Municipal Election Timing and the Politics of Urban Growth

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Abstract

In this paper, I show that the timing of city council elections plays an important role in shaping municipal land use policy. Cities that hold their elections off-cycle (on a date separate from high-profile national elections) tend to place more restrictions on new housing development. This stems from an asymmetry in the costs and benefits of urban growth: the benefits of growth are broadly shared, but the costs are concentrated. As a result, citizens that oppose new growth are likely to form a larger share of the electorate in municipal-specific elections. Using an extensive dataset on local election timing from California, I demonstrate that that cities with off-cycle elections issue fewer building permits and have higher home prices than comparable cities with on-cycle elections. This finding holds both in a cross-sectional matching analysis and a difference-in-difference analysis of cities that shifted their election timing.

The most recent version of this paper is available at:
`https://sites.lsa.umich.edu/ornstein/election-timing/`

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1 Introduction

In May 2013, the city council of Ann Arbor, Michigan met to discuss the construction of a new high-rise apartment building in the downtown core. Residents packed the council chamber for two hours of debate, voicing concerns that the 150-foot tall building would overshadow the neighborhood's nearby historic homes. At the end of deliberations, the council narrowly approved the construction, by a 6-5 margin.

"Audience members jeered and literally hissed at council members." reported the *Ann Arbor News* (Stanton 2013), storming out to shouts of "Shame on you!" and "Disgusting!"

Land use policy is among the most contentious issues in local politics, and municipal governments wield considerable power in determining the rate of population growth within their jurisdictions. But I mention this particular episode to highlight a curious pattern that emerged from the city council vote. At the time, Ann Arbor held its city council elections every year, electing half of the council in odd-numbered years, and half in even-numbered years. When the dust settled, the vote on the new apartment building split the council nearly perfectly by election timing. Of the councilmembers elected in even years, all but one voted to approve the construction. Of those elected in odd years, all but one voted to reject it.[1]

In this paper, I argue that the pattern we observe here is not mere coincidence, and that the timing of municipal elections has significant, observable consequences for land use policy and the growth of cities. When elections are held off-cycle (i.e. on a date separate from high profile elections like presidential or congressional races), citizens that oppose new housing development are more likely to turn out to vote than supporters. These citizens, in turn, elect councilmembers that are more willing to use municipal zoning authority to limit urban growth.

[1] Several months later, the lone odd-year city councilmember who voted to approve construction was up for re-election. She was soundly defeated, by nearly 30 percentage points.

Although it may seem like a purely local issue, municipal land use policy has an profound impact on the broader economy. The most tightly regulated US cities tend to have higher rents than we would expect from construction costs and wages alone (Glaeser & Gyourko 2003, Quigley & Raphael 2005). In turn, these excess housing slow economic growth by pricing workers out of cities where they would be most productive. One estimate suggests that easing housing restrictions in the three most productive US cities alone would increase aggregate GDP by roughly 9.5% (Hsieh & Moretti 2015).

In addition, by pricing poorer households out of more affluent areas, restrictive land use policies exacerbate residential segregation, both by race (Rothwell & Massey 2009) and by income (Rothwell & Massey 2010). Such segregation has been shown to affect civic participation (Oliver 1999), public goods provision (Trounstine 2015), and even life expectancy (Chetty et al. 2016). Restrictions on new residential construction are also largely responsible for the recent decline in regional income convergence (Ganong & Shoag 2017), as Americans from poor regions are less able to move to opportunity in growing metropolitan areas. Finally, density restrictions in central cities promote suburban sprawl, which increases both commuting costs and carbon emissions (Glaeser & Kahn 2010).

Given these tremendous costs, why do citizens that oppose population growth so often get their way in municipal politics, at the expense of citizens that would benefit from new housing construction? This fact is particularly puzzling in light of much of the foundational scholarship in American urban politics. Molotch (1976) famously describes the city as a "growth machine", a political entity whose principal aim is to promote business interests through population growth. Peterson (1981) makes a similar argument: because labor and capital are mobile across municipal boundaries, city governments are poorly suited to enact redistributive policy, and are instead most likely to pursue developmental policies that grow their property tax base. And yet, in the late 20th and early 21st centuries, many city governments have abandoned this growth machine model, and have instead severely curtailed new housing development through stringent zoning regulations.

I argue that off-cycle election timing provides one explanation for the stringency of municipal land use regulation. Citizens that oppose new residential development are likely to be overrepresented in off-cycle, municipal-specific elections for three reasons. First, **homeowners** are more likely to show up to municipal-specific elections than renters, and homeowners tend to view new development more skeptically. Second, the electorate in off-cycle elections differs **demographically** from on-cycle electorates. And finally, the **concentrated costs** of new housing development suggest that opponents of growth will be more highly motivated to turn out to municipal elections than the beneficiaries, and will form a larger share of the electorate in low-turnout, off-cycle elections. I will expand on these points in Section 3.

To test this theory empirically, I employ an extensive dataset on municipal elections from California over the past twenty years. In both OLS and matching analysis, I show that cities where elections are held off-cycle issue fewer new building permits and have significantly higher median home values than comparable cities with on-cycle elections. Because this cross-sectional analysis may not eliminate all city-specific unobserved confounders, I also conduct a difference-in-difference analysis. The pattern holds across time as well; cities that switched to on-cycle elections subsequently issued more new building permits and saw slower home price growth between 2002 and 2016 than comparable cities that kept their elections off-cycle.

Figures 2 and 1 preview this empirical analysis. In each panel, I plot the trajectory of cumulative new building permits issued and median home value per sqft for a set of California cities with off-cycle city council elections. This is paired with equivalent trajectories for a set of California cities with on-cycle elections, matched on demographic characteristics, median income, climate, developable land area, local amenities, and population in the initial time period. I will discuss the details of how I construct this matched control group in Section 7. For now, note that the off-cycle cities tended to issue fewer new building permits throughout the period, especially during the pre-Recession housing boom. And by the present day, median home prices in these cities were substantially higher, on average $75 per square foot.

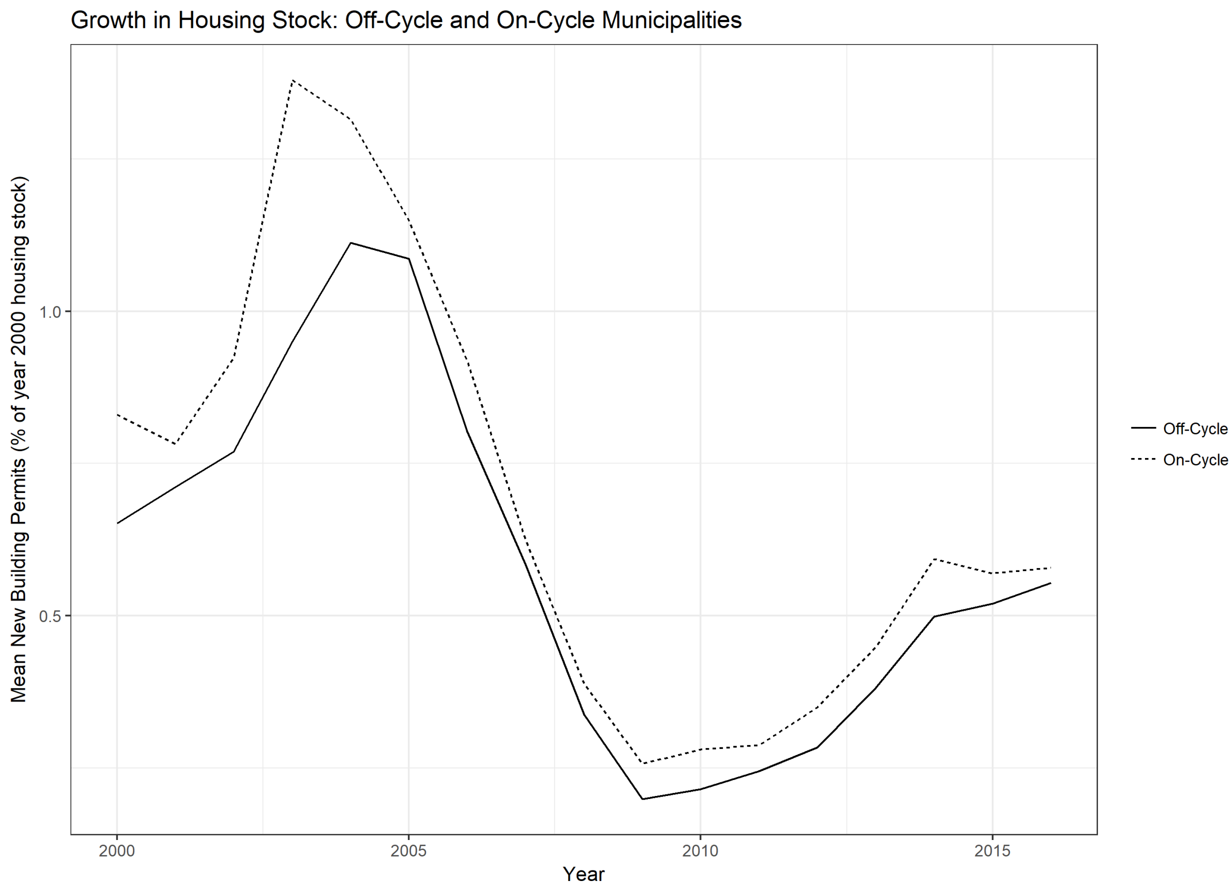

Figure 1: Mean new building permits issued per year, comparing cities with mostly on-cycle elections against those with mostly off-cycle elections, matching on demography, median income, public amenities, and population in the year 2000.

