POLITICAL CONSEQUENCES OF LOWERING THE VOTING AGE: EVIDENCE FROM INDIA'S 61ST AMENDMENT

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ABSTRACT. Large gaps in political preferences between the young and the old suggest that compositional changes in the electorate can be expected to alter electoral outcomes. We study the political consequences of a franchise extension that enfranchised to 50 million new voters - the lowering of the voting age from 21 to 18 in India in 1988 - using a difference-in-differences design and find that the extension had zero or mildly negative effects on the size of the registered electorate and voter turnout. Consequently, the reform had minimal effects on electoral competition. We interpret these findings as a possible explanation for the youth representation gap in politics – that parties correctly anticipate low youth participation and as such fail to tailor electoral campaigns or public policies to the youth electorate, which potentially further disillusions young voters, thereby producing a vicious cycle of youth under-representation.

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1. Introduction

Policy preferences typically vary across the life cycle: young voters may care more about education, employment, and climate, while older voters may care more about fiscal and socio-cultural issues. Since electoral politics the world over is largely the preserve of middle aged men[1], one might reasonably suspect that public policy may not be representative of the preferences of all voters. This problem is particularly acute in the developing world, where the median age is typically even lower, but the political class is often dynastic and geriatric[2]. Lowering the voting age has been proposed as a policy to improve the representation of youth interests by generating electoral pressure on political parties to compete over young voters (vote16USA 2020). In this paper, we estimate the effects the largest recorded youth enfranchisement, the lowering of the voting age from 21 to 18 in India in the late 1980s, and find negligible electoral effects.

Compositional changes in the electorate, such as through women's suffrage (Miller 2008; Teele 2018; Morgan-Collins 2021), introduction of compulsory voting (Fowler 2013), or voting technology that serves as a de-facto enfranchisement of a subset of the electorate (Hidalgo 2012; Fujiwara 2015; Desai and Lee 2019), have been shown to have substantial political and policy consequences. However, these political consequences are contingent on the newly enfranchised group exercising their newly granted right to vote. Little evidence exists on whether newly-enfranchised youth exercise this right. Answering this question satisfactorily is challenging because these reforms are simultaneously implemented at the national level, therefore leaving us with no obvious comparison units, while across-country comparisons are rife with omitted-variables bias. The scant work that attempts to study the consequences of lower voting ages compares national level turnout before and after the reduction (McAllister 2014) and is likely confounded by aggregate trends in turnout, which were steadily decreasing in OECD countries in the 20th century. Yet, despite the scant evidence on electoral consequences of voting-age changes, there is policy-interest in voting-age changes, as exemplified by the failed amendment to lower the voting age to 16 in HR1 (vote16USA 2021) in the US, as well as similar movements across multiple OECD countries.

In this paper, we study the political consequences of a large franchise extension to the youth – the lowering of the voting age from 21 to 18 – implemented by the 61st amendment of India in December 1988 (Sharma 2021). In the late 1980s, then prime minister Rajiv Gandhi spearheaded reforms to lower the voting age from 21 to 18, and consequently added nearly 50 million new eligible voters (ANI 2019; Pachauri 1989), which was nearly 7% of the population or 13.5% of the voting age population[3], which constitutes the largest franchise extension since Indian independence and democratisation in 1950. We use variation in the age-composition of constituencies at the time of the reform to conduct difference-in-differences comparisons before and after the implementation of

[1] the global median age of legislators is 53 which is well over the median age of 29 (Union 2012; Nations 2015)

[2] We provide summary statistics on the age representation gap in India in appendix A.1

[3] This figure is based on the population in 1985, which was 784 million (Desa 2015), and the voting age population (≥ 21, which was ≈ 380 million (Pachauri 1989))

the amendment in 1989 to estimate the effects of the youth franchise extension on voter turnout, incumbent politicians' re-election rates, and electoral competition in state level politics. We find that the policy led to no statistically detectable changes in the size of the registered electorate, turnout, incumbency, and political competition, and if anything, marginally lowered turnout rates and competition.

These findings are in stark contrast to the expectations of the policy in the Indian news media at the time (Pachauri 1989), where there was widespread optimism about youth participation and the anticipated increase in electoral competition thanks to the sheer size of the youth vote. Our finding that a unprecedented injection of young voters into the electorate had negligible political consequences suggests that legal changes alone are unlikely to improve youth representation in politics. This finding also offers a potential explanation for the persistence of this stark gap: parties correctly anticipate low participation from the youth in electoral politics, and as such tailor their electoral strategies to older voters. This low-representation state is sustained by and feeds into low youth entry into politics.

Our findings contribute to the extensive literature in political economy on the causes and consequences of franchise extensions (Acemoglu and Robinson 2001; Aidt and Jensen 2013; Aidt and Franck 2015; Berlinski and Dewan 2011). Much of this literature focusses on reforms in the 1800s that enfranchised adult men, and as such the consequences of such franchise extensions are typically studied with an eye towards redistribution and finds mixed evidence for the theoretical prediction that redistribution should rise following franchise extensions (Meltzer and Richard 1981). Studies on recent de-jure and de-facto franchise extensions of the poor (Fujiwara 2015; Cassan, Iyer, and Mirza 2020), find substantial electoral and public policy effects. Studies focussing on womens' suffrage in the US (Morgan-Collins 2021) point the pivotal role of suffrage movement strength in converting de-jure changes into de-facto representation of political interests; our findings are consistent with this theory and represent a negative case where the absence of a social movement to coordinate newly enfranchised young voters may explain the negligible electoral consequences of the policy.

Our findings also contribute to the literature on causes and consequences of low youth participation and representation in politics. Scholars typically point to supply-side explanations for low participation such as low political ambition among young people (Lawless and Fox 2015) and restrictive minimum age requirements. Some recent work, such as McClean (2021) and Curry and Haydon (2018), also examines the consequences of youth under-representation and finds that electing older politicians affects welfare spending and redistributive policies more generally, which are marred by intergenerational conflict. Yet, much existing work is missing an explanation for why political parties choose not to tailor campaigns and nominate candidates to turn out a large and potentially pivotal portion of the electorate. By focussing on a particular episode of enormous youth enfranchisement, we study a 'best-case' setting for youth representation, and find that even here, the franchise extension had negligible electoral consequences.

The rest of the paper is organised as follows: 2 describes the data and research design, 3 reports results from the difference-in-differences and event study analyses and examines potential mechanisms, and 4 concludes.

2. Data and Design

2.1. Data.

2.1.1. **Treatment.** Like most franchise extensions, the franchise extension applied to all elections held after 1988. In order to evaluate the effects of the policy, we use the age composition of different constituencies to generate variation in the 'intensity of treatment' of the policy. Constituencies with relatively younger populations were naturally affected more by the franchise extension.

To construct this treatment intensity measure, we need granular data on age composition of sub-national administrative units. To our knowledge, age composition is only available at the state level until the 1981 census. However, since we're interested in effects on state-legislature elections, this is insufficiently granular. We therefore use the 1991 census, which was first Indian census that reports age composition and cohort-level education at the district level. We define 'treatment' as having youth share above the state-median. While these are nominally measured post-treatment (1991, a year after the first elections with lower voting age) voter registration is sufficiently protracted in India to rule out any large short run changes in political competition because of migration. This measure is valid under the assumption that there was not much migration from 'treated' (younger) to 'control' (older) constituencies in response to the policy change, which we believe is likely. We report the spatial distribution of age in 1991, as well as our coding of binary treatment status using the median as the cutoff, in figure 1. We find a fair amount of variation in the age composition of constituencies within and across states; while some of the densest states in the gangetic plains (Uttar Pradesh, Bihar, Jharkhand) lean older, while Deccan and Southern states have a mix young and old constituencies. In our preferred difference-in-differences specification, we use *within*-state variation in age-composition relative to state medians.

