Elbows Up: Economic Coercion and U.S. Border Labor Markets in the 2025 Trade War

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Abstract

In early 2025 the United States imposed tariffs on Canada and paired them with public statements questioning Canadian sovereignty. Canadians redirected their travel: return trips from the United States fell about a quarter while trips overseas rose about nine percent, and the roughly US$2 billion withdrawn is almost entirely a leisure-trip withdrawal. We trace where that cost landed inside the United States. Across 352 metropolitan areas with predetermined (2024) visitor exposure, leisure and hospitality employment fell with land-crossing exposure—roughly 0.57 percent per standard deviation, with a within-metro triple-difference placing a conservative floor of 0.30 percent beneath it—but not with Canadian air-destination exposure; the two gradients are statistically distinct ($t = -4.1$). Administrative passenger data show why: Canadian passengers were a few percent of arrivals even at the most exposed major airports, while the land day-trip economy had no such buffer. A southern-border placebo (Mexico faced the same tariffs with no comparable withdrawal) and the Canadian overseas comparison tie the decline to the U.S.-specific redirection of Canadian travel, not the tariffs the two countries shared. The gross local loss is at most on the order of ten to thirty thousand jobs; an independent spending-side calculation from Canadian travel-survey data implies the same range. The gradient did not unwind when the tariffs were struck down in February 2026.

Keywords: economic coercion; weaponized interdependence; consumer boycott; local labor markets; tourism; trade war.
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*All errors are my own. For replication files and additional information please contact Philip Luck at philip.a.luck@gmail.com. Working paper, this version July 2026.

1 Introduction

Economic coercion generates a return flow of costs to the state that imposes it, and that flow has a local incidence. The study of economic statecraft concentrates on the coercer's instruments and the target's compliance (Baldwin, 1985; Farrell and Newman, 2019; Drezner, 1999): who holds the chokepoint, whether sanctions change behavior, and who bears the cost inside the target state. Costs that flow back to the coercer have been measured at the level of national trade and financial flows (Crozet and Hinz, 2020; Besedeš et al., 2017), but rarely below it. The costs studied here are imposed by a target's *decentralized* response, expressed through consumers and sub-national governments rather than a state's formal retaliation. That response has not been traced to the coercer's own local labor markets. The 2025 confrontation between the United States and Canada is an unusually observable case.

In early 2025 the United States imposed tariffs on Canadian goods and paired them with repeated public statements questioning Canadian sovereignty. The Canadian reaction arrived mainly through a collapse in travel south, with provincial liquor monopolies pulling American whiskey in parallel. Statistics Canada recorded Canadian return trips from the United States falling about a quarter over the year (Statistics Canada, 2026). The National Travel and Tourism Office's I-94 series shows Canadian air arrivals dropping from 9.65 million in 2024 to 8.47 million in 2025, a decline of 12.3 percent (U.S. NTTO, 2026) (discussed in Section 8). The withdrawal was selective. Canadian survey data show leisure visits to the United States fell about 26 percent while visits to friends and relatives fell 12 percent, with spending on those family visits held. The roughly US$2 billion that Canadians withdrew over the last three quarters of 2025 is therefore almost entirely a leisure-trip withdrawal (Section 2, Online Appendix I.K).

The goal of this paper is to measure the blowback that followed these events in U.S. communities. The design pairs cross-border-visitor exposure measured before the boycott (2024) with employment outcomes through 2025 and into 2026; because exposure is predetermined, the design isolates how the post-onset path of local employment varies with a community's prior dependence on Canadian visitors.

The central empirical difficulty is attribution. Tariffs and the boycott landed on the same border communities at the same moment, so a decline near the crossings is consistent with either. Much of this paper is devoted to separating them. The headline estimate is cross-sectional: leisure and hospitality employment fell with land-crossing exposure, by about 0.57 percent per standard deviation, stable across the inference methods appropriate for a small treated set. We then defend that decline against four rival explanations. First, the Canadian-side data rule out the most basic alternative. Canadian trips to the United States fell while Canadian trips overseas rose, so the pullback was a redirection away from the United States, not the general retreat from foreign travel that a weaker dollar or softer incomes would have produced. Second, a southern-border placebo: Mexico was tariffed on the same schedule but faced no annexation rhetoric and no comparable travel boycott. Built identically, its exposure gradient is a clean zero. Third, a region check: forcing identification within Census division and month retains three-quarters of the gradient, so the decline is not a slow North-versus-Sun-Belt divergence. Fourth, the metro's own tariff shock: adding a community's predetermined tariff exposure, and dropping Detroit, among the most tariff-exposed crossing metros, leaves the estimate intact.

The mode decomposition serves a different function: it maps where the cost fell. Air-served Sun-Belt destinations with high Canadian exposure show no detectable loss. Administrative airline data show why: Canadian passengers were a trivial share of traffic even at the most exposed major airports. The incidence concentrates on the land day-trip margin, where the local service economy depends on Canadian visitors. The pullback also outlasted the tariffs that accompanied it, persisting through the February 2026 court ruling that struck them down.

The paper makes two contributions. The first is to place the episode inside the economic-coercion framework, treating the travel withdrawal as a measurable cost that returns to the coercer. The second is to quantify it: on the order of ten to thirty thousand leisure and hospitality jobs lost in exposed land-border communities. That count is an upper bound on the gross local incidence, and the national net is smaller: whatever domestic spending filled the gaps at air destinations was demand redirected from elsewhere in the United States, not new expenditure.

We build directly on Kurmann et al. (2026), who provide the first U.S.-local estimate of the 2025 tourism collapse. They report roughly a 6 percent employment contraction at the most Canadian-exposed establishments, document that the Canadian withdrawal was nearly uniform across land and air crossings, and pool their visitor-exposure measure accordingly. Our value added over that benchmark is threefold. First, we show that the uniform withdrawal maps into a sharply *non-uniform* employment incidence. Second, we locate the mechanism behind the air null in administrative T-100 passenger data, independent of the employment result itself. The withdrawal was near-uniform in percentage terms across destinations, but Canadian passengers were a few percent of arrivals even at the most exposed major airports, so the gap was absorbed by a stable domestic base. Third, we add the Canadian-side overseas comparison that separates a destination-specific boycott from a general travel contraction, a confound the U.S.-side data alone cannot rule out. Taken together, these reorient the question from a generic tourism shock to the return cost of coercion.

Our estimate is an association, not a causal claim: the boycott is not randomly assigned across places, the post-onset window is short, and the outcome series are not seasonally adjusted. The falsification tests strengthen the reading of the decline as blowback from the coercion episode; none of them makes it causally identified.

Section 2 describes the episode; Section 3 develops the framework and derives testable predictions; the data, design, and results follow in Sections 4–8; Section 9 quantifies the incidence; and the conclusion draws implications for coercion strategy.

2 The episode

2.A From annexation rhetoric to a boycott

The 2025 confrontation began as rhetoric before it became policy, and that order of events shaped the response. Through late 2024 and into January 2025, President Trump repeatedly suggested that Canada should become the fifty-first state and referred to the Canadian prime minister as a "governor." Those statements reframed an ordinary trade dispute as a challenge to national sovereignty, and they did so before any tariff took effect. On February 1, 2025, the administration issued executive orders imposing a 25 percent tariff on Canadian goods, with a 10 percent rate on energy, under the International Emergency Economic Powers Act. The statutory rate on non-USMCA Canadian goods was later raised to 35 percent, effective August 1, 2025. The duties were paused on February 3 for thirty days and took effect on March 4, at which point Canada answered with countervailing tariffs of its own, a first tranche that included U.S. wine, spirits, and beer.[1]

The popular reaction preceded the tariffs. The threat to sovereignty, more than the price of any traded good, mobilized a consumer response under the banners "Buy Canadian" and "elbows up," the latter borrowed from the hockey player Gordie Howe. Canadians booed the U.S. anthem at sporting events, substituted domestic brands for American ones, and, above all for U.S. communities, stopped traveling south. Contemporaneous polling registered how broad the shift was: by mid-February 2025, nearly half of Canadians reported cancelling or delaying planned trips to the United States, and a clear majority said they were turning away from American products.[2] Provincial governments moved in parallel. Several liquor boards pulled U.S. wine and spirits in February, and on March 4 Ontario's Liquor Control Board removed all U.S. products by order of the premier.

Two features of this sequence explain the incidence we go on to estimate. First, the boycott rested on national-identity sentiment rather than relative prices, which made it broad and left it under no single actor's control; its durability is documented in Section 6. Second, the most consequential decision available to an ordinary participant was to stop crossing the border for a day-trip or a short visit. That choice concentrates the cost in the non-tradable service economy of the land-border communities rather than in traded goods, so the geography of the blowback follows directly from the form the boycott took.

The pullback was specific to the United States, as the Canadian-side data demonstrate (Figure 1). Over 2025, Canadian-resident return trips from the United States fell about a quarter year over year, while trips from overseas destinations *rose* about nine percent; Section 7 uses this contrast as falsification evidence.[3] Within travel to the United States the decline is concentrated in automobile trips, with air travel falling far less, the same land-versus-air split that structures the incidence we estimate below. Within automobile travel the pullback was uniform across trip duration: same-day and overnight trips each fell by about a third, so what concentrates the incidence at the crossings is predetermined exposure, the weight of Canadian visitor spending in border-metro demand, not a differentially deeper day-trip pullback.[4] The survey side adds an intensive margin

[1]Timeline drawn from the underlying executive orders and contemporaneous reporting: the February 1 IEEPA executive order (25 percent on Canadian goods, 10 percent on energy), the February 3 thirty-day pause, and the March 4 implementation, met the same day by Canada's 25 percent counter-tariff on a first $30 billion tranche of U.S. goods.

[2]Angus Reid Institute, fielded February 16–18, 2025 (n = 3,310 Canadian adults): 48 percent said they were cancelling or delaying plans to travel to the United States, 59 percent said they would boycott U.S. products, and 78 percent said they were already buying, or were likely to buy, more Canadian goods.

[3]Kurmann et al. (2026) present the same land-and-air breakdown of the Canadian withdrawal from the same Statistics Canada source (their Figure B2) and read it as evidence that the decline was roughly uniform across crossing types. We add the overseas comparison, which is what separates a destination-specific boycott from a general travel contraction, and we put the breakdown to a different use below, tracing a mode-specific employment incidence.

[4]Statistics Canada frontier counts (Table 24-10-0053, traveller-type dimension): same-day automobile trips −33.3% and overnight automobile trips −32.2% over March–December 2025 against the same months of 2024, with neither recovering after the February 2026 ruling (−31.1% and −33.6% in March–April 2026 against 2024). The du-

the frontier counts cannot see. National Travel Survey aggregates for the second through fourth quarters of 2025, against the same quarters of 2024, show the withdrawal was purpose-selective: leisure visits to the United States fell about 26 percent while visits to friends and relatives fell about 12 percent, so the trips that survived skew toward family travel. The surviving overnight trips were not shorter—nights per visit rose about 9 percent as the short discretionary trips exited the mix—but spending per night fell about 5 percent in nominal Canadian dollars while rising 7 percent on overseas trips, and restaurant-and-bar spending in the United States fell by a quarter against a rise overseas.[5] For the employment design this matters as composition: the visitors who still come are disproportionately family-visitors, the segment that spends the least per day on the restaurants, recreation, and accommodation where the measured incidence falls.

The bottom panels of Figure 1 quantify the visual contrast, regressing the log gap between each pair of series on event-time dummies with calendar-month effects and a differential trend. The United States–overseas gap is flat through 2024, then falls to an average of -0.42 log points, roughly a third, over the thirteen months from onset; the automobile–air gap falls to -0.23, roughly a fifth. The U.S. series turns down from January 2025, and the estimated gap breaks with the February 2025 executive order—before the March tariffs took effect. That timing is consistent with the sovereignty dispute, not relative prices. And the pullback did not unwind when the tariffs were later struck down (Section 6).

2.B Two instruments, two margins

The Canadian reaction ran through two channels: a travel withdrawal and a wave of provincial spirits delistings, the Ontario board's removal of roughly 3,600 U.S. products being the largest.[6] These two channels land on different margins, which is what makes one of them measurable here. The travel withdrawal's cost falls on non-tradable service *labor* in the day-trip economy, where it registers in local employment; the delisting's cost falls on producer *revenue* in a few distilling states,

ration margin extends to within-automobile travel the near-uniformity of the withdrawal that Kurmann et al. (2026) document across crossing modes.

[5] Statistics Canada Table 24-10-0070 (National Travel Survey), visits, nights, and expenditures by geography of visit, main trip purpose, and visit duration; year-over-year changes averaged over 2025Q2–Q4. Survey aggregates, quoted without sampling inference. The Canadian dollar depreciated against the U.S. dollar early in the episode, which raises the Canadian-dollar cost of a given basket of U.S. activity; the nominal per-night decline therefore understates the real one. Total Canadian spending in the United States fell about 19 percent. The U.S.-side air-visitor survey shows the same signature independently: among Canadian air visitors in 2025, mean nights in the United States rose about 6 percent while spending per visitor-day fell about 10 percent (NTTO Survey of International Air Travelers, Canada, CY2023–CY2025).

