# When Do Imperial Legacies Fade? Human Capital and State-Minority Alignment on the Habsburg-Ottoman Frontier

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**Abstract**

When do imperial legacies fade? We trace the evolution of human capital along the former Habsburg–Ottoman frontier in Yugoslavia using a two-dimensional boundary regression discontinuity design and seven censuses from 1931 to 1991. We document a large initial discontinuity: literacy was more than 40 percentage points lower on the former Ottoman side of the frontier in 1931, with even larger gaps among women. These differences narrowed substantially under a common state and the expansion of mass schooling, but convergence was uneven. Literacy gaps persisted longer in Muslim-populated districts, even after differences in schools and teachers had largely disappeared. A separate regression discontinuity design exploiting the 1929 closure of sharia courts in Bosnia provides complementary evidence that weakening a state-recognised institution linking Muslim communities to the successor state reduced educational take-up, especially among women and school-age cohorts. The findings show that historical disparities can erode substantially, but that the pace of convergence depends on state-minority institutional alignment.

**Keywords:** Imperial legacies, historical persistence, human capital, state-minority alignment, regression discontinuity, Habsburg, Ottoman, Yugoslavia

**Acknowledgements.** This research received no external financial support. The authors have no financial relationships or other potential conflicts of interest to disclose.

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## 1 Introduction

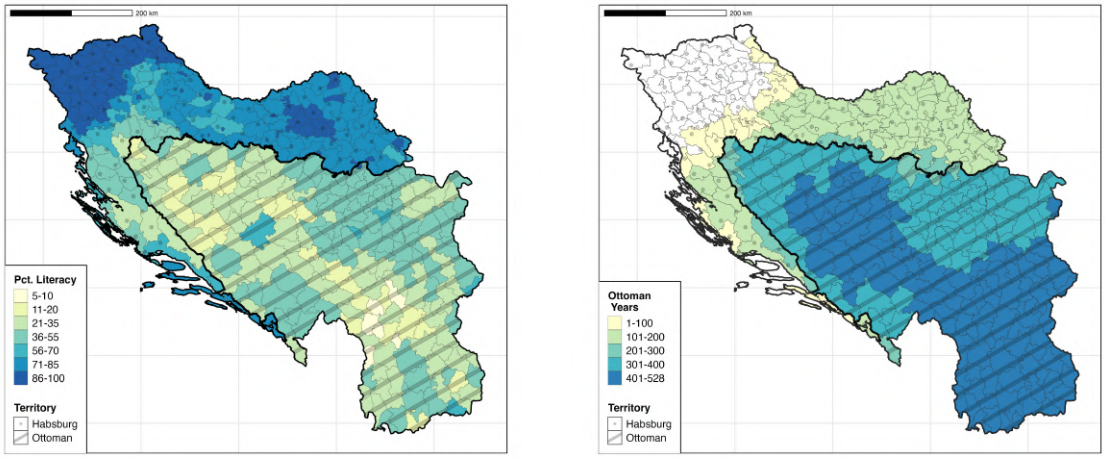
Why do some historical inequalities disappear while others survive long after the institutions that created them have vanished? A large literature shows that historical institutions, political regimes, and cultural shocks can leave effects that persist for generations (Dell, 2010; Nunn & Wantchekon, 2011; Lowes *et al.*, 2017; Becker *et al.*, 2016). Yet persistence is not inevitable. Political borders disappear, states replace one another, and successor governments extend common laws, schools, and public services across populations with very different institutional histories. The important question is therefore not only whether history matters, but when its effects fade. Recent work has begun to study the conditions under which historically transmitted culture adapts to a changing environment (Giuliano & Nunn, 2021). We ask a related question about institutions and human capital: when populations with sharply different historical legacies are incorporated into a common state, what determines whether inherited disparities converge?

We argue that convergence depends not only on what the successor state provides, but also on the relationship between communities and the institutions through which the state provides it. This distinction is particularly important for education. Mass schooling is one of the principal instruments through which modern states build human capital and attempt to integrate heterogeneous populations (Bandiera *et al.*, 2019; Alesina *et al.*, 2021). But providing schools need not generate equal educational take-up. The effects of educational expansion can depend on pre-existing social institutions (Ashraf *et al.*, 2020), assimilation policies can provoke minority backlash (Fouka, 2020), and religious institutions can adapt to the expansion of public education in ways that limit the state's nation-building objectives (Bazzi *et al.*, 2026). We refer to the institutional relationship underlying such responses as *state-minority alignment*: the extent to which a minority community regards the institutions through which the state governs schooling and communal life as legitimate and compatible with its existing institutional order.[1]

The former Habsburg-Ottoman frontier in Yugoslavia provides an unusually informative setting in which to study this process. For centuries, neighbouring communities were governed by two empires that differed substantially in their organisation of primary education. From the late eighteenth century the Habsburg state developed an increasingly standardised system of compulsory schooling, while education in the Ottoman Empire remained more decentralised and closely connected

[1] State-minority alignment does not imply political loyalty to a particular government, nor do we assume that non-Muslim populations were uniformly supportive of the Yugoslav state. The concept refers more narrowly to the compatibility and legitimacy of state institutions within a community.

***Figure 1.*** *Literacy rates and duration of Ottoman rule, Yugoslavia 1931*




*Sources*: Literacy rates are from the population census of Yugoslavia of 1931. Duration of Ottoman rule calculated based on sources listed in section A of the appendix.

to religious communities (Cvrček, 2020). These different systems left sharply different stocks of human capital when imperial rule ended (Figure 1). After 1918, however, both sides of the former frontier became part of the same Yugoslav state. After the Second World War, the socialist government greatly expanded compulsory schooling, adult literacy programmes, and teacher provision while promoting female education and reducing the role of autonomous religious institutions. Yugoslavia therefore allows us to observe populations beginning with very different imperial inheritances and then living for decades under a common state and an increasingly extensive system of mass public education.[2]

Our empirical strategy exploits the Habsburg-Ottoman boundary fixed by the Treaty of Belgrade in 1739 and remaining in place until the dissolution of the empires in 1918. We reconstruct the frontier geographically and combine it with district-level census data covering seven waves between 1931 and 1991. Rather than treating the entire border as a single natural experiment, we use a two-dimensional boundary regression discontinuity design (RD2D) that estimates treatment effects locally at many points along the frontier (Cattaneo *et al.*, 2025a,b). This is important because historical borders are rarely conditionally random everywhere. We combine the econometric design with historical evidence, covariate-balance tests, and density tests to identify the frontier stretches where assignment is most credible. The strongest comparisons occur along segments of the Sava River, where geography provided

[2] The country inherited two alphabets (Cyrillic and Latin), three major religions (Orthodox 47%, Catholic 40%, Muslim 11% according to the 1921 census; Yugoslavia Opšta Državna Statistika 1932), and several languages. Serbo-Croatian was the mother tongue of approximately 74% of the population and served as the primary language of instruction throughout Yugoslavia's existence.

a convenient low-stakes demarcation and neighbouring districts are balanced on pre-treatment geographic characteristics. The design therefore allows the effect of imperial rule to vary spatially rather than averaging credible and strategically chosen stretches of the frontier into a single coefficient.

We first establish that the historical divide in human capital was exceptionally large. In 1931, literacy was 43.5 percentage points lower in districts on the former Ottoman side in the baseline comparison and 37.2 points lower after controlling for geography. A conventional one-dimensional local RD gives a smaller but still substantial discontinuity of 25.7 percentage points, while the preferred RD2D estimates reveal particularly large effects along the most credible balanced frontier segments. The disparity was larger still among women: female literacy was more than 50 percentage points lower on the former Ottoman side in the baseline specifications, with local differences approaching 60 percentage points along some of the most credible segments. The frontier therefore separated neighbouring populations with profoundly different levels of accumulated human capital even thirteen years after the border itself had disappeared.

We then follow this discontinuity over six decades. The central result is substantial convergence. Between 1931 and 1991, the former Ottoman-side literacy penalty shrank dramatically across all three estimators, reaching the single digits in the preferred local comparisons by the end of the socialist period. Because literacy is bounded above, some decline in percentage-point differences occurs mechanically as both sides approach universal literacy. Re-estimating the convergence path using log-odds and arcsine transformations nevertheless confirms that a large share of the original gap genuinely closed. Imperial legacies were therefore neither immutable nor automatically self-reproducing: a human-capital discontinuity that was enormous in 1931 had mostly eroded sixty years later.

This aggregate convergence, however, conceals systematic heterogeneity. Catch-up was markedly less complete in districts with larger Muslim populations. Restricting attention to the former Ottoman side of the frontier, where Muslim districts can be compared with the Orthodox and Catholic communities among which they were historically embedded, a 10 percentage-point higher Muslim population share is associated with 3.6 percentage points lower literacy in 1931. The gradient declines sharply as literacy expands, but remains statistically significant in 1991. The same pattern is particularly visible for women. In contrast, Orthodox populations in Ottoman-legacy regions, which also began with low levels of literacy and conservative social structures, converged much more completely. The persistence of the residual gap therefore does not map simply onto Ottoman exposure: communities

subject to the same historical treatment followed different trajectories after entering the successor state.

We next ask whether slower convergence in Muslim-populated districts reflected unequal educational provision. The evidence increasingly rules out persistent differences in physical school supply. By 1961, the number of primary schools per capita does not vary systematically with Muslim population share. Muslim-share districts initially have fewer teachers per capita, but this deficit disappears and then reverses: by 1981 and 1991 they have, if anything, slightly more teachers. Yet literacy differences among the cohorts educated before provision fully equalised remain visible in later censuses. Together with evidence of lower Muslim enrolment already under the interwar kingdom, this pattern shifts attention from persistent shortages of schools toward differences in educational take-up and the cumulative consequences of earlier under-enrolment.

To investigate this mechanism more directly, we exploit a second natural experiment within Bosnia-Herzegovina. Sharia courts had been incorporated into the Yugoslav legal order after 1918 and provided a state-recognised institution linking Muslim family law and communal life to the successor state. A 1929 reform retained these courts only in districts with at least 5,000 Muslims according to the predetermined 1921 census, closing them below the threshold until all remaining sharia courts were abolished in 1946. This permits us to vary one dimension of the institutional relationship between Muslim communities and the state while holding the broader religious and cultural setting fixed. In settlements with a positive Muslim population share near the threshold, losing the court is associated with roughly 9 percentage points higher illiteracy in 1961. The effect is concentrated among women and among cohorts young enough to have been of school age during or immediately after the period of court closure; we find no robust corresponding effect for men or for older cohorts, and no rise in illiteracy in settlements without a Muslim population. Because several predetermined characteristics are imperfectly balanced at the cutoff and assignment is based on only 51 historical districts, we interpret this design as complementary rather than definitive causal evidence. Its Muslim-specific, gendered, and cohort-timed pattern nevertheless supports the interpretation that institutional alignment affected how fully families took up state-provided education.

Our first contribution is to the literature on historical persistence. A large body of work documents long-run effects of historical institutions and shocks on economic outcomes, political behaviour, trust, and culture (Dell, 2010; Nunn & Wantchekon, 2011; Lowes *et al.*, 2017). Within Central and Southeastern Europe, imperial rule has been linked to persistent differences in financial development, cultural values,

and trust in public institutions (Grosjean, 2011,a; Becker *et al.*, 2016). This literature has established convincingly that historical experiences can survive the institutions that originally generated them. Our contribution is to study the other side of the persistence question: how such effects unwind. Closest conceptually is Giuliano & Nunn (2021), who study conditions governing cultural persistence and change. We observe the same historical discontinuity repeatedly over seven censuses, allowing us to trace its transition from a very large initial human-capital gap toward convergence. The results show that persistence is better understood as a dynamic outcome than as a fixed property of a historical treatment. Common successor-state institutions can erode deep historical differences, but the speed and completeness of that process depend on how groups interact with those institutions.

Second, we contribute to work on education, diversity, and nation building. States have long used mass schooling not only to accumulate human capital but also to create common identities and integrate heterogeneous populations (Bandiera *et al.*, 2019; Alesina *et al.*, 2021; Cantoni *et al.*, 2017). Whether such efforts succeed is less clear. Bazzi *et al.* (2019) show that the effect of diversity on national integration depends on the structure of intergroup contact, while Fouka (2020) demonstrates that coercive assimilation through schooling can strengthen rather than weaken minority identity. Most closely related, Bazzi *et al.* (2026) show that Islamic educational institutions responded endogenously to Indonesia's expansion of mass public schooling, weakening its secular nation-building effects. We provide complementary evidence from a setting in which the state eventually succeeded in equalising much of the physical provision of schooling. Doing so generated enormous aggregate convergence, but not uniform convergence across communities. This distinction identifies an important limit to state capacity as an explanation for human-capital integration: building schools and supplying teachers can remove one constraint without ensuring that historically distant groups engage with state institutions to the same degree. Nation building therefore depends not only on the reach of the state, but also on the institutional relationship between the state and the populations it seeks to incorporate.

Third, the paper contributes to the political economy of religion and human capital. Existing research increasingly rejects interpretations of religion as a fixed cultural characteristic with uniform economic effects. Chaudhary & Rubin (2011), studying colonial India, show that the relationship between Muslim populations and literacy depended on the interaction between religious institutions and the collapse of Muslim political authority. Squicciarini (2020) shows that Catholic religiosity affected economic development through opposition to changes in educational con-

tent, while Malik & Mirza (2022) links historically embedded Muslim religious elites to the evolution of literacy following political change in Pakistan. More broadly, Becker *et al.* (2024) emphasise the multiple institutional and human-capital channels through which religion can shape development. Related economic-history evidence likewise shows that educational and economic disparities may follow ethnic or linguistic relations with the state rather than confessional differences themselves (Jain, 2017; Kersting *et al.*, 2020). Our results fit this institutional view. Muslim-populated Yugoslav districts did not remain behind simply because they were Muslim: their literacy rose dramatically, and most Muslim-majority societies elsewhere experienced substantial female educational convergence over the twentieth century. What distinguishes the persistent component in our setting is the interaction between religious minority status and the institutions of the successor state. The sharia-court experiment reinforces this interpretation by varying a Muslim-specific state institution within Bosnia rather than comparing Muslims with other religions. Religion, in this account, matters through its relationship with political institutions rather than as an immutable barrier to human-capital accumulation.

Our findings provide a dynamic account of imperial legacies. Historical institutions can create disparities that survive political borders and regime change for decades, but those disparities need not be permanent. The Yugoslav experience shows both the integrating power and the limits of common state institutions: mass education accompanied a remarkable erosion of the Habsburg-Ottoman human-capital divide, while convergence remained slower where the successor state's institutions were less closely aligned with those of a religious minority. Understanding when historical legacies fade therefore requires studying not only the institutions inherited from the past, but also the relationship between communities and the states that replace them.

The next section describes the historical setting. Section 3 presents the data, Section 4 the identification strategy, Section 5 the results, and Section 6 concludes.

## 2 Background

### *2.1 Imperial institutions and education*

The Habsburg and Ottoman Empires ruled much of Central and Eastern Europe for centuries, but governed very differently. After the eighteenth-century Theresian and Josephinian reforms, the Habsburg monarchy ran a professionalised, merit-based civil service with cadastral and fiscal registers, standardised schooling, and central oversight (Becker *et al.*, 2016; Ingrao, 2000; Judson, 2016). From Maria Theresa's 1774 *Allgemeine Schulordnung* on, it treated compulsory primary schooling as an instrument of civic discipline, pushing inspectors, and standard curricula into rural parishes and multiethnic peripheries (Judson, 2016; Cvrček, 2020). By the mid-nineteenth century schooling was a state-subsidised bureaucratic domain - and successor states inherited the capacity (inspectors, training colleges, standard curricula, etc.) to run mass campaigns of their own (Becker *et al.*, 2016; Vogler, 2019).

The Ottoman order, by contrast, sorted subjects by religion and ran communal affairs - family law and schooling above all - through recognised religious communities (millets). Muslims were exempt from the non-Muslim head tax (*cizye*) and alone could serve in the military or hold senior posts (İnalcık, 1973; Barkey, 2008). Schooling was decentralised and mostly religious: elementary instruction in mosque-attached Qur'anic schools (*mektebs*), advanced study in *medreses* that trained the religious-legal elite (Barkey, 2008; Fortna, 2011), with parallel Christian and Jewish networks under monasteries and rabbinical authorities (Sharkey, 2017). These institutions were locally funded and unevenly spread, and from the late seventeenth century tax-farming further eroded central control (İnalcık & Quataert, 1994). The empire thus left a thin, patchy administrative apparatus and no central educational infrastructure for successor states to inherit.

For the lands that became Yugoslavia, the contrast was stark: Habsburg-legacy regions entered the twentieth century with denser school networks, more secular curricula, and higher literacy, while Ottoman-legacy regions had thin local bureaucracies and schooling still rooted in religious instruction.

### *2.2 The Habsburg-Ottoman border*

Drawn after the Treaty of Karlowitz (1699) and fixed by the Treaty of Belgrade (1739), the Habsburg-Ottoman frontier ran from the Adriatic to the Carpathians, following the Una, Sava, and Danube before crossing the Banat plain (Fodor, 2009; Stoye, 1994; Rothenberg, 1966). Unlike earlier porous frontiers, the post-1739 line was

marked by cairns and surveyed by joint commissions of engineers and cartographers - a clearly demarcated, enforceable boundary. Its logic was military: it primarily followed terrain and defensibility, which is what makes some stretches usable as a quasi-experimental cutoff (Fodor, 2009; Kurtaran, 2018; Gümüşçü, 2010).

Geographic borders are rarely random (Kocher & Monteiro, 2016; Paine *et al.*, 2025), and the credibility of a geographic regression discontinuity must be judged locally, point by point (Keele & Titiunik, 2015; Cattaneo & Titiunik, 2022). So we treat the frontier not as one uniform cutoff but as a string of local natural experiments of varying quality. Fortress-adjacent stretches around Belgrade and Petrovaradin were placed for military, political, or economic reasons and are unlikely to be exogenous to later development. River segments along the Una and Sava were adopted as convenient lines to demarcate, of little strategic or economic value - what Paine *et al.* (2025, p. 17) call "conditional randomness." These "low-stakes" stretches anchor our identification. As a historian of the border put it, the Una was "too narrow a stream, with too many fords" (Stoye, 1994, p. 91), while "[t]he boundary along the Sava and its Slavonian hinterland interested him less [...] A world of flood, forest and marsh faced hillier but otherwise similar country across the river. In Slavonia there were no fortresses, but no need for them" (Stoye, 1994, p. 186).

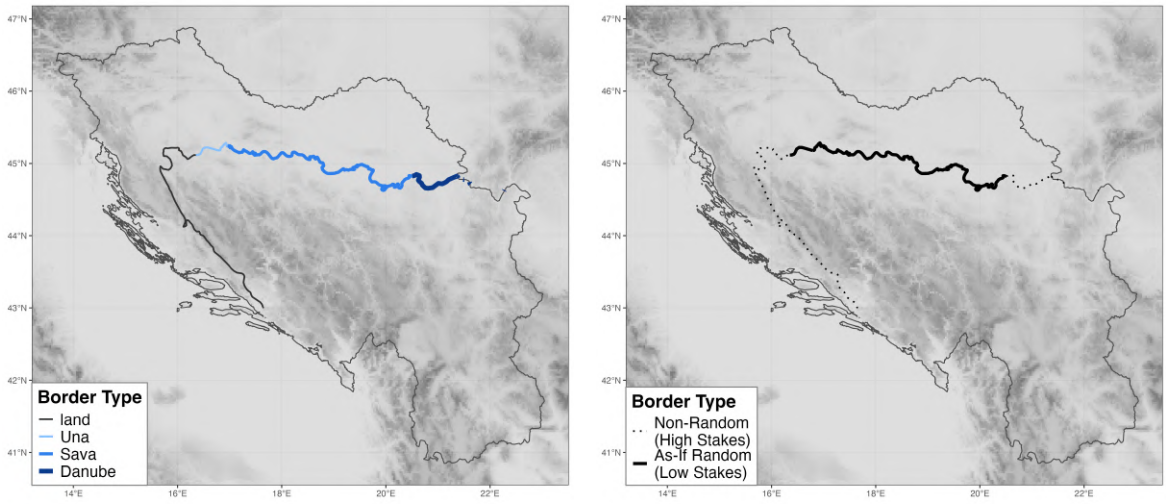
The Una-Sava zone was also heavily depopulated during the sixteenth- and seventeenth-century wars, and both empires then resettled it by design - the Habsburgs through the Vojna Krajina (Military Frontier), settling Serb, Vlach, and other refugees as soldier-farmers (Rothenberg, 1960, 1966; Kaser, 1997), the Ottomans through the *serhat*, their own military borderland, settled with garrison communities. So the border did not split a settled, self-selected population: both sides were resettled by imperial design, so differences across the line reflect state placement rather than households sorting themselves - which weakens the sorting and manipulation concern common to geographic RDDs. Because the two empires settled the zone with different populations, compositional differences remain, and we address those directly through the local balance tests below and, for religion specifically, the within-Bosnia sharia design.

Because the Una and Sava stretches were drawn for geographic convenience rather than strategic or economic intent, they are the most credible quasi-experiments for studying imperial legacies: geography, not politics, set exposure to empire, providing identifying variation.[3] Figure 2 maps the border, separating river segments (Una, Sava, Danube) from land segments and grading each by credibility. We validate the segments in three ways - balance on five pre-treatment geographic covariates

[3] See subsection D.3 of the appendix for a longer discussion on the as-if random assumption.

(elevation, slope, precipitation, temperature, wheat suitability), McCrary density tests (McCrary, 2008) at each point, and the historical record - and lean on the Sava segments, where all three line up and which form the core of the design. The Una is well balanced and historically credible but shows some density bunching at points along it (Appendix Figure A.9), so we read its estimates with more caution.

***Figure 2.*** *Border segments classified by identification credibility*




*Notes*: Both panels display the Habsburg-Ottoman border over an elevation raster within Yugoslavia. Left panel distinguishes land and river segments: land (dark grey), Una (light blue), Sava (medium blue), and Danube (dark blue). Right panel classifies segments by identification credibility: solid lines mark as-if-random (low-stakes) river segments where geography determined the border; dotted lines mark non-random (high-stakes) segments where strategic or military considerations influenced border placement.

### *2.3 Literacy and schooling in Yugoslavia*

Yugoslavia inherited a patchwork of school systems built on deep imperial legacies. In Habsburg-legacy areas, Maria Theresa's *Ratio Educationis* (1777) had laid down a secular primary-school network well before 1918, and literacy in some districts already reached 50-60% by the late nineteenth century (Cvrček, 2020; Lampe, 1996).

Muslim communities across Bosnia, Sandžak (straddling modern Serbia and Montenegro), Kosovo, and Macedonia leaned far more on religious *mektebs* and *medreses*. Austro-Hungarian rule did open secular schools in Bosnia and Herzegovina (1878-1918), but Muslim enrolment lagged: many families kept their children from schools run by a Christian occupying administration they did not regard as their own - an early instance of the alignment friction - and provision itself remained thin (Donia & Fine, 1994; Velikonja, 2003). Catholic and Orthodox networks added

scattered "islands" of mixed religious-secular schooling, but reached only so far (Brown, 1946; Lampe, 1996). By 1931 the result was a sharp jump in literacy at the frontier, tracking the institutional contrast above. These starting points went on to shape who got schooled, how fast literacy rose, and how far later reforms could equalise it (Kornai, 1992; Lampe, 1996).

The Kingdom of Serbs, Croats, and Slovenes (renamed Yugoslavia in 1929) tried to weld its regions together through standardised schooling. The 1929 Law of Popular Schools mandated eight years of compulsory education, widened vocational tracks, and aimed to harmonise curricula (Farmerie, 1972). But capacity lagged the law: rural areas lacked buildings, inspectors, textbooks, and trained teachers, attendance went unenforced, and local languages and religious calendars slowed any uniform rollout (Lampe, 1996; Tomich, 1963; Vučenov, 1955). The state had legislated a modern school system before it could deliver one everywhere.

After the Second World War, the Communist regime did two things at once: it expanded schooling and dismantled Muslim institutional autonomy. It abolished sharia courts, nationalised *vakuf* property, closed *mektebs* by decree, and destroyed or damaged 756 mosques (Mekić, 2017; Alibasić & Zubčević, 2009). This dismantling had in fact begun earlier, under the interwar monarchy: the 1929 Law on Sharia Courts already closed courts in Muslim-minority districts (Karčić, 2005). On the schooling side, the 1945 reforms lengthened compulsory education, standardised curricula, and expanded teacher training (Zivojnović & Levi, 1959), while mass adult-literacy campaigns lifted national literacy from about 50% to 75% by 1953 (Ernjaković & Krneta, 1959; Tomich, 1963). The state provided the inspectors, administrators, and teachers needed to enforce attendance in long-underserved communes (Bombelles, 1968; Šetinc, 1980; Tomich, 1963).

Census records reveal that by 1961 literacy had risen everywhere, but the frontier pattern held: Habsburg-legacy districts ran at 76–98% (10th-90th percentile) against 58–81% on the Ottoman side. By 1991 the north and west reached 94–100%, while Bosnia and Herzegovina, Macedonia, and Montenegro still lagged at 85–95%. The whole Ottoman-side distribution shifted right between 1931 and 1991, but stayed just below the Habsburg median.

Like communist regimes across Eastern Europe, Yugoslavia treated mass education as a tool of modernisation and gender equality - meant not just to build human capital and socialist loyalty, but to break down traditional hierarchies, gender norms above all (Ramet, 1992, 2006). These reforms paid off most for women. In Ottoman-legacy regions female literacy, barely 15% in 1931, passed 77% by 1981, and the cross-frontier female gap shrank from 56 points in 1931 to about 12 by 1991. The

party made schooling both compulsory and enforceable: parents who kept daughters home risked fines or public denunciation, while families who complied received supplies and stipends (Brunnbauer, 2000). Literacy drives and evening classes reached women who had missed school, and teachers were sent to remote areas; in strongly patriarchal districts - many of them Muslim - authorities even held classes in mosques and community centres to ease resistance (Popov & Gojković, 2000; Sekulić, 2004). The starting point was well documented: the 1931 census volume traced female illiteracy to mountainous, sparsely settled regions marked by "patriarchal attitudes of older generations, especially regarding girls, religious considerations, and other causes" (*patrijarhalna shvatanja starijih generacija, naročito ženske dece, verski obziri i drugi uzroci*) (Kraljevina Jugoslavija Opšta Državna Statistika, 1938b). But patriarchy was no Muslim monopoly: the Orthodox communities of the Ottoman-legacy regions, themselves religiously conservative and poorly schooled in 1931, had nearly converged by 1991. That contrast is suggestive: cultural conservatism does not on its own explain the lingering gaps; what mattered, we argue, was whether a community trusted the state delivering the schooling.

## 3 Data

The analysis combines newly assembled historical, census, and spatial data to trace human-capital differences along the former Habsburg-Ottoman frontier and to study why those differences converged at different rates. We reconstruct the changing imperial frontier between 1326 and 1922 from 152 georeferenced historical maps and link it to local administrative units observed in seven Yugoslav censuses between 1931 and 1991. For each census wave, we harmonise historical place names and boundaries, geocode the relevant local units, and construct measures of literacy and religious composition. We complement these data with district-level measures of schools and teachers for the socialist period and, for the within-Bosnia analysis, with settlement-level educational outcomes linked to the 1921 districts that determined whether a sharia court survived the 1929 reform.

The resulting dataset therefore has three components. The first traces the Habsburg-Ottoman human-capital discontinuity and its evolution from 1931 to 1991. The second measures religious composition and educational provision to study the spatial heterogeneity of convergence. The third exploits the 1929 sharia-court reform within Bosnia-Herzegovina to examine whether changes in a Muslim-specific state institution affected subsequent educational outcomes. Appendix A documents the underlying sources, historical-boundary reconstruction, geographic matching, and

coding procedures in detail. Appendix B describes the reconstruction of sex-specific literacy where published census tabulations are incomplete, while Appendix L examines migration and post-imperial population sorting.

### *3.1 The historical frontier*

We digitised 152 historical maps to reconstruct the changing Habsburg-Ottoman frontier over the long period in which imperial exposure accumulated. The frontier underwent four major configurations following the treaties of Vasvár (1664), Karlowitz (1699), Passarowitz (1718), and Belgrade (1739). Our baseline treatment is defined by the line established in 1739. This frontier remained the de facto Habsburg-Ottoman boundary until imperial dissolution in 1918 and therefore captures the longest stable period of differential institutional exposure immediately preceding Yugoslav unification.

To verify that the 1739 line meaningfully separates areas with different imperial histories, we reconstruct cumulative years of Ottoman rule using the full sequence of historical maps. The 1739 frontier produces a sharp and persistent discontinuity in Ottoman exposure of 177.53 years (Appendix D.2). We therefore code local administrative units on the Ottoman side of this line as treated and those on the Habsburg side as controls. Because the empirical strategy allows treatment effects to vary along the boundary, the reconstruction also distinguishes the Una, Sava, Danube, and land segments of the frontier and preserves the precise border geometry used in the two-dimensional boundary RD.

### *3.2 Literacy, 1931–1991*

Our main outcome is the literacy rate reported in the Yugoslav censuses of 1931, 1948, 1953, 1961, 1971, 1981, and 1991. The 1931 census classified individuals aged 10 and above according to whether they could both read and write; subsequent censuses applied the same definition to the population aged 9 and above.[4] The seven waves allow us to observe the initial human-capital discontinuity shortly after Yugoslav unification and then follow its evolution across six decades of common statehood.

Administrative geography changed considerably over this period. The 1931 census reports literacy for 378 *srezovi*, while the later socialist censuses increasingly use *opštine*; by 1991 the system contains 508 local administrative units. We geocode every unit separately for each census wave by matching historical names to period

[4] Where the published census volumes use slightly different age groupings or tabulation conventions, we harmonise the denominator as closely as the source material permits; Appendix A provides the census-specific definitions.

administrative maps and modern spatial references. The unit of analysis in a given census is therefore the local administrative unit reported by that census rather than an artificially imposed time-invariant geography. The RD estimators compare these units locally around the fixed 1739 frontier in each wave.

The 1931 census provides the baseline cross-sectional outcome used in Table 2 and Figure 4. The later waves are used to trace the evolution of the frontier discontinuity in Figures 5–6 and to estimate the religious-composition gradients reported in Table 3.

We observe male and female literacy directly wherever the published census tables permit. Some sex-specific municipal tabulations are unavailable in 1981 and 1991. For these cases, we reconstruct male and female literacy using the observed overall literacy rate, local sex composition, and the corresponding higher-level sex-specific census totals, imposing consistency with the published aggregate rate. Appendix B describes the procedure and validation exercises. For Macedonia and Montenegro in 1991, where municipality-level literacy returns are not available, we estimate local overall literacy using each unit's 1981 literacy and the 1981–1991 relationship observed in republics for which both waves are available. We therefore distinguish throughout between directly observed and reconstructed outcomes and report robustness to excluding imputed observations in the appendix.

### *3.3 Religious composition*

Religious composition is central to the analysis of heterogeneous convergence. The 1931 census reports religion directly, allowing us to construct local Muslim, Orthodox, and Catholic population shares before the socialist period. Post-war censuses increasingly recorded nationality rather than religion. We therefore reconstruct religious composition for 1948–1991 by mapping census nationality categories to the historically corresponding religious communities, following the procedure described in Appendix C.

This reconstruction is particularly straightforward for the large groups relevant to our analysis: Muslim/Bosniak populations, predominantly Orthodox Serbs and Montenegrins, and predominantly Catholic Croats and Slovenes. We use these measures to examine whether the decline of the Habsburg-Ottoman literacy discontinuity differed systematically with local religious composition. Because religious shares are not predetermined geographic characteristics, they are not included among the covariates used to validate the frontier RD; instead, they are outcomes of historical settlement and are used to study heterogeneity in the convergence process.

