

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 paired tariffs on Canada with public statements questioning Canadian sovereignty, and Canadian consumers answered with a decentralized travel boycott: return trips from the United States fell by a quarter while trips overseas rose. We estimate the cost of that boycott—the consumer response, set apart from provincial delistings and federal counter-tariffs—inside the coercing country, using predetermined cross-border-visitor exposure across 352 metropolitan areas. Tourism-specific employment fell about 0.3 percent per standard deviation of land-crossing exposure, with no loss at Canadian air destinations (a land–air difference of about 0.6 percentage points), a gross local loss of roughly six to thirty thousand jobs concentrated in land-border communities—one job per six hundred to fifteen hundred foregone trips, and about two to eleven cents of earnings per dollar of tariff revenue collected on Canadian goods. A southern-border placebo ties the loss to the boycott rather than the tariffs: Mexico faced identical tariffs, saw no withdrawal, and shows no gradient. The gradient did not narrow after the courts struck the tariffs down in February 2026. A substantial part of the cost of the coercion, then, fell inside the country that imposed it.

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 August 2026.

1 Introduction

It has long been understood that economic coercion imposes costs on the sender as well as the target (Baldwin, 1985; Drezner, 1999). The literature on economic statecraft concentrates on the coercer’s instruments and the target’s compliance (Farrell and Newman, 2019): who holds the chokepoint, whether sanctions change behavior, and who bears the cost inside the target state. Yet the costs that flow back to the coercer have been measured only at the level of national trade and financial flows (Crozet and Hinz, 2020; Besedeš et al., 2017), and very little is known about where they land inside the coercing country. 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. As far as we are aware, that response has not been traced to the coercer’s own local labor markets; that gap is the one we address here. The 2025 confrontation between the United States and Canada is an unusually observable case.

We do two things in this paper. First, we measure the Canadian response itself. Because the response is visible in frontier counts, travel surveys, balance-of-payments mirrors, administrative passenger data, and local tax collections, the episode supports a more complete description of a decentralized coercion response than any prior case, and most of what the description shows is measured directly rather than estimated. Second, we trace where the cost of that response landed in U.S. communities, pairing 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.

On the one hand, the response was close to costless for the Canadians who imposed it, since travel was redirected rather than forgone. On the other hand, it was expensive for particular American places: about 23 cents of Canadian spending in the United States were withdrawn per dollar of tariff revenue collected on Canadian goods over the same months—gross spending rather than a welfare measure, since tariff revenue is a transfer—and two to eleven cents of U.S. earnings lost; the loss is concentrated in a handful of land-border communities. Because no single actor controlled the response, the cost persisted after its trigger was removed; crossings through July 2026 remained nearly a fifth below their 2024 level, and the employment gradient did not narrow, after the courts struck the tariffs down in February 2026. And the cost reached a different electoral map than state-directed tariff retaliation: communities that leaned against the governing party, concentrated in presidential battleground states. Section 12 develops what these facts imply for how the costs of coercion should be counted.

We present the description before the incidence estimates because it rests on direct measurement rather than on identifying assumptions; it is the better-measured half of the paper. The Canadian reaction arrived mainly through a collapse in travel south, with provincial liquor monopolies pulling American whiskey in parallel. Canadian return trips from the United States fell about a quarter over the year while trips overseas rose, a redirection rather than a retreat from foreign travel (Statistics Canada, 2026). The withdrawal was selective, sparing family visits while discretionary travel collapsed, and it ran on the extensive margin: fewer trips, not shorter ones, carried the roughly US\$2 billion net withdrawal over the last three quarters of 2025. Section 4 assembles these margins source by source and closes with a single table that runs every data set through the same

exposed-versus-comparison design.

The central empirical difficulty is attribution. Tariffs and the boycott landed on the same border communities at the same moment, and the exposed metros are also the Great Lakes industrial corridor, so a decline near the crossings is consistent with several stories. Much of this paper is devoted to separating them. The estimate we carry is the within-metro triple-difference, which sets tourism-adjacent sectors against other sectors in the same metro and month: -0.30 percent per standard deviation of land-crossing exposure. The raw metro-level gradient is larger, about 0.57 percent, but it carries a regional trend that pre-dates the episode and stands only as an upper bound.

The mode decomposition maps where the cost fell. Air-served Sun-Belt destinations with high Canadian exposure show no detectable loss: Canadian traffic was too small a share of arrivals to register, a dose argument Section 8 develops with administrative passenger data. The incidence concentrates on the land day-trip margin, where the local service economy depends on Canadian visitors.

The decline also survives the rival explanations. A general Canadian retreat from foreign travel is contradicted by the overseas comparison; the tariffs are contradicted by a southern-border placebo on the identically tariffed Mexican border; regional trends and the metro’s own tariff shock are absorbed or controlled without attenuating the estimate. Section 9 takes them in turn.

We build directly on Kurmann et al. (2026), who provide the first U.S.-local estimate of the 2025 tourism collapse: roughly a 6 percent employment contraction at the most Canadian-exposed establishments, with a visitor-exposure measure pooled across land and air because the Canadian withdrawal was nearly uniform across crossing modes. Our contribution relative to that benchmark 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 show that the uniform withdrawal maps into a sharply *non-uniform* employment incidence. Beyond that benchmark, we document the margins a pooled foot-traffic design cannot see: the withdrawal’s purpose selectivity, its extensive-margin structure, its persistence past the tariff reversal, its sales-tax incidence, and the dose mechanism behind the air null. To these we add the Canadian-side overseas comparison that the U.S.-side data alone cannot supply. And we quantify the incidence at roughly six to thirty thousand leisure and hospitality jobs in exposed land-border communities, a gross local loss with the national net smaller still. None of this makes the estimates experimental: the boycott is not randomly assigned across places, so what follows are associations, and Section 10 states what they do and do not establish.

In Section 2 we describe the episode, and in Section 3 we develop the framework and derive three testable predictions. Section 4 measures the withdrawal itself—its direction, selectivity, margins, dollars, persistence, and cost to those imposing it—and collects every source in a unified design. The data and incidence design are presented in Sections 5 and 6. In Section 7 we show that tourism-adjacent employment fell about 0.3 percent per standard deviation of land-crossing exposure, and in Section 8 that the identically exposed air destinations show no loss; Section 9 shows that neither incomes, exchange rates, nor the tariffs themselves account for the pattern; Section 10 states what the estimate can and cannot claim; and in Section 11 we quantify the incidence before Section 12 concludes the argument.

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 4.C. 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. What the withdrawal these events produced looked like in the data—its direction, timing, selectivity, and dollars—is the subject of Section 4.

2.B Two instruments, two margins

The Canadian reaction ran through two channels—a travel withdrawal and a wave of provincial spirits delistings, of which the Ontario board’s removal of roughly 3,600 U.S. products was the largest. U.S. spirits exports to Canada fell by about half over March–December 2025, with no

¹Timeline drawn from the underlying executive orders and contemporaneous reporting; Canada’s March 4 counter-tariff covered a first \$30 billion tranche of U.S. goods.

²Angus 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. A separate question in the same release found 85 percent replacing, or planning to replace, U.S. products with Canadian ones.

recovery through May 2026; Brown-Forman reported its Canada sales down 62 percent, its chief executive calling the action “worse than a tariff.”³ 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, and the dose is too small to register in headcount: Canada took roughly \$110 million of Kentucky and Tennessee spirits exports in 2024, and 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 falls on revenue by construction, the producer-side counterpart of the dose argument of Section 8, so we keep the channel to the side of the employment estimates. 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 a cost the coercion literature 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. Sender-side costs enter the statecraft 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.⁴ We localize one such cost, which arrives through the target public’s redirection of its own consumption rather than through the sanctioned good, and extend the relevant actor set from the state to

³Provincial 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. The spirits-export figure is from Census trade data and includes Kentucky and Tennessee. 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.

⁴Baldwin (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 accounting.

consumers and provinces. 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); as far as we are aware, 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, 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 9). 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 and persistence evidence of Section 4 bears on the second prediction; the incidence estimates of Sections 7 and 8 bear on the first and third.

4 The withdrawal measured

This section measures the withdrawal itself. Every number in it is a direct reading of frontier counts, survey aggregates, balance-of-payments accounts, passenger data, or administrative tax collections. None rests on the identification assumptions the incidence estimates require, so the description stands with more confidence than any gradient that follows.

4.A A U.S.-specific redirection

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 9 uses this contrast as

falsification evidence.⁵ 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.⁶

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. The timing points to the sovereignty dispute, not to relative prices.

4.B Purpose selectivity and the intensive margin

The survey side adds margins 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 and visits for other purposes about 38 percent, while visits to friends and relatives fell about 12 percent, so the trips that survived skew toward family travel. The selectivity runs along the discretionary margin, and family travel is the one category that held; it does not isolate leisure. The surviving overnight trips were not shorter, but spending per night fell while rising on overseas trips, and restaurant-and-bar spending in the United States fell by a quarter against a rise overseas.⁷

The same survey prices the withdrawal. 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. A Bennet decomposition over trip cells—which splits the spending change into the part due to fewer trips and the part due to changed spending per surviving trip—attributes the entire decline to the extensive margin: relative to a hypothetical world in which trip counts stayed at their 2024 level, fewer trips contribute $-C\$4.2$ billion, while the longer surviving trips offset C\$1.25 billion. The withdrawal was almost entirely fewer trips. The dollars, like the visits, withdraw from discretionary rather than family travel: spending on visits to friends and relatives

⁵Kurmann 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.

⁶Statistics Canada frontier counts (Table 24-10-0053, traveller-type dimension): same-day automobile trips -33.3% and overnight -32.2% over March–December 2025 against 2024, with neither recovering after the February 2026 ruling.

