Victory, Institutions, and State Formation in Latin America

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Abstract

War outcomes alone cannot explain why defeat produced fiscal collapse in some states but not others. We argue that inherited colonial institutions condition war's fiscal consequences by shaping local administrative capacity. In a country-year panel of fourteen Latin American countries (1865-1913) with subnational institutional exposure measures, government revenue fell most sharply after defeat where inherited capacity was weakest, while it held up where capacity survived. Railroad construction shows no comparable pattern, consistent with its dependence on foreign concessionaires rather than domestic administration. Case studies of Peru, Paraguay, and Colombia trace how inherited capacity shaped war's fiscal consequences and postwar recovery.

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1 Introduction

The classical bellicist argument holds that external war pressures rulers to extract resources, centralize authority, and build the administrative machinery required to sustain coercion (Tilly, 1985). Recent work shows this logic applies, at least partially, to Latin America: nineteenth-century interstate war outcomes, not merely participation, shaped long-run fiscal and institutional trajectories (Schenoni, 2020, 2024). Yet identical war outcomes produced starkly different fiscal consequences across countries. Some defeated states saw fiscal capacity collapse; others proved resilient. Some victors consolidated extractive authority; others did not. Knowing who won or lost explains the direction of change, but not its magnitude or durability. What shaped how much the coalition disruption of war actually mattered?

We argue that colonial institutions moderate the fiscal consequences of war outcomes. Our central result concerns how defeat erodes fiscal capacity conditional on inherited institutions, a distinct claim that the elite-realignment mechanism we build on already implies. Colonial institutions shape how sharply a given war outcome affects fiscal capacity without changing themselves. By local administrative capacity we mean the governance infrastructure that colonial rule built, or failed to build, at the subnational level (Engerman & Sokoloff, 1997; Faguet, Matajira, & Sánchez, 2025; Mahoney, 2010). This extends Schenoni (2020, 2024)'s elite-realignment mechanism with an institutional dimension it leaves open: when war disrupts the central coalition, whether local fiscal extraction survives that disruption depends on this inherited capacity. Where colonial rule needed to organize local labor, it built this capacity as a byproduct of extraction (Faguet et al., 2025); where no such extraction took place, missions and other colonial administrators sometimes organized a dispersed population without any preexisting hierarchy to build on. Where this capacity existed, it could constrain how far peripheral elites empowered by defeat were able to dismantle the central government's fiscal authority; where it did not, defeat could remove the only thing sustaining that extraction, making collapse more likely. We do not directly test whether war builds state capacity through the fiscal-pressure mechanism emphasized by classical bellicism.[1]

War can also reshape local administrative capacity itself, rather than simply testing

[1]Even within the broader claim that war makes states, conflict produced different types of states under different conditions (Acemoglu & Robinson, 2019).

what colonial rule already built. Defeat can dissolve the local fiscal arrangements protected by the wartime coalition (Schenoni, 2020, 2024), while victory can let core elites build new capacity on inherited foundations, a slower process that depends on existing capacity and faces resistance from peripheral elites (Soifer, 2015). The distinction matters. Colonial institutions *moderate* the fiscal consequences of war without changing themselves, but when war reshapes local administrative capacity, the resulting postwar institutions, not the original colonial ones, *mediate* those consequences by carrying them forward independently of the colonial inheritance.

If local administrative capacity actively shapes how war's effects unfold, we cannot treat it as a fixed background condition. The standard empirical approach does exactly that, absorbing colonial institutions into country fixed effects and differencing away the variation this argument says matters. We treat colonial institutions as an active moderator instead. We cannot observe cabildos, tributary registries, and enforcement offices directly at the scale a panel requires. We operationalize local administrative capacity instead through the conditions associated with whether colonizers built them: extractive activity paired with a population dense enough to coerce built this capacity, while extractive activity without such a population did not. Bruhn and Gallego (2012) classify colonial regions along exactly these two dimensions, economic activity and the coercibility of local labor, and we use their classification as our measure. Using a difference-in-differences framework with a triple interaction, we extend Schenoni (2020)'s country-year panel with these subnational colonial institutional data, aggregated to country-level shares of exposure to extractive colonial institutions, and interact them with war outcomes to test whether the fiscal consequences of defeat were larger where colonial rule had failed to generate durable local administrative capacity. The panel estimates this moderating relationship; it cannot formally test the mediating relationship because we lack comparable data on how local administrative capacity changed after each war. The case studies provide qualitative evidence on the institutional processes through which war's fiscal consequences persisted. The design also does not test whether war altered countries' productive profiles. The Bruhn and Gallego (2012) classification captures colonial economic organization that predates the conflicts in our sample. We use it to examine how inherited administrative structures condition the fiscal consequences of war, not whether war transforms economic structure itself. The sample covers fourteen Latin American countries from 1865 to 1913, spanning the region's major interstate wars and the decades over which their fiscal consequences accumulated. These wars were large enough to test the mechanism, not brief border skirmishes: sustained mobilization, heavy fiscal demands, and clearly defined winners and losers characterize the conflicts in our sample, including the Paraguayan War (1864–1870) (Bethell, 1996) and the War of the Pacific (1879–1883), both of which reshaped regional power balances for decades afterward.

We examine two state capacity outcomes: government revenues and railroad construction. Government revenues are the paper's primary, confirmatory outcome and the field's standard measure of extractive capacity (Centeno, 2002; Thies, 2005). Thies (2005) generalizes Tilly's bellicist model to Latin America using the tax ratio as his outcome, but his analysis stops at 1900 because reliable nineteenth-century fiscal data did not exist in the source he used. Our revenue series, built from the CNTS archive, reaches into exactly the period his data could not cover, so it extends this literature's standard measure rather than substituting a different one. Defeat significantly eroded fiscal revenues in countries with greater exposure to extractive colonial institutions, while countries with stronger inherited local administrative capacity proved more resilient.

Railroad construction is a second, deliberately falsifiable outcome, not a competing core measure. Railroad construction shows no systematic relationship with war outcomes, and we expect this null. Foreign concessionaires, financed by external capital, built most nineteenth-century Latin American railroads and routed them according to elite interests in export access and land values, not the administrative machinery of the central state (Bignon, Esteves, & Herranz-Loncan, 2015; Bulmer-Thomas, 2003; Queralt, 2022). Railroad construction reflected the political economy of elite investment and factor endowments, not the extractive capacity the colonial legacy shaped. If the mechanism runs through local administrative capacity, its signature should appear in revenues and should not appear in railroads. That is what we find, and the divergence between the two outcomes is itself part of the test.

Three case studies, Peru, Paraguay, and Colombia, trace the processes the panel can only estimate in the aggregate. Peru and Paraguay both suffered catastrophic defeat but differed sharply in colonial institutional inheritance; Colombia examines the same mechanism after victory. They provide qualitative evidence, not a formal mediation test.

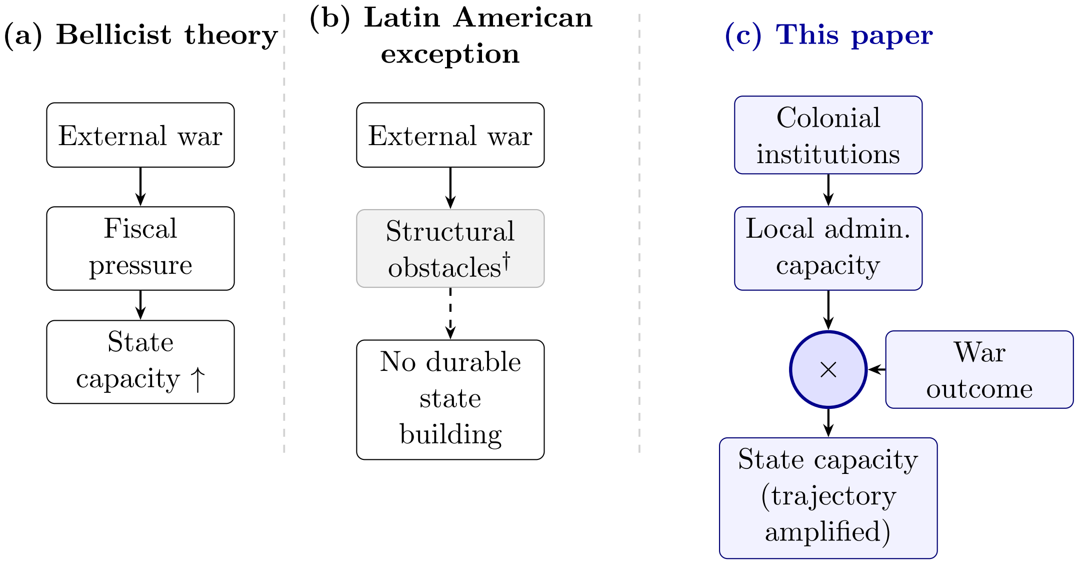
The paper contributes to three literatures. First, it distinguishes two roles institutions can play in translating war into fiscal change (Centeno, 2002; Thies, 2005; Tilly, 1985). Colonial institutions moderate the fiscal consequences of a given war outcome through the local administrative capacity they built or failed to build without changing themselves. The postwar institutions war can create or reshape mediate the longer-run consequences instead, carrying them forward independently of the original colonial inheritance. The mechanism itself is elite realignment (Schenoni, 2020, 2024), not the direct fiscal pressure of war (Goenaga, Sabaté, & Teorell, 2023), which is why their null average effect is consistent with, rather than a challenge to, this argument. Second, it advances research on colonial persistence (Engerman & Sokoloff, 1997; Mahoney, 2010), which typically shows colonial institutions shifting long-run averages over decades. We show the same institutions also shape something faster and more uneven: how sharply the elite realignment a war triggers translates into fiscal change. Third, it shows that fiscal capacity and infrastructure expansion answer to different political economies, and historical measures of state capacity need to respect that difference. Government revenue reflects domestic bargaining over who controls the fiscal apparatus; railroad construction reflects foreign concessionaires and capital markets, not the central government's own reach (Bignon et al., 2015; Bulmer-Thomas, 2003; Queralt, 2022). Treating both as interchangeable proxies for state capacity obscures this distinction. The core finding is this: war outcomes did not independently remake states. They revealed and accelerated what colonial institutions had already shaped about the local administrative capacity available to survive war.

2 Main Argument

Classic bellicist theories hold that external conflict pressures rulers to expand extraction, consolidate authority, and construct the institutions needed to sustain coercion (Tilly, 1985). War making and state making are mutually reinforcing: rulers bargain with societal actors to finance armies and, in the process, build more capable states. For Latin America, many scholars contend that the conditions underpinning this sequence were only weakly present: fiscal systems remained narrow, political authority stayed dispersed, and major interstate wars occurred comparatively infrequently (Bulmer-Thomas, 2003; Centeno, 2002). More recent work offers a different view, showing that nineteenth-century Latin America did experience wars intense enough to redirect institutional trajectories, and that war outcomes, rather than participation alone, shaped long-run development (Schenoni, 2020, 2024). Yet if war outcomes were the central turning point, a puzzle remains: why did comparable victories or defeats sometimes produce durable gains in state capacity while in other cases they left it unchanged or accelerated its decline?

