Inhibiting Indoctrination:

Autocratic Benefits and the Limited Effects of Indoctrination after Democratization

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Abstract

Can autocratic indoctrination shape contemporary democratic preferences? We argue that while ideological indoctrination can influence democratic preferences after a regime transition, its effects are significantly conditioned by the cognitive dissonance between individuals' lived experiences under autocracy and the indoctrinating propaganda. Using a Regression Discontinuity Design and survey data from the Baltic countries, we estimate the causal impact of exposure to Soviet indoctrination through compulsory elementary education in the aftermath of the Soviet occupation in 1944. Our results show that exposure to Soviet elementary education reduces democratic preferences and increases support for authoritarianism after democratic transitions. Most importantly, our findings also demonstrate that benefiting from the Soviet regime conditions the effect of educational indoctrination on support for democracy. Those who did not benefit are more resistant to indoctrination and exhibit significantly stronger democratic support compared to those who did benefit from the regime.

Keywords: democratic preferences; indoctrination; autocratic legacies; political economy; benefits; Soviet Union

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Introduction

Under what conditions can autocratic regimes effectively indoctrinate their populations? Extensive scholarship has explored the role of indoctrination as a mechanism for regime stability, emphasizing its function in legitimizing autocratic rule. However, the impact of indoctrination may extend well beyond the lifespan of the regime itself, manifesting in forms of 'nostalgia' for the autocratic past even in seemingly consolidated democracies (Alesina and Fuchs-Schündeln 2007; Neundorf and Pop-Eleches 2020). If indoctrination can indeed persist after democratization, it carries profound implications for democratic resilience, as it can potentially undermine support for democratic norms and institutions, and even fuel the resurgence of extremist movements that evoke the former dictatorship (Darden and Grzymala-Busse 2006; Neundorf 2010; Voigtländer and Voth 2015). This study examines the conditions under which indoctrination successfully shapes political attitudes over the long term, raising critical questions about the durability of democratic consolidation and, more importantly, the persistence of support for authoritarianism even after democratic transitions.

Recent studies have increasingly examined how the legacies of past dictatorships shape contemporary political preferences (Dinas and Northmore-Ball 2020; Pop-Eleches and Tucker 2013). Despite being recognized as a core mechanism of authoritarian control, the conditions that determine the effectiveness of indoctrination remain significantly understudied (Neundorf and Pop-Eleches 2020; Darden and Grzymala-Busse 2006). While evidence suggests that indoctrination can shape democratic attitudes even after transitions to democracy, we argue in this article that its impact is unlikely to be uniform across individuals, even when they are exposed to indoctrination during the same formative ages. We propose a conditional framework for understanding the effect of authoritarian indoctrination on post-transitional support for democracy.

We make two key arguments. First, building on existing literature, we assert that indoctrination is most effective when experienced as part of basic socialization, particularly through early education (Darden and Grzymala-Busse 2006; Alesina,

Giuliano and Reich 2021; Neundorf and Pop-Eleches 2020; Neundorf et al. 2024). Second, we argue that the acceptance of indoctrination is, however, significantly moderated by individuals' lived experiences under the regime. Individuals exposed to authoritarian indoctrination are likely to view democracy more negatively, while forming more favorable perceptions of authoritarianism in general, and of the former regime in particular. Nevertheless, this effect diminishes when there is cognitive dissonance between the regime's indoctrinating propaganda and individuals' actual lived experiences. Consequently, indoctrination is less likely to influence those who did not benefit from the regime to improve their socioeconomic well-being.

To test these theoretical expectations, this article examines the cases of Estonia, Latvia, and Lithuania — three Baltic countries that were incorporated into the Soviet Union at the end of World War II. The Soviet Union first occupied these countries in 1940, but despite a wave of terror, it was unable to implement any significant policy, particularly in the sphere of education, that would enable systematic indoctrination, due to the interruption by Nazi occupation in 1941. It was only in 1944, when the Soviet Union reoccupied these countries, that it was able to enact sweeping educational changes and, with them, introduce substantial ideological indoctrination. We leverage this relatively abrupt imposition of Soviet indoctrination in 1944 to study its impact on individuals exposed to Soviet education during their formative years, and how this effect is moderated by their lived experiences under Soviet rule.

We employ a Regression Discontinuity (RD) design to estimate the causal impact of Soviet indoctrination through compulsory elementary education on post-transitional democratic preferences. Our approach leverages the essentially exogenous timing of respondents' birth years, which determines the completion of their mandatory education at age of 15 (Titma and Saar 1995). We construct an RD design that distinguishes between cohorts who completed their mandatory schooling before the 1944 occupation – being too old to have experienced Soviet education; and those who completed their education post-occupation – who were sufficiently young to be influenced by the Soviet educational system.

To further explore the formation of these preferences, we examine how individuals' lived experiences under the Soviet regime influence the enduring effects of indoctrination over time. We conduct a detailed analysis of two sub-samples of respondents, stratified by whether or not they followed career paths associated with receiving regime benefits. Each sub-sample includes a control and a treatment group, defined by their exposure to Soviet indoctrination. Because subjective perceptions regarding whether respondents benefited from the Soviet regime would create endogeneity problems in our analysis, we focus on the pre-treatment decision to attend – or not attend – technical schools (Tekhnikums). Students enroll in these specialized technical secondary schools after compulsory education, but the decision to attend is made prior to this point. Tekhnikums played a crucial role in supplying the technical labor force necessary for Soviet post-war industrial expansion, as they were established and operated by individual sector ministries, which recruited employees for state-run production directly from their graduates (Peters 1956). These technical school graduates constituted the vanguard of the Soviet regime, occupying a practically useful, economically rewarded, and symbolically elevated social position within the Soviet system. We argue that this is a valid operationalization of benefiting from the Soviet regime, and we provide empirical evidence to support this claim.

This article makes two primary contributions to the literature on autocratic legacies and democratization. First, it challenges the conventional assumption that the effects of indoctrination are uniform, even when imposed on a mass scale through elementary education. By examining its impact on post-transitional politics, this study reveals that the efficacy of indoctrination is contingent on individuals' lived experiences under the regime. It is most effective for those who have tangible reasons to accept the regime's narrative — those who have benefited from it. In contrast, individuals whose lived experiences contradict the regime's self-portrayal are less susceptible to its indoctrinating efforts. Second, and more importantly, this article advances our understanding of how autocratic legacies shape post-transitional political attitudes, particularly democratic attitudes. By identifying the conditions under which

indoctrination persists beyond regime change, this study offers critical insights into the development of democratic attitudes in emerging democracies, which are crucial for sustaining democratic consolidation and preventing democratic backsliding.

This article begins by reviewing the existing literature on indoctrination, emphasizing the gaps in understanding the specific mechanisms through which indoctrination shapes individual attitudes and behavior. We then develop our theoretical framework through an analysis of Soviet educational indoctrination. The subsequent section details our data sources and methodological approach, with particular attention to the causal identification strategy employed. The empirical analysis follows, presenting our findings and demonstrating their robustness across various alternative specifications. We conclude by reflecting on the broader implications of our results.

Indoctrination and Political Attitudes

Indoctrination is a critical tool in the strategic repertoire of authoritarian regimes, which rely on a combination of legitimation, repression, and cooptation to ensure regime stability and durability (Gerschewski 2013). The study of the effects of authoritarian indoctrination falls within a broader scholarship examining the impact of historical legacies on contemporary politics, particularly how autocratic antecedents shape current political outcomes (Pop-Eleches and Tucker 2017; Dinas and Northmore-Ball 2020). Indoctrination is often conceptualized as an alternative to political repression, aimed at securing regime legitimacy by fostering approval and support rather than instilling fear and violence (Gerschewski 2013; Neundorf et al. 2017). Recent research has increasingly focused on the long-term effects of ideological indoctrination on political attitudes and behavior following transitions to democracy. This growing body of work has demonstrated that exposure to authoritarian indoctrination significantly shapes political attitudes (Alesina and Fuchs-Schündeln 2007; Voigtländer and Voth 2015; Neundorf and Pop-Eleches 2020), influences political

identification (Neundorf 2009; Dinas and Northmore-Ball 2020), affects democratic values (Darden and Grzymala-Busse 2006; Neundorf 2010; Neundorf et al. 2024), and alters levels of political participation (Pop-Eleches and Tucker 2013).

