Should We Flee? The Mortality and Economic Impacts of Hurricane Evacuation Orders

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Abstract

I estimate the mortality and county-level economic impacts of hurricane evacuation orders using a novel instrumental variables framework. Relative to similarly affected but non-evacuated areas, evacuated counties experience fewer deaths but lower personal income, employment, and business establishments for at least two years following the storm. I show that these economic losses stem from a stalled recovery driven by congestion in disaster aid, insurance payouts, and access to contractors. Finally, I develop a threshold decision rule for evacuation orders that balances the newly estimated mortality benefits against persistent economic costs, and use it to retrospectively evaluate historical evacuation decisions.

JEL Classifications: Q54, J38, R11

Keywords: Natural Disasters, Public Policy, Regional Economics

1 Introduction

From 2014 to 2022, a dozen named hurricanes swept across the Gulf and Southeast Atlantic coasts, prompting evacuation orders in more than 400 counties and temporarily displacing an estimated 19 million people. For communities in the path of these storms, evacuation is the most visible and immediate form of public action—an attempt to save lives by moving people out of harm's way. For policymakers, it remains one of the few tools available to mitigate risk before disaster strikes. Yet evacuation orders are complex social and economic events: costly to issue, burdensome to follow, only partially obeyed, and little studied for their longer-term effects on the people and places they aim to protect.

This paper is the first to undertake a causal analysis of hurricane evacuation orders to examine their life-saving efficacy and persistent economic impacts. I develop an instrumental variable strategy to exploit quasi- random discrepancies between storm forecasts and realized storm intensity to estimate 1) the number of lives saved due to an evacuation order and 2) post-disaster economic outcome differences between places that evacuated vs places that did not given equivalent realized storm intensity. I then use these new estimates to inform a decision-rule model to understand past evacuation decision thresholds, and to determine optimal thresholds which may be of use to policymakers and practitioners.

The basic research design compares two otherwise identical counties that experience the same realized storm conditions but differ in their forecasted risk: forty-eight hours before a storm, some counties enter the 50-mile band around the forecasted track, increasing the probability of evacuation, while others do not. By conditioning on distance to the realized storm track and an objective intensity function based on storm characteristics, I isolate variation in evacuation orders driven by forecast error—an information shock that shifts evacuation likelihood without altering actual storm intensity.

I find that evacuations save lives. For severe hurricanes, an evacuated county can expect approximately 90 fewer deaths per 100,000 residents on average. This exceeds annual mortality reductions from many well-regarded public health programs such as Medicaid expansion or implementation of critical vaccines — a difference reflecting the efficiency of targeting a concentrated period of acute, identifiable risk.

However, these benefits are not without lasting costs. Counties subject to evacuation orders experience downturns in employment and wages persisting for at least two years. Personal income per capita remains approximately 2 percent lower than in comparably damaged counties without evacuation orders throughout the first two post-storm years, recovering only by the third. Home prices decline within

six months of the storm and do not return to levels observed in equally affected but non-evacuated counties for nearly four years. Moreover, up to 5 percent of pre-storm businesses permanently close in evacuated areas relative to equally impacted counties without evacuation orders.

The mechanisms underlying this downturn are complex, but the evidence points to a queuing process that introduces lags into the rebuilding timeline. I find evidence of the following: evacuated counties do not begin recovery activities—applying for aid, filing insurance claims, and contracting for repairs—until weeks after their non-evacuated counterparts. The evidence to support this claim comes from industry-level analyses and an examination of building permits and National Flood Insurance Program (NFIP) claims.

Finally, using a decision-under-uncertainty model, I balance the costs and benefits of evacuation orders to infer the implied value of a statistical life (VSL) from past evacuation decisions under the newly estimated parameters. I then estimate the model to assess whether historical evacuation orders appear justified when evaluated against commonly used VSL benchmarks. In this draft, I lay out the model and subsequent drafts will include these policy simulations.

1.1 Literature and Contribution

Although evacuation orders have been discussed in the public policy, natural hazards, and—more recently—economics literatures, no prior study has estimated their population-wide causal effects on mortality or their longer-run economic consequences. Much attention has been paid to the question of what makes evacuations successful in terms of compliance. Prior work shows that evacuation orders are most likely to be followed when residents have time to prepare and when communities are older, whiter, and view both the storm as dangerous and the evacuation as protective (Whitehead et al., 2000; Anand et al., 2023; Molina et al., 2021). But an antecedent question is of critical importance—do they save lives, and at what economic cost?

From past evidence, we know that – contrary to their goal – hurricane evacuation orders can increase mortality in specific contexts. Using an instrumental variables strategy based on forecasts, Dosa et al. (2012) find that evacuations of nursing homes increase mortality. In addition, Hurricane Rita (2005) provides a cautionary tale: 100 of the 119 storm-related deaths have been attributed to the evacuation itself through road accidents, lack of food and water, heat exposure, and poor traffic management (Baker, 2018). Evacuation orders remain a central public policy tool despite these cautionary examples, yet we have no *ex post* causal estimates of how they reduce mortality at the population level. We do know that

hurricanes cause large numbers of excess deaths persisting for as long as 15 years after the event (Young and Hsiang, 2024), and that permanent relocation from disaster-affected areas to lower-mortality regions can substantially reduce long-run mortality risk (Deryugina and Molitor, 2020).

In addition to not understanding the benefits of this commonly used policy intervention, we do not fully understand its costs. A recent contribution toward closing this gap comes from Gellman et al. (2024), who uses a structural travel cost model to estimate that the welfare costs of evacuation can be large relative to the total deaths from hurricanes. However, we currently know very little about lasting economic impacts beyond these direct evacuation costs.

Even before considering evacuation, hurricanes leave a complex economic and public health footprint. In the short run, they tend to reduce employment but raise wages in affected areas (Belasen and Polachek, 2008; Groen et al., 2020). For individuals directly exposed to major storms, incomes often rise (Deryugina et al., 2018), and regions see higher wages for as long as eight years afterward (Roth Tran and Wilson, 2025), likely reflecting the influence of federal aid, insurance payouts, and other safety-net programs (Deryugina, 2017). Yet over much of the twentieth century, hurricanes and other natural disasters were associated with slower regional growth as residents migrated away from damaged or high-risk areas (Boustan et al., 2020), while more recent events appear to boost growth for as long as eight years (Roth Tran and Wilson, 2025). Although Sacerdote (2012) and Deryugina et al. (2018) explore outcomes for permanent and long-term individual evacuees and their children, no study has yet attempted to disentangle the effect of evacuation orders on local economic conditions from the effect of the storm itself over the medium or long run—a setting in which masses of people leave and then most return.

Belasen and Polachek (2008), Groen et al. (2020), and Deryugina et al. (2018) find consistent evidence that hurricanes generate short-term labor supply shocks, typically reducing employment and raising wages in affected regions, with some evidence of the opposite pattern—higher employment and lower wages—in nearby unaffected areas. In contrast, I find that evacuation orders cause declines in both employment and wages, suggesting that they operate through both labor supply and labor demand channels. This pattern is consistent with the idea that mandatory evacuation disrupts not only the availability of workers but also the demand for local goods and services, and cannot be fully explained by the direct personal costs of evacuation that Gellman et al. (2024) documents.

Emergency managers face not only the challenge of compliance with evacuation orders but also the difficulty that these orders are based on forecasts that are highly valuable yet inherently imperfect. A growing literature shows that improvements in hurricane forecasting have delivered substantial welfare

gains in recent decades, and that further advances would yield even larger benefits (Lazo and Waldman, 2011; Molina et al., 2021; Molina and Rudik, 2024). Nevertheless, forecast uncertainty remains significant, forcing decision-makers to weigh the costs of false alarms against the risks of insufficient response. This residual uncertainty is central to the empirical strategy of this paper: it creates quasi-experimental variation in evacuation decisions that is plausibly exogenous to local economic conditions.

The contributions of this paper are fourfold. First, I causally estimate the mortality benefits of evacuation orders—a parameter not currently available to decision-makers—and show how it can be integrated into the evacuation decision. Second, I estimate the lasting economic costs of evacuation orders and examine the mechanisms driving them: in part, a queuing process in which evacuated counties face delays in accessing insurance payouts and contractors relative to their non-evacuated counterparts. Third, I develop a decision model that can inform policy and serve as a practical tool for evacuation managers, and deploy it—alongside other tools—to evaluate and improve evacuation decisions. Finally, I extend our understanding of evacuation compliance by using cell phone mobility data to produce the first complete zone-level compliance estimates for all evacuation-inducing storms from 2014 to 2022.

2 Data

This study combines multiple datasets to examine the causal effect of hurricane evacuation orders on mortality and local economic outcomes across eight hurricane-prone southeastern states. To capture the full mortality impact of hurricanes—including indirect deaths not immediately attributed to the storm—I draw on all-cause mortality data from the Centers for Disease Control and Prevention (CDC) Wide-ranging Online Data for Epidemiological Research (WONDER) database and the NIH National Vital Statistics System multiple cause-of-death microdata files (National Center for Health Statistics). Together, these sources allow me to estimate excess deaths and identify whether evacuation orders mitigate or exacerbate hurricane-related mortality.

2.1 Storm Data

To construct my storm data, I combine evacuation order data with tropical cyclone forecast and observed track data from the National Hurricane Center (NHC). Specifically, I use the NHC's HURDAT2 "Best Track" database (National Hurricane Center, b), which provides the official record of tropical cyclone tracks and intensities at six-hour intervals, to measure realized storm conditions. Historical fore-

cast cone data, also from the NHC (National Hurricane Center, a), provide predicted storm paths in the days prior to landfall, which I use to identify plausibly exogenous variation in evacuation order exposure. Storm surge forecast data come from the NHC's probabilistic storm surge archive, and flooding levels come from U.S. Geological Survey high-water marks (U.S. Geological Survey, 2024). I supplement these with data on hurricane-related deaths, injuries, and property damages from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) (Center for Emergency Management and Homeland Security, Sheldus Database, 2024).

Data on hurricane evacuation orders come from the Hurricane Evacuation Orders Database (HEvOD) (Anand et al., 2023), which provides comprehensive coverage of mandatory and voluntary evacuation orders issued at both the evacuation zone level (aligned with census tracts) and the county level, depending on local jurisdictional practices. The sample covers counties in Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Texas that received at least one evacuation order, and includes the 14 Atlantic Basin hurricanes from 2012–2022 that elicited evacuation orders in these states. Figure 1 shows the geographic distribution of mandatory evacuation orders across the region over this period.

