

The Violent Origins of Political Capture: Evidence from Romanian Collectivization

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Research Proposal

Motivation

Until 1918 the territory of present-day Romania was divided between the Habsburg Empire and the Old Kingdom, and the border between them follows the Carpathians for much of its length. Studies of imperial borders show that they can still be seen in trust, corruption and political attitudes today (Grosfeld and Zhuravskaya, 2015; Becker et al., 2016), but they measure legacies in the present and say little about the intervening period, in which a communist state expropriated rural capital through arrests, deportations and the elimination of wealthy peasants (*chiaburi*) during the collectivization campaign of 1949-1962. Archival aggregates point to large differences in the intensity of that campaign. A December 1961 report on arrests of peasants in the early 1950s recorded 34,738 cases, with regional totals ranging from 16,146 in the Hunedoara region to 98 in Ploiești and 99 in Bacău (Iordachi and Dobrințu, 2014). This regional variation is visible in Table 1. The proposal, hence, asks two questions. First, did the imperial division shape how violently the state could expropriate rural capital? Second, do the imperial division, intensity and type of repression that followed shape political capture after 1989?

Related Literature and Gap

The first strand of literature concerns historical borders and the legacies of empires. Becker et al. (2016) use a geographic regression discontinuity at the former Habsburg border in several Eastern European counties and find that Habsburg affiliation raises trust in, and lowers corruption of, local courts and police, supported by falsification tests with moved borders. Grosfeld and Zhuravskaya (2015) find with a spatial regression discontinuity that some partition-era differences in Poland faded while others, such as religiosity, persisted, and Dell (2010) shows that a colonial forced-labour boundary still shapes consumption and land tenure. Pop-Eleches (2007) shows across countries that historical legacies circumscribe post-communist regime trajectories. These studies measure the persistence of institutions, but not how inherited institutions shaped the state's capacity to coerce under authoritarianism.

Literature studying political legacies of state violence is relevant, too. Rozenas, Schutte and Zhukov (2017), using archival data on deportations in western Ukraine, find that communities exposed to more intense deportation now vote less for pro-Russian parties, so violence lowers long-run support for the perpetrator's heirs. Naumenko (2021) estimates, with district-level data, that collectivization explains up to 52 percent of excess deaths in the 1933 Ukrainian famine. Another branch of literature touches upon terrain: Nunn and Puga (2012) show that rugged terrain protected populations from slave raiders, which suggests that ruggedness also raises the cost of state coercion.

TABLE 1: HISTORICAL ARRESTS AND COLLECTIVIZATION (1959–1960)

	Obs.	Mean	Std. Dev	Min	Median	Max
Panel A: Full Sample Averages						
Peasant Arrests Index	16	50.79	135.85	0.05	4.40	538.79
Chiaburi Arrests Index	16	37.01	118.94	0.05	1.29	480.33
Collectivization Rate	16	43.56	26.61	12.00	35.50	97.00
	Old Kingdom			Habsburg		
	Obs.	Mean	Std. Dev	Obs.	Mean	Std. Dev
Panel B: Comparison by Imperial Legacy						
Peasant Arrests Index	8	7.15	11.10	8	94.40	187.00
Chiaburi Arrests Index	8	5.30	9.34	8	68.70	167.00
Collectivization Rate	8	50.20	33.30	8	36.90	17.50
	Plains (TRI < Median)			Mountains (TRI > Median)		
	Obs.	Mean	Std. Dev	Obs.	Mean	Std. Dev
Panel C: Comparison by Geography						
Peasant Arrests Index	8	26.90	53.50	8	74.70	188.00
Chiaburi Arrests Index	8	11.10	17.20	8	63.00	169.00
Collectivization Rate	8	57.40	31.20	8	29.80	10.50

Notes: Arrest data are from 1959, and collectivization rates from 1960. Arrest indices measure arrests per 10,000 rural inhabitants using 1956 census counts. Panel B divides the 16 historical regions along the pre-1918 imperial border, while Panel C splits regions at the median Terrain Ruggedness Index (TRI).

Romanian collectivization itself is well documented in detail by historians (Kligman and Verdery, 2011; Iordachi and Dobrinu, 2014), but the quantitative evidence I found in my master’s thesis (2026) is limited to regional totals compiled from *Securitate*-based figures (SRI, 1994; Ivănescu, 2015), which cover 16 regions (Table 1). To my knowledge, no study assembles commune-level repression data for Romania or links it to post-1989 politics. This proposal contributes in three ways. It connects the imperial division to the mechanics of communist coercion and to modern capture, whereas the border literature measures the persistence of institutions in the present. It distinguishes selective violence against wealthy peasants from arbitrary violence against other peasants, a distinction that matters for the predictions of the violence-legacy literature. And it links the commune-level record to the clientelist capture documented in my thesis (2026), so that the origins of capture are studied instead of assumed.

