Econ H191 Thesis Proposal V2
Measuring the Impact of Israel's 2021–2023 Port Reforms on Labor Productivity Mediated by Capital Deepening

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1 Abstract, Design, and Data

This paper measures the impact of Israel's recent (2021–2023) port reforms on the sector's persistently low labor productivity. The reforms were aimed at breaking monopoly conditions by introducing intra-port competition through new deep-water terminals in Haifa and Ashdod, and by privatizing Haifa's legacy operator. Besides the overall effect of the reforms on labor productivity, the study quantifies the share of productivity gains mediated by capital deepening, a channel of special interest given the endemically low levels of capital per worker in Israel's non-tradable sectors. Finally, this paper contrasts the effects of privatization and intra-port competition on labor productivity.

I use an event-study DiD for staggered adoption, as developed by Sun–Abraham (2021) and Callaway–Sant'Anna (2025), to evaluate the impact of each of the three reforms on K/L and on labor productivity (LP). I then use IV-mediation to distinguish gains in LP through the K/L channel as opposed to other factors by implementing a 2SLS with a yet-to-be-defined proxy to instrument K/L.

The final data I need is monthly or quarterly panel data on each port detailing output, K/L, and LP. None of these metrics are directly reported, so I will have to build proxies for each one. For output, I'll use TEU's processed, perhaps multiplied by a factor of service-time KPIs such as waiting time, dwell time, and output per ship-hour. I can access most if not all of the above metrics from Israel's Administration of Shipping and Ports report Statistical Yearbook – Shipping and Ports 2024," which details port statistics. For related summary statistics I can use Israel's State Comptroller's 2024 report The Ports Sector in Israel and Aspects of Operation and Service" Both are publicly available online.

For K/L, I treat K and L separately. To get K, I'll make a manual monthly capital stock proxy which combines meters of berth completed, STS and RGM cranes (multiplied by a factor of their height, outreach, and tandem lift), and perhaps software used such as OCR. I can access data on all of the above (with the possible exception of software, which I may remove from the proxy) from dated news reports and from the official websites of the Ashdod port and the Haifa Bayport Terminal. As for the legacy terminal in Haifa, I may need to send an email to someone (I have connections so I'll try and figure it out). As for Labor, pre-reform worker count is accessible through the Ministry of Finance Public Bodies Wage Report. For levels of L after the first reform in 2021, I will have to either request the data directly, or build a proxy/estimate of monthly hours worked using data that is published and available online: annual hours worked, accident rate per 100,000 hours, monthly "activity weights", and quarterly labor utilization ranges.

For LP, I'll just use my proxies for output and L to calculate LP = output / L.

2 Literature Review

Two literature strands motivate this study: Israel-specific diagnostics and international evidence on port reforms.

In Israel, following massive protests related to high cost of living, the Trajtenberg Committee Report (2011) points out port inefficiency driven by monopoly power as an important economy-wide cost, and recommends introducing intra-port competition by building new terminals in the Haifa and Ashdod ports. This recommendation was formally adopted by the government in late 2011. The Taub Center's analyses (Brand and Regev, 2015a; 2015b) document widening productivity gaps between Israel and the OECD and emphasize structural frictions in non-tradables, motivating attention to sectors like ports. In their study Why Is Labor Productivity in Israel So Low?" Hazan and Tsur (2021) show that accumulated factors (including K/L) explain most of Israel's productivity gap from peer OECD economies. Joseph Zeria's 2021 book similarly diagnoses a missing capital" gap and calculates as an example that Israeli output per worker is roughly 25

When it comes to economic literature on port reforms and labor productivity more generally, a consensus seems to emerge, which is that intra-port competition (i.e., multiple terminals under different ownership) seems to be the chief engine of productivity improvement, and that capital deepening is an important effect of competition. Additionally, privatization alone does not seem to have a decisive impact on productivity gains. In a 2010 paper, Cheon, Dowall, and Song analyze port reforms globally and find that competition-inducing reforms drive up TFP growth, and emphasize that productivity gains are more likely when privatization is paired with competitive pressure and technology upgrades (likely leading to capital deepening). Additional studies show that competition is a stronger predictor of productivity gains than privatization, with ports run by multiple terminal operators outperforming private monopolies (Cullinane, Ji and Wang, 2005; Tongzon and Heng 2005). However, the channel through which efficiency is increased is not measured (e.g., capital deepening vs. higher labor utilization). Finally, the OECD Policy Roundtable Competition in Ports and Port Services (2011) synthesizes global experiences and concludes that introducing intra-port competition improves productivity and lowers user costs. Country-specific case studies provide confirmation for the patterns discussed above. Estache, González, and Trujillo's 2002 paper documents efficiency gains in Mexican ports following reforms, driven by frontier shifts" (e.g., capital deepening) and catch-up" (e.g., better utilization). No measurement is done to calculate the proportion of gains realized through each channel. Similarly, González and Trujillo's 2009 paper shows that intra-port competition yielded larger efficiency improvements in Spanish ports than privatization alone.

