Compulsory Voting and Income Inequality*

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Abstract

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 both 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. To address them, this paper focuses on the phenomenon of compulsory voting – legal requirements that compel citizens to vote in elections. Specifically, by applying a new statistical tool called “synthetic control method” to a rare case of abolishing compulsory voting in Venezuela, we show that *not* forcing people to vote – and a resultant sharp drop in voter turnout – yielded a more unequal distribution of income. Our evidence supports Arend Lijphart’s claim, forcefully 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 both 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 participate. Specifically, by applying a new statistical tool called “synthetic control method” (Abadie, Diamond, and Hainmueller 2010, 2012; and Abadie and Gardeazabal 2003) to a rare case of abolishing compulsory voting in Venezuela, we examine whether compulsory voting reduces income inequality.

Our investigation provides empirical evidence for a well-known proposition Arend Lijphart forcefully advanced in his 1996 presidential address to the American Political Science Association (Lijphart 1997). For Lijphart, 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 (p.1). Drawing on evidence from an array of studies, Lijphart contends, “Low voter turnout means unequal and socioeconomically biased turnout … [and] unequal participation spells unequal influence” (p.2). He then argues, “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. The normative foundation of his argument is that, in a democracy, the preferences of every citizen should have equal weight in electing representatives and determining policy.

Despite its normative importance and practical relevance in policy debates, Lijphart’s proposition has not come under rigorous empirical scrutiny. Numerous studies using individual-level public opinion data and/or aggregate electoral data investigate the impacts of voter turnout (or other measures of electoral participation) on various outcome variables. Most of these studies, however, face various methodological shortcomings in making valid causal inference, which we will discuss in a later section. Furthermore, to
the best of our knowledge, there are only a few studies directly examining the impacts of compulsory voting on economic or social outcomes. The only existing study we are aware of that properly addresses methodological concerns and estimates the consequences of compulsory voting is a recent work by Fowler (2013), which uses the synthetic control method to estimate the causal effects of the introduction of compulsory voting in Australia on pension spending.

Our approach is similar, but there are some important differences. First, whereas Fowler focuses on the introduction of compulsory voting in Australia in 1924, we focus on its abolishment in Venezuela in 1993. Second, whereas limited available data allow Fowler to use only three data points (1910, 1920, and 1930) to estimate the effects of compulsory voting, we use longer time-series data for more accurate estimation. Third, whereas Fowler uses pension spending as an outcome variable, we use income inequality. Finally, we use archival records and survey data to scrutinize the “causal process observations” (Brady and Collier 2004, 2010; Brady, Collier, and Seawright 2006) and to examine the validity of a key assumption in our causal interpretation – an important qualitative procedure in any causal analysis (Dunning 2008a, 2012).

In the following, we first examine Lijphart’s proposition, making explicit its assumptions and logic, and present a testable hypothesis. Second, we explain why existing studies as well as some other potential studies based on the regression framework fail to serve as valid tests of the hypothesis. Third, we discuss how we seek to address methodological challenges by applying the synthetic control method and show the results of analysis, which suggest that not forcing people to vote produced a more unequal distribution of income in Venezuela. After showing the statistical estimates, we turn to qualitative analysis. Specifically, we discuss that our theoretical model presented in Section 2 is consistent with Venezuelan political history, although there remain some ambiguities. Based on archival and survey research, we also argue that the sharp increase in income inequality after abolishing the compulsory voting is most likely an unintended consequence for politicians. This finding validates a key underlying assumption of our hypothesis and improves our confidence in making a causal interpretation. The final section concludes.
2. Why Compulsory Voting Affects Income Inequality

Lijphart (1997) argues that compulsory voting is an effective remedy for biased turnout, which in turn brings about biased influence among eligible voters. Among many possible observable implications of his theory, in this study, we focus on examining the following:

**Hypothesis:** Income is more unequally distributed where voting is voluntary than where it is compulsory.

In other words, the abolishment of compulsory voting in Venezuela, which we examine in this study, is expected to bring about more unequal distribution of wealth in society. The logic and some underlying assumptions are illustrated in Figure 1.

![Figure 1](image_url)

The first assumption is that when voting is voluntary, wealthier people vote at higher rates than do poorer ones. This pattern has been found to be empirically robust over time and across most countries (Jackson, Brown, and Wright 1998; Leighley and Nagler 1992; Singh 2011; Tingsten 1937). The top panel in Figure 1 shows a hypothetical distribution of voters, arrayed from the poor to the rich across the horizontal axis, with the vertical axis representing their utility from voting. The potential benefits of voting are assumed to be constant across citizens, but the costs are higher – and thus, overall utility is lower – for the poor. These costs can be informational (Downs 1957; Matsusaka 1995). For example, since poverty corresponds everywhere with low education levels, the efforts required for voters to gain information about candidates and policy platforms should be larger for the poor than the rich (Gordon and Segura 1997). The costs can also be logistical. Poor would-be voters may lack access to transportation

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1 In their research study, Kasara and Suryanarayan (2013) show that this pattern does not always hold cross-nationally.

2 It is necessary to assume that everyone has an equal chance to enjoy the same benefits of voting for the purpose of understanding a logical connection between voter turnout and policy influence. In other words, it is tautological to assume that wealthier citizens enjoy greater benefits of voting (because they vote).
to get to the polls, or flexible work schedules, that allow wealthier citizens more easily to cast their ballots. For these reasons, the scatter plot in the top panel, which uses simulated data, posits lower expected utility of voting for the poor than the rich.

The second assumption is that compulsory voting increases voter turnout. Prior studies have shown that voter turnout is high as long as the compulsory voting law is properly enforced with substantial punishments for not voting (Fowler 2013; Jackman 2001). In Figure 1, when voting is compulsory, everyone is assumed to vote. When it is voluntary, citizens go to the polls when the net utility is positive – above the horizontal line in the top panel.

The third assumption is that the poor prefer more redistribution than the rich do, such that the poor-rich axis in Figure 1 is a proxy for an ideological space from left to right. It should be no controversy to assume that the poor support policies that yield disproportional benefits to the poor vis-à-vis the rich. Such policies include progressive taxation on incomes and comprehensive tax deductions for the poor. Perhaps more importantly, given that the economically less advantaged people are more susceptible to economic growth (and to job market fluctuation) and more depending on public sector jobs (including jobs created by public works), the poor vis-à-vis the rich should have stronger preference over larger government with larger public spending.

Under these reasonable assumptions, how does compulsory voting affect the distribution of policy preferences? If everyone votes, the distribution of economic status is the one with the shaded area in the bottom panel of Figure 1. The median voter position is at M. When voting is voluntary, the relatively higher non-voting rate among poor citizens yields a richer median voter (at M’>M). Therefore, compulsory voting, by diminishing the wealth bias to voluntary non-voting, should shift the redistributive preferences of the median voter back toward the median in the full population.

The fourth assumption is that the elected government responds to the median voter’s preference. In the literature of American politics, some scholars argue that the U.S. government responds to the opinions of rich voters (Bartels 2010; Gilens 2005), while others claim that the government is equally responsive to the preferences of the
poor and the rich (Soroka and Wlezien 2008). This debate is, however, not necessarily relevant to the logic behind Lijphart’s proposition. If the government’s responsiveness is biased in favor of the rich (i.e., if the government does not simply respond to the median voter’s policy preference), what is theoretically expected under a voluntary voting system is that the government chooses a policy preferred by a voter at K’, which is on the right side of the median voter’s position at M’. If voting is compulsory and the government’s responsiveness is biased in the same way, then the government chooses a policy preferred by a voter at K, which is again on the right side of the median voter’s position at M. As long as K’ is on the right side of K, then compulsory voting is expected to flatten the after-tax distribution of income in society.

