



Documentos de Trabajo 60

‘Can I register to vote before I am 18?’ Information Costs and Participation

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Octubre 2014

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Abstract

Using a natural experiment we identify the effect of a particular procedural information cost on electoral registration of young first-time voters. Given that registration closing day is typically before election day, first-time voters who become eligible as they reach the minimum age requirement may face uncertainty on whether this rule is due at either closing day or election day. We argue that this uncertainty generates a discontinuity between individuals turning 18 a day before, and a day after, closing day. We provide empirical evidence of this using registration of Chilean first-time voters over two decades and across 12 elections. Implementing a sharp regression discontinuity design we estimate that registration decreases about 20% at the cutoff, causing an average drop of about 10% of turnout of the whole cohort of young first-time voters.

Keywords. Registration costs, procedural information costs, information acquisition, political participation.

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Information is essential for electoral participation. This conclusion is reached by ample investigation and discussion in the literature. While most of this work has focused on knowledge about the candidates or their policies,¹ a more basic form of information required is about the electoral procedure itself. Frequently asked questions such as ‘when?’, ‘where?’ and ‘how?’ to both register and vote, can have lasting consequences on the rate at which citizens, in particular first-time voters, end up participating. Especially when registration is self-initiated.² However, procedural information costs are hard to observe because they are endogenous to other election-related costs and benefits that determine the decision to participate. Some tests on the effect of procedural information costs have used direct mailing (Wolfinger et al. (2005); García-Bedolla and Michelson (2009)), and field or natural experiments (Brady and McNulty (2011) and Braconnier et al. (2013)). All these studies establish that procedural information costs have a significant and large impact on registration and voting.

This paper provides new evidence on the large effects that procedural information costs have on political participation, using a natural experiment that allows identification of this effect on young first-time voters. We consider the link between two particular procedural rules: the minimum age eligibility requirement and registration closing date (henceforth CD). In many countries, and several US states, first-time voters are eligible to vote if they meet the minimum age requirement – typically 18 years - at election day (henceforth ED), while registration CD is well before the election. The potential effect on registration of the gap between the timing of the eligibility rule and CD, is illustrated in the following example.

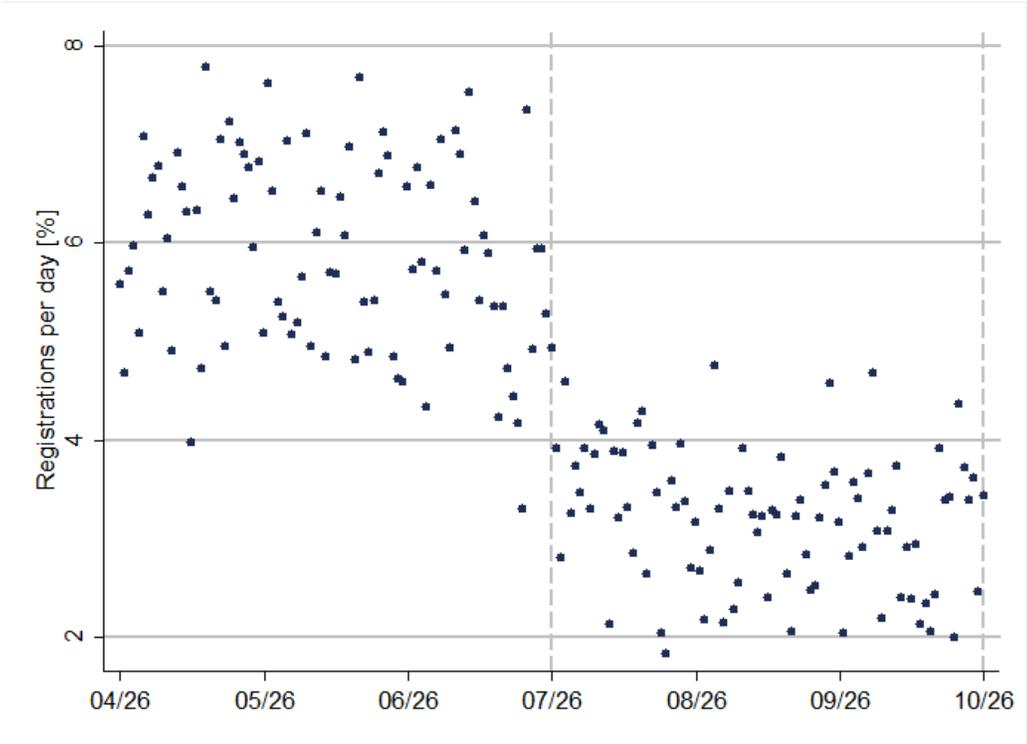
Figure 1 plots the registration rate on the electoral roll for the 2008 Chilean municipal election, which took place on October 26th, in terms of ‘18th-year-birthday’ dates. The registration rate is calculated thus as the fraction of registrants over the whole population, of individuals

¹See, among others, Thomas and Poole (1987), Matsusaka (1995), Feddersen and Pesendorfer (1996), Lassen (2005), and Larcinese (2007).

²In 37 out of 90 countries other than the US, registration is self-initiated (<http://aceproject.org>), which according to Wolfinger and Rosenstone (1980) is a more “complicated procedure”, often involving “more obscure information”.

turning 18 a particular date, per day. The registration rate corresponding to 06/26, for instance, is 6.56 percent, meaning that 55 young Chileans out of 838 turning 18 on June 26th 2008, registered to vote in that election.

Figure 1: Registration Rate per day



The eligibility rule allows all citizens that turn 18 before ED (second dashed line in the figure), to register and vote. The remarkable feature in figure 1 is the abrupt fall in the number of registrants turning 18 at the first dashed line, three months before the election. That date, July 26th, 2008, corresponds to registration CD.

We claim in this paper that the fall in registration at CD, the conspicuous pattern in the figure, is due to an information problem concerning procedural rules. Notice that individuals to the right of CD registered, necessarily, with 17 years. A natural issue for these individuals is whether they can register before they are 18, or not. Individuals who believe they cannot register have no incentives to acquire information about the registration process before they

turn 18, even when politically motivated. For them, incentives for information acquisition emerge exactly at their 18th birthday. As a consequence, they will only register, if so, if their birthday is before CD. Only then would they have enough time for registration after having acquired the needed information. Following this argument, we interpret the “jump” at CD as a measurement of the effect that uncertainty about a particular procedural rule has on political participation.

The paper gives empirical evidence of the effects of information costs using a national dataset comprising information on the registration of all registered citizens in Chile in two decades. We study all Chilean elections between 1989 and 2009. The Chilean registration system exhibits some particular advantages in the identification of the discontinuity at CD. Firstly, the gap between ED and CD is particularly large, between 3 and 6 months depending on the election. This made any strategic party behavior harder to implement, particularly because citizens had to be registered before the candidates themselves. Secondly, the former registration system in Chile required the minimum voting age at CD until 1988, changing it, from then on, to one with minimum voting age at ED. This change in the rule makes the information problem more acute, since first time-voters typically gather information from older citizens.