⁷Statistics Canada Table 24-10-0070 (National Travel Survey), year-over-year changes averaged over 2025Q2–Q4; survey aggregates, quoted without sampling inference. Nights per visit rose about 9 percent as short discretionary trips exited the mix; spending per night fell about 5 percent in nominal Canadian dollars against a 7 percent rise on overseas trips, and early-episode CAD depreciation means the nominal decline understates the real one. The U.S.-side air-visitor survey shows the same signature independently: mean nights up about 6 percent, spending per visitor-day down about 10 percent (NTTO Survey of International Air Travelers, Canada).

rose (the purpose-level breakdown is in Online Appendix I.N). 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.⁸ For the employment estimates that follow, the selectivity 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.

4.C Persistence after the tariff reversal

The pullback outlasted the tariffs that accompanied it, which bears on the sentiment-versus-price reading, though it separates the two only partially. 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 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 muddled one, since a new instrument arrived the same week. The traffic responded slowly and only in part. Over the five post-reset months, March through July 2026, northern land crossings ran 18 percent below the same months of 2024, Canadian automobile trips ran 29 percent below theirs through June (the latest month the Statistics Canada series covers), and overseas trips remained elevated. A partial rebound did begin in the spring: from April 2026 crossings grew 9 to 16 percent year over year, but off the collapsed 2025 base, so that a year and a half after the boycott began, and months after its statutory trigger was gone, roughly two-thirds of the gap remained.⁹ 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 largely unresponsive to them on the way out. The employment counterpart of this persistence, the exposure gradient split at the reset, appears with the incidence estimates in Section 7.

4.D A unified design across sources

Table 1 closes the description by collecting every data source the episode touches 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, 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 incidence geography developed below predicts should not move: the dose argument of Section 8 implies no

⁸BEA International Transactions Accounts, travel services exports, Canada, quarterly unadjusted; the roughly 20 percent mirror gap against the Canadian survey figure reflects coverage differences and is normal for services mirrors. Statistics Canada’s total travel-payments series *rose* about C\$0.9 billion over the window, the redirection in macro form. Construction detail and the job-equivalent benchmarks built from these figures are in Online Appendix I.N.

⁹Land day-trips are the fastest-adjusting travel margin, with no booking lag, so the slow unwinding is informative about the response’s stickiness rather than about frictions in travel itself.

air-metro imprint, and the synthetic-control case studies of Online Appendix I.K find no Buffalo divergence. The revenue-side rows are the sharpest corroboration outside the employment data. 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.

The description carries one more fact worth stating before the incidence estimates: the withdrawal was close to costless for those imposing it. Canadians did not stop traveling; they traveled elsewhere, with overseas trips up about nine percent in the boycott year and still elevated after the reset. Scaled against tariff revenue, about 23 cents of Canadian spending in the United States disappeared per dollar the Treasury collected on Canadian goods over the same months—gross spending rather than a welfare measure, since that revenue is a transfer; the earnings counterpart is two to eleven cents (Section 11).¹⁰

5 Data

5.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 $\sum_p w_{mp} \text{vol}_p$ 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 $\exp(-\text{dist}/\tau)$, $\tau = 200$ 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

¹⁰Construction: the numerator is the NTS-measured US\$2.1 billion decline in Canadian spending in the United States over 2025Q2–Q4 (Section 4); the denominator is the \$9.3 billion of calculated duties the United States collected on Canadian goods over the same months, April–December 2025 (Census HS6 calculated duties; the March start of the tariffs falls outside the survey window). Both sides are in U.S. dollars over the same window. Annualizing the numerator and dividing by the full March–December revenue of \$9.9 billion gives 29 cents; we quote the window-matched figure. Computed in the replication package (`boycott_ext/08`).

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.J).

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 9 traces the southern border at San Diego, El Paso, Laredo, and the lower Rio Grande Valley.

5.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.¹¹ The border-county panel covers 649 counties in the thirteen border states plus Alaska.¹² Online Appendix Table A1 reports summary statistics for the metro cross-section.

6 Empirical design

Onset is dated identically in every specification, with $\text{post} = 1$ from 2025-03 onward, the month of the first broad wave of delistings and the visible start of the travel pullback.¹³ To estimate the

¹¹The 38 metros dropped for want of a centroid are almost all metropolitan divisions whose parent areas remain in the sample. In the continuous sample Detroit enters once, as its metropolitan statistical area; dropping it leaves the headline gradient at -0.58% .

¹²Every input is public: Bureau of Transportation Statistics Border Crossing data, BLS Current Employment Statistics and the Quarterly Census of Employment and Wages, the NTTD 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.

¹³The result does not depend on this date: onsets from December 2024 through April 2025 leave the deseasonalized land-volume gradient between -0.51% and -0.56% (Online Appendix Table A2). The check is read on the

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

$$y_{mt} = \beta (\text{exposure}_m^z \times \text{post}_t) + \alpha_m + \delta_t + \varepsilon_{mt}, \quad (1)$$

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. In other words, β answers the question: after March 2025, did employment fall faster in the metros that had depended more on Canadian visitors? 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.

That assumption is strong, because the high-exposure metros are also the Great Lakes industrial corridor. We therefore take equation (1) as the descriptive starting point and the same specification run *within* metro and month as the estimate. Stacking two outcomes per metro-month, leisure and hospitality against the rest of nonfarm employment, and adding metro-by-sector, metro-by-month, and sector-by-month fixed effects leaves a coefficient identified only off the differential movement of tourism-adjacent sectors inside a single metro in a single month. Any northern-tier recession, any metro-wide demand shock, and any place-specific seasonal pattern is absorbed by construction. Section 7 reports both.

The inference challenge is spatial. The high-exposure metros all lie on the northern tier, so cluster-robust standard errors over-reject and the many-shifter shift-share asymptotics of Adão et al. (2019) do not apply. The main tables therefore report permutation inference, which reassigns the static exposure across metros and invokes no large-cluster asymptotics, alongside CRV1 standard errors; Online Appendix I.A confirms the land-volume gradient under Conley spatial-HAC standard errors and under state-level and spatially blocked wild-cluster and permutation tests (Conley, 1999; MacKinnon and Webb, 2018). The land-volume coefficient falls outside all 2,000 placebo exposure assignments, while the air and southern-border coefficients sit near the center of theirs (Online Appendix Figure A4).

Three limitations are worth stating at the outset: the boycott is not randomly assigned, so the design recovers an association rather than an experiment; the metro series are not seasonally adjusted, though the result survives explicit seasonal adjustment (Section 9); and the static coefficient inherits the pre-trend sensitivity of its event-study path, which Section 10 takes up.

7 The land-border decline

Table 2 reports the core estimates. The estimate this paper carries is the within-metro triple-difference of Panel B, -0.30% per standard deviation; Panel A’s metro-level gradients are the descriptive starting point and, as this section shows, an upper bound. In Panel A 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. Figure 2 plots the cross-section behind the coefficient, each metro’s post-onset change in log leisure and hospitality

deseasonalized series because an earlier onset mechanically draws more winter months into the post window on the raw series.

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: concentrated blowback is detectable only through the continuous predetermined gradient.

That gradient is not, on its own, a tourism result. Run the same specification on the 87 percent of metro employment that is *not* leisure and hospitality and exposure still predicts a 0.28% decline, with the largest sectoral declines in wholesale trade and financial activities, neither of which a day-trip reaches. Absorbing census-division-by-month effects removes it. The non-tourism gradient falls to an insignificant 0.12% while leisure and hospitality holds at 0.45% . The confound is regional, not a visitor-economy multiplier (Online Appendix I.E).

The within-metro triple-difference, constructed as Section 6 describes, removes exactly this. The residual movement is correctly signed and significant: a tourism-specific land effect of -0.30% per standard deviation (Table 2, Panel B; permutation $p = 0.046$; cluster-robust and wild-cluster-restricted $p = 0.072$ and 0.077)—about 16,400 jobs when extrapolated across the exposed metros, or 6,500 on the visits-based construction (Section 11)—while general tourism dependence in the baseline two-way design runs positive ($+0.37\%$, $p = 0.002$). One inference qualification attaches to this estimate, stated in Section 10.

The design has its own placebo, and it is the sharpest test in the paper. Run the identical triple-difference on the southern border—where the same tariffs applied but crossings did not decline—and the Mexico-exposure $\times$ tourism $\times$ post coefficient is -0.11% , insignificant under every method we apply (cluster-robust $p = 0.45$, permutation $p = 0.46$, state-blocked permutation $p = 0.47$), against the northern anchor’s -0.30% .¹⁴ A shared secular trend in border-tourism employment would produce a gradient on both borders. It produces one only where the visitors stopped coming. We therefore read the -0.30% as the estimate and the metro-level -0.57% as an upper bound that carries a regional component with it; Section 10 sets out what each supports. The -0.30% is itself conservative in one direction: if the boycott reached the control sector through local spending, the differential understates the loss. Two finer cuts, county QCEW sub-sectors and synthetic-control case studies of the most day-trip-dependent metros, sit in Online Appendix I.K.¹⁵

Administrative revenue data corroborate the map from outside the employment system. Blaine, the Washington port town most dependent on Vancouver day trips, shows an 8.7 log-point relative decline in taxable retail sales against interior comparison counties, while the New York border counties, whose metros sit in deeper domestic markets, show none (Table 1); and in the synthetic-control case studies of Online Appendix I.K, Bellingham—remote, Vancouver-dependent—diverges 2.7 percent below its synthetic while the higher-market-potential Buffalo does not. Both patterns reproduce the concentrated-incidence geography the gradient identifies, from tax collections rather than payrolls.

The decline is more consistent with an inward shift in tourism labor demand than with a

¹⁴The southern estimate uses the same metro-months and exposure construction, differing only in which border’s ports enter; deseasonalizing both sides leaves the contrast intact (-0.28% north, -0.12% south).

