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, an act of economic coercion against a close ally. This paper asks what such coercion costs and where the burden falls. Canadians responded by sharply curtailing travel to the United States; we trace where that redirection landed inside the coercing country, at the local-labor-market level. Using predetermined (2024) cross-border-visitor exposure across 390 metropolitan areas, we find leisure and hospitality employment fell by roughly -0.57% per standard deviation of land-crossing exposure after onset. Falsification tests tie the decline to the Canada episode rather than to the tariffs: the tariffed southern border shows no parallel gradient, controlling for a metro's tariff exposure leaves the estimate intact, and Canadian overseas travel rose even as trips to the United States collapsed. The incidence concentrates on land-border communities. Canadians were too small a share of air traffic to shift airport-adjacent labor markets; at the land margin, the pullback amounts to ten to thirty thousand jobs, worth several hundred million to a billion dollars a year in earnings. When tariffs were struck down in February 2026, neither travel nor employment recovered within our window. Part of the cost of the coercion fell on the country that imposed it, concentrated in border communities that leaned against the party whose policy provoked the backlash.

Keywords: economic coercion; weaponized interdependence; consumer boycott; local labor markets; tourism; trade war.

JEL codes: F51, F14, J21, R12.

*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: who holds the chokepoint, whether sanctions change behavior, and who bears the cost inside the target state. Costs that flow back to the coercer are acknowledged in principle and rarely measured in practice. Those imposed by a target's *decentralized* response—expressed through consumers and sub-national governments rather than a state's formal retaliation—are almost never quantified at the coercer's own local-labor-market level. The 2025 confrontation between the United States and Canada offers an unusually observable opportunity to do so.

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 (**?**). 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% (**?**).

We measure the blowback that followed these events in U.S. communities. Our 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% per standard deviation, robust across the inference methods appropriate for a small treated set. Four comparisons then defend that decline against 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.

A separate decomposition, by travel mode, does different work: it locates where the cost falls. 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 contribution is to place the episode inside the economic-coercion framework, treating the travel withdrawal as a measurable cost that returns to the coercer, and to quantify it: on the order of ten to thirty thousand leisure and hospitality jobs lost in exposed land-border communities, a count we treat as an upper bound on the gross local incidence. The national figure is smaller, because the air-destination null (Section **??**) removes the air-travel margin.

We build directly on **?**, who provide the first U.S.-local estimate of the 2025 tourism collapse, report roughly a 6% employment contraction at the most Canadian-exposed establishments, and document that the Canadian withdrawal was roughly uniform across land and air crossings, pooling 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, the contribution we intend for a geoeconomics audience.

We are deliberately disciplined about what the data can support. The exposure cross-section is predetermined, but the boycott is not randomly assigned across places, the post-onset window is short, and the outcome series are not seasonally adjusted. We write "associated with" and "consistent with" throughout. The falsification tests strengthen the reading of the decline as blowback from the coercion episode; none of them makes the episode causally identified, and we say so where it matters.

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% tariff on Canadian goods, with a 10% rate on energy, under the International Emergency Economic Powers Act. 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.[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

[1] Timeline drawn from the underlying executive orders and contemporaneous reporting: the February 1 IEEPA executive order (25% on Canadian goods, 10% on energy), the February 3 thirty-day pause, and the March 4 implementation, met the same day by Canadian countervailing tariffs.

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. 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 **??**. And 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 **??**). 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 **??** 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 bottom panels of Figure **??** 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, and the automobile–air gap to -0.23. 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, consistent with a response driven by the sovereignty dispute rather than by relative prices, and the pullback did not unwind when the tariffs were later struck down (Section **??**).

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

[3] **?** 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 in 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 duration margin extends to within-automobile travel the near-uniformity of the withdrawal that **?** document across crossing modes.

2.B Two instruments, two margins

The Canadian reaction ran through channels from private to state action, and only one end is visible to a labor-market design. The travel withdrawal sits nearest the private end—no central organizer, no enforcement—but was not purely spontaneous: in his February 1 national address the prime minister urged Canadians to "choose Canada" and to consider "changing your summer vacation plans to stay here in Canada," and premiers echoed the cue as the first delistings were announced.[5] It is better described as elite-cued consumer action than as either spontaneous sentiment or state direction. That distinction matters in Section **??**, since cues that amplify a response can in principle help unwind it. What we measure is unaffected by where on that spectrum it sits: a demand shock correlated with exposure that followed a specific act of U.S. pressure.

Provincial liquor monopolies provided the state-level counterpart: they delisted U.S. wine and spirits, the Ontario board's removal of roughly 3,600 products being the largest single case.[6] These two channels land on different margins, which is what makes one of them measurable here. The travel boycott'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, where a capital-intensive sector absorbs it without shedding headcount. The local-labor-market evidence that follows is therefore almost entirely about the travel boycott. Whether its cost is visible in U.S. local labor markets, and where, is the empirical question.

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. **?**, extending an asymmetric-interdependence tradition that runs from **?** through **?**, 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 routes a cost back through the hub's own non-tradable service economy. Blowback is the complement to chokepoint power, a return flow the coercion ledger acknowledges and has rarely measured at this resolution.

