Election Timing Revisited: Evidence from California's Voter Participation Rights Act

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Abstract

This paper studies the effects of increased voter participation in municipal and school district elections, taking advantage of the 2018 implementation of the California Voter Participation Rights Act, which required 236 California local governments to move their election timing on-cycle. I find that the switch to on-cycle elections had its intended effect on voter turnout: a substantially larger, more diverse set of voters participate in local elections following the switch. However, contrary to expectations from the literature, I find no evidence that this increase in voter turnout yields downstream effects on descriptive representation, the composition of the candidate pool, the incumbency advantage, housing policy, or public employee salaries. These results suggest a weaker link between election timing and policy than previous studies have estimated.

Keywords: Election Timing, California Voter Participation Rights Act

1 Introduction

How does mass participation in elections influence politics? This question is central to democratic theory, but is notoriously difficult to study empirically. One way that American politics scholars have approached the question is by studying the role of municipal election timing, comparing local governments where elections are held concurrently with state and federal elections (on-cycle) with those held on other dates (off-cycle). It is well-established that on-cycle election timing yields a substantial increase in voter turnout (Berry and Gersen, 2010), and, in turn, political scientists have argued that this increase in voter participation produces several downstream effects on politics: reducing the power of special interest groups (Anzia, 2011, 2013), increasing the incumbency advantage (de Benedictis-Kessner, 2018), and improving the government's responsiveness to public opinion (Dynes, Hartney and Hayes, 2021).

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Most of these studies of election timing are cross-sectional, which makes causal identification challenging.[1] Local governments in the United States are typically free to set their election calendar as they please, introducing problems of selection bias and unobserved confounders. In this study, I address this problem by focusing on the implementation of the California Voter Participation Rights Act (SB 415), which compelled 100 municipal governments and 136 school districts to switch their election timing from off-cycle to on-cycle in January 2018. By comparing outcomes in these local governments before and after the switch with outcomes from other California local governments where elections were already held on-cycle, I am able to more credibly estimate the effects of election timing.

The results of this analysis confirm some previous findings on election timing and voter turnout. The implementation of SB 415 yielded, on average, a 21 percentage point increase in voter turnout, and significantly increased the diversity of voters casting ballots at the local level. Prior to SB 415, off-cycle elections in California were dominated by senior citizens and homeowners, but following the shift to on-cycle elections, younger voters, non-white voters, and renters were significantly more likely to vote in local elections. Despite these large changes in voter participation and demographics, I find little evidence of downstream effects on elected officials and policy outcomes. On-cycle elections do not appear to increase the share of underrepresented groups elected to local public office, nor do they affect the rate at which incumbents are re-elected. I also find no evidence that the election timing switch affected policy outcomes like public employee salaries or new housing permits.

In the next section, I review the literature on election timing and outline seven hypotheses about the effects of SB 415. Section three describes the data and research design, and section four presents the results. I conclude with a discussion of why my findings are so contrary to expectations—and their implications how one should think about power and policymaking in US local government.

2 Background and Hypotheses

Although federal Election Day in the United States is officially the Tuesday following the first Monday in November, most US elections are not held on that day (Berry and Gersen, 2010). Across

[1] Anzia (2012*a*) is a notable exception, leveraging a state-level policy change in Texas that mandated a particular set of school districts move their elections on-cycle. This study has a similar design, but examines a state law that applied to *all* local governments—including municipalities and school districts—reducing the potential for selection bias.

the United States there are tens of thousands of local governments, including roughly 3,000 counties, 19,000 municipalities, 14,000 school districts, and 35,000 special districts (Berry, 2009). At this lower level, elections are commonly held off-cycle—on a date separate from federal or state elections.

The historical roots of this practice are deep. As Anzia (2012*b*) documents, several municipal governments experimented with off-cycle election timing in the late 19th century as a play for partisan political advantage. In the decades that followed, the Progressive movement advocated off-cycle elections as part of a package of reforms designed to weaken urban political machines. The institution has proven remarkably persistent. Today, roughly 80% of US municipalities continue to hold their elections off-cycle (Anzia, 2013).

The most prominent consequence of holding elections off-cycle is lower voter turnout. Because voting entails a non-negligible cost in time and resources, citizens are more likely to vote when there are multiple concurrent elections on the ballot, particularly high-profile national elections like the presidency. Berry and Gersen (2010) estimate a 20-30 percentage point decrease in turnout when California municipal elections are held off-cycle. This finding is replicated in quasi-experimental studies as well; local governments that were compelled to shift the timing of their elections saw large subsequent changes in voter turnout (Anzia, 2012*a*; Garmann, 2016).

A decade ago, it was generally believed that low-turnout off-cycle elections in the United States favored the Republican Party, whose voters are older and more affluent, and therefore more likely to turn out for low-salience contests (Kogan, Lavertu and Peskowitz, 2018). This belief is likely what prompted the Democratic-controlled California State Legislature to pass SB 415—the California Voter Participation Rights Act—in 2015. This bill required any political subdivision in California to hold their elections on-cycle if it failed to meet certain voter turnout requirements. The relevant passage reads as follows:

> *This bill, commencing January 1, 2018, would prohibit a political subdivision, as defined, from holding an election other than on a statewide election date if holding an election on a nonconcurrent date has previously resulted in voter turnout for a regularly scheduled election in that political subdivision being at least 25% less than the average voter turnout within the political subdivision for the previous 4 statewide general elections, except as specified.*

Attentive readers who are at this point anticipating a regression discontinuity design may be disappointed to learn that this 25% threshold offers little empirical leverage in practice. Out of 110 municipal governments in California that held their elections off-cycle before January 2018, none had sufficient voter turnout to exempt it from the law. By 2020, nearly every local government in the state of California was holding their elections on-cycle.[2]

In the following analyses, I leverage this exogenous shock in election timing to test seven hypotheses suggested by the literature.

First, I expect on-cycle elections to boost voter turnout, consistent with a wealth of evidence in both cross-sectional and quasi-experimental settings (Berry and Gersen, 2010; Anzia, 2012*a*; Garmann, 2016). Second, I expect on-cycle elections to yield a more *diverse* electorate in terms of age, race, income, and homeownership. Because older, higher-income, and white voters are more likely to turn out to vote regardless of election timing, these groups should form a larger share of the off-cycle electorate (Kogan, Lavertu and Peskowitz, 2018). We also know from prior research that homeowners play a dominant role in local politics (Fischel, 2001; Einstein, Glick and Palmer, 2019), and are particularly motivated to vote during off-cycle elections when land use issues appear on the ballot (Yoder, 2020). I expect that the shift to on-cycle elections will yield a larger share of renters voting in local elections.

Third, I expect that this more diverse pool of voters will yield a more diverse pool of candidates and elected officials in local government. Prior research finds that renters (Einstein, Ornstein and Palmer, 2022) and members of the working class (Carnes, 2018; Kirkland, 2021) are substantially less likely to run for and win local political office than homeowners and white collar professionals. In on-cycle contests where the electorate is more socioeconomically diverse, I expect that more candidates from these backgrounds will choose to run for local public office, and will subsequently be more likely to win. There is some evidence that women candidates for school board and city council are more competitive on-cycle (Anzia and Bernhard, 2022), so I expect that women will be more likely to run for office in cities that switch to on-cycle elections as well.

[2]In March 2020, the state's Second Court of Appeals ruled that SB 415 did not apply to charter cities, on the grounds that California's home rule provision explicitly grants charter cities the power to legislate "the times at which...the several municipal officers...shall be elected" (see *City of Redondo Beach v. Padilla*). Despite this ruling, the vast majority of charter cities held their elections on-cycle by 2020, either because they switched their timing in 2018, not anticipating a court challenge, or already held their elections on-cycle. As of 2023, only 10 cities in California continue to regularly hold their elections off-cycle (including Redondo Beach, the city that brought the lawsuit), but this is too small a sample for statistical analysis.

Fourth, I expect that the switch to on-cycle elections will bolster the incumbency advantage, consistent with theory and evidence from (de Benedictis-Kessner, 2018). Because voters in off-cycle elections tend to be better-informed about local issues and candidates (Oliver and Ha, 2007), they are more likely to make choices on the basis of personal connections and community knowledge, rather than heuristics like incumbency status (Oliver, Ha and Callen, 2012). Furthermore, if switching to on-cycle elections prompts a new, diverse set of candidates to enter local races, this could increase the competitiveness of local elections and endanger incumbents.

Fifth, I have mixed expectations about the effect of election timing on school teacher salaries. On the one hand, there is evidence that off-cycle elections empower organized interest groups like public school employees, who form a larger share of the electorate in low-turnout contests for school board (Anzia, 2013). This could cause school boards to pass larger pay raises for teachers, as Anzia (2012*a*) documents in Texas. On the other hand, Kogan, Lavertu and Peskowitz (2018) show that the off-cycle electorate is older, more conservative, and less likely to have children enrolled in public schools, which could push school districts towards *reducing* taxes and public employee salaries. The direction of the average treatment effect depends on which of these effects is strongest.

Sixth, I expect that the switch to on-cycle elections will significantly reduce municipal expenditures on police relative to cities that did not switch. Unlike school teacher salaries, this effect should theoretically be stronger, since off-cycle elections both empower organized interests (e.g. police unions) and the off-cycle electorate tilts more strongly towards pro-police groups, like white voters, conservative voters, and senior citizens (Goldstein, 2021).

Finally, I expect that the increase in participation from younger voters and renters will encourage local politicians to pursue more expansive housing policy. One of the most prominent organized interest groups empowered by off-cycle elections is homeowners (Fischel, 2001), a group that frequently advocates for restrictions on new homebuilding, regardless of political ideology (Marble and Nall, 2021). If off-cycle elections dilute the political power of homeowners in favor of renters, I expect that to be reflected in a higher rate of residential construction permits for cities that switched their election timing in 2018.

3 Data and Methods

The data employed in this study are drawn from multiple sources, including the California voter file, property tax records, election results, public finance data from the California Controller's office, and the US Census Bureau's American Community Survey. For information on voter and candidate characteristics, I refer to a nationwide voter file provided by L2. These data contain over 190 million unique voter records, compiled from every state and county voter registry in the United States. This includes names, addresses, dates of birth, and turnout records for every election since 2000. In addition, L2 imputes ethnicity based on each voter's address and surname, and imputes homeownership based on a merge with county-level property tax records.

Because the voter file only includes voters who are registered to vote at their current residence as of 2022, my dataset will be missing records from any voters who moved out of state during the period of analysis. This missing data problem becomes more acute the further back in time we go, and if certain groups of voters (e.g. renters) are more likely to change residence than other groups, it will bias my estimates of voter demographics in these earlier elections. To address this concern, I do not include elections in the dataset that occur prior to 2010.

My data on election results come from the California Election Data Archive (CEDA), an extensive database of every election held in the state of California since 1996.[3] Subsetting the data to elections for school board member, mayor, and city councilmember (or the equivalent legislative body, like County Supervisor in San Francisco), I code each election as on-cycle if it coincided with an even-year state or federal election, and off-cycle otherwise. Each candidate for public office in California provides a ballot designation that reports their incumbency status or occupation. I use this field to generate a measure of occupation type for each candidate (working class or non-working class) according to the criteria from Carnes and Lupu (2016).[4] To determine the race, age, and homeownership status of candidates, I conduct a probabilistic record linkage with the L2 voter file based on a fuzzy string match for full name and exact match on city of residence (Ornstein, 2024).

Out of 480 cities in the CEDA dataset, 346 cities (72.1% of the total) have elected their officials on-cycle continuously since 2010; I refer to this group as *Always Treated*. There are an additional 120 cities (25%) that switched their election timing from off-cycle to on-cycle at some point between

[3] Available at `http://www.csus.edu/isr/projects/ceda.html`.

[4] See Appendix B for details on the classification procedure.

2010 and 2023. Of the switchers, 100 cities (83%) did so after 2018 to comply with SB 415. I refer to this group as *Switchers*. This leaves only 14 cities with either consistently off-cycle or inconsistent election timing. I omit these cities from the analysis, focusing on the comparison between *Always Treated* and *Switchers*.

Because treatment assignment is not staggered (all cities in my sample hold their elections on-cycle after January 1, 2018), throughout the paper I estimate average treatment effects using the two-way fixed effects estimator (TWFE), regressing outcomes on treatment status, unit fixed effects, and year fixed effects, clustering standard errors at the city level.[5] The key identifying assumption for this difference-in-difference design is "parallel trends"—absent treatment, the cities in the *Switcher* group would have exhibited the same trend in outcomes as the *Always Treated*.

Because this assumption is more credible for treatment and control groups with similar observed covariates (Abadie, 2005), I match each city in the *Switcher* group with a similar city in the *Always Treated* group (one-to-one nearest neighbor matching using Mahalnobis distance) on a set of place-level variables from the 2018 American Community Survey (5-year estimates).[6] In the matched sample, there are no significant differences between the two groups on these covariates (see Appendix A for balance statistics). Unless otherwise specified, all subsequent analyses are conducted using this matched sample of local governments.

Hankinson and Magazinnik (2023) document a large number of California municipal governments shifting from at-large to district-based elections in response to litigation under the California Voter Rights Act of 2001—a total of 129 cities between 2010 and 2020. To avoid conflating the effects of these two electoral reforms, I also match my sample of cities based on whether a city shifted to district elections during the period of analysis. This variable is well-balanced between groups in the matched sample: 15% of the *Switcher* group and 16% of the *Always Treated* group.

There is a clear geospatial pattern in election timing prior to 2018. The cities that held their elections off-cycle before SB 415 are predominantly in San Mateo or Los Angeles counties, as the map in Figure A13 illustrates. This is a source of imbalance between the Always Treated and Switcher groups that could pose a threat to inference if wherever trends in outcomes are similarly

[5] When treatment is staggered over time, TWFE does not produce consistent estimates of the average treatment effect except under heroic assumptions (Goodman-Bacon, 2021).