¹⁵The county cross-border-retail gradients are insignificant once confidentiality-suppressed cells are coded as missing rather than as zeros; coding them as zeros would fabricate a spurious gradient. The metro leisure-and-hospitality headline is unaffected, being built from the published CES metro series.

withdrawal of labor supply: average weekly wages in the tourism sub-sectors do not rise with land-crossing exposure and establishment counts are flat, where a supply withdrawal would bid wages up. The evidence is consistent with a demand shift, though we cannot rule out compositional change in measured wages.¹⁶

The gradient outlasted the reset (Section 4.C). To measure the employment counterpart of the traffic persistence we extend the panel through July 2026, five post-reset months, and split the post period at the February 2026 reset. Table 4 builds column by column to the specification we prefer for this question, the $\text{division} \times \text{sector} \times \text{month}$ triple-difference of column (3): it compares tourism-adjacent employment against other sectors in the same metro and month, and lets the tourism-versus-rest differential trend vary freely by census division, so the estimate survives both the metro-wide shocks that the within-metro comparison removes and the regional differential trends that Section 10 identifies as the remaining threat. On that specification the differential is -0.22 percent per standard deviation of land-crossing exposure over the boycott-under-tariffs window and -0.52 percent over the post-reset months (permutation $p = 0.014$, state-blocked $p = 0.058$), and the post-reset deepening is itself significant: the test of post-reset against boycott-window returns $p = 0.007$ (0.062 state-blocked; Table A3). The boycott-window coefficient on this restricted window, -0.22 (permutation $p = 0.124$), is smaller and noisier than the full-sample triple-difference of -0.30 , as a shorter window under a more saturated specification should be; the two are consistent, and the persistence conclusion rests on the split, not on the window coefficient alone.¹⁷ Remarkably, employment in the exposed metros' tourism sectors was no better a year and a half after onset than during the boycott year itself, and the point estimate is worse. Figure 3 shows the monthly path: the differential builds through the boycott year to roughly -0.5 percent by February 2026 and then holds in a -0.3 to -0.5 percent band through July, months in which crossings were partially rebounding.¹⁸ We read the two facts together as lagged adjustment: local service employment followed the level of visitor demand, not its statutory trigger, and it had not yet followed the beginning of the demand recovery either.¹⁹

¹⁶Average weekly wages confound the price of labor with its composition, and hours per worker, the margin a demand shock moves first, are absent from the QCEW. 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.

¹⁷These estimates deseasonalize with calendar-month factors estimated on the pre-onset window only (2023–01 to 2025–02); the STL deseasonalizer used elsewhere is unreliable at the sample endpoint, where it manufactures a spuriously monotone post-reset deepening. Detail and the STL comparison path are in the replication package (`boycott_ext/57`).

¹⁸The pre-onset path in Figure 3 also shows the positive excursion around months -13 to -10 that Section 10 discusses: the distant leads drift in the direction that biases the post-onset break toward zero, not away from it.

¹⁹Robustness (Online Appendix Table A3): the metro-level and within-metro columns of Table 4 bracket the preferred estimate; the identical split on Mexican land-crossing exposure, where the same tariffs applied but no boycott occurred, shows no significant gradient in either window; and the post-reset coefficient of column (2), the plain within-metro comparison, carries state-blocked permutation $p = 0.16$, so the within-division contrast is what sharpens the inference. July 2026 employment is a preliminary CES estimate.

8 Travel mode and the size of the dose

The previous section located the cost at the land crossings. This section shows where the cost did not fall—the air destinations—and why: Canadian traffic was too small a share of demand there to register. 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 $+0.09\%$. Within census division the pooled index is -0.02% and indistinguishable from zero, while its land and air components remain -0.42% and $+0.11\%$. An analyst working with the combined measure would recover a muted, partly cancelled effect, or nothing at all. Pooling hides both halves of the result.

The air-destination gradient is statistically indistinguishable from zero: in Table 2 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.²⁰ Once the dose is measured the air null is close to arithmetic. Pooling both exposures and testing the difference directly, the land coefficient is distinguishable from the air at $t = -4.1$, and the air coefficient’s 95% confidence interval, $[-0.08\%, +0.14\%]$, rejects that air destinations lost as much as the land point estimate. This is the one result that does not rest on the regional comparison. Forcing identification within census division and month, where land and air metros are compared only against neighbors, the difference narrows but holds at -0.52 percentage points ($t = -2.3$, permutation $p = 0.007$).²¹ Nor is the null an artifact of measurement or thin support: rebuilt from administrative T-100 passenger data, estimated only on the positive-exposure metros, or estimated without Las Vegas, the air gradient stays small, positive, and insignificant, with the land–air difference unchanged (Online Appendix I.J).

Why did a real withdrawal leave no employment imprint at the air destinations? The answer the passenger data give is dilution rather than active substitution (Figure 4). 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 against ordinary movements in the domestic base (Online Appendix I.J). The dollars agree with the employment 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.N).

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

²⁰This 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.

²¹Online Appendix I.E, which also reports the state-by-month cut. That cut is uninformative rather than contradictory: with exposure clustered inside states the difference carries a standard error of 0.64 percentage points, so its confidence interval contains both zero and the baseline estimate. The proximity-versus-air contrast does not survive the division cut, consistent with proximity’s weaker showing throughout.

where it is a small share of a deep market. Interacting land exposure with domestic market potential makes this margin continuous: 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 $p < 0.001$; it separates Bellingham, a remote Vancouver-dependent crossing, from Detroit and Buffalo, large metros embedded in dense domestic markets (Online Appendix I.L).

9 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 them accounts for it. What the checks isolate is a redirection of Canadian travel away from the United States specifically; we use “boycott” for that redirection and its incidence, and return to the question of motive at the end of the section.

Income and exchange-rate effects. An income shock or a general depreciation reduces all foreign travel, and the observed pattern is a redirection away from the United States specifically (Section 4). The one version redirection alone cannot rule out, a *bilateral* Canadian-dollar 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, recovered to its 2024 level by early 2026, and at the day-trip elasticities of Chandra et al. (2014) accounts for only low-single-digit percentage points of the 23.7 percent crossing decline, which persisted after the currency had recovered (Online Appendix I.C, Figure A3). The currency cannot carry the decline.

A Canada-specific land withdrawal. It might be thought that 2025 saw a broad turn away from U.S. travel to which the Canadian pullback merely belonged. Arrivals by country of residence discriminate, and that is not what we find. Canada’s all-mode arrivals fell 20.2 percent, the third-largest drop among 38 origins and six times the overseas mean; a broad, shallow softening of inbound *air* travel is present across origins, and what is Canada-specific is the *land* withdrawal, the day-trip channel no overseas origin has. 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).

The southern-border placebo. Mexico was tariffed on the same 2025 schedule but faced no annexation rhetoric and no comparable “don’t travel” boycott, and the identical design run on the 28 Mexican land ports returns a clean zero at both the metro level and in the triple-difference (Section 7; Table 3; Figure 2, Panel B). 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, which

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. Division-by-month fixed effects retain 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 estimate away from zero, to -0.71% , with leave-one-border-metro-out stable; and deseasonalizing employment metro by metro with an STL decomposition leaves it at -0.55% ($p < 0.001$). The air gradient stays nonnegative throughout (Online Appendix I).

The motive mix. The organized “Buy Canadian” boycott is the redirection’s leading proximate cause, but the design does not separate it from other U.S.-specific deterrents—border-crossing apprehension, say—that moved the same way at the same time. 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.²² Second, across the 38 arrival origins of Online Appendix I.D, the only two declines larger than Canada’s 20.2 percent belong to Denmark (-26.3 percent), the other origin whose sovereignty was publicly questioned in the same months, and Venezuela (-20.4 percent), an origin with high enforcement salience. Origin-specific withdrawals of Canada’s size arise under both channels; we are agnostic on the motive and claim only the redirection and its incidence. The third is the withdrawal’s purpose selectivity (Section 4), and it cuts both ways: the resilience of family visits is hard to reconcile with a deterrent that priced border risk uniformly across trip purposes, but the 38 percent collapse of business-and-errand travel is at least as consistent with an enforcement channel as with a consumer boycott. The selectivity constrains the shape any account must take without discriminating between the channels.

10 What the gradient does and does not establish

A single concern governs how much the metro-level gradient can bear: a regional trend that pre-dates the episode. It arrives twice. In the dynamic path it appears as 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 (Online Appendix I). In the cross-section it appears as the sectoral breadth of Section 7. Both are the same regional trend seen from different angles, and differencing tourism against other sectors within the metro removes what is common to the metro.

A five-year pre-boycott window, 2015–2019, shows what is at stake in that difference. Over those

²²The DHS registration rule for stays of thirty days or more was published March 12, 2025 and took effect April 11; the most widely covered detention case drew national Canadian coverage in mid-March. The gap is -0.01 log points in January 2025 and -0.18 in February (Figure 1).

years land-crossing exposure predicts declining employment across essentially every sector, leisure and hospitality included (-0.79% per standard deviation, about a third of a percentage point a year), while the tourism-minus-other-sectors differential is flat (-0.14% , $p = 0.35$) with the sign running against the estimated effect. The pre-period trend is metro-wide, and it is exactly what the within-metro comparison differences out; two qualifications, a Great Lakes-only differential of -0.45% and one rejected mid-2024 placebo lead test, are reported in Online Appendix I.H. The triple-difference’s remaining pre-trend record—immediate leads flat under design-based tests, distant leads drifting in the direction that biases toward zero, the estimate holding when the tourism-versus-rest trend varies freely by division, and an insignificant southern-border version—is in Online Appendix I.G.

The three gradients differ because they absorb different amounts of regional variation, so they form a ladder rather than competing estimates. The metro-level -0.57% is a descriptive association carrying a regional component that pre-dates the episode, and stands as an upper bound; absorbing division-by-month effects leaves -0.45% , at the cost of about three-fifths of the identifying variation; the within-metro triple-difference, -0.30% , carries the strongest pre-trend record and is conservative to the extent that local spending reached the control sector, though it is imprecisely estimated: under the spatially blocked permutation of Online Appendix I.A, which holds each placebo draw as spatially clustered as the real exposure, its p rises to 0.10–0.18. What the paper establishes across all three is a tourism-specific decline at the land crossings and its absence at the air destinations. What it does not establish is a causal magnitude, and we do not attempt more precision than the design can carry: the mode contrast, not any one coefficient, is the result to carry away. Border proximity does not survive the within-division cut, so we use it as corroboration only and do not let it anchor magnitudes.

