

When are Junctures Critical?

The Legacies and Non-Legacies of the Suppression of Local Self-Government

(Working Paper*)

Daniel W. Gingerich, University of Virginia (dwg4c@virginia.edu)

Jan P. Vogler, Aarhus University (jan.vogler@ps.au.dk)

December 17, 2025

Abstract

The suppression of local self-government is a common feature of imperial rule and centralized authoritarianism. Extant scholarship considers such interventions to be *potentially* legacy-producing. But under which circumstances do these denials of political autonomy lead to sustained changes in political behavior? We develop a novel framework that elucidates when suppression of local self-rule will or will not produce political legacies. Two factors are crucial: the duration of an intervention and the scope of repression. Enduring interventions characterized by encompassing repression are the most likely to generate persistent changes. Conversely, transient episodes characterized by limited repressiveness are unlikely to produce legacies. Given our theory's broad scope, we conduct empirical analyses in two markedly different settings: Poland, which was split between three major empires, and Brazil, where a military regime installed appointed mayors in select cities. We complement these assessments with a meta-analysis based on 33 further studies. The combined results demonstrate that the suppression of local self-government has varying potential to create legacies.

* Comments are welcome.

The most recent version of this paper may be obtained at the following URL:
[Download the most recent version of the paper at this link.](#)

Acknowledgments

Helpful comments were provided by Taylor Boas, Lenka Bustikova, Anna Callis, Christopher Carter, Paweł Charasz, Volha Charnysh, Ali Cirone, David Collier, Jane Esberg, Anna Grzymała-Busse, Lotem Halevy, Sebastian Juhl, Torben Iversen, Jae Yeon Kim, Andrew Little, Cathie Jo Martin, Pau Vall Prat, Haru Saijo, Steven Samford, Mariano Sánchez-Talanquer, Itai Sened, Eduardo Silva, Roya Talibova, Guadalupe Tuñón, Hubert Tworzecki, Jason Wittenberg, and several anonymous reviewers. We also thank participants and discussants of presentations at the University of Copenhagen, Stanford University, Tulane University, the University of California at Berkeley, MPSA's annual conference, EPSA's annual conference, the annual conference of the Council for European Studies, Harvard's Seminar on the State and Capitalism Since 1800, the APSA European Politics and Society Section's Early Career Workshop, the Virtual Workshop in Historical Political Economy (VWHPE), the Danish Workshop in Historical Political Economy at the University of Southern Denmark, the Practices of Comparative-Historical Analysis Conference at Aarhus University, the Danish Political Science Association's (DPSA) annual conference, and APSA's annual meeting.

Introduction

Can contemporary support for illiberal political forces—such as anti-system parties and authoritarian successor parties (ASPs)—be traced to historical events? A growing literature in political economy responds in the affirmative. The central insight of this work is that political institutions and values evolve *jointly* over time and are complementary to one another (Besley and Persson, 2019; Bisin, Rubin, Seror and Verdier, 2024; Persson and Tabellini, 2021; Ticchi, Verdier and Vindigni, 2013).¹ This implies that institutional frameworks and political behavior can be mutually reinforcing (Bustikova and Corduneanu-Huci, 2017; Charrasz, 2022). Consequently, plausibly exogenous shocks—such as external interventions—that impose changes in regime type can have long-lasting effects on the values and behavior of members of society (Aaskoven, 2022a). From the perspective of critical juncture scholarship, these changes may be viewed as aspects of new, self-reinforcing equilibria (Capoccia and Kelemen, 2007).² At the local level, regime shifts that suppress self-government can take place because of either imperialism (De Juan and Pierskalla, 2017; Guardado, 2024) or centralized authoritarian rule (Simpser, Slater and Wittenberg, 2018).

Interventions of this kind may generate patterns of socialization and behavioral adjustment that conform with the character of the imposed regime, producing greater numbers of citizens with authoritarian mindsets in a society once dominated by democrats and vice versa (Acemoglu, Egorov and Sonin, 2021). Accordingly, instances of externally imposed institu-

¹Related scholarship notes that in order for major institutional changes to last, they need to be supported by the (updated) preferences of key political groups or coalitions (Saylor and Wheeler, 2017).

²Comparable self-reinforcing patterns have been identified with respect to demography and public service provision (Pardelli and Kustov, 2022) as well as elite political behavior and property rights regimes (Mangonnet, 2023). A more critical perspective on the issue is provided by Aaskoven (2022b).

tional change may constitute critical junctures that kick-start path-dependent feedback loops between initial institutions and political culture. Thus, to understand contemporary support for or rejection of anti-democratic actors, it is imperative to identify pivotal historical experiences associated with lasting cultural and institutional change.

Yet it is not at all obvious *ex ante* when an externally imposed change in regime type constitutes a critical juncture and when it does not. The US-led post-World War II reconstruction efforts in West Germany and Japan—characterized by the external imposition of democracy—clearly represented critical junctures in that they catalyzed the establishment of democratic political cultures in societies where authoritarian values were previously dominant (Haddad, 2012; Puaca, 2009). On the other hand, recent US-led reconstruction efforts in Afghanistan and Iraq, which also featured the crafting of domestic institutions by external actors, have not had similar effects (Coburn and Larson, 2014; Waldner, 2009). So while the existence of a complementarity between institutions and values points to the *possibility* that external interventions might generate lasting legacies for political behavior, understanding whether or not they are likely to do so in any particular instance requires further analysis.

This article develops a novel argument about when externally imposed institutional change associated with the suppression of self-government will lead to sustained adjustments in the affected population’s attitudes and behavior. It emphasizes two factors: the duration of suppression (“transient” or “enduring”) and the character of repression (“limited” or “encompassing”). Episodes of suppression that are *both* enduring and characterized by encompassing repression, restricting myriad aspects of political life and subjecting the populace to state violence, are the most likely to generate persistent changes in political behavior.³ Conversely, episodes of suppression that are transient and characterized by narrowly

³In other words—as we also elaborate in more detail in our theory section and the Appendix (A.2.1)—we suggest that the potential for a legacy is maximized when both conditions are simultaneously present.

targeted repression are the least likely to produce legacies.

Given our theory’s broad applicability, we illustrate the utility of our approach via empirical analyses of interventions in various settings. We begin with a detailed analysis of two specific cases: Poland, which was historically split between three empires, and Brazil, where a military regime installed appointed mayors in a large number of cities. In the former case, we show that the long-lasting and highly coercive rule of Russia generated a cultural preference for illiberal political actors, manifested in the present day by support for the authoritarian-populist Law and Justice party (PiS). With respect to Brazil, we show that the relatively transient and more limited repression by the military regime failed to generate lasting political preferences in favor of illiberal ASPs. In both cases, we bring to bear contextual knowledge and leverage geographic variation in historical interventions and the outcomes of local elections to infer the existence (or non-existence) of legacies.

Since our theoretical perspective is not limited to a single world region or just one kind of external intervention, we then proceed by conducting a broad *meta-analysis* of empirical patterns in a multitude of studies on imperial and authoritarian suppression of self-government. For this purpose, we code 33 prior studies (with three studies being counted twice because they differentiate between two intervention types, yielding 36 total observations) and subsequently analyze empirical regularities in these data. We find further evidence in favor of our theory, especially regarding the interaction of intervention length and repressiveness.

Our study makes several contributions to comparative politics scholarship. In elucidating why external interventions may or may not produce legacies, we deepen the literature on “historical persistence,” a body of work that locates its causal variables in the (often distant) past and its outcomes in the present or more recent past.⁴ Extant studies in this literature

⁴Comprehensive reviews of this literature are provided in [Abad and Maurer \(2021\)](#), [Acharya, Blackwell and Sen \(2023\)](#), [Cirone and Pepinsky \(2022\)](#), [Maseland \(2025\)](#), [Simpser, Slater and Wittenberg \(2018\)](#), and

typically exploit spatial variation within a single country or region to show that a particular historical event had a lasting legacy. While this approach illuminates historical dynamics in particular cases, it provides limited insight into when and why legacies take hold, since published studies are almost exclusively stories of “success” (i.e., instantiated legacies).

In contrast, our agenda here is to: (1) demonstrate empirically that events in a similar class (e.g., the suppression of local self-government) that could plausibly constitute the basis of a legacy do *not always* do so, and (2) explain why some events in the class generate legacies while others do not.⁵ To this end, we develop a novel template for the *comparative* investigation of legacy establishment, one that combines a comparative case study approach at the macro-level (leveraging cross-case variation) with design-based causal inference at the micro-level (leveraging within-case variation). Such a template has the virtue of taking seriously admonitions in the literature on comparative historical analysis about the need to incorporate negative cases in legacy research (Capoccia, 2015; Capoccia and Kelemen, 2007), while at the same time subjecting assessments of the existence of legacies to contemporary standards for causal inference (Dunning, 2012; Morgan and Winship, 2015). Additionally, we complement these evaluations with a meta-analysis covering cases in all world regions, thereby verifying the broader generalizability of our claims. To the best of our knowledge, our use of this specific template represents one of the first explicit attempts to foster knowledge accumulation in historical political economy (HPE) by integrating design-based inference with the comparative method as well as a meta-analysis (Callis, Dunning and Tuñón, 2024).

We also seek to enrich scholarship on historical junctures. Analytically, we consider

Voth (2021). For an overview that focuses on colonial legacies, see De Juan and Pierskalla (2017).

⁵In taking this step, we respond to disquiet among historically oriented social scientists about the absence of attention to failed or non-existent legacies of past events that could have plausibly constituted a legacy (Abad and Maurer, 2021; Acharya, Blackwell and Sen, 2023; Collier, 2022; Voth, 2021; Wittenberg, 2015).

episodes of suppression to be instances of such junctures, which may or may not prove to be critical (i.e., legacy-inducing) based upon their duration and repressiveness. By treating these features of historical junctures as variables that take on distinct values across cases, we position ourselves to conduct a counterfactual analysis of the juncture-legacy relationship (Capoccia and Kelemen, 2007; Soifer, 2012). As such, we further the discipline’s understanding of when “hypothesized” (Boas, 2022), “potential” (Handlin, 2022), or “possible” (Kaufman, 2022) critical junctures may become *actual* ones.⁶ The novelty of our approach lies in the combination of (1) a theoretical framework that carefully delineates expectations about the characteristics of junctures that do or do not produce legacies and (2) an empirical framework that uses a principled transnational comparison complemented by a broad meta-analysis of existing works to execute an exacting empirical test of those expectations.⁷

The remainder of our study is organized as follows. First, we introduce our theory, differentiating between types of interventions that are more or less likely to generate lasting legacies. Subsequently, we justify our selection of cases and characterize the nature of external intervention in each instance. In our empirical analysis, we provide evidence that episodes of the suppression of local self-government led to a lasting legacy of pro-authoritarian voting behavior in Poland but not in Brazil. Then we present a meta-analysis of patterns in legacy manifestation that is based on 33 studies. In the conclusion, we summarize our insights and

⁶Since duration is one of our variables, we self-consciously depart from treatments of historical junctures that define such episodes as taking place during narrow time windows (especially relative to their legacies). Commitment to such a definition would imply that the study of potentially long-lasting phenomena such as colonialism, forced labor regimes, communism, and the suppression of self-government—all core topics in historically oriented scholarship in political science—is outside the bounds of historical juncture research.

⁷On the use of transnational comparisons—which combine subnational and cross-national analyses—see Sellers (2019). For a call for rigorous hypothesis testing in critical juncture research, see Waldner (2022).

explain how our approach can be applied to the genesis of legacies in other contexts.

When does the Suppression of Local Self-Government Create Legacies?

The suppression of local self-government materializes in a wide variety of forms. It may occur after foreign conquest, as when a colonizing power installs officials from the metropole to directly administer a conquered territory (De Juan and Pierskalla, 2017). It may also be the consequence of conflict dynamics internal to a nation-state, as in post-civil war settings when a victor installs overseers to rule over the territories of vanquished foes (Liu, 2022). More quotidianly, it often occurs in the wake of transitions from democratic to authoritarian rule, as when newly empowered authoritarian elites eliminate or abridge the capacity of particular communities to select their local political officials.

For our purposes in this paper, we define episodes of the “suppression of self-government” as *interventions* executed by either an imperial power or a centralized authoritarian regime that significantly reduce or eliminate entirely the capacity of local populations to select their political officeholders. This may occur in tandem with state violence targeted at proponents of ideas or actions hostile to the regime behind the intervention. So defined, the suppression of self-government is distinct from instances of (locally determined) authoritarianism, where local actors decide of their own accord to adopt exclusionary political practices.⁸

Studies of the suppression of self-government feature prominently in historical legacy scholarship, albeit with debate about the character and direction of relevant effects. Anal-

⁸In this respect, we consider suppressions of self-government to be instances of “alien rule” (Hechter, 2013), which are qualitatively distinct from indigenous local authoritarianism. See Appendix section A.2.2 for further elaboration on this critical point and how it affects shifts in political behavior.

yses examining contemporary differences across Italian regions suggest that sustained experiences with self-government affect outcomes such as civic engagement (Putnam, Leonardi and Nanetti, 1993, 121–162), individuals’ levels of self-efficacy (Guiso, Sapienza and Zingales, 2016), and economic development (Di Liberto and Sideri, 2015). Further research on local interventions ties autocratic and imperial interventions to persistent anti-democratic attitudes and poor governance outcomes in countries including Chile (González, Muñoz and Prem, 2021), Indonesia (Martinez-Bravo, 2014; Martinez-Bravo, Mukherjee and Stegmann, 2017), Romania (Vogler, 2023), Croatia (Popescu, 2023a,2), Peru (Guardado, 2018), and Vietnam (Dell, Lane and Querubin, 2018). Similarly, scholarship on lived experiences with communism frequently links the suppression of self-government to anti-democratic attitudes (Besley and Persson, 2019; Neundorf, 2010; Pop-Eleches and Tucker, 2017).⁹

Yet other literatures imply that the link between experiences with self-government and democratic values and behavior is more complex. Scholarship on colonialism and imperial rule reports legacies of self-governance that often point in contradictory directions.¹⁰ Moreover, research on foreign military intervention implies that externally imposed regime changes do not inherently foster the development of either democratic or non-democratic institutions (De Mesquita and Downs, 2006; Downes and Monten, 2013).

⁹Vice versa, Doucette (2024) finds that positive historical experiences with self-government lead to higher levels of support for democratic parties in the long run and Gailmard (2024) suggests that decentralized colonial governance systems can have positive long-term effects on checks and balances in subsequent periods.

¹⁰Some studies link more direct or longer forms of colonial rule, both of which imply weaker local self-governance, to positive long-run outcomes such as enhanced prospects for democracy (Hariri, 2012; Lange, 2004), less corruption (Lange, 2004), and stronger norms of cooperation (Chaudhary, Rubin, Iyer and Shrivastava, 2020). Others tie indirect rule or colonial neglect, both of which imply stronger local self-governance, to improved public good provision (Iyer, 2010) and higher levels of economic development (Mahoney, 2010).

These heterogeneous findings raise a critical question: When is the suppression of self-government likely to generate legacies for political behavior? Our claim is that such external abridgments of self-government are more or less likely to produce legacies depending on their (1) duration and (2) the scope of repression. Given the specific class of historical events that we consider here (episodes of “suppression of local self-government”), these two features are crucial for legacy formation because they influence the types of political skills citizens adopt and, even more importantly, the values parents bequeath to their children. It is the joint action of skill adoption and intergenerational value transmission—induced by the specific characteristics of the intervention itself—which determines the likelihood that an episode of suppression will come to shape behaviors such as voting for anti-democratic actors.

To situate our theory and contrast it with existing scholarship, it is helpful to draw on the distinction between productive and permissive conditions made in prior work on critical junctures. Permissive conditions are those that create a window of opportunity for a legacy, while the productive conditions represent the driving forces that bring it about (Soifer, 2012). In our framework, the permissive condition is the suppression itself and the institutional change associated with it, which varies along the two dimensions that we specify. The productive conditions are the choices of citizens about skill adoption and value transmission in response to these institutional changes.

But how specifically do citizens make these choices? Consider first the duration of a suppression of self-government. Duration matters because it shapes perceptions among the populace about the intervention’s long-term time horizon. For individuals living in a territory subject to intervention, each additional year that passes under external rule increases perceptions of the solidity of the regime.¹¹ As citizens in the intervened community come to

¹¹This can be rationalized as the result of Bayesian learning, in which individuals with uncertainty about regime stability update their beliefs as more time under the current system passes (e.g., Hill, 2017).

perceive the state of suppression as more likely to persist throughout their professional lives, many will begin to invest in political skills that allow them to succeed in their new environment. Such skills are forms of expertise or learned capabilities—consciously adopted—that are tailored to the specifics of the regime created by the intervention. For instance, investing in one’s capacity to be an ideological thought leader and in cultivating ties to ruling party factions may be central to professional advancement and personal well-being in a system characterized by the absence of self-rule (Egorov and Sonin, 2011; Liu, 2019; Shih, Adolph and Liu, 2012). Because of the fixed costs of such skill investments, individuals who adopt regime-specific skills during the intervention are likely, once the intervention ends, to support political actors who are either directly linked to the now-defunct regime, such as authoritarian successor parties (ASPs), or who seek to create an environment favorable to those with authoritarian predilections, such as anti-system parties.

The duration of an intervention is also relevant for parental decisions about the socialization of children, a process that plays a critical role in the formation of political preferences (e.g., Beck and Jennings, 1991; Dinas, Fouka and Schläpfer, 2021). If self-rule is perceived as unlikely to reemerge in the foreseeable future, many parents will instill outlooks and orientations that maximize their children’s ability to achieve social (and economic) success in the new (more authoritarian) institutional environment. Extolling the virtues of popular deliberation and principled dissent, for example, would hinder their children’s ability to navigate social, economic, or political groups that are critical under the current regime; better, in this context, to inculcate a pride in obedience to authority and satisfaction with political disengagement. On the other hand, if self-government is expected to reemerge shortly, then maintaining the old value system would be preferable.

Accordingly, longer episodes of suppression, which convince many parents that the absence of self-government today will likely continue indefinitely, incentivize the transmission

of authoritarian-compatible values. Shorter episodes, which tend to leave open the possibility of a reversion to the old order, incentivize the transmission of values compatible with self-rule. Consequently, the greater the longevity of an episode of the suppression of self-government, the more likely it will be that a critical mass of citizens manifests a preference for anti-democratic political actors in the post-intervention period.¹²

Of course, value change via parental socialization is a slow-moving process. At any given time, it takes place only within households that have children in an impressionable age range—roughly, late childhood through adolescence (Neundorf and Smets, 2017). Multiple waves of children in this range need to be socialized before a meaningful aggregate shift in values can take place. A timespan of roughly a human generation is a plausible lower threshold in which this could occur. Accordingly, we categorize episodes of suppression shorter than a human generation (roughly thirty years) as *transient*, and longer episodes as *enduring*. All else being equal, we expect durable value shifts to be more likely to result from the latter than the former.¹³

Our distinction between authoritarian or imperial rule that lasts less than one generation vs. one or more generations is substantively meaningful and grounded in insights from different literatures. First, although there is disagreement regarding the specific period in a

¹²For evidence regarding both the alteration of attitudes and preferences through forms of authoritarian repression and the transmission of those attitudes and preferences via parental socialization, see Acharya, Sen and Blackwell (2018), Gouda and Rigterink (2025), and Surdea-Hernea and Plopeanu (2025a).

¹³In line with our discussion, Pop-Eleches and Tucker (2017, 296–307) also consider the potential importance of regime duration for attitudinal legacies. However, they concentrate on detecting differences in the intensity of the legacies of communism in post-communist countries, a set of cases for which regime duration was fairly extensive across the board (i.e., lasting longer than one generation). By contrast, we consider instances of the suppression of self-government that vary qualitatively in terms of duration in order to maximize leverage in assessing the relevance of the transient/enduring distinction for legacy formation.

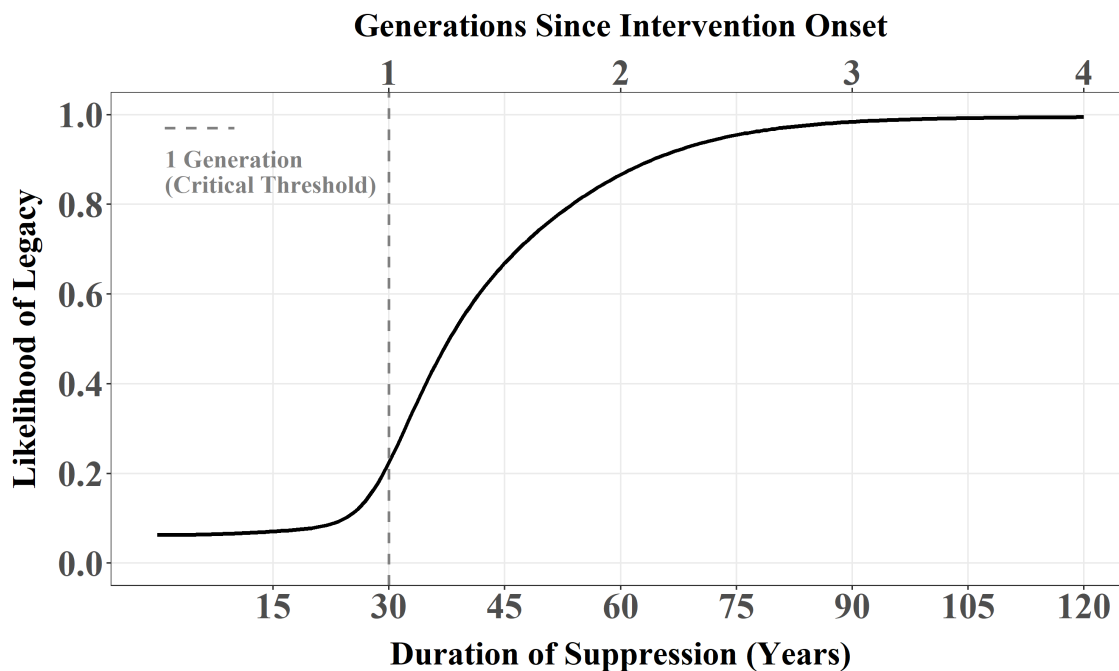
person's life that matters most for political attitude formation, there is extensive evidence (1) that those attitudes form over *several* years and (2) that this process takes place prior to full adulthood (Bartels and Jackman, 2014; Ghitza, Gelman and Auerbach, 2023; Krosnick and Alwin, 1989; Stoker and Jennings, 2008). Thus, interventions that last at least one generation can realistically shape the political preferences of a substantial proportion of citizens who are in their impressionable years, whereas shorter interventions would imply that many citizens may experience only partial or no exposure during the relevant life stage.

Second, for closely related reasons, literature from both sociology and political science often uses the concept of a generation to distinguish between cohorts of citizens who hold divergent normative values. Those values are at least partly grounded in shared historical experiences of those who were born in and lived through the same period (Inglehart, 1977; Mannheim, 1952; Pilcher, 1994). As a growing number of people witness similar historical conditions, it is more likely that common values and a shared identity based on this experience will emerge. Thus, it is no surprise that systematic comparisons in both political views and political behavior across generations are commonplace in the academic discourse (e.g., Abramson and Inglehart, 1992; Friedman and Schultz, 2024; Grasso, Farrall, Gray, Hay and Jennings, 2019; Jocker, van der Brug and Rekker, 2024; Osborne, Sears and Valentino, 2011). The same cumulative logic applies to the external interventions that we examine here, making the threshold of one generation both substantively and methodologically meaningful.

Finally, our theoretical model also explicitly incorporates a *continuous* influence of the duration of authoritarian or imperial rule on relevant outcomes, which makes it compatible with studies that suggest a continuous effect of exposure duration to suppressions (e.g., Olsson, 2009; Pop-Eleches and Tucker, 2020). Indeed, from our perspective, the adoption of new skills related to modified governance processes can take place within a relatively short time after intervention. However, as we also emphasize, the more durable and critical

change takes place in the value dimension. Change on this dimension requires the *sustained alteration* of socialization and attitude formation during impressionable years among a sufficient proportion of citizens, which can only take place once significant time has passed. These dynamics are displayed in **Figure 1**, which provides a visualization of the relationship between suppression duration and the likelihood of a legacy for voting behavior.

Figure 1: Likelihood of a legacy based on the duration of a suppression of self-government



As the figure shows, the relationship is characterized by a sigmoid curve. For suppressions lasting less than a human generation, the likelihood of a legacy is low. After that point, as the dynamics of intergenerational value transmission start to kick in, the likelihood of a legacy increases *markedly with duration*. After multiple generations have been subjected to the suppression of self-governance, the marginal impact of duration begins to decline, as the legacy becomes more firmly locked in place. Thus, while the model incorporates continuous effects, the most critical change occurs after one generation when the value transmission mechanism becomes dominant.

Now consider the scope of repression. This concept can be analytically split into two subcomponents: (1) restrictions on citizen involvement in governance; and (2) violent suppression of ideas and actions that are hostile toward the authoritarian or imperial regime. Repression in the first subcomponent is high if the intervener removes any institutional channels for local populations to articulate and lobby for their interests. Vice versa, it is low if there are clear formal mechanisms for the population to articulate any wishes or grievances.

Even if such channels of formal participation are limited in scope, their presence allows citizens to influence outcomes in some policy domains. For instance, even if the local chief executive is appointed by the intervener, some form of elected parliamentary representation may remain. This would provide citizens with opportunities to influence policy outcomes and allow them to gain experience relevant to democratic politics. Thus, low repression in the first subcomponent exists when at least one formal channel of political participation remains for the local population that allows them to influence governance in meaningful ways. In contrast, high repression exists when the authoritarian or imperial intervener has removed all meaningful formal channels for political participation. According to this scheme, an example of high repression of self-governance is Portuguese colonial rule in Angola and Mozambique until the 1960s, which featured a centralized administrative hierarchy that wholly excluded native Africans.¹⁴ Conversely, an example of low repression of self-governance would be British colonialism in India after the establishment of legislative councils in 1909, which permitted Indians to elect representatives with deliberative powers. While admittedly a simplification, the binary understanding of this subdimension captures a theoretically crucial distinction: whether citizens possess any institutionalized means to voice preferences and influence policies. This difference has cascading effects on regime-citizen interactions, making it analytically valuable even as specific cases exhibit variation *within* these categories.

¹⁴Many Spanish colonies also exhibited highly repressive practices in this dimension (Mahoney, 2010).

Repression in the second dimension is high if an authoritarian or imperial intervener frequently uses coercive power to take violent action—including forceful intimidation, physical punishment, incarceration, or murder—against those who express ideas or engage in actions against its rule. It is low when an authoritarian or imperial intervener is unlikely to use its coercive power against those who express such thoughts or engage in relevant actions.

As with the first subcomponent, the distinction made in the second subcomponent is substantively meaningful as well. Specifically, whether a population has to fear violent repression when ideas against the regime are expressed has major consequences for key aspects of society, such as personal security or the level of freedom of speech. Furthermore, as we elaborate in more detail below, violent repression also likely induces behavioral change, making it a relevant factor in shaping a population’s behavioral patterns. An example of high violence in repression is Pinochet’s military regime in Chile (1973–1990), which systematically employed detention, torture, and extrajudicial killings against political opponents. An example of low violence in repression is the British colonial administration in Hong Kong, which managed political dissent largely through legal controls and deportation rather than frequent violent suppression of opposition voices. Although the intensity of repression exists on a continuum, the binary classification employed here identifies a critical distinction: whether violent coercion constitutes a *routine* response to dissent or remains an exceptional measure. This threshold shapes citizens’ expectations about the costs of political expression and thus their willingness to engage in opposition activities.

In practice, these two aspects of repression tend to go together: cases which feature more expansive opportunities for political participation often tend to feature lower levels of violent suppression (and vice versa) (Blakeley, 2012; Corduneanu-Huci and Osa, 2003). This is no coincidence: If a population affected by a suppression of self-government is given some formal channels of political influence that allow it to affect decisions in meaningful ways, this reflects

that the intervening authority generally has a less repressive governance approach and that the population has fewer incentives to develop or exhibit anti-regime ideas or actions (as many grievances have a chance to be addressed through that mechanism). Vice versa, an intervening authority that leaves no meaningful space for representation is also more likely to (1) provoke the development, articulation, and execution of anti-regime ideas and actions and (2) follow a governance approach grounded in violent suppression. Accordingly, we simplify our characterization of repressive scope by using two (binary) categories, “Encompassing Repression” (which is high on both subcomponents) and “Limited Repression” (which is low on both subcomponents), as these reflect the combinations that most frequently occur.¹⁵

So defined, the scope of repression matters for two reasons. First, it determines the regime-specificity of political skills. The greater the number of arenas for participation and/or the less intense violent suppression during the episode of suppression, the greater the opportunities for citizens to take advantage of previously acquired skills related to self-government.¹⁶ By the same token, the greater the continuity in the set of relevant political elites, the less need citizens will feel to develop new talents or invest in new social networks.

¹⁵Although this distinction provides us with analytical clarity and parsimony, it by no means implies that intermediate cases are impossible. Rather, we also expect to encounter various instances that feature a combination of low and high repression across the subdimensions. In this regard, see our meta-analysis extension in the Appendix (A.26), which examines how separating both subcomponents affects our results. The predominance of corresponding values on both dimensions is also reflected by the fact that three out of the four cases used as examples above feature them. The exception is British colonial rule in Hong Kong, which—despite low levels of violence—featured a fully appointed Legislative Council and a colonial governor with near-absolute power. While a council represented citizens, it only enjoyed minimal political influence.

¹⁶In this regard, Kitschelt (1999) shows that countries with higher levels of political contestation during communism had greater chances to develop political systems with quality democratic representation in the post-communist period due to citizens’ and elites’ experiences with programmatic debate.

Encompassing repression incentivizes large aggregate changes in political skills, whereas limited repression reduces the extent to which an intervention will lead to skill changes. Since political skills investments drive regime preferences, more extensive repression during the episode of suppression should be associated with stronger preferences for anti-democratic political actors in the post-intervention period.

Second, the scope of repression matters because it determines the degree of mismatch between values that produce self-fulfillment under self-government and the realities of life under external intervention. Episodes of suppression that are characterized by limited repression, which offer real, albeit abridged, opportunities for political participation, present a smaller mismatch than do episodes characterized by encompassing repression. Consequently, parents may find it attractive to bequeath democratic values to their children when repression is limited, as they anticipate their offspring may at least experience some true opportunities for participation in the arenas of politics that remain open. By the same token, violent suppression of anti-regime ideas and actions creates incentives for parents to teach their children authoritarian/populist values, as doing so makes it less likely that their offspring will be subjected to state violence. In sum, because democratic values are more likely to be sustained via parental socialization during episodes of suppression featuring limited repression, voting for anti-democratic actors in the post-intervention period should be more muted in these cases than for interventions characterized by encompassing repression.

Having clarified the independent influence of duration and scope of repression on legacy formation, we proceed to consider how each dimension mediates the effect of the other. In general, we view these two dimensions of suppression to be mutually reinforcing. There is a significantly higher likelihood of a legacy if a suppression is *both* enduring and repressive than if only one of these conditions is present. In the former circumstance (of an enduring *and* repressive intervention), citizens will perceive that a quick return to the status quo ante

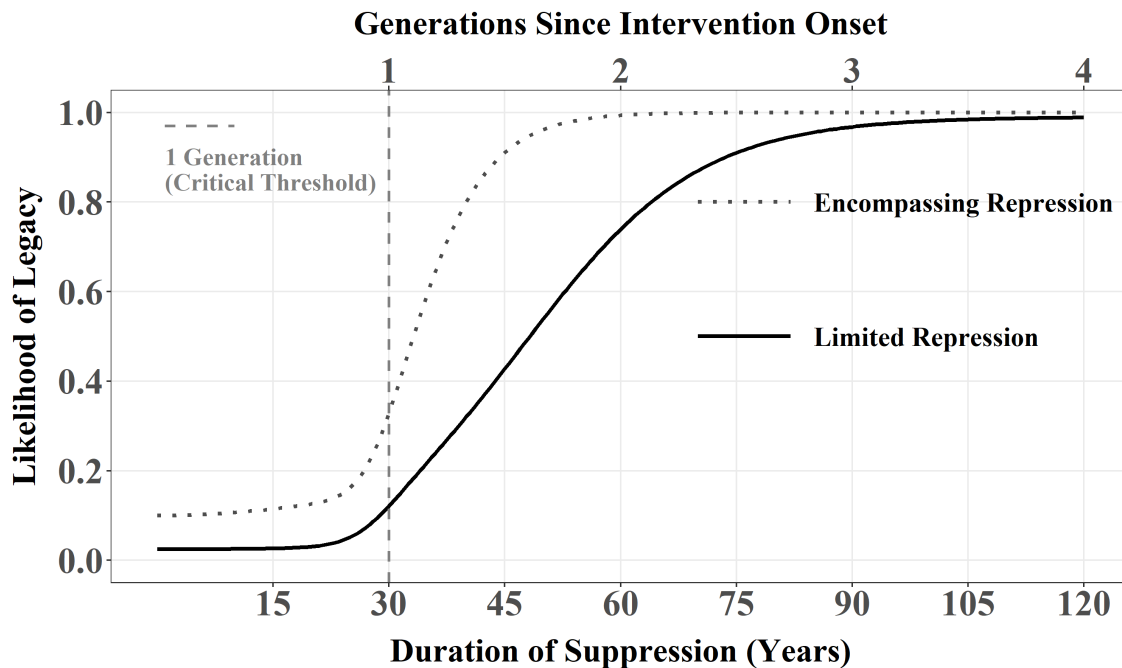
is implausible and that resistance to behavioral change will result in state-based violence. The result is an overwhelming incentive to adapt for large parts of the population. If a suppression is enduring but features only limited repression, citizens have modest incentives to change their behavior, as channels for participation may remain and they are not subject to the violent suppression of anti-regime ideas and actions. Under these circumstances, the incentives for behavioral adjustment will only affect some parts of the population. Similarly, if a suppression features high repression but only lasts for a short time, many citizens may hold onto the hope that the situation may revert to the pre-intervention status quo in the near future. For this reason, a considerable segment of the population will be disinclined toward behavioral adjustment.¹⁷ In short, if an intervention proves to be *both* long-lasting and highly repressive, the likelihood of a permanent legacy increases markedly.¹⁸

Figure 2 shows how the duration and repressiveness of a suppression of self-government jointly determine the likelihood of a legacy. If repression is limited (depicted by the solid line), the likelihood of a legacy starts out low and increases slowly with the duration of the suppression until the single generation threshold is crossed. If repression is encompassing (depicted by the dotted line), the likelihood of a legacy starts out higher and increases more rapidly with the duration of the suppression. Although the curves indicate that a legacy is consistently highly likely for very long suppressions (90+ years), they illustrate how repressiveness conditions the impact of duration for short or intermediate episodes.

¹⁷This logic implies that *consistency* in the level of repression over time is also important. A long intervention with an erratic application of repression is unlikely to generate a legacy, since citizens' expectations would not congeal around the notion that high repression will be in effect for the foreseeable future.

¹⁸We elaborate on this aspect of our theory in more detail in the Appendix (section A.2.1). Moreover, in our meta-analysis below, we also incorporate an interaction term of length and repressiveness. The results suggest that interventions that combine both features are indeed significantly more likely to produce legacies.

Figure 2: Likelihood of a legacy based on the duration and repressiveness of a suppression of self-government



Importantly, neither aspect of our theoretical framework (length or depth) requires *all* citizens of an affected polity to change their own political skills or the way they raise their children. Our argument is simply that these two features of interventions lead to gradual changes in beliefs and behavior among an incrementally increasing number of citizens. As long as the length or depth of an episode of suppression keeps encouraging a growing proportion of the citizenry to adapt to the new institutional environment, the materialization of a legacy becomes more likely. Once a critical mass has been socialized in this way, a lasting change to the political behavioral equilibrium is achieved and a legacy has been instantiated.

Our argument is summarized in **Figure 3**. A suppression of self-government that is both enduring and characterized by encompassing repression has the greatest potential to generate a legacy for voting behavior, as we expect considerable shifts in political skills and values. In contrast, a suppression that is transient and which has a limited scope of repression has the least potential to generate a legacy, as neither political skills nor values are likely to

Figure 3: When does the suppression of self-government create legacies for pro-authoritarian voting behavior?

