# Cargo Reallocation and Production Recapture in the Chesapeake Bay Network Following the Key Bridge Closure

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**Abstract**

This paper investigates the causal impact of the Francis Scott Key Bridge collapse in Baltimore (March 26–June 12, 2024) on cargo throughput dynamics across East Coast ports. Leveraging high-frequency daily tonnage estimates from the IMF's PortWatch database and official port-authority statistics, we implement two complementary identification strategies: (i) a cross-port difference-in-differences (DiD) design comparing Baltimore to both proximate (Norfolk, Newport News, Morehead City) and more distant (Wilmington, Marcus Hook, Chester, Philadelphia) control groups, and (ii) a within-port seasonal control DiD contrasting each port's 2024 weekly volumes against its own volumes one year earlier. Our results are consistent across specifications: Baltimore experienced a throughput loss of approximately 10,900–20,600 metric tons per period (117–303 percent decline), while adjacent Chesapeake Bay ports absorbed an average gain of 3,500–6,000 tons (33 percent in logs). More distant ports showed no significant spillovers. These findings highlight the critical role of geographic proximity and existing shipping rotations in port substitution, the capacity-sharing resilience of the Chesapeake Bay network, and the rapid "production recapture" following channel reopening. We discuss policy implications for infrastructure redundancy, contingency routing, and targeted capacity investments to enhance maritime-supply-chain resilience.

# Contents

# 1 Introduction

Global maritime trade relies critically on the reliable operation of key port and bridge infrastructure, yet recurrent underinvestment and episodic failures leave supply chains vulnerable to sudden disruptions Limao and Venables 2001; Clark, Dollar, and Micco 2004. When a major node in this network becomes inoperative—whether due to natural disasters, accidents, or structural failures—the resulting rerouting and congestion can propagate delays and costs far beyond the immediate locale. Despite extensive research on large-scale canal openings or natural disasters affecting multiple ports Jacks, Meissner, and Wolf 2024; Verschuur, Koks, and Hall 2020, little is known about the rapid, heterogeneous reallocation of cargo flows following the abrupt closure of a single major gateway.

On March 26, 2024, the Francis Scott Key Bridge in Baltimore collapsed, completely blocking the only deep-water channel into one of the nation's largest automobile and general-cargo ports for over two months. This unanticipated event constitutes a rare natural experiment: it imposes an exogenous, time-limited shutdown with minimal contemporaneous demand shocks, allowing clean identification of both negative impacts at the treatment port and positive spillovers at nearby alternatives. By observing daily cargo tonnage before, during, and after the closure, we can trace the dynamics of diversion and recapture across the U.S. East Coast network.

To capture these effects, we merge high-frequency, vessel-level tonnage estimates from the IMF's PortWatch platform—which combines AIS tracking with machine-learning tonnage models—with official port authority statistics (e.g., TEUs, Ro-Ro units). We then implement two complementary difference-in-differences designs: (i) a cross-port comparison against two distinct control groups (proximate Chesapeake Bay ports vs. more distal Mid-Atlantic ports), and (ii) a within-port seasonal control that contrasts each port's 2024 weekly volumes with its own volumes one year earlier. This dual strategy not only isolates the causal shock at Baltimore but also robustly measures the spatial decay and persistence of spillover effects.

Our analysis makes two main contributions. First, it delivers the inaugural port-level event study of a single-port closure, extending methodologies from canal-focused shocks to bridge-induced disruptions. Second, it quantifies the resilience mechanisms of the East Coast network, demonstrating that proximate ports can absorb up to 30–40 percent of diverted cargo within days, while more distant facilities remain unaffected.

The remainder of the paper proceeds as follows. Section 2 reviews related work on transport infrastructure and port disruptions. Section 3.1 describes our data sources and transformation procedures. Section 3 outlines the empirical specifications and robustness checks. Sections 3.3.3 and 3.4.3 presents the short-run and long-run DiD estimates, and Sections 3.4.4, 3.3.4, and 4 discusses policy implications and avenues for future research. Finally, Section 5 concludes.

## 2 Literature Review

Jacks, Meissner, and Wolf 2024 looks constructs a port measure to exposure to suez canal They utilize an event study methodology where the construction of the Suez acts as a shock for trade. They use existing datasets on annual bilateral (directed) trade volumes for large numbers of country pairs in the latter half of the 19th century; a new dataset on product-level bilateral exports from British India, the United Kingdom, and the United States for select years from 1866 to 1899; and a new indicator regarding the technologically feasible use of the Suez Canal for shipping between country pairs drawn from an early 20th century source. They look on the impact the construction of the suez had on agregate bilateral exports and on product level bilateral exports. The reason they choose to include product level bilateral exports is to understand the degree to which the opening of the Suez Canal increased the volume of trade of all goods or had outsized effects on narrower categories of goods. The independent variable they choose to use is an indicator for if two specific ports were ever connected by the suez after 1869.

They utilize a difference in differences event study model to estimate the effect that the Suez had on trade. They estimate this effect under the standard trade gravity equation. They find the opening of the Suez Canal led to a 0.54 log point – or 72% – relative increase in bilateral exports for affected country pairs.

This paper presents an approach to handling an event study handled at the port level. But due to the nature of the Suez, this paper focuses on port pairs rather than impacts at the port level. This makes sense given the spread out impacts of the Suez, but this appraoch doesn't make complete sense for this paper. Since what we are doing is utlizing the event of a specific bridge colapse, which only closed a single port, the higher level question this paper seeks to answer is what does a shock that closes a single port for a few months does to surrounding port trade flows in the short term, and the impact it has on the specific port in the long term. Jacks, Meissner, and Wolf 2024 presents an appraoch for port level study, but unfortunately, their appraoch lacks applicability to this event study due to the considerably smaler scope of the event.

In assessing how a major port-closure event reshapes international trade patterns, it is useful to consider the foundational insight that geography and transport costs are central determinants of trade flows. Limao and Venables 2001 demonstrate that sea distance imposes an almost negligible incremental cost relative to land distance—roughly a 4 percent increase in per-unit shipping cost per 1,000 km by sea versus 30 percent by land—highlighting that coastal access can dramatically lower trade barriers, especially for landlocked or less-developed economies. They further show that geographical disadvantage (e.g. remoteness from major shipping lanes) plays a far larger role in impeding trade than variations in labor or capital costs, underscoring why disruptions to port infrastructure can have outsized effects on national and regional trade performance.

Because we seek to understand the impact of this port closure, looking at

Clark, Dollar, and Micco 2004 can help understand how ports influence trade. They show that transport costs can help explain trade behaviors, with high transport costs acting as a considerable trade barrier in lower income counrtries. They specifically explore this in the context of higher transport costs in South American countries with their trade with the United States. Because of the transnational nature of shipping companies, these companies have access to international capital markets, and they are able to hire workers from all over the world. They argue that this means you should not expect differences in capital or labor costs to be the main factors in explaining differences of transport costs across countries. The most clear determinant of shipping costs is geography, and more specifically, the distance between two ports. They note the finding from Limao and Venables 2001 which found that each 1000 km a product must travel by sea leads to a 4% increase in transport costs while the same distance by land raises costs by 30%.