? established the analytical frame for economic statecraft as the use of economic means for foreign-policy ends; we take from him the injunction to judge an instrument of statecraft against

[5] Prime Minister Trudeau, national address, February 1, 2025; Ontario Premier Ford, announcing the LCBO delisting on February 2, 2025 ("There's never been a better time to choose an amazing Ontario-made or Canadian-made product"); British Columbia ordered U.S. liquor from Republican-led states pulled the same weekend.

[6] The LCBO removed roughly 3,600 U.S. products on March 4, 2025, by order of the Ontario premier. A delisting is mechanically sharper than a tariff, stripping the product from the only legal channel rather than raising its price; because bourbon must by law be made in the United States, Kentucky and Tennessee distillers cannot re-source to keep shelf space, and Brown-Forman reported its Canada sales down 62% with its chief executive calling the action "worse than a tariff." U.S. distilled-spirits exports to Canada from Kentucky and Tennessee fell about 50% after the delistings, yet distillery employment shows no matching movement, consistent with this producer-revenue margin; we keep the channel to the side of the employment estimates.

the costs and efficacy of its alternatives, a comparison for which the sender-side cost measured here has been a missing input. **?** 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. 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. That is a measurement the statecraft literature acknowledges in principle and rarely prices (**?**), rather than inferring from aggregate trade flows or case-coded outcomes. Whether the episode changed either government's conduct is beyond what a single observational case can establish, and we do not take it up. The incidence of the tariffs the two countries exchanged is by now well measured (**??**); the consumer boycott that ran alongside them is not.

The closest empirical precedent sits in trade rather than in the sanctions literature. **?** 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 (**?**). Our design belongs to that family, with two differences: the outcome is on the coercer's side rather than the target's, and we decompose the incidence by travel mode, a margin whose responsiveness to cost and distance has its own literature on cross-border travel (**?**). The broader boycott canon studies trade flows, brand share, or stock returns. **?** shows boycotts bite branded final goods and spare intermediates; **?** document market-share losses for French-sounding brands; **?** and **?** link soured political relations to lower bilateral trade; **?** find stock-price damage to exposed firms. None of this work measures local-labor incidence on the coercer, and none differentiates travel mode. On the target's side, the withdrawal is an episode of political consumerism driven by consumer animosity (**????**).

Our framework yields three incidence predictions, shared with the boycott-incidence literature, that structure the empirical work. Incidence should concentrate where the target's spending used to be, so it should rise with predetermined Canadian-visitor exposure; it should be mode-specific, because Canadian spending is a large share of the land day-trip economy and is marginal at major air destinations; and replacement demand can offset what is lost, so the same national shock can leave a sharp local imprint in one place and none in another. A response that rests on a sovereignty provocation rather than a price carries three further signatures a price or income shock would not: its onset should track the provocation rather than the tariff schedule, it should extend across consumption margins at once, and it should not unwind when the formal instrument is withdrawn, since no bargaining counterparty controls it. The boycott canon lends the last prediction particular force: consumer boycotts are typically brief where detectable at all (**???**), so durability is itself an anomaly a sentiment account must predict. The timing evidence of Section **??**, the cross-margin breadth of Section **??**, and the persistence test of Section **??** take the three in turn.

4 Data

4.A Cross-border-visitor exposure

Exposure is built entirely from the pre-onset (2023–24) cross-section, which licenses using lagged shares for weights while the outcome is read off the timely employment series. 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%), 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 **??**).

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

The identifying geography is mapped in Online Appendix Figure **??**. 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 **??** 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 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, so the exclusion loses no distinct geography and 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%). The border-county panel covers 649 counties in the thirteen border states plus Alaska. Table **??** reports summary statistics for the metro cross-section.[7]

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.[8] The workhorse specification is 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 inference problem 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 **?** 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 **??** 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 (**?**). The binary design, which reduces to ten treated units, makes the small-sample problem acute and returns a null. Online Appendix Figure **??** 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.

[7] 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.

[8] 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.

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 **??** 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

Our headline estimate is the land-crossing-volume gradient. A one-standard-deviation increase in distance-weighted land-crossing volume is associated with a 0.57% decline in leisure and hospitality employment after onset, significant across inference methods (Table **??**). Figure **??** 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 is a triple-difference that nets out every metro-wide shock through metro$\times$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$\times$tourism$\times$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 ($+0.37\%$, $p = 0.002$). Because 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 and not a generic story about tourism-dependent places, and it is the one corroborating design that does not inherit the pre-trend concern discussed in Section **??**; we therefore 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.[9] Synthetic controls for the most day-trip-dependent metros corroborate the pattern: Bellingham, which depends heavily on Vancouver day-trippers, diverges most clearly below

[9] 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.

its synthetic, with Detroit next and the higher-market-potential Buffalo and Burlington showing no meaningful gap.

The decline is more consistent with an inward shift in tourism labor demand than with a withdrawal of labor supply, though on this margin 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, whereas a supply-side withdrawal of workers would have bid wages up. The margin a demand shock moves first, hours per worker, is absent from the QCEW, and two caveats and the underlying county estimates keep this a reading rather than a finding (Online Appendix **??**).