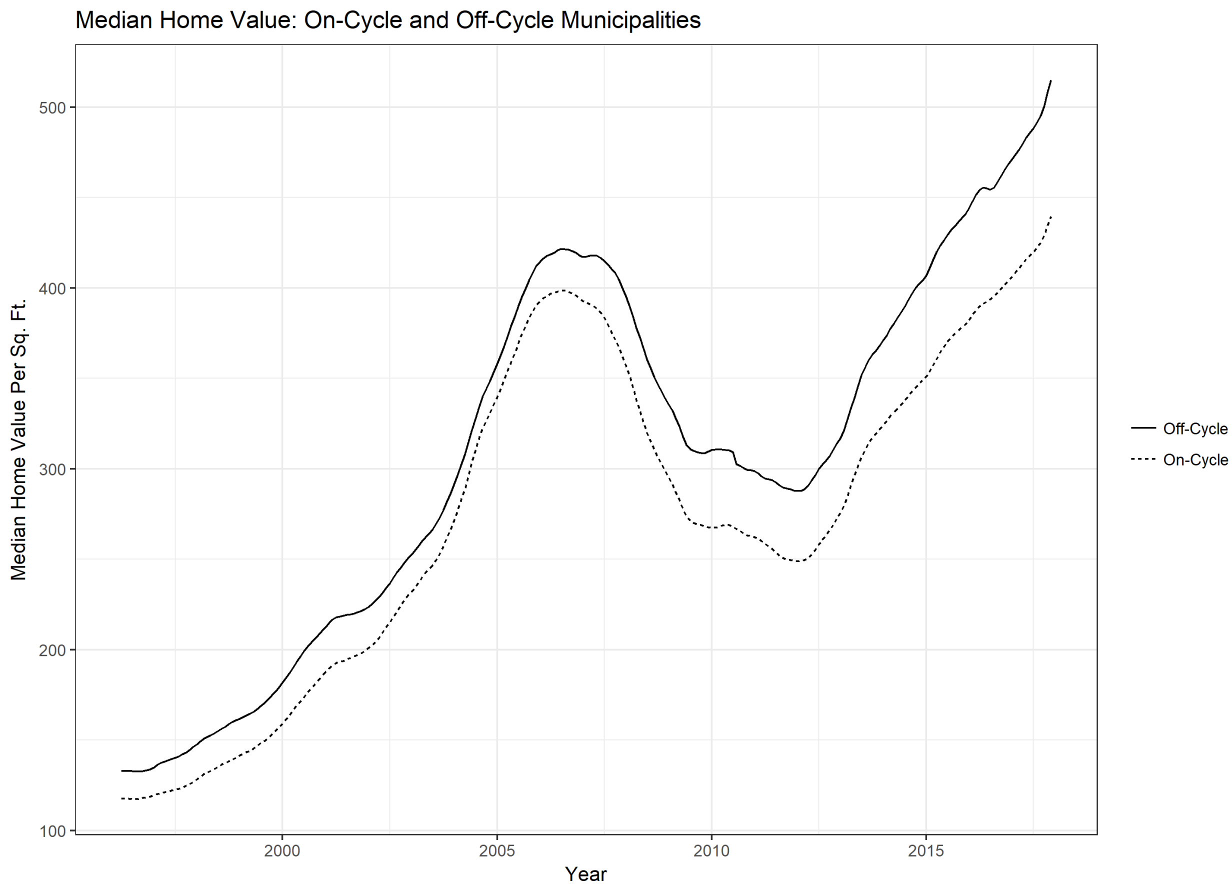

Figure 2: Trajectory of median home value per sqft, comparing cities with mostly on-cycle elections against those with mostly off-cycle elections, matching on demography, median income, public amenities, and population in the year 2000.

The paper proceeds as follows. In the next section, I briefly sketch the history of municipal zoning in the United States, and discuss the role that city councils play in its implementation. Following that, I review the literature on election timing, and discuss why groups that oppose new residential development are likely to be overrepresented in off-cycle elections. Section four introduces a brief case study on how election timing influenced the politics of land use in Palo Alto, California. In section five, I show that ballot initiatives restricting new infill housing development receive more support when they appear on off-cycle ballots. Section six describes my dataset on land use policy outcomes, and section seven discusses the results of my empirical analysis. Section eight concludes.

2 Background: Municipal Zoning

New York City adopted the first comprehensive zoning code in 1916. Responding to fears that skyscrapers would shroud the island of Manhattan in perpetual shadow – and diminish the value of property on Fifth Avenue – city planners drew up a map of the city divided into zones. Within each zone, the city designated maximum building heights and permitted land uses (Fischel 2015). Despite early objections that municipal zoning violated the Fifth Amendment's prohibition on seizure of private property without due process, the Supreme Court ultimately upheld the constitutionality of these ordinances in 1926's *Ambler Realty v. Village of Euclid* (Wolf 2008). Since that time, municipal governments have been granted broad discretion to regulate land use within their borders. Today, urban land use policy is determined by a patchwork of over 19,000 municipalities, comprising tens of thousands of local legislators, zoning board members, and city planners.

These regulations take many forms. The most common is to specify permitted land use for each parcel (e.g. residential, commercial, industrial). This type of zoning ("Euclidean") is intended to separate some activities from others – e.g. keeping industrial pollutants away from shopping areas, or prohibiting commercial uses from sprouting up in quiet residential neighborhoods.

In addition to regulating the *type* of land use, zoning also typically regulates the *intensity* of land use. For example, zoning ordinances will often specify a maximum residential density that is allowed within each zone. Other ordinances might mandate a percentage of every lot area that must be dedicated to open space, or a minimum distance that buildings must be set back from the street. Another popular restriction is the maximum floor area ratio (FAR), which limits the total floor area of buildings relative to the size of the lot on which they sit. In practice, these regulations all but ensure that large swaths of US cities are set aside for single-family homes, even when a more intensive land use (townhouses, apartment buildings) would be more appropriate given demand.

Other land use ordinances that are seemingly unrelated to housing can nevertheless limit the number of housing units built in a city. Take, for instance, the near-ubiquitous requirement that developers set aside parking for each new building they construct. Even in cities without formal zoning codes, these requirements can be onerous; the city of Houston mandates that for each studio apartment, developers must set aside 1.25 parking spaces (Lewyn 2005)! Not only does all that mandated parking take up real estate that could be used for housing, but abundant, inexpensive parking further incentivizes urban sprawl, by reducing the cost of automobile commutes (Shoup 1999).

Over time these regulations have accumulated in such a way that building new, affordable housing has become prohibitive in many metropolitan areas. In the century since New York City's zoning code was first implemented, the length of the text has ballooned from 14 pages to 4,126 pages. It has been estimated that roughly 40% of Manhattan's housing stock would be illegal to build today (Bui et al. 2016).[2]

How is municipal land use policy determined? In practice, much of the regulatory authority lies with the elected city council. In nearly every US municipality, the city council

[2]Although New York City as a whole is twice as populous today as it was in 1910, the population of Manhattan itself peaked in the 1910 Census, just before the introduction of zoning.

is responsible for adopting and amending the city's comprehensive plan. Of 2,729 municipalities surveyed by the Wharton Residential Land Use Regulation Survey (Gyourko et al. 2008), 94% reported that rezoning decisions require a majority (or supermajority) vote in city council. In addition, 70% of municipalities surveyed require planning commission approval for any new building. These committees tend to be appointed rather than elected (there are no instances in my dataset of an elected zoning board or planning commission member), so any group looking to influence the composition of those committees would have to do so through mayoral or city council elections.

Who shows up to these elections depends in part on when they are held. We turn to this topic in Section 3.

3 Off-Cycle Elections Empower Slow-Growth Interests

Although "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 & Gersen 2010). The United States comprises 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 presidential, congressional, or gubernatorial elections.

The historical roots of this practice are deep. As Anzia (2012*a*) documents, several city governments experimented with 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 sticky. Today, roughly 80% of US municipalities continue to hold their elections off-cycle (Anzia 2012*a*).

The most prominent consequence of holding elections off-cycle is lower voter turnout.

Because voting entails a non-trivial time cost, citizens are more likely to vote when there are multiple concurrent elections on the ballot, particularly high-profile national elections like the presidency. Berry & Gersen (2010) document a 20 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*b*, Garmann 2016).

But this decrease in turnout is not uniform. Kogan et al. (2017) compile an extensive dataset drawn from voter files to examine the differences between on-cycle and off-cycle electorates. They find that the electorate in off-cycle elections is very different demographically from those that turnout to vote in presidential years. In particular, the off-cycle electorate is much older (roughly 10-20 percentage points more senior citizens than in presidential years).

Citizens that have a larger stake in local politics are more likely to show up to local-specific elections. For example, when school district elections are held off-cycle, members of teachers unions are more likely to turn out to vote than those with smaller stakes in school district policymaking. In such districts, there is a significant increase in the average teacher's salary (Anzia 2011, Berry & Gersen 2010). Similarly, because most special districts (e.g. water districts, library districts) hold their elections off-cycle, groups that benefit from the district's services are more likely to show up to vote than those that do not, resulting in higher levels of taxes and spending (Berry 2008).

In the two examples above, we see the classic Olsonian logic of collective action at work (Olson 1965). A small group receives concentrated benefits from additional government spending (e.g. teachers receive higher salaries; library patrons get better libraries). But the larger bulk of the population bears very small per capita costs from the necessary increase in taxes or debt. This produces an enthusiasm gap when it comes to turning out supporters (Anzia 2012*b*). The beneficiaries of additional spending are much more likely to organize and turn out their supporters than those that oppose it.

But how does all this relate to the politics of local land use? To complete my argument, I argue that restrictions on housing development generate a similar pattern of concentrated benefits and diffuse costs. As such, off-cycle elections produce a differential mobilization of three groups: homeowners, older voters, and neighbors of proposed new development. These three explanations are not mutually exclusive, and I suspect that each one explains part of the empirical relationship I present in Section 7.

