Abstract
What difference does it make if the state makes people vote? The question is central to normative debates about the rights and duties of citizens in a democracy, and to contemporary policy debates in a variety of countries over what actions states should take to encourage electoral participation. By focusing on a rare case of abolishing compulsory voting in Venezuela, we show that not forcing people to vote yielded a more unequal distribution of income. Our evidence supports Arend Lijphart’s claim, advanced in his 1996 presidential address to the American Political Science Association, that compulsory voting can offset class bias in turnout and, in turn, contribute to the equality of influence.
1. Introduction

What difference does it make if more, or fewer, people vote? What difference would it make if the state makes people vote? These questions are central to normative debates about the rights and duties of citizens in a democracy (Lacroix 2007, Lever 2010), and to contemporary policy debates in a variety of countries over what actions states should take to encourage electoral participation (International Institute for Democracy and Electoral Assistance 2006). To address them, this paper focuses on the phenomenon of compulsory voting – legal requirements that compel citizens to vote in elections. Focusing on a rare case of abolishing compulsory voting in Venezuela, we provide empirical support for the proposition that compulsory voting can reduce income inequality.

In his 1996 presidential address to the American Political Science Association, Arend Lijphart contended that class bias, “the inequality of representation and influence… not randomly distributed but systematically biased in favor of more privileged citizens… and against less advantaged citizens,” is the central “unresolved dilemma” of democracy (Lijphart 1997:1). The normative foundation of this argument is that, in a democracy, the preferences of every citizen should have equal weight in determining policy. Lijphart contends that “[l]ow voter turnout means unequal and socioeconomically biased turnout… [and] unequal participation spells unequal influence” (p. 2). From this, he concludes that compulsory voting is “the strongest of all the institutional factors” (p. 8) in its potential to remedy the pernicious effects of class bias in turnout.

Numerous scholars have investigated the impact of voter turnout on various electoral outcomes (see Fowler 2013 for a review). Most of these studies, however, face
methodological shortcomings because they are based on cross-sectional regression without a convincing identification strategy for causal inference, or because they rely on instrumental variables based on exogenous “shocks” to turnout (e.g. weather events) that are not relevant to Lijphart’s central claim – the level of voter turnout influenced by whether voting is compulsory or mandatory would affect policy outcomes.

Two recent studies properly address these methodological concerns. Fowler (2013) estimates the effects of the introduction of compulsory voting in Australia on election outcomes and pension spending; and Bechtel, Hangartner, and Schmid (2015) examine the effects of the introduction of compulsory voting in the Swiss canton of Vaud on the results of federal referendums. Both studies use an important change in the voting rule as leverage for causal inference, effectively examining the counterfactual question: What would have happened if the compulsory voting rule had not been introduced? Our study extends this line of inquiry by examining an electoral reform that pushed in the opposite direction, the abolition of compulsory voting.

The case of abolition on which we focus is Venezuela, one of a few countries that enforced compulsory voting for a long period but then dropped it. None of the others offers as promising an environment to study the national-level policy effects of abandoning compulsory voting, either because they abolished compulsory voting before reliable data on economic inequality are available (Spain and Netherlands), because they abolished it recently so that effects on inequality could not yet be evident (Chile), or because they retained the legal requirement in some regions after the national-level statute was eliminated (Switzerland and Austria).

Using Venezuela as a critical case, we investigate the downstream effect of
compulsory voting on income inequality.\footnote{Chong and Olivera (2008) estimate the impact of compulsory voting on income inequality. Most of their estimates are based on cross-national regressions, and they use some instrumental variables for robustness tests. Studies in this vein provide substantial leverage, but also confront a problem in that their treatment variable – whether a country has a compulsory voting system or not – is \textit{causally prior} to almost all variables included in their models, because most countries that have compulsory voting introduced it many decades ago. Therefore, their control variables (or exogenous variables in their two-stage least square regressions) are post-treatment variables, the inclusion of which can introduce bias in causal inference (Rosenbaum 1984). An alternative approach, which our study and other recent studies take, is to focus on cases in which an institution was \textit{introduced} or \textit{abolished}. The relative rarity of such cases imposes its own limitation, but we regard this approach as complementary to the effort to make valid causal inference on the impact of institutional arrangements.} The outcome variables Fowler (2013) uses are voter turnout, the Australian Labor Party’s vote share, its seat share, and pension spending as a share of GDP. Bechtel, Hangartner, and Schmid (2015) use turnout in referendums, as well as their measures of electoral support for left policy positions. The estimated effects of compulsory voting on voter turnout and on leftists’ vote share support the proposition that compulsory voting mitigates class bias in voter turnout, and the differences detected in policy-relevant variables indicate responsiveness to the changing electorates. Our study takes the next step of estimating the \textit{socio-economic consequences} of turnout bias – the ultimate outcome variable for Lijphart (1997).
In what follows, Section 2 reviews Lijphart’s theory. Section 3 provides background on the Venezuelan case. Section 4 presents difference-in-differences (DD) regression models that estimate the downstream effect on income inequality. The results suggest that not forcing people to vote yielded a more unequal distribution of income in Venezuela than would have obtained had compulsory voting been maintained. Section 5 discusses alternative interpretations and shows the results of further tests. The last section concludes and discusses policy implications.

2. Lijphart’s Theory of Unequal Influence

Lijphart’s central argument is that compulsory voting is the strongest remedy for unequal political influence due to unequal turnout. An observable implication derived from the argument is that income is more unequally distributed when voting is voluntary than if it were compulsory. Making the underlying assumptions in Lijphart’s theory explicit is key to understanding the causal mechanisms connecting compulsory voting and income inequality.