We argue that colonial institutions moderate the effect of war outcomes on fiscal capacity, the state's ability to extract revenue domestically. For a given war outcome, the local administrative capacity a country inherited before conflict began shapes the trajectory fiscal capacity follows afterward. By local administrative capacity we mean local governance infrastructure (cabildos, tributary registries, enforcement offices) that colonial rule left behind and that can continue collecting revenue on its own, independently of whichever coalition controls the central government. Colonial institutions produced different levels of local administrative capacity depending on how colonial authorities organized extraction and local governance. Colonial and early postcolonial development produced deep subnational variation in this local infrastructure across Latin America (Engerman & Sokoloff, 1997; Mahoney, 2010). This variation did more than set background conditions: it shaped how much fiscal capacity could survive a major shock to the central coalition. Where colonial rule left no local administrative capacity, defeat could remove the central coalition and leave nothing behind to sustain extraction. Where colonial rule left durable local administrative capacity, that capacity could keep functioning on its own, and could even give core elites something to build on after a favorable shock. Explaining divergent postwar trajectories therefore requires knowing not just who won or lost, but what local administrative capacity war outcomes landed on. Figure 1 contrasts three logics: war building capacity directly (a), structural obstacles blocking it entirely (b), and war interacting with local administrative capacity to push a country's trajectory further up or down (c).

Figure 1: Competing theoretical logics: war outcomes and state capacity



Note: Panel (a): Tilly (1985). Panel (b): Centeno (2002). †Narrow fiscal base, dispersed political authority.

Different factor endowments, labor systems, and degrees of indigenous coercion generated this variation within Latin America's national borders (Diaz-Cayeros & Jha, 2016; Faguet et al., 2025; Guardado, 2018; Maloney & Valencia-Caicedo, 2016). Colonial administrative reforms later reshaped fiscal authority and elite coalitions further into the republican period (Chiovelli, Fergusson, Martínez, Torres, & Valencia Caicedo, 2023; Garfias & Sellars, 2025). Bruhn and Gallego (2012) classify colonial economic activities into good, bad, and ugly institutions based on economies of scale and the coercibility of indigenous labor. Bad and ugly institutions correlate with lasting inequality, weaker political representation, and worse long-run institutional outcomes (Bruhn & Gallego, 2012). Extractive colonial activity did not always leave local administrative capacity behind: it depended on whether colonizers needed to organize local labor to extract it. Where they did, encomienda municipalities built cabildos, enforcement offices, and fiscal registries that became the earliest nodes of the local state (Faguet et al., 2025).

War outcomes shift the domestic balance between core elites, who favor centralization and fiscal expansion, and peripheral elites, who resist central authority to preserve local autonomy (Bulmer-Thomas, 2003; Schenoni, 2020). Defeat delegitimizes the wartime coalition that core elites built, empowering peripheral elites to institutionalize state-weakening policies over time. Victory does the reverse. This is the political shock that tests whether local administrative capacity can survive: peripheral elites gain the opportunity to dismantle fiscal authority only when defeat delegitimizes the coalition protecting it.

Local administrative capacity does not only shape how a war outcome plays out; war can also change that capacity itself, and it does so asymmetrically. Defeat dissolves the wartime coalition immediately, and the local fiscal arrangements that depended on that coalition's protection collapse with it (Schenoni, 2020), an effect fast enough to show up within the postwar period this paper examines. Victory works more slowly: core elites can build new administrative arrangements on top of what colonialism left behind, but doing so takes years, depends on how much capacity already exists to build on, and faces resistance from peripheral elites who did not defect (Soifer, 2015), so its effects fall largely outside the window this paper studies. War can reshape local administrative capacity this way, whether by dissolving it after defeat or building it up after victory. When it does, the resulting postwar institutions, not the original colonial inheritance, *mediate* the longer-run fiscal consequences of war. War does not create institutional vulnerability; it reveals and accelerates trajectories already in motion.

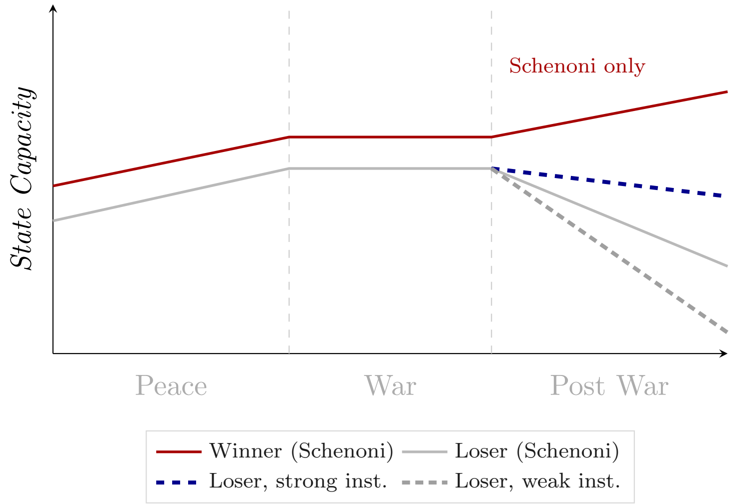
We test three observable implications, two of the moderating relationship and one of the mediating relationship. The moderating mechanism operates at the subnational level: local administrative capacity survives or collapses region by region. Fiscal capacity in this period, however, was itself centralized. Customs duties, collected at ports and borders, were the dominant source of government revenue across nineteenth-century Latin America, and by the eve of the First World War no country drew less than half of public revenue from them (Bulmer-Thomas, 2003). War outcomes are also a national-level treatment: countries, not provinces, sign peace treaties, cede territory, and take on war debt. Local administrative capacity therefore feeds mainly into a national fiscal system built around customs revenue, rather than into parallel subnational fiscal systems of its own. We measure institutional exposure as the national share of bad, ugly, and good institutions, which matches the level at which both the treatment and the outcome operate.

First, fiscal capacity should respond to war outcomes differently depending on that exposure, since local administrative capacity sustains extraction independently of central coalition politics. The Bruhn and Gallego (2012) typology specifies where this effect should be sharpest. *Ugly* institutions combined extractive intent with a population dense enough to coerce directly, generating the strongest local administrative capacity. *Bad* institutions combined the same extractive intent with too few people to coerce, so colonizers built no local administrative capacity at all. Defeat should therefore produce the most severe fiscal decline there. *Good* institutions saw no extractive activity. Where no exploitable hierarchy existed, colonial and missionary administrators sometimes built local governance from scratch instead. This capacity was not built for extraction, but it persisted just as durably. Second, railroad expansion should show no systematic relationship with war outcomes. Nineteenth-century railroads were built primarily by foreign concessionaires under state-backed guarantees and concession contracts, not by the state itself (Bignon et al., 2015; Bulmer-Thomas, 2003; Queralt, 2022). Their construction reflected foreign investment decisions and concession politics, not the state's administrative or fiscal strength (we return to the trade-finance logic behind this in the Discussion). We do not expect symmetric effects of victory for either implication within the postwar window this paper examines: Figure 2 shows why, with loser trajectories diverging sharply by institutional exposure while a winner's trajectory would only diverge on a longer horizon than this window covers.[2] Third, war-induced change in postwar institutions, not the original colonial inheritance, should mediate the longer-run fiscal consequences of war. We cannot test this directly, since we lack panel data on subnational institutional change after each war; we examine it instead through case evidence.

Three conditions bound the domain in which this argument applies. First, colonial institutions must have generated differentiated subnational elite structures that survived relatively intact into the conflict period. Second, wars must have been of sufficient duration and territorial stakes to alter the relative power of central and peripheral coalitions; short or low-intensity conflicts are unlikely to trigger the elite realignment the mechanism requires. Unlike the Tilly mechanism that Goenaga et al. (2023) examine, our mechanism runs through elite realignment rather than fiscal pressure, which is why their null average effect is consistent with our argument rather than a challenge to it. Third, fiscal capacity must reflect domestic extraction rather than external transfers. The cases we examine here satisfy all three conditions.

[2] We extend this logic to internal revolt episodes in the Appendix, drawing on Madrid and Schenoni (2024).

Figure 2: War outcomes and diverging postwar trajectories



Note: Solid lines reproduce Schenoni (2020)'s expected trajectories assuming no interaction between war outcomes and colonial institutions. Dashed lines show the trajectories we expect for defeated states: among defeated states, strong local administrative capacity moderates fiscal decline while weak capacity deepens it. The spread between the two trajectories represents the moderating relationship. We do not specify an expected trajectory for winners within the postwar window: victory creates a political opportunity, but translating that opportunity into measurable fiscal gains follows a longer and more contingent process. War does not cause this divergence; it reveals and accelerates institutional trajectories already in motion.

3 Data and Empirical Strategy

Testing the main argument requires two measures: war outcomes and the colonial institutions that, we argue, shaped how much local administrative capacity a country carried into war. We draw each measure from a different source.

3.1 Data and Variables

We build the analysis on two data sources. Schenoni (2020) supplies a panel of war outcomes, state capacity indicators, and time-varying controls for Latin American countries between 1865 and 1913. Bruhn and Gallego (2012) classify colonial economic activities into *good*, *bad*, and *ugly* categories based on economies of scale and the coercibility of indigenous labor. We use their subnational institutional classification and pre-colonial population density, which Bruhn and Gallego (2012) derive from the factor-endowment framework in Engerman and Sokoloff (1997) and anthropological population records from Denevan (1992)[3].

War outcomes follow the Correlates of War classification in Sarkees and Wayman (2010), which defines an interstate war as a military confrontation between sovereign states that kills at least 1,000 people in battle in a single year. State capacity indicators (railroad

[3] Table A4 reproduces a summary table based on Bruhn and Gallego (2012).

mileage and national government revenue per capita), together with controls for military size, public expenditure per capita, exports per capita, school enrollment, legislative effectiveness, and urbanization, all come from the Cross-National Time-Series Data Archive, 2025 edition (Banks & Wilson, 2025).

3.2 Sample

The sample combines the country–year panel from Schenoni (2020) with the institutional measures from Bruhn and Gallego (2012). Merging the two sources retains fourteen countries: Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Guatemala, Honduras, Mexico, Peru, Paraguay, El Salvador, Uruguay, and Venezuela. The final panel comprises 686 observations over 1865–1913, with 49 observations per country. The appendix reports descriptive statistics.

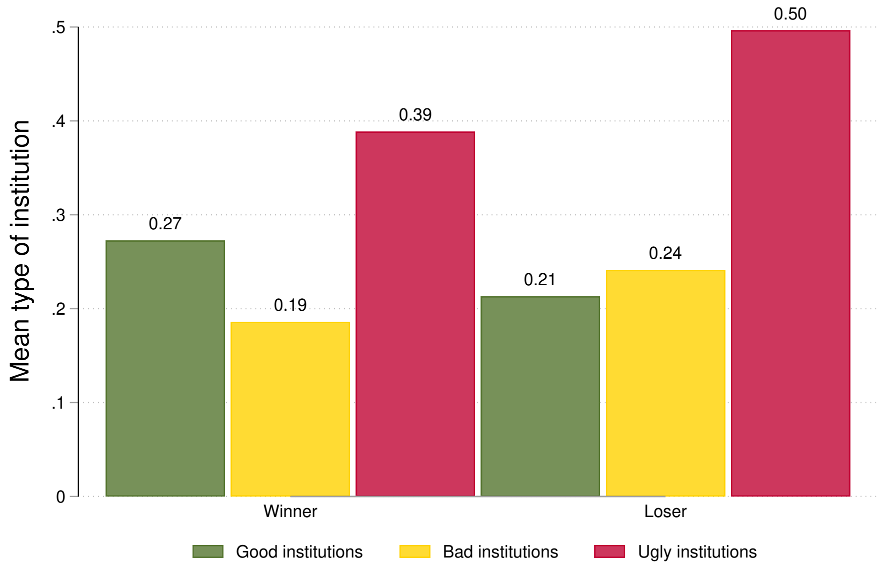
3.3 Empirical model

War outcomes may affect state capacity differently depending on a country's colonial institutional exposure. Schenoni (2020) absorbs colonial institutions into country fixed effects, which treats each country as institutionally uniform and discards the very differences our argument turns on.