The data provides striking evidence of a discontinuity in the registration rate of those individuals turning 18 about CD. The effect is significant in the aggregate sample, in all different types of elections —presidential, parliamentary and municipal—, and in 11 out of 12 individual elections. We also note that in spite of changes to the ED-CD gap, which evolved from 6 to 4, and 3 months, during the period, we observe the discontinuity in all cases. The effect is also sizable. At the discontinuity, the registration rate of those individuals turning 18 a day after CD is reduced in about 20 percent when compared to the rate of those that did so a day before. The overall effect on the cohort of first-time voters as a whole is as large as 10 percent.

We also compare the socioeconomic composition on both sides of the discontinuity. Research on registration shows that registered individuals are more educated, belong to higher income

groups, and are more socially connected, than non-registered citizens.³ The cutoff at CD provides a unique opportunity to compare how different groups in the population are affected by registration information costs. We test the changes on four socioeconomic and demographic variables - income, schooling, rurality and gender - when we cross the discontinuity. We find significant effects on income and schooling, suggesting that poorer and less educated citizens are more affected by the uncertainty and information costs' asymmetry at CD, while gender and rurality are statistically the same on both sides of the cutoff. These effects are mild, however. Income of the younger registered individuals is approximately 4 percent higher to the right of the discontinuity, and 7 percent on average.

In the following section we discuss several possible determinants and mechanisms explaining the pattern in figure 1. Next, we discuss the Chilean registration system, the data, and the identification strategy. We then present our main empirical findings and results. Finally, we study the socioeconomic composition of the electorate on both sides of the discontinuity. The last section concludes.

‘CAN I REGISTER TO VOTE BEFORE I AM 18?’

In this section we discuss the potential explanations underlying the pattern observed in figure 1. A remarkable fact in this pattern is that individuals increase their propensity to register, *discontinuously*, when turning 18. In plain words, “something” is dramatically changing when individuals turn 18. Here we discuss what this “something” might be.

The administrative costs surrounding the registration process constitute a direct factor influencing participation.⁴ The question, however, is whether these costs change when an individual turns 18, or not. It would be the case, for instance, if registration were allowed to be dealt together with other paperwork due after the individual has turned 18 (e.g. the driving licence; or having registration offices located next to other public bureaus where other

³See Wolfinger and Rosenstone (1980); Piven and Cloward (1988); Mitchell and Wlezien (1995); Timponi (1998); and Knack and White (2000).

⁴See Rosenstone and Wolfinger (1978), Highton (2004), and Burden and Neiheisel (2013).

civic or public undertakings are dealt). In that case the individual would sink basically the same amount of administrative costs in exchange for a larger amount of benefits drawn from a bundle of errands.⁵ As we discuss further below, however, none of these alternatives apply to the Chilean registration system, which required a visit in person to a specific location with no purpose other than registration. Furthermore, we find no other procedure alike the registration procedure that first-time voters would need or want to undertake when turning 18.

Alternatively, changes to both the intrinsic or extrinsic benefits of voting may be causing the discontinuous pattern. After all, individuals register with the very purpose of participating in the election. As the election is fixed in time, individuals may discount the future benefits of voting when registering. Yet, there is no reason why this discount should decrease all of the sudden as individuals turn 18. A related candidate is “awareness”. As the election gets closer, citizens may receive information at no cost from the government, candidates, and political parties. That is, individuals’ attention to the election may change over time. The argument ruling out awareness as the underlying mechanism explaining the registration drop at CD is, again, that the change is discontinuous. That individuals put more attention to political issues after having turned 18 is quite plausible. Yet, this attention is hardly changing abruptly a day before, to a day later, one’s birthday, at any rate. It could be argued that parents usually advise their 18 years old youngsters to register and vote, but here we require such “talk” to occur exactly at their birthday, and not a day later. Overall, neither preferences nor awareness are changing discontinuously the very day individuals become of age.

Our explanation, on the contrary, is based on procedural information costs. We notice that young first-time voters have little knowledge about the registration process in general, in particular about ‘when’ the minimum age is required, at ED or CD. Indeed, in several US states registration services’ websites there is a FAQ of the following kind: “I will be 18 by election day, but I am not going to be 18 before the voter registration deadline. Can I still register and

⁵This principle underlies the “motor voter” programs in the US, which reduce the impact of administrative costs by bundling two different activities.

vote?”.⁶

The key point concerning uncertainty about this particular procedural rule is that it affects registration discontinuously. Let’s suppose that while first-time voters know that in order to participate they must register at some office at some point in time, they do not know precisely where the office is located, nor when is registration CD. In addition, let’s assume “some” of these individuals have a wrong belief about the eligibility rule: they believe that 18 years is required for registration. Under these circumstances, these individuals will lack of incentives to acquire any kind of information regarding registration before they turn 18, because whichever the case, they believe registration is not permitted to them. Incentives for information acquisition emerge, in fact, exactly at these individuals’ 18th birthday, when registration is eventually possible according to their beliefs. These individuals could only vote if their birthday is before CD, because then they would still have enough time to register.

The latter explanation, based on information, implies that only citizens to the right of CD, who know the rule, could end up registering. However, this mechanism can be sketched such that information and uncertainty is initially the same for all individuals. Indeed, suppose that every first-time voter is uncertain about the rule being one where 18 years is due at CD, instead of ED. That is, she attaches a probability, or the belief, to the rule being one of these two alternatives. This uncertainty, which is only meaningful to individuals not having yet turned 18, increases the risk of an unprofitable information investment, that is, the risk of having sunk information costs to find that one is not eligible. This risk abruptly vanishes whenever an individual turns the minimum age, and emerges as a discontinuous pattern precisely when such individuals cannot take advantage of this diminished risk: a day after CD. In either case, the information problem is subtle. What changes when crossing the discontinuity is not information itself but the incentives to acquire information.

⁶From the Arizona voter guide in <http://www.azvoterguide.com/voter-registration-faqs/>. We found similar FAQ in registration websites of other 10 states in the US, as well as registration websites in the UK, South Africa and Hong Kong.

EMPIRICAL STRATEGY

In this section we describe the empirical strategy for identification and estimation of the discontinuity illustrated by figure 1.

The Chilean Registration System

During the period studied in this paper, that is, since democracy was re-installed in 1989, and until the registration system was reformed in 2012, electoral participation in Chile required self-initiated registration in the electoral roll. Noticeably, registration was voluntary while voting, conditional on being registered, was compulsory.⁷ Unlike registration procedures in other countries, notably the US, where registration must be carried out by voters in every election, in Chile it sufficed to be registered once in order to participate in any upcoming election. Registered citizens could undo their registration, or move to another electoral district, at the cost of going through a procedure as costly in time and administrative costs as registration itself. Except for the latter cases, registration was therefore once and for all.⁸

Registration in Chile was extremely costly. Citizens had to register in person at the registration office corresponding to the district where the citizen desired voting. There were few registration offices per electoral district, and most of the time they were not located next to other bureaucratic offices, such as municipal offices or local courts, that the public were used visiting. There were also important information and scheduling costs arising from the registration's opening days calendar. Registration offices were open only the first seven working days of every month, except 90 days before CD, when registration could be performed every working day. Few offices and few days for registration imposed even higher costs through congestion.

