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)

May 2, 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. Contrariwise, transient episodes characterized by limited repressiveness are unlikely to produce legacies. Given our theory's broad character, 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 certain cities. In addition, we conduct a meta-analysis based on 23 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, and Jason Wittenberg. We also thank participants and discussants of presentations at the University of Copenhagen, Stanford 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, 2023; 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; Charasz, 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, 2022*a*). 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

[1]Related 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).

[2]Comparable 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 (2022*b*).

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

Yet when an externally imposed change in regime type constitutes a critical juncture, and when it does not, is not at all obvious ex ante. 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 theoretical and empirical 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] Contrariwise, episodes of suppression that are transient and characterized by narrowly 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 a variety of 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 externally 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 narrower repression implemented by the military regime failed to generate lasting political preferences in favor of illiberal authoritarian successor parties. 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.

As our theoretical perspective is not limited to any single world region or just one type 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 23 existing studies (with two studies being counted twice because they explicitly differentiate between at least two kinds of interventions) and subsequently

[3]In 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.

analyze empirical regularities in these data. We find strong additional evidence in favor of our theory, especially regarding the interactive character of intervention length and levels of repressiveness, that supports the wide generalizability of our claims.

The contribution of this study to the literature is manifold. In elucidating why external interventions may or may not produce legacies, we deepen the literature on "historical persistence," a body of scholarship that locates its causal variables in the (often distant) past and its outcomes in the present day or more recent past.[4] Extant studies in this literature typically exploit spatial variation within a single country or region to demonstrate that a particular historical event has had a lasting legacy. While this research strategy illuminates historical dynamics in particular cases, it provides limited insight into when and why legacies take hold, since published persistence 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) which 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 the establishment of legacies, one that combines a comparative

[4]Comprehensive reviews of this literature are provided in Abad and Maurer (2021), Acharya, Blackwell and Sen (2023), Cirone and Pepinsky (2022), Simpser, Slater and Wittenberg (2018), and Voth (2021). For an overview that focuses on colonial legacies in particular, see De Juan and Pierskalla (2017).

[5]In taking this step, we respond to mounting disquiet among historically oriented social scientists about the absence of attention given 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).

case study approach at the macro-level (leveraging cross-case variation) with design-based causal inference at the micro-level (leveraging within-case variation). This fine-grained evidence is further complemented by an extensive multi-country analysis that covers cases in all world regions. 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 research on legacies (Capoccia, 2015; Capoccia and Kelemen, 2007), while at the same time not only subjecting assessments of the existence of legacies to contemporary standards for causal inference (Dunning, 2012; Morgan and Winship, 2015), but also verifying the broader generalizability of the claims. To the best of our knowledge, our paper's use of this specific template represents one of the very first explicit attempts to foster knowledge accumulation in historical political economy (HPE) by integrating design-based inference with the comparative method (Callis, Dunning and Tuñón, 2024).

We also seek to enrich scholarship on historical junctures. Analytically, we consider 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* critical junctures.[6] The novelty of

[6]Since duration is a variable in our analysis, we self-consciously depart from treatments of historical junctures that define such episodes as taking place during narrow time periods or generally being short in length relative to the legacies they (may) produce. Commitment to such a definition would imply that the

our approach lies in the combination of a theoretical framework that carefully delineates expectations about the characteristics of junctures that do or do not produce legacies and an empirical framework that uses a principled transnational comparison in combination with 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 theoretical framework, differentiating between types of interventions that are more or less likely to generate lasting legacies for political behavior. 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 around the world that is based on 23 existing studies. In the conclusion, we summarize our insights and explain how our approach can be applied to the genesis of legacies in other contexts.

---

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.

[7]On the use of transnational comparisons in comparative politics—which combine subnational and cross-national analyses—see Sellers (2019). For a call for greater rigor in hypothesis testing in historical juncture research, see Waldner (2022).

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 responsible for the intervention. So defined, the suppression of self-government is conceptually 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 scholarship on historical legacies, albeit with debate about the character and direction of relevant effects.

[8]In 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.

Analyses examining contemporary differences across Italian regions suggest that extended experiences with self-government are relevant for outcomes such as civic engagement (Putnam, Leonardi and Nanetti, 1993, 121–162), the belief that one's actions can meaningfully shape life prospects (Guiso, Sapienza and Zingales, 2016), and economic development (Di Liberto and Sideri, 2015). 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), and Vietnam (Dell, Lane and Querubin, 2018). Similarly, scholarship on lived experiences with communism links the suppression of self-government to attitudes hostile to democracy (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 changes in regime type do not inherently foster the development of either democratic or non-democratic

[9]Vice versa, Doucette (2024) finds that positive historical experiences with self-government institutions lead to higher levels of support for democratic parties in the long run.

[10]Some studies link direct colonial rule and/or longer exposure to 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 and/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).

institutions (De Mesquita and Downs, 2006; Downes and Monten, 2013).

These heterogeneous findings beg an important 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 for political behavior 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, the distinction between productive and permissive conditions in prior works on critical junctures could be meaningful. 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 terri-

tory 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 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 skills 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 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

[11] This can be rationalized as a result of a Bayesian learning process, in which individuals have uncertainty about regime stability but (positively) update their beliefs based on the time elapsed under the current system (e.g., Hill, 2017).

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 on 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.

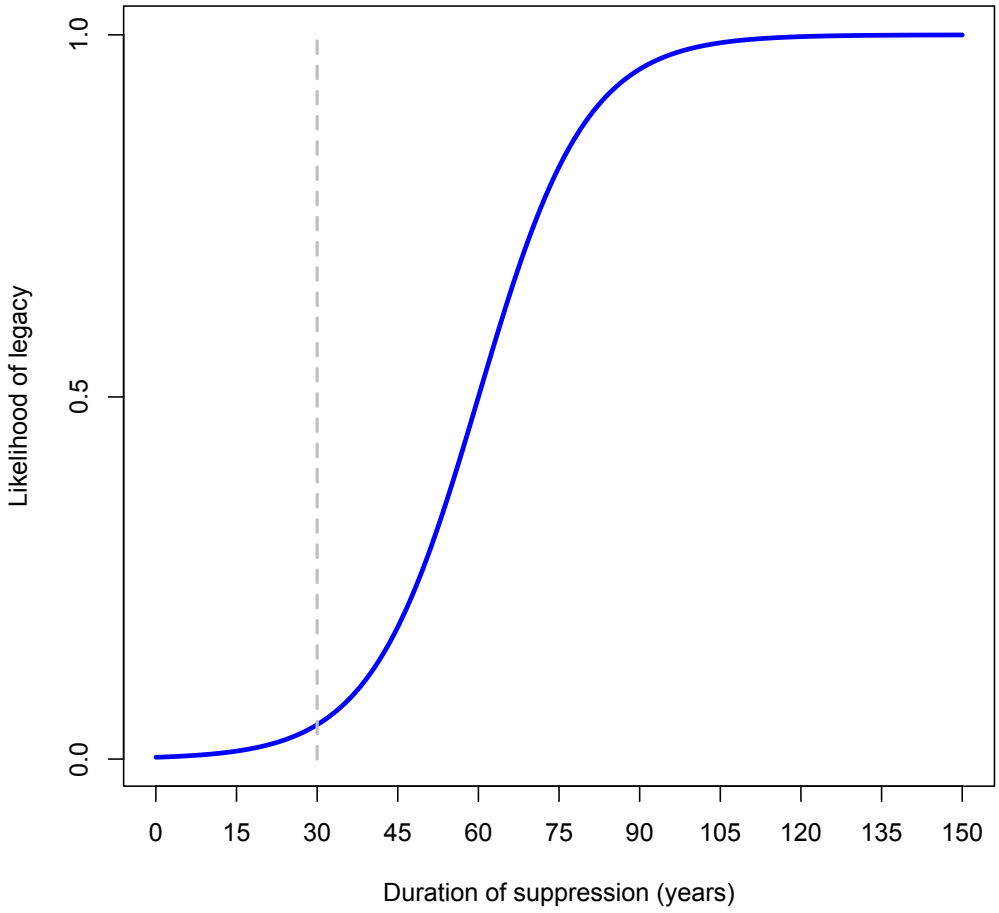
Of course, value change via parental socialization is a slow-moving process. At any given time, it takes place only within households that have children in an impressionable age range—roughly, late childhood through adolescence (Neundorf and Smets, 2017). Multiple waves of children in this range need 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 equal, we expect durable values shifts to be more likely to result from the latter than the former.[12]

[12]In 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

Figure 1 provides a visualization of our expectations about the relationship between the duration of a suppression and the likelihood of a legacy for voting behavior. 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, the likelihood of a legacy increases markedly with duration, as the dynamics of intergenerational value transmission start to kick in. 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.

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.

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



Note: The vertical line at 30 years denotes the threshold for a single generation.

Now consider the scope of repression. This concept can be analytically split into two subcomponents: (1) involvement of (broad parts) of the citizenry in governance processes through the provision of limited channels of formal participation (especially elections of officials, such as members of the legislature); and (2) violent suppression of ideas and actions that are hostile toward the authoritarian regime or imperial power. Episodes of suppression of self-government have varied markedly across time and place in both these aspects of

repression, although cases which feature more expansive opportunities for political participation tend to feature lower levels of violent suppression (and vice versa) (Blakeley, 2012; Corduneanu-Huci and Osa, 2003).