11 Magnitude and incidence

11.A Jobs, dollars, and the cost-exchange ratio

The blowback is small in national aggregate and concentrated locally, the incidence pattern the framework of Section 3 predicts. Applying an estimated land gradient to baseline employment across the exposed metros implies a gross local loss of roughly six to thirty thousand leisure and hospitality jobs, and because Section 10 leaves a ladder of gradients rather than a single one, the job count is a bracket. A linear extrapolation of each gradient across exposed metros gives 16,400 to 31,600 across the three rungs; a visits-based instrumental-variables construction, a reparameterization into visits units rather than fully independent evidence, gives 6,500 to 12,500 (Online Appendix I.M). Read per lost visit, against the 9.7 million foregone crossings, the constructions imply one leisure-and-hospitality job for every six hundred to fifteen hundred Canadian land trips that did not happen—the most portable statement of the intensity.

We read the lower half of that bracket as the part attributable to the withdrawal: the upper rungs rest on the metro-level gradient, whose pre-existing regional trend of about a third of a percentage point a year, compounded over the window, accounts for much of that gradient on its own (Section 10). The lower rungs, built from the within-metro differential, isolate the episode.

Two external benchmarks bracket the same object. The upper end sits inside the 13,900-to-42,100 range that Kurmann et al. (2026) report and our lower end falls below it, which is what a narrower outcome set should do: their measure covers 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, implies roughly 17,000 to 31,000 *national* job-equivalents from the measured C\$2.95 billion decline in Canadian spending in the United States over 2025Q2–Q4 (Online Appendix I.N). There is a reassuring similarity between the two sets of estimates: that the national figure exceeds our land-border count is expected, and the gap is uninformative about where the remainder fell. The per-visit intensity above also sits more comfortably against visitor-spending-per-job norms (Faber and Gaubert, 2019) than the one-per-three-hundred implied by the metro-level gradient alone, which 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. They are gross rather than net, since the withdrawal left no measurable loss at the air destinations (Section 8) and whatever domestic spending filled the gaps came from elsewhere in the United States. They are a headcount rather than a welfare cost, which would be the reallocation friction plus the foregone gains from trips that did not happen. And they cover one channel in one set of places, counting leisure and hospitality directly and not local-multiplier spillovers onto nearby employment (Moretti, 2010).

Valued at the sector’s wage, the local loss runs from roughly two hundred million to a billion dollars a year—the instrumented lower rung of the job bracket at the \$25,000 wage to the extrapolated upper rung at \$35,000—a concentrated, place-specific service-sector cost of the kind the coercion literature has long posited and rarely priced.²³ Set against the roughly \$9.9 billion in tariff revenue the United States collected on Canadian goods between March and December 2025 (Census calculated duties), the implied earnings loss is two to eleven cents per tariff dollar, and the gross-spending counterpart is the 23 cents of Section 4.D. Both are size benchmarks rather than welfare ratios.

11.B The political geography of the incidence

The job losses reached a different electoral map than prior trade retaliation. The 2018–19 agricultural retaliation fell on Republican-leaning farm counties, a targeting state retaliators chose deliberately (Fetzer and Schwarz, 2021; Kim and Margalit, 2021) and that cost the incumbent party votes (Blanchard et al., 2024); the 2025 travel boycott, which no one aimed, landed where Canadian spending was. 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 (Pearson -0.17 , $p < 0.01$): the most exposed decile of metros cast 50.5 percent of its two-party vote for the Republican candidate against 55.6 percent elsewhere, and the three most exposed metros—Buffalo, Detroit, and Bellingham—all lean Democratic, a contrast Kurmann et al. (2026)

²³We 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.

also note.²⁴ The 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 localized economic shocks move votes (Autor et al., 2020; Colantone and Stanig, 2018; Margalit, 2011). Whether these communities translate loss into pressure on the administration is a transmission question this cross-tabulation does not answer.

12 Implications for coercion strategy

Four implications follow, each resting on a result established above.

First, a decentralized response can impose a cost the coercing state feels, and standard coercion accounting will miss it. The response needs no symmetric chokepoint and no formal state retaliation; it is hard to negotiate away, because no single actor controls it; and it did not ease when the formal instrument did (Section 4.C). It is also poorly matched to the usual instruments of measurement. National aggregates score the loss near zero, and a coarse binary treatment on the same data returns a null (Table 2). The observable costs were asymmetric besides, a concentrated U.S.-side loss against a Canadian redirection rather than a sacrifice, though we do not measure Canadian welfare. A government weighing a coercive instrument against its alternatives, in the sense of Baldwin (1985), needs sender-side costs of this concentrated kind in the ledger, and getting them there requires exposure gradients at fine geography, decomposed by the channel the target’s public actually uses.

Second, the ally case is distinctively exposed to this channel. Senders rarely coerce allies (Section 3). The episode adds a reason beyond low expectations of future conflict: the dense people-to-people interdependence that makes an ally an ally, the day-trips, family visits, and seasonal migration of an open border, is the surface a decentralized response acts on. Coercing a distant adversary places little such custom at risk. Coercing a neighboring ally puts a large channel of return cost in play that neither government controls.

Third, the bite is bounded by the target’s share of local demand and the depth of replacement demand (Section 8), and that logic travels. Had Mexican consumers responded to the same tariffs in kind, the incidence map would run through the day-trip economies of San Diego, El Paso, and the Rio Grande Valley; a European consumer response to a transatlantic confrontation would arrive by air into large gateway metros, where the dose argument predicts dilution and little detectable local imprint. 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. The travel decline reached a different electoral map than the 2018–19 agricultural retaliation (Section 11.B): a decentralized response cannot be aimed, so it lands where the target’s spending was, and in this episode that meant communities that leaned against the governing party, concentrated in presidential battleground states. A coercer

²⁴County presidential returns for 2024 (McGovern, 2024), aggregated to core-based statistical areas with the Census Bureau’s July 2023 delineation; 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 to within a tenth of a point. This is a descriptive cross-tabulation, not a statement about how exposure affected voting.

expecting blowback to fall where tariff retaliation once fell will misjudge where this cost lands, and which constituencies carry it.

13 Conclusion

We began by asking where the cost of coercing an ally lands inside the country that imposes it. The 2025 U.S. coercion of Canada produced a response the data record with unusual completeness: a redirection of Canadian travel away from the United States specifically, selective on purpose, carried by fewer trips rather than shorter ones, worth about US\$2 billion over three quarters, begun before the tariffs took effect and still mostly in place, its unwinding slow and partial, five months after they were struck down in February 2026. Part of its cost returned home, falling on land-border communities. A southern-border placebo and a battery of checks tie that blowback to the episode rather than to the tariffs, the metros' own shocks, regional growth, or seasonality. The incidence is travel-mode-specific: 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. That contrast is the finding we would put weight on, and it holds within census division as well as across the country. The gross local loss runs from about six to thirty thousand jobs depending on how much regional variation the design absorbs—one job for every six hundred to fifteen hundred foregone trips, two hundred million to a billion dollars a year in earnings, gross rather than net (Section 11).

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

We conclude by noting the direction in which these results could be extended. The design here—a predetermined exposure gradient, continuous and decomposed by travel mode, paired with a second border for falsification, inference built for few treated units, and a unified exposed-versus-comparison reading of every source the episode touches (Table 1)—should carry to the next confrontation, whether a Mexican consumer response on the southern border or a European one arriving by air.

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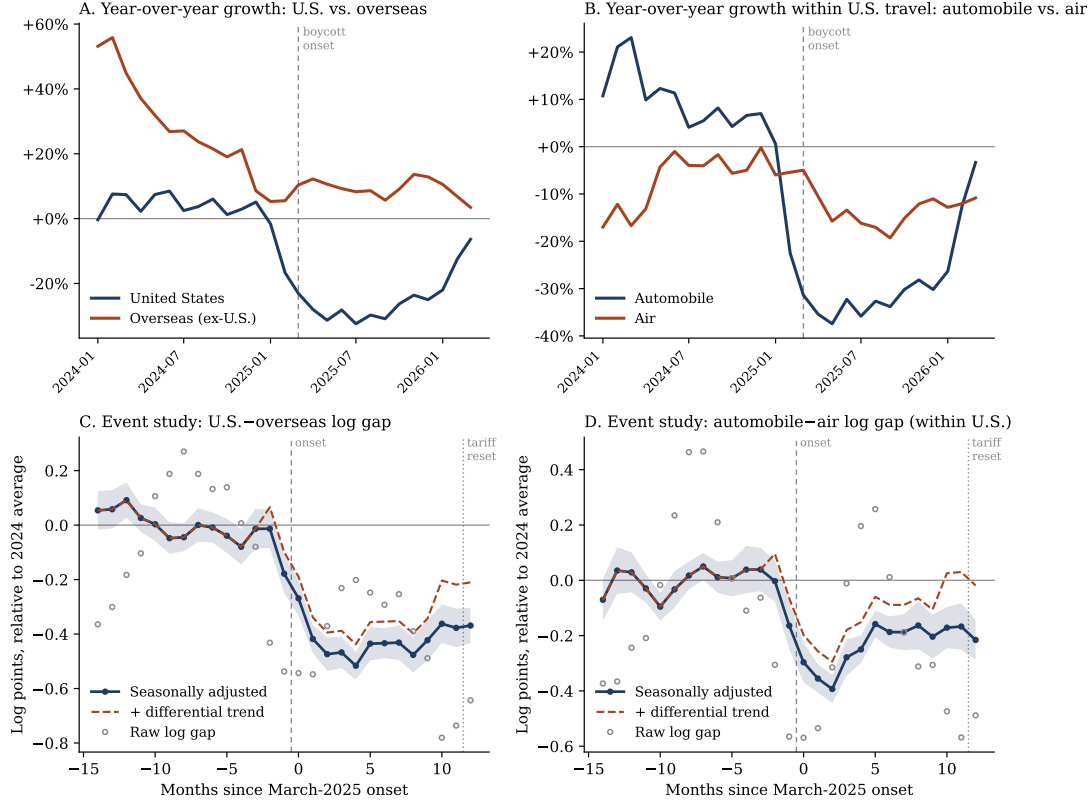


Figure 1. The Canadian Pullback Was Specific to the United States

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; 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.

