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Executive Summary

Israel's 2021–2023 seaport reform – introducing private terminals in Haifa and Ashdod and privatizing Haifa's legacy port – sought to tackle chronically low productivity in a union-dominated duopoly [1] [2]. The original thesis aimed to quantify these reforms' impact on labor productivity and the share of gains due to capital deepening (higher capital per worker). However, the unavailability of granular labor-hour data ("L") necessitates a pivot. This report proposes ~30 rigorous research designs under three thematic **umbrellas** that avoid immediate reliance on L, yet remain upgradeable if labor hours or Net Ship Working Time data become available. Each design emphasizes credible causal identification – through event studies, Regression Discontinuity in Time (RDiT), synthetic controls, Data Envelopment Analysis (DEA), quantile Difference-in-Differences (DiD), or triple-differences – and specifies concrete outcomes, data sources, feasibility considerations, and how adding L later can enhance the analysis.

Umbrella 1 – Port Reforms: We extend the port study comparatively. Designs here examine Israel's staggered port reforms in a broader context: cross-country panels of port reform outcomes, synthetic controls comparing Israel to "no-reform" counterfactuals, and intra-Israel comparisons isolating competition vs. privatization effects. These ideas build on literature showing competition is the primary driver of port efficiency gains (more so than privatization alone) [3] [4]. They avoid immediate L-dependence by using published port productivity metrics (e.g. throughput, ship wait times) and enable future inclusion of labor productivity once L data can be obtained.

Umbrella 2 – Reforms of Non-Tradable Sectors: We pivot to other Israeli non-tradable industries (and analogous cases abroad) where privatization, regulatory change, or competition policy have been or could be implemented to boost productivity and lower costs. These ~10 ideas span telecom (e.g. the dramatic 2012 mobile market liberalization that cut prices ~90% [5] [6]), civil aviation (Open Skies agreement), banking (credit market reforms), electricity (market opening), public transport (bus service tendering), and consumer goods (tariff reductions). Each design leverages natural experiments or policy changes to identify causal effects on outcomes like prices, output per firm, service quality, or investment – crucially without requiring direct measurement of labor hours. Given that weak competition in domestic sectors is a key reason Israel's overall productivity lags peers [7] [8], these studies fill evidence gaps on which reforms truly deliver efficiency gains versus unintended consequences. They remain flexible to incorporate labor productivity measures later if L data become accessible (for example, to see if lower prices came from efficiency or labor cost-cutting).

Umbrella 3 – Low Labor Productivity in Israel: These ideas address deeper structural mechanisms behind Israel's persistent labor productivity gap – currently ~24% below the OECD average and unchanged over decades [9]. We propose causal studies into sectoral frictions, capital shallowness, and diffusion barriers. Topics include misallocation (adapting Hsieh & Klenow's approach to Israel), the impact of large-scale immigration on productivity (FSU influx of the 1990s [10] [11]), the effect of infrastructure improvements (e.g. new rail lines) on regional productivity, and whether raising labor costs (e.g. minimum wage hikes) forces productivity gains via capital substitution. These designs often exploit policy shocks or heterogeneous exposure to identify causal effects. They prioritize outcomes like output per worker (or proxies) and TFP, steering clear of immediate L requirements. Given Hazan & Tsur's finding that **accumulated factors**

(physical and human capital) explain ~76% of Israel's output-per-worker gap [12], many ideas focus on capital deepening and skill diffusion as levers. When detailed labor-hour data eventually become available, these studies can be upgraded to directly compute labor productivity or to decompose total productivity changes into intensive (per-hour) versus extensive (hours) margins.

Methodology & Design Quality: For each umbrella, we first synthesize literature and identify knowledge gaps. We then present an **Idea Catalog** – a structured one-pager per idea detailing research question, identification strategy, data, anticipated findings, risks, and L-data upgrade path. Each idea is grounded in precedent (academic studies or official reports) yet innovates to address gaps. Across the board, we emphasize high causal credibility through difference-in-differences (with **Sun & Abraham (2021)** or **Callaway & Sant'Anna (2021)** estimators to handle staggered adoption [13]), **triple-differences** (to isolate mechanisms like competition vs. ownership), **synthetic controls** (for single-case reforms), **RDiT** (for sharp policy implementation dates), **DEA/SFA** (to measure efficiency changes without needing L), and **quantile DiD** (to explore distributional effects, e.g. did reforms lift laggard firms most or least). All designs are scoped to be feasible within a semester using public data (e.g. Israeli CBS publications, Ministry reports, OECD/World Bank datasets, RePEc/SSRN papers) and the student's existing connections (e.g. port authority yearbooks). Feasibility risks (data gaps, confounders) are noted for each, with mitigation strategies.

Top Recommendations: From the 30 options, we highlight 3 high-potential projects (two under U2, one under U1) as most promising for an H191 thesis pivot: **(1) Mobile Telecom Liberalization & Productivity** (U2) – leverage the **2012–2015 "cellular revolution"** to causally measure consumer welfare gains versus any productivity/investment trade-offs, using a difference-in-differences with OECD comparators [5] [6]. **(2) Port Reform Productivity Effect (Competition vs Privatization)** (U1) – employ a **staggered event study** and **triple-diff** to separately identify how Haifa's new private terminal and subsequent privatization impacted port throughput and efficiency, using Ashdod as a comparison (no privatization) [14] [15]. **(3) Open Skies Policy Impact on Airfares and Tourism** (U2) – treat Israel's 2013 Israel–EU Open Skies agreement as a quasi-experiment to measure airfare reductions and traffic growth [5] [16], using a synthetic control or interrupted time series. These projects are compelling due to strong **policy relevance** (cost of living and trade competitiveness), robust identification, and readily available data (price indices, port operational stats, tourism records). Each can later be augmented with labor-hour metrics – for instance, adding dockworker hours to the port study to compute actual labor productivity, or examining airline industry employment changes post-Open Skies – to enrich the analysis when possible.

In sum, this report charts a path to pivot the thesis toward impactful, doable research on Israeli reforms and productivity, ensuring the analysis remains rigorous and contributory even without immediate access to labor-hour microdata. Below, we detail the context and literature for each umbrella, then present a catalog of ~10 ideas per category, and finally provide structured artifacts (JSON, TSV) summarizing all ideas, along with an estimator and figure/table game plan for the recommended projects. The goal is to equip the thesis with multiple "plan B" options that maintain empirical ambition and policy insight while navigating data constraints.

Introduction and Context

Persistently Low Productivity in Israel: The Challenge

Israel's labor productivity (output per worker) has stubbornly trailed peer countries for decades [17] [18]. Despite Israel's reputation as a "Start-Up Nation" with a world-class tech sector, aggregate productivity per hour remains ~24% below the OECD average, with the gap **"not narrowed over the past twenty years"** [19] [20]. This productivity shortfall is often attributed to a dualistic economy: a highly productive export-oriented sector alongside **inefficient non-tradable industries** that serve the domestic market [21] [22]. Key diagnostics include:

- **Low Physical Capital per Worker:** Israel's capital stock is unusually shallow. Hazan & Tsur (2021, provided input) find Israel's **equipment stock per worker is about 50% of that in small OECD peers** (and non-residential structures ~60%) [23]. This "missing capital" manifests especially in infrastructure and machinery-intensive sectors [8]. It implies that many Israeli workers have fewer or older tools, dragging down their output.

- **Human Capital Paradox:** Israelis are well-educated on paper (high schooling years), but skill surveys reveal **deficits in practical literacy/numeracy**. When adjusting human capital measures to include cognitive skills (PIAAC scores), **accumulated factors (physical and human capital) explain ~76% of Israel's productivity gap vs. small OECD economies** [12] – a much larger share than previously thought. In other words, low total factor productivity (TFP) is less to blame than deficits in capital and effective skills. This suggests raising capital per worker and improving skill diffusion could close much of the gap.

- **Structural Frictions in Non-Tradables:** A series of studies (Taub Center, OECD, IMF) have flagged Israel's domestic sectors – e.g. ports, electricity, food retail, transportation, construction – as **rife with monopolies, heavy regulation, and low innovation** [24] [8]. These sectors face limited foreign competition and often have powerful organized interests (unions, cartels). **Brand & Regev (2015)** show five large non-tradable industries accounted for 81% of the widening productivity gap 1995–2009 [25] [21]. They conclude that **lack of competition in local services is the primary culprit**, not tradeables or high-tech. For instance, sheltered service industries did not experience the same productivity spurts that exposed manufacturing did after trade liberalization [26] [27].

- **Diffusion Barriers:** There is limited spillover from the high-productivity frontier to the rest. The high-tech sector's growth has not lifted productivity elsewhere, due to **low labor mobility between tradable and non-tradable sectors** [28] [29]. Skilled workers concentrate in tech/export firms, while a large share of workers in domestic services remain less skilled. This segmentation means wage pressure from the booming sector doesn't translate into widespread capital investment or process upgrades in lagging sectors [30] [31]. One policy assumption – that boosting high-tech will indirectly force traditional sectors to modernize – may be flawed if workers and practices don't circulate [28] [29].

Against this backdrop, recent governments have pursued structural reforms targeting the **non-tradable sector inefficiencies**. The 2011 social justice protests (over high living costs) galvanized efforts to introduce competition in sheltered markets [32] [33]. Notably, the Trajtenberg Committee (2011) explicitly linked high

consumer prices to **port inefficiency, food import barriers, and airline cartelization**, among others [34] [33]. Major reform initiatives followed: the **ports competition reform**, **Open Skies in aviation**, retail import tariff cuts, telecom market liberalization, and attempts at banking and electricity reform. Each aimed to increase competition, lower costs, and potentially boost productivity.

Original Thesis Plan and Data Pivot

The student's original H191 thesis proposal (Kehat, 2025) honed in on one such reform: Israel's seaport overhaul. The plan was to measure the causal impact of the 2021–2023 port reforms on port labor productivity and the share of productivity gains attributable to **capital deepening (K/L)**. The design combined a **staggered Difference-in-Differences event study** (for each reform's effect on capital per worker and output per worker) with an **IV-mediation analysis** to decompose how much of the labor productivity change was mediated by increased capital intensity [13] [35]. In particular, Israel's reform provides a unique natural experiment to disentangle two oft-conflated strategies: introducing competition (via new terminals) versus privatizing an incumbent. The timeline: Haifa Port got a private competitor in Sept 2021, Ashdod in late 2022, and Haifa's government-run port was privatized in Jan 2023 [36] [14]. This allowed a within-country comparison: **Haifa experienced both competition and privatization; Ashdod had competition but remains state-run**, effectively serving as a control to isolate the impact of ownership change [14] [15]. The proposal outlined careful steps to control for confounders (e.g. using the other port as a contemporaneous control, adding spillover indicators) [37] [38], and to instrument K/L using engineering milestones (new cranes, dredging) [39] [40]. The **data requirement**, however, was intense: monthly port-level panel data on output, capital, and labor hours. While output and some efficiency KPIs (e.g. TEU per work-hour annually) are published in the **Shipping & Ports Yearbook** and State Comptroller reports [41] [42], the **labor-hours series (L)** is not publicly available at needed frequency. The student planned to request internal data or construct proxies (e.g. using annual TEU/hour KPIs to impute total hours) [43] [44]. As of now, those L microdata remain elusive, jeopardizing the core labor productivity measures.

Implication: The thesis must pivot to research questions and designs that do not *immediately* depend on obtaining port labor-hour microdata. This does not mean abandoning the productivity theme – rather, we seek alternate outcomes or creative proxies to study similar mechanisms. All designs should be conceived such that they are **upgradeable**: if later the labor data become available (whether port hours or "Net Ship Working Time" or similar productivity indicators), the analysis can be enriched or recalibrated to directly measure labor productivity or delve into labor-specific channels. But crucially, the designs must stand on their own using currently accessible data.

Research Pivot Strategy

We identify three thematic umbrellas to organize the exploration of new thesis directions:

- **U1: Port Reforms – Comparative or Expanded Analysis.** This umbrella builds on the original topic but broadens scope. We consider studying port reform impacts in a cross-country setting or over a longer horizon, and employing methods like synthetic control or DEA that don't require internal labor data. The idea is to leverage the rich global literature on port privatization/competition to either generalize the Israel case or to exploit variation beyond the two-port scenario. For example, examining other countries' port reforms (e.g. Mexico 1993, Spain 1990s, or recent developing-country reforms) via panel event studies [45] [46]; or using Israel as a case study in a synthetic control

comparing its post-reform trajectory to similar countries without such reforms. Designs in U1 will use **observable outcomes like throughput, turnaround time, or efficiency scores** as proxies for productivity, avoiding a strict need for labor input. This umbrella aligns with literature gaps such as separating competition vs. privatization effects [47] [48] and improving identification beyond simple pre/post comparisons [49].

- **U2: Reforms of Non-Tradable Sectors – Israel & Abroad.** Here we pivot to other sectors where low productivity is rooted in lack of competition or mis-regulation. The umbrella spans privatizations, deregulations, or competition-enhancing policies in domestic markets. We prioritize sectors highlighted by Israeli policymakers and researchers as problematic: e.g. telecommunications, energy, transportation, finance, and retail services [8] [50]. Many of these underwent reforms in the 2010s. For instance, Israel's **mobile telecom reform** around 2012 led to an influx of new carriers and a drastic drop in prices [5] [6]; the **Open Skies agreement (2013)** opened air travel to European low-cost carriers, expected to reduce airfares [51] [16]; the **Strum Committee reforms (2016)** mandated banks to divest credit card subsidiaries to spur competition in consumer credit; the **electricity market reform (2018)** began breaking up the state electric monopoly; etc. We will also consider analogous reforms abroad (to draw lessons or as quasi-control cases), such as bus service deregulation in the UK, or telecom privatizations in other countries, etc. Each idea will formulate a causal hypothesis (e.g. "Did opening sector X to competition improve productivity/efficiency in X?") and propose a method to test it (often DiD with another country or sector as control). These studies generally focus on **outcomes like prices, output, quality, or firm productivity** rather than aggregate labor productivity, thus side-stepping the need for L data. They address the broad gap: which specific market reforms actually translate into productivity gains vs. merely lower consumer prices (or in some cases, unintended effects like under-investment). Given the centrality of non-tradables in Israel's productivity gap [8] [50], evidence here has high policy relevance.

- **U3: Low Labor Productivity – Sectoral Mechanisms and Frictions.** This umbrella digs into structural reasons behind low productivity, beyond any single reform. The ideas are more exploratory/analytical but still employ causal designs where possible. We consider factors like **misallocation of resources, labor market frictions, capital deepening barriers, and technology diffusion lags**. Examples: applying the Hsieh & Klenow (2009) misallocation framework to Israel's firm data to quantify deadweight loss from capital misallocation; evaluating how a **large exogenous labor supply shock** (the 1 million Soviet immigrants in the 1990s) affected productivity in various industries (e.g. did sectors that absorbed more immigrants see productivity rises or declines? – using a differences design) [11] [52]; assessing whether **infrastructure investments** (like new highways or railways) lead to productivity improvements in connected areas via better allocation of labor and capital (using spatial DiD); testing if raising the minimum wage induced low-wage firms to automate and increase output per worker (a kind of forced capital deepening scenario). These studies can often use **industry or regional-level data** (where labor input might be available from national accounts in aggregate, though maybe not hours – value-added per worker could proxy productivity). They tackle the big-picture question: what policies or shocks can truly boost labor productivity in the lagging bulk of Israel's economy? And conversely, which commonly cited frictions (e.g. skill mismatch, market concentration, under-investment) have demonstrable causal impacts on productivity? While these ideas might be less directly tied to the port story, they resonate with the thesis's underlying theme of raising productivity and the role of capital deepening (or lack thereof). Each can be enhanced later by bringing in micro-data on labor or capital if accessible (e.g.

incorporating actual hours to refine productivity measures, or using firm-level data to validate mechanisms).

Research Approach: For each umbrella, the report will first **review key literature** and establish what is known versus what gaps remain. We compile ~5–15 key citations per umbrella (from provided inputs and external sources such as RePEc/SSRN working papers, OECD/World Bank studies, etc.) to ground our understanding. We then summarize these in a **Literature & Gap Matrix** – clearly delineating established findings (the "known") and unanswered questions or contradictory evidence (the "missing") that our proposed ideas could address. This ensures each idea is well-motivated by a gap in knowledge. We explicitly note which sources are **Provided Input** (e.g. Hazan & Tsur, the student's dossier/proposal) versus **External** (e.g. global studies or data from international organizations) when citing, to maintain transparency.

Finally, under each umbrella, we present the **Idea Catalog**: roughly ten 1-page idea cards. Each card follows a consistent schema (see Structured Artifacts section) capturing: the idea's title, research question, causal identification strategy (design/estimator), outcome variables, data sources, potential pitfalls, and upgrade path if L data becomes available. The ideas are distinct yet interrelated, offering a menu of options for pivoting the thesis. They have been designed with H191 constraints in mind – i.e. scoped for completion in a few months by one student, using accessible data and established econometric techniques, and emphasizing credible causal inference.

By offering around 30 ideas, the student can choose one primary new direction (or possibly a couple of smaller complementary analyses) that best fits their interest and data opportunities, knowing it will be grounded in solid literature and feasible methodology. The following sections provide the in-depth analysis for each umbrella and then the catalogs of ideas.

Umbrella 1: Port Reforms (Comparative/Expanded Analysis)

Literature Review – Port Reforms & Productivity

A rich body of literature examines how port sector reforms (especially in the wave of 1990s–2000s privatizations and deregulations) affect efficiency and productivity. Two consistent themes emerge: **competition** is critical, and **ownership change alone is not a panacea**. Key findings include:

- **Competition vs. Privatization:** Global empirical studies show introducing intra-port competition (multiple terminal operators) tends to yield larger productivity gains than privatization in isolation. For example, **Cheon, Dowall & Song (2010)** analyze dozens of ports worldwide and find that transferring operations to private entities **only significantly raises total factor productivity when coupled with competition** – i.e. when private entry breaks monopoly power [3]. Private monopolies often fail to improve efficiency much more than public ones; it's the **competitive pressure** that drives operators to optimize. Similarly, **Cullinane, Ji & Wang (2005)** using DEA efficiency analysis found that **a port with multiple competing terminals outperforms a privatized single-operator port** [4]. **Tongzon & Heng (2005)** also report that efficiency differences post-

reform correlate more with new capital investment and technology (often spurred by competition) than with ownership per se [53] [54]. These studies underpin the Israeli reform's logic: simply privatizing a state-run port may not achieve much if it remains a monopoly – hence Israel did both (new entrants and later privatization) [55] [56].

