The Political Calculus of Land-Use Reform

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Abstract: Institutional design plays a central role in shaping when policymakers are willing to pursue reforms, especially in local contexts where the political costs of action are high. Theoretically, institutional reform can reshape the incentives of local decision makers to address collective problems. Housing policy provides a hard test of this logic: despite widespread recognition of an affordability crisis, entrenched local political pressures block land-use reform. This paper examines how changes to fiscal autonomy can alter these dynamics, focusing on a state-level program in Massachusetts providing municipalities with financial support for zoning reform. Using synthetic control and difference-in-differences designs, I show the policy produced measurable increases in housing permits. Event history analysis reveals that adoption was concentrated in jurisdictions predisposed toward growth. I argue the program's effectiveness derived from its capacity to enhance fiscal capacity and reduce the perceived political costs of development, thereby reshaping local policymakers' incentives.

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Introduction

Political science research demonstrates that institutional arrangements fundamentally shape when and how policymakers are willing to act. Elected officials often operate under strong constraints that discourage altering the status quo, particularly at the local level and in contested policy domains, where the costs of reform are concentrated and highly visible while the benefits are diffuse, delayed, and difficult to claim politically. Institutional arrangements can shift electoral incentives or enhance policymakers' capacity to manage constituent concerns, and can therefore be critical for enabling action in otherwise gridlocked domains (Mullin and Hansen, 2023; Hankinson and Magazinnik, 2023). This study examines the role of one such institution, local fiscal autonomy, defined as the extent to which local governments have independent authority over revenue generation and expenditure decisions, and argues that increases in fiscal autonomy can transform the political feasibility of reform.

I evaluate this claim in the context of housing policy through an analysis of Massachusetts' Chapter 40R zoning incentive program. The case demonstrates how institutional design can enable policy change by reducing the fiscal and political costs local officials associate with reform. By providing additional funding to municipalities that revise their zoning to permit greater density, the program offsets the fiscal pressures of growth and equips officials with resources to address concerns about congestion, service strain, or neighborhood change. In effect, the incentive functions like an increase in fiscal autonomy: it expands local governments' discretionary capacity while leaving policymaking authority and responsibility intact. This shift in the local fiscal landscape alters the political calculus of reform, making it more feasible even in settings characterized by intense and persistent opposition.

Using a combination of synthetic control and difference-in-differences designs, I show that Chapter 40R measurably increased housing production in Massachusetts during the post-reform period. To understand local variation, I then turn to an event history analysis, which reveals that municipalities with a preexisting political openness to development were the ones most likely to adopt the incentive and drive these gains. Theoretically, the findings suggest that fiscal incentives matter not by changing public opinion but by reshaping the institutional environment in which local policymakers calculate risk. More broadly, the paper contributes to debates on institutional design, fiscal federalism, and housing politics by demonstrating that incentive-shaping institutions can ease political constraints and enable reform even when constituent opposition remains strong.

Incentives and the Politics of Reform

Political elites are widely understood as strategic, office-seeking actors who anticipate voter reactions and adjust their behavior accordingly (Arnold, 1990; Mayhew, 1974). This *electoral connection* creates strong incentives to avoid actions that might alienate voters, and some accounts even highlight the risk of pandering (Mullin and Hansen, 2023), where politicians follow public opinion despite believing that doing so is not in voters' long-term interest. This concern is especially pronounced at the local level, where policymakers frequently confront issues that impose concentrated costs while generating diffuse benefits (Fischel, 2005; Einstein et al., 2019; Gerber and Hopkins, 2011; Trounstine, 2020; Glaeser et al., 2005).

Yet research in political behavior makes clear that the pathway from constituent demand to policy action is far from mechanical. Policymakers may misperceive public preferences (see Broockman and Skovron 2018; but also Dias et al. 2025), or rely on biased or incomplete information. Even when policymakers accurately understand public preferences, they may prioritize some constituencies over others, responding selectively to

groups that are more electorally salient, better organized, or more politically influential (Fastenrath and Marx, 2025; Soontjens and Persson, 2024). Voters, for their part, vary in their likelihood of participating, in how clearly they attribute responsibility, and in whether they reward effort or outcomes (Rastogi and Laurison, 2025; Hall and Yoder, 2022; Bisgaard, 2015). Electoral incentives therefore matter, but the relationship between public preferences and policymaking is imperfect and contingent.

Moreover, imperfect responsiveness is not only a function of misperception or voter heterogeneity. It also reflects the reality that governing requires making decisions under constraints: fiscal limits, legal mandates, temporal pressures, and conflicting constituent priorities. Under these conditions, policymakers may find themselves compelled to adopt policies they anticipate will be unpopular, simply because the status quo is untenable or because alternative choices carry even greater risks. In that scenario, policymakers want to manage the risk of electoral backlash. If a politician is hesitant to pass a policy out of concern over electoral backlash, they should become more willing to support the policy when they can convince voters of its necessity, compensate voters for unpopular decisions, shift or diffuse blame, obscure responsibility, or soften the material impact of policies in ways that reduce the likelihood of electoral punishment.

One key determinant of policymakers' ability to employ strategies that mitigate electoral risk is the institutional environment. Research on institutions and political behavior shows that institutions structure political processes on both the elite side and the constituent side. Institutions shape how preferences are aggregated, how decisions are made, and how responsibility is attributed. As Hoefer notes, institutions change the behavior of individuals who work within them, and the decisions of those involved in policymaking are guided by the rules and processes of the institutions in which they operate (2022).

In local policymaking, institutional design shapes political behavior in fundamental respects. First, institutions that allocate policymaking authority determine which deci-

sions local governments can make and when state or federal actors may override local choices (see, for example, research on state preemption (Glaeser et al., 2005; Fischel, 2005; Hirt, 2012; Michener, 2023; Hicks et al., 2018; Briffault, 2018; Kim et al., 2021)). Second, electoral rules such as eligibility requirements and whether elections are held at large or by district determine the composition of the electorate and therefore the groups whose reactions local officials most anticipate (Hankinson and Magazinnik, 2023; Hoefer, 2022).

