VOTING FOR DEMOCRACY: CHILE'S PLEBISCITO AND THE ELECTORAL PARTICIPATION OF A GENERATION

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This paper assesses the long-term consequences of voting for democracy. We study Chile's 1988 plebiscite, which ended 15 years of dictatorship and reestablished democracy. Taking advantage of individual-level voting data, we implement an age-based RD design comparing long-run registration and turnout rates across marginally eligible and ineligible individuals. We find plebiscite eligibility increased electoral turnout three decades later. The magnitude of the initial mobilization emerges as the mechanism. Plebiscite eligibility induced a sizable share of less educated voters to register compared to other upstream elections. The event contributed to the emergence of one party rule the twenty years following democratization.
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Important political events often make indelible impressions on the minds and future actions of voters. Mere participation in an election has been shown to impact future partisanship (Madestam et al., 2013; Kaplan and Mukand, 2014), the degree of polarization (Mullainathan and Washington, 2009) and voter turnout (Meredith, 2009; Coppock and Green, 2016; Fujiwara, Meng and Vogl, 2016). In fact, early-life political events which are particularly salient may have even larger long-term effects (Sears and Valentino, 1997; Sears and Funk, 1999; Alesina and Fuchs-Schündeln, 2007; Prior, 2010; Laudenbach, Malmendier and Niessen-Ruenzi, 2019). In this paper, we examine the long-run impacts of participating in one of the most consequential elections in recent history: Chile's 1988 plebiscite, which was held to determine whether the Latin American nation would return to democracy after a 15-year long military dictatorship. Augusto Pinochet came to power under a military coup in 1973 and maintained autocratic control through civil rights restrictions and military rule. In 1980, under international pressure for

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human rights abuses, the military government wrote a new constitution, which called for a plebiscite to be held eight years later on the restoration of democratic rule. The plebiscite was held on October 5^{th}, 1988, and Pinochet unexpectedly lost.[1] The success of the 'No' vote (i.e, the end of the military regime) then ushered in elections for a new president in 1989 and the restoration of a democratically elected regime in 1990.

This paper quantifies the impact of voting on democracy itself upon future voter registration and electoral turnout. We estimate a regression discontinuity design using age-based plebiscite eligibility. Only citizens who had turned 18 by the closing of the registration rolls on August 30, 1988 were allowed to participate in the election.[2] Taking advantage of individual-level voter data for upwards of 13 million Chileans, coupled with information on individuals' weeks of birth and registration outcomes, we first show that upwards of 60% of marginally age-eligible Chileans registered for the 1988 plebiscite. Moreover, we find that these registration gaps persisted. Twenty years later, in 2009, marginally eligible plebiscite participants were still registered at a 17 percentage point higher rate than those born merely one week later. In Chile's old electoral system, citizens who registered to vote remained on the rolls permanently; as a result, the 2009 effects reflect incomplete catch-up by plebiscite ineligibles. While actual turnout data for pre-2010 elections is unavailable, we note that voting was mandatory for registered individuals through the 2009 election, and turnout rates exceeded 86% through 2009.

Chile switched from a voluntary to an automatic registration system after the 2009 election, which implied that any pre-reform differences in registration rates across the plebiscite cutoff automatically disappeared. Taking advantage of voter-level data on actual turnout for the 2013 and 2017 Presidential elections as well as for the 2016 municipal election, we thus estimate downstream turnout impacts of plebiscite eligibility which are not mediated by registration differences. We find that marginal eligibility to vote in the 1988 plebiscite on the restoration of democracy raised turnout by 5.2 and 2.9 percentage points for the 2013 and 2017 Presidential elections, or 10.4% and 6.0% of baseline participation rates, respectively. Importantly, as we observe the entirety of the Chilean population rather than just those who are registered to vote, our estimates overcome the differential registration bias which emerges in the existing literature (Nyhan, Skovron and Titiunik, 2017).

We further analyze the downstream effects of actual plebiscite voting by estimating a fuzzy regression discontinuity design and find that having voted in the 1988 plebiscite increased 2013 and 2017 turnout rates by 8.1 and 4.5 percentage

[1]The Constitution called for the plebiscite to be a Yes/No vote on whether a candidate chosen by the military regime would stay in power for eight years, or whether Chile would return to democratic rule, by holding its first presidential election in 1989. Most polls conducted in 1988 showed the 'Yes' option to have a commanding lead (Boas, 2015).

[2]Since eligible individuals could only register once they had turned 18, we implement the RD design using a one-sided eight-week donut hole, focusing on individuals with more than eight weeks to register.

points, respectively. We also find similar effects for the lower-stakes 2016 municipal election and show that the results are robust to a number of bandwidth choices. These results thus indicate that having voted in Chile's most consequential election had substantial downstream effects even three decades after the return to democracy.

Since the existing studies on downstream voting effects have largely focused on the United States (Meredith, 2009; Coppock and Green, 2016), our estimates are not directly comparable to the literature. As a result, we benchmark the estimated plebiscite turnout effects using age-based discontinuities around other upstream elections. We focus on Chile's first five presidential elections following the restoration of electoral democracy, the first of which took place in December 1989, followed by elections in 1993, 1999, 2005 and 2009. We similarly estimate regression discontinuity designs on turnout in the two presidential elections and one municipal election and only find significant turnout effects in three out of fifteen coefficients. Moreover, the magnitudes of the effects of the other upstream elections are at most one-fourth of the magnitude of the plebiscite effect for a given downstream election. We also find that the substantially larger downstream effects of the plebiscite are due to the size of the initial mobilization rather than greater persistence in voting from the initial mobilization. Last, eligibility for three pre-plebiscite elections—encompassing the 1970 Presidential election, parliamentary elections in 1973 and a Constitutional reform plebiscite in 1980—each do not have a significant effect on downstream electoral turnout, further underscoring the unique nature of the 1988 plebiscite.

We also examine heterogeneous impacts across a number of dimensions, a first in this literature. In the lead-up to the plebiscite, the 'No' campaign against continued rule by the Pinochet government focused its advertisements towards women (Hirmas, 1993); however, we fail to find larger effects for women. We do find suggestive evidence of larger downstream effects for individuals living in historically left-leaning municipalities, with statistically significant impacts in the 2013 election.

In addition, merging in two other administrative data sources which contain detailed information on individual educational attainment, we analyze whether the set of compliers varied across upstream elections.[3] We find that plebiscite eligibility induced a higher fraction of high school dropouts to initially register to vote in comparison with other upstream elections. Moreover, in specifications with larger bandwidths, which include plebiscite eligibles who had more time to register, the share of high school dropouts who registered to vote increases significantly. These results indicate that the salience of an election as well as the time allowed for registration both affect electoral participation and heterogeneously so by socioeconomic status. Since Chile's old electoral system required permanent

[3]We analyze information linking educational attainment data to registration outcomes under the old electoral system, allowing us to explore heterogeneous registration outcomes by education level. Nonetheless, since our turnout data is de-identified, we cannot examine turnout effects by education.

registration (with high turnout rates), we note that the 1988 plebiscite induced a larger share of less educated Chileans to vote. Using survey data, we document that this group tends to support left-leaning parties in Chile. As a result, we lastly posit that the structure of the plebiscite likely contributed to the 20-year period of one-party rule — a common feature in newly democratized countries — by the left-leaning (*Concertación*) coalition in Chile.

Prior work has also examined the impacts of upstream election eligibility on downstream turnout in the United States using the age-18 eligibility cut-off. Using data from California, Meredith (2009) documents that presidential election eligibility increases subsequent participation up to four years later. Fujiwara, Meng and Vogl (2016) show persistence in voting using initial variation in rain on election day. Coppock and Green (2016) show persistent effects of early-life electoral participation on future voter turnout over a period of two decades. We note, however, that Nyhan, Skovron and Titiunik (2017) have shown that registration itself is endogenous, leading to sample selection bias in the estimation of voting persistence. By contrast, since we observe turnout outcomes for the entire Chilean population, our empirical strategy for estimating the impact of initial voter participation upon future voting is robust to this criticism.

As the first paper to document substantial heterogeneity in concurrent registration rates by educational attainment, we also contribute to a literature providing quasi-experimental evidence on the factors which drive voter turnout, see (Gerber and Green, 2000; Gerber, Green and Shachar, 2003; Gerber, Green and Larimer, 2008; Arceneaux and Nickerson, 2009; Nickerson, 2015; Braconnier, Dormagen and Pons, 2017; Green and Gerber, 2019), among others. Moreover, given the prevalence of one-party rule in various countries after the reinstatement of democracy, we present suggestive evidence that the nature of the Chilean plebiscite may have contributed to the twenty years of *Concertación* rule, by inducing less educated citizens to vote over a period of decades. Relatedly, our results fit in with an extensive literature analyzing how salient events shape preferences and outcomes (Alesina and Fuchs-Schündeln, 2007; Giuliano and Spilimbergo, 2014).

We also contribute to a growing literature analyzing Chile's 1988 plebiscite. Other papers have used cross-sectional variation to estimate the impact of exposure to military repression (proxied by distance to a military base) (Bautista et al., 2021*a*) and the penetration of the 'No' campaign (González and Prem, 2018), defined by TV-ownership rates across municipalities, on support for the 'No' position in the plebiscite. To the best of our knowledge, ours is the first paper to consider the long-term electoral consequences of the plebiscite. Furthermore, we present the first estimates of downstream electoral persistence in a non-US context using reliable administrative data.

