

Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia

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ABSTRACT

Despite extensive research on voting, there is little evidence connecting turnout to tangible outcomes. Would election results and public policy be different if everyone voted? The adoption of compulsory voting in Australia provides a rare opportunity to address this question. First, I collect two novel data sources to assess the extent of turnout inequality in Australia before compulsory voting. Overwhelmingly, wealthy citizens voted more than their working-class counterparts. Next, exploiting

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the differential adoption of compulsory voting across states, I find that the policy increased voter turnout by 24 percentage points which in turn increased the vote shares and seat shares of the Labor Party by 7–10 percentage points. Finally, comparing across OECD countries, I find that Australia's adoption of compulsory voting significantly increased turnout and pension spending at the national level. Results suggest that democracies with voluntary voting do not represent the preferences of all citizens. Instead, increased voter turnout can dramatically alter election outcomes and resulting public policies.

In most democracies, voting is voluntary. Citizens can expend a small cost in exchange for a small chance that they will change the outcome of an election. Voters are those who care deeply about politics, have a lot at stake, possess disposable time and resources, or receive utility from the act of voting itself (Riker and Ordeshook, 1968). As a result, voters may be unrepresentative of the population as a whole. Typically, they are wealthier and more educated than nonvoters which could bias public policy in favor of the few. In this paper, I turn to early twentieth century Australia to address a fundamental question of democracy: Would election results and public policy be different if everyone voted?

Previous evidence on this question is conflicted and suffers from methodological limitations. Correlational studies show that the association between turnout and partisan election results can be large (McAllister and Mughan, 1986; Nagel, 1988; Radcliff, 1994; Pacel and Radcliff, 1995; Fisher, 2007; Hill, 2010), small (Nagel and McNulty, 1996; Grofman *et al.*, 1999; Martinez and Hill, 2007), or nonexistent (DeNardo, 1980; Erikson, 1995). Further studies of survey data conclude that higher turnout may benefit left-wing parties significantly (Herron, 1998; Mackerras and McAllister, 1999), minimally (Citrin *et al.*, 2003; Martinez and Gill, 2005; Bernhagen and Marsh, 2007; Pettersen and Rose, 2007), or not at all (Highton and Wolfinger, 2001; Rubenson *et al.*, 2007). Finally, studies of small exogenous shocks to turnout suggest that the effects of turnout on election results and public policy can be large (Anzia, 2011, 2012; Fowler, 2012; Hansford and Gomez, 2010) or small (Berry and Gersen, 2011; Knack and White, 1998; Sled, 2008; Stein, 1998; van der Eijk and van Egmond, 2007).

As a result of methodological limitations, no previous study adequately addresses the primary question of interest. Confounding variables, reverse causation, and model misspecification may bias the correlational and survey studies. The most compelling causal evidence on the effects of turnout comes from studies of small shocks to turnout. These studies address a separate question about the effects of marginal changes to voter turnout, but they do not assess the effects of near-universal turnout. If marginal voters are unrepresentative of the entire population of nonvoters, then these studies do not speak to the counterfactual question at hand. To determine what would happen if everyone voted, we need a policy change that closely mimics the ideal counterfactual — one where almost everyone is brought to the polls.

This paper analyzes the adoption of compulsory voting laws in Australia as a unique opportunity to assess the effects of near-universal turnout. First, I present a brief history of compulsory voting laws in Australia and explain why this policy change provides such a unique opportunity. Next, I analyze two novel data sources to determine which types of citizens voted in Australia before the adoption of compulsory voting. Overwhelmingly, wealthy and property-owning citizens turned out at higher rates leading to the hypothesis that compulsory voting benefits the Labor Party and progressive policies. Then, I exploit the differential adoption of compulsory voting laws across Australian state assembly elections. A difference-in-difference analysis shows that compulsory voting caused a 24 percentage point increase in voter turnout and a 7–10 percentage point increase in the vote shares and seat shares of the Labor Party. Finally, I exploit the adoption of compulsory voting at the national level to test for public policy effects. A synthetic control analysis comparing Australia with other comparable OECD nations demonstrates that the national adoption of compulsory voting caused significant increases in voter turnout and pension spending. I conclude by discussing the implications of these results for modern democracies with voluntary voting.

1 Compulsory Voting in Australia

When it comes to democracy, Australia is an innovator. Australia was one of the first nations to establish universal suffrage, instant-runoff voting, and the secret ballot. Keeping with the tradition of a fair, expansive democracy, compulsory voting was first advocated by Alfred Deakin at the turn of the

twentieth century (Evans, 2006). Deakin was the second Prime Minister of the Commonwealth and a member of the Protectionist Party. The political system in Australia approximated a two-party system with the Labor Party on the left pitted against several coalition parties on the right. The primary dimension of conflict between the parties was economic, with working-class voters and the Labor Party supporting larger government and more progressive policies relative to upper-class voters and the coalition parties (Hirst, 2002).

Australia's first system of compulsory voting was implemented in the state of Queensland in 1914. The Liberal Party government led by Digby Denham implemented the policy on the belief that the opposing Labor Party was better at "getting out the vote" (Evans, 2006). The other Australian states eventually followed suit, implementing compulsory voting for state elections at different times. Victoria was next in 1926, followed by New South Wales and Tasmania in 1928, Western Australia in 1936, and South Australia in 1941. For national elections, compulsory voting was implemented in 1924. Except in Queensland where the Labor Party initially opposed the policy, compulsory voting received unanimous support from all parties at the national level and in each state assembly. Table 1 presents the timing of the adoption of compulsory voting for national elections and for each state. This policy was adopted under the control of multiple different parties and with broad roll call support from all the parties.

Table 1. The adoption of compulsory voting (CV) across federal and state elections.

State	CV adopted	First election w/CV	Roll call	Controlling party
Queensland	1914	1915	47/72	Liberal
Federal Elections	1924	1925	75/75	Nationalist/Country
Victoria	1926	1927	65/65	Labor
New South Wales	1928	1930	90/90	Nationalist/Country
Tasmania	1928	1931	30/30	Nationalist
Western Australia	1936	1939	50/50	Labor
South Australia	1941	1944	39/39	Liberal/Country

Compulsory voting worked in the following way. Every voter was expected to show up to the polling place and cast a ballot on Election Day. They were not obligated to support any of the candidates or parties; they could cast a blank ballot if they preferred. Any citizen who did not vote and could not provide a valid excuse would have to pay a fine. For national elections, the fine was two pounds, and traveling and physical illness were explicitly listed as valid excuses. For state assembly elections, the fines varied but were never severe. According to anecdotal reports, the government was lenient with excuses and only a fraction of nonvoters were asked to pay the fine.

A historical reading of this period reveals four primary reasons that compulsory voting was implemented. First, it was believed that compulsory voting was a natural extension of compulsory registration, which was already in place (Hirst, 2002). If everyone voted, it would be easier to administer the election and detect fraud. Second, the fines levied on nonvoters could help to defray the cost of administering the election (Mackerras and McAllister, 1999). Third, compulsory voting was seen as the only way to ensure a fair election result (Hirst, 2002; Evans, 2006). During World War I, the issue of conscription was contentious and divisive. Parliament did not want to make a controversial decision on its own, so it proposed a public referendum on the issue. However, since the issue would potentially affect every Australian family, the Parliament felt that the referendum would only be legitimate if everyone voted. The debate surrounding the referendum planted the seed for compulsory voting in all federal elections (Evans, 2006).