[6] Median Age, Percent of Residents Over 65 Years of Age, Median Household Income, Percent White, Percent Black, Percent Hispanic, Percent Asian, Percent Bachelor's Degree or Higher, Population, Percent of Housing Units Owner Occupied.

spatially auto-correlated.

Building permits data come from the US Census Building Permits Survey, which collects counts of all new housing units approved by each permit-approving jurisdiction in the United States, conducted annually since 1980. I use the year-to-date cumulative permits files for each year from the western Census region.[7] For each municipality-year, my measure of permitting intensity is the number of units permitted per 10,000 residents. When estimating the effect of election timing on housing policy outcomes, I also match on an additional variable that is likely to affect the rate of new homebuilding: the percent of developable land within city limits. Municipalities with an abundance of developable land are likely to have an easier time expanding their housing supply than land-constrained cities, since greenfield development is significantly less costly than infill development (Saiz, 2010). I generate a measure of developable land for each municipality in my dataset through a three-step process. First, I use the National Land Cover Dataset (NLCD) to identify all parcels of land within a city that were undeveloped as of 2016 (pre-treatment). I then identify which of those parcels are developable, following criteria from (Saiz, 2010). I exclude any land that is classified as wetlands in the NLCD, as well as any terrain that is too steep to build on (grade greater than 15 percent), which I compute from the USGS Digital Elevation Model (90 sq. meter grid cells).[8] I combine this information to compute the fraction of land area within each municipality that matches these criteria (undeveloped, not-too-steep, and not wetlands).

Financial data is compiled annually by the California State Controller's Office (SCO). The chief advantage of this dataset compared to the US Census Annual Survey of Governments (another common dataset used to study municipal finance) is that there is no survey nonresponse; every California municipality is required by law to report its financial data to the state through a uniform Financial Transactions Report (California Code, section 53891). In FY 2016-2017, the SCO moved their reporting system online, and as a result, there appear to be some inconsistencies in expenditure values between 2016 and 2017. Because of this, I restrict my analyses to expenditure categories that are consistently reported across time periods, like police operating expenditures.

My data on school teacher salaries comes from the California Department of Education's "Salary and Benefits Schedule for the Certificated Bargaining Unit (Form J-90)". Each year, school districts

[7] For the year 2023, I have year-to-date data as of August.

[8] Data available from the US Geological Survey, accessed through the `FedData` package in R (Bocinsky, 2017).

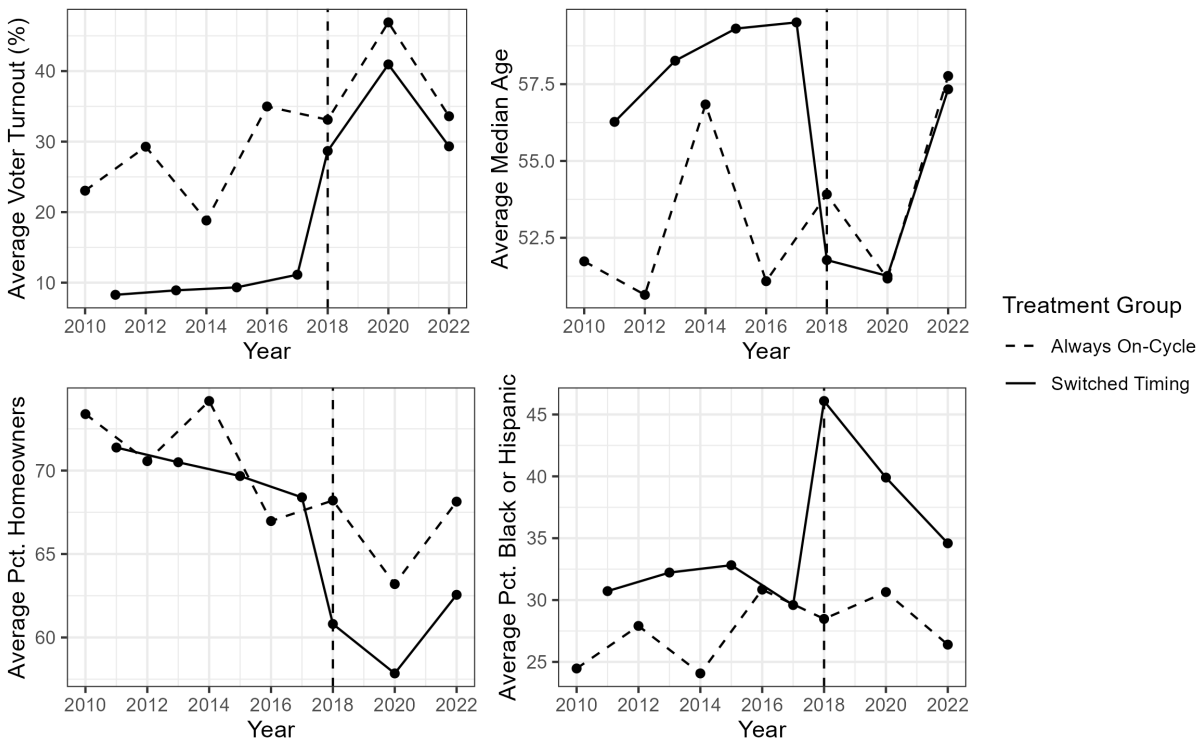

Figure 1: Voter turnout and demographics by year and treatment group, matched cities sample.

in California set a salary schedule, including a minimum salary for teaching staff. I use this minimum salary as my primary measure of school employee expenditures, since it better reflects the bargaining power of teachers than average teacher salary, which is determined partly by the salary schedule partly by the seniority of the teaching staff in a particular district.

4 Results

4.1 Voter Turnout

Consistent with expectations, I find that the switch to on-cycle elections yielded a significant increase in voter turnout (approximately 10% higher turnout in statewide primary elections and 21% higher turnout in November general elections). The on-cycle electorate is also significantly younger on average (8 years younger in November general elections), less likely to be homeowners (about 9 percentage points), and has a larger share of Black and Hispanic voters (about 9 percentage points). Figure 1 and Table 1 report those effects.

Table 1: Estimated effects and 95% confidence intervals for holding elections concurrent with statewide primary and general election dates on the composition of the electorate. All models include year and city fixed effects, weighted by the number of voters. Matched cities sample. Standard errors clustered at the city level.

	Median Age	Pct. Homeowners	Pct. Black or Hispanic
Primary	−3.542	−3.867	5.581
	[−6.960, −0.123]	[−5.942, −1.792]	[0.067, 11.094]
General	−8.238	−8.582	8.673
	[−11.115, −5.362]	[−10.647, −6.517]	[3.354, 13.992]
Num.Obs.	1151	1151	1151

4.2 Candidates and Descriptive Representation

Contrary to expectations, I find no evidence that the switch to on-cycle elections affected the demographics of candidates for local public office. Figure 2 plots the prevalence of candidate characteristics before and after 2018. Although the candidate pool became more diverse on average across all local elections—younger candidates, more renters, more women candidates—the cities where elections remained on-cycle throughout the entire period exhibited the exact same trends as those that switched. If anything, there appears to be a small (albeit statistically insignificant) *negative* effect of on-cycle timing on the percent of candidates in working class occupations.

4.3 Incumbency Advantage

I expected to find that the switch to on-cycle elections would bolster the incumbency advantage in municipal elections, but, as illustrated in Figure 3, I find no evidence for this effect. The rate at which incumbents were re-elected to office declines in both groups of cities during this period, and in fact the decline in incumbency advantage is even *greater* for the cities that were compelled to switch their election timing on-cycle. The estimated average treatment effect is roughly -1 percentage point in the rate of incumbent re-election (95% CI: [-8.2, 6.3]).

4.4 Public Employee Salaries

Similarly, I find only weak evidence that the switch to on-cycle elections reduces public employee salaries (Figures 4 and 5). The estimated effect of on-cycle elections on minimum teacher salary is approximately -0.3% (95% CI: [-1.3%, 0.6%]), and the estimated effect on police spending per

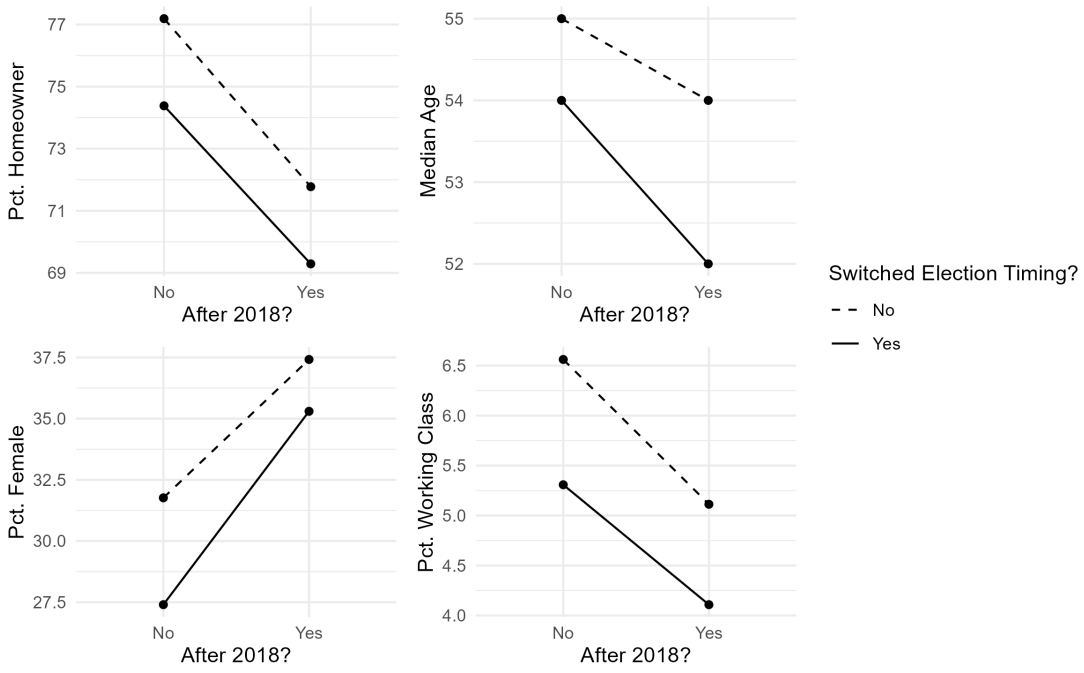

Figure 2: Candidate characteristics before and after the implementation of SB 415, matched sample of cities.

capita is approximately -2.1% (95% CI: [-5.1%, 0.8%]). Neither estimate is distinguishable from zero at conventional levels of statistical significance.

4.5 Housing Policy

Only 84 cities in the *Switcher* group have complete building permits data submitted to the Census Building Permit Survey during our period of analysis (2010-2023). The cities with missing data are small, with a median population of 9,508. In the following analysis, I omit these cities along with Los Angeles and San Diego to ensure that the treatment and control groups are balanced on population. I match the remaining 83 *Switcher* cities with those in the *Always Treated* group as specified in Section 3.

I find no significant effects of election timing on building permits (Figure 6). The estimated effect on annual building permits per 10,000 residents is 1.22 (95% CI: [-3.9, 6.35]), and the estimated effect on multifamily building permits per 10,000 residents is 1.13 (95% CI: [-2.7, 4.93]).

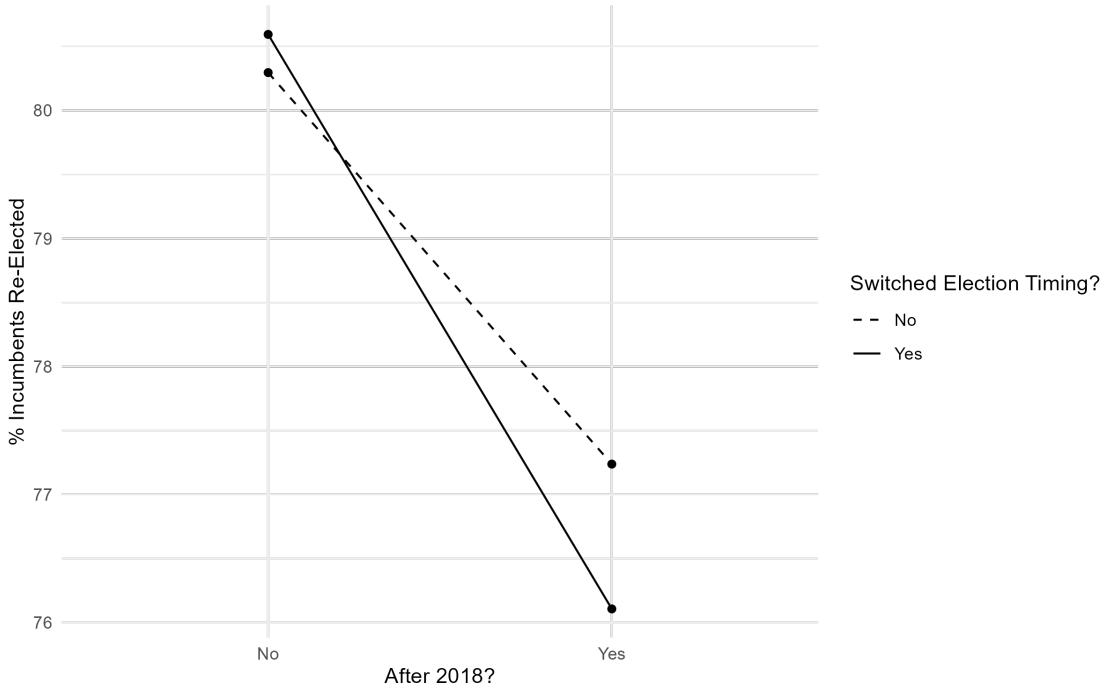

Figure 3: The rate at which incumbents were re-elected before and after the implementation of SB 415, by treatment group (matched sample).