	Limited Repression	Encompassing Repression
Transient Intervention	<i><u>Minimal</u></i> potential for long-term legacies	Moderate potential for long- term legacies
Enduring Intervention	Moderate potential for long- term legacies	<i><u>Highest</u></i> potential for long-term legacies

shift much. Imperial or authoritarian interventions that are either (1) enduring but limited in their scope of repression or (2) transient but encompassing in their repression represent intermediate cases. Legacies may emerge in these cases, but they will typically have a high “decay rate” (Acharya, Blackwell and Sen, 2023).¹⁹

Clarifying Scope Conditions

Our theoretical focus is on how citizens adjust their behavior in the wake of external institutional interventions that abridge self-government. An alternative strategy for citizens who find themselves in these situations may be to engage in forms of passive or active resistance.

¹⁹The off-diagonal cases may produce lasting legacies if one of the two dimensions we highlight takes on an extreme value. For instance, interventions which are transient but characterized by extreme levels of repression could produce rapid adoption of regime compatible skills and beliefs (e.g., Aaskoven, 2022a; Homola, Pereira and Tavits, 2020). Of course, the fact that legacies may emerge in such cases does not per se contradict our argument that legacies are generally *most likely* to emerge when duration and repression are both high, and *least likely* to emerge when they are both low.

Indeed, given the probabilistic character of our framework, we would expect that a subset of the population will seek to hold onto its—potentially very strong—prior beliefs; with some of these citizens also engaging in multifaceted forms of resistance against the intervener. However, this type of resistance by citizens with strong anti-authoritarian beliefs is not of immediate relevance to the outcome examined here, because these citizens would have been unlikely to change their beliefs in the first place.

Nevertheless, it remains important to clarify under which conditions the vast majority of citizens would have incentives to change their behavior in line with the intervener's institutions rather than join those who resist due to the strength of their prior beliefs. In this respect, we must make a crucial theoretical qualification in the form of two scope conditions. Specifically, in contemplating the consequences of a suppression of self-government, we concentrate on instances of external rule where: (1) a central goal of the external authority (i.e., the agent responsible for the suppression of local self-government) is to elicit compliance from the population in the intervened territory; (2) this authority has full coercive control of the intervened territory and swiftly restores it if any contestation occurs.

Because compliance with the new set of institutions is a key goal of the external authority, citizens can avoid the harshest forms of violence and possibly even make socioeconomic gains so long as they conform in word and deed to the edicts of said authority. Consequently, our framework does *not* extend to contexts of ethnic cleansing, genocide, or other types of extreme violence targeted toward populations that is primarily or exclusively based on ascriptive characteristics. Nor does it extend to contexts where violence is largely indiscriminate or exercised via collective punishment. In the former, subjugated populations are targeted because they have the “wrong” identities, and these identities are either immutable or only changeable at enormous cost. In the latter, individuals are often targeted irrespective of compliance. In our framework, it is the fact that most citizens have viable paths to avoid

repression that leads to the forms of pro-regime behavioral adjustment we emphasize.²⁰

In addition, our second scope condition of high levels of coercive control by the intervening authority implies that violent strategies of resistance are unlikely to be successful in fundamentally changing the situation. Thus, it is improbable that these strategies are considered viable options by most citizens. Under such circumstances, adaptation and adjustment to the new institutional framework—rather than violent resistance—is the primary type of behavioral change that we would expect to observe (cf. Wang, 2021).²¹ Accordingly, this scope condition advises a focus on private decisions about skill acquisition and value transmission rather than resistance against the intervening authority. It is also relevant because a low level of coercive control would immediately and clearly signal to citizens that the intervention is unlikely to be sustainable in the long run, implying that the incentives for costly behavioral changes are practically non-existent.²²

These scope conditions are important because they delineate contexts where our theoretical mechanisms operate from contexts where repression is more likely to backfire by producing resentment rather than adaptation. A substantial body of research demonstrates that repression can generate (long-term) backlash or resistance (e.g., Dinas and Northmore-Ball, 2020; Fouka and Voth, 2023; Peisakhin and Queralt, 2025; Rozenas, Schutte and Zhukov, 2017). However, these findings frequently emerge in contexts characterized by (1) indis-

²⁰Conversely, one would expect that long-run resentment would be a reaction to repression in settings in which citizens are targeted based on ascriptive characteristics or violence is indiscriminate. See Lupu and Peisakhin (2017) and Rozenas, Schutte and Zhukov (2017), respectively, for evidence regarding this point.

²¹Vice versa, if the coercive power of an intervening authority is perceived to be weak, this can provoke violent resistance. In some historical cases, intervening authorities have then used excessive force to restore their rule (see Pierskalla, De Juan and Montgomery, 2019).

²²Similarly, Leuschner and Hellmeier (2024) find that concessions by authoritarian rulers are often seen as a sign of weakness and can therefore lead to increased protest mobilization.

criminate violence or collective punishment that provide no reliable path to safety through compliance (e.g., [Fontana, Nannicini and Tabellini, 2023](#); [Samad, 2025](#); [Zhukov, 2014](#)),²³ (2) targeting that is mainly based on immutable ascriptive characteristics (e.g., [Cederman, Wimmer and Min, 2010](#); [De Juan, Gläsel, Haass and Scharpf, 2025](#); [Kopstein and Wittenberg, 2018](#); [Leipziger, Rørbæk and Skaaning, 2025](#); [Nalewajko, 2025](#)), or (3) weak or inconsistent coercive control that can signal regime fragility (e.g., [Carter and Gingerich, 2025](#); [Curtice and Behlendorf, 2021](#); [Finkel and Gehlbach, 2020](#); [LeBas and Young, 2024](#); [Moss, 2016](#)).²⁴

By contrast, our framework most directly applies to cases where compliance is typically both demanded and rewarded with a higher level of personal security and/or socioeconomic prosperity, and where the regime demonstrates sustained capacity to maintain order. While adaptation is not impossible in other contexts, under these scope conditions it becomes significantly more probable that most citizens shift from open/violent resistance to forms of adaptation (cf. [Kim-Leffingwell, 2023](#); [Neundorf, Gerschewski and Olar, 2020](#); [Wang, 2021](#)).²⁵

In short, two scope conditions are critical to our theoretical mechanisms. A key goal of the intervener must be to elicit compliance from the population and the intervener must be capable of maintaining order through coercive means (or be able to swiftly restore it if any serious contestation occurs). Importantly, in contexts where repression is indiscriminate/exercised via collective punishment, identity-based, or accompanied by regime fragility, we would expect that backfire effects become more likely among greater shares of the affected populations than many of the adaptation dynamics we theorize here.

²³For an analysis of the reactions of occupying forces to violent resistance, see also [Zhukov \(2017\)](#).

²⁴On the importance of (administrative/coercive) state capacity for regime stability, see [Andersen, Møller, Rørbæk and Skaaning \(2016\)](#), [Andersen, Knutsen and Skaaning \(2025\)](#), and [Higashijima and Mitchell \(2025\)](#).

²⁵For a related perspective on why citizens vote for the perpetrators of violence or electoral coercion in postwar democratic contexts, see [Daly \(2019\)](#).

The Cases: Suppression of Self-Government in Poland and Brazil

We illustrate our argument through the analysis of (1) the imperial partition of Poland and (2) centralized military rule in Brazil. We selected these cases for four reasons. First, it is reasonable to characterize the suppression of local self-government in both cases as potentially legacy-inducing events. Indeed, the extant literature on imperialism would suggest a high likelihood of a legacy in Poland (Grosfeld and Zhuravskaya, 2015; Vogler, 2019), whereas the existing literature on authoritarian regimes would suggest a high likelihood of a legacy in Brazil (González, Muñoz and Prem, 2021; Martinez-Bravo, 2014).²⁶ Second, there is variation *across the cases* in the features of the types of suppression specified by our framework as driving the likelihood of a legacy (i.e., they exhibit a theoretically relevant contrast). Third, *within the cases* there is internal spatial variation in the presence or character of suppression that permits one to establish the existence or non-existence of a legacy using the tools of design-based causal inference. Fourth, as we explain below, the initial conditions in the cases are sufficiently similar to justify using a comparison between them to test our theory.

In each of the two cases, we carefully chose the period in which we conduct our empirical examination. First of all, our theory is mainly about the long-run effects of the suppression of self-government on electoral behavior in open elections, implying that we can only consider behavior in periods after an imperial or authoritarian intervention in which open elections are permitted. Additionally, our key guiding principle was to establish *hard-test conditions* with respect to both Poland and Brazil. As our theoretical claim is that enduring and repressive episodes of suppression of self-government (such as in the case of Russia) are

²⁶On the latter point, see Kitschelt and Singer (2018) for a discussion of how party-voter linkage strategies are shaped by past authoritarian interventions.

most likely to produce a stable long-term legacy, in the case of Poland we evaluate our empirical predictions using outcomes with a significant temporal distance from the original intervention. This represents a hard-test condition because in any examination in the period immediately following an intervention, patterns of (authoritarian) political behavior may exhibit a degree of stickiness before moving to a non-authoritarian equilibrium. To avoid the detection of such short-term legacies with a high decay rate, we resort to an analysis of Poland after the Cold War.²⁷ Furthermore, in the Appendix (section A.4), we explain our choice of the 2010s as the most appropriate time within the post-Cold War period.

Conversely, when the suppression of local self-government is both short in duration and limited in repression, we predict that there will be no legacy at all. Under such circumstances, an empirical test immediately following the intervention represents hard-test conditions because if there is (even just minimal) stickiness in authoritarian behavioral patterns, such an analysis in the immediate aftermath of the intervention would likely detect it and provide evidence *against* our theory. Accordingly, in the case of Brazil, we examine the first two elections following centralized authoritarian rule.

Initial Conditions

Even though we chose cases from different world regions to test our theory, it is noteworthy that—despite differences in formal political institutions—early nineteenth-century

²⁷An alternative strategy would be to focus on elections in interwar Poland. Yet, in addition to departing from hard-test conditions, this would be inappropriate for our purposes for two reasons. First, the legal and administrative frameworks imposed by the three powers persisted for years after Poland had formally gained independence (Tarnowska, 2012; Tarnowska, 2013; Vogler, 2019), making it impossible to disentangle institutional effects from patterns of political behavior. Second, the Polish party system of the interwar period was geographically highly fragmented, with different parties present across the former partitions (Sokołowski, 2013). Thus, direct comparisons of electoral success across different areas are often unavailable.

Poland and mid-twentieth-century Brazil shared a number of crucial features in common prior to the onset of the suppressions we study here. As depicted in Table 1, both polities featured electoral processes that drew upon selectorates of a non-negligible size, as well as other key factors commonly argued to shape the nature of political representation: economic production based on large-scale (latifundia) agriculture, political machines based on family dynasties, clientelism as a key modality of political influence, and formal institutions that privileged local elites over the central government. As all of these factors may be viewed as potential confounders, similarity across the cases significantly bolsters our claim of comparability.

Table 1: Similarities between the cases in initial conditions

	Poland before partition	Brazil before military regime
Non-negligible selectorate	✓	✓
Latifundia agriculture	✓	✓
Family dynasties	✓	✓
Clientelism	✓	✓
Institutions favoring local elites	✓	✓

The scope of participation in political decision-making at the local level was non-negligible in both contexts. This may seem surprising for Poland, since the political system of that country—“Golden Liberty” (*Złota Wolność*)—was formally aristocratic, vesting legal rights and opportunities for political participation in the noble class. Yet, the nobility itself was quite extensive, constituting approximately 10 percent of the population in the period preceding partition (Gella, 1998, 13).²⁸ Indeed, “because of [the nobles’] sheer numbers, more people enjoyed active political rights in Poland than in any other major European state before

²⁸Considering that only male nobles had electoral rights, this implies that the franchise in Poland (5% of total population) was roughly half of that of the United States (11% of total population) at the beginning of the nineteenth century (Lepore, 2018, 163).

the French Revolution” (Lukowski, 1991, 10). Additionally, although peasants were excluded from formal participation, given the intimate settings of town and rural life in Poland, non-nobles could informally convey their grievances to the nobles, who then might consider these in their deliberations in the local assemblies (the *sejmiks*).²⁹ In Brazil, the democratic period preceding the military coup (1945–1964) featured a competitive and fractionalized party system. Popular elections were held regularly for a plethora of offices, including the presidency, governorships, mayoralties, and legislative offices at the federal, state, and municipal level. Participation was modest by twentieth-century standards—registered voters peaked at 26% of the population in 1962—but the trend was toward greater inclusion in politics (Carvalho, 2016, 150). The relatively small franchise was due to a literacy restriction in the Constitution of 1946, which remained in force until rescinded by a constitutional amendment in 1985.³⁰

Moreover, extensive agrarian estates were central to economic and political life in both pre-partition Poland and much of mid-twentieth-century Brazil. In Poland, economic production was organized around the export of grain through ports in the Baltic basin. Given large estates’ ability to better withstand fluctuations in weather, price shocks, and warfare, the agricultural sector experienced a sustained increase in land concentration over time. The result was the consolidation of enormous estates under the control of a small sliver of the nobility: the magnates (*magnateria*). Their economic supremacy over lesser nobles allowed them to centralize control over local assemblies and become the dominant political figures in pre-partition Poland (Davies, 2005a; Lukowski, 1991; Stone, 2001). In Brazil, latifundia agriculture had its roots in the colonial era, when sugar mills and plantations populated the northeastern coast, large-scale cattle ranching penetrated the hinterland, and large farms

²⁹This represents an (informal) petition system as referenced in Appendix section A.2.2.

³⁰As we further elaborate in the Appendix (section A.2.2), our theory applies to a broad variety of initial contexts, including those with initially low levels of public participation in decision-making processes.

(*fazendas*) produced cotton, tobacco, and coffee for export. Shortly after independence, the Brazilian Empire institutionalized the administrative and political power of the large land-lords, granting them military powers and titles in exchange for executing security functions the state was unable to provide (Uriconchea, 1980). This created a dominant class of rural elites called coronels (*coronéis*). With the passage of time, these elites and their progeny leveraged their control over land and the public administration to dominate politics in the hinterland (Flynn, 1978; Leal, 1949; Vilaça and Albuquerque, 2003).

Closely related to the previous point is that, at the heart of the political machines constructed by the magnates in Poland and the coronels in Brazil were their extended family networks. Political systems in both countries revolved around local-level family dynasties. The Polish national parliament was dominated by the sons of the magnates (Lukowski, 1991, 98) and the magnate families used their influence over parliament to restructure inheritance law to consolidate landholdings within their hands (Davies, 2005a, 175). They also carefully orchestrated marriage alliances within the magnate class to accumulate estates and control over strategic posts within the state (Stone, 2001, 262–263). Similarly, Brazil’s coronels were typically the heads of large landowning clans that provided employment and mediated access to state resources for citizens in their regions (Chilcote, 1990; Hagopian, 1996; Lewin, 1987). Competition among these clans constituted the central political cleavage in many rural municipalities, so much so that it was understood that a vote for a particular political party represented a vote for a particular clan (Carvalho, 1966).³¹ No less concerned about fragmenting patrimony than their Polish counterparts, Brazil’s landholding families concentrated inheritances through endogamous marriage, while concurrently pursuing exogamous marriage alliances to extend the scope of their properties and influence (do Rêgo, 2008).

³¹In essence, local-level party politics consisted of “quarrels among families transposed to the political plane” (Blondel, 1957, 155, author’s translation).

Partly as a consequence of these dynamics, clientelism was a key modality of electoral influence in both systems. The noble class in Poland was made up of a variety of economic strata, the most populous of which was the petty nobility. While enjoying political rights, the petty nobility was poor and insecure—characterized by dependency on the magnates via lease-holding arrangements or as members of their retinues (Davies, 2005b, 170–171). They constituted a “noble *lumpenproletariat* ... with nothing to offer except a vote and a sword” (Zamoyski, 1987, 137). As dutiful members of the magnates’ clientele, the petty nobility “offered political support at [local assemblies] that enabled magnates to achieve prominence and gain valuable patronage appointments” (Stone, 2001, 295). In Brazil, the political machines controlled by the coronels mobilized voters by using control over employment in the private and public sectors, by exploiting tenancy relationships, or by selective allocation of state benefits. They also saw to the registering of voters and organized their transportation to the polls (Blondel, 1957; Carvalho, 1958). Their control over the rural electorate was so extensive that voting behavior in family-dominated towns could be predicted with “nearly mathematical precision” (Carvalho, 1966, 173, author’s translation). In fact, the coronels routinely operated as vote brokers, literally selling the votes of their dependents to high-ranking politicians (such as aspirants to federal deputy or governor) who sought political support in the towns they controlled (Gingerich, 2020).

Finally, formal institutions sustained local elite power in both cases. The Polish Crown was dependent on the national assembly for basic functions like taxation and security, and the national assembly, in turn, was hamstrung by the local assemblies, who selected the representatives of the national assembly and imposed legally binding instructions upon them. Thus, by dominating the local assemblies, the magnates could dominate many aspects of state policy.³² Decentralization in Brazil derived from the country’s federal system, which

³²If this did not work, they could induce their representative to use the *liberum veto*, which allowed any

granted significant prerogatives to states and municipalities, including in the areas of policing and education (Abrucio, 1998; Samuels, 2003). The system also provided strong fiscal autonomy to these units and generated extreme malapportionment in the National Congress favoring rural areas. Taken together, these arrangements benefited coronels and their families, since they could exploit municipal resources to sustain their machines and lean on their representatives in Congress to tilt national policy in their favor.

In sum, on the eve of their respective interventions, both Poland and Brazil had political systems that combined meaningful elements of local self-government and social and legal environments with several important similarities. These circumstances highlight the virtue of comparing the cases to test our theory.

Characterizing the Episodes of Suppression

Poland

In Poland, different areas of the country are associated with varying prospects for legacies based on the characteristics of past imperial rule. Major European powers began to partition the Polish lands in the late eighteenth century. In 1815, after the Napoleonic Wars, Poland's fate was again decided by multiple empires: At the Congress of Vienna, the territories inhabited by the Poles were split between Austria, Russia, and Prussia. These partitions lasted until Polish independence in 1918.³³

The duration of the suppression of self-government was long-lasting by any criterion: 123 years if dated to the third partition (1795) and 103 years if dated to the fourth (1815). This represented sufficient time for Poles living under foreign occupation to adapt their practices

single member of the national assembly to dissolve a legislative session.

³³A map of the historical boundaries can be found in the Appendix (Figure A1).

and attitudes in response to the character of intervention, be it through a shift in political skills, political values, or both. However, while the intervention's duration was (roughly) constant across the three territories, the scope of repression differed markedly.

The scope of repression was most encompassing under Russian rule. Russia governed its Polish territories in a top-down fashion, using a high level of state coercion and precluding opportunities for meaningful or broad political participation (Davies, 2005*b*, ch. 2; Vogler, 2019, 814–815). Attempts by Poles to advocate for their rights were violently quashed through the use of military power, demonstrating that there was no alternative to submission to the imperial hierarchy (Davies, 2005*b*).

By contrast, the scope of repression, including the use of violence by the state, was relatively limited under Prussian rule (Davies, 2005*b*, 85; Vogler, 2019, 812–813). While the Prussian state denied full self-government to the Poles, it nevertheless provided several meaningful channels of participation, including, as of 1849, the right to limited political representation in a Prussian representative assembly. Moreover, during the period of Imperial Germany (1871–1914), Poles were given full voting rights in federal parliamentary elections and were permitted to establish political parties. Thus, we can most clearly distinguish between the character of foreign rule of Prussia and Russia. Although comparable in duration, Russian rule remained highly repressive throughout the period, while Prussia's state had a *Rechtsstaat* character and permitted substantial political participation for several decades.³⁴

Compared to Prussia and Russia, the character of Austrian rule was uneven. The early years of Austrian occupation featured strict censorship of the Polish press and severe oppression of Polish attempts at self-government, both indicative of encompassing repression.

³⁴Given the clear distinction between Prussia and Russia specifically, our empirical analysis primarily focuses on this comparison. We include the comparisons with Austria in the Appendix (section A.11), but as elaborated below, this case is ambiguous in light of our framework.

In later years, however (especially after 1867), Poles were given the opportunity to participate in the Austrian bureaucracy. Yet political rights—including voting rights—remained severely restricted until the last few years of foreign rule (Davies, 2005*b*; Vushko, 2015). Most importantly, the first mass elections, which allowed a large share of the population to participate in democratic political processes, only took place in 1907 (Howe, Szöcsik and Zuber, 2022), shortly before the disintegration of the empire and likely unable to change political attitudes in a sustainable fashion.

In terms of our theory (see Figure 3), due to the distinct combination of a long and highly repressive intervention, the Russian-ruled territory of Poland is located in the lower righthand quadrant, indicating the highest potential for a legacy related to pro-authoritarian voting. The area ruled by Prussia is located in the lower lefthand quadrant, indicating a moderate potential for such a legacy. Finally, the location of the Austrian-ruled territory is ambiguous given the shifting character of Austrian rule over time, which makes categorization difficult.

Brazil

In the case of Brazilian military rule (which resulted from a coup d'état by the armed forces in 1964), the local suppression of self-government was much shorter and much less encompassing than in the case of Poland. The entire interlude of military rule lasted only twenty-one years (1964–1985). Moreover, political restrictions, while certainly severe relative to previous periods, nevertheless still allowed for various forms of participation.

Specifically, unlike other military regimes in the region, Brazil's military permitted open competition among (pre-approved) political actors for many offices.³⁵ It also utilized violent repression sparingly, committing human rights abuses with significantly lower frequency than

³⁵On the factors that influence an authoritarian regime's incentive to adopt competitive (multiparty) elections, see Mitchell (2023).

the regimes established in Argentina, Chile, and Uruguay (Heinz and Frühling, 1999). Although the military was critical of Brazil's democracy, it neither extinguished party politics altogether nor organized society into a single-party state. Rather, in its second institutional act (AI-2) of 1965, it dissolved the existing system, substituting it with an officially sanctioned two-party system.³⁶

In addition to creating the foundations of the new party system, AI-2 created a mechanism for the selection of political officeholders. The president and vice-president were chosen by the Chamber of Deputies and governors by their state legislatures. This ensured that high-level executive officeholders would either be military officers (the president) or their clients (governors). However, state, federal, and municipal legislative offices were contested at regular intervals through popular elections. Thus, while the highest-level executive offices were removed from democratic contention, most formerly elected offices remained subject to a popular vote.³⁷

The type of suppression of local self-government on which we focus took place within this larger institutional context. Due to AI-3 and subsequent government decrees, (1) state capitals, (2) municipalities designated as areas of strategic interest, (3) municipalities with hydromineral wealth, and (4) municipalities within federal territories were prohibited from selecting their mayors through elections. Rather, these mayors were appointed by the governor (a military loyalist), in concordance with the state assembly or the President. In total, 180 different municipalities (out of roughly 4,000) had appointed mayors.

³⁶The parties in this system were the Brazilian Renewal Alliance (ARENA) and the Brazilian Democratic Movement (MDB). ARENA was the authoritarian government's official support party. The MDB was the officially tolerated opposition party.

³⁷However, some politicians—especially those suspected of communist sympathies—were prohibited from running for office (Skidmore, 1988).

The location of the Brazilian case in our framework (Figure 3) is clear: it belongs in the upper left-hand quadrant, indicating a minimal potential for a legacy. From the perspective of our theory, the suppression of municipal self-government (which in most cases lasted less than twenty years) was too brief to catalyze major shifts in political value systems. Moreover, the continued use of elections for various offices and the rarity of state violence directed at regime critics meant that the skills and values acquired under the prior democratic regime would still basically conform to the realities of life in affected municipalities, providing few incentives for fundamental changes. In brief, the *particular combination* of a short and comparatively non-repressive intervention makes a legacy unlikely.³⁸

Measuring Legacies — Leveraging Spatial Variation within the Cases

In addition to capturing variation in our theory, these cases also facilitate the measurement of our outcome (the presence or absence of a legacy). To properly assess whether or not an episode of suppression has generated a legacy, its within-country dynamics must have unfolded in such a way as to permit causal inference. Ideally, there would either be some quasi-random element to the spatial distribution of the suppression of self-government or exact knowledge of the conditions that generated it (cf. Mattingly, 2017; Müller-Crepon, 2020; Ricart-Huguet, 2022). The episodes of suppression in both Poland and Brazil share this rare virtue, thereby permitting us to categorize the cases according to the presence or absence of a legacy with a high degree of confidence.

³⁸In addition to fitting our theoretical schema well, we view the narrow scope of Brazil's municipal interventions as beneficial from an inferential standpoint, in that it alleviates concerns about the treatment being bundled with other institutional changes. See the Appendix for many additional details about the historical and institutional backgrounds of both our cases.

For the case of Poland, multiple studies have confirmed that imperial border placement was quasi-random, with no significant differences in geography or pretreatment characteristics among the localities incorporated into different empires (Grosfeld and Zhuravskaya, 2015, 56–60; Vogler, 2019). Moreover, historians describe the conditions on the ground as not influencing border placement in any way (Chapman, 2006, 56; Hoensch, 1990, 180; Kuk, 2021, 143–144). Thus, we have a strong claim of quasi-randomness based on a combination of empirical evidence and contextual information.

For the case of Brazil, we have nearly exact knowledge of the conditions that led to the appointment of mayors in some municipalities but not others. Additionally, we can measure these factors directly. This implies that an estimation strategy based on selection-on-observables may closely approximate the true causal impact of intervention.

A Lasting Legacy of the Suppression of Self-Government: Evidence from Poland

With respect to Poland, we examine the electoral success of the party “Law and Justice” (*Prawo i Sprawiedliwość*, PiS) in mayoral elections in the 2010s.³⁹ A populist party with clear illiberal tendencies, the PiS had strong electoral support at all levels of government during the 2000s and 2010s (Charnysh, 2017). The party utilized its power to dismantle Poland’s constitutional system of checks and balances, leading observers to characterize it as a danger to democracy (Markowski, 2019; Sadurski, 2018). Given the PiS’s anti-system orientation, we consider its success as reflecting authoritarianism-compatible values among the electorate.⁴⁰

³⁹As touched upon previously, from the perspective of our framework, focusing on these elections represents a “hard test” case because they took place roughly one century after the disintegration of the empires that had partitioned Poland.

⁴⁰A critic of this approach may highlight the circumstance that the PiS generally does not have candidates

As previously discussed, we expect that municipalities in the formerly Russian territory will be more likely to have mayors who are affiliated with this party than municipalities in the Prussian territory. Due to the more ambiguous character of Austrian rule, we abstain from making strong predictions about this case and relegate the relevant results to the Appendix (section A.11). In order to conduct our analysis, we primarily use data by [Charasz and Vogler \(2021\)](#), which is mainly based on data by [Statistics Poland \(2021\)](#).

Dependent Variables

We employ two measures of the PiS's electoral success:

1. ***Mayor PiS***: This variable is equal to 1 if the mayor elected in a municipality belonged to the party Law and Justice (PiS) for the time period indicated (2010–2014, 2014–2018); 0 otherwise.
2. ***Mayor PiS (Broad Definition)***: This variable is equal to 1 if the mayor elected in a municipality belonged to the PiS or was supported by its electoral committee in 2014–2018; 0 otherwise.

Treatment Variable

We concentrate on the distinction between Russian and Prussian rule, utilizing a treatment variable equal to 1 if a municipality historically belonged to the Russian partition (1815–1914) and 0 if it belonged to the Prussian partition (1815–1918). Due to extensive population

in every municipality. However, in line with our theory, we expect that some municipalities simply do not have the sociocultural profile for the PiS to be a viable party (due to historical circumstances). If no PiS candidate emerges due to these sociocultural conditions, it is not a contradiction, but rather a confirmation of our theory. Nevertheless, to account for the possibility of some candidates being ideologically aligned with the PiS's positions, but not formally belonging to the party, one of our alternative measures also takes into account if a candidate was merely endorsed by the PiS. Accordingly, both theoretically and empirically the absence of PiS candidates in some municipalities does not pose a significant problem.

resettlement, we exclude municipalities that belonged to interwar Germany from the analyses (e.g., Charnysh and Peisakhin, 2022).⁴¹ Although both partitions were equally lengthy, Russian rule was highly repressive in comparison with Prussian rule. Thus, according to our theory, the comparison between formerly Russian and Prussian areas has a high potential to reveal a legacy of stronger pro-authoritarian voting tendencies in the former relative to the latter.

Covariates

To account for the possibility that our results merely reflect cross-regional socioeconomic differences, we also present models with municipal-level control variables. These include the level of elevation, population density, the share of the population that lives in urban areas, the unemployment rate, the average monthly salary of the county’s inhabitants (as a percentage of the national average), the share of the “working age” population (ages 18–64 for men, 18–59 for women), the share of the “elderly” population (ages 65+ for men, 60+ for women), and the logarithm of population size.⁴²

Geographic Regression Discontinuity Design

We estimate the effect of imperial partitions by utilizing a geographic regression discontinuity design (GRDD). Specifically, we treat the imperial borders as quasi-random cutoffs and use the distance to the border (in km) as the forcing variable. The regressions have the following format:

$$y_i = \beta_0 + \beta_1 \text{Russia}_i + \beta_2 \text{ELV}_i + \mathbf{x}_i' \boldsymbol{\beta} + f(\text{geographic location}) + \varepsilon_i \quad (1)$$

⁴¹See the Appendix (sections A.7 and A.13) for additional discussion on this point.

⁴²Descriptive statistics for our Polish data are presented in the Appendix (Table A1).

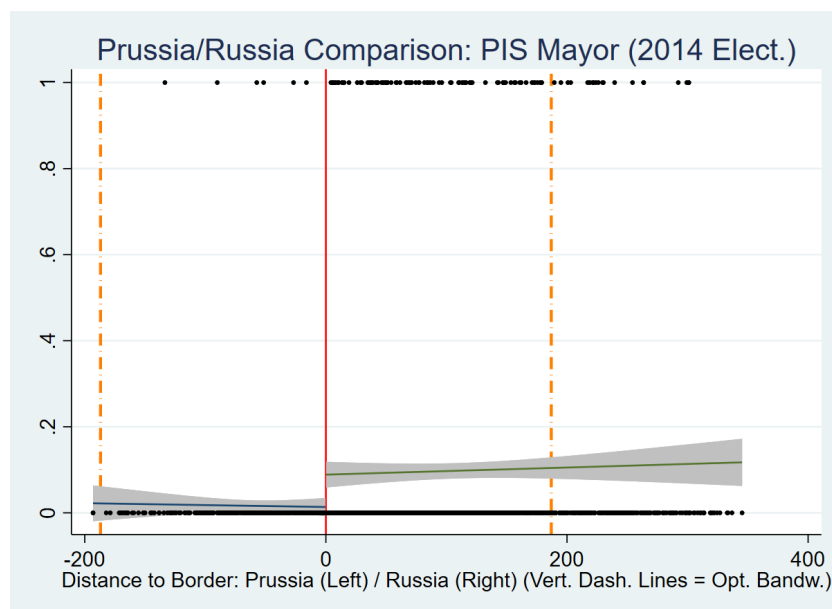
where y_i is the dependent variable for municipality i . β_1 represents the difference in the value of the dependent variable between municipalities that belonged to the Russian Empire and those that belonged to Prussia. β_2 is the coefficient for elevation (ELV). Control variables are contained in vector $\mathbf{x}$. In addition, all regressions include a location function: $f(\text{geographic location})$. We employ three variants of this function. The first expresses geographical location as a linear function of distance to the border and the interaction of distance with the relevant empire dummy. The second does the same but uses a second-order polynomial for distance. The third expresses location as a linear function of distance, but also includes latitude and longitude.⁴³

Results

We find consistent evidence that the contrast between more limited repression (Prussian partition) and more encompassing repression (Russian partition) produces long-term political differences in the electoral success of the populist-authoritarian PiS. [Figure 4](#) illustrates our GRDD approach. It depicts the discrete increase in the proportion of mayors belonging to the PiS in the 2014 elections as one crosses the geographic boundary between the Prussian and Russian partitions (at $x = 0$). [Figure 5](#) provides an alternative visualization of the discontinuity. It shows that the distribution of mayors belonging to the PiS in the 2014 elections is concentrated in the partition previously controlled by Russia.

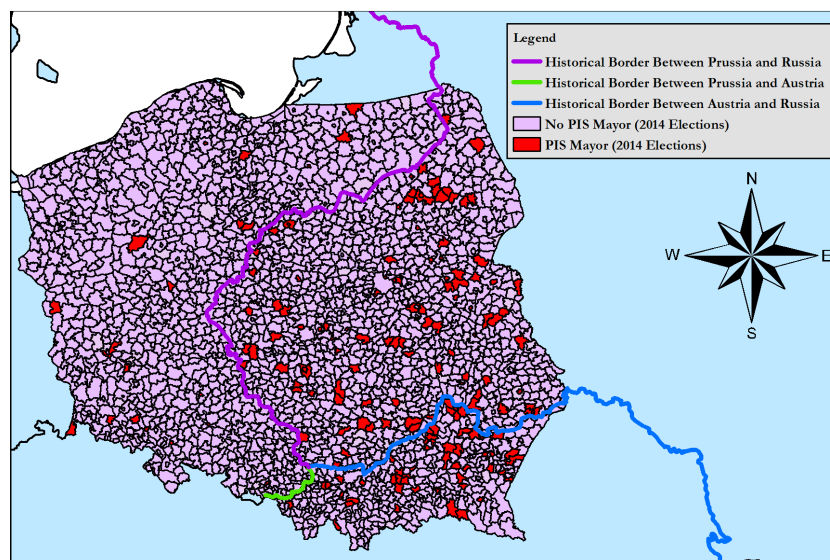
⁴³Details are presented in the Appendix (section A.8).

Figure 4: Impact of Prussia vs. Russia Partition on having a PiS mayor



Note: This figure represents a comparison of the success of PiS candidates in mayoral elections (2014 elections) between former Prussian and Russian municipalities in Poland. Municipalities in the former Prussian partition are on the left; municipalities in the former Russian partition are on the right. The line at zero represents the historical border. The x-axis represents municipalities' distance to the historical border. The corresponding regression is model 1 in Table 2.

Figure 5: Map of PiS-affiliated mayors in the 2014 elections



Note: This map shows the success of PiS candidates in mayoral elections (2014 elections) across all three partitions of Poland. The Prussian partition is the western one; the Russian partition is the eastern one; the Austrian partition is the southern one.

Table 2 presents the GRDD results for the Prussia/Russia comparison without control variables. The table indicates that municipalities in the formerly Russian partition are significantly more likely to have a PiS-affiliated mayor. Indeed, being located within the former Russian partition leads to a roughly 0.08 increase in the probability that a municipality will elect a PiS mayor. These findings are robust to the adoption of the latitude/longitude specification and the inclusion of controls (as shown in Appendix section A.9).

Table 2: Local Political Leadership Outcomes (Prussia/Russia Comparison)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Br.) (2)	Mayor PiS (2010) (3)	Mayor PiS (4)	Mayor PiS (Br.) (5)	Mayor PiS (2010) (6)
Russia	0.075*** (0.026)	0.081*** (0.027)	0.056** (0.023)	0.066* (0.035)	0.075** (0.036)	0.029 (0.031)
Elevation	0.00003 (0.0001)	0.00004 (0.0001)	0.0002 (0.0001)	0.00005 (0.0001)	0.0001 (0.0001)	0.0002* (0.0001)
Dist. PR-RU	-0.0001 (0.0003)	-0.0001 (0.0003)	0.0001 (0.0003)	-0.0004 (0.001)	-0.0004 (0.001)	0.00001 (0.001)
Dist. PR-RU Sq.				0.00000 (0.00001)	0.00000 (0.00001)	0.00000 (0.00001)
Russia*Dist.	0.0001 (0.0003)	0.0001 (0.0003)	-0.0001 (0.0003)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Russia*Dist. Sq.				-0.00000 (0.00001)	-0.00000 (0.00001)	-0.00000 (0.00001)
Constant	0.010 (0.028)	0.007 (0.029)	-0.007 (0.025)	-0.0001 (0.036)	-0.002 (0.037)	-0.013 (0.031)
Observations	1,435	1,435	1,437	1,435	1,435	1,437
R ²	0.019	0.021	0.018	0.020	0.021	0.022
Adjusted R ²	0.016	0.018	0.015	0.016	0.017	0.018

Note: OLS

*p<0.1; **p<0.05; ***p<0.01

Evidence for the Underlying Mechanisms

Existing scholarship on Poland offers multiple pieces of evidence in line with our claim that the intervention executed by Prussia led to a shift in cultural values that was distinct from the one produced by the intervention executed by Russia. Specifically, [Hryniewicz \(1996\)](#) demonstrates that citizens in the formerly Prussian parts of Poland prioritize self-fulfillment over financial security as a key goal of their work lives. This is in line with the notion that values related to self-efficacy are more prevalent there. Similarly, [Vogler \(2019\)](#)

provides evidence that local administrations in the formerly Russian partition have lower levels of transparency and professionalism. Finally, building on the literature that connects self-efficacy and educational attainment in society (e.g., [Ayllón, Alsina and Colomer, 2019](#); [Grabowski, Call and Mortimer, 2001](#); [Schunk, 1989](#)) as well as the literature that connects populism to distrust in higher education and experts (e.g., [Merkley, 2020](#); [Read, 2018](#)), we show in the Appendix (section A.12) that there are significantly fewer mayors with higher education in the formerly Russian partition when compared to the Prussian partition. In addition to these results, in the Appendix (section A.5), we also discuss further detailed and case-specific evidence for the proposed within-family transmission mechanisms. Taken together, these pieces of evidence indicate that there are important differences in cultural values related to self-efficacy, egalitarianism, and transparency in line with our predictions.

