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The Economic Impacts of Shipping Disruption in the Panama Canal on U.S. States

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Abstract

This study examines the economic effects of the 2023–24 Panama Canal drought on the United States. Using quarterly state-level data from 2022 to 2025, it applies difference-in-differences and event-study methods to estimate impacts on real GDP, unemployment, industry dynamics and consumer prices. States with high port exposure and trade dependence on canal routes serve as the treatment group. The analysis finds no significant decline in output or employment, suggesting resilience in U.S. transport networks. A modest inflationary effect of 0.66 percent appears in the most exposed regions, indicating that re-routing and logistical flexibility limited real losses while raising prices slightly.

Contents

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1 Introduction

Over 80 percent of global trade by volume is transported by sea (United Nations Conference on Trade and Development, 2024). The stability of the maritime transportation industry, and therefore of international trade, is dependent on a small number of strategic maritime chokepoints. Among these, the Panama Canal occupies a central position. Stretching 80 kilometres, it connects the Atlantic and Pacific Oceans and facilitates around 5 percent of global maritime trade (Panama Canal Authority, 2025). The canal is especially vital for the U.S., carrying much of its trade with Asia and South America (Bureau of Transportation Statistics, 2024), and over 40 percent of U.S. container traffic (Sola, 2025).

The canal's importance extends beyond trade volume. A vessel travelling from the East Coast of the United States to Japan via the canal saves around 3,000 miles compared to the shortest alternative route (Panama Canal Authority, 2025). By shortening shipping routes it lowers transport costs and increases supply chain efficiency. Between 2023 to 2024, drought in the Gatun Lake lowered canal water levels, forcing a reduction in available transit slots (Sola, 2025). With the United Nations Conference on Trade and Development (UNCTAD) (2024), reporting in October 2024 that disruptions in both the Suez Canal and the Panama Canal are straining supply chains, increasing costs and reshaping trade patterns.

The economic implications of disruptions to major maritime chokepoints are well documented. Historical events such as the closure of the Suez Canal have provided natural experiments for assessing the relationship between trade costs, economic growth and welfare. Feyrer (2021), demonstrates that the closure of the Suez between 1967-1975 depressed bilateral trade flows and subsequently national income. Other studies discuss the impacts to global supply chains from natural disasters, such as the 2011 Japanese earthquake (Carvalho et al., 2021; Boehm et al., 2019), and many studies focus on the Covid-19 pandemic's impact (Bonadio et al., 2021). Despite these insights, little empirical work has examined the Panama Canal disruption specifically.

This study analyses whether the drought-induced disruption to the Panama Canal had measurable economic impacts on its largest user, the United States. More specifically, the analysis will focus on impacts to the states and regions most exposed to the canal. It examines whether more port-reliant states experienced greater relative declines in output, employment, and sectoral activity compared to suitable controls, and whether regional consumer price dynamics reflected a transmission of higher transport costs to consumers. Answering these questions complements the existing literature on acute maritime and supply chain shocks, by examining a climate driven, and potentially recurring shock. Focusing on the United States allows for an examination of regional-level impacts and the resilience of its logistical network.

The empirical analysis applies a difference-in-differences (DiD) framework to assess the economic impact of the canal restrictions between 2023Q3 to 2024Q3. Treated states are defined geographically, based on major port exposure, and quantitatively through a trade-exposure index capturing reliance on canal routes. Two-way fixed effects account for unobserved heterogeneity across states and over time, while event-study specifications trace the dynamic evolution of the treatment effects before and after the disruption. Additional insights are drawn from port throughput and intermodal volume data.

The results suggest that the economic effects of the Panama Canal disruption were modest and uneven. Across both the basic and trade exposure models, no significant impacts appeared on state GDP, transport sector activity, or employment, indicating substantial resilience in aggregate output. The main exception is a small, statistically weak rise in unemployment in trade-exposed states, though this effect is sensitive to pre-trends and placebo tests. At the regional level, inflation appears to have risen slightly by around 0.7 percent in treated divisions consistent with delayed pass-through from higher shipping costs. Overall, the disruption produced limited measurable impacts, with adjustments concentrated in labour markets and consumer prices rather than output.

The remainder of this paper is organised as follows: the next section reviews the literature on maritime chokepoints, supply chain resilience and the economic consequences of trade disruptions. Section three outlines the econometric framework, sample construction and the assignment of treated status. Section four presents and discusses the main findings of the DiD estimates for output, labour markets, and consumer prices, alongside robustness checks and alternative specifications. Section four also explores re-routing and resilience using port and intermodal data. Finally, Section five concludes by summarising the key findings and their implications for policy alongside potential areas for further research.

2 Literature Review

Since its opening in 1914, the Panama Canal has stood as one of the most significant transport infrastructure projects in economic history. Constructed at an estimated cost of over $6 billion (in today's dollars), at the time the most expensive public works project in American history (Maurer and Yu, 2008). Prior to its completion, trade between the U.S. East Coast and Pacific markets relied on overland rail transfers or the lengthy Cape Horn route, both of which imposed prohibitive shipping costs on bulk goods. By cutting thousands of miles off major routes, the canal delivered an immediate and permanent reduction in trade costs. Although the First World War delayed the canal's full opening until July 1920 (Maurer and Rauch, 2023), early observers such as Huebner (1915), predicted that once trade recovered traffic would exceed 10.5 million net tonnes, a figure soon surpassed as annual cargo volumes quintupled between 1915 and 1925 (Maurer and Rauch, 2023).

Brancaccio et al. (2020), emphasise the canal's economic significance. They demonstrate that eliminating the canal would reduce total trade by up to 3 percent and have much larger regional impacts, causing trade for North-East America to fall by as much as 28 percent. The U.S. remains the canal's primary beneficiary, with over 40 percent of U.S. container traffic, valued at roughly $270 billion annually, passing through the waterway (Sola, 2025). East and Gulf Coast ports, in particular, depend on the canal to connect with Asian markets, with agricultural exports from New Orleans and Baton Rouge alone amounting to $8.9 billion in 2023 (Bureau of Transportation Statistics, 2024). Maurer and Rauch (2023) find that the canal's opening transformed county-level market potential in a manner comparable to a major trade liberalisation.

The canal's historical trajectory also underscores the vulnerability of global trade to infrastructure constraints. Recent events have highlighted its vulnerability to disruption. Climate-driven droughts between 2023-2024 forced the Panama Canal Authority to restrict transits, auction slots, and mandate lighter vessel loads, creating supply chain frictions reminiscent of prior global trade shocks (Sola, 2025). The UNCTAD (2024b), stated in February of 2024, total transits through the canal had plummeted by 49 percent from its peak.

The economic significance of the Panama Canal lies in its role as a structural reduction in trade costs. By shortening the distance between the Atlantic and Pacific, the canal effectively serves as a natural experiment in the relationship between openness and growth. The seminal contribution of Romer and Frankel (1999) argued that trade has a strong positive causal effect on income. However, their reliance on geographic remoteness as an instrument has been criticized for conflating the effects of trade with other long-run determinants of development (Feyrer, 2021, 2019). Redding and Venables (2004) develop an alternative theoretical trade and geography model to derive evidence that the geography of access to markets is statistically significant in explaining cross-country variation in per capita income

Feyrer (2021) provides a robust identification strategy by exploiting the closure and reopening of the Suez Canal, which created an exogenous shock to shipping distances. His results show that longer transport distances significantly reduced bilateral trade, and that lower trade volumes, in turn, depressed national income. This approach provides strong evidence that changes in distance, the kind directly affected by the Panama Canal, causally affect GDP. The theoretical foundation for this relationship is provided by the gravity model of trade. As summarized by Anderson (2011), the gravity framework explains bilateral trade flows as a function of economic size and trade costs, with transport distance central to resistance. Within this framework, the Panama Canal can be interpreted as a permanent reduction in effective distance, expanding trade flows and raising welfare.

There is extensive literature on transportation infrastructure, such as railways, canals and improved ships, and the lowering of trade costs, and shortening distances supporting the growth of global economic output (Feyrer, 2019; Dunn and Leibovici, 2023; Pascali, 2017; Donaldson, 2018; Bakker et al., 2021). Brancaccio et al. (2020), discussing the importance of infrastructure, suggests that removing the Panama Canal would lead to a decline of 3% in world trade. A major logistics disruption, such as the one affect-ing the Panama Canal, increases trade costs primarily through time and increased freight rates. The literature on shipping costs emphasizes that transit time is a critical component of these costs. Hummels (2007) notes that for many goods, particularly high-value or time-sensitive ones, each day in transit is equivalent to a significant ad valorem tariff. Hummels and Schaur (2013) provide rigorous empirical evidence for this, showing that firms are willing to pay substantially higher freight rates for faster shipping speeds. Therefore, when the Panama Canal drought forces ships to reroute via longer pathways, the resulting increase in shipping time represents a direct and significant economic cost, depressing trade and output.

Firm-level studies show that production networks and input substitution are critical channels for the propagation of shocks. The theoretical framework for this is provided by Acemoglu et al. (2012), who model how idiosyncratic shocks to specific firms can generate aggregate fluctuations through input-output linkages. This network effect has been extensively documented empirically. Studies of major natural disasters show how a disruption to one set of suppliers descend through the economy, with significant negative consequences for downstream industries that rely on their specific inputs (Barrot and Sauvagnat, 2016; Boehm et al., 2019).

The work of Carvalho et al. (2021), studying the Great East Japan Earthquake, is another example of evidence on how a shock to a set of supplier firms cascades through the supply chain. Similarly, Bonadio et al. (2021) use a global input-output model to demonstrate that disruptions in one country's production network can lead to substantial GDP losses in other countries through the severing of these complex supply links. These studies establish the mechanism by which a disruption to the flow of goods through the Panama Canal would be expected to harm the output within the U.S. and its firms, even those that do not trade directly.

Industries do employ adaptive strategies like redundancy and flexibility to counter supply chain instability (Rice and Caniato, 2003; Wu et al., 2007). However, behavioural patterns such as the 'bullwhip effect,' where small demand fluctuations amplify into large order swings up the supply chain, can create massive volatility (Lee et al., 1997; Wu et al., 2007). Major disruptions intensify this uncertainty, forcing widespread tactical adaptations. Despite these known challenges, Wu et al. (2007) suggest only 5 to 25 percent of Fortune 500 companies are adequately prepared for such events.

The structure within the transportation industry itself is also an important factor in responses to shocks. The international shipping industry is not perfectly competitive, with service providers holding market power, which can have the effect of reducing trade (Hummels et al., 2009; Fink et al., 2002). This structure becomes crucial during a disruption. The work of Brancaccio et al. (2020) provides a framework explaining that because the short-run demand for shipping is highly inelastic, a capacity constraint leads to a dramatic spike in freight rates. This creates an ambiguous effect on the sector's economic share; while the quantity of services may fall, the sharp price increase could raise the sector's total revenue and its nominal GDP share.

Trade disruptions such as the Panama Canal drought have consequences that extend beyond output into the labour market. Managerial approaches to supply chain disruptions frequently treat such events as one-off issues instead of ongoing risks, a perspective that exposes domestic workers to international volatility through short-term adjustments like layoffs and reduced hours (Levy, 1995). This tendency is empirically demonstrated by Alessandria et al. (2023), who found that during the 2020–2022 disruptions, firms' inventory management decisions played a key role in transmitting shocks to the broader economy.

Recent empirical work strengthens this argument with direct labour market evidence. Ulate et al. (2025) finds that global supply chain disruptions have potential significant and negative consequences for local labour markets in the United States, reducing employment growth in the most exposed sectors. Similarly Dix-Carneiro and Kovak (2017), show that the effects of major trade liberalization in Brazil, were long term and localised in the regions most exposed. They found that the decrease in formal sector employment in the most exposed regions, lasted for two decades, explaining the persistence of these effects by an immobile labour force.

The evidence from Boehm et al. (2019), and Barrot and Sauvagnat (2016) of supply chain disruptions moving through the production networks, implies that employment effects could extend beyond export orientated firms that rely on the canal. This mechanism mirrors the network logic discussed by Acemoglu et al. (2012), where hub disruptions propagate throughout the economy. The canal's role as a hub for U.S.-Asia trade means its drought-induced disruption functions as a labour market shock with potential wide reach.

The inflationary consequences of trade disruptions are conceptually straightforward: higher transport costs raise import prices, which feed through to producer and consumer prices. Carrière-Swallow et al. (2023) suggests that the role of increasing shipping costs in driving inflation has often been overlooked in academic literature, however as the widespread disruptions of 2020-2022, which saw global shipping costs skyrocket, a global-scale natural experiment was provided and subsequently a small wave of research that empirically verified the link between supply chain pressures and inflation.