### *3.4 School and teacher provision*

To distinguish persistent differences in educational provision from differences in educational take-up, we assembled district-level measures of primary-school infrastructure for the socialist period. The data report the number of primary schools and teachers for 1961, 1981, and 1991, which we scale by the local population to construct schools and teachers per 1,000 inhabitants.

These outcomes allow us to test whether Muslim-populated districts continued to face systematically weaker educational supply after the post-war expansion of mass schooling. Figure 7 relates school and teacher provision to the Muslim population share within the RD2D analysis sample. Comparable local supply data are not available for all seven census waves, so this exercise does not identify the entire historical path of school provision. Its purpose is narrower: to establish whether persistent literacy differences by the later socialist period can be explained by contemporaneous shortages of schools or teachers.

### *3.5 The 1929 sharia-court reform*

The within-Bosnia analysis requires a separate dataset linking historical administrative institutions to later settlement-level educational outcomes. Under the 1929 Law on Sharia Courts, a district retained its sharia court only if it contained at least 5,000 Muslims according to the 1921 census. We digitise the 1921 district boundaries for Bosnia-Herzegovina and record the corrected Muslim population count for each of the 51 districts. Twelve districts fell below the threshold and lost their courts, while the remaining districts retained them until the communist authorities abolished all sharia courts in 1946.

The outcome data come from the 1961 settlement census. We match each 1961 settlement (*naselje*) to its historical counterpart in the 1885 Habsburg gazetteer and coordinates, assign it to its corresponding 1921 district, and thereby attach the predetermined 1921 Muslim count that determines treatment. This produces settlement-level measures of total, male, and female illiteracy, together with illiteracy by age group. The age-specific outcomes allow us to distinguish cohorts that were of school age during or after the 1929–1946 treatment window from cohorts whose schooling largely preceded the reform.

We measure pre-reform local religious composition using the 1910 census. Because a large share of settlements had no Muslim population, we distinguish settlements with zero Muslim share from those with positive shares and divide the latter into lower- and higher-Muslim-share groups for the heterogeneous RD analysis. The

sharia design also uses a set of predetermined local characteristics—elevation, the 1910 Orthodox and Catholic population shares, 1910 ethnic fractionalisation, and an 1885 road-access indicator—that exhibit imbalance around the administrative cutoff and therefore enter as covariates in the controlled local-linear specifications. Appendix P describes the matching procedure, while Appendix Q reports balance, density, bandwidth, and inference tests.

### *3.6 Geographic covariates*

The geographic RD design relies on continuity of potential outcomes at the historical frontier. To assess this assumption, we use five predetermined geographic characteristics: elevation (metres), slope (degrees), annual precipitation (cm), annual temperature (°C), and FAO rainfed-wheat suitability. These variables capture terrain, climate, and agricultural potential—factors that could independently influence settlement patterns, economic development, and the historical supply of schooling.

***Table 1.*** *Summary Statistics: Habsburg vs. Ottoman Side, Yugoslavia 1931*

| | Habsburg side | | | Ottoman side | | | | |
|---|---|---|---|---|---|---|---|---|
| Variable | $N$ | Mean | SD | $N$ | Mean | SD | Diff. | $p$-value |
| Literacy (%) | 175 | 76.4 | 16.1 | 203 | 33.0 | 12.0 | 43.5*** | <0.001 |
| Female literacy (%) | 175 | 70.6 | 20.9 | 203 | 14.9 | 9.0 | 55.7*** | <0.001 |
| Muslim share (%) | 175 | 0.1 | 0.3 | 203 | 20.3 | 24.9 | -20.2*** | <0.001 |
| Orthodox share (%) | 175 | 22.0 | 25.3 | 203 | 72.7 | 30.6 | -50.7*** | <0.001 |
| Religious frac. | 175 | 0.2 | 0.2 | 203 | 0.2 | 0.2 | -0.0 | 0.208 |
| Public servants (%) | 175 | 2.9 | 5.1 | 203 | 1.6 | 1.7 | 1.4*** | <0.001 |
| Elevation (m) | 175 | 260.8 | 229.2 | 203 | 627.7 | 344.6 | -366.9*** | <0.001 |
| Slope (deg.) | 175 | 1.9 | 2.1 | 203 | 3.8 | 1.9 | -1.9*** | <0.001 |
| Temperature (°C) | 175 | 10.6 | 1.6 | 203 | 9.6 | 1.6 | 1.0*** | <0.001 |
| Precipitation (cm) | 175 | 77.5 | 25.1 | 203 | 68.8 | 17.2 | 8.7*** | <0.001 |
| Wheat suitability | 175 | 2.9 | 1.4 | 203 | 2.2 | 1.1 | 0.8*** | <0.001 |
| Pop. density | 175 | 162.6 | 383.3 | 203 | 65.2 | 179.8 | 97.4*** | 0.002 |

*Notes*: Unit of observation is the district (*srez*). Habsburg side: districts on the Habsburg side of the 1739 frontier ($T_i = 0$). Ottoman side: districts on the Ottoman side ($T_i = 1$). Diff. is the difference in means (Habsburg − Ottoman). $p$-values from two-sample $t$-tests. *Sample*: all 378 frontier districts of the 1931 census (175 Habsburg-side, 203 Ottoman-side); unlike the regression tables, it is *not* restricted to the RD2D estimation bandwidth. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The RD2D framework tests balance in these characteristics separately at each of the 88 evaluation points along the frontier rather than assuming that the entire boundary is comparable. The points are spaced at least 10 kilometres apart and are described formally in Section 4. We classify an evaluation point as covariate-

balanced when none of the five characteristics exhibits a statistically detectable discontinuity there. These local balance tests are important because Table 1 shows that the two sides of the frontier differ in several characteristics when compared globally. The preferred analysis therefore relies on the locally balanced frontier segments, particularly along the Sava, while the OLS specifications use the same geographic variables directly as controls.

## 4 Empirical strategy

This section sets out the identification strategy. We first define the treatment as a municipality's position relative to the 1739 frontier; we then specify the two-dimensional boundary regression discontinuity (RD2D), which estimates the Ottoman literacy penalty locally at many points along the border rather than as a single pooled effect; and we close by stating the assumptions under which those estimates are causal, together with the evidence that supports them.

### *4.1 Border treatment*

Treatment is being on the Ottoman side of the 1739 frontier at imperial dissolution: $T_i = 1$ for municipalities under Ottoman jurisdiction, $0$ for those under Habsburg rule, fixed by the historical boundary.[5] We treat the border not as one natural experiment but as many local ones of varying credibility - which lets us map how the Habsburg-Ottoman gap differs across segments (rivers vs. fortress-adjacent stretches) instead of forcing a single global effect. Every geographic RD assumes that units just across the boundary have comparable potential outcomes - the "as-if-random" condition (Sekhon & Tiutiunik, 2017, p. 13). Few borders meet it everywhere: (Paine *et al.*, 2025) show colonial lines often track pre-existing political and geographic features, and (Kocher & Monteiro, 2016) insist "qualitative historical knowledge is essential for validating natural experiments" (p. 952). We follow both - judging conditional randomness locally and grounding each segment in the historical record.

A natural objection is that this assignment is ill-defined because the Habsburg-Ottoman frontier shifted repeatedly before 1739, so that land north of the final line had at times been Ottoman, and land to the south at times Habsburg. Three

[5] Why 1739? Of the frontier's four main configurations (1664, 1699, 1718, 1739), the Treaty-of-Belgrade line of 1739 is the one that held: it remained the de facto Habsburg-Ottoman boundary until imperial dissolution in 1918, so it fixed each municipality's cumulative imperial exposure over the long period in which the literacy gap formed, and it produces the largest and most stable discontinuity in years of Ottoman rule (177.53 years; Appendix D.2).

points address this. First, the 1739 line was the *stable* frontier that held until imperial dissolution in 1918; it therefore fixed each municipality's imperial exposure across the long 1739-1918 period during which the literacy gap took shape. Reconstructing the full sequence of frontier changes from 1326 to 1922 using 152 georeferenced historical maps (Appendix A) confirms that the 1739 boundary marks a sharp, durable break in the duration of Ottoman rule. Second, the design is *local*: we compare municipalities immediately adjacent to the demarcated line, so changes of control deep in the interior, far from the cutoff, do not affect assignment at the frontier. Third, the one *systematic* departure from clean assignment also runs in our favour: Bosnia and Herzegovina sits on the Ottoman side of the core Una and Sava segments, yet passed to Habsburg administration in 1878 and remained under it until 1918, so these Ottoman-coded municipalities absorbed four decades of Habsburg schooling and bureaucracy on top of their Ottoman inheritance. That late Habsburg rule raised literacy on the treated side - even if Muslim enrolment lagged - compressing the measured Ottoman penalty toward zero. Our estimates are therefore a conservative lower bound on the gap that uninterrupted Ottoman rule would have produced.

### *4.2 Specification*

Formally, let $Y_i$ denote district-level literacy, $\mathbf{X}_i = (\text{lon}_i, \text{lat}_i)$ the bivariate geographic location of district $i$, and $\mathcal{B}$ the Habsburg-Ottoman frontier. The causal parameter of interest at each boundary point $\mathbf{b} \in \mathcal{B}$ is:

$$\tau(\mathbf{b}) \equiv \mathbb{E}[Y_i(1) - Y_i(0) \mid \mathbf{X}_i = \mathbf{b}], \tag{1}$$

where $Y_i(1)$ and $Y_i(0)$ denote potential outcomes under Ottoman and Habsburg rule, respectively. Under the continuity assumptions of the two-dimensional boundary RD framework (Cattaneo *et al.*, 2025a,b), this parameter is identified by:

$$\tau(\mathbf{b}) = \lim_{\mathbf{x}\to\mathbf{b},\, \mathbf{x}\in\mathcal{A}_1} \mathbb{E}[Y_i \mid \mathbf{X}_i = \mathbf{x}] - \lim_{\mathbf{x}\to\mathbf{b},\, \mathbf{x}\in\mathcal{A}_0} \mathbb{E}[Y_i \mid \mathbf{X}_i = \mathbf{x}], \tag{2}$$

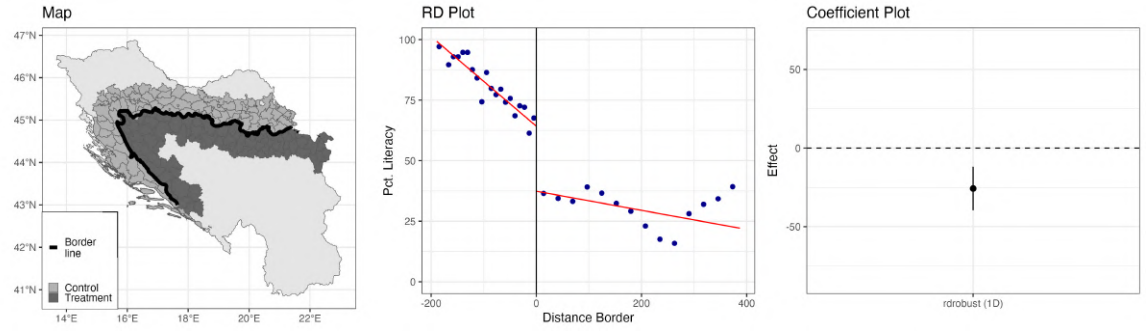
where $\mathcal{A}_1$ and $\mathcal{A}_0$ are the Ottoman (treated) and Habsburg (control) sides. $\tau(\mathbf{b})$ may vary along $\mathcal{B}$, so the effect can differ across segments with different histories and geographies.

This departs from standard GRD, which adds border-segment fixed effects and reports one pooled effect, assuming treatment is equally as-if-random everywhere. Pairing idiographic history with RD2D, we instead split the border by how plausibly each stretch is as-if-random - singling out the low-value river segments (Una, Sava)

most likely to qualify. RD2D then tests balance (elevation, slope, soil, and so on) point by point, so we can locate the segments, riverine or otherwise, where the design is credible.

To show what RD2D adds over standard rdrobust, Figure 3 runs the familiar one-dimensional RD, with the 1739 border as the cutoff and signed distance to it as the running variable. The left panel maps the districts inside the MSE-optimal bandwidth on each side (control vs. treatment); the middle panel plots binned averages with triangular-kernel local-linear fits on each side; the right panel reports the bias-corrected estimate with a robust 95% confidence interval. This gives one average effect for the whole frontier, but it blurs local heterogeneity and is sensitive to irregular border geometry (kinks, bends, river meanders).[6]

***Figure 3.*** *Evaluation with RDRobust*




*Notes:* Left panel: map of Yugoslav districts shaded by literacy rate, with districts inside the MSE-optimal bandwidth highlighted (light grey = control, dark grey = treatment). Middle panel: RD plot with binned averages and local-linear fits (triangular kernel) estimated separately on each side of the cutoff. Right panel: bias-corrected point estimate with robust 95% confidence interval.

RD2D, by contrast, estimates bias-corrected effects at many points along the boundary, using the location $\mathbf{X}_i = (\text{lon}_i, \text{lat}_i)$ as the running variable with MSE-optimal bandwidths (Cattaneo *et al.*, 2025b). It estimates points with a local linear fit ($p = 1$) and does robust bias-corrected inference with a local quadratic ($q = 2$) and a triangular kernel; the resulting points are the bias-corrected estimates $\hat{\tau}_q(\mathbf{b})$, and the bands are uniform, valid across all evaluation points at once.[7] Around each point it fits separate regressions on each side within a rectangular product-kernel window, with side-specific MSE-optimal bandwidths. Letting the effect vary along the frontier is precisely the design's purpose: it reveals where the Ottoman penalty is largest among the credible (balanced, no-sorting) segments (Figure 4), whereas

[6] Section I of the appendix presents treatment effects across a range of bandwidths and kernels.

[7] Because estimation ($p = 1$) and inference ($q = 2$) use different polynomial orders, a point estimate can occasionally fall outside the robust bands where the conditional expectation is highly curved (Cattaneo *et al.*, 2025b).

distance-based designs average credible and less credible segments together and can attenuate the effect exactly where the comparison is cleanest.

Variance estimation matches the estimator: the OLS and religious-share regressions use Conley (1999) spatial HAC standard errors (Bartlett kernel, 30 km cutoff),[8] while RD2D uses heteroskedasticity-robust standard errors and RDRobust clusters at the county level, which in our sample nearly coincides with the district itself. The district is the unit of observation, so there is no meaningfully finer level to cluster on, and we do not cluster at a coarser one either: the only candidate is the nine *banovine* created in 1929, which are themselves a product of interwar state-building - and thus endogenous to the dynamics we study - and which would in any case be too few for reliable asymptotic inference (Cameron *et al.*, 2008).

### *4.3 Identifying assumptions*

Identification rests on the continuity of potential outcomes at the frontier: absent the imperial border, literacy and its determinants would vary smoothly across the 1739 line, so any discontinuity at the line is attributable to the treatment rather than to some other gradient. The line was fixed in 1739 and has not moved, so units cannot manipulate which side they fall on; the relevant threat is instead whether populations *sorted* across the border after the empires fell, which would break continuity by making the two sides systematically different for reasons unrelated to imperial rule. We assess this in three ways.

First, we run McCrary density tests at all 88 evaluation points; along the most credible Sava segments, on which identification rests, none is significant.[9] Second, the five geographic covariates are balanced at the Sava points (Appendix Figures A.7 and A.8), so observable pre-treatment characteristics do not differ across the line. Third, the frontier populations were put in place by imperial design, not by self-selection: the Habsburg *Vojna Krajina* and Ottoman *serhat* resettled the depopulated corridor, and its fall set off no large forced migration - unlike the post-Ottoman population exchanges in Greece - to reshuffle them afterward.[10] So the compositional

[8] The 30 km cutoff is the mean centroid-to-centroid distance between contiguous districts (29.3 km), so it absorbs spatial correlation between neighbours. Appendix Table A.2 shows the results are essentially unchanged at 60 km and 90 km.

[9] Some discontinuity appears near the Dalmatian coast and the Una (Appendix Figure A.9), so we treat those points with caution, since differential migration would leave a jump in settlement intensity.

[10] The most important post-WWII movement in the frontier zone was the expulsion of ethnic Germans (*Volksdeutsche*) from Habsburg-legacy regions, chiefly Vojvodina. Appendix L documents these and other minority outflows (Italians, Turks, Albanians) and tests their impact using gridded population change between 1931 and 1961. Because that expulsion was concentrated in Vojvodina, far from

contrast across the line reflects where the empires placed people, not households sorting across the border, and is itself part of the imperial legacy rather than a threat to the design (Cattaneo *et al.*, 2025a).

A complementary falsification asks whether the discontinuity tracks the historical frontier or merely a smooth north-south gradient (remoteness from Istanbul or Vienna): shifting the entire border in 10-km steps, artificial borders lose significance beyond roughly 20 km from the true line (Appendix G).

A second assumption is no cross-border spillovers (SUTVA). If schooling policy or cultural norms diffused across the line, control-side units would be partly treated and the estimated jump would *understate* the true discontinuity. Any such spillover therefore biases the estimate toward zero, leaving it a conservative lower bound rather than a spurious effect.

## 5 Results

### *5.1 Literacy differences at the frontier in 1931*

Table 2 and Figure 4 document a large discontinuity in literacy along the former Habsburg-Ottoman frontier in 1931. Across all specifications, districts on the former Ottoman side had substantially lower literacy than neighbouring districts on the former Habsburg side. In the simplest OLS specification, the gap is $-43.5$ percentage points. Adding geographic controls for elevation, slope, temperature, precipitation, and wheat suitability reduces the estimate only modestly, to $-37.2$ percentage points. The one-dimensional local RD estimate is smaller, at $-26$ percentage points, consistent with the fact that pooling all frontier segments averages across locations where the historical border is more and less suitable for local comparison.

Figure 4 presents the corresponding two-dimensional boundary RD estimates. The first row maps literacy rates in 1931, with darker shading indicating higher literacy. The second row reports RD2D estimates at 88 evaluation points along the frontier, spaced at least 10 kilometres apart. Redder points indicate larger negative effects of Ottoman rule. Opacity indicates covariate balance: opaque points are balanced on all five geographic covariates, while translucent points fail at least one balance test (Appendix E). The estimates are negative almost everywhere, and 73 of the 88 point estimates are statistically significant.

The most credible evidence comes from the covariate-balanced evaluation points. These are shown in the third row of Figure 4, together with the pooled OLS and

the Una-Sava segments that anchor the RD2D, the local estimates are largely insulated from it, and they are unchanged when population-loss regions are dropped altogether.

***Table 2.*** *The Ottoman Literacy Penalty in 1931*

| | (1) OLS | (2) OLS + Controls | (3) RDRobust |
|---|---|---|---|
| *Panel A: Population literacy* | | | |
| Ottoman treatment | $-43.5^{***}$ | $-37.2^{***}$ | $-25.7^{***}$ |
| | (1.7) | (2.0) | (9.0) |
| $N$ | 378 | 378 | 166 |
| Bandwidth (km) | | | 61.3 |
| *Panel B: Female literacy* | | | |
| Ottoman treatment | $-55.7^{***}$ | $-51.6^{***}$ | $-34.5^{***}$ |
| | (2.0) | (2.2) | (10.7) |
| $N$ | 378 | 378 | 167 |
| Bandwidth (km) | | | 61.5 |
| Geographic controls | No | Yes | — |
| Inference | Conley | Conley | Cluster-robust |
| Kernel | — | — | Triangular |

*Notes*: Dependent variable is literacy rate (percentage). Columns (1)–(2): Conley (1999) spatial HAC standard errors (Bartlett kernel, 30 km cutoff) in parentheses. Column (1): OLS of literacy on Ottoman treatment. Column (2): adds geographic controls (elevation, slope, temperature, precipitation, wheat suitability). Column (3): local polynomial RD (rdrobust) with bias-corrected estimates, cluster-robust standard errors (clustered by county), and triangular kernel. *Sample*: columns (1)–(2) use all 378 frontier districts of the 1931 census; column (3) restricts to rdrobust's MSE-optimal bandwidth around the frontier (the effective $N$ is reported in the table). The 30 km cutoff equals the mean centroid distance between contiguous districts (29.3 km). Table A.2 in the appendix reports robustness to alternative cutoffs. RD2D estimates at individual border points are reported in Figure 4. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

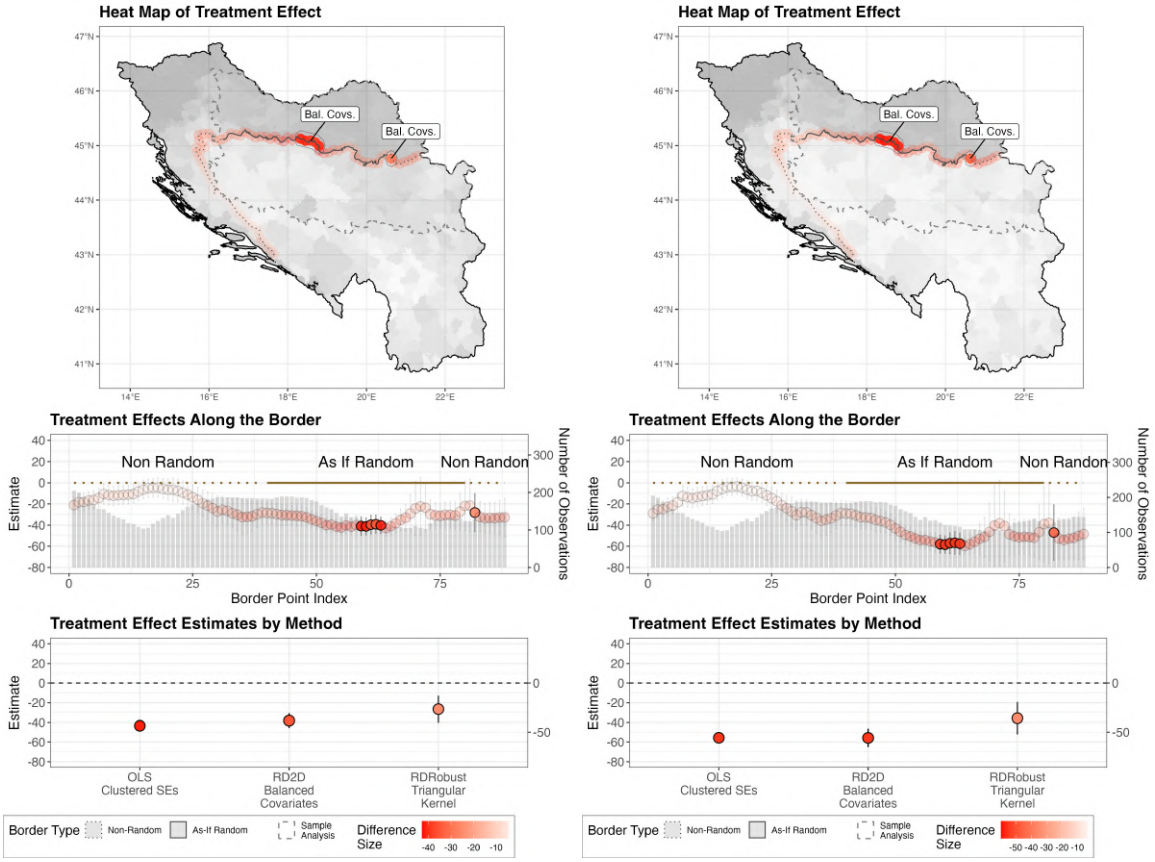
one-dimensional RD estimates. Across the balanced RD2D points, the estimated Ottoman literacy penalty ranges from $-41$ to $-28$ percentage points. The largest and most credible estimates cluster along the Sava, where five of the six balanced points are located. This segment is particularly useful for identification because the river border was drawn largely for geographic convenience, making it a plausible setting for conditional local comparison.

These results are robust to alternative ways of estimating the discontinuity. Appendix H reports distance-based RD estimates, with and without a kink adjustment,[11] as well as standard geographic RD estimates. All three approaches produce similar conclusions. We therefore follow (Cattaneo *et al.*, 2025b) and use the bivariate-location RD2D method as our preferred specification, since it is better suited to

[11] The kink adjustment corrects for bends in the frontier: where the border changes direction, distance to the boundary is a non-smooth function of a unit's location, which can bias a standard distance-based estimator (Cattaneo *et al.*, 2025b).

irregular border geometry and allows us to assess covariate balance at each evaluation point.

***Figure 4.*** *Two-dimensional boundary RD estimates of the Ottoman literacy penalty in Yugoslavia, 1931.*




*Notes*: Left column: population literacy. Right column: female literacy. Row 1 maps literacy rates in 1931 (darker = higher). Row 2 shows RD2D point estimates along the frontier (darker red = larger Ottoman penalty). Opacity indicates covariate balance: opaque points = balanced on all covariates; translucent points = unbalanced on at least one covariate. Row 3 compares estimates from three methods, with 95% confidence intervals: OLS, RD2D at balanced-covariate points, and RDRobust (standard 1D); the RD2D interval is the joint-covariance confidence interval for the average across the six balanced points, which accounts for the spatial correlation among the neighbouring border points rather than treating them as independent (the naive standard error of the mean would understate it). The dashed outline in Row 1 marks the analysis-sample window formed by the MSE-optimal bandwidth neighbourhoods around the balanced evaluation points.

The literacy penalty was especially large for women. Panel B of Table 2 shows that female literacy was 55.7 percentage points lower on the former Ottoman side in the OLS specification and 51.6 percentage points lower after adding geographic controls. The one-dimensional RD estimate is 34.5 percentage points. The RD2D estimates (Figure 4, right column) along the balanced Sava points are larger still, exceeding 57 percentage points. This magnitude is visible in the raw data: in 1931, female literacy

averaged only 15% on the former Ottoman side, compared with 71% on the former Habsburg side, a 56-percentage point raw gap. The sharpest discontinuities therefore appear precisely where the local comparison is most credible.

### *5.2 Long-term convergence*

Figure 5 follows the Ottoman literacy penalty across the seven census waves between 1931 and 1991. All three estimators point to substantial convergence. In the preferred RD2D estimates at the balanced Sava points, the literacy gap falls from $-41.2$ percentage points in 1931 to roughly $-4$ percentage points by 1991. The pooled OLS estimates follow a similar trajectory, declining from $-43.5$ to $-6.8$ percentage points. On the raw percentage-point scale, therefore, most of the initial discontinuity had disappeared by the end of the socialist period.

This raw decline, however, partly reflects the bounded nature of literacy. As literacy approaches universality on both sides of the former frontier, remaining differences mechanically become smaller in percentage-point terms. Appendix Table A.1 therefore re-measures convergence with bounded-outcome transformations (log-odds and arcsine), which correct for the mechanical compression of gaps as literacy approaches its ceiling. These transformations still imply substantial convergence, but the estimated degree of closure is smaller: between 28.8% using log-odds and 68.4% using the arcsine transformation. The main conclusion is therefore not that the Ottoman literacy penalty simply vanished, but that mass schooling greatly reduced it while leaving some persistent differences once ceiling effects are taken into account.

Convergence was also spatially uneven. By 1991, the literacy penalty had nearly disappeared along the northern and western segments of the former frontier, where the relevant comparison areas were predominantly Orthodox- and Catholic-majority. In contrast, the gap remained visible along the central and southern segments, where Muslim-majority districts were more common. Female literacy shows the same pattern (Figure 6). The female gap narrowed sharply after 1931, consistent with the socialist state's campaign against illiteracy and its emphasis on female education. Yet this convergence slowed after the 1960s in Muslim-majority areas.

