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 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 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 focusing on a rare case of abolishing compulsory voting in Venezuela, we examine whether compulsory voting reduces income inequality.

Our results support a well-known proposition advanced by Arend Lijphart 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). The normative foundation of this argument is that, in a democracy, the preferences of every citizen should have equal weight in electing representatives and determining policy. Lijphart contends that “[l]ow voter turnout means unequal and socioeconomically biased turnout… [and] unequal participation spells unequal influence” (p. 2), and 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.

Despite its normative importance and practical relevance in policy debates, rigorous empirical scrutiny of Lijphart’s claim has been limited. Numerous scholars have
investigated the impact of voter turnout on various outcome variables (see Fowler 2012 for an extensive review). Most of these studies, however, face methodological shortcomings because they are based on relatively simple cross-sectional regression without a convincing identification strategy for causal inference (Angrist and Krueger 2001), or because they rely on instrumental variables based on exogenous “shocks” to turnout (e.g. weather events) that are not relevant to how the level of voter turnout influenced by whether voting is compulsory or mandatory affects electoral and policy outcomes.

Two recent studies address these methodological concerns. Fowler (2013) estimates the causal effects of the introduction of compulsory voting in Australia on election outcomes and pension spending. Similarly, Bechtel, Hangartner, and Schmid (2013) 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 these two recent studies by examining an electoral reform that pushes in the opposite direction, the *abolishment* of compulsory voting in Venezuela. Venezuela is one of a handful of countries that enforced compulsory voting for long periods but then dropped it. None of the others offers as promising an environment to study the national-level policy effects of abandoning compulsory voting, however, 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).

Our contributions to the literature are twofold. First, we investigate the downstream effects of compulsory voting on socio-economic outcomes – specifically, on income inequality.¹ 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 (2013) 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

¹ Chong and Olivera (2008) estimate the impact of compulsory voting on income inequality. Most of their estimates are based on simple cross-national regressions, while they use some instrumental variables as robustness tests. A problem in their study, however, is that their treatment variable – whether a country has a compulsory voting system or not – is causally prior to almost all variables included in their models, because most countries introduced a rule of compulsory voting many decades ago. Therefore, their control variables (or included exogenous variables in their two-stage least square regressions) are post-treatment variables. The inclusion of such variables introduces bias in causal inference (Rosenbaum 1984). Without focusing on the cases in which an institution was introduced or abolished, it is difficult to make valid causal inference on the impacts of institutional arrangements (Przeworski 2004).
in policy-relevant variables indicate responsiveness to the changing electorates. This paper takes the next step of estimating the socio-economic consequences of turnout bias. Second, in addition to estimating the causal effects of abolishing compulsory voting in Venezuela, we identify causal mechanisms connecting compulsory voting and income inequality. Specifically, we articulate a set of observable implications of Lijphart’s original claim and examine their validity in light of the archival record as well as economic, electoral, and public opinion data.

In what follows, Section 2 reviews Lijphart’s theory and introduces five observable implications. Section 3 provides some background on the Venezuelan case, and then examines the first four observable implications, drawing on a variety of quantitative and qualitative data. Section 4 presents difference-in-differences (DD) regression models that estimate the downstream effect of abolishing compulsory voting on income inequality. The results from these two sections 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. The last section concludes and discusses policy implications.

2. Lijphart’s Theory of Unequal Influence

Lijphart’ 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. Before testing it statistically, it is important to make a few underlying assumptions explicit and to introduce additional observable implications that help us understand causal mechanisms.
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 economic growth and job market fluctuation, and more dependent on public sector jobs, including jobs created by public works, 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, for example, marginal tax rates will be of little consequence to most citizens but highly salient to the rich (Kasara and Suryanarayan 2013). 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; Gordon and Segura 1997). The costs can also be logistical: Poor would-be voters may lack access to transportation to get to the polls, or flexible work schedules that allow wealthier citizens more easily to cast their ballots. 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 (Irwin 1974, Martkainen, Martikainen, and Wass 2005; Singh 2011; Tingsten 1937).

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 in the utility functions of the poor than the rich. Prior studies have shown that voter turnout is high as long as compulsory voting is enforced with substantial punishments for not voting (Fowler 2013; Jackman 2001).