Hummels (2007) estimation that each additional day in transit is equivalent to an ad valorem tariff, means the several day re-routing around the Panama Canal imposes an immediate inflationary tax on trade flows. Carrière-Swallow et al. (2023), extend this and show that a doubling of global shipping costs raises headline inflation by about 0.7 percentage points on average, whereas Herriford et al. (2016) estimates a 15 percent increase in shipping costs lead to a 0.10 percentage point increase.

Santacreu and LaBelle (2022), using the Global Supply Chain Pressure Index for the U.S., estimate that pandemic-era disruptions contributed at least 1 percentage point to core goods inflation during 2021–22, with producer prices responding almost immediately and consumer prices with a lag of roughly three months. The findings of Carrière-Swallow et al. (2023), also document a lagging effect where shipping costs continue to raise inflation for over a year even after costs normalize, due to staggered contracts and inventory dynamics. Confirming these findings with a structural model, Bai et al. (2024) show that global supply chain disruptions have clear causal effects on inflation, especially in manufacturing and durable goods sectors.

Beyond global outcomes the literature has examined other types of idiosyncratic supply shocks to firms, such as the early lockdown in China. Lafrogne-Joussier et al. (2022) show how this localized shock propagated through supply chains, creating measurable disruptions downstream. Cevik and Gwon (2024) explicitly study the link between weather anomalies, supply chain pressures, and inflation, finding that unexpected weather events are a significant driver of the supply chain bottlenecks that fuel price increases. This provides a direct conceptual link between the environmental cause of the Panama disruption and its expected economic consequences.

Although the literature provides a rich foundation for understanding how trade disruptions affect output, labour, and prices, it is not fully aligned with the nature of the Panama Canal drought. Much of the research on trade and growth depends on variation that is either long-run and institutional or short-run and acute (Feyrer, 2021; Romer and Frankel, 1999). The drought does not fit neatly into either category, it was neither an instantaneous event like the Suez closure or a permanent policy change. This ambiguity means that existing estimates of trade elasticities, derived from sudden shocks, may misrepresent how output is shaped by more drawn-out disruptions.

Similarly, firm-level network studies convincingly show that supplier shocks cascade through production networks because firms cannot easily substitute inputs (Barrot and Sauvagnat, 2016). However, this evidence is often drawn from abrupt natural disasters like earthquakes and floods, where supply chains collapse almost overnight. The slower nature of the Panama Canal situation could have allowed for gradual adaptations such as inventory management, contract renegotiation, and re-routing. While the conclusion that substitution is costly still applies, the magnitudes and timing of the effect are less clear when firms face a year-long constraint rather than a single catastrophic break.

Similarly in the labour market literature there is a divide between studies identifying enduring employment effects from permanent shocks like trade liberalization (Dix-Carneiro and Kovak, 2017) and those emphasizing temporary volatility from shorter supply chain issues (Alessandria et al., 2023). The drought straddles these categories, it was long enough to trigger regional exposure effects but was not a permanent change. The current evidence, therefore, offers little guidance on whether a disruption of this length should be expected to produce long-term damage or shorter-term volatility.

In parallel, recent inflation research has made strides in quantifying the pass-through from shipping costs to consumer prices (Carrière-Swallow et al., 2023). Yet these studies often treat shipping costs as uniform and exogenous. They tend to abstract away from the market power held by shipping firms, which shapes the distribution and persistence of cost pass-through (Hummels et al., 2009; Fink et al., 2002). By doing so, inflation studies risk underestimating how chokepoint disruptions can amplify price pressures in specific, dependent sectors and regions.

Across these fields, the central problem is that existing empirical models were designed for different kinds of shocks. The Panama Canal drought highlights the need to understand disruptions that are finite but prolonged, recurring, and climate-driven. These events fall into a blind spot, where it is too drawn-out to fit natural-disaster frameworks, yet too temporary to be treated as structural policy changes. Consequently, the literature risks both overstating the immediate, acute costs and understating the distinctive and persistent adjustment dynamics of climate-driven trade bottlenecks.

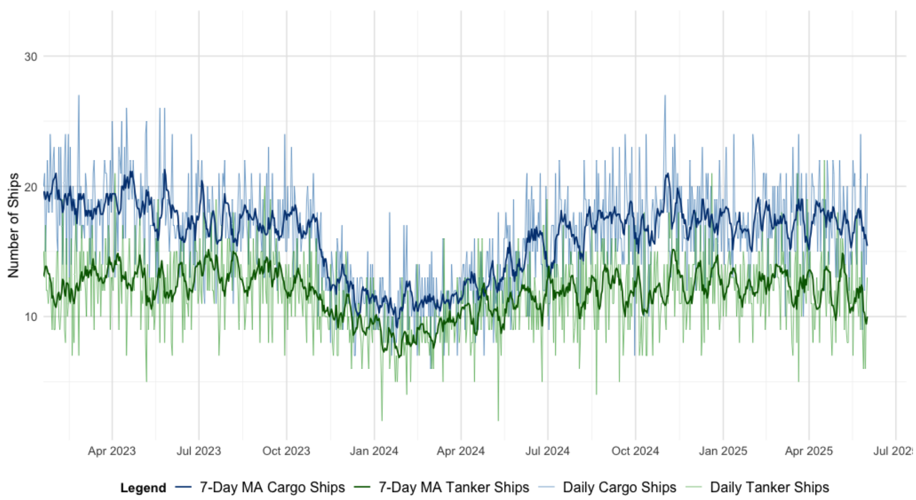
3 Methodology

3.1 Data and Sample Construction

3.1.1 Period of Analysis

The quarterly panel spans 2022Q1–2025Q1, covering six pre-treatment and seven post-treatment quarters around the 2023Q3 Panama Canal disruption. The treatment period begins in 2023Q3, when ship transits dropped sharply due to drought-induced restrictions (Figure 1). The pre-treatment window (2022Q1–2023Q2) provides a credible baseline for testing parallel trends, while the post-treatment period (2023Q3–2025Q1) captures both immediate and lagged responses—well suited for the DiD and event-study analysis.

Figure 1: Panama Canal Daily Transit Calls



Source: PortWatch (2025).

3.1.2 Data Sourcing and Variable Construction

The panel combines quarterly state-level data on output, prices, and labour markets for 2022Q1–2025Q1. Variables were constructed to capture key macroeconomic channels potentially affected by the canal disruption. The dataset ensures consistency across variables and supports uniform estimation across models, full variable details available in appendix Table A1.

3.1.3 Real GDP per Capita

In order to analyse the impact on output suggested in the literature (Dunn and Leibovici, 2023; Feyrer, 2019, 2021), quarterly real GDP data and annual state population estimates from the Bureau of Economic Analysis (2025c,a) were used to compute real GDP per capita; population data were interpolated across quarters. The variable is expressed in natural logs to reduce heteroskedasticity and allow percentage interpretation of coefficients. This measure provides a consistent indicator of state-level economic performance, adjusting for population size and enabling comparison across states.

$$\ln\left(\text{Real GDP per capita}_{s,t}\right) \tag{1}$$

3.1.4 Transportation and Warehousing GDP Share

Industry-level GDP data was again sourced from the Bureau of Economic Analysis (2025b)).

To measure the relative importance of this sector to state economies, transportation GDP was divided by total state GDP:

$$\text{Transport Share}_{s,t} = \frac{\text{Transport GDP}_{s,t}}{\text{Total GDP}_{s,t}} \cdot 100 \tag{2}$$

The transportation and warehousing (T&W) sector is the industry most directly exposed to the canal disruption, making its inclusion essential in analysing industry dynamics. Although the literature focuses on maritime shipping, its findings are directly relevant given the sector's central role in global logistics. Literature shows that shipping demand is highly inelastic (Brancaccio et al., 2020) and that carriers possess significant market power (Hummels et al., 2009). Under capacity constraints, these dynamics can sharply raise freight rates, effects that potentially spill over into the wider T&W sector through higher costs and pricing pressures. Measuring the T&W share of GDP captures both the direct contraction in activity and the indirect transmission of shipping price shocks.

3.1.5 Unemployment Rate

State unemployment rates were obtained from the Federal Reserve Bank of St. Louis (FRED), which aggregates Bureau of Labor Statistics (Federal Reserve Bank of St. Louis, 2025b) Local Area Unemployment Statistics. Monthly unemployment rates were aggregated to quarterly by taking the simple average of the three months in each quarter. The quarterly frequency ensures alignment with GDP and employment data.

$$\text{Unemployment Rate}_{s,t} \tag{3}$$

This variable captures the labour market dimension of the disruption, building on a well established literature that documents the localized employment effects of trade shocks (e.g., Autor et al., 2013; Dix-Carneiro and Kovak, 2017). Canal-related disruptions that constrain trade and production are expected to generate adjustment costs that manifest in higher unemployment, particularly in states more directly exposed to the shock. Including the unemployment rate therefore provides a measure of how the disruption translated into labour market distress at the sub-national level, complementing output and price variables.

3.1.6 Transportation and Warehousing (T&W) Employment

Employment in the transportation and warehousing sector was sourced through the U S Bureau of Labor Statistics (2025b). As with unemployment, the monthly series were aggregated to quarterly averages. The variable was then adjusted for population and transformed into logs:

$$\text{Log Transport Employment per Capita}_{s,t} = \ln\left(\frac{\text{Transport Employment}_{s,t}}{\text{Population}_{s,t}}\right) \tag{4}$$

This variable tracks how employment within the transportation and warehousing sector adjusted during the disruption. Indicating whether firms reduced headcount in response to lower volumes or absorbed the shock through other adjustments. Together with the unemployment rate, this measure provides a complementary view of labour market dynamics, distinguishing between sector-specific adjustments and broader regional employment conditions. For consistency across states, employment is expressed relative to population and log-transformed allowing for proportional comparisons.

3.1.7 Inflation

Inflation was measured using the U S Bureau of Labor Statistics (2025a) Consumer Price Index for All Urban Consumers (CPI-U, Not Seasonally Adjusted). Unlike GDP and employment, CPI data is not available at the state level, only for census divisions. The nine census divisions therefore form the unit of observation in inflation models.

Monthly CPI values were aggregated to quarterly averages. Two dependent variables were then created:

1. **CPI Levels (index)** Quarterly CPI was calculated as the simple average of the three monthly CPI values in each quarter:

$$CPI^{level} = \frac{CPI_{m1} + CPI_{m2} + CPI_{m3}}{3} \tag{5}$$

2. **Log-transformed CPI** To express CPI in approximate percentage terms, the natural log was taken and multiplied by 100:

$$CPI^{log} = \ln\left(CPI^{level}\right) \cdot 100 \tag{6}$$

These variables are modelled to examine the inflationary effects of the supply chain disruption. Prior research shows a strong empirical link between shipping costs, logistics frictions, and consumer price inflation (Santacreu and LaBelle, 2022; Carrière-Swallow et al., 2023), while Cevik and Gwon (2024) connect weather anomalies directly to such pressures. This provides a clear basis for expecting an inflationary impact from the Panama Canal drought. Including both levels and logs enables assessment of absolute and proportional price effects, with the log specification mitigating skew and aiding comparability across regions.

3.2 Treatment Assignment

The credibility of the identification strategy rests on a clear definition of treatment status, which determines which states are considered exposed to the canal disruption and which serve as valid controls.

3.2.1 Basic Model

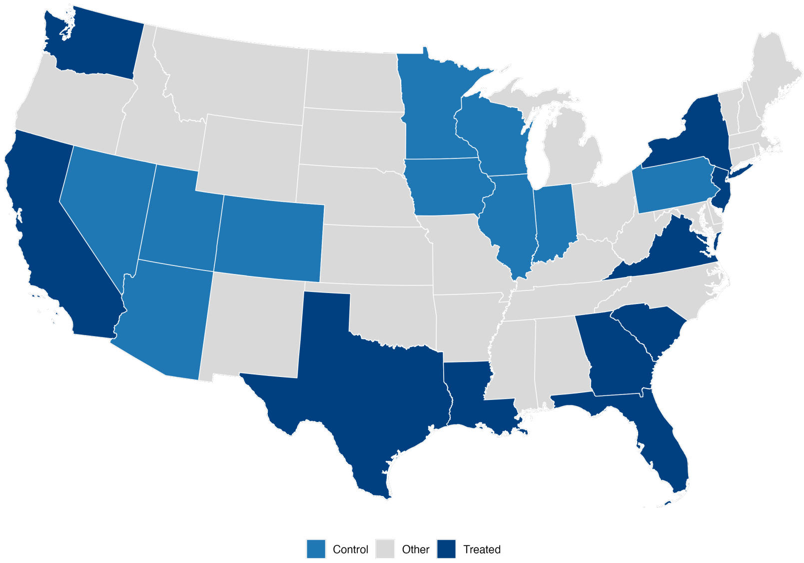


Figure 2: Basic Port Access Model - Treatment & Control groups

The first approach followed a basic geographical criteria. Treated states were defined as those hosting major container ports. Using the U.S. Army Corps of Engineers (2025) dataset on port throughput (Table 1), the ten states with the highest port exposure were identified: New Jersey, Georgia, Texas, Virginia, South Carolina, Florida, Louisiana, Washington, California, and New York.