Overall, three gaps are evident in this literature. First, most studies either bundle competition and privatization together, or look at them over different ports. Second, while several studies mention multiple mechanisms through which reforms increased labor productivity, no study measures the productivity gains mediated by a particular channel such as labor utilization or capital deepening. Third, the identification in these studies is too coarse, as evidence is often annual or at most quarterly, and sometimes estimated with naive two-way fixed effects, which may wrongly use post-reform (i.e., treatment) data as control in staggered rollout contexts such as Israel's, potentially biasing results. For the Israeli ports specifically, no study causally measures productivity gains due to the reform (reports and studies only point out the overall efficiency improvements over time), and no attempt is made to distinguish between gains made from different channels. These gaps are all addressed by my paper's empirical design.

3 Empirical Design and addressing gaps in the literature

I exploit Israel's staggered implementation of three reforms to its two primary ports as a natural experiment. My design works in two large steps: (i) staggered-adoption event-study difference-in-differences as developed by Sun and Abraham (2021) and Callaway and Sant'Anna (2023; 2025) to trace the causal effects of each reform on proxied K/L (the mediator) and proxies LP (the outcome). (ii) I use IV-Mediation (as developed by Dippel and Ferrara in their 2020 paper) to quantify the share of LP gains mediated by K/L versus other channels. Proximal monthly or panel data allows me to separate the effects of each of the three reforms: Haifa competition (September 2021), Ashdod competition (Q4 2022), and Haifa privatization (January 2023). This design addresses previous gaps in the scholarship, as I detail below.

First, the unique staggered rollout of Israel's port reforms – including the fact that competition entry and privatization reforms were implemented separately in the same port with a two-year gap – combined with the relatively high-frequency (monthly or quarterly) proxies I construct, allows me to better control for identification problems which limited the robustness of previous studies. More concretely, I am able to completely separate the effects of competition-induction and privatization by having each port serve as its own before/after control, while the other port serves as a same-month control. This two-layer design (testing the treatment within the port over time and across the ports in the same period) improved the credibility and robustness of my results.

Second, I implement 2SLS IV-mediation to quantify the proportion of each reform's productivity gain mediated by capital deepening versus other channels. I do this by instrumenting K/L with various dated and manually factorized engineering milestones such as the introduction of new cranes, meters of berth completed, and so on. For the 2SLS, I first regress K/L on the proxy and on various controls (also using more advanced techniques developed by Clarke in his 2017 paper to control for spillover effects from the other port). In the second stage, I regress LP on the instrumented $(K/L)_{\text{hat}}$. Following this process I am then able to decompose total LP gains from each reform into indirect" (i.e., via K/L, our mediator of interest) and direct" (i.e., not via K/L) components.

4 Observed Data and building proxies

The key observed data we work with is officially published statistical reports detailing throughput and key performance indicators (KPIs). From official monthly reports we record port-level monthly TEU (20-foot-equivalent unit, a standard metric in port statistics) and tons throughput, as well as terminal-level TEU by quarter once the new terminals are opened. From yearly published KPIs we extract yearly TEU per work-hour, and various service-quality measures (e.g., average waiting, berth, and stay times) reported at the port level and - once available - at the terminal level. The table below summarizes the observed series and KPIs.