The final impetus for compulsory voting involved partisan interests. The Labor Party on the left and the coalition parties on the right hoped to reduce a perceived advantage of their opponents (Mackerras and McAllister, 1999; Evans, 2006). Coalition (non-Labor) voters were more likely to have cars so that they could drive themselves and others to the polling place. On the other hand, the Labor Party had a larger supply of campaign workers and therefore a more extensive effort to bring Labor supporters to the polls. In order to reduce this wasteful competition, both sides felt that compulsory voting would make them better off.

All four reasons are somewhat practical. Compulsory voting would aid in the administration of elections, raise money, increase legitimacy, and reduce costly campaign efforts of the parties. One party did not push for compulsory voting at the expense of another. All parties supported the policy, and to the extent that the parties made a strategic calculation, they all thought

that compulsory voting would benefit them. Therefore, it appears that the adoption of compulsory voting arose more for practical or principled reasons than it did for strategic reasons.

While the adoption of compulsory voting was not random, the timing of the policy does not appear to be related to changes in political, economic, or demographic factors — a claim that I explicitly test later in this paper. Therefore, compulsory voting provides a rare opportunity to test for the effects of near-universal turnout on election outcomes and public policy. To assess the effects of compulsory voting on partisan election results, I exploit the differential timing of compulsory voting laws across Australian states with a simple difference-in-differences design. Then, in order to assess the policy consequences of compulsory voting, I employ synthetic control methods to compare changes in Australia's pension spending over time with changes in other comparable nations. Both of these designs require a parallel trends assumption which is extremely plausible, justified with data, and significantly weaker than the assumptions required for previous studies of the effects of near-universal turnout.¹

2 Who Voted in Australia before Compulsory Voting?

Education and class biases in turnout have been well documented in advanced democracies (Gosnell, 1927; Leighley and Nagler, 1992; Linder, 1994; Powell, 1980; Tingsten, 1937; Topf, 1995; Verba and Nie, 1972; Verba *et al.*, 1995). In general, wealthier and more educated citizens vote at higher rates than those of lower socioeconomic status. This effect is greatest for those nations with low rates of turnout such as the United States and Switzerland but still present in nations with high turnout such as Canada and the United Kingdom. Even in modern day Australia, which achieves 95 percent turnout through compulsory voting, poorer and less-educated citizens are more likely to abstain (McAllister, 1986). However, due to the lack of data, class biases in turnout have not been previously studied in Australia before the adoption of compulsory voting. Australia does not typically report

¹ For example, previous studies of the effects of near-universal turnout (Herron, 1998; MacKerras and McAllister, 1999; Highton and Wolfinger, 2001; Citrin *et al.*, 2003; Martinez and Gill, 2005; Bernhagen and Marsh, 2007; Petterson and Rose, 2007; Rubenson *et al.*, 2007) require the stronger assumption that observational differences in voter turnout across individuals are exogenous.

the names of individuals who turned out to vote, and no survey data were collected on voting before compulsory voting was established.

To my knowledge, only two sources provide data on the types of individuals who turned out in Australia during this period. First, in 1877, the State of Victoria reported voter turnout separately for property-owners and non-property-owners for that year's state assembly election.² Second, following Victoria's 1899 referendum on Australian federation, the state honored the event by publishing the names of all who had turned out. Analyzing both sources, I uncover the extent of turnout inequality in Australia before compulsory voting. As in many democracies today, wealthy, property-owning citizens were much more likely to vote than poor, non-property-owning citizens. This analysis does not suggest that property ownership or property values exhibit a causal effect on turnout. Similarly, these factors are not necessarily the most important predictors of voting. The analysis is descriptive, uncovering as best as possible who voted and who abstained from voting before the adoption of compulsory voting.

2.1 Turnout by Property Ownership in Victoria's 1877 Election

After his election to the Victoria State Assembly in 1877, Robert Clark, a working-class miner, requested a report of voter turnout by property-ownership. To my knowledge, this is the only Australian electoral return which provides any breakdown of turnout by demographic characteristics. The report presents turnout data for 37 of Victoria's 55 districts. These 37 districts contained 580,000 residents and 170,000 eligible voters. For most of the missing districts, the election for legislative assembly was uncontested, so no votes were cast.

At that time in Victoria, property-owning males were automatically registered to vote. If a non-property-owner wanted to register, he would have to pay 1 shilling. The property requirement was not severe; 59 percent of voting-age males were automatically registered as property-owners. This group included farmers, masons, shepherds, storekeepers, butchers, and "gentleman." Non-property owners included strictly working-class citizens such as laborers, servants, cooks, and gardeners. The 1877 report presents for each district the approximate number of eligible voters, the number of registered

² *Votes and Proceedings of the Legislative Assembly, 1877–78*, Vol. 1, p. 767.

property-owners, the number of registered non-property-owners, the number of voters who were property-owners, and the number of voters who were non-property-owners. From these data, we can back out the proportions of eligible property-owners and eligible non-property-owners who turned out in the state election. Alarming, only 18 percent of eligible non-property-owners turned out to vote compared to 66 percent of property-owners. Given the burden of registration, only 32 percent of eligible non-property-owners bothered to pay the shilling and get on the roll. Even conditional on registering, only 57 percent of those individuals turned out. Surprisingly, property-owners were more likely to vote than even the subset of non-property-owners who had paid to become registered. Put another way, property-owners comprised 84 percent of the electorate even though they only comprised 59 percent of the eligible voters.

2.2 Turnout by Property Ownership and Property Value in the 1899 Referendum

In 1899, the Australian states held a referendum on forming a federation. The measure was sure to pass in Victoria, but the state government wanted a high turnout to ensure legitimacy. To incentivize voters, the government offered a certificate to all those who turned out and recorded their names in a commemorative book. This book, held in the Parliamentary Library of Victoria, contains the names, occupations, and locations of the 163,783 men³ who turned out from the State of Victoria. By this time the registration fee was removed and registration was mandatory, so that all white men should have been registered and eligible to vote in the referendum.

Despite this exciting data source, there is no comparable list of all eligible voters from which we could assess their propensity to turn out in the referendum. However, the city of Bendigo has preserved some historical rolls of all registered voters, and they fortunately have their list from the exact same year, 1899. Bendigo rolls list the names, occupations, addresses, and property values of all registered voters. For each individual, the roll indicates the value of his residence and whether he is the owner or just an occupier of that residence. The 1899 Bendigo roll contains the names of 3,510 men. For each individual, I have digitized the data and manually searched for that person in the book of referendum voters. Sixty-one percent of Bendigo's

³ Women, unfortunately, did not yet have the right to vote in Victoria.

registered voters are confirmed as having voted in the referendum, consistent with historical accounts of the turnout rate in Bendigo (Maslunka, 1983).

