

The Anti-Left Legacy of the Pinochet Dictatorship

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Abstract

Educational systems can serve as powerful tools for attempted authoritarian indoctrination. What is the legacy of education under authoritarianism, and does it persist after the regime ends? We exploit a discrete change in the school curriculum generated by the Chilean 1973 coup. After the coup, the military junta overhauled the curriculum to align with its ideology. We use a regression discontinuity to compare people who were barely exposed to this new curriculum to those whose education ended before the reform. Results indicate that being educated under the junta-controlled system had a lasting impact on political ideology; we find a negative effect on left self-identification, a null effect on both right-wing and centrist self-identification, and a positive, albeit imprecise, effect on non-ideological identification. Effects are driven by those without a college education. We explore potential mechanisms, finding suggestive evidence that indoctrination may have both persuaded some to reject leftist platforms and increased ideological ambivalence and political apathy.

Keywords

indoctrination, authoritarian legacy, education, ideology, 1973 Chilean Coup

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Data Availability Statement included at the end of the article

Introduction

Since the establishment of mass education systems in the XIX century, public education has been a powerful tool for the state to instill values and normative notions about the world. Today, state-sponsored education is a key source of socialization, that is, a place where people acquire habits and views that shape their understanding of their society, including normative ideas about politics. This process is not, by any means, neutral. Oftentimes, regimes have notions of “ideal type” citizens, a certain picture of how citizens should behave to advance state goals. In this sense, we may use the less charitable term *indoctrination* to describe the state’s attempts to instill a particular set of values (Neundorf et al., 2022). In authoritarian regimes, where maintaining law and order is paramount, indoctrination may be especially appealing, since it can help ensure a compliant population and prevent “threatening” ideas from spreading.

In this paper, we study the impact of attempted indoctrination policies enacted through high school education by an authoritarian regime. In particular, we focus on enduring political attitudes years after the regime ended, allowing us to examine the long-term legacies of such indoctrination attempts. Education as an avenue of obtaining popular support is one of the key tools used by authoritarian regimes that often have unfettered access to educational systems (Neundorf & Pop-Eleches, 2020).

We consider the case of Chile, a country that experienced a military dictatorship for 17 years under General Augusto Pinochet. Pinochet grasped power in a military coup on September 11th, 1973, which overthrew democratically elected president Salvador Allende. Crucially, the coup caused a radical change in both *educational practices* and the *content* taught in schools. One of the junta’s main aims was to eradicate so-called “Marxist ideas” from the educational curricula. We hypothesized that the shock induced by the 1973 coup and the educational reforms that were implemented could have altered political attitudes, resulting in long-lasting changes.

We use the coup as a discrete event to study the impact of education indoctrination attempts on micro-level political outcomes.¹ We mainly focus on ideological identification, a clear indicator of people’s political worldviews. Importantly, ideology on the left-right scale was also the central political cleavage during and after the Pinochet era (Visconti, 2021). In addition, the repression carried out by the dictatorship targeted those with left-leaning ideologies. As secondary outcome measures, we examine positive and normative views about democracy, primarily to shed light on the possible pathways underpinning our main findings.

Disentangling the causal impact of education from broader societal transformations presents a significant challenge, as the new curriculum

instituted by the regime accompanied many additional shocks. To overcome this challenge, we exploit the *sudden and radical change* in the secondary educational process caused by the coup. Because the coup led to an abrupt shift in the education system, some students were only briefly exposed to the military regime's curriculum. Those who finished high school just before the coup never encountered it at all. We, therefore, identify a "treatment" group of people who were exposed to at least one year of high school education under the Pinochet regime and a barely older "control" group that was not exposed to high school education under the authoritarian regime. We claim that, at the minimum possible window around the cutoff, there is plausible local randomization, allowing us to identify indoctrination efforts' causal effects.

Using a regression discontinuity design and data from public opinion surveys in the minimum window—i.e., comparing students with one year of Pinochet-era high-school exposure to those with zero years—we find that the educational indoctrination attempt decreased identification with the left by more than five percentage points, with the strongest effects on extreme left identification. We also find a positive effect on non-ideological identification, though the estimates are imprecise and should be interpreted cautiously. However, the reform did not increase identification with either the center or the right. Further analyses show that the effect is driven by respondents whose highest level of education was high school. A placebo test among respondents from the same control and treatment birth cohorts who never attended high school returns a null result, reinforcing that exposure to indoctrination attempts in high school drives our findings. Our results are also robust to alternative data sources and the inclusion of available pre-treatment covariates.

Our heterogeneous analyses confirm that the anti-left effect is concentrated among those without a college education. We highlight that college education may mitigate regime attempts at indoctrination, by providing opportunities for critical thinking and exposure to diverse perspectives. However, it is also possible that individuals who pursue college are already less susceptible to indoctrination.

When considering potential mechanisms, both active persuasion and ideological disengagement emerge as possible explanations. We find evidence that the coup not only increased political apathy, with those who attended high school under Pinochet expressing higher levels of apoliticism, but also may have actively persuaded some individuals to reject platforms and principles associated with the left.

This article contributes to several strands of research. First, we incorporate Chile into the growing body of work about how authoritarian regimes persistently affect citizens' attitudes and behaviors (Cantoni et al., 2017; Pop-Eleches & Tucker, 2020). By adding this country, we include a compelling

case in a body of literature that has heavily focused on Europe —especially Eastern Europe— and Asia. Chile allows us to examine a context where a military coup led to the sudden adoption of an authoritarian education system and state-driven curriculum change. While our findings are specific to Chile, we can generate insights that might extend elsewhere, although other contexts should be studied in their own right.

A second contribution lies in documenting key dimensions of heterogeneity. Specifically, there is heterogeneity regarding the affected outcomes: the anti-left effect does not imply an endorsement of the regime's own ideology. Although it could be surprising to observe a negative effect (on the left) instead of a positive one (on the right), the context of a virulent anti-Marxist stance may help us understand the results. Eliminating Marxism was the explicit goal in the first years of the regime, more than promoting a specific ideological agenda. The so-called “neoliberal” economic policies, accompanied by a coherent ideological justification, acquired prominence many years later.

Additionally, our findings suggest that indoctrination attempts do not affect all citizens equally. The anti-left legacy is most pronounced among those who did not pursue higher education. This may be because college could counterbalance attempted indoctrination by encouraging critical thinking or exposing students to alternative perspectives. However, those who choose to attend college could differ systematically in ways that also affect their susceptibility to indoctrination attempts. We find that individuals educated under Pinochet who subsequently went to college are no less likely to self-identify as left-leaning. This result reveals that authoritarian legacies may be shaped not only by state-led interventions but also by additional opportunities for ideological reflection. By examining differences across groups, we join a growing body of work that finds that authoritarian legacies are conditioned by demographic traits (Elsayed et al., 2023; Tertychnaya 2020).

Third, we contribute to a growing body of work that emphasizes how public goods provision, such as education, can advance the goals of authoritarian regimes (Albertus, 2015; Paglayan, 2022). Rather than focusing on how education serves the state's goals at its initial phase, we focused on a sudden and radical change in the normative orientation of school curricula.

Finally, this article contributes to the literature on the legacies of the Pinochet regime in different areas. Scholars have found an impact of the regime on college enrollment (Bautista et al., 2023, 2025), mortality (González et al., 2024), female labor market outcomes (González et al., 2023) and social mobility (Bautista et al., 2025). We add to this scholarship by establishing a long-lasting ideological legacy of the regime's educational intervention.