The first is that socioeconomic status is a proxy for preferences over economic policies that reduce inequality. Specifically, Lijphart assumes that the poor prefer policies that minimize inequality more than the rich do. Such policies include progressive taxation on incomes and comprehensive tax deductions for the poor. Just as importantly, given that the economically less advantaged people are more susceptible to job market fluctuation and more dependent on public sector jobs, the poor should have stronger preference than the rich for public spending.

The second assumption is that when voting is voluntary, its net utility is greater for the rich than for the poor. There are various reasons this might be the case. For
example, the rich might have more riding on policymakers’ decisions. In a country where only a minority earns sufficient formal income to pay income taxes, marginal tax rates will be of little consequence to most citizens but highly salient to the rich (Kasara and Suryanarayan 2015). Voting may also be more onerous for the poor. Poverty corresponds everywhere with low education levels, so the efforts required for voters to gain information about candidates and policy platforms should be larger for the poor than the rich (Downs 1957; Matsusaka 1995). The costs can also be logistical. The poor may lack transportation or flexible work schedules that allow access to the polls. In short, when voting is voluntary, turnout should be higher among the rich than the poor – that is, there should be a positive socioeconomic bias to participation. Such bias is strong and longstanding in the United States (Jackson, Brown, and Wright 1998; Leighley and Nagler 1992), and has been documented in various other national contexts as well (Hooghe and Pelleriaux 1998, Jaitman 2013, Irwin 1974, Martkainen, Martikainen, and Wass 2005; Singh 2011a; Tingsten 1937).

2 In more recent cross-national studies, however, scholars have reached competing conclusions on whether compulsory voting mitigates stratification of participation by education (Gallego 2010; Quintellier et.al. 2011).

3 We do not use the word “positive” to imply any normative judgment, but rather a simple positive correlation between wealth and voting.

4 Franklin (1999) and Kasara and Suryanarayan (2015) question the prevalence of positive turnout bias across nations, and Maldonado (2011) finds that the demographic correlates of voting in compulsory systems mirror those in voluntary systems. Our
The third assumption is that imposing sanctions on non-voters should mitigate this bias. This should be the case particularly insofar as sanctions such as fines weigh more heavily on the poor than the rich. Prior studies have shown that voter turnout is high as long as compulsory voting is enforced with substantial punishments (Fowler 2013; Jackman 2001) and that socioeconomic bias in turnout is mitigated under compulsory voting (Singh 2011b and 2015).

Under these three assumptions, the median voter’s position is expected to shift to the right under voluntary voting relative to compulsory voting. The final assumption is that the elected government responds to shifts in the median voter’s preference. As a result, when compulsory voting is introduced (abolished), the elected government should pursue policies that make the distribution of income more (less) equal.

Following Lijphart’s theory and logical assumptions underlying it, a shift from compulsory to voluntary voting is expected to have the following effects: a decline in voter turnout; a shift to the right in the preference of the richer median voter; the election of politicians who support less redistribution; the implementation of less progressive

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preliminary analysis using the Comparative Studies of Electoral Systems and AmericasBarometer by the Latin American Public Opinion Project (LAPOP), however, supports the traditional view that positive bias is the norm cross-nationally and its inverse is extremely rare.

5 We do not require a stronger assumption that governments accurately implement the preferred policy of the median voter.
economic policies; and an increase in economic inequality.⁶ In the next section, we examine the empirical validity of the first four effects in the context of Venezuela. We then examine the validity of the fifth, and final, effect in Section 4.

3. The Case of Venezuela

Throughout the late 1970s and 1980s, Venezuelans’ confidence in their political institutions declined. In response, Venezuela adopted a variety of electoral reforms during the time period under consideration. In 1984, President Jaime Lusinchi created the Presidential Commission on the Reform of the State (COPRE) to propose reforms aimed at reestablishing citizen engagement and trust. COPRE-initiated reforms included direct elections of mayors and candidate-preference votes within lists for municipal councilors in 1988, direct elections of state governors in 1989, and then in 1993 the introduction of a mixed system for the Chamber of Deputies together with the end of compulsory voting. These latter two reforms are of particular interest because of their potential connection to the politics of redistribution.

From 1958 to 1988, Venezuelan legislators were elected using a closed-list proportional representation (PR) system using the country’s states as districts. The 1993 reform shifted Chamber of Deputies elections to a mixed-member system resembling Germany’s, in which half the seats are contested by plurality in single-member districts (SMDs) while the other half are allocated to achieve overall proportionality. The electoral law of 1958 also established compulsory voting, along with the specific sanctions for

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⁶ A shift from voluntary to compulsory voting is expected to produce the opposite effect at each link of the causal chain.
non-participation, which included restrictions on employment for, or contracting with, public entities, on conducting bank transactions, on holding public office, on securing a passport or traveling outside the country, or enrolling in a state university, for a period of six months after the election in which a citizen did not vote (Rosales 1986). Congress eliminated sanctions in the Organic Suffrage Law of 1993 (Molina and Perez Baralt 1995, 1996).