5 Discussion

Whereas previous studies have found large effects of municipal election timing on outcomes like incumbency advantage (de Benedictis-Kessner, 2018) and public spending (Anzia, 2012*a*; Dynes, Hartney and Hayes, 2021), I find no evidence for these effects following the implementation of SB 415 in California. What might explain this discrepancy?

The difference is unlikely to be due simply to lack of statistical power; the null hypothesis tests described above are high-powered enough to detect effect sizes similar in magnitude to those Anzia (2012*a*) reports for school teacher salaries in Texas (1.3 percent) and de Benedictis-Kessner (2018) reports for the incumbency advantage (27 percentage points). Furthermore, the figures above make clear that a purely cross-sectional analysis—matching on observed covariates—would erroneously conclude that on-cycle elections cause an increase building permits, decrease in police spending, and decrease in school teacher salaries. Across the entire time series, there are large and statistically significant differences between the two matched groups of local governments on these outcomes. Whatever is causing that gap, it persists even after the implementation of SB 415, suggesting that

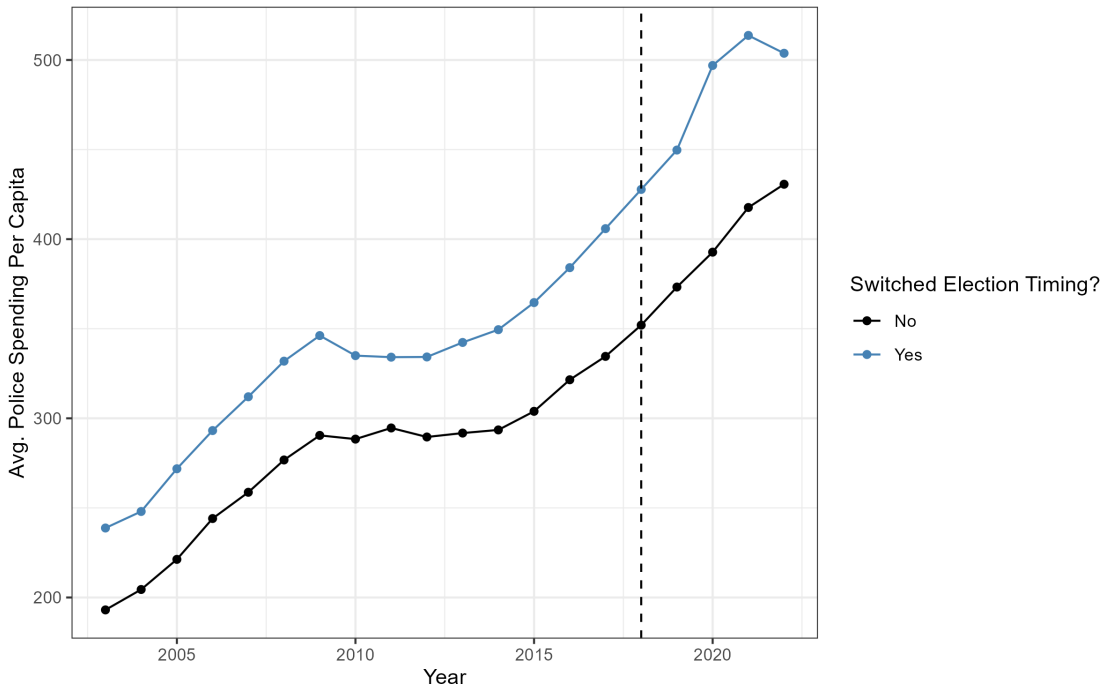

Figure 4: Police spending per capita by year and treatment group (matched cities).

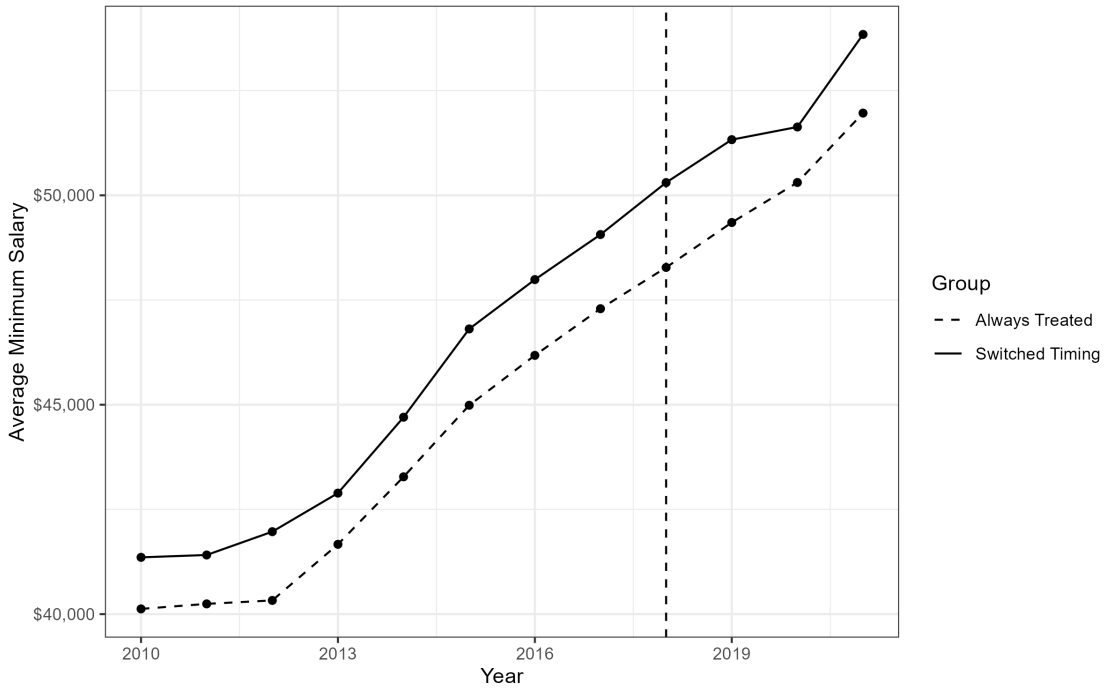

Figure 5: Average minimum teacher salary by year and treatment group (all school districts).

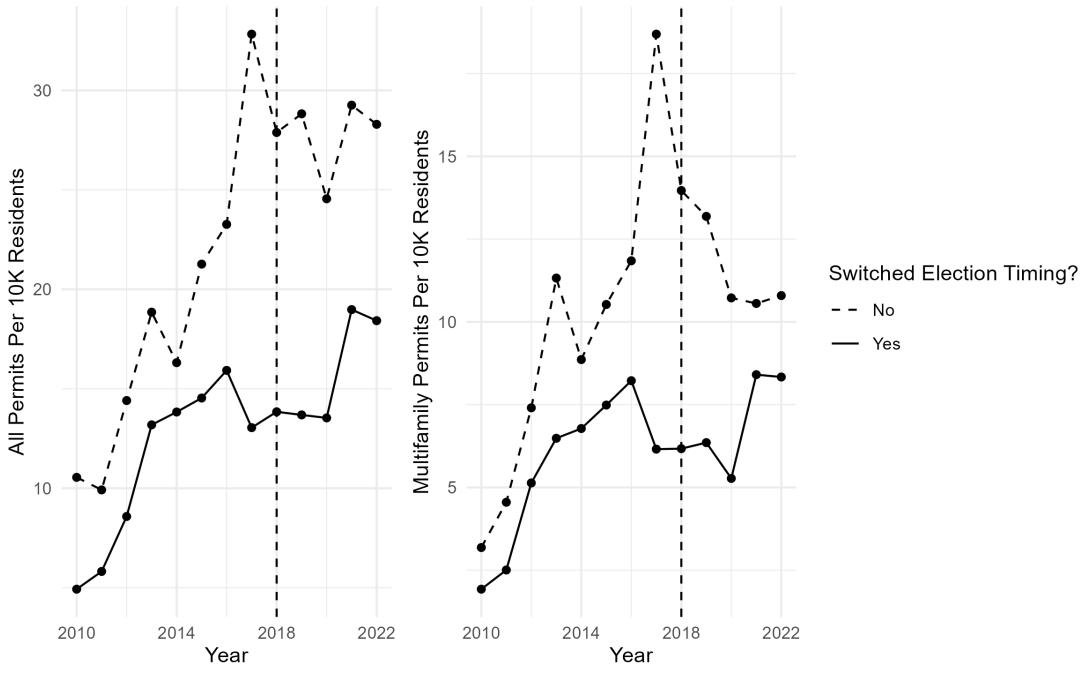

Figure 6: Average number of building permits per 10,000 residents by year and treatment group (matched sample of cities). Solid lines are all building permits, dashed lines are multifamily building permits.

the timing of elections is not a significant causal factor.

Another explanation is that local governments in California are peculiar in ways that would dampen the effect of election timing. For example, the state of California has strong public sector unions and collective bargaining rights for teachers, in sharp contrast to Texas. If the only way that public school teachers in Texas can exercise collective action is through their influence on school board elections, then it is perhaps unsurprising that Anzia (2012*a*) finds significant effects of election timing in Texas, but I find no effect in California.

Similarly, perhaps the null effects on incumbency advantage have to do with the time period that I analyze. de Benedictis-Kessner (2018) pools election results from 1950 to 2014, a period during which the incumbency advantage in American elections was historically much larger (Jacobson, 2015). As the incumbency advantage has weakened since the mid-20th century, one might expect to observe a weaker effect of election timing on the incumbency advantage as well. As with any empirical study, we face a tradeoff between internal and external validity. Focusing on California during this time period allows for cleaner causal identification, but the estimates presented here are

only generalizable to the extent that these California local governments are representative of local governments more generally.

If the timing of elections affects who turns out to vote but not who wins public office (as the evidence here suggests), then the link between mass participation in elections and policy is likely to be significantly weakened. Even six years after the implementation of SB 415, a substantial majority of local elected officials are white collar professionals and homeowners (Carnes, 2018; Einstein, Ornstein and Palmer, 2022). If the timing of elections does not meaningfully affect representation, then it is unsurprising that municipal governments continue to implement policies that benefit wealthy homeowners. Political scientists have long argued that American local politics is characterized by the outsized influence of residents with deep knowledge and interest in local issues—the "political stratum" (Dahl, 1961), the "homevoters" (Fischel, 2001), the "stakeholders" (Oliver, Ha and Callen, 2012), and the "neighborhood defenders" (Einstein, Glick and Palmer, 2019). Whatever one calls these groups, their influence over local government policy may not be so easily disrupted by election timing reforms. For mass participation to affect policymaking, it may require a deeper engagement with politics than showing up every few years to cast a ballot.

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A Balance Statistics

Table A1: Balance statistics for unmatched California municipal governments (part 1).

	Median Age	Log Population	Pct. White	Pct. Bachelors Degree
(Intercept)	37.9***	10.1***	46.8***	28.8***
	(0.4)	(0.1)	(1.3)	(1.0)
Switched Timing	2.4**	0.2	−9.0**	9.2***
	(0.8)	(0.2)	(2.8)	(2.2)
Num.Obs.	446	446	446	446

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A2: Balance statistics for matched California municipal governments (part 1).

	Median Age	Log Population	Pct. White	Pct. Bachelors Degree
(Intercept)	38.9***	10.5***	40.2***	34.4***
	(0.7)	(0.1)	(2.6)	(2.1)
Switched Timing	1.4	−0.2	−2.4	3.6
	(1.0)	(0.2)	(3.7)	(3.0)
Num.Obs.	200	200	200	200

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A3: Balance statistics for unmatched California municipal governments (part 2).