The Chilean registration system exhibits some particular advantages in the identification

⁷This unusual combination is unique in the world: most countries have either automatic registration with voluntary voting (e.g., Germany, Britain), both voluntary registration and voluntary voting (the United States), or automatic registration and compulsory voting (Belgium).

⁸As a matter of fact, many individuals moved to other counties without updating their registration addresses. A survey in 2013 showed that 11% of the population was “misregistered”; that is, registered in a county other than the one they lived in (*Centro de Estudios Públicos, CEP, survey N ° 69, 2013*).

of the discontinuity at CD. First, the ED-CD is particularly large in the sample, between 3 and 6 months depending on the election, making any strategic political party behavior harder to implement, particularly because citizens' deadline for registration expired well before the deadline for inscription of candidates' candidatures. Second, the former registration system in Chile required the minimum voting age at CD until 1988, changing it, from then on, to one with minimum voting age at ED. This change in the rule makes the information problem more acute, since young first-time voters typically gather information from older citizens. We study all Chilean elections between 1989 and 2009.

Another aspect of the electoral system worth mentioning, specially in the light of the upcoming discussion, is the eligibility rule on the minimum age requirement for registration and participation. This rule experienced a change right before the beginning of the period analyzed in our study. Until 1973, when the Chilean democracy was interrupted with a *coup d'état*, the minimum age was 21 and required at CD.⁹ In 1988, at the end of the dictatorship, when Chileans had to vote in favor or against the continuation in power of General Pinochet, the registration rule remained the same on 18: CD was fixed a month before ED and only citizens that had turned 18 at or before that date could register. Crucially, in all subsequent elections, from 1989 to 2009, eligibility was on having turned 18 before or at ED. This is a key trait of the context in which we carry out our study. Most adults at the beginning of the transition to democracy learnt a rule on eligibility of young first-time voters that did not apply any longer in subsequent elections. Therefore our interpretation of the discontinuity at CD: young first-time voters are more likely to face uncertainty on whether the minimum age requirement is due at CD, or ED instead.

In 2012 a reform to the electoral system made registration automatic and voting voluntary. The main reason to implement the reform was the extremely low turnout rate among young first-time voters.

⁹We found evidence of this in the discussion of the 17,824 Electoral Law in 1970.

Data

We use data on registration from the Chilean Electoral Service (SERVEL). The data contains information of the latest registration of all individuals registered between January 1987 and November 2011. The total number of registered citizens in the electoral roll, as of November the 31st of 2011, is 8,120,440. Information on registration includes birth date, registration date, gender, address, and occupation; the last two at the time registration took place.

We study all elections in Chile in the post authoritarian period, except the first one, held in 1988, where the eligibility rule applied at CD. In table 1 we find all elections considered in our study: five presidential elections, three of them concurrent with parliamentary elections (1993, 2005 and 2009); five municipal elections; and two parliamentary elections. We observe that all presidential and parliamentary elections were held in December, while all municipal elections were held in October, excepting the first municipal election in 1992, which was held in June.

We note from table 1 that the ED-CD gap varies across elections. Depending on the election, CD was up to 180, 120 and 90 days (6, 4, and 3 months, respectively) before ED. The first presidential election exhibits the largest CD. The gap decreased in the following election because at the beginning of the transition to democracy majors were not elected, and the Municipal Law calling for elections on June 28 was only ready on March 19, 1992. Accordingly, CD was postponed to March 25, only 3 months before the election. In subsequent elections, the gap increased to four months, and finally reduced to three months in 2005. This variation enriches our identification and estimation strategy.

In what follows, our study focuses on the number of registrants instead of the registration rate, which is calculated as the fraction of registrants over the population as a whole for the respective cohort. In this study we only had access to the number of births in Chile per day for births that took place since 1988 onwards. Registration rates as reported in 1, therefore,

Table 1: Elections in Chile: 1989-2009.

Year	Election Type	Election Date (ED)	Closing Date (CD)	ED-CD
1989	Presidential	December 14	June 15	182
1992	Municipal	June 28	March 25	95
1993	Presidential	December 11	August 12	121
1996	Municipal	October 27	June 28	121
1997	Parliamentary	December 11	August 12	121
1999	Presidential	December 12	August 13	121
2000	Municipal	October 29	June 28	123
2001	Parliamentary	December 16	August 17	121
2004	Municipal	October 31	July 2	121
2005	Presidential	December 11	September 10	92
2008	Municipal	October 26	July 26	92
2009	Presidential	December 13	September 13	91

can only be obtained for the last two elections: 2008 and 2009. Accordingly, after studying electoral demographic behavior in Chile in the last 20 years, however, we conclude that ignoring the possibility that the composition of birth rates per day are changing significantly around the cutoff is on the safe side. Indeed, the average annual pace at which the population changes is -0.01 , implying a -0.004 percent change per day.¹⁰ The difference in CD dates, as described in table 1, provides a test on the robustness of our findings to any seasonal effects influencing birth rates. Furthermore, our findings remain essentially the same when using registration rates, instead of the number of registrants, as dependent variable for those elections for which this is possible (that is, the 2008 and 2009 elections).¹¹

¹⁰Survey of Population Statistics, from the Chilean National Statistical Service or INE.

¹¹In fact both the coefficient's magnitude and statistical significance are larger than those reported for specifications using the number of registrants. Available upon request.

Identification

The identification strategy we pursue in this paper consists in the implementation of a sharp discontinuity regression (SRD) design (Imbens and Lemieux (2008)). The main assumption in a SRD is that assignment to the treatment is determined by the value of an exogenous forcing variable. In our case the forcing variable is the time at which an individual turns 18, and the cutoff point is the registration closing date t_R .

For every election, we consider only individuals whose registration date is less than one year before the respective ED. For a particular election, described by dates t_E and t_R , we consider the subsample of individuals $i \in \Lambda = \{i | t^E - 1 \text{ year} < t_i^R \leq t^R\}$. For instance, the 2009 Presidential Election was held in December the 13th, while CD was three months before. The sample in this case is composed of all citizens who registered anytime between December the 13th of 2008 and September the 13th of 2009.

To formally describe our variable of interest, we select a bandwidth h and a total number of bins K , and we build a set of $k = 1, \dots, 2K$ bins such that $b_k = t_R - (K - k + 1)h$. These bins just scale and translate time k , from $b_1 = t_R - K \times h$, to $b_{2K} = t_R + (K - 1) \times h$. The selection criterion of h and K are both arbitrary, being our preferred values $h = \text{“1 day”}$ and $K = \text{“90 days”}$.

We are interested in the number of individuals turning 18 within each bin

$$N_k = \sum_{i \in \Lambda} 1[b_k < t_{18}^i \leq b_{k+1}]$$

The variable N_k counts thus the number of registered individuals at each h -window of time (bin).

The simplest selection for a set of bins is $k = 1$, which defines just one h -window on each side of the cutoff t_R . Table 2 displays N_k in that case, for $h = 90, 30$, and 10 days, respectively.

Table 2: Samples close to Discontinuity.

