Shared Maintenance and Bridge Sufficiency: Evidence from U.S. Interstate Border Bridges

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Abstract

In a federal system, is the shared governance of infrastructure assets by sub-national jurisdictions inefficient? We examine this question in the context of highway bridges in the United States, where approximately 800 roadway bridges cross state lines and the responsibility for maintenance is shared between neighboring states. Shared maintenance is associated with a significantly lower sufficiency rating (0.2 SD) and structural condition (0.2 SD) at endline compared with initially similar controls. Consistent with a model of decision-making under shared governance, we find that the treatment effect is largest among bridges that see less traffic. The shared bridge penalty is increasing in proxies for coordination cost and declines in asymmetries between states in bridge value.

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1 Introduction

A longstanding question in public finance is how responsibility for public goods should be allocated when they generate spillovers across jurisdictions. In the absence of externalities, decentralization allows localities to tailor public good provision to the tastes and needs of their residents (Oates, 1972). When provision in one jurisdiction affects surplus elsewhere, however, decentralized provision can lead to underinvestment, and centralized decision-making may be needed to internalize these externalities.[1]

An especially difficult case arises when responsibility for a public good is itself shared across jurisdictions. Shared responsibility can internalize some of the benefits of investment, but it also creates a coordination problem where neighboring states must agree on the level of investment and work together on its implementation. As a result, dividing responsibility for a public asset may lead to underinvestment even when all parties benefit from its maintenance. The consequences can be severe, as illustrated vividly in 1999 when a truck fire ignited inside the Mont Blanc Tunnel connecting France and Italy, killing 39 people. Investigators found that the tunnel's safety regime had been managed under a split French-Italian ownership structure with no unified authority and safety had suffered as a result.

In this paper, we study interjurisdictional coordination in the context of roadway bridges that cross state borders within the United States. The nation's road network, by carrying goods and people across borders, is a public good with clear spillovers across space. The federal government provides some coordination by collecting fuel taxes, allocating funding to states, and setting guidelines for its use. States nonetheless retain substantial latitude to fund the projects they deem highest priority for their own transportation needs. Construction and maintenance decisions are therefore largely made by states, even when the benefits extend to their neighbors.

Ownership and maintenance responsibility of border bridges are commonly split between the neighboring jurisdictions, whether states, counties, or cities on either side of the border. Approximately 800 bridges with shared maintenance are in service in the United States, and they provide a particularly useful setting for studying how divergent interests affect both public good provision and the allocation of responsibility for investment. The external benefits of maintenance are unusually large for cross-border bridges. However, undertaking investment requires two governments to propose a project, agree on its scope, and fund it. This leads to underinvestment due to coordination costs and the possibility of free-riding on the planning work of the neighboring states.

[1] Centralized decision-making may not itself resolve the externality, as disagreements between jurisdictions over the optimal level of public spending may instead be expressed through bargaining in the legislature (Besley and Coate, 2003).

We estimate the effect of shared maintenance responsibility on bridge condition using inspection records from the National Bridge Inventory (NBI) for 1996 through 2018. Our primary outcome is the sufficiency rating, which combines measures of structural condition, traffic relative to bridge capacity, and several other factors that receive less weight. We also consider measures of bridge condition alone. We compare these outcomes at endline for shared bridges with a matched set of control bridges near the same border, matching on structure kind and type, traffic volume, bridge area, National Highway System status, and the baseline value of the outcome. By conditioning on baseline outcomes, our estimates compare the subsequent trajectories of shared and control bridges rather than differences in their condition at the beginning of the sample.

We find that shared bridges end the sample period with sufficiency ratings 4.48 points lower, or 0.2 standard deviations, than comparable bridges with similar ratings at baseline. The effect is higher among bridges owned by state highway agencies, where the difference is 5.03 points. The condition of a shared state-owned bridge's structural components, an important input into the sufficiency rating, is 0.2 standard deviations lower, and shared state-owned bridges are 13.8 percentage points more likely to be classified as structurally deficient. For locally owned bridges, by contrast, the estimated effect is much smaller and not statistically distinguishable from zero.

Sharing agreements commonly designate a lead state that will undertake inspections and propose maintenance, repairs, and other improvements.[2] Any investments beyond routine maintenance proposed by the lead state must be approved by the other, giving it veto power. We specify a model of maintenance investment that captures these institutional details. A project goes forward only if the lead state bears the cost of planning it and engaging its neighbor, and the neighbor then agrees to pay its half. The lead state captures only part of the benefit but pays the entire cost of initiating the project. Consequently, not all surplus enhancing investments are made. Underinvestment is most likely for low-value bridges, and the degree of underinvestment grows as the cost of coordinating between the states is higher. The model implies an interaction between bridge value and coordination costs, as variation in the latter will be irrelevant for high-value bridges.

To test the predictions of the model, we form proxies for the value of the bridge overall, and the likely asymmetry in value between the two states. Using average daily traffic volume (ADT) as a proxy for the bridge value, we find that the effect is concentrated among bridges with below-median traffic. The estimated treatment effect for low-ADT bridges is 8.0 sufficiency rating points, while the estimate for high-traffic bridges is around half as large and not statistically distinguishable from zero. This reasoning can also help explain the larger

[2]Costs are shared at a pre-specified agreed upon percentage and are most often equally shared.

treatment effect for state-owned bridges, which represent a smaller part of the infrastructure portfolio than a similarly sized locally-owned bridge would.

Finally, we consider how differences between the two jurisdictions shape the outcome. We find that asymmetries in traits that are related to higher coordination costs, such as political alignment, lead to a larger treatment effect. Specifically, measuring political differences by the gap in the two states' Republican vote share in the 2000 presidential election, we find that the shared-bridge penalty grows by roughly 3 sufficiency rating points for each standard deviation of partisan distance. Asymmetry in the value of the crossing works in the opposite direction. Measuring it by the imbalance in commuting flows between the two states, we find that among low-traffic bridges a one standard deviation increase in commuting asymmetry reduces the penalty by close to 8 points, consistent with a model in which a state that values the crossing more is willing to bear the cost of bringing projects forward. Differences in highway spending between the states, a coarser proxy for asymmetry in the stakes, have no detectable effect.

Our results contribute most directly to the literature on fiscal federalism and the assignment of spending responsibility across levels of government in the presence of spillovers (Oates, 1972; Besley and Coate, 2003; Lockwood, 2002; Hatfield and Padró i Miquel, 2012). Agrawal et al. (2024) survey the recent literature on fiscal federalism generally, and also specifically with respect to the case of spillovers. Empirical work in this area has largely studied decentralization at the level of programs or jurisdictions, asking whether more fragmented government is associated with faster growth or with fiscal policies that respond to those of neighboring states (Case et al., 1993; Akai and Sakata, 2002; Hammond and Tosun, 2011; Hatfield and Kosec, 2013).

Several closely related papers consider environmental externalities across jurisdictional boundaries. Theory predicts that decentralized governments will under-abate pollution whose damages fall on neighbors (Oates and Schwab, 1988; List and Mason, 2001; Ogawa and Wildasin, 2009), and a number of papers find that water and air quality deteriorate near state and county borders, consistent with free-riding on downstream or downwind jurisdictions (Sigman, 2002, 2005; Gray and Shadbegian, 2004; Konisky and Woods, 2010; Lipscomb and Mobarak, 2016; Hatfield and Kosec, 2019). States also appear to set environmental policy strategically in response to their neighbors (Fredriksson and Millimet, 2002).

Our findings also relate to the economics of highway provision in a federation. Transportation infrastructure generally, and highways in particular, generate strong spillovers across jurisdictions (Allen and Arkolakis, 2022). Jiang (2026) estimates a quantitative spatial model of highway investment by state planners and finds that states place a weight of roughly one-half on the welfare of other states' residents, leading to underinvestment of about

15 percent relative to the national optimum. Federal aid could align state incentives with the national return but is subject to crowdout (Knight, 2002). A recent literature examines the causes of rising infrastructure costs (Brooks and Liscow, 2023) and how the political process can affect the efficiency of the allocation of highway spending (Glaeser and Ponzetto, 2018; Kalaitzidakis and Kalyvitis, 2004).

The remainder of the paper is organized as follows. Section 2 presents a model of proposal and approval under a sharing agreement and derives its empirical predictions. Section 3 describes the data. Section 4 describes the estimation strategy and Section 5 presents the results. Section 6 concludes.

2 Theoretical Framework

2.1 Fiscal Federalism and Shared Infrastructure

The canonical fiscal federalism model establishes that decentralized provision of public goods is efficient except when spillovers extend to other jurisdictions (Oates, 1972; Besley and Coate, 2003). For interstate roads, and especially for bridges that connect neighboring jurisdictions, spillovers are important. Sharing maintenance responsibility between neighbors is a natural response, since it allows the two states to weigh the full return to maintenance against its cost.

Shared governance, however, introduces a second decision-maker into every project. In the sharing agreements that we have been able to review, one state is typically designated as the lead for the bridge. It conducts inspections and submits proposals and budgets to its neighbor. Costs are split evenly, and any expenditure above normal maintenance requires the approval of the other state.[3] A surplus enhancing bridge project can fail to materialize if the coordination frictions reduce the project payoff, or if the private benefit of the project is too small for the lead state to propose it or for the secondary state to approve it. The model that we present below delivers testable predictions regarding which bridges, and which pairs of states, should see the largest shortfall in investment.