A further dimension of institutional design concerns the fiscal arrangements under which local governments operate. These institutions do not determine who holds authority or who participates. Instead, they shape the resources available for policymaking and the costs associated with particular choices. In this way, fiscal institutions influence the incentive structure that elected officials confront when evaluating whether they can absorb the electoral risks associated with adopting a potentially unpopular policy.

Fiscal autonomy refers to a jurisdiction's capacity to independently raise and allocate resources. In the United States, municipalities have limited autonomy because most taxing authority resides at the state level. Individual income taxes are a major source of revenue for states, yet they provide relatively little revenue for local governments. Most municipalities rely heavily on the property tax, and even that revenue source is often constrained by state-level limits. When fiscal autonomy is low, local governments have few options for mitigating the electoral risks associated with policy decisions through increased spending for compensation, service expansion, or buffering burdens on residents. By contrast, increases in fiscal autonomy expand the discretionary resources available to local officials while leaving their authority and responsibility intact. To illustrate this, imagine a city that decides to expand access to a local public service, such as opening enrollment to users from neighboring jurisdictions. If that city has high fiscal autonomy, it can make the policy change while simultaneously investing in additional capacity, such as hiring more staff or expanding service provision, so that

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existing residents do not experience a decline in quality.[1] It follows that increased fiscal autonomy should lead to policymakers pursuing policies that might otherwise be politically costly. This logic is especially relevant in policy areas where residents perceive new policies or regulatory change as imposing direct and concentrated costs. Housing policy is one such domain, and it provides a productive test of the argument that local fiscal autonomy shapes policymakers' willingness to pursue politically risky reforms. Crucially, the relevance of fiscal autonomy in this setting lies not in municipal housing expenditure, but in the ability of local governments to cushion the distributive and political consequences of regulatory reform.

Housing

Decisions about new housing are among the most politically costly choices local officials confront, given the well-documented opposition from homeowners concerned about congestion, service strain, neighborhood change, and property values. These preferences generate strong electoral pressures, making housing development a domain where the political costs of action are unusually salient and the potential for institutional mediation especially clear. As a result, housing offers a hard test of the broader argument: if shifts in fiscal incentives can alter policymaker behavior in a domain characterized by intense constituent opposition, this provides strong evidence that incentive-shaping institutions meaningfully condition the electoral calculus of local officials.

Housing scarcity is a widespread social and economic problem. Between 2019 and 2024, house prices across the U.S. have risen by 54 percent (Mena, 2024). According to data from the 2020 census, 67 percent of current renter households reported

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[1]As such, fiscal autonomy is conceptually distinct from higher-level governments financing a particular policy outcome. When a state directly funds a policy, such as by building housing itself or by mandating implementation while covering the full cost, responsibility for the policy decision shifts upward and the local electoral calculus is altered primarily through an authority shift.

wanting to buy a home, but being unable to do so (Lee et al., 2020). Across rental markets, 49 percent of households are housing burdened (National Equity Atlas, nd),[2] and approximately 770,000 people experienced homelessness in the U.S. in 2024 (U.S. Department of Housing and Urban Development, 2024). The under-supply of housing carries significant social and economic consequences. It deepens existing inequalities (Hwang et al., 2015; Trounstine, 2020), as different groups face unequal risks and levels of market exposure. Housing market pressures also shape political behavior, fueling support for the radical right (Abou-Chadi et al., 2016), populism (Ansell et al., 2022), and opposition to redistributive policies (Hager and Vief, 2022). At the macro level, housing constraints further impede economic growth (Ganong and Shoag, 2017; Hsieh and Moretti, 2019).

Given the scale and salience of the problem, it is striking how difficult it has been to reform housing policy. Much of the existing research traces this difficulty to local political incentives. A prominent explanation contends that local officials are highly responsive to homeowners, who often oppose new housing and favor restrictive land-use policies(Hou, 2017; Einstein et al., 2019; Fischel, 2015; Trounstine, 2020; Glaeser and Gyourko, 2018; Bertaud, 2018; Glaeser and Ward, 2009; Hankinson and Magazinnik, 2023). Existing research attributes this opposition to a range of different mechanisms, including symbolic preferences (Broockman et al., 2024), economic anxieties over house prices (Hankinson, 2018), efforts to exert control over neighborhood composition (Trounstine, 2020), preservationist tendencies (Larsen and Nyholt, 2024), and concerns about congestible public goods such as schools, roads, and parks, where a fixed per capita funding level leads to a decline in quality as the population grows (Krimmel, 2021; LaBriola, 2024; Wildasin, 1987). As a result, local officials, highly responsive to residents' concerns, face steep political risks, and reform stalls.

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[2]A household is considered housing burdened when spending on rent exceeds 30 percent of total household income (National Equity Atlas, nd).

Building on this research, I argue that fiscal institutions play a central role in shaping housing policy. When municipalities can increase revenue alongside population growth, they are better positioned to address the sources of resident opposition or to compensate current residents for perceived losses. This logic is particularly clear in the case of congestion, one of the most prominent and tangible concerns raised in opposition to new housing, and a mechanism that has received sustained attention in economics but comparatively less in political science. Where revenue is flexible, population growth need not imply declining service quality: local governments can expand capacity, sustain per capita spending, and mitigate congestion-related concerns. Conversely, when revenues are fixed while demand rises, policymakers face a stark trade-off: accommodating growth risks overstretching local services, making restrictive zoning a politically safer choice. While congestion provides the clearest illustration of this mechanism, similar dynamics likely apply to other sources of opposition, including distributional concerns and demands for compensation. By expanding the discretionary resources available to local officials, fiscal autonomy alters this political calculus, reducing the costs of approving new development even in the face of persistent opposition. Thus, increased fiscal autonomy should make local governments more willing to approve new housing despite electoral risk.

From Theory to Evidence: The Case of Chapter 40R

Direct changes to local fiscal autonomy are rare and typically occur only through major constitutional, statutory, or court-ordered reforms. One such case is the school finance equalization reforms in California, which substantially reduced local fiscal discretion. Krimmel (2021) shows that this downward shift in fiscal autonomy led municipalities to adopt stricter land-use regulation. While such structural changes are infrequent and offer limited over-time variation, state fiscal incentives provide a useful alternative for empirical analysis. They replicate the core mechanism of fiscal autonomy by providing discretionary revenue that effectively relaxes local budget constraints. This paper examines one such case: Massachusetts' Chapter 40R, a state-level program designed to

increase housing supply in the state by providing direct incentives to municipalities.