2.1.2. **Electoral outcomes.** We restrict our analysis to state-assembly (*Vidhan-Sabha*) instead of also analysing national-parliamentary (*Lok-Sabha*) elections three main reasons. First, since Lok Sabha constituencies much larger and aggregate multiple districts, the across-constituency variation in 'treatment intensity' quite small; Pachauri (1989) estimates that the amendment enfranchised nearly 100,000 new voters in each of the 545 Lok-Sabha constituencies. Secondly, the number of Lok-Sabha constituencies within each state is much smaller than the number of Vidhan-Sabha constituencies, thereby making within-state difference-in-differences necessitated by our preferred specification intractably noisy. Third, because the two parliamentary elections immediately following the amendment, the parliamentary elections of 1989 and 1991, were marred by controversy and the latter was called off-cycle following the dissolution of parliament, which

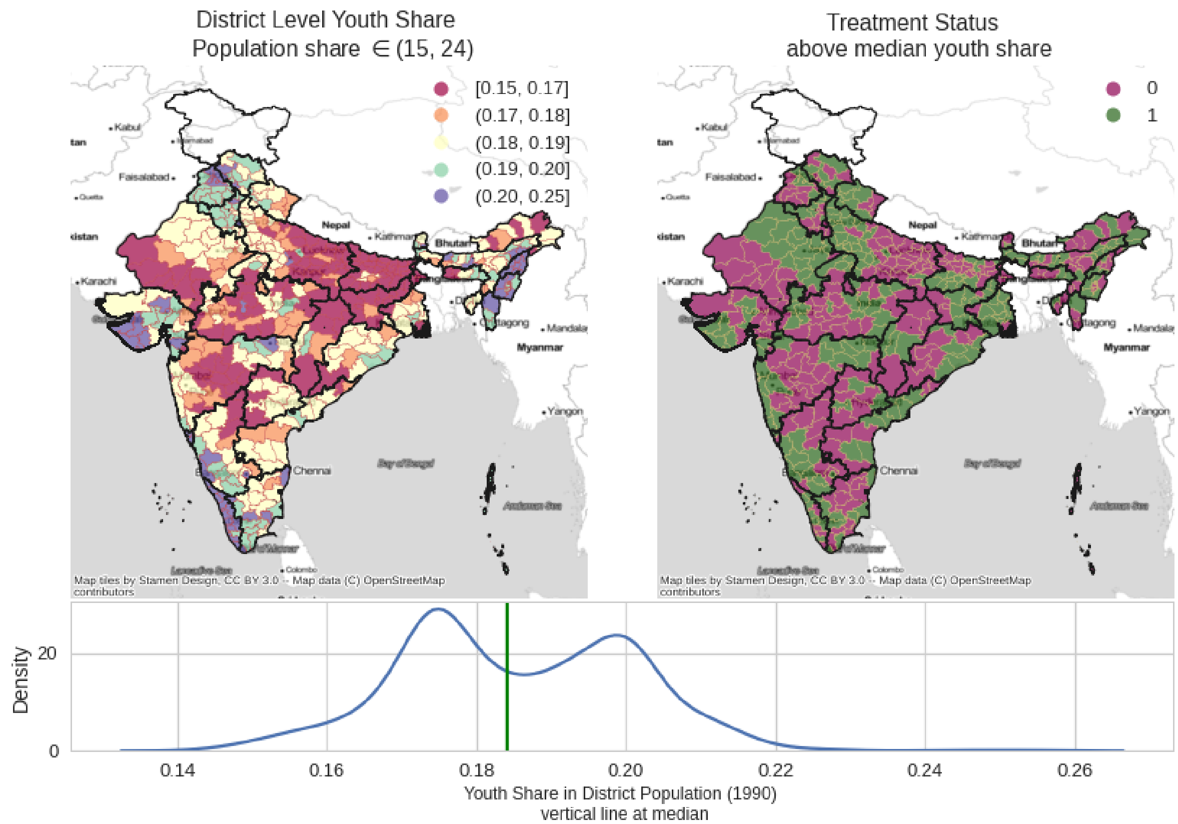

FIGURE 1. Spatial distribution of youth share quantiles (left) and treatment discretised at state-median (right). District level data from 1991 census. Bottom panel plots density of the youth share median

makes it unsuitable to evaluate the dynamic effects of the policy. Furthermore, the 1991 election was marred by low-turnout in the initial phase followed by a surge in turnout prompted by the assassination of Rajiv Gandhi between the two rounds of the election (Blakeslee 2018). Since these shocks pull constituencies in opposite directions, and since the preceding events altered the stakes of the election substantially beyond the nominal increase in the electorate from the amendment, we deem the first two post-period hopelessly contaminated for a reasonable difference-in-differences comparison for Lok-Sabha elections.

To prepare the analysis sample, we spatially merge districts level age composition summaries to assembly-constituency shape-files for the 3rd delimitation (Infomap 1980). Assembly constituencies are typically but not always wholly contained inside districts [4]. We then merge the assembly constituency level treatment measure to assembly-constituency level electoral data from Jensenius and Verniers (2017), who collate all elections at the parliamentary (Lok-Sabha) and state parliamentary (Vidhan-Sabha) level since 1960. This

[4]The median district contains 7 assembly constituencies. When assembly constituencies overlap more than 1 district (beyond merge error, which we set to be 1% area), we use areal weights to aggregate the two districts' age composition to an AC level one.

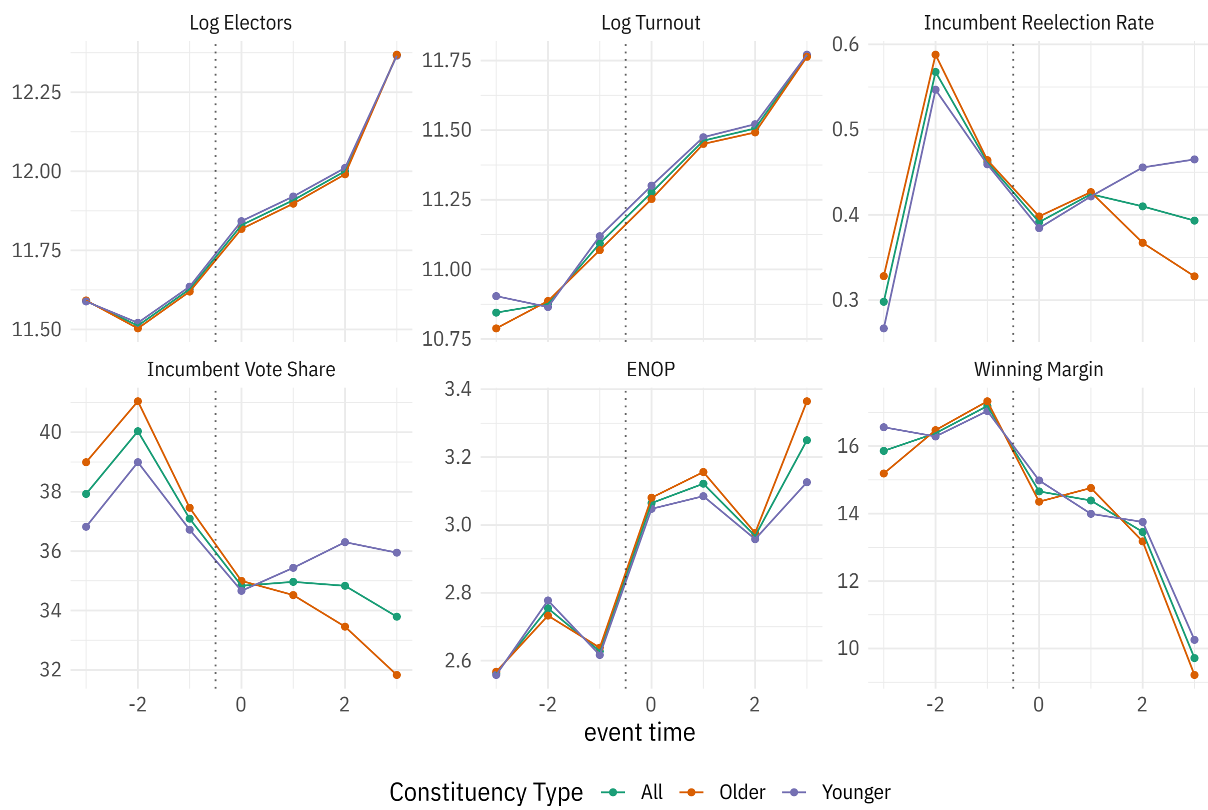

FIGURE 2. Aggregate trends in political outcomes by constituency type. Dotted line delineates the pre-amendment and post-amendment periods

allows us to construct a panel of assembly constituencies from 1975 onwards (since electoral boundaries were redrawn then) with registered voter numbers, turnout, incumbent re-election and vote share, effective number of parties, and winning margin. We report the aggregate time series (in red) and separate trends for treatment and control areas for the six political outcomes of interest in fig 2. The two groups appear to be trending in tandem for most variables before and after the implementation of the 61st amendment.