[3]The 1966 agreement governing the Interstate 5 bridges over the Columbia River assigns operation, maintenance, and repair to Oregon, provides that costs "shall be borne equally by the two states," and states that for any improvement "over and above the cost of normal operations, maintenance and repair," plans and the contract amount must be approved by Washington. Texas's bridge manual describes the same division between a "Managing Party" and a "Contributing Party" for crossings of the Red and Sabine Rivers, with design, construction, and major maintenance split evenly and routine work performed by the managing state at its own expense. Minnesota and Wisconsin report an even split on all of their border bridges with the lead role assigned bridge by bridge, and Iowa and Illinois alternate the lead on successive Mississippi River crossings. Federal bridge inspection standards have required a written agreement naming a lead agency for every border bridge since 2022.

2.2 Model

Consider two states, indexed by $i \in \{1, 2\}$, that share a bridge.[4] In each period a maintenance need arises. Completing the project costs c and generates a benefit $\pi_i > 0$ to each state, reflecting the value its residents place on the crossing. The total benefit is $\pi = \pi_1 + \pi_2$, and the project is efficient if $\pi \geq c$. Under the sharing agreement, the cost of a completed project is split evenly, so each state pays $c/2$.

Before a project can be undertaken, one state must bring it forward. Doing so entails a cost $\kappa > 0$ of identifying the need, planning the work, and engaging the neighboring state, which we refer to as the coordination cost. This cost is borne entirely by the proposing state and is not reimbursed. Under the agreements described above, the proposing state is the designated lead, which we label state 1. The other state, state 2, then approves or declines the project. If it approves, the project is completed and each state pays its half; if it declines, nothing is built and the lead state has spent κ for nothing.

State 2 approves a proposed project if and only if its benefit covers its share of the cost,

$$\pi_2 \geq \frac{c}{2}. \tag{1}$$

Anticipating this decision, state 1 proposes a project if and only if it expects approval and its own benefit covers its share of the cost plus the coordination cost,

$$\pi_1 \geq \frac{c}{2} + \kappa. \tag{2}$$

A shared bridge is maintained in a given period only when both conditions hold.

Under sole responsibility, a single state receives the full benefit π, bears the full cost c, and plans the project internally at a cost $\kappa_0 < \kappa$. It undertakes every project with $\pi \geq c + \kappa_0$. The comparison with the shared bridge is clearest in the symmetric case $\pi_1 = \pi_2 = \pi/2$, where conditions (1) and (2) reduce to $\pi \geq c + 2\kappa$. Projects with

$$c + \kappa_0 \leq \pi < c + 2\kappa \tag{3}$$

are undertaken by a sole owner but not on a shared bridge. The lead state captures half of the benefit of the project but pays the entire coordination cost, and no project goes forward unless the lead state finds it worthwhile to propose. Shared responsibility can therefore only reduce maintenance relative to the sole-responsibility benchmark, and the width of the interval in (3) is increasing in κ. Because the decision recurs over time, a shared bridge

[4] In the model, we will refer to the two jurisdictions as states, though in practice they may be sub-state jurisdictions such as counties or cities instead.

accumulates a shortfall of maintenance relative to the unshared benchmark.

Two features of the model deserve comment. First, the predictions do not depend on which state is the lead. Approval by the non-lead state is required regardless of who proposes, and the project fails whenever either state's benefit falls short of its obligations. Allowing either state to propose at cost κ does not change the conditions for a project to go forward: the state with the larger benefit would propose, and the other would still have to approve.[5] We do not observe which state leads on each bridge in our data, and the mechanism does not require it.

Second, the model replaces the free-rider problem of a volunteer's dilemma with a hold-up problem. In a volunteer's dilemma, each state would be willing to bring the project forward if it knew the other would not, and the project fails because each waits for the other (Diekmann, 1985). The sharing agreements we observe remove this ambiguity by designating who proposes. What remains is that the proposing state sinks the coordination cost while receiving only part of the benefit, and that the non-lead state can decline to pay. The two mechanisms deliver the same prediction for the level effect and for the role of bridge value, and differ in the role of asymmetry between the two states, as we discuss below.

2.3 Empirical Predictions

The model generates four testable predictions.

Prediction 1: average level effect. Relative to the sole responsibility benchmark, shared bridges receive maintenance less often, so their sufficiency ratings should be lower at the end of the observation window than those of comparable sole-responsibility bridges.

Prediction 2: bridge value. We proxy for the benefits of maintenance π using the average volume of daily traffic on a bridge. For a high-traffic bridge, the benefit of a typical project sits well above $c + 2\kappa$ and both conditions (1) and (2) hold. For a low-traffic bridge, a larger share of projects falls in the interval (3), where a sole owner would act but the lead state does not find it worthwhile to propose. The treatment effect should therefore be larger for low-ADT bridges and smaller for high-ADT bridges.

Prediction 3: coordination costs. Frictions between the two states raise the cost κ of planning a project jointly and engaging the neighbor. These frictions widen the interval (3) and so increase the number of efficient projects that go unproposed. We proxy for frictions

[5] With either state able to propose, a project is undertaken if and only if $\max_i \pi_i \geq c/2 + \kappa$ and $\min_i \pi_i \geq c/2$. Designating a lead state fixes which state bears κ but does not alter the set of projects that fail.

with the partisan distance between the two states. The penalty should be larger where frictions are greater, so the interaction of partisan distance with the shared indicator should be negative. Because the lost projects lie just above the sole-owner threshold, an increase in κ does more damage where the benefits of maintenance are close to its cost. We therefore expect the interaction with partisan distance to be concentrated among low-ADT bridges and close to zero for high-ADT bridges.

Prediction 4: asymmetry in value. Differences in bridge value between neighboring states could either lessen or worsen the shared bridge penalty. Starting from a symmetric valuation, we ask whether adding valuation asymmetry changes the likelihood of a project and in what direction. Hold the total value π fixed in the interval (3), where a sole owner would act but a shared bridge with equal stakes is not maintained. Under symmetry the proposal condition (2) binds while the approval condition (1) is slack by κ: the non-lead state would be willing to pay up to $\pi/2$ but is asked only for $c/2$. Shifting value toward the proposing state by up to κ relaxes (2) without violating (1). In particular, with $\pi_2 = c/2$ and $\pi_1 = \pi - c/2$, the project goes forward whenever $\pi \geq c+\kappa$, recovering the projects in (3) with $\pi \geq c + \kappa$ and leaving only those between $c + \kappa_0$ and $c + \kappa$ unbuilt. A moderate asymmetry in value therefore leads to accepted projects that would otherwise not occur because of the coordination cost.

However, if the asymmetry is great enough, projects could become less likely. To illustrate this, assume the lead state has the higher value. For a given total value $\pi = \pi_1 + \pi_2$, a large enough asymmetry would leave the secondary state with a value below $c/2$, and it would therefore reject a proposed project. In this case, the coordination problem would be even worse under asymmetry.

One realistic extension to the model that would restore the benefits of asymmetry would be to allow for costly renegotiation of sharing terms, so that in extreme cases of asymmetry the high-value state could agree to incur the bulk of the project costs.[6] With renegotiation, a high degree of value asymmetry could once again solve the coordination problem, in a similar manner as in standard games of public good provision, where asymmetries can overcome the free-rider problem if the public good is provided by the party with the highest valuation (Olson, 1965; Bergstrom et al., 1986).

In the empirical section, we consider two proxies for asymmetry in value, the imbalance in commuting flows between the two states and differences in state highway disbursements.

[6] If renegotiating the terms of the agreement costs r, the project proceeds whenever $\pi_1 \geq c - \pi_2 + \kappa + r$, that is, whenever $\pi \geq c + \kappa + r$, so that asymmetry is never fatal but extreme asymmetry is costly. The Louisville-Southern Indiana Ohio River Bridges project, in which Kentucky bears roughly 72 percent of project cost, shows that unequal shares are negotiated for major projects.

3 Data

3.1 Data

The primary data used in this study comes from the National Bridge Inventory (NBI). The Federal Highway Administration requires states to inspect all public highway bridges at least every 24 months and to report inspection results to the NBI. The NBI contains records for every public highway bridge in the United States, covering approximately 600,000 unique structures per year of the sample window.

The analysis sample covers 1996 to 2018, the period for which the NBI data structure and coding conventions are most consistent. The full dataset contains approximately 13.4 million bridge-year observations covering approximately 802,000 unique bridges. Data are obtained from FHWA's annual release files, with variable definitions following the NBI Recording and Coding Guide (Federal Highway Administration, 2012). The analysis sample keeps only bridges maintained by state or local government agencies within the continental United States.[7]

A bridge is defined as treated if it records a shared maintenance arrangement with another state. States report the percentage of maintenance costs that are split with the neighboring state, and we define shared bridge status if this share is between 5 and 95 percent. The small number of bridges with a reported share that is low but not zero or one are treated as effectively not shared. The cost-share is exactly 50% for 82 percent of treated bridge-years. Treatment status changes during the sample for 251 bridges. We exclude these observations from the analysis sample because switching is endogenous to large maintenance expenditures, and due to the risk of measurement error. Figure 1 displays the location of all treated bridges, and Figure 2 displays a histogram of the cost-share percentage.