The literature has examined the specific conditions under which authoritarian indoctrination succeeds or fails. Some studies suggest that indoctrination can on occasion lead to resistance, when individuals either politically disengage or come to support alternative political positions or organizations (Wittenberg 2006; Tertytchnaya 2020). Most works, however, are consensual in showing that indoctrination works best when carried out in early socialization, and particularly, during earlier education (Darden and Grzymala-Busse 2006; Dinas and Northmore-Ball 2020; Neundorf and Pop-Eleches 2020; Alesina, Giuliano and Reich 2021; Neundorf et al. 2024). Pop-Eleches and Tucker (2013: 45) show that the effects of ideological socialization in communist regimes was the strongest "for those who experienced communism in their early formative years (between ages six and seventeen)". These authors further show that the effectiveness of indoctrination increases "with the number of years an individual spent under communism". Similarly, Neundorf et al. (2024: 19) argue that "educational imprinting will have the strongest impact during a person's impressionable years", which also correspond, according to these authors, to the years of compulsory education, between the ages of 5 and 15. Alesina, Giuliano and Reich (2021: 2286) further highlight the role of compulsory education, showing that autocrats often use mass education "to nation-build, to indoctrinate in order to preserve the status quo, and to reduce the threat of overthrow".

Yet, despite the growing work on different facets of authoritarian indoctrination and its legacies, Neundorf and Pop-Eleches (2020: 1843) suggest that "the study of the mechanisms underlying the production and reproduction of these [authoritarian] legacies is still in its infancy." Therefore, while indoctrination can be highly influential, its effects are unlikely to be uniform across individuals, even when they are exposed to indoctrination during the same formative ages. Against this backdrop, it becomes essential to investigate the conditions that make indoctrination more or less effective.

In this study, we focus on Soviet indoctrination during the immediate post-war period in the three Baltic countries of Estonia, Latvia, and Lithuania, which were incorporated into the Soviet Union following their occupation by the Red Army in 1944. We propose a two-step argument. First, consistent with the existing literature, we focus on the role of indoctrination through early-age socialization in education. Second, we argue that while such indoctrination can be powerful, its effects are not uniform. We suggest that indoctrination efforts are significantly moderated by individuals' lived experiences under the regime, which can mitigate its impact. Specifically, we argue that individuals who do not benefit from the authoritarian regime are significantly less likely to succumb to indoctrination.

Soviet Education and Indoctrination

Soviet indoctrination was importantly carried out through education. The education system was in fact the first encounter of the citizen with Soviet institutions (Titma and Saar 1995). Given the collectivist logic of Communism, the education system provided very little freedom of choice, and focused on the elite-defined needs of the society as a whole (ibid.). The Soviet school system started with general schooling of 8 grades, which normally ran from the age of 7 to 14 (Titma and Saar 1996). At the age of 15, which ended compulsory schooling, students were tracked into general secondary schools, specialized secondary schools, or into vocational training. Titma and Saar (1995: 39) suggest that while 100% of students completed the first 8 compulsory classes, only about 60% continued schooling beyond the age of 15.[1]

Aware of its formative influence on children, communist propaganda pervaded throughout the school system, and every subject was amended to include ideological indoctrination (Pilve 2014: 47). The most significantly influenced subject was history, which placed a great emphasis on the teaching of "Soviet patriotism" and its role in the

[1]Titma and Saar (1996: 8) provide evidence that the mean number of years of education was 15.3 for men and 14.6 for women.

global class struggle of the workers. Natural sciences highlighted the achievements of Soviet research, while music and art celebrated Russian composers and painters, incorporating the teaching of patriotic songs.

Soviet education was effectively introduced in the Baltic countries only after the second Soviet occupation in 1944. Although a temporary school curriculum was drawn up after the first Soviet occupation in 1940, Pilve (2014: 45) suggests that "[i]t was rather formal and did not yet affect the actual content of education." The opposite was true after the second Soviet occupation of 1944, when schools reopened in the fall of that year. In the immediate aftermath of World War II, Soviet authorities intensified propaganda efforts aimed at undermining Baltic independence and cultural identity, primarily by altering the study of history and language. The Soviet education system eliminated the study of local Baltic history, partly due to the unavailability of textbooks acceptable to the Soviet regime (Pilve 2014: 51). Soviet indoctrination also took the form of russification. Although the learning of the Russian language was nominally voluntary, Pilve (2014: 61) notes that "[t]he pressure to learn Russian was so great that in practice it was not possible for pupils to decide not to study it."

This period also marked the height of the Stalinist personality cult. Stalin was portrayed as a figure of paternal benevolence, caring for children as he cared for the nations of the Soviet Union, in exchange for their child-like, unquestioning devotion (Kelly 2004, 107–8). The years between 1944 and Stalin's death in 1953 witnessed the most intense efforts at educational indoctrination, targeting not only pupils but also teachers, who were required to attend lectures delivered by party-member colleagues (Pilve 2014, 57). Indoctrination efforts were renewed after the 1956 Hungarian Revolution (ibid., 58), when the personality cult of Stalin was replaced by the cult of the Communist Party.

Given this context, we expect that exposure to Soviet schooling, and particularly elementary education, which forms the core of socialization beyond the family, should significantly shape individual preferences. The impact of Soviet education is likely to be especially pronounced due to the abrupt 'treatment' effect following the imposition of

Soviet schooling in the Baltic states immediately after the second Soviet occupation in 1944. Specifically, we expect that the pervasive communist ethos of collectivism, deference to individual or party leadership, and the unquestioning acceptance of ideologically framed narratives in early education, should undermine support for democracy and increase acceptance of authoritarianism, particularly Soviet authoritarianism.

Hypothesis 1 *Exposure to Soviet elementary education decreases democratic preferences and increases support for (Soviet) authoritarianism.*

Regime Experience and Indoctrination

Importantly, we argue that the long-term effects of indoctrination are, however, significantly conditioned by individuals' lived experiences under the Soviet regime. While Soviet schooling exerted great socializing effort on school children likely to persist throughout their lives, this propaganda was largely semantic, and frequently misaligned with lived experience. Perhaps most astutely observed in Václav Havel's famous essay *The Power of the Powerless* (1978: section IV), the communist regime "is a world of appearances trying to pass for reality," and as such is "thoroughly permeated with hypocrisy and lies" (ibid.). Havel goes on to describe what amounts to a profound cognitive dissonance between the dictates of official doctrine and the experienced lives under communist authoritarianism.

Cognitive dissonance is an incongruence between two perceptions, such as smoking while knowing that it is bad for your health (Festinger 1957). Individuals may respond to cognitive dissonance in a number of ways, such as discounting dissonant information, lowering its salience, seeking alternative information, or changing their behavior (Harmon-Jones and Mills 2019). We expect that Soviet citizens who experienced cognitive dissonance between official regime propaganda and their everyday realities employed all of these strategies to cope with the discomfort caused by this dissonance. A common avenue, described by numerous observers of communist regimes, was paying formal lip service to the official expectations of the regime, while privately sharing its

absurdity with family and close friends (e.g. Pipes 2001).[2]

Consequently, we hypothesize that the influence of elementary school indoctrination is significantly moderated by the extent to which individuals experience cognitive dissonance between the official regime propaganda which they must parrot, and the world around them that they actually experience. We expect that this dissonance will be greater for individuals who did not benefit from the Soviet regime[3]. Conversely, cognitive dissonance should be less pronounced among individuals who did benefit from it. These individuals may perceive less dissonance or may be more inclined to discount information that contradicts official doctrine. Taking this into account, we argue that individuals exposed to indoctrination but not benefiting from the regime are significantly less likely to support authoritarianism following transitions to democracy, and are more likely to support democracy instead.

Hypothesis 2 *Benefiting from the Soviet regime moderates the effect of educational indoctrination on support for democracy. Individuals who did not benefit are less susceptible to indoctrination, resulting in lower support for (Soviet) authoritarianism and higher democratic preferences, compared to those who did benefit from the regime.*

Soviet Regime Benefits

An important concern we encounter is how to best assess whether a person benefited from the Soviet regime or not. Given that our data is retrospective – collected after the collapse of the Soviet regime – it is difficult to rely on subjective measures of perceived regime benefits, as these may capture Soviet benefits, or any other confounding factors that may affect democratic preferences. Importantly, any benefits from the Soviet regime would have been received after the 'treatment' of Soviet education, which began in the late 1940s. To address this, we leverage our knowledge of specific educational

[2]Havel (1978) refers to this as 'living a lie' and 'living the truth'.