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. 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. 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 suppressions 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 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 towards behavioral adjustment.[13] In short, if an intervention proves to be *both* long-lasting

[13]The same logic implies that *consistency* in the application of repression over time is also important. A long intervention with an erratic application of repression would be unlikely to generate a legacy, since

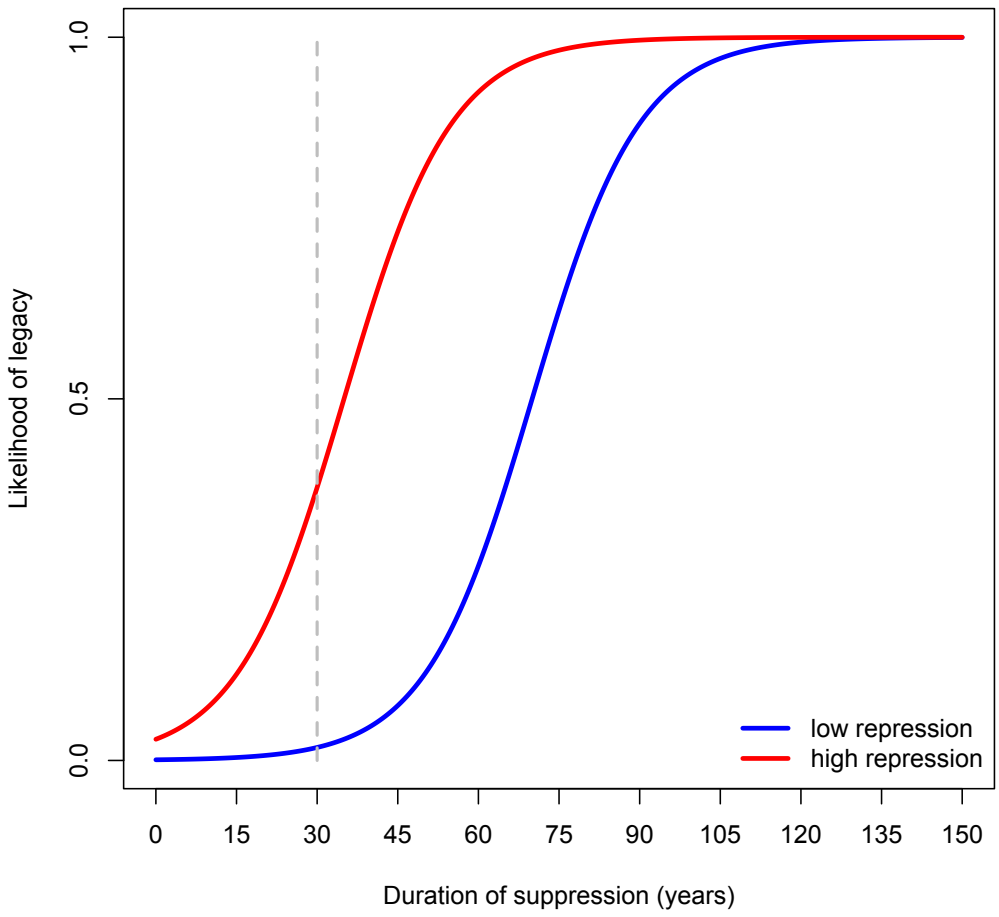
and highly repressive, the likelihood of a permanent legacy increases markedly.[14]

Figure 2 shows how the duration and repressiveness of a suppression of self-government jointly determine the likelihood of a legacy. If repression is low (depicted by the blue line), the likelihood of a legacy starts out low and increases very slowly with the duration of the suppression until the single generation threshold is crossed. If repression is high (depicted by the red line), the likelihood of a legacy starts out higher and increases much more rapidly with the duration of the suppression. Although the curves indicate that a legacy is highly likely for very long suppressions (90+ years, three generations) irrespective of repressiveness, they illustrate how repressiveness critically conditions the impact of duration for suppressions of short or intermediary length.

citizens' expectations would not congeal around the notion that high repression will be the reality for the foreseeable future.

[14]We 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-governance



Note: The vertical line at 30 years denotes the threshold for a single generation.

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 propor-

tion 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 long-term change to the political behavioral equilibrium is achieved. In other words, a legacy has been instantiated.

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

	Limited Repression	Encompassing Repression
Transient Intervention	***Minimal*** potential for long-term legacies	Moderate potential for long-term legacies
Enduring Intervention	Moderate potential for long-term legacies	***Highest*** potential for long-term legacies

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 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 possibly emerge in these cases, but they will typically have

a high "decay rate" (Acharya, Blackwell and Sen, 2023).[15]

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. 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 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

[15]The 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, 2022*a*; 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.

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 if any contestation occurs, control is swiftly restored.

Because compliance with the new set of institutions is a key goal of the external authority, citizens can avoid the harshest forms of violence 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 towards populations that is primarily or exclusively based on ascriptive characteristics. Nor does it extend to contexts where violence is mostly indiscriminate. 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 targeted irrespective of compliance. In our framework, it is the fact that citizens have a viable path to avoid repression that leads to the forms of pro-regime behavioral adjustment we emphasize.[16]

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 to the new institutional framework is the primary type of behavioral change that we would expect to observe. Accordingly, this scope condition allows a focus on private decisions about skill

[16]Contrariwise, 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.

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.

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).

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 three 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).[17] 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., the cases exhibit a theoretically relevant contrast).

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

Third, *within the cases* there is internal spatial variation in the presence and/or nature of suppression that permits one to establish the existence or non-existence of a legacy using the tools of design-based causal inference.

In each of the two cases, we carefully chose the period in which we conducted 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 historical episodes 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 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.[18] Furthermore, in the Appendix (section A.4), we explain our choice of the 2010s as the most

[18] An 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

appropriate time within the post-Cold War period.

Vice versa, 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.

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 Poland and mid-twentieth-century Brazil also had a number of crucial political features in common prior to the onset of the suppressions we study here. Specifically, both polities had decentralized and oligarchical political systems that were driven by family dynasties. Moreover, governance at the subnational level was critically important for the lived experiences of citizens. Central authorities simply could not and did not dictate policy from the capital. Instead, they had to carefully cultivate the support of regional elites to pursue any meaningful political agenda.

In Poland, the system of so-called Nobles' Democracy—"Golden Liberty" (*Złota Wolność*)—was formally aristocratic: the local nobility enjoyed extensive legal rights and opportunities for political participation and deliberation. Although nominally a monarchy, too, kings were *elected by the nobles and highly constrained by institutions* such as the national par-

(Sokołowski, 2013). Thus, direct comparisons of electoral success across different areas are often unavailable.

liament (the *Sejm*) and local assemblies (the *semjiks*).[19] Although peasants were excluded from formal participation in this system, the Nobles' Democracy offered meaningful—albeit abridged—opportunities for participation in two senses. First, 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.[20] Second, the noble class itself was quite extensive, approximately 10 percent of the population in the period preceding partition (Gella, 1998, 13). Considering 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). Strikingly, from the perspective of selectorate theory (De Mesquita, Smith, Siverson and Morrow, 2005)—which puts heavy emphasis on the size of the "selectorate" that has the ability to determine the government—the difference between the Polish and the early American political system is therefore one of degree rather than of kind.[21]

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 of-

[19] On the comparatively high level of political decentralization of Poland in the early modern period, see also Karaman and Pamuk (2013).

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

[21] As we further elaborate in the Appendix (section A.2.2), our theory applies to a broad variety of initial contexts, including those that initially only have relatively low levels of public participation in decision-making processes.

fices 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).[22] The system exhibited cleavages based on urbanization and education, which reflected distinctive socioeconomic circumstances and political traditions in the countryside versus the large cities (Soares, 2001). In rural areas, local and subnational politics was often dominated by political machines headed by powerful political families (Chilcote, 1990; do Rêgo, 2008). The heads of these organizations operated as vote brokers, mobilizing economically dependent voters in exchange for resources allocated by high-ranking politicians (Gingerich, 2019).[23] They also routinely served as mayors, using patronage with the municipal government to consolidate their political machines. The brokers-cum-mayors were central to the political ambitions of governors and legislators, who, in turn, were indispensable for governance at the national level (Abrucio, 1998).

Thus, on the eve of their respective interventions, both Poland and Brazil had political systems that combined some meaningful elements of local self-government with social and legal environments that were hierarchical and exclusionary. These circumstances highlight the comparability of many elements of these cases. To further address potentially remaining concerns about initial conditions driving the results, below we also present a broad meta-analysis of cases with vastly different initial conditions and an extended theoretical discussion

[22]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.

[23]The absence of a fully secret ballot during much of this period limited rural voters' autonomy and facilitated the mobilizational efforts of brokers (Gingerich, 2020).

in the study's Appendix (section A.2.2).

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.[24]

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 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, 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).

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

By contrast, the scope of repression, including the use of violence through the state, was relatively limited under Prussian rule (Davies, 2005, 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 entire period, while Prussia's state had a *Rechtsstaat* character and permitted substantial political participation for several decades.[25]

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

[25]Given 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.10), but as elaborated below, this case is ambiguous in light of our framework.

attitudes in a sustainable fashion.

In terms of our theory outlined in 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.[26] It also utilized violent repression sparingly, committing human rights abuses with significantly lower frequency than the regimes ensconced 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

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

act (AI-2) of 1965, it dissolved the existing system, substituting in its place an officially sanctioned two-party system.[27]

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.[28]

The type of suppression of local self-government that we focus on took place within this larger institutional context. Due to AI-3 and subsequent decrees promulgated by the government, (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 4000) had appointed mayors.

---

[27]The 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.

[28]However, 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.[29]

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 need to 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. The episodes of suppression in both Poland and Brazil share this rare virtue, thereby permitting us to categorize the cases ac-

[29]In 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.

cording to the presence or absence of a legacy with a high degree of confidence.

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 Sprawiedliwosc*, PiS) in mayoral elections in the 2010s.[30] 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

[30] As 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.

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.[31]

As previously discussed, we expect that municipalities in the formerly Russian territory will be more likely to have mayors that 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.10). 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.

[31] A critic of this approach may highlight the circumstance that the PiS generally does not have candidates 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.

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 resettlement, we exclude municipalities that belonged to interwar Germany from the analyses (e.g., Charnysh and Peisakhin, 2022).[32] 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.[33]

[32]See the Appendix (sections A.6 and A.12) for additional discussion on this point.

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

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:

$$y_i = \beta_0 + \beta_1\ Russia_i + \beta_2\ ELV_i + \mathbf{x_i'}\ \boldsymbol{\beta} + f(geographic\ location) + \varepsilon_i \tag{1}$$

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: $f(geographic\ location)$. 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.[34]

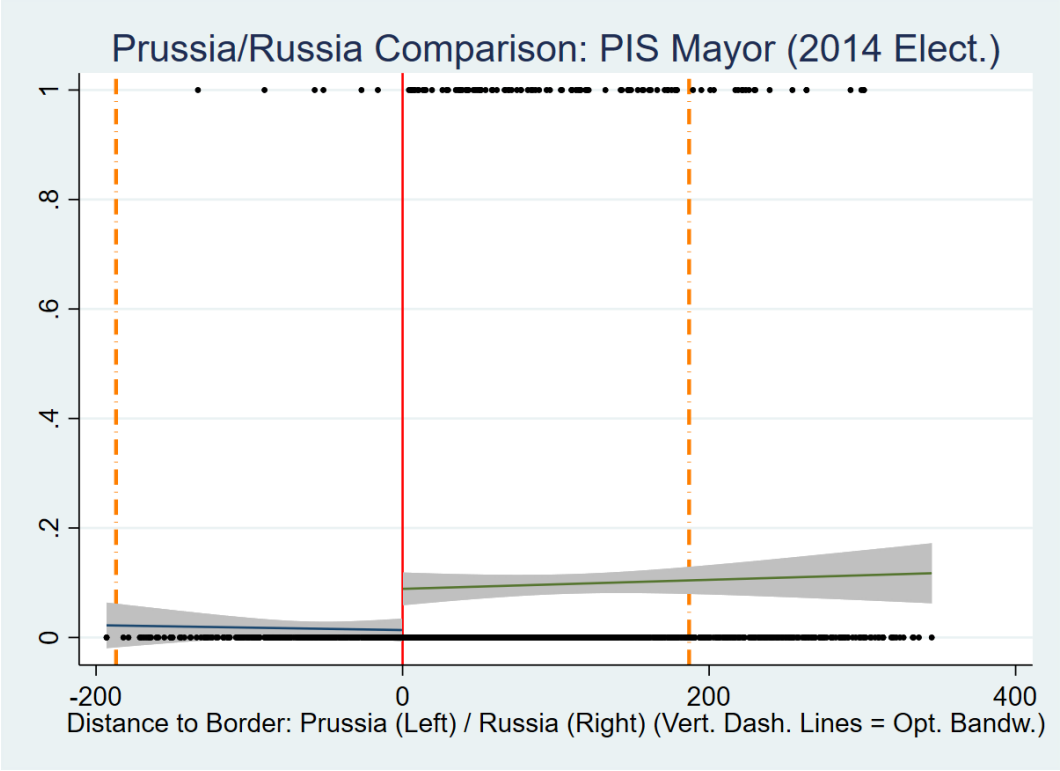
Results

We find consistent evidence that the contrast between more limited repression (Prussian partition) and more encompassing repression (Russian partition) produces long-term political

[34]Details are presented in the Appendix (section A.7).

