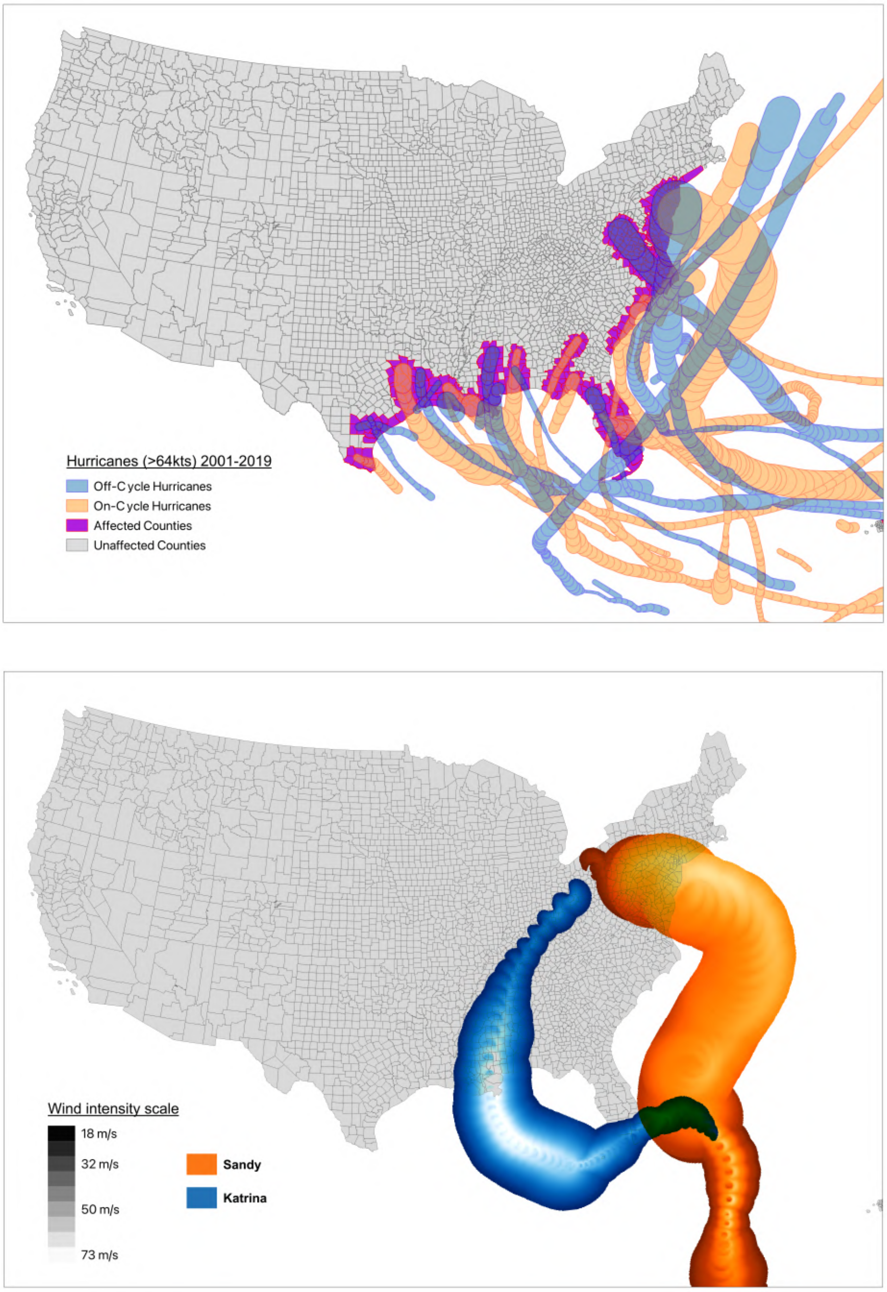


Figure 1: On- vs. Off-Cycle Hurricanes' Spatial Distribution and Wind Extent

Notes: The top panel displays the radius of maximum winds of all hurricanes that have made landfall in the U.S. between 2001 and 2019 using IBTrACS hurricane data. The bottom panel displays the wind distribution of hurricanes Sandy and Katrina according to the CLIMADA wind field model using IBTrACS hurricane data. Saffir-Simpson wind intensities: tropical storms (18–32 m/s), minor hurricanes (categories 1 and 2; 32–50 m/s), major hurricanes (categories 3–5; $\geq$50 m/s).

hurricane season. In our sample, 51.5% of the hurricanes occurred less than 365 days before Election Day (on-cycle), and nearly half of the counties ever hit were hit by an on-cycle hurricane. Importantly, the average wind speed during on-cycle hurricanes (90 mph) is not statistically different from that of off-cycle hurricanes (94 mph). Similarly, the average air pressure during on-cycle hurricanes (966 mb) is not statistically different from the average air pressure during off-cycle hurricanes (963 mb).[6] Appendix Table A.2 provides detailed information about the timing and extent of on- and off-cycle hurricanes.

The upper panel in Figure 1 shows our sample's spatial distribution of on- and off-cycle hurricanes. Here, the extent is defined by the radius of maximum winds. On- and off-cycle hurricanes are geographically balanced. The bottom panel in Figure 1 shows how the CLIMADA wind field model applies to two of the most famous on- and off-cycle hurricanes of our sample period—Sandy and Katrina, respectively, and displays how the absolute wind intensities diffuse in space. We later use this information to control for local wind intensities and distinguish hurricanes' impacts by wind category. Finally, Appendix Table A.3 displays the 2001 values of our main outcomes of interest—local population and public budget characteristics— for counties ever hit by a hurricane between 2001 and 2019. Counties hit first by on-cycle hurricanes are statistically similar to counties hit first by off-cycle hurricanes along these attributes.

4 Empirical Evidence

In this section, we employ a difference-in-difference strategy adapted for staggered designs to determine how electoral motives, as proxied by the timing of hurricanes relative to Election Day, affect the provision of local public goods and the subsequent population sorting after a disaster.

4.1 Empirical Strategy

We are primarily interested in understanding how hurricanes affect the relative attractiveness of locations across the electoral cycle compared to the rest of the economy. Throughout the analysis, our identifying assumption is that, conditional on county and year fixed effects, a hurricane's occurrence and path are as good as random. A fortiori, the occurrence of a hurricane in a given place at a specific temporal distance from the next Election Day is as good as random and is therefore uncorrelated with other local economic shocks. In estimating the impact of on- and off-cycle hurricanes (i.e., our treat-

[6] The standard storm surge associated with these values is typically 9 to 12 feet high and 50 to 100 miles wide. See the online glossary provided by the U.S. Southeast Coastal Ocean Observing Regional Association (SECOORA).

ments) on the outcome variables, we would ideally compare treated areas to identical but unaffected areas. However, the empirical challenge lies in specifying an appropriate comparison group to recover causal effects. Compared to an ideal setting, control counties might neither be perfectly identical (e.g., if they are far away from exposed regions) nor completely unaffected by hurricanes (e.g., located in exposed regions).

First, our comparison group could be contaminated because hurricanes are staggered events. The difference-in-difference literature has recently pointed out potential threats to identification when treatment timing varies across units and periods (Borusyak et al., 2022; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021; Baker et al., 2022). With treatment rollout, already-treated units act as controls, potentially leading to average treatment effects of the opposite sign. To address this issue, we implement Callaway and Sant'Anna (2021)'s estimator, which extends Sant'Anna and Zhao (2020)'s procedure to staggered treatment designs. Earlier literature acknowledged the difficulties in finding ideal comparison groups and developed several semi or non-parametric techniques to improve the chances that the parallel trend assumption holds conditional on covariates (e.g., Abadie, 2005; Heckman et al., 1997, 1998). The Doubly robust estimator, recently designed by Sant'Anna and Zhao, 2020 and extensively used in this paper, builds on this literature.[7]

However, accounting for staggered designs and using the Doubly robust estimator might not guarantee the validity of our strategy, as our control group could fundamentally differ in trends from our treated units. For example, if the population declines in never-treated counties compared to treated ones before the treatment, there is a high chance it would do so past the treatment. In this case, one would overestimate the average impact of hurricanes on population sorting. A natural approach to make comparisons across units as accurately as possible is to restrict the control group only to areas subject to being hit by hurricanes.[8] However, satisfying the pre-trend requirements does not ensure parallel post-treatment outcome evolution. In particular, restricting the sample of analysis to coastal counties suffers from potential spillover effects, likely affecting post-treatment trends.

Indeed, because hurricanes are geographically clustered events, physically closer control units will mitigate the risks of violating the pre-trend assumption at the expense of the control group no longer

[7] In the case of large natural disaster shocks such as hurricanes, the Callaway and Sant'Anna (2021) estimator might be preferable over the class of imputation estimators (e.g., Borusyak et al., 2022; Gardner, 2022; Wooldridge, 2021), also suited for staggered treatment designs, as serial correlation in population and local public good provision might be high. See Roth et al. (2022) for a discussion.

[8] For instance, Strobl (2011) focuses on the coastal counties defined by the Strategic Environmental Assessments Division of the National Oceanic and Atmospheric Administration (NOAA). Similarly, Deryugina (2017) restricts her sample to counties belonging to Atlantic coastal states to avoid including units that may fundamentally differ in trends from the treated ones before the shock occurs.

identifying the counterfactual trend (Butts, 2021). For instance, unaffected neighboring regions might indirectly benefit from increased public spending in affected areas, stimulating labor markets and local trade networks (Belasen and Polachek, 2008, 2009). Further, unaffected areas in the same exposed region could not benefit from improved quality of life through collaborative risk reduction programs,[9] hence reducing disamenities from exposure and thus attracting more populations (McNamara and Keeler, 2013). Such spillovers and endogenous responses might impact both treated counties and unaffected but physically close or similar counties along the coast. If hurricanes, especially occurring close to elections, also encourage economic activity sorting in unaffected counties, our main effect would be biased downward by including such physically close areas in our control group.

With this trade-off in mind, Appendix A.3.1 reports our main outcome results using alternative comparison groups (namely counties directly surrounding affected areas, counties exposed to future tropical storms and hurricanes, unexposed counties, and the rest of the continental United States). Importantly, we show that these estimates are not significantly different from one another. However, on average, control groups subject to spatial spillovers (i.e., close to the treated areas) yield smaller ATTs, and control groups subject to different pre-trends (i.e., distant from treated areas but not exposed to storms) yield larger ATTs, as expected from our discussion. For our policy simulations in Section 7, we are interested in how hurricanes affect the relative attractiveness of affected locations across the electoral cycle with respect to the rest of the economy. Therefore, our baseline estimates contain the rest of the United States in our main comparison group, including not-yet-treated counties.

Last, note that some areas in our sample experienced more than one hurricane during this period, while others experienced one hurricane before or after. We only use the first hurricane occurrence in any given region for the estimation (i.e., the treatment is "absorbing")' i.e., we filter out hurricanes that occurred before 2001 or after 2019 and any hurricane between 2001 and 2019 in a location that had previously experienced such a disaster. In doing so, we focus on the impact of ever receiving the treatment during our sample period (Sun and Abraham, 2021).

We use a simple event study design to estimate the dynamic effects of both on- and off-cycle hurricanes. Our main specification reads as

$$Y_{it} = \alpha_i + \gamma_t + \sum_{l} \mu_l . \mathbb{1}\{t - E_i = l\} + \epsilon_{it}, \tag{1}$$

[9] At the national level, for example, both hurricanes Katrina and Sandy were sufficiently large to trigger amendments to federal disaster emergency and preparation management, namely the Post-Katrina Emergency Management Reform Act of 2006 and the Sandy Recovery Improvement Act of 2013.

where Y_{it} is the (log of the) outcome of interest in county i at year t, $\mathbb{1}$ is an indicator for having ever been hit by an on- or off-cycle hurricane, and E_i is the year of the first on- or off-cycle hurricane experienced in county i in the sample period. County fixed effects (α_i) and year fixed effects (γ_t) are also included in the regression, and standard errors are clustered at the county level. Following Callaway and Sant'Anna (2021), in pre-treatment years the base year is the immediately preceding year. Later, we show that our results remain quantitatively and statistically similar when controlling for the hurricane's intensity upon landfall, using larger sample periods and alternative estimators.

4.2 Main Results

Following the empirical literature on post-disaster efforts (e.g., Besley and Case, 1995; Besley and Burgess, 2002; Eisensee and Strömberg, 2007; Healy and Malhotra, 2009) and on economic activity sorting (e.g., Boustan et al., 2012; Hornbeck, 2012; Bernstein et al., 2021), we are primarily interested in measuring how local public spending and the population react to on- and off-cycle hurricanes.

Local government's post-disaster efforts. Measuring governmental output is complex as it is essentially nonmarketable. To assign a dollar value to the provision of local public goods and services, we thus rely on the BEA's estimates of government output, aggregating governments' spending to produce and provide public assets and services at the county level.[10] Note that the BEA definition of government output is conservative as it assumes that the net return for general government fixed assets is null.

The upper panel of Figure 2 presents the dynamic effects of hurricanes on such a county-level measure of government output. In line with the literature on public responses to natural disasters (Besley and Burgess, 2002; Eisensee and Strömberg, 2007; Healy and Malhotra, 2009), electoral cycles generate strikingly different responses. Indeed, the provision of local public goods and services increases sharply in the aftermath of an on-cycle hurricane compared to the rest of the U.S. and peaks after four years at about +7.7%. After the fourth year, public goods provision steadily returns to pre-treatment levels. In contrast, local public goods provision remains statistically unchanged when counties are affected by an off-cycle hurricane. Overall, the corresponding average treatment effect of on-cycle hurricanes on the treated (ATT) is +4.1% ($p < 0.01$), or \$155 per capita (in 2001 dollar

[10]Since 2001, the BEA measures the purchases made by the federal, state, and local governments on inputs of labor, intermediate goods and services, and investment expenditures. Upper governmental consumption expenditures and gross investments are distributed to counties using various data sources on employment, GDP, net electricity generation data, and wages and salaries. See the BEA—Chapter 9: Government Consumption Expenditures and Gross Investment for a detailed description of the methodology.

values–see Table A.3). In contrast, the ATT for counties hit by off-cycle hurricanes is not significantly different from zero.

Population sorting. The bottom panel of Figure 2 presents the dynamic population response to a hurricane, depending on whether the disaster occurred close (i.e., on-cycle) or far (i.e., off-cycle) from Election Day. Our results document a sharp, significant, positive, and persistent population response when hurricanes occur close to Election Day. In particular, the population grows by 4.6% seven years after an on-cycle hurricane and 10.4% after 13 years compared to the rest of the country. The ATT of on-cycle hurricanes on the population is about 4.7% ($p < 0.01$), which corresponds to around $6,568$ individuals for the average on-cycle county (see Table A.3). This effect is almost immediate: like local public goods provision, the population grows in the second year after the on-cycle natural disaster. In contrast, counties hit by off-cycle hurricanes do not display any population response.[11]

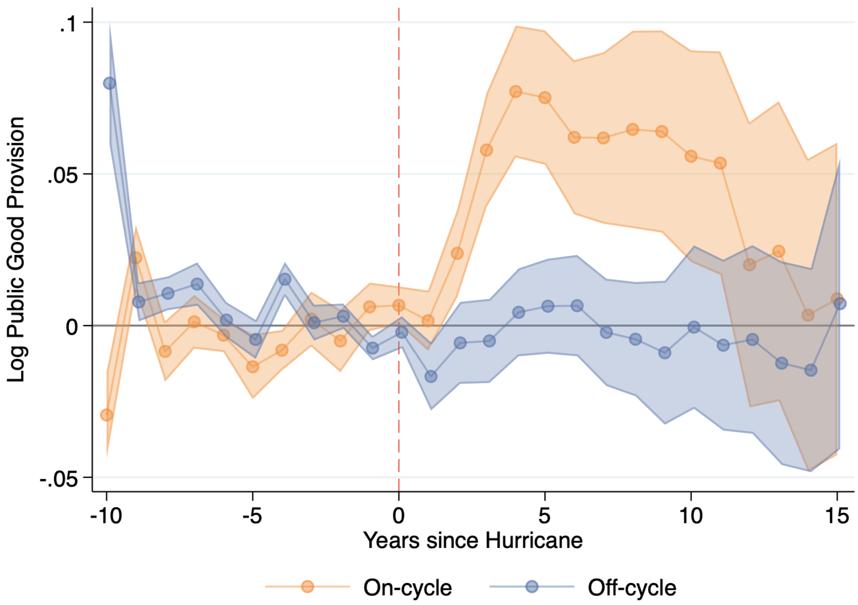
4.3 Internal and External Validity

As mentioned in Section 4.1, Appendix A.3.1 informs our main outcome results using alternative comparison groups. Additionally, Appendix Figure A.4 shows that both the public good provision and the population responses remain qualitatively similar when applying the estimators of Abadie (2005) or Sun and Abraham (2021), holding the comparison group (never-treated) and comparison year (the year before the shock) constant. Similarly, Appendix Figure A.5 shows that the public good provision and the population responses remain unchanged when controlling for wind velocity upon landfall.

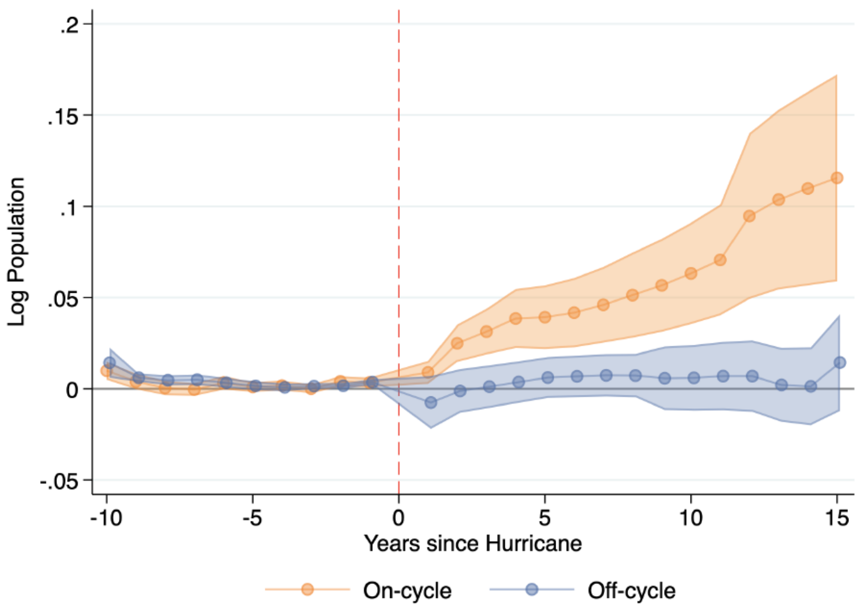
We further explore the external validity of our results. First, we extend our period of analysis until 1969 and find that the population has increased dramatically in counties hit by on-cycle hurricanes ever since (Figure A.6) at least the Stafford Act of 1988 (Figure A.9), indicating that our main result is not the product of recent extreme weather events. Second, we use extreme wildfires as treatments to ensure that our main findings are not due to a specific type of disaster (here, hurricanes).[12] Figure A.7 shows that the population significantly increases in counties hit by on-cycle wildfires as opposed to

[11] To better understand the spatial sorting pattern, we split the population stock into flows of those who stay (stayers), migrants moving in (inflow), and migrants moving out (outflow) in Appendix A.2. To do so, we use IRS county-to-county migration data, which tracks the number of exemptions filed by taxpayers at the county level.

[12] To do so, we overlap wildfires' spatial extents between 1988 (i.e., the year of the Stafford Act) and 2019 from the Interagency Fire Perimeter History dataset with land use information from 1987 (Sohl et al., 2016) and select wildfires having burnt at least 100 hectares of 1987 urban lands. Note, however, that the occurrence and extent of a wildfire are less exogenous. For instance, wildfires are often caused by criminal activity, and their extent may vary with the quality of local public services. As a result, only a handful of wildfires could be qualified as catastrophic events during our sample period (see Section 2).



(a) On- vs. Off-Cycle Hurricane Effect on Local Public Goods Provision



(b) On- vs. Off-Cycle Hurricane Effect on Population

Figure 2: Main Results

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of public good and service provision (i.e., government output; see the BEA's definition) (Panel (a)) and the log of the population (Panel (b)), aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

off-cycle wildfires. This pattern supports the idea that our main result is general and can be extended to other types of catastrophic events and is not limited to hurricanes.

Nevertheless, it is unclear whether economic activity will systematically sort in exposed areas after a disaster. As mentioned in Section 1, sorting responses hinge on both the exogenous disaster damage and the endogenous private and public responses to the initial shock, irrespective of the electoral cycle. Depending on the strength of these responses relative to the initial shock, natural disasters may or may not trigger population sorting in exposed areas. To that extent, one should not expect similar results in periods or areas where post-disaster intervention is not as electorally important or may be limited. Hazards can prompt households to move away from danger, particularly in situations where there is insufficient support after a disaster (e.g., Boustan et al., 2012; Hornbeck, 2012). However, these effects might differ in modern contexts (e.g., Kocornik-Mina et al., 2020; Tellman et al., 2021; Balboni, 2019; Fried, 2021; Magontier and Martinez Mazza, 2023) where government intervention is more likely.

4.4 Mechanisms

Having established that population and local public goods provision increase significantly after an on-cycle hurricane, we want to document the channels linking these two results.

Electoral motives. One possibility is that these responses are affected by a simultaneous apolitical mechanism (Noy and Nualsri, 2011). However, in our design, the electoral timing (i.e., every other year) tends to ensure it is not the case. An extensive literature on political business cycles (e.g., Rogoff, 1990; Rogoff and Sibert, 1988; Alesina et al., 1993; Persson and Tabellini, 2012) points out that the impact of regular electoral cycles on real variables such as GDP or unemployment are unlikely. In our case, such business cycles, which could distort hurricanes' impact on economic activity sorting (e.g., Hallegatte and Ghil, 2008), are orthogonal to federal elections.[13] Instead, the literature has shown that politicians may strategically use short-term budgetary instruments such as fiscal transfers for signaling purposes before an election (e.g., Alesina, 1988; Foremny and Riedel, 2014). To ensure that population sorting is indeed affected by electoral motives and not other concurrent non-electoral mechanisms, we explore the treatment effects along alternative political dimensions. We categorize the counties into politically aligned counties with the federal government in federal elections versus unaligned counties (Figure A.8, upper left), counties that voted for democrats versus republicans in federal elections

[13] An exploration of the NBER Business Cycle Dating time series shows that economic expansions and contractions do not correlate with the U.S. federal electoral cycle.

(Figure A.8, upper right), and counties belonging to a swing state versus a non-swing state (Figure A.8, bottom). The results show that after hurricanes, politically aligned counties, republican counties, and counties in swing states are likely to experience population sorting. Respectively, politically unaligned counties, democrat counties, and counties in non-swing states do not experience the population sorting phenomenon described earlier. Note that these political dimensions cannot be treated as exogenous as the timing of hurricanes relative to Election Day. However, they support our hypothesis that electoral motives shape post-disaster reactions and the subsequent sorting patterns.

Additionally, Figure A.9 shows that this population effect is driven mainly by events after the 1988 Stafford Act (see Section 2), which gave the president increased discretionary power over post-disaster policies, reinforcing the idea that federal electoral motives play a significant role. Finally, we differentiate the hurricanes into major and minor hurricanes by exploiting the full wind field distribution. The effect is particularly salient when we focus on the major hurricanes (i.e., category 3–5 hurricanes with wind velocity $\geq$ 50m/s, measured by the CLIMADA wind field model). Panel (a) of Figure A.10 shows that significantly more public goods and services are provided in counties hit by on-cycle hurricanes with higher wind speeds. Similarly, Panel (b) of Figure A.10 shows that only major winds are likely to drive the sorting response, echoing that the DRF voted by federal authorities primarily reacts to more catastrophic events.

Fiscal revenues redistribution. To examine how counties affected by on-cycle hurricanes finance this significant increase in public goods provision, we collect information on local budgets from the Census of Governments aggregated at the county level. Such increased spending is unlikely to be financed by local authorities. Indeed, Appendix Figures A.11 and A.12 show that the impacts of on-cycle hurricanes on local own-collected revenues and local debt are statistically insignificant despite a short-lived positive response in own-collected revenues after an on-cycle hurricane. However, we observe an economically and statistically significant rise in intergovernmental transfers to the counties affected by on-cycle hurricanes.

Appendix Figure A.13 shows that this reaction is ignited immediately after the shock and peaks at year three at almost +12.7% compared to counties not yet affected by an on-cycle hurricane. This strong response quickly dissipates after the fifth year. Counties affected by off-cycle hurricanes experience increased intergovernmental transfers but with much smaller magnitudes than the effects of on-cycle hurricanes. This result, however, suggests that states and federal governments support local administrations' budgets after an off-cycle event but do not overcompensate for the disaster shock.

Overall, the ATT of the on-cycle hurricanes is +4.3% ($p < 0.01$), or $45 per capita. In contrast, the ATT of the off-cycle hurricanes remains not statistically significant.

To see if other accounting components are affected by the timing of hurricanes, we build on Piketty et al. (2017) to generate county-level distributional accounts between 2001 and 2019. Appendix Section B.2 details the methods and data used to compute aggregate spending and revenues at the local level. This procedure of computing the total collected taxes before and total spending after redistribution for each county in the United States allows us to calculate net transfer rates as the ratio of net spending (i.e., total local expenditures net of total revenues collected locally) to local GDP. In this system, the spending rate in the average county, a net recipient, is 16.59% higher than the local tax rate. Appendix Figure B.7 illustrates the dynamic impacts of hurricanes on net transfers. On average, on-cycle hurricanes cause an increase in net transfers by 3.12 percentage points (or $54 per capita), representing 18.8% of the net transfers in the average county. In contrast, fiscal transfers do not significantly respond to off-cycle hurricanes.

Local amenities and labor demand shocks. Essential post-disaster policy programs, e.g., the FEMA Disaster Relief or Mitigation Programs, aim to help cover the costs of restoring basic infrastructures (Sylves, 2019). Amid the disaster's course, further damages may occur due to access difficulties, lack of drinkable water, or energy shortages. The construction and reparation of roads, bridges, and highways, as well as reforming essential utility local distribution (i.e., water, gas, electricity), is therefore a priority to revive affected neighborhoods, all the more given the piling costs from delays (Olsen and Porter, 2011; Pradhan and Arneson, 2021).

The surge in basic infrastructure restoration demand may affect population sorting in two ways. First, these infrastructures are repaired and upgraded to sustain future damage. For instance, public administrations might elevate roads and bury electrical lines, and the impact of on-cycle hurricanes on immediate post-disaster FEMA grants to local administrations supports this claim. Appendix A.5.3 shows that federal assistance can be twice as large when a hurricane is on cycle than when it is off cycle. In line with these results, Appendix Figure A.15 provides further evidence supporting the existence of an electoral cycle in the activity of the transportation and utility infrastructures construction industries. The American Society of Civil Engineers gave an average D/D+ grade to U.S. civil infrastructure systems between 1998 and 2013 (Grigg, 2015). Post-disaster support thus allows local infrastructure restoration beyond pre-disaster quality, which might be outdated (Hornbeck and Keniston, 2017). This support translates into durable improvements in neighborhoods' quality of life, higher relative attractiveness, and local multiplier effects.

At the same time, the higher post-disaster transfers after on-cycle disasters may not only lead to a redevelopment of the local infrastructure, allowing local productivity and economic activity to recover. Consistent with the Dutch disease hypothesis, they also shift labor from tradable to non-tradable sectors (see, e.g., Allcott and Keniston, 2017; Bulte et al., 2018; Corden and Neary, 1982; Rajan and Subramanian, 2011). Indeed, we observe a boom in the construction sector followed by a contraction of the local economy four to five years after the on-cycle post-disaster efforts and a large industrial recomposition toward service industries. Appendix Figure A.16 documents the existence of the electoral cycle along these dimensions. In contrast, we do not see such changes after off-cycle events.