Attempted Indoctrination in Autocracies

Although scholars in Political Science have studied the impact of state indoctrination on a myriad of outcomes, the concept of indoctrination remains elusive and vague.² In this paper, we take the concept developed by Neundorf et al. (2022), who define indoctrination as a “deliberate regime-led process of socializing ‘ideal-type’ citizens who support the values, principles, and norms of a given regime —whether democratic or autocratic — and who thus voluntarily comply with regime demands and remain loyal in times of crisis.”

Four important aspects of this definition are particularly relevant to this work. First, indoctrination is part of the socialization process; therefore, early life education is a key component—although not the only one. In fact, as Neundorf and Pop-Eleches (2020) point out, citizens may also consume information from the media and are socialized by their families and social networks. Second, indoctrination is not an exclusive attribute of authoritarian regimes. In a sense, every regime that attempts to instill certain values engages in a degree of indoctrination.³ Third, the specific type of indoctrination that a government pursues may depend on regime type; an authoritarian regime could be interested in inculcating patriotism, whereas a democracy might be eager to promote civic participation. Fourth, indoctrination is an input measure, that is, an attempt by a regime to produce an outcome. The actual consequences of that attempt remain an empirical question, depending, to a large extent, on the coherence of the indoctrination effort.

Under this definition, we can specify our research aim: we study the impact of an *indoctrination attempt*, through high school education, made by an authoritarian regime in the wake of a coup. We view this as a context with high indoctrination potential because teenagers forming their political identities may be receptive to indoctrination attempts.⁴

Indoctrination Through Schooling

Why would an authoritarian regime attempt to engage in indoctrination through schooling? By controlling the curriculum, dictators can manipulate instruction and disseminate information to further their political agenda. Simultaneously, they can stifle critical thinking and promote a culture of obedience. Using education to impart a specific ideology and see dissent as threatening to national unity helps ensure a compliant citizenry and the spread of pro-regime ideas and political beliefs. If these ideas are adopted, they can form stable and durable ideologies, particularly when they are internalized and reinforced. This normative support — or commitment to a regime’s tenets — may help establish loyal bases of support that can weather short-term crises, leading to enduring regime support (Neundorf et al., 2024).

Previous scholarship — typically focused on Asian countries — has generally found that indoctrination attempts promote pro-regime attitudes.⁵ Research suggests this to be the case when indoctrination attempts are implemented through education. For instance, in Taiwan, [Bai and Li \(2020\)](#) exploited the end of the authoritarian regime in 1987, to analyze the impact of authoritarian indoctrination. They found that exposure to authoritarian education increased the chance of supporting the Kuomintang Party and simultaneously decreased the probability of identifying as Taiwanese. However, this effect appeared to reverse in the 90s, as the 1997 curriculum reform increased the prevalence of Taiwanese identity rather than dual identity ([Chen et al., 2023](#)). In China, [Cantoni et al. \(2017\)](#) found that a major textbook reform enacted between 2004 and 2010 increased positive views about China's government system, changed perceptions of democracy, and increased skepticism toward a market-based economy. However, other research in this context suggests that state indoctrination through school textbooks only affected those individuals whose parents had worked for the government ([Kao, 2021](#)). In a cross-national study of educational indoctrination during authoritarianism, [Tetteh & Edgell \(2024\)](#), concluded that it quelled democratic mobilization but may be linked to higher levels of pro-autocracy mobilization. Meanwhile, [Voigtländer and Voth \(2015\)](#) studied state indoctrination in Nazi Germany and found that it increased anti-semitism, especially in areas with higher preexisting prejudice toward Jewish people. Research also shows that those educated before the fall of the Berlin Wall have lower support for democratic institutions and capitalism ([Cheruvu, 2023](#); [Elsayed et al., 2023](#)). Finally, in Tunisia, [Finkel, Neundorf, and Rascón Ramírez \(2024\)](#) found that exposure to three-minute videos on civic education solidified democratic attitudes and behaviors, suggesting that even light-touch educational interventions can promote desired political attitudes. While the current literature suggests indoctrination effects exist, questions remain. To what extent do these effects persist over time? Among which societal groups do they occur? Which political attitudes are most likely to change? Some scholarship has already explored these questions (e.g., [Dinas and Northmore-Ball \(2020\)](#); [Pop-Eleches and Tucker \(2017\)](#)). However, we aim to offer additional insight by examining a distinct case where a military dictatorship introduced a sudden educational reform after seizing power in a coup.

There are reasons to believe that students of high school age could be particularly receptive to indoctrination attempts. According to the preadult socialization model, the adolescent and early adult years are an especially formative time when it comes to establishing durable political attitudes ([Gimpel et al., 2003](#); [Sapiro, 2004](#); [Stoker & Bass, 2011](#); [Willeck & Mendelberg, 2022](#)). Indeed, studies find that pre-adult exposure to communism can have lasting effects political attitudes and behaviors ([Pop-Eleches & Tucker, 2013, 2014](#)). The educational process is typically included as one of

the key socializing agents, together with the family and the community. However, socialization through education may vary based on the regime type and the specific nature of the regime in power, though often in degree rather than in absolute difference. In dictatorships, regimes may strive to impart obedience, patriotism, or a given ideological worldview.⁶ These goals are not necessarily absent in democracies, but the focus might instead be on trying to transmit values associated with an ideal-type democratic citizen—civic behavior, political participation, and critical thinking, among others. However, democratic educational systems may also try to instill patriotism and social cohesion. Lastly, it is worth noting that political socialization is a long and complex process that could take a while to be internalized. Once internalized, however, these values may be strong drivers of political participation.

Having described our theoretical framework and existing literature, we pose our main hypothesis:

- (1) The educational indoctrination attempted by authoritarian regimes alters ideological identification. Specifically, we expect increased identification with the ideologies aligned with the dictatorship and decreased identification with those opposed.

The primary rationale of this hypothesis is the control of curricula that facilitates incorporating ideas favorable to the regime. At the same time, it may eliminate, or actively discourage, ideas deemed unfavorable.

We will also estimate three types of heterogeneous effects as exploratory analyses. First, it could be that people with college education went through an additional source of political socialization, potentially altering their ideology. However, it is also possible that those who pursued higher education were already different in ways that made them less susceptible to the regime's ideological influence. These results, therefore, can shed light on either of these two possibilities as well as the segments of the population most affected by indoctrination. Furthermore, we estimate effects by survey year to explore whether the effects persist many years after the dictatorship ended or not. Finally, we estimate heterogeneous effects by geography: the Santiago region, the south, and the north. Chile is an extremely centralized country, as almost half of the population lives in the Santiago area, the country's capital. All the central government offices are located there, including the presidential palace. It is plausible that schools located in Santiago were more affected by the indoctrination attempt, as they were closer to the center of political power.

Context

We examine the Chilean case. On September 11, 1973, Pinochet led a military coup that toppled the democratically elected government of Salvador Allende.

The coup was successful in its purpose. The junta took total control of the country in less than a day, overcoming some low-scale pockets of resistance. The aftermath of the coup was brutal and bloody. In the first four months, it is estimated that 1832 people were murdered, and thousands were exiled and tortured ([Museo de la Memoria y los Derechos Humanos Chile, 2024](#)). Correspondingly, schools were closed for the month of September, resuming activities in October. As in most dictatorships, educational reform became a priority for the new regime.

According to [Perez Navarro and Zurita \(2021\)](#) — whose main source is official government decrees—, the Pinochet approach to education consisted of three principles: (i) an “ideological cleansing” of allegedly Marxist content of the school curriculum; (ii) the implementation of new rituals, charged with patriotism and national symbols and (iii) the enactment of traditional educational practices, such as memorization and dictation.