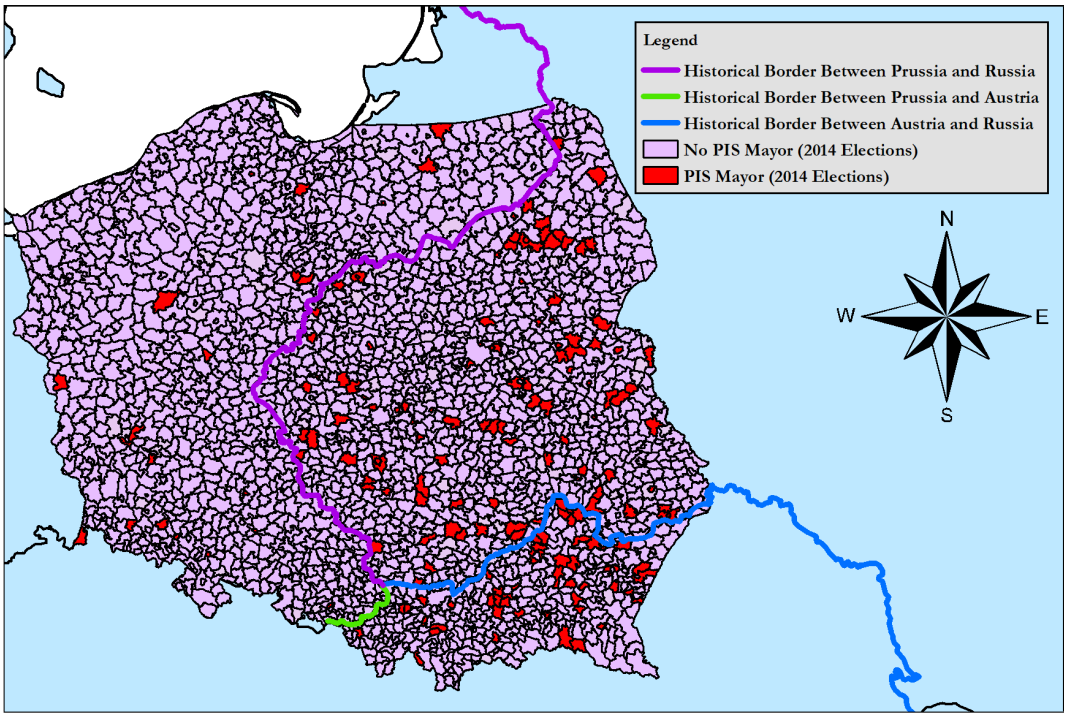
differences in the electoral success the populist-authoritarian PiS. Figure 4 illustrates our GRDD approach. It depicts the discrete increase in the proportion of mayors belonging to the PiS in the 2014 elections as one crosses the geographic boundary between the Prussian and Russian partitions (at $x = 0$). 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.

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 1.

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 1 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.8).

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

Table 1: Local Political Leadership Outcomes (Prussia/Russia Comparison)

	Dependent variable:					
	Mayor PIS	Mayor PIS (Br.)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Br.)	Mayor PIS (2010)
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	0.075***	0.081***	0.056**	0.066*	0.075**	0.029
	(0.026)	(0.027)	(0.023)	(0.035)	(0.036)	(0.031)
Elevation	0.00003	0.00004	0.0002	0.00005	0.0001	0.0002*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. PR-RU	−0.0001	−0.0001	0.0001	−0.0004	−0.0004	0.00001
	(0.0003)	(0.0003)	(0.0003)	(0.001)	(0.001)	(0.001)
Dist. PR-RU Sq.				0.00000	0.00000	0.00000
				(0.00001)	(0.00001)	(0.00001)
Russia*Dist.	0.0001	0.0001	−0.0001	0.001	0.001	0.001
	(0.0003)	(0.0003)	(0.0003)	(0.001)	(0.001)	(0.001)
Russia*Dist. Sq.				−0.00000	−0.00000	−0.00000
				(0.00001)	(0.00001)	(0.00001)
Constant	0.010	0.007	−0.007	−0.0001	−0.002	−0.013
	(0.028)	(0.029)	(0.025)	(0.036)	(0.037)	(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

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.11) that there are significantly fewer mayors with higher education in the formerly Russian partition when compared to the Prussian partition. 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 da Social Democracia Brasileira*, 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).[35] Given previous findings about the legacies of dictatorship-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).[36]

[35]The split led to the PFL leadership allying with the democratic opposition in the 1985 presidental elections. In the Appendix (section A.15), 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.

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

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), 1979*a*; Tribunal Superior Eleitoral (TSE), 1979*b*). The list was then supplemented using the information contained in decree laws dealing with national security areas promulgated after the election (1973–1981), which listed additional municipalities with appointed mayors.[37] 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.[38]

[37]The aforementioned laws were Decree Law Nº 1.272 (May 29, 1973), Decree Law Nº 1.273 (May 29, 1973), Decree Law Nº 1.284 (August 28, 1973), Decree Law Nº 1.316 (March 12, 1974), Decree Law Nº 1.480 (September 9, 1976), Decree Law Nº 1.481 (September 9, 1976), and Complementary Law Nº 41 (December 22, 1981).

[38]The 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).

Covariates

In selecting the covariates, we exploit knowledge about the factors considered by the military government 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 exploit knowledge about the dimensions upon 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 Nº. 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 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.[39]

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.[40]
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).
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

[39]Of 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.

[40]Distances 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 Kelvin S. do Prado (`http://github.com/kelvins/Municipios-Brasileiros`).

section A.16). 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.17). 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 a linear probability regression model 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.[41] Since we recognize that 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

[41]To implement coarsened exact matching (Iacus, King and Porro, 2012), we discretized the distance to the nearest land border into the following categories: $\leq$250km, [250km,500km), [500km,1000km),$\geq$ 1000km. 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.

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 vote patterns for Goulart in the 1960 vice-presidential election. Figure A9 in the Appendix presents the data using box-and-whisker plots. As shown therein, there is no evidence for ideological selection: The distribution of vote shares for Goulart in municipalities that were assigned appointed mayors was nearly identical to that encountered municipalities that were allowed to continue electing

their mayors. This is true examining the full sample, including state capitals, as well as in a restricted sample with state capitals excluded.[42]

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. Moreover, the two parties whose electoral success we consider were strongly linked in the public mind with authoritarian rule.[43] If any legacy of intervention for pro-authoritarian voting had been created, one would expect it to be discernible in these elections.[44]

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

[42]Kolmogorov-Smirnov tests confirmed the similarity of the distributions. P-values for said tests were equal to 0.125 and 0.220 in the full and restricted samples, respectively.

[43]In 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).

[44]As mentioned earlier, from the perspective of our framework, focusing on these elections therefore represents a "hard test" case.

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

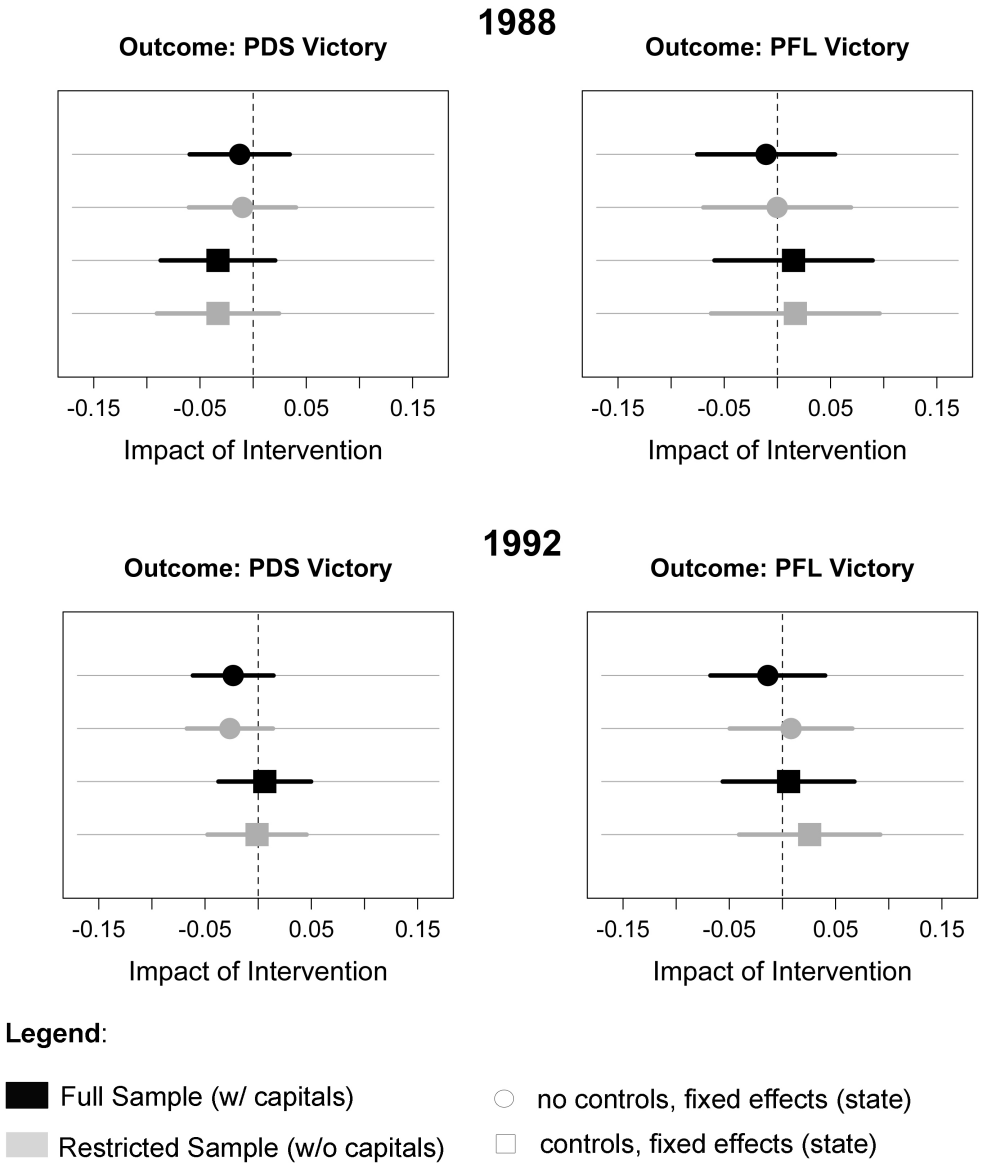
Focusing on the CEM results, Figure 7 presents the point estimates and 95%-confidence intervals depicting the average treatment for the treated (ATT) due to having had an appointed mayor. Estimates are broken down by outcome, electoral cycle, and inclusion of state capitals. Here again we find that estimates of the effect of *Intervened* were statistically indistinguishable from zero and had small magnitudes.[45]