3.1 The Homevoter Hypothesis

In his influential book, *The Homevoter Hypothesis*, Fischel (2001) describes how resident homeowners came to dominate American municipal politics during the late 20th century. Because their financial portfolio largely consists of a single, highly-leveraged, undiversified, immobile asset, homeowners develop a (wholly justified) concern for maintaining home values in their community. And municipal government policy is an important determinant of home values. Studies have repeatedly demonstrated that home prices respond to factors like local tax policy (Hamilton 1976), public school quality (Black 1999), transportation infrastructure (Hess & Almeida 2007), placement of public parks (Troy & Grove 2008), and crime risk (Linden & Rockoff 2008, Pope & Pope 2012).

But arguably it is zoning policy, by regulating the overall supply of housing, that exerts the most direct influence on home values. Homeowners tend to support greater restrictions on new construction than renters. Marble & Nall (2017) conduct a series of survey experiments to assess urban residents' views towards new housing development. In these surveys, homeowners consistently report stronger opposition to new housing construction than renters. This effect is stronger than that of any other demographic variable or experimental manipulation. Hankinson (2017) finds a similar result. Although there is some support for building restrictions among renters in gentrifying neighborhoods, homeowners consistently support these policies more strongly than renters.

All of this suggests that homeowners will be more likely than renters to turnout to municipal-specific elections, and vote for candidates that share their concern for maintaining home values and limiting new construction. Dipasquale & Glaeser (1999), for example, find that homeowners are 25 percentage points more likely to report voting in local elections. Einstein et al. (2017) find that homeowners are more than twice as likely to speak at local zoning board meetings than renters. In municipalities with such a large gap in political participation, municipal governments are likely to be more responsive to homeowners' concerns. But when municipal elections are held on-cycle, this turnout discrepancy may disappear, as renters turn out for the more high-profile elections.[3]

3.2 Voter Demographics

However, the Homevoter Hypothesis does not tell the entire story. In many suburban municipalities, homeowners make up a decisive majority of residents. Renters in these communities are not be a sufficiently large voting bloc to swing municipal elections, even when they show up. In such places, election timing can only influence outcomes if there are heterogeneous preferences *among homeowners*.

One possible source of this heterogeneity is age. In their overlapping-generations model on the political economy of urban growth, Ortalo-Magne & Prat (2014) identify age as an important determinant of zoning policy preferences. Older agents are more likely to oppose new construction because they have made greater investments in real estate over the course of their lives, and are less able to recoup a loss in the value of that capital.

As we've already mentioned, Kogan et al. (2017) find that off-cycle electorates are much older than on-cycle electorates on average. If older residents prefer slow growth, then this could be another channel through which election timing affects the incentives of city council-

[3] De Benedictis-Kessner (2017) documents an increase in mayoral incumbency advantage when municipal elections are held on-cycle, suggesting that on-cycle voters – drawn to the polls for other reasons – are less informed on average about municipal politics.

members. It remains to be seen whether this trend, identified during the years of a Democratic presidency, remains true during a Republican presidential administration. Nevertheless, this relationship holds true during the period I investigate in the empirical analysis (my dataset concludes in 2016).

3.3 Diffuse Benefits, Concentrated Costs

There is one final mechanism through which opponents of growth may be overrepresented in off-cycle elections: the asymmetry between the concentrated costs of new development and its more diffuse benefits. In the same manner that teachers are more likely to show up to school board elections – because they have more to gain – the neighbors of potential new development are more likely to show up to municipal elections – because they have more to lose.

New housing development imposes concentrated costs on nearby residents. A larger population can increase neighborhood traffic congestion and compete for scarce parking spaces. New residents crowd local public amenities like libraries, parks, or beaches. Tall apartment buildings block neighbors' sunlight and impede their views.

By comparison, the benefits that come from new housing are diffuse and uncertain. Building additional housing stock puts downward pressure on rents. Denser, walkable development in the urban core reduces average commute times (Wheaton 1998). Larger cities may benefit from economies of scale in administrative costs (Blom-Hansen et al. 2014). But each of these benefits accrue to the metropolitan area at large, and the marginal benefit that any individual voter reaps from a new housing development is minuscule. These diffuse benefits are unlikely to motivate citizens to turn out and vote in city council elections.

Einstein et al. (2017) compile a novel dataset of meeting minutes from local zoning board meetings in the Boston area. They find that the residents who attend these meetings were more likely to be older, male, and homeowners. And they overwhelmingly spoke out in opposition of new development (63% opposed compared to 15% in favor). The reasons cited for this opposition include a number of concentrated costs imposed on the neighborhood, including: traffic, environmental degradation, flooding, public safety, aesthetics, and parking. By matching these records to individual-level voter files, they also determine that the residents who comment at local zoning board meetings are also more likely to turn out to local elections.

Taken together, these three mechanisms suggest that off-cycle electorates will be, on average, more skeptical of new housing development, and are likely to elect city councilmembers that share this skepticism. Before turning to more systematic empirical evidence on this proposition, let us briefly discuss an illustrative case study.

4 Case Study: Palo Alto's Measure S

The city of Palo Alto, California lies in the heart of Silicon Valley. Over the past two decades, demand for housing in the area has caused home values to nearly quintuple. The question of how to create affordable housing – and whether to permit large amounts of new supply – is a very salient issue in local politics.

It was amidst this controversy that, in November 2010, the residents of Palo Alto passed Measure S, a referendum shifting the city's elections on-cycle. Although Palo Alto is an outlier in terms of home prices, its experience with the change in election timing offers an instructive case study into the political dynamics described in the last section. Prior to 2010, Palo Alto city council members were elected during odd-numbered years. But following the referendum's passage, city council elections were moved to coincide with national elections on even-numbered years. Proponents of the change argued that it would boost voter turnout and decrease the cost of administering municipal elections.

The first claim was certainly proven true. As Figure 3 (panel A) illustrates, on average

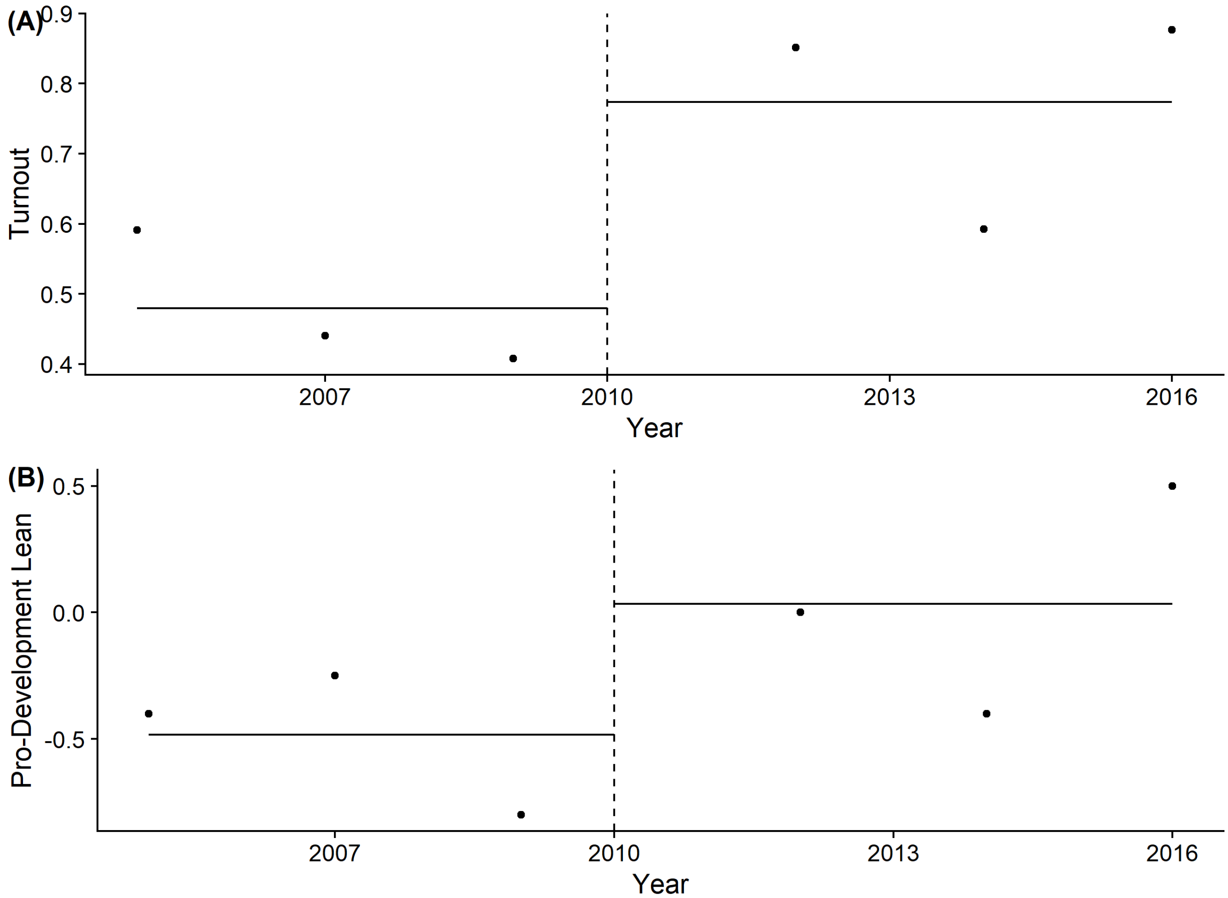

Figure 3: Following the switch to on-cycle, Palo Alto city council elections saw much higher turnout (A), and more pro-development city councilmembers were elected (B). Solid lines denote averages before and after the passage of Measure S (dotted line).

47% of registered voters turned out to vote for city council in the three elections prior to Measure S. Afterwards, turnout increased dramatically. About 85% of registered voters turned out in 2012 and 2016, and 60% turned out during the congressional midterm in 2014.[4]

But did the *composition* of the electorate change, to the advantage of pro-development candidates? To explore this question, I consulted the archives of the local newspaper (The Palo Alto Observer), which has conducted interviews with each candidate for city council going back to 2005. Because housing policy is such a prominent issue, the candidates have typically been asked to state their opinion on local zoning and housing development policies. For every candidate between 2005 and 2016, I manually code whether each candidate's

[4]Santa Clara Registrar of Voters: https://www.sccgov.org/sites/rov/resources/pages/pasteresults.aspx

platform is pro-development (+1), slow-growth (−1) or unclear (0). Pro-development candidates express willingness to relax height restrictions, deregulate accessory dwelling units, lower density requirements, and build new housing near transit corridors. Slow-growth candidates emphasize maintaining Palo Alto's character, express concerns about overcrowding in schools, etc.