The ideological cleansing started almost immediately. Two weeks after the coup, the director of Primary Education declared that all materials with a Marxist orientation be removed from schools. Among these books were titles such as “The Struggle for the Land,” “A History of Shantytowns,” and many others. These measures had a temporary status, as the junta was preparing a more permanent reform to be enacted years later. For instance, for the 1974 school year, the regime replaced the entire curriculum with a transitory one, focusing on Social Sciences, Spanish Language, and Philosophy ([Perez Navarro & Zurita, 2021](#)).

The new content was deeply nationalistic, with a strong emphasis on patriotism and worship for national heroes, such as Mr. Diego Portales, a key figure in the early stages of the Chilean Republic. The new rituals also did not take long to arrive. A few days after the coup, the Ministry of Education ordered every school in the country to start each week with a flag-raising ceremony, which was required to take place in a site of honor. Additionally, the authorities ordered that every student learn the third and fifth stanzas of the national anthem, even though traditionally, people only knew the fifth one. The third stanza — which is not sung today — is an ode to the “brave soldiers” that have kept the country together.

Included in Pinochet’s educational reform was a deliberate attempt to de-emphasize critical thinking and, conversely, emphasize memorization and dictation as the primary methods for instruction. The idea was to create a more vertical relationship between the students and the teachers. In fact, under the previous two administrations — Frei and Allende —, there was an attempt to create more horizontal relationships among the actors of the educational process. For the new dictatorship, horizontal relationships were dangerous and antithetical to its objectives ([Perez Navarro & Zurita, 2021](#)).

The junta ordered school principals to comply with these new practices, including proper self-presentation. To promote compliance with the new guidelines, the Armed Forces closely monitored schools' curricula and visited schools without advance warning. For their part, students were disciplined for stepping out of line — with disciplinary actions aimed to “effectively contribute to the formation of the student’s personality” (Perez Navarro & Zurita, 2021). In fact, in 1974, the minister of education designated by Pinochet, Hugo Castro, created a new teacher organization called “Colegio de Profesores.” In each school, there was a representative in charge of supervising teachers, focusing on everyday conversations, behavior in the classroom, and overall compliance with the new regime. All types of student and parent organizations were also prohibited (Cofré, 1998).

The Varieties of Indoctrination Global Dataset — developed by Neundorf et al. (2023)— confirms the radical change experienced by Chilean schools after the coup in all dimensions of indoctrination.⁷ In fact, Figure 1 shows that the indoctrination potential and the coherence increased considerably immediately after the 1973 coup, suggesting that the regime aimed to create an ideal citizen. Moreover, there is also a big jump in the level of patriotic indoctrination and the centralization of the education system.

It is worth noting that in 1973, 78% of students were enrolled in public schools, that is, schools administered by the state, as opposed to private schools. The coverage of secondary education was 42.9%, meaning that a majority of people did not complete high school education at the time (Zurita, 2021).

In addition, a key part of the indoctrination effort was the dismissal of secondary teachers in the context of the ideological cleansing. In 1971, approximately 90,000 teachers were working in the public sector. In the

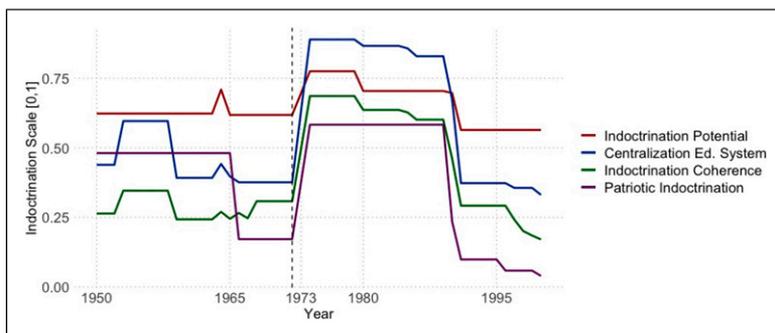


Figure 1. Education indoctrination indicators Chile 1950–2000. Source: Neundorf et al. (2023).

aftermath of the coup, it is estimated that thousands were fired for political reasons.⁸ In fact, merely belonging to one of the parties that supported Allende was grounds for dismissal (Cofré, 1998).

Teachers were also affected by the targeting of unions, which were considered Marxist bastions. On December 11, 1973, a government decree suspended paying retirement funds to the teachers' union, directly impacting their wages. A year later, the government prohibited the *de jure* existence of all teacher unions. The main teachers' union — called Sindicato Unico de Trabajadores — had approximately 100,000 members in 1973; two years later, they had only 800 (Cofré, 1998).

Finally, it is worth noting that there is no evidence of significant far-right paramilitary activity in schools after the coup. The primary far-right group at the time, Patria y Libertad, stopped operating after September 11, 1973. The Pinochet regime was largely state-centric and did not actively promote non-state organizations, even when they aligned with its ideology. In fact, the regime was highly averse to anything resembling political activity and saw independent organizations as potential threats.

Data and Sample

We use data from several waves of a public opinion poll conducted by the Centro de Estudios Públicos (CEP). This Chilean think tank has conducted such polls regularly since the early 1990s.⁹ The CEP survey is often considered the most methodologically rigorous poll in the country because it is conducted in person, and its sample has been nationally representative since 1994. The dataset is a repeated cross-section, as each wave of the CEP survey samples a new set of respondents.

The CEP survey includes several measures of political identification, partisanship, and political attitudes that can be used for this study. We use the Consolidated Data set, which includes all the surveys conducted between 1994 and 2023, with harmonized versions of the main variables of interest. The question about ideology was asked in 1995 and 2005 onwards.

Among the respondents of the CEP survey, we selected people who were born between 1950 and 1960. These subjects were between 13 and 23 years old when the 1973 coup happened. Thus, we included those who completed all their primary and secondary education under Allende, and people who had at least one year of high school education under Pinochet.

As an additional restriction, for the main analysis, we only included subjects who finished high school, given our research design. We aim to estimate the impact of the educational reforms implemented the year after the coup. To this end, we aim to ensure that (i) subjects were enrolled in secondary school, (ii) that we have people with at least one year of high school under Pinochet in the treatment group; and (iii) that we include subjects who had

completed all their secondary education under Allende in the control group. Thus, to exclude school drop-outs or people who started working after middle school, we kept subjects with completed high school education or more. Finally, we removed subjects in the last year of high school in 1973, since it is difficult to determine whether they belong to the treatment or control group.¹⁰

Measures

The nature of the measures — especially the running variable and the treatment — are the key components of the research design. We now explain these variables in depth, including the substantive meaning of the treatment and the precise way the running variable was constructed.

In regression discontinuity designs, the running variable determines who is considered treatment and who is considered control. In our case, the running variable is defined as the number of years a given individual was exposed to high school education under the Pinochet regime. This variable can be either positive or negative, corresponding to the relative timing of schooling in relation to the coup, with negative values for individuals who completed high school before the regime began. To construct the variable, we use the respondent's age at the moment of answering the survey and the survey year to back out their birth year and age at the time of the coup. For instance, an individual who responded to the 2003 CEP poll and reported an age of 47 would have been likely born in 1956. This would mean that she was 17 when the coup occurred in 1973.¹¹ The running variable, therefore, takes a value of 1, indicating that she was exposed to at least one year of high school education under Pinochet. On the contrary, a person who reported being 49 when responding to the 2003 CEP survey would have been born in 1954. In this case, the running variable takes a value of -1 since, most likely, s/he would have already completed his/her high school education at the time of the coup. This logic can be extended: a running variable value of 2 indicates people born in 1957, while -2 indicates people born in 1953, and so on.