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

[45]Moreover, 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.18. For evidence of the robustness of our findings to migration patterns, see Appendix section A.20. 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.21. For a discussion of the potential relationship between appointed mayors and clientelism, see Appendix section A.22.

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



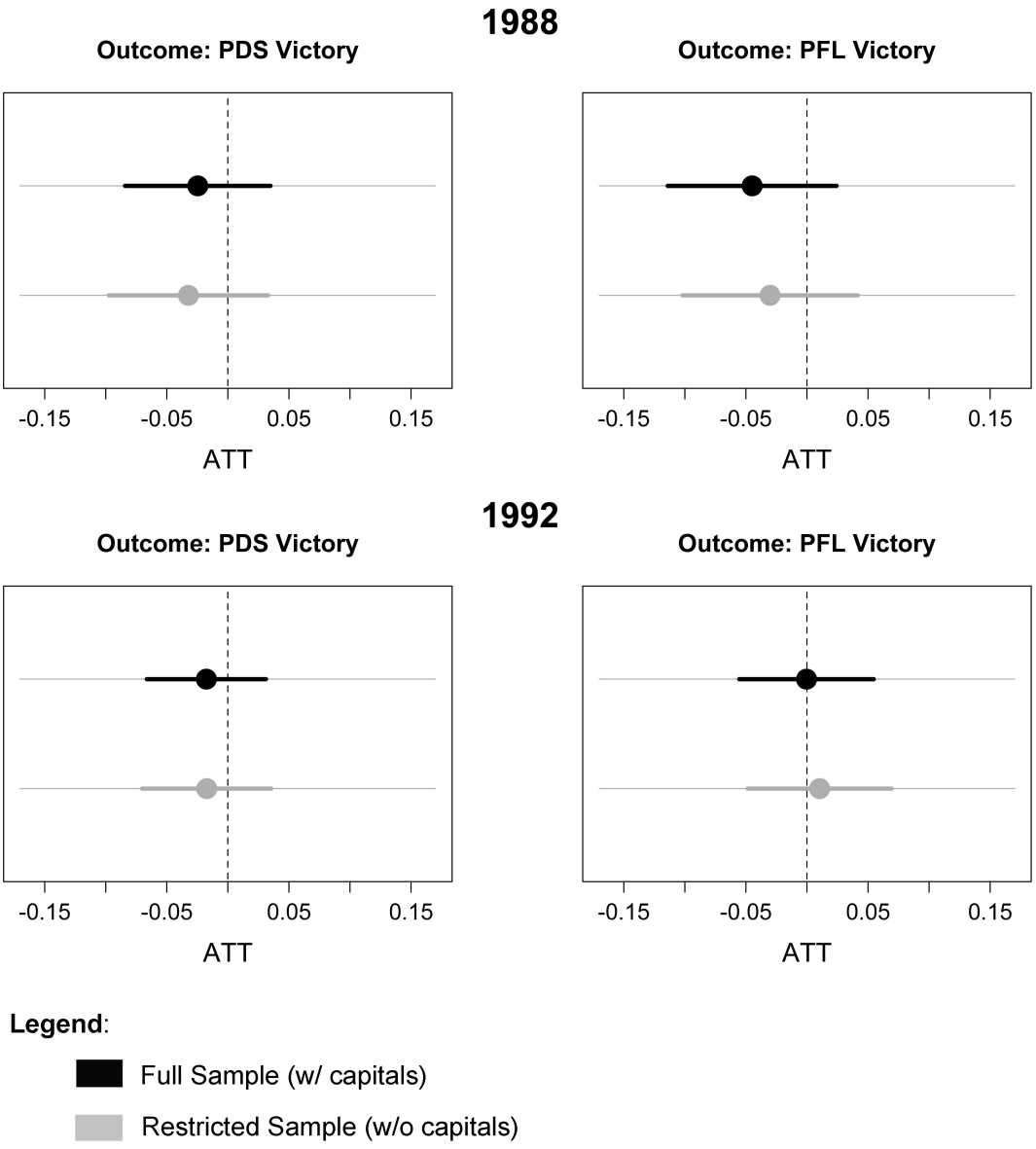
Note: Fixed effects for state included in all regressions. Detailed results for all underlying regressions can be found in Table A15 and Table A16 in the Appendix.

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 of political networking and mobilization in spite of military rule and local intervention.[46] 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).

[46]For 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."

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 (which is based on distinct causal inference designs) has revealed empirical patterns that are in line 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 detailed and 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 mainly caused by the denial of self-government (which is 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.[47]

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

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 confounding variables—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 cases with a multitude of initial conditions. If we identify comparable patterns across extremely heterogenous 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 have coded 23 existing studies that investigate the long-term effects of denials of self-government through authoritarianism or imperialism. The key criteria for studies to enter into 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) consider a dimension of political behavior closely related to or directly reflecting support for democracy as one of the dependent variables, and (3) be published in a reputable political science or economics journal or authored by an established researcher in the field (in the case of publicly available working papers). In Table 2, we list all studies considered by us and which type of intervention they investigate. Importantly, two of the studies differentiate explicitly between the legacies of more and less repressive types of authoritarian regimes (see observations #10, #11, #15, and #16), which (in each case) allowed us to obtain *two* observations for our meta-analysis (as we also differentiate between levels of repressiveness there).

We proceed by coding the different studies based on the categories relevant to our theory to make them comparable with each other (for details, see Table 3). Based on our theoretical

Table 2: 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	Pop-Eleches and Tucker (2020)	Authoritarian (hardline communism)
5	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	Bernhard and Karakoç (2007)	Authoritarian (authoritarianism)
11	Bernhard and Karakoç (2007)	Authoritarian (totalitarianism)
12	Doucette (2024)	Authoritarian
13	Xue (2021)	Authoritarian
14	Krishnarajan, Doucette and Andersen (2023)	Authoritarian
15	Pop-Eleches and Tucker (2014)	Authoritarian (regular communism)
16	Pop-Eleches and Tucker (2014)	Authoritarian (hardline communism)
17	Giuliano and Nunn (2013)	Authoritarian
18	Guiso, Sapienza and Zingales (2016)	Authoritarian
19	Grosfeld and Zhuravskaya (2015)	Imperial
20	Nathan (2019)	Imperial
21	Charnysh and Pique (2024)	Imperial
22	Lee and Paine (2019)	Imperial
23	Lechler and McNamee (2018)	Imperial
24	Besley and Persson (2019)	Imperial
25	Aaskoven (2022*a*)	Imperial

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 line 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 .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 and features 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 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 and/or coefficient values or rely on the authors' descriptions regarding effect size.

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.

Table 3: 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 econ. 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 orgs. p.c.) in cities without historical self-government	Italy	Different	3+	0.5	1
19	Decline in positive attitude toward democracy from repressive rule (Russia vs. Austria)	Poland	1815–1914	3+	1	2
20	Anti-Democratic vote coordination/clustering, reflecting clientelism (invented chiefs vs. no chiefs)	Ghana	1901–1957	2	0	1
21	Voting for post-authoritarian (ex-communist) parties in post-Cold War election	Poland	1939–1945	0	1	0
22	Long-term effect of British colonialism on democracy levels (30 years after independence)	Multiple	Different	2	0	0
23	Reduction of voter turnout in areas with colonially approved/appointed chiefs	Namibia	1884–1915	1	0.5	2
24	Negative impact of imperial rule (colonialism/USSR rule) on democratic values	Multiple	Different	1	0.5	1
25	Decrease in vote for European cooperation (high terror during foreign occupation vs. low terror)	Denmark	1940–1945	0	1	1

We then proceed to evaluate patterns in our data via linear regression. The findings are summarized in Table 4. Despite the relatively small number of only 25 observations, the results are striking. First, both the length of intervention as measured in generations as well as the level of repression have individually significant effects (as shown in columns 1, 2, 3, and 6). Both dimensions of intervention significantly increase long-term outcomes associated with undemocratic political behavior. Additionally, as columns 4, 5, 7, and 8 demonstrate, in line with our framework, there is also a potential interaction effect between length and repressiveness. Although the coefficients of columns 5 and 8 are not significant, this is likely related to the high demands on statistical power associated with the introduction of two baseline and one interaction terms. While the individual coefficients in these models do not exhibit statistical significance, the interaction effect in both is close to statistical significance at $\alpha < 0.1$ and the overall model displays statistical significance as well (as represented in the F-statistic). This is meaningful for such a low N and highlights the interactive effects of repression and intervention length. As columns 7 and 8 show, the effects remain substantively similar when we compare only within the two categories through the inclusion of a dummy variable for imperial rule. In light of the small sample size, the combined evidence from all models speaks in favor of our theory. The high statistical significance of the interaction term in particular highlights the relevance of cases on the *main diagonal* of the theory summary, which were emphasized by our theoretical framework. Finally, all key findings presented and discussed here are robust to the use of ordered logit models as an alternative modeling choice.