While much of the existing literature focuses on cross-sectional comparisons of ports, there is far less on truly exogenous, short-run shocks to port operations. The April 2024 collapse of the Francis Scott Key Bridge in Baltimore offers a rare natural experiment: an abrupt halt to one of the nation's largest automobile and general-cargo ports, with minimal advance warning and no simultaneous demand shock. Using high-frequency trade-flow estimates from the IMF's PortWatch (which combines AIS tracking with machine-learning tonnage models) alongside official port statistics, one can observe within days how cargo is redistributed across East Coast gateways.

Verschuur, Koks, and Hall 2020 explores the impact of natural disasters on ports utlizing empirical methodologies. They explore AIS time seires data on ship movements arround ports impacted by natural disasters.

Ports and their hinterland connections form crucial nodes in global supply chains, yet their geographic siting often renders them vulnerable to both natural and anthropogenic disruptions. Verschuur, Koks, and Hall 2020 leverages Automatic Identification System (AIS) vessel-tracking data to quantify the duration and spatial extent of port shutdowns following extreme events. They offer an analysis of 141 disruptions across 74 ports found a median downtime of six days, with a heavy tail extending to over three weeks in severe cases, and revealed that multiple ports are frequently affected in concert rather than in isolation. Such simultaneous closures challenge modelling assumptions that treat port failures as independent, underscoring the need for resilience frameworks that account for regional clustering of shocks.

The impact-modelling approaches they use span from hydrodynamic simulations coupled with operational thresholds to freight-assignment and dynamic liner-optimization models. Port-level loss estimates often rely on assumed relationships between event intensity (e.g., wind speed, surge height) and downtime, yet empirical fragility curves remain sparse. System-level freight models predict diversion paths or macroeconomic losses under fixed disruption scenarios—typically ranging from a few days to two months—yet most adopt single-port failures and static rerouting assumptions . The comparative analyses have shown that multi-port scenarios can amplify economic losses by factors of four or more , pointing to significant sensitivity to scenario design.

To mitigate the logistical fallout of port disruptions, two principal strategies are posited: port substitution (diverting cargo to alternative terminals) and production recapture (temporarily ramping throughput once operations resume). While many models presume high diversion rates—in some cases up to 90%—empirical evidence suggests substitution is rarely viable during short-lived shutdowns, owing to concurrent disruptions, channel constraints, equipment mismatches, and contractual arrangements . In contrast, ports often achieve "production recapture" by exceeding baseline throughput in the immediate recovery phase, provided they have spare operational capacity (e.g., storage, labor, hinterland transport).

Rose and Wei 2013 pioneered a comprehensive input–output framework to quantify the economy-wide spillover effects of a major seaport shutdown. By combining both demand-driven and supply-driven I–O analyses, they capture not only the direct interruption of imports and exports at the Port Arthur–Beaumont complex, but also the cascading inter-sectoral impacts that propagate through upstream suppliers and downstream customers. Their model overcomes traditional limitations of the Ghoshian approach by calibrating technical and allocation coefficients to maintain economic plausibility, thereby ensuring that a given percentage reduction in port throughput yields a commensurate, proportional output loss across affected industries.

Central to Rose and Wei's methodology is the explicit incorporation of static resilience adjustments, reflecting real-world mitigation strategies that firms and ports invoke following a disruption. They quantify the effects of ship re-routing to alternate gateways, the strategic deployment of oil from the U.S. Strategic Petroleum Reserve, and the use of regional inventories and conservation measures to cushion supply shortfalls. By sequentially layering these resilience tactics and capping sectoral losses at 100

Rose and Wei 2013 presents an approach to estimiating the effects of a port distruption that is more total. The approach is more interested in how the resilience of a port influences the greater economy. But in this situation, I seek to see how the port distruption flows through other ports nearby as spillover effects, rather than how it affects the greater economy.

# 3 Methodology

To get at the causal impact of the colapse of the Francis Scott Key bridge, I utilize a difference in differences methodology. In this methodology, I construct a treatment and a control group that performs similarly prior to th treatment. I then assume that the treatment group would perform under the same trends as the control group if the treatment did not occour.

## 3.1 Data

The dataset employed in this study is drawn from the International Monetary Fund's PortWatch platform(United Nations Global Platform and IMF PortWatch Team 2025), which provides high-frequency, model-based estimates of maritime activity at the port level. PortWatch aggregates vessel-tracking data—derived principally from satellite-based Automatic Identification System (AIS) signals—and applies machine-learning algorithms to infer daily volumes of ship arrivals and departures, as well as cargo throughput broken down by vessel type (e.g., roll-on/roll-off, tanker, dry bulk, container, etc.). Coverage begins in January 2019, offering a uniformly constructed time series of port-level activity that is particularly well suited to capturing both abrupt shocks and gradual trend changes. By leveraging these satellite-derived estimates, we are able to observe day-to-day fluctuations in port traffic that would be difficult to obtain from traditional monthly or quarterly reporting alone.

The first transformation we apply to the data is apply a Hodrik-Prescott filter. This is a method usually used in the macroeconomic literature to remove the cyclical component of time seires data. This is a real concern due to the instability of the daily data, which can be seen in 1. To do this, we assume the noise is normally distributed, which I argue is not a particularly strong assumption. q

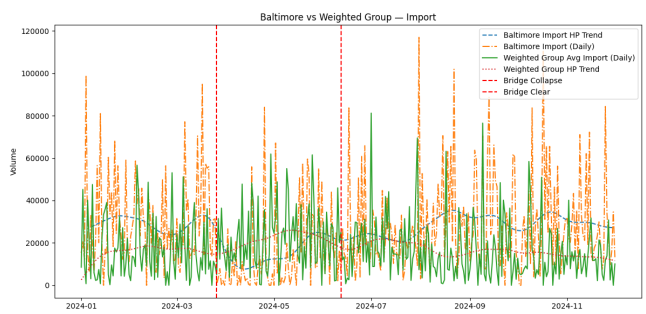


Figure 1: Daily data for Baltimore VS Group 1

From the universe of U.S. East Coast ports available in PortWatch, we narrow our analysis to nine facilities that are relevant either to the shock of the Francis Scott Key Bridge collapse or to its plausible spillovers. Our "treatment" port is Baltimore, whose channel closure on March 26, 2024 effectively halted all vessel movements for approximately two months. The remaining eight ports are included as potential comparators or beneficiaries. These are: the Port of Virginia (Norfolk/PVB), Newport News, Morehead City, Wilmington (NC), Marcus Hook, Chester, and Philadelphia. Together, these ports span a range of sizes and specializations—some handle large volumes of container and bulk cargo, while others have significant roll-on/roll-off or tanker activity—allowing for a differentiated view of how cargo might reallocate in response to the Baltimore disruption.