Table 1. One design, every source: the boycott as a difference-in-differences across data sets

Outcome	Exposed unit	Comparison unit	Log points $\times$ 100			
			Exp.	Comp.	Diff.	Source
<i>Travel volumes</i>						
Land crossings (passengers)	U.S.–Canada border	U.S.–Mexico border	−27.1	−2.0	−25.1	BTS
Canadian return trips	from U.S.	from overseas	−32.9	+9.7	−42.5	StatCan
Canadian trips from U.S. by mode	automobile	air	−39.9	−14.0	−25.9	StatCan
NTS visits	to U.S.	overseas	−32.1	+10.0	−42.1	NTS
NTS visits to U.S. by purpose	leisure	friends/relatives	−31.4	−13.8	−17.7	NTS
<i>Trip intensity</i>						
Nights per overnight visit ^a	U.S. trips	overseas trips	+11.2	+0.3	+10.9	NTS
Spend per night (overnight)	U.S. trips	overseas trips	−6.6	+7.4	−14.0	NTS
<i>Single differences, air exit survey^b</i>						
Nights per trip, air visitors	Canadian air visitors	—	+5.4	—	+5.4	NTTO
Spend per visitor-day, air	Canadian air visitors	—	−9.9	—	−9.9	NTTO
<i>Dollars</i>						
NTS total spending	in U.S.	overseas	−20.5	+17.4	−37.9	NTS
U.S. travel exports	to Canada	all other countries	−18.5	+1.1	−19.7	BEA
<i>Local outcomes</i>						
Taxable retail sales	Whatcom Co., WA	interior WA counties	−0.4	+1.8	−2.1	WA DoR
Taxable retail sales	Blaine, WA (port town)	interior WA counties	−7.0	+1.8	−8.7	WA DoR
Sales-tax collections ^c	NY border counties	interior NY counties	+6.7	+1.6	+5.2	NY OSC
L&H employment ^d	top-decile metros	all other metros	−0.3	+0.2	−0.4	BLS

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, and 2025Q1–Q3 for the Washington taxable-retail-sales rows, the quarters the Department of Revenue had published) against the same window of 2024, exposed unit minus comparison unit, in log points $\times$ 100 (so –31.4 corresponds to about –27 percent). Aggregate and survey rows are quoted without sampling inference; construction detail is in Online Appendix I.N and `ext.D_unified.did.csv`. New York border counties are Erie, Niagara, and Clinton; interior comparison counties are Spokane and Clark (WA) and Onondaga and Albany (NY).

^a The positive sign is composition, not increased travel: short trips exit first, so the trips that survive are longer (the extensive/intensive decomposition of Section 4).

^b Single difference over calendar years (no comparison population).

^c Predicted non-mover: the New York border counties’ metros sit in deeper domestic markets, where the incidence geography of Section 7 predicts no imprint (Buffalo also shows no synthetic-control divergence, Online Appendix I.K).

^d Crude binary version of Table 2’s continuous gradient, shown for format comparability; exposure-permutation $p = 0.068$.

Sources: BTS, Bureau of Transportation Statistics; StatCan, Statistics Canada frontier counts; NTS, Statistics Canada National Travel Survey; NTTO, National Travel and Tourism Office air exit survey; BEA, Bureau of Economic Analysis; WA DoR, Washington Department of Revenue; NY OSC, New York Office of the State Comptroller; BLS, Bureau of Labor Statistics. Author’s calculations throughout.

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

Exposure (+1 SD $\times$ post)	Coef. (%)	CRV1 p	Perm. p
<i>Panel A. Two-way fixed-effects gradients</i>			
Land-crossing volume (BTS)	−0.57 (0.15)	< 0.001	0.001
Border proximity	−0.39 (0.14)	0.004	0.011
Air-destination share	+0.09 (0.05)	0.085	0.520
Binary border indicator	−0.86 (0.60)	0.153	0.309
<i>Panel B. Within-metro triple difference</i>			
Land volume $\times$ tourism $\times$ post	−0.30 (0.16)	0.072	0.046
<i>Panel C. Land–air mode contrast (exposures pooled)</i>			
Land – air difference (pp)	−0.60 ($t = -4.1$)	< 0.001	
within division $\times$ month	−0.52 ($t = -2.3$)		0.007

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). Panel B stacks leisure and hospitality against the rest of nonfarm employment ($n = 28,864$) and interacts land-crossing exposure with a tourism indicator and post, with metro-by-sector, metro-by-month, and sector-by-month fixed effects, so identification comes only from the differential movement of the tourism sector within a metro and month (Section 7); the wild-cluster-restricted p is 0.077, and the spatially blocked permutation p is 0.10–0.18 (Online Appendix I.A). Panel C pools the land-crossing-volume and air-destination exposures in a single specification and tests the difference of their coefficients, in percentage points, unconditionally and within census division and month (Section 8); the air coefficient’s 95% confidence interval, $[-0.08\%, +0.14\%]$, rejects an air loss as large as the land point estimate. Cluster-robust (metro) standard errors are in parentheses (t -statistics for the Panel C differences); CRV1 is the cluster-robust p -value and Perm. is permutation inference (2,000 reassignments). Because cluster-robust inference over-rejects here (Section 6), permutation is the preferred read where the two disagree, and Table A2 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.

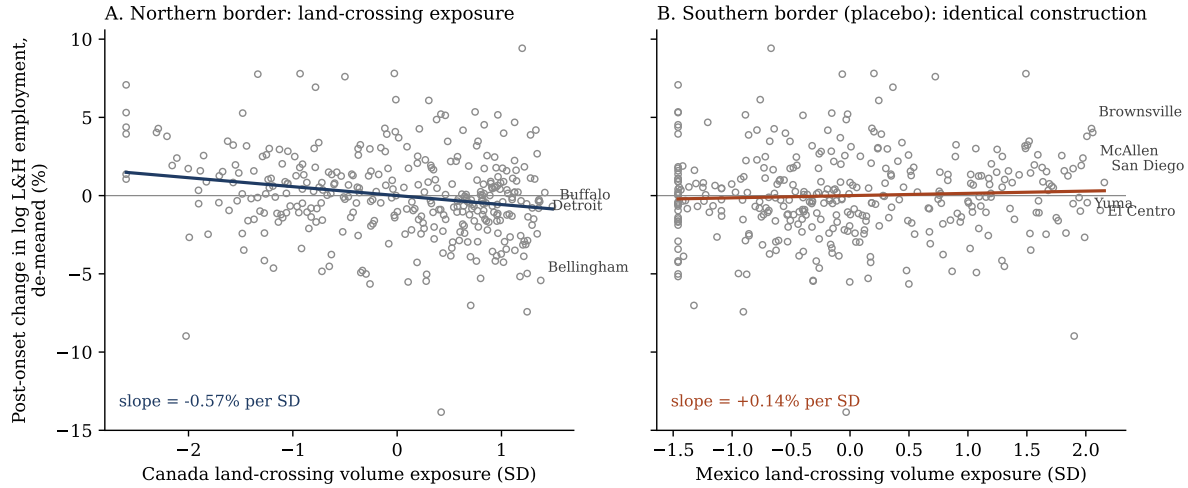


Figure 2. The Cross-Section Behind the Difference-in-Differences

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-means 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 2, permutation $p = 0.001$) and $+0.14\%$ for the identically constructed Mexican exposure (Panel B; Table 3, permutation $p = 0.33$), the southern-border placebo of Section 9. The most exposed metros on each border are labeled. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author's calculations.

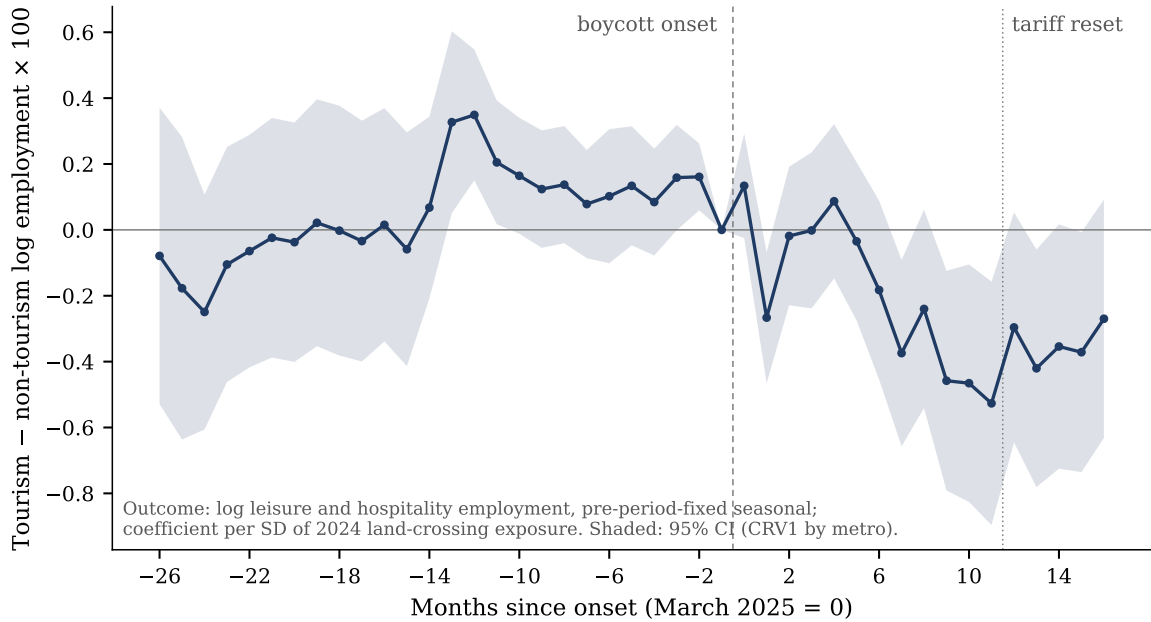


Figure 3. Within-Metro Triple-Difference Event Study through July 2026

Notes: Coefficients on exposure $\times$ tourism $\times$ event-month (reference -1 , February 2025) from the stacked triple-difference with metro $\times$ sector, metro $\times$ month, and sector $\times$ month fixed effects; outcome deseasonalized with pre-period-fixed calendar-month factors. Shaded band: 95 percent confidence interval, cluster-robust by metro. Dashed line: boycott onset (March 2025); dotted line: first full post-reset month (March 2026). The last two months are preliminary CES estimates.