Discussion. Overall, the redevelopment and investments in basic infrastructure, motivated by electoral motives, generate durable improvements in the local quality of life and distort the local economy. Both channels affect the relative attractiveness of locations and the spatial distribution of economic activity across space. However, quantifying the relative importance of each channel in the data is a complex task. For example, isolating higher-quality amenities from increased public spending would be challenging given the ensuing endogenous population sorting and other general equilibrium effects. The next section presents a dynamic spatial model with post-disaster policies whose structure allows us to distill and quantify these channels' exogenous parts. We discuss the relative importance of the electoral cycle for local amenities and productivity in Section 6.4.

5 A Dynamic Spatial Model with Electorally-Motivated Policies

This section builds on our previous empirical findings that electoral-cycle-driven post-disaster policies can distort location decisions. We aim to provide the most straightforward setup to analyze the role of policies motivated by electoral prospects in the spatial distribution of economic activity. In doing so, we embed electoral-cycle-driven public policies in a dynamic spatial equilibrium model.

Post-disaster policies affect the spatial distribution of economic activity in a way that requires a dynamic framework that entails economic growth effects. Hence in building our dynamic spatial model, we follow Desmet et al. (2018) and Desmet et al. (2021) but make the following necessary amendments. Governments in every region provide local public services (among other things, seawalls, water reservoirs, elevated roads, safe schools, and general safety). A fiscal transfer scheme reallocates resources across jurisdictions, as in Henkel et al. (2021). Local governments can adjust their post-disaster efforts (i.e., the transformation rate of public spending into durable public goods and amenities valued by workers) given the size of electoral-cycle-driven post-disaster transfers.

Moreover, we allow for the possibility of post-disaster efforts distorting the local economy, which could reduce long-term overall growth and welfare. We consider an economy consisting of $r \in \mathcal{S}$ regions with land density $H(r)$ for each region r. There is a mass $\bar{L}$ of homogeneous workers who are (imperfectly) mobile across regions. The initial population size in location r is given by $H(r)\bar{L}_0(r)$.

Preferences. An infinitely lived representative household i who resides in location r in period t has lived in locations $\bar{r}_- = (r_0, ..., r_{t-1})$ in the previous periods. The household derives utility from consumption of a private good $c_t(r)$ and public services $g_t(r)$ according to the following Cobb-Douglas preferences, where $0 < \gamma < 1$:

$$u_t^i(\bar{r}_-, r) = \left[\left(\frac{g_t(r)}{H(r)\bar{L}_t(r)^\eta} \right)^\gamma \cdot c_t(r)^{1-\gamma} \right] a_t(r) \epsilon_t^i(r) \prod_{s=1}^{t} m(r_s, r_{s-1})^{-1}. \tag{2}$$

The parameter $\eta \in [0; 1]$ governs the degree of rivalry in public services in location r, with $\eta = 0$ capturing the case of a pure local public good and $\eta = 1$ of fully rival per capita transfers per unit of land. Agents discount the future at rate $\beta < 1$, and so the utility of household i in the first period is given by $\sum_t \beta^t u_t^i(\bar{r}_{t-}^i, r_t^i)$, where r_t^i denotes the location decision at t, $\bar{r}_{t-}^i$ denotes the history of locations before t, and r_0^i is given.

The local amenity term $a_t(r) = \bar{a}_t(r)\bar{L}_t(r)^{-\lambda}$ contains two parts. The first part is a fundamental amenity term ($\bar{a}_t(r)$) that includes public infrastructure build-up in the past and environmental amenities like warm weather, clean air, and water. Moreover, it entails the rate at which the government transforms public spending into utility valued by households (similar to Fajgelbaum et al., 2019). The second part is endogenous and reacts negatively to population per unit of land at r in period t ($\bar{L}_t(r)^{-\lambda}$), where $\lambda \geq 0$ governs the strength of that endogenous dispersion force. Local disasters and the electoral cycle affect each location's fundamental amenity level through the function $\Lambda_t^a(r)$.

$$\bar{a}_t(r) = (1 + \Lambda_t^a(r))\bar{a}_{t-1}(r). \tag{3}$$

The size of $\Lambda_t^a(r)$ depends on the local disaster probability and the electoral cycle. It defines the percentage change of $\bar{a}_t(r)$ in response to a disaster and the accompanying post-disaster efforts across the electoral cycle. If a natural disaster hits a region r at period t ($\mathbb{1}_t(r) = 1$), the rate at which the local government transforms public funds into amenities valued by workers is changed by $\Lambda_t^a(r)$.

When there is no disaster ($\mathbb{1}_t(r) = 0$), the level of fundamental amenity remains unchanged. Then, it is given by $\bar{a}_t(r) = \bar{a}_{t-1}(r)$.

Post-disaster efforts vary across the electoral cycle and could affect the fundamental amenities differently. φ_a represents the additional effect on the transformation rate during an on-cycle year, $\mathcal{I}_t = 1$, compared to an off-cycle year $\bar{\lambda}_a$.

$$\Lambda_t^a(r) = (\bar{\lambda}_a + \varphi_a \cdot \mathcal{I}_t) \cdot \mathbb{1}_t(r). \tag{4}$$

Our specification also accounts for idiosyncratic location preferences $\epsilon_t^i(r)$ and moving costs $m(r_s, r_{s-1})$ from having resided in different places in the past. Idiosyncratic taste shocks are independent and identically distributed across households, locations, and time according to a Fréchet distribution with shape parameter $1/\Omega$ and scale parameter 1. A greater value of Ω implies more variety in agents' tastes across locations, acting as an additional dispersion force. We follow Desmet et al. (2018) in simplifying the dynamic mobility decisions of the representative households to a sequence of static decisions.[14]

A household derives income from net-of-tax ($t_t(r)$) labor income $(1 - t_t(r))w_t(r)$ and from the local ownership of land $R_t(r)/\bar{L}_t(r)$. Economic agents cannot write debt contracts with each other.

The fraction of households living at r at time t is given by[15]

$$\frac{H(r)\bar{L}_t(r)}{\int_S H(u)\bar{L}_t(u)du} = \frac{u_t(r)^{1/\Omega} m_2(r)^{-1/\Omega}}{\int_S u_t(u)^{1/\Omega} m_2(u)^{-1/\Omega} du}. \tag{5}$$

Production technologies. The specification of the production technology closely follows Desmet et al. (2018), who develop and discuss all its dynamic features. In what follows, we rely on their formal derivations and depiction of the optimization problems. We add, however, the term $\Lambda_t^{\tau}(r)$, which incorporates the net effect of natural disaster and post-disaster efforts on local productivity. In every region, r, a continuum of firms produces a unique variety ω of a differentiated intermediate good under perfect competition using a constant-returns-to-scale technology in land and labor. Output per

[14] We assume that the mobility cost of moving from r to s is a function of an origin-specific cost term, $m_1(r)$, and a destination-specific cost term, $m_2(s)$. This means that $m(r, s) = m_1(r)m_2(s)$ with, $m(r, r) = 1$ for all $r, s \in S$ such that $m_2(r) = m_1(r)^{-1}$. The intuition is that the representative household only pays the permanent utility flow cost of moving to a specific location while residing there. Once it moves away, the household gets compensated by a permanent utility flow benefit, which is inversely proportional to the initial cost of moving there. In other words, migration decisions are reversible, and therefore the location choice of households only depends on current variables and not on past or future ones.

[15] See Desmet et al. (2018) for a complete derivation.

unit of land of variety, ω, is given by

$$q_{\omega,t}(r) = \phi_{\omega,t}(r)^{\gamma_1} z_{\omega,t}(r) L_{\omega,t}(r)^{\mu}, \tag{6}$$

where $L_{\omega,t}(r)$ denotes the amount of labor per unit of land. Since land is a fixed factor with share $1-\mu$, agglomerating labor in a location yields decreasing returns, which acts as a congestion force.

Each firm's productivity is determined by its innovation decision, $\phi_{\omega,t}(r) \geq 1$, and an idiosyncratic location-variety-specific productivity shifter, $z_{\omega,t}(r)$. Firms can invest in innovation by employing $\nu\phi_{\omega,t}(r)^{\xi}$ additional units of labor per unit of land, where $\xi > \gamma_1/[1-\mu]$. The location-variety-specific innovation decision creates a local advantage to scale. As captured by γ_1/ξ, it is an agglomeration force whose strength increases along with the returns to innovation.

The exogenous productivity shifter is the realization of a random variable that is independent and identically distributed across varieties and time according to a Fréchet distribution with cumulative distribution function $F(z,r) = e^{-T_t(r)z^{-\theta}}$. The scale parameter $T_t(r)$ governs the level of productivity in a location and is affected by agglomeration externalities due to high population density and past endogenous innovations. We let $T_t(r) = \tau_t(r)\bar{L}_t(r)^{\alpha}$, where $\tau_t(r)$ represents a location-specific productivity term and the positive impact of $\bar{L}_t(r)$ on $T_t(r)$ captures additional agglomeration economies such as knowledge spillovers. The shape parameter $\theta > 0$ governs the dispersion in productivity draws across locations. A broader dispersion in draws across locations increases the effect of population density on average productivity such that the strength of this agglomeration force is increasing in α/θ.

In turn, fundamental productivity, $\tau_t(r)$, is determined by the net impact of local disasters and post-disaster efforts as well as an endogenous dynamic process given by

$$\tau_t(r) = (1 + \Lambda_t^{\tau}(r)) \left(\phi_{t-1}(r)^{\theta\gamma_1} \left[\int_{\mathcal{S}} \eta(r,s)\tau_{t-1}(s)ds \right]^{1-\gamma_2} \tau_{t-1}(r)^{\gamma_2} \right). \tag{7}$$

Local disasters and post-disaster efforts directly affect each location's fundamental productivity term through the impact function $\Lambda_t^{\tau}(r)$. Value $\mathbb{1}_t(r) = 1$ indicates if a natural disaster hits a region r at period t, and $\mathbb{1}_t(r) = 0$ indicates a storm does not hit the region. The term $\phi_{t-1}(r)^{\theta\gamma_1}$ represents the shift in the local distribution of shocks that results from the last periods' innovation decisions of firms, which are assumed to now constitute the local technology. The individual contemporaneous effect of innovation directly affects the production function in (6). The term $\left[\int_{\mathcal{S}} \eta(r,s)\tau_{t-1}(s)ds\right]^{1-\gamma_2} \tau_{t-1}(r)^{\gamma_2}$ denotes the level of past productivity that firms build on, with $\int_{\mathcal{S}} \eta(r,s) = 1$. It also comprises the locations' productivity level $\tau_{t-1}(r)$ and technology diffusion from other locations. The function $\eta(r,s)$ denotes the spatial decay in the strength of technology diffusion. Note that $\eta(r,s)$ also governs the spatial distribution of productivity shocks in response to natural disasters. That is, the dynamic evolution of a location's technology level is not only shifted up by past innovations but also affected by the impact of natural disasters in all other locations if $\gamma_2 < 1$.

Land markets are competitive. A continuum of potential entrants competes in prices (à la Bertrand); i.e., all firms bid for land to enter the market. Since there is a continuum of potential entrants, all firms bid until the winning firm makes zero profits net of the fixed innovation costs. Thus, in this economy, the solution to the dynamic innovation problem is to choose the level of innovation that maximizes profits (or, equivalently, land bids) over time. Future firms' profits are zero because all future gains of today's innovations will accrue to the fixed factor land reflected by the local land price, $R_t(r)$. The firms' innovation decisions remain unaffected by the effect of future productivity gains of current innovations via (7). Again, this implies that the firm's optimization problem reduces simply to a static problem. In sum, individual firms in location r take input costs as given and maximize current profits per unit of land by choosing the amount of labor per unit of land for production and innovation:

$$\max_{L_{\omega,t}(r),\phi_{\omega,t}(r)} p_{\omega,t}(r,r)\phi_{\omega,t}(r)^{\gamma_1} z_{\omega,t}(r)L_{\omega,t}(r)^{\mu} - w_t(r)L_{\omega,t}(r) - w_t(r)\nu\phi_{\omega,t}(r)^{\xi} - R_t(r), \tag{8}$$

where $p_{\omega,t}(r,r)$ is the price charged by the firm of a good sold at r.

Prices and export shares. Intermediate goods markets are competitive, so firms sell goods at marginal cost after accounting for transport costs. Let $\zeta(s,r) \geq 1$ denote the iceberg trade cost of transporting a good from r to s. Then, the price of an intermediate good ω produced at r and sold at s is given by

$$p_{\omega,t}(s,r) = \frac{\zeta(s,r)mc_t(r)}{z_{\omega,t}(r)}, \tag{9}$$

where $mc_t(r) \equiv [1/\mu]^{\mu}[\nu\xi/\gamma_1]^{1-\mu}[\gamma_1 R_t(r)/w_t(r)\nu(\xi(1-\mu)-\gamma_1)]^{(1-\mu)-(\gamma_1/\xi)}w_t(r)$ denotes the marginal input cost at location r.

The probability density that an intermediate good produced in r is bought in s is given by

$$\pi_t(s,r) = \frac{T_t(r)\left[mc_t(r)\zeta(r,s)\right]^{-\theta}}{\int_S T_t(u)\left[mc_t(u)\zeta(u,s)\right]^{-\theta} du} \quad \text{for all} \quad r,s \in S. \tag{10}$$

A final good $Q_t(r)$ is assembled from the continuum of intermediates according to the following constant elasticity of substitution aggregator:

$$Q_t(r) = \left[\int_S q_t(r,s)^\rho dr \right]^{\frac{1}{\rho}}. \tag{11}$$

Here, $q_t(r,s)$ denotes the quantity of the variety produced in location r and used for assembly in location s, and $1/[1-\rho]$ represents the elasticity of substitution between intermediates with $0 < \rho < 1$. Final goods are not traded across regions, and assembly has no extra costs. This final good $Q_t(r)$ can either be used directly for private consumption $c_t(r)$ or by local governments to provide public services $g_t(r)$. Thus, we have $Q_t(r) = H(r)\bar{L}_t(r)c_t(r) + g_t(r)$.

As a result, the final good's price in place s at time t is determined by the average price of the various goods assembled in location r:

$$P_t(s) = \bar{p}\chi_t(s)^{-\frac{1}{\theta}}, \tag{12}$$

with $\chi_t(s) = \int_S T_t(u) \left[mc_t(u)\zeta(u,s)\right]^{-\theta} du$ and $\bar{p} = \left[\Gamma\left(\frac{-\rho}{(1-\rho)\theta} + 1\right)\right]^{-\frac{1-\rho}{\rho}}$.

Government. In describing the public sector in this economy, we closely follow Henkel et al. (2021). Labor income is taxed at rate $t_t(r)$, which generates an overall tax revenue equal to $t_t(r)w_t(r)H(r)\bar{L}_t(r)$ in region r at time t. The federal government budget constraint is thus given by $\int_S t_t(s)w_t(s)H(s)\bar{L}_t(s)ds$. In every period t, the federal government redistributes overall tax revenues to local governments at the rate $\theta_t(r)$. The transfer rate relative to local aggregate labor income is positive ($\theta_t(r) > 0$) for recipients and negative ($\theta_t(r) < 0$) for donor regions.

We keep the specification of the public sector as simple as possible. However, it is flexible enough to take the model to the data. A few comments are in order about our setup. We assume the government can commit to a tax policy sequence at time zero and cannot issue bonds to borrow money from the future. Our model abstracts from horizontal tax competition and national public goods. However, note that the amenity term consumed by households implicitly captures any national public goods provided by the federal government. We abstract from progressive tax schedules and dead-weight losses of income taxation. However, although households supply labor inelastically, we will see later that they respond to regional differences in tax and transfer rates through migration. Therefore, local governments face a mobile tax base since households choose their locations endogenously.

Post-disaster policy and the electoral cycle. Motivated by the empirical findings of the previous section, we introduce a relationship between fiscal transfers and natural disasters in our spatial general equilibrium model. Letting $\theta_t(r)$ denote the transfer rate, the response of the federal government to natural disasters in setting local transfer rates reads as follows:

$$\theta_t(r) = \left(1 + \Lambda_t^\theta(r)\right) \theta_{t-1}(r). \tag{13}$$

A natural disaster in region r raises its weight for the upper governmental layers in channeling public funds to this region. The transfer rate of the previous period $\theta_{t-1}(r)$ is adjusted by $\Lambda_t^\theta(r)$ if a natural disaster hits region r at period t, $\mathbb{1}_t(r) = 1$. More importantly, transfer rates depend on the electoral cycle:

$$\Lambda_t^\theta(r) = (\bar{\lambda}_\theta + \varphi_\theta \cdot \mathcal{I}_t) \cdot \mathbb{1}_t(r). \tag{14}$$

When the disaster occurs within an election year, $\mathcal{I}_t = 1$, the transfer rates could react differently than during off-cycle year. $\varphi_\theta > 0$ governs the size of this political cycle effect.

The local governments use the available public funds to provide a sequence of local public services $g_t(r)$. Without any political-cycle-driven transfers, the level of local public goods reads as $g_t(r) = \left[t_t(r) + (1 + \bar{\lambda}_\theta \cdot \mathbb{1}_t(r))\theta_{t-1}(r)\right] w_t(r) H_t(r) \bar{L}_t(r)/P_t(r)$. Given the political cycle effect, however, the effective budget that is available for local public goods provision in region r during an on-cycle year is thus given by

$$g_t(r) = \left[t_t(r) + (1 + (\bar{\lambda}_\theta + \varphi_\theta \cdot \mathcal{I}_t) \cdot \mathbb{1}_t(r))\theta_{t-1}(r)\right] w_t(r) H(r) \bar{L}_t(r)/P_t(r). \tag{15}$$

Competitive equilibrium. The following conditions define a dynamic competitive equilibrium in this economy:

1. **Labor market clearing.**

$$\int_S H(r)\bar{L}_t(r)dr = \bar{L}. \tag{16}$$

2. **Land market clearing.** Land is assigned to the highest bidder such that for all $r \in S$:

$$R_t(r) = \left[\frac{\xi - \mu\xi - \gamma_1}{\mu\xi + \gamma_1}\right] w_t(r)\bar{L}_t(r). \tag{17}$$

3. **Goods market clearing.** Total labor income in region r, $w_t(r)H(r)\bar{L}_t(r)$, must equal region

r's total sales to all locations $s \in S$:

$$w_t(r)H(r)\bar{L}_t(r) = \int_S X_t(s,r)ds \quad \text{for all} \quad r \in S, \tag{18}$$

where $X_t(s,r) = \pi_t(s,r)[(1+\theta_t(s))w_t(s)H(s)\bar{L}_t(s)]ds$ includes government transfers across regions.[16]

4. **Balanced public budget.** The total amount of transfers paid must equal the total amount received such that

$$\int_S \theta_t(s)w_t(s)H(s)\bar{L}_t(s)ds = 0. \tag{19}$$

Moreover, each local government spends its entire budget on local public goods:

$$[t_t(r) + \theta_t(r)]w_t(r)H(r)\bar{L}_t(r) = P_t(r)g_t(r).$$

5. **Spatial sorting.** Given migration costs and their idiosyncratic preferences, households choose where to live, so (5) holds for all $r \in S$.

6. **Utility.** The utility associated with net real income and amenities in location r is given by

$$\begin{aligned} u_t(r) =& a_t(r)\left(\frac{(t_t(r)+\theta_t(r))w_t(r)\bar{L}_t(r)}{\bar{L}_t(r)^{\eta}P_t(r)}\right)^{\gamma}\left(\frac{(1-t_t(r))w_t(r)+R_t(r)/\bar{L}_t(r)}{P_t(r)}\right)^{1-\gamma} \\ =& \bar{a}_t(r)\bar{L}_t(r)^{-\lambda+\gamma(1-\eta)}\frac{w_t(r)}{P_t(r)}\Theta_t(r) \quad \text{for all} \quad r \in S, \end{aligned} \tag{20}$$

where $\Theta_t(r) \equiv \left[(t_t(r)+\theta_t(r))^{\gamma}\left(\frac{\xi}{\mu\xi+\gamma_1} - t_t(r)\right)^{1-\gamma}\right]$, the price index is given by (12), and land markets are in equilibrium.

7. **Dynamic evolution of technology, amenity levels, and transfer rates.** Technology evolves according to (7) and amenity according to (3), and transfer rates evolve according to (13) for all $r \in S$.

Substituting utility (20), (12), and bilateral exports probabilities (10) into the goods market clear-

[16] Note that net government transfers imply trade imbalances in equilibrium. Total imports must equal local labor income plus total net transfers, so $(1+\theta_t(r))w_t(r)H(r)\bar{L}_t(r) = \int_S X_t(r,s)ds$. Comparing this expression with (18), we observe that the difference between exports and imports is given by $-(\theta_t(r))w_t(r)H(r)\bar{L}_t(r)$, while $\int_S \theta_t(s)w_t(r)H(r)\bar{L}_t(r)ds = 0$.

ing condition (18), we obtain

$$w_t(r)^{1+\theta} H(r) \bar{L}_t(r)^{1-\alpha+(1-\mu-\frac{\gamma_1}{\xi})\theta} = \kappa_1 \int_S \zeta(r,s)^{-\theta} \tau_t(r) \left[\frac{\bar{a}_t(s)}{u_t(s)}\right]^{\theta} \Theta_t(s)^{\theta}(1+\theta_t(s)) \\ w_t(s)^{1+\theta} H(s) \bar{L}_t(s)^{1-[\lambda-\gamma(1-\eta)]\theta} ds, \tag{21}$$

where

$$\kappa_1 = \left[\frac{\mu\xi+\gamma_1}{\xi}\right]^{-\left[\mu-\gamma+\frac{\gamma_1}{\xi}\right]\theta} \mu^{\mu\theta} \left[\frac{\xi\nu}{\gamma_1}\right]^{-\frac{\gamma_1\theta}{\xi}} \bar{p}^{-\theta}.$$

Second, combining (20) and (12) allows us to rewrite the price index equation as follows:

$$\bar{L}_t(r)^{[\lambda-\gamma(1-\eta)]\theta} w_t(r)^{-\theta} = \kappa_1 \Theta_t(r)^{\theta} \left[\frac{\bar{a}_t(r)}{u_t(r)}\right]^{\theta} \\ \int_S \tau_t(s) \zeta(s,r)^{-\theta} w_t(s)^{-\theta} \bar{L}_t(s)^{\alpha-(1-\mu-\frac{\gamma_1}{\xi})\theta} ds. \tag{22}$$

Conditional on $\tau_t(\cdot)$, $\bar{a}_t(\cdot)$, $\bar{L}_{t-1}(\cdot)$, $\zeta(\cdot,\cdot)$, $m(\cdot,\cdot)$, $H(\cdot)$, $t_t(\cdot)$, $\theta_t(\cdot)$, $\Lambda_t^{\tau}(\cdot)$, $\Lambda_t^{a}(\cdot)$, $\Lambda_t^{\theta}(\cdot)$, and given parameter values, the system (21) and (22) together with (5) could be solved for the equilibrium wages, population density, and utility for any t and all $r \in S$. $\tau_t(\cdot)$ comes directly from (7), $\bar{L}_{t-1}(\cdot)$, $a_t(\cdot)$ comes from (3), and $\theta_t(\cdot)$ comes from (13). Appendix B.1 presents all proofs and derivations.

Desmet et al. (2018) show that $\lambda + (1-\mu) + \Omega \geq \frac{\alpha}{\theta} + \frac{\gamma_1}{\xi}$ is a sufficient condition to ensure the existence and uniqueness of a stable equilibrium in their model. However, equilibria may also exist if that condition is not satisfied. In our framework with local public goods and fiscal transfers, the respective sufficient condition reads as follows:

Condition 1: $\lambda + (1-\mu) + \Omega \geq \frac{\alpha}{\theta} + \frac{\gamma_1}{\xi} + \gamma(1-\eta)$.

In words, the static congestion forces given by $(\lambda; (1-\mu); \Omega)$ are at least as strong as the sum of the static agglomeration forces $(\alpha/\theta; \gamma_1/\xi)$ and the sharing of public facilities $(\gamma(1-\eta))$. Notice that the net static agglomeration spillover is then negative, $\alpha/\theta + \gamma_1/\xi + \gamma(1-\eta) - \lambda - (1-\mu) - \Omega \leq 0$, so that an inflow of population into region r reduces utility $u_t(\cdot)$, ceteris paribus.

Balanced growth path. The spatial distribution of employment is stable along a balanced growth path (BGP), and the economic growth rate is equal across all areas. Following the steps in Desmet et al. (2018), we can show that a BGP exists if

Condition 2: $\lambda + (1-\mu) + \Omega \geq \frac{\alpha}{\theta} + \frac{\gamma_1}{\xi} + \gamma(1-\eta) + \gamma_1/([1-\gamma_2]\xi)$.

Intertemporal spillovers from previous innovations ensure that the economy does not stagnate in the long run. High-density places that have been innovative in the past are still productive locations nowadays. These areas attract more labor and expand their market, making them ideal locations to innovate today. The influence of past productivity on the rest of the economy in establishing a location's productivity guarantees that this dynamic agglomeration effect does not lead the economy to concentrate in one area over time. The additional term $\gamma_1/([1-\gamma_2]\xi)$ represents this dynamic part of agglomeration economies.

6 Quantification: Post-Disaster Policies in the United States

To bring our model to the data, we consider the post-disaster policies in the United States described in Section 2. As for the empirical analysis, we operate at the county level. Our baseline year is 2001. To quantify the model, we need values for all the economy-wide parameters. Further, we require location-specific values for initial fundamental amenity and productivity levels, migration, and bilateral transport costs. We also need to estimate the impact of natural disasters and post-disaster efforts on amenities, productivity, tax, and transfer rates. We choose baseline parameter values by relating to those in the existing literature and borrow the remaining parameter estimates from Desmet et al. (2018). Table 1 lists the parameters used in our model quantification. Appendix Section B.3.1 provides sensitivity checks around critical parameters of the model.

6.1 Tax and Transfer Rates

To calculate local tax rates $t_t(\cdot)$ and local transfer rates $\theta_t(\cdot)$, we use data from the Government Finance Database (Pierson et al., 2015), from the BEA, the White House Historical Tables, and the Federal Reserve Bank of St. Louis FRED. We break down all tax revenues and expenditures to the county level and normalize them with these locations' GDPs. Appendix Section B provides a more detailed description of the data sources we use and our calculation steps.