Year	Election Type	h=90 days			h=30 days			h = 10 days		
		N_1	N_2	$(N_1 - N_2)/N_1$	N_1	N_2	$(N_1 - N_2)/N_1$	N_1	N_2	$(N_1 - N_2)/N_1$
1989	Presidential	11304	7725	32%	3529	2580	27%	1168	835	29%
1992	Municipal	13653	3105	77%	3077	1174	62%	822	417	49%
1993	Presidential	17527	12766	27%	5540	4435	20%	1882	1480	21%
1996	Municipal	6401	3769	41%	2077	1325	36%	659	435	34%
1997	Parliamentary	4128	2081	50%	1296	759	41%	434	256	41%
1999	Presidential	5453	3596	34%	1848	1225	34%	638	426	33%
2000	Municipal	2805	1544	45%	926	542	41%	259	179	31%
2001	Parliamentary	3499	1888	46%	1040	644	38%	341	236	31%
2004	Municipal	3509	1843	47%	1104	629	43%	366	226	38%
2005	Presidential	8947	6869	23%	3200	2552	20%	1168	819	30%
2008	Municipal	4084	2328	43%	1361	808	41%	440	285	35%
2009	Presidential	8872	6169	30%	3013	2239	26%	1119	734	34%
Total		90182	53683	40%	28011	18912	32%	9296	6328	32%

The first two columns ($K = 1$, $h = 90$) describe our baseline case. The total number of observations is reduced from 8,120,440 to 143,865, the latter being the number of individuals i satisfying both $i \in \Lambda$ and $t_R - 90 < t_{18}^i \leq t_R + 90$, in any election. The values N_1 and N_2 correspond to the total number of registered citizens on each side of the cutoff. We observe that $N_1 > N_2$, meaning that registration is higher among those turning 18 before CD. The next two columns repeat the count for $h = 30$ and $h = 10$ days, respectively. Again, $N_1 > N_2$ in every election, and although the percentage differences decrease, they hardly converge to zero. In the 10 days case ($h = 10$), the number of registered individuals falls about 30% at the cutoff, an outcome which is quite stable across elections.¹²

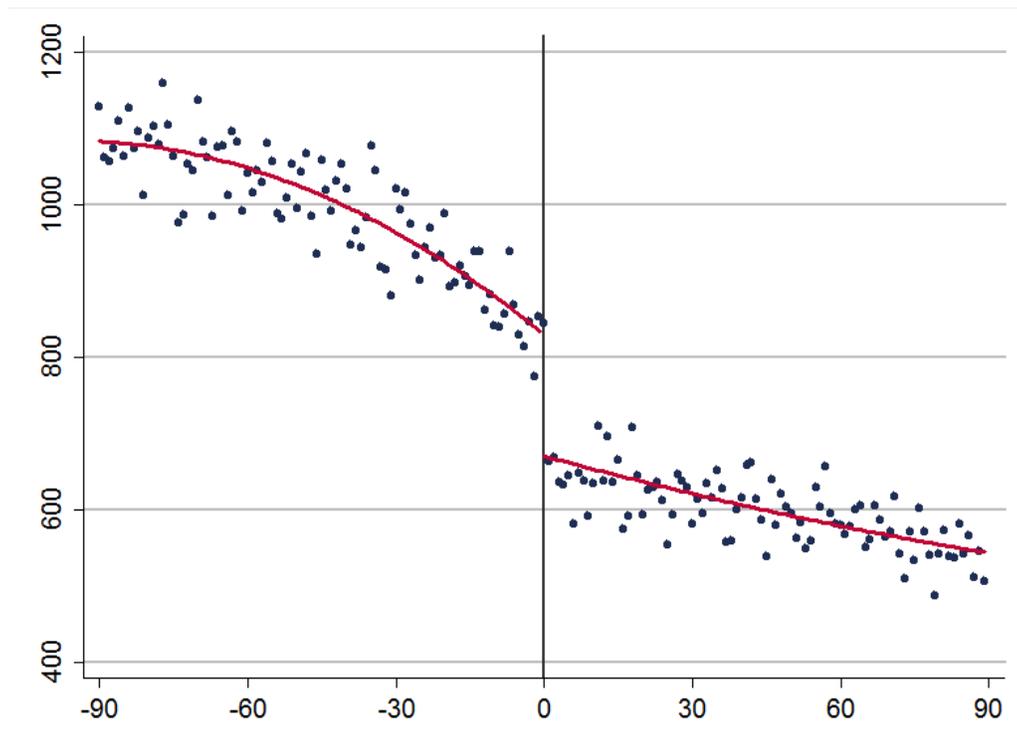
Now we turn to the graphical analysis of the sample and the discontinuity. We consider all elections combined, fixing for each the same h and K in the construction of the bins. Accordingly, the CD for each election is in the middle of the bin sets, allowing us to aggregate the number of registered citizens. We choose $h \times K = 90$ days, since this is the minimum time distance between CD and ED in elections in our sample. Figures 2 and 3 show the number of registered citizens per bin, with $h =$ “1 day” and $h =$ “5 days”, respectively. We include smoothing polynomial fits on each side of the cutoff.

We observe that registration steadily decreases with age; that is, when we move to the right in the figure. This means that age effects on registration are quite strong. If the election were in December, for instance, registration of those turning 18 in March would be 5% higher than those turning 18 in April, according to the figure. Although the downward sloping trend is noticeable on both sides of the discontinuity, it is quadratic to the left and linear to the right.

Most importantly, we observe a stark discontinuity at zero. The number of registered youngsters, according to the quadratic-left trend at the discontinuity (that is, those turning eighteen one day before CD), is 830. The number of registered individuals on the other side of

¹²The only exception is the 1992 Municipal Election, which exhibits a larger drop. As we explained further above, this election followed a very debated reform, and registration time after its approval was about one week long. Hence the awkward pattern observed in this election.

Figure 2: Number of registered, 1 day bandwidth (180 bins).



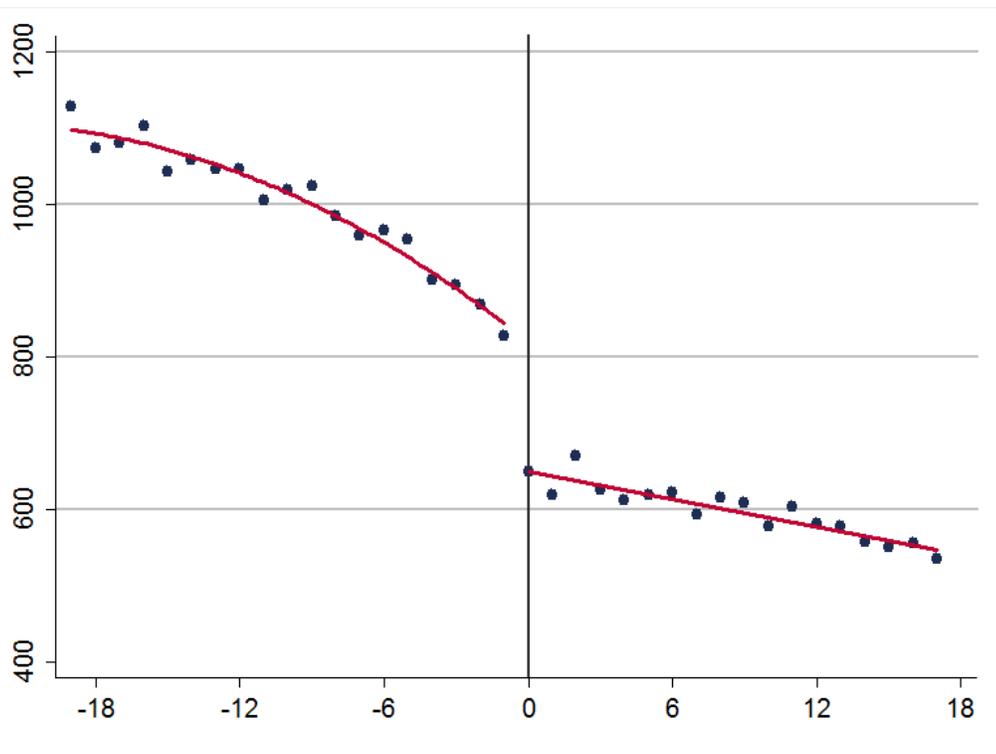
the cutoff, namely those who turn 18 one day after CD, is 670. The fall is thus about 20% at the discontinuity.