Under these three assumptions, the median voter’s position is expected to shift to the right under voluntary voting relative to compulsory voting, as illustrated in Figure 1.

[Figure 1 about here]

The top panel shows a hypothetical distribution of voters, arrayed from the poor to the rich across the horizontal axis, with the vertical axis representing their net utility from

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

3 Kasara and Suryanarayan (2013) question the prevalence of positive turnout bias across nations. Our 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. The results are available from the authors upon request.
voting. 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. If everyone votes, the distribution of socioeconomic status is the one with the shaded area in the bottom panel of Figure 1. The median voter’s position is at $M$. When voting is voluntary, the relatively higher non-voting rate among poor citizens yields a richer median voter (at $M' > M$). Thus, compulsory voting, by diminishing the socioeconomic turnout bias in voluntary non-voting, should shift the redistributive preferences of the median voter back toward the median in the full population.

The final, but not the least, 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 observable implications: (1) a decline in voter turnout; (2) a shift to the right in the preference of the richer median voter; (3) the election of politicians who support less redistribution; (4) the implementation of less progressive economic policies; and (5) an increase in economic inequality. In the next section, we examine the empirical validity of the first four

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4 We do not require a stronger assumption that governments accurately implement the preferred policy of the median voter.

5 A shift from voluntary to compulsory voting should produce the opposite effect at each link of the causal chain.
observable implications. We then examine the validity of the fifth, and final, effect in Section 4.

3. The Case of Venezuela

Compulsory voting was enshrined in Venezuela in the Constitution of 1961, which remained in place until the adoption of the current Constitution of 1999. The specific sanctions were provided for in the electoral laws: They include 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). The requirement to vote was enforced consistently for the first couple of decades.

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 with 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, Congress eliminated sanctions in the Organic Suffrage Law of 1993 (Molina and Perez Baralt 1995, 1996). Language describing voting as a duty remained in the Constitution until the

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6 An important question is whether or not the electoral reformers intended to influence the distribution of income in Venezuela. In the Appendix, based on archival materials, we argue that the sharp increase in income inequality after abolishing the compulsory voting is most likely an unintended consequence for politicians.
promotion of the new charter in 1999, but the requirement, which was enforced more aggressively during the 1960s and 1970s than subsequently, lost legal sanctioning mechanisms from 1993 on.7

In this section, we first discuss how this institutional change affected voter turnout and shifted the median voter’s position. We then discuss political changes in Venezuela before and after the abolishment of compulsory voting in 1993. The available evidence is consistent with Lijphart’s theory and helps us interpret the statistical estimate of the downstream effects on income inequality presented in Section 4.

3.1 Positive Turnout Bias

The first link in the causal chain posited by Lijphart’s hypothesis is that turnout should decline sharply when compulsory voting is abandoned. This effect was clear and well documented in the Venezuelan case, particularly in the scholarship of Jose Enrique Molina (1995; see also Molina and Perez Baralt 1995). Specifically, voter turnout in national elections had been consistently over 70% in the 1970s and 1980s but dropped to 50.0% in the 1993 election, immediately after legal sanctions for non-voting were abolished (International IDEA 2013).

The next link holds that the turnout drop should be concentrated more heavily among poorer voters who prefer more progressive redistribution than among those who oppose it, thus shifting the position of Venezuela’s median voter to the right. We draw on

7 We thank Jose Enrique Molina (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.
data from two national-level Venezuelan surveys conducted in 1983 and 1995 that provide some leverage (Baloyra and Torres 1983; Canache 2002). The 1983 survey was conducted just before that year’s general election, and asked voters about their participation in four contexts – the 1973 and 1978 elections, their intention to participate in the upcoming 1983 election, and whether they would vote if voting were not compulsory, as it was at that time. The 1995 survey asked voters whether they had voted in the 1993 election – the first election after sanctions of non-voting were eliminated. The two surveys differ in their level of disaggregation of responses on household income, but the data allow us to group respondents into income quartiles, which facilitates comparisons across the surveys.

Table 1 shows the self-reported turnout rate, by quartile, for the 1973, 1978, and 1993 elections, as well as expressed intention to vote in 1983 and the 1983-era responses to the conjectural question about voting if it were not compulsory.

