

# Assessing the Relationship Between Compulsory Voting and the Over-Representation of Extreme Parties

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## Abstract

In this paper, we address the issue of partisan bias in compulsory voting systems. Given the current global trends – declining trust in mainstream political parties, rising support for left- and right-wing radicals, growing populism and anti-elite sentiment – we seek to determine how they manifest themselves in an environment where citizens are required to vote by law. To answer this question, a quasi-experimental design is proposed. The data show that forced activity does not affect either extreme forces support rates (from a cross-country perspective) or the rationality of their voters (from an individual-level perspective). As far as we know, this is the first attempt to generalize the role of compulsory voting in extreme politics, as well as the first one to refute this role with ample evidence.

## Resumen

En este artículo, abordamos el problema del sesgo de partido en los sistemas de votación obligatoria. Dadas las tendencias mundiales actuales (la disminución de la confianza en los partidos políticos convencionales, el aumento del apoyo a los radicales de izquierda y derecha, el aumento del populismo y el sentimiento anti-elitista), nuestro objetivo es determinar cómo se manifiestan en un entorno en el que los ciudadanos deben votar por ley. Para responder a esta pregunta, ofrecemos un diseño cuasi

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**Correction (April 2024):** Article updated to correct few minor corrections in section Extremeness and (Ir) Rationality of the Forced Vote.



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experimental. Los datos muestran que la actividad coercitiva no afecta el nivel de apoyo de las fuerzas extremas (desde una perspectiva multinacional) ni la racionalidad de sus votantes (desde una perspectiva individual). Por lo que sabemos, este es el primer intento de generalizar el papel del voto obligatorio en la política extrema, así como el primer intento de refutar este papel con suficiente evidencia.

### **Keywords**

Compulsory voting, extreme parties, electoral choice, political representation

### **Palabras clave**

Voto obligatorio, partidos extremos, opción electoral, representación política

## **Introduction**

With the spread of mass democracy, the debate over voting participation quickly turned from an abstract issue of political philosophy to a matter of legislative practice. Today, 27 countries from all over the world resort to compulsory voting<sup>1</sup> (CV), a controversial practice of “incentivized” participation, which implies that attending a polling station is not just a civic duty, but a citizen’s legal obligation. While there is little doubt that such a system is not politically neutral (Bechtel et al., 2016; Jensen and Spoon, 2011; Selb and Lachat, 2009 and others), the question of which actors it benefits the most remains open.

In light of yet another crisis of representation, not to mention the notorious rise of populism, compulsory voting can play a dual role: on the one hand, it expands the electoral field and potentially contributes to the ideological self-determination of voters; on the other, it attracts a large number of disinterested, dissatisfied and socially disadvantaged people, whose current choice is highly susceptible to anti-system (anti-elite, left- or right-wing radical) rhetoric. In addition, CV is usually associated with a wide range of spillovers, the multidirectional influence of which remains a matter of dispute among scientists (Birch, 2016; Dassonneville et al., 2017; Shineman, 2018; Singh, 2022).

This study sharpens the issue of CV electoral effects. Suppose that what some perceive as a more accurate representation is actually a CV-generated bias in aggregate preferences. Then there is a procedural problem – elections mistranslate the public will, which happens, to put it mildly, quite often. But if we consider the literally “extreme” case of this bias, when the surplus of votes (parliamentary seats, ministerial positions, etc.) is retained by the radicals, the procedural problem turns into a political one. To reject this hypothesis – and thus justify CV – we need to show that “mandatory” voting for extreme forces is indistinguishable from “voluntary” one and lends itself to the same rational analysis. Therefore, we set ourselves the task of answering a two-step question: does the CV-system (1) lead to additional support for extreme political forces and (2) if it does, can this support be considered irrational?<sup>2</sup>

Our empirical strategy is as follows. First, we build a series of cross-country models aimed at evaluating CV global effect. Second, we investigate the personal motivation of

voters and analyze post-election polls in Latin America, which has the longest history of compulsory voting. Methodologically, we develop a two-level design combining classical regression with quasi-experimental matching (PSM) and discontinuity (RDD) techniques. At the macro level, it enables us to create a kind of counterfactual out of similar parties in countries with/without CV; at the micro one, the RD approach allows for a deeper analysis of individual rationality, providing us with the means to overcome ecological fallacy.

The structure of the following text is fairly simple. The first section is devoted to the academic discussion that has developed around the topic. We revise the documented effects of high turnout and coercion to it, and formulate hypotheses projecting these effects onto extreme forces. The next two parts contain an empirical development of the question: we start by describing the variables, data, and methods we use, and then turn to the model outputs and their robustness tests. The final part presents concluding remarks in the broader context of academic discussion.

## **Theory and Hypotheses**

### *Changing Beneficiaries of High Turnout*

Perhaps the only unambiguously recognized effect of CV is an increase in turnout. Although researchers argue about its absolute value (Blais, 2006; Franklin, 1999; Hirczy, 1994), there is no doubt that a large-scale influx of voters changes the alignment of political forces. After the introduction of compulsory voting, the party market experiences an external shock, and subsequently establishes a qualitatively different equilibrium. In any given election year, these changes have their beneficiaries and victims, even if in the long run new votes are “smeared” across political camps (Hansford and Gomez, 2010).

Many studies have been conducted on how the increased turnout, mediated by the natural demand for equality among lower status citizens, favors left-wing parties (see Lutz and Marsh, 2007 and the review therein). Conversely, low participation was considered an important reason for their failures: insufficient education and lack of motivation made their electorate more susceptible to information asymmetry, less aware of the procedure and thus bearing relatively high costs from coming to the polls. A far-reaching conclusion of these works was the fact that, knowing the reasons why citizens abstain from voting, one can model a marginal increase in different (then leftist) parties’ support depending on the additional turnout. This picture, representing a special case of a more general problem,<sup>3</sup> is increasingly criticized today – mainly because the very question formulation is somewhat incorrect, while the political distance along the spectrum has weakened, which makes the expected preferences of non-voters less evident (see the discussion of mixing effects in Grofman et al., 1999). In what follows, we present several arguments showing that the dynamics of these preferences play into the hands of radical forces.

First, despite the fact that the root causes of absenteeism have not undergone significant changes, the competition for this “sleeping” electorate has intensified over the years. Social Democrats gradually entered the political mainstream, and a variety of

non-systemic forces began to attract disenchanted citizens. Nationalists and separatists have substantially expanded their electoral base, appealing, among others, to citizens who have never participated in elections before (Ansolabehere and Puy, 2016; Mudde, 2016). A similar expansion is being carried out by the extreme left, who vehemently oppose “revisionism” and “neoliberalization” of the socialist movement (March and Rommerskirchen, 2011). As a result, we see an increasing pressure from the ideological supply side, with parties of all sorts of left and right luring a disoriented consumer (Guiso et al., 2017). The parties with the most resonant agenda, for obvious reasons, have some head start in this race.