The coincidence of proportional elections and compulsory voting raises the question of whether the former change affected the politics of income inequality. Both Austen-Smith (2000) and Iversen and Soskice (2006) argue that PR elections encourage, and SMDs discourage, progressive redistribution. Yet, there are reasons to be cautious about applying either model to the Venezuelan case. First, although Venezuela’s reform created SMD competition for half of the Chamber’s seats, the overall distribution of seats remained proportional, mitigating the Duvergerian imperative of SMD competition on

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7 Language describing voting as a duty remained in the Constitution until the promulgation of the new charter in 1999. The elimination of any legal sanctions from 1993 on, however, marks that year as the end of compulsory voting. We thank XXX (personal communication, January 21, 2013) for this account of the subtle changes in the *de jure* and *de facto* status of compulsory voting in Venezuela over time.

8 Scholarly debate on the effects of SMD versus PR on redistribution is not yet resolved. Chang et.al. (2011), for example, make the converse argument that SMD competition yields policies with diffuse economic benefits and PR encourages concentration of benefits.
which both models depend. Second, in the Iversen and Soskice model, the redistributive effect is driven by differences between multi-party and two-party parliaments in the formation of coalition governments, whereas Venezuela was, and remains, a presidential system. For these reasons, we argue, the theoretical grounds to expect an impact on income distribution from abandoning compulsory voting are stronger than from the shift to a mixed electoral system. Nevertheless, it is worth taking seriously the prospect that the mixed system reform could affect economic inequality, and we return to test the proposition empirically in a later section. For now, we focus on the logical sequence underpinning Lijphart’s theory of compulsory voting.

The first link in that causal chain is that turnout should decline when compulsory voting is abandoned. This effect was clear and well documented in the Venezuelan case (Molina 1995; Molina and Perez Baralt 1995). Voter turnout in national elections had been above 75% in every election since the adoption of compulsory voting. Specifically, it was 80% in 1958, 75% in 1963, 92% in 1968, 90% in 1973, 79% in 1978, 83% in 1983, and 80% in 1988 (Molina 1995). As soon as sanctions for non-voting ceased, in the 1993 election, however, turnout sharply dropped to 54%, 20% points below the lowest turnout during the period with compulsory voting.

The second link holds that the turnout drop should be concentrated more heavily among poorer voters than wealthier voters, diminishing demand for progressive redistribution. Venezuelan surveys provide some leverage here, although the data are less than ideal (Baloyra and Torres 1983; Canache 2002). A pre-election survey in 1983 that asked voters to recall their participation ten and five years prior, in the 1973 and 1978 elections, found 6.9% and 10.4% gaps, respectively, in reported participation between the
top and bottom income quartiles. A 1995 survey asking voters to recall participation in the 1993 election produced a corresponding gap of 12.4%. This may suggest a widening gap after the abolishment of compulsory voting, but the increase is only a marginal one and subject to all the limitations of respondents’ recall over long time periods. Surveys that asked more directly about the effects of compulsory voting show more pronounced differences across income groups. The 1983 survey mentioned above, for instance, asked whether respondents intended to vote in the upcoming election that year, and whether they would vote if voting were not compulsory (Baloyra and Torres 1983). Virtually all respondents, regardless of socioeconomic status, expressed an intention to vote, but only 25% of those in the lowest income quartile indicated they would do so if voting were not compulsory, compared with 54% in the top income quartile. Similarly, a survey conducted in 1993, two months before compulsory voting was struck from the electoral law, asked whether respondents would vote that year if voting were not obligatory. The survey dichotomizes all respondents into a low-income group (88% of respondents) and a high-income group (12%), but one third of the poorer respondents said they would not vote if it were not compulsory, as compared with one fifth of the wealthier group (Centro de Investigaciones y Estudios Politicos y Administrativo, y Institucion de Opinion Publica 1993). Subject to the limitations of these data, there is suggestive evidence that compulsory voting diminished socioeconomic bias in participation below levels that would apply under voluntary voting.

Beyond the size and composition of the electorate, the next places to look for effects are in electoral outcomes – which parties and politicians are elected – and in their policies that affect the distribution of wealth. The period of our investigation was one of
enormous turbulence, economically and politically, following on decades during which Venezuela had been regarded as an island of relative prosperity and stability in a much stormier Latin American sea. Venezuelan democracy during this period has been well chronicled (Coppedge 1994 and 1996; Crisp 2000; Karl 1997). Does that history suggest a shift in the median voter’s preference?

For our purposes, the top panel of Figure 1 provides a schematic of party competition from the 1960s through the 1980s, when Venezuelan elections were dominated by the center-left social democratic Accion Democratica (AD) and the center-right Christian Democratic (COPEI) parties, with a regular, much smaller electoral presence of smaller parties, under varying labels, on the left. AD and COPEI dominated all seven national elections from 1958-1988, taking more than 85% of the votes between them in each of the last four contests. Throughout the long period in which voting was compulsory, Venezuelans grew familiar with a set of options with established locations on a left-right ideological spectrum. The center-left AD had the strongest claim to Venezuela’s median voter, winning five of seven presidential elections from 1958 to 1988.

[Figure 1]

By 1993, however, Venezuela had experienced a period of economic crisis and political discontent under AD President Carlos Andres Perez. The contest for COPEI’s presidential nomination that year split the party. Rafael Caldera, who had founded the COPEI in the 1950s and served as president from 1968 to 1973, had resuscitated his political career with a speech in the Senate in 1992 decrying the disproportionate burdens that economic reforms at the time were imposing on the poor. Caldera’s attempt to
recapture COPEI’s nomination for the presidency failed, however, and he bolted the party to run with the backing of a coalition of smaller parties spanning the ideological spectrum (Buxton 2003; Di John 2005). The coalition ran under the banner “Convergencia” (Convergence).