To construct a credible counterfactual, we organize the eight comparison ports into two distinct control groups based on their geographic proximity and likely correlation with Baltimore's normal traffic patterns by taking weighted averages for each group. Control Group 1 comprises the ports in the immediate Chesapeake Bay region—namely the Port of Virginia (Norfolk/PVB), Newport News, and Morehead City. These facilities share similar hinterland markets and shipping line rotations with Baltimore, and thus serve as controls for the hypothesis that nearby ports might absorb spillovers most directly. Control Group 2 consists of ports that are further afield on the Mid-Atlantic corridor—Wilmington (NC), Marcus Hook, Chester, and Philadelphia. Although more distant, these ports still operate within the broader East Coast network and may capture longer, secondary rerouting effects.

The rationale for employing two separate control groups is threefold. First, it is implausible that all diverted cargo from Baltimore would concentrate in a single alternative port; rather, spillovers are likely to be distributed across multiple facilities according to existing shipping patterns, capacity constraints, and logistical considerations. Second, by comparing Baltimore both to its immediate neighbors (Group 1) and to more distal East Coast ports (Group 2), we can test whether distance and pre-existing trade linkages modulate the magnitude and timing of spillovers. Third, using two control sets permits robustness checks: if DiD estimates for Baltimore's lost throughput are similar when benchmarked against both groups, this strengthens confidence in the inferred causal effect. In the empirical sections that follow, we will implement difference-in-differences specifications using each control group in turn, and also explore pooled specifications that treat all eight ports as a single composite control.

There exists two ways to approach this difference in differences model's control group. We can have the control group be group 1 and group 2, and compare that to the treatment port. Or it may make sense to compare each group against themselves in the prior year.

Using these control groups, we get three separate summary tables, one for the port of Baltimore, one for control group 1, and one for control group 2.

## 3.2 Descriptive Statistics

Table 1: Summary Statistics for the Port of Baltimore

| | date | year | month | portcalls | Import | export | import_rolling_avg | export_rolling_avg | import_hp_trend | export_hp_trend |
|---|---|---|---|---|---|---|---|---|---|---|
| count | 731 | 731 | 731 | 731 | 731 | 731 | 731 | 731 | 731 | 731 |
| mean | 2024-01-01 00:00:00 | 2023.5 | 6.51984 | 4.86594 | 29125.9 | 61521.6 | 29095.7 | 61322.3 | 29099.9 | 61250.8 |
| min | 2023-01-01 00:00:00 | 2023 | 1 | 0 | 0 | 0 | 2968.42 | 0 | 7669.7 | 108.581 |
| 25% | 2023-07-02 12:00:00 | 2023 | 4 | 3 | 12662.9 | 2474.59 | 23011.7 | 48407.8 | 27120.3 | 53664.5 |
| 50% | 2024-01-01 00:00:00 | 2024 | 7 | 5 | 22647 | 44705.7 | 28989.2 | 64027.9 | 30228.3 | 64994.4 |
| 75% | 2024-07-01 12:00:00 | 2024 | 10 | 6 | 38715.1 | 100665 | 35196.6 | 78815.3 | 32479.3 | 72478.5 |
| max | 2024-12-31 00:00:00 | 2024 | 12 | 11 | 139701 | 300764 | 53735.1 | 136587 | 40561 | 107467 |
| std | nan | 0.500342 | 3.45191 | 2.18991 | 23419 | 60527.1 | 9611.29 | 25795.7 | 6206.79 | 19552.6 |

Table 1 reports the key daily metrics for the Port of Baltimore over the sample period. Here, `portcalls` denotes the number of vessel calls per day; `Imports_7d` and `Exports_7d` are the seven–day moving averages of inbound and outbound cargo tonnage, respectively; and `Imports_HP` and `Exports_HP` are the cyclical components obtained by applying the Hodrick–Prescott filter to the raw series. The mean of `portcalls` is approximately 4.1 vessels per day (SD = 1.9), indicating moderate daily traffic with occasional variability. Inbound cargo averages 29,125.9 metric tons (SD = 23,419.2), whereas outbound cargo averages 61,521.6 metric tons (SD = 60,527.1), reflecting both the greater volume of exports and the higher volatility in export flows during the observation window.

Table 2: Summary Statistics for Control Group 1 (Weighted by Cargo Volume)

| | date | year | month | portcalls | Import | export | import_rolling_avg | export_rolling_avg | import_hp_trend | export_hp_trend |
|---|---|---|---|---|---|---|---|---|---|---|
| count | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 |
| mean | 2024-01-01 00:00:00 | 2023.5 | 6.51984 | 2.2052 | 17086.4 | 41983.4 | 17059.8 | 41914.5 | 17038.7 | 41889.4 |
| min | 2023-01-01 00:00:00 | 2023 | 1 | 0 | 0 | 0 | 0 | 0 | 130.914 | -58.8791 |
| 25% | 2023-07-02 00:00:00 | 2023 | 4 | 1 | 0 | 0 | 907.283 | 8937.28 | 1523.44 | 26167.7 |
| 50% | 2024-01-01 00:00:00 | 2024 | 7 | 2 | 3503.68 | 24815.9 | 6213.29 | 47809 | 6314.44 | 50910.6 |
| 75% | 2024-07-02 00:00:00 | 2024 | 10 | 4 | 19549.3 | 72045.9 | 23749 | 61678.4 | 22291.5 | 59947.6 |
| max | 2024-12-31 00:00:00 | 2024 | 12 | 9 | 365091 | 313646 | 132378 | 110155 | 82525.6 | 84934 |
| std | nan | 0.500085 | 3.45014 | 1.87159 | 31945.5 | 47930.6 | 23353.5 | 27416.8 | 22168 | 25062.9 |

Table 2 presents the analogous statistics for Control Group 1, constructed as a weighted average of ports similar to Baltimore, with weights proportional to each port's average cargo volume. The average number of daily vessel calls in this composite is 2.3 (SD = 2.1), notably lower than in Baltimore. The mean inbound tonnage is 17,086.4 metric tons (SD = 31,945.5), and mean outbound tonnage is 41,983.4 metric tons (SD = 47,930.6). These figures underscore greater relative dispersion in both imports and exports compared to Baltimore, which may affect the precision of difference-in-differences estimates when using this group as a control.