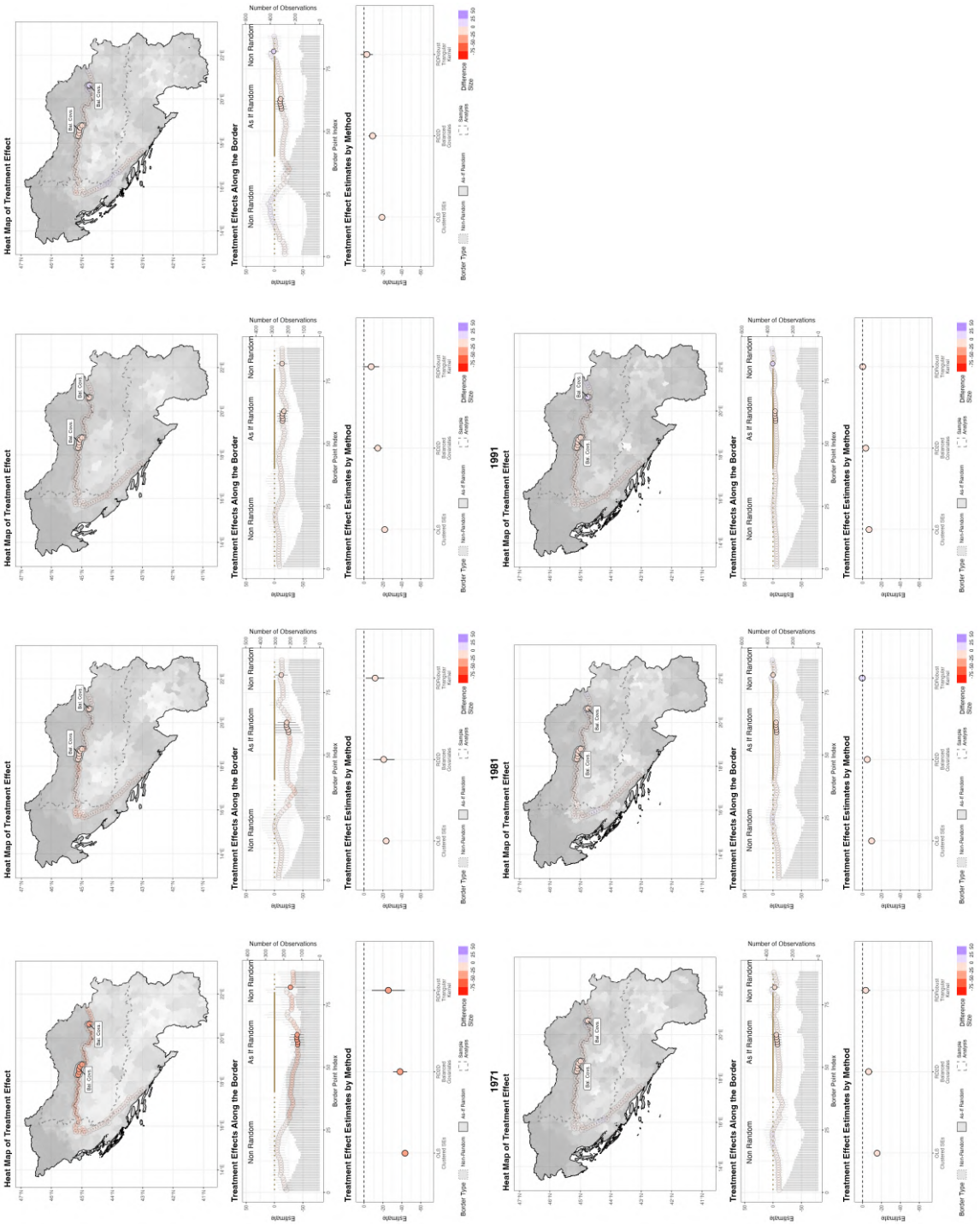



**Figure 5.** *Evolution of the Ottoman literacy penalty, 1931-1991: literacy maps, RD2D point estimates, and coefficient comparisons across seven census waves*

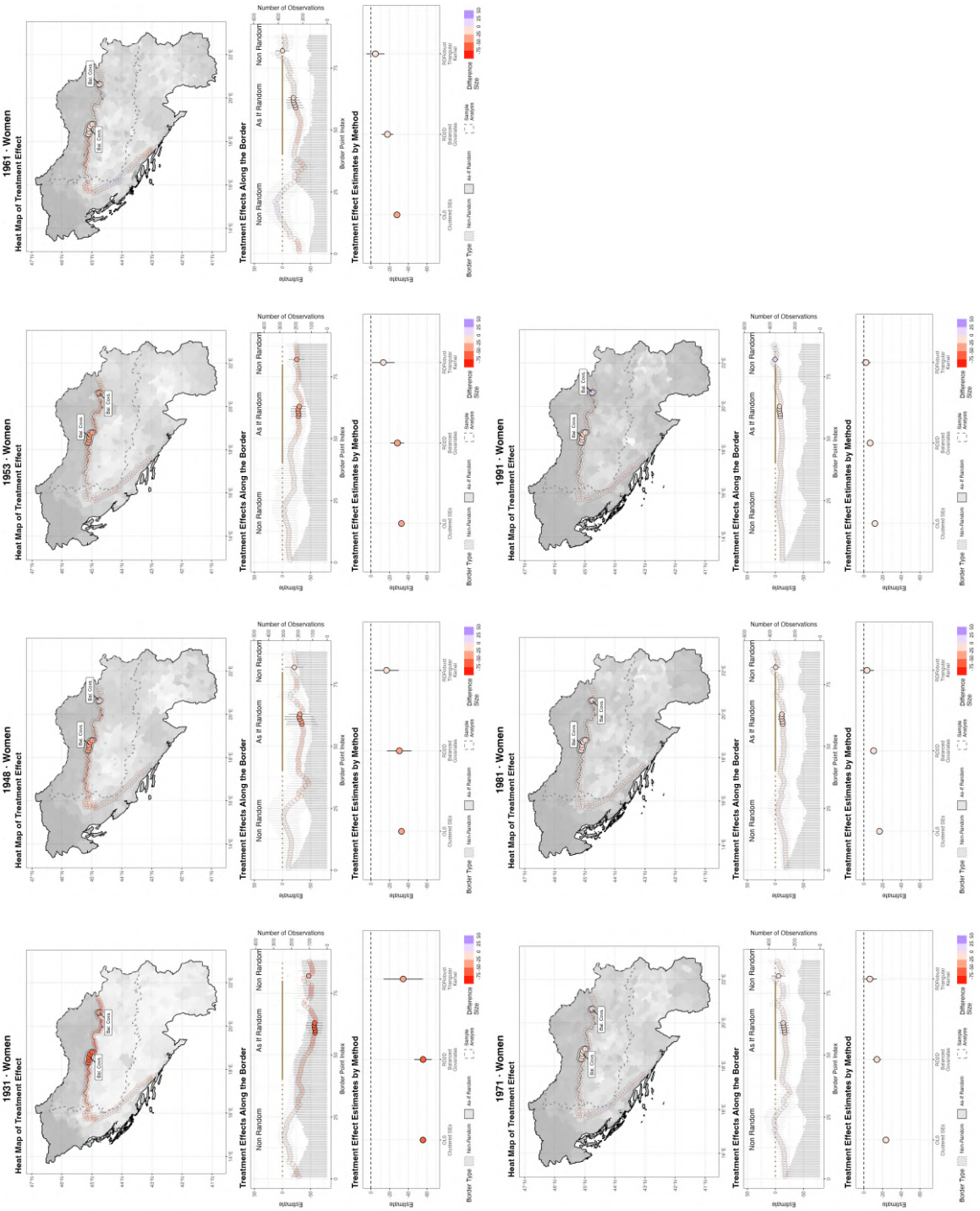



**Figure 6.** *Evolution of the Ottoman literacy penalty for women, 1931-1991: literacy maps, RD2D point estimates, and coefficient comparisons across seven census waves*

### 5.3 *Religious composition and differential convergence*

The Ottoman penalty faded where surrounding districts were Orthodox- or Catholic-majority and persisted where they were Muslim-majority (compare Figures 5-6 with the religious maps in Appendix Figures A.1–A.3), suggesting that convergence tracked religious composition. We test this directly by regressing district literacy on religious composition, and ask whether the lingering gap reflects a Muslim-specific penalty - *state-minority alignment.*

**Table 3.** *The Muslim-Share Literacy Gradient across Samples, 1931–1991*

| | 1931 | 1948 | 1953 | 1961 | 1971 | 1981 | 1991 |
|---|---|---|---|---|---|---|---|
| *Panel A: Ottoman side of the RD2D bandwidth (preferred)* | | | | | | | |
| Pct. Muslim | $-0.36^{***}$ | $-0.33^{***}$ | $-0.24^{***}$ | $-0.20^{***}$ | $-0.08^{***}$ | $-0.04^{**}$ | $-0.04^{**}$ |
| | (0.04) | (0.05) | (0.04) | (0.03) | (0.03) | (0.02) | (0.02) |
| $N$ | 104 | 128 | 109 | 201 | 156 | 159 | 159 |
| Mean dep. var. (% literate) | 36.6 | 66.0 | 64.6 | 70.2 | 77.7 | 85.6 | 90.5 |
| *Panel B: Full district sample* | | | | | | | |
| Pct. Muslim | $-0.75^{***}$ | $-0.54^{***}$ | $-0.46^{***}$ | $-0.38^{***}$ | $-0.23^{***}$ | $-0.12^{***}$ | $-0.08^{***}$ |
| | (0.04) | (0.03) | (0.03) | (0.02) | (0.02) | (0.01) | (0.01) |
| $N$ | 378 | 426 | 351 | 774 | 500 | 508 | 508 |
| Mean dep. var. (% literate) | 53.1 | 73.0 | 71.9 | 79.1 | 83.2 | 89.4 | 93.1 |
| *Panel C: Both sides of the RD2D bandwidth* | | | | | | | |
| Pct. Muslim | $-0.85^{***}$ | $-0.52^{***}$ | $-0.44^{***}$ | $-0.41^{***}$ | $-0.18^{***}$ | $-0.11^{***}$ | $-0.08^{***}$ |
| | (0.08) | (0.05) | (0.05) | (0.04) | (0.03) | (0.02) | (0.01) |
| $N$ | 193 | 206 | 172 | 357 | 239 | 243 | 243 |
| Mean dep. var. (% literate) | 56.2 | 74.2 | 72.3 | 78.2 | 82.2 | 88.5 | 92.4 |
| Std. errors | Conley (30 km) | | | | | | |

*Notes*: Dependent variable is district-level literacy rate (percentage). Each panel reports the coefficient on the district Muslim population share from a separate bivariate regression (no interactions), estimated on a different sample: Panel A the Ottoman-side districts inside the RD2D bandwidth (our preferred sample), Panel B all frontier districts, Panel C both sides of the RD2D bandwidth. The 1948, 1953, and 1971 columns are added from the post-war nationality censuses. The gradient is negative and declines toward zero in every sample; it is smallest on the Ottoman side, because the Habsburg side holds very few Muslims and including it inflates the raw gap. We therefore take the Ottoman-side estimate as the conservative benchmark. Standard errors (in parentheses) and the dependent-variable mean are reported for each panel because the sample differs. Conley (30 km) spatial HAC standard errors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 3 estimates it in three samples: the Ottoman side of the RD2D bandwidth, the full district sample, and both sides of the bandwidth. Our preferred sample is the Ottoman side alone. Because the Habsburg side held very few Muslims, pooling both sides mechanically widens the gap - so the Ottoman-side estimate, the smallest of the three, is also the most conservative, and it compares Muslim districts with the aligned Orthodox and Catholic populations they actually lived alongside.

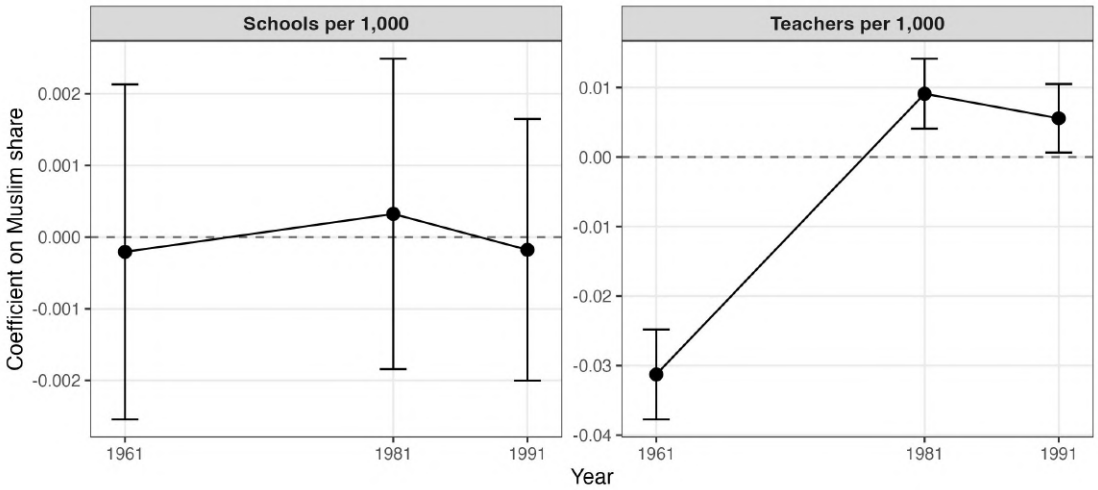
On the preferred Ottoman-side sample (Panel A), a 10 percentage-point higher

Muslim share is associated with 3.6 percentage points lower literacy in 1931. The deficit narrows sharply as mass schooling expands - to 2.0 points by 1961 and about 0.4 by 1991 - but stays statistically significant six decades on. Muslim areas converged, in other words, without fully catching up. The pattern is the same, and larger, in the full sample (Panel B) and the both-sides bandwidth (Panel C), where the literate Habsburg districts enter the comparison; the Ottoman-side figures are the conservative version of the same result.

### *5.4 School provision and the supply-side explanation*

The persistence of lower literacy in Muslim-share districts could reflect either weaker demand for schooling or lower provision of schools and teachers. We next examine the supply-side explanation directly. We collected district-level data on the number of primary schools and teachers between 1961 and 1991, the period for which comparable data are available. We then regress schools and teachers per 1,000 inhabitants on the Muslim share of the district population.

***Figure 7.*** *School and Teacher Provision and Muslim Population Share, 1961-1991 (RD2D sample)*




*Notes: OLS coefficients (with 95% confidence intervals) from regressions of schools per 1,000 and teachers per 1,000 inhabitants on district Muslim population share, estimated within the RD2D bandwidth for 1961, 1981, and 1991, with no additional controls. Conley (1999) spatial HAC standard errors (30 km cutoff).*

The results in Figure 7 provide little support for the view that Muslim-share districts remained less literate because they had fewer schools. Across the 1961, 1981, and 1991 censuses, the Muslim-share coefficient is small and statistically insignificant when the outcome is the number of primary schools per 1,000 inhabitants. School

availability therefore appears to have been broadly equalised across districts by the socialist period. This is consistent with the Yugoslav state's large-scale expansion of primary education after World War II.

Teacher availability also fails to explain the persistence of the literacy gap. In 1961, Muslim-share districts had significantly fewer teachers per capita: a 10 percentage-point higher Muslim share is associated with 0.3 fewer teachers per 1,000 inhabitants. This deficit is economically modest, and it disappears over time. By 1981, the coefficient changes sign: a 10 percentage-point higher Muslim share is associated with 0.1 more teachers per 1,000 inhabitants. By 1991, the coefficient remains positive but smaller, implying 0.05 more teachers per 1,000 inhabitants for a 10 percentage-point increase in Muslim share. These magnitudes are small, and the direction of the relationship is inconsistent with a persistent lack of teacher supply in Muslim-share districts.

By the late socialist period, then, Muslim-share districts had as many schools as other districts and, if anything, more teachers. Whatever kept literacy lower there, it was not a shortage of either. The gap must instead reflect demand: in the decades before provision caught up, fewer children were enrolled, and the adults who missed school then still appear as illiterate in later censuses - a legacy of past under-enrolment, not an ongoing shortfall. Nor is this weak demand a by-product of communism: the enrolment gap is already visible in 1932, under the interwar kingdom (Appendix O).

### *5.5 Sharia-court closure and educational demand*

By the late socialist period, school provision had equalised across the frontier, yet the literacy gap in Muslim-share districts held - and it was widest for women. With supply no longer the binding constraint, the persistence of the gap points to the demand side: how fully families took up the schooling on offer. Two explanations fit. Conservative gender norms may have made families slower to educate daughters, a pattern documented most fully for Muslim societies (Fish, 2002; Norris & Inglehart, 2003). Or the cause may lie in alignment: families invest in a state's schools when they see the state as their own, and the interwar Yugoslav state had strained that relationship with its Muslim population. The two need not compete, and norms are unlikely to be the whole story - other religiously conservative communities on the Ottoman side, the Orthodox, converged (Section 5.3), while Muslim ones did not. Whatever set the size of the initial gap, something else governed whether it later closed.

The 1929 sharia-court reform in Bosnia-Herzegovina offers a natural experiment

to test this mechanism. Instead of comparing Muslim-majority and non-Muslim-majority areas across the former imperial frontier - a comparison that tangles Muslim culture with Muslim alignment to the state - we exploit within-Bosnia variation in a Muslim-specific state institution, which moves alignment while holding religion and culture fixed: the closure of sharia courts in some districts but not others. Their removal therefore allows us to evaluate whether weakening this institutional link between the state and its Muslim population affected subsequent educational take-up.[12] The empirical design compares settlement-level educational outcomes across districts that were similar in their institutional setting but differed discontinuously in whether they retained a sharia court after 1929. We then examine whether the effects were stronger in areas with larger Muslim populations. This separates, as far as the data allow, the role of general religious composition from variation in state-minority institutional alignment.

Sharia courts in late-Ottoman and post-Ottoman Bosnia-Herzegovina were state-recognised tribunals administering Islamic personal-status law, including marriage, divorce, inheritance, and child custody (Karčić, 2005). They operated alongside the civil court system, typically as departments of local district courts. Every district in Bosnia-Herzegovina had a sharia department, and the Yugoslav state preserved this arrangement after 1918. This changed in 1929 under the dictatorship of King Aleksandar I (Djokić, 2007). The Law on the Organisation of Sharia Courts and on Sharia Judges (*Zakon o uređenju šerijatskih sudova i o šerijatskim sudijama*) introduced a sharp administrative cutoff rule: sharia courts would be maintained only in districts with at least 5,000 Muslims, measured using the 1921 census. Twelve of the fifty-one districts in Bosnia-Herzegovina fell below this threshold and lost their courts. Because the running variable was fixed by the 1921 census, eight years before the reform, manipulation around the cutoff is implausible. All remaining sharia courts were abolished by the communist regime in 1946, leaving a seventeen-year window of differential treatment (1929–1946).

We exploit this administrative cutoff using a regression discontinuity design. The unit of observation is the settlement (*naselje*). We locate each 1961 Bosnia-Herzegovina settlement by matching it to its 1885 Habsburg counterpart within its municipality and spatially joining it to its parent 1921 district (Appendix P),

[12] The court did more than settle questions of family law: its judges (*qadis*) lent state schooling a local Muslim endorsement it otherwise lacked. Karčić (2019, p. 113) records that the qadis presented themselves as "champions of cultural progress amongst Muslims," and that sending their own children to the new schools helped "break down the suspicions that existed amongst the Muslim masses against modern education." Withdrawing the local court removed that endorsement - the concrete link between a family-law institution and whether households, daughters above all, took up state schooling.

so the running variable is the parent district's 1921 Muslim count. Treatment is defined at the 1921 district level: settlements in districts with fewer than 5,000 Muslims are assigned to the lost-court group, while settlements in districts at or above the threshold are assigned to the retained-court group. Figure 8 maps the 1921 Muslim count across the fifty-one districts, with the twelve that fell below the cutoff outlined. The estimation uses a controlled local-linear RD, adjusts for the five pre-treatment covariates that are imbalanced at the cutoff (elevation, the 1910 Orthodox and Catholic shares, 1910 ethnic fractionalization, and an 1885 road indicator), and clusters standard errors at the municipality level, with robust bias-corrected inference (Calonico *et al.*, 2014). McCrary density tests show no evidence of bunching around the 5,000-Muslim threshold (Appendix Q).

A genuine effect of the closure should carry three marks. First, it should be *Muslim-conditional*: the 1929 rule works through a Muslim institution, so any *rise* in illiteracy should appear only where Muslims lived, not in the zero-Muslim settlements, which went through the same administrative reshuffle but had no Muslim community for the closure to reach. Second, it should be *gendered*: sons were sent to school in any case, but a daughter's enrolment was the discretionary decision - the one that hinged on whether the family trusted state institutions, and the courts the state had just closed were precisely those governing family life. The cost should therefore fall on women, not men. Third, it should be *cohort-timed*: it should appear in the cohorts of school age during or after the 1929-1946 window and not in those already grown.

Table 4 reports the results, pooled and by tercile of the 1910 Muslim share (T1 is the zero-share group; T2 and T3 split the strictly positive shares at their median). Pooled across all settlements, the closure has no detectable effect on 1961 illiteracy: the discontinuity in population illiteracy is small and insignificant, as is the effect on women. That is what the mechanism implies. The 1929 rule acts through a Muslim institution and can bite only where Muslims are present, so pooling dilutes it with the many zero-Muslim settlements the closure could never have reached; the informative variation is by Muslim share.

The effect emerges where Muslim communities were actually present, and it falls on women. In T2 - settlements with a positive but below-median Muslim share - losing the local court raised 1961 population illiteracy by about 9 percentage points, with the burden concentrated among women: female illiteracy rose by roughly 5.5 points (against a baseline near 28), while men's illiteracy is essentially unchanged.[13]

[13] The population, male, and female discontinuities are estimated as separate regressions, each on its own MSE-optimal bandwidth and sample, so the population estimate need not equal a share-weighted average of the male and female estimates.

In T3, where the Muslim share is highest, far fewer settlements sit near the cutoff and the estimate is imprecise, but it points the same way, with higher female illiteracy on the lost-court side. In T1 - the zero-Muslim settlements the closure could not reach - illiteracy does not *rise*. For women it falls, by about five points on the lost-court side, and significantly so. But this reinforces the T2 estimate rather than undermining it. The two groups move in opposite directions: illiteracy falls where no Muslims lived and rises where they did. A confound at the cutoff - some feature of the districts rather than the courts - would push both the same way; that they diverge instead points to an effect specific to Muslim communities. And whatever lowered illiteracy where no Muslims lived would, if it also reached Muslim settlements, shrink the measured rise rather than create it. So the true effect is, if anything, larger than we report.

Why the closure fell hardest on women follows from the household economics of schooling. In patriarchal household economies, families treat sons' education as the default investment - higher private returns through labour-market and bureaucratic access - and daughters' as discretionary (Becker, 1981); consistent with this, Muslim women's labour-force participation in 1953 was 20.8%, against 56.7% for Muslim men - a wider gender gap than among non-Muslims (32.4% versus 62.7%) (Dyker, 1972). Where a community trusted the state, compulsory schooling and its enforcement overrode this calculus and closed the gap for all children; where the closure had eroded that trust, daughters were withdrawn first. Historians and ethnographers record Bosnian Muslim families treating the state's schools as alien - an institution they did not see as their own, even a threat to their way of life, and least of all a place for their daughters (Bringa, 1995; Sorabji, 1989). A contemporary observer mocked how charged the question was: in Bosnia a fatwa was sought on whether a school for Muslim girls could be opened, while nobody asked whether bars serving alcoholic drinks could - although Islamic teaching makes schooling children of both sexes obligatory (*farz*) and alcohol strictly forbidden (*haram*) (Karčić, 2005, p. 128). It is exactly the fear a local qadi's endorsement had earlier helped calm. Because boys and girls in the same settlement faced the same schools, the gendered effect cannot be attributed to a settlement-level confounder such as poverty or remoteness.

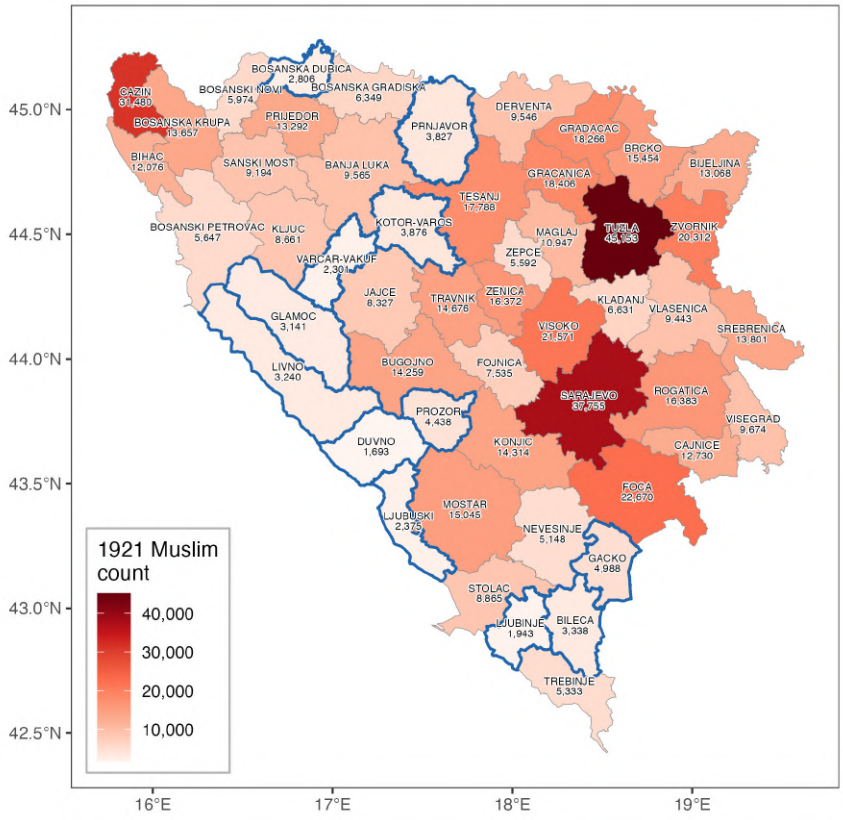
The same mechanism leaves a mark in time. If losing the court reduced schooling while it was gone, only the cohorts of school age in those years should carry the effect. Within T2, 1961 illiteracy is about 10 points higher on the lost-court side among the younger cohorts - those aged 10-19 and 20-34, schooled during and in the wake of the 1929-1946 closure - while the older cohorts, already schooled before 1929, show no significant difference. That the imprint appears in the cohorts the closure

could have reached, and not in those too old to have been touched by it, is what the design predicts, and it lets the older cohorts serve as an informal within-sample placebo.

Two caveats qualify the design. First, several pre-treatment covariates jump at the cutoff: lost-court districts sit at higher elevation and differ in confessional composition (Appendix Q), so the estimates lean on covariate adjustment rather than on raw comparability and are best read as suggestive local evidence. Second, the running variable is coarse - it takes only fifty-one district values, twelve below the cutoff - so the comparison rests on a handful of cutoff-adjacent districts; the T2 women's effect is robust to functional form and kernel and stable across bandwidths from the optimum upward (Appendix Q), though it weakens at the narrowest bandwidths and under a wild-cluster bootstrap at the district level. What survives these caveats is the three-part pattern set out earlier: the estimates are Muslim-conditional, gendered, and cohort-timed, all at the same threshold. A confound - district size, urbanisation, simple chance - could deliver any one of these marks. Only the sharia-court channel predicts all three at the same threshold; a culture-only account predicts none of them. With that in hand, the pattern supports the demand-side reading: the closure did not reduce the physical availability of schools; it removed a state-recognised institution that had connected Muslim personal-status law to the Yugoslav legal order, and where that institutional tie weakened, the expansion of mass schooling translated less fully into literacy.

The within-Bosnia experiment sharpens a comparison the paper has drawn throughout. Across most Muslim-majority societies the male-female literacy gap closed over the twentieth century - Figure 9 shows Albania, Jordan, and Malaysia approaching parity, Turkey at 0.93, and Indonesia, Iran, and Egypt between 0.86 and 0.95 by their last observation - while Yugoslavia's Muslim districts, though they narrowed the gap from 0.36 in 1931 to 0.84 in 1991, stalled earlier than almost every comparator, ending between Morocco and Egypt and well below Albania, Turkey, or Jordan (Cooray & Potrafke, 2011; Norton & Tomal, 2009; Roudi-Fahimi & Moghadam, 2003). Islam as such does not hold literacy down; most Muslim societies converge. What distinguished these regions was minority status and institutional misalignment - exactly the variable the sharia-court closure isolates within Bosnia's Muslim population, holding religion, language, and culture fixed.

***Figure 8.*** *Corrected 1921 Muslim Count per District (the Running Variable)*



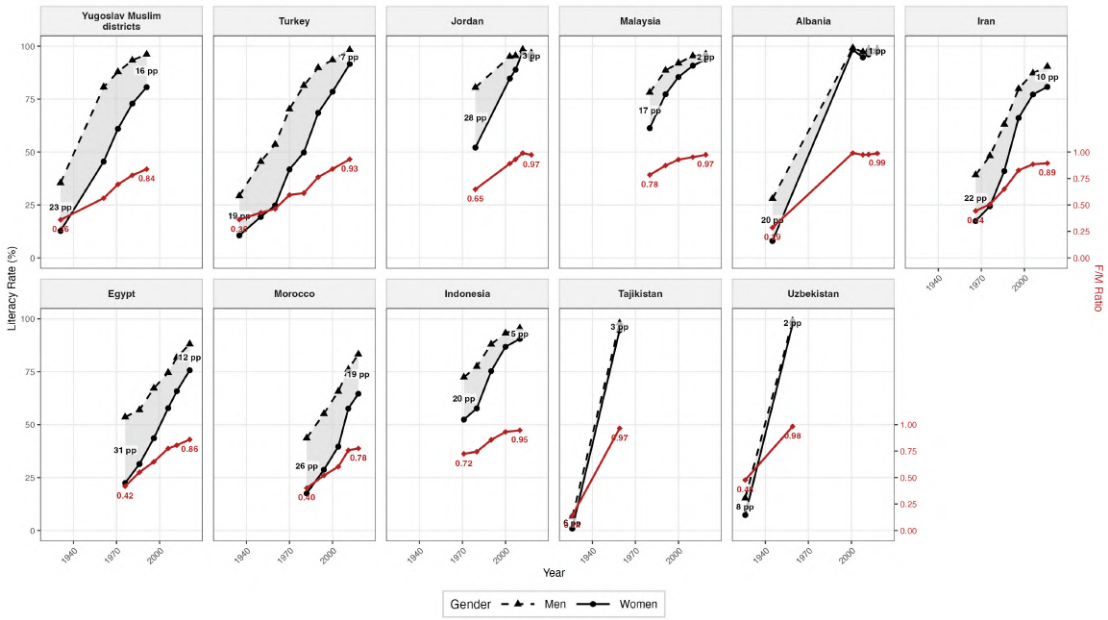

*Notes: Bosnia-Herzegovina districts (srezovi) shaded by their 1921 Muslim count, the running variable behind the 1929 assignment. The twelve districts below the 5,000-Muslim cutoff - which lost their sharia court under the ZSS - are outlined in blue; each district is labelled with its name and count. The cutoff slices the districts into a lost-court and a retained-court group that are otherwise institutionally, demographically, and geographically continuous.*

***Table 4.*** *Tercile RD Estimates of the 1929 Sharia-Court Closure on 1961 Illiteracy*

| | Illiterates per 100 inhabitants (pos. $\tau$ = harm) | | | Illiterates by age band, per 100 (pos. $\tau$ = harm) | | | |
|---|---|---|---|---|---|---|---|
| | Population | Men | Women | 10–19 | 20–34 | 35–64 | 65+ |
| *Pooled (all retained-court + lost-court naselja)* | | | | | | | |
| Estimate | 3.32 | $-2.29^{***}$ | 2.13 | $-6.04$ | $-2.24^{***}$ | 4.97 | 2.31 |
| 95% CI | $[-3.9, 2.2]$ | $[-4.7, -1.1]$ | $[-1.9, 2.6]$ | $[-19.3, 3.9]$ | $[-10.0, -2.0]$ | $[-2.6, 6.8]$ | $[-4.5, 6.0]$ |
| $p$ (robust) | 0.585 | 0.001 | 0.748 | 0.192 | 0.003 | 0.382 | 0.785 |
| Bandwidth | 2325 | 1719 | 2454 | 1736 | 1818 | 2453 | 2496 |
| Effective $N$ | 703 | 557 | 717 | 460 | 597 | 712 | 686 |
| DV mean | 39.6 | 9.9 | 29.6 | 15.4 | 31.7 | 64.3 | 77.5 |
| *T1 (1910 Muslim share* $= 0$*)* | | | | | | | |
| Estimate | $-0.66$ | 2.20 | $-5.11^{**}$ | 6.67 | $-1.66$ | $-4.27$ | $-0.71$ |
| 95% CI | $[-11.4, 3.2]$ | $[-2.4, 3.2]$ | $[-11.8, -0.5]$ | $[-1.8, 5.2]$ | $[-13.6, 2.0]$ | $[-15.0, 14.4]$ | $[-8.5, 10.8]$ |
| $p$ (robust) | 0.266 | 0.782 | 0.034 | 0.345 | 0.147 | 0.971 | 0.815 |
| Bandwidth | 2078 | 2644 | 1651 | 2103 | 1946 | 1371 | 1752 |
| Effective $N$ | 364 | 446 | 269 | 288 | 337 | 265 | 299 |
| DV mean | 37.5 | 8.9 | 28.5 | 14.0 | 27.9 | 61.5 | 75.9 |
| *T2 (1910 Muslim share* $\in (0, 0.64]$*)* | | | | | | | |
| Estimate | $8.96^{***}$ | 2.94 | $5.54^{***}$ | $9.44^{*}$ | $11.20^{***}$ | 6.15 | 2.17 |
| 95% CI | $[2.9, 12.1]$ | $[-1.6, 1.5]$ | $[2.5, 8.9]$ | $[-0.2, 10.0]$ | $[4.1, 10.2]$ | $[-7.7, 6.9]$ | $[-8.2, 7.2]$ |
| $p$ (robust) | 0.002 | 0.949 | 0.000 | 0.062 | 0.000 | 0.917 | 0.894 |
| Bandwidth | 2811 | 2274 | 2702 | 2572 | 2601 | 2544 | 2919 |
| Effective $N$ | 272 | 209 | 274 | 241 | 257 | 261 | 261 |
| DV mean | 37.2 | 9.1 | 28.1 | 13.5 | 29.2 | 61.6 | 76.1 |
| *T3 (1910 Muslim share* $> 0.64$*)* | | | | | | | |
| Estimate | 0.17 | $-0.48$ | 2.29 | $-16.08$ | 4.48 | 5.14 | 3.32 |
| 95% CI | $[-11.7, 4.9]$ | $[-7.5, 3.0]$ | $[-2.7, 3.6]$ | $[-52.0, 10.9]$ | $[-10.9, 11.9]$ | $[-4.0, 9.8]$ | $[-10.3, 12.9]$ |
| $p$ (robust) | 0.425 | 0.407 | 0.774 | 0.201 | 0.933 | 0.404 | 0.822 |
| Bandwidth | 2843 | 3000 | 3001 | 2354 | 4238 | 3070 | 3870 |
| Effective $N$ | 135 | 135 | 136 | 91 | 193 | 135 | 173 |
| DV mean | 44.2 | 11.9 | 32.3 | 18.9 | 38.5 | 70.2 | 80.6 |

*Notes:* Each cell is a separate controlled local-linear RD (triangular kernel, MSE-optimal bandwidth, *opština*-clustered robust bias-corrected inference; (Calonico *et al.*, 2014)), pooled and by 1910 Muslim-share tercile. Running variable: the parent district's 1921 Muslim count; cutoff: 5,000 Muslims. Because 37% of naselja have a zero 1910 Muslim share, T1 is the zero-share group and T2/T3 split the strictly positive shares at their median. The five imbalanced pre-treatment covariates (Appendix Q) enter as linear controls; the stratifying 1910 Muslim share is excluded. Estimate = (lost − retained), so harm is a positive $\tau$; the point estimate is local-linear while the 95% CI (in brackets) and $p$ are robust bias-corrected, so the interval is centred on the bias-corrected estimate and need not contain the point. Effective $N$ is the sample inside the MSE-optimal bandwidth; the DV mean row is the baseline illiteracy on each tercile's naselja (reported per tercile because the sample changes). $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

***Figure 9.*** *Male and Female Adult Literacy in Muslim-Majority Populations, 1926-2021*




*Notes: Dashed lines/triangles: men. Solid lines/circles: women. Grey ribbon: male-minus-female gap (pp). Red line/diamonds (right axis): female-to-male literacy ratio. Sources: Yugoslav census panel (this paper); TÜİK and World Bank WDI for Turkey; Mickiewicz (1973) (1926 and 1959 Soviet census literacy) for Soviet Central Asia; World Bank WDI for other countries. The near-universal late-Soviet rates should not be taken at face value - literacy was self-reported, not tested, and standards were laxer in Central Asia than near Moscow (Liebowitz, 1986); the relevant evidence is the pattern of convergence, not the endpoint levels.*

# 6 Conclusion

This paper asks when historical legacies fade. Using the former Habsburg-Ottoman frontier in Yugoslavia, we trace a large human-capital discontinuity over seven censuses from 1931 to 1991. In 1931, literacy was more than 40 percentage points lower on the former Ottoman side of the frontier, with even larger differences among women. Over the following six decades, this gap narrowed dramatically under common state institutions and the expansion of mass schooling. Historical differences were therefore highly persistent, but not permanent.