[6] Provincial boards pulled U.S. product in February, and on March 4, 2025 the LCBO, the importer of record for U.S. beverage alcohol in Ontario, removed roughly 3,600 U.S. products; Canada's federal 25 percent counter-tariff on wine, spirits, and beer took effect the same day, so the goods channel bundles the delistings, the counter-tariff, and the consumer boycott. U.S. spirits exports to Canada fell by about half over March–December 2025 (Census; the Distilled Spirits Council's 2025 export report puts American-origin spirits shipments to Canada down about 70 percent), including a 50 percent decline from Kentucky and Tennessee, with no recovery through May 2026. Canada took roughly $110 million of the two states' spirits exports in 2024; against a two-state distilling workforce near ten thousand in a capital-intensive industry, the predicted employment response is far below what a state-level design could detect. The incidence therefore falls on producer revenue by construction, the producer-side counterpart of the dose argument of Section 8, and we keep the channel to the side of the employment estimates. Brown-Forman reported its Canada sales down 62 percent, its chief executive calling the action "worse than a tariff." The travel withdrawal was partly spontaneous and partly elite-cued: in his February 1 national address the prime minister urged Canadians to "choose Canada," and premiers echoed the call as the first delistings were announced.

a dose too small against a capital-intensive sector's employment base to register in headcount. The local-labor-market evidence that follows is therefore about the travel withdrawal: whether its cost is visible in U.S. local labor markets, and where.

3 Blowback as a cost on the coercer

The economic-coercion literature gives us the vocabulary for the coercer's side of this exchange and little for the target's decentralized response. Farrell and Newman (2019), extending an asymmetric-interdependence tradition that runs from Hirschman (1945) through Keohane and Nye (1977), show how hub states exploit dominant positions in global economic networks to surveil and to cut off adversaries—the panopticon and chokepoint effects of weaponized interdependence. That logic runs from the hub outward; the episode studied here runs in the opposite direction, and through no chokepoint at all. The asymmetric dependence that gives the hub its leverage is the same dependence that concentrates the target public's custom in particular hub communities, and when the hub coerces, an uncoordinated response by that public can route a cost back through the hub's own non-tradable service economy. Blowback—the return flow that is the complement to chokepoint power—is one the coercion ledger acknowledges but has rarely measured at this resolution.

Drezner (1999) emphasizes that coercion imposes costs on the sender as well as the target, and his conflict-expectations argument makes the ally case distinctive: senders rarely initiate coercion against allies, where expectations of future conflict are low, which is part of what made the 2025 episode unusual. Statecraft analysis scores sender-side costs, when it scores them at all, at the level of the coercer's economy as a whole.[7] We localize one such sender-side cost—which arrives through the target public's redirection of its own consumption rather than through the sanctioned good—measure it at high frequency and fine spatial resolution, and extend the relevant actor set from the state to consumers and provinces. Sender-side costs enter the ledger, where they enter at all, as national aggregates: the trade foregone by sanctioning firms and their banks (Crozet and Hinz, 2020; Besedeš et al., 2017; Hufbauer et al., 2007), not the employment of particular sender communities. The incidence of the tariffs the two countries exchanged is by now well measured (Amiti et al., 2019; Fajgelbaum et al., 2020); the consumer boycott that ran alongside them is not.

The closest empirical precedent sits in trade rather than in the sanctions literature. Ahn et al. (2022) study the 2019 Korean boycott of travel to Japan as a regional incidence shock and find large losses concentrated where Japanese regions had depended most on Korean visitors, with a follow-on analysis of the China–Japan case (Greaney and Kiyota, 2026). Our design belongs to that family, with two differences: (i) the outcome is on the coercer's side rather than the target's; and (ii) we decompose the incidence by travel mode, a margin whose responsiveness to cost and distance has its own literature on cross-border travel (Chandra et al., 2014). The broader boycott canon studies trade flows, brand share, or stock returns (Heilmann, 2016; Pandya and Venkatesan, 2016; Michaels and Zhi, 2010; Fuchs and Klann, 2013; Fisman et al., 2014); none of it measures local-labor incidence on the coercer or differentiates travel mode. On the target's side, the withdrawal is an episode of political consumerism driven by consumer animosity (Micheletti,

[7] Baldwin (1985) established the analytical frame for economic statecraft; the injunction to judge an instrument against the costs and efficacy of its alternatives makes the sender-side cost measured here a missing input to the standard ledger.

2003; Stolle and Micheletti, 2013; Klein et al., 1998; Shimp and Sharma, 1987). Our framework yields three predictions that structure the empirical work. First, incidence should concentrate where the target's spending was and vary by mode: Canadian spending is a large share of the land day-trip economy and marginal at major air destinations. Second, if the response is the target public's, rooted in the sovereignty provocation, it should track the provocation rather than the tariff schedule, and it should not unwind when the formal instrument is withdrawn, since no bargaining counterparty controls it. Consumer boycotts are typically brief where detectable at all (Chavis and Leslie, 2009; Ashenfelter et al., 2007; Heilmann, 2016), making durability a predicted anomaly; durability alone, however, is shared by a persistent U.S.-side deterrent, so the timing of onset carries the discriminating weight (Section 7). Third, domestic replacement can offset losses where demand is deep, so the same national shock can leave a sharp local imprint at the isolated border crossing and none at the large air hub. The timing evidence of Section 2, the cross-margin breadth of Section 2.B, and the persistence test of Section 6 take the three in turn.

4 Data

4.A Cross-border-visitor exposure

Central to our analysis is the Bureau of Transportation Statistics Border Crossing series, which records inbound personal-vehicle passenger counts at every U.S.–Canada land port. Two features make it the natural exposure base. First, the counts are pre-onset (2023–24), so the shares that weight our measures are lagged while the outcome is read off the timely employment series. Second, the land ports isolate the day-trip margin, holding it apart from the air and snowbird margin that a pooled visitor share would blur. We construct three measures, two for the land day-trip margin and one for the air margin.

Land-crossing volume is the headline land measure. Each U.S.–Canada crossing's 2024 inbound personal-vehicle passenger count (Bureau of Transportation Statistics Border Crossing data, 89 ports with coordinates) is distance-allocated to nearby metros with gravity weights $w_{mp} = \exp(-d_{mp}/\tau)$, $\tau = 150$ km, and the metro index $\sum_p w_{mp}\,\mathrm{vol}_p$ is logged and z-scored across metros. The most exposed metros are Buffalo, Detroit, and Bellingham.

Border proximity is the great-circle distance from each metro centroid to the nearest crossing, converted to a decay term $\exp(-\mathrm{dist}/\tau)$, $\tau = 200$ km. It is continuous over all metros and is the classic distance-decay proxy for day-trip dependence.

Air-tourism destination share comes from the National Travel and Tourism Office's Survey of International Air Travelers, the 2024 Canadian-air destination shares mapped to metro codes. It captures the air and snowbird segment that the land measures miss; the leading metros are Las Vegas (13.6 percent), New York, Los Angeles, Miami, and Orlando. Keeping this measure separate from the land measures is what distinguishes the design from a pooled visitor-exposure share, and it is what lets us estimate the land-versus-air contrast directly. Because a survey share carries measurement error that would attenuate an air coefficient toward zero, we corroborate it with an administrative exposure built from BTS T-100 segment data, each metro's 2024 Canada-to-U.S. passengers at its airports (Online Appendix I.H).

All three measures are z-scored over the metro cross-section, so each coefficient reads as the change in the outcome associated with a one-standard-deviation increase in exposure.

The identifying geography is mapped in Online Appendix Figure A5. Canadian land-crossing exposure traces the northern tier, concentrated at Buffalo, Detroit, and Bellingham; Canadian air-destination exposure sits in the Sun Belt; and the Mexican land-crossing exposure that anchors the placebo of Section 7 traces the southern border at San Diego, El Paso, Laredo, and the lower Rio Grande Valley.

4.B Outcomes, sample, and period

The primary outcome is log leisure and hospitality employment from the metro Current Employment Statistics series. Metropolitan leisure and hospitality is a broad aggregate, most of it unrelated to cross-border day-trips even in the border metros, so a per-standard-deviation gradient on the whole-metro series is a demanding object; the within-metro tourism-versus-nontourism triple-difference below is the check that the movement is specific to the tourism margin rather than a metro-wide swing. For the finer cross-border-retail tests we use county QCEW employment in five tourism-adjacent sub-sectors: accommodation, food services, arts and recreation, grocery stores, and gas stations. The metro panel covers 15,990 metro-months across 390 metros over the forty-one months from January 2023 through May 2026. Of the 390 metros, ten form the legacy binary border set (nine distinct metros; Detroit enters as both a metropolitan statistical area and a metropolitan division) and 46 carry a positive air-destination share; the continuous-exposure specifications estimate on the 14,432 metro-months (352 metros) with non-missing employment and exposure. The exclusion loses no distinct geography.[8] The border-county panel covers 649 counties in the thirteen border states plus Alaska.[9]

5 Empirical design

Onset is dated identically in every specification, with post = 1 from 2025-03 onward, the month of the first broad wave of delistings and the visible start of the travel pullback.[10] To estimate the

[8]The 38 metros dropped for want of a centroid are all but one metropolitan divisions, subdivisions of the largest metropolitan areas whose parent areas remain in the sample; the dropped units are larger, not smaller, than the average metro. Detroit's double-listing therefore affects only the binary set, and in the continuous sample Detroit enters once, as its metropolitan statistical area (dropping it leaves the headline gradient at −0.58%).

[9]Every input is public: Bureau of Transportation Statistics Border Crossing data, BLS Current Employment Statistics and the Quarterly Census of Employment and Wages, the NTTO Survey of International Air Travelers and I-94 series, BTS T-100 international and domestic segment data, Census import records (the realized tariff rates), Transportation Security Administration throughput, and Statistics Canada Table 24-10-0053. A replication package—exposure construction, the estimation panel, and the small-cluster and spatial inference routines—will accompany the published version.

[10]The result does not depend on this date. Re-estimating with onset moved to December 2024, January, February, or April 2025 leaves the deseasonalized land-volume gradient between −0.51% and −0.56%—a January onset gives −0.53% (permutation $p < 0.001$), against the −0.55% March baseline. On the raw series an earlier onset mechanically deepens the gradient by drawing more winter months into the post window, which is the cold-month border seasonality the STL adjustment removes, so we read the onset check on the deseasonalized series.

employment response to boycott exposure, we adopt a two-way fixed-effects difference-in-differences,

$$y_{mt} = \beta \left(\text{exposure}^{z}_{m} \times \text{post}_{t}\right) + \alpha_m + \delta_t + \varepsilon_{mt}, \tag{1}$$

with metro fixed effects α_m and month-of-sample fixed effects δ_t. The coefficient β is the per-standard-deviation exposure gradient on employment after onset. The key identifying assumption is that absent the boycott, employment trends across metro areas would not have varied systematically with their 2024 Canadian land-crossing exposure. The headline gradient rests on this two-way design; the inference challenge is spatial. Because the high-exposure metros all lie on the northern tier, exposure is spatially clustered and the effective number of independent draws is smaller than the metro count, so cluster-robust standard errors over-reject and the many-shifter shift-share inference of Adão et al. (2019) does not apply directly. The main tables report two inference methods for each headline term: CRV1 cluster-robust standard errors and permutation inference—a design-based randomization test that reassigns the static exposure across the cross-section and invokes no large-cluster asymptotics. Because the dependence is spatial, the tests built for it carry the most weight; Online Appendix I.A confirms the land-volume gradient under Conley spatial-HAC standard errors, state-level and spatially blocked wild-cluster and permutation inference, with the low-powered nine-division cut reported but not leaned on (MacKinnon and Webb, 2018). The binary design, which reduces to ten treated units, makes the small-sample problem acute and returns a null. Online Appendix Figure A4 displays the three randomization distributions: the land-volume coefficient falls outside all 2,000 placebo assignments, while the air and southern-border coefficients sit near the center of theirs.

Two limitations bear stating at the outset. The boycott is not randomly assigned across places, so the design recovers an association rather than an experiment. And the metro series are not seasonally adjusted, while cold-weather border metros have a differential seasonal amplitude that month-of-sample fixed effects only partly remove. We therefore lean on the fixed-effects point estimate together with permutation and bootstrap inference, we confirm in Section 7 that the result survives explicit seasonal adjustment, and we set aside the dynamic event-study path, whose joint pre-trend test rejects on the non-adjusted series.

6 The land-border decline

Table 1 reports the core estimates; the headline is the land-crossing-volume gradient. A one-standard-deviation increase in distance-allocated land-crossing volume is associated with a 0.57% decline in leisure and hospitality employment after onset, significant across inference methods. Section 9 translates this gradient into a job count. Figure 2 plots the cross-section behind the coefficient, each metro's post-onset change in log leisure and hospitality employment against its predetermined exposure; because the panel is balanced, the fitted slope is identically the two-way fixed-effects estimate. Border proximity yields the same pattern one step removed from the crossing, at -0.39% per standard deviation. A legacy binary border indicator is right-signed but insignificant under every method, its permutation distribution roughly six times as wide: concentrated blowback is detectable, but only through the continuous predetermined gradient, and a binary design on the same data returns a null.

