

# The Geography of Repression and Support for Democracy: Evidence from the Pinochet Dictatorship\*

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We show that state-led repression under dictatorship can bolster political opposition and indirectly contribute to democratization in the medium term. Studying the military dictatorship of Augusto Pinochet in Chile, we exploit the fact that the predetermined location of military bases predicts local levels of civilian victimization, but is unrelated to historical political preferences, as well as to state presence during the dictatorship. We use geographic proximity to these bases to construct instrumental variables and estimate the causal effect of exposure to repression on citizens' revealed preferences for democracy in the high-stakes plebiscite of 1988. We find that counties with a higher rate of civilian victimization, as a result of their proximity to a military base, also had a higher rate of voter registration for the plebiscite, as well as a lower vote share in support of Pinochet. However, these counties do not show differential political alignment in national or sub-national elections after the return to democracy. Our results indicate that repression under dictatorship backfires when a democratic window of opportunity arises, but it does not fundamentally affect political preferences in the longer run.

**Keywords:** Chile, human rights, repression, dictatorship, democratization, elections

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\*This version: October 2018. First version: September 11, 2018. We would like to thank Ernesto Dal Bó, Fred Finan, Francisco Gallego, James Robinson, Gerard Roland and seminar participants at PUC-Chile for comments and suggestions. González thanks Fondecyt (Project 11170258) for financial support. Bautista and Martínez thank CISSR at the University of Chicago for financial support. Muñoz thanks the Center for Effective Global Action for financial support. Prem thanks Universidad del Rosario for financial support. Juliana Aguilar, Piera Sadini, Luis Serrano, David Vargas, Cristine von Dessauer, and Catalina Zambrano provided outstanding research assistance.

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# 1 Introduction

During Augusto Pinochet's 17-year rule as military dictator of Chile (1973-1990), the state murdered more than 3,000 people and tortured over 38,000. Repression was targeted at political activists, left-wing sympathizers, and members of trade unions, in an attempt to "extirpate marxism" from the country (Constable and Valenzuela, 1991). In 1988, a plebiscite was held to determine whether Pinochet should continue in power. The plebiscite was mandated by the country's new Constitution, drafted under tight government control in 1980, and was the first approximately free election to take place in Chile since 1973. 55% of Chileans voted "No" to Pinochet, precipitating the end of the regime and accelerating the democratic transition. In this paper, we study the relationship between the acts of repression carried out by the military regime and the behavior of voters in the plebiscite, as well as in elections taking place after the return to democracy.

State repression is one of the most pervasive features of authoritarian regimes (Acemoglu and Robinson, 2006; Svobik, 2012). However, its effectiveness in quieting dissent and producing political stability remains largely unknown. In the words of Christian Davenport (2007, p.17): "One explanation for state repression is that authorities use it to stay in power, but the literature contains not one systematic investigation of this proposition."<sup>1</sup> In a world in which 40% of the population still lives in countries classified as undemocratic (Freedom House, 2018), understanding the political consequences of repression is one of the most important tasks for scholars in this field.

The world did become increasingly democratic at the end of the twentieth century, Chile being a case in point (Huntington, 1991; Haggard and Kaufman, 2016). But to function properly, a democracy requires engaged citizens that are unafraid to express their views and hold the government accountable (Przeworski et al., 1999; Ashworth, 2012). In this regard, it is important to understand whether state-led violence during previous periods of authoritarian rule has long-lasting effects on political participation or political preferences (Simpser et al., 2018). Prior work has hypothesized that the legacy of authoritarianism may constitute an obstacle to the effective functioning of young democracies around the world, but the empirical evidence on this matter is scant (Karl, 1990; Linz and Stepan, 1996).

The Pinochet dictatorship in Chile provides an ideal setting for the study of the effectiveness of repression and its political consequences in the longer term. In this paper, we aim to answer two related questions. First, whether increased exposure to repression in certain areas of Chile succeeded in quieting dissent and generating support for Pinochet or whether, on the contrary, repression led to disaffection with the regime and bolstered political opposition. For this purpose,

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<sup>1</sup> This conclusion is echoed in a more recent overview (Davenport and Inman, 2012).

we examine whether voter registration and the “No” vote share in the 1988 plebiscite differed systematically in counties exposed to different levels of civilian victimization in the previous years. Secondly, we also want to know whether repression helped shape political preferences in the longer run, after the return to democracy, as measured by voting patterns in national and subnational elections.

These are not easy questions to answer. As mentioned above, state violence in Chile was selective and targeted, making it likely that the variation in the intensity of repression across counties is associated with pre-existing differences in political preferences and political participation, as well as in other unobservable characteristics affecting our outcomes of interest. As a result, a simple comparison of counties with different rates of civilian victimization is likely to yield biased estimates of the effects of repression.

To address this problem, we construct a novel dataset on the location of all military bases built prior to the government of Salvador Allende, the socialist president elected in 1970 and overthrown by a military *junta* in the coup of September 11, 1973. We argue that the predetermined location of military bases at the time of Allende’s election (hence, also at the time of the coup) led to a higher intensity of repression in the surrounding areas, but was unrelated to local political and economic characteristics. The intuition is simple. In the democratic decades before the coup, the placement of military bases responded to logistical factors and to national security concerns. It did not have the oversight and persecution of left-wing sympathizers and political activists as one of its objectives. To back this claim, we provide evidence that the location of military bases is unrelated to the county-level outcome of the presidential elections of 1964 and 1970, the local election of 1971 and the parliamentary election of March 1973, the last one before the coup. We also show that the stock of housing, the level of land inequality, and the implementation of the agrarian reform – one of the most important policies in the years before the coup – was similar in counties with and without military bases.

After the coup, however, proximity to military bases facilitated repression. For example, 13 of the 16 counties visited by the infamous ‘caravan of death’, which murdered or disappeared 97 people in October 1973, were home to a military unit (Verdugo, 2001). More generally, we show that the number of documented centers of detention and torture during the dictatorship is higher in counties that had a military base. Our first-stage regressions confirm that the number of victims of the regime, as a share of population in 1970, is disproportionately higher in counties with a military base and decreases monotonically with the distance to the nearest one.

We exploit the plausibly exogenous variation in the intensity of repression provided by the location of the bases to construct instrumental variables based on measures of geographic proximity.

We use these instruments to estimate the causal effect of repression on the political behavior of Chilean voters in the 1988 plebiscite and in subsequent elections after the return to democracy. For this purpose, we rely on newly-digitized administrative electoral data.

Our two-stage least squares (2SLS) estimates indicate that a one-unit increase in the number of victims per 10,000 inhabitants (0.5 standard deviations) led to an 8 percentage-point increase in the number of registered voters for the plebiscite and to a 2.6 percentage-point increase in the share of votes against Pinochet. These effects are large and provide evidence of a positive causal effect of repression on political participation and preferences for democracy in the medium run, at least for counties that experienced higher repression because of their proximity to a military base.

The results are largely replicated in the ensuing presidential election of 1989, which took place with Pinochet still in power, and to some extent in the local elections of 1992, when Chileans reaffirmed their support for the democratic transition. However, we do not find evidence of differential political alignment in counties with higher rates of civilian victimization in six presidential elections and six separate sub-national elections that have taken place after 1993. Taken together, the evidence indicates that repression under dictatorship backfires when a democratic window of opportunity arises, but it does not affect political preferences in the longer term.

Our preferred specification includes province fixed effects – the smallest administrative unit after counties – as well as control variables for the distance to Santiago, the distance to the corresponding regional capital, the population in 1970 and the vote shares of candidates Salvador Allende and Jorge Alessandri in the 1970 presidential election (left-wing winner and right-wing runner-up, respectively). The results are unaffected if we exclude these controls or use different combinations. Further tests show that the results are not driven by any particular province or region, nor by regional capitals. The results are also robust to different ways of measuring repression or proximity to military bases, as well as to the inclusion of further spatial controls. Placebo tests show that proximity to other facilities, such as airports, does not predict violence and that proximity to military bases is uncorrelated with the outcome of presidential and legislative elections in the decade before the coup.

As with any instrumental variables design, our results could be threatened by violations to the exclusion restriction. In our setting, we worry that the location of military bases affected the behavior of voters through channels other than the intensity of repression. For instance, it is plausible that the military regime used the network of military bases to channel government spending and public good provision. We address this and related concerns in two ways. First, using a novel data set of local infrastructure projects we show that the location of military bases is unrelated to the amount of government spending during the dictatorship. Secondly, we allow

for violations of the exclusion restriction and employ the method proposed by Conley et al. (2012) to gauge their quantitative importance. We find that the direct effect of bases on support for the regime would have to be *negative* and quite large (more than 50% of the reduced-form effect) to make our estimates statistically insignificant.

This paper contributes to the academic literature studying the effects of exposure to violence on political attitudes and behavior. A recent overview by Bauer et al. (2016) concludes that much more is known about the effects of civil conflict than about state repression (e.g. Bellows and Miguel, 2009; Blattman, 2009).<sup>2</sup> The topic has attracted some attention in the last few years and a handful of well-identified studies have examined the long-run political consequences of state-led violence against civilians.<sup>3</sup> Lupu and Peisakhin (2017) find that descendants of Crimean Tatars idiosyncratically exposed to greater hardships during the Soviet-led deportation of the group to Uzbekistan have a stronger anti-Russian sentiment. Similarly, Rozenas et al. (2017) show that the plausibly exogenous location of railroads affected Soviet deportations from Ukraine to Siberia and helped shape contemporary anti-Russian sentiment. Zhukov and Talibova (2018) use the same strategy to show that repression within Soviet Russia is associated with lower voter turnout today.

Contrary to these studies, we focus on an election organized by the same regime perpetrating the acts of repression. As a result, our paper is better positioned to provide insights on whether exposure to repression instills fear or submission in voters' minds. The type of violence we study is also of a much smaller scale and more highly targeted than in previous work. More importantly, the existing literature has mostly analyzed how violence across groups, defined by nationality or ethnicity, affects the way in which individuals self-identify for political purposes. We focus on the effects of exposure to violence on voters' support for democracy and on their political preferences in the traditional left-right spectrum. Somewhat closer to us is the study by Wang (2018), which exploits the location of sulphur mines to show that localities that experienced more repression during China's Cultural Revolution are more critical of the country's political system today. Still, our paper is, to the best of our knowledge, the first to examine the effect of repression on citizens' attitude towards democracy, elicited through a real, high-stakes election.

There is a small literature that has also relied on surveys to study the long-run consequences of dictatorship in Latin America. Bautista (2014a) finds that direct victims of repression in Chile did not change their interest in politics or their ideology, but did withdraw from political parties and trade unions. Relatedly, Brum (2018) uses a cross-national survey to show that exposure to dic-

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<sup>2</sup> Similarly to us, Balcells and Torrats-Espinosa (2018) find that ETA terrorist attacks led to increases in political participation in the 1990s, but had no effect on political preferences.

<sup>3</sup> Earlier contributions include Bernhard and Karako (2007) and Balcells (2012). More recently, Garcia-Ponce and Pasquale (2015) study the short-run impact of repression on reported attitudes towards the government in Zimbabwe.

tatorship during youth is associated with lower support for democracy and stronger identification with a left-wing political ideology. Bautista (2014b) also provides evidence of inter-generational effects of repression, showing that the children of victims in Chile display more right-wing preferences and less interest in politics. We complement this line of work by introducing a novel research design based on the plausibly exogenous location of military bases and by using actual election outcomes to gauge political attitudes and preferences.

The paper also makes a contribution to the large literature on the causes of democratic transitions, in particular to the strand focusing on ‘democratization by elections’ (Gandhi and Lust-Okar, 2009; Lindberg, 2009). While previous work in this area has focused on the possibility that repeated elections help develop and strengthen democratic institutions, we study the unusual window of democratic opportunity provided by a high-stakes plebiscite concerning the continuation of an autocratic regime. In this regard, our findings lend support to the hypothesis posited by Treisman (2017) that democratization often results from miscalculations on the ruler’s part. Furthermore, our paper is also the first to document the unintended consequences of state repression for the survival of an authoritarian regime.

Finally, this paper also complements a growing quantitative literature on the Pinochet regime and other dictatorships of the southern cone. Girardi and Bowles (2018) document the positive response of the Santiago stock market to the 1973 coup. Klor et al. (2017) show that repression against union leaders in neighboring Argentina was greater in firms with connections to the military *junta*. González and Prem (2018b,c) and González et al. (2018b) study crony capitalism during the Pinochet dictatorship and its legacy after the transition to democracy. Regarding the 1988 plebiscite, Boas (2015) and González and Prem (2018a) use complementary strategies to show that exposure to television ads from the opposition coalition had a positive effect on the “No” vote share.

The remainder of this paper is organized as follows. Section 2 provides background information on the dictatorship and the various forms of repression it engaged in, as well as on the 1988 plebiscite. Section 3 provides a theoretical motivation for the relationship between repression and political behavior. Section 4 introduces the data we use, while section 5 presents our research design, including the instrumental variables strategy. Our results for the 1988 plebiscite are in section 6, while the results for elections after 1988 are in section 7. In section 8 we briefly discuss additional exercises, including several placebo tests and evidence against alternative explanations. Section 9 concludes.

## 2 Historical Background: The Pinochet dictatorship, repression, and democratization

### 2.1 *The organization of repression*

The democratically elected government of president Salvador Allende was overthrown on September 11, 1973. That same day a military *junta* suspended the Constitution and declared itself the supreme executive and legislative body of the country. The *junta* was led by General Augusto Pinochet, the commander-in-chief of the army, and made one of its aims to extirpate the “Marxist cancer” that was allegedly terrorizing the country.<sup>4</sup>

The first days after the coup were characterized by mass raids by army and police units in factories, shantytowns, mining camps, and universities. The military moved quickly to arrest or often summarily execute members of left-wing political parties and trade unions, as well as other political activists. For example, on October 9th 1973, a military convoy including members of the “Cazadores” and “Maturana” regiments left the military base in Valdivia county and traveled to the nearby foresting compound of Panguipulli, where 17 local workers were apprehended and shortly afterwards executed (Rettig, 1996, p. 391). Similar episodes took place in San Bernardo county, where truck-fuls of men from the local army regiment would arrive to areas occupied by peasants during the Frei and Allende governments, pick out their victims and take them to the nearby Cerro Chena compound for execution (Rettig, 1996, p. 224-226).

Military bases (“regimientos”) were focal points through which political prisoners regularly transited. For instance, some of Allende’s close collaborators were taken to the headquarters of “Tacna” regiment shortly after the presidential palace was stormed by military units participating in the coup (Rettig, 1996, p. 119). They were executed two days later, but their bodies were never recovered. Military bases were also the place where some of those wanted by the regime voluntarily surrendered. This was the case of Luis Alaniz Álvarez and José Rodríguez Acosta, who surrendered to the local military authorities of Arica and La Serena respectively and were executed within a few days (Rettig, 1996, p. 249,276). For others, proximity to a military facility simply raised the probability of a brush-up with the authorities. Gastón Arias had the bad luck of being stopped as he drove past the military base in Punta Arenas. A passer-by identified him as an ‘extremist’ and he was immediately detained. He would spend 100 days in captivity, during which time he was subjected to torture (Kunstman and Torres, 2008, p.88).

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<sup>4</sup> Good overviews on the Pinochet regime are provided by Valenzuela and Valenzuela (1986), Constable and Valenzuela (1991) and Cavallo et al. (2011).

Due to the large number of prisoners, hundreds of detention centers were improvised across the country, employing facilities ranging from schools to stadiums. These were used to hold thousands of prisoners in terrible conditions. Many were brutally tortured, some were killed. For example, folk singer Victor Jara, a member of the Communist Youth, was arrested on September 12 and was last seen alive at the detention center set up inside of Chile Stadium on September 15. The stadium was conveniently located 2.5 km away from Tacna regiment's headquarters. Jara's body was discovered the following day with both face and hands disfigured. The autopsy revealed 44 gunshot wounds.

A few weeks after the coup, a military unit led by General Sergio Arellano-Stark toured several cities in the span of roughly one month, rounding up and murdering almost 100 people along the way. This "Caravan of Death" aimed to set an example of how sympathizers of the previous government should be treated. Arellano-Stark and his troops used a helicopter to move around the country and usually landed in military bases. Of the 16 counties where they stopped, 13 were home to a military base when Allende came to power.<sup>5</sup>

By October 1973, different branches of the military started to realize that greater coordination was needed with regards to their surveillance and intelligence activities. In consequence, the National Intelligence Directorate (known by its Spanish acronym, DINA) was created in November of 1973 and became an autonomous agency by June of 1974. The repressive apparatus changed as a result. Its main target was the insurgent Revolutionary Left Movement (known as MIR), as well as the Socialist and Communist parties. Detentions were conducted by men dressed in plain clothes, who would take prisoners without any formal arrest warrant. Secret detention and torture centers spread throughout the country. One of the most well known, Villa Grimaldi, had the "ideal characteristics for its new obscure function, such as its strategic location in the outskirts of the city and its proximity to the Telecommunication Regiment of the Army and Tobalaba Airbase" (Corporación Villa Grimaldi, 2018). Detainees who entered these places were tortured and, in many cases, were also disappeared.

Shortly after its creation, DINA started operating outside of Chile's borders, going on to assassinate Pinochet's predecessor as commander-in-chief of the army, Carlos Prats, in Buenos Aires in September of 1974 and Allende's former minister Orlando Letelier in Washington D.C. two years later. Increased foreign pressure, especially from the US, led to the dissolution of DINA in 1977 and its replacement by the National Center of Information (CNI in Spanish). The CNI remained in charge of surveillance and repression until the end of the dictatorship, but the intensity of civilian victimization decreased substantially compared to the previous years. Still, an economic crisis in

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<sup>5</sup> These counties were Rancagua, Curicó, Talca, Linares, Concepción, Temuco, Valdivia, Puerto Mont, Cauquenes, La Serena, Copiapó, Antofagasta, Calama, Iquique, Pisagua and Arica



the early 1980s led to a wave of protests across the country. These demonstrations coincided with attempts by political parties and unions to organize and create a movement seeking democracy. Massive raids were used once more as a mechanism to crack down and keep shantytowns under control.

According to the Rettig report, produced by the first truth commission set up after the return to democracy, 3,216 people were either killed or disappeared by the military over its 17-year hold on power. Although these deaths were distributed over time, 57% occurred in 1973 during the first onslaught by the military against its opponents (Policzer, 2009). The complementary Valech Report, produced by a second truth commission, concluded in 2011 that 38,254 people had been imprisoned for political reasons, 94% of which had been tortured. As with killings and disappearances, the number of people tortured was mostly concentrated in the first year of the dictatorship (61%), when the armed forces were most involved.

## 2.2 *The 1988 plebiscite*

In the year after the coup, Pinochet persuaded the other members of the governing *junta* to make him the chief executive and then president. His role was later reaffirmed by a plebiscite that took place in 1978 under highly coercive conditions. Pinochet's position was further consolidated by a new Constitution drafted under tight military control in 1980 (Barros, 2002; Cavallo et al., 2011). The Constitution was popularly approved through another controversial plebiscite on that same year and came into force on March 11, 1981. It made Pinochet president for eight years, with the *junta* continuing as the legislative body of the country.

As part of its temporary clauses, the Constitution stipulated that before the end of Pinochet's term the heads of police and the armed forces would propose a presidential candidate for the following eight years, who would have to be ratified through a plebiscite. If this candidate lost, the permanent clauses of the Constitution would come into force and an open presidential election would have to be called. Pinochet signalled his intention to run for re-election as early as 1986, but was only formally announced as the official candidate in August of 1988, little over a month before the date set for the plebiscite, October 5th. The text on the ballot presented to voters on that day would posit a simple question: "Plebiscite for President of the Republic: Augusto Pinochet Ugarte \_\_ YES \_\_ NO."

The organization of the plebiscite was not without controversy, as the country did not have an updated voter registry, with the *junta* having declared the existing one void in 1973. In anticipation of the plebiscite, voter registration begun in early 1987, but excluded people that in the government's eyes had engaged in 'terrorist activities' or that supported 'totalitarian views based on class

warfare' (El País, 1987). Radical left-wing parties denounced these restrictions, but registration was otherwise promoted by most political organizations and parties. As a result, 7.5 million people had registered to vote by September 1988, corresponding to more than 90% of the estimated voting population. Still, there was substantial variation in registration across counties, as we discuss below. Voting was mandatory, conditional on registration, so voter turnout in the plebiscite would reach 98%.

The country also lacked a functioning institution in charge of electoral organization. The absence of an institutional framework to guarantee fair elections was solved by allowing international and local supervision of the voting process, which helped limit vote-buying and manipulation of the vote tally (Engel and Venetoulis, 1992; Santa-Cruz, 2005). The combination of these factors meant that the 1988 plebiscite came to be the first approximately free election to take place in Chile since the parliamentary elections of March 1973.

Opinion polls initially predicted an easy victory for Pinochet, but as the election approached the outcome became more uncertain and the expected vote share for the “No” option steadily climbed (Cauce, 1988; Méndez et al., 1988). Campaigning was an important factor in the final weeks before the vote. Both sides were allowed to produce daily 15-minute spots that were aired on national television in the month preceding the plebiscite. Those produced by the “No” campaign revealed sensitive information, including previously-censored material related to human rights violations. The “No” ads also stood out for their originality and the high-quality of their production and positively affected the share of votes for “No” (Boas, 2015; González and Prem, 2018a).

Another source of uncertainty was related to the possibility that the military regime could call off the election or disregard the results. This worry was aggravated on the day of the election, as there was a substantial delay in the publication of the results, allegedly as a consequence of Pinochet’s initial reluctance to accept the outcome of the vote (Huneeus, 2006). The official results were only recognized in the early hours of the following day, after the other members of the military *junta* expressed their support for the outcome. The “No” option had won with 55% of the votes. Chile’s transition to democracy was under way.