The gradient has also outlasted the tariffs that accompanied the boycott. A February 2026 court ruling struck down the IEEPA tariffs, removing the statutory 35% rate on non-USMCA Canadian goods and cutting the realized aggregate U.S. tariff rate by roughly a third. The realized rate on Canadian imports moved little, hovering near three percent on either side of the ruling, because USMCA-compliant trade was exempt throughout; the reset is therefore a salient statutory and political reversal rather than a large realized-price change. 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 $p \leq 0.001$.[10] The traffic series show the same pattern: from March through May 2026 northern land crossings were still 20% below the same months of 2024, essentially the boycott-year gap, and Canadian automobile trips a third below theirs, while overseas trips remained elevated. The redirection, whatever its precise motive, did not respond to the reversal of the instrument that had accompanied its onset. Two readings fit, and the design cannot fully separate them: 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 **??**). 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-rate series sharpens the price-versus-sentiment reading: the realized price shock on Canadian goods was 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 it is also consistent with the tariffs, with a weaker Canadian dollar or softer Canadian incomes, with a local manufacturing downturn, with measurement noise, or with seasonality. This section takes each alternative in turn and shows what the data rule out. 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

[10]Three months is a short window; the raw-series split is steeper but carries spring seasonality, so we quote the deseasonalized figures. And the ruling reversed the tariff, not the rhetoric that provoked the boycott, so the test separates price from sentiment only partially.

motive among several pointing in the same direction.

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 **??** 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 does not 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 (**??**). That move was too small and too brief: the loonie depreciated about 4.8% against the U.S. dollar at onset but only 1.9% against the euro, recovered to its 2024 level by early 2026, and at the day-trip elasticities of **?** could account for only low-single-digit percentage points of the observed 23.7% crossing decline, which persisted after the currency had recovered (Online Appendix **??**, Figure **??**).

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%, 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 (−11.4%) 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 **??** (Online Appendix **??**). This does not separate the boycott from a U.S.-specific land deterrent such as enforcement apprehension, consistent with the caveat above.

Generic tariff friction: 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-weighted 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 **??**; Figure **??**, 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% decline on the northern border. The placebo therefore compares a border that kept its visitors to one that lost a quarter of them, and the contrast is the strongest single piece of evidence that the blowback is the U.S.-specific travel redirection rather than the trade instrument the two countries 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 **??**. 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 $p = 0.002$). 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%. And 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 **??** bears directly on the dynamic path, which shows the converge-then-break pattern familiar from import-competing geographies: high-exposure metros sit on a relatively higher, declining path before onset, so the average post-onset effect is sensitive to a relative-magnitudes pre-trend violation, which a Rambachan–Roth sensitivity confirms (Online Appendix **??**; the dynamic path is in Online Appendix Figure **??**). 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 trend concern entirely 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 **??**). The division-by-month comparison guards against regional confounds but uses the same cross-metro exposure variation, and the deseasonalized in-time placebo at a counterfactual March-2024 onset returns a small, insignificant gradient (−0.12% to −0.15%, about a quarter of the headline) rather than a strict zero, 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. 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 **??** 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 Why the cost concentrates: travel mode and the size of the dose

The land decline is only one part of the incidence; the absence of any loss at the air destinations, and the reason for it, is the other. 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%—so an analyst working with a combined visitor-exposure measure would recover a muted, partly cancelled effect and miss both the concentration at the crossings and the absence of any imprint at the air destinations.

The air-destination gradient is statistically indistinguishable from zero: in Table **??** 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% nationally.[11] The mode contrast is the paper's sharpest descriptive result, and once the dose is measured the air null is close to arithmetic; the air leg's contribution is to measure the dose, not to produce a surprising zero. Pooling both exposures and testing the difference directly, the land coefficient is distinguishable from the air at $t = -4.1$ (volume) and $t = -3.2$ (proximity); the air coefficient's 95% confidence interval, $[-0.0008, +0.0014]$, 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 **??**).

Destination-level passenger data explain why a real withdrawal left no employment imprint, and the explanation is primarily dilution rather than active substitution (Figure **??**): 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 **??**).

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 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 **??**).

9 Magnitude and incidence

The blowback is small in national aggregate and concentrated locally, the incidence pattern the framework of Section **??** 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 construction independent of that functional form, cumulating the realized loss of crossings over metros at an instrumental-variables response, gives roughly 12,500 (95% 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 **??**, read the whole range as an upper bound (Online Appendix **??** 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 **?** 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. Read per lost visit, the constructions imply one

[11] This is the 2024-to-2025 change in NTTO I-94 air arrivals. Measured instead against the 2023–24 average, as in **?**, 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.

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 (?): the one-per-three-hundred end would require each forgone 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 ??), 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 (?), and it is silent on whatever the boycott did through other margins. Fourth, the counts inherit the upper-bound status of Section ??: 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.[12]

The distributional politics run opposite to the more familiar tariff-retaliation case. The 2018–19 agricultural retaliation fell on Republican-leaning farm counties (?), and a broader literature ties trade-driven economic shocks to nationalist and anti-incumbent voting (??). That work studies import competition; the 2025 travel boycott 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% of its two-party vote for the Republican candidate, against 55.6% in less-exposed metros and the pronounced Republican tilt of the farm counties that absorbed the earlier retaliation.[13] The three most exposed metros—Buffalo, Detroit, and Bellingham—all lean Democratic, a contrast ? 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.

[12]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 five to ten 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.

[13]County presidential returns for 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% Republican) to within a tenth of a point. This is a descriptive cross-tabulation, not a statement about how exposure affected voting.