The second panel of Figure 1 locates the main presidential contenders in the 1993 election according to the most comprehensive data on ideological placement of presidential candidacies in Latin American (Baker and Greene 2011). Specifically, it shows each candidate’s ideological position on a continuous scale ranging from 1 (left most) to 20 (right most). AD (ideological position = 11.4), discredited by Perez’s presidency, hemorrhaged voters to its competitors on both flanks. Causa Radical (ideological position = 7.9) was one beneficiary, more than doubling the left’s traditional vote share, but on the right, Caldera’s Convergencia and the rump COPEI (ideological position for both camps = 14.8) won a majority of the vote. In the four-way race, Caldera’s 31% was sufficient to capture the presidency.

This account of events is consistent with Lijphart’s premise that moving from compulsory to voluntary voting should produce a class bias in abstention that pushes the median voter right, diminishing demand for progressive redistribution. To recap, the center-left AD dominated elections during the period with compulsory voting, but the winner of the first election without compulsory voting in 1993, Caldera, was center-right.

Before embracing this interpretation unreservedly, however, is important to acknowledge that narrative accounts of Venezuelan politics in this period portray a fluid electoral environment in which ideological locations of candidacies were hard to pin down. When he was elected in 1988, AD’s Carlos Andres Perez had, like Caldera,
previously served as president – in his case, from 1973 to 1978 during an oil boom. Venezuelans associated Perez with generous government spending, and he did little to dissuade this expectation during his 1988 campaign (Buxton 2003). Once inaugurated, however, Perez imposed an economic austerity package described as the *Gran Virage* (Great Turnaround). Cuts in fuel subsidies translated into public-transportation-rate hikes, which in turn sparked a series of riots in February 1989 known as the *Caracazo*. Perez called out the military, whose heavy-handed response left more than 350 dead (Di John 2005). The president’s approval ratings dropped from about 50% to 35% during his first year in office (Stokes 2001). His presidency never fully recovered, and was threatened by two separate military coup attempts in 1992, neither successful, but the first of which made a media celebrity out of a young colonel with a gift for populist rhetoric: Hugo Chávez. A scandal involving government appropriations prompted Perez’s removal from office and replacement by an interim president in the last year of his term. When he ran in 1993, Caldera excoriated the economic hardship imposed on the poor under Perez’s administration (Buxton 2003). Perez’s *Gran Virage* had pulled the AD to the right, and Caldera’s 1993 incarnation was an effort to move left from the traditional COPEI that Caldera himself had helped to found four decades earlier.

This alternative narrative raises the question of whether the 1993 electoral result genuinely represented a rightward shift. The longstanding reputations of the main actors, as well as the most authoritative dataset on candidate ideologies, suggest it did (Baker and Greene 2011). Yet electoral environment was tumultuous, and the traditional party system was disintegrating. For a clearer picture of the political and economic flux of those years, we turn to economic policies these successive administrations pursued.
Figure 2 plots the share of GDP accounted for by government consumption of goods and services, plus government (gross) investment expenditures, against (net) Gini coefficients, our measure of inequality after government taxes and transfers. The vertical bars show the 95% confidence interval for the Gini estimates. Government consumption levels (gray dots in Figure 2) bounced around during Carlos Andres Perez’s term (1989-1993), on net decreasing from just above 32% in 1989 to just above 30% in 1993. The 1993 level, however, is barely below the level from 1988, the year before Perez’s Gran Viraje package of economic reforms. Overall, given the measurement error inherent in the Gini coefficients, there is no statistically discernible movement in inequality during the Perez years. This is an important but perhaps counter-intuitive finding. Perez is often portrayed as the president who promoted drastic neoliberal reforms, but neither the government share of GDP nor the Gini coefficient changed substantially during his term.

[Figure 2 here]

The pattern only becomes dramatic under Caldera’s administration (black dots in Figure 2), when government spending dropped sharply and monotonically, with corresponding increases in our measure of inequality. The negative correlation (−0.76, p = 0.01, 1989-1998) between these variables suggests that the post-1993 austerity policies were an important determinant of the increase in inequality.

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9 The data source for the government share as the percentage of GDP is Feenstra, Inklaar and Timmer (2013). The data sources of the Gini coefficients and their uncertainty estimates are Solt (2009, 2013).
Summary

Venezuelan politics from 1988 to 1998 were buffeted by an array of forces broader than just the end of compulsory voting, but all the evidence we encounter is consistent with the proposition that the end of compulsory voting decreased participation, increased socioeconomic bias, and pushed the center of gravity in the Venezuelan electorate to the right. The policy changes that ensued increased inequality. Although Perez’s Gran Viraje is often characterized as a pivotal moment in accounts of Venezuelan politics of the period, neither the share of the economy accounted for by government spending nor conventional measures of income inequality changed under his government. The economic data suggest that it was Caldera’s administration that exposed, more systematically, the vulnerability of the poorest Venezuelans.

4. Estimating the Effect on Inequality

We now examine more systematically the final and most consequential effect of Lijphart’s theory – the effect of abandoning compulsory voting on income inequality.