How well did pro-development candidates perform in Palo Alto city council elections before and after the shift in election timing? Figure 3 (panel B) provides some suggestive evidence. Prior to Measure S, roughly 25% of the candidates elected to city council were pro-development. That fraction increased to 50% after the city shifted to on-cycle elections. The most dramatic result was in 2016, when a slate of candidates running on an explicitly pro-development platform won an unprecedented victory. Three out of the four elected councilmembers that year expressed pro-development opinions in their interviews.

Of course, this single case is far from conclusive. There are a number of reasons why more city councilmembers would have expressed pro-development sentiments toward the end of this period (the housing market collapse and its aftermath spring to mind). But it seems likely that the shifting election timing played some role in the election of these new development-minded candidates. To investigate this proposition in a more systematic fashion, we'll now turn to evidence from a comprehensive elections dataset in California cities, and explore how election timing affects popular support for pro-development ballot initiatives, as well as observable land use policy outcomes, including permitting and median home prices.

5 Ballot Initiatives

Over the past two decades, California has stood out among US states for its unique reliance on the ballot initiative to shape land use policy. Slow-growth citizen groups frequently resort to direct democracy to constrain the ability of city councils to permit new development

(Gerber & Phillips 2004). There are several popular tools in this arsenal. One is the Urban Growth Boundary (UGB), a requirement that all new residential development take place within a specified boundary, beyond which the municipality will not extend city services (Gerber 2005). As of writing, at least 85 municipalities in California have adopted some form of UGB via ballot measure. Another tool is the initiative requirement, a rule that prohibits certain types of development (particularly multifamily housing) unless expressly approved by ballot initiative. Finally, California voters will often use ballot measures to directly shape the city's zoning code: imposing restrictions on building heights, setbacks, parking requirements, environmental review, traffic impacts, etc.

As a result, there is now a large set of data on how voters react when asked to weigh in on municipal land use decisions. In this section, I investigate whether the timing of those elections affected the electorate's willingness to permit new development. To do so, I employ the California Election Data Archive, an extensive database of every election held in the state of California since 1996.[5] For each ballot measure, the CEDA database includes the municipality, election date, ballot question, and number of voters that voted for and against the measure. Using the text of the ballot question, I manually code whether the measure restricted or approved new residential development, removing initiatives that did not pertain to land use, or only applied to nonresidential development. I also categorize each measure based on the type of housing development (Infill or Greenfield), and the type of restriction (UGB, initiative requirement, height restriction, etc.).

Before I proceed with the analysis, two caveats are in order. First, it is important to note that the timing of ballot initiatives is endogenous. When deciding to place an initiative on the ballot, citizen groups deliberately attempt to do so during a time when it is most likely to attract supporters.[6] This selection bias should attenuate the observed effect of election timing on pro-development outcomes.

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

[6] For example, 80% of the initiatives proposing UGBs are placed *on-cycle*, and on average, 62% of the electorate votes in favor. Curbing sprawl, it seems, is quite popular among Californians at large.

Second, bear in mind that the existence of popular initiatives on land use is *itself* a development control. Municipalities that require new development to face the voters before it can go forward are placing an additional (ornery) veto player into the permitting process. As such, the types of housing development that are proposed tend to be significantly watered down, and likely to come paired with developer-funded public goods Gerber (2005). For example, many of the ballot initiatives in the CEDA dataset allow new housing, but on the condition that a portion of the land area be preserved as permanent open space. I code these initiatives as "pro-housing" because they expand the housing stock relative to current law, but that is a coding decision upon which reasonable people may disagree.

Using this coding scheme, I identify 59 initiatives that were placed on the ballot to approve or prohibit new infill development, and 157 initiatives pertaining to greenfield development on the urban fringe (this includes UGBs and open space requirements). 74 initiatives did not obviously fall into either category (e.g. annual permit caps). As Table 1 reports, initiatives to block urban sprawl are highly popular in California. Of the ballot measures analyzed, the pro-housing share averaged 40% for greenfield measures, regardless of election timing. Initiatives to permit new infill development were significantly more popular, but their success depended on election timing. Figure 4 illustrates the vote share garnered by the pro-housing side of these initiatives, broken down by election timing. Among infill development initiatives, the pro-housing side received roughly 7 percentage points more support when the election was held on-cycle (corresponding to a 14pp swing). This effect holds even when controlling for city-level characteristics and metropolitan area fixed effects in an OLS regression (Table 1). However, among initiatives relating to greenfield development and urban sprawl, election timing does not appear to affect support for development.

All of this tentatively suggests that off-cycle voters are less likely to support new development that intensifies land use within existing neighborhoods. In the land-constrained cities on the California coast, where any new housing development is necessarily infill development, this eliminates the potential for new housing entirely. In the next section, we will discuss

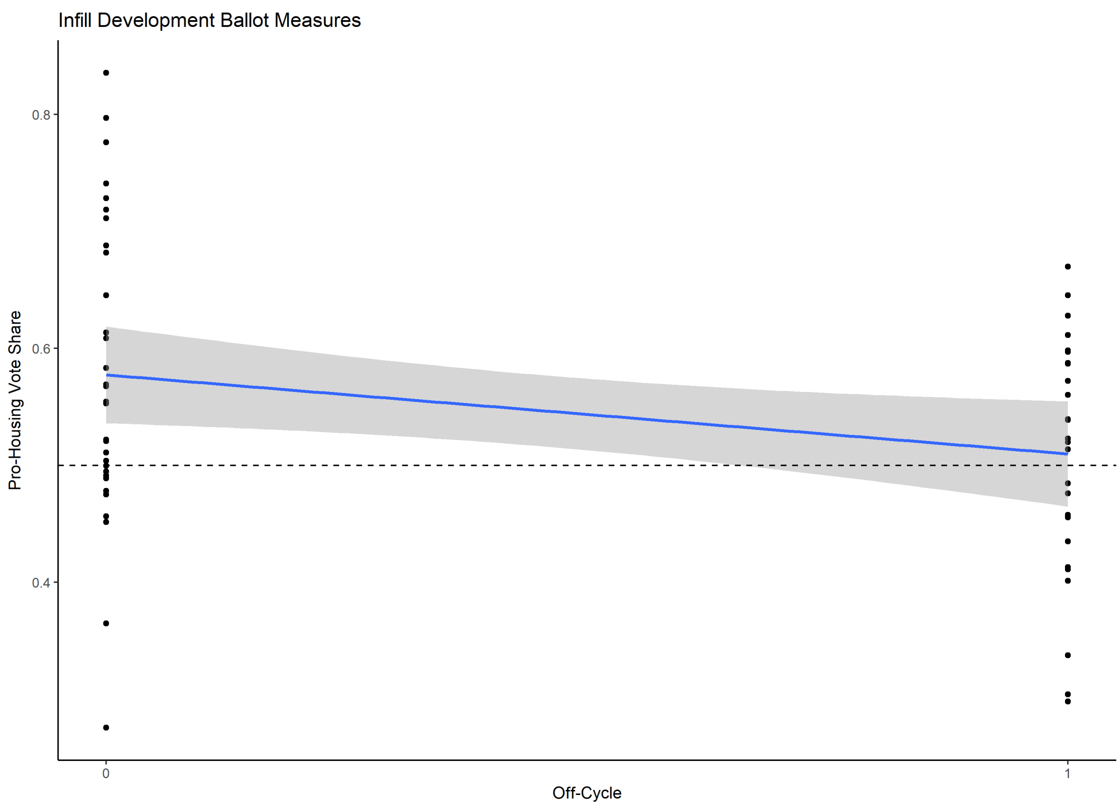

Figure 4: New infill development attracts roughly 7-8pp less support when the ballot initiative is held off-cycle.

the effects this has on observable land use policy outcomes, including new building permits and median home prices.

6 Data

Owing to its extensive records on municipal election timing going back two decades, the empirical evidence in this paper comes entirely from the state of California. So it is worth noting the ways in which California cities differ from their counterparts in the rest of the United States. First, California has experienced consistent, rapid population growth throughout its history as a state. Since 1840, there has not been a single decade during which its population grew by less than 10%.[7] This is significant, because it has required a continual expansion of the housing supply to accommodate new migrants. This trend has largely been reflected at the city level as well. Unlike other areas of the country, where cities have experienced protracted population decline, 78% of California's cities are currently at their population peak, and only six cities are below 90% of their population peak (author's calculations). As a result, there is no overhang of housing supply in shrinking cities to drive down home prices (Glaeser & Gyourko 2005). In nearly every city, new construction is required to keep up with expanding demand.

Second, California has a unique situation regarding local public finance, owing to a 1976 measure called Proposition 13. Passed by referendum as part of the broader "tax revolt", Prop 13 places strict limits on municipal governments' ability to raise property taxes. All property tax rates are statutorally capped at 1% of assessed property value, and assessments can only increase at a maximum of 2% per year. As a result, the effective tax rate paid in high-demand real estate markets is substantially below 1% (Ferreira 2010). The effect that Proposition 13 has on homeowner behavior is well-researched: people are simply less likely to move. Because purchasing a new home results in a reassessment by the local government,

[7]https://www.census.gov/population/www/censusdata/files/table-16.pdf

many residents are "locked-in" to their homes, paying favorable property tax rates (Ferreira 2010). There is less scholarly agreement, however, on how Prop 13 affects municipal land use policies. Some scholars suggest that Prop 13 makes new residential development less attractive, because their property taxes will be insufficient to pay the cost of new public services (Quigley & Rosenthal 2005). However, because new housing is assessed at market value rather the statutorially constrained assessments of older housing stock, this could *increase* the incentive to build new housing, particularly in areas that have undergone rapid home price growth.