2.2. **Research Design.**

Since the treatment applies everywhere after 1988, we propose using a difference-in-differences style comparison between places 'more' and 'less' affected by the franchise extension to estimate the effects of the policy. The estimand of interest, unlike in conventional DiD settings, is the difference in treatment effects (individually identified using a pre-post comparison conditional on fixed-effects and time trends) rather than the ATT.

We begin with a nonparametric first-differences regression of the form

$$\Delta y_i = f(\text{youth share}_i) + \varepsilon_i$$

where we regress changes in political outcomes Δy_i between the last pre-amendment and first post-amendment election on the share of young voters in constituency i. We estimate this function nonparametrically using local linear regression (and overlay a linear fit) in order to avoid functional form assumptions on the effect of youth share on political outcomes. This specification also helps evaluate potential non-linearities in treatment effect as a function of youth share. The consistency of this estimation strategy relies on the assumption of uniform-moderation which stipulates that treatment effects are monotonic in the moderator (age-composition of constituencies).

We then use a difference-in-differences design to compare turnout and political competition in constituencies before and after the implementation of the 61st amendment. States hold elections every five years, but operate on different cycles[5]. We standardise these elections into event-time relative to the first post-amendment election (0), and estimate fixed-effects regressions of the form

$$Y_{ijt} = \alpha_i + \gamma_t + \tau D_{ijt} + \varepsilon_{ijt} \tag{2.1}$$
$$Y_{ijt} = \alpha_i + \psi_{jt} + \tau D_{ijt} + \varepsilon_{ijt} \tag{2.2}$$

where i indexes constituencies, j indexes states, and t indexes time, with $\alpha_i, \gamma_t, \psi_{jt}$, and denoting constituency , election, and state $\times$ election fixed effects. D_{ijt} is the 'treatment' dummy, which takes on a value of 1 for youth constituencies (i.e. constituencies with above-median youth share in the state, which is time invariant because we only observe it for one cross-section) after the amendment passed. This is akin to a standard difference-in-differences regression, where the 'treated' and 'post' dummies are included in the constituency- and time FEs. Since the 'treatment' only varies at the district level, we deem this the level of treatment assignment and cluster standard errors at the district level throughout, which is more conservative than clustering by the panel unit (constituency level).

For 2.1 to yield consistent estimates, we need parallel trends across states for political outcomes. The particular parallel trends is somewhat non-standard, since in this case it demands parallel trends between two treated groups with differential intensity. Since political outcomes are typically a function of state-level party politics and policy changes, we believe that the general parallel trends assumption is likely implausible. To address this concern, we use specification 2.2, which adds state $\times$ election fixed-effects. This restricts comparisons the *within-state* variation in political outcomes, and so accounts for many potential time-varying state level confounders. This means that we estimate difference-in-differences between younger and older constituencies *within each state*, which connects cleanly with our 'treatment' definition. The distinction between 2.1 and 2.2 amounts to whether we are comparing outcomes in the green and magenta regions in fig 1 throughout the whole country (as in 2.1) or within each state boundaries (as in 2.2). We consider parallel trends to be more likely to hold in 2.2, and as such consider this our preferred

[5]We report the last pre-period and first post-period election in table A1. However, since we standardise this into event time, this is not a *staggered* difference in differences, which is a design rife with problems (Goodman-Bacon 2018)

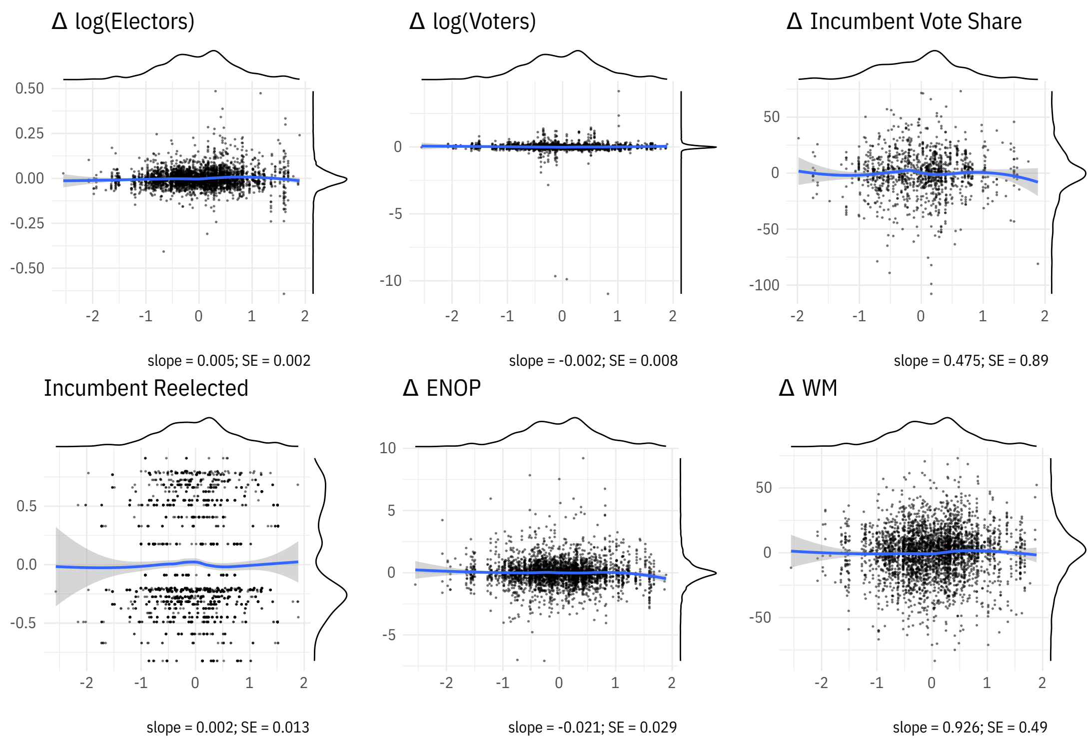

FIGURE 3. First differences in electoral outcomes plotted against standardised youth share. Differences are computed between levels in the first post-amendment election minus the last pre-amendment election (i.e. $t \in \{-1, 0\}$, and residualised on state fixed-effects. We report a linear LOESS smoother and the linear regression coefficient from the continuous treatment below each panel.

specification. We also estimate event-study regressions that decompose the treatment effect over time (relative to the period immediately preceding the amendment, $t = -1$).

3. Results

3.1. First-differences with continuous treatment. We begin by using a simple first-differences regression where we regress changes in political outcomes on constituency youth-share in 1990. In figure 3, we plot the distribution of first-differences as a function of continuous variation in the 'treatment' - the share of the population in the franchise-extension age group, both residualised on state-fixed effects (which is equivalent to our preferred State $\times$ time fixed effects specification in the two-period setting).We find very little non-linearity across all outcomes, and consistently find precisely estimated null effects (reported as the slope from the linear regression in each panel)[6].

[6]To evaluate the robustness of this approach, we replicate the same figure but with first-differences between last two pre-amendment election outcomes and report it in A7. We find negligible effects in the placebo.