Table 4: Meta-Analysis Results

	Dependent variable:							
	Relative Effect Size							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Generations	0.320**		0.361***		−0.224	0.354***		−0.218
	(0.115)		(0.103)		(0.366)	(0.106)		(0.375)
Repression		0.720*	0.893**		−0.259	0.852**		−0.265
		(0.401)	(0.332)		(0.764)	(0.352)		(0.782)
Gen. * Repr.				0.460***	0.681		0.449***	0.669
				(0.098)	(0.410)		(0.103)	(0.421)
Imperialism Dummy						−0.105	−0.091	−0.066
						(0.246)	(0.227)	(0.239)
Constant	0.780***	0.736**	0.096	0.799***	1.091	0.163	0.834***	1.116
	(0.202)	(0.308)	(0.311)	(0.135)	(0.670)	(0.354)	(0.163)	(0.691)
Observations	25	25	25	25	25	25	25	25
R^2	0.253	0.123	0.438	0.489	0.503	0.443	0.493	0.505
Adjusted R^2	0.221	0.085	0.387	0.467	0.432	0.363	0.447	0.406
F Statistic	7.790**	3.218*	8.561***	22.020***	7.084***	5.556***	10.688***	5.098***

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 local political autonomy to result in sustained legacies manifest in voting behavior? We argue that *enduring* interventions associated with *encompassing* repression have the highest potential to sustainably change political attitudes and behavior. In contrast, interventions that are *limited* in both 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 crucial differences between the imperial powers that occupied it for more than a century: Prussia allowed for relatively 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 political 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 theoretical key 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 that belong to the populist and antidemocratic 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 intervention and its relatively limited repressive scope help explain this outcome. Additionally and in line with our theory, the results of the meta-analysis highlight that both the length of an intervention as well as its levels 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.

As our study has revealed that the broad patterns of both authoritarian and imperial denials of local self-government are 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 nuanced differences in intervener-population interactions over time. A related potential area of future inquiry would be how intertemporal variation in the consistency of repression applied to populations in intervened territories 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 the instantiation of legacies. 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 events in the chosen class 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.[48] Fourth, one selects cases that permit the theory to be tested. Said cases must ideally exhibit appropriate cross-case and within-case variation to allow for rigorous causal inference.[49] Finally, one empirically assesses the existence of a legacy in the cases and situates the findings relative to the expectations generated by the theory.

[48]Importantly, 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.

[49]With 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, as such variation is necessary to establish the existence or non-existence of a legacy in each (aggregate) case.

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 certain types of events that make them more or less likely to produce lasting change. Naturally, adding a cross-national dimension to legacy studies may present certain challenges. We harbor no illusions that finding cases with precisely the right types of cross-case and within-case variation will be an easy task.

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 comparisons, 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 on legacies. 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 the generalizability of empirical findings. In sum, there is a great deal to be gained from a comparative approach to the study of historical legacies.

References

Aaskoven, Lasse. 2022*a*. "Foreign occupation and support for international cooperation: Evidence from Denmark." *World Politics* 74(2):285–325.

Aaskoven, Lasse. 2022*b*. "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.

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.

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.

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. 2023. "Culture, institutions and the long divergence." *Journal of Economic Growth* .

Blakeley, Ruth. 2012. State Violence as State Terrorism. In *The Ashgate Research Companion to Political Violence*, ed. Marie Breen-Smyth. Ashgate pp. 63–78.

Boas, Taylor. 2022. Potential Mistakes, Plausible Options: Establishing the Legacy of Hypothesized Critical Junctures. In *Critical junctures and historical legacies*, 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.

Carvalho, José Murilo de. 2016. *Cidadania no Brasil: O Longo Caminho*. Civilização Brasileira.

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, Paweł 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 Pique. 2024. "Razing the Church: The Enduring Effect of Nazi Repression in Poland." *Available at 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*, 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.

Davies, Norman. 2005. *God's Playground A History of Poland: Volume II: 1795 to the Present*. Oxford Univ. Press.

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, Sjifra 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, Alastair Smith, Randolph M. Siverson and James D. Morrow. 2005. *The Logic of Political Survival*. MIT Press.

De Mesquita, Bruce Bueno and George W. Downs. 2006. "Intervention and democracy." *International Organization* 60(3):627–649.

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, 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 Stavnskær. 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.

Flynn, Peter. 1978. *Brazil: A Political Analysis*. Westview Press.

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.

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." PhD Dissertation: Pontifíca Universidade Católica do Rio Grande do Sul.

Gingerich, Daniel W. 2014. "Brokered politics in Brazil: an empirical analysis." *Quarterly Journal of Political Science* 9(3):269–300.

Gingerich, Daniel W. 2019. "Ballot Reform as Suffrage Restriction: Evidence from Brazil's Second Republic." *American Journal of Political Science* 63(4):920–935.

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).

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.

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.

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*, 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.

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.

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. EUROREG - 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).

Iyer, Lakshmi. 2010. "Direct versus indirect colonial rule in India: Long-term consequences." *The Review of Economics and Statistics* 92(4):693–713.

Karaman, K. Kivanc and Şevket Pamuk. 2013. "Different paths to the modern state in Europe: the interaction between warfare, economic structure, and political regime." *American Political Science Review* 107(3):603–626.

Kaufman, Robert. 2022. Critical Junctures and Contemporary Latin America: A Note of Caution. In *Critical junctures and historical legacies*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 381–386.

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.

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.

Kuk, Leszek. 2021. Die Bestimmungen des Wiener Kongresses hinsichtlich Polens vor dem Hintergrund der polnischen Frage im 19. Jahrhundert (1795–1918). In *Die polnische Frage und der Wiener Kongress 1814-1815*, ed. Bogusław Dybaś. Verlag der Österreichischen Akademie der Wissenschaften pp. 133–158.

Lange, Matthew K. 2004. "British colonial legacies and political development." *World Development* 32(6):905–922.

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.

Lepore, Jill. 2018. *These truths: A history of the United States*. WW Norton & Company.

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.

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.

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.

Merkley, Eric. 2020. "Anti-intellectualism, populism, and motivated resistance to expert consensus." *Public Opinion Quarterly* 84(1):24–48.

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.

Nathan, Noah L. 2019. "Electoral consequences of colonial invention: Brokers, chiefs, and distribution in northern Ghana." *World Politics* 71(3):417–456.

Neundorf, Anja. 2010. "Democracy in transition: A micro perspective on system change in post-socialist societies." *Journal of Politics* 72(4):1096–1108.

Neundorf, Anja and Kaat Smets. 2017. Political socialization and the making of citizens. In *Oxford Handbook Topics in Politics*. Oxford University Press.

Pardelli, Giuliana and Alexander Kustov. 2022. "When coethnicity fails." *World Politics*

74(2):249–284.

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.

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.

Power, Timothy J. 2000. *Political right in postauthoritarian Brazil: elites, institutions, and democratization*. Penn State 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.

Rozenas, Arturas, Sebastian Schutte and Yuri 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.

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.

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.

Skidmore, Thomas E. 1988. *The politics of military rule in Brazil, 1964-1985*. Oxford University Press.

Soares, Gláucio Ary Dillon. 2001. *A Democracia Interrompida*. FGV Editora.

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/.

Tarnowska, Anna. 2012. *Z dziejów unifikacji administracji II Rzeczypospolitej*. Wydawnictwo Naukowe Uniwersytetu Mikołaja Kopernika.

Tarnowska, Anna. 2013. "Podstawy prawne skarbowości samorzadowej w II RP." *Prawo Budzetowe Państwa i Samorzadu* 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). 1979*a*. *Boletim Eleitoral No. 339 (Outubro)*. TSE.

Tribunal Superior Eleitoral (TSE). 1979*b*. *Boletim Eleitoral No. 340 (Novembro)*. TSE.

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*, ed. David Collier and Gerardo L. Munck. Rowman & Littlefield pp. 159–182.

Xue, Melanie Meng. 2021. "Autocratic Rule and Social Capital: Evidence from Imperial China." Available at SSRN 2856803.

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

Appendix

May 2, 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 polities 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,

[1]For 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).

[2]Conspiracy 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 = 25) 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

[3]Of 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.

fact that, even in fully developed modern democracies, some segments of the population vote for radical 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, allowing them to effectively compensate for some of the most negative consequence of the absence of formal participation channels.[4] In addition, cultural dissimilarities to the representatives of an intervening authority—whether they come from another region or another country—could constitute 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

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

[5] Cultural 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).

to avoid forms of violent repression through behavioral change. Our theoretical framework rests on the 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.)

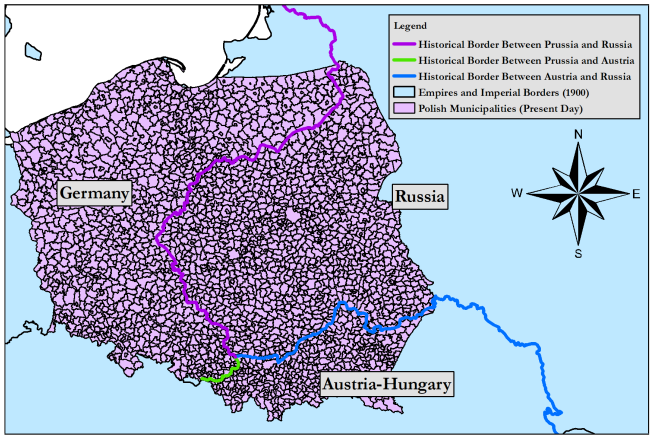
In short, both resistance and resentment are topics that are important to acknowledge. As we discuss, some forms of resistance against an intervention can be expected in almost all contexts. However, resistance itself does in no way contradict our theoretical predictions. Rather is compatible with our 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 antidemocratic 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 antidemocratic 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. Drinóczi 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. In line 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 Descriptive Statistics: Poland

Table A1 shows descriptive statistics for the Polish case.

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

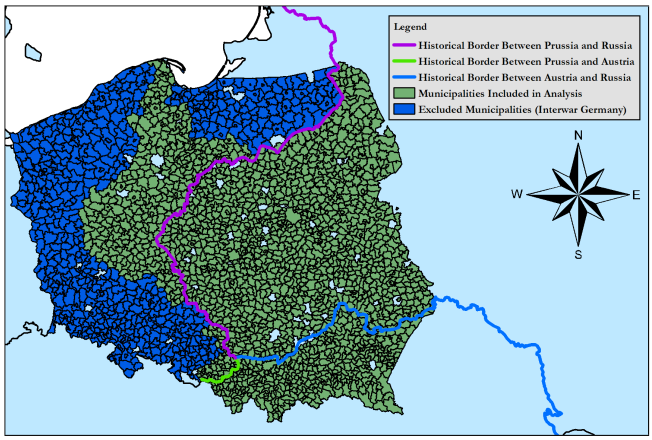
[6] On this issue, see also Charnysh (2017).

[7] Furthermore, 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.