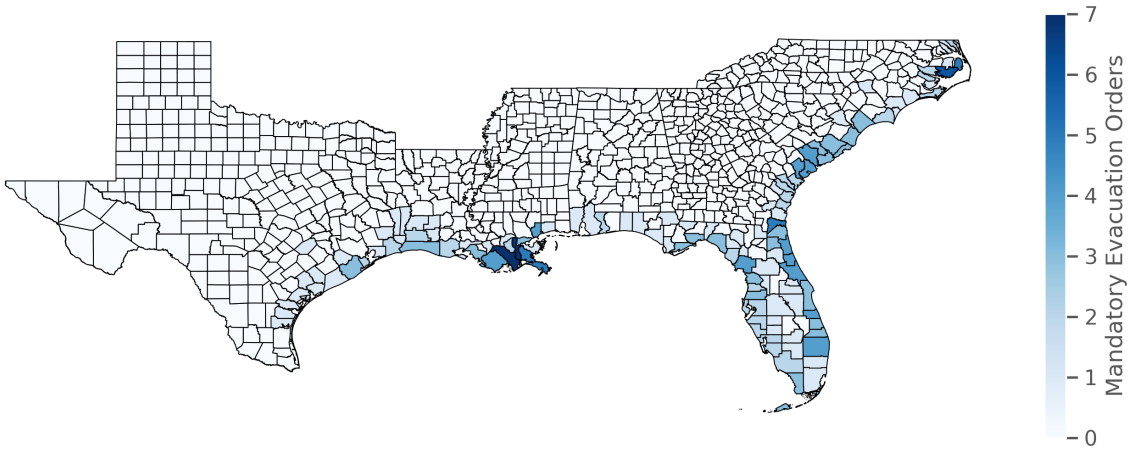


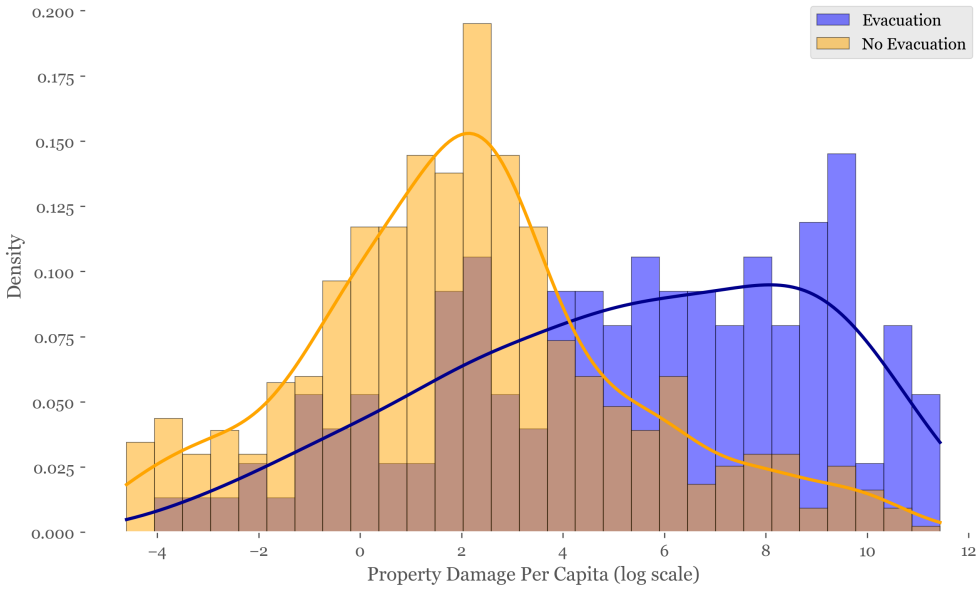
Figure 1: Geography of Mandatory Evacuation Orders

Hurricane evacuation orders are typically issued between one and five days prior to landfall, with approximately 80 percent occurring within 24 to 48 hours of expected impact. Orders vary along several dimensions, including whether they are mandatory or voluntary and the geographic scope of coverage, ranging from narrow coastal evacuation zones and mobile-home or low-lying areas to partial counties, entire counties, and—rarely—entire states. Although formal evacuation orders are generally lifted within two to four days after landfall, population return and economic activity take substantially longer

to normalize, typically requiring between one and three weeks. Evacuation orders are implemented in a graduated manner, expanding from the most exposed coastal zones to broader inland areas as storm risk increases. In the empirical analysis, I focus on mandatory evacuation orders extending beyond the most vulnerable coastal zones, where compliance is highest and where the economic and mortality tradeoffs of evacuation are most salient. I will also expand analysis to look at the marginal reduction in deaths from one higher level of evacuation, which is explained in more detail in Section 4.

An important empirical question is whether evacuated and non-evacuated counties are comparable in terms of storm damages. Figure 2 shows the distribution of county-level property damages for evacuated and non-evacuated counties. When the sample is restricted to severe hurricanes (Categories 4–5), the damage distributions are similar across evacuated and non-evacuated counties (Figure A2). This similarity suggests that storm damages are comparable across these areas, supporting the overlap assumption required for causal identification and indicating that evacuated and non-evacuated counties provide meaningful counterfactual comparisons. [1]

Figure 2: Distribution of Per Capita Property Damage, Cat 2+



Notes: This figure shows the distribution of county-level property damage per capita (log scale) for counties with and without evacuation orders following Category 2 or higher hurricanes. Blue bars and the solid blue line denote evacuated counties; orange bars and the solid orange line denote non-evacuated counties, and brown represents ovrlapping distributions. Bars represent normalized histograms and solid lines indicate kernel density estimates. The similarity in the distributions is consistent with overlap in storm damages across evacuated and non-evacuated counties. Property damage data comes from SHELDUS and Evacuation data comes from HEvOD.

[1] Although property damage is informative for assessing overlap, it is not used as a control variable in the main specifications, as it is plausibly endogenous to evacuation decisions through households' ability to undertake protective actions. For the model, I will use an objective intensity function, described later on in this section

2.2 Economic Data

I measure local economic outcomes using multiple sources. For monthly employment dynamics, I use the Local Area Unemployment Statistics (LAUS) program from the Bureau of Labor Statistics (Bureau of Labor Statistics) and for home prices I use monthly Redfin median listing data aggregated to quarters. To examine establishment-level employment, firm entry and exit, and wage dynamics at the annual frequency, I incorporate data from County Business Patterns (U.S. Census Bureau, 2024). County-level income and GDP measures come from the Bureau of Economic Analysis (BEA) Regional Economic Accounts (Bureau of Economic Analysis).

To understand population displacement and behavioral responses to evacuation orders, I utilize aggregated smartphone mobility data (Advanced Research, 2025). These data allow me to track movement patterns before, during, and after hurricane events and evacuation orders. Finally, I examine impacts on recovery using data from Builty (Builty, 2022), which provides detailed information on building permit issuance across the United States

2.3 Variable Construction

2.3.1 Excess Deaths

I use excess deaths—that is, deaths over expected—rather than direct disaster fatality counts for two reasons. First, disaster-related deaths are notoriously difficult to attribute. Official fatality counts typically rely on narrow cause-of-death certification criteria, capturing only deaths directly caused by storm forces such as structural collapse, flying debris, or drowning (Santos-Burgoa et al., 2018). This approach systematically excludes deaths that are causally linked to the storm but not immediately apparent—such as cardiac events during evacuation, delayed medical care due to infrastructure disruptions, traffic accidents, in-home storm-induced hazards like carbon monoxide poisoning, or chronic disease exacerbation when medications and treatment regimens are interrupted (Kishore et al., 2018; Cruz-Cano and Mead, 2019). The problem is compounded when local death certification systems are themselves disrupted: overwhelmed officials may not follow CDC disaster-mortality coding protocols, leading to further undercounting even of deaths that occur during the storm itself (Santos-Burgoa et al., 2018; Young and Hsiang, 2024). An all-cause excess mortality framework sidesteps these attribution problems entirely, capturing the full short-run mortality impact without requiring that each death be individually linked to the storm.

The consequences of undercounting based on direct attribution can be severe. Following Hurricane Maria, Puerto Rico's official death toll was reported as 64, while subsequent analyses using excess mortality methods estimated between 1,000 and 4,600 deaths depending on the time horizon and methodology (Santos-Lozada and Howard, 2018; Kishore et al., 2018; Cruz-Cano and Mead, 2019). Similar discrepancies have been documented for Hurricanes Katrina, Harvey, Florence, and other mainland storms (Santos-Lozada and Howard, 2018).

Second, even if measurement error in official fatality counts were classical—or at least preserved ordinal rankings above zero—the extreme concentration of reported deaths at zero poses distinct econometric challenges. The vast majority of county–storm observations record no direct fatalities, generating a highly right-skewed and zero-inflated distribution that complicates standard regression inference. Moreover, inclusion in datasets such as SHELDUS typically requires substantial county-level damage, implying that low-fatality events are systematically omitted even under perfect measurement among included observations.

To address both of these concerns, I construct a measure of monthly excess deaths by estimating expected mortality from a model with county and calendar-month fixed effects. Importantly, I estimate this baseline model on a restricted sample that excludes county-month observations in which a severe storm made landfall or an evacuation order was in effect. This ensures that the expected mortality counterfactual is not contaminated by treatment effects I seek to estimate.

$$\text{Deaths}_{cty} = \alpha_c + \lambda_t + \gamma_y + \epsilon_{cty}, \tag{1}$$

where α_c captures baseline mortality levels in county c, λ_t absorbs seasonal variation across calendar months, and γ_y accounts for year-specific trends. I estimate this model on the full sample and compute predicted values $\widehat{\text{Deaths}}_{cty}$ for each county-month-year observation. Excess deaths are then defined as the difference between observed and predicted deaths for all county-months, including those affected by storms and evacuations.

$$\text{ExcessDeaths}_{cty} = \text{Deaths}_{cty} - \widehat{\text{Deaths}}_{cty}. \tag{2}$$

This measure captures the full mortality footprint of a storm—including indirect and delayed deaths—while producing a continuous outcome with variation across the sample. A county may record zero

official storm fatalities yet exhibit meaningfully elevated mortality in the weeks following landfall. While no perfect method exists, I also supplement this method with the official reports of deaths by 'natural disaster or extreme weather' in the CDC's restricted mortality files to show that findings are directionally similar when using official reports.

2.3.2 Storm Intensity Index

To control for objective storm severity, I construct an intensity index $I_{cs}(X)$ that summarizes the physical destructiveness of storm s at county c. The index combines wind speed and storm surge additively:

$$I_{cs}(X) = \underbrace{\left(\frac{\max(w_{cs} - 50, 0)}{74 - 50}\right)^3}_{\text{Wind Component}} + \underbrace{\ln(1 + S_{cs})}_{\text{Surge Component}} \tag{3}$$

where w_{cs} is maximum sustained wind speed in knots at county c, S_{cs} is maximum storm surge in feet.

The wind component follows the natural hazards literature, which finds that damages scale with approximately the cube of wind speed above a threshold (Emanuel, 2011; Nordhaus, 2010). The threshold of 50 knots reflects the minimum wind speed typically associated with building damage (Vickery et al., 2006), and addresses a key identification concern: without it, the index would assign positive values to storms causing no structural damage, potentially undermining overlap between treated and control counties. Normalizing by 48 knots—the difference between the Category 1 threshold and the damage threshold—yields a unit-free index where $I_{cs} = 1$ corresponds to a minimal hurricane.

The additive structure follows FEMA's HAZUS-MH methodology, which estimates combined hurricane losses by summing separate wind and flood damage functions (Vickery et al., 2006; FEMA, 2018). Wind and surge largely damage distinct building components, making an additive specification more appropriate than a multiplicative one. The logarithmic transformation of storm surge captures diminishing marginal damage at higher inundation levels, consistent with standard depth-damage functions (FEMA, 2018).

3 Empirical Strategy

In a perfect experimental design, researchers would observe an approaching storm and randomly assign evacuation orders across comparable jurisdictions. Lacking such an experiment, I approximate

one by comparing two otherwise identical counties—A and B—that experience the same realized storm conditions but differ in their forecasted risk. forty-eight hours before a storm, some counties enter the 50-mile band around the forecasted track, increasing the probability of evacuation, while others do not. I then control for actual distance to the storm and an objective intensity function based on storm characteristics. This instrument serves as a direct information shock: it shifts evacuation likelihood without altering storm intensity or ex post outcomes. Figure 3 illustrates the basic comparison with Hurricane Ian (2022). forty-eight hours before landfall, emergency managers observe the forecast (orange), and counties in purple issue mandatory evacuation orders. The black line shows the realized track. By conditioning on realized distance and storm characteristics, I compare counties that experienced the same damage but, due to exogenous forecast error, differed in whether they evacuated.