Panel (a) of Figure 3 depicts the transfer rates (i.e., the ratio of net spending—total local expenditures net of total revenues collected locally—to local GDP) of each U.S. county in 2001. It is worth mentioning that there is no specific scheme to equalize fiscal capacities across jurisdictions in the United States. Thus, any differences between local expenditures and tax revenues come from tax policies or specific (mandatory, discretionary, and supplemental) spending programs of higher government layers. For example, the federal government collects more taxes from high-income than low-income states via the federal income tax but spends more on social security, Medicare, and other programs in low-income than in high-income places. Hence, transfer rates are more likely to be negative (i.e.,

Table 1: Parameter Values

	Preferences	
$\beta = 0.965$	Discount factor for present values	Desmet et al. (2018)
$\lambda = 0.399$	Amenity congestion w.r.t. local population	Weighted avg. expenditure share on housing from BLS (c.f. isomorphism discussed in Allen and Arkolakis (2014))
$\Omega = 0.5$	Location taste heterogeneity parameter in Frechet distribution	Monte et al. (2018)
$\gamma = 0.417$	Cobb-Douglas preferences weight on public good	Cross-county mean of the counties' tax revenue to GDP ratio
$\eta = 0.405$	Degree of rivalry in public services	Cross-county mean of the counties' transfers to individuals to total government expenditures ratio
	Production	
$\alpha = 0.06$	Elasticity of productivity w.r.t. local population	Desmet et al. (2018)
$\gamma_1 = 0.319$	Elasticity of productivity in $t+1$ w.r.t. innovation in t	Desmet et al. (2018)
$\gamma_2 = 0.99246$	Elasticity of productivity in $t+1$ w.r.t. productivity in t	Desmet et al. (2018)
$\eta(r,s) = 1$	Parameter driving scale of technology diffusion	Desmet et al. (2018)
$\mu = 0.8$	Labor share in production	Desmet et al. (2018)
$\nu = 0.15$	Intercept parameter in innovation cost function	Desmet et al. (2018)
$\xi = 125$	Elasticity of innovation costs w.r.t. innovation	Desmet and Rossi-Hansberg (2015)
$\sigma = 4$	Elasticity of substitution	(c.f. Bernard et al. (2003): 3; Allen and Arkolakis (2014): 9)
$\theta = 6.5$	Trade elasticity	Desmet et al. (2018) (c.f. Eaton and Kortum (2002): 8.3; Simonovska and Waugh (2014): 4.6)
	Impact Function	
$\lambda_\tau = 0.0000$	Semi-elasticity of productivity w.r.t. disaster probability	Estimated
$\lambda_{\bar{a}} = 0.0000$	Semi-elasticity of amenities w.r.t. disaster probability	Estimated
$\lambda_\theta = 0.0000$	Semi-elasticity of transfer rate w.r.t. disaster probability	Estimated
$\lambda_t = 0.0000$	Semi-elasticity of tax rate w.r.t. disaster probability	Estimated
	Impact of Electoral Cycle	
$\varphi_\tau = -0.3773$	Impact of electoral cycle on productivity	Estimated
$\varphi_\theta = 0.0312$	Impact of electoral cycle on transfer rate	Estimated
$\varphi_a = 0.0837$	Impact of electoral cycle on amenities	Estimated
$\varphi_t = 0.0000$	Impact of electoral cycle on tax rates	Estimated

donating counties, closer to the first decile in Figure 3) in high-income counties (e.g., in New England, Southern Florida, the California coast, or south of the Michigan Lake), while transfer rates are more likely to be positive (i.e., receiving counties, closer to the tenth decile in Figure 3) in low-income counties (e.g., in the Southern United States, along the Appalachians, and states like Oregon, Montana, and Nebraska).

6.2 Migration Costs

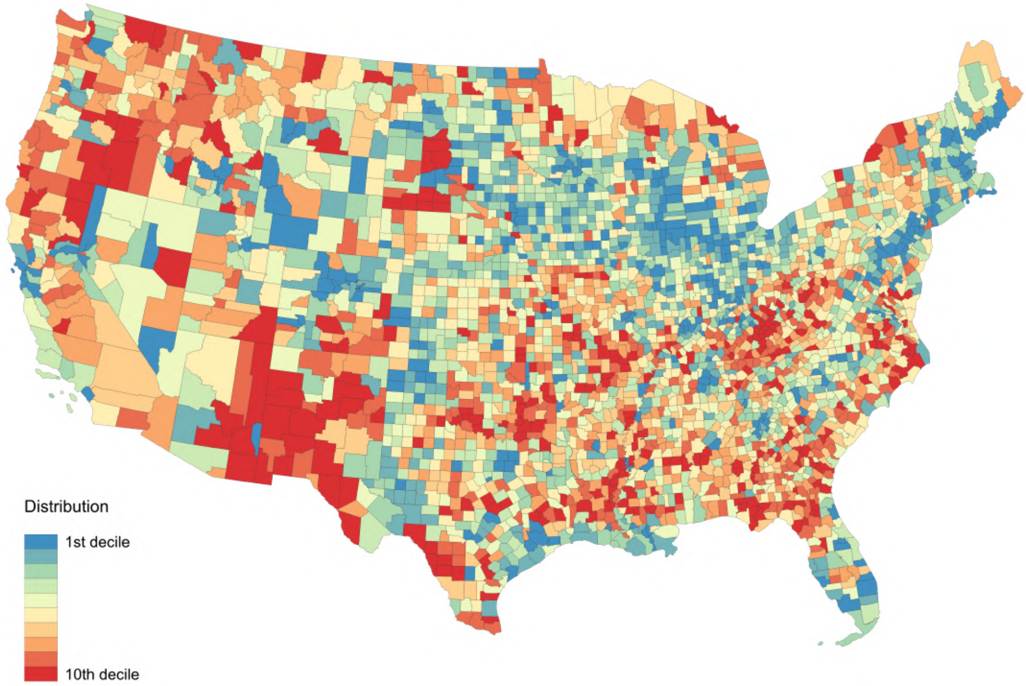
We recover $m_2(\cdot)$ (i.e., the time-invariant, exogenous migration costs) from the time-invariant origin-destination fixed effects on migration flows using the IRS county-to-county migration data for the years 1990–2018. We aim to capture the average first-nature and long-run historical characteristics affecting bilateral movement between counties over this period. We find that the impact of these fixed characteristics on migration inflows correlates highly with their impact on migration outflows, supporting the assumption of symmetric mobility costs (Desmet et al., 2018). Appendix Section B.2 provides a more detailed description of the data analysis.

Panel (b) of Figure 3 plots our recovered migration costs. Populous counties at the coast display the lowest migration costs (i.e., closer to the first decile in Panel (b) of Figure 3), whereas inland, rural counties bear high migration costs. A closer exploration of this geographic distribution indicates that even seemingly isolated counties with high migration costs (e.g., Alpine County, CA, or Wahkiakum County, WA) tend to exhibit harsh topographic features such as mountain ranges. Appendix Section B.2 further shows that, as expected, these figures negatively correlate with net in-migration rates since 1950 as measured by Winkler et al. (2013).

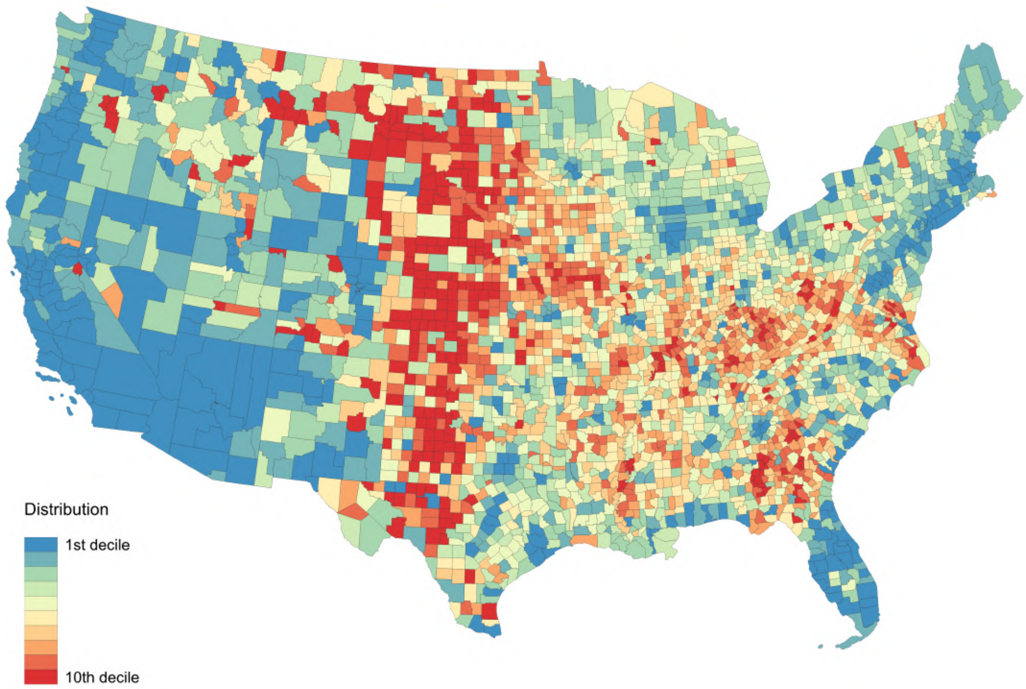
6.3 Amenities, Productivities, and Utility Levels

Next, we recover fundamental productivities, $\tau_t(r)$, amenities, $\bar{a}_t(r)$, and utility levels $u_t(r)$, for every county from our general equilibrium model given our parameter values, and calculations for trade and migration costs. By directly using our $m_2(r)$ estimates from the IRS migration patterns, we can tell $\bar{a}_t(r)$ apart from $u_t(r)$ given the structure of the model (in particular, equation (5)). Specifically, we plug in our calculations of trade costs $\zeta(s, r)$, data on land $H(r)$, tax rates $t_t(r)$, transfer rates $(\theta_t(r))$, and migration costs $m_2(r)$ as well as population $L_t(r)$ and wages $w_t(r)$ into (21), (22), and (5) to solve for $\bar{a}_t(r)$, $\tau_t(r)$, and $u_t(r)$ for each year between 2001 and 2019.

Figure 4 plots the spatial distribution of amenities (Panel (a)) and productivities (Panel (b)) in 2001. Panel (a) shows that the amenity level is higher in Florida, New England, on the West Coast,



(a) Transfer Rates



(b) Estimated Migration Costs

Figure 3: Transfer Rates and Estimated Migration Costs

Notes: This figure plots the transfer rates, θ, for our baseline year 2001 (Panel (a)) and the migration costs, $m_2(r)$, (Panel (b)). The warm colors indicate higher deciles and the blue shadings indicate lower decile counties.

and generally in large urban hubs and is low in some remote areas of the Rocky Mountains or the Great Plains. Panel (b) shows that productivity is higher in California, Washington, New England, and shoreline, urbanized counties in general.[17]

6.4 Impact of Natural Disasters and Election Cycle

We identify the causal effect of natural disasters and the electoral cycle on amenities, productivity, tax rates, and transfer rates using the following empirical specification:

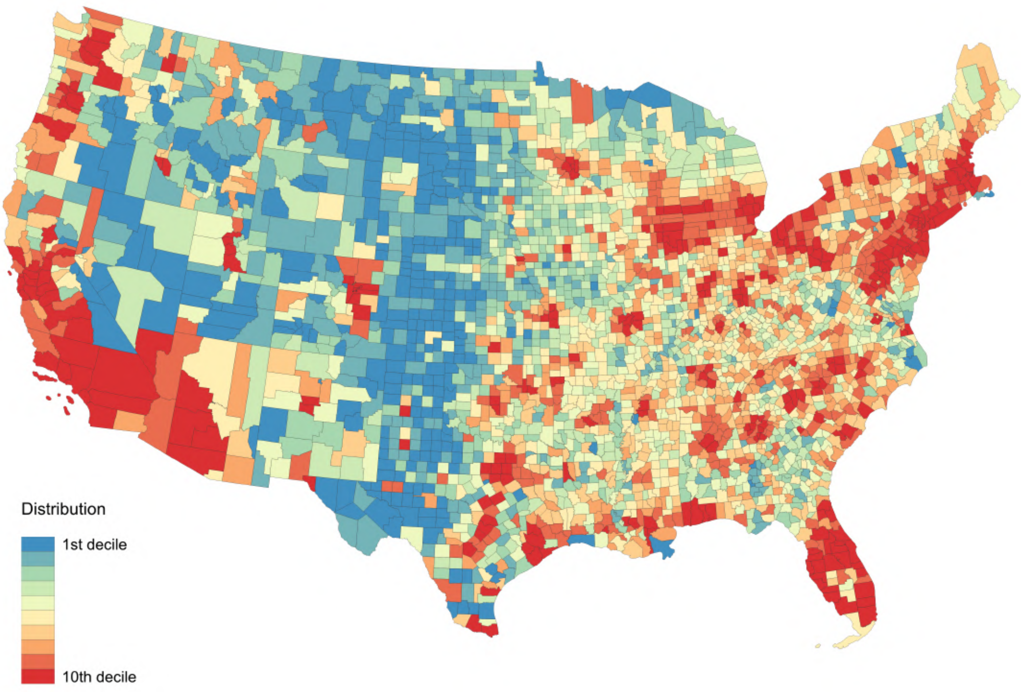
$$y_t(r) = \alpha(r) + \gamma_t + \left(\bar{\lambda}_y + \varphi_y \cdot \mathcal{I}_t\right) \cdot \mathbb{1}_t(r) + \epsilon_t(r), \tag{23}$$

where $y_t(r) \in \{\log \bar{a}_t(r), \log \tau_t(r), \theta_t(r), t_t(r)\}$ are the logarithm of fundamental amenities, the logarithm of fundamental productivities, the level of transfer rates, and the level of tax rates. $\mathbb{1}_t(r)$ is an indicator function for location r having been hit for the first time during our sample period by a natural disaster in period t. When the disaster occurs within an election year, $\mathcal{I}_t = 1$. Finally, $\alpha(r)$ and γ_t are county and year fixed effects, respectively, and standard errors are clustered at the county level. As in equation (1), we use Callaway and Sant'Anna (2021) estimators to account for the staggered nature of our treatment.

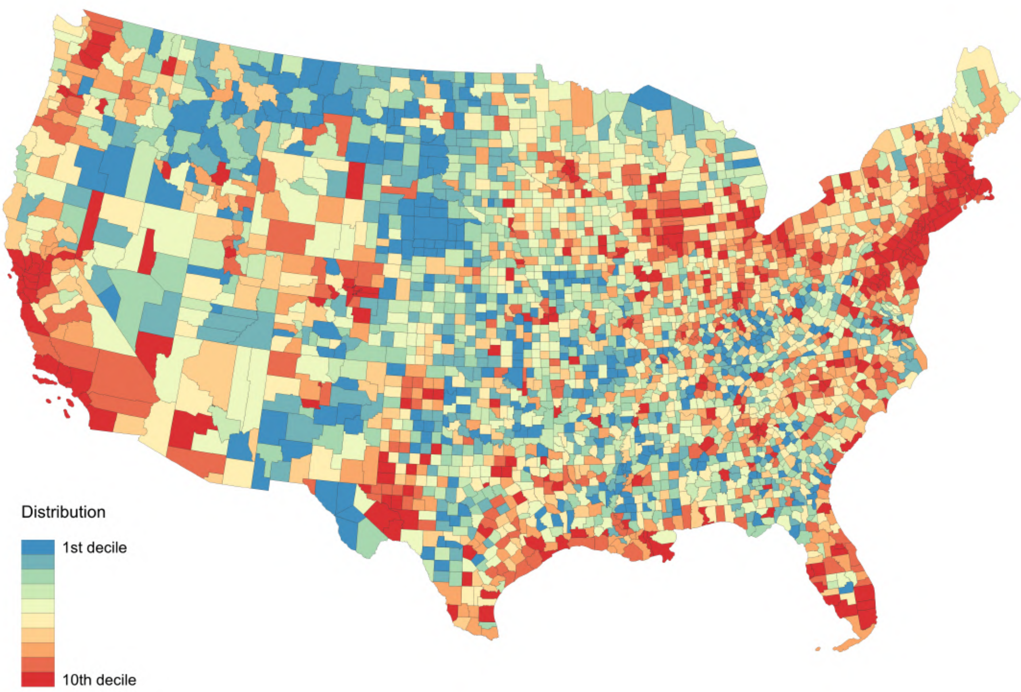
Note that in our model, exogenous amenities, $\bar{a}_t(r)$, and transfer and tax rates, $\{\theta_t(r), t_t(r)\}$, remain constant at exogenous previous periods levels except if they are shocked by a natural disaster. However, fundamental productivity $\tau_t(r)$ is the product of two terms: an exogenous part, $\tau_{t-1}(r)^{\gamma_2}$, and an endogenous part, $\phi_{t-1}(r)^{\theta\gamma_1}\left[\int_{\mathcal{S}} \eta(r,s)\tau_{t-1}(s)ds\right]^{1-\gamma_2}$ (see equation (7)). Because the latter is endogenous to τ_{t-1}, explicitly controlling for $\phi_{t-1}(r)^{\theta\gamma_1}\left[\int_{\mathcal{S}} \eta(r,s)\tau_{t-1}(s)ds\right]^{1-\gamma_2}$ would bias the impact of natural disasters on productivity. We, therefore, control for the lagged exogenous components of the model, $\{\log \bar{a}_{t-1}(r), \theta_{t-1}(r), t_{t-1}(r)\}$, to avoid these issues of over-controlling while identifying the impact of natural disasters on $\tau_t(r)$. In doing so, we isolate the effect of natural disasters on productivity, either directly—through its exogenous component—or indirectly—through its endogenous component. We then quantify the impact functions by $\Lambda_t^y(r) = (\bar{\lambda}_y + \varphi_y \cdot \mathcal{I}_t) \cdot \mathbb{1}_t(r)$.

Our estimates in Appendix Table 1 and Appendix Figure B.7 show that amenities and transfer rates increase by an average of 8.37% and 3.12 percentage points after an on-cycle hurricane. These positive shifts incentivize population sorting into regions hit by on-cycle disasters. This effect is, however, toned down by a substantial productivity decline: on average, −37.73%, which is consistent

[17] Appendix Figure B.6 shows that our recovered utility levels positively correlate with a measure of subjective well-being ($\rho = 0.2819$) as in Desmet et al. (2018, 2021) and with the Local Human Development Index ($\rho = 0.2325$) as in Cruz and Rossi-Hansberg (2021).



(a) Fundamental Amenities



(b) Fundamental Productivities

Figure 4: Estimated Fundamental Amenities and Productivities

Notes: This figure plots the fundamental amenities combined with the rate of transformation (Panel (a)) and productivities (Panel (b)) for our baseline parameter values and baseline year 2001. The warm colors indicate higher deciles and the blue shadings indicate lower decile counties.

with the local GDP per capita losses observed in the data after an on-cycle disaster. Except for tax rates, all ATTs are statistically significant (with $p < 0.01$) in the on-cycle case. However, while we cannot rule out the absence of response in the off-cycle case, we cannot leverage statistically significant ones either. The transfer rate would increase by an average of 0.89 percentage points and amenities by 1.03%, and productivities would decline by 9.5%, but none of these responses would differ significantly from zero. Finally, in either case, the tax rate responses are insignificant from a statistical and economic point of view: +0.0055 percentage points after an off-cycle hurricane and 0.05 percentage points after an on-cycle one. Together, these results mean that electoral motives originally trigger any statistically significant response of the fundamentals to the hurricane.

Discussion. In our model, the observed population sorting pattern after on-cycle disasters cannot be explained solely by higher post-disaster transfers. Hence, higher fundamental amenities work as a compensating differential to rationalize the assumption of a spatial equilibrium jointly with the population sorting pattern in the data. The structure of our model allows us to specifically disentangle the impact of natural disasters on quality of life, productivity, and government spending, respectively.

Our model's increasing local amenity findings echo the channels discussed in Section 4.4 of the empirical results. Overall, our plausibility checks (see Figure A.15) revealed that the transformed amenity values represent improvements in the local quality of life. Local communities, supported by the federal and state governments, use post-disaster transfers to build higher-quality public infrastructure and housing after an on-cycle disaster, which translates into higher local amenity levels.

Our model also mirrors the local economy distortions documented in Section 4.4. In intervening in the local economy, the government might provide higher amenity levels and distort regional productivity. We show how more elevated post-disaster subsidies close to Election Day crowd out the manufacturing industry, lower productivity, and reduce economic growth, consistent with the Dutch disease hypothesis (e.g., Allcott and Keniston, 2017; Bulte et al., 2018; Corden and Neary, 1982; Rajan and Subramanian, 2011).

6.5 In-Sample Performance

To assess the model's in-sample performance, we simulate population and wage dynamics between 2001 and 2019. We use the reported parameters of Table 1, our estimated trade and migration costs, the recovered fundamentals, 2001 local tax, and transfer rates to solve for the equilibrium wages, population, and utility. We update fundamental amenities, productivity, and transfer rates according to our estimated impact functions. Appendix Figure B.8 presents the model performance in explaining the population and wage dynamics in response to natural disasters between 2001 and 2019. The model performs well in reproducing the population and wage average dynamics using our model-implied impact functions. Population increases and wages decline in response to on-cycle disasters. The average treatment effects of on- and off-cycle hurricanes on the population and wages predicted by our model are not statistically different from those leveraged in the data. We also present the correlation between observed population and wage levels and their simulated counterparts, which shows that the model reproduces the spatial distributions of population and wages well. Specifically, the correlation between population levels in the data and model is 0.997, and the correlation between simulated and actual wages is 0.869.

7 Counterfactual Analysis

Next, we assess the importance of electoral-cycle-driven post-disaster policies for the spatial distribution of economic activity and the aggregate economy. To do so, we first simulate our baseline scenario under the current electoral-cycle-driven post-disaster policy. Then, we simulate a scenario where we remove the impact of the electoral cycle on the post-disaster economy.

Using historical and synthetic storm tracks, we start from the initial spatial equilibrium in the baseline year 2001 and simulate the model forward for 80 years. We use only an 80-year simulation period mainly for two reasons. First, determining the far future fundamental amenity distribution requires understanding the long-run evolution of anthropogenic adaptation to climate change consequences. Making such an attempt is far beyond the scope of this paper, and not accounting for this critique would weaken the potential of our results. Second, it is still being determined whether the political and electoral institutions will remain as of today in the far future and whether electoral motives will be unchanged. While difficult to amend, these latter are, of course, subject to constitutional changes.

Considering these two reasons, we maintain current climate conditions unchanged and avoid modeling climate change scenarios as determined by the various Intergovernmental Panel on Climate Change reports. Indeed, it is likely that neither the frequency nor the intensity of future major hurricanes will drastically change in the North Atlantic basin for at least 50 to 100 years (Emanuel, 2011; Knutson et al., 2020). Of course, other disastrous consequences of climate change will emerge in the upcoming decades. However, accounting for multiple disaster types in our simulations would require a deep understanding of how each hazard would dynamically evolve at the local level, even more so under several unified, hypothetical climate change scenarios. Beyond the technical challenge, we have yet to be aware of any attempt made in the economic or climate science literature.

To inform our model, we generate synthetic hurricane paths using the STORM dataset (Bloemendaal et al., 2020). Appendix Figure B.4 shows examples of the distribution of on- and off-cycle hurricanes in this dataset. Again, storms are geographically balanced. As in our empirical analysis, we use the radius of maximum winds and the first landfall within a 20-year time window to identify treated counties, as shown in Appendix Figure B.5.[18]

We compare our baseline scenario, which features post-disaster grants, amenities, and productivity levels that vary across the electoral cycle, to a counterfactual scenario without these impacts. More specifically, in our counterfactual scenario, the impact of natural disasters on fiscal transfers, amenities, and productivity is independent of the electoral cycle. In other words, we set $\varphi_y = 0$ and impose $\Lambda_t^y(r) = \bar{\lambda}_y \cdot \mathbb{1}_t(r)$ for any t and for all $r \in S$ in equations (3), (13), and (7), where $y \in \{\bar{a}_t(r), \theta_t(r), \tau_t(r)\}$. Further, we assume fixed values for the exogenous parameters. We maintain the same values of the local tax rates $t_t(r)$, the trade costs $\zeta(s,r)$, and the migration costs $m_2(r)$ as in the initial equilibrium in 2001. Using the system (21) and (22) together with (5), we then solve for the new (counterfactual) equilibrium wages, population density, and utility for any t and all $r \in S$. We update $\tau_t(\cdot)$ according to (7), $a_t(\cdot)$ according to (3), and $\theta_t(\cdot)$ according to (13).

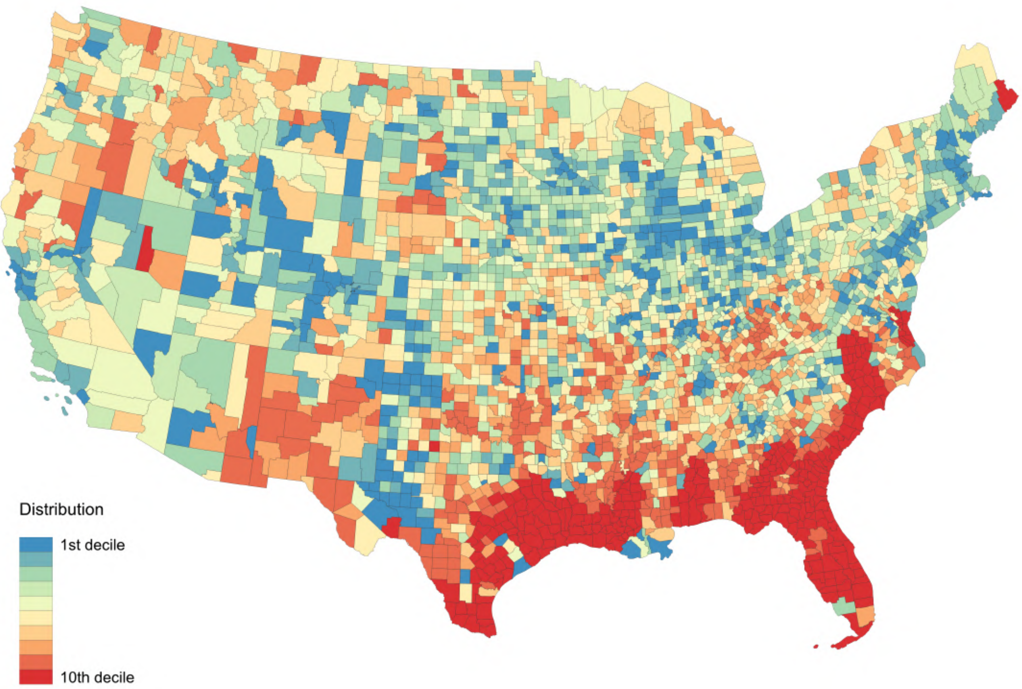
In Appendix B.3.3, we also present the results of a policy that removes the post-disaster transfers' transformation rate into local amenities or their impact on local productivity. Finally, we illustrate how to use our dynamic spatial model for evaluating the efficiency cost of the electoral-cycle-driven post-disaster policy.

7.1 Population Sorting

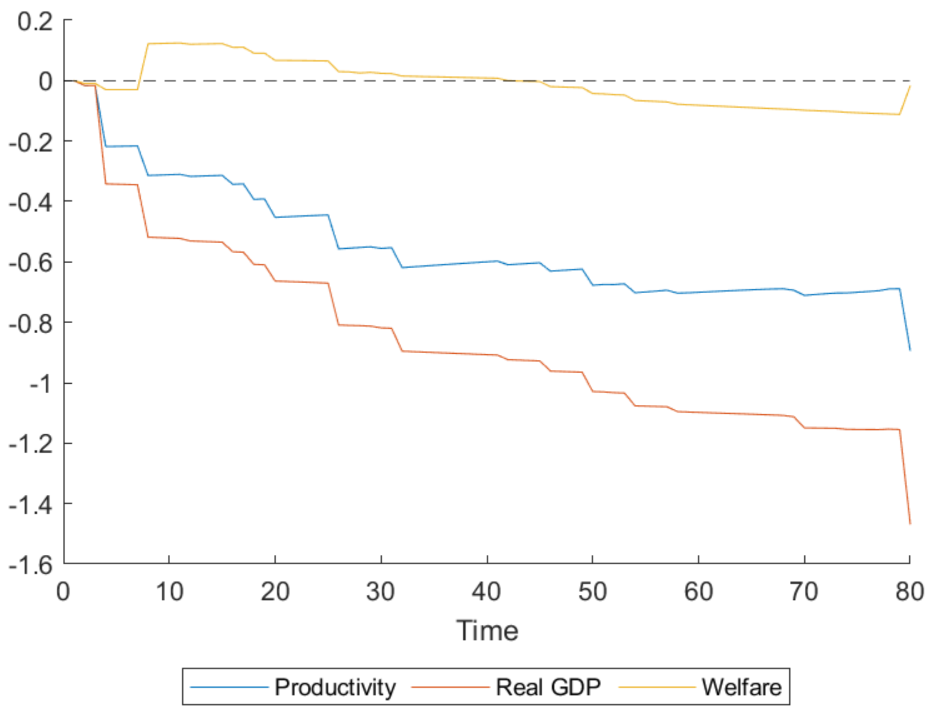
Fiscal transfers change the spatial distribution of economic activity by creating incentives for households to move toward regions receiving transfers (Fajgelbaum and Gaubert (2020); Henkel et al. (2021)). Analogously, switching off the effect of the electoral-cycle-driven transfers induces fewer people to stay in places hit by a natural disaster. Economic activity instead moves toward high-fiscal-revenue regions that are financing the transfers.