Applying this definition yields 766 shared bridges, of which 734 have a complete set of matching covariates. The pool of potential controls consists of the roughly 649,000 remaining bridges with complete covariates. Bridges enter and exit the inventory over the sample period as new structures are built and old ones are replaced or retired, so the window over which we observe a bridge varies.[8] To hold the observation window roughly fixed across bridges, our primary sample is a balanced panel of bridges observed at least once during both 2000–2004 and 2014–2018. This leaves 455 shared bridges and approximately 417,000 potential controls.[9] If the most poorly maintained shared bridges are the ones replaced, the estimated

[7]This excludes private, railroad, toll-authority, and federal bridges. Bridges outside the continental United States do not share borders with other jurisdictions in the data.

[8]Three in five shared bridges are observed over a span of at least 16 years, while one in eight is observed for three years or fewer.

[9]Importantly, exit from the inventory is unrelated to shared status. Among bridges present by 2004,

treatment effect is likely attenuated in the balanced panel. Appendix Table A6 reports the number of observations associated with the various sample restrictions.

In Figure 1, we show the location of the shared bridges in the balanced sample and the matched set of controls that will be obtained later in the empirical results. Shared bridges are observed throughout the country, and they tend to have nearby matched controls. That said, the map does reveal shared maintenance bridges are concentrated along borders defined by major rivers. In the empirical design, we will consider robustness with respect to restricting the pool of bridges to those crossing spans of water.

The primary outcome variable is the sufficiency rating (SR), a composite index ranging from 0 to 100 constructed from measures of structural condition, adequacy, and essentiality for public use. Structural condition is measured on a 1-10 scale for each of the relevant bridge components.[10]

In addition to inspection outcomes, the NBI records a set of physical and traffic characteristics for each structure, which we use to find comparable control bridges. These include the year the bridge was built, its length, deck width, and number of traffic lanes, the material and design of the main span, average daily traffic (ADT), the share of traffic that is trucks, and whether the route carried is on the National Highway System. We measure each at the bridge's first observation in the data. We summarize bridge size as the product of structure length and the number of lanes.[11] One variable requires an imputation. The coding guide permits the truck share to be left blank when ADT does not exceed 100 vehicles, and we set it to zero in these cases, which is the modal value among low-traffic bridges that report it. Where the truck share is blank at baseline for a bridge with higher traffic, we use the first value reported in a later inspection.

3.2 Summary Statistics

Table 1 reports summary statistics for the sample of treated bridges, all untreated bridges, and the subsample of matched controls. Treated bridges have a worse sufficiency rating at baseline, and this difference is wider at the end of the sample. Similarly, the average component condition is lower for treated than control bridges, both at baseline and at endline.

22 percent of shared bridges exit before 2014 compared with 26 percent of other bridges, and the difference within border pair is -0.7 percentage points with a standard error of 4.2. The shared bridges that do exit are in notably worse condition, with an average final sufficiency rating of 49.5 compared with 61.1 for other exiting bridges.

[10] These are: the deck (which carries traffic and includes the wearing surface), the superstructure (the load carrying elements between the supports), the substructure (the bridge supports, such as abutments, piers, and so on), and, if relevant, channels and culverts.

[11] Deck width is not recorded for culverts and is missing for 14 percent of bridge-years. The matching distance uses the log of deck area, the product of deck width and length, where it is available.

The same comparison for the treated against the matched set of controls previews the main result, as the sufficiency ratings of the two groups are similar at the beginning of the sample, but a substantial gap opens by the end. The characteristics of shared bridges are notably different than control bridges along many dimensions. They are larger, see more traffic, and are more likely to be on the national highway system. It will be important to match on key traits, and then control for bridge characteristics in the weighted regressions to account for differences between treated and control units.[12]

4 Estimation Strategy

4.1 Empirical design

The identification challenge is that treated and control bridges may differ systematically in ways that independently predict sufficiency rating trajectories. Treated bridges may be located at crossings with different traffic patterns, structural characteristics, or regional economic conditions. There is little panel variation in shared status to implement a standard difference-in-differences design.[13] Instead, we employ a research design that identifies the effects of shared responsibility from the different trajectories of bridges in similar condition at baseline, which relies on a weaker identification assumption.

As described in the theory section, we hypothesize that bridges under shared maintenance receive less maintenance and improvement investments than similar bridges maintained by a single jurisdiction. Therefore, shared bridges should experience a differential trend in sufficiency or condition over our sample period. Because the shortfall in investment accrues every year, its effect on condition accumulates over the observation window rather than being fully reflected in the baseline level that we condition on. Our identifying assumption is that conditional on baseline condition and observed bridge characteristics, shared-maintenance and single-state bridges would experience the same expected change in condition during the observation window if both were maintained by a single state. This permits the possibility that shared maintenance affects baseline bridge condition, either through selection into treatment based on initial levels of bridge sufficiency or through prior deterioration. This

[12] Appendix Figure A1 plots the distributions of log deck area and log ADT for treated and control bridges before and after matching. The unmatched control distribution of area sits well to the left of the treated distribution; the size caliper discards the part of the treated distribution without comparable controls, and the matched distributions overlap closely.

[13] Panel variation could be problematic, in that sharing agreements could change endogenously in advance of major investments. Furthermore, because the effects of a change in shared maintenance would not be instantaneous, identification would still rest on comparing changes in condition over long differences, as in the design we use here.

makes it weaker than the familiar conditional independence assumption.

Formally, we assume $E[Y_{ie}(0) \mid D_i, X_i, B_i] = E[Y_{ie}(0) \mid X_i, B_i]$ where Y_{ie} is the value of the outcome variable at endline, X_i are baseline covariates, and B_i is the value of the outcome variable at baseline.[14] Our "match-first/observe-last" empirical design matches each treated bridge to a sole-responsibility control bridge on a set of baseline (first-observation) characteristics, using Mahalanobis nearest-neighbor matching with replacement. In the second step, last-observation outcomes, either sufficiency rating or bridge condition, are compared across matched pairs, with additional regression adjustment for residual baseline differences, which removes the finite-sample bias that arises from matching on continuous covariates (Abadie and Imbens, 2006). We also consider the average annual change in sufficiency rating as an additional outcome. Doing so places bridges observed for different lengths of time on a comparable annualized scale.

Specifically, for treated bridge i and its matched control, indexed by matched set $s(i)$, we estimate

$$Y_{ie} = \beta D_i + \gamma B_i + X_i'\delta + \alpha_{s(i)} + \varepsilon_i, \tag{4}$$

weighting each observation by its matching weight, where X_i contains the first and last year observed, age, log area, and log ADT, and $\alpha_{s(i)}$ is a matched-set fixed effect. Because controls are matched with replacement, β estimates the average treatment effect on the treated. We also report a specification with only the matched-set fixed effect.

The matching covariates include the baseline outcome, age at first observation in the data, log bridge area, log of average daily traffic (ADT), truck share of ADT, a National Highway System indicator, border-pair indicators, and exact matching on structure type. Calipers of 1.0 standard deviations on bridge area, and 0.25 standard deviations for baseline outcome and log ADT, are imposed to prevent area-mismatched pairs from inflating estimates. We also report the robustness of the results to alternate choices for the caliper. Because controls are matched with replacement, a control bridge that serves as the match for several treated bridges appears several times in the matched sample. [15] Standard errors are therefore clustered by bridge, which absorbs the correlation across repeated appearances of the same control (Abadie and Spiess, 2022).

[14] Conditioning on baseline outcomes could introduce bias due to mean reversion if inspections have measurement error. The sign of this bias is unclear a priori. As a robustness check, we also re-match and re-estimate using the mean outcome in the first two inspections as the baseline, rather than just the first inspection alone, to reduce measurement error. On the balanced panel, the regression-adjusted estimate for all bridges moves from −4.48 (s.e. 1.30) to −4.37 (s.e. 1.20), and the unadjusted estimate from −5.51 (s.e. 1.14) to −5.26 (s.e. 1.10).

[15] Appendix Table A7 reports the extent of reuse for each matched sample. In the preferred all-owner match, 436 treated bridges draw on 385 distinct controls, no control is used more than four times, and the Kish effective number of controls is 341. Reuse is similarly limited in the owner and traffic strata.

Figure 3 presents standardized mean differences before matching, after matching without a size caliper, and after matching with the preferred calipers. Prior to matching, treated and control bridges differ substantially on several key covariates: baseline sufficiency rating is lower for treated bridges, while bridge area, log average daily traffic, truck share of traffic, and whether the bridge is on the National Highway System are all significantly higher. After matching, only bridge area and truck share remain outside the 0.1 SMD threshold, and once a loose size caliper is applied, bridge area is statistically similar between treated and control. All subsequent analysis uses the loose caliper as the preferred specification. Appendix Table A1 reports the regression-adjusted estimate for state agency bridges under each choice: −3.14 (s.e. 1.70) with no size caliper, −5.12 (s.e. 1.36) with the strict caliper, and −5.03 (s.e. 1.45) with the preferred caliper. The estimate without a size caliper is smaller and less precise, consistent with the poorly matched large treated bridges adding noise rather than signal.