Another advantage of our approach is that ours is the first paper to consider the long-term effects of an election held under dictatorial rule. Other work has analyzed downstream effects in developing countries using survey data, including De Kadt (2017) in South Africa and Holbein and Rangel (2020) in Brazil, but always under democratic rules. Corvalan and Cox (2018) examine the impact of Chile's post-plebiscite upstream election eligibility but only on downstream registration rates in pre-reform elections. Importantly, our empirical strategy and administrative data sources allow us to separately identify registration and turnout effects. In fact, Chile's electoral reform implies that we recover a turnout effect which is not explained by persistence in registration but rather reflects a pure effect of voting on future voting — a first in this literature.

The rest of the paper proceeds as follows: In Section I, we discuss institutional details. In Section II, we introduce our data sources and present summary statistics. In Section III, we present our empirical strategy. Section IV presents our main results of the long-run effects of plebiscite eligibility on persistent downstream registration and voting spanning up to three decades. Section V documents how our findings vary by gender, education-level and partisan orientation of municipality. We also discuss the implications of our results for partisan mobilization and relate them to single party dominance in newly democratized countries. Finally, Section VI concludes.

I. Institutional Details

Political Background. In 1970, Salvador Allende and the Socialist Party came to power in a narrowly won and highly contested electoral victory. Allende and his Popular Unity coalition of communists, socialists, social democrats and radicals faced off against the center-left Christian Democrats, led by Radomiro Tomic, and the right-wing National Party candidate Jorge Alessandri. Allende received the 36.6% of the votes as compared to Alessandri's 35.2% and Tomic's 28.1% and formed a government with the support of the Christian Democrats.

On September 11, 1973, Salvador Allende's government was overthrown in a military coup led by General Augusto Pinochet. Pinochet's regime suspended civil rights, raided the homes of suspected opposition supporters, and both kidnapped and murdered potential members of the opposition. The Rettig and Valech reports, conducted after the end of the dictatorship, estimated that the regime was responsible for the murder of 3,216 individuals and the torture of 38,254 Chileans.

Under international pressure over human rights abuses, Pinochet sought to legitimize his regime through a plebiscite proposing a constitutional reform (Varas, 1982). The plebiscite took place on September 11, 1980 and the Constitution was ratified with 67.5% of the vote. The new Constitution ushered in a new eight-year rule for Pinochet, which began on March 11, 1981 and was set to last through March 11, 1989. The Constitution called for the military regime to propose a new candidate for the next eight-year term at least 90 days prior to the end of Pinochet's rule. This candidate would be ratified in a plebiscite in which a "Yes" vote would imply an eight-year term for the proposed candidate, beginning on March 11, 1989 and lasting through March, 1997. A "No" vote would first extend Pinochet's rule for an additional year and then trigger a democratic Presidential election to be held 90 days prior to the end of Pinochet's extended term — in

December, 1989.

While the 1980 Constitution had made voting mandatory, the norms for electoral participation were not defined until the restitution of the Electoral Commission in 1986 (*SERVEL* in Spanish). The guidelines established by SERVEL in 1986 did not require Chileans to register to vote — thus leaving Chile with a unique system of voluntary registration with mandatory voting only for registered citizens.

1988 Plebiscite. The guidelines laid out in the 1980 Constitution implied the plebiscite would be held in 1988, yet a specific date was not announced in advance. Voter registration opened on February 25, 1987, and all Chilean citizens older than 18 years old became immediately eligible to register to vote.[4] By the end of 1987, over 3 million Chileans had registered, reaching 40% of the voting-age population. On August 30^{th} of 1988, the military regime announced that the candidate for the 'Yes' option would be Augusto Pinochet, and that the plebiscite would be held on October 5^{th}. SERVEL also closed voter registration on August 30, with 7.4 million Chileans having registered to vote, encompassing over 90% of the voting age population. Registration was even high among young Chileans; 70% of 18-24 year olds registered in time for the plebiscite.[5]

In the lead-up to the plebiscite, the Pinochet government gave both the 'Yes' and 'No' campaigns fifteen minute-long sequential advertisement slots on national television — called the *franja*— every night. The regime and the opposition, a coalition of political parties named *Concertación*, both presented videos supporting their respective positions and the videos were syndicated on all television stations across the country every day between September 5^{th} and October 1^{st} from 8:30 to 9PM. González and Prem (2018) find that a one standard deviation increase in television exposure to the *franja* increased 'No' support by two percentage points.

Most polls conducted in 1988 showed the 'Yes' option to be leading among registered voters (Boas, 2015). However, 97% of all registered individuals voted in the plebiscite and the 'No' option won with 54.7% of the vote. As a result, Pinochet's rule was extended for a year, through March 11^{th}, 1990 and Presidential elections were called for December, 1989.

During 1989, the military regime and the opposition agreed on a number of reforms to the Constitution. A Constitutional referendum was held on July 30^{th} and these reforms were ratified by 85.7% of the electorate. The *Concertación*

[4]SERVEL's electoral guidelines published in 1986 mentioned that citizens who turned 18 prior to an election, but after the registration closing date could still register to vote. Nonetheless, this rule did not apply for the 1988 plebiscite, as the plebiscite date had not been announced in advance. As a result, Chileans who turned 18 between February 25^{th} 1987 and registration closing date for the plebiscite could only register to vote upon turning 18.

[5]The age cut-off and with the sudden announcement of the registration closing date implies that Chileans who turned 18 on August 31^{st} were ineligible to vote in the plebiscite. As such, those who turned 18 on August 30 had only one day to register on that day whereas, for example, Chileans born on July 30, 1970 had a full month to register.

candidate, Patricio Aylwin, won the Presidential election with 55% of the vote, becoming Chile's first democratically-elected President in seventeen years and ushering in twenty years of *Concertación* presidents.[6]

Post-Plebiscite Elections and Electoral Reform. In the years following the restoration of democracy, eligible registrants increasingly registered to vote at lower rates. By the time of the 2009 Presidential elections, only 20% of 18-24 year olds had registered to vote and only two-thirds of the entire voting age population had done so (Contreras and Navia, 2013). The large decline in voter registration was partly due to an electoral system which combined voluntary registration with mandatory voting. In contrast to plummeting registration rates, electoral participation among registered voters remained quite high, reaching its nadir of 86.7% in 2009.

Table 1—Aggregate Voter Turnout for Presidential Elections

	Eligible	Registered	Votes Cast	Share Registered	Share Voting	Turnout Rate
1988	8.06	7.44	7.25	0.922	0.899	0.975
1989	8.24	7.56	7.16	0.917	0.868	0.947
1993	8.95	8.09	7.38	0.903	0.824	0.912
1999	9.95	8.08	7.27	0.813	0.731	0.900
2005	10.80	8.22	7.21	0.761	0.667	0.877
2009	12.23	8.29	7.19	0.678	0.588	0.867
2013	13.19	13.39	6.63	1.000	0.496	0.496
2017	14.08	14.08	6.65	1.000	0.472	0.472

Note: Table 1 presents summary statistics of voter registration and turnout for the 1988 plebiscite and for all Presidential elections since 1989. The numbers in the first three columns are expressed in millions. *Source:* Table 1 in Contreras and Navia (2013) (1988-2009); *Servicio Electoral de Chile (SERVEL), Estadísticas de Participación (2013-2017).*

Partly motivated by the aging of the electorate, Chile undertook a sizable change in its electoral system in 2009, moving away from a system with mandatory voting and voluntary registration to one with universal automatic registration and voluntary voting. The new registration system thus resembles that of countries such as Italy, Norway, and Spain as well as by Washington, D.C. and 16 U.S. states including California, Georgia, Maryland, Michigan and Oregon. All eligible adults were immediately registered, and all minors were automatically registered at age 18. As a result, the number of registered voters increased from 8.5 to 13.4 million. The new electoral system was first used in the 2012 municipal elections. Despite the sizable increase in the number of registered citizens, turnout actually fell from 7.0 to 5.8 million voters. The decline in voter turnout persisted through the 2013 and 2017 presidential elections, falling from 7.3 million

[6]Chile's post-dictatorship electoral system created a "top two" (two-stage) electoral system for president. In the first round, if a candidate captures an outright majority of the vote, she/he wins the presidency. Otherwise, the election proceeds to a second round with the two top candidates, as in the 1999, 2005, 2009, 2013 and 2017 Presidential elections.

voters in the 2009 election to 6.7 million in both the 2013 and 2017 presidential elections. Table 1 shows registration and turnout over time for all presidential elections, documenting the large registration rates for early elections, along with the sizable decline in turnout following the 2012 electoral reform.