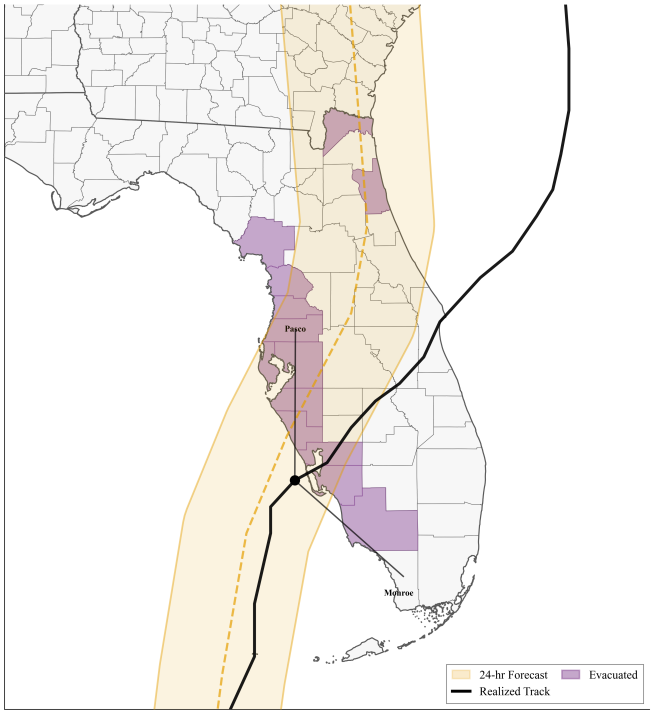


Figure 3: Example of Identification Strategy: Hurricane Ian (2022) Forecast and Track

Notes: Shaded area shows the NHC forecast cone 48 hours before landfall; solid black line shows the realized track from HURDAT2. Counties in purple issued mandatory evacuation orders. Labeled counties illustrate the identification strategy: similar distance to realized track but different forecast exposure. Hurricane Ian made landfall near Fort Myers, Florida on September 28, 2022. *Sources:* NHC, HEvOD.

The instrument is an indicator for whether county c entered the forecast cone 48 hours before storm s made landfall:

$$Z_{cs} \;=\; \mathbb{1}\{\text{county } c \text{ in 48-hour forecast cone for storm } s\}. \tag{4}$$

The first stage relates this information shock to evacuation orders:

$$\text{EvacOrder}_{cs} \;=\; \pi_1\, Z_{cs} \;+\; \pi_2\, \text{Dist}_{cs}^{\text{landfall}} \;+\; \phi\, I_{cs}(x) \;+\; \eta_s \;+\; \theta_{ty} \;+\; \nu_{cs} \tag{5}$$

where $\text{Dist}_{cs}^{\text{landfall}}$ is the realized distance from county c to the storm track, $I_{cs}(x)$ is an intensity function of wind and storm surge (equation (3)) capturing objective realized storm conditions at the county level, η_s is a storm category (Saffir-Simpson Scale 1-5) fixed effect, and θ_{ty} is a month-by-year fixed effect. The coefficient π_1 measures the increase in evacuation probability from entering the forecast cone.

The second stage estimates the effect of evacuation orders on mortality:

$$\text{ExcessDeaths}_{cs\tau} \;=\; \gamma_1\, \widehat{\text{EvacOrder}}_{cs} \;+\; \gamma_2\, \text{Dist}_{cs}^{\text{landfall}} \;+\; \phi\, I_{cs}(x) \;+\; \eta_s \;+\; \theta_{ty} \;+\; \varepsilon_{cs\tau} \tag{6}$$

where τ indexes months relative to landfall and γ_1 is the local average treatment effect of an evacuation order on excess deaths per 100,000 residents. Because the dependent variable is already expressed as a deviation from each county's historical baseline mortality, county fixed effects are absorbed by construction and are omitted from the specification.

Event-Study Framework for Employment

In order to map the dynamic employment effects of mandatory evacuation orders—and address their potential endogeneity—I extend the forecast-cone instrument to an event-study framework. The first stage is

$$\text{EvacOrder}_{cs} \;=\; \pi_1\, Z_{cs} \;+\; \pi_2\, \text{Dist}_{cs}^{\text{landfall}} \;+\; \phi\, I_{cs}(x) \;+\; \alpha_c \;+\; \lambda_t \;+\; \eta_s \;+\; \nu_{cst}, \tag{7}$$

where α_c is a county fixed effect, λ_t is a calendar-month fixed effect, and η_s η_s is a storm category (Saffir-Simpson Scale 1-5) fixed effect,

Second stage. The second stage is an extends an IV framework to the event-study specifications in Deryugina (2017) and Henkel et al. (Forthcoming), regressing outcomes on a set of event-time dummies to capture the dynamic trajectory of the local economy in the periods prior to and following a destructive

hurricane:

$$\log(Y_{ct}) = \sum_{\substack{\tau=-3 \\ \tau \neq -1}}^{4} \left[\beta_\tau + \delta_\tau \widehat{\text{EvacOrder}}_{cs}\right] \mathbb{1}\{t - T_{cs} = \tau\} \; + \; \gamma_2 \, \text{Dist}_{cs}^{\text{landfall}} \; + \; \phi \, I_{cs}(x)$$

$$+ \; \alpha_c \; + \; \lambda_t \; + \; \eta_s \; + \; \varepsilon_{cst}, \tag{8}$$

where $\widehat{\text{EvacOrder}}_{cs}$ (and its interactions with the event-time dummies) comes from the first stage. Coefficients β_τ trace the baseline post-storm path for counties that experienced a hurricane but were not evacuated, while δ_τ captures the dynamic Local Average Treatment Effect (LATE) of evacuation orders in county c at τ months from hurricane arrival. County fixed effects α_c, calendar-time fixed effects λ_t, and storm fixed effects η_s control for unobserved heterogeneity across counties, time periods, and storm events. Following Roth Tran and Wilson (2025), I report standard errors that are two-way clustered at the county level and at the state-by-storm level, using the wild cluster bootstrap procedure proposed by MacKinnon et al. (2021). Clustering by county accounts for arbitrary serial correlation, while clustering by state-by-storm allows for cross-sectional correlation in errors within a state for a given storm, such as that arising from common state-level policies and storm-specific shocks.

The identification strategy rests on two critical assumptions. First, to satisfy the exclusion restriction, entrance into the forecast cone must not affect mortality or economic outcomes conditional on the realized storm track and objective intensity controls. Table 1 provides evidence for this assumption. For different distance bands from realized landfall, I report realized storm conditions for the full sample, treated compliers, and non-evacuated counties. Within each distance bin, differences in mean wind speed, Saffir–Simpson category, and storm surge are small in magnitude, and any remaining variation in realized conditions is absorbed by the objective hazard controls included in all specifications.

The community characteristics in Table 2 reinforce the case for the exclusion restriction and speak to external validity. Within each distance band, demographic and economic characteristics are broadly balanced across treated compliers and non-evacuated counties. Treated compliers do exhibit modestly higher median incomes and lower poverty rates, which likely reflects the concentration of evacuation orders along wealthier Gulf and Atlantic coastlines rather than selection on community type conditional on storm exposure. These differences do not threaten identification—I verify that all results are robust to directly controlling for observable community characteristics—but they raise the natural question of whether the LATE generalizes beyond compliers. The relevant condition is not that complier and

Table 1: Storm Characteristics by Distance to Realized Landfall

	Full Sample				In Cone × Evacuated				Not Evacuated			
Distance (km)	N	Wind (kt)	SSHS	Surge (ft)	N	Wind (kt)	SSHS	Surge (ft)	N	Wind (kt)	SSHS	Surge (ft)
0–50	38	80.9	2.3	11.4	17	87.9	2.8	13.1	18	73.8	1.4	8.6
		(11.0)	(1.5)	(14.3)		(8.5)	(1.3)	(15.2)		(9.7)	(1.1)	(15.5)
50–125	268	82.0	2.2	8.7	35	83.9	2.7	2.5	221	81.3	2.1	9.9
		(10.5)	(1.3)	(12.7)		(9.8)	(1.4)	(4.6)		(10.6)	(1.2)	(14.4)
125–220	502	82.1	2.2	1.8	8	83.4	2.9	0.0	474	81.6	2.1	1.3
		(14.3)	(1.2)	(4.7)		(12.9)	(1.6)	(0.0)		(13.9)	(1.2)	(2.6)
>220	810	76.4	1.4	2.8	9	91.0	2.8	5.4	785	76.0	1.3	1.7
		(9.8)	(0.7)	(5.8)		(11.7)	(0.7)	(2.8)		(9.5)	(0.6)	(4.8)

Notes: Each cell reports the mean with standard deviations in parentheses. Wind speed in knots; SSHS is the Saffir–Simpson Hurricane Scale category. Storm surge (ft) is computed using coastal counties only, based on USGS high-water mark data. "In Cone × Evacuated" denotes county–storm observations inside the NHC forecast cone that received a mandatory evacuation order.

non-complier subpopulations be distributionally identical, but that their covariate supports overlap, so that differences in estimated effects can be attributed to treatment status rather than to compositional imbalance across covariate cells (Angrist and Fernández-Val, 2013; Angrist et al., 1996). Figure A1 confirms that this condition is satisfied: kernel densities of covariates—demographic, economic, and storm-related—share broad common support across evacuated and non-evacuated observations. Applying the logic of Abadie (2003), who shows that the complier subpopulation can be fully characterized by its observable covariates, this profiling exercise demonstrates that compliers span the full range of community types in the sample rather than clustering in a narrow or anomalous subset. Taken together, the covariate balance supports the exclusion restriction, the broad overlap supports the external relevance of the LATE, and the robustness of all estimates to observable controls provides a direct check against residual compositional confounding (Dehejia et al., 2021). It is important, also, that the forecast cone instrument is not just picking up differences in surprise vs. well-forecasted storms, since there is evidence that forecasts alone can reduce mortality (Molina et al., 2021; Shrader et al., 2023; Anand, 2025). If, for example, large numbers of people take actions to reduce mortality or change economic outcomes because of the cone – and not because of the order – then this would be violated. I show in detail in the robustness section that 1) Being in the cone with no evacuation order or a voluntary evacuation order does not cause large numbers of people to evacuate and 2) omitting evacuated observation and estimating the direct effect of the cone does not decrease mortality or harm economic outcomes and 3) that estimates are robust to FEMA-response funding that might mitigate post-disaster harms and that, and estimates are robust to including absolute forecast error as a control. While there may be small protective measures do to the