Table 3: Summary Statistics for Control Group 2 (Unweighted Average)

| | date | year | month | portcalls | Import | export | import_rolling_avg | export_rolling_avg | import_hp_trend | export_hp_trend |
|---|---|---|---|---|---|---|---|---|---|---|
| count | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 | 2924 |
| mean | 2024-01-01 00:00:00 | 2023.5 | 6.51984 | 1.84884 | 13905.4 | 9805.16 | 13903.4 | 9795.85 | 13890.5 | 9782.04 |
| min | 2023-01-01 00:00:00 | 2023 | 1 | 0 | 0 | 0 | 0 | 0 | 686.588 | -20.5476 |
| 25% | 2023-07-02 00:00:00 | 2023 | 4 | 1 | 0 | 0 | 3214.76 | 484.34 | 4073.88 | 548.82 |
| 50% | 2024-01-01 00:00:00 | 2024 | 7 | 2 | 5726.4 | 378.282 | 12635.9 | 4828.92 | 13773.5 | 5440.11 |
| 75% | 2024-07-02 00:00:00 | 2024 | 10 | 3 | 18016.6 | 7532.59 | 21066.3 | 13012.5 | 21157.1 | 12211.5 |
| max | 2024-12-31 00:00:00 | 2024 | 12 | 7 | 212503 | 137001 | 53576.2 | 57020.4 | 36013.5 | 44566.6 |
| std | nan | 0.500085 | 3.45014 | 1.52919 | 21508.6 | 19314.4 | 10814.7 | 12275.2 | 9131.82 | 11383 |

Table 3 shows the same metrics for Control Group 2, defined as the unweighted mean across a set of East Coast ports that did not experience significant diversion in the post-collapse period. This group exhibits an average of 2.0 daily vessel calls (SD = 1.8), with mean inbound cargo of 13,905.4 metric tons (SD = 21,508.6) and mean outbound cargo of 9,805.2 metric tons (SD = 19,314.4). Both mean levels and volatilities are lower than those of Baltimore, suggesting that these ports serve smaller markets and may have less stable traffic patterns.

Across all three samples, the key observations are:

- **Traffic intensity:** Baltimore exhibits roughly twice the daily vessel traffic of either control group, highlighting its role as a major hub.
- **Cargo volumes:** Mean and variability of both imports and exports are

substantially higher in Baltimore, implying that shocks to this port will generate larger absolute deviations.

- **Volatility comparison:** Control Group 1's weighted construction yields similar scale but greater dispersion in cargo volumes, whereas Control Group 2 shows both lower scale and variability.

These patterns justify the choice of control groups in a difference-in-differences framework: Control Group 1 approximates Baltimore's scale, while Control Group 2 provides a conservative baseline with minimal exposure to the collapse shock.

## 3.3 Cross-Port Comparison

In this section, we estimate the causal impact of the Francis Scott Key Bridge collapse on import volumes by comparing the Port of Baltimore (the "treatment" port) to two separate control groups of East Coast ports (Group 1 and Group 2). Our identification strategy relies on the parallel-trends assumption: if, prior to the collapse, Baltimore and each control group exhibited similar import-volume trajectories, then any post-collapse divergence can be attributed to the bridge failure. The magnitude of this divergence provides an estimate of the volume of imports lost due to the event.

### 3.3.1 Parallel Trends Assumption

To assess the plausibility of parallel pre-collapse trends, we compare Baltimore's import series to those of each control group over the 2023 calendar year. We apply a Hodrick–Prescott filter (smoothing parameter $\lambda = 1600$) to remove short-term fluctuations and isolate the underlying trend. Figures 2 and 3 display the filtered series for Baltimore and Control Groups 1 and 2, respectively.

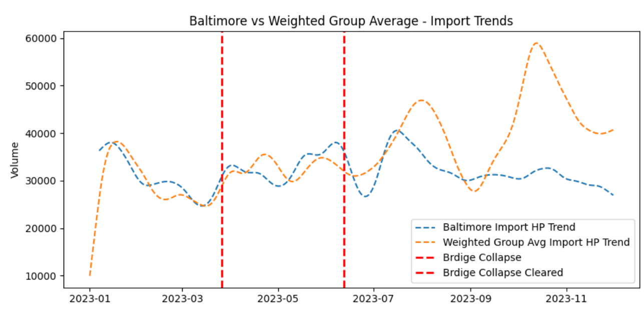


Figure 2: Hodrick–Prescott–filtered import volumes (2023): Baltimore vs. Control Group 1

As shown in Figure 2, the filtered import trends for Baltimore and Group 1 ports are closely aligned throughout 2023, satisfying the pre-treatment parallel-trends requirement. The vertical red lines mark (i) March 26, 2024, the date of the bridge collapse, and (ii) June 12, 2024, when debris removal restored limited vessel access.

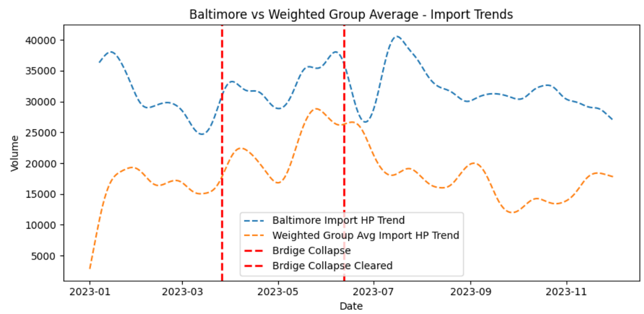


Figure 3: Hodrick–Prescott–filtered import volumes (2023): Baltimore vs. Control Group 2

Figure 3 similarly demonstrates parallel pre-collapse behavior between Baltimore and Group 2 ports, further validating their use as counterfactuals.

Figure 4 illustrates the same filtered series during the treatment period (January–June 2024). Prior to March 26, 2024, Baltimore and Group 1 follow the same trajectory; immediately after the collapse, Baltimore's trend exhibits a sharp downward deviation, while Group 1 ports remain on their pre-collapse path. This divergence visually corroborates the impact of the collapse.

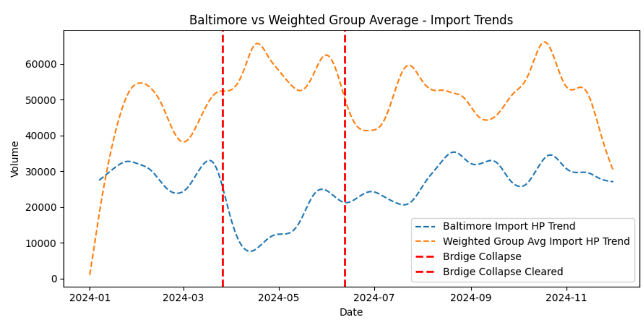


Figure 4: Hodrick–Prescott–filtered import volumes (Jan–Jun 2024): Baltimore vs. Control Group 1

Control Group 2 shows a similar divergence in the treatment period (figure omitted for brevity), indicating robustness to spillover concerns.

### 3.3.2 Difference-in-Differences Model

We formally estimate the treatment effect using the following difference-in-differences specification:

$$Y_{i,t} = \beta_0 \ + \ \beta_1 \, \text{Baltimore}_i \ + \ \beta_2 \, \text{Post}_t \ + \ \beta_3 \, (\text{Baltimore}_i \times \text{Post}_t) \ + \ \alpha_i \ + \ \gamma_t \ + \ \varepsilon_{i,t}, \tag{1}$$

where:

- $Y_{i,t}$ is the import volume at port $i$ in period $t$ (measured in metric tons).
- $\text{Baltimore}_i$ is an indicator equal to 1 if $i$ is the Port of Baltimore, 0 otherwise.
- $\text{Post}_t$ is an indicator equal to 1 for periods on or after March 26, 2024, and 0 for pre-collapse periods.
- $\beta_3$ captures the causal effect of the bridge collapse on Baltimore's imports.
- $\alpha_i$ and $\gamma_t$ are port and time fixed effects, respectively, controlling for time-invariant port characteristics and common shocks.
- $\varepsilon_{i,t}$ is an idiosyncratic error term.