The discussion above implies that the government’s relative responsiveness to the rich vis-à-vis the poor is independent of whether voting is mandatory or voluntary. If the government is more likely to respond to the rich under a compulsory voting system than under a voluntary voting system, there is no guarantee that K’ is on the right side of K. On the other hand, if the government is more likely to respond to the poor under a compulsory voting system, the gap between K’ and K may be expanded. In either of these cases, it is difficult to claim that the voting rule has a causal effect on the distribution of income, because politicians’ motivation to affect the distribution of income (i.e., our outcome variable) through the change in voting rule (i.e., our treatment variable) could be a confounding factor producing the observed non-causal association between voting rule – whether compulsory or voluntary voting – and income inequality.

In sum, there are four underlying assumptions of Lijphart’s hypothesis. Whereas the first three are less controversial, the validity of the fourth assumption needs an empirical investigation. Thus, we also examine the following:

**Assumption:** Politicians do not intend to make voting compulsory or voluntary in order to change the distribution of income.

We undertook an extensive search for written materials on the electoral reform in

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3 Also see a study by Kelly and Enns (2010), which attempts to resolve the contradictory findings in the literature.
Venezuela, as well as for a more recent, similar reform in Chile. As we recognize that politicians may not state their full intentions in advancing electoral reform, we also examine whether voters’ attitudes toward economic inequality and for compulsory voting are correlated. If they are not, it is unlikely that politicians’ efforts to change the rules around compulsory voting are manifestations of broader agendas to affect the distribution of income.

3. Methodological Challenges

How do we test the hypothesis introduced in the previous section? It may seem that the most straightforward way is to estimate the impact of voting rule – compulsory or voluntary – on income equality using regression. Before discussing its methodological problems, we first discuss the difficulty of using voter turnout as a treatment variable.

3.1 Voter Turnout as a Treatment Variable?

Undoubtedly, voter turnout is one of the most widely studied variables in political science, and many scholars have examined how turnout affects electoral and policy outcomes. Fowler (2013) argues, however, that these regression-based studies to estimate the consequences of voter turnout are likely to be biased for various reasons, including the failure to control for confounding variables, reverse causation, and model misspecification, and that the most convincing studies on the effects of turnout are those focusing on small exogenous/random shocks that affect voter turnout.\(^4\) We agree that such shocks, unrelated to the causal effects of voter turnout on outcome variables (i.e., electoral and/or policy outcomes), could be used as leverage to make valid causal inference.

For the purpose of understanding the impact of voter turnout under compulsory voting, however, Fowler makes another valid criticism. The studies leveraged by exogenous shock, he notes, “address a separate question about the effects of marginal changes to voter turnout, but they do not assess the effects of near universal voter turnout. If marginal voters are unrepresentative of the entire population of nonvoters, then these

\(^4\) See Fowler (2013) for a list of these studies. Also see Horiuchi and Saito (2009).
studies do not speak to the counterfactual question at hand” (Fowler 2013, 3). We agree. The estimated effects of a variation in voter turnout influenced by exogenous shocks (e.g., the “as-if” random timing of election, the amount of rainfalls on the Election Day) are the “local” average treatment effects (LATEs) that are irrelevant to Lijphart’s proposition; and more specifically, our hypothesis introduced in Section 2.

To estimate the theoretically relevant LATEs – whether the level of voter turnout *influenced by whether voting is compulsory or mandatory* affects the distribution of income – we considered the following regression model with instrumental variables:

\[ y_{it} = \alpha + \beta \cdot x_{it} + \gamma \cdot z_{it} + \sigma_i + \tau_t + \epsilon_{it}, \]  

(1)

where \( y_{it} \) is the degree of income inequality in country \( i \) in year \( t \); \( x_{it} \) is the level of voter turnout in the most recent election held in country \( i \) as of year \( t \); \( z_{it} \) is a set of control variables correlated with both \( y_{it} \) and \( x_{it} \); \( \sigma_i \) and \( \tau_t \) are country fixed effects and year fixed effects, respectively; and \( \epsilon_{it} \) is a random disturbance term. Potential instrumental variables are a dummy variable indicating whether a country has an enforced compulsory voting system (\( w_i \)) and a dummy variable indicating whether a country has an unenforced compulsory voting system (\( v_i \)).

An important note is that these instrumental variables are country specific and *time invariant* during the period of investigation – i.e., the period in which the cross-national data on income distribution and voter turnout are available (i.e., after the 1960s). This is due to the fact that in almost all countries, the values of these instrumental variables do not change within each country for many decades. Specifically, those countries introduced compulsory voting did so many decades ago (e.g., Australia in 1924, Belgium in 1919, Latin American countries in the 1930s to 1950s), and almost all of them – except a few recent exceptions, which we will focus on in our paper – have neither abolished it nor changed from an enforced system to an unenforced system (or

\footnote{These variables are used in the study on the impacts of compulsory voting on income inequality (Chong and Olivera 2008). There are some other ways to specify instrumental variables for this potential analysis, but the variable specification is irrelevant here.}
vice versa). No country introduced a compulsory voting system during the last four decades. The last country to do so was Nauru in 1965.

Under the following two assumptions, regression with instruments yields a consistent estimate of $\beta$. First, these instruments are strongly correlated with the treatment variable ($x_{it}$) conditional on a set of observable covariates. Second, they are uncorrelated with $\varepsilon_{it}$ conditional on the covariates. The first assumption is satisfied logically and empirically (see Jackman 2001 and Lijphart 1997 for a review). The second assumption is not satisfied, however, because these instrumental variables should be correlated with lagged treatment variables (i.e., $x_{it-1}, x_{it-2}, x_{it-3}$...) not included in the model.\footnote{We cannot add these lagged variables as additional endogenous variables unless we have a sufficient number of instrumental variables for them. When the number of instrumental variables is smaller than the number of variables to be instrumented, such a model is simply unidentified. An alternative specification may be to take the average value of each observable variable and run a 2SLS regression using cross-national data. The same problem, however, applies.}

### 3.2 Compulsory Voting as a Treatment Variable?

After recognizing that regression with instruments is not applicable in our study, we then considered simpler reduced-form regression.\footnote{We could consider a model with a subscript $t$ for panel data analysis, but a fundamental problem we discuss here remains the same.}

\[
y_i = \alpha + \beta \cdot w_i + \gamma \cdot z_i + \varepsilon_i
\]  
(2)

This is essentially a model Chong and Olivera (2008) use in their study, which estimates the impacts of compulsory voting on income distribution using cross-national data. It drops a variable measuring voter turnout ($x_i$) and directly estimates the impact of compulsory voting ($w_i$) on income inequality ($y_i$) after controlling for a range of
Although this may be the seemingly most straightforward approach to test our hypothesis, the regression analysis without valid instruments or other identification strategies for causal inference is vulnerable to common problems, including omitted variable bias, reverse causality, model misspecification, and so on.

In this model, moreover, there is another serious problem known as “post-treatment bias” (Rosenbaum 1984). To estimate the impact of compulsory voting on income inequality, Chong and Olivera (2008) add control variables measuring the levels of economic development, educational attainment, and democracy. These variables, which are measured for their period of investigation (specifically, 1960-2000), are causally posterior to their treatment variable – i.e., whether a country adopts a compulsory or voluntary voting system before 1960. Adding variables that are, at least in part, consequences of a treatment variable in a standard regression framework would introduce the post-treatment bias. Although this type of bias was not sufficiently acknowledged in the political science literature until recently (King 2010), a growing number of studies now explicitly address the problem, and either drop post-treatment variables in regression analysis (e.g., Horiuchi, Komatsu, and Nakaya 2012) or cope with it based on more sophisticated analysis (Imai et al 2011).