Discussion of Identification Assumptions

For the SRD to be valid, two identification assumptions must hold (Imbens and Lemieux (2008)). First, the probability of treatment assignment at the threshold must not be correlated to the outcomes. In our case, the treatment selection is based only on one observable variable, age. Selection is therefore exogenous and trivially uncorrelated with the outcome because individuals cannot manipulate the selection criteria. Consequently, the standard unconfoundness assumption required for identification in a SRD is directly satisfied.

The second assumption is that the conditional distribution function must be continuous, or, alternatively, that no co-variates other than the “forcing variable” are changing at the

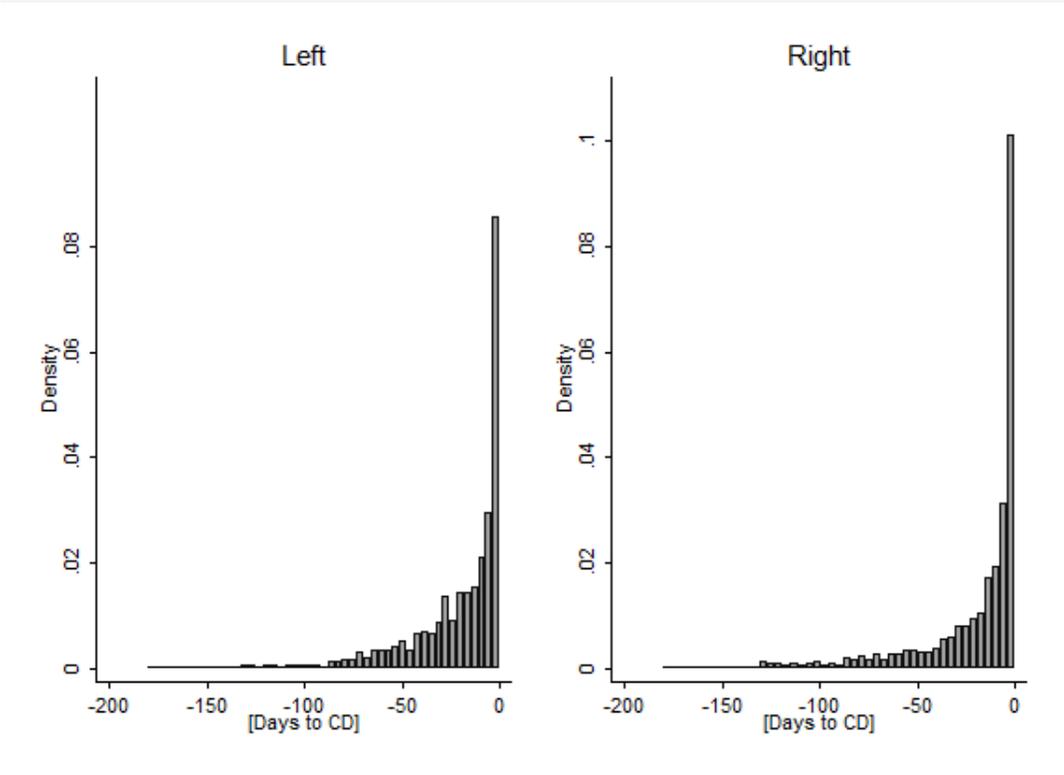
Figure 3: Number of registered, 5 days bandwidth (36 bins).



discontinuity. This assumption is key in supporting the claim that the group on one side of the cut-off allows estimation of the counterfactual for individuals belonging to the group on the other side of it.

Another confounding effect that may be leading the jump on registration at the cutoff is some unobservable registration administrative cost. However, there is no evidence of administrative costs changing at CD. This can be verified in the following figure, where we display the histograms of registration by registration date, both to the left and to the right of the discontinuity. We observe that both histograms are strikingly alike in structure and levels.

Figure 4: Registration date histograms.



As the histograms show, individuals turning 18 before CD do not hurry their registration. On both sides of the discontinuity, individuals are registering close to CD. Indeed, about one third of registrations are performed during the last week’s opening days before CD. This behavior is

consistent with the idea that voting registration is a type of activity having immediate costs and future rewards (see O’Donoghue and Rabin (1999)): an individual incurs in registration costs immediately, while the reward is postponed until ED. Accordingly, all agents have incentives to delay registration.

ESTIMATION AND RESULTS

This section describes the regression discontinuity approach that we use in order to identify the effect of procedural information on registration. It exploits sharp differences in the registration rate at each side of the cutoff at CD.

Figures 2 and 3 exhibit a sharp discontinuity at $t_{18}^i = t_R$. Accordingly, we estimate a piece-wise function for aggregate registration N as a function of t_{18} on both sides of the cutoff. We define the treatment dummy D as

$$D = 1[t_{18} < t_R]$$

To estimate the treatment effect, we run the following pooled regression on both sides of the cutoff point

$$N = \alpha + \gamma D + f(T) + \varepsilon$$

where γ is the treatment effect, and f is a piece-wise function such that $f(T) = f_L(T) + D[f_R(T) - f_L(T)]$, equal to f_L (f_R) at the left (right) of the discontinuity, where $T \equiv t_{18} - t_R$. We allow these functions to differ on both sides of the discontinuity. In practice, both f_L and f_R are modeled as polynomials. We estimate and report a number of specifications to see to what extent the results are sensitive to the order of the polynomial. We assume that errors are clustered by day to account for within day correlation.¹³

¹³This is the tighter and more demanding assumption we can impose on errors. The alternative assumption, robust standard errors, delivers trivial results: the significance is maintained, with t-statistics about ten times higher than the ones obtained with clustered errors.

Impact on Registration

Table 3 displays estimation results for all elections combined. We provide three different specifications: linear, quadratic and polynomial. In the latter case we add all factors up to the polynomial's fourth degree, choosing the specification that optimizes the AIC criterion. We also provide results for each type of election.