That map does not obviously carry less political weight: the exposed metros are concentrated in presidential battleground states (36% of the top-exposure decile against 19% of the rest, driven by the Michigan crossings), and because localized economic shocks move votes (???), a cost falling on swing-state metros need not lack electoral leverage. Whether these communities translate loss into pressure on the administration is a transmission question this cross-tabulation does not answer.

10 What blowback means for coercion strategy

Four implications follow, each resting on a result established above. First, a decentralized response can impose a cost the coercing state bears. It needs no symmetric chokepoint and no formal state retaliation, and it is hard to negotiate away because no single actor controls it, though official cues that amplified it might in principle help unwind it (Section ??). The cost also did not ease when the instrument did: the February 2026 judicial reversal of the tariffs restored none of the lost travel in its first months, though that window is short (Section ??). The weaponized-interdependence account of hub power should be read alongside its complement, the return flow of cost when the hub's own consumers abroad withdraw. Recovering that flow is also a measurement problem: it is concentrated enough that a coarse binary treatment misses it entirely (Table ??), so analysis of blowback should expect concentrated, mode-specific incidence and measure for it accordingly.

Second, the observable costs were asymmetric. The concentrated loss fell on the U.S. side, while on the Canadian side travel was redirected rather than forgone: overseas trips rose about nine percent during 2025 (Figure ??) and remained elevated a year later (Section ??). We do not measure Canadian welfare, and some forgone trips surely had no close substitute, so this is a statement about observable travel flows, not a welfare comparison.

Third, the bite is bounded by the target's share of local demand and the depth of replacement demand (Section ??); a target choosing where to press, and a coercer estimating the cost at home, both need the mode-specific map rather than the national aggregate. Fourth, the domestic politics are asymmetric: because a decentralized response lands where the target's spending was, not where a retaliating government would aim, the travel boycott reached a different electoral map than the 2018–19 agricultural retaliation (Section ??), and a coercer expecting blowback to fall where tariff retaliation once fell will misjudge where this cost lands—though whether it reaches the administration as political pressure our data leave open. These implications hold only where their preconditions are met: a target public with high-frequency discretionary consumption inside the coercer and a mobilizable national identity, which Canada satisfies unusually well.

11 Conclusion

When the United States coerced Canada in 2025, part of the cost returned to the United States through a sharp decline in Canadian travel and fell on land-border communities. We measure that blowback and tie it 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 is travel-mode-specific and geographically concentrated: leisure and hospitality employment fell with land-crossing exposure, while air-served Sun-Belt destinations were spared, Canadian traffic being too small a share of their demand to register in employment when it withdrew. 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; the national net is smaller, because the air leg of the withdrawal left no measurable loss. 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.

The contribution is to price this cost—a measurable, place-concentrated, mode-specific return flow on the coercer—without adjudicating whether it changed U.S. conduct, which a single observational episode cannot settle. This case also supplies a template for the next confrontation, where the return flow may be harder to trace: a predetermined exposure gradient, continuous and decomposed by travel mode, paired with a second border for falsification and inference built for few treated units.

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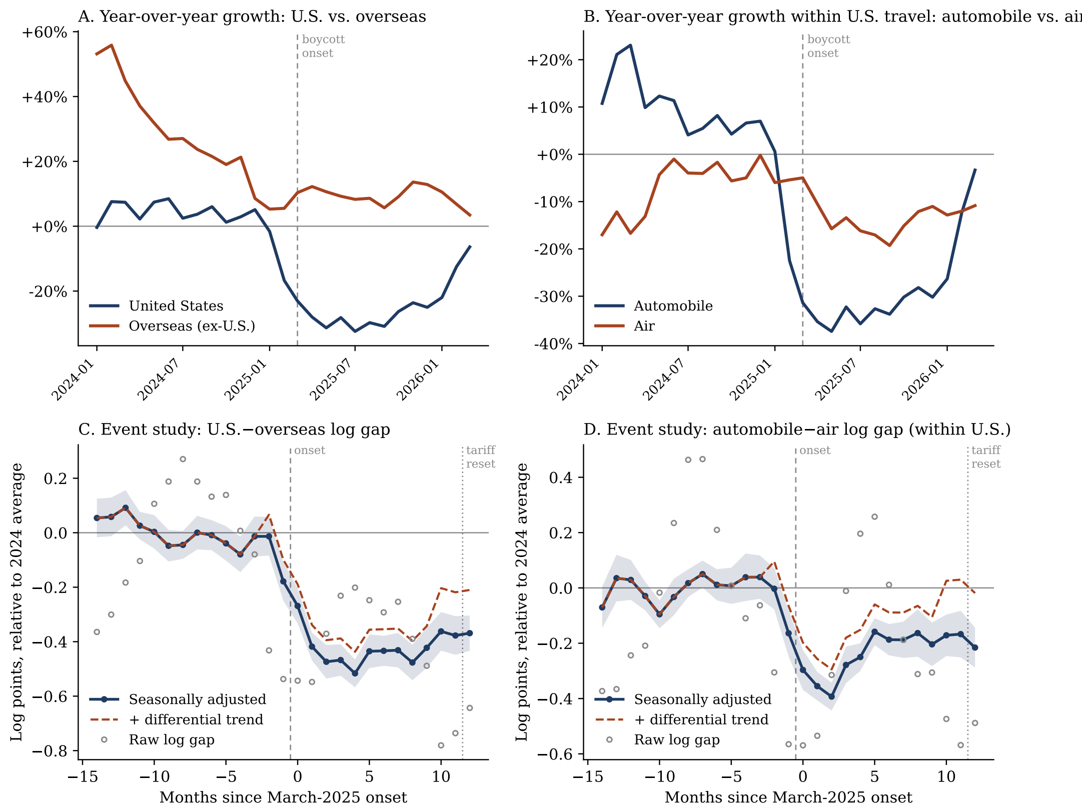

Figure 1. The Canadian Pullback Was Specific to the United States

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), 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 (**?**). 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.