To remain balanced with the treated group control states were selected as ten states either inland or not within proximity to major ports. These states were plausibly unaffected directly by the canal drought. The states were selected for their similarity in baseline economic structure to the treated group.

To avoid the risk of the control group being dominated by one geographic cluster with unique shocks, the group consists of states within different regions of the U.S.. Alaska and Hawaii were excluded due to their geographical separation from the mainland U.S. and distinct economic structures, and Maryland due to the Baltimore bridge collapse in March 2024, which would confound estimates of canal effects.

Table 1: Top 15 U.S. Water Ports by Total Tonnage, 2023

Rank	Port	Total tonnes (millions)
1	Houston Port Authority, TX	309.5
2	South Louisiana, LA, Port of	217.5
3	Corpus Christi, TX	189.8
4	New York, NY & NJ	132.3
5	Beaumont, TX	87.9
6	Port of Long Beach, CA	85.4
7	New Orleans, LA	75.0
8	Port of Greater Baton Rouge, LA	73.0
9	Virginia, VA, Port of	68.8
10	Lake Charles Harbor District, LA	64.2
11	Port of Los Angeles, CA	55.4
12	Mobile, AL	52.2
13	Plaquemines Port District, LA	50.5
14	Baltimore, MD	49.9
15	Port of Savannah, GA	49.2

Source: U.S. Army Corps of Engineers (2025).

3.2.2 Trade Exposure Model

Recognising that the port-access definition may not fully capture variation in exposure to the canal disruption, an alternative criteria based on trade exposure was constructed. This approach uses trade data to construct a quantitative measure of dependence on the canal, combined with propensity score matching (PSM) to identify a balanced control group.

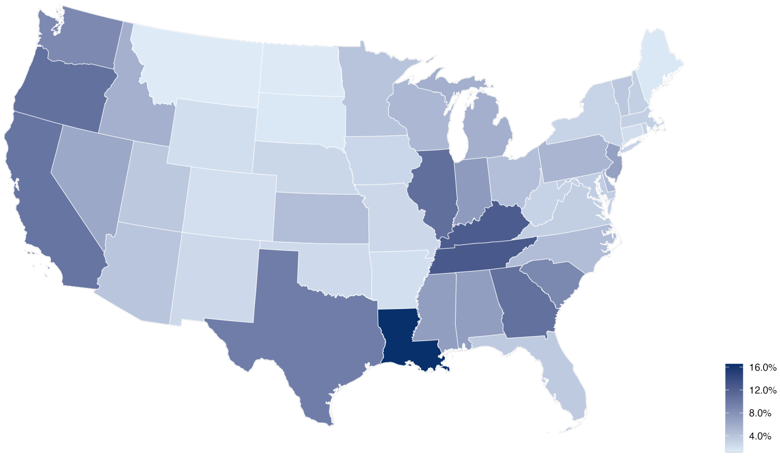


Figure 3: Map of Trade Exposure to the Panama Canal

2022 state-level imports and exports were obtained from U S Census Bureau (2025), categorised by origin and destination. Partner countries most reliant on the Panama Canal were identified (e.g. China, Japan and South American economies). For each state, imports and exports with these partners were summed to create a measure of total canal relevant trade flows. To ensure comparability across economies of different size, this value was normalised by nominal state GDP from 2022 (Federal Reserve Bank of St. Louis, 2025a). The resulting ratio, expressed as a percentage, creates a pre-disruption measure of relative exposure:

$$\text{Trade Exposure}_s = \frac{\text{Imports}_{s,2022} + \text{Exports}_{s,2022}}{\text{GDP}_{s,2022}}, \tag{7}$$

The ten states with the highest exposure ratios were classified as treated, while control states were identified using propensity score matching. A control pool was defined from the remaining states subject to exclusions. This then produced an eligible pool of inland and low-exposure states from which the final control states were drawn from.

Propensity score matching (PSM) was employed to identify states most similar to the treated group on observable characteristics in the pre-treatment period. The procedure was conducted separately for two outcome sets, reflecting the differences in the dynamics of macroeconomic output variables (Log Real GDP per Capita & Transportation and Warehousing GDP Share) and labour market variables (Unemployment Rate & Log Trans-

Table 2: Estimated State Exposure to the Panama Canal (in Billions of USD)

State	Total Canal Trade	GDP	Trade Exposure (%)
Louisiana	49.35	298.42	16.54%
Tennessee	62.28	488.67	12.74%
Kentucky	32.66	261.54	12.49%
Illinois	113.22	1040.40	10.88%
Georgia	83.48	780.00	10.71%
Oregon	31.90	299.00	10.67%
California	379.14	3660.40	10.36%
Texas	236.18	2440.00	9.69%
South Carolina	26.61	301.95	8.81%
Washington	64.60	743.00	8.69%

Source: U S Census Bureau (2025); Federal Reserve Bank of St. Louis (2025a).

portation and Warehousing Employment)

For each set, average values of the relevant outcomes were calculated over the pre-treatment window (2022Q1–2023Q2). These averages served as covariates in a logit regression, with treatment status as the dependent variable. The resulting fitted probabilities represent each state's likelihood of being treated given its pre-treatment characteristics. States in the control pool were then matched to treated states using nearest neighbour matching without replacement. To maintain comparability, exactly ten matched controls were retained for each set.

Following matching, the validity of the parallel trends assumption was assessed. For each outcome, a pre-period regression was estimated in the form:

$$Y_{st} = \alpha_s + \gamma_t + \beta, (\text{Treat}s \times t) + \varepsilon st, \tag{8}$$

where Y_{st} is the outcome for state s in quarter t, and β captures whether pre-treatment trends differed systematically between treated and control groups. Wald tests of joint significance were performed on the interaction terms.

The control groups reported in the analysis therefore consist of states that were statistically comparable to the treated group on pre-treatment averages and which were subsequently tested for parallel trends in the pre-period. Full test results are reported in the appendix (Table A6).

1. **GDP Control Group:** Maine, New Hampshire, New Jersey, Michigan, New Mexico, Wisconsin, West Virginia, Mississippi, Kansas, and New York.

2. **Labour Control Group:** Wisconsin, North Carolina, Indiana, Michigan, New York, Mississippi, Ohio, Pennsylvania, Delaware, and New Jersey.

3.2.3 Inflation Model

The Bureau of Labor Statistics does not publish CPI at the state level, so census division-level data was used, which aggregates states into nine geographic divisions. This meant defining treatment and control groups at the divisional level rather than the state level. Treated divisions were those containing the principal U.S. coastal gateways handling Asian and Latin American shipping flows: Pacific, South Atlantic, West South Central, and Middle Atlantic. Control divisions were selected as New England, East North Central, West North Central, and Mountain. These divisions are more inland-oriented, less reliant on canal shipping, and therefore provide a plausible counterfactual trajectory for price levels.

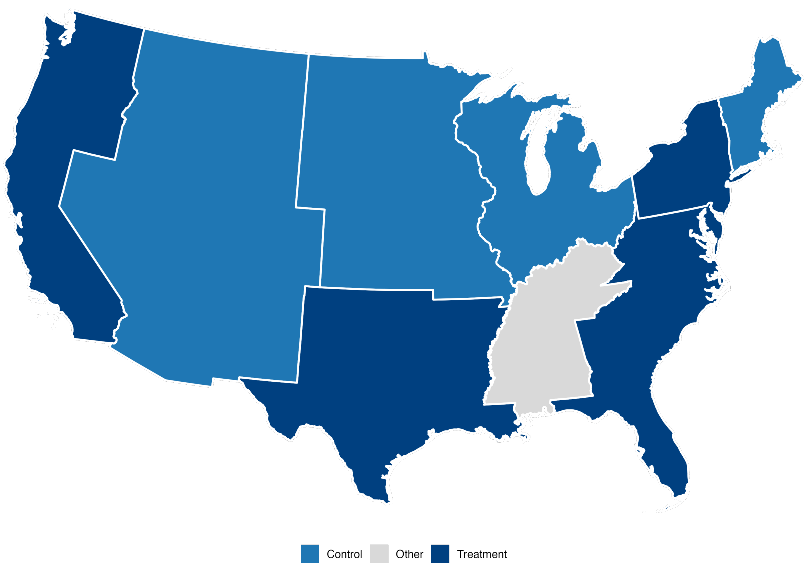


Figure 4: Inflation Model - Treatment and Control groups

This inflation specification complements the state-level models by capturing consumer-level impacts, particularly whether supply chain frictions transmitted into regional price dynamics.

3.3 Summary Statistics

3.3.1 Basic Model

Appendix Table A2 reports summary statistics for the basic port access model. Treated states exhibit slightly higher average log GDP per capita (11.12, ≈\$67,000) and greater heterogeneity than controls (11.06, ≈\$63,000). Unemployment rates are modestly higher in treated states (3.8% and 3.6%), while the T&W)sector accounts for a substantially larger share of GDP (8.0% vs. 4.8%). Log T&W employment per capita, however, is lower in treated states, suggesting a more capital-intensive port and logistics sector. These contrasts highlight the structural differences motivating the treated–control classification.

3.3.2 Trade Exposure Model

Summary statistics for the trade exposure specification are presented in appendix Tables A3 and A4. Across the GDP outcome set, treated states display higher average log GDP per capita and a greater T&W share of GDP, consistent with their higher trade dependence. In the labour outcome set, treated states show slightly higher unemployment and similar T&W employment. Overall, confirming that the most canal-reliant states are more transport-oriented and exposed to shipping-related shocks.

3.3.3 Inflation Model

Appendix Table A5 summarises the CPI data. Baseline consumer price levels are very similar between treated and control divisions. The mean CPI level is 122.96 for treated divisions and 124.16 for controls, while mean log CPI values are 481.12 and 482.07, respectively. The narrow dispersion across divisions suggests broadly comparable inflation environments before treatment, supporting the validity of the DiD models.

3.4 Econometric Specification

3.4.1 Difference-in-Differences Framework

The primary empirical strategy is a difference-in-differences (DiD) design, estimated using a two-way fixed effects (TWFE) specification. First formalized in the policy evaluation literature by Ashenfelter and Card (1985), this approach exploits longitudinal variation to compare changes in outcomes across treated and control units before and after an intervention. The specification controls for unobserved, time-invariant heterogeneity across states and for shocks common to all states, under the identifying assumption that, in the absence of treatment, treated and control states would have followed parallel trends in outcomes.

In the canonical two-period setting, DiD compares pre and post-intervention outcomes for treated and control groups. The framework can be extended to multiple periods and panel data (Goodman-Bacon, 2021), where the TWFE regression is specified as:

$$Y_{st} = \alpha + \beta(Treated_s \times Post_t) + \gamma_s + \delta_t + \epsilon_{st} \tag{9}$$

where Y_{st} is the outcome of interest (e.g., log real GDP per capita or unemployment rate) for state s in period t. $Treated_s$ is a binary indicator for treated states, $Post_t$ is a binary indicator for periods after the onset of the Panama Canal disruption, γ_s are state fixed effects, δ_t are time fixed effects, and ϵ_{st} is the error term. The coefficient of interest, β, identifies the average treatment effect on the treated. Standard errors are clustered at the state level to account for serial correlation within states (Bertrand et al., 2004). Since all treated states experienced the disruption simultaneously and remain treated thereafter, the design avoids the complications associated with staggered adoption or varying treatment timing highlighted in recent critiques (Goodman-Bacon, 2021).

3.4.2 Event-Study Specification

While the TWFE DiD provides an average treatment effect, it masks potential dynamic responses. The disruption to shipping through the Panama Canal may have produced lagged consequences as firms adjusted supply chains. To capture these dynamics, an event-study specification is estimated:

$$Y_{st} = \alpha + \sum_{k \neq -1} \beta_k (Treated_s \times 1\{EventTime_t = k\}) + \gamma_s + \delta_t + \epsilon_{st} \quad (10)$$

where $EventTime_t$ measures the number of quarters relative to treatment ($k = 0$ at 2023Q3). The coefficients β_k trace the evolution of outcomes before and after treatment, relative to the quarter immediately prior ($k = -1$).