The treatment is a dichotomous variable equal to one if the subject was exposed to at least one year of high school education under Pinochet and zero if the subject was not exposed — where exposure is defined by the running variable described above. More substantively, the treatment consists of all the changes implemented by the Pinochet regime immediately after the coup, especially during the 1974 school year. Therefore, it is a bundle of interventions, including the new content, practices, and rituals described in the context section.

Given this definition of treatment, it is necessary to discuss the rules for assignment to different grades. Before 1965, Chile had six years of primary

school and six years of high school. At the age of six, children had to enter first grade; importantly, children must have reached that age before the start of the school year, which was generally early March for the whole country (*Biblioteca Nacional, Biblioteca Nacional*). In practice, this means that subjects were defined as treated if they were six years old before the start of the school year of 1963.

This definition allows for two types of non-compliance. First, some individuals born in January or February 1956 — classified as treated — may have started primary school in 1962 rather than 1963 and completed high school in 1973 instead of 1974. Second, some individuals may have repeated a grade or started school later, completing high school at 19 instead of 18. For example, someone who began school in 1961 would finish in 1973 rather than 1972. The main solution is to exclude the cohort that graduated in 1973, which directly addresses the issue for those born in early 1956 and creates a ‘buffer year’ for students who repeated a grade.

There are two other practical reasons to exclude those respondents, despite losing observations and the ability to estimate the effect of brief exposure to the regime’s educational changes. First, given that the coup happened in that year, it is hard to define them as either treated or control, since the coup happened two months before the end of the school year. These students completed most of their final year when Allende was still in power, so they were only exposed to the coup shock for a couple of months, which might warrant their placement in the control group. However, the drastic changes may have been enough to alter their attitudes, so there are also reasons why they could be considered “treated.” Second, there is potential for error in determining the exact birth year, as we lack information on the birth month. This becomes problematic in cases where, for example, a respondent surveyed in July, 2000 reports being 45 years old. Based on this information, we might assign 1955 as their birth year. However, this imputation could be inaccurate if the respondent was born in December 1954, in which case they should belong to the control group.

The exclusion of individuals whose year of birth is assigned as 1955 reduces the likelihood of misclassification between treatment and control groups, minimizing the risk that an individual is incorrectly categorized as control when they should be in treatment, or vice versa.¹² However, some misclassification related to the dosage of the treatment — i.e., the extent of exposure — may still persist. To account for this limitation, we avoid making strong inferences about the impact at various levels of exposure.

We are mainly interested in two sets of outcomes that we hypothesized could be affected by the educational reforms enacted after the coup. First, we examine ideological identification: whether a person identifies with the left, the right, the center, or none. Given that the educational reforms enacted after

the coup explicitly sought to stop the spread of Marxist ideology, this outcome represents the core of our analysis.

The CEP survey systematically asked respondents about their positioning in the left-right di-mension on a 1 to 10 scale since the early 90s (where one is extreme left and ten is extreme right), so we have enough observations to use this variable. We convert this variable to categorical to capture whether an individual responded that they had an ideology (many did not, see [Figure A1](#)), and if so, whether her self-reported ideology would be considered left, right, or center. We defined a left-wing respondent as someone who places herself between 1 and 4; a right-wing one places herself between 7 and 10, whereas a centrist chooses either 5 or 6. All these indicators are dummy variables, meaning that the value of 1 means that the respondent identifies with either the right, the left, or the center; zero otherwise. In addition, we define hard leftists (rightists) as people answering 1 or 2 (9 or 10), while soft leftists (rightists) are ones placing themselves in 3 or 4 (7 or 8). People without ideological identification are those who did not respond or answered “Don’t know” to the ideology question.

Additionally, we consider views about democracy.¹³ The first question we examine is the agreement with either of the following phrases: (i) Democracy is always better than any other regime; (ii) Sometimes, an authoritarian regime is preferable; and (iii) I am indifferent between an authoritarian or a democratic regime. We consider a respondent to hold pro-democratic views if they select the first option. This question captures a normative dimension of democracy in the sense that people reveal how much they value living in such a regime.

Moreover, we looked at perceptions of how democracy is actually working. The main difference with the previous question is that it is not phrased in a normative way, implying that it does not capture whether democracy should be preferred over other regimes. Instead, it asks the respondent to assess the success of democracy in Chile when the survey was conducted. The question’s phrasing is the following: “How well or poorly do you think that democracy works in Chile?”. The choices are the following: Very poorly, Poorly, Regular, Well, and Very well. Drawing on this variable, we create a dummy variable equal to one if the respondent answered either poorly or very poorly and code the variable zero otherwise.

Note that compared to political ideology, these attitudinal outcomes have fewer observations since these questions were asked in fewer waves; thus, some of the analyses for these outcomes will be underpowered.

Empirical Strategy

Before presenting the details of the empirical strategy, it is important to define our estimand. In essence, we are quantifying the effect of an indoctrination *attempt*. However, we do not know the intensity of such an attempt for each sample respondent. In fact, there could be people for whom the coup did not

change their educational experience, for instance, people from private schools. Therefore, the quantity that we estimate is an intent-to-treat effect (ITT), as we lack information on how the treatment was actually experienced by the subjects.

To identify the effect of the indoctrination attempt on ideological identification, we implement a regression discontinuity design (RD), using birth cohort as the running variable and being exposed to at least one year of high school under Pinochet as the treatment variable. The main specification can be described as follows:

$$Y_{ics} = \alpha + \beta_1(\text{Cohort})_{ics} + \beta_2(\text{Coup})_{ics} + \beta_3(\text{Coup} * \text{Cohort})_{ics} + \delta_s + \varepsilon_{cs} \quad (1)$$

Where the outcome of interest Y , of individual i , of birth cohort c , surveyed in wave s , is regressed on birth cohort, the treatment variable (*Coup*), and the interaction between the two. The parameter δ accounts for survey fixed effects. The parameter of interest is β_2 , the effect of finishing high school education after the coup. In all models, standard errors are clustered at the cohort-survey level.¹⁴ Note that we are implicitly controlling for respondents' age — an obvious confounder of this model — because the running variable *Cohort* is perfectly collinear with such a variable.

The intuition behind this design is to compare survey respondents who finished high school in 1972 — that is, under the Allende government —, to those who finished in 1974. The latter group was exposed to at least one year of high school education under Pinochet, whereas the former was not. Because these two groups experienced the same external events, such as the Allende government and the coup, at approximately the same age, the main difference between the two is their experience in high school. In other words, we are less concerned about cohort-level differences among people born two years apart compared to people with a larger age gap.

Local Randomization Approach

In this RD design, the running variable (years exposed to high school education under the Pinochet regime) is discrete, which may cause several issues, both in terms of bias and efficiency.

For instance, [Dong \(2015\)](#) demonstrates that the standard RD estimation — treating the running variable as continuous and using a local polynomial specification — can lead to inconsistent estimates of the treatment effect. Moreover, [Kolesár \(2024\)](#) shows when using a discrete running variable, clustering at the running variable level could create confidence intervals with poor coverage properties.

To deal with the first problem, we use the local randomization approach, following the steps detailed by Cattaneo et al. (2024), mainly because the number of mass points around the threshold is insufficient for a continuity-based approach.¹⁵ Moreover, given the characteristics of our running variable, the exact location of the minimum window around the threshold is known: one year at both sides of the cutoff. Thus, if the running variable is X_i , and the treatment is defined as

$T_i = X_i \geq c$, the identifying assumption is that a window $W = [c - w, w + c]$, for $w > 0$ exists such that all units were assigned above or below the threshold *as if randomly* (Cattaneo et al., 2024). In our setting, this means that having finished high just before or after the 1973 coup is unrelated to the potential outcomes.