Suppression of Self-Government *without* a Legacy: Evidence from Brazil

For the case of Brazil, we examine the success of the country's two authoritarian successor parties during the 1988 and 1992 mayoral elections. These two parties were the Social Democratic Party (*Partido Democrático Social*, PDS) and the Liberal Front (*Partido da Frente Liberal*, PFL). The PDS was the direct descendant of the official authoritarian (regime-supporting) party, ARENA (which was simply renamed as the PDS in 1980). The PFL was composed of leading figures from the PDS who split with the party over its presidential nominee in 1985 ([Power, 2018](#)).⁴⁴ Given previous findings about the legacies of dictatorship-

⁴⁴The split led to the PFL leadership allying with the democratic opposition in the 1985 presidential elections. In the Appendix (section A.16), we provide a discussion of why the PFL should be considered an ASP nonetheless. In all of our analyses below, we present results for the PDS and PFL separately.

era mayors in the region (González, Muñoz and Prem, 2021), it is plausible that the PDS and PFL might have had a higher chance of winning mayoral elections as a consequence of the appointment of mayors during the authoritarian period. Nevertheless, our framework classifies this intervention as both transient and limited in repressive scope. Thus, based on our theory, we deem the likelihood of a legacy as being low. To assess whether or not a legacy existed, municipal-level electoral data on mayoral elections held by each of Brazil's twenty-six state-level electoral tribunals were collected and coded specifically for this study (excluding the federal district).⁴⁵

Dependent Variables

We examine two dependent variables in our analysis:

1. ***PDS Mayor***: This variable is equal to 1 if the mayor elected in a given municipality belonged to the PDS or was elected by a coalition of parties that included the PDS; 0 otherwise.
2. ***PFL Mayor***: This variable is equal to 1 if the mayor elected in a given municipality belonged to the PFL or was elected by a coalition of parties that included the PFL; 0 otherwise.

Treatment Variable

Our treatment variable is ***Intervened***, equal to 1 for municipalities that had an appointed mayor during military rule; 0 otherwise. This variable is coded from the Supreme Electoral Tribunal's compilation of electoral statistics for the 1972 and 1976 elections, which includes a list of all municipalities that had appointed mayors (Tribunal Superior Eleitoral (TSE), 1979a; Tribunal Superior Eleitoral (TSE), 1979b). The list was then supplemented using the

⁴⁵In the majority of cases where the data were not in electronic format, this entailed coding the data from PDFs of the original electoral acts.

information contained in decree laws dealing with national security areas promulgated after the election (1973–1981), which listed additional municipalities with appointed mayors.⁴⁶ Municipalities that were newly created in the post-authoritarian period and located within the previous boundaries of an intervened municipality were also coded as 1.⁴⁷

Covariates

In selecting the covariates, we exploit knowledge about the factors considered by the military regime in deciding which municipalities to prohibit from having elections. We make use of the fact that the features that made municipalities of national security interest were clearly defined, as were the characteristics that made them sites of hydromineral wealth. We also use knowledge about the dimensions in which state capitals differ from other municipalities.

When using the language of national security in reference to municipalities, the military government was typically referring to concerns about controlling its interior border zones. These concerns were manifested in efforts like the National Integration Scheme in 1970, which brought colonists from more populated areas of Brazil to settle in the Amazon (Flynn, 1978, 452), and the promulgation of Law N^o. 6.634 (May 2, 1979), which prevented foreigners from acquiring land in border areas. Accordingly, a municipality's degree of national security concern was a function of its distance to the border.

In targeting sites of hydromineral wealth for intervention, the actions of the military government reflected a long-term preoccupation in Brazil with the therapeutic and medicinal

⁴⁶The laws were Decree Law N^o 1.272 (May 29, 1973), Decree Law N^o 1.273 (May 29, 1973), Decree Law N^o 1.284 (August 28, 1973), Decree Law N^o 1.316 (March 12, 1974), Decree Law N^o 1.480 (September 9, 1976), Decree Law N^o 1.481 (September 9, 1976), and Complementary Law N^o 41 (December 22, 1981).

⁴⁷The coding for these cases is based on the municipal administrative histories provided by the Brazilian Institute of Geography and Statistics (IBGE) in its *Cidades* website (cidades.ibge.gov.br).

value of mineral water, one dating back to the early/mid-nineteenth century (Marrichi, 2017). Such sites also played an important role in the growth of the hotel and tourism industries (Franco, 2017). Thus, the decision of the military government to prohibit elections in locations with hydromineral wealth can be interpreted as an attempt to shield these valued resources from the perceived risks of political mismanagement.

As stated earlier, a third major rationale for intervention was that a municipality was a state capital. Of course, Brazilian state capitals are *sui generis*, so structuring relatively pure as-if-random comparisons based on these units is infeasible. However, we do have information on some of the major ways they differ from other municipalities. Besides the fact that they are the seats of government, state capitals tend to be more populous than most other municipalities and have higher levels of human development.⁴⁸

Given these considerations, we employ a select set of covariates in order to maximize the credibility of our causal inferences based on (conditional) differences between municipalities that experienced an intervention and those that did not. Our covariate set is as follows:

1. ***Distance to Border (Log.)***: This is the logarithm of the distance (in kilometers) from the center of a municipality to the nearest land border.⁴⁹
2. ***Mineral Water***: This variable is equal to 1 if a municipality was listed as having a concession to extract mineral water according to a study commissioned by the Ministry of Mines and Energy on the distribution of mineral water (Queiroz, 2004); 0 otherwise.
3. ***Population (Log.)***: This variable is equal to the logarithm of the population size of the municipality (measured in 1996). Data come from the Institute for Applied Economic Research (IPEA; www.ipeadata.gov.br).

⁴⁸Of course, these covariates merely represent approximations for the kinds of features that make state capitals unique. Therefore, we present all our results also with state capitals excluded, acknowledging that only the exclusion of these units may fully solve the problems associated with unobserved confounders.

⁴⁹Distances were constructed using shape files for Brazilian municipalities (for the year 1991) compiled by the Brazilian Institute of Geography and Statistics (IBGE) (IBGE, 2011), as well as data on latitude and longitude compiled from the IBGE by do Prado (<http://github.com/kelvins/Municipios-Brasileiros>).

4. ***Human Development:*** This variable is the municipal human development index (measured in 1991). It is a composite indicator of well-being based on municipal-level outcomes in the dimensions of health, education, and economic prosperity. Data come from the Institute for Applied Economic Research (IPEA; www.ipeadata.gov.br).

Descriptive statistics for our Brazilian data are presented in the Appendix (Table A13 in section A.19). These are complemented by an analysis of the impact of the aforementioned covariates on the likelihood that a municipality would experience an intervention (Figure A10 in section A.20). Consistent with the above discussion about the factors that drove the military's decision to intervene locally, we show that intervention was strongly and negatively related to a municipality's distance from the nearest land border and positively related to the presence of mineral water and population size. The relationship to human development was negative, albeit small in magnitude.

Design

We adopt a research design that maximizes comparisons across municipalities with similar characteristics. In particular, we focus on within-state variation between municipalities that experienced interventions and those that did not, holding constant the covariates described above. In this regard, we employ two functionally similar estimation strategies. First, we estimate the impact of intervention by utilizing linear probability regression with fixed effects by state. Second, we estimate the impact of intervention by utilizing exact matching on state and the presence of mineral water in conjunction with coarsened exact matching on distance to the nearest land border, population, and human development.⁵⁰ Since we recognize that

⁵⁰To implement coarsened exact matching (Iacus, King and Porro, 2012), we discretized the distance to the nearest land border into the following categories: $\leq 250\text{km}$, $[250\text{km}, 500\text{km})$, $[500\text{km}, 1000\text{km})$, $\geq 1000\text{km}$. Population size was discretized into categories defined by the quartiles of that variable. Human development was discretized into categories defined as terciles. The R package **cem** was utilized to conduct the analysis.

state capitals may differ from non-state capitals along many dimensions beyond population size and human development, we present all our results both with state capitals included as well as excluded, with the latter representing the most conservative empirical strategy.

Ruling Out Ideological Selection

One potential source of concern about the empirical strategy described above is ideological selection. It is possible that the military government was more likely to impose appointed mayors in municipalities that exhibited support for leftist candidates in the years leading up to the intervention in 1964. If this was the case, then our estimates of the effect of the suppression of local self-rule could suffer from omitted variable bias, since the ideological leanings of municipalities before military rule are likely correlated with support for the two authoritarian successor parties as well.

To address this possibility, we examine support for Brazil's most prominent left-wing politician of the era: João Goulart. Goulart was the sitting president deposed by the military. His support for broad social reforms and his perceived sympathy with communist regimes was used by military and civilian actors to justify the coup. Goulart came to the presidency by way of the vice-presidency, an office to which he was independently elected in 1960. Thus, if the Brazilian military was engaged in ideological selection when choosing where to impose appointed mayors, one would expect that this would be evident in the 1960 vote patterns for Goulart. Figure A9 in Appendix section A.18 presents the data using box-and-whisker plots. As shown there, there is no evidence for ideological selection: The distribution of vote shares for Goulart in municipalities with appointed mayors was nearly identical to that encountered in municipalities that were allowed to continue electing their mayors. This is true examining the full sample with state capitals, as well as in a restricted sample without them.⁵¹

⁵¹Kolmogorov-Smirnov tests confirmed the similarity of the distributions. P-values for said tests were

Results

Before turning to the findings, it is important to clarify that key aspects of our evaluation of the Brazilian case create, if anything, a bias *in favor* of encountering a legacy of pro-authoritarian voting. The 1988 and 1992 elections were the first nationally held municipal elections after the end of military rule, meaning that the lived experiences of intervention were still fresh. Also, the two parties whose electoral success we consider were strongly linked in the public mind with authoritarian rule.⁵² If any legacy of intervention for pro-authoritarian voting had been created, one would expect it to be discernible in these elections.⁵³

Our findings are presented in [Figure 6](#) (based on a linear probability model, LPM) and [Figure 7](#) (based on coarsened exact matching, CEM). The underlying conclusions from both sets of estimations are very similar: We detect no appreciable effect of a legacy of intervention on electoral support for ASPs in Brazil. [Figure 6](#) presents the point estimates and 95%-confidence intervals of the impact of having had an appointed mayor for sixteen different specifications of the LPM.

Estimates are presented separately by outcome (PDS victory, PFL victory), electoral cycle (1988, 1992), inclusion of state capitals in the sample (included, excluded), and inclusion of control variables (included, excluded). Given the historical variation in voting behavior across Brazilian regions, state fixed effects were included in all analyses. As is evident in [Figure 6](#), in none of the various specifications did the coefficient on *Intervened* reach conventional levels of statistical significance. Moreover, the effects were consistently close to zero

equal to 0.125 and 0.220 in the full and restricted samples, respectively.

⁵²In the 48th Congress (1987–1991), nearly 90% of the PDS’s congressional delegation was made up of former ARENA politicians and officials; for the PFL, the figure was nearly 80% (Power, 2000, 75).

⁵³As mentioned earlier, from the perspective of our framework, focusing on these elections therefore represents a “hard test” case.

in magnitude (ranging from a reduction of three percentage points to a similar increase).

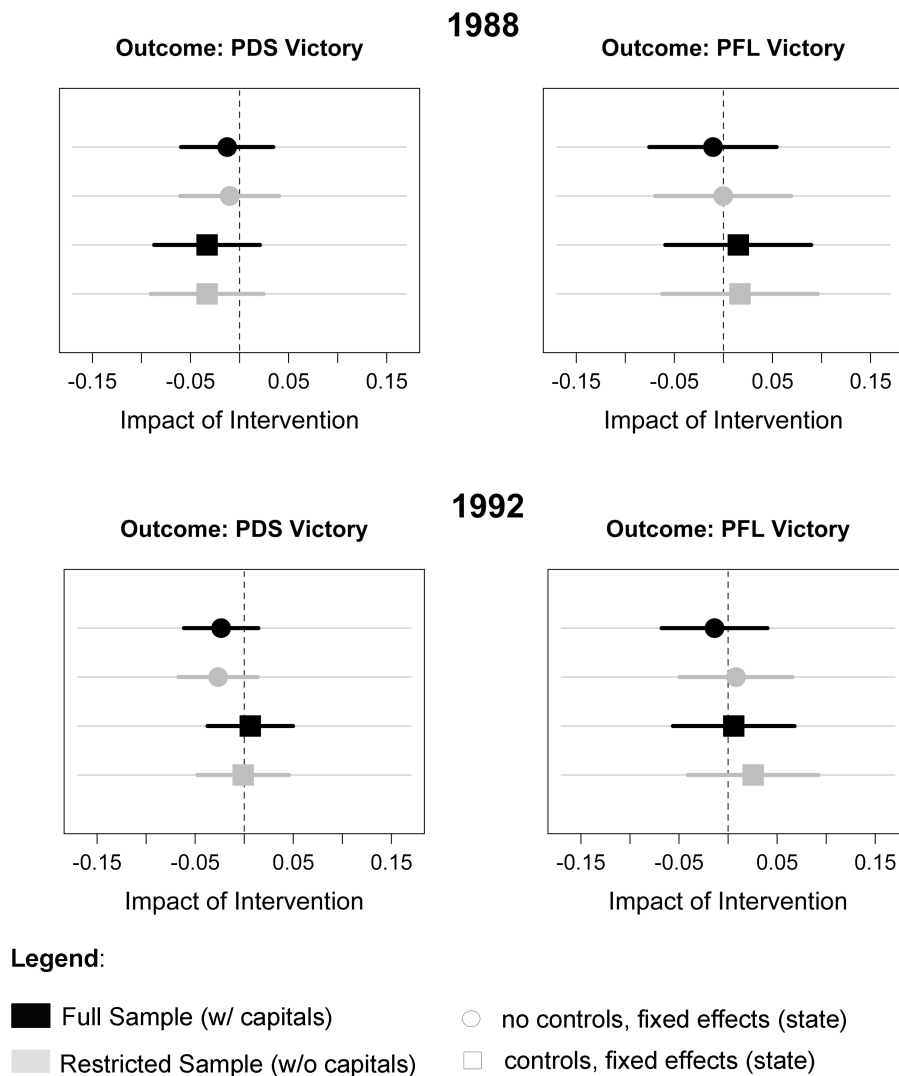
Focusing on the CEM results, [Figure 7](#) presents the point estimates and 95%-confidence intervals depicting the average treatment for the treated (ATT) due to having had an appointed mayor. Estimates are broken down by outcome, electoral cycle, and inclusion of state capitals. Here again we find that estimates of the effect of *Intervened* were statistically indistinguishable from zero and had small magnitudes.⁵⁴

Evidence for the Underlying Mechanisms

Extant scholarship provides evidence that is consistent with our arguments about why a legacy of intervention was unlikely to emerge in Brazil. In addition to the fact that the episodes of suppression were too short to affect intergenerational values transmission, there was little impetus for citizens to fundamentally change their political skills. As shown by studies examining political career trajectories in the states of Minas Gerais ([Hagopian, 1996](#)) and Rio Grande do Sul ([Gerardi, 2015](#)), there was considerable congruence between state- and local-level political elites before and after the onset of military government. This lack of elite turnover, taken in conjunction with the fact that so many political offices could still be contested electorally, made it feasible for political actors to continue using existing strategies

⁵⁴Moreover, with respect to the case of Brazil, we provide additional context and conduct a variety of extensions of our analysis in the Appendix. For a discussion of contemporaneous institutional reforms enacted during Brazil's military government and why they do not affect the relevance of our null findings for appointed mayors, see Appendix section A.21. For evidence of the robustness of our findings to migration patterns, see Appendix section A.23. For an extension of our analysis using the election of mayors from the pro-democracy Workers' Party as the dependent variable, see Appendix section A.24. For a discussion of the potential relationship between appointed mayors and clientelism, see Appendix section A.25.

Figure 6: Linear Probability Model of Impact of Intervention on Support for Authoritarian Successor Parties in Brazil



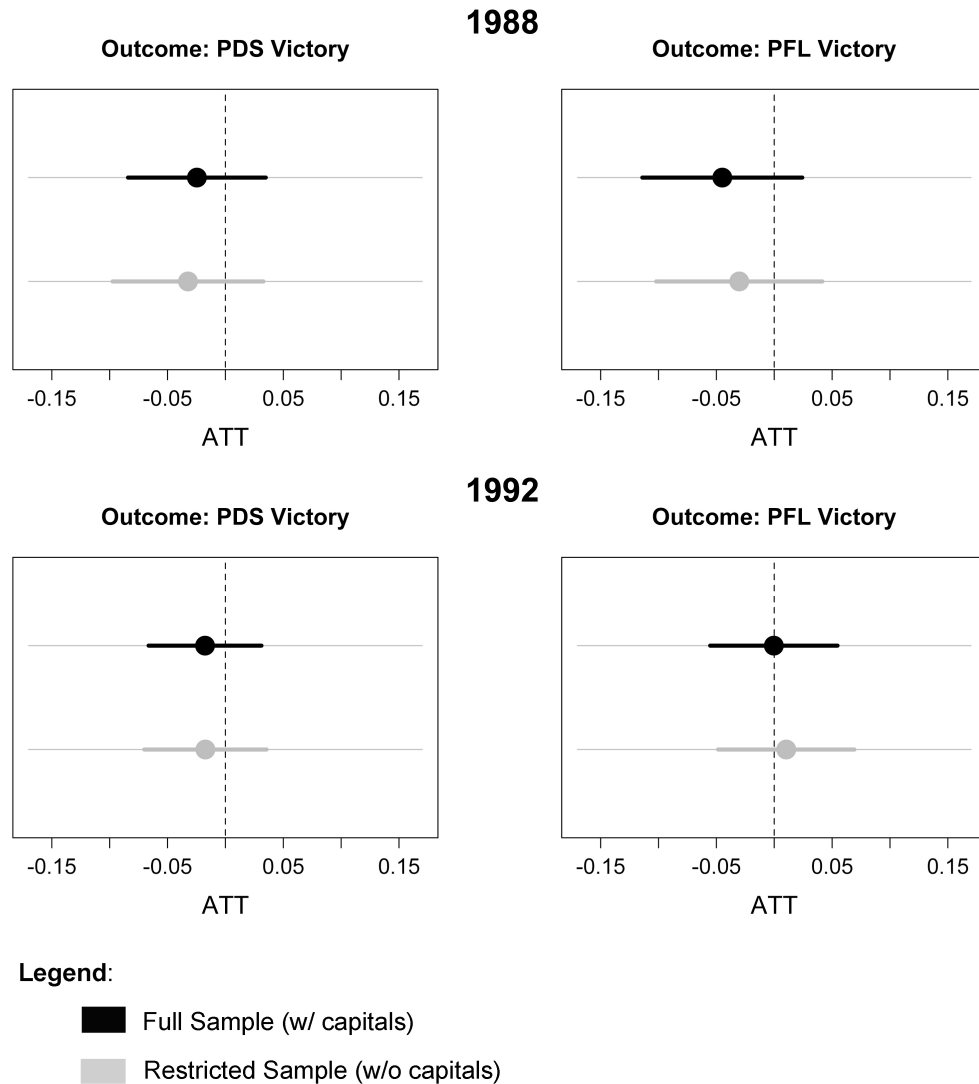
Note: Fixed effects for state included in all regressions. Detailed results for all underlying regressions can be found in Table A15 and Table A16 in the Appendix.

of political networking and mobilization in spite of military rule and local intervention.⁵⁵

⁵⁵For the case of Rio Grande do Sul, Gerardi (2015, 158, translated by the authors) notes that “the fact of someone being chosen to occupy the post of mayor via the closed recruitment system did not isolate these individuals from the electoral market ... nominated mayors supported councilmen, deputies (state, federal), politicians regularly facing elections.”

Consistent with this point, recent research on Minas Gerais demonstrates continuity in the use of local elites as vote brokers before and after the period of military rule (Gingerich, 2014; Gingerich, 2020).

Figure 7: Coarsened Exact Matching Estimates of Impact of Intervention on Support for Authoritarian Successor Parties in Brazil



Note: Detailed results for all underlying regressions can be found in Table A17 in the Appendix.

Meta-Analysis: The Manifestation of Legacies in a Variety of Contexts

Our prior analysis of two concrete country cases (based on distinct causal inference designs) has revealed empirical patterns that are consistent with our framework. However, given our theory's aspiration to explain patterns of legacy manifestation in a broad variety of contexts, it would be desirable to complement this initial set of fine-grained, case-specific results with a more extensive analysis that rests on a larger number of countries, features different types of interventions, and covers all world regions.

Such an analysis would not only allow us to examine if the suggested patterns hold for a multitude of settings, but also address three critical concerns that may remain about the previously tested cases. The first potential concern is that the treatment in the case of Poland is a *bundled treatment*, i.e., Russian imperial rule was not only associated with the denial of self-government, but also with relatively poor economic conditions. Such concerns about bundled treatments can be alleviated through the analysis of a broad number of imperial and authoritarian interventions that exhibit more variation along secondary dimensions. Specifically, if we can detect the suggested patterns across a variety of interventions that all have the denial of self-government in common—but simultaneously differ widely on other aspects—it becomes more plausible that the observed long-term effects are causally related to the denial of political autonomy (the primary commonality of the cases).

A second concern is that our country cases, although both exhibiting the characteristics of denial of local self-government, still represent distinct forms of intervention (imperial vs. authoritarian), potentially explaining (parts of the) divergence in the observed outcomes. This concern can also be alleviated if we show that the proposed regularities can be observed across a larger number of *both* imperial and authoritarian interventions, indicating that it is

not the distinct type of intervention that explains the difference in outcome magnitude.⁵⁶

A third potential concern that we seek to address through this meta-analysis is that variation in preexisting conditions—representing the possible presence of unobserved confounders that shape aspects of imperial or authoritarian rule (see [Blaydes and Grzymala-Busse, 2025](#); [Hariri, 2012](#); [Hartnett and Saleh, 2025](#); [Sinding Bentzen, Hariri and Robinson, 2019](#); [Wilfahrt, 2022](#); [Zaw, 2024](#))—could be (partly) responsible for the detected patterns in the Brazil-Poland comparison. Although we provide a discussion above that highlights the similarities between those cases, it would nevertheless be desirable to check if the predicted patterns can be found when analyzing a large number of instances with diverse initial conditions. If we identify comparable patterns across extremely heterogeneous initial contexts, it also becomes less plausible that specific preexisting conditions are responsible for our results.

For the aforementioned reasons, we conduct a broad meta-analysis in which we code 33 existing studies that investigate the long-term effects of denials of self-government through authoritarianism or imperialism. The key criteria for studies to enter our pool were that contributions needed to (1) consider denials of self-government or state repression of the population as the primary independent variable, (2) employ a dimension of political behavior meaningfully related to or directly reflecting support for or the strength of democracy as one of the dependent variables, and (3) be published in a reputable social science journal or authored by an established researcher in the field (in the case of publicly available working papers). In [Table 3](#), we list all considered studies and which type of intervention they investigate. Importantly, three of the studies differentiate explicitly between the legacies of more and less repressive types of authoritarian regimes (see observations #4, #5, #10, #11, #15, and #16), which (in each case) allowed us to obtain *two* observations for our meta-

⁵⁶For this purpose, we also present the results of a *within-intervention type* analysis by introducing a fixed effect for imperial interventions.

analysis (as we also differentiate between levels of repressiveness). This choice indicates that we also take within-study variation along our key analytical categories seriously.

We proceed by coding the different studies based on our theoretical categories to make their results comparable to each other (for details, see [Table 4](#)). Consistent with our framework, we code each study according to three relevant dimensions: (a) the length of the intervention in terms of exposed generations (with values 0, 1, 2, or 3+), (b) the level of repression (with values 0, 0.5, or 1), and (c) the relative effect size (with values 0, 1, or 2).

Our coding rationale is as follows: Since our theory identifies one generation as the critical threshold shaping the likelihood of a legacy, the most important distinction is between less than 1 generation (< 30 years), which we code as 0, and 1 or more generations. In this respect, we code every intervention that lasts longer than 60 or 90 years as 2 or 3+ generations, respectively. Given enormous differences in considered time periods between studies, we assign a maximum value of 3(+) generations to intervention length, assuming that—in line with our theory—any intervention lasting 3 or more generations has the potential to generate *considerable* shifts in the relevant dimensions of democratic political behavior.

In terms of repression, we distinguish between three different levels: 0, 0.5, and 1. These values represent the historical degree of repression in the respective context. In accordance with our theory, we code societies as 0 if historical denials of self-government allowed for some channels of participation and did *not* feature extreme levels of violent repression of regime opponents. We assign the value 0.5 if regimes feature moderate levels in both dimensions or a combination of a high and low value in each. Finally, we assign 1 if suppression does not allow for any meaningful political participation for the vast majority of citizens and features high or extremely high levels of violent repression.

To achieve comparability of outcomes, we reformulate them in terms of detrimental effects on long-term indicators related to democratic political behavior. For instance, if a study

Table 3: Studies Considered in our Meta-Analysis

Study #	Authors	Intervention Type
1	González, Muñoz and Prem (2021)	Authoritarian
2	Martinez-Bravo (2014)	Authoritarian
3	Acharya, Blackwell and Sen (2015)	Authoritarian
4 (counted twice)	Pop-Eleches and Tucker (2020)	Authoritarian (hardline communism)
5 (counted twice)	Pop-Eleches and Tucker (2020)	Authoritarian (softline communism)
6	Gingerich and Vogler (2021)	Authoritarian
7	de Leeuw, Rekker, Azrout and van Spanje (2021)	Authoritarian
8	Charnysh and Ziblatt (2021)	Authoritarian
9	Homola, Pereira and Tavits (2020)	Authoritarian
10 (counted twice)	Bernhard and Karakoç (2007)	Authoritarian (authoritarianism)
11 (counted twice)	Bernhard and Karakoç (2007)	Authoritarian (totalitarianism)
12	Doucette (2024)	Authoritarian
13	Xue (2021)	Authoritarian
14	Krishnarajan, Doucette and Andersen (2023)	Authoritarian
15 (counted twice)	Pop-Eleches and Tucker (2014)	Authoritarian (regular communism)
16 (counted twice)	Pop-Eleches and Tucker (2014)	Authoritarian (hardline communism)
17	Giuliano and Nunn (2013)	Authoritarian
18	Guiso, Sapienza and Zingales (2016)	Authoritarian
19	Dehdari and Gehring (2022)	Authoritarian
20	Surdea-Hernea and Ploeanu (2025b)	Authoritarian
21	Miller (2015)	Authoritarian
22	Hicken and Martínez Kuhonta (2011)	Authoritarian
23	Sidel (2008)	Authoritarian
24	Pérez-Liñán and Mainwaring (2013)	Authoritarian
25	Grosfeld and Zhuravskaya (2015)	Imperial
26	Nathan (2019)	Imperial
27	Charnysh and Piqué (2024)	Imperial
28	Lee and Paine (2019)	Imperial
29	Lechler and McNamee (2018)	Imperial
30	Besley and Persson (2019)	Imperial
31	Michalopoulos and Papaioannou (2016)	Imperial
32	Wucherpfennig, Hunziker and Cederman (2016)	Imperial
33	Zachary, Deloughery and Downes (2017)	Imperial
34	Pop-Eleches (2007)	Imperial
35	Lankina and Libman (2019)	Imperial
36	Aaskoven (2022a)	Imperial

suggests that towns *with* historical self-government have *lower* long-term vote shares for an authoritarian party (as in [Doucette \(2024\)](#)), we reformulate the effect magnitude as the *higher* vote share of towns *without* historical self-government, which is the logical equivalent.

Additionally, based on the authors' provided evaluations as well as precise effect magnitudes in percent change (whenever these are provided in the study), we recode the effect values on outcomes to 0, 1, or 2. In this coding scheme, 0 represents a minor or non-existent legacy effect (if precise magnitudes in terms of percent changes are offered, this typically corresponds to a change of $< 5\%$), 1 represents a moderate effect ($> 5\%$ and $< 10\%$), and 2 represents a large effect (typically a change of $> 10\%$). In studies that do not offer magnitudes in terms of percent changes, we either derive values from the provided summary statistics or coefficient values or rely on the authors' descriptions and assessments regarding substantive effect size. Although we aim to be as transparent as possible regarding our coding rationale, some choices are inherently subjective. We fully acknowledge this shortcoming.⁵⁷

In studies that feature outcomes for multiple countries, we rely on averages in all three dimensions. For instance, when a study analyzes the effects of communist rule across several Eastern Europe countries (as in [Pop-Eleches and Tucker \(2020\)](#)), we use our knowledge of the average intervention length in those cases to code the number of generations as 1 (since communist rule lasted for approximately 45 years, which is > 30 but < 60 years). Similarly, in those multi-country studies, we use the observed effects across the entire population to assign average values to the other relevant dimensions of repression levels and effect size.

⁵⁷We also code a key insight from one entirely qualitative study by [Sidel \(2008\)](#). In this case, we assess the outcome—which is the prevention of the emergence of a democracy-supporting bourgeoisie—as equivalent to an effect magnitude of 2, representing a large effect.

Table 4: Detailed Coding of Results of All Studies Considered in Our Meta-Analysis

Study #	Outcome of Interest	Country	Period	Gen. #	Repression	Effect
1	Vote share advantage of prior authoritarian mayors in post-authoritarian period	Chile	1973–1990	0	1	1
2	Likelihood of authoritarian successor party to win elections after end of authoritarianism	Indonesia	1967–1998	1	1	1
3	Black voter turnout reduction (2008) (effect of increase in historical slavery from 0 to 99%)	United States	1526–1865	3+	1	2
4	Increase in left-authoritarian citizens after 45 years of hardline communism	Multiple	1945–1990	1	1	2
5	Increase in left-authoritarian citizens after 45 years of softline communism	Multiple	1945–1990	1	0.5	1
6	Increase in authoritarian party vote (areas without history of self-government vs. with history)	Germany	1351–1871	3+	1	2
7	Decrease in support for democracy among citizens aligned with past authoritarian regime	Multiple	Different	1	0.5	1
8	Nazi Party vote share increase (Great Depression) in districts with historical vote manipulation	Germany	1871–1918	1	0.5	1
9	Vote increase of extreme right wing parties (within 50 km of former Nazi concentration camp)	Germany	1933–1945	0	1	0
10	Reduction of participation in civil society organizations in former authoritarian states	Multiple	Different	1	0.5	1
11	Reduction of participation in civil society organizations in former totalitarian states	Multiple	Different	1	1	2
12	Increase in vote share for the Nazi Party in towns without historical self-government (vs. with)	Germany	1371–1745	3+	0.5	2
13	Reduction in civic engagement (charity formation) in areas with historical state repression	China	1661–1788	3+	1	2
14	Decrease in democracy support if youth spent in economically successful authoritarian regimes	Multiple	Different	2	0.5	1
15	Decrease in support for democracy in the wake of regular communist regimes	Multiple	Different	1	0.5	1
16	Decrease in support for democracy in the wake of hardline communist regimes	Multiple	Different	1	1	2
17	Decrease in support for democracy in countries without a history of local democracy (vs. with)	Multiple	Different	1	0.5	1
18	Decrease in civic capital (nonprofit organizations p.c.) in cities without historical self-government	Italy	Different	3+	0.5	1
19	Increase in voting based on regional identity vs. national identity due to repressive policies	France	1871–1953	2	0.5	2
20	Increase in severe injuries during anti-regime confrontations in areas with severe historical repression	Romania	1945–1989	1	1	2
21	Decrease in democratic survival time resulting from prior absence of elections under authoritarianism	Multiple	Different	1	0.5	2
22	Increase in electoral volatility resulting from the coercive suppression of political competition	Philippines	1972–1986	0	1	2
23	Prevention of a democracy-supporting bourgeoisie through suppression of economic self-determination	Multiple	Different	1	0.5	2
24	Decrease in democracy score as a result of persistent authoritarianism (vs. persistent democracy)	Multiple	1900–1977	2	1	2
25	Decline in positive attitudes toward democracy from repressive rule (Russia vs. Austria)	Poland	1815–1914	3+	1	2
26	Anti-democratic vote coordination/clustering, reflecting clientelism (invented chiefs vs. no chiefs)	Ghana	1901–1957	2	0.5	1
27	Voting for post-authoritarian (ex-communist) parties in post-Cold War election	Poland	1939–1945	0	1	0
28	British colonial legacy on autocracy/democracy levels (30 years after independence) (vs. other empires)	Multiple	Different	2	0	0
29	Reduction of voter turnout in areas with colonially approved/appointed chiefs	Namibia	1884–1915	1	0.5	2
30	Negative impact of imperial rule (colonialism/USSR rule) on democratic values	Multiple	Different	1	0.5	1
31	Increase in likelihood of ethnic group discrimination and civil conflict (from colonial partitioning)	Multiple	1884–1960s	2	0.5	1
32	Increase in likelihood of civil conflict that results from previous suppression of local self-government	Multiple	1880s–1960s	2	0.5	2
33	Decrease in the value of economic exchange resulting from coerced replacement of political leader	Multiple	Different	0	0.5	2
34	Decrease in democracy score as a result of high institutional (imperial) penetration by communism	Multiple	Different	2	1	2
35	Decrease in electoral quality as a result of historical Communist party saturation (min. to max.)	Ukraine	1920–1991	2	1	2
36	Decrease in vote for European cooperation (high terror vs. low terror during foreign occupation)	Denmark	1940–1945	0	1	1

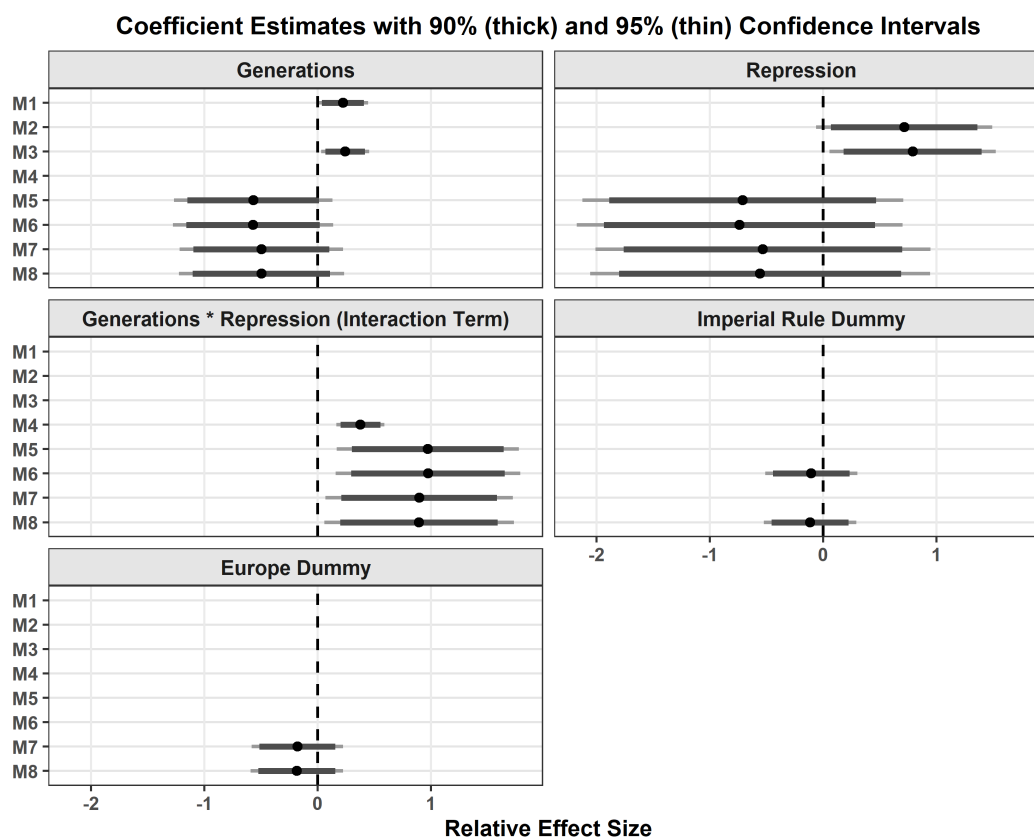
In addition to all aforementioned aspects of the included studies, we also introduce a dummy variable that takes on the value 1 for contributions that deal with “imperialism” as the intervention mode. Furthermore, to account for the possibility that the results are different between Europe and other world regions, we include a dummy for studies that primarily or exclusively focus on the European continent (with value 1 if this is the case).⁵⁸ The inclusion of these fixed effects allows us to limit our analysis to within-category variation.