[3]We define regime benefits as any advantage that enhances an individual's quality of life, by improving their well-being in measurable ways. These benefits can be tangible, such as higher salaries or improved housing, or intangible, such as elevated social status or increased political influence.

trajectories of individuals. In particular, we consider whether a person completed specialized technical secondary education, in the so-called "Tekhnikum." Tekhnikums were specialized secondary schools that trained semi-professionals, such as technicians, machine operators, or nurses, for specific industries (Titma and Saar 1996). While Tekhnikums provided graduates with certification that allowed them to pursue higher university education, the majority ended their formal education upon graduating from these specialized secondary schools and entered the industrial sectors for which they were trained (Titma and Saar 1995).[4]

Tekhnikums were a crucial source of skilled technical semi-professional labor essential to the Soviet system, particularly during its industrial expansion in the immediate post-war period (Jenkins 1990). During this time, the Soviet regime placed great emphasis on widespread industrialization, relying heavily on the implementation of mechanization and process automation (Simon 1954). Tekhnikums were so central to the provision of technical labor that they were established and operated by individual sector ministries, which recruited employees for state-run production directly from among their graduates (Peters 1956). Consequently, students of these schools were tracked from the age of 15 and trained in these specialized institutions to prepare them for their future roles in the Soviet system (Titma and Saar 1995). These graduates represented, in a sense, the vanguard of the Soviet regime. They were in high demand within a system taking pride in its rapid industrial expansion, and its symbolic elevation of the small (these were not university-educated engineers, nor regime elites), but technically competent machine operators.

The Soviet system effectively elevated the social status of these technical semi-professionals in both social and economic terms. As **Figure 1** demonstrates, technical secondary school graduates were over-represented in the third income quartile, underlining their relatively privileged economic position. Furthermore, Jenkins (1990:

[4]In our sample, we identify only those respondents whose highest level of education is the Tekhnikum, thus excluding any respondents who went on to complete higher degrees.

59) reports that a large percentage of technical students "see technical education as providing a basis for social mobility...", a claim supported by our data. Specifically, 82.2% of technical school graduates reported achieving a higher level of education than their parents, compared to only 69.0% among other educational groups.

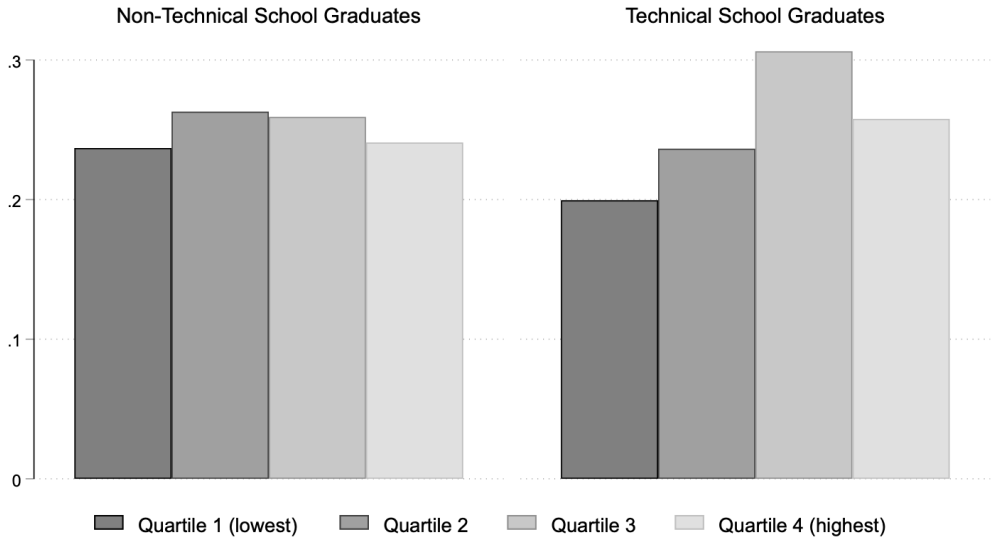
We thus argue that graduating from a Tekhnikum, an outcome largely determined by one's education choice prior to the age of 15[5], and which selects an individual into a practically useful, economically rewarded, and symbolically elevated social group in the Soviet system, is a reasonable operationalization of benefiting from the Soviet regime. Our data underline the appropriateness of this choice by showing that Tekhnikum graduates' retrospective evaluation of the Soviet regime is significantly more positive than those of other groups. **Table 1** empirically shows that technical school graduates are significantly more likely to report having benefited from the Soviet system than others. This demonstrates that, despite the choice to pursue technical education being made prior to the end of one's elementary education (before the year in which one turns 15), its effect has a long-lasting influence.

Our theory rests on two core expectations. First, children who experienced Soviet elementary education (turned 15[6] on or after the Soviet occupation of 1944), were exposed to Soviet indoctrination, and their support for democracy should be on average lower than that of children who did not experience Soviet elementary education (turned 15 before 1944). Second, the effect of Soviet indoctrination is conditioned by individuals' lived experiences under the Soviet regime. Among those who experienced Soviet schooling, indoctrination is expected to be particularly effective for individuals who benefited from the regime – a condition we proxy through attendance at technical secondary schools (Tekhnikums) – but not necessarily for those with different educational (and thus occupational) trajectories.

[5]This is critical from the point of view of our method, as this selection occurs prior to the treatment of completing basic education.

[6]Fifteen is the age at which one completes their elementary education.

Figure 1: Average Individual Income Distribution by Non-Technical School and Technical School Graduates



To summarize, we expect the impact of indoctrination to be significant for those who benefited from the Soviet regime (i.e., those who attended technical secondary schools after the Soviet takeover), resulting in lower support for democracy, and higher support for authoritarianism[7]. Those who did not benefit from the regime (i.e., those who did not attend technical secondary schools) should be more likely to experience cognitive dissonance between Soviet indoctrinating propaganda and their lived realities. For this group, we expect the effect of Soviet elementary education to attenuate towards zero, and these individuals to have similar views of democracy as those who did not experience Soviet indoctrination at all.

[7]It is important to note that the expectation that Tekhnikum graduates should have lower support for democracy is a strong test of our theory, given that Tekhnikum graduates tended to have above median income (see *Figure 1*), which should – according to most expectations – generally increase their democratic proclivity (Boix 2003; Kitschelt 2002).

Table 1: Technical School Respondents and Benefits' Distribution

	Benefits 1	Benefits 2	Benefits 3
	(1)	(2)	(3)
Technical School	0.194**	0.132*	2.953*
	(0.06)	(0.05)	(1.34)
Observations	11,475	11,475	10,363
R^2	0.029	0.025	0.038
Controls	Yes	Yes	Yes

Note: The results represent the perception of regime benefits among those who attended technical schools. The table reports estimates from OLS regressions. *Benefits 1* corresponds to respondents who claim that, when comparing the overall economic situation of their household with what it was before the major economic transformation following the democratic transition, the past was much better or a little better. *Benefits 2* corresponds to respondents who, in the same question, claim that the past was much better. *Benefits 3* corresponds to respondents' placement of the Socialist economic system before independence on a scale for ranking how well the economic system worked (100 being the best; -100 being the worst). All respondents included were born in the Baltic states. R^2 values for Models (1) and (2) correspond to pseudo R^2, as they are logistic models. All models include survey-country and survey-year fixed effects, and control for the age and gender of respondents. Full tabular results are provided in *Appendix A*. Robust standard errors are in parentheses. $^{\dagger}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$.

Data

Our empirical design utilizes data from the New Baltic Barometer I-VI Trend Dataset, spanning six waves from 1993 to 2004[8] (Rose 2010). These surveys were conducted in the three Baltic states – Estonia, Latvia, and Lithuania. They were administered in either a Baltic language or Russian, depending on the respondents' preference (Rose 2000). Our analysis focuses exclusively on respondents who answered the survey in one of the Baltic languages, as opposed to Russian. Given that the place of birth is not recorded for all respondents, we use the language divide to exclude Russian speakers

[8]Round I in 1993, round II in 1995, round III in 1996, round IV in 2000, round V in 2001, and round VI in 2004.

from the analysis, keeping only speakers of one of the Baltic languages. We do this to minimize potential selection bias from individuals born in Russia who relocated to the Baltics during the occupation (Rose 2000: 3). Including Russian speakers would introduce two main challenges: first, the nationalistic effect, with the expectation that Russians might be more pro-Soviet, and therefore, more anti-democratic than Baltic-speaking respondents; second, these individuals might have been exposed to Soviet indoctrination through schooling in Russia, complicating our analysis since the surveys do not specify where the respondents received their education. Our analysis encompasses cohorts who finished their compulsory education between 1909 and 2001. In order to correct for non-responses and the potential over-representation of respondents from certain regions (e.g., urban centers), we include survey weights that account for these differences[9] (Cox 2024).

Democratic Preferences

Our outcome variables, reflecting respondents' democratic preferences, are measured using three different approaches. First, we broadly assess democratic versus autocratic preferences. The first outcome variable is assigned a value of one if respondents indicate a preference for democracy over any other form of government, including authoritarianism or indifference. Similarly, the second outcome variable is assigned a value of one if respondents prefer democracy over specific types of autocratic government, such as the restoration of the Communist regime, military rule, rule by a strongman, and suspension of parliament. While the first two dependent variables broadly gauge pro-democratic preferences, the third outcome variable more narrowly assesses these preferences by specifically comparing them to Soviet Union authoritarianism. This third variable, also dichotomous, is assigned a value of one for respondents who either "strongly disagree" or "somewhat disagree" with the statement,

[9]It is important to note that survey weights were calculated without using any endogenous post-treatment variables (e.g., income, education, marital status, etc.). They were calculated solely based on urban/rural residence, gender, age, and region (Rose 2005).