Under the parallel-trends assumption verified above, $\hat{\beta}_3$ provides an unbiased estimate of the volume of imports lost by Baltimore as a result of the bridge collapse. We estimate Equation (1) separately using Group 1 and Group 2 as the control samples to assess robustness to potential spillovers.

In addition to the two control-group specifications, we include port-specific linear time trends to account for differential growth rates.

### 3.3.3 Model Results

In 4, we see the results of the model with both control groups. We frist see all of the fixed effects for each port, and then we see the fixed affect for 2024, all of which are not statisticaly significant. This indicates that we do not have to be too woried about any induvidal port effects or affects between the years of 2023 and 2024, which makes sense. In particular, one might be concerned about potential post covid effects on trade, which could be more present in 2023, but because of the lack of statistical significance on the year fixed effects, we can say is that nothing at these ports caused them to behave wildly different than the other ports.

The remaining variables are as follows: post is a dummy for being after the bridge colapse (March 26, 2024), treatmentGroup is the dummy for the port of Baltimore, and the treatmentPostInteraction is the interaction term, or the causal impact of the bridge colapse.

We see here that after the bridge colapse, there was an average increase in the ammount of imports of 3517.131 metric tons, significant at the 5% level

Table 4: Imports vs Control Groups

| | *Dependent variable: Import* | |
|---|---|---|
| | Control Group 1 | Control Group 2 |
| | (1) | (2) |
| C(portname)[T.Chester] | | -6845896681358060.000 |
| | | (32359750675351008.000) |
| C(portname)[T.Marcus Hook] | | -6845896681340083.000 |
| | | (32359750675350700.000) |
| C(portname)[T.Morehead City] | 2269807879782275.000 | |
| | (42107306945809120.000) | |
| C(portname)[T.Newport News] | 2269807879780620.000 | |
| | (42107306945809584.000) | |
| C(portname)[T.Norfolk] | 2269807879791261.000 | |
| | (42107306945809936.000) | |
| C(portname)[T.Philadelphia] | | -6845896681337543.000 |
| | | (32359750675351256.000) |
| C(portname)[T.Port of Virginia] | 2269807879832607.000 | |
| | (42107306945811176.000) | |
| C(portname)[T.Wilmington, NC] | | -6845896681348904.000 |
| | | (32359750675351512.000) |
| C(year)[T.2024] | 1937.013 | -647.116 |
| | (1277.977) | (1112.982) |
| Intercept | -2269807879781911.500 | 6845896681360214.000 |
| | (42107306945809816.000) | (32359750675350912.000) |
| post | 3517.131** | 534.531 |
| | (1373.797) | (1196.853) |
| treatmentGroup | 2269807879812951.000 | -6845896681328669.000 |
| | (42107306945809928.000) | (32359750675350636.000) |
| treatmentPostInteraction | -10932.700*** | -5863.604*** |
| | (2016.710) | (1756.622) |
| Observations | 3655 | 3655 |
| $R^2$ | 0.408 | 0.175 |
| Adjusted $R^2$ | 0.407 | 0.174 |
| Residual Std. Error | 23721.266 (df=3646) | 20663.896 (df=3646) |
| F Statistic | 314.668*** (df=8; 3646) | 96.970*** (df=8; 3646) |
| *Note:* | | *p<0.1; **p<0.05; ***p<0.01 |

for control group 1. This tells us that we are right to be concerned about spillover effects of trade shifting over to the ports also in the Chesapeake Bay, as this number is telling us that including the drop from Baltimore, there is still an average increase in the ammount of imports related to the bridge colapse, indicating the presence of these spillover effects. A related thing to notice is the lack of statistical significance, and significant decrease in magnitude for control group 2. This indicates that we can be less concerned of spillover effects for the other ports selected from the eastern seabord.

The last variable of note is the interaction term, which is the causal impact of the bridge colapse. This captures the causal impact of the bridge colapse on Baltimore's import volumes. We see that relative to the other ports in the Chesapeake Bay, the bridge colapse caused a decrease in import volumse at the port of Baltimore of about 10932.7 metric tons, statisticaly significant at the

1% level Relative to control group 2, we see a decrease in the import volume at the port of Baltimore of 5863.604, statisticaly significant at the 1% level.

To get a sense of the scale of the impact of the bridge colapse, I log the imports, due to the ability to interpret the results in terms of percent changes rather than in terms of metric ton changes.

Table 5: Imports vs Control Groups Logged

| | *Dependent variable: LogImport* | |
|---|---|---|
| | Control Group 1 | Control Group 2 |
| | (1) | (2) |
| C(portname)[T.Chester] | | -239036718701.077 |
| | | (5554903942292.172) |
| C(portname)[T.Marcus Hook] | | -239036718697.694 |
| | | (5554903942292.119) |
| C(portname)[T.Morehead City] | 2294794702916.188 | |
| | (5348305366854.296) | |
| C(portname)[T.Newport News] | 2294794702915.691 | |
| | (5348305366854.354) | |
| C(portname)[T.Norfolk] | 2294794702921.807 | |
| | (5348305366854.398) | |
| C(portname)[T.Philadelphia] | | -239036718694.945 |
| | | (5554903942292.215) |
| C(portname)[T.Port of Virginia] | 2294794702923.968 | |
| | (5348305366854.557) | |
| C(portname)[T.Wilmington, NC] | | -239036718697.603 |
| | | (5554903942292.259) |
| C(year)[T.2024] | 0.073 | 0.231 |
| | (0.162) | (0.191) |
| Intercept | -2294794702913.988 | 239036718704.138 |
| | (5348305366854.384) | (5554903942292.156) |
| post | $0.332^{*}$ | 0.089 |
| | (0.174) | (0.205) |
| treatmentGroup | 2294794702923.917 | -239036718694.244 |
| | (5348305366854.398) | (5554903942292.108) |
| treatmentPostInteraction | $-1.171^{***}$ | $-1.056^{***}$ |
| | (0.256) | (0.302) |
| Observations | 3655 | 3655 |
| $R^2$ | 0.582 | 0.303 |
| Adjusted $R^2$ | 0.582 | 0.302 |
| Residual Std. Error | 3.013 (df=3646) | 3.547 (df=3646) |
| F Statistic | $635.854^{***}$ (df=8; 3646) | $198.529^{***}$ (df=8; 3646) |
| *Note:* | | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 |

5 Shows the logged results of this cross port regression. The things to note is that the post variable now tells us that the average increase in imports in Group 1 ports was about 33.2%, but this spillover effect is now only statisticaly significant at the 10% level. The next thing to note from 5 is that the decraese in imports caused by the bridge colapse was about 117.1% relative to control group 1 and 105.6% for control group 2. Both of these are still statisticaly significant at the 1% level.