Colonial institutions varied within countries, not just across them (Dell, 2010; Maloney & Valencia-Caicedo, 2016; Valencia-Caicedo, 2019). Bruhn and Gallego (2012) document that variation at the subnational level: they classify individual colonial-era jurisdictions, not whole countries, as *good*, *bad*, or *ugly*. We aggregate that subnational classification into a national-level variable, the *share* of each country's territory covered by each institutional type. This distinction matters for how to read what follows. Our unit of analysis is the country-year, the level at which war outcomes are decided and fiscal data are recorded; we do not run a subnational panel, and we make no claim to running one. But the variable that measures institutional exposure is not a coarse label assigned to each country as a whole. It is built from real subnational variation in colonial institutions, which Bruhn and Gallego (2012) document jurisdiction by jurisdiction and which Schenoni (2020)'s country fixed effects would otherwise absorb. Subnational variation enters this paper through that share variable, as an ingredient in a national-level empirical model, not as a separate level of observation.

Figure 3 shows the result: colonial institutional environments vary substantially even within winners and losers. The prevalence of ugly institutions is particularly striking among defeated countries, where the average share runs roughly 11 percentage points higher than among winners.

Figure 3: Average type of institutions by war outcome



Note: The institutional classification follows Bruhn and Gallego (2012). War outcomes come from Schenoni (2020).

Strong national-level effects on this measure can reflect concentrated subnational variation rather than a homogeneous countrywide legacy, exactly what a country fixed effect would hide.

We extend Schenoni (2020)'s difference-in-differences framework, which estimates the effect of losing an interstate war on postwar state capacity using country and year fixed effects. Rather than absorb colonial legacies into fixed effects, we let the effect of war outcomes vary with a country's exposure to each institutional type (the share of national territory historically classified as *good*, *bad*, or *ugly* by Bruhn and Gallego (2012)). The empirical specification is:

$$
\begin{aligned}
Y_{ct} = {} & \alpha_c + \delta_t + \beta_1\, Defeat_c + \beta_2\, Post_{ct} + \beta_3\, Institutions_c \\
& + \beta_4\,(Defeat_c \times Post_{ct}) + \beta_5\,(Defeat_c \times Institutions_c) \\
& + \beta_6\,(Post_{ct} \times Institutions_c) + \beta_7\,(Defeat_c \times Post_{ct} \times Institutions_c) \\
& + X'_{ct}\theta + \varepsilon_{ct},
\end{aligned}
\tag{1}
$$

where Y_{ct} denotes the outcome variable of interest for country c in year t. Our main outcome variables are railroad mileage, measured in kilometers, and government revenue. Both variables come from Banks and Wilson (2025). The terms α_c and δ_t represent country and year fixed effects, respectively, which control for time-invariant country characteristics and common temporal shocks. $Defeat_c$ is an indicator variable equal to one for countries that lost an interstate war, and $Post_{ct}$ is an indicator for the postwar period. $Institutions_c$ captures historical institutional exposure measured as the country-level share of territory associated with a given institutional type as classified by Bruhn and Gallego (2012); in the empirical analysis, $Institutions_c$ corresponds to the share of *good*, *bad*, or *ugly* institutions (estimated in separate specifications). The vector X_{ct} includes the set of time-varying controls introduced by Schenoni (2020).

Following Bruhn and Gallego (2012), we classify colonial institutional environments as *good*, *bad*, or *ugly* based on the interaction between factor endowments and pre-colonial population density. Some factor endowments (suitability for plantation crops or mining, for instance) generated strong incentives for extractive colonial activities, particularly when combined with access to coercible labor. *Ugly* institutions arise in regions with extractive factor endowments and pre-colonial population density above the median, where colonizers exploited local labor directly. *Bad* institutions correspond to regions with extractive factor endowments but low pre-colonial population density, where labor scarcity pushed colonizers toward imported labor instead. *Good* institutions describe regions with neither extractive factor endowments nor dense pre-colonial population, where colonial activities did not depend on systematic labor exploitation. Table A4 reproduces a summary table from the original paper.

The coefficient β_4 captures the average postwar effect of defeat when $Institutions_c =$

0. The coefficient β_6 allows postwar trajectories to vary with institutional exposure independently of defeat, while the coefficient β_7 is the main parameter of interest and measures how the postwar effect of defeat varies with the intensity of historical institutional exposure. All specifications include the same set of time-varying controls used by Schenoni (2020). All standard errors are clustered at the country level to account for serial correlation within countries over time[4].

Because the model includes triple interactions, we interpret results through the postwar marginal effect of defeat evaluated at different levels of institutional exposure. This maps the specification directly onto the heterogeneous effects we expect.

The argument yields two testable implications, each with a corresponding falsification criterion. Fiscal capacity should respond to defeat differently depending on *bad*-institution exposure, since bad institutions left no local administrative capacity behind to cushion the coalition's collapse; a null or wrong-signed triple interaction on government revenue would contradict the argument. Railroad expansion should show no systematic response to defeat, since railroads reflect concession politics and elite investment rather than the local administrative capacity shaped by colonial institutions; a railroad response that tracked *bad*-institution exposure the same way revenue does would undercut the claim that the two outcomes answer to different mechanisms. The argument does not address how railroad construction might vary across institutional subtypes beyond this flat null. Any such pattern in the data is exploratory by construction, and we treat it that way rather than as a further test of the mechanism.

Two measurement decisions come before the tables, so readers can separate them from what the results show. We correct a clustering error in Schenoni (2020)'s standard errors, applied identically across every outcome and institutional category, independent of what the triple interactions later show. We also apply the inverse hyperbolic sine transformation to both outcome variables, following Norton (2022), for a skewed, zero-heavy distribution known before any heterogeneity test. The raw-measure and log-transformed railroad results, and the sovereign-bond-yield placebo, are robustness checks instead, run after the main results to test stability rather than to build the specification.

[4]Standard errors reported in Schenoni (2020) are not correctly clustered at the country level; correcting them leaves fiscal results unchanged but makes railroad effects considerably more fragile.

4 Results

We evaluate both implications, beginning with a baseline replication.

4.1 Replicating results and comparing samples

Tables 1 and 2 compare the original Schenoni (2020) sample with the final merged sample. We found a clustering error in Schenoni (2020)'s standard errors and correct it throughout. We also report wild bootstrap p-values clustered at the country level.

Table 1 reproduces the coefficient estimates in Schenoni (2020), correcting standard errors for the right number of country clusters. Railroad expansion, one of the two proposed measures of state capacity, loses statistical significance. Logged government tax revenues remain significant at the 10% level. Columns 3 and 4 apply the IHS transformation specified above. Under IHS, the railroad coefficient changes sign and stays insignificant, while the revenue results barely move from the log specifications. Wild bootstrap inference confirms the pattern: railroads remain non-significant, and government revenues attain significance only at the 12% level.

Table 2 reports estimates for the merged sample, which cuts the number of country clusters to fourteen. The effect on government revenues grows larger and more precisely estimated. Railroad expansion stays statistically insignificant and continues to shift with functional form assumptions.[5]

Railroad expansion is a fragile proxy for state capacity in this sample; government revenues hold up far better across specifications. We adopt the IHS transformation for railroad extension, as specified above. As a robustness check, appendix specifications using the raw measure yield the same conclusion. We do not treat railroad construction as a direct measure of state capacity, and we examine why in detail in the next subsection.

[5] As a robustness check, we also transform the railroad variable using logarithms; the results lead to the same conclusions.

Table 1: Schenoni Sample

	(1) Railroad exp	(2) log Gov revenue	(3) IHS Railroad	(4) IHS Gov revenue
Defeat × Post	-391.912	-0.185	0.157	-0.185
	(449.192)	(0.105)*	(0.728)	(0.105)*
Observations	882	882	882	882
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Clusters	18	18	18	18
Mean	1294.40	6.10	5.53	6.79
W.bootstrap p-value	0.47	0.12	0.87	0.12

Cluster standard errors at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Final sample merged with Bruhn and Gallego

	(1) Railroad exp	(2) log Gov revenue	(3) IHS Railroad	(4) IHS Gov revenue
Defeat × Post	-706.559	-0.296	0.663	-0.296
	(679.602)	(0.096)***	(0.818)	(0.096)***
Observations	686	686	686	686
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Clusters	14	14	14	14
Mean	1640.44	6.07	6.11	6.77
W.bootstrap p-value	0.37	0.02	0.56	0.02

Cluster standard errors at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Heterogeneous effects from colonial institutions

Appendix Tables A1, A2, and A3 report the full regression results for the triple-interaction model in equation 1. For IHS government revenue, the triple interaction involving exposure to *bad* institutions is negative and statistically distinguishable from zero at the 10% level, while the corresponding interactions for *good* and *ugly* institutions are not. We focus on *bad*-institution exposure because it represents the institutional environment with the least inherited local administrative capacity and produces the clearest estimated heterogeneity in post-defeat fiscal trajectories.[6]

The same *bad*-institution measure also provides the clearest empirical test of our expectation that railroads show no systematic relationship with war outcomes. *Bad* institutions are the category with no local administrative capacity at all, so if railroad construction ran through that capacity the way revenues do, it should respond to *bad*-institution exposure too. It does not: Table A3 shows the triple interaction of defeat, postwar, and *bad* institutions on IHS railroad expansion is small and statistically indistinguishable from zero (wild bootstrap $p = 0.70$). This confirmatory test produces the null relationship we expect.

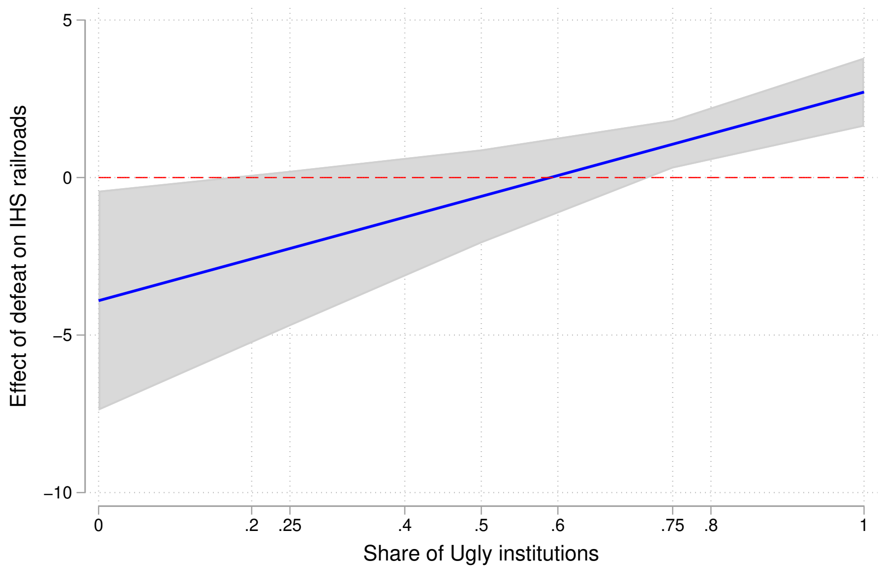
A second, exploratory pattern appears under exposure to *ugly* institutions specifically.[7] Figure 4 plots the marginal effect of defeat on IHS railroad expansion across levels of ugly institutional exposure: at higher exposure, extractive factor endowments combined with high pre-colonial population density, railroad construction rises after defeat. We flagged this possibility as exploratory in advance, rather than as a further test of the mechanism, and we probe it below rather than treat it as confirmation.