Convergence, however, was uneven. Literacy gaps closed much more slowly in Muslim-populated districts, even after differences in schools and teachers had largely disappeared. A separate regression discontinuity design based on the 1929 closure of sharia courts in Bosnia provides complementary evidence that weakening a state-recognised institution linking Muslim communities to the successor state reduced educational take-up, particularly among women and cohorts of school age during the treatment period. Taken together, the results point to state-minority institutional alignment as an important condition shaping how fully common public institutions translate into convergence.

The broader implication is that historical persistence should not be viewed as a fixed property of past institutions. Successor states can substantially erode inherited disparities, but equalising formal institutions and public provision may not be sufficient. Whether historical gaps disappear also depends on how communities relate to the institutions intended to integrate them. Understanding when legacies fade therefore requires studying not only the persistence of the past, but also the political and institutional conditions under which societies move beyond it.

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# Appendices

## A Ottoman Rule – Historical Maps

To measure the duration of Ottoman rule, we developed a Geographic Information Systems (GIS) dataset illustrating the evolving borders of the Ottoman Empire from its inception in 1326 under Osman I to its dissolution in 1922. We compiled this dataset by scanning and digitising 152 maps from repositories including the Ottoman Archives and the University of Chicago Map Collection. Some are individual maps; others come from historical atlases. They range from empire-wide depictions to detailed maps of Ottoman control in individual countries, and are drawn from sources in English, German, French, Serbo-Croatian, Hungarian, Italian, Turkish, Ottoman Turkish, and Latin. We georeferenced and compared different maps against each other, which is why this data is more accurate than other measures capturing the spread of empires at the turn of centuries (e.g., Nüssli (n.d.)). This approach is also much more fine-grained than country averages proposed by Dimitrova-Grajzl (2007) or the region averages employed by Grosjean (2011) based on Nüssli (n.d.), which only reveal border changes at the turn of the century.

The historical maps that were used cover the following years:

- 14th Century: 1326, 1331, 1352, 1354, 1355, 1360, 1361, 1362, 1363, 1367, 1371, 1372, 1377, 1381, 1382, 1383, 1385, 1386, 1387, 1389, 1391, 1392, 1393, 1394, 1395, 1396, 1397, 1398
- 15th Century: 1402, 1415, 1416, 1417, 1428, 1430, 1439, 1442, 1443, 1444, 1448, 1450, 1453, 1455, 1456, 1458, 1459, 1460, 1461, 1462, 1463, 1466, 1469, 1470, 1471, 1475, 1476, 1478, 1479, 1480, 1481, 1482, 1485, 1499
- 16th Century: 1500, 1503, 1512, 1520, 1521, 1522, 1526, 1528, 1534, 1537, 1538, 1540, 1541, 1544, 1550, 1552, 1557, 1566, 1570, 1577, 1579, 1582, 1591, 1593
- 17th Century: 1606, 1617, 1646, 1648, 1664, 1669, 1672, 1683, 1684, 1685, 1686, 1687, 1688, 1689, 1690, 1691, 1692, 1693, 1695, 1697, 1699
- 18th Century: 1715, 1718, 1721, 1723, 1732, 1733, 1734, 1736, 1739, 1772, 1774, 1775, 1779, 1783, 1791, 1792, 1793, 1795, 1797
- 19th Century: 1800, 1803, 1805, 1807, 1809, 1810, 1814, 1820, 1829, 1830, 1834, 1838, 1855, 1856, 1860, 1863, 1864, 1878, 1880, 1881, 1882, 1885, 1889, 1895
- 20th Century: 1908, 1912, 1913, 1918, 1920.

Sources: American Geographical Society of New York (191-), Balta (1989, 1992); Barnes (2015); Bayerle (1973); Bérenger (1990); Birken (1979, 1981), Great Britain.

War Office. General Staff. Geographical Section. (1918); Cvijić (1918); Davies (1879), Delilbaşı & Arıkan (2001); Domanovszky (1940); David & Fodor (2000); Dávid (1999), Gopčević (1889), Halasi-Kun (1964), İnalcık *et al.* (2013), Johnston (1879, 189-), Káldy-Nagy & Mikesy (1960); Káldy-Nagy (1961); Káldy-Nagy (1971); Kiel (1993a,b, 1989), Kiepert (185-, 1869, 1876, 1880, 1881, 1895), İnalcık (1954); Fekete (1943); Szántai (1996); Langhans (1910); Laut (1991), Levasseur (1864); Liechtenstern & Amon (1795), Ljušić & Rajić (2003); Ljušić *et al.* (2006); Magocsi (2002), Unknown Author (1890), Pitcher (1972); Regan & Kaniški (2003), Andree (1912), Sarınay *et al.* (2001, 2003, 2006, 2007, 2009a,b); Spaho & Aličić (2007); Sršan & Hafizović (2001); Ştefănescu *et al.* (1996a,b); Stojaković (1987), United States Government (1908); Ward *et al.* (1912).

## B Literacy Rates in Yugoslavia, 1931–1991

*1931*

Literacy in Yugoslavia was first reported in the census of 1931 (Kraljevina Jugoslavija Opšta Državna Statistika, 1938b). People aged 11 and over who could read and write were considered literate. District-level data distinguished literates and literacy rates by sex and age group.

*1948 and 1953*

Post-WWII Yugoslav censuses recorded literacy. People aged 10 and over who could read and write were considered literate. The 1948 census (Savezni Zavod za Statistiku i Evidenciju, 1955b) reported the number of illiterates and illiteracy rates, whereas the 1953 census (Savezni Zavod za Statistiku, 1960) reported the number of literates and literacy rates. Both censuses distinguished district-level data by sex and age group.

*1961*

The 1961 census (Savezni Zavod za Statistiku, 1968) reported, for each district, the number of illiterates and corresponding population by sex and age group. It is therefore straightforward to calculate literacy rates.

The 1961 Yugoslav census also reports (Savezni Zavod Za Statistiku, 1965), for each settlement (*naselje*), the overall illiteracy rate and the *composition* of illiterates by sex, that is, the percentage of all illiterates who are male and the percentage who are female. These composition shares sum to approximately 100% but do not directly give gender-specific illiteracy *rates*, because the latter depend on the sex distribution of the population. To convert composition shares into rates, we use the identity:

$$\text{illiteracy}_{g,i} = \frac{s_{g,i} \times \text{illiteracy}_{\text{overall}}}{w_g},$$

where $s_g$ is the share of illiterates in settlement $i$ who belong to gender $g$ and $w_g$ is that gender's share of the adult population. We set $w_m = 0.48$ and $w_f = 0.52$, consistent with the 1961 Yugoslav census aggregate (51.2% female). This procedure is applied uniformly to all settlements and involves no imputation; it is an exact algebraic decomposition conditional on the population sex ratio.

*1971*

The 1971 census (Savezni Zavod za Statistiku, 1974) reported the number of illiterates and corresponding population, by sex and age group, at the district level. We calculated literacy rates using these data.

*1981 and 1991*

For Serbia and Kosovo in 1981 and 1991, we used female and male literacy rates from Republički Zavod Za Statistiku (1981, 1991). For Slovenia in 1981 and 1991, we used data from Statistični urad Republike Slovenije. [Statistical Office of the Republic of Slovenia] (1981, 1991). Finally, for Bosnia, we used female and male literacy figures from Federalni zavod za statistiku Federacije Bosne i Hercegovine (1998), which cover both 1981 and 1991.

For Croatia, Macedonia, and Montenegro, we imputed male and female literacy rates using district-level data and national benchmarks, as detailed below. For Croatia the district totals themselves are observed, and only the split by sex is imputed. For Macedonia and Montenegro in 1991 no district-level returns were available to us, so the district totals are also estimated; the procedure is set out in full at the end of this appendix, under *Estimating District Totals for Macedonia and Montenegro in 1991.*

**Croatia (1981 and 1991)**: We estimated sex-specific literacy rates from district-level data in Savezni Zavod Za Statistiku (1987) for 1981 and from the 1991 Croatian census for 1991. Because district tables did not report male and female populations separately, we assumed a national adult sex distribution of roughly 48% men and 52% women, based on aggregate 1981 census figures. Using national literacy rates (97.5% for men and 91.6% for women in 1981, and 98.8% and 95.2% in 1991 — both taken from Table 5-6 of (Croatian Bureau of Statistics, 2005), which reports the 1971, 1981, 1991 and 2001 censuses side by side), we computed the corresponding female-to-male illiteracy ratios. Each district's overall illiteracy was then decomposed into male and female components using these ratios and the assumed sex shares, from which literacy for each sex was derived as the complement. Finally, we applied proportional shrink-to-mean rescaling, adjusting district values by a constant factor so that their average exactly matches the national mean, before mapping the resulting male and female literacy rates.

**Macedonia (1981 and 1991)**: We estimated sex-specific literacy rates from district-level data in Savezni Zavod Za Statistiku (1987); for 1991 no district-level returns were available for this republic, and the district totals are estimated by the procedure set out at the end of this appendix. Because district tables did not report male and female populations separately, we assumed a national adult sex distribution of roughly equal shares between women and men. For 1981, we used the female-to-male illiteracy ratio from the 1961 census as a proxy for the national gender gap, applying it to decompose each district's overall illiteracy into male and female components, which we then converted to literacy rates. For 1991, since an official national literacy mean was unavailable, we interpolated a national target between the 1981 (89.1%) and 1994 (94.06%) benchmarks (Macrotrends, 2024), yielding targets of 97% for men and 91% for women, and repeated the same decomposition procedure. Finally, we applied proportional shrink-to-mean rescaling, adjusting district values by a constant factor so that their averages exactly matched the national means, before producing the male and female literacy and illiteracy estimates for mapping

and analysis.

**Montenegro (1981 and 1991)**: We estimated sex-specific literacy rates from district-level data in Savezni Zavod Za Statistiku (1987); for 1991 no district-level returns were available for this republic, and the district totals are estimated by the procedure set out at the end of this appendix. Because district tables did not include sex-specific counts, we used national female-to-male illiteracy ratios together with assumed adult sex shares to decompose each district's overall illiteracy into male and female components, which we then converted to literacy rates. For 1981, the adjustment was calibrated to match national literacy levels of 96.32% for men and 83.24% for women (CountryEconomy.com, 1981), while for 1991 we used targets of 96.97% for men and 89.89% for women from the same source. In both years, we used proportional shrink-to-mean rescaling, adjusting district values by a constant factor so that their averages matched the national means, before reporting sex-specific literacy and the complementary illiteracy rates.

*Estimating District Totals for Macedonia and Montenegro in 1991*

The procedures above take each district's overall illiteracy as observed and divide it between men and women. For Macedonia and Montenegro in 1991 that input is not observed. The federal yearbooks report national literacy for both republics, but we were unable to obtain municipality-level returns for either, and the 1981 volume (Savezni Zavod Za Statistiku, 1987) is the last source giving district figures for them. Rather than drop the two republics from the 1991 maps, we estimate their district totals from their own 1981 values and report the procedure here in full.

**Estimation sample**. The model is fitted on the districts for which 1991 district-level illiteracy is directly observed: Bosnia (Federalni zavod za statistiku Federacije Bosne i Hercegovine, 1998), Serbia and Kosovo (Republički Zavod Za Statistiku, 1991), Slovenia (Statistični urad Republike Slovenije. [Statistical Office of the Republic of Slovenia], 1991), and Croatia, whose 1991 returns come from the Croatian census of that year. These supply 454 districts in the assembled data.

**Specification.** Writing $i_d^{91}$ for the overall illiteracy rate of district $d$ in 1991, $i_d^{81}$ for its 1981 rate, $m_d^{81}$ for its 1981 Muslim share, and $c(d)$ for the republic containing it, we fit by ordinary least squares

$$i_d^{91} \;=\; \alpha \;+\; \beta\, i_d^{81} \;+\; \gamma\, m_d^{81} \;+\; \delta_{c(d)} \;+\; \varepsilon_d,$$

and two companion equations of the same form with female and male illiteracy on the left-hand side. The 1981 Muslim share and the republic effects enter only as predictors of the 1981–1991 change; we attach no causal interpretation to either coefficient, and the Muslim-share coefficient is in any case small and imprecisely estimated.

**Prediction.** For each Macedonian and Montenegrin district we evaluate the fitted overall equation at that district's own 1981 illiteracy and Muslim share, holding the republic effect at the Bosnian level. A stochastic disturbance drawn from $N(0, \hat{\sigma}^2)$,

with $\hat{\sigma}$ the residual standard deviation of the fitted equation and a fixed random seed, is added so that the predicted series retains district-level dispersion rather than lying exactly on the regression surface. The result is then truncated to the interval $[0.1,\ i_d^{81}]$, imposing that illiteracy is positive and did not rise between 1981 and 1991.

**Division by sex.** The two companion equations are used only for their ratio. Let $r_d$ denote the predicted female-to-male illiteracy ratio for district $d$. Male and female rates are then recovered from the predicted total by

$$i_{m,d} \;=\; \frac{i_d^{91}}{w_f r_d + w_m}, \qquad i_{f,d} \;=\; r_d\, i_{m,d},$$

with $w_m = 0.48$ and $w_f = 0.52$ as elsewhere in this appendix, which guarantees that the two sexes aggregate to the predicted total. We note that in assembling the analysis panel this district-specific ratio is subsequently replaced by a single national ratio per republic, so the male and female series we use carry no district-level variation in the gender gap beyond that already present in the total.

**Status of these figures.** They are model-based estimates, not census counts. Both republics lie well away from the 1739 frontier, but the 1991 frontier estimates are computed on the full set of districts rather than on a restricted frame, and Macedonian and Montenegrin units fall inside the local bandwidth at some boundary points. Quantities computed on the full 1991 sample, including the national convergence figures reported in the text, are therefore not independent of these estimates. We report the procedure in full for that reason, and treat any 1991 quantity that depends on the two republics as provisional pending recovery of the published returns.

# C Religious Composition of Yugoslavia

*Data*

The 1931 Yugoslav census (Kraljevina Jugoslavija Opšta Državna Statistika, 1938a) reports district-level population by religion. We calculate the percentage share of Muslims, Orthodox Christians, and Catholics in each district.

Post-WWII censuses in Socialist Yugoslavia reported ethnic nationality (*narodnost*) rather than religion. The censuses clearly distinguished Serb, Croat, Slovene, Macedonian, Montenegrin, and other minority nationalities (e.g. Albanian, Hungarian) and ethnicities (e.g. Roma, Vlach). The category for Muslims who did not identify with a particular nationality, for example, Albanian, somewhat changed across censuses. They were categorized as "Muslims who didn't opt" (*neopredeljeni muslimani*) in 1948 and as "Yugoslavs who didn't opt" (*Jugosloveni neopredeljeni*) in 1953. The 1961 census introduced the category "Muslims in the sense of ethnic affiliation" (*Muslimani u smislu etničke pripadnosti*) and the 1971 census renamed it "Muslims in the sense of ethnic nationality" (*Muslimani u smislu narodnosti*). In the 1981 and 1991 census, they were simply presented under "Muslims".

Post-WWII, we calculate the share of Muslims by including population identified as Muslim or Albanian. For the share of Orthodox Christians, we include Serbs, Montenegrins, and Macedonians. We obtain the share of Catholics including Croats, Slovenes, and Hungarians. The underlying nationality counts come from the corresponding federal census volumes — (Savezni Zavod za Statistiku i Evidenciju, 1955a) for 1948, (Savezni Zavod za Statistiku, 1959) for 1953, (Savezni Zavod za Statistiku, 1967) for 1961, (Savezni Zavod za Statistiku, 1974) for 1971, (Savezni Zavod Za Statistiku, 1987) for 1981, and the 1991 federal nationality census (Savezni Zavod za Statistiku, 1992) — supplemented for Croatia by the settlement-level nationality returns of (Republika Hrvatska, Republički Zavod Za Statistiku, 1992). The religious fractionalisation index is a Herfindahl measure — one minus the sum of the squared group shares — with the shares renormalised to sum to one over these three groups (together about 95% of the population); the residual "other" category, which the censuses record inconsistently, is excluded so the index is comparable over time. One caveat applies to 1948: in the Sandžak, the 1948 census in Serbia recorded most Bosniak Muslims as Serbs rather than under the "undeclared Muslim" category (which was used almost exclusively in Bosnia), so the 1948 Muslim share is understated — and the Orthodox share correspondingly overstated — in that region. The published totals confirm this is genuine rather than a transcription error: Serbia proper reported only 6,586 undeclared Muslims in 1948, against 808,921 nationally, almost all in Bosnia. This affects only the 1948 map panels of Figures A.1 and A.3; the RD2D estimation sample lies outside the Sandžak, so the regression tables and the over-time estimates are unaffected.

*Maps*

Figure A.1 maps the geographic distribution of Muslims in Yugoslavia across seven census waves between 1931 and 1991 (1931, 1948, 1953, 1961, 1971, 1981, and 1991).[14] Muslims predominantly lived in Bosnia and Herzegovina, Sandžak, and parts of Kosovo and Macedonia.[15] This likely reflects a direct Ottoman effect stemming from voluntary conversions to Islam or indirect pressures to convert, driven by the benefits derived from lower taxation and access to administrative roles in the Ottoman bureaucracy (Barkey, 2008; Faroqhi, 2005; İnalcık, 1973). The maps reveal a clear geographical delineation, where the Habsburg regions have minimal Muslim populations compared to the Ottoman regions. The location of Muslim population remained largely stable across the period.

Figure A.2 maps the distribution of Catholics in Yugoslavia across the same seven census waves. Catholics are primarily concentrated in the northwest regions of Yugoslavia, particularly Croatia and Slovenia, territories formerly under Habsburg rule (Fine, 1987; Lampe, 1996). The highest densities (81-100%) are in northern Croatia, Istria, and Slovenia. Regions under Ottoman influence show almost no Catholic presence (<10%).

Figure A.3 maps the distribution of Orthodox Christians across the same seven census waves. Orthodox Christians dominate the eastern and southern regions of Yugoslavia, primarily Serbia, Montenegro, and Macedonia, as well as parts of Bosnia and Herzegovina. These also coincide to a great extent with the Ottoman territories (Sugar, 1977). Orthodox Christianity was likely preserved due to the resilience of local institutions such as the Serbian Orthodox Church, which helped maintain religious identity (Jelavich, 1983; Roudometof, 2001).

[14] The 1931 census records religious affiliation directly; for the post-war waves religious composition is reconstructed from census nationality — Muslim = Muslimani + Albanians, Catholic = Croats + Slovenes + Hungarians, Orthodox = Serbs + Montenegrins + Macedonians — with the available national categories varying slightly by census year (construction details in Appendix L).

[15] The 1948 map panel understates the Muslim share in the Sandžak, and Figure A.3 correspondingly overstates the Orthodox share there: the 1948 census in Serbia recorded most of the region's Bosniak Muslims as Serbs rather than as "undeclared Muslims", so they do not appear as Muslim in that year (see Appendix C). The 1953 and later waves are unaffected, as is the frontier estimation sample, which lies outside the Sandžak.

***Figure A.1.*** *Muslims in Yugoslavia 1931-1991*

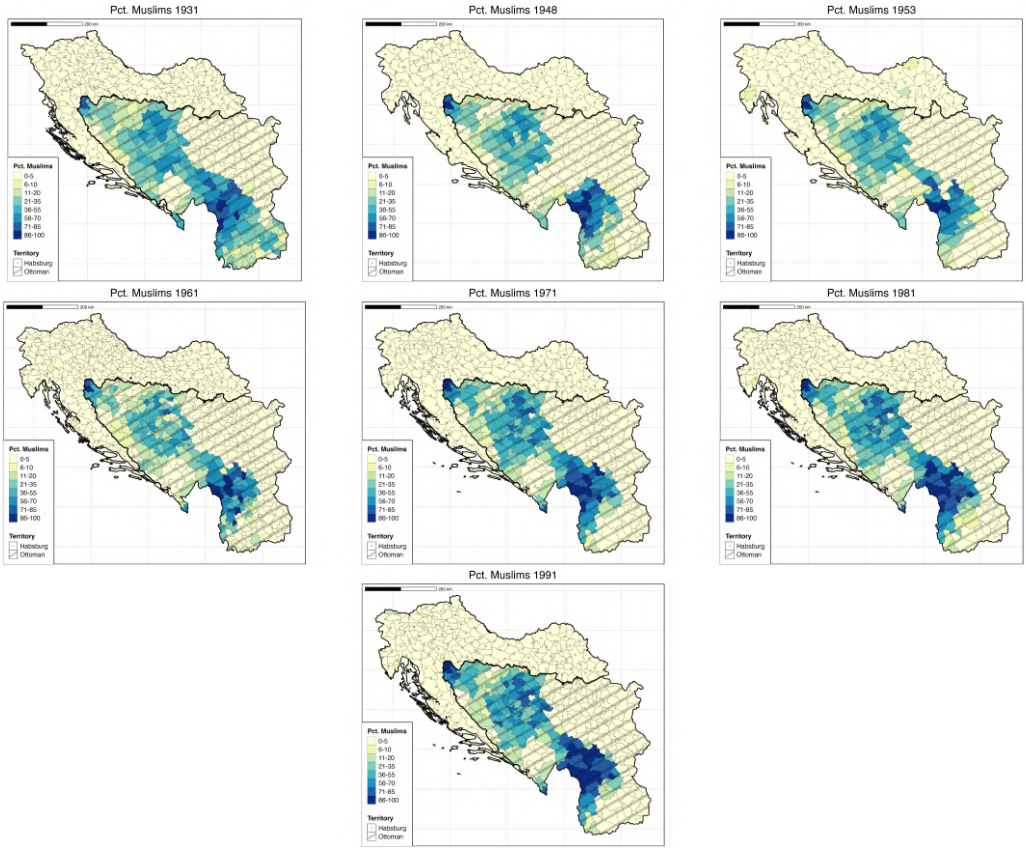


***Figure A.2.*** *Catholics in Yugoslavia 1931-1991*

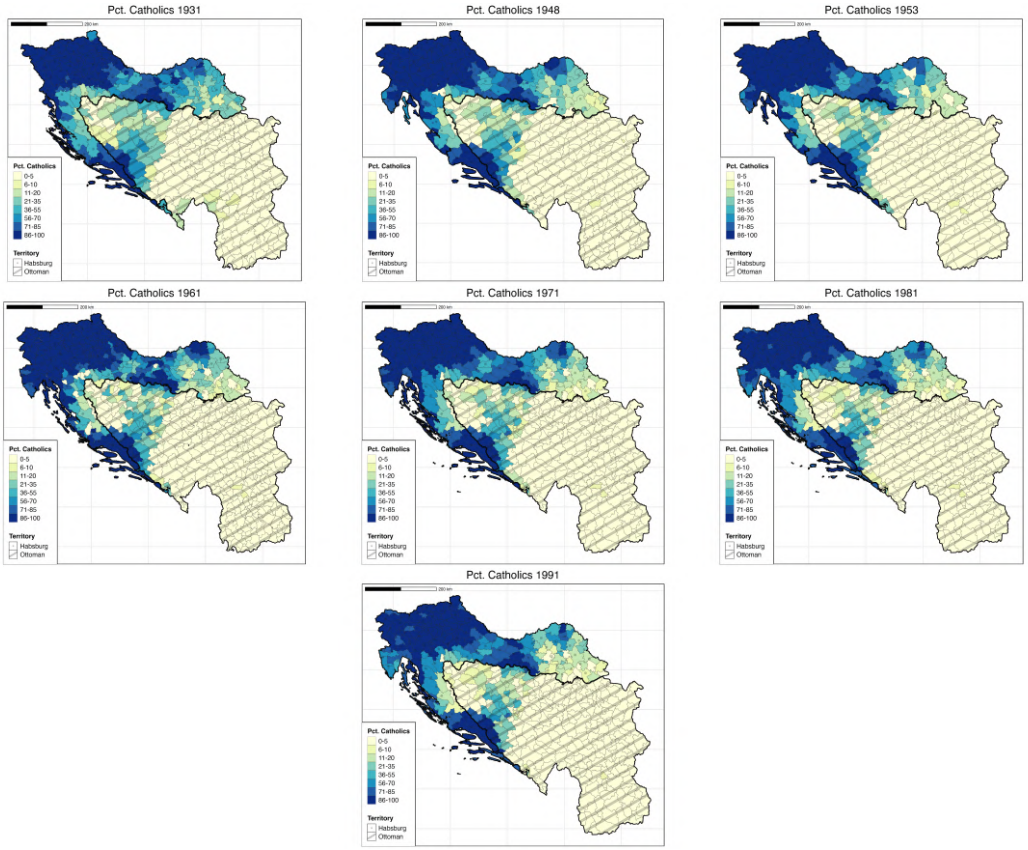
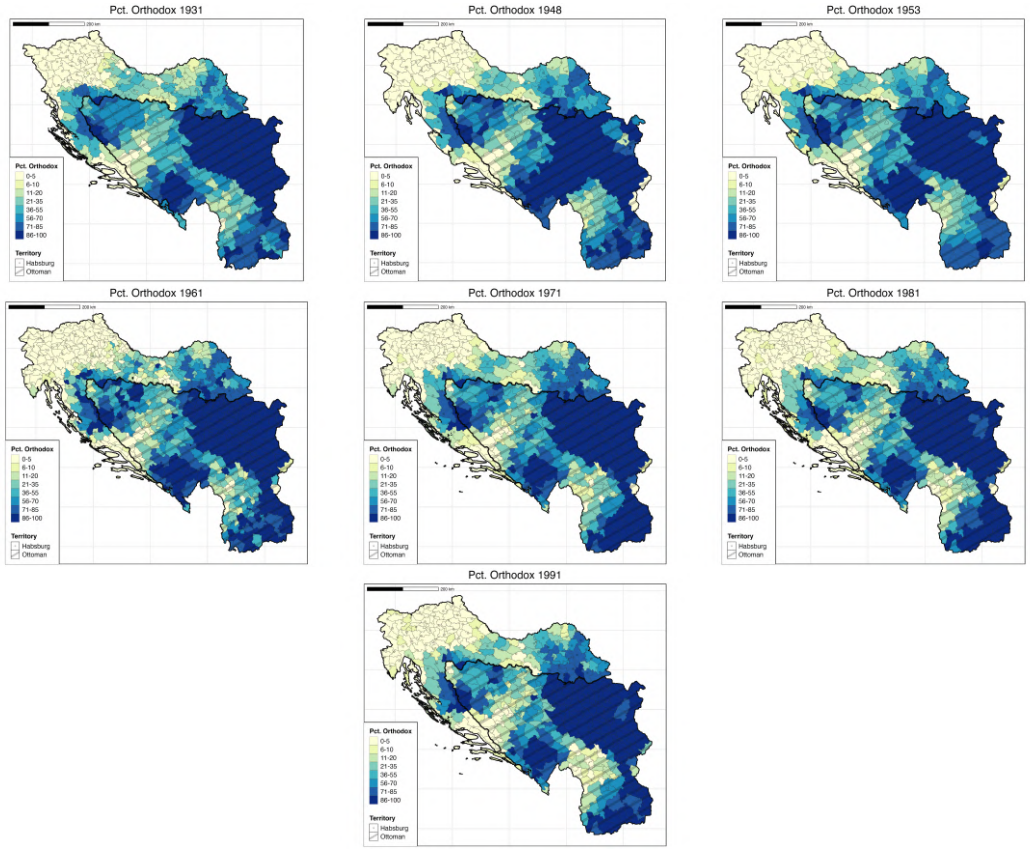


***Figure A.3.*** *Orthodox Christians in Yugoslavia 1931-1991*



# D Drawing of the Border

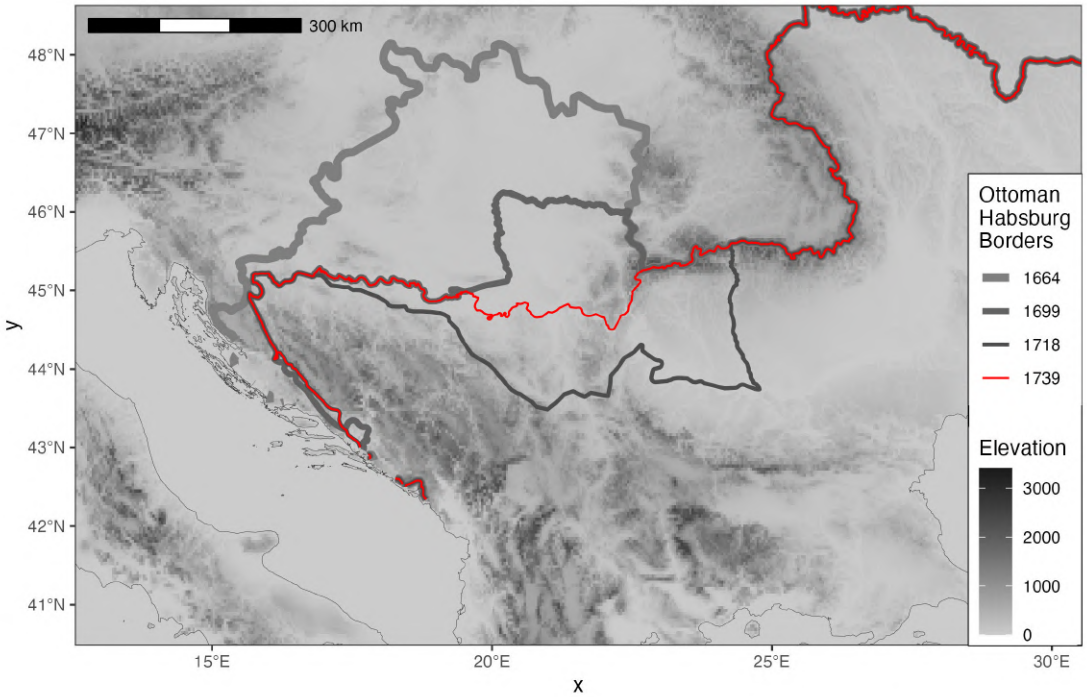
A central assumption of any RD design is the “as-if-random" assumption. As explained by Sekhon & Tiutiunik (2017, p. 13), this means that all units have the same probability of receiving treatment or control. Recent work suggests that borders are never random (Paine *et al.*, 2025) and that “qualitative historical knowledge is essential for validating natural experiments" (Kocher & Monteiro, 2016, p. 952). This does not mean that geographic borders can no longer be used on GRD designs. For example, (Paine *et al.*, 2025, p. 17) explain that for “cases in which a particular feature (e.g. river) was chosen over another, without much justification, a claim of conditional randomness may be justified." They also explain that making such an assessment “requires consulting the history and features of the borders" (ibidem). In the next subsection, we present a summary of the historical research we conducted about the Habsburg-Ottoman border.