Table 2: Community Characteristics by Distance to Realized Landfall

	Mean			Std. Dev.		
	Full	Treated	Not Evac.	Full	Treated	Not Evac.
Distance: 0–50 km						
Population (000s)	151.6	147.6	133.4	145.4	128.6	117.0
Median income ($000s)	45.7	50.2	41.1	8.3	7.3	7.6
Poverty rate (%)	17.9	14.9	21.3	5.8	3.4	6.3
Elderly share (%)	17.3	16.2	17.0	6.6	6.3	5.2
Child share (%)	6.1	6.3	6.1	1.4	1.5	1.2
Black share (%)	14.8	14.4	15.7	8.5	7.4	9.8
Hispanic share (%)	13.5	7.3	19.9	19.8	6.3	27.0
N	38	17	18			
Distance: 50–125 km						
Population (000s)	156.9	241.1	144.5	363.7	340.1	374.7
Median income ($000s)	44.4	47.4	43.9	9.4	7.2	9.8
Poverty rate (%)	18.9	16.7	19.2	5.2	3.7	5.4
Elderly share (%)	15.4	15.5	15.2	4.5	4.0	4.4
Child share (%)	6.4	6.2	6.5	1.1	0.9	1.1
Black share (%)	24.5	18.6	25.3	15.2	11.6	15.5
Hispanic share (%)	12.4	8.6	13.1	16.6	6.6	17.8
N	268	35	221			
Distance: 125–220 km						
Population (000s)	136.5	158.2	134.3	365.8	112.3	374.7
Median income ($000s)	41.9	44.8	41.5	10.2	8.0	10.1
Poverty rate (%)	20.0	17.4	20.2	5.7	4.7	5.7
Elderly share (%)	16.0	20.4	16.0	4.6	8.6	4.5
Child share (%)	6.2	5.3	6.2	0.9	1.2	0.9
Black share (%)	27.3	16.7	27.7	16.9	18.2	17.0
Hispanic share (%)	10.1	7.5	10.2	14.9	2.3	15.3
N	502	8	474			
Distance: >220 km						
Population (000s)	93.2	165.5	91.8	199.8	114.5	201.9
Median income ($000s)	41.0	50.7	40.7	9.2	9.7	9.1
Poverty rate (%)	20.1	15.3	20.2	5.6	4.2	5.5
Elderly share (%)	16.0	18.4	16.0	3.7	3.8	3.7
Child share (%)	6.0	5.7	6.0	1.0	0.7	1.0
Black share (%)	28.9	19.4	29.0	17.4	12.3	17.5
Hispanic share (%)	5.7	5.3	5.7	4.7	2.6	4.7
N	810	9	785			

Notes: Population in thousands; median household income in thousands of dollars; all share variables as percentages. County characteristics from 2015 ACS 5-year estimates. "Treated" denotes county–storm observations inside the NHC forecast cone that received a mandatory evacuation order; "Not Evac." denotes counties that did not receive an evacuation order.

forecast aside from evacuation, I find that they are quite small compared to the impact of evacuation and do not represent an important violation of the exclusion restriction.

Second, forecast cone entry must predict evacuation decisions. Approximately 53 percent of evacuations occur when the county lies within the 50-mile band around the predicted central track 48 hours before landfall, compared to just 30 percent of non-evacuated county-storm observations. This yields first-stage F-statistics exceeding 120 in the event-study models. The first two columns in Table 3 show the first stage for mortality estimation with and without community controls. Columns (3) and (4) show the corresponding models for evacuation zone levels, where zones range from zero through five; column (4) includes community controls. All specifications include objective storm intensity controls.

A natural concern is instrument strength. Lee et al. (2022) show that for conventional t-ratio inference to have correct size in an IV setting, the first-stage F-statistic may need to far exceed the traditional Staiger–Stock threshold of 10. However, their analysis applies most forcefully in over-identified settings and when endogeneity is severe. In the present case, I follow the framework of Angrist and Kolesár (2024), who revisit inference in the just-identified, single-instrument setting that characterizes this paper. They argue that instead of screening on an arbitrary F-statistic threshold, the relevant criteria are: (1) the instrument moves treatment in the predicted direction, and (2) the degree of endogeneity is moderate rather than extreme. Endogeneity here refers to what Angrist and Kolesár denote ρ: the correlation between the unobserved determinants of the outcome and the unobserved determinants of treatment, after conditioning on controls.[2] The key question is whether, after conditioning on storm fixed effects and storm intensity controls, the remaining drivers of evacuation decisions are strongly correlated with the remaining drivers of mortality and economic outcomes.

[2] Angrist and Kolesár (2024) define ρ as the correlation between the numerator and denominator of the Wald estimator's deviation from the true parameter. Under homoskedasticity, this reduces to the correlation between the structural error ε_i and the endogenous regressor x_i. When $|\rho|$ is small, conventional IV confidence intervals maintain near-nominal coverage even at modest F-statistics; distortions become serious only when $|\rho|$ approaches one.

Table 3: First Stage: Effect of Forecast Cone on Evacuation Orders

	Any Mandatory		Zone Levels	
	(1)	(2)	(3)	(4)
In Forecast Cone	0.0307***	0.0355***	0.0795***	0.0883***
	(0.0082)	(0.0083)	(0.0234)	(0.0237)
Storm Controls	Yes	Yes	Yes	Yes
County Controls	No	Yes	No	Yes
Observations	3,313	3,313	3,313	3,313
R^2	0.196	0.213	0.131	0.139
Kleibergen-Paap F	16.85	22.77	15.23	18.79

Notes: Standard errors clustered at the county level in parentheses. Columns (1)–(2) instrument for a binary evacuation indicator; columns (3)–(4) instrument for a categorical evacuation intensity measure. ***p<0.01, **p<0.05, *p<0.1.

Both conditions are satisfied here. Cone entry is a single binary instrument that raises the probability of evacuation by approximately 3 percentage points beyond what storm severity and baseline community characteristics predict, and does so with the correct sign. The remaining endogeneity—reflecting local discretion in issuing orders, political considerations, and emergency management capacity—operates through channels that are plausibly weakly correlated with the residual determinants of mortality and economic recovery, particularly after conditioning on storm fixed effects, objective storm intensity controls, and baseline community characteristics. Angrist and Kolesár (2024) show that at moderate values of ρ, conventional confidence intervals for just-identified IV maintain near-nominal coverage even when F-statistics fall well below the Lee et al. (2022) threshold. My F-statistics of 14 and 16 in the mortality estimates comfortably exceed this lower bar.

4 Results

4.1 Mortality

Table 4, Panel A presents estimates of the effect of mandatory evacuation orders on excess mortality during hurricanes. Columns (1) and (2) report ordinary least squares estimates, while columns (3) and (4) present two-stage least squares estimates that address the endogeneity of evacuation decisions. In the IV specifications, I instrument evacuation orders with forecast cone entry. Here I consider the existence of an evacuation order on excess mortality regardless of scale. Treatment is defined as the presence of any

evacuation order, and I later allow for different levels of evacuations (Zone A, Zone B, partial county).

Table 4: Effect of Evacuation on Hurricane Mortality

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Panel A: Binary Evacuation Order				
Evacuation Order	7.76**	5.22	-156.64**	-124.72**
	(3.40)	(3.23)	(70.16)	(57.03)
First-stage F			16.85	22.77
Panel B: Zone-Level Evacuation Intensity				
Evacuation Intensity	0.49	0.19	-60.44**	-50.09**
	(1.77)	(1.75)	(28.37)	(24.13)
First-stage F			15.23	18.79
Storm Controls	Yes	Yes	Yes	Yes
County Controls	No	Yes	No	Yes
Observations	3,313	3,313	3,313	3,313

Notes: Standard errors clustered at the county level in parentheses. Columns (1)–(2) report OLS estimates; columns (3)–(4) report 2SLS estimates where evacuation measures are instrumented with forecast cone coverage. Panel A uses a binary indicator for any mandatory evacuation order. Panel B uses an ordinal intensity measure (0–5). Storm controls: distance to realized track, log storm surge, hurricane category indicators, max wind speed. County controls: mobile home share, disability share, no vehicle share, poverty rate, elderly share. ***p<0.01, **p<0.05, *p<0.1.

Column (4) reports my preferred specification. The point estimate indicates that mandatory evacuation orders reduce excess deaths by approximately 120 per 100,000 residents in the month of hurricane impact. This estimate represents the local average treatment effect for counties where evacuation decisions are influenced by forecast cone entry and forecasted distance.

The OLS estimates in columns (1) and (2) are substantially attenuated and statistically insignificant, consistent with positive selection bias: jurisdictions issue evacuation orders precisely when storms are expected to be most severe, biasing OLS estimates toward zero or positive values. The IV estimates, which isolate variation in evacuation orders uncorrelated with unobserved storm severity, reveal substantial mortality reductions.

In addition to considering *any mandatory evacuation*, I also estimate the model to understand marginal effects of an additional region in 4, Panel B. To approximate this linearly, I create an ordinal treatment variable that scales from 0 to 5 to represent the proportion of the county that is evacuated. Thus, the

treatment takes a value corresponding to the level of evacuation, which is tied to the expected level of inundation.

Table 5: Evacuation Order Scope Categories

Value	Evacuated Region	County-Storms
0	No Mandatory Evacuation Order	3,156
1	Mobile Homes and Low-Lying Areas	21
2	Zone A	43
3	Zone B	13
4	Partial County	33
5	Whole County	14

Notes: Observations are at the county-storm level. Evacuation scope increases with value, reflecting progressively larger shares of county population under mandatory evacuation orders. Zone designations follow state emergency management conventions, with Zone A typically representing areas most vulnerable to storm surge. Source: HEvOD.

Under the forecast cone instrument, the coefficient is interpreted as the local average treatment effect of a one-unit increase in evacuation scope for county-storms whose evacuation order level was shifted upward by forecast cone entry. The identifying assumption is now that forecast moves evacuation decision but does not directly impact outcomes conditional on realized distance from the hurricanes track and realized objective storm conditions.

The magnitude of these estimates warrants discussion. A reduction of 90–125 excess deaths per 100,000 is large in absolute terms, but is consistent with what the existing literature implies and with what the research design isolates. First, this is a LATE for complier counties—those induced to evacuate by forecast cone entry—which are disproportionately within 50 km of the storm track and facing Category 3 or higher conditions (Table 1). The estimate does not represent the average effect of evacuation across all hurricane-exposed counties; it captures the effect for communities taking near-direct hits from the most dangerous storms, precisely the setting where evacuation should matter most. Second, the outcome is all-cause excess mortality, which captures the full mortality footprint of a hurricane rather than the narrow set of deaths attributed to storm forces in official statistics. Young and Hsiang (2024) estimate that the average U.S. tropical cyclone causes 7,000–11,000 excess deaths over 15 years—two to three orders of magnitude above the average of 24 in government counts—and Deryugina and Molitor (2020) show that Katrina-affected populations experienced persistently elevated mortality that was reversed only by permanent relocation to lower-risk areas. My estimates capture the acute, concentrated component of this broader mortality burden: the deaths that occur in the month of landfall, in the counties hit hardest, when the counterfactual is sheltering in place.

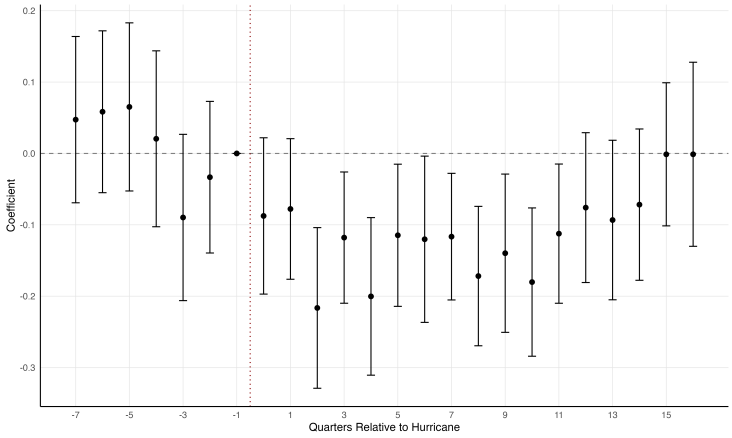
Third, that counterfactual is what makes the magnitude interpretable. The comparison group is not unexposed counties; it is counties experiencing the same realized wind speeds, storm surge, and distance to the storm track *without* an evacuation order. The estimate therefore reflects the difference between leaving and staying during a direct hit—a margin where the mortality stakes are, by definition, highest. Expressed differently, the point estimate implies roughly 1 additional death per 1,000 residents from remaining in place during a severe hurricane, a figure well within the range implied by Shrader et al. (2023), who estimate that the mortality value of accurate forecasts is large precisely because they enable protective actions—chief among them evacuation—that avert deaths during peak exposure. The OLS estimates, which are attenuated toward zero by the positive selection of evacuation into the most dangerous storms, further confirm that the IV is recovering an effect for the right margin: jurisdictions that evacuate face the worst storms, and only by isolating forecast-driven variation can we observe the lives saved.