Event-studies provide a flexible way to assess the parallel trends assumption, coefficients on pre-treatment leads ($k < 0$) should be statistically indistinguishable from zero if treated and control states evolved similarly before the disruption. The Event-study will also reveal whether the estimated effects emerge immediately or with a delay, and whether they persist or fade over time.

This specification has become standard in applied economics. Card and Krueger (1994) used a two-period version to study minimum wage increases, while more recent work has demonstrated the value of dynamic DiD designs in uncovering both causal effects and violations of identifying assumptions. For example, Redding and Sturm (2008) showed that West German cities closer to the East German border declined only after division, strengthening the causal interpretation. Similarly,Autor et al. (2013) used an event-study framework to document the gradual labour market impacts of rising Chinese import competition in the United States.

3.4.3 Potential Limitations

The TWFE DiD estimator has been the subject of recent criticism, particularly in settings with staggered treatment timing. Goodman-Bacon (2021) shows that when units adopt treatment at different times, TWFE estimates may combine comparisons in ways that introduce bias, including negative weights. Sun and Abraham (2021) further highlight that heterogeneous treatment effects across cohorts can lead to misleading estimates in such designs.

In the context of the Panama Canal however these concers are minimal as all treated states are exposed to the disruption simultaneously in 2023Q3, and treatment persists thereafter. The absence of staggered adoption means the estimator does not suffer from the problematic weighting structure that arises in more complex treatment settings. Robustness checks using event-study specifications and alternative control group definitions are conducted to ensure that the results reflect genuine effects rather than features of the empirical design.

3.5 Identification Strategy and Validation

The credibility of the difference-in-differences design depends on the assumption that, absent the Panama Canal disruption, treated and control states would have evolved along parallel paths. To assess this, a range of diagnostic exercises were implemented.

Event-study specifications were estimated for each outcome, allowing a visual assessment of pre-treatment dynamics. These plots show whether treated and control states followed comparable trajectories prior to the disruption. In addition to visual inspection, formal Wald tests of the joint insignificance of lead coefficients were carried out (Appendix A3). These tests provide a statistical evaluation of pre-trends and reveal mixed results, highlighting both the strengths and limitations of the design.

Placebo exercises were also undertaken to probe the robustness of the results. A falsification DID model artificially shifted the treatment date to 2022Q3 (Appendix A4), and placebo event-studies were estimated under the same assumption (Appendix A5). These exercises help determine whether the empirical strategy detects spurious effects when no treatment should exist. Estimates from the placebo models are mixed some are small and insignificant, lending reassurance that the main findings are not mechanical artifacts of the research design while others provide evidence of a placebo effect.

4 Findings and Discussion

4.1 Basic Model Findings

Table 3 presents the TWFE DiD estimates of the basic model, where treatment is assigned by port exposure. Across all four outcomes the estimates are small in magnitude and rarely statistically significant. Log real GDP per capita increases marginally by 0.0145% at the 10% level, while transport GDP share, and T&W employment per capita remain statistically indistinguishable from zero. The unemployment rate is also not statistically significant, and interpreting the Wald tests and the event-study plots suggests that the identifying assumption of parallel pre-trends is questionable in this case, with some evidence of divergence in the pre-treatment period. This indicates that the unemployment results should be interpreted cautiously, as they may reflect underlying differences between treated and control states rather than the effect of the disruption.

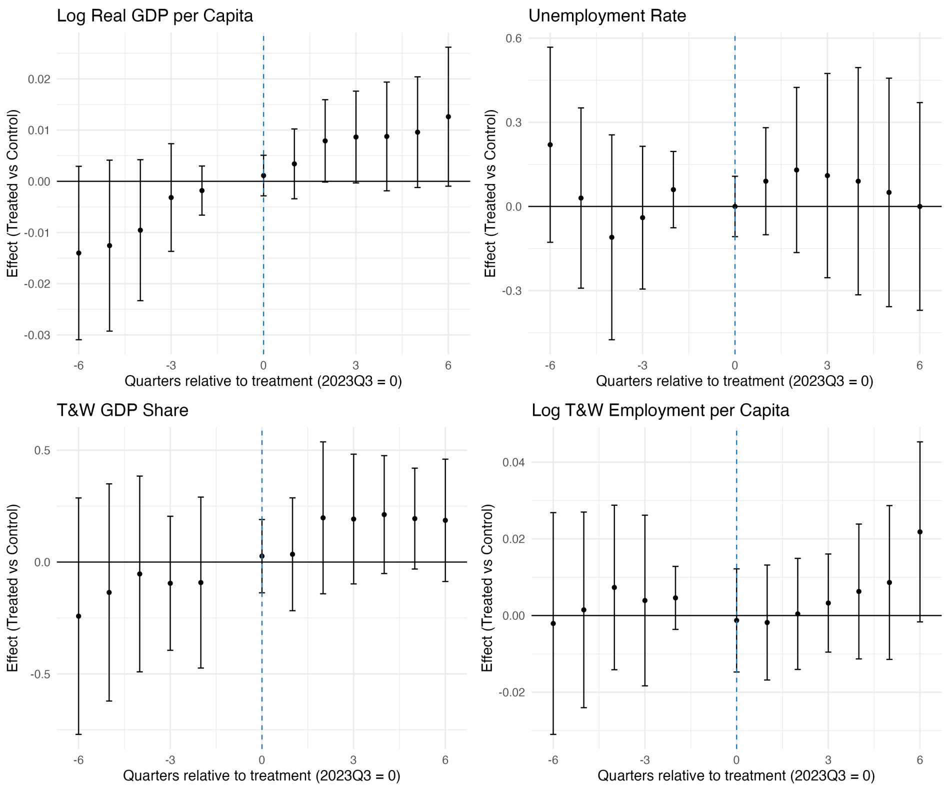
Table 3: Difference-in-Differences Model Results (Basic Model)

Outcome	**Treated × Post**	**Obs.**	**Within R^2**	**Parallel Trends**
Log GDP per Capita	0.0143*	260	0.121	Yes
	(0.0064)			
Unemployment Rate	0.0405	260	0.002	No
	(0.1512)			
Transport GDP Share	0.2521	260	0.039	Yes
	(0.2429)			
Log T&W Employment per Capita	0.0028	260	0.002	Yes
	(0.0115)			

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. The final column reports whether pre-treatment dynamics were broadly consistent with parallel trends (based on Wald tests and event-study figures).
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5 plots event-study coefficients for the four outcomes relative to 2023Q3. For log real GDP per capita, pre-treatment coefficients fluctuate slightly below zero but remain statistically indistinguishable from it, with post-treatment estimates shifting mildly positive yet always within wide confidence intervals. Transport GDP share and log T&W employment per capita both display coefficients centred around zero throughout, with large confidence bands in the case of GDP share and relatively tight bands for employment. By contrast, unemployment exhibits more volatile pre-treatment dynamics, with coefficients above and below zero and broader confidence intervals; this visual instability aligns with the Wald test, which formally rejected the null of parallel pre-trends for this outcome. Post-treatment, none of the outcomes display clear or statistically significant responses to the disruption, reinforcing the interpretation of limited aggregate effects.

Figure 5: Event-Study Estimates of Disruption Impact



Notes: Event-study coefficients (β_k) with 95% confidence intervals. The dashed line marks 2023Q3 (treatment onset). 2023Q2 ($k = -1$) is the omitted reference.

The small, marginally significant increase in log real GDP per capita (0.0143%) runs counter to theoretical expectations of a negative supply shock, which should depress output by raising trade costs (Feyrer, 2021; Brancaccio et al., 2020). Several explanations for this are possible. Aggregate state GDP may mask localized declines, disruptions concentrated in port cities may be offset by other sectors or regions. Alternatively, re-routing through alternative ports may have cushioned the shock, as observed in earlier canal closures (Feyrer, 2019). Higher freight costs may boost nominal activity in transport services, inflating GDP without real productivity gains (Hummels et al., 2009). Taken together, the weakly positive effect is more consistent with measurement and aggregation issues than with genuine economic expansion. This interpretation is further tempered by the placebo results in Appendix A4, which yield a similarly positive and marginally significant GDP effect when the disruption is artificially backdated to 2022Q3, suggesting that part of the baseline estimate may be attributable to underlying pre-trends rather than the disruption itself.

The unemployment regression yields a small and statistically insignificant coefficient (0.0876), suggesting no clear aggregate labour market adjustment to the disruption. However, these results must be interpreted with caution given evidence of pre-trends. The placebo regression similarly produces an unstable but insignificant coefficient of opposite sign, reinforcing the interpretation that the labour market results are highly sensitive to specification and do not provide reliable evidence of disruption effects. The direction of the baseline estimate is nonetheless consistent with broader evidence that aggregate unemployment responses to trade shocks tend to be muted, as firms often adjust margins other than headcount (Alessandria et al., 2023). Contrastingly, localised labour markets often experience sharper employment losses in highly exposed regions (Autor et al., 2013; Dix-Carneiro and Kovak, 2017). The null effect at the aggregate state level may reflect the averaging of heterogeneous regional responses, with port-related declines offset by gains elsewhere. It may also capture the lagged nature of labour market adjustment, since frictions and immobility slow the reallocation of workers in response to shocks (Dix-Carneiro and Kovak, 2017).

The positive but insignificant effect on the GDP share of transportation and warehousing is an interesting result. The literature suggests that capacity constraints should reduce sectoral output (Brancaccio et al., 2020), but higher freight rates can inflate nominal value added, raising GDP share despite lower volumes. This echoes the dynamics observed in shipping markets where inelastic demand and market power drive revenue growth under capacity stress (Hummels et al., 2009). The large standard errors, however, signal substantial cross-state heterogeneity, while some port states may have seen temporary increases in relative transport activity, others likely contracted. The placebo estimates for transport GDP share are again small and insignificant.

Employment in the transport sector shows no measurable response. This result is consistent with prior evidence that labour is relatively sticky in the short run (Ulate et al., 2025). Aggregated quarterly data may also obscure short-run layoffs or localised disruptions. Additionally, port-intensive states often rely on capital rather than labour intensive logistics infrastructure, further dampening observable employment effects. The null finding is therefore plausible but does not rule out sharper local shocks. The placebo results confirm this interpretation, with transport employment coefficients remaining very small and indistinguishable from zero.

Econometrically, the lack of significant state-level effects should not be taken as proof of no economic consequences. Instead, the results highlight the limitations of state aggregates, the short post-treatment horizon, and the potential for adaptive supply chain responses. These considerations motivate the robustness exercises and alternative treatment specifications, including the exclusion of large, diversified economies and the more robustly defined trade-exposure model.

4.1.1 Robustness Check: Excluding Large Economies

A potential concern with the baseline specification is that results may be disproportionately influenced by California and New York, given their size and economic diversity. To test this, the models were re-estimated excluding these two states. Table 4 shows that the estimates remain broadly consistent with the main results. The effect on log real GDP per capita is again marginally significant at the 10% level and slightly larger than in the baseline, suggesting that the weak positive effect is not driven solely by California or New York. However, both the Wald test and the event-study plot show signs of pre-trend divergence, with at least one pre-period coefficient significantly different from zero, so should be interpreted cautiously. For unemployment and transport employment, coefficients remain small and insignificant, but mild Wald rejections again suggest potential violations of the identifying assumptions. Only transport GDP share shows clear pre-treatment stability, supported by both the event-study and the Wald test result.