Several factors contributed to this turn of events. Pinochet had been able to manipulate the outcome of the previous plebiscites in 1978 and 1980 and had enjoyed comfortable victories (Fuentes, 2013). This may have led to overconfidence in his entourage and to a political miscalculation on his part (Treisman, 2017). Support from the United States, which had been pivotal at the time of the coup, had been winding throughout the 1980s, partly as a result of DINA’s criminal activities in foreign soil (Kornbluh, 2013). At the same time, highly-organized opposition forces became increasingly active, as reflected in a series of national strikes starting in 1983. Such events were

probably difficult to anticipate at the time the Constitution was drafted in 1980. As many commentators have highlighted, though, the transition to democracy was integral to the 1980 Constitution and the ensuing political system was highly satisfactory to the military government (Valenzuela, 1997; Acemoglu and Robinson, 2006; Londregan, 2007; Albertus and Menaldo, 2018).

Following the plebiscite, Pinochet's term was extended for an extra year and presidential elections were called for December 14, 1989. A large coalition of parties opposed to the dictatorship, known as 'Concertación', chose Patricio Aylwin as its candidate, who won with 55% of the vote. Concertación candidates, participating in regular presidential elections, would go on to govern Chile until 2010. Pinochet remained as commander-in-chief of the army for another eight years and was awarded a lifetime seat in congress afterwards. But he died in 2006 under house arrest, amid several judicial processes in which he faced charges for the human rights violations committed during his time in power.

### **3 Theoretical background: Repression and political behavior**

We aim to establish whether differential exposure to repression across counties in Chile had a causal effect on local measures of political participation and political preferences in the 1988 plebiscite and subsequent elections after the return to democracy. Before presenting the details of our research design, it is appropriate to justify why such a relationship may arise at the local level. This is not trivial. After all, if people are equally informed about events and interpret them in a homogeneous way, the implications of any act of repression for citizens' behavior should not depend on the location of the event. We argue that the context we study fails to satisfy these assumptions because it is unlikely that all people were equally well informed about the actions of the regime and because we expect the proximity to the events to affect the way in which people process them psychologically.

We first argue that a local relationship between exposure to repression and political outcomes may result from informational frictions. It is true that at the extremes of information availability no such relationship should arise. If information is perfect, then everyone is equally well informed and the location of events becomes irrelevant. Analogously, if information is entirely unavailable, people are completely unaware of events and we should not observe a response anywhere. But these extremes are unlikely to occur in reality. In an intermediate informational environment, in which some information is available, a local relationship between repression and political outcomes may arise if people living closer to the location of events are relatively better informed about them.

We have good reasons to believe that the Chilean context corresponds to this last scenario.

Information about the abuses committed by the regime was certainly not perfect and there was a systematic attempt on the part of the government to cover up its actions. From the day of the coup, all media channels were censored, but this was only one of several efforts made by the government to mislead and misinform about its wrongdoings. In 1975, DINA operatives planted mutilated and burnt corpses in several locations in Argentina, identified them as alleged victims of forced disappearance, and claimed they had died as a result of internal struggles among extremist groups (Kornbluh, 2013, p.330) A now well-known headline from pro-government newspaper *La Segunda* went as far as to claim that “There are no such disappeared” in February 1977. Even in the run-up to the plebiscite, content regarding the repression was not allowed to be broadcast during the “No” campaign’s allotted television slot (La Tercera, 1988).

Such efforts are likely to have misled people about the excesses of the regime and may have contributed to increased support for Pinochet in the plebiscite. There is ample evidence from other settings that news coverage affects the salience of issues for voters, as well as political beliefs and behaviors (Enikolopov et al., 2011; Martin and Yurukoglu, 2017; Mastrorocco and Minale, 2018). However, we believe that the government was more successful at keeping people ill-informed about repression in areas farther away from the events. In counties with higher rates of civilian victimization, residents may have acquired information through informal sources and may have even known a victim or a relative of one. In this regard, it is a significant fact that victims were often arrested in broad daylight, in front of coworkers or relatives.

The informational advantage held by people in close proximity to victims was plausibly complemented by the heightened effect of direct exposure to violence on beliefs, emotions and behavior. Even if people in different locations are equally well-informed, it seems likely that knowing about abuses occurring near them, perhaps even affecting others they know, has a stronger psychological effect than knowing about similar events in a more distant location. The importance of proximity in the processing of traumatic events has also been documented in various other settings (Schlenger et al., 2002; Hersh, 2013; Lopes et al., 2015). The behavior of the military government in Chile, ranging from arbitrary detentions to summary executions, and including outright denial of victimization and more than a thousand instances of forced disappearance, is likely to have had a especially heavy toll on the families, friends, neighbors and colleagues of the victims.

It is true that the psychological stress generated by traumatic events like the ones we study may lead to inaction and submissiveness in their immediate aftermath. In this regard, Young (2018) documents a negative relationship between experimentally-induced fear and measures of political dissent. However, it is not at all clear how these emotions may evolve and shape political beliefs and behaviors over time. It seems plausible that with a sufficient amount of time since the peak of the violence and in an environment in which the threat of renewed repression is relatively low,

as was the case in Chile in 1988, the increased desire for justice or accountability resulting from more direct exposure to state repression dominates over the paralyzing effect of fear.

A related question is how the exposure to repression under dictatorship shapes the behavior of voters after the return to democracy. As already mentioned, repression in Chile was highly targeted and aimed at exterminating the ‘marxist cancer’ allegedly threatening the country. It is not obvious whether the selective violence perpetrated by the Pinochet regime was ‘effective’ in the longer run, perhaps changing people’s political preferences or their willingness to disclose them.<sup>6</sup> On the one hand, it seems plausible that in counties more exposed to repression people associate left-wing parties with the conditions that led to the dictatorship and its human cost and, in consequence, hesitate to support these parties electorally. On the other hand, it could very well be the case that the greater knowledge about the abuses committed during the dictatorship in places that experienced more repression persistently tilts local voters to the left, in an attempt to punish the political right for its connections to the military regime or express support for the victims on the left. This is an open empirical question that we aim to answer.

In this regard, it is important to understand that many of the leading figures in Chilean politics after democratization have strong links to the political establishment before and during the dictatorship. Eduardo Frei Ruiz-Tagle, president between 1994 and 2000, is the son of Eduardo Frei Montalva, who was the president that preceded Salvador Allende. Frei Montalva initially supported the coup, but eventually became one of the most prominent opposition figures during the dictatorship. Frei Ruiz-Tagle was president when Pinochet was detained in London in 1998 and emphatically demanded his return to Chile. His successor for the 2000-2006 period, Ricardo Lagos, was also a major opposition figure to Pinochet and one of the leaders of the pro-democracy movement in the 1980s. In turn, he was succeeded by Michelle Bachelet for the 2006-2010 period, a daughter of a member of the Air Force who died under custody in 1974 after being repeatedly detained and subjected to torture for his opposition to the military regime. Bachelet, who would govern Chile again for the 2014-2018 period, was herself detained and tortured at the Villa Grimaldi compound in 1975. She was the second member of the Socialist Party to be elected president after Salvador Allende in 1970. Hence, it is fair to say that the Pinochet dictatorship has remained a dominant reference in Chilean politics long after the return to democracy, one that may still be affecting voters’ choices through the legacy of repression.

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<sup>6</sup> Survey evidence from Chile suggests that both mechanisms may be at play in contemporary Chile (Bautista, 2014a,b).

## 4 Data Construction

To answer our research questions, we mainly rely on information about the victims of the dictatorship, the location and construction date of military bases, and electoral outcomes from 1964 to 2018. In this section we present the main data sources, define the most important variables and discuss some summary statistics. More detailed information about the data can be found in online appendix A.

Data on victims of the dictatorship comes from the final report produced by the National Commission for Truth and Reconciliation. This commission, headed by former minister and ambassador Raúl Rettig, was created by President Aylwin in 1991 and its goal was to clarify and document the human rights violations committed by the Pinochet regime. The report, popularly known as the “Rettig report,” provides detailed information on 3,216 victims who were disappeared (1,093) or executed (2,123) between 1973 and 1990, including the county in which they were detained or executed. Using this information, we define our main measure of exposure to repression, the civilian victimization rate, as the total number of documented victims (killed or disappeared) between 1973 and 1990, per 10,000 inhabitants as measured by the 1970 census. We interpret this variable as a proxy for the overall intensity of the acts of repression carried out by the dictatorship in a county. Ideally, this number should also include the number of people who were tortured, but that information is currently classified. Reassuringly, we do observe a positive correlation between the number of victims and the number of documented centers of torture.

To be able to connect the geography of repression with the presence of military bases, we constructed a dataset with all army units (“regimientos”) that were active by the time that Salvador Allende became president in 1970. For this purpose, we digitized historical records kept at Military Libraries and Historical Museums (e.g. González Salinas, 1987). We complemented this information with reports prepared by the army in response to our Freedom-of-Information requests. For each unit, we recorded the county in which it was located in 1970 and its exact date of creation or, in some cases, of its most recent relocation. The latter piece of information allows us to ensure that we only exploit variation resulting from the *predetermined* geographic distribution of military bases at the time of Allende’s election, and not from the potentially endogenous placement of military units during Allende’s government or in the dictatorship. After restricting attention to those operating in 1970, our final data includes 52 military bases located in 34 counties. The main variables we use are a binary indicator that takes the value of one if a county had at least one military base in 1970 and the log of distance to the nearest base in the same year.

We use administrative electoral data from the National Electoral Service, some of which we digitized for this study. Regarding the 1988 plebiscite, we use two county-level variables as out-

comes: the share of people who registered to vote and the vote share for the “No” option (“Yes” being the only other alternative). The former constitutes novel data which we digitized from archival documents kept at the Electoral Service, while the latter is publicly available. Registration was voluntary and we take the registration rate per county to reveal residents’ willingness to participate in the plebiscite.<sup>7</sup> We define the voter registration rate as the number of people that registered to vote in the plebiscite divided by the county’s population according to the population census of 1970. This is the same population count we use to construct the civilian victimization rate and corresponds to the last population census before the coup. Voting was mandatory, conditional on registration, so turnout was almost universal at 97.5%. For each county we also observe the percentage of votes in favor of Pinochet’s continuation (“Yes” option) and the percentage against this option (“No”). We use the “No” vote share as the second dependent variable. Data for other elections in the period 1964-2017 comes from records kept at the National Electoral Service, some of which we digitized.

After dropping counties with missing data on victims or elections, as well as 13 outliers in the repression measure, we are left with 276 counties.<sup>8</sup> Table 1 provides summary statistics. There is substantial variation in repression across counties. The *country* as a whole experienced a repression rate of 2.3 victims per 10,000 inhabitants, but the *county* most affected had as many as 12 victims per 10,000 inhabitants.<sup>9</sup> Military bases were present in 12% of the counties, which were home to 30% of the population in 1970. The average county was 35 km away from the nearest military base in 1970, while the average person was 22 km away. According to our data, aggregate registration for the plebiscite amounted to 71.16% of the *total* population in 1970. Still, there was substantial variation in registration across counties, with some having registration rates as low as 21% and others having rates above 100%.<sup>10</sup> According to the official records, the “No” option’s aggregate vote share was 54.7%, which is almost identical to the vote share we observe in our reduced sample of 54.8%. Variation in support for “No” was also high across counties, with the vote share for this option being as low as 3% in some places and as high as 77% in others.

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<sup>7</sup> The only other elections between 1973 and 1988 were the plebiscites of 1978 and 1980, which took place without an electoral registry. Furthermore, the county-level data with electoral results is allegedly missing, and the validity of the elections has been seriously questioned (Fuentes, 2013).

<sup>8</sup> The outliers are mostly very small counties that housed detention centers or that experienced large massacres. As part of our robustness checks, we verify that our results are qualitatively similar if we use the full sample.

<sup>9</sup> To put these numbers in perspective, consider that a homicide rate above 2 per 10,000 inh. is classified as high by the United Nations. Furthermore, the top two most violent countries in the world in 2012 experienced 9 and 5 homicides per 10,000 inhabitants respectively (UNODC, 2013).

<sup>10</sup> Registration rates above 100% are to be expected as a result of population growth between 1970 and 1988, among other factors. As part of our robustness checks, we verify that the results are unaffected if we censor the registration rate at 100%.

Complementing the summary statistics, the maps in Figure 1 show the geographic distribution of dictatorship victims and military units across the country. In panel (a), we observe that repression is not concentrated in any particular region. The map in panel (b) marks with a red star those counties with at least one military base. It also uses a darker color to show increased proximity to military bases, according to the logarithm of the distance to the closest one. Military bases were spread throughout the country, with relatively more presence in the central and northern regions.

## 5 Empirical strategy

This section describes the econometric strategy we employ to estimate the causal effect of exposure to repression on the behavior of voters in the 1988 plebiscite and subsequent elections after the return to democracy. We first present our baseline specification. We then discuss the challenges to interpreting the OLS estimates as capturing a causal relationship. The section ends by presenting our instrumental variables strategy.

### 5.1 Baseline equation

The baseline regression equation we use throughout the analysis has the following form:

$$Y_{ip,t \geq 1988} = \beta \cdot \text{Victims}_{i,t \in [1973, 1988]} + \tau X_{i,t \leq 1970} + \lambda_p + \varepsilon_{ip} \quad (1)$$

where  $Y_{ip,t \geq 1988}$  is an electoral outcome in county  $i$ , located in province  $p$ , in an election held in 1988 or after. We study outcomes in the 1988 plebiscite and the 1989 presidential election, the last one with Pinochet in power, as well as all other national and subnational elections after democratization. Our measure of repression and explanatory variable of interest is the civilian victimization rate,  $\text{Victims}_{i,t \in [1973, 1988]}$ , defined in the previous section. The main coefficient of interest is  $\beta$ , which measures the relationship between a one-unit increase in the civilian victimization rate and the appropriate electoral outcome.

Equation (1) also includes a vector of predetermined control variables,  $X_{i,t \leq 1970}$ . We include in this vector variables that were fixed by the time Salvador Allende took office in 1970 and that we expect to be strong predictors of electoral outcomes at the county level in 1988 and afterwards. Specifically, we include the vote shares of the top two candidates in the 1970 presidential election, the socialist Allende and the conservative Jorge Alessandri, as proxies for local political preferences before the dictatorship. These controls are highly relevant because political preferences may have affected the targeting of violence during the dictatorship and may have also persisted after the



return to democracy (Valenzuela and Scully, 1997). We also control for the geographic location of counties by including two spatial variables, the logarithm of the distance from a county’s geographic centroid to Santiago, capital of Chile, and the logarithm of the distance to the capital of the region where the county is located. Regional capitals are the most relevant administrative units after the country’s capital. Later we show that the results are robust to the inclusion of other spatial controls. The last variable in the vector  $X_{i,t \leq 1973}$  is population in 1970, which we record directly from the population census.

In addition to the control variables, equation (1) also includes a full set of province fixed effects  $\lambda_p$ .<sup>11</sup> The last variable in equation (1) is  $\epsilon_{ip}$ , a robust error term with a mean of zero. The small number of regions (13) and provinces (25) in the country prevents us from geographically-clustering the standard errors in our preferred specification (Angrist and Pischke, 2009). However, we show that the results are unaffected if we allow the error term to be correlated within provinces and implement the appropriate small-cluster correction (Cameron et al., 2008; Roodman, 2015). All our regressions are weighted by population in 1970 to ensure that we give equal importance to the actions of all voters, no matter the size of the county in which they reside. As a result, our estimated parameters can be interpreted as representing empirical relationships in the population and not as cross-county relations.

There are two important threats to interpreting OLS estimates of  $\beta$  as capturing the causal effect of exposure to repression on our outcomes of interest. First, even though we are controlling for strong predictors of these outcomes, the OLS estimates could still be affected by omitted variables correlated with the geography of repression. For instance, hard-to-measure levels of social capital may have affected both the intensity of repression and voter behavior in 1988 and beyond. Second, there is likely to be (classical or non-classical) measurement error in the number of dictatorship victims at the county level. For example, counties that benefited more from policies implemented by the military dictatorship, may be both less likely to report abuses and more likely to have voted for Pinochet in 1988.

We deal with these concerns in several ways. To begin with, we use variation in the OLS coefficient estimates and in the regression R-squared as we incorporate the control variables to gauge the importance of potential omitted variables, following Altonji et al. (2005) and Oster (2018). More importantly, we implement a two-stage least squares strategy exploiting the plausibly exogenous location of military bases when Allende came to power. In the next section, we present

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<sup>11</sup>At the time of the coup, the country was divided into 25 provinces, each subdivided into counties (comunas). An alternative classification using regions had been used for planning purposes since the early 1960s. In 1975, the military regime formally established 13 regions as the first level of sub-national government. Our results are unaffected if we use the more recent and less conservative region fixed effects instead.

the details of this empirical strategy.

## 5.2 Two-stage least squares

To overcome endogeneity concerns, we use two-stage least squares estimation (2SLS). This empirical strategy requires us to identify at least one instrumental variable meeting three conditions: *validity*, *relevance* and an *exclusion restriction*. The relevance condition implies that the instruments must be correlated with the endogenous variable, the civilian victimization rate in our case. The validity condition requires the instruments to not be correlated with the error term  $\epsilon_{ip}$  in equation (1), that is to say, not be correlated with any other factor affecting the outcomes. The exclusion restriction requires the instruments not to influence the outcomes by a mechanism other than the intensity of repression.

In order to generate instrumental variables, we exploit the plausibly exogenous geographic distribution of military bases in 1970. In the remainder of this section, we provide some intuition for why the proximity to military bases is a suitable instrument in our setting. We then present our first-stage specifications. Finally, we provide evidence in support of the validity condition. We leave a detailed discussion of the exclusion restriction for section 8.2.

Two main ideas underlie our instrumental variables strategy. The first is that the location of military units up to 1970 responded to strategic reasons other than the goal of controlling and repressing the civilian population. The historical record indicates that several of the oldest infantry regiments, such as Buin, Maipo, Yungay, and Rancagua were created in the early nineteenth century with the purpose of defending the country from a possible invasion by the king of Spain (González Salinas, 1987, p. 19). During the 19th century, these regiments evolved and dissolved responding to international conflicts, such as the War of the Pacific (1879-1883). During the first half of the 20th century, the organization of the regiments also changed due to technological changes in weaponry, transportation and telecommunications. While it is true that the Allende government faced strong opposition even before coming to power, the historical record indicates that up to 1970 the military high command was not engaged in political interference or coup plotting. The commander-in-chief of the armed forces at the time, Rene Schneider, went as far as to publicly reaffirm that the armed forces would respect the outcome of the 1970 election and enforce the constitution.<sup>12</sup>

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<sup>12</sup>Declassified documents show at-the-time CIA director Richard Helms acknowledging that “there was no positive assurance of success [of a coup] because of the apolitical history of the military in Chile” (Kornbluh, 2013, p.9). That the ‘Schneider doctrine’ was not cheap talk is further reflected by the fact that Schneider was assassinated by US-backed plotters, shortly before Allende took office, in response to his unwillingness to impede the Allende presidency. His replacement as commander-in-chief of the army, Carlos Prats, would be a pivotal figure in the failure

The second idea underlying our choice of instruments is that proximity to military bases was an important factor affecting the intensity of repression after the exogenous shock provided by the coup. As pointed out in section 2, military units perpetrated most of the acts of violence against civilians during the early days of the Pinochet regime. Accounts of human rights abuses during the dictatorship, such as the Rettig report, systematically link various forms of repression to the military “regimientos.” These sources also indicate that prisoners often transited through military bases on their way to centers of detention, torture and execution. In this regard, we hypothesize that a larger distance to these bases likely increased the cost of patrolling and apprehension, weakened informant networks, and created a protective buffer for the civilian population. Our first-stage regressions allow us to test this hypothesis.

As mentioned above, we employ two measures of proximity to military bases in 1970, a binary indicator and the log distance to the nearest base. We use these variables to estimate the following two versions of the first-stage equation:

$$\text{Victims}_{i,t \in [1973, 1988]} = \gamma_1 \mathbb{1}(\text{Military Base}_{i,t \leq 1970}) + \tau X_{i,t \leq 1970} + \lambda_p + \varepsilon_{ip} \quad (2)$$

$$\text{Victims}_{i,t \in [1973, 1988]} = \gamma_1 \mathbb{1}(\text{Military Base}_{i,t \leq 1970}) + \gamma_2 \ln(\text{Dist. Base}_{i,t \leq 1970}) + \tau X_{i,t \leq 1970} + \lambda_p + \varepsilon_{ip} \quad (3)$$

where  $\mathbb{1}(\text{Military Base}_{i,t \leq 1970})$  is the first instrument, an indicator that takes the value of one if there is at least one military base in county  $i$  in 1970. Equation (3) adds a second instrument to the first stage,  $\ln(\text{Dist. Base}_{i,t \leq 1970})$ , which measures exposure to bases in other counties and is defined as the log distance to the nearest base. We verify the robustness of the results to other measures of proximity to military bases below. The remaining variables are defined as in equation (1).

The coefficients of interest in equations (2) and (3) are  $\gamma_1$  and  $\gamma_2$ , which capture the relationship between the spatial distribution of military units and exposure to repression at the hands of the dictatorship. In order for the excluded instruments to be relevant, we need  $\gamma_1 \gg 0$  and  $\gamma_2 \ll 0$ . The former inequality implies that counties with a military base experienced substantially larger rates of civilian victimization than those without one, while the latter implies that counties more isolated from the military experienced significantly lower rates of victimization than those more close by.

In addition to the previous relevance conditions, the two instruments must also satisfy the validity condition and be uncorrelated with other factors affecting our outcomes of interest. Both this

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of a coup attempt against Allende that took place in June of 1973 and would later be murdered by DINA operatives in Buenos Aires in 1974.

condition and the exclusion restriction are essentially untestable and constitute our identification assumptions. Our main concern with regards to the validity of the instruments is that military bases may have been endogenously placed in counties with characteristics affecting the behavior of voters. We partially address this concern by only considering bases that were already in place when Salvador Allende came to power in 1970, shutting down the potentially endogenous placement of military units after that year. Below, we follow an even more conservative approach and verify the robustness of our results to the exclusion of bases built after 1950 or even after 1900.