Two further cuts sharpen the geography and guard against a metro-wide confound. The most demanding—and the one design that removes the metro-wide converge-then-break—is a triple-difference that nets out every metro-wide shock through metro×month fixed effects, so that identification comes only from the differential movement of tourism-adjacent sectors against other sectors inside the same metro and month. Its construction stacks two outcomes per metro-month, leisure and hospitality against the rest of nonfarm employment, and interacts land exposure with a leisure-and-hospitality indicator and post; metro-by-sector, metro-by-month, and sector-by-month fixed effects then leave the land-exposure×tourism×post coefficient identified purely within a metro and month. It leaves a tourism-specific land effect that is correctly signed and significant, −0.30% per standard deviation (permutation $p = 0.046$; cluster-robust and wild-cluster-restricted $p = 0.072$ and 0.077), while general tourism dependence in the baseline two-way design runs positive (+0.37%, $p = 0.002$). This specification absorbs any local manufacturing recession, any metro-wide demand shock, any metro-specific linear trend, and any place-specific seasonal pattern. It is a within-metro check that the decline is specific to Canada-border exposure, not to tourism dependence in general. We read its −0.30% as the conservative floor beneath the cross-sectional −0.57%. A finer county design by tourism-adjacent sub-sector adds nothing: once confidentiality-suppressed QCEW cells are coded as missing rather than as zeros, the cross-border-retail gradients near the crossings are small and insignificant, so we do not lean on it.[11] Synthetic controls for the four most day-trip-dependent metros give a mixed but mechanism-consistent picture rather than uniform corroboration. Bellingham, which depends heavily on Vancouver day-trippers and sits far from replacement demand, diverges clearly below its synthetic (−2.7 percent post-onset against a pre-period fit of 0.012 RMSE). The higher-market-potential Buffalo and Burlington show no gap; Buffalo in fact sits slightly above its synthetic. Detroit's exact pre-period fit (RMSE near zero across 335 donors) marks its series as overfit, so we do not read its gap as informative. The synthetic evidence therefore supports the market-potential mechanism of Section 8, not the level gradient metro by metro.

The decline is more consistent with an inward shift in tourism labor demand than with a withdrawal of labor supply, though the data are only suggestive: average weekly wages in the tourism sub-sectors do not rise with land-crossing exposure and establishment counts are flat, consistent with falling demand rather than a supply withdrawal that would bid wages up. Two caveats keep this a reading rather than a finding: average weekly wages confound the price of labor with its composition, and flat sub-annual establishment counts are weak evidence against firm exit. Hours per worker—the margin a demand shock moves first—are absent from the QCEW.[12]

Beyond the level effect, the persistence pattern bears on the sentiment-versus-price reading, though it separates the two only partially. The gradient has outlasted the tariffs that accompanied the boycott. On February 20, 2026 the Supreme Court struck down the IEEPA tariffs (*Learning Resources, Inc. v. Trump*), removing the statutory 35 percent rate on non-USMCA Canadian goods; a replacement 10 percent tariff under Section 122, from which USMCA-compliant goods were

[11]Coding suppressed cells as zeros would fabricate large negative sub-sector swings and a spurious gradient. The metro leisure-and-hospitality headline is unaffected: it is built from the published CES metro series, not the suppression-prone county cells.

[12]In a county difference-in-differences (county and quarter fixed effects), the log-wage gradient on land-crossing exposure is null to slightly negative and the log-establishment gradient is flat, consistent with demand rather than supply. The Quarterly Workforce Indicators worker-flow test is too coarse (eight to eleven border-state clusters, one post-quarter) to separate a hiring freeze from elevated separations, though it shows no separation spike.

exempt, took effect four days later. The realized tariff rate on Canadian imports fell by roughly one percentage point because USMCA-compliant trade was exempt throughout, and the reset is therefore a salient statutory and political reversal rather than a large realized-price change—and a muddied one, since a new instrument arrived the same week. Splitting the post period at that reset, the deseasonalized land-volume gradient is -0.50% per standard deviation over the boycott-under-tariffs window and -0.74% in the first three post-reset months, both permutation $p \leq 0.001$.[13] The traffic series show the same pattern: from March through May 2026 northern land crossings were still 20 percent below the same months of 2024, essentially the boycott-year gap (though about nine percent above the collapsed 2025 months, a partial rebound), and Canadian automobile trips a third below theirs, while overseas trips remained elevated. The employment gradient deepened over months in which crossings partially rebounded against 2025, a divergence consistent either with lagged adjustment or with the residual differential trend discussed in Section 7. Two readings fit: a sentiment shock that outlasted its trigger, or a still-deepening path the reversal did not arrest (the event-study gradient was already widening into the reset, Online Appendix Figure A1). One asymmetry favors reading the non-recovery as informative: land day-trips are the fastest-adjusting travel margin, with no booking lag, so if any margin could have responded to a salient reversal within three months it is this one. The realized price shock on Canadian goods was in any case modest in both directions, rising from 0.1 percent to a peak of 3.8 and settling near 3.2, so a travel collapse of a quarter was disproportionate to realized prices on the way in and unresponsive to them on the way out.

7 Distinguishing the boycott from coincident shocks

A decline near the crossings is consistent with the boycott but also with the tariffs, a weaker loonie, softer Canadian incomes, a local manufacturing downturn, or seasonality. This section tests each alternative in turn. None of these coincident shocks survives. One caveat frames the rest: what the checks isolate is a redirection of Canadian travel away from the United States specifically. The organized "Buy Canadian" boycott is its leading proximate cause, but our design does not separate it from other U.S.-specific deterrents—border-crossing apprehension, say—that moved the same way at the same time. We use "boycott" for the redirection and its incidence, not as a claim to have isolated one motive among several pointing in the same direction. Three observations bear on the motive mix without settling it. The timing favors the provocation at onset. The redirection gap opens in February 2025, the month of the executive order and after three months of annexation rhetoric, before the enforcement measures most salient to Canadian travelers, whose news arrived from mid-March onward. The gap's subsequent deepening overlaps with that news, so the alignment dates the onset rather than the full depth.[14] And across the 38 arrival origins of Online Appendix I.D, the two declines larger than Canada's belong to Denmark, the other origin whose sovereignty was publicly questioned in the same months, and Venezuela, an origin with

[13] Three months is a short window; the raw-series split is steeper but carries spring seasonality, so we quote the deseasonalized figures.

[14] The registration requirement for stays of thirty days or more was published as a Department of Homeland Security interim final rule on March 12, 2025 and took effect April 11, 2025; the most widely covered detention case, a Canadian national held twelve days from March 3, 2025, drew national Canadian coverage in mid-March. The gap is -0.01 log points in January 2025 and -0.18 in February (Figure 1).

high enforcement salience. Origin-specific withdrawals of Canada's size arise under both channels, which is why we claim the redirection and its incidence rather than the motive. The third is the withdrawal's purpose selectivity (Section 2). Leisure visits fell about 26 percent while visits to friends and relatives fell 12 percent with their spending held, a pattern natural for a discretionary consumer boycott and less natural for an enforcement deterrent, which would weigh at least as heavily on the family visits that continued. The observation is suggestive rather than decisive, since we cannot observe how the two trip purposes price border risk.

Income and exchange-rate effects. A weaker loonie and a softer Canadian economy would also have reduced travel to the United States, and on the U.S. incidence side that channel is hard to separate from the boycott. Two features of the data separate them. The first is on the Canadian side: as Figure 1 shows, Canadian trips to the United States fell sharply over 2025 while Canadian trips overseas rose—an income shock or a broad depreciation reduces all foreign travel, while the observed pattern is a redirection away from the United States specifically. The second addresses the one version redirection alone cannot rule out—a *bilateral* Canadian-dollar move that raises the price of U.S. trips relative to other destinations, the day-trip being the most exchange-rate-sensitive travel margin (Chandra et al., 2014; Campbell and Lapham, 2004). That move was too small and too brief. The loonie depreciated about 4.8 percent against the U.S. dollar at onset but only 1.9 percent against the euro, and it recovered to its 2024 level by early 2026. At the day-trip elasticities of Chandra et al. (2014), the move could account for only low-single-digit percentage points of the observed 23.7 percent crossing decline, which persisted after the currency had recovered (Online Appendix I.C, Figure A3).

A Canada-specific land withdrawal. A distinct alternative is that 2025 saw a broad turn away from U.S. travel, not specific to Canada, to which the Canadian pullback merely belonged. Arrivals by country of residence discriminate. Canada's all-mode arrivals fell 20.2 percent, the third-largest drop among 38 origins and six times the overseas mean, but a broad, shallow softening of inbound *air* travel is present too, and Canada's air arrivals (down 11.4 percent) sit inside that band. What is Canada-specific is the *land* withdrawal, the day-trip channel no overseas origin has, and because the employment gradient is identified off land-crossing exposure the broader air softening does not contaminate it; it registers instead as the air null of Section 8 (Online Appendix I.D). This does not separate the boycott from a U.S.-specific land deterrent such as enforcement apprehension, consistent with the caveat above.

The southern-border placebo. Mexico was tariffed on the same 2025 schedule. It faced no annexation rhetoric and no comparable "don't travel" boycott. We build the identical distance-allocated crossing-volume and proximity exposure for the 28 Mexican land ports and run the identical specification on the same panel and onset. If tariffs-as-border-friction drove the northern result, both borders should show it. They do not (Table 2; Figure 2, Panel B). The southern-border gradient is a clean zero, slightly positive and insignificant under every inference method, while the northern gradient is sharply negative. The comparison rests on a premise worth checking, that southern crossings did not themselves collapse for unrelated reasons, and they did not: southern personal-vehicle passengers were within two percent of their prior-year level over the post-onset window and pedestrian crossings rose, against a 23.7 percent decline on the northern border. The placebo therefore compares a border that kept its visitors to one that lost a quarter of them. That contrast is the strongest single piece of evidence that the blowback is a U.S.-specific travel redirection, not the tariff friction the two borders shared.

Three further alternatives the data also reject. A regional growth pattern, the metros' own tariff shock, and seasonality could each in principle generate a border gradient. None does. Because these tests are estimation detail rather than identifying comparisons, we state the results here and report them in full in Online Appendix I. First, the regional-growth concern: forcing identification within Census division by month—comparing each border metro only with others in its region and month—retains three-quarters of the land-volume gradient, −0.44% per standard deviation (permutation $p = 0.002$). Second, the metros' own tariff shock: adding each metro's predetermined tariff exposure and manufacturing share moves the land estimate from −0.57% to −0.71% rather than toward zero, and dropping any single border metro, Detroit included, leaves it between −0.59% and −0.55%. Third, seasonality: deseasonalizing leisure and hospitality employment metro by metro with an STL decomposition leaves the gradient at −0.55% ($p < 0.001$), if anything slightly larger than the unadjusted estimate. The air gradient stays nonnegative throughout.

What the estimate can claim. The seasonal-adjustment limitation flagged in Section 5 bears on the dynamic path. That path shows the converge-then-break pattern familiar from import-competing geographies: high-exposure metros sit on a relatively higher, declining path before onset. The average post-onset effect is therefore sensitive to a relative-magnitudes pre-trend violation, which a Rambachan–Roth sensitivity confirms (Online Appendix I; Online Appendix Figure A1). We do not read the headline cross-sectional gradient as free of this concern, since it rests on the same predetermined exposure variation as the event study. The corroboration that escapes the metro-wide trend concern comes, strictly, from one design: the within-metro triple-difference, which nets out every metro-wide shock and identifies only off the differential movement of tourism-adjacent sectors within a metro and month (Section 6). Its pre-trend record is stronger than the level design's, and Online Appendix I.F sets it out in full. The immediate pre-onset leads are flat under design-based tests, and the distant leads drift in the direction that biases the estimate toward zero; a sign-restricted Rambachan–Roth bound survives late-horizon violations up to three-quarters the size of the largest pre-period violation. Letting the tourism-versus-rest trend vary freely by census division leaves the triple-difference at −0.32% (permutation $p = 0.035$), and the same design run on the southern border, where crossings did not decline, returns an insignificant −0.11%. The deseasonalized in-time placebo at a counterfactual March-2024 onset returns a small, insignificant gradient, about a quarter of the headline, consistent with modest pre-onset drift; netting that drift from the headline would leave a gradient nearer −0.4%. The southern-border placebo and the Canadian-side redirection are unaffected by the trend concern. Table 3 widens this corroboration to every data source the episode touches. The same year-over-year difference-in-differences, applied to travel volumes, trip intensities, dollars (including the BEA-measured U.S. side of the flow), and administrative local tax outcomes across five independent statistical systems, returns the same sign pattern. The rows that do not move, New York border-county tax collections and the air margins, are the ones the incidence geography predicts should not. We therefore read the −0.57% gradient as a descriptive association whose sign and order of magnitude these checks support, treat the triple-difference (-0.30%) as its conservative floor, and treat the implied job counts of Section 9 as an upper bound. Border proximity, the secondary land measure, does not survive the within-division cut, so we use it as corroboration only and do not let it anchor magnitudes.