4.2 Economic Impacts

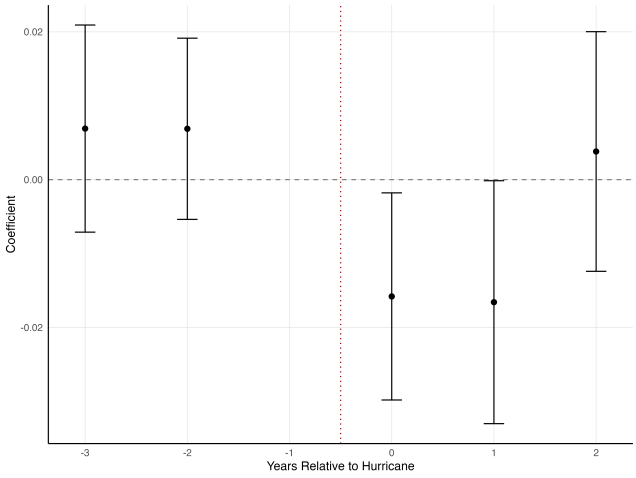
In addition to finding that evacuation orders save lives, I find that they have important lasting economic impacts for up to three years following the hurricane. I present these IV event studies for employment in Figure 5 at the monthly and yearly level, and in Figure 4 for home prices, personal income per capita, and business establishments from Equation (8). Recall that these should be interpreted as the dynamic local average treatment effect of an evacuation order—for those induced to evacuate by being forecasted to receive a direct hit—on outcomes relative to places struck by similar conditions but that did not receive such a forecast and thus did not evacuate. The assumption of parallel trends is well supported across specifications: I fail to reject that any pre-event coefficient differs from zero at the 5% level. In some of the more granular specifications (monthly BLS employment and quarterly median home prices), there is additional noise in the pre-period, but nothing indicating a systematic trend.

Figure 4B shows the estimated effects on income per capita. I find an immediate drop of about 2 percent for evacuated places, which recovers by two years after the storm. A short-term drop in income per capita is arguably unsurprising—mass departures, temporary relocations, and the direct costs of evacuation documented by Gellman et al. (2024) could plausibly depress local demand in the years immediately following.

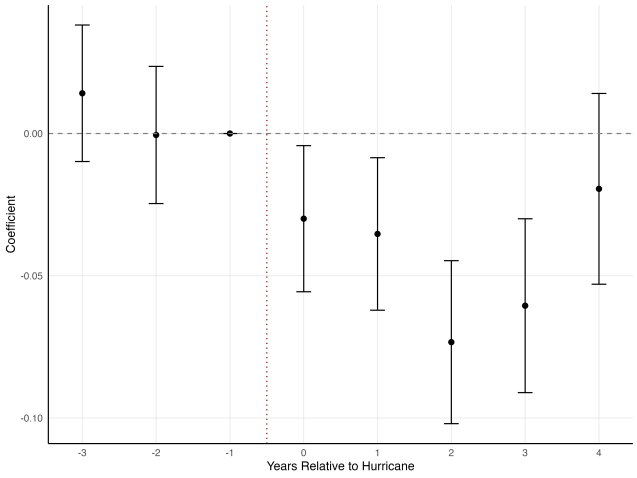
Figure 4: Effect of Evacuation Orders on Local Economic Outcomes



(A) Median Home Prices



(B) Income Per Capita



(C) Establishments

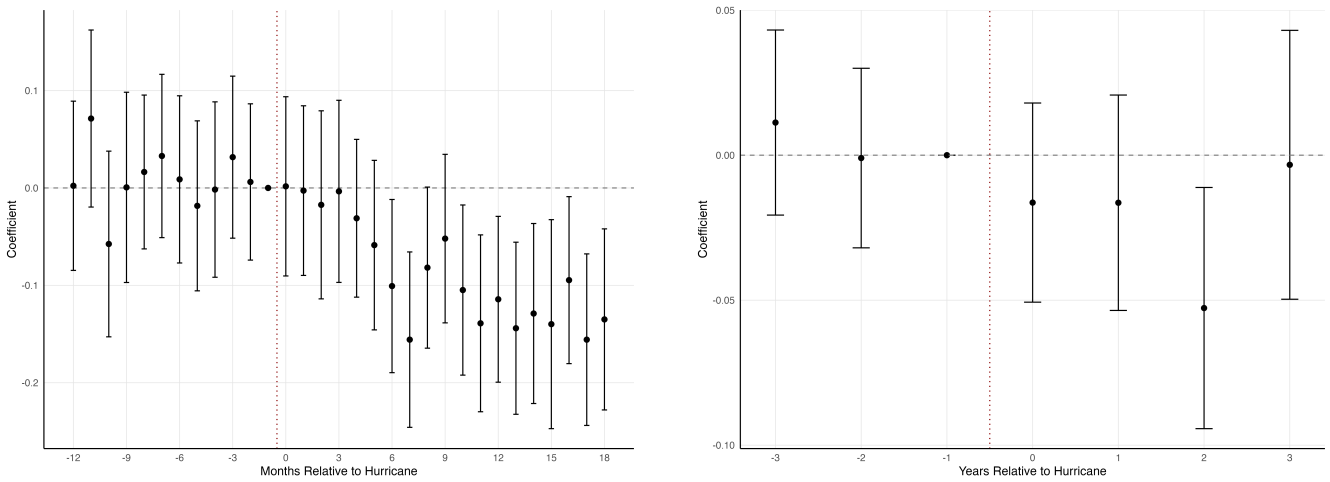
Notes: IV event-study estimates from equation (8), comparing evacuated counties to non-evacuated counties with similar realized storm exposure. Panel A uses quarterly data; Panels B and C use annual data. Event time $t = -1$ is the reference period. Error bars show 95% confidence intervals. Standard errors are two-way clustered by county and state-by-storm. Outcomes are in logs. Sources: HEvOD, SHELDUS, NHC, BEA, Redfin.

The home price results in Figure 4A, which are based on quarterly median home prices, are notable. If evacuation orders merely resulted in the same dynamics as non-evacuated counties, persistent price effects conditional on storm intensity would not be expected. Instead, home prices remain depressed through the fourth year following a storm by about 1-2%. Together with the observed establishment closures, this pattern is consistent with a sustained decline in local demand in evacuated regions.

I estimate the model for both monthly data from BLS LAUS and the BEA annual data, I find employment losses in the range of 1 to 5 percent, occurring about one year after an evacuation order. This suggests that the demand impacts seen in housing, income per capita, and business establishments occur either before or in tandem with a drop in employment. The monthly estimates are more precise; I

find evidence that employment losses in year one peak in the second year after evacuation, then recover by year three, whereas in the yearly results they are significant only at the 10% level in the year of the evacuation and the first year following.

Figure 5: Effect of Evacuation Orders on Employment



(A) Monthly BLS LAUS Data (B) BEA Annual Data

Notes: Notes: IV event-study estimates from equation (8), comparing evacuated counties to non-evacuated counties with similar realized storm exposure. Panel A uses monthly data; Panels B uses annual data. Event time $t = -1$ is the reference period. Error bars show 95% confidence intervals. Standard errors are two-way clustered by county and state-by-storm. Sources: HEvOD, SHELDUS, NHC, BEA, BLS

These findings are particularly striking because the past literature on hurricane impacts suggests that-even if they generate long-term gains—the years immediately following the storm usually see losses in employment, emblematic of short-term labor demand shocks from uninhabitable structures (Roth Tran and Wilson, 2025; Belasen and Polachek, 2008; Boustan et al., 2020). But here I find drops in employment, home prices, and personal income alongside increased business closures, suggesting both labor supply *and* labor demand shocks.

While the findings may be surprising, their scale makes sense in context. A 2 percent decline in income per capita is comparable to the local economic effects of other well-documented shocks in the literature. For example, large manufacturing plant closures reduce local earnings by 1–3 percent for several years (Greenstone et al., 2010), and military base closures reduced local income by similar magnitudes over comparable time horizons (Hooker and Knetter, 2001). The employment effects I estimate—losses of 1 to 5 percent peaking in the second year—are likewise consistent with the adjustment dynamics observed following major employer exits or trade shocks (Autor et al., 2013). The key distinction is that evacuation orders represent a policy-induced shock rather than a market-driven one, and unlike plant closures, the initial displacement is temporary by design. That the economic consequences persist de-

spite this suggests that the disruption to local economic activity—through temporary population loss, business interruptions, and the costs of evacuation itself—can set in motion dynamics that outlast the evacuation period. These results highlight that the decision to issue an evacuation order involves a meaningful tradeoff: while such orders save lives, they also impose lasting economic costs on affected communities.

I thus spend the next section investigating these patterns by examining industry-specific impacts, insurance and building permit timing, and the role of evacuation compliance in these dynamics.

4.3 Mechanisms and Compliance

Evacuation orders can affect local economies through several channels. First, evacuation expenses may crowd out local spending, reducing demand in affected areas (Gellman et al., 2024). Second, temporary displacement disrupts labor supply and business operations—workers cannot return immediately, and firms halt production even when physical damage is limited. A third channel—delayed access to recovery resources—may explain the persistent effects I document. If evacuees cannot return promptly, damage assessments are delayed relative to non-evacuated areas. Insurance claims and FEMA applications are filed later, placing evacuated counties at the back of the queue for adjusters, aid disbursement, and contractors. Relief resources flow first to accessible areas, leaving evacuated counties to recover more slowly despite similar damage. I test this queuing mechanism below.

Local Demand form Evacuation Spending. Given the work by (Gellman et al., 2024), I take the first pathway as a given – some of the short-term local demand reduction must be driven by the mean approximately $1000 expense incurred by evacuees. But these losses would not explain the persistent drop in – at the very least – personal income two years beyond the storm. Thus, I will explore other pathways.

Displacement. Using BEA annual data, I find no evidence of population reductions in evacuated counties during the first two post-storm year. As an alternative, higher frequency mobility data has been used to explore compliance Anand et al. (2023) and mobility Gellman et al. (2024) from evacuation orders have been recently studied using cell-phone mobility data. But neither study examines length of evacuation and the persistence, an important dimension for understanding post-evacuation regional economic dynamics. I employ the Advan Weekly Patterns Plus data to explore how visits to public spaces change in the aftermath of an evacuation.

Specifically, I estimate an event study that conditions on wind speed, storm surge, and realized storm

distance (and is robust to the inclusion of Property damage). I explore three trajectories – storms in the region that are not substantially harmed by a hurricane, storms that are damaged but not evacuated (DBNE) and counties that evacuate.[3]

A few clear trends emerge from this analysis. First, regardless of storm impacts, resident mobility in non-evacuated counties remains near or slightly above baseline throughout the storm period, suggesting very little voluntary evacuation absent a mandatory order. Second, in evacuated counties, a sharp drop in resident mobility is visible in the week before and the week of storm impact, with the mobility index falling approximately 10 percent below baseline at t=0. Third, in both evacuated and non-evacuated counties, there is a jump in mobility one week after the storm. For non-evacuated counties, this represents approximately 3 percent above-baseline activity — likely residents engaged in cleanup, supply runs, and storm recovery staff. For evacuated counties, mobility returns nearly to baseline at t=1, before declining again in subsequent weeks. Fourth, by week 10, evacuated counties show 6–7 percent fewer resident POI visits compared to baseline, while non-evacuated counties remain at or slightly above baseline. This persistent gap — even after controlling for wind exposure, storm surge, and distance (and robust to including property damage) — suggests that a meaningful share of evacuated residents have not yet returned. Finally, by week 14, both groups converge near baseline.