Setting and Hypotheses

Collectivization proceeded in three phases, 1949-1953, 1954-1956, and 1957-1962. Between September 1948 and November 1949 the *Securitate* arrested 23,597 people, of whom 10,152 were peasants and 2,655 were *chiaburi*, so most arrested peasants were not classed as wealthy (Iordachi and Dobrinu, 2014). *Chiabur* lists existed for virtually every commune (Kligman and Verdery, 2011). In 1950 the 58 counties were replaced by 28 regions and 177 *raions* (Law no.5 of 1950). The regions were reduced to 18 in 1952 and to 16 by 1968, when counties were restored (Law no.2 of 1968). Because the campaign was administered through these units, their boundaries are candidate sources of variation in intensity, which the data can test.

For the first question (see [Motivation](#)), I organize predictions around the cost of coercion, which should rise with terrain ruggedness, following Nunn and Puga (2012), and fall with how legible households and landholdings were to the state. Furthermore, the imperial division could act through three channels. Habsburg-era institutions such as land registries may have made holdings more legible, the ethnic and religious composition of the two territories differed and infrastructure may have lowered the cost of reaching villages. If these channels operate, repression should be higher on the Habsburg side but also be dampened by ruggedness. However, these channels are difficult to disentangle given that Habsburg lands are now the most rugged parts of present-day Romania. Nevertheless, if I find that neither the border nor ruggedness matters, repression reflects local resistance or the zeal of local officials, which the commune data can examine.

For the second question, the imperial division may affect capture through repression or through other legacies such as trust and the organization of local politics, so the analysis separates the two. Three hypotheses about repression are tested (see [Table 2](#)). First, under elite elimination, arrests of *chiaburi* removed the rural notables on whom local patronage networks could later be built, so PSD control of local executives is lower where *chiabur* arrests were high. Secondly, under backlash, following Rozenas et al. (2017) and treating the PSD as the heir of the communist apparatus, indiscriminate violence lowers support for it, so PSD support is lower where arrests of middle and poor peasants were high, too. The two hypotheses operate on different margins — the supply of local patrons in the first hypothesis and demand among voters in the second one. Third, under no legacy, differences reflect selection of where violence occurred. A finding of the first hypothesis would show that selective violence reduced the supply of local patrons, a finding of the second would support the theory that violence reduced demand for the heirs of its perpetrators, and a finding of both that the two margins reinforce each other.

TABLE 2: TESTABLE IMPLICATIONS OF THE CONCEPTUAL FRAMEWORK

Outcome Variable	Elite Elimination	Popular Backlash	No Legacy
<i>Supply Margin: Rural Notables</i>			
PSD Mayoral and Executive Control	—	0	0
<i>Demand Margin: Electorate</i>			
PSD Legislative Vote Share	0	—	0

Notes: (+) indicates a higher incidence or positive effect, (-) indicates a lower incidence or negative effect, and (0) indicates no effect or no change.

Empirical Strategy

The two questions need different sources of variation, so they are answered with different designs, after repression has been measured at the commune level.

Commune-level repression is built from *Securitate* and county archive records (arrests, deportations, show trials), classified by social category and campaign phase and divided by the 1956 census population. The regional totals compiled from *Securitate*-based figures (SRI, 1994; Ivănescu, 2015) cover only 16 regions and cannot be used for estimation, but they serve as a validation benchmark against which commune counts summed within a region are compared. As a second intensity measure, I use the share of households on *chiabur* lists, which exist for virtually every commune (Kligman and Verdery, 2011) and remain available where arrest records are incomplete.

The first design asks whether inherited institutions shaped repression. Let c be a commune, d_c its signed distance to the pre-1918 border (positive on the Habsburg side), D_c an indicator for $d_c > 0$, and R_c repression per 1,000 inhabitants.

$$R_c = \alpha + \tau D_c + \beta_1 d_c + \beta_2 D_c d_c + X_c' \gamma + \mu_{k(c)} + \varepsilon_c, \quad |d_c| \leq h \quad (1)$$

Here τ is the jump in repression at the border, $\mu_{k(c)}$ is a border-segment fixed effect, X_c contains ruggedness, altitude and 1930 census characteristics, and h is the bandwidth. I use local linear estimation with robust bias-corrected inference (Calonico et al., 2014). The border mostly follows the Carpathian ridge, so ruggedness may itself be discontinuous there. I therefore test smoothness of ruggedness and altitude, restrict to segments where they are balanced, interact the border with ruggedness, and compare the estimate with placebo borders moved by fixed distances, as in Becker et al. (2016). The estimate is the combined effect of everything that differs across the border, including institutions, ethnic and religious composition and infrastructure, not of any single institution. For outcomes observed before and after, such as population and literacy, I estimate difference-in-discontinuities (Grembi et al., 2016), comparing the discontinuity after collectivization with the interwar one.