Just as in 1877, we can assess whether property owners in Bendigo were more likely to vote than occupiers. Also, we can assess whether property values, a good proxy for wealth, are related to turnout. Figure 1 presents kernel regressions of turnout across property values separately for owners and occupiers. Consistent with expectation, owners were 10 percentage points more likely to vote than occupiers. Also, property values are highly correlated with turnout for both owners and occupiers. A single standard deviation increase in property value is associated with a 7 percentage point increase

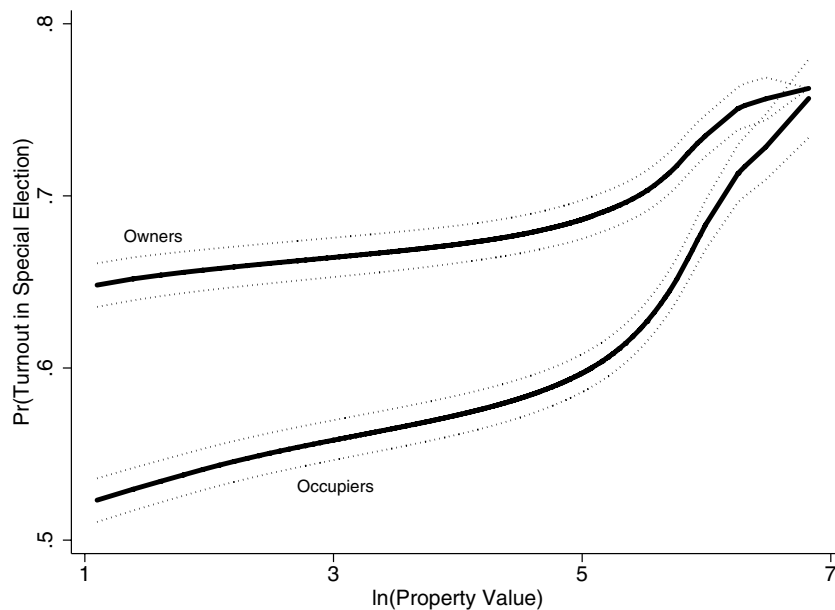


Figure 1. Turnout inequality in Bendigo, 1899.

The solid curves represent kernel regressions indicating the probability that an individual citizen turned out to vote in the 1899 special election as a function of the value of his residence. Separate kernel regressions are shown for property-owners and occupiers (non-owners). The dotted lines indicate standard errors. The kernel regression use the Epanechnikov distribution with a bandwidth of 1.5 and a p-width of 0.57, but the general finding is insensitive to specification.

in an individual's probability of voting for owners and a 10 percentage point increase in an individual's probability of voting for occupiers.⁴

These findings from 1877 and 1899 demonstrate a large degree of turnout inequality in Australia before the adoption of compulsory voting. Due to the lack of available data, these findings cannot be replicated outside of Victoria or Bendigo. However, we have no reason to suspect that turnout inequality was unique to these regions of Australia. Contemporary electoral reports indicate that turnout and registration rates in Bendigo were similar with other municipalities in Victoria and Victoria was comparable with other states in Australia. This analysis constitutes, to my knowledge, the first and only possible analysis of individual-level voting behavior in Australia before compulsory voting. Acknowledging the limitations, I take these results as evidence of a larger phenomenon across Australia.

As in many democracies today, Australia's wealthy citizens voted at much higher rates than working-class citizens under a system of voluntary voting. For this reason, any lessons drawn from Australia may be applicable to other democratic nations today. Also, since working-class citizens in Australia systematically preferred the Labor Party, these analyses suggest that compulsory voting and increased voter turnout would benefit the Labor Party and lead to more progressive policies in Australia. The next two sections explicitly test these hypotheses.

3 The Effects of Compulsory Voting in State Assembly Elections

In order to test the effect of compulsory voting on election results, I have collected the results of every state legislative assembly election from 1910 to 1950.⁵ The six Australian states provide an excellent opportunity to test for the effects of near-universal turnout because they adopted compulsory voting at different times. Employing a differences-in-differences design, I estimate the effect of compulsory voting on three different dependent variables: voter turnout, Labor Party vote share, and Labor Party seat share. As compulsory voting is implemented in one state, we can compare that

⁴ These numbers are estimated through separate ordinary least squares (OLS) regressions of turnout on property value for owners and occupiers.

⁵ Data were collected from the Australian Politics and Election Database, hosted by the University of Western Australia (<http://elections.uwa.edu.au/electionsearch.lasso>). 1910 is the first year for which I can obtain reliable electoral data for every state. Subsequent results are not sensitive to the choice of included years.

state's changes in voting behavior with the changes of other states at the same time. This design — in its simplest form — requires a “parallel trends assumption” that voting behavior would, on average, have parallel trends across states in the absence of any changes in compulsory voting laws. Later in the present paper, this assumption is discussed in detail, justified with statistical tests, and relaxed by allowing for state-specific trends.

Voter turnout and the electoral success of the Labor Party, measured in two different ways, are the primary outcomes of interest. During this time period, Australia had multiple parties competing for office. However, several conservative parties would typically form a coalition against the Labor Party, so the environment approximates a two-party system. The clear differences between the Labor Party and the coalition parties largely revolved around economic issues; Labor Party preferred government intervention to protect workers, and coalition leaders preferred free markets. By analyzing the Labor Party's success, we capture the bulk of political competition occurring during this time period. Also, given the economic divide between the Labor and coalition parties, we have a clear prediction about the effect of increased turnout on the Labor Party's success.

Before turning to the explicit econometric tests, I present the raw data for the analysis in Figure 2. The top panel of the figure presents the level of voter turnout in every election and the bottom panel presents the Labor Party's vote share. Hollow triangles represent elections before compulsory voting, and solid triangles represent elections after the adoption of compulsory voting in that particular state. The naked eye can readily detect the significant effect of compulsory voting on voter turnout. In each state, turnout jumped dramatically when compulsory voting laws were implemented. However, the effect of compulsory voting on Labor Party vote share is less apparent, because there is so much natural variation in this variable from year to year. (This variation may explain why conservative parties in Australia continued to support compulsory voting laws even though the tests in this study reveal that they suffered from the policy.)

To quantify the effects of compulsory voting, I estimate the following equation using OLS separately for each dependent variable (turnout, Labor vote shares, and Labor seat shares):

$$DV = \alpha(\text{Compulsory Voting})_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (1)$$

This model includes a dummy variable for compulsory voting, state fixed effects (γ_i), and year fixed effects (δ_t), allowing each state and each year to