The first three columns provide evidence that information costs hinder registration: the discontinuity is negative and statistically significant for every specification. To interpret these figures, the drop in γ must be compared with constant α , which corresponds to the registration level immediately to the left of the cutoff. The last row displays the ratio $\hat{\gamma}$ to $\hat{\alpha}$ ($\hat{\gamma}/\hat{\alpha}$). The drop is about 20% at the discontinuity, meaning that the information effect is causing one of every five individuals turning 18 one day after CD to drop out from registration. This effect is sizable, although the effect on the whole cohort of young first-time voters is narrower.

Consider the aggregate figures from the baseline sample (columns 1 and 2 in table 2). The daily average registration on both sides of the cutoff is on average 1002 and 596 individuals, respectively. The effect at the discontinuity, which is -165 according to column 3 in table 3, should be compared with the sum of individuals on both sides of the cutoff, which is equal to 1598. These figures suggest that the effect on registration of the cohort as a whole is about 10%, which still is a very large effect. These results confirm that information costs have important effects on turnout.

As for the specifications, the information criterion is maximized in the fourth degree polynomial (column 3), with a point estimate slightly lower than the one obtained in the quadratic model (column 2). Furthermore, these two specifications exhibit a similar goodness of fit. Accordingly, in the following estimation results we consider the quadratic specification as our preferred one. All regressions reported below are robust to specifications other than the quadratic one.

Table 3: SRD estimates of the effect of information on registration: all elections combined.

Elections	All	All	All	Presidential	Municipal	Parliamentary
T	-2.76*** (0.17)	-5.24*** (0.58)	-7.28*** (2.67)	-1.59*** (0.57)	-3.11*** (0.35)	-0.46*** (0.14)
T^2		-2.71*** (0.63)	-9.01 (12.64)	-1.04* (0.59)	-1.37*** (0.36)	-0.22 (0.16)
T^3			-6.21 (22.08)			
T^4			-1.53 (12.52)			
D	-214.73*** (11.76)	-184.54*** (14.79)	-165.00*** (21.27)	-91.40*** (13.88)	-74.48*** (8.19)	-20.78*** (4.97)
$D \cdot T$	1.53*** (0.21)	4.57*** (0.76)	5.98 (3.63)	1.52** (0.70)	2.53*** (0.41)	0.41 (0.25)
$D \cdot T^2$		2.08** (0.80)	9.89 (17.28)	0.23 (0.74)	1.68*** (0.43)	0.16 (0.25)
$D \cdot T^3$			5.59 (29.64)			
$D \cdot T^4$			1.06 (16.49)			
Constant	868.98*** (9.27)	830.39*** (10.72)	815.90*** (17.08)	531.31*** (11.60)	233.58*** (6.95)	70.41*** (2.44)
$\hat{\gamma}/\hat{\alpha}$	-0.247	-0.222	-0.202	-0.172	-0.319	-0.295
Obs.	143,865	143,865	143,865	89,228	43,041	11,596
R-squared	0.96	0.97	0.97	0.86	0.96	0.81

In the last 3 columns of table 3 we have estimated the quadratic specification with observations aggregated by election type: Presidential, Municipal and Parliamentary. Again, the effect of information costs is significant for all kinds of elections. The ratio of $\hat{\gamma}$ to $\hat{\alpha}$ is smaller for presidential elections, as we will discuss below.

In table 4, we show estimations of the quadratic model for each of the 12 elections in the sample.

Table 4: SRD estimates of the effect of information o registration by election.

Election	1989	1992	1993	1996	1997	1999	2000	2001	2004	2005	2008	2009
T	-0.50*** (0.19)	-2.51*** (0.24)	-1.03*** (0.24)	-0.13 (0.15)	-0.12 (0.11)	-0.10 (0.15)	-0.14 (0.10)	-0.30*** (0.11)	-0.09 (0.11)	0.17 (0.22)	-0.28** (0.13)	-0.01 (0.28)
T^2	-0.14 (0.20)	-1.06** (0.26)	-0.68** (0.28)	0.05 (0.16)	0.01 (0.11)	-0.09 (0.17)	-0.14 (0.10)	-0.2 (0.12)	0 (0.11)	0.08 (0.25)	-0.28** (0.14)	-0.06 (0.30)
D	-16.51*** (5.35)	-22.64*** (4.89)	-18.88** (7.38)	-17.90*** (3.89)	-14.13*** (2.45)	-14.65*** (4.30)	-10.08*** (2.95)	-6.9 (4.83)	-12.62*** (2.81)	-16.48** (7.41)	-12.35*** (3.12)	-19.36** (8.69)
$D \cdot T$	0.31 (0.25)	2.25*** (0.27)	1.25*** (0.38)	0.03 (0.19)	0.08 (0.13)	0 (0.21)	0.14 (0.12)	0.26 (0.22)	0.03 (0.14)	-0.32 (0.35)	0.18 (0.16)	-0.06 (0.41)
$D \cdot T^2$	0.36 (0.26)	1.15*** (0.28)	0.18 (0.41)	-0.02 (0.21)	-0.02 (0.14)	0.14 (0.22)	0.12 (0.12)	0.18 (0.22)	0.04 (0.14)	-0.26 (0.36)	0.37** (0.18)	-0.2 (0.42)
Cons.	106.35*** (4.04)	67.42*** (4.25)	166.19*** (4.52)	64.66*** (2.77)	40.52*** (2.04)	59.54*** (3.01)	29.57*** (2.44)	31.94*** (2.20)	35.85*** (2.29)	106.99*** (4.00)	42.14*** (2.67)	101.86*** (5.48)
$\hat{\gamma}/\hat{\alpha}$	-0.15524	-0.33581	-0.1136	-0.27683	-0.34872	-0.24605	-0.34089	-0.21603	-0.35202	-0.15403	-0.29307	-0.19006
Obs.	19,029	16,758	30,293	10,170	6,209	9,049	4,349	5,387	5,352	15,816	6,412	15,041
R-sq.	0.77	0.95	0.73	0.70	0.76	0.53	0.56	0.60	0.70	0.38	0.55	0.44

All elections exhibit a negative discontinuity at $t_{18} = t_R$, and only in the Parliamentary election in 2001 this effect is not significant. In all other cases the effect is significant at 1%. Non reported regressions give the same results for other polynomial specifications and other time window lengths around the discontinuity.¹⁴ In all cases we find a negative significant effect.

As table 4 shows, registration is different across elections in at least two respects. First, we note that the salience of the contest matters. The more important elections in Chile are the presidential ones, in particular those concurrent with parliamentary elections. We observe that for these elections the $\hat{\gamma}/\hat{\alpha}$ ratio is always below 20%, with the average being equal to 15,3%. On the contrary, municipal and non concurrent parliamentary elections exhibit an average drop of 30,4%, almost doubling the fall in the most salient type of contest. As expected, the effect of information costs on participation is negatively correlated to the election's saliency.

Additionally, the ED-CD gaps differ across elections, as reported in table 1. The data does not show a clear pattern relating drops at the cutoff to the gap; elections with a gap equal to 3 months exhibit similar drops than elections with 4 months ED-CD gaps.