Next, we evaluate patterns in our data via linear regression. The findings are summarized in [Figure 8](#) and [Table 5](#). Despite the small number of only 36 observations, the results are striking. First, both the length of intervention as measured in generations and its level of repression have individually significant effects (as shown in columns 1–3).⁵⁹ Both intervention dimensions significantly increase long-term outcomes that either directly reflect or are plausibly associated with detrimental effects on democracy. Also, as columns 4–8 demonstrate, in line with our framework, there is a pronounced interaction effect between length and repressiveness. In all models, the interaction effect is significant at $\alpha < 0.05$ and the overall model displays statistical significance as well (as represented in the F-statistic). This confirms our expectations about the interactive effect of repression and intervention length. As columns 6–8 show, the effects remain substantively similar when we compare only within relevant categories through the inclusion of dummy variables for imperialism and Europe. In light of the small sample, the combined evidence from all models strongly speaks in favor of our theory. The high significance of the interaction term in particular underscores the relevance of cases on the *main diagonal* of our theoretical framework. Finally, all key findings presented and discussed here—especially the high level of significance of the interaction term—are robust to the use of ordered logit models.

⁵⁸Approximately 40% of all studies are either exclusively or primarily focused on Europe.

⁵⁹Given the small sample size, we use a significance threshold of $\alpha < 0.1$.

Figure 8: Meta-Analysis Results



Note: This figure represents a coefficient plot that includes information on the magnitude and statistical significance of all coefficients relevant to our meta-analysis. The referenced models are in Table 5.

Table 5: Meta-Analysis Results

	<i>Dependent variable:</i>							
	Relative Effect Size							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Generations	0.224** (0.109)		0.244** (0.104)		-0.567 (0.343)	-0.569 (0.347)	-0.495 (0.353)	-0.495 (0.357)
Repression		0.714* (0.382)	0.789** (0.360)		-0.711 (0.695)	-0.739 (0.705)	-0.532 (0.725)	-0.557 (0.734)
Gen. * Repr.				0.379*** (0.104)	0.973** (0.395)	0.975** (0.400)	0.897** (0.405)	0.895** (0.410)
Imperial Rule Dummy						-0.106 (0.199)		-0.116 (0.200)
Europe Dummy							-0.177 (0.197)	-0.184 (0.200)
Constant	1.127*** (0.186)	0.929*** (0.295)	0.529 (0.325)	1.065*** (0.140)	1.787*** (0.594)	1.845*** (0.610)	1.703*** (0.603)	1.762*** (0.618)
Observations	36	36	36	36	36	36	36	36
R ²	0.110	0.093	0.223	0.280	0.347	0.353	0.364	0.371
Adjusted R ²	0.084	0.067	0.176	0.259	0.286	0.270	0.281	0.266
F Statistic	4.215**	3.498*	4.741**	13.238***	5.670***	4.229***	4.428***	3.535**

Note: OLS

*p<0.1; **p<0.05; ***p<0.01

Conclusion

Historically, the suppression of local self-government by empires or centralized authoritarian regimes was common around the world (Simpser, Slater and Wittenberg, 2018). When can we expect such denials of political autonomy to result in sustained legacies manifested in voting outcomes? We argue that *enduring* interventions associated with *encompassing* repression have the highest potential to sustainably change political behavior. In contrast, interventions with *limited* duration and repression are the least likely to produce a legacy.

To illustrate the applicability of our argument, we first examine the distinctive episodes of suppression that occurred in Brazil during the military regime and in Poland during its imperial partition. In Poland, there were key differences between the imperial powers that occupied it for a century: Prussia allowed for broad political participation by the Poles and its rule was comparatively constrained, with a limited use of violence against the population. By contrast, Russia's rule remained highly repressive in both dimensions throughout. Moreover, in Brazil, the authoritarian regime installed its allies as mayors of certain cities, but did so in a manner that was limited in both duration and repressiveness. In addition, we present a meta-analysis that covers a broad variety of cases in all world regions, exhibiting significant divergence across the key theoretical dimensions of intervention length and repressiveness.

The study's results highlight that episodes of suppression have *vastly* different potentials to create legacies depending on their characteristics. In Poland, citizens in the areas that were subject to more than a century of repressive and militarized foreign rule (through Russia) show a clear tendency to elect mayors who belong to the populist and anti-democratic right-wing party Law and Justice. In Brazil, on the other hand, the experience of externally appointed mayors imposed during the military regime did not appear to leave a legacy in terms of support for authoritarian successor parties. The shorter duration of the interven-

tion and its relatively limited repressive scope help explain this outcome. Additionally and consistent with our theory, the results of the meta-analysis highlight that both the length of an intervention and its level of repressiveness are key determinants of the materialization of a legacy. In particular—and corresponding with our proposition about the *interactive* character of the two relevant dimensions—interventions that *combine* high repressiveness and longevity are most likely to lead to a legacy.

Since our study has revealed initial evidence in accordance with our theory, the next step in the research agenda would be to investigate how case-specific nuances may amplify or attenuate the proposed mechanisms. Thus, future studies that consider further interventions could more comprehensively examine divergence in initial conditions and fine-grained differences in intervener-population interactions over time. Another potential area of inquiry would be how intertemporal variation in repression consistency shapes legacy formation.

Beyond the specific contribution to understanding the consequences of suppression of self-government, this paper offers a template for future comparative research on legacy instantiation. As we see it, there are five critical steps in such endeavors. First, one selects a class of potentially legacy-inducing events (e.g., demographic shocks, labor coercion, suppression of self-government). Second, one chooses the outcome(s) for which legacies of class events will be assessed (e.g., voting behavior, economic development, political violence). Third, given the choice of event class and outcome(s), one develops a theory relating variation in the characteristics of events within the class (e.g., size of the demographic shock, length of suppression, severity of coercion) to variation in the likelihood that a legacy is instantiated.⁶⁰ Fourth, one selects cases that permit the theory to be tested. Said cases

⁶⁰Importantly, theories of this type are *limited* in that they link a *specific class of events* to legacies defined on a *specific outcome or set of outcomes*. They do not consist of generic statements about how *any* class of event might lead to a legacy for *any* type of outcome.

must ideally exhibit appropriate cross-case and within-case variation to allow for rigorous causal inference.⁶¹ Finally, one empirically assesses the existence of a legacy in the cases and situates the findings relative to the expectations generated by the theory.

The virtue of such an approach is that it can simultaneously respect existing evidentiary standards for establishing the existence or non-existence of a legacy in any given case, while leveraging the comparative method to improve understanding about the aspects of events that make them more or less likely to produce lasting change. Naturally, adding a cross-national dimension to such studies may present a challenge. We harbor no illusions that finding cases with precisely the right cross-case and within-case variation will be effortless.

Finally, our template requires that potential legacy-inducing events be defined broadly enough to make a comparative approach possible. This may lead to seemingly unconventional studies, with cases potentially differing in contextual details or historical setting. While cognizant of the risks of conceptual stretching (Sartori, 1970), we believe that movement in this direction is salutary for the political economy literature. First, this is because we believe that the question of why legacies do or do not form—a puzzle that *necessitates* a comparative lens—is a problem of first-order importance. Second, situating key aspects of national experiences into *classes* of legacy-inducing events contributes to knowledge accumulation, since doing so clarifies the theoretical payoff of the results from country case studies. Third, findings from studies based on multiple (partly divergent) cases also allow for critical reflection of variation in historical circumstances, including contextual commonalities and differences between the cases that may affect empirical generalizability. In sum, there is a great deal to be gained from a comparative approach to the study of historical legacies.

⁶¹With respect to the former, there must be variation across the cases in the event characteristics specified by the theory as driving the likelihood of a legacy. With respect to the latter, there must be internal spatial variation in the presence of the potentially legacy-inducing event.

References

- Aaskoven, Lasse. 2022a. "Foreign occupation and support for international cooperation: Evidence from Denmark." *World Politics* 74(2):285–325.
- Aaskoven, Lasse. 2022b. "The Political Effects of Wealth Inequality: Evidence from a Danish Land Reform." *Comparative Politics* 55(1):143–164.
- Abad, Leticia Arroyo and Noel Maurer. 2021. "History never really says goodbye: a critical review of the persistence literature." *Journal of Historical Political Economy* 1(1):31–68.
- Abramson, Paul R. and Ronald Inglehart. 1992. "Generational replacement and value change in eight West European societies." *British Journal of Political Science* 22(2):183–228.
- Abrucio, Fernando Luiz. 1998. *Os barões da federação: os governadores e a redemocratização brasileira*. Editora Hucitec.
- Acemoglu, Daron, Georgy Egorov and Konstantin Sonin. 2021. Institutional change and institutional persistence. In *The Handbook of Historical Economics*, ed. Alberto Bisin and Giovanni Federico. Elsevier pp. 365–389.
- Acharya, Avidit, Matthew Blackwell and Maya Sen. 2015. "A culture of disenfranchisement: How American slavery continues to affect voting behavior." *Working Paper*.
- Acharya, Avidit, Matthew Blackwell and Maya Sen. 2023. Historical Persistence. In *The Oxford Handbook of Historical Political Economy*, ed. Jeffery A. Jenkins and Jared Rubin. Oxford University Press.
- Acharya, Avidit, Maya Sen and Matthew Blackwell. 2018. *Deep roots: How slavery still shapes southern politics*. Princeton University Press.
- Andersen, David, Carl Henrik Knutsen and Svend-Erik Skaaning. 2025. "Dimensions of State Capacity and Modes of Democratic Breakdown." *Political Studies*.
- Andersen, David, Jørgen Møller, Lasse Lykke Rørbæk and Svend-Erik Skaaning. 2016. State capacity and political regime stability. In *The State-Democracy Nexus: Conceptual Distinctions, Theoretical Perspectives, and Comparative Approaches*, ed. Jørgen Møller and Svend-Erik Skaaning. Routledge pp. 103–123.
- Ayllón, Sara, Ángel Alsina and Jordi Colomer. 2019. "Teachers' involvement and students' self-efficacy: Keys to achievement in higher education." *PloS ONE* 14(5):e0216865.
- Bartels, Larry M. and Simon Jackman. 2014. "A generational model of political learning." *Electoral Studies* 33:7–18.
- Beck, Paul Allen and M. Kent Jennings. 1991. "Family traditions, political periods, and the development of partisan orientations." *Journal of Politics* 53(3):742–763.
- Bernhard, Michael and Ekrem Karakoç. 2007. "Civil society and the legacies of dictatorship." *World Politics* 59(4):539–567.
- Besley, Timothy and Torsten Persson. 2019. "Democratic values and institutions." *American Economic Review: Insights* 1(1):59–76.
- Bisin, Alberto, Jared Rubin, Avner Seror and Thierry Verdier. 2024. "Culture, institutions and the long divergence." *Journal of Economic Growth* 29(1):1–40.
- Blakeley, Ruth. 2012. State Violence as State Terrorism. In *The Ashgate Research Companion to Political Violence*, ed. Marie Breen-Smyth. Ashgate pp. 63–78.
- Blaydes, Lisa and Anna Grzymala-Busse. 2025. "Historical State Formation within and

- beyond Europe.” *World Politics* 77(1):205–222.
- Blondel, Jean. 1957. *As Condições da Vida Política no Estado da Paraíba*. FGV.
- Boas, Taylor. 2022. Potential Mistakes, Plausible Options: Establishing the Legacy of Hypothesized Critical Junctures. In *Critical junctures and historical legacies: insights and methods for comparative social science*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 373–379.
- Bustikova, Lenka and Cristina Corduneanu-Huci. 2017. “Patronage, trust, and state capacity: The historical trajectories of clientelism.” *World Politics* 69(2):277–326.
- Callis, Anna, Thad Dunning and Guadalupe Tuñón. 2024. Causal Inference and Knowledge Accumulation in Historical Political Economy. In *The Oxford Handbook of Historical Political Economy*, ed. Jeffery A. Jenkins and Jared Rubin. Oxford University Press pp. 55–74.
- Capoccia, Giovanni. 2015. Critical Junctures and Institutional Change. In *Advances in Comparative Historical Analysis in the Social Sciences*, ed. James Mahoney and Kathleen Thelen. Cambridge University Press pp. 147–179.
- Capoccia, Giovanni and R. Daniel Kelemen. 2007. “The study of critical junctures: Theory, narrative, and counterfactuals in historical institutionalism.” *World Politics* 59(3):341–369.
- Carter, Christopher and Daniel W. Gingerich. 2025. “National identity after conquest.” *American Journal of Political Science*.
- Carvalho, José Murilo de. 2016. *Cidadania no Brasil: O Longo Caminho*. Civilização Brasileira.
- Carvalho, José Murilo de. 1966. “Barbacena: A Família, a Política e uma Hipótese.” *Revista Brasileira de Estudos Políticos* (20):153–193.
- Carvalho, Orlando M. 1958. *Ensaio de sociologia eleitoral*. Universidade de Minas Gerais. Revista Brasileira de Estudos Políticos.
- Cederman, Lars-Erik, Andreas Wimmer and Brian Min. 2010. “Why do ethnic groups rebel? New data and analysis.” *World Politics* 62(1):87–119.
- Chapman, Tim. 2006. *The Congress of Vienna: Origins, processes and results*. Routledge.
- Charasz, Pawel. 2022. “Essays on the Political Economy of City Status.” Ph.D. Dissertation, Duke University.
- Charasz, Pawel and Jan P. Vogler. 2021. “Does EU funding improve local state capacity? Evidence from Polish municipalities.” *European Union Politics* 22(3):446–471.
- Charnysh, Volha. 2017. “The rise of Poland’s far right: How extremism is going mainstream.” *Foreign Affairs*.
- Charnysh, Volha and Daniel Ziblatt. 2021. “Consequences of Competition Under Autocracy for Democratic Elections: From Imperial to Weimar Germany.” *Working Paper*, Available at <http://charnysh.net/>.
- Charnysh, Volha and Leonid Peisakhin. 2022. “The role of communities in the transmission of political values: Evidence from forced population transfers.” *British Journal of Political Science* 52(1):238–258.
- Charnysh, Volha and Ricardo Piqué. 2024. “Razing the Church: The Enduring Effect of Nazi Repression in Poland.” Available at SSRN 4579429, <https://dx.doi.org/10.2139/ssrn.4579429>.
- Chaudhary, Latika, Jared Rubin, Sriya Iyer and Anand Shrivastava. 2020. “Culture and

- colonial legacy: Evidence from public goods games.” *Journal of Economic Behavior & Organization* 173:107–129.
- Chilcote, Ronald H. 1990. *Power and the Ruling Classes in Northeast Brazil: Juazeiro and Petrolina in Transition*. Cambridge University Press.
- Cirone, Alexandra and Thomas B. Pepinsky. 2022. “Historical persistence.” *Annual Review of Political Science* 25:241–259.
- Coburn, Noah and Anna Larson. 2014. *Derailing democracy in Afghanistan: Elections in an unstable political landscape*. Columbia University Press.
- Collier, David. 2022. Critical juncture framework and the five-step template. In *Critical junctures and historical legacies: insights and methods for comparative social science*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 33–52.
- Corduneanu-Huci, Cristina and Maryjane Osa. 2003. “Running uphill: Political opportunity in non-democracies.” *Comparative Sociology* 2(4):605–629.
- Curtice, Travis B and Brandon Behlendorf. 2021. “Street-level repression: Protest, policing, and dissent in Uganda.” *Journal of Conflict Resolution* 65(1):166–194.
- Daly, Sarah Zukerman. 2019. “Voting for victors: why violent actors win postwar elections.” *World Politics* 71(4):747–805.
- Davies, Norman. 2005a. *God’s Playground A History of Poland: Volume I: The Origins to 1795*. Columbia University Press.
- Davies, Norman. 2005b. *God’s Playground A History of Poland: Volume II: 1795 to the Present*. Columbia University Press.
- De Juan, Alexander, Christian Gläsel, Felix Haass and Adam Scharpf. 2025. “The political effects of witnessing state atrocities: Evidence from the Nazi death marches.” *Comparative Political Studies* 58(14):3143–3178.
- De Juan, Alexander and Jan Henryk Pierskalla. 2017. “The comparative politics of colonialism and its legacies: An introduction.” *Politics & Society* 45(2):159–172.
- de Leeuw, Sijfra E., Roderik Rekker, Rachid Azrout and Joost H. P. van Spanje. 2021. “Are would-be authoritarians right? Democratic support and citizens’ left-right self-placement in former left-and right-authoritarian countries.” *Democratization* 28(2):414–433.
- De Mesquita, Bruce Bueno and George W. Downs. 2006. “Intervention and democracy.” *International Organization* 60(3):627–649.
- Dehdari, Sirus H. and Kai Gehring. 2022. “The origins of common identity: Evidence from Alsace-Lorraine.” *American Economic Journal: Applied Economics* 14(1):261–292.
- Dell, Melissa, Nathan Lane and Pablo Querubin. 2018. “The historical state, local collective action, and economic development in Vietnam.” *Econometrica* 86(6):2083–2121.
- Di Liberto, Adriana and Marco Sideri. 2015. “Past dominations, current institutions and the Italian regional economic performance.” *European Journal of Political Economy* 38:12–41.
- Dinas, Elias and Ksenia Northmore-Ball. 2020. “The ideological shadow of authoritarianism.” *Comparative Political Studies* 53(12):1957–1991.
- Dinas, Elias, Vasiliki Fouka and Alain Schläpfer. 2021. “Family history and attitudes toward out-groups: evidence from the European refugee crisis.” *Journal of Politics* 83(2):647–661.
- do Rêgo, André Heráclio. 2008. *Família e coronelismo no Brasil: uma história de poder*. A

Girafa.

- Doucette, Jonathan Stavnskaer. 2024. "Pre-Modern Institutions and Later Support for Autocrats in Democratic Elections." *British Journal of Political Science* pp. 1–9.
- Downes, Alexander B. and Jonathan Monten. 2013. "Forced to be free?: Why foreign-imposed regime change rarely leads to democratization." *International Security* 37(4):90–131.
- Dunning, Thad. 2012. *Natural experiments in the social sciences: A design-based approach*. Cambridge University Press.
- Egorov, Georgy and Konstantin Sonin. 2011. "Dictators and their viziers: Endogenizing the loyalty–competence trade-off." *Journal of the European Economic Association* 9(5):903–930.
- Finkel, Evgeny and Scott Gehlbach. 2020. *Reform and rebellion in weak states*. Cambridge University Press.
- Flynn, Peter. 1978. *Brazil: A Political Analysis*. Westview Press.
- Fontana, Nicola, Tommaso Nannicini and Guido Tabellini. 2023. "Historical roots of political extremism: The effects of Nazi occupation of Italy." *Journal of Comparative Economics* 51(3):723–743.
- Fouka, Vasiliki and Hans-Joachim Voth. 2023. "Collective remembrance and private choice: German–Greek conflict and behavior in times of crisis." *American Political Science Review* 117(3):851–870.
- Franco, Amanda Cristina. 2017. Os primeiros registros do uso de águas termais e a formação das estâncias hidrominerais no Brasil. In *Termalismo e crenoterapia no Brasil e no mundo*, ed. Fernando Hellmann and Daniel Maurício de Oliveira Rodrigues. Editora Unisul pp. 55–75.
- Friedman, Sally and David Schultz. 2024. *Generational Politics in the United States: From the Silents to Gen Z and Beyond*. University of Michigan Press.
- Gailmard, Sean. 2024. *Agents of empire: English Imperial governance and the making of American political institutions*. Cambridge University Press.
- Gella, Aleksander. 1998. *Development of Class Structure in Eastern Europe: Poland and Her Southern Neighbors*. SUNY Press.
- Gerardi, Dirceu. 2015. "Recrutamento, Nomeação e Carreiras Políticas de Prefeitos em ASN do Rio Grande do Sul." Ph.D. Dissertation, Pontifícia Universidade Católica do Rio Grande do Sul.
- Ghitza, Yair, Andrew Gelman and Jonathan Auerbach. 2023. "The great society, Reagan's revolution, and generations of presidential voting." *American Journal of Political Science* 67(3):520–537.
- Gingerich, Daniel W. 2014. "Brokered politics in Brazil: an empirical analysis." *Quarterly Journal of Political Science* 9(3):269–300.
- Gingerich, Daniel W. 2020. "Buying Power: Electoral Strategy before the Secret Vote." *American Political Science Review* 114(4):1086–1102.
- Gingerich, Daniel W. and Jan P. Vogler. 2021. "Pandemics and Political Development: The Electoral Legacy of the Black Death in Germany." *World Politics* 73(3).
- Giuliano, Paola and Nathan Nunn. 2013. "The Transmission of Democracy: From the Village to the Nation-State." *American Economic Review* 103(3):86–92.
- González, Felipe, Pablo Muñoz and Mounu Prem. 2021. "Lost in transition? The persistence

- of dictatorship mayors.” *Journal of Development Economics* 151(1-15).
- Gouda, Moamen and Anouk S. Rigterink. 2025. “Historical Slavery Predicts Contemporary Violent Crime: Inequality and Political Attitudes as Mediators.” *Social Science Quarterly* 106(5):e70078.
- Grabowski, Lorie J. Schabo, Kathleen Thiede Call and Jeylan T. Mortimer. 2001. “Global and economic self-efficacy in the educational attainment process.” *Social Psychology Quarterly* 64(2):164–179.
- Grasso, Maria Teresa, Stephen Farrall, Emily Gray, Colin Hay and Will Jennings. 2019. “Thatcher’s children, Blair’s babies, political socialization and trickle-down value change: An age, period and cohort analysis.” *British Journal of Political Science* 49(1):17–36.
- Grosfeld, Irena and Ekaterina Zhuravskaya. 2015. “Cultural vs. economic legacies of empires: Evidence from the partition of Poland.” *Journal of Comparative Economics* 43(1).
- Guardado, Jennifer. 2024. The Historical Political Economy of Colonialism. In *The Oxford Handbook of Historical Political Economy*, ed. Jeffery A. Jenkins and Jared Rubin. Oxford University Press pp. 559–580.
- Guardado, Jenny. 2018. “Office-selling, corruption, and long-term development in Peru.” *American Political Science Review* 112(4):971–995.
- Guiso, Luigi, Paola Sapienza and Luigi Zingales. 2016. “Long-term persistence.” *Journal of the European Economic Association* 14(6):1401–1436.
- Haddad, Mary Alice. 2012. *Building democracy in Japan*. Cambridge University Press.
- Hagopian, Francis. 1996. *Traditional Politics and Regime Change in Brazil*. Cambridge University Press.
- Handlin, Samuel. 2022. A New Critical Juncture? Analyzing Party Transformation in South American Politics. In *Critical junctures and historical legacies: insights and methods for comparative social science*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 333–349.
- Hariri, Jacob Gerner. 2012. “The autocratic legacy of early statehood.” *American Political Science Review* 106(3):471–494.
- Hartnett, Allison S. and Mohamed Saleh. 2025. “Precolonial Elites and Colonial Redistribution of Political Power.” *American Political Science Review*.
- Hechter, Michael. 2013. *Alien rule*. Cambridge University Press.
- Heinz, Wolfgang S. and Hugo Frühling. 1999. *Determinants of gross human rights violations by state and state-sponsored actors in Brazil, Uruguay, Chile, and Argentina, 1960–1990*. Martinus Nijhoff.
- Hicken, Allen and Erik Martínez Kuhonta. 2011. “Shadows from the past: Party system institutionalization in Asia.” *Comparative Political Studies* 44(5):572–597.
- Higashijima, Masaaki and Austin M. Mitchell. 2025. “Democracy and state development: Patterns of public spending in the first two waves of democratization.” *Comparative Political Studies* 58(11):2436–2472.
- Hill, Seth J. 2017. “Learning together slowly: Bayesian learning about political facts.” *Journal of Politics* 79(4):1403–1418.
- Hoensch, Jörg Konrad. 1990. *Geschichte Polens [History of Poland]*. Verlag Eugen Ulmer.
- Homola, Jonathan, Miguel M. Pereira and Margit Tavits. 2020. “Legacies of the Third Reich: Concentration camps and out-group intolerance.” *American Political Science Review*

- 114(2):573–590.
- Howe, Philip J., Edina Szöcsik and Christina I. Zuber. 2022. “Nationalism, class, and status: How nationalists use policy offers and group appeals to attract a new electorate.” *Comparative Political Studies* 55(5):832–868.
- Hryniewicz, Janusz. 1996. Czynniki rozwoju regionalnego [Factors of regional development]. In *Oblicza polskich regionów [Faces of the regions of Poland]*, ed. B. Jałowiecki. EU-ROREG - M. Swianiewicz pp. 89–129.
- Iacus, Stefano, Gary King and Giuseppe Porro. 2012. “Causal Inference Without Balance Checking: Coarsened Exact Matching.” *Political Analysis* 20(1):1–24.
- IBGE. 2011. *Evolução da divisão territorial do Brasil 1872 - 2010*. Instituto Brasileiro de Geografia e Estatística (IBGE).
- Inglehart, Ronald. 1977. *The silent revolution: Changing values and political styles among Western publics*. Princeton University Press.
- Iyer, Lakshmi. 2010. “Direct versus indirect colonial rule in India: Long-term consequences.” *The Review of Economics and Statistics* 92(4):693–713.
- Jocker, Thomas, Wouter van der Brug and Roderik Rekker. 2024. “Growing up in a polarized party system: Ideological divergence and partisan sorting across generations.” *Political Behavior* 46(4):2263–2286.
- Kaufman, Robert. 2022. Critical Junctures and Contemporary Latin America: A Note of Caution. In *Critical junctures and historical legacies: insights and methods for comparative social science*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 381–386.
- Kim-Leffingwell, Sanghoon. 2023. “Alternative legacies of authoritarianism: Pro-dictator bias in ideology.” *Party Politics* 29(3):580–586.
- Kitschelt, Herbert. 1999. *Post-communist party systems: Competition, representation, and inter-party cooperation*. Cambridge University Press.
- Kitschelt, Herbert and Matthew Singer. 2018. Linkage strategies of authoritarian successor parties. In *Life after Dictatorship: Authoritarian Successor Parties Worldwide*, ed. James Loxton and Scott Mainwaring. Cambridge University Press pp. 53–83.
- Kopstein, Jeffrey S. and Jason Wittenberg. 2018. *Intimate violence: Anti-Jewish pogroms on the eve of the Holocaust*. Cornell University Press.
- Krishnarajan, Suthan, Jonathan Doucette and David Andersen. 2023. “Early-adulthood economic experiences and the formation of democratic support.” *British Journal of Political Science* 53(2):387–406.
- Krosnick, Jon A. and Duane F. Alwin. 1989. “Aging and susceptibility to attitude change.” *Journal of Personality and Social Psychology* 57(3):416.
- Kuk, Leszek. 2021. Die Bestimmungen des Wiener Kongresses hinsichtlich Polens vor dem Hintergrund der polnischen Frage im 19. Jahrhundert (1795–1918) [The provisions of the Congress of Vienna concerning Poland against the background of the Polish Question in the 19th century (1795–1918)]. In *Die polnische Frage und der Wiener Kongress 1814–1815 [The Polish Question and the Congress of Vienna 1814–1815]*, ed. Bogusław Dybaś. Verlag der Österreichischen Akademie der Wissenschaften.
- Lange, Matthew K. 2004. “British colonial legacies and political development.” *World Development* 32(6):905–922.
- Lankina, Tomila and Alexander Libman. 2019. “Soviet Legacies of Economic Development,

- Oligarchic Rule, and Electoral Quality in Eastern Europe's Partial Democracies: The Case of Ukraine." *Comparative Politics* 52(1):127–148.
- Leal, Victor Nunes. 1949. *Coronelismo, enxada, e voto: O município e o regime representativo no Brasil*. Revisita Forense.
- LeBas, Adrienne and Lauren E. Young. 2024. "Repression and dissent in moments of uncertainty: Panel data evidence from Zimbabwe." *American Political Science Review* 118(2):584–601.
- Lechler, Marie and Lachlan McNamee. 2018. "Indirect colonial rule undermines support for democracy: Evidence from a natural experiment in Namibia." *Comparative Political Studies* 51(14):1858–1898.
- Lee, Alexander and Jack Paine. 2019. "British colonialism and democracy: Divergent inheritances and diminishing legacies." *Journal of Comparative Economics* 47(3):487–503.
- Leipziger, Lasse Egdal, Lasse Lykke Rørbæk and Svend-Erik Skaaning. 2025. "Does Ethnopolitical Exclusion Cause Civil War Onset via Grievances? Evidence from 15 Case Studies." *Perspectives on Politics*.
- Lepore, Jill. 2018. *These truths: A history of the United States*. WW Norton & Company.
- Leuschner, Elena and Sebastian Hellmeier. 2024. "State concessions and protest mobilization in authoritarian regimes." *Comparative Political Studies* 57(1):3–31.
- Lewin, Linda. 1987. *Politics and Parentela in Paraíba: a case study of family-based oligarchy in Brazil*. Princeton University Press.
- Liu, Hanzhang. 2019. "The logic of authoritarian political selection: evidence from a conjoint experiment in China." *Political Science Research and Methods* 7(4):853–870.
- Liu, Shelley X. 2022. "Control, coercion, and cooptation: How rebels govern after winning civil war." *World Politics* 74(1):37–76.
- Lukowski, Jerzy. 1991. *Liberty's Folly: The Polish-Lithuanian Commonwealth in the eighteenth century, 1697–1795*. Routledge.
- Lupu, Noam and Leonid Peisakhin. 2017. "The legacy of political violence across generations." *American Journal of Political Science* 61(4):836–851.
- Mahoney, James. 2010. *Colonialism and Postcolonial Development*. Cambridge University Press.
- Mangonnet, Jorge. 2023. "Abolition Drives Property Rights: Theory and Evidence from Imperial Brazil." *Working Paper*.
- Mannheim, Karl. 1952. The sociological problem of generations. In *Essays on the Sociology of Knowledge*, ed. Paul Kecskemeti. Routledge & Kegan Paul pp. 163–195.
- Markowski, Radoslaw. 2019. "Creating authoritarian clientelism: Poland after 2015." *Hague Journal on the Rule of Law* 11(1):111–132.
- Marrichi, Jussara Marques Oliveira. 2017. O Termalismo no Brasil: história, ciência e memória entre 1839 e 1950. In *Termalismo e crenoterapia no Brasil e no mundo*, ed. Fernando Hellmann and Daniel Maurício de Oliveira Rodrigues. Editora Unisul pp. 76–100.
- Martinez-Bravo, Monica. 2014. "The role of local officials in new democracies: Evidence from Indonesia." *American Economic Review* 104(4):1244–1287.
- Martinez-Bravo, Monica, Priya Mukherjee and Andreas Stegmann. 2017. "The non-democratic roots of elite capture: Evidence from Soeharto mayors in Indonesia." *Econometrica* 85(6):1991–2010.