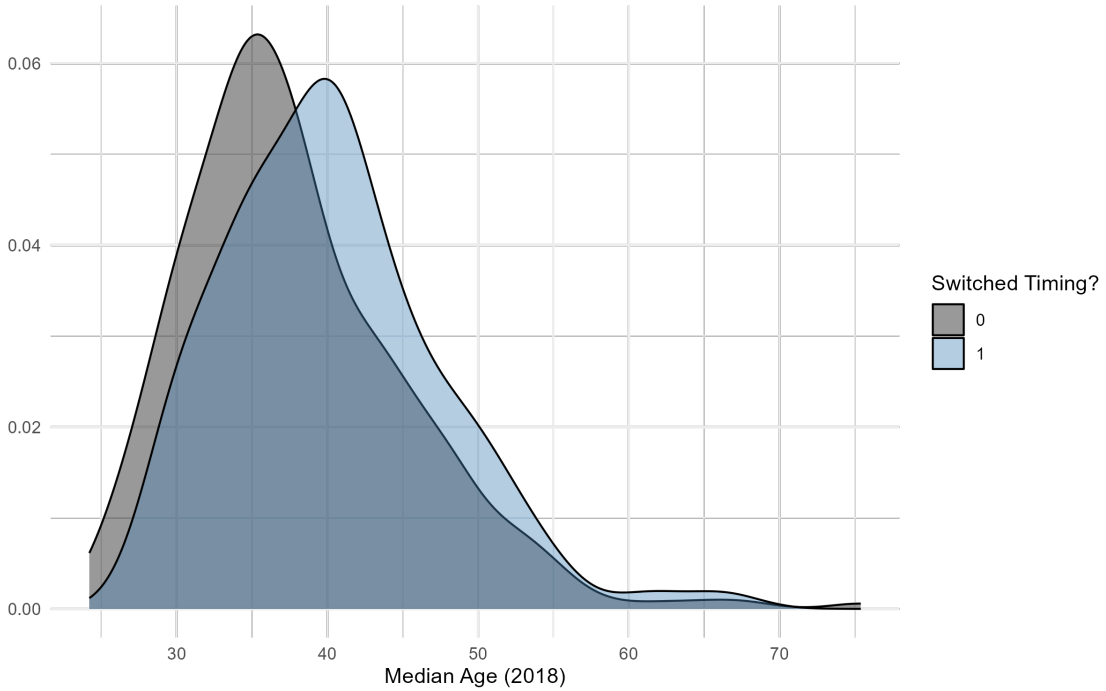
	Median HH Income ($1000)	Pct. Homeowner	Pct. Developable	Switched to District Elections
(Intercept)	34.0***	58.4***	32.1***	0.3***
	(0.9)	(0.7)	(1.1)	(0.0)
Switched Timing	6.5***	−0.9	−13.7***	−0.2**
	(1.9)	(1.6)	(2.4)	(0.0)
Num.Obs.	446	446	446	440

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

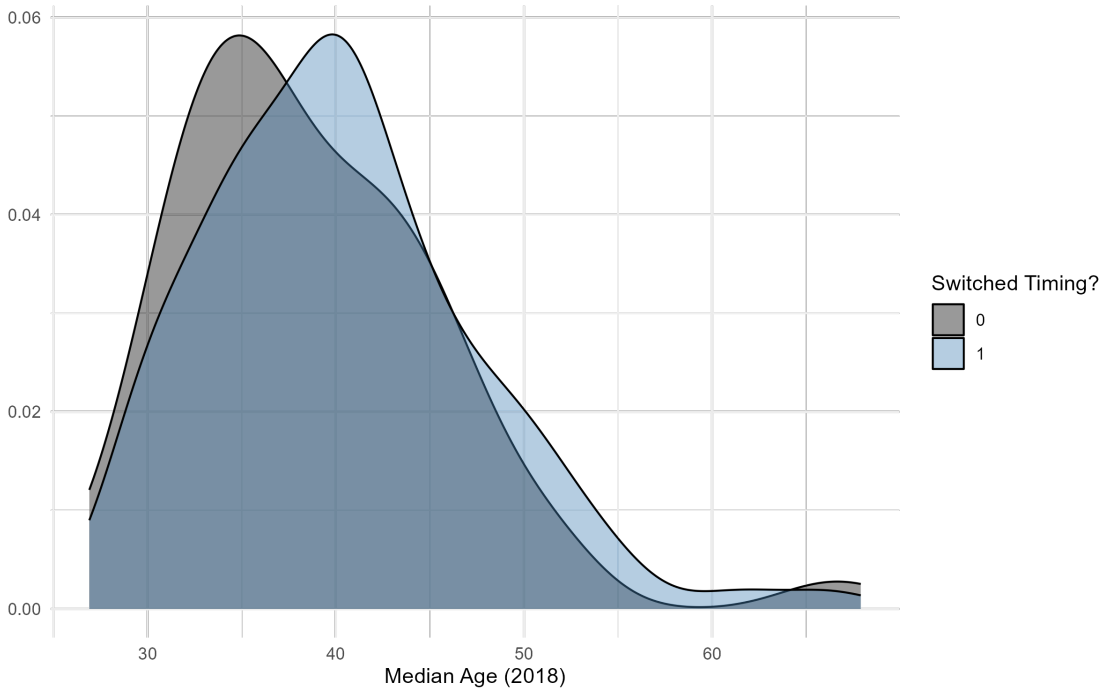
Table A4: Balance statistics for matched California municipal governments (part 2).

	Median HH Income ($1000)	Pct. Homeowner	Pct. Developable	Switched to District Elections
(Intercept)	38.4***	59.6***	21.6***	0.2***
	(1.9)	(1.6)	(2.0)	(0.0)
Switched Timing	2.0	−2.1	−3.2	0.0
	(2.7)	(2.2)	(2.8)	(0.1)
Num.Obs.	200	200	200	200

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

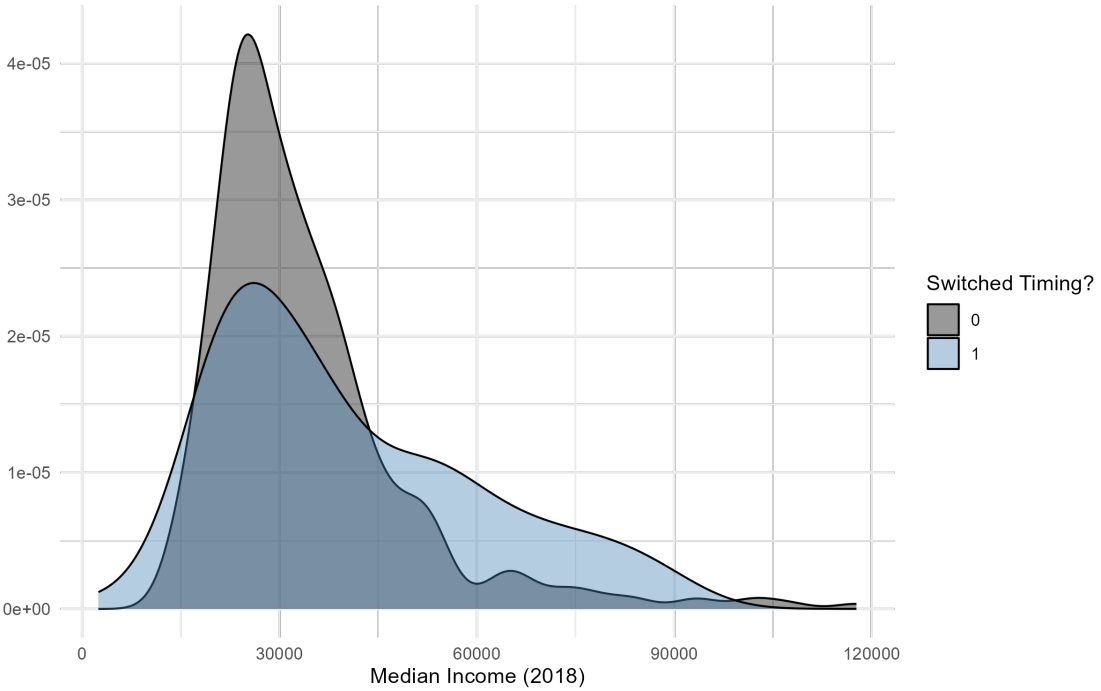


(a) Before Matching



(b) After Matching

Figure A1: Distribution of median age (2018) before and after matching.



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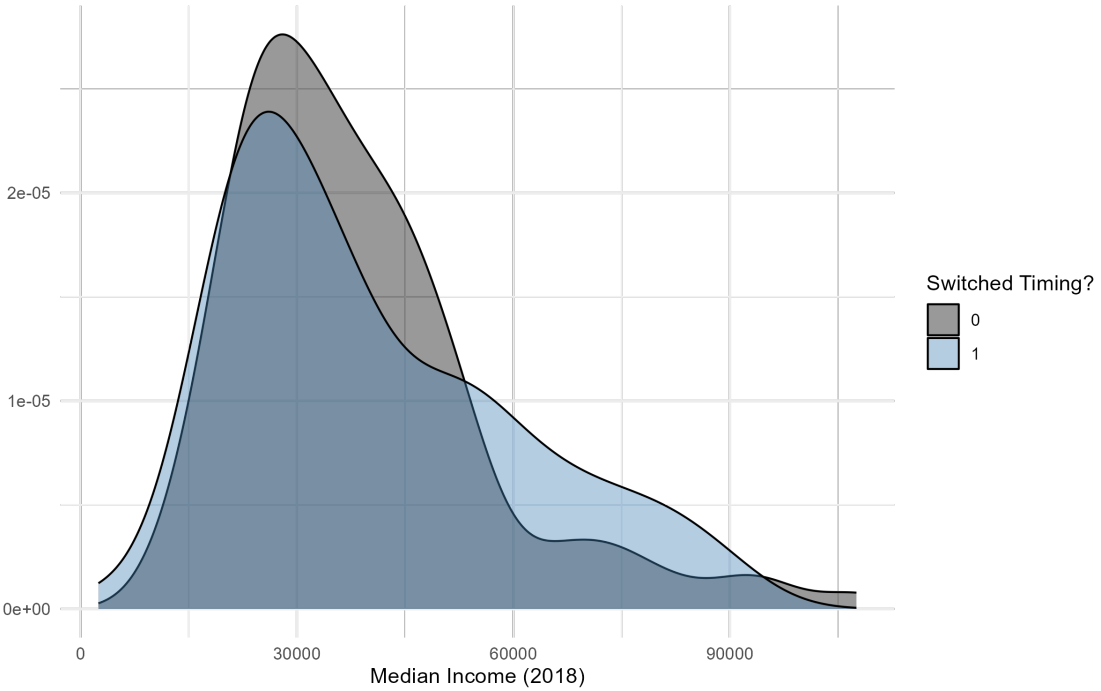
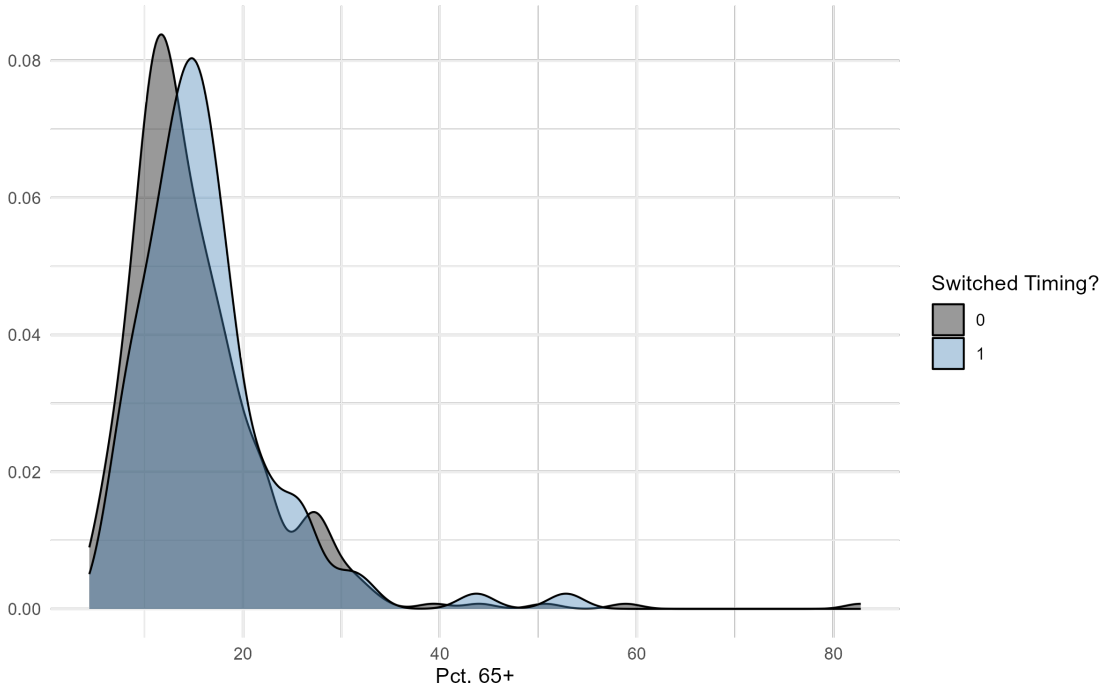
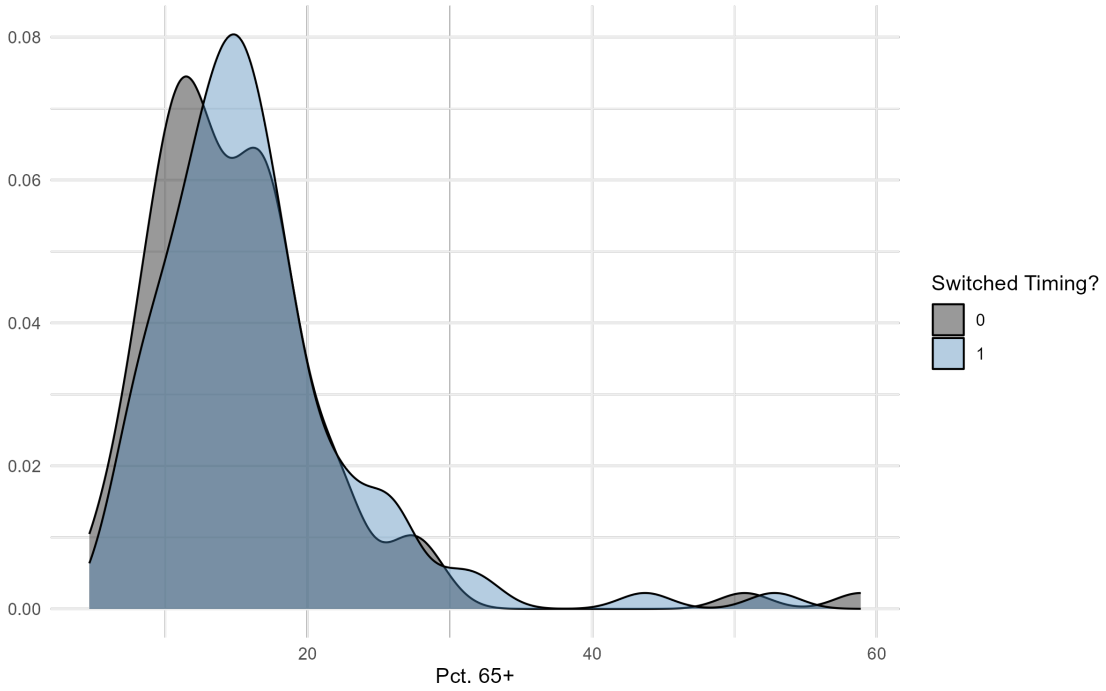


(b) After Matching

Figure A2: Distribution of median income (2018) before and after matching.