"we would be better off if we were still part of the Soviet Union." This assessment occurs within the context of the question, which acknowledges the existence of the current democratic system[10].

Regime Benefits

As previously mentioned, the reception of regime benefits is identified based on respondents' choice of attending or not attending technical schools. This decision is made pre-treatment, before the completion of respondent compulsory education. Respondents who indicate that their highest level of education is "technical college" are considered part of the regime benefits sub-group, while all other respondents comprise the non-benefits sub-group.

Indoctrination

Due to the challenges in measuring indoctrination (Neundorf et al. 2024), we take advantage of the exogenous variation in exposure to compulsory schooling before and after the Soviet occupation of the Baltic states in 1944 to establish our treatment and control groups. They correspond to cohorts either exposed or not exposed to Soviet indoctrination through the educational system. We detail our empirical strategy in the following section.

Other Variables

Because our treatment focuses on the timing of completion of mandatory education, some authors have cautioned that there is a risk of introducing bias by including control variables that are themselves products of education (Marshall 2016; Cavaille and Marshall 2019). Therefore, to mitigate this risk, we control only for a pre-treatment variable accounting for the gender of respondents. Additionally, we incorporate survey-country

[10]This question starts by stating that "our present system of government is not the only one that this country has had," highlighting the contrast between the current democratic system and the previous, non-democratic Soviet Union system of government.

and survey-year fixed effects, as well as covariates for town size at the time of the survey.

Empirical Strategy

We employ a Regression Discontinuity design to estimate the causal impact of Soviet indoctrination through education on democratic preferences. To understand the shaping of these preferences, we analyze two sub-samples of respondents: those who benefited from the regime by graduating from technical schools, and those who did not. This approach allows us to examine how the presence of regime benefits moderates the effect of indoctrination on post-transitional democratic preferences. For each sub-sample, we construct an RD design that distinguishes between cohorts who finished their mandatory schooling before the occupation – being too old to have experienced Soviet education; and those who completed their education post-occupation – who were sufficiently young to be influenced by the Soviet educational system. We take advantage of the essentially exogenous timing of respondents' birth years, which determines the completion of their mandatory education at age of 15 (Titma and Saar 1995). This research strategy aligns with a broader field of studies that investigate shifts in political attitudes just before and immediately following the implementation of new educational reforms (see, for example: Cavaille and Marshall 2019; Marshall 2016).

We define our treatment — being exposed to mandatory Soviet schooling — for respondent i from cohort j in country c as:

$$Treatment_{jc} = \begin{cases} 0 & \text{if year completion of mandatory education}_{jc} < 1944, \\ 1 & \text{if year completion of mandatory education}_{jc} \geq 1944 \end{cases} \tag{1}$$

Then, we fit our model:

$$y_{ijc} = \beta \text{ Treatment}_{jc} + f(x_{jc}) + \varepsilon_{ijc}, \tag{2}$$

where y_{ijc} corresponds to the three dependent variables capturing respondents' preference for democracy, $f(x_{jc})$ is a function of the running variable x_{jc}: year of completion of mandatory education. f is a local linear regression using the Calonico, Cattaneo, and Titiunik (2014) recommended optimal bandwidth and default triangular kernel, as also recommended by Imbens and Lemieux (2008).

We identify two treatment groups and two control groups, each corresponding to one of our two sub-samples. As mentioned, the first sub-sample includes respondents who attended and graduated from technical schools, having pursued careers associated with higher levels of benefits, and the second sub-sample includes respondents who did not attend Tekhnikums after completing their mandatory elementary education and therefore did not necessarily benefit from the regime (see **Table 2**). In the first sub-sample, the treatment group consists of respondents who were exposed to Soviet schooling – and therefore to indoctrination – by completing mandatory education after 1944. The control group includes those who attended technical schools but were not exposed to Soviet education, having completed mandatory education before 1944. This cohort of individuals completed their mandatory education just prior to the Soviet occupation, finishing in 1943, and subsequently enrolled in Tekhnikums in 1944 – the year of their establishment[11]. Similarly, in the second sub-sample, the treatment group consists of those who finished mandatory schooling after 1944, being exposed to Soviet compulsory education (and indoctrination), while the control group includes those who were not exposed to Soviet schooling, having finished mandatory elementary schooling before 1944.

[11]It is plausible that these individuals were already predisposed towards technological fields and chose to enroll in Tekhnikums as soon as they became available in 1944. Alternatively, they may have initially enrolled in conventional secondary schools and later transferred to Tekhnikums.

Table 2: Treatment and Control Groups across Sub-samples

	Treatment Group (exposure to Soviet Schooling)	**Control Group** (no exposure to Soviet Schooling)
Sub-sample 1: Technical School Graduates	Benefits and Indoctrination	Benefits Only
Sub-sample 2: Remaining Population	Indoctrination Only	Neither

Validity

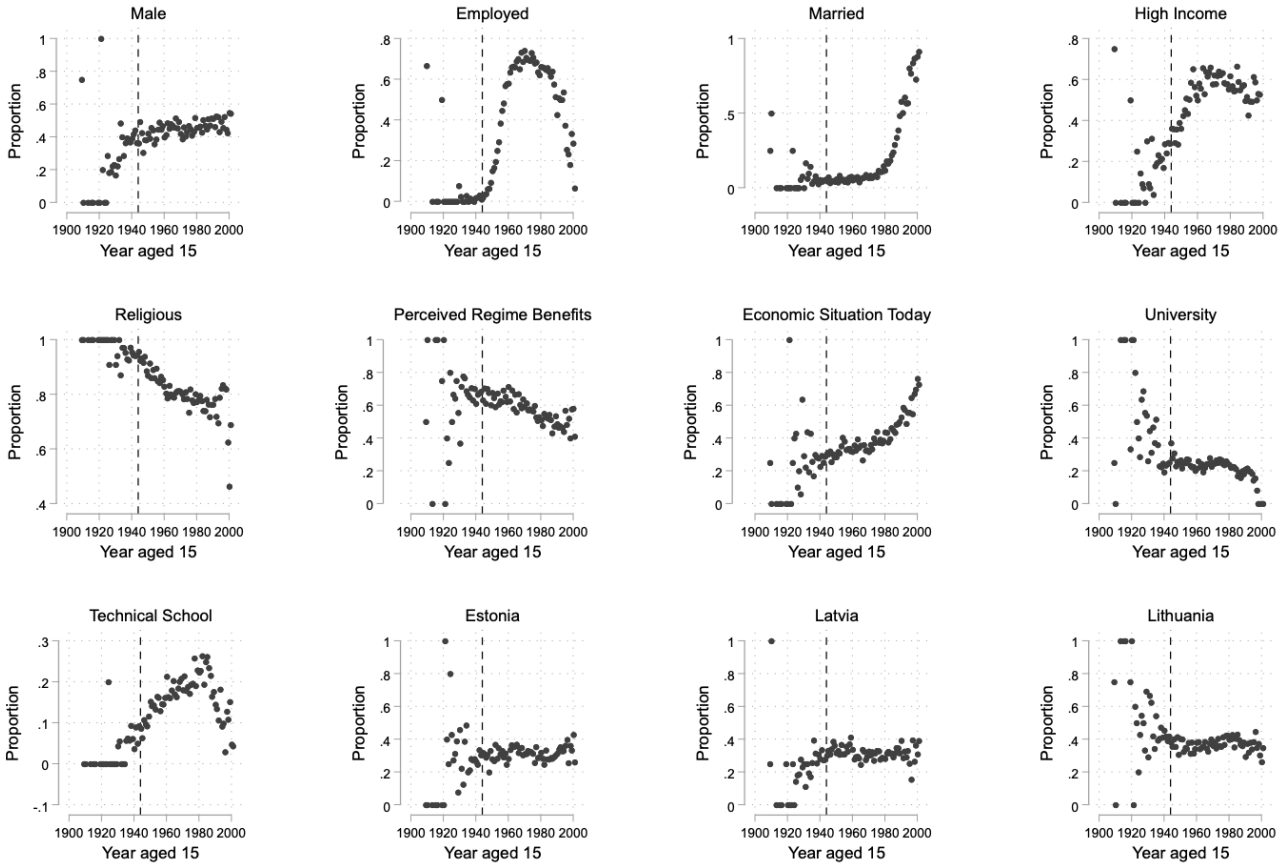
A fundamental assumption of RD designs is that observations cannot influence the variable that assigns them to different groups. Although it seems improbable that parents could have predicted the Soviet occupation well over a decade before their child's birth, we still investigate potential manipulation of the assignment variable. To bolster this assumption, we conduct a McCrary test in order to investigate whether there is a discontinuity in the number of observations around the threshold. Following McCrary (2008), such discontinuity can suggest that subjects adjust their behavior in anticipation of expected changes, indicating manipulation of the assignment variable. We validate this assumption in *Appendix B*. As shown in **Figure 5** of the appendix, the McCrary test on the running variable reveals no significant difference in density at the discontinuity. According to the results of this test, the difference in density at the cutoff point is -0.008 (insignificant p-value of -0.106). Furthermore, we also perform the density test proposed by Cattaneo, Jansson, and Ma (2020), which similarly shows no significant difference in density, with an insignificant p-value of 0.690.

