Supplementary Materials (Online)

A1 Ethnic settlement data

The HEG dataset covering ethnic settlement area is based on a candidate set of approximately 200 historical ethnic maps compiled from online map collections and leading libraries such as the British Library, Library of Congress, and the Blbliothèque Nationale de France. From this candidate set, we selected 73 high-quality maps with (a) high geographic resolution, (b) broad spatial coverage (i.e. depicting large subregions or the entirety of Europe), (c) authors of varying nationality, and (d) no obvious political biases.[16]

Practically all ethnic categories appearing on our maps refer to linguistic rather than religious or regional ethnic identity markers. That said, some maps differ in the level of linguistic granularity they encode and therefore need to be standardized for our purposes. To address this "grouping problem" of European ethnolinguistic identities, we match all raw linguistic map labels to the Ethnologue language tree (Lewis 2009) and construct a time-invariant master list of relevant ethnolinguistic groups by subsuming linguistically closely related labels from different maps under the linguistic node that occurs on the majority of maps that depict the respective language family.[17]

To get at temporal variation in specific groups' settlement areas, we combine the publication date of individual maps as well as hand-coded secondary data on the relatively few periods of large-scale ethnic change due to forced resettlement, genocide, or mass migrations. This information is used to code, for each group on our ethnic master list, the maps that are valid for a specific sub-period between 1816 and 1945.[18]

Finally, we draw on all maps belonging to a specific group-time period combination to construct a best-guess settlement polygon. Figure A1 illustrates this procedure for the Hungarian map period before WWII. The first step is to overlay the digitized multipolygons of all 12 maps that show the Hungarians. Second, we rasterize these polygons and calculate, for each raster cell, the share of maps that encode it as populated by Hungarians. The third and final step applies a 0.5 cutoff rule to construct a best-guess polygon that contains all cells that at least six maps regard as populated by Hungarians. These best-guess polygons may, of course, overlap, which indicates mixed settlements.

Any data on ethnic settlements covering as broad a geographic and temporal scope as 19th and 20th century Europe are prone to some measurement error. We address this challenge by pre-selecting only the highest quality maps, hand-coding periods of significant change, and com-

16. The publication of these maps range from the 1850s to 2019. For the present project, we restrict ourselves to the period 1816-1945.

17. If, for example, two maps contain the Bavarian dialect while twenty maps depict Germans, the Germans are listed as relevant group and subsume all dialects. In other cases, more disaggregate categories are chosen. Croats, Serbians, and Bosniaks appear on many more maps than does the aggregate South Slavic language family.

18. To address concerns that accurately reflecting temporal change in ethnic settlements comes at the cost of introducing endogeneity problems to our analyses, we run robustness checks only relying on the earliest available maps.

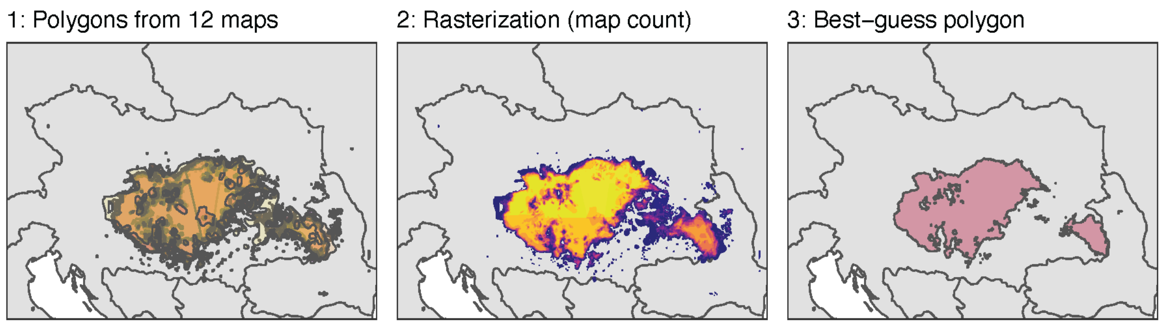


Figure A1: Constructing ethnic best-guess polygons: Hungarian example

bining information from multiple maps. These steps ensure a relatively accurate dataset and minimize concerns about systematic biases in our units of analysis.

Other concerns relate to endogenous ethnic settlement areas and sample selection. Ethnic geography may be affected by past conflict, nation-building policies, and other political forces. While there is no perfect solution to this issue, we run robustness checks using temporally stable grid cells as units of analysis.

Our historical maps might also miss small and extinct groups that were assimilated into broader national or linguistic categories. As a result, large and politically mobilized ethnicities are likely to be overrepresented in our sample. Since these groups are bigger and more likely to be active in politics, they can be expected to have a higher baseline risk of making secessionist claims or being involved in territorial conflict. If relevant, this selection issue should make it harder to identify effects on conflict and separatism.

A2 Validation of railway data

We validate the quality of the main spatial railway data using a set of hand-geocoded historical railway maps for Austria Hungary. We collected a total of 12 maps for 1855, 1864, 1869, 1870, 1876, 1881, 1884, 1885, 1991, 1995, 1901 from the Rumsay historical map collection.[19] Each map is georeferenced and its railway lines drawn with the help of contemporary OpenStreetMaps railroad data. This helps improving the precision of lines, and only in few cases additional lines needed to be drawn by hand.

As a first visual validation exercise we compare the main railway dataset (henceforth RShapes) to the hand-drawn Austro-Hungarian lines. The left-hand plots in Figure A2 overlay the two sets of lines for 4 years: 1855, 1870, 1884, and 1901. The comparisons suggest that

19. See https://www.davidrumsey.com/.

RShapes correctly identifies the main rail lines in Austria-Hungary. If anything, it somewhat underestimates the density of rail connections, especially in 1901.

As a second step we sample points on the hand-drawn lines circa every kilometer and estimate the average distance of these points to the nearest RShapes line, as well as computing the share of points that lie within a 5 and a 10-kilometer buffer around RShapes lines. These two metrics should give a quantitative measure of the two line sets' agreement. The right-hand plots in Figure A2 describe the points and buffers. The plot subtitles report that more than 80 percent of points are nested within 5 kilometers from the Austro-Hungarian lines, and more than 90 percent lie within 10 kilometers.

Figure A3 also provides the trends of these statistics over time. Plot A3a indicates that the distance of the rail lines contained in the two railway datasets is at its highest in 1864 with about 6 kilometers on average, and it decreases over time. As a result, the share of points along RShapes within 5 and 10 kilometers from the Austro-Hungarian lines increases over time.

Plots in Figure A3 plot the average distance between rail lines and the share of points on the hand-drawn lines that fall within 10 kilometers from RShapes lines. Both statistics show fairly low error rates. In particular, the share of points within the 10-kilometer buffer shows fairly high consistency over all observed years, as more than 75 percent of all points are within the buffer area.