Finally, California consists of two very distinct regions. The coastal cities are land-constrained, wealthy, liberal, and most have recovered easily from the housing price collapse in 2007. The inland and north coast cities are more land-abundant, conservative, and have had greater difficulty recovering from the Great Recession. In the empirical analysis, I conduct a matching analysis to ensure that we are comparing cities within, rather than across, these regions.

6.1 The Election Timing Variable

To generate my measure of municipal election timing, I refer once again to the California Election Data Archive. Subsetting the data so that I only consider elections for mayor and city council (or the equivalent legislative body, like County Supervisor in San Francisco)[8], I then determine whether each election was held on November during an even-numbered year: if yes, I code it on-cycle, if no, off-cycle.

Once that step is complete, I compute for each municipality the fraction of elections between 1996 and 2016 that were held off-cycle. This measure, `pct.off.cycle`, is my primary independent variable. The measure reveals a substantial amount of heterogeneity in election timing. 25% of the cities in my sample held all of their elections off-cycle during this

[8] I include mayoral elections in the measure as well, because mayors typically vote on the city council and appoint members to municipal zoning and land use committees.

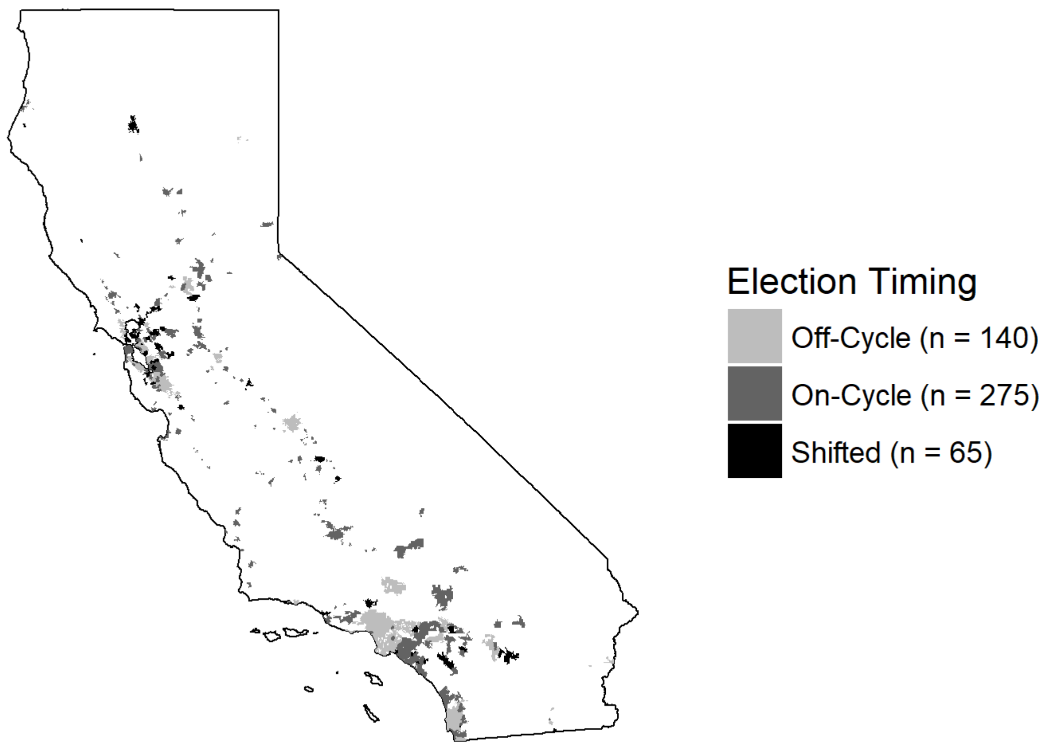

Figure 5: Map of municipalities in the dataset. Shading denotes whether the majority of municipal elections (1996-2016) were off-cycle or on-cycle.

period, while 41% held their elections on-cycle. Roughly 13% of cities switched the timing of their elections during the survey period, a fact that will prove useful for the difference-in-difference analysis (Section 7.3). Figure 5 maps the cities in my dataset, shaded by election timing.

6.2 Dependent Variables

In my empirical analyses, I employ three outcome variables. The first is a direct measure of regulatory stringency, the number of new building permits issued each year by the municipal government. These data come from the Census Bureau's Building Permits Survey, conducted annually since 1980. The other two outcome variables are measures of median home prices.

Although not a direct measure of land use regulation, prices provide a useful proxy for the elasticity of housing supply in an area, after accounting for demand-side factors like median income and urban amenities.[9] In all of the following analyses, I use a measure of median sale price per square foot from the real-estate website Zillow.[10]

6.3 Developable Land

Municipalities with an abundance of nearby developable land are likely to have an easier time expanding their housing supply than land-constrained cities, because it merely requires building out, rather than building up (Saiz 2010). To account for this potential confounder, I generate a measure of nearby developable land for each municipality in my dataset. This entails a three-step process. First, I use the National Land Cover Dataset (NLCD) to identify the parcels of land within a 20km radius of the city center that are undeveloped. 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 USGS Digital Elevation Model (90 sq. meter grid cells).[11] Finally, I compute the fraction of land within 20km of the city center that matches these criteria (undeveloped, not-too-steep, and not wetlands). The result is my `percent.developable` variable.

6.4 Other Covariates

From the American Community Survey I collect covariate data on population, median income, educational attainment, and demographic composition for every city in California with

[9] See Saiz (2010) for a more thorough explanation on how supply elasticity affects home price levels, and Glaeser et al. (2005) for an example of an empirical analysis using home prices relative to construction costs to infer the stringency of land use regulation.

[10] https://www.zillow.com/research/data/

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

a population greater than 10,000.

Because many municipalities cite cost savings as a motivation for changing their election timing, omitting data on local fiscal conditions may bias my estimates. Cities with large per capita debt burdens may be more likely to switch to on-cycle elections, and also to pursue tax-base enhancing real estate developments. To account for this possibility, I collect data on outstanding debt per capita, expenditures per capita, and taxes per capita from the US Census of Governments.[12]

I also employ a measure of city-level ideology developed by Tausanovitch & Warshaw (2014) using multilevel regression and poststratification. If liberal cities – in an effort to turn out Democratic voters – are more likely to hold their elections on-cycle, and liberal cities also have more restrictive zoning policies – as Kahn (2011) documents in California – then omitting local-level ideology could bias my estimates. Note that this estimate is only available for cities with population greater than 20,000.

Hedonic models of urban quality of life (e.g. Roback (1982)) suggest that amenities like pleasant climate are likely to affect median home values. So I also compute average January and July temperatures for each municipality from the high-resolution WorldClim dataset (Hijmans et al. 2005).

Home prices are also sensitive to the quality of local public goods. In particular, the performance of nearby public schools is strongly capitalized into property values, as border discontinuity studies reveal (Black 1999). A review of the literature suggests that one standard deviation increase in test scores is associated with home prices that are four percent higher (Nguyen-Hoang & Yinger 2011). To account for this effect, I include school district-level data on the Academic Performance Index, a measure computed annually by the California Department of Education to track school district performance and hold local officials accountable. Payson (2017) documents the importance of this measure in local

[12] Available at http://www2.census.gov/pub/outgoing/govs/special60/. The filename is "IndFin1967-2012.zip".

school board elections; see that paper for a more detailed description of the measure. For each city in my dataset, I assign an API score based on the school district with the most territorial overlap.[13]

7 Results

My empirical analysis proceeds in three parts. First, I estimate the relationship between off-cycle elections, home prices, and building permits using cross-sectional OLS. As predicted, off-cycle elections are associated with higher home values and fewer new building permits. Second, I perform a matching analysis, comparing cities with off-cycle elections against a matched set of cities that hold their elections on-cycle. This analysis yields a similar result. Finally, to hold unobserved city effects constant, I restrict my focus to those cities that switched their election timing between 1996 and 2016. This difference-in-difference analysis is consistent with the cross-sectional results: cities that switched to on-cycle elections had slower growth in home prices and issued roughly three times as many building permits as those that did not.

7.1 Cross-Sectional Correlations: OLS

To begin, I estimate the a series of linear regression models of the following form:

$$Y_i = \beta_1 T_i + \beta_2 X_i + \varepsilon_i$$

where Y_i is either a measure of median home prices in 2014 or the logarithm of new units permitted by city i between 2010 and 2016. The variable T_i is the percentage of elections in city i held off-cycle between 1996 and 2016, X_i is a matrix of city-level covariates, and ε_i is

[13]Where multiple school districts overlap with a municipality, I assign the API scores for the unified school district, and use scores from secondary or elementary districts only if there is no unified school district. Data files available at `https://www.cde.ca.gov/ta/ac/ap/apidatafiles.asp`.

an iid error term.

As reported in Tables 2 and 3, the estimated relationship between off-cycle election timing and building permits is negative across all specifications of the model. The magnitude of the effect is striking: the estimate reported in Column (4) suggests that off-cycle cities issued just half as many building permits between 2010 and 2016 as comparable cities with on-cycle elections. A similar pattern shows up in the median home price regressions (Table 4). Median home prices are roughly $61 higher per square foot in cities with off-cycle elections.

7.2 Matching Analysis

To complement the OLS estimation above, I also conduct a matching analysis (Rubin 1973). This estimation strategy compares treated observations (cities with off-cycle elections) to a matched sample of control observations (cities that hold elections on-cycle). The objective of the matching algorithm is to ensure that both samples, while differing on treatment condition, are on average balanced across potential confounding variables. I define the "treatment" group as those cities with a majority of city council elections between 1996 and 2016 held off-cycle, and all other cities as the control group. Dichotomizing the treatment in this manner is not terribly problematic, since most cities in my sample hold either 100% or 0% of their elections off-cycle. As before, I include as covariates each city's median income, population, nearby developable land, per capita debt burden, and the percentage of residents that are white, college-educated, and over 65 years of age as covariates. I also perform an exact match on metropolitan statistical area, so that each treated city is compared to a matched control city within the same CBSA.[14]

The two groups are well-balanced on the matching covariates, as indicated by the Kolmogorov-Smirnoff statistics in the second half of Tables 5 through 7. For each outcome variable, I

[14]In all specifications, I identify the matched control group using Diamond & Sekhon's Genetic Matching algorithm (Diamond & Sekhon 2012), courtesy of the `Matching` package in `R` (Sekhon 2011). Owing to the heavily right-skewed city size distribution, I drop three cities with population greater than 500,000.