We provide further evidence of the plausible exogeneity of the location of military bases in Table 2. In this table we show results of univariate regressions of many important political and economic characteristics on our two measures of proximity to military bases. We focus on the estimates including province fixed effects in columns 3 and 5, as all our regressions below include this set of fixed effects. The results indicate that counties with differing exposure to the military were essentially identical in 1973, except for their proximity to Santiago and the regional capital, which we control for in all regressions below. We fail to find evidence of differential support for Salvador Allende’s UP party in presidential, legislative and local elections in the decade before the coup. This is important because support for Allende is likely to be correlated with patterns of victimization during the dictatorship as well as with voters’ behavior after 1988. These counties were also similar in the number of houses per capita in 1970, a variable that is arguably related to income levels. They also had similar levels of land inequality, and experienced the agrarian reform with the same intensity up to 1973. Population density in 1960 and 1970 is also similar across the different sets of counties, assuaging concerns related to social capital and the possibility of collective action.

## **6 Results: Voters’ behavior in the 1988 plebiscite**

This section presents OLS and two-stage least squares (2SLS) estimates of the effect of exposure to repression during the Pinochet regime on two outcomes related to the 1988 plebiscite: voter registration and support for the “No” option. We then present a battery of robustness exercises.

### *6.1 OLS estimates*

Table 3 presents estimates of four different specifications of equation (1), all including province fixed effects. The dependent variable in Panel A is the voter registration rate, while in panel B it is the vote share for the “No” option. The results show that there is a positive, robust, and statistically significant correlation between the number of victims of repression per 10,000 inhabitants

and both voter registration and votes against Pinochet’s continuation in power. Column 1 only includes province fixed effects, while the remaining columns sequentially include additional controls. We observe that our coefficients of interest are robust to the inclusion of strong predictors of the outcomes we study. The results from our preferred specification in column 4 indicate that a one-unit increase in the civilian victimization rate is associated with a three percentage-point (pp.) increase in the rate of voter registration and with a 0.9 pp. increase in the vote share for “No.” These increases correspond to 4.2% and 1.6% changes relative to the respective sample means. Panels (a) and (b) in Figure 2 provide visual analogues to the regression estimates from column 4. These scatter plots show a strong, positive correlation between repression during the dictatorship and both political participation and rejection of Pinochet in the plebiscite.

The coefficients for the controls, as well as the R-squared at the bottom of the table, indicate that these variables are strong predictors of our outcomes of interest. The predictive power of the model almost triples in the case of voter registration and almost doubles in the case of the “No” vote share when we include all the controls. However, their introduction has little effect on the magnitude of our coefficients of interest and even helps to increase the precision of our estimates. Following Altonji et al. (2005) and Oster (2018), we use this information to conduct a “coefficient stability” analysis. The objective is to establish whether our estimates are robust to potential omitted variables that are correlated with the included controls. At the bottom of the table, we present our implementation of Oster’s proposed estimator of the bias-adjusted treatment effect.<sup>13</sup> Reassuringly, the results do not differ substantially from our baseline estimates. Nevertheless, there remain various identification concerns that prevent us from interpreting this correlation as capturing the causal effect of repression on voters’ behavior. To overcome these threats, we implement next a two-stage least squares (2SLS) strategy.

## 6.2 Two-stage least squares estimates

Columns 1 and 2 in Table 4 present estimates of equations (2) and (3). As expected, column 1 shows that there is a positive and statistically significant relation between immediate proximity to military bases and repression against civilians during the dictatorship. The point estimate indicates that counties with a military base had on average a 1.8-point higher civilian victimization rate than those that did not. This is a large increase in repression. It corresponds to slightly less than a one standard-deviation change, or to an increase of 80% over the sample average (see Table 1). The  $F$ -statistic for this single excluded instrument is 16.5, indicating that the resulting 2SLS estimates

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<sup>13</sup>A key input in this calculation is  $R_{\max}$ , the hypothetical  $R^2$  from a regression of the outcome on the treatment and both the observed and unobserved control variables. In our analysis, we set  $R_{\max}$  at its most conservative and largest possible value of 1.

are unlikely to be biased as a result of weak instruments.

Similarly, column 2 shows that counties relatively distant from military bases experienced significantly less repression. The point estimate for the second instrument indicates that a 10% increase in the distance to the closest base leads to a *decrease* in our measure of repression of 0.06 units, which is equivalent to a 2.5% decrease from the sample average. Panel (c) in Figure 2 plots this relationship using the specification with full controls. Even though the specification with a single instrument delivers a stronger first stage, the F-statistic for the specification with two instruments, at 11.1, remains above the commonly-used threshold value of 10 and is strong enough to assuage concerns about weak instruments. Furthermore, we think that the two instruments capture complementary aspects about the geography of repression and consider both throughout the analysis.<sup>14</sup>

Columns 3-6 in Table 4 present estimates of the reduced-form relationship between our measures of proximity to military bases and the outcomes in the 1988 plebiscite. Given the assumed exogeneity of the instruments, these coefficients can be interpreted as the causal effects of proximity to these bases. We find that counties with a military base had significantly more people who registered to vote in the plebiscite and that the registration rate decreased with distance to the nearest base (columns 3-4). The estimates indicate that counties with bases had on average a 13-19 pp. higher voter registration rate than those without and that a 10% increase in the distance to the nearest base led to a 0.8 pp. decrease in voter registration. In a similar way, counties geographically more exposed to the military also voted significantly more against Pinochet's continuation in power (columns 5-6). In this case, we observe that counties with bases had a 4.7-5.6 pp. higher vote share for "No" and that a 10% increase in the distance to the nearest base increases support for Pinochet by 0.12 pp. We return to these estimates below in order to check how small violations of the exclusion restriction affect our 2SLS estimates of  $\beta$  in equation (1).

Table 5 presents our 2SLS estimates. If our instrumental variables assumptions are satisfied, then these coefficients capture the causal effect of repression on voters' behavior in the plebiscite. In the presence of heterogeneous effects of repression across individuals, the 2SLS estimates capture the local average treatment effect (LATE) of repression on the political behavior of those individuals, the compliers, that were more exposed to it because of their geographic proximity to

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<sup>14</sup>Our measure of repression only includes victims that were executed or disappeared. These numbers only capture some of the manifestations of repression in Chile (i.e. the number of victims of torture was an order of magnitude larger than the number of those who died), but data constraints prevent us from examining other forms of repression. However, Table A1 in the online appendix shows that the presence of a military base has a strong positive correlation with the presence and number of centers of torture in a county, suggesting positive spatial correlation between different forms of violence. The location of these centers is not related to the distance to the nearest base, which is consistent with military units wanting to avoid costly travel time between official bases and unofficial centers of torture.

a military base.<sup>15</sup>

The results in Table 5 show that repression had a large and statistically significant effect on voter registration and support for democracy in the 1988 referendum. In counties where the civilian victimization rate was one unit higher, we observe an increase in voter registration of 7.6-8.9 percentage points, depending on the choice of instruments, as well as a 2.6-2.7 percentage-point increase in the “No” vote share. These effects are large and correspond to increases of roughly 0.3 standard deviations in both cases. Equivalently, they represent respective increases of 11% and 5% over the corresponding sample averages (see Table 1). The estimates are quite precise and are hardly affected if we allow the error term to be correlated within provinces using the wild cluster bootstrap (p-value in brackets).<sup>16</sup>

The 2SLS estimates in Table 5 are more than twice as large as the corresponding OLS estimates in Table 3. This difference can arise for several reasons. First, measurement error in the civilian victimization rate can lead to attenuation bias in OLS. Such measurement error can be the result of victims being wrongly assigned to counties. It can also arise because of data limitations with regards to other forms of repression, such as torture, or because of selective reporting. A second reason for the discrepancy between OLS and 2SLS is potential downward bias of the former due to omitted variables. For example, lower stocks of unobservable social capital in certain counties may have facilitated the dictatorship’s ability to carry out acts of repression as well as hindered political participation and opposition to Pinochet at the time of the plebiscite. A third reason has to do with the complier counties being more responsive to repression than the average county. This seems likely, as our characterization of compliers in Appendix B indicates that the violence experienced by these counties was disproportionately concentrated in 1973, when repression was more intense and brutal.

### 6.3 *Robustness checks*

In this section, we summarize the battery of robustness tests we perform, leaving all relevant tables and figures for the online appendix. We check the robustness of our results for the 1988 plebiscite along five margins: the specification, the sample of counties, the measures of proximity to the military, the measure of repression, and the measurement of the outcomes.

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<sup>15</sup>The LATE interpretation of our 2SLS estimates also requires a monotonicity assumption that we believe is quite likely satisfied, as there is no reason to think that being farther away from a military base increases exposure to repression, all else equal. In Appendix B we provide a characterization of the complier counties.

<sup>16</sup>Additionally, the Hansen *J*-statistics cannot reject that the over-identifying restrictions are valid for both outcomes (*p*-values of 0.28 for voter registration and 0.63 for “No” vote share). However, this test must be interpreted with caution as proof of the validity of the instruments (Deaton, 2010; Parente and Silva, 2012).

Regarding the specification, we verify that the first-stage, reduced-form and 2SLS coefficients are similar if we use different combinations of controls. Following the sequence in Table 3, we consider four versions of equations (1)-(3) in which we progressively include covariates. Tables A2-A5 show that the estimates are of similar magnitude and statistical significance for different combinations of controls. The results are similarly unaffected if we employ a machine-learning algorithm to determine the optimal combination of controls (Table A6). The results are also robust to the inclusion of flexible spatial controls. Table A7 replicates the 2SLS analysis when we add polynomials of latitude and longitude to our main specification (columns 1-4), as well as different measures of the spatial centrality of a county. For the latter, we use (i) the average distance from a county's population-weighted centroid to all other counties (columns 5-8), and (ii) the Moran eigenvectors with positive eigenvalues, as proposed by Dray et al. (2006) and applied by Rozenas et al. (2017) (columns 9-12). Lastly, in Table A8 we check that the results are not an artifact of the population weights we employ.

We further show that our results are not driven by particular groups of counties, specific geographic regions or outliers. Figure A1 shows that the results are unaffected if we drop randomly-chosen groups of twenty counties from the estimation. The coefficients are also similar if we drop all counties from any of the 13 regions in the country, including the metropolitan region of Santiago, home to 34% of the country's population in 1970 (Figure A2). The same is true if we remove from the sample any of the 25 provinces (Figure A3). Similarly, Table A9 shows that the results are unaffected and the strength of our instruments actually increases if we remove the 13 regional capitals. This is important as all of the capitals were home to a military base and they are likely to be more urbanized, populated and developed than other counties. Finally, we show that our results are qualitatively similar if we use the full sample, including the 13 outliers in our measure of repression (Table A10).

As mentioned above, we think that the location of military bases is more likely to be uncorrelated with relevant political and economic conditions at the time of the coup for those bases that were built many years, and even decades, before it took place. To ensure that our results are not biased by the potentially-endogenous location of bases built closer to the time of the coup, in Table A11 we replicate the analysis excluding all bases built after 1950. The results remain unchanged. In Table A12 we follow an even more conservative approach and exclude all bases built after 1900. The results are quite similar to those with the full sample, even though the instruments are expectedly weaker. Additionally, we also examine the sensitivity of our results to different ways of measuring proximity to military bases. Our first-stage results are very similar if we calculate a county's distance-weighted exposure to all bases in the country, instead of the minimum distance to a base (columns 1-3 in Table A13). They are also unaffected if we use distance to the closest



base as our only instrument (columns 4-6). If we complement our set of military bases, corresponding to army “regimientos”, with air force bases and military academies, the results remain unchanged and the instruments get stronger (Table A14).

Our baseline measure of repression, the civilian victimization rate, is based on the county of detention or execution of the victims. For a subset of the victims, we have been able to establish the county of residence or work. We replicate the analysis using the corresponding county-level measures of civilian victimization and find consistent results (Table A15). In Table A16 we also replicate the analysis after recalculating the civilian victimization rates using only victims from the first year after the coup, before DINA became an independent agency and became responsible for most repression-related activities. All of our results go through. Table A17 further shows that the results are very similar if we employ a binary measure of repression, taking a value of one for counties in the top quartile of the distribution of the civilian victimization rate.

The last set of robustness checks concerns the way we measure the outcomes. In Table A18, we examine the sensitivity of the results to the normalization of the number of registered voters by population in 1970. Columns 1 and 2 show that the results are unaffected if we do not winsorize the voter registration rate. Columns 3 and 4 similarly show the robustness of the results if we cap the voter registration rate at 100%. One difference between our measures of voter registration and “No” vote share is that the former is normalized by population in 1970, while the latter is normalized by the number of valid votes. In Table A19 we show that the results for this outcome are unchanged if we homogenize the denominator for both outcomes and normalize the number of “No” votes by 1970 population.

## **7 Results: Voters’ behavior after the 1988 plebiscite**

Having established the robustness of our identification strategy and of the results for the 1988 plebiscite, we now study the relationship between repression and voters’ revealed political preferences in multiple elections held over the following two decades. We proceed in chronological order and end with a discussion about how we interpret these results. In all of these cases we estimate equation (1) using the vote share for a candidate or a party in a specific election as dependent variable and using proximity to military bases or the instrumented civilian victimization rate as explanatory variables (i.e. reduced form and 2SLS). We do not study voter registration or turnout for elections after 1988 because registration rates remained high and stable in later years and voting was compulsory until 2012.

A year after the plebiscite, the 1989 presidential election took place. This was the first election

held after the plebiscite and occurred while Pinochet was still in power. We use as outcome variables the share of votes received by Patricio Aylwin, the candidate put forth by the ‘Concertación’ coalition of pro-democracy parties, and the vote share for Hernan Büchi, Pinochet’s former Minister of Finance. The omitted category in this case corresponds to Francisco Errázuriz, a moderate. Table 6 shows that the civilian victimization rate is positively associated with the Aylwin vote share and it is negatively correlated with the Büchi vote share. Our 2SLS estimates in columns 7-10 indicate that one more victim per 10,000 inhabitants led to a 1.3-1.5 percentage-point higher vote share for Aylwin, depending on the choice of instruments, and to a reduction of 1.7-2.0 percentage points in the vote share for Büchi.

Six presidential elections have taken place in Chile after 1989. For these elections, we examine the vote share of the ‘Concertación’ coalition candidate, winner in four out of six contests. We also study support for candidates to the right and to the left of this coalition. Tables A23 and A24 in the online appendix show the way we have classified candidates, parties and coalitions in national and local elections. The panels in Figure 3 present 2SLS estimates and 95% confidence intervals for each faction in each presidential election, using presence of military bases as the excluded instrument (estimates with two instruments available in the online appendix). Overall, we find close to zero effects of exposure to repression during the Pinochet dictatorship on political preferences in democracy. The vertical line in 2012 denotes the introduction of automatic registration and voluntary voting. We do observe a shift to the left in counties with greater exposure to repression after this year, but the estimates are noisy and remain statistically insignificant.

Figure 4 replicates the 2SLS analysis for the seven local elections that have taken place after democratization. We study vote shares in mayoral elections and classify candidates in an analogous way to the presidential elections. The only difference is that the very small number of far left parties competing in these elections forces us to group them with all other minor parties. In the case of local elections, we do observe that counties with greater exposure to repression during the dictatorship showed stronger support for “Concertación” and left-wing candidates in 1992, the first election of any kind after Pinochet left office, but fail to find any sign of differential political alignment in the six elections since, up to 2016.

An important difference between the 1989 presidential election and the ones that followed, is that the former took place under dictatorship, with a candidate directly linked to Pinochet, and with significant uncertainty about the political future. In the words of Angell and Pollack (1990), “The [1989] elections were in many ways a replay of the plebiscite” (p. 2). Hence, we interpret the behavior of voters in 1989 as a confirmation of the preferences for democracy that they had stated in 1988. A similar argument can be made about the 1992 local elections, which were the first elections of any kind to be held without Pinochet in power. After 1992, it is plausible that the consolidation

of the democratic transition allowed other matters to gain salience in the political debate and that the “Concertación” coalition became less associated with the pro-democracy movement and more with the political issues of the day. It is also likely that efforts at accountability and reconciliation, such as the release of the Rettig and Valech reports, as well as the construction of the Museum of Memory and Human Rights in Santiago, allowed people to start feeling that the human rights violations that took place were recognized by Chilean society as a whole.

In sum, even though Pinochet remains a dominant reference in Chilean politics up to this day, and despite the fact that most of the prominent political figures in the country are positively or negatively associated with the dictatorship, we fail to find evidence of a robust long-run relationship between exposure to repression and electoral outcomes. Taken together, the evidence indicates that people of differing political inclinations joined forces in response to the repression and increasingly opposed the dictatorship during the democratic window of opportunity between 1988 and 1992, but that this did not systematically affect their political alignment after the return to democracy. In short, we find no evidence of a legacy of repression during dictatorship on political preferences after democratization.

## **8 Additional exercises: Placebo tests and alternative explanations**

In this section, we present the results of a series of placebo tests that enhance the credibility of our findings. We then provide evidence against alternative explanations and examine possible violations of the exclusion restriction.

### *8.1 Placebo tests*

Panels (a) and (b) in Figure A6 show the distributions of coefficients for the first-stage regression with one instrument (equation 2) that result when we randomly assign military bases among counties. This permutation test provides us with a distribution-free estimate of the probability that our first-stage relationship arises by chance. In panel (a) we allow the placebo bases to be allocated to any county, while in panel (b) we restrict the assignment to counties within the same province as those actually holding bases. As expected the distributions are centered at zero. More importantly, we find that our estimated first-stage coefficient is above the 99th percentile of the resulting distributions in both cases.

Another placebo test for our first stage consists of examining whether proximity to facilities other than military bases predicts the intensity of repression. This exercise aims to distinguish between the influence of military bases of repression and the influence of characteristics that make a

location suitable for such a facility. For this purpose, we analyze whether counties with other types of strategic infrastructure, such as ports and airports, also had higher civilian victimization rates. We also look at counties having official land entry points into the country. The results in Table A20 indicate that our first-stage is robust to the inclusion of measures of presence or proximity to ports, airports or entry points. Furthermore, these additional variables are not significantly related to the intensity of repression.

In Table A21 we replicate the previous reduced-form and 2SLS analysis for elections in the decade before the coup. We look at vote shares for the two top candidates in the presidential elections of 1964 and 1970, as well as the performance of Salvador Allende's UP coalition in the local elections of 1971 and the legislative elections of 1973, which took place only months before the coup. The intuition for this exercise is that in the absence of repression (i.e. before the coup) the location of the military bases should not explain electoral outcomes. In all columns we observe that the presence of military bases (panel A), or the related repression (panel B), is not associated with differential vote shares for the right- or left-wing candidates. Taken together, the null findings for the 1964-1970 period and the 1993-2017 period strongly suggest that the documented relationship between exposure to repression and voting outcomes during the democratic window of opportunity (1988-1992) is driven by unobservable factors affecting political preferences.

## 8.2 *Potential violations of the exclusion restriction*

Besides the relevance and validity conditions, our 2SLS estimates of the effects of repression also rely on an exclusion restriction stating that proximity to military bases only affected our outcomes of interest through increased exposure to repression. In this regard, we worry that a military dictatorship like Pinochet's may have made extensive use of the network of military facilities in the country for matters related to public administration. If the presence of the state was more strongly felt in counties nearer to military bases, this may have affected the behavior of voters in the 1988 plebiscite even in the absence of repression. For instance, the state could have invested more in places with military bases, causing people to vote increasingly in favor of Pinochet's continuation in office, which would bias our 2SLS estimates downwards. But bases could have also revealed undesirable characteristics of the military regime through actions unrelated to repression, such as favoritism in procurement, leading voters to reject Pinochet's continuation and biasing our estimates upwards. Military units may have also directly influenced voter registration in areas near to their bases, positively or negatively, although this seems unlikely given that there was considerable monitoring (Engel and Venetoulis, 1992; Santa-Cruz, 2005). Yet another possibility is that the presence of the bases was directly intimidating, given information from other sources about the

various forms of repression taking place. What seems clear is that the sign of the bias resulting from violations of the exclusion restriction is not obvious ex-ante.

To address the possibility of differential public spending in counties closer to military bases, we use a newly-digitized dataset on local infrastructure projects implemented by the Ministry of Housing and Urban Planning during the dictatorship. Using this data we construct a measure of aggregate per-capita state spending on urban projects per county.<sup>17</sup> In addition, we disaggregate this measure and separately look at what we consider to be highly ‘visible’ projects, i.e. construction in public spaces and housing, and other less ‘visible’ projects, i.e. sanitary projects and indoor equipment. This distinction is important and could be related to patterns of vote-buying (Marx, 2017). We then re-estimate equation (3) using these measures of public spending as dependent variables. Table A22 in the online appendix presents the results. We find that the aggregate and disaggregate measures of public spending during the dictatorship are empirically unrelated to any of our measures of proximity to military bases. The estimated coefficients are statistically indistinguishable from zero in all cases, indicating that public spending was not disproportionately concentrated in counties closer to military units.

A different and more agnostic approach involves acknowledging that the exclusion restriction may be *partially* violated and trying to gauge the quantitative importance of any such violation. Following Conley et al. (2012), we allow the presence of military bases to directly affect our outcomes of interest by including a coefficient  $\gamma \neq 0$  in equation (1). This exercise makes it possible for us to calculate how important alternative explanations would have to be to make our estimates statistically insignificant. Here we rely on the reduced-form estimates reported in columns 3 and 5 of Table 4, following Nunn and Wantchekon (2011) and Satyanath et al. (2017). Results are presented in Figure A7 in the appendix and show that, to make our estimates insignificant, the direct effect of military bases on registration and the “No” vote share would have to be *positive* and quite large, equivalent to 46% and 68% of the reduced form coefficients respectively. These findings provide further evidence against alternative explanations based on preferential access to government services and are also inconsistent with increased intimidation and restrictions on voters in the areas near military bases.