Is this assumption plausible? While it is difficult to test due to limited pre-treatment covariates for balance tests, we claim that the assumption is most defensible within the smallest possible window, that is, one year above and below the cutoff. Expanding the window increases the risk of bias because of potential cohort effects. In our data, a cohort effect means that a person born, say, in 1950 experienced a vastly different period than one born in 1960, making them less comparable. Because of this, we primarily highlight the observed effects at the minimum window around the threshold where identification is strongest, while also presenting effects at larger windows for transparency and to improve statistical power.

Limitations

The main limitation of this design is one that is common to any RD: the bias-variance trade-off. As mentioned above, the most unbiased estimate would come from comparing respondents who finished high school in 1972 (control) to those who finished in 1974 (treated) since it is more plausible that cohort-specific characteristics are similar among these two groups. While this is our primary approach, given the relatively small number of observations, it is challenging to have enough statistical power with only these two groups. Thus, we decided to increase the bandwidth and include more observations in some specifications to increase power, although we are aware of the risk of bias.

Results

Table 1 presents some basic descriptive statistics for the treatment and control groups. This is not analogous to a balance table, because all the variables, except for gender, are post-treatment, and therefore, they could have been affected by the coup. In the top panel, we compare the group born between 1950 and 1954 to those born between 1956 and 1960. We see that treated observations are younger (by design), slightly less educated, and poorer,

Table 1. Descriptive Statistics.

	Cohorts: 50–60			Total
	Control (50-54)	Treatment (56-60)	p-value diff	
High school	48.8	52.4	0.09*	51.0
Less than college	27.7	29.3	0.41	28.6
College or more	23.5	18.3	0.00***	20.4
Upper	6.9	6.1	0.37	6.4
Upper middle	19.5	17.6	0.18	18.4
Middle	53.3	50.3	0.09*	51.5
Lower	20.3	26.1	0.00***	23.7
Female	56.5	58.0	0.41	57.4
Age	60.9	54.4	0.00***	57.1
Urban	92.3	92.6	0.81	92.5
N	1387	1953		3340

	Cohorts: 54 and 56			Total
	Control (54)	Treatment (56)	p-value diff	
High school	52.1	57.8	0.22	55.1
Less than college	22.9	25.1	0.64	24.1
College or more	25.0	17.1	0.04**	20.8
Upper	5.6	4.8	0.76	5.2
Upper middle	21.3	16.1	0.09*	18.6
Middle	50.3	52.0	0.70	51.2
Lower	22.8	27.1	0.22	25.0
Female	59.2	62.7	0.37	61.0
Age	59.0	56.8	0.00***	57.8
Urban	92.3	89.5	0.25	90.9
N	338	373		711

* $p < .1$; ** $p < .05$; *** $p < .01$.

which is consistent with previous research showing that the dictatorship negatively affected higher education enrollment (Bautista et al., 2023). The sole pre-treatment variable is gender, where we observe a non-significant difference. When comparing only the 1954 to the 1956 cohort, we observe, in general, a similar distribution. Appendix A Figures A1 to A3 present distributions of our key dependent variables.

How did the educational indoctrination attempt after the 1973 coup affect ideological identification years later? Our regression discontinuity model shows that the coup decreased identification with the left, but it did not increase identification with either the right or the center (Table 2). The most

Table 2. RD Estimates 1973 Coup on Ideological Identification.

Dependent variable:		Left identification				
Birth Cohort	54 and 56	53–57	52–58	51–59	50–60	
	(1)	(2)	(3)	(4)	(5)	
Coup	−0.057**	−0.093	−0.109**	−0.085**	−0.058*	
	(0.024)	(0.069)	(0.045)	(0.038)	(0.032)	
Obs.	711	1391	2058	2683	3340	
R ²	0.046	0.03	0.02	0.01	0.01	
Mean DV	0.19	0.19	0.20	0.20	0.20	
Dependent variable:		Right identification				
Birth Cohort	54 and 56	53–57	52–58	51–59	50–60	
Coup	0.008	−0.017	0.022	0.013	0.041	
	(0.023)	(0.057)	(0.043)	(0.034)	(0.029)	
Obs	711	1391	2058	2683	3340	
R ²	0.035	0.019	0.016	0.015	0.013	
Mean DV	0.173	0.173	0.174	0.177	0.175	
Dependent variable:		Center identification				
Birth Cohort	54 and 56	53–57	52–58	51–59	50–60	
Coup	0.006	0.018	0.022	0.026	−0.004	
	(0.029)	(0.076)	(0.053)	(0.045)	(0.039)	
Obs	711	1391	2058	2683	3340	
R ²	0.038	0.029	0.021	0.016	0.015	
Mean DV	0.364	0.356	0.352	0.354	0.362	
Dependent variable:		No ideological identification				
Birth Cohort	54 and 56	53–57	52–58	51–59	50–60	
Coup	0.044*	0.093	0.066	0.046	0.020	
	(0.023)	(0.061)	(0.042)	(0.037)	(0.031)	
Obs	711	1391	2058	2683	3340	
R ²	0.039	0.046	0.035	0.03	0.032	
Mean DV	0.271	0.28	0.276	0.272	0.263	

* $p < .1$; ** $p < .05$; *** $p < .01$. The sample includes survey respondents with at least a year of higher education.

Standard errors are clustered at the cohort-survey level. Source: CEP survey 1994–2023

straightforward result to interpret is the one displayed in column 1, which compares the birth cohort of 1956 — exposed to one year of high-school education under Pinochet — to the birth cohort of 1954 — which was not exposed to high school under the dictatorship. As the dependent variable is dichotomous, we can interpret the coefficients as changes in percentage points: an additional year of high school under the dictatorship decreased support for the left by 5.7 percentage points, which is significant at conventional levels. When adding additional birth cohorts, the point estimates are similar in magnitude, ranging from 0.057 to 0.109.

Regarding the other ideological identification outcomes, we do not observe any substantial effect. The point estimates of the effect on right-wing and center identification are generally close to zero.

Additional contextual detail may help understand these null results, alongside the significant decreases in left identification. Together with the anti-Marxist language (e.g. “El cáncer marxista”), the military junta directed their rhetorical attacks on politicians, including right-wing ones. In fact, one of the most famous phrases of Pinochet was “los señores políticos”, implying that politicians were a tribe of useless, lazy dilettantes. Likewise, the early years of the regime were tightly controlled by the military institutions — the Navy, the Air Force, the Army, and Carabineros —, which occupied the vast majority of ministerial positions. Only by the late 70s and the 80s did a higher share of right-wing civilians enter the regime. In this sense, in the early years of the regime, there was not a clear right-wing ideological doctrine that guided the regime’s actions. Such an ideological base came years later when Pinochet incorporated Chicago-trained economists into the government and when traditional conservatives gave the regime a new institutional basis through the 1980 constitution.¹⁶ Therefore, it makes sense to observe a decrease in leftist individuals without an increase in right identification, as leftists were explicit targets of the regime.

Although we find a null result on right identification, we do observe a positive effect in no ideological identification of 4.4 percentage points in column 1, albeit with relatively large standard errors. Still, the best evidence suggests that leftist people could have moved to the non-ideological camp.

A relevant question is whether the effect is more pronounced on people who identify with the hard left or soft left. [Table 3](#) shows the effect, distinguishing between these two different outcomes. While the point estimates are negative in both cases, the effects are generally larger and statistically significant for the hard left outcome. In this sense, evidence suggests that indoctrination in high school education under Pinochet decreased self-reported identification with more extreme leftist positions.