- **Evidence from Country Cases:** Numerous country case studies in the 1990s/2000s document significant port productivity improvements following reforms, particularly when competitive elements were introduced. **Estache, González & Trujillo (2002)** on Mexico's 1993 port reform (decentralizing a federal monopoly into regional authorities and allowing private concessions) found a short-term **~15% efficiency jump** after privatization and decentralization [57], with improvements attributed to both **"frontier shifts" (technology/capital upgrades)** and **"catch-up" (better labor utilization)** [58]. However, they did not quantify each channel. In Spain, González & Trujillo (2009) observe that ports which enabled intra-port competition (multiple terminal operators) had **higher efficiency gains** than those that merely privatized one operator [59] [60]. **Brooks & Cullinane (2007)** (not directly cited above but part of the literature) similarly suggest landlord port models (public ownership of land, private operation of terminals) yield better results than service ports (public operation) or full privatization without competition. These cases align with the notion that competition-induced capital deepening and improved labor management are behind the efficiency gains. In Israel's context, such case evidence supports evaluating the separate contributions of the **new private terminals (competition effect)** versus the **sale of Haifa Port Company (privatization effect)**.

- **Efficiency Measurement Approaches:** Methodologically, port productivity studies have used various techniques – from simple cargo-per-hour metrics to advanced DEA (Data Envelopment Analysis) and Malmquist productivity indices. DEA studies (Cullinane et al. 2005; Tongzon 2001; Barros 2006) treat ports as units producing output (e.g. TEUs, ship calls) from inputs (labor, capital, land) and calculate efficiency scores. Reforms often show up as efficiency score improvements or productivity growth in Malmquist indices. For instance, a **2025 study by Danladi et al.** applied Malmquist DEA to 39 lower-middle-income country ports (2001–2012) and found **+1.9% average annual productivity growth**, driven mainly by **technical efficiency improvements** (pure efficiency change) [61]. They link many of these improvements to reform efforts (commercialization, corporatization) in those countries [45] [62]. This suggests analyzing Israel's reform via an efficiency frontier approach could be fruitful, especially if granular input data can be gathered.

- **Identification Challenges:** A gap in older studies is that many used before-after comparisons or panel regressions without fully convincing controls. Some used annual data only, making it hard to separate reform impact from general trends (global trade boom, etc.). **Sun & Abraham (2021)** and others have since highlighted biases in "naïve" two-way fixed effects if reforms are staggered [63]. Israel's staggered timeline (2021, 2022, 2023 events) actually permits a cleaner event-study approach to trace dynamics, provided monthly/quarterly data are used [13]. Few port studies have exploited high-frequency data; doing so is an innovation this thesis can bring, as noted in the proposal [49] [37]. Additionally, **Clarke (2017)** and others discuss controlling for spillovers (e.g. one port's reform possibly affecting the other via shipping network shifts) [39] – something rarely addressed in earlier literature. Our designs will incorporate such controls (e.g. indicators for rival port's treatment status [64] [65]).

- **Mechanism Gaps:** Perhaps the most novel gap the thesis touched on is quantifying **mechanisms** – in particular, how much of productivity gains come from **capital deepening vs. better labor utilization**. The literature acknowledges both occur but doesn't measure the split [66] [67]. Cheon et al. (2010) hinted that reforms often coincide with technology upgrades (new cranes, automation) – a capital effect – and require workforce changes, but no study pinned down the portion of output gains due to increased K/L. Israel's case, with measurable additions of capital (e.g. brand-new terminals with mega-ship cranes) and changes in labor practices (likely higher labor productivity at the new terminals), offers an opportunity to decompose that. **Dippel & Ferrara (2020)** introduced an IV-based mediation in a trade context, which the proposal aimed to adapt [68] [35]. No known port study has applied such mediation analysis. The gap remains: isolating the **indirect effect** of reforms via capital deepening from the **direct effect** (organizational/competitive efficiency) [15] [55]. Our designs under U1 keep this goal in mind, although full mediation may wait for L data. In the interim, we can still compare indicators of capital investment (cranes added, etc.) and productivity outcomes qualitatively.

- **Israeli Port Context:** Prior to this thesis work, **no academic study had rigorously evaluated Israel's port reforms**. Available sources were policy reports and news: e.g. Israel's State Comptroller 2020 report noted improved efficiency over time but did not perform causal analysis; OECD's 2011 roundtable summary anticipated that intra-port competition would improve Israeli port performance [69] [70]. The **research dossier (Provided)** chronicles the narrative: severe pre-reform inefficiencies (17-hour waits, labor strikes) [71] [72], a 2005 corporatization that helped modestly (waits down to ~4–6 hours by 2012) [73] [74], but only with the 2021–22 new terminals did real competition arrive, followed by Haifa's privatization in 2023 [75] [14]. Initial outcomes reported: By end of 2022, the new Haifa Bayport (SIPG) grabbed 88% of Israel's transshipment traffic, dramatically improving capacity utilization [76] [77]. Haifa's legacy port saw an exodus of volume and labor adjustments (the new terminal operates with ~150 workers vs. ~1,300 at the old port, implying a quantum leap in productivity per worker) [78] [79]. These early data points need formal analysis, but they suggest **huge efficiency gains** – and also hint at distributional impacts (legacy ports losing business and possibly shedding labor). Another aspect: the Haifa port privatization sale in 2023 came with a required **$290M investment pledge** from the buyers [80] [80], signifying expected capital deepening. Meanwhile, Ashdod Port Company (still state-run) did invest in new cranes and tech preemptively, but remains less efficient, and its full privatization is stalled politically [81] [82]. Thus, by 2024 we have a natural experiment: **Haifa = competition + private management; Ashdod = competition + state management**. This juxtaposition directly addresses a gap pointed out by Cullinane & Song (2002): how does private-vs-public ownership performance compare *when competition is present*? Our triple-diff design in U1 will exploit this.

In summary, the literature establishes that **port reforms can substantially raise productivity**, especially via competition-induced efficiency and investment [3] [4]. However, it also shows the importance of careful analysis to isolate channels and avoid overstating effects. The Israeli case offers a contemporary testbed to contribute evidence on the competition vs. privatization debate and the capital vs. labor mechanism – filling gaps in both global literature and Israeli policy evaluation. We will lean on methodologies suggested by recent econometric advances (event-study DiD with proper controls [13] [37], IV for mediation [68] [35]) to ensure credible results. The following gap matrix summarizes the key knowledge versus gaps, guiding our idea generation.

Gap Matrix – Port Reforms Literature vs. Our Focus

What is Known (Literature)	What is Missing (Gaps)
Competition is key: Ports with intra-port competition outperform monopolies, and reform success often hinges on introducing multiple operators [4] [53]. Privatization alone gives limited gains without competition.	Causal evidence isolating **competition vs. privatization** effects in one setting. Past studies often bundled them or compared different ports, not the same port under both scenarios [47]. Israel's staggered reform is ideal to separate these (gap the thesis can fill).
Productivity gains documented: Numerous cases (Mexico, Spain, etc.) show post-reform efficiency jumps (10–20%+). Both technical change (capital/technology) and efficiency catch-up (labor utilization) are implicated [58] [83].	**Quantification of mechanisms**: No study measures what share of the productivity gain came via increased K/L vs. better labor use [67]. The mediation of competition → K/L → output is presumed but not measured. We lack direct decomposition of reform benefits by channel – a novel angle for this thesis.
Measurement techniques: DEA and Malmquist indices show efficiency improvements; simple ratios (TEU/worker, ship per hour) improve post-reform. Competition correlates with higher capital investment (new equipment) and throughput [54] [84].	**High-frequency causal estimates**: Few if any studies used monthly data or an event-study framework to *trace dynamics* of productivity around reform implementation. Identification in time is weak in prior work (annual data, pre-post). We don't know the *timing* of when gains materialize or if there are anticipatory effects or transition costs – something a monthly event study in Israel could reveal [49] [37].
Global context: Efficiency gains from port reforms contribute to trade cost reductions. Clark et al. (2004) found port efficiency differences significantly affect shipping costs. Improved ports can reduce maritime transport costs by double-digit percentages [85].	**Trade and spillover effects**: Understudied is how domestic port reforms affect broader outcomes like trade flows, user prices, or rival ports. In Israel's case, did improving Haifa hurt Ashdod (or force it to improve)? Did import costs drop and trade volumes rise? These spillovers and general equilibrium effects are not fully quantified in literature or Israeli reports – an area for extended analysis (e.g. triple-diff or synthetic control on trade outcomes).
Israeli ports pre-reform: Notorious for inefficiency (long waits, powerful unions) [71] [2]. Earlier minor reforms (2005 corporatization) had some effect but left duopoly intact [73] [74]. The expectation (OECD 2011) was that new terminals + privatization would significantly boost productivity and lower cargo handling costs [69] [70].	**Israel post-reform empirical study**: There is a vacuum of rigorous empirical work on the actual impact of the 2021–23 reforms – a gap this thesis can fill. We have anecdotal data (e.g. Bayport's rapid market capture, labor cuts, etc.) but no formal analysis attributing causality. Also, as reforms are very recent, data is only now becoming available (monthly port stats through 2024). A timely opportunity exists to be the first study analyzing these reforms' effects with econometric rigor.

Using these insights, Umbrella 1 will pursue ideas that exploit Israel's reform timing and available data, and/or incorporate cross-country evidence, to answer: **How much did port reforms improve**

performance, through what channels, and what lessons does Israel's experience offer on competition vs. privatization? Each idea avoids reliance on confidential labor-hour data by using alternate outputs (throughput volumes, efficiency ratios, wait times, cost indices, etc.), but is designed such that adding labor input later (e.g. to compute output per worker) would strengthen the conclusions.

Idea Catalog – Port Reforms (U1)

Each idea card below outlines a distinct research project related to port reforms. We provide a title, research question, identification strategy (with estimator), outcome measures, data sources, potential challenges, and how the design could integrate labor-hour (L) data if it becomes available.

U1.1 – The Causal Impact of Israel's Port Reforms on Throughput and Efficiency

Question: Did the 2021–2023 port reforms in Israel causally increase port output and operational efficiency, and if so, when and by how much? *Sub-questions:* How do the effects unfold over time (dynamic event study)? Are gains larger at Haifa (first reform) than Ashdod?

Identification Strategy: Event-Study Difference-in-Differences leveraging the staggered rollout of three reform events: Haifa new terminal (Sept 2021), Ashdod new terminal (late 2022), Haifa privatization (Jan 2023). We use Sun & Abraham (2021) corrections to compute unbiased event-time effects [13] [37]. Essentially, we treat each reform as a "treatment" applied to a specific port at a specific time, with the other port serving as a control in that period. We include port fixed effects and month fixed effects, plus a "spillover" indicator for whether the *other* port has a new competitor at time t (to isolate direct effects) [64] [65]. This two-port panel (monthly 2018–2024) yields a **2x3 matrix** of event studies (2 ports × 3 reform events) which we estimate jointly using interaction terms. The primary coefficients of interest are the average post-treatment jumps in outcomes for each event. We will use **clustered standard errors** at port level (two clusters; wild bootstrap for inference due to low cluster count) [86] [87].

Outcomes: (1) **Throughput volume** (containers or tons) – measures output. (2) **Ship turnaround/wait time** – measures efficiency/service quality. (3) **Operational productivity** – e.g. TEU per ship or gross moves per hour (if monthly data available from internal reports). These are all **observable** via published sources: the Administration of Shipping & Ports Yearbooks report monthly TEUs and annual average wait times [88] [89]. We expect throughput to jump post-competition (as new capacity draws traffic) and wait times to drop. These are proxies for productivity improvements perceived by users.

Data: Provided Input: Israel's **"Shipping and Ports Statistical Yearbook 2024"** gives monthly throughput by port (2008–2024) [90] and annual KPIs (2018–2024) like TEU per work-hour and ship wait times [91] [92]. We will digitize these (the student already compiled much of this in Table 1) [90] [92]. Additional data from the **Israel Ports Company** website or Ministry of Transport bulletins can extend KPI series (e.g. wait times prior to 2018, if available). No confidential data is needed; all figures are public.

Feasibility & Risks: With only 2 ports, the sample is tiny – careful inference (e.g. wild bootstrap) is needed. Staggered timing helps identification, but contemporaneous factors (COVID in 2020–21, global shipping disruptions) could confound results. We mitigate by including year-month fixed effects (common shocks) [93] [94], and by possibly adding external control series (e.g. total East Med container volumes) in a supplementary analysis. Another risk: Ashdod's "treatment" (new terminal) was gradual (partial operations Aug 2021, full by late 2022) [95] [96]. We'll define its treatment date as Q4 2022 for analysis; using an event-

study helps reveal any phased-in effect. If parallel pre-trends look violated (e.g. if Ashdod saw decline before its competitor arrived due to anticipation or other issues), we may include port-specific trends or use synthetic control (Idea U1.3). Data collection is straightforward since we have the yearbook.

Upgrade with L Data: If monthly labor hours per port become available (even later), we can directly compute **labor productivity = output per hour** and re-run the event study on that outcome. The design easily accommodates it, essentially merging the two-step approach (reform → output, reform → hours) into one (reform → output/hour). Additionally, with L we can check whether output gains came partly from increased hours (labor utilization) versus pure efficiency: e.g. did hours worked increase after competition or decrease? Currently, we assume improved **throughput + lower wait times** indicates better productivity, but L data would confirm if fewer hours achieved more output. This could help decompose whether reforms led to downsizing (fewer labor hours, same output) or higher output with stable hours.

U1.2 – Competition vs. Privatization: A Triple-Difference Analysis

Question: What is the added impact of **privatizing** a port when competition is already present? Specifically, in Israel's case, did Haifa Port's 2023 privatization yield productivity or investment gains beyond those from the 2021 new competitor – by comparing to Ashdod Port, which had a new competitor but remained state-run?

Identification Strategy: Difference-in-Differences-in-Differences (DDD), exploiting three layers: (i) Before vs. After 2023, (ii) Haifa (privatized) vs. Ashdod (not privatized) as the cross-section, and (iii) controlling for the prior competition treatment which both experienced. Essentially, we compare the change in outcomes post-Jan 2023 for Haifa relative to Ashdod, over and above any common changes and the earlier competition effects. The DDD formula: (Haifa_post2023 – Haifa_pre) – (Ashdod_post2023 – Ashdod_pre), where both ports had competition by then. We include fixed effects and prior event dummies for competition to isolate the pure privatization effect [14] [15]. This is like adding an interaction term for (Haifa × post-2023) in a DID already controlling for port-specific time trends. We will verify parallel trends in 2022 for Haifa vs. Ashdod after both got competition but before privatization – the assumption being, absent privatization they'd evolve similarly in 2023.

Outcomes: Key outcomes could include: (1) **Throughput share** – Did Haifa's volume jump or market share change in 2023 relative to Ashdod? (2) **Productivity metrics** – e.g. TEU per ship-hour at Haifa vs. Ashdod in 2023 (the Comptroller or IPC might have early data). (3) **Capital expenditure or capacity** – e.g. number of cranes, berth utilization. If data allow, we can use a proxy like news of crane purchases or port expansion in 2023. Indeed, Haifa's new owners were *obliged to invest ₪1B+* in modernization [80]; Ashdod, meanwhile, announced some investments but with less momentum [80] [78]. We expect Haifa's privatization to boost investment and perhaps efficiency (due to new management and technology) relative to Ashdod. This could appear as an acceleration in throughput or a faster improvement in KPI like turnaround time at Haifa post-sale.

Data: External & Provided: There is no single dataset for "investment by port," but we can compile qualitative and quantitative indicators. The student's **research dossier** cites that pre-privatization, both ports were gearing up capital spending (Ashdod bought cranes, Haifa awaited private buyer) [80] [97]. After privatization, sources like Calcalist (July 2022) provide stats: Haifa's sale prospectus had financials and implied labor productivity leaps (150 workers vs 1300) [98] [79]. We can use **labor productivity (TEU/worker)** in 2023 derived from those headcounts as a rough measure – it suggests Haifa's new operator

might achieve **~9x** productivity of the old (if those numbers hold). Ashdod's workforce was still ~1,300 with only partial attrition, so its productivity likely lagged. We will gather: monthly throughput 2022–2023 (from yearbook), any reported workforce changes, and KPI updates (the 2024 yearbook might show 2023 TEU/hour by port). For capital, we might include a dummy or count of new STS cranes operational by port-year (from news: Ashdod got some new cranes 2022, Haifa's new owners planned upgrades 2023).

Feasibility & Risks: The DDD outcome might be subtle over one year. If 2023 global conditions (e.g. post-COVID trade normalization) affected ports differently, that could confound results. But we assume those are similar across ports (both saw some volume drop in 2023 due to world trade dip). Another risk: Ashdod's anticipated partial privatization via IPO was shelved in 2023 for political reasons [99] [100], but regulators did make small changes (e.g. reduced port tariffs) that might affect Ashdod in late 2023. We'll track such policy to ensure our difference is truly privatization. Data on a short horizon (2022–24) is limited; statistical power is low. The approach will be more of a **level difference** check: Haifa vs Ashdod divergence in 2023. If needed, we can extend into 2024 as data become available, or use higher-frequency (quarterly TEUs at new vs legacy terminals) for more observations.

Upgrade with L Data: Having actual labor-hour data per port would hugely improve this analysis: we could measure **labor productivity (output per hour) at Haifa vs Ashdod** around the privatization. If Haifa's productivity jumped relative to Ashdod's after Jan 2023, that's direct evidence of privatization effect. Also, we could examine **employment or hours**: Did Haifa's labor input drop faster than Ashdod's after sale (indicating efficiency gains through workforce reduction)? Currently, we rely on anecdotal headcounts; L data would confirm how quickly the new owner optimized labor. In essence, L data would let us separate whether privatization's benefit came from (a) reducing labor hours for same output (thus raising output/hour) or (b) inducing more output (ships handled) with similar labor – or both. That decomposition is valuable for mechanism.

U1.3 – Synthetic Control: Israel's Port Sector without Reform

Question: How would have Israel's port performance evolved **without the 2021–2023 reforms**? Can we estimate a counterfactual trajectory for key performance metrics in the absence of new terminals and privatization?

Identification Strategy: Synthetic Control Method (Abadie et al.) – We construct a weighted combination of other countries' port throughput or efficiency trends to serve as a "synthetic Israel" that did not implement similar reforms during 2021–2023. By comparing actual Israel to this synthetic control after 2021, we infer the reform's causal impact. This approach is single-unit focused, well-suited since we have one treated unit (Israel's port sector) and a donor pool of similar port sectors. We'll define an aggregate outcome for Israel's ports (e.g. total container throughput nation-wide, or average waiting time), then find a synthetic that matches Israel's pre-2018 trends in that outcome. Donor candidates: possibly other small open economies' ports (e.g. Belgium, Greece, New Zealand) that did not undergo major structural changes in that exact period. Alternatively, we could use a **placebo within-country** synthetic: e.g. treat *Ashdod's legacy port* as "untreated" and use Haifa + others to synthesize it – but that's less standard. The primary plan is cross-country. We ensure no donor had a port overhaul in 2021–23; many did reforms earlier (90s/00s), but those effects stabilized by the 2010s.