8 Travel mode and the size of the dose

The land gradient maps where the cost fell; the air null maps where it did not, and the dose mechanism explains why. The dollars agree with the map: allocating the measured decline in Canadian spending across travel modes, the automobile margins carry about four-fifths of it and air at most a fifth (Online Appendix I.K). A pooled Canadian-visitor exposure measure mixes land day-trip economies with air destinations, and the two behave very differently. Pooling the land-volume and air-destination measures into a single exposure index returns a gradient of -0.36% per standard deviation (permutation $p = 0.015$): the land loss of -0.57% averaged against the air null of $+0.09\%$. An analyst working with the combined measure would recover this muted, partly cancelled effect, missing both the concentration at the crossings and the absent imprint at the air destinations.

The air-destination gradient is statistically indistinguishable from zero: in Table 1 it is $+0.09\%$ per standard deviation with a permutation p of 0.52. Las Vegas, Orlando, Miami, and the Sun-Belt air destinations did not lose leisure and hospitality employment in proportion to their Canadian-air exposure, even though Canadian air arrivals fell 12.3 percent nationally.[15] The mode contrast is the sharpest descriptive result in the paper. Once the dose is measured, the air null is close to arithmetic: the air leg's job is to measure the dose, and the zero is what that dose implies. Pooling both exposures and testing the difference directly, the land coefficient is distinguishable from the air at $t = -4.1$; the air coefficient's 95% confidence interval, $[-0.08\%, +0.14\%]$, rejects that air destinations lost as much as the land point estimate. Nor is the null an artifact of measurement or thin support: rebuilt from administrative T-100 passenger data the air gradient is $+0.15\%$ per standard deviation (permutation $p = 0.31$) with the difference unchanged ($t = -4.3$), and winsorizing or dropping Las Vegas, or estimating only on the 46 positive-exposure metros, leaves it small, positive, and insignificant (Online Appendix I.H).

Destination-level passenger data explain why a real withdrawal left no employment imprint. The explanation is primarily dilution rather than active substitution (Figure 3), for three reasons. The decline in Canadian passengers was unrelated to a destination's Canadian share of arrivals. That share was a few percent at the large gateways and twelve percent at Palm Springs, the most dependent major destination. And the implied loss of total arrivals, a few tenths of a percentage point to about one, was an order of magnitude too small to register in metropolitan employment against ordinary movements in the domestic base (Online Appendix I.H).

This land-versus-air split is a special case of a more general pattern: a boycott bites where the target's spending is a large share of local demand and replacement demand is thin, and dissipates

[15] This is the 2024-to-2025 change in NTTO I-94 air arrivals. Measured instead against the 2023–24 average, as in Kurmann et al. (2026), the air decline is larger, on the order of a quarter, because 2023 air travel was still elevated in the post-pandemic rebound; the argument here is unaffected by the baseline, since even the larger decline removes only about one percent of the exposed metros' arrivals.

where it is a small share of a deep market. Interacting land exposure with domestic market potential makes this margin continuous: the gradient is steep in isolated border corners and flat in metros embedded in dense domestic markets (Online Appendix I.I).

9 Magnitude and incidence

The blowback is small in national aggregate and concentrated locally, the incidence pattern the framework of Section 3 predicts. Applying the estimated land gradients to baseline employment across the exposed metros implies a gross local loss in the tens of thousands of leisure and hospitality jobs. Two constructions discipline the range. A linear extrapolation of the volume gradient gives roughly 31,600; a second construction, cumulating the realized loss of crossings over metros at an instrumental-variables response—a reparameterization into visits units rather than fully independent evidence (Online Appendix I.J)—gives roughly 12,500 (95 percent confidence interval 1,500–23,500). We quote the span as on the order of ten to thirty thousand, centered nearer the instrumented figure than the extrapolated ceiling, and, per Section 7, read the whole range as an upper bound (Online Appendix I.J gives the constructions, including a proximity-based one we no longer lean on). These figures sit inside the 13,900-to-42,100 range that Kurmann et al. (2026) report, a loose check, since their outcome set is wider, covering small-establishment retail as well as leisure and hospitality, from an independent design and exposure measure. A spending-side benchmark from Canadian survey data, with no employment estimate in it, lands on the same range: the measured C$2.95 billion decline in Canadian spending in the United States over 2025Q2–Q4, run through the local-consumption share and a leisure and hospitality labor share, implies roughly 17,000 to 31,000 national job-equivalents (Online Appendix I.K). Read per lost visit, the constructions imply one leisure-and-hospitality job for every three hundred to eight hundred Canadian land trips that did not happen, with the instrumented end, one per roughly eight hundred, the better benchmarked against visitor-spending-per-job norms (Faber and Gaubert, 2019): the one-per-three-hundred end would require each foregone day-trip to have carried several times the leisure-and-hospitality payroll that plausible day-trip spending delivers.

Those figures describe a gross local incidence, and four distinctions keep them from being read as more than they are. First, the count is gross rather than net. The same Canadian withdrawal that thinned traffic at the land crossings left no measurable loss at the air destinations (Section 8), and whatever domestic spending filled the gaps was redirected from elsewhere in the United States, so the national change in service-sector employment is smaller than the loss concentrated at the border. Second, it is a headcount and not a welfare cost; the welfare loss is the friction of reallocating displaced workers plus the foregone gains from trips that did not happen, neither of which a job count captures. Third, it covers one channel in one set of places, the land day-trip economy, counting only leisure and hospitality jobs directly and not the local-multiplier spillovers onto other nearby employment (Moretti, 2010), and it is silent on whatever the boycott did through other margins. Fourth, the counts inherit the upper-bound status of Section 7: the pre-onset drift qualifies every count built on the gradient, and domestic spending displaced toward control metros pushes the relative gradient away from zero.

Valued at the sector's wage, the local loss is on the order of several hundred million to a billion dollars a year—a concentrated, place-specific service-sector cost of tens of thousands of jobs, the kind the coercion literature has long posited and rarely priced.[16]

Beyond their aggregate size, the job losses have a political geography that distinguishes them from prior episodes of trade retaliation. The distributional politics run opposite to the more familiar tariff-retaliation case. The 2018–19 agricultural retaliation fell on Republican-leaning farm counties, a targeting of the coercer's electoral map that state retaliators chose deliberately (Fetzer and Schwarz, 2021; Kim and Margalit, 2021) and that cost the incumbent party votes (Blanchard et al., 2024); a broader literature ties trade-driven economic shocks to nationalist and anti-incumbent voting (Autor et al., 2020; Colantone and Stanig, 2018). That work studies state-directed retaliation and import competition; the 2025 travel boycott, which no one aimed, reached a different electoral map. Merging each metro's land-crossing exposure to county returns from the 2024 presidential election, exposure is negatively correlated with the Republican two-party vote share across metros (Pearson -0.17, $p < 0.01$; Spearman -0.18). The most exposed decile of metros cast 50.5 percent of its two-party vote for the Republican candidate, against 55.6 percent in less-exposed metros and the pronounced Republican tilt of the farm counties that absorbed the earlier retaliation.[17] The three most exposed metros—Buffalo, Detroit, and Bellingham—all lean Democratic, a contrast Kurmann et al. (2026) also note. The supported statement is comparative: a consumer travel boycott and sub-national delisting reach a different, less governing-party-aligned map than state-to-state tariff retaliation, and a coercer weighing the domestic costs of a confrontation should expect them to concentrate in a different set of places. That map does not obviously carry less political weight: the exposed metros are concentrated in presidential battleground states (36 percent of the top-exposure decile against 19 percent of the rest, driven by the Michigan crossings), and because localized economic shocks move votes (Autor et al., 2020; Colantone and Stanig, 2018; Margalit, 2011), a cost falling on swing-state metros need not lack electoral weight. Whether these communities translate loss into pressure on the administration is a transmission question this cross-tabulation does not answer.

10 Conclusion

We began by asking whether the U.S. coercion of Canada in 2025 generated a return flow of cost with a measurable local incidence inside the United States. It did: part of the cost returned through a sharp decline in Canadian travel and fell on land-border communities. We tie that blowback to the U.S.–Canada episode through a southern-border placebo and a battery of checks that weigh against the tariffs, the metro's own shock, regional growth patterns, and seasonality. The cost

[16] We value the employment loss at a representative leisure and hospitality annual wage in the \$25,000 to \$35,000 range (BLS Quarterly Census of Employment and Wages, 2024). A national leisure and hospitality average wage is not cleanly published at the level we need, so the dollar figure is a range and the job count does not depend on it. For scale, the implied earnings loss is on the order of three to eleven cents per dollar of the roughly \$9.9 billion in tariff revenue the United States collected on Canadian goods between March and December 2025—a size benchmark, not a welfare ratio, since that revenue is a transfer to the U.S. Treasury rather than a gain to the coercer.

[17] County presidential returns for 2024 (McGovern, 2024), aggregated to core-based statistical areas with the Census Bureau's July 2023 delineation and merged to the land-crossing exposure measure; 347 metros in the estimation sample (the correlation is -0.18 over all 386 merged metros). The returns reproduce the certified national two-party split (50.8 percent Republican) to within a tenth of a point. This is a descriptive cross-tabulation, not a statement about how exposure affected voting.

is travel-mode-specific and geographically concentrated: leisure and hospitality employment fell with land-crossing exposure, while air-served Sun-Belt destinations—where Canadian traffic was too small a share of demand to register—were spared. The gross local loss is at most on the order of ten to thirty thousand jobs in exposed communities: one leisure-and-hospitality job for every three hundred to eight hundred foregone trips, and several hundred million to a billion dollars a year in earnings, gross rather than net (Section 9). That cost also persisted beyond the tariffs. Their judicial reversal in February 2026 brought back neither the travel nor the jobs in the months our window covers.

We price this cost without adjudicating whether it changed U.S. conduct; a single observational episode cannot settle that. Whether the cost reaches the administration as political pressure is the transmission question these data leave open.

Three properties of this episode generalize, as properties of the measured redirection rather than of a single identified motive (Section 7). The sender-side cost arrived through no chokepoint and no formal state retaliation, and it did not ease when the tariffs were reversed. Incidence concentrates where the target's spending was, bounded by the depth of local replacement demand, so the mode-specific map rather than the national aggregate is what a coercer needs. And the travel withdrawal reached a different electoral geography than state-directed tariff retaliation, landing on communities a governing coalition may not have in view.

The design here—a predetermined exposure gradient, continuous and decomposed by travel mode, paired with a second border for falsification and inference built for few treated units—should carry to the next confrontation. It remains to be seen whether the return flow there will be as easy to trace.

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Table 1. Continuous-Exposure Difference-in-Differences, Leisure and Hospitality Employment

Exposure (+1 SD $\times$ post)	Coef. (%)/(SE)	CRV1 p	Perm. p
Land-crossing volume (BTS)	-0.57	< 0.001	0.001
	(0.15)		
Border proximity	-0.39	0.004	0.011
	(0.14)		
Air-destination share	$+0.09$	0.085	0.520
	(0.05)		
Binary border indicator	-0.86	0.153	0.309
	(0.60)		

Notes: The dependent variable is log metropolitan leisure and hospitality employment; each coefficient is the per-standard-deviation exposure gradient interacted with the post-March-2025 indicator, from a separate two-way (metro and month-of-sample) fixed-effects regression, in percent of baseline leisure and hospitality employment. The three continuous-exposure rows estimate on n = 14,432 metro-months; the binary-border row uses n = 15,990 and the ten contiguous land-border metros (nine distinct, Detroit double-listed). Cluster-robust (metro) standard errors are in parentheses; CRV1 is the cluster-robust p-value and Perm. is permutation inference (2,000 reassignments). Because cluster-robust inference over-rejects here (Section 5), permutation is the preferred read where the two disagree, and Table A1 reports the wild-cluster and spatially clustered confirmations. All estimates are observational.
Sources: BLS Current Employment Statistics; Bureau of Transportation Statistics; U.S. NTTO; author's calculations.