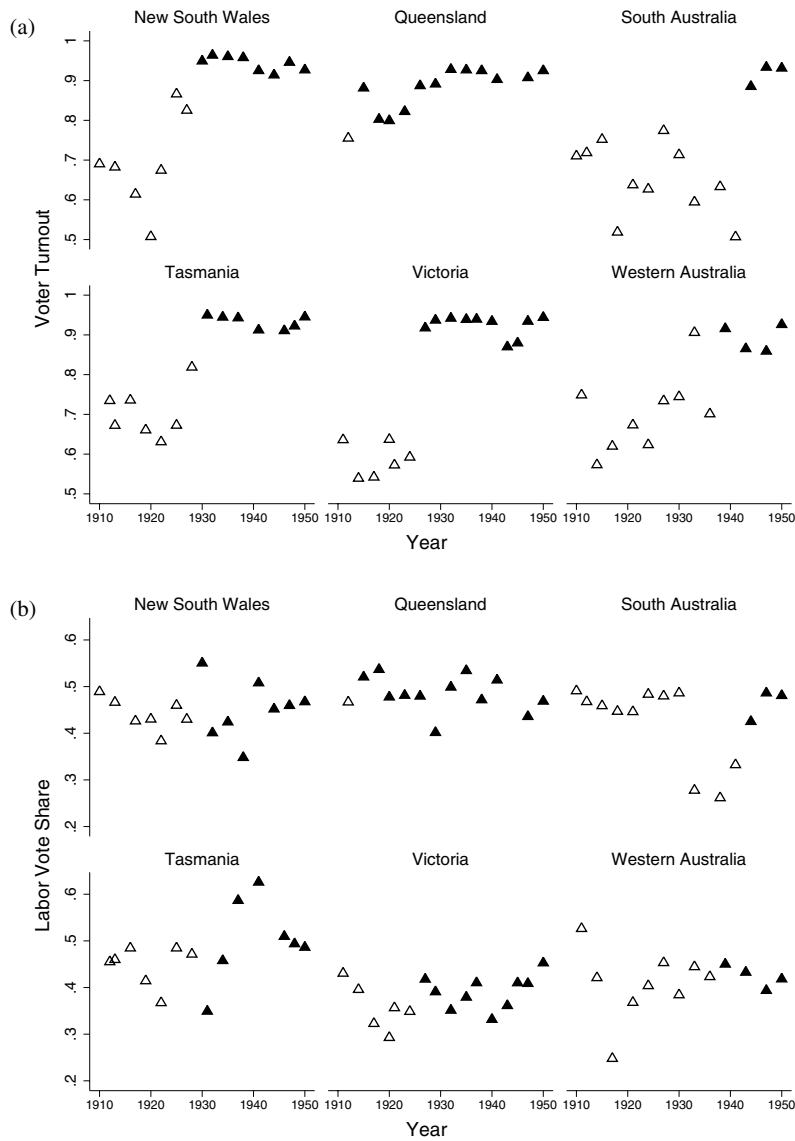


Figure 2. Turnout and labor vote share across state assembly elections (1910--1950).

The graph presents the level of voter turnout (top panel) and Labor Party vote share (bottom panel) in every state legislative assembly election between 1910 and 1950. Hollow triangles indicate an election held before the adoption of compulsory voting in that state, and solid triangles indicate an election after the adoption of compulsory voting.

have their own idiosyncratic effect on the dependent variables. The coefficient α indicates the effect of compulsory voting on the dependent variable. As a robustness test, I also include linear time trends for each state, parametrically relaxing the parallel trends assumptions by allowing each state to have a unique trend over time.

Table 2 presents the results of all three difference-in-differences regressions along with the three corresponding models with state-specific trends. Column 1 shows that the implementation of compulsory voting increased turnout by 24 percentage points on average, from 67 to 91 percent. Columns 3 and 5 show that compulsory voting increased the Labor Party's vote share and seat share by 9 and 7 percentage points, respectively. These results are both substantively and statistically significant. Since the Labor Party's average seat share was 43 percent before the adoption of compulsory voting, these effects were meaningful enough to change the control of power in many legislative assemblies. Columns 2, 4, and 6 present the robustness tests allowing for state-specific trends and all three results are virtually unchanged. These additional tests lend credibility to the parallel

Table 2. Effects of compulsory voting in state assembly elections.

	DV = Turnout		Labor vote share		Labor seat share	
	(1)	(2)	(3)	(4)	(5)	(6)
Compulsory voting	.243 (.042)	.245 (.046)	.092 (.033)	.093 (.040)	.069 (.048)	.098 (.065)
State fixed effects	X	X	X	X	X	X
Year fixed effects	X	X	X	X	X	X
State-specific trends		X		X		X
Observations	85	85	85	85	85	85
R-squared	.925	.933	.759	.790	.660	.721
SER	.056	.052	.049	.045	.110	.098

State-clustered standard errors in parentheses.

Each observation is a state assembly election between 1910 and 1950. "Compulsory Voting" is a dummy variable, indicating whether compulsory voting laws were in place. The dependent variables are coded to theoretically range from 0 to 1, so the coefficients in columns 1, 3, and 5 indicate that compulsory voting increased voter turnout, Labor vote share, and Labor seat share by 24, 9, and 7 percentage points, respectively.

trends assumption, because relaxing the assumption is inconsequential for the subsequent estimates.

According to these results, average turnout in the absence of compulsory voting during this period would have been 67 percent, and the Labor Party would have received 44 percent of the votes. However, average turnout with compulsory voting would have been 91 percent, and Labor would have received 53 percent of the votes. This suggests that 78 percent of the new (marginal) voters brought to the polls by compulsory voting supported the Labor Party compared to just 44 percent of older (regular) voters.⁶ This estimate is substantively large but quite plausible, given what we now know about turnout inequality prior to compulsory voting. Among the 91 percent of voters who voted under compulsory voting, only 61 percent of Labor voters turned out to the polls under voluntary voting while 88 percent of non-Labor supporters turned out. These calculations independently confirm the extent of turnout inequality in Australia before compulsory voting. Before compulsory voting, non-Labor supporters were 1.4 times more likely to vote compared to Labor supporters. When compulsory voting caused an exogenous increase in turnout, election results changed dramatically.

3.1 Estimating the Standard Errors

While the previous analysis includes 85 different elections, each election does not represent an independent observation. Serial correlation or state-specific clustering could lead traditional approaches to overestimate the precision of our estimates. Table 2 reports state-clustered standard errors which are designed to accommodate these concerns, but these standard errors can be misleading when the number of clusters is small. See Bertrand *et al.* (2004) for a more detailed discussion of these issues.

In the Online Appendix, I discuss these issues and demonstrate the statistical significance of these results under numerous different methods for estimating standard errors. For example, the estimated effect of compulsory voting on Labor vote share is statistically significant ($p < 0.05$) even when calculating the standard errors by nonparametric bootstrap, block bootstrap, or random permutation.

⁶ The 78 percent figure comes from solving this simple equation for X: $0.67 \cdot 0.44 + 0.24 \cdot X = 0.91 \cdot 0.53$. See Fowler (2012) for more details on this test.

3.2 *Tests for Confounding Demographic Trends*

The parallel trends assumption implies that the timing of the adoption of compulsory voting did not coincide with any state-specific changes in demographic or political factors that could have independently influenced voter turnout or election results. Historical analysis of the adoption of compulsory voting lends credence to this assumption. The insensitivity of the results to the inclusion of state-specific trends provides further support. In this section, I search for further evidence that could potentially falsify or bolster the parallel trends assumption. I find that the adoption of compulsory voting across states was not correlated with any changes in economic or demographic variables.