4.1 Data

To measure income inequality, we rely on the net (i.e., after-tax, after-transfer) Gini coefficient as measured by the Standardized World Income Inequality Database (SWIID), Version 4.0 (Solt 2009; 2013). An advantage of using the SWIID dataset is that it has greater coverage across countries and over time than any other cross-national income inequality databases. Specifically, by combining a variety of existing data including the United Nations University’s World Income Inequality Database (WIID) and the
Luxembourg Income Study (LIS), the SWIID dataset provides comparable Gini coefficients for 173 countries for as many years as possible from 1960 to 2012.\textsuperscript{10}

The question we examine in this statistical analysis is: What was the estimated trajectory of income inequality had Venezuela not experienced a change in 1993? To examine this counter-factual question using cross-national, time-series data, we draw on a set of countries that share historical, political, economic, and social context with Venezuela – the Latin American and Caribbean countries that were net oil exporters as of 1993: Argentina, Bolivia, Colombia, Ecuador, Mexico, and Trinidad and Tobago.\textsuperscript{11} Figure 3 shows estimated inequality across these six countries, plus Venezuela.

![Figure 3 here]

Our statistical analysis uses three periods of investigation, 1978-1998, 1983-1998 and 1988-1998. The number of observations is 127, 102, and 76, respectively. The coefficient estimates using shorter periods are less efficient due to smaller numbers of observations, but country-specific trend variables, which we will introduce shortly, may more accurately capture subtle fluctuation in income inequality in each country.

\textsuperscript{10} Another noticeable feature of the SWIID dataset is that it includes 100 estimates of the Gini coefficient for each country in each year. With the 100 multiply imputed estimates for each country in each year, we can obtain not only a point estimate (i.e., the average of multiply imputed values) but also an interval estimate of inequality.

\textsuperscript{11} Suriname was also a net oil exporter, but data on income inequality are not available for the time period under analysis. Data on oil exports are from the Teorell et.al. (2013).
These periods include four elections – the 1978, 1983, and 1988 elections, the last three elections before the elimination of sanctions in Venezuela, and the subsequent 1993 election. We do not extend the post-reform period beyond 1998. The 1998 presidential election was marked by the landslide victory of Hugo Chávez, a former military officer who attempted a coup d’état in 1992, won the election in 1998, and remained in the presidency until his death in 2013. The rise of Chavismo is expected to add additional systematic variation to the post-1993 trajectories of income inequality. This means that the longer the post-reform period for our analysis, the more difficult it would become to interpret the direct effect of “treatment” (i.e., in our case, the change that occurred in 1993). Methodologically, post-1993 changes in politics and policies are “post-treatment” events, which are in part consequences of the reform in 1993. Incorporating post-treatment factors into our statistical analysis and estimating mediation effects would introduce methodological challenges beyond the scope of this paper (Imai et. al. 2011; Rosenbaum 1984).

4.2 Statistical Models

We rely on a difference-in-differences (DD) design, an approach with a long tradition in social science research aimed at estimating the effect of an event, intervention, or “treatment,” using time-series observations that include periods both prior and subsequent to the event – in our case, the abolishment of compulsory voting in Venezuela.
in 1993. We run the following regression model using the cross-national and time-series data.

\[ Y_{it} = b_0 + b_1 T_{1it} + b_2 T_{2it} + f_i(t) + v_i + u_t + \varepsilon_{it} \]

The outcome variable \( Y_{it} \) is the net Gini coefficient for country \( i \) in year \( t \). The base category \((i = 0, t = 0)\) is Venezuela in 1993. Accordingly, the intercept coefficient \( b_0 \) predicts the Gini coefficient in Venezuela in 1993. There are two treatment variables. The first treatment \( T_{1it} \) is 1 for Venezuela after 1993 (exclusive), while it is 0 otherwise. Formally, \( T_{1it} = I(i = 0, t > 0) \). The second treatment \( T_{2it} \) is the number of years since 1993 for Venezuela, \( T_{2it} = t \cdot I(i = 0, t > 0) \). While the coefficient \( b_1 \) measures the average change in the outcome variable in Venezuela after 1993, the coefficient \( b_2 \) measures the annual increase in the outcome variable in Venezuela after 1993. After running this model, we then undertake a Chow test (Chow 1960). The null hypothesis we

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12 There is a newer, non-parametric approach called the synthetic control method (Abadie, Diamond, and Hainmueller 2010, 2015; and Abadie and Gardeazabal 2003). We prefer the traditional approach based on parametric regression, mainly because it is difficult to account for the uncertainty estimates of income inequality in the synthetic control method. In our preliminary research, we used the averages of 100 multiply-imputed estimates of income inequality as the outcome variable (see Footnote 10), under a strong assumption that they were observed facts without uncertainty, and estimated the impacts of the intervention in 1993 on Venezuela’s income inequality using the synthetic control method. The results are essentially similar to what we report in this paper.
intend to reject is that these coefficients are jointly zero: \( H_0: b_1 = b_2 = 0 \); namely, there is no “structural break” in Venezuela in 1993.

The model also includes country-specific trend variables \( f_i(t) \), country-specific fixed effects \( \nu_i \), and year-specific fixed effects \( u_t \). The last term \( \epsilon_{it} \) denotes a stochastic disturbance. Since we do not have a priori theory on the functional form for the country-specific trajectories of the Gini coefficient during the entire period of investigation, we repeat the analysis using linear and quadratic trends. In total, we run 6 regression models – three periods (1978-1998, 1983-1998, and 1988-1998) \( \times \) two functional forms (linear and quadratic).