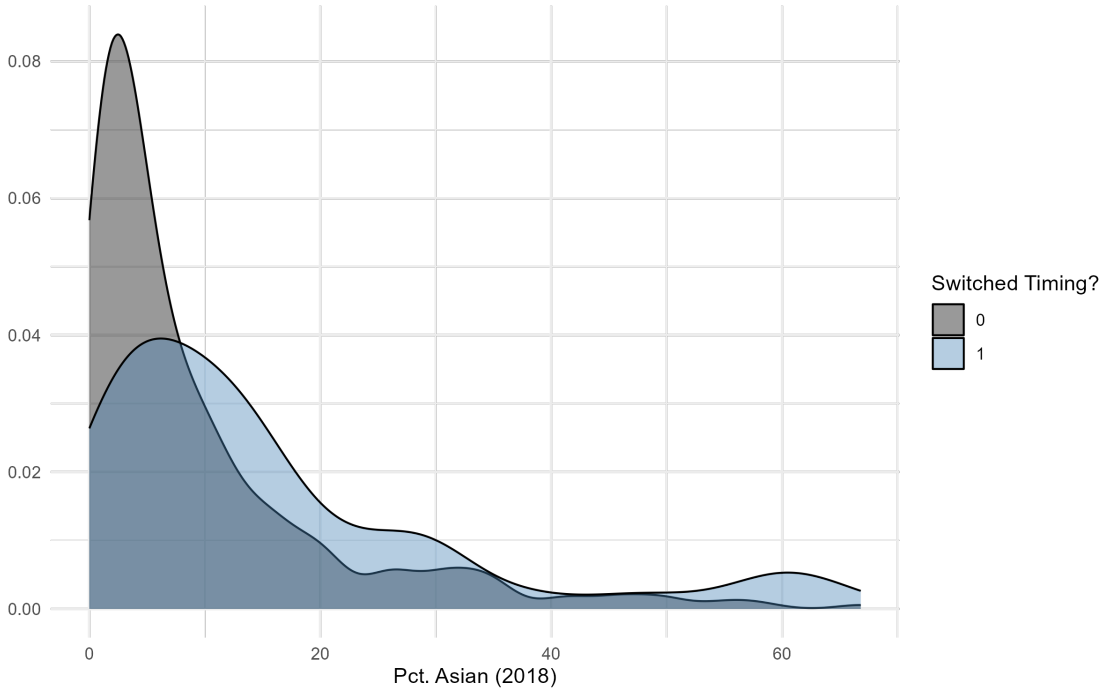


(a) Before Matching

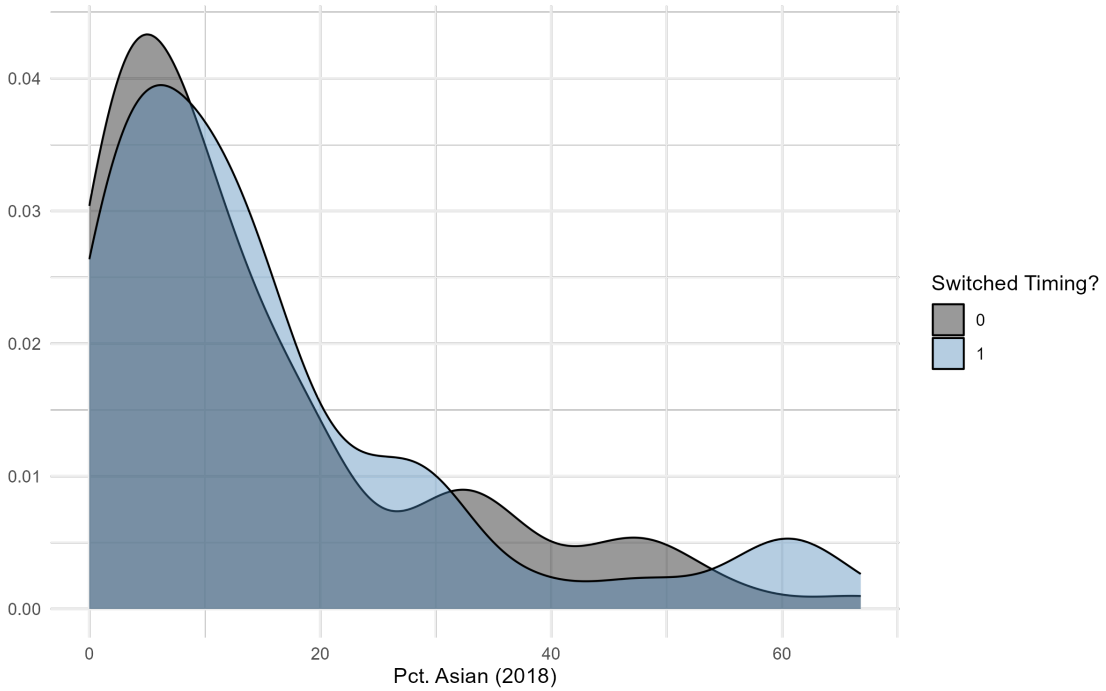


(b) After Matching

Figure A3: Distribution of pct. aged 65 and over before and after matching.

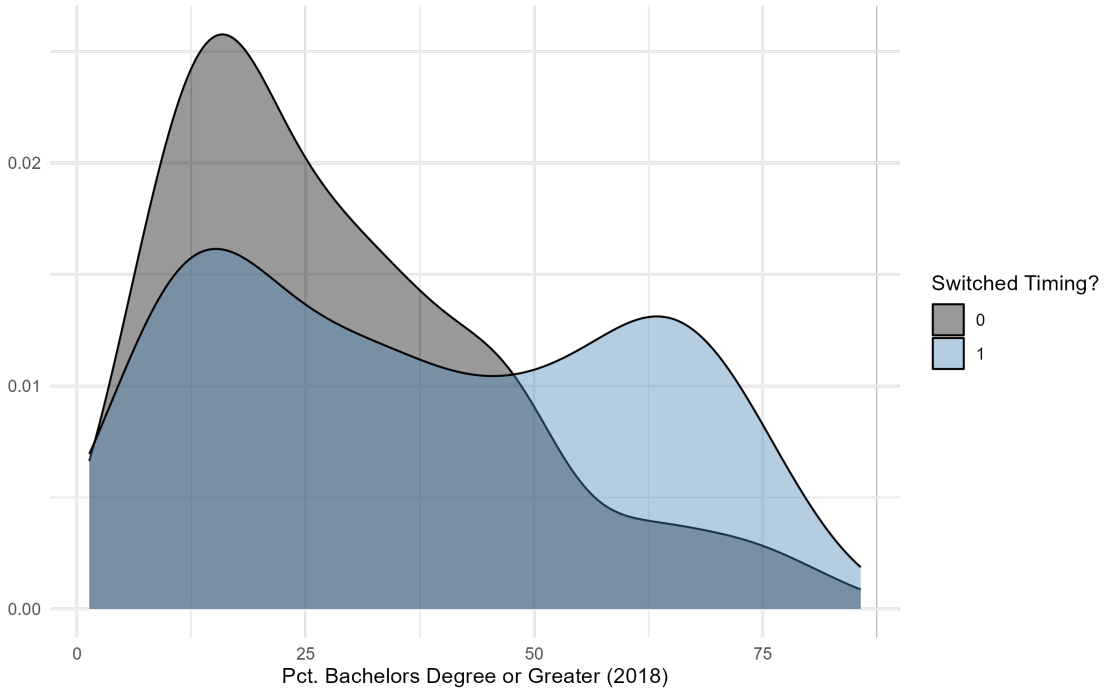


(a) Before Matching

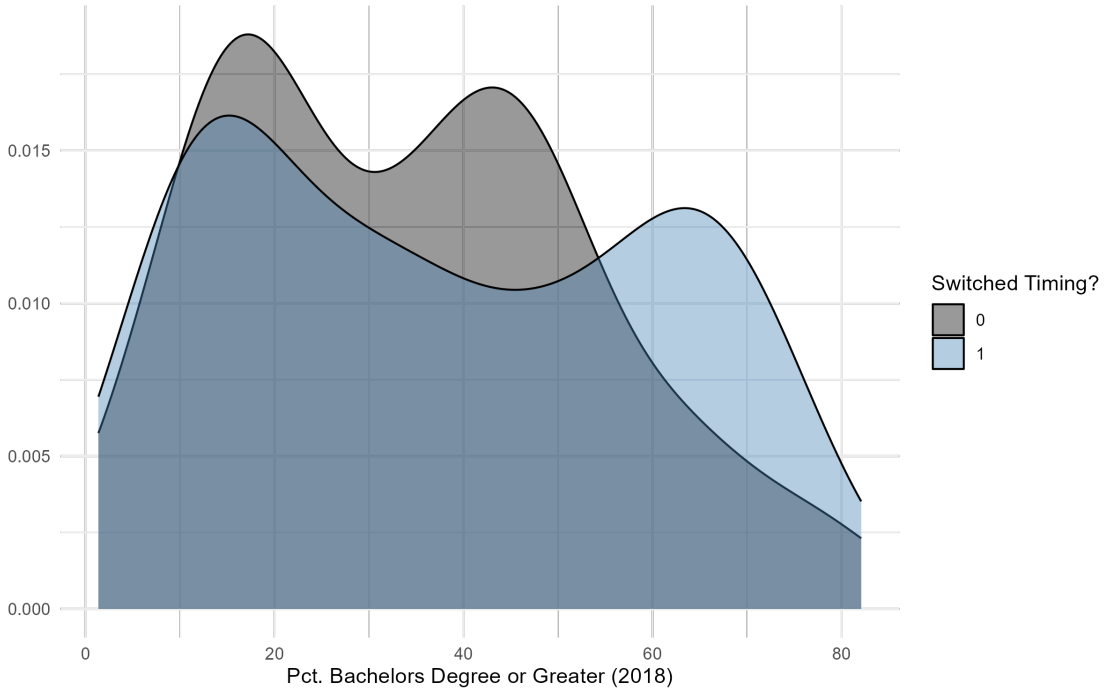


(b) After Matching

Figure A4: Distribution of Pct. Asian (2018) before and after matching.

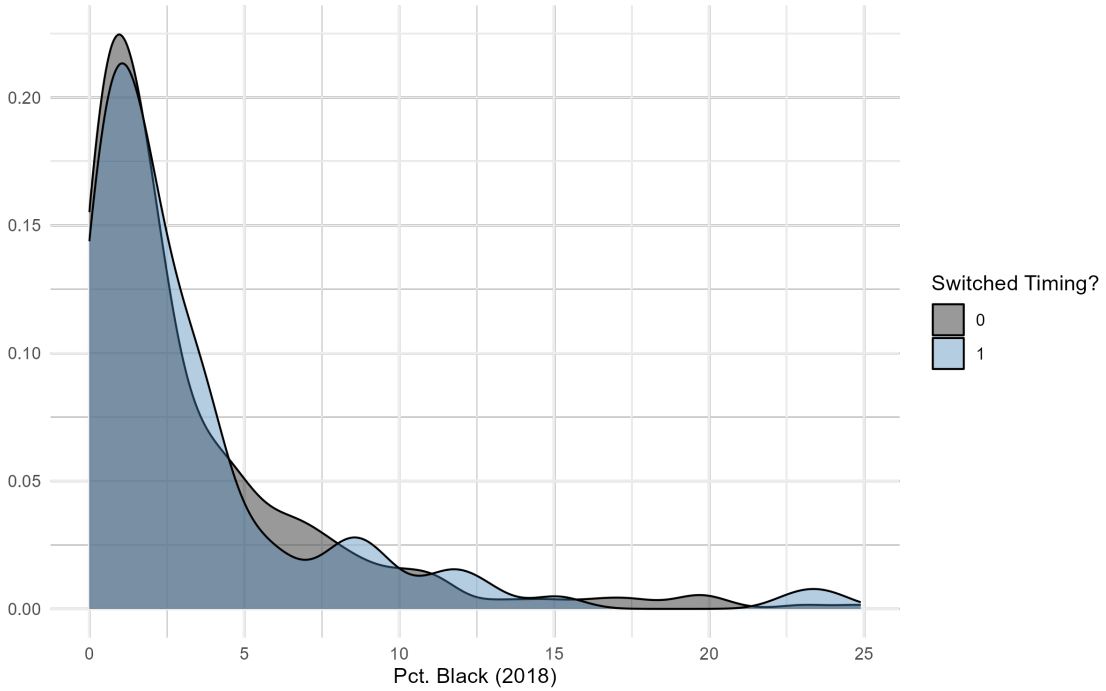


(a) Before Matching

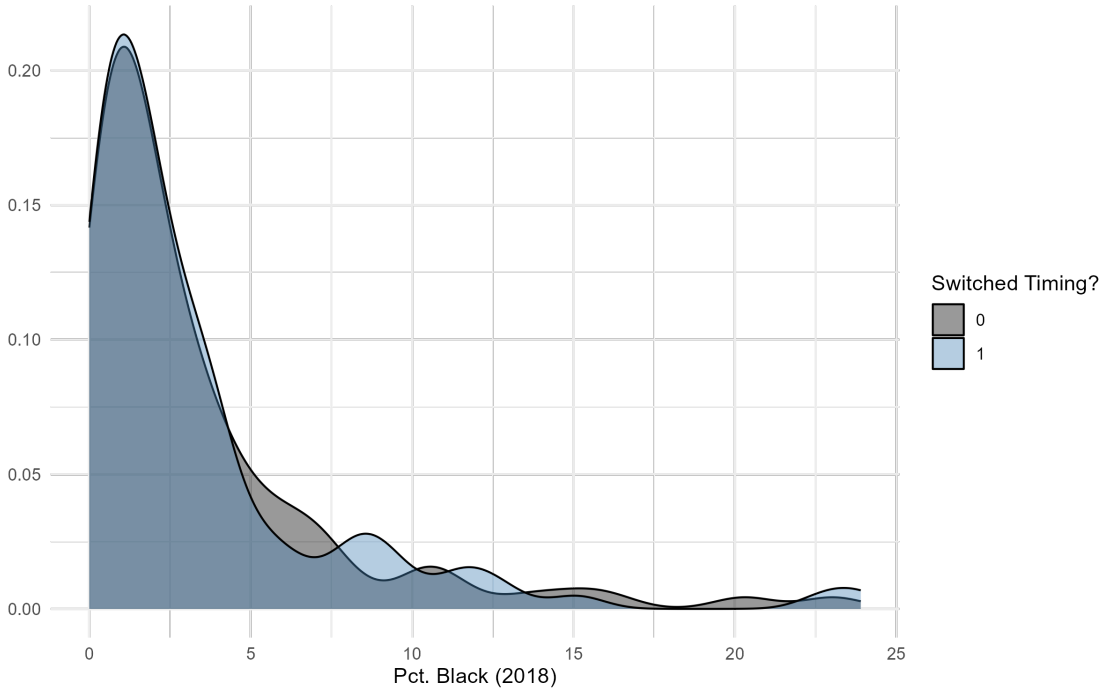


(b) After Matching

Figure A5: Distribution of Pct. Bachelor's Degree and Above (2018) before and after matching.