## 9 Conclusion

We study the political consequences of exposure to repression during the dictatorship of Augusto Pinochet in Chile. We find that repression bolsters political opposition and can contribute to de-

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<sup>17</sup>Appendix A provides further information on data construction.

mocratization in the medium term. More specifically, we observe that counties exposed to greater repression registered to vote in larger numbers for the 1988 plebiscite and increasingly voted against Pinochet's continuation in power. These effects are found in both OLS and 2SLS analyses, the latter using proximity to military bases to generate instrumental variables, and are quite robust to changes in sampling, measurement and specification, as well as to potential violations of the exclusion restriction in the case of 2SLS.

These findings go in the opposite direction of what is presumed to be the immediate effect of repression, which is the fear-induced generation of political acquiescence. In a world in which large-scale forms of repression have become the exception, and in which authoritarian regimes increasingly engage in electoral politics (Levitsky and Way, 2010), our results indicate that repression can only go so far in ensuring political survival. This fact can help explain the rise of 'informational autocrats' in recent times, who have become more reliant on the manipulation of information, rather than on violence, to remain in power (Guriev and Treisman, 2018).

Our findings for the 1988 plebiscite largely replicate for the 1989 presidential election and the 1992 local elections, in which we observe increased support for candidates from the pro-democracy "Concertación" coalition in counties with more exposure to repression. These were, respectively, the last election to take place with Pinochet in power and the first one after he left office. However, our study of more than a dozen national and subnational elections over a 25-year period after the return to democracy fails to find evidence of a legacy of repression on electoral outcomes. This finding is ever more surprising given the salience of Pinochet and the dictatorship in Chilean politics up to this day and stands in contrast with the previous literature (e.g. Rozenas et al., 2017). One likely explanation is that previous studies have focused on forms of repression driven by ethnic or nationalistic cleavages, which are likely to have more long-lasting consequences.

Taken together, our findings indicate that exposure to repression under dictatorship has a causal effect on preferences for democracy, but does not necessarily shape party affiliations after democratization. Chile's experience was not unique. It was one of many countries to experience dictatorship and state repression against political opponents as part of the larger geo-political game being played during the cold war. It was also one of many countries to experience democratization at the end of the XX century. Hence, we have reason to believe that our findings are likely to resonate in several young democracies located in various parts of the world.

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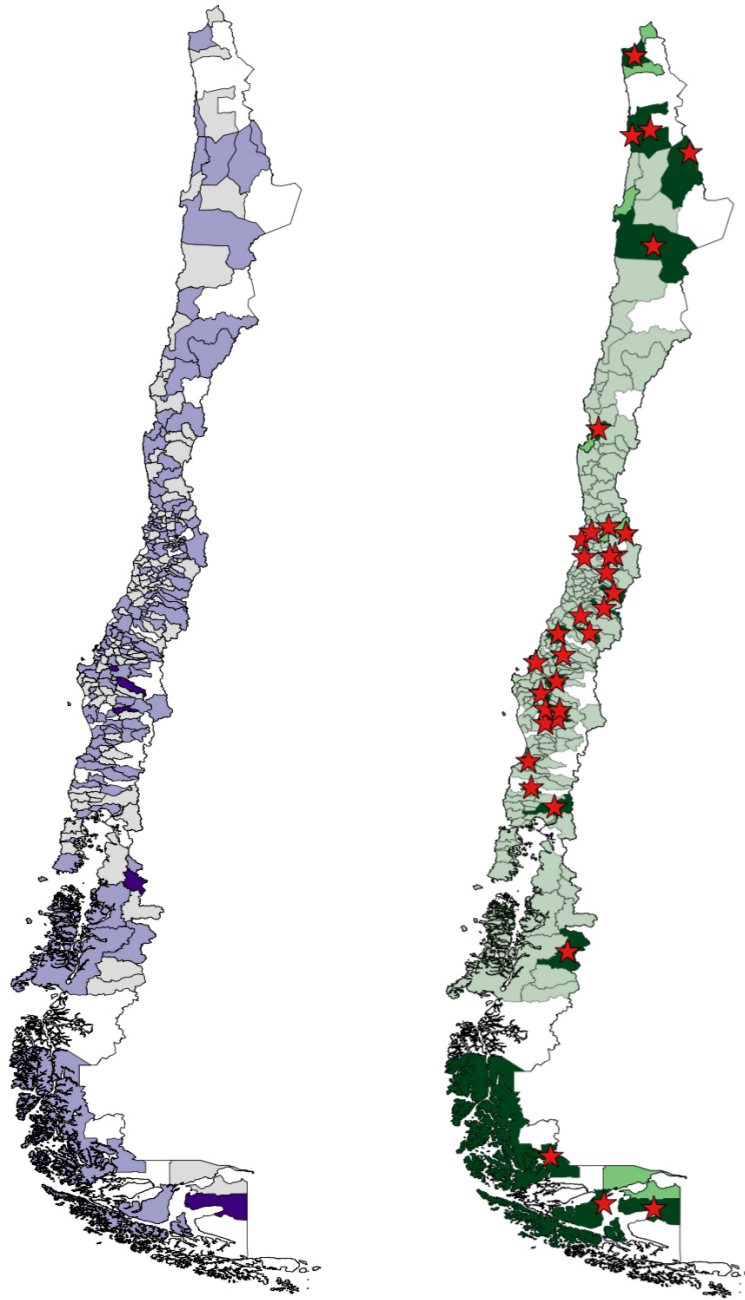
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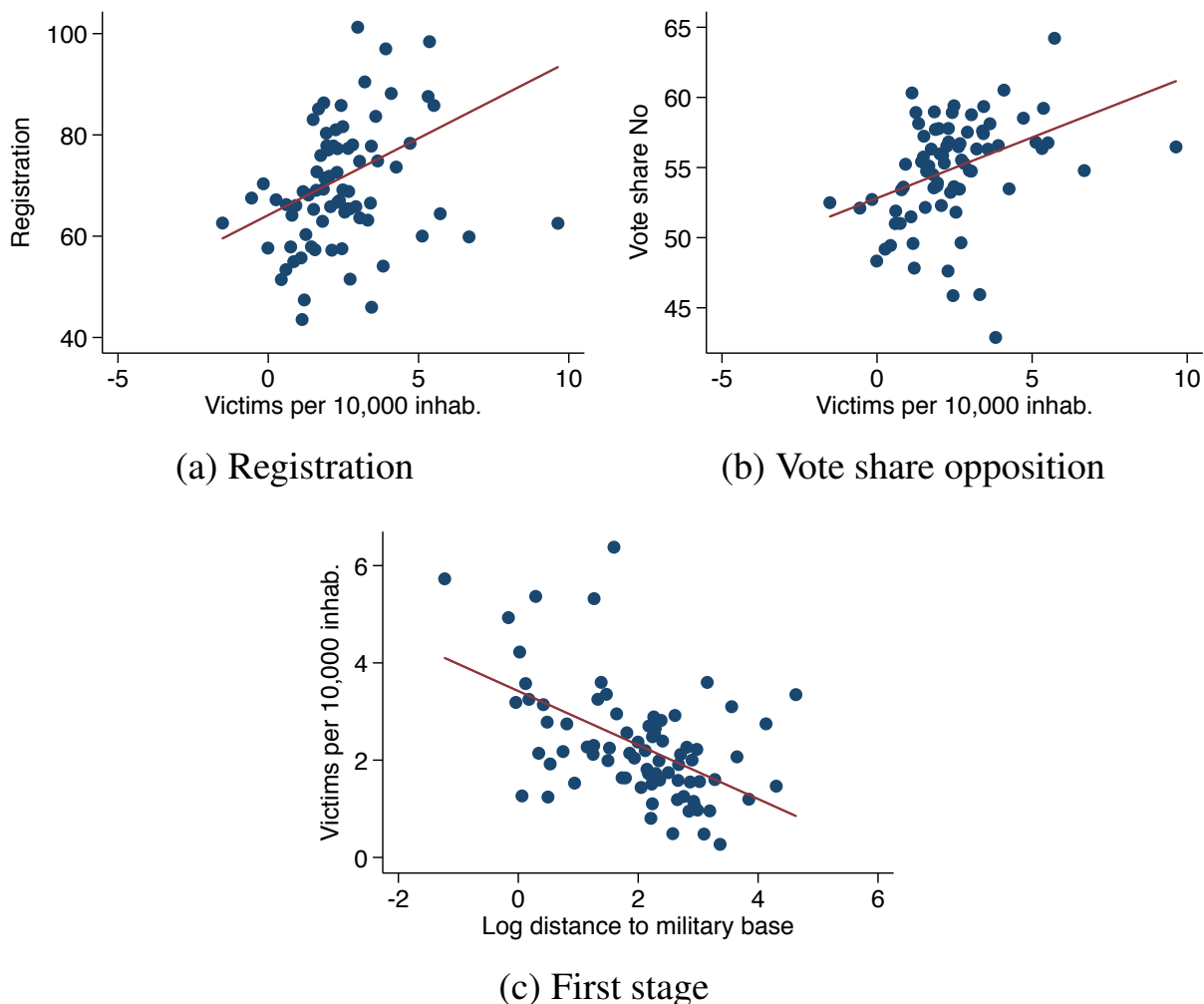
**Figure 1:** Spatial variation in repression and exposure to military bases



(a) Victims per 10,000 inhab.      (b) Military bases in 1970

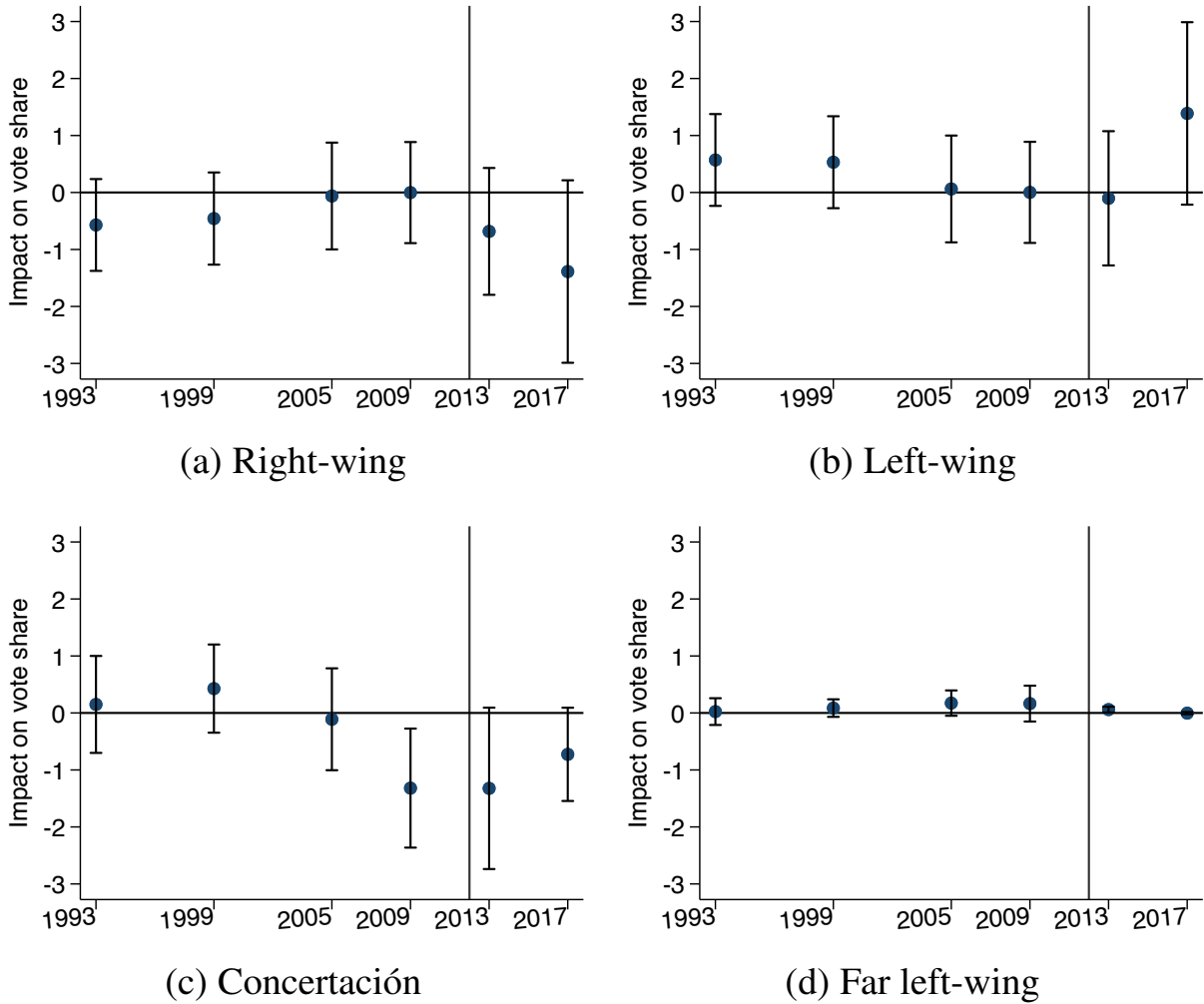
Notes: Panel (a) uses darker color in counties with more victims per 10,000 inhabitants, gray for counties without victims, and white for counties not included in the final sample. Panel (b) uses darker color for counties more exposed to military bases operating in 1970 and white for counties not included in the final samples. In the latter panel we also include red stars that denote the presence of at least one military base in the county.

**Figure 2: Repression and democratization**



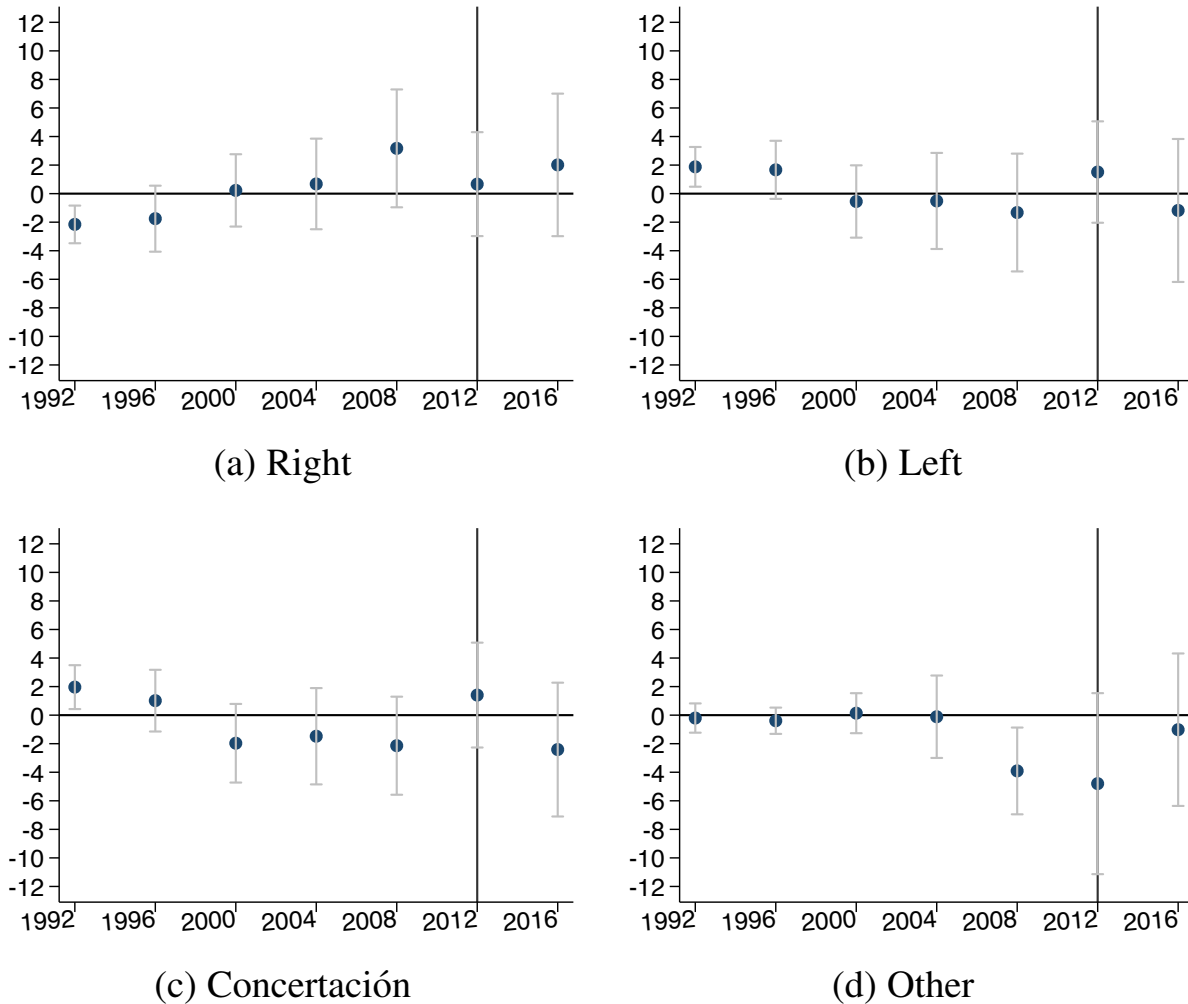
Notes: Panels (a) and (b) present the linear fit between electoral outcomes in the 1988 plebiscite and the number of victims per 10,000 inhabitants using the cross-section of 276 counties and controlling for vote shares in 1970, distances to Santiago and the corresponding regional capital, population in 1970, and province fixed effects. For coefficient estimates see column 4 in Table 3. Panel (c) presents the linear fit between victims per 10,000 inhab. and the logarithm of the distance to the closest military base, one of our instruments in the first-stage, controlling for the same variables that in the two previous panels. For coefficient estimates see column 2 in Table 4.

**Figure 3:** Presidential elections after the Pinochet dictatorship



Notes: Each panel presents six coefficients estimated using a two-stage least squares procedure and our main regression specification, equation (1), where the instrument is the indicator variable for counties with military bases in 1970. The circles represent the point estimate and the vertical lines the 95% confidence interval. We estimate each coefficient using data from one of the six presidential elections after democratization: 1993, 1999, 2005, 2009, 2013, and 2017. Panel (a) uses the vote share for right-wing candidates as dependent variable, panel (b) uses the vote share for left-wing candidates, panel (c) uses the vote share of candidates from the coalition called *Concertación* – coalition of five of the six presidents of Chile after the transition to democracy – and panel (d) uses the vote share for candidates to the left of *Concertación*. The vertical line in 2012 denotes the introduction of automatic registration and voluntary voting.

**Figure 4:** Local elections after the Pinochet dictatorship



Notes: Each panel presents six coefficients estimated using a two-stage least squares procedure and our main regression specification, equation (1), where the instrument is the indicator variable for counties with military bases in 1970. The circles represent the point estimate and the vertical lines the 95% confidence interval. We estimate each coefficient using data from one of the seven local elections after democratization: 1992, 1996, 2000, 2004, 2008, 2012 and 2016. Panel (a) uses the vote share for right-wing mayoral candidates as dependent variable, panel (b) uses the vote share for left-wing candidates, panel (c) uses the vote share of candidates from the coalition called *Concertación* – coalition of five of the six presidents of Chile after the transition to democracy – and panel (d) uses the vote share for all other candidates. The vertical line in 2012 denotes the introduction of automatic registration and voluntary voting.



**Table 1: Descriptive statistics**

	Unweighted	Weighted		Min	Max
	Mean	Mean	St. Dev		
<b>Main variables</b>	(1)	(2)	(3)	(4)	(5)
Victims per 10,000 inhabitants	1.38	2.31	2.01	0.00	11.89
Indicator military base	0.12	0.30	0.46	0.00	1.00
Log distance to closest base	3.55	3.10	1.19	0.88	8.18
Vote share NO in 1988	48.44	54.82	9.49	3.26	76.77
Registration in 1988	72.50	71.16	25.20	20.61	146.19
<b>Control variables</b>					
Vote share Alessandri in 1970	34.86	34.09	8.79	7.80	67.86
Vote share Allende in 1970	35.04	37.17	10.84	4.17	76.78
Log distance to Santiago	5.52	4.72	1.92	0.94	8.23
Log distance to regional capital	3.87	2.80	1.65	0.00	8.21
Population in 1970	29,010	–	–	339	321,250

Notes: Descriptive statistics for 276 counties in Chile. The “main variables” are the dependent variables, the endogenous variable, and the instruments in the two-stage least squares framework. The “control variables” are those used as controls in our main specification, i.e. equation (1). The statistics in columns (2) and (3) are weighted by county population in 1970, except for “Population in 1970.” We construct electoral outcomes from administrative data kept at Chile’s Electoral Service. The number of victims by county comes from the Rettig report. Population in 1970 comes from the housing census. All distances are calculated from a county’s centroid.