The Chapter 40R program encourages Massachusetts municipalities to create higher-density housing near transit and commercial areas by offering financial incentives to participating municipalities. Formally known as the Smart Growth Zoning and Housing Production Act, the program was introduced in 2004 (Mass.gov, 2024). Its goal is to encourage local governments to update zoning regulations to allow for more compact housing and mixed-use developments (Citizens' Housing and Planning Association [CHAPA], 2018). The law sets out specific requirements for creating these "zoning overlay districts," which municipalities must follow. These requirements include placing the districts near transit, commercial areas, and places with existing infrastructure or other suitable conditions for development. Additionally, the law requires that the new developments have densities of at least 8 to 20 units per acre (Citizens' Housing and Planning Association [CHAPA], 2018),[3] and that 20 percent of the new units be affordable. While the state sets these standards, municipalities still retain control over planning by setting design guidelines for the projects, but the types of development allowed are guaranteed, giving developers more security (Citizens' Housing and Planning Association [CHAPA], 2018). The Department of Housing and Community Development oversees the program, and municipalities submit proposals for new districts to the department for approval. Once a district is approved, the municipality becomes eligible for an immediate incentive payment between $10,000 and $600,000, there are no rules on how municipalities can use the incentive payment (Citizens' Housing and Planning Association [CHAPA], 2018).[4] In addition, municipalities receive a $3,000 payment for each unit built. The state also provides extra funds to help cover the additional costs associated with higher density, particularly for infrastructure and schools (Mass.gov, 2024). Since the introduction of the program, 49 of the 351 municipalities in Mas-

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[3]Density rules vary depending on the type of housing, with single-family homes allowed 8 units per acre, townhouses 12 units per acre, and condominiums or apartments 20 units per acre (Mass.gov, 2024).

[4]The size of the payment depends on how many additional units could be built compared to what the original zoning would have allowed.
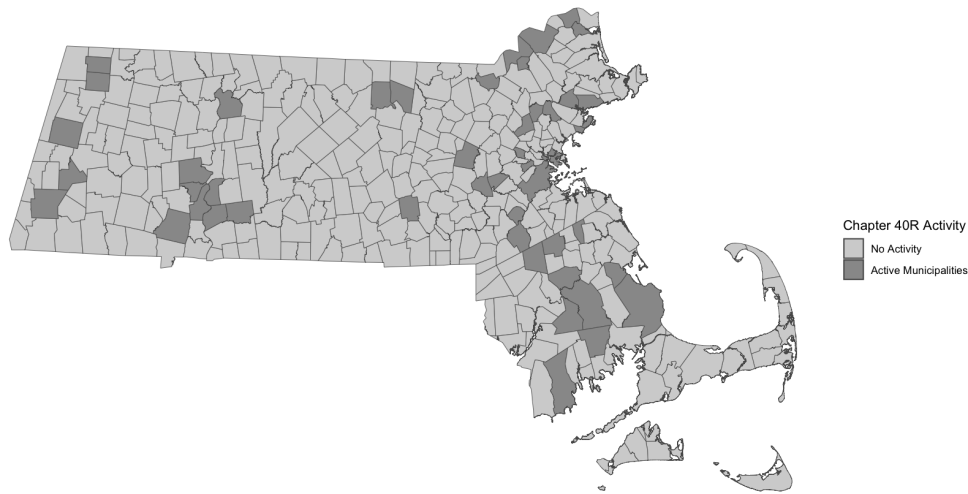
Figure 1: *Adopters and Non-Adopters in Massachusetts.*

sachusetts have implemented at least one district. Figure 1 displays the spatial patterns of adoption.

I argue that in the Massachusetts case, the state incentive structure functions as a mechanism that increases local revenue in a manner analogous to an expansion of fiscal autonomy. Rather than changing local taxing authority, the program secures additional state funding, thereby relaxing local budget constraints. This shift alters the political and fiscal calculus facing local officials and creates conditions under which reform becomes more feasible. Viewing state incentives as a pathway to land-use reform positions institutional design, and fiscal institutions in particular, as central determinants of housing policy outcomes. Chapter 40R may reduce the political risks of rezoning through two channels. First, it may directly alleviate opposition by ensuring that new development is accompanied by investments in infrastructure or public services. Second, it may indirectly facilitate reform by providing discretionary resources that compensate communities for accommodating growth, even when the underlying concerns remain. For the theoretical argument, the distinction between alleviation and compensation is less

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important than their shared effect: both mechanisms shift the political cost–benefit calculation for local officials by reducing the perceived risks of approving new development. Importantly, this argument is agnostic about policymakers' underlying motivations. Local officials may be intrinsically pro-housing, motivated by concerns about residents burdened by current housing market conditions, or focused primarily on the implications of housing supply for economic growth. What matters is not the source of these preferences, but their ability to overcome electoral threats from opposing residents, an ability that altered incentives can strengthen even when substantive opposition persists.

Empirical Strategy

To evaluate this theoretical argument, the paper uses a a combination of methods to evaluate both the overall effect of Chapter 40R and the political dynamics through which it operates. I begin by estimating the policy's impact on aggregate housing supply in Massachusetts. While prior research has examined whether and under what conditions state interventions influence housing production (Mitchell, 2004; Lewis, 2005; Marantz and Zheng, 2020; Fisher, 2013; Fisher and Marantz, 2015), much of this work remains correlational, in part because estimating causal effects is notoriously difficult: policy adoption is often endogenous to local conditions that themselves shape the trajectory of housing outcomes, leaving the effectiveness of such interventions uncertain. To sidestep this challenge, I use two complementary empirical strategies that exploit different sources of variation. First, at the state level, I compare Massachusetts to a synthetic control constructed from other states, a method that mitigates concerns of selective comparisons by employing a transparent, data-driven weighting procedure to generate a credible counterfactual trajectory of housing supply in the absence of Chapter 40R. Second, at the municipal level, I compare adopting municipalities to non-adopters using a difference-in-differences design. Specifically, rely on the Callaway–Sant'Anna DID estimator with not-yet-treated controls that accounts for the fact that municipalities adopt the policy at different points in time. Together, these approaches allow me to assess

both the aggregate effect of Chapter 40R and whether the municipalities that enrolled actually built more housing after implementation.