Justifiable “pre-treatment” covariates are variables measuring the determinants of each country’s decision to make voting compulsory or voluntary. A challenge is to undertake archival research to understand the origins of compulsory voting in those countries that introduced it many decades ago. Even such extensive research digging into historical materials from various countries in various languages were possible, it would be difficult, if not impossible, to quantify some key determinants and incorporate them in regression analysis.

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8 We add just one variable for compulsory voting for simplification. Whether to add two (or more) variables related to compulsory voting is irrelevant to our discussion.

9 In regression analysis, we estimate the marginal effect of a treatment variable holding other variables constant. If at least one of the control variables is a consequence of the treatment variable, it is illogical to assume a variation in the treatment variable without any variation in the post-treatment variable.
In sum, the standard regression framework is not applicable to test our hypothesis. A fundamental challenge in studying consequences of compulsory voting is that this particular institution was adopted many decades ago and has rarely changed since. As Przeworski (2004) argues, estimating the impacts of such time-invariant institutions is not straightforward despite a large volume of studies claiming the causal effects of institutional arrangements.

4. Does Compulsory Voting Affect Income Inequality?

Although the regression-based approach is invalid to understand whether the compulsory voting affects the distribution of income, it does not mean that it is impossible to answer our question. In this section, we first introduce a new approach recently proposed by Abadie, Diamond, and Hainmueller (2010, 2012) and Abadie and Gardeazabal (2003) – the synthetic control method. We then show the results of our estimation and various placebo tests.

4.1 Synthetic Control Method

If we want to examine the causal effects of a specific policy change or event, in theory, we need to compare the outcome variables between the “treated” case and a comparable “control” case, in which the assignment of the treatment did not occur. In practice, however, finding such a comparable case – a case in which all relevant pre-treatment conditions are similar to the treated case but the treatment assignment was absent – is nearly impossible. For example, we cannot find any country that was almost identical to pre-collapse Soviet Union but did not experience the collapse of the regime in 1991.

To address this methodological limitation, Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010, 2012) suggest making a counterfactual “synthetic control” case. Specifically, they suggest using pre-treatment variables (“predictors”) for observation units (e.g., countries) in which the assignment of a treatment (“an intervention”) did not occur and statistically generate the weighted
average of an outcome variable for these units.\textsuperscript{10} If we can successfully make a synthetic control case, which is a \textit{counterfactual} for the specific case of research interest, the trends of the outcome variable should be almost identical between the treated case and the synthetic control case \textit{before} the intervention. If the intervention does indeed have a causal effect on the outcome variable, the trends should then diverge \textit{after} the intervention. Abadie, Diamond, and Hainmueller (2010, 2012) demonstrate the validity of this approach by examining the impact of a large-scale tobacco control program that California implemented in 1988 on annual per-capita cigarette sales in California, and the impact of German reunification in 1990 on economic growth in Germany. Montalvo (2011) takes the same approach to study the electoral impact of the terrorist attacks of March 11, 2004, in Madrid.

Fowler (2013) also uses this approach to examine the causal effects of introducing compulsory voting in Australia in 1924. In our study, the “treated” case is Venezuela’s abolition of compulsory voting. Our results are consistent with Fowler’s, but we believe that our case is even better suited to estimate the consequences of compulsory voting because we can make a synthetic control using better data with a longer time period before and after the intervention and more predictors. Richer data allow us to produce more reliable estimates and undertake various placebo tests.\textsuperscript{11}

With regard to the timing of intervention, we need to fill in the details. Compulsory voting was enshrined in Venezuela in the Constitution of 1961 and remained constitutionally required until the adoption of the new Constitution of 1999. Levels of enforcement of the constitutional requirement, however, were not constant throughout the whole period. The requirement to vote was enforced consistently for the first couple of decades. The specific sanctions were provided for in the electoral laws and included

\textsuperscript{10} In this paper, we avoid explaining technical details of the synthetic control method, including how to assign weights to generate a valid synthetic control case. For these, see Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010, 2012).

\textsuperscript{11} Because the number of data points is too small (i.e., only three), Fowler (2013) could not conduct an “in-time placebo” test, one of the two placebo tests Abadie, Diamond, and Hainmueller (2010, 2012) suggest.
restrictions on contracting with public entities, on holding public office, on securing a passport, or enrolling in a state university, for a period of six months after the election in which a citizen did not vote (Rosales 1986). In the mid-1980s, public disillusionment with Venezuela’s dominant political parties increased, with broad sentiment that the parties were unable to formulate effective economic policies and perceptions of widespread corruption. A manifestation of this disillusionment was an increase in demands that citizens ought to have the right to choose whether to vote or not. During this period, the electoral authorities scaled back enforcement of the sanctions associated with compulsory voting, and finally, the Organic Suffrage Law of 1993 eliminated the sanctions (Molina and Perez Baralt 1995, 1996). In the Constitution, the duty to vote remained until the promulgation of the new charter in 1999, but the requirement was enforced more aggressively during the 1960s and 1970s than subsequently, and backed by no legal sanctions from 1993 on.\footnote{After the abolishment of enforced compulsory voting in Venezuela in 1993, voter turnout sharply dropped. In Parliamentary elections in Venezuela, voter turnout had been consistently over 70\% previously, but dropped to 50.0\% in 1993. For these reasons, we focus on 1993 as the critical year when a major policy shift occurred.}

The focus of our analysis is the impacts of the removal of legal sanctions for non-voting on the net (i.e., after-tax) Gini coefficient, a commonly used measure of income

\footnote{We thank Jose Molina (personal communication, January 21, 2013) for this account of the subtle changes in the \textit{de jure} and \textit{de facto} status of compulsory voting in Venezuela over time.}

\footnote{All voter turnout statistics we use are available at the website of the International Institute for Democracy and Electoral Assistance (http://www.idea.int/). The denominator of voter turnout is the voting-age population (VAP). According to the website of the International IDEA (http://www.idea.int/vt/countryview.cfm?id=236), accessed on March 30, 2013, voting in the 1993 Parliamentary election in Venezuela was compulsory, but this is not an accurate description.}
inequality.\textsuperscript{14} The period of investigation is from 1988 to 1998, and thus the number of years covered in our analysis is 11. This period includes three elections – the 1988 election, the last election before the elimination of sanctions, the 1993 election right after the elimination, and the 1998 election. We do not extend the longer period before and after 1993, because movement in contextual factors further from the treatment likely drove “post-treatment” variations in income inequality that was unrelated to the intervention itself.

To make synthetic Venezuela, we use the following predictors.\textsuperscript{15} First, we use the net Gini coefficient in 1993 and 1998, and its average value during the entire pre-intervention period (i.e., 1988 to 1993). These three variables are essential to improve the fit of synthetic Venezuela’s trajectory of income inequality vis-à-vis that of real Venezuela. We also use the average voter turnout of elections held between 1988 and 1992 (a year before the intervention year). In theory, synthetic Venezuela must have similar voter turnout before the intervention. We also include the purchasing power parity adjusted GDP per capita, the government (consumption and investment) share of GDP, Polity Score (a commonly used measure of democracy), fuel exports as the percentage of merchandise exports, ores and metal exports as the percentage of merchandise exports, a dummy variable for former Iberian colonies, a dummy variable for Latin American and Caribbean countries.\textsuperscript{16} The first three are expected to have strong correlation with income inequality across countries. The next two reflect the extent to which economies are

\textsuperscript{14} There exist several datasets for income inequality, but we use the Standardized World Income Inequality Database, Version 3.1, produced by Solt (2009). Based on many existing datasets, which are not directly comparable, Solt developed a new dataset covering greater coverage across countries and over time, by maximizing the comparability across various indicators.

\textsuperscript{15} When there are missing values in a given variable, we filled them in by linear interpolation (but not extrapolation). Since we take the average values for most predictors during the pre-intervention period, this interpolation does not introduce bias.