Our analysis of the impact of plebiscite eligibility and plebiscite participation upon long-run voter turnout captures effects of two separate regimes. Up through the 2009 Presidential election, actual turnout is not directly measured but largely reflects registration since voting was mandatory for the registered. During this period of time, gaps between plebiscite eligibles and those who were ineligible to vote in the plebiscite narrowed over time due to catch-up of by initial ineligibles. After the 2012 electoral reform, the new government implemented automatic registration and voluntary voting. In the voting persistence literature focused on the U.S. context (Meredith, 2009; Coppock and Green, 2016), it is not clear whether estimated effects reflect the impact of voter registration upon future voting or of voting itself upon future voting. Fujiwara, Meng and Vogl (2016) improve upon the prior literature by showing that estimates of persistence do not change substantially when estimates are restricted to a subset of states which do not purge inactive voters from registration rolls. However, even in these states, voters who move across state lines get dropped from the registration rolls and also many voters accidentally get dropped. In our paper, the automatic registration system implemented after 2009 allows us to rule out the reduction of administrative barriers to voting from the act of registration as a mechanism generating persistence. Thus, our estimates reflect a pure effect of voting upon future voting.

II. Data Sources and Summary Statistics

A. Data Sources

Our main data source comes from de-identified individual-level voting data provided by SERVEL for the 2013 and 2017 first-round Presidential elections and for the 2016 municipal election. In addition to individual-level turnout data for these elections, this data set includes information on the birth year and week of Chileans, which we use to determine plebiscite eligibility. Moreover, we observe the year of registration for those who registered voluntarily under the old electoral system. We additionally observe gender and *comuna* of residence at the time of the election.

We take advantage of voters' *comuna* of residence to merge various *comuna*-level characteristics. First, we use data from Chile's last two censuses, conducted in 1992 and 2002, which provide information on *comuna*-level covariates including the share of households with electricity, water, and a toilet in their house respectively, the share of TV ownership along with the literacy and the *comuna* unemployment rate (Minnesota Population Center, 2020). Furthermore, we analyze heterogeneous downstream effects of the plebiscite by political affiliation by merging in *comuna*-level vote shares in the 1970 Presidential election for Allende

(Bautista et al., 2021*b*). Our analysis of heterogeneous impacts across comuna-level characteristics necessitates that flows of people in and out of *comunas* do not on average change aggregate *comuna* characteristics. While this is a strong assumption, we use Chile's household survey (CASEN 2015) and compute that fewer than one-third of Chilean adults have moved *comunas* since birth. For Chileans who have moved since the upstream election, our procedure imputes incorrect *comuna*-level characteristics, which would lead to attenuation bias if migration were random.

Given our interest in examining the heterogeneous impacts of plebiscite eligibility across individuals' educational attainment, we use a second dataset constructed from a variety of administrative data sources. This dataset uses administrative data from SERVEL, which contains exact date of birth, gender and exact registration date for individuals who had voluntarily registered in the old electoral system. The individual-level SERVEL data is then combined with two other administrative data sets which contain information on individuals' educational attainment. First, Chile's Unemployment Insurance (UI, *Seguro de Cesantía*) database contains matched employee-employer data for all formal sector employment contracts signed since November 2002. This data source covers all Chileans who spent at least one month employed in the formal sector since 2002. These records include upwards of seven million workers. UI data includes employment status but critically for our analysis, it also contains educational attainment. Since UI data does not capture individuals who have not held formal sector employment since 2002, we complement our analysis with administrative records from the Bureau of Social Protection (FPS, *Ficha de Protección Social* of 2009). The FPS data includes all individuals (along with their family members) who applied for any social program in Chile, covering two-thirds of the Chilean population. From the FPS data, we observe individuals' educational attainment, as well. These sources of information were merged, generating individual-level records containing educational attainment and date of registration.[7] To ensure that the sample is representative of the Chilean population, we compare it to the SERVEL turnout data for the 2013 election. The 2013 turnout data includes 13.39 million Chileans born before 1995, whereas our data set includes 11.37 million individuals — we observe educational attainment for 9.98 million of them. As a result, we recover educational attainment for 75% of the voting-age population in the 2013 presidential election.[8]

This data set allows us to examine long-term differences in registration rates and to examine compliers' educational attainment across different bandwidths

[7]The link across various administrative data sources was carried out at the secure server of Chile's Ministry of Finance using anonymized identifiers. Individuals are classified by whether they were high school dropouts, high school graduates or had at least some post-secondary education by 2009.

[8]The nature of these administrative data sources implies that we better recover educational attainment for individuals who were of working age in 2013. As a result, our match rate is in the 66% range for individuals born in the 1950s, rising to 73.1% and 77.5% for those born in the 1960s and 1970s, respectively. We formally test for differences in match rates across each upstream election cut-off and find no significant differences.

and upstream elections. Nonetheless, we do not observe educational attainment in the de-identified SERVEL turnout data. Thus, we cannot estimate heterogeneous impacts of plebiscite eligibility on downstream turnout.

Finally, we also use political opinion survey data conducted by the *Centro de Estudios Publicos* (CEP) for all the election years from 1989-2009 (Centro de Estudios Publicos, 2009). This data set contains demographic data, most notably, socioeconomic status, as well as self-reported turnout and partisanship. We use this data to examine the likely partisan impacts of the plebiscite and to test for differential turnout in pre-reform electoral system.

B. Summary Statistics

The combination of our data sources allow us to analyze voting behavior for over 13 million Chileans. Table A.1 presents summary statistics. 60% of our sample had voluntarily registered to vote by 2009, 49.5% and 47.2% actually voted in the 2013 and 2017 presidential elections, respectively. In columns 4 and 5, we compare individuals who were marginally eligible to participate to those who were marginally ineligible, restricting our attention to Chileans who turned 18 in a 12-month window across the plebiscite eligibility cut-off (6 months on either side). 86% of marginally eligible individuals had registered to vote by 2009, in contrast to just 69% of marginally ineligible Chileans. Moreover, we find analogous results in terms of voting in the 2013 presidential election, with the marginally older group having turned out at a 55% rate compared to a 50% turnout rate for their younger counterparts. Similar differences emerge for the 2016 municipal and 2017 presidential elections.

III. Empirical Strategy and Model Selection

To identify the impact of plebiscite eligibility on downstream electoral turnout, we take advantage of the sharp cut-off introduced by the age-18 eligibility requirement, which implied that Chileans born after August 30, 1970 were ineligible to vote in the 1988 plebiscite. We follow Meredith (2009), Coppock and Green (2016) and Fujiwara, Meng and Vogl (2016) among others and implement a regression discontinuity design. We regress downstream registration and turnout on initial eligibility, controlling for the relationship between registration or turnout in the future election on birth date. Our basic regression model can be specified as follows:

$$Y_i^j = \alpha^j + \delta^j Before_i + \mu^j(Cutoff_i) + Before_i \times \mu^j(Cutoff_i) + \varepsilon_i^j \tag{1}$$

where Y_i^j is a binary variable which represents either registration by person i in or before the registration deadline for the election in year j or voter turnout by individual i in downstream election j. $Before_i$ is a dummy variable which equals 1 if person i turned 18 prior to the eligibility cutoff for the 1988 plebiscite, $Cutoff_i$.

$\mu^j(Cutoff_i)$ is a flexible function of the distance (in weeks) of person i's age-18 birthday to the same cut-off. The interaction term allows for the relationship between plebiscite eligibility and long-term voting behavior to vary depending upon the distance to the cut-off.

The identifying assumption behind the regression discontinuity design presented is that the unobserved characteristics of individuals are continuous across the cut-off, that is, eligible and ineligible individuals should only differ in terms of their ability to have voted in the 1988 plebiscite. In practice, we implement equation (1) using a one-sided donut-hole approach. We include all marginal ineligibles so the control group matches the treatment group on observables and unobservables. However, we exclude marginal eligibles who turned 18 within eight weeks of the registration cut-off. We omit eight weeks as initial registration rates stabilize for individuals who had eight or more weeks to register (Figure 1). Since they could only register to vote upon turning 18, estimating the standard regression discontinuity design would capture the effect of plebiscite eligibility for those with limited time to register instead of the direct effect of plebiscite eligibility. Removing these individuals from the analysis thus allows us to better recover the effect of being eligible to vote for the average person. Our main estimates are qualitatively and statistically robust to including individuals who turned 18 close to the cutoff.

While our main focus is on the impact of eligibility for the 1988 plebiscite, we also consider eligibility thresholds for other upstream presidential elections, including the 1989, 1993, 1999, 2005 and 2009 elections. This analysis provides a credible internal benchmark to determine whether the impacts of plebiscite eligibility are salient vis-à-vis other upstream elections. We do so by re-estimating equation (1) for each election separately. Thus, for any pair of these elections $\{k, j\}$ with $j \geq k$ we estimate:

$$(2) \quad Y_i^j = \alpha_k^j + \delta_k^j Before_{ik} + \mu_k^j(Cutoff_{ik}) + Before_{ik} \times \mu_k^j(Cutoff_{ik}) + \varepsilon_{i,k}^j$$

where $Before_{ik}$ is a dummy variable which equals 1 if person i turned 18 prior to the eligibility cutoff for upstream election k. Equation (2) thus allows us to recover the estimated effect of eligibility in upstream election k on registration/turnout outcomes in downstream election j.