Second, polls around the world show declining trust in “old political solutions” (McCoy et al., 2018). The number of people who sympathize with the new movements and their charismatic leaders is growing rapidly (needless to say, these leaders often profess views far from the center). However since novelty and being non-systemic do not imply adherence to strict policy positions, it becomes easier for candidates to manipulate with populist rhetoric and expressive voting (Jennings, 2011). This problem is best illustrated by Latin American countries, which are at the same time developing economies, highly politically polarized societies, and the main implementers of compulsory voting (for more, see Maldonado, 2015). Over the past half century, the continent has experienced several left- and right-wing waves, and the variety of ruling regimes ranging from quasi-communist to ultra-liberal. Its swings from one extreme to another continue to this day, with, for instance, the openly far-right Jair Bolsonaro and the Workers’ Party Fernando Haddad and Lula da Silva competing for the Brazilian presidency.

Third and last, there may be a more subtle way of creeping radicalization. As Lutz and Marsh (2007) argue, numerous turnout side effects are so contradictory, that “any bias in election outcomes is typically rather small and is not in a specific direction” (p. 539). The interchange between the “core” (voters with a high degree of political consciousness and civic responsibility) and an approximately equal-sized “swamp” occurs from election to election, which makes their results directly dependent on the mobilization capacity of parties and candidates (Da Silva, 2018). Parties, in turn, can become institutionally more active due to CV, even if the system has no direct effect on their electorate (Held, 2023). Fringe parties are encouraged to further radicalize their rhetoric, increasing their distance from the “mainstream” (Singh, 2021). Another possible negative incentive is voter bribery; when votes are relatively cheap, parties naturally tend to create clienteles, binding entire communities to themselves (Gans-Morse et al., 2014). Given that repeated turnout strengthens party attachment (Huber et al., 2005; Singh and Thornton, 2013), we no longer face a pure representation problem, but a combination of negative selection and path dependence. If the first acquaintance with the party system resulted in the choice of an extreme one, in the future it can be maintained due to inertia, without additional biases.

Of course, our assumptions are not mutually exclusive, but complement each other. The long-noted demand for “non-mainstream” political groups is reinforced by their increased supply, as well as the peculiarities of electoral systems, in some cases provoking the transformation of sympathy into relatively stable support. This line of reasoning is

not perfect, and can be criticized, for one, from the perspective of the falling number of radical parties. But above all, it makes sense when applied to an open market, where freedom of participation implies freedom to not participate. This is clearly not the case in compulsory voting systems.

### *Extremeness and (Ir)Rationality of the Forced Vote*

So, the political market can experience serious failures due to excessive activity. There are at least three channels through which the turnout of usually non-voting people can bolster the popularity of extreme forces. But what happens if this turnout is of a forced nature? To what extent does the irritation and apathy, common among coerced voters, translate into support for the radicals?

CV is known for its ability to both smooth out and reinforce pre-existing electoral biases. Thus, after the introduction of penalties for non-voting, experts usually record a weakening of class and educational disparities, and an increase in age and territorial ones (Barnes and Rangel, 2018; Cepaluni and Hidalgo, 2016; Contreras et al., 2016; Power, 2009). Arguments about positive educational impact (e.g., Bruce and Costa Lima, 2019; Elliott, 2017; Sheppard, 2015) often collide with counter-arguments about the cost of this impact. For example, Miles and Mullinix (2019) record an increased level of anger among CV-obligated respondents, Singh and Roy (2018) – weak information seeking, and several recent studies in a row – a higher proportion of random votes and spoiled ballots (Freire and Turgeon, 2020; Katz and Levin, 2018; Singh, 2019b). All these spillovers may well result in a “no” vote, when a person does not know whom he wants to support, but knows exactly whom he wants to punish.

The number of anti-system or anti-incumbent ballots may also increase for a less prosaic reason. A kind of mainstream in CV studies argues that encouraging poorly interested citizens to vote makes aggregated choices less accurate (Dassonneville et al., 2019; Hooghe and Stiers 2017; Singh, 2016). Multiplying this with the system’s propensity to promote anti-democratic sentiment (Singh, 2018) and ideologically diverse small parties (Miller and Dassonneville, 2016; Singh, 2019a), we get an increased likelihood of voting for extreme candidates on an “occasional” basis. In practice, it can manifest itself in a higher sensitivity of new voters to advertising and catchy slogans, which populists have done so well.

From all that we have said, one can conclude that there is a welfare-inferior equilibrium, in which compulsory voting stimulates massive surplus participation that rewards specific political entities. The main issue, however, boils down to the massiveness of this participation. Indeed, if the total electorate is large enough and the cost of participation is correspondingly low, both systems lead to an optimal election result (see the mathematical rationale in Krishna and Morgan, 2012), but in situations where voters are divided, entry barriers for parties are low and the cost of acquiring information for those forced to vote is a priori high, outcomes may over- or under-represent certain forces. We proceed from the most unfavorable scenario, in which the broad appeal of extreme candidates makes them the most likely recipients of both deliberately protest vote and “erroneous” (inaccurate) voting introduced by the compulsory system. If this

is the case, changes in the response variable (percentage support for extreme forces/indicator of its quality) are provably related to the application of mandatory voting. Or, in terms of hypotheses:

- $H_1$ : Countries that practice compulsory voting tend to show a higher percentage of votes for extreme parties than those that do not.
  - $H_{11}$ : The stricter the law on CV is enforced, the higher the support for extreme political forces is.
- $H_2$ : Compulsory voting negatively affects the quality of the choice of those supporting extreme parties.

Note that we do not presume any one-to-one linear relationship between the severity of the law, extreme parties' support, and inaccuracy. Rather, we hypothesize that the negative effects of CV are more pronounced when a strict law is in place. Regarding extremity, we rely on its general definition as distance from the political center. If rationality is understood as the ability to identify the political party closest to oneself, it is convenient to assess this proximity on the "left-right" scale. So, we implicitly assume an ideologically close vote to be rational, realizing that this is just one way to approach the voters' calculations.

## Data and Methods

### *Cross Country Perspective*

The data we use is relatively new to CV research: the final base consists of 18 indicators over a period of nearly 45 years (descriptive statistics can be found in Appendix). To the best of our knowledge, no attempt has yet been made to approach such a wide range of characteristics, not least due to the novelty of the main dataset used (V-party). With the exception of election-specific response variable, each indicator is a potential predictor of the extreme parties' popularity in the  $i$ -th country in the  $t$ -th year from 1976 to 2019. However, the unit of analysis is party-year rather than country-year, since the data is structured around the parties' vote shares.

The key dependent variable is the proportion of votes won by a particular extreme party in legislative elections (%).<sup>4</sup> This information is fully provided by the V-party project (*v2pavote* in Lührmann et al., 2019). We operationalize extreme parties the following way: considering only electoral democracies (Polity combined score  $> 0$ ), we select from competing parties those most often characterized as illiberal or populist. By "the most" we mean the maximum values of the corresponding indices (*v2xpa\_illiberal* and *v2xpa\_popul* in Lührmann et al., 2019)<sup>5</sup> and the range within one standard deviation from them. In what follows, we mostly use the combined variable – i.e., consider both populist and illiberal parties to be extreme, even if some of them do not score a maximum on both parameters; in basic tests, where it is important to check for meaningful differences, they are used separately. To ensure the sustainability of the results, we also use more stringent "democratic criteria", testing the model on samples with higher Polity scores.

The effect we should observe under the first hypothesis is related to the categorical variable “CV strictness”. It reflects the extent to which a given country applies compulsory voting, including:

- no implementation (voting is voluntary),
- no sanctions or sanctions that are not enforced,
- sanctions that impose minimal costs upon the offending voter,
- sanctions that impose considerable costs upon the offending voter.