\textsuperscript{16} The data source for these variables is the Quality of Government Dataset (version 6 April 2011), available at http://www.qog.pol.gu.se (accessed on December 19, 2012).
dependent on extractive mineral exports, characteristics widely regarded to shape the politics of economic redistribution (Ross 2001; Dunning 2008b). The last two variables capture cultural, historical, and geopolitical context. For these predictors, we use the averages of 1988 to 1993. Finally, we use whether or not a country used an enforced compulsory voting system from 1988 to 1992. To make a counterfactual case for Venezuela, it is important to use this predictor, as well.

A remaining question is which countries we should use as a “donor pool” for synthetic Venezuela. One could simply add as many countries as possible, but this approach can give unreasonably larger weights to countries that happen to have similar trajectories of income inequality during the pre-intervention period but do not share historical, political, and economic contexts with Venezuela. To make sensible comparisons, we limit the donor pool to former Iberian colonies (with similar historical and institutional contexts), and/or Latin American and Caribbean countries (with similar regional economic contexts), and/or countries with enforced compulsory voting.

4.2 Results

The results of estimation are shown in Figure 2.

[Figure 2 about here]

The left panel shows the trajectory of net Gini coefficient for Venezuela (the black line) and synthetic Venezuela (the gray line). As expected, it shows that the level of inequality was similar between the factual and counterfactual cases until 1993 but the level of income inequality sharply increased only for the factual case.

17 See footnote 13 for the source of information on compulsory voting.

18 Once we add lagged outcome variables to predict the pre-intervention trajectory of an outcome variable, the gap between a treated case and a synthetic control case tends to be smaller. Yet, a set of countries used on make the synthetic control may not necessarily be theoretically relevant. This is a potential methodological issue that should be properly addressed in any causal analysis based on the synthetic control method.

19 Some of these countries are dropped from analysis because of the missing values in the outcome variable (i.e., the net Gini coefficient).
Before discussing the result of a placebo test shown in the right panel of Figure 2, we show the list of countries used to make synthetic Venezuela (Table 1) and the balance of predictors (Table 2).

[Table 1 and Table 2 about here]

The countries (and the weight in parentheses) used to make synthetic Venezuela are Peru (0.057), Trinidad and Tobago (0.489), and Uruguay (0.454). This result largely makes sense. Trinidad and Tobado is Venezuela’s neighbor sharing many socio-economic conditions. Like Venezuela, oil exports are its main source of state revenue, so the movements of world oil markets shape prospects for redistributive economic policies. Peru and Uruguay are South American neighbors with shared colonial and historical characteristics. Like Venezuela, Peru’s economy relies substantially on extractive (some oil, but also other minerals) exports. Uruguay and Venezuela enjoyed similar levels of overall economic prosperity through much of the 20th Century. Voting is also compulsory in these countries. The mean values of the predictors are similar between Venezuela and synthetic Venezuela except some context-specific predictors, such as the fuel exports as the percentage of merchandise exports.

To assess the significance of the estimated treatment effect, we conducted a placebo test suggested by Abadie, Diamond and Hainmueller (2010). Specifically, we applied the synthetic control method to estimate the effect of intervention in 1993 to every other country in the donor pool listed in Table 1. That is, for each country, we created a synthetic control using the same set of predictors and examined whether the trajectories of income inequality diverged between the factual and counterfactual cases after 1993. The results are shown in the right panel of Figure 2. The vertical axis indicates the difference in the net Gini coefficient between the factual (treated) case and

\[\text{20 We dropped Venezuela to make a synthetic control case of other country.}\]

\[\text{21 Following Abadie, Diamond and Hainmueller (2010), in making this figure, we dropped some countries with the poor fit of the net Gini coefficient before 1993. Specifically, we excluded a country if its Root Mean Squared Prediction Error (RMSPE) is more than two times that of Venezuela.}\]
the counterfactual (synthetic control) case. The grey lines denote the gaps for all other countries except Venezuela, while the black line is for Venezuela. It clearly shows that Venezuela experienced the unusually shape increase in the net Gini coefficient after the intervention. The probability that Venezuela’s gap becomes the largest among all thirteen countries included in the right panel of Figure 1, by chance, is $1/13 = 0.077$. This is equivalent to say that the effect of the 1993 intervention in Venezuela on income equality is significant at 10% level.

Figure 3 shows the results of another placebo test called the “in-sample placebo test” (Abadie, Diamond and Hainmueller 2012).

[Figure 3 about here]

In this analysis, the abolishment of compulsory voting in Venezuela is counterfactually assumed to have happened in 1988, the last election held under enforced compulsory voting. Since this does not reflect the truth, the gap in the net Gini coefficient between Venezuela and synthetic Venezuela should not diverge after 1988. The left panel shows that the trajectories are indeed similar between Venezuela and synthetic Venezuela before and after 1988. The right panel shows that the slightly widening gap between Venezuela and synthetic Venezuela after 1988 is not unusual among the countries used in this analysis.

Given the results of these two placebo tests, we are inclined to believe that the intervention in 1993 in Venezuela caused an unusually sharp increase in income inequality during the post-intervention period. We are not fully sure, however, whether the “intervention in 1993” that matters is indeed the abolishment of compulsory voting. One might rightly note that the end of compulsory voting was not the only major electoral reform to hit Venezuela in 1993. From 1958-1988, the Chamber of Deputies had been elected by closed-list proportional representation (PR), using the country’s states as districts. In 1993, Venezuela switched to a mixed-member system resembling Germany’s mixed-member proportional (MMP) system, with half the seats contested by plurality in single-member districts (SMDs) while the other half were allocated to achieve overall proportionality.
Is there reason to think the shift to the MMP system affected income inequality? Iversen and Soskice (2006) make the case that PR elections encourage, and SMDs discourage, progressive redistribution. There are reasons to be skeptical about applying Iversen and Soskice’s model to the Venezuelan case, however. First, although Venezuela’s reform created SMD competition for half of the Chamber’s seats, the overall distribution of seats remained fully proportional, so the Duvergerian logic of SMD competition on which Iversen and Soskice’s model depends would not apply. Second, in the Iversen and Soskice’s account, the redistributive effect is driven by differences in the formation of coalition governments in multi-party versus two-party parliaments, whereas Venezuela was, and remains, a presidential system.

Nevertheless, given that the end of compulsory voting coincided with another major electoral reform, it is worth taking seriously the prospect that the reform could affect economic inequality. To test this idea, we ran another placebo test, on Bolivia, which adopted a MMP system similar to Venezuela’s in 1994, and first held elections using the system in 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 Bolivia moved from a pure, closed-list PR system with regional districts to a mixed-member compensatory system.

The results are presented in Figure 4.

[Figure 4 about here]

The trajectories are similar between Bolivia and synthetic Bolivia before 1997, but the gap tends to expand after the electoral reform. Yet, as the right panel shows, the magnitude of increase after 1997 in Bolivia is not necessarily one of the most unusual countries. This result suggests that the electoral reform from a closed-list PR system to a MMP system in Venezuela in 1993 may also have some effects but is unlikely to be the most relevant factor explaining Venezuela’s sharp increase in income inequality after 1993.

In sum, all these results suggest that the removal of legal sanctions for non-voting in Venezuela in 1993 fueled the observed increase in income equality after 1993. But we
are also obliged to ask whether the political history of the case is consistent with the theory that motivates our analysis, and with the inferences that follow from it.