**Table 2: Pre-existing differences in counties with varying exposure to the military**

	Avg without military presence (1)	Indicator military base		Log distance to nearest base	
		Without Province FE (2)	With Province FE (3)	Without Province FE (4)	With Province FE (5)
Vote share Salvador Allende in 1964	40.23 ( 11.15)	-0.90 ( 1.64)	-2.50 ( 1.68)	0.75* ( 0.41)	0.80 ( 0.49)
Vote share Eduardo Frei in 1964	54.82 ( 11.37)	0.13 ( 1.72)	2.22 ( 1.65)	-1.33*** ( 0.42)	-0.88* ( 0.46)
Vote share Salvador Allende in 1970	37.07 ( 12.21)	0.31 ( 1.84)	0.25 ( 1.86)	0.22 ( 0.48)	-0.09 ( 0.57)
Vote share Jorge Alessandri in 1970	34.11 ( 9.81)	-0.07 ( 1.67)	0.74 ( 1.43)	-0.27 ( 0.38)	-0.28 ( 0.40)
Vote share UP municipal election 1971	48.52 ( 12.95)	1.95 ( 2.33)	0.56 ( 2.20)	-1.17** ( 0.56)	-0.03 ( 0.62)
Indicator UP mayor 1971	0.32 ( 0.47)	-0.10 ( 0.10)	-0.10 ( 0.09)	0.01 ( 0.02)	0.03 ( 0.02)
Vote share UP legislative election 1973	44.81 ( 11.87)	-1.52 ( 1.68)	-1.50 ( 1.90)	0.89** ( 0.41)	0.58 ( 0.55)
Log distance to Santiago	4.30 ( 1.98)	1.39*** ( 0.42)	0.18* ( 0.11)	0.19* ( 0.11)	0.04 ( 0.03)
Log distance to regional capital	3.04 ( 1.30)	-0.78 ( 0.51)	-1.32*** ( 0.37)	0.48*** ( 0.12)	0.53*** ( 0.09)
Indicator landlocked	0.72 ( 0.45)	-0.16 ( 0.12)	-0.03 ( 0.08)	-0.01 ( 0.03)	0.01 ( 0.02)
Houses per capita in 1970	0.20 ( 0.04)	0.00 ( 0.00)	-0.00 ( 0.00)	-0.00 ( 0.00)	0.00 ( 0.00)
Land inequality in 1965	0.84 ( 0.15)	0.03 ( 0.03)	0.04** ( 0.02)	-0.00 ( 0.01)	-0.00 ( 0.01)
Agrarian reform until 1973	0.21 ( 0.25)	-0.02 ( 0.05)	-0.02 ( 0.03)	0.02 ( 0.01)	0.02*** ( 0.01)
Trade liberalization	-0.20 ( 0.18)	0.02 ( 0.06)	0.03 ( 0.03)	-0.00 ( 0.02)	-0.01 ( 0.01)
Population density 1960	1431.41 ( 2659.18)	-1174.32** ( 517.96)	-364.50 ( 341.04)	-235.46* ( 120.28)	-152.45 ( 96.88)
Population density 1970	2723.20 ( 4715.53)	-2408.34** ( 955.76)	-877.05 ( 622.61)	-417.81** ( 212.13)	-247.33 ( 153.77)

Notes: Column 1 provides the average and standard deviation (in parenthesis) for each variable for counties without military bases. Column 2 shows point estimates and standard errors of a univariate regression of each variable on the dummy for presence of military bases in 1970. Column 3 adds province fixed effects. Columns 4 and 5 replicate the analysis for the continuous measure of log distance to the nearest base. All vote shares are measured as percentages, from 0 to 100. All regressions are weighted by population in 1970. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 3: Repression and the 1988 plebiscite: OLS estimates***Dependent variables: electoral outcomes at the 1988 plebiscite*

	(1)	(2)	(3)	(4)
<b>A. Registration</b>				
Victims per 10,000 inhab.	2.70* (1.46)	2.61** (1.24)	3.31*** (1.11)	3.03*** (0.92)
Vote share Alessandri in 1970		0.64 (0.54)	1.10** (0.52)	1.03** (0.44)
Vote share Allende in 1970		-0.18 (0.41)	0.27 (0.36)	0.39 (0.39)
Log distance to Santiago			23.65*** (4.75)	11.28*** (3.20)
Log distance to regional capital			-0.70 (1.37)	-6.10*** (1.33)
Population in 1970				-19.81*** (3.43)
<b>B. Vote share NO</b>				
Victims per 10,000 inhab.	1.14** (0.47)	1.14*** (0.24)	0.84*** (0.19)	0.87*** (0.18)
Vote share Alessandri in 1970		-0.21* (0.12)	-0.22** (0.11)	-0.21** (0.10)
Vote share Allende in 1970		0.44*** (0.08)	0.46*** (0.07)	0.44*** (0.07)
Log distance to Santiago			0.52 (1.28)	1.72 (1.15)
Log distance to regional capital			-1.46*** (0.23)	-0.94*** (0.28)
Population in 1970				1.92** (0.76)
Counties	276	276	276	276
R-squared (Panel A)	0.208	0.263	0.442	0.581
R-squared (Panel B)	0.432	0.721	0.755	0.764
Province fixed effects	x	x	x	x
Coefficient stability estimate (Panel A)				3.41
Coefficient stability estimate (Panel B)				0.67

Notes: This table presents estimates using electoral outcomes in the 1988 plebiscite as dependent variables and as right-hand side variables the number of victims during the dictatorship and different combinations of control variables (equation 1). All regressions include 25 province fixed effects and are weighted by county-level population in 1970. The coefficient stability in the last two rows correspond to a bounding exercise that accounts for the effect of unobservable variables that are correlated with the included controls, following the method proposed by Oster (2018). Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 4: Proximity to military bases: First stage and reduced form**

	<i>Victims per 10,000 inhab.</i> (first-stage)		<i>Registration</i> (reduced form)		<i>Vote share NO</i> (reduced form)	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Excluded instruments</b>						
Indicator military base	1.76*** (0.43)	2.17*** (0.46)	13.38*** (4.73)	19.11*** (3.74)	4.68*** (0.87)	5.56*** (0.91)
Log distance to military base		-0.57*** (0.21)		-7.90*** (2.87)		-1.22** (0.48)
<b>Controls</b>						
Vote share Alessandri in 1970	0.02 (0.04)	0.01 (0.04)	0.95** (0.48)	0.89** (0.42)	-0.25** (0.11)	-0.26** (0.10)
Vote share Allende in 1970	0.02 (0.03)	0.01 (0.02)	0.32 (0.40)	0.22 (0.37)	0.41*** (0.07)	0.40*** (0.07)
Log distance to Santiago	-0.96*** (0.37)	-0.65* (0.34)	5.09 (3.99)	9.42** (4.12)	-0.39 (1.11)	0.28 (1.15)
Log distance to regional capital	-0.16 (0.14)	-0.14 (0.15)	-5.70*** (1.34)	-5.33*** (1.40)	-0.73** (0.28)	-0.67** (0.28)
Population in 1970	-0.47 (0.34)	-0.54* (0.32)	-22.71*** (4.21)	-23.63*** (3.77)	0.94 (0.86)	0.80 (0.82)
Counties	276	276	276	276	276	276
<i>F</i> -stat excluded instruments	16.53	11.06				
R-squared	0.498	0.520	0.578	0.606	0.771	0.776
Province fixed effects	x	x	x	x	x	x

Notes: Each column in this table presents estimates of our two versions of the first-stage (columns 1 and 2, i.e. equations 2 and 3), and the corresponding reduced forms (columns 3-6, i.e. equation 1 using the instruments instead of victims in the right-hand side). The bottom of the table also presents the strength of the first-stage using a *F*-test for the statistical significance of the excluded instrument(s). All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 5:** Repression and the 1988 plebiscite: 2SLS estimates*Dependent variables: electoral outcomes at the 1988 plebiscite*

	Registration		Vote share NO	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	7.60*** (2.62) [0.07]	8.94*** (2.33) [0.03]	2.66*** (0.72) [0.00]	2.55*** (0.67) [0.01]
<b>Controls</b>				
Vote share Alessandri in 1970	0.82** (0.41)	0.76* (0.45)	-0.29** (0.12)	-0.29** (0.12)
Vote share Allende in 1970	0.19 (0.35)	0.14 (0.38)	0.37*** (0.09)	0.37*** (0.08)
Log distance to Santiago	12.41*** (3.29)	12.74*** (3.43)	2.17* (1.31)	2.14* (1.30)
Log distance to regional capital	-4.47*** (1.34)	-3.99*** (1.51)	-0.30 (0.42)	-0.33 (0.40)
Population in 1970	-19.13*** (2.94)	-18.93*** (3.05)	2.19** (0.88)	2.17** (0.86)
Counties	276	276	276	276
Province fixed effects	x	x	x	x
<i>F</i> -stat excl. instruments	16.53	11.06	16.53	11.06

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. Robust standard errors in parenthesis. In square brackets we present *p*-values when we allow errors to be correlated within provinces. Because there are only 25 provinces in the country, we use the small sample correction proposed by Cameron et al. (2008) using the code written by Roodman (2015). Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 6:** Repression and the 1989 presidential elections

	OLS			Reduced form			2SLS				
	Aylwin	Büchi		Aylwin	Büchi		Aylwin	Büchi			
	(1)	(2)		(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Victims per 10,000 inhab.	0.35** (0.17)	-0.54*** (0.19)						1.52*** (0.56)	1.25*** (0.46)	-2.04*** (0.60)	-1.67*** (0.52)
Indicator military presence				2.76*** (0.81)	2.73*** (0.86)	-3.70*** (0.78)	-3.65*** (0.89)				
Log distance to military base					-0.18 (1.79)		0.30 (1.61)				
<b>Controls</b>											
Vote share Alessandri in 1970	-0.39*** (0.11)	0.55*** (0.15)		-0.43*** (0.10)	-0.43*** (0.10)	0.60*** (0.14)	0.60*** (0.15)	-0.47*** (0.10)	-0.45*** (0.10)	0.66*** (0.14)	0.63*** (0.14)
Vote share Allende in 1970	0.33*** (0.07)	-0.16* (0.08)		0.30*** (0.07)	0.30*** (0.07)	-0.11 (0.08)	-0.11 (0.08)	0.29*** (0.07)	0.30*** (0.07)	-0.10 (0.09)	-0.11 (0.08)
Log distance to regional capital	1.15 (1.21)	-2.41** (1.03)		0.20 (1.16)	0.15 (0.97)	-1.13 (1.07)	-1.05 (0.95)	1.35 (1.39)	1.30 (1.30)	-2.67** (1.30)	-2.61** (1.19)
Log distance to Santiago	-0.51* (0.26)	0.20 (0.24)		-0.42 (0.26)	-0.42 (0.26)	0.09 (0.22)	0.09 (0.22)	-0.30 (0.32)	-0.35 (0.30)	-0.08 (0.34)	-0.01 (0.31)
Population in 1970	-0.59 (0.72)	-0.80 (0.67)		-1.10 (0.76)	-1.10 (0.76)	-0.11 (0.70)	-0.12 (0.71)	-0.64 (0.76)	-0.63 (0.73)	-0.73 (0.80)	-0.74 (0.74)
Counties	276	276		276	276	276	276	276	276	276	276
R-squared	0.716	0.690		0.726	0.726	0.706	0.706				
Province fixed effects	x	x		x	x	x	x	x	x	x	x
F-test of excluded instruments								18.66	12.34	18.66	12.34

Notes: This table presents OLS estimates, reduced forms, and two-stage estimates using our main specification (equation 1) but with electoral outcomes in the 1989 presidential election as dependent variables. Hernan Büchi was the presidential candidate of the coalition of political parties that supported the Pinochet regime, and Patricio Aylwin was the presidential candidate of the opposition coalition, runner-up and winner of the election respectively. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

# Appendix (for online publication)

## Appendix A Further information about the data

### A.1 *Victims*

As mentioned in the main text, we mostly rely on information about victims of the dictatorship from the Rettig report. This report was digitized by the Museum of Memory and Human Rights, an institution that draws attention to human rights violations committed in Chile during the dictatorship. From the resulting dataset, we observe each victim’s full name, the county of detention or execution, the exact date of detention or execution, political affiliation (if any), age, and occupation. We have complemented this information by manually reconstructing the county of residence and work for a large subset of the victims. We also add 66 cases of surviving victims who were tortured and for whom similar information is available at judicial records kept by the same museum. However, we must exclude victims for which the county of detention/execution is unknown and victims who were assassinated abroad, which reduces the total number to 3,150 (98% of total). We also exclude from the analysis those counties lacking 1970 population data – leaving us with 289 (85% of the 340 with plebiscite data). We drop four further counties because they lack results for the 1970 presidential election, as well as 13 outliers in terms of victims per 10,000 inhabitants. Our final sample contains full information for 276 counties. Figure A8 provides more details about this attrition.

### A.2 *Military bases*

Our measures of proximity to military bases are calculated using the location of army regiments (“regimientos”). These regiments have various functions: infantry, armored cavalry, artillery, engineering, communications, transportation and logistics. We also have information about air force bases and military academies, which we use for robustness checks. Our measure of distance is calculated as the logarithm of the distance from a county’s centroid to that of the centroid of the nearest county with a base, ignoring one’s own bases. These are straight-line ‘as-the-crow-flies’ distances.

### A.3 *Electoral outcomes*

We define the voter registration rate as the number of people that registered to vote in the plebiscite divided by population in 1970. This normalization can give rise to registration rates above 100% because of internal migration or population growth, or because people registered in counties different from where they lived. In any case, counties with more registered voters than population are

small and have little weight in our estimations. We winsorize the voter registration rate at the 98th percentile. As part of our robustness checks, we show that the results are unaffected by this choice.

Regarding the “No” vote share, we divide the number of votes for this option by the number of valid votes, excluding null and blank votes. Results are unaffected if we use the total number of votes cast as denominator. The correlation between both measures is 0.999.

Our sample delivers very similar aggregate election outcomes to the real aggregates including all counties, suggesting little sample selection from the counties we drop. For example, the aggregate vote share for the “No” option was 54.71% and it is an almost identical 54.82% in our sample. Similarly, in our data 37.17% of people voted for Salvador Allende and 34.09% for Jorge Alessandri in the 1970 elections, compared to 36.63% and 35.29% respectively in the full aggregate.

#### *A.4 Other sources*

Our analysis also uses information from the 1965 agricultural census. We use county-level measures of land inequality from the census to characterize the mostly rural society of the time. We also use data measuring the percentage of agricultural land expropriated during the implementation of the agrarian reform, which was one of the most important national policies of the 1960s and 1970s. The source for both of these pieces of data is Cuesta et al. (2017).

The 1970 population and housing census provides us with population counts. We use this census, instead of the more recent one from 1982, as population may have endogenously responded to repression by then. For instance, estimates of the number of people in exile due to the dictatorship range from 130,000 to 200,000, corresponding to 1.5-2.3% of the total population in 1970 (Orellana, 2015). Similarly, the 1992 census may reflect population movements triggered by the return to democracy. We also use the 1970 census to construct county-level measures of wealth based on the number of houses per capita, which is arguably related to the level of income in the locality.

Finally, we use newly digitized data on local infrastructure projects funded by the military regime between 1979 and 1990. Examples of these projects include the construction of roads, houses, and sewers, among others (see González et al. 2018a for details). This data comes from annual reports prepared by the Ministry of Housing and Urban Planning, which was in charge of approximately five percent of the annual budget. The data include approximately 8,000 projects throughout Chile. We convert these projects into monetary units, which provides a measure of the amount of financial resources the Pinochet regime spent in each county.



## Appendix B Characterization of the complier counties

In any instrumental variables design, the sub-population induced to take (or not to take) the treatment because of the variation in the instrument is referred to as the set of ‘compliers.’ In our case, the compliers are the counties that were exposed to different amounts of repression because of their proximity to (or distance from) military bases. Following the technique proposed by Abadie et al. (2002), we can characterize this set of counties. This exercise allows us to evaluate the external validity of our estimates and also provides insights about the variation we are exploiting.

To facilitate the interpretation, we focus on a binary treatment and a binary instrument. Regarding repression, we use a dummy equal to one if the number of victims per 10,000 inhabitants in the county is in the top quartile of the distribution. The average number of victims per 10,000 inhabitants in the top quartile is 4.3. As part of our robustness checks, Table A17 presents two-stage estimates using this specification. We refer to these counties as experiencing ‘high’ repression. Regarding military bases, we focus on the indicator for presence. We define as ‘treated compliers’ those counties with bases and high repression, while counties without bases and without high repression are called ‘untreated compliers.’ We then estimate the following regression:

$$Y_{i,t} = \mu R_{i,t \in [1973, 1988]} + \tau X_{i,t \leq 1970} + \lambda_p + \varepsilon_{ip} \quad (4)$$

where  $Y_{i,t}$  is a variable we use to characterize compliers and  $R_{i,t \in [1973, 1988]}$  is the indicator for high repression. The parameter  $\mu$  measures the average characteristic among treated compliers. We can replace  $R_{i,t \in [1973, 1988]}$  by  $1 - R_{i,t \in [1973, 1988]}$  to characterize untreated compliers.

Panel A in Table A25 speaks to the external validity of our estimates. Columns 1-3 show that the average characteristics of complier counties are similar to those of the average county, with the exception that compliers voted relatively more for the left-wing candidate in 1970. Thus, our instrumental variables estimates capture the effect of repression on counties with similar wealth and inequality than the average county but with different political preferences. Moreover, the comparison between columns 1 and 2 confirms the *internal* validity of our econometric design because treated and untreated complier counties were very similar before 1973.

Panel B studies county characteristics after 1973. The difference between treated and untreated compliers is equivalent to the local average treatment effect. Reassuringly, the “Plebiscite” sub-panel shows that the estimate we obtained when using the ‘high’ repression indicator is similar to what we obtained using the continuous treatment (see Table A17 for details). Moreover, the ‘Repression year’ sub-panel suggests that our first stage is stronger in counties that experienced violence at the beginning of the dictatorship. This result is consistent with historical details provided in section 2.1, where we document how the repressive apparatus changed after 1974, with DINA becoming mostly responsible. Finally, the ‘Profession’ and ‘Age categories’ sub-panels show that victims in complier counties were more likely to have been middle-age laborers or farmers affiliated to a political party.

## Appendix C Additional Figures and Tables

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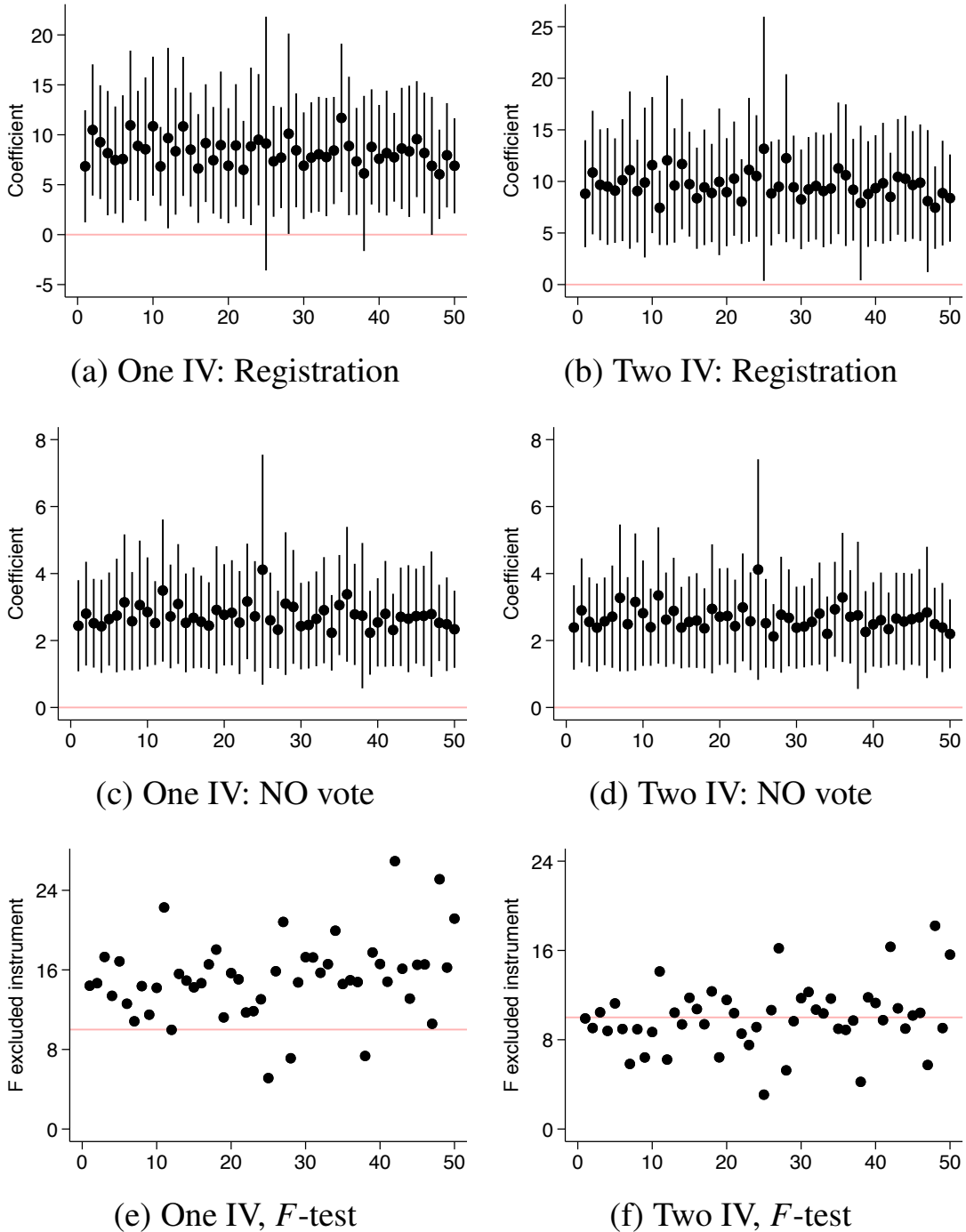
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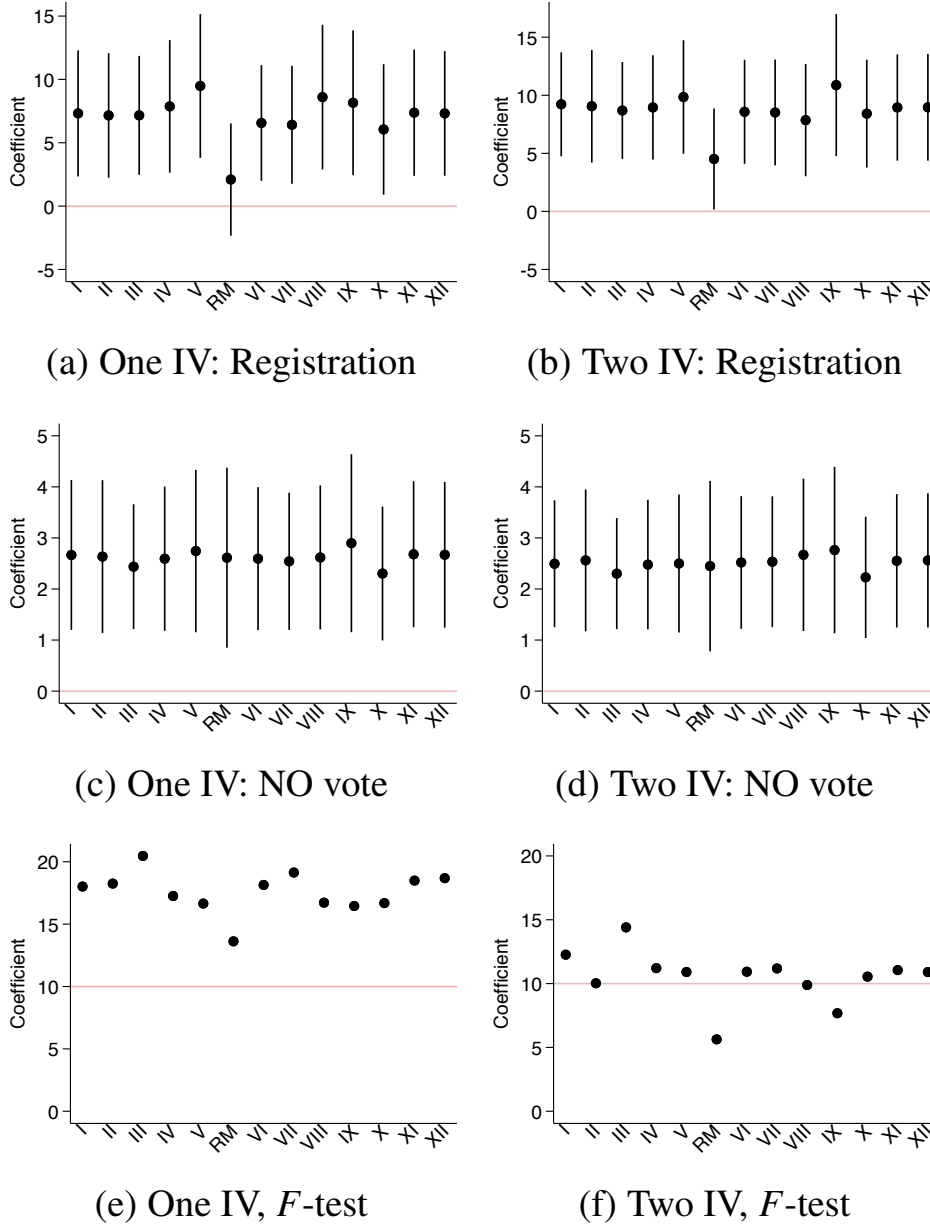
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**Figure A1: Robustness of results to excluding sets of counties**