A number of patterns are clear. First, except for the question on intention to vote in 1983, when virtually every respondent indicated the affirmative, there is evidence of some positive turnout bias. Recalling their participation in 1983, respondents in lower income quartiles reported greater non-participation in 1973 and 1978 than did those with higher incomes. The difference across quartiles is not statistically significant for 1973 ($p = 0.19$), when those in the lowest quartile turned out at 82.5% and those in the highest at 89.4%. It is, however, significant for 1978 ($p = 0.01$), when the gap between the lowest

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8 We used respondent-level data and measured Chi-square for significant tests.
(81.7%) and the highest (92.1%) groups rose to 10.4% points. Thus, even with enforced compulsory voting, Venezuela experienced positive turnout bias. The responses to the conjectural question about whether respondents would vote if voting were not compulsory are more striking. Only a quarter of those in the lowest income quartile (24.7%) indicated an intention to participate, compared with 54.2% in the top quartile ($p = 0.00$). The responses to the 1995 survey on participation in the 1993 indicate a 12.4% point gap ($p = 0.05$) between turnout rates in the top and bottom income quartiles.

On the whole, these data confirm positive turnout bias and also suggest that the magnitude of that bias increased after the end of compulsory voting. All these results are consistent with the proposition that the preferences of Venezuela’s median voter for redistributive policy was pushed right, under the first assumption of Lijphart’s theory, when voting became voluntary relative to where it would have been had voting remained compulsory.

### 3.2 Interpreting Political Change in 1993

If the end of compulsory voting changed the size and composition of the Venezuelan 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. We face a challenge here insofar as the Venezuelan party system was entering a state of decomposition during the period of our analysis. This complicates the task of mapping preferences for economic redistribution onto electoral results.

The period of our investigation, which includes some years before and after the abandonment of compulsory voting, was one of enormous turbulence, economically and politically, following on decades during which the country 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). For our purposes, the top panel of Figure 2 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 2 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

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 2 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.

It is important to acknowledge that many accounts of Venezuelan politics portray a fluid electoral environment in which the fit of theory to data may not be quite so straightforward. When he was elected in 1988, AD’s 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 (Stokes 2001). His presidency never fully recovered, and 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 Chávez. In the last year of his term, Perez faced corruption charges 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 stood to the right of Caldera, whose campaign excoriated Perez’s neoliberalism (Buxton 2003). The bottom panel of Figure 2 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 the 1993 election represented a shift to the right, but neither does it provide evidence to that effect.

To resolve the ambiguity over which of these accounts better represents Venezuelan politics in the mid-1990s, we now turn to the deeds and the words of Venezuelan politicians themselves.

3.3 The Deeds of Venezuelan Politicians

Importantly, with respect to policy, there was a shift toward greater austerity under Caldera than under Perez, 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 was 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 3 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.9

[Figure 3 about here]

The vertical bars show the 90% confidence interval for the Gini estimates. Government consumption levels (grey dots in Figure 3) bounced around during Perez’s term, 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 hollow dot in Figure 3) – 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 is often portrayed as the president who promoted drastic neoliberal reforms, but neither the

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).
government share of GDP nor the Gini coefficient changed substantially during his term.

The pattern only becomes dramatic under Caldera (black dots in Figure 3), when government spending dropped dramatically and monotonically, with corresponding increases in our measure of inequality. The negative correlation ($-0.76, p = 0.01, 1989-1998$) 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 is consistent with the idea that government spending, with its effects on labor markets and wages, affects inequality even more than transfer programs (Stack 1978; Morgan and Kelly 2013).

### 3.4 The Words of Venezuelan Politicians

As a further check on the proposition that the center of electoral gravity shifted right in 1993, we examined the content of debates conducted in Venezuela’s Congress during three-year windows on either side of the reform – specifically, transcripts of floor debates as recorded in the *Sesiones del Dia* from 409 sessions of the Chamber of Deputies from 1991-1996. To evaluate the rhetorical content of the floor debates, we converted text into word clouds that give greater prominence to words that appear more frequently. The idea was to use the clouds to determine whether parliamentary floor debates changed after the end of compulsory voting in ways that reveal an ideological shift in rhetoric. We pooled the transcripts from the 225 sessions from the three years before the 1993 election

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10 We used the Wordle online application, available at <http://www.wordle.net>.
(1991-1993) and for the 184 sessions from the three successive years (1994-1996).