In addition, to formally test for whether the effects of the plebiscite are statistically different from other upstream elections, we also consider a regression discontinuity design that jointly assesses the impacts of eligibility across upstream elections. Let E_i^k be a dummy variable which equals one if person i turned 18 around the eligibility cut-off for upstream election k, such that $\sum_{k=0}^{j} E_i^k = 1$. Thus, if we define the 1988 plebiscite as the baseline ($k = 0$) election, we can

write:
(3)

$$Y_i^j = \sum_{k=0}^{j} E_i^k \cdot \left[\alpha_k^j + \delta_k^j Before_{ik} + \mu_k^j(Cutoff_{ik}) + Before_{ik} \cdot \mu_k^j(Cutoff_{ik})\right] + \epsilon_i^j$$

from where we can test whether eligibility to vote in the 1988 plebiscite has a differential effect on Y_i^j relative to eligibility in other upstream elections (we examine the coefficient on $\beta_k^j = \delta_0^j - \delta_k^j$ for any upstream election k prior to j). To formally test for differences in estimates across upstream elections, we estimate equation (3) using voter turnout in the 2013, 2016 and 2017 elections as outcomes. To construct the set of right-hand side variables, we use election eligibility for 1988 (baseline), 1989, 1993, 1999, 2005 and 2009.

For implementation, we follow Gelman and Imbens (2019) and choose a linear functional form as our main specification. To select a bandwidth, in principle, one could examine the optimal CCT bandwidth (Calonico, Cattaneo and Titiunik, 2014) across upstream and downstream elections as well as for each specification. However, this strategy yields a large number of different values, which are not comparable across elections and outcome variables. We therefore select a 26-week bandwidth, which gives us a full year of coverage for each upstream election. In the Appendix, we show our results are robust to different values ranging between three weeks and one-year.[9] We use a uniform kernel to estimate equations (1)-(3), yet our results are robust to alternative kernel choices.

IV. Main Effects

A. Effects on Voter Registration

We first present our benchmark estimates of plebiscite eligibility upon downstream registration and downstream voting over a period of three decades. We begin by showing our results in the raw data and then follow up with our econometric estimates. In Figure 1, we plot 1988 plebiscite registration rates by birth week. We see that approximately 20% of the cohort who were born in the last week of August registered in time for the plebiscite. Upwards of 40% of the cohort born in the second to last week of August registered to vote. Thus, even having one additional week to register dramatically increased registration rates. The rate of increase in registration rates per additional week of time to register

[9]In Table A.2, we present evidence on covariate balance by estimating equation (2) with a linear polynomial and a 26 week bandwidth using different covariates as outcomes. We do not find significant differences in any covariate across the plebiscite cut-off. In a few of the other upstream elections, we find minor differences in educational attainment across the eligibility cut-off, which are likely driven by Chile's school enrollment cut-off on April 1. A 26-week bandwidth around elections held in December capture some individuals in different school cohorts (McEwan and Shapiro, 2008). We present balance in education in Table A.2 using a 13-week bandwidth, finding only one significant difference in 15 coefficients. In Section IV, we show our results are robust to a 13-week bandwidth.

is large — about two-thirds of those who turned 18 eight weeks prior to the cutoff had registered to vote. There is a smaller, though steady, rate of increase in registration rates over the next 4 months. Those who had six months to register signed up at a near 75% rate. All in all, we estimate equation (1) using a one-sided eight-week donut hole in order to identify off of registrants who would normally have registered without a surprise and sudden closure of voter registration. We find that plebiscite eligibility increased contemporaneous registration by 66 percentage points.

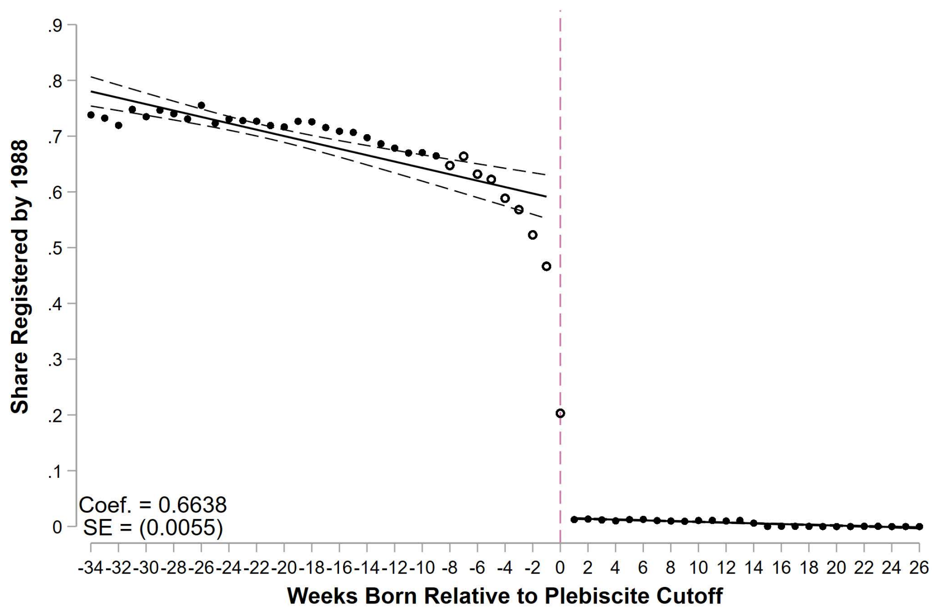

FIGURE 1. THE EFFECT OF PLEBISCITE ELIGIBILITY ON PLEBISCITE PARTICIPATION

Note: Figure 1 shows graphical evidence of registration rates in 1988 by week of birth within a year of registration closing for the plebiscite. Week 0 corresponds to the August 30^{th}, 1970 birth cohort week. The estimated coefficients and standard errors follow from estimates of equation (1), using a one-sided eight-week donut hole specification for plebiscite eligibles as described in Section III.

While the initial differences in registration rates across the cutoff are not surprising in that they are largely mechanical, these patterns are highly persistent over time. The first line in Figure 2 presents regression discontinuity estimates of the impact of marginal plebiscite eligibility upon contemporaneous registration along with its impacts on downstream registration, as well. By the 1989 Presidential election, sizable differences in registration rates among plebiscite eligibles and ineligibles remained, exceeding 40 percentage points. Registration rates increased significantly for both eligibles and non-eligibles over the next two decades (Table A.3), yet marginal plebiscite-eligibility led to registration rates which were

17 percentage points higher than their ineligible counterparts by 2009.

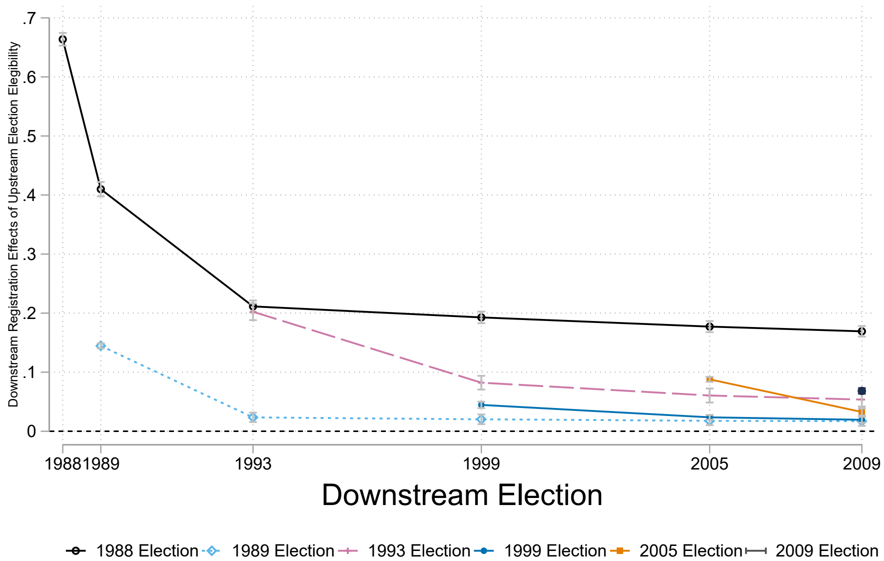

Figure 2. Concurrent and Downstream Registration Effects of Upstream Election Eligibility

Note: Figure 2 presents estimates of equation 2 using a linear functional form with a 26 week bandwidth across each election cut-off. For plebiscite eligibility (black solid line), we use a one-sided eight-week donut hole specification. Each line presents the impacts of eligibility for different upstream elections (1988 Plebiscite, 1989 1993, 1999, 2005 and 2009 Presidential elections) on concurrent and downstream registration outcomes. The first point in each line corresponds to the impact of initial eligibility on concurrent registration. The subsequent points in each line show the impacts of eligibility in an upstream election on registration by each downstream election. The light gray vertical lines around each point represent 95% confidence intervals. The estimated coefficients are presented in Table A.3.

These results are consistent with rational political behavior. Registration in Chile before the 2012 electoral reform was costly not only due to the time it took to figure out how to register and to then sign up, but also because it entailed a permanent future commitment to voting enforced by the possibility of non-trivial fines. Since the 1988 plebiscite was particularly salient, it is certainly possible that the costs of registration were the same for marginally eligible and marginally ineligible cohorts but that the benefits of registration were substantially higher for the marginally eligible given the importance of the plebiscite itself.