The important takeaway here is that there are important dynamics happening in evacuated counties that are unrelated to pure property destruction. If in the weeks following the storm there are fewer residents inspecting their properties, apply for grants, and hiring contractors, then we make expect two equivalently damaged counties have different post-storm trajectories.

Queuing. The preceding results document persistent economic losses in evacuated counties—lower employment, wages, and establishment counts—that are difficult to explain through direct storm damage alone or direct costs alone. I hypothesize that these losses reflect a *queuing mechanism*: evacuation displaces residents, delaying their re-entry into administrative processes—insurance claims, aid applications, building permits—that are prerequisites for reconstruction. Because these processes operate on a first-come, first-served basis, even short delays compound into sustained disadvantages as non-evacuated residents secure contractors, permits, and aid first.

I test this mechanism using three complementary approaches. First, I examine industry-specific employment impacts to identify whether losses concentrate in recovery-related sectors. Second, I use building permit data from Builty to measure whether permit issuance in evacuated counties lags that

[3]This is brief because these are not ready to share

in non-evacuated peers. Third, I use application-level data from FEMA's Individuals and Households Program (IHP)—which provides rental assistance, repair, and replacement funding for damaged properties—to directly estimate whether evacuated residents take longer to apply for aid, conditional on the type and severity of damage to their homes.

The industry-level results provide initial evidence consistent with this mechanism. The decline in construction employment in evacuated counties is counterintuitive: one would expect areas with severe damage—and thus the greatest need for reconstruction—to exhibit the *highest* construction employment following a major hurricane. That the opposite occurs implies the presence of frictions that delay reconstruction activity in evacuated counties, or that rebuilding demand is absorbed by contractors based in neighboring, non-evacuated areas.

I investigate the queuing mechanism directly using two administrative data sources. First, building permit data show that permit issuance in evacuated counties lags non-evacuated counties by several months. Using the IV framework, building permits in complier evacuated counties follow parallel trends with complier non-evacuated counties in the pre-period, but remain depressed 48 weeks after the storm. There is a large expected drop during the evacuation period itself, but no subsequent surge to compensate—permits do not recover to baseline for nearly half a year, and there is no evidence of pent-up demand being released after residents return. That is, in counties with substantial storm damage, building permits persistently lag, consistent with evacuated residents being unable to initiate reconstruction as quickly as their non-evacuated counterparts.

Second, using 6 million individual IHP applications matched to county-storm characteristics, I estimate the effect of evacuation on the time between storm landfall and aid application. Conditional on storm fixed effects, individual home damage type (flood, wind, roof, structural destruction), FEMA-verified property loss, and county-level storm exposure, residents in evacuated counties apply for federal aid approximately 3.5 days faster than those in non-evacuated counties. While this may appear to contradict the queuing hypothesis, it is consistent with evacuation increasing awareness of available assistance: displaced residents are more likely to encounter emergency management infrastructure (shelters, disaster recovery centers) and information campaigns. The relevant comparison for the queuing mechanism is not the *application* itself but rather the downstream bottleneck—once applications from both groups enter the system, the surge in volume from evacuated counties creates processing backlogs that delay inspections, approvals, and disbursements, consistent with GAO findings that FEMA's call center workforce was overwhelmed following the 2017 hurricane season (GAO-20-503). The building

permit evidence confirms that this administrative delay translates into slower reconstruction timelines.

The mechanism implied by these findings is as follows. Resident A and Resident B experience the same type and severity of storm damage. Because of the forecast cone, Resident B's county issued a mandatory evacuation order while Resident A's did not. Although Resident B may apply for aid slightly sooner due to contact with emergency services, the concentration of applications from evacuated counties creates downstream bottlenecks—in FEMA processing, insurance adjusting, and contractor availability. Resident A, whose county did not evacuate, faces less competition for these scarce recovery resources. They secure an insurance adjuster, obtain a building permit, and hire a contractor sooner. This head start compounds: contractors begin work and hire in non-evacuated counties first, generating local multiplier effects that spill over into adjacent industries. Meanwhile, evacuated counties wait—for inspections, for approvals, for available contractors—and this delay propagates through the local economy as lower employment, lower wages, and fewer operating establishments, effects that persist two years after the storm.

Table 6: Effect of Evacuation Orders on IHP Application Delay

	Time Between Storm and Application				
	State FE	Storm+St FE	+ Indiv Dmg	+ County Exp	(5) + SHELDUS Dmg
	(1)	(2)	(3)	(4)	(5)
Mandatory Evacuation	-3.858***	-3.464***	-3.362***	-2.210**	-3.346***
	(1.108)	(0.8113)	(0.7832)	(1.010)	(0.7718)
Observations	6,040,026	6,040,026	5,596,781	2,004,771	6,040,026
Adjusted R^2	0.11083	0.19736	0.19856	0.31251	0.19901
state_fips fixed effects	✓	✓	✓	✓	✓
NAME fixed effects		✓	✓	✓	✓

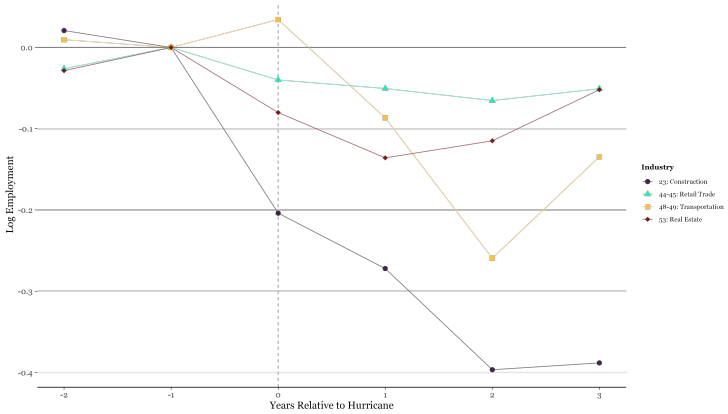
Dep. var: days from incident to IHP application (winsorized 1/99).
Col 3 adds individual damage controls (home/flood/roof damage, destroyed, log FVL).
Col 4 adds county-storm exposure (distance to track, hazard, surge).
Col 5 adds log SHELDUS property damage. Clustered SE at county level.

I provide additional evidence of this stagnated recovery by looking at industry-specific effects. Below, I present IV event-study estimates by industry for employment, pay per worker, and the number of establishments using annual County Business Patterns data. The most pronounced decline occurs in construction employment (Figure 6A, dark purple). A skeptical reader might still believe that even within the IV framework, I am simply not account for unobserved sources of damage. But if that were the case, we would expect construction employment and building permits to increase – not decline –

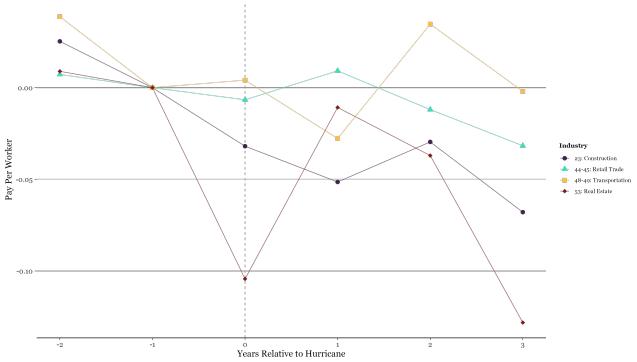
as residents seek to recover to baseline levels. Thus, this decline is notable because we see the opposite effect in building permits in the year following the storm and in construction employment and in the 2 years following the storm.

The logic is straightforward: evacuees do not return to their properties for a week or more. By the time they return, non-evacuees have already filed insurance claims, applied for FEMA aid, and secured contractors. Evacuated counties are thus placed at the back of the recovery queue. Rebuilding is delayed not just a week, but because of contractors, permits are delayed by up to a month and do not surpass non-evacuated counties, despite the expectation of *more* repairs Since recovery from hurricanes takes years, the consequences of this delay—reduced construction spending, lower employment, fewer workable homes—could manifest in lower county income for years. This is especially true given that federal transfers and insurance payouts can represent a substantial share of local economic activity in affected areas. Deryugina (2017) estimates that total post-disaster transfers—including both direct disaster aid and non-disaster social insurance programs—average 780 to $1,150$ per capita in the decade following a hurricane, roughly 1.5 to 2 percent of annual per capita income.

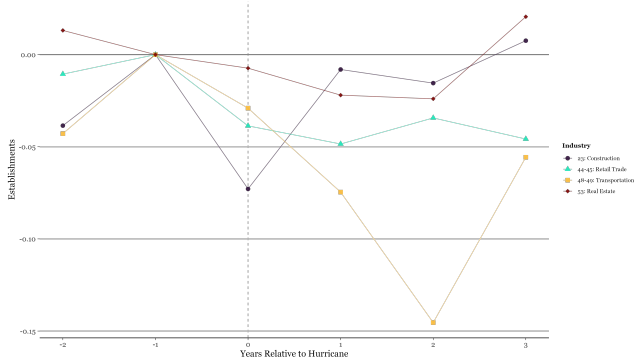
Figure 6: Effect of Evacuation Orders on Industry Outcomes



(A) Employment



(B) Pay Per Worker



(C) Establishments

Notes: Notes: IV event-study estimates from equation (8), comparing evacuated counties to non-evacuated counties with similar realized storm exposure. Event time $t = -1$ is the reference period. Confidence bars are not show for visual clarity. Standard errors are two-way clustered by county and state-by-storm. Outcomes are in logs. Sources: HEvOD, SHELDUS, NHC, CBP.

5 Robustness

This section evaluates the sensitivity of the mortality estimates to instrument strength, influential observations, threats to the exclusion restriction, and alternative specifications. The main finding—that evacuation orders substantially reduce excess mortality—is robust across all checks.

Reduced Form

Table 7 reports the reduced-form effect of forecast cone entry on excess deaths directly, bypassing the first stage entirely. Counties entering the 48-hour forecast cone experience 4.8 fewer deaths per 100,000 with storm controls and 4.4 fewer deaths per 100,000 with the full set of controls. Dividing by the corresponding first-stage coefficients recovers the 2SLS estimates in Table 4. Because the reduced

form imposes no assumptions beyond the exclusion restriction, these estimates would remain valid even under arbitrarily weak instruments. The reduced form also provides additional insight into the "surprise vs. expected" storm concern: at most, roughly 5 deaths per 100,000 are averted by an accurate forecast alone—less than half of even the lower bound of the confidence interval for the IV mortality estimates.

Table 7: Reduced Form: Effect of Forecast Cone on Mortality

	(1)	(2)
In Forecast Cone	-4.81***	-4.43**
	(1.74)	(1.75)
Storm Controls	Yes	Yes
County Controls	No	Yes
Observations	3,313	3,313

Notes: Dependent variable: excess deaths per 100,000 residents. Reduced form estimates of forecast cone entry on mortality (no instrumentation). Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.1.