Outcomes: (1) **Total container throughput (TEUs) relative to GDP** – to control for economic size. If reforms significantly boosted port efficiency, Israel might handle more TEUs per unit of economic activity

than it otherwise would (by attracting transshipment, etc.). (2) **Average ship turnaround time** – we could compare Israel's trend in ship waiting/turnaround (from IPC reports) to a synthetic built from countries with stable port policies. (3) **Port Productivity Index** – if data allow, construct something like tons handled per port employee (for those countries where data exist, albeit that reintroduces labor – but many port authorities publish headcount). Since labor data is tricky, we likely stick to throughput or service quality metrics.

Data: External Data: UNCTAD's annual **Review of Maritime Transport** and data center provide country-level port throughput (TEUs) figures. World Bank's **Logistics Performance Index (LPI)** (2018, 2022) has a quality score for port services by country, which could be used as a rough measure (though infrequent). Also, academic datasets: e.g. Notteboom (various papers) list throughput of major ports; OECD's International Transport Forum might have port performance stats. We will compile 2008–2020 data for, say, 10–15 countries (including Israel) for pre-period fitting. Israel's pre-trend: throughput was stagnating or growing slowly (Ashdod/Haifa often congested). The donor set could include regional peers (Mediterranean ports) and advanced small economies (to capture similar trade patterns). The **synthetic control algorithm** will output weights – e.g. maybe it picks Greece (no major reform recently, but affected by trade) and a bit of Portugal, etc., to mimic Israel's pre-2021 performance. Then we see post-2021 divergence: e.g. if Israel's throughput in 2022–23 exceeds synthetic by X%, that suggests reform effect (accounting for global conditions via donors).

Feasibility & Risks: One challenge: COVID in 2020 caused wild swings in port volumes globally, making pre-trend matching weird. We might start matching from 2012–2019 to avoid COVID distortion, then allow 2020–21 as part of treatment window (though new port opened late 2021). Another risk: Few countries are directly comparable to Israel (unique geography, security issues). The synthetic might fit poorly or choose countries that had unrelated shocks. We will check placebo tests: apply the method to donors and see if any fake "reform" effect shows up spuriously. Data availability is decent for throughput; for wait time or other efficiency, cross-country data is spottier (perhaps in World Bank port indicators, if any). We might proxy efficiency by using the Liner Shipping Connectivity Index (LSCI), which reflects how well-connected a country's ports are (often improving with competition/capacity). Israel's LSCI jumped in 2022 (due to Bayport handling big ships). We could synthesize LSCI as outcome.

Upgrade with L Data: If labor data becomes available for multiple countries (perhaps via ILO or national sources on port workforce), we could refine the synthetic to directly compare **labor productivity** (e.g. TEU per port employee) across countries. That would be ideal to estimate the counterfactual "Israel without reform" in terms of labor productivity. Alternatively, without broad labor data, if we stick to throughput or wait times, adding Israel's own L later allows another check: for example, synthetic control suggests a +15% throughput effect by 2023; if we then see labor hours only rose 0–5%, we'd infer a large productivity gain. In essence, L data could help confirm whether the output gains identified by synthetic control were achieved via productivity (likely yes) or just scaling up labor.

U1.4 – Global Panel Event Study of Port Reforms

Question: Do port competition/privatization reforms improve productivity across countries on average? How does Israel's reform impact compare to international norms?

Identification Strategy: We assemble a **panel of country-port data** and use a **Callaway & Sant'Anna (2021)** style staggered DiD, where treatment is a port reform implemented in a certain year in each country.

Each country can contribute a "reform event" (or none if no reform), and we estimate average dynamic effects. For instance, many Latin American countries restructured ports in the 1990s; some Asian countries in 2000s; Israel in 2020s, etc. We code an indicator for major port reform onset by country-year (from World Bank or OECD reports). Then we run a panel regression with country and year fixed effects, treating countries that haven't reformed yet as controls for those that just did, excluding already-treated observations as per C&S to avoid contamination. This yields an average effect trajectory (e.g. +X% efficiency by 1 year after, +Y% by 5 years). It's essentially a differences design applied globally.

Outcomes: Likely **port productivity or efficiency index** at country level. We could use **gross throughput per port worker** (if available for enough countries at some frequency). Another could be **port TFP** from studies: e.g. a recent paper by Cano-Leiva et al. (2023) computed technical efficiency scores for ports with/without private terminal management [84] [101] – we might use their reported efficiencies if accessible. If not, a simpler metric is **throughput per capita** or per GDP, under assumption that more efficient ports handle more trade relative to the economy. Or **waiting time / ship dwell time** if international data (UNCTAD sometimes surveys average port stay durations). The choice will depend on data coverage; throughput and possibly LSCI or LPI port component might be broadest.

Data: External multi-country data: UNCTAD's dataset includes country TEU throughput and sometimes number of ships and maybe port employment (less likely). Alternatively, compile port-level data for large ports from e.g. Lloyd's or port authorities, then aggregate. For labor, the ILO has some employment series by sector (maybe "water transport" which includes ports). For identifying reforms, sources like Trujillo & Serebrisky (2003) list dates of port reforms in Latin America; Notteboom & Rodrigue (2012) list some globally; Brooks (2009) provides timeline for many countries shifting to landlord models [102]. We will create a table: e.g. Argentina reform=1994, Brazil=1993, India=2000 (gradual), etc., Israel=2021–23 (we might treat 2021 as start). Countries with multiple stages (like Israel) can be coded with the first big competitive entry. The panel spans ~1980–2025, annually.

Feasibility & Risks: This is ambitious for undergrad and data cleaning heavy. Also, heterogeneity: not all reforms are equal (some were just corporatization, some full privatization). We'll define "treatment" as introduction of private competition (the strongest form). Results give an average effect; Israel might differ. But interestingly, we can see if Israel's effect size is in line or bigger (perhaps bigger because they did both competition and privatization optimally sequenced). Potential problem: serial correlation and staggered timing – hence using modern DiD methods is important to avoid bias from comparing to already treated units. If data proves too sparse (esp. labor productivity measure), we may simplify to an illustrative analysis of throughput growth rates pre/post. But ideally, we get something like: on average, countries increased port throughput per capita by e.g. 10% within 5 years of reform, relative to synthetic control. If our estimate for Israel from Idea U1.3 was, say, +15%, that's plausible and slightly above average (maybe due to bigger inefficiency to start).

Upgrade with L Data: The value here would be limited unless we gather int'l labor data. But one upgrade: if Israel's microdata arrives, we can insert Israel's specific estimates into this global context. For example, the panel might show that globally, port labor productivity (if measured) rose by X% after reform – we could then confirm if Israel's measured increase aligns or diverges. Also, if L data allows a better metric like **value-added per worker** in port services (which one could calculate if we had wage bill or similar), we could use that in the panel for at least OECD countries (some national accounts might have "gross value added of port operations" and employment). Realistically, the global panel can proceed without Israel's micro L, but adding Israel's own data later can enrich the comparative narrative.

U1.5 – Efficiency Frontier Analysis (DEA) of Israeli Ports Pre- vs Post-Reform

Question: Did Israel's ports move closer to the global efficiency frontier after the reforms? In other words, how did their operational efficiency ranking change, and how much of that was due to technical change (new capital) vs. efficiency catch-up?

Identification Strategy: Data Envelopment Analysis (DEA) & Malmquist Productivity Index. We treat each major port (or port-terminal) as a decision-making unit. We will compile input and output data for a sample of ports (including Haifa, Ashdod, and say 20–30 benchmark ports worldwide of similar size) for two periods: pre-reform (e.g. 2018–2020 average) and post-reform (2022–2023). Using DEA, we find the efficient frontier in each period – basically the maximum output attainable for given inputs. We then see how Haifa and Ashdod's efficiency scores change. The Malmquist index decomposition gives: **Efficiency change (catch-up)** and **Technical change (frontier shift)** for each port [61] [103]. For Israeli ports, a large positive efficiency change would mean they caught up to best practices (likely due to improved management/ competition), whereas technical change could reflect the impact of new capital (if the whole frontier shifts or if Israel's own position improved by acquiring cutting-edge tech). No formal control group is needed since DEA inherently benchmarks against peers. While not "causal" in the strict sense, a big improvement from pre to post likely indicates reform impact, given no other major changes.

Outcomes/Measures: DEA efficiency score (0–1) for each port in each period; **Malmquist index components** for Haifa and Ashdod from pre to post. We may compute separate DEA for two outputs: (a) **Throughput (TEUs)**, (b) **Quality (e.g. ship turnaround speed)** if we incorporate that as an output. Inputs: (i) **Labor** – number of port employees (we can use 2018 data from State Comptroller: Haifa ~1000, Ashdod ~1300 then; for others, use AAPA or port authority reports). (ii) **Capital** – we'll use proxies: e.g. number of ship-to-shore gantry cranes, berth length (meters), yard area, and perhaps whether port has automation. Many of these for Israeli ports are documented (the dossier notes new 800m berths, deep 17m draft, STS cranes etc.) [104] [105]. We will gather comparable stats for peer ports (sources: World Bank Port Infrastructure data, academic papers, or port websites). If multiple inputs are used, we may run DEA in two stages (or use an index like EBM, but likely basic DEA CCR model with inputs aggregated).

Data: Provided and External: The student dossier and proposal list Israel's port infrastructure additions (new cranes, etc.). For peers: many ports publish annual reports listing employees, cranes, throughput. Academic sources like Cullinane et al. (2006) have tables of inputs for sample ports [46] [106]. We can include Mediterranean ports (Piraeus, Valencia, etc.) and some Asia or US ports for global frontier. This is moderate data work but feasible.

Feasibility & Risks: DEA can be sensitive to data accuracy. We must ensure comparing like with like (e.g. exclude pure transshipment hubs if too different, or run separate DEA by port type). Israeli ports pre-reform might be outliers (inefficient); post-reform might still not reach frontier if best practices are far (like Singapore etc. are way bigger). But focusing on change is fine. Malmquist results could be limited by just two time points; ideally use annual 2018–2023 to get a trend, but we have input changes mainly at reform points. Also, this approach doesn't "prove" causality, but shows consistency with improvements. It complements the econometric DiDs by illustrating the magnitude of efficiency gain in industry terms (e.g. "Haifa's efficiency score went from 0.6 to 0.9 after reform, moving from bottom quartile to top 5 globally"). That narrative is powerful for policy. Risk: lack of detailed labor data for peers (we might use approximations or focus on capital-only DEA as a robustness, though that measures capital productivity, not labor).

Upgrade with L Data: Having precise **labor hours** for Haifa and Ashdod would refine their input data (instead of using headcount which ignores overtime or idle time differences). If we could get labor hours for peers (some port studies have average working hours or shifts), even better. But even just for Israel, using hours rather than headcount would likely show an even bigger improvement post-reform (since not only did output rise, but labor hours likely dropped due to workforce reduction and efficiency). For instance, if Haifa port's labor hours fell after privatization (with voluntary retirements, etc.), the efficiency score gain would be understated by using headcount. With hours, we'd more accurately place it on the frontier. So once available, we'd recompute Israel's post-reform position with actual hours to validate the productivity jump. This could strengthen claims that "per labor-hour, Haifa's output nearly doubled" etc., aligning with the idea that labor practices improved drastically.

U1.6 – RDiT Around New Terminal Openings

Question: Was there an immediate, discrete jump in performance at the moment a new private terminal began operations? In other words, can we detect a *sharp break* in Haifa's or Ashdod's metrics right at the entry of competition (Sept 2021 for Haifa, Nov 2022 for Ashdod)?

Identification Strategy: Regression Discontinuity in Time (RDiT) centered on the reform go-live dates. We examine a narrow window (e.g. 12 months before vs. 12 months after each entry) and fit a regression of the outcome on time (in months relative to the break) with a possible discontinuity at the break. Formally: Outcome = α + β * Post + f(time) + ε, where Post=1 for months after the opening, and f(time) is a polynomial or piecewise linear trend on each side. If the reform caused an abrupt improvement, we expect a significant positive β (jump at discontinuity). We do this separately for Haifa's competitor (Sep 2021) and Ashdod's (late 2022). Since these happened at different times, we can treat them independently in RDiTs, or potentially pool by normalizing time (but pooling is tricky because each port's level differs). The RDiT provides a visual and statistical test for an **instant change** as opposed to gradual trend change. It's essentially testing if, say, September 2021 was a structural break.

Outcomes: The most high-frequency outcomes available: likely **monthly average ship wait time** at Haifa/Ashdod (if the port authority collected it – they had yearly, maybe internal monthly), or **monthly throughput per ship call** (which could reflect efficiency per call). Another is **market share**: e.g. Haifa's share of total throughput may jump once Bayport opens (if Bayport mainly takes new volume, maybe less so immediately). Alternatively, **vessel size**: with Bayport, Haifa started handling mega-ships; we could see an immediate increase in average vessel size or TEU per call after Sept 2021. Data: the IPC likely has monthly ship call numbers and TEU, which we can combine to TEU/call. That could be proxy for productivity per call (if labor per call didn't jump proportionally). We look for a discontinuous rise in TEU/call at Bayport opening.

Data: Provided (dossier) & external: The research dossier notes the exact date Bayport opened (1 Sep 2021, with a COSCO mega-ship arrival) [95] [96] and that Southport ramped up by late 2022 [95] [96]. We will try to get monthly series – possibly from the **Israel Ports Company monthly reports** (if any are public; if not, maybe through contacts). Alternatively, the **MarineTraffic** API or AIS data could give average vessel sizes calling each port by month, to infer changes. Also, news reported e.g. "in Nov 2022, MSC Oscar (largest ship) called Ashdod" [96] – indicating a before/after difference qualitatively. For wait times, anecdotal: pre-Bayport, ships queued; post, likely immediate relief. We might derive wait time indirectly by comparing monthly capacity vs. throughput.

Feasibility & Risks: RDiT assumes no other change at the cutoff and that the system was near steady-state before. COVID and global shipping snarls peaked mid-2021; by Sep 2021, conditions were abnormal (lots of backlog worldwide). The opening of Bayport happened amidst that – possibly the effect of Bayport was partly to ease a COVID-related backlog, which complicates attributing a clean jump. We can mitigate by examining *Ashdod* as a placebo for the Haifa cutoff (should be no jump at Sep 2021 in Ashdod if it's reform-specific). Similarly, check Haifa around Nov 2022 as placebo for Ashdod's event. This placebo test strengthens causal interpretation if only the treated port shows a jump at its date. Another issue: Southport's opening wasn't a single day full capacity – partial ops started months earlier. That may blur the discontinuity. We might instead RDiT on Haifa's privatization date (Jan 2023) for a quick shock (less likely to show immediate effect, but maybe some efficiency changes day 1). In analysis, a clear jump might or might not emerge; even if not, a null could imply the reform effect took a few months to manifest, which aligns with more gradual adaptation.

Upgrade with L Data: If we had, say, daily or weekly labor hours or productivity metrics around the date, we could sharpen the RDiT. For example, if the port authority tracked "TEU per gang-hour" weekly, a sudden rise after Bayport might be visible. With L data, one could also examine if labor deployment changed abruptly: e.g. did Haifa Port Company reduce shifts right when Bayport opened (since workload shifted)? That might show as a drop in hours (i.e., a discontinuity in L). Pairing that with output discontinuity could give a fuller picture: maybe output didn't jump (it redistributed), but labor hours fell (less overtime due to lower congestion), thus productivity per hour actually jumped – which one wouldn't see in throughput alone. Without L, RDiT on output might miss that. So L data could reveal a productivity jump even if output was flat (for Haifa legacy port, potentially).

U1.7 – Impact of Port Reform on Trade Costs and Import Prices

Question: Did the port reforms reduce import/export costs for the Israeli economy? For instance, can we see a decline in import freight rates, insurance costs, or consumer prices of imported goods attributable to more efficient ports?

Identification Strategy: Triple-Difference (Time × Country × Product). We exploit that Israel's reform should primarily affect goods that heavily depend on seaports (vs. those arriving by air or produced domestically). Also, we can compare Israel's trends to other countries without port changes. Concretely: use data on **import CIF-FOB spreads** or **freight costs as % of goods value** for Israel vs. others, pre vs. post 2021, and by product category (where heavy/bulky goods like metals, machinery might benefit more from port efficiency than light goods or airfreight goods like diamonds). A triple diff could be: (Israel vs. OECD peer) × (Post-reform vs. Pre) × (Seaport-intensive goods vs. others). Alternatively, use **World Bank's Doing Business "Trading Across Borders" indicators**, which measure time and cost to import a container – these might show a drop for Israel after 2022. We can DiD Israel's indicator vs. other countries' over 2018–2024. Another approach: look at **Israeli import price indices** for goods where port charges are significant (e.g. cars, appliances) vs. those with minimal port costs (digital goods, services) pre/post. If port throughput improved, ships spend less time waiting, maybe shipping lines lowered congestion surcharges. Indeed, before reform Israeli importers often paid demurrage due to delays; after reform, such fees might shrink. We'd need to find evidence of this in cost data.

Outcomes: (1) **Import freight cost percentage** – difference between CIF (cost+insurance+freight) and FOB (free on board) import values, from IMF or OECD statistics, which indicates transport cost. (2) **Container shipping rates to Israel** – perhaps obtained from shipping companies or indices (e.g. Freightos index

might have a Tel Aviv route). (3) **Import price index of consumer goods** – certain imported goods' prices might fall relative to trend if port costs drop. (4) **Logistics Performance Index (customs dimension)** – not likely to move in two years, but possibly an improvement in LPI sub-score for timeliness. Primary expectation: a modest reduction in transport cost or time. For example, Reuters in 2021 cited the government's aim that competition would cut import costs and consumer prices [16].

Data: External: The IMF Direction of Trade Statistics and OECD trade databases sometimes have CIF vs. FOB import value (Israel's Central Bureau of Statistics might too). We can calculate freight cost ratio. Doing Business reports (2019, 2020) give time/cost to import a standard container to Israel vs. others – note: Israel's 2020 figure was high due to port delays. In DB2022 (if it existed) or other sources, we'd see improvement. For import price indices: Israel's CPI has categories for tradable goods; if port reform lowered costs, one might see cheaper imported durables relative to non-tradables. However, many factors influence CPI (exchange rates, etc.), so a direct attribution is difficult. Perhaps better: track container lines surcharges – e.g. Maersk had a congestion surcharge for Israel pre-2021; did they remove or reduce it after Bayport opened? We could use shipping bulletins or ask industry sources.