Table 1: Observed throughput series and extracted KPIs (coverage and counts)

Series	Variable	Granularity	Range	N
Haifa	Throughput (TEU)	Monthly	01-2008–10-2021	166
Haifa	Throughput (tons)	Monthly	01-2008–08-2025	200
Ashdod	Throughput (TEU)	Monthly	01-2008–10-2021	166
Ashdod	Throughput (tons)	Monthly	01-2008–08-2025	200
Haifa–Bayport	Throughput (TEU)	Quarterly	Q3-2021–Q4-2024	14
Haifa–Bayport	Throughput (tons)	Monthly	07-2021–08-2025	50
Ashdod–HCT	Throughput (TEU)	Quarterly	Q3-2021–Q4-2024	14
Ashdod–HCT	Throughput (tons)	Monthly	08-2021–08-2025	49
Haifa	KPI: TEU/hour (Π)	Annual	2018–2024	7
Ashdod	KPI: TEU/hour (Π)	Annual	2018–2024	7
Haifa–Legacy	KPI: TEU/hour (Π)	Annual	2018–2024	7
Ashdod–Legacy	KPI: TEU/hour (Π)	Annual	2018–2024	7
Haifa–Bayport	KPI: TEU/hour (Π)	Annual	2021–2024	4
Ashdod–HCT	KPI: TEU/hour (Π)	Annual	2022–2024	3

Labor proxy: Since labor hours are not publicly reported, we must create a labor proxy based on the data outlined in the table above. First, we recover annual labor hours from the published TEU-per-hour KPI:

Let y index calendar years, i index terminals (or ports for y<2023), and let TEUi, y denote the observed annual throughput aggregated from monthly/quarterly data for entity i. Let Πi, y denote TEU per work-hour for the same entity and year. Then the implied annual hours are:

$$H_{i,y} = \frac{\mathrm{TEU}_{i,y}}{\Pi_{i,y}}\,.$$

These annual port/terminal data points for L are free of bias, but such data is not granular enough for this study's needs. Currently, I am in the process of getting more granular data straight from the port authority, as more granular data is not published online in any report I am aware of. Nonetheless, I outline below one (albeit somewhat problematic) method to increase the granularity of our labor-hour measure:

By scaling observed quarterly throughput with the annual KPI, we are able to synthetically assign labor-hours to each quarter per port/terminal:

$$L_{i,y,q} = \frac{\mathrm{TEU}_{i,y,q}}{\Pi_{i,y}}, \qquad q \in \{1,2,3,4\}, \quad \sum_{q=1}^{4} L_{i,y,q} = H_{i,y},$$

This step increases granularity but relies on the assumption that labor hours per move are approximately constant within a year (i.e., that labor productivity is constant within a year):

$$\Pi_{i,y,q} \equiv \frac{\mathrm{TEU}_{i,y,q}}{L^{\mathrm{true}}_{i,y,q}} \approx \Pi_{i,y} \quad \text{for all } q \in \{1,2,3,4\}.$$

Given the nature of this study, such an assumption is arguably unacceptable. Hence, I will rely on receiving more granular labor-hour data from the non-public source, or will otherwise devise another way to get more granular labor data.

Labor productivity (once I receive more granular labor-hours data): We compute labor productivity directly as container moves per work-hour:

$$\mathrm{LP}_{p,t} = \frac{\mathrm{TEU}_{p,t}}{H_{p,t}} \quad (\text{port} \times \text{month}), \qquad \mathrm{LP}_{i,q} = \frac{\mathrm{TEU}_{i,q}}{H_{i,q}} \quad (\text{terminal} \times \text{quarter}).$$

Where $H_{p,t}$ denotes observed work-hours in that period.

Labor productivity proxy: Until granular hours arrive, we proxy productivity by combining a cargo-mix factor with the published TEU-per-work-hour KPI:

$$\widehat{\mathrm{LP}} = \left(\tfrac{\mathrm{tons}}{\mathrm{TEU}}\right) \times \mathrm{KPI}^{\mathrm{TEU/hour}}.$$

Pre-reform (port×month). For port p and month m, set

$$\widehat{\mathrm{LP}}_{p,m} = \left(\tfrac{\mathrm{tons}_{p,m}}{\mathrm{TEU}_{p,m}}\right) \mathrm{KPI}^{\mathrm{TEU/hour}}_{p,q(m)},$$

where the port-quarter KPI is implied by terminal-year KPIs via terminal TEU shares,

$$\mathrm{KPI}^{\mathrm{TEU/hour}}_{p,q} = \sum_{i \in p} \alpha_{i,p,q}\, \mathrm{KPI}^{\mathrm{TEU/hour}}_{i,y(q)}, \qquad \alpha_{i,p,q} = \frac{\mathrm{TEU}_{i,q}}{\sum_{j \in p} \mathrm{TEU}_{j,q}}.$$

Post-reform (terminal×quarter; baseline). For terminal i in port $p(i)$ and quarter q, set

$$\widehat{\mathrm{LP}}_{i,q} = \left(\frac{\sum_{m \in q} \mathrm{tons}_{p(i),m}}{\mathrm{TEU}_{p(i),q}}\right) \mathrm{KPI}^{\mathrm{TEU/hour}}_{i,y(q)}.$$