Table 4: Difference-in-Differences Model Results (Excluding NY and CA)

Outcome	Treated × Post	Obs.	Within R^2	Parallel Trends
Log GDP per Capita	0.0159*	234	0.141	No
	(0.0071)			
Unemployment Rate	0.0277	234	0.001	Yes
	(0.1485)			
Transport GDP Share	0.1651	234	0.018	No
	(0.2952)			
Log T&W Employment per Capita	0.0026	234	0.001	No
	(0.0133)			

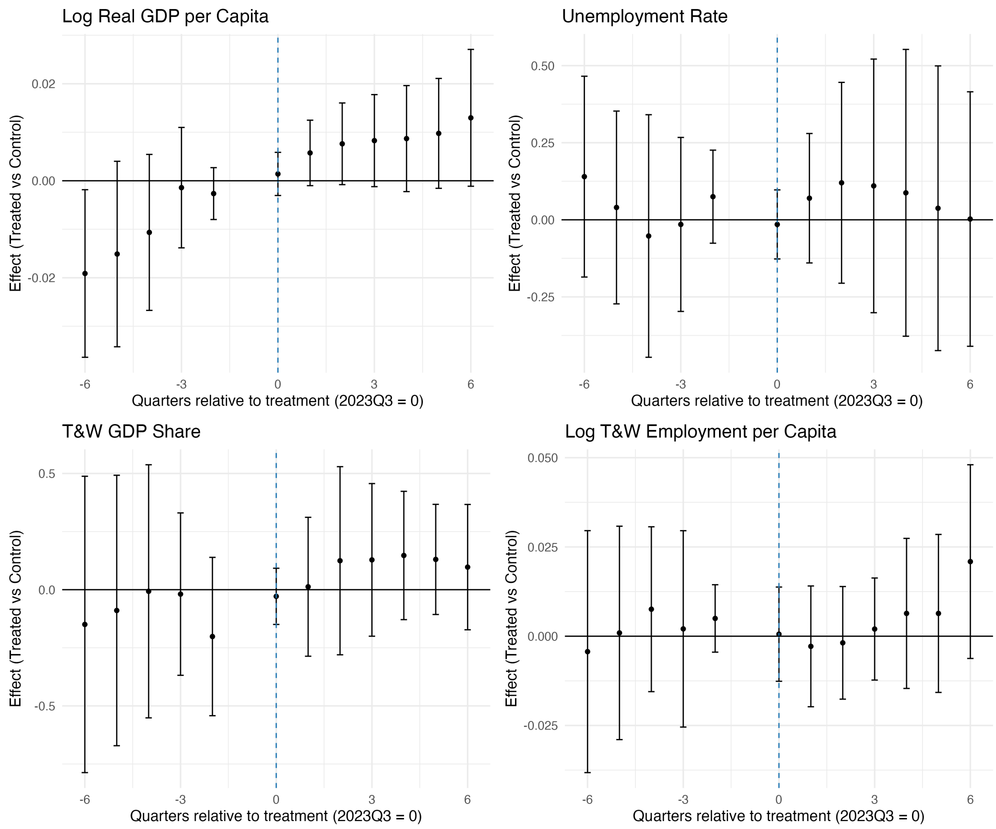
Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. The final column reports whether pre-treatment dynamics were broadly consistent with parallel trends (based on Wald tests and event-study figures).
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The dynamic results in Figure 6 broadly replicate the full-sample patterns. GDP per capita shifts mildly upward after the disruption with wide confidence intervals, while transport outcomes remain centred around zero. Unemployment displays pre-period volatility, consistent with the Wald test evidence of pre-trends, reinforcing the need for caution in attributing any effects.

The placebo estimates in Table A8 reinforce this caution. A marginally significant effect appears for GDP per capita when treatment is artificially set in 2022Q3, suggesting that part of the estimated impact may be capturing underlying dynamics rather than the disruption itself. The other outcomes remain statistically indistinguishable from zero, consistent with both the baseline and exclusion specifications.

The exclusion of California and New York confirms that the null results for unemployment, transport GDP share, and transport employment are

Figure 6: Event-Study Estimates Excluding California and New York



Notes: The vertical dashed line marks the disruption onset (2023Q3). The quarter immediately prior (2023Q2) is the omitted reference category.

not attributable to their inclusion. The modest GDP effect is fragile and likely reflects residual pre-trends rather than a robust treatment response, underscoring the limits of inference from state-level aggregates in this setting.

4.2 Trade Exposure Model Findings

To refine the identification strategy beyond the simple geographic treatment definition, an alternative model was estimated based on quantitative measures of trade exposure to the Panama Canal. With the ten states with the highest exposure ratios assigned to the treatment group, and the matched control group selected using propensity score matching (PSM) on pre-treatment outcomes. GDP-related outcomes were matched on GDP variables, and labour outcomes matched on labour variables.

The results are presented in Table 5. Relative to the baseline model, the estimates are modest in size and largely statistically insignificant. For Log GDP per Capita, the coefficient is very minimal (-0.0015) and not significant, indicating that the previously observed weak positive effect is not robust. The Transportation and Warehousing GDP Share (0.176) is positive but imprecisely estimated, and Log T&W Employment per Capita (-0.0013) is also minimal, with large standard errors.

The most notable finding is for the unemployment rate, where the coefficient is 0.285 significant at the 10%, suggesting a modest increase in unemployment in more trade-exposed states following the canal disruption. The magnitude is economically meaningful, corresponding to an increase of nearly one-third of a percentage point relative to pre-treatment levels. This finding resonates with research showing that trade shocks disproportionately manifest in employment rather than output aggregates (e.g., Ulate et al., 2025; Dix-Carneiro and Kovak, 2017). The Wald tests confirm parallel pre-trends for GDP, Transport GDP Share, and Unemployment, but reject them for T&W Employment.

Table 5: Difference-in-Differences Model Results (Trade Exposure Specification)

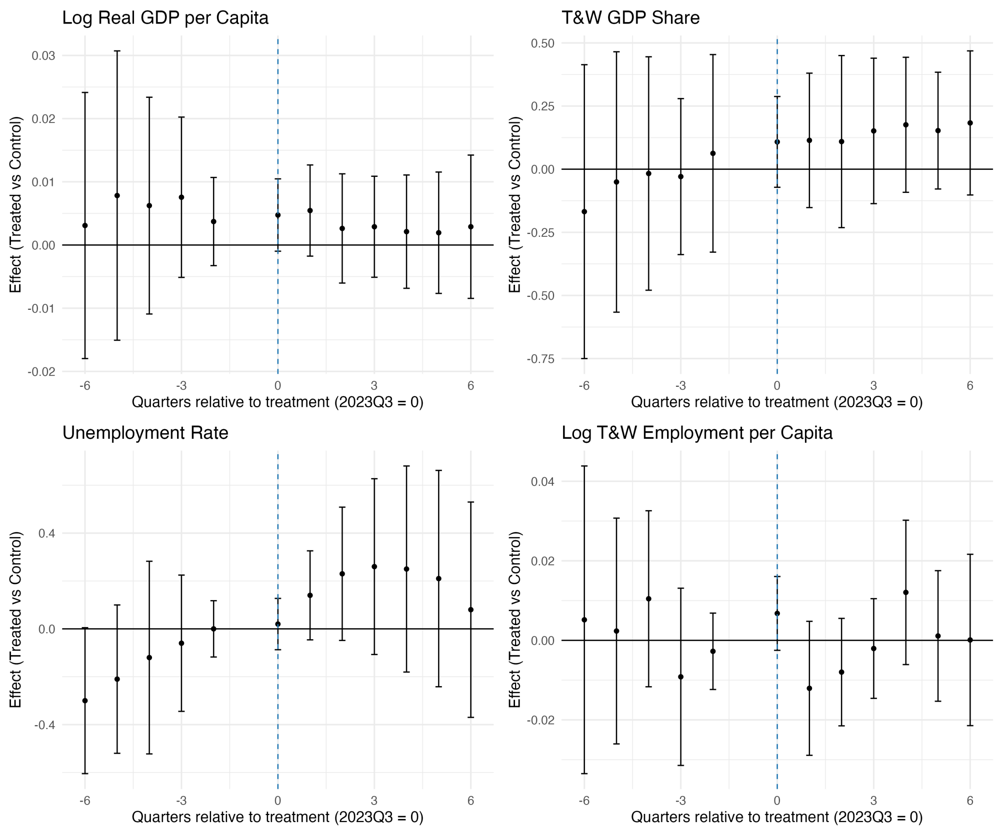
Outcome	**Treated × Post**	**Obs.**	**Within R^2**	**Parallel Trends**
Log GDP per Capita	-0.0015	260	0.001	Yes
	(0.0072)			
Transport GDP Share	0.1759	260	0.017	Yes
	(0.2610)			
Unemployment Rate	0.2850*	260	0.071	Yes
	(0.1643)			
Log T&W Employment per Capita	-0.0013	260	0.000	No
	(0.0106)			

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. The final column reports whether pre-treatment dynamics were broadly consistent with parallel trends (based on Wald tests and event-study figures).
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The event-study estimates in Figure 7 provide additional detail on the dynamic responses. For Log GDP per Capita, coefficients fluctuate narrowly around zero throughout the sample, and confidence intervals consistently cover zero. This suggests that output in high-exposure states did not diverge materially from controls either before or after the disruption. The Transport & Warehousing GDP Share displays somewhat more variation, while the pre-treatment estimates are balanced, post-treatment coefficients drift slightly upward, indicating a possible increase in sectoral activity. However, the effect remains statistically indistinguishable from zero, as wide intervals highlight the imprecision of these estimates. Taken together, both outcomes reinforce the conclusion that the disruption had little measurable impact on aggregate or sectoral output at the state level.

In the pre-treatment period of the unemployment plot, coefficients are consistently negative but slope upward toward zero, indicating that treated states had lower unemployment than controls but were already converging before the disruption. This undermines the credibility of the parallel trends assumption and raises the possibility that part of the observed post-treatment increase reflects continuation of this pre-existing trajectory rather than a causal effect of the disruption. After 2023Q3, the coefficients remain on this upward path, with several quarters of positive estimates in the range of 0.2–0.4 percentage points, broadly consistent with the DiD coefficient of 0.285. Log T&W Employment per Capita, however, displays substantial volatility even before the disruption, with coefficients alternating above and below zero. This instability, together with the Wald rejection of parallel pre-trends, reinforces that the model is poorly identified for this outcome. Overall, while the event-study provides tentative evidence of labour market pressures in more exposed states, the results must be interpreted with caution given the upward pre-trend in unemployment and erratic dynamics in sectoral employment.

Figure 7: Event-Study Estimates for the Trade Exposure Model



Notes: The vertical dashed line marks the disruption onset (2023Q3). The quarter immediately prior (2023Q2) is the omitted reference category.

The placebo specification (Table A9) further emphasises these concerns.

When the treatment date is artificially shifted to 2022Q3, the unemployment coefficient remains positive and statistically significant, while the other outcomes again show no systematic response. In combination with the upward pre-trend in unemployment and the instability in sectoral employment, this suggests that the apparent post-treatment rise in unemployment may partly reflect residual dynamics rather than a clean causal effect of the disruption. Taken together, the trade exposure specification eliminates the anomalous GDP effect from the baseline and points to potential labour market pressures in more exposed states, but the weight of the evidence, including the placebo results and Wald test failures, implies that these findings should be treated with caution.

4.3 Inflation Model Findings

This section examines the disruption's impact on consumer prices across U.S. Census divisions. It tests whether supply chain frictions from the canal drought led to higher regional inflation in more exposed areas, with Table 6 reporting the estimated treatment effects for CPI in both level and log-transformed forms.

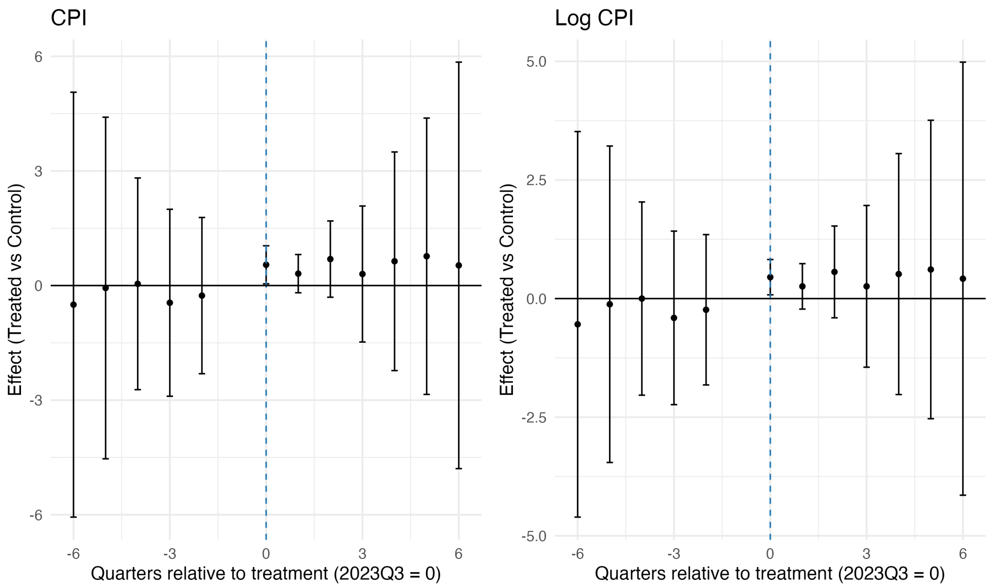
Table 6: Difference-in-Differences CPI Model Results

Outcome	**Treated × Post**	**Obs.**	**Within R^2**	**Parallel Trends**
CPI Levels	0.7455	65	0.104	Yes
	(0.4738)			
Log CPI (x100)	0.6571*	65	0.130	Yes
	(0.2954)			

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. The final column reports whether pre-treatment dynamics were broadly consistent with parallel trends (based on Wald tests and event-study figures).
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 8 reports the event-study plots. The pre-treatment coefficients are centred around zero and statistically insignificant, lending support to the parallel trends assumption. In the post-treatment period, coefficients in both CPI and log CPI models move upward from roughly the second quarter after the disruption. While confidence intervals remain wide, the pattern suggests a gradual and persistent price response. The log specification in particular shows a clearer upward drift, consistent with the marginally significant DiD coefficient.