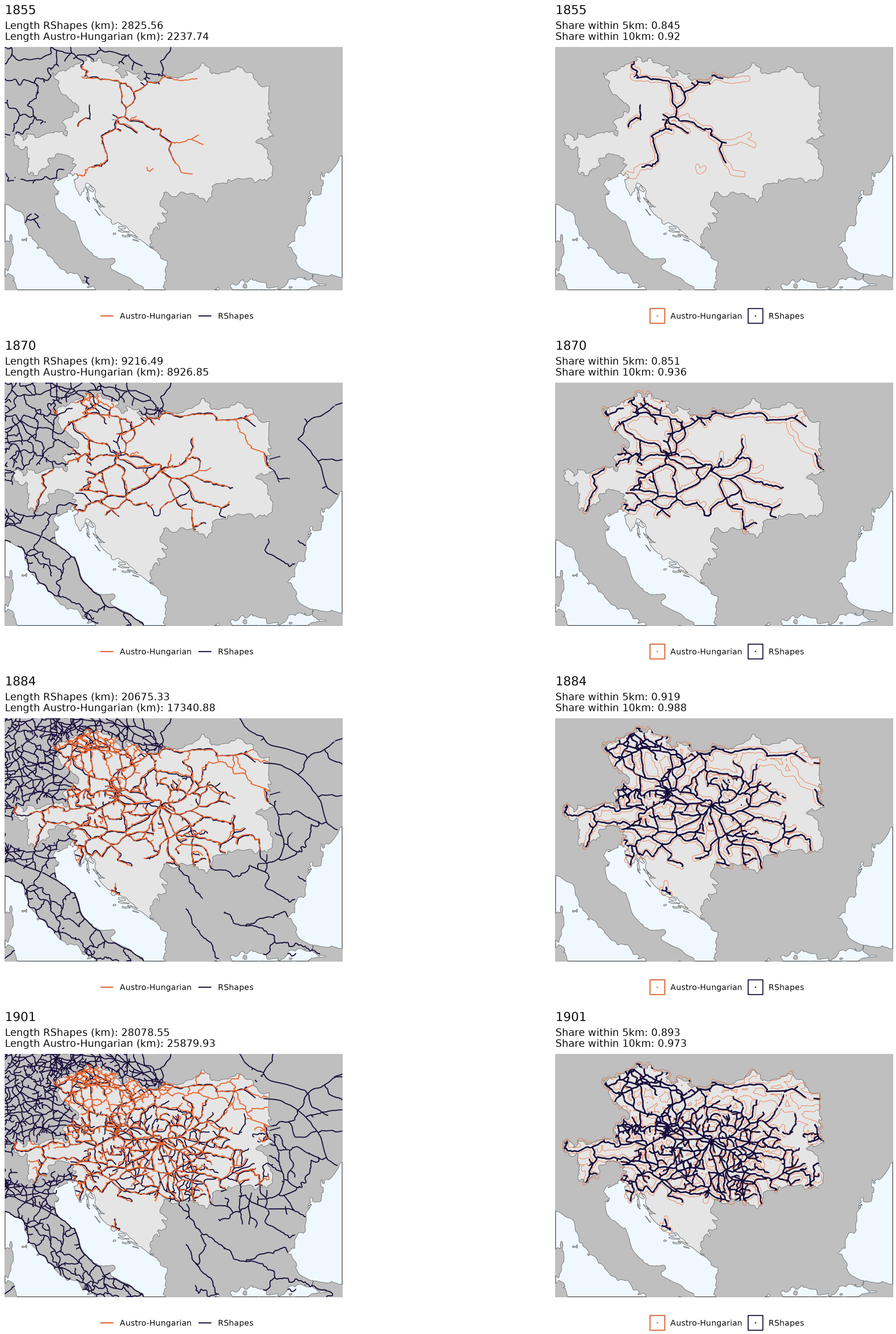
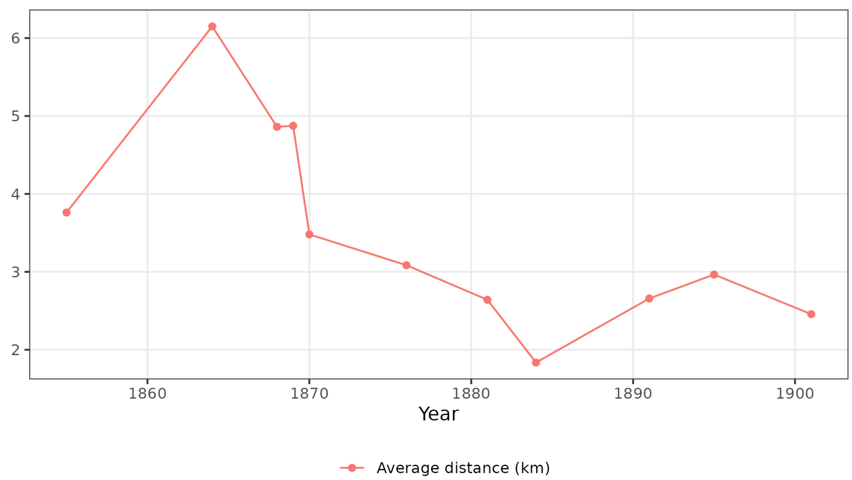
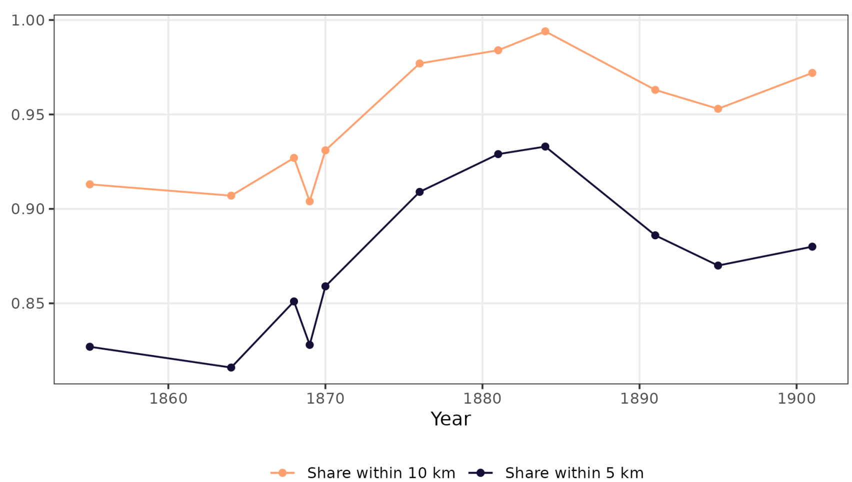


Figure A2: Comparison of RShapes and Austro-Hungarian railway data.



(a) Average distance between RShapes and Austro-Hungarian lines.



(b) Share of RShapes points within 5 or 10km from Austro-Hungarian lines.

Figure A3: Similarity of RShapes and Austro-Hungarian railway data over time.

A3 Descriptive statistics

Table A1: Descriptive statistics

	Min	Mean	Median	Max	Std. dev.
Combined outcome*	0.000	1.115	0.000	100.000	10.500
Successful secession*	0.000	0.131	0.000	100.000	3.613
First claim*	0.000	0.569	0.000	100.000	7.522
Civil war*	0.000	0.438	0.000	100.000	6.606
Rails (Y/N)	0.000	0.512	1.000	1.000	0.500
First railway year	1835.000	1870.176	1868.000	1921.000	19.739
National Market Access	-16.498	-4.545	-3.096	4.205	4.656
State Reach	0.000	344.898	381.408	421.340	93.884
Internal Connectivity	0.000	188.765	194.527	205.476	19.981
Ling. Dist to Core	0.087	0.736	0.684	1.000	0.281
Pop. Share Core Group	0.056	0.525	0.443	0.995	0.229
Group Population (log)	7.876	12.432	12.409	17.209	1.743
GDP per capita (log)	6.460	7.816	7.772	9.302	0.469
Fiscal Capacity (VDEM)	-3.034	1.252	1.504	3.178	0.828
Liberal Democracy (VDEM)	0.027	0.336	0.218	0.951	0.282

* Note: The outcome is multiplied by 100 to improve legibility.

A3.1 Descriptives of outcome variables

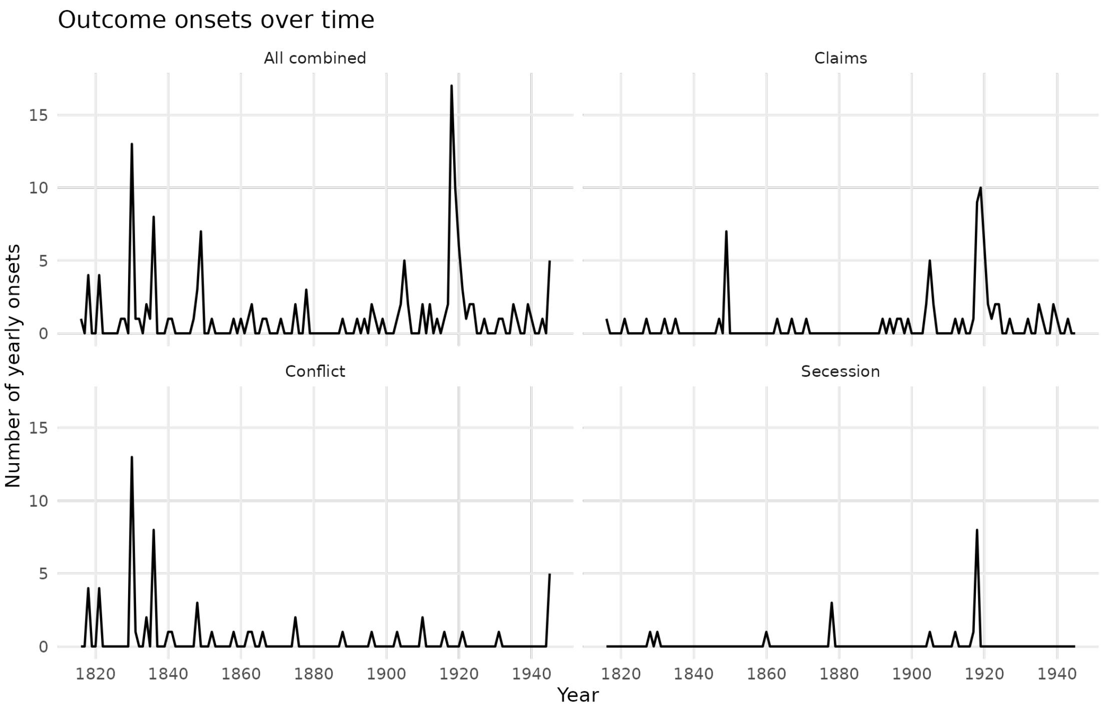
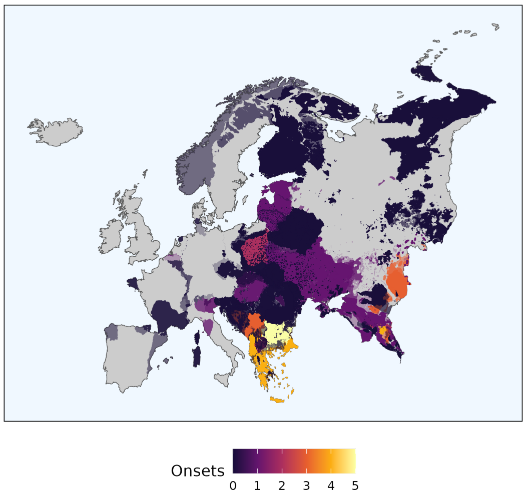
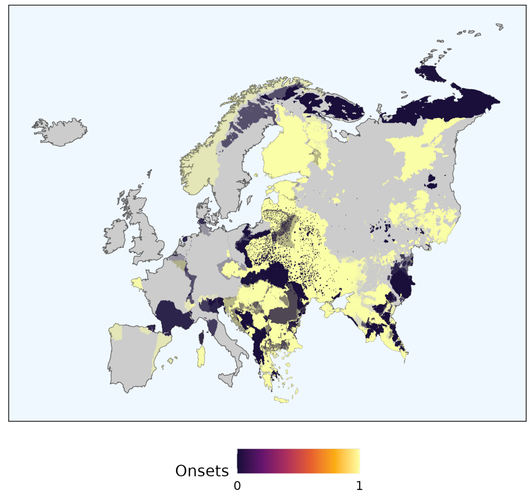