A second concern in RD designs is the potential endogeneity of covariates, which can affect the results. Specifically, if covariates influencing treatment assignment are themselves affected by or correlated with the treatment, it can introduce bias. Since the validity of RD hinges on the premise that groups on either side of the treatment threshold are comparable, it is vital to ensure these observable characteristics are evenly balanced around the threshold. This assessment helps to ensure any observed differences in outcomes are truly attributable to the treatment rather than to imbalances in covariates or underlying group differences. We run tests on a battery of

demographic and socio-economic observational characteristics that can also potentially predict democratic preferences. Some are related to income (Boix 2003; Kitschelt 2002), education (Cavaille and Marshall 2019), religion (Huber and Stephens 2012), gender (Hansen and Goenaga 2021), marital status (Norris and Inglehart 2019), and nationality (Erhardt, Wamsler and Freitag 2021).

Figure 2 confirms that these demographic and socio-economic observational characteristics are essentially continuous, not differing significantly across the threshold. Hence, it can be inferred that respondents on either side of the discontinuity generally exhibit statistical similarity in the relevant covariates. This implies that individuals who received the treatment are not statistically distinguishable from those who did not, based on the observed characteristics.

Figure 2: Balance Test Plots for Relevant Covariates



Note: The selected observational variables are binary and indicate whether respondents are (from top-left to bottom-right): male, employed full-time at the time of the survey, married, have an individual monthly income in the third or fourth quartiles (high income), belong to a religious group (as opposed to being non-believers), perceived as having benefited during the Soviet regime, rate their current household economic situation as very or fairly satisfactory, have completed university, have completed technical school education, and were interviewed in each of the Baltic countries. Full tabular results are presented in *Appendix B*, Table 8.

Main Results

The results in **Table 3** provide robust causal evidence that exposure to the indoctrinating effects of Soviet schooling results in a lower preference for democracy only among cohorts who also graduated from technical schools during the Soviet regime. This seems to indicate that ideological indoctrination via the educational system is only effective if respondents also benefited from the regime, thus experiencing low levels of cognitive dissonance between what the regime preaches and promotes, and their own socioeconomic well-being and symbolic recognition under the regime. Our coefficients of interest represent the effect on democratic preferences of exposure to indoctrination through the Soviet schooling system, compared with exposure to no indoctrination. *Models 1-3* restrict the analysis to respondents who attended technical schools, pursuing careers associated with the distribution of greater regime benefits during the Soviet regime, while *Models 4-6* correspond to those who did not attend technical schools, and thus, were less likely to be exposed to the same level of regime benefits.

Specifically, as indicated in *Column 1*, being exposed to Soviet indoctrination while simultaneously receiving regime benefits is associated with a 1.032-point decrease in democratic preferences compared to authoritarian preferences. Conversely, as shown in *Column 4*, for those cohorts who did not pursue careers associated with a greater distribution of regime benefits, the results are considerably smaller and statistically insignificant. Similarly, results in *Column 2* show that when comparing democracy against other authoritarian regime types (e.g., Communist regime, army rule, rule by a strongman, and suspension of parliament), respondents who were exposed to Soviet indoctrination in schools tend to prefer democracy 0.315 points less if they were also exposed to regime benefits. Conversely, in *Column 5*, we see that this effect does not apply to those who did not receive regime benefits by enrolling in technical schools. For this sub-group, the results are smaller and statistically insignificant, akin to the findings in *Column 4*. The results of these two columns can be visualized graphically in **Figure 3**.

Table 3: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on Regime Benefits' Distribution

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-1.032*	-0.315*	-0.138**	-0.138	-0.049	0.017
	(0.44)	(0.23)	(0.11)	(0.28)	(0.08)	(0.05)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
Bandwidth	6.820	6.337	4.385	6.366	7.385	7.521

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

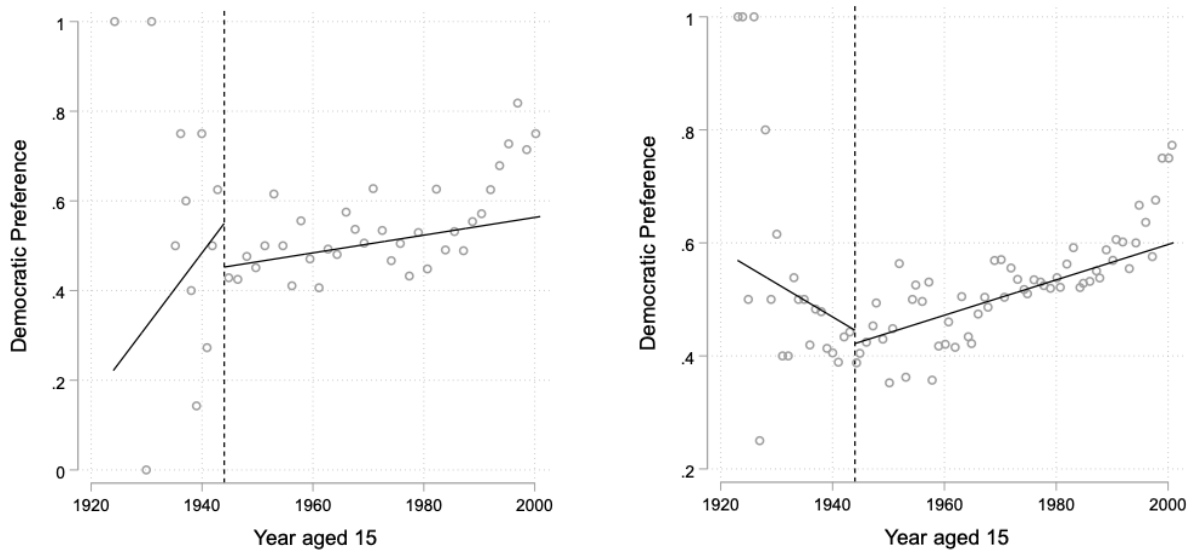
The consistency of these results remains when examining the effects of the treatment on the preference for democracy specifically when considered in opposition to Soviet Union authoritarianism. *Column 3* indicates that respondents who benefited and were exposed to Soviet schooling indoctrination are 0.138 points more likely to prefer the Soviet autocratic regime over the current democracy. *Column 6* shows that the results for those who were indoctrinated but did not experience regime benefits are statistically insignificant. Interestingly, the results in this column, although insignificant, are positive. While this model does not allow us to make a causal claim, it suggests a potential avenue for future studies. It is possible that when respondents are asked specifically whether they prefer Soviet authoritarianism or democracy, the cognitive dissonance associated with indoctrination paired with the lack of benefits is so high that these respondents actually exhibit positive democratic preferences compared to their preference for the Soviet regime. This could imply that cognitive dissonance produces a negative effect on preference for Soviet authoritarianism (a "punishment effect"), and consequently, a more positive preference for democracy.

When combined, these results suggest that treated respondents who pursued career

paths associated with a higher distribution of benefits during the Soviet regime, and who indeed perceive themselves as having derived high levels of benefits from such regime (see *Table 1*), are less likely to prefer democracy compared to treated respondents who did not follow such career paths, and who believe they did not benefit more during the regime than in the present day. Furthermore, the results indicate that indoctrination through Soviet schooling has a detrimental effect on democratic preferences—sometimes referred to as the "nostalgia for the past" effect—only when respondents follow educational paths leading to greater benefits during the regime.

These findings illuminate the mechanisms through which indoctrination operates, suggesting that education-based indoctrination is effective only when respondents have a baseline of material security provided by the regime. In fact, among those who did not follow educational paths leading toward greater benefits, the results are statistically insignificant, indicating that indoctrination alone may have no effect at all due to the presence of cognitive dissonance. More importantly, these results shed light on how autocratic legacies can undermine democratic attitudes following democratic transitions, particularly when autocrats leverage regime benefits to reinforce the effects of indoctrination.