After generating clouds based on the full text of the debates, we examined the clouds and identified words that appeared prominently because of their frequency but have no ideological content. They include proper nouns (i.e. representatives’ names, Venezuelan cities, etc.), standard articles of speech (pronouns, conjunctions, common modifiers and verbs, etc.), and words that are central to parliamentary procedure ("session," "amendment," "approval," "remarks," etc.). After identifying ideology-free words in the clouds, we returned to the text files and conducted search-and-delete commands, then produced new clouds, and so on. Five iterations of this process yielded a set of 324 eliminated words.

The word clouds for the 1991-1993 and 1994-1996 parliamentary sessions are shown in Figure 4, and Table 2 shows the English translation of words that appear prominently in one or the other, or both, word clouds.

[Figure 4 and Table 2 about here]

There are some words that are prominent in both periods – "budget," "citizens," "custom," "law," "public," and "workers" (presupuesto, ciudadanos, costumbre, derecho, publico, and trabajadores, respectively). More importantly, we identify some differences in prominent words between the two periods. Specifically, “the people” (pueblo) is more prominent in floor debate before the abandonment of compulsory voting, as are "Constitution," "democratic," and "reform" (Constitucion, democratico, and reforma) whereas after 1993, we see greater prominence for "bank," "debt," “development,” “fund,” "resources," and “security” (banco, deuda, desarrollo, fondo, recursos, and seguridad).
The decline of “the people” after 1993 is noteworthy, as is the corresponding rise in prominence of words that appear less central to leftist rhetoric and more closely associated with concerns for fiscal austerity and financial security. Of course, the clouds themselves do not provide insight into how these words were being used. Discussions focused on “banks” and “debt” after 1993 might have consisted primarily of populist attacks on the financial industry.

As a further check on this possibility, we searched the debate transcripts for each year, extracting all the sentences containing the words identified in Table 2 as having either declined or risen in prominence after 1993. Reviewing the text confirms that the prominence of keywords reflects predominant themes of debate. So, for example, “the people,” which was far more prominent up through 1993 than after, was consistently used to describe collective interests in a way that evokes solidarity – for instance, likening IMF-endorsed economic policies to “a shackle around the ankle of the people of Venezuela” (1993, line 1,376). By contrast, the increasing prominence of “security” after 1993 was, in part, due to debate in 1994 over a proposed law to improve “security of people and property” (1994, lines 4,510, 4,547, 6,381, 6,506, 13, 673, 14,305, 15,321, 22,847, 24,531, 29,173), and the rise of “debt” was driven by discussions on how to reorganize state spending priorities to cover rising public debt in the mid-1990s (1994, lines 106, 3,019, 4,267, 7,209, 13,259, 19,699, 21,746, and hundreds more).

11 A text file with the sentences containing each of these keywords, grouped by year, is available from the authors in PDF format for review.
3.5 Summary

It will come as no surprise to students of Venezuelan politics that the 1988-1998 period is difficult to pin neatly onto theory, but the theory we seek to test in this paper is consistent 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. Although Perez’s *Gran Virage* 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 the AD government. The economic data suggest that it was Caldera’s policy that exposed, more systematically, the vulnerability of the poorest Venezuelans. Furthermore, the content of parliamentary debates from before and after 1993 suggests that legislators placed less emphasis on social solidarity, and more on fiscal austerity, after compulsory voting ended. Finally, the content of both policy and parliamentary debate are consistent with public opinion survey evidence suggesting that socioeconomic bias in electoral turnout increased when participation became purely voluntary, pushing Venezuela’s median voter to the right.

4. Estimating the Effect on Inequality

In this section, we examine the final and most consequential observable implication of Lijphart’s theory – the effect of abandoning compulsory voting on income inequality. We introduce data, specify our statistical models, and then show the results.
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.