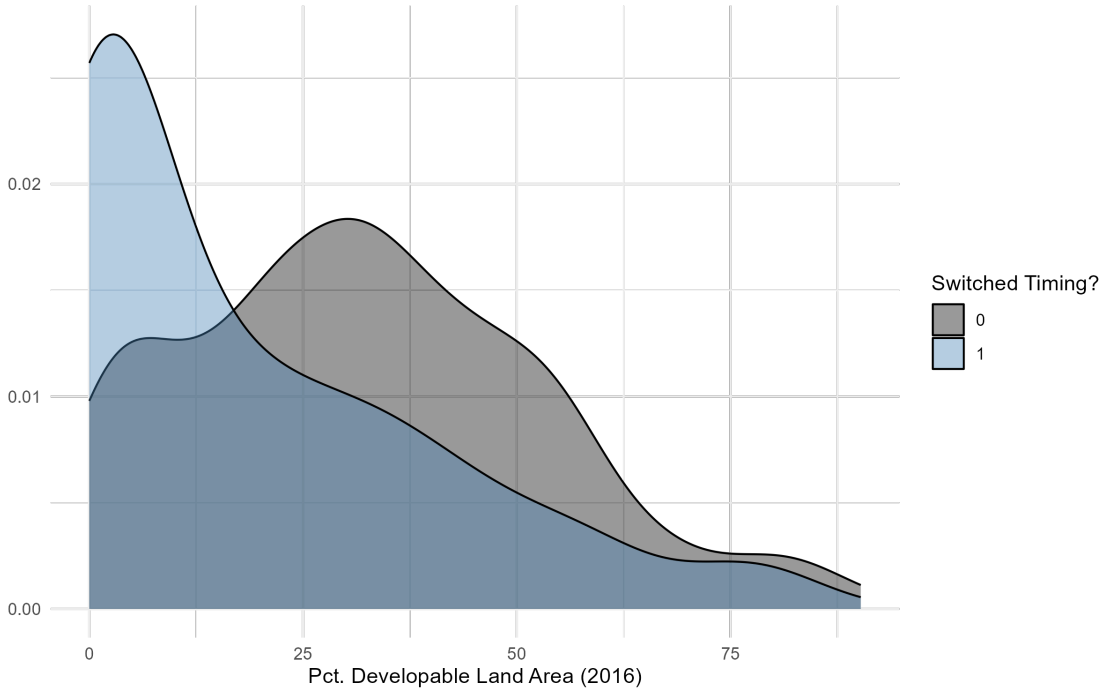


(a) Before Matching

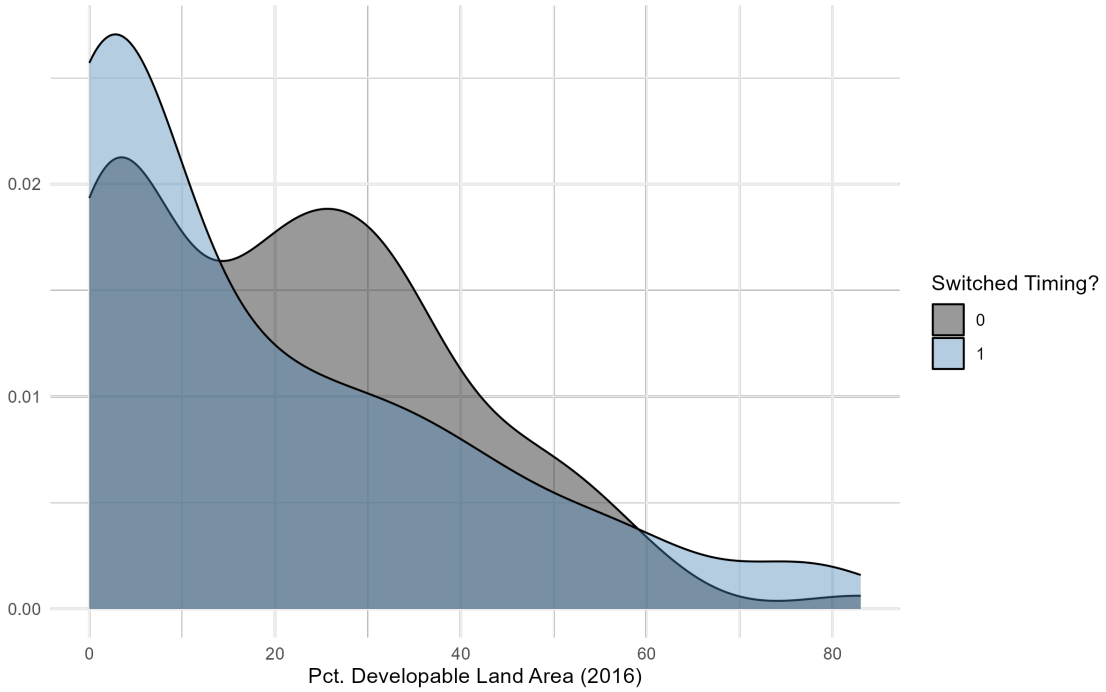


(b) After Matching

Figure A6: Distribution of Pct. Black (2018) before and after matching.

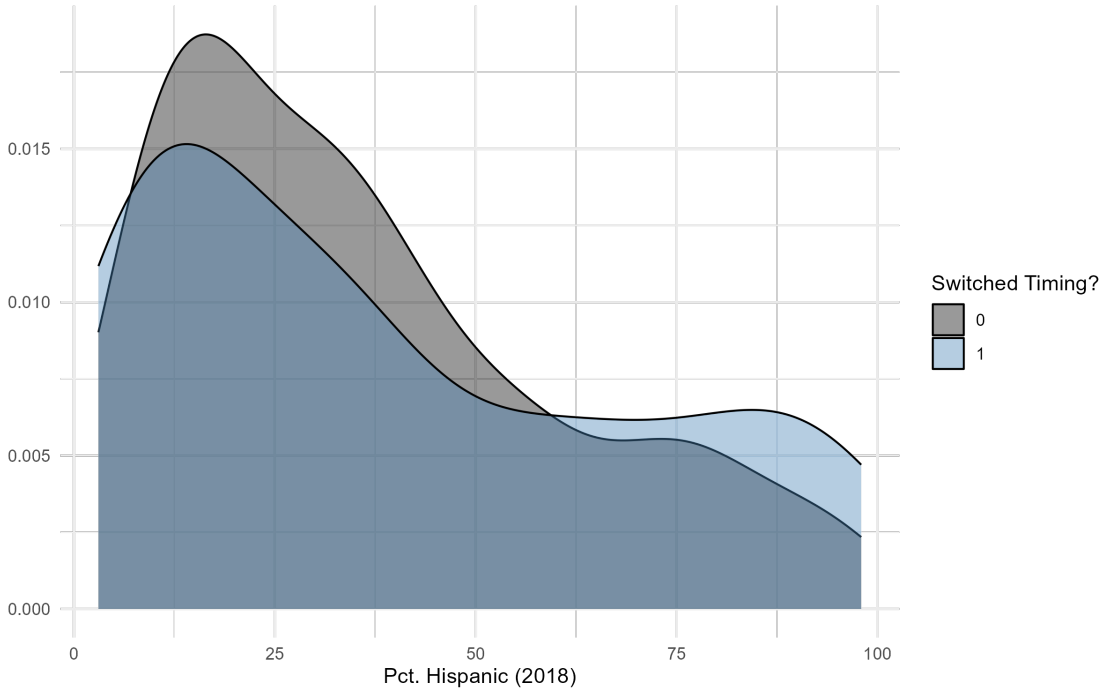


(a) Before Matching

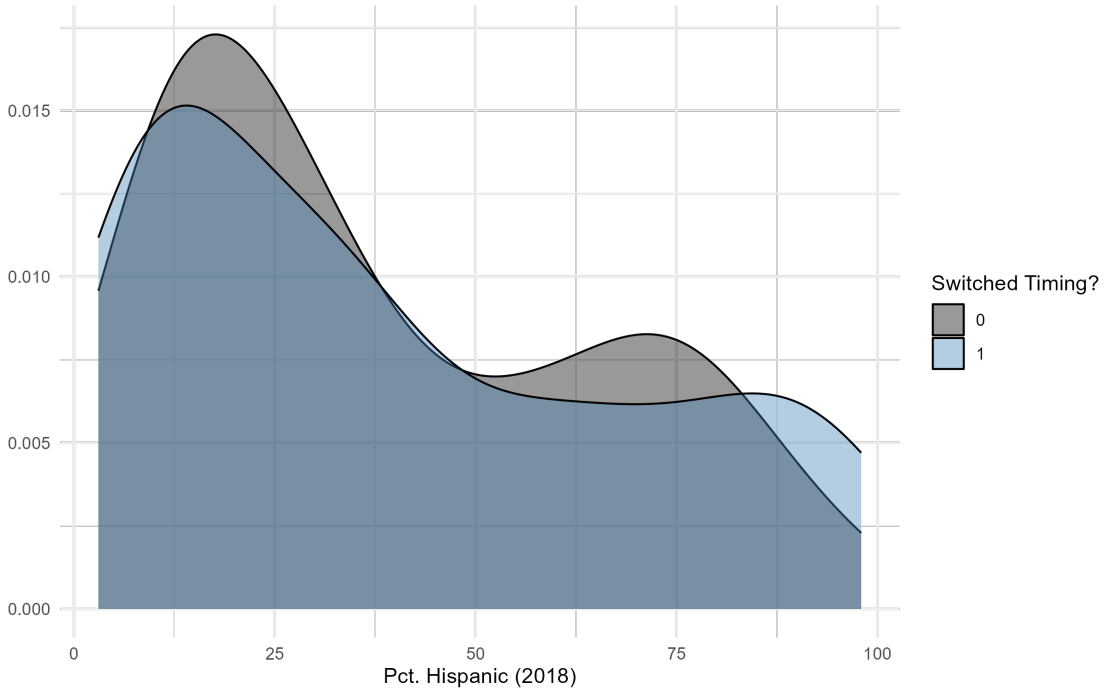


(b) After Matching

Figure A7: Distribution of Pct. Developable Land (2018) before and after matching.