Feasibility & Risks: Effects on trade costs might be small and hard to discern amid volatility (especially with COVID/fuel price swings). Also, concurrent factors: in 2022, global freight rates were extremely high (pandemic supply chain issues) and then fell in 2023 – one could wrongly attribute that to local reform. That's why a diff-in-diff vs. other countries is needed: if freight costs fell globally but *fell relatively more for Israel*, that's telling. We'll include at least one control country (maybe Mediterranean neighbor like Greece or a synthetic from others to control global trend). Data on CIF-FOB gap for Israel might not be readily public; if not, we may rely on anecdotal evidence (like forwarder reports of lower port storage fees). Another risk: time frame may be too short (cost changes might take time to pass through to consumer prices). Still, any immediate metric like port charges per container (IPC might publish tariffs – if Bayport undercut prices, average handling fee per TEU might drop ~15%). That could be shown as outcome too.

Upgrade with L Data: Not directly applicable, since this is more about costs external to port labor. However, if L data were available, it could inform a **mechanism**: e.g. if labor productivity improved, presumably unit labor cost in port operations fell, enabling lower fees. One could model how much of a drop in handling cost per container is explained by lower labor hours per container. But that's more of an accounting exercise. If the thesis extended to welfare analysis, having L would allow calculating if labor cost savings were passed on (if not all, maybe profits increased). But that's beyond scope.

U1.8 – Local Economic Spillovers of Port Improvements

Question: Did the port reforms have spillover effects on the local economies of Haifa and Ashdod? For instance, did manufacturing or logistics firms in Haifa region experience growth relative to those in Ashdod or elsewhere, due to improved port efficiency or privatization?

Identification Strategy: Difference-in-Differences at regional industry level. Compare outcomes for businesses in sectors that rely on port logistics (e.g. exporters, import-intensive industries) in Haifa metro vs. other regions, before vs. after the Haifa Bayport opening (2021) and privatization (2023). Haifa got both reforms, Ashdod only competition, other regions none – we can set up a triple diff: (Haifa vs. others) × (post-2021) × (port-dependent industries vs. not). Alternatively, simply compare Haifa vs. Ashdod in port-related sectors after each phase. The idea: a more efficient Haifa Port might reduce shipping time/cost for nearby firms, boosting their output or employment. Conversely, Ashdod firms might not see as much

benefit until Southport came fully. Data permitting, use firm-level or industry-level panel (e.g. manufacturing output by district). Another approach: use **nighttime lights** as a proxy for economic activity in port cities vs. comparable cities. If Haifa's lights (especially around port industrial zone) brightened relatively after 2021, that suggests increased activity. We'll use a DiD: port city vs. inland cities pre/post reform.

Outcomes: (1) **Industrial output or exports by region** – e.g. value of manufacturing shipments in Haifa district vs. central district (Tel Aviv, which uses Ashdod port more) over time. (2) **Employment in logistics/ warehousing** – these might grow near Haifa if port throughput expanded. (3) **Nighttime light intensity** – high-frequency satellite data (monthly) could show a trend break in brightness for Haifa port area after new terminal (due to round-the-clock operations with big ships, etc.). (4) **Property values or investment** – maybe Haifa port privatization led to planned waterfront development (Adani talked of investing in logistics parks). But that's longer term. Short term, we can check if new distribution centers opened around Haifa vs. Ashdod.

Data: External: The Israel CBS publishes some regional economic data (GDP by metro annually, employment by city). Customs data may allow breaking exports by exit port (if a firm in north tends to use Haifa). If accessible, we could gather firm-level exports and see if northern firms' export volumes grew relatively after 2021. Alternatively, use **DMSP/VIIRS night lights** (download from NASA; resolution ~0.5km, we'd aggregate around Haifa Port and Ashdod Port coordinates). Lights are a rough proxy but have been used in literature to detect local economic changes when official data are lacking.

Feasibility & Risks: Local spillovers might be subtle and possibly overshadowed by bigger macro trends (COVID recovery, etc.). Also, Haifa vs Ashdod vs others have inherent differences. We attempt to isolate by focusing on port-reliant industries (like chemical factories that import materials). If we can identify specific firms' outcomes (maybe via Orbis company data filtered by region), a micro-DiD is possible. But data collection might be heavy. Night lights approach is easier but interpretation can be noisy (maybe lights increased simply due to the port cranes working nights). However, that's actually an outcome of interest – more nighttime work equals more throughput. Risk: attributing any observed difference squarely to the reform is hard without more data points (only 2 port cities). We may treat this idea as exploratory/ descriptive.

Upgrade with L Data: Not essential here, since this is about external spillovers. If anything, knowing the ports' actual productivity gains (with L) bolsters the premise that local firms faced improved logistics. But no direct insertion of L into this analysis.

U1.9 – Labor Market Effects on Port Workers (Wages and Employment)

Question: Beyond productivity, what was the impact of port reforms on the port workforce? Did labor downsizing or wage changes occur as a result of competition and privatization, and can we quantify this in a causal way?

Identification Strategy: Difference-in-Differences (Labor Outcomes). We consider port workers as the treated group and use other similar workers as controls. For example, compare wage trends of port workers vs. other transportation sector workers before and after the reform. Israel's Ministry of Finance **Public Sector Wage Report** provides data on average salaries by government-owned company; e.g. it has statistics for Ashdod Port Co. and Haifa Port Co. employees [107] [97]. If after reform (especially after

privatization of Haifa), the wages or employment count changed significantly relative to the still-public Ashdod or relative to past trend, that suggests an effect. A triple diff can be: (Haifa port workers vs. all other SOE workers) × (post-privatization vs pre) × (maybe controlling for any overall wage freeze that year). We could also examine **employment count**: Haifa Port Company headcount reportedly dropped (some moved to Bayport, some retired); Ashdod's remained higher. Use Ashdod as control: DDD (Haifa vs Ashdod) × (post-2023) × (port worker vs admin perhaps). Another approach: use **Labor Force Survey microdata** to identify individuals working in "water transport" or "port services" vs. similar occupations (rail or airport workers) over 2018–2024, then do DiD on their employment rate or wage.

Outcomes: (1) **Headcount of employees** at Haifa Port Co. and Ashdod Port Co. – expecting Haifa's to drop sharply with privatization (some to new owner, some layoffs or reallocation), Ashdod's to maybe drop slightly after Southport (lost traffic) or remain until future changes. (2) **Average wages** – possibly, competition might curtail overtime pay or force wage moderation at the legacy ports (already high salaries ~double public avg [108] [109]). If data show a freeze or reduction in real wages at Ashdod once competition hit (due to less overtime), that's an effect. At Haifa, after sale, new owner might implement different pay scales (though likely kept similar union agreements initially). (3) **Pension or retirement rates** – if we had that, maybe many older workers took early retirement packages at privatization. But that data is not public; we might infer from headcount changes by age.

Data: Provided/External: The **State Comptroller 2020 report** likely gave baseline numbers: ~1,050 workers Haifa, ~1,400 Ashdod pre-reform [78] [79]. The MoF wage report (annual) lists average salary for each government firm's employees; 2023's report (if published by late 2024) will show Haifa Port Co for 2022 (prior to sale) and 2023 (likely only part-year public). If the sale removed Haifa from report (since now private), we have to glean from other sources. Perhaps we use up to 2022 for wage trend; or include Bayport's workforce (but they likely pay differently – maybe lower base but more efficient). Alternatively, **Histadrut (union) announcements** or news might state something like "no layoffs until 2027 per agreement" – which happened in 2005 reform. If so, then maybe wages didn't drop, employment did via attrition. We'll gather what's available. Another source: National Insurance Institute's labor stats by industry could show total employment in "ports and shipping" yearly.

Feasibility & Risks: Causally attributing changes is tricky: port labor was undergoing negotiated changes anyway (older workers retiring, hiring freezes) irrespective of reform. But the competitive pressure likely accelerated workforce efficiency. We use Ashdod vs Haifa to isolate privatization specifically – but note both faced competition, which itself could impact labor (less overtime due to fewer delays). So an alternative: measure effect of competition intro (2021) on port labor outcomes using Ashdod vs itself: e.g. compare Ashdod's overtime hours in 2020 vs 2022 (if data available from, say, port company reports). Data may be sparse. This idea may end up descriptive: e.g. "by 2024, direct port company employment fell ~20%, while volumes rose, implying labor productivity gain – clearly reform-driven." We might not get a tight statistical significance but can document it.

Upgrade with L Data: Here, L data (hours) is the missing piece we're pivoting from. If we eventually get monthly hours per port, then we can directly measure how labor input changed and when. For instance, an event study on labor hours might show an immediate drop at Haifa 2023 (privatization) and a slower decline at Ashdod. Combined with output, that gives actual labor productivity changes (the core of original thesis!). So definitely, integrating L would turn this into a full analysis of labor productivity and welfare of workers (did some lose jobs? likely yes, through attrition). Essentially, U1.9 becomes the labor side complement to U1.1's output side once L is available.

U1.10 – Port Reform and Efficiency Distribution: A Quantile Approach

Question: Did the port reforms benefit only the average ship/cargo, or did they also eliminate worst-case delays? How did the **distribution** of ship turnaround times or ship-level productivity change?

Identification Strategy: Quantile Difference-in-Differences. Using micro-level data on ship calls (if attainable: time of arrival, time of departure, cargo handled for each ship), we can examine effects at different quantiles of performance. For example, perhaps the top 10% fastest turnarounds weren't much faster (they were efficient even before), but the slowest 10% saw dramatic improvement (as congestion eased). We would compare the distribution of, say, hours per ship (from arrival to finish) before and after the reform. A quantile DiD would involve calculating, for each quantile (median, 90th percentile of turnaround), the difference pre vs. post for treated port vs. control (the control could be the other port or the same port pre-reform acting as baseline). Alternatively, we could use ship size as a grouping: e.g. examine turnaround for large ships vs. small (maybe large ones benefited more from new deep-water facilities). This is more of an exploratory analysis to capture heterogeneity of impact.

Outcomes: Ship turnaround time (hours from entry to exit) or **waiting time before berth**. Possibly **cargo handling rate per ship** (tons per hour for each call). We'd derive a distribution of these per month or year. Also, **queue length distribution** (max queue length per week, etc.). We suspect that prior to reform, there were occasional extreme delays (long queues causing some ships to wait days [72]). Post-reform, those extremes likely vanished (e.g. average went from 17 hours wait to near zero at times [73] [74]). Quantile analysis will highlight that the **tail improved** – important for reliability.

Data: Provided (some aggregates), external (micro): The annual KPI said average wait ~17h in early 2000s and ~3-6h by 2012 [73] [74]. But we want distribution. Possibly the port authority or MarineTraffic data could be scraped to get each ship's times. MarineTraffic provides timestamps for port calls (some data is free, or academic alliance). If not, we might approximate from news or port logs. Even if micro isn't available, we could use published 25th/75th percentiles if any. The **OECD 2018 peer review** or others might have stated something like "90% of ships waited under X hours after reform vs Y before." If not, this idea might be more hypothetical due to data constraints.

Feasibility & Risks: Getting detailed ship-level data might be beyond an undergrad's scope unless an API is used effectively. But maybe focus on a simpler slice: e.g. measure **variance** in monthly performance metrics pre vs post. A large drop in variance could imply fewer outliers (less occasional congestion spikes). The risk is spending effort on data extraction with little payoff if the pattern is obvious from means. However, if done, it bolsters that the reform not only improved average but also reliability – a key benefit claimed by policymakers (no more unpredictable delays).

Upgrade with L Data: Not directly needed for distribution of wait times. But if we had granular labor deployment data, one could examine if labor productivity improvements were uniform or mostly came from eliminating downtime. That's complex. Better to focus on the operational metrics where L isn't needed.

End of Umbrella 1 Catalog. Each idea above can be pursued independently or some in combination (e.g. U1.1 and U1.2 naturally go together to analyze competition vs. privatization). Collectively, they provide multiple angles on port reform impacts without immediate reliance on confidential labor-hour data. Next,

we move to Umbrella 2, expanding the inquiry to other non-tradable sector reforms critical to Israel's economy.

Umbrella 2: Reforms of Non-Tradable Sectors (Israel & Abroad)

Literature Review – Competition, Privatization, and Regulation in Non-Tradables

Israel's low productivity in non-tradable industries is mirrored in high consumer prices and reports of inefficiency [8] [50]. Economic theory and international evidence suggest that **market structure and regulation** in sectors shielded from global competition can significantly affect performance. Relevant literature strands include:

- **Competition and Prices:** Introducing competition in monopolistic markets typically yields lower prices and can improve productivity as firms streamline to stay competitive. A striking Israeli example is **mobile telecommunications**. Before 2012, Israel's cellular market was an oligopoly of 3 firms with high prices. Reforms around 2011–2012 (led by then-minister Moshe Kahlon) added new entrants and enabled Mobile Virtual Network Operators. Studies and reports note that within a few years, **cellphone bills dropped by ~70–90%** [5] [6] – a massive consumer welfare gain. However, as Reuters (2015) documented, the fierce price war eroded profits and led to a **20–40% drop in capital spending** by the incumbent telecom firms [110] [111]. Israel's 4G rollout lagged due to this, raising the question of an optimal competition level [112] [113]. This shows competition can have ambiguous effects on productivity: on one hand, it forces efficiency; on the other, if too intense, it may undercut investment in infrastructure, potentially harming long-run productivity (a phenomenon also debated in Europe's telecom sector). For thesis ideas, this suggests analyzing not just consumer price outcomes but also **investment and quality metrics** post-reform. The telecom case is well-documented (BoI 2013, OECD 2016) [114] [115] and provides a blueprint for quantifying such trade-offs.

- **Privatization and Performance:** Privatization of state-owned enterprises (SOEs) is often pursued to improve efficiency by instilling profit incentives and reducing political interference. Empirical surveys (Megginson & Netter 2001) find privatization tends to increase profitability and productivity in competitive sectors, but in utilities or natural monopolies, outcomes depend on regulatory quality. In Israel, many large non-tradable firms remained state-owned or oligopolistic until the 2000s. For example, **Bezeq (landline telecom)** was a government monopoly until gradually privatized by 2005; studies by Gronau (2006) [116] [117] noted that Israel's unique case of the regulator being also the owner complicated the liberalization process. Ultimately, liberalization led to improved services (e.g. Internet proliferation) but required complementary regulation to ensure competition (e.g. local loop unbundling). Another case: **El Al (airline)** – privatized in 2003, but as a national carrier it faced limits (security costs, union issues) and only after open skies did real price effects occur. So privatization alone, especially if the firm retains market power, may yield limited consumer benefit (prices might even rise if a private monopoly maximizes profit). The **OECD (2018) and IMF (2016)** have urged Israel

to continue privatizing or introducing competition in sectors like electricity, ports, postal services, but always paired with regulatory frameworks to prevent private monopolies from abusing power [8] [50]. For our ideas, this underscores examining cases where privatization was accompanied by (or lacked) competition.

- **Regulatory Reforms & Deregulation:** Removing excessive regulation (red tape, entry barriers, price controls) in services can spur productivity by allowing more entrants or innovation. **Brand & Regev (2015)** found that industries in Israel exposed to import competition or that underwent deregulation saw faster productivity growth [118] [119]. Notably, after the 2011 protest, Israel passed laws to ease parallel imports (so retailers could import without official distributors) and to break cartels in food and consumer goods. While academic evaluation is scant, initial outcomes included cheaper imported food brands and more supermarket competition. The "Food Law" (2014) and subsequent import tariff cuts (like for cheese and butter in late 2010s) are ripe for study. Internationally, there's literature on product market regulations: e.g. **Nicoletti & Scarpetta (2003)** used OECD regulation indices and found that lower entry barriers in services correlated with higher productivity growth, especially as those services feed into other sectors (e.g. cheaper telecom raises productivity elsewhere) [120] [121]. The IMF (2018) also projected that **product-market reform could boost Israel's GDP by ~6.9% over a decade** [122] [123]. Thus, evaluating specific Israeli deregulatory moves (telecom, aviation, retail import) can provide micro evidence of these macro projections.

- **Non-Tradables and Cost of Living:** Because these sectors are not exposed to import competition, inefficiencies translate directly into higher local prices. Hazan & Tsur (2021) note high labor costs in non-tradables inflate Israel's price level [124] [125]. The Government Competition Authority reports often mention monopolies (e.g. in cement, in banking fees) raising costs. **Claude Giorno (OECD 2016)** concluded that enhancing competition in Israeli markets is critical to raising productivity and reducing the cost of living [126] [127]. The 2011 Trajtenberg report explicitly targeted ports, food retail, banking, and housing as areas where breaking monopolies would yield consumer benefits [32] [34]. This is the policy impetus behind many reforms we consider. For example, the **Open Skies agreement with the EU (2013)**: Prior to that, Israel had restrictive bilateral quotas and El Al's high cost base kept fares up. Opening to low-cost carriers was expected to increase tourism and cut ticket prices by 20–30% [16]. Studies like Piermartini & Rousová (2013) predicted 5% higher passenger traffic globally if all adopted open skies [128] [128]. We'll see if data show such effects for Israel (e.g. a surge in flights to Europe, fare decreases).

- **Financial Sector Reforms:** Israel's banking sector has long been concentrated (two banks ~60% market share). Low competition can lead to high fees and interest spreads. The **Strum Committee (2016)** aimed to increase competition by separating credit card companies from banks (executed in 2018–2019) and easing entry for new banks (the first new Israeli bank in decades started in 2022). A BoI report in 2019 noted early signs of narrower interest spreads after the reform and growth in non-bank consumer credit [129] [130]. However, academic evaluation is pending. In theory, more competition in banking could improve efficiency (banks innovate, cut costs) but too much can risk stability (if it erodes margins too far). Our ideas might consider initial outcomes: e.g. did the cost of credit drop for consumers? Did bank profitability or efficiency ratios change? International evidence (e.g. from EU bank liberalization) shows competition tends to reduce fees, but results on productivity (cost-to-income ratio improvements) vary.

- **Utilities and Infrastructure:** The **electricity sector reform (2018)** breaks up Israel Electric Corp's monopoly by selling some generation to private IPPs and allowing competition in retail gradually [131] [132]. Studies (Pollitt, etc.) on electricity market liberalization generally find improved generation efficiency and lower prices, provided regulation ensures competition. Israel's reform is ongoing, but a cost-benefit study (EPRI 2018) warned that partial implementation could fail to yield benefits [133] [134]. An early outcome: by 2022, ~30% of power generation was by private producers, and retail competition started for large customers. We can attempt to measure if electricity prices for industry fell or if outages decreased (service quality improved). Another domain: **public transport** – Israel tendered many intercity bus routes to private operators since 2000s, injecting competition against the Egged/Dan duopoly. Researchers (e.g. Einstein 2018) found that tenders did cut costs per km for the government and increased service in peripheral areas. But concerns about driver wages and service quality exist. This suggests analyzing cost efficiency improvements and whether competition impacted labor conditions (which ties into productivity vs equity debates).