Having examined the policy's effectiveness, I then turn to the mechanism of local adoption. Even if a program succeeds in aggregate, its impact ultimately depends on the extent and distribution of municipal participation. I argue that Chapter 40R's incentive payments function as a form of additional revenue that municipalities can use to expand public goods provision. This creates a new policy option alongside (1) raising taxes to increase services or (2) restricting demand through land use regulation. To examine these dynamics, I analyze municipal adoption patterns using event-history analysis, identifying the factors that drove local uptake of Chapter 40R.

Evaluating Chapter 40R's Impact on Housing Supply

The synthetic control approach, developed by Abadie, Hainmueller, and coauthors (Abadie and Gardeazabal, 2003; Abadie et al., 2010a, 2015), constructs a weighted combination of untreated units to serve as a counterfactual, allowing for the estimation of treatment effects in cases where no direct comparison group exists.[5] The outcome of interest is permitted units in the time period 1995-2019. I use the `Synth` package

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[5]To assess the effect of a treatment on the treated unit absent a natural control group, the synthetic control approach facilitates comparison between the treated unit and a synthetic version of that unit which does not receive the treatment. This synthetic unit is constructed based on a donor pool of units of observations (here, the other 49 U.S. states) as similar to the treated unit (Massachusetts) as possible along a set of relevant parameters. Each state in the donor pool is assigned a weight based on the pre-reform data. This allows for the construction of a control unit with parallel trends to the treated unit in the pre-reform period, and to impute the counterfactual potential outcomes for the treated unit in the absence of the policy intervention. As such, the synthetic unit represents the trajectory of the outcome variable in the post-reform period one would expect the treated unit to have followed in the absence of the treatment, whereas the trajectory of the treated unit in the post-reform period represents the actual trajectory of that unit after having received the treatment. Comparing the two then allows for an estimation of whether the treatment had an effect.

in R to construct the synthetic control unit and run the analyses that follow (Abadie et al., 2010b). I identify a set of predictor variables likely to affect housing supply outcomes, namely rate of urbanization, population size, annual personal income, personal consumption expenditure, presidential election outcomes, and racial composition of the population.[6] The main results are presented in Figure 2. The top panel shows the actual outcome (solid line) alongside the imputed outcome (dashed line). The bottom panel displays the discrepancies (gaps) between the treated and synthetic outcomes over time (both pre- and post treatment). The observed values are consistently higher than the predicted values from the synthetic control unit. The presence, consistency and magnitude of these gaps, specifically when comparing trends pre-and post treatment, provide insights into the impact of the treatment on the outcome.

In the post-reform period (2004–2019), I estimate an Average Treatment effect on the Treated (ATT) of 2.933, meaning that, on average, Massachusetts had 2,932 more housing units per year than its synthetic counterpart. The Cumulative Treatment Effect over the entire post-reform period is 43.988 units, suggesting a substantial overall increase in housing supply attributable to the policy. To put this effect into context, I compare the ATT to annual housing permit levels in Massachusetts. I find that the average ATT as a percentage of mean housing permits during the post-reform period is 23.9 percent, meaning the estimated treatment effect corresponds to nearly a 24 percent increase in annual housing production relative to typical levels. This suggests that Chapter 40R played a meaningful role in expanding the housing supply.

To assess the robustness of the synthetic control estimates, I include a series of robustness checks in the Appendix: First, I implement permutation tests, reassigning treatment status to each of the donor states to benchmark the Massachusetts effect against the distribution of placebo effects. I present three versions: (i) excluding states with pre-treatment

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[6]I calculate predictor weights only for the pre-reform period to ensure unbiased results in constructing the synthetic unit (Vives-i Bastida, 2022). More details on this set-up, including an overview of the variables and their data sources is included in the Appendix.

MSPE values in the upper 75 percent of the distribution while retaining the donor states that form the synthetic unit; (ii) including all states in the donor pool; and (iii) restricting to the subset of states that make up the synthetic comparison unit. Second, I carry out a placebo-in-time test, assigning treatment to Massachusetts before the true adoption year to confirm that the synthetic control closely tracks the treated unit in the absence of treatment. Finally, I replicate the main analysis excluding Connecticut from the donor pool, given its relatively high weight in the baseline synthetic control. Across all three robustness checks, the results reinforce the main findings: the permutation tests show that the gap for Massachusetts consistently exceeds those of placebo states, the placebo-in-time test confirms that pre-treatment trajectories align as expected, and the Connecticut exclusion test reproduces the main pattern, with slightly smaller post-reform gaps attributable to a weaker pre-treatment fit.
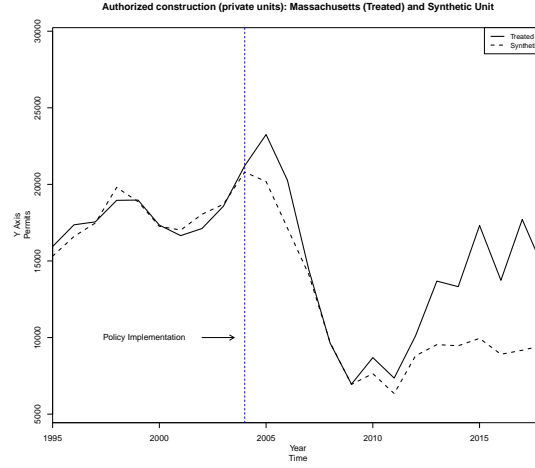
(a) *Observed and imputed outcomes for Massachusetts*
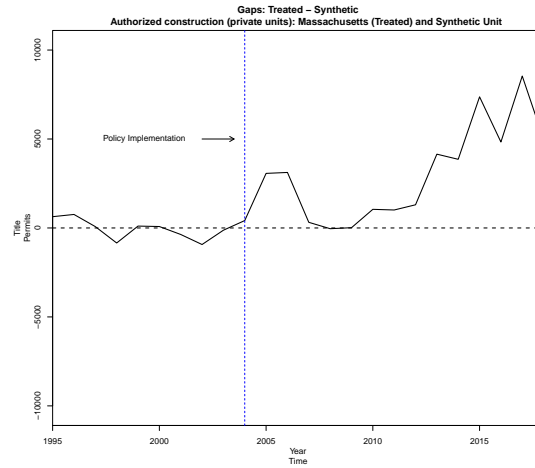
(b) *Treatment effect estimates for Massachusetts*