4.2 Traffic Heterogeneity

The model predicts a larger shared-maintenance penalty for low-traffic bridges, where the benefit of a project to the lead state does not cover its share of the cost plus the cost of bringing the project forward. We test this prediction using state agency bridges, splitting the sample at the median ADT of treated bridges (7,569 vehicles per day). Within each traffic group, we re-match using the covariates, calipers, and exact match on structure type from the main specification, then estimate equation (4). This procedure keeps treated and control bridges in the same traffic group and applies the matching criteria and balance checks separately to each group. We use this traffic split throughout the heterogeneity analysis.

4.3 Friction Asymmetry

We test Predictions 3 and 4 using three measures of the differences between the states sharing a border. The first is commuting asymmetry, the absolute difference between the number of residents of each state who work in the other, as a share of their sum. We compute it from the LEHD Origin-Destination Employment Statistics (LODES), which report job counts by home and work census block, aggregating interstate flows to the state pair and averaging over 2010–2014.[16] Commuters are the users of a crossing, so the imbalance in flows proxies for how the value of the crossing divides between the two states. The second is the spending gap, the absolute difference in total annual highway disbursements between

[16]The ratio is noisier where flows are small, so specifications that include commuting asymmetry also control for the log of total commuter flow between the two states and its interaction with treatment. Every border pair with a shared state agency bridge has at least 100 commuters per year in each direction.

the two states, averaged over 2000–2018.[17] This measure is a second, coarser proxy for asymmetry in the stakes of the two states in the crossing. The third is the partisan gap, the absolute difference in the Republican share of the two-party vote in the 2000 presidential election, which proxies for the frictions of coordinating with a politically distant neighbor. We standardize each measure to mean zero and standard deviation one across the border pairs with bridges in our sample, so that each is fixed within a border pair. The measures are weakly correlated across border pairs, so we enter them jointly.

For a measure of state differences $f_{b(i)}$, where $b(i)$ denotes the border pair for bridge i, we add the measure and its interaction with treatment to the regression-adjusted specification:

$$Y_{ie} = \beta_1 D_i + \beta_2 \left(D_i \times f_{b(i)}\right) + \beta_3 f_{b(i)} + \gamma B_i + X_i'\delta + \alpha_{s(i)} + \varepsilon_i. \tag{5}$$

We estimate equation (5) for the full matched sample of state agency bridges and separately for the low- and high-ADT matched samples. Although friction is constant within a border pair, matched sets can include bridges from different borders, allowing us to identify β_3. This coefficient captures the relationship between asymmetry and bridge outcomes common to shared and sole-responsibility bridges.

Our main interest is β_2, particularly in the low-ADT sample. For the partisan gap, the model predicts $\beta_2 < 0$: a higher cost of coordinating with the neighboring state raises the number of efficient projects that the lead state does not propose. For commuting asymmetry, Prediction 4 implies $\beta_2 > 0$ among low-ADT bridges, where a moderate asymmetry in value allows projects to proceed that would fail under equal stakes, and $\beta_2 \approx 0$ among high-ADT bridges, where both conditions of the model hold under any division of the benefit. The same prediction applies to the spending gap as a second proxy for asymmetry in value. As in the main specifications, we cluster standard errors by bridge.

5 Results

5.1 Main Endpoints Results

Table 2 reports the baseline ATT estimates of shared bridge status on the sufficiency rating. Columns (1) and (2) provide estimates for all bridges, while (3)–(4) and (5)–(6) restrict to bridges maintained by state agencies and local governments, respectively. Each specification matches treatment to control bridges on the covariates described above, while the specifications shown in even-numbered columns also include controls for bridge covariates in the

[17] This is specifically drawn from table SF-4 of FHWA Highway Statistics. The 2008 edition of table SF-4 is unavailable, so we omit that year when calculating the spending gap.

weighted regression. We find that shared bridges have a sufficiency rating that is 5.51 points lower at endline, or 0.245 standard deviations. In the regression-adjusted specification, this point estimate is equal to −4.48, likely reflecting the small remaining differences in covariates after matching.[18]

The estimated treatment effect is largely driven by state-maintained bridges. For these bridges, column (3) suggests that the shared bridge treatment effect is −5.62 sufficiency rating points. The regression-adjusted specification in column (4) yields a point estimate of −5.03. These estimates are substantially larger than those for locally maintained bridges. If a city or county is responsible for bridge maintenance, the shared-bridge effects are only −1.58 and −2.30 for the unadjusted and regression-adjusted specifications, and are not statistically significant. Henceforth, the main part of the paper will focus on the state-maintained bridges in which the treatment effect is concentrated. Where noted below, we also present results for local bridges in the appendix.

The difference in the effect sizes between state and local bridges is theoretically ambiguous. In the context of our model, a different effect size between these two types could be due to either differences in coordination costs or bridge value. Coordination costs are more important for the local government if they represent a fixed cost. On the other hand, coordination costs could be lower, as the local government will have fewer infrastructure assets to manage. Furthermore, a city could be more aligned with respect to politics or other preferences with its counterpart on the other side of the border. With respect to value, a bridge of a given size will be more important relative to the roadway network of a city or county than the state as a whole. The value of the bridge is therefore high enough to a locality to overcome frictions or inattention that would prevent investment in a larger jurisdiction.

We also consider alternative outcomes to sufficiency rating, and we present the baseline specifications for these outcomes in Table 3. For comparison, column (1) of this table repeats the regression-adjusted estimate for sufficiency rating among state-maintained bridges. In column (2), the dependent variable is an indicator for a sufficiency rating below 80. This is the threshold at which a bridge becomes eligible for federal rehabilitation funding, so this outcome captures whether shared status pushes bridges across a policy-relevant margin. It is also robust to any nonlinearity in how the sufficiency formula maps condition into points.

[18] Appendix Table A2 reports the same specifications on the full unbalanced sample, which adds bridges that enter or exit the inventory during the sample period. The estimates are qualitatively similar but smaller: −3.86 and −3.81 for all bridges, −2.29 and −2.91 for state agency bridges, and −1.54 and −1.32 for local bridges, in the unadjusted and regression-adjusted specifications respectively. In the unbalanced sample the pooled estimate exceeds the estimate in either ownership stratum. Because each sample is matched separately, the pooled match can pair a treated bridge with a control maintained by a different level of government, and the within-owner estimates are the cleaner comparison. The balanced panel does not display this pattern.

We find that shared bridges are 13.8 percentage points more likely to fall below the threshold, which is substantial relative to the treatment-group mean of 0.70.[19]

Next, we examine the condition of the bridge components. One drawback of sufficiency rating is that it reflects both bridge condition and other dimensions of how well the bridge meets the transportation needs of the area. It could be that underinvestment in maintenance is more important for bridge condition than for sufficiency rating. Also, if there is selection into treatment based on unobserved travel needs, then sufficiency differences conditional on bridge characteristics could be biased. Bridge components, namely the deck, superstructure, substructure, channel, and culvert, are each rated by inspectors on a scale from 0 to 9, where 9 indicates excellent condition, 7 good, 5 fair, 4 poor, and 3 or below serious to critical deterioration. A rating of 4 or lower on the deck, superstructure, or substructure triggers the structurally deficient designation. We average across the rated components to form a single continuous bridge-level measure of condition. Consistent with the difference in sufficiency rating, we find that bridge condition is 0.219 points lower for shared bridges, or 0.2 standard deviations.[20]

Lastly, in columns (4) and (5) of Table 3, we present specifications in which the outcome is the average annual change over the sample in sufficiency rating and bridge condition. The examination of these outcomes has two motivations. First, it places bridges on a common annualized scale, so that the estimates do not depend on the length of the observation window. Second, this outcome connects more closely to the hypothesized reason for the shared bridge treatment effect, that underinvestment in maintenance accumulates over time. We find that treated bridges lose an additional 0.259 sufficiency rating points and 0.011 condition points per year. Over the 21-year average observation window, these annual differences accumulate to roughly the endline level differences in columns (1) and (3).[21]

One concern with these comparisons is that shared bridges are, almost by construction, crossings of major rivers, while the matching procedure does not condition on what the bridge spans. Scour, ice, and navigation clearance could then produce a worse trajectory for treated bridges with no role for shared governance. Table 4 repeats each column of Table 3 with the sample restricted to bridges over a waterway, which includes 262 of the 299 shared state

[19] As with sufficiency rating, we find that the effect on the rehabilitation-eligibility indicator is concentrated among state-maintained bridges, as shown in Appendix Table A4.

[20] As with sufficiency rating and rehabilitation eligibility, we find that the effect of sharing on component condition is concentrated in state-maintained bridges. See Appendix Table A5.

[21] Appendix Table A3 reports the per-year outcomes for the unbalanced sample, where observation windows vary widely across bridges and the annualized outcome does the most work. The estimates are −0.068 (s.e. 0.106) for sufficiency rating and −0.008 (s.e. 0.006) for condition, smaller than in the balanced panel and not statistically significant. Bridges observed for only a few years contribute noisy annual changes, which is one reason we prefer the balanced panel.

agency bridges, and with the match run within that set and exact on whether the waterway carries navigation. The estimates are somewhat smaller but tell the same story. Shared bridges have sufficiency ratings 4.3 points lower, are 11.6 percentage points more likely to fall below the rehabilitation threshold, and lose 0.246 sufficiency rating points per year, each statistically significant. The effect on the level of component condition falls to −0.128 and is no longer significant at conventional levels, though the annual change in condition remains negative and significant.[22]

5.2 Bridge value and state asymmetries

The model predicts that the shared-bridge penalty should depend on bridge value and on its asymmetry between the sharing states. In Table 5, we present results where we estimate how the treatment effect varies by the average daily traffic (ADT) on the bridge, which proxies for the value of the crossing. In this table, we also estimate how the effect varies with the three measures of state differences described above: commuting asymmetry and the spending gap, which proxy for how the value of the crossing divides between the two states, and the partisan gap, which proxies for the frictions of coordinating with a politically distant neighbor. All three are standardized absolute differences. One standard deviation of the partisan gap is roughly 5 percentage points.