5. Qualitative Analysis of Causal Mechanisms

Two questions about the sequence of events in Venezuela are critical to evaluating our results so far. First, did electoral politics contribute to the spike in economic inequality in the mid-1990s? Specifically, was there a shift in the median voter’s position and, consequently, any shift in policies that diminished the poor’s economic status vis-à-vis that of the rich? Second, if politics drove these shifts, was compulsory voting merely epiphenomenal, or did it have an independent causal impact? To address the first question, we examine the politics and policies during the presidencies of Carlos Andres Perez (1989-1993) and Rafael Caldera (1994-1999). On the second question, we examine the intentions behind the set of electoral reforms that ended compulsory voting in Venezuela. This section of the paper also examines a new case of abandoning compulsory voting, Chile in 2011, where the motivations resembled those in Venezuela two decades earlier, but in which potential effects on economic inequality are not yet evident. These qualitative inquiries are aimed at understanding the causal processes underlying our hypothesis and strengthening our causal interpretation of quantitative findings.

5.1 Did the Median Voter’s Position Shift Right?

Our particular interest here is the Venezuelan political party system, how preferences for economic redistribution mapped onto it, and how, in turn, the abolishment of compulsory voting (and a resultant sharp decline in voter turnout) might have shifted the political balance. The period from which the data in our analysis are drawn, 1988-1998, was one of enormous turbulence, economically and politically, following on decades during which the country was 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 1997; Crisp 2000; Karl 1997). For our purposes, the top panel of Figure 5 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.

[Figure 5 about here]

In each of the four presidential elections from 1973-1988, AD and COPEI took more than 85% of the votes between them – and three times above 90%. Throughout the long period in which voting was compulsory, then, Venezuelans were familiar with a set of options with established locations on a left-right ideological spectrum. The center-left AD, moreover, had the strongest claim to Venezuela’s median voter, having won five of seven presidential elections from 1958-1988, and three of four in the 1973-1988 period.

By the time compulsory voting was abolished in 1993, Venezuela had experienced a period of economic crisis and political discontent under AD President Carlos Andres Perez. The contest for COPEI’s presidential nomination in 1993 split the party. Rafael Caldera, who had founded the COPEI in the 1950s and served as president from 1968-1973, 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 move the COPEI to the left failed, and he lost the nomination. He then bolted the party and ran for president as the head of a coalition of parties spanning the ideological spectrum (Buxton 2003; Di John 2005).

The second panel of Figure 5 presents a spatial representation of these events, based on a static interpretation of Venezuelan party competition. By this account, the AD, discredited by Perez’s presidency, hemorrhaged voters to its main competitors. The Causa Radical was one beneficiary, but the big winner was Caldera, who had left his former party on the right flank, and captured the presidency with a plurality of 31%. This account of events is consistent with the spatial model outlined in Figure 1, in which the end of compulsory voting produced a class bias in abstention that pushed the median voter right, diminishing demand for progressive redistribution.

A fuller account of the politics of the period, however, compels us to acknowledge that the fit of theory to data may not be quite so straightforward. When he was elected in 1988, Carlos Andres Perez had previously served as president as well –
from 1973-1978, during an oil boom. Venezuelans associated Perez with generous (perhaps exorbitant) government spending, and he did little to dissuade this expectation during his 1988 campaign despite the fact that Venezuela was, at the time running substantial deficits and facing rising inflation (Buxton 2003). Upon assuming the presidency, Perez immediately imposed a package of economic policies described as the *Gran Virage* (Great Turnaround), including decreased tariffs, the introduction of a value-added tax, the elimination of government price controls, and the reduction of subsidies on basic goods, utilities, and most famously, fuel. Price increases on the last were passed on to citizens in the form of 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).

Perez’s approval ratings dropped from about 50% to 35% during his first year in office, and presidency never fully recovered (Stokes 2001). His presidency was threatened by military coup attempts in February and again in November of 1992, neither successful, but the first of which made a media celebrity out of a young colonel with a gift for populist rhetoric: Hugo Chavez Frias. In the last year of his term, Perez faced corruption charged over the misappropriation of government funds. Abandoned by his former allies in Congress, he was removed from office in 1993 and replaced with an interim president.

This account of Perez’s presidency, emphasizing his unexpected lurch to the right on economic policy, suggests that, by the time of the 1993 election, perhaps the AD was regarded as being to the right of Caldera, who ran a campaign of excoriating Perez’s neoliberalism (Buxton 2003). The bottom panel of Figure 5 illustrates that possibility. The fit between this model of ideological competition and the one described in Figure 1 is more ambiguous. It does not preclude the prospect that, in the 1993 electorate, Venezuela’s median voter had shifted right, but neither does it provide evidence to that effect.

### 5.2 Did Any Policy Shift Increase Income Inequality?

Without panel survey data from the 1988-1993 period, we cannot determine for certain whether the median voter’s preference indeed shifted right after the abolishment of
compulsory voting. We note, however, that there was a shift to even more severe austerity under Caldera, which drove the dramatic increase in income inequality after 1993. Caldera faced a massive bank crisis immediately upon taking office, and opted to bail out the failing financial institutions at a cost equivalent to 18% of GDP, draining funds from government spending in other areas (Di John 2005, p.113). The end of subsidized credit also generated thousands of business bankruptcies. By 1995, Caldera accepted a $1.4 billion loan from the International Monetary Fund, the conditions for which included privatizations of state-owned enterprises, further cuts to subsidies on fuel and basic goods, and a substantial increase in the value-added tax (Tanzi 2000). The fiscal squeeze, exacerbated by persistently low oil prices, lasted throughout Caldera’s term. Budgets for 1997 and 1998 were based on forecasts of crude prices of $16/barrel, but prices of $11/barrel decimated government coffers (Buxton 2003).

Figure 6 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.\footnote{The data source for the government share as the percentage of GDP is Heston, Summers and Aten (2009).}

The vertical bars show the 90% confidence interval for the Gini estimates (Solt 2009). Government spending did decrease during Perez’s term, from just above 22% in 1989 to just above 20% in 1993. But the 1993 level is barely below the level from 1988 – the year before Perez’s Gran Virage! Meanwhile, 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 was seen as the president promoted drastic liberal reforms, but neither the government share of GDP nor the Gini coefficient changed substantially. The pattern only becomes dramatic under Caldera, when government spending dropped dramatically and monotonically, with corresponding increases in our measure of inequality. The negative correlation (−0.91) between these variables strongly suggests that the post-1993 austerity policies were an
important determinant of the increase in inequality. Throughout the period under consideration, moreover, the reduction in inequality from transfers (the difference between the gross/ market Gini coefficient and the net Gini, post-transfers) was relatively stable, even as both indices rose sharply in the Caldera years. This implies that government spending has a strong impact on income inequality through its effect on employment and wages, even beyond the effects of transfer programs (Stack 1978).

Were Caldera’s policies a response to a shift in the median voter’s position? This is a difficult question to answer empirically. The best survey-based study of abstention in the 1993 election (Molina 1995, p.33) indicates that, as of July 1993, poorer respondents were three percent less likely to express an intention to vote in the election five months away than were wealthier ones. But expressed intention is an imperfect measure of action and, at any rate, the matter of whether relative abstention rates across socioeconomic groups changed from 1988 to 1993 is not addressed. With respect to policy, although Perez’s Gran Virage is characterized as a pivotal moment in accounts of Venezuelan politics of the period, as we have shown, neither the share of the economy accounted for by government spending nor conventional measures of income inequality changed under the AD government. The economic data suggest that it was Caldera’s policy that exposed, more systematically, the vulnerability of the poorest Venezuelans.

It will come as no surprise to students of Venezuelan politics that the 1988-1998 period is difficult to pin neatly onto theory, and we would be overreaching to claim that ours fully encapsulates that chaotic time, but the theory we seek to test in this paper seems to be consistent, to a large extent, with a spatial model of Venezuelan party competition in which decreased participation, with class bias, moved the center of gravity in the electorate to the right, triggering policy changes that increased inequality.