Planned variant (terminal-specific mix). Where terminal-month tons are available, define

$$\widehat{\mathrm{LP}}^{\mathrm{term}}_{i,q} = \Big(\frac{\sum_{m\in q}\mathrm{tons}_{i,m}}{\mathrm{TEU}_{i,q}}\Big)\,\mathrm{KPI}^{\mathrm{TEU/hour}}_{i,y(q)}\,.$$

This proxy preserves a clear identity (mix × KPI) and aligns frequencies across series; it will be replaced by the direct ratio LP = TEU/hours as soon as granular hours are obtained.

Capital measurement (K): overview. To construct the capital stock K, we build a proxy that captures one-time shocks (dated additions of e.g. deep water-berth meter, STS and yard cranes, etc.) and combine it with published financial metrics including deflated net property, plant, and equipment (PPE) to create a hybrid K proxy. With the K proxy we are able to build a K/L proxy:

$$M_{p,t} = \ln\left(\frac{K_{p,t}}{L_{p,t}}\right),$$

which is the mediator (capital deepening, $\ln(K/L)$) used in the mediation analysis.

5 Econometric Design

The econometric design is meant to estimate how Israel's 2021-2023 port reforms – the construction of new deep-water terminals with the entry of private operators to run them ,and the subsequent privatization of legacy operators – impacted labor productivity in the Haifa and Ashdod ports. The design has two parts: First, a staggered event-study quantifies the dynamic impact of each reform on labor productivity. Second, a 2SLS mediation design estimates the effect of capital deepening on productivity and decomposes the total reform effect into indirect (through K/L) and direct components.

5.1 Model 1 – Event-Study: Reform → ln(LP)

EQUATION:

$$Y_{p,t} = \alpha_p + \delta_t + \beta_0\,\mathbf{1}\{\tau^{(r)}_{p,t} = 0\} + \beta_1\,\mathbf{1}\{1 \le \tau^{(r)}_{p,t} \le 6\} + \beta_2\,\mathbf{1}\{7 \le \tau^{(r)}_{p,t} \le 24\} + \psi\,S_{p,t} + \varepsilon_{p,t}.$$

TERMS:

- **Outcome** $Y_{p,t}$: log labor productivity at port p in month t.
- **Fixed effects** α_p, δ_t: port FE (time-invariant differences); month FE (nationwide shocks/seasonality).
- **Event time** $\tau^{(r)}_{p,t}$: months since reform $r \in$ competition, privatization at port p.
- **Spillover flag** $S_{p,t}$: indicator that the other port is post-reform to capture spillover effects.

IDENTIFICATION:

- **Treatment (for reform r):** months with $\tau_{p,t}^{(r)} \geq 0$ at port p.
- **Controls (not-yet-treated only):** (i) the same port's pre months ($\tau < 0$); (ii) the other port's months before it experiences the same reform. Notice: Months when both ports are already treated for r drop out (This way we isolate the reform's average treatment effect).
- **Estimator (SA/CS):** Sun–Abraham/Callaway–Sant'Anna to get average treatment effects using only not-yet-treated comparisons.

INTERPRETATION:

- β_0: immediate change at the reform month.
- β_1: average change in the first six months after reform.
- β_2: average change in months 7–24 after reform.

Interpret each β as a percent change relative to pre-reform using only valid not-yet-treated comparisons. Together these coefficients communicate whether each reform raised productivity, how quickly effects appear, and whether they persist.

5.2 Model 2 – Mediation: ln(K/L) → ln(LP) (2SLS)

EQUATIONS:

$$\begin{aligned} \text{First stage:} \quad & M_{p,t} = \alpha_p + \delta_t + \lambda_1 T_{p,t}^{\text{comp}} + \lambda_2 T_{p,t}^{\text{priv}} + \omega, Z_{p,t} + u_{p,t} \\ \text{Second stage:} \quad & Y_{p,t} = \alpha_p + \delta_t + \beta_M \widehat{M}_{p,t} + \theta_1 T_{p,t}^{\text{comp}} + \theta_2 T_{p,t}^{\text{priv}} + \varepsilon_{p,t}. \end{aligned}$$

TERMS:

- **Mediator** $M_{p,t}$: log capital deepening $\ln(K/L)$.
- **Reform timing** $T_{p,t}^{\text{comp}}, T_{p,t}^{\text{priv}}$: calendar-time dummies (post-competition; post-privatization).
- **Instruments** $Z_{p,t}$: concrete engineering milestones that shift K/L by increasing K, e.g., STS crane commissioning by class (Panamax/Post-Panamax/Super-Post-Panamax), berth deepening and channel dredging completion, yard crane deliveries (RTG/RMG), terminal/yard automation go-live (TOS, gate OCR), and new berth segments entering service.
- **Fitted mediator** $\widehat{M}_{p,t}$ and **elasticity** β_M.
- **Fixed effects** α_p, δ_t: as in Model 1 (FE in both stages).