However, the variance in the ED-CD across elections provides an exercise to check the robustness of our regressions. We study whether discontinuities other than the one observed at closing date, emerge. In order to do so, we split observations into three groups: those belonging to elections in which the distance between ED and CD is of 6 months, 4 months and 3 months, respectively. Then, for each of these groups we test whether there is a discontinuity at 6, 4 and 3 months, respectively, or not. Results are reported in table 5.

For every case we observe that the discontinuity is negative and significant at the election type's respective CD, while not statistically significant in all other cases.

¹⁴We study windows of 60 and 120 days away from CD on each side of the discontinuity, respectively, whenever possible (in 4 elections the distance between CD and ED is less than 120 days).

Table 5: Robustness check.

	180 days		120 days		90 days	
<i>Elections with ED-CD equal to 180 days</i>						
T	-0.958***	-0.749	-0.336*	0.0932	0.0278	-0.159
T^2		0.00677		0.0142		-0.0062
D	-12.70**	-25.80***	10.15**	7.589	5.091	2.685
$D \cdot T$	0.924**	2.935**	0.34	1.02E-05	-0.254	0.554
$D \cdot T^2$		-0.0786*		-0.0172		-0.0139
Observations	6,109	6,109	5,224	5,224	5,423	5,423
R-squared	0.604	0.629	0.113	0.118	0.027	0.035
<i>Elections with ED-CD equal to 120 days</i>						
T	-0.379	1.028	-2.501***	-4.531***	-1.015	4.625**
T^2		0.0469		-0.067		0.186**
D	-1.993	-22.68	-83.36***	-54.71**	3.123	-23.83**
$D \cdot T$	-0.741	0.48	2.128***	0.637	0.953	-4.740*
$D \cdot T^2$		-0.132		0.181*		-0.185**
Observations	29,938	29,938	23,390	23,390	19,262	19,262
R-squared	0.229	0.253	0.892	0.9	0.119	0.249
<i>Elections with ED-CD equal to 90 days</i>						
T	-0.183	-1.29	-0.886	-1.75	-2.207***	0.746
T^2		-0.0367		-0.0286		0.0971
D	-13.24	-22.75	-21.68	-10.7	-65.64***	-93.33***
$D \cdot T$	-0.636	3.268	-0.894	-1.32	1.134	0.674
$D \cdot T^2$		-0.0538		0.0705		-0.178*
Observations	26,945	26,945	22,693	22,693	17,441	17,441
R-squared	0.202	0.22	0.59	0.593	0.837	0.847

THE IMPACT OF INFORMATION COSTS ACROSS DIFFERENT SOCIAL GROUPS

A possible outcome of information hurdles in registration is that citizens are not randomly affected by them. Registration procedures are significant not only because they influence the overall rate at which individuals participate, but also because they may affect participation rates differently across socio-economic groups.

In this section, we check whether citizens registered at either side of the cutoff differ in terms of their socioeconomic status (SES) or not. Income inequality is notoriously high in Chile. Unfortunately, we do not have individual information about SES variables in our data set.¹⁵ As a second-best strategy, we use individual addresses in the data set and impute averages of SES variables observed at the county (*comuna*) level, using information contained in the Chilean socioeconomic Survey CASEN 2012.¹⁶ We are aware that we are dismissing all the within-comuna variation, but Chile has a high level of socioeconomic demographic segregation, noticeable across *comunas*, making our strategy at least plausible.

We define z^i as the SES variable imputed to individual i ; depending on the specification this variable can be either income, schooling, rurality or gender. Income is defined as disposable income per capita, “schooling” as the number of years of education, “rural” is defined as a dummy taking value 1 if the household is rural and zero otherwise; gender, finally, is defined as a dummy equal to 1 when female, zero when male. All these variables are averaged at the *comuna* level and then imputed to all individuals registered in that *comuna*. As gender is also in the individual dataset the imputation strategy is not necessary and we check the results on this single variable from individual analysis.

First, we provide a graphic description of our results. Using the bins described in section 3, we define the average SES variable Z_k across registered citizens as

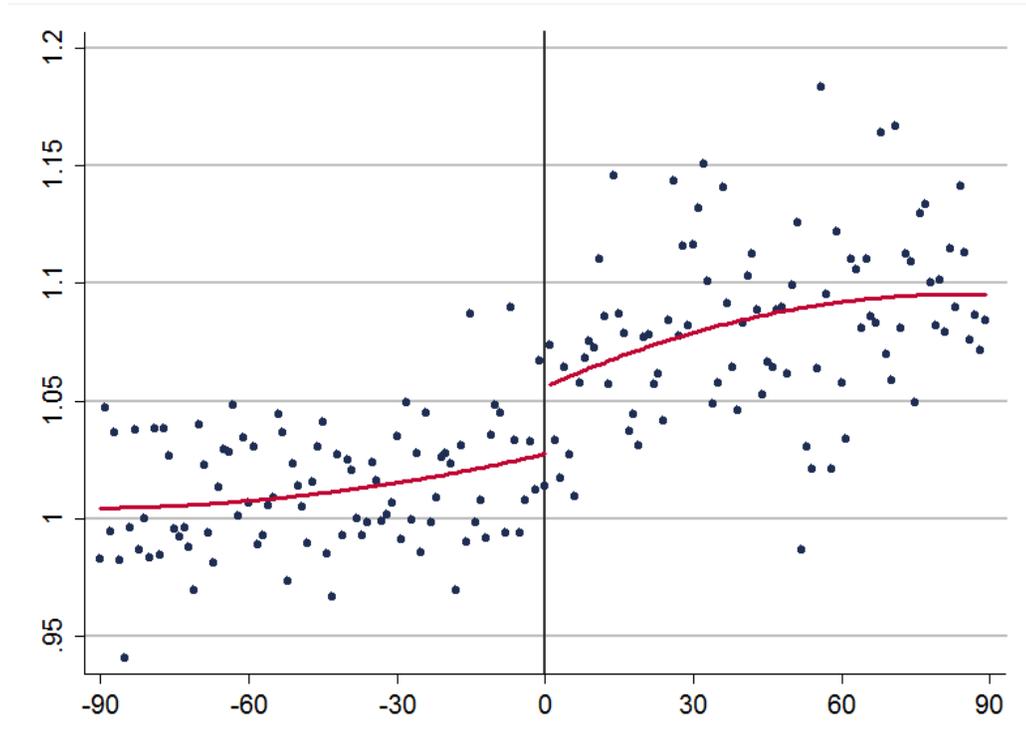
¹⁵A related variable is “occupation” at the time of registration, but almost all young first-time voters were “students” when their registration was performed.

¹⁶We use the latest CASEN because SES variables change very little over time. We tried different survey years, obtaining similar results.

$$Z_k = \frac{1}{N_k} \sum_{i \in \Lambda} z^i \times 1[b_k < t_{18}^i \leq b_{k+1}]$$

Again we consider all elections combined, choosing the same h and T in the construction of the bins. Figures 5 and 6 show Z_k at either side of the discontinuity for income and rurality, using $h = 1$ day. We include smoothing polynomial fits on each side of the cutoff.