5.3 Why Was Compulsory Voting Abolished?

The synthetic control method’s main inferential advantage over most previous studies is in providing a more plausible counterfactual case for the trajectory of economic inequality in Venezuela if it had not abolished compulsory voting. In large part, that follows from making use of information about the trajectories of countries that comprise our “synthetic Venezuela,” which did not experience electoral reform.
As we discussed in Section 2, however, our causal interpretation also depends on a key 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 constructed by the synthetic control method – our “synthetic Venezuela” – would not provide an appropriate point of comparison for the real Venezuela. 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. The perspectives of supporters of progressive redistribution, social scientists, and constitutional law scholars were included. A question is: Was the potential impact of eliminating compulsory voting on economic inequality central to the debate among them?

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 that “electoral 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.23 Among the COPRE’s list of reports, we identified eleven volumes that address subjects of electoral reform and citizen participation. Reviewing those, we found no discussion of

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23 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).
the prospect that ending compulsory voting would alter the level of support for progressive redistribution in the Venezuelan electorate. We also reviewed publications on electoral reform proposals from Venezuela’s Consejo Supremo Electoral (2013) from the same period, as well as assorted other academic publications, and similarly found no discussion of the redistribution hypothesis associated with compulsory voting.

The absence of dogs barking about economic inequality at first appears puzzling, given that the proposition of socioeconomic bias in voting turnout is so long established (Gosnell 1927 and 1930; Tingsten 1937). The context of Venezuelan political debate of that period, however, provides some perspectives. By the late 1980s, the principal concern of reformers was the widespread disenchantment among citizens with the extraordinary control over access to public office and state resources exercised by Venezuela’s two predominant political parties, the AD and the COPEI, and the equally extraordinary centralization of authority within those parties (Coppedge 2004). By 1991, public opinion surveys showed confidence in the parties in single digits, and parties and Congress as the least trusted of Venezuelan institutions, well below corporations, the mass media, and unions (Buxton 2001, pp.73-74). Even before Venezuela ended enforced compulsory voting, abstention in national elections began to tick up from single digits in the 1970s to the teens in the 1980s (Buxton p.59; Molina 1995; Molina and Perez Baralt 1995). The focus of reform debates in that era, therefore, was on how to revive a sense of citizen engagement and restore legitimacy to representative institutions.

In the archival sources on reform we reviewed, the sense of perceived crisis was about imperviousness of political (specifically, party) elites to citizens’ demands. Discussions about reform were about how to alter Venezuelan elections to foster a sense of citizen empowerment over politicians – a closer electoral connection between voters and their representatives. Far more attention was devoted to the establishment of direct elections for mayors and governors, to the adoption of open party lists for state

24 The COPRE reports reviewed are included in an appendix, rather than in our list of bibliographic references, as they are not cited directly in the paper. We wish to thank Aaron Watanabe, who scoured the Harvard library system for these primary sources and provided reports on their contents.
legislatures and municipal council elections, and to the creation of single-member districts to elect half the members of the Chamber of Deputies than to compulsory voting (Brewer-Carias 1989; Zambrano 1989; Paredes Pisani 1991; Rachadell 1991). These reforms had in common the establishment of a personal vote with which citizens could use the ballot to reward or punish individual politicians, rather than only their parties as corporate entities.

To the extent that compulsory voting entered these debates, it was seen as a logically consistent piece in a larger puzzle of citizen empowerment against party elites, a product of resentment against an imperious requirement, imposed by elites on regular citizens, that everyone must vote (Rosales 1986). Reviewing the package of reforms in the late 1980s that included the end of compulsory voting, Buxton (2001) describes the overall motivation as an attempt to increase citizen investment in the system. Consider the section headings in Angel Enrique Zambrano’s 1989 essay supporting, among other reforms, the abolishment of compulsory voting:

- “The search for new forms of citizen expression”
- “Maintaining the autonomy of civil society”
- “Bases of a new form of citizen participation”
- “Implementing new forms of representation” (Zambrano 1989).

Writing a decade later, Buxton (2001) notes that the reformers were surprised in 1993 when voter turnout dropped precipitously.

To sum up, 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 elites.

5.4 Do Supporters of Compulsory Voting Prefer Redistribution?

Although we found no sources suggesting that reformers expected voluntary voting to
affect the distribution of income, it is still possible that voters who supported the abolishment of compulsory voting were richer voters who opposed redistributive policies, and that politicians and academics were aware of these preferences. To investigate this possibility, we also examine surveys asking questions about the preference for compulsory or voluntary voting. We are not aware of any such data from Venezuela, but good data are available from Chile, the most recent case of abolishing compulsory voting.

In December 2011, Chile ended compulsory voting as part of a package that also made voter registration automatic. As in Venezuela, public discussion preceding the reform was preoccupied with plummeting citizen engagement with politics and spreading disillusionment with the country’s established parties. Also, as in Venezuela, some Chilean reformers expected that participation would increase once compulsory voting was abolished, but instead participation declined in the first post-reform election.

At the time of the reform, rates of electoral participation in Chile had been falling for two decades, since the reestablishment of democracy in the early 1990s, although the extent of the trend depends on how it is measured. Participation rates among registered voters had dropped from just above to just below 90%, still high by comparative standards. An idiosyncrasy in Chilean electoral law, however, held that whereas registering to vote was voluntary, once on the voter rolls, Chileans were legally required to show up at the polls. In effect, registering exposed a citizen to legal sanction that could be avoided by simply failing to join the registry. Hundreds of thousands of would-be voters exercised exactly that option during the 1990s and 2000s. As Table 3 and Figure 7 show, registered voters as a percentage of the voting age population (VAP) fell from just above 90% in 1990 to below 70% by 2011, and turnout as a share of VAP dropped correspondingly.

[Table 3 and Figure 7 about here]

Chile’s 2011 reform made registration automatic and voting voluntary. The expectation that overall turnout might rise followed from the first component of the reform, and from the sharp increase in members of the voter rolls. Instead, the downward trend in
participation accelerated in the 2012 election.25

The Chilean debate over reform was not oblivious to the redistribution hypothesis. In 2011, as the reform was working its way through Congress, a leading public policy think tank, the Centro de Estudios Publicos (CEP), published a volume devoted to whether voting should be compulsory. Advocates and opponents alike explicitly acknowledged Lijphart’s argument, then expressed skepticism that any socioeconomic bias existed in Chilean electoral participation, and shifted the focus of debate to whether or not state sanctions to compel voting constitute a justifiable violation of individual liberty (Sierra 2011; Chaqui 2011). The disposition of Chilean legislators toward the 2011 reform also supports the proposition that support for compulsory voting and for economic redistribution were unrelated in Chile. All parties in Congress supported the reform legislation and the vote on final passage in the Chamber of Deputies was 106 in favor to 1 against (Chamber of Deputies 2011). In short, the content of the reform debate, and the behavior of politicians, both suggest that attitudes toward economic equality and toward compulsory voting were orthogonal in Chile.

Examination of public opinion data from the period surrounding the reform is largely consistent with this picture, although suggests a possible shift following the reform. The CEP, together with the United Nations Development Program and other partners conducted nationwide surveys in 2008, 2010 and 2012 that included questions on attitudes toward compulsory voting and on economic redistribution (CEP et.al. 2008, 2010, and 2012). In both of the pre-reform era surveys, public opinion leaned overwhelmingly against compulsory voting – 77% to 22% both years – and there was no measurable difference between supporters and opponents of compulsory voting in attitudes toward economic redistribution. In the 2012 survey, which was conducted in the wake of the reform and after the 2012 municipal elections, support for compulsory voting doubled to 46% (against 53% who still favored voluntary voting), and for the first time, the data suggest a slight difference between supporters and opponents of compulsory voting on attitudes toward redistribution.