Severe sanctions can include heavy fines, threats of imprisonment, and removal from the voter register – i.e., disenfranchisement. For tentative analysis, which unveils the very relationship between CV and voting results, a binarized version of this variable is used. All the relevant information is taken from the V-dem database (*v2elcomvot* in Coppedge et al., 2021).

As independent variables, we choose aggregate factors that are proven to predict the success of extreme forces (Arzheimer, 2018; Jetten and Mols, 2021; March and Rommerskirchen, 2011, 2015). In essence, however, they should be interpreted as control ones: we assume that the distribution of CV – the assignment of mandatory voting “treatment” – across countries is relatively random, so we want to make sure that there are no serious disparities between them. We seek to allow for comparability in terms of political institutions (electoral system, effective number of parties, public involvement in decision-making), social cleavages (tertiary school enrollment, media coverage bias, ideological polarization, religious, ethnic and spatial conflicts) and economic development (equality in the allocation of resources, GDP and its dynamics per capita, growth in income per capita).<sup>6</sup> In addition, we intend to consider the impact of voter participation, both in absolute numbers and relative to past elections (IDEA, n.d.). By format, most variables are either an ordinal index-type score or some measurable continuous quantity.

In total, the data covers 87 countries, in the “cleaned” sample there are slightly fewer of them (72). By cleaning, we mean the removal of influential observations<sup>7</sup> and, most importantly, observations with missing values. We do not resort to imputing them due to the likely sensitivity of the results. If we use one of the methods of filling cells through regression predictions, it turns out that the original variation between countries is altered, and we formally equalize the actually measured indicators with fitted values. At the same time, reducing the sample does not reverse the initial interpretation. As a result, we start working with about 600 observations, but then their number is gradually reduced to around 200.

By itself, the structure of our data hints at building a panel regression model, where the electoral outcome of extreme forces would be the response, CV application – the main regressor, and the remaining country characteristics considered as controls. However, the calculation of the least squares is problematic here, since either the issue of parameter collinearity or endogeneity due to their neglect invariably arises. Moreover, given the apparent imbalance in CV “assignment” and the high variance in the distribution of

vote shares, we would like to find a more robust method for comparing extreme parties' results. We start with tests of the “difference-in-means” type, which also perform the function of checking the covariate balance. We split the country sample by CV application and examine differences in the variables of interest. This is done using a joint F-test (ANOVA procedure).

Having identified a number of varying factors  $X_n$ , we estimate a conditional logistic model to quantify the propensity of each observation to be affected by “treatment” (that is, to operate under compulsory voting):

$$P(CV_{it} = 1 | X_{it1}, \dots, X_{itm}) = \text{logit}^{-1}(\alpha_i + \beta_1 X_{it1} + \dots + \beta_n X_{itm} + \delta_{j,t}),$$

where  $i$ ,  $t$ , and  $j$  denote countries, time periods, and macroregions, respectively. We use a conditional model instead of a conventional logit to further account for temporal variation: the last term in the main equation reflects macroregion-year fixed effects. Within-group estimates are obtained using standard likelihood maximization.

Then, based on this probability, a genetic search algorithm is implemented. We resort to one-to-one propensity score matching to create a kind of counterfactual from countries where CV is not used, but which are as close as possible to CV users. While PSM may – and in our case, will – be specification-sensitive, we argue that it makes the original samples more comparable to each other (see section 3.1 below). Now, the matched data can be used to perform an ordinary LS regression<sup>8</sup>:

$$\text{VoteShare}_{it} = \beta_0 + \beta_1 CV\_strictness_{it} + \beta_2 Electoral\_system_{it} + \beta_3 Latin\_America_i + \varepsilon_{it},$$

where  $CV\_strictness$  denotes a four-level variable of CV application (from non-use to use with severe avoidance penalties),  $Electoral\_system$  reflects the corresponding nominal variable (including proportional, majoritarian, mixed and others), and  $Latin\_America$  is a continent-specific dummy. As already noted, the countries of this region have the longest historical experience of compulsory voting, which usually makes them a separate object of study (see Carreras (2016), Singh (2018, 2019a, 2019b), etc.). Thus, the  $\beta_1$  coefficient will be the desired estimate of CV global impact, while the  $\beta_3$  will justify the use of Latin American data for further modeling.

### *Individual Level Perspective*

Having confirmed the empirical value of the LA case study, we shift the focus of the work to electoral sociology. For this, it is desirable that the data include at least two countries, temporally centered on the period of populist rise. Like many of our predecessors, we opt for the comprehensive survey organized by CSES – the Comparative Study of Electoral Systems (CSES, n.d.). We combine information from the first four (1996–2016) and most recent (2016–2021) waves, concentrating on countries that used CV at the time of the survey.

This setting allows us to address the voters of Argentina and Brazil. The processed dataset consists of 865 observations – these are citizens whose candidate in the last elections is considered “extreme”. We leave the definition of extremeness to the regional

experts who rated the respondents' choice on an 11-point left-to-right scale (see variables *imd3100\_lr\_cses* and *e3100\_lr\_cses* in CSES, n.d.). Our previous approach, which also accounts for the deviations of such estimates, is not needed here: the scatter of expert opinions is exactly the two categories by which we define political radicals. In other words, we simply consider respondents to be extreme if their choice falls in the intervals 0–2 and 8–10 on the corresponding scale.

Our research interest is directly related to these estimates. To be precise, we are interested in the difference between some conventional positioning of a political force and the self-positioning of its supporter. The absolute value of this difference, expressed as respondents' self-identification<sup>9</sup> minus the above-mentioned expert assessment, is the resulting variable in all subsequent models. Substantially, it reflects personal (in)accuracy in voting, provided that political preferences can be formulated in terms of left and right. We return to this limitation later.

On the other side of the equation, we want to see a variable indicating whether a person is required to vote or not. The laws governing compulsory voting in Latin America make it relatively easy for us to separate the two groups: we know that voting is voluntary for eligible citizens between 16 and 18, and for those over 70 y.o. Thus, the age variable becomes a natural proxy for "treatment" – an individual's exposure to CV law. With a sufficiently large sample we have, the remaining factors can be considered insignificant. There are, however, at least two variables that cannot be randomized due to their higher order: one's belonging to a country and a specific age group. Their contribution is evaluated separately.

Once the key variables are defined, the identification strategy becomes straightforward. Our task is to estimate the magnitude of the accuracy gap generated by the obligation to vote at 18 and its termination at 70. To do this, we first build a general linear model:

$$Accuracy_i = \beta_0 + \beta_1 Age_i + \beta_2 Treatment_i + \varepsilon_i,$$

in which individuals 18+ and 70- are considered a kind of "test group", belonging to which is reflected in the "Treatment" dummy variable.