Figure 2: *Synthetic Control Results for Housing Supply Analysis*

To complement the state-level analysis and verify that the aggregate treatment effect is driven by municipal participation, I estimate group-time ATTs using Callaway–Sant'Anna DID with not-yet-treated controls and municipality-clustered inference.[7] I use per-capita permitted units as the outcome to account for differences in municipality size. The approach compares changes in outcomes over time between adopting and non-adopting municipalities, attributing any systematic post-adoption divergence to the policy. Importantly, it accounts for staggered adoption, recognizing that municipalities adopt at different points in time rather than in a single common

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[7]Data on local implementation of Chapter 40R is collected from the Massachusetts Government website, Executive Office of Housing and Livable Communities.

post-treatment period. Of the 351 municipalities in Massachusetts, 49 adopted at least one Chapter 40R district after its introduction in 2004.
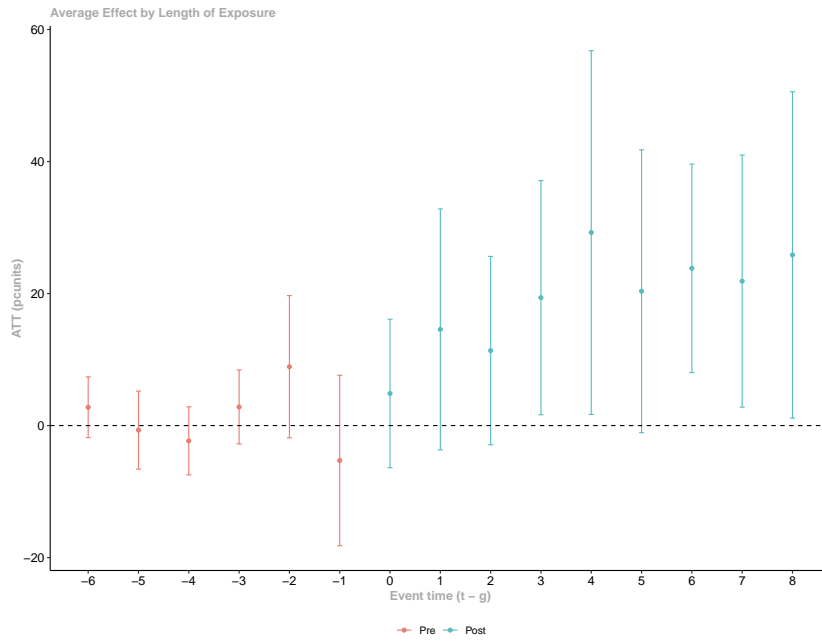
Event-study estimates show a clear post-adoption rise in per-capita permits. In additional analyses reported in the Appendix, I implement a lead-placebo that assigns adoption three years earlier and restricts estimation to true pre-periods to test for potential anticipation. This yields small but positive ATTs, suggesting short-run pre-movement. I therefore decompose these placebo effects into event-time and cohort-specific components, confirming that the pattern is concentrated in a subset of periods and cohorts. Guided by this evidence, I re-estimate with a two-year anticipation window. As reported in Table 1, the resulting post-treatment effect remains large and significant ($\approx$ +18 in per capita units), and is robust to alternative specifications, including restricting the control group to never-treated municipalities. Consistent with this, the event-study estimates in Figure 3 show no evidence of systematic pre-trends and a sustained positive treatment effect in the years following adoption. Additional robustness checks, namely calendar-time aggregation and leave-one-cohort-out estimation (in the Appendix), yield the same conclusion. Together, these checks confirm a strong positive effect after accounting for modest anticipatory dynamics.

	Levels (per-capita permits)		Log (percent)
	(1) Main: anticipation=2	(2) Never-treated controls	(3) Percent effect
ATT	18.42*	18.77*	42.33*
	(6.92)	(6.93)	(—)
95% CI	[4.86, 31.97]	[5.18, 32.35]	[4.72, 93.45]

Notes: Callaway–Sant'Anna DID. The main specification uses Not Yet Treated controls and is doubly robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0. Level-spec ATTs are in per-capita permit units. The log-spec (col. 3) reports the percent effect computed as $100 \times (\exp(\widehat{ATT}) - 1)$. SE in parentheses, not reported on the transformed scale.

Table 1: *Estimated Overall ATT Across Model Specifications.*

Figure 3: *Event-study estimates of the effect of Chapter 40R adoption on per-capita housing permits, estimated with a two-year anticipation window. Points show average treatment effects (ATT) by years relative to adoption (t = 0), with 95% confidence intervals.*

Taken together, the evidence indicates that Chapter 40R had a substantial effect on housing production. The synthetic control analysis shows that, compared to a weighted combination of other U.S. states, Massachusetts experienced a significant increase in the number of permits issued after the program's introduction. Complementing this state-level finding, the difference-in-differences analysis demonstrates that adopting municipalities themselves drove these gains, with event-study estimates revealing a large and sustained post-adoption rise in per-capita permits. Across robustness checks, the results remain precise and positive, confirming that the aggregate increase in housing supply can be attributed to municipal implementation of Chapter 40R.

Understanding Municipal Adoption of Chapter 40R

While the preceding analyses demonstrate that Chapter 40R increased housing production, the political process underlying this effect remains less well understood. Municipalities varied considerably in whether and when they adopted the policy, raising the

question of what local factors shaped these patterns.[8] Over half of adopters acted within the first five years, with 25 municipalities establishing at least one density district by 2008. Figure 4 illustrates both the yearly and cumulative adoption trends, as well as the spatial distribution of early and late adopters. Adoption occurred across most of the state's counties, with the exception of Barnstable, Dukes, and Nantucket.