Panel (a) of Figure 5 depicts ratios in local population size after 80 years in our baseline scenario relative to our counterfactual without the effect of the electoral cycle. In our baseline, more households

[18] Recall from Sections 4 and 6.4 that in using an absorbing treatment, we capture the average effect of ever being treated within the sample time window.



(a) Population Size after 80 Years: Baseline vs. Counterfactual



(b) Percentage Changes in Aggregate Productivity, Real GDP, and Welfare: Baseline vs. Counterfactual

Figure 5: Aggregate Changes of Electoral-Cycle-Driven Post-Disaster Polices

Notes: The map in Panel (a) depicts the ratio in local population size between current post-disaster policies and a counterfactual scenario without electoral-cycle-driven post-disaster policies after 80 years of simulation. The warm color represents more households in the baseline scenario after 80 years relative to the counterfactual. Panel (b) depicts the changes in aggregate productivity, real GDP, and welfare given the current post-disaster policies compared to the counterfactual scenario.

would stay in coastal areas with higher hurricane risks around the Gulf of Mexico, Southeast Florida, and the Atlantic Ocean at the expense of highly productive areas such as New York, Chicago, and the San Francisco Bay Area. Compared to a scenario without electoral-cycle-driven post-disaster policies, the current post-disaster policies increase on-cycle county populations by 13.06% on average in 80 years. At the aggregate level, the policies induce around $25,500$ individuals (i.e., the size of a median county in 2001) to change their residence yearly. In Appendix B.3.1, we discuss the sensitivity of our results. Not surprisingly, higher migration or smaller trade elasticities would lead to more extensive migration flows and population sorting toward on-cycle counties.

7.2 Local Amenities and Public Goods Provision

In addition to population sorting, the electoral-cycle-driven post-disaster policy also impacts the government budget via changes in expenditure on publicly provided goods, tax revenues, and transfers. We now discuss our results, considering the implied changes in these components. Higher transfers and tax revenues allow for more public goods provision and thus make treated regions relatively more attractive. In Section 6.4, we found that after on-cycle events, transfers and fundamental amenities increase in response to natural disasters, creating additional incentives for people to move there.

Switching off the impact of the electoral cycle on post-disaster grants negatively affects local government budgets and therefore local public goods provision across treated regions. Moreover, it removes the transformation rate of public funds into local amenity values and the negative impact on local productivity. In our counterfactual scenario, the level of local amenities and public goods provision remains the same in places struck by an on-cycle disaster, contrary to our baseline. At the same time, public goods provision increases in the former donor regions since they need to give fewer tax revenues to other counties (especially those hit by disasters).

The electoral-cycle-driven post-disaster policy also has an aggregate impact on the total government budget. In our counterfactual scenario, public goods provision is shifted across space since we remove the increases in direct post-disaster transfers. Further, in our counterfactual, we remove the impact of the policy on the aggregate net tax revenue from production and location adjustments. Later in Section 7.5, we discuss the aggregate efficiency costs of this budgetary impact.

7.3 Real GDP and Productivity

The population sorting patterns resulting from the current system of post-disaster policies relative to removing the electoral cycle lead to additional productivity and real GDP changes across counties.

In particular, on-cycle regions that attract more populations experience a slight increase in productivity via endogenous agglomeration economies and innovation in our baseline. This sorting into the disaster-affected areas leads to out-migration from donor regions, which get less productive and innovative. Higher post-disaster efforts, however, also distort the local economy and reduce productivity significantly after an on-cycle event, as illustrated in Section 6.4. In sum, the electoral-cycle-driven post-disaster policies lead to a decline in average productivity and real GDP at the national level.

Panel (b) of Figure 5 shows that aggregate productivity decreases in our baseline relative to the counterfactual scenario. Removing the electoral-cycle-driven post-disaster policy implies no local changes in amenity and productivity values and transfers and thus less population sorting to coastal areas. The current electoral-cycle-driven post-disaster policy leads to a loss of aggregate productivity of around 0.90% and real GDP by 1.47% after 80 years relative to our counterfactual scenario. In present discounted value (PDV) terms, the policy yields an output loss of 1.17%. When we switch off the impact of the policy on local productivity, the aggregate real GDP change would be even more negative. The reason for this is that productivity declines due to an actual bigger loss of productivity spillovers. In Appendix B.3.3, we describe more rigorously the procedure and implications of our additional counterfactual exercise. Overall, the current post-disaster policies in the U.S. lower aggregate productivity and real GDP at the national level.

7.4 Welfare

Population sorting affects productivity, real GDP, endogenous amenity values, and congestion forces. In our counterfactual, amenities stay the same in coastal areas after being hit by on-cycle disasters, which attracts fewer people to these places compared to the baseline scenario. Donor regions not exposed to natural disasters now gain population size in our counterfactual. This reallocation of labor relaxes local congestion in coastal areas, but also lowers amenities in areas receiving an inflow of population due to the increased congestion. Those implied changes in local amenities are essential for the aggregate welfare implications of post-disaster policies. Our measure of aggregate welfare accounts for these local amenity effects, whereas aggregate productivity and real GDP do not.

Our simulation shows that due to the electoral motives affecting the distribution of post-disaster grants, by 2081, welfare declines mildly by 0.02% in the baseline relative to our counterfactual. Even if we measure welfare as the present discounted value of the population-weighted average of $u(r)$, the resulting welfare change is only +0.17%. Stated differently, even though the current system of post-disaster policies leads to a loss in aggregate productivity and real GDP, it barely affects aggregate welfare compared to a scenario without the electoral cycle.

This result can be explained as follows. With less (or no) support from post-disaster policies in our counterfactual scenario, amenities stay the same in locations hit by an on-cycle natural disaster. This occurs because post-disaster programs must be included to transform public funds into higher-quality amenities that households value. We thus find that the population gets less concentrated in the coastal areas affected by natural disasters in our counterfactual scenario. A higher rate of transformation of public funds into local amenity values inflates the aggregate amount of amenities that households can benefit from in the baseline compared to the counterfactual. Hence, the aggregate welfare change is smaller when we switch off the transformation of fundamental amenities in an additional counterfactual (see Appendix B.3.3).

Recall that we have quantified the model such that the static dispersion forces are higher than the agglomeration forces at the margin. As a result, we also see in our baseline a more considerable net increase in amenities (due to a higher concentration of population in coastal areas) than the accompanying productivity losses due to out-migration from the most productive cities. Due to in-migration, these net donor regions are more productive in our counterfactuals. Some net donor regions are located on the Atlantic coast in the initial equilibrium (see Figure 4), and these places feature high fundamental productivity and amenity levels but are also initially congested in the initial equilibrium. However, in our counterfactual scenario, these places also become much more congested without electoral-cycle-driven post-disaster policies. The latter effect dominates the overall welfare effects in our counterfactual simulations. However, in the long run, dynamic agglomeration spillovers that account for past productivity on the rest of the economy are more important than static ones.

To summarize, post-disaster policies are, on average, associated with further redevelopment in affected areas, which increases the level of amenities in the economy. However, these increased amenities come at high productivity costs, mainly because over-congested but productive places finance these post-disaster policies. The latter productivity effect cancels the positive impact of amenities on welfare.

7.5 Evaluating Efficiency

To make our aggregate results comparable to other policies, we approximate the efficiency cost by the marginal value of public funds (MVPF) (Mayshar, 1990; Hendren and Sprung-Keyser, 2020). In doing so, we assess the benefit or cost (i.e., willingness to pay) and the corresponding net cost for each county of the electoral-cycle-driven post-disaster policy. Relying on our dynamic spatial model for this exercise allows us to include not only the policy's initial cost but also all other general equilibrium effects like location decisions, price adjustments on the government budget, production externalities, and dynamic growth effects associated with changes in the spatial distribution of economic activity. Remember that the policy reallocates the population mass of a median-sized county across space annually with a sizable impact on the aggregate economy.

In evaluating the net impact of the electoral-cycle-driven post-disaster policy on government budgets and public goods provision, we calculate the present discounted value of the sum of local government budgets. We calculate foregone tax revenues and transfer paid to derive the net cost of maintaining the current electoral-cycle-driven post-disaster policies. Our analysis incorporates specifically the harmful effects of spatial misallocation of labor and productivity distortions on future tax revenues. Due to significant locational responses to the policy, their effects on tax revenues are big compared to the post-disaster transfer. Discounting the foregone tax revenues and post-disaster transfers to the present value, we get an annual \$64 tax revenue loss per \$14 average annual post-disaster grant. Overall, the current policy makes the average U.S. taxpayer pay for it by \$22 billion per year, that is, \$78 per capita per year. Thus, the fiscal externality alone comprises roughly 82% of the total net cost of the policy. Our model-based aggregate measure suggests that the policy has misallocated the population mass of $459,000$ individuals, i.e., around 0.16% of the total population, across space, resulting in net costs of around \$396 billion between 2001 and 2019. Examining the size of higher post-disaster transfers allocated during the same period with a simple back-of-the-envelope exercise yields only \$289 billion because it understates the policy's aggregate consequences.[19]

We next examine how much taxpayers would be willing to pay for the policy and how this compares to its net costs. Our simulated changes of present discounted utility between our baseline and counterfactual allow us to calculate each region's marginal utility of the electoral-cycle-driven post-disaster policy. Dividing the marginal utility of the policy by the marginal utility of income in 2001 gives the willingness to pay, $WTP(r) = \Delta \text{PDV} u(r) / \left(\frac{\partial u(r)}{\partial w(r)} |_{2001} \right)$, of each household in location r for the current electoral-cycle-driven post-disaster policy. Regions never hit by an on-cycle hurricane have a negative average annual WTP component of \$61. In contrast, on-cycle regions have a positive WTP of \$780 per capita per year. Combined, this yields an aggregate willingness to pay, $WTP = \int_S WTP(s) H(s) \bar{L}(s) ds$, of \$33 per capita and year.

We then compute the $MVPF$ of the electoral-cycle-driven post-disaster policy by dividing the

[19] We briefly describe our approach in Appendix B.3.2.

aggregate willingness to pay WTP by the net cost to the government:

$$\text{MVPF} \equiv \frac{\text{WTP}}{\text{Net Cost}}.$$

We derive a small MVPF for the electoral-cycle-driven post-disaster policy. Tax revenue is diverted to less valuable public expenditures each period, resulting in a social loss. For every dollar cost to the government for the policy, U.S. residents would be willing to pay only \$0.43. For comparison, we also calculate the MVPF of additional counterfactual policies that shut down the amenity or productivity effects of the policy. The MVPF of these counterfactual policies is much higher than that of the policy, confirming the intuition that the amenity and productivity channels increase efficiency costs.

8 Conclusion

This paper provides new empirical and theoretical evidence on the spatial consequences of public policies driven by electoral motives. We exploit the exogenous timing of hurricanes relative to Election Day in the United States to study the impact of electoral motives on post-disaster efforts and subsequent population sorting.

We first show empirically that on-cycle hurricanes lead to electorally motivated increases in local public goods provision, especially in core transportation and energy infrastructures, which generate durable improvements in the local quality of life but also distort local labor markets. As a result, on-cycle hurricanes lead to a significant, immediate, and long-lasting net increase in population, indicating that individuals sort into exposed areas following an electorally motivated post-disaster intervention. In contrast, off-cycle disasters increase intergovernmental transfers to affected counties positively but more mildly. These transfers do not translate into statistically significant responses in local public goods provision or population, indicating that the policy response does not overcompensate for the negative disaster shock, contrary to on-cycle hurricanes.

We next introduce the relationship between electoral cycles and post-disaster efforts as a new feature in a dynamic spatial general equilibrium model. In our quantitative simulations, we switch off the impact of electoral motives on post-disaster policies. Economic growth is negatively affected by electoral-cycle-driven post-disaster policies, which lower productivity and real GDP by pushing people to hazard-prone coastal areas and away from high-productivity areas. Under the current electoral-cycle-driven post-disaster policies, U.S. residents would be willing to pay only \$0.43 for every dollar of electorally motivated post-disaster transfers.

This work highlights that while fiscal redistribution occurs in space, it might be driven by electoral motives. Consequently, electoral motives may alter the spatial distribution of fiscal resources and thus trigger inefficient sorting responses as individual decisions are interrelated due to spillover effects. A more concerted effort may be needed to shut down harmful spillovers while permitting productive ones to flourish. In the context of post-disaster policies in the United States, we show that electorally motivated transfers lead populations to inefficiently sort in hazard-prone areas with sizable negative implications for the aggregate economy. Therefore, our work also calls into question the institutional design of public policies bearing high political interests. In our case study, efficiency requires the amount and type of post-disaster efforts to be independent of electoral motives, which entails moving towards more autonomous, apolitical managing institutions.

In bridging political economy and economic geography, our model framework can be applied to many other cases where electoral motives affect spatially redistributive policies. One promising research avenue is incorporating other public policies that vary along the electoral cycle into our general equilibrium framework. The existence of other electorally motivated policies, such as corporate (Foremny and Riedel, 2014) or value-added taxation (Hallerberg and Scartascini, 2017; Benzarti et al., 2020), may intensify the inefficient spatial sorting patterns in general equilibrium. We hope this new approach provides researchers with an adequate tool to pursue exciting projects evaluating to what degree electoral motives affect the aggregate economy.

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APPENDIX

This Appendix provides (1) additional empirical results, internal and external validity checks supporting our findings from the main text, (2) theoretical derivations, (3) a description of the quantification of the model, and (4) sensitivity checks and additional simulation results.

A Empirical Appendix

Subsection A.1 of this empirical part of the Appendix provides some background information on U.S. hurricanes. Subsection A.5.3 shows that U.S. federal assistance varies significantly across the electoral cycle. Subsection A.2 uses the IRS county-to-county migration data to decompose the population stock into stayers, migrants moving in (inflow), and out (outflow) of counties. Subsection A.3 shows that our main empirical results are consistent across alternative comparison groups, estimators, and specifications. Subsection A.4 provides external validity checks. Finally, Subsection A.5 documents the channels behind our main empirical results.

A.1 Hurricanes and Post-Disaster Policies

Table A.1 lists the number of historical hurricanes in the North Atlantic basin from 1851 to 2019. Table A.2 provides a list of the U.S. hurricanes in our sample. From 2001 to 2019, 33 hurricanes occurred, all during the hurricane season, between July and November. The intensity and the wind speed of tropical cyclones vary across hurricanes. We match the date of hurricanes (Column (1)) to the closest Election Day date (Column (9)) and calculate how many days are left until the next Election Day when the hurricanes hit. We define a hurricane as an on (off) cycle if the next election is set less (more) than 365 days after the hurricane hits. Table A.3 presents descriptive statistics of treated areas' 2001 primary outcomes of interest for counties ever hit by any on-or off-cycle hurricanes between 2001 and 2019.

Table A.1: HISTORICAL HURRICANES IN THE NORTH ATLANTIC BASIN (1851–2019)

	Tropical Storms	**Hurricanes**	**Major Hurricanes**	**U.S. Hurricanes**
Total Numbers	1,625	917	315	294
Annual Mean	9.67	5	1.875	1.75
Annual Median	9	5	2	2
Annual Max	28	15	7	7
Annual Min	1	0	0	0

Notes: Descriptive statistics are extracted from the "North Atlantic Hurricane Basin (1851–2019) Comparison of Original and Revised HURDAT" from NOAA. All columns report descriptive statistics about tropical storms that have formed in the North Atlantic basin broken down into different categories: tropical storms ($\geq$ 39 mph), hurricanes ($\geq$ 73 mph), and major hurricanes ($\geq$ 111 mph). U.S. hurricanes are hurricanes that made landfall in the U.S. Mean, median, maximum, and minimum are defined annually.

Table A.2: List of U.S. Hurricanes (2001–2019)

(1) Date	(2) Subbasin	(3) Name	(4) Max. Wind (kts)	(5) Pressure (mb)	(6) Pressure (mb) of the outermost closed isobar	(7) Radius (miles) of the outermost closed isobar	(8) Radius (miles) of maximum winds	(9) Next Election Day	(10) Days until next Election Day	(11) On-cycle Hurricane
2002-10-03	GM	LILI	80	963	1012	200	10	2002-11-05	33	1
2003-07-15	GM	CLAUDETTE	80	979	1013	180	15	2004-11-02	476	0
2003-09-19	NA	ISABEL	65	969	1010	300	45	2004-11-02	410	0
2004-08-03	NA	ALEX	85	973				2004-11-02	91	1
2004-08-14	NA	CHARLEY	65	997				2004-11-02	80	1
2004-09-05	NA	FRANCES	95	958				2004-11-02	58	1
2004-08-29	NA	GASTON	65	986				2004-11-02	65	1
2004-09-16	GM	IVAN	107	937				2004-11-02	47	1
2004-09-26	NA	JEANNE	95	953				2004-11-02	37	1
2005-07-06	GM	CINDY	65	991	1012	150	20	2006-11-07	489	0
2005-07-10	GM	DENNIS	110	942	1011	250	10	2006-11-07	485	0
2005-08-29	NA	KATRINA	65	954	1005	300	27	2006-11-07	435	0
2005-09-14	NA	OPHELIA	75	979	1013	200	30	2006-11-07	419	0
2005-09-24	GM	RITA	89	940	1009	300	20	2006-11-07	409	0
2005-10-24	GM	WILMA	105	950	1005	300	30	2006-11-07	379	0
2007-09-13	NA	HUMBERTO	74	986	1012	90	12	2008-11-04	418	0
2008-07-24	NA	DOLLY	60	981	1010	180	15	2008-11-04	103	1
2008-09-01	NA	GUSTAV	90	954	1007	232	25	2008-11-04	64	1
2008-09-13	GM	IKE	95	951	1007	312	35	2008-11-04	52	1
2011-08-28	NA	IRENE	60	958	1007	382	100	2012-11-06	436	0
2012-08-29	GM	ISAAC	70	966	1008	275	40	2012-11-06	69	1
2012-10-29	NA	SANDY	80	940	1004	500	110	2012-11-06	8	1
2014-07-04	NA	ARTHUR	85	975	1013	160	20	2014-11-04	123	1
2016-09-02	NA	HERMINE	65	982	1009	240	25	2016-11-08	67	1
2016-10-09	NA	MATTHEW	70	981	1008	270	40	2016-11-08	30	1
2017-08-26	GM	HARVEY	115	941	1009	180	15	2018-11-06	437	0
2017-09-11	NA	IRMA	65	961	1008	350	20	2018-11-06	421	0
2017-09-20	NA	MARIA	115	935	1009	180	15	2018-11-06	412	0
2017-10-08	GM	NATE	75	983	1006	250	25	2018-11-06	394	0
2018-09-14	NA	FLORENCE	62	973	1012	205	30	2018-11-06	53	1
2018-10-11	NA	MICHAEL	65	968	1007	215	17	2018-11-06	26	1
2019-07-13	GM	BARRY	65	993	1007	180	40	2020-11-03	479	0
2019-09-06	NA	DORIAN	90	956	1014	300	25	2020-11-03	424	0

Notes: The data displayed in the table correspond to the date of maximum winds when the hurricane hit the U.S. Subbasin: "GM"= Gulf of Mexico, "NA" = North Atlantic, and "CS" = Caribbean Sea. The outermost closed isobar determines the maximum extent of the cyclone.

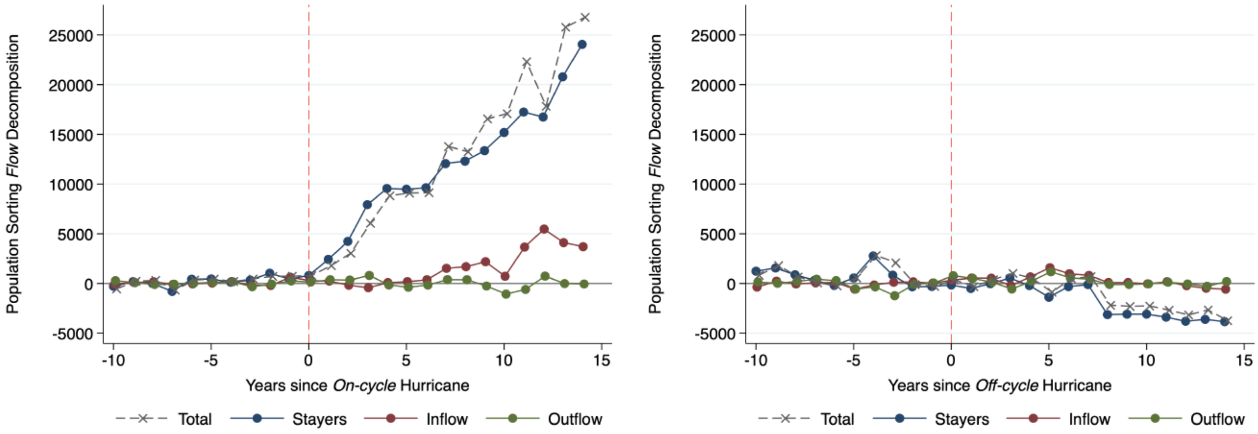
Table A.3: Descriptive Statistics of Treated Areas' 2001 Main Outcomes

	(1) All μ σ	(2) On-cycle μ σ	(3) Off-Cycle μ σ	(4) Mean Diff (3)-(2) (t-stat)
Local Characteristics				
Total Population	160,864.3	139,749.2	174,342.1	34,592.87
	(451,487.2)	(349,878.8)	(50,6054.8)	(0.73)
GDP per capita (in \$2001)	29,492.23	28,907.02	29,865.77	958.75
	(37,946.38)	(53,426.63)	(23,346.32)	(0.24)
Local Public Budget (in thsnd. dollars)				
Public Good Provision	740,801.7	528,698	876,187	347,488.9
	(2,034,228)	(1,366,036)	(2,356,955)	(1.64)
Own-collected Revenues	380,060.5	285,091.4	440,679.1	155,587.7
	(1,814,153)	(856,882.4)	(221,8977)	(0.82)
Intergovernmental Revenues	216,745.4	145,142	26,2449.7	117,307.7
	(1,156,179)	(334,854.8)	(145,4943)	(0.97)
Debt Issued	83,853.07	59,558.95	99,359.96	39,801.01
	(585,148.1)	(266,453.1)	(718,371.1)	(0.65)
Observations	385	150	235	

Notes: Descriptive statistics of treated areas' 2001 main outcomes for counties (column 1) ever hit, (column 2) first hit by on-cycle hurricanes, and for those (column 3) first hit by off-cycle hurricanes between 2001 and 2019. μ and σ are the mean and standard deviation, respectively.

A.2 IRS Tax Returns and County-to-County Flows

To better understand how population sorting operates after an on-cycle hurricane, we split the population stock into flows of stayers, migrants moving in (inflow), and migrants moving out (outflow) using the IRS county-to-county migration data. When we regress each flow separately against our primary treatment, we observe that the increase in population after on-cycle hurricanes follows an increasing number of people deciding to stay. While giving some insights, the IRS data limitations prevent a straightforward interpretation of these results.



(a) On-Cycle (b) Off-Cycle

Figure A.1: On- vs. Off-Cycle Hurricane Treatment Effect on Population Flows

Notes: This figure plots the estimates of the event study and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the total population and the number of stayers, inflows, and outflows. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

A.3 Internal Validity

We report further checks to document the internal validity of our results.

A.3.1 Alternative Control Groups

As developed in Section 4, one might be worried that our control group could follow different pre-trends or be subject to regional spillovers, threatening our identification strategy. This section documents that our main outcome results are robust to using alternative comparison groups. We define eight possible comparison groups amongst untreated counties: counties not exposed to hurricanes, counties exposed to tropical storms, counties exposed to hurricanes, counties within 1000, 500, 250, and 100 kilometers from the centroid of affected counties, and finally, all counties in the continental United States. We investigate how including not-yet-treated counties in these groups affects our main results. To measure exposure to storms, we plot 1000 years of simulated storm tracks from the STORM dataset (Bloemendaal et al., 2020) which mimics the current climate conditions from the storm database used for our primary analysis. In doing so, we intend to capture storm exposure at the extensive margin.

Unexposed locations, typically far from affected areas, such as counties in Colorado or Washington, are likely following different trends before the hurricane. Figure A.2 depicts these locations in gray. If the population declines in these unexposed counties compared to treated ones before the treatment, there is a high chance it would do so past the treatment, in which case our main result would be upward biased. The closer we get to exposed areas (in green and blue in Figure A.2) and, a fortiori, affected areas, the more likely the spillover effects. If hurricanes, especially those occurring close to elections, also encourage economic activity sorting in untreated but exposed counties, our main effect would be biased downward by including such physically close areas in our control group.