We then add more flexibility to the model by allowing the conditional slope to vary between age subsamples. This is equivalent to adding an interaction term to the specification, which now looks like

$$Accuracy_i = \beta_0 + \beta_1 Age_i + \beta_2 Treatment_i + \beta_3 Age_i Treatment_i + \varepsilon_i$$

From estimating a simple gap coefficient, we next move on to a more complex RDD modeling. We use the age group variable to separately estimate the difference in two cutoffs: between 17 and 18 and between 69 and 70. The assignment rule is deterministic, which hints at a sharp design, but we lack full data on respondents' birth dates; for this reason, we examine several bandwidths, including 1, 2, and 4 years. Formally speaking, we build four models with local linear and quadratic regressions on either side of each cutpoint to get an estimate of the discontinuity. As cross-country variation in CV stringency has disappeared, we do not include exogenous covariates further,<sup>10</sup> and structurally the equations are similar to those given above.

Finally, let us consider the issue of sustainability of these models' results. With such a focused sample and several specifications, we hope to make a case for causal inference, at least within South America. In addition, robust standard errors (HC3) are used throughout discontinuity modeling. We realize, however, that the key claim to be made here is rather conceptual and related to irrationality, which we approximate through left-right positioning. CSES questionnaires typically use other political dimensions in countries for which the left-right split is irrelevant, but in our case, there are none of them. In Brazil, the level of familiarity with this scale is slightly below average, but this does not actually affect the variation in estimates. On the other hand, the (ir)rationality of "extreme" voters may be accompanied by the (ir)rationality of voters in general, and then the grounds for identifying this electoral group become vague. To separate the effects, we replicate four discontinuity models on a total of 6220 observations, i.e., on all respondents, regardless of their candidate. If the detected trends diverge, we can state the specificity of the far-left and far-right; if not, there is some common causality.

## Results

### *Cross Country Perspective*

We start presenting results with second-level models. Our hypotheses suggest that, except for a very limited number of factors, countries with and without CV have no systematic sociopolitical differences. In other words, we implicitly assume that the "assignment" of CV is quasi-random. Table 1 illustrates the results of formal tests that allow us to verify this assumption.

Preliminarily, we can confirm at least part of our speculations. Omitting non-significant differences for illiberal parties, we see that countries with compulsory voting demonstrate at least a 5% absolute increase in support for populist ones ( $F = 8.75$ ,  $p = 0.01$ ). Other parameters differ slightly, that is, they have little influence in this matter. We note, however, a much higher level of ethno-religious divisions and per capita GDP in such societies. The first, as well as deeper social inequality (*Resources distribution*), is quite understandable here: a significant proportion of CV users is occupied by developing countries, which are characterized by relatively high domestic polarization. The second, most likely, can be explained by the heterogeneous composition of CV non-users. Among them are both the European industrial states and young democracies of the Afro-Asian region, whose economic development has little to do with the specifics of voting.

Knowing this, we try to further balance the two groups by matching party-countries on observed differences. We calculate propensity scores based on turnout, ethno-religious conflicts, and logarithm of GDP per capita (these are X's in the corresponding logit model). The remaining factors are considered randomized. The result of implementing such a procedure is presented in Table 2.

Now, the dependent variable is the electoral outcome of illiberal and populist parties, the "treatment" one is the use of CV. The estimate of the ATT is 4.41 ( $SE = 2.11$ ,  $p = 0.036$ ),

**Table 1.** Summary Statistics.

CV (binary)	0			1			Test
	N	Mean	SD	N	Mean	SD	
Illiberal parties' vote share	272	24.728	18.918	52	22.985	17.9	F = 0.377
Populist parties' vote share	201	15.274	10.762	74	20.126	15.061	F = 8.75***
Eff N of parties	474	4.986	2.786	121	4.877	2.693	F = 0.15
Engaged society	480	1.299	0.888	121	1.386	0.938	F = 0.915
Tertiary education	201	30.414	24.013	74	34.687	20.825	F = 1.835
Media perspectives	480	1.384	0.948	121	1.507	0.912	F = 1.644
Political polarization	480	-0.028	1.309	121	0.124	1.273	F = 1.322
Religious & ethnic tensions	387	0.605	0.218	108	0.671	0.203	F = 7.969***
Urban-Rural divide	480	0.307	0.227	121	0.279	0.21	F = 1.548
Resources distribution	480	0.692	0.24	121	0.649	0.287	F = 2.796*
log GDP per capita	437	9.131	1.084	107	9.5	0.668	F = 11.332***
GDP per capita growth	437	0.028	0.048	107	0.024	0.037	F = 0.567
Income per capita growth	403	2.939	8.191	90	1.701	3.898	F = 1.954
Turnout (absolute)	480	0.652	0.129	121	0.762	0.117	F = 71.941***
Turnout (relative)	461	-0.01	0.104	121	0.002	0.088	F = 1.326

Statistical significance markers: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . Note: Inferential test is one-way ANOVA. Source: Own Elaboration Based on V-Party, V-dem, M. Gallagher Election Indices, Global State of Democracy Indices, World Bank, and IDEA (Hereinafter Referred to as Combined Dataset).

**Table 2.** Propensity Score Matching Summary.

Estimate.....	4.4083
SE.....	2.1084
T-stat.....	2.0909
p.val.....	0.036538
Original number of observations.....	417
Original number of treated obs.....	96
Matched number of observations.....	96
Matched number of observations (unweighted).....	96

Note: Estimate is the Average Treatment for the Treated (ATT). SE are Abadie-Imbens standard errors. Source: Own Elaboration Based on Combined Dataset.

implying that under CV, extreme parties gain an average of 4.4 p.p. more than their counterparts in voluntary systems.

This approximation of causal inference shows that the effect, although formally significant, is in fact rather modest. Moreover, given the cross-country variation, our matching model can potentially be subject to unobserved confounder bias – sensitivity analysis indicates that a 10 p.p. change in the probability of “receiving” CV is enough to shake our

**Table 3.** Results from Multiple OLS Regression.

	Estimate	Std. Error	t value	Pr(> t )
(Intercept)	15.2198	3.0505	4.989	1.39e-06***
CV (no or weak sanctions)	2.1743	2.4438	0.890	0.375
CV (minimal sanctions)	-0.2016	3.0252	-0.067	0.947
CV (strict sanctions)	-21.0887	14.6598	-1.439	0.152
ES (majoritarian)	1.7175	4.8318	0.355	0.723
ES (proportional)	-1.2717	2.9546	-0.430	0.667
Latin American	11.9405	2.2054	5.414	1.89e-07***

Residual standard error: 14.49 on 185 degrees of freedom

Multiple R-squared: 0.1572, Adjusted R-squared: 0.1299

F-statistic: 5.751 on 6 and 185 DF, p-value: 1.649e-05

Signif. codes: 0 '\*\*\*' 0.001 '\*\*' 0.01 '\*' 0.05 '.' 0.1 ' ' 1. Note: The dependent variable is the percentage support for extreme forces in parliamentary elections. Observations are matched by propensity scores.