To conduct these tests, I collected data for each state from the Australian censuses in 1911, 1921, 1933, and 1947.⁷ For all six states at each of these four time points, I obtained data on the state's population and the proportion of the state's population that was under 21, married, born in Australia, identifying with the Church of England, and working in the manufacturing sector. Then, treating each variable as an outcome variable, I regress each variable on a dummy variable for compulsory voting, state fixed effects, and year fixed effects. This procedure mimics the difference-in-differences regressions shown previously. The results of each regression are shown in Table 3. For each test, the placebo "effect" of compulsory is statistically and substantively indistinguishable from zero. These results provide further support for the parallel trends assumption and demonstrate that the adoption of compulsory voting across states was not correlated with these demographic or economic changes.

4 **The Effects of Compulsory Voting on Public Policies**

The significant effects of compulsory voting on election results suggest that compulsory voting may have influenced public policy as well. The Labor Party held systematically different positions than the other parties over many issues including unions, social spending, immigration policies, and the

⁷ One natural robustness test would involve including these time-varying covariates as control variables in the original regressions. However, these tests cannot be conducted because these variables are not available for all election years. As a close substitute, I test whether changes in these covariates coincide with the timing of compulsory voting across states.

Table 3. Tests for confounding demographic trends.

Dependent variable	“Effect” of compulsory voting	Standard error	<i>p</i> -Value
ln(Population)	−.036	.035	.343
Under 21	−.002	.006	.785
Married	.004	.002	.070
Born in Australia	.004	.015	.817
Church of England	−.011	.011	.359
Manufacturing	.005	.004	.297

Each row represents a difference-in-differences regression where the dependent variable is regressed on a dummy variable for compulsory voting, state fixed-effects, and year fixed-effects. The estimate is the coefficient on “compulsory voting.” Standard errors are clustered by state. With the exception of ln(Population), the dependent variables are coded as proportions (0 to 1), so a coefficient of .004 indicates the compulsory voting corresponds to an increase in the dependent variable by 0.4 percentage points.

size of government. Therefore, a greater presence of Labor members in state assemblies and the national Parliament may have had significant effects on the lives of Australians at the time. Moreover, the new wave of working-class voters may have led all parties to shift in a progressive direction. A historical account of Australia’s national politics around this time period suggests several potential effects of compulsory voting on public policy.

The majority party’s platform changed dramatically after the adoption of compulsory voting for federal elections in 1924. In the 1922 election the Nationalist Party maintained power, announcing a conservative domestic policy: “First and foremost, we are against class legislation and class government” (Hughes, 1922). In 1925, the first election under compulsory voting, the Nationalist Party maintained power but shifted dramatically on these issues: “It has to be recognized that even under the conditions existing in Australia, the wages of our workers are not sufficient to enable them to safeguard against these evils [sickness, unemployment, and old-age]” (Bruce, 1925). In just a three-year span, the majority party made a clear turn toward progressive domestic policies, and the introduction of more working-class voters into the electorate and increased presence of the Labor Party may have provided impetus for such a change.

Statistical evidence of the effects of compulsory voting comes from data on social spending across various nations over time (Lindert, 1994). I focus specifically on pension spending, because this was a key issue of disagreement between the parties, just as there was tension between the left and right in many advanced democracies over pension policies (Baldwin, 1990). In Australia, the political parties disagreed strongly regarding the size and structure of the federal pension program, but for various reasons, they did not diverge significantly on other social issues such as education, health, welfare, and housing (Hirst, 2002). Because the Labor Party and working-class voters systematically preferred a more progressive pension program relative to the coalition parties and upper-class voters, I hypothesize that compulsory voting increased pension spending in Australia.

To test this hypothesis, I estimate the effect of compulsory voting at the national level on voter turnout and pension spending by comparing Australia with 20 other developed nations across three different time points: 1910, 1920, and 1930.⁸ Again, I rely on a difference-in-differences design. However, in order to maximize the plausibility of the parallel trends assumption, I employ synthetic control methods (Abadie *et al.*, 2010) to construct synthetic control units that are comparable with Australia before the adoption of compulsory voting.

For each test, the synthetic control unit is the weighted average of other nations that best mirrors Australia in terms of voter turnout or pension spending in 1910 and 1920. New Zealand, France, Canada, and the United Kingdom receive the greatest weights in the turnout analysis, because they closely mirror Australia's level and trend in turnout before compulsory voting, while Denmark and New Zealand receive the greatest weights in the pension analysis. The incorporation of other economic, demographic, and political variables into the weighting algorithm does not change the subsequent results. Moreover, simpler difference-in-differences designs which weigh all comparison units equally yield similar results. More details on these analyses are available in the Online Appendix.

Figure 3 presents the trends in voter turnout (top panel) and pension spending (bottom panel) for Australia and the synthetic control units. Their

⁸ Turnout and pension data are from Lindert (1994) with corrections made to Australia's turnout data. This cross-country design could also be employed to assess the impact of compulsory voting on the electoral success of left-wing parties, but the state assembly design is much cleaner for that purpose and it avoids the challenges of coding left-wing parties across countries and comparing across different electoral systems.

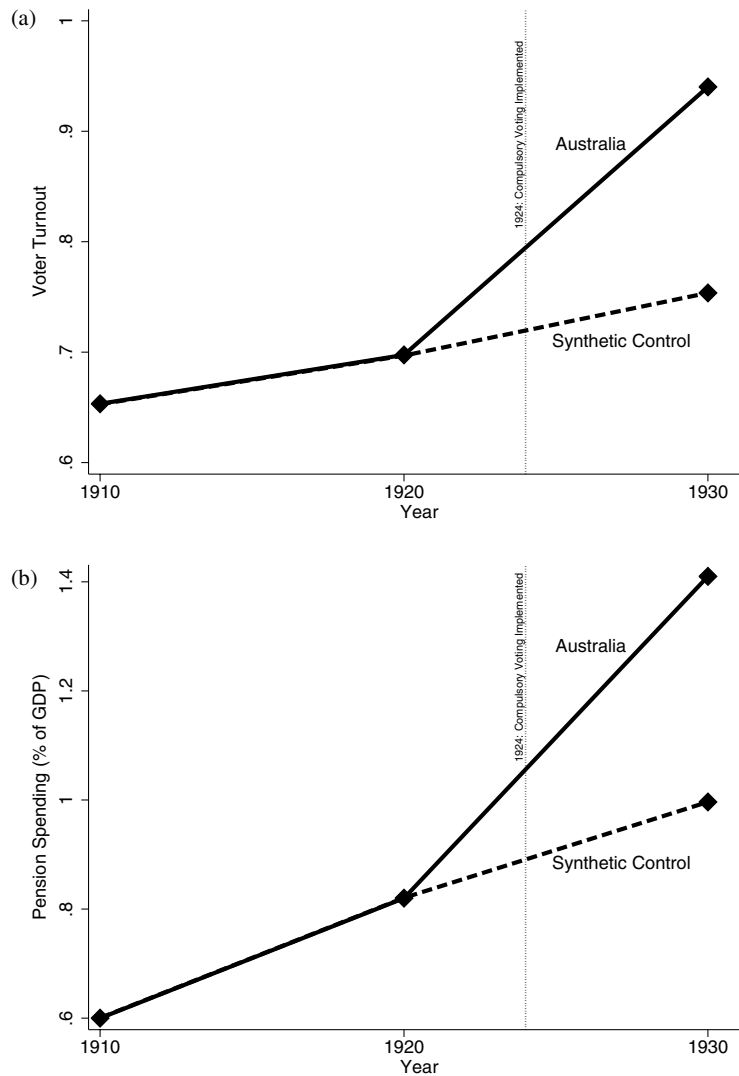


Figure 3. The effects of compulsory voting on turnout and pension spending.