- Maseland, Robbert. 2025. Analyzing the Deep Determinants of Institutions: Methodological Choices and Challenges. In *Handbook of New Institutional Economics (2nd Edition)*, ed. Claude Ménard and Mary M. Shirley. Springer pp. 1089–1115.
- Mattingly, Daniel C. 2017. “Colonial legacies and state institutions in China: Evidence from a natural experiment.” *Comparative Political Studies* 50(4):434–463.
- Merkley, Eric. 2020. “Anti-intellectualism, populism, and motivated resistance to expert consensus.” *Public Opinion Quarterly* 84(1):24–48.
- Michalopoulos, Stelios and Elias Papaioannou. 2016. “The long-run effects of the scramble for Africa.” *American Economic Review* 106(7):1802–1848.
- Miller, Michael K. 2015. “Democratic pieces: Autocratic elections and democratic development since 1815.” *British Journal of Political Science* 45(3):501–530.
- Mitchell, Austin M. 2023. “Fiscal conditions for multiparty elections in dictatorships.” *Journal of Peace Research* 62(2):230–245.
- Morgan, Stephen L. and Christopher Winship. 2015. *Counterfactuals and causal inference*. Cambridge University Press.
- Moss, Dana M. 2016. “Transnational repression, diaspora mobilization, and the case of the Arab Spring.” *Social Problems* 63(4):480–498.
- Müller-Crepon, Carl. 2020. “Continuity or change?(In) direct rule in British and French colonial Africa.” *International Organization* 74(4):707–741.
- Nalewajko, Kasia. 2025. “Allies of the Weak: La Résistance and Jews in the Holocaust.” *American Political Science Review* .
- Nathan, Noah L. 2019. “Electoral consequences of colonial invention: Brokers, chiefs, and distribution in northern Ghana.” *World Politics* 71(3):417–456.
- Neundorff, Anja. 2010. “Democracy in transition: A micro perspective on system change in post-socialist societies.” *Journal of Politics* 72(4):1096–1108.
- Neundorff, Anja, Johannes Gerschewski and Roman-Gabriel Olar. 2020. “How do inclusionary and exclusionary autocracies affect ordinary people?” *Comparative Political Studies* 53(12):1890–1925.
- Neundorff, Anja and Kaat Smets. 2017. Political socialization and the making of citizens. In *Oxford Handbook Topics in Politics*. Oxford University Press.
- Olsson, Ola. 2009. “On the democratic legacy of colonialism.” *Journal of Comparative Economics* 37(4):534–551.
- Osborne, Danny, David O. Sears and Nicholas A. Valentino. 2011. “The end of the solidly democratic South: The impressionable-years hypothesis.” *Political Psychology* 32(1):81–108.
- Pardelli, Giuliana and Alexander Kustov. 2022. “When coethnicity fails.” *World Politics* 74(2):249–284.
- Peisakhin, Leonid and Didac Queralt. 2025. “The Political Legacy of Elite Repression.” *Comparative Political Studies* .
- Pérez-Liñán, Aníbal and Scott Mainwaring. 2013. “Regime legacies and levels of democracy: evidence from Latin America.” *Comparative Politics* 45(4):379–397.
- Persson, Torsten and Guido Tabellini. 2021. Culture, institutions, and policy. In *The Handbook of Historical Economics*, ed. Alberto Bisin and Giovanni Federico. Elsevier pp. 463–489.
- Pierskalla, Jan, Alexander De Juan and Max Montgomery. 2019. “The territorial expansion

- of the colonial state: Evidence from German East Africa 1890–1909.” *British Journal of Political Science* 49(2):711–737.
- Pilcher, Jane. 1994. “Mannheim’s sociology of generations: an undervalued legacy.” *British Journal of Sociology* 45(3):481–495.
- Pop-Eleches, Grigore. 2007. “Historical legacies and post-communist regime change.” *The Journal of Politics* 69(4):908–926.
- Pop-Eleches, Grigore and Joshua A. Tucker. 2014. “Communist socialization and post-communist economic and political attitudes.” *Electoral Studies* 33:77–89.
- Pop-Eleches, Grigore and Joshua A. Tucker. 2017. *Communism’s Shadow: Historical Legacies and Contemporary Political Attitudes*. Princeton University Press.
- Pop-Eleches, Grigore and Joshua A. Tucker. 2020. “Communist legacies and left-authoritarianism.” *Comparative Political Studies* 53(12):1861–1889.
- Popescu, Bogdan G. 2023a. “The Developmental Legacies of Border Buffer Zones: The Case of Military Colonialism.” *Journal of Historical Political Economy* 3(1):31–63.
- Popescu, Bogdan G. 2023b. *Imperial Borderlands: Institutions and Legacies of the Habsburg Military Frontier*. Cambridge University Press.
- Power, Timothy J. 2000. *Political right in postauthoritarian Brazil: elites, institutions, and democratization*. Penn State University Press.
- Power, Timothy J. 2018. The Contrasting Trajectories of Brazil’s Two Authoritarian Successor Parties. In *Life after dictatorship: authoritarian successor parties worldwide*, ed. James Loxton and Scott Mainwaring. Cambridge University Press pp. 229–253.
- Puaca, Brian. 2009. *Learning Democracy: Education Reform in West Germany, 1945–1965*. Berghahn Books.
- Putnam, Robert D., Robert Leonardi and Raffaella Nanetti. 1993. *Making democracy work: Civic traditions in modern Italy*. Princeton University Press.
- Queiroz, Emanuel Teixeira de. 2004. *Águas Minerais do Brasil: Distribuição, Classificação e Importância Econômica*. Departamento Nacional de Produção Mineral.
- Read, Barbara. 2018. “Truth, masculinity and the anti-elitist backlash against the university in the age of Trump.” *Teaching in Higher Education* 23(5):593–605.
- Ricart-Huguet, Joan. 2022. “The origins of colonial investments in former British and French Africa.” *British Journal of Political Science* 52(2):736–757.
- Rozenas, Arturas, Sebastian Schutte and Yuri M. Zhukov. 2017. “The political legacy of violence: The long-term impact of Stalin’s repression in Ukraine.” *Journal of Politics* 79(4):1147–1161.
- Sadurski, Wojciech. 2018. “How democracy dies (in Poland): a case study of anti-constitutional populist backsliding.” *Revista Forumul Judecatorilor* (1):104–178.
- Samad, Yunas. 2025. “The Balochistan Quagmire: A Cycle of Violence, Political Mobilization, and Repression.” *Georgetown Journal of International Affairs*, <https://gjia.georgetown.edu/2025/03/14/the-balochistan-quagmire-a-cycle-of-violence-political-mobilization-and-repression/>.
- Samuels, David. 2003. *Ambition, Federalism, and Legislative Politics in Brazil*. Cambridge University Press.
- Sartori, Giovanni. 1970. “Concept Misformation in Comparative Politics.” *American Political Science Review* 64(4):1033–1053.
- Saylor, Ryan and Nicholas C. Wheeler. 2017. “Paying for war and building states: The

- coalitional politics of debt servicing and tax institutions.” *World Politics* 69(2):366–408.
- Schunk, Dale H. 1989. “Self-efficacy and achievement behaviors.” *Educational Psychology Review* 1:173–208.
- Sellers, Jefferey M. 2019. “From Within to Between Nations: Subnational Comparison across Borders.” *Perspectives on Politics* 17(1):85–105.
- Shih, Victor, Christopher Adolph and Mingxing Liu. 2012. “Getting ahead in the communist party: explaining the advancement of central committee members in China.” *American Political Science Review* 106(1):166–187.
- Sidel, John T. 2008. “Social origins of dictatorship and democracy revisited: colonial state and Chinese immigrant in the making of modern Southeast Asia.” *Comparative Politics* 40(2):127–147.
- Simpser, Alberto, Dan Slater and Jason Wittenberg. 2018. “Dead but not gone: Contemporary legacies of communism, imperialism, and authoritarianism.” *Annual Review of Political Science* 21:419–439.
- Sinding Bentzen, Jeanet, Jacob Gerner Hariri and James A. Robinson. 2019. “Power and persistence: The indigenous roots of representative democracy.” *The Economic Journal* 129(618):678–714.
- Skidmore, Thomas E. 1988. *The politics of military rule in Brazil, 1964–1985*. Oxford University Press.
- Soifer, Hillel. 2012. “The causal logic of critical junctures.” *Comparative Political Studies* 45(12):1572–1597.
- Sokołowski, Jacek K. 2013. “The parties on the political scene of Poland of the interwar period, 1918–1939.” *Krakowskie Studia z Historii Państwa i Prawa* 6(1).
- Statistics Poland. 2021. “Local Data Bank.” <https://stat.gov.pl/en/>.
- Stoker, Laura and M. Kent Jennings. 2008. “Of time and the development of partisan polarization.” *American Journal of Political Science* 52(3):619–635.
- Stone, Daniel. 2001. *The Polish-Lithuanian State, 1386–1795*. University of Washington Press.
- Surdea-Hernea, Vlad and Aurelian-Petrus Plopeanu. 2025a. “The Past is Never Dead: the Legacy of Historical Violence on Corruption. Evidence from Post-Communist Countries.” Available at SSRN 5816682, <https://dx.doi.org/10.2139/ssrn.5816682>.
- Surdea-Hernea, Vlad and Aurelian-Petrus Plopeanu. 2025b. “The Repression-Dissent Nexus in High-Threat Environments: Evidence From the Romanian Gulag.” *Comparative Political Studies*.
- Tarnowska, Anna. 2012. *Z dziejów unifikacji administracji II Rzeczypospolitej*. Wydawnictwo Naukowe Uniwersytetu Mikołaja Kopernika.
- Tarnowska, Anna. 2013. “Podstawy prawne skarbowości samorządowej w II RP.” *Prawo Budżetowe Państwa i Samorządu* 1(2):41–59.
- Ticchi, Davide, Thierry Verdier and Andrea Vindigni. 2013. “Democracy, dictatorship and the cultural transmission of political values.” IZA discussion paper.
- Tribunal Superior Eleitoral (TSE). 1979a. *Boletim Eleitoral No. 339 (Outubro)*. TSE.
- Tribunal Superior Eleitoral (TSE). 1979b. *Boletim Eleitoral No. 340 (Novembro)*. TSE.
- Uricoechea, Fernando. 1980. *The Patrimonial Foundations of the Brazilian Bureaucratic State*. University of California Press.

- Vilaga, Marcos Vinicius and Roberto Cavalcanti de Albuquerque. 2003. *Coronel, Coronéis*. Bertrand Brasil.
- Vogler, Jan P. 2019. “Imperial Rule, the Imposition of Bureaucratic Institutions, and their Long-Term Legacies.” *World Politics* 71(4):806–863.
- Vogler, Jan P. 2023. “The Complex Imprint of Foreign Rule: Tracking Differential Legacies along the Administrative Hierarchy.” *Studies in Comparative International Development* 58(2):129–194.
- Voth, Hans-Joachim. 2021. Persistence—myth and mystery. In *The Handbook of Historical Economics*, ed. Alberto Bisin and Giovanni Federico. Elsevier pp. 243–267.
- Vushko, Iryna. 2015. *The Politics of Cultural Retreat: Imperial Bureaucracy in Austrian Galicia, 1772-1867*. Yale University Press.
- Waldner, David. 2009. *The Limits of Institutional Engineering: Lessons from Iraq*. United States Institute of Peace.
- Waldner, David. 2022. Qualitative Causal Inference and Critical Junctures: The Problem of Backdoor Paths. In *Critical junctures and historical legacies: insights and methods for comparative social science*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 159–182.
- Wang, Yuhua. 2021. “The political legacy of violence during China’s Cultural Revolution.” *British Journal of Political Science* 51(2):463–487.
- Wilfahrt, Martha. 2022. *Precolonial legacies in postcolonial politics: Representation and Redistribution in decentralized West Africa*. Cambridge University Press.
- Wittenberg, Jason. 2015. “Conceptualizing historical legacies.” *East European Politics and Societies* 29(2):366–378.
- Wucherpennig, Julian, Philipp Hunziker and Lars-Erik Cederman. 2016. “Who inherits the state? Colonial rule and postcolonial conflict.” *American Journal of Political Science* 60(4):882–898.
- Xue, Melanie Meng. 2021. “Autocratic Rule and Social Capital: Evidence from Imperial China.” Available at SSRN 2856803, <https://dx.doi.org/10.2139/ssrn.2856803>
- Zachary, Paul, Kathleen Deloughery and Alexander B. Downes. 2017. “No business like FIRC business: Foreign-imposed regime change and bilateral trade.” *British Journal of Political Science* 47(4):749–782.
- Zamoyski, Adam. 1987. *The Polish Way: A Thousand-Year History of the Poles and their Culture*. John Murray.
- Zaw, Htet Thiha. 2024. “The Pre-Colonial Roots of Colonial Coercion: Evidence From British Burma.” *Comparative Political Studies* 57(12):1939–1977.
- Zhukov, Yuri M. 2014. *A Theory of Indiscriminate Violence*. Ph.D. Dissertation, Harvard University.
- Zhukov, Yuri M. 2017. “External resources and indiscriminate violence: Evidence from German-occupied Belarus.” *World Politics* 69(1):54–97.

When are Junctures Critical?
The Legacies and Non-Legacies of the
Suppression of Local Self-Government

Appendix

December 17, 2025

A Appendix

A.1 Theory Supplement: Extended Discussion of the Mechanisms

In the main body of our study, we elaborate on the conditions under which the suppression of self-government can be expected to result in long-term legacies. Therein, we also distinguish between the extent of repression and the duration of intervention as key factors that determine whether or not a political legacy will materialize. In this supplementary section, we provide additional detail on the specific mechanisms that we expect to lead to changes in political behavior as a consequence of the removal of local self-government. Importantly, as we describe below and in line with our main theory, all these mechanisms are most likely to apply to a case of *enduring* interventions with *encompassing repression* and least likely to apply in the case of *transient* interventions with *limited repression* (underscoring the interactive character of our framework).

In general, we consider two main types of external interventions as suppression of local self-government: imperial conquest and rule (both of a direct and indirect character) and local political control through a centralized authoritarian state. The commonality between these two kinds of interventions is the denial of full local political autonomy by a non-local actor. As we discuss below, when political autonomy is denied, the negative effects on affected communities are multifaceted.

A.1.1 An Overview of Relevant Mechanisms

What are the consequences of the suppression of local self-government? Especially if sustained over time, the inability of individuals to participate in and take responsibility for political leadership in their community may shape the manner in which they view their fellow citizens and even come to view themselves. The classic treatise by Putnam, Leonardi and Nanetti (1993) was among the earliest and most influential works to make this point, tying regional variation in civic community in the present day to historical experiences of political autonomy or subjugation. An implication of Putnam, Leonardi and Nanetti's argument is that in the long run self-government breeds interpersonal trust, which facilitates citizens' capacity to coordinate their electoral support around political leaders who provide better public goods and services. Sustained suppression of self-government, by the same token, may undermine interpersonal trust and weaken electoral accountability.

Extant evidence from laboratory settings is consistent with the postulated link between experience with self-government and a cooperative disposition among citizens. Dal Bó, Foster and Putterman (2010) show that individuals are significantly more likely to engage in cooperative behavior if they are able to choose policies themselves (through voting) than if policies are imposed upon them. Grossman and Baldassarri (2012) demonstrate that cooperation depends on how leaders are chosen: Individuals able to choose their leaders themselves contribute more to public goods than individuals whose leaders are chosen by lottery. Similarly, Markussen, Putterman and Tyran (2014) report that formal and informal mechanisms of curbing free-riding are more effective when they have been democratically selected by subjects. Furthermore, Kamei (2016) finds evidence of legacy effects: Individuals who participate in a democratic policymaking process continue to exhibit high levels of cooperative behavior even when subsequently placed in undemocratic contexts. Sausgruber, Sonntag and Tyran (2021) show that individuals react more pro-socially to policies that are selected democratically than to those for which they have no input. Most recently, Haas, Hassan and Morton (2020) provide evidence that interpersonal trust among subjects from established democracies is more resilient to negative shocks than is the case for subjects from new democracies. As indicated in our main theory section, if these effects can persist over long time periods, they are most likely to lead to sustained changes in political behavior—even after an external intervention has come to an end. Similarly, the many different highlighted pathways through which removal of local autonomy negatively affects political behavior indicate that forms of intervention that affect more dimensions (i.e., that are more extensive in their repressiveness) will have the most comprehensive consequences.

Moreover, all of these findings imply that interpersonal trust and cooperative attitudes spring from sustained experiences with self-government, and that they are likely to wither as a consequence of the suppression of self-government, especially if such suppression is sustained over long periods of time. Yet a

disposition towards cooperation is not the only aspect of citizens' worldviews that may be affected by the suppression of self-government. Guiso, Sapienza and Zingales (2016) suggest an additional channel of influence: individuals' beliefs about self-efficacy. Revisiting Putnam, Leonardi and Nanetti's arguments about the legacies of communal government in Italy, the aforementioned study demonstrates that schoolchildren from the former communal republics in the North hold fundamentally different beliefs about the role of effort versus luck in shaping life outcomes than do schoolchildren in other areas. For the first group of children, effort trumps luck, whereas the opposite is the case for the second group. Thus, the suppression of self-government—by weakening a community's opportunity to fully develop a sense of self-efficacy and responsibility for governance—may mitigate the intergenerational transmission of the belief that one's actions can meaningfully shape life prospects. A key insight from these findings about potential long-term changes to culture and the intergenerational transmission of values is that the norms undergirding citizen participation and democratic governance can be influenced in ways such that the effects are visible long after self-government has resumed.

The implications of a sustained and repressive suppression of self-government for downstream political behavior follow largely from the norms and belief systems outlined above. We postulate that, depending on context, episodes of the suppression of self-government contribute to: (1) support for populist or anti-system politicians and parties; or (2) support for authoritarian successor parties (ASPs).

A.1.2 Specific Mechanisms Regarding Populist (Anti-System) Parties

Consider first support for populist or anti-system politicians and parties. A growing body of evidence links the electoral prospects of anti-system actors to low levels of social capital and trust. This is particularly well documented for European party systems, with the extant studies on Latin America similarly indicating the existence of such a relationship.¹

Keefer, Scartascini and Vlaicu (2019) provide a theoretical framework that explains why this should be so. In politics characterized by low levels of interpersonal trust, voters cannot count on one another to coordinate around responsible candidates with the skill and willpower to provide high-quality public goods and services. Rather, in the spirit of “each voter for themselves,” a pattern of electoral free riding emerges where voters gravitate towards politicians offering immediate, simple, and often personalized solutions to complex policy problems (the hallmark of populist parties).

In such a context, political platforms that promise to dispossess (so-called) elites, that scapegoat immigrants or other out-groups, and/or that advocate the transfer of resources to “virtuous” members of the polity are likely to draw in large segments of the electorate. Of course, such appeals are the “bread and butter” of populist, anti-system politicians. Thus, by undermining interpersonal trust, the suppression of self-government may in the long-run prime the electorate in favor of anti-system actors.

Long-run changes in self-efficacy beliefs may likewise play a role in generating support for anti-system actors. Social psychological research has shown that reducing subjects' personal control in an experimental setting strengthens beliefs about the existence of powerful political and personal enemies (Sullivan, Landau and Rothschild, 2010) and leads to the perception of conspiracies (Whitson and Galinsky, 2008).² Anti-system politicians are in this way inherently advantaged by a polity characterized by a low sense of self-efficacy, since the use of conspiratorial language about elites operating as “enemies of the people” (to borrow a phrase frequently used by Joseph Stalin, and more recently by Donald Trump) is a nearly universal feature of their political rhetoric (cf. Hawkins, 2009; Mudde, 2007; Myers and Hawkins, 2011). Accordingly, the fact that the suppression of self-government may undermine collectively held beliefs about self-efficacy provides another reason to expect that they will favor anti-system political actors. As with the previous mechanism,

¹For analyses tying low social capital and trust to support for anti-system parties in Europe, see Coffé, Heyndels and Vermeir (2007), Hooghe, Marien and Pauwels (2011), Berning and Ziller (2017), and Hooghe and Dassonneville (2018). For studies on Latin America tying low trust to voting for anti-system candidates and support for the policies advocated by such candidates, see Doyle (2011) and Keefer, Scartascini and Vlaicu (2019).

²Conspiracy beliefs, in turn, have been empirically linked with populist attitudes that drive support for anti-system politicians (Castanho Silva, Vegetti and Littvay, 2017).

this mechanism is more likely to apply when instances of the suppression of self-government are sustained and associated with encompassing repression.

A.1.3 Specific Mechanisms Regarding Authoritarian Successor Parties

Now consider support for ASPs. Scholarship points to two factors that may play a role in shaping the relationship between a sustained episode of suppression of self-government and support for these organizations: (1) ideology and (2) political organization. ASPs will be most successful when citizens internalize rather than reject the regime ideology (Neundorf and Pop-Eleches, 2020; Pop-Eleches and Tucker, 2017). Internalization clearly occurs in certain contexts, especially in societies where the authoritarian regime is able to heavily invest in indoctrination efforts over a long time period (Alesina and Fuchs-Schündeln, 2007; Cantoni, Chen, Yang, Yuchtman and Zhang, 2017; Pop-Eleches and Tucker, 2020). Where episodes of suppression are most prolonged and/or comprehensive, the internalization of regime ideology will likely be greatest. Because a sustained absence of self-government undermines communal cohesiveness as well as citizens' collective sense of self-efficacy, authoritarian subjects in such contexts may not have the independence of mind nor social support necessary to resist the regime's indoctrination efforts. Consequently, if sustained for an extensive period, indoctrination takes root, creating an ideological bias in favor of authoritarian successor parties after self-government has resumed.

Support for authoritarian successor parties also rests on organizational factors (Grzymala-Busse, 2002; Loxton, 2018; Miller, 2021; Serra, 2013). Possibly one of the most important among these is the capacity to mobilize voters. Generally speaking, authoritarian successor parties that have extensive clientelist networks and enjoy privileged access to state resources will be most successful on election day. Where self-government is suppressed over long periods of time, one would expect authoritarian successor parties to have these attributes. Indeed, examining a sample of political parties around the world, Kitschelt and Singer (2018) find evidence for precisely this link: Authoritarian successor parties emergent from episodes of the suppression of self-government lasting ten years or more have more extensive clientelist networks and expend more effort on clientelism than other parties. This is compatible with our expectations: Sustained and comprehensive episodes of the suppression of self-government may permit authoritarian elites to co-opt and/or subsume local notables and family dynasties within the official party, thereby giving the authoritarian successor party a significant advantage in its capacity to exploit practices such as vote brokerage for electoral gain.

A.1.4 Summary

In short, in this section we have proposed a variety of mechanisms that connect sustained and repressive episodes of the suppression of self-government to long-term changes in political behavior. For this reason, the applicability of each of these mechanisms is clearly moderated by the two factors discussed in our main framework. In general, there is ample evidence from a broad range of studies that any sustained denial of local political autonomy has the potential to negatively affect participatory behavior and bolster the efforts of parties that are explicitly anti-system (such as many populist parties and authoritarian successor parties).

A.2 Theory Supplement: Clarifications Regarding Different Aspects of the Theory

In this section, we provide further details on different aspects of our theoretical framework. We begin by clarifying in more detail that we view the two main dimensions of our framework as working in an *interactive* fashion to generate legacies. Then we elaborate on why and how our theoretical mechanisms apply to a broad variety of initial contexts. Finally, we provide further thoughts on the issues of resistance and resentment that expand upon the initial discussion in the main body of the manuscript.

A.2.1 On the Interactive Character of the Primary Dimensions of Intervention

With respect to the two dimensions of our framework, namely (1) the level of repression and (2) the length of the intervention, it is important to clarify that we consider these two dimensions to be *interactive* in shaping the potential for long-term legacies. Specifically, we expect a substantially higher likelihood of a legacy if an intervention is *both* enduring *and* repressive. The key reason behind this proposition is that these kinds of interventions maximize the incentives for behavioral and attitudinal change. Specifically, under a lengthy and highly repressive episode of suppression, citizens will simultaneously perceive that (1) there is no plausible scenario of a quick return to the status quo ante and (2) sustained resistance to behavioral change will result in (significant) state-based violence. Thus, while the costs for behavioral persistence on the part of citizens are extremely high (because it likely subjects them to violent forms of repression), there no longer is a trade-off between the initial costs of change and long-term payoffs (as the situation is likely to endure). Accordingly, the incentive structure is heavily biased toward promoting quick and lasting adaptation of political behavior.

In comparison, in cases of enduring episodes with *limited* repression, citizens typically merely have moderate incentives to change their behavior as some channels for participation may remain open and citizens are not subject to the violent suppression of anti-regime ideas and actions. Under these circumstances, the incentives for behavioral shifts will only affect some parts of the population and manifest in a slower fashion.

Similarly, even in episodes of suppression that feature high repression, if such an intervention only lasts for a very short time, the underlying processes suggested by us (especially in terms of parental socialization and value transmission) may not apply to sufficiently large parts of the population. Only a superficial change in displayed behavior and not a sustained change in underlying value systems is likely for most, which in turn reduces the likelihood that an enduring legacy will manifest.

From a technical-empirical perspective, we may think of these propositions as potentially reflected in an interaction term: When an instance of suppression is both encompassing (=1) and enduring (=1), it generates an even higher incentive to change behavior than if only one of these variables takes on the value of 1. This perspective on the matter is also confirmed by our meta-analysis, in which we find that in the full model—that includes both the base and the interaction terms of repression and intervention length—the *interaction term* is the one that is both (1) of high magnitude and (2) closest to conventional levels of statistical significance, which is impressive given the small number ($N = 36$) of observations.

In short, we theoretically argue and find empirical evidence for an interactive effect of the length and repressiveness of an intervention. Our discussion here complements our initial elaboration on the issue in the main body of the text.

A.2.2 On the Framework's Applicability to a Broad Variety of Initial Conditions

In the theory section, we have elaborated on some specific conditions under which our framework applies. Specifically, we have highlighted two critical scope conditions: (1) that the intervening authority seeks compliance with its rules from the affected population and (2) that the coercive capacity of the intervener is consistently high enough to repel violent uprisings for the period of intervention.³ In addition to these two scope conditions that qualify our framework's suitability in different contexts, in this section we highlight that our theory applies to a broad variety of cases with potentially vastly diverging preexisting belief compositions of the population.

Specifically, an intervention does not need to take place in a perfect or even a flawed democracy to have the hypothesized effect. The key reason for the broader applicability of our framework—including to oligarchic or more authoritarian political contexts—is that, in every society there are some citizens who hold authoritarian beliefs and some who hold more liberal or anti-authoritarian beliefs. This is evident in the fact that, even in fully developed modern democracies, some segments of the population vote for radical

³Of course, a complete breakdown in this ability at the end of the intervention period is not contradictory to our framework as almost all interventions considered by us are temporally limited. Rather this scope condition is about the relatively *consistent* presence of coercive capacity *throughout* the duration of the intervention period. Uprisings against the intervener may occur during the episode of suppression, but they need to be defeated quickly to restore order.

authoritarian parties. In this regard, [Valentim \(2024\)](#) has shown that the recent major electoral successes of authoritarian right-wing parties across a number of democracies can be related to deeply held authoritarian beliefs by parts of the population (that had previously not been openly revealed). Furthermore, as the collapse of the USSR (and other authoritarian Eastern bloc states) has impressively demonstrated, even in decades-old—and seemingly stable—fully authoritarian systems, some segments of the population often secretly hold more liberal values that are revealed at an opportune moment ([Kuran, 1987](#); [Kuran, 1991](#)).

Against this background, we would like to once again highlight our theory's explicitly *probabilistic* character. Specifically, our framework merely suggests that, due to a change to incentive structures, some segments of the population will incrementally shift their behavior and the values they instill in their children. Thus, as long as there is an initial mix of different belief systems in any specific context (which, as we argue above, is extremely likely—even in decades-old democracies or previously stable authoritarian systems), our mechanism applies regardless of the precise preexisting distribution of beliefs within the population.

We should furthermore emphasize that even if the initial context was one of local authoritarianism, there is still a major qualitative difference to an intervention by an imperial or central authority that puts in place an official with no social connection or historical embeddedness in regional socioeconomic and cultural structures. This qualitative difference arises from the circumstance that local authoritarian rulers who have traditionally governed a territory are much more likely to be deeply familiar with the population's conditions and needs. This allows them to effectively compensate for some of the most negative consequence of the absence of formal participation channels.⁴ In addition, cultural dissimilarities between the local population and the representatives of an intervening authority—whether they come from another region or another country—could create higher levels of (mutual) distrust, making it harder for intervening authorities to effectively learn about the true grievances and wishes of the population.⁵ Thus, even if an external authority creates a formal or informal petition system to learn about the population's grievances, it may be markedly less effective than one created by a local authoritarian ruler. Accordingly, even in contexts with preexisting local (semi-) authoritarian structures, an external intervention can still change the mode of governing and the incentive system for the local population to engage in (any form of) political participation.

In short, our discussion has highlighted that in almost all political settings we find a mixed composition of initial population beliefs, which points to the applicability of our probabilistic framework to many different contexts. Additionally, there is a qualitative difference between (possibly preexisting) local authoritarianism and an external intervention, underscoring that even in contexts in which there was no democracy initially, an external intervention can change the institutions of (and incentive structures associated with) governance in fundamentally detrimental ways.

A.2.3 Further Discussion of Resistance and Resentment

In the main body of the paper, we briefly touched upon the issues of resistance and resentment against the intervening authority. In this Appendix section, we provide further thoughts on both topics.

With respect to resistance, it is relevant to note that some forms of active or passive resistance against an intervention are likely to emerge in many different contexts, simply because—in line with the discussion above—some parts of the population can be expected to hold deeply anti-authoritarian beliefs. These population members are not of direct relevance to our predictions, however, as our theory is explicitly probabilistic and acknowledges the low likelihood of changing the behavioral patterns of those with the strongest initial sets of beliefs. Thus, the occurrence of resistance does not per se contradict any aspect of our theory; rather it is fully compatible with the probabilistic view regarding the preexisting distribution of attitudes and subsequent (incremental) attitudinal change that we embrace.

The topic of resentment against the intervener is slightly more complex. Specifically, resentment on the part of the population is most likely to emerge when there is no viable path for the affected populations to avoid forms of violent repression through behavioral change. Our theoretical framework rests on the

⁴We have discussed related dynamics for the case of Poland in the case selection part of the main paper.

⁵Cultural dissimilarities between an external intervener and the local population can contribute to such a perception of distance. Note that cultural dissimilarities here may not be limited to differences between nations and ethnicities but could also exist between socioeconomic or population groups from the *same* ethnicity or nation (who may be separated by socialization, socioeconomic status, or geographic origin).

associated incentive structures that suggest that populations may gain advantages or avoid repercussions by changing their behavior. As long as there is such a viable path, the development of resentment is systematically reduced. For these specific reasons, we introduced the second important scope condition of a viable path of avoidance of repression through behavioral change. Once this scope condition applies, it is less likely that citizens will begin to join the violent resistance. (Yet they might choose to resort to violent resistance if behavioral change does not sufficiently reduce the chances of them being subject to violence.)

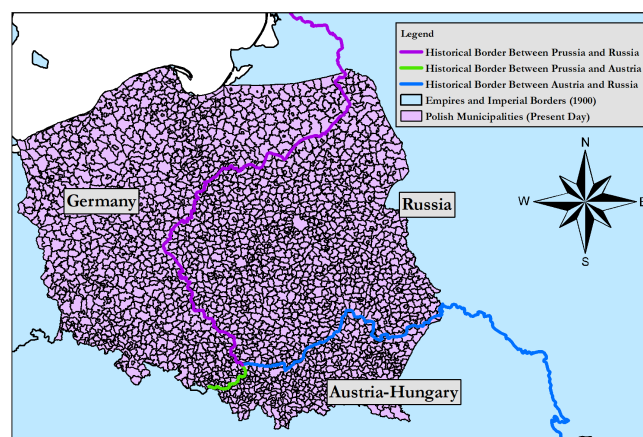
In short, both resistance and resentment are topics that are important to acknowledge. As we discuss, some forms of resistance against an intervention can be expected in almost all contexts. However, resistance itself does in no way contradict our theoretical predictions. Rather, it is compatible with our theory's probabilistic character. Additionally, resentment may emerge if there is no viable path for the population to avoid repression. In this respect, the scope conditions introduced by us are extremely important to highlight as they significantly reduce the likelihood of this phenomenon.

A.3 First Case Supplement: Extended Discussion of the Historical Background

A.3.1 The Historical Borders

Figure A1 shows both the historical borders of the imperial partitions of Poland in combination with the administrative boundaries of contemporary municipalities.

Figure A1: Map of the Historical Boundaries



Note: This map shows the borders of contemporary municipalities in Poland as well as the borders between the three major empires that historically ruled Poland (1815–1914). Prussia was a part of the German Empire as of 1871, which is why this part is labeled “Germany” here.

A.3.2 The Suppression of Self-Government by Prussia

The first one among the imperial powers that ruled parts of the Polish lands was Prussia. Prussia began introducing its own legal-administrative system in the occupied Polish territories in the late eighteenth century (Hoensch, 1990, 181; Lukowski and Zawadzki, 2006, 137; Prazmowska, 2011, 131; Wandycz, 1975, 14–15; Vogler, 2019, 812–813).

After 1815, Prussia controlled large parts of Western Poland, including many territories with significant Polish population majorities. Even in those territories, the Polish people had to follow Prussian laws and accept the Prussian system of government. Accordingly, they were denied the right to fully self-govern.

While the Prussian state denied full self-government to the Poles, its institutions were highly reliable and effective, especially when compared to Russian rule in the eastern parts of Poland. Prussian state representatives were also subject to an advanced legal system that punished corruption and unjustified violent behavior, implying a low level of state-directed violence against the population. Additionally, as of 1849, the Polish minority enjoyed limited political representation in the Prussian *Landtag* (an important representative assembly of the Prussian state), which provided some initial avenues for political participation. Particularly in comparison with early Austrian rule and the extremely coercive and militarized Russian rule throughout the entire period 1815–1914, Prussia’s system was seen as relatively benign (Davies, 2005, 85; Vogler, 2019, 812–813).

Most importantly, in the period of Imperial Germany (1871–1914), the Poles were given full voting rights in federal parliamentary elections and were permitted to establish political parties that represented their minority interests. While self-government was inhibited when it came to the design of administrative and legal institutions, the fact that the Polish minority had the right to organize politically over several decades gave the Poles important and sustained experience with democratic processes and political participation.

A.3.3 The Suppression of Self-Government by Russia

The second imperial power that ruled large parts of the Polish lands throughout the nineteenth century was Russia. Compared to Prussia, Russia’s foreign rule was significantly more repressive, antagonistic, and militarized.

The Russian state was primarily seen as an oppressive force against the Polish people that used coercion and military force to maintain its rule. It governed the Polish territories with a high level of coercion and arbitrariness (Davies, 2005, ch. 2; Raphael, 2000, 67–71, 74–75; Vogler, 2019, 814–815). This state of affairs, taken in conjunction with the absence of any democratic forms of self-government and poor living standards, provoked several armed uprisings against the Russian state and military throughout the nineteenth century. Unsurprisingly, these uprisings were smashed by Russia through the relentless use of military power (Davies, 2005; Lukowski and Zawadzki, 2006; Prazmowska, 2011; Wandycz, 1975).

Accordingly, while the Poles in the western territories were forced to accept Prussian institutions but had the right to have their own political parties in the German parliament, in the east they were not only completely denied the right to self-govern, but also experienced a militarized suppression that lasted for decades. Any attempts at collective action against the repressive Russian state were brutally put down and squashed hopes for an independent state with forms of democratic/inclusive self-government.

A.3.4 The Suppression of Self-Government by Austria

The third imperial power that ruled some of the Polish territories was Austria. The lands of the Austrian partition are historically known as Galicia. With respect to Polish self-government, Austria had a more mixed history than Prussia and Russia. Even though it also acted as a highly oppressive state from 1815 to 1867, after 1867 it gradually began to give more rights to the Poles, including the hiring of Polish personnel in the regional/local bureaucracy and the use of the Polish language in administrative affairs (Davies, 2005; Lukowski and Zawadzki, 2006; Prazmowska, 2011; Vogler, 2019; Vushko, 2015).

Austrian rule also began with a very strict censoring of the Polish press and the oppression of all Polish attempts at self-government. Especially in the period 1849–1859, the so-called period of *neoabsolutism*—as a response to the failed 1848 revolution—the Austrian state intensified its attempts to control its entire territory, including through political repression in Galicia (Deak, 2015; Judson, 2016). Yet, after 1867, the Austrian state began to change its strategy of rule. Beginning in this year, Austria granted greater levels of administrative participation to the Poles. More Polish personnel were hired to work in the public administration and Poles were permitted to send representatives to Vienna.

At the same time, several social and political hierarchies persisted. First and most importantly, while administrative autonomy had been given to the province of Galicia, it was still subject to the general laws of the Austrian state, meaning that foreign rule persisted (albeit in less severe form). Moreover, unlike the German Empire in 1871, the Austrian state did not introduce full and equal voting rights to the Poles of Galicia. Instead, there was a class-based voting system that prevented many people from having any

influence on political processes. Only for two elections (in 1907 and 1911) were full voting rights given to the Poles (see also [Howe, Szöcsik and Zuber, 2022](#)), meaning that the majority of inhabitants of the Austrian partition (like their counterparts in the Russian partition) gained little experience with democratic processes. Accordingly, the greater level of Polish participation in the bureaucracy of Galicia did not change the fact that the Austrian state did not allow for full democratic participation until 1907, a few years before the end of Austrian rule in the territory.

In short, when it comes to the suppression of self-government, the Austrian case is more ambiguous than the other two. At first, the Austrian state's rule in Poland was highly repressive. While it allowed for administrative decentralization after 1867, the introduction of fully democratic institutions happened so late in the Austrian Empire (1907) that it might not have had a sufficiently profound impact to shape long-term prospects for effective and sustained experience with self-government.

A.4 First Case Supplement: Extended Discussion of the Chosen Time Period and Outcome Measure in the Polish Case

In this section, we elaborate in more detail on two of our choices with respect to the empirical test. First, we elaborate on why we chose the 2010s as our main period of observation (rather than the 1990s or 2000s). Second, we elaborate on why the success of the PiS as a party represents the best outcome measure in light of our theoretical framework.

We chose the period of the 2010s—rather than the 1990s or early 2000s—as our main observation time because in the preceding two decades the Polish political system was still in a state of flux. During these earlier decades, frequent changes to electoral rules and major national debates about the orientation of the political-economic system meant that the viability of parties and party-citizen interactions were heavily oscillating. For those reasons, until the late 2000s, the Polish party system was not in a state of equilibrium. Instead, it underwent several dramatic changes, with new parties rising, several parties disappearing, and completely new coalitions forming ([Markowski, 2006](#); [Millard, 1994](#)). In the 2010s, however, initial debates about the country's fundamental political-economic and geopolitical orientation had been settled and electoral rules became more stable. Thus, the Polish party system moved closer to a state of relative stability. These circumstances mean that patterns of electoral success by parties in the 2010s are much less likely to be related to idiosyncratic or short-lived factors associated with shifting electoral rules or variation in clarity about party viability.

Against this background, the Polish “Law and Justice” (PiS) party in the 2010s represents a perfect measure of the strength of populist, anti-system parties in line with our theoretical framework. This is because both ideologically and practically its anti-democratic orientation was clear to observers of Polish politics. For instance, [Charnysh \(2017\)](#) describes how the party did not condemn—and thus often implicitly legitimized—extremist right-wing political positions. Among others, she also points to a growing radicalization of the PiS party elite in the late 2000s and early 2010s.

This radicalization of the PiS was not limited to rhetoric or superficial displays of anti-democratic positions. Instead, once the party had gained political power in the mid-2010s, it very actively sought to dismantle Poland's democratic system of checks and balances. Specifically, it not only disempowered the Constitutional Tribunal—one of Poland's most important judicial institutions at the federal level—to remove a possible (democratically and legitimately instituted) veto player to its legislative goals ([Sadurski, 2019](#), chap. 3), but also shaped the entire rest of the justice system in accordance with its own partisan interests, concentrating an enormous amount of legal (and political) power ([Sadurski, 2019](#), chap. 4). These actions were accompanied by assaults on a whole host of other institutions that are central to functioning democracies, including the Polish media system, a viable political opposition, and a neutral civil service ([Sadurski, 2019](#), chap. 5).