Column (1) of Table 5 reproduces the main estimate for state-maintained bridges from column (4) of Table 2. Column (2) adds the spending gap and the partisan gap and their interactions with the shared bridge indicator. We find that the shared-bridge penalty grows with partisan distance. A one standard deviation increase in the partisan gap increases the maintenance shortfall by around 0.14 standard deviations of the sufficiency rating. By contrast, a larger spending gap does not have a statistically significant effect on the treatment effect. Its interaction is positive but small and imprecisely estimated. Column (3) adds commuting asymmetry. In the full sample its interaction with the shared bridge indicator is 0.65 (s.e. 2.01), indistinguishable from zero, and the partisan and spending gap interactions are unchanged.

The remaining columns of Table 5 split the sample at the median average daily traffic (ADT) among treated state agency bridges, re-matching within each stratum. Prediction 2 of the model is that the treatment effect should be concentrated among low-value bridges, where the smaller benefit of maintenance leaves more projects that the lead state does not

[22]The condition columns re-match on baseline condition, as in Table 3, and within water crossings this match retains 239 of the 299 shared state agency bridges rather than 262. The smaller estimate reflects which treated bridges survive the baseline-condition caliper rather than the water restriction itself. Estimated on the same matched sample as column (1), with baseline sufficiency rating as the control, the condition effect is −0.156 (s.e. 0.073).

find worthwhile to propose. We find evidence consistent with this prediction. For bridges below median ADT, shown in column (4), shared maintenance lowers the sufficiency rating by 8.0 points, noticeably larger than the effect overall of 5.03. For bridges above median ADT, shown in column (7), the estimate is 3.9 points and is not statistically distinguishable from zero ($p = 0.13$), though the confidence interval does not rule out effects as large as those in the full sample.

Columns (5), (6), (8), and (9) add the interactions within each stratum. Among low-ADT bridges, column (5) shows a partisan gap interaction somewhat larger in magnitude than in the full sample, at -3.4 points per standard deviation, although with half the sample the estimate is no longer statistically significant ($p = 0.12$). The spending gap interaction remains small and insignificant. Column (6) adds commuting asymmetry, and here the pattern predicted by the model appears. A one standard deviation increase in commuting asymmetry reduces the shared-bridge penalty by 7.8 points (s.e. 3.7), while the partisan gap interaction grows to -6.8 points (s.e. 2.4) and becomes precisely estimated. The spending gap interaction stays small and insignificant. Among high-ADT bridges, shown in columns (8) and (9), all three interactions are close to zero, with the partisan gap at -0.6 and commuting asymmetry at 0.6, though both are imprecisely estimated. The treatment effect itself is -5.1 points and marginally significant in column (8) and -3.3 points and insignificant once commuting asymmetry is added.

Taken together, the pattern across columns is consistent with the model. Coordination costs are relevant only among low-value bridges, for which the higher coordination cost raises the likelihood of inaction. For bridges that receive more traffic, the benefit of maintenance is high enough that projects are proposed and approved despite the cost of coordinating. The evidence on this point is suggestive rather than definitive, because the stratified interactions are not precisely estimated and we cannot reject that the low- and high-ADT interactions are equal. Asymmetry in the value of the crossing, measured by commuting flows, offsets the penalty among low-value bridges, where a state with a larger stake is willing to bear the cost of bringing projects forward, and has no effect among high-value bridges, where projects proceed regardless. The spending gap, our second proxy for asymmetry in value, has no detectable effect in any sample. It is a coarser measure, since a difference in the size of two highway programs need not translate into a difference in the value either state places on a particular crossing, and we read the commuting result as the more informative test of Prediction 4.

5.3 Near-Border Falsification

A concern with the design is that bridges near a state line might be maintained less well regardless of whether responsibility for them is shared. Border regions are often far from a state's population centers and from the district offices that program maintenance, and a state may rationally place less weight on roads whose users are disproportionately out-of-state travelers. Either mechanism would lower the condition of bridges near a border for reasons unrelated to shared governance, and because every shared bridge sits on a state line, the main specification cannot separate the effect of sharing from the effect of location.

To address this concern we conduct a placebo test using bridges that are near a state line but do not cross it. These bridges share the location of the treated group but, because they lie entirely within one state, cannot be jointly maintained. We designate state agency bridges within a short distance of the nearest state line as the placebo group and match them to interior bridges 10 to 50 kilometers from the line, leaving the 5 to 10 kilometer band empty as a buffer. The matching procedure, covariates, and estimating equation are otherwise identical to the primary specification. If proximity to a border depresses maintenance on its own, the placebo coefficient should be negative and of a magnitude comparable to the shared-bridge effect. If instead the main estimate reflects shared responsibility, the placebo coefficient should be close to zero.

One complication is that bridges near a state line but not on it are far smaller than the interstate crossings in the treated group, which tend to span major rivers. We address this in two ways. Columns (2) and (4) of Table 6 reweight the placebo group to match the size distribution of shared bridges, by decile of length times lanes. Column (3) instead restricts both the placebo and control groups to bridges of at least 1,000 lane-meters.

Table 6 reports the results. Column (1) reproduces the primary estimate for state agency bridges. Column (2) defines the placebo group as bridges within 5 kilometers of a state line, reweighted by size. The estimated effect of proximity to a border is -0.2 points and is statistically indistinguishable from zero. Column (3) applies the size restriction instead of reweighting. The point estimate is larger, at -1.9 points, but only 201 placebo bridges and 88 effective control bridges survive the restriction and the estimate is imprecise.[23] Column (4) narrows the placebo group to bridges within 1 kilometer of a state line, the bridges whose location most closely resembles that of a shared crossing. Here we find a statistically significant effect of -1.7 points. The estimate is about one-third the size of the shared-bridge effect in column (1), and the two are statistically distinguishable.

Taken together, the placebo estimates provide little evidence that the main result reflects

[23] A further restriction to 2,000 lane-meters leaves 44 effective control bridges and is reported in Appendix Table A7.

the location of shared bridges rather than their maintenance arrangement. The specification with the largest sample yields a precise zero. The specification that most closely mimics the location of a shared bridge yields a negative estimate, which suggests that proximity to a border may account for a modest portion of the main effect, but the bulk of the shared-bridge penalty remains after netting it out. We read column (4) as an upper bound on the location confound, since bridges within 1 kilometer of a state line include some that serve border crossings and may themselves be affected by conditions on the shared bridge nearby.

6 Conclusion

This paper considers a previously unexplored topic in the fiscal federalism literature, namely whether shared responsibility for infrastructure assets leads to underinvestment. This speaks to the broader issue of public good provision in the presence of interjurisdictional spillovers, and the ability of joint decision making between affected jurisdictions to overcome the externality problem. We specifically study the case of bridges that carry highway traffic across state lines. These bridges generate large interstate externalities, and the responsibility for their upkeep is commonly shared between neighboring states. Using inspection data from the National Bridge Inventory, we find that bridges with shared responsibility have sufficiency rating scores that are around 5 points lower, and have structural components that are in worse condition, than otherwise similar interior bridges.

We present a model built on the terms of the agreements that govern shared bridges, in which one state must bear the cost of proposing a maintenance project and the other must approve paying for half of it. The proposing state captures only part of the benefit but pays the entire cost of bringing the project forward, so efficient projects go unproposed, and the approval requirement prevents a state that values the bridge highly from acting on its own. The model predicts that less maintenance will take place on shared bridges, that the shortfall will be concentrated among low-value bridges, that it will grow with the cost of coordinating between the states, and that a moderate asymmetry in the value of the crossing between the two states will reduce it. We find evidence consistent with each. Shared bridges with low daily traffic have worse outcomes. When states that are distant in their political preferences share bridge costs, the outcomes are worse, and this relationship is concentrated among low-traffic bridges. Among low-traffic bridges, the penalty is smaller where commuting flows between the two states are more unbalanced, though a coarser proxy for asymmetry, the gap in the states' highway spending, does not reduce it. These results point toward settings where shared management of infrastructure resources is more likely to be successful.