Gaps and Open Questions in Literature: While the need for competition is well-recognized, specific causal impacts of each Israeli reform are not fully studied. Did mobile competition hurt network quality long-term? Did open skies actually increase tourism receipts significantly? How much did banking reform lower loan interest for SMEs? There's also a gap in understanding **optimal policy design**: e.g. the telecom case raises the question of balancing competition with sufficient margins for reinvestment. Similarly, in ports we saw competition + privatization was considered ideal, is that pattern generalizable? Another cross-cutting theme is **political economy** – many reforms faced fierce opposition (port unions, airline workers strike [135], IEC worker opposition, etc.). So sometimes reforms are partial compromises, which yield smaller gains (Ashdod Port still public, IEC still dominant). That complicates evaluation – e.g. is slow progress in electricity reform why we don't see price drops yet? These nuances inform our idea designs: we will note if reforms were partial and account for that in expected outcomes.

The gap matrix below summarizes known outcomes vs. missing evidence for Israel's key non-tradable sector reforms, guiding our Umbrella 2 ideas.

Gap Matrix – Non-Tradables Reform Knowledge vs. Gaps

Known / Theoretical Expectations	Missing Evidence / Questions
Telecom (Mobile): 2012 entry of new carriers led to ~90% price drop, proving huge consumer surplus gain [5] [6]. Network investment fell and 4G rollout delayed [111] [112] – raising concern of long-run quality.	Has productivity improved in telecom? (e.g. cost per subscriber down?) No formal study quantifies the trade-off in Israel between price drop and network quality. Did usage or digital adoption increase thanks to cheap prices (a positive externality)? This needs analysis.
Aviation (Open Skies): Expected to lower fares ~5–20% and increase flights [16]. Israeli tourism did hit record highs by 2018, suggesting a boost.	How much of the airfare reduction can be causally attributed to Open Skies vs. other factors (oil prices, competition from Turkish hubs, etc.)? Did El Al's efficiency or productivity change (they had to cut costs to compete)? No causal study yet isolates Open Skies impact on prices/ passenger numbers.

Known / Theoretical Expectations	Missing Evidence / Questions
Banking:	
Concentration long led to high fees and interest spreads. The Strum reforms (2016–19) intended to spur competition; early signs show growth of non-bank credit and possibly narrower spreads (as reported by BoI).	No clear causal evidence yet: Did the separation of credit card companies **cause** better terms for consumers (lower interest on cards, reduced fees) or did it just shuffle market structure? Also, did incumbent banks improve efficiency in response (cut costs, innovate) or not yet? Gap: rigorous evaluation of banking reform outcomes on consumer costs and bank productivity.
Electricity: Israel Electric Corp was inefficient (overstaffed, high cost), reforms begun to introduce IPPs. Theory and other countries' data suggest generation competition lowers costs and improves service reliability.	Outcomes unmeasured: Have electricity prices dropped or slowed in increase due to new IPPs? Has IEC's productivity (e.g. employees per MWh) improved post-reform or in anticipation? No published causal analysis; plus reform not fully implemented (IEC still dominates), so effect might be small so far. Gap: need monitoring of efficiency gains or lack thereof.
Public Transport: Privatization of bus lines via competitive tendering saved costs in several cases and increased service volume for same budget (reports by MoT).	Did these cost savings come at expense of labor conditions (lower bus driver wages/productivity)? Also, has service quality or ridership changed significantly when a route is competitively tendered vs. monopoly? Detailed quantitative evaluation is scarce.
Retail & Imports: Tariff cuts and import reforms after 2011 aimed to reduce high food prices (e.g. dairy tariffs cut, parallel imports allowed). Some price drops observed (cottage cheese ~5% down after protests, per CPI).	Causal attribution lacking: e.g. did the tariff removal on hard cheese in 2015 directly result in cheaper cheese for consumers, and by how much? Are domestic producers forced to raise productivity due to import competition? We suspect yes (Brand & Regev noted productivity rose in tradable sectors that faced import competition [27]) but need to isolate specific reforms.
General Productivity Impact: Theoretical models and cross-country regressions say reducing entry barriers and monopoly power should raise TFP in affected industries and economy-wide (via input-output linkages).	Micro-level causal links in Israel are under-explored. Which specific reform gave the biggest productivity bang for buck? E.g. ports vs telecom vs others – an interesting comparative question. Also, distributional effects: do these productivity gains translate to wage growth or lower consumer prices primarily?

The Umbrella 2 ideas will address some of these gaps by focusing on tangible outcomes of specific reforms with careful identification. We draw on both provided material (e.g. Hazan & Tsur on productivity, Trajtenberg's recommendations) and external sources (case studies, OECD data) to design the studies.

Idea Catalog – Non-Tradable Sector Reforms (U2)

U2.1 – The 2012 Mobile Telecom Shake-Up: Prices, Consumer Surplus, and Network Investment

Question: How did the entry of new mobile operators and the resulting price war in 2012–2015 affect consumers and the telecom sector's productivity? Did lower prices lead to increased usage and welfare gains, and what were the impacts on network investment and quality?

Identification Strategy: Difference-in-Differences using International Comparisons. We compare Israel's mobile market outcomes before vs. after 2012 against other OECD countries that did not experience a similar policy shock during that period. For instance, using OECD data on mobile tariffs or revenue per user, we do a DiD: Israel vs. OECD-average (or vs. a synthetic control built from countries with stable market structure) over 2007–2018. The treatment is the Israeli reform around 2012 (gradual entry of Golan Telecom, HOT Mobile, MVNOs). We will analyze multiple outcomes: price, usage, investment. Also, within Israel, we can use a **before-after analysis** supplemented by telecom-specific context. The control group approach handles global trends like smartphone adoption that affected all countries. We expect a significant drop in price metrics in Israel relative to others post-2012. For investment, we examine if capital expenditure (CAPEX) in networks fell relative to a counterfactual. We may complement with a **synthetic control** focusing on, say, average revenue per user (ARPU): create a synthetic Israel from other countries' ARPU trends (which are correlated with price).

Outcomes: (1) **Mobile service price index** – Israel's CPI sub-index for "telephone services" (which dropped sharply ~2012–2013). We compare that to CPI telecom in other countries or overall CPI to isolate telecom-specific drop [136] [137]. (2) **ARPU (Average Revenue Per User)** – a measure that encapsulates price per user; OECD Communications Outlook has data in USD for each country. (3) **Consumer surplus change** – we can estimate this by combining price drop with increase in subscriptions or usage. For usage: (4) **Minutes or data use per subscriber** – did it jump as prices fell? Israeli mobile data usage did surge mid-2010s (if BoI reports have stats). (5) **Network investment** – annual CAPEX of Israeli mobile operators (from company reports or Ministry of Communications) as % of revenue, and compare to OECD trends. Also (6) **Network quality** – e.g. 4G rollout timing or average download speeds (Ookla has global speed rankings). If speeds in Israel lagged peers post-2012, that indicates an investment effect. We will focus on quantifying trade-offs: e.g. "prices fell ~80% [5] [6], usage doubled, but CAPEX fell 30% and Israel's 4G deployment was 2-3 years late [112] [113]."

Data: Provided Input & External: The provided content (Reuters, etc.) gives qualitative figures on price drops and investment decline [5] [111]. Quantitative data: The **OECD Digital Outlook 2015** and **OECD Broadband Database** have ARPU, usage, investment for members. Israel's CBS and MoC publish annual communications reports (e.g. BoI Annual Report 2013 discussed the reforms and noted penetration and price changes [114] [114]). We can extract: – Price: Israel telecom CPI 2005–2020 (from CBS). – ARPU: OECD's data for mobile ARPU (maybe in USD PPP). – CAPEX: use operators' financial reports (Cellcom, Partner, Pelephone, Golan) – BoI 2014 said cellular network investment fell by ~-X% by 2014 (we'll confirm exact figures). – Speeds: Akamai's State of Internet report 2012 vs 2016 might rank Israel's broadband speed (though mobile-specific speed maybe Ookla). – Subscriptions: number of SIMs or penetration (which actually rose above 100% as more people got multiple SIMs for data etc.). Possibly track that.

Feasibility & Risks: The effect on price is huge and obvious (so much that identification isn't the challenge; rather quantification is). We must be careful attributing cause: maybe some price drop would have happened with tech progress, but nothing like 90%. Using other OECD as control addresses that (their prices fell modestly or not at all in that window). One risk: some countries also had changes (e.g. France had Free Mobile entry in 2012, similar effect). We will exclude those or incorporate that in synthetic weighting. Another risk: measuring consumer surplus requires assumptions (demand elasticity). We might estimate elasticity from usage increase: if minutes doubled when price fell 80%, that implies elasticity ~ -0.3 (just an example), then consumer surplus gain can be approximated. We'll keep it simple, focus on direct metrics. Data on quality (like dropped calls, speed) might be harder to get quantitatively; we might rely on proxies like 4G coverage percent. If data is thin, we'll qualitatively note quality impact.

Upgrade with L Data: If we had detailed labor data in telecom (not likely needed here), one could see if telecom sector productivity (output per employee) changed. Possibly, after reform, operators laid off staff (they did cut thousands of jobs [138] [139]) and outsourced more. That could show up as improved labor productivity. Without focusing on that, we still discuss productivity in a broader sense (cost per user, etc.). If desired, adding official productivity data for telecom (e.g. value-added per worker from national accounts) could show an uptick or downturn; that might reflect the adjustment (some inefficiency removed but also less investment could mean less value-added). It's a bit beyond the main scope, but a possible extension to see if the sector got "leaner" in terms of operations – likely yes since workforce shrank with similar subscriber base.

U2.2 – Open Skies Policy: Effect on Airfares, Traffic, and Tourism

Question: Did the 2013 Israel-EU Open Skies agreement lead to lower flight prices and increased air passenger traffic to/from Israel? What was the impact on Israel's tourism sector and the national airline's market share?

Identification Strategy: Synthetic Control or Interrupted Time Series for airfare and traffic trends. We take Israel's data on average airfares or passengers and compare to a synthetic built from countries without a policy change in 2013. For airfares: could use **Price of flight from TLV to common destinations** (e.g. Tel Aviv–London route price index) – though that might be too specific. Alternatively, use **aviation connectivity indexes**: e.g. number of direct flight routes, seats available. The Open Skies was implemented gradually 2013–2018, but announcement was 2013. We can consider 2013 as a break for expectations (airlines started planning entry) and 2015 or so as when effects fully kick in. A synthetic control can use pre-2013 data on tourist arrivals or flight frequency, matching Israel with a combo of similar tourist destinations (maybe Greece, Portugal, etc.), then see if Israel outperforms post-2013. Also, a **difference-in-differences** with an appropriate control: maybe compare Israel to a control group of non-EU markets (like Israel vs. Turkey or vs. non-EU Middle East) over time. If Open Skies was effective, Israel should see a larger increase in EU flights and tourist inflows after 2013 than these controls. Another angle: **Route-level analysis**: treat each route (city-pair) as observation – those affected by Open Skies (EU–Israel routes) versus unaffected (non-EU routes like to US or domestic Eilat flights) in a before-after. Use a panel of route frequencies or prices. That would be a diff-in-diff within Israel's routes.

Outcomes: (1) **Average airfare price** – possibly measured via CPI component for "international flights" or data from agencies on ticket prices. The Israeli CPI did record cheaper flights after 2013. For example, anecdotal: flights to Europe became much cheaper with low-cost carriers. (2) **Passenger traffic** – number of passengers through Ben Gurion Airport annually. This hit records: ~12M in 2010s to 24M by 2019. But

global travel was also rising; we isolate the differential. (3) **Tourist arrivals** – from CBS, how many foreign tourists, and tourism revenue. (4) **Airline competition metrics**: number of airlines operating to TLV, Herfindahl index of market share on routes. E.g. El Al's share of passenger traffic fell (IIRC from ~40% to ~25% by late 2010s as Ryanair, EasyJet, Wizz entered). (5) Possibly **service frequency** – number of weekly flights on major routes (Tel Aviv-Paris, etc.). All these should improve (prices down, traffic up, competition up).

Data: External: The Israel Ministry of Tourism and IATA provide stats: tourist entries by air monthly, average hotel occupancy (as proxy if tourism rose). The CBS Statistical Abstract has tables on air transport (passengers by country, flights by carrier, etc.). Likely we have: – Annual passengers at TLV and Ramon (new Eilat intl airport) – these jumped after open skies. – Tel Aviv flight prices: maybe glean from travel agencies or a known price index (though not sure if publicly compiled). If needed, we might use anecdotal routes: e.g. track the advertised fare TLV–London in July each year (some reports might show a drop from $500 to $300 after easyJet came). – We can also look at **Eurostat data**: EU destinations to Israel – number of passengers, to see jump. – El Al's financial reports mention the increased competition and yield changes (yield = revenue per passenger-km likely fell). – Official CPI: The Israeli CPI has "overseas travel" component which partly reflects air travel cost; indeed, 2015-2018 had deflation in that category. – For synthetic control, potential donors: countries like New Zealand or something (no open skies change) to match Israel's pre-trend in tourist arrivals or flight cost index.

Feasibility & Risks: Open Skies effect might be somewhat confounded by geopolitical events (2014 Gaza war temporarily hit tourism; 2020 COVID collapsed it, but we'll focus up to 2019). Also fuel prices fell 2014 which lowered fares globally. We control by comparing to others. If using route-level diff-in-diff: one must ensure unaffected routes aren't indirectly affected (e.g. more capacity to Europe might shift capacity from US? Possibly minor). Also, one might consider that prior to 2013, Israeli airlines had labor cost issues limiting competitiveness – Open Skies forced them to restructure (El Al did cost-cuttings, like pilot wage cuts). That could have productivity implications at El Al. But likely beyond our data. The biggest risk is data on prices is not directly given. We might rely on CPI or anecdotal evidence rather than a rigorous price series. The volume metrics are easier to quantify: e.g. tourists from Europe increased by X% more than trend after 2013.

Upgrade with L Data: Not applicable unless we consider labor in airlines. If one had data from airlines, we might see if El Al's productivity (ASK per employee) increased after facing competition (they did retire some old fleet, etc.). But that goes deep into firm-level. Possibly out of scope.

U2.3 – Banking Sector Reform: Impact on Loan Rates and Bank Efficiency

Question: Did the post-2016 banking reforms (particularly the separation of credit card companies from banks, completed ~2018) lead to increased competition manifested in lower interest spreads or fees for consumers? Additionally, how did incumbent banks respond in terms of efficiency or service?

Identification Strategy: Interrupted Time Series / Structural Break Analysis using bank-level and market-level data. We will examine trends in **interest rate spreads** (difference between loan interest and deposit interest) and **bank fees** (like account fees, credit card fees) before and after 2017–2018. If competition increased, we expect these spreads/fees to narrow. We can strengthen inference by comparing to an untreated segment: e.g. **mortgages** were somewhat competitive already and less impacted by credit card spinoff, whereas **consumer credit** was targeted by the reform (credit cards, personal loans). Thus, a

diff-in-diff: interest rates on consumer loans vs. mortgage rates, pre vs post. If consumer loan rates dropped relative to mortgages (which banks still dominate) after reform, that suggests increased competition in that segment. Also compare Israel's trends to other countries: perhaps use IMF data on interest spreads internationally to ensure any narrowing is not just global low-rate environment. Another approach: **bank-level panel** – look at the big 2 banks vs. smaller banks or new entrants. If the reform benefited new entrants (the spun-off credit card companies became separate financing entities), maybe they gained market share or offered better rates. If we had data, diff-in-diff could be (large banks vs small) × (pre vs post) on metrics like net interest margin. However, new entrants only recently (the first new bank "Bank of Digital" opened 2022). We might focus on outcome rather than structure.

Outcomes: (1) **Interest spreads** – specifically the spread on consumer credit. Bank of Israel publishes average interest on various loan types. E.g., credit card loan interest average vs. Prime rate. We anticipate a decline after 2018 if competition (from new non-bank lenders and separated card companies) forced banks to lower rates. (2) **Bank fees** – average fees per account (BoI publishes an annual bank fees report, showing, e.g., reduction in checking account fees after transparency measures in mid-2010s). Strum aimed to reduce some account switching frictions too. So maybe measure number of people switching banks or average fees/year. (3) **Bank efficiency ratio** – cost-to-income ratio for large banks might improve if they had to cut costs (some anecdotal: banks closed branches and digitized more in late 2010s to stay competitive). BoI data has those ratios for each bank, can see if any structural break in trend. (4) **Market share of non-bank lenders** – e.g. volume of credit by non-bank institutions (like the now independent credit card firms, or fintechs). The reform intended to grow that from small base; if that grew significantly, it indicates success in fostering competition.

Data: External: Bank of Israel Annual Reports and **Financial Stability Reports** cover these metrics. The 2019 BoI report (after card separation) commented on initial outcomes: perhaps "consumer credit growth moved to non-banks" or "interest rates on overdraft fell slightly". We'll gather: – Time series of interest rate on overdraft/credit lines and on housing loans. – Fee survey results (BoI does publish average fees by bank each year). – Data on credit card loan yields pre/post separation (the card companies might have had to compete more once independent). – Bank performance data from their financials (like cost-income ratio). – Possibly household survey data on how many have multiple banking relationships now vs before (if any exists).

Feasibility & Risks: The reform's effect might be gradual and partially masked. Also, multiple reforms overlapped: alongside Strum, other steps like the "Credit Data Sharing" reform (2019) created a credit score bureau to help competition. We might lump these as one broad reform wave. Risk: macro factors (interest rates fell to near zero in 2018-2021) could narrow spreads independent of competition. We handle by comparing loan types or to other countries. Also the timeframe is short pre-COVID; and COVID in 2020 changed banking (lots of stimulus loans etc.). So we probably analyze up to 2019. Data likely show modest changes (Israeli bank spreads were high historically, maybe shaved a bit). If results are weak, that itself is a finding: perhaps reforms didn't yet strongly affect consumer costs (some critics say Strum didn't fully deliver). Also, no obvious "control country" for Israeli banking scenario – we might use OECD average bank net interest margin as control which was more stable.

Upgrade with L Data: Possibly irrelevant unless analyzing bank productivity (e.g. output per employee in banking). Actually, could do: banks did reduce headcount significantly in late 2010s (digitalization). That was partly a productivity push because of competition/profit pressures. If data available, one might correlate the

reform period with an acceleration in labor productivity in banks (cost saved per transaction). But that would be a stretch to causally link. We'll likely not include that unless data is easy.

U2.4 – Electricity Market Reform and Power Prices

Question: Has the partial liberalization of Israel's electricity sector (since 2018) led to changes in electricity prices or efficiency (e.g. generation costs, outages)? Or is it too early to tell? Essentially, we test whether introducing private generators and planned IEC restructuring affected consumer tariffs or system performance.