Accordingly, the assessments of scholars have been extremely critical and indicate a severe form of democratic backsliding. [Drinóczy and Bień-Kacała \(2019\)](#) think of the developments in Poland at this time as the emergence of “illiberal constitutionalism”—a process that includes the relativization and undermining of democratic principles. Similarly, [Markowski \(2019\)](#) suggests that the PiS created a form of “authoritarian

clientelism,” which is at odds with the principles of liberal democracy and the rule of law. All of these arguments show that the PiS was not only a perceived, but a real threat to democracy.

Despite the aforementioned developments, [Nalepa \(2021\)](#) presents evidence that suggests that there was some degree of uncertainty on the part of many Polish citizens regarding the authoritarian tendencies of the PiS. Importantly, this argument and evidence do not represent a direct contradiction to our theory. After all, PiS was and is a fundamentally populist party (with the strength of populism being a key consequence of the suppression of self-government). Populist parties typically do not have a strong commitment to democratic and/or constitutional norms, making them at least a *potential* threat to democracy. Consistent with our framework, the inability of citizens to clearly recognize such a potential threat and to allow for its political leaders to rise to power can be seen to at least in part be related to previous (enduring) episodes of the suppression of self-government (via the mechanisms outlined by us above). Indeed, that using more aggressive political rhetoric and borrowing from extreme agendas both represent potential threats to democracy was known long before PiS received the opportunity to dismantle Poland’s constitutional system (see, for instance, [Rupnik, 2007, 24](#)). In line with this anticipation, exclusionary identity politics (rhetoric) became a key component of (justifying) democratic backsliding in Poland ([Sata and Karolewski, 2020](#)).⁶

Based on our framework in the main body of the study and our comprehensive analysis of the historical background of the Polish case (see [subsection A.3](#)), we predict that the Law and Justice (PiS) party should be strongest in areas that experienced the most severe suppression of self-government and had the least experience with democratic participation. In the comparison of Prussia and Russia, the Russian areas clearly were subject to more militarized, repressive foreign rule that also did not have any truly democratic components that could serve as the template for self-governance processes. While Prussian rule also had some (albeit more limited) repressive elements, it was generally much more constrained and offered the Poles significantly more robust channels for political participation, including full voting rights for males above the age of 25 as of the year 1871.⁷

A.5 First Case Supplement: Extended Discussion of Evidence for the Mechanisms of Intertemporal Transmission in Poland

As elaborated in the main body of the paper, there is comprehensive evidence for imperial legacies in cultural values that are substantively related to aspects of democratic organization. The evidence suggests that (1) citizens in the formerly Prussian parts of Poland prioritize self-fulfillment over financial security as a goal of work ([Hryniewicz, 1996](#)), which is a sign of self-efficacy and thus relevant to the belief that one can meaningfully shape one’s own life (which is relevant to the political arena as well); (2) bureaucracies in the formerly Russian partition have lower levels of transparency and professionalism, implying more frequent abuses of public offices for personal gains (which is harmful to democratic stability) ([Vogler, 2019](#)). Furthermore, we demonstrate that there are significantly fewer mayors with a university degree in the formerly Russian partition when compared to the Prussian partition, linking this to different political cultures [subsection A.12](#).

Although the evidence for cultural legacies is very strong, it would be desirable to complement it with an in-depth discussion of the relevant transmission mechanisms. Our framework suggests that intergenerational socialization, especially within the family, is key. In this respect, a study by [Wałęcka-Matyja \(2022\)](#) that is based on psychological surveys of 68 parents (ages 41–67) and 51 adult children (ages 18–30) in Poland is relevant for multiple reasons: First, from a theoretical perspective, she suggests that the permanent presence of parents in their children’s lives plays a critical role in their normative socialization. Children imitate, model, and identify with their parents in a process of social learning. Second, in her empirical analysis, she finds that there is strong intergenerational transmission of values, especially as related to family values and world views. Family values also include politically relevant dimensions, such as individualism. In addition, regarding world views, there is clear evidence of transmission of values related to benevolence of people and the world, control of negative events, and self-controllability, which all have an important sociopolitical dimension as well. Third, she also finds evidence for a generational shift in materialistic values specifically.

⁶On this issue, see also [Charnysh \(2017\)](#).

⁷Furthermore, as we discuss in more detail below, the Austrian case does not allow for a straightforward prediction due to the ambiguous/shifting character of Austrian rule over time.

In this area, there is less intergenerational stability/transmission, indicating that different generational life experiences (especially growing up in a capitalist society that is more focused on consumerism) likely make a difference. Although the latter finding is more closely related to the economic than the political circumstances, it justifies our theory's substantive focus on generations (as previously discussed).

Additional evidence for the intergenerational transmission of values in Poland from the critical years right after authoritarian rule comes from [Kraaykamp and Nieuwbeerta \(2000\)](#). In this study that uses survey data from 1993 with more than 2,500 Polish respondents, the authors find that parental cultural capital was one of the strongest predictors (in addition to individuals' education levels) for high-culture participation. This underscores the relevance of within-family transmission of cultural values, which the authors demonstrate to be substantial. This evidence highlights that the transmission mechanisms we propose were not broken by the transition from communism to democracy in the early 1990s.

These specific findings from Poland are compatible with a broad literature that underscores the relevance of within-family socialization for the transmission of cultural values ([Min, Silverstein and Lendon, 2012](#); [Williams, Bravo, Umaña-Taylor, Updegraff, Jahromi, Martinez-Fuentes and Elias, 2020](#)), including those relevant to political beliefs ([Jennings, Stoker and Bowers, 2009](#); [Jennings and Niemi, 1968](#)).

Could institutional or elite persistence, such as the persistence of bureaucracies, also explain the intergenerational transmission of values in the Polish case? There are three critical reasons that directly speak against this proposition. First, research suggests that societal culture is a key determinant of bureaucratic behavior, with regional variation in norms affecting the decisions of street-level bureaucrats and not the other way around ([Rueß, Schneider and Vogler, 2025](#)). Evidence for comparable dynamics has also been presented for Poland specifically, with [Vogler \(2019\)](#) arguing that administrative culture across Poland is shaped by regional divergence in cultural traits and societal structures.

Second, since we aim to explain persistence since the period of imperial partition in the long nineteenth century, an account that centers on institutional persistence would not be credible as the primary transmission mechanism. Specifically, Poland experienced multiple fundamental institutional transformations that affected all aspects of its formal political, legal, and administrative framework ([Vogler, 2019](#)). Additionally, there were near-total disruptions in government and bureaucratic personnel, including large parts of the elite that were murdered by the Nazis and Soviets, making alternative explanations centered on institutional or elite persistence implausible.

Third and finally, some of the evidence we present is more plausibly linked to societal values than to bureaucratic or institutional persistence. In particular, the increased propensity of citizens to elect politicians with higher education in the western parts of Poland can be more directly connected to aspects of personal belief systems, such as the relationship between self-efficacy and educational attainment (e.g., [Ayllón, Alsina and Colomer, 2019](#); [Grabowski, Call and Mortimer, 2001](#); [Schunk, 1989](#)) as well as the literature that connects populist belief systems to distrust in experts and higher education (e.g., [Merkley, 2020](#); [Read, 2018](#)).⁸

Accordingly, although it cannot be ruled out that aspects of institutional persistence have supported or worked in parallel with societal transmission for limited time periods (e.g., the post-war or post-communist periods only), we believe that the available evidence—including the new evidence introduced here—strongly supports the perspective that within-family transmission is the most important aspect of the value stability that has been documented with respect to the imperial partitions.

In short, the combination of (1) the insights that were already discussed in the main body of the study (including clear evidence that cultural traits of the population still differ substantially between the historical partitions), (2) the case-specific empirical evidence regarding cultural socialization through the family in Poland that we presented here, (3) general theoretical claims and confirming empirical results about the importance of parental political values for children's world views, and (4) the implausibility of institutional or elite persistence mechanisms provides strong support for our assumptions about intergenerational transmission with respect to the Polish case.

A.6 Descriptive Statistics: Poland

[Table A1](#) shows descriptive statistics for the Polish case.

⁸The link between historical self-government and individuals' self-efficacy is also highlighted in [Guiso, Sapienza and Zingales \(2016\)](#).

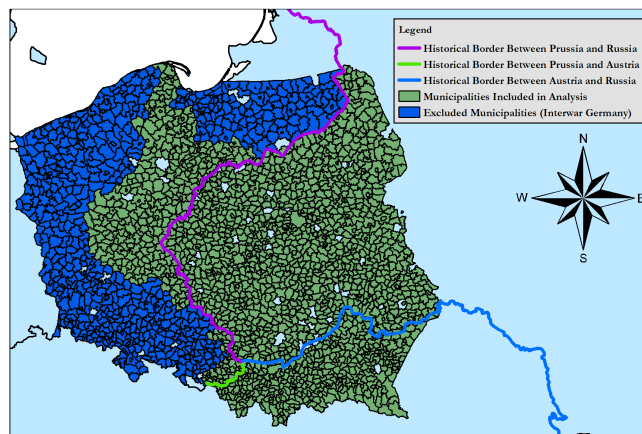
Table A1: Descriptive Statistics: Poland

Variable	n	Min	q ₁	$\bar{x}$	$\tilde{x}$	q ₃	Max	IQR
Mayor PiS (2014–2018)	2445	0.00	0.00	0.07	0.00	0.00	1.00	0.00
Mayor PiS (Broad) (2014–2018)	2445	0.00	0.00	0.07	0.00	0.00	1.00	0.00
Mayor PiS (2010–2014)	2448	0.00	0.00	0.05	0.00	0.00	1.00	0.00
Russia	2448	0.00	0.00	0.44	0.00	1.00	1.00	1.00
Interwar Germany	2448	0.00	0.00	0.26	0.00	1.00	1.00	1.00
Austria	2448	0.00	0.00	0.15	0.00	0.00	1.00	0.00
Elevation	2448	-2.49	105.49	186.25	155.57	228.27	1207.07	122.78
Pop. Density	2448	4.37	41.46	221.12	64.08	129.04	3991.21	87.58
Urban Share	2448	0.00	0.00	24.20	0.00	46.98	100.00	46.98
Unemployment Rate	2448	0.97	3.45	5.43	4.87	7.02	18.17	3.57
Avg. Monthly Salary (%)	2448	65.40	77.20	83.50	81.45	87.10	166.00	9.90
Working Age. Pop. Share	2448	46.90	61.00	62.08	62.10	63.20	68.60	2.20
Elderly Pop. Share	2448	10.80	17.10	19.25	19.10	21.10	40.70	4.00
Population (Log.) (2014)	2448	7.20	8.52	9.08	8.93	9.48	14.37	0.96
Population (Log.) (2010)	2448	7.22	8.53	9.09	8.93	9.47	14.35	0.94

A.7 Empirical Examination (Case 1) Supplement: Further Information on the Underlying Dataset—Relevant Polish Counties

With respect to Poland, for historical reasons, we exclude a number of municipalities from our geographic regression discontinuity analysis. Specifically, we exclude all territories that historically belonged to interwar Germany. The reason for us to remove this set of observations is that, after World War II, a massive population resettlement took place. Many Poles from the easternmost and southern parts of Poland were forced to relocate to the west. As a part of this process, they were resettled into the former territories of Germany (while the previous German inhabitants of these areas fled to the west or were forcibly removed). Given these massive population resettlements (Charnysh, 2019; Charnysh and Peisakhin, 2022), we cannot treat the areas of interwar Germany in the same way as the parts that belonged to interwar Prussia. Thus, we have excluded them from our geographic regression discontinuity analysis. This decision is visualized in Figure A2.

Figure A2: Map of the Data Used in Our Analysis



Note: This map shows the municipalities that are included in our GRDD regressions. Only green municipalities were included in those regressions; blue municipalities were excluded for the reasons described above.

A.8 Empirical Examination (Case 1) Supplement: Further Information on the Geographic Regression Discontinuity Models

In this section, we provide the explicit mathematical functions that we use to measure the geographic location of Polish municipalities in the different versions of our GRDD.

Distance to Border:

$$f(\text{geographic location}) = \gamma_1 \text{ distance to border}_i + \gamma_2 \text{ distance to border}_i * EMP_{ji} \quad (1)$$

Distance to the border is measured in km, with negative values denoting one empire in a pairwise comparison, positive values denoting another one. Coefficients of the distance terms are represented by γ .

Distance to Border with Second-Order Polynomial:

$$f(\text{geographic location}) = \gamma_1 \text{ distance to border}_i + \gamma_2 \text{ distance to border}_i^2 + \gamma_3 \text{ distance to border}_i * EMP_{ji} + \gamma_4 \text{ distance to border}_i^2 * EMP_{ji} \quad (2)$$

Distance to the border is again measured in km, with negative values denoting one empire in a pairwise comparison, positive values denoting another one. Coefficients of the distance terms are again represented by γ .

Distance to Border with Latitude and Longitude:

$$f(\text{geographic location}) = \gamma_1 x + \gamma_2 y + \gamma_3 \text{ distance to border}_i + \gamma_4 \text{ distance to border}_i * EMP_{ji} \quad (3)$$

In this regression format, x stands for latitude and y stands for longitude. Coefficients are again represented by γ .

A.9 Empirical Examination (Case 1) Supplement: Additional Results

The following tables and figures complement the discussion of the results in the main body of the paper. [Table A2](#) shows the regression results of our Prussia/Russia comparison with a number of covariates included. [Figure A3](#) and [Figure A4](#) represent additional RDD graphs of the alternative outcome measures. Finally, [Figure A5](#) and [Figure A6](#) represent additional maps of the alternative outcome measures.

A.10 Empirical Examination (Case 1) Supplement: Simple Dummy Variable Analysis

In the main body of the paper, we presented an empirical comparison of municipalities in the formerly Prussia and Russian partitions that was based on a geographic regression discontinuity design. The key advantage of this approach is that it allows us to rule out alternative explanatory factors that do not jump discontinuously at the critical threshold, which is the imperial border (such as the distance to Russia).

At the same time, in the interest of full transparency, in this Appendix section, we also present the results of a simple dummy variable analysis that does not rely on a GRDD and considers the full sample of Polish municipalities. We mainly consider this a robustness check through which we can potentially confirm the general direction of our empirical results with a broader sample.

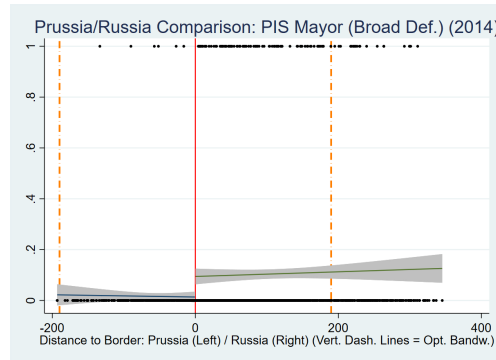
Table A2: Local Political Leadership Outcomes (Prussia/Russia Comparison) (With Controls)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)	Mayor PiS (4)	Mayor PiS (Broad) (5)	Mayor PiS (2010) (6)
Russia	0.066** (0.028)	0.072** (0.028)	0.050** (0.024)	0.092*** (0.032)	0.102*** (0.033)	0.053* (0.027)
Elevation	-0.00000 (0.0002)	0.00001 (0.0002)	0.0002 (0.0002)	0.00001 (0.0001)	0.00001 (0.0001)	0.0002* (0.0001)
Lat.	-0.003 (0.017)	-0.003 (0.017)	0.004 (0.014)			
Long.	0.011 (0.009)	0.010 (0.010)	0.002 (0.008)			
Pop. Density				0.00004 (0.00003)	0.00004 (0.00003)	0.00000 (0.00002)
Urban Share				0.0001 (0.0004)	0.0001 (0.0004)	0.0001 (0.0003)
Unemploy. Rate				-0.002 (0.004)	-0.002 (0.004)	0.0003 (0.003)
Avg. Monthly Salary				0.001 (0.001)	0.001 (0.001)	0.0002 (0.001)
Work. Age Pop. Share				0.019** (0.009)	0.022** (0.009)	-0.006 (0.008)
Elderly Pop. Share				0.002 (0.005)	0.002 (0.006)	-0.008 (0.005)
Pop. (Log) (2014)				0.004 (0.015)	0.007 (0.015)	
Pop. (Log) (2010)						-0.001 (0.013)
Dist. PR-RU	-0.0002 (0.0004)	-0.0002 (0.0004)	0.0001 (0.0004)	-0.0002 (0.0005)	-0.0003 (0.001)	0.0002 (0.0004)
Russia * Dist. PR-RU	0.0001 (0.0003)	0.0001 (0.0004)	-0.0001 (0.0003)	0.0004 (0.001)	0.0003 (0.001)	-0.0001 (0.001)
Constant	-0.003 (0.760)	0.002 (0.781)	-0.248 (0.664)	-1.339** (0.637)	-1.535** (0.652)	0.498 (0.544)
Observations	1,435	1,435	1,437	867	867	868
R ²	0.020	0.022	0.018	0.037	0.040	0.022
Adjusted R ²	0.016	0.018	0.014	0.024	0.027	0.009

*p<0.1; **p<0.05; ***p<0.01

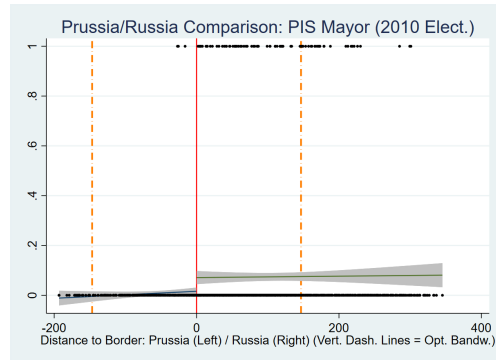
Note: OLS

Figure A3: GRDD Graph 2: Mayor PiS (Broad)



Note: This figure represents a comparison of the success of PiS candidates in mayoral elections (2014 elections, broad definition) between former Prussian and Russian municipalities in Poland. Municipalities in the former Prussian partition are on the left; municipalities in the former Russian partition are on the right. The line at zero represents the historical border. The x-axis represents municipalities' distance to the historical border. The corresponding regression is model 2 in Table 1 in the main paper.

Figure A4: GRDD Graph 3: Mayor PiS (2010)



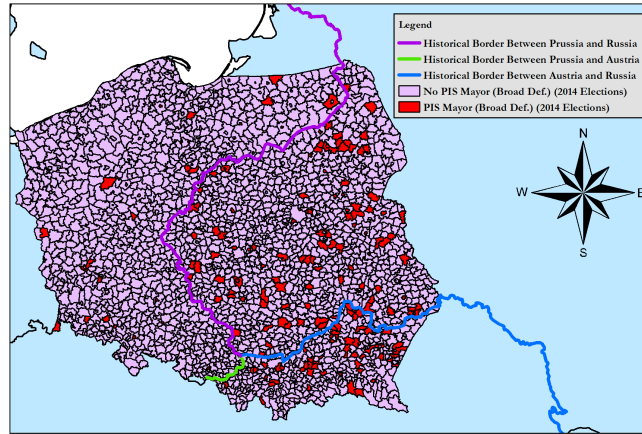
Note: This figure represents a comparison of the success of PiS candidates in mayoral elections (2010 elections) between former Prussian and Russian municipalities in Poland. Municipalities in the former Prussian partition are on the left; municipalities in the former Russian partition are on the right. The line at zero represents the historical border. The x-axis represents municipalities' distance to the historical border. The corresponding regression is model 3 in Table 1 in the main paper.

A.10.1 Additional Independent Variables in the Simple Dummy Variable Analysis

As we present an analysis of the full sample of Polish municipalities here, in addition to a variable for “Russia” and the baseline of “Prussia”, we add two further independent variables:

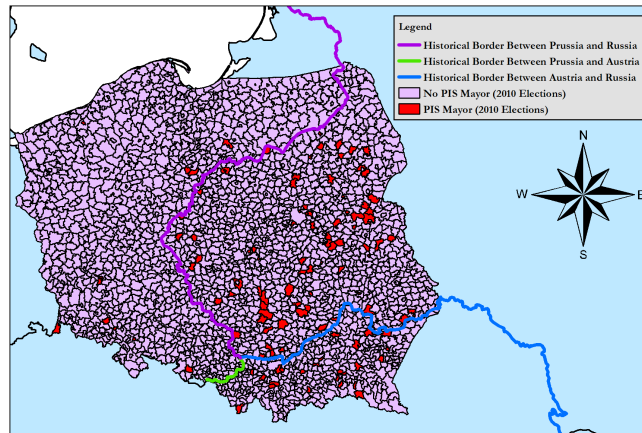
1. **Austria:** This dummy variable indicates if a municipality historically belonged to the Austrian partition (1815–1914). (Additional results related to Austria (in pairwise GRDD comparisons) are further below, in section A.9.)
2. **Interwar Germany:** This dummy variable indicates if a municipality historically belonged to Interwar Germany (1918–1939). These territories experienced significant population transfers from the formerly Russian partition and from eastern Galicia after World War II (Charnysh, 2019). Thus, they

Figure A5: Map of PiS-affiliated Mayors (Broad Definition) in the 2014 Elections



Note: This map shows the success of PiS candidates in mayoral elections (2014 elections, broad definition) across all three partitions of Poland. The Prussian partition is the western one; the Russian partition is the eastern one; the Austrian partition is the southern one.

Figure A6: Map of PiS-affiliated Mayors in the 2010 Elections



Note: This map shows the success of PiS candidates in mayoral elections (2010 elections) across all three partitions of Poland. The Prussian partition is the western one; the Russian partition is the eastern one; the Austrian partition is the southern one.

need to be distinguished from other parts of the Prussian partition.⁹

A.10.2 Models in the Simple Dummy Variable Analysis

This additional empirical test is based on simple dummy variable analyses. The regressions have the following format:

$$y_i = \beta_0 + \sum_{j=1}^n \beta_j EMP_{ji} + \beta_{n+1} ELV_i + \mathbf{x}_i' \boldsymbol{\beta} + \varepsilon_i \quad (4)$$

⁹As shown in Figure A2, these observations are excluded in our geographic regression discontinuity models.

where y_i is the dependent variable for municipality i . β_j represents the difference in the value of the dependent variable between municipalities that belonged to empire (EMP) j and those that belonged to the baseline category. The baseline category is Prussian municipalities that did not belong to interwar Germany. β_{j+1} is the coefficient for elevation (ELV). Control variables are contained in vector $\mathbf{x}$.

A.10.3 Results

Table A3 and Table A4 show the results of the dummy variable regressions. In the Appendix, we also add control variables. While this introduces the possibility of posttreatment bias, said variables might also have an important independent influence.

As our findings in the main body of the paper, these additional results also confirm that there are significant differences across the partitions in accordance with our expectations. In particular, even when considering the full sample, there is a significantly higher likelihood that municipalities have a PiS-affiliated mayor in the formerly Russian partition when compared to the Prussian partition.

Importantly, as shown in the following Appendix section (subsection A.11), the initial finding presented here that Austria is more likely to have PiS mayors does not hold in some of the more rigorous GRDD regressions. This indicates that underlying geographic patterns and their possible effects on social organization could play a role in explaining the discrepancy.

Table A3: Local Political Leadership Outcomes

	<i>Dependent variable:</i>		
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)
Russia	0.080*** (0.015)	0.087*** (0.016)	0.065*** (0.012)
Interwar Germany	0.006 (0.016)	0.008 (0.017)	0.005 (0.014)
Austria	0.112*** (0.019)	0.123*** (0.019)	0.053*** (0.015)
Constant	0.016 (0.013)	0.016 (0.013)	0.008 (0.011)
Observations	2,445	2,445	2,448
R ²	0.028	0.031	0.020
Adjusted R ²	0.027	0.030	0.019

Note: OLS

*p<0.1; **p<0.05; ***p<0.01

A.11 Empirical Examination (Case 1) Supplement: Results of Comparisons with Austria

In the main body of the study we have omitted the pairwise (GRDD) comparisons of Prussia/Austria and Austria/Russia. We have done this because the classification of the Austrian case is more ambiguous due to the shifting character of Austrian rule over time. As explained in some more detail above (subsection A.3), Austria's rule initially was highly oppressive, but later adopted a more participatory style in administrative affairs. Nevertheless, Austria only introduced full democratic participation in federal elections very late (in 1907), meaning that it might not have had a profound long-term impact on the dynamics discussed by us. Finally, socioeconomic structures and the urban landscape might differ to an extent in the Austrian partition that an effective comparison with Prussia and Russia is more difficult (Charasz, 2021). For these reasons, in the main body of the study we focus on the distinction between Prussia and Russia.

Despite these important points, we include the results of the additional comparisons for full transparency below.

Table A4: Local Political Leadership Outcomes (With Controls)

	<i>Dependent variable:</i>		
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)
Russia	0.111*** (0.017)	0.119*** (0.017)	0.088*** (0.014)
Interwar Germany	0.017 (0.018)	0.020 (0.018)	0.018 (0.015)
Austria	0.146*** (0.022)	0.157*** (0.023)	0.054*** (0.018)
Elevation	−0.0001** (0.00005)	−0.0001** (0.00005)	0.00004 (0.00004)
Pop. Density	0.00004** (0.00002)	0.00004** (0.00002)	0.00003* (0.00001)
Urban Share	0.0005** (0.0002)	0.0004 (0.0002)	0.0003 (0.0002)
Unemploy. Rate	−0.002 (0.002)	−0.002 (0.002)	−0.001 (0.002)
Avg. Monthly Salary	−0.001* (0.001)	−0.001** (0.001)	−0.0005 (0.0005)
Work. Age Pop. Share	0.005 (0.004)	0.005 (0.005)	−0.003 (0.004)
Elderly Pop. Share	−0.003 (0.003)	−0.003 (0.003)	−0.006*** (0.002)
Pop. (Log) (2014)	0.001 (0.009)	0.009 (0.010)	
Pop. (Log) (2010)			0.001 (0.008)
Constant	−0.155 (0.328)	−0.213 (0.339)	0.285 (0.272)
Observations	2,445	2,445	2,448
R ²	0.044	0.047	0.029
Adjusted R ²	0.040	0.043	0.025

Note: OLS

*p<0.1; **p<0.05; ***p<0.01

A.11.1 Austria/Russia Comparison

Table A5 includes the main GRDD results for the Austria/Russia comparison without control variables. The results highly depend on specification and can therefore be classified as inconclusive. The initial results no longer show any level of significance when alternative DVs or second-order polynomials of distance are used.

Table A6 includes additional results for the Austria/Russia comparison with the latitude/longitude specification and control variables. In many cases, the significance of the key variable depends on model specification, making the initial results fragile.

Overall, these findings highlight the more ambiguous character of Austrian rule in the southern partition of Poland. Specifically, the results clearly show that this ambivalent character of Austrian rule did not produce a coherent legacy that differs from the long-term effects of Prussian or Russian rule in a consistent way.

A.11.2 Prussia/Austria Comparison

Table A7 includes the main GRDD results for the Prussia/Austria comparison without control variables. Similar to the Austria/Russia comparison, the initial results are not confirmed by the more complex models.

Table A8 includes additional results for the Prussia/Austria comparison with the latitude/longitude specification and control variables. Once additional covariates are introduced, the results are no longer significant.

Similar to the previous Austria/Russia comparison, the results remain inconclusive. This may be partly related to the smaller sample size that is available to us (when it comes to the analysis of Austrian municipalities), but it may also be a long-term outcome of the more ambiguous and changing character of Austrian rule in Galicia (the southern partition of Poland).

A.12 Empirical Examination (Case 1) Supplement: Mayors with Higher Education

One of our paper's theoretical claims is about the cultural values that parents instill in their children as a consequence of severe and long episodes of the suppression of self-government. Specifically, those kinds of suppression episodes in particular reduce the likelihood of parents emphasizing cultural values related to self-efficacy, professionalism, and egalitarianism in their children's upbringing. Instead, our theory suggests that parents in these contexts tend to emphasize cultural values related to authoritarianism/populism and beliefs in hierarchical social orders.

As touched upon in the main body of the paper, with respect to the case of Poland, there are already multiple pieces of empirical evidence that suggest that values related to self-efficacy, professionalism, and egalitarianism are significantly more prevalent in the formerly Prussian parts of Poland when compared to the formerly Russian parts (for details, see [Hryniewicz, 1996](#); [Vogler, 2019](#)).

Additionally, in **Table A9**, we present new empirical evidence showing that municipalities in the formerly Russian parts of Poland are significantly less likely to have mayors with higher education when compared to the formerly Prussian partition. Building on the literature that connects self-efficacy and educational attainment (e.g., [Ayllón, Alsina and Colomer, 2019](#); [Grabowski, Call and Mortimer, 2001](#); [Schunk, 1989](#)) and the literature that connects populism and distrust in experts and higher education (e.g., [Merkley, 2020](#); [Read, 2018](#)), we suggest that these novel results are indicative of differences in the aforementioned cultural dimensions in line with our argument.

Overall, the combination of insights from the literature and these new results supports the notion that variation in imperial rule led to major differences in cultural values across the partitions that are still observable today.

Table A5: Local Political Leadership Outcomes (Austria/Russia Comparison)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)	Mayor PiS (4)	Mayor PiS (Broad) (5)	Mayor PiS (2010) (6)
Russia	-0.075** (0.037)	-0.085** (0.038)	0.039 (0.031)	-0.102* (0.053)	-0.103* (0.055)	0.003 (0.045)
Elevation	-0.0002** (0.0001)	-0.0002** (0.0001)	-0.00002 (0.0001)	-0.0002** (0.0001)	-0.0002** (0.0001)	-0.00001 (0.0001)
Dist. AU-RU	0.001 (0.001)	0.001 (0.001)	-0.00002 (0.001)	0.002 (0.002)	0.002 (0.002)	0.001 (0.002)
Dist. AU-RU Sq.				-0.000002 (0.000002)	-0.000002 (0.000002)	-0.000001 (0.000002)
Russia * Dist. AU-RU	-0.001 (0.001)	-0.001 (0.001)	0.00002 (0.001)	-0.003 (0.002)	-0.003 (0.002)	-0.001 (0.002)
Russia*Dist. AU-RU Sq.				0.00002 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)
Constant	0.230*** (0.035)	0.253*** (0.036)	0.062** (0.030)	0.265*** (0.048)	0.281*** (0.049)	0.084** (0.040)
Observations	1,420	1,420	1,422	1,420	1,420	1,422
R ²	0.009	0.011	0.002	0.010	0.012	0.003
Adjusted R ²	0.007	0.008	-0.0004	0.006	0.008	-0.001

Note: OLS
* p<0.1; ** p<0.05; *** p<0.01

Table A6: Local Political Leadership Outcomes (Austria/Russia Comparison) (With Controls)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)	Mayor HE (4)	Mayor PiS (5)	Mayor PiS (Broad) (6)
Russia	-0.082** (0.038)	-0.089** (0.039)	0.031 (0.032)	-0.033 (0.051)	-0.051 (0.053)	0.069 (0.043)
Elevation	-0.0002* (0.0001)	-0.0002* (0.0001)	0.00001 (0.0001)	-0.0002** (0.0001)	-0.0003** (0.0001)	-0.00001 (0.0001)
Lat.	0.010 (0.057)	-0.015 (0.059)	0.053 (0.048)			
Long.	0.009 (0.008)	0.012 (0.008)	-0.003 (0.007)			
Pop. Density				0.00003 (0.0001)	0.00000 (0.0001)	0.0001** (0.00005)
Urban Share				0.00004 (0.001)	-0.0002 (0.001)	0.00005 (0.0005)
Unemply. Rate				-0.001 (0.005)	-0.001 (0.005)	0.0003 (0.004)
Avg. Monthly Salary				-0.002 (0.001)	-0.002 (0.001)	0.0001 (0.001)
Work. Age Pop. Share				-0.004 (0.011)	-0.010 (0.011)	0.002 (0.009)
Elderly Pop. Share				-0.010 (0.007)	-0.011 (0.007)	-0.008 (0.005)
Pop. (Log) (2014)				0.029 (0.025)	0.050* (0.026)	
Pop. (Log) (2010)						0.006 (0.021)
Dist. AU-RU	0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	-0.00005 (0.001)
Russia * Dist. AU-RU	-0.001 (0.001)	-0.001 (0.001)	-0.0001 (0.001)	-0.002* (0.001)	-0.002* (0.001)	-0.0003 (0.001)
Constant	-0.491 (2.751)	0.722 (2.835)	-2.524 (2.307)	0.553 (0.819)	0.833 (0.848)	-0.012 (0.676)
Observations	1,420	1,420	1,422	710	710	711
R ²	0.012	0.014	0.003	0.035	0.041	0.022
Adjusted R ²	0.008	0.009	-0.001	0.020	0.026	0.006

Note: OLS
*p<0.1; **p<0.05; ***p<0.01

Table A7: Local Political Leadership Outcomes (Prussia/Austria Comparison)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)	Mayor PiS (4)	Mayor PiS (Broad) (5)	Mayor PiS (2010) (6)
Austria	0.116*** (0.042)	0.127*** (0.044)	0.062** (0.031)	0.043 (0.052)	0.054 (0.054)	0.038 (0.038)
Elevation	-0.0003*** (0.0001)	-0.0003*** (0.0001)	-0.00004 (0.0001)	-0.0003*** (0.0001)	-0.0003*** (0.0001)	-0.00005 (0.0001)
Dist. PR-AU	0.0001 (0.0001)	0.0001 (0.0001)	-0.00001 (0.0001)	0.0004 (0.0003)	0.0005 (0.0003)	-0.00004 (0.0002)
Dist. PR-AU Sq.				-0.00000 (0.00000)	-0.00000 (0.00000)	0.00000 (0.00000)
Austria * Dist. PR-AU	0.0002 (0.0002)	0.0002 (0.0002)	0.00005 (0.0002)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Russia*Dist. PR-AU Sq.				-0.00000* (0.00000)	-0.00000 (0.00000)	-0.00000 (0.00000)
Constant	0.070** (0.035)	0.077** (0.036)	0.010 (0.025)	0.100** (0.042)	0.108** (0.044)	0.009 (0.031)
Observations	729	729	729	729	729	729
R ²	0.075	0.085	0.023	0.082	0.091	0.026
Adjusted R ²	0.070	0.079	0.018	0.074	0.084	0.018

Note: OLS
* p<0.1; ** p<0.05; *** p<0.01

Table A8: Local Political Leadership Outcomes (Prussia/Austria Comparison) (With Controls)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) (2)	Mayor PiS (2010) (3)	Mayor HE (4)	Mayor PiS (5)	Mayor PiS (Broad) (6)
Austria	0.113*** (0.044)	0.123*** (0.045)	0.050 (0.032)	0.092 (0.065)	0.099 (0.068)	0.045 (0.056)
Elevation	-0.0001 (0.0001)	-0.0002* (0.0001)	-0.0001 (0.0001)	-0.00001 (0.0001)	-0.00004 (0.0001)	0.00000 (0.0001)
Lat.	0.129*** (0.065)	0.111* (0.066)	-0.074 (0.047)			
Long.	-0.014 (0.019)	-0.010 (0.020)	0.027* (0.014)			
Pop. Density				0.00002 (0.00005)	0.00001 (0.0001)	-0.00001 (0.00004)
Urban Share				0.0002 (0.001)	0.0001 (0.001)	-0.0001 (0.001)
Unemp. Rate				-0.017 (0.018)	-0.015 (0.019)	-0.010 (0.015)
Avg. Monthly Salary				0.0002 (0.002)	0.0003 (0.002)	0.0004 (0.002)
Work. Age Pop. Share				-0.019 (0.018)	-0.021 (0.019)	0.008 (0.015)
Elderly Pop. Share				-0.020* (0.012)	-0.022* (0.012)	0.00004 (0.010)
Pop. (Log) (2014)				-0.015 (0.029)	0.00002 (0.030)	
Pop. (Log) (2010)						0.014 (0.025)
Dist. PR-AU	0.001** (0.001)	0.001* (0.001)	-0.001 (0.0004)	-0.001 (0.002)	-0.001 (0.002)	-0.0004 (0.002)
Austria * Dist. PR-AU	-0.001 (0.0005)	-0.001 (0.0005)	0.0003 (0.0003)	-0.0001 (0.002)	0.0002 (0.002)	0.001 (0.002)
Constant	-6.114** (3.006)	-5.317* (3.096)	3.216 (2.185)	1.698 (1.260)	1.745 (1.318)	-0.609 (1.074)
Observations	729	729	729	191	191	191
R ²	0.081	0.089	0.028	0.051	0.051	0.026
Adjusted R ²	0.073	0.082	0.020	-0.008	-0.007	-0.034

Note: OLS
*p<0.1; **p<0.05; ***p<0.01

Table A9: Local Political Leadership Outcomes (Extension: Mayor with Higher Education)

	<i>Dependent variable:</i>		
	Mayor with Higher Education		
	Simple Dummy Variables (1)	Regression Discontinuity Design (2 & 3)	
	(1)	(2)	(3)
Russia	−0.069*** (0.018)	−0.053* (0.029)	−0.079** (0.039)
Interwar Germany	−0.047** (0.019)		
Austria	−0.052** (0.022)		
Elevation		−0.0002* (0.0001)	−0.0002* (0.0001)
Dist. PR-RU		−0.0001 (0.0003)	0.0003 (0.001)
Dist. PR-RU Sq.			−0.00000 (0.00001)
Russia*Dist.		0.0001 (0.0004)	0.0002 (0.001)
Russia*Dist. Sq.			0.00000 (0.00001)
Constant	0.954*** (0.015)	0.978*** (0.031)	0.984*** (0.040)
Observations	2,445	1,435	1,435
R ²	0.006	0.013	0.014
Adjusted R ²	0.005	0.010	0.010

Note: OLS

*p<0.1; **p<0.05; ***p<0.01

A.13 Empirical Examination (Case 1) Supplement: Accounting for Differences in Relative Migration Levels

Migration could potentially interfere with the results in the Polish case in two distinct ways. First, after World War II, there was extensive forced migration across large parts of Eastern Europe, including in the territories of contemporary Poland. Specifically, many Poles were forcibly resettled from territories in the east (which had previously mostly been under Russian imperial control) to former German territories in the west. These massive migration movements (with people *not* leaving their culture behind in old territories, but instead bringing it with them to the westernmost regions) could interfere with our theory that mainly relies on a *cultural* transmission mechanism. To address this possibility, as briefly touched upon in the main text and detailed in [subsection A.7](#), we already systematically excluded the affected western districts that historically belonged to interwar Germany in all of our main analyses (e.g., [Charnysh and Peisakhin, 2022](#)).