## *D.1 The Habsburg-Ottoman Border(s)*

One important question for our empirical strategy is how to deal with the shifting nature of the Habsburg-Ottoman border over time. Throughout the early modern period, various treaties (e.g., Karlowitz 1699, Passarowitz 1718, Belgrade 1739) redefined the frontier, with each demarcation persisting for different durations and carrying varying administrative consequences. To account for these temporal shifts, we focus on a subset of historically significant border years (1664, 1699, 1718, and 1739) that capture turning points in Ottoman territorial control.

The 1648 Treaty of Westphalia is widely regarded as the origin of the modern state system based on fixed, sovereign borders (Abramson & Carter, 2016). The 1664 border marks the height of Ottoman territorial presence following the Treaty of Vasvár, whereas the 1699 Treaty of Karlowitz represents a decisive Ottoman retreat after the failed Siege of Vienna. The 1718 Treaty of Passarowitz reflects the peak of Habsburg gains into former Ottoman lands, and the 1739 Treaty of Belgrade marks a partial Ottoman recovery. All of these border configurations are displayed in Figure A.4. This raises the important question of which border is most appropriate to use in a regression discontinuity design.

***Figure A.4.*** *Four Historical Border Configurations*




*Notes: Elevation raster overlaid with the Habsburg-Ottoman frontier at four historically significant dates: 1664 (Treaty of Vasvár), 1699 (Treaty of Karlowitz), 1718 (Treaty of Passarowitz), and 1739 (Treaty of Belgrade). The 1739 border, used as the RD cutoff, is the most stable and produces the largest discontinuity in Ottoman exposure (see Figure A.5).*

### *D.2 Justifying the 1739 Border for Regression Discontinuity Design*

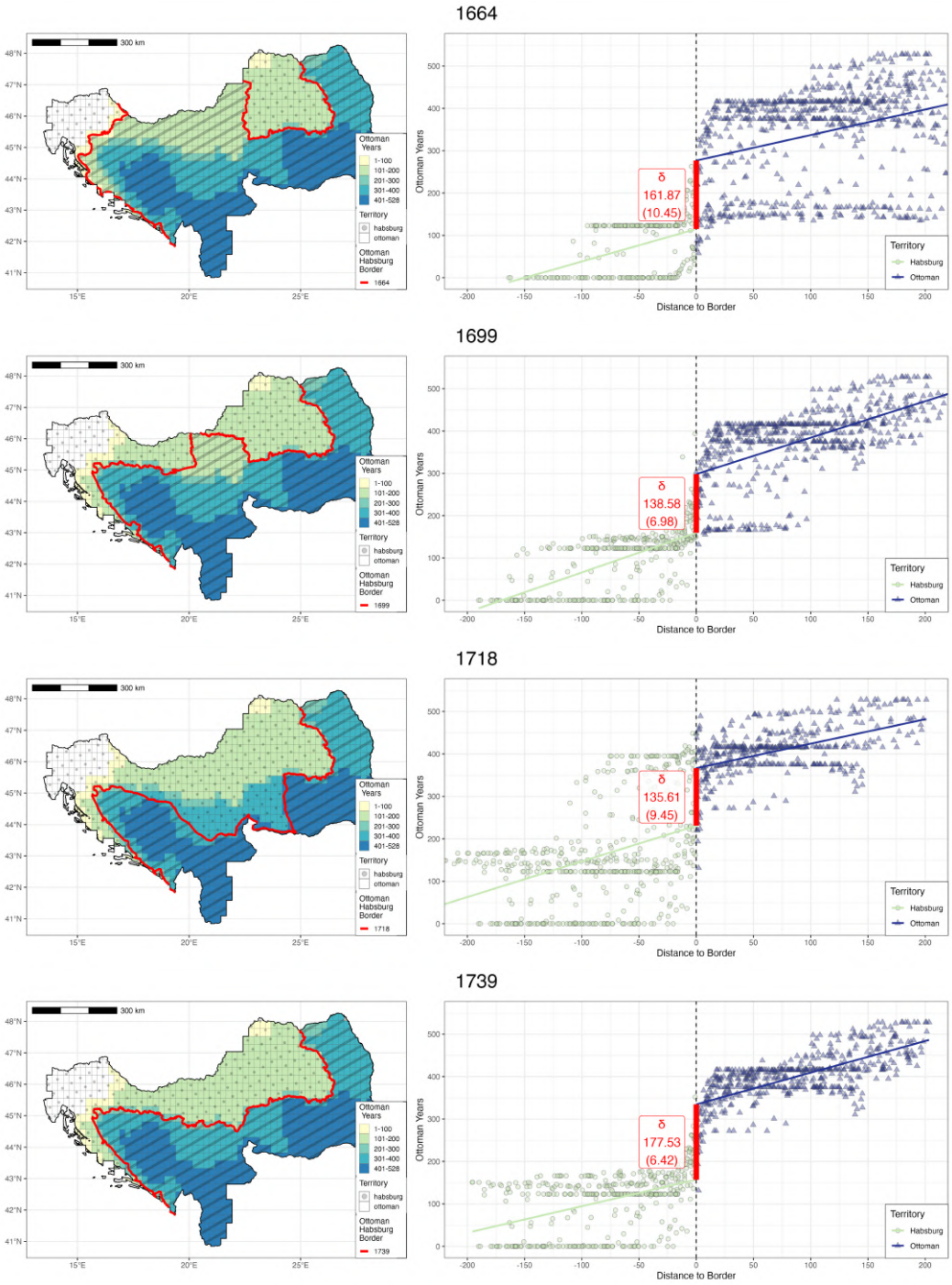
To justify the choice of the 1739 Habsburg-Ottoman border as the cutoff for our regression discontinuity design, we conduct a series of linear regressions for the former Yugoslav regions. As a first step, we dissolve the shapefiles for Serbia, Croatia, Bosnia and Herzegovina, Montenegro, Kosovo, and Slovenia into a single unified polygon. We then overlay this area with a regular grid of approximately 28 km (north-south) by 20 km (east-west) cells. For each grid cell, we calculate the duration of Ottoman rule using the historical shapefiles described in the previous section.

The border configurations examined correspond to the years 1664, 1699, 1718, and 1739. These dates are historically significant, each marking a major geopolitical reorganisation between the two empires. The 1664 border followed the Austro-Turkish War and the Treaty of Vasvár, representing the height of Ottoman territorial reach in Central Europe. The 1699 Treaty of Karlowitz marked the first major territorial retreat of the Ottoman Empire following the failed Siege of Vienna. The 1718 Treaty of Passarowitz reflected the peak of Habsburg expansion into former Ottoman lands, incorporating northern Serbia and parts of Wallachia. The 1739 Treaty of Belgrade then re-established Ottoman control over much of Serbia, producing a relatively stable frontier that endured for several decades.

The results, presented in Figure A.5, indicate that the 1739 border yields the largest estimated discontinuity in Ottoman exposure (177.53 years). The reported jump, or $\delta$, represents the predicted difference in years under Ottoman rule between locations immediately adjacent to either side of the border. Specifically, we estimate two separate linear regressions of Ottoman exposure on distance to the border, one for units on the Ottoman side and one for those on the Habsburg side, and compute the predicted values at the border point (distance = 0). The jump is calculated as the difference between these predicted values, and its standard error is derived from the squared sum of the standard errors of the respective predictions.

Earlier borders, such as those from 1664, 1699, and 1718, also produce substantial differences but explain less of the spatial variation in treatment intensity. These results support our claim that the 1739 border marks the most meaningful and enduring institutional divide and therefore provides the most appropriate setting for RD estimation. This conclusion is consistent with historical interpretations of the Treaty of Belgrade as establishing a long-standing and administratively significant frontier for much of the eighteenth century.

***Figure A.5.*** *Discontinuity in Ottoman Exposure by Border Year*



Beyond identifying which border produces the largest discontinuities in exposure, it is equally important to understand how the line of demarcation itself was decided. The negotiations and boundary-making processes surrounding the Treaty of Belgrade (1739) shed light on whether the resulting frontier can plausibly be regarded as exogenous. This matters for evaluating a core assumption of geographic regression discontinuity (GRD) designs: the as-if random assignment of treatment. Whether this assumption holds determines whether GRDs can be interpreted as genuine natural experiments, in which "a natural or social process has assigned units to distinct conditions in a way that is as good as random" (Kocher & Monteiro, 2016, p. 952).

The credibility of GRD results hinges on the plausibility of the claim that the placement of the Habsburg-Ottoman border was effectively random, and that the outcomes we seek to explain are not driven by unobserved imbalances between the territories on either side of the border. If the as-if randomization assumption does not hold, it becomes impossible to "distinguish the effects of causes from the effects of the conditions under which they operate" (Kocher & Monteiro, 2016, p. 952).

For example, Kocher & Monteiro (2016) challenge the validity of the as-if random assumption in Ferwerda & Miller (2014), who estimate the causal effect of delegating authority to the Vichy government on violent resistance during WWII. Kocher and Monteiro argue that the placement of the Line of Demarcation was not random but strategically chosen by Nazi Germany to maintain control over vital railway lines. As a result, the occupied zone contained a higher density of double-track railways, making it a more likely target for sabotage. This undermines the claim that differences in resistance levels near the line reflect the causal effect of indirect rule.

Similarly, Paine *et al.* (2025) question the as-if random assumption in the context of African borders. They argue that the placement of borders during the Scramble for Africa was shaped by precolonial political organisation and geographic features such as rivers and lakes. In other words, the location of borders was heavily influenced by preexisting conditions on the ground, rather than being arbitrarily drawn.

These critiques may appear to cast doubt on the continued viability of GRD designs. However, both Kocher & Monteiro (2016) and Paine *et al.* (2025) ultimately defend the use of GRDs, arguing that, when supported by detailed historical analysis and grounded in strong "qualitative historical knowledge" - GRDs can still yield meaningful causal insights. Paine *et al.* (2025) go further, arguing that in cases where borders were drawn along geographic features such as rivers, "a claim of conditional randomness may be justified." They emphasise, though, that "making such an assessment requires consulting the history and features of the border" (Paine *et al.*, 2025, p. 17).

When consulting the historical record, Kocher & Monteiro (2016, p. 953) recommend conducting idiographic research,[16] which involves tracing the specific causal

[16] This is similar to what Dunning (2012, pp. 212-218) calls "treatment-assignment causal process observations", or what Collier (2011) refers to more broadly as a causal process observation.

processes by which units were assigned to treatment and control groups.[17] Three factors should guide this description of the assignment process: (1) the information available to decision-makers at the time, (2) the incentives they faced, and (3) their institutional or material capacity to influence assignment. Thus we follow these questions in explaining how the Habsburg-Ottoman border was drawn under the 1699 Karlowitz and 1739 Belgrade treaties.

**(1) What information was available to the negotiators?**
Before the late seventeenth century, European frontiers were typically porous zones rather than clear lines of demarcation. What matters for the GRD is that the Habsburg-Ottoman boundary became a clearly delineated frontier after the Treaty of Karlowitz (1699), and in durable form after the Treaty of Belgrade (1739). Roughly 70% of the 1739 line coincided with the 1699 demarcation.[18] Joint commissions of engineers and cartographers conducted on-the-ground surveys to acquire detailed information about the physical, demographic, and economic features of the frontier. They documented the course of rivers, marshes, and ridgelines to identify defensible and stable natural boundaries, while also recording the locations of forts, villages, and trade routes. Surveyors noted soil quality, road conditions, and access to water. Rivers or mountain ridges became the anchor for the demarcation line (Fodor, 2009; Kurtaran, 2018; Gümüşçü, 2010). As Stoye writes, "in war, or if war threatened, the river became a military frontier adjoining a communication system for the forces" (Stoye, 1994, p. 83). Since mass literacy emerged only later, there is little reason to think negotiators had information about education outcomes when fixing the boundary.

**(2) What incentives (military, political, or economic) shaped their choices?**
The commissions' information-gathering was guided by pragmatic military, political, and economic considerations, consistent with the principle of *uti possidetis*, whereby each side sought to retain control over territories it effectively occupied at the close of hostilities (Ingrao, 2000; Rothenberg, 1966; Shaw & Shaw, 1977; Stoye, 1994). Militarily, both empires prioritized defensible terrain (rivers, ridgelines, and marshes) that could serve as natural fortifications and reduce the need for costly garrisons. Politically, they aimed to establish a clearly demarcated and mutually recognized border to minimize local skirmishes and stabilize diplomatic relations. Economically, surveyors paid particular attention to trade corridors along the Danube, Sava, and Drava rivers, ensuring that critical commercial routes and customs revenues remained under their respective control. The resulting boundary reflected both the military balance on the ground and a shared interest in a stable framework for defense and commerce.

[17] Both Kocher & Monteiro (2016, p. 953) and Dunning (2012, pp. 212-218) also emphasise the importance of explaining (ii) how the treatment was administered, and (iii) how the causes connect with the observed outcomes.

[18] On the shift from jurisdictional to territorial sovereignty, see Febvre (1962). On Karlowitz, Belgrade, and the survey commissions, see Fodor (2009); Ingrao (2000); Rothenberg (1966); Shaw & Shaw (1977).

Yet, the emphasis on defensibility and economic value was not applied uniformly along the frontier. While many rivers were chosen as convenient lines of demarcation, not all held equal strategic or commercial importance. As Stoye observes, the Una was "too narrow a stream, with too many fords" (Stoye, 1994, p. 91), while the "boundary along the Sava and its Slavonian hinterland interested him less. [...] A world of flood, forest and marsh faced hillier but otherwise similar country across the river. In Slavonia there were no fortresses, but no need for them." (Stoye, 1994, p. 186). These marginal stretches, defined more by geographic convenience than by deliberate military or economic logic, constitute the most credible quasi-experimental settings for assessing imperial legacies. In these areas, geography rather than politics determined exposure to empire, generating precisely the kind of local variation that permits causal inference about the long-term developmental imprint of Ottoman versus Habsburg rule.

**(3) Did negotiators have the capacity to shift the line toward or away from particular communities?**

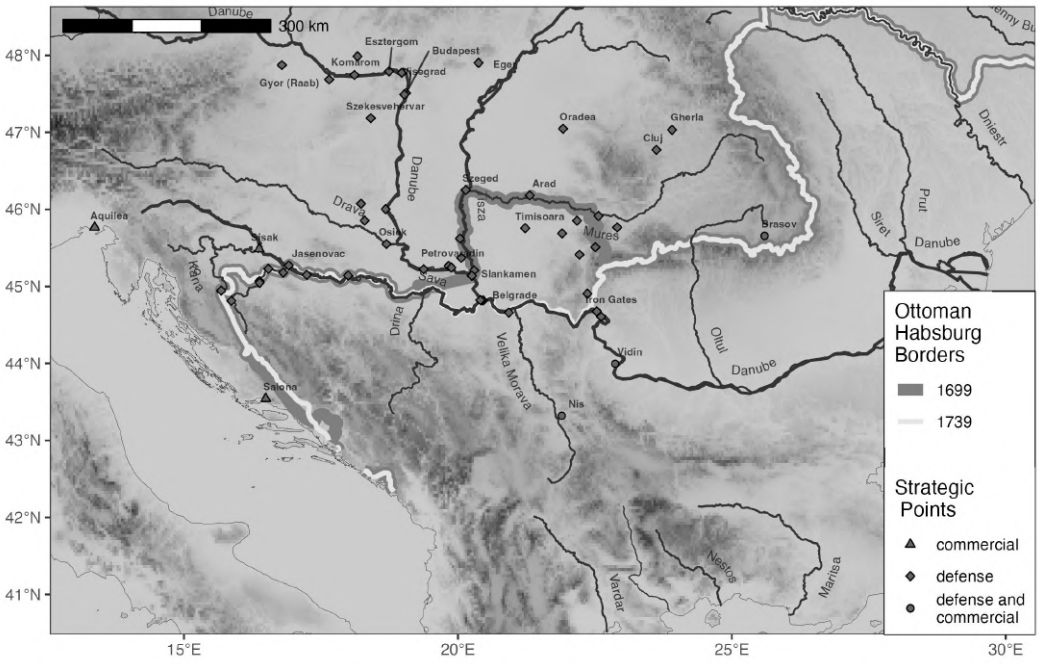
Negotiators possessed some room to maneuver, but this capacity was constrained by coalition politics, logistics, and the need for enforceable borders. Deliberations often balanced symbolic parity with technical bargaining, and commissions implemented agreements by reconciling maps with terrain (Fodor, 2009; Stoye, 1994; Kurtaran, 2018). Where fortresses or trade routes were at stake, the line could be adjusted; elsewhere it followed natural features to avoid future tensions.

These strategic, economic, and diplomatic considerations structured both the logic of negotiation and the concrete proposals for where the frontier should run. The map in Figure A.6 illustrates how these considerations translated into territorial arrangements, showing the key locations and rivers cited in historical sources alongside the final borders which overlap to a great extent decided at the Treaties of Karlowitz (1699) and Belgrade (1739).

Guided by these three criteria, we pre-specify primary GRD estimates on the Una and Sava segments, surveyed, militarily marginal, and exhibiting smooth geographic covariates across the cutoff, while reporting adjacent segments as robustness checks. This strategy privileges historical plausibility over blanket frontier coverage, which strengthens the credibility of the RD design.

A natural concern is whether populations on either side of the river already differed before imperial rule, which would threaten the continuity assumption even if the border itself was drawn for geographic convenience. Three considerations mitigate this. First, the Una-Sava frontier zone was heavily depopulated during the Long Turkish War (1593-1606) and subsequent conflicts; both empires resettled the corridor under administrative direction-the Habsburgs through the Vojna Krajina with Serb and Vlach soldier-farmers (Rothenberg, 1960, 1966; Kaser, 1997), the Ottomans through the *serhat*, their military borderland, settled with garrison communities. The populations on both sides were thus largely constituted by imperial settlement policy rather than inherited from pre-existing communities. Even where resettlement introduced different groups on either side, migration and settlement are spatially

***Figure A.6.*** *Strategic Points under Karlowitz Treaty*




*Notes: Key fortifications, commercial centres, and rivers cited in primary sources on the Treaties of Karlowitz (1699) and Belgrade (1739). Shapes distinguish commercial points (triangles), defensive points (diamonds), and dual-purpose sites (circles). The overlapping borders illustrate how negotiated adjustments concentrated on strategically salient locations while leaving marginal river stretches (Una, Sava) largely undisturbed.*

continuous processes unlikely to generate the sharp discontinuity at the boundary required to violate the continuity assumption. Second, pre-modern rivers in the region functioned as trade arteries and crossing points, not as barriers separating distinct populations; Stoye's observation that the Una had "too many fords" (Stoye, 1994, p. 91) underscores the permeability of these watercourses. Third, the RDD identification assumption is local, not global: it requires that potential outcomes be continuous at the cutoff, not that populations be identical everywhere. Our balance tests on five geographic covariates and McCrary density tests at each evaluation point directly assess this condition. The Sava segments satisfy both and form the core of the design; the Una is balanced on covariates and historically credible but shows some density bunching, so we read it with more caution.

# E Covariate Balance

The key identifying assumption for a GRD is that relevant factors besides treatment vary smoothly at the boundary (Keele & Titiunik, 2015, p. 130). This assumption is needed for observations located just across the Habsburg side of the boundary to be an appropriate counterfactual for observations across the Ottoman Empire. However, unlike traditional GRDs, which estimate global effects, the RD2D methodology allows the identification of locations that are most credible for evaluating the impact of Ottoman rule on literacy.

Figures A.7 and A.8 report the RD2D covariate-balance estimates at each boundary evaluation point for five geographic covariates: elevation, slope, precipitation, temperature, and wheat suitability. The results reveal a clear spatial pattern. In the western and mountainous segments near the Croatian coast, the Ottoman side is systematically higher in elevation, wetter, and cooler, with sizable and often statistically significant differences. Moving northward along the Sava River corridor, however, these imbalances shrink toward zero across all five covariates. Slope and temperature show imbalances along some segments but are well balanced at the credible Sava and Una evaluation points, and wheat suitability differences are negligible in the northern stretches.

The segments where covariate balance is strongest - the Una and Sava River stretches - are precisely those identified on historical grounds as the most credible quasi-experimental settings (Section D.3). The RD2D estimates reported in the main text are anchored primarily to the Sava segments.

**FIGURE A.7.** *Border Segment Covariate Balance*

*Notes: Each panel reports the RD2D point estimate of the Habsburg-Ottoman difference in a geographic covariate at each boundary evaluation point, ordered along the frontier. Estimates near zero indicate local balance. Error bars are uniform confidence bands. Solid segments denote low-stakes (as-if-random) sections; dotted segments denote high-stakes sections.*

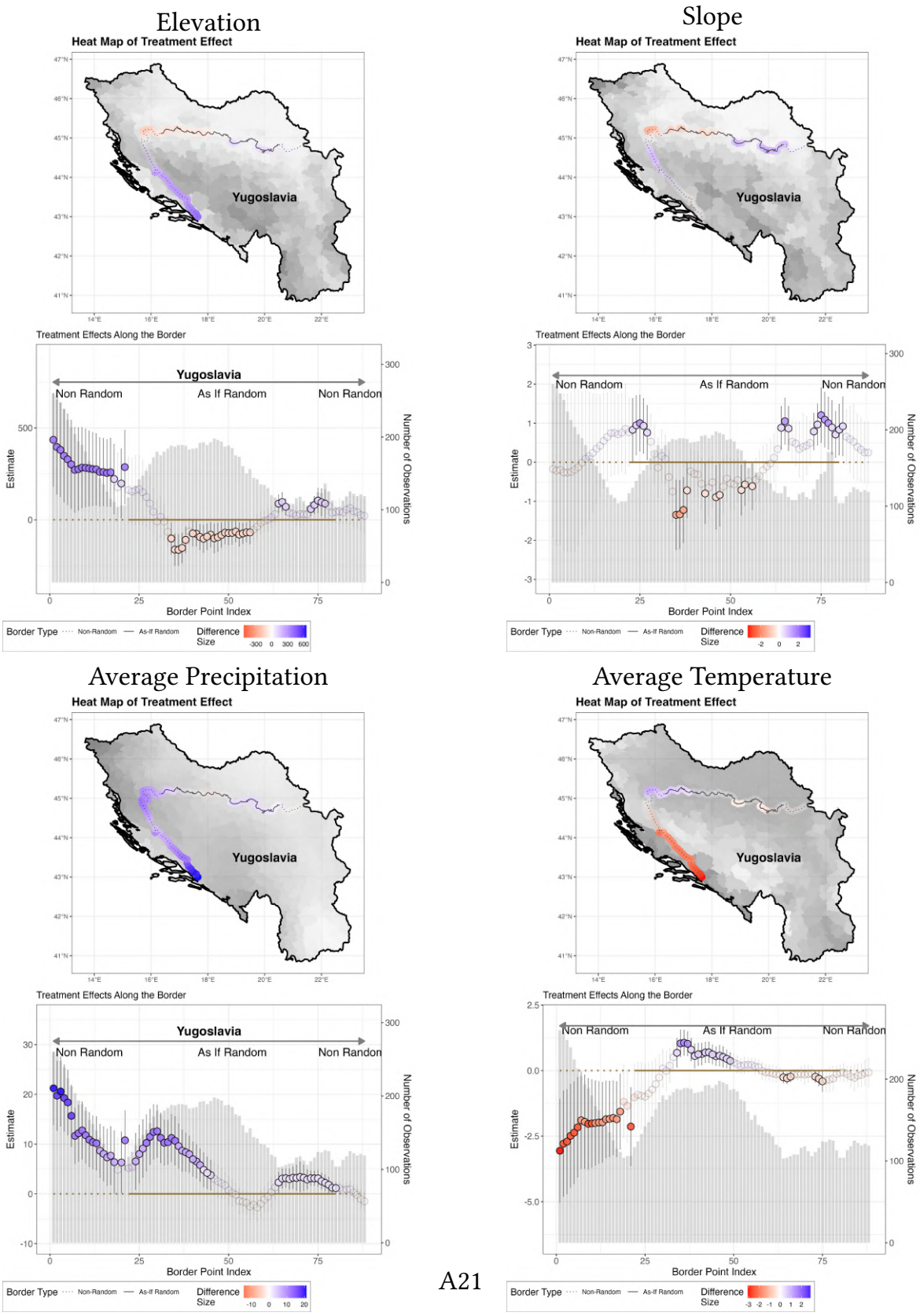
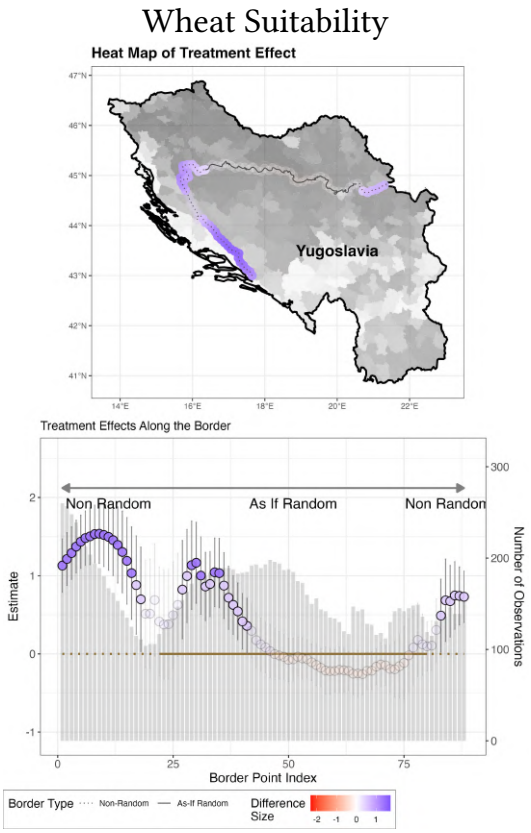


***Figure A.8.*** *Border Segment Covariate Balance (continued)*




*Notes: See Figure A.7. Wheat suitability from FAO global raster data on rainfed cultivation potential.*

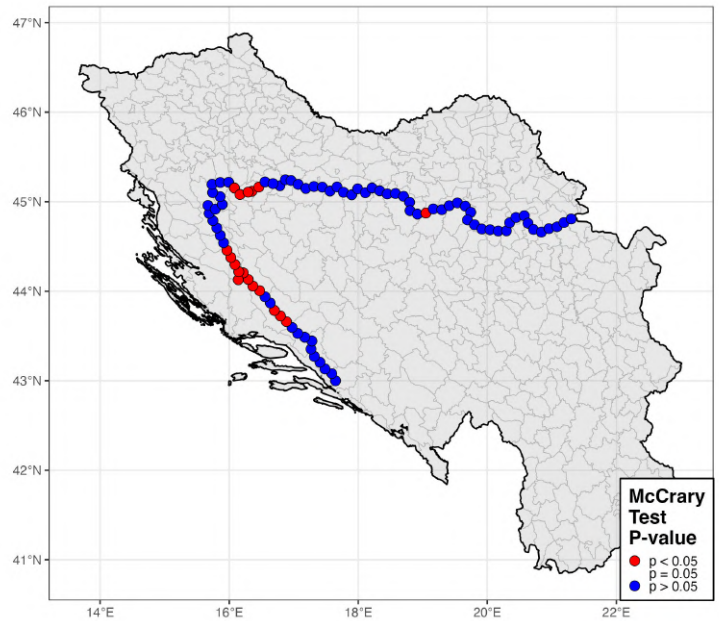
## F Tests for Manipulation around the Threshold

An additional identifying assumption is the absence of selective sorting across the treatment threshold. This assumption would be violated if people with different characteristics systematically moved from the Ottoman to the Habsburg side, thereby indirectly affecting literacy outcomes.

To test for such sorting, we employ the McCrary density test (McCrary, 2008), which detects discontinuities in the distribution of the running variable, in this case distance to the border. A discontinuity would indicate potential manipulation, such as a higher population density on the Habsburg side due to migration.

While the traditional McCrary test is typically estimated once over the full sample, this approach may miss localized sorting along particular border segments. To account for this, we compute the test at 88 points along the border using the `rddensity` function of Cattaneo *et al.* (2018). Figure A.9 reports the resulting p-values, revealing significant discontinuities at several locations along the border, particularly near the Dalmatian coast, the Una river, and the Croatia-Serbia boundary. Consequently, discontinuity estimates at these segments should be interpreted with caution.

***Figure A.9.*** *Density of Running Variable with Confidence Intervals*



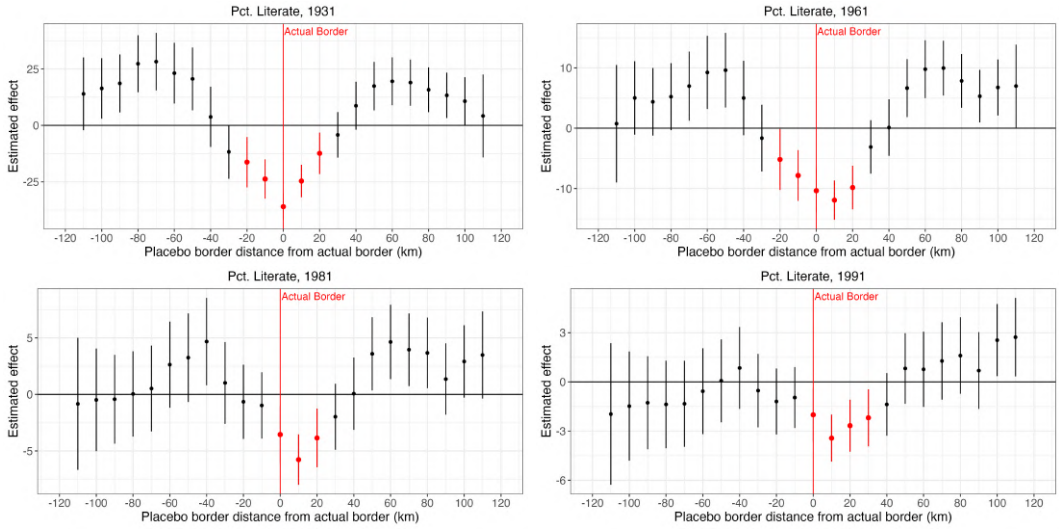

*Notes: McCrary density test p-values at each of the 88 boundary evaluation points, computed using* `rddensity` *(Cattaneo* et al.*, 2018). A low p-value indicates a discontinuity in population density at the border, suggesting potential selective sorting. Significant discontinuities appear near the Dalmatian coast, the Una River, and the Croatia-Serbia boundary; estimates at these segments should be interpreted with caution.*

## G Falsification: Artificial Border Shifts

To provide further evidence for whether the results can be interpreted as a genuine effect of the Ottoman rule as opposed to a continuous difference in the North-South dimension or remoteness from Istanbul/Vienna, we create “placebo" borders at 10-kilometer intervals from the actual Habsburg-Ottoman border, then re-estimated the local linear regression tests. This approach is similar to that used by Lee & Schultz (2012). It is meant to ascertain if running any line with the same shape as the line that divided the Ottoman from the Habsburg Empire in a north-south direction would generate a similar effect. The results presented in Figure A.10 show precisely how many kilometers north and how many kilometers south a different border location would still yield a significant result. The figures depict the literacy rates for 1931, 1961, 1981, and 1991, along with their 95 percent confidence intervals from a series of tests using the actual border and placebo borders in increments of 10 km. The results indicate that the effect remains significant for border shifts of up to 20 km in the early census years (1931, 1961). By 1991, significance extends further south (up to 30 km) but narrows northward, consistent with the convergence pattern documented above.