[Figure 2](#) shows a visualization of the discontinuity at the threshold when using a bandwidth of four years at each side. Clearly, we see a discrete jump immediately after the coup, suggesting a sharp decrease in left identification

Table 3. RD Estimates 1973 Coup on Left Ideological Identification.

Dependent variable:		Hard left identification				
Birth Cohort	54 and 56	53-57	52-58	51-59	50-60	
	(1)	(2)	(3)	(4)	(5)	
Coup	-0.044*** (0.015)	-0.060* (0.035)	-0.073*** (0.026)	-0.061*** (0.022)	-0.037* (0.019)	
Obs.	711	1391	2058	2683	3340	
R ²	0.036	0.037	0.019	0.012	0.007	
Mean DV	0.079	0.07	0.073	0.072	0.074	

Dependent variable:		Soft Left identification				
Birth Cohort	54 and 56	53-57	52-58	51-59	50-60	
Coup	-0.013 (0.018)	-0.033 (0.061)	-0.036 (0.039)	-0.024 (0.033)	-0.021 (0.029)	
Obs.	711	1391	2058	2683	3340	
R ²	0.063	0.032	0.016	0.013	0.01	
Mean DV	0.113	0.121	0.125	0.126	0.125	

* $p < .1$; ** $p < .05$; *** $p < .01$. The sample includes survey respondents with at least a year of higher education.

Standard errors are clustered at the cohort-survey level. Source: CEP survey 1994–2023

for subjects exposed to education under Pinochet. Note that the effect is largest at the threshold, which suggests that identification with the left recovered over time. Per the local randomization approach, the effect is best identified at the threshold. Thus, units close to the threshold are the most comparable, and causal identification is expected to weaken further from the cutoff.

Heterogeneous Effects

In this section, we estimate heterogeneous effects as exploratory analyses. Results should be interpreted cautiously, as in some cases, there are a small number of observations for each subgroup, especially at smaller bandwidths. Nonetheless, these results can give us insights regarding the subgroups that drive the observed effect.

The anti-left legacy could be driven by respondents with high school as their highest educational level since those with a post-secondary education may have been exposed to additional and potentially competing messaging. Whereas the military junta sought to stifle critical thinking in high school, the regime did not exert the same control over college curricula. Those in college may have had additional opportunities to engage with

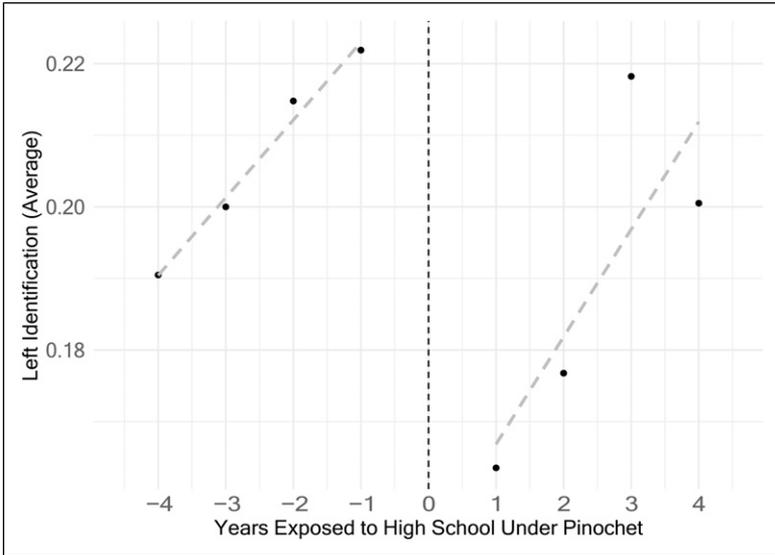


Figure 2. Visualization RD Results. The dashed line represents the predicted value of left identification at different values of the running variable. The bandwidth is four years on each side. Source: CEP survey 1994–2023.

different ideas and perspectives. Higher education — particularly through Catholic institutions¹⁷ — offered a relative buffer against regime control, creating a somewhat more favorable environment for free speech (Baeza Correa, 2004). However, there is also the possibility for self-selection: individuals who continued to higher education may have already differed in ways that made them less susceptible to the indoctrination attempts in high school.

In Table 4, we confirm this expectation. The top panel shows that, across bandwidths, there is a negative effect among people whose highest education level is high school. Among people with at least one year of higher education, we observe a non-significant effect. In other words, the effect documented in the results section is *entirely* explained by people who finished their educational process under Pinochet. This suggests that even under a dictatorship with strong attempts at indoctrination, the experience of higher education — or the characteristics of those who pursued it — may have mitigated the overall effect.

Next, we estimate heterogeneous effects by geography: south, north, and the Santiago region. This strategy is imperfect since we do not have data on where individuals attended secondary school, but only on where they were living at the time they responded to the survey. However, research suggests

Table 4. RD Estimates 1973 Coup on Left-Wing Ideological Identification by Education.

Dependent variable:	Highest educational degree: High school Left identification				
	54 and 56	53–57	52–58	51–59	50–60
	(1)	(2)	(3)	(4)	(5)
Coup	−0.107*** (0.039)	−0.160 (0.105)	−0.210*** (0.073)	−0.164*** (0.058)	−0.112** (0.051)
Obs.	284	525	758	994	1232
R ²	0.094	0.047	0.029	0.019	0.011
Mean DV	0.187	0.196	0.203	0.197	0.201

Dependent variable:	Highest educational degree: More than high school Left identification				
	54 and 56	53–57	52–58	51–59	50–60
	(1)	(2)	(3)	(4)	(5)
Coup	−0.038 (0.057)	−0.117 (0.136)	−0.008 (0.089)	−0.002 (0.074)	0.012 (0.061)
Obs.	231	463	701	936	1184
R ²	0.105	0.06	0.038	0.018	0.012
Mean DV	0.234	0.222	0.223	0.223	0.221

* $p < .1$; ** $p < .05$; *** $p < .01$. The sample includes survey respondents with at least a year of higher education.

Standard errors are clustered at the cohort-survey level. Source: CEP survey 1994–2023

that internal migration — especially inter-regionally — is rare (Rodrigo & Mateo-Peinado, 2023).¹⁸ In Table B3, we find that the effect is much more pronounced among respondents who answered the survey in Santiago compared to the other two regions.

We also consider whether the effect varies according to when the survey answers were recorded, which indicates how much time has elapsed since the coup. To capture whether the overall effect varies across time, we split the sample into two subsets, with a roughly equal number of observations: those interviewed between 1994 and 2014 and those interviewed from 2015 onwards.¹⁹ Table B4 shows that the point estimates in both samples are negative, with a similar magnitude. While responses recorded between 2014 and 2023 have lower variance — implying that some of them are statistically significant despite the small sample size —, the similar point estimates across years suggest that the effect of the educational indoctrination attempt on respondents' attitudes remains relatively stable over time. We also see similar point estimates across the age of respondents (Table B2).

Robustness Checks

As robustness checks, we implemented four strategies. First, we altered the sample. If we believe that the main mechanism behind the effect operates through high school education, then we should not observe an effect among people who were not in high school during those years. This is because those respondents were likely already working or staying at home when the coup happened. [Tables D1 and D2](#) display the results when comparing the outcomes of the same birth cohorts but among respondents whose highest educational degree is middle school, using left and hard-left outcomes, respectively. We observe a null effect in all bandwidths, further confirming the validity of our results.

Second, we estimated the RD models with the only two pre-treatment covariates we could find in our data set: gender and age. Indeed, even variables such as urban status or region correspond to characteristics of where the respondent lived at the moment of answering the survey, not when the coup happened. We find very similar results when controlling for these variables (see [Tables D3 and D4](#)).