Identification Strategy: Before-after comparison with neighboring countries or historical trend, possibly using synthetic control. Electricity prices in Israel can be compared to a synthetic built from countries with similar fuel mixes but no reform around that time. Alternatively, compare industrial vs. residential tariffs: the reform initially allowed large industrial consumers to choose suppliers (some competition), whereas residential still mostly captive. So a diff-in-diff: industrial power price vs. residential, pre vs post-2018. If competition gave large users better deals from IPPs, we'd see industrial tariffs drop relative to residential. We also examine internal efficiency of IEC: e.g. thermal efficiency or reserve margin, to see if improved. Another angle is reliability: look at **SAIDI/SAIFI** (outage duration/frequency indices) pre vs post. If competition spurred improvements or investments that reduced outages, that's a benefit. Possibly compare to a control like water utility (no reform) for reliability changes (though not directly comparable). Given limited time since 2018, results may be modest; we'll quantify what's observable.

Outcomes: (1) **Electricity tariff (NIS per kWh)** – separately for residential and industrial. Israel's regulator sets tariffs; they slightly fell ~2018 (because gas fuel got cheaper) but then rose in 2022 (fuel costs). We want underlying trend net of fuel. If data available, use **cost components**: generation cost per kWh likely dropped as IPPs are more efficient combined-cycle plants. (2) **Market share of private generation** – from near 0 in 2010 to ~30% by 2020. That shows progress but not outcome per se; maybe tie it to price. (3) **IEC productivity** – e.g. employees per MWh or per customer. IEC had ~12,000 employees, slated to drop a bit after spinning off units. Check if labor productivity improved (the Comptroller might have data). (4) **Outage rate** – MoE reports might have minutes of outage per customer. If new plants eased strain, outages might reduce, but again many factors (weather) influence it.

Data: External: The **Israel Electricity Authority** publishes annual reports with tariffs and performance. Tariff breakdowns (generation, transmission costs). We can get average tariffs 2010-2022 (some in IEA or World Bank). For comparison, we might look at **OECD electricity price** series: synthetic control from those with similar energy inputs (e.g. gas-based systems) to predict Israel's price sans reform. Possibly use **Spain or Italy** that also did earlier reforms as part of synthetic? Not fully needed if we do within-country industrial vs residential. Industrial consumers could negotiate with IPPs from 2018; if we see a divergence in price trends, that's evidence. The **PUA (Electric Authority)** website may have data on competitive supply uptake by industry. Also, **IMF 2018 Israel Selected Issues** might project reform effects on prices (we can compare actual).

Feasibility & Risks: Price effects might be small so far because the reform is partial and fuel costs dominate. Also, residential tariffs are cross-subsidized by industrial historically; reform might start closing that gap (which could *raise* industrial or lower – unclear). If we don't find much, that itself is a finding (lack of effect so far). Also COVID changed usage patterns in 2020, complicating analysis; we might cut off by 2019. Another risk: attributing any price change directly to competition vs. fuel vs. regulatory decisions is

complex. We can control somewhat (e.g. examine generation cost separately). This idea may conclude that it's premature to see strong results, but it sets a baseline.

Upgrade with L Data: In this sector, relevant L data would be utility workforce etc. If we had that, we could confirm if IEC reduced headcount or improved productivity. Possibly incorporate that in a narrative (IEC had a plan to cut ~1,800 jobs by 2025 as part of reform; by 2020 had cut e.g. 800 – those are numbers to mention but not a formal analysis).

U2.5 – Bus Service Tendering: Cost Efficiency and Service Outcomes

Question: Did introducing competitive tendering for bus routes (privatizing segments of Egged/Dan operations to new operators in 2000s–2010s) reduce operating costs or improve service levels? Essentially, evaluate the efficiency gain from moving from monopoly to competitive contracts in public transport.

Identification Strategy: Difference-in-Differences across bus routes or regions. Use the staggered rollout of tenders: e.g. between 2000 and 2020, many areas shifted from Egged or Dan (cooperative monopolies) to private operators via tenders. We compare those routes that underwent tender to those still operated by Egged (which retained some lines, especially in certain cities until recently). We need data on cost per km or subsidy per passenger for each route or cluster. Possibly, Ministry of Transport reports (or academic studies) have before/after comparisons. Another approach: compare **fares or frequency** in tendered vs non-tendered areas, before vs after. If competition saved costs, government either saved subsidies or increased service (more frequency) for same budget. So outcomes might be: cost per bus-km, frequency of service, ridership. We treat tender introduction as treatment, with year and route fixed effects for DiD. If route-level data is too granular, do region-level: e.g. Jerusalem's buses tendered gradually (Kavim etc. vs Egged still had some lines); Tel Aviv had Dan partially, rest tendered. Or city clusters – might be tricky but feasible.

Outcomes: (1) **Operating cost per km** – If we find sources, perhaps Egged's cost vs. new operator cost on similar lines (Egged historically had higher wages and inefficiencies). A study by Ministry around 2015 said savings of ~35% in cost per km in tendered lines. (2) **Service quantity** – bus-km per capita in that area, or frequency on routes. Many tenders mandated increasing service. (3) **Ridership or satisfaction** – did ridership grow more in tendered areas due to better service? Alternatively, (4) **Bus driver wages/ conditions** – a potential downside: were cost savings partly from paying drivers less? Could examine average driver wage in Egged vs. private firms (Egged drivers are union, private sometimes not initially). Data maybe via union reports. (5) **Fare levels** – generally regulated, so likely constant, but competition could indirectly keep fare increases in check or allow more routes without raising fares.

Data: External: Government reports (e.g. Auditor reports or academic by Naor and Shnir or NTA) had data on costs. For instance, an OECD 2015 working paper on Israel's transport said competitive tendering cut costs significantly (we need specifics) [140] [141]. The Ministry's statistical yearbook might have ridership by operator. Possibly: ridership in the intercity bus sector grew from X to Y, partly due to extended coverage by private lines. We also can use budget data: Egged's government subsidy was NIS Z in 2000 vs. Z' per km in 2010 after some lines spun off. That shows efficiency. Another source: World Bank or ITF case study on Israel's bus reform. If data limited, we rely on reported figures.

Feasibility & Risks: The data might not be easily accessible publicly. We may rely on secondary sources that already evaluated parts of this (though likely not formal econometrics). If we can't do a rigorous DiD with

numbers, we can present a quasi-quantitative analysis: e.g. “Line Group 1 in Netanya after tender: cost per km fell 30%, frequency +20%. Meanwhile in Haifa (untendered at that time), cost remained high, frequency limited.” We must be cautious to attribute to competition rather than other improvements (like new buses etc. often came with new operators). But those investments are part of the tender outcome. Potential confounder: increased government subsidy to improve service could itself raise ridership, not just competition. However, competition allowed that subsidy to stretch further. We’ll frame it accordingly.

Upgrade with L Data: Not needed beyond possibly highlighting labor differences. If we had micro-data on drivers, we could quantify if productivity per driver increased (like drivers per km improved? Private companies often schedule more efficiently and use part-time drivers). That’s detail probably unnecessary for the thesis, though a labor perspective might mention driver shortage issues etc.

U2.6 – Import Tariff Cuts and Consumer Prices: The Case of Dairy Products

Question: Did reducing import barriers on specific non-tradable-dominated product markets (like dairy) result in lower consumer prices and pressure on domestic producers to become more efficient? We focus on the example of Israel’s dairy reforms (2011–2018) such as tariff reduction on hard cheeses and allowing imports of cottage cheese, to see if prices for those goods fell relative to others.

Identification Strategy: Triple-Difference using product-level price data. Essentially: (Treat vs Control products) × (Before vs After tariff cut) × (Israel vs other country) for extra robustness. Concretely, within Israel’s CPI, compare price indices of products with tariff cuts vs. those without, around the reform dates, and perhaps use OECD consumer price for those products as a control for global price trends. For example, Israel cut tariffs on some hard cheeses (like yellow cheese) in 2015; we compare the price index of “cheese” in Israel before/after 2015 to the price trend of other food items or to cheese prices in OECD average (which likely didn’t have a discrete change). Similarly, quotas on butter imports were loosened in 2017 – did Israeli butter prices converge to world price? We can do a smaller diff: cheese vs. fresh milk (milk remained highly protected with price controls) pre/post, within Israel. That isolates effect of liberalizing cheese relative to a near-identical category still controlled. Also, look at quantity: did imports of cheese surge, and did domestic output respond? If data from the dairy board exist, we can see domestic production trends. This reveals if domestic producers had to cut margins or improve productivity to compete.

Outcomes: (1) **Retail price index** for targeted product (e.g. “cheese and eggs” component in CPI, or if finer, price per kilo of a certain cheese). The CBS might have average prices for staples. The famous cottage cheese price protest of 2011 led to some price freeze; our method might incorporate that event: e.g. did cottage cheese price drop after 2011 protest (likely temporarily). Use that as an event as well, with yogurt as control, etc. (2) **Import volumes** of dairy – if reform worked, imports increased. (3) **Market share of imports vs local** – showing competition intensification. (4) **Profit margins of domestic firms** (Tnuva’s profit might have fallen, though data not public – but perhaps the State Comptroller or antitrust authority had figures). As a proxy, maybe stock price of publicly traded food companies when tariff cuts announced. (Though Tnuva was private and then Chinese-owned by 2015). Possibly skip that. (5) **Quality or variety** – anecdotal but maybe more cheese brands available after reform (consumer welfare beyond price).

Data: Provided & External: Hazan & Tsur mention the cottage cheese case and that protests led to increased competition and maybe slight productivity boost in those firms [27]. The Trajtenberg report recommended removing quotas; by 2015 some were removed. Data: – CPI by item: we can get annual % change for “milk, cheese and eggs” vs overall food. After 2015, dairy inflation was low or negative if I recall,

because of price agreements. – Trade data: import quantity of cheese 2010–2020 from CBS. – Possibly Bank of Israel reports on food prices (BoI 2019 might discuss food inflation slowed due to tariff reductions). – A counterfactual could be meat or other protected food which saw no liberalization: e.g. fresh milk remained price-controlled, so compare its stable price to cheese's decline. – Also, OECD data on cheese prices or "international cheese price index" to control for global dairy price (milk powder etc.). The period had some global volatility, but we adjust via other products.

Feasibility & Risks: Isolating the effect is challenging because government also imposed **price controls** on some dairy items when competition was slow. For instance, after butter shortages, they had to regulate price. So policy mix might muddy results. But showing any price drop in freed-up categories helps. Also consumption patterns changed (some substituting expensive domestic for cheaper imported cheese). We rely on macro indicators mostly, which might understate micro effects. Data at granular level could be limited (CPI might not have a separate index for "imported yellow cheese" vs. "domestic", for example). We may thus use aggregate category. But doing triple diff with other countries' cheese price could strengthen it: e.g. if in 2015 global dairy prices rose 10%, but Israel's cheese prices fell 5%, that's a strong sign reform overcame global trend by 15 percentage points in favor of consumers. Risk: short time windows and multiple overlapping changes. We'll carefully identify key reform timing for each product.

Upgrade with L Data: Not directly, except potentially noting if domestic dairy industry changed factor usage. For example, did dairy employment drop as firms automated to cut costs? If data from the Manufacturers' Association exist, could mention, but not necessary.

U2.7 – Anti-Concentration Reforms: Impact on Investment and Firm Dynamics

Question: Did the 2013–2017 "Anti-Concentration Law" – which forced large business conglomerates in Israel to delayer their pyramids and divest cross-holdings – have any measurable effect on corporate investment, new firm entry, or capital allocation efficiency? Essentially, testing if reducing concentration improved competition in markets or freed credit for other firms.

Identification Strategy: Firm-level panel analysis using difference-in-differences between firms affected by the law vs. those not affected. The law targeted the ~10 largest business groups (e.g. IDB, Delek, etc.), requiring them to reduce pyramid layers to 2 by 2017 and sell off either financial or real sector arms. We identify which publicly traded companies were under pyramids and thus had to change control vs. those that were independent. We then examine outcomes like their capital expenditure (CAPEX) growth, leverage, or profitability, relative to unaffected firms, pre (2010–2013) vs. post (2014–2018). The hypothesis: breaking pyramids might improve governance and efficiency in those firms (some evidence: after IDB split, subsidiaries performed better). Or it could have forced asset sales that allowed more competition (like if a conglomerate had to sell a subsidiary to a new entrant). On a market level, we could see if sectors that were dominated by a few groups saw increased entry post-law. For example, banking was dominated by bank-owned insurance, etc., which were separated (credit card separation is part of that trend). The law's effects are diffuse, but focusing on target vs. non-target firms is feasible.

Outcomes: (1) **Firm investment rate (CAPEX/Assets)** – expecting possibly higher if governance improved or if freed from group constraints. Or maybe lower if they lost internal capital support. (2) **Valuation (Tobin's q)** – studies of pyramid dismantling often check if firm value increased when removed from pyramid discounts. If data is there, check if the stock price or q of previously pyramid-affiliated firms rose relative to peers. (3) **Industry concentration indices** – e.g. HHI in banking, insurance, etc., before vs after

the reforms (the law also encouraged more competition in finance by limiting cross-ownership of banks and non-banks). A drop in concentration in some markets could be measured. (4) **New entries or IPOs** – maybe count new independent firms emerging, though attribution tough. Another interesting outcome: (5) **Credit allocation** – if previously capital was tied in pyramids, after breakup maybe mid-sized firms got more bank credit. The IMF (2019) suggested anti-concentration can improve small firm financing. We could see if share of credit to small businesses rose after 2017 relative to trend.

Data: External: Firm-level data from Tel Aviv Stock Exchange (TASE) or Orbis. Identifying conglomerate-affiliated firms vs not: possible from lists (e.g. the Competition Authority likely published which pyramids existed and their subsidiaries). IDB, Tshuva group, Ofer, etc. We mark those. Then gather their financials (maybe 20 firms vs. 20 controls matched by industry/size). The period is short and coincides with other things (2015-16 was low interest etc.). But maybe differences show by 2018. The Israeli SEC (ISA) might have had a report on compliance with the law. OECD Economic Survey 2020 might comment on any effect (maybe not yet). If data assembly is too heavy, we might rely on qualitative evidence: e.g. major pyramid IDB's collapse and breakup by 2015 led to more competitive telecom market (Cellcom and Partner used to both be under IDB influence to some degree; after, separate owners – maybe prices changed, etc.). Those are hard to quantify distinctly though.

Feasibility & Risks: This is a more advanced corporate finance question. Possibly too complex for an undergrad to do fully, but basic comparisons can be attempted. The risk is results not clearly attributable given all concurrent changes. Also, some group dissolutions were due to near-bankruptcy (IDB) not just compliance. That confounds effects (e.g. IDB firms performed better after because new owners turned them around, or just mean reversion). We'd use controls to mitigate that. The story might end up being: "the law achieved de-layering by 2017; short-term effects included some asset sales and entry (like new players bought assets), but measurable productivity or investment changes are not clearly evident within a couple years." That itself is insight.

Upgrade with L Data: Not applicable.

U2.8 – Ridesharing Entry and Taxi Productivity (International Case)

Question: (External case study) How does the introduction of ridesharing services (Uber/Lyft) in a city affect the productivity and service quality of the taxi industry? Since Israel has limited Uber (only partially in 2017, then banned), we use data from other cities as an analog for competition in a local service.

Identification Strategy: Difference-in-Differences using city-by-year data. Compare cities (or countries) where Uber entered vs. those where it was absent or entered later. Outcomes: taxi utilization rates, consumer wait times, and driver earnings. This idea is a proxy for competition in a highly regulated non-tradable (taxi service). For instance, use New York vs. Chicago: Uber entered both but at different intensities and timing; or use European cities where some banned Uber as quasi-controls. There are studies (e.g. Rogers 2015) showing Uber reduced taxi ridership significantly but improved overall consumer welfare via shorter waits. We can lean on their methodologies. The aim is to illustrate how tech-driven competition can force incumbents to adapt or lose productivity (e.g. many idle taxis).

Outcomes: (1) **Taxi trip volume** – often falls as Uber picks up trips (so incumbent productivity per driver might fall, negative for them but positive for consumer surplus). (2) **Average waiting time for a ride** – typically decreases for consumers due to rideshare supply. (3) **Fare prices** – sometimes fall if taxis lower

fares or if effective price goes down. (4) **Driver utilization** – fraction of time taxis have a fare vs. empty. Possibly declines for taxi drivers individually, but total rides increase. We could interpret increased competition pushes out inefficient usage.

Data: External: This idea would rely on existing research data. For example, papers using NYC taxi meter data pre/post Uber (there is open data on NYC taxi trips). Studies found taxi ridership dropped ~30% from 2013 to 2016 (Uber era). If we did an analysis, we could simply graph that or do a quick DiD with another city or using exogenous variation (like city regulations). But given the complexity, we might present findings from literature: e.g. "Uber's entry in SF cut taxi demand by X%, but total trips (Uber+taxi) soared, indicating latent demand was served; wait times fell by Y; taxi productivity measured as trips per taxi per day dropped, illustrating incumbent losses [120] [121] ."

Feasibility & Risks: Since the question context is Israel, including this external case is optional. However, it provides a modern angle on competition in a non-tradable service. If space/time limited, this might be trimmed. But it's easily understood by readers and parallels local reforms (both involve injecting competition via new entrants in a regulated market). Using secondary sources should suffice rather than fresh analysis.

Upgrade with L Data: N/A in Israeli context.

U2.9 – Higher Education Expansion (1990s) and Regional Productivity

Question: (Cross-cutting structural reform) Did the massive expansion of public colleges in the 1990s – which established new institutions across Israel – have an effect on regional productivity or wages by increasing local human capital? This addresses diffusion of skills (non-tradable education service reform).

Identification Strategy: Differences across cities over time. Some peripheral cities got new colleges in mid-90s (e.g. Sapir College in Negev, etc.), while others already had universities (Haifa, Tel Aviv) or still have none. We use an **event-study** around college openings: treat the city (or region) as "treated" with a bump in local supply of educated workers with a lag (graduates entering workforce). Outcomes could be average wages in that city (or growth in number of high-skilled jobs) compared to control cities without a college. The assumption: easier local access to higher education raises local human capital and potentially productivity in local firms (though grads could also move to center). Some might stay and improve local services or open businesses. This is similar to studies on land-grant colleges in the US improving county income. We control for initial city characteristics by diff-in-diff.

Outcomes: (1) **Average wage** or GDP per capita in the locality. (2) **Share of high-skilled employment** (college-educated workforce fraction). (3) **Number of firms or employment in skill-intensive sectors** (like tech, finance) in that area – expecting a rise if more grads available locally. Alternatively, (4) **Migration patterns** – does brain drain slow from those areas? Harder to measure.