To illustrate the plebiscite's unique and historic nature – it was an opportunity to effectively end the dictatorship – in the remaining lines of Figure 2, we present regression discontinuity estimates of the impact of marginal eligibility in other presidential elections. Whereas the 1989 presidential election was held just 14 months after the 1988 plebiscite, the light blue line shows that only 14.5% of marginal eligibles registered to vote. This is despite the fact that, in contrast to the plebiscite, the registration deadline was announced months ahead of time. This 78% decline in the impact of marginal eligibility on contemporaneous registration suggests that the electoral fervor surrounding the return to democracy had quickly died down, potentially due to the absence of mass mobilization (González and Prem, 2018).[10] The substantially smaller effects of marginal eligibility on concurrent registration persisted for all subsequent elections in the pre-reform era. Only the 1993 effect is larger (20.2 percentage points) than the 1989 effect, and the effects for all other years are below 10 percentage points. All in all, marginal eligibility for all other upstream elections resulted in far smaller effects on 2009 registration rates compared to the 1988 plebiscite.

B. Effects on Voter Turnout

We turn to the individual-level voter turnout data to examine the impacts of plebiscite eligibility on turnout for the 2013, 2016 and 2017 elections. Since Chile's 2009 electoral reform led to automatic registration for all age-eligible Chileans, the estimated impacts of plebiscite eligibility on downstream registration rates disappeared following the reform.

Figure 3 displays raw voter turnout rates for the 2013 and 2017 presidential elections by birth week cohorts for those born between 1950 and 1990. Figure 3 shows a large secular decline in turnout rates across birth cohorts: 70% of Chileans born in 1950 turned out for the 2013 election, doubling the participation of their counterparts born 40 years later. One discontinuity which shows up clearly over the entire 40-year period and across both elections: that which corresponds to the eligibility threshold for the 1988 plebiscite.

Our main specification (equation (3)) jointly estimates the effects of marginal upstream election eligibility upon voter turnout in the 2013, 2016 and 2017 elections. These results are presented in Table 2. The first row shows the estimated impact of plebiscite eligibility. These estimates are statistically significant across all three elections. Eligibility to participate in the plebiscite increased voter turnout in the first round of the 2013 and 2017 presidential elections by 5.2 and 2.9 percentage points, respectively. Relative to baseline turnout rates in both elections — 49.6% and 47.2%, respectively — the estimated impacts of plebiscite eligibility correspond to an increased turnout rate of 10.5% and 6.1% in the 2013 and 2017 elections.[11]

[10] An alternative explanation for the decline in the initial eligibility effect is that 1988 plebiscite marginal eligibles were those who had just turned 18. On the other hand, marginal eligibles for subsequent elections captured those who would turn 18 just before the election. If most potential voters pay attention to voter registration only upon turning 18, closing registration early while allowing voting-eligible 17 year olds to register may reduce the impact of marginal eligibility. First stage results are robust to longer bandwidths — which include marginal eligibles who had turned 18 by the registration deadline — suggesting the results are robust to such concerns.

[11] In our main results, we cluster standard errors at the week-of-birth level. We also consider clustering at the month-of-birth level, yet this approach yields a small number of clusters. For robustness, we first use the wild cluster bootstrap and separately use Newey-West standard errors with varying lag structures. Significance levels remain unchanged.

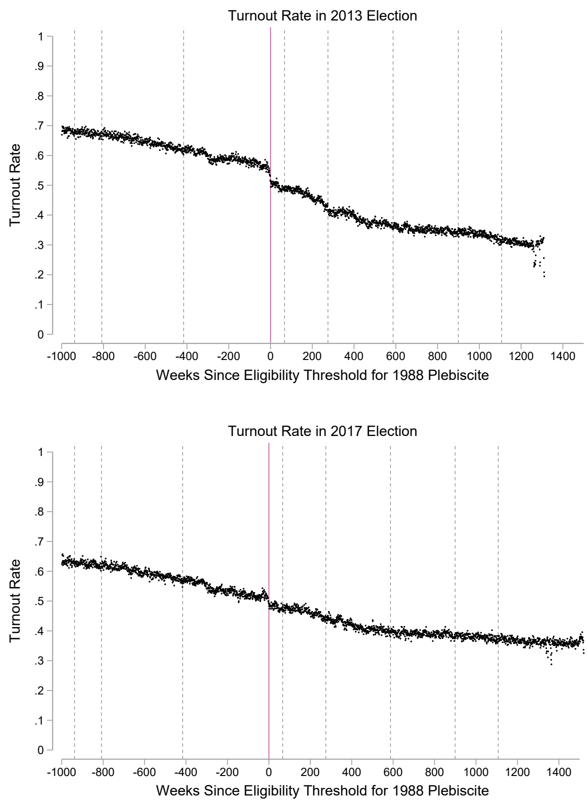

Figure 3. Long-Term Differences in 2013 and 2017 Election Turnout Rates by Birth Cohort

Note: Figure 3 shows graphical evidence of the share of individuals who had turned out to vote for the 2013 and 2017 Presidential elections by week of birth cohort. Week 0 corresponds to the August 30^{th}, 1970 birth cohort. Gray lines denote age-based cutoffs for eligibility in Presidential elections which took place in 1970, 1989, 1993, 1999, 2005 and 2009, as well as the 1973 Congressional elections and the 1980 referendum.

We also find a significant effect on a lower-stakes municipal election held in 2016; upstream eligibility resulted in increased turnout by 2.7 percentage points, or 7.5%, relative to baseline participation rates. In Figure A.1, we present estimates of plebiscite eligibility on downstream election turnout using placebo cut-offs, spaced at multiples of 6 months from the actual cut-off date within a six-year window of the plebiscite registration date. We find that the actual cutoff is associated with substantially larger downstream turnout effects vis-à-vis the placebo cutoffs. Moreover, in Figure A.2, we show that the regression discontinuity estimates are robust to bandwidths ranging from three weeks to up to one year. In light of differences in pre-2009 registration rates across the eligibility cut-off, we note that plebiscite eligibility led marginal eligibles to participate in

Table 2—Estimated Effects of Upstream Election Eligibility on 2013, 2016 and 2017 Turnout

	2013 Election	2016 Election	2017 Election
Before × Plebiscite	0.0525	0.0275	0.0294
	(0.0049)	(0.0039)	(0.0036)
Before × 1989 Election	-0.0003	0.0050	0.0029
	(0.0030)	(0.0030)	(0.0037)
	[0.0000]	[0.0000]	[0.0000]
Before × 1993 Election	0.0135	0.0060	-0.0034
	(0.0040)	(0.0043)	(0.0043)
	[0.0000]	[0.0002]	[0.0000]
Before × 1999 Election	-0.0078	-0.0046	-0.0017
	(0.0029)	(0.0035)	(0.0035)
	[0.0000]	[0.0000]	[0.0000]
Before × 2005 Election	-0.0073	-0.0005	-0.0101
	(0.0046)	(0.0034)	(0.0034)
	[0.0000]	[0.0000]	[0.0000]
Before × 2009 Election	-0.0050	-0.0074	-0.0058
	(0.0047)	(0.0042)	(0.0041)
	[0.0000]	[0.0000]	[0.0000]
Observations	1,586,262	1,581,918	1,581,856

Note: Table 2 presents estimates of equation (3) using a linear functional form with a 26 week bandwidth across each election cut-off. Each coefficient corresponds to the effect of eligibility for each upstream election on turnout in the 2013, 2016 and 2017 elections. The estimates for the 1988 plebiscite follow from a specification which uses a one-sided eight-week donut hole for plebiscite eligibles. Standard errors in parentheses, clustered at the week-of-birth level. In brackets, we report the p-values of the estimated differences of the impacts of upstream eligibility for the Plebsicite vis-à-vis other (1989 1993, 1999, 2005 and 2009) upstream Presidential elections.

an additional 2.28 elections between 1989 and 2009.[12] As such, differences in pre-2009 electoral participation induced by initial eligibility may have contributed to the downstream turnout impacts documented here. All in all, the original event has therefore had an impact over a time period corresponding to around half of an adult's political life.

The results in Table 2 are further confirmed by the graphical evidence presented in Figure 4, which again show a linear decline in turnout for cohorts closer to the eligibility cutoff. This decline can be explained by the results shown in Figure 1, as cohorts born closer to the cutoff were substantially less likely to register in time than those born even a few weeks earlier. Meanwhile, turnout rates are

[12]We calculate this number by estimating equation (1) using registration by each election year as the outcome variable. We multiply the estimated coefficient by the average turnout in that election to estimate additional participation in each 1989-2009 election.

mostly flat across the cutoff for Chileans on the margins of eligibility, except for those who came of age around the time of the 1988 plebiscite.