Figure 8: Event-Study Estimates of Disruption Impact on CPI



Notes: The vertical dashed line marks the disruption onset (2023Q3). The quarter immediately prior (2023Q2) is the omitted reference category.

The model for CPI Levels yields a positive but insignificant coefficient of 0.75, implying a weak and imprecise effect. In contrast, the log-transformed model produces a coefficient of 0.66 ($p < 0.10$), suggesting that consumer prices in treated divisions increased by around 0.7% relative to controls in the post-treatment period. Although only marginally significant, this result is consistent with the literature on shipping costs and inflationary pass-through. Prior research shows that increases in transport costs often materialize in higher consumer prices with a lag, as supply chain frictions gradually propagate from producer prices to final goods (Santacreu and LaBelle, 2022; Carrière-Swallow et al., 2023). The lagged positive coefficients in the event study align with this mechanism, highlighting the time it takes for disruptions at the canal to filter into retail inflation.

The placebo specification, reported in Appendix Table A10, provides an important validity check. When the treatment is artificially set to 2022Q3, the coefficients for both CPI and log CPI are small, statistically insignificant, and lack any consistent pattern. This strengthens the interpretation that the post-2023Q3 inflation effects are not spurious or driven by unobserved pre-trends, but instead reflect the actual disruption.

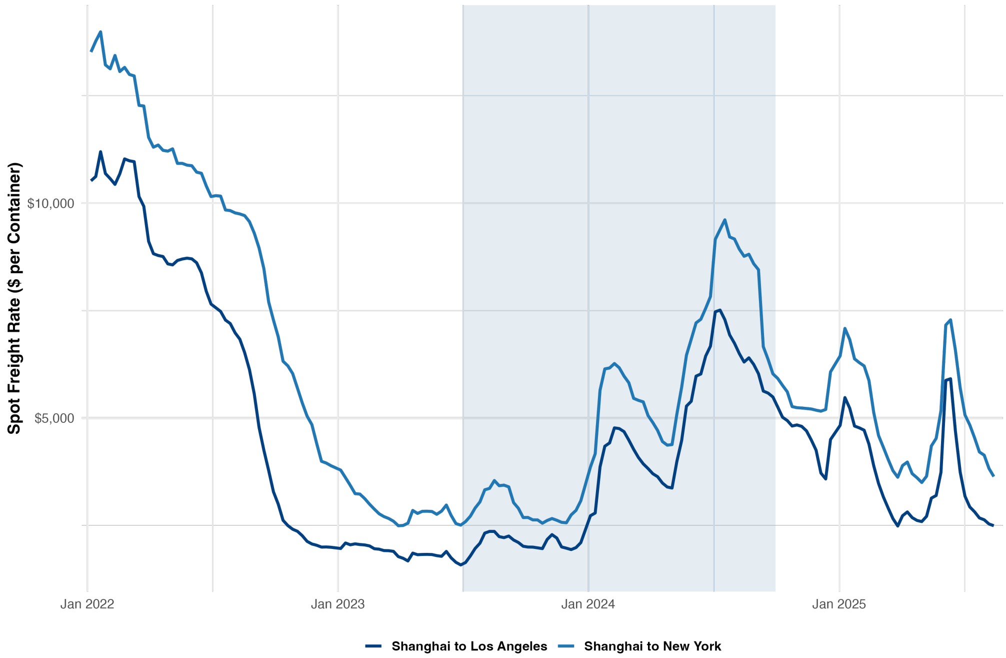
These findings complement evidence from earlier supply chain shocks. The global shipping crisis of 2020–22 demonstrated that spikes in freight costs have persistent and measurable effects on consumer prices, lasting over a year (Carrière-Swallow et al., 2023; Alessandria et al., 2023). Similarly, structural models confirm that external trade disruptions feed into inflationary pressures through both direct import costs and wider general equilibrium effects (Bai et al., 2024). The Panama Canal disruption, though smaller in scale, appears to have generated comparable but modest dynamics at the regional level. Additionally, the environmental trigger of the disruption connects with emerging research linking climate driven anomalies to supply chain bottlenecks and inflationary pressure (Cevik and Gwon, 2024).

The CPI models evidence is suggestive rather than conclusive. The level specification shows weak and imprecise effects, while the log specification indicates a modest but consistent inflationary increase. The dynamic results point to delayed adjustment, in line with established pass-through mechanisms observed during prior supply chain shocks. The Panama Canal case, although smaller in scale, appears to have generated comparable but more muted dynamics. Overall, the findings imply that the disruption likely contributed to upward price pressure in affected divisions, but the magnitude remains modest and precision limited.

4.4 Interpreting Null Results

The largely insignificant results in the state-level DiD and event-study models can be better understood in the context of how shipping markets and the U.S. logistics networks adapted to the disruption. The container freight costs and port activity data show that the canal shock did not create an absolute block on trade flows but instead triggered a process of re-routing that diffused its economic impact across regions. Freight rate indices, particularly the WCI series, highlight sharp cost increases on Asia–East Coast routes via the canal (Figure: 9), while West Coast lanes rose but remained relatively cheaper. At the same time, container throughput data reveal that volumes shifted toward West Coast ports such as Los Angeles, with a relative softening in East Coast ports like Savannah. This pattern is consistent with a deliberate reallocation of cargo away from constrained canal

Figure 9: Comparison of route costs, East Coast vs West Coast



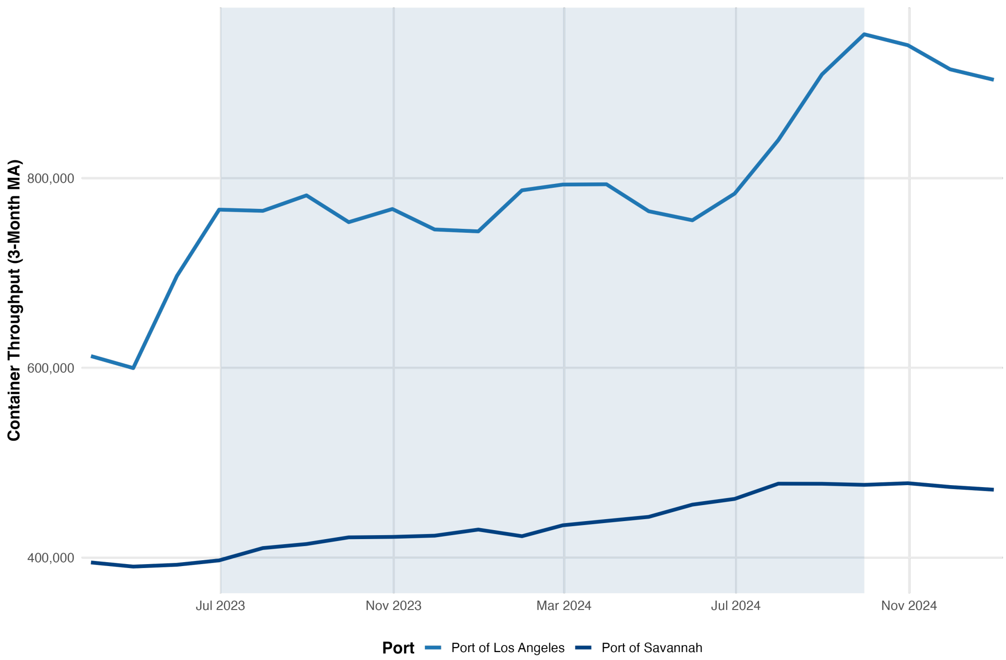
Source: Bloomberg L.P. (2025f,g).

passages and toward alternative entry points, followed by inland transport to reach final markets.

The intermodal rail data presented in figure 11 support this interpretation. Rising unit volumes on Union Pacific during the disruption indicate that containers arriving at West Coast ports were not destined solely for West Coast markets but were increasingly carried inland to meet demand on the East Coast. This reallocation of cargo flows, alongside the divergence in port activity, highlights how U.S. logistics networks adapted to the canal's capacity constraints by restructuring established trade routes and distribution patterns. Simultaneously, the Cass Freight Shipments Index shows a broad decline, reflecting a cooling of overall domestic freight demand potentially linked to wider macroeconomic conditions rather than the canal disruption.

While aggregate shipments contracted, the rail data suggest that a larger share of the remaining flows was being funnelled through intermodal routes. The integration of these freight patterns helps explain why no sharp effects emerge in the state-level GDP or employment regressions. Treated states may have faced higher transport costs, but goods continued to flow, albeit along longer and more indirect routes. This pattern exemplifies the adap-

Figure 10: TEU throughput comparison: Port of Los Angeles vs. Port of Savannah 3-month MA

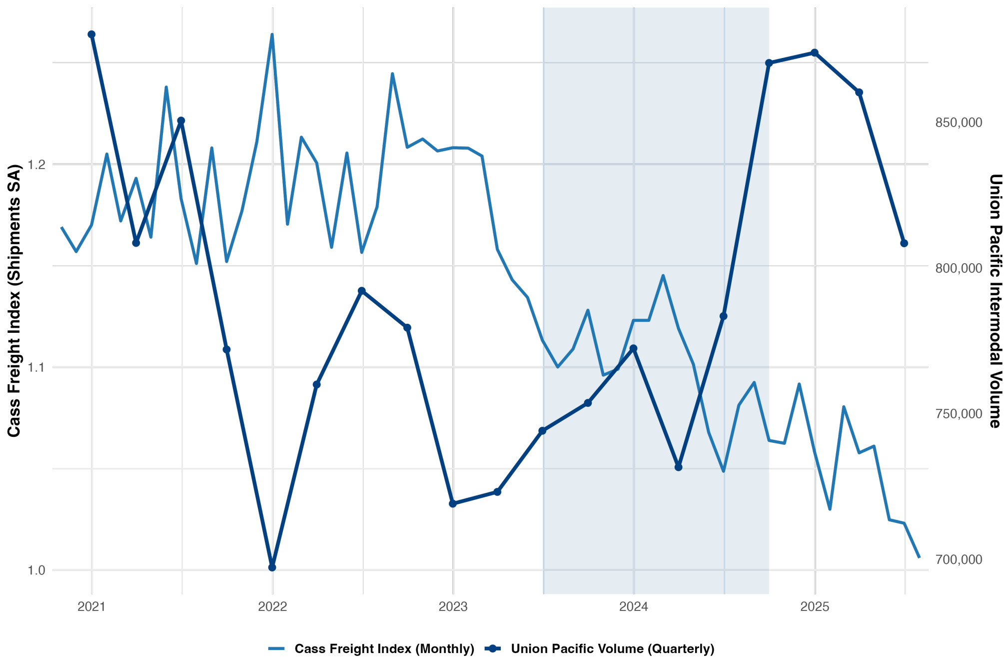


Source: Bloomberg L.P. (2025d,c).

tive capacity of supply chains described by Baldwin and Freeman (2022), where substitution and re-routing dampen the localised economic consequences of major shocks.

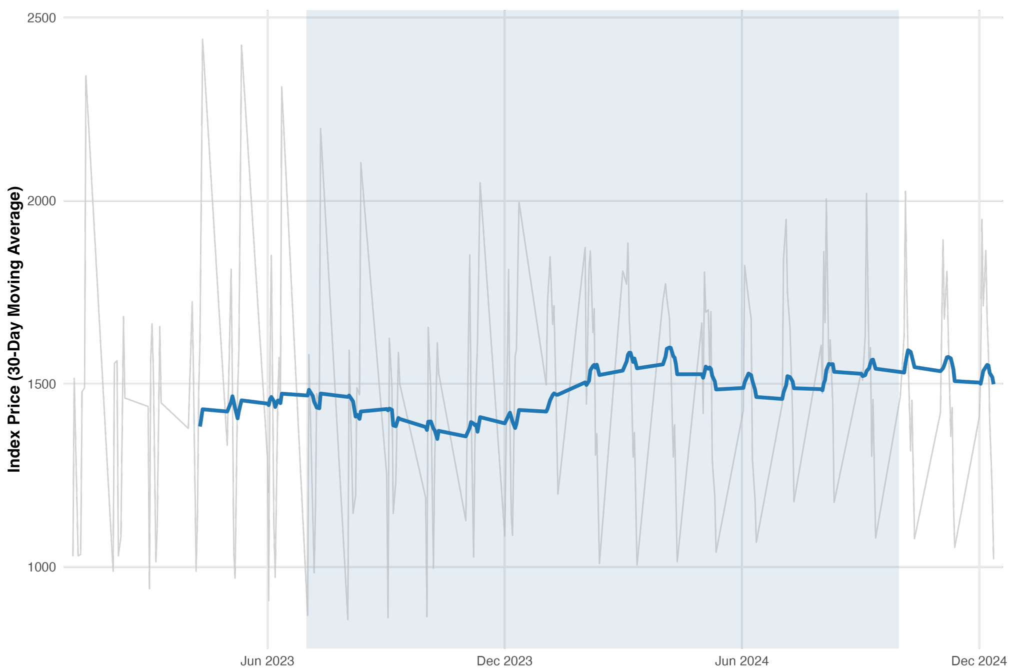
The market's reaction to the canal drought is somewhat evidenced in bulk shipping data. The Baltic Panamax Index registered a temporary, slight increase, a reflection of the vessel scarcity that emerged as carriers began re-routing (Figure 12). However, the swift stabilization of this index indicates that this friction was not sustained. This observed resilience is consistent with the operational flexibility of modern shipping networks, as documented by Notteboom et al. (2021) in their analysis of the COVID-19 and 2008-2009 crises. They note that carriers typically respond to shocks by implementing blank sailings and redeploying assets, while ports mitigate localized disruptions by leveraging alternative inland corridors. These strategies appear to have contained the canal drought's impact, turning it into a brief rise in transport frictions rather than the prolonged bottlenecks and output losses seen after the 2008–09 financial crisis.