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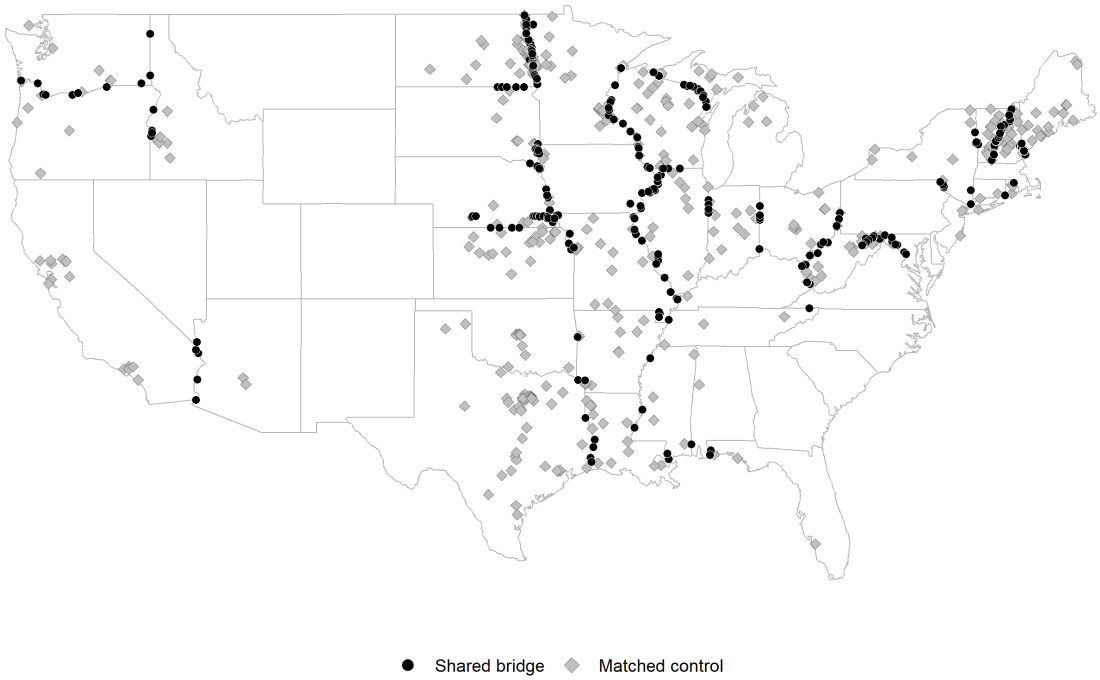


Figure 1: Bridge Location by Sharing Status

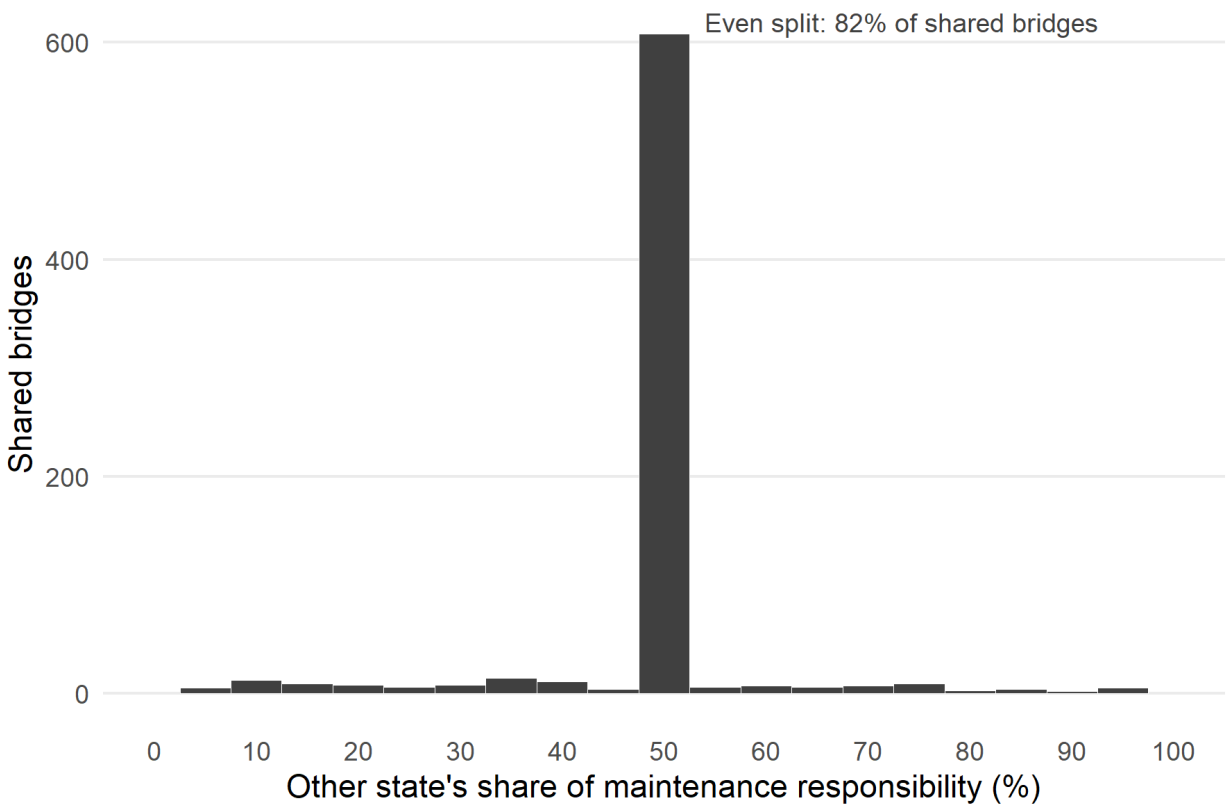


Figure 2: Distribution of Reported Maintenance Cost Share Among Shared Bridges

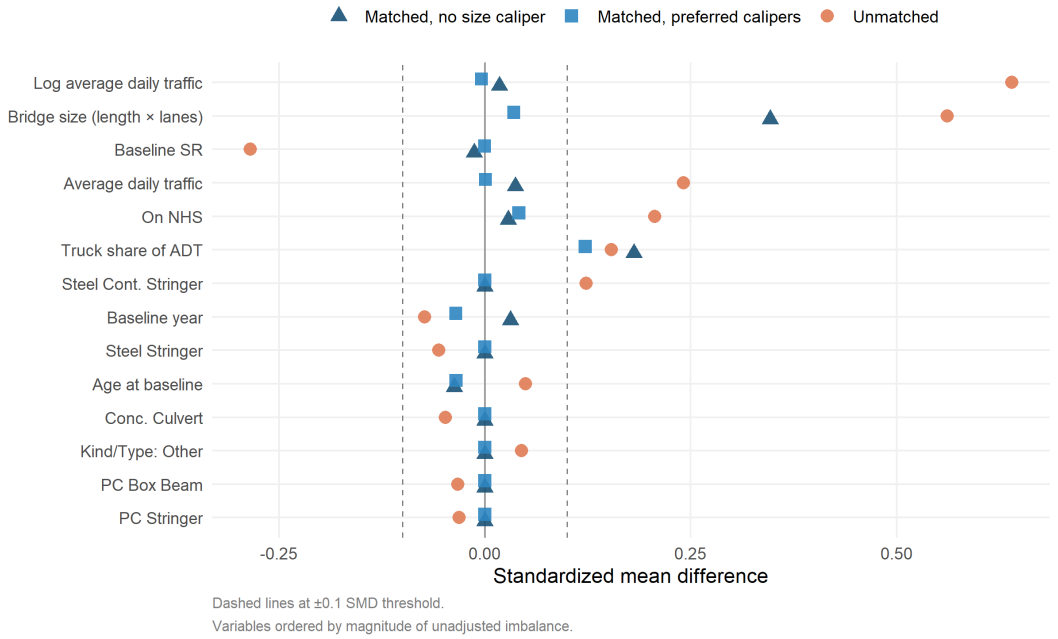


Figure 3: Covariate Balance Across Matching Specifications

Table 1: Summary Statistics: Treated, Control, and Matched Control Bridges

		Controls		Std. mean diff.	
Variable	Treated	All	Matched	Unmatched	Matched
Baseline sufficiency rating	71.00	77.39	71.25	-0.285	-0.000
	(22.45)	(21.41)	(22.55)		
Sufficiency rating, last obs.	69.58	78.83	75.53	-0.409	-0.241
	(22.63)	(20.44)	(20.90)		
Baseline component condition	6.79	6.86	6.71	-0.061	0.082
	(1.10)	(1.11)	(1.09)		
Component condition, last obs.	6.23	6.44	6.49	-0.222	-0.261
	(0.94)	(0.95)	(0.96)		
Traffic volume	9,913.67	5,637.81	8,894.42	0.241	0.001
	(17,713.33)	(17,735.19)	(14,974.71)		
Truck share of traffic (%)	7.87	6.66	6.74	0.154	0.123
	(7.85)	(7.63)	(5.96)		
Bridge size (length × lanes, m)	1,269.36	109.71	894.85	0.561	0.055
	(2,065.28)	(446.75)	(1,316.89)		
Age at baseline	33.24	32.14	34.18	0.050	-0.034
	(22.17)	(23.30)	(22.43)		
On national highway system	0.40	0.20	0.34	0.420	0.085
	(0.49)	(0.40)	(0.47)		
Years observed	21.42	21.32	21.42	0.059	-0.004
	(1.64)	(1.72)	(1.70)		
Deficient at baseline	0.58	0.44	0.56	0.282	0.018
	(0.49)	(0.50)	(0.50)		
Deficient at last obs.	0.62	0.40	0.51	0.461	0.211
	(0.48)	(0.49)	(0.50)		
Bridges	455	416,587	385		

Notes: Balanced panel (bridges observed at least once in 2000–2004 and at least once in 2014–2018), switchers excluded. Cells report the mean with the standard deviation in parentheses. "All" controls are the full unmatched pool; "Matched" controls are the preferred match (Mahalanobis, with replacement, exact on structure category, calipers on bridge size, baseline sufficiency rating and log traffic volume), with moments weighted by matching weights. Standardized mean differences are computed within each sample and scaled by the treated-group standard deviation of that sample; the matched columns compare the 436 treated bridges retained by the caliper (of 455) with their weighted matched controls. Component condition is the mean of the recorded deck, superstructure, substructure, channel, and culvert ratings, on a 0–9 scale; its matched column uses the sufficiency rating match, so baseline condition is not a matching variable there.