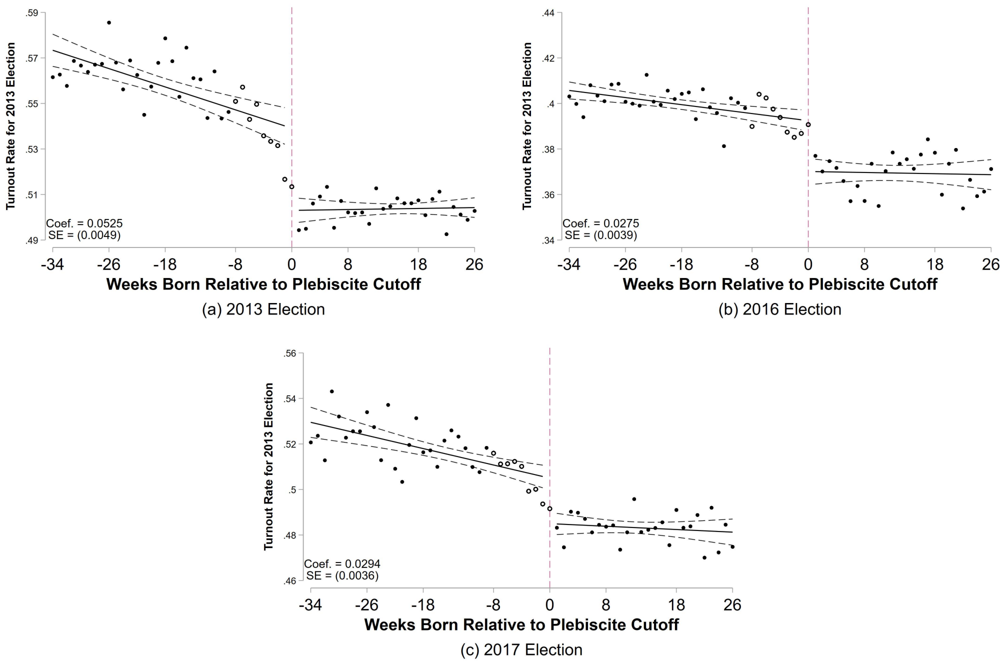

Figure 4. Differences in Downstream Election Turnout Rates Across Plebiscite Eligibility Cutoff

Note: Figure 4 shows graphical evidence of differences in 2013, 2017 Presidential election and 2016 municipal election turnout rates in a linear specification across the eligibility cut-off (26-week bandwidth) in the 1988 plebiscite. The estimated coefficients and standard errors follow from estimates of equation (1), using a one-sided eight-week donut hole specification for plebiscite eligibles as described in Section III. Nine empty circles denote the removed donut hole which are shown on the figure but not used in estimation.

Table 2 further documents the comparative effects of eligibility for other upstream elections (relative to the plebiscite) on downstream turnout rates. For the 2013 election, we find that plebiscite eligibility had a significantly larger impact than any other upstream election. We document similar findings for the 2016 and 2017 elections, finding that the differential downstream voting impacts of other elections are all statistically distinguishable from the plebiscite effect.

Table 2 further shows that the effects of the 1988 plebiscite do not generalize to other elections. For instance, while marginal eligibility for the 1993 Presidential election increased turnout in 2013, the effect faded for the two subsequent elections.[13] Moreover, we find that 2005 election eligibility depressed turnout in

[13]To address concerns of covariate imbalance in educational attainment for other upstream elections,

2017.[14] We also examine the impacts of eligibility for the three elections prior to the 1988 plebiscite — encompassing the 1970 Presidential election, the 1973 Parliamentary election and the 1980 plebiscite — and we fail to find significant impacts on electoral turnout in 2013 and 2017 (Figure A.6). These results confirm the long-term electoral impacts of the 1988 plebiscite. We posit these differential downstream impacts are driven by the salience and importance of the plebiscite vis-à-vis the other upstream elections. However, as we only consider one high-stakes election, our results may not generalize to a broader theory on the relationship between initial election salience and downstream turnout effects (Franklin and Hobolt, 2011; Dinas, 2012).

C. Mechanisms: Persistence and Initial Mobilization

Two alternative channels could explain our estimated impact of plebiscite eligibility on downstream electoral turnout: a large mobilization (i.e. turnout) in the original plebiscite and a high degree of turnout persistence afterwards. For a given degree of persistence, a larger initial mobilization results in larger downstream effects as the size of the treated group is larger. For a given level of initial mobilization, a higher degree of persistence leads to larger downstream effects since the effects last longer. Though mobilization was obviously larger for the 1988 plebiscite, it is possible that participation in the plebiscite also engendered a more persistent attachment to voting.

In particular, we regress turnout in downstream election j on upstream turnout in election k, instrumenting upstream turnout in election k with eligibility in the sample of marginal potential registrations. In other words, we essentially estimate a fuzzy regression discontinuity model where our first stage comes from equation (3) and our second stage is given by:

$$Y_i^j = \alpha + \sum_{k=0}^{K} \gamma_k^j \widehat{Y_i^k} + e_i^j \tag{4}$$

where Y_i^j denotes having turned out to vote in the post-reform downstream election j ($j > k$). γ_k^j captures the 'persistence' effect – that is, the extent to which having voted in upstream election k results in *persistent* turnout in downstream election j.[15]

columns (1)-(3) in Table A.4 present estimates of equation (2) using a 13-week bandwidth. These estimates are not different than results using a 26-week bandwidth, underlying the robustness of our results.

[14]We present graphical evidence of these findings in Figures A.3-A.5. Coppock and Green (2016) have also documented that participation in certain upstream elections in the United States has negative consequences on downstream turnout. For example, participating in an election where ex-post the executive disappointed voters could make those who voted relatively less likely to participate in the future.

[15]The causal interpretation of the effect of upstream participation on downstream turnout holds if

Our fuzzy regression discontinuity model differs from a standard fuzzy RD in one key respect. We only observe registration as opposed to turnout for the upstream elections. However, to interpret γ_k^j in equation (4) as a persistence-in-voting coefficient, Y_i^k must measure turnout rather than registration. As a result, we adjust registration by multiplying it by the electoral turnout rates reported in Table 1.

Table 3—Estimated Effects of Upstream Election Participation on 2013, 2016 and 2017 Turnout

	2013 Election	2016 Election	2017 Election
Before × Plebiscite	0.0811	0.0426	0.0455
	(0.0072)	(0.0059)	(0.0055)
Before × 1989 Election	-0.0019	0.0364	0.0212
	(0.0220)	(0.0222)	(0.0270)
	[0.0003]	[0.7894]	[0.3769]
Before × 1993 Election	0.0734	0.0324	-0.0183
	(0.0204)	(0.0228)	(0.0233)
	[0.7219]	[0.6661]	[0.0078]
Before × 1999 Election	-0.1954	-0.1134	-0.0412
	(0.0744)	(0.0878)	(0.0861)
	[0.0002]	[0.0764]	[0.3145]
Before × 2005 Election	-0.0947	-0.0067	-0.1315
	(0.0602)	(0.0439)	(0.0447)
	[0.0038]	[0.2663]	[0.0001]
Before × 2009 Election	-0.0848	-0.1257	-0.0984
	(0.0802)	(0.0708)	(0.0696)
	[0.0396]	[0.0178]	[0.0393]
Observations	1,586,262	1,581,918	1,581,856

Note: Table 3 presents estimates of a fuzzy regression discontinuity design (equation (4)) using a linear functional form with a 26 week bandwidth across each election cut-off. The results refer to the estimated impacts of upstream election participation on turnout in the 2013, 2016 and 2017 elections. The estimates for the 1988 plebiscite follow from a specification which uses a one-sided eight-week donut hole for plebiscite eligibles. In brackets, we report the p-values of the estimated differences of the impacts of upstream participation in the plebiscite vis-à-vis other (1989, 1993, 1999, 2005 and 2009) upstream elections. Standard errors in parentheses, clustered at the week-of-birth level.

Table 3 presents our persistence estimates across upstream and downstream elections. We remark that the persistence parameter equals the reduced form

the exclusion restriction is satisfied, which requires that initial eligibility affects downstream turnout solely through initial registration. Yet in the plebiscite, the campaign itself may have politicized eligible voters, driving turnout irrespective of their initial participation. We thus interpret the estimates in this sub-section as a scaled estimate of the results presented in Table 2.

estimate presented in Table 2 divided by the turnout-adjusted first stage. As a result, we find that having voted in the plebiscite results in a higher turnout rate equal to 8.1 percentage points in the 2013 presidential election, or 16% relative to baseline participation rates. The persistence estimate declines to 4.3 percentage points for the 2016 election, which represents 12% of baseline participation due to lower turnout in municipal elections. On the other hand, the estimated impact falls to 4.5 percentage points by Chile's 2017 presidential election, yet the turnout effects remain statistically significant and sizable almost 30 years after the plebiscite.[16]

We also present the persistence effects of other upstream elections to consider whether the effects of the initial plebiscite effects are particularly long lasting. Voting in the plebiscite had larger effects on 2013 turnout than having voted in any other election, yet the effects are not statistically different than for the 1993 election. For the 2017 presidential election, the persistence effects of the plebiscite are not distinguishable from those of the 1989 and 1999 elections. Similarly, the effect of the plebiscite on turnout in the 2016 election are only statistically larger than those of the 1999 and 2009 upstream elections. Lastly, we find that voting in the 1999, 2005 and 2009 may have depressed turnout in downstream elections, which may arise due to the reasons discussed above.[17]

While we had previously found that the plebiscite had a far larger downstream impact than any other upstream election (Table 2), the difference in the estimated persistence effects across upstream elections is not as large. In fact, the plebiscite persistence estimates are also not necessarily larger than those found in the United States (Coppock and Green, 2016). In contrast, the mobilization effects for the plebiscite are significantly larger than the mobilization effects for the presidential elections. As such, our results show the large impacts of plebiscite eligibility on downstream participation are not predominantly due to an unusually high degree of persistence, but rather because of an unusually large initial mobilization of the vote.