Figure 3: Effect of Soviet Exposure During Schooling Years on Democratic Preferences By Benefits Status



Note: *Model 2* represented on the **left**; *Model 5* represented on the **right**.

Cohort Analysis

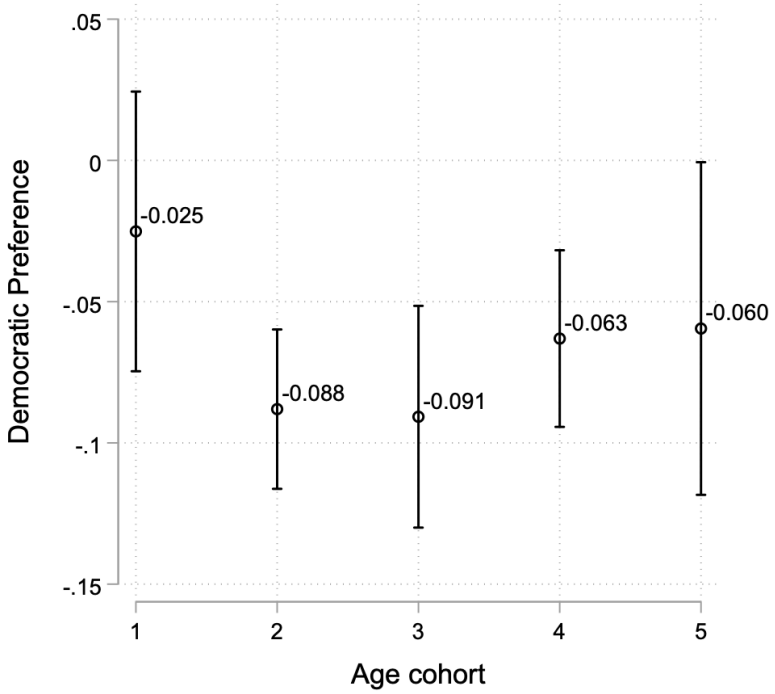
To complement the causal quasi-experimental RD analysis, we also conducted an ordinary least squares (OLS) marginal analysis aimed at examining the perception of regime benefits by age cohort, assuming that different cohorts were exposed to varying levels of indoctrination. This analysis focuses on the perception of regime benefits during the Soviet regime by survey respondents, and how the marginal effect of these benefits affects democratic preferences.

The results, depicted in **Figure 4**, suggest that the level of indoctrination plays a crucial role in how benefits influence democratic preferences. In the first cohort (baseline), which was not exposed to indoctrination, the marginal effect of regime benefits on democratic preferences is small and statistically insignificant. In cohorts exposed to higher levels of indoctrination (cohorts 2 and 3), the marginal effect of benefits is greater, leading these cohorts to prefer democracy less compared to cohorts

who were less exposed to indoctrination. In cohorts exposed to medium levels of indoctrination after Stalin's death (cohorts 4 and 5), the marginal effect of benefits on the outcome is smaller than in the previous two cohorts but still relatively high and statistically significant.

These results are consistent with the RD analysis, indicating that indoctrination and regime benefits have a cumulative and reinforcing effect on democratic preferences, with indoctrination being statistically insignificant when considered alone.

Figure 4: Cohort Analysis: The Impact of Regime Benefits on Democratic Preferences



Note: The dependent variable corresponds to a measure of democratic preference *versus* Soviet authoritarianism (*DV3*). Results represent the average marginal effect (OLS) of regime regime benefits (measured by the variable *Benefits 2*) for different age cohorts of 15 years each. *Cohort 1* (born before 1929) was exposed to 0 years of Soviet-school indoctrination. *Cohort 2* ($1929 \geq$ born < 1944) was fully exposed to indoctrination during its mandatory schooling years. Indoctrination was highest during this period as most of it was before the death of Stalin in 1953. *Cohort 3* ($1944 \geq$ born < 1959) was born after the second Soviet occupation and fully exposed to Soviet indoctrination. *Cohort 4* ($1959 \geq$ born < 1974) was exposed to Soviet indoctrination, but during the early 1960s, Stalinism began to be discredited and the first challenges to the Soviet empire emerged (Mishler and Rose 2007). *Cohort 5* (born on or after 1974) experienced less indoctrination, growing up during *perestroika*. The results of the fitted and marginal robust models are displayed in *Appendix C*. All models include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Confidence intervals are at the 95% level.

Robustness

To further highlight the validity of our findings, this section addresses three alternative mechanisms that could plausibly be associated with decreased democratic preferences following the second Soviet occupation of the Baltic States in 1944: the first Soviet occupation in 1940, the Nazi occupation of 1941, and, finally, decreased satisfaction with economic conditions at the time the surveys were carried out, which could potentially be endogenous to the RD main results. Additionally, following Cavaille and Marshall (2019), we also test a placebo reform occurring five years before our main cutoff date of 1944. The results of the robustness checks[12] clearly indicate that neither the two prior occupations of the Baltics, nor the effect of current socioeconomic conditions, nor the placebo reform, replicate our main findings. The statistically significant differences are broadly consistent with chance and do not interfere with our main estimates. We also demonstrate that the estimates remain robust overall when using different bandwidth selectors and sizes, alternative kernel functions (see *Tables 12 - 16* in *Appendix E*). Finally, in order to create a more homogeneous control group, we restrict it to include only vocational secondary school graduates and traditional secondary school graduates (*Appendix F*).

First Soviet Occupation of 1940

The Baltic States were first occupied by the Soviet Union in 1940. As previously mentioned, although the Soviet Union initiated a wave of terror, it was unable to implement significant policies, particularly in the sphere of education, due to the interruption by the Nazi occupation in 1941. It was only after the Soviet re-occupation in 1944 that sweeping educational changes were introduced, along with substantial

[12]Due to sample constraints, the tests were run on the two latter dependent variables (*DV2* and *DV3*). Nevertheless, we believe that the second outcome is constructed similarly to the first outcome variable, as they are both broad measures of democratic preference versus authoritarian preference. Only *DV3* corresponds to the opposition between democratic preferences and Soviet authoritarianism, in particular.

indoctrination. Despite the limited success of implementing indoctrination through the educational system during this period, we test the effect of the first occupation to ensure that the long-lasting exposure to indoctrination via schooling during the second occupation is driving the results, rather than the very limited exposure to indoctrination from 1940 until the Nazi occupation in 1941. The statistically insignificant results in all models in **Table 4** confirm these expectations.

Table 4: Robustness Test A. – Effect of the First Soviet Occupation of 1940

	Benefits		No Benefits	
	DV2	DV3	DV2	DV3
	(1)	(2)	(3)	(4)
Soviet Schooling Effect	0.457	-0.278	-0.013	-0.013
	(0.43)	(0.21)	(0.09)	(0.07)
Observations	1,960	1,263	7,565	3,664
Outcome Mean	0.489	0.797	0.489	0.797
Bandwidth	4.994	4.285	8.557	7.306

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. The cut-off was changed to 1940, the year of the first Soviet Occupation of the Baltic States, *ceteris paribus*. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Nazi Occupation of 1941

Similarly, we test the effects of the Nazi occupation that began in 1941 and lasted until the second Soviet occupation. Although it is likely that ideological indoctrination was implemented through the schooling system under Nazi rule, technical schools were a feature of the Soviet system and did not exist in the same form during the Nazi occupation. Therefore, the results should not capture these regime benefits link with respondents who were graduates of technical schools. Moreover, the effects on the dependent variable relating to the opposition between democratic preferences and Soviet authoritarianism (*DV3*) should be statistically insignificant, as Nazi indoctrination is not expected to

influence respondents' views on Soviet authoritarianism.

Results in **Table 5** confirm these expectations. All models are statistically insignificant when holding the threshold at the 0.05 level, the required level for strong statistical significance. The result in *Model 1* can be explained by chance, since, as mentioned, technical schools were only characteristic of the Soviet educational system and do not necessarily capture benefits by the Nazi regime.

Table 5: Robustness Test B. – Effect of the Nazi Occupation of 1941

	Benefits		No Benefits	
	DV2	DV3	DV2	DV3
	(1)	(2)	(3)	(4)
Soviet Schooling Effect	-0.491†	-0.098	0.045	-0.056
	(0.41)	(0.25)	(0.09)	(0.04)
Observations	1,960	1,263	7,565	3,664
Outcome Mean	0.489	0.797	0.489	0.797
Bandwidth	4.713	4.614	8.324	9.155

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. The cut-off was changed to 1941, the year of the Nazi Occupation of the Baltic States, *ceteris paribus*. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Effect of other Potential Endogenous Variables

We also investigate whether the results are simply driven by respondents' economic satisfaction at the time of the survey. Since economic dissatisfaction can negatively impact democratic preferences (Kitschelt 2002), we examine whether the combination of indoctrination and regime benefits may have caused post-transitional economic dissatisfaction in respondents, which may be endogenous to democratic preferences. To determine if the second Soviet occupation impacted respondents' current economic satisfaction, we estimate the effect of exposure to indoctrination on economic satisfaction at the time of the survey for both technical school graduates, and others, as

shown in **Table 6**. The insignificant results indicate that the findings are not explained by respondents' economic satisfaction at the time of the survey, which could be endogenous to negative democratic preferences.