Figure 5 traces the same relationship using pre-colonial population density alone. Because pre-colonial population density partly defines the *ugly* classification, the near-identical pattern matches the labor-market explanation we anticipated for an exploratory railroad result rather than institutional transformation: dense local populations supplied cheap labor for large

[6] Goenaga et al. (2023) cover nine of the fourteen countries in our sample; Guatemala, Honduras, and Paraguay (three loser countries with high *bad*-institution exposure) are absent from their data. With only Bolivia and Peru as losing countries in that subsample, the triple-interaction term becomes mechanically unidentifiable. Estimating the bare defeat × postwar interaction on the restricted nine-country sample still yields a large and significant coefficient (-6.95 percentage points of GDP, $p = 0.002$), confirming that the fiscal penalty of defeat is not an artifact of country coverage.

[7] For railroad expansion, the wild bootstrap p-value associated with the interaction involving ugly institutions is approximately 0.13.

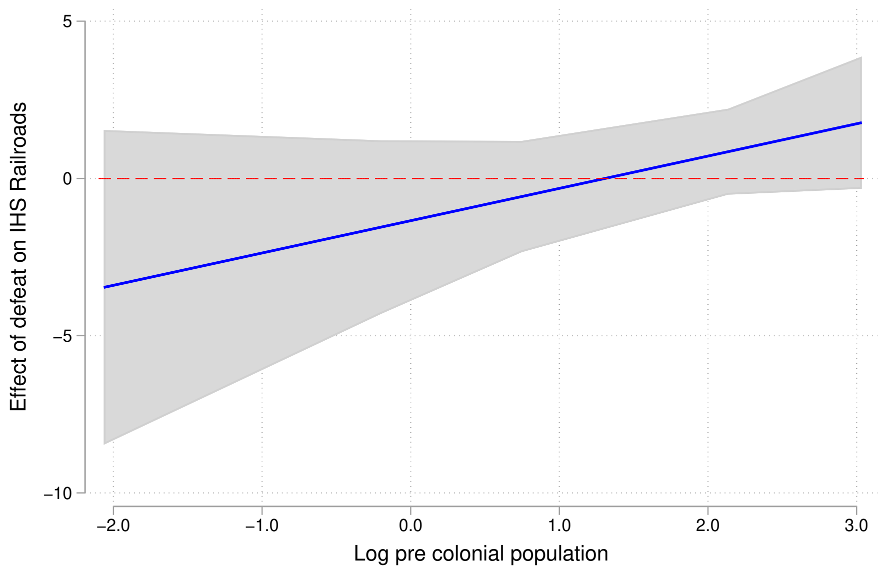
Figure 4: Heterogeneous effects on railroad mileage (IHS): marginal effect of defeat across the share of ugly institutions



Note: Marginal effects of defeat on railroad extension by type of institution

infrastructure projects, and local elites had their own reasons, rents from trade, land values, exports, to promote railroad construction independent of the state's fiscal or administrative condition. Figure A2 in the Appendix reports the same specification on the raw measure, where confidence intervals are wide and indistinguishable from zero, so even this secondary pattern is fragile on its own terms.

Figure 5: Heterogeneous effects in railroad extension (IHS). Effect of defeat relative to the log of pre colonial population density

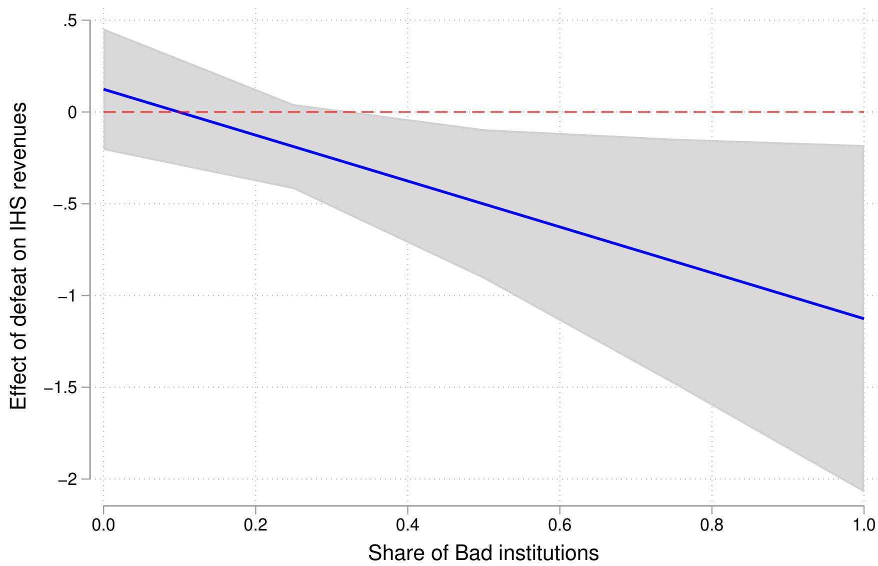


Note: Marginal effect of defeat on railroad mileage across pre-colonial population density

Railroad expansion is therefore not a reliable indicator of state capacity here: the argument's actual test, exposure to *bad* institutions, is null throughout, and the one place railroads move at all runs through labor markets rather than administrative capacity. Because Schenoni (2020) uses railroad extension as an outcome, we also report raw-measure results for comparability; their absence of significant effects is consistent with both patterns above.

Figure 6 turns to fiscal capacity, where the argument's central empirical implication lies. Higher exposure to *bad* institutions (factor endowments that foster extractive arrangements independently of pre-colonial population density) amplifies the revenue decline following defeat. The relationship is stable: government revenues show a consistent, monotone response to institutional exposure across specifications. Fiscal capacity captures what railroad expansion does not (the state's ability to extract resources and sustain centralized authority) and is the more reliable measure of state-building in this historical context.

Figure 6: Heterogeneous effects on government revenue (IHS): marginal effect of defeat across the share of bad institutions

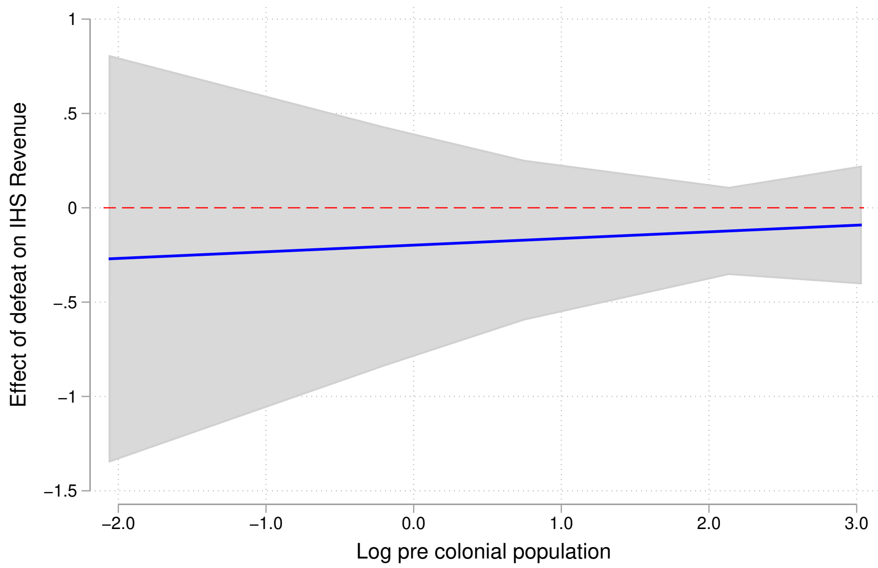


Note: Marginal effects of defeat on government revenue by type of institution

Figure 7 examines whether the estimated fiscal consequences of defeat vary with pre-colonial population density. The marginal effect remains relatively flat and statistically indistinguishable from zero across the distribution of population density. The analysis therefore provides no evidence that population density alone conditions the revenue result. This pattern is consistent with the institutional interpretation and weighs against demographic differences alone accounting for the relationship.

Infrastructure expansion and fiscal capacity behave as distinct channels of postwar state building, and the argument explains why. Railroad construction answers to labor availability and localized investment incentives in extractive environments, dynamics that need not indicate centralized state strengthening at all. Government revenues, by contrast, directly track how inherited local administrative capacity conditions the state's ability to extract resources and consolidate authority after war. Institutional heterogeneity shapes the state-building consequences of war through fiscal capacity, not through locally driven infrastructure expansion.

Figure 7: Heterogeneous effects in government revenue (IHS). Effect of defeat relative to the log of pre colonial population density



Note: Marginal effect of defeat on government revenue across pre-colonial population density

A final robustness check asks whether the revenue result reflects inherited colonial administrative structures or contemporaneous sovereign financial conditions instead. We replace the *bad*-institution exposure measure with country-level mean sovereign bond yields from Queralt (2022), which capture postwar financial credibility and credit access rather than the colonial administrative legacy. If sovereign risk and capital-market access account for the heterogeneous fiscal consequences associated with colonial exposure, bond yields should produce a similar interaction. They do not: the triple interaction of defeat, the postwar indicator, and bond yields on IHS government revenue is statistically indistinguishable from zero and points in the opposite direction from the main result (Appendix Figure A1). This null weighs against postwar sovereign financial conditions alone accounting for the revenue relationship, although it cannot exclude other sources of confounding. It supports our interpretation that inherited local administrative capacity shaped states' fiscal responses to defeat. The Appendix extends the analysis to internal revolt episodes using event-study methods: the same good-ugly-bad hierarchy appears in post-revolt fiscal trajectories.

5 Illustrative Cases

The panel results provide evidence of the moderating relationship: the estimated fiscal consequences of defeat vary with exposure to inherited colonial institutions. The panel cannot show how this relationship operated within particular countries or how the institutional arrangements that emerged after war carried its fiscal consequences forward. The cases address these questions through qualitative historical evidence. Peru and Paraguay both suffered major defeats but differed sharply in their colonial institutional inheritances. Comparing them shows how these inherited differences shaped the fiscal consequences of coalition disruption and how the postwar arrangements adopted by new governing coalitions sustained fiscal collapse or supported recovery. Colombia examines the same process after victory, showing how inherited local capacity shaped the institutional response available to centralizing elites. The cases therefore examine both moderation and mediation, but at different levels of evidence: they trace how inherited institutions conditioned the initial consequences of war and how subsequent institutional arrangements carried those consequences forward. They do not provide a formal test of mediation, which would require comparable measures of institutional change across countries and over time. Nor are they controlled comparisons, since the three countries differ in many ways beyond their colonial institutions and war outcomes. Their purpose is to assess whether the historical record contains the actors, institutional sequences, and contrasting fiscal trajectories implied by the argument.