Table 2: Shared Maintenance and Endpoint Sufficiency Rating

Dependent variable:	Sufficiency rating					
Sample	All bridges		State agency		Local agency	
	(1)	(2)	(3)	(4)	(5)	(6)
Shared bridge	-5.51***	-4.48***	-5.62***	-5.03***	-1.58	-2.30
	(1.14)	(1.30)	(1.31)	(1.45)	(2.16)	(2.56)
Bridge covariates	No	Yes	No	Yes	No	Yes
Matched pair FE	✓	✓	✓	✓	✓	✓
Observations	872	872	578	578	274	274
R^2	0.73958	0.75912	0.76896	0.78913	0.68155	0.69206

Notes: The sample is the balanced panel: bridges observed at least once during 2000–2004 and at least once during 2014–2018. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. Sufficiency rating is the bridge's last observation in the data. Even numbered specifications also include the following covariates in the weighted regression: baseline sufficiency rating, first and last year observed, age at baseline, log deck area, and log traffic volume. State agency bridges are those whose maintenance responsibility is at the state level, and local agency bridges are maintained by county or city governments, each re-matched within type. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Alternative Outcome Definitions: State Agency Bridges

	SR (1)	Deficient (2)	Condition (3)	ΔSR/yr (4)	ΔCond./yr (5)
Shared bridge	-5.03***	0.138***	-0.219***	-0.259***	-0.011***
	(1.45)	(0.038)	(0.070)	(0.064)	(0.003)
Matched pair FE	✓	✓	✓	✓	✓
Observations	578	578	558	578	558
R^2	0.78913	0.71623	0.64814	0.74435	0.78299

Notes: The sample is the balanced panel: bridges observed at least once during 2000–2004 and at least once during 2014–2018. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. Sample is restricted to state agency bridges only. Columns: SR is the sufficiency rating; Deficient is an indicator for a sufficiency rating below 80; Condition is the component condition rating; ΔSR/yr and ΔCond./yr are the changes per year observed. Sufficiency rating is the bridge's last observation in the data. Structurally deficient is an indicator for a sufficiency rating below 80. Component condition is the mean of the recorded deck, superstructure, substructure, channel, and culvert ratings, and its column uses a separate match on baseline condition in place of baseline sufficiency rating. Changes per year are the last minus first value divided by the years observed; the change in condition uses the condition match. All columns include bridge covariates. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Restricting to State Agency Water Crossings

	SR (1)	Deficient (2)	Condition (3)	ΔSR/yr (4)	ΔCond./yr (5)
Shared bridge	-4.32***	0.116***	-0.128	-0.246***	-0.009**
	(1.56)	(0.040)	(0.081)	(0.065)	(0.004)
Matched pair FE	✓	✓	✓	✓	✓
Observations	524	524	478	524	478
R^2	0.779	0.689	0.593	0.778	0.757

Notes: Balanced panel, matched with the preferred calipers. State agency bridges over a waterway (NBI item 42B), re-matched within water crossings with an exact match on navigation control (NBI item 38). Deficient is an indicator for a sufficiency rating below 80. Condition columns use the match on baseline condition. Changes per year are last minus first value over years observed. All columns include bridge covariates. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Heterogeneity by Traffic Volume and State Asymmetries

Dependent variable:	Sufficiency rating								
Sample	All			Low ADT			High ADT		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Shared bridge	-5.03***	-5.77***	-6.06***	-8.00***	-8.56***	-9.01**	-3.90	-5.13*	-3.31
	(1.45)	(1.55)	(2.24)	(2.05)	(2.15)	(3.58)	(2.54)	(3.08)	(4.45)
Spending gap		0.958	0.809		0.281	-1.59		-0.264	-1.38
		(0.847)	(1.74)		(1.21)	(1.62)		(1.70)	(2.31)
Partisan vote gap		6.11***	5.45**		1.84	6.17**		-3.27	-3.10
		(1.99)	(2.18)		(2.66)	(3.01)		(3.91)	(4.30)
Shared bridge × Spending gap		0.905	1.24		-1.76	1.42		-1.75	-1.03
		(1.37)	(1.61)		(1.32)	(2.08)		(3.85)	(3.91)
Shared bridge × Partisan vote gap		-3.13*	-3.20*		-3.43	-6.78***		-0.283	-0.637
		(1.71)	(1.78)		(2.23)	(2.44)		(3.55)	(3.73)
Commuting asymmetry			0.018			-6.38*			-2.99
			(3.43)			(3.48)			(3.58)
Shared bridge × Commuting asymmetry			0.654			7.76**			0.633
			(2.01)			(3.73)			(3.10)
Matched pair FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	578	576	576	284	284	284	276	274	274
R^2	0.789	0.799	0.802	0.784	0.789	0.802	0.701	0.717	0.721

Notes: Balanced panel, matched with the preferred calipers. State agency bridges. Outcome is the sufficiency rating at the bridge's last observation. Spending gap: state highway disbursements. Partisan gap: Republican two-party vote share, 2000. Commuting asymmetry: absolute difference in interstate commuter flows between the two states as a share of their sum, from LODES origin-destination data averaged over 2010–2014. Columns (3), (6), and (9) also control for the log of total commuter flow between the two states and its interaction with the shared indicator. Traffic volume split at the treated-sample median. Gaps are standardized absolute differences between the two states. All columns include bridge covariates. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Placebo Exercise with Near-Border Bridges

Dependent variable:	Sufficiency rating			
Sample	Shared bridges	Placebo: non-shared bridges near a state line		
	(1)	(2)	(3)	(4)
Treated group	Shared	0–5 km	0–5 km	0–1 km
Control group	Non-shared	10–50 km	10–50 km	10–50 km
Size adjustment	None	Reweighted	Size $\geq$ 1,000 lane-m	Reweighted
Shared bridge	-5.03***			
	(1.45)			
Near border (placebo)		-0.208	-1.90	-1.71**
		(0.626)	(1.39)	(0.672)
Treated bridges	289	10,950	201	2,153
Effective control bridges	225	631	88	349
Matched pair FE	✓	✓	✓	✓
Observations	578	21,900	402	4,306
R^2	0.78913	0.72462	0.73510	0.76358

Notes: Column 1 repeats the state agency estimate from Table 2. Columns 2–4 replace shared bridges with state agency bridges near a state line but not on one, which cannot be shared, and match them to interior bridges 10–50 km from the nearest line; 5–10 km is left empty as a buffer. A non-zero coefficient in these columns indicates a location confound rather than an effect of shared maintenance. Distances are to the nearest state line. Near-border interior bridges are far smaller than shared crossings, so columns 2 and 4 reweight the placebo group to the decile distribution of shared-bridge size, and column 3 instead restricts both groups to bridges of at least 1,000 lane-meters (length times lanes). All columns include bridge covariates. A further restriction to 2,000 lane-meters leaves 44 effective control bridges and is not reported (Table A7). Effective control bridges is the Kish effective sample size $(\sum_b w_b)^2 / \sum_b w_b^2$, where w_b is the total weight carried by control bridge b, so a control reused many times is not counted as many independent observations. The sample is the balanced panel: bridges observed at least once during 2000–2004 and at least once during 2014–2018. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. Sufficiency rating is the bridge's last observation in the data. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A Supplemental Tables and Figures