Table 6: Robustness Test C. – Effect of other Potential Endogenous Variables: Economic Situation Today

	Benefits	No Benefits
	Material Today	Material Today
	(1)	(2)
Soviet Schooling Effect	0.072	0.034
	(0.26)	(0.06)
Observations	1,873	10,344
Outcome Mean	0.343	0.343
Bandwidth	5.125	8.107

Note: The dependent variables correspond to a measure of economic satisfaction today. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Finally, the results of the placebo test occurring five years before the second Soviet occupation are also statistically insignificant, thus not interfering with the results of our main analysis (see *Appendix D*).

Conclusion

The social science literature is increasingly interested in the long-term effects of authoritarianism. While this body of work has established that exposure to autocratic indoctrination can significantly shape political preferences and behavior in post-transitional contexts, the specific underlying mechanisms remain underexplored. This article contributes to this literature by analyzing the conditional effect of authoritarian indoctrination on democratic preferences. It argues that indoctrination is particularly effective in shaping long-term preferences when delivered through early socialization in elementary education. Additionally, and more importantly, it highlights

the moderating influence of individuals' lived experiences under the regime on the enduring effects of indoctrination over time.

This article empirically demonstrates that indoctrination works conditionally in the context of individuals receiving significant benefits from the indoctrinating authoritarian regime. When the indoctrinating content of regime propaganda aligns with the lived experiences of individuals, indoctrination is significantly more likely to work than in cases where individuals experience cognitive dissonance between the heroic claims of the regime, and their lives. We make our case by examining the impact of the imposition of Soviet elementary education in the Baltic countries following their second Soviet occupation in 1944, while also considering the economic and symbolic benefits received by graduates of technical secondary schools during this period. We provide two important findings. First, that children exposed to elementary Soviet education (those who turned 15 on or after 1944) are less likely to support democracy than those who completed elementary education prior to the Soviet occupation (those who turned 15 before 1944). Second, we show that individuals exposed to Soviet indoctrination via elementary education, nonetheless, do not succumb to it uniformly. Those who are socioeconomically and symbolically elevated to the vanguard of the regime by virtue of completing secondary technical schooling, tracking them into growing Soviet industries, benefit from the regime, internalize their early indoctrination, and exhibit lower levels of democratic support. Those who follow other life paths likely encounter cognitive dissonance between classroom propaganda and their lived experience, which undermines the influence of earlier indoctrination, and produces higher levels of democratic support. Importantly, we show that our findings are robust to diverse types of model specification.

Our findings have significant implications for the study of indoctrination specifically, and authoritarian regime legacies more broadly. First, we reaffirm the established understanding that authoritarian indoctrination is particularly effective when integrated into early socialization through elementary education. More importantly, we challenge the conventional assumption that the effects of educational indoctrination are uniform.

We demonstrate that the impact of indoctrination is non-uniform and conditional, being more effective when individuals' lived socioeconomic experiences align with the claims made by propagandist narratives. Second, and of greater significance, this article deepens our comprehension of how the legacies of autocratic regimes affect political attitudes in the aftermath of transitions, with a particular focus on democratic attitudes. By identifying the conditions that allow indoctrination to persist after regime change, this study provides valuable insights into the development of democratic attitudes in emerging democracies, which are crucial for fostering democratic consolidation and mitigating the risks of democratic backsliding.

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Online Appendix

Inhibiting Indoctrination:

Autocratic Benefits and the Limited Effects of Indoctrination after Democratization

Contents

1 Appendix A: Table 1 – Full Results

Table 7: Technical School Respondents and Regime Benefits' Distribution

	Benefits 1	Benefits 2	Benefits 3
	(1)	(2)	(3)
Technical School	0.194**	0.132*	2.953*
	(0.06)	(0.05)	(1.34)
Male	-0.155**	-0.176**	-3.348**
	(0.04)	(0.04)	(0.96)
Age	0.013**	0.013**	0.173**
	(0.00)	(0.00)	(0.03)
Constant	-1.435**	-0.366**	9.850**
	(0.08)	(0.07)	(1.75)
Observations	11,475	11,475	10,363
R^2	0.029	0.025	0.038
Controls	Yes	Yes	Yes

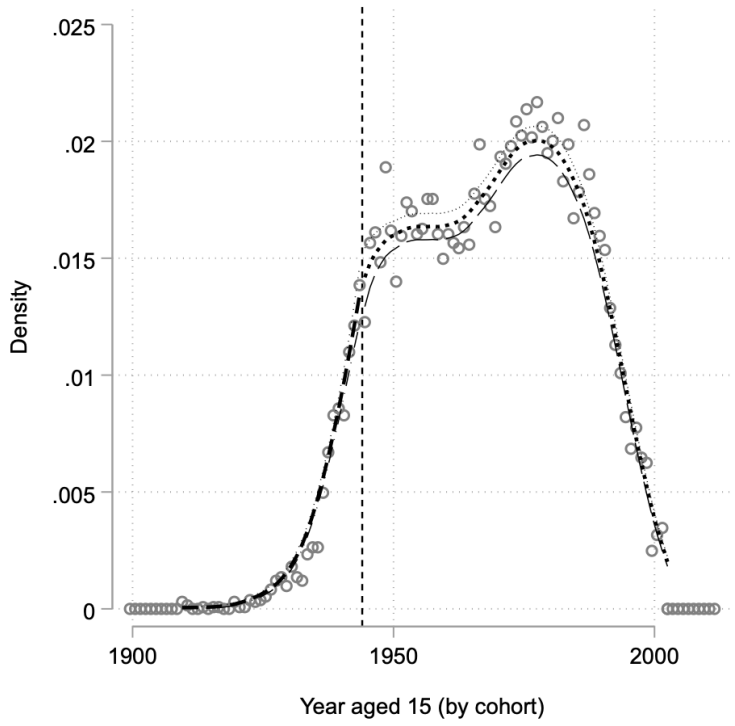
Note: The results represent the perception of regime benefits among those who attended technical schools. The table reports estimates from OLS regressions. Interpretation of *Benefits 1-3* is the same as in *Table 1*. R^2 values for Models (1) and (2) correspond to pseudo R^2, as they are logistic models. All models include survey-country and survey-year fixed effects. Robust standard errors are in parentheses. $^{\dagger}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$.

2 Appendix B: RDD Identification Assumptions Tests

2.1 Density Tests

The graphical depiction of the McCrary density test (McCrary 2008) is shown in **Figure 5**. This test finds no significant difference in density at the discontinuity.

Figure 5: McCrary Density Test



2.2 Covariates Balance Test (Continuity)

The tabular results of **Figure 2** are presented in Table 8. Here, we show that respondents on either side of the discontinuity generally exhibit statistical similarity in the relevant covariates. This implies that individuals who received the treatment are not statistically distinguishable from those who did not, based on the observed characteristics.

Table 8: Covariates Balance Test (Continuity)

	(1) Male	(2) Employed	(3) Married	(4) High Income	(5) Religious	(6) Perceived Regime Benefits
Soviet Schooling Effect	0.011	0.004	0.001	0.005	0.004	-0.023
	(0.05)	(0.02)	(0.03)	(0.05)	(0.03)	(0.06)
Observations	13,275	13,243	13,236	9,604	12,548	10,788
Bandwidth	9.033	5.371	7.516	13.093	8.941	7.935

(*continuation*)	(7) Economic Situation Today	(8) University	(9) Technical School	(10) Estonia	(11) Latvia	(12) Lithuania
Soviet Schooling Effect	0.021	0.077†	-0.008	-0.018	0.043	-0.026
	(0.05)	(0.05)	(0.03)	(0.05)	(0.05)	(0.04)
Observations	12,835	13,225	13,225	13,286	13,286	13,286
Bandwidth	9.211	7.681	11.721	10.350	10.020	12.441

Note: Models represent the total effect of Soviet exposure during schooling years on the various covariates. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