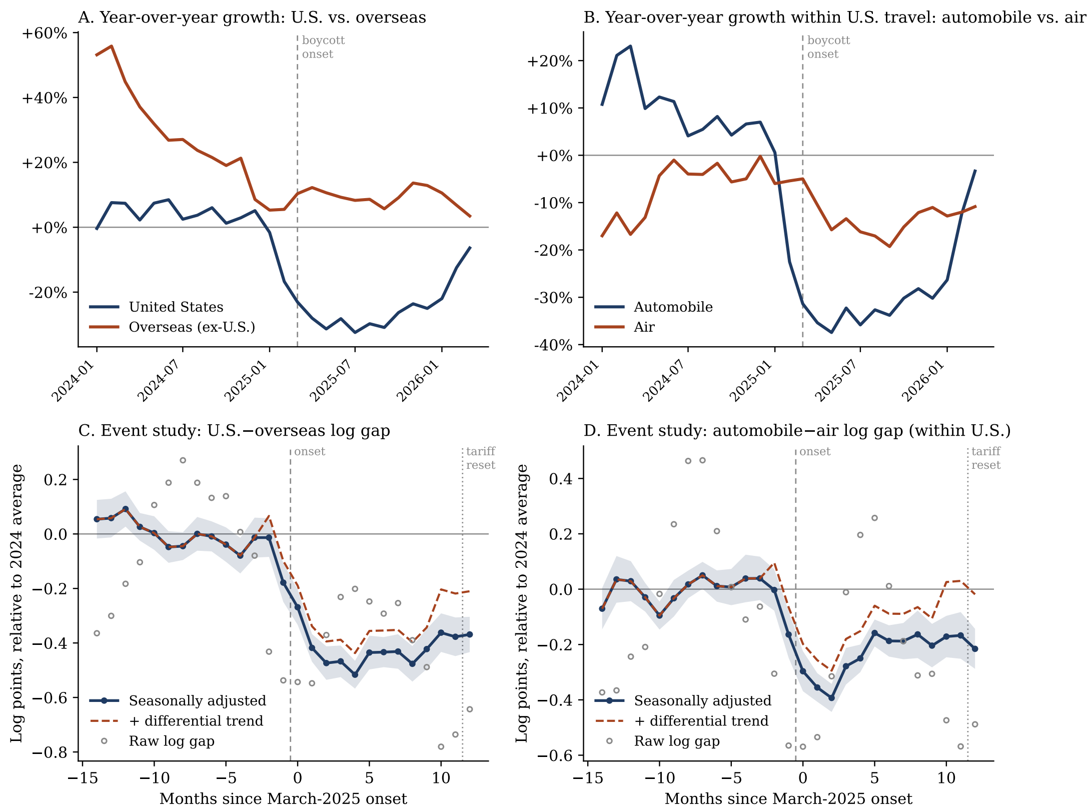

Figure 1. The Canadian Pullback Was Specific to the United States

Notes: Canadian-resident return trips from Statistics Canada frontier counts (Table 24-10-0053, monthly, not seasonally adjusted). Panels A and B plot year-over-year growth of trips returning from the United States against overseas destinations, and, within U.S. travel, automobile against air. Panels C and D estimate the same contrasts as event studies: the log gap is regressed on month dummies (January 2023 through March 2026; the panels display coefficients from January 2024 on), calendar-month effects, and, in the dashed variant, a differential linear trend, with 2013–2019 as baseline and 2020–2022 excluded; coefficients are centered on the 2024 average and bands are 95% Newey–West (12-lag) intervals (Newey and West, 1987). The 2024 leads stay within ± 0.10 log points of zero; a Wald test rejects strict flatness within them ($p = 0.03$ Panel C, $p < 0.001$ Panel D), but the drift is small relative to the post-onset break and, for automobile–air, opposite in sign. The gap breaks at the February 2025 executive order and stays depressed through the February 2026 reset (dotted line); open markers show the raw gap. *Sources:* Statistics Canada Table 24-10-0053; author's calculations.

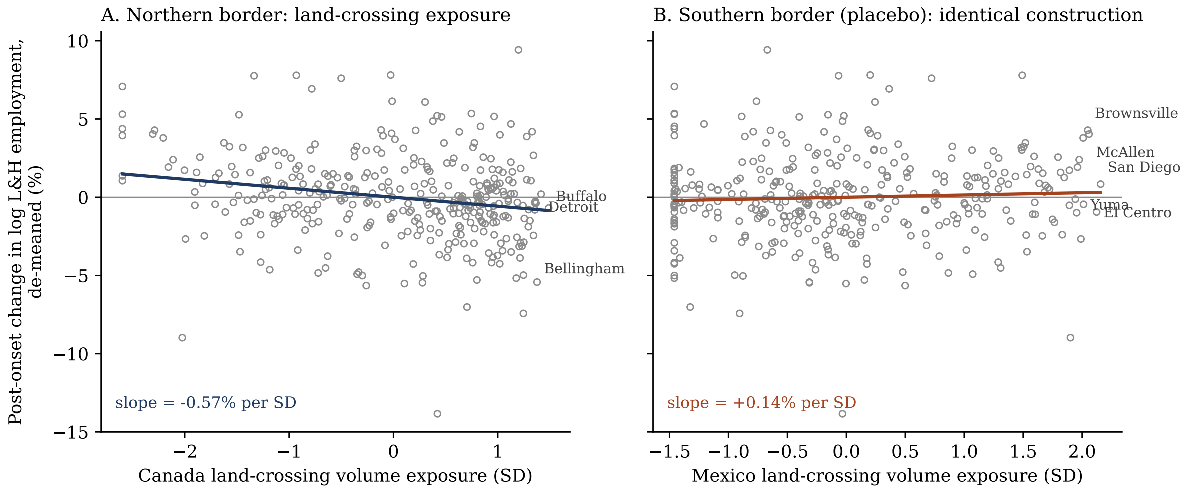

Figure 2. The Cross-Section Behind the Difference-in-Differences

Notes: Each circle is one of the 352 metropolitan areas in the continuous-exposure estimation sample. The vertical axis is the metro's mean log leisure and hospitality employment over the post-onset months (March 2025 through May 2026) minus its pre-onset mean (January 2023 through February 2025), de-meaned across metros and expressed in percent. Because the panel is balanced, the slope of this cross-section equals the two-way fixed-effects coefficient exactly: -0.57% per standard deviation of Canadian land-crossing exposure (Panel A; Table 1, permutation $p = 0.001$) and $+0.14\%$ for the identically constructed Mexican exposure (Panel B; Table 2, permutation $p = 0.33$), the southern-border placebo of Section 7. The most exposed metros on each border are labeled. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

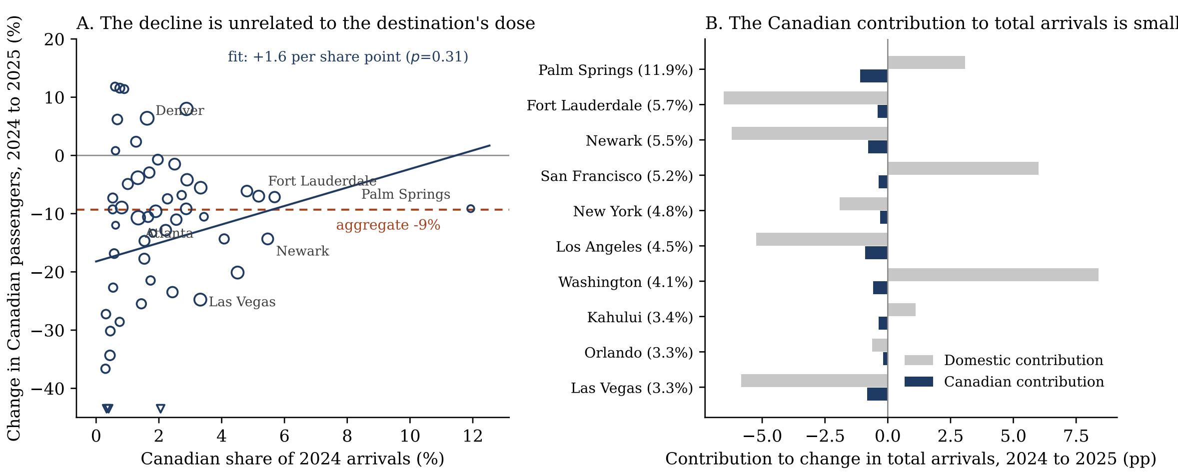

Figure 3. The Size of the Air-Destination Dose

Notes: Destination-airport evidence from BTS T-100 segment data, all carriers. Panel A plots each airport's 2024-to-2025 change in Canadian arriving passengers against its 2024 Canadian share of arrivals (Canadian plus domestic), for the 52 airports with at least 10,000 Canadian passengers in 2024; marker area increases with 2024 arrivals. The decline is unrelated to the share (fit +1.6 percentage points per share point, $p = 0.31$; excluding three airports where Canadian service was discontinued, plotted as triangles, +0.3, $p = 0.69$). Panel B decomposes the change in total arrivals at the ten largest destinations (≥ 1 million 2024 arrivals) with the highest Canadian shares (in parentheses) into Canadian and domestic contributions; the Canadian contribution is -0.2 to -1.1 percentage points, small against domestic movements. *Sources:* Bureau of Transportation Statistics T-100 international and domestic segment data; author's calculations.

Table 2. Northern Border versus Southern-Border Placebo

Exposure (+1 SD × post)	Coef. (%)/(SE)	CRV1 p	Perm. p
Panel A. Northern border (Canada): headline			
Land-crossing volume	−0.57	< 0.001	0.001
	(0.15)		
Border proximity	−0.39	0.004	0.011
	(0.14)		
Panel B. Southern border (Mexico): placebo			
Land-crossing volume	+0.14	0.307	0.329
	(0.14)		
Border proximity	+0.14	0.415	0.302
	(0.18)		

Notes: The dependent variable is log metropolitan leisure and hospitality employment. Each panel applies the identical distance-allocated crossing-volume and border-proximity construction, the first to the 89 Canadian land ports and the second to the 28 Mexican land ports, and estimates the same two-way fixed-effects specification on the same 390-metro panel and the same March-2025 onset. Coefficients are in percent of baseline leisure and hospitality employment. Cluster-robust standard errors are in parentheses; the permutation p-value reassigns the static exposure across metros (2,000 replications). Both borders faced the 2025 tariffs; only the northern border faced the boycott. Southern crossing volumes were essentially flat over the post-onset window (personal-vehicle passengers −1.9% year over year, pedestrians +7.6%), against −23.7% on the northern border. Panel A estimates are significant at the 1 percent level under all inference methods; see Table 1.
Sources: Bureau of Transportation Statistics; BLS Current Employment Statistics; author's calculations.

Table 3. One design, every source: the boycott as a difference-in-differences across data sets

Outcome	Exposed unit	Comparison unit	Δ_T	Δ_C	DiD
Travel volumes					
Land crossings (PV passengers)	U.S.–Canada border	U.S.–Mexico border	−27.1	−2.0	−25.1
Canadian return trips	from United States	from overseas	−32.9	+9.7	−42.5
Canadian trips from U.S. by mode	automobile	air	−39.9	−14.0	−25.9
NTS visits	to United States	overseas	−32.1	+10.0	−42.1
NTS visits to U.S. by purpose	leisure	friends/relatives	−31.4	−13.8	−17.7
Trip intensity					
Nights per overnight visit	U.S. trips	overseas trips	+11.2	+0.3	+10.9
Spend per night (overnight)	U.S. trips	overseas trips	−6.6	+7.4	−14.0
Nights per trip, air visitors[a]	Canadian air visitors	—	+5.4	—	+5.4
Spend per visitor-day, air[a]	Canadian air visitors	—	−9.9	—	−9.9
Dollars					
NTS total spending	in United States	overseas	−20.5	+17.4	−37.9
U.S. travel exports (BEA)	to Canada	to all other countries	−18.5	+1.1	−19.7
Local outcomes					
Taxable retail sales	Whatcom Co., WA	Spokane+Clark Cos.	−0.4	+1.8	−2.1
Taxable retail sales	Blaine, WA (port town)	Spokane+Clark Cos.	−7.0	+1.8	−8.7
Sales-tax collections	Erie+Niagara+Clinton Cos., NY	Onondaga+Albany Cos.	+6.7	+1.6	+5.2
L&H employment[b]	top-decile exposure metros	all other metros	−0.3	+0.2	−0.4

Notes: Every row is the same two-by-two design: log change over the boycott window (2025Q2–Q4 quarterly, March–December 2025 monthly, calendar year for the air survey) against the same window of 2024, exposed unit (Δ_T) minus comparison unit (Δ_C), in log points × 100. Aggregate and survey rows are quoted without sampling inference; construction detail is in Online Appendix I.K and `ext_D_unified_did.csv`.

[a] Single difference over calendar years (no comparison population).

[b] Crude binary version of Table 1's continuous gradient, shown for format comparability; exposure-permutation $p = 0.068$.

Sources: BTS; Statistics Canada; BEA; NTTO; Washington Department of Revenue; New York Office of the State Comptroller; BLS; author's calculations.

Online Appendix

I Robustness and supplementary constructions

Table A1 consolidates the falsification and robustness checks summarized in Sections 7 and 8, each re-estimating the headline land-crossing-volume gradient under an alternative specification, sample, or outcome transformation. Across the battery the estimate stays between about −0.30% per standard deviation (the triple-difference, its conservative floor) and −0.71% (with tariff and manufacturing-share controls). It is stable in sign and order of magnitude throughout. The subsections that follow give the detail behind the checks Sections 7 and 8 state in summary form—regional controls, the metro's own tariff shock, seasonality and the pre-trend, the distance-decay bandwidth grid, the administrative air exposure, the market-potential gradient, and the job-count constructions—followed by the identifying geography.

I.A Regional controls

The land–air contrast could in principle be regional rather than modal: every high-land-exposure metro sits on the northern tier and every large air destination in the Sun Belt, so a slow southward drift of employment growth could masquerade as a boycott gradient. Forcing identification within region—division×month fixed effects, so that Buffalo is compared with other Mid-Atlantic metros in the same month—retains three-quarters of the land-volume gradient, −0.44% per standard deviation (permutation $p = 0.002$, cluster-robust $p = 0.057$). The proximity gradient does not survive this cut (−0.14%, insignificant), so within-region variation supports the volume measure and we read proximity as corroboration only. A state×month specification absorbs most of the identifying variation outright, exposure being spatially clustered within states; it returns a noisy −0.10% whose confidence interval comfortably contains the baseline, so that test is uninformative rather than contradicting. The air gradient stays nonnegative under every regional control.