We do not add other country-specific and time-variant covariates for two reasons. First, as we discussed earlier, the post-reform observations for these country-specific and time-variant variables are likely to be influenced by the reform of 1993 itself. Therefore, adding these variables could introduce post-treatment bias (Rosenbaum 1984). The observations before the reform are obviously not influenced by the reform itself, but we assume that the average effects of these variables on the outcome variable are, to a large extent, captured by the country-specific trend variables. Furthermore, the country-specific and year-specific fixed effects control for any country-specific attributes (for example, demographics, or political and economic history) and any time-specific attributes (for example, years in which major economic shocks happened at the global level, such as changes in the prices of natural resources and financial commodities).

4.3 Results

Regression results are presented in Table 1. The last two columns show that the null hypothesis of no structural break in Venezuela in 1993 can be rejected in four of six
models at the 5% level and at the 10% level in a fifth, suggesting that the trajectory of the Gini coefficient systematically changed in Venezuela in 1993.  

[Table 1 here]

Rather than interpreting how the trajectory changed by examining the coefficient estimates for the intercept and the two treatment variables ($\hat{b}_0$, $\hat{b}_1$, and $\hat{b}_2$) presented in Table 1, it is more intuitive to compare the factual and counterfactual trends graphically. Figure 4 summarizes such analysis for the period from 1983 to 1998. The height of each gray bar is the observed values of Gini coefficients in Venezuela, whereas the solid dots are predicted values based on our regression analysis (see Table 1; 1983-1998; Linear).

[Figure 4]

Venezuela’s Gini coefficients gradually decreased from about 43% in 1983 to about 38% in 1992. This pattern suddenly changed after 1993 and the Gini coefficient

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13 We also undertook permutation tests. For each of the countries included in our analysis, we estimated whether a structural change in the trajectory of income inequality took place in 1993. The model we estimated is exactly the same as the one we use for our main analysis. The results show that Venezuela was the only case clearly suggesting a structural change in 1993. Specifically, of the 36 non-Venezuelan model specifications, F tests suggest a structural break in only three (and only two at the 5% level). At a reviewer’s request, we also replicated our results with Peru included in the set of donor countries. Doing so did not change the results.

14 To be more precise, they are the averages of multiply-imputed SWIID data (see Footnote 10).
increased to almost 45% until a year or two before the 1998 election. Figure 4 also shows a counterfactual trajectory. The hollow dots are predictions under an assumption that the coefficients for the Venezuela-specific, post-1993 changes were zero. Recall that we only focus on countries that are sufficiently similar to Venezuela as comparison units – that is, Latin American and Caribbean net oil exporters – and our DD models control for all observable and unobservable country-specific and time-invariant factors, year-specific and country-invariant (that is, region-specific) factors, and country-specific trends (in two functional forms). Therefore, we can confidently interpret that these additional dots show the Gini coefficients in Venezuela if there were no Venezuela-specific, systematic change after 1993 (i.e., when \( b_1 = b_2 = 0 \)). The gap between these factual and counterfactual trends represents our estimate of the treatment effects.

5. Discussion

Strictly speaking, the results in Table 1 and Figure 4 only show that something happened in 1993 to change Venezuela’s trajectory of economic inequality. Our qualitative analysis in Section 3 suggests that the abolishment of compulsory voting was the key treatment, but we want to rule out other potential explanations. Another important question is whether or not the increase in income inequality was an intended (or expected) consequence. We discuss these issues in turn.

\[ \text{Specifically, to calculate the counterfactual trend, we subtracted } \hat{b}_1 T_{1it} + \hat{b}_2 T_{2it} \text{ from the predicted value for each observation } \hat{Y}_{it}. \]
5.1 Placebo Tests

The comparison set of countries was selected to match Venezuela’s social, economic, regional, and political context. Yet, some shared factors might be particularly important to understand how elections affect inequality. For example, perhaps trends in worldwide oil markets fostered sharp increases in inequality among heavily oil-dependent economies after 1993, or electoral rules other than compulsory voting contribute to inequality. We conducted placebo tests to explore these possibilities.

In the first, we replicated the difference-in-differences analysis reported in Table 1, above, for Trinidad and Tobago, the country in our comparison set for which oil exports as a share of GDP are closest to Venezuela’s. The results show that none of the models can we reject the null (or even come close) of no structural break in 1993 for Trinidad and Tobago. Thus, we do not find evidence that 1993 was a pivot year for the region’s second most oil-dependent country.

Our second placebo test builds on the fact that the end of compulsory voting in Venezuela coincided with the shift from pure PR elections for its legislature to a mixed-member proportional system, as described in Section 3. As it happens, our data include another country that implemented precisely the same reform during the time period under examination here. Bolivia had previously elected its Chamber of Deputies by proportional representation in department-level districts. In 1994, Bolivia then adopted a mixed-member proportional system, and first held elections using the new system in

\[\text{\footnotesize\textsuperscript{16}}\text{Bolivia’s nine departments are its main subnational political and administrative units, analogous to Venezuela’s twenty-four states.}\]
1997. A number of countries adopted mixed-member systems combining SMD and PR competition during the 1990s, including New Zealand, Italy, Japan, Russia, and Hungary. Bolivia’s reform is the most closely analogous to Venezuela’s insofar as both countries moved from pure, closed-list PR systems with regional districts to mixed-member compensatory systems.

In this placebo test, the period of investigation is from 1989 to 2002 with the year of treatment in 1997. The length of post-treatment period is the same as the one we use in our analysis of the case of Venezuela. Due to data limitation, however, the length of pre-treatment period is bit shorter. Otherwise, we use the same statistical model and the same data source, and we run the same tests. The results show that none of the coefficients for the treatment variables is statistically significant, and the \( p \) values of the F statistics are very large. Therefore, we again fail to reject the null hypothesis of no structural break.