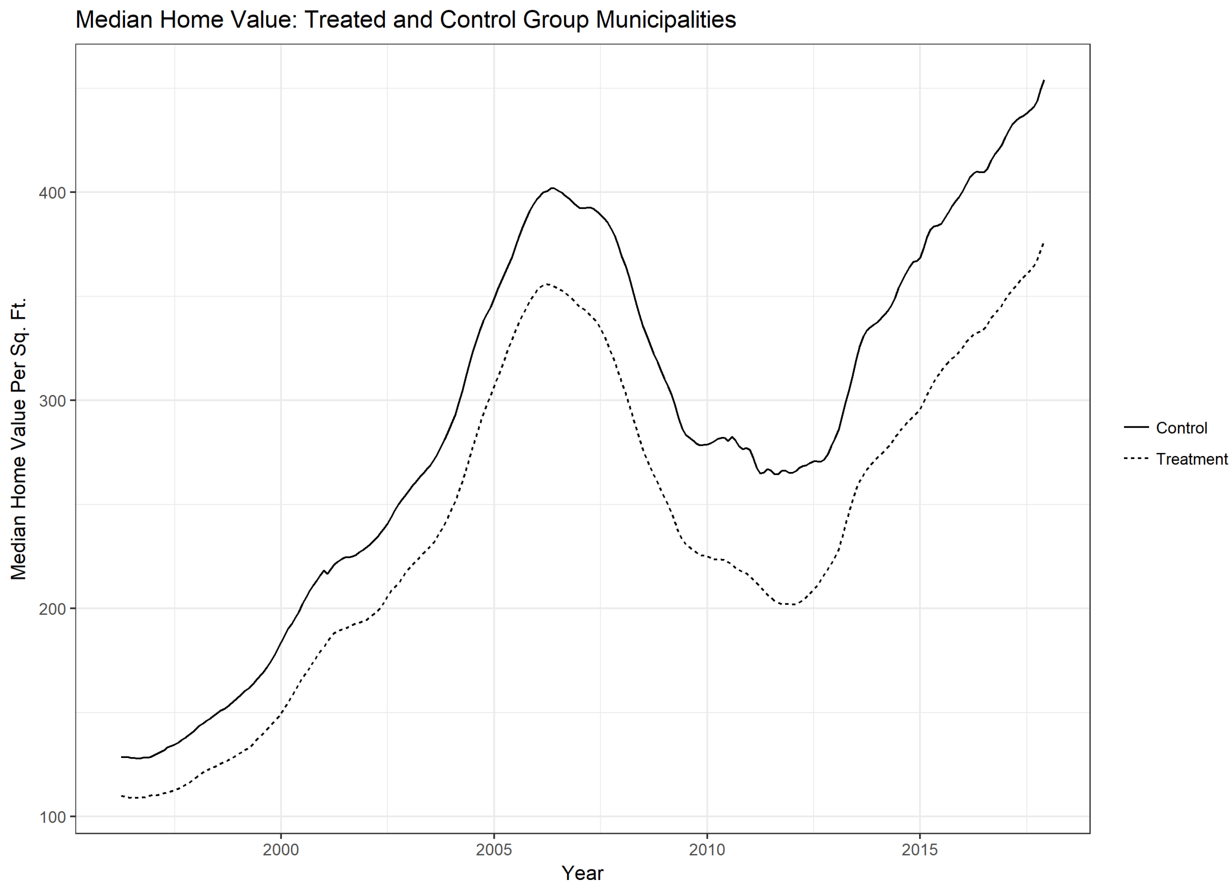

Figure 6: Median real home prices grew more slowly in cities that moved their city council elections on-cycle than in comparable cities that did not.

compute the average treatment effect on the treated units (ATT). These estimates are similar to those from the OLS: the median home value in treated cities is roughly $75 higher per square foot than in control cities, and they issued half as many building permits.

7.3 Difference-in-Difference

Matching ensures that the treatment and control groups are balanced on *observed* covariates, but there may yet be unobserved city-level characteristics affecting housing policy. To adjust for these unobserved covariates, we will now investigate within-city variation through a difference-in-difference analysis.

To do so, I compare the growth in home prices between cities that shifted their election timing from off-cycle to on-cycle, and those cities where elections remained off-cycle the entire period. As before, I create a matched control group, balancing on median income, population, demographics, developable land, and per capita debt burden.[15] I perform a similar analysis for the growth of newly permitted housing stock.

In total, I identify 65 cities that shifted their election timing from off-cycle to on-cycle during the period of study. As illustrated in Figure 5, these cities are located throughout the state, although a plurality are within or around the San Francisco metropolitan area. Their mean population is roughly 55,000, median income is on average $55,000, and roughly 30% of their population is college educated. These and other covariate balance statistics are listed in Table 8.

The cities that shifted their election timing are broadly similar to the cities that did not, with three notable exceptions. First, they tend to have a greater share of nearby developable land (26% compared to 9%). Second, they tend to have a larger percentage of white residents (54% and 44%, respectively). And finally, they hold more municipal debt per capita ($2000 compared to $1400). Because each of these characteristics may affect the price and growth of the housing stock, I opt for the more conservative approach of creating a matched control group prior to estimating the difference-in-difference. Post-match, there are no significant differences between the groups, as measured by a Kolmogorov-Smirnoff statistic.

Figures 6 and 7 illustrate the results. Both groups begin with roughly the same average sale price per square foot (only a $24 difference). But home prices grow much more slowly in the treatment group, and by 2015, the difference is nearly $100. This coincides with a large difference in the number of new building permits issued between the treatment and control group. Collectively, the control group permitted roughly 50,000 new housing units between 1996 and 2016, while the treatment group issued nearly 200,000 during that same period.

[15] This matching is not strictly necessary for a difference-in-difference analysis as long as one assumes that the potential outcomes in both groups follow "parallel trends". However, the parallel trends assumption is more plausible after matching on observed covariates, so one could consider this test even more conservative than a standard difference-in-difference. See Abadie (2005) for a detailed discussion of semi-parametric difference-in-difference estimators.

Eyeballing the data, it appears that the most dramatic leap in new homebuilding occurred in the run-up to the housing collapse (2000-2007). This accords with intuition, but it is striking how much steeper that line during this period is for the cities that switched to on-cycle elections. Homebuilding in the control municipalities ticks up only slightly, while in the treatment group, the housing stock expands nearly 5% each year, before converging with the control group by 2009. Nearly all of the difference in new housing stock between the two groups came about during that period.[16] In Table 8, I report the estimates, balance statistics, and measures of uncertainty. Median home value per square foot grew, on average, by $17 less in the cities that moved their elections on-cycle. And those treated cities issued roughly two-and-a-half times as many permits as the control group between 2000 and 2016 (about 4,300 new units per city on average).

8 Conclusion

The debate over land use policy is often framed as a choice between local self-determination and broader economic efficiency. Should a city like Palo Alto be compelled to permit more housing in order to benefit people that do not currently live there, but would like to? Or do the current citizens have a right to determine for themselves the density and character of their own community? Indeed, much of the formal modeling literature on this topic proceeds from this assumption as well: residents of a municipality vote on the amount of new housing they want in their jurisdiction, and the median voter result holds. The evidence I present here suggests that this is not quite the right framing. Because municipal elections are poorly attended affairs, and the actors with the most political influence in city government are disproportionately drawn from groups that oppose new housing construction. As a result, the equilibrium housing policy reflects neither the will of the median voter, nor the optimal

[16] Nine of the cities in the treatment group switched their election timing on or after 2010, too late to have explained this pattern. However, the difference-in-difference estimate is robust to dropping those observations.