Another noticeable feature of the SWIID dataset is that it includes 100 estimates of the Gini coefficient for each country in each year. These estimates are produced based on a statistical method of multiple imputations. Inherently, any measure of income inequality is an “estimate” based on specific data and methods, rather than a numerical summary of observed facts. Therefore, some estimates (e.g., those based on extensive household surveys in developed countries) have smaller uncertainty, while others (e.g., those based on unreliable sources in developing countries) have much larger uncertainty. Importantly, with the 100 multiply imputed estimates for each country in each year, we can obtain not only a point estimate but also an interval estimate of inequality. To the best of our knowledge, the SWIID dataset is the only available dataset that allows us to take into account such cross-national and time-series variations in uncertainty when investigating the causes and consequences of income inequality.

To be precise, the question we examine in this statistical analysis is: What would the level of income inequality have been if the compulsory voting rule had not been
abolished in 1993 in Venezuela? To examine this counter-factual question using cross-national, time-series data, it is imperative to use countries that are sufficiently similar in historical, political and social backgrounds. For this reason, in our analysis, we focus on the following Latin American and Caribbean countries – Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Cuba, Dominican Republic, Ecuador, El Salvador, Guatemala, Haiti, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela, and Trinidad and Tobago. We use three periods of investigation, 1978-1998, 1983-1998 and 1988-1998. The number of observations is 337, 281, and 212, 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.

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, because 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 adds 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

12 Countries and years without reliable estimates for the Gini coefficients are excluded from analysis. See Solt (2009, 2013) for details.
interpret the *direct* effect of “treatment” (i.e., in our case, the abolishment of compulsory voting). 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 indirect, or “mediation,” effects would introduce methodological challenges beyond the scope of this paper (Imai et. al. 2011; Rosenbaum 1984).

### 4.2 Statistical Models

Our statistical analysis is based on a difference-in-differences (DD) design (Angrist and Pischke 2009), an approach with a long tradition in social science research aimed at estimating the effect of a specific event, 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.\(^\text{13}\)

Specifically, we run the following regression model using the cross-national and time-series data.

\(^\text{13}\) There is a newer, non-parametric approach similar to the difference-in-differences design, which is called the synthetic control method (Abadie, Diamond, and Hainmueller 2010; 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 analyses available from the authors upon request, we apply the synthetic control method using the point estimates of income inequality (with a very strong assumption that there is no measurement error in the Gini coefficient) and produce similar results.
\[ Y_{it} = b_0 + b_1 T_{1it} + b_2 T_{2it} + f_i(t) + v_i + u_t + \varepsilon_{it} \]

The outcome variable \( Y_{it} \) is the 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 (exclusive), the coefficient \( b_2 \) measures the annual increase in the outcome variable in Venezuela after 1993 (exclusive). After running this model, we then undertake a Chow test (Chow 1960). The null hypothesis we 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 \( v_i \), and year-specific fixed effects \( u_t \). The last term \( \varepsilon_{it} \) 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, quadratic, cubic and quartic trends. In total, we run 12 regression models – three periods (1978-1998, 1983-1998, and 1988-1998) \( \times \) four functional forms (linear, quadratic, cubic and quartic).

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 the average effects of these variables on the outcome variable are captured by the country-specific trend variables. Furthermore, the country-specific and year-specific fixed effects control for attributes of each country (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).

4.2 Results

The results of regressions are presented in Table 3.

[Table 3 about here]

The last two columns show that the null hypothesis of no structural break in Venezuela in 1993 can be rejected in 9 out of 12 models at the 5% level, and in a tenth model (1978-1998, cubic) at the 10% level ($p = 0.06$). These results suggest that the trajectory of the Gini coefficient systematically changed in Venezuela in 1993. 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 3, it is more intuitive to compare the factual and counterfactual trends graphically. Figure 5 summarizes such analysis for the period from 1983 to 1998. The results for the other two periods are essentially the same.

[Figure 5 about here]

The triangular dots (in black) are the Gini coefficients in the SWIID data. The vertical lines associated with them indicate the 95% confidence intervals based on the 100
multiple imputations in the SWID data. The circular dots (in light gray) are the predicted values obtained from our DD regression analysis. The vertical lines associated with them indicate the 95% confidence intervals of prediction, by taking into account the uncertainty in the estimated Gini coefficients. The figure shows that all the four models (linear, quadratic, cubic, and quartic) fit the data fairly well. Venezuela’s Gini coefficients gradually decreased from about 43% in 1983 to about 38% in 1993. This pattern suddenly changed after 1993 and the Gini coefficient increased to almost 45% until a year or two before the 1998 election.

