

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 Genealogy. 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.

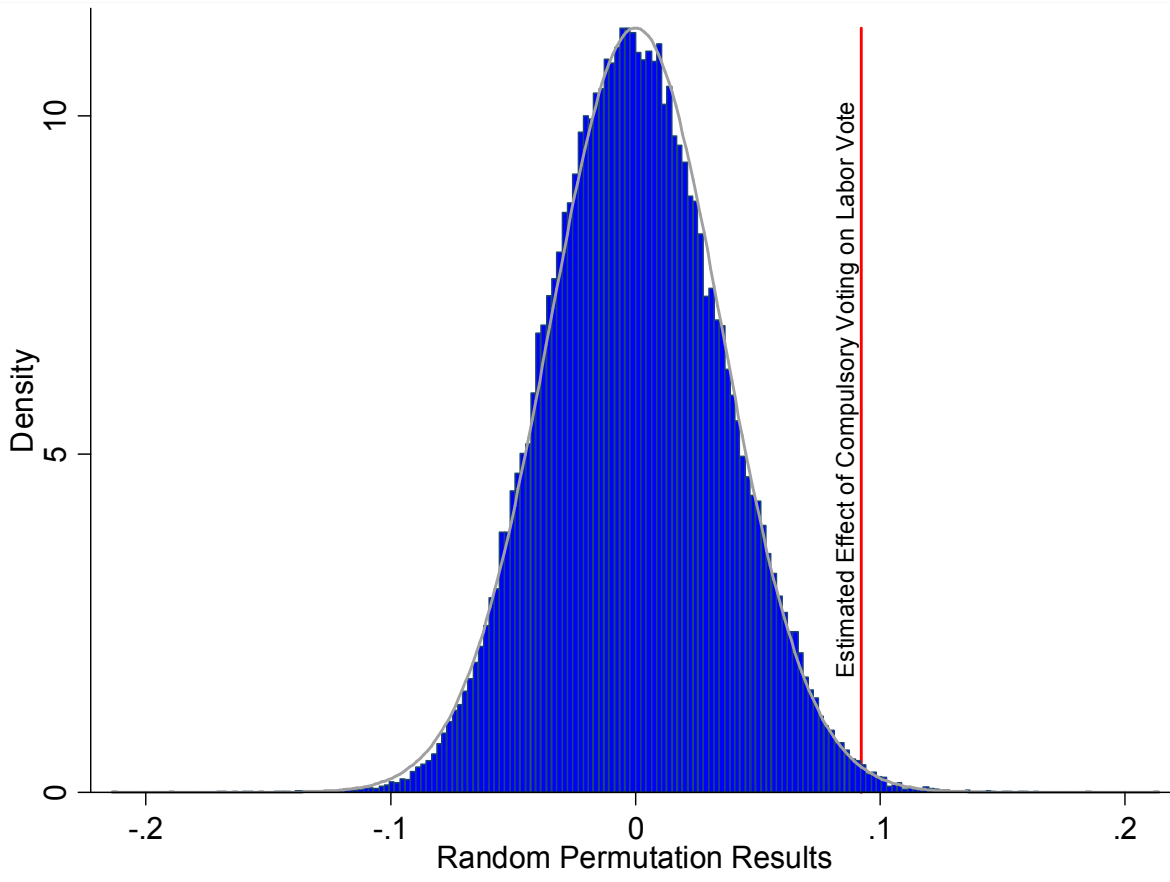
The figure shows the results of a random permutation test, showing the statistical significance of the difference-in-difference estimate of the effect of compulsory voting on Labor voteshare. For each state, I randomly choose a year between 1910 and 1950 (drawn from a uniform distribution) and assign 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,

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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