The figure presents the level of voter turnout (top) and pension spending (bottom) in Australia and in a synthetic control nation across three points in time. The synthetic control unit is a weighted average of other OECD countries that closely mirrored Australia before the adoption of compulsory voting for national elections in 1924. Both turnout and pension spending increased dramatically in Australia relative to the synthetic control nation after the adoption of compulsory voting.

levels of turnout and pension spending are nearly identical in 1900 and 1910. However, after the adoption of compulsory voting in 1924, voter turnout and pension spending increased dramatically relative to the synthetic control units. Difference-in-differences calculations indicate that compulsory voting increased voter turnout by 18.6 percentage points and pension spending by 0.41 percentage points of gross domestic product (GDP). In 1930, the pension spending of the control group was just under 1 percent of GDP, so this effect represents a more than 40 percent increase in the number of federal dollars going toward old age pensions — a substantively significant effect.

To assess the statistical significance of these estimates, I conduct placebo tests on the other countries, pretending that a policy change occurred between 1920 and 1930 in each of the comparison units. For each of the 20 countries other than Australia in this analysis, I construct a synthetic control for each country, excluding Australia as a potential control unit. I then calculate the difference-in-differences in pension spending between each country and its synthetic control from 1920 to 1930. The results of these placebo tests are provided in the Online Appendix. For both turnout and pension spending, only one country exhibits a difference-in-differences estimate as large as Australia. The dramatic increases in turnout and pension spending in Australia between 1920 and 1930 are substantively large and much greater than we would expect to see by chance alone. Taken together, the analyses of state assembly elections and federal policy over time provide strong evidence that near-universal turnout can have significant consequences for both partisan election results and public policy.

5 Discussion of Alternative Explanations

Alternative explanations of this study's findings will likely arise in two forms. First, the timing of the adoption of compulsory voting may have been endogenous to changes in turnout or support for the Labor Party. Specifically, there may have been some reason that states adopted compulsory voting precisely when the Labor Party was expected to increase its electoral success. Second, compulsory voting may have influenced election results through some mechanism other than increased turnout. Perhaps the introduction of compulsory voting caused people to change their partisan attitudes in favor of the Labor Party, and the effect of the policy on elections had nothing to do with increased turnout among the working class.

In any observational analysis, alternative explanations cannot be ruled out entirely. However, I have made the best possible effort to raise and assess the plausibility of these explanations through historical evidence, previous research, and my own statistical tests.

Was the timing of compulsory voting truly exogenous? The difference-in-differences analyses in this paper assume that election results and public policies would have followed similar trends across states and countries in the absence of any changes in compulsory voting laws. For example, the results would be biased if compulsory voting was adopted at a time when the Labor Party was expected to improve its electoral success. Perhaps the Labor Party pushed for compulsory voting as it was gaining momentum. Conversely, the other parties may have pushed for compulsory when they expected to lose support in the next election. However, the adoption of compulsory voting received widespread support from all political parties, so these explanations are unlikely. Moreover, previous election results are uncorrelated with the timing of compulsory voting. As discussed previously, compulsory voting was primarily implemented for practical reasons. After numerous discussions with Australian historians, I am unaware of any confounding variable which could have simultaneously led to compulsory voting and an increase in support for the Labor Party. The decision to adopt compulsory voting appears to be orthogonal to the partisan attitudes of the citizenry and any trends in these attitudes. Also, the state-level results are unchanged if the parallel trends assumption is relaxed by allowing for state-specific trends. Lastly, tests for confounding demographic trends in Table 3 reveal no evidence that the adoption of compulsory voting was correlated with demographic or economic changes.

Even if compulsory voting increased the electoral success of the Labor Party, it may have done so through some mechanism other than near-universal turnout. This study of the effects of compulsory voting is less interesting for the study of democracy if the mechanism has to do with something other than increased turnout. Could compulsory voting have dramatically changed election results or public policy independent of its effects on turnout? Perhaps voters were upset with the coalition parties for passing compulsory voting and decided to shift their support to the Labor Party. This seems unlikely since all parties supported the policy change. Perhaps the presence of new voters at the polls changed the political attitudes of old voters causing them to shift toward the Labor Party. However, if anything, we might expect that this phenomenon would work against my findings if

upper-class Australians became lionized against the Labor Party because of the new presence of working-class voters at the polls. A historical analysis suggests no reason that compulsory voting would cause a shift toward the Labor Party through any mechanism other than increasing turnout.

Changes in voter turnout do not take place in a vacuum. The introduction of new voters to the polls will lead candidates to change their campaign strategies and shift their policy platforms. However, these phenomena do not pose a problem for my estimates because they are all part of the downstream effects of compulsory voting and increased turnout. Presumably, the shift in political platforms is one mechanism for the observed changes in pension spending, over and above the increased electoral success of the Labor Party.

6 Discussion and Conclusion

Advanced democracies expend incredible resources in the administration of elections in the hope that election results and public policies will closely reflect the preferences of the citizenry. However, a significant proportion of citizens often abstain from voting. Worse yet, those who abstain are systematically different from those who vote, meaning that election results may not accurately reflect the preferences of all citizens. Aware of this problem, political scientists have extensively studied voter turnout, typically assessing the correlates and causes of voting. Despite this collective effort to understand turnout, there is little evidence connecting voting to tangible outcomes. How would elections and public policies change if everyone voted?

Previous attempts to address this question suffer from methodological problems. Correlations between turnout and various outcomes lack a causal interpretation because confounding variables influence both turnout and the outcomes of interest and the outcomes of interest may influence voter turnout. Comparisons of voters and nonvoters within an electorate also suffer from the possibility of confounding variables or systematic measurement error in surveys. Previous quasi-experimental approaches lack external validity because the subset of voters influenced by the quasi-experiment is unrepresentative of all nonvoters. In short, previous research has failed to answer the counterfactual question of interest. In a democracy with voluntary voting, researchers have been unable to determine the effects of near-universal turnout.

The analysis presented here largely overcomes the problems of previous studies. The timing of the introduction of compulsory voting in different Australian states appears to be exogenous to partisan attitudes and other political events, so difference-in-differences methods can estimate the causal effects of compulsory voting laws. Moreover, because compulsory voting caused a substantial increase in voter turnout, the subset of citizens influenced by the policy is nearly the entire population of nonvoters. This analysis brings us closer than ever before to answering the extreme counterfactual question: “what if everyone voted?”

Before the implementation of compulsory voting, Australia was similar to many advanced democracies today. Less than 70 percent of citizens voted in federal or state elections, and wealthy citizens were much more likely to vote than working-class citizens. Given these patterns, the lessons learned from Australia may be applicable to many democracies today. With the extent of turnout inequality in the United States, Switzerland, Poland, and many other countries, the effects of compulsory voting could be just as great in these contexts.