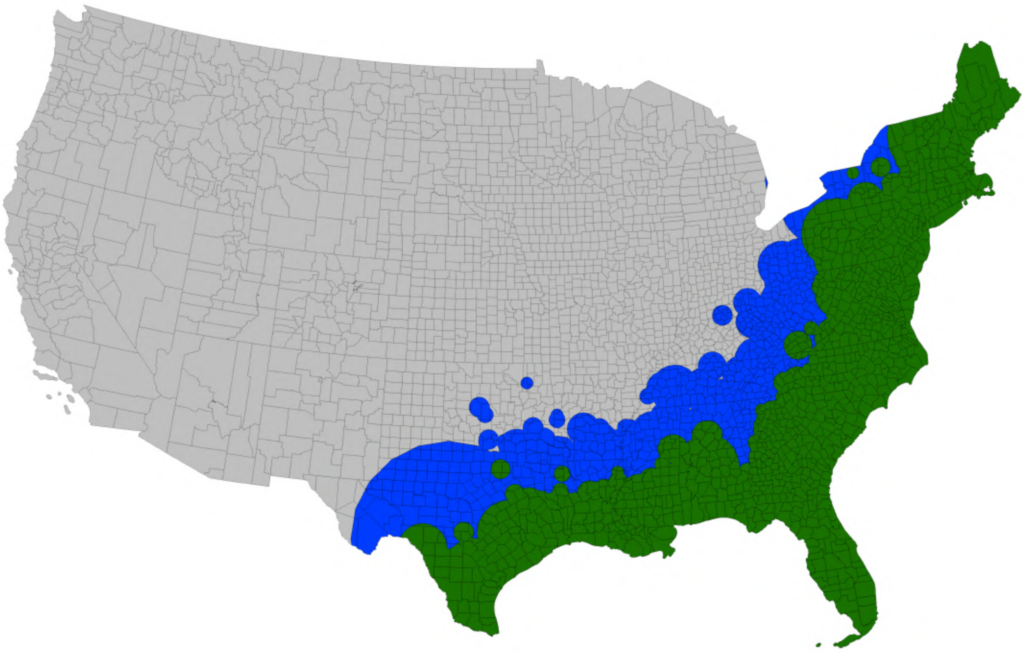
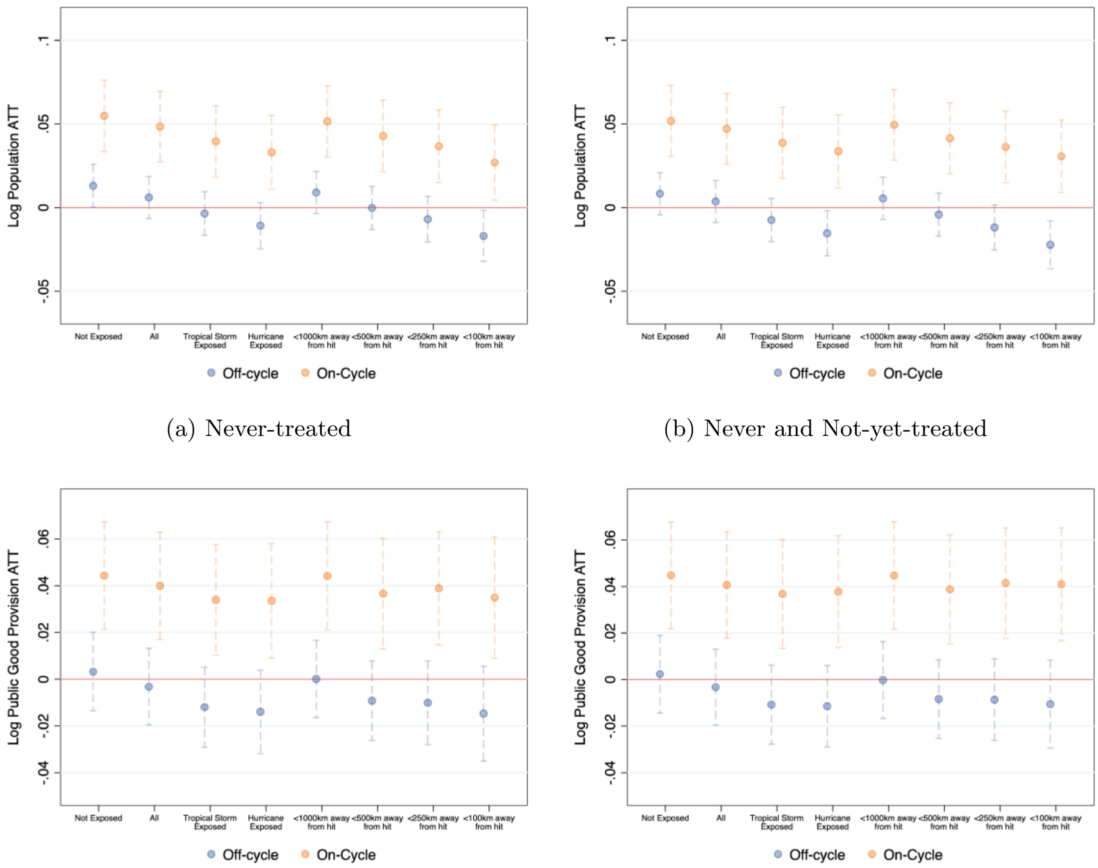


Figure A.2: Areas Exposed to Tropical Storms

Notes: This figure displays the locations exposed to tropical storms (blue and green areas) and hurricanes (only green areas). Gray areas are not exposed. Exposure was defined using 1000 years of simulated storm tracks from the STORM dataset (Bloemendaal et al., 2020).

To ensure our results' validity, we test our main specification (see Equation 1) against these alternative comparison groups. Using these alternative comparisons, Figure A.3 depicts the average impact of on- and off-cycle hurricanes on population and local public good provision. First, the effect of the electoral cycle remains in all specifications. Second, the ATTs are not statistically different across specifications. Finally, on average, control groups subject to spillovers (i.e., close to the treated areas) yield smaller ATTs. As expected, control groups subject to different pre-trends (i.e., not exposed to storms) yield larger ATTs. Together, these results are reassuring that we are correctly estimating the causal impact of on- and off-cycle hurricanes on population and local public good provision.



(a) Never-treated (b) Never and Not-yet-treated

(c) Never-treated (d) Never and Not-yet-treated

Figure A.3: On- vs. Off-Cycle Hurricane Treatment Effect Using Alternative Control Groups

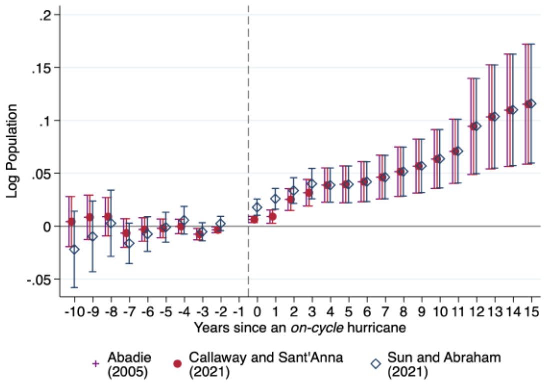
Notes: This figure plots the ATT estimates of the event study and corresponding 95% confidence bands of different specifications of equation (1) for alternative control groups. The dependent variables are the log of population (Panels (a) and (b)) and public good provision (Panels (c) and (d)). The comparison groups include never-treated counties (Panels (a) and (c)) and never and not-yet-treated counties (Panels (b) and (d)). Each panel includes in column (1) counties not exposed to any storm, (2) the rest of the U.S., (3) counties exposed to tropical storms, including hurricanes, (4) counties exposed to hurricanes only, (5-8) counties located within [1,000, 500, 250, 100 km] from treated counties. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

A.3.2 Alternative Estimators

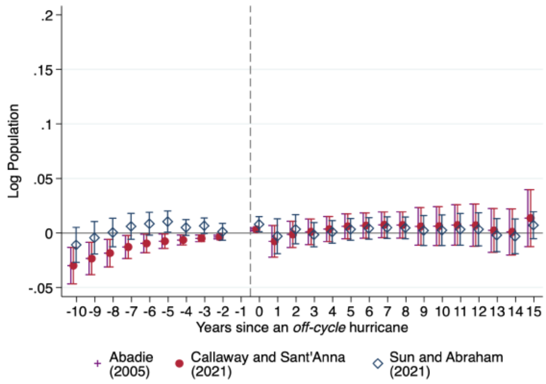
We now show in Figure A.4 that our baseline results for the primary outcomes remain qualitatively similar when applying the estimators of Abadie (2005) or Sun and Abraham (2021).

A.3.3 Controlling for Wind Speed upon Hit

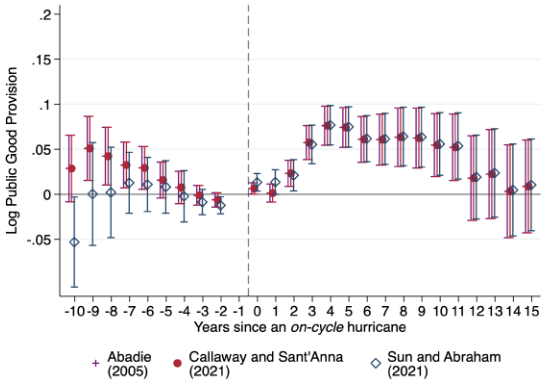
Figure A.5 depicts that the public good provision and the population responses remain unchanged when controlling for wind velocity upon landfall.



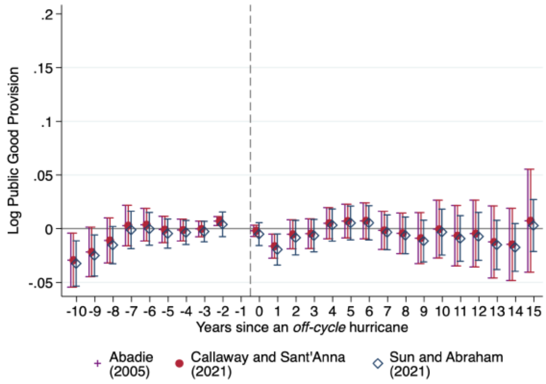
(a) On-Cycle



(b) Off-Cycle



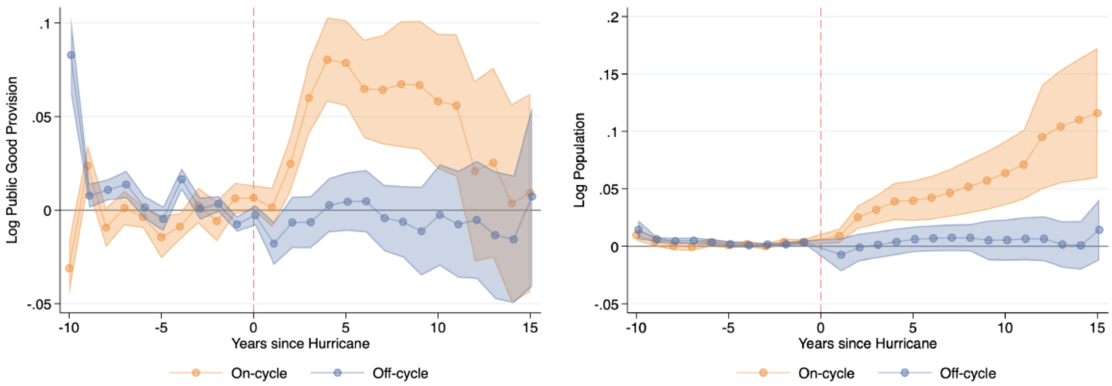
(c) On-Cycle



(d) Off-Cycle

Figure A.4: On- vs. Off-Cycle Hurricane Treatment Effect Using Alternative Estimators

Notes: This figure plots the event study estimates and corresponding 95% confidence bands of different specifications of equation (1) for various estimators. The dependent variables are the log of population (Panels (a) and (b)) and public good provision (Panels (c) and (d)). The comparison group includes all never-treated counties. Estimates are normalized to the year preceding the shock. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.



(a) Public Goods provision

(b) Population

Figure A.5: ON- VS. OFF-CYCLE HURRICANE TREATMENT EFFECT CONTROLLING FOR WIND VELOCITY

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of public good and service provision (i.e., government output; see the BEA's definition) (Panel (a)) and the log of the population (Panel (b)), aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regressions control for wind velocity upon landfall. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

A.4 External Validity

In this subsection of the Appendix, we provide evidence of the external validity of our results.

A.4.1 Extending the Time Period (1969-2019)

As our analysis extends back to 1969, Figure A.6 demonstrates that the population of counties hit by on-cycle hurricanes has grown significantly, signifying that recent extreme weather events do not drive our main result.

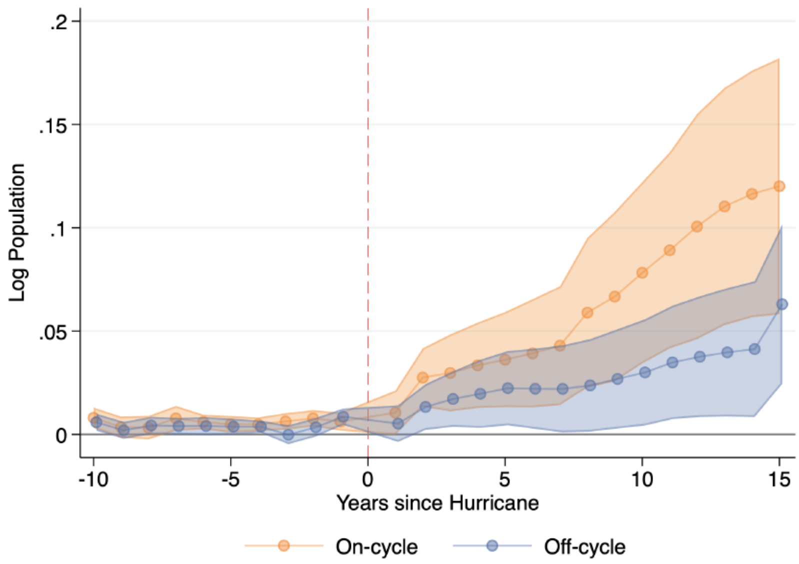


Figure A.6: On- vs. Off-Cycle Hurricane Treatment Effect on Log Population (1969–2019)

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of the population aggregated at the county level between 1969 and 2019. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

A.4.2 Largest Wildfires (1988-2019)

We now use extreme wildfires as treatments to convince ourselves that our main results can be generalized to other types of catastrophe events. There is a noticeable increase in population in counties impacted by on-cycle wildfires compared to counties impacted by off-cycle wildfires, as shown in Figure A.7.

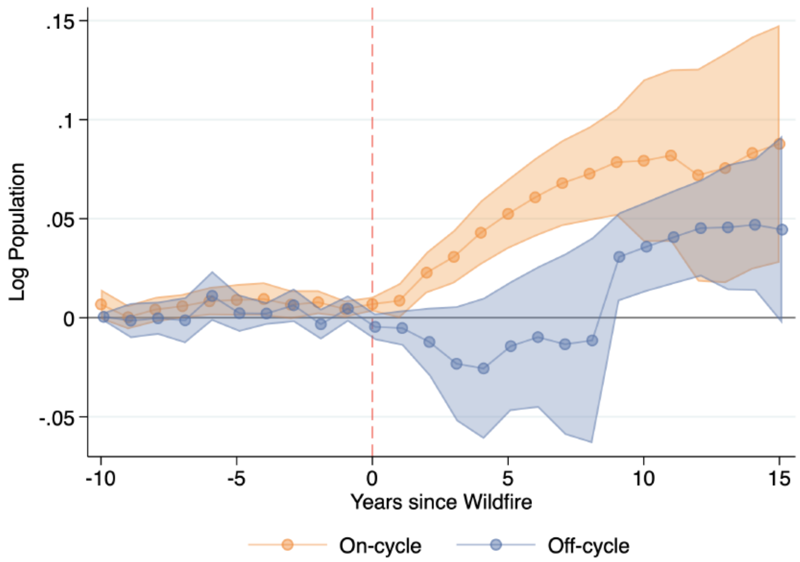


Figure A.7: On- vs. Off-Cycle Wildfire Treatment Effect on Log Population (1988–2019)

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of the population aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a wildfire. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

A.5 Mechanisms

In this subsection, we investigate the possible mechanisms behind the main empirical results we document above.

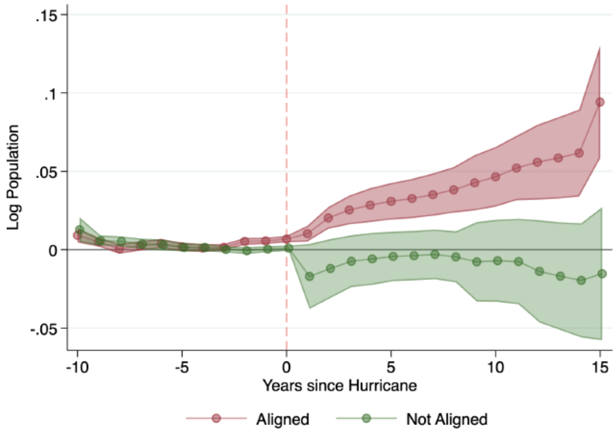
A.5.1 Electoral Motives

It may be that our main results are not driven by electoral motives but by a concurrent apolitical mechanism. This subsection further checks how electoral motives may affect the local public good provision and population sorting.

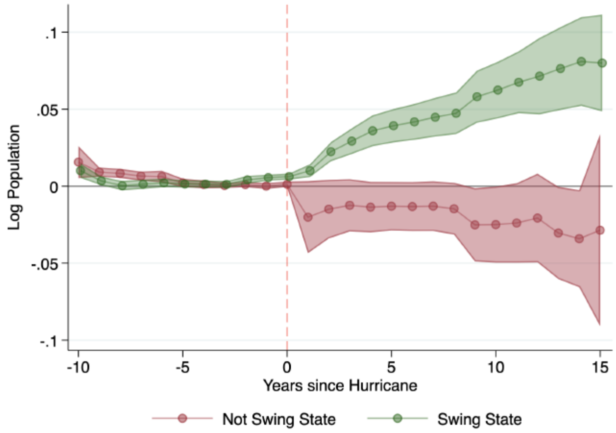
The literature has traditionally focused on pork-barrel transfers to swing voters (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996), which is a profitable strategy when the incumbents' office is highly disputed. In Presidential elections, such transfers could help the incumbent party secure the Electoral College's votes. When incumbents' offices are little disputed, ideological favoritism flourishes (Trounstine, 2006; Burgess et al., 2015; Curto-Grau et al., 2018). This latter case may be particularly notable for intergovernmental earmarked transfers, such as post-disaster grants, which typically involve spending rules determined by the grantor and the coordination of local authorities. Favoring co-partisan local authorities helps secure the incumbent's position at the expense of opposition parties

(Curto-Grau et al., 2018).

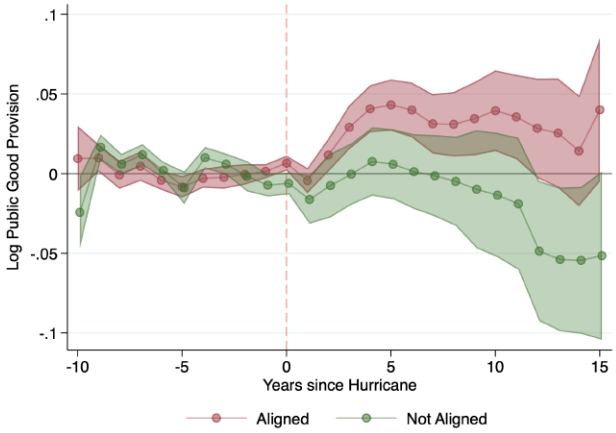
We gather electoral information from the MIT Election Lab to define political alignment and swing state status dummies. A politically aligned county is defined as having voted for the incumbent President's party in the House of Representatives on the last Election Day. Because Congressional elections are not available at the county level, we assign votes at the congressional district level to counties using population weights accounting for topographic suitability (Ferrara et al., 2021; Eckert et al., 2020). Swing states are states where the margin of victory in the last Presidential Election was below the median.



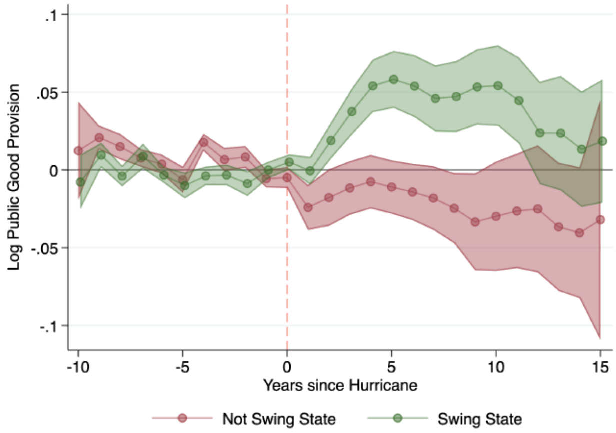
(a) Political Alignment



(b) Swing States



(c) Political Alignment



(d) Swing States

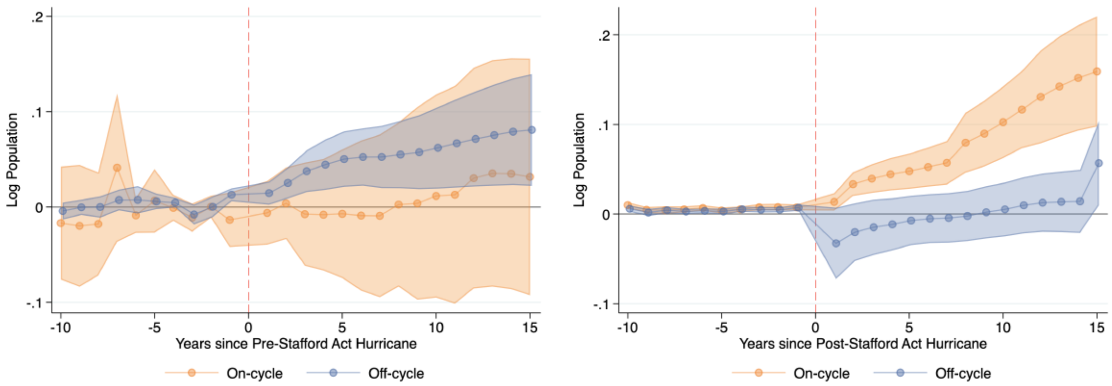
Figure A.8: Treatment Effect by Political Dimension

Notes: This figure plots estimates of the event study and corresponding 95% confidence bands of different specifications of equation (1). The dependent variables are the log of the population (Panels (a) and (b)) and public good provision (Panels (c) and (d)). The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

Using these alternative electoral dimensions supports the mechanisms developed in Section 4.4:

post-disaster efforts are a function of electoral incentives. Figure A.8 shows that counties aligned with the incumbent President's party or belonging to a swing state receive, on average, a more significant public good provision in the aftermath of a hurricane, leading to an extensive population sorting.

Our subsequent findings reinforce that federal electoral motives play a significant role in population sorting. Population sorting in affected areas started after the 1988 Stafford Act (see Figure A.9), which gave the President greater discretion over post-disaster policies (see Section 2).



(a) Before the Stafford Act of 1988

(b) After the Stafford Act of 1988

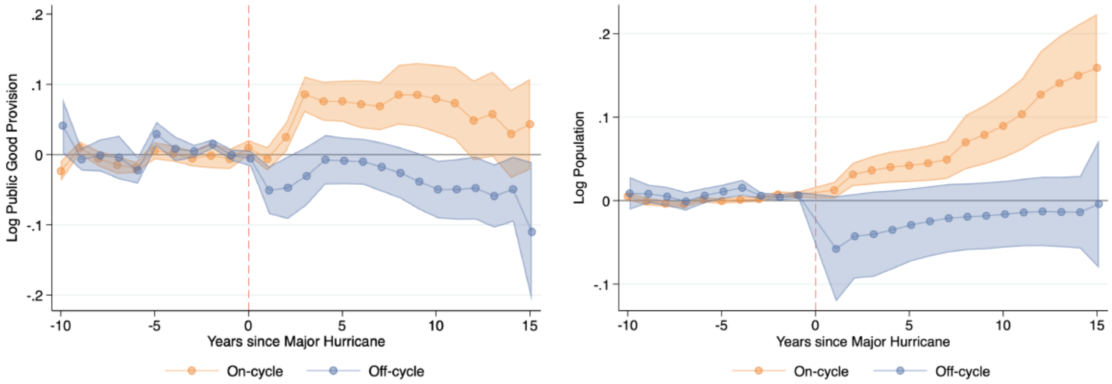
Figure A.9: On- vs. Off-Cycle Hurricane Treatment Effect on Log Population before and after the Stafford Act of 1988

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of the population aggregated at the county level between 1969 and 1988 (Panel (a)) and between 1988 and 2019 (Panel (b)). The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

Panel (a) Figure A.10 indicates that counties hit by on-cycle hurricanes with significant wind speeds provide significantly more public goods and services. In the same way, major winds are likely to trigger a sorting response, confirming that the DRF voted by federal authorities primarily responds to catastrophic events (see Panel (b) of Figure A.10).

A.5.2 Fiscal Revenues Redistribution

We now examine how counties affected by on-cycle hurricanes finance this significant increase in public goods provision. First, despite a short-lived positive response in own-collected revenues following an on-cycle hurricane, the impacts of on-cycle hurricanes on local own-collected revenues and local debt are statistically insignificant (see Figures A.11 and A.12). Finally, we document a statistically and economically significant rise in intergovernmental transfers to these counties (see Figure A.13).



(a) Local Public Goods Provision

(b) Population

Figure A.10: On- vs. Off-Cycle Major Hurricane Treatment Effect

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of public good and service provision (i.e., government output; see the BEA's definition) (Panel (a)) and the log of the population (Panel (b)), aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a major hurricane (i.e., category 3–5 hurricanes with wind velocity $\geq$ 50m/s measured by the CLIMADA wind field model). The regression models include county and year fixed effects. Standard errors are clustered at the county level.

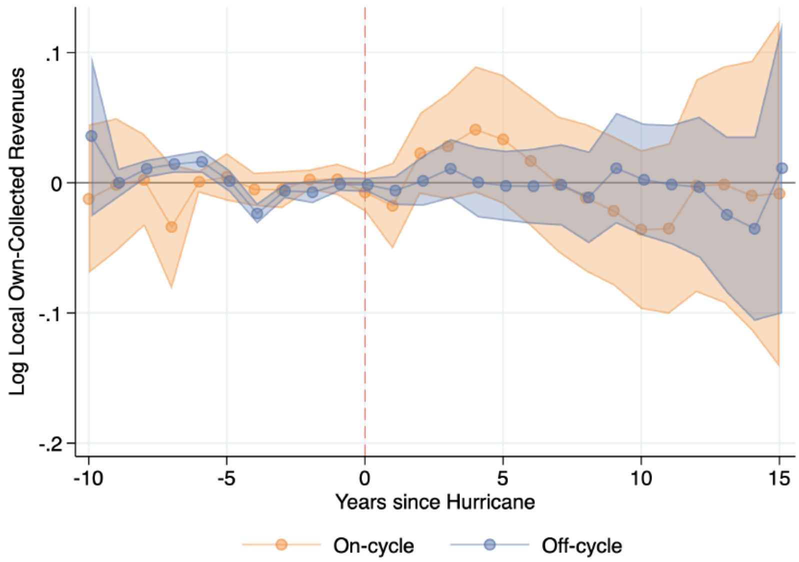


Figure A.11: On- vs. Off-Cycle Hurricane Treatment Effect on Local Own-Collected Revenues

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of local revenues collected by local authorities at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

A.5.3 On-Cycle Hurricanes and FEMA Post-Disaster Grants

In this subsection, we investigate whether the timing of hurricanes relative to Election Day is a good predictor of FEMA post-disaster grants. The sample considers all counties that were ever hit by a

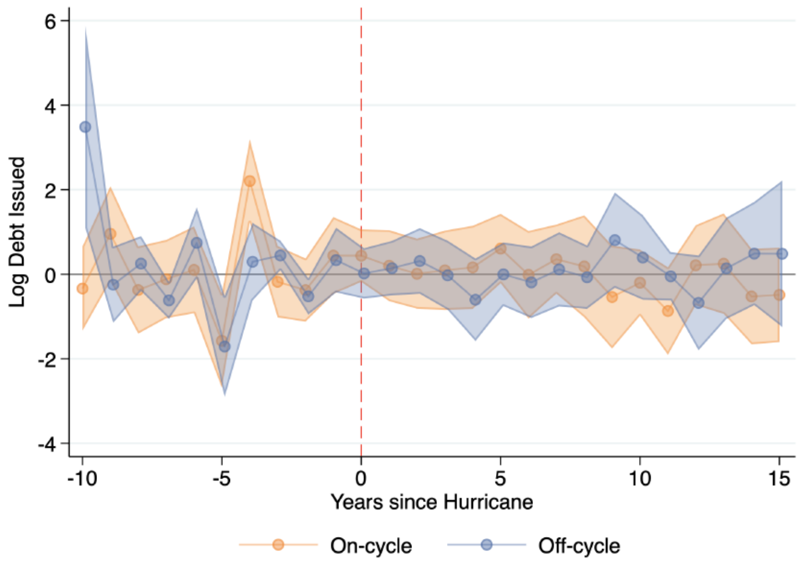


Figure A.12: On- vs. Off-Cycle Hurricane Treatment Effect on Local Debt Issued

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of local debt issued at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

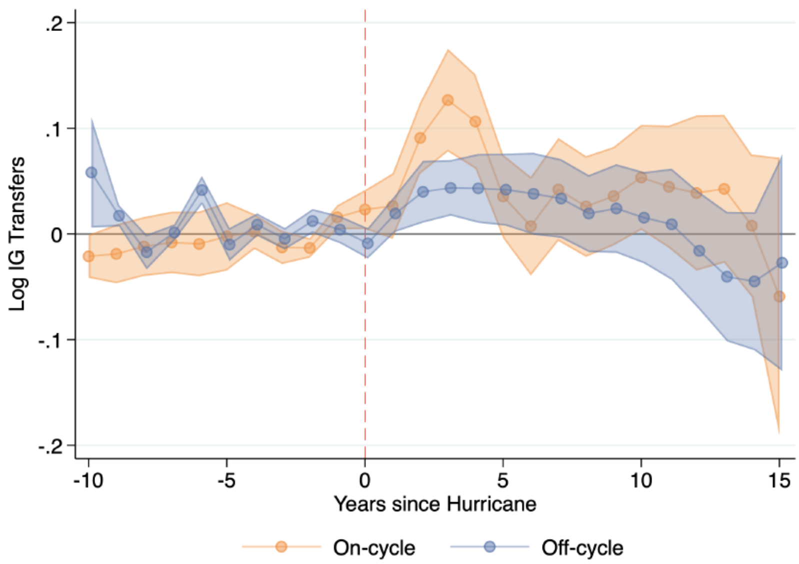


Figure A.13: On- vs. Off-Cycle Hurricane Treatment Effect on Intergovernmental Transfers

Notes: This figure plots event study estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable is the log of intergovernmental transfers at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

hurricane and received a Presidential Disaster Declaration for a hurricane between 2001 and 2019. Most post-disaster grant programs are run by FEMA (individual assistance, public assistance, hazard mitigation grant). Because post-disaster grants are earmarked, unique cash flows to economic agents, an event study specification would be less insightful. Instead, we estimate the following specification:

$$\text{Grants}_{it} = \alpha_i + \gamma_t + \beta_1.\text{On-Cycle}_{it} + \beta_2.\text{On-Cycle}_{it} \times \text{Intensity}_{it} + \epsilon_{it}, \tag{1}$$

where Grants_{it} is the log per capita post-disaster grants a county received conditional on having been hit by a hurricane and declared for the related grant program. On-Cycle_{it} indicates whether the hurricane occurred less than 365 days before Election Day, and Intensity_{it} stands for the demeaned wind intensity. Here, β_1 captures the impact of an average on-cycle hurricane compared to an average off-cycle hurricane, whereas β_2 captures the additional effect of deviating from the average intensity. Finally, α_i and γ_t are county and Congress term fixed effects that account for any potential location and time-invariant co-founders. Alternatively, we use the log number of days between a hurricane's landfall and Election day as our main treatment variable.