The third robustness check was conducted with an alternative data source. In addition to the CEP data, we used the Bicentenario Survey, which is also a nationally representative public opinion poll conducted face-to-face, although it has considerably fewer waves than CEP.²⁰ In [Table D5](#), we show the results of the same RD applied to the Bicentenario data: we found very similar results, especially when comparing the 1956 and 1954 cohorts. This helps assuage concerns that our findings are an artifact of the CEP data as opposed to an effect of the educational reforms implemented by the junta. Specifically, there is a negative effect on left identification, a null effect on both right and center identification, and a positive effect on non-ideological positioning. However, there are a couple small differences: the point estimates on left-wing identification are somewhat larger; and, the effect on no and right ideology are also larger and sometimes statistically significant. Note that the survey years of the Bicentenario survey do not match the CEP data exactly.

The fourth robustness check consists of creating placebo cutoffs. These tests help determine whether the effect observed at the actual cutoff is specific to the reform or could be explained by random chance or exposure to other elements of the coup. In [Table G1](#), we estimate the regression coefficients using two placebo estimates: (i) one year above the cutoff, and (ii) one year below. We detect no systematic placebo effects, although one coefficient is marginally significant.

Finally, for additional robustness, we include [Appendix F](#), where we present randomization inference at the minimum window around the threshold which yields similar conclusions and the RDHonest procedure

(Kolesár, 2024), where intervals are centered near our estimates but are wider, as expected with a discrete running variable.

Mechanisms

Why might those exposed to high school education under the Pinochet dictatorship be less likely to self-identify as left leaning? While our primary intention is to document the main effect, we consider several possible mechanisms driving the high-level result.

One possibility is that the climate of fear created during the Pinochet era — alongside the actual and dire consequences of being vocally leftist — created a reluctance to *express* a left ideology. This aligns with Rozenas and Zhukov (2019), who found that fear of retribution deterred opposition after Stalin’s “Terror by Hunger.” In our case, we believe a fear-based explanation to be, at best, incomplete, for several reasons. First, we examine data collected at least four years after the transition to democracy, when Pinochet was no longer in power, although he was commander in chief of the army. While there may have been norms against self-identifying as left leaning during the dictatorship, the democratic era, especially after 2010, should have alleviated these fears. In fact, we found that the effect persists more than two decades after the dictatorship ended (see Table B4). Second, a norm against left-wing self-identification should affect all citizens — not just those who attended high school during the Pinochet years — and thus cannot fully explain our results. Finally, following this logic, a norm against expressing leftist ideology should also imply an increase in right-wing identification, but we do not observe an effect on that outcome.

A second possibility is that those attending high school were actively persuaded to reject ideas and platforms associated with the left and embrace those associated with the regime. To explore this possibility, we draw on additional CEP data to examine secondary outcomes, as described in the Data and Sample section. If the mechanism driving lower levels of left identification is indeed persuasion toward the regime’s ideas, we may observe a higher acceptance of authoritarianism and an increase in the perception that democracy is functioning poorly. Tables C1 and C2 display the RD estimates of indoctrination attempts on whether respondents believe that democracy is working poorly and on whether democracy is always preferable over any other form of government. Across the models, we see a positive effect on these outcomes, suggesting that, in addition to an anti-left effect, treated subjects are also more skeptical about democracy and more open towards authoritarianism.²¹ However, we do not observe an increase in identification with the right, which could indicate that respondents were not entirely persuaded by the military junta’s curriculum; or, it could imply that the emphasis was on

persuading against the left. In their view, the left was mainly responsible for the events leading to the military coup.

Another possible explanation centers on a lack of exposure to left ideas. As discussed earlier, a central tenet of the curriculum involved cleansing any “marxist” content or ideas associated with left platforms. Those in high school after the coup were, most likely, less exposed to these ideas compared to earlier cohorts. In addition, we discussed that teachers were fired for political reasons during the Pinochet era, such that many of the voices that could have introduced left ideas were silenced. While we lack a measure of respondents’ familiarity with key ideas associated with the left, it is unlikely that the anti-left effect we observe is primarily due to a lack of exposure to leftist ideas. Respondents on the left side of the discontinuity — who had one year of education under Pinochet — still experienced the majority of their education under the previous regimes — Frei and Allende — and would have presumably been exposed to leftist ideas.

A more plausible interpretation is that the curricular reforms left some ideologically detached (Neundorf & Pop-Eleches, 2020; Tertychnaya, 2020). Individuals who had their final year of schooling under Pinochet may have faced a stark rejection of some of the content learned previously. In contrast, those who finished their education before the Pinochet era would have been more consistently exposed to leftist ideas throughout their education. For those in the treatment group, this may have generated a sort of ideological whiplash that led to depoliticization, apathy, or uncertainty. This interpretation aligns with the analyses provided above; in fact, Tables 2 and D5 suggests increases in not expressing an ideological identification.

Additionally, the heterogeneous results we presented earlier — related to differences in effects according to whether respondents attended college — might shed light on this dynamic. We found that the anti-left effect is concentrated among those who did not have a college education (Table 4). Those who attended college, in contrast, are no less likely to express left-leaning ideologies. While we cannot rule out that those who attended college were less susceptible to indoctrination in the first place, one potential explanation is that higher education provided an alternative site of socialization, where resistance to the regime was more active. This experience may have served to reengage people that were apathetic or confused.

To explore this, we estimated treatment effects on the “no ideology” outcome separately for those who attended college and those who did not. Among those without a college education, the point estimate is positive, suggesting increased ideological disengagement, whereas among those who attended college, the estimate is negative. Although neither result is statistically significant — we are constrained by sample size —, the opposite signs of the point estimates align with the idea that higher education may have counteracted ideological disengagement. While these results should be

interpreted with caution due to statistical power limitations, [Table B1](#) offers tentative support for this interpretation.

In summary, in addition to possibly persuading individuals to reject principles and platforms associated with the left, the post-coup education reform might have increased political apathy, especially among those whose formal education ended with high school. However, the null effect among college attendees suggests that additional mechanisms — such as exposure to alternative ideas or resistance through their higher education — may also be at play. At the same time, this pattern is consistent with a selection effect: individuals who pursued higher education may have been systematically different from those who did not. Future work could further investigate whether aspects of college education itself — such as exposure to alternative perspectives — or the characteristics of those who pursue higher education help explain their resistance to indoctrination attempts.

Conclusion

In this paper, we show that the educational reforms implemented shortly after the 1973 military coup left a measurable anti-left legacy on those exposed to this indoctrination effort. To identify the impact of the authoritarian indoctrination attempt, we compare teenagers at the time of the coup who had barely completed their high school education to those who had not. While all of these individuals experienced the broader societal changes at play, only the latter group experienced the changes in schooling. Ultimately, although we do not find evidence of increased identification with the right due to the junta-led educational reforms, we do find that these reforms had a lasting impact on political views, especially among those who did not attend college and live in the Santiago region.

Our results confirm the power that the dictatorship had over ideological and political attitudes in the Chilean case. The effects are observed many years after the regime ended, aligning with findings in previous work and other cases ([Bai & Li, 2020](#); [Cantoni et al., 2017](#); [Chen et al., 2023](#); [Voigtländer & Voth, 2015](#)). However, we detect important dimensions of heterogeneity that may pave the way for future empirical and theoretical work. First, we find that ideological shifts are uneven; while we detect decreased identification with the left, we do not find a simultaneous increase in right-wing ideology. Instead, we suggest that the anti-left trend most likely coexists with increased apathy or disengagement, perhaps due to the particular stance of the regime back then. Second, not all citizens are affected equally, and those who are college-educated appear to be less influenced by the regime's indoctrination efforts. Selection effects may contribute to this pattern, since those who attended college may have been more politically engaged or less susceptible to attempted indoctrination. The idea that experiences such as attending college, where individuals might encounter differing perspectives and be encouraged

to think critically, could continue to shape ideological views even after exposure to strong messaging at a formative age could have implications outside the context of this research.