Figure 11: CASS Freight Shipments Index (seasonally adjusted) compared with Union Pacific intermodal unit volumes (Quarterly)



Source: Bloomberg L.P. (2025b,e).

Figure 12: 30-day moving average of the Baltic Exchange Panamax Index, a benchmark for bulk shipping rates on Panamax-sized vessels



Source: Bloomberg L.P. (2025a).

5 Conclusion

The capacity of maritime bottlenecks to have a major influence on international trade is well-established. A substantial body of literature analyses the effects of supply chain disruptions, including those caused by natural disasters in Japan, the COVID-19 pandemic, and historical and current events impacting the Suez Canal. However, what remains less clear in the current literature are the effects of recurring, climate-driven disruptions. The Panama Canal plays a vital role in global trade, but droughts during 2023–2024 reduced its operational capacity. This raises an important question for both the present and future: how might such disruptions affect the United States, its principal user, if droughts persist.

Difference-in-Differences (DiD) models were employed to analyse the state-level economic impacts of the disruption. The results indicate small and statistically insignificant effects. In the basic model, log real GDP per capita shows a weakly significant but negligible increase, while the other models remain insignificant. Wald and event-study tests confirm parallel trends for most outcomes, although unemployment displays pre-treatment divergence. Placebo tests with a shifted treatment date yield similarly small and insignificant effects. In the placebo model for log GDP per capita the results were also significant suggesting the weak GDP result reflects noise rather than a true impact. Excluding California and New York produces consistent results, confirming that the null findings are not driven by these large, diversified economies.

The trade-exposure models, which assigned treatment to the most canal-reliant states and used matched control groups, yielded broadly similar conclusions to the baseline. The previously weak positive GDP effect disappeared, replaced by a small negative and statistically insignificant estimate. Transport GDP share showed a positive but imprecise coefficient, offering no evidence of a meaningful sectoral response. The unemployment rate increased by around 0.29 percentage points and was marginally significant, suggesting a potential but modest deterioration in labour market conditions among the most exposed states. However, the event-study reveals an upward pre-trend in unemployment, and the placebo regression also produced a significant coefficient, indicating that this effect may partly reflect pre-existing divergence rather than a true post-disruption impact. Finally, transport and warehousing employment showed no systematic effect, with

Wald tests rejecting parallel pre-trends.

The consumer price index (CPI) model provided modest evidence of upward price effects following the disruption. The level specification produced a positive but statistically insignificant coefficient, while the log specification indicated a 0.66 percent increase in prices for treated divisions, significant at the 10 percent level. Event-study estimates showed that these effects did not appear immediately after the disruption but emerged with a two-quarter lag, consistent with gradual pass-through from higher shipping costs to consumer prices. The placebo specification yielded null results, reinforcing that the observed price increases likely reflect genuine post-disruption inflationary pressure rather than pre-existing trends.

To explain the modest economic impacts observed in U.S. states, this analysis explored potential adaptive strategies within supply chains. The literature suggests that firms can mitigate disruptions by re-routing shipments. Using datasets on port throughput, intermodal volumes, and shipping rates, the viability of diverting cargo to West Coast ports and transporting it overland to the East Coast to diffuse the effect was examined. The evidence seems to support this hypothesis, the Port of Los Angeles, for instance, experienced an increase in traffic, while Union Pacific's intermodal volumes also grew. These dynamics could explain why the econometric estimates show limited state-level impacts. Treated states bore higher transport costs, but supply chains adapted through re-routing and substitution ensuring the goods continued to flow, and production maintained. The costs manifested in inflationary pressure, consistent with the CPI results, transport frictions eventually filtered through to consumer prices with a lag.

The lack of significant or large GDP effects contrasts with the findings of Feyrer (2021) from historical closures of the Suez Canal. Feyrer (2021) showed that longer shipping distances caused substantial declines in international trade and subsequently national income. Similarly Brancaccio et al. (2020), argued that a theoretical removal of the Panama Canal would cause a reduction in global trade by 3.3%. The canal disruption between 2023-24, however suggests the U.S. benefits from adaptable supply chains and strong infrastructure to dissipate any impacts to GDP. It is important to note that the literature by Feyrer (2021), and Brancaccio et al. (2020) discuss closures or removals of critical chokepoints whereas the Panama

Canal only suffered restrictions on traffic volume rather than a full closure.

The inflationary pass-through observed in the CPI model aligns with recent research on the role of shipping costs in driving consumer prices. Carrière-Swallow et al. (2023) estimate that a doubling of global freight rates increases inflation by 0.25–0.5 percentage points. Alessandria et al. (2023) similarly found that pandemic era shipping shocks had large effects on U.S. inflation. The lagged adjustment evident in the event-study plots also mirrors Santacreu and LaBelle (2022), who document that supply chain pressures transmit to consumer prices with delays. The modest but marginally statistically significant rise in regional unemployment is consistent with literature on labour market frictions following trade shocks (Dix-Carneiro and Kovak, 2017; Ulate et al., 2025). The estimated 0.28 percentage point unemployment increase in canal reliant states demonstrates that, despite supply chain adaptation, localised labour markets still bear tangible adjustment costs and remain vulnerable to disruptions.

If recurrent droughts continue to reduce transit capacity in the Panama Canal, the U.S. cannot rely indefinitely on reactive re-routing. Policy responses could include supporting investment in the Canal's water management infrastructure to alleviate drought risks, through partnerships with Panama. At the same time, the U.S. might reconsider the extent of its dependence on Canal routed trade, diversifying supply chains toward partners and routes less exposed to this bottleneck. Encouraging greater use of West Coast ports, expanding capacity along alternative corridors, and strengthening regional trade ties could mitigate the systemic risks that emerge when a single maritime artery becomes constrained. These strategies move beyond coping with shocks ex post and instead address the structural vulnerabilities that climate change is making increasingly persistent.

The findings come with some limitations, the reliance on state and division-level aggregates risks obscuring more acute localised effects. The short observation window may also underestimate the persistence of shocks, while the binary treatment design simplifies more complex supply chain exposure. These caveats suggest the need for future research using more detailed data on firms and industries, longer time horizons to track cumulative effects, and comparative analyses across other canal-dependent economies. Using logistics data such as freight rates, port throughput, and vessel flows directly in causal models would help link adaptive transport strategies to economic outcomes.

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Appendix

A1: Variable Table

Table A1: Variables, Definitions, and Data Sources

Variable	Definition	Source
Log Real GDP per Capita	Quarterly real GDP (millions of chained 2017 dollars) divided by annual population (state residents), log-transformed.	BEA
Transportation and Warehousing Share of GDP	Transportation and warehousing real GDP (quarterly, in millions of chained 2017 dollars) divided by total real GDP, expressed as a percentage.	BEA
Unemployment Rate	Monthly unemployment rate, seasonally adjusted (%), aggregated to quarterly averages.	BLS / FRED
Log Transportation and Warehousing Employment	Transportation and warehousing employment (monthly, thousands of employees) divided by annual population, aggregated to quarterly, log-transformed.	BLS, BEA
CPI Levels	Consumer Price Index for All Urban Consumers, Census Division level, base December 2017=100. Quarterly average of monthly series.	BLS
Log CPI	$100 \times \ln(\text{CPI})$ for quarterly CPI values.	BLS

A2: Summary Statistics

Table A2: Basic Model Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Panel A: All States (N = 20)					
Log GDP per Capita	260	11.09	0.16	10.77	11.44
Unemployment Rate	260	3.72	0.80	2.20	5.80
Transport GDP Share	260	6.37	4.24	1.83	21.80
Log T&W Employment per Capita	260	0.78	0.18	0.47	1.22
Panel B: Control States (N = 10)					
Log GDP per Capita	130	11.06	0.08	10.92	11.23
Unemployment Rate	130	3.62	0.88	2.20	5.80
Transport GDP Share	130	4.77	1.60	1.83	8.03
Log T&W Employment per Capita	130	0.85	0.16	0.63	1.22
Panel C: Treated States (N = 10)					
Log GDP per Capita	130	11.12	0.20	10.77	11.44
Unemployment Rate	130	3.83	0.69	2.50	5.50
Transport GDP Share	130	7.98	5.32	2.08	21.80
Log T&W Employment per Capita	130	0.72	0.17	0.47	1.07

Table A3: Trade Exposure Summary Statistics (GDP Set: Treated vs Controls)

Variable	Obs.	Mean	Std. Dev.	Min	Max
Panel A: All (Treated + GDP Controls)					
Log GDP per Capita	260	11.01	0.21	10.58	11.44
Transport GDP Share	260	5.90	4.43	2.08	21.80
Panel B: GDP Control States (N = 10)					
Log GDP per Capita	130	10.97	0.22	10.58	11.44
Transport GDP Share	130	4.69	2.58	2.18	11.54
Panel C: Treated States (N = 10)					
Log GDP per Capita	130	11.05	0.19	10.77	11.40
Transport GDP Share	130	7.10	5.46	2.08	21.80

Table A4: Trade Exposure Summary Statistics (Labour Set: Treated vs Controls)

Variable	**Obs.**	**Mean**	**Std. Dev.**	**Min**	**Max**
Panel A: All (Treated + Labour Controls)					
Unemployment Rate	260	3.92	0.60	2.60	5.50
Log T&W Employment per Capita	260	0.81	0.19	0.47	1.15
Panel B: Labour Control States (N = 10)					
Unemployment Rate	130	3.80	0.55	2.60	5.40
Log T&W Employment per Capita	130	0.80	0.18	0.47	1.07
Panel C: Treated States (N = 10)					
Unemployment Rate	130	4.05	0.62	2.80	5.50
Log T&W Employment per Capita	130	0.82	0.20	0.52	1.15

Table A5: Summary Statistics for CPI Models

Variable	**Obs.**	**Mean**	**Std. Dev.**	**Min**	**Max**
Panel A: All Divisions (N = 8)					
CPI Level	65	123.44	4.90	113.05	132.28
Log CPI (x100)	65	481.50	3.98	472.78	488.49
Panel B: Control Divisions (N = 4)					
CPI Level	26	124.16	5.43	113.63	132.28
Log CPI (x100)	26	482.07	4.38	473.30	488.49
Panel C: Treated Divisions (N = 4)					
CPI Level	39	122.96	4.53	113.05	131.58
Log CPI (x100)	39	481.12	3.70	472.78	487.96

A3: Pre-treatment Parallel Trends

Table A6: Parallel Trends Wald Tests (Pre-Treatment Period: 2022Q1–2023Q2)

Outcome	**F-statistic**	**p-value**	**df1**	**df2**
Basic Model				
Log Real GDP per Capita	0.94	0.456	5	90
Unemployment Rate	2.58*	0.031	5	90
Transport GDP Share	1.78	0.125	5	90
Log T&W Employment per Capita	1.78	0.126	5	90
Basic Model (Excluding NY and CA)				
Log Real GDP per Capita	2.30*	0.052	5	80
Unemployment Rate	1.87	0.108	5	80
Transport GDP Share	5.97***	0.0001	5	80
Log T&W Employment per Capita	2.71*	0.026	5	80
Trade Exposure Matched Controls				
Log Real GDP per Capita	1.19	0.320	5	90
Transport GDP Share	0.46	0.802	5	90
Unemployment Rate	1.54	0.187	5	90
Log T&W Employment per Capita	2.82*	0.021	5	90