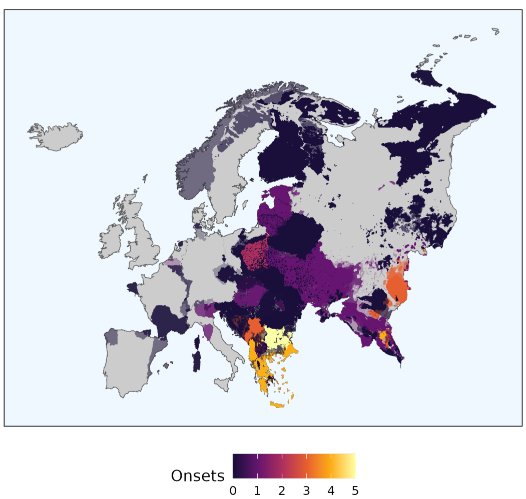
Figure A4: Temporal trends for outcome variables.



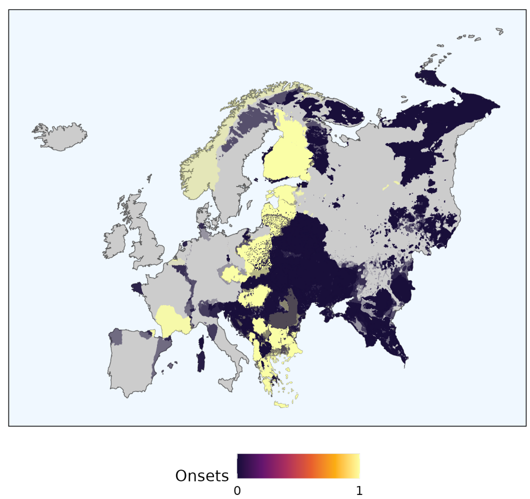
(a) All combined



(b) Claims



(c) Conflict



(d) Secession

Figure A5: Spatial maps of outcome variables.

A4 Network proxies for state reach, market-based and internal connectivity

We first divide Europe in grid cells with approx. 10 km resolution, each of which is associated with a population estimate for the year 1830 (Goldewijk, Beusen, and Janssen 2010). We then build a planar graph using cell centroids as vertices and straight connecting lines to their eight queen neighbors as "footpath" edges, which we overlay and intersect with the railroad lines for each year. On the resulting graph, we can query the estimated minimum travel time between any two points in Europe for any year covered by our data.

To derive the necessary edge-weights, we assume a speed of 6 km/h on "footpath" edges,[20] and 60 km/h for rail travel. The latter is close to the maximum average long-distance speeds achieved by steam-powered trains in 19th century France, Italy, and the United Kingdom. While not entirely accurate, we currently lack more detailed data on changes in speeds over time and, even more challenging, variation in speeds by railroad line.[21]

The **state reach** proxy is calculated as a population-weighted mean of travel times between all cells in an ethnic segment and the cell that contains the respective national capital, using the 1830 population estimates. It is then inverted,[22] to ensure that high values point to high levels of state capacity (i.e., low travel times).

The **national market access** proxy follows Donaldson and Hornbeck (2016) and is defined as the average cell-level travel time to cities with more than 10'000 inhabitants in 1800 located in the same country.[23] Travel times to different cities are weighted by market size (i.e., city population, from Buringh 2021) and distant cities are weighted down by a trade elasticity parameter based on travel times using parameters estimated by Donaldson and Hornbeck (2016). In particular, we compute the market access of a grid cell i as

$$MA_i = \sum_{c=1}^{C} w_c \, \delta(i,c)^{-3.8}$$

, where c indexes cities in the same state as i with a population size (weight) w_c located at a distance of travel time $\delta(i,c)$ from grid cell i.[24]

We again aggregate cell-level market access values to ethnic segment-years by taking the population-weighted average across all cells contained in a segment polygon. Note that market

20. Approximately the speed of horse cart travel and walking.

21. Introducing temporal variation in speeds (within reasonable limits) would not, generally, affect comparisons in our analytical framework much as most of their effect would be soaked up by our year and country-year fixed effects. Introducing measures of track quality would likely improve the precision of our measures and avoid attenuation bias.

22. Using the following formula: $x_{inv} = min(x) + max(x) - x$

23. The measure is closely related to Schürmann and Talaat's (2002) measure of peripherality.

24. We add 1 hour to all travel times to avoid division by 0.

access and travel time to capital do not only vary due to local railway construction within specific segments but also as a result of rails built elsewhere that increase the overall connectivity within national networks.

Finally, the **internal connectivity** proxy is constructed as the average travel time between any two inhabitants of a ethnic segment, again based on the 1830 population data. It is then inverted,[25] to ensure that high values point to high levels of state capacity (i.e., low travel times).

The use of time-invariant population data for the main analysis limits the precision of our measures, it has the strong advantage that demographic developments caused by factors *other* than railroads do not affect our analysis. For example, all three measures of national market access, state reach, and internal connectedness can change due to changes in local demography, changes which are likely driven by a host of factors that are not related to railway networks and the economic modernization they bring about, but may cause conflict, thereby biasing our results. We do, however, conduct a set of analysis using time-variant population data from Goldewijk, Beusen, and Janssen (2010, for grid cells) and Buringh (2021, for cities) to construct all three measures. The results robust to this change and are presented and discussed in Appendix A10.

A5 Data on nationalist claims

This data collection effort is inspired by the Self-Determination Movements dataset (Sambanis, Germann, and Schädel 2018), and Wimmer and Feinstein (2010). The latter code the foundation year of the first nationalist organization for 145 territories that were independent states in 2001. This restriction to territories that eventually became independent involves obvious selection issues which we overcome by using ethnic segments as the relevant unit of analysis.

Our coding covers all ethnic segments in historical Europe and further distinguishes the type of nationalist claims that specific nationalist organizations make. Nationalist organizations are defined as formal and non-personalistic organizations that make political claims in the name of an ethnic group. Importantly, the definition excludes cultural organizations such as national reading groups, which have been important for the expansion of literacy and national identity among rural communities, but which do not make explicit political claims (Darden 2009). We distinguish between central and peripheral nationalist claims. Central claims are either claims for minority representation in the central government or majority demands for exclusive control of the state. Peripheral claims include non-core group demands for national independence, more autonomy within the existing state, or irredentism, i.e. unification with a co-ethnic homeland abroad. For the present project, we restrict the focus to national independence and regional autonomy claims by non-core groups, as these appear as the theoretically most relevant category.

25. Using the following formula: $x_{inv} = min(x) + max(x) - x$

A6 Railroad simulation

Intuition: Our simulation procedure starts from the intuition that, in the absence of local policing or external military goals or economic motivations, state-provided railroad networks would aim to maximize the connectedness within a country's population. We furthermore assume that each state has a fixed budget of railroad kilometers to build every year. On that basis, we build a planar graph that covers all of Europe. This network connects each cell of a population raster with a resolution of .5 decimal degrees (≈50km at the equator and less as one moves North) to its eight nearest neighbors.

Up to 1833, the network only consists of foot- and carriage paths on which one can travel 6km/h. Our simulation algorithm, described in full detail in Appendix A6, now "builds" the observed railroad mileage for every consecutive year as upgrades to these baseline paths, increasing the allowed travel speed to 60km/h for every edge transformed into a railroad line. In doing so, the algorithm heuristically places railroad lines such that they minimize the average travel time between any two inhabitants of the same country. The crucial input to this algorithm is a time-*in*variant estimated population grid for the year 1830 from Goldewijk, Beusen, and Janssen 2010.

The resulting simulation is driven by the spatial interaction of four factors. First, the continental population distribution in 1830 ensures that most rails are build around and between population centers. We choose to temporally fix the population distribution at its estimate for 1830 to preclude that changes in the population distribution – which might be caused by observed railroads or other proximate causes of conflict – affect and potentially bias our simulation. Second, (changing) country borders affect which areas are central or peripheral to states' networks. Third, states observed annual railroad budget affects the evolution of the railroad over time. And fourth, the stock of simulated railroads build in previous years affects where the next set of lines are being built. Our use of stringent ethnic segment and (state-)year fixed effects control for each of these factors.