INTERPRETATION:

- From Part 1, take the total reform effects for the three windows (event month; months 1–6; months 7–24).

- From the first stage, compute the average change in predicted M over the same windows (ΔM).

- **Indirect effect via capital deepening:** $\text{IE} = (\Delta M) \times \beta_M$.

- **Direct effect:** $\text{DE} = \text{TE} - \text{IE}$.

- **Mediated share:** IE/TE (when $\text{TE} > 0$).

These outputs jointly quantify the reform's impact (magnitude and timing) and the mechanism share attributable to capital deepening, stated in a way that is easy to read on two proposal pages.

6 Overview of Data and Initial Results

I am currently waiting for terminal-level monthly labor-hours data from the port authorities, which they have confirmed will be delivered during the week of November 17. Once that data arrives, I will construct a monthly labor-productivity panel and feed it into already-existing event-study code. However, because I do not yet have access to that data, I do not include a summary statistics table here, and will either leave tables blank or fill them with synthetic values based on my current proxy for labor productivity (LP). Using this proxied data, I have constructed the following time-series visualization of labor productivity from 2018 to 2025:

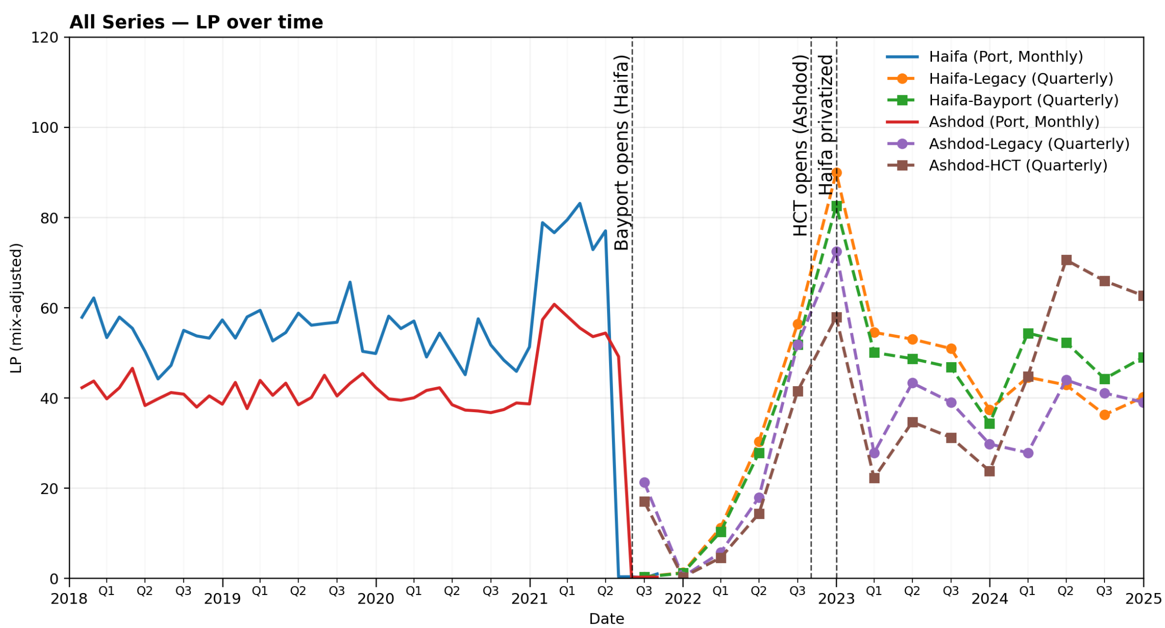

Figure 1: Labor productivity (LP) by port and terminal, 2018–2025. Monthly port-level series are shown for Haifa and Ashdod; quarterly series are shown for entrant and legacy terminals. Vertical dashed lines mark the three reforms: Bayport opening (Haifa, 09–2021), HCT opening (Ashdod, 11–2022), and Haifa legacy privatization (01–2023).