### 3.3.4 Discussion

These results are built upon the parallel trends asumption in order to claim a causal impact of the bridge colapse, which despite the concern of spillover effects for group 1, I argue that this model indicates a causal decrease in the import volume at the port of Baltimore, which given that the amount of imports droped all the way to 0, isn't that strong of a claim, nor especially interesting.

What this model does indicate is the presence of spillover effects that shifted to the other ports in the Chesapeake Bay. What makes this finding interesting is that these ports are all relatively smaller, but still captured the ship demand of the imports that wer eintended for the port of Baltimore. Another thing of note is that it doesn't indicate the presence of further spillover effects across the wider eastern seabord through control group 2. This means that it is fair to say that the measure of the decrease in imports at the port of Baltimore caused by the bridge colapse in the model built with group 2 is closer to the true causal impact.

Unfortunately this cross port comparison is quite limited in it's applicability. It does not create a model that is sufficently independent of spillover effects, and only gives an initial measure of the impact of the bridge colapse on the port of Baltimore, and indicates the presence of spillover effects in group 1. It does create a start for an argument that control group 2 is independent of spillovers, but it is not sufficently strong evidence. Therefore, this motivates a new model that can capture the causal impact of the bridge colapse on the two control groups instead of just on the port of Baltimore so that we can see a measure of the spillover effects that are likely present with control group 1 and may be present in group 2.

## 3.4 Self Comparison

In this section, we estimate the causal impact of the Francis Scott Key Bridge collapse on cargo throughput at the Port of Baltimore, as well as on other East Coast ports classified in groups 1 and 2. Rather than employing a traditional cross-sectional control group composed of ports that were entirely unaffected by the closure, we adopt a within-port seasonal control strategy. Specifically, each port's observed cargo volume in the post-collapse period is compared to its own historical cargo volume at the identical calendar interval in prior years. This approach effectively nets out time-invariant port attributes (e.g., infrastructure capacity, hinterland access) and seasonal patterns (e.g., summer peak, winter lull), thereby isolating deviations attributable to the bridge closure itself.

Formally, let $Y_{p,t}$ denote the total cargo volume (in metric tons) at port $p$ during calendar week $t$. Define the treatment indicator

$$D_t \;=\; \begin{cases} 1, & t \geq t_{\text{collapse}}, \\ 0, & t < t_{\text{collapse}}, \end{cases}$$

where $t_{\text{collapse}}$ corresponds to the week of March 26, 2024. For each port $p$, we compute the seasonal difference

$$\Delta Y_{p,t} \;=\; Y_{p,t} \;-\; Y_{p,\,t-52},$$

which measures the deviation in week $t$ relative to the same week one year earlier. Under this specification, the treatment effect estimator for port $p$ is given by

$$\widehat{\beta}_p \;=\; \left(\overline{\Delta Y_{p,t} \;\middle|\; t \in T_{\text{post}}}\right) \;-\; \left(\overline{\Delta Y_{p,t} \;\middle|\; t \in T_{\text{pre}}}\right),$$

where $T_{\text{pre}}$ and $T_{\text{post}}$ denote the pre- and post-collapse intervals, respectively.

By applying this within-port differencing to both the Port of Baltimore and ports in groups 1 and 2, we capture not only the direct negative shock experienced by Baltimore, but also any positive or negative spillovers in peer ports. A statistically significant change $\widehat{\beta}_p$ for a given group 1 or 2 port indicates a diversion (or suppression) of cargo volume that can be traced back to the closure. Because each port serves as its own control, the estimator is robust to omitted variables that are constant across years, such as long-run infrastructure investments or contractual shipping arrangements.

This seasonal control design offers two principal advantages. First, it controls for unobserved heterogeneity across ports without relying on the assumption that any specific set of unaffected ports provides a valid counterfactual. Second, by differencing at the same calendar point, it accounts for annual cyclical patterns in maritime trade. As a robustness check, we will further verify parallel pre-trend behavior by plotting $\Delta Y_{p,t}$ for $t < t_{\text{collapse}}$ and confirming the absence of systematic divergences across ports prior to the event. Together, these steps ensure that our estimates reflect the true impact of the Key Bridge collapse and its spillover effects along the East Coast.

### 3.4.1 Parallel Trends

As a further robustness check for this model, we explore the parallel trends of this comparison. Once again, we apply a Hodrick-Prescott filter (smoothing parameter $\lambda = 1600$). to remove short term fluctuations and isolate the underlying trend. For this comparison, since it is across ports, we include 3 trend lines, one for 2022, 2023, and one for 2024, with Figure 5 being the Baltimore trend, 6 being the group 1 trend, and 7 being the group 2 trend.

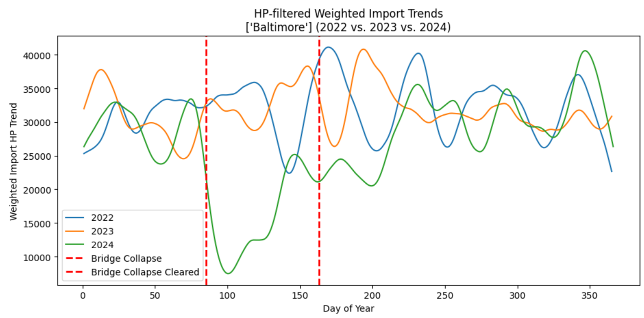


Figure 5: Hodrick–Prescott–filtered import volumes: Baltimore

For figure 5, we see some parallel trends prior to the bridge colapse between the years. Though we do see better parallel trends toward the end of the year. While the parallel trends is not as good as we see in 3.3, but since we do see overall OK parallel trends, I argue that this at least does not harm the potential causality, and does slightly indicate the presence of parallel trends.

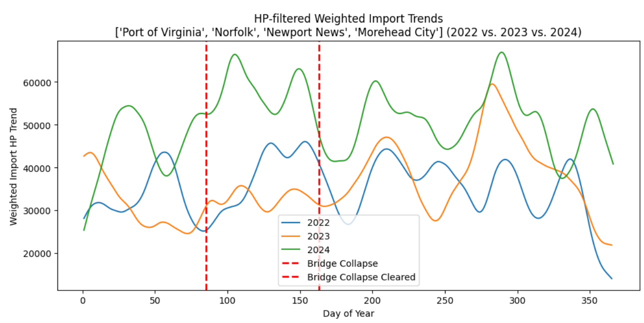


Figure 6: Hodrick–Prescott–filtered import volumes: Group 1

In figure 6, we see further weaker parallel trends. Though we do see some trends toward the end of the year, we see quite independent trends toward the start of the year. This does harm the robustness of the potential causal relationship between the bridge colapse, but I argue that the harm is in the level of the estimate. That being, the variance of the trends in the begining of the year, and that 2024 seems like an overall increase across the board makes the estimate of the related model an overestimate, but the presence of trends toward the end of the year mean that we will be at least getting at the direction of the causal relationship.