The simulation is different from others' in the literature (Bogart et al. 2022; Faber 2014; Jedwab, Kerby, and Moradi 2017) in that it does not presuppose any fixed set of *nodes* that must be connected to the railway network but instead lets the algorithm find appropriate nodes to connect. While the latter approach works well for identifying the local effects of localities' access to the railway, it leaves the overall structure of the network fixed and is therefore not suitable for our approach. In addition, we are interested in the spatial evolution of the network across many years which allows us to instrument changes in railway access within ethnic segments *over time*. In contrast, the above mentioned studies focus on identifying networks' structure at a given point in time, which does not allow for capturing dynamic evolution across more than two periods.

Technical details: We simulate railroad networks following closely the approach developed by Müller-Crepon, Hunziker, and Cederman (2021). We thus assume that states that invest in railroad infrastructure minimize the following objective function in any given year t:

$$LOSS = \frac{1}{I^2} * \sum_{j=0}^{I} \sum_{i=0}^{I} time_{j,i}, \tag{A1}$$

where $i, j \in I$ denote the inhabitants of the territory controlled by a given state who are separated by travel time $time_{i,j}$. In simple words, states aim to minimize the average travel time within their population.

To capture the pre-railroad population distribution of Europe, we turn to *estimates* in 1830 from the HYDE 3.1 data (Goldewijk, Beusen, and Janssen 2010). This estimate is derived from broad, macro level population and urbanization estimates by country (e.g., Maddison 2010), subnational census data where available, and various geographic datasets. While there is a risk that the cross-sectional differences in population are reversely affected by future railroads since part of the data is back-projected, our use of time-*invariant* population data makes it very unlikely that this would spoil our time-*variant* simulations.

Railroad investments in any state and year are constrained by the mileage of railroads we observe being built in that year in any given state territory in our Rshapes data. Because our network is much coarser and straighter than observed railroads, we deflate that budget by a factor of 2. Each railroad line has the same quality, as we lack information on variance on that dimension.

Railroads are built by upgrading the edges of a pre-determined network of foot- and carriage paths. Given computational constraints in the repeated computation of the loss function (Eq. A1), we adjust the resolution of this baseline network to amount to .5 decimal degrees. The simulation algorithm proceeds sequentially in the following manner:

Algorithm:

1. For each state observed in t, starting at $t = 1834$, crop the Europe-wide network with all roadroads hitherto simulated to that state's territory. If the state's railroad budget for t is positive:

    (a) If no simulated railroads exist yet in the state, draw 10 seed vertices V_s with a probability proportional to their population. Sample one incident edge per vertex V_s and upgrade it to become a railroad "seed edge" and part of the collection of built lines E_b. Subtract length of built lines from budget.

    (b) Select all neighboring edges of E_b, evaluate their impact on $LOSS$ and keep 10 most promising edges as E_p.

(c) Upgrade edge $e \in E_p$ that minimizes *LOSS*. Select neighboring edges of e that have not yet been upgraded and add to E_p. Update $B_q = B_q - length_e$.

(d) Repeat step (c), and, in every 10th round, step (b), until budget B_q for a given state in year t is spent.

2. Move to the next year, t = t+1, until arriving in 1922, the last year covered by our railroad data.

A7 Choice of estimators

This paper analyzes a setting in which the construction of railways can be analyzed as a non-reversible treatment with staggered adoption and a control group mostly composed of not-yet-treated units. The econometric literature in political science and economics identified several challenges to traditional estimation techniques in this type of settings and proposed several estimators that address these challenges. We address this literature and its empirical implications by estimating comparable models from estimators that make different assumptions and estimation choices in order to assess the robustness of our findings.

Our baseline model is the traditional two-way fixed effects (TWFE) estimator, originally proposed to estimate average treatment effects on the treated in settings with contemporary treatment adoption (Angrist and Pischke 2009), and recently criticized for producing biased estimates in settings with staggered adoption (e.g. Goodman-Bacon 2021; Sun and Abraham 2021; Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021). All alternative solutions to the TWFE estimator address i) the problematic comparisons implicit to TWFE estimates, which sometimes use already-treated units as control observations for later cohorts, and ii) intransparent and sometimes counterintuitive weights given to cohort-specific treatment effect estimates in TWFE, whereby some units' estimated effect is given negative weight. (For an overview of the literature, see Roth et al. (2023) and Chaisemartin and D'Haultfœuille (2022).)

We choose two alternatives to TWFE that we believe represent the best choice for our empirical setup. First, we use the two-stage DiD estimator proposed by Gardner (2021) and implemented in the `did2s` R package (Butts and Gardner 2021). The intuition of the method consists in using the residualized control units (after partialling out unit and time fixed effects, and the necessary control variables) to impute the counterfactual outcomes for the treated units in a first stage. The second stage regresses the observed and the imputed outcome variables on the treatment indicator with a simple linear regression. A major benefit of the `did2s` package is that it allows to interact the main treatment variable with other factors to study heterogeneous effects. Therefore, estimates based on the two-stage DiD are used in all the main results in the main paper, as well as the main robustness test.

Second, we employ the `fect` package proposed by Liu, Wang, and Xu (2024). It follows a similar imputation approach to Gardner (2021), yet with significant differences. To begin with,

Table A2: Alternative panel data estimator: `fect` package

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
Rails (Y/N)	1.622***	1.625**	1.625**	1.385*
	(0.455)	(0.541)	(0.505)	(0.685)
Civil war history	−6.789***	−6.789***	−6.789***	−6.945***
	(1.598)	(1.550)	(1.981)	(1.398)
Time since civil war	−0.095	−0.095	−0.095	−0.104
	(0.095)	(0.088)	(0.105)	(0.089)
Time since ind. or aut. claim	0.027***	0.027*	0.027**	0.027
	(0.008)	(0.012)	(0.010)	(0.022)
Method	FE	IFE	MC	CFE
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country-Year FE	No	No	No	Yes

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. Method acronyms: 'FE' = two-way fixed effects; 'IFE' = interactive fixed effects; 'MC' = matrix completion; 'CFE' = complex fixed effects. Segment clustered standard errors in parentheses. + *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

it allows for different ways to impute counterfactual outcomes with interactive fixed effects and matrix completion models, which borrow from the fields of computer science and factor analysis. Additionally, `fect` computes the period-wise unit treatment effects as simple differences in means between observed and imputed outcomes, making fewer assumptions about effect linearity. Finally, the `fect` approach is more robust to temporal effect spillovers that might bias TWFE estimates (Liu, Wang, and Xu 2024).

Table A2 reports average treatment effect estimates from four models similar to the main results in Table 1. Model 1 uses a specification in which the counterfactual outcomes of treated units are predicted based on the trends of control units net of fixed effects for segments and years. Models 2 and 3 respectively use an interactive fixed effects method similar to generalized synthetic controls (Xu 2017), and a matrix completion method (see Athey et al. 2021) to predict counterfactual outcomes. Both methods are more flexible than TWFE in capturing heterogeneous temporal trends in the control group, and therefore might produce better counterfactuals. Model 4 uses a complex fixed effects estimator that allows to add country-year fixed effects on top of segment and year fixed effects, thereby resembling more Columns 2 and 4 in Table 1. Across all models, we obtain consistently similar positive and statistically significant estimates in line with the main results. Moreover, in line with Table 1, estimates with country-year fixed effects in Model 4 are smaller in magnitude than the ones with segment and year fixed effects.

Finally, we note that our data structure,does not allow the use of alternative estimators that require the presence of never treated units (of which there are exceedingly few in our setting) such as the ones introduced by Sun and Abraham (2021), Goodman-Bacon (2021), and

Wooldridge (2022) and implemented by McDermott (2023) and Butts and Gardner (2021), nor those limited to two time periods (Sant'Anna and Zhao 2020).

A8 Robustness tests

Results with country-year fixed effects

Event study

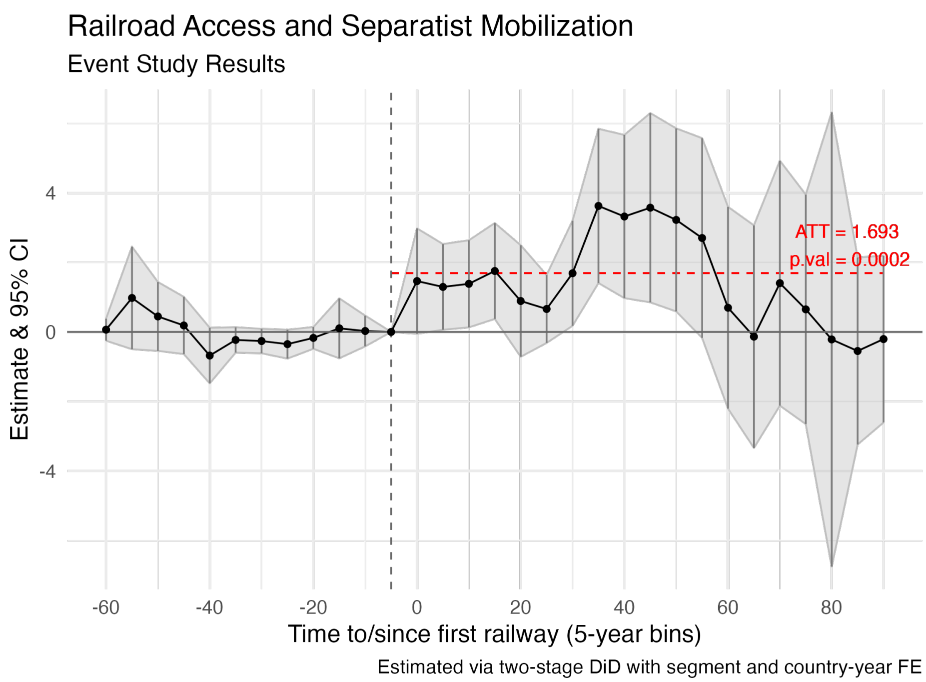


Figure A6: Event study plot
(ATT estimates based on Column 4 in Table 1)