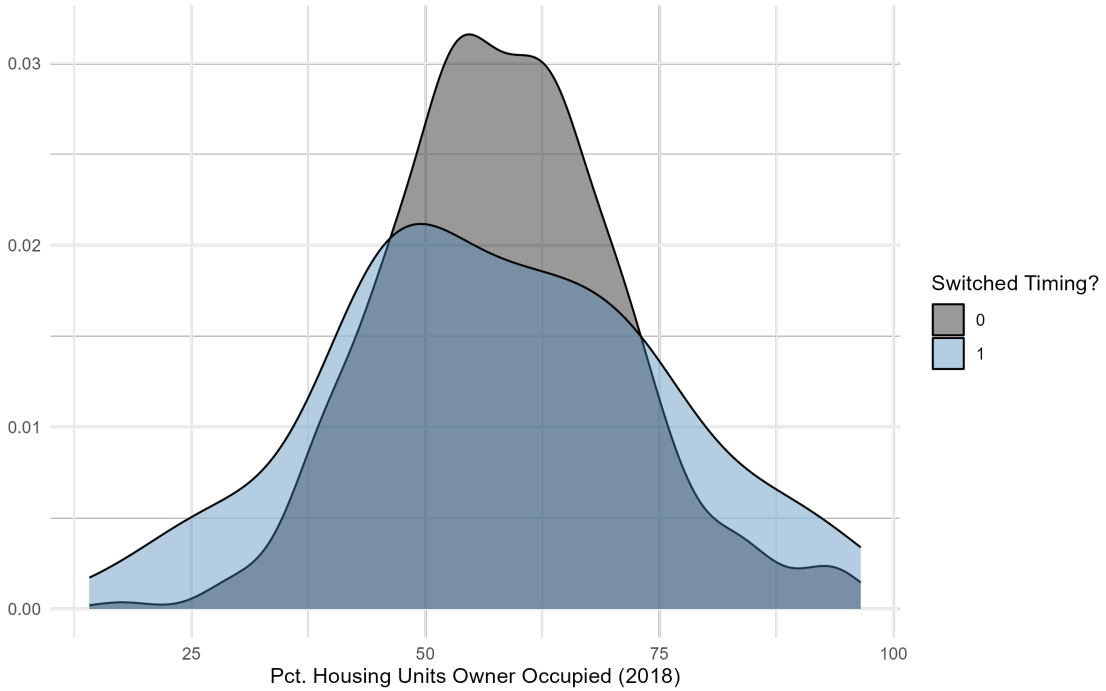


(a) Before Matching

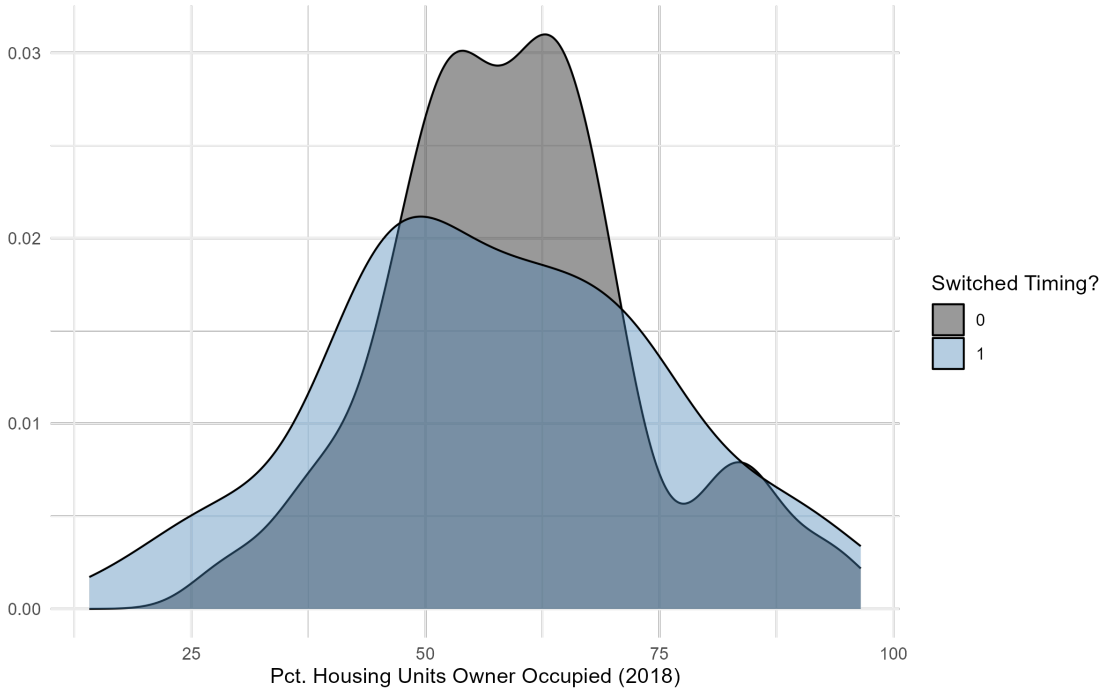


(b) After Matching

Figure A8: Distribution of Pct. Hispanic (2018) before and after matching.

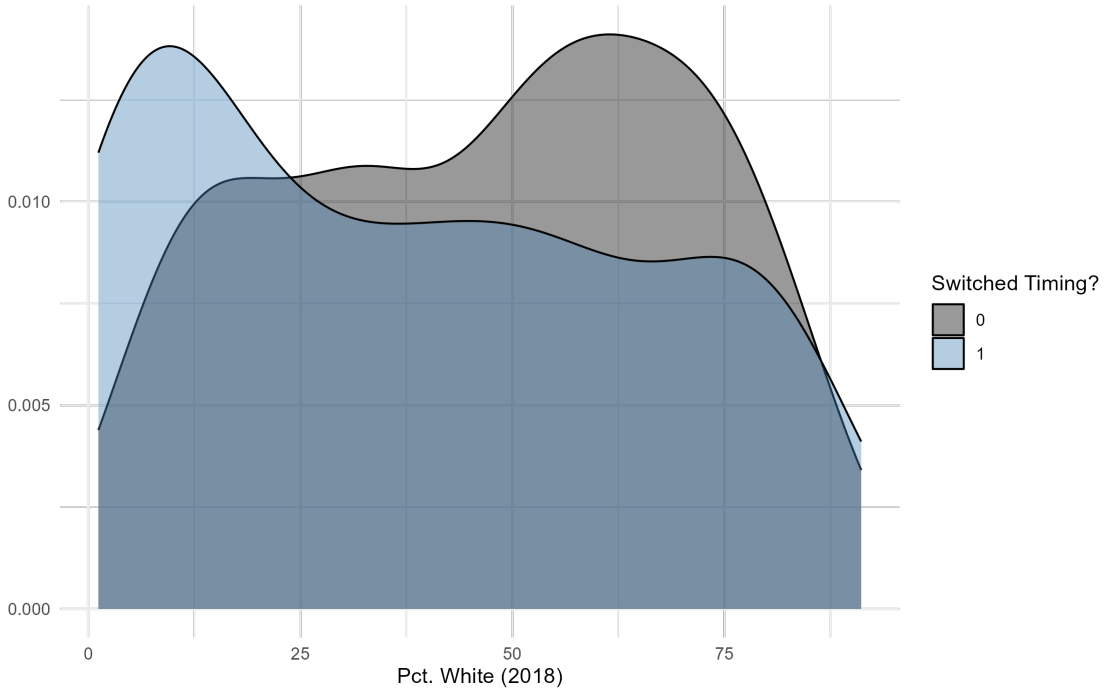


(a) Before Matching

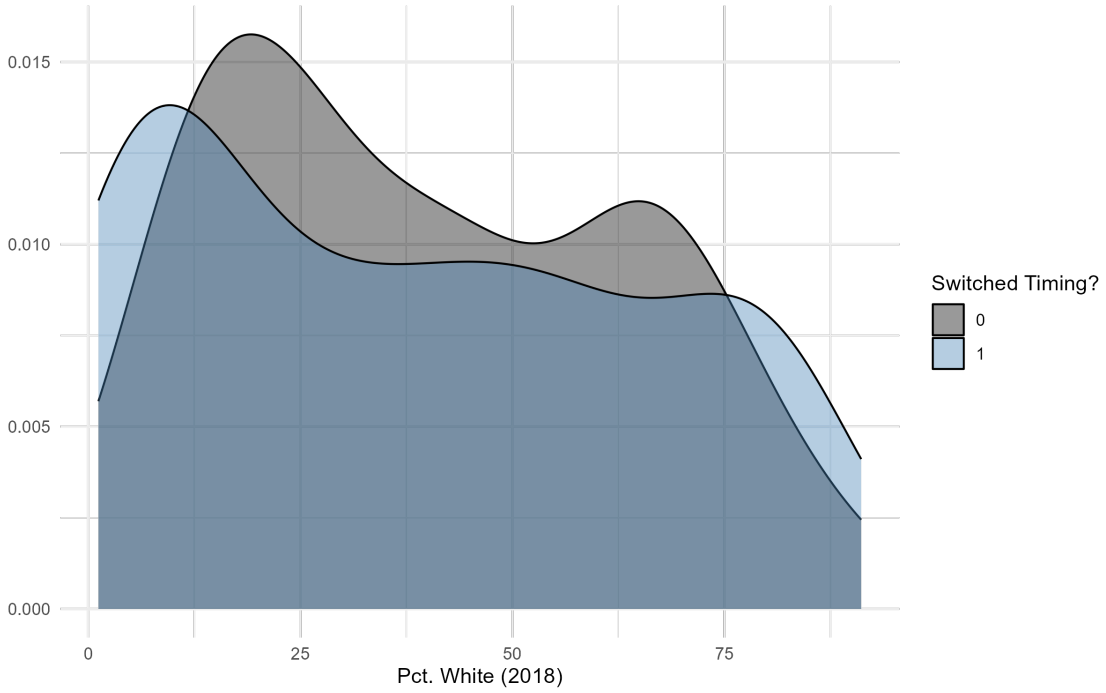


(b) After Matching

Figure A9: Distribution of Pct. Homeowner (2018) before and after matching.

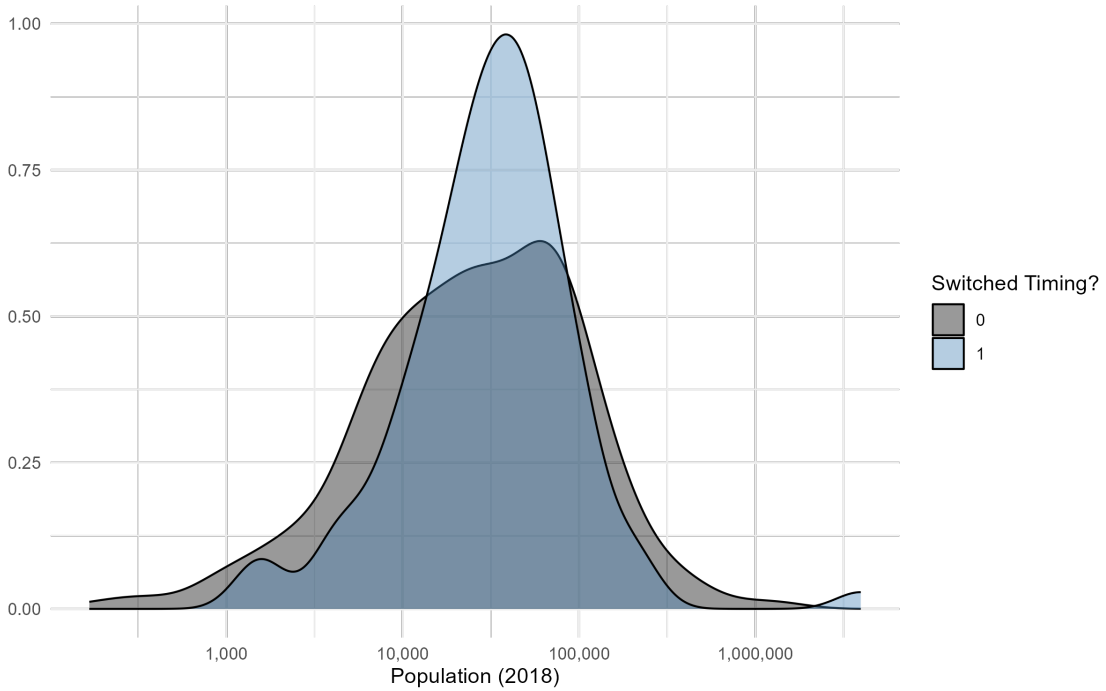


(a) Before Matching

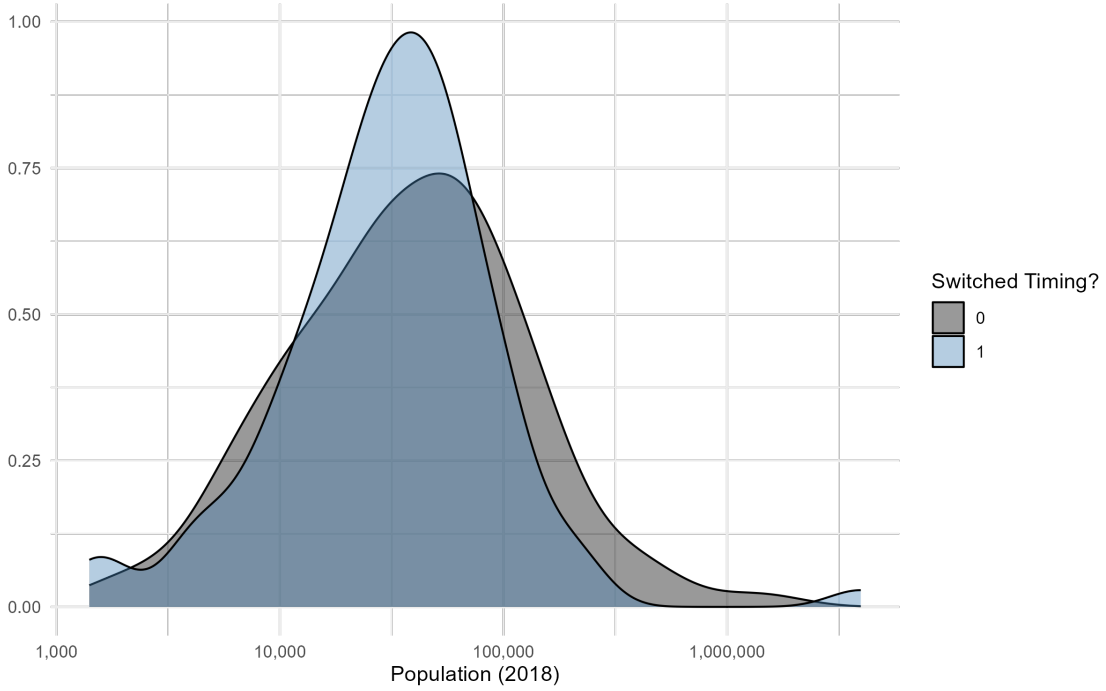


(b) After Matching

Figure A10: Distribution of Pct. White (2018) before and after matching.