25 Only municipal offices were contested in 2012. Turnout may recover somewhat with the presidential and congressional elections later this year.
The three panels in Figure 8 show responses to a question on redistribution, with separate histograms for supporters and opponents of compulsory voting in each case.

In 2008, the question was: “Should incomes be made more equal, or should they reward individual effort?” In 2010 and 2012, the question was more individually oriented: “To be a good citizen, how important is it to help Chileans who are poorer than you?”

26 The striking thing about the top two panels of the figure is that the distributions of support for economic redistribution among supporters and opponents of compulsory voting are indistinguishable. The bottom panel, however, suggests that, after the reform took effect, a divergence of opinion appeared with those supporting compulsory voting 10% more likely (55% to 45%) to put the highest priority on helping the poor. Recall that the share of Chileans who expressed support for compulsory voting also jumped in the 2012 survey.

What happened? Consistent with elite discourse and with legislative voting patterns – and with the patterns observed in Venezuela – the motivations behind the reform appear to have been unrelated to attitudes toward economic redistribution. After the reform took effect, however, there is some evidence of a shift in public opinion, perhaps as a result of having observed the drop-off in participation during the 2012 municipal elections, and perhaps the low turnout was socio-economically biased and relatively poor voters started to feel negative consequences of non-voting. It may take more time for Chilean opinion on compulsory voting to settle in the wake of the reform. Yet there is some indication of buyer’s remorse with respect to the end of compulsory voting. Our analysis of the Venezuelan experience suggests that among those who most prioritize progressive redistribution, remorse may be appropriate.

6. Conclusion

Given the subsequent rise of Hugo Chavez, any argument that the ideological location of

26 In the 2008 question, lower values (left) indicate more egalitarian attitudes. In 2010 and 2012, by contrast higher values (right) indicate greater support for redistribution.
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 to the left, not through bias in abstention but by directly changing voter preferences on policy, and how those preferences mapped onto candidate choice. Lupu’s (2010) work on the evolution of Chavez’s support coalition suggests that class-based polarization of the Venezuelan electorate peaked in the 1998 election. At any rate, our purpose in this paper is not to explain the rise of chavismo, 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, yet discussion of the redistribution hypothesis 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 is currently entertaining 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

27 Methodologically, these post-1993 changes in politics and policies are “post-treatment” events. It is unsurprising that these events add additional systematic variations to the post-1993 trajectories of income inequality. This means that the longer the post-intervention period we examine, the more difficult it becomes in interpreting the direct effect of intervention (i.e., in our case, the abolishment of compulsory voting). Some new methods to distinguish between the direct treatment effect and the indirect effects (called “mediation effects”) have been proposed recently (e.g., see Imai et al 2011), but refining the synthetic control method to uncover the complicated nature of post-treatment effects is beyond the scope of this paper.
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 not address economic redistribution (Ungar 2011).

For matters of inference, keeping the issue of redistribution out of debates over compulsory voting has its methodological advantages. Specifically, as we have argued, the apparent absence of redistributive motivations for the Venezuelan reform in 1993 increases the validity of the synthetic control method we employ in this paper to estimate the reform’s unintended impact on economic inequality. Our results, however, suggest that from a normative perspective, questions of inequality and redistribution belong at the center of these debates. The Venezuelan results suggest that 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.
Appendix

Publications from the Venezuelan Presidential Commission for State Reform


References


Figure 1: Biased Turnout and Biased Influence

Note: This figure illustrates hypothetical relationships between voters’ economic status and utility of voting, as well as the location of the median voter’s preference. It is assumed that poorer voters are less likely to vote, but more strongly prefer redistribution. M = the median preference of all eligible voters. M’ = the median preference of only those who vote. Under voluntary voting, voters are assumed to vote when the utility of voting is positive (i.e., above the vertical line on the top panel). Under compulsory voting, all voters are assumed to vote.
Figure 2: The Effect of Factual Intervention in Venezuela in 1993

Note: The dark and grey lines in the left panel show the trajectories of the net Gini coefficient for Venezuela and Synthetic Venezuela, respectively. The dark line in the right panel shows the difference in the net Gini coefficient between Venezuela and Synthetic Venezuela. The grey lines in the right panel show the results of placebo tests, in which the treatment was artificially and counter-factually assigned to each comparison unit.
Table 1: Synthetic Weights for Venezuela

<table>
<thead>
<tr>
<th>Country</th>
<th>Weight</th>
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</thead>
<tbody>
<tr>
<td>Argentina</td>
<td>0</td>
</tr>
<tr>
<td>Australia</td>
<td>0</td>
</tr>
<tr>
<td>Austria</td>
<td>0</td>
</tr>
<tr>
<td>Brazil</td>
<td>0</td>
</tr>
<tr>
<td>Chile</td>
<td>0</td>
</tr>
<tr>
<td>Colombia</td>
<td>0</td>
</tr>
<tr>
<td>Costa Rica</td>
<td>0</td>
</tr>
<tr>
<td>Dominican Republic</td>
<td>0</td>
</tr>
<tr>
<td>Ecuador</td>
<td>0</td>
</tr>
<tr>
<td>El Salvador</td>
<td>0</td>
</tr>
<tr>
<td>Guatemala</td>
<td>0</td>
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<tr>
<td>Jamaica</td>
<td>0</td>
</tr>
<tr>
<td>Mexico</td>
<td>0</td>
</tr>
<tr>
<td>Peru</td>
<td>0.057</td>
</tr>
<tr>
<td>Philippines</td>
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<tr>
<td>Singapore</td>
<td>0</td>
</tr>
<tr>
<td>Trinidad and Tobago</td>
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</tr>
<tr>
<td>Turkey</td>
<td>0</td>
</tr>
<tr>
<td>Uruguay</td>
<td>0.454</td>
</tr>
</tbody>
</table>

Note: The weight is assigned by the synthetic control method.
Table 2: The Means of Predictors Before the Intervention

<table>
<thead>
<tr>
<th>Predictors</th>
<th>Venezuela</th>
<th>Synthetic Venezuela</th>
</tr>
</thead>
<tbody>
<tr>
<td>Net Gini Coefficient in 1993</td>
<td>39.6</td>
<td>39.7</td>
</tr>
<tr>
<td>Net Gini Coefficient in 1988</td>
<td>40.9</td>
<td>40.5</td>
</tr>
<tr>
<td>Net Gini Coefficient</td>
<td>39.9</td>
<td>40.0</td>
</tr>
<tr>
<td>Voter Turnout</td>
<td>72.7</td>
<td>84.6</td>
</tr>
<tr>
<td>GDP Per Capita</td>
<td>10.1</td>
<td>8.7</td>
</tr>
<tr>
<td>Government Share of GDP</td>
<td>21.6</td>
<td>14.1</td>
</tr>
<tr>
<td>Polity Score</td>
<td>8.7</td>
<td>9.1</td>
</tr>
<tr>
<td>Fuel Exports as % of Merchandise Exports</td>
<td>79.5</td>
<td>31.1</td>
</tr>
<tr>
<td>Oes and Metals Exports as % of Merchandise Exports</td>
<td>7.6</td>
<td>3.0</td>
</tr>
<tr>
<td>Former Iberian Colony</td>
<td>1</td>
<td>0.5</td>
</tr>
<tr>
<td>Latin American and Caribbean Country</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Compulsory Voting</td>
<td>1</td>
<td>0.5</td>
</tr>
</tbody>
</table>

Note: For the first two predictors, the values of specific years are used, while the average values are used for other variables. The averaged period for Voter Turnout and Compulsory Voting is from 1988 to 1992 (a year before the intervention). Compulsory Voting is time invariant for all countries during this period. The averages for the remaining variables are for the period between 1988 and 1993. Former Iberian Colonies and Latin American and Caribbean Countries are time-invariant dummy variables.
Figure 3: The Effect of Counterfactual Intervention in Venezuela in 1988