The same border design is applied to political capture and the thesis outcomes, which should give the total imperial legacy in modern clientelism. Comparing this discontinuity with and without controls for repression is a descriptive decomposition of how much of the legacy accompanies repression, not causal mediation, since repression is itself an outcome of the border. To learn which differences account for the border effect, I re-estimate it conditional on 1930 land inequality and ethnic composition as well as on railway density from historical maps supported by QGIS, and I interact the border with each of them. Because the literature finds that some imperial legacies disappear and others remain (Grosfeld and Zhuravskaya, 2015), the existence of a discontinuity in capture is an empirical question and not an assumption.

The second design asks whether repression shaped political capture. The pre-1918 border cannot instrument for repression in a regression on modern capture, because it affects politics through many channels besides collectivization. I use instead the 1950-1968 regional boundaries, which were drawn by the central state and abolished in 1968 and so do not coincide with modern counties. I keep only boundary segments that do not follow pre-war county borders, the pre-1918 border or current county boundaries, so that a discontinuity can arise only from the period 1950-1968. Let b index a segment, s_c be the signed distance to it, and Z_c indicate the side with higher regional arrest totals, defined from regional aggregates and not from the commune data used in estimation.

$$R_c = a_b + \pi Z_c + g(s_c) + X'_c \varphi + v_c \quad (2)$$

$$P_c = \alpha_b + \rho R_c + f(s_c) + X'_c \gamma + \varepsilon_c \quad (3)$$

Equation (2) is the first stage and π is testable directly. In (3), P_c is political capture (PSD vote share, control of local executives given vote share, and other outcomes of economic development), R_c is instrumented by Z_c , and ρ is the effect of regional repression intensity. Two tests discipline the exclusion restriction. Regional boundaries changed in 1952 and later, so arrests in a given campaign phase should jump at borders in force during that phase and not at boundaries drawn later. Borders that were county boundaries in 1968 but not in 1950 serve as placebos. A remaining concern is that regional party networks, not arrests, persist into present-day clientelism so ρ is read as the effect of regional repression intensity as a bundle. Given that the Habsburg territories combine high *chiabur* arrests, high ruggedness and low PSD support, the comparison of neighboring communes within boundary segments is what separates repression from these regional differences. With only 16 regions, the number of usable border segments is likely to be small, so showing a first stage is the primary task for the archival commune data.

A third part of the analysis traces the human-capital channel. I estimate event studies of population, literacy and farm structure on repression intensity, with the interwar and 1948 censuses as pre-periods and the 1956, 1966, and 1977 censuses as post-periods, and I bound violations of parallel trends with the sensitivity approach of Rambachan and Roth (2023), which applies to difference-in-differences and

event-study designs.

Inference and robustness checks follow the standard for boundary designs. I report bandwidth sensitivity and donut specifications, placebo regressions on 1930 covariates, and results excluding Banat and Dobrogea, where mass deportations to the Bărağan took place. Since persistence regressions can show spurious significance from spatial autocorrelation, I add spatial noise placebos and spatial basis controls (Conley and Kelly, 2025), and I combine multiple outcomes into standardized indices. If commune-level arrest records can be recovered for only some regions, I run all designs on counties with full coverage and use *chiabur* lists as the intensity measure elsewhere.

Outcomes

Repression data come from *Securitate* records (CNSAS) and county archives, with the regional totals of SRI (1994), as reported by Ivănescu (2015), as a benchmark, together with *chiabur* lists held in local archives. Records are unevenly preserved across archives, so commune counts are likely to contain measurement error, which I assess by comparing regional sums with the benchmark. Population, literacy, and farm structure come from the interwar, 1948, 1956, 1966 and 1977 censuses, and the regional boundaries of 1950-1968 are digitized from historical maps. Political capture and the modern outcomes, such as business formation and fiscal dependency, are measured at the commune or county level using the electoral and fiscal data from the Permanent Electoral Authority and the National Institute of Statistics, respectively. Finally, ruggedness and altitude come from SRTM 90m digital elevation model data (Riley et al., 1999).

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