Table 3. Northern Border versus Southern-Border Placebo

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

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 5 percent level under every inference method, and land-crossing volume at the 1 percent level; see Table 2.

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

Table 4. Employment Persistence after the Tariff Reset

	(1) Metro level	(2) Within metro	(3) Within division
Exposure $\times$ boycott window	−0.51 (0.14)	−0.25 (0.16)	−0.22 (0.23)
Exposure $\times$ post-reset	−0.89 (0.21)	−0.39 (0.23)	−0.52 (0.32)
Permutation p , boycott window	< 0.001	0.079	0.124
Permutation p , post-reset	< 0.001	0.063	0.014
Blocked permutation p , post-reset	0.010	0.164	0.058
Metro (or metro $\times$ sector) FE	✓	✓	✓
Month FE	✓		
Metro $\times$ month, sector $\times$ month FE		✓	
Metro $\times$ month, division $\times$ sector $\times$ month FE			✓
Metros	352	352	348

Notes: Coefficients are in percent of employment per standard deviation of 2024 land-crossing exposure, on the panel extended through July 2026. The boycott window is 2025–03 to 2026–02; post-reset is 2026–03 to 2026–07. Column (1) is log leisure and hospitality employment (metro and month fixed effects); columns (2) and (3) are the within-metro-month tourism-minus-nontourism differential, the exact collapse of the stacked triple-difference. All outcomes are deseasonalized with calendar-month factors estimated on the pre-onset window only. Cluster-robust (by metro) standard errors in parentheses. Permutation p -values reassign the static exposure across metros (2,000 replications, both coefficients jointly per draw); blocked permutation reassigns state-mean exposure across states. Column (3) drops four metros without a census division. July 2026 employment is a preliminary CES estimate (revised September 2026).

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

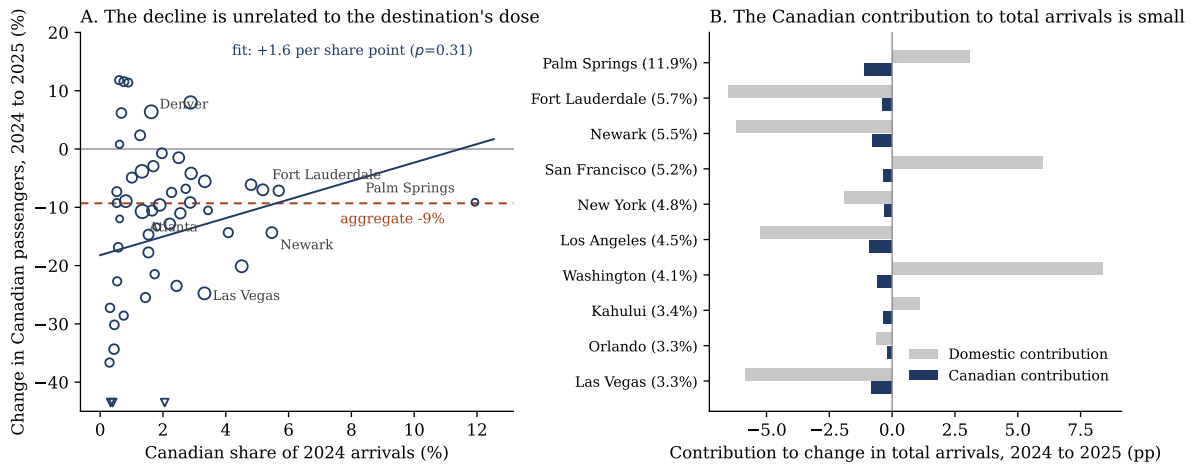


Figure 4. The Size of the Air-Destination Dose

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.

Online Appendix

I Robustness and supplementary constructions

Table A2 consolidates the falsification and robustness checks summarized in Sections 8–10, 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 8–10 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 synthetic-control case studies, 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 $\times$ 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 $\times$ 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 cut-off 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. Because the choice of block definition matters, we report the full grid: the level gradient returns $p = 0.0025$ over forty spatial blocks, 0.0045 over twenty, 0.013 over ten, 0.006 with states as blocks, and 0.024 with

census divisions, clearing conventional levels under every partition. The same test applied to the within-metro triple-difference returns $p = 0.10$ to 0.18 , against 0.046 under an i.i.d. permutation; the triple-difference is identified off a within-metro contrast but its exposure varies across metros, and that variation is spatially clustered. 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 9 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 9 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, Denmark (−26.3 percent) and Venezuela (−20.4 percent), are origins with U.S.-specific frictions of their own over the same months, 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, 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 9 identifies the redirection rather than a single motive.

I.E Sectoral composition and the regional confound

Section 7 reports that the metro-level gradient is not tourism-specific. Running the headline specification on each CES supersector in turn makes the point concrete; Table 5 reports the full battery. Unconditionally the gradient is broad: wholesale trade (−0.84% per standard deviation, permutation $p = 0.005$) and financial activities (−0.67%, $p = 0.003$) show the two largest declines, both larger in absolute value than retail trade (−0.36%) and neither reachable by a cross-border day-trip. Government is flat (+0.06%, $p = 0.72$). A local multiplier running out from the visitor economy would concentrate in retail, transport, and other services and leave wholesale and finance alone; it does not, which points to a regional trend common to the Great Lakes industrial corridor.

Census-division-by-month effects remove it. The non-tourism aggregate falls from −0.28% ($p = 0.001$) to −0.12% ($p = 0.33$), while leisure and hospitality holds at −0.45% (permutation $p = 0.003$). We do not interpret individual sector cells within division, for two reasons. Thirteen sectors estimated under two specifications produce wrong-signed significant coefficients at about the rate chance predicts. And the division-by-month control is expensive: it absorbs roughly three-fifths of the identifying variation in exposure, against about one-tenth for a control that groups metros by the border segment they face. Two of the sign reversals it produces—financial activities from −0.67% to +0.14%, professional and business services from −0.12% to +0.45%—are as consistent with that variance loss as with the removal of a confound, and we do not read them as evidence either way. The two facts we rely on are the breadth of the unconditional gradient and the tourism-versus-rest split once regional trends are absorbed.

The two sectors carrying the unconditional gradient are worth characterizing, since they are the clearest evidence that it is regional. Both declines pre-date the episode: on placebo onsets inside the pre-boycott period the wholesale and finance gradients are −0.59% and −0.67%, the latter equal to its own post-onset value, and over 2015–2019 wholesale is −1.03%. Neither is the metro’s own tariff shock: a sector-loaded tariff control leaves both slightly more negative, though that control finds no employment gradient in manufacturing and so bounds the tariff channel weakly. The wholesale gradient is also not a goods-trade channel. Holding allocated crossing volume fixed and varying only the freight intensity of the ports a metro draws on, metros facing more truck-intensive crossings

Table 5. The Land-Crossing Gradient Sector by Sector

Sector	Share, % ^b	Metro + month FE		Metro + division×month FE	
		Coef.	Perm. p	Coef.	Perm. p
<i>Treated sector</i>					
Leisure & hospitality	10.5	−0.57	< 0.001	−0.45	0.003
<i>Day-trip visitor channel</i>					
Retail trade	9.3	−0.36	0.003	−0.20	0.092
Transportation, warehousing & utilities	4.6	−0.60	0.100	+0.25	0.479
Other services	3.8	−0.14	0.494	+0.65	0.002
<i>Ambiguous channel</i>					
Wholesale trade	3.9	−0.84	0.004	−0.55	0.046
Mining, logging & construction	4.9	+0.33	0.264	−0.42	0.141
<i>No day-trip channel</i>					
Financial activities	6.2	−0.67	0.002	+0.14	0.495
Information	2.2	−0.28	0.612	−0.44	0.451
Professional & business services	15.8	−0.12	0.620	+0.45	0.072
Education & health services	17.6	−0.36	0.072	−0.70	< 0.001
Manufacturing	6.8	−0.30	0.198	+0.52	0.029
Government	13.8	+0.06	0.720	−0.09	0.602
<i>Memo</i>					
Trade, transportation & utilities ^a	17.9	−0.48	0.001	−0.12	0.338

Notes: Each cell is a separate estimate of the headline land-crossing-volume specification with the named sector's log employment as the dependent variable: coefficient on exposure × post per +1 standard deviation of exposure, in percent, with the exposure-permutation p beside it. Sector panels group supersectors by whether a cross-border day-trip has a direct channel into them. Sector coverage in the published metro CES series varies, so samples range from 311 to 352 metros (12,751 to 14,432 metro-months). Twenty-six cells estimated under two specifications will produce wrong-signed significant coefficients at roughly the rate chance predicts, and we do not interpret individual within-division cells (see text). Cluster-robust p -values and sample sizes per cell are in `ext_D.supersector_incidence.csv`.