V. Heterogeneous Effects and Partisanship Effects

How did plebiscite eligibility affect downstream electoral outcomes across different groups? Despite the fact that we do not observe turnout outcomes by educational attainment through 2009, we can characterize differential registration effects by education, a first in the literature. We also analyze heterogeneous effects by gender and indirectly by partisanship. Since the downstream plebiscite estimates presented in Section IV vary to some degree by bandwidth, we also check for heterogeneity in complier characteristics across bandwidths.

[16]In Figure A.7, we show the robustness of the estimated effects of plebiscite participation on downstream turnout to bandwidths ranging from two weeks to one year.

[17]Table A.5 shows our estimates for other upstream elections are robust to using a 13-week bandwidth.

A. Gender, Partisanship and Education

Gender. Since Pinochet's opposition decided to target women in their *franja* slot based upon focus groups and research by consulting firms (Hirmas, 1993), we first examine the heterogeneous effects of plebiscite eligibility by gender. We estimate equation (1), interacting eligibility with gender. We fail to find statistically different effects by gender, on both plebiscite registration and downstream electoral turnout, yet the relative impacts are larger for men in light of their lower baseline electoral turnout. Estimates by gender are shown in Table A.6 and in Figure A.8.

Partisanship: Effects by Salvador Allende's 1970 Support. Since we do not directly observe voters' partisan affiliation at the individual level, we rely on pre-plebiscite measures of political affiliation in order to analyze how downstream effects vary by partisanship. We thus consider heterogeneous effects by Allende vote share at the *comuna* level in the last pre-dictatorship election, held in 1970. Allende's support was highly heterogeneous across the country, as he received less than 15% of the vote in *comunas* such as Providencia and over 65% of electoral support in Coronel and Lota. Similar to González and Prem (2018), we estimate heterogeneity in initial registration by prior Allende vote-share to analyze whether the plebiscite differentially mobilized the left and also in downstream persistence, to analyze whether the long-term effects were larger for left-leaning groups.

Table 4—Heterogeneous Effects of Plebiscite Eligibility by Partisanship: Allende Support

	1988 Plebiscite	2009 Registration	2013 Election	2016 Election	2017 Election
Before	0.662	0.170	0.052	0.031	0.029
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Before × Allende %	-0.055	0.061	0.106	0.001	0.034
	(0.035)	(0.042)	(0.048)	(0.053)	(0.052)
Observations	226,255	226,255	226,255	225,273	224,809

Note: Table 4 presents evidence of heterogeneous effects of plebiscite eligibility on concurrent plebiscite registration, 2009 registration and downstream 2013, 2016 and 2017 election participation in a linear, 26-week bandwidth specification using a one-sided eight-week donut hole for plebiscite eligibles by (demeaned) 1970 Allende vote share. We control for 1992 Census comuna characteristics including unemployment rate, literacy rate and the share of household with electricity, water and toilet in the home. Standard errors in parentheses, clustered at the week-of-birth-comuna level.

We estimate an interactive regression discontinuity design, interacting each term in equation (1) with $Allende_{ic}$, which corresponds to Salvador Allende's vote share in the 1970 election in person i's *comuna* (c) of residence at the time of registration. We also control for various *comuna*-level characteristics measured in the 1992 census, including *comuna*-level unemployment rate, literacy rate, and various measures of household well-being. We present our results in Table 4. The first column shows that eligible Chileans living in high-Allende support *comunas* had lower registration rates for the plebiscite vis-à-vis their counterparts in less left-leaning localities, though the differences are not statistically significant. On the other hand, in the last three columns, we show that plebiscite eligibles who lived in left-leaning *comunas* had higher downstream turnout rates, yet the effect is only statistically significant for the 2013 election. The coefficient for the 2013 election indicates that an increase in the Allende share from 0% to 100% is associated with a 10.6 percentage point higher impact of plebiscite eligibility on downstream turnout. The estimated effects for the 2016 municipal election are significantly smaller in magnitude, but equal 3.4 percentage points in the 2017 election, albeit not statistically significant. These results are suggestive, especially since we do not observe *comuna* of residence at the time of the plebiscite; yet they suggest that participating in the plebiscite may have had larger long-term effects for left-leaning individuals.

Educational Attainment. An extensive literature has documented higher turnout rates among highly educated citizens, both in developed countries (Milligan, Moretti and Oreopoulos, 2004; Sondheimer and Green, 2010; Marshall, 2019; Kaplan, Spenkuch and Tuttle, 2022) and in Latin America (Haime, 2017). However, to the best of our knowledge, the existing literature has not yet examined how upstream election eligibility affects participation differentially by education. While we do not observe turnout effects by education, we examine heterogeneous registration effects by education, providing an important contribution to the literature.

We estimate equation (1) using the donut-hole specification with a linear polynomial and a six-month bandwidth separately for high school dropouts, high school graduates and those who have gone beyond high school. We present the results in Table 5. The first panel shows the estimated effects for the plebiscite. We find larger first-stage effects for more highly educated individuals, as eligibility induces 56 percent of those with at least some post-secondary education to register, relative to 45 percent of high school dropouts. On the other hand, by 2009, we find slightly larger registration effects for high school dropouts compared to their higher-educated peers. In other words, even though a higher fraction of high school graduates were initially registered, a higher fraction of non-high school graduates who were registered would not have registered to vote subsequently but for the plebiscite. Moreover, since high school dropouts have far lower baseline 2009 registration rates, plebiscite eligibility resulted in downstream registration rates which were 28% higher than those for their ineligible counterparts — significantly higher than the corresponding effect (16%) for those in the highest-education group.

Table 5—Heterogeneous Effects of Upstream Election Eligibility by Educational Attainment

	Initial Registration			2009 Registration		
	HS Dropouts (1)	HS Grad. (2)	> HS Grad. (3)	HS Dropouts (4)	HS Grad. (5)	> HS Grad. (6)
Panel A. 1988 Plebiscite						
Before	0.449	0.529	0.558	0.168	0.157	0.125
	(0.019)	(0.011)	(0.006)	(0.010)	(0.004)	(0.007)
Control Mean	0.000	0.000	0.000	0.601	0.688	0.774
Observations	61,687	92,092	27,593	61,687	92,092	27,593
Panel B. 1989 Election						
Before	0.053	0.085	0.169	-0.011	0.012	0.039
	(0.003)	(0.003)	(0.005)	(0.004)	(0.006)	(0.008)
Control Mean	0.000	0.000	0.000	0.580	0.660	0.723
Observations	63,286	98,873	31,549	63,286	98,873	31,549
Panel C. 1993 Election						
Before	0.085	0.135	0.200	0.020	0.045	0.071
	(0.017)	(0.012)	(0.011)	(0.014)	(0.008)	(0.009)
Control Mean	0.000	0.000	0.000	0.323	0.378	0.505
Observations	54,416	99,126	36,959	54,416	99,126	36,959
Panel D. 1999 Election						
Before	0.008	0.009	0.059	0.022	0.017	0.012
	(0.003)	(0.004)	(0.008)	(0.010)	(0.003)	(0.008)
Control Mean	0.000	0.000	0.000	0.185	0.236	0.388
Observations	47,421	121,034	48,213	47,421	121,034	48,213
Panel E. 2005 Election						
Before	0.014	0.035	0.116	0.005	0.011	0.060
	(0.002)	(0.003)	(0.005)	(0.003)	(0.004)	(0.009)
Control Mean	0.000	0.000	0.000	0.066	0.103	0.215
Observations	28,074	132,316	57,646	28,074	132,316	57,646
Panel F. 2009 Election						
Before	0.010	0.047	0.063	0.010	0.047	0.063
	(0.003)	(0.004)	(0.003)	(0.004)	(0.008)	(0.007)
Control Mean	0.000	0.000	0.000	0.000	0.000	0.000
Observations	35,805	174,064	7,373	35,805	174,064	7,373

Note: Table 5 presents evidence following from equation (1) documenting heterogeneous effects of upstream election eligibility on concurrent registration (first three columns) and 2009 registration in a linear, 6-month bandwidth specification (last three columns). For the 1988 plebiscite specification, we use a one-sided two-month donut hole for plebiscite eligibles. In Panel F, note that columns (1)-(3) are identical to columns (4)-(6). Standard errors in parentheses, clustered at the month-of-birth level.

In the remaining panels, we examine whether registration effects vary by upstream election. We find multiple substantial differences. First, initial mobilization (first stage) effects of post-1988 elections are smaller in magnitude uniformly for all educational groups than for the plebiscite, confirming the results presented in Table A.3. Second, we find far larger initial mobilization effects for the beyond-high-school groups vis-à-vis high school dropouts in each election. Third, the mobilization gap across educational groups is by far the smallest for the plebiscite: while the ratio of the first-stage coefficient for these two groups equals 1.25 in the plebiscite, it exceeds 2.3 in all other upstream elections. Fourth, different from the plebiscite, where we still see 16.8 percentage point higher registration rates in 2009, we find that initial eligibility for high school dropouts yields small differences in 2009 registration rates among eligibles relative to ineligibles for all other elections. The largest downstream effect for the 1999 presidential election, only reaches 2.2 percentage points, or one-eighth of the estimated plebiscite effect. These results thus indicate that plebiscite eligibility induced a sizably larger share of less educated individuals to initially register to vote and initial eligibility was associated with higher downstream registration rates for this group only for the plebiscite.