Figure 5 also shows a counterfactual trajectory. The diamond dots (in dark gray) are the predicted values under an assumption of no structural break in Venezuela in 1993. Recall that we only focus on countries that are sufficiently similar to Venezuela as comparison units – that is, Latin American and Caribbean countries – and our difference-in-differences models control for all observable and unobservable country-specific and time-invariant factors, year-specific and country-invariant (that is, regions-specific) factors, and country-specific trends (in various 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 \( b_1 = b_2 = 0 \)).\(^{14}\) In other words, they show our best estimate of what would the Gini coefficients have been if the compulsory voting rule had not been abolished in 1993 in Venezuela. Each

\(^{14}\) Specifically, to calculate the counterfactual trend, we subtracted \( \hat{b}_1 T_{1t} + \hat{b}_2 T_{2t} \) from the predicted value for each observation \( \hat{Y}_{it} \).
panel clearly shows a downward trend. The gaps between these counterfactual trends and the trends of predicted values are our estimates of the treatment effects.

### 4.3 A Placebo Test (Bolivia in 1997)

Strictly speaking, the results in Table 3 and Figure 5 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. 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. We would note that there are reasons to be cautious about applying Iversen and Soskice’s 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 fully proportional, weakening the Duvergerian imperative of SMD competition on which Iversen and Soskice’s model depends. Second, in the Iversen and Soskice 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. These reservations aside, 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 a placebo test on Bolivia, which adopted a MMP system similar to Venezuela’s in 1994, and first held elections using the new 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 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 are presented in Table 4 and Figure 6.

[Table 4 and Figure 6 about here]

None of the coefficients for the treatment variables is statistically significant, and the P values of the Chow F statistics are very large. Therefore, we fail to reject the null hypothesis of no structural break. Figure 6 shows that Bolivia’s Gini coefficient increased until 1997, and then started to decrease after that. This pattern is opposite to the case of Venezuela. The higher-order polynomial models have better fit, but the counterfactual trajectories are almost identical to the trajectories based on the models’ predicted values.
From this result, we argue that the electoral reform from a closed-list PR system to a MMP system in Venezuela in 1993 is unlikely to be a relevant factor explaining Venezuela’s sharp increase in income inequality after 1993. In sum, all the results in this section suggest that the removal of legal sanctions for non-voting in Venezuela in 1993 fueled the observed increase in income equality after 1993.

5. 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 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. 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 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).

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.
Appendix: Intended or Unintended Consequences

In our statistical analysis, we attempt to provide a plausible counterfactual case for the trajectory of economic inequality in Venezuela if it had not abolished compulsory voting. In our effort to make causal inference, we also 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. 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, “[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.\textsuperscript{15} 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 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 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.\textsuperscript{16}

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

\textsuperscript{15} 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).

\textsuperscript{16} Citations to the COPRE reports reviewed are included at the end of this appendix. The Consejo Supremo Electoral reports are cited elsewhere in this manuscript, and citations are included in the general references section. We wish to thank Aaron Watanabe, who scoured the Harvard library system for these primary sources and provided reports on their contents.
(Gosnell 1927 and 1930; Tingsten 1937). The context of Venezuelan political debate of that period, however, provides some perspective. 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 1996). 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). 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 through 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 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; Paredes Pisani 1991; Rachadell 1991; Zambrano 1989). 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 actually 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
References to COPRE Reports


References


Figure 1: Biased Turnout and Biased Influence

Note: This figure illustrates hypothetical relationships between voters’ socioeconomic status and utility of voting, as well as the location of the median voter’s preference. $M =$ 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).
Table 1: Venezuela Turnout Rate by Income Quartiles

<table>
<thead>
<tr>
<th>Income Quartiles</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Bottom</td>
</tr>
<tr>
<td>% (voted in 1973)</td>
<td>82.5</td>
</tr>
<tr>
<td>% (voted in 1978)</td>
<td>81.7</td>
</tr>
<tr>
<td>% (intend to vote in 1983)</td>
<td>99.2</td>
</tr>
<tr>
<td>% (would vote if voting were not compulsory)</td>
<td>24.7</td>
</tr>
<tr>
<td>% (voted in 1993)</td>
<td>55.5</td>
</tr>
</tbody>
</table>