A.6 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.7 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:

$$f(geographic\ location) = \gamma_1\ distance\ to\ border_i + \gamma_2\ distance\ to\ border_i * EMP_{ji} \tag{1}$$

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:

$$\begin{aligned} f(geographic\ location) = \gamma_1\ distance\ to\ border_i + \gamma_2\ distance\ to\ border_i^2 + \\ \gamma_3\ distance\ to\ border_i * EMP_{ji} + \gamma_4\ distance\ to\ border_i^2 * EMP_{ji} \end{aligned} \tag{2}$$

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:

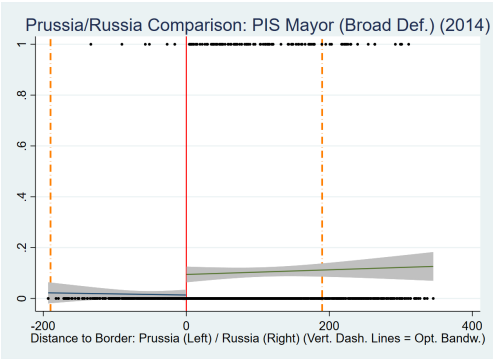
$$f(geographic\ location) = \gamma_1 x + \gamma_2 y + \gamma_3\ distance\ to\ border_i + \gamma_4\ distance\ to\ border_i * EMP_{ji} \tag{3}$$

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

A.8 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.

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.

A.9 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).

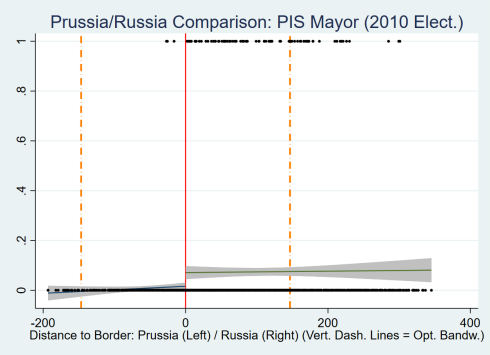
At the same time, in the interest of full transparency, in this Appendix section, we also present the results of a simple dummy variable analysis that does not rely on a GRDD and considers the full sample of

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

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	0.066**	0.072**	0.050**	0.092***	0.102***	0.053*
	(0.028)	(0.028)	(0.024)	(0.032)	(0.033)	(0.027)
Elevation	−0.00000	0.00001	0.0002	0.00001	0.00001	0.0002*
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Lat.	−0.003	−0.003	0.004			
	(0.017)	(0.017)	(0.014)			
Long.	0.011	0.010	0.002			
	(0.009)	(0.010)	(0.008)			
Pop. Density				0.00004	0.00004	0.00000
				(0.00003)	(0.00003)	(0.00002)
Urban Share				0.0001	0.0001	0.0001
				(0.0004)	(0.0004)	(0.0003)
Unemply. Rate				−0.002	−0.002	0.0003
				(0.004)	(0.004)	(0.003)
Avg. Monthly Salary				0.001	0.001	0.0002
				(0.001)	(0.001)	(0.001)
Work. Age Pop. Share				0.019**	0.022**	−0.006
				(0.009)	(0.009)	(0.008)
Elderly Pop. Share				0.002	0.002	−0.008
				(0.005)	(0.006)	(0.005)
Pop. (Log) (2014)				0.004	0.007	
				(0.015)	(0.015)	
Pop. (Log) (2010)						−0.001
						(0.013)
Dist. PR-RU	−0.0002	−0.0002	0.0001	−0.0002	−0.0003	0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.001)	(0.0004)
Russia * Dist. PR-RU	0.0001	0.0001	−0.0001	0.0004	0.0003	−0.0001
	(0.0003)	(0.0004)	(0.0003)	(0.001)	(0.001)	(0.001)
Constant	−0.003	0.002	−0.248	−1.339**	−1.535**	0.498
	(0.760)	(0.781)	(0.664)	(0.637)	(0.652)	(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

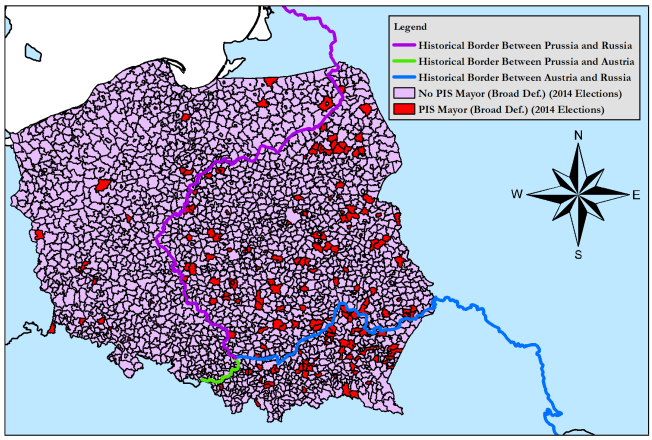
Note: OLS *p<0.1; **p<0.05; ***p<0.01

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.

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.

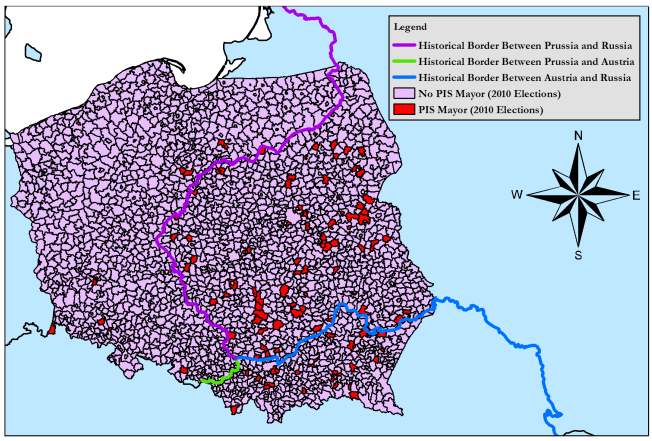
Polish municipalities. We mainly consider this a robustness check through which we can potentially confirm the general direction of our empirical results with a broader sample.

A.9.1 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.)

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.

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 need to be distinguished from other parts of the Prussian partition.[8]

A.9.2 Models in the Simple Dummy Variable Analysis

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

$$y_i = \beta_0 + \sum_{j=1}^{n} \beta_j \; EMP_{ji} + \beta_{n+1} ELV_i + \mathbf{x_i'} \; \boldsymbol{\beta} + \varepsilon_\mathbf{i} \tag{4}$$

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.9.3 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.10), the initial finding presented here that Austria is more likely to have PiS mayors does not hold in some of the more rigorous GRDD re-

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

gressions. 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

	Dependent variable:		
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	(1)	(2)	(3)
Russia	0.080***	0.087***	0.065***
	(0.015)	(0.016)	(0.012)
Interwar Germany	0.006	0.008	0.005
	(0.016)	(0.017)	(0.014)
Austria	0.112***	0.123***	0.053***
	(0.019)	(0.019)	(0.015)
Constant	0.016	0.016	0.008
	(0.013)	(0.013)	(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

Table A4: Local Political Leadership Outcomes (With Controls)

	Dependent variable:		
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	(1)	(2)	(3)
Russia	0.111***	0.119***	0.088***
	(0.017)	(0.017)	(0.014)
Interwar Germany	0.017	0.020	0.018
	(0.018)	(0.018)	(0.015)
Austria	0.146***	0.157***	0.054***
	(0.022)	(0.023)	(0.018)
Elevation	−0.0001**	−0.0001**	0.00004
	(0.00005)	(0.00005)	(0.00004)
Pop. Density	0.00004**	0.00004**	0.00003*
	(0.00002)	(0.00002)	(0.00001)
Urban Share	0.0005**	0.0004	0.0003
	(0.0002)	(0.0002)	(0.0002)
Unemply. Rate	−0.002	−0.002	−0.001
	(0.002)	(0.002)	(0.002)
Avg. Monthly Salary	−0.001*	−0.001**	−0.0005
	(0.001)	(0.001)	(0.0005)
Work. Age Pop. Share	0.005	0.005	−0.003
	(0.004)	(0.005)	(0.004)
Elderly Pop. Share	−0.003	−0.003	−0.006***
	(0.003)	(0.003)	(0.002)
Pop. (Log) (2014)	0.001	0.009	
	(0.009)	(0.010)	
Pop. (Log) (2010)			0.001
			(0.008)
Constant	−0.155	−0.213	0.285
	(0.328)	(0.339)	(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.10 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.

A.10.1 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.10.2 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.11 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

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

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	−0.075**	−0.085**	0.039	−0.102*	−0.103*	0.003
	(0.037)	(0.038)	(0.031)	(0.053)	(0.055)	(0.045)
Elevation	−0.0002**	−0.0002**	−0.00002	−0.0002**	−0.0002**	−0.00001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. AU-RU	0.001	0.001	−0.0002	0.002	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Dist. AU-RU Sq.				−0.00002	−0.00002	−0.00001
				(0.00002)	(0.00002)	(0.00002)
Russia * Dist. AU-RU	−0.001	−0.001	0.00002	−0.003	−0.003	−0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Russia*Dist. AU-RU Sq.				0.00002	0.00002	0.00001
				(0.00002)	(0.00002)	(0.00002)
Constant	0.230***	0.253***	0.062**	0.265***	0.281***	0.084**
	(0.035)	(0.036)	(0.030)	(0.048)	(0.049)	(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)

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

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	(1)	(2)	(3)	(4)	(5)	(6)
Austria	0.116***	0.127***	0.062**	0.043	0.054	0.038
	(0.042)	(0.044)	(0.031)	(0.052)	(0.054)	(0.038)
Elevation	−0.0003***	−0.0003***	−0.00004	−0.0003***	−0.0003***	−0.00005
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. PR-AU	0.0001	0.0001	−0.00001	0.0004	0.0005	−0.00004
	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(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	0.00005	0.001	0.001	0.001
	(0.0002)	(0.0002)	(0.0002)	(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**	0.077**	0.010	0.100**	0.108**	0.009
	(0.035)	(0.036)	(0.025)	(0.042)	(0.044)	(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)