To sum up, our difference-in-difference analysis shows a structural break in 1993 for inequality in Venezuela, consistent with an explanation based on the end of compulsory voting. We replicated that analysis for two regional cousins for which competing explanations might apply. But we find no evidence that oil dependence in the mid-1990s or the electoral reform from a closed-list PR system to a MMP system in accounts for the sharp increase in income inequality in Venezuela after 1993.

5.2 Intended or Unintended Consequences

In our effort to make causal inference, we make an assumption that Venezuela’s intervention, the decision to abolish compulsory voting, was independent of the outcome we are interested in here, economic inequality. That is, if the abolition of compulsory voting were part of a broader effort to alter redistributive policies that shape economic
inequality, then the counterfactual case would not provide an appropriate point of comparison.

To determine whether this presents a problem, we searched the archival record for evidence on what forces drove Venezuela’s electoral reform. Deliberations over electoral reform in Venezuela in the late 1980s and early 1990s were extensive and involved a diverse array of actors, including social scientists, constitutional law scholars, and politicians from parties across the ideological spectrum. Was the potential impact of eliminating compulsory voting on economic inequality central to that debate?

As far back as the 1970s, academic observers of Venezuelan politics appeared to recognize the logic of the redistribution hypothesis when considering the potential effects of changes in turnout. Writing during the period of full enforcement of compulsory voting and highest turnout, Baloyra and Martz (1979) noted, “(E)lectoral demobilization would introduce socioeconomic distinctions in voter turnout” (p. 71). A decade later, however, when debate over electoral reform was proceeding in earnest, the focus of discussion had shifted to the legitimacy of Venezuela’s representative institutions.

The single largest source of archival materials on Venezuelan electoral reform is the Presidential Commission on the Reform of the State (COPRE), which was created in 1984. Drawing on academics, politicians, party functionaries, and civil society groups, the COPRE produced dozens of reports and compilations that reflect an array of viewpoints on state reform during the years leading up to the end of compulsory voting.17

17 The COPRE remained in existence until 1999, but its period of greatest activity was during the late 1980s and first years of the 1990s (Cuñarro Conde 2004).
Among the COPRE’s list of reports, we identified eleven volumes that address subjects of electoral reform and citizen participation. Carefully reviewing those, we found no discussion of the prospect that ending compulsory voting would affect political support for progressive redistribution in the Venezuelan electorate. We also reviewed publications on electoral reform proposals from Venezuela’s Consejo Supremo Electoral from the same period (Rosales 1986, Zambrano 1989), as well as assorted other academic publications, and similarly found no discussion of the redistribution hypothesis associated with compulsory voting.

Although we cannot know with certainty, or document comprehensively, the impetus for the abolition of compulsory voting in Venezuela, we find no evidence that the reform was a piece of a broader set of forces pushing against economic equality. Those engaged in the reform debate at the time, and in the immediate aftermath, did not focus on any socioeconomic bias in non-voting. Indeed, the end of the voto obligatorio was part of a larger wave of electoral reforms that were pitched as egalitarian overall – empowering ordinary citizens relative to party.

6. Conclusion

Given the subsequent rise of Hugo Chávez, any argument that the ideological location of Venezuela’s median voter shifted right in the 1990s might appear perplexing. We emphasize that our claims are limited to the (short-term) effect of the abolishment of compulsory voting in the 1993 election. The policies that followed and their effects on the poorest Venezuelans may well have subsequently pushed the electoral center of gravity back to the left by directly changing voter preferences on policy and how those preferences mapped onto candidate choice. Lupu’s (2010) work on the evolution of
Chávez’s support coalition, for example, suggests that class-based polarization of the Venezuelan electorate peaked in the 1998 election. One might also ask why Chavez did not reintroduce compulsory voting when he overhauled Venezuela’s institutional landscape in 1999-2000, and whether he preferred a set of rules amenable to selective, rather than universal, mobilization. Our purpose in this paper is not to explain the rise of Chavismo, much less the strategies of Chavez himself, but rather to bring new evidence to bear on a longstanding debate over whether and how levels of electoral participation affect economic inequality.

Debates over compulsory voting are ongoing in a variety of countries, but discussion of the redistribution theory, which Lijphart forcefully advanced in 1996, is often muted. Australia, an electoral innovator on many fronts, adopted compulsory voting state by state from 1914 to 1941, but the government of Queensland State recently entertained a proposal to end the practice (Fowler 2013; Wroe 2013). A report published by the state’s government includes the following sentence as one of the arguments in favor of compulsory voting: “Governments must consider the total electorate in policy formulation and management” (Queensland Department of Justice and Attorney General 2013, p.35). Beyond that, however, there is no discussion of potential specific consequences of the proposal on income distribution. Colombia is entertaining a move in the opposite direction – to adopt compulsory voting after not having done so in its nearly 200-year history. Yet a sustained evaluation of the reform’s virtues and drawbacks does

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18 This does not negate the importance of further investigation on these topics, which we leave for future research.
not address economic redistribution (Ungar 2011).