Data: External: CBS has data by sub-district on wages and maybe education levels (Labor Force Survey). Identify ~5 treated towns (e.g. Ashkelon region with a new college vs. similar region without one). The 1990s expansion included colleges in e.g. the Galilee, Ariel, Ashkelon, etc. We could get stats circa 1995 vs 2005. Possibly glean from Taub Center or Bank of Israel research; they often study returns to education regionally. Also, Hazan & Tsur mention the expansion and that Israel's measured human capital rose, but actual skills didn't as much [142] [143] . So curious if the expansion improved outcomes or just produced degrees with less

effect. That's part of what we examine: maybe wages didn't rise as expected if quality was an issue, which Hazan & Tsur hint at (high education but skill not commensurate).

Feasibility & Risks: Many confounders – 1990s also saw massive immigration which affected regions differently (some colleges were partly to serve immigrants). We can't fully isolate that. Also, college location might correlate with general regional growth trends (maybe chosen in places already growing or where demand existed). So identification is not super strong. This might be more descriptive: show that regions with new colleges saw X% more wage growth than those without, albeit we can't claim strict causality. This idea is a bit tangential to competition, but fits umbrella's "non-tradable reforms" (education is non-tradable service, and expansion was a policy). It speaks to improving productivity via skill diffusion, a mechanism Hazan & Tsur flagged (but they found skill quality lacking). We may include it to ensure coverage of "diffusion barriers."

Upgrade with L Data: Not directly needed.

U2.10 – International Benchmark: OECD Product Market Regulation vs. Productivity

Question: Using cross-country data, how does Israel's improvement in product market regulation (PMR) indices relate to productivity growth relative to other OECD countries? This broadens the inquiry: checking if places where regulations eased saw faster TFP gains, situating Israel's reforms in an international context.

Identification Strategy: Panel regression or correlation analysis. We use OECD's PMR index (which scores regulation tightness in sectors like retail, network industries, etc.) at several points (2008, 2013, 2018) and see if reductions in PMR correlate with higher labor productivity or TFP growth over that period, across countries. We can do a differences regression: ΔTFP (2018–2008) = β * ΔPMR + controls. If competition reforms are beneficial, β should be negative (less regulation -> more productivity gain). This isn't strictly causal (could be reverse or confounded by broad reforms). But controlling initial level of productivity, etc., might give insight. Specifically, for Israel: OECD PMR shows Israel went from one of highest regulated to closer to average by 2018 (though still above average in some areas). Did its productivity gap close accordingly? So far, not much, indicating either reforms incomplete or lagged effect. This analysis can highlight that contrast – e.g. Israel improved PMR score significantly, yet productivity didn't catch up as much as predicted, possibly due to enforcement lags or other frictions (like skills).

Outcomes: Labor productivity growth (GDP per hour) or **TFP growth**. We can look at total economy or sector-level (maybe services sector productivity). (One could refine: correlate services PMR changes with services productivity growth specifically.)

Data: External: OECD PMR database (they have economy-wide and sectoral indices for 2008, 2013, 2018). OECD productivity database or Penn World Table for TFP. Israel's PMR dropped notably in retail, telecom, etc., after 2011 measures, but still had high barriers in professional services and some network sectors [9] [144]. We'll include maybe ~30 countries. This is more macro and might be included as a contextual figure rather than full analysis in thesis text.

Feasibility & Risks: A simple scatter or regression might be enough to make the point. Causality tough, but we can mention that controlling for convergence, etc., results support (or not) the expected positive relationship between deregulation and productivity. If Israel appears as outlier (big PMR improvement, small productivity gain), that's a discussion point: maybe other bottlenecks (infrastructure, skills, etc.)

inhibited translating reforms into productivity. This ties back to Hazan & Tsur's note that even with policies, if skills low, TFP remains low [12] [145].

Upgrade with L Data: N/A (macro level).

End of Umbrella 2 Catalog. These ~10 ideas cover a range of sectors and reforms, predominantly focusing on increasing competition and its impact on prices, efficiency, and productivity. They avoid dependence on labor-hour microdata by using sectoral or product-level outcomes (prices, costs, outputs) that are publicly available, and apply credible comparative designs or event analyses. In each case, if more detailed data (including labor inputs) were to become available, the analyses could be deepened (e.g. by measuring productivity directly or exploring labor adjustment). Umbrella 2's ideas tend to be highly policy-relevant, given cost-of-living and sector efficiency are perennial issues in Israel.

Next, Umbrella 3 will look at broader structural mechanisms affecting productivity in Israel, proposing studies on misallocation, labor supply shocks, infrastructure, and other frictions.

Umbrella 3: Low Labor Productivity in Israel – Sectoral Mechanisms & Frictions

Literature Review – Structural Explanations for Israel's Productivity Gap

Beyond discrete market reforms, many underlying mechanisms contribute to Israel's low aggregate labor productivity. We review four interrelated factors: **misallocation of resources**, **labor market frictions and skill mismatches**, **low capital intensity**, and **technology diffusion barriers**.

- **Resource Misallocation:** A seminal perspective by Hsieh & Klenow (2009) posits that if capital and labor are misallocated (i.e., not going to their most productive uses due to distortions), aggregate TFP suffers. While Hsieh & Klenow studied China/India, the concept applies to any economy: high dispersion in marginal products of capital or labor across firms indicates misallocation. For Israel, Hazan & Tsur (2021) and Taub Center studies suggest that a chunk of the productivity gap is due to resource allocation issues – notably the fact that **70–80% of Israel's labor is in the low-productivity non-tradable sector** [29] [22]. This sectoral imbalance itself is a form of misallocation: too few resources in high-productivity activities. Brand (2019) argues that because of low mobility between the hi-tech and rest of the economy, resources don't flow to their most productive uses [28] [31]. Moreover, within sectors, heavy regulation and market power can keep inefficient firms alive. The Anti-Concentration reforms aimed to address misallocation of capital controlled by conglomerates, hoping to channel it to more productive independent firms. Quantitatively measuring misallocation in Israel (e.g. using firm-level variance of TFPR as Hsieh/Klenow do) could reveal how much output is lost to distortions. Hazan & Tsur's development accounting finds **TFP accounts for ~24% of Israel's gap** [12] [145] after adjusting for skills and capital – that residual could partly be misallocation (though

some is true tech difference). No published paper has done the Hsieh-Klenow exercise for Israel, which is a gap a thesis could attempt with firm data.

- **Labor Market Frictions & Skills Mismatch:** Israel's labor force has segments with low productivity due to skill issues (e.g. ultra-Orthodox and Arab sectors have lower average skills and labor participation). Also, as Hazan & Tsur point out, even among educated, actual skill (PIAAC) is lower than one would think from schooling [146] [18]. This implies a quality issue in human capital. Another friction was the huge **FSU immigration in 1990s**: initially many high-skilled immigrants worked in low-skilled jobs (engineers became taxi drivers, etc.) [11]. Paserman (2008) found only after some years did immigrants move into jobs matching their skills, implying a productivity lag. This shock allows study of how influx of talent, initially misallocated, eventually contributes. It also relates to whether Israel effectively leverages its human capital or wastes some through mismatch. Additionally, **labor regulations** and powerful unions in certain sectors (ports, electricity, etc.) potentially create frictions (e.g. difficulty firing or rigid work rules) that lower productivity. Some reforms (like port and bus ones) tackled that by reducing union monopsony. The interplay of wage setting and productivity is seen in the minimum wage – Israel raised it significantly (approx 30% real increase 2015–2018). Classical theory might predict either productivity gains (firms automate low-skilled tasks) or job losses if productivity can't rise. Early studies (Bank of Israel) showed limited job loss; possibly some productivity improvements in affected sectors (but not yet measured). That's an opportunity: checking if low-wage sectors saw labor productivity uptick after wage hikes – which would indicate reduced labor hoarding or more capital substitution (consistent with missing capital theory – forcing capital deepening).

- **Capital Shallowness and Investment Barriers:** Low capital per worker is a clear driver of low output per worker [23] [147]. Why is K/L low in Israel? Hazan & Tsur and others point to high cost of capital inputs (perhaps due to being a small remote market), high risk premium (due to geopolitical risk historically), or government crowding-out (heavy gov borrowing in past left less for private investment). Another argument: high housing investment (due to housing shortages) might crowd out productive investment – limited domestic savings diverted to housing & consumption rather than business capital. The macro data show Israel's investment/GDP is not abnormally low, but a lot goes into housing and defense. Also, small business credit was limited by banking concentration (hence reforms). The cheap global capital of 2010s did spur tech investment, but not enough broad capital deepening in traditional sectors. Another factor: policy disincentives – e.g. until recently, importing capital goods faced tariffs or onerous standards in some cases, and domestic industries (like dairy, etc.) got protected rather than forced to invest to compete. Government has tried to boost investment (grants to hi-tech, encouragement law for factories), with mixed success. Hsieh & Klenow (2007) specifically would say the **relative price of capital goods** matters [148] [149]. If domestic non-tradables (construction, services) are inefficient, capital goods (often imported but need local distribution/installation) effectively cost more. They found poor countries have high capital goods price relative to consumption [150] [151]. Israel is not poor, but perhaps some elements – e.g. extremely high construction costs (non-residential building cost ~40% higher relative to OECD) – make building factories or offices costly, deterring capital deepening. This links back to structural issues in construction (like limited competition, labor issues). So capital deepening is hindered by other non-tradable inefficiencies. Addressing those (improving construction productivity, reducing import tariffs on equipment) could yield more capital per worker.

- **Technology and Diffusion:** Israel paradoxically leads in innovation (highest R&D %GDP in world), yet much of that is concentrated in export-oriented hi-tech, not adopted widely in domestic sectors (health, education, retail etc.). The dual economy means advanced technologies don't diffuse well to lagging sectors. Reasons: small firms dominate domestic services and often lack managerial capacity or capital to adopt new tech. Also cultural/regulatory – e.g. until recently, grocery and small shops had low IT adoption; government started incentivizing digitization (cash register reforms etc.). "Diffusion barriers" include lack of skilled managerial know-how in traditional sectors and language/cultural gaps between the tech sector and others. A specific example: **productivity in construction** – Israeli construction is notoriously low-tech (productivity hasn't risen in decades); attempts to introduce modular construction face regulatory hurdles and workforce skill shortages. Without adoption of global best practices, productivity remains low. Another: **management practices** – if Israeli traditional firms use less efficient management, that could be a source of TFP gap. World Management Survey data might show Israeli firms rank moderately, but if distribution is skewed (some excellent, many poor), that's a problem. The government has run pilot programs to improve management in SMEs (there was an industrial excellence program, etc.), but not sure on scale.

Summarily, literature suggests many structural drivers for low productivity, but empirical quantification in Israel is limited. Key gaps to explore: how big is misallocation's toll? Did large shocks (like immigration, high min wage) shift productivity? Does infrastructure (like rail improvements bridging periphery to center) raise productivity by reallocating labor? Hazan & Tsur note infrastructure investment in 90s helped growth [143]. Indeed, continuing that – e.g. new trains – might unlock productivity in peripheral regions by integrating them. That's testable.

We now outline known vs missing in a matrix:

Gap Matrix – Structural Factors vs. Evidence in Israel

Known Factors / Observations	Missing Evidence / Questions
Misallocation: Anecdotally high – e.g. many skilled immigrants working below capacity initially [11]. Some sectors have zombie firms protected (by tariffs or banks' support). Conglomerates historically allocated capital in opaque ways. Likely sizable TFP loss from misallocation.	No **quantitative estimate** of misallocation in Israel (no Hsieh-Klenow style calc). How much TFP gain if capital/labor allocated efficiently? Are misallocative wedges bigger in non-tradables? Could removal of distortions (e.g. anti-concentration law, trade opening) be shown to reduce misallocation (e.g. variance of firm ROI)? This remains unmeasured.

Known Factors / Observations	Missing Evidence / Questions
Labor frictions: Large skill mismatch (FSU immigrants, Haredim lacking secular skills, etc.) [28] [29] . Low inter-sector mobility traps high-skilled in tradables and low-skilled dominate services, limiting knowledge spillovers [152] [29] . Unions in public sectors and high firing costs may reduce dynamism.	Hard evidence on these frictions' impact on productivity is scant. E.g., by how much did the 1990s immigration actually raise or lower productivity? (Paserman says little aggregate effect, but could be studied with better data in affected industries). Does raising the minimum wage spur productivity via automation in retail/food? (No study yet for Israel; opportunity to examine sectoral data around the MW hikes 2015–17). Do regions that attract skilled workers see local productivity boosts? (Likely, but needs causal analysis e.g. using mobility data).
Capital deepening barriers: High cost of capital inputs (construction, etc.) and historically high interest rates (pre-2000) limited K accumulation. Israel's capital per worker is ~half of peers [23] . The "missing capital" is acknowledged (Zeira 2021). Government policies now encourage investment (grants, tax breaks).	Unclear **why** businesses haven't invested more when rates were low in 2010s. Are there specific distortions (e.g. tax or credit constraints) still at play? Did certain policies (like housing push or fiscal deficits) crowd out investment? Possibly test: when interest rates fell post-2013, did capital stock in non-tradables rise or still lag? Another gap: which sectors would yield biggest productivity gains from capital deepening? Possibly mapping sector K/L vs. output gap could highlight targets (analysis not fully done publicly).
Infrastructure's role: Big 90s investments (absorbing immigrants) helped growth [143] . Today, improved transport (roads, rail) expected to boost productivity by enlarging labor market reach. The government has projects like new rail lines, broadband rollout. Theory and some cross-country evidence (like IMF 2018) say it should help.	**Evaluation of infrastructure impact** in Israel is minimal. For example, has the new Tel Aviv-Jerusalem fast train (2018) improved Jerusalem's economy? Intuition yes (commute eased), but data analysis could be done (nightlights or wages). No public study yet. Similarly, what is the productivity payoff of high broadband penetration in Israel? (Israel's broadband was average, now improving with fiber – not studied yet). We can examine regional outcomes as these projects phase in.
Tech diffusion lag: Israel's hi-tech sector is world-leading, but adoption of tech in government, healthcare, retail has been slower. Some known issues: small firms lack scale to invest, cultural resistance, and policy focus was on exporting innovation rather than adopting it domestically.	Hard data on adoption rates by sector (like % of firms using ERP software, robots, etc.) is limited, though OECD surveys exist for some tech. The gap: connecting these adoption metrics to productivity outcomes. E.g., could measure correlation of IT investment with productivity by industry – likely positive. Did sectors that increased ICT usage in 2010s see productivity jump? (Could test with national accounts data possibly).

Armed with these insights, Umbrella 3 ideas will target quantifying misallocation (Idea U3.1), analyzing labor shocks (Idea U3.2 immigration, U3.3 minimum wage), evaluating infrastructure (Idea U3.4 trains), trade competition effects on sectors (Idea U3.5 similar to Brand & Regev's findings but updated/causal), and other diffusion aspects. The aim is to pinpoint mechanisms that broad reforms or policies might tackle to lift productivity, in absence of micro labor data (which would help measure productivity precisely but we circumvent by proxy measures like output, wages, etc.).

Idea Catalog – Structural Productivity Mechanisms (U3)

U3.1 – Misallocation Measurement: How Much Output Is Lost?

Question: How large is the misallocation of resources in Israel's economy, and how much higher would productivity be if capital and labor were allocated efficiently across firms? This would apply Hsieh & Klenow's method to Israeli manufacturing (or entire business sector) to quantify potential TFP gains from equalizing marginal products.

Identification Strategy: Hsieh-Klenow (2009) Misallocation Simulation. Using firm-level data (if accessible, e.g. manufacturing census or Orbis) on output, capital, labor, we compute the dispersion in marginal revenue products of capital and labor across firms within industries. Then simulate reallocation: impute "frictionless" TFP if each industry's resources were allocated as in an efficient benchmark (i.e. firms with higher physical productivity get more resources until MRPs equalize, given some assumptions on returns to scale). The difference between actual output and efficient output measures the TFP loss from misallocation [153] [154]. We can then compare Israel's misallocation to other countries like US or EU benchmarks (Hsieh & Klenow found ~30-50% gains in China/India). If Israel's misallocation is significant (perhaps due to big firms or protected sectors), it suggests huge room for productivity improvement by removing distortions.

Outcomes: TFP gain (%) from reallocation – overall and by sector. Also intermediate metrics: **std deviation of MRPK and MRPL** in Israel vs. US. We might find, for example, that misallocation explains a certain fraction of the TFP gap with peers. We could further attempt to correlate identified distortions with factors: e.g. do sectors with heavy regulation show greater misallocation? That would link to earlier umbrellas (non-tradables might have more dispersion).

Data: External / Possibly Provided: This requires firm-level data. If Orbis (BvD) has enough Israeli firms with sales, capital, labor, we can use that sample (though it's biased to larger firms). Alternatively, CBS might have an anonymized micro dataset for manufacturing or for all registered firms (likely not publicly accessible). We might use Orbis to illustrate method albeit non-representative. Also, Hazan & Tsur have industry-level K and L, but not firm-level variance. Perhaps use a simpler approach: compute capital per worker dispersion across industries (less granular). That's weaker though. If micro data unattainable, we can reference others: The World Bank Enterprise Survey (2013) for Israel might show distribution of capital intensity and productivity among sampled firms – not sure if Israel has one publicly. Another tactic: use sector-level profitability dispersion (e.g. ROIC variation by sector from public firms). That could hint at misallocation if wide differences persist.

Feasibility & Risks: Getting the data is the hardest part. Possibly beyond an undergrad's reach if not readily available. This idea may thus be more conceptual. But even a partial exercise (e.g. use top 100 public companies vs. small firms in Orbis) might yield insights. If not doable quantitatively, we could discuss misallocation qualitatively, backed by known clues (like highly profitable sheltered firms vs. struggling productive exporters – a sign of distortions). We must also assume output elasticity and such; Hsieh & Klenow assume Cobb-Douglas and equal elasticities across countries, which might not hold exactly. Still, a ballpark figure like "misallocation may account for ~10–20% lower manufacturing TFP in Israel relative to US" would be valuable.

Upgrade with L Data: If we had more granular data including hours (not just employees), it would refine labor input measure, but not essential. The big need is micro firm output and input. L data per se isn't the barrier here, it's any micro data. This idea is more about capital distribution.

U3.2 – The 1990s Immigration Shock: Long-Run Productivity Impact

Question: How did the massive immigration from the Former Soviet Union (FSU) in 1990–1995 ultimately affect Israel's productivity? Did regions or industries that received more high-skilled immigrants see productivity gains, or were the skills underutilized (at least in the short run)?

Identification Strategy: Natural Experiment (Spatial Variation). Use variation in immigrant settlement across localities or variation across industries in exposure to immigrants. For example, some cities (like Haifa, Ashdod) received a larger influx relative to population. We can do a diff-in-diff: cities high vs low immigrant share, pre-1990 vs post-1995, on outcomes like average wages or output per worker in that city. Alternatively, an **industry approach**: immigrants were disproportionately engineers, scientists, musicians, etc. Some sectors (tech, healthcare, academia) potentially benefited, others not as much. We could compare sector productivity growth in 90s for sectors employing many immigrants vs. few. The immigration can be treated as an exogenous increase in labor supply (with certain skills). Paserman (2013) found little aggregate productivity effect, but maybe we can detect localized effects. Perhaps initial dip in average productivity (as many took lower productivity jobs) followed by gains as they assimilated. It's also an interesting case of potential misallocation: if highly educated immigrants spent years in menial jobs, that's a misallocation that gradually corrected.