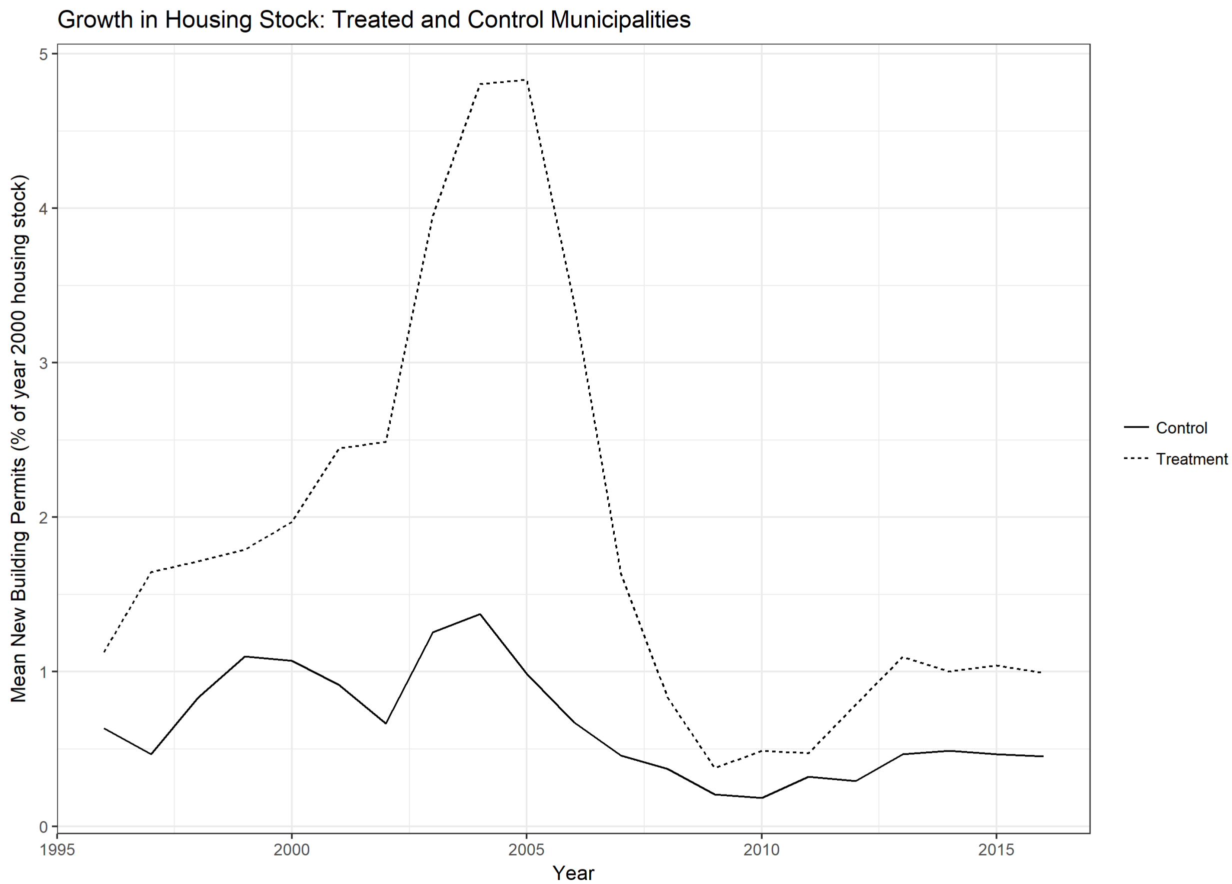

Figure 7: Compared to cities that kept their elections off-cycle, cities that shifted to on-cycle elections issued permits for roughly four times as many new housing units between 1996 and 2015.

growth of the housing supply that a benevolent urban planner would pursue.

There are at least two ways I hope to expand this study in future work. First, the empirical analysis is restricted to California, due to the lack of a comprehensive dataset on municipal election timing in other states. For many of the reasons discussed above, California is a unique case, and findings in this region may not generalize more broadly. A concerted effect to collect data on city council election timing outside of California would help establish the external validity of the findings presented here.

Finally, although I have done what I could to alleviate endogeneity concerns, the fact remains that my sample consists of cities that self-selected into their institutional rules. An interesting avenue for future research would be to identify cities where election timing is assigned exogenously (e.g. by state-level mandate). Fortunately, we've recently observed such an exogenous treatment assignment. In September 2018, California passed SB 415, a law requiring that lower-level governments hold their elections currently with statewide elections (wherever off-cycle elections attract 25% lower voter turnout than the average on-cycle election). Over the next several years, we should begin to see how this shock to election timing affects municipal-level public policy. Readers are encouraged to remind me to write a follow-up paper in 2028.

Despite these limitations, the evidence presented here provides a compelling glimpse at yet another significant consequence of election timing. If restrictive land use policy is partly the product of organized interests mobilizing during low-turnout elections, then it raises fundamental questions about the nature of representation in municipal government. And it suggests that a relatively simple institutional reform could yield broad welfare gains.

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Appendix 1: Spatial Econometric Tests

In the foregoing analysis, I have assumed that median home prices in one city are statistically independent of home prices in neighboring jurisdictions. This is, however, a heroic assumption. Because homebuyers are not constrained by buy homes within a single municipality, factors that affect the price of housing in one city are likely to affect nearby municipalities as well. As a result, land use policies are likely to exhibit spillover effects. A supply restriction in one city can increase home prices throughout the metropolitan area.

The good news is that these spillover effects are likely to bias *against* my hypothesis. If off-cycle elections cause City A to enact restrictive zoning, which increases home prices in both City A and neighboring City B, then I should be more likely to observe a null result when comparing home prices within a metro area. Nevertheless, it is a useful robustness test to explicitly model the spillover effect between jurisdictions, and see if it alters my substantive conclusion. To do so, I model home prices with a spatial autoregressive lag model, as follows:

$$Y_i = \rho W Y + \beta X_i + \varepsilon_i$$

where Y is a vector of median home values and W is a spatial weights matrix, with each W_{ij} containing a measure of "closeness" between city i and j. In the following analysis, I populate the W matrix using the inverse distance between the centroids of each pair of municipalities (Column 1) and a 50km threshold (Column 2).[17] A positive ρ implies that median home values are positively correlated across space, holding X_i constant. In the presence of such autocorrelation, omitting the ρWY term would bias the estimates of β. Table 9 reports the coefficient estimates from this model; despite the addition of the spatial lag term, the estimated coefficient on Off-Cycle elections remains significant. Bear in mind that the β coefficient reported here is not, as in an OLS, equivalent to the estimated effect size. Rather, one can think of it as the "pre-spatial feedback" impulse, analogous to a coefficient estimate in a lagged-dependent variable time series model (Franzese & Hays 2007).

Appendix 2: Heterogeneous Treatment Effects

The effect of off-cycle election timing may vary depending on context. For example, new single family developments may provoke less political opposition in off-year elections than multifamily housing. As the ballot initiative results suggest, public support for urban sprawl restrictions do not vary with election timing, but support for new infill developments does. To test this hypothesis, I recompute the cross-sectional regression analysis separately for single family and multifamily housing. As Figure 8 shows, the estimated effect of election timing is slightly stronger for multifamily housing than for single-family housing, but this difference is not statistically significant. Note that 24% of the municipalities in my dataset permitted zero multifamily units between 2010-2016, so I drop those observations when multifamily permits are the dependent variable below.

[17] I have also estimated the model using a threshold distance matrix, spatial contiguity matrix, and a shared-CBSA matrix, without meaningfully altering the results.

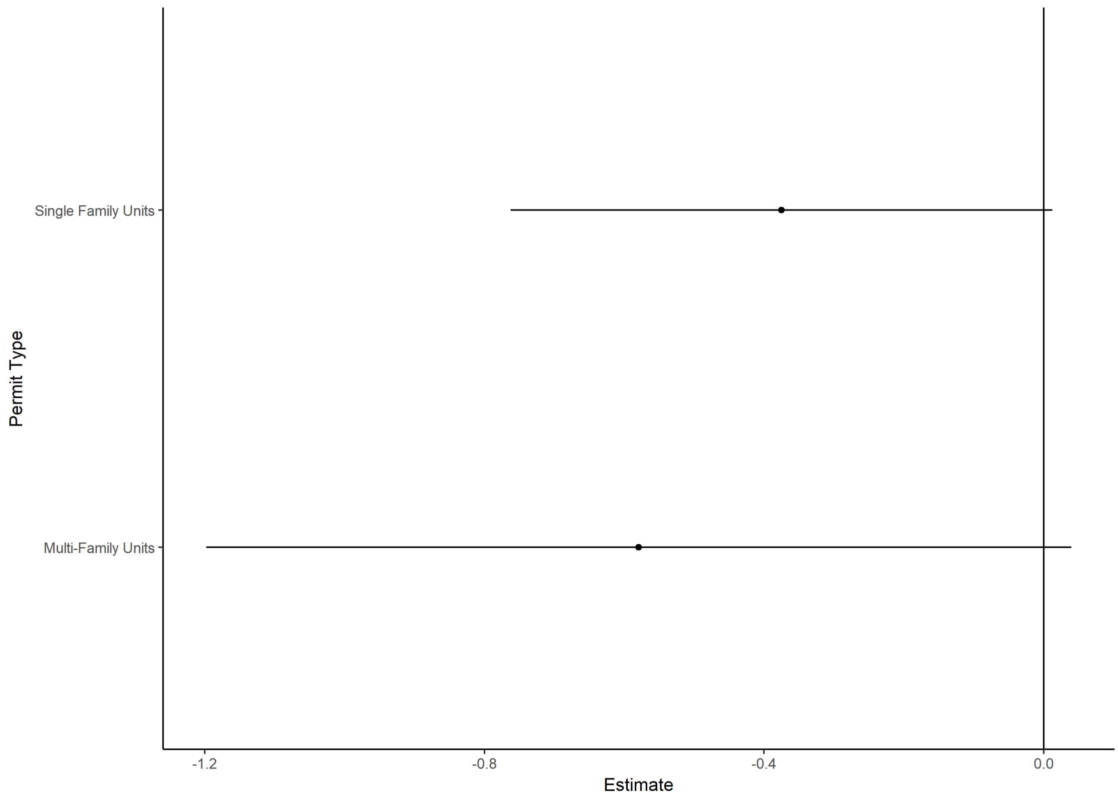

Figure 8: Estimated effect of off-cycle elections on log new building permits (2000-2016), by type of housing.

Table 1: Relationship between election timing and success of pro-housing ballot initiatives, by type of development. City-level controls include mean temperature, log population (2000), median income, pct. white, pct. over 65, pct. college graduates, pct. nearby developable land area (2001), school district Academic Performance Index (2003), and debt per capita (2002).