Table A.4 presents results on whether counties hit by an on-cycle hurricane are more likely to receive more significant per capita spending from the federal government. Column (1) shows that conditional on hurricane intensity, being hit by an on-cycle hurricane leads to a 113.2% increase in FEMA grants per capita compared to an off-cycle hurricane. The effect increases significantly with wind velocities above the mean. This result holds when conditioning the effect on Congress term and county fixed effects (Column (2)). This result is also robust to using an alternative treatment—the log number of days between a hurricane landfall and Election day: Columns (3) and (4) document that the farther a hurricane occurs from Election Day, the smaller will be the post-disaster funds transferred by FEMA. As in columns (1) and (2), deviations above average wind intensity positively impact the transferred sums.

Figure A.14 illustrates the results from Table A.4 broken down by types of grants. This general result is mainly driven by grants targeted to local public administrations (as opposed to private individuals) through mitigation and relief grants. However, the impacts of the different programs are generally statistically similar.

The results are consistent with the literature: state governments' bailout activities are more responsive where electoral accountability is greater (Besley and Burgess, 2002; Strömberg, 2004; Schneider and Kunze, 2023). Previous studies also show that governments appear to be more generous with disaster relief during election years, which can lead to suboptimal policy outcomes (Cole et al., 2012). In addition, the literature has argued that relief grants are generally favored over mitigation grants because voters have a clear pre-disaster counterfactual to judge their representative's action (Healy and Malhotra, 2009). Here, our results confirm the hypothesis that electoral motives drive post-disaster

Table A.4: On-Cycle Hurricanes and Post-Disaster Grants

	(1) All grants	(2) Relief grants	(3) Mitigation grants	(4) To Public
On-Cycle	1.132***	1.588***		
	(0.219)	(0.558)		
On-Cycle × Intensity	0.124***	0.066*		
	(0.021)	(0.029)		
Log Days			-0.631***	-1.188***
			(0.095)	(0.240)
Log Days × Intensity			0.029***	0.023***
			(0.002)	(0.004)
Observations	420	420	420	420
County FE	N	Y	N	Y
Congress FE	N	Y	N	Y
Adj. R^2	0.131	0.521	0.423	0.731

Notes: The dependent variable is the log per capita post-disaster grants a county received conditional on having been hit by a hurricane and declared for the related grant program. *LogDays* is the log number of days between a hurricane's landfall and Election day. *Intensity* stands for demeaned wind intensity ($m.s^{-1}$). The observation is at the yearly county level between 2001 and 2019. The sample includes all counties that were ever hit by a hurricane and received a Presidential Disaster Declaration for a hurricane between 2001 and 2019. Standard errors are clustered at the county level and are reported in parentheses.

efforts, but relief and mitigation grants appear to be evenly favored during on-cycle events.

A.5.4 Local Amenities and Labor Demand Shocks

We also provide evidence suggesting that governments' intervention improves local quality of life and distorts local productivity. First, Figure A.15 depicts the existence of an electoral cycle in the activity of the transportation and utility infrastructures construction industries. Next, Figure A.16 shows how more elevated post-disaster subsidies near Election Day crowd out the manufacturing industry, lower productivity, and reduce economic growth. In contrast, we do not see such changes after off-cycle events.

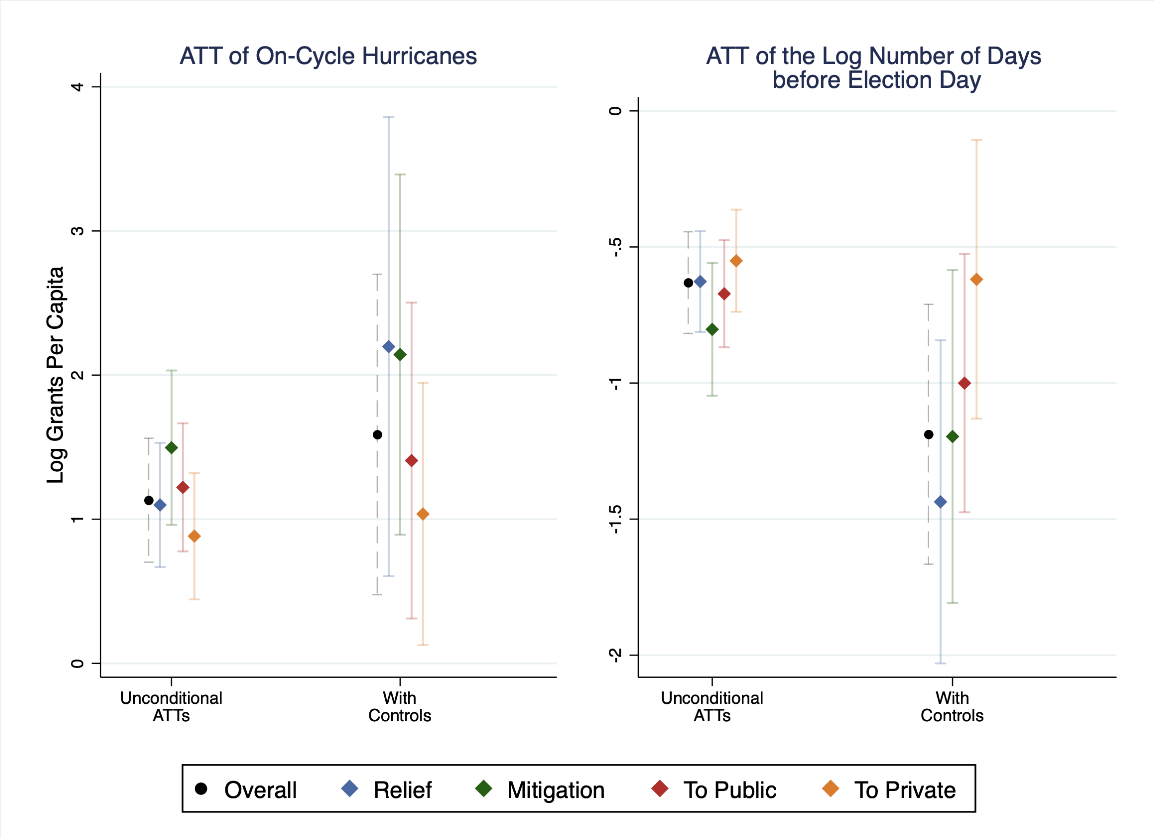


Figure A.14: Impact of Hurricane Timing on Post-Disaster Grants

Notes: This Figure displays the impact of on-cycle disasters and the log number of days before Election Day on post-disaster grants (β_1 in equation 1). The sample includes all counties that were ever hit by a hurricane and received a Presidential Disaster Declaration for a hurricane between 2001 and 2019. As in Table A.4, columns (2) and (4) condition the treatment effects on Congress term and county fixed effects. Standard errors are clustered at the county level.

B Theory Appendix

Subsection B.1 of this theoretical part of the Appendix presents derivations for the main paper. Subsection B.2 provides further information about the quantification of our model. Subsection B.3.1 reports counterfactuals for alternative parameter constellations. Subsection B.3.2 presents a simple back-of-the-envelope exercise that indicates the size of the post-disaster transfers allocated to counties hit by on-cycle hurricanes between 2001 and 2019. Subsection B.3.3 demonstrates that population sorting and aggregate dynamics depend critically on the transformation of amenities and distortion of productivity.

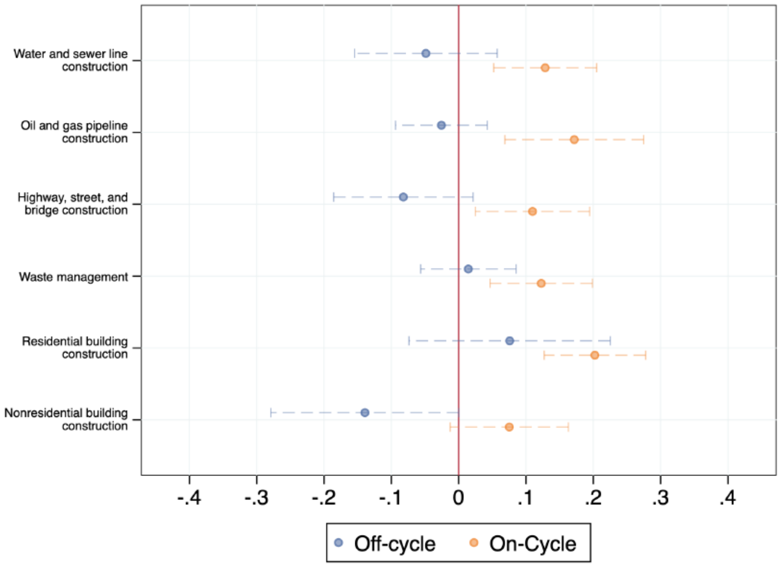


Figure A.15: On- vs. Off-Cycle Hurricane ATT on Post-Disaster Redevelopment

Notes: This figure plots ATT estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variable on the y-axis is the log of the number of establishments in selected NAICS industries aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

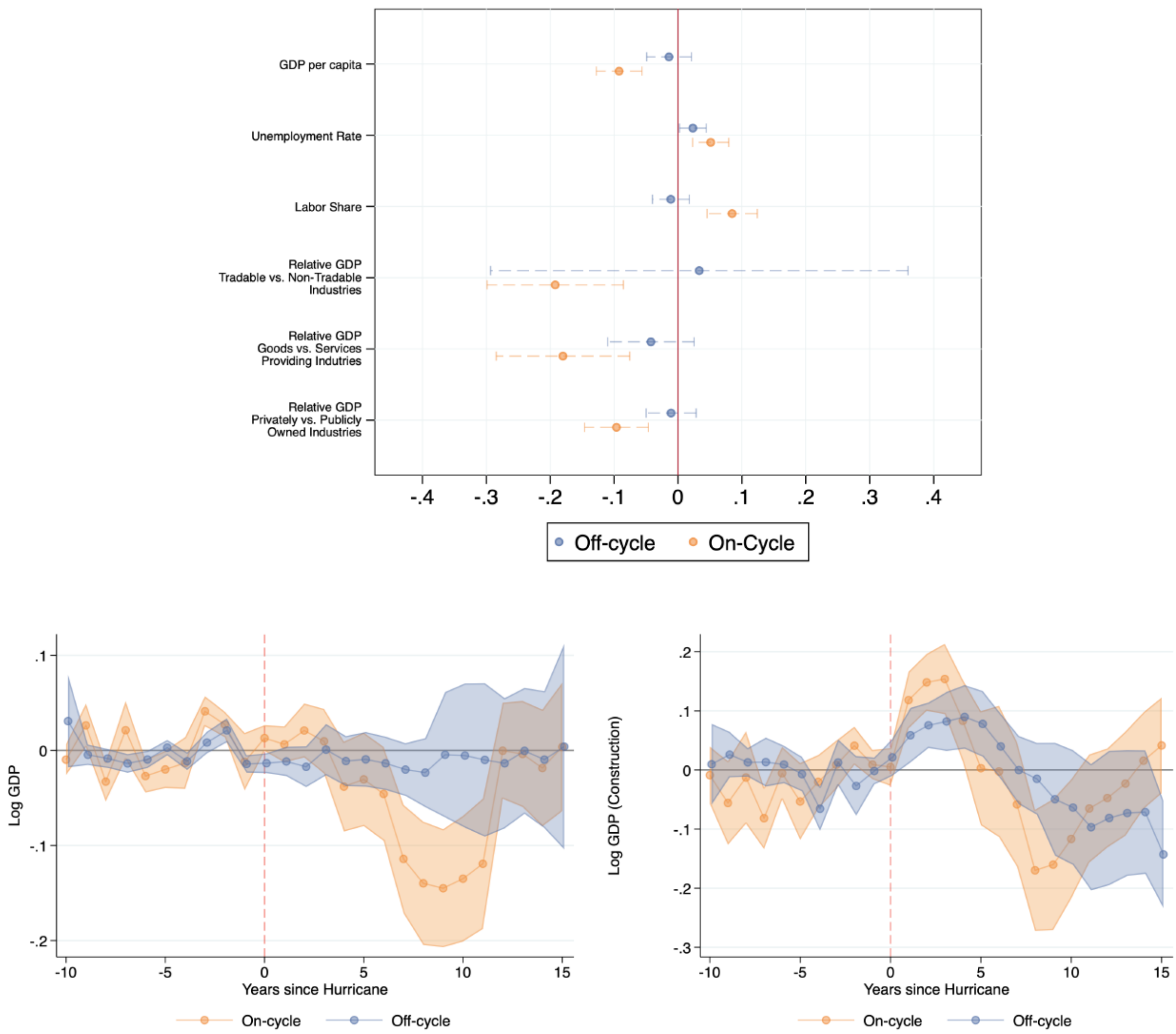


Figure A.16: On- vs. Off-Cycle Hurricane ATT on Local GDP

Notes: This figure plots ATT estimates and corresponding 95% confidence bands of different specifications of equation (1). The dependent variables on the y-axis are logged and aggregated at the county level. The comparison group includes the rest of the U.S. Event variables are dummies equal to 1 for a hurricane. The regression models include county and year fixed effects. Standard errors are clustered at the county level.

B.1 Derivations

The utility associated with net real income and amenities in location r is given by

$$
\begin{aligned}
u_t(r) =& a_t(r)\left(\frac{g_t(r)}{H(r)\bar{L}_t(r)^{\eta}}\right)^{\gamma} c_t(r)^{1-\gamma} \\
=& a_t(r)\left(\frac{(t_t(r)+\theta_t(r))w_t(r)\bar{L}_t(r)}{\bar{L}_t(r)^{\eta}P_t(r)}\right)^{\gamma}\left(\frac{(1-t_t(r))w_t(r)+R_t(r)/\bar{L}_t(r)}{P_t(r)}\right)^{1-\gamma} \\
=& \bar{a}_t(r)\bar{L}_t(r)^{-\tilde{\lambda}}\frac{w_t(r)}{P_t(r)}\Theta_t(r) \quad \text{with} \quad \tilde{\lambda} = \lambda - \gamma(1-\eta),
\end{aligned} \tag{2}
$$

where $\Theta_t(r) \equiv \left[(t_t(r)+\theta_t(r))^{\gamma}\left(\frac{\xi}{\mu\xi+\gamma_1} - t_t(r)\right)^{1-\gamma}\right]$ represents the combined public policy component. The price index is given by (12), and land markets are in equilibrium such that $R_t(r) = \left(\frac{\xi(1-\mu)-\gamma_1}{\mu\xi+\gamma_1}\right) w_t(r)\bar{L}_t(r)$ for all locations r and $a_t(r) = \bar{a}_t(r)\bar{L}_t(r)^{-\lambda}$.

We also know that the final good's price in place r at time t is determined by the average price of the various goods assembled in location r:

$$
\begin{aligned}
P_t(r) =& \left[\Gamma\left(\frac{-\rho}{(1-\rho)\theta}+1\right)\right]^{-\frac{1-\rho}{\rho}} \left[\int_S T_t(s)[mc_t(s)\zeta(r,s)]^{-\theta} ds\right]^{-\frac{1}{\theta}} \\
=& \bar{p}\chi_t(r)^{-\frac{1}{\theta}},
\end{aligned} \tag{3}
$$

with $mc_t(r) = [1/\mu]^{\mu}[\nu\xi/\gamma_1]^{1-\mu}[\frac{\gamma_1 R_t(r)}{w_t(r)\nu(\xi(1-\mu)-\gamma_1)}]^{(1-\mu)-(\gamma_1/\xi)} w_t(r)$,
and $\bar{p} = \left[\Gamma\left(\frac{-\rho}{(1-\rho)\theta}+1\right)\right]^{-\frac{1-\rho}{\rho}}$, where Γ denotes the gamma function.

Goods market clearing implies that total labor income in region r, $w_t(r)H(r)\bar{L}_t(r)$, must equal region r's total sales to all locations $s \in S$:

$$
\begin{aligned}
w_t(r)H(r)\bar{L}_t(r) =& \int_S X_t(s,r) ds \\
=& \int_S \pi_t(s,r)[(1+\theta_t(s))w_t(s)H(s)\bar{L}_t(s)] ds,
\end{aligned} \tag{4}
$$

where $X_t(s,r) = \pi_t(s,r)[(1+\theta_t(s))w_t(s)H(s)\bar{L}_t(s)]ds$ includes government transfers across regions. The probability density that an intermediate good produced in r is bought in s is given by

$$
\pi_t(s,r) = \frac{T_t(r)\left[mc_t(r)\zeta(r,s)\right]^{-\theta}}{\int_S T_t(u)\left[mc_t(u)\zeta(u,s)\right]^{-\theta} du} \quad \text{for all} \quad r,s \in S. \tag{5}
$$

Substituting (3) into (2), we obtain

$$u_t(r) = \bar{a}_t(r)\bar{L}_t(r)^{-\tilde{\lambda}}\Theta_t(r)\frac{w_t(r)}{\left[\int_S T_t(s)[mc_t(s)\zeta(r,s)]^{-\theta}ds\right]^{-\frac{1}{\theta}}\bar{p}}. \tag{6}$$

We can rewrite this as

$$\left(\frac{\bar{a}_t(r)}{u_t(r)}\right)^{-\theta}\bar{L}_t(r)^{\tilde{\lambda}\theta}\Theta_t(r)^{-\theta}w_t(r)^{-\theta} = \bar{p}^{-\theta}\int_S T_t(s)[mc_t(s)\zeta(r,s)]^{-\theta}ds,$$

from which we get the first set of equations that $u_t(\cdot)$, $\bar{L}_t(s)$, and $w_t(\cdot)$ have to solve

$$\left(\frac{\bar{a}_t(r)}{u_t(r)}\right)^{-\theta}\bar{L}_t(r)^{\tilde{\lambda}\theta}\Theta_t(r)^{-\theta}w_t(r)^{-\theta} = \kappa_1\int_S \tau_t(s)\zeta(r,s)^{-\theta}w_t(s)^{-\theta}\bar{L}_t(s)^{\alpha-(1-\mu-\gamma_1/\xi)\theta}ds, \tag{7}$$

where $\kappa_1 \equiv \left[\frac{\mu\xi+\gamma_1}{\xi}\right]^{[(\mu-\gamma)+\frac{\gamma_1}{\xi}]\theta}\mu^{\mu\theta}\left[\frac{\xi\nu}{\gamma_1}\right]^{-\frac{\gamma_1\theta}{\xi}}\bar{p}^{-\theta} = \kappa_2\cdot\bar{p}^{-\theta}$ and $T_t(s)mc_t(s)^{-\theta} = \tau_t(s)\bar{L}_t(s)^{\alpha-(1-\mu-\gamma_1/\xi)\theta}w_t(s)^{-\theta}\cdot\kappa_2$.

Inserting (5) and (3) into the goods market clearing condition (4), we get

$$w_t(r)H(r)\bar{L}_t(r) = \bar{p}^{-\theta}\int_S T_t(s)[mc_t(s)\zeta(r,s)]^{-\theta}P_t(s)^{\theta}(1+\theta_t(s))w_t(s)H(s)\bar{L}_t(s)ds.$$

Substituting (2), and employing $P_t(s) = \left(\frac{\bar{a}_t(s)}{u_t(s)}\right)\bar{L}_t(s)^{-\tilde{\lambda}}w_t(s)\Theta_t(s)$, symmetric trade costs $\zeta(r,s) = \zeta(s,r)$, and $T_t(r) = \tau_t(r)\bar{L}_t(s)^{\alpha}$ yields the second set of equations that $u_t(\cdot)$, $\bar{L}_t(s)$, and $w_t(\cdot)$ have to solve:

$$\tau_t(r)^{-1}w_t(r)^{1+\theta}H(r)\bar{L}_t(r)^{1-\alpha+(1-\mu-\gamma_1/\xi)\theta} = \kappa_1\int_S\left(\frac{\bar{a}_t(s)}{u_t(s)}\right)^{\theta}\zeta(r,s)^{-\theta}\Theta_t(s)^{\theta} \times (1+\theta_t(s))w_t(s)^{1+\theta}H(s)\bar{L}_t(s)^{1-\tilde{\lambda}}ds. \tag{8}$$

The third set of equations that $u_t(\cdot)$, $\bar{L}_t(s)$, and $w_t(\cdot)$ have to solve is given by (5). While $\tau_t(\cdot)$ comes directly from (7) and $\bar{L}_{t-1}(\cdot)$, $a_t(\cdot)$ comes from (3), and $\theta_t(\cdot)$ comes from (13).

B.2 Quantification

In this subsection of the Appendix, we discuss the data used for the quantitative analysis of the model. We provide further information on how we recover the fundamentals and utility levels $u_t(r)$, for every county from our general equilibrium model. We discuss the effect of natural disasters and the electoral cycle on amenities, productivity, tax, and transfer rates. Finally, we assess the model's in-sample performance.

B.2.1 Data Sources and Construction

Geographical land area. $H(\cdot)$ come from the 2010 U.S. Gazetteer Files.

Wages. $w_0(\cdot)$ is measured as GDP per capita from the Bureau of Economic Analysis (BEA) website, under Regional Data, Economic Profiles for all U.S. counties. Wages are normalized to have a mean of one.

Total population. $\bar{L}_0(\cdot)$ is calculated by using data on the population distribution for each year between 2001 and 2019 from the U.S. Census and Surveillance Epidemiology and End Results (SEER) population database for all U.S. counties. For each year, we transform the population into population per unit of land.

Tax and transfer rates. Critical ingredients for the quantification of our model are tax ($t_0(\cdot)$) and transfer rates ($\theta_0(\cdot)$) for each county between 2001 and 2019 in the United States. To the best of our knowledge, we are unaware of any attempt to explore how government redistribution affects local fiscal resources while looking at such fine geographical units. Hence, using a similar methodology as Piketty et al. (2017), we compute the total collected taxes before and total spending after redistribution for each county in the United States from 2001 to 2019. In combining tax, expenditure, and public finance data and expressing total tax collected and total local spending as a percentage of local GDP, we then calculate tax ($t_0(\cdot)$) and transfer rates ($\theta_0(\cdot)$) for each county between 2001 and 2019 in the United States. Table B.1 summarizes all the data sources we use to compute local total revenue and spending.

To deliver a complete picture of the distribution of fiscal resources across counties, we incorporate all levels of government in our tax and transfer rates calculations. Specifically, we separate taxes and spending at the federal and state from the county level. We assign all tax revenues and all forms of government spending (including federal, state, and local taxes) to the local level under specific assumptions explained in more detail below. We normalize so that both local tax revenues before and local spending after redistribution add to aggregate spending. Specifically, we scale collected revenues so that aggregate revenues equal aggregate spending. Between 2001 and 2019, the normalizing ratio of aggregate spending to revenues is almost equivalent to 1. It equals 1.04, on average, with a standard deviation of 0.05. Then, we subtract all collected tax revenues from total local spending to calculate total transfers per county.

We start by collecting tax revenues by state and local governments reported by the Government Finance Database (Pierson et al., 2015), which provides information on government revenues from the U.S. Census Bureau's Census of Governments and the Annual Survey of State and Local Government Finances collected by the federal, state, and local governments. We supplement this dataset using various information at the federal and state level from the BEA, the White House Historical Tables, and the Federal Reserve Bank of St. Louis FRED. In particular, we include total outstanding debt, federal and state corporate taxes, and other federal and state taxes. No specific local information is available that comprises all collected income taxes from federal, state, and local governments, and therefore we instead rely on the NBER TAXSIM model to derive the federal and state income taxes for the average taxpayer by county. Hence, we assume that the average taxpayer pays individual income taxes in each county. As input variables for the NBER TAXSIM model, we use the information on marital status, number of underage dependent members in the household, wage, dividends, rents, social security compensations, other types of transfers to individuals, property tax, and mortgage value of the average household in each county. To calculate the total collected income taxes, we multiply this average amount of income taxes by the number of people over 18 years old in each county.

Corporate taxes are reported at the federal, state, and local levels. We allocate federal and state corporate tax revenues to counties according to their local GDP shares. For the remaining taxes, we proceed similarly (such as property taxes, sales taxes, and excise taxes). For states, we compute the difference (total taxes collected minus individual taxes minus corporate taxes) as we do not have a specific "other taxes" measure here. At the federal level, we compute the sum of "excise" and other taxes. For all federal, state, and county levels, we compute the individuals' "after-tax" income (i.e., gross income minus individual taxes at all levels minus social security contributions). We take the ratio (remaining taxes/after-tax income) at the federal and state levels and multiply it by local (county) after-tax income. As a result, our calculation of the total collected revenues captures the operation of the social security and tax system before redistribution. It includes all individual, corporate, and other taxes collected by local, state, and federal authorities, which we allocate to counties and aggregate into one measure. Moreover, we account for all employer and employee social security contributions and all intergovernmental (IG) transfers from local governments.

Government revenue is usually less than government expenditure. To match aggregate spending, we also include the net debt contracted (i.e., the debt issued net of the interest paid on outstanding debt) by local, state, and federal authorities. Higher government debt can mean higher taxes, lower government transfers, or both in the future. So to allocate the net debt to the current generation of U.S. residents, we increase the total collected taxes in each county proportionally to local GDP. This procedure implies that we assume that economic output is the primary collateral that allows for debt contraction. Alternatively, like Piketty et al. (2017), we could assume that any government debt translates into raised taxes and lowered public spending. In this sense, we would allocate 50% of the net debt in proportion to total collected tax revenues and 50% in proportion to total local spending. This alternative allocation rule would still be progressive but effectively lead to slightly lower average tax rates yet higher average spending rates.