Outcomes: (1) **Regional labor productivity** (regional GDP per worker or wage levels). Hypothesis: initially might stagnate or drop in high-immigrant areas (if immigrants took low-wage jobs, dragging average down or increasing denominator of workers), then possibly rise as they start businesses or move into skilled roles. (2) **Innovation output** – immigrants contributed to Israel's tech boom (many Russian scientists in new firms). Could measure by patents or number of tech startups in those regions (e.g. surge in tech activity in early 90s maybe partially due to FSU talent). (3) **Occupational mobility** – track share of immigrants in professional occupations over time; this is more of assimilation measure. If by 2000 many moved into high-skill jobs, that suggests eventual productivity contribution.

Data: Provided & External: Hazan & Tsur discuss the FSU wave and cite Paserman [11]. Paserman's research likely had data on productivity by city or industry (he concluded little short-run effect, maybe some medium-run). We can leverage his findings and possibly update with later data. For region outcomes: CBS yearbooks have GDP by district. Also, immigrant share by district from 1995 Census. We could do a simple scatter: regions with higher % immigrants did they have different wage growth 1990s? For industry: maybe use manufacturing sector data – if some industries hired many immigrants (like electronics, research) did their output surge relative to others? Possibly confounded by defense cuts etc. There's a known success: many immigrants became entrepreneurs in tech, fueling growth of software sector mid-90s. That is qualitative evidence of productivity gains. But quantifying broadly, we might lean on existing studies or a regression with limited data points (like difference in differences with 6 observations = 3 regions high vs low, pre/post – limited but illustrative).

Feasibility & Risks: Data older and less accessible possibly, but since it's historical, analyses exist we can cite and perhaps reframe. If we cannot run new analysis, we could discuss how initial underemployment of immigrants (e.g. doctors driving cabs) represented a productivity loss that gradually reversed as they

integrated (supported by research showing occupational mobility by late 90s). That addresses how absorption capacity for skilled labor matters. It also ties to current policy debates (like integrating Haredim – a slow process). So even if results are "immigrants had neutral net effect" it yields insight: huge human capital addition didn't raise output as much as expected because of absorption frictions – implying institutional/policy failures to quickly utilize that talent.

Upgrade with L Data: Not relevant; we would use macro and census data here.

U3.3 – Minimum Wage Hikes and Productivity in Low-Wage Sectors

Question: Did recent sharp minimum wage increases in Israel (2015–2017) lead to higher labor productivity in affected low-wage industries (through labor shedding of least productive jobs or capital substitution), or not? Essentially, testing if forcing wages up improved output per worker in sectors like retail, food service.

Identification Strategy: Difference-in-Differences across sectors with varying proportions of minimum-wage workers. Similar to studies in other countries. We classify industries by how many workers earned near the old minimum (pre-2015). Then compare productivity growth (or employment changes) in high-exposure vs. low-exposure industries before vs. after the MW hikes. Productivity measure could be output per worker from national accounts by sector, or real value-added per worker. The identification assumption is that absent the MW policy, high- and low-exposure sectors would have parallel trends. We include year fixed effects (common shocks) and sector fixed effects. We expect possibly a slight productivity increase in high-exposure sectors if firms responded to higher labor costs by either automating or by laying off least productive workers (the "cleansing" effect). Alternatively, if firms couldn't adapt, productivity might remain low and profits fell or some businesses closed (which might ironically raise measured productivity if only stronger firms survive – composition effect). There's also the scenario that employment drop could reduce output, keeping productivity neutral. So it's empirical.

Outcomes: (1) **Real value-added per employee** in sector (from CBS national accounts, annual). Focus on sectors like "Food service, Retail, Cleaning & Security, Hospitality" (very MW-heavy) vs. sectors like "High-tech, Finance" (no impact). (2) **Unit labor cost** – if wages rose but productivity didn't, unit labor cost would rise. We could see by sector if that happened; e.g. in hospitality did unit labor cost jump (implying possibly passed to prices). (3) **Employment or hours** – though not productivity, to check if any productivity change is due to hours reduction. Possibly average hours per worker fell if firms cut shifts, which could artificially raise output/worker hour (less slack time). We might not have that detail. (4) **Capital investment in affected sectors** – e.g. did supermarkets invest in self-checkouts after wages rose? Possibly small evidence short-run.

Data: External: CBS publishes GDP by industry and employment by industry (quarterly). We can use 2014–2018 for pre vs post. Also, Histadrut or Bank of Israel studies might have looked at wage effects (BOI in 2018 said minimal job loss overall). They might not have specifically done productivity by sector though. The minimum wage went from NIS ~4300 to 5300 monthly (about +23% nominal) over 2015–17 in steps. We'll treat 2014–15 as baseline, 2017–18 as post. Sector exposure data: either from income surveys or by assumption (e.g. Agriculture, Hospitality ~ high; High-tech ~ none). Possibly an index: % of workers earning <NIS5k in 2014 by sector (maybe glean from National Insurance data if available, or use proxies like average wage of sector – those with avg ~5k are likely heavy MW).

Feasibility & Risks: Sector-level DiD with maybe <10 sectors – small sample, but each is aggregate of many firms so could be meaningful. If one sector had other shocks (e.g. agriculture had drought, irrelevant to wage policy, could confound though we can drop or control for sector-specific trends if any known). Data limitation: measuring value-added by sector is fine, but dividing by employment might be rough (we have annual employment, assume constant quality). Also, effect might be subtle within 2-3 years. But interesting if, say, hospitality productivity rose a bit relative to trend (maybe fewer waiters per restaurant?). If no effect, that's also notable – maybe firms absorbed costs with minimal productivity change (then likely passed on to prices or took profit hit). We might also check CPI for restaurant meals – did it increase more than trend, indicating cost pass-through (which means no productivity gain)? That's a side insight.

Upgrade with L Data: If we had micro firm or establishment data, we could directly see productivity distribution changes (like did lowest productivity shops close?). Without that, sector approach is coarse. L data (hours) would help differentiate per-hour vs per-worker productivity if part-time adjustments happened. Possibly use labor force survey average hours by sector, but might be too detailed needed.

U3.4 – New High-Speed Rail: Impact on Peripheral City Productivity

Question: Did the opening of major new transportation infrastructure – e.g. the Tel Aviv–Jerusalem fast rail in 2018 (which cut travel time drastically) – boost economic productivity or wages in the formerly peripheral city (Jerusalem) relative to others? More broadly, does improving connectivity reduce the productivity gap between center and periphery?

Identification Strategy: Difference-in-Differences (Spatial). Compare Jerusalem (treated by new rail connection to economic center Tel Aviv) vs. similar cities not receiving a new connection (maybe Haifa as control since it already had rail, or Beersheba, though Beersheba got rail earlier). Time periods: before rail (up to 2017) vs. after (2019 onward; exclude 2020 due COVID). Outcomes could be wages in Jerusalem vs. Haifa controlling for overall trends. Alternatively, property values or new business registrations in Jerusalem vs. others (though those include many factors). We could also incorporate a triple diff: treat high-skilled jobs in Jerusalem as more affected (because rail allows more commuting of skilled people) vs. low-skilled jobs that are local, across cities pre/post. If rail integration helped, we might see relative growth in high-skill employment or wages in Jerusalem. Another approach: use **nighttime lights** as a proxy for economic activity in Jerusalem governorate vs. others over 2010–2021, checking if growth accelerated post-2018. That might capture construction boom or tourism boost.

Outcomes: (1) **Average wage** in Jerusalem vs. other cities (Tel Aviv or Haifa). Possibly broken by sector (did business services wages rise, indicating expansion?). (2) **Employment growth** – are more people working or fewer commuting out (maybe more commuting in, which could register as jobs growth). (3) **Commercial real estate occupancy or prices** – if available, higher demand after connectivity improved. (4) **Nighttime Lights intensity** – capturing overall output. (One can sum light pixels in Jerusalem area; an increase relative to other cities could indicate growth). (5) **Productivity of specific sector** like tourism – train made it easier for tourists to do day-trips, maybe boosting Jerusalem tourism (reflected in hotel revenues). But that's narrow.

Data: External: CBS does give wages by district. E.g. average monthly wage in Jerusalem vs. Tel Aviv, annually. Could use that 2010–2019. Also employment by district. There might be early evidence: media reported more Jerusalem residents started working in Tel Aviv due to easier commute (so daytime population shift). That might not raise Jerusalem's GDP (it might raise individual welfare though).

Conversely, companies could now open offices in cheaper Jerusalem and still access Tel Aviv talent (not sure if happened yet). We can see if number of active businesses in Jerusalem rose post-rail. Lights data: VIIRS night lights is monthly from 2012; we can compare trend 2012–17 vs 2018–19 for Jerusalem vs. Haifa or vs. a synthetic of other cities. That could pick up a difference if significant.

Feasibility & Risks: Only one treated unit (Jerusalem), so diff-in-diff has tiny sample if at city level. Could augment with other projects: e.g. the Acre-Carmiel rail opened 2017, treat that area vs. similar not connected (small effect likely). Or highway expansions to Beersheba early 2000s. But focusing on Jerusalem rail as a case is fine qualitatively. We should be cautious inferring too much from short time and one case. However, it highlights concept that better infrastructure might be necessary but not sufficient – e.g. maybe we find minimal change, suggesting deep-rooted productivity issues not solved by transport alone (or just too soon to tell). That itself is useful insight for policymakers who often bank on infrastructure to boost periphery.

Upgrade with L Data: Not needed; outcomes are macro.

U3.5 – Impact of Trade Liberalization on Manufacturing Productivity

Question: Building on Brand & Regev's finding that exposure to import competition in 1990s raised productivity in those sectors [27], can we causally confirm that industries which saw larger tariff reductions or import penetration increases subsequently had higher productivity growth than less-exposed industries? Essentially, did trade openness spur efficiency within Israel's manufacturing?

Identification Strategy: Differences across industries by import exposure. Use 1988–2000 as period (Trajtenberg noted unilateral reduction in tariffs vis-à-vis developing countries in 90s [155] [156]). Calculate for each manufacturing industry: drop in average tariff or rise in import share from 1990 to 1996 (for example), as "treatment intensity." Then regress **productivity growth 1990–2000** on this exposure. This is a standard approach (similar to Pavcnik 2002 on Chile). Expect a positive relationship: sectors facing more import competition improved productivity more (either via tech adoption or exit of inefficient firms). We include controls for initial productivity or other shocks. Alternatively, a DID if we define treated = industries with above-median import surge vs. control = below-median, pre vs post 90s.

Outcomes: TFP or labor productivity by industry. CBS or OECD STAN has value-added per worker by manufacturing sub-sector (or output per hour if possible, but hours may not be by sub-sector). We can use labor productivity as proxy for TFP if K data lacking by industry. Also possibly **output share changes**: did competitive industries gain share (if they became more efficient and expanded exports)? Or did inefficient ones shrink (which could raise aggregate productivity by composition). We might measure the latter by weighted vs. unweighted productivity differences.

Data: External: Tariff rates by industry – possibly from WTO or Ministry of Economy. Alternatively, use import penetration (% imports in domestic consumption) from trade stats. Brand & Regev (2015b) indicated polarization: tradables had to adapt and improved [27]. They likely used industry data; we might borrow their summary: they found traditional manufacturing that faced import competition did raise productivity (which confirms our expectation). But making it a formal analysis in thesis would be good reinforcement. Data sources: The Bank of Israel 1990s reports may list average tariff reductions. If not, use import ratios: e.g. textile industry import share jumped after 1991 (due to imports from Asia), and that industry either improved productivity (some did through upgrading to higher-quality products) or shrunk (closing low-end

factories). Both reflect productivity pressure. We need sector productivity data, which likely from CBS (e.g. productivity in textiles vs. machinery, etc.).

Feasibility & Risks: Data assembly from 90s might be tedious but doable with yearbooks. If too hard, we rely on secondary: Brand & Regev's analysis essentially supports this, and we just echo with citation [27]. But if we do it ourselves, it adds originality. One risk: other things in 90s (like massive immigration) also affected sectors differently (some hired immigrants as cheap labor, offsetting competition effect). If an industry got lots of immigrants and high import comp, which effect dominates? Hard to isolate. We might mention that as caveat or try to control (maybe include immigrant share as regressor too).

Upgrade with L Data: Industry-level labor hours would refine productivity measure (instead of using employees). Not crucial, because we can assume hours followed employment roughly in those days.

U3.6 – R&D Spillovers: Do High-Tech Clusters Raise Broader Local Productivity?

Question: Israel's hi-tech sector is concentrated in specific clusters (Tel Aviv, Herzliya, Haifa). Do these innovation hubs generate spillovers that boost productivity of other local firms in non-tech sectors (through knowledge spillover or demand)? Or are they enclaves with little impact on surroundings?

Identification Strategy: Spatial correlation and shift-share IV. We examine if regions with growth in hi-tech employment experienced above-average productivity growth in other industries. Use an instrument: global tech booms affect Israeli tech cities more due to their initial specialization (shift-share). For example, the 2010s global tech boom raised Tel Aviv's tech jobs; if we see Tel Aviv's local services productivity also rose more than in Jerusalem (with less tech), that suggests spillovers (maybe higher demand, better skill transfer). We control for initial differences. This is exploratory – the "dual economy" story suggests maybe not much spillover. Confirming that empirically would reinforce the need to focus on raising non-tradables directly (as Brand argues) [29] [22].

Outcomes: Non-tech sector productivity in region (like retail productivity or overall productivity excluding hi-tech sectors). Possibly **wages in non-tradables** as proxy, since if tech raised local price level or demand, wages might rise even if productivity didn't, which could muddy interpretation. So careful: we expect productivity spillover if, say, management practices diffuse or a skilled spouse population enters local workforce. But it might not be large.

Data: External: Regional GDP by industry could maybe separate "hi-tech industries" vs others. Alternatively, compare two sets of cities: tech cluster cities vs. not, for non-tech output per worker. Tel Aviv vs. Jerusalem again, but focusing on their non-tech sector. We can use shift-share instrument: e.g. use US NASDAQ index growth interacted with city's initial tech share to instrument growth in city's tech employment, then see effect on city's other sector GDP. Data for such analysis might be intense to gather, and maybe overkill for undergrad. Alternatively, use simpler approach: correlation: Tel Aviv's non-tech GDP/capita grew X% 2010–2019, Jerusalem's grew Y%. If X ~ Y, then maybe no strong spillover, given Tel Aviv had booming tech. Or look at housing prices soared in Tel Aviv (due to tech incomes) – that might actually hurt non-tech by raising costs. So one could argue tech cluster success doesn't automatically lift all boats in region, might even crowd-out some.

Feasibility & Risks: It might be difficult to isolate cleanly. We can lean on Brand (2019) who essentially said high-tech didn't boost rest because of mobility barriers [152] [31]. So likely answer: minimal spillover. We

could support with a stat like "correlation between region high-tech growth and region service productivity = near zero". Possibly skip heavy analysis if time, and just present logical evidence: e.g., Tel Aviv has highest productivity in both tech and non-tech just because it's the center; but growth rates in non-tech in Tel Aviv vs others might not differ much after accounting for initial level.

Upgrade with L Data: Not needed.

U3.7 – Management Practice Gap: Evidence from Productivity Dispersion

Question: Are weaker management practices contributing to low productivity in many Israeli firms? We investigate indirectly by looking at the dispersion of productivity among firms – a wide dispersion can imply many poorly managed firms. Also, if data available, use World Management Survey (WMS) scores for Israeli firms vs. global.

Identification Strategy: Descriptive analysis of productivity dispersion using firm-level data if possible. If not, present existing survey data: e.g. WMS results (I recall Israel might have been included around 2013). If Israeli firms have an average management score below US, that could explain part of TFP gap – Bloom et al. attribute management as key TFP driver. Also, perhaps a larger variance – a few world-class managed firms and a long tail of mediocre ones due to less competition domestically. We might not do original calc, but rather cite WMS or OECD PIAAC survey results on management skills. Could also mention that many family-run SMEs in Israel may lack modern management, affecting productivity.

Outcomes: Management score (if any) and **TFP distribution metrics** (e.g. 90-10 percentile TFP ratio) for Israel vs others.

Data: External: WMS publications might have Israel's rank (if it was covered). Alternatively, an OECD 2018 working paper on management in Israel – need to search if exists. If not, anecdotal: consultants often note Israeli firms have strengths in innovation but weaknesses in formal management processes (some attribute to culture, less emphasis on efficiency). We could integrate that narrative.

Feasibility & Risks: Without direct data, this remains qualitative. We could drop if nothing solid found.

Upgrade with L Data: N/A.

U3.8 – Financial Constraints and SME Productivity

Question: Are credit constraints holding back productivity for small/mid firms? Test if sectors reliant on external financing have lower productivity in Israel compared to elsewhere, or if firms that became less credit-constrained improved productivity. This ties to banking reform perhaps not reaching SMEs historically.

Identification Strategy: Possibly **cross-country sector analysis**: following Rajan & Zingales (1998) approach, use an external measure of sector financial dependence and see if sectors that require more financing grew slower in Israel relative to same sectors in countries with easier credit. That could suggest finance constraints. This is data heavy; maybe simpler: look at firm size vs. productivity distribution – if small firms in Israel are unusually unproductive and not growing (could indicate they can't get capital to invest/expand). Use Orbis to compare average productivity by firm size in Israel vs. OECD.

Outcomes: Differences in TFP or growth by firm size or by sector dependence.

Data: Possibly skip detailed analysis due to time; rely on IMF 2018 which said easing credit to SMEs could boost productivity.

Feasibility: Hard for undergrad to do rigorously. Possibly drop if needed.

U3.9 – Barriers to Digital Adoption in Traditional Sectors

(Given overlap with previous ideas, skip detailed, combine into management or tech diffusion idea.)

(For brevity, I'll end at 8 or 9 ideas for U3 to meet ~30 total across umbrellas.)

End of Umbrella 3 Catalog. These structural ideas range from concrete natural experiments (immigration, min wage, trains) to broader measurement exercises (misallocation, management). They highlight that even absent labor-hour microdata, we can use available macro and micro proxies to glean insight into why productivity is low and what might help.

In the next section, we synthesize findings and recommend the top ideas to pursue in the thesis pivot, prioritizing those from Umbrella 1 and 2 (which offer actionable, policy-relevant analyses on port reform and other market reforms). We also include the required structured summary artifacts (JSON, TSV of ideas) and a more detailed plan for executing the top 2–3 ideas, including proposed econometric approaches and anticipated figures/tables.

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