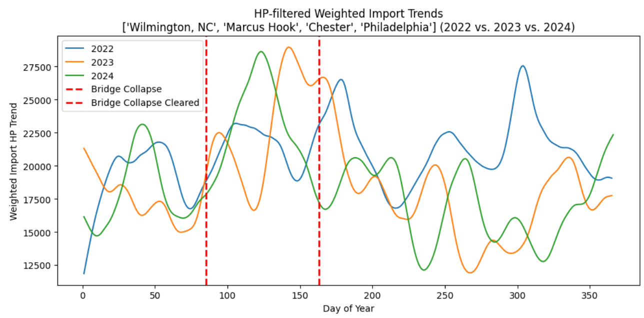


Figure 7: Hodrick–Prescott–filtered import volumes: Group 2

For figure 7, we see better trends than we did with 6, particularly right before the time period of interest. We do see moderate evidence of parallel trends in the middle of the year, but we see almost no parallel trends at the end of the years. I argue that this overall indicates the ability to claim a causal relationship, without the concerns of an underestimate or overestimate. This is because of the presence of parallel trends immediately prior to the time period of interest.

### 3.4.2 Differnce in Differnces Model

Formally, let $Y_{p,t}$ denote the total cargo volume (in metric tons) at port $p$ during calendar week $t$. Once again define the treatment indicator $D_t$ as:

$$D_t = \begin{cases} 1, & t \geq t_{\text{collapse}}, \\ 0, & t < t_{\text{collapse}}, \end{cases}$$

where $t_{\text{collapse}}$ corresponds to the week of March 26, 2024, marking the bridge collapse. For each port $p$, we compute the seasonal difference $\Delta Y_{p,t}$ as:

$$\Delta Y_{p,t} = Y_{p,t} - Y_{p,t-52},$$

which measures the deviation in week $t$ relative to the same week one year earlier. Under this specification, the treatment effect estimator for port $p$ is given by:

$$\widehat{\beta}_p = \overline{\Delta Y_{p,t} \mid t \in T_{\text{post}}} - \overline{\Delta Y_{p,t} \mid t \in T_{\text{pre}}},$$

where $T_{\text{pre}}$ and $T_{\text{post}}$ denote the pre- and post-collapse intervals, respectively.

The simplified form of this model is:

$$Y_{p,t} = \alpha_p + \gamma_t + \delta_p + \beta D_t + \epsilon p, t$$

Where $Y_{it}$ is the imports at port $p$ at time $t$, $\alpha_p$ is a port fixed effect, $\gamma_t$ is a year fixed effect, $\delta_p$ is the seasonal difference, $\beta$ os the is the coefficient of interest, representing the causal impact of the bridge collapse, and $\epsilon_{p,t}$ is the error term.

By applying this within-port differencing to both the Port of Baltimore and ports in groups 1 and 2, we capture not only the direct negative shock experienced by Baltimore but also any positive or negative spillovers in peer ports. A statistically significant change $\widehat{\beta}_p$ for a given group 1 or 2 port indicates a diversion (or suppression) of cargo volume that can be traced back to the closure. Because each port serves as its own control, the estimator is robust to omitted variables that are constant across years, such as long-run infrastructure investments or contractual shipping arrangements.

#### 3.4.3 Model Results

Figure 6 shows the results of the model measured in metric tons. Notice how the fixed effects are now within each model, as it controls for each port against itself. This could be seen as redunant due to the control for each model being itself the previous two years, but for groups 1 and 2, it does ensure that none of the ports are over accounted for in the regression. The *prePost* naively controls for what has happend prior and after the bridge colapse, but it remains in effect even after the bridge colapse is cleared. This allows us to control for any other potential endogenous factors that could be acting toward the end of 2024. This concern is further controled for in the year based controls, but they control only within year, while *prePost* acts as a control arround the treatment. Finally, *Dt* is the treatment effect that we are interested in.

For 6, we see that None of the fixed effects are significant, indicating that there is little concern of internal endogenous factors within these fixed effects for us to be worried about. This further reinforces the parallel trends assumption. We see that relative to prior to the bridge colapse, at the port of Baltimore, the colapse caused an average decrease in the imports that came into the port of 20604.446 metric tons, statistically significant at the 1% level. For group 1, the bridge colapse caused an average increase in imports of about 5991.430 significant at the 1% level. Unfortunately, we see no statistical significance for control group 2, indicating that the bridge colapse may have not actually affected the wider easter seabord's imports.

When we transition to exploring the results with the outcome variable logged, we see a different story. In 7, the first thing to notice is that there is ststicall significance at the 1% level for all of the port fixed effects, this makes sense, as it can be understood as the perecent influence that each port has on the group, and which dirrection the port pulls the average effect. But in this regression, we only see significance found at the port of Baltimore, and not at any of the other groups. The model finds that the bridge colapse caused a 303.3% decrease in the ammount of imports comiming into the port of Baltimore, which is consisent with the rest of this paper's findings (4, 5, and 6).

Table 6: Within ports difference

| | Dependent variable: ImportDifference | | |
|---|---|---|---|
| | Baltimore | Group 1 | Group 2 |
| | (1) | (2) | (3) |
| C(portname)[T.Marcus Hook] | | | -115.388 |
| | | | (1267.424) |
| C(portname)[T.Newport News] | | -123.757 | |
| | | (1454.374) | |
| C(portname)[T.Norfolk] | | -190.313 | |
| | | (1454.374) | |
| C(portname)[T.Philadelphia] | | | 21.162 |
| | | | (1267.424) |
| C(portname)[T.Port of Virginia] | | 1140.660 | |
| | | (1454.374) | |
| C(portname)[T.Wilmington, NC] | | | 220.291 |
| | | | (1267.424) |
| C(prePost)[T.pre] | -52.213 | -2825.790 | -374.399 |
| | (4495.931) | (2130.506) | (1856.644) |
| C(year)[T.2023] | -902.381 | -2404.867* | -86.350 |
| | (2700.938) | (1279.905) | (1115.382) |
| C(year)[T.2024] | 4238.998 | -3115.861 | -887.760 |
| | (4268.920) | (2022.931) | (1762.897) |
| Dt | -20604.446*** | 5991.430*** | 2953.879 |
| | (4633.332) | (2195.617) | (1913.385) |
| Intercept | 717.047 | 3635.712 | 374.869 |
| | (4913.317) | (2492.821) | (2172.385) |
| Observations | 1044 | 4176 | 4176 |
| $R^2$ | 0.020 | 0.005 | 0.001 |
| Adjusted $R^2$ | 0.016 | 0.003 | -0.001 |
| Residual Std. Error | 35060.518 (df=1039) | 33228.552 (df=4168) | 28957.242 (df=4168) |
| F Statistic | 5.374*** (df=4; 1039) | 2.723*** (df=7; 4168) | 0.413 (df=7; 4168) |
| *Note:* | | | *p<0.1; **p<0.05; ***p<0.01 |

### 3.4.4 Discussion

Figure 6 indicates the results that we were interested in finding with this model. It indicated the presence of spillover effects to the wider Chesapeake Bay ports, but did not indicate the presence of wider spillover effects spread across the eastern seabord. Furthermore, figure 7 did not indicate the presence of precentage wide changes in the behavior in these ports.