Spatial correlation in the northern-tier exposure also bears on inference directly. Because the design is a single national shock interacted with a spatially clustered cross-section, three complementary checks guard against the over-rejection that metro-level clustering can produce. (i) Conley spatial-HAC standard errors (Conley, 1999), allowing arbitrary spatial correlation to a 300 km cutoff and serial correlation to six months, leave the land-volume gradient significant ($p = 0.016$ at 300 km, $p = 0.003$ at 200 km). (ii) Clustering at the state rather than the metro level, which permits arbitrary within-state correlation, gives a cluster-robust $p = 0.005$ and a wild-cluster-bootstrap $p = 0.034$ over 52 state clusters (Cameron et al., 2008); at the coarser nine-division level the wild bootstrap is insignificant ($p = 0.12$, few enough clusters that the test has little power, MacKinnon and Webb, 2018). The division-level sample excludes Puerto Rico's four metros, which belong to no Census division, and on that slightly smaller sample the coefficient is −0.45%. (iii) A spatially-blocked permutation, which permutes exposure at the level of contiguous spatial blocks so that each placebo draw is as spatially clustered as the real exposure, returns $p = 0.001$ over forty blocks and $p = 0.014$ with states as blocks. Finally, because domestic spending displaced from the border could contaminate nearby control metros, dropping low-exposure controls within 150 km of a high-exposure metro leaves the gradient at -0.61%, if anything slightly larger, so any such contamination biases the estimate toward zero.

I.B The metro's own tariff shock

The most-exposed land metros are also among the most tariff-exposed. Detroit is the second-largest crossing metro and the center of the auto-tariff shock, so a land-border decline could in principle be a local manufacturing recession reaching into services. Two checks argue against it. Adding a metro's predetermined tariff exposure and its manufacturing share, each interacted with post, does not attenuate the land coefficient; it moves from -0.57% to -0.71% per standard deviation. Dropping each border metro in turn leaves the estimate in a tight band between -0.59% and -0.55%, all significant, and leave-out-Detroit returns -0.58%, essentially the baseline. The continuous gradient does not depend on the one metro where the tariff confound is strongest, which is itself an advantage of the continuous design over a binary one.

I.C The bilateral exchange rate

Section 7 rules out a bilateral Canadian-dollar move as the driver of the U.S.-specific travel decline; Figure A3 gives the underlying series. The loonie depreciated about 4.8 percent against the U.S. dollar between the 2024 average and January–March 2025, but only 1.9 percent against the euro over the same window, so the onset move was largely dollar-specific rather than a broad Canadian weakening. It was also transitory: the bilateral rate had returned to its 2024 level by the first quarter of 2026, and over the March–December 2025 decline window the average depreciation was only 1.5 percent. At the day-trip real-exchange-rate elasticities of Chandra et al. (2014)—about 0.9 in absolute value when the U.S. dollar is strong, as in 2025—even crediting the full onset depreciation as permanent implies a trip decline in the low single digits, an order of magnitude short of the observed 23.7 percent. The currency also runs the wrong way for persistence: through early 2026 crossings held about 20 percent below their 2024 level while the bilateral rate was back to par, and later in 2025 the loonie weakened against the euro even as Canadians raised their overseas travel. This is consistent with the border-county evidence that real-exchange-rate movements of this size move cross-border retail modestly (Campbell and Lapham, 2004).

I.D Third-country arrivals

Section 7 distinguishes a Canada-specific land withdrawal from a general aversion to U.S. travel using NTTO arrivals to the United States by country of residence. Over the post-onset window (March 2025 through April 2026 against the same months a year earlier), Canadian all-mode arrivals fell 20.2 percent, the third-largest decline among the 38 origins with at least 200,000 annual arrivals and about six times the overseas mean of 3.4 percent; Mexican and Japanese arrivals rose. The two larger declines are themselves informative about the motive question of Section 7: Denmark (-26.3 percent), the other origin whose sovereignty was publicly questioned over the same months, and Venezuela (-20.4 percent), the origin for which U.S. enforcement was most salient. Both channels generate origin-specific withdrawals of Canada's magnitude, so the cross-origin ranking corroborates the U.S.-specificity of the Canadian pullback without adjudicating its motive. The Canada-specificity is a statement about *magnitude* and about the land channel, not about air travel in isolation. A broad but shallow softening of U.S. inbound air travel is visible across origins—Germany −12.4 percent, France −7.7 percent, with several smaller European and Oceanian origins deeper—and Canada's own air arrivals, down 11.4 percent, sit inside that band rather than above it. The gap between Canada's 20.2 percent all-mode decline and its 11.4 percent air decline is the land day-trip collapse, a margin no overseas origin shares. Because the employment gradient is identified off land-crossing exposure and overseas air origins do not cross land borders, the general air softening cannot contaminate the land estimate; it is instead consistent with the air null of Section 8. The test cannot separate the boycott from other U.S.-specific land-crossing deterrents such as border-enforcement apprehension, which is why Section 7 identifies the redirection rather than a single motive.

I.E Seasonality and the pre-trend

The metro series are not seasonally adjusted, and the dynamic path on the raw series carries residual seasonality. Deseasonalizing log leisure and hospitality employment metro by metro with an STL decomposition (Cleveland et al., 1990) and re-estimating leaves the land-volume gradient at −0.55% ($p < 0.001$) and proximity at −0.34% ($p = 0.010$), with air still null; the deseasonalized gradient is, if anything, slightly larger than the unadjusted one, so the decline is not an artifact of cold-weather border seasonality. A specification that avoids the decomposition entirely points the same way: adding metro-by-calendar-month fixed effects, which absorb each metro's own seasonal profile without estimating a decomposition, leaves the land-volume gradient at −0.58% (permutation $p < 0.001$) and proximity at −0.36%, with air null. The forty-one-month panel spans only a little over three seasonal cycles, so we read the STL pass as corroboration that the raw and adjusted estimates coincide rather than as a free-standing seasonally adjusted series.

Two formal checks show why the cross-sectional estimate, not the dynamic path, must carry the result. First, an in-time placebo: re-dating onset to March 2024, a year before the boycott, and re-estimating on the true pre-period produces no exposure gradient on the deseasonalized series (land volume between −0.0012 and −0.0015, permutation $p \approx 0.25$–0.34), so there is no spurious gradient at an arbitrary pre-boycott date.[18] Second, a Rambachan–Roth sensitivity confirms that the dynamic path cannot carry the result on its own. The pre-period exhibits the converge-then-break pattern familiar from import-competing geographies: high-exposure metros sit on a relatively higher, declining path before onset, and the post-onset coefficient breaks downward, reaching about −0.49% by the end of the window, continuing through the February 2026 tariff reset. The average post-onset effect is therefore sensitive to a relative-magnitudes pre-trend violation (Rambachan and Roth, 2023), which is why we read the headline as a descriptive association and the job counts as an upper bound (Section 7). The same fragility appears parametrically: adding an exposure-specific linear trend over the full window, which lets high-exposure metros follow their own declining path throughout, absorbs the post-onset break, leaving a post coefficient near zero

[18] On the *raw* series the same placebo returns a significant *positive* gradient once the winter months are dropped from the pre-window—the differential summer seasonality of the border metros, not a treatment effect, and exactly the contamination the STL adjustment removes, which is why the placebo is read on the deseasonalized series.

(+0.12%, insignificant) with a negative trend term. That is expected given the converge-then-break shape, and it is precisely why identification rests on the designs that do not share the trend: the triple-difference, whose metro-by-month fixed effects absorb any metro-specific trend and which still returns -0.30% (Section 6), and the placebos. Figure A1 displays that path, with the air gradient flat at zero throughout.

The Canadian-side series carry their own lead diagnostics: the Wald tests reported with Figure 1 reject strict flatness of the 2024 leads ($p = 0.03$ for the United States–overseas contrast, $p < 0.001$ for automobile–air), with the drift small relative to the post-onset break and, for the automobile–air contrast, opposite in sign to it.

I.F The triple-difference pre-trend record

Section 7 summarizes the pre-trend record of the within-metro triple-difference; this subsection reports it in full. On the deseasonalized series the immediate pre-onset leads are flat: a joint test over the six months before onset does not reject ($p = 0.14$ asymptotic; $p = 0.17$ under design-based inference that recomputes the test across exposure permutations, $p = 0.44$ state-blocked). A joint test over the full 25-lead window does reject ($p = 4.5 \times 10^{-6}$), driven by distant 2023 leads that form a small negative offset converging toward zero. That shape differs from the level design's converge-then-break in the direction that matters: the pre-lead slope is positive (+0.013 percent per month), so extrapolating it across the post window would predict a gradient near +0.20 percent, and trend continuation therefore understates rather than manufactures the post-onset break, which reaches about -0.57% per standard deviation by the end of the window and continues through the tariff reset (Figure A2). A relative-magnitudes sensitivity analysis (Rambachan and Roth, 2023) on this event study formalizes the direction argument: unrestricted, the average post-onset effect is not robust to violations of any size (breakdown $\bar{M} = 0$, as for the level design), but once the violation is restricted to continue the observed upward pre-drift—the empirically motivated direction, and the one that biases the estimate toward zero—the late-horizon average (months six through fourteen) survives violations up to three-quarters the size of the largest pre-period violation ($\bar{M} = 0.75$). Two further checks bear on the tourism-specific trend concern directly. Replacing the sector-by-month fixed effects with division-by-sector-by-month effects, so the tourism-versus-rest differential trend may vary freely across census divisions, leaves the triple-difference at -0.32% (permutation $p = 0.035$). And the same design run on the southern border, where crossings did not decline, returns -0.11% (permutation $p \approx 0.45$): whatever secular tourism-versus-rest trend border metros share does not produce a gradient where the withdrawal did not occur. The proximity version of the triple-difference is right-signed but insignificant (-0.19 percent, $p = 0.17$), consistent with proximity's weaker showing throughout. The division-by-month comparison of Online Appendix I.A guards against regional confounds but uses the same cross-metro exposure variation.

I.G Sensitivity to the distance-decay bandwidth

The two land exposures embed a distance-decay bandwidth, $\tau = 150$ km for the gravity allocation of crossing volume and $\tau = 200$ km for the proximity decay. Table A2 rebuilds each exposure across a grid of τ and re-estimates the headline gradient. The estimate is stable in sign, magnitude, and significance throughout: the volume gradient stays between -0.57% and -0.46% per standard deviation and the proximity gradient between -0.46% and -0.23%, all significant at the five percent level, so the result is not an artifact of the bandwidth choice. The volume gradient peaks near $\tau \approx 150$–200 km; the proximity gradient tightens as the window widens, because a longer decay length pulls more inland metros into the gradient.

I.H The air null under an administrative exposure

The air-destination share is a survey object, and classical measurement error in an exposure attenuates its coefficient toward zero, so the air null could in principle be noise in the regressor. We rebuild the exposure from administrative T-100 segment data covering every carrier, Canadian ones included: each metro's 2024 Canada-to-U.S. passengers at its airports, with each airport assigned to the metropolitan or micropolitan area whose boundary contains it (full-resolution TIGER/Line 2023 polygons; airports outside every such area carry 0.01 percent of Canadian traffic). Canada-to-U.S. passengers fell from 15.9 to 14.4 million between 2024 and 2025, or 9.3 percent, consistent with the I-94 arrivals decline once T-100's broader coverage is taken into account, since it counts every passenger on the segment rather than Canadian residents alone. The employment gradient on the administrative exposure is $+0.15\%$ per standard deviation (cluster-robust $p = 0.15$, permutation $p = 0.31$); on a Canadian-share-of-arrivals version, $+0.17\%$ (permutation $p = 0.24$; the cluster-robust p of 0.047 lies on the positive side, the opposite of a loss); and the pooled land–air difference re-estimated with the administrative measure is -0.77%, $t = -4.3$. The null also survives its support. Estimated only on the 46 metros with positive survey exposure it is $+0.20\%$ and insignificant; winsorizing Las Vegas to the second-highest share, or dropping it, leaves $+0.09\%$ and $+0.08\%$; in raw units the gradient is $+0.73\%$ per ten percentage points of destination share, insignificant throughout. Nor is the null an artifact of averaging over a non-winter post window that would miss a delayed snowbird response: restricting the post period to the first full snowbird season, November 2025 through April 2026, leaves the air gradient statistically indistinguishable from zero on both the survey and the administrative measures (survey $+0.26\%$, permutation $p = 0.22$; administrative Canadian-share -0.02%, $p = 0.92$).

The same data explain the null. The Canadian decline was somewhat deeper at the most exposed decile of metros, 9.6 percent against 6.4 percent elsewhere, with no statistically significant exposure gradient in the decline itself; individual destinations vary around those aggregates (Las Vegas fell by a quarter), but the variation is unrelated to exposure, and Figure 3 displays the airport-level pattern. Canadian passengers were 3.3 percent of Las Vegas's 2024 arrivals and 2.9 percent at the median top-decile metro, so the withdrawal removed a few tenths of a percent of arrivals at the exposed destinations, and domestic arrivals fell 1.4 percent at the exposed metros and 1.3 percent elsewhere. The air-destination dose was an order of magnitude too small to move metropolitan employment. The most air-exposed destinations in fact added leisure and hospitality jobs in 2025 ($+0.35$ percent year over year) while the most land-exposed metros shed them (-0.16 percent); nationally, Canadian air arrivals fell 1.19 million between 2024 and 2025 while total checkpoint throughput rose 2.67 million, though the destination-level data neither require nor demonstrate active domestic reallocation toward the exposed airports.