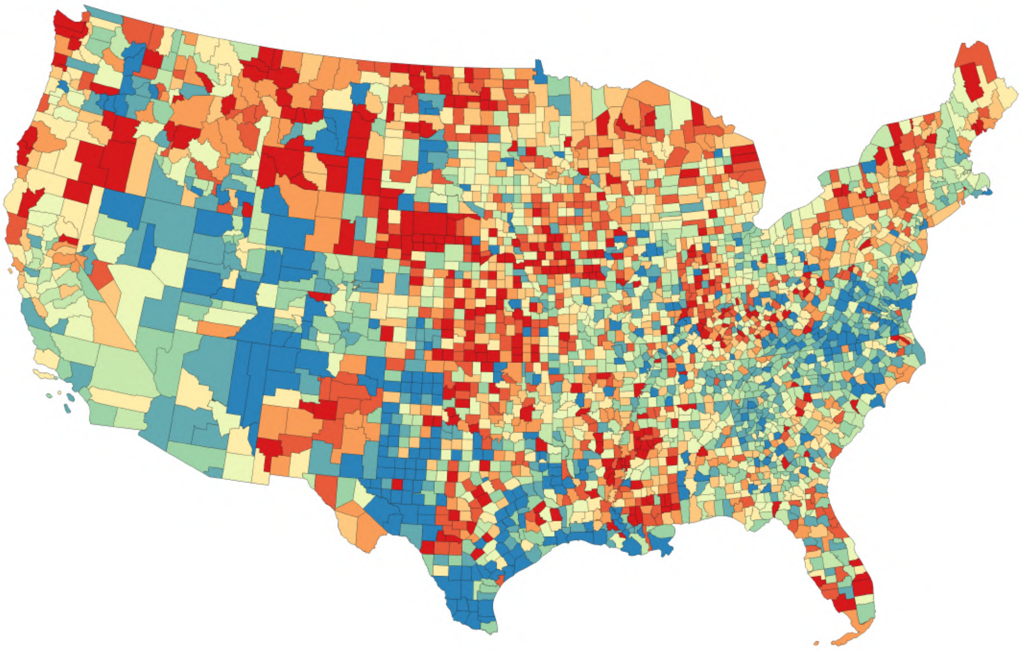
In calculating total local public spending, we use the BEA measure of local public goods and services (i.e., the government output). To assess the value of these non-marketable assets, the BEA measures input payments on labor, intermediate goods and services, and investment expenditures by federal, state, and local governments. The consumption expenditures and gross investments from federal and state governments are allocated to counties based on employment, GDP, net electricity generation, and wages and salaries.[20] We further account for intergovernmental transfers to local governments and social transfers to individuals (such as unemployment benefits, Medicare and Medicaid, veterans support, and pensions). Moreover, we add refundable tax credits and compensation for the local administration. Overall, our measure of total local spending captures the total income available for public spending.

Finally, we calculate the average tax rate as the ratio of the total tax collected locally to the local GDP. Again, the average tax rates we compute account for all sources of tax revenues (individual, corporate, and other taxes) collected by local, state, and federal authorities. They also account for all employer and employee social security contributions, intergovernmental transfers, and net debt contracted by local, state, and federal authorities. The transfer rate is calculated as the difference between the spending rate (i.e., the ratio of total spending in local areas to local GDP) and the tax rate.[21] Both measures are winsorized at the 0.5% level to account for a few outliers.

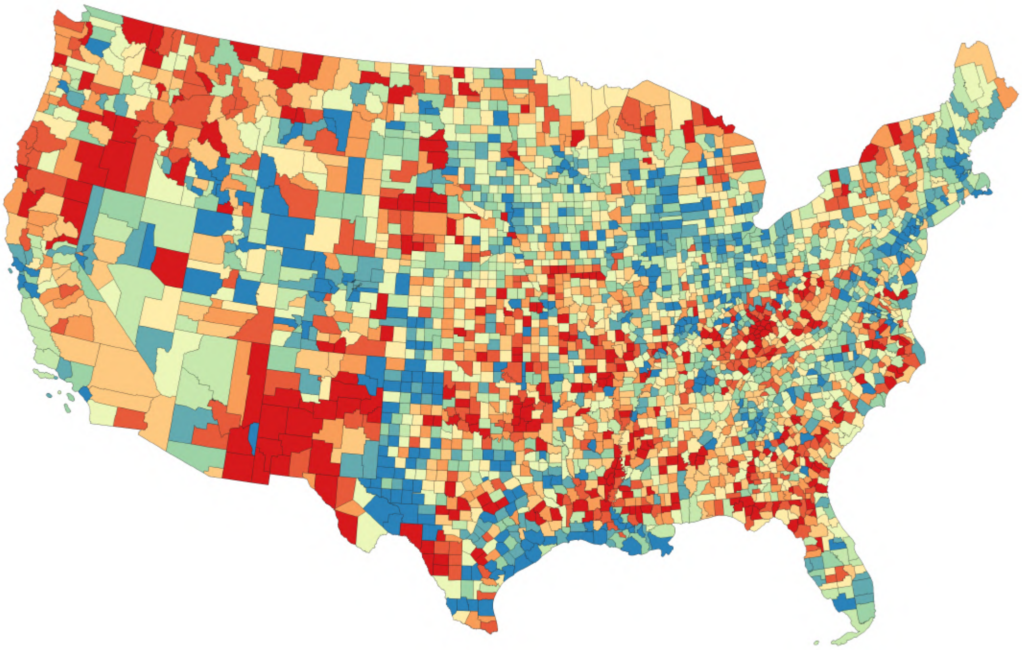
Figure B.1 shows how tax rates (Panel (a)) and spending rates (Panel (b)) vary across counties in 2001. The tax rate was lower in Arizona, Nevada, Virginia, and some Texan counties. Spending rates were lower along New England's coast, California's coast (between San Francisco and Los Angeles),

[20] See the BEA's definition for a detailed description of the methodology.

[21] One could argue that intergovernmental transfers to or from local governments are systematically earmarked for specific use. In that case, including intergovernmental transfers in our fiscal redistribution measures might lead to some double counting. We calculate the alternative tax and transfer rates net of such intergovernmental transfers. These latter highly correlate with our main measures: 98.71% for transfer rates and 99.02% for tax rates.



(a) Tax Rates



(b) Spending Rates

Figure B.1: Calculated Tax and Spending Rates

Notes: This figure plots the tax rate (Panel (a)) and spending rate (Panel (b)) for our baseline year 2001. The warm colors indicate higher deciles.

Texas, and Louisiana's Gulf Coast, and, in general, in urbanized counties.

Changes in tax and transfer rates affected the dynamic of total income available for public spending. Figure B.2 shows how the average tax and expenditure rates vary over time. For the United States, the average tax rate increased slightly from 39% in 2001 to 43% in 2019. At the same time, the expenditure rates increased from about 55% of local GDP in 2001 to close to 60% in 2019. During the Great Recession, tax credits in the context of the Affordable Care Act increased expenditure rates. Higher transfers mainly financed them through the Economic Stimulus Payments in 2008, the American Opportunity Tax Credit, and the Making Work Pay Tax Credit (as documented in Piketty et al., 2017).

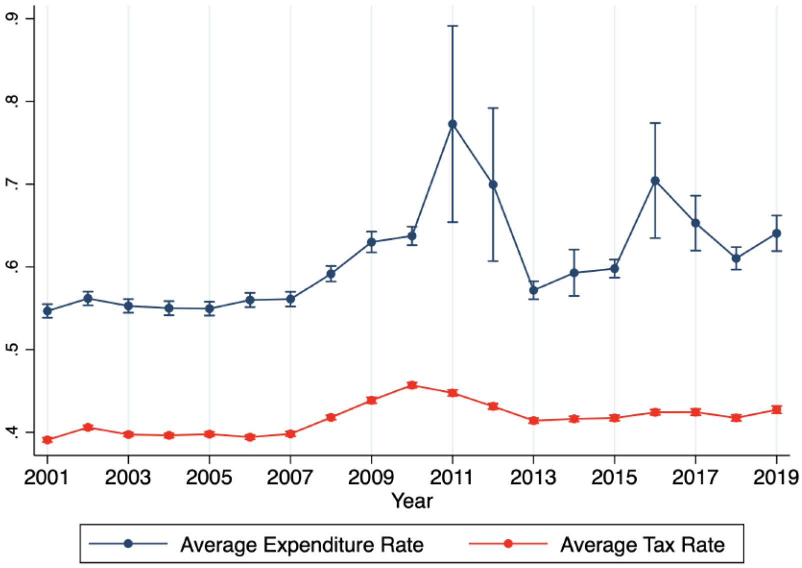


Figure B.2: Average Tax and Expenditure Rates

Notes: This figure depicts average tax and expenditure rates at the county level between 2001 and 2019 in the United States.

Trade costs. $\zeta(\cdot, \cdot)$ are constructed from detailed geospatial data on rail, road, and water networks. Geographic data on the road network come from the National Highway Planning Network (NHPN), and railroad network data come from the Federal Railroad Administration (FRA) and the Bureau of Transportation Statistics (BTS). Geospatial data on the water network come from the U.S. Army Corps of Engineers Geospatial Center and the BTS. We calculate instantaneous trade costs between every county in the U.S. using the "fast marching method" algorithm based on the cost parameters as assigned in (Allen and Arkolakis, 2014).

Table B.1: Data Sources to Compute Local Tax and Transfer Rates

Category	Source	Note
Total Collected Revenues:		
1) Tax Revenues		
a) Individual		
Federal	NBER TAXSIM model	Accounts for the average marriage status, number of underage dependent members in the household, wage, dividends, rents, social security compensations, other types of transfers to individuals, property tax, and mortgage value.
State	NBER TAXSIM model	Accounts for the average marriage status, number of underage dependent members in the household, wage, dividends, rents, social security compensations, other types of transfers to individuals, property tax, and mortgage value.
Local	Census of Governments	Extracted from The Government Finance Database – General Revenues Own-Sources (R04)
b) Corporate		
Federal	The White House Historical Tables	Table 2.1 – Local Corporate tax revenue is the product of federal corporate tax rate (Total Federal Corporate Revenues/Total GDP) to local GDP
State	Census of Governments	Extracted from The Government Finance Database – Computed as the product of state corporate tax rate (Total State Corporate Revenues/Total GDP) to local GDP
Local	Census of Governments	Extracted from The Government Finance Database – General Revenues Own-Sources (R04)
c) Other		
Federal	The White House Historical Tables	Table 2.1 – Local Corporate tax revenue is the product of federal other tax rate (Total Federal Other Revenues/Total GDP) to local GDP
State	Census of Governments	Extracted from The Government Finance Database – Computed as the product of state corporate tax rate (Total State Corporate Revenues/Total GDP) to local GDP
Local	Census of Governments	Extracted from The Government Finance Database – General Revenues Own-Sources (E002)
2) Social Security	Bureau of Economic Analysis (BEA)	Dataset CAINC4, variable 61 & 37
3) IG Transfers	Census of Governments	Extracted from The Government Finance Database – Total IG Expenditures (E02)
4) Net Debt		
Federal	St. Louis FRED	First difference in Total Debt Outstanding net of Total Interests on Debt weighted by local GDP share
State	Census of Governments	Extracted from The Government Finance Database – First difference in Total Debt Outstanding (D01) net of Total Interests on Debt (E010) weighted by local GDP share
Local	Census of Governments	Extracted from The Government Finance Database – First difference in Total Debt Outstanding (D01) net of Total Interests on Debt (E010)
Total Spendings:		
1) Local public goods	Bureau of Economic Analysis (BEA)	Dataset CAINC2, variable 83
2) IG Transfers	Census of Governments	Extracted from The Government Finance Database – Total IG Revenues (R31)
3) Individual Transfers	Bureau of Economic Analysis (BEA)	Dataset CAINC35, variable 2000
4) Tax credits	Bureau of Economic Analysis (BEA)	Dataset CAINC4, variable 5000
5) Local Administration		
Federal	Bureau of Economic Analysis (BEA)	Dataset CAINC6n, variable 2001 & 2002
State	Bureau of Economic Analysis (BEA)	Dataset CAINC6n, variable 2011
Local	Bureau of Economic Analysis (BEA)	Dataset CAINC6n, variable 2012

Migration costs. To measure $m_2(r)$—the time-invariant, exogenous migration costs—we build a balanced origin-destination matrix for the years 1990–2018 using the IRS's county-to-county migration data. For each county, we proxy the number of migrants with the number of exemptions in the tax returns. We then regress the log number of migrants, coming or leaving, on origin-destination fixed effects.

In our main estimation, we employ an inverse hyperbolic sine transformation to account for the absence of bilateral migrations flows (Bellemare and Wichman, 2020). However, we acknowledge the difficulty of identifying average treatment effects as percentage effects ignoring the baseline units (Chen and Roth, 2022; Mullahy and Norton, 2022). Nonetheless, regressing untransformed migration flows or shares of migration flows to local populations on origin-destination fixed effects would not unskew our main outcome variable. We are still interested in capturing the decreasing returns of migration costs. In this case, Chen and Roth (2022) suggest explicitly taking a stand on how we value the relative importance of the extensive margin relative to the intensive margin.

In the present case, in which we want to relate migration patterns to origin-destination fixed effects, assessing the relative importance of each margin is a challenging exercise.[22] However, we follow Chen and Roth (2022) in explicitly defining two alternative measures for $m_2(r)$. In the first one, we crudely set $log(y)$ if $y > 0$ and $log(y) = 1$ if $y = 0$. In the second one, we replace $log(y)$ with the negative of the percentage change between the average number of non-migrants and the average number of migrants if $y = 0$. Both alternative measures produce $m_2(r)$ distributions that highly correlate with our main specification—99.99% and 99.32%, respectively. These alternative measures also yield geographic distributions similar to the one in Panel (b) of Figure3.

We use origin-destination fixed effects as our goal is to recover the impact of time-invariant factors between counties of origin and destination. These fixed effects will capture the influence of exogenous, first-nature elements (such as geographic distance, topography, and average temperature differences) and historical connections on mobility. Larger values of the origin-destination fixed effects imply smaller migration costs.

We then derive the weighted mean of these fixed effects by county of origin (respectively, destination) weighting by area of the county of destination (respectively, origin), after normalizing them relative to staying in the same county. Finally, we take the additive inverse and normalize this vector to have a minimum of 1 (see equation (5)) to capture exogenous migration costs. This procedure

[22]One could argue that migration patterns inside a country relate mostly to how personal endowments (such as education, social ties, and income) dominate migration costs. In absolute, there are no migration barriers inside a country (e.g., as a formal border) that would systematically prevent migration flows.

Table B.2: $m_2(r)$ Correlations with Decadal Net Migration Rates

	Average (1950-2010)	1950	1960	1970	1980	1990	2000
$\rho(m_2(r))$	-0.4257	-0.5141	-0.4046	-0.2199	-0.3338	-0.1587	-0.2047

Notes: This table displays the recovered $m_2(r)$ migration costs with the decadal net migration rates measured in Winkler et al. (2013). The net migration data were retrieved from the Net Migration for U.S. Counties project.

leaves us with a single, county-specific, normalized migration costs measure for both migration inflows and outflows. As shown in Figure B.3, these two measures have the merit of being almost perfectly symmetric (correlation of 0.9847). Taken together, this supports Desmet et al. (2018)'s assumption of symmetric mobility costs in simplifying the dynamic mobility decisions to a sequence of static decisions.

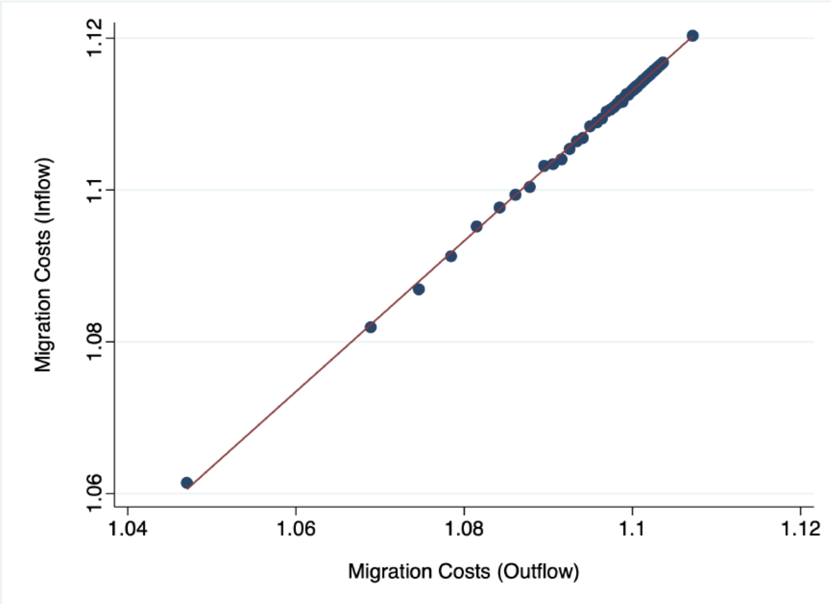


Figure B.3: Symmetry of Migration Costs

We set $m_2(r)$ to correspond to the recovered migration inflow costs. Using data from Winkler et al. (2013), we show in Table B.2 that these migration costs correlate negatively with net migration rates (i.e., migration inflow rates net of migration outflow rates). As expected, on average, the larger the migration costs, the lower the net migration rates. Note that the correlation's sign is the same for all decades since 1950 but the amplitude of the correlation varies across time. Correlations between $m_2(r)$ and net migration rates have been smaller in recent decades. This pattern indicates that time-invariant components between locations, such as Euclidean distance, may have become less stringent for migration flows over time.

Synthetic hurricane paths. To calculate synthetic hurricane paths for the years 2001–2081 in the United States, we use the STORM dataset (Bloemendaal et al., 2020). The data use historical data from the IBTrACS repository to predict more than 10,000 hurricane synthetic tracks and corresponds to current climate conditions. Figures B.4 and B.5 show examples of the distribution of on- and off-cycle hurricanes in this dataset.

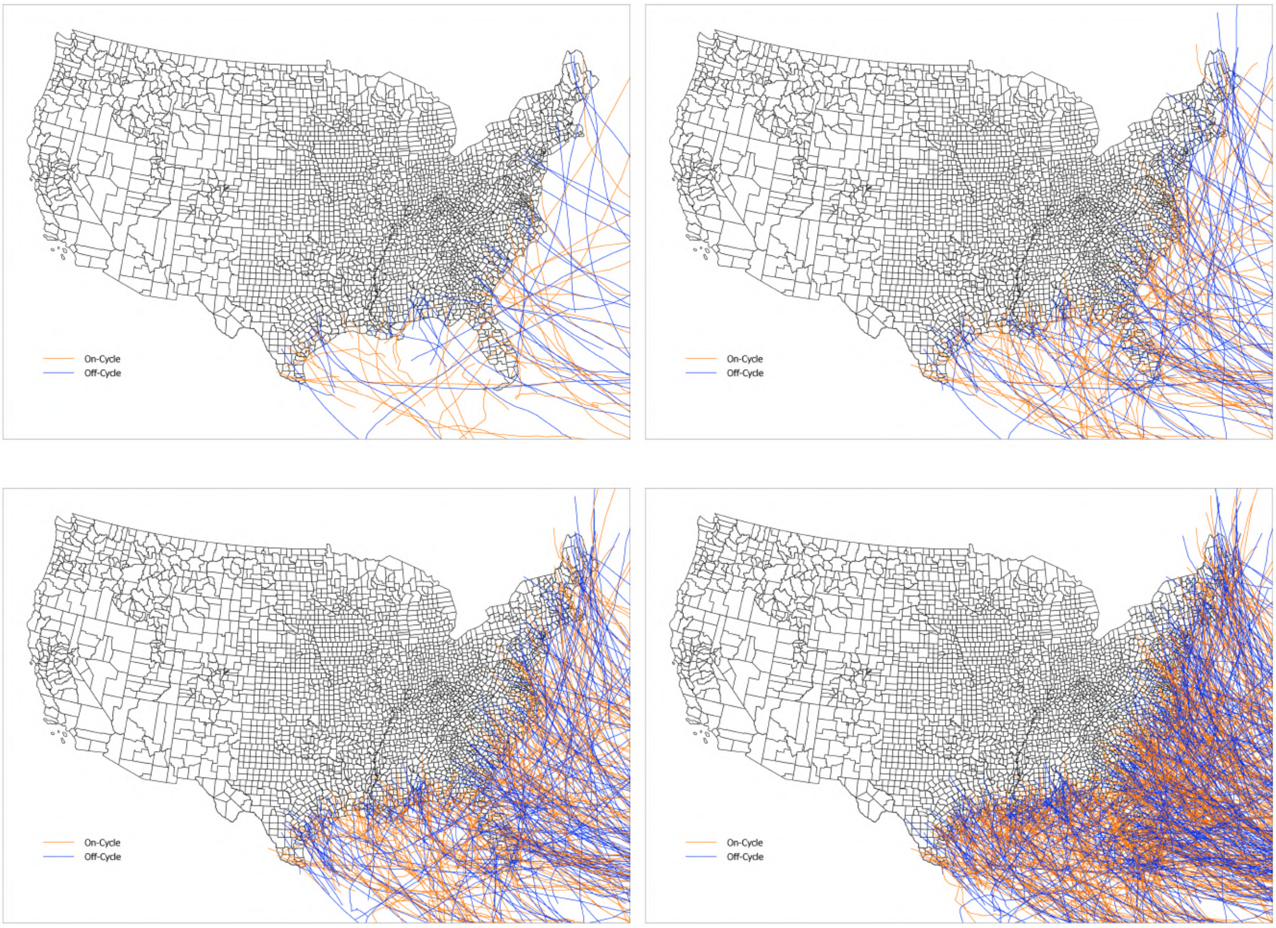


Figure B.4: ON- VS. OFF-CYCLE SYNTHETIC HURRICANE TRACKS OVER 100, 250, 500, AND 1,000 YEARS

B.2.2 Amenities, Productivities, and Utility Levels

In estimating migration costs directly from observed migration patterns and recovering utility levels $u_t(r)$ from the structure of the model, our approach deviates from the approach in the literature so far. For example, Desmet et al. (2018, 2021) use data on subjective well-being from the Gallup World Poll, and Cruz and Rossi-Hansberg (2021) use data on the Human Development Index to recover $u_t(r)$. Our alternative method has two advantages. First, it limits the degrees of freedom in inferring $u_t(r)$ at the local level and is more robust to measurement error. Second, by using an external dataset, we

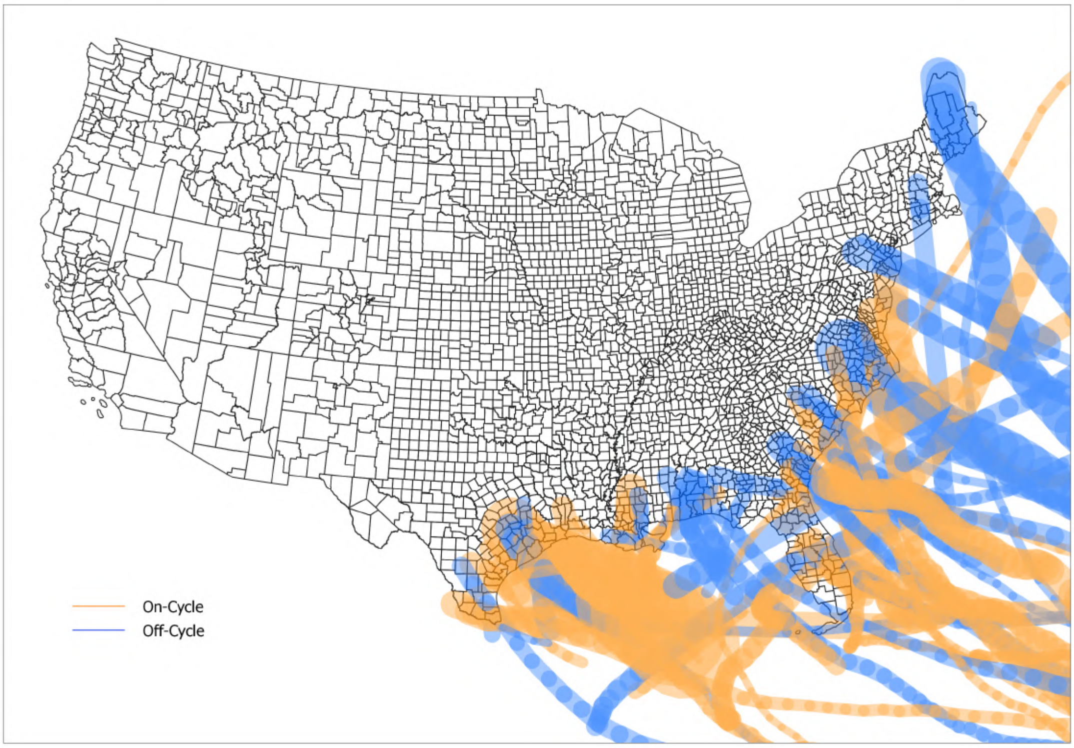


Figure B.5: On- vs. Off-Cycle Synthetic Hurricane Radius of Maximum Winds over 80 Years

ensure not to use the same information (e.g., local GDP) twice in measuring $u_t(r)$.

To calculate a measure of subjective well-being, we break down national first-moment statistics from the Gallup World Poll into county-level estimates. We follow the literature on the relationship between subjective life satisfaction and income (Frey and Stutzer, 2002; Kahneman and Deaton, 2010; Deaton and Stone, 2013) showing that life satisfaction positively correlates with relative income within a cohort. We compute county-level human development indices using data on local GDP from the BEA, life expectancy from Dwyer-Lindgren et al. (2017, 2022), and education spending per individual under 18 years old from the Census of Governments. We then apply the Human Development Index methodology to these three dimensions to retrieve local county estimates.

Figure B.6 shows that despite using an alternative strategy, our recovered measure of $u_t(r)$ positively correlates with subjective well-being ($\rho = 0.2819$) as in Desmet et al. (2018, 2021) and with the Local Human Development Index ($\rho = 0.2325$) as in Cruz and Rossi-Hansberg (2021).

B.2.3 Impact of Natural Disasters and Election Cycle

Given the procedure described in Section 6.4, Figure B.7 documents the impact of natural disasters and the electoral cycle on amenities, productivity, tax, and transfer rates.

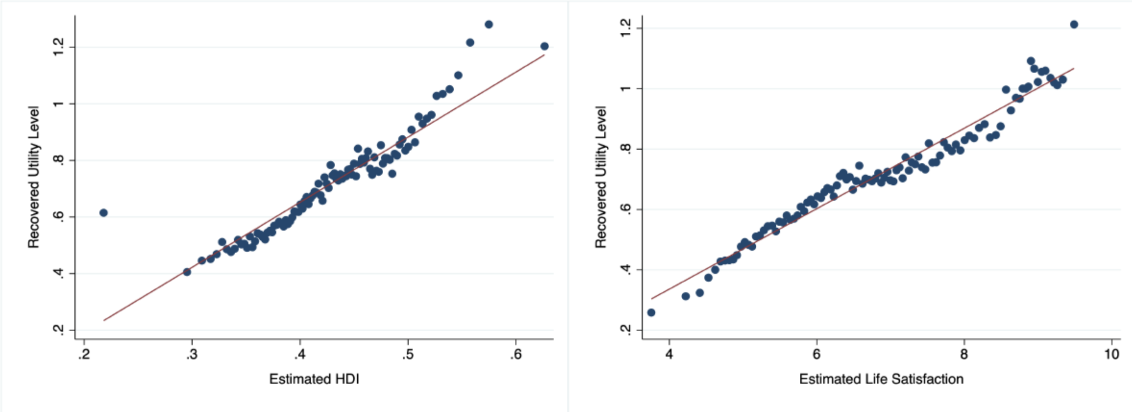


Figure B.6: Comparison of $u_t(r)$ Levels with Local Human Development Index and Life Satisfaction Levels

Notes: This figure plots the correlation between the recovered $u_t(r)$ from Section 6.3 and the local human development index (left) and life satisfaction (right) levels. Each bin corresponds to a percentile. The correlation between $u_t(r)$ and the human development index is 0.2325, and the correlation between $u_t(r)$ and the life satisfaction levels is 0.2819.