^a Aggregate of wholesale trade, retail trade, and transportation, warehousing & utilities.

^b The sector's 2024 share of metro nonfarm employment.

show *higher* wholesale employment (+1.0% per standard deviation, $p = 0.0004$), the opposite sign to a freight-exposure mechanism. It is not composition either: exposure is essentially uncorrelated with the wholesale employment share ($r = 0.07$), and the share does not move. At the three-digit level the decline sits in durable-goods wholesaling rather than agents and brokers, where a customs-brokerage channel would appear. It weakens to −0.36% ($p = 0.37$) within division by month. We carry it as a regional gradient of unidentified origin that bears on the level specification and not on the within-metro comparison.

The same ladder applies to the mode contrast, and it survives: the land-minus-air difference is −0.60 percentage points ($t = -4.1$) unconditionally and −0.52 points ($t = -2.3$, permutation $p = 0.007$) within division. Under state-by-month effects the difference is −0.12 points with a standard error of 0.64; with exposure clustered inside states, that specification's confidence interval contains both zero and the baseline estimate and so does not discriminate between them. The pooled single-index gradient, by contrast, falls from −0.36% to −0.02% within division, so the cost of pooling land and air rises rather than falls under the more demanding comparison.

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 (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.²⁵ 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 10). 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 7), 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.

²⁵On 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.

I.G The triple-difference pre-trend record

Section 9 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 is uninformative in both directions and we do not lean on it: unrestricted, the average post-onset effect is not robust to violations of any size (breakdown $\bar{M} = 0$), and the restricted upper bounds are not monotone in $\bar{M}$ —a pattern we read as grid noise rather than robustness—so we report no breakdown value. 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.H A five-year pre-boycott window

The estimation panel begins in 2023, allowing a two-year pre-period. The underlying Bureau of Labor Statistics state and area series run back to 2006, and we use 2015–2019—dropping 2020–2022 as pandemic-contaminated—to test the pre-period over a longer horizon.

Over 2015–2019, land-crossing exposure predicts declining employment in essentially every sector. Total nonfarm falls 0.67% per standard deviation (permutation $p < 0.001$); leisure and hospitality falls 0.79%, a trend of about 0.34 percentage points a year, which exceeds the post-onset gradient this paper estimates. The pattern is not driven by particular places or years. Leaving out each of the twenty-five most exposed metros in turn moves the leisure and hospitality slope within $[-0.81\%, -0.76\%]$, and dropping Buffalo, Detroit, or Bellingham individually moves it by less than 0.01 points. Exposure deciles are monotone, with the most exposed decile growing 1.33 points a year more slowly than the least. Estimated year by year the gradient is steady and still widening at the end of the window (-0.59% in 2015, -0.67% in 2019), so it is not an artifact of the mid-decade commodity cycle.

The measure is a 2024 cross-section applied retroactively, but the geography of Canadian cross-

ing volume is close to static: rebuilding exposure from contemporaneous 2015–2019 volumes, with the same gravity allocation and decay bandwidth, reproduces the gradient (-0.73% for leisure and hospitality), and the two cross-sections correlate at 0.998. Trimming or winsorizing exposure leaves the result intact or strengthens it.

The tourism-minus-other-sectors differential behaves differently, which is why identification rests on the within-metro design rather than the level. Over the same five years the differential is -0.14% ($p = 0.35$), with the sign running against the post-onset estimate, and it is flat under every leave-one-out, trimming and winsorizing variant and on the leakage-free 2023–2024 window. Within the Great Lakes alone it is -0.45% ($p \approx 0.02$); grouping metros by the border segment they face does not absorb this, since a segment-by-month control has no leverage on a trend running inside the segment. In-time placebo windows end in December 2024 throughout, because the Canadian withdrawal is visible in the crossing data from January 2025 and later placebo windows would pick up the treatment. Of the design-based lead tests run at placebo onsets inside the window, one, at mid-2024, rejects flatness; the others do not, which is the second qualification noted in Section 10.

I.I 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 A4 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.J 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, and Canadian passengers were only about 3 percent of arrivals even at the most exposed destinations, so the withdrawal removed a few tenths of a percent of their arrivals—a dose an order of magnitude too small to move metropolitan employment. The most air-exposed destinations in fact added leisure and hospitality jobs in 2025 while the most land-exposed metros shed them; Figure 4 displays the airport-level pattern.

I.K Synthetic-control case studies

Synthetic controls for the four most day-trip-dependent metros give a mixed but mechanism-consistent picture rather than uniform corroboration of the level gradient. 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—the loss concentrates where replacement demand is thin—not the level gradient metro by metro.

I.L 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.M Job-count constructions

The bracket of Section 11 crosses two constructions with the three gradients of Section 10. The first construction extrapolates a 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. 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 second 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 at the metro-level gradient, the same units as the exposure construction, and cumulating this over metros gives roughly 12,500 (95% confidence interval 1,500–23,500). Because that cumulation is linear in the coefficient, it rescales in proportion across the ladder.

Gradient	Per SD	Extrapolated	Instrumented
Within-metro triple-difference (flat pre-period)	−0.30%	16,400	6,500
Leisure and hospitality within division $\times$ month	−0.45%	24,600	9,800
Metro-level gradient (upper bound)	−0.57%	31,600	12,500

Set against the 9.7 million foregone crossings, these aggregates bracket the intensity between roughly one job per six hundred and one per fifteen hundred lost trips. 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 10.

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

Section 4 reports the headline dollar margins from National Travel Survey aggregates (Statistics Canada Table 24-10-0070): a C\$2.95 billion decline over 2025Q2–Q4 (about US\$2.1 billion), driven entirely by the extensive margin, two-thirds leisure with family-visit spending up, and a BEA mirror agreeing to within about 20 percent. This subsection adds construction detail and the benchmark calculations built on those figures. By purpose, leisure accounts for C\$1.95 billion of the decline (66 percent) and travel for other purposes for C\$1.11 billion (38 percent), while spending on visits to friends and relatives rose C\$0.12 billion. Within the leisure decline the leisure-overnight cell alone is C\$1.92 billion. The same purpose ordering holds on visit counts, where the other-purpose decline (38 percent) exceeds leisure’s (27 percent) and family visits fall least (13 percent)—a pattern Section 9 reads as constraining but not discriminating between the candidate motives. 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.

The spending decline supplies a benchmark for the job counts of Section 11 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 11 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.

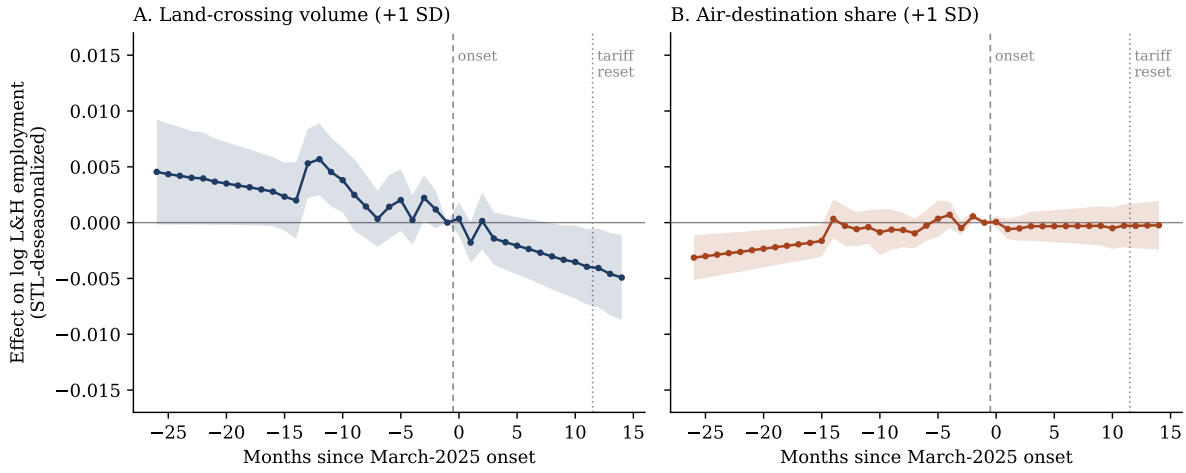
I.O The identifying geography

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 9.

Table A1. 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.

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

Table A2. Robustness of the Land-Crossing-Volume Gradient

Specification	Land-volume gradient	Inference / note
Baseline (Table 2)	−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 $\times$ post, mfg. share $\times$ 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 $\times$ month fixed effects	−0.44%	perm. $p = 0.002$; proximity does not survive
State $\times$ month fixed effects	−0.10%	ns; SE quadruples, CI contains baseline
Metro $\times$ calendar-month fixed effects	−0.58%	$p < 0.001$; proximity −0.36%
Exposure $\times$ 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 (full block grid)	−0.57%	$p = 0.0025$ (40 blocks), 0.0045 (20), 0.013 (10), 0.006 (states), 0.024 (divisions)
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 $\times$ sector $\times$ month fixed effects	−0.32%	perm. $p = 0.035$; tourism trend free by division
southern-border (Mexico)	−0.11%	ns (perm. $p \approx 0.45$); no gradient
triple-difference		absent the withdrawal
Honest-DiD, sign-restricted late horizon	not reported	bounds non-monotone in $\bar{M}$ and within 4×10^{-4} of zero throughout; treated as grid noise. Unrestricted avg. post $\bar{M} = 0$
Post-reset persistence (split at 2026m2, panel to 2026m7)	−0.22% / −0.52%	boycott window / post-reset, division $\times$ sector $\times$ month spec. (Table 4); perm. $p = 0.014$, blocked 0.058 on post-reset. Pre-period-fixed seasonal; the STL deseasonalizer overstates the post-reset deepening at the sample endpoint
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

Table A3. Persistence Split: Robustness

	Boycott window		Post-reset		p , difference
North, within metro, i.i.d. permutation	−0.25	(0.073)	−0.39	(0.057)	0.156
North, within metro, state-blocked	−0.25	(0.196)	−0.42	(0.161)	0.367
North, division×month FE (preferred)	−0.22	(0.124)	−0.52	(0.014)	0.007
state-blocked				(0.058)	0.062
South (Mexico) placebo, within metro	−0.12	(0.399)	−0.22	(0.276)	0.324

Notes: Post-period split of the within-metro tourism-minus-nontourism differential at the February 2026 reset, panel through July 2026, pre-period-fixed seasonal factors throughout; coefficients in percent per standard deviation of land-crossing exposure with permutation p -values in parentheses (2,000 replications, both split coefficients jointly per draw; state-blocked draws reassign state-mean exposure across states). The Mexico row applies the identical construction to the 28 southern land ports on the same metros. The final column tests post-reset minus boycott-window. Companion to Table 4.