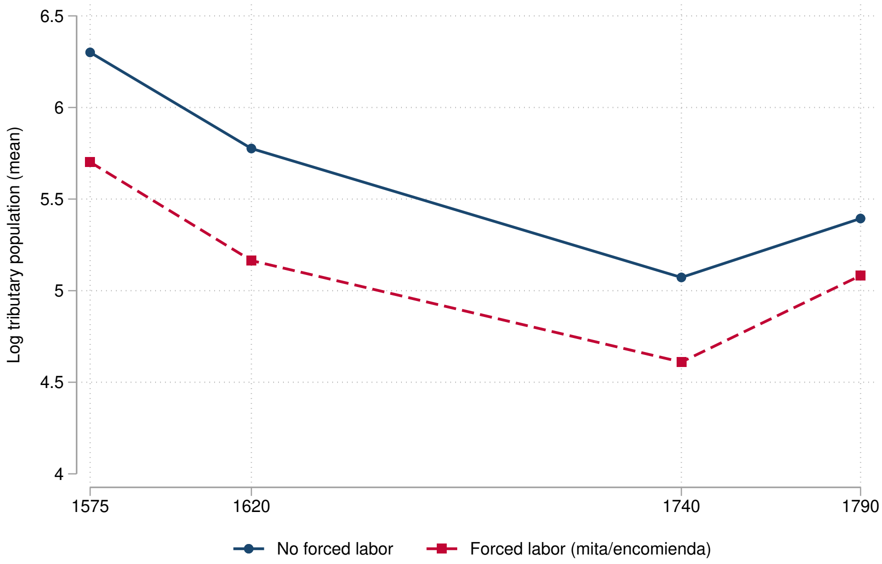
5.1 Peru: Colonial Foundations and Fiscal Collapse

Peru lost the War of the Pacific (1879–1883); our dataset classifies it as a loser (Schenoni, 2020, Table 1). Schenoni (2020) documents the political consequences of defeat for Peru in detail: the Partido Civil's collapse, Piérola's coup, and the fiscal divergence the synthetic control estimates confirm. Our contribution is different. We trace the colonial institutional origins of the vulnerability that defeat exposed. The statistical results show that defeat amplifies fiscal decline where exposure to *bad* institutions is high. Peru illustrates why: defeat exposed a fiscal system with little durable, locally rooted administrative capacity capable of operating independently of the central coalition, a weakness produced by three centuries of centralized colonial extraction.

Spanish colonizers built little autonomous local governance in highland Peru. They grafted extraction onto existing Inca administrative hierarchies instead. The corregidores, royal bureaucrats appointed by the Crown, monopolized fiscal collection, justice, and local administration, displacing the encomenderos who had sought to build locally rooted power (Contreras, 2002; Lohmann Villena, 2001). Cabildos existed in indigenous towns but barely functioned, competing with pre-Hispanic curaca institutions that the Spanish coopted rather than replaced (cited in Bermúdez, 1989; Contreras, 2002). The mita assigned labor directly to mine operators through a state bureaucratic apparatus, bypassing local governance entirely (Contreras, 2002). When the mita collapsed and the Bourbons reformed the corregidor system, little durable administrative capacity could operate independently of the Crown. Colonial authorities had concentrated governance in centrally appointed offices rather than building autonomous local institutions. The demographic base of extraction was shrinking in parallel. Tributary populations in forced labor districts fell sharply from 1575 onwards and never recovered to earlier levels, despite a partial Bourbon recovery by 1790 (Figure 8) (Abad & Maurer, 2025). By independence, Peru's interior retained limited locally rooted administrative capacity capable of sustaining domestic extraction independently of the central government. Indigenous tribute sustained some revenue in densely populated departments until its abolition in 1854. Guano revenues consolidated in the 1850s and masked the structural weakness underneath (Contreras, 2012). The fiscal crisis that preceded the war began in the 1870s precisely because guano was running out and nothing had been built to replace it (Contreras, 2012).

The Partido Civil attempted to build a centralizing project on these fragile foundations. President Manuel Pardo organized municipalities, created a national census, established meritocratic promotion in the military, and pushed new taxes on income, property, and sugar exports (Basadre, 1946; Mc Evoy, 1994). Peripheral elites resisted at every step (Mc Evoy, 1994, 2017). Nicolás de Piérola's revolts in 1872, 1874, and 1877 showed how deeply entrenched that resistance was. The civilistas held on, but the coalition was the only thing sustaining fiscal

Figure 8: Decline in tributary population across Peruvian districts, 1575–1790



Note: Mean log tributary population across 500 Peruvian *repartimientos*, by institutional type. Source: Abad and Maurer (2025).

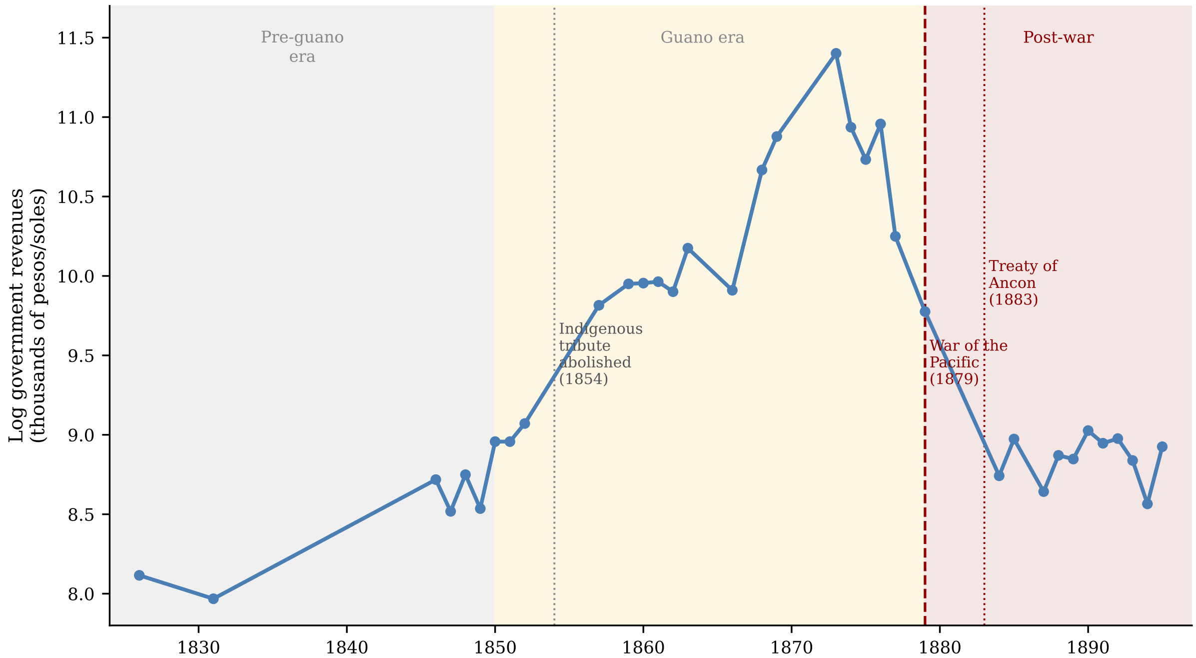
authority. Weak local institutions left these reforms dependent on the central coalition and unable to sustain tax collection independently of its political fortunes.

Defeat removed the coalition. Early military reverses let Piérola orchestrate a coup in December 1879, dismantling a decade of institutional progress (Mc Evoy, 2017). He forged alliances with the church, traditional militias, and local strongmen. Chilean forces occupied Lima in 1881. After the Treaty of Ancón in 1883, the public blamed the civilistas for leading the country to ruin, and the party collapsed (Mc Evoy, 2017). The pax cacerista struck a fiscal decentralization deal with eighteen departments: each would collect its own taxes (Contreras, 2001). The arrangement exposed the underlying weakness: departments received responsibility for collecting taxes but lacked durable local institutions capable of exercising that authority effectively.

The postwar fiscal record documents the collapse in precise detail (Figure 9). Before the war, contributions from all interior departments combined amounted to less than one percent of the national budget: the state had survived on guano and saltpeter, not domestic extraction (Contreras, 2001). After the war, the government tried to create specialized fiscal agents, apoderados fiscales, to replace the subprefects who had previously handled collection. The experiment failed completely. Entire provinces found no one willing to take the job (Contreras, 2001). Armed resistance to tax collectors spread across Ayacucho, Huancavelica, and

Cajamarca (Contreras, 2001; Manrique, 1988). By 1893 the Juntas Departamentales collected only 57 percent of budgeted revenues, and after 1894 the data disappear entirely (Contreras, 2001). The SCM estimates confirm the divergence: no other country with Peru's prewar fiscal trajectory experienced so sudden and permanent a collapse after 1883 (Schenoni, 2020). Nothing about losing a war made this collapse inevitable; Paraguay lost a far more devastating war and recovered, precisely because it inherited different local administrative capacity, as the next case shows.

Figure 9: Peruvian government revenues, 1826–1895



Note: Log of total government revenues actually collected. Currency changes from pesos to soles in 1864 (1 peso = 0.8 soles). Source: Contreras (2012), Cuadro 2.4.

Peru therefore illustrates the central process described by the argument: fiscal capacity collapsed after defeat not because war destroyed a strong fiscal apparatus, but because defeat removed the coalition sustaining extraction in an institutional environment with too little autonomous local capacity to provide a fiscal floor. The sharpest contrast is with Paraguay, discussed next: both countries suffered catastrophic defeat, but Paraguay's colonial inheritance left different local administrative capacity in place, and its fiscal trajectory diverged from Peru's as a result. Colombia offers a different contrast again, a victory rather than a defeat, where core elites built on local administrative capacity that Peru never had.

5.2 Paraguay: Colonial Institutional Inheritance and Fiscal Recovery After Defeat

Paraguay lost the War of the Triple Alliance (1864–1870); our dataset classifies it as a loser (Schenoni, 2020, Table 1). One data limitation matters here: our revenue series begins in 1865, a year after the war started, so the figures show no prewar fiscal baseline. They capture the postwar trajectory instead, which is what our argument needs. Paraguay is the key within-loser contrast to Peru: both countries suffered catastrophic military defeat, but their colonial institutional inheritances differ sharply, good in Paraguay, bad to ugly in Peru (see descriptive statistics in Table A6). We expect these institutional differences to produce different fiscal trajectories even when the war outcome is the same.

The War of the Triple Alliance, fought between Paraguay and the allied forces of Argentina, Brazil, and Uruguay, was the bloodiest interstate conflict in Latin American history (Bethell, 1996). Paraguay lost roughly 70 percent of its population. A postwar census counted 221,079 survivors: 106,254 women, 86,079 children, and 28,746 men (Alix-Garcia, Schechter, Valencia Caicedo, & Zhu, 2022). Mandatory recruitment of all capable males began in 1862; by 1867 conscription was universal; by early 1869 the Paraguayan army consisted largely of children, elderly men, women, and amputees (Alix-Garcia et al., 2022). Francisco Solano López died in battle in 1870, ending the conflict.