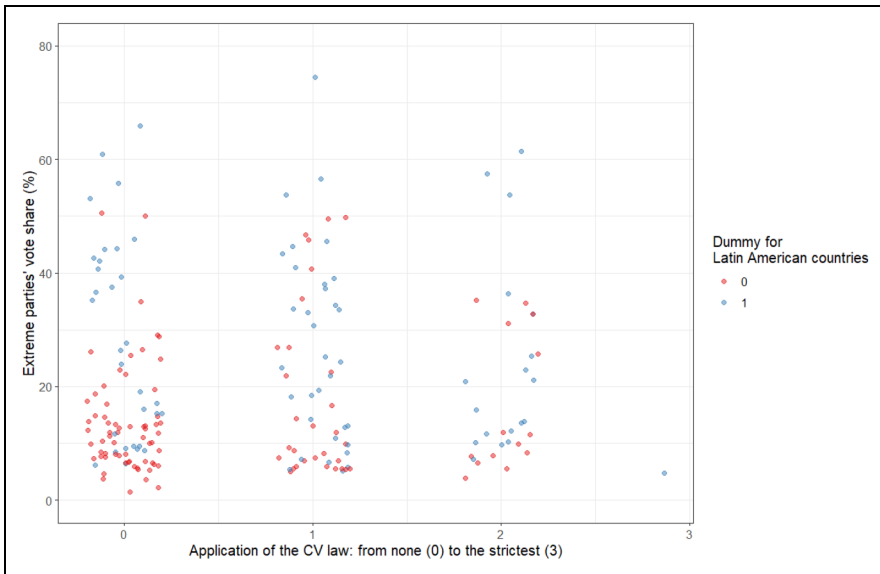
Source: Own Elaboration Based on Combined Dataset.

confidence in the results. However, creating such a counterfactual is still a surer way than regressing parties' performance on the total sample. This is confirmed by post-matching analysis of covariates, which demonstrate much better comparability of the "control" and "test" groups.<sup>11</sup>

That being said, we then use matched data to obtain regression estimates. With the same outcome, the key predictor here is the application-stratified CV. The electoral system (ES) is also included in the equation since our initial tests could not account for its nominal nature. Estimation results can be found in Table 3.

Thus, our last-step model does not establish any provable link between the implementation of CV and extreme parties' electoral outcomes. The limited effect we recorded above turns out to be insufficient to overcome the threshold of statistical significance. Functionally, the linkages found can be depicted on a complex jitter plot (Figure 1).

Two important conclusions can be drawn from this graph. First, it is noticeable that the presumed relationship is weakly expressed and rather non-linear. Second, the only unequivocally significant predictor of the extreme forces' success is their belonging to the Latin American continent. But before proceeding to its detailed consideration, let us say a few words about the robustness of the models built. To make sure of it, we carry out all the same procedures for the "more democratic" subsamples – for the countries with combined Polity score above 3 and 6, respectively. Theoretically, this brings the observations even closer, since meeting these criteria implies a higher level of internal competition, which, as indicated in the previous section, can correct for a wide variety of electoral biases. A summary of such replication is provided in the Appendix, it confirms our baseline results and allows us to go further.



**Figure 1.** Jitter Plot on the Resulting Data. Note: although the only remaining observation of the “third” category (Peru 2001) is of no meaningful value, it is not a statistical outlier, so we do not remove it from the overall graph. Re-estimation of the model without this category does not influence the results.

Source: Own Elaboration Based on V-party and V-dem.

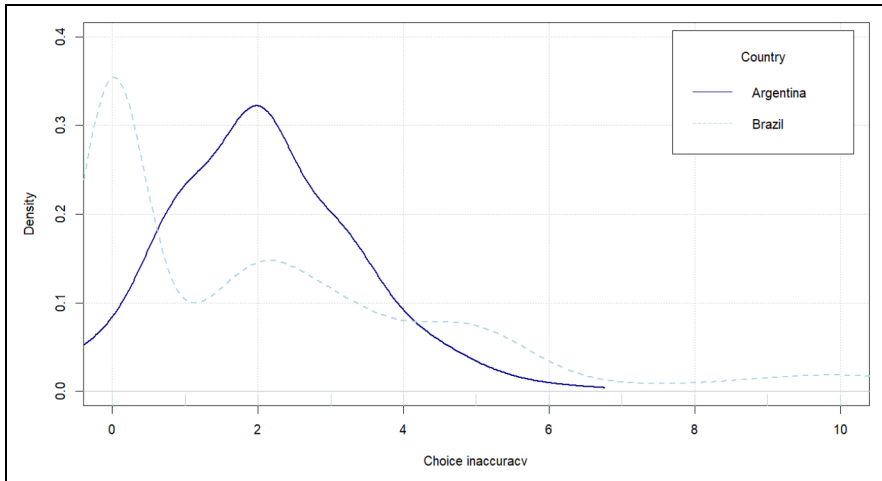
### *Individual Level Perspective*

In fact, the results of lower-level modeling do not favor our hypotheses either. To illustrate this, we need to begin with the outcome variable. Recall that in this regard we rely on an artificial indicator calculated as difference in grades – its distribution is not known in advance and is also subject to estimation. Figure 2 shows its multihump density smoothed via Gaussian kernel.

Several interesting patterns can be deduced from here, but we would like to emphasize two of them: the positive skew and the thickness of the right tail. Condensation on the left side of the graph means nothing more than a fairly high rationality of extreme voters – most of them are not mistaken or slightly mistaken in choosing the closest candidate. The heavy right tail, especially noticeable in the Brazilian subsample, probably refers to the previous part of the study: minor groups of inaccurate voters may be an echo of a negative effect for which we did not find solid confirmation. Alternatively, this may be a consequence of Brazilians being less ideologically divided.

Some clarity is provided by RDD-type models. At first glance, one can only conclude that there is some starting level of inaccuracy among voters (Table 4, model 1). Personal choice turns out to be unaffected by the interaction of predictors, not to mention the age

variable alone (Table 4, model 2). In sum, belonging to the test group has an overly dispersed effect with large standard errors – the corresponding coefficient is relatively small and cannot be interpreted.



**Figure 2.** Kernel Density Estimate of the Dependent Variable. Note: the bandwidth is calculated using Silverman’s rule of thumb.

Source: Own Elaboration Based on CSES.

**Table 4.** Output of Simple Regression Discontinuity Models.

	<i>Dependent variable:</i>	
	Choice inaccuracy	
	(1)	(2)
Age	-0.007 (0.006)	0.020 (0.024)
Treatment	-0.110 (0.530)	1.721 (1.684)
(Age):(Treatment)		-0.028 (0.025)
(Intercept)	2.704 *** (0.641)	0.948 (1.662)
Residual Std. Error	2.638 (df = 862)	2.637 (df = 861)
F-Statistic	0.640	0.864

Statistical significance markers: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Note: The gap coefficient is estimated through the “Treatment” dummy. Source: Own Elaboration Based on CSES.

Threshold-specific regressions reveal exactly the same picture. The estimates in Tables 5 and 6 are average treatment effects for 1-, 2- and 4-years bandwidths. Note that these coefficients are at best of marginal significance, while most of the values obtained have a high probability of being purely coincidental. It is indicative that zero effect persists for all

**Table 5.** Estimates of Local Linear Regressions for Different Bandwidths.

	(1) Linear 17–18			(2) Linear 69–70		
	LATE	Half-BW	Double-BW	LATE	Half-BW	Double-BW
Bandwidth	2 y.	1 y.	4 y.	2 y.	1 y.	4 y.
Observations	84	49	134	32	19	55
Estimate	0.6626	0.0500	0.4154	-0.9476	-1.0500	-0.4677
Std. Error	1.4903	0.8983	1.3902	1.8726	0.9051	1.3847
z value	0.44461	0.05566	0.29879	-0.5060	-1.1601	-0.3378
Pr(> z )	0.6566	0.9556	0.7651	0.6128	0.2460	0.7355
F-statistic	2.139e-03	3.722e-05	2.699e-01	2.4675	0.0820	0.4926
Num. DoF	3	2	3	3	2	3
Denom. DoF	80	46	130	28	16	51
p-value	2.755e-04	7.443e-05	3.060e-01	0.1655	0.1567	0.6220

Note: Robust SE are calculated using HC3 estimator. The first model assesses the causal effect for the younger subgroup, the second – for the older one.