3.2. **Fixed-effects regression estimates.** Next, we report results from estimating the different regressions specifications for log number of registered voters and log turnout count (voters who turned out) [7] in table 1. Both these coefficients are to be interpreted as a percent-change. We find that the effect of youth-franchise on log-number of voters is effectively zero to the third decimal point in our preferred specification (column 2). Similarly, we estimate a small effect of youth franchise on turnout on the order of 1 percentage point, although this is also statistically indistinguishable from 0.

These results can be thought of as an ecological 'first stage' effect of the youth franchise, which is necessary for any potential effects on political outcomes. Since we fail to find that the size of the electorate and turnout rates meaningfully changed in response to the enfranchisement, we may anticipate that there were few, if any, downstream effects. Since these outcomes are aggregated for the entire electorate, the standard ecological inference problem applies : we don't strictly observe youth registration and turnout and therefore only indirectly test for their magnitudes holding registration and turnout rates in the rest of the electorate constant. While a large displacement of other segments of the electorate by newly enfranchised youth voters is also consistent with our findings, we find such an magnitude implausible in the setting under consideration.

Table 1. Turnout Regression results

	log(# electors)		log(# voters)	
	(1)	(2)	(3)	(4)
Youth X Post 1989	0.0074	0.0082	0.0068	0.0128
	(0.0073)	(0.0053)	(0.0274)	(0.0165)
Observations	16,676	16,676	16,677	16,677
Cons fixed effects (3,256)	✓	✓	✓	✓
Election fixed effects (7)	✓		✓	
State $\times$ Election fixed effects (80)		✓		✓

Notes: robust SEs clustered by district in parentheses

We then turn to studying whether incumbents were voted-for and re-elected at higher rates (conditional on running, hence fewer observations). We find that in our preferred specifications, the confidence interval for the effect covers zero and is mildly positive, suggesting that the effect was likely zero.

Finally, we examine whether the introduction of young voters altered political competition by studying the effects on the Effective Number of Parties (the inverse Hirschman-Herfindahl index of vote shares), and the winning margin of the winning candidate. In both cases, we find that the effects were very small or zero. The ENOP results are notable in

[7] We choose to work with the number of voters turning out (the numerator in turnout rates) as opposed to computed turnout rates because the latter is a ratio with #electors as the denominator, and interpreting it requires assumptions on how much the denominator grows relative to the numerator. Working with the numerator alone simplifies the interpretation.

TABLE 2. Incumbency Regression results

	Incumbent Reelected		Incumbent Vote Share	
	(1)	(2)	(3)	(4)
Youth X Post 1989	0.0351	0.0239	2.297	1.723
	(0.0411)	(0.0232)	(2.006)	(1.013)
Observations	11,215	11,215	11,214	11,214
Cons fixed effects (3,249)	✓	✓	✓	✓
Election fixed effects (7)	✓		✓	
State × Election fixed effects (80)		✓		✓

Notes: robust SEs clustered by district in parentheses

how much they differ between the basic two-way specification and the within-state specification, suggesting that changes in political competition are largely based on uniform swings by state.

TABLE 3. Political Competition Regression results

	ENOP		Winning Margin	
	(1)	(2)	(3)	(4)
Youth X Post 1989	-0.0724	-0.0689	0.3035	0.2097
	(0.0476)	(0.0377)	(0.8421)	(0.6293)
Observations	16,668	16,668	16,668	16,668
Cons fixed effects (3,256)	✓	✓	✓	✓
Election fixed effects (7)	✓		✓	
State × Election fixed effects (80)		✓		✓

Notes: robust SEs clustered by district in parentheses

3.3. **Event Study estimates.**

We now proceed to decompose treatment effect dynamics using an event study regression, thereby both verifying the plausibility of parallel trends, as well as examining treatment effect dynamics using the basic specification, one with state-level time trends, and state×year fixed effects (our preferred specification) in fig 4. For the most part, we find parallel trends is plausible most specifications, since the lead-estimates ($t = -2$) are generally zero. Estimates are also remarkably stable across specifications, with the state × year FE specification yielding the most precise estimates. Decomposing the effects over time also allows us to address the potential concern that negligible effects in the first election immediately following the amendment (denoted by 0 in our event study figures) may have been driven by lack of information among the newly enfranchised, and that effects would appear in subsequent elections.

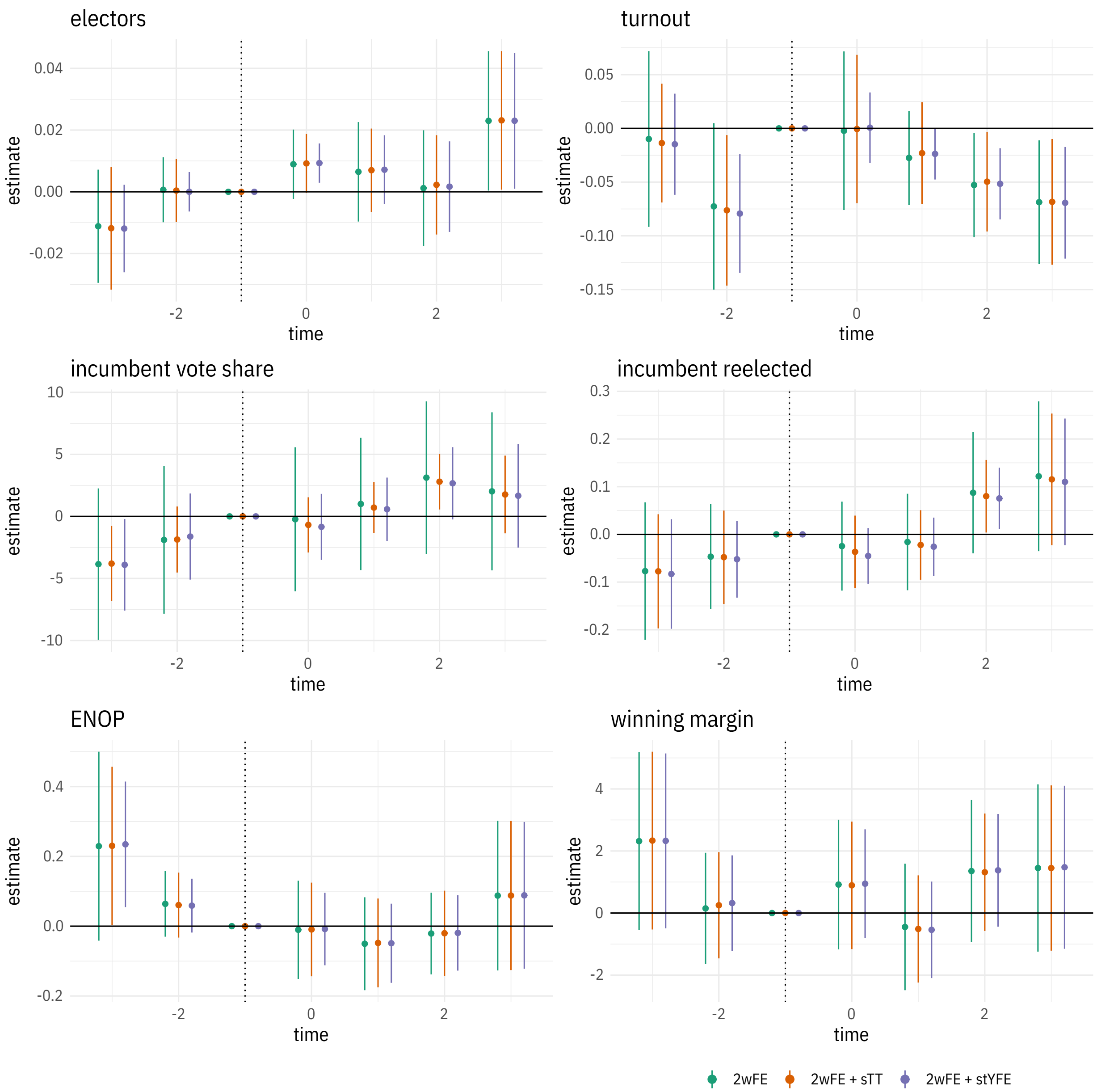

FIGURE 4. Event Study: Log Registered Voters, Log Turnout, Incumbent Re-elected (binary), Incumbent vote share, ENOP, and winning margin

We begin with the event study results for number of registered voters and voter turnout counts in row 1. For the log-voters outcome we find that confidence intervals for all specifications cover zero and rule out large growth in the electorate in response to the franchise extension. In the latter, we find that if anything, turnout fell, although these effects are small and appear with a lag. Next, we examine incumbent re-election and vote share in row 2, and again find that the estimates cover zero and rule out meaningful effects. Finally, we study the effects on ENOP and winning margin in row 3. We find that estimates are generally small and statistically indistinguishable from zero. Again, we rule out meaningful effects for both outcomes.