Sources: We draw on data from two national-level Venezuelan surveys conducted in 1983 and 1995 (Baloyra and Torres 1983; Canache 2002). The 1983 survey was conducted just before the 1983 election, and asked voters about their participation in four contexts – the 1973 and 1978 elections, their intention to participate in the upcoming 1983 election, and whether they would vote if voting were not compulsory, as it was at that time. The 1995 survey asked voters whether they had voted in the 1993 election.
Figure 2: 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 3**: Government Share of GDP and Net Gini Coefficient in Venezuela, 1988-1998

Figure 4: Word Clouds for Venezuelan Chamber of Deputies Floor Debates

(A) 1991-1993

(B) 1994-1996

Note: The text data are transcripts of floor debates as recorded in the Sesiones del Dia from 409 sessions of the Chamber of Deputies from 1991-1996. The Wordle <http://www.wordle.net > is used to make these word clouds.
**Table 2**: Word Cloud Text Analysis

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
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<tbody>
<tr>
<td>Constitution</td>
<td>budget</td>
<td>bank</td>
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<tr>
<td>democratic</td>
<td>citizens</td>
<td>debt</td>
</tr>
<tr>
<td>the people</td>
<td>custom</td>
<td>development</td>
</tr>
<tr>
<td>reform</td>
<td>power</td>
<td>fund</td>
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<td>security</td>
</tr>
<tr>
<td></td>
<td>workers</td>
<td></td>
</tr>
</tbody>
</table>

*Note: The text data are transcripts of floor debates as recorded in the *Sesiones del Dia* from 409 sessions of the Chamber of Deputies from 1991-1996. Some of the words that are more prominent in either of the two periods (1991-1993, 1994-1994) are listed in the first and third column. The third column lists some words prominent in both periods.*
Table 3: The Regression Results, Venezuela

<table>
<thead>
<tr>
<th>Period</th>
<th>Trend</th>
<th>$T_{1it}$</th>
<th>$T_{2it}$</th>
<th>Intercept</th>
<th>$H_0: b_1 = b_2 = 0$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$\hat{b}_1$</td>
<td>$\hat{b}_2$</td>
<td>$\hat{b}_0$</td>
<td>F</td>
</tr>
<tr>
<td>1978-1998</td>
<td>1st</td>
<td>-0.45</td>
<td>(2.60)</td>
<td>0.81</td>
<td>(0.70)</td>
</tr>
<tr>
<td></td>
<td>2nd</td>
<td>2.89</td>
<td>(1.88)</td>
<td><strong>2.96</strong></td>
<td>(0.84)</td>
</tr>
<tr>
<td>1998</td>
<td>3rd</td>
<td>3.38</td>
<td>(1.80)</td>
<td>1.39</td>
<td>(1.67)</td>
</tr>
<tr>
<td></td>
<td>4th</td>
<td>1.50</td>
<td>(1.79)</td>
<td>2.96</td>
<td>(1.59)</td>
</tr>
<tr>
<td>1983-1998</td>
<td>1st</td>
<td>1.77</td>
<td>(2.08)</td>
<td><strong>1.36</strong></td>
<td>(0.57)</td>
</tr>
<tr>
<td></td>
<td>2nd</td>
<td>2.53</td>
<td>(1.54)</td>
<td><strong>2.16</strong></td>
<td>(1.02)</td>
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<tr>
<td>1998</td>
<td>3rd</td>
<td>2.72</td>
<td>(1.68)</td>
<td>2.27</td>
<td>(1.61)</td>
</tr>
<tr>
<td></td>
<td>4th</td>
<td>0.29</td>
<td>(1.27)</td>
<td>2.49</td>
<td>(1.32)</td>
</tr>
<tr>
<td>1988-1998</td>
<td>1st</td>
<td>2.29</td>
<td>(1.60)</td>
<td><strong>1.51</strong></td>
<td>(0.57)</td>
</tr>
<tr>
<td></td>
<td>2nd</td>
<td><strong>2.61</strong></td>
<td>(1.30)</td>
<td>1.09</td>
<td>(1.40)</td>
</tr>
<tr>
<td>1998</td>
<td>3rd</td>
<td>-0.64</td>
<td>(1.27)</td>
<td><strong>2.95</strong></td>
<td>(1.27)</td>
</tr>
<tr>
<td></td>
<td>4th</td>
<td>0.25</td>
<td>(1.20)</td>
<td>0.53</td>
<td>(1.86)</td>
</tr>
</tbody>
</table>