Table 1. Summary Statistics

	Mean	SD	10th	Median	90th
Leisure & hospitality employment (000s)	41.8	89.9	5.0	14.6	102.2
Total nonfarm employment (000s)	391.3	867.5	48.8	127.5	1,064.9
Canadian air-destination share (%)	0.2	1.3	0.0	0.0	0.2
Distance to nearest Canada crossing (km)	853	650	175	711	1,695
Distance to nearest Mexico crossing (km)	1,601	811	451	1,644	2,626

Notes: Metro cross-section, 390 metropolitan areas, 15,990 metro-months (forty-one months, January 2023 through May 2026). Distributional moments are computed over the 352 metros with centroid coordinates that form the continuous-exposure estimation cross-section; employment is the 2024 metro mean. Ten metros constitute the legacy binary land-border set and 46 carry a positive Canadian air-destination share; 15 metros lie within 100 km of a Canada land crossing and 11 within 100 km of a Mexico crossing. The border-county panel covers 649 counties in the thirteen border states plus Alaska.
Sources: BLS Current Employment Statistics; U.S. NTTO Survey of International Air Travelers; Bureau of Transportation Statistics Border Crossing data; author's calculations.

Table 2. Continuous-Exposure Difference-in-Differences, Leisure and Hospitality Employment

Exposure (+1 SD × post)	Coef./(SE)	CRV1 p	Perm. p
Land-crossing volume (BTS)	−0.0057	< 0.001	0.001
	(0.0015)		
Border proximity	−0.0039	0.004	0.011
	(0.0014)		
Air-destination share	+0.0009	0.085	0.520
	(0.0005)		
Binary border indicator	−0.0086	0.153	0.309
	(0.0060)		

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 log points ($-0.0057 \approx -0.57\%$). 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 **??**), permutation is the preferred read where the two disagree, and Table **??** 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.

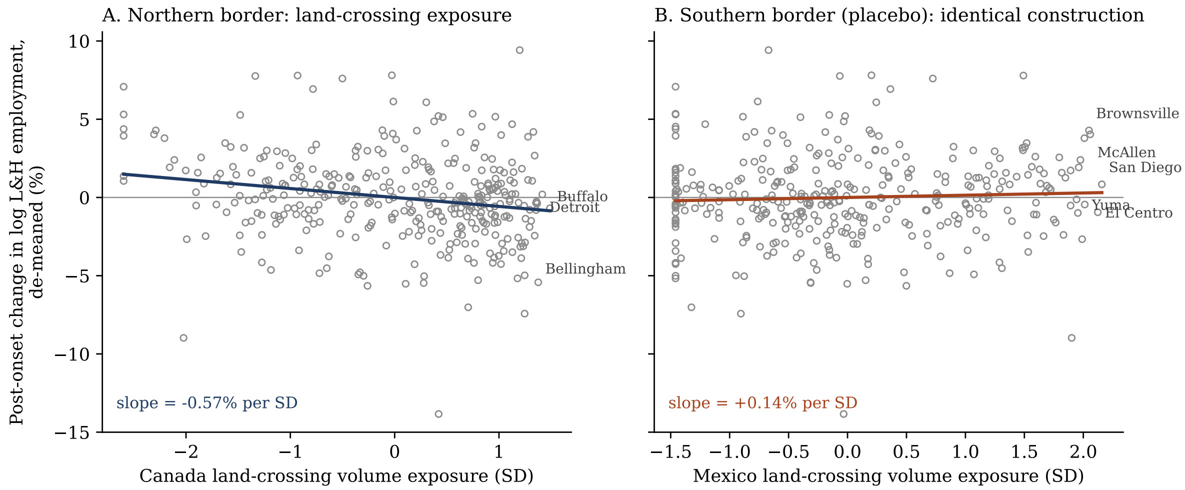

Figure 2. The Cross-Section Behind the Difference-in-Differences

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 **??**, permutation $p = 0.001$) and +0.14% for the identically constructed Mexican exposure (Panel B; Table **??**, permutation $p = 0.33$), the southern-border placebo of Section **??**. The most exposed metros on each border are labeled. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