3.4. **Heterogeneity.** We now examine whether the null effects of franchise extension on political outcomes are different for constituencies with high and low levels of youth education, since one of the motivations for the policy was to specifically empower the educated youth of India. To evaluate this, we use fully moderated interactions with bins of constituency illiteracy rate and education rates (defined as the share of the youth population with no literacy and secondary-school education respectively) as suggested by Hainmueller, Mummolo, and Xu (2019).

We report these results graphically in figures A2. We find that the treatment effect appears to not vary significantly with either youth illiteracy of youth higher-education rates (binned into high / medium / low). The binned estimates are largely consistent with a simple linear interaction (indicated by the black line in the figures), and as such we fail to detect substantial heterogeneity in these effects.

Next, we examine treatment effect heterogeneity by constituency 'type' in fig A3 by estimating the event-study separately for the three types of constituencies. India's system of electoral reservations ensures that certain constituencies in both the national parliament (Lok Sabha) and state parliaments (Vidhan Sabha) are reserved for candidates from ST and SC groups on the basis of population shares. SC and ST were experiencing relatively high population growth at the time (Kulkarni and Alagarajan 2005), so one has reason to believe that electoral effects would be largest in these constituencies. However, we detect very little heterogeneity in most outcomes with the exception of winning margin, where we find that SC constituencies became less competitive over time, possibly due to incumbent entrenchment.

3.5. Robustness Checks.

3.5.1. **Panel Estimators.** Conventional two-way fixed effects regressions estimates are typically inconsistent for the Average Treatment effect on the Treated (ATT) estimand under treatment effect heterogeneity (Sant'Anna and Zhao 2020; Imai and Kim 2020). While the problem is most severe in designs that use staggered treatment adoption (which is not the case for our setting), some estimators proposed to address the treatment heterogeneity problem in panel data settings have the added benefit of typically performing pre-treatment matching, which alleviates concerns regarding the parallel trends assumption by matching treated units with control units with similar outcome trajectories. This is in the spirit of the synthetic control method (Abadie, Diamond, and Hainmueller 2010), although our setting is unsuitable for use of synthetic control methods since we have only two pre-treatment time periods.

We therefore use the panel-matching estimator (Imai and Kim 2019) that explicitly matches on trajectories even for short panels and also allows us to exact-match on covariates. This effectively estimates the treatment effects using the subset of units for which the parallel trends assumption holds. To mimic our preferred specification, we exact-match on state, which effectively restricts the pool of matches for any treated constituency to other constituencies within that state, thereby holding many potential time-varying confounders constant. We report the panel-matching estimates for our six outcomes in fig A4, and find similar results to the regression specification and event study. One notable difference is that we the negative turnout effects are now statistically significant.

3.5.2. **Recoding the treatment.** In the main analysis, we define the treatment threshold at the median, which mechanically means that we assign similar units to treatment and control depending on whether they cross this arbitrary threshold. As an alternative, to maximise the contrast between the two groups, we restrict our analysis to constituencies that are below the 25th percentile in youth share (which we call control) or exceed the 75th percentile in youth share (which we call treatment). This yields a 'maximum-contrast' version of the analysis sample where treatment and control groups are more meaningfully different in ex-ante youth share. We then re-estimate the treatment effect using our preferred specification (eqn 2.2) and report the estimates alongside the primary estimates from the corresponding specifications (from tables 1-3) in A5. Similarly, we re-estimate the event-study specifications and report the two sets of coefficients in A6. The point estimates for turnout are somewhat larger and more negative, but overall, the estimates are effectively identical and (mechanically) more noisily estimated.

4. Conclusion

We estimate the electoral effects of the extension of the franchise to 18-21 year olds in India in the late 1980s and find that the extension had negligible electoral consequences. This precise null electoral effect of an unprecedented number of young voters offers pessimistic predictions regarding the effectiveness of lower voting ages as a means of improving youth representation. These findings also provide a potential explanation for why political parties continue to cater campaigns and electoral messaging to older voters - they correctly anticipate that there are limited electoral penalties for doing so. This result also suggests what likely *won't* work to increase the substantive representation of the youth - simply giving them the vote is insufficient, since they seemingly don't use it. Increasing youth representation and engagement in politics likely demands a different, more creative, set of policies.

We suggest that the lack of electoral effects may be driven by the absence of organisations that coordinate newly enfranchised voters, which was a key source of the effects of women's suffrage in the US (Morgan-Collins 2021). Future work in contexts with age-disaggregated turnout and preference data may be able to uncover these mechanisms directly by estimating cohort-specific turnout effects, as well as differences in political preferences across age cohorts.

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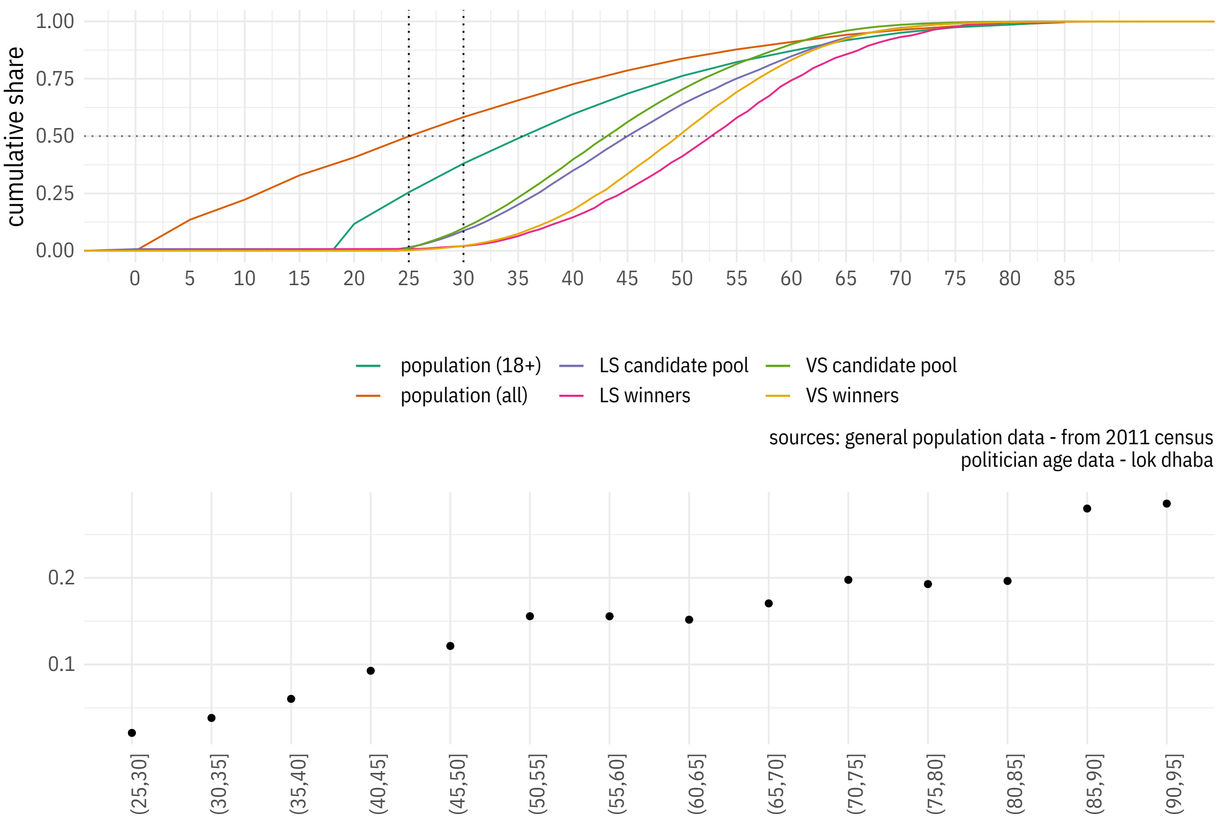

Figure A1. Cumulative distribution functions of age for the overall population, voting age population, candidates, and elected legislators at both state and national levels (top) and win-rates by age ventile (bottom). The vertical lines in the top panel denote the minimum age requirement to run – 25 for LS and 30 for VS.