I.I The market-potential gradient

Section 8 reports that the land decline concentrates where the target's spending is a large share of local demand and thins where local demand is deep. We make that margin continuous by interacting land-crossing exposure with a domestic market-potential index, the gravity sum of other U.S. metros' employment within short-haul reach, in the market-access tradition of Donaldson and Hornbeck (2016). The gradient is -0.84% per standard deviation in isolated border corners with little domestic demand to draw on and a flat -0.02% where market potential is high, the interaction significant at $p < 0.001$. Market potential proxies jointly for a low visitor share of local demand and for the depth of replacement spending, so the interaction identifies their combined gradient rather than isolating either channel; we read it as a joint incidence gradient, not as a causal estimate of replaceability. It is the continuous form of the discrete land-versus-air contrast, separating Bellingham, a remote Vancouver-dependent crossing, from Detroit and Buffalo, large metros embedded in dense domestic markets.

I.J Job-count constructions

The gross-loss range of Section 9 rests on three constructions. The first two extrapolate the estimated per-standard-deviation gradient: multiplying each metro's exposure by the gradient and by its 2024 baseline leisure and hospitality employment, summed over metros with positive land-crossing exposure, gives roughly 31,600 jobs on the land-volume gradient and 15,000 on the proximity gradient. This is a linear extrapolation across the exposure distribution and is sensitive to that functional form, so we read it as an order of magnitude rather than a point estimate. The third construction is independent of the functional form: instrumenting each metro's realized lost crossings with its predetermined exposure (first-stage $F \approx 570$ in levels) gives -0.75 log points of leisure and hospitality employment per million lost distance-allocated crossings, the same units as the exposure construction, and cumulating this over metros gives roughly 12,500 (95% confidence interval 1,500–23,500). Set against the 9.7 million foregone crossings, the extrapolated and instrumented aggregates bracket the intensity between one job per three hundred and one per eight hundred lost trips. A log-log version implies an elasticity of metropolitan leisure and hospitality employment to Canadian land visits near 0.38, but its first stage is thin: the realized crossing decline was close to uniform in percentage terms across ports, so the identifying variation is predetermined exposure levels rather than differential decline rates. The land-crossing series observes Canadian land visits only; the air channel is not separately identified at this frequency. Because the instrument is the same predetermined exposure that drives the reduced form, the instrumented construction is a reparameterization into visits units rather than independent evidence, and its confidence interval is sampling uncertainty conditional on the identification caveats of Section 7.

I.K The withdrawal in dollars: margins, a spending-side benchmark, and modes

National Travel Survey aggregates (Statistics Canada Table 24-10-0070) price the withdrawal directly. Canadian spending in the United States fell by C$2.95 billion over 2025Q2–Q4 against the same quarters of 2024, about US$2.1 billion at the window-average exchange rate. The U.S.-side mirror agrees on the order of magnitude: BEA-measured travel exports to Canada fell US$2.6

billion over the same three quarters (US$3.4 billion over the full year), a decline larger than the change in total U.S. travel exports to all countries combined, which other markets partly offset.[19] A Bennet decomposition over trip cells (purpose by duration) attributes the entire decline to the extensive margin: fewer trips contribute −C$4.2 billion, while the intensive margin *offsets* C$1.25 billion because the surviving trips are longer (the short discretionary trips exited the mix). The leisure-overnight cell alone accounts for C$1.9 billion of the decline; spending on visits to friends and relatives was flat to slightly higher. The dollar withdrawal, in other words, is a leisure-trip withdrawal. In job-equivalent units (same shares and wages as below), the trip margin alone would put 24,000 to 44,000 positions at risk and the lengthening of surviving trips gives back 7,000 to 13,000. The same-day cells contribute only about C$0.34 billion of the national dollar decline, which is not a tension with the paper's border-metro incidence but the reason for it: day-trip dollars are small in the aggregate and concentrated in a handful of small border economies, while the far larger leisure-overnight dollars are spread across the whole country, which is why the national total is modest and the measurable employment imprint is local. Scaled against tariff revenue, roughly 29 cents of Canadian spending in the United States disappeared per dollar the Treasury collected on Canadian goods over March–December 2025 (gross spending, not earnings; compare the three-to-eleven-cent earnings figure of Section 9).

The spending decline supplies a benchmark for the job counts of Section 9 from independent data. Accommodation, restaurant-and-bar, and recreation spending—the categories that meet local leisure and hospitality labor—are 68 percent of Canadian spending in the United States. Applying a 30 to 40 percent labor share of leisure and hospitality revenue and the $25,000–$35,000 annual wage of Section 9 to the annualized local-consumption decline implies roughly 17,000 to 31,000 national leisure-and-hospitality job-equivalents. That an aggregate spending-side calculation, from survey data and with no employment estimate in it, lands on the same range as the exposure-gradient constructions is a consistency check on both; and because the spending figure is national while the gradient counts land-border metros, the overlap leaves little room for additional air-metro losses, consistent with the air null of Section 8.

Allocating the dollar decline across travel modes with the frontier counts (Table 24-10-0053) and the survey's duration-specific spend per visit (assuming spend per visit within a duration class is common across modes, which understates the longer-haul air trips and so bounds the air share from below), the automobile margins carry about four-fifths of the dollar decline—69 percent overnight automobile, 11 percent same-day automobile—and air at most about a fifth. The mode split of the dollars matches the mode split of the measured employment incidence. Outputs and construction in `ext_D_nts_decomposition.csv` and `ext_D_nts_boe.csv`; survey aggregates are quoted without sampling inference.

Table 3 collects every data source touched by the episode under a single design: the same year-over-year difference-in-differences, an exposed unit against its natural comparison, over the same boycott window. Volumes, intensities, dollars, and local outcomes carry a consistent sign pattern,

[19] BEA International Transactions Accounts, travel services exports, Canada, quarterly unadjusted (same-quarter comparison); the roughly 20 percent mirror gap against the Canadian survey figure reflects coverage differences (BEA travel includes education and health travel) and is normal for services mirrors. Statistics Canada's balance-of-payments travel account publishes a U.S. split only annually and, as of this writing, only through 2024; its 2025 total-payments series *rose* about C$0.9 billion over the window, the redirection of Section 7 in macro form—spending shifted to other destinations rather than stopping.

and the two rows that do not—New York border-county sales-tax collections, which outgrew their comparisons, and the air intensity margins—are the rows the paper's incidence geography predicts should not move: the synthetic-control section finds no Buffalo divergence, and the dose argument of Section 8 implies no air-metro imprint. The revenue-side rows are the sharpest new corroboration: Blaine, the Washington port town most dependent on Vancouver day trips, shows an 8.7 log-point relative decline in taxable retail sales while the New York border counties show none, reproducing from administrative tax data the concentrated-incidence map the employment gradient identifies.

I.L The identifying geography

Figure A5 maps the predetermined exposure that identifies the estimates: Canadian land-crossing volume on the northern tier, Canadian air-destination share in the Sun Belt, and the Mexican land-crossing volume that anchors the southern-border placebo of Section 7.

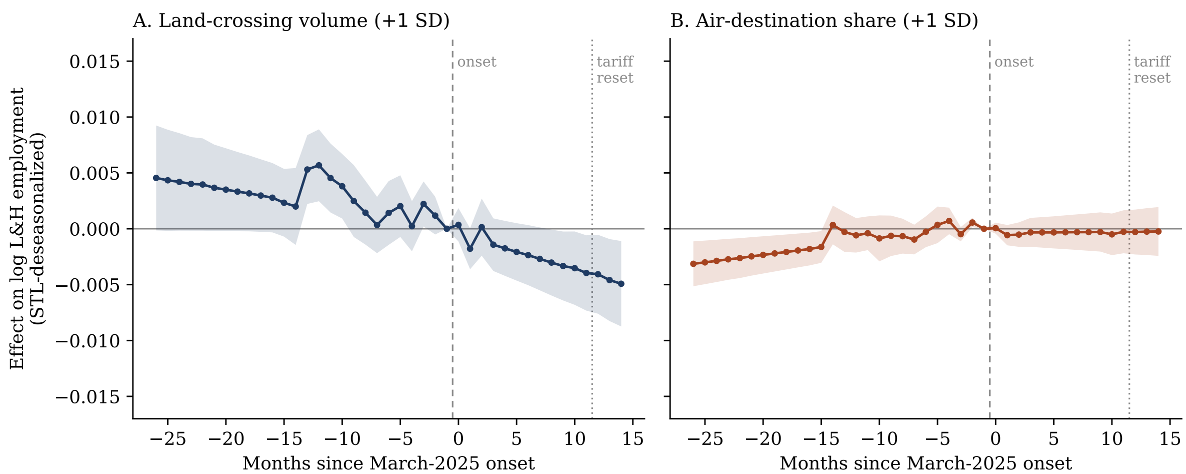

Figure A1. Event-Study Gradients on the Deseasonalized Series

Notes: Event-time coefficients on exposure interacted with month-since-onset dummies (reference February 2025, $t = -1$), estimated on STL-deseasonalized log metropolitan leisure and hospitality employment with metro and month-of-sample fixed effects; shaded bands are cluster-robust 95% intervals (metro clusters). Panel A is the land-crossing-volume exposure: high-exposure metros sit on a higher, declining path before onset, converge by the reference month, and break downward afterward, declining to about -0.49% per standard deviation at the end of the window, continuing through and beyond the February 2026 tariff reset (dotted line). Panel B is the air-destination exposure, flat at zero after onset. The paper does not rest on this path: a Rambachan–Roth sensitivity shows the average post-onset coefficient is not robust to relative-magnitudes pre-trend violations of any size (breakdown $\bar{M} = 0$), which is why we read the headline as a descriptive association supported by the triple-difference, the division-by-month comparison, the in-time placebo, the southern-border placebo, and the Canadian-side redirection (Section 7). *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

Table A1. Robustness of the Land-Crossing-Volume Gradient

Specification	Land-volume gradient	Inference / note
Baseline (Table 1)	−0.57%	perm. $p = 0.001$; CRV1, WCR (Rademacher/Webb), perm. agree
metro wild-cluster restricted bootstrap	−0.57%	Rademacher $p = 0.0002$; Webb $p = 0.0002$ (352 clusters)
+ own-tariff exposure × post, mfg. share × post	−0.71%	confound moves estimate away from 0
Normalized exposure (volume per 2024 L&H worker, logs)	−0.62%	$p < 0.001$; not merely “large crossing nearby”
Pooled land + air exposure (single index)	−0.36%	perm. $p = 0.015$; land loss averaged against air null
Leave-one-border-metro-out (range over 18)	−0.59% to −0.55%	all significant
Leave-out-Detroit (enters once, as MSA)	−0.58%	essentially the baseline
Division×month fixed effects	−0.44%	perm. $p = 0.002$; proximity does not survive
State×month fixed effects	−0.10%	ns; SE quadruples, CI contains baseline
Metro × calendar-month fixed effects	−0.58%	$p < 0.001$; proximity −0.36%
Exposure × linear trend (full window)	+0.12%	ns; trend absorbs converge-then-break (see triple-diff)
Conley spatial-HAC SE (300 km, 6-mo. lag)	−0.57%	$p = 0.016$; $p = 0.003$ at 200 km
Wild-cluster bootstrap, state clusters	−0.57%	$p = 0.034$ (52 clusters); division $p = 0.12$ (9, excl. PR)
Spatially-blocked permutation (40 blocks)	−0.57%	$p = 0.001$; state blocks $p = 0.014$
Drop controls < 150 km from treated (SUTVA)	−0.61%	$p < 0.001$; contamination biases toward 0
STL-deseasonalized outcome	−0.55%	$p < 0.001$ (proximity −0.34%, $p = 0.010$)
Triple-difference (tourism vs. rest, within metro-month)	−0.30%	perm. $p = 0.046$; CRV1 0.072, WCR 0.077
pre-onset leads, design-based flatness test	—	fail to reject flat: perm. $p = 0.17$ (i.i.d.), 0.44 (state-blocked)
division×sector×month fixed effects	−0.32%	perm. $p = 0.035$; tourism trend free by division
southern-border (Mexico) triple-difference	−0.11%	ns (perm. $p \approx 0.45$); no gradient absent the withdrawal
Honest-DiD, sign-restricted late horizon	$\bar{M} = 0.75$	months +6..+14 robust to violations continuing the upward pre-drift; unrestricted avg. post $\bar{M} = 0$
Post-reset persistence (split at 2026m2, deseas.)	−0.50% / −0.74%	boycott window / post-reset; both perm. $p \leq 0.001$
Onset date Dec 2024–Apr 2025 (deseasonalized)	−0.51% to −0.56%	stable; Jan. onset −0.53%, perm $p < 0.001$
Placebo onset (March 2024, deseasonalized)	−0.12% to −0.15%	ns (perm. $p \approx 0.25$–0.34); small, insignificant pre-period gradient; consistent with modest pre-onset drift in the raw series
Honest-DiD (Rambachan–Roth, avg. post)	breakdown $\bar{M} = 0$	avg. post not robust to any relative-magnitudes violation; not relied on
Low domestic market potential (substitutability)	−0.84%	vs. −0.02% where market potential high
interaction (land × market-potential)	—	$p < 10^{-3}$

Table A2. Land-Gradient Sensitivity to the Distance-Decay Bandwidth τ

	Land-crossing volume		Border proximity	
τ (km)	Coef. (%)	CRV1 p	Coef. (%)	CRV1 p
50	−0.46	0.002	−0.23	0.027
100	−0.53	< 0.001	−0.29	0.016
150 (volume)	−0.57	< 0.001	−0.34	0.009
200 (prox.)	−0.57	< 0.001	−0.39	0.004
250	−0.55	< 0.001	−0.43	0.002
300	−0.52	< 0.001	−0.46	0.001

Notes: Each cell re-estimates the headline two-way (metro and month-of-sample) fixed-effects gradient on log metropolitan leisure and hospitality employment for a one-standard-deviation increase in the stated land exposure interacted with the post-March-2025 indicator, after rebuilding that exposure at the given bandwidth τ and z-scoring it over the estimation cross-section (n = 14,432 metro-months). Coefficients are in percent of baseline leisure and hospitality employment. The headline specifications use $\tau = 150$ km for land-crossing volume and $\tau = 200$ km for border proximity (marked). CRV1 is the cluster-robust p-value (metro clusters); at the headline bandwidths the permutation p-values are 0.001 (volume) and 0.009 (proximity). All estimates are observational.