Note: Four models are estimated for each period of investigation. Each model includes country-specific fixed effects, year-specific fixed effects, and country-specific linear, quadratic, cubic, or quartic trend variables. The base category (for the constant term) is the Gini coefficient in Venezuela in 1993. The number of observations is 337 (for 1978-1998), 281 (for 1983-1998), and 212 (for 1988-1998), respectively. 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. The coefficients and F-statistics that are significant at the 5% level are highlighted in bold.
Figure 5: The Regression Results, Venezuela

Note: The triangle dots (in black) are the Gini coefficients in the SWIID data. The vertical lines associated with them indicate the 95% confidence intervals based on 100 multiple imputations. The circle dots (in light gray) are the predicted values obtained from DD regression analysis. (See Note in Table 3 for model specification.) The vertical lines associated with them indicate the 95% confidence intervals of prediction, by taking into account the uncertainty in the estimated Gini coefficients. The diamond dots (in dark gray) are the predicted values under an assumption of no structural break in Venezuela in 1993 (i.e., when $b_1 = b_2 = 0$)
Table 4: The Regression Results, Bolivia

<table>
<thead>
<tr>
<th>Trend</th>
<th>$T_{1it}$</th>
<th>$T_{2it}$</th>
<th>Intercept</th>
<th>$H_0: b_1 = b_2 = 0$</th>
</tr>
</thead>
<tbody>
<tr>
<td>1$^{st}$</td>
<td>$\hat{b}_1$ = 2.05 (2.84)</td>
<td>$\hat{b}_2$ = -0.82 (2.02)</td>
<td>$\hat{b}_0$ = 53.70 (11.23)</td>
<td>F = 0.36, P = 0.701</td>
</tr>
<tr>
<td>2$^{nd}$</td>
<td>$\hat{b}_1$ = 0.92 (1.63)</td>
<td>$\hat{b}_2$ = 0.35 (3.70)</td>
<td>$\hat{b}_0$ = 50.54 (15.22)</td>
<td>F = 0.04, P = 0.958</td>
</tr>
<tr>
<td>3$^{rd}$</td>
<td>$\hat{b}_1$ = 1.25 (1.51)</td>
<td>$\hat{b}_2$ = -3.09 (5.91)</td>
<td>$\hat{b}_0$ = 63.84 (23.66)</td>
<td>F = 0.24, P = 0.792</td>
</tr>
<tr>
<td>4$^{th}$</td>
<td>$\hat{b}_1$ = 0.01 (1.11)</td>
<td>$\hat{b}_2$ = -2.33 (6.02)</td>
<td>$\hat{b}_0$ = 62.32 (23.77)</td>
<td>F = 0.15, P = 0.859</td>
</tr>
</tbody>
</table>

Note: Four models are estimated. The period of investigation is from 1989 to 2002. Each model includes country-specific fixed effects, year-specific fixed effects, and country-specific linear, quadratic, cubic, or quartic trend variables. The base category (for the constant term) is the Gini coefficient in Bolivia in 1997. The number of observations is 275. The first treatment variable ($T_{1it}$) is the average change in the outcome variable in Bolivia after 1997 (exclusive). The second treatment variable ($T_{2it}$) is the annual increase in the outcome variable in Bolivia after 1997 (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. The coefficients and F-statistics that are significant at the 5% level are highlighted in bold.
**Figure 6**: The Regression Results, Bolivia

*Note*: The triangle dots (in black) are the Gini coefficients in the SWIID data. The vertical lines associated with them indicate the 95% confidence intervals based on 100 multiple imputations. The circle dots (in light gray) are the predicted values obtained from DD regression analysis. (See *Note* in Table 3 for model specification.) The vertical lines associated with them indicate the 95% confidence intervals of prediction, by taking into account the uncertainty in the estimated Gini coefficients. The diamond dots (in dark gray) are the predicted values under an assumption of no structural break in Bolivia in 1997 (i.e., when $b_1 = b_2 = 0$).