Understanding why some municipalities embraced Chapter 40R while others refrained is critical to assessing the policy's broader implications. Building on the discussion above, housing policy is a contested domain of local governance, and municipal decisions are often shaped by the preferences of existing residents, many of whom resist new development due to concerns about congestion (Einstein et al., 2019; Fischel, 2015; Trounstine, 2020). To satisfy these preferences, local governments typically preserve public goods quality by restricting housing supply (Fischel, 2005). Chapter 40R, however, altered this calculus by providing fiscal incentives: municipalities received an immediate payment upon approval of a rezoning plan, additional payments per unit constructed, and extra funds to offset potential strain on infrastructure and schools. In effect, the program granted municipalities greater fiscal autonomy, enabling them to sustain public goods quality despite increased demand. In theory, this incentive structure should reduce the need for restrictive land use policies and increase the likelihood of adoption.

In practice, the data presented in Figure 4 indicate variation in municipalities' ability or willingness to respond to the incentive. As the financial incentive becomes available, some cities choose to participate, increasing their housing supply, while others do not. This suggests that an exogenous policy shift, whether through state incentives or broader regulatory changes, can reshape the local fiscal environment and, in turn, alter how policymakers weigh their decisions. Yet, while it reconfigures the trade-off between the electoral risks of allowing new development and the potential economic benefits of

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[8]Of the 351 municipalities in Massachusetts, 49 had adopted at least one district by the program's introduction in 2004. By 2022, 48 municipalities had adopted, marking the end of the study period.

growth, this does not mean that all municipalities will revise their land use regulations. The uneven pattern of adoption is itself theoretically significant: the incentive creates an opportunity for reconsideration, but whether that opportunity translates into policy change depends on local context. Local factors shape the trade-off between competing pressures, determining when the potential gains from new development are sufficient to reduce resident opposition—or when they outweigh the electoral risks for policymakers.
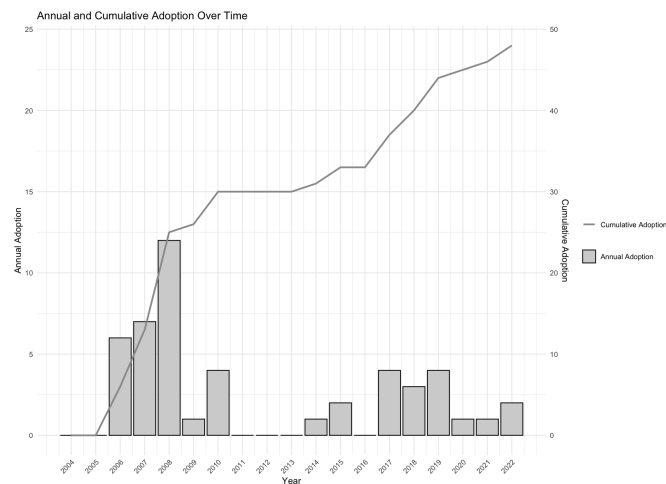
(a) *Yearly and Cumulative Adoption of Chapter 40R Districts.*
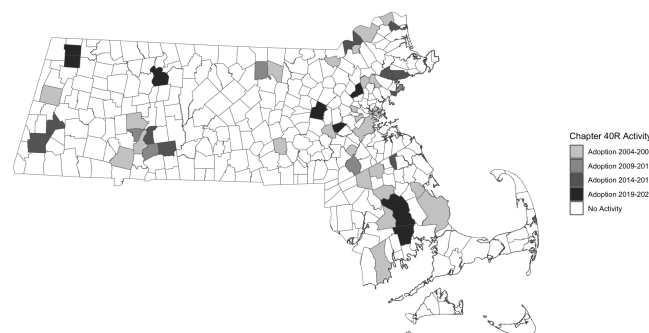
(b) *Adoption Patterns.*

Figure 4: *Local Adoption of Chapter 40R*

Building on the theoretical claim that Chapter 40R reshapes the incentive structure facing local policymakers, the analysis that follows adopts an exploratory approach to identifying which local characteristics are associated with the decision to adopt Chapter 40R. The aim is not to adjudicate between competing explanations, but to investigate the local conditions under which fiscal incentives are most likely to generate policy

change. I focus on three main predictors: I examine whether adoption is more likely in municipalities experiencing housing market strain or facing relatively low levels of political opposition to development. These factors may influence whether the benefits of new housing construction, now amplified by state incentives, are seen as outweighing the electoral risks. In addition to these factors, I consider the possibility that adoption is shaped by institutional constraints, specifically, by the threat of state intervention under Chapter 40B. Since 1969, Chapter 40B has allowed developers to appeal local zoning decisions in municipalities where less than 10 percent of housing qualifies as affordable. In such cases, the state may override local restrictions, effectively reducing municipal control over development. Chapter 40R may therefore be attractive not only as an opportunity for fiscal gain, but also as a strategy for retaining local discretion in the face of potential state action. While the motivations behind adoption are not directly observable, the relationship between affordable housing share and adoption allows for an empirical probe into whether municipalities falling below the 10 percent threshold are more likely to adopt defensively, or whether adoption is more common in cities that have already made substantial investments in affordable housing. Rather than testing mutually exclusive hypotheses, the analysis examines these factors as potentially complementary mechanisms shaping local responsiveness to a state-level incentive.

I estimate a Cox proportional hazards model (event-history analysis) with county fixed effects using the `survival` package in R (Therneau, 2023), which accounts for timing and estimates the probability that a municipality adopts Chapter 40R in a given year, conditional on not having already done so.[9] I operationalize housing market strain using the share of rent-burdened households (paying more than 30 percent of income on rent), use recent construction activity as a proxy for local development dynamics, and

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[9]In the EHA dataset, each municipality-year is a separate observation. The dependent variable, policy adoption, is coded as zero for all years until adoption, and as one in the adoption year, after which that municipality exits the dataset. Each year, municipalities that have yet to adopt make up that year's risk set, with its size decreasing as more municipalities adopt. The dataset begins in 2004, the year the policy was introduced.

a municipality's ability to meet the 10 percent affordability requirement under Chapter 40B, coded as a binary variable where 1 indicates a share of affordable housing below 10 percent.[10] In addition, the model incorporates a time-varying measure of neighborhood adoption to capture potential diffusion effects, along with controls for median household income and local operating budgets.