Complier Characteristics. Lastly, we follow Abadie (2003) to assess how the characteristics of compliers vary across upstream elections and bandwidths to understand how different elections and time to registration affects the types of individuals that sign up to vote. In this context, the complier ratio compares the characteristics of marginally eligible individuals who registered to vote to those who turned turned 18 around the eligibility cut-off. The complier ratio for high school dropouts is higher for the plebiscite than for all other elections and, importantly, increases with longer bandwidths. As such, less educated individuals are more likely to register when they have additional time to do so. We also find that the 1988, 1989 and 1993 elections had a higher male complier ratio, yet this pattern reversed in subsequent elections (Figure A.9).

B. Partisanship Effects

The results presented so far show a sizable share of Chileans over 18 were induced to register to vote due to age-based eligibility, and that these individuals were relatively more likely to be less educated vis-à-vis compliers in other subsequent elections. As a result, the plebiscite permanently shifted the composition of the Chilean electorate under the old electoral system. We thus examine whether the plebiscite had an impact on subsequent electoral outcomes, given the twenty years of *Concertación* presidents after the reinstatement of democracy. The analysis presented here is suggestive, as we do not observe individual-level partisan turnout/support.

In order to compute a back-of-the-envelope estimate of the impact of the plebiscite upon the *Concertación* vote share, we rely on four pieces of information. First, we recover the number of plebiscite eligibles by educational attainment group.[18] We then multiply this number by the estimated downstream election registration effect by education group presented in Table 5. We further adjust this number

[18] We construct this number as follows. From the merged administrative data, we directly observe the number of individuals who turned 18 prior to the eligibility cut-off by attainment group. In Table A.1, we had shown that the merged administrative data under counts the number of eligible individuals. We address this issue by multiplying the number of eligibles by education group by 1.4, which is the ratio of eligible individuals observed in the SERVEL data to the number in the merged administrative data. We thus assume that attainment is missing at random. Since the registration data was collected in 2009, we restrict our analysis to eligible individuals born in 1930-1970 to avoid including older citizens who had died by 2009, which provides a conservative estimate of partisanship effects.

by the average turnout rate for each presidential election, which ranged between 86.7% and 94.5%, as shown in Table 1. Lastly, we impute the partisanship effect by taking advantage of pre-election polls conducted by CEP in 1989, 1993, 1999, 2005 and 2009 — these polls include measures of heterogeneous support for the *Concertación* by educational attainment.[19,20]

Table 6—Vote Gain from the 1988 Plebiscite

Year of Election		1989	1993	1999	2005	2009
Turnout Rate		0.947	0.912	0.900	0.877	0.867
Size of treatment effect	HS Drop. (3.32 million[a])	0.315	0.167	0.166	0.164	0.168
	HS Grads (2.44 million[a])	0.333	0.167	0.160	0.159	0.157
	> HS Grads (0.59 million[a])	0.299	0.130	0.123	0.127	0.125
Concertación vote share	HS Dropouts	0.589	0.610	0.526	0.527	0.588
	HS Graduates	0.558	0.591	0.496	0.521	0.561
	> HS Grads	0.504	0.562	0.445	0.489	0.517
Total effect of the plebiscite		3.73%	2.54%	0.22%	0.54%	0.54%
on the left wing vote share		(0.07)	(0.09)	(0.02)	(0.02)	(0.02)
Concertación vote margin		5.17%	7.98%	1.31%	3.49%	-1.60%

Note: (a): figures in parenthesis represent the number of individuals eligible by education group (E_k). These are calculated from the number of eligible individuals born between 1930-1970 from the merged administrative data multiplied by the ratio of non-missing educational attainment. The turnout rate follows from Table 1 (T_t). The size of the treatment effect follows from the specification estimated in the Panel A of Table 5 and from results available upon request for the 1993, 1999 and 2005 elections (γ_t^k). Lastly, the *Concertación* vote share (L_t^k) follows from CEP data from surveys conducted 1-2 months prior to each Presidential election (1989-2009) and shows stated the share of *Concertación* voters by educational attainment. CEP surveys include respondents' socioeconomic status. We use information from the 1999, 2005 and 2009 surveys, which include respondents' SES survey and educational attainment, to impute voting intent by educational attainment for all Presidential elections using the cross SES-education tabulation. The non-*Concertación* share (R_t^k) is equal to one minus L_t^k. We examine the impacts on first round elections. We calculate the effect of the plebiscite on the *Concertación* vote share in election t (η_t) as follows: $\eta_t = \sum_{k=1}^{K} E_k \times T_t \times \gamma_t^k \times (L_t^k - R_t^k)$. Bootstrapped standard errors from 1,000 replications are reported in parentheses.

We present our results in Table 6. We find significant gains for the *Concertación* in the 1989 and 1993 elections, reaching 3.7 and 2.5 percentage points, respectively, which correspond to 72% and 32% of the average margin of victory for the coalition, respectively. Over time, first party dominance should mechanically fall as party allegiance dwindles over time, since the impact on persistent turnout

[19]These surveys were conducted 1-2 months prior to each election and include 1,000-1,500 respondents. Since CEP surveys do not include a consistent measure of educational attainment, we rely on a socioeconomic status indicator which classifies respondents in three categories. The 1999, 2005 and 2009 CEP surveys include respondents' educational attainment and socioeconomic status, we rely on this cross-tabulation to impute stated vote shares by education group.

[20]The 1999, 2005 and 2009 Presidential elections were decided in a run-off election. Since the CEP surveys in those years were carried out prior to the first round, our measure of *Concertación* support includes individuals who intended to vote for either the *Concertación* or for left-wing parties which supported the *Concertación* in the run-off.

reduces and as older cohorts are replaced by younger untreated cohorts. While the effects decline for the 1999 and 2005 elections, largely due to a changing education-*Concertación* gradient, the effects remain positive through the 2009 Presidential election. Furthermore, while we cannot extend this exercise through the 2013 and 2017 elections, the results presented in Table 4 indicated larger effects in left-leaning municipalities, suggesting the plebiscite may have shifted electoral outcomes for close to three decades in Chile. We further note that the estimates presented in Table 6 are likely lower bounds. First, we make a conservative assumption by only considering eligible individuals as 'treated' if they were born between 1930 and 1970. More importantly, we do not observe partisanship and therefore cannot directly estimate the differential turnout impacts upon those who would vote left versus right (the maximum differential voting rates for the left across our three educational groups and all elections is eight percentage points). Thus, since education is weakly correlated with and thus an imperfect signal for partisanship, using education as a proxy should strongly attenuate our estimates. Even so, we find moderate partisan impacts even two decades after the 1988 plebiscite.

Our estimates provide a potential partial explanation of one party dominance in newly democratic (including post-colonial) states (Magaloni, 2006; Magaloni and Kricheli, 2010). First, we use information from the Polity IV dataset to document the extent of party transitions in newly democratized countries. On average, the first post-dictatorship party remains in power longer than the second party, but this result is driven by a long right tail, as the first post-dictatorship party has remained in power for more than eighteen years in five different countries, including Chile (Figure A.10).[21] While the existing literature has examined the importance of the extensive margin of support (broad popularity) for the party establishing democracy as a mechanism for lengthy initial one-party dominance, we suggest an additional and novel mechanism which is also quantitatively important. We add an intensive margin mechanism: the party that wins democratic rights may become popular (extensive margin), but it may also bolster turnout (intensive margin) for decades to come.

VI. Conclusion

Electoral participation can be consequential even many decades later. We document that voting for the restoration of democracy in Chile's 1988 plebiscite, which ended 15 years of military rule, boosted turnout in the 2017 presidential election

[21]These cases include *Concertación* (Chile, 21 years), People's Progressive Party (Guyana, 23 years), Mozambique Liberation Front (Mozambique, 25 years), South West African People's Organization (Namibia, 30 years) and New Front for Democracy and Development (Suriname, 18 years). A Kolmogorov-Smirnov test indicates the distributions are not statistically different, but the right-tail for the distribution of the first party in power is longer than for the second party. Table A.7 includes the list of democratic transitions, and the length of government for the first (and second) party in power. We recover information on the year of democratic transitions from the Polity IV dataset (Marshall, Gurr and Jaggers, 2018).

by 4.5 percentage points. We further demonstrate that the long-lasting impacts of the plebiscite differ across elections mainly due to initial mass mobilization rather than differential persistence of voting. We document heterogeneous effects in concurrent registration and turnout rates by gender, by town partisanship and by education.

Different from the results in the existing literature, our findings reflect a pure effect of voting on future voter turnout as Chile abandoned voluntary registration as a precondition for voting after the 2009 presidential election. Since our empirical strategy does not rely upon voter registration files, our findings are robust to the biases resulting from the selectivity of registration, a common problem in this literature.

Finally, we provide suggestive evidence that electoral participation in the plebiscite shifted the electorate to the left by bolstering future turnout for *Concertación*. Since our findings emerge from the analysis of a distinctive event, these results may not generalize to other contexts. However, our results suggest that first elections after a period of autocracy can have long-term effects. Increased turnout for the party that wins democracy can help explain one party dominance in newly democratized countries.

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