“Democracy’s unresolved dilemma” is that elections do not accurately reflect the preferences of the citizenry (Lijphart, 1997). Systematic turnout inequality means that some citizens will be better represented than others. In this study, I exploit a rare opportunity to test the extent of this dilemma. Before the introduction of compulsory voting in Australia, election results and public policy were drastically different from the preferences of the citizens. When near-universal turnout was achieved, elections and policy shifted in favor of the working-class citizens who had previously failed to participate. While Australia has largely resolved the problem, inequalities in voter turnout remain in most advanced democracies. Increased turnout has tangible effects on partisan election results and public policies, and those effects will benefit the disadvantaged subset of citizens who otherwise would have abstained from the political process.

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Online Appendix

Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia

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This document provides additional information as a supplement to the original paper. Details are provided regarding the coding of data, different estimates of standard errors, the robustness of the results, and the synthetic control analysis. Feel free to contact the author with questions or comments.

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I. Coding of Bendigo Turnout in the 1899 Referendum

The 1899 electoral rolls from Bendigo provide the name, occupation, address, property value, and ownership status of every registered voter in the town. I digitized these records by manually transcribing the information and double checking for accuracy. There are three different records, one for each of Bendigo's three wards. I removed all women from the record since they were not legally allowed to vote in the statewide election. Likely females were identified by first name. Also, the record typically did not list an occupation for women, which helped to resolve the registrants' gender in ambiguous cases.

The record of citizens who voted in the 1899 Referendum provides the name, address, town, and occupation of all Victorian men who voted in the election. This record is available in the State Library of Victoria and is available for purchase on cd-rom from MacBeth Geneology. The record includes over 3000 hand-written pages.

After digitizing the records of Bendigo registrants, I manually searched for each male registrant in the record of voters. In the simplest cases, matches were made by name and town. For example, there is one person named Benjamin Aarons in the Bendigo electoral rolls. I did not find a Benjamin Aarons from Bendigo in the list of voters, so I coded this individual as having abstained. Similarly, there is one person names James Abberton. I did find one James Abberton from Bendigo on the list of voters, so he is coded as having voted. When available, I recorded middle initials and suffixes (i.e. Jr.), but Thomas J. Alderson would still be coded as having voted if I found a Thomas Alderson (with no middle name or initial) from Bendigo in the list of voters.

When multiple Bendigo registrants had the same name, the coding was more complicated. To simplify this problem, I removed the 7 last names from the analysis where more

than 20 Bendigo registrants shared the name (Brown, Jones, Miller, Roberts, Smith, Thomas, Williams). If I attempt to code these names as well, the results are unchanged. Suppose that two Bendigo registrants share the same name, Arthur Armstrong. I would search for an Arthur Armstrong from Bendigo in the list of voters. If I found two, then I coded both as having voted. If I found none, then both were coded as having not voted. If I found 1, I would compare middle initials, suffixes, occupations, and street names to determine which one voted and which one did not.

In some rare cases, the same voter appears to be listed in the Bendigo rolls twice. For example, someone might reside in two residences in different wards, appearing on the roll in two separate wards. In these cases, I deleted one of the records. As a rule, I deleted the observation where the address did not match with the list of voters, assuming that this record constituted a secondary residence.

Importantly, I prevented myself from seeing the data on property ownership or property value when making these decisions, so I could not have influenced the results by adjusting my coding decisions to fit the data. This data was hidden and merged in after all coding was complete. As a result, any mistakes in coding will likely diminish rather than augment any differences in turnout across property ownership and property values.

II. More Details Regarding Standard Errors in the Analysis of State Assembly Elections

Statistical inferences are tricky with difference-in-difference designs, particularly when the number of treated units is small (Bertrand et al. 2004). In this section, I focus on the 9 percentage point estimate of the effect of compulsory voting on Labor vote share (Model 3 in Table 2 of the original paper), and I discuss various approaches to estimating the standard errors. Table A1 presents the standard error (point estimate = .092) estimated in 6 different ways.

The first row presents the traditional OLS standard errors. This approach could overestimate the precision of the estimate under the likely possibilities of serial correlation and heteroskedasticity. The second row corrects for heteroskedasticity using the Huber-White approach. The third row allows for state-specific serial correlation and heteroskedasticity, estimating state-clustered standard errors. Because this is the typical approach used for panel designs, I report these standard errors throughout the paper. However, this approach can be misleading if the number of clusters is too small.

The fourth row presents the result of a non-parametric bootstrap. With this approach, I randomly sample 85 observations from my data set with replacement (85 is the number of elections in the original analysis) and estimate the effect of compulsory voting with that new sample. I repeat this procedure 10,000 times and estimate the standard error as the standard deviation of these bootstrap estimates. The block bootstrap, shown in row 5, is identical to the non-parametric bootstrap with one key exception. Instead of sampling observations independently, I sample states as blocks. This approach accounts for the possibility that we do not have 85 independent observations but 6 independent states.

Lastly, I design a random permutation test specifically for the problem at hand. For each state, I randomly choose a year between 1910 and 1950 (drawn from a uniform distribution) and

assign that year as the adoption of compulsory voting in that state. Then I estimate the placebo effect of compulsory voting under this hypothetical scenario. I repeat this procedure 100,000 times and report the standard error as the standard deviation of those permutation estimates. Figure A1 presents the results of those random permutations. We see that the estimates are normally distributed with mean zero and standard deviation of .035. The estimated effect from the real data is shown as a red line. Less than 0.5% of the random permutations resulted in an estimate larger than the actual estimate of .092, suggesting that our estimate is much larger than we would expect to see under the null hypothesis where compulsory voting has no effect.

The estimated effect of compulsory voting on Labor vote is statistically significant ($p < .05$) for all approaches to estimating the standard errors. Due to the difficulty in making statistical inferences with this type of data, I would not recommend focusing on any one particular approach. Instead, I would emphasize that every approach yields the same inference: this effect is much larger than we would expect by chance alone.