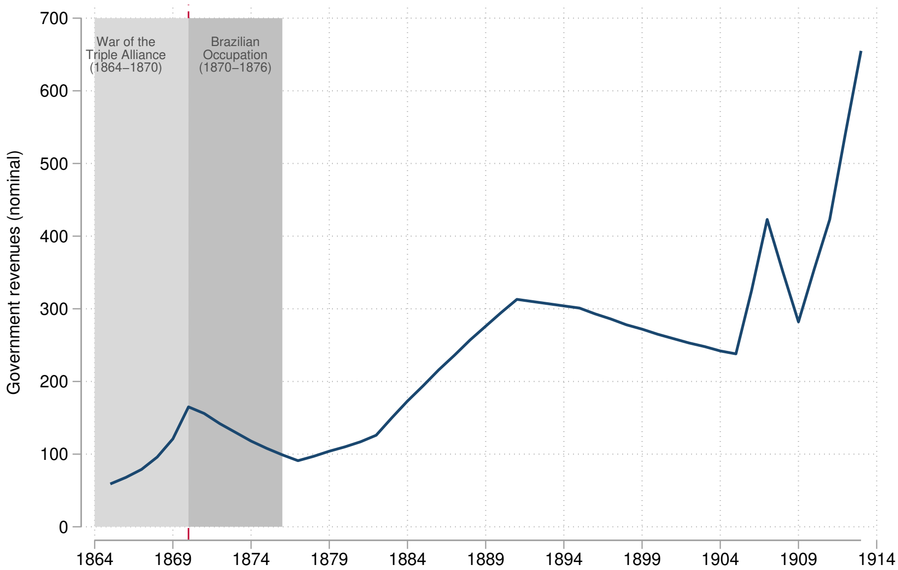
In the Bruhn and Gallego (2012) classification, Paraguay falls predominantly in the *good*-institution category, with a 50% share of good institutions, while Peru falls predominantly in the bad-to-ugly range, with shares of 46% and 42%, respectively. The difference reflects how colonial authorities organized local populations in the absence of an established hierarchy. In highland Peru, Spanish authorities grafted tribute collection onto Inca administrative structures rather than constructing durable local institutions. Paraguay had no comparable hierarchy. Guaraní society featured decentralized political and economic organization, including female-headed households and a central role for women in agriculture (Potthast-Jutkeit, 1991). Jesuit missions and other colonial institutions therefore organized settlements, production, and education locally. These arrangements did not amount to a postwar fiscal apparatus, but they left an organized and economically productive population on which later governing coalitions could build. This inheritance preceded and remained distinct from the personalist state created by the López dynasty. This inheritance is not local administrative capacity in the sense Peru and Colombia illustrate: no governance offices, tributary registries, or enforcement apparatus survived the war for a peripheral coalition to inherit. What survived instead was a durable economic and social foundation, an organized, literate, and productive population, that gave postwar coalitions the resources to construct fiscal extraction anew rather than restore an existing apparatus.

Paraguay's version of this mechanism is a variant on Peru's, not a repeat of it. In Peru, defeat delegitimized a core coalition that held on for years before peripheral elites, led by Piérola, finally displaced it in 1879. In Paraguay, the core coalition did not outlast the war itself: Francisco Solano López died in the war's final battle, and no comparable central coalition remained afterward for peripheral elites to displace. Reconstruction fell to peripheral, regional actors from the outset. Abente (1989) documents that surviving cattle ranchers and military leaders formed a loose governing alliance in the years after 1870, operating under the oversight of the Brazilian occupation force and, later, the Brazilian legation; by the turn of the century, this rural elite had in turn given way to an immigrant mercantile elite that came to dominate Paraguay's export trade. The mechanism is the one Peru illustrates, core coalitions losing ground to peripheral actors after defeat, but Paraguay's core elite administers the transition differently: rather than a coalition weakening its grip over years, defeat ends the core coalition immediately, and peripheral reconstruction begins right away.

What this peripheral coalition inherited mattered more than the identities of its members. Even the fiscal apparatus on which the López state had relied, including state ranches assembled from confiscated Jesuit mission lands, did not survive as state property: postwar governments divided and sold most of it (Williams, 1973). The population organized around the former missions proved more durable. Comparing communities near Jesuit mission sites with those near abandoned missions and neighboring Franciscan missions, Valencia-Caicedo (2019) finds that Guaraní populations within former mission areas retained higher literacy and income for two and a half centuries. Pastore (1994) and Abente (1989) also show that smallholder tobacco and yerba mate production dated to the colonial period and continued to anchor Paraguay's export economy after 1870. Postwar ranchers and merchants therefore inherited a locally organized and productive population rather than starting from nothing. This organized and productive population did not preserve the prewar fiscal apparatus, but it gave new governing coalitions resources with which to reconstruct fiscal extraction.

Brazilian forces occupied Paraguay from 1870 (Alix-Garcia et al., 2022). Government revenues fell sharply after defeat, from 165 in 1870 to 91 by 1877, a 45 percent decline, but then rebounded.[8] By 1888 revenues had returned to wartime levels, and by 1906–1913 they reached 655 (Figure 10). The recovery was slow, taking roughly 36 years from trough to sustained growth, but it happened. The comparison to Peru makes it more telling rather than less: Paraguay lost thirteen years before Peru did, suffered a demographic catastrophe with no analogue in Peru's war, and still ended the period with a growing fiscal base, while Peru's revenues had become unmeasurable by the 1890s. Neither the timing nor the severity of defeat alone explains that divergence; the locally organized and productive population inherited from the colonial period gave postwar coalitions the basis to reconstruct fiscal extraction, not a surviving fiscal apparatus.

Figure 10: Paraguayan government revenues, 1865–1913



Note: National government revenue per capita in cents of current US dollars. The series begins in 1865 and therefore captures wartime rather than prewar Paraguay; Alix-Garcia et al. (2022) document prewar fiscal capacity of the López state. Dashed vertical line marks 1870 (death of Francisco Solano López and end of the War of the Triple Alliance). Source: Banks and Wilson (2025).

The contrast with Peru captures the institutional heterogeneity represented by the triple interaction. Paraguay's colonial inheritance left a locally organized and productive pop-

[8]All revenue figures are from Banks and Wilson (2025) and measured in cents of current US dollars per capita, the unit reported in the Cross-National Time-Series Data Archive for variable *revexp4*.

ulation that survived the destruction of the wartime state and supported the gradual reconstruction of fiscal extraction under new governing coalitions. Peru's colonial system left a more centralized and fragile fiscal structure that depended heavily on the ruling coalition and external revenues. When defeat disrupted that coalition, its successors struggled to collect taxes locally. The cases therefore do not show that the same administrative institutions survived in Paraguay while disappearing in Peru. They show that different colonial inheritances gave postwar coalitions different resources with which to reconstruct fiscal authority. Colombia, discussed next, provides clearer evidence of inherited municipal institutions continuing to operate after war.

5.3 Colombia: Local Administrative Capacity and Fiscal Resilience

The Colombo-Ecuadoran War of 1863 ended in a Colombian victory; Colombia enters our dataset on the winning side (Schenoni, 2020, Table 1). The argument's clearest expectation concerns defeat because losing immediately weakens the central coalition and gives peripheral elites an opportunity to dismantle fiscal authority. Victory creates no equivalent immediate change: it strengthens core elites politically, but converting that advantage into fiscal consolidation requires building on existing administrative arrangements and overcoming peripheral resistance over years, a slower process the panel's postwar window may not capture. Colombia therefore does not test a symmetric expectation about victory. Instead, it shows how inherited local administrative capacity continued operating after war and provided centralizing elites with an institutional foundation for fiscal consolidation.

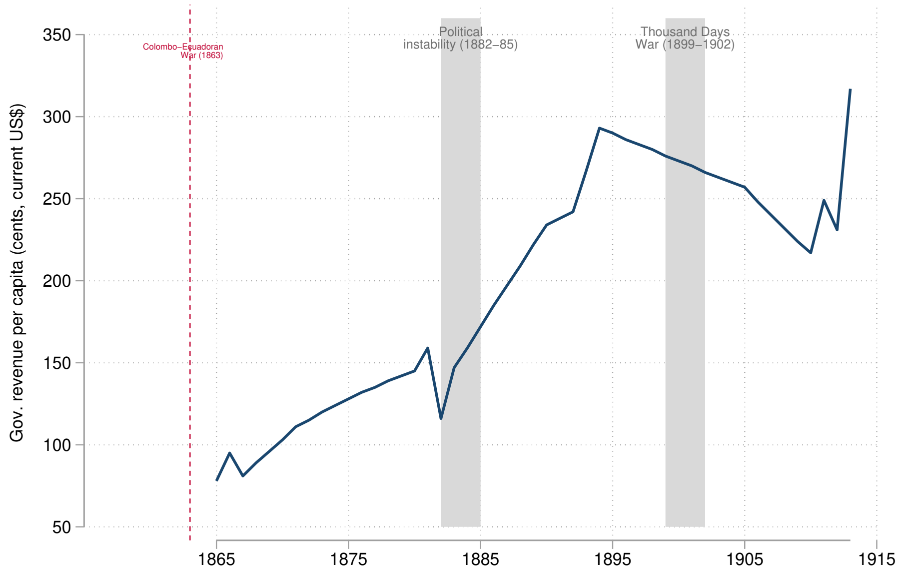
In the Bruhn and Gallego (2012) classification, much of Colombia falls under *ugly* institutions: extractive factor endowments combined with pre-colonial indigenous population dense enough to make local labor exploitation viable. Extraction of this type required building local governance. Unlike highland Peru, where Spanish tribute grafted onto pre-existing Inca administrative hierarchies without producing anything new, Colombia's encomienda operated in an environment where settlers had to organize tributary communities themselves. They built cabildos, assigned tribute obligations, established enforcement offices, and created fiscal registries. Settlers built these arrangements for extraction, not inclusion, but they generated local administrative capacity that outlasted the institution that created it.

Faguet et al. (2025) provide subnational evidence for this mechanism, using a neighbor-pair design that compares adjacent Colombian municipalities with and without encomienda. Encomienda anchored Spanish settlement and local governance where settlers needed institutions to control labor, collect tribute, and coordinate local public goods: cabildos administered justice, property rights, and public works, and later corregimientos added tax collection on trade, gold, silver, and tolls. This colonial apparatus persisted for centuries. Faguet et al. (2025) show that encomienda municipalities had significantly higher land values by 1870, seven years after the Colombo-Ecuadoran War, and significantly higher per capita municipal revenues by 1916, just past the end of our sample period; in their decomposition of encomienda's total effect into separate channels, local state capacity recorded in 1794 alone accounts for 48% of encomienda's effect on modern unmet basic needs and 35% of its effect on modern infant mortality. The local administrative apparatus colonialism built did not need to be rebuilt after 1863; it simply continued operating, and constrained how far fiscal dismantlement could proceed whenever a major shock arrived.

Government revenues grew steadily from 1865 to 1881, roughly doubling over sixteen years (Figure 11). This trajectory shows that core elites entered the postwar period with a functioning local administrative apparatus on which they could build. Colombia contrasts with Peru, which entered the postwar period with a fragile fiscal base and little autonomous local administrative capacity to cushion the coalition's collapse.