Sources: BLS Current Employment Statistics; Bureau of Transportation Statistics; Census cartographic boundaries; author's calculations.

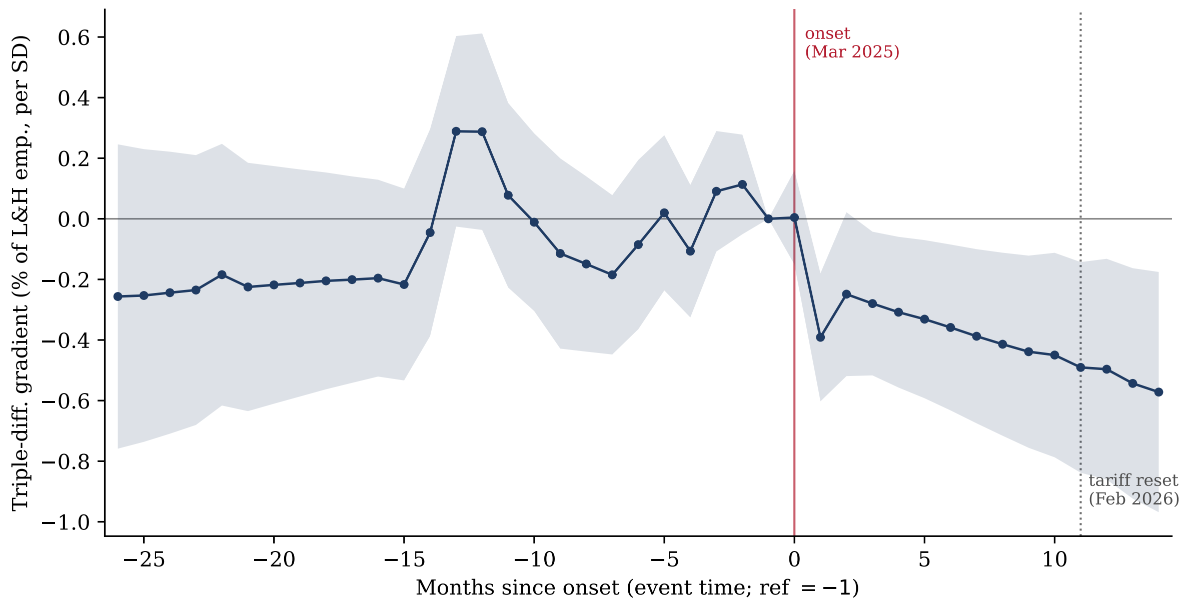

Figure A2. Triple-Difference Event Study on the Deseasonalized Series

Notes: Event-time coefficients on Canadian land-crossing exposure interacted with a leisure-and-hospitality indicator and month-since-onset dummies (reference February 2025, $t = -1$), from the within-metro triple-difference of Section 6, with metro-by-sector, metro-by-month, and sector-by-month fixed effects, estimated on STL-deseasonalized log metropolitan employment; shaded bands are cluster-robust 95% intervals (metro clusters). The immediate pre-onset leads are flat—a joint Wald test over the five lead coefficients before the reference month ($t = -6$ through $t = -2$) does not reject flatness ($p = 0.14$)—and the tourism-differential gradient breaks downward after onset (solid line, March 2025), reaching about -0.57% per standard deviation by the end of the window (numerically coincident with, but a different object from, the -0.57% level gradient of Table 1) and continuing through the February 2026 tariff reset (dotted line). A joint test over the full 25-lead window rejects flatness ($p = 4.5 \times 10^{-6}$), driven by the distant 2023 leads visible at the left of the panel: a small negative offset converging toward zero, whose positive slope (+0.013 percent per month) is the wrong sign to inflate a negative post-onset break—extrapolated forward it would predict a positive gradient. The single-coefficient version of this design is the -0.30% triple-difference of Section 6, -0.29% on the deseasonalized series. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

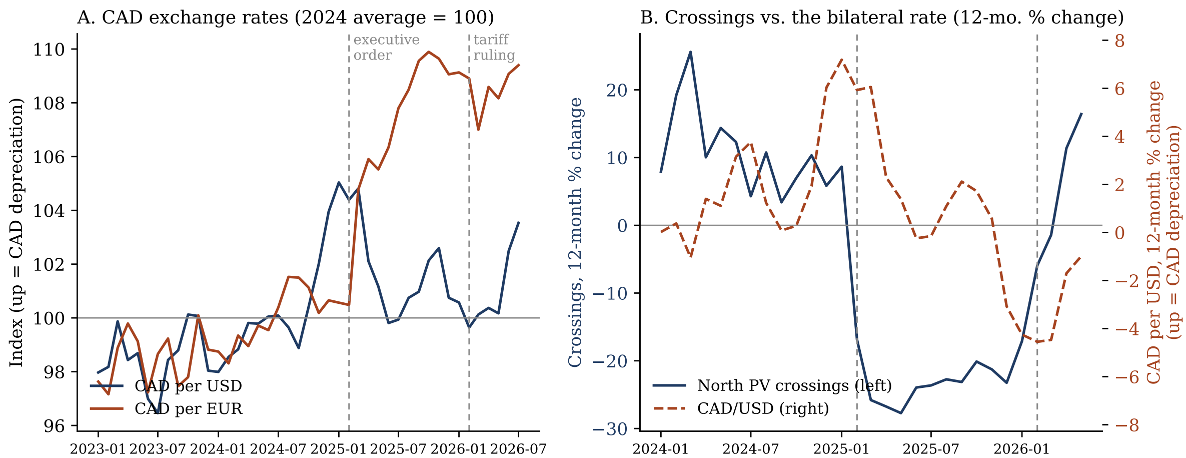

Figure A3. The Canadian Dollar Cannot Account for the Travel Decline

Notes: Panel A plots the monthly Canadian dollar against the U.S. dollar and against the euro, each indexed to its 2024 average = 100 (Bank of Canada daily rates from the Valet API, collapsed to monthly means), with vertical lines at the February 2025 executive order and the February 2026 tariff ruling. The bilateral depreciation against the U.S. dollar peaks near 5% at onset, is larger than the contemporaneous move against the euro, and returns to its 2024 level by early 2026. Panel B overlays the twelve-month change in northern personal-vehicle crossings on the twelve-month change in the bilateral rate (the crossing series is on the left axis, the exchange rate on the much narrower right axis); crossings fall by roughly a quarter at onset, and the positive twelve-month changes in early 2026 reflect the collapsed 2025 base—in levels, crossings remain about 20% below 2024 (Section 6) after the currency has recovered. *Sources:* Bank of Canada; Bureau of Transportation Statistics; author's calculations.

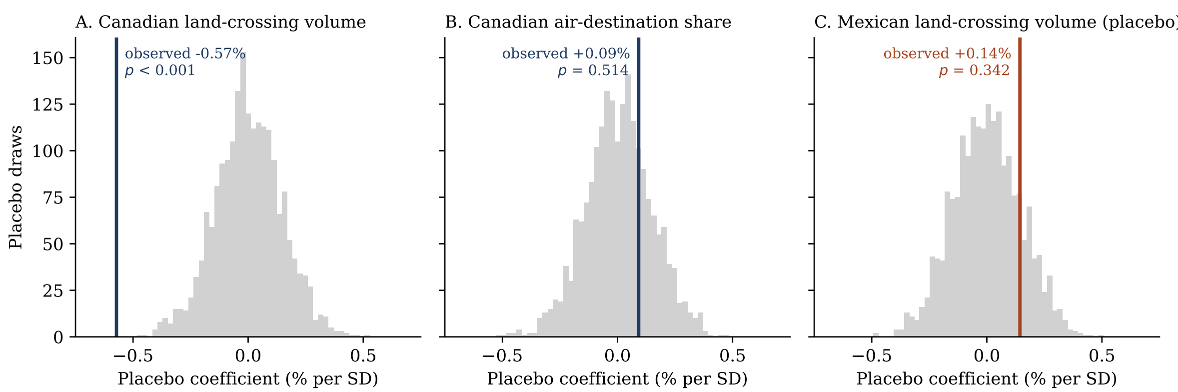

Figure A4. Randomization Distributions for the Exposure Gradients

Notes: Each panel reassigns the static metro-level exposure at random across the 352-metro cross-section 2,000 times, re-estimates the two-way fixed-effects gradient of Table 1 under each placebo assignment, and plots the resulting distribution of placebo coefficients; the vertical line marks the coefficient at the actual assignment. The Canadian land-crossing coefficient lies outside every placebo draw ($p < 0.001$); the Canadian air-destination and Mexican land-crossing coefficients sit inside their null distributions ($p = 0.51$ and $p = 0.34$). These draws are independent of those behind Tables 1 and 2, so the p-values differ from the tabled ones only by simulation noise. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; U.S. NTTO; author's calculations.

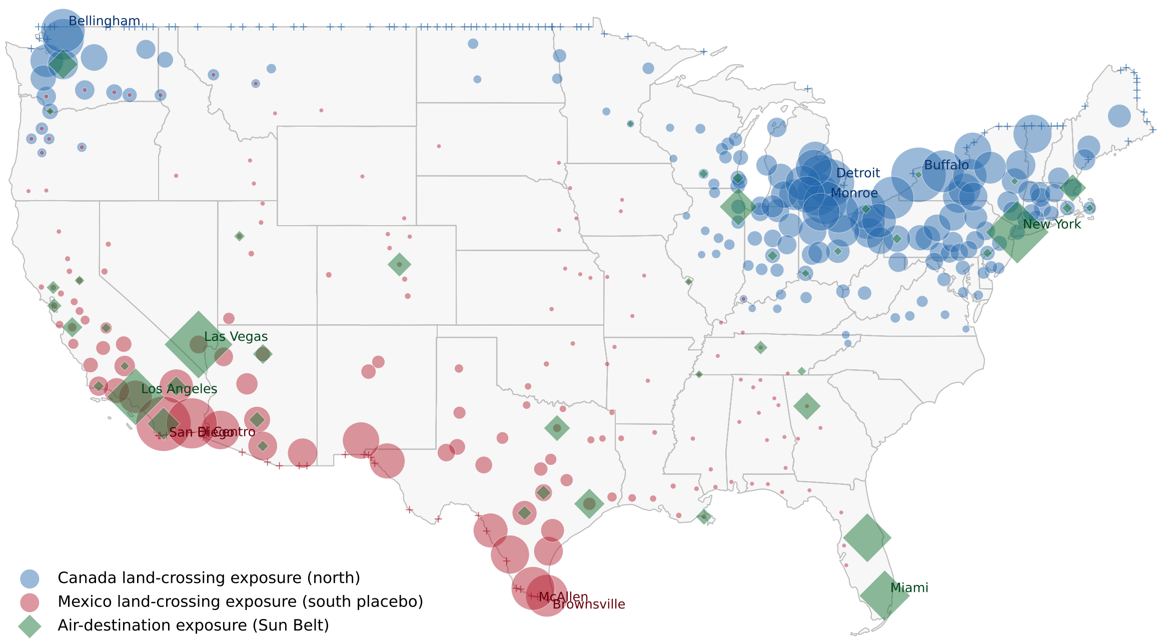

Figure A5. The Identifying Geography on Both Borders

Notes: Each marker is a metropolitan area in the upper part of the exposure distribution (above the 55th percentile of the relevant measure; low-exposure metros are omitted for legibility). Circle area scales with land-crossing exposure, the 2024 inbound personal-vehicle volume gravity-allocated over all Canadian (blue) or Mexican (red) ports of entry with a 150-km decay; green diamonds are Canadian air-destination exposure (NTTO Survey of International Air Travelers). Crosses mark the 89 Canadian and 28 Mexican land ports of entry. The southern-border land exposure provides the placebo of Section 7; the air-destination margin provides the mode contrast of Section 8. *Sources:* Bureau of Transportation Statistics; U.S. NTTO; Census cartographic boundaries.