Notes: Wald tests assess the joint insignificance of treatment–time interaction coefficients in the pre-treatment period (2022Q1–2023Q2). Null hypothesis: treated and control states follow parallel pre-trends. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A4: Placebo Timing Results (Fake Start 2022Q3)

Table A7: Difference-in-Differences Model Results (Basic Model - Placebo Test)

Outcome	Treated × Post	Obs.	Within R^2
Log GDP per Capita	0.0167* (0.0076)	260	0.087
Unemployment Rate	-0.0905 (0.1396)	260	0.005
Transport GDP Share	0.2620 (0.2484)	260	0.022
Log T&W Employment per Capita	0.0051 (0.0127)	260	0.003

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Placebo Difference-in-Differences Model Results (Excluding NY and CA, Treatment in 2022Q3)

Outcome	Treated × Post	Obs.	Within R^2
Log GDP per Capita	0.0207* (0.0080)	234	0.125
Unemployment Rate	-0.0518 (0.1257)	234	0.002
Transport GDP Share	0.1544 (0.2966)	234	0.008
Log T&W Employment per Capita	0.0059 (0.0148)	234	0.004

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Difference-in-Differences Model Results (TRADE EXPOSURE Placebo Specification)

Outcome	**Treated × Post**	**Obs.**	**Within R^2**
Log GDP per Capita	-0.0018 (0.0093)	260	0.001
Transport GDP Share	0.2013 (0.2715)	260	0.012
Unemployment Rate	0.3468** (0.1435)	260	0.055
Log T&W Employment per Capita	-0.0041 (0.0144)	260	0.002

Notes: Standard errors clustered at the state level are reported in parentheses. All models include state and quarter fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

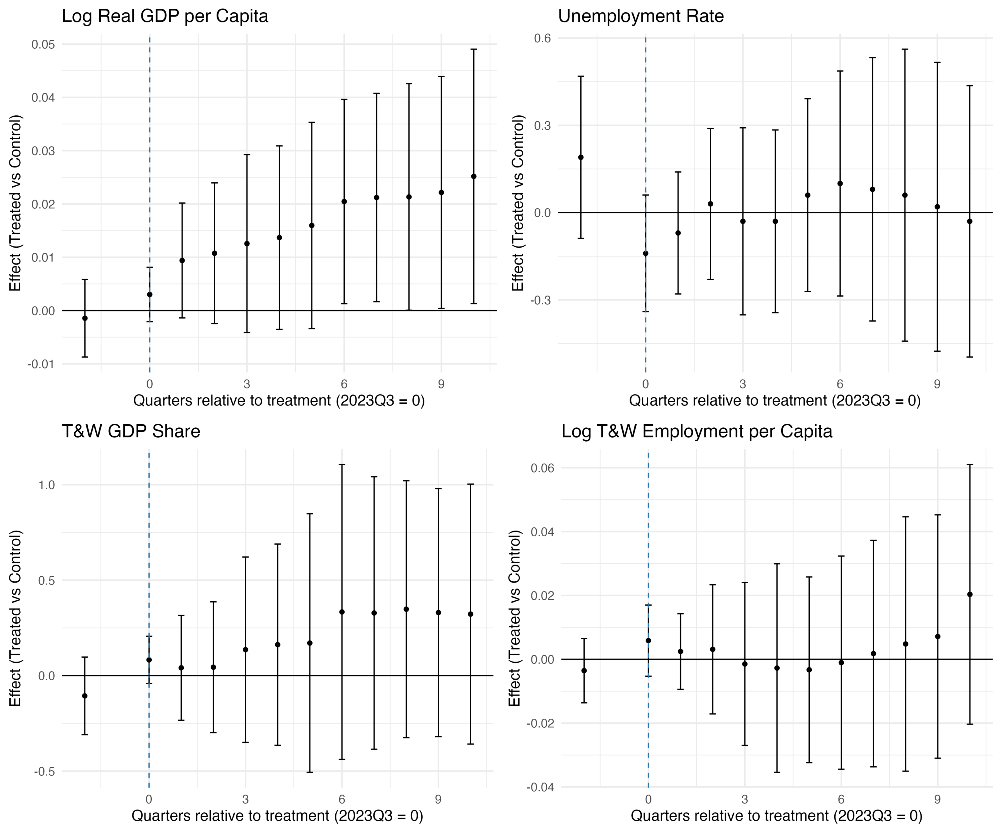
Table A10: Placebo Difference-in-Differences Results for CPI Divisions

Outcome	**Treated × Post**	**Obs.**	**Within R^2**
CPI	0.5651 (1.048)	65	0.031
Log CPI	0.5524 (0.7022)	65	0.048

Notes: Standard errors clustered at the division level are reported in parentheses. All models include division and quarter fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

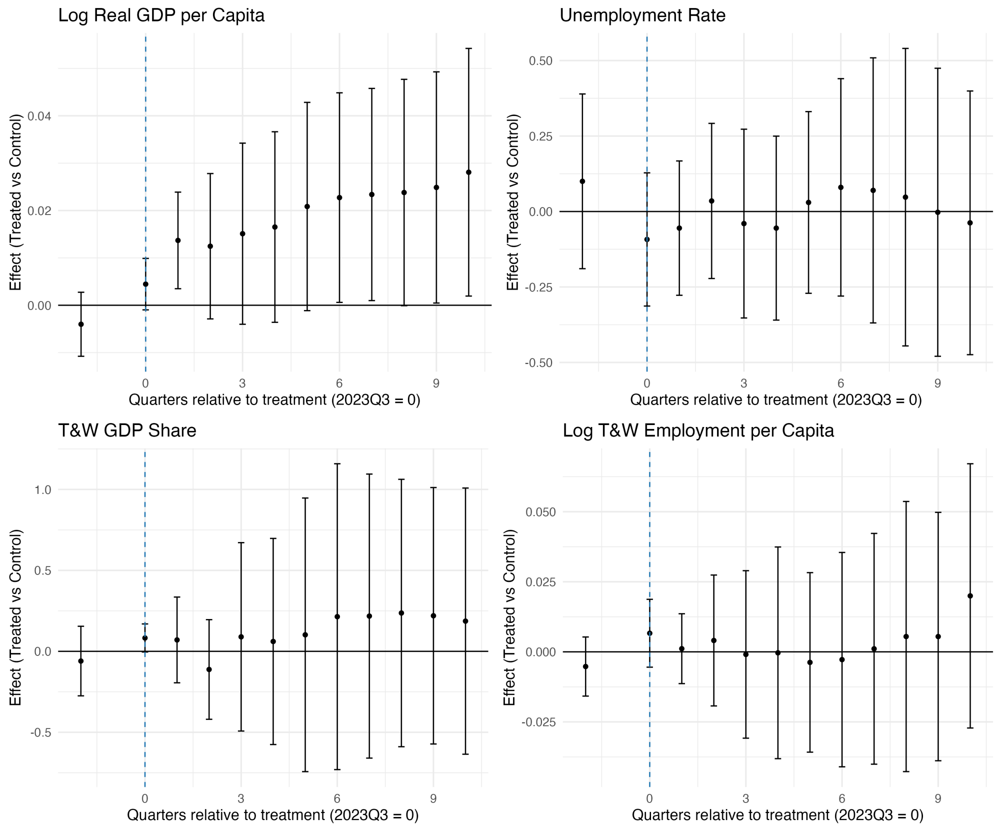
A5: Placebo Timing Event-Study Plots

Figure 13: Event-Study Plots: Placebo Basic Model



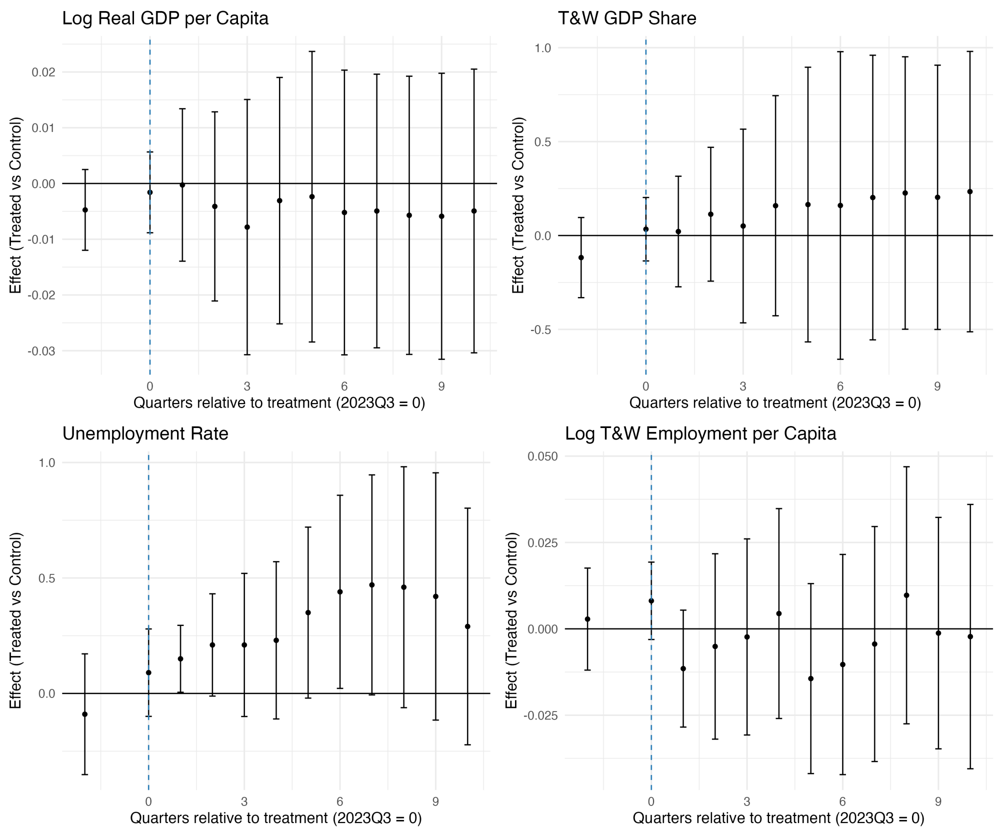
Notes: The vertical dashed line marks the disruption onset (2022Q3). The quarter immediately prior (2022Q2) is the omitted reference category.

Figure 14: Event-Study Plots: Placebo Excluding California and New York



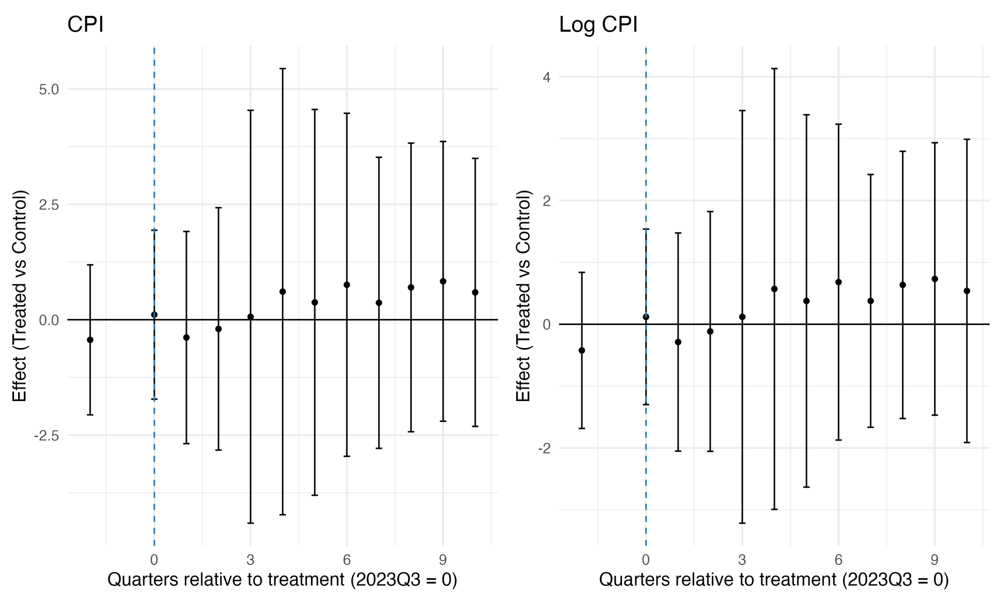
Notes: The vertical dashed line marks the disruption onset (2022Q3). The quarter immediately prior (2022Q2) is the omitted reference category.

Figure 15: Event-Study Plots: Placebo Main Model



Notes: The vertical dashed line marks the disruption onset (2022Q3). The quarter immediately prior (2022Q2) is the omitted reference category.

Figure 16: Event-Study Plots: Placebo Inflation Model



Notes: The vertical dashed line marks the disruption onset (2022Q3). The quarter immediately prior (2022Q2) is the omitted reference category.