Appendix A. Additional Tables and Figures

A.1. **Age representation in India.** We begin with a simple illustration of the representation gap in Indian politics in fig A1. The distribution of age in the politician candidate and legislator pools at all levels of government are well to the right of general age distribution (in magenta) and voting age population (in blue). Rampal (2019) documents that while more than 50% of the Indian population is under the age of 30, less than 2% elected representatives are [8], and the average age in parliament has consistently been over 50 for the last thirty years. Indian youth register for elections and turn out at extremely low rates: their turnout rates have lagged behind overall turnout in all elections for the last 30 years (Kumar 2009) and the registration rates for voters under the age of 20 was below 30% in the last election (Yadav 2018).

[8]largely because of minimum age requirements; the state-legislature minimum age only slightly lower than the median voter's age, which is 35 in the 2011 census, and was lower in 1989

	State_Name	last election pre-amendment	first election post-amendment
1	Andhra_Pradesh	1985	1989
2	Assam	1985	1991
3	Bihar	1985	1990
4	Gujarat	1985	1990
5	Haryana	1987	1991
6	Himachal_Pradesh	1985	1990
7	Karnataka	1985	1989
8	Kerala	1987	1991
9	Madhya_Pradesh	1985	1990
10	Maharashtra	1985	1990
11	Odisha	1985	1990
12	Punjab	1985	1992
13	Rajasthan	1985	1990
14	Tamil_Nadu	1984	1989
15	Uttar_Pradesh	1985	1989
16	West_Bengal	1987	1991

TABLE A1. Last election pre-franchise expansion for each State

Software used: JORDAHL (2014), WICKHAM (2010), BERGÉ (2018).

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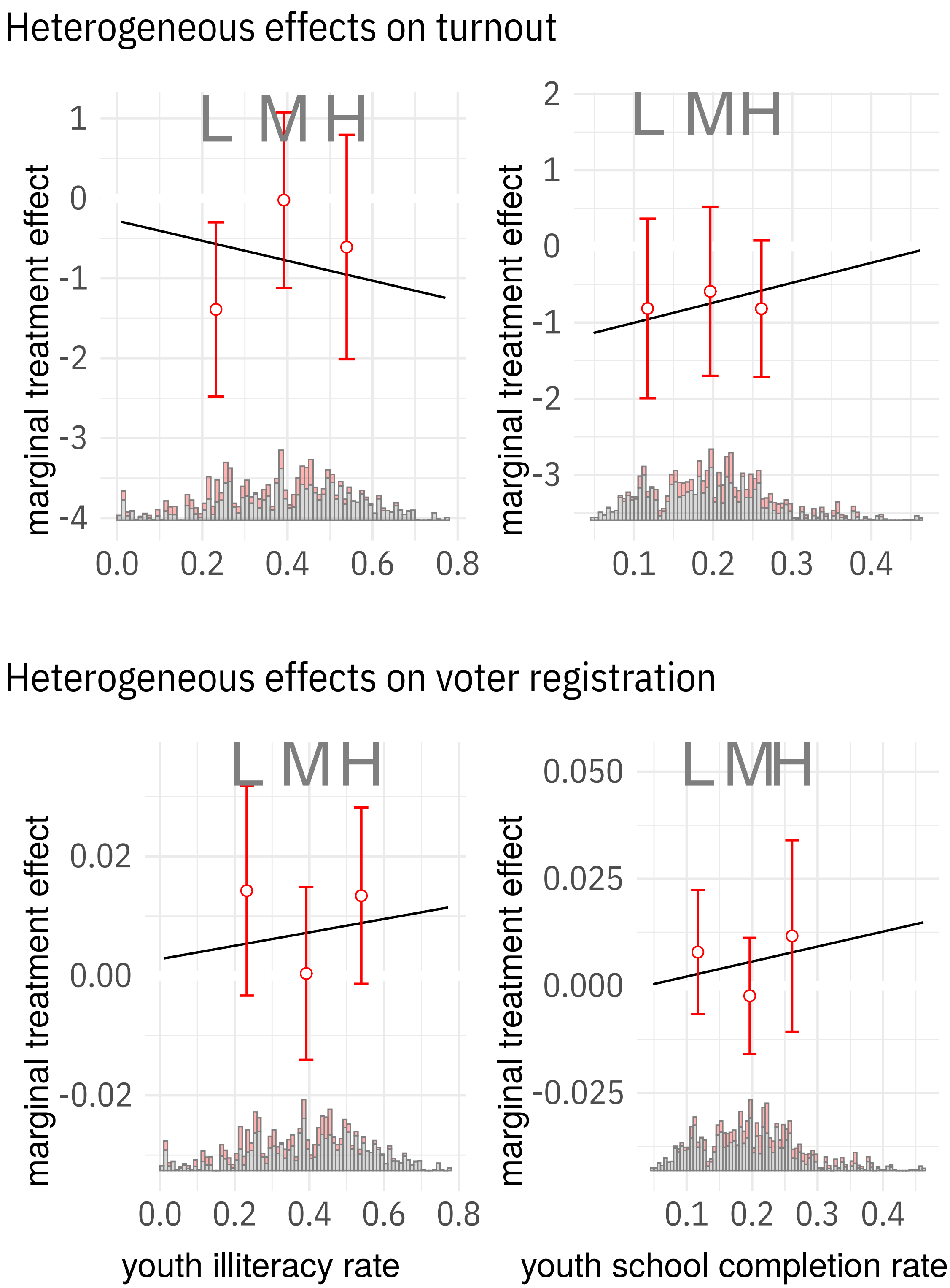

FIGURE A2. Heterogeneous Treatment Effects of Youth Franchise on Log Number of voters and turnout rate by levels of illiteracy and higher-education rates in the youth population. In both cases, we fail to find substantial variation in treatment effects by levels of the moderator.