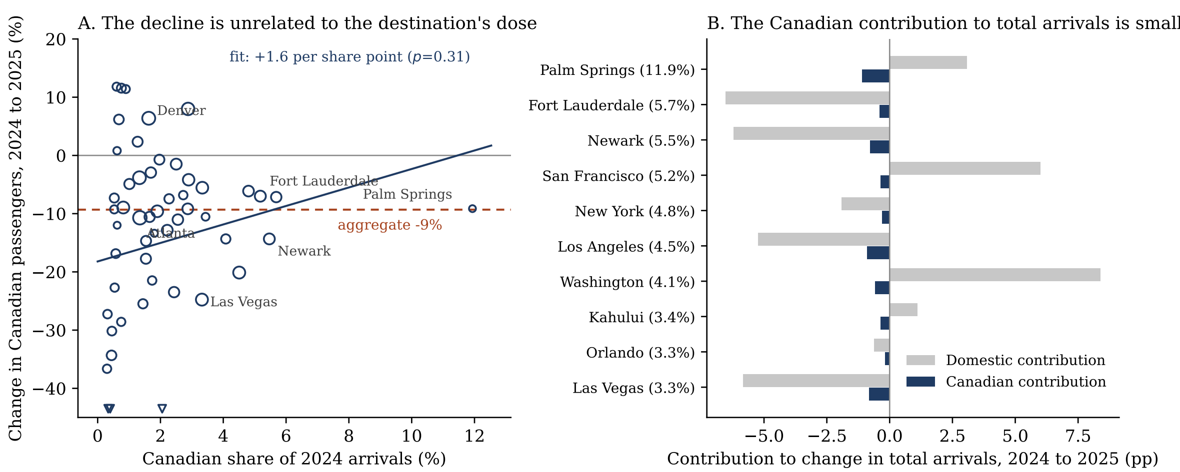

Figure 3. The Size of the Air-Destination Dose

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 is proportional to 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 ($\geq$ 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 3. Northern Border versus Southern-Border Placebo

Exposure (+1 SD $\times$ post)	Coef./(SE)	CRV1 p	Perm. p
Panel A. Northern border (Canada): headline			
Land-crossing volume	-0.0057^{***}	< 0.001	0.001
	(0.0015)		
Border proximity	-0.0039^{***}	0.004	0.011
	(0.0014)		
Panel B. Southern border (Mexico): placebo			
Land-crossing volume	+0.0014	0.307	0.329
	(0.0014)		
Border proximity	+0.0014	0.415	0.302
	(0.0018)		

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. 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. *** denotes significance at the 1 percent level.

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

Online Appendix

I Robustness and supplementary constructions

Table ?? consolidates the falsification and robustness checks summarized in Sections ?? and ??, each re-estimating the headline land-crossing-volume gradient under an alternative specification, sample, or outcome transformation; the estimate is stable in sign and order of magnitude throughout. The subsections that follow give the detail behind the checks Sections ?? and ?? 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. Conley spatial-HAC standard errors (?), 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). 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 (?); at the coarser nine-division level the wild bootstrap is insignificant ($p = 0.12$, few enough clusters that the test has little power, ?). 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%. 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 Labor demand versus supply

The reading in Section **??** that the tourism-employment decline reflects a demand shift rather than a withdrawal of labor supply rests on the border-county QCEW, and two caveats keep it a reading rather than a finding. In a county difference-in-differences (county and quarter fixed effects, confidentiality-suppressed cells coded as missing), the log-wage gradient on land-crossing exposure is null to slightly negative and the log-establishment gradient is flat; a supply-side withdrawal of workers would instead have bid wages up. But average weekly wages confound the price of labor with its composition, so a smaller, higher-tenure workforce can hold the average up even as demand contracts, and flat sub-annual establishment counts are weak evidence against firm exit. A Quarterly Workforce Indicators worker-flow test is too coarse to separate a hiring freeze from elevated separations (eight to eleven border-state clusters, one post-quarter), though it shows no separation spike.

I.D The bilateral exchange rate

Section **??** rules out a bilateral Canadian-dollar move as the driver of the U.S.-specific travel decline; Figure **??** gives the underlying series. The loonie depreciated about 4.8% against the U.S. dollar between the 2024 average and January–March 2025, but only 1.9% 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%. At the day-trip real-exchange-rate elasticities of **?**—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%. The currency also runs the wrong way for persistence: through early 2026 crossings held about 20% 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 (**?**).

I.E Third-country arrivals

Section ?? 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%, 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%; Mexican and Japanese arrivals rose. 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%, France −7.7%, with several smaller European and Oceanian origins deeper—and Canada's own air arrivals, down 11.4%, sit inside that band rather than above it. The gap between Canada's 20.2% all-mode decline and its 11.4% 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 ??. The test cannot separate the boycott from other U.S.-specific land-crossing deterrents such as border-enforcement apprehension, which is why Section ?? identifies the redirection rather than a single motive.

I.F 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 (?) 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.[14] 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 (?), which is why we read the headline as a

[14] 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.

descriptive association and the job counts as an upper bound (Section **??**). 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 (+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 **??**), and the placebos. Figure **??** 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 **??** 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.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 **??** 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% of Canadian traffic). Canada-to-U.S. passengers fell from 15.9 to 14.4 million between 2024 and 2025, or 9.3%, 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 −0.0077 log points, $t = -4.3$. 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% against 6.4% 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 **??** displays the airport-level pattern. Canadian passengers were 3.3% of Las Vegas's 2024 arrivals and 2.9% 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% at the exposed metros and 1.3% 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% year over year) while the most land-exposed metros shed them (−0.16%); 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 **??** 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 **?**. 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 $p < 10^{-3}$. 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 **??** 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 31,570 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 **??**.