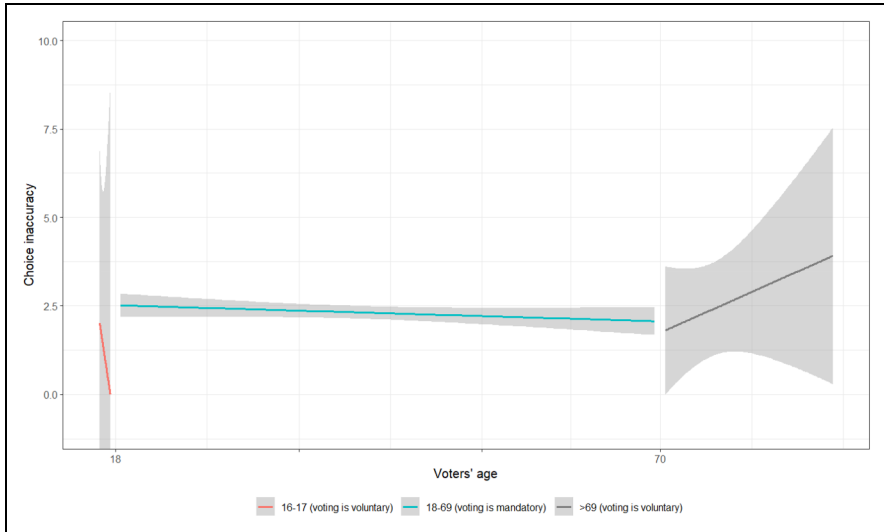
Source: Own Elaboration Based on CSES.

**Table 6.** Estimates of Local Quadratic Regressions for Different Bandwidths.

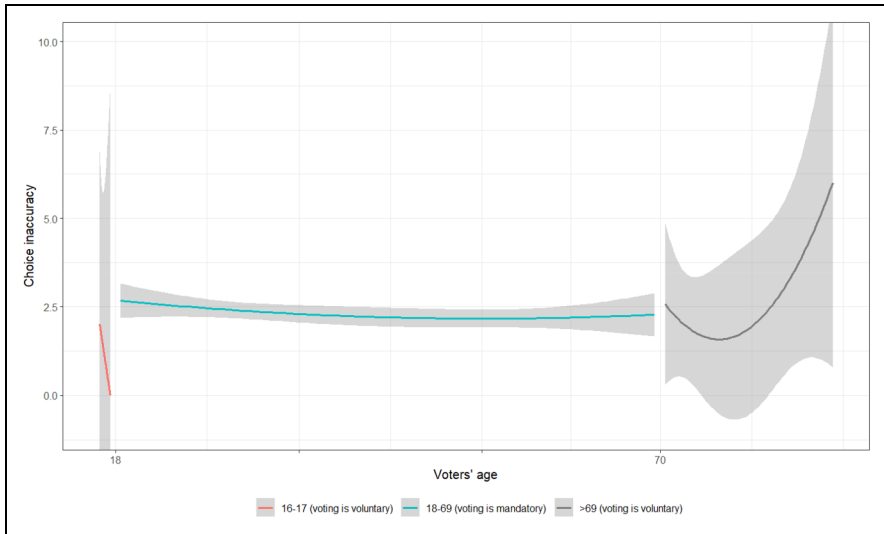
	(3) Quadratic 17–18		(4) Quadratic 69–70	
	LATE	Double-BW	LATE	Double-BW
Bandwidth	2 y.	4 y.	2 y.	4 y.
Observations	84	134	32	55
Estimate	0.7621	0.5338	5.5667	0.8242
Std. Error	2.0433	1.4482	4.3070	3.8260
z value	0.37298	0.36857	1.2925	0.2154
Pr(> z )	0.7092	0.7124	0.1962	0.8294
F-statistic	1.595e-03	2.430e-01	1.7946	0.5321
Num. DoF	4	4	4	4
Denom. DoF	79	129	27	50
p-value	1.041e-05	1.731e-01	0.3181	0.5746

Note: Robust SE are calculated using HC3 estimator. The first model assesses the causal effect for the younger subgroup, the second – for the older one. Both equations include the covariate of voter's age squared. Half-bandwidth models are not reproduced due to a lack of observations. Source: Own Elaboration Based on CSES.

types of models – for different functional forms, different numbers of observations covered, different age thresholds and a combination of all these. Moreover, the results are robust to expanding the sample by supporters of other parties (see Appendix). Hence, it can be



**Figure 3.** Linear Approximation of Discontinuities.  
Source: Own Elaboration Based on CSES.



**Figure 4.** Polynomial (Quadratic) Approximation of Discontinuities.  
Source: Own Elaboration Based on CSES.

assumed that the prevailing accuracy of voters and the high spread in ideological estimates overwhelm almost all the distortions caused by compulsory voting. Figures 3 and 4 depict these findings.

## **Discussion and Conclusion**

The political implications of compulsory voting have attracted academic attention since the late 1970s. Studies on CV-induced support for the left have been replicated many times, and models have been rebuilt to reflect methodological progress. We, in turn, try to apply the accumulated potential to the reality of recent years – to the electoral rise of extreme forces. We offer two perspectives on CV influence, institutional (at the country level) and personal (at the level of individual voters), and test their plausibility with regression tools. Our hypotheses suggest that strict enforcement of CV law causes substantial biases in aggregate rationality, leading to over-representation of marginal parties.

In pairwise cross-country tests, we find some support for this explanation. The performance of extreme parties turns out to be slightly (4–5 p.p.) better when there is a legal obligation to vote. This gap is unlikely to be accidental, although it does not appear in more complex models. Controlling for states' similarity, continental location, and electoral systems, we see the CV effect diluted to statistical insignificance. The hypothesis of deteriorating rationality receives even less support. Voters in the control group – those for whom voting is optional – show such a wide range in accuracy (as seen in the graphs preceding this section) that it is virtually impossible to distinguish them from the obliged ones. In this regard, the electorate of ideological radicals is little different from the electorate of any other politicians.

As for other possible explanations, we would like to name just a few. So far, the most plausible theory seems to be the “net neutrality” of CV, which is based on the argument of mutually exclusive effects. If this is the case, system-enhanced turnout affects support for the radicals the same way as it affects support for the left – sporadically, ambiguously, and usually not significantly. Another interpretation is that rational choice – especially in the form of “proximity voting” – is biased when analyzing extreme politics. Then CV spillovers are to be found in habit formation, social discipline, and so on. Lastly, the fact that we do not observe extreme forces' landslides or their supporters' inattention does not mean that the underlying claim is false. CV may well manifest itself electorally, but the manifestation in one's specific favor may be obscured by numerous confounders that have yet to be found. All of these hypotheses are consistent with the results of the study.