Figure 1 plots labor productivity (LP) for Haifa and Ashdod from 2018 through 2025, where port-level data split into two terminal-level series for each port following its corresponding competition-entry reform. Vertical lines mark the timing of each reform. Once I receive the monthly labor-hours data, I will update this figure and describe how LP evolves around each reform: whether we see discrete jumps, gradual trends, or no meaningful change at those dates.

6.1 Econometric Design

To estimate causal effects on LP, I use a difference-in-differences event-study design at the port/terminal-month level, with each reform treated as an event:

- Haifa Port's competition entry with the introduction of the private Bayport terminal in 09-2021
- Ashdod Port's competition entry with the introduction of the private HCT terminal in 11-2022
- Haifa-Legacy terminal's privatization in 01-2023

As Sun and Abraham (10) show, in settings with staggered adoption and potentially heterogeneous effects across cohorts and over event time, a naive two-way fixed-effects event study implicitly uses already-treated terminal-months as controls for later-treated terminals, so the resulting event-time coefficients are non-transparent and can even be biased.

To circumvent this problem, I implement a *not-yet-treated* (NYT) strategy in the spirit of Callaway and Sant'Anna (2) and Sun and Abraham (10). For each reform and treated port/terminal, I define treatment at the month when that reform first becomes active for that terminal, and then restrict the control months so that treated observations are compared only to terminal-months that have not yet experienced that same reform (or never experience it, in the case of privatization). More concretely, for each reform clock and its corresponding impacted terminals, I estimate a separate event-study regression of the following form:

$$\ln(\mathrm{LP}_{i,t}) = \sum_{m\in\mathcal{M},\, m\neq -1} \beta_m \,\mathbf{1}\{M_{i,t} = m\} + \gamma_i + \delta_t + \varepsilon_{i,t}, \tag{1}$$

where i indexes terminals (or ports in pre-split periods), t indexes calendar months, and $M_{i,t}$ denotes event time (in months) relative to the relevant reform for that regression. I define $M_{i,t} = 0$ in the month when the reform goes live. Hence, I omit the $m = -1$ bin from the regression, so each coefficient β_m measures the change in ln(LP) at event time m relative to the last pre-reform month, netting out terminal fixed effects γ_i and calendar-month fixed effects δ_t.

In each regression, the sample of (i, t) observations is restricted according to the NYT design. To make this clearer, Table 2 lists, for each regression, the terminal-months used as treatment and the terminal-months used as control. The resulting event-time coefficients β_m can be interpreted as causal effects of the relevant reform on ln(LP), relative to the last pre-reform month, using only not-yet-treated or never-treated comparisons for that reform:

Table 2: Not-yet-treated windows by reform and regression target

Reform	**Regression target**	**Treatment months**	**Control months**
Haifa competition entry (Bayport opens 09-2021)	Haifa-Bayport terminal	Haifa port: 09-2019 to 08-2021 Haifa-Bayport: 09-2021 to 10-2022	Ashdod port: 09-2019 to 07-2021 Ashdod-Legacy: 08-2021 to 10-2022 Ashdod-HCT: 08-2021 to 10-2022
	Haifa-Legacy terminal	Haifa port: 09-2019 to 08-2021 Haifa-Legacy: 09-2021 to 10-2022	Ashdod port: 09-2019 to 07-2021 Ashdod-Legacy: 08-2021 to 10-2022 Ashdod-HCT: 08-2021 to 10-2022
Ashdod competition entry (HCT effective 11-2022)	Ashdod-HCT terminal	Ashdod port: 11-2020 to 07-2021 Ashdod-HCT: 08-2021 to 09-2023	Haifa port: 11-2020 to 08-2021 Haifa-Bayport: 09-2021 to 09-2023 Haifa-Legacy: 09-2021 to 09-2023
	Ashdod-Legacy terminal	Ashdod port: 11-2020 to 07-2021 Ashdod-Legacy: 08-2021 to 09-2023	Haifa port: 11-2020 to 08-2021 Haifa-Bayport: 09-2021 to 09-2023 Haifa-Legacy: 09-2021 to 09-2023
Haifa privatization (Haifa-Legacy sold 01-2023)	Haifa-Legacy terminal	Haifa port: 01-2021 to 08-2021 Haifa-Legacy: 09-2021 to 09-2023	Haifa-Bayport: 09-2021 to 09-2023 Ashdod port: 01-2021 to 07-2021 Ashdod-Legacy: 08-2021 to 09-2023 Ashdod-HCT: 08-2021 to 09-2023