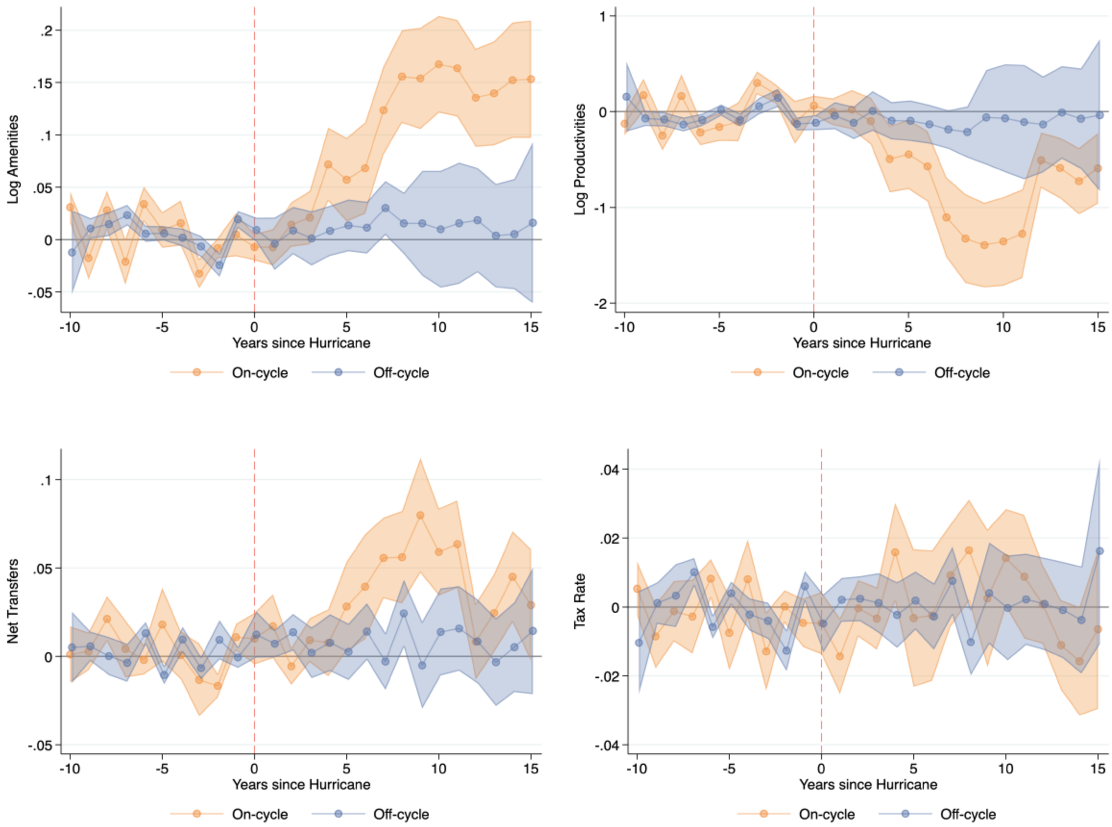


Figure B.7: On- vs. Off-Cycle Hurricane Treatment Effect on Fundamental Amenities, Productivities, Transfer, and Tax Rates

Notes: This figure plots the estimates of the event study and corresponding 95% confidence bands of different specifications of equation (1). The dependent variables are the log of fundamental amenities, the log of fundamental productivities, and transfer and tax rates. Event variables are dummies equal to 1 for a hurricane. The regression model includes county and year fixed effects. Standard errors are clustered at the county level.

B.2.4 In-Sample Performance

Figure B.8 shows that our quantified model reproduces the population and wage dynamics between 2001 and 2019 using our model-implied impact functions.

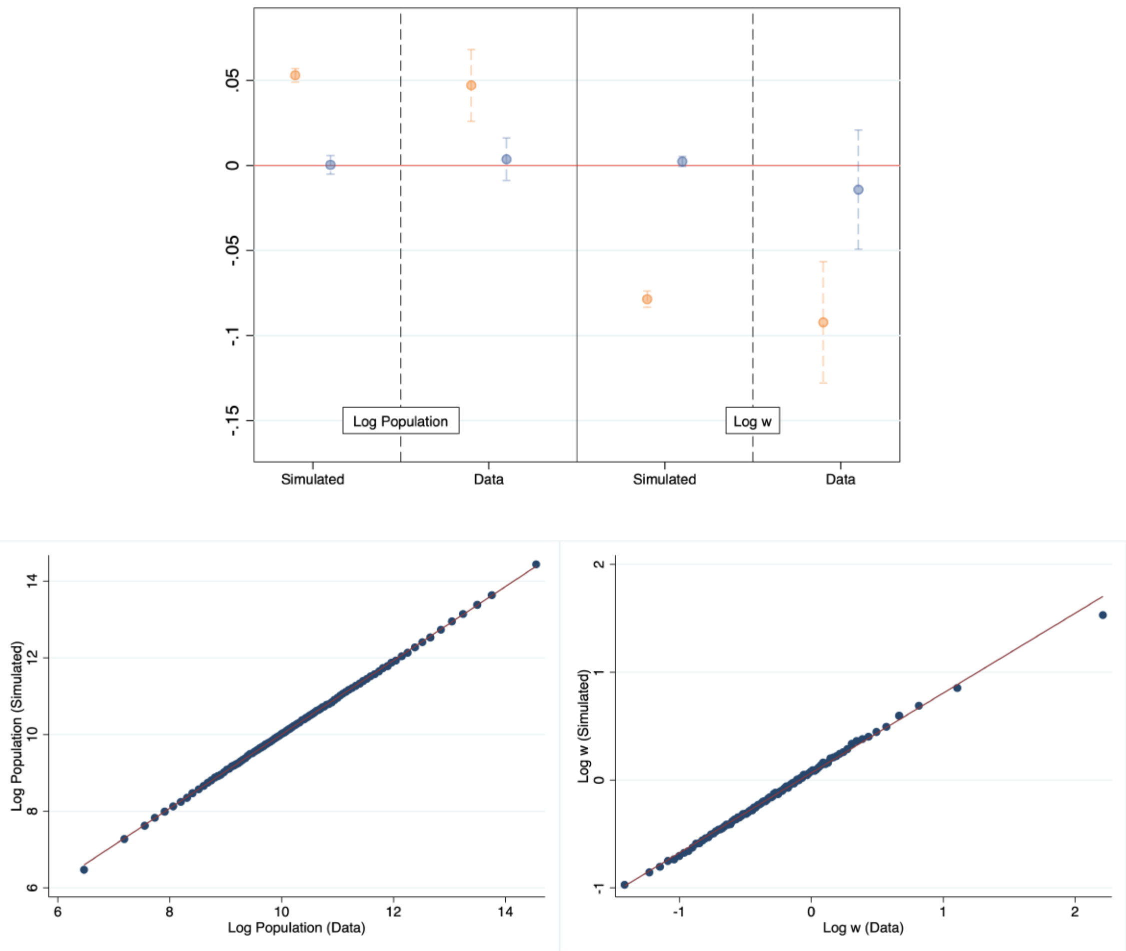


Figure B.8: In-Sample Performance

Notes: The upper figure plots our main estimates of the impact of on- and off-cycle hurricanes on population and wages, both in the baseline simulation and in the data. Each average treatment effect in the data is insignificantly different from its simulation counterpart. The bottom figures plot the correlation between predicted and actual population (left) and wages (right) over the 2001–2019 period. Each bin corresponds to a percentile. The correlation between the predicted and actual population levels is 0.997, and the correlation between predicted and actual wages is 0.869.

B.3 Counterfactual Analysis

In this part of the appendix, we report sensitivity checks for our counterfactual analysis from Section 7 of the paper. Further, we show that our structural model implies a more critical role for post-disaster policies in shaping the aggregate economy than suggested by a simple back-of-the-envelope calculation. Finally, we highlight the importance of the amenity and productivity channel for our results. As in Section 7, we contrast the current electoral-cycle-driven post-disaster policy with a counterfactual scenario without these impacts. However, in two alternative baseline scenarios, we now remove the policy's post-disaster transfers' transformation rate into local amenities or their impact on local productivity.

B.3.1 Sensitivity Checks

To assess the sensitivity of our results, we also report in Table B.3 aggregate effects for alternative parameter constellations of migration and trade elasticities. While remaining inside the parameter range where we find an equilibrium with our numerical approach, we slightly increase the migration elasticity ($\Omega = 0.4$ and $\Omega = 0.45$).[23] Further, we allow θ to take the 8.3 value estimated by Eaton and Kortum (2002) and the 4.6 value estimated by Simonovska and Waugh (2014). It is evident from Table B.3 that aggregate output declines in *all* cases regardless of the precise parameter constellation. The loss tends to be stronger the smaller Ω is. More negligible levels of Ω imply weaker congestion forces and thus larger migration flows. Those migration flows from more to less productive regions, in turn, translate into lower aggregate productivity. This channel becomes even more paramount when the trade elasticity θ is lower.

Table B.3: Sensitivity: Aggregate Effects under Different Parameter Settings

θ	Ω	Δ *Welfare* *in percent*	Δ Real GDP *in percent*	Δ *Population of On-Cycle counties* *in percent*
4.6	0.5	0.16	-1.09	9.56
6.5	0.5	0.17	-1.17	13.06
8.3	0.5	0.18	-1.21	15.36
6.5	0.4	0.16	-1.18	15.37
6.5	0.45	0.17	-1.17	14.12

Notes: This table reports percentage changes in the present discounted value of welfare and real GDP and the percentage change in the population of on-cycle regions under different parameter values of θ and Ω given the current post-disaster policies compared to the counterfactual scenario.

B.3.2 A Back-of-the-Envelope Calculation

We perform a simple back-of-the-envelope exercise that indicates the size of the post-disaster transfers allocated to counties hit by on-cycle hurricanes between 2001 and 2019. Doing so allows us to calculate

[23] Our baseline quantification satisfies Condition 1 stated above. It assumes higher static dispersion forces than agglomeration forces at the margin and ensures the existence and uniqueness of equilibrium. Given our baseline parameterization, the county-level estimates of migration elasticities $1/\Omega = 2$ for the United States of Monte et al. (2018) provide an upper bound, where we find an equilibrium with our numerical procedure.

the extent of transferring tax revenues across regions during the electoral cycle. The present analysis ignores the causal population sorting impact and its implied spillovers on the aggregate government budget (e.g., tax revenue). In the main text, we extend this back-of-the-envelope exercise by conducting counterfactual exercises with a dynamic spatial general equilibrium model to overcome the limitations of this simple calculation.

Our estimates of Table 1 show that transfer rates increase by an average of 3.12 percentage points after an on-cycle hurricane. At the same time, we cannot rule out the absence of a response in the off-cycle case. Given the same post-disaster treatment across the electoral cycle, holding prices, and population fixed, we should see 3.12 percentage point less post-disaster transfers allocated during on-cycle years. We can take the difference between the actual post-disaster transfers and this counterfactual with equal post-disaster assistance during on- and off-cycle years. For each county hit by an on-cycle hurricane during 2001–2019, we then arrive at the implied redistribution of post-disaster transfers arising from the electoral-cycle-driven post-disaster policy. With this information, we calculate the total amount of tax revenue redistribution to regions affected by on-cycle hurricanes. This estimate suggests that the policy in the United States resulted in direct transfers of approximately $289 billion (in 2001 dollar values) across regions (or $53.68 in average annual post-disaster transfers) between 2001 and 2019.

This back-of-the-envelope calculation, however, has several important caveats. First, it only measures the direct redistribution that arises from the higher post-disaster transfers during on-cycle years. It does not capture the general equilibrium effects of natural disasters and post-disaster policies that affect economic activity locally and elsewhere. The post-disaster policy increases local public goods provision and net-of-tax earnings by providing higher disaster transfers during on-cycle years despite similar disaster exposures, thus providing further welfare gains to the local population in regions hit by a natural disaster during on-cycle years.

Second, the calculation takes the existing spatial distribution of economic activity as given and may overstate some consequences of eliminating the higher post-disaster transfers during on-cycle years. Finally, dynamic growth effects are associated with changes in the spatial distribution of economic activity in response to post-disaster transfers, and therefore focusing on the impact of the electoral cycle on post-disaster transfers alone understates the aggregate consequences of the post-disaster policy. Addressing these issues requires moving to a dynamic spatial model with post-disaster policies, which we do in the main text. Ultimately, our structural model implies a more critical role for post-disaster policies in shaping aggregate welfare than the $53.68 billion suggested by the back-of-the-envelope calculation.

B.3.3 Amenity Transformation and Productivity Distortion

The transformation of amenities and productivity distortion channels are essential for population sorting and aggregate dynamics. We run two alternative baseline scenarios to help us understand the respective importance of each channel. In the first alternative scenario, we simulate the population sorting patterns and the aggregate effects of removing the post-disaster transfers' transformation rate into local amenities. More specifically, we set the parameter $\varphi_{\bar{a}_t(r)}$ to zero and adjust the fundamental local amenity term to keep the value unchanged from 2001. Intuitively, in this alternative baseline scenario, local governments cannot use the higher post-disaster transfers to significantly improve infrastructures and thus enhance the local quality of life.

In the second additional scenario, we keep post-disaster transfers and their transformation rate into local amenities as in the baseline model. However, we modify the extent of productivity effects in the adjustment process of the local economy. Specifically, we reduce the impact of electoral-cycle-driven post-disaster policies on local productivity from -38% to 0%. Intuitively, in this second alternative baseline scenario, the post-disaster policies do not distort the local economy. We then compare these alternative baseline experiments to our main counterfactual scenario in which the impact of natural disasters on fiscal transfers, amenities, and productivity are identical across the electoral cycle.

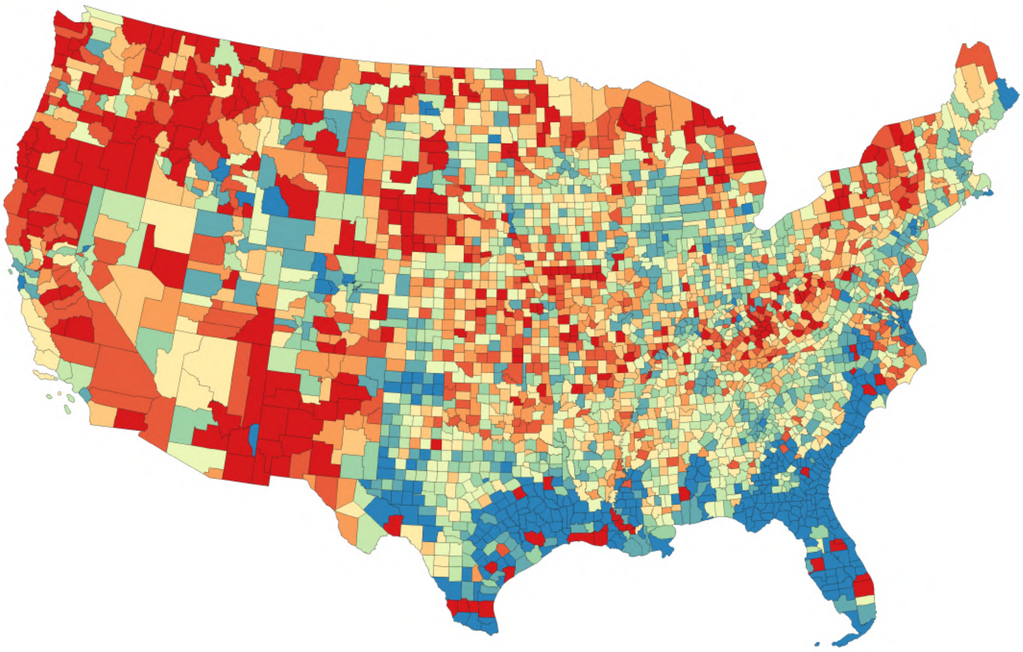
We first depict ratios in local population size after 80 years in our baseline scenario without the amenity transformation after an on-cycle hurricane (Panel (a) of Figure B.9) and their aggregate effects on productivity, GDP, and welfare (Panel (b)) relative to our counterfactual without any electoral cycle effect. Compared to our main counterfactual scenario, the population declines in exposed coastal areas. Despite increased fiscal transfers and reduced congestion, no exogenous improvements made in amenities could compensate for the significant local productivity loss (-37.73%, on average). In our main results, described in Section 7, we find that the current electoral-cycle-driven post-disaster policies increase on-cycle county populations by up to 13.06% in 80 years. In comparison, it would be only 1.15% without the local quality of life improvements (Figure B.9). At the same time, highly productive areas still bear the cost of this fiscal redistribution, making them slightly less attractive relative to other regions and therefore losing populations. Previously uncrowded places and small urban centers, i.e., neither coastal nor over-congested cities, traditionally net recipients of the fiscal system, become appealing.

While the aggregate productivity is lower in this first alternative baseline scenario compared to our main counterfactual, the impact on aggregate GDP remains mildly unchanged. The aggregate productivity loss would still be 0.74%, whereas GDP would increase by 0.05% without improvements in local quality of life (see Panel (b) of Figure B.9). This result occurs because the labor force's relocation out of both exposed regions and large productive cities dampens the productivity distortions caused by the post-disaster intervention. In other words, fewer workers live in less productive areas, so the local effect on fundamental productivity is not fully capitalized into aggregate GDP. At the same time, this labor force massively relocates to less productive counties, canceling any mild negative effect that would have affected aggregate production otherwise. Aggregate welfare increases as very dense counties are decongested at no extra cost; i.e., the aggregate output remains unchanged. Specifically, welfare gain would reach a sizable 0.19% without the local quality of life improvements (see Panel (b) of Figure B.10).

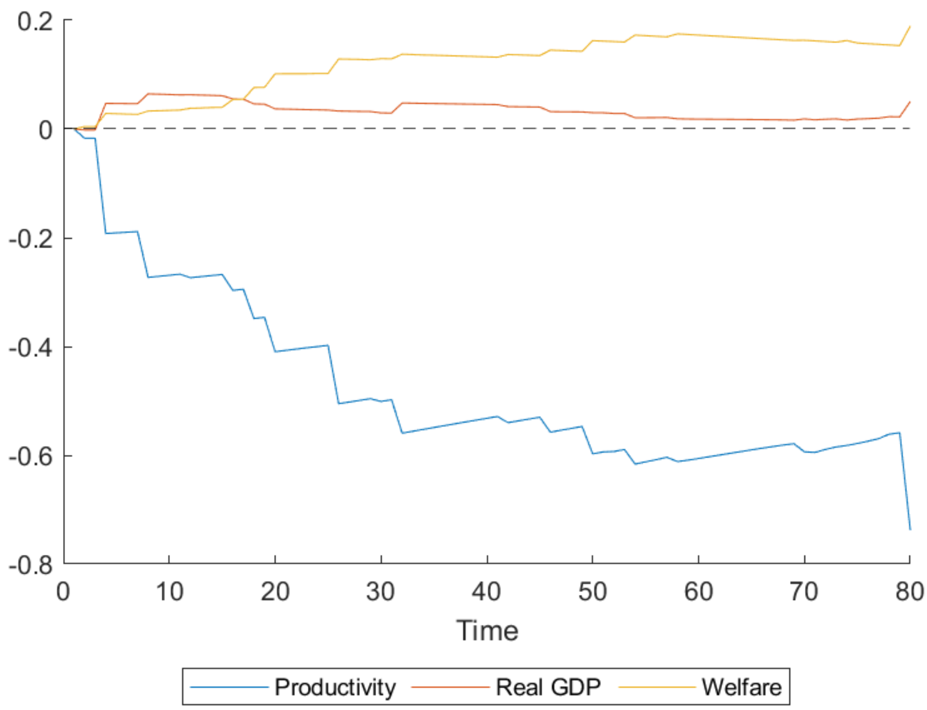
We now illustrate changes in local population size after 80 years in our baseline scenario without the productivity distortions after an on-cycle hurricane (Panel (a) of Figure B.10) and their aggregate effects on productivity, GDP, and welfare (Panel (b)) relative to our counterfactual without any electoral cycle effect. Compared to our main counterfactual scenario, the population sorts even more into exposed coastal areas as, on top of increased fiscal transfers, no local productivity losses tone down the improved amenities (+8.37%, on average). On-cycle county populations would increase by 20.89% without the productivity distortion after on-cycle hurricanes (Figure B.10). In this scenario, all non-coastal counties, and even more highly productive areas that still bear the cost of this fiscal redistribution, lose population to the benefit of these more exposed counties (Figure B.9).

In this alternative scenario, aggregate productivity is slightly lower than in our main counterfactual, but the impact on aggregate GDP is highly damaging. The aggregate productivity loss would only be 0.26%, and the loss of real GDP would be 1.61% after 80 years without local labor markets distortions (see Panel (b) of Figure B.10). This new result happens because the labor force's relocation into exposed regions decongests highly productive areas. Fewer workers live in dense, productive regions, while less crowded coastal regions become endogenously more productive, partially compensating for the aggregate productivity loss caused by population sorting. Similarly, the output losses caused by the reallocation of the labor force from dense, productive areas further dominate the output gains from moving into exposed coastal areas, causing a sizable aggregate GDP differential after 80 years. In turn, aggregate welfare would have increased significantly absent endogenous sorting responses. Instead, the solid exogenous amenity improvements brought by the post-disaster efforts after on-cycle hurricanes are compensated by a significant endogenous economic contraction, causing aggregate welfare to decline mildly by 0.02%.

In our first alternative baseline scenario (i.e., without amenity improvements), welfare increases because endogenous amenity improvement due to endogenous population sorting occurs at no GDP loss. In our second alternative baseline scenario (i.e., without productivity distortion), welfare remains almost unchanged because exogenous amenity improvements compensate for endogenous output losses due to endogenous population sorting. In other words, we observe improved aggregate quality of life in either case. However, the endogenous sorting pattern caused by local amenity improvements impacts more substantially large productive areas, causing sizable aggregate output losses.



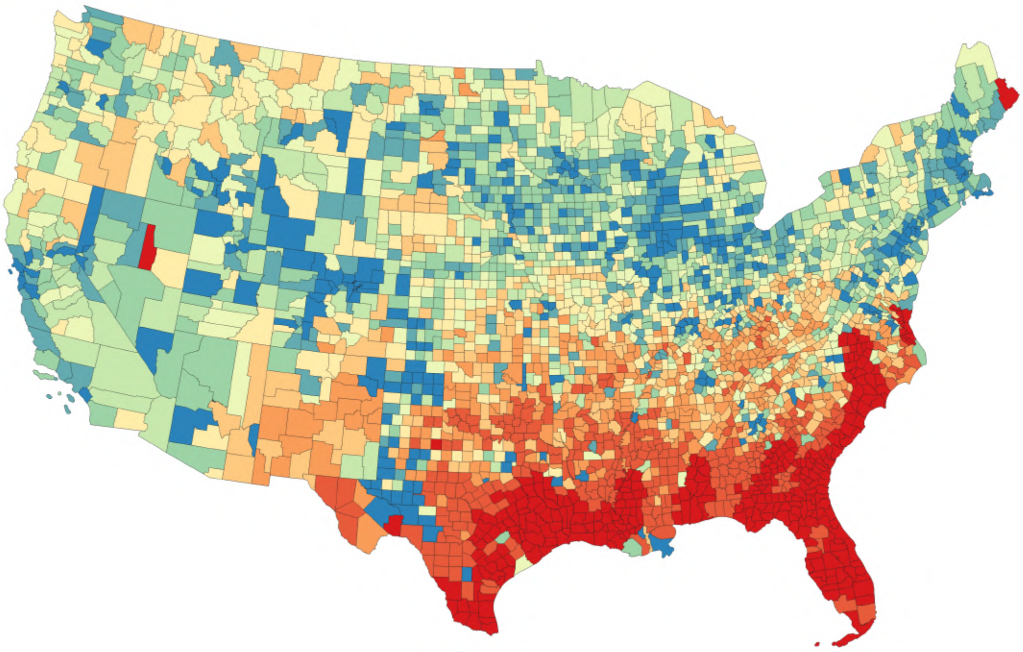
(a) Population Size after 80 Years: Baseline (w/o Amenity Transformation) vs. Counterfactual



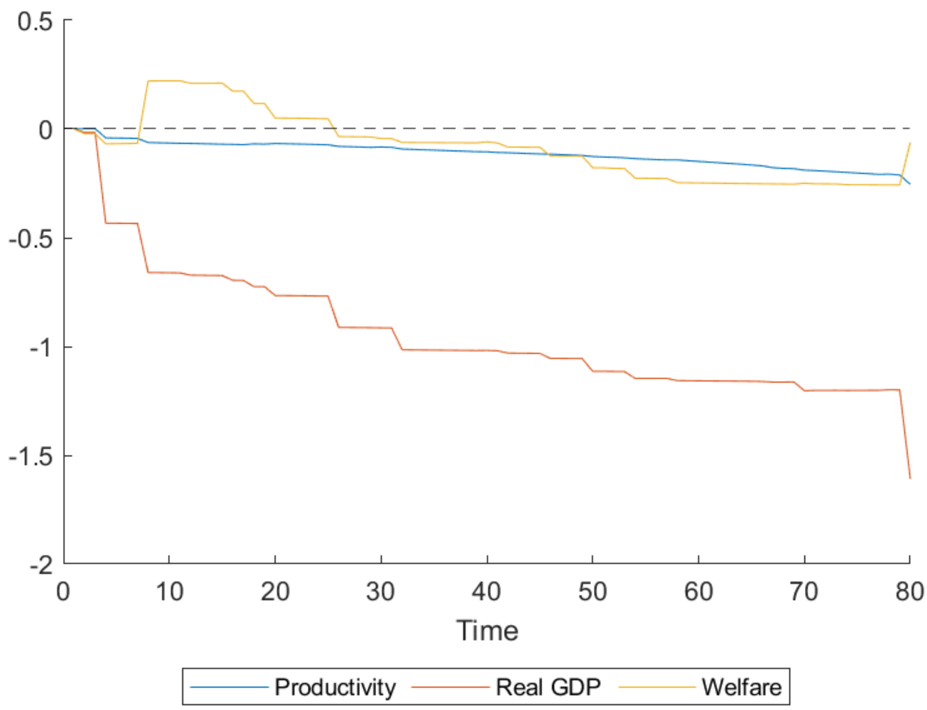
(b) Percentage Changes in Aggregate productivity, Real GDP, and Welfare: Baseline vs. Counterfactual

Figure B.9: Aggregate Changes of Electoral-Cycle-Driven Post-Disaster Polices

Notes: The map in Panel (a) depicts the ratio in local population size between current post-disaster policies and a counterfactual scenario without electoral-cycle-driven post-disaster policies' amenity transformation after 80 years of simulation. The warm color represents more households in the baseline scenario after 80 years relative to the counterfactual. Panel (b) depicts the changes in aggregate productivity, real GDP, and welfare given the current post-disaster policies compared to the counterfactual scenario.



(a) Population Size after 80 Years: Baseline (w/o Productivity Distortion) vs. Counterfactual



(b) Percentage Changes in Aggregate Productivity, Real GDP, and Welfare: Baseline vs. Counterfactual

Figure B.10: Aggregate Changes of Electoral-Cycle-Driven Post-Disaster Polices

Notes: The map in Panel (a) depicts the ratio in local population size between current post-disaster policies and a counterfactual scenario without electoral-cycle-driven post-disaster policies' productivity distortion after 80 years of simulation. The warm color represents more households in the baseline scenario after 80 years relative to the counterfactual. Panel (b) depicts the changes in aggregate productivity, real GDP, and welfare given the current post-disaster policies compared to the counterfactual scenario.

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