**FIGURE A.10.** *Placebo Border Effects*

*Notes: Estimated treatment effects using artificial borders shifted in 10-km increments north and south of the actual Habsburg-Ottoman frontier, for 1931, 1961, 1981, and 1991 literacy. Error bars are 95% confidence intervals. The true border (0 km) produces the largest and most significant effect. In the early census years (1931, 1961), effects become insignificant beyond ±20 km. By 1991, significance extends further south (up to 30 km) but narrows northward, consistent with asymmetric convergence. The discontinuity is specific to the historical frontier rather than a smooth north-south gradient.*

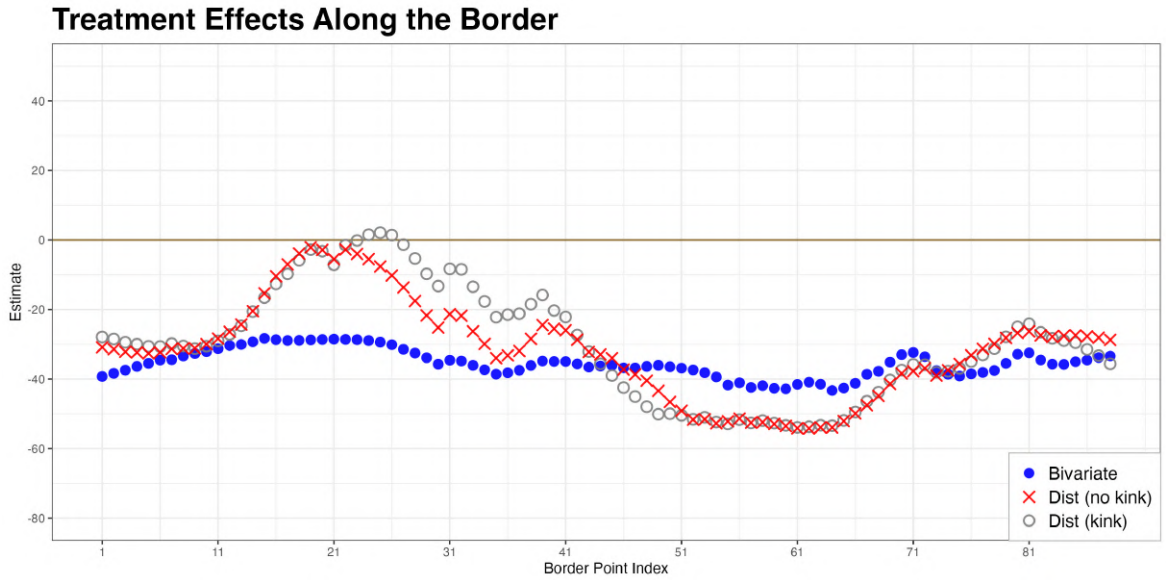


# H Multi-Score RD Design: Bivariate Location vs. Distance Approach

When it comes to the multi-score regression discontinuity (RD) design, Cattaneo *et al.* (2025b) explain that the approach based on the *bivariate location* relative to the boundary, presented in the main text, is an alternative to a *univariate distance* from the boundary. They show that the bivariate location method has both theoretical and practical advantages, which is why it is the main method presented in the main text. In particular, they explain that standard RD methods are not valid when distance is used, because features of the assignment boundary (e.g., smoothness, kinks, or irregularities) can generate substantial misspecification bias. This issue is especially problematic in geographic RD designs (Keele *et al.*, 2015). For this reason, Cattaneo *et al.* (2025a,b) recommend using the bivariate location score directly, together with guidance on bandwidth selection (Calonico *et al.*, 2020) and robust bias correction (Calonico *et al.*, 2014, 2018).

Nevertheless, for completeness, we also report the results from the two distance-based estimations (with and without assuming the presence of border kinks), together with the bivariate regression discontinuity estimates. Figure A.11 therefore compares the two point-estimation approaches and demonstrates empirically the bias associated with the distance-based method. The blue dots correspond to the treatment effect estimated using the data-driven MSE-optimal bandwidth. The crosses correspond to the distance-based treatment effect that ignores border kinks, while the empty circles represent the distance-based treatment effect that accounts for border kinks. Figure A.11 indicates substantial bias in the distance-based approaches (both with and without border kinks) along land sections of the border in Yugoslavia and less bias along river sections of the border.

***Figure A.11.*** *Border Segment Heterogeneous Effects*




*Notes: Comparison of three RD estimation approaches at each boundary evaluation point. Blue dots: bivariate location RD2D estimates (main specification). Empty circles: distance-based estimates accounting for border kinks. Crosses: distance-based estimates ignoring kinks. Substantial bias in the distance-based approaches appears along land sections of the border; bias is smaller along river sections, consistent with* Cattaneo et al. *(2025b).*

# I Traditional GRD

In this section we perform a standard geographic regression discontinuity analysis for completeness and comparability with prior work, since much of the existing literature has relied on distance-to-border designs. Including these results provides a benchmark against which the bivariate location-score approach in the main text can be evaluated. Similar to the previous section, we examine the impact of Ottoman rule on 1931 literacy rates. The key assumption is that units close to the border are similar in observed and unobserved characteristics, aside from the imperial institution they inherited. The main model of interest can be written as:

$$\text{Literacy}_v = \beta_0 + \beta_1 \cdot \text{Ottoman}_v + f(\text{geographic location}_v) + \varepsilon_v \tag{3}$$

where $\text{Ottoman}_v$ is an indicator variable equal to 1 if a settlement is inside the Ottoman area and 0 otherwise; $f(\text{geographic location})$ is the RD polynomial, which controls for smooth functions of a district location in space. We run the RD parametrically in a first instance.

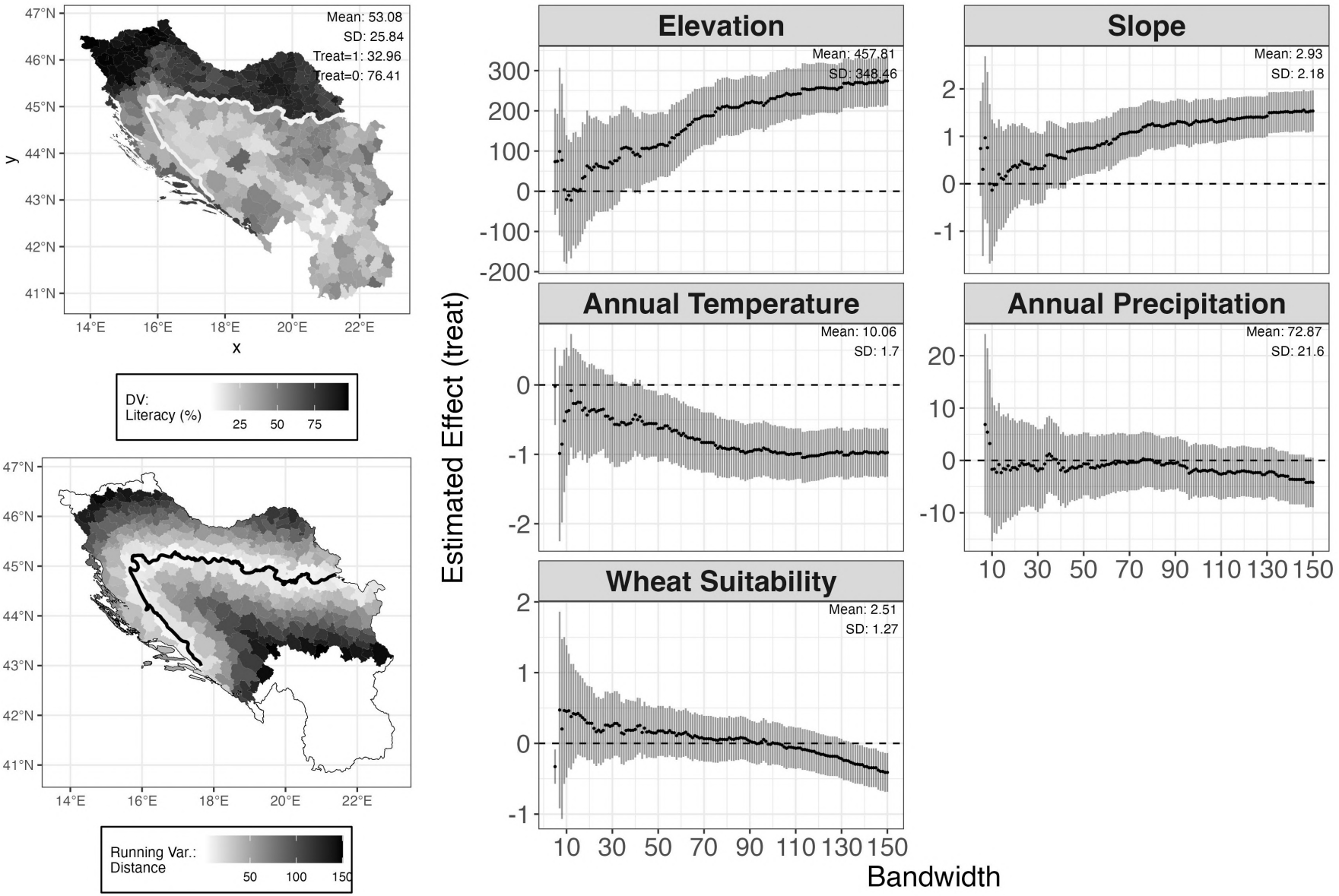
We also estimate the RD model non-parametrically, following (Cattaneo *et al.*, 2019; Gelman & Imbens, 2019), where the function for geographic location is a local linear polynomial in the distance to the border. As Cattaneo *et al.* (2019) recommend, we use a triangular and uniform weighting kernel and calculate the optimal bandwidth bw using the MSE-minimizing procedure. We report them as a comparison here using the `rdrobust` estimation.

The primary running variable is the Euclidean distance to the Habsburg-Ottoman border. We assign treatment (treat = 1) to districts historically governed by the Ottoman Empire (using the territorial configuration established under the Treaty of Belgrade of 1739) and use the distance variable to define the running variable in both the non-parametric and parametric implementations.

### *Covariate Balance in Traditional GRD*

Figure A.12 displays covariate balance across bandwidths from 5 to 150 km for elevation, slope, precipitation, temperature, and wheat suitability. At narrow bandwidths (below roughly 40 km), none of the five covariates shows a statistically significant imbalance. As the bandwidth expands, elevation and slope begin to diverge - treated districts are systematically higher and steeper - and temperature shows a modest cooling gradient on the Ottoman side. Precipitation and wheat suitability remain balanced throughout. These patterns are consistent with the mountainous terrain in the western and southern border segments and reinforce the advantage of the RD2D approach, which restricts inference to locally balanced evaluation points rather than pooling across heterogeneous segments.

**FIGURE A.12.** *Covariate Balance in Traditional GRD*




*Notes: Each facet reports the estimated Habsburg-Ottoman difference in a geographic covariate (elevation, slope, precipitation, temperature, wheat suitability) across bandwidths from 5 to 150 km. Estimates near zero indicate covariate balance. Error bars are 95% OLS confidence intervals. The left map shows the dependent variable (literacy 1931); the right map shows the running variable (distance to the border). Diamonds mark adjacent-district comparisons; triangles mark nonparametric* `rdrobust` *estimates with the uniform kernel and squares with the triangular kernel*

*Traditional GRD Results*

As a first step, we estimate a simple linear model using only the districts that are geographically adjacent to the Habsburg-Ottoman border, comparing literacy outcomes by treatment status. This specification offers the most direct comparison between treated and control units and minimizes differences in both observed and unobserved characteristics between the Ottoman and Habsburg areas. These effects are visible as diamond shapes in each facet of Figure A.13.

Beyond this localized comparison, we examine the robustness of treatment effects across functional forms and bandwidth choices. We estimate a series of parametric linear models using bandwidths from 10 km to 150 km, in increments of 1 km. We begin with a simple specification that includes only the treatment indicator (see facet "Treat Only" of Figure A.13). Next, we incorporate region fixed effects[19] to account for broader regional variation (facet "Treat + Region FE"). We then add controls for geographic coordinates (latitude and longitude) to capture spatial trends (facet "Treat + Latitude + Longitude"). In subsequent specifications, we include the running variable itself (distance to the Ottoman border), facet "Treat + Distance to Border," followed by a polynomial expansion of the geographic coordinates to allow for nonlinear spatial patterns (facet "Treat + Lat + Lon + $\text{Lat}^2$ + $\text{Lon}^2$ + Lat $\times$ Lon"). Additional models introduce a quadratic term for the distance to the border (facet "Treat + Distance + $\text{Distance}^2$"), and finally, we estimate a fully saturated model that includes a range of geographic and climatic covariates, such as elevation, slope, average temperature, precipitation, and wheat suitability (facet "Treat + Geography + Climate Covariates"). All models are estimated with heteroskedasticity-robust standard errors.

For each model and bandwidth, we extract treatment effect estimates, confidence intervals, p-values, and metadata (sample size, number of counties). We record which covariates were dropped due to collinearity, to transparently document model behavior across varying sample windows. (Some covariates are dropped in very close proximity to the border, around 10-15 km.)

We also estimate two non-parametric models using the `rdrobust` estimator (Keele & Titiunik, 2015), which fits local polynomial regressions with MSE-optimal bandwidth selection. This approach allows for flexible estimation of treatment effects without imposing strong global parametric assumptions about the relationship between the running variable (distance to the border) and the outcome (literacy). Specifically, the estimator applies a simple polynomial functional form, but only locally around the cutoff, avoiding the risks of global model misspecification. For robustness, we use two kernel specifications: uniform and triangular (Calonico *et al.*, 2014). These non-parametric estimates are shown as squares and triangles overlaid on the parametric results in Figure A.13.

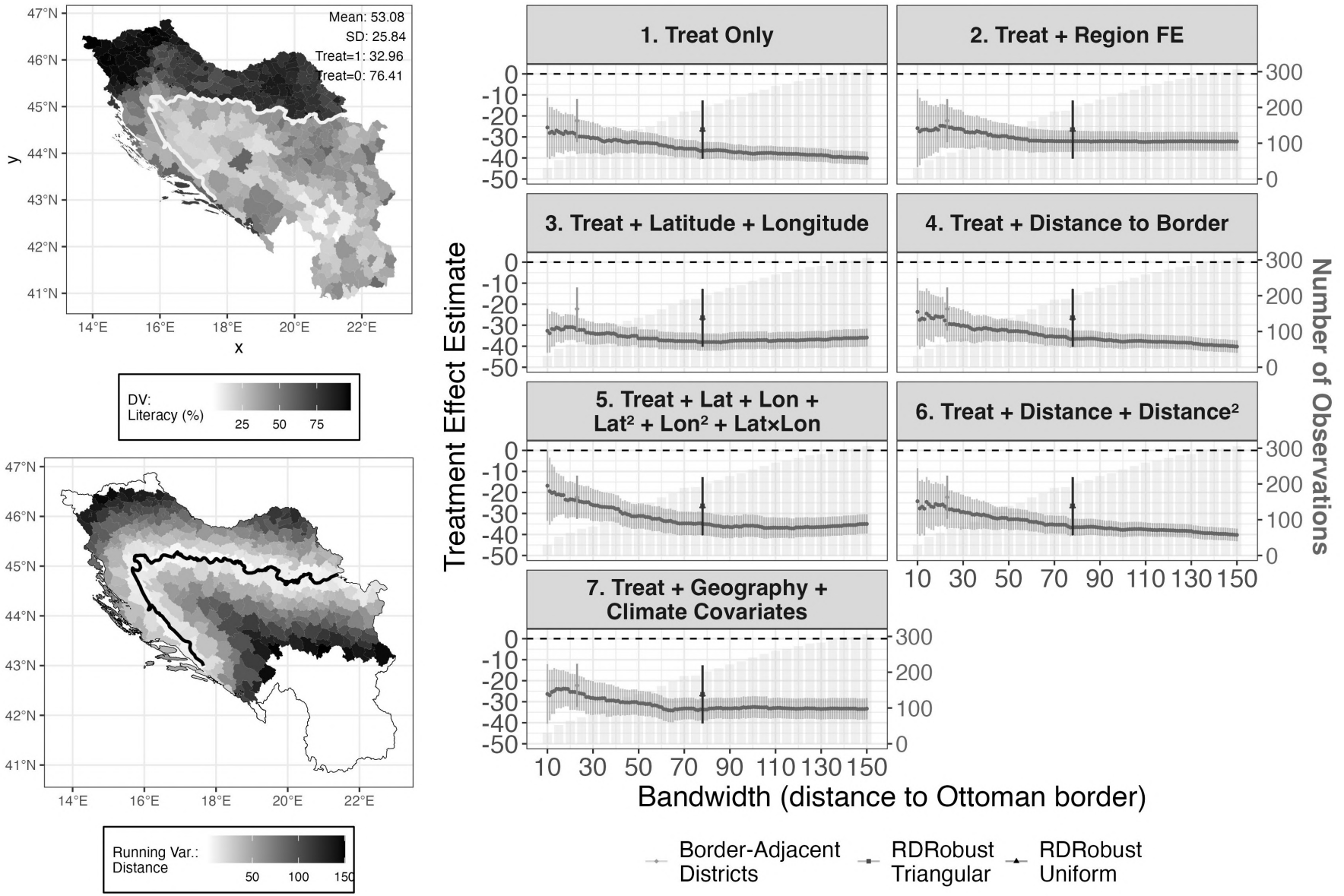
Results are visible alongside two maps that display the main dependent variable,

[19] The region fixed effects correspond to the banovinas in Yugoslavia in the early 1930s that take their name after the rivers present on their territory: Danube, Drava, Drina, Littoral, Morava, Sava, Vardar, Vrbas, and Zeta.

percentage literacy in 1931, and the running variable - distance from the Habsburg-Ottoman border in increments of 1 km. Figure A.13 also shows the results across models and bandwidths using: (1) point estimates and confidence intervals for treatment effects; (2) bars representing the number of observations at each bandwidth (3) overlays for the non-parametric RD estimates and adjacent-districts model. To ensure clarity, we differentiate models using both shades of grey and shape (triangle, square, diamond).

In terms of magnitude, the estimated effect of Ottoman rule on literacy is strongly negative and substantively large across all specifications (Figure A.13). The simplest comparison using only border-adjacent districts, the two nonparametric `rdrobust` estimates, and the fully saturated parametric model all yield treatment effects in the range of roughly 25-30 percentage points, reinforcing the robustness of the discontinuity at the imperial border. This gap remains stable as the bandwidth expands and as increasingly flexible spatial controls and covariates are added.

**FIGURE A.13.** *Treatment Effects in Traditional GRD*




*Notes: Each facet reports the estimated Ottoman treatment effect on 1931 literacy across bandwidths from 10 to 150 km, under progressively richer parametric specifications (treatment only; region FE; spatial polynomials; geographic and climatic controls). Grey bars show sample sizes at each bandwidth. Diamonds mark adjacent-district comparisons; triangles mark nonparametric* `rdrobust` *estimates with the uniform kernel and squares with the triangular kernel*

. Heteroskedasticity-robust standard errors.

## J Robustness: Bounded-Outcome Transformations

A potential concern with the convergence analysis is that percentage-point gaps mechanically compress as literacy approaches universal levels, since one cannot have more than 100% literacy. Yugoslavia's Ottoman literacy gap fell from 43.5 to 6.8 percentage points between 1931 and 1991, an apparent 84.5% closure. A sceptical reader might conclude that this represents near-complete convergence. However, this figure is misleading: as both sides of the frontier approach high literacy, percentage-point differences shrink mechanically regardless of whether the underlying inequality has genuinely been eliminated.

To address this concern, we re-estimate the OLS treatment-effect regressions using three standard transformations for bounded outcomes: (1) the log-odds (logit) transformation, $\ln(p/(1-p))$; (2) the probit (inverse-normal) transformation, $\Phi^{-1}(p)$; and (3) the arcsine square-root transformation, $\arcsin(\sqrt{p})$.[20] Each maps the bounded $[0, 1]$ interval onto a wider scale that penalises gaps near the ceiling more heavily than raw percentage points, but each does so with a different functional form. If the convergence finding is robust across all three, rather than an artefact of the specific curvature of any one function, the finding is on solid ground.

Table A.1 presents the results for Yugoslavia. The pattern is striking and consistent across all three transformations. While percentage-point convergence suggests 84.5% of the 1931 gap was closed, every bounded-outcome transformation reveals a substantially smaller degree of genuine convergence. The convergence rate drops from 84.5% in percentage points to just 28.8% in log-odds, 46.9% in probit, and 68.4% in arcsine terms. The three transformations thus agree that Yugoslavia's convergence was much less complete than raw percentage points suggest, a conclusion consistent with the paper's central argument that imperial legacies persisted despite formal equalisation of schooling infrastructure.

The intuition is straightforward: when literacy on both sides of the frontier approaches high levels, percentage-point gaps mechanically shrink. Yugoslavia's residual gap, though only 6.8 percentage points in levels, remains large in proportional terms. This mechanical compression is precisely why bounded-outcome transformations are necessary for evaluating convergence claims about outcomes near the boundary of their support.

[20] For the log-odds and probit transformations, we apply a small boundary adjustment $p_{\text{adj}} = \max(\min(p, 0.9999), 0.0001)$ before computing the transformation to avoid undefined values at exactly 0% or 100%. This has negligible impact on the results since observed literacy rates in the data are well within these bounds. The arcsine transformation is defined on $[0, 1]$ and requires no boundary adjustment.

***Table A.1.*** *Convergence in Ottoman–Habsburg Literacy Gaps: Bounded-Outcome Transformations*

| | Percentage Points | | % of 1931 Gap Closed by 1991 | | |
|---|---|---|---|---|---|
| | 1931 Gap | 1991 Gap | Log-Odds | Probit | Arcsine |
| Yugoslavia | $-43.5^{***}$ | $-6.8^{***}$ | 28.8% | 46.9% | 68.4% |
| | (1.7) | (0.4) | | | |
| *For comparison: PP convergence = 84.5%* | | | | | |

*Notes:* The 1931 and 1991 gaps are the OLS Ottoman-treatment coefficients on literacy (percentage points) from the full district samples, as in Table 2 column (1); Conley (1999) spatial HAC standard errors (30 km cutoff) in parentheses. Because literacy is bounded at 100, percentage-point convergence can overstate catch-up as the Habsburg side nears the ceiling. The last three columns therefore recompute the share of the 1931 gap closed by 1991 after transforming the literacy rate to an unbounded scale (log-odds, probit, arcsine): convergence remains large on every scale, but smaller than the raw percentage-point figure of 84.5%. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

## K Spatial Robustness: Conley Standard Errors

Table A.2 reports the main Ottoman-treatment effects of Table 2, column (1), and their 1961-1991 census-wave analogues with Conley (1999) spatial HAC standard errors at distance cutoffs of 30, 60, and 90 km. The baseline 30 km cutoff equals the mean centroid-to-centroid distance between contiguous districts (29.3 km); the 60 km and 90 km cutoffs double and triple that distance. All coefficients remain significant at the 1% level across all cutoff distances.

***Table A.2.*** *Spatial Robustness: Conley (1999) Standard Errors at Multiple Distance Cutoffs*

| | 30 km | 60 km | 90 km |
|---|---|---|---|
| *Panel A: Population literacy, 1931* | | | |
| Ottoman treatment | $-43.5^{***}$ | $-43.5^{***}$ | $-43.5^{***}$ |
| | (1.7) | (2.6) | (3.3) |
| *Panel B: Female literacy, 1931* | | | |
| Ottoman treatment | $-55.7^{***}$ | $-55.7^{***}$ | $-55.7^{***}$ |
| | (2.0) | (3.0) | (3.8) |
| *Panel C: Ottoman treatment across census years* | | | |
| 1931 | $-43.5^{***}$ | $-43.5^{***}$ | $-43.5^{***}$ |
| | (1.7) | (2.6) | (3.3) |
| 1961 | $-19.0^{***}$ | $-19.0^{***}$ | $-19.0^{***}$ |
| | (1.0) | (1.5) | (1.9) |
| 1981 | $-9.4^{***}$ | $-9.4^{***}$ | $-9.4^{***}$ |
| | (0.6) | (0.9) | (1.1) |
| 1991 | $-6.8^{***}$ | $-6.8^{***}$ | $-6.8^{***}$ |
| | (0.4) | (0.6) | (0.8) |

*Notes:* Panels A and B report the OLS Ottoman-treatment coefficient of Table 2, column (1) — population literacy (Panel A) and its female-literacy analog (Panel B) — on the full 1931 district sample (378 districts). Panel C reports the population-literacy coefficient estimated on the full district sample of each census wave ($N = 378/774/508/508$). The point estimate is identical across columns by construction; only the Conley (1999) spatial HAC standard error (Bartlett kernel) changes as the distance cutoff increases. The baseline 30 km cutoff equals the mean centroid distance between contiguous districts (29.3 km); 60 km and 90 km are double and triple that distance. *** p<0.01, ** p<0.05, * p<0.10.

## L Migration

An important question is the extent to which in- and out-migration might be impacting the results. The most important period that witnessed migration is 1930-1960. Political repression, economic conditions, and ethnic tensions drove outmigration among Yugoslavia's national minorities under communism. The most significant outflows occurred among Germans, Italians, Turks, and Albanians, while other groups, such as Hungarians and Romanians, also saw notable departures. Because these movements ran through the end of the 1950s — the German and Italian expulsions in the late 1940s, and the Turkish and Albanian Muslim emigration that accelerated after the 1953 Yugoslav–Turkish agreement — we benchmark population change over 1931–1961 rather than against the newly available 1948 or 1953 censuses: 1948 predates the emigration to Turkey and 1953 falls in the middle of it, whereas by 1961 the post-war redistribution had largely run its course.

The German minority, once numbering around 500,000, faced near-total expulsion after World War II. As early as 1943, the Communist-led Anti-Fascist Council of National Liberation "divested the Germans of all their rights and confiscated their land" (Shoup, 1963, p. 72). By 1963, fewer than 60,000 remained, with many forcibly relocated, deported to the Soviet Union, or fleeing to Germany and Austria. Estimates indicate that "183,000 remained unaccounted for" (*ibidem*).

The Italian minority, primarily in Istria and Rijeka, experienced mass migration after the redrawing of borders in 1945. Around 100,000 Italians emigrated between 1945 and 1948 (p. 80), driven by fears of persecution and economic instability. This exodus left "deserted and decaying fishing villages along Istria's western coast" (Shoup, 1963, p. 80).

The Turkish minority, mainly in Macedonia, also declined significantly due to voluntary migration facilitated by agreements between Yugoslavia and Turkey. Official records noted that "2,204 families had received permission to emigrate" by 1954 (Shoup, 1963, p. 80). Albanians (mostly Muslim) faced severe repression, particularly in Kosovo. Armed uprisings in 1944-45 led to prolonged conflict, and by 1945, "bitter fighting continued into spring, and martial law was not lifted until July" (Shoup, 1963, p. 71). Though not subject to forced expulsion, many left due to economic hardship and ongoing discrimination.

These movements map onto the three main religious groups in distinct ways. Forced expulsions primarily affected Catholics on the Habsburg side: ethnic Germans from Vojvodina and Italians from Istria, depleting the control group. Orthodox populations remained relatively stable geographically, though internal redistribution occurred. Muslim populations on the Ottoman side experienced voluntary outflows - Turkish emigration from Macedonia and Albanian departures from Kosovo - reducing the treatment group. The direction of potential bias thus differs by group: Habsburg-side Catholic losses could inflate the estimated Ottoman penalty (by removing educated populations from the control side), while Ottoman-side Muslim losses could attenuate it (by removing less-literate populations from the treatment side).

Model 4 in the robustness analysis below directly controls for percentage changes in Muslim, Orthodox, and Catholic populations between 1931 and 1961, capturing both forced and voluntary movements across all three groups.

Unfortunately, there are no local-level data to indicate these group changes between 1931 and 1961. To address this gap, we calculate migration by investigating population changes from 1931 to 1961 in absolute numbers.

### *L.1 Absolute Differences in Population*

The fact that the units in 1931 and 1961 are not the same poses a challenge. For example, the unit in 1931 is *srez* or county. The unit in 1961, 1981, and 1991 is *opština* or municipality. The units for 1931 and 1961 are visible in the top left maps of figures A.14 and A.15.

As a first step, we calculated population density at a *srez* level in 1931 and at an *opština* level in 1961 by dividing the total population of an administrative unit by its area (see the top left maps of figures A.14 and A.15). We then created grids of approximately 24 by 24 km to make the censuses from 1931 more comparable to the one in 1961. Each administrative unit was then overlaid onto a regular grid, and the area of intersection ($A_{ij}$) between an administrative unit $i$ and a grid cell $j$ was computed (see top right maps in figures A.14 and A.15). The proportion of the administrative unit's area intersecting each grid cell was used to distribute population accordingly (see bottom left maps of figures A.14 and A.15):

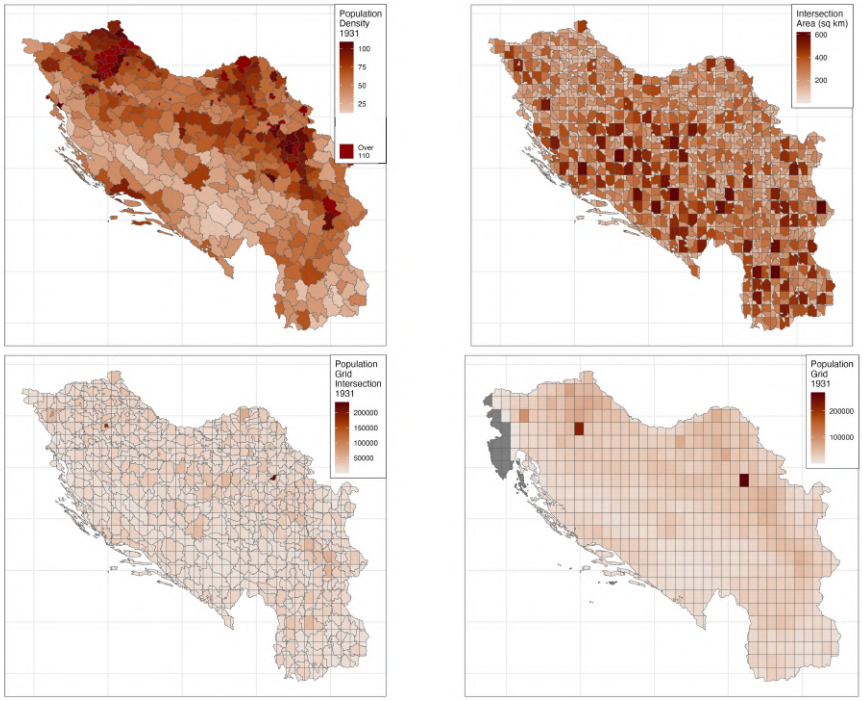
$$P_{ij} = D_i \times A_{ij}$$

where $P_{ij}$ represents the estimated population of grid cell $j$ based on the portion of administrative unit $i$ that overlaps with it. $D_i$ represents the population density for administrative unit $i$. This procedure was applied separately for 1931 and 1961, ensuring that each grid cell contained a population estimate for both years. This method ensures that population estimates are spatially consistent despite changes in administrative boundaries, enabling a robust comparison of demographic trends over time. As a final step, we also scaled the population estimate by computing an adjustment factor from total population sums in the three datasets (Yugoslavia 1931, Yugoslavia 1961, and grid data), then applied it to adjust the grid population values (see the results in the bottom right maps of figures A.14 and A.15).