Running these regressions yields the dynamic treatment effect estimates summarized in Tables 3 and 4. Unlike settings suited to regression–discontinuity designs, where the parameter of interest is the jump exactly at the cutoff, I expect competition entry and privatization to affect LP gradually over many months after implementation. Focusing on a single event-time coefficient would, therefore, miss most of the adjustment path. Instead, I treat the post-reform event-study coefficients $\beta_m : m > 0$ as a dynamic path of the average treatment effect, and summarize this path by taking simple averages over pre-specified post-reform windows (in this case, months 1–24, 1–12, and 13–24).

Following the modern DiD event-study literature (e.g. 2; 10), I summarize the sequence of horizon-specific coefficients $\{\beta_m\}$ by taking simple averages over pre-specified event-time windows. For any window $[a, b]$, I define

$$\overline{\beta}_{[a,b]} \;=\; \frac{1}{b-a+1}\sum_{m=a}^{b}\beta_m,$$

so each reported window estimate is a linear combination of the underlying event-study ATT's and can be interpreted as the average causal effect of the reform on $\ln(\mathrm{LP})_t$ over that window.

In the tables below, for each regression, I report five window averages: (i) a "Full post" window $m \in [1, M_{\text{post}}]$, where M_{post} is the last observed post-reform month, summarizing the average effect over the entire post period; (ii) a common two-year window $m \in [1, 24]$, which allows direct comparison across ports; (iii) a first post year $m \in [1, 12]$, capturing short-run effects; (iv) a second post year $m \in [13, 24]$, capturing whether effects persist, strengthen, or decay; and (v) an "Average pre" window $m \in [-13, -2]$, which serves as a pre-trend check: under the identification assumptions of this design, the coefficient for this window is expected to be close to zero and statistically insignificant. All coefficients are interpreted as changes in ln(LP) relative to the last pre-reform month ($m = -1$). I convert window averages to percentage changes in LP via $100 \cdot (e^{\overline{\beta}_{[a,b]}} - 1)$.

Table 3: Average Post-Reform Effects on ln(LP) by Port and Terminal

Panel A: Haifa (competition clock)				
	SIPG (entrant)		*Legacy terminal*	
	(1) Baseline	(2) +PortTr	(3) Baseline	(4) +PortTr
All post $m \in [1, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 24]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 1, $m \in [1, 12]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 12]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 2, $m \in [13, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[13, 24]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Average pre, $m \in [-13, -2]$	β (SE)	β (SE)	β (SE)	β (SE)
Pre-trends: F-test p-value	p	p	p	p
Observations	N	N	N	N
Within R^2	R^2	R^2	R^2	R^2
Panel B: Ashdod (competition clock)				
	HCT (entrant)		*Legacy terminal*	
	(5) Baseline	(6) +PortTr	(7) Baseline	(8) +PortTr
All post $m \in [1, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 24]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 1, $m \in [1, 12]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 12]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 2, $m \in [13, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[13, 24]$	$\Delta\%$	$\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Average pre, $m \in [-13, -2]$	β (SE)	β (SE)	β (SE)	β (SE)
Pre-trends: F-test p-value	p	p	p	p
Observations	N	N	N	N
Within R^2	R^2	R^2	R^2	R^2

Outcome is $\ln(\mathrm{LP}_{it})$ at the terminal×month level. All columns are event-study regressions with terminal and calendar-month fixed effects; columns labeled "+PortTr" additionally include port-specific linear time trends. Event time m is measured in months relative to competition entry, with $m = -1$ omitted; not-yet-treated (NYT) observations serve as controls under the relevant competition clock. "Implied $\%\,\Delta$ LP" converts window averages via $100 \cdot (e^{\overline{\beta}_{[a,b]}} - 1)$. "Average pre, $m \in [-13, -2]$" summarizes lead coefficients and is used as a pre-trend check: under the identifying assumptions, this estimate should be small and statistically insignificant. "Pre-trends: p(Leads F-test)" reports the p-value of a joint test that all lead coefficients $m \leq -2$ are zero. Standard errors (in parentheses) are clustered by port (two clusters); significance stars are based on wild-cluster bootstrap p-values: ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$. "Within R^2" is the within-variation R^2 from the fixed-effects regression.