Note: The dark and grey lines in the left panel show the trajectories of the net Gini coefficient for Venezuela and Synthetic Venezuela, respectively. The dark line in the right panel shows the difference in the net Gini coefficient between Venezuela and Synthetic Venezuela. The grey lines in the right panel show the results of placebo tests, in which the treatment was artificially and counter-factually assigned to each comparison unit.
Figure 4: The Effect of Counterfactual Intervention in Bolivia in 1993

Note: The dark and grey lines in the left panel show the trajectories of the net Gini coefficient for Bolivia and Synthetic Bolivia, respectively. The dark line in the right panel shows the difference in the net Gini coefficient between Bolivia and Synthetic Bolivia. The grey lines in the right panel show the results of placebo tests, in which the treatment was artificially and counter-factually assigned to each comparison unit.
**Figure 5**: Electoral Politics in Venezuela, 1973-1998


- Left (various) 5-10%
- AD 43-58%
- COPEI 34-47%

B) Venezuelan presidential election of 1993 – simple (static) version

- Causa R 22%
- AD 24%
- Convergencia (Caldera) 31%
- COPEI 23%

C) Venezuelan presidential election of 1993 – alternative version

- Causa R 22%
- Convergencia (Caldera) 31%
- AD 24%
- COPEI 23%
Figure 6: Government Share of GDP and Net Gini Coefficient in Venezuela, 1988-1998

### Table 3: Registration and Voter Turnout in Chile

<table>
<thead>
<tr>
<th>Date</th>
<th>Office</th>
<th>Turnout</th>
<th>Registered /VAP</th>
<th>Turnout /Registered</th>
<th>Turnout /VAP</th>
</tr>
</thead>
<tbody>
<tr>
<td>10/05/88</td>
<td>Pl</td>
<td>7,251,933</td>
<td>91%</td>
<td>98%</td>
<td>89%</td>
</tr>
<tr>
<td>07/30/89</td>
<td>Pl</td>
<td>7,082,084</td>
<td>91%</td>
<td>94%</td>
<td>85%</td>
</tr>
<tr>
<td>12/14/89</td>
<td>Ch</td>
<td>7,158,646</td>
<td>90%</td>
<td>95%</td>
<td>86%</td>
</tr>
<tr>
<td>12/14/89</td>
<td>Pr</td>
<td>7,158,727</td>
<td>90%</td>
<td>95%</td>
<td>86%</td>
</tr>
<tr>
<td>12/14/89</td>
<td>Se</td>
<td>7,158,442</td>
<td>90%</td>
<td>95%</td>
<td>86%</td>
</tr>
<tr>
<td>06/28/92</td>
<td>Mu</td>
<td>7,043,827</td>
<td>88%</td>
<td>90%</td>
<td>79%</td>
</tr>
<tr>
<td>12/11/93</td>
<td>Ch</td>
<td>7,385,016</td>
<td>89%</td>
<td>91%</td>
<td>81%</td>
</tr>
<tr>
<td>12/11/93</td>
<td>Pr</td>
<td>7,376,691</td>
<td>89%</td>
<td>91%</td>
<td>81%</td>
</tr>
<tr>
<td>10/27/96</td>
<td>Mu</td>
<td>7,079,418</td>
<td>84%</td>
<td>88%</td>
<td>73%</td>
</tr>
<tr>
<td>12/14/97</td>
<td>Ch</td>
<td>7,046,351</td>
<td>82%</td>
<td>87%</td>
<td>72%</td>
</tr>
<tr>
<td>12/12/99</td>
<td>Pr</td>
<td>7,271,584</td>
<td>79%</td>
<td>90%</td>
<td>71%</td>
</tr>
<tr>
<td>01/16/00</td>
<td>Pr</td>
<td>7,326,753</td>
<td>79%</td>
<td>91%</td>
<td>72%</td>
</tr>
<tr>
<td>10/29/00</td>
<td>Mu</td>
<td>7,089,886</td>
<td>78%</td>
<td>88%</td>
<td>68%</td>
</tr>
<tr>
<td>12/16/01</td>
<td>Ch</td>
<td>7,034,292</td>
<td>76%</td>
<td>87%</td>
<td>66%</td>
</tr>
<tr>
<td>10/31/04</td>
<td>Co</td>
<td>6,874,315</td>
<td>72%</td>
<td>86%</td>
<td>61%</td>
</tr>
<tr>
<td>10/31/04</td>
<td>Ma</td>
<td>6,872,675</td>
<td>72%</td>
<td>86%</td>
<td>61%</td>
</tr>
<tr>
<td>12/11/05</td>
<td>Ch</td>
<td>7,207,351</td>
<td>72%</td>
<td>88%</td>
<td>63%</td>
</tr>
<tr>
<td>12/11/05</td>
<td>Pr</td>
<td>7,207,278</td>
<td>72%</td>
<td>88%</td>
<td>63%</td>
</tr>
<tr>
<td>01/15/06</td>
<td>Pr</td>
<td>7,162,345</td>
<td>72%</td>
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<td>63%</td>
</tr>
<tr>
<td>10/26/08</td>
<td>Co</td>
<td>6,950,508</td>
<td>67%</td>
<td>86%</td>
<td>58%</td>
</tr>
<tr>
<td>10/26/08</td>
<td>Ma</td>
<td>6,959,075</td>
<td>67%</td>
<td>86%</td>
<td>58%</td>
</tr>
<tr>
<td>12/13/09</td>
<td>Ch</td>
<td>7,263,537</td>
<td>68%</td>
<td>88%</td>
<td>59%</td>
</tr>
<tr>
<td>12/13/09</td>
<td>Pr</td>
<td>7,264,136</td>
<td>68%</td>
<td>88%</td>
<td>59%</td>
</tr>
<tr>
<td>01/17/10</td>
<td>Pr</td>
<td>7,203,371</td>
<td>68%</td>
<td>87%</td>
<td>59%</td>
</tr>
<tr>
<td>10/28/12</td>
<td>Co</td>
<td>5,744,774</td>
<td>104%</td>
<td>43%</td>
<td>45%</td>
</tr>
<tr>
<td>10/28/12</td>
<td>Ma</td>
<td>5,780,672</td>
<td>104%</td>
<td>43%</td>
<td>45%</td>
</tr>
</tbody>
</table>

*Note: Ch = Chamber of Deputies, Co = Councils, Ma = Mayors, Mu = Municipal, Pl = Plebiscite, Pr = President. Turnout for Senate elections after the founding election of 1989 are not reported because subsequent Senate contests were held in only half of all districts during each cycle. Data sources: Instituto Nacional de Estadisticas (http://www.ine.cl/); Nohlen 2005; Servicio Electoral de la Republica de Chile (http://www.servel.cl/controls/neochannels/neo_ch1/neochn1.aspx).*
Figure 7: Voter Turnout in Chilean Elections

Note: See Note in Table 3. VAP = Voting Age Population.
**Figure 8:** Public Opinion in Chile, 2008-2012

**Attitudes Toward Income Redistribution**

![Histograms showing public opinion on income redistribution](image1)

*Note: 1 – Favors Redistribution, 10 – Opposes Redistribution; 2008 Survey*

**Must a Good Citizen Help the Poor?**

![Histograms showing public opinion on helping the poor](image2)

*Note: 1 – Not Important, 7 – Important; 2010 Survey*

**Must a Good Citizen Help the Poor?**

![Histograms showing public opinion on helping the poor](image3)

*Note: 1 – Not Important, 7 – Important; 2012 Survey*

*Note: The histograms on the left are among the respondents who support compulsory voting, while the histograms on the right are among the respondents who support voluntary voting. See the main text for data sources.*