To summarize, figures A.14 and A.15 show the four steps performed to calculate population size at the grid level in 1931 and 1961. The top-left maps show the original units (srez for 1931 and opština for 1961), while the top right maps display the grid intersected with the polygons corresponding to the srez for 1931 and opština for 1961, depicting the area of the intersecting polygons. The bottom left maps show the population density at the intersecting polygons level while the bottom right maps indicate the final scaled calculations of population size at a grid level.

As a final step, we calculated the difference in population between 1961 and 1931 at a grid level. The left map in Figure A.16 shows the absolute difference, with

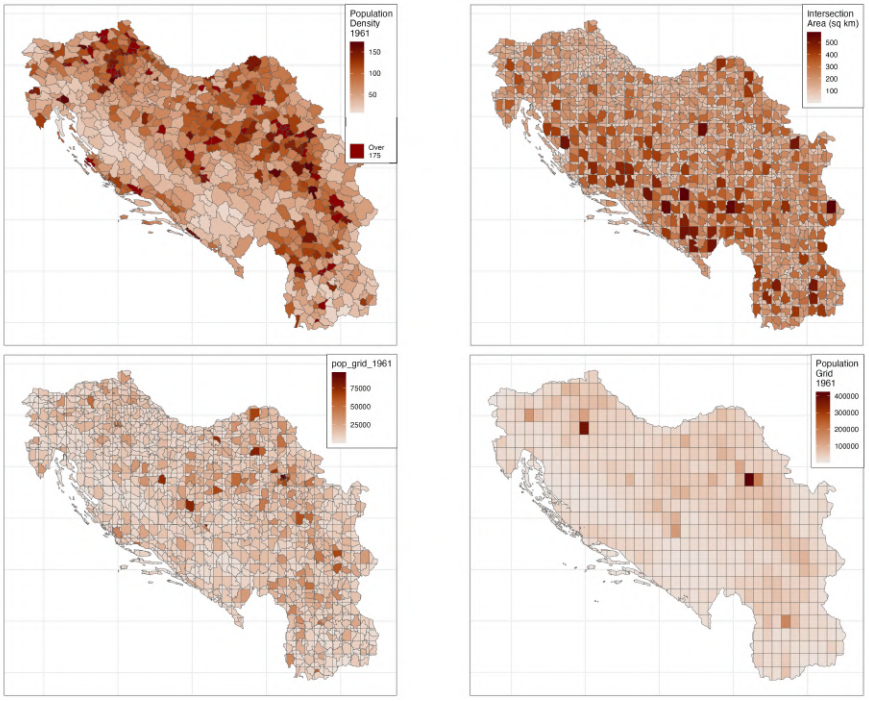
***Figure A.14.*** *Population Grid Level 1931*




*Notes: Four-step procedure to construct spatially comparable population estimates. Top left: original srez (county) boundaries with population density. Top right: approximately 24×24 km (0.25°) grid overlaid on administrative polygons showing intersection areas. Bottom left: population density at the intersection level. Bottom right: final scaled population estimates at the grid level.*

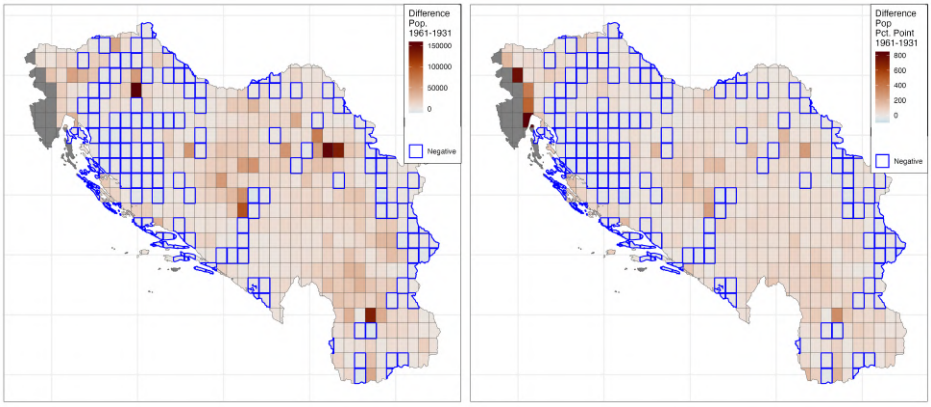
positive changes in darker colors and the areas with no change or negative change in lighter colors; the right map shows the percentage change. In both maps, we delineate the areas with negative change in blue contours. The blue grids are regions where there were fewer people in 1961 compared to 1931, suggesting a process of outmigration.

**FIGURE A.15.** *Population Grid Level 1961*




*Notes: Same procedure as Figure A.14, using opština (municipality) boundaries for 1961.*

**FIGURE A.16.** *Population Changes 1961-1931*




*Notes: Grid-level difference in estimated population between 1961 and 1931. Left: absolute difference (darker colours indicate positive growth). Right: percentage change. In both panels, blue contours delineate grid cells with negative change (fewer people in 1961 than 1931), suggesting outmigration. Grid cells are approximately 24× 24 km; see Figures A.14-A.15 for construction details.*

### *L.2 Migration by Religious Group*

We also calculated the percentage change in the three religions: Muslims, Catholics, and Orthodox. The percentage Muslims, Orthodox, and Catholics are clearly indicated in the 1931 census. For 1961, the three shares are constructed from census nationality exactly as described in Appendix C. The top maps in figures A.17, A.18, and A.19, show the rasterized percentage of Muslims, Catholics, and Orthodox in 1931 and 1961. The bottom left map shows the rasterized difference in percentage points between 1961 and 1931, while the bottom right map depicts the zonal statistic at the 1961 *srez* level.

***Figure A.17.*** *Difference Muslims in Percentage Points in Yugoslavia 1931-1961*

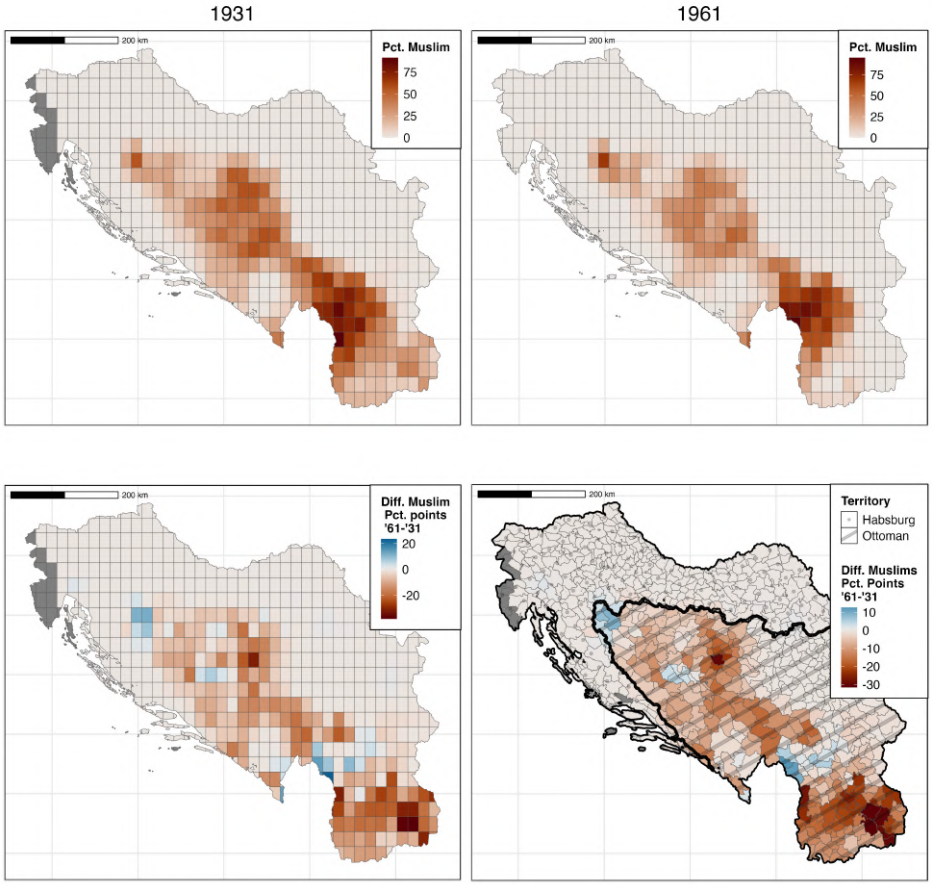


***Figure A.18.*** *Difference Catholics in Percentage Points in Yugoslavia 1931-1961*

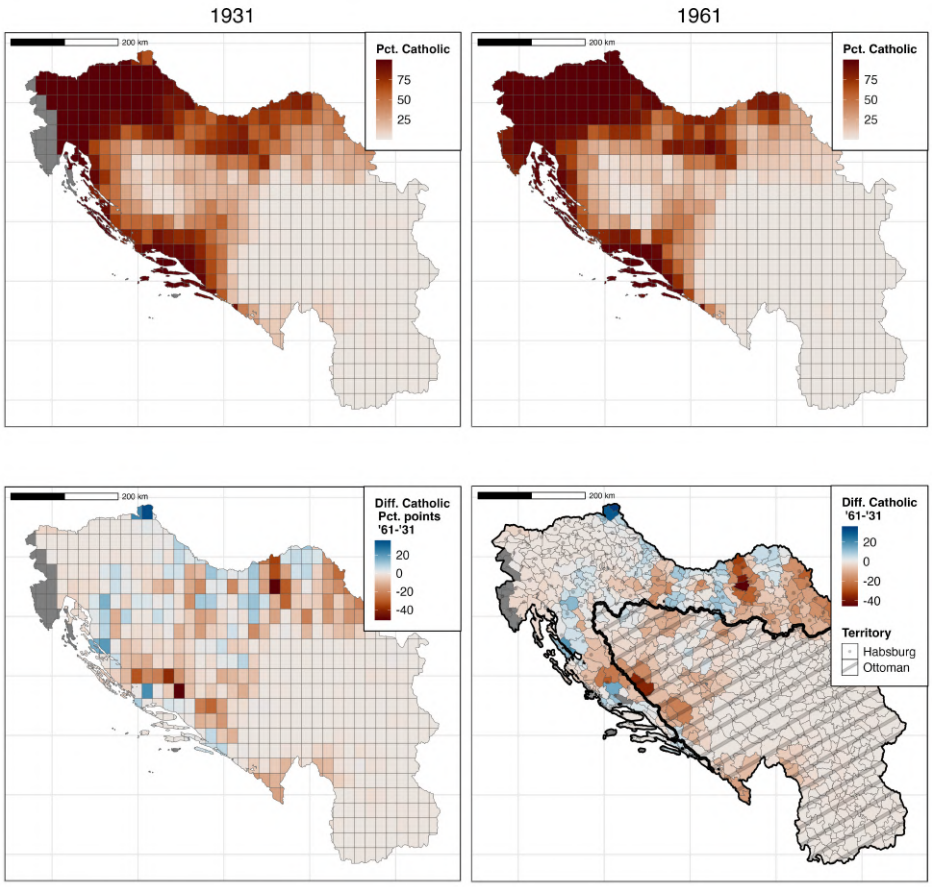
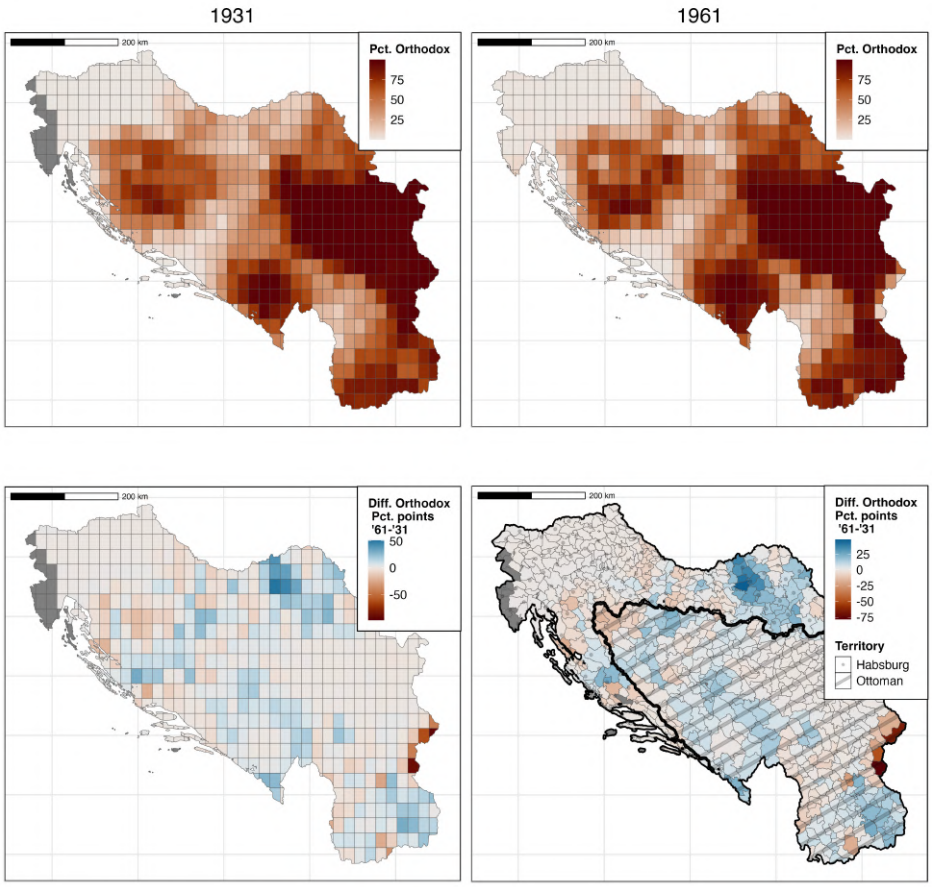


***Figure A.19.*** *Difference Orthodox in Percentage Points in Yugoslavia 1931-1961*



### *L.3 Controlling for Migration*

Given that some regions experienced migration, it is important to see how the evolution of literacy changes when controlling for migration. We thus compare a baseline OLS specification regressing literacy on the Ottoman-treatment indicator (with cluster-robust standard errors) against four models: (1) the baseline model applied to a restricted sample, excluding units where the population in 1961 was smaller than in 1931; (2) the full sample with an additional control for absolute population change between 1931 and 1961; (3) the full sample adding a control for percentage population change alongside the absolute change; and (4) the full sample with controls for percentage change in the Muslim, Orthodox, and Catholic populations between 1931 and 1961 (in 1961 added on top of the population-change controls).

Figures A.20 and A.21 show all four models compared against the original specification depicted in gray. Across all specifications, the estimates remain highly consistent, suggesting that migration does not significantly alter our main findings. Two features of the design explain why. First, Model 1 re-estimates the effect after dropping every grid unit that lost population between 1931 and 1961. That set contains the expelled-German districts of Vojvodina and the Italian districts of Istria, precisely the places where the departure of better-educated groups could mechanically compress the literacy gap, and the Ottoman coefficient barely moves. Second, and more fundamentally, the German expulsion was concentrated in Vojvodina, on the northeastern Pannonian plain, far from the Una and Sava segments that anchor the RD2D. Because the boundary design compares only municipalities immediately astride the frontier, it never places Vojvodina's depleted districts at the cutoff. The expulsion concern therefore weighs most heavily on broad full-sample Habsburg-Ottoman contrasts and least on the local RD2D estimates we emphasise, the opposite of what we would expect if migration were driving the result.

***Figure A.20.*** *Re-running the Analysis while Controlling for Migration*

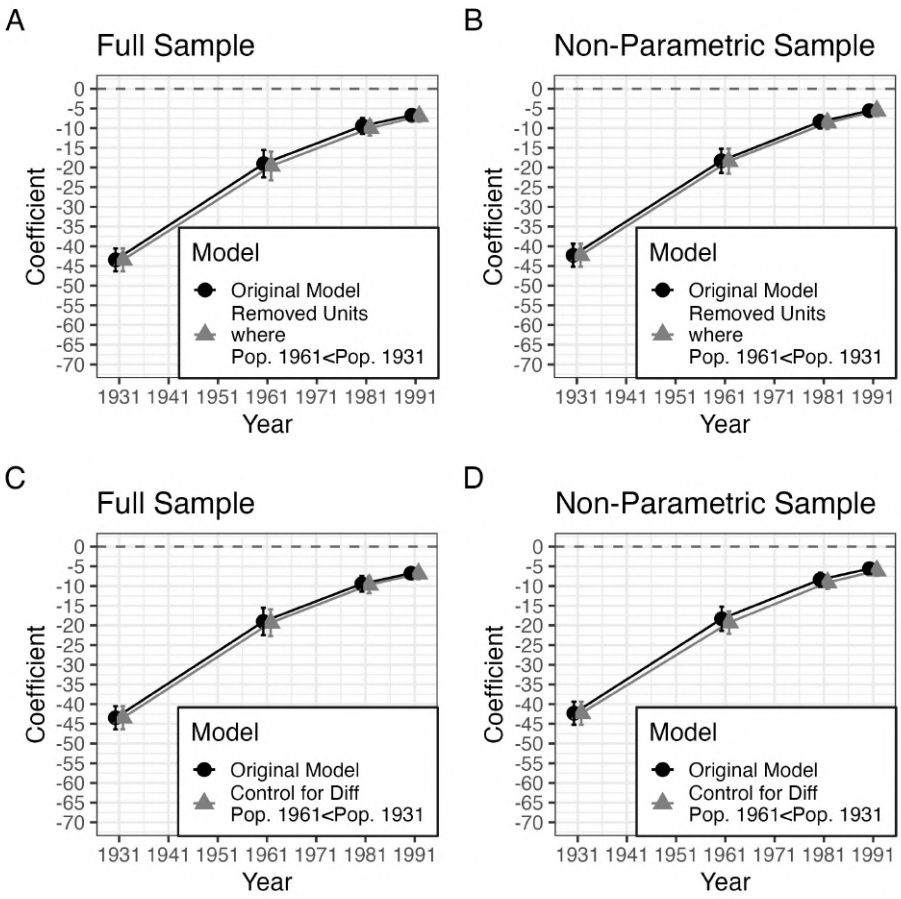
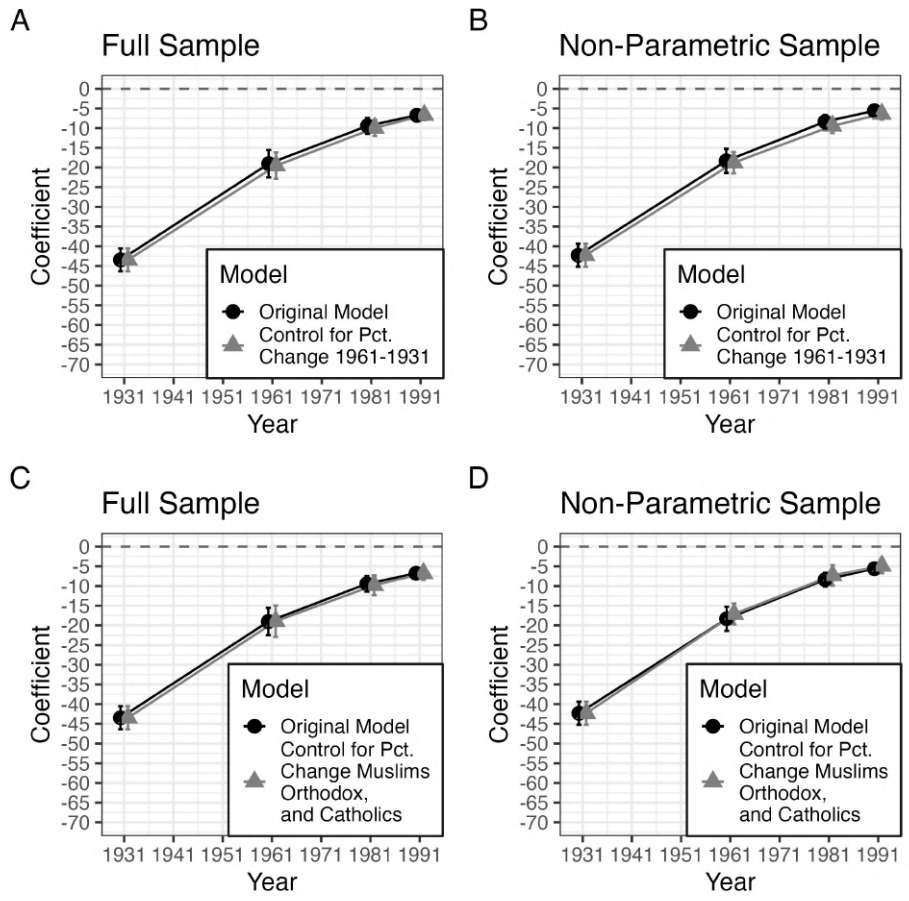


***Figure A.21.*** *Re-running the Analysis while Controlling for Migration (continued)*



## M Gender-Disaggregated Muslim-Share Regressions: Robustness

Table A.3 reports the bivariate coefficient on Muslim population share from regressions of male and female literacy rates across the seven census years. The sample is the Ottoman side of the RD2D bandwidth, the same preferred sample as Table 3 (Panel A).

The key pattern is the gender asymmetry in the Muslim-share gradient. In 1931 the penalty is large for men but small for women: female literacy was low across the entire Ottoman side, in Muslim and non-Muslim districts alike, so Muslim share explains little of it, whereas male literacy tracked Muslim share closely. From 1948 onward, once female literacy began to rise, the Muslim-share penalty is larger for women than for men, and the gap widens through the socialist period.

**TABLE A.3.** *Effect of Muslim Population Share on Literacy by Gender: Robustness*

| | 1931 | | 1948 | | 1953 | | 1961 | | 1971 | | 1981 | | 1991 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Men | Women | Men | Women | Men | Women | Men | Women | Men | Women | Men | Women | Men | Women |
| Pct. Muslim | $-0.676^{***}$ | $-0.101^{***}$ | $-0.316^{***}$ | $-0.354^{***}$ | $-0.213^{***}$ | $-0.281^{***}$ | $-0.161^{***}$ | $-0.253^{***}$ | $-0.056^{***}$ | $-0.102^{***}$ | $-0.023^{**}$ | $-0.069^{**}$ | $-0.015^{**}$ | $-0.070^{***}$ |
| | (0.059) | (0.037) | (0.043) | (0.066) | (0.031) | (0.046) | (0.023) | (0.044) | (0.017) | (0.039) | (0.011) | (0.034) | (0.007) | (0.027) |
| $N$ | 104 | 104 | 128 | 128 | 109 | 109 | 201 | 201 | 156 | 156 | 159 | 159 | 159 | 159 |

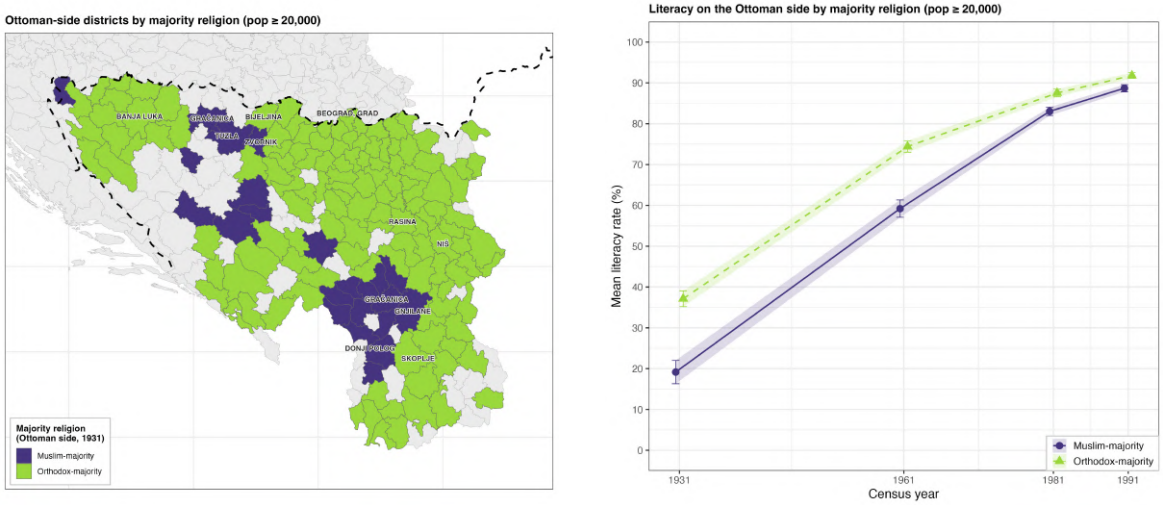
*Notes:* Each cell reports the OLS coefficient on Muslim population share (%) from a bivariate regression of male or female literacy rate on Muslim share. Conley (30 km) spatial HAC standard errors in parentheses. Sample: the Ottoman side of the RD2D bandwidth, the paper's preferred sample (Panel A of Table 3); the 1948, 1953, and 1971 columns are added from the post-war nationality-and-literacy censuses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

## N Muslim Share and Poverty: A Descriptive Check

Section 5.3 shows that Muslim population share predicts slower literacy convergence even after controlling for elevation, population density, and republic fixed effects. A natural concern is that Muslim share proxies for poverty or rural isolation rather than state-minority misalignment. To address this, we restrict the sample to Ottoman-side districts with populations above 20,000, holding urbanization roughly constant, and classify each as Muslim-majority (>50% Muslim) or Orthodox-majority (>50% Orthodox), discarding mixed districts. This yields 24 Muslim-majority and 128 Orthodox-majority districts in 1931, rising to 61 and 128 respectively by 1981 as administrative boundaries were redrawn into smaller units.

Figure A.22 presents the results. The left panel maps the included districts by majority religion in 1931; the right panel plots mean literacy rates with 95% confidence intervals across the four Yugoslav censuses (1931, 1961, 1981, 1991). Muslim-majority districts start at 19.2% literacy versus 36.9% for Orthodox-majority districts, an 17.7 percentage-point gap among comparably sized districts on the same side of the frontier. Both groups converge under communist schooling, but the gap never fully closes: 15.2 pp in 1961, 4.5 pp in 1981, and 3 pp in 1991. Since these are all districts above the same population threshold, located on the same side of the imperial frontier, the persistent gap cannot be attributed to rural isolation or differential poverty. The pattern is instead consistent with the alignment mechanism: among Ottoman-heritage districts of comparable size, those where the majority population experienced status reversal under the successor state lagged behind those where it did not.

***Figure A.22.*** *Literacy on the Ottoman Side by Majority Religion (pop ≥ 20,000)*




*Notes: Sample restricted to Ottoman-side districts (treat = 1) with total population ≥ 20,000. Districts classified as Muslim-majority if >50% Muslim, Orthodox-majority if >50% Orthodox. Left: geographic distribution. Right: mean literacy rate with 95% confidence intervals. Dashed line shows the Habsburg-Ottoman frontier.*

# O Muslim Share and Educational Outcomes

Figure A.23 reports OLS coefficients on Muslim population share for a broader set of educational outcomes within the RD2D bandwidth, complementing the main supply-and-demand figure in the text. Three patterns emerge that, taken together, distinguish between extensive-margin participation (school attendance) and intensive-margin engagement (learning, cultural exposure, media consumption).

First, the census literacy outcomes confirm the paper's central finding: Muslim population share is negatively associated with literacy (overall, male, and female), with the female literacy deficit substantially larger than the male deficit. Both gradients narrow between 1981 and 1991 but remain statistically significant, consistent with persistent but attenuating misalignment.

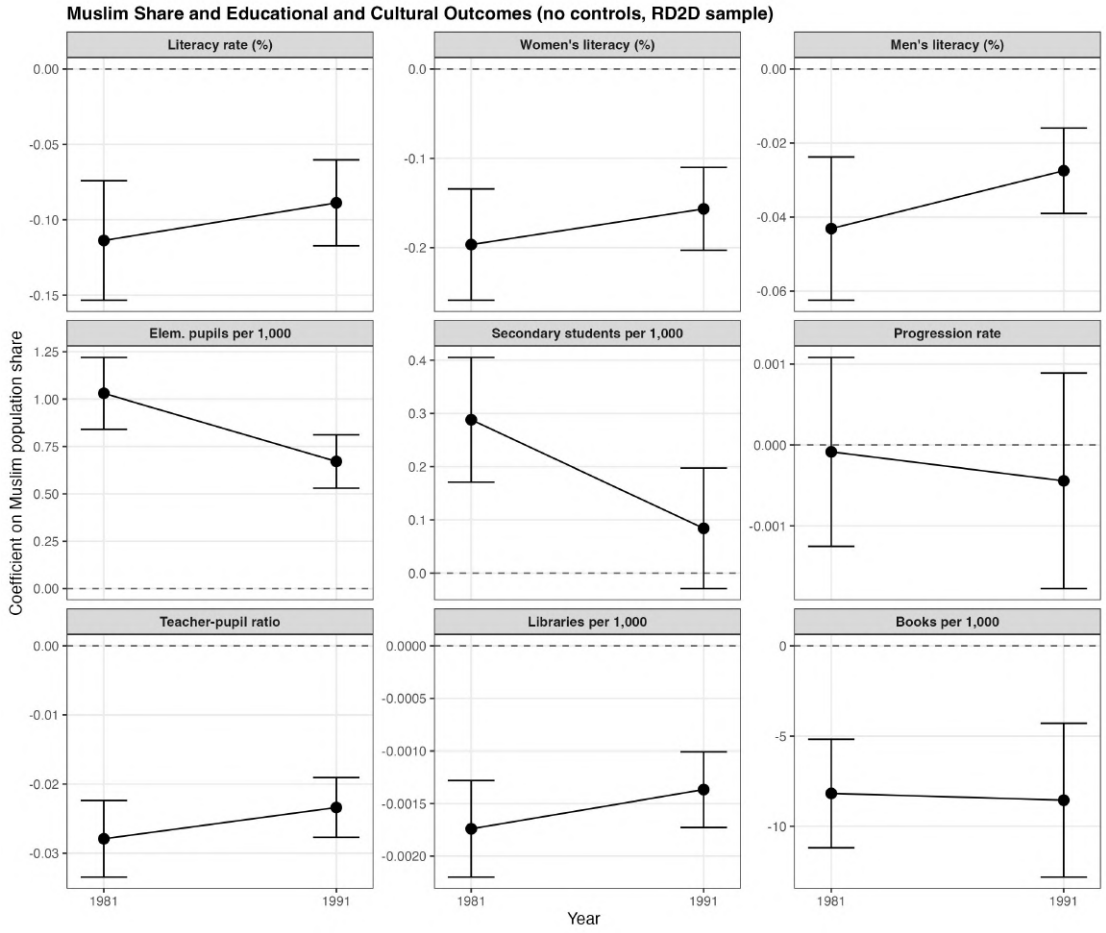
Second, enrollment measures sharpen the puzzle: Muslim-share areas have *more* elementary pupils per capita. This partly reflects demographic composition (higher fertility produces more school-age children per capita), but it unambiguously rules out the possibility that Muslim families kept children out of school. Elementary schooling was compulsory under Yugoslav law, and the data confirm that formal participation was at least as high in Muslim-share areas. The combination of higher enrollment with lower literacy is the central paradox: more children sitting in classrooms, yet worse outcomes. This demographic composition also implies that the literacy gaps reported in the main text are conservative: higher fertility produces a younger age distribution, and younger cohorts had greater exposure to socialist-era schooling, biasing Muslim-area literacy *upward*. The penalty persists despite this mechanical advantage. Similarly, the 1932 secondary enrollment deficit (Figure A.24) understates the true gap: higher fertility implies more school-age children per capita, so equal enrollment rates would have produced higher per-capita enrollment in Muslim-share areas, not lower. The observed deficit thus reflects an even larger shortfall in enrollment among school-age children. Secondary enrollment per capita is significantly *positive* in 1981 (Muslim-share areas send more students to secondary school), though this gradient fades by 1991. Progression rates show no significant gradient, indicating similar transition rates from primary to secondary education.