The trajectory after 1881 is equally instructive. The first disruption came in 1882, when a contested presidential succession and rising tensions between Liberal factions drove revenues sharply downward. The conflict escalated into civil war in 1884–85, as Liberal radicals challenged the coalition Rafael Núñez assembled; Núñez's coalition defeated them by August 1885. Revenues recovered within a few years, never collapsing to pre-consolidation levels. The same resilience appears on a larger scale during the Thousand Days War of 1899–1902, the most severe internal conflict of the period, which disrupted the central coalition and pressed heavily on revenues. Government revenues declined steadily between 1895 and 1910 (Figure 11) but did not collapse the way the argument expected for bad-institution environments, where fiscal authority rests entirely on coalition compliance and its removal leaves nothing behind. The encomienda's local administrative capacity provided a floor: local institutions continued to sustain some extraction even when the central coalition was severely weakened.

Figure 11: Colombian government revenues per capita, 1865–1913



Note: Government revenue per capita in cents of current US dollars. Shaded bands mark periods of political instability and civil conflict (1882–85; 1899–1902). Dashed vertical line marks the Colombo-Ecuadoran War (1863). Source: Banks and Wilson (2025) via Schenoni (2020).

Colombia's revenue trajectory makes visible the moderating role local administrative capacity plays on the winning side of a war outcome. After 1863, victory reinforced centralizing elites, and encomienda-built capacity, still functioning after nearly three centuries, gave them more fiscal room than they would otherwise have had. When internal conflicts later eroded coalition authority, that same capacity provided a floor and sustained extraction even as central political control weakened. Both episodes are the same moderating process the panel estimates, observed twice: once after an external war, once after internal ones.

Summary The three cases illuminate different parts of the argument. Peru lost with little autonomous local administrative capacity in place, and its fiscal system collapsed along with the coalition that had sustained it. Paraguay lost just as catastrophically, but its organized and productive population gave new governing coalitions resources with which to reconstruct fiscal extraction. Colombia won, and its inherited local administrative capacity continued functioning through the postwar period. Peru and Colombia provide the clearest qualitative evidence of moderation; Paraguay shows how postwar institutional reconstruction carried those consequences forward instead. This is qualitative evidence on moderation and mediation, not a formal mediation test.

6 Discussion and Conclusion

This paper examines how inherited colonial institutions conditioned the fiscal consequences of war outcomes. Defeat reduced government revenue most sharply where local administrative capacity was weakest, and the case evidence above traces why: colonial rule left some governing coalitions with local institutions capable of sustaining extraction on their own, and others without them.

The results also clarify which features of colonial institutions mattered. Bad and ugly institutions both supported extraction, but they required different forms of local organization. In areas with ugly institutions, dense local populations led colonizers to establish cabildos, fiscal registries, enforcement offices, and other institutions that organized labor and collected revenue. In areas with bad institutions, sparse local populations did not require the same administrative apparatus (Bruhn & Gallego, 2012; Faguet et al., 2025). Defeat therefore produced larger revenue losses in countries with greater bad-institution exposure, not in countries with extractive institutions in general.

The comparison between government revenue and railroad construction further clarifies the scope of the argument. Government revenue directly records the state's ability to extract resources from domestic actors (Centeno, 2002; Thies, 2005). Railroad construction instead relied heavily on foreign concessionaires, external capital, export interests, and available labor (Bignon et al., 2015; Halperin Donghi, 1998; Queralt, 2022). Bad-institution exposure does not produce the same post-defeat relationship for railroads that it produces for revenue. The relationship between railroad construction and ugly-institution exposure appears only with the inverse hyperbolic sine transformation, receives weaker statistical support, and disappears when we use the raw measure. We therefore treat that result as exploratory evidence about labor and investment incentives rather than as evidence of administrative consolidation. This does not mean that fiscal resources played no role in railroad construction. Bignon et al. (2015) show that government revenue, driven largely by trade, supported railway expansion across the region. Yet trade-based revenue remained volatile throughout the period, while foreign concessionaires and export interests continued to shape the location and timing of construction. These constraints did not disappear as more time passed after war. We therefore do not interpret the railroad null as evidence that fiscal capacity simply required a longer period to produce infrastructure.

Our research design has three important limitations. First, we do not estimate the independent causal effect of war outcomes. Countries entered the conflicts with different institutions, fiscal systems, and military capacities, and these differences may also have affected whether they won or lost. Colonial institutional exposure predates the wars by as much as three centuries, so nineteenth-century war outcomes cannot explain the institutional measure. We also replace colonial exposure with sovereign bond yields in a placebo analysis. Bond yields do not produce the same interaction, which weighs against postwar sovereign financial conditions alone accounting for the relationship among institutional exposure, defeat, and fiscal decline (Queralt, 2022). The placebo cannot eliminate every source of confounding. Second, the panel cannot formally test mediation because we lack comparable measures of institutional change after each war. The cases trace postwar institutional processes, but they cannot substitute for a formal mediation analysis. Third, the paper does not examine whether war changed countries' productive profiles. We use colonial economic organization to measure institutions inherited before conflict, not economic changes caused by war.

The argument applies most directly to settings where earlier institutions created durable local differences in administrative capacity, conflict altered the balance between central and peripheral coalitions, and governments depended on domestic extraction. A preliminary appendix analysis examines whether inherited institutions also conditioned fiscal responses to internal revolts (Madrid & Schenoni, 2024). The results follow the same institutional ordering found in the interstate-war analysis: countries with good institutions experienced the strongest post-revolt revenue growth, countries with ugly institutions experienced more moderate growth, and countries with bad institutions failed to sustain their initial fiscal gains. Internal revolts created fiscal pressure rather than the coalition realignment associated with interstate defeat (Centeno, 1997, 2002; Schenoni, 2020). The exercise therefore examines a different political process, but it suggests that inherited local administrative capacity may influence state responses to several types of political disruption. Future research could evaluate this possibility using direct measures of local tax collection, administrative enforcement, and institutional change after conflict.

The paper contributes to three debates already noted above. First, it extends Schenoni's elite-realignment mechanism by showing that inherited local administrative capacity shaped the fiscal consequences of coalition disruption. The revenue results support this political mechanism, while the railroad results question whether infrastructure expansion captures the same dimension of state capacity. More broadly, the findings qualify bellicist accounts that treat similar war outcomes as producing comparable institutional consequences across states. Second, the paper extends research on institutional persistence: these institutions condition how states respond to later political shocks, not only their long-run average levels. Third, the analysis brings subnational institutional variation into a national study of war and fiscal capacity. Country fixed effects account for time-invariant differences in average levels, but they cannot recover how a shock's effect varies across institutional environments unless the model includes the relevant interaction, because that variation is exactly what a fixed effect absorbs. This limitation is not specific to our two outcome variables. Whenever an institutional variable conditions how a country responds to a shock rather than only shifting its baseline level, standard fixed-effects practice in comparative-historical political economy will absorb that conditioning relationship instead of revealing it. By estimating the interaction directly, we recover the heterogeneity that produced divergent fiscal trajectories across Latin America, and the same design choice should apply wherever comparative-historical research treats a time-invariant institutional variable as a control rather than as a potential moderator.

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Appendix

Table A1: Heterogeneous effects ugly institutions

	(1) Railroad exp	(2) IHS Railroad	(3) IHS Gov Revenue
Defeat × Post × Ugly	-518.809	6.621	0.368
	(1509.996)	(2.156)***	(0.422)
Observations	686	686	686
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Clusters	14	14	14
Mean	1640.44	6.11	6.77
W.bootstrap p-value	0.82	0.13	0.51

Cluster standard errors at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Heterogeneous effects good institutions

	(1) Railroad exp	(2) IHS Railroad	(3) IHS Gov Revenue
Defeat × Post × Good	1062.808	-5.149	-0.043
	(2535.225)	(3.150)	(0.730)
Observations	686	686	686
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Clusters	14	14	14
Mean	1640.44	6.11	6.77
W.bootstrap p-value	0.75	0.24	0.96

Cluster standard errors at the country level in parentheses

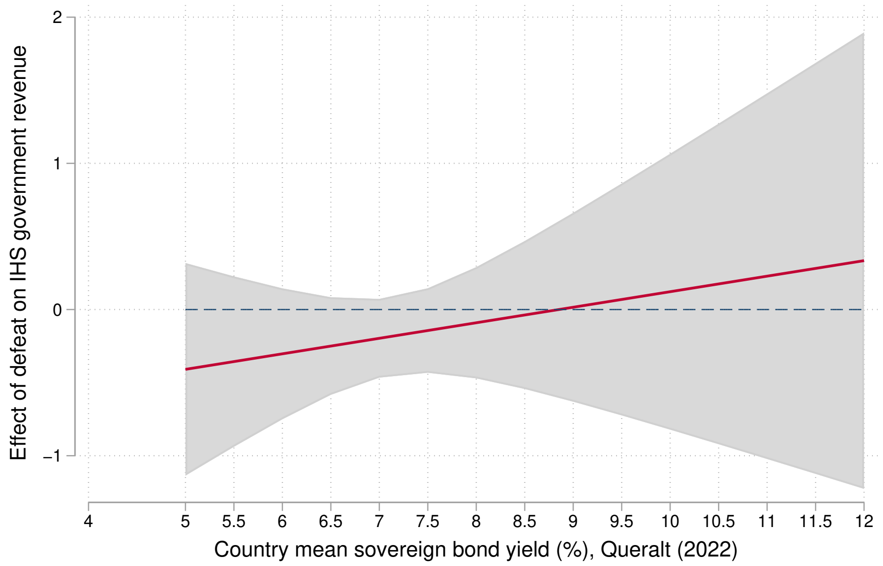
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Heterogeneous effects bad institutions

	(1) Railroad exp	(2) IHS Railroad	(3) IHS Gov Revenue
Defeat × Post × Bad	-3804.582	-2.249	-1.250
	(4120.693)	(4.190)	(0.592)*
Observations	686	686	686
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Clusters	14	14	14
Mean	1640.44	6.11	6.77
W.bootstrap p-value	0.41	0.70	0.06

Cluster standard errors at the country level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A1: Placebo: effect of defeat on government revenue across sovereign bond yield levels



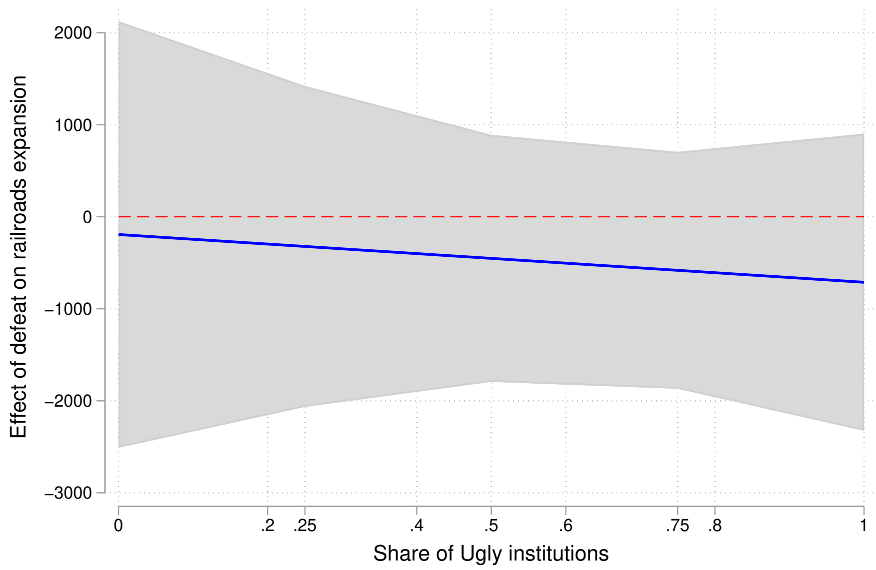
Note: Marginal effect of defeat in the postwar period on IHS government revenue across country-level mean sovereign bond yields. Bond yield captures financial credibility rather than colonial institutional heritage. 12-country sample (Colombia and Venezuela excluded because neither has bond issuances in the dataset). Country and year fixed effects. SE clustered at country level. Source: Queralt (2022).