To conclude, let us say a few words about the limitations and prospects for further research. Throughout the work, we try to use the most complete data and a variety of models so that the figures obtained are as replicable and generalizable as possible. It is no secret, however, that we operate within fixed space-time boundaries, rely on rather narrow operationalizations of key concepts (“extreme party”, “support level”, “voting quality”), and actually model straight-line dependencies. Perhaps the most far-reaching of the assumptions made is the measurability of rationality. Trying to assess the accuracy

of people's choices, we make several simplifications in a row (we identify self-positioning quality with rationality, consider expert assessments as a rational baseline, and use a left-right scale proxy, realizing that the distance between its values can be perceived differently). In our view, work in several fronts could improve the situation, in order of importance: using other measures of positional accuracy, invoking non-reactive evidence of voting rationality, searching for mediators through which CV can influence individual preferences, targeting countries and subnational units in which CV is associated with the success of extreme parties. In addition, one can narrow the study field to a specific ideological orientation, using our and other approaches to test the CV factor there. Thus, it may still be possible to do what we could not – to link mandatory voting with voting for extreme forces.

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### **Notes**

1. Here and throughout the work, the relevant information on the use of CV is based on the International Institute for Democracy and Electoral Assistance materials (IDEA, n.d.). Although containing some inaccuracies, this database is a conventional source of electoral information in larger projects – for example, in Varieties of Democracy (V-dem).
2. We must notice that conceptualization here is of primary importance: our results directly depend on how we define the extremeness of parties and the quality of voting for them. For more details, see section 2.
3. We would formulate it as “is there a meaningful and relatively stable bias that sporadic activity of non-voters introduces into the political landscape?”.
4. We deliberately avoid using data from presidential campaigns: assigning a political orientation to specific candidates and expecting ideologically close votes in majoritarian conditions seems unreasonable.
5. As stated in the accompanying materials, parties with higher values are more likely to show a lacking commitment to democratic norms and use populist rhetoric prior to elections. Unfortunately, the limited sample does not allow us to cluster them according to their

- ideological orientation. In the future, this could well be done using estimates of the parties' positions of the on a right-left scale (in particular, *v2pariglef* in Lührmann et al., 2019) and their standard deviation.
6. The sources of information on these variables are various international TSCS-datasets, but most of them are collected by the V-dem (*v2elparrel*, *v2dlengage*, *v2petersch*, *v2merange*, *v2cacamps*, *v2xpe\_exlgeo*, *v2xeg\_eqdr*, *e\_migdppln* and *e\_migdpgro* in Coppedge et al., 2021). As for the rest of the predictors, their sources are Gallagher (2021) – *Eff Nv*, Tufis and Hudson (2021) – *v\_23\_10*, and World Bank (n.d.) – *NY.ADJ.NNTY.PC.KD.ZG*. All variables and their sources are listed in the dedicated table in the Appendix.
  7. There are only a few of them: Namibia 1999, Namibia 2004 and Namibia 2014. The elections in the corresponding years were won by the illiberal SWAPO by a huge margin (over 75% of the votes). As a result, these observations have a Cook's distance greatly exceeding 1.
  8. Here the question may arise as to whether we should model a hierarchical relationship, taking into account the efforts and circumstances of the parties as well as country-wide variables. There are several objections to this approach. First, the process of selecting units (documented parties of a specific ideological position) suggests that there is almost no intra-country variation. Second, we have no practical reason to believe that in countries divided by the use of CV and approximated by all the rest, party characteristics will differ. In our case, this argument is supported by data – e.g., we do not observe any significant difference in party funding between the subsamples (see *v2pafunds* in Lührmann et al., 2019).
  9. In the CSES datafiles, these are the variables *imd3006* for the first four waves and *e3020* for the fifth one.
  10. The key assumption of RDD-model is that voters are comparable on either side of the cutoff. Why do we think that this is true and do not overload the model with covariates? First, all the key demographics within the thresholds 17–18 and 69–70 are balanced. Second, as noted by Singh (2021), there are no other significant eligibilities that could overlap with age cutoffs – in Latin America, these could only be permission to purchase alcohol and tobacco (comes at 18) and the right to retire (in all countries under consideration, it occurs earlier than at 69).
  11. The output of these verification procedures can be found in the Appendix.

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## Appendix

**Table A1.** Variables and Databases for the Cross-Country Model.

Operationalization table			
Measured concept	Variable – Original name	Units	Source
Extreme forces' success	Illiberal / populist parties' vote share – v2pavote	Per cent	V-party
Compulsory voting	CV application – v2elcomvot (binarized)	Categories 0–1 (no, yes)	V-dem
	CV strictness – v2elcomvot	Categories 0–3 (no, weak enforcement, minimal costs, considerable costs)	
Political institutions factor	Electoral system – v2elparlel (reordered)	Categories 0–2 (mixed, majoritarian, proportional)	V-dem
	Eff N of parties – Eff N <sub>v</sub>	Index values	M. Gallagher Election Indices
Social cleavages factor	Engaged society – v2dlengage	Index values	V-dem
	Tertiary education – v2petersch	Per cent	V-dem
	Media perspectives – v2merange	Index values	V-dem
	Political polarization – v2cacamps	Index values	V-dem
	Religious & ethnic tensions – v_23_10	Index values	Global State of Democracy Indices
Economic development factor	Urban-Rural divide – v2xpe_exlgeo	Index values	V-dem
	Resources distribution – v2xeg_eqdr	Index values	V-dem
	log GDP per capita – e_migdppln	Logged values	
	GDP per capita growth – e_migdpgro	Per cent	
	Income per capita growth – NY.ADJ.NNTY.PC.KD.ZG	Per cent	World Bank
Voter participation	Turnout (absolute) – Voter Turnout	Per cent	IDEA
	Turnout (relative) – Voter Turnout (first difference)		

Source: Own Elaboration and Bases Listed Inside.

**Table A2.** Descriptive Statistics for the Cross-Country Model.

Statistic	N	Mean	Median	St Dev.	Min	Max
<i>Year</i>	601				1976	2019
<i>Macroregion</i>	601				1	6
<b>Illiberal parties' vote share</b>	324	24.4	17.2	18.7	2.0	80.0
<b>Populist parties' vote share</b>	275	16.6	12.8	12.2	1.5	62.6
CV application	601	0.2	0	0.4	0	1
CV strictness	601	0.3	0	0.6	0	3
Electoral system	601	1.1	1	0.6	0	3
Eff N of parties	595	5.0	4.2	2.8	1.6	18.1
Engaged society	601	1.3	1.3	0.9	-1.6	3.5
Tertiary education	275	31.6	24.2	23.2	0.4	92.7
Media perspectives	601	1.4	1.4	0.9	-2.1	2.8
Political polarization	601	0.00	0.2	1.3	-3.7	3.1
Religious & ethnic tensions	495	0.6	0.7	0.2	0.2	1.0
Urban-Rural divide	601	0.3	0.3	0.2	0.04	0.9
Resources distribution	601	0.7	0.8	0.3	0.1	1.0
log GDP per capita	544	9.2	9.3	1.0	6.7	11.2
GDP per capita growth	544	0.03	0.03	0.05	-0.2	0.3
Income per capita growth	493	2.7	2.5	7.6	-88.4	22.0
Turnout (absolute)	601	0.7	0.7	0.1	0.3	1.0
Turnout (relative)	582	-0.01	-0.01	0.1	-0.5	0.8

Note: Dependent variables are given in bold. Source: Own Elaboration Based on Combined Dataset.