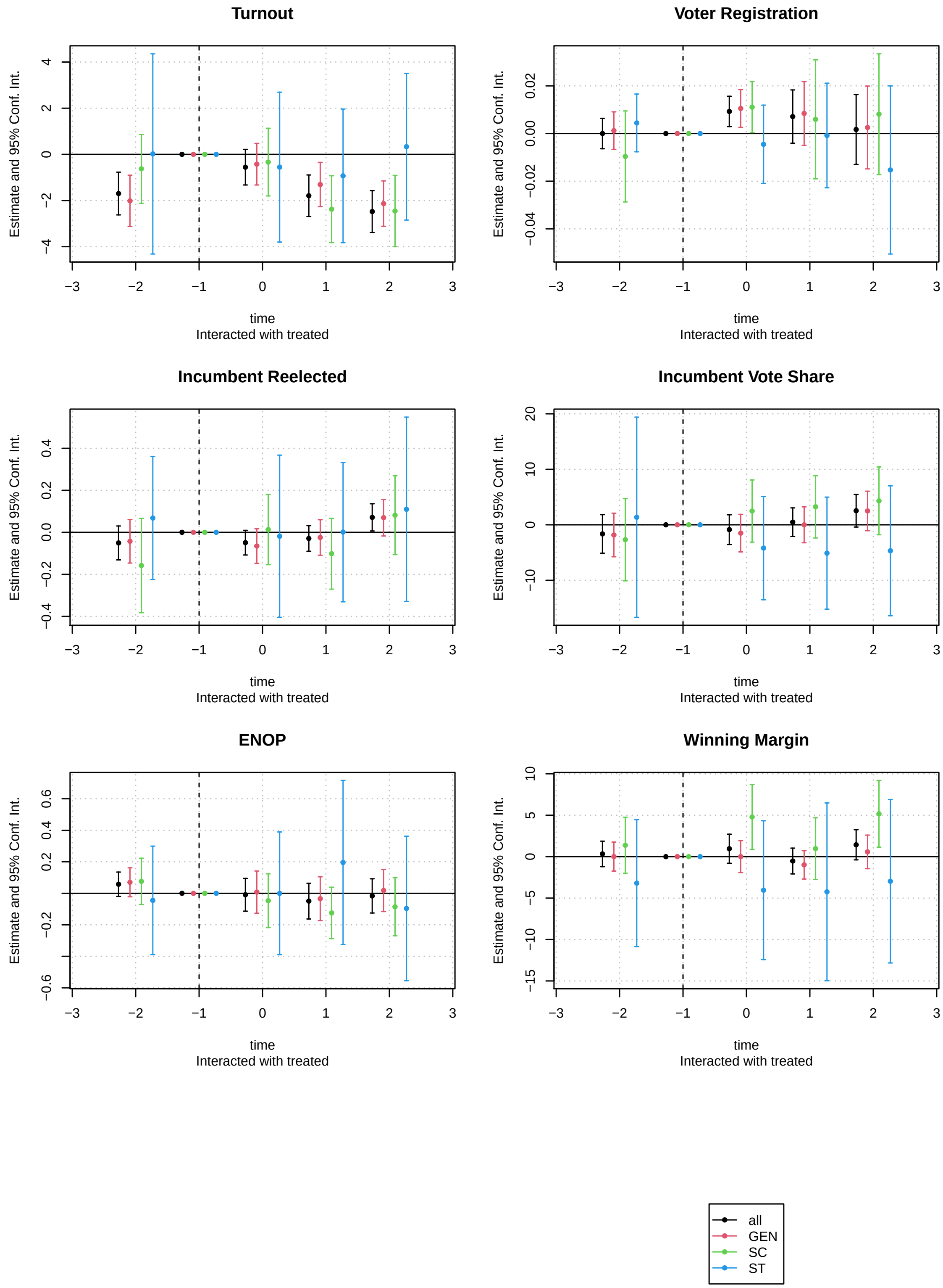

Figure A3. Event study estimates by constituency type

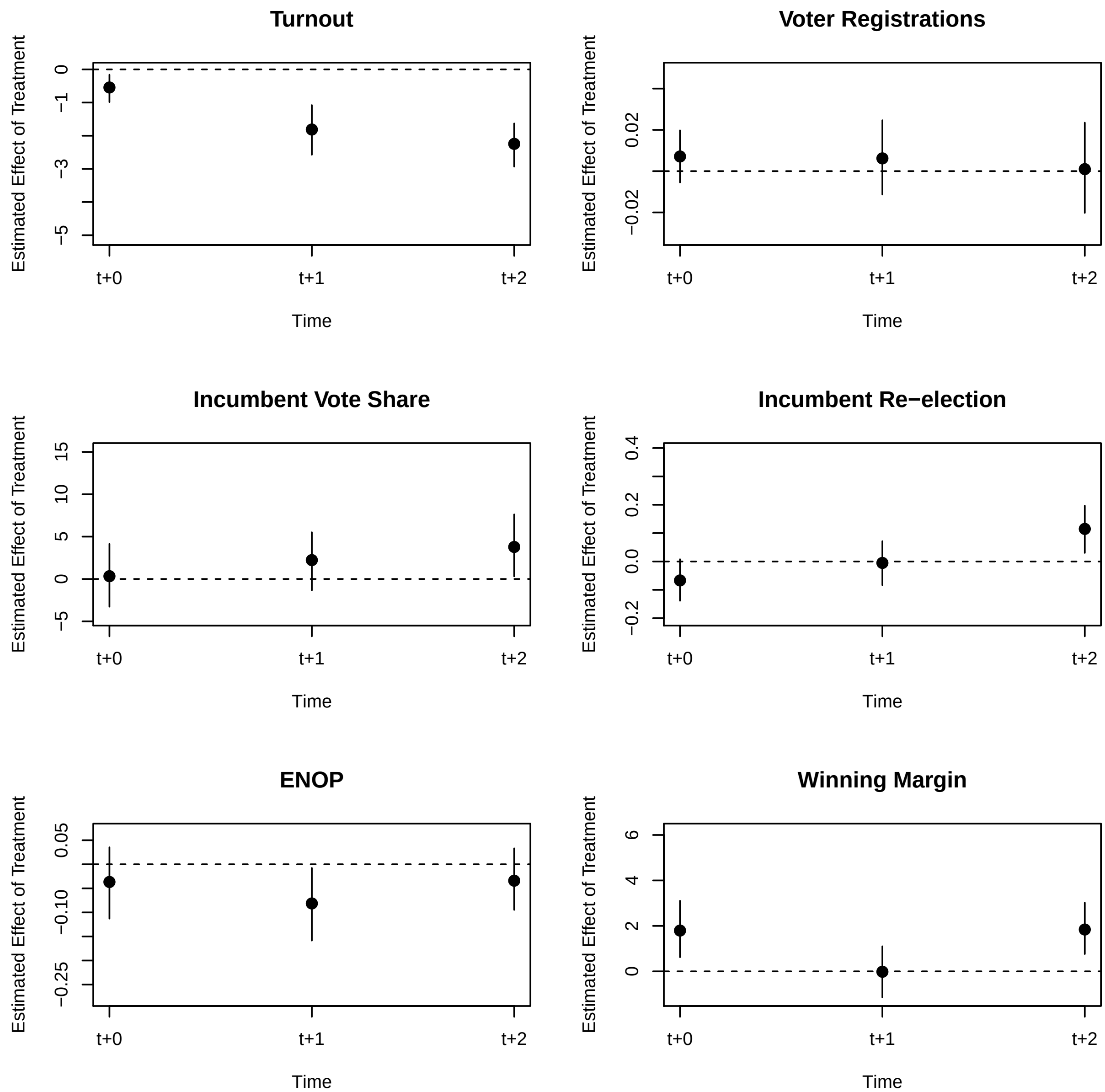

Figure A4. Panel-matching estimates for political outcomes. We match on pre-treatment trajectory for each outcome and exact match on state. Standard errors are bootstrapped.

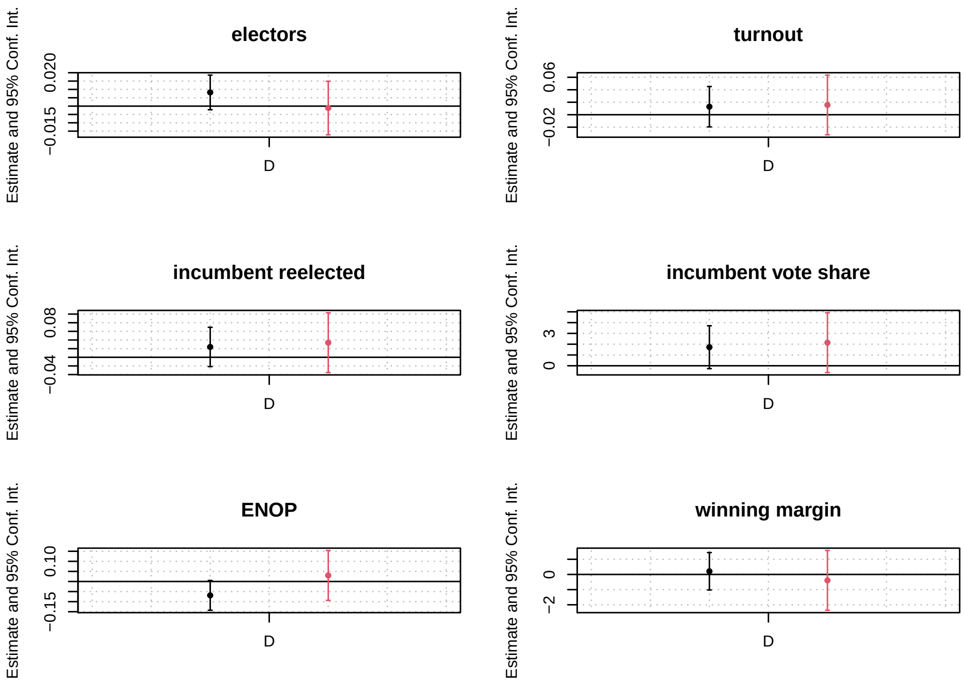

FIGURE A5. Comparing estimates from treatment thresholded at the medium versus contrasting between ≤p25 and ≥p75 constituencies