Predictor	Coef	Std Err	Exp(Coef)
Share Rent Burdened	0.01	0.27	1.01
New Construction	-0.15	0.37	0.86
Affordability	-1.39*	0.58	0.25
Median Income	-0.00	0.00	1.00
Operating Budget	-0.00	0.00	1.00
Neighbor	0.34*	0.14	1.40
Concordance: 0.85 (Se = 0.03)			
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1			

Table 2: *Local Policy Adoption: Cox proportional hazards model estimates with county fixed effects.*

Table 2 presents the results of the survival analysis.[11] The most consistent and robust finding concerns the share of affordable housing in a municipality. If adoption were

[10]Data on the operating budget is from the Massachusetts Division of Local Services database, population data and data on the composition of the housing stock is from the American Community Survey, data on Republican vote share is from the MIT Election Lab, and data on communities' stock of low-and moderate-income housing as well as data on the adoption of Chapter 40R is from the Massachusetts Government Executive Office of Housing and Livable Communities. The Appendix includes an overview of the variables and their data sources.

[11]The analysis reports coefficients as hazard functions (column 2). The hazard records the probability of adoption at time *t* conditional on not having adopted prior to this point. Coefficients with hazard ratios lower than 1 indicate a decrease in the likelihood of adoption, while hazard ratios greater than 1 indicate an increase. For example, a hazard ratio of 1.60 means that a one-unit increase in the predictor increases the likelihood of adoption by 60 percent. Conversely, a hazard ratio of 0.60 implies that a one-unit increase in the predictor decreases the likelihood of adoption by 40 percent (Ragusa, 2010). I report additional model specifications, as well as coefficients from Ridge and Lasso regularization techniques in the Appendix.

primarily a defensive strategy in response to the threat of intervention under Chapter 40B, we would expect higher adoption rates in cities with affordable housing shares below the 10 percent threshold, the level at which the state can override local zoning decisions. However, the results indicate the opposite: municipalities with higher levels of affordable housing are significantly more likely to adopt Chapter 40R. Substantively, municipalities that meet the 10 percent affordability mandate are 24.9 percent more likely to adopt compared to those that fall below the threshold. This pattern suggests that adoption is not motivated by regulatory pressure but is instead more common in cities already engaged in affordable housing provision. An alternative specification using a continuous measure of affordability supports this interpretation. Here, each additional percentage point in affordable housing share is associated with a 22 percent increase in the likelihood of adoption.[12] This result strengthens the conclusion that 40R is taken up by municipalities already oriented toward housing reform, rather than those merely seeking to avoid state intervention. In contrast, the variables capturing housing market strain and political opposition (operationalized as the share of rent-burdened households and recent construction activity) do not exhibit statistically significant relationships with adoption in the primary models. While these dynamics may still shape policymaker perceptions, the results suggest that they are less central to explaining variation in adoption than the broader institutional and policy environment. Finally, the analysis reveals evidence of a diffusion effect: for each additional municipality in the state that has adopted Chapter 40R, the likelihood of adoption increases by 10.5 percent. This finding suggests that policy decisions are not made in isolation but are influenced by the behavior of other jurisdictions. Although the incentive appears to be most effective in municipalities already predisposed toward reform, this neighborhood effect implies that early adopters may play an important role in encouraging broader uptake over time. Taken together, the results are consistent with a view of policy change as contingent on both institutional context and local political alignment. While Chapter 40R lowers the cost of reform by offering financial support, municipalities vary in their

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[12]See Appendix for results. Alternative model specifications yield coefficients of 1.219 and 1.285.

willingness or ability to act on this opportunity.

Discussion & Conclusion

This paper shows that Massachusetts' Chapter 40R, a state policy that offers financial rewards to municipalities that increase density, increases housing supply. The aggregate effect, however, is driven by municipalities that were already more predisposed to pursue additional housing. Estimating the causal effect of housing policy is notoriously difficult, since policy change is endogenous to local political and economic conditions. To address this challenge, this paper has presented three complementary analyses that exploit distinct sources of variation. First, at the state level, a synthetic control approach constructs a credible counterfactual trajectory of housing supply in the absence of Chapter 40R, showing that the policy measurably increased overall housing production in Massachusetts. Second, at the municipal level, a staggered difference in differences design (Callaway SantAnna with not yet treated controls) demonstrates that adopting municipalities built significantly more housing after enrollment than non adopters. Finally, an event history (Cox proportional hazards) model examines the conditions under which municipalities opt into the incentive. Empirically, the study thus provides novel evidence on the effectiveness of a state land use intervention in increasing housing supply, addressing a gap in a literature that has been largely correlational and limited by the small number of state level cases (Schuetz, 2023).

Theoretically, I argue that this pattern is consistent with a political mechanism: the additional revenue provided by 40R allows municipalities to adopt what is otherwise a politically costly policy in the face of local opposition. By giving local governments access to resources they can use to compensate residents or alleviate perceived burdens, the incentive alters officials' assessment of the political risk associated with policymaking. This finding also has direct policy implications: state fiscal incentives can serve as

a meaningful tool in efforts to address the housing affordability crisis. Going forward, this case of state policy experimentation may therefore offer valuable insights for other states grappling with the ongoing housing affordability crisis. While few states have implemented land use interventions to date, Massachusetts stands out as one of the rare examples of sustained policymaking activity in this domain (Schuetz, 2023).

At the same time, the case reveals something broader about how contextual factors shape local policymakers' incentives and their calculus about whether they can afford to adopt politically costly policies. While the Massachusetts program is a specific fiscal incentive, it speaks to a more general mechanism. Institutional design is an important lever for reconciling the tension between local responsiveness and the need to confront collective challenges. Any institutional reform that relaxes local constraints whether fiscal, administrative, or political can reduce policymakers' perceived risk and thereby increase their willingness to pursue policies that may be unpopular in the short term. Municipalities routinely navigate policies that impose concentrated costs and produce diffuse benefits, and research on intergovernmental policymaking has typically focused on how higher level governments constrain local authority by controlling policymaking powers. This study expands that view by showing how institutions in this domain can also enable local policy action.