Instrumental variable analysis

Table A3: Instrumenting Railroads: Country-Year FE

	Rails (Y/N)	100 × Separatism		
	First Stage	OLS	Reduced Form	Second Stage
Rails (Y/N, simulated)	0.279***		0.904**	
	(0.064)		(0.318)	
Rails (Y/N)		1.111**		
		(0.364)		
Rails (Y/N, instrumented)				3.242*
				(1.259)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
First Stage F	18.781			18.781
Mean DV	0.512	1.115	1.115	1.115
Observations	13 007	13 007	13 007	13 007

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Sample definition

Table A4: DiD Models: Drop Cases After First Onset

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
Rails (Y/N)	1.247**	0.965**	0.893**	0.924**
	(0.386)	(0.366)	(0.302)	(0.333)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	1.037	1.037	0.941	0.9
Observations	8679	8679	7650	6667

Notes: The unit of analysis is the ethnic segment year. State-leading segments, segments smaller than 2000 sqkm, and those with past secessionsit civil war and claims for independence or autonomy dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table A5: DiD Models: Dropping Never-Treated Units

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
Rails (Y/N)	1.664***	0.702*	4.616***	4.477***
	(0.430)	(0.340)	(1.055)	(0.938)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	1.215	1.215	1.168	1.217
Observations	11 114	11 114	9759	7479

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Limiting sample to treatment variation

The main independent variable *Rail (Y/N)* is based on a dynamic railway network between 1834 and 1922. In our main models, we allow outcomes to unfold past 1922 to capture longer-time effects of railways construction that we would miss by censoring the outcome with the treatment variation. However, as an additional robustness test this section provides a complete set of results in which the data stops in 1922, including the DiD and event-study models, mechanism, and heterogeneity analysis.

Table A6: DiD estimates: Railroads and Separatism (1816-1922)

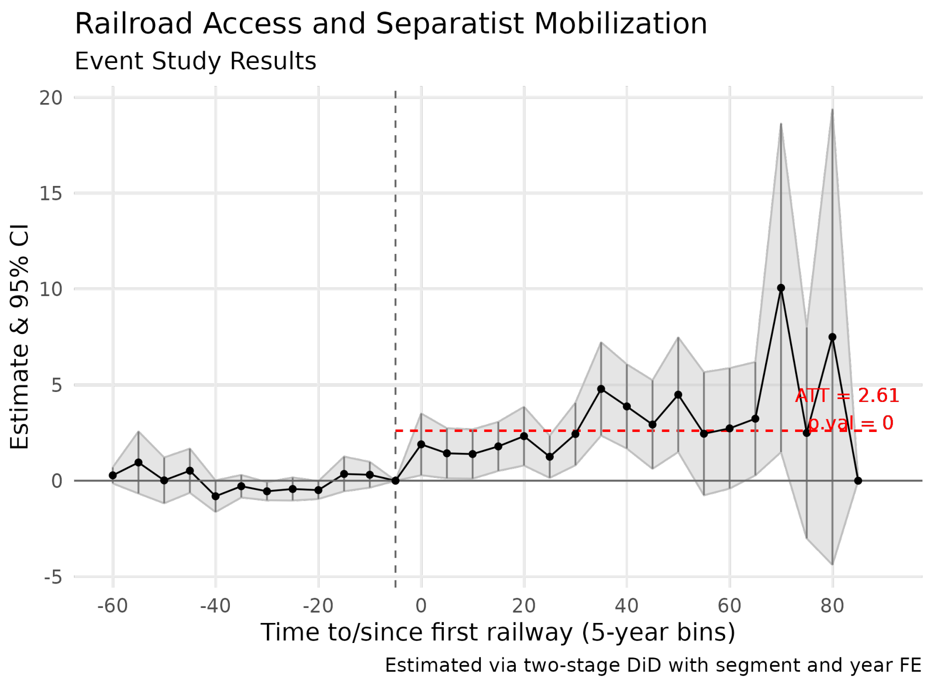
	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
Rails (Y/N)	1.472***	1.057**	2.610***	2.179***
	(0.366)	(0.352)	(0.593)	(0.489)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	1.214	1.214	1.156	1.141
Observations	10 379	10 379	9951	8415

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7: Network Structure and Causal Mechanisms (1816-1922)

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.141			0.017
	(0.101)			(0.088)
State Reach		−0.008*		−0.009**
		(0.003)		(0.003)
Internal Connectivity			0.018*	0.019**
			(0.007)	(0.007)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean DV	1.241	1.214	1.214	1.241
Observations	10 072	10 379	10 379	10 072

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



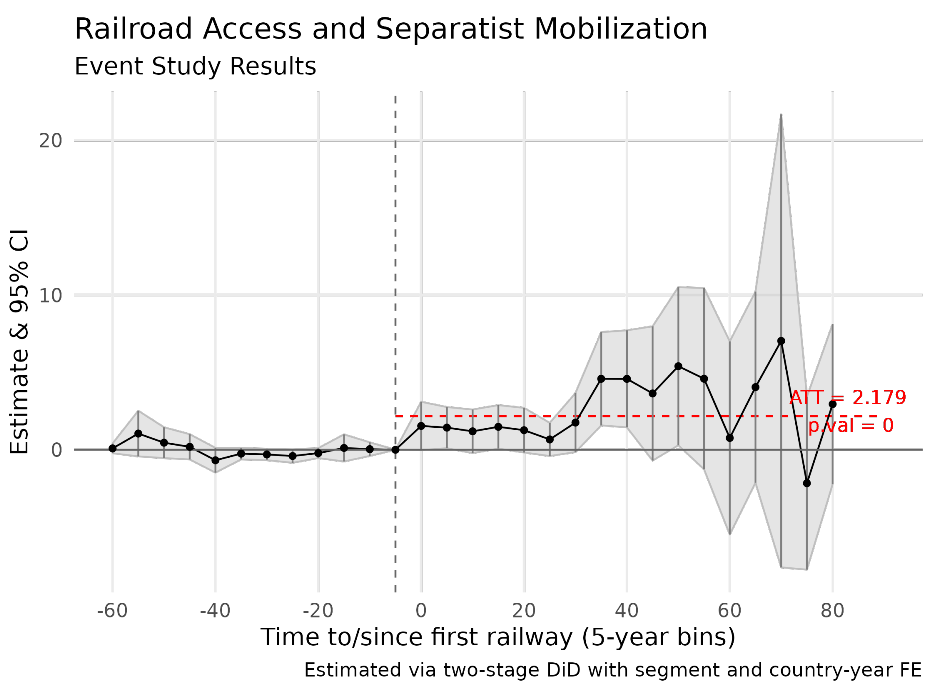
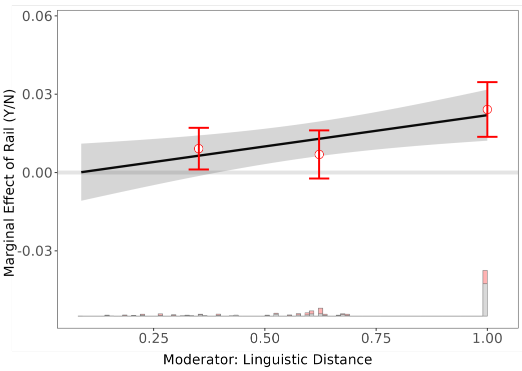
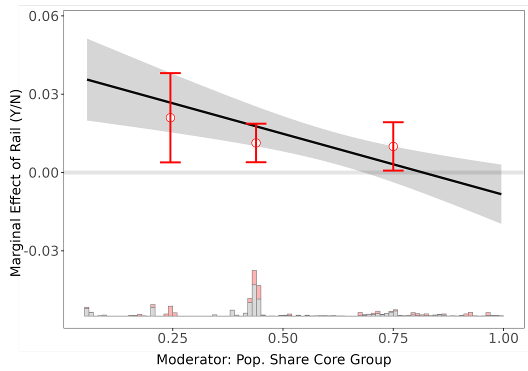


Figure A7: Event study plots (1816-1922)
(ATT estimates based on Columns 3 and 4 in Table A6)