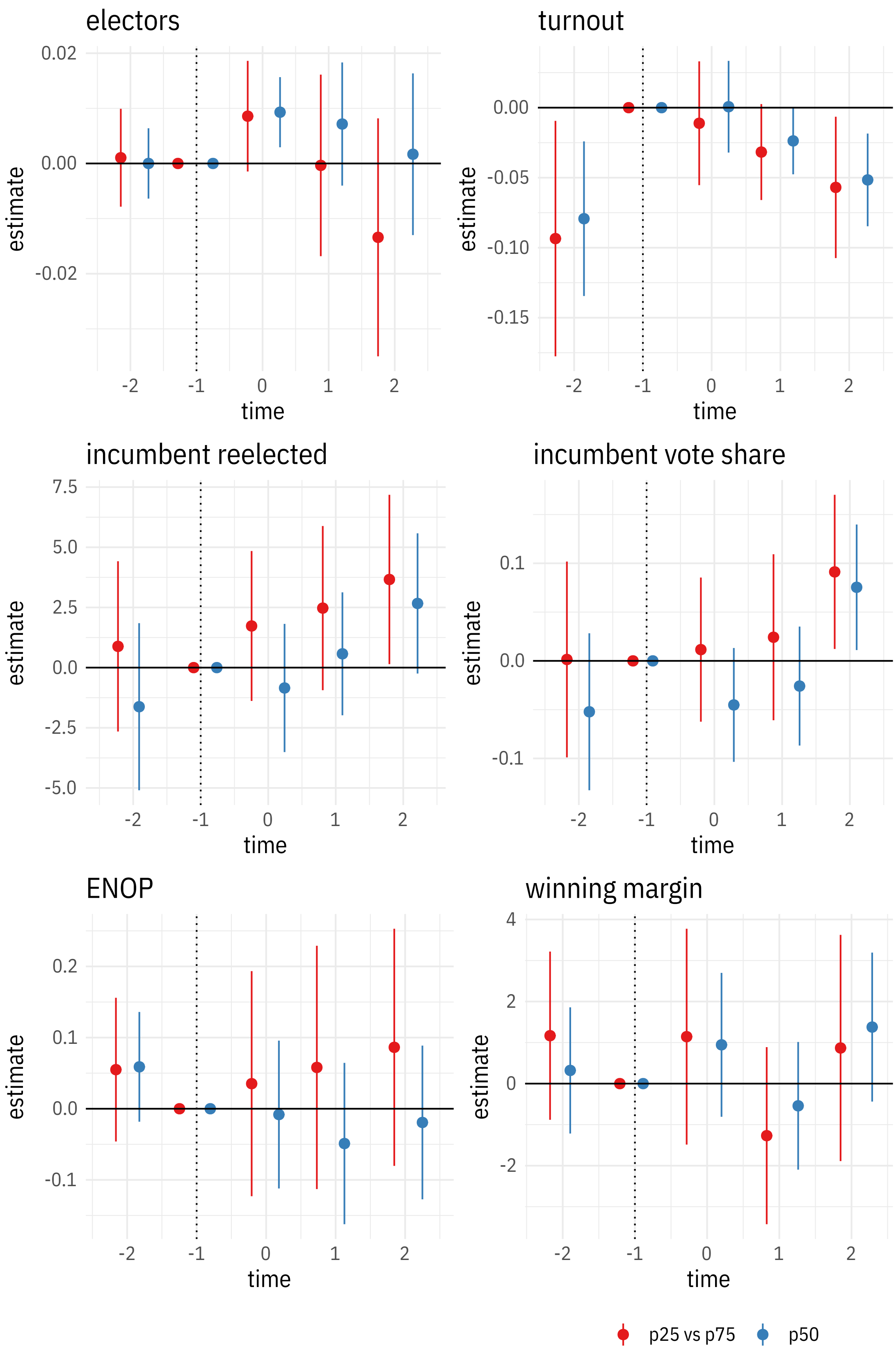

FIGURE A6. Comparing event study estimates from treatment thresholded at the medium versus contrasting between ≤p25 and ≥p75 constituencies

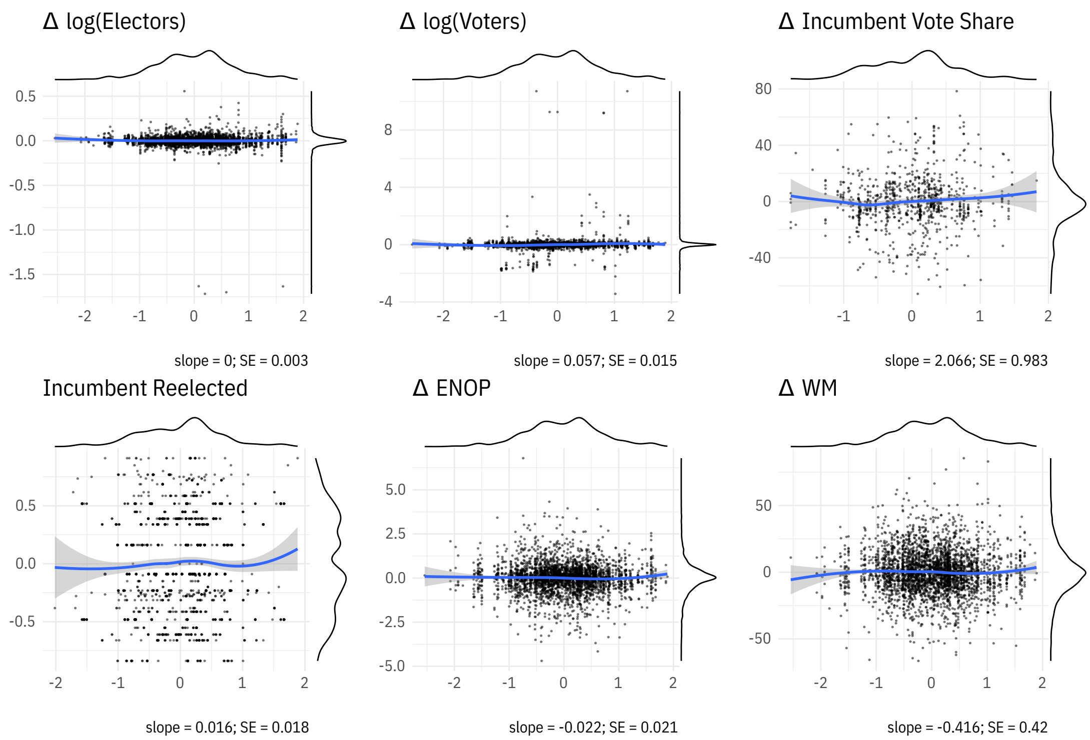

FIGURE A7. Placebo first differences in electoral outcomes plotted against standardised youth share. Differences are computed between levels in $t \in \{-2, -1\}$, and residualised on state fixed-effects. We report a linear LOESS smoother and the linear regression coefficient from the continuous treatment below each panel.