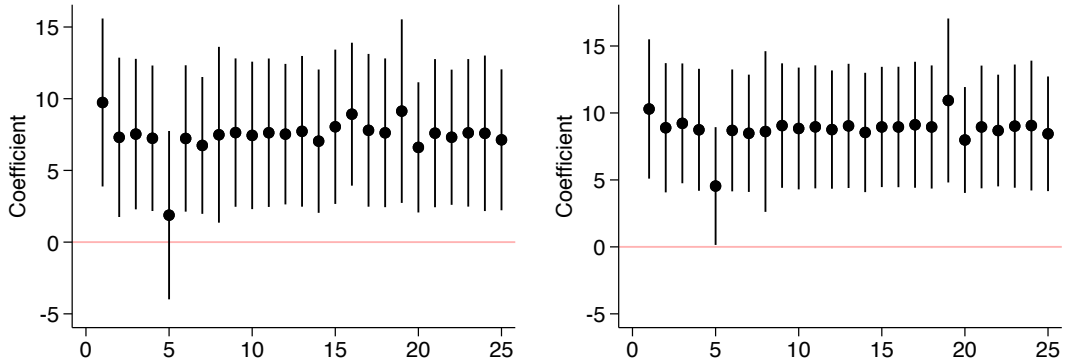
Notes: The y-axis represents the value of the two-stage least squares coefficient associated to victims per 10,000 inhabitants in panels (a), (b), (c), and (d), and the  $F$ -test for the excluded instrument(s) in panels (e) and (f). The  $x$ -axis corresponds to 50 different samples of counties, where we exclude 20 randomly chosen counties each time.

**Figure A2: Robustness of results to excluding regions**



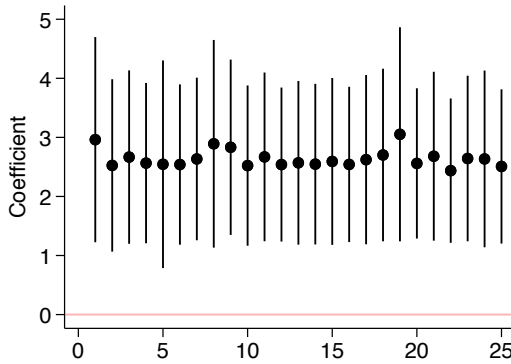
Notes: The y-axis represents the value of the two-stage least squares coefficient associated to victims per 10,000 inhabitants in panels (a), (b), (c), and (d), and the  $F$ -test for the excluded instrument(s) in panels (e) and (f). The x-axis corresponds to 13 different samples of counties, where we exclude all counties from a region – the largest administrative unit – each time. Regions are identified by roman numbers (I,II,..., XII) with the exception of the Metropolitan Region (RM) where Santiago is located.

**Figure A3: Robustness of results to excluding provinces**

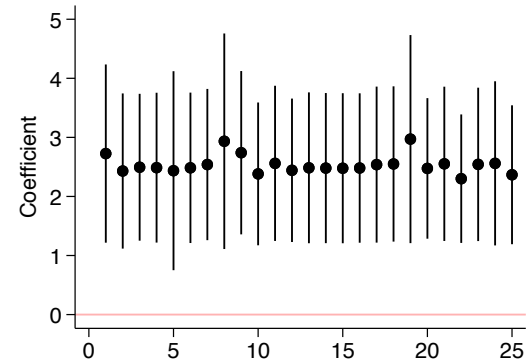


(a) One instrument: Registration

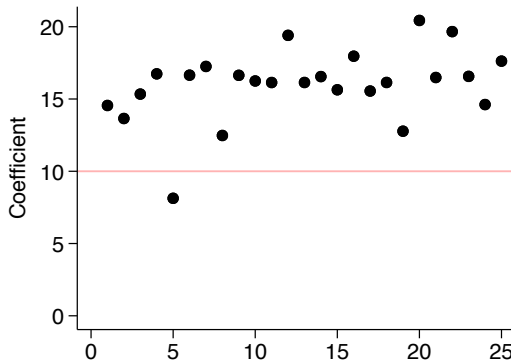
(b) Two instruments: Registration



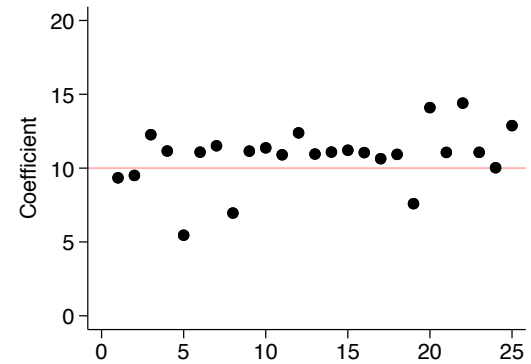
(c) One instrument: vote NO



(d) Two instruments: vote NO



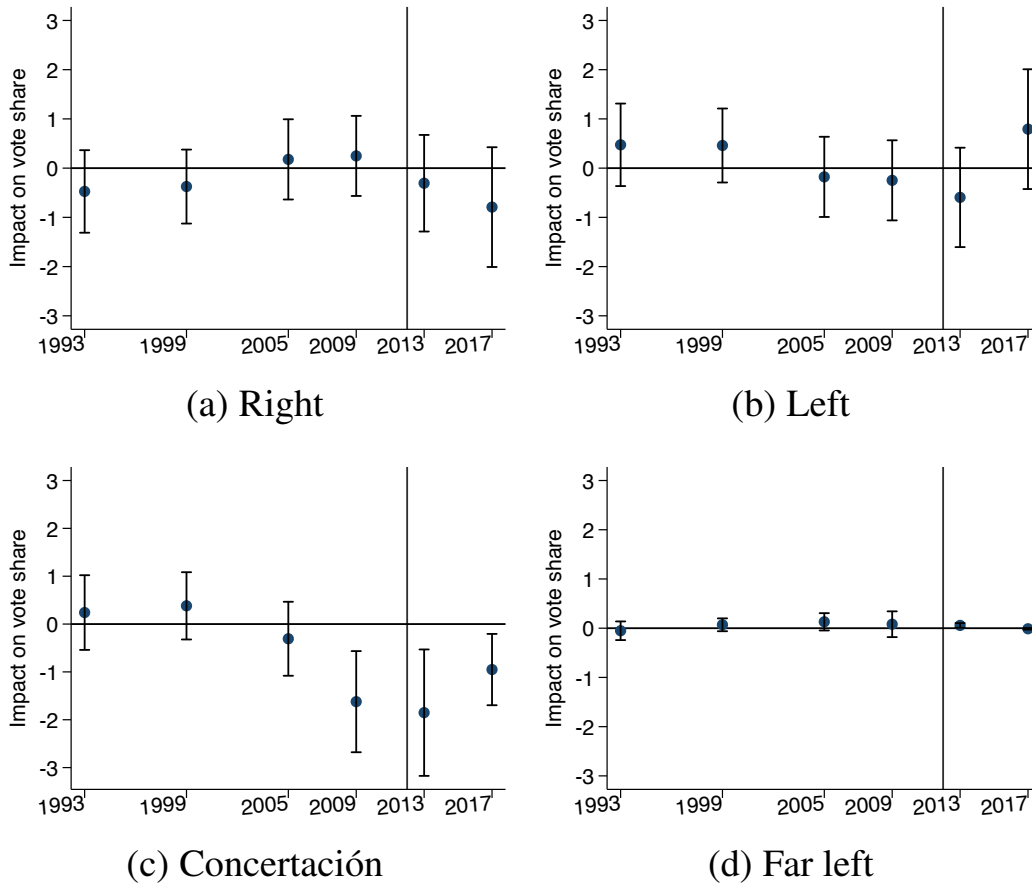
(e) One instrument,  $F$ -test



(f) Two instruments,  $F$ -test

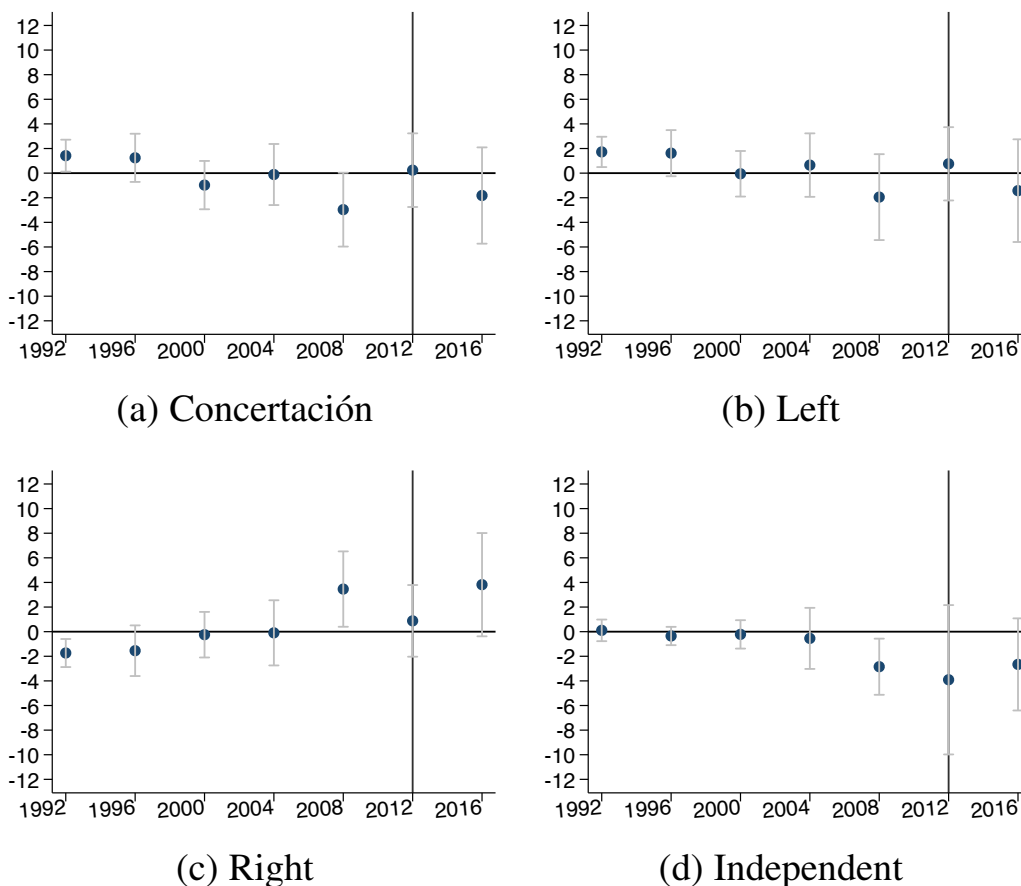
Notes: The y-axis represents the value of the two-stage least squares coefficient associated to victims per 10,000 inhabitants in panels (a), (b), (c), and (d), and the  $F$ -test for the excluded instrument(s) in panels (e) and (f). The  $x$ -axis corresponds to 25 different samples of counties, where we exclude all counties from a province – the second largest administrative unit – each time. Provinces are identified by numbers 1, . . . , 25.

**Figure A4:** Presidential elections after the Pinochet dictatorship: two instruments



Notes: Each panel presents six coefficients estimated using a two-stage least squares procedure and our main regression specification, equation (1), where the instruments are the dummy for presence of military bases and the  $\ln$  distance to the nearest base in 1970. The circles represent the point estimate and the vertical lines the 95% confidence interval. We estimate each coefficient using data from one of the six presidential elections after democratization: 1993, 1999, 2005, 2009, 2013, and 2017. Panel (a) uses the vote share for right-wing candidates as dependent variable, panel (b) uses the vote share for left-wing candidates, panel (c) uses the vote share of candidates from the coalition called *Concertación* – coalition of five of the six presidents of Chile after the transition to democracy – and panel (d) uses the vote share for candidates to the left of *Concertación*. The vertical line in 2012 denotes the introduction of automatic registration and voluntary voting.

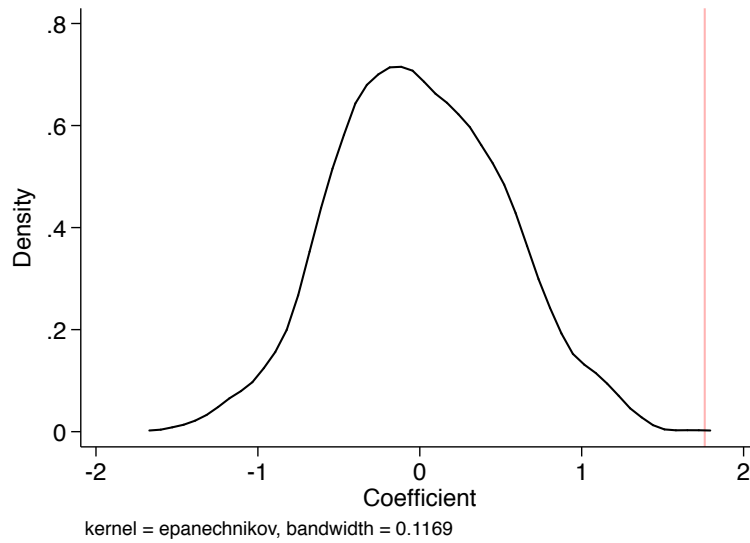
**Figure A5:** Local elections after the Pinochet dictatorship: two instruments



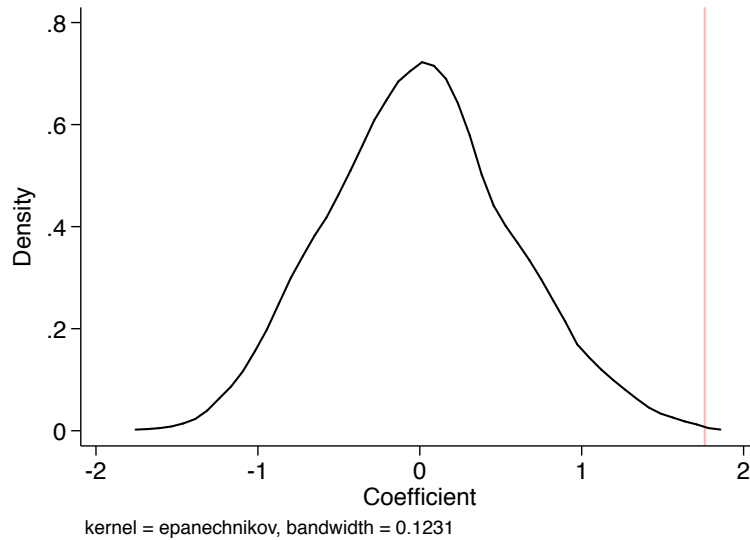
Notes: Each panel presents six coefficients estimated using a two-stage least squares procedure and our main regression specification, equation (1), where the instruments are the dummy for presence of military bases and the  $\ln$  distance to the nearest base in 1970. The circles represent the point estimate and the vertical lines the 95% confidence interval. We estimate each coefficient using data from one of the seven local elections after democratization: 1992, 1996, 2000, 2004, 2008, 2012 and 2016. Panel (a) uses the vote share for right-wing mayoral candidates as dependent variable, panel (b) uses the vote share for left-wing candidates, panel (c) uses the vote share of candidates from the coalition called *Concertación* – coalition of five of the six presidents of Chile after the transition to democracy – and panel (d) uses the vote share for all other candidates. The vertical line in 2012 denotes the introduction of automatic registration and voluntary voting.



**Figure A6: Random treatment assignment (Placebo)**



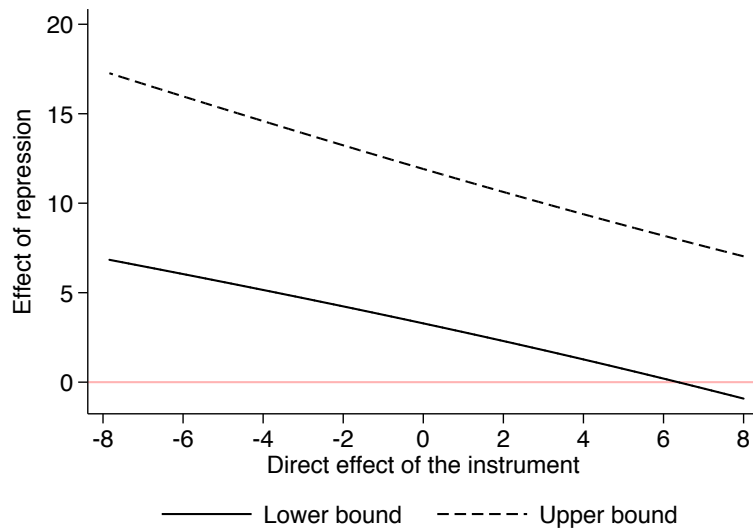
(a) Countrywide



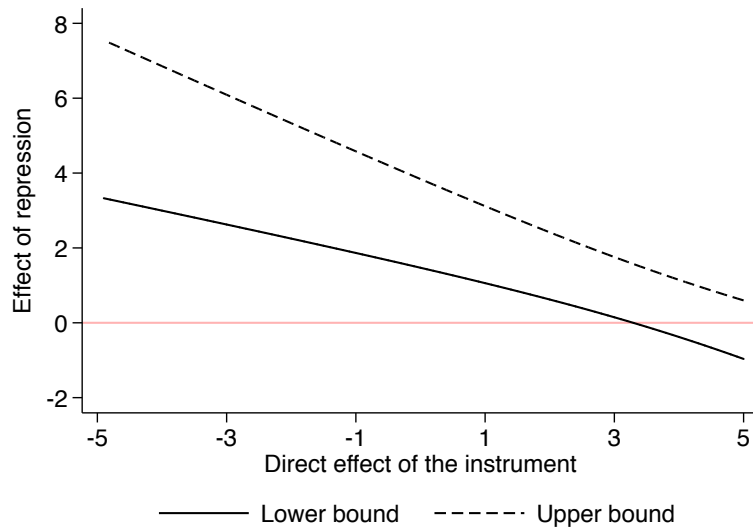
(b) Within-province

Notes: This figure presents the distribution of point estimates from a series of first stage regressions in which the instrument is randomly assigned across counties. Panel (a) randomly assigns the 34 treated units among all counties in the country, while panel (b) randomly assigns the treatment within provinces, based on the actual number of treated counties in that province. We perform each set of randomizations 1,000 times. The red line shows the point estimate from the first stage presented in column 1 of table 4.