In addition to this first concern, a second potential problem arises from within-country migration in the postwar period. Municipalities with generally high migration rates in particular may be prone to changing their sociocultural profiles more quickly over time, making it imperative to account for relative migration levels. Accordingly, in this section we present a series of extensions of our key empirical results that aim at addressing this potential issue. In the first set of results (which is contained in [Table A10](#)), we control for a municipality's migration levels per 1000 inhabitants. We find that all our central results hold when introducing this covariate, indicating that migration does not significantly influence the observed patterns.

In addition, we present a second series of replication analyses of our main results in which we completely exclude the 20% of locations with the highest migration rates (both in terms of in- and out-migration) ([Table A11](#)). After excluding these municipalities from the dataset, we again find all of our key results substantively unchanged. The combination of (a) excluding districts that were historically subject to systematic population resettlement, (b) controlling for contemporaneous migration levels, and (c) rerunning our primary analyses with a dataset that excludes high-migration locations suggests that our results are not driven by variation in migration levels.

Table A10: Local Political Leadership Outcomes (Extension: Controlling for Migration)

	<i>Dependent variable:</i>					
	Mayor PiS	Mayor PiS (Broad) RDD	Mayor PiS (2010)	Mayor PiS	Mayor PiS (Broad) RDD (Optimal Bandwidth)	Mayor PiS (2010)
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	0.105*** (0.027)	0.112*** (0.027)	0.074*** (0.023)	0.092*** (0.028)	0.099*** (0.029)	0.058*** (0.025)
Elevation	0.0001 (0.0001)	0.0001 (0.0001)	0.0002*** (0.0001)	0.00004 (0.0001)	0.00004 (0.0001)	0.0003*** (0.0001)
Pop. Density	0.0001*** (0.00002)	0.00004* (0.00002)	0.00003 (0.00002)	0.00004** (0.00002)	0.00003 (0.00002)	0.00002 (0.00002)
Urban Share	0.001* (0.0003)	0.001* (0.0003)	0.0004 (0.0003)	0.0005 (0.0003)	0.0005 (0.0003)	0.0002 (0.0003)
Unemply. Rate	-0.005 (0.003)	-0.005 (0.003)	-0.002 (0.003)	-0.004 (0.003)	-0.004 (0.003)	-0.0002 (0.003)
Avg. Monthly Salary	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.0003 (0.001)	-0.0004 (0.001)	0.00004 (0.001)
Work. Age Pop. Share	0.0001 (0.007)	0.002 (0.007)	-0.011* (0.006)	0.005 (0.007)	0.007 (0.007)	-0.002 (0.007)
Elderly Pop. Share	-0.008* (0.004)	-0.007 (0.004)	-0.012*** (0.004)	-0.007 (0.004)	-0.006 (0.004)	-0.008*** (0.004)
Pop. (Log) (2014)	-0.001 (0.014)	0.006 (0.014)		0.002 (0.014)	0.011 (0.014)	
Pop. (Log) (2010)			-0.0001 (0.012)			-0.002 (0.012)
Migration Levels (per 1000)	-0.002 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.002 (0.001)	-0.002 (0.001)	-0.0004 (0.001)
Dist. PR-RU	0.00001 (0.0003)	-0.00002 (0.0003)	0.0003 (0.0003)	-0.00002 (0.0003)	-0.00005 (0.0003)	0.0001 (0.0003)
Russia * Dist. PR-RU	0.0001 (0.0003)	0.0002 (0.0003)	-0.0002 (0.0003)	0.0004 (0.0004)	0.0004 (0.0004)	0.00003 (0.0004)
Constant	0.201 (0.494)	-0.021 (0.508)	0.915** (0.432)	-0.183 (0.525)	-0.395 (0.537)	0.289 (0.475)
Observations	1,435	1,435	1,437	1,236	1,244	1,110
R ²	0.043	0.043	0.033	0.044	0.046	0.030
Adjusted R ²	0.035	0.035	0.025	0.034	0.036	0.019

Note: OLS
*p<0.1; **p<0.05; ***p<0.01

Table A11: Local Political Leadership Outcomes (Extension: Excluding High-Migration Locations)

	<i>Dependent variable:</i>					
	Mayor PiS (1)	Mayor PiS (Broad) RDD (2)	Mayor PiS (2010) (3)	Mayor PiS (4)	Mayor PiS (Broad) RDD (Optimal Bandwidth) (5)	Mayor PiS (2010) (6)
Russia	0.112*** (0.029)	0.121*** (0.030)	0.085*** (0.026)	0.097*** (0.030)	0.106*** (0.031)	0.068** (0.028)
Elevation	0.0001 (0.0001)	0.0001 (0.0001)	0.0002** (0.0001)	0.00004 (0.0001)	0.0001 (0.0001)	0.0003*** (0.0001)
Pop. Density	0.00003 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)	0.00002 (0.00002)	0.00001 (0.00002)	-0.00000 (0.00002)
Urban Share	0.001*** (0.0004)	0.001*** (0.0004)	0.001* (0.0003)	0.001** (0.0004)	0.001** (0.0004)	0.0004 (0.0003)
Unemploy. Rate	-0.007** (0.003)	-0.006** (0.003)	-0.002 (0.003)	-0.006* (0.003)	-0.005 (0.003)	0.00000 (0.003)
Avg. Monthly Salary	-0.002** (0.001)	-0.002** (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002* (0.001)	-0.0003 (0.001)
Work. Age Pop. Share	-0.00001 (0.008)	0.001 (0.008)	-0.013* (0.007)	0.004 (0.008)	0.005 (0.008)	-0.004 (0.008)
Elderly Pop. Share	-0.007 (0.005)	-0.006 (0.005)	-0.012*** (0.004)	-0.005 (0.005)	-0.004 (0.005)	-0.008* (0.005)
Pop. (Log) (2014)	-0.003 (0.015)	0.003 (0.015)		0.001 (0.015)	0.009 (0.015)	
Pop. (Log) (2010)			0.001 (0.013)			0.001 (0.014)
Dist. PR-RU	-0.0002 (0.0004)	-0.0002 (0.0004)	0.0001 (0.0003)	-0.0002 (0.0004)	-0.0003 (0.0004)	-0.00000 (0.0004)
Russia * Dist. PR-RU	0.0005 (0.0004)	0.0005 (0.0004)	-0.0001 (0.0003)	0.001 (0.0004)	0.001* (0.0004)	0.0002 (0.0004)
Constant	0.288 (0.561)	0.123 (0.572)	1.045** (0.503)	-0.050 (0.590)	-0.207 (0.602)	0.388 (0.564)
Observations	1,147	1,147	1,149	990	997	882
R ²	0.053	0.053	0.036	0.047	0.050	0.035
Adjusted R ²	0.043	0.044	0.026	0.036	0.039	0.023

*p<0.1; **p<0.05; ***p<0.01

Note: OLS

A.14 First Case Supplement: Proximity to Russia as an Alternative Explanation

Our findings reveal a clear empirical pattern: Municipalities in the formerly Russian partition of Poland are much more likely to have a PiS-affiliated or PiS-endorsed mayor than municipalities in the formerly Prussian partition. From the perspective of our theoretical framework, these patterns can be linked to differences in the character of the historical suppression of self-government between these major imperial powers.

However, a possible alternative explanation is that geographic proximity to Russia is (partly) responsible for the observed dynamics. Geographic proximity to Russia could (potentially) make it more likely that authoritarian cultural values prevalent in the Russian Empire (and later the Soviet Union) diffuse and influence culture in Poland, especially in the eastern parts of the country.

Although this explanation is plausible at first glance, there are two key reasons why it is not a sufficient or strong alternative explanation for the patterns observed by us.

The first and most important reason is that our analysis largely relies on a geographic regression discontinuity design (GRDD) through which we identify a discontinuous jump in key outcome variables at the historical imperial border between Prussia and Russia. The alternative explanation of proximity to Russia would only be a viable alternative account if distance to Russia would also jump discontinuously at the threshold, which it does not. Accordingly, our research design allows us to rule out this alternative explanation.

In addition to the above—mostly technical—reason, the influence of Russian cultural values would be a much stronger possible alternative account if it was not for the extreme and persistent historical (political and cultural) antagonism between Poles and Russians (Davies, 2005). In fact, even when Russia ruled large parts of Poland, the Poles generally considered Russians to be culturally inferior (Weeks, 1994). This rejection of Russia, especially Russian culture and political influence, persisted throughout the post-imperial period. Among others, it was visible in the fact that Poland had one of the strongest anti-Soviet political movements in the eastern bloc in the form of “Solidarność” (Solidarity) (Kubow, 2013). The historian Timothy Garton Ash goes as far as to say that “the Polish national identity is historically defined *in opposition to Russia*” (Ash, 1991, 5, emphasis added).¹⁰ For these reasons, cultural and political influences of Russia or the diffusion of values as a result of proximity are also not a strong alternative explanation from a substantive perspective.

In sum, proximity to Russia is not a strong alternative explanation for the observed geographic divergence in electoral outcomes for two main reasons. First, from a purely technical perspective, there is not a discontinuous jump in proximity to Russia at the critical threshold in our regression discontinuity design. Second, from a substantive-historical perspective, proximity to Russia as a source of cultural or political diffusion is not a strong alternative explanation due to the forceful and persistent rejection of Russian influence by the Polish people.

A.15 Second Case Supplement: Additional Information on Brazil

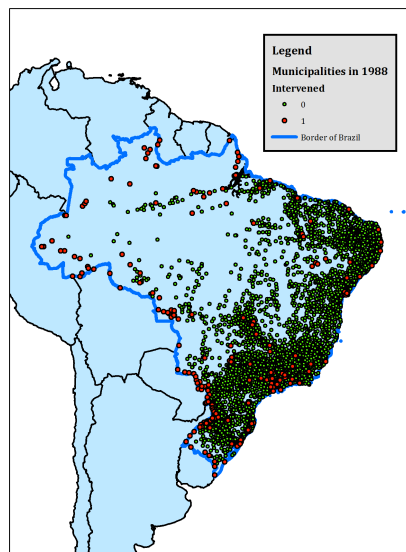
In this section, we provide additional information about the Brazilian case.

A.15.1 The Municipalities with Appointed Mayors

Figure A7 shows the municipalities of our 1988 sample and Figure A8 shows the municipalities of our 1992 sample. Please note that data availability was slightly higher for 1992 than 1988, which is the main reason for a minor divergence in the number of observations between the two time periods.

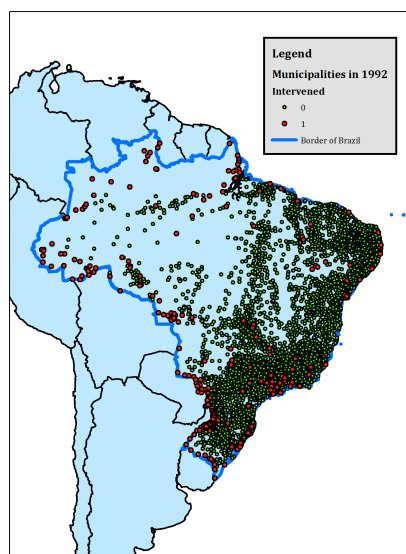
¹⁰Also cited in Kubow (2013, 5).

Figure A7: Map of the Municipalities with Appointed Mayors (1988)



Note: This map shows the municipalities of Brazil in 1988. The green dots represent municipalities that did not previously have appointed mayors (“Intervened” = 0). The red dots represent municipalities that had appointed mayors under the authoritarian regime (“Intervened” = 1).

Figure A8: Map of the Municipalities with Appointed Mayors (1992)



Note: This map shows the municipalities of Brazil in 1992. The green dots represent municipalities that did not previously have appointed mayors (“Intervened” = 0). The red dots represent municipalities that had appointed mayors under the authoritarian regime (“Intervened” = 1).

A.16 Second Case Supplement: Why Should the PFL be Considered an Authoritarian Successor Party?

With respect to the PFL—which is one of the two parties featured in the Brazilian case—there have been various different characterizations in the relevant literature. Specifically, some authors have suggested to think of it simply as a conservative-liberal party, while others have made the case that it should be viewed as an ASP. We have chosen to side with the latter set of contributions for a number of reasons.

It is important to first acknowledge that the *main objection* against classifying this party as an ASP is grounded in the fact the PFL joined the Democratic Alliance in support of the reform candidacy of Tancredo Neves, facilitating the defeat of the unpopular PDS candidate, Paulo Maluf. Neves' victory helped cement the democratic transition which was underway in Brazil at the time.¹¹ In spite of this important fact, the PFL is often described as an offshoot of ARENA (Power, 2000), a “reactive” ASP (Power, 2018), or a continuist pro-military party (Haggard and Kaufman, 1997). For the purposes of our paper, we deem such pro-military characterizations of the PFL as convincing. With respect to our theory and empirics, what is most relevant about the PFL is *how the party was perceived by the public, particularly in the 1988 and 1992 elections*. As even the most reform-minded leaders of the PFL recognized, the party was associated in the public mind with support for the military regime. This is the reason why, according to the PFL's former president, Jorge Bornhausen, the party changed its name in 2007, attempting to clear it of its pro-military image (Ribeiro, n.d., 46).

There are good reasons for why the public would have associated the party with the prior military regime. First, the vast majority of the legislative contingent of the PFL was constituted by former members of ARENA (see also Fn. 43 in the main text). Second, the leading figures in the PFL (Aureliano Chaves, Marco Maciel, Jorge Bornhausen, Antonio Carlos Magalhães, Guilherme Palmeira, José Sarney) all served as (appointed) governors under the military regime, as well as in other important positions in the prior authoritarian government. One such figure (Magalhães) was also an appointed mayor during the military period (in Salvador, Bahia). Third, some of the leading figures in the PFL (Chaves, Magalhães, Sarney) were, as civilian politicians, active supporters of the military intervention in 1964. Fourth, many legislators belonging to the PFL held attitudes hostile to democracy. This is evident in a survey of the Brazilian Congress conducted by Timothy Power in the early 1990s, which found that PFL legislators were the second most hostile to democracy (second only to the PDS) among major parties (5% or more of seats) in terms of support for a constitutional provision designating the armed forces as responsible for maintaining internal law and order, views about the economic performance of authoritarian versus democratic regimes, and views about the ability of authoritarian versus democratic regimes to maintain social order (Power, 2000, 135–137). Fifth, the PFL's legislative voting behavior was consistent with the notion of hostility towards participatory democracy. As evidence, consider that during the National Constituent Assembly (1987–1988), the members of the Assembly voted for an amendment (which ultimately failed) to change elections for president from a direct popular vote to an indirect vote based on an electoral college. As shown in Table A12, 81% of PFL legislators voted in favor of the amendment, the highest percentage of any party (Diário da Assembléia Nacional Constituinte, 23 de Março de 1988). Finally, some observers hold the view that the PFL's membership in the Democratic Alliance merely was a strategic gambit designed maintain access to power in spite of growing popular rejection of the military government and an inevitable transition to civilian rule. The key actors in Brazil's democratic transition, which began in earnest in the late 1970s, were industrial labor unions, ecclesiastical base communities within the Catholic Church, and later, elite members of the business community (Skidmore, 1988). The PFL only separated from the PDS in the presidential elections of 1985 once it became publicly known that Maluf had no chance to win.¹² Joining the Democratic Alliance gave the PFL leadership the vice-presidency and later the presidency itself (after Neves' unexpected death), as well as cabinet positions that cemented control over their state-level political machines. Therefore, it may plausibly be viewed as an opportunistic move rather than one made out of conviction.

¹¹See Ribeiro n.d. for a discussion of this history that offers a generally sympathetic view of the calculus of the PFL leadership.

¹²According to pre-election opinion polls, Maluf had the support of only 10% of voters (Haggard and Kaufman, 1997, 275).

Table A12: Roll Call Voting Results on Amendment for Presidential Elections based on an Electoral College (March 23, 1988) (*Source*: Diário da Assembléia Nacional Constituinte, 23 de Março de 1988)

Party	Yes (%) [count]	No (%) [count]	Abstain (%) [count]
PCB	0% [0]	100% [3]	0% [0]
PCdoB	0% [0]	100% [5]	0% [0]
PDC	75% [3]	25% [1]	0% [0]
PDS	58% [19]	33% [11]	9% [3]
PDT	13% [3]	87% [20]	0% [0]
PFL	81% [96]	17% [20]	3% [3]
PL	50% [4]	50% [4]	0% [0]
PMDB	45% [115]	52% [132]	2% [6]
PSB	75% [3]	25% [1]	0% [0]
PT	0% [0]	100% [15]	0% [0]
PTB	37% [7]	63% [12]	0% [0]
PTR	0% [0]	0% [0]	100% [1]

All told, this history explains why voting for the PFL would have been an attractive choice for individuals who had authoritarian proclivities and were sympathetic to the legacy of the military regime. Ultimately, this is the choice that is the focus of both our theory and empirical analysis.

A.17 Second Case Supplement: Extended Discussion of Evidence for the Mechanisms of Intertemporal Transmission in Brazil

Although studies explicitly studying the intergenerational transmission of values are few and far between for Brazil, those studies that do address this topic have shown that key cultural and political traits are transmitted intergenerationally. One recent paper examines this phenomenon within *quilombos*, which are communities comprised of the descendants of fugitive slaves (Lambais, 2025). Utilizing geo-coded data on the location of *quilombos*, the author finds that proximity to *quilombos* substantially increases the wages of Black Brazilians, improves educational attainment, and leads to greater support for Black political candidates. The key mechanism explaining these findings is the intergenerational transmission of culture. Within and proximate to *quilombos*, one finds greater rates of homogamous family formation, including same race marriages and marriages between followers of Afro-Brazilian religions, as well as the transmission of ancestry-based skills. As such, the study shows that family-based intergenerational transmission—embedded in community institutions—has shaped cultural, economic, and civic outcomes for Black Brazilians.

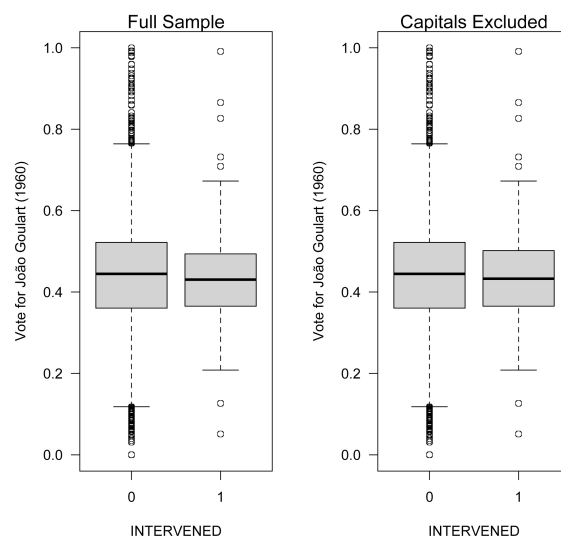
Another recent paper explores the intergenerational transmission of culture in Brazil by examining the transfer of knowledge in the medical domain (Santoro, Chaves and Albuquerque, 2020). Bringing to bear longitudinal ethnographic survey data from the rural community of Carão, Pernambuco, the study examines how knowledge about medicinal plants is acquired. The authors find that the transmission of knowledge is primarily via vertical transmission, i.e. passed down from parents to children. While not squarely in the political domain, this study nevertheless confirms our argument that key aspects of cultural transmission occur within the household. Overall, by demonstrating that cultural transmission from parent to child is quite important in varied domains, these studies imply that a legacy *could have* formed in the Brazilian case had the intervention been longer and more repressive in nature.

A.18 Second Case Supplement: Vote Shares for João Goulart in the 1960s

As discussed in the main body of the study, it is important for us to account for the political orientation of a municipality prior to the authoritarian regime (as this may influence the likelihood of intervention). In this respect we chose the vote share for João Goulart in the 1960 vice-presidential election as a strong proxy.

Figure A9 shows its distribution between the two types of municipalities, indicating that this is not a strong explanatory factor for intervention.

Figure A9: Vote Shares for João Goulart in the 1960 Vice-Presidential Election



Note: These box plots show two sets of comparisons of the vote share for João Goulart. The first part of the plot (on the left) includes the full sample, the second part of the plot (on the right) excludes state capitals. In each part, the comparison is between municipalities that did not previously have appointed mayors (“Intervened” = 0) and municipalities that had appointed mayors under the authoritarian regime (“Intervened” = 1). The plots indicate that there was no ideological selection of municipalities, ruling out an important possible form of omitted variable bias.

A.19 Descriptive Statistics: Brazil

Table A13 shows descriptive statistics for the case of Brazil.

Table A13: Descriptive Statistics: Brazil

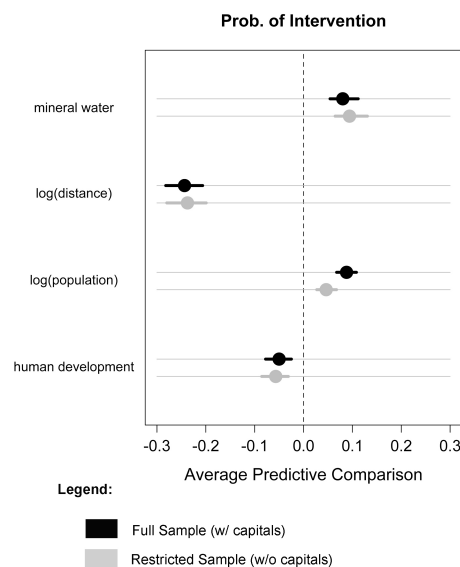
Variable	n	Min	q ₁	$\bar{x}$	$\tilde{x}$	q ₃	Max	IQR
PDS Mayor (1988)	4349	0.00	0.00	0.13	0.00	0.00	1.00	0.00
PDS Mayor (1992)	4923	0.00	0.00	0.10	0.00	0.00	1.00	0.00
PFL Mayor (1988)	4349	0.00	0.00	0.28	0.00	1.00	1.00	1.00
PFL Mayor (1992)	4923	0.00	0.00	0.20	0.00	0.00	1.00	0.00
Intervention	4930	0.00	0.00	0.05	0.00	0.00	1.00	0.00
Mineral Water	4930	0.00	0.00	0.08	0.00	0.00	1.00	0.00
Distance (Log.)	4923	-0.92	6.07	6.55	6.87	7.39	7.74	1.32
Population (Log.) (1996)	4923	6.64	8.64	9.42	9.34	10.03	16.10	1.39
Human Development (1991)	4923	0.36	0.53	0.62	0.64	0.70	0.85	0.17

A.20 Empirical Examination (Case 2) Supplement: Drivers of Intervention

Since our empirical strategy is based on knowledge of the factors that drove the military government's mayoral appointments, here we provide evidence that these factors were indeed strongly associated with the suppression of local self-rule. To that end, we estimate a logistic regression in which we regress *Intervened* onto the four covariates described above. We then plot average predictive comparisons (APCs) (Gelman and Pardoe, 2007) for the covariates to depict the influence of each on the likelihood of intervention.

Figure A10 presents the findings. For *Mineral Water*, the APC was calculated as the average predicted change in the probability of intervention due to all municipalities being assigned a value of 1 instead of 0 on this variable. For the remaining covariates (which are continuous), the APCs were calculated as the average predicted changes in the probability of intervention due to all municipalities being assigned a value equal to the 95th percentile on a given covariate instead of the 5th percentile. As seen in the figure, in all cases the APCs were statistically significant. Most striking was the influence of *Distance to Border (Log.)*: A change from the 5th percentile of this variable to the 95th percentile leads to an increase in the likelihood of intervention of approximately 24 percentage points. The impact of *Mineral Water* was also substantial, leading to a 8–9 percentage point increase in the likelihood of intervention. The associations of population and human development with intervention were generally weaker, with the former being positively associated with intervention and the latter negatively associated with it.

Figure A10: Impact of Covariates on the Likelihood of Intervention (Logistic Regression)



Note: Shown are average predictive comparisons (APCs) (Gelman and Pardoe, 2007). For the binary covariate (*Mineral Water*), the APC shown is the average predicted change in the probability of intervention due to all municipalities being assigned a value of 1 instead of 0. For the continuous covariates, the APCs shown are the average predicted changes in probability due to all municipalities being assigned a value equal to the 95th percentile on a given covariate instead of the 5th percentile. Detailed results for all underlying regressions can be found in Table A13 in the Appendix.

A.21 Second Case Supplement: Contemporaneous Institutional Changes in Brazil

To what degree might contemporaneous institutional changes in Brazil explain our null findings for the legacy of appointed mayors on voting for ASPs in Brazil? Some scholars, for instance, have emphasized the role of the *sublegenda* system in structuring electoral politics during Brazil's military dictatorship (Ferraz, Finan and Martinez-Bravo, 2020). Introduced in 1966, the system was designed to simultaneously increase the electoral chances of incumbent ARENA candidates and weaken the MDB. It accomplished this by allowing the two parties created by the military government to run multiple candidates for majoritarian elections (i.e., mayor, senator). Votes for candidates pooled at the level of the party, so that the winning candidate was the most voted candidate in the party with the most votes, not the most voted candidate overall. This benefited ARENA, especially in rural areas, since the party's membership was primarily constituted by former members of Brazil's often feuding right wing parties: the Social Democratic Party (PSD) and Democratic National Union (UDN). The *sublegenda* system allowed the former PSD and UDN politicians to continue competing at the local level but avoid splitting the right-wing vote, thereby reducing the chances of victory by the MDB. The system was in essence a protection device for traditional local elites (of the right) in Brazil, as it prevented deeply rooted intra-elite squabbles from bringing the opposition to power.

While recognizing that institutions like the *sublegenda* system tilted majoritarian elections in favor of ARENA during the dictatorship, we suggest that this does *not* reduce the relevance of our null findings for post-transition Brazil. This is for two reasons. First, in focusing on the distinction between appointed versus elected mayors, our analytical concern is the modality of selection as opposed to simply the ideology or party affiliation of the office holder. In municipalities where mayors were popularly elected, citizens had the capacity to participate in campaign activities, lobby their neighbors, and ultimately cast their vote for one of the candidates. Contrariwise, in municipalities with appointed mayors, the selection process involved negotiations and discussions between elites and representatives of the military regime only, which meant an *exclusion* of the local population. In this sense, the appointed versus elected mayors distinction clearly reflects the presence or absence of local-self government, the effects of which we assess for the post-authoritarian era through our empirical analyses.

Second, popular elections for mayor under the military government often featured very serious competition between the two parties, despite the armed forces putting their thumb on the scale in favor of ARENA. In 1972, the MDB won mayoral elections in 463 out of the 3785 municipalities that still held popular elections (12%) (Tribunal Superior Eleitoral (TSE), 1979a). In 1976, the number increased to 614 out of 3790 (16%) (Tribunal Superior Eleitoral (TSE), 1979b). By 1982, the PMDB (the renamed MDB) had won 1,377 out of 3,941 mayoral races (35%), eclipsing the number of mayors elected by the PDS (the renamed ARENA) in states like São Paulo, Rio de Janeiro, Paraná, and Goiás (Tribunal Superior Eleitoral (TSE), 1988). Where elections were allowed, victory by the opposition was always a possibility. Moreover, in the waning years of the regime, opposition victory frequently became the norm. Thus, there were real stakes to the mayoral elections held under the military government. They were by no means window dressing with a pre-determined outcome.

In short, we conclude that the distinction between appointed and elected mayors is meaningful even under the institutional constraints imposed by the authoritarian regime (including in the form of the *sublegenda* system). Ultimately, it is this distinction we explore. Furthermore, even under this institutional constraint, the opposition party was able to achieve significant electoral success in a number of circumstances.

A.22 Empirical Examination (Case 2) Supplement: Full Regression Tables for the Analysis of the Brazilian Case

This subsection contains detailed regression tables for all figures located in the paper. Specifically, Table A14 shows the results associated with Figure A10, Table A15 and Table A16 show the results associated with Figure 6, and Table A17 shows the results associated with Figure 7.

Table A14: Logistic Regression of Municipal Intervention on Covariates (Figure A10 in the Appendix)

	(1)	(2)
Distance-to-border	-1.213*** (0.063)	-1.246*** (0.065)
Mineral Water	1.549*** (0.212)	1.836*** (0.230)
Population (log.)	0.665*** (0.072)	0.398*** (0.005)
Human Development	-4.149*** (1.050)	-5.051*** (1.111)
<i>N</i>	4,921	4,890
Capitals Included?	Y	N

Note: Standard errors in parentheses; *90% confidence level, **95% confidence level, ***99% confidence level.

Table A15: Linear Probability Model with State Fixed Effects (w/ Controls) (Figure 6, Part 1 in the Main Paper)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PDS Mayor, 1988		PFL Mayor, 1988		PDS Mayor, 1992		PFL Mayor, 1992	
Intervened	-0.033 (0.027)	-0.033 (0.029)	0.015 (0.038)	0.017 (0.040)	0.006 (0.022)	-0.001 (0.024)	0.006 (0.032)	0.026 (0.034)
Distance-to-border	-0.016* (0.009)	0.017* (0.009)	0.003 (0.013)	0.003 (0.013)	0.015** (0.007)	-0.014* (0.007)	0.004 (0.010)	0.007 (0.010)
Mineral Water	-0.010 (0.018)	0.011 (0.019)	0.012 (0.025)	0.011 (0.026)	0.022 (0.016)	0.021 (0.016)	0.003 (0.022)	0.002 (0.023)
Population (log.)	-0.004 (0.005)	-0.004 (0.005)	-0.024*** (0.007)	-0.024*** (0.007)	-0.018*** (0.004)	-0.019*** (0.004)	-0.018*** (0.006)	-0.017*** (0.006)
Human Development	0.189* (0.108)	0.185* (0.109)	0.008 (0.149)	0.002 (0.150)	-0.013 (0.085)	-0.002 (0.086)	0.019 (0.122)	0.050 (0.123)
<i>N</i>	4,327	4,301	4,327	4,301	4,914	4,883	4,914	4,883
Capitals Included?	Y	N	Y	N	Y	N	Y	N

Note: Standard errors in parentheses; *90% confidence level, **95% confidence level, ***99% confidence level.

Table A16: Linear Probability Model with State Fixed Effects (w/o Controls) (Figure 6, Part 2 in the Main Paper)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PDS Mayor, 1988		PFL Mayor, 1988		PDS Mayor, 1992		PFL Mayor, 1992	
Intervened	-0.013 (0.024)	-0.010 (0.026)	-0.011 (0.033)	-0.000 (0.035)	-0.024 (0.019)	-0.027 (0.021)	-0.014 (0.028)	0.008 (0.029)
<i>N</i>	4,349	4,323	4,349	4,323	4,923	4,892	4,923	4,892
Capitals Included?	Y	N	Y	N	Y	N	Y	N

Note: Standard errors in parentheses; *90% confidence level, **95% confidence level, ***99% confidence level.

Table A17: Coarsened Exact Matching, Average Treatment Effect for the Treated (Figure 7 in the Main Paper)

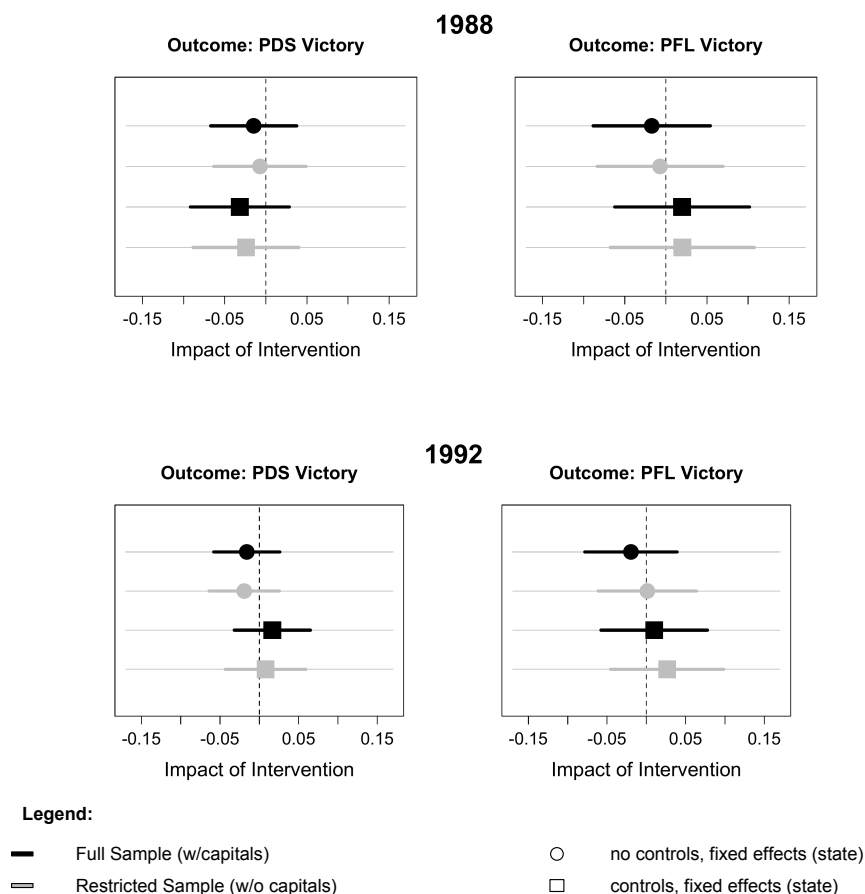
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PDS Mayor, 1988		PFL Mayor, 1988		PDS Mayor, 1992		PFL Mayor, 1992	
ATT (Intervened)	-0.025 (0.030)	-0.032 (0.033)	-0.045 (0.035)	-0.030 (0.037)	-0.018 (0.025)	-0.017 (0.027)	-0.000 (0.028)	0.010 (0.030)
<i>N.treated-all</i>	212	186	212	186	266	235	266	235
<i>N.treated-matched</i>	166	149	166	149	205	183	205	183
<i>N.controls-all</i>	4,115	4,115	4,115	4,115	4,648	4,648	4,648	4,648
<i>N.controls-matched</i>	1,224	1,122	1,224	1,123	1,497	1,388	1,497	1,388
Capitals Included?	Y	N	Y	N	Y	N	Y	N

Note: Standard errors in parentheses; *90% confidence level, **95% confidence level, ***99% confidence level.