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

Table A4. Land-Gradient Sensitivity to the Distance-Decay Bandwidth τ

τ (km)	Land-crossing volume		Border proximity	
	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.

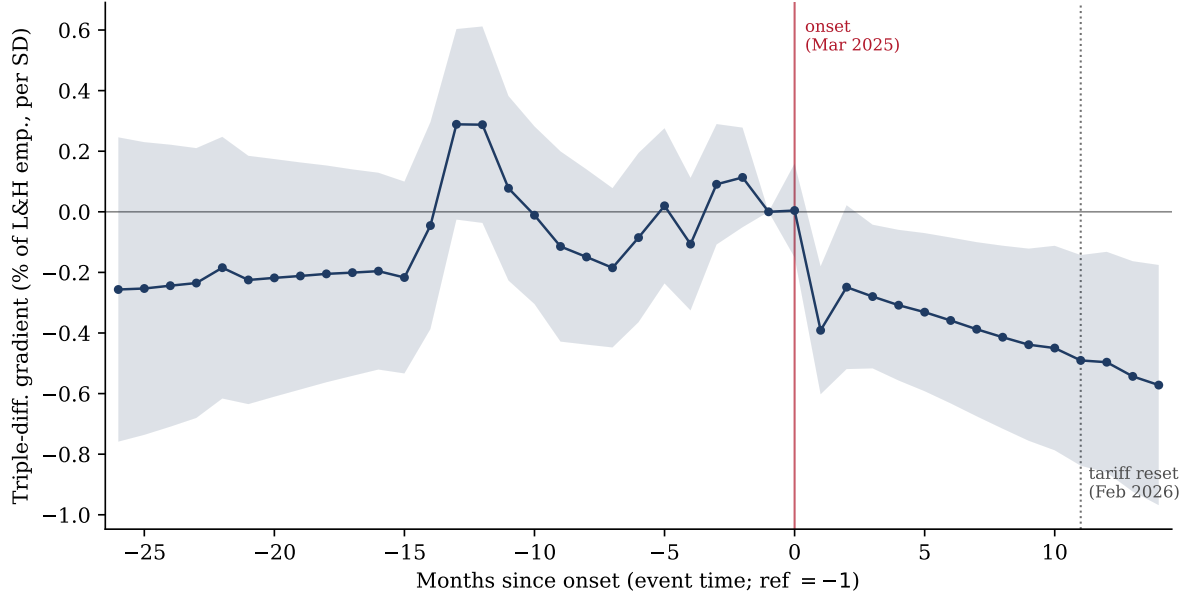


Figure A2. Triple-Difference Event Study on the Deseasonalized Series

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 7, 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 2) 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 7, -0.29% on the deseasonalized series. *Sources:* BLS Current Employment Statistics; Bureau of Transportation Statistics; author’s calculations.

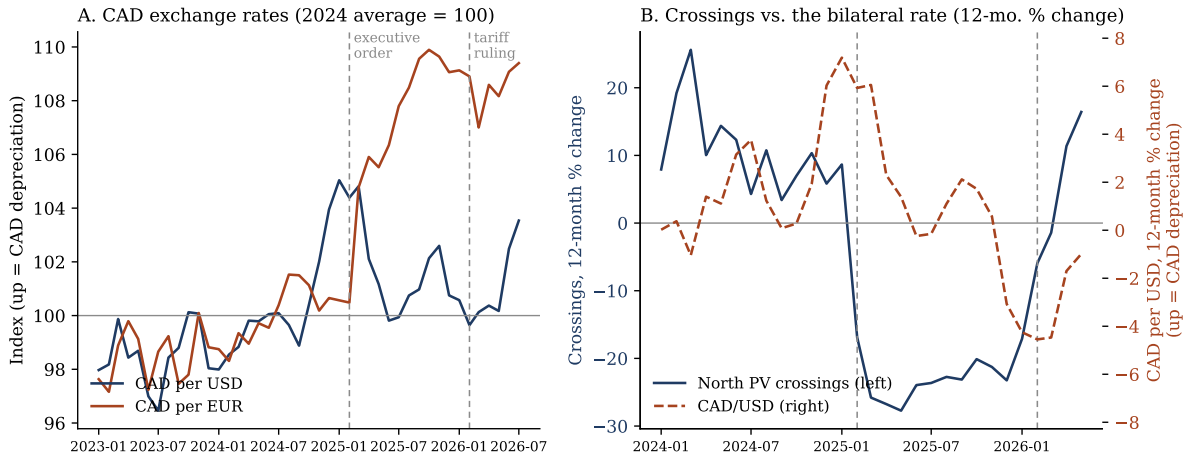


Figure A3. The Canadian Dollar Cannot Account for the Travel Decline

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 4.C) after the currency has recovered. *Sources:* Bank of Canada; Bureau of Transportation Statistics; author's calculations.

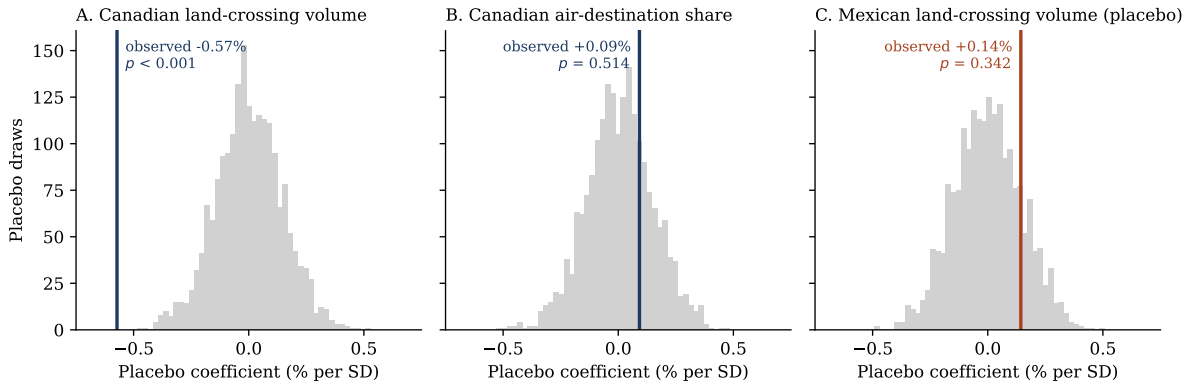


Figure A4. Randomization Distributions for the Exposure Gradients

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 2 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 2 and 3, 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.

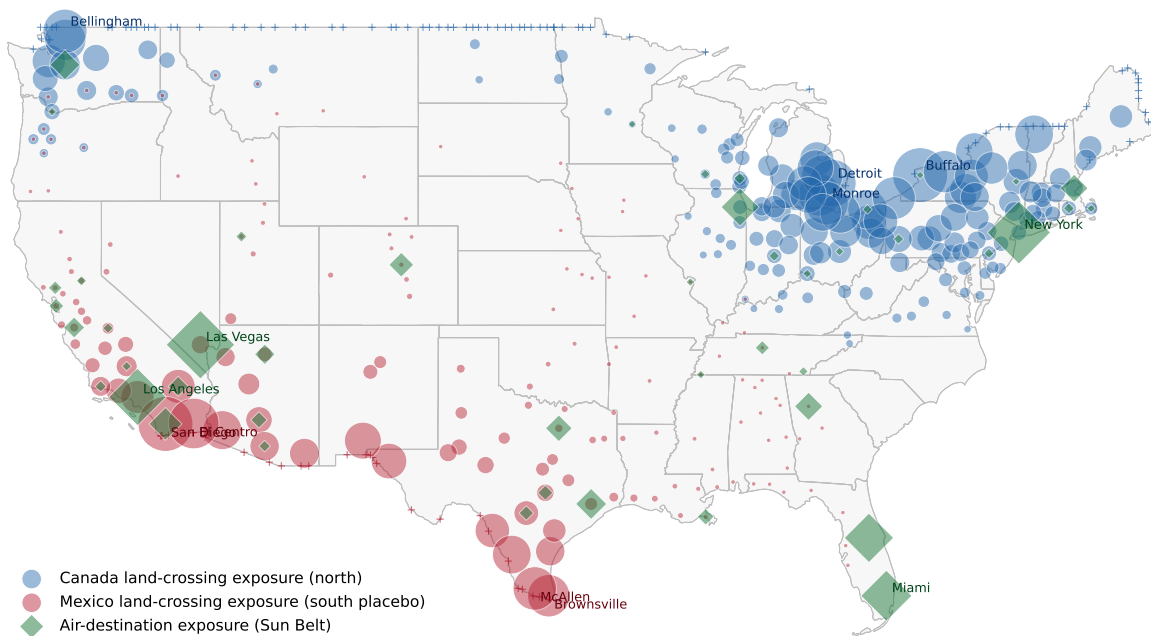


Figure A5. The Identifying Geography on Both Borders

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 9; the air-destination margin provides the mode contrast of Section 8. *Sources:* Bureau of Transportation Statistics; U.S. NTTO; Census cartographic boundaries.