3 Appendix C: Cohort Analysis – Tabular Results

Table 9: Cohort Analysis – Logistic Model

	Democratic Preference (*DV3*)
	(1)
Benefits	-0.380
	(0.40)
Cohort 2	0.767†
	(0.46)
Cohort 3	-0.261
	(0.40)
Cohort 4	-0.175
	(0.37)
Cohort 5	-0.108
	(0.46)
Benefits * Cohort 2	-1.122*
	(0.52)
Benefits * Cohort 3	-0.541
	(0.47)
Benefits * Cohort 4	-0.349
	(0.45)
Benefits * Cohort 5	-0.343
	(0.55)
Male	0.069
	(0.10)
Constant	4.336**
	(0.39)
Observations	4,859
R^2	0.106
Controls	Yes

Note: The dependent variable corresponds to a measure of democratic preference *versus* Soviet authoritarianism (*DV3*). Regime benefits (variable *Benefits 2*) were interacted with each age cohort as described in *Figure 4*. Logistic model includes survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Table 10: Cohort Analysis – Average Marginal Effects Model

	Democratic Preference (*DV3*)
	(1)
Benefits = 1	
Cohort 1	-0.025
	(0.03)
Cohort 2	-0.088**
	(0.01)
Cohort 3	-0.091**
	(0.02)
Cohort 4	-0.063**
	(0.02)
Cohort 5	-0.060*
	(0.03)
Observations	4,859
Controls	Yes

Note: Results represent the average marginal effect of regime benefits (measured by the variable *Benefits 2*) for different age cohorts of 15 years each, as described in *Figure 4*. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

4 Appendix D: Placebo Test: Occupation 5 Years Before 1944

Table 11: Robustness Test D. – Effect of Placebo Occupation in 1939

	Benefits		No Benefits	
	DV2	DV3	DV2	DV3
	(1)	(2)	(3)	(4)
Soviet Schooling Effect	0.246	0.150†	-0.030	-0.034
	(0.45)	(0.22)	(0.11)	(0.07)
Observations	1,960	1,263	7,565	3,664
Outcome Mean	0.489	0.797	0.489	0.797
Bandwidth	3.767	2.349	7.873	7.444

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. The cut-off was changed to 1939, five years before the second Soviet Occupation of the Baltic States, *ceteris paribus*. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

5 Appendix E: Bandwidth and Kernel Modifications

5.1 E.1: Modifying the Bandwidth Selector

In this subsection we modify the bandwidth to the CER-optimal bandwidth selector, instead of the default MSE-optimal bandwidth selector.

Table 12: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on Regime Benefits' Distribution, using CER-optimal bandwidth selector

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-0.995*	-0.411*	-0.053*	-0.147	-0.085	0.001
	(0.45)	(0.25)	(0.07)	(0.30)	(0.09)	(0.06)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
Bandwidth	4.958	4.338	3.068	4.437	4.725	4.990

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal CER bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

5.2 E.2: Modifying the Kernel Function

In this subsection, we change the kernel function used to construct the local-polynomial estimator from the default triangular kernel to the uniform, and to the Epanechnikov kernel options.

Table 13: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on Regime Benefits' Distribution, using the uniform kernel option

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-1.090**	-0.268†	-0.227**	-0.047	-0.064	0.010
	(0.43)	(0.26)	(0.11)	(0.39)	(0.08)	(0.06)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
Bandwidth	5.852	4.376	3.651	3.355	5.146	4.556

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and uniform kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Table 14: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on Regime Benefits' Distribution, using the Epanechnikov kernel option

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-1.056**	-0.349†	-0.138**	-0.145	-0.059	0.019
	(0.43)	(0.27)	(0.11)	(0.33)	(0.08)	(0.05)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
Bandwidth	6.285	4.983	4.503	4.897	6.449	6.769

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and Epanechnikov kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

5.3 E.3: Bandwidth Expansion

In this subsection, we expand the bandwidths utilized to values 50% and 100% higher than the optimal bandwidth used in the main analysis. We report that the results remain statistically significant in all models.

Table 15: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on the Distribution of Regime Benefits, using a 50% Expansion in the Optimal Bandwidth

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-0.819*	-0.452*	-0.228*	-0.271	-0.083	-0.009
	(0.33)	(0.23)	(0.10)	(0.25)	(0.08)	(0.08)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
(Original Bandwidth	6.820	6.337	4.385	6.366	7.385	7.521)
Bandwidth 50% Change	10	9.505	6.577	10	9.505	6.577

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth expanded 50%, and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust estimates are displayed, and robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

Table 16: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on the Distribution of Regime Benefits, using a 100% Expansion in the Optimal Bandwidth

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-0.936*	-0.453*	-0.138*	-0.171	-0.065	0.006
	(0.46)	(0.21)	(0.07)	(0.22)	(0.07)	(0.06)
Observations	590	1,960	1,263	1,367	7,565	3,664
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
(Original Bandwidth	6.820	6.337	4.385	6.366	7.385	7.521)
Bandwidth 100% Change	13.640	12.674	8.770	13.640	12.674	8.770

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth expanded 100%, and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust estimates are displayed, and robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

6 Appendix F: Restricted Control Group

Table 17: Effect of Mandatory Soviet Schooling on Democratic Preferences Based on Regime Benefits' Distribution. Restricted Control Group

	Benefits			No Benefits		
	DV1	DV2	DV3	DV1	DV2	DV3
	(1)	(2)	(3)	(4)	(5)	(6)
Soviet Schooling Effect	-1.032*	-0.315*	-0.138**	-0.480	-0.178	0.109
	(0.44)	(0.23)	(0.11)	(0.75)	(0.20)	(0.14)
Observations	590	1,960	1,263	508	3,396	1,487
Outcome Mean	0.592	0.489	0.797	0.592	0.489	0.797
Bandwidth	6.820	6.337	4.385	7.994	6.362	5.019

Note: The dependent variables correspond to measures of democratic preference versus different types of authoritarianism. The control group in this analysis was restricted to exclusively include vocational secondary school graduates and traditional secondary school graduates. All models are local linear estimates using the Calonico, Cattaneo, and Titiunik (2014) optimal MSE bandwidth and triangular kernel. All estimates include survey-country and survey-year fixed effects, survey weights, and account for the pre-treatment covariate *male*, as well as town size covariates. Robust standard errors are in parentheses. †p<0.1; *p<0.05; **p<0.01.

7 Appendix G: Descriptive Statistics

Table 18: Descriptive Statistics of All Variables Used in the Analyses

	Observations	Mean	St. Dev.	Min	Max
Age	21,541	45.611	16.376	18	99
Age Cohort	21,601	3.060	1.145	1	5
Baltic Language	21,601	0.616	0.486	0	1
Benefits (Tekhnikum)	21,485	0.157	0.364	0	1
Benefits 1	18,050	0.335	0.472	0	1
Benefits 2	18,050	0.608	0.488	0	1
Benefits 3	19,017	31.484	50.245	-100	100
Birthplace	15,376	0.776	0.417	0	1
Birthyear	21,541	1951.797	16.665	1894	1986
Country Code	21,601	2.002	0.817	1	3
DV1	2,775	0.592	0.492	0	1
DV2	14,417	0.489	0.500	0	1
DV3	7,591	0.797	0.402	0	1
Employed	21,529	0.471	0.499	0	1
Estonia	21,601	0.333	0.471	0	1
High Income	16,021	0.498	0.500	0	1
Latvia	21,601	0.333	0.471	0	1
Lithuania	21,601	0.335	0.472	0	1
Male	21,588	0.441	0.497	0	1
Married	21,509	0.144	0.351	0	1
Material Today	20,935	0.343	0.475	0	1
Religious	20,311	0.828	0.378	0	1
Restricted Control	10,672	0.317	0.465	0	1
Survey Weights	21,601	1.037	0.409	0.078	4.870
Townsize	20,551	2.761	1.746	1	5
Townsize Dummy 1	20,551	0.428	0.495	0	1
Townsize Dummy 2	20,551	0.089	0.285	0	1
Townsize Dummy 3	20,551	0.077	0.266	0	1
Townsize Dummy 4	20,551	0.107	0.309	0	1
Townsize Dummy 5	20,551	0.300	0.458	0	1
University	21,485	0.250	0.433	0	1
Year	21,601	1997.397	3.942	1993	2004
Year Aged 15	21,541	1966.797	16.665	1909	2001
Year Dummy 1	21,601	0.284	0.451	0	1
Year Dummy 2	21,601	0.155	0.362	0	1
Year Dummy 3	21,601	0.135	0.342	0	1
Year Dummy 4	21,601	0.145	0.352	0	1
Year Dummy 5	21,601	0.142	0.349	0	1
Year Dummy 6	21,601	0.139	0.346	0	1

The variables are displayed in alphabetical order.

References

[1] Calonico, Sebastian, Matias D. Cattaneo, and Rocío Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression- Discontinuity Designs." *Econometrica*. 82 (6): 2295–326.

[2] McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics*, 142 (2): 698–714.