To conclude, the Venezuelan results suggest that from a normative perspective, questions of inequality and redistribution should belong at the center of debates over compulsory voting. Ending compulsory voting, and the subsequent drop-off in electoral participation, contributed to increasing economic inequality in the 1990s above levels Venezuelans would otherwise have experienced.
Acknowledgement

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References


Figure 1: Electoral Politics in Venezuela, 1973-1998

A. During the consolidated post-Punto Fijo Era, 1973-1988

<table>
<thead>
<tr>
<th>Party</th>
<th>1973-1988</th>
</tr>
</thead>
<tbody>
<tr>
<td>Left, various (5-10%)</td>
<td></td>
</tr>
<tr>
<td>AD (43-58%)</td>
<td></td>
</tr>
<tr>
<td>COPEI (34-47%)</td>
<td></td>
</tr>
</tbody>
</table>

B. 1993 Election: Voter-revealed leftism scale (Baker and Greene 2011)

<table>
<thead>
<tr>
<th>Party</th>
<th>Score</th>
</tr>
</thead>
<tbody>
<tr>
<td>Causa Radical (22%)</td>
<td>7.9</td>
</tr>
<tr>
<td>AD (24%)</td>
<td>11.4</td>
</tr>
<tr>
<td>COPEI (23%)</td>
<td>14.8</td>
</tr>
<tr>
<td>&amp; Convegencia/Caldera</td>
<td>20</td>
</tr>
<tr>
<td>(31%)</td>
<td></td>
</tr>
</tbody>
</table>

Note: The percentages in parentheses are vote shares.
**Figure 2:** Government Share of GDP and Net Gini Coefficient in Venezuela, 1988-1998

*Note:* The vertical axis shows the averages of multiply-imputed Gini coefficients in the SWIID data. The vertical lines associated with them indicate the 95% confidence intervals. The Government Share of GDP includes government consumption of goods and services and government (gross) investment. In each of the six years up through 1993, Government Share of GDP switched direction (i.e., rising, falling, rising, etc.), whereas it declined monotonically starting in 1994. Sources: Feenstra, Inklaar and Timmer (2013), Solt (2009, 2013).
Figure 3: Gini Coefficients in Selected Net Oil Exporters, 1983-1998

Note: The vertical axis shows the averages of multiply-imputed Gini coefficients in the SWIID data. The vertical lines associated with them indicate the 95% confidence intervals. Source: Solt (2009, 2013).
Table 1: The Regression Results, Venezuela

<table>
<thead>
<tr>
<th>Period</th>
<th>Trend</th>
<th>Constant</th>
<th>$T_{1it}$</th>
<th>$T_{2it}$</th>
<th>$H_0: b_1 = b_2 = 0$</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>1978-1998</td>
<td>Linear</td>
<td>$\hat{b}_0$</td>
<td>$\hat{b}_1$</td>
<td>$\hat{b}_2$</td>
<td>F</td>
<td>P</td>
</tr>
<tr>
<td></td>
<td></td>
<td>41.73***</td>
<td>-1.76</td>
<td>0.49</td>
<td>0.18</td>
<td>0.839</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.57)</td>
<td>(2.86)</td>
<td>(0.79)</td>
<td>(1.57)</td>
<td>(2.86)</td>
</tr>
<tr>
<td></td>
<td>Quad.</td>
<td>37.50***</td>
<td>2.17</td>
<td>3.49***</td>
<td>0.000</td>
<td>11.79</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.31)</td>
<td>(1.97)</td>
<td>(0.91)</td>
<td>(1.31)</td>
<td>(1.97)</td>
</tr>
<tr>
<td>1983-1998</td>
<td>Linear</td>
<td>39.29***</td>
<td>1.09</td>
<td>1.21*</td>
<td>3.74</td>
<td>0.029</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.34)</td>
<td>(2.22)</td>
<td>(0.65)</td>
<td>(1.34)</td>
<td>(2.22)</td>
</tr>
<tr>
<td></td>
<td>Quad.</td>
<td>37.92***</td>
<td>1.94</td>
<td>2.90**</td>
<td>5.09</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.19)</td>
<td>(1.65)</td>
<td>(1.15)</td>
<td>(1.19)</td>
<td>(1.65)</td>
</tr>
<tr>
<td>1988-1998</td>
<td>Linear</td>
<td>38.30***</td>
<td>2.23</td>
<td>1.65**</td>
<td>5.34</td>
<td>0.008</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.19)</td>
<td>(1.77)</td>
<td>(0.70)</td>
<td>(1.19)</td>
<td>(1.77)</td>
</tr>
<tr>
<td></td>
<td>Quad.</td>
<td>38.29***</td>
<td>2.23</td>
<td>1.69</td>
<td>2.59</td>
<td>0.088</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.03)</td>
<td>(1.51)</td>
<td>(1.80)</td>
<td>(1.03)</td>
<td>(1.51)</td>
</tr>
</tbody>
</table>

Note: Two models are estimated for each period of investigation. Each model includes country-specific fixed effects, year-specific fixed effects, and country-specific linear or quadratic trend variables. The base category (for the constant term) is the Gini coefficient in Venezuela in 1993. The first treatment variable ($T_{1it}$) is the average change in the outcome variable in Venezuela after 1993 (exclusive). The second treatment variable ($T_{2it}$) is the annual increase in the outcome variable in Venezuela after 1993 (exclusive). The null hypothesis is that the coefficients for these two treatment variables are jointly zero ($H_0: b_1 = b_2 = 0$). The numbers in parentheses are standard errors.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-sided).
Figure 4: The Regression Results, Venezuela

Note: The height of each gray bar indicates the average multiply-imputed Gini coefficient in Venezuela. The solid dots are predicted values based on regression analysis (see Table 1; 1983-1998, Linear). The hollow dots are predictions under an assumption of no structural break in Venezuela in 1993 (i.e., when \( b_1 = b_2 = 0 \))