I.K The identifying geography

Figure **??** 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 **??**.

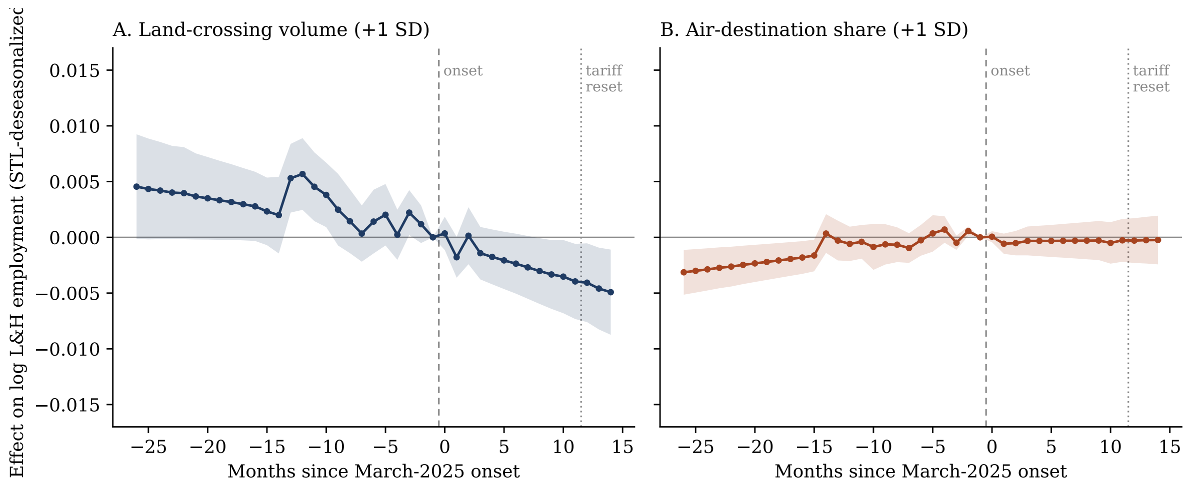

Figure A1. Event-Study Gradients on the Deseasonalized Series

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 **??**). *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

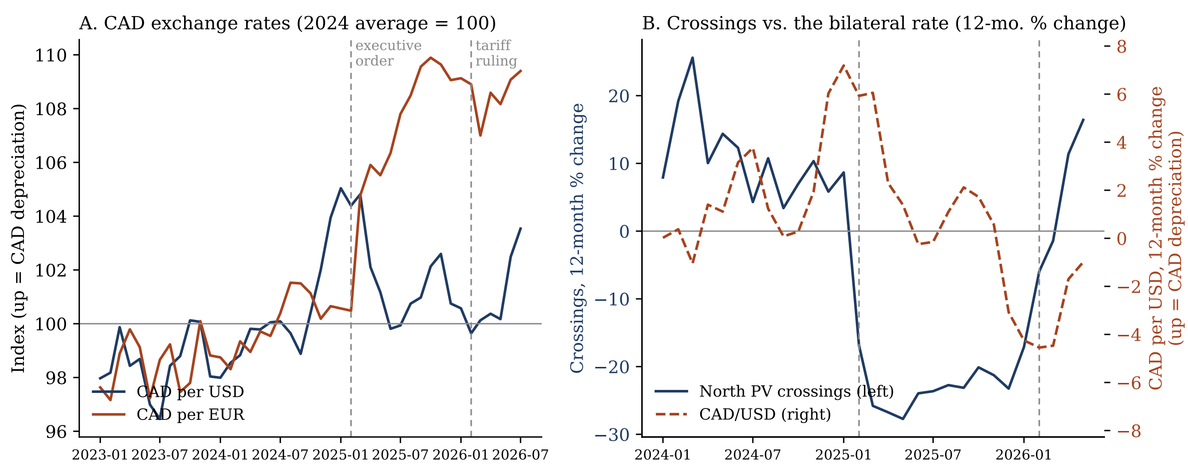

Figure A2. The Canadian Dollar Cannot Account for the Travel Decline

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 noon rates, 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; crossings fall roughly 20–24% and stay down after the currency has recovered. *Sources:* Bank of Canada; Bureau of Transportation Statistics; author's calculations.

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

Specification	Land-volume gradient	Inference / note
Baseline (Table **??**)	−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
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); no pre-period gradient
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)	—	$p < 10^{-3}$
Land vs. air difference (pooled, formal)	—	volume $t = -4.1$ ($p < 10^{-4}$); prox. $t = -3.2$ ($p = 0.001$)
air gradient 95% CI	[−0.08%, +0.14%]	reject air loss as large as land point estimate

Notes: Each row re-estimates the headline two-way fixed-effects gradient on log metropolitan leisure and hospitality employment for a one-standard-deviation increase in distance-allocated Canadian land-crossing volume interacted with the post-March-2025 indicator, under the stated variation. The metro-by-calendar-month row absorbs each metro's own seasonal pattern; the Conley row reports spatial-HAC standard errors (Bartlett kernels in space and time); the wild-cluster-bootstrap and spatially-blocked-permutation rows allow arbitrary within-region spatial correlation; and the buffer row drops low-exposure control metros within 150 km of a treated metro. The substitutability rows interact land exposure with a gravity index of other U.S. metros'

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	−0.0046	0.002	−0.0023	0.027
100	−0.0053	< 0.001	−0.0029	0.016
150 (volume)	−0.0057	< 0.001	−0.0034	0.009
200 (prox.)	−0.0057	< 0.001	−0.0039	0.004
250	−0.0055	< 0.001	−0.0043	0.002
300	−0.0052	< 0.001	−0.0046	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). 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.