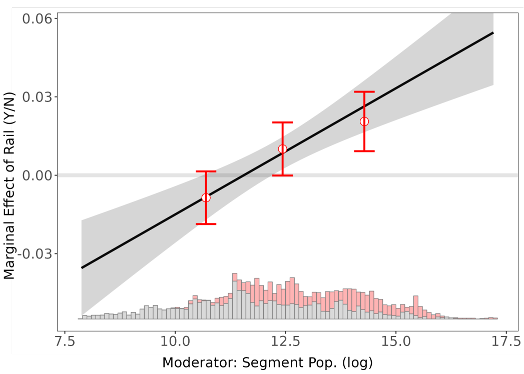
Figure A8: Marginal Effect Plot & Binning Estimates (1816-1922)



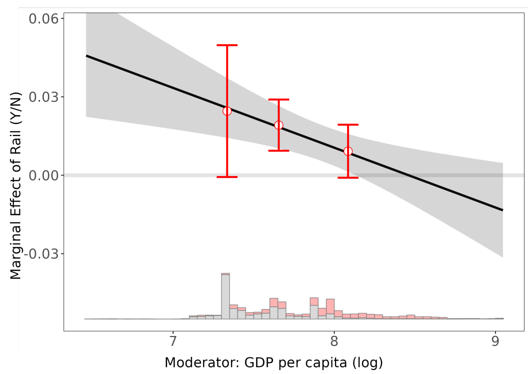
(a) Linguistic distance



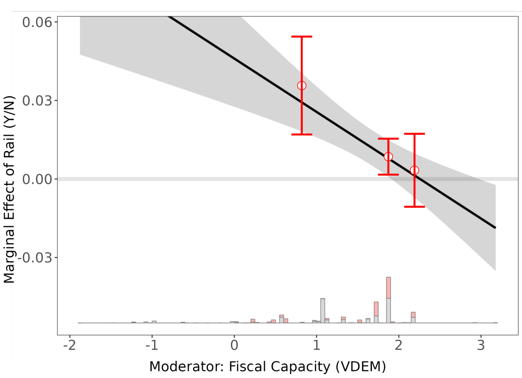
(b) Share core group



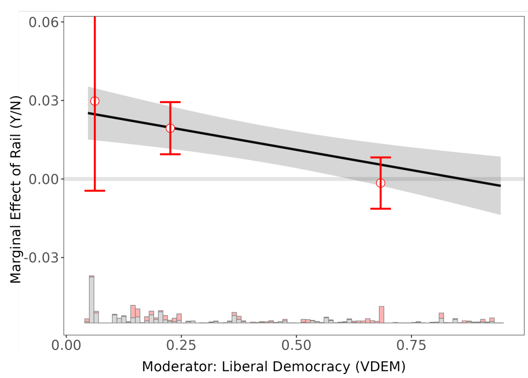
(c) Segment population



(d) Per capita GDP



(e) Fiscal capacity



(f) Liberal democracy

Disaggregating the separatism outcome

Table A8: Railroads and Secession (1816-1945)

	100 × Secession			
	(1)	(2)	(3)	(4)
Rails (Y/N)	0.023	−0.051	0.296***	0.304***
	(0.079)	(0.091)	(0.071)	(0.090)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	0.131	0.131	0.145	0.122
Observations	13 007	13 007	11 711	9818

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A9: Railroads and Separatist Conflict (1816-1945)

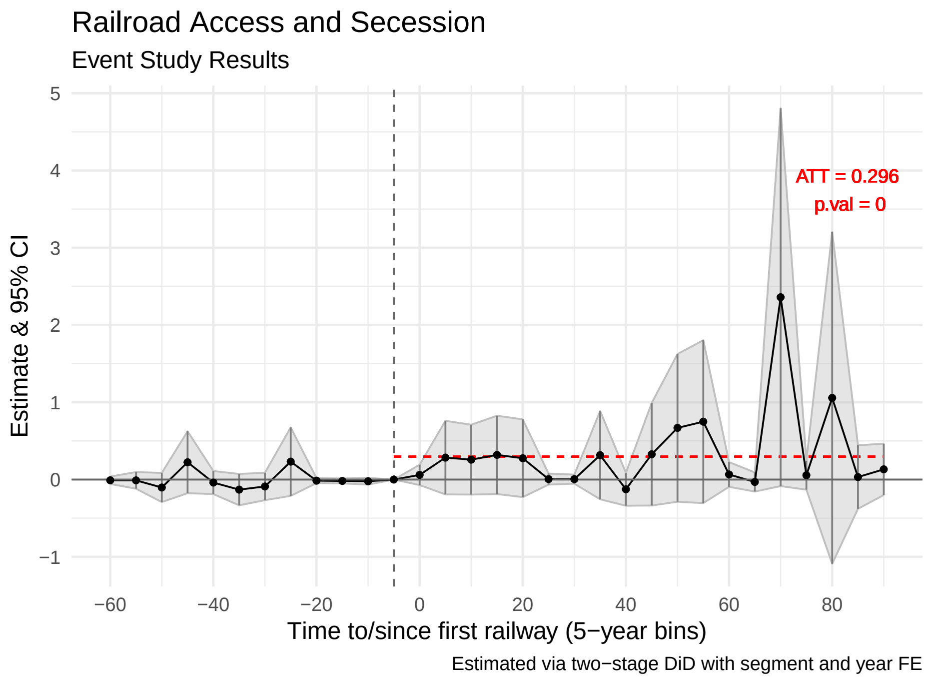
	100 × Terr. CW			
	(1)	(2)	(3)	(4)
Rails (Y/N)	0.832***	0.517**	1.168***	0.850**
	(0.216)	(0.190)	(0.340)	(0.261)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	0.438	0.438	0.478	0.519
Observations	13 007	13 007	11 711	9818

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A10: Railroads and Separatist Claims (1816-1945)

	100 × Independence or Autonomy Claim			
	(1)	(2)	(3)	(4)
Rails (Y/N)	0.625*	0.611+	0.659*	0.583+
	(0.282)	(0.327)	(0.296)	(0.345)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	0.569	0.569	0.478	0.458
Observations	13 007	13 007	11 711	9818

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



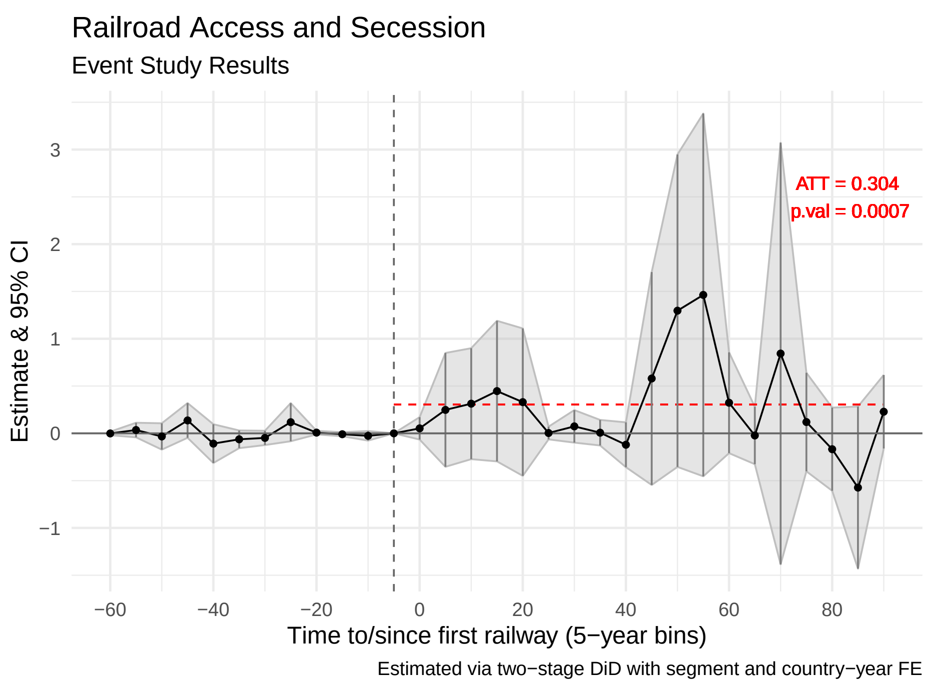
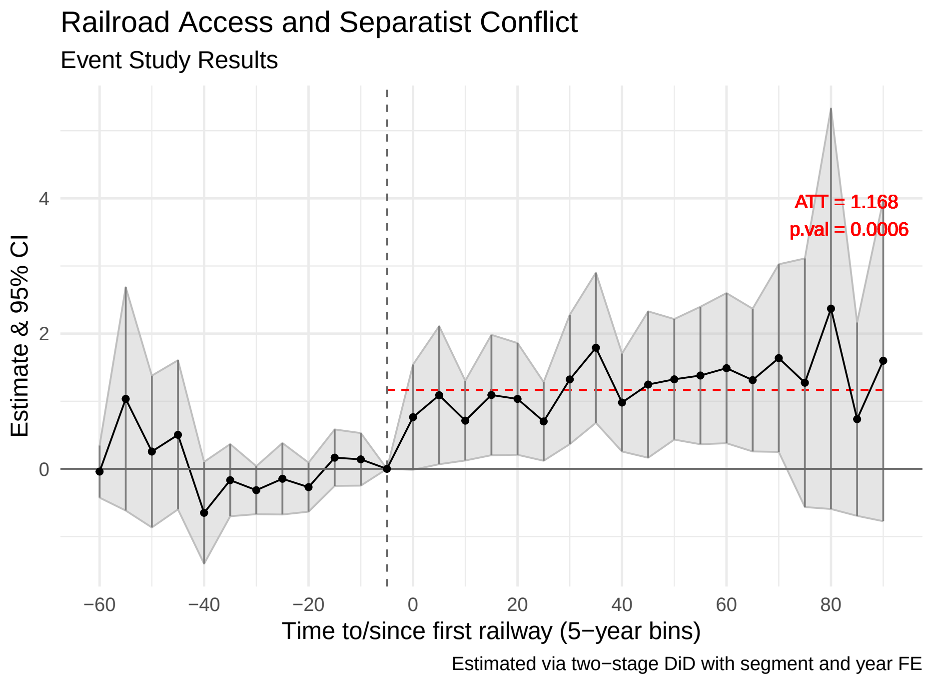