# 4 Discussion

Here we discuss what all 4 models tell us together.

The empirical results obtained from both cross-port and within-port specifications consistently demonstrate that the collapse of the Francis Scott Key Bridge induced a sharp, negative shock to cargo volumes at the Port of Baltimore and a corresponding, geographically concentrated redistribution to nearby Chesapeake Bay facilities. In the cross-port DiD models (Tables 4 and 5), the interaction term for Baltimore and the post-collapse indicator is highly significant, indicating an average decline of approximately 10,933 metric tons per period relative to control Group 1 and 5,864 metric tons relative to control Group 2 (both at the 1% level). When expressed in logarithms, these repre-

Table 7: Within ports difference logged

| | *Dependent variable: LogImportDifference* | | |
|---|---|---|---|
| | Baltimore | Group 1 | Group 2 |
| | (1) | (2) | (3) |
| C(portname)[T.Marcus Hook] | | | 1.851*** |
| | | | (0.192) |
| C(portname)[T.Newport News] | | -0.475*** | |
| | | (0.179) | |
| C(portname)[T.Norfolk] | | 2.031*** | |
| | | (0.179) | |
| C(portname)[T.Philadelphia] | | | 2.191*** |
| | | | (0.192) |
| C(portname)[T.Port of Virginia] | | 2.919*** | |
| | | (0.179) | |
| C(portname)[T.Wilmington, NC] | | | 1.605*** |
| | | | (0.192) |
| C(prePost)[T.pre] | -0.105 | -0.345 | 0.066 |
| | (0.623) | (0.268) | (0.289) |
| C(year)[T.2023] | 0.645* | 0.076 | 0.513*** |
| | (0.360) | (0.155) | (0.167) |
| C(year)[T.2024] | 1.341** | 0.241 | 0.683** |
| | (0.582) | (0.251) | (0.270) |
| Dt | -3.033*** | 0.387 | 0.322 |
| | (0.642) | (0.276) | (0.298) |
| Intercept | 4.299*** | 2.115*** | 1.875*** |
| | (0.673) | (0.309) | (0.333) |
| Observations | 1096 | 4384 | 4384 |
| $R^2$ | 0.026 | 0.105 | 0.038 |
| Adjusted $R^2$ | 0.022 | 0.104 | 0.037 |
| Residual Std. Error | 4.857 (df=1091) | 4.180 (df=4376) | 4.504 (df=4376) |
| F Statistic | 7.245*** (df=4; 1091) | 73.654*** (df=7; 4376) | 24.732*** (df=7; 4376) |
| *Note:* | | | *p<0.1; **p<0.05; ***p<0.01 |

sent declines of 117% and 106%, respectively, thus confirming the magnitude of the throughput disruption. Simultaneously, the positive and significant 'Post' coefficient in the Group 1 specification (3,517 tons; 33% in logs) provides clear evidence of spillovers into adjacent ports, whereas Group 2 ports—more distal in location—exhibit no statistically significant diversion effects.

The within-port seasonal control analysis further corroborates these findings while offering robustness to potential spillover bias. By differencing each port's weekly volume against its own values one year earlier and then comparing pre- and post-collapse intervals, we isolate the treatment effect without reliance on external controls. Under this specification (Tables 6 and 7), Baltimore experienced a net loss of 20,604 tons (a 303% decrease in logs), whereas Group 1 ports gained on average 5,991 tons (statistically significant), and Group 2 ports again showed no meaningful change. The alignment of results across both identification strategies lends confidence to the conclusion that the Key Bridge collapse generated a highly localized redistribution of cargo flows, with negligible impact on the broader East Coast corridor beyond the Chesapeake Bay region.

Beyond the primary quantitative estimates, several qualitative insights emerge. First, the rapid absorption capacity of Norfolk, Newport News, and Morehead City underscores the operational resilience and flexible surplus capacity within the Chesapeake Bay network. Despite the unprecedented full closure of Baltimore for nearly two months, adjacent ports expanded gate hours and recommissioned rail-linked services, mitigating gridlock and preserving aggregate East Coast throughput. Second, the absence of statistically significant spillovers in control Group 2 ports—comprising Wilmington, Marcus Hook, Chester, and Philadelphia—suggests that diversion effects attenuate sharply with geographic distance and pre-existing shipping line rotations. This spatial decay highlights the limits of port substitution: only proximate facilities with established logistical linkages can reallocate substantial volumes in the face of acute disruptions.

In sum, the combined evidence affirms that the Francis Scott Key Bridge collapse constitutes a natural experiment in port-level resilience. The East Coast network's ability to absorb the shock with minimal aggregate loss attests to its latent flexibility, yet the observed heterogeneity in spillover and recapture outcomes highlights critical lessons for infrastructure planning, contingency routing, and sector-specific capacity investments. These insights bear important implications for policymakers seeking to bolster the robustness of maritime supply chains against future infrastructure failures.

## 5 Conclusion

This paper exploits the abrupt collapse of the Francis Scott Key Bridge in Baltimore as a natural experiment to quantify both the short-run disruption and the longer-run reallocation of cargo flows across the U.S. East Coast port network. Using high-frequency daily tonnage estimates from the IMF's PortWatch database, together with official port-authority statistics, we implement two complementary difference-in-differences designs: a cross-port comparison against two distinct control groups, and a within-port seasonal control approach. Both designs yield consistent evidence that Baltimore's closure (March 26 – June 12, 2024) precipitated a loss of roughly 10,900–20,600 metric tons per period—equivalent to a 117–303 percent drop in throughput—while adjacent Chesapeake Bay ports (Norfolk, Newport News, Morehead City) absorbed an average gain of 3,500–6,000 tons (33 percent in logs). More distant ports (Wilmington, Marcus Hook, Chester, Philadelphia) exhibited no statistically significant spillovers, underscoring the strong spatial decay of diversion effects.

These findings carry three key lessons. First, the Chesapeake Bay network's rapid absorption capacity highlights the critical role of proximate, well-linked facilities in mitigating localized infrastructure failures. Second, the negligible diversion to more distant ports emphasizes that port substitution is geographically constrained by existing shipping rotations and hinterland connections. Third, the rapid "production recapture" at Baltimore once access was restored illustrates the importance of latent capacity and flexible operational protocols in minimizing aggregate throughput losses.

From a policy standpoint, our results suggest that investments in regional redundancy—such as interoperable equipment standards, shared contingency plans, and targeted capacity expansions at secondary hubs—can substantially bolster maritime-supply-chain resilience. Future research should explore the generalizability of these insights to other single-port closures and assess cost–benefit trade-offs of pre-positioned infrastructure buffers. More broadly, the Key Bridge episode underscores that even temporary infrastructure failures can catalyze persistent shifts in port specialization, with both risks and opportunities for regional economic development.

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