Table 4: Haifa Privatization — Dynamic and Window-Average Effects

	Panel A: Haifa–Legacy (privatization)		**Panel B: Haifa Bayport (placebo)**	
	(1) Baseline	(2) +PortTr	(3) Baseline	(4) +PortTr
All post $m \in [1, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 24]$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 1, $m \in [1, 12]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[1, 12]$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Post year 2, $m \in [13, 24]$	β (SE)	β (SE)	β (SE)	β (SE)
Implied $\%\,\Delta$ LP, $[13, 24]$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$	$\pm\Delta\%$
Average pre, $m \in [-13, -2]$	β (SE)	β (SE)	β (SE)	β (SE)
Pre-trends: F-test p-value	p	p	p	p
Observations	N	N	N	N
Within R^2	R^2	R^2	R^2	R^2

Outcome is $\ln(\mathrm{LP}_{it})$ at the terminal×month level. All columns are event-study regressions with terminal and calendar-month fixed effects; columns labeled "+PortTr" additionally include port-specific linear time trends. Event time m is defined relative to the privatization of the Haifa–Legacy terminal (January 2023, $m = 0$); the month $m = -1$ is omitted, and not-yet-treated (NYT) observations under the privatization clock serve as controls. Window-average rows report equal-weight means of the event-time effects β_m over the indicated ranges. "Average pre, $m \in [-13, -2]$" summarizes the lead coefficients and is used as a pre-trend check: under the identifying assumptions, this estimate should be small and statistically insignificant. "Implied $\%\,\Delta$ LP" converts window averages via $100 \cdot \left(e^{\overline{\beta}_{[a,b]}} - 1\right)$. "Pre-trends: F-test p-value" reports the p-value of a joint test that all lead coefficients $m \in [-13, -2]$ are jointly statistically insignificant. Panel A reports privatization effects for Haifa Legacy; Panel B applies the same clock as a placebo to Haifa Bayport (SIPG), which is never privatized. Standard errors (in parentheses) are clustered by port; significance stars are based on wild-cluster bootstrap p-values: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. "Within R^2" is the within-variation R^2 from the fixed-effects regression.

Discussion of results is delayed until I receive labor data from the Israel Ports Company the week of November 17.

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Appendix A. Full Dynamic Event-Time Tables

A.1. Full Event-Time Estimates — Haifa

Table 5: Full Event-Time Estimates — Haifa

	SIPG (entrant)			*Legacy*			
m	(1) Baseline	(2) +PortTr	(3) +Tr&Shocks	(4) Baseline	(5) +PortTr	(6) +Tr&Shocks	$N(m)$
-4	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
-3	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
-2	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
0	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
1	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
2	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
3	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
4	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
5	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
6	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
7	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
8	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
9	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
10	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
11	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
12	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
13	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
14	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
15	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
16	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
17	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
18	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
19	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
20	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
21	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
22	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
23	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
24	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n

Notes: Event time m is measured in months relative to the relevant competition-entry reform ($m = 0$). The month $m = -1$ is omitted as the reference bin. All specifications include terminal and month fixed effects; "+PortTr" adds port-specific linear trends; "+Tr&Shocks" additionally includes COVID (2020–21) and late-2023 war-shock controls. β_m are event-time coefficients with standard errors in parentheses. $N(m)$ reports the number of terminal×month observations supporting each event-month m under the baseline NYT specification.

A.2. Full Event-Time Estimates — Ashdod

Table 6: Full Event-Time Estimates — Ashdod

	HCT (entrant)			*Legacy*			
m	(1) Baseline	(2) +PortTr	(3) +Tr&Shocks	(4) Baseline	(5) +PortTr	(6) +Tr&Shocks	$N(m)$
-4	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
-3	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
-2	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
0	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
1	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
2	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
3	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
4	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
5	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
6	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
7	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
8	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
9	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
10	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
11	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
12	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
13	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
14	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
15	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
16	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
17	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
18	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
19	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
20	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
21	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
22	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
23	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n
24	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	n

Notes: Same NYT event-study specification as Table 5, but with event time m defined relative to Ashdod's competition-entry reform. The omitted reference bin is $m = -1$. All specifications include terminal and month fixed effects; "+PortTr" adds port-specific linear trends; "+Tr&Shocks" additionally includes COVID (2020–21) and late-2023 war shock controls. β_m are event-time coefficients with standard errors in parentheses. $N(m)$ reports the number of terminal×month observations supporting each event-month m under the baseline NYT specification.