**Table A3.** Sensitivity Analysis Results.

Rosenbaum Sensitivity Test for Wilcoxon Signed Rank P-Value		
Unconfounded estimate .... 0.0617		
Gamma	Lower bound	Upper bound
1.0	0.0617	0.0617
1.1	0.0257	0.1279
1.2	0.0101	0.2206
1.3	0.0038	0.3320
1.4	0.0013	0.4506
1.5	0.0005	0.5653

Note: Gamma is odds of differential assignment to treatment due to unobserved factors. Source: Own Elaboration Based on Combined Dataset.

**Table A4.** Post-Matching Covariate Balance.

	log GDP per capita		Turnout (absolute)		Religious & ethnic tensions	
	Before Matching	After Matching	Before Matching	After Matching	Before Matching	After Matching
mean treatment	9.485	9.485	0.75351	0.75351	0.67896	0.67896
mean control	9.3024	9.5824	0.64641	0.67569	0.63194	0.65003
std mean diff	26.083	-13.91	88.817	64.535	23.703	14.586
mean raw eQQ diff	0.33104	0.2324	0.1081	0.077818	0.061126	0.06684
med raw eQQ diff	0.22	0.25	0.10815	0.07805	0.048611	0.041667
max raw eQQ diff	1.45	0.58	0.1644	0.1188	0.16667	0.16667
mean eCDF diff	0.06966	0.084112	0.22313	0.17342	0.080482	0.076885
med eCDF diff	0.073501	0.0625	0.25866	0.1875	0.051597	0.052083
max eCDF diff	0.13912	0.21875	0.34531	0.30208	0.26334	0.20833
var ratio (Tr/Co)	0.4082	0.57387	0.85548	0.992	0.89686	0.76272
T-test p-value	0.053342	0.41958	3.781e-12	2.5044e-05	0.045952	0.30547
KS Bootstrap p-value	0.066	0.022	< 2.22e-16	< 2.22e-16	< 2.22e-16	0.004
KS Naive p-value	0.11447	0.02023	4.4403e-08	0.00031364	7.0764e-05	0.031008
KS Statistic	0.13912	0.21875	0.34531	0.30208	0.26334	0.20833

Note: The number of bootstrap samples is 500. Only the key predictors of the logit model are presented, the rest can be provided upon request. Source: Own Elaboration Based on Combined Dataset.

**Table A5.** Results from Last-Step Multiple OLS Regression.

	Estimate	Sth. Error	t value	Pr(> t )
(Intercept)	13.7859	2.6917	5.122	7.64e-07 ***
CV (no or weak sanctions)	0.7580	2.4003	0.316	0.753
CV (minimal sanctions)	-0.5242	2.9368	-0.178	0.859
CV (strict sanctions)	-21.8604	14.1847	-1.541	0.125
ES (majoritarian)	3.0426	4.5338	0.671	0.503
ES (proportional)	1.4998	2.7252	0.550	0.583
Latin American	11.3747	2.1677	5.247	4.24e-07 ***

Residual standard error: 14.01 on 183 degrees of freedom

Multiple R-squared: 0.147, Adjusted R-squared: 0.119

F-statistic: 5.256 on 6 and 183 DF, p-value: 5.09e-05

Signif. codes: 0 '\*\*\*' 0.001 '\*\*' 0.01 '\*' 0.05 '.' 0.1 ' ' 1

Note: Data are country-by-country matched observations. Only democracies with a Polity combined score > 3 are sampled. Source: Own Elaboration Based on Combined Dataset.

**Table A6.** Results from Last-Step Multiple OLS Regression.

	Estimate	Sth. Error	t value	Pr(> t )
(Intercept)	14.4227	3.3578	4.295	2.87e-05 ***
CV (no or weak sanctions)	0.2940	2.4781	0.119	0.906
CV (minimal sanctions)	-0.5712	3.0122	-0.190	0.850
CV (strict sanctions)	-21.4926	14.4786	-1.484	0.139
ES (majoritarian)	5.9529	4.9724	1.197	0.233
ES (proportional)	0.1775	3.1821	0.056	0.956
Latin American	11.6925	2.2182	5.271	3.91e-07 ***

Residual standard error: 14.31 on 177 degrees of freedom

Multiple R-squared: 0.1465, Adjusted R-squared: 0.1176

F-statistic: 5.063 on 6 and 177 DF, p-value: 8.08e-05

Signif. codes: 0 '\*\*\*' 0.001 '\*\*' 0.01 '\*' 0.05 '.' 0.1 ' ' 1

Note: Data are country-by-country matched observations. Only democracies with a Polity combined score > 6 are sampled. Source: Own Elaboration Based on Combined Dataset.

**Table A7.** Estimates of Local Linear Regressions for Different Bandwidths.

	(1) Linear 17–18			(2) Linear 69–70		
	LATE	Half-BW	Double-BW	LATE	Half-BW	Double-BW
Bandwidth	2 y.	1 y.	4 y.	2 y.	1 y.	4 y.
Observations	471	325	740	206	121	357
Estimate	-0.4867	-0.1026	-0.5297	-0.1513	-0.0290	0.1208
Std. Error	0.8945	0.4205	0.8902	0.8125	0.4929	0.6143
z value	-0.5441	-0.2439	-0.5950	-0.1862	-0.0587	0.1966
Pr(> z )	0.5864	0.8073	0.5518	0.8523	0.9532	0.8441
F-statistic	0.1672	0.0003	0.1702	1.6803	0.0033	1.1527
Num. DoF	3	2	3	3	2	3
Denom. DoF	467	322	736	202	118	353
p-value	0.1630	0.0006	0.1669	0.3449	0.0067	0.6556

Note: Robust SE are calculated using HC3 estimator.

The first model assesses the causal effect for the younger subgroup, the second – for the older one. All voters, not just “extreme” ones, are sampled. Source: Own Elaboration Based on CSES.

**Table A8.** Estimates of Local Quadratic Regressions for Different Bandwidths.

	(3) Quadratic 17–18			(4) Quadratic 69–70		
	LATE	Half-BW	Double-BW	LATE	Half-BW	Double-BW
Bandwidth	2 y.	1 y.	4 y.	2 y.	1 y.	4 y.
Observations	471	325	740	206	121	357
Estimate	-0.6442	-0.1026	-0.6494	1.3444	-0.0289	0.8247
Std. Error	0.9706	0.4205	0.8961	1.2247	0.4929	0.7994
z value	-0.6637	-0.2439	-0.7247	1.0978	-0.0587	1.0317
Pr(> z )	0.5069	0.8073	0.4686	0.2723	0.9532	0.3022
F-statistic	0.1253	0.0003	0.3154	1.2580	0.0033	1.4247
Num. DoF	4	2	4	4	2	4
Denom. DoF	466	322	735	201	118	352
p-value	0.0534	0.0006	0.2644	0.5757	0.0067	0.4505

Note: Robust SE are calculated using HC3 estimator.

The first model assesses the causal effect for the younger subgroup, the second – for the older one. Both equations include the covariate of voter’s age squared. All voters, not just “extreme” ones, are sampled. Source: Own Elaboration Based on CSES.