**Figure A7: Relaxing the exogeneity assumption**



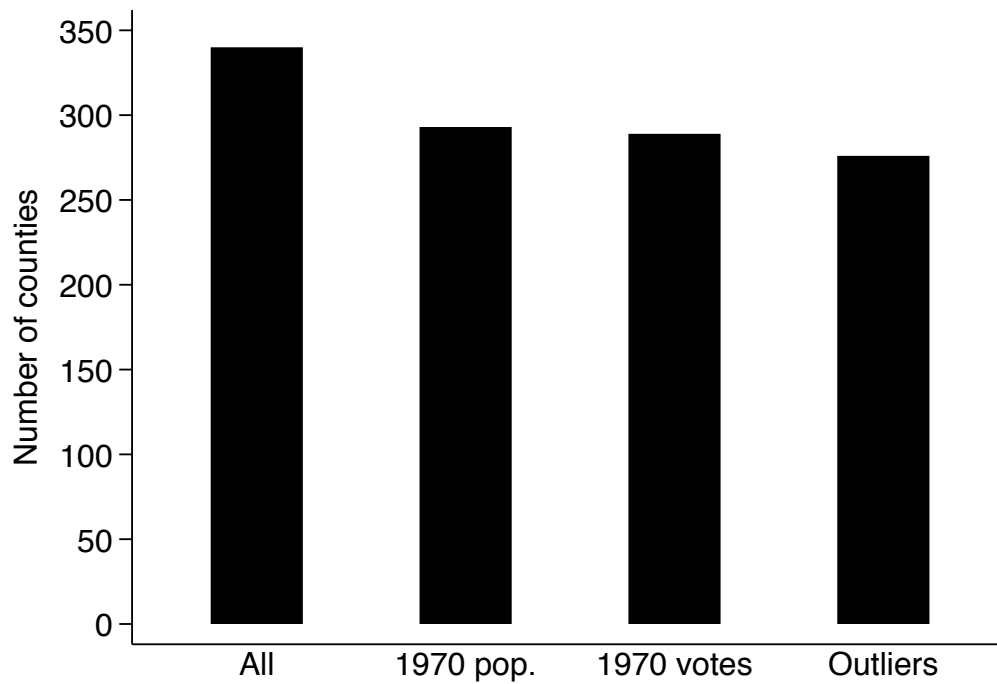
(a) Registration



(b) Vote share NO

Notes: These figures present results from a bounding exercise in which we allow military bases to affect outcomes directly. The  $x$ -axis measures (theoretical) direct effects of military bases on registration (Panel A) and the NO vote share (Panel B). The  $y$ -axis measures the corresponding effect of repression. Overall, we find that to make the effect of repression non-different from zero we need the direct effect of bases to be 6.2 and 3.2 in panels A and B, equivalent to 46% (6.2/13.4) and 68% (3.2/4.7) of the reduced form effect. See Conley et al (2012) for details.

**Figure A8:** Characterization of sample attrition



Notes: This table describes the attrition process in our sample. The universe of potential counties in our data is 340 counties, i.e. those with vote shares data in the 1988 plebiscite (“All”). The sample decreases to 293 counties because of missing population data in the 1970 census (“1970 pop.”). Then the sample decreases to 289 because of missing 1970 vote shares (“1970 votes”). Finally, the sample decreases to 276 counties after deleting 5% of counties we considered to be outliers in terms of victims per 10,000 inhab. (“Outliers”).

**Table A1: Military bases and torture centers***Dependent variable is the number of torture centers*

	Number of torture centers		Presence of torture center		Torture centers per 10,000 inhab.	
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator military presence	4.91*** (0.86)	4.94*** (0.84)	0.06* (0.03)	0.09** (0.04)	0.24*** (0.09)	0.23** (0.10)
Log distance to closest military base		-0.05 (0.56)		-0.04 (0.03)		0.01 (0.04)
Observations	276	276	276	276	276	276
Baseline controls	x	x	x	x	x	x
Province fixed effects	x	x	x	x	x	x
R-squared	0.834	0.834	0.119	0.128	0.565	0.565
Avg. dependent variable	2.48	2.48	0.84	0.84	1.14	1.14

Notes: This table presents the empirical relationship between military bases and the number of torture centers, where people were murdered, tortured, and kept as political prisoners. We interpret these results as military bases increasing *overall* repression, not just murders. Baseline controls include vote shares for Alessandri and Allende in 1970, the logarithm of distances to Santiago and the corresponding regional capital, and population in 1970. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A2: Robustness of first-stage to different combinations of controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Dependent variable is dictatorship victims per 10,000 inhab.</i>								
<b>Instruments</b>								
Indicator military presence	1.59*** (0.31)	1.55*** (0.32)	1.60*** (0.42)	1.76*** (0.43)	2.04*** (0.30)	2.01*** (0.32)	1.95*** (0.45)	2.17*** (0.46)
Log distance to military facility					-0.65*** (0.17)	-0.64*** (0.17)	-0.52*** (0.19)	-0.57*** (0.21)
<b>Controls</b>								
Vote share Alessandri in 1970		0.04 (0.05)	0.02 (0.04)	0.02 (0.04)		0.02 (0.04)	0.02 (0.04)	0.01 (0.04)
Vote share Allende in 1970		0.03 (0.03)	0.02 (0.03)	0.02 (0.03)		0.01 (0.02)	0.01 (0.03)	0.01 (0.02)
Log distance to Santiago			-0.62** (0.26)	-0.96*** (0.37)			-0.29 (0.26)	-0.65* (0.34)
Log distance to regional capital			-0.06 (0.12)	-0.16 (0.14)			-0.03 (0.13)	-0.14 (0.15)
Population in 1970				-0.47 (0.34)				-0.54* (0.32)
Observations	276	276	276	276	276	276	276	276
R-squared	0.457	0.460	0.486	0.498	0.500	0.501	0.505	0.520
Province fixed effects	x	x	x	x	x	x	x	x
F-test excluded instruments	25.64	23.03	14.36	16.53	22.75	19.96	9.335	11.06

Notes: This table checks the robustness of our two versions of the first-stage when we control for different sets of control variables. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A3: Robustness of reduced form (registration) to different combinations of controls**

*Dependent variable is electoral outcome in the 1988 plebiscite*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Instruments</b>								
Indicator military presence	11.37** (5.09)	10.93* (5.58)	5.39 (4.80)	13.38*** (4.73)	8.67* (5.14)	8.60 (5.50)	9.41** (4.50)	19.11*** (3.74)
Log distance to military facility					3.88 (4.06)	3.25 (3.71)	-5.88** (2.72)	-7.90*** (2.87)
<b>Controls</b>								
Vote share Alessandri in 1970		0.64 (0.61)	1.17* (0.61)	0.95** (0.48)	0.72 (0.62)	0.72 (0.62)	1.13* (0.60)	0.89** (0.42)
Vote share Allende in 1970		-0.17 (0.43)	0.32 (0.38)	0.32 (0.40)	-0.08 (0.42)	-0.08 (0.42)	0.25 (0.36)	0.22 (0.37)
Log distance to Santiago			21.56*** (4.56)	5.09 (3.99)	25.28*** (4.75)	25.28*** (4.75)	9.42** (4.12)	9.42** (4.12)
Log distance to regional capital			-0.89 (1.24)	-5.70*** (1.34)	-0.47 (1.30)	-0.47 (1.30)	-5.33*** (1.40)	-5.33*** (1.40)
Population in 1970				-22.71*** (4.21)				-23.63*** (3.77)
Observations	276	276	276	276	276	276	276	276
R-squared	0.212	0.266	0.406	0.578	0.221	0.273	0.422	0.606
Province fixed effects	x	x	x	x	x	x	x	x

Notes: This table checks the robustness of the reduced forms when we control for different sets of control variables. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A4: Robustness of reduced form (“NO”) to different combinations of controls***Dependent variable is electoral outcome in the 1988 plebiscite*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Instruments</b>								
Indicator military presence	5.94*** (1.29)	6.00*** (0.87)	5.01*** (0.88)	4.64*** (0.88)	7.84*** (1.26)	7.37*** (0.91)	5.89*** (0.90)	5.56*** (0.91)
Log distance to military facility					-2.74*** (0.69)	-1.90*** (0.52)	-1.29*** (0.50)	-1.22*** (0.48)
<b>Controls</b>								
Vote share Alessandri in 1970		-0.22* (0.12)	-0.26** (0.11)	-0.25** (0.11)		-0.27** (0.11)	-0.26** (0.10)	-0.26** (0.10)
Vote share Allende in 1970		0.43*** (0.08)	0.41*** (0.07)	0.41*** (0.07)		0.38*** (0.07)	0.39*** (0.07)	0.40*** (0.07)
Log distance to Santiago			-1.07 (1.20)	-0.34 (1.39)			-0.26 (1.28)	0.28 (1.15)
Log distance to regional capital			-0.92*** (0.25)	-0.73** (0.28)			-0.83*** (0.24)	-0.67** (0.28)
Population in 1970				0.95 (0.85)				0.80 (0.82)
Observations	276	276	276	276	276	276	276	276
R-squared	0.458	0.748	0.769	0.772	0.492	0.764	0.774	0.776
Province fixed effects	x	x	x	x	x	x	x	x

Notes: This table checks the robustness of the reduced forms when we control for different sets of control variables. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A5: Robustness of 2SLS results to different combinations of controls**

*Dependent variable is electoral outcome in the 1988 plebiscite*

<b>Registration</b>	One instrument			Two instruments				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Victims per 10,000 inhab.	7.169** (3.019)	7.051** (3.308)	3.377 (2.611)	7.600*** (2.623)	3.253 (2.368)	3.433 (2.369)	5.011** (2.084)	8.940*** (2.335)
<b>Vote share NO</b>								
Victims per 10,000 inhab.	3.747*** (0.862)	3.871*** (0.893)	3.138*** (0.920)	2.655*** (0.720)	3.882*** (0.686)	3.595*** (0.698)	3.004*** (0.824)	2.552*** (0.665)
Observations	276	276	276	276	276	276	276	276
Votes shares in 1970		x	x	x		x	x	x
Distances			x	x			x	x
Population in 1970				x				x
Province fixed effects	x	x	x	x	x	x	x	x
F-test of excluded instruments	25.64	23.03	14.36	16.53	22.75	19.96	9.335	11.06

Notes: This table checks the robustness of the two-stage estimates when we control for different sets of control variables. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



**Table A6:** Robustness of results to use of LASSO for selecting controls*Dependent variables: electoral outcomes at the 1988 plebiscite*

	Registration		Vote share NO	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	7.29*** (2.62)	8.76*** (2.32)	2.70*** (0.73)	2.57*** (0.67)
Observations	276	276	276	276
Province fixed effects	x	x	x	x
ML Controls	x	x	x	x
Instrumental variables	1	2	1	2
<i>F</i> - stat excl. instruments	16.55	11.13	16.55	11.13

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. We select controls using LASSO as proposed by Belloni et al (2014). Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A7: Robustness of results to inclusion of spatial controls**

*Dependent variables: electoral outcomes at the 1988 plebiscite*

Controls: Dependent variable:	Lat/Lon polynomials			Spatial centrality			Moran Eigenvectors					
	Registration (1)	Vote share NO (2)	Vote share NO (3)	Registration (4)	Vote share NO (5)	Registration (6)	Vote share NO (7)	Registration (8)	Vote share NO (9)	Registration (10)	Vote share NO (11)	Registration (12)
Victims per 10,000 inhab.	7.93*** (2.78)	9.33*** (2.36)	2.88*** (0.80)	2.72*** (0.71)	7.31*** (2.48)	8.61*** (2.16)	2.46*** (0.64)	2.34*** (0.58)	7.31*** (2.48)	8.94*** (2.33)	2.66*** (0.72)	2.55*** (0.67)
<b>Additional spatial controls</b>												
Longitude	-27.76 (27.23)	-33.85 (28.74)	-22.48** (9.36)	-21.80** (9.28)								
Latitude	-18.36 (15.96)	-21.10 (16.92)	-6.25 (5.09)	-5.94 (4.78)								
Longitude squared	-0.17 (0.16)	-0.20 (0.17)	-0.13** (0.05)	-0.13** (0.05)								
Latitude squared	-0.22 (0.24)	-0.24 (0.26)	-0.09 (0.08)	-0.09 (0.08)								
Log avg. distance to all other counties					-33.91 (22.23)	-38.17 (23.39)	-22.60* (12.38)	-22.18* (12.13)				
Observations	276	276	276	276	276	276	276	276	276	276	276	276
Eigenvectors with eigenvalues > 0									x	x	x	x
Baseline controls	x	x	x	x	x	x	x	x	x	x	x	x
Province fixed effects	x	x	x	x	x	x	x	x	x	x	x	x
F-test excluded instruments	14.78	10.59	14.78	10.59	18.20	13.04	18.20	13.04	18.20	11.06	16.53	11.06

Notes: This table checks the robustness of results to the inclusion of spatial variables that capture a potential effect of the geographic location of counties. Columns 1-4 include second degree polynomials of latitude and longitude, columns 5-8 include the logarithm of the average distance to all other counties, and columns 9-12 include Moran eigenvectors with positive eigenvalues as controls. Odd columns use one instrument and even columns use two instruments. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A8:** Robustness of results to exclusion of population weights*Dependent variables: electoral outcomes at the 1988 plebiscite*

	Registration		Vote share NO	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	10.69*** (3.83)	10.79*** (3.88)	3.59*** (1.23)	3.61*** (1.25)
<b>Controls</b>				
Vote share Jorge Alessandri in 1970	0.67* (0.40)	0.67* (0.40)	-0.31** (0.13)	-0.31** (0.13)
Vote share Salvador Allende in 1970	0.34 (0.33)	0.34 (0.33)	0.36*** (0.09)	0.36*** (0.09)
Log distance to Santiago	5.37* (2.98)	5.38* (3.00)	1.16 (1.12)	1.16 (1.13)
Log distance to regional capital	-4.48** (2.12)	-4.46** (2.13)	-0.06 (0.69)	-0.05 (0.69)
Population in 1970	-20.03*** (4.49)	-20.08*** (4.51)	3.34** (1.41)	3.34** (1.42)
Observations	276	276	276	276
Province fixed effects	x	x	x	x
<i>F</i> - stat excl. instruments	10.60	6.184	10.60	6.184

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. Robust standard errors in parenthesis. In square brackets we present *p*-values when we allow errors to be correlated within provinces. Because there are only 25 provinces in the country, we use the small sample correction proposed by Cameron et al. (2008) using the code written by Roodman (2015). Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A9: Robustness of results to exclusion of regional capitals**

	OLS			Reduced forms			Two-stage estimates			
	Regist.	NO		Registration	NO		Registration	NO		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Victims per 10,000 inhab.	3.07*** (0.99)	0.87*** (0.19)					8.88*** (2.53)	9.00*** (2.33)	2.36*** (0.64)	2.29*** (0.59)
Indicator military presence			20.36*** (4.28)	21.71*** (4.01)	5.41*** (1.02)	5.64*** (1.01)				
Log distance to military facility				-6.03** (2.59)		-1.03 (0.67)				
<b>Controls</b>										
Vote share Alessandri in 1970	1.08*** (0.38)	-0.22** (0.11)	0.84** (0.39)	0.88** (0.38)	-0.28*** (0.11)	-0.28** (0.11)	0.91** (0.39)	0.90** (0.39)	-0.27** (0.11)	-0.26** (0.11)
Vote share Allende in 1970	0.30 (0.37)	0.44*** (0.07)	0.06 (0.36)	0.07 (0.35)	0.38*** (0.07)	0.38*** (0.07)	0.15 (0.35)	0.14 (0.35)	0.40*** (0.08)	0.40*** (0.08)
Log distance to Santiago	15.62*** (3.82)	1.40 (1.21)	10.81** (4.34)	11.65*** (4.12)	0.10 (1.14)	0.24 (1.15)	17.79*** (4.13)	17.84*** (4.12)	1.96 (1.33)	1.93 (1.33)
Log distance to regional capital	-10.22*** (2.71)	-0.50 (0.82)	-13.44*** (3.04)	-9.89*** (3.17)	-1.38* (0.80)	-0.77 (0.84)	-7.81*** (2.95)	-7.76*** (2.90)	0.12 (0.88)	0.09 (0.86)
Population in 1970	-19.33*** (3.83)	1.97** (0.88)	-22.99*** (4.19)	-23.57*** (4.04)	0.98 (0.96)	0.88 (0.94)	-17.72*** (3.49)	-17.68*** (3.47)	2.38** (0.96)	2.36** (0.93)
Observations	264	264	264	264	264	264	264	264	264	264
R-squared	0.567	0.758	0.592	0.602	0.767	0.769				
Region fixed effects	x	x	x	x	x	x	x	x	x	x
F-test of excluded instruments							21.65	12.86	21.65	12.86

Notes: This table checks the robustness of results to the exclusion of regional capitals from the estimating sample. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A10: Robustness of results to inclusion of outliers***Dependent variables: electoral outcomes at the 1988 plebiscite*

	Registration		Vote share NO	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	7.15*** (2.16)	5.91*** (1.93)	1.50** (0.68)	1.16** (0.59)
<b>Controls</b>				
Vote share Alessandri	0.96* (0.53)	1.00** (0.47)	-0.19 (0.13)	-0.18 (0.12)
Vote share Allende	0.51 (0.44)	0.48 (0.41)	0.55*** (0.10)	0.54*** (0.09)
Ln distance to regional capital	13.99*** (4.71)	12.57*** (4.24)	3.65*** (1.36)	3.26*** (1.19)
Ln distance to capital	-6.01*** (1.37)	-5.96*** (1.27)	-1.44*** (0.38)	-1.43*** (0.34)
Population in 1970	-22.98*** (3.93)	-21.56*** (3.81)	-0.38 (0.99)	0.01 (0.85)
Observations	289	289	289	289
Province fixed effects	x	x	x	x
<i>F</i> -stat excl. instruments	10.49	5.472	10.49	5.472

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A11: Robustness of results to use of military bases built pre-1950**

	First-stage					Reduced forms					Two-stage estimates				
	Registration		NO		(1)	Registration		NO		(6)	Registration		NO		(10)
	(1)	(2)	(3)	(4)		(5)	(7)	(8)	(9)						
Victims per 10,000 inhab.	1.51*** (0.39)	1.93*** (0.42)	11.62** (5.22)	17.14*** (4.27)	4.56*** (0.87)	5.58*** (0.86)	7.67** (3.24)	9.46*** (2.53)	3.01*** (0.73)	2.82*** (0.67)					
Indicator military presence		-0.64*** (0.20)		-8.42*** (2.58)											
Log distance to military base															
<b>Controls</b>															
Vote share Alessandri	0.02 (0.04)	0.01 (0.04)	0.98** (0.47)	0.84** (0.42)	-0.25** (0.11)	-0.27*** (0.10)	0.81** (0.41)	0.73 (0.46)	-0.31** (0.13)	-0.30** (0.12)					
Vote share Allende	0.02 (0.03)	0.01 (0.02)	0.36 (0.40)	0.22 (0.37)	0.42*** (0.07)	0.39*** (0.07)	0.19 (0.36)	0.12 (0.39)	0.35*** (0.09)	0.36*** (0.09)					
Log distance to Santiago	-0.82** (0.37)	-0.38 (0.33)	6.14 (4.04)	11.87*** (4.33)	-0.22 (1.12)	0.84 (1.13)	12.43*** (3.32)	12.87*** (3.50)	2.25* (1.36)	2.21* (1.33)					
Log distance to regional capital	-0.25* (0.13)	-0.29** (0.13)	-6.36*** (1.33)	-6.89*** (1.26)	-0.92*** (0.28)	-1.02*** (0.27)	-4.44*** (1.38)	-3.80** (1.52)	-0.17 (0.45)	-0.24 (0.41)					
Population in 1970	-0.49 (0.34)	-0.65** (0.33)	-22.89*** (4.39)	-24.96*** (3.82)	0.76 (0.87)	0.38 (0.80)	-19.12*** (2.90)	-18.85*** (3.06)	2.24** (0.95)	2.21** (0.91)					
Observations	276	276	276	276	276	276	276	276	276	276					
R-squared	0.473	0.502	0.569	0.601	0.769	0.776									
Province fixed effects	x	x	x	x	x	x	x	x	x	x					
F-test of excluded instruments							14.77	10.56	14.77	10.56					

Notes: This table presents OLS estimates, reduced forms, and two-stage estimates using our main specification (equation 1). For these estimations we only use military bases constructed before 1950. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A12: Robustness of results to use of military bases built pre-1900**

	First-stage					Reduced forms					Two-stage estimates				
	Registration		NO		(1)	Registration		NO		(6)	Registration		NO		(10)
	(1)	(2)	(3)	(4)		(5)	(7)	(8)	(9)						
Victims per 10,000 inhab.	1.24** (0.56)	1.94*** (0.62)	11.27* (6.43)	21.17*** (4.77)	3.81*** (1.13)	5.51*** (1.15)	9.09* (4.94)	11.64*** (3.84)	3.07** (1.24)	2.75*** (0.99)					
Indicator military presence		-0.70*** (0.24)		-9.92*** (2.44)		-1.70*** (0.50)									
Log distance to military base															
<b>Controls</b>															
Vote share Alessandri	0.04 (0.04)	0.04 (0.04)	1.12** (0.49)	1.04** (0.43)	-0.19* (0.11)	-0.20* (0.11)	0.75 (0.46)	0.63 (0.53)	-0.31** (0.14)	-0.30** (0.12)					
Vote share Allende	0.04 (0.03)	0.03 (0.03)	0.49 (0.42)	0.40 (0.38)	0.47*** (0.07)	0.46*** (0.07)	0.13 (0.40)	0.02 (0.44)	0.35*** (0.10)	0.36*** (0.09)					
Log distance to Santiago	-0.54 (0.38)	-0.06 (0.36)	7.85** (3.75)	14.71*** (3.73)	0.60 (1.16)	1.78 (1.23)	12.78*** (3.53)	13.41*** (3.89)	2.27 (1.42)	2.19 (1.35)					
Log distance to regional capital	-0.30** (0.14)	-0.34** (0.14)	-6.63*** (1.34)	-7.22*** (1.25)	-1.06*** (0.32)	-1.16*** (0.31)	-3.93** (1.74)	-3.02 (1.87)	-0.15 (0.55)	-0.26 (0.45)					
Population in 1970	-0.42 (0.36)	-0.64* (0.36)	-22.72*** (4.55)	-25.84*** (3.96)	0.96 (0.88)	0.43 (0.86)	-18.91*** (2.94)	-18.53*** (3.32)	2.25** (0.95)	2.20** (0.88)					
Observations	276	276	276	276	276	276	276	276	276	276					
R-squared	0.444	0.474	0.563	0.602	0.757	0.765									
Province fixed effects	x	x	x	x	x	x	x	x	x	x					
F-test of excluded instruments							4.935	5.450	4.935	5.450					