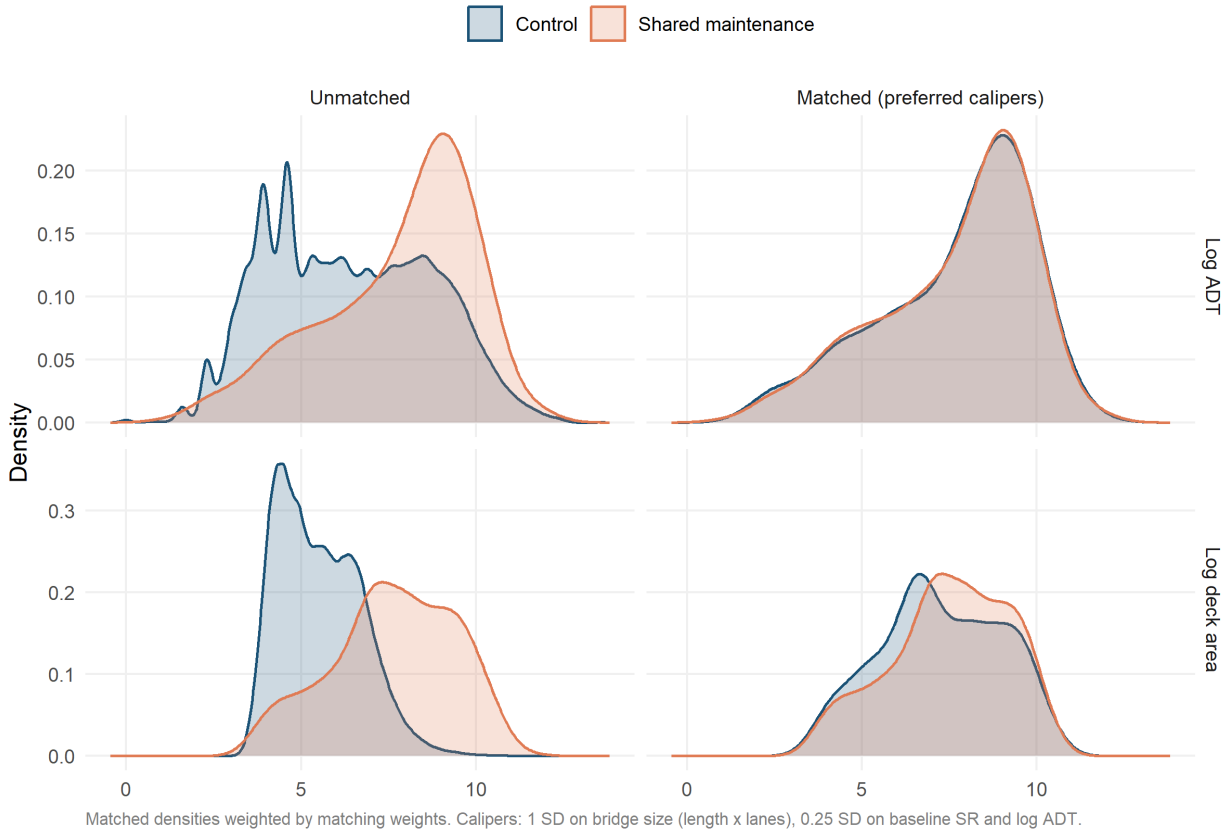


Figure A1: Common Support on Bridge Area and Traffic Volume, Before and After Matching

Table A1: Caliper Sensitivity: Endpoint Sufficiency Rating, State Agency Bridges

Dependent variable:	Sufficiency rating		
	No size caliper	0.5 SD size caliper	1 SD size caliper
	(1)	(2)	(3)
Shared bridge	-3.14*	-5.12***	-5.03***
	(1.70)	(1.36)	(1.45)
Matched pair FE	✓	✓	✓
Observations	622	542	578
R^2	0.76875	0.79335	0.78913

Notes: State agency bridges. The caliper on bridge size (length times lanes) is varied across columns, in standard deviation units, holding the 0.25 SD calipers on baseline sufficiency rating and log traffic volume fixed; the 1 SD column is the specification used throughout the paper. Sufficiency rating is the bridge's last observation in the data. All columns include bridge covariates. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Full Unbalanced Sample: Sufficiency Rating by Owner

Dependent variable:	Sufficiency rating					
Sample	All bridges		State agency		Local agency	
	(1)	(2)	(3)	(4)	(5)	(6)
Shared bridge	-3.86***	-3.81***	-2.29**	-2.91***	-1.54	-1.32
	(0.931)	(0.972)	(1.00)	(1.07)	(1.58)	(2.01)
Bridge covariates	No	Yes	No	Yes	No	Yes
Matched pair FE	✓	✓	✓	✓	✓	✓
Observations	1,411	1,411	963	963	402	402
R^2	0.76954	0.79074	0.81741	0.83060	0.80659	0.81784

Notes: Unbalanced panel: every bridge with complete matching covariates, without the balanced-panel restriction used in the main text. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. Sufficiency rating is the bridge's last observation in the data. Even numbered specifications also include the following covariates in the weighted regression: baseline sufficiency rating, first and last year observed, age at baseline, log deck area, and log traffic volume. State agency bridges are those whose maintenance responsibility is at the state level, and local agency bridges are maintained by county or city governments, each re-matched within type. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Full Unbalanced Sample: By outcome

	Sufficiency rating (1)	Change in SR per year (2)	Change in condition per year (3)
Shared bridge	-3.81***	-0.068	-0.008
	(0.972)	(0.106)	(0.006)
Matched pair FE	✓	✓	✓
Observations	1,411	1,354	1,357
R^2	0.79074	0.59799	0.59874

Notes: Unbalanced panel, all owners, without the balanced-panel restriction used in the main text. Changes per year are the last minus first value divided by the years observed. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. Sufficiency rating is the bridge's last observation in the data. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Structural Deficiency: Nested Specifications by Owner

Dependent variable:	Structurally deficient					
Sample	All bridges		State agency		Local agency	
	(1)	(2)	(3)	(4)	(5)	(6)
Shared bridge	0.103***	0.091***	0.138***	0.138***	-0.036	-0.035
	(0.030)	(0.031)	(0.033)	(0.038)	(0.050)	(0.057)
Bridge covariates	No	Yes	No	Yes	No	Yes
Matched pair FE	✓	✓	✓	✓	✓	✓
Observations	872	872	578	578	274	274
R^2	0.66432	0.67678	0.69946	0.71623	0.67111	0.67756

Notes: The sample is the balanced panel: bridges observed at least once during 2000–2004 and at least once during 2014–2018. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. The outcome is an indicator for a sufficiency rating below 80 at the bridge's last observation, estimated by OLS. Even numbered specifications also include the following covariates in the weighted regression: baseline sufficiency rating, first and last year observed, age at baseline, log deck area, and log traffic volume. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Average Component Condition: Nested Specifications by Owner

Dependent variable:	Component condition					
Sample	All bridges		State agency		Local agency	
	(1)	(2)	(3)	(4)	(5)	(6)
Shared bridge	-0.212***	-0.177***	-0.238***	-0.219***	-0.103	-0.063
	(0.055)	(0.058)	(0.063)	(0.070)	(0.115)	(0.142)
Bridge covariates	No	Yes	No	Yes	No	Yes
Matched pair FE	✓	✓	✓	✓	✓	✓
Observations	882	882	558	558	278	278
R^2	0.63238	0.65151	0.63326	0.64814	0.62060	0.63027

Notes: The sample is the balanced panel: bridges observed at least once during 2000–2004 and at least once during 2014–2018. Treated and control bridges matched on the outcome variable at baseline, log average daily traffic volume, truck share of traffic, age, a National Highway System indicator, log deck area, structure category, and border-pair fixed effects using Mahalanobis matching with replacement, with exact match on structure category (the five most common combinations of main-span material and design in the inventory, plus a residual category). Calipers of 1 SD on bridge size (length times lanes) and 0.25 SD on the baseline outcome and log traffic volume are applied. The outcome is the mean of the recorded deck, superstructure, substructure, channel, and culvert condition ratings at the bridge's last observation, on a 0–9 scale. Matching is on baseline condition in place of baseline sufficiency rating. Even numbered specifications also include the following covariates in the weighted regression: baseline sufficiency rating, first and last year observed, age at baseline, log deck area, and log traffic volume. Standard errors clustered by bridge in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Sample Construction and Attrition

Sample	Bridges	Treated	Observations
NBI bridge-year panel, 1996–2018	801,867	1,017	13,425,043
Switchers excluded	801,616	766	13,420,521
Endpoints collapse (one observation per bridge)	801,616	766	801,616
Complete cases on matching covariates	649,683	734	649,683
Balanced panel (observed 2000–04 and 2014–18)	417,042	455	417,042
Matched, no size caliper	863	455	910
Matched, 0.5 SD size caliper	802	426	852
Matched, 1 SD size caliper (preferred)	821	436	872
State agency stratum, matched	542	289	578
Local agency stratum, matched	265	137	274
Condition outcome, matched on baseline condition	838	441	882
Condition outcome, state agency stratum	525	279	558
Low ADT stratum, matched	271	142	284
High ADT stratum, matched	262	138	276
Spending gap non-missing (state agency, matched)	540	287	576
Unbalanced panel (no restriction), matched	1,320	707	1,414
Unbalanced panel, state agency stratum, matched	881	483	966

Notes: Bridges and treated counts are unique bridge identifiers; observations are rows in the estimation sample. Matching is with replacement, so matched rows exceed unique matched control bridges. Owner, balanced-panel, and ADT strata are re-matched within stratum rather than subset from the pooled match. Treated counts in the raw panel row reflect bridges ever coded as shared before switchers are dropped.

Table A7: Reuse of control bridges by specification

Sample	Treated bridges	Matched observations	Unique controls	Maximum reuse	Effective controls
All owners, preferred calipers	436	872	385	4	341
State agency	289	578	253	4	225
Local agency	137	274	128	3	120
Unbalanced panel (appendix)	707	1,414	613	5	531
Low traffic volume stratum	142	284	129	3	117
High traffic volume stratum	138	276	124	3	113
Near-border placebo, reweighted	10,950	21,900	7,785	17	631
Near-border placebo, area 1000+	201	402	132	9	88
Near-border placebo, area 2000+ (not in Table 5)	68	136	52	3	44

Notes: Matching is with replacement, so a control bridge may be matched to more than one treated bridge. Maximum reuse is the largest number of treated bridges matched to a single control. Effective controls is the Kish effective sample size, $(\sum_b w_b)^2 / \sum_b w_b^2$, where w_b is the total matching weight carried by control bridge b; it counts a control used twenty times once with weight twenty rather than as twenty independent observations. Placebo rows are the near-border samples of Table 5; the reweighted row's weights include the bridge-size reweighting. The size $\geq$ 2,000 lane-meter restriction is reported here only: nearly all of its placebo bridges are matched to a few controls, so its estimate is omitted from Table 5.