Table A4: Classification of Colonial Activities into Good, Bad, and Ugly

	Precolonial population density	
Colonial activities	**Low**	**High**
Without economies of scale	**Good**: No exploitation of labor	**Ugly**: Exploitation of local labor
With economies of scale	**Bad**: Exploitation of imported labor	**Bad**: Exploitation of local and imported labor

Note: Table adapted from Bruhn and Gallego (2012)

Figure A2: Heterogeneous effects in railroad extension (raw measure). Effect of defeat relative to the share of ugly institutions



Note: Marginal effect of defeat in the postwar period on railroad extension (raw measure, miles of line) as a function of the share of ugly institutional exposure. The line plots the conditional marginal effect across the observed range of ugly institutional shares; the shaded band represents 95% confidence intervals from cluster-robust standard errors. Country and year fixed effects. SE clustered at country level. Sample: 14 countries, 1865–1913. Institutional data: Bruhn and Gallego (2012). Source: Schenoni (2020).

Table A5: Sample Statistics

	count	mean	sd	min	max
IHS trans. railroad extension	686	6.107	2.524	0.000	10.558
IHS trans. government revenue pc	686	6.768	0.858	4.771	8.805
Defeat dummy	686	0.429	0.495	0.000	1.000
Share of ugly institutions	686	0.435	0.303	0.000	1.000
Share of good institutions	686	0.247	0.262	0.000	0.737
Share of bad institutions	686	0.210	0.158	0.000	0.481
Government exp. pc	686	668.878	648.151	52.000	3318.000
Exports pc	686	1150.605	1118.154	190.000	6146.000
Military personnel	686	14.190	25.321	1.000	354.000
Eff. legislature	686	1.583	0.749	0.000	3.000
School enrollment	686	9.997	18.117	0.000	150.000
Urbanization	686	201.972	384.904	0.000	2381.000
Land area	686	1403799.208	2089410.551	21393.000	8512000.000

Table A6: Summary Statistics by Country (wbcode), Part 1

	IHS railroad ext.	IHS gov rev pc	Defeat dummy	Share ugly inst.	Share good inst.	Share bad inst.	Gov exp pc
ARG	8.878564	7.695	0	0	.7083333	.0833333	1189.796
BOL	5.095364	5.906068	1	.2222222	.5555556	.2222222	249.3673
BRA	8.839394	6.904317	0	.1481481	.2222222	.4814815	577.7347
CHL	7.947178	7.985246	0	.5384616	0	.2307692	1734.204
COL	5.949901	5.900809	0	.5333334	.1666667	.2	216.7347
ECU	5.36526	6.258308	1	.4090909	.1818182	.3636364	331.2857
GTM	4.175113	6.650563	1	1	0	0	518.9592
HND	3.530579	6.490454	1	.6111111	0	.3888889	331.102
MEX	8.640624	6.198462	0	.625	.03125	.28125	257.6327
PER	7.200138	7.250605	1	.4583333	.0416667	.4166667	997.9592
PRY	5.107092	5.990843	1	.2777778	.5	.0555556	249.5102
SLV	4.046446	6.787612	0	1	0	0	457.7551
URY	5.950372	8.181219	0	0	.7368421	0	1880.878
VEN	4.775313	6.551748	0	.2631579	.3157895	.2105263	371.3673

Table A7: Summary Statistics by Country (wbcode), Part 2

	Exports pc	Military personnel	Eff. legislature	School enrollment	Urbanization	Land area
ARG	2965.163	13.55102	2.836735	7.265306	675.1429	2807857
BOL	568.6735	2.55102	1.632653	6.040816	0	1589306
BRA	1047.98	58.04082	1.857143	9.551021	1074.286	8348265
CHL	2334.551	21.5102	3	42.7551	328.4694	620265.3
COL	376.1224	3.918367	1.306122	10.08163	79.63265	1083939
ECU	560.9388	2.795918	1.265306	1	0	284000
GTM	741.8163	27.67347	1.55102	19.02041	0	109000
HND	526.7551	1.102041	1	15.28571	0	112000
MEX	381.7143	27.65306	1.183674	8.510204	393.0408	1972000
PER	859.7755	9.387755	1.571429	.8163266	97.81633	1335429
PRY	585.4898	7.408163	.8979592	.3061225	0	171020.4
SLV	742.9592	12.22449	1	7.959184	0	21393
URY	3696.082	5.204082	2.061224	9.653061	179.2245	176000
VEN	720.449	5.632653	1	1.714286	0	1022714

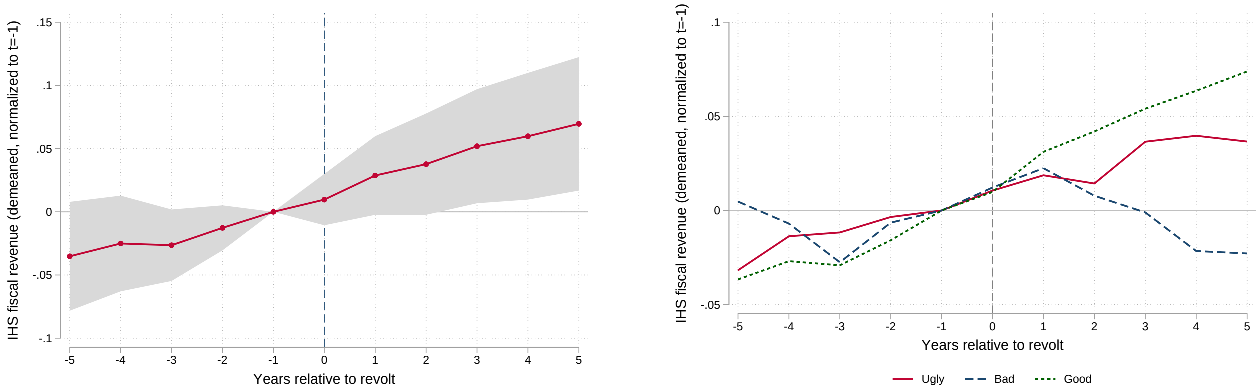
Extension: Colonial Institutions and Fiscal Mobilization Under Internal Pressure

The main analysis tests the institutional capacity argument through a specific political mechanism: interstate defeat triggers coalition dissolution, empowering peripheral elites to dismantle fiscal authority, with the depth of that decline conditioned by the local administrative nodes colonial institutions built or failed to build. Internal conflicts (revolts, caudillo wars, and uprisings) lie outside the formal scope of that mechanism. Centeno (1997) argues that in nineteenth-century Latin America, *both* interstate and internal conflicts were too "limited" (fought by small armies, over short periods, without the mass mobilization or sustained fiscal demands that forged European fiscal-military states) to generate durable increases in state capacity (Centeno, 2002). His skepticism applies to the interstate domain the main analysis addresses and equally to internal revolts. This extension does not contest that characterization. It asks instead whether, even among limited conflicts of this kind, the colonial institutional inheritance explains why some states converted internal pressure into fiscal capacity and others could not. The political mechanism connecting revolts to fiscal outcomes is not the same as the elite-realignment channel of the main analysis: revolts create fiscal pressure that states must mobilize resources to survive, rather than producing the coalition dissolution that interstate defeat triggers. What may be common across both settings is the underlying administrative infrastructure: whether colonial institutions left local nodes capable of sustaining extraction when political shocks arrived.

We use the Latin American Revolts Database (LARD), assembled by Madrid and Schenoni (2024), which records annual revolt episodes across Latin America during the nineteenth century. The LARD data are available for ten of the fourteen countries in our main sample (El Salvador, Guatemala, Honduras, and Mexico are not covered) and are merged with the main dataset at the national level. Because the data are organized by country and year, within-country variation across municipalities or provinces is not exploited, and the sample is smaller than the main analysis. The design follows an event-study logic: we construct windows of several years before and after each revolt episode, normalize government revenues in the year preceding each revolt, and trace how fiscal capacity evolves in the aftermath. We then disaggregate by colonial institution type to ask whether the post-revolt revenue trajectory differs systematically across institutional environments.

Revolts on average coincide with gradual fiscal consolidation in the years that follow (Figure A3, left panel), consistent with the fiscal mobilization logic: internal pressure pushes states to extract more, and on average they succeed. Disaggregating by colonial institution type reveals a clear hierarchy (Figure A3, right panel). Countries with good institutions show the sharpest and most sustained post-revolt fiscal growth. Countries with ugly institutions display a more moderate and plateauing response. Countries with bad institutions experience only a brief initial uptick before revenues decline below the pre-revolt level. The administrative capacity to convert political pressure into fiscal extraction followed the colonial inheritance.

Figure A3: Event study: IHS government revenue around revolt events



Note: Mean IHS government revenue in the years surrounding revolt events, normalized to zero in the year preceding each revolt. Left panel: all countries pooled. Right panel: by colonial institution type (good, ugly, bad); institution shares from Bruhn and Gallego (2012) subnational data aggregated to country level. Sample: 10 countries, 1865–1913. Source: Madrid and Schenoni (2024).

These results are consistent with the institutional capacity argument, though the mechanism differs from the main analysis. Interstate defeat works through elite realignment: coalition dissolution creates the political opening for peripheral elites to dismantle fiscal authority, and colonial administrative nodes constrain how far that dismantlement can proceed. Internal revolts work differently: they create fiscal pressure that states must mobilize resources to survive, and the colonial administrative inheritance conditions whether that mobilization can be sustained. We do not claim the two settings test the same causal channel. What the results suggest is that the same underlying infrastructure, the local administrative nodes that colonial institutions built or failed to build, shapes fiscal outcomes under both types of shock, even if the politics connecting shock to outcome differ.[9] Madrid and Schenoni (2024) document

[9]We ran the same event-study exercise using IHS railroad expansion as the outcome. The results are not informative: the institutional ordering does not hold and point estimates are noisy across all event-time horizons.

that political violence declined substantially across South America over the nineteenth century, a trajectory they associate with consolidating state authority. Colonial institutions shaped which states were positioned to achieve that consolidation: where local administrative capacity existed, states converted the fiscal pressure of revolt into durable extraction; where it did not, neither the fiscal response nor the consolidation followed.

Consistent with the discussion in the main analysis, railroad expansion is a weak proxy for state capacity in this context and we do not report those results.