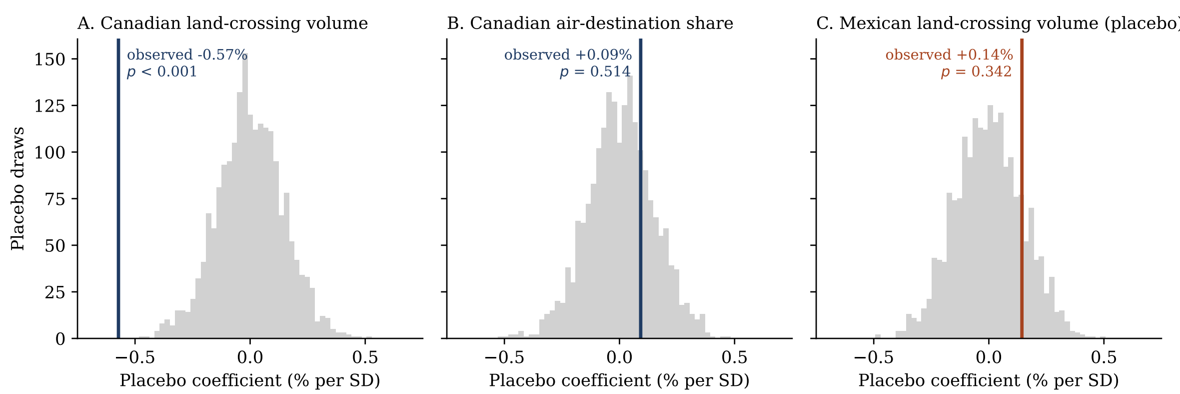

Figure A3. Randomization Distributions for the Exposure Gradients
Notes: Each panel reassigns the static metro-level exposure at random across the 352-metro cross-section 2,000 times, re-estimates the two-way fixed-effects gradient of Table **??** under each placebo assignment, and plots the resulting 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 **??** and **??**, 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.

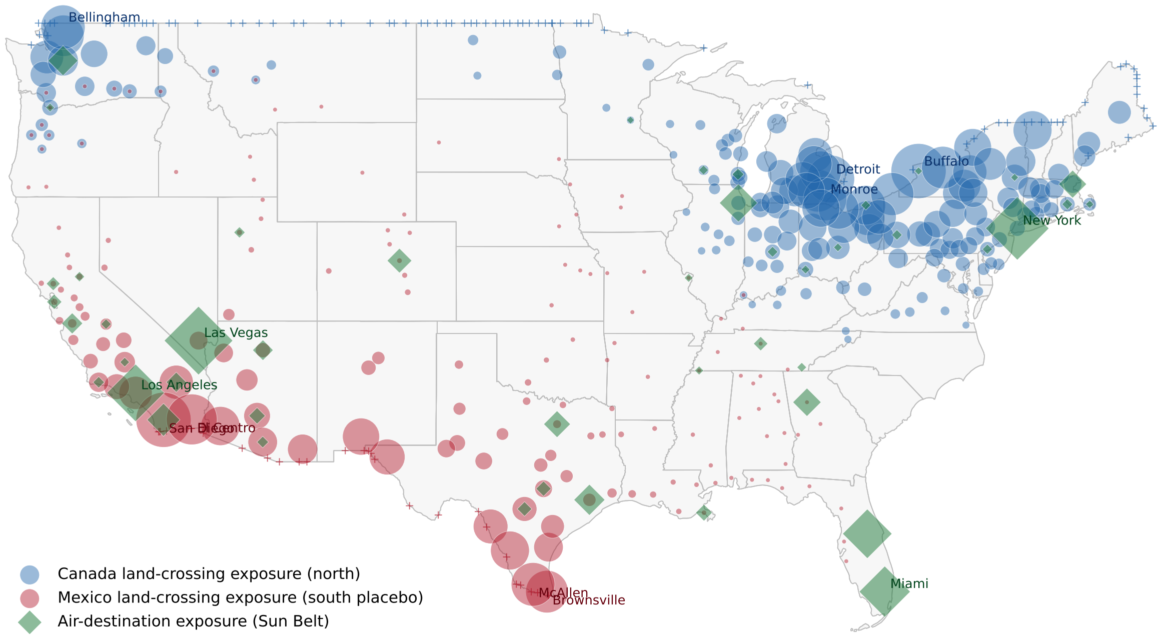

Figure A4. The Identifying Geography on Both Borders

Notes: Each marker is a metropolitan area in the estimation panel. Circle area is land-crossing exposure, the distance-allocated 2024 inbound personal-vehicle volume at the nearest Canadian (blue) or Mexican (red) ports of entry; 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 **??**; the air-destination margin provides the mode contrast of Section **??**. *Sources:* Bureau of Transportation Statistics; U.S. NTTO; Census cartographic boundaries.