Figure A9: Event study plots
(ATT estimates based on Columns 3 and 4 in Table A8)



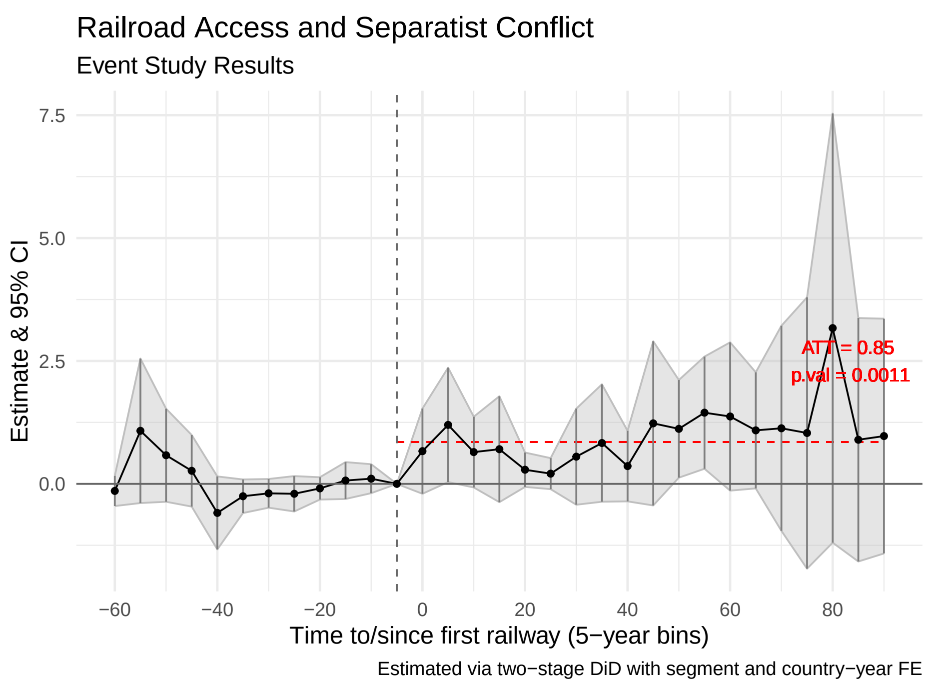
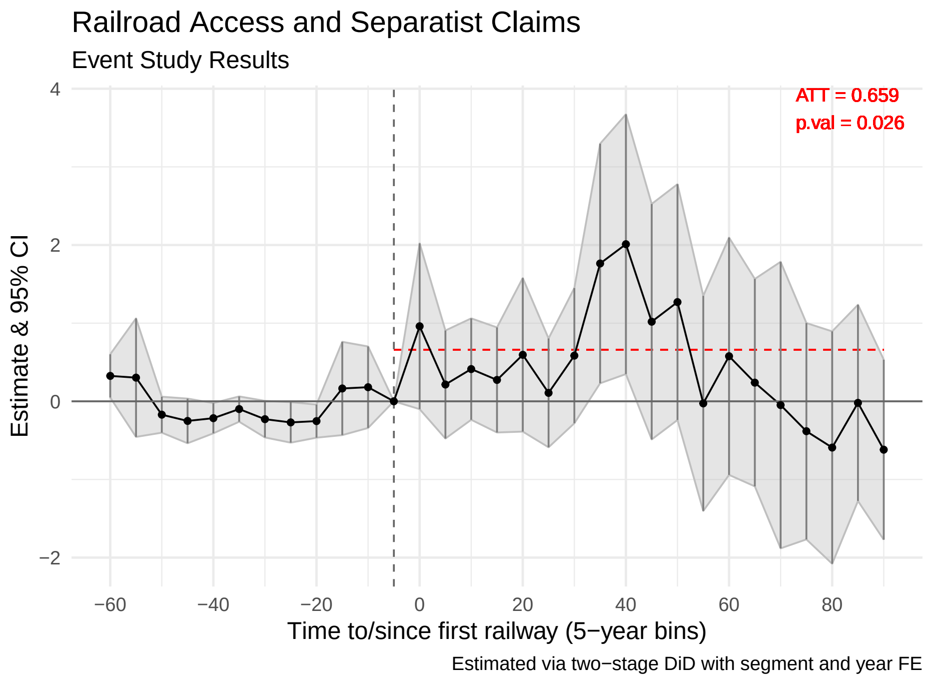


Figure A10: Event study plots
(ATT estimates based on Columns 3 and 4 in Table A9)



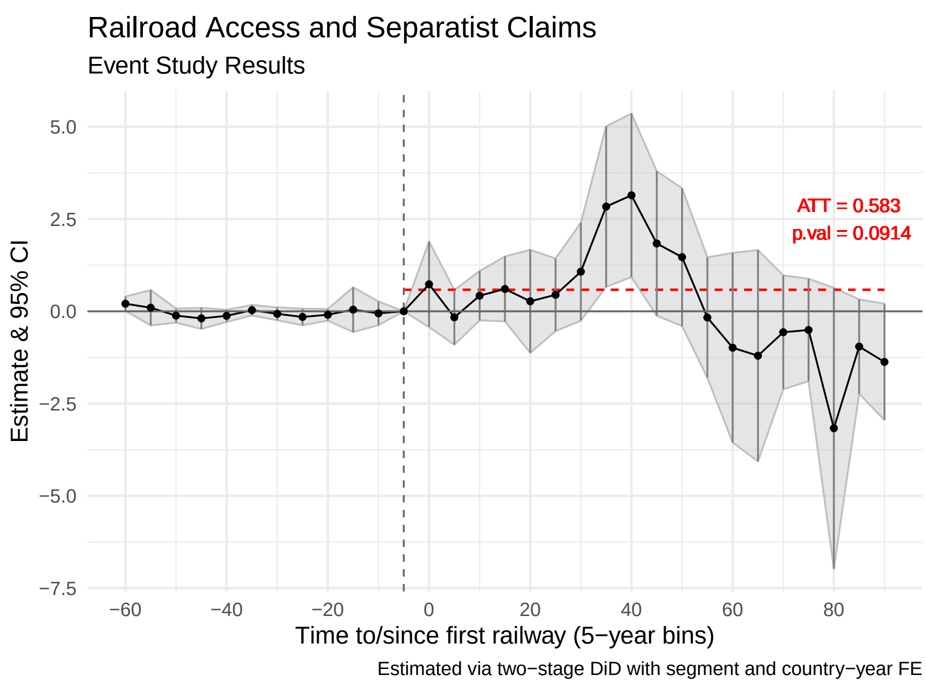


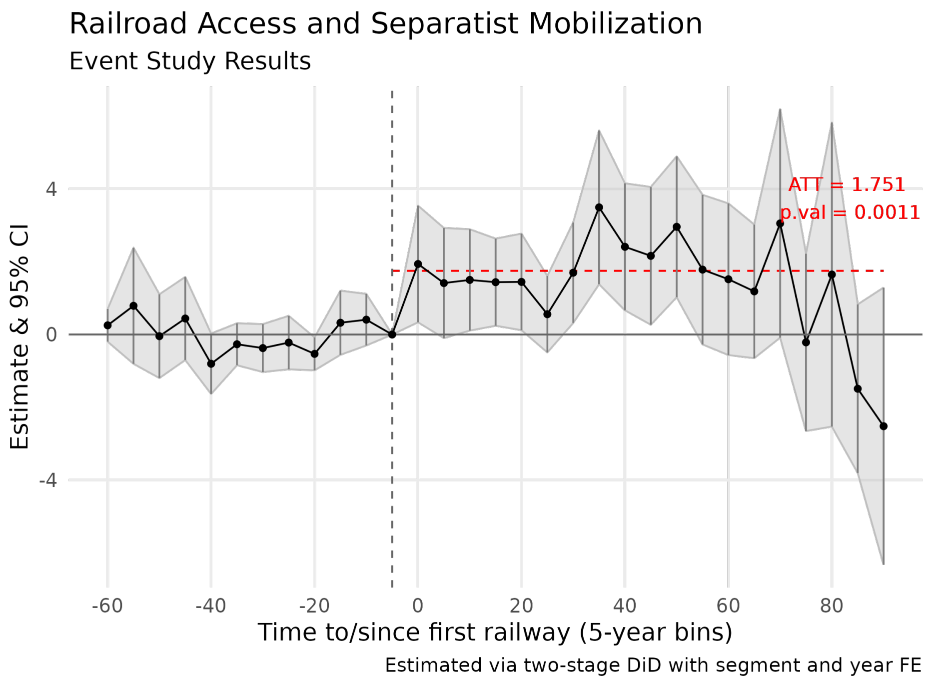
Figure A11: Event study plots
(ATT estimates based on Columns 3 and 4 in Table A10)