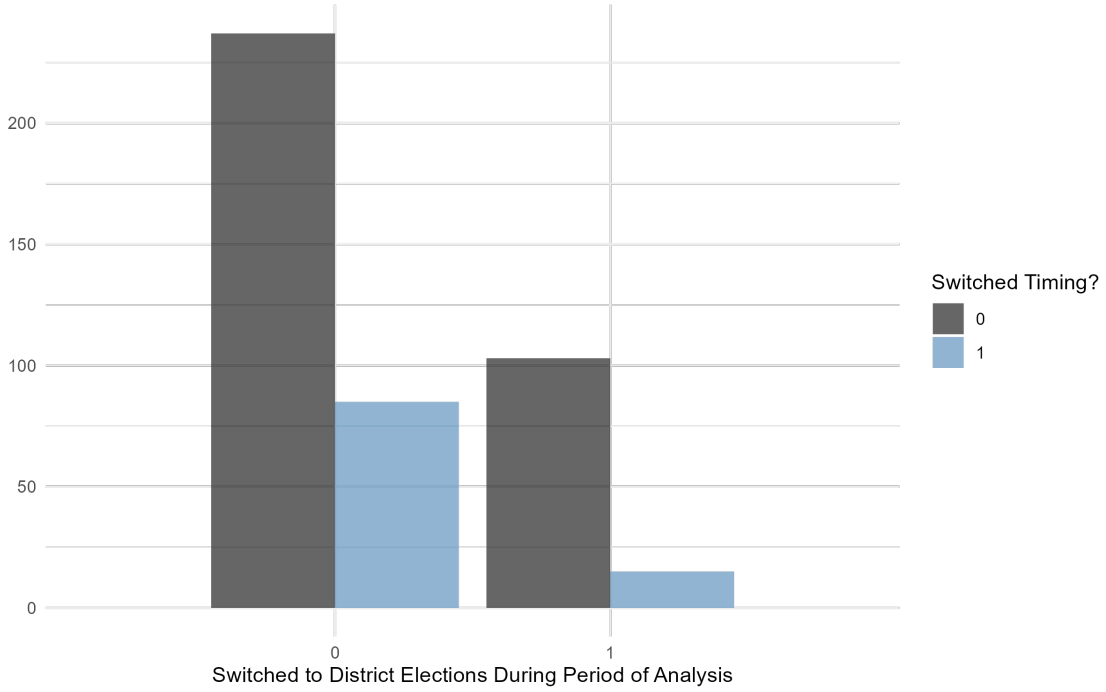


(a) Before Matching

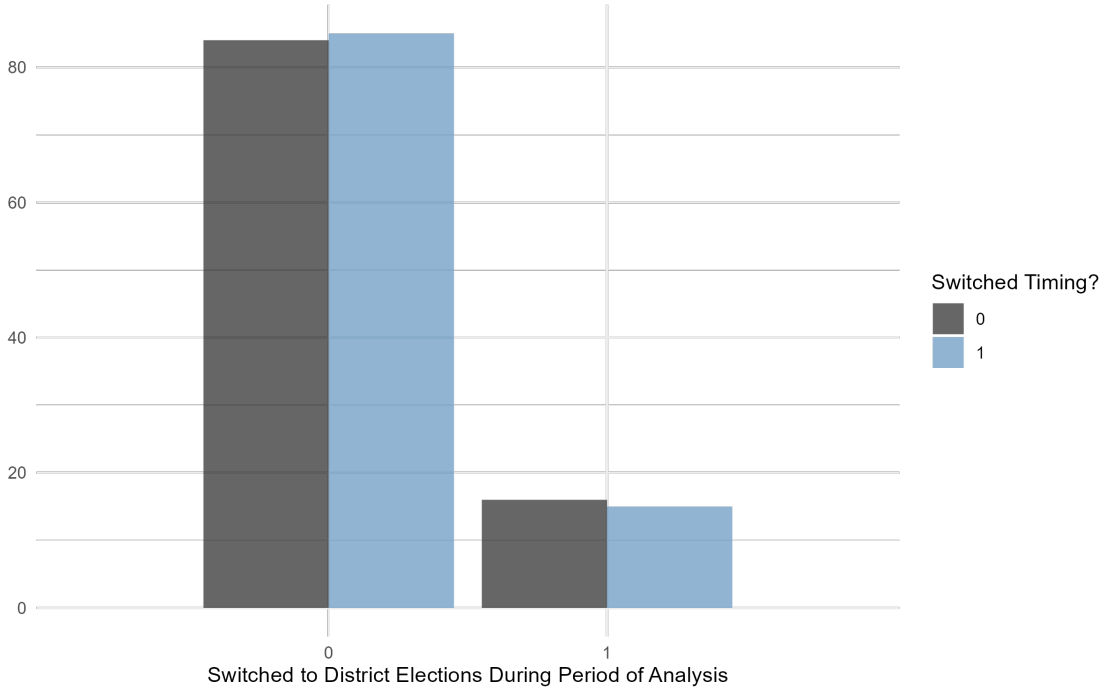


(b) After Matching

Figure A11: Distribution of Population (2018) before and after matching.



(a) Before Matching



(b) After Matching

Figure A12: Share of cities that switched from at-large to district elections during the period of study, before and after matching.

Figure A13: Map of cities in the full sample by treatment group

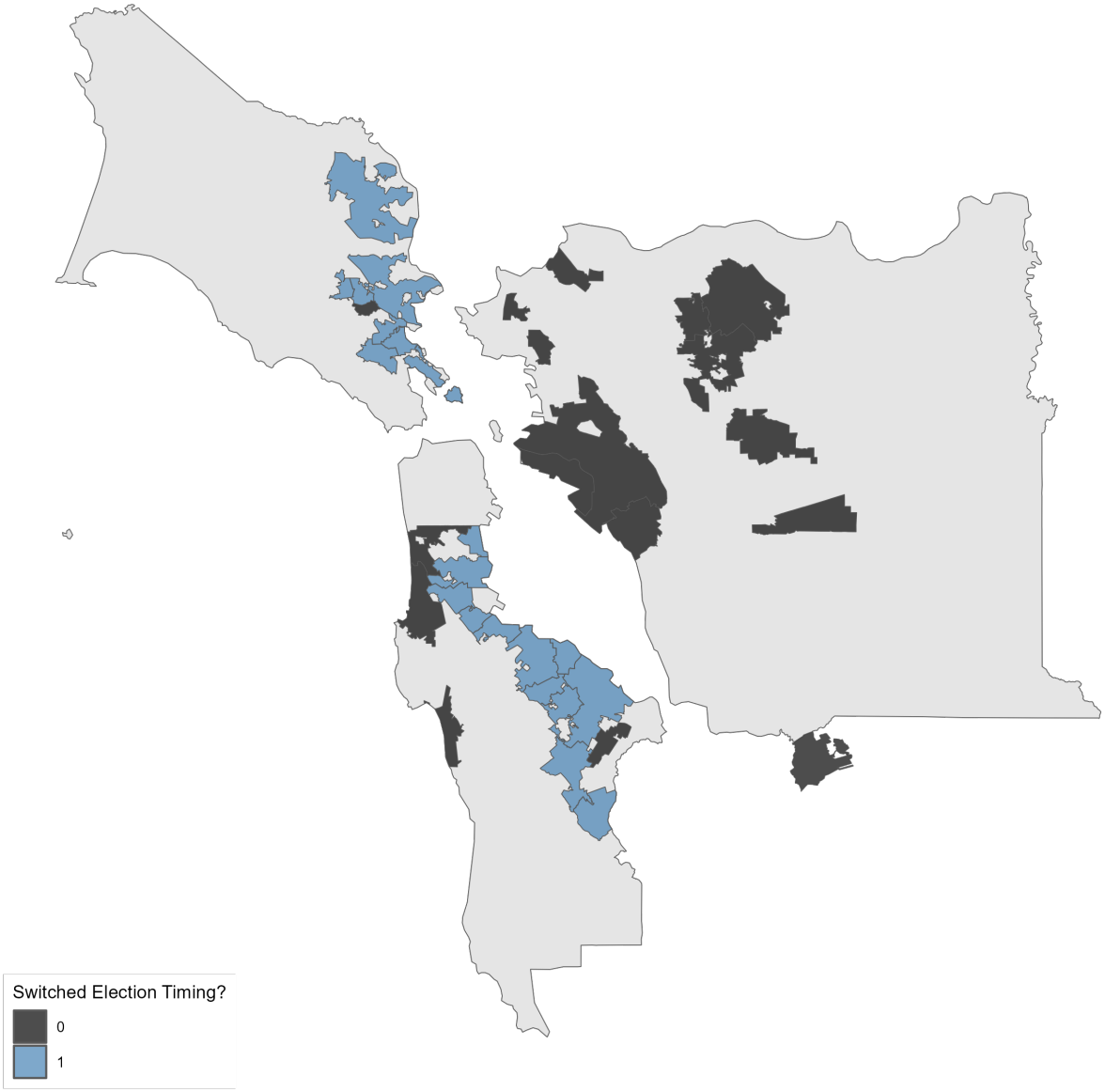


Figure A14: Map of cities in the matched sample, San Francisco MSA

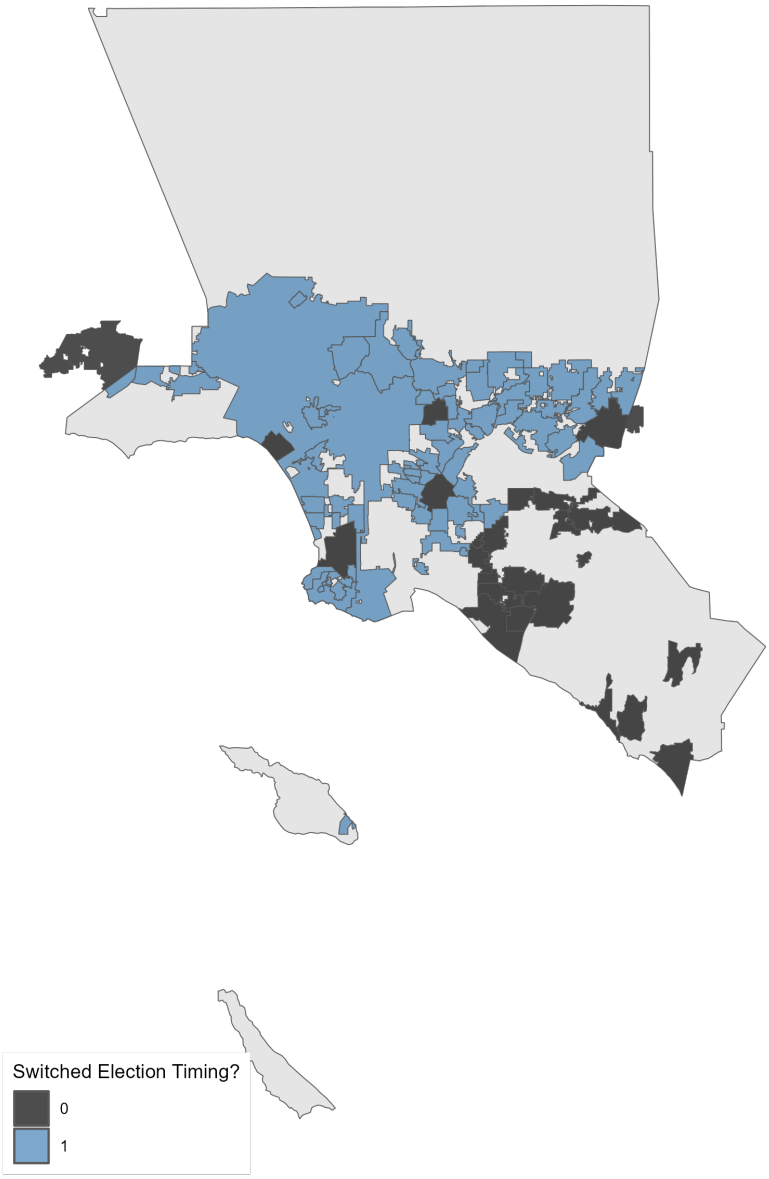


Figure A15: Map of cities in the matched sample, Los Angeles MSA

B Classifying Occupations

I classify each candidate's occupation as listed in their ballot designation using the following few-shot prompt to ChatGPT (Ornstein, Blasingame and Truscott, 2022). The opening paragraph of the prompt is drawn from Carnes and Lupu (2016) footnote 1, and classifies occupations as either "Working Class", "Not Working Class", "Public Official", or "NA".

```
User:  Classify each occupation below as ‘‘Working Class’’ or ‘‘Not Working Class’’.
A person’s occupation is working class if he or she is employed in manual labor
jobs (e.g., factory worker), service industry jobs (e.g., restaurant server),
clerical jobs (e.g., receptionist), or union jobs (e.g., field organizer).
If what is listed is not an occupation, return ‘‘NA’’. If what is listed is a
public official, return ‘‘Public Official’’.

User:  Architect
Assistant:  Not Working Class

User:  Rental Service Technician
Assistant:  Working Class

User:  Risk Analyst
Assistant:  Not Working Class

User:  Homeowner
Assistant:  NA

User:  Parcel Delivery Driver
Assistant:  Working Class

User:  Retired Welder
Assistant:  Working Class
```

```
User:  Incumbent
Assistant:  Public Official

User:  Business Owner
Assistant:  Not Working Class

User:  Custom Cabinet Builder
Assistant:  Working Class

User:  [occupation to be classified]
```

To validate the classifier, I randomly selected 200 occupations classified by the LLM and independently hand-coded them according to the instructions in the prompt. Intercoder reliability was quite good—out of that random sample, there were only 9 occupations (4.5% of the total) where the LLM and author disagreed on whether the occupation should be labeled as Working Class. Below is the list of those nine occupations, bolded if the LLM classified them as Working Class but the author did not and unbolded otherwise.

- **Community Health Worker**
- Labor Representative/Board member
- **Truss Designer**
- Retired Administrative Secretary
- Peace Officer
- **Mine Operations**
- **Community Housing Organizer**
- **Radiation Protection Technician**
- **Chef/Father**