A.23 Empirical Examination (Case 2) Supplement: Robustness of Findings for Brazil to Migration Patterns

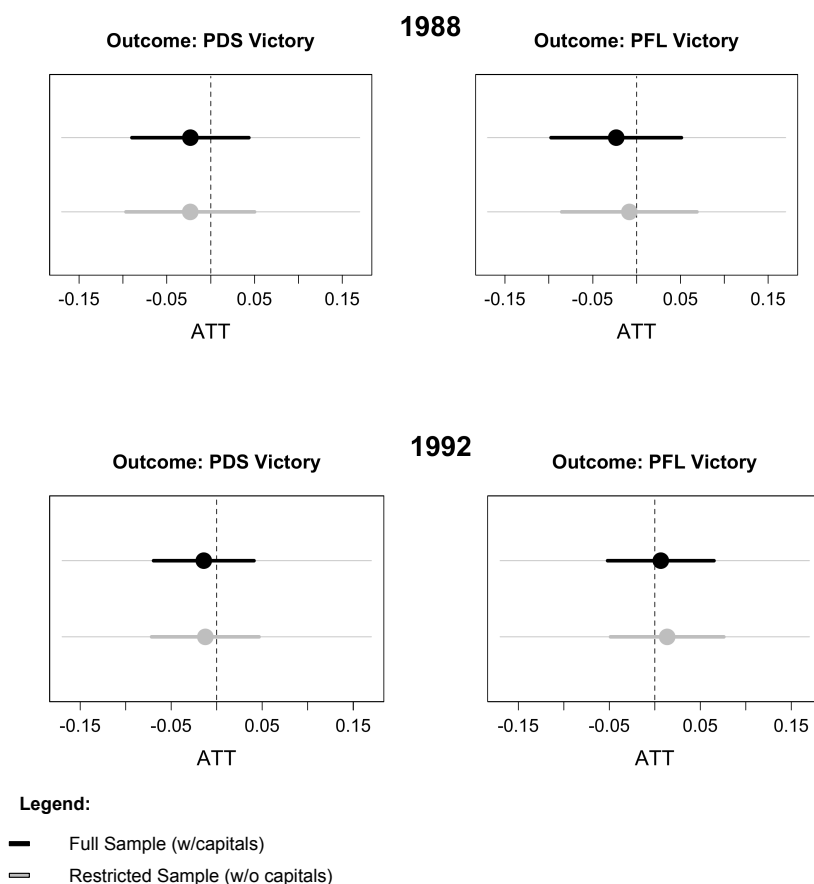
Here we present a reanalysis of our findings for the Brazilian case when the municipalities with the highest levels of migration are excluded from our datasets. Utilizing the Brazilian national census of 2000, we calculated the percentage of the population in each municipality that was resident in the corresponding state for less than ten consecutive years. This provides a measure of how mobile the population in a given municipality is. We then excluded from our datasets the municipalities for which said percentage was greater than or equal to 10%. This resulted in the removal of 286 observations from our 1988 dataset and 396 from our 1992 dataset. [Figure A11](#) and [Figure A12](#) show the results from our linear probability model and coarsened exact matching analysis, respectively, for the restricted samples. As is clear in the figures, our estimates of the (close to zero) effect of intervention are virtually unchanged by excluding municipalities with high migration.

Figure A11: Linear Probability Model of Impact of Intervention - High Migration Municipalities Excluded



Note: Fixed effects for state included in all regressions.

Figure A12: Coarsened Exact Matching Estimates of Impact of Intervention - High Migration Municipalities Excluded



Note: Details of coarsening procedure described in main text.

A.24 Empirical Examination (Case 2) Supplement: Election of Workers' Party (PT) Mayors as an Alternative Dependent Variable

We recognize that one potential consequence of the Brazilian military government's intervention in municipal self-government may have been to build and strengthen solidarity among opponents of military rule (those whose values were so strongly opposed to authoritarianism that they were unlikely to adapt), thereby generating a "backfire effect." To evaluate the evidence for this proposition, this Appendix section presents the results of our statistical analyses using the election of mayors from the Workers' Party (*Partido dos Trabalhadores*, PT) as the dependent variable. Specifically, our outcome is equal to 1 if the mayor elected in a given municipality belonged to the PT or was elected by a coalition of parties that included the PT; 0 otherwise. The rationale for examining local support for the PT is that this party played a key role in mobilizing support for the democratization process in Brazil, particularly within the state of São Paulo (Keck,

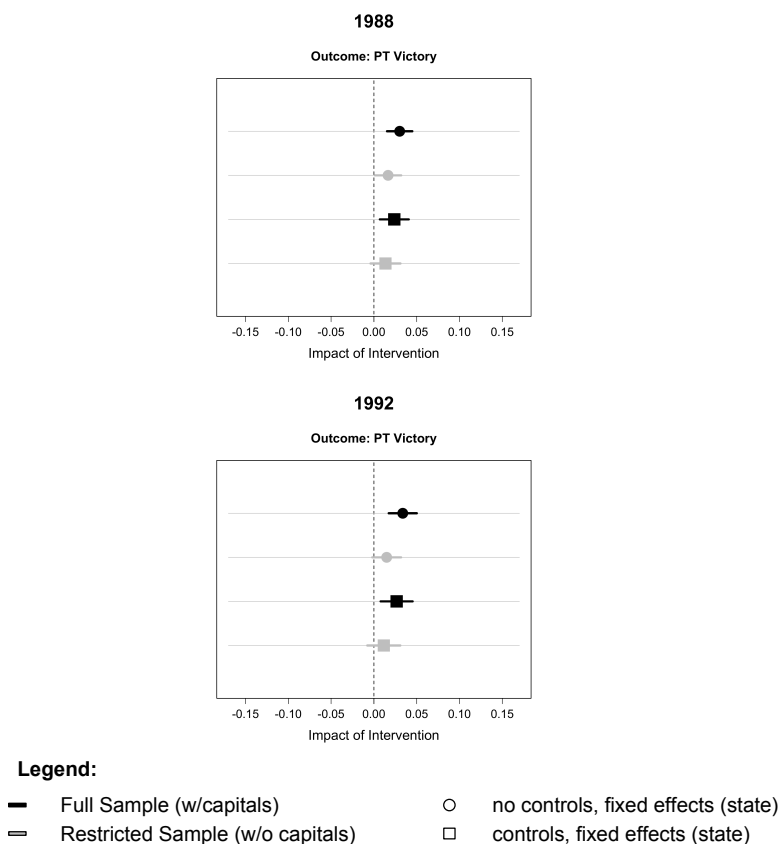
1992). So support for the PT may—to a certain degree—reflect a *rejection* of authoritarian rule.¹³

At the same time, it is important to recognize two limitations associated with using PT support in this manner. First, during the period we consider in the manuscript, the PT was a strongly ideologically left party that was committed to the socialist transformation of Brazilian society. Its move toward the center came later, beginning in the mid- to late 1990s (Hunter, 2010). Thus, many individuals who rejected authoritarian rule but supported free markets may *not* have supported PT candidates. (Similarly, some individuals sympathetic to authoritarianism of the left may have supported the party.) Second, the PT was still a small party with very limited geographical presence outside of industrial centers in the South and in state capitals. It only won, directly or via a coalition, 43 mayoral elections in 1988 and 72 in 1992. (By contrast, the PDS won 568 and 469 mayoral elections in 1988 and 1992, respectively, while the PFL won 1220 and 992 such elections in the same electoral cycles.) The PT's concentration in state capitals and large cities was a legacy of its origins in Brazil's urban-based industrial union movement. Given the extremely unequal distribution of PT presence across socioeconomic contexts, creating a meaningful comparison of substantively similar units by examining the results with state capitals excluded from the analysis is particularly important.

Figure A13 and Figure A14 present our findings for the LPM analysis and CEM analysis, respectively. The results are mixed, but on balance the models relying on the most appropriate comparisons—taking into account all relevant confounders—indicate there was no legacy of support for the PT in municipalities with appointed mayors. The difference in findings ultimately rests on whether state capitals are included or excluded from the analysis. Estimations that include state capitals indicate a positive and statistically significant relationship between appointed mayors and support for the PT, whereas those that exclude state capitals indicate no effect of appointed mayors. It is plausible that the reason why there is contrast in the results is due to unmeasured features of the selection process. For instance, a critical feature of state capitals that always set them apart from other municipalities, even before military dictatorship, was the density of the unionized industrial workforce. Naturally, the PT—as a party of workers—was most likely to build its support base in these areas. But that would have been true irrespective of the presence or absence of appointed mayors during the dictatorship. For these reasons, by dropping extreme outliers like São Paulo, Belo Horizonte, and Porto Alegre, the non-state capital analysis creates more meaningful comparisons between like-units. The fact that there is no statistically significant relationship in this more appropriate analysis leads us to conclude that there is no convincing evidence of a pro-PT legacy effect.

¹³For a more detailed discussion of the linked issues of resistance and resentment, see [subsubsection A.2.3](#).

Figure A13: Linear Probability Model of Impact of Intervention - Election of PT Mayors



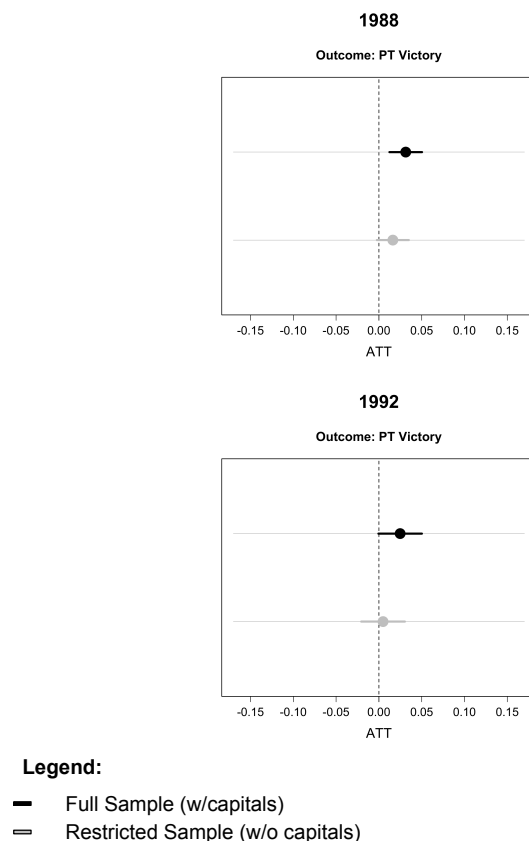
Note: Fixed effects for state included in all regressions.

A.25 Second Case Supplement: Appointed Mayors as Disruptors of Clientelism?

A potential objection to the interpretation of our findings for the Brazilian case may be that the introduction of appointed mayors could have disrupted clientelistic networks, which, in turn, might have weakened the capacity of ASPs to mobilize votes. While we acknowledge that the imposition of appointed mayors had the potential to weaken local political families and machines—which often were key actors involved in clientelism—it is equally important to recognize that the imposition of appointed mayors may also have produced new and comparatively powerful clientelist networks. Recall that appointed mayors were put in place by governors (who were themselves allies of the military government) in consultation with the president (headed by a general of a branch of the armed forces). This implies that any appointed mayor was likely closely aligned with the governor and president, and may have also been personally connected to one or both. Moreover, the finances of municipalities during this period were highly dependent on transfers from the federal and state governments (de Andrade, 2004). Consequently, the scope for favoritism in the provision of resources to municipalities was substantial.¹⁴ Given the inherent affinity between appointed mayors and their municipal funding sources, appointed mayors had the opportunity to build up particularly formidable clientelist machines.

¹⁴This favoritism is evident in the fact that “mayors from the PMDB encountered greater difficulty obtaining the resources their municipalities lacked than did those from the government party, ARENA” (de Andrade, 2004, 212, author translation).

Figure A14: Coarsened Exact Matching Estimates of Impact of Intervention - Election of PT Mayors



Note: Details of coarsening procedure described in main text.

A good example of these dynamics is offered by the political career of Paulo Maluf, the failed PDS candidate for president in 1985. Maluf began his political career as the appointed mayor of São Paulo (1969–1971), a position he obtained due to his personal friendship with then President General Artur da Costa e Silva (Puls, 2000). During his tenure, he was showered with funding from the federal government, allowing him to inaugurate a large number of major construction projects, ranging from roads to bridges and new schools. (The most noteworthy was the “President Costa e Silva Expressway,” an elevated highway passing through the center of the city.) This experience allowed Maluf to construct an effective municipal political machine, one that only became more powerful over time given his subsequent appointments as state Secretary of Transportation (1971–1975) and governor of the state of São Paulo (1979–1982). When he ran for federal deputy in 1982, he received the most votes of any federal deputy in Brazilian history at that time. In 1992, he was popularly elected as mayor of São Paulo running on the PDS label. After his term ended, he was able to shepherd his protégé and handpicked successor, Celso Pitta, into the mayor’s office.

Although the precise circumstances of the Maluf political machine are likely exceptional—as, according to our data, appointed mayors are *not* systematically associated with greater PDS support overall—it illustrates the point that ASPs may have been as likely to benefit from clientelism under appointed mayors as to suffer because of its disruption. Since the appointed mayors-clientelism channel cuts both ways—potentially undermining traditional political families that would have otherwise mobilized voters for ASPs, but also deepening the resource base for clientelism under ASP-affiliated mayors—we suspect that its overall effect in terms of ASP support is probably a wash. For this reason, we do not suspect that our null findings for the Brazilian case can be attributed primarily to potential disruptions in clientelist networks.

A.26 Meta-Analysis Supplement: Splitting Repression into Two Dimensions

In our theoretical framework we suggest that two aspects of imperial or authoritarian rule have the potential to shape the behavior of affected populations with a sufficient length of exposure: (1) denials of meaningful opportunities for political participation and (2) the violent suppression of anti-regime ideas and actions.

From our perspective, there are strong theoretical reasons to believe that both aspects of imperial or authoritarian interventions create strong incentives for shifts in behavior that could ultimately lead to legacies. Since we view both aspects as components of the broader concept of repression, we have decided to merge both binary variables into a single dimension in our original meta-analysis. As a result, the overall variable of repression can take on the three values 0, 0.5, and 1.

While grounded in our theory, merging these dimensions represents a specific empirical choice. Alternatively, we can split the variable into two separate dimensions and check how our results are affected by this choice, which is what we do in this extension of our meta-analysis.

Specifically, we code all cases from the meta-analysis along two new dimensions, namely *repression dimension 1: denial of participation* and *repression dimension 2: violent suppression*. In most cases, we assign either the values 0 or 1. The coding is based on the same assessment previously used in the meta-analysis. This represents a necessary simplification when dealing with a large number of diverse historical episodes. For the purpose of simplification, we primarily rely on this binary distinction. In addition, with respect to contributions that cover multiple countries or contexts at the same time, we sometimes use the value 0.5 to represent an average across the considered cases.

The results of our regressions are contained in Table A18. They suggest that the denial of meaningful opportunities for political participation is the dimension that has a more profound potential to generate a legacy as it is significant in models 1 and 2. However, as theoretically expected, the interaction term of violence and generations exhibits statistical significance. Moreover, the stronger baseline results regarding the impact of denial of participatory mechanisms could also reflect the fact that our focus in the meta-analysis was on outcomes that are meaningfully related to or directly reflecting support for or the strength of democracy. It would make sense that with such a conceptual focus, the denial of meaningful opportunities for participation would turn out to be the more relevant dimension.

Table A18: Meta-Analysis Extended Results: Splitting Repression into Two Dimensions

	<i>Dependent variable:</i>			
	Relative Effect Size			
	(1)	(2)	(3)	(4)
Repression Dimension 1: Denial of Participation	0.986** (0.475)	0.933** (0.449)		
Repression Dimension 2: Violent Suppression	0.186 (0.222)	0.245 (0.212)		
Generations		0.231** (0.103)		
Repression Dimension 1 * Generations			0.311*** (0.095)	
Repression Dimension 2 * Generations				0.290*** (0.097)
Constant	0.443 (0.445)	0.132 (0.442)	1.066*** (0.161)	1.229*** (0.117)
Observations	36	36	36	36
R ²	0.147	0.263	0.238	0.206
Adjusted R ²	0.095	0.193	0.215	0.183
F Statistic	2.844*	3.798**	10.606***	8.843***
Note: OLS		*p<0.1; **p<0.05; ***p<0.01		

In addition, we need to highlight one more time that the relatively small number of 36 observations imposes severe power limitations on any statistical analysis. For this reason, we also refrain from displaying

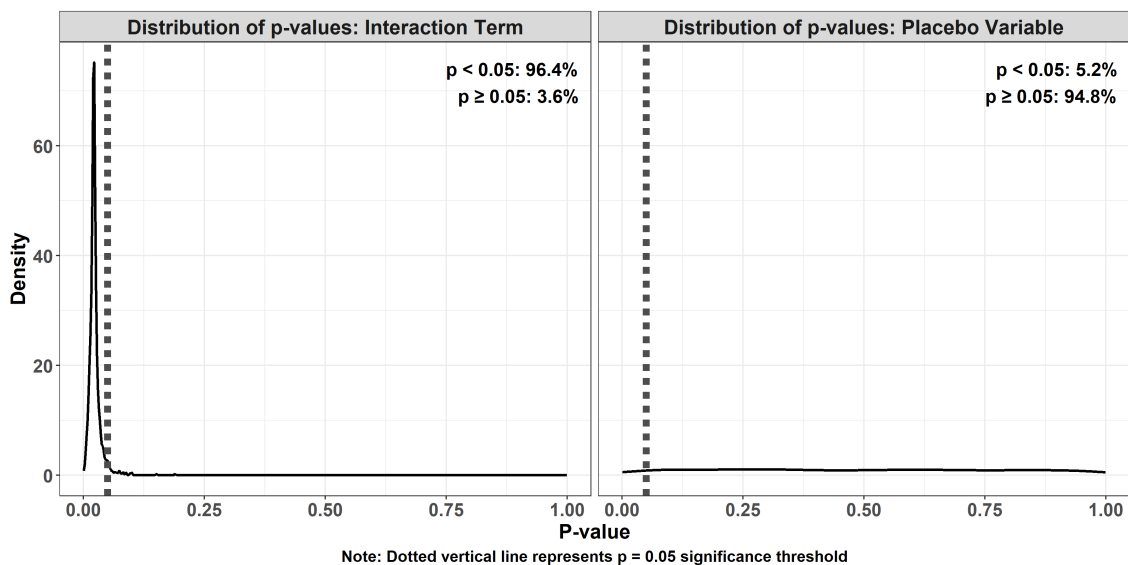
results that include more variables, as this number of observations simply does not allow us to meaningfully test a large number of distinct variables simultaneously. In fact, with 4 possible values on the generations variable, by additionally introducing 2 separate dimensions of repression that can take on the values 0, 0.5, and 1, we already generate a total of $4 \times 3 \times 3 = 36$ possible combinations of values across all independent variables, which simply puts too many demands on a dataset with only 36 observations. For this reason, we refrain from displaying the results of additional combinations (that do not generate significant results).

A.27 Meta-Analysis Supplement: Introducing a Placebo Variable

In our meta-analysis we primarily rely on two key explanatory variables, namely *repression* and *generations*, as well as their interaction. In a small sample of just 36 observations, it may be possible that the results that we have obtained are merely an expression of statistical error. We are concerned about this possibility and introduce a placebo test to answer two questions: (1) How much does the introduction of an additional variable affect the primary result of our meta-analysis, namely that the interaction term exhibits high levels of statistical significance? (2) How likely is it that any random discrete variable that is uniformly distributed over three values (identical to our key factors) exhibits statistical significance in a relatively small sample?

To answer these questions, we create a discrete, uniformly distributed variable that can take on the values 0, 1, and 2, mimicking our two key explanatory attributes. Then we run simulations that represent 1000 random draws from it. Each time, we include it in our primary model (M5 in the main body of the study) and re-estimate the results. While this does not represent a traditional placebo analysis in which we consider an alternative treatment that is closely conceptually related, the analysis could deliver further insights, especially regarding (1) the robustness of our findings to the inclusion of further variables and (2) the difficulty of obtaining significant results on a factor we know to be statistically unrelated to the outcome.¹⁵

Figure A15: Distribution of p-values in Regressions (M5) with a Placebo Variable



Note: Plot shows the distribution of p-values in 1000 regressions/simulations that include a placebo variable, each drawn at random. Left panel shows p-values for the interaction term (*Generations* × *Repression*); right panel shows p-values for the placebo variable.

¹⁵Given the challenges that arise from merely coding our primary treatment variables and given the large number of different cases and countries in our sample, the identification of a placebo treatment that is both conceptually similar and for which data can be obtained was infeasible. This is why we opted for an analysis based on a true random placebo that we know to be unrelated to the outcomes by construction.

The results of the analysis are contained in [Figure A15](#). They reveal two critical patterns: First, in the vast majority of cases (96.4%), the introduction of an additional variable does not change the p-value of the interaction term coefficient to a level above 0.05. Second, the uniformly distributed discrete placebo variable only exhibits a significance level of $p < 0.05$ in 5.2% of the cases, which is in line with statistical predictions about how often we would obtain a Type I error for an unrelated variable with the set threshold (5%).¹⁶

In sum, these findings strongly suggest that our key result is unaffected by the introduction of a placebo variable and that this placebo variable (which mimics the characteristics of our key explanatory variables) only exhibits significance in a small number of cases, substantially bolstering our confidence in the original results.

¹⁶We use $\alpha < 0.05$ as an orientation in this figure because that is the level of significance reached by M5 in the main study. In general, given the small sample size, we still believe that $\alpha < 0.1$ is an appropriate overall threshold.

References

- Alesina, Alberto and Nicola Fuchs-Schündeln. 2007. “Goodbye Lenin (or not?): The effect of communism on people’s preferences.” *American Economic Review* 97(4):1507–1528.
- Ash, Timothy Garton. 1991. *The Polish Revolution: Solidarity*. Granta Books.
- Ayllón, Sara, Ángel Alsina and Jordi Colomer. 2019. “Teachers’ involvement and students’ self-efficacy: Keys to achievement in higher education.” *PloS ONE* 14(5):e0216865.
- Berning, Carl C. and Conrad Ziller. 2017. “Social trust and radical right-wing populist party preferences.” *Acta Politica* 52(2):198–217.
- Cantoni, Davide, Yuyu Chen, David Y. Yang, Noam Yuchtman and Y. Jane Zhang. 2017. “Curriculum and ideology.” *Journal of Political Economy* 125(2):338–392.
- Castanho Silva, Bruno, Federico Vegetti and Levente Littvay. 2017. “The elite is up to something: Exploring the relation between populism and belief in conspiracy theories.” *Swiss Political Science Review* 23(4):423–443.
- Charasz, Paweł. 2021. “From Feudalism to Populism: Evidence from Poland.” *Working Paper*.
- Charnysh, Volha. 2017. “The rise of Poland’s far right: How extremism is going mainstream.” *Foreign Affairs*.
- Charnysh, Volha. 2019. “Diversity, institutions, and economic outcomes: Post-WWII displacement in Poland.” *American Political Science Review* 113(2):423–441.
- Charnysh, Volha and Leonid Peisakhin. 2022. “The role of communities in the transmission of political values: Evidence from forced population transfers.” *British Journal of Political Science* 52(1):238–258.
- Coffé, Hilde, Bruno Heyndels and Jan Vermeir. 2007. “Fertile grounds for extreme right-wing parties: Explaining the Vlaams Blok’s electoral success.” *Electoral Studies* 26(1):142–155.
- Dal Bó, Pedro, Andrew Foster and Louis Putterman. 2010. “Institutions and behavior: Experimental evidence on the effects of democracy.” *American Economic Review* 100(5):2205–29.
- Davies, Norman. 2005. *God’s Playground A History of Poland: Volume II: 1795 to the Present*. Columbia University Press.
- de Andrade, Luis. 2004. O município na política brasileira: revisitando coronelismo, enxada e voto. In *Sistema político brasileiro: uma introdução*, ed. Lúcia Avelar and Antônio Octávio Cintra. Fundação Editora da UNESP pp. 205–218.
- Deak, John. 2015. *Forging a Multinational State: State Making in Imperial Austria from the Enlightenment to the First World War*. Stanford University Press.
- Doyle, David. 2011. “The legitimacy of political institutions: Explaining contemporary populism in Latin America.” *Comparative Political Studies* 44(11):1447–1473.
- Drinóczi, Tímea and Agnieszka Bień-Kacała. 2019. “Illiberal constitutionalism: The case of Hungary and Poland.” *German Law Journal* 20(8):1140–1166.
- Ferraz, Claudio, Frederico Finan and Monica Martinez-Bravo. 2020. “Political power, elite control, and long-run development: Evidence from Brazil.” *National Bureau of Economic Research Working Paper No. w27456*.
- Gelman, Andrew and Ian Pardoe. 2007. “Average predictive comparisons for models with nonlinearity, interactions, and variance components.” *Sociological Methodology* 37(1):23–51.
- Grabowski, Lorie J. Schabo, Kathleen Thiede Call and Jeylan T. Mortimer. 2001. “Global and economic self-efficacy in the educational attainment process.” *Social Psychology Quarterly* 64(2):164–179.
- Grossman, Guy and Delia Baldassarri. 2012. “The impact of elections on cooperation: Evidence from a lab-in-the-field experiment in Uganda.” *American Journal of Political Science*

56(4):964–985.

- Grzymala-Busse, Anna M. 2002. *Redeeming the communist past: The regeneration of communist parties in East Central Europe*. Cambridge University Press.
- Guiso, Luigi, Paola Sapienza and Luigi Zingales. 2016. “Long-term persistence.” *Journal of the European Economic Association* 14(6):1401–1436.
- Haas, Nicholas, Mazen Hassan and Rebecca Morton. 2020. “Negative campaigns, interpersonal trust, and prosocial behavior: The mediating effect of democratic experience.” *Electoral Studies* 63:102087.
- Haggard, Stephan and Robert R. Kaufman. 1997. “The political economy of democratic transitions.” *Comparative Politics* 29(3):263–283.
- Hawkins, Kirk A. 2009. “Is Chávez populist? Measuring populist discourse in comparative perspective.” *Comparative Political Studies* 42(8):1040–1067.
- Hoensch, Jörg Konrad. 1990. *Geschichte Polens [History of Poland]*. Verlag Eugen Ulmer.
- Hooghe, Marc and Ruth Dassonneville. 2018. “A spiral of distrust: A panel study on the relation between political distrust and protest voting in Belgium.” *Government and Opposition* 53(1):104–130.
- Hooghe, Marc, Sofie Marien and Teun Pauwels. 2011. “Where do distrusting voters turn if there is no viable exit or voice option? The impact of political trust on electoral behaviour in the Belgian regional elections of June 2009.” *Government and Opposition* 46(2):245–273.
- Howe, Philip J., Edina Szöcsik and Christina I. Zuber. 2022. “Nationalism, class, and status: How nationalists use policy offers and group appeals to attract a new electorate.” *Comparative Political Studies* 55(5):832–868.
- Hryniewicz, Janusz. 1996. Czynniki rozwoju regionalnego [Factors of regional development]. In *Oblicza polskich regionów [Faces of the regions of Poland]*, ed. B. Jałowiecki. EUROREG - M. Swianiewicz pp. 89–129.
- Hunter, Wendy. 2010. *The Transformation of the Workers’ Party in Brazil, 1989–2009*. Cambridge University Press.
- Jennings, M. Kent, Laura Stoker and Jake Bowers. 2009. “Politics across generations: Family transmission reexamined.” *Journal of Politics* 71(3):782–799.
- Jennings, M. Kent and Richard G. Niemi. 1968. “The transmission of political values from parent to child.” *American Political Science Review* 62(1):169–184.
- Judson, Pieter M. 2016. *The Habsburg Empire: A New History*. The Belknap Press of Harvard University Press.
- Kamei, Kenju. 2016. “Democracy and resilient pro-social behavioral change: an experimental study.” *Social Choice and Welfare* 47(2):359–378.
- Keck, Margaret. 1992. *The workers’ party and democratization in Brazil*. Yale University Press.
- Keefer, Philip, Carlos Scartascini and Razvan Vlaicu. 2019. “Social Trust and Electoral Populism: Explaining the Quality of Government.” Available at SSRN 3460914, <https://dx.doi.org/10.2139/ssrn.3460914>.
- Kitschelt, Herbert and Matthew Singer. 2018. Linkage strategies of authoritarian successor parties. In *Life after Dictatorship: Authoritarian Successor Parties Worldwide*, ed. James Loxton and Scott Mainwaring. Cambridge University Press pp. 53–83.
- Kraaykamp, Gerbert and Paul Nieuwbeerta. 2000. “Parental background and lifestyle differentiation in Eastern Europe: Social, political, and cultural intergenerational transmission in five former socialist societies.” *Social Science Research* 29(1):92–122.
- Kubow, Magdalena. 2013. “The solidarity movement in Poland: Its history and meaning in collective memory.” *The Polish Review* 58(2):3–14.

- Kuran, Timur. 1987. "Preference falsification, policy continuity and collective conservatism." *The Economic Journal* 97(387):642–665.
- Kuran, Timur. 1991. "Now out of never: The element of surprise in the East European revolution of 1989." *World Politics* 44(1):7–48.
- Lambais, Guilherme. 2025. "Cultural Heterogeneity, Intergenerational Transmission, and Local Development: Evidence from Quilombos in Brazil." *Working Paper, Lusíada University of Lisbon*.
- Loxton, James. 2018. Introduction: Authoritarian Successor Parties Worldwide. In *Life after Dictatorship: Authoritarian Successor Parties Worldwide*, ed. James Loxton and Scott Mainwaring. Cambridge University Press.
- Lukowski, Jerzy and Hubert Zawadzki. 2006. *A concise history of Poland*. Cambridge University Press.
- Markowski, Radoslaw. 2006. "The Polish elections of 2005: Pure chaos or a restructuring of the party system?" *West European Politics* 29(4):814–832.
- Markowski, Radoslaw. 2019. "Creating authoritarian clientelism: Poland after 2015." *Hague Journal on the Rule of Law* 11(1):111–132.
- Markussen, Thomas, Louis Putterman and Jean-Robert Tyran. 2014. "Self-organization for collective action: An experimental study of voting on sanction regimes." *Review of Economic Studies* 81(1):301–324.
- Merkley, Eric. 2020. "Anti-intellectualism, populism, and motivated resistance to expert consensus." *Public Opinion Quarterly* 84(1):24–48.
- Millard, Frances. 1994. "The shaping of the Polish party system, 1989-93." *East European Politics and Societies* 8(03):467–494.
- Miller, Michael K. 2021. "Don't Call It a Comeback: Autocratic Ruling Parties After Democratization." *British Journal of Political Science* 51(2):559–583.
- Min, Joohong, Merrill Silverstein and Jessica P. Lendon. 2012. "Intergenerational transmission of values over the family life course." *Advances in Life Course Research* 17(3):112–120.
- Mudde, Cass. 2007. *Populist radical right parties in Europe*. Cambridge University Press.
- Myers, David J. and Kirk A. Hawkins. 2011. "Venezuela's Chavismo and Populism in Comparative Perspective." *Perspectives on Politics* 9(3):741.
- Nalepa, Monika. 2021. "Transitional justice and authoritarian backsliding." *Constitutional Political Economy* 32(3):278–300.
- Neundorf, Anja and Grigore Pop-Eleches. 2020. "Dictators and Their Subjects: Authoritarian Attitudinal Effects and Legacies." *Comparative Political Studies* 53(12):1839–1860.
- Pop-Eleches, Grigore and Joshua A. Tucker. 2017. *Communism's Shadow: Historical Legacies and Contemporary Political Attitudes*. Princeton University Press.
- Pop-Eleches, Grigore and Joshua A. Tucker. 2020. "Communist legacies and left-authoritarianism." *Comparative Political Studies* 53(12):1861–1889.
- Power, Timothy J. 2000. *Political right in postauthoritarian Brazil: elites, institutions, and democratization*. Penn State University Press.
- Power, Timothy J. 2018. The Contrasting Trajectories of Brazil's Two Authoritarian Successor Parties. In *Life after dictatorship: authoritarian successor parties worldwide*, ed. James Loxton and Scott Mainwaring. Cambridge University Press pp. 229–253.
- Prazmowska, Anita J. 2011. *A history of Poland*. Palgrave Macmillan.
- Puls, Mauricio. 2000. *O Malufismo*. Publifolha.
- Putnam, Robert D., Robert Leonardi and Raffaella Nanetti. 1993. *Making democracy work: Civic traditions in modern Italy*. Princeton University Press.

- Raphael, Lutz. 2000. *Recht und Ordnung: Herrschaft durch Verwaltung im 19. Jahrhundert [Law and Order: Rule through Administration in the 19th Century]*. Fischer.
- Read, Barbara. 2018. "Truth, masculinity and the anti-elitist backlash against the university in the age of Trump." *Teaching in Higher Education* 23(5):593–605.
- Ribeiro, Ricardo Luiz Mendes. n.d. "O surgimento do PFL e a redemocratização: saída pelo centro e desembarque perfeito." *Working Paper, University of São Paulo*.
- Rueß, Stefanie, Gerald Schneider and Jan P. Vogler. 2025. "Illiberal Norms, Media Reporting, and Bureaucratic Discrimination: Evidence from State-Citizen Interactions in Germany." *Comparative Political Studies*.
- Rupnik, Jacques. 2007. "Is East-Central Europe backsliding? From democracy fatigue to populist backlash." *Journal of Democracy* 18(4):17–25.
- Sadurski, Wojciech. 2019. *Poland's Constitutional Breakdown*. Oxford University Press.
- Santoro, Flávia Rosa, Leonardo S. Chaves and Ulysses Paulino Albuquerque. 2020. "Evolutionary aspects that guide the cultural transmission pathways in a local medical system in Northeast Brazil." *Heliyon* 6(6).
- Sata, Robert and Ireneusz Pawel Karolewski. 2020. "Caesarean politics in Hungary and Poland." *East European Politics* 36(2):206–225.
- Sausgruber, Rupert, Axel Sonntag and Jean-Robert Tyran. 2021. "Disincentives from redistribution: Evidence on a dividend of democracy." *European Economic Review* 136:103749.
- Schunk, Dale H. 1989. "Self-efficacy and achievement behaviors." *Educational Psychology Review* 1:173–208.
- Serra, Gilles. 2013. "Demise and resurrection of a dominant party: understanding the PRI's comeback in Mexico." *Journal of Politics in Latin America* 5(3):133–154.
- Skidmore, Thomas E. 1988. *The politics of military rule in Brazil, 1964–1985*. Oxford University Press.
- Sullivan, Daniel, Mark J. Landau and Zachary K. Rothschild. 2010. "An existential function of enemyship: Evidence that people attribute influence to personal and political enemies to compensate for threats to control." *Journal of Personality and Social Psychology* 98(3):434.
- Tribunal Superior Eleitoral (TSE). 1979a. *Boletim Eleitoral No. 339 (Outubro)*. TSE.
- Tribunal Superior Eleitoral (TSE). 1979b. *Boletim Eleitoral No. 340 (Novembro)*. TSE.
- Tribunal Superior Eleitoral (TSE). 1988. *Dados estatísticos: eleições federais, estaduais, e municipais realizadas em 1982*. TSE.
- Valentim, Vicente. 2024. *The normalization of the radical right: A norms theory of political supply and demand*. Oxford University Press.
- Vogler, Jan P. 2019. "Imperial Rule, the Imposition of Bureaucratic Institutions, and their Long-Term Legacies." *World Politics* 71(4):806–863.
- Vushko, Iryna. 2015. *The Politics of Cultural Retreat: Imperial Bureaucracy in Austrian Galicia, 1772–1867*. Yale University Press.
- Walęcka-Matyja, Katarzyna Kamila. 2022. "Family, material and worldview values and the process of intergenerational transmission." *Kwartalnik Naukowy Fides et Ratio* 49(1):53–66.
- Wandycz, Piotr S. 1975. *The lands of partitioned Poland, 1795–1918*. University of Washington Press.
- Weeks, Theodore R. 1994. "Defining Us and Them: Poles and Russians in the "Western Provinces," 1863–1914." *Slavic Review* 53(1):26–40.
- Whitson, Jennifer A. and Adam D. Galinsky. 2008. "Lacking control increases illusory pattern perception." *Science* 322(5898):115–117.
- Williams, Chelsea D., Diamond Y. Bravo, Adriana J. Umaña-Taylor, Kimberly A. Updegraff, Lau-

dan B. Jahromi, Stefanie Martinez-Fuentes and María de Jesus Elias. 2020. “Intergenerational transmission of cultural socialization and effects on young children’s developmental competencies among Mexican-origin families.” *Developmental Psychology* 56(2):199–206.