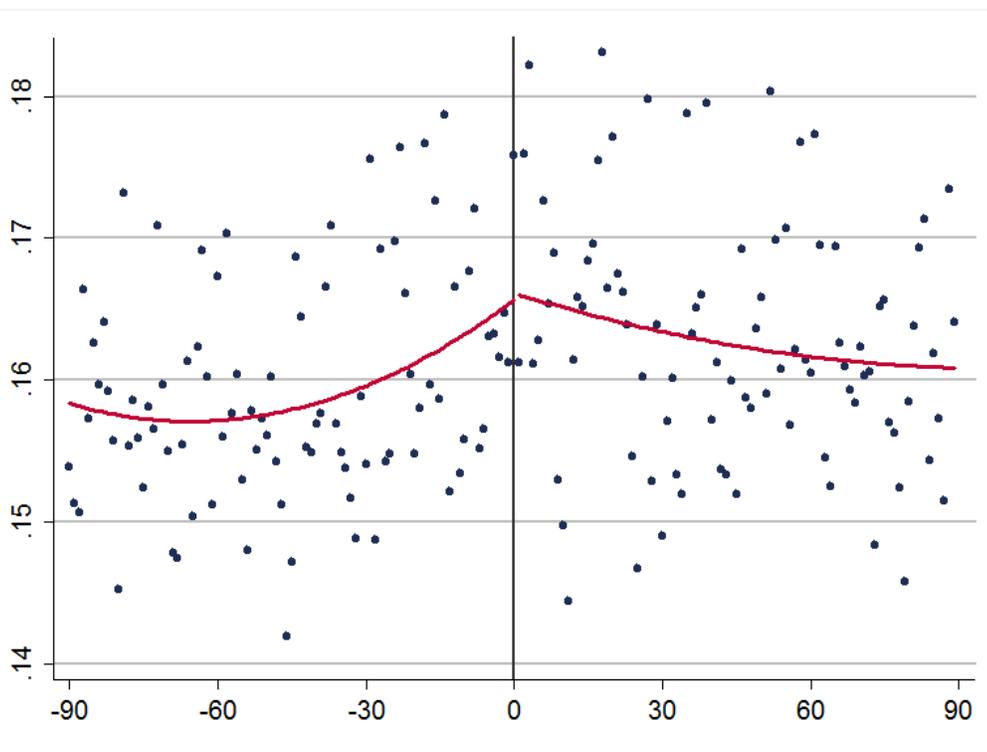
Figure 5: Average Income of registered, 1 day bandwidth (180 bins).



First, we observe that income is significantly higher to the right of the discontinuity, suggesting that young first-time voters affected by uncertainty come from higher income backgrounds, on average, than those not facing this uncertainty (to the left of the discontinuity). Second, as opposed to income, the level of rurality across registered young first-time voters is pretty much the same on both sides of CD. The results suggest that people with higher SES are more likely to acquire procedural information on registration.

We test this hypothesis running the following regression. To estimate the treatment effect,

Figure 6: Average Rurality of registered, 1 day bandwidth (180 bins).



we run the following pooled regression on both sides of the cutoff point

$$Z = \alpha + \gamma D + g(T) + \varepsilon$$

where γ is the new treatment effect, g is a piece-wise function with different functional forms at either side of the cutoff and $T = t_{18} - t_R$. We test a number of specifications, reporting the linear and quadratic specification for each SES dependent variable. We assume errors are clustered per day. Table 6 displays our results.

Table 6: SES and gender composition of registered individuals on both sides of the cutoff.

Variables	Gender		Rurality		Schooling		Income	
T	-0.01*	-0.01	0.08***	0.26**	0.00	0.00	0.00**	0.00
	(0.01)	(0.02)	(0.03)	(0.11)	0.00	(0.01)	0.00	0.00
T^2		0.00		0.20*		0.00		0.00
		(0.02)		(0.11)		(0.01)		0.00
D	0.35	0.52	2.89	0.09	0.52**	0.54*	0.40***	0.27**
	(0.46)	(0.71)	(2.64)	(3.79)	(0.21)	(0.32)	(0.09)	(0.13)
$D \cdot T$	0.01*	0.00	-0.15***	-0.33*	0.01**	0.02	0.00	0.01
	(0.01)	(0.04)	(0.05)	(0.18)	0.00	(0.02)	0.00	(0.01)
$D \cdot T^2$		0.01		-0.20		0.00		-0.01
		(0.04)		(0.19)		(0.02)		(0.01)
Constant	532.74***	532.80***	162.79***	165.66***	104.39***	104.33***	10.24***	10.27***
	(0.28)	(0.43)	(1.65)	(2.21)	(0.13)	(0.18)	(0.06)	(0.09)
Obs.	143,865	143,865	143,865	143,865	143,865	143,865	143,865	143,865
R-squared	0.02	0.02	0.10	0.11	0.34	0.34	0.59	0.59

Table 6 shows two results. First, there are no appreciable differences in the role of information across gender groups, nor when we compare rural groups with urban ones. That information effects are the same across gender groups is an expected result. For rurality, it can be argued, one would expect a major effect on urban citizens given the larger set of information

means available to them. However, it is well documented that rural people vote more (Geys (2006)), and thus the incentives to acquire information may be larger in rural backgrounds.

Secondly, we observe a significant though moderate effect of schooling and income at the discontinuity. Youngsters to the right of the cutoff are significantly more educated and richer than their counterparts to the left of the cutoff. At the cutoff income of the younger registered individuals is approximately 4% higher than income of individuals on the other side of the discontinuity. This difference is about 7% when averaging across groups on either side of the discontinuity.

Overall, our results are consistent with the large strand of the literature suggesting that information affects registration of the poor differently than registration rates of the rich. This asymmetry, however, is quite moderate in our data.

CONCLUSIONS

This article exploits a natural experiment to identify and measure to what extent uncertainty and information about procedural rules affect the electoral participation of young first-time voters. We find robust and sizable effects, although our strategy focuses on the participation of young cohorts only. However, through generational replacement, young voters determine the future turnout profile of the electorate as a whole. Young first-time voting may be decisive in the formation of participation habits that influence electoral behavior throughout an individual's lifetime (Franklin, Mark et al. (2004), Meredith (2009)).

A plausible concern regarding our findings is on their external validity. Several remarks are worth mentioning. Firstly, we are aware that we are studying a particular procedural cost. Naturally, the rule studied here is not analogous to other election rules, but our work encourages the development of identification strategies in order to estimate the effects of procedural information costs in other environments. Secondly, and related to the specific mechanism discussed in this paper, we notice that the ED-CD gap, which is the basis of our natural experiment, is present in several registration systems worldwide. While it is true that identification is easier

to get in the Chilean context, which exhibits an extreme case of early closing date, we do not find evidence of the gap's magnitude having a significant effect on our results. Similarly, we do not observe that the effect identified in the data declines throughout the period, as one would expect if consecutive cohorts had learned more about the minimum age eligibility rule after it changed in 1989. Overall, the singularities observed in the Chilean case are not necessarily causing the large effect that we document here, and then the question about whether such effects are present in other countries cannot be neglected beforehand.

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