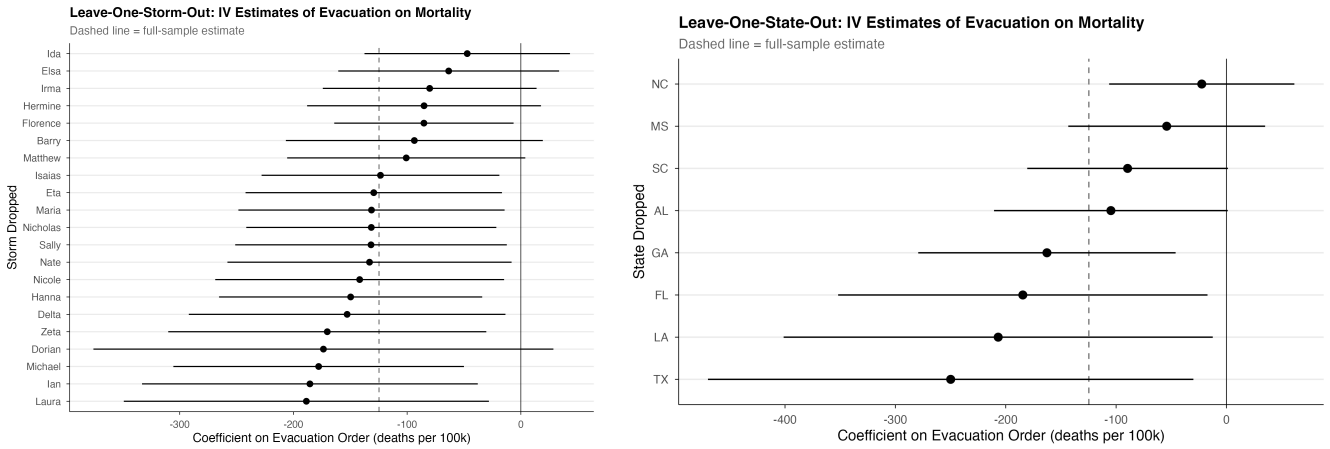
To address instrument strength directly, I compute Anderson–Rubin confidence intervals, which have correct coverage regardless of first-stage strength (). The AR 95% confidence interval for the preferred specification is $[-287, -28]$, compared to the Wald interval of $[-237, -13]$. Both exclude zero. As discussed in Section 3, Angrist and Kolesár (2024) show that in the just-identified case with moderate endogeneity, conventional 2SLS inference maintains near-nominal coverage at F-statistics well below the Lee et al. (2022) thresholds. The AR intervals confirm this.

Sensitivity to Individual Storms and States

I re-estimate the preferred specification dropping each of the 21 storms one at a time. Figure 7(a) reports the results. All 21 point estimates are negative, ranging from -189 to -47. No individual storm moves the estimate outside the full-sample confidence interval. The largest attenuation occurs when dropping storms with few evacuated counties (Ida, Elsa), consistent with reduced identifying variation rather than outlier dependence.

Figure 7(b) repeats the exercise dropping each of the eight sample states. All estimates remain negative. Dropping North Carolina or Mississippi—the states contributing the most observations—

attenuates the estimate modestly; dropping Texas increases it. The result survives dropping Florida, ruling out the possibility that it is driven by Florida-specific emergency management practices or coastal infrastructure.



(a) Leave-One-Storm-Out **(b)** Leave-One-State-Out

Figure 7: Sensitivity of Mortality Estimates to Influential Observations

Notes: Each point reports the 2SLS coefficient on evacuation orders from the preferred specification (Table 4, Panel A, column (4)), re-estimated after dropping the indicated storm or state. Bars denote 95% confidence intervals. Dashed line marks the full-sample point estimate. Standard errors clustered by county.

Instrument Validity

The exclusion restriction requires that forecast cone entry affects mortality only through its effect on evacuation orders, conditional on realized storm conditions. Two pieces of evidence support this assumption.

First, Table A1 reports the relationship between forecast cone entry and predetermined county characteristics, conditional on storm controls. Of nine covariates, four are statistically significant at the 5% level: mobile home share, no-vehicle share, poverty share, and median income. The normalized magnitudes are small—between 3 and 7 percent of the covariate mean—and the joint R^2 of all covariates and storm controls on cone entry is 0.094. These modest imbalances reflect the geographic concentration of evacuation-prone counties along wealthier coastlines rather than selection on unobservables. Critically, the point estimate is stable across specifications with and without these controls (Table 4, columns (3) vs. (4)), indicating that the imbalances do not confound the treatment effect.

Alternative Specifications

Second, I add log per-capita property damage as a control to test whether the instrument is proxying for unobserved storm severity not captured by the distance and intensity measures. Property damage is excluded from the baseline because it is endogenous to evacuation: households that leave cannot board windows, move vehicles, or shut off utilities, and damage documentation differs across evacuated and non-evacuated areas. Column (3) of Table A2 reports the result. The point estimate moves from -125 to -157, and the first-stage F-statistic remains above 14. The insensitivity of the estimate to this control—despite its endogeneity working against finding a result—is strong evidence that the instrument isolates forecast-driven variation in evacuation rather than residual differences in storm exposure.[4]

Two-Way Fixed Effect Weighting

Finally, the event-study specifications face the risk of negative weighting under staggered treatment timing, a well-documented concern with two-way fixed effects estimators. I verify robustness using two recently proposed alternatives that accommodate an instrumental variable. The first follows a (not yet proven version of Gardner (2022), estimating fixed effects on the not-yet-treated sample and regressing the residualized outcome on the instrumented treatment. The second uses the framework of Miyaji (2025), which is more computationally intensive and currently requires balanced panels. Results are robust to both approaches.[5]

6 Decision Model

6.1 The Basic Cost-Loss Framework

Consider the canonical decision problem under uncertainty faced, in this case, by an emergency manager. The manager must decide whether to issue an evacuation order before observing whether the storm will strike. Table 8 summarizes the payoff structure.

In the simplest case, if the probability of inundation p_t is known from forecasts, the manager compares expected costs under each action. The expected cost of evacuating is:

$$\mathbb{E}[\text{Cost} \mid \text{Evacuate}] = p_t \cdot C + (1 - p_t) \cdot C = C. \tag{9}$$

[4]The slight increase in magnitude is consistent with property damage being a "bad control": conditioning on an outcome possibly affected by treatment can amplify rather than attenuate the estimated effect (Angrist and Pischke, 2009)
[5]Tables forthcoming.

Table 8: Basic Cost-Loss Decision Framework

	Inundated	Not Inundated
Evacuate	C	C
Don't Evacuate	L	0

Notes: C denotes the cost of evacuation; L denotes the loss (in lives and property) if the storm strikes without evacuation.

The expected cost of not evacuating is:

$$\mathbb{E}[\text{Cost} \mid \text{Don't Evacuate}] = p_t \cdot L + (1 - p_t) \cdot 0 = p_t L. \tag{10}$$

The manager should evacuate if the expected cost of evacuating is less than the expected cost of not evacuating:

$$C \leq p_t L. \tag{11}$$

Rearranging yields the threshold rule:

$$p_t \geq \frac{C}{L}. \tag{12}$$

This threshold rule reflects the standard tradeoff in self-protection decisions (Ehrlich and Becker, 1972): incur a certain cost to reduce exposure to a probabilistic loss. The meteorological forecasting literature has developed this framework extensively under the name "cost-loss model" (Murphy, 1977; Katz and Murphy, 1997), applying it to decisions about issuing weather warnings, protecting crops from frost, and similar protective actions under forecast uncertainty.

Three features complicate the analysis beyond the simple cost-loss model. First, *imperfect compliance*: not all residents in an evacuated region will evacuate, and some outside the evacuated region do. Second, *heterogeneous risk*: potential losses L vary with storm intensity, local geography, and population vulnerability. Third, *economic disruption*: evacuation orders generate costs beyond direct evacuation expenses, including persistent income losses that my empirical estimates identify.

I now develop a richer model that incorporates these features while preserving the threshold structure of the basic framework, in order to derive a decision rule for hurricane evacuations and explore policy simulations.

6.2 Setup

Consider a county c facing a hurricane threat at time t, where t indexes forecast advisory cycles (typically every 6 hours prior to landfall). The emergency manager observes a strike probability $p_t \in [0, 1]$ derived from the NHC forecast cone, a storm intensity forecast s_t (maximum sustained wind speed, surge height), and time-invariant county characteristics X_c (demographics, infrastructure, baseline health).

The decision maker weighs whether to issue a mandatory evacuation order. I characterize the optimal static decision rule conditional on (p_t, s_t, X_c).

6.3 Benefits: Expected Lives Saved

Let $m_c(s_t, X_c)$ denote the expected excess fatalities in county c if the storm strikes with intensity s_t and no evacuation order is issued. This baseline mortality risk depends on storm surge height, wind speed, housing vulnerability, and demographic composition.

If an order is issued, a fraction $\alpha_{tc} \in [0, 1]$ of the at-risk population N_c complies and evacuates. The expected lives saved conditional on a strike is:

$$\text{Lives Saved}_{tc} = m_c(s_t, X_c) \cdot \alpha_{tc}. \tag{13}$$

Incorporating the strike probability p_t, the ex-ante expected lives saved is $p_t \cdot m_c(s_t, X_c) \cdot \alpha_{tc}$.[6]

6.3.1 Evacuation Compliance

The benefits of an evacuation order depend on the share of residents who actually leave. I treat compliance α_{tc} as an empirical object that I estimate directly from Advan data as described above.

For the cost-benefit analysis, I require predicted compliance $\hat{\alpha}_{tc}$ as an input to the expected lives saved calculation. I estimate a flexible prediction equation of the form:

$$\alpha_{tc} = f(p_t, s_t, \tau, X_c) + \varepsilon_{tc} \tag{14}$$

where τ measures time since the order was issued and X_c includes county-level vehicle ownership, income, and road network capacity. Section 4.3 details the estimation and reports coefficients. I use fitted values based on historical patterns to estimate $\hat{\alpha}_{tc}$ for a given storm-county pair.

[6] It may also make sense to include averted injuries; hospital admissions data may serve this purpose. I leave this extension to future work.

I do not model individual household evacuation decisions or strategic interactions between residents. The prediction equation is sufficient for my welfare calculations because I am not asking how compliance *would change* under policies outside the support of the data—only how observed compliance variation maps to mortality and economic outcomes. I do, however, allow the timing of the order relative to landfall to enter as a parameter in the equation above.

6.3.2 Monetized Benefit

Using the Value of a Statistical Life V, the ex-ante expected benefit of issuing an evacuation order is:

$$B_{tc} = p_t \cdot V \cdot m_c(s_t, X_c) \cdot \alpha_{tc}. \tag{15}$$

This corresponds to $p_t \cdot L$ in the basic framework, where $L = V \cdot m_c(s_t, X_c) \cdot \alpha_{tc}$ now varies with storm characteristics and endogenous compliance. I use the U.S. Department of Transportation's current VSL guidance of $V = \$13.7$ million (2024 dollars) as the baseline, with sensitivity analysis using the range $[\$11.2\text{M}, \$15.0\text{M}]$.

6.4 Costs of Evacuation

Issuing an evacuation order generates direct costs comprising three components. Private per-capita costs k_c capture transportation, lodging, food, and lost wages incurred by evacuating households. Public per-capita costs g_c cover emergency shelters, staffing, and traffic management. A fixed activation cost F_c reflects command center setup and initial communications. The total direct cost for $\alpha_{tc} N_c$ evacuees is therefore

$$C_{tc}^{\text{direct}} = F_c + (k_c + g_c) \cdot \alpha_{tc} \cdot N_c. \tag{16}$$

I set $g_c = \$300$ per capita based on FEMA shelter cost estimates and calibrate k_c following Gellman et al. (2024).

6.4.1 Economic and Health Disruption Costs

The empirical estimates from Section 4.2 show that counties receiving evacuation orders experience per-capita income losses of approximately 2% relative to non-ordered counties, with effects persisting beyond one year. This represents a cost component absent from standard cost-loss applications.