Third, cultural infrastructure (libraries and book stocks per capita) shows persistent negative gradients, consistent with lower voluntary engagement rather than worse provision. Combined with the media engagement results above, these patterns indicate that differences in household uptake of state-associated institutions-rather than access to schooling-shape educational outcomes. The state ensured attendance; what it could not compel was the depth of household engagement with the broader modernisation project.

## *O.1 Secondary Schooling in 1932*

Secondary schooling shows that the demand-side friction predates communism. Using newly digitised data on secondary schools and enrollment in 1932 - at the same

***Figure A.23.*** *Muslim Population Share and Educational and Cultural Outcomes (RD2D Sample)*



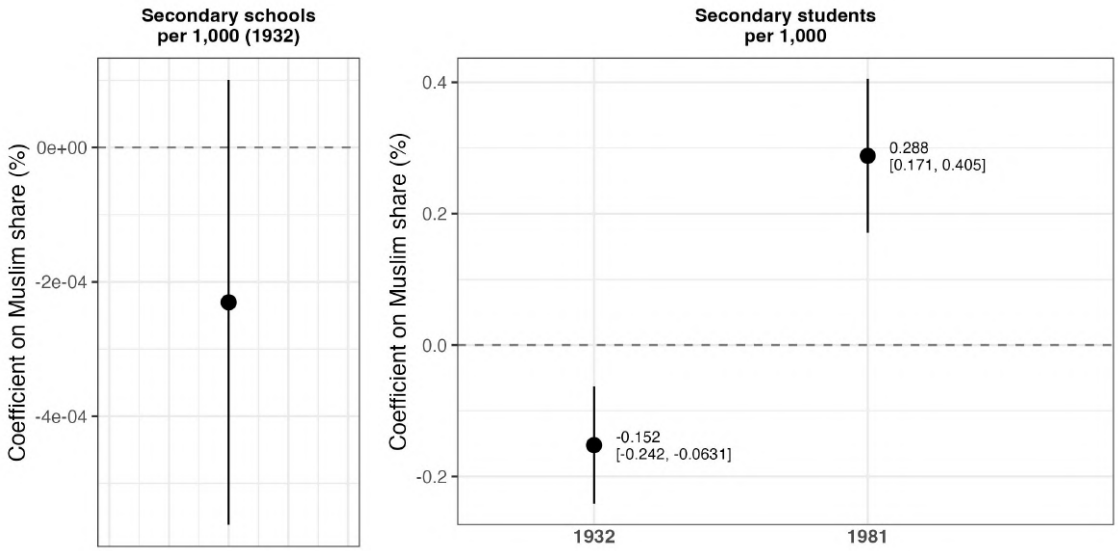

*Notes: Each panel reports OLS coefficients on Muslim population share (percentage) from regressions of the indicated outcome variable, with 95% confidence intervals. No additional controls; sample restricted to the RD2D bandwidth. Conley (30 km) spatial HAC standard errors. Row 1: census literacy rates (1981, 1991). Row 2: municipal-level enrollment measures - elementary pupils and secondary students per 1,000, and the progression rate (secondary students per elementary pupil) (1981, 1991; source: Statistički godišnjak Jugoslavije). Row 3: the teacher-pupil ratio and cultural infrastructure (libraries and books per 1,000) (1981, 1991; same source)*

.

interwar-district level as the 1931 baseline - we find that secondary school density per 1,000 shows no significant Muslim-share gradient within the RD2D bandwidth (Figure A.24, Panel A). Secondary student enrollment, however, is substantially lower in higher-Muslim-share districts (Panel B left): the coefficient is $-0.15$ students per 1,000 per percentage point of Muslim share ($p < 0.01$). The enrollment gap thus appeared within fourteen years of the political reconfiguration that accompanied the creation of the Kingdom of Serbs, Croats, and Slovenes in 1918, under a capitalist parliamentary regime rather than a communist one. This rules out the alternative that demand-side resistance was a response to communist anti-religious policy specifically; it was a response to the marginalisation of Muslim political status under

the successor state itself. By 1981, the per-capita gradient had reversed sign (+0.29 students per 1,000, Panel B right). The reversal reflects demography rather than renewed demand differences: higher-Muslim districts had younger populations, and therefore more secondary-age children per 1,000 inhabitants.[21] The progression rate - secondary students per elementary pupil, which conditions on the school-age pipeline - shows no Muslim-share gradient at all in 1981 ($p = 0.89$). The demand deficit had closed for secondary schooling even as literacy gaps persisted. The same age structure biases the 1932 estimate upward, so the interwar enrollment deficit is, if anything, understated.

***Figure A.24.*** *Muslim Population Share and Secondary School Supply vs. Enrollment*




*Notes: OLS coefficients on Muslim population share (percentage) from regressions of secondary schools per 1,000 in 1932 (Panel A) and secondary students per 1,000 in 1932 and 1981 (Panel B, left and right within panel), with 95% confidence intervals. No additional controls; sample restricted to the RD2D bandwidth (1932: 185 of the 193 bandwidth districts with yearbook data; 1981: all 243 bandwidth districts). Conley (30 km) spatial HAC standard errors. The positive 1981 estimate reflects the younger age structure of higher-Muslim districts (more secondary-age children per 1,000 inhabitants); the demography-robust progression rate (secondary students per elementary pupil) shows no 1981 gradient (see text).*

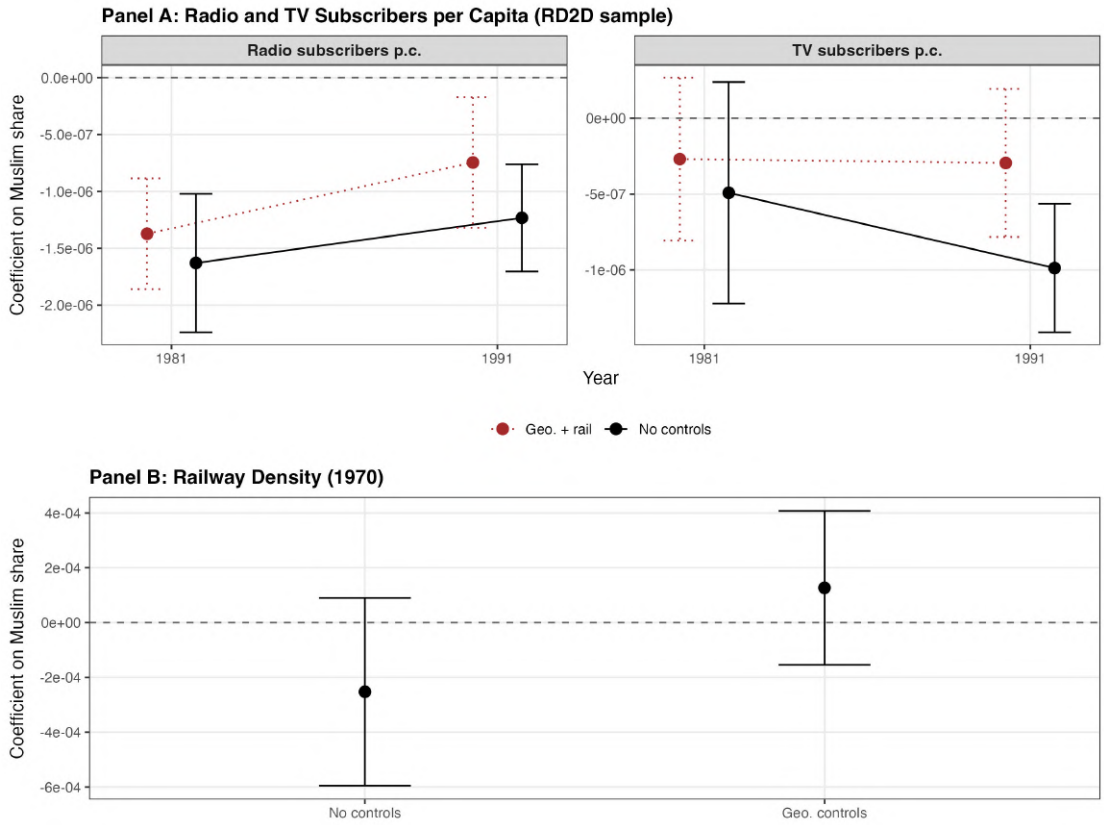
## *O.2 Media Engagement and Market Access*

Radio and TV subscriptions per capita (Figure A.25, Panel A) provide the cleanest demand-side reading in the supply-side data. Unlike schooling, a subscription was neither compulsory nor rationed: the state broadcast to everyone, and uptake turned

[21] The 1981 census files report no district-level age composition, but the compulsory tier provides the test. Elementary schooling was essentially universal by 1981, so elementary pupils per 1,000 inhabitants proxy the school-age population share; they rise by 0.87% of the sample mean per percentage point of Muslim share (Conley $t = 10.6$), a larger relative surplus than the secondary-enrollment one (0.76%). The secondary surplus is therefore fully accounted for by the size of the school-age population.

on whether a household chose to pay to bring the state's voice into its home. The negative Muslim-share gradient, robust to the geographic and economic controls, is therefore difficult to attribute to provision; it reads as voluntary disengagement from state institutions. Railway density (1970) shows no significant association with Muslim population share (Figure A.25, Panel B), indicating that differential market access does not confound the supply-demand comparison. Additional educational and cultural outcomes are reported in Figure A.23 and are consistent with this pattern.

***Figure A.25.*** *Muslim Population Share, Media Subscriptions, and Railway Density (RD2D Sample)*




*Notes: OLS coefficients on Muslim population share (percentage) with 95% confidence intervals, within the RD2D bandwidth. Panel A: radio and TV subscribers per capita (1981, 1991), with no controls (black) and with geographic and economic controls (brown). Panel B: railway density (1970), with and without geographic controls. Conley (30 km) spatial HAC standard errors.*

## P Sharia Court RDD: Data Construction

The settlement-level analysis in Section 5.5 requires two links: each 1961 BiH census settlement to a geographic coordinate, and each coordinate to its 1921 district (the running variable). For the first, we match every 1961 *naselje* to its 1885 Habsburg counterpart (from a digitised Habsburg settlement cross-section) within its 1961 *opština*, using Jaro-Winkler string distance with optimal bipartite assignment; matches with JW $\leq 0.11$ are accepted, the harder cases (renamed or respelled places) are resolved by a targeted web search, and the *opština* centroid serves as a fallback. Names are normalised before matching: Cyrillic is transliterated to Latin, Habsburg-era ethnonym suffixes (e.g. *-turski*, *-srpski*) are stripped, and diacritics are folded. For the second, we dissolve the 1921 *srez* polygons into base districts and spatially join each settlement's coordinate to the district that contains it (nearest district for the few points outside any polygon). Pre-treatment covariates (elevation, the 1910 religion shares, the 1885 road indicator) are taken from the Habsburg cross-section by the same coordinate. The result covers over 90% of 1961 BiH settlements with a confident coordinate and district assignment.

The running variable is each base district's 1921 combined Muslim count (the *srez* together with its *grad*), from the 1921 census. A handful of city *gradovi* that had no district of their own - Doboj, Velika Kladuša, Kozarac, and Bosanska Kostajnica - are folded into their parent districts (Tešanj, Cazin, Prijedor, and Bosanski Novi respectively), whose census totals already include them, so no population is double-counted. All fifty-one districts enter the analysis: twelve fall below the 5,000-Muslim cutoff and lost their courts, while thirty-nine at or above the threshold retained them.

# Q Sharia Court RDD: Robustness

This appendix reports robustness checks for the sharia-court regression discontinuity estimates in Section 5.5.

We first test covariate balance: Table A.4 reports an RD at the 5,000-Muslim cutoff for each of twelve pre-treatment covariates - five naselje-level (elevation, the 1910 Orthodox and Catholic shares, 1910 ethnic fractionalization, and 1885 road access, tested with *opština*-clustered SEs) and seven srez-level (1895 Islamic/Orthodox/Catholic school counts, 1931 illiteracy for the total/men/women, and a post-treatment WWII check, tested on the 51-district panel). The five naselje-level covariates are imbalanced at the cutoff - lost-court districts sit about 0.4 of a standard deviation higher (125 m), with fewer Orthodox and more Catholics - and enter every headline specification as linear controls; the srez-level covariates, including 1931 near-pre-treatment illiteracy, are balanced. The 1910 Muslim share is the stratifying variable and is not balance-tested. The table's final row addresses a confound rather than a covariate: because the 1961 outcomes are measured well after the Second World War, we test whether wartime victimisation - WWII victims (1941-45) from the 1964 federal census of war victims (Savezni Zavod Za Statistiku, 1966), in the municipality-level digitisation of the Žrtvoslov project (Žrtvoslov Project / HRdemos, 2023), as a share of the 1931 population - is discontinuous at the cutoff. It is not (the estimate is statistically insignificant), so differential wartime losses are unlikely to explain the closure effects; and the post-war drive for universal literacy, if anything, would have narrowed rather than widened any earlier difference.

***Table A.4.*** *Covariate Balance at the 5,000-Muslim Cutoff*

| Covariate | $\hat{\tau}$ | S.E. | $p$ | $N$ below | $N$ above | $N$ clust | Mean (SD) |
|---|---|---|---|---|---|---|---|
| Elevation (m) | 124.481*** | (20.854) | <0.001 | 69 | 265 | 14 | 541.09 (304.23) |
| % Orthodox (1910) | −0.269*** | (0.081) | <0.001 | 169 | 320 | 21 | 0.44 (0.43) |
| % Catholic (1910) | 0.137*** | (0.013) | <0.001 | 337 | 350 | 30 | 0.20 (0.34) |
| Ethnic fractionalization (1910) | −0.123*** | (0.020) | <0.001 | 68 | 265 | 14 | 0.17 (0.20) |
| Road in 1885 | 0.057* | (0.031) | 0.063 | 421 | 459 | 44 | 0.19 (0.39) |
| Islamic schools (srez, 1895) | −2.593 | (3.335) | 0.437 | 12 | 10 | 22 | 12.20 (11.85) |
| Orthodox schools (srez, 1895) | −0.595 | (0.787) | 0.450 | 12 | 11 | 23 | 1.14 (1.31) |
| Catholic schools (srez, 1895) | −0.102 | (0.297) | 0.731 | 10 | 8 | 18 | 0.31 (0.76) |
| % illiterate (1931, total) | 2.762 | (6.774) | 0.683 | 12 | 12 | 24 | 73.26 (8.39) |
| % illiterate (1931, men) | 12.499 | (14.214) | 0.379 | 12 | 15 | 27 | 59.08 (12.60) |
| % illiterate (1931, women) | 4.427 | (6.155) | 0.472 | 11 | 8 | 19 | 86.75 (7.32) |
| WWII victims, % of 1931 pop. (post-treat.) | 2.129 | (5.469) | 0.697 | 8 | 7 | 15 | 9.64 (8.44) |

*Notes:* Each row is the Table 4 RD with the named pre-treatment covariate as the *outcome* (conventional rather than bias-corrected inference); a significant estimate is evidence of imbalance. The first five rows are naselje-level with *opština*-clustered SEs; the srez-level rows (1895 confessional schools, 1931 illiteracy, WWII victims) use the 51-district assignment panel, each district its own cluster. $N$ below / $N$ above are observations inside the bandwidth; $N$ clust is *opština* clusters (naselje rows) or districts (srez rows). The stratifying 1910 Muslim share is not balance-tested. The final row, below the rule, is a deliberately *post-treatment* check: WWII victims (1941–45, 1964 federal war-victims census, SIV64) as a share of the 1931 population; it does not differ significantly across the cutoff, so differential wartime losses are unlikely to explain the closure effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5 reproduces the headline tercile table (Table 4) with no covariate adjustment. The T2 women's-illiteracy effect that carries the result is present in both - larger, in fact, without controls - so it is not manufactured by the covariate adjustment; the controls bring it down to the headline estimate by absorbing the imbalance they correct for.

***Table A.5.*** *Tercile RD Estimates without Covariate Adjustment*

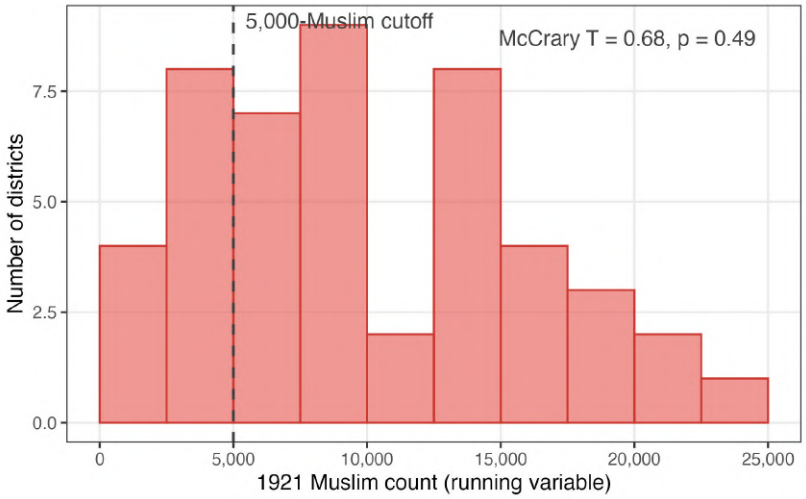
| | Illiterates per 100 inhabitants | | | Illiterates by age band, per 100 | | | |
|---|---|---|---|---|---|---|---|
| | Population | Men | Women | 10–19 | 20–34 | 35–64 | 65+ |
| *Pooled (all retained-court + lost-court naselja)* | | | | | | | |
| Estimate | −2.59* | −1.53 | −0.26 | −7.05 | −8.86*** | 6.63 | 3.07 |
| 95% CI | [−9.7, 0.3] | [−5.4, 0.5] | [−4.5, 1.6] | [−28.0, 6.9] | [−16.1, −4.8] | [−1.7, 9.9] | [−3.7, 6.4] |
| $p$ (robust) | 0.065 | 0.103 | 0.338 | 0.236 | 0.000 | 0.165 | 0.591 |
| Bandwidth | 1208 | 1187 | 1304 | 1252 | 1045 | 1686 | 2270 |
| Effective $N$ | 422 | 422 | 435 | 353 | 294 | 568 | 687 |
| *T1 (1910 Muslim share* $= 0$*)* | | | | | | | |
| Estimate | −3.14 | −0.31 | −3.40 | −5.72*** | −8.05** | 7.06 | 1.10 |
| 95% CI | [−12.3, 2.8] | [−4.1, 2.1] | [−10.5, 1.2] | [−12.1, −3.3] | [−21.8, −0.6] | [−3.4, 14.7] | [−7.5, 18.2] |
| $p$ (robust) | 0.221 | 0.540 | 0.118 | 0.001 | 0.038 | 0.219 | 0.417 |
| Bandwidth | 1634 | 1678 | 1382 | 1117 | 1221 | 2451 | 1385 |
| Effective $N$ | 259 | 298 | 265 | 121 | 216 | 397 | 258 |
| *T2 (1910 Muslim share* $\in (0, 0.64]$*)* | | | | | | | |
| Estimate | 6.29 | −0.71** | 7.98*** | 13.45* | −7.36*** | 6.46 | 1.67 |
| 95% CI | [−1.1, 7.9] | [−3.5, −0.0] | [3.9, 8.4] | [−0.7, 10.7] | [−12.3, −6.0] | [−5.7, 8.0] | [−8.5, 7.3] |
| $p$ (robust) | 0.141 | 0.046 | 0.000 | 0.088 | 0.000 | 0.743 | 0.876 |
| Bandwidth | 1668 | 1216 | 2372 | 2084 | 1161 | 1818 | 2353 |
| Effective $N$ | 156 | 109 | 211 | 187 | 87 | 190 | 203 |
| *T3 (1910 Muslim share* $> 0.64$*)* | | | | | | | |
| Estimate | −0.39 | −1.79 | 2.72 | −23.09 | 8.74 | 11.92*** | 9.29 |
| 95% CI | [−11.9, 5.9] | [−9.3, 3.0] | [−1.6, 4.7] | [−68.5, 14.8] | [−6.0, 16.9] | [3.3, 17.6] | [−2.8, 20.1] |
| $p$ (robust) | 0.511 | 0.319 | 0.333 | 0.206 | 0.353 | 0.004 | 0.138 |
| Bandwidth | 2093 | 2180 | 2420 | 1638 | 4621 | 2832 | 3941 |
| Effective $N$ | 106 | 106 | 108 | 85 | 227 | 133 | 173 |
| DV mean | 39.6 | 9.9 | 29.6 | 15.4 | 31.7 | 64.3 | 77.5 |
| DV SD | 13.5 | 6.1 | 8.8 | 16.0 | 17.3 | 17.2 | 17.8 |

*Notes:* As Table 4, but with *no* covariate adjustment (pooled and by 1910 Muslim-share tercile). Estimates are conventional local-linear coefficients; brackets are robust bias-corrected 95% CIs (Calonico *et al.*, 2014), centered on the bias-corrected estimate and so need not contain the conventional point; stars follow the robust $p$-value. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We next test for manipulation of the running variable: Figure A.26 reports a histogram of the 1921 Muslim count at the assignment-unit level (one observation per district). The Cattaneo-Jansson-Ma density test ($T = 0.68$, $p = 0.49$) finds no evidence of bunching at the 5,000 threshold, supporting the assumption that the cutoff was not manipulated. This test is on the running variable, not the outcome, so it does not depend on the choice of denominator.

Table A.6 re-estimates the T2 cell - the cell that carries the result - using a local quadratic polynomial ($p = 2$) in place of the linear fit, to absorb any smooth curvature in the running variable. The women's-illiteracy estimate stays positive and marginally significant (3.76, against the 5.54 linear headline), so the result is not

***Figure A.26.*** *McCrary Density Test at the 5,000-Muslim Cutoff*




*Notes: Histogram of the 1921 combined Muslim count per base district. Dashed vertical line: 5,000-Muslim cutoff. McCrary test statistic annotated.*

an artifact of functional form, though the quadratic is data-hungry in a cell this size.

***Table A.6.*** *Local-Quadratic Robustness ($p = 2$), T2 Cell*

| | Illiterates per 100 inhabitants | | | Illiterates by age band, per 100 | | | |
|---|---|---|---|---|---|---|---|
| | Population | Men | Women | 10–19 | 20–34 | 35–64 | 65+ |
| *T2 (1910 Muslim share* $\in (0, 0.64]$*)* | | | | | | | |
| Estimate | 4.08 | 2.12* | 3.76* | 6.08 | 2.08 | 2.45 | −1.09 |
| 95% CI | [−3.4, 8.9] | [−3.6, 0.3] | [−0.3, 8.0] | [−4.8, 8.1] | [−5.1, 2.7] | [−12.7, 5.3] | [−15.1, 4.3] |
| $p$ (robust) | 0.382 | 0.096 | 0.072 | 0.623 | 0.538 | 0.418 | 0.272 |
| Bandwidth | 2851 | 3479 | 2803 | 3140 | 2814 | 3238 | 3385 |
| Effective $N$ | 272 | 354 | 274 | 258 | 266 | 281 | 340 |
| DV mean | 39.6 | 9.9 | 29.6 | 15.4 | 31.7 | 64.3 | 77.5 |
| DV SD | 13.5 | 6.1 | 8.8 | 16.0 | 17.3 | 17.2 | 17.8 |

*Notes:* As Table 4, but fitting the T2 cell with a local *quadratic* ($p = 2$) instead of the linear polynomial (bandwidth re-selected). The women's estimate stays positive and marginally significant, so the result is not an artifact of functional form. Estimates are conventional local-linear coefficients; brackets are robust bias-corrected 95% CIs (Calonico *et al.*, 2014), centered on the bias-corrected estimate and so need not contain the conventional point; stars follow the robust $p$-value. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table A.7 re-evaluates the T2 cell under three standard-error specifications: HC (no clustering), *opština*-clustered (the headline), and a wild-cluster bootstrap at the 1921 district (9,999 Rademacher draws), the small-cluster method appropriate to the assignment unit. The point estimates and bandwidths are unchanged; only the inference varies. The women's-illiteracy effect is significant under *opština* clustering and marginally so under HC, but not under the district bootstrap, which pools the settlements into far fewer clusters and widens the interval - an honest small-cluster

caveat rather than a clean pass.

***Table A.7.*** *Cluster-Robustness for the T2 Cell*

| | Population | Men | Women |
|---|---|---|---|
| Estimate | 8.96 | 2.94 | 5.54 |
| S.E. (HC) | (3.69)** | (1.50) | (2.64)** |
| S.E. (*opština*) | (2.37)*** | (0.79) | (1.63)*** |
| $p$ (WCB, district) | 0.440 | 0.543 | 0.433 |
| Bandwidth | 2811 | 2274 | 2702 |
| Effective $N$ | 272 | 209 | 274 |

*Notes:* The T2 cell (1910 Muslim share $\in (0,\ 0.64]$). Point estimates and bandwidths are those of Table 4; only the inference changes. Three SE specifications: HC (no clustering), *opština*-clustered (the headline), and a wild-cluster bootstrap at the 1921 district (9,999 Rademacher draws), the small-cluster method appropriate to the assignment unit. The Estimate row carries no stars; the markers on each S.E. / $p$ row come from that row's own inference. The women's effect is significant under *opština* clustering and marginally so under HC, but not under the district bootstrap, which pools the settlements into far fewer clusters and widens the interval—an honest small-cluster caveat. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8 re-estimates the T2 cell's three illiteracy outcomes under triangular (headline), uniform, and Epanechnikov kernels. The women's effect stays positive and significant under all three kernels; population and men's illiteracy are less steady, consistent with the women's outcome being the one the design best supports. The four age cohorts are omitted here: near the cutoff the district-level running variable has too few distinct values to sign them stably.

The design's natural placebo is the zero-Muslim (T1) group. The 1929 rule is defined on a district's Muslim population and acts through a Muslim institution, so it can bite only where Muslims are present; the closure should therefore not *raise* 1961 illiteracy in settlements with no Muslims. In the harm direction it does not: for female illiteracy, the outcome that carries the result, the T1 estimate is negative rather than positive. It is not a clean zero, however: it is significantly negative, about $-5$ points ($p < 0.05$), a discontinuity in the zero-Muslim group where the mechanism cannot operate. Yet it does not account for the T2 result. T1 and T2 point in opposite directions ($-5$ against $+5.5$, $p < 0.001$), whereas a generic size-or-location confound at the threshold would move both the same way; and whatever lowers women's illiteracy where no Muslims live would, if it also reached Muslim-present settlements, only pull the T2 estimate toward zero, so the rise there is if anything understated rather than manufactured. The anomaly is moreover confined to women - population illiteracy in T1 is flat ($-0.7$) and men's shows no jump - whereas a real difference

***Table A.8.*** *Kernel Robustness, T2 Cell*

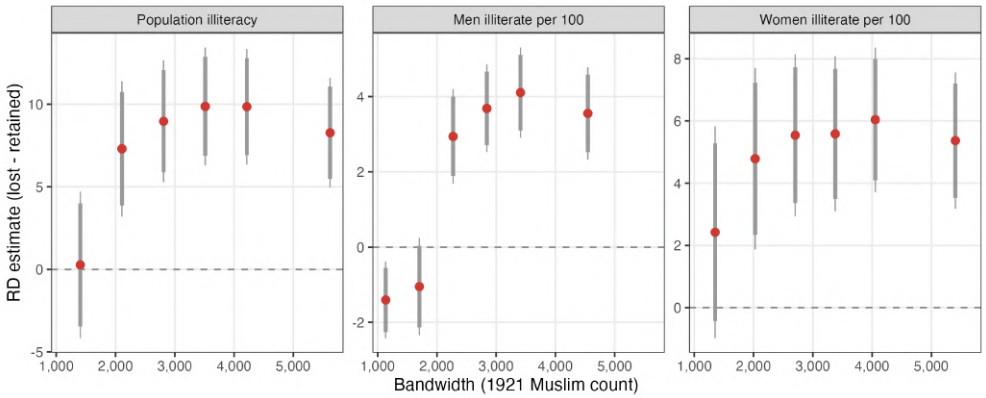
| | Illiterates per 100 inhabitants | | |
|---|---|---|---|
| | Population | Men | Women |
| *Triangular (headline)* | | | |
| Estimate | $8.96^{***}$ | $2.94$ | $5.54^{***}$ |
| 95% CI | $[2.9,\ 12.1]$ | $[-1.6,\ 1.5]$ | $[2.5,\ 8.9]$ |
| $p$ (robust) | $0.002$ | $0.949$ | $0.000$ |
| Bandwidth | $2811$ | $2274$ | $2702$ |
| Effective $N$ | $272$ | $209$ | $274$ |
| *Uniform* | | | |
| Estimate | $-1.08$ | $-1.71$ | $3.33^{**}$ |
| 95% CI | $[-2.7,\ 7.5]$ | $[-2.5,\ 0.9]$ | $[0.9,\ 7.6]$ |
| $p$ (robust) | $0.349$ | $0.330$ | $0.013$ |
| Bandwidth | $1049$ | $1105$ | $1684$ |
| Effective $N$ | $82$ | $82$ | $158$ |
| *Epanechnikov* | | | |
| Estimate | $10.21^{***}$ | $3.71$ | $6.49^{***}$ |
| 95% CI | $[3.8,\ 13.2]$ | $[-0.9,\ 2.3]$ | $[3.2,\ 9.9]$ |
| $p$ (robust) | $0.000$ | $0.403$ | $0.000$ |
| Bandwidth | $2282$ | $2139$ | $2184$ |
| Effective $N$ | $209$ | $203$ | $205$ |

*Notes:* As Table 4, but re-estimating the T2 cell's three illiteracy outcomes under the triangular (headline), uniform, and Epanechnikov kernels (bandwidth re-selected per outcome and kernel). Estimates are conventional local-linear coefficients; brackets are robust bias-corrected 95% confidence intervals (Calonico *et al.*, 2014), which are centered on the bias-corrected estimate and therefore need not contain the conventional point; stars follow the robust $p$-value. The women's effect stays positive and significant under all three kernels. Age cohorts are omitted: near the cutoff the district-level running variable has too few mass points to sign them stably. $^{***}\,p<0.01$, $^{**}\,p<0.05$, $^{*}\,p<0.10$.

between the districts on either side of the cutoff would not fall on women alone, which points to a chance result in a single cell rather than a systematic break. We read the T1 magnitude cautiously in any case: with a coarse running variable the MSE-optimal bandwidth, and hence the point estimate, is sensitive to individual district values, so its size is less informative than its sign. A scan over arbitrary placebo cutoffs is uninformative in this setting: with only fifty-one distinct district values, twelve below the cutoff, such a scan reflects the coarseness of the running variable rather than the validity of the threshold, and is suited to the finely-spaced population running variables for which it was developed.

Finally, Figure A.27 re-estimates the T2 cell's three illiteracy outcomes across bandwidths from half to twice each outcome's MSE-optimal window (the bias bandwidth scaled in proportion, so the optimal-bandwidth point reproduces the headline). The women's estimate is positive across the whole range, significant from the optimal window outward and weakening only at the narrowest windows; population illiteracy moves alongside it, while men's illiteracy is the unsteady one, reversing sign at the narrowest windows. The women's headline therefore survives this check.

***Figure A.27.*** *Bandwidth Sensitivity*




*Notes: RD estimate (lost − retained) for the T2 cell's three illiteracy outcomes - population, men, and women - across bandwidths from 0.5× to 2.0× each outcome's MSE-optimal $h$ (red points), with conventional 95% (thin) and 90% (thick) confidence intervals centred on the point. Same controlled specification as Table 4, opština-clustered. Women's illiteracy stays positive across the whole range, population illiteracy alongside it, while men's reverses sign at the narrowest windows.*

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