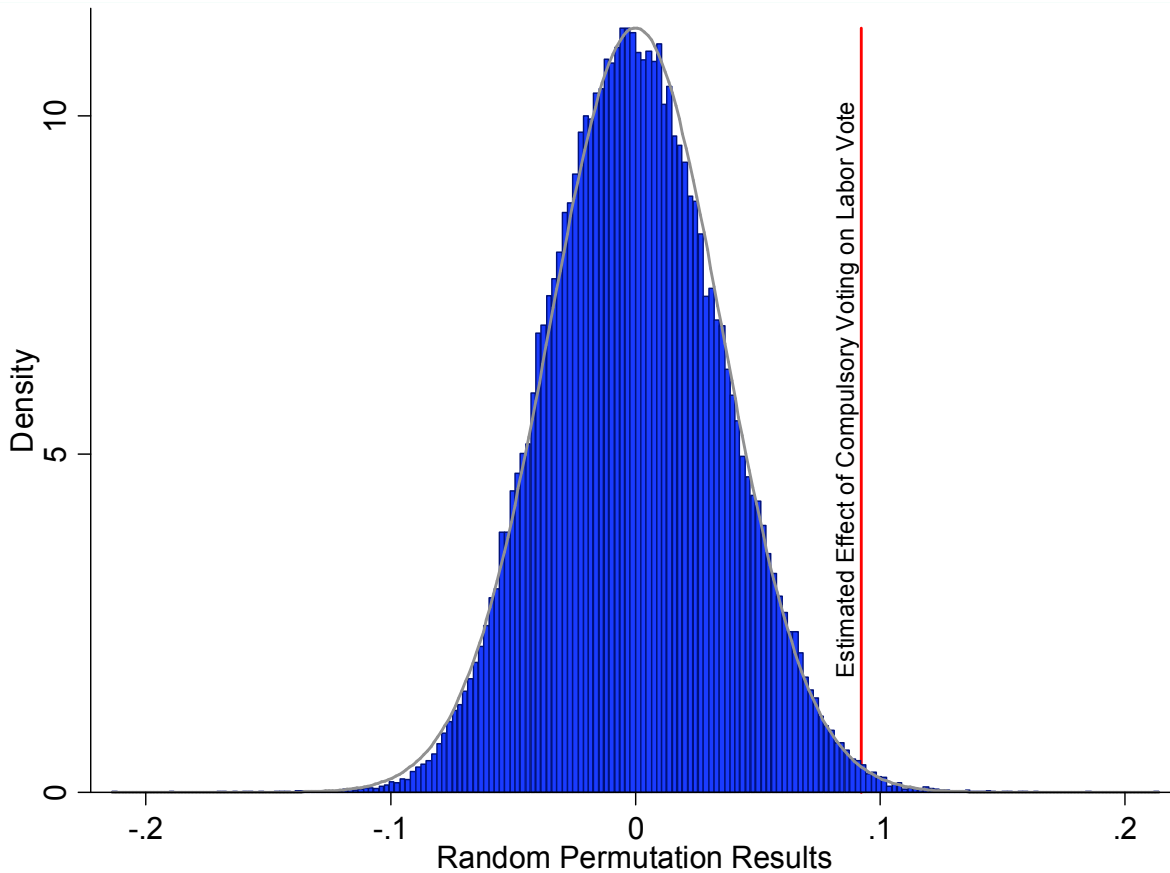
The figure shows the results of a random permutation test, showing the statistical significance of the difference-in-difference estimate of the effect of compulsory voting on Labor voteshare. For each state, I randomly choose a year between 1910 and 1950 (drawn from a uniform distribution) and assign that year as the adoption of compulsory voting in that particular state. Then I estimate the placebo effect of compulsory voting under this hypothetical scenario. I repeat this procedure 100,000 times. The figure presents the results of those random permutations. We see that the estimates are normally distributed with mean zero and standard deviation of .035. The estimated effect from the real data is shown as a red line. Less than 0.5% of the random permutations resulted in an estimate larger than the actual estimate of .092,

suggesting that our estimate is much larger than we would expect to see under the null hypothesis where compulsory voting has no effect.

Table A1. Standard Error on the Estimated Effect of Compulsory Voting on Labor Vote

Traditional	(.028)**
Heteroskedasticity Robust	(.031)**
State-Clustered	(.033)*
Non-parametric Bootstrap	(.040)*
Block Bootstrap	(.046)*
Random Permutation	(.035)*

Figure A1. Random Permutation Results



III. Robustness of State Assembly Results to Alternative Specifications

The table demonstrates the robustness of the results to various specifications. Rows 1 and 2 are copied from Column 3 and 4 in Table 2 of the original paper. These models estimate the effect of compulsory voting (coded from 0 to 1) on Labor Party voteshare (also coded from 0 to 1). Both estimates include state fixed effects and year fixed effects, and the latter allows for state-specific trends. The next 4 rows model time in different ways. Although these estimates are slightly smaller than in Row 1, the estimates are substantively and statistically significant for all model specifications. All models include state fixed effects.

Row 3 models the time trend linearly. Row 4 includes year and year squared, modeling the time trend as a second order polynomial. Row 5 also includes year cubed, modeling the time trend as a third order polynomial. Lastly, Row 6 includes different linear time trends before and after the adoption of compulsory voting. Compulsory voting may have changed a state's trend in Labor vote share in addition to its mean. This model mimics a regression discontinuity design where we estimate the expectation of Y at a threshold by local linear regression. This model estimates the expected jump in Labor vote share immediately after a state adopts compulsory voting. All standard errors are clustered at the state level.

Table A2. Robustness Checks

Specification	Estimate	Standard Error
Year Fixed Effects	.092	.033
State-Specific Time Trends	.093	.040
Linear Time Trend	.062	.028
Quadratic Time Trend	.061	.022
Cubic Time Trend	.057	.032
Different Time Trend after CV	.060	.023

IV. More Details on the Synthetic Control Analysis

The paper employs the synthetic control method of Abadie, Diamond, and Hainmueller (2010) to estimate the effects of compulsory voting at the federal level on turnout pension spending. I estimate that compulsory voting increased voter turnout by 18.6 percentage points and pension spending by 0.41 percentage points of GDP. Below I provide more details on these estimates.

The synthetic control groups for each test are the weighted combinations of 20 other OECD countries that best match Australia in 1910 and 1920 (before compulsory voting) in terms of turnout, pension spending, and trends in these variables. Table A3 presents the weights estimated for each country for the two separate analyses. 10 comparison countries are excluded from the turnout analysis because turnout data is unavailable or missing, while all 20 comparison countries are included in the pension analysis. New Zealand, France, Canada, and the United Kingdom receive the greatest weights in the turnout analysis. Denmark and New Zealand receive the greatest weights in the pension analysis.

To address statistical significance of the findings, I conduct placebo tests in the other nations where there was no policy change between 1920 and 1930. For each nation, I construct a synthetic control unit, excluding Australia as a potential control country. Then, pretending that a policy change took place between 1920 and 1930 in that country, I calculate the difference-in-difference estimate. The table presents these placebo estimates for each country. Again, only 10 comparison countries are analyzed for turnout while 20 countries are analyzed for pension spending. In both cases, only one country's absolute value is as great as the estimate in Australia, suggesting that the change in Australia's turnout and pension spending between 1920 and 1930 is statistically and substantively larger than we would expect to see by chance alone.

Table A3. Country Weights in Synthetic Control Analyses

Country	Turnout Analysis	Pension Analysis
Argentina		.002
Austria		.002
Belgium		.004
Brazil		.002
Canada	.100	.002
Denmark	.056	.741
Finland	.061	.002
France	.109	.009
Greece		.002
Italy		.002
Japan		.002
Mexico		.002
Netherlands	.055	.009
New Zealand	.427	.187
Norway	.031	.006
Portugal		.002
Spain		.003
Sweden	.015	.005
United Kingdom	.085	.014
United States	.060	.002

Table A4. Placebo Tests for the Effect of Compulsory Voting on Pension Spending

Country	Turnout Estimates	Pension Estimates
Australia	.186	.414
Argentina		-.069
Austria		.437
Belgium		.140
Brazil		-.069
Canada	-.251	.151
Denmark	.049	.090
Finland	-.155	-.069
France	-.018	.022
Greece		-.036
Italy		-.069
Japan		-.069
Mexico		-.069
Netherlands	.080	-.197
New Zealand	.074	.198
Norway	.013	-.063
Portugal		-.069
Spain		-.054
Sweden	.060	.192
United Kingdom	.095	-.052
United States	.074	-.069