While our main results corroborate the influence of secondary education in forming lasting political attitudes — a finding of interest to scholars of authoritarian legacies — we have also sought to clarify the mechanisms at play. The regime may have actively persuaded people to follow their ideas, where anti-Marxism played an important role. However, the available evidence adds nuance to the concept of active persuasion, as we do not find a pro-right effect. While this could suggest a lack of successful persuasion, it may also indicate that the primary goal was to instill rejection of the left rather than support for the right. In addition, the increase in treated respondents who do not express a political ideology suggests that the educational changes may have increased ideologically ambivalence and disengagement. The reform had multiple dimensions — including curriculum changes, shifts in pedagogy emphasizing memorization over critical thinking, and more disciplinary control — and it is not clear which had the most lasting impact.

A relevant question concerns the generalizability of our findings. One of the limitations of the RD design is that it identifies an effect around a threshold — in our case, in a specific cohort. It is thus unclear whether these effects persist among people who, for instance, experienced their entire educational process under the dictatorship, lacking any comparison point. What we can say, judging by electoral results in Chile in the last 30 years, is that the left-of-center parties have been quite successful, suggesting that a potential anti-left effect may have dissipated in later years. At the same time, parties on the far left — such as the Communist Party — are indeed rejected by most Chileans, which could be a consequence of Pinochet's legacy.

We should discuss the content of being “leftist” or “rightist” in the Chilean context. The party system that emerged in the 90s was shaped by the authoritarian experience, implying that the meaning of the left and right was determined by support and opposition to the dictatorship. In other words, as [Tironi and Agüero \(1999\)](#) claims, the cleavage in the early post-authoritarian period was an authoritarianism/democracy split. The identification between the right and authoritarianism may partly explain why we do not observe a pro-right effect. This context-specific meaning of ideology may limit the extent to which our results generalize to other settings where left and right hold different connotations.

It is worth speculating how the findings we detect could extend to authoritarian regimes in power around the world today. Most 20th century dictators grasped power in coups and were openly repressive. Today, many authoritarian regimes are more subtle, undermining institutions from within the system and utilizing elections as tools to obtain legitimacy. As such, future research can seek to understand the impact of indoctrination attempts in modern contexts and how these attempts can be countered.

Another promising avenue might be to consider how the attitudinal effects we document extend to other forms of political participation, including voting behavior and engagement with political institutions, across different political contexts and regime types. If a dictatorship increases political apathy, such an attitude may translate into behavioral outcomes, such as a lack of involvement with the community and a reluctance to pay taxes, among others, with broader implications for democracy.

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Data Availability Statement

The data that support the findings of this study are available in the Harvard Dataverse at <https://doi.org/10.7910/DVN/QF8BTB>, reference number QF8BTB. Researchers may access the data under the terms specified in the repository (Argote & Voytas, 2025).

Supplemental Material

Supplemental material for this article is available online.

Notes

1. Replication materials and code can be found at Argote & Voytas (2025).
2. See Hassan et al. (2022), Paglayan (2022), and Lott (1999) as examples of different uses of the term.

3. For instance, [Alesina et al. \(2021\)](#) argue that the threat of democratization offers greater incentives for active nation-building policies, with the aim of homogenizing the population. Moreover, [Paglayan \(2021\)](#) shows that mass education was used as a tool to teach obedience in post-civil war contexts.
4. See Context section for evidence of the intensity of the attempt and the characteristics of the ideal-type citizens promoted by the regime.
5. Although those born after indoctrination attempts may reject regime ideas, see [Dinas and Northmore-Ball \(2020\)](#).
6. For example, [Lee \(2024\)](#) documents socialist, nationalist, pro-leader, and communitarian ideals in North Korean textbooks.
7. See Supplementary Material of [\(Neundorf et al., 2022\)](#).
8. There is some ambiguity in the exact number of teachers dismissed. The “Comisión por la Verdad y Reconciliación” mentions the firing of thousands of people under political pretenses, but it does not specify the occupation ([de Verdad y Reconciliación, 1991](#)). [Cofré \(1998\)](#) claims that “several thousand” teachers were fired in the first years of the regime without specifying a number. Finally, press articles such as [Muñoz \(2017\)](#) claim that 26,000 teachers were fired without providing a specific reason.
9. Full data can be accessed at this link: <https://www.cepchile.cl/opinion-publica/encuesta-cep/>.
10. See the definition of the running variable and treatment group below for a further discussion of this decision.
11. We understand that, for example, someone responding to the CEP poll in 2003 and reporting 47 years could have been either in 1956 or 1955, depending on which month the survey was taken and the month of birth. To avoid misclassifying observations into treatment or control, we eliminated from the sample people who were assigned 1955 as the year of birth—or people who we defined as 18 in 1973. Such a “buffer year” impedes misclassification. We further explain this decision in the Treatment subsection.
12. Following this logic, some treated observations may have been seniors in 1973 because we only observe respondents’ ages at the time of the survey, making it difficult to determine the exact year they were in their final year of school. This issue arises because individuals born in late 1955 may have been incorrectly assigned a birth year of 1956, leading them to be classified as treated when they should have been excluded. This misclassification only affects the treatment group—those exposed to the new regime—and does not introduce control group contamination. Because these individuals were exposed to the new regime for at most two months, any misclassification would likely bias our estimates toward zero, making our results more conservative. We provide more details regarding the month of the fieldwork and the plausible magnitude of the misclassification in Appendix H.
13. Unfortunately, only a few questions about political attitudes have been asked in repeated waves, so our outcomes possibilities were limited.

14. Another option would be to cluster at the cohort level since, strictly speaking, the treatment was assigned by cohort. However, for some of the models, there would only be two clusters, which produced excessively small standard errors. Therefore, we decided to cluster at the survey-cohort level, which assumes some correlation based on the survey year and on the cohort. This makes sense because the survey year captures a possible period effect, and the cohort a date-of-birth effect.
15. See [Appendix F](#) for more information about the mass points and the density around the threshold and for more information on the bandwidths where the local randomization assumption is plausible.
16. This alliance between neoliberal economists and religious conservatives has been denominated “Chicagó- Gremialismo”.
17. Some within the Catholic Church actively resisted the regime. For instance, the Vicaría de la Solidaridad, led by Cardenal Silva Enríquez, helped find people who were illegally detained.
18. [Rodrigo and Mateo-Peinado \(2023\)](#) suggest that 13.36% of respondents to national surveys from 2006-2017 reported relocating to a different Chilean commune within the past five years, the minority (37.6%) of whom reported relocating to a different Chilean macro-region (of which there are seven).
19. Due to statistical power constraints, we could not split the sample into more subsets.
20. The Bicentenario Survey was conducted by Pontificia Universidad Católica de Chile in partnership with the survey company Adimark. In general, this survey focuses more on societal values instead of political variables. It has, on average, about 2000 observations per wave. We used data between 2006 and 2019, with the exception of 2010 and 2011, as in those years, ideology was not asked. The 2020 wave also did not ask about ideology. The data can be accessed at this link: <https://encuestabicentenario.uc.cl/resultados/>.
21. Note that most of these coefficients are not statistically significant, partly due to the fewer number of observations compared to ideological identification.

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