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor HE	Mayor PIS	Mayor PIS (Broad)
	(1)	(2)	(3)	(4)	(5)	(6)
Austria	0.113***	0.123***	0.050	0.092	0.099	0.045
	(0.044)	(0.045)	(0.032)	(0.065)	(0.068)	(0.056)
Elevation	−0.0001	−0.0002*	−0.0001	−0.00001	−0.00004	0.00000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Lat.	0.129**	0.111*	−0.074			
	(0.065)	(0.066)	(0.047)			
Long.	−0.014	−0.010	0.027*			
	(0.019)	(0.020)	(0.014)			
Pop. Density				0.00002	0.00001	−0.00001
				(0.00005)	(0.0001)	(0.00004)
Urban Share				0.0002	0.0001	−0.0001
				(0.001)	(0.001)	(0.001)
Unemply. Rate				−0.017	−0.015	−0.010
				(0.018)	(0.019)	(0.015)
Avg. Monthly Salary				0.0002	0.0003	0.0004
				(0.002)	(0.002)	(0.002)
Work. Age Pop. Share				−0.019	−0.021	0.008
				(0.018)	(0.019)	(0.015)
Elderly Pop. Share				−0.020*	−0.022*	0.00004
				(0.012)	(0.012)	(0.010)
Pop. (Log) (2014)				−0.015	0.00002	
				(0.029)	(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)	(0.001)	(0.0004)	(0.002)	(0.002)	(0.002)
Austria * Dist. PR-AU	−0.001	−0.001	0.0003	−0.0001	0.0002	0.001
	(0.0005)	(0.0005)	(0.0003)	(0.002)	(0.002)	(0.002)
Constant	−6.144**	−5.317*	3.216	1.698	1.745	−0.609
	(3.006)	(3.096)	(2.185)	(1.260)	(1.318)	(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

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

	Dependent variable:		
	Mayor with Higher Education		
	Simple Dummy Variables (1)	Regression Discontinuity Design (2 & 3)	
	(1)	(2)	(3)
Russia	−0.069***	−0.053*	−0.079**
	(0.018)	(0.029)	(0.039)
Interwar Germany	−0.047**		
	(0.019)		
Austria	−0.052**		
	(0.022)		
Elevation		−0.0002*	−0.0002*
		(0.0001)	(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	0.0002
		(0.0004)	(0.001)
Russia*Dist. Sq.			0.00000
			(0.00001)
Constant	0.954***	0.978***	0.984***
	(0.015)	(0.031)	(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.12 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.6, 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.

A.13 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).[9] 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.

[9] Also cited in Kubow (2013, 5).

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

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	RDD			RDD (Optimal Bandwidth)		
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	0.105***	0.112***	0.074***	0.092***	0.099***	0.058**
	(0.027)	(0.027)	(0.023)	(0.028)	(0.029)	(0.025)
Elevation	0.0001	0.0001	0.0002**	0.00004	0.00004	0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pop. Density	0.0001**	0.00004*	0.00003	0.00004**	0.00003	0.00002
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Urban Share	0.001*	0.001*	0.0004	0.0005	0.0005	0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Unemply. Rate	−0.005	−0.005	−0.002	−0.004	−0.004	−0.0002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Avg. Monthly Salary	−0.001	−0.001	−0.001	−0.0003	−0.0004	0.00004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Work. Age Pop. Share	0.0001	0.002	−0.011*	0.005	0.007	−0.002
	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Elderly Pop. Share	−0.008*	−0.007	−0.012***	−0.007	−0.006	−0.008**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Pop. (Log) (2014)	−0.001	0.006		0.002	0.011	
	(0.014)	(0.014)		(0.014)	(0.014)	
Pop. (Log) (2010)			−0.0001			−0.002
			(0.012)			(0.012)
Migration Levels (per 1000)	−0.002	−0.002	−0.001	−0.002	−0.002	−0.0004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Dist. PR-RU	0.00001	−0.00002	0.0003	−0.00002	−0.00005	0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Russia * Dist. PR-RU	0.0001	0.0002	−0.0002	0.0004	0.0004	0.00003
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Constant	0.201	−0.021	0.915**	−0.183	−0.395	0.289
	(0.494)	(0.508)	(0.432)	(0.525)	(0.537)	(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)

	Dependent variable:					
	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)	Mayor PIS	Mayor PIS (Broad)	Mayor PIS (2010)
	RDD			RDD (Optimal Bandwidth)		
	(1)	(2)	(3)	(4)	(5)	(6)
Russia	0.112***	0.121***	0.085***	0.097***	0.106***	0.068**
	(0.029)	(0.030)	(0.026)	(0.030)	(0.031)	(0.028)
Elevation	0.0001	0.0001	0.0002**	0.00004	0.0001	0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pop. Density	0.00003	0.00002	0.00001	0.00002	0.00001	−0.00000
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Urban Share	0.001***	0.001***	0.001*	0.001**	0.001**	0.0004
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0003)
Unemply. Rate	−0.007**	−0.006**	−0.002	−0.006*	−0.005	0.00000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Avg. Monthly Salary	−0.002**	−0.002**	−0.001	−0.001	−0.002*	−0.0003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Work. Age Pop. Share	−0.00001	0.001	−0.013*	0.004	0.005	−0.004
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
Elderly Pop. Share	−0.007	−0.006	−0.012***	−0.005	−0.004	−0.008*
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Pop. (Log) (2014)	−0.003	0.003		0.001	0.009	
	(0.015)	(0.015)		(0.015)	(0.015)	
Pop. (Log) (2010)			0.001			0.001
			(0.013)			(0.014)
Dist. PR-RU	−0.0002	−0.0002	0.0001	−0.0002	−0.0003	−0.00000
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Russia * Dist. PR-RU	0.0005	0.0005	−0.0001	0.001	0.001*	0.0002
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Constant	0.288	0.123	1.045**	−0.050	−0.207	0.388
	(0.561)	(0.572)	(0.503)	(0.590)	(0.602)	(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

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

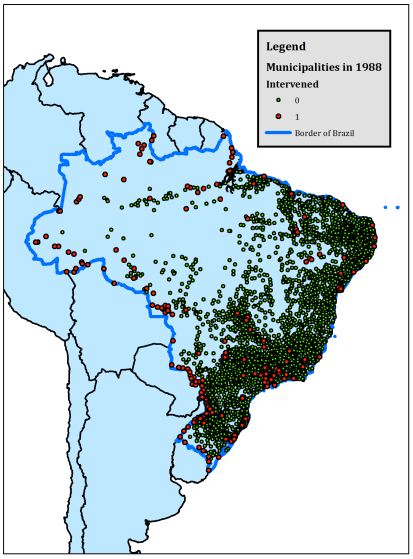
A.14 Second Case Supplement: Additional Information on Brazil

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

A.14.1 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.

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).

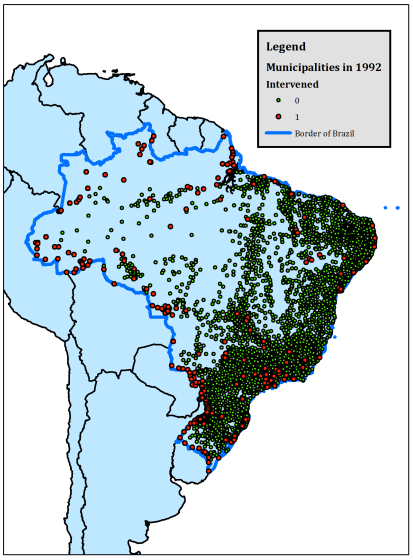
A.15 Second Case Supplement: Why Should the PFL be Considered an Authoritarian Successor Party?

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.[10] 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

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

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).

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 (Diáro 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.[11] 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.

Table A12: Roll Call Voting Results on Amendment for Presidential Elections based on an Electoral College (March 23, 1988) (*Source:* Diáro 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.15.1 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.

A.16 Descriptive Statistics: Brazil

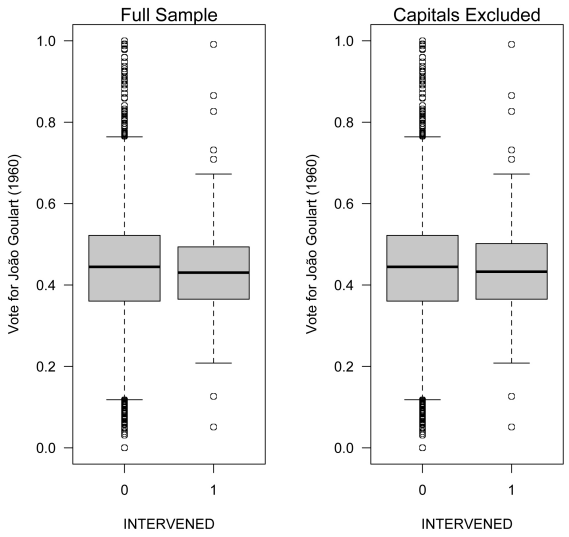
Table A13 shows descriptive statistics for the case of Brazil.

A.17 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

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

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.

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.

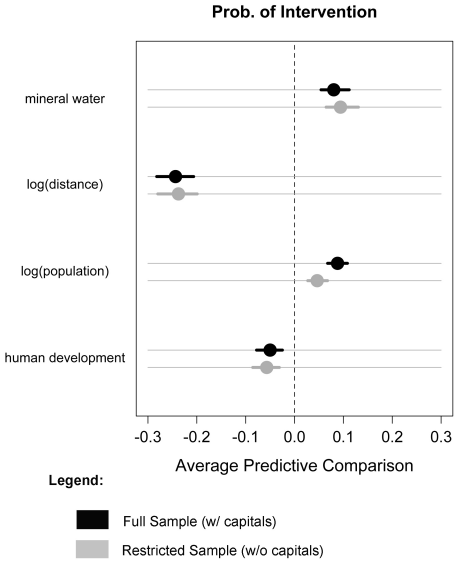
A.18 Second Case Supplement: Contemporaneous Institutional Changes in Brazil

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

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

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.