	Dependent variable:		
	Percent Pro-Housing		
	(1)	(2)	(3)
Off-Cycle	0.02	0.02	0.04
	(0.03)	(0.03)	(0.03)
Infill	0.19***	0.20***	0.15***
	(0.03)	(0.03)	(0.04)
Off-Cycle * Infill	−0.09	−0.10	−0.12*
	(0.05)	(0.05)	(0.05)
Academic Performance Index (2003)			−0.001**
			(0.0003)
Constant	0.39***	0.32***	−1.76
	(0.01)	(0.03)	(1.36)
CBSA Fixed Effects	No	Yes	Yes
City-Level Controls	No	No	Yes
Observations	216	200	194
R^2	0.17	0.27	0.36
Note:	*p<0.05; **p<0.01; ***p<0.005		

Table 2: Estimated OLS coefficients and standard errors, regressing log new building permits (2000-2016) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:		
	Log Permits (2000-2016)		
	(1)	(2)	(3)
Pct. Off-Cycle	-1.17^{***}	-0.55^{***}	-0.36^{*}
	(0.18)	(0.16)	(0.17)
Log Population	0.94^{***}	1.07^{***}	1.06^{***}
	(0.06)	(0.05)	(0.05)
Median Income		0.00	0.00
		(0.00)	(0.00)
January Median Temp.		-0.04	-0.06
		(0.02)	(0.04)
July Median Temp.		0.04^{***}	0.06^{**}
		(0.01)	(0.02)
Pct. White		1.20^{***}	1.17^{*}
		(0.42)	(0.48)
Pct. Over 65		-0.93	-2.44
		(1.64)	(1.67)
Pct. College Grad		-0.40	0.16
		(0.78)	(0.84)
Debt Per Capita (2002)		0.25^{***}	0.24^{***}
		(0.04)	(0.04)
Pct. Developable (2001)		2.25^{***}	2.36^{***}
		(0.43)	(0.57)
Academic Performance Index (2003)		0.001	0.001
		(0.001)	(0.001)
Constant	-3.29^{***}	-9.51^{***}	-9.97^{***}
	(0.95)	(1.48)	(2.76)
CBSA Fixed Effects	No	No	Yes
Observations	330	324	317
R^2	0.44	0.69	0.74
Note:	$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.005$		

Table 3: Estimated OLS coefficients and standard errors, regressing log new building permits (2010-2016) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:			
	Log Permits (2010-2016)			
	(1)	(2)	(3)	(4)
Pct. Off-Cycle	-1.02^{***}	-0.79^{***}	-0.71^{***}	-0.52^{*}
	(0.19)	(0.19)	(0.21)	(0.23)
Log Population	1.15^{***}	1.20^{***}	1.19^{***}	1.21^{***}
	(0.06)	(0.06)	(0.06)	(0.08)
Median Income		0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)
January Median Temp.		-0.01	-0.01	0.01
		(0.02)	(0.04)	(0.05)
July Median Temp.		0.02	0.07^{*}	0.08^{*}
		(0.02)	(0.03)	(0.03)
Pct. White		0.35	0.56	0.39
		(0.50)	(0.62)	(0.72)
Pct. Over 65		-4.27^{*}	-5.31^{*}	-8.97^{***}
		(2.06)	(2.13)	(2.62)
Pct. College Grad		3.01^{***}	2.53^{*}	4.71^{***}
		(1.00)	(1.08)	(1.45)
Debt Per Capita		0.14^{***}	0.14^{***}	0.17^{***}
		(0.03)	(0.03)	(0.05)
Pct. Developable		1.76^{***}	1.75^{*}	2.04^{*}
		(0.50)	(0.72)	(0.86)
Academic Performance Index		0.002	0.003	0.002
		(0.002)	(0.002)	(0.002)
Ideology Score				1.19
				(0.62)
Constant	-8.95^{***}	-13.84^{***}	-18.03^{***}	-19.03^{***}
	(0.97)	(1.89)	(3.55)	(3.97)
CBSA Fixed Effects	No	No	Yes	Yes
Observations	358	351	342	266
R^2	0.50	0.61	0.65	0.66
Note:	*p<0.05; **p<0.01; ***p<0.005			

Table 4: Estimated OLS coefficients and standard errors, regressing median home value per sqft (2017) on percent off-cycle elections and covariates in a sample of California cities.

	Dependent variable:			
	Median Home Value Per Sqft (2017)			
	(1)	(2)	(3)	(4)
Pct. Off-Cycle	150.45***	99.97***	69.35***	61.47**
	(30.09)	(19.84)	(19.19)	(21.75)
Log Population		−2.27	−13.21*	−16.73*
		(6.29)	(5.71)	(7.69)
Median Income		0.005***	−0.0002	0.001
		(0.0003)	(0.001)	(0.001)
January Median Temp.		7.40***	14.90***	11.10*
		(1.87)	(4.08)	(4.50)
July Median Temp.		−14.82***	−13.34***	−14.30***
		(1.35)	(2.65)	(3.07)
Pct. White			−24.49	−45.71
			(55.26)	(66.87)
Pct. Over 65			−85.06	272.87
			(144.84)	(243.07)
Pct. College Grad			664.51***	525.85***
			(97.42)	(134.87)
Debt Per Capita			−1.86	5.27
			(2.88)	(4.26)
Pct. Developable			−15.78	−25.97
			(65.87)	(80.04)
Academic Performance Index			0.24	0.23
			(0.17)	(0.20)
Ideology Score				−162.25**
				(57.92)
Constant	343.19***	780.31***	536.64	816.69*
	(17.40)	(162.42)	(323.94)	(369.62)
CBSA Fixed Effects	No	No	Yes	Yes
Observations	362	361	338	264
R^2	0.06	0.63	0.79	0.80
Note:				*p<0.05; **p<0.01; ***p<0.005

Table 5: Matching Analysis (Home Values): Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Median Home Value (per sqft)	499.6	423.8	75.8	0.0003
Number of Cities	126	67		
	Mean, Treatment	**Mean, Control**	**K-S Statistic**	**K-S Bootstrap p-value**
Balance Statistics				
Median Income	73,243	72,605	0.119	0.314
Population (2010)	71,608	70,842	0.142	0.118
Jan. Mean Temp	52.38	52.02	0.158	0.052
Jul. Mean Temp	72.71	72.39	0.134	0.126
Pct. White (2010)	0.38	0.39	0.174	0.046
Pct. College Grad	0.34	0.32	0.159	0.06
Pct. Over 65	0.124	0.122	0.087	0.666
Academic Performance Index	793.6	803.7	0.190	0.012
Pct. Developable (2011)	0.131	0.147	0.214	<2e-16
Debt Per Capita (2007)	2.23	1.84	0.134	0.148

Table 6: Matching Analysis: Building Permits (2010-2016). Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Log Permits (2010-2016)	7.91	8.56	−0.65	0.025
Number of Cities	127	67		
	Mean, Treatment	**Mean, Control**	**K-S Statistic**	**K-S Bootstrap p-value**
Balance Statistics				
Median Income	73,032	72,046	0.118	0.298
Population (2010)	71,208	71,531	0.150	0.1
Jan. Mean Temp	52.4	52.2	0.157	0.098
Jul. Mean Temp	72.9	72.6	0.126	0.226
Pct. White (2010)	0.38	0.39	0.173	0.026
Pct. College Grad	0.34	0.32	0.150	0.114
Pct. Over 65	0.124	0.122	0.087	0.656
Academic Performance Index	793	801	0.181	0.03
Pct. Developable (2011)	0.137	0.159	0.204	0.008
Debt Per Capita (2007)	2.23	1.91	0.118	0.292

Table 7: Matching Analysis: Building Permits (2000-2016). Effect of off-cycle elections and balance statistics.

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Log Permits (2000-2016)	10.11	10.67	−0.56	0.014
Number of Cities	124	69		
	Mean, Treatment	**Mean, Control**	**K-S Statistic**	**K-S Bootstrap p-value**
Balance Statistics				
Median Income	55,604	55,947	0.144	0.154
Population (2000)	68,082	64,230	0.096	0.566
Pct. White (2000)	0.44	0.45	0.12	0.292
Pct. College Grad	0.30	0.30	0.112	0.38
Pct. Over 65	0.111	0.111	0.088	0.638
Pct. Developable (2001)	0.148	0.165	0.272	<2e-16
Debt Per Capita (2002)	1.41	1.46	0.144	0.134

Table 8: Difference-in-difference, comparing cities that switched to on-cycle elections (treatment) and those that remained off-cycle (control).

	Mean, Treatment	Mean, Control	Difference in Means	T-Test p-value
Outcome Variables				
Δ Median Value per Sq. Ft. (2002-2014)	78.2	95.8	-17.6	0.026
New Units Permitted (2000-2016)	4,300	2,545	1,755	0.024
Number of Cities	27	27		
	Mean, Treatment	**Mean, Control**	**K-S Statistic**	**K-S Bootstrap p-value**
Balance Statistics				
Median Income (2000)	56,392	54,395	0.222	0.484
Population (2000)	53,187	55,053	0.148	0.89
Mean Jan. Temp	50.6	50.9	0.185	0.658
Mean Jul. Temp	74.0	73.0	0.222	0.472
% White (2000)	50.0	56.2	0.222	0.482
% College Grad (2000)	28.7	32.1	0.222	0.48
% Over 65 (2000)	12.0	13.0	0.296	0.174
API (2003)	696	706	0.222	0.436
% Developable (2001)	22.4	18.4	0.259	0.282
Debt Per Capita (2002)	2,655	1,858	0.296	0.168

Table 9: Estimated coefficients estimates from the spatial autoregressive lag model.

	Dependent variable:	
	median.hv.sqft.2017	
	(1)	(2)
Pct. Off-Cycle	35.83**	44.14***
	(14.23)	(16.66)
Log Population	−13.68***	−17.19***
	(3.77)	(4.35)
Median Income	0.001***	0.001**
	(0.0003)	(0.0004)
January Median Temp.	7.85***	4.55**
	(1.36)	(1.99)
July Median Temp.	−4.32***	−3.95***
	(1.06)	(1.52)
Pct. White	−74.88**	−95.90**
	(36.00)	(42.24)
Pct. Over 65	86.16	104.99
	(103.89)	(119.92)
Pct. College Grad	605.91***	738.92***
	(69.27)	(78.63)
Debt Per Capita	−1.16**	−1.11**
	(0.46)	(0.51)
Pct. Developable Land	−1.69	80.15**
	(16.96)	(40.48)
Test Scores	0.02	−0.10
	(0.13)	(0.14)
ρ	0.97***	0.52***
	(0.02)	(0.05)
Observations	412	405
LR Test (df = 1)	165.44***	75.41***
Note:	*p<0.1; **p<0.05; ***p<0.01	