Let $\bar{y}_c$ denote annual per-capita income in county c. The present value of income losses is:

$$C_{tc}^{\text{income}} = \theta(s_t, X_c) \cdot \bar{y}_c \cdot N_c \tag{17}$$

where $\theta(s_t, X_c) \in [0.015, 0.025]$ represents the proportional income loss as a function of storm intensity and county characteristics, calibrated to the IV-DID estimates. I use $\theta = 0.02$ as the baseline, corresponding to a 2% annual income reduction.

This cost component captures multiple channels: direct evacuation-related wage losses, business closures and reduced sales, supply chain disruptions, and reduced investment due to increased uncertainty. The magnitude varies with storm category, coastal proximity, and industry composition—tourism-dependent economies face larger losses.

Evacuations also disrupt healthcare access through nursing home and hospital patient relocations, medication and dialysis interruptions, and postponed procedures. I model this as a per-capita health disruption cost:

$$C_{tc}^{\text{health}} = h_c \cdot \alpha_{tc} \cdot N_c \tag{18}$$

where h_c varies with county health infrastructure and vulnerable population share.

6.4.2 Total Cost

Aggregating across components:

$$C_{tc} = \underbrace{F_c + (k_c + g_c + h_c) \cdot \alpha_{tc} \cdot N_c}_{\text{Direct costs}} + \underbrace{\theta(s_t, X_c) \cdot \bar{y}_c \cdot N_c}_{\text{Economic disruption}}. \tag{19}$$

Unlike the textbook model where C is fixed, total costs here depend on endogenous compliance α_{tc} and vary across counties and storms.

6.5 Decision Rule and Threshold Probability

The emergency manager issues an order if and only if expected benefits exceed expected costs:

$$B_{tc} \geq C_{tc}. \tag{20}$$

Substituting the expressions derived above:

$$p_t \cdot V \cdot m_c(s_t, X_c) \cdot \alpha_{tc} \geq F_c + (k_c + g_c + h_c) \cdot \alpha_{tc} \cdot N_c + \theta(s_t, X_c) \cdot \bar{y}_c \cdot N_c. \tag{21}$$

Solving for p_t yields the threshold strike probability:

$$p_t \geq p^*_{tc} \equiv \frac{F_c + (k_c + g_c + h_c) \cdot \alpha_{tc} \cdot N_c + \theta(s_t, X_c) \cdot \bar{y}_c \cdot N_c}{V \cdot m_c(s_t, X_c) \cdot \alpha_{tc}}. \tag{22}$$

This generalizes the basic cost-loss rule $p_t \geq C/L$. The threshold p^*_{tc} is the ratio of total evacuation costs to potential losses averted—precisely the cost-loss ratio, now enriched with parameters previously unmeasured in emergency response.

6.5.1 Comparative Statics

The threshold framework generates testable predictions about how evacuation decisions vary across contexts. I summarize these as propositions that map directly to the empirical analysis in Section 3.

Storm intensity. Higher s_t increases baseline mortality $m_c(s_t, X_c)$, lowering p^*_{tc}. Stronger storms warrant evacuation at lower strike probabilities.

Population size. Larger N_c raises both costs and benefits. Direct costs and benefits scale proportionally with population, but economic disruption costs scale with N_c while benefits scale with $\alpha_{tc} \cdot N_c$. The net effect on p^*_{tc} is ambiguous and depends on compliance rates.

Income. Higher $\bar{y}_c$ increases economic disruption costs, raising p^*_{tc}. Wealthier counties face higher opportunity costs of evacuation.

Vulnerability. Counties with higher baseline mortality risk—due to geography, housing stock, or demographics—have lower p^*_{tc}.

Economic disruption. The magnitude of θ directly affects optimal policy: if evacuation orders cause larger income losses than previously recognized, optimal thresholds are higher than current practice implies.

6.6 Policy Simulations

I use the threshold framework to conduct two policy exercises.

6.6.1 Revealed Preference: Implied Value of Statistical Life

Emergency managers' observed decisions reveal the value they implicitly place on lives saved. For each evacuation order issued when the strike probability equaled p_t^*, I invert the threshold condition to recover the implied VSL:

$$\hat{V}_{tc} = \frac{F_c + (k_c + g_c + h_c) \cdot \alpha_{tc} \cdot N_c + \theta \cdot \bar{y}_c \cdot N_c}{p_t^* \cdot m_c(s_t, X_c) \cdot \alpha_{tc}}. \tag{23}$$

This exercise asks: given the costs managers incurred and the risks they faced, what VSL rationalizes the decision to evacuate? Comparing $\hat{V}_{tc}$ to the DOT guidance value of $13.7 million indicates whether managers are too cautious (high implied VSL), not cautious enough (low implied VSL), or approximately optimal.

6.6.2 Retrospective Evaluation of Observed Orders

For each historical evacuation order, I evaluate whether the decision was justified at different forecast horizons. At 24 and 48 hours prior to landfall, I observe the NHC forecast (p_t, s_t) and predict compliance $\hat{\alpha}_{tc}$ using the SafeGraph-based prediction equation. I then compute net benefits:

$$NB_{tc} = \underbrace{p_t \cdot V \cdot m_c(s_t, X_c) \cdot \hat{\alpha}_{tc}}_{\text{Expected lives saved}} - \underbrace{F_c + (k_c + g_c + h_c) \cdot \hat{\alpha}_{tc} \cdot N_c + \theta \cdot \bar{y}_c \cdot N_c}_{\text{Total costs}}. \tag{24}$$

An order is retrospectively justified if $NB_{tc} > 0$. This exercise incorporates the economic disruption costs identified in Section 4, which represent new parameters in the cost-benefit calculus. I report the share of historical orders that remain justified under the revised cost estimates and characterize which storm and county features predict unjustified evacuations.

6.7 Implications for Policy Evaluation

This framework clarifies how my empirical estimates inform welfare analysis. The causal effect of evacuation orders on income, $\theta(s_t, X_c)$, enters the cost side of the threshold calculation. Prior work estimating evacuation costs has focused on direct expenses—lodging, transportation, lost wages during the evacuation period itself. My difference-in-differences estimates reveal an additional cost channel: persistent economic disruption that continues well after residents return. My mortality estimation further provides a causally identified estimate of life-saving efficacy.

7 Conclusion

This paper provides the first causal estimates of both the mortality benefits and medium-term economic costs of hurricane evacuation orders. Exploiting quasi-experimental variation from forecast error, I show that mandatory evacuation orders substantially reduce mortality during severe hurricanes while generating persistent declines in local economic activity. These findings quantify a fundamental tradeoff in disaster policy: evacuation orders save lives but can slow economic recovery in affected communities for several years.

The mortality effects are large relative to other public health interventions. For severe storms, evacuation orders reduce excess deaths by roughly 90–110 per 100,000 residents in the month of landfall. This estimate reflects the local average treatment effect for counties induced to evacuate by forecast cone entry, relative to remaining in place during a direct hit from a severe hurricane. Ordinary least squares estimates obscure these benefits, underscoring the importance of isolating forecast-driven variation in evacuation decisions.

At the same time, evacuation orders impose economically meaningful and persistent costs. Relative to similarly damaged counties that did not evacuate, evacuated counties experience declines in employment, personal income, business establishments, and home prices that persist for two to four years. The magnitude of these losses—approximately a 2 percent decline in per capita income and employment reductions of 1–5 percent—is comparable to the local effects of major economic shocks such as large plant closures or trade-induced job displacement, despite evacuation being temporary by design.

The pattern of results suggests that evacuation orders operate through both labor supply and labor demand channels. Unlike prior work on hurricanes, which typically finds short-run employment losses paired with wage increases, evacuated counties experience simultaneous declines in employment, wages, and establishments. Evidence from industry-level outcomes and recovery timing points to a queuing mechanism as a central driver: evacuated areas experience delayed access to building permits, insurance payouts, and contractors, placing them at the back of the recovery queue even when physical damages are comparable.

The decision model developed in this paper integrates these empirical estimates into a cost–loss framework faced by emergency managers. By incorporating persistent income losses alongside direct evacuation costs and mortality risk, the model implies higher optimal evacuation thresholds for lower-intensity storms or wealthier counties, while still strongly favoring evacuation for high-risk storms and

vulnerable populations. This framework provides a transparent and policy-friendly way to evaluate evacuation decisions under forecast uncertainty using empirically grounded parameters.

These findings suggest that recovery policy is a critical complement to evacuation policy. Interventions that expedite permitting, insurance claims processing, and contractor access for evacuated areas could mitigate the recovery delays documented here, preserving the mortality benefits of evacuation while reducing its economic costs. More broadly, the results highlight that disaster policies should be evaluated not only by their immediate effectiveness but also by their downstream economic consequences.

Several limitations warrant note. The mortality analysis relies on publicly available CDC data with limited geographic granularity (for now), and the sample covers hurricanes in eight southeastern states from 2012–2022. In addition, while the evidence is consistent with a queuing mechanism, direct administrative data on recovery processes remain incomplete. Future work using finer-grained mortality and recovery data could further refine these estimates and assess their generalizability. In addition, private insurance data with claim timing could enhance the understanding of these mechanisms

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8 Appendix

Table A1: Balance Test: Effect of Forecast Cone on Pre-Determined Covariates

	Coefficient	Mean	Normalized
Mobile Home Share	0.0155***	0.2113	0.0732
	(0.0043)		
Disability Share	-0.0000	0.0003	-0.0983
	(0.0000)		
No Vehicle Share	0.0030***	0.0613	0.0488
	(0.0009)		
Poverty Share	0.0069***	0.1998	0.0348
	(0.0023)		
Elderly Share	-0.0010	0.1610	-0.0061
	(0.0017)		
Median Income	-1237.2817***	41427.8346	-0.0299
	(380.9859)		
Total Population	-19930.2975	112312.9746	-0.1775
	(15602.6726)		
Black Share	0.0038	0.2787	0.0137
	(0.0063)		
Hispanic Share	-0.0072	0.0799	-0.0903
	(0.0052)		
Observations		3,313	
Joint R^2		0.0942	

Notes: Each row reports the coefficient on forecast cone entry from a separate regression of the covariate on the instrument, controlling for distance to realized track, log storm surge, hurricane category, and max wind speed. Normalized column divides the coefficient by the covariate mean. Standard errors clustered at the county level. 4 of 9 covariates significant at 5%. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table A2: Robustness of Mortality Estimates to Alternative Specifications

	(1)	(2)	(3)
Evacuation Order	-156.64**	-124.72**	-157.37**
	(70.16)	(57.03)	(76.01)
Storm Controls	Yes	Yes	Yes
County Controls	No	Yes	Yes
Property Damage	No	No	Yes
Observations	3,313	3,313	3,313
First-stage F	16.85	22.77	14.17
AR 95% CI		[-287, -28]	

Notes: Dependent variable: excess deaths per 100,000 residents. All columns instrument evacuation orders with forecast cone entry. Column (3) adds log per capita property damage as a control. Columns (4)–(5) cluster standard errors by county and state-by-storm. AR 95% CI reports the Anderson-Rubin confidence interval, which is valid regardless of instrument strength. ***p<0.01, **p<0.05, *p<0.1.



Kernel density estimates for county−−storm observations. Evacuated = In Cone × Evacuation Order issued. Storm surge restricted to coastal counties.

Figure A1: Covariate Distributions by Evacuation Status

Notes: Kernel density estimates for county–storm observations. Evacuated = In Cone × Evacuation Order issued. Storm surge restricted to coastal counties.

Figure A2: Distribution of Per Capita Property Damage, Cat 4 and 5 Only