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), 1979*a*). In 1976, the number increased to 614 out of 3790 (16%) (Tribunal Superior Eleitoral (TSE), 1979*b*). 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.19 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***	-1.246***
	(0.063)	(0.065)
Mineral Water	1.549***	1.836***
	(0.212)	(0.230)
Population (log.)	0.665***	0.398***
	(0.072)	(0.005)
Human Development	-4.149***	-5.051***
	(1.050)	(1.111)
N	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	-0.033	0.015	0.017	0.006	-0.001	0.006	0.026
	(0.027)	(0.029)	(0.038)	(0.040)	(0.022)	(0.024)	(0.032)	(0.034)
Distance-to-border	-0.016*	0.017*	0.003	0.003	0.015**	-0.014*	0.004	0.007
	(0.009)	(0.009)	(0.013)	(0.013)	(0.007)	(0.007)	(0.010)	(0.010)
Mineral Water	-0.010	0.011	0.012	0.011	0.022	0.021	0.003	0.002
	(0.018)	(0.019)	(0.025)	(0.026)	(0.016)	(0.016)	(0.022)	(0.023)
Population (log.)	-0.004	-0.004	-0.024***	-0.024***	-0.018***	-0.019***	-0.018***	-0.017***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.004)	(0.004)	(0.006)	(0.006)
Human Development	0.189*	0.185*	0.008	0.002	-0.013	-0.002	0.019	0.050
	(0.108)	(0.109)	(0.149)	(0.150)	(0.085)	(0.086)	(0.122)	(0.123)
N	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	-0.010	-0.011	-0.000	-0.024	-0.027	-0.014	0.008
	(0.024)	(0.026)	(0.033)	(0.035)	(0.019)	(0.021)	(0.028)	(0.029)
N	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)

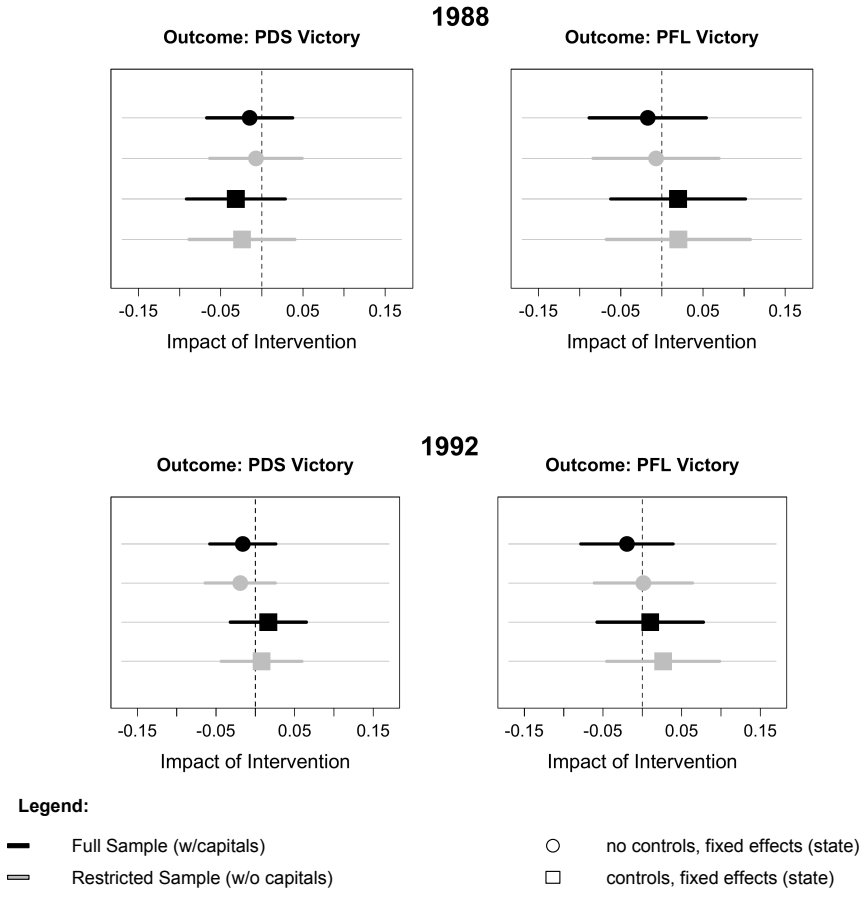
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PDS Mayor, 1988		PFL Mayor, 1988		PDS Mayor, 1992		PFL Mayor, 1992	
ATT (Intervened)	-0.025	-0.032	-0.045	-0.030	-0.018	-0.017	-0.000	0.010
	(0.030)	(0.033)	(0.035)	(0.037)	(0.025)	(0.027)	(0.028)	(0.030)
N.treated-all	212	186	212	186	266	235	266	235
N.treated-matched	166	149	166	149	205	183	205	183
N.controls-all	4,115	4,115	4,115	4,115	4,648	4,648	4,648	4,648
N.controls-matched	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.20 Empirical Examination (Case 2) Supplement: Robustness of Findings for Brazil to Migration Patterns

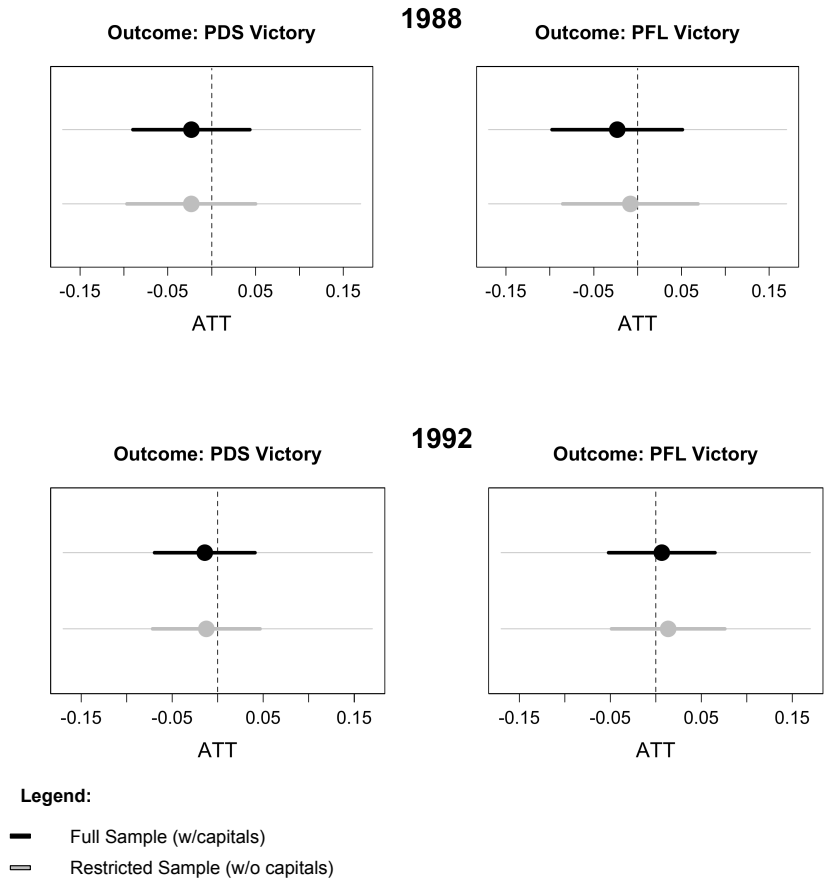
Here we present a reanalysis of our findings for the Brazilian case when the municipalities with the highest levels of migration are excluded from our datasets. Utilizing the Brazilian national census of 2000, we calculated the percentage of the population in each municipality that was resident in the 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.21 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,

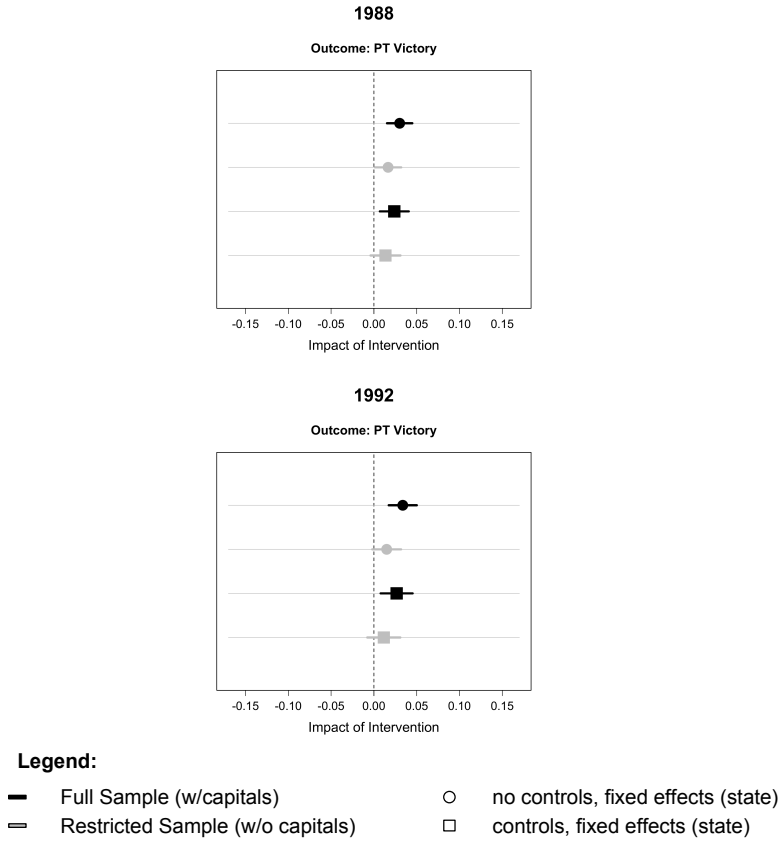
1992). So support for the PT may—to a certain degree—reflect a *rejection* of authoritarian rule.[12]

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.

[12]For 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



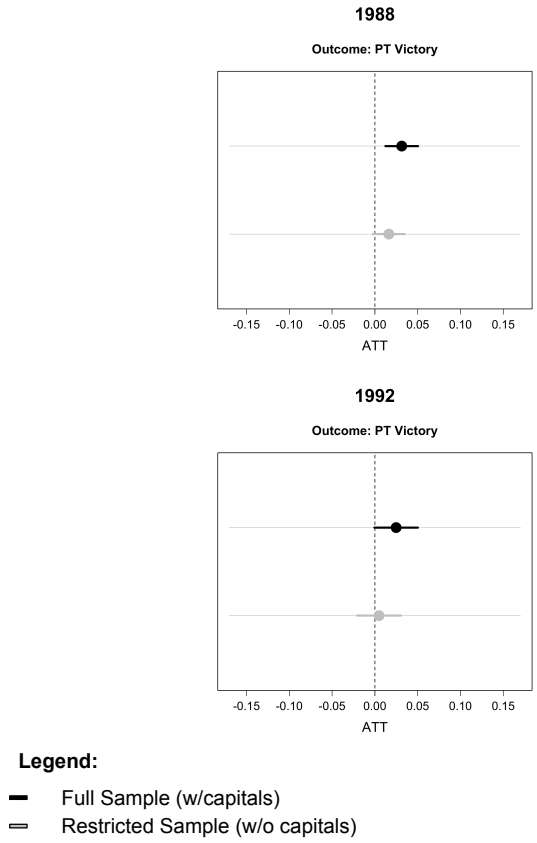
Note: Fixed effects for state included in all regressions.

A.22 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.[13] Given the inherent affinity between appointed mayors and their municipal funding sources, appointed mayors had the opportunity to build up particularly formidable clientelist machines.

[13]This 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 which 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 protegé 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.

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*. Oxford Univ. 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.

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* .

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.

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.

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.
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 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.
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.
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). 1979*a*. *Boletim Eleitoral No. 339 (Outubro)*. TSE.

Tribunal Superior Eleitoral (TSE). 1979*b*. *Boletim Eleitoral No. 340 (Novembro)*. TSE.

Tribunal Superior Eleitoral (TSE). 1988. *Dados estatisticos: 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.

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.