Notes: This table presents OLS estimates, reduced forms, and two-stage estimates using our main specification (equation 1). For these estimations we only use military bases constructed during the 19th century. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A13: Robustness of results to different measures of exposure to military**

	Victims per 10,000 inhab.	Registration	Vote share NO	Victims per 10,000 inhab.	Registration	Vote share NO
	(1)	(2)	(3)	(4)	(5)	(6)
Victims per 10,000 inhab.		7.82*** (2.14)	2.07*** (0.54)		8.66*** (2.53)	3.03*** (0.79)
Indicator military presence	2.05*** (0.44)					
Exposure to military bases	1.68** (0.71)					
Log distance to military bases				-0.55*** (0.14)		
<b>Controls</b>						
Vote share Alessandri in 1970	0.00 (0.04)	0.81* (0.43)	-0.27** (0.11)	0.02 (0.04)	0.77* (0.44)	-0.31** (0.13)
Vote share Allende in 1970	0.01 (0.02)	0.18 (0.36)	0.39*** (0.08)	0.02 (0.03)	0.15 (0.38)	0.35*** (0.10)
Log distance to Santiago	-0.62* (0.36)	12.46*** (3.29)	2.02* (1.17)	-0.68** (0.33)	12.67*** (3.39)	2.26 (1.38)
Log distance to regional capital	-0.14 (0.15)	-4.39*** (1.41)	-0.51 (0.37)	-0.13 (0.15)	-4.09*** (1.43)	-0.16 (0.46)
Population in 1970	-0.54 (0.33)	-19.10*** (3.00)	2.10*** (0.81)	-0.55* (0.33)	-18.97*** (3.01)	2.24** (0.95)
Observations	276	276	276	276	276	276
R-squared	0.520			0.500		
Province fixed effects	x	x	x	x	x	x
F-test of excluded instruments	10.65	10.65	10.65	15.91	15.91	15.91

Notes: This table checks the robustness of results when we use different functional forms for the geographic exposure to military bases. In this specification we use the inverse-distance weighted average to bases in other counties. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



**Table A14: Robustness of results to inclusion of Air Force bases***Dependent variables: electoral outcomes at the 1988 plebiscite*

	Registration		Vote share NO	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	7.27*** (2.44)	8.25*** (2.02)	2.52*** (0.65)	2.34*** (0.62)
<b>Controls</b>				
Vote share Jorge Alessandri in 1970	0.83** (0.41)	0.79* (0.44)	-0.29** (0.12)	-0.28** (0.11)
Vote share Salvador Allende in 1970	0.21 (0.35)	0.17 (0.37)	0.37*** (0.08)	0.38*** (0.08)
Log distance to Santiago	12.33*** (3.24)	12.57*** (3.32)	2.13* (1.28)	2.09 (1.27)
Log distance to regional capital	-4.58*** (1.32)	-4.24*** (1.43)	-0.35 (0.40)	-0.41 (0.37)
Population in 1970	-19.18*** (2.95)	-19.03*** (3.01)	2.17** (0.86)	2.14*** (0.82)
Observations	276	276	276	276
Province fixed effects	x	x	x	x
<i>F</i> -stat excl. instruments	19.54	16.89	19.54	16.89

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A15:** Robustness of results to victim assignment by county of residence or work

	OLS		2SLS			
	Registration	NO	Registration	Registration	“NO” vote share	“NO” vote share
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: residence county</b>						
Victims per 10,000 inhab.	1.96* (1.08)	0.93*** (0.23)	10.39** (4.40)	12.18*** (3.73)	3.63*** (1.03)	3.46*** (0.97)
R-squared	0.559	0.764				
F- stat excl. instruments			12.69	7.551	12.69	7.551
<b>Panel B: work county</b>						
Victims per 10,000 inhab.	2.70** (1.12)	0.85*** (0.25)	13.63** (5.75)	15.91*** (5.04)	4.76*** (1.58)	4.49*** (1.42)
R-squared	0.562	0.755				
F- stat excl. instruments			10.58	6.375	10.58	6.375
Observations	276	276	276	276	276	276
Province fixed effects	x	x	x	x	x	x
Baseline controls	x	x	x	x	x	x

Notes: This table presents OLS and 2SLS estimates using our main specification (equation 1). In panel A, we assign victims to counties depending on their county of residence, while in panel B we replicate the analysis using the county where they worked. Place of residence is missing for 17.1% of victims, while place of work is missing for 41.7%. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A16: Robustness of results to exclusion of victims after creation of DINA**

	OLS				First-stage				Reduced forms				Two-stage estimates			
	Registration		NO		Registration		NO		Registration		NO		Registration		NO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)				
Victims per 10,000 inhab.	2.29** (1.05)	0.86*** (0.22)														
Indicator military presence			1.27*** (0.40)	1.60*** (0.44)	13.38*** (4.73)	19.11*** (3.74)	4.68*** (0.87)	5.56*** (0.91)	10.54** (4.21)	12.22*** (3.76)	3.68*** (1.26)	3.43*** (1.09)				
Log distance to military base				-0.46** (0.18)		-7.90*** (2.87)		-1.22** (0.48)								
<b>Controls</b>																
Vote share Alessandri	1.21** (0.47)	-0.15 (0.11)	-0.04 (0.02)	-0.04* (0.02)	0.95** (0.48)	0.89** (0.42)	-0.25** (0.11)	-0.26** (0.10)	1.34*** (0.45)	1.36*** (0.45)	-0.11 (0.11)	-0.12 (0.11)				
Vote share Allende	0.50 (0.40)	0.47*** (0.07)	-0.01 (0.02)	-0.02 (0.02)	0.32 (0.40)	0.22 (0.37)	0.41*** (0.07)	0.40*** (0.07)	0.43 (0.37)	0.42 (0.38)	0.45*** (0.09)	0.45*** (0.08)				
Log distance to Santiago	9.66*** (3.40)	1.18 (1.14)	-0.14 (0.20)	0.12 (0.20)	5.09 (3.99)	9.42** (4.12)	-0.39 (1.11)	0.28 (1.15)	6.52* (3.59)	5.88 (3.61)	0.11 (1.16)	0.21 (1.12)				
Log distance to regional capital	-6.81*** (1.43)	-1.10*** (0.29)	-0.02 (0.11)	-0.00 (0.12)	-5.70*** (1.34)	-5.33*** (1.40)	-0.73** (0.28)	-0.67** (0.28)	-5.44*** (1.45)	-5.16*** (1.61)	-0.64 (0.42)	-0.68* (0.39)				
Population in 1970	-20.24*** (3.83)	1.80** (0.79)	-0.24 (0.21)	-0.30 (0.19)	-22.71*** (4.21)	-23.63*** (3.77)	0.94 (0.86)	0.80 (0.82)	-20.16*** (3.59)	-20.15*** (3.68)	1.83** (0.85)	1.83** (0.83)				
Observations	276	276	276	276	276	276	276	276	276	276	276	276				
R-squared	0.560	0.757	0.420	0.445	0.578	0.606	0.771	0.776								
Province fixed effects	x	x	x	x	x	x	x	x	x	x	x	x				
F-test of excluded instruments									9.920	6.864	9.920	6.864				

Notes: This table presents OLS estimates, reduced forms, and two-stage estimates using our main specification (equation 1). However, the civilian victimization rate is calculated using only victims from before the creation of the dictatorship's secret police, DINA, in May, 1974. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A17: Robustness of results to use of discrete measure of repression**

	First stage	Two-stage estimates	
	High number of victims	Registration	Vote NO
	(1)	(2)	(3)
Indicator military presence	0.35*** (0.09)		
Indicator high victims per 10,000 inhab.		37.87** (15.84)	13.23*** (3.90)
<b>Controls</b>			
Vote share Alessandri in 1970	-0.00 (0.01)	0.98* (0.52)	-0.23 (0.16)
Vote share Allende in 1970	0.00 (0.01)	0.28 (0.42)	0.40*** (0.10)
Log distance to regional capital	-0.12 (0.08)	9.77** (4.39)	1.24 (1.28)
Log distance to capital	-0.05 (0.04)	-3.81** (1.69)	-0.07 (0.49)
Population in 1970	-0.02 (0.09)	-21.84*** (4.27)	1.24 (1.21)
Observations	276	276	276
Province FE	Yes	Yes	Yes
<i>F</i> -test of excluded instruments	14.08	14.08	14.08

Notes: This table presents an alternative specification of our main estimation equation. We use an indicator for the endogenous variable (“High number of victims”) and the indicator for the presence of a military base as the instrument. We do this to facilitate the interpretation of the characteristics of compliers. “High number of victims” is an indicator that takes the value one if the share of victims is in the top quartile of the empirical distribution. The average number of victims per 10,000 inhabitants in the top quartile is 4.3. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A18:** Robustness of results to use of other measures of voter registration*Dependent variables: electoral outcomes at the 1988 plebiscite*

	No winsorization		Winsorizing at 1	
	(1)	(2)	(3)	(4)
Victims per 10,000 inhab.	8.15*** (2.90)	9.54*** (2.64)	6.79*** (2.21)	7.92*** (1.88)
<b>Controls</b>				
Vote share Jorge Alessandri in 1970	0.89* (0.47)	0.83 (0.51)	0.67* (0.40)	0.62 (0.43)
Vote share Salvador Allende in 1970	0.23 (0.39)	0.17 (0.42)	0.09 (0.33)	0.04 (0.35)
Log distance to Santiago	9.82** (4.99)	10.16** (5.10)	13.98*** (3.03)	14.26*** (3.17)
Log distance to regional capital	-5.59*** (1.97)	-5.09** (2.07)	-3.46*** (1.18)	-3.06** (1.32)
Population in 1970	-24.11*** (6.69)	-23.90*** (6.71)	-15.15*** (2.50)	-14.98*** (2.62)
Observations	276	276	276	276
Province FE	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes
Province fixed effects	x	x	x	x
<i>F</i> -stat excl. instruments	16.53	11.06	16.53	11.06

Notes: This table presents two-stage least squares estimates of equation (1) using our two versions of the first stage, i.e. equations (2) and (3). The instruments are an indicator for military presence in 1970 in columns 1 and 3 and also the geographic distance to the closest military base in columns 2 and 4. The bottom of the table also presents the strength of the first-stage, measured by the *F*-stat of excluded instruments. All regressions are weighted by county population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A19:** Robustness of results to normalization of NO vote share by 1970 population

	OLS		Reduced forms		Two-stage estimates	
	(1)	(2)	(3)	(4)	(5)	(5)
Victims per 10,000 inhab.	2.65*** (0.87)			6.85*** (2.07)	7.23*** (1.89)	
Indicator military presence		12.06*** (3.19)	15.60*** (2.80)			
Log distance to military base			-4.88** (1.91)			
<b>Controls</b>						
Vote share Alessandri	0.40 (0.29)	0.32 (0.31)	0.28 (0.28)	0.20 (0.34)	0.19 (0.35)	
Vote share Allende	0.45* (0.24)	0.39 (0.25)	0.32 (0.23)	0.27 (0.25)	0.26 (0.26)	
Log distance to Santiago	6.38* (3.44)	0.82 (4.12)	3.49 (4.05)	7.42** (3.65)	7.51** (3.69)	
Log distance to regional capital	-4.87*** (1.27)	-4.47*** (1.38)	-4.24*** (1.38)	-3.36** (1.38)	-3.23** (1.39)	
Population in 1970	-12.73*** (4.86)	-15.33*** (5.38)	-15.90*** (5.30)	-12.10*** (4.54)	-12.05*** (4.55)	
Observations	276	276	276	276	276	
R-squared	0.413	0.411	0.431			
Province fixed effects	x	x	x	x	x	
F-test of excluded instruments				16.53	11.06	

Notes: This table presents OLS estimates, reduced forms, and two-stage estimates using our main specification (equation 1). The dependent variable is the number of votes for the “No” option divided by county population in 1970. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A20: Placebo test using types of facilities***The dependent variable is victims per 10,000 inhab.*

	Ports		Airports		Entry points	
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator military presence	1.28*** (0.40)	1.61*** (0.43)	1.05** (0.45)	1.26** (0.49)	1.28*** (0.40)	1.51*** (0.46)
Log distance to military base		-0.50** (0.20)		-0.43** (0.17)		-0.42** (0.18)
Indicator other facility	0.06 (0.31)	-0.19 (0.30)	0.64 (0.47)	0.70 (0.43)	-0.30 (0.37)	-0.16 (0.38)
Log distance to other facility		0.11 (0.18)		-0.27 (0.19)		-0.29 (0.21)
R-squared	0.420	0.447	0.429	0.465	0.422	0.453
Observations	276	276	276	276	276	276
Province FE	x	x	x	x	x	x
Controls	x	x	x	x	x	x

Notes: This table replicates our first-stage analysis, examining whether proximity to facilities other than military bases helps explain the civilian victimization rate. Presence of ports (columns 1-2) and airports (columns 3-4) is measured before 1970. Columns 5-6 consider an indicator for counties with international points of entry to Chile, according to the 2010 Hiking Guide (“Guía Caminera”) published by the Military Institute of Geography. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A21: Placebo test using elections before the coup***The dependent variable is a vote share for a candidate (or party) in an election.*

	Presidential Elections				Local Election	Legislative Election
	1964		1970		1971	1973
	Frei	Allende	Alessandri	Allende	Popular Unity	
<b>Panel A: Reduced form</b>	(1)	(2)	(3)	(4)	(5)	(6)
Indicator military presence	0.53 (1.62)	-0.93 (1.58)	0.47 (1.62)	1.54 (1.81)	1.49 (2.42)	0.03 (1.90)
Log distance to Santiago	0.37 (2.58)	-0.25 (2.57)	-0.97 (2.76)	-1.13 (2.72)	-0.30 (3.77)	-0.55 (3.11)
Log distance to reg. capital	-1.63*** (0.59)	1.77*** (0.59)	-1.16** (0.57)	1.70*** (0.63)	1.54* (0.84)	1.54** (0.70)
Population in 1970	-1.01 (1.96)	1.58 (1.98)	-2.18 (2.20)	2.32 (2.03)	2.36 (2.93)	1.21 (2.45)
<b>Panel B: 2SLS</b>						
Victims per 10,000 inhab.	0.30 (0.88)	-0.52 (0.87)	0.26 (0.86)	0.86 (0.95)	0.83 (1.26)	0.02 (1.00)
Log distance to Santiago	0.67 (2.31)	-0.77 (2.31)	-0.71 (2.35)	-0.27 (2.57)	0.52 (3.49)	-0.54 (2.79)
Log distance to reg. capital	-1.58*** (0.56)	1.68*** (0.56)	-1.12** (0.52)	1.83*** (0.61)	1.67 (0.79)	1.55** (0.66)
Population in 1970	-0.86 (1.68)	1.32 (1.68)	-2.06 (1.87)	2.72 (1.95)	2.74 (2.67)	1.21 (2.17)
Counties	267	267	276	276		276
R-squared (A)	0.459	0.373	0.374	0.402	0.337	0.302
R-squared (B)	0.459	0.372	0.374	0.401	0.336	0.303
First-stage F-stat (B)	16.8	16.8	18.1	18.1	18.1	18.1
Province fixed effects	x	x	x	x	x	x

Notes: Panel A presents reduced-form estimates of the relationship between military bases and electoral outcomes before the dictatorship. Panel B presents the corresponding 2SLS estimates of the same outcomes on victims per 10,000 inhabitants. First-stage F-tstatistic We use the same controls as in our baseline specification except that we exclude vote shares in previous elections (results are similar if we include them). We lose nine observations in columns 1 and 2 because of the appearance of new counties. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



**Table A22: Military bases and state presence***Amount of money per capita spent in local projects*

	Total		Visible projects		Other projects	
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator military base	0.06 (0.06)	0.06 (0.08)	0.04 (0.06)	0.04 (0.07)	0.01 (0.02)	0.02 (0.02)
Log distance to military base		-0.00 (0.05)		0.01 (0.05)		-0.01 (0.01)
<b>Controls</b>						
Vote share Alessandri in 1970	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)	-0.00 (0.00)	-0.00 (0.00)
Vote share Allende in 1970	0.01* (0.01)	0.01* (0.01)	0.01** (0.01)	0.01* (0.01)	-0.00 (0.00)	-0.00 (0.00)
Log distance to Santiago	0.02 (0.10)	0.02 (0.12)	0.02 (0.09)	0.01 (0.11)	0.00 (0.02)	0.01 (0.02)
Log distance to regional capital	-0.01 (0.03)	-0.01 (0.03)	-0.03 (0.03)	-0.03 (0.03)	0.01** (0.01)	0.01** (0.01)
Population in 1970	0.02 (0.10)	0.02 (0.10)	-0.01 (0.09)	-0.01 (0.09)	0.03** (0.01)	0.03** (0.01)
Counties	276	276	276	276	276	276
R-squared	0.46	0.48	0.42	0.45	0.60	0.60
Avg. dependent variable	0.44	0.44	0.39	0.39	0.05	0.05
Province fixed effects	x	x	x	x	x	x

Notes: This table presents estimates of a regression using state spending in urban projects in the period 1979-1989 as dependent variable and our instruments as right-hand side variables. We interpret this regression as a test of the relationship between exposure to military bases and state presence during the dictatorship. The bottom of the table presents the average of the dependent variable to help interpret the magnitude of coefficients. All regressions are weighted by county-level population in 1970. Robust standard errors in parenthesis. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A23: Characterization of candidates in presidential elections 1993 – 2017**

Coalition	1993	1999	2005	2009	2013	2017
RIGHT	A. Alessandri J. Piñera	J. Lavín	S. Piñera J. Lavín	S. Piñera	E. Matthei	S. Piñera J.A. Kast
LEFT	E. Frei M. Max Neef E. Pizarro C. Reitze	R. Lagos G. Marín T. Hirsch	M. Bachelet T. Hirsch	E. Frei J. Arrate M. Enríquez	M. Bachelet M. Enríquez M. Claude A. Sfeir R. Miranda	M. Enríquez A. Guillier C. Goic B. Sánchez A. Navarro E. Artés
CONCERTACIÓN	E. Frei	R. Lagos	M. Bachelet	E. Frei	M. Bachelet	A. Guillier C. Goic
FAR-LEFT	E. Pizarro	G. Marín	T. Hirsch	J. Arrate	R. Miranda	E. Artés

Notes: Own construction based on official data from the Electoral Service.

**Table A24: Characterization of coalitions in local elections 1992 – 2016**

	1992	1996	2000	2004	2008	2012	2016
RIGHT	Participación y Prog. U. Centro Centro	Unión por Chile Prog. Centro Centro	Alianza Centro Centro	Alianza Centro Centro	Alianza	Alianza	Chile Vamos Amplitud
LEFT	Concertación P. Comunista	Concertación La Izquierda Humanista	Concertación La Izquierda Humanistas y Ecologistas	Concertación Juntos Podemos Hum. y Eco.	Concertación Dem. Juntos Podemos Hum. y Eco. Concertación Prog. Chile Limpio Fza. Norte	Concertación Dem. Chile en Otra El Cambio Por Ti Más Humanos Desarrollo Norte Chile Justo	Concertación Dem. Alt. Democrática Cambios la Historia Nueva Mayoría P. Reg. Magallanes Poder Eco. y Ciud. Marco Cambio Norte Verde
CONCERTACIÓN	Concertación	Concertación	Concertación	Concertación	Concert. Dem. Concert. Prog.	Concertación Dem.	Concertación Dem.
FAR-LEFT	P. Comunista	La Izquierda	La Izquierda	Juntos Podemos	Juntos Podemos	Chile Justo	Chile Justo
INDEPENDENT	Independientes	Independientes	Independientes	Nueva Alt. Indep. Independientes	Independientes	Independientes Region. e Indep.	Independientes Reg. e Indep.

Notes: Own construction based on official data from the Electoral Service.

**Table A25: Characterization of compliers**

	Treated Compliers	Untreated Compliers	Full sample
	(1)	(2)	(3)
<b>A. Pre-1973 characteristics:</b>			
Houses per capita in 1970	0.20	0.21	0.20
Land inequality 1965 (Gini)	0.92	0.87	0.85
Agrarian reform intensity	0.09	0.20	0.20
Vote share Allende 1970	0.37	0.34	0.27
Vote share Alessandri 1970	0.00	0.11	0.20
<b>B. Post-1973 characteristics:</b>			
Plebiscite:			
Registration	109.52	71.65	71.16
Vote share “No”	61.74	48.51	54.82
Repression year:			
In 1973	0.55	0.04	0.44
In 1974	0.16	0.15	0.11
≥1975	0.31	0.42	0.33
Profession:			
Laborer	0.39	0.13	0.25
Farmer	0.10	0.02	0.09
Military	0.09	0.00	0.07
Bureaucrat	0.11	0.00	0.07
Student	0.04	0.09	0.10
Affiliated to political party	0.44	0.23	0.39
Age categories:			
∈ [18, 25]	0.33	0.27	0.33
∈ [25, 60]	0.65	0.20	0.50
≥ 60	0.00	0.08	0.02

Notes: This table presents an empirical characterization of the complier counties. Panel A shows that compliers were relatively similar to the average county in the full sample. Panel B describes counties that experienced repression because of the presence of military bases. See Abadie et al. (2002) for details. The treatment in this exercise is an indicator that takes the value one if the share of victims is in the top quartile of the empirical distribution.