A8.1 Including irredentism

Table A11: Railroads and Separatism or Irredentism (1816-1945)

	100 × Secession, Terr. CW or Claim (incl. Irredentism)			
	(1)	(2)	(3)	(4)
Rails (Y/N)	1.567***	1.157**	1.751**	1.596**
	(0.388)	(0.368)	(0.536)	(0.495)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes
Estimator	TWFE	TWFE	2S-DiD	2S-DiD
Mean DV	1.199	1.199	1.127	1.12
Observations	13 007	13 007	11 711	9818

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



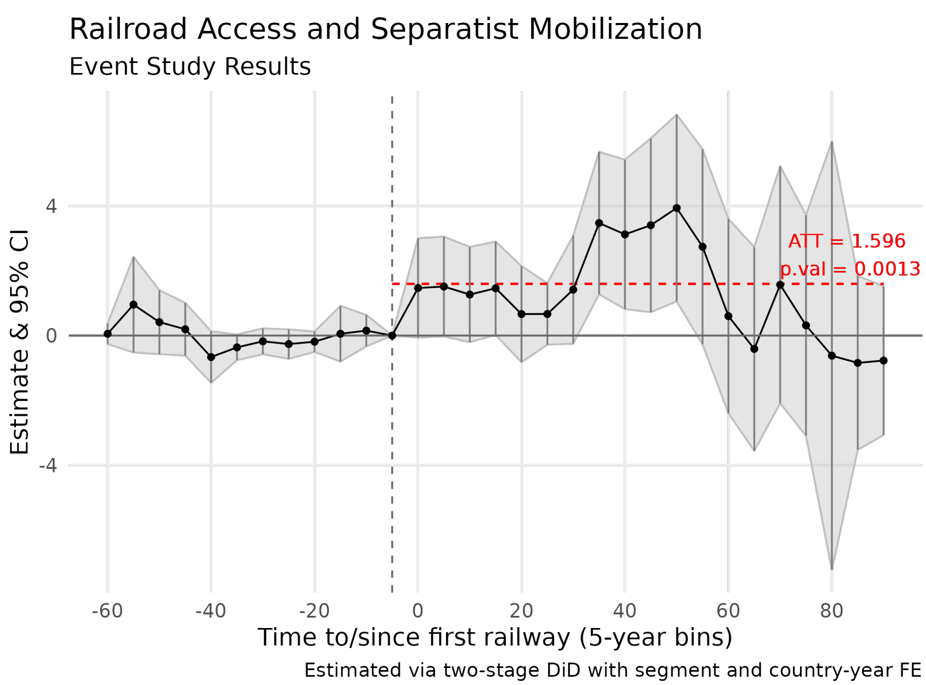


Figure A12: Event study plots
(ATT estimates based on Columns 3 and 4 in Table A11)

A9 Conditional effects: regresssion table

Linear interactions

Table A12: Separatism: Interaction Models

	100 × Secession, Terr. CW or Claim					
	(1)	(2)	(3)	(4)	(5)	(6)
Rails (Y/N)	0.493	3.484***	−10.291***	16.278***	2.761***	2.331***
	(0.611)	(0.817)	(2.103)	(4.308)	(0.629)	(0.506)
Rails × Ling. Dist to Core	1.373+					
	(0.820)					
Pop. Share Core Group		0.505				
		(2.328)				
Rails × Pop. Share Core		−3.834**				
		(1.202)				
Group Population (log)			−0.141			
			(0.255)			
Rails × Group Pop.			0.908***			
			(0.170)			
GDP per capita (log)				0.922		
				(1.033)		
Rails × GDP p.c.				−1.882***		
				(0.533)		
Fiscal Capacity (VDEM)					−0.229	
					(0.254)	
Rails × Fiscal Cap.					−0.912**	
					(0.339)	
Liberal Democracy (VDEM)						0.141
						(1.309)
Rails × Lib. Dem.						−2.317**
						(0.818)
Segment FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean DV	1.115	1.115	1.115	1.134	1.134	1.146
Observations	13 007	13 007	13 007	12 788	12 788	12 649

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A10 Additional mechanism analyses

Table A13: Network Structure (Country-Year Fixed Effects)

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.091			−0.021
	(0.104)			(0.102)
State Reach		−0.012***		−0.013***
		(0.003)		(0.003)
Internal Connectivity			0.014+	0.012+
			(0.007)	(0.007)
Segment FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Mean DV	1.131	1.115	1.115	1.114
Observations	12 643	13 007	13 007	11 652

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The mechanism estimates based on measures of the structure of railroad networks remain consistent when we use time-variant population data to measure segments' average market access, state reach, and internal connectivity. Time-variant population data increases measurement precision, it risks bias from "baked-in" omitted variables that affect demographic developments. Results with year (Table A15) and country-year fixed effects (Table A16 show stable effects of state reach and internal connectivity. Counterintuitively, the effect of national market access turns positive and statistically significant when using only year fixed effects, a finding which is not robust to country-year fixed effects. This suggests its potential origin in bias introduced by the time-varying population data which the country-year fixed effects can partially account for.

Table A14: Network Structure (Standardized Coefficients)

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.664+			−0.004
	(0.387)			(0.351)
State Reach		−0.742**		−0.773**
		(0.248)		(0.253)
Internal Connectivity			0.309*	0.328*
			(0.140)	(0.132)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean DV	1.131	1.115	1.115	1.131
Observations	12 643	13 007	13 007	12 643

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. All explanatory variables are standardized. *+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table A15: Network Structure and Causal Mechanisms with Time-Variant Population Data

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	0.141*			0.239**
	(0.069)			(0.071)
State Reach		−0.009**		−0.010***
		(0.003)		(0.003)
Internal Connectivity			0.015*	0.022**
			(0.007)	(0.007)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean DV	0.963	1.115	1.115	0.963
Observations	11 732	13 007	13 007	11 732

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. *+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table A16: Network Structure and Causal Mechanisms with Time-Variant Population Data and Country-Year FEs

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.076			0.052
	(0.101)			(0.103)
State Reach		−0.013***		−0.012***
		(0.003)		(0.003)
Internal Connectivity			0.014+	0.012+
			(0.007)	(0.007)
Segment FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes
Mean DV	0.963	1.115	1.115	0.963
Observations	11 732	13 007	13 007	11 732

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. *+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table A17: Network Structure and Causal Mechanisms with 5-year Leads

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.072			0.092
	(0.084)			(0.081)
State Reach		−0.008**		−0.009**
		(0.003)		(0.003)
Internal Connectivity			0.017**	0.020***
			(0.006)	(0.006)
Δ National Market Access $t_{+5} - t_0$	−0.037			0.109
	(0.122)			(0.124)
Δ State Reach $t_{+5} - t_0$		−0.007		−0.010+
		(0.005)		(0.006)
Δ Internal Connectivity $t_{+5} - t_0$			0.049	0.058
			(0.039)	(0.040)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean DV	1.002	1.115	1.115	1.002
Observations	11 771	12 110	12 110	11 771

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. *+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table A18: Network Structure and Causal Mechanisms with 5-year Leads and Country-Year FEs

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access	−0.012			0.131
	(0.095)			(0.101)
State Reach		−0.011***		−0.013***
		(0.003)		(0.003)
Internal Connectivity			0.014*	0.016*
			(0.006)	(0.006)
Δ National Market Access $t_{+5} - t_0$	0.118			0.209
	(0.164)			(0.182)
Δ State Reach $t_{+5} - t_0$		0.000		−0.002
		(0.007)		(0.008)
Δ Internal Connectivity $t_{+5} - t_0$			0.036	0.039
			(0.042)	(0.042)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean DV	1.002	1.115	1.115	1.002
Observations	11 771	12 110	12 110	11 771

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A19: Network Structure: With and without Log-transform

	100 × Secession, Terr. CW or Claim			
	(1)	(2)	(3)	(4)
National Market Access (log)	−0.027	0.016		
	(0.076)	(0.102)		
State Reach (log)	−1.137**	−1.193**		
	(0.394)	(0.454)		
Internal Connectivity (log)	0.743*	0.594+		
	(0.289)	(0.313)		
National Market Access			0.056	0.024
			(0.046)	(0.041)
State Reach			−0.008**	−0.012***
			(0.003)	(0.003)
Internal Connectivity			0.016*	0.013+
			(0.007)	(0.007)
Segment FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Country-year FE	No	Yes	No	Yes
Observations	12 643	12 643	13 007	13 007
Mean DV	1.131	1.131	1.115	1.115

Notes: The unit of analysis is the ethnic segment year. State-leading segments and segments smaller than 2000 sqkm dropped. All models control for the number of past conflicts and peace years indicators. Segment clustered standard errors in parentheses. *+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

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