The findings suggest that institutional arrangements such as fiscal autonomy which similarly increase local access to revenue should have analogous effects, making it more feasible for municipalities to pursue policies they might otherwise avoid because of their political cost. This inference aligns with research by Krimmel, who finds the mirror image: when localities lose fiscal autonomy, they become more reluctant to take politically risky actions (2021). In this sense, the study contributes to an emerging literature on how fiscal policy design shapes local policymaking (see also Larsen and Kettel, 2025; Hilbig and Wiedemann, 2024). Moreover, the dynamics observed here complement research showing that other contextual factors, such as electoral rules that alter the distribution

of political pressure or media environments that increase the salience of policy inaction, can shift policymakers' incentives in ways that facilitate policy adoption. Together, these insights underscore how institutional context conditions the political feasibility of local policy choices.

Finally, further empirical work is needed to explore two additional dynamics: one highlighted directly by the findings here, and another that lies beyond the scope of this study but remains central to broader debates on housing policy. First, the evidence suggests that Chapter 40R's incentive was most effective in reinforcing existing commitments to affordable housing rather than overcoming entrenched opposition. In this sense, the policy primarily activated "early adopters" municipalities already inclined toward reform. This raises important questions about the broader effectiveness of incentive based state interventions: if uptake is concentrated among places predisposed to adopt pro housing measures, the returns on state investment may be limited. Second, an essential unresolved question concerns whether increased housing supply actually results in lower house prices, a critical dimension of the housing affordability crisis. While additional market rate supply may offer some relief, it is unlikely to be sufficient without complementary targeted measures. Conversely, political feasibility may favor non targeted approaches. The effectiveness of market rate construction in addressing affordability is debated (Been et al., 2019), underscoring that while new supply is necessary, it may not be sufficient on its own, and that government intervention may be crucial to ensure affordability across income levels. Ultimately, both the effectiveness of incentive based policies and their affordability impacts hinge on a better understanding of how public spending translates into measurable housing outcomes.

Data availability statement

The data that support the findings of this study as well as replication materials will be made available upon publication.

Competing interests declaration

The author(s) declare none.

Funding declaration

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Online Appendix for
"The Political Calculus of Land-Use Reform"

Table of Contents

A Synthetic Control Analysis

This section reports additional specifications for the synthetic control analyses included in the manuscript. I follow the approach described in Abadie, Diamond, and Hainmueller (2011) using the `Synth` package in R to construct the synthetic control unit and all additional analyses(Abadie et al., 2010b). I identify a set of predictor variables likely to affect housing supply outcomes, namely rate of urbanization, population size, annual personal income, personal consumption expenditure, presidential election outcomes, and racial composition of the population. I calculate predictor weights only for the pre-reform period to ensure unbiased results in constructing the synthetic unit (Vives-i Bastida, 2022). Table A1 displays the weights assigned to each of the predictor variables in modelling the synthetic control unit from the donor pool. Table A2 displays the weights assigned to those U.S. states which were assigned a weight greater than 0.000. For the outcome variable, I use annual data on housing permits for private units. I collected data outcome is housing production, measured as annual permits for all units across the 50 U.S. States, the treated unit as well as the donor pool. I include the years 1995-2019 in the observation period. In addition to data on the outcome variable, I collected data on the six predictor variables for the pre- treatment period as well as for the treatment year (1995-2004).

	v.weights	Treated	Synthetic	Sample Mean
Urbanization	0.14	874.56	804.06	693.80
Population	0.14	6250015.56	5547162.79	5494154.43
Income	0.14	219795464.56	184324902.82	156181109.08
PCE	0.04	28918.78	27014.95	22440.26
Race	0.47	801.78	768.96	739.49
Vote	0.07	0.00	0.01	0.49

Table A1: *Pre-treatment characteristics of Massachusetts, the synthetic unit, and the population-weighted average of the states in the donor pool, by predictor variable.*

Robust inference is challenging in the setting of a single treated unit. I therefore offer complementary tests analogous to the procedures introduced by Abadie et al. (2011). First, I conduct a permutation test where treatment status is assigned to untreated units

Id	State	w.weights
7	Connecticut	0.75
35	Ohio	0.03
38	Pennsylvania	0.22

Table A2: *Weights assigned to individual states from the donor pool.*

from the donor pool to enable the comparison across discrepancies (gaps) between treated and synthetic outcomes for the different units. Here, the treatment is reassigned to each unit to estimate the model for the placebo. The gaps, or the difference between each placebo and the placebo's synthetic unit, is plotted for each iteration and overlaid with the plot for Massachusetts. The permutation test visually compares the gaps between each unit and its synthetic control and allows for an assessment of how unusual the estimated effect is. If the treatment effect in Massachusetts reflects more than just noise, then the gap for Massachusetts should be larger and different from the gaps for the placebo states, meaning those states that were not treated. I have three different specifications for the permutation test. For the first specification, displayed in Figure A1, I include all states, and for the second specification, displayed in Figure A2, I include only the states from the donor pool that form the synthetic unit (states that were assigned a weight of above 0.000). Finally, Figure A3 dispplays a specification including only states from the donor pool with a good pre-reform fit, specifically, I set the threshold for the Mean Squared Prediction Error (MSPE) based on the 25th percentile of the distribution of MSPE values among the donor units (Following the approach in Abadie et al. (2011)). This means that only donor units with MSPE values in the lowest 25 percent of the distribution are selected for inclusion in the permutation test. Intuitively, this threshold means that the test includes only donor units with relatively low MSPE values, indicating a good fit.[13] Across all three specifications, the gap for Massachusetts are shown to be larger and different from the gaps for the placebo states, indicating that the treatment effect is significant.

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[13]Due to large gaps in the pre-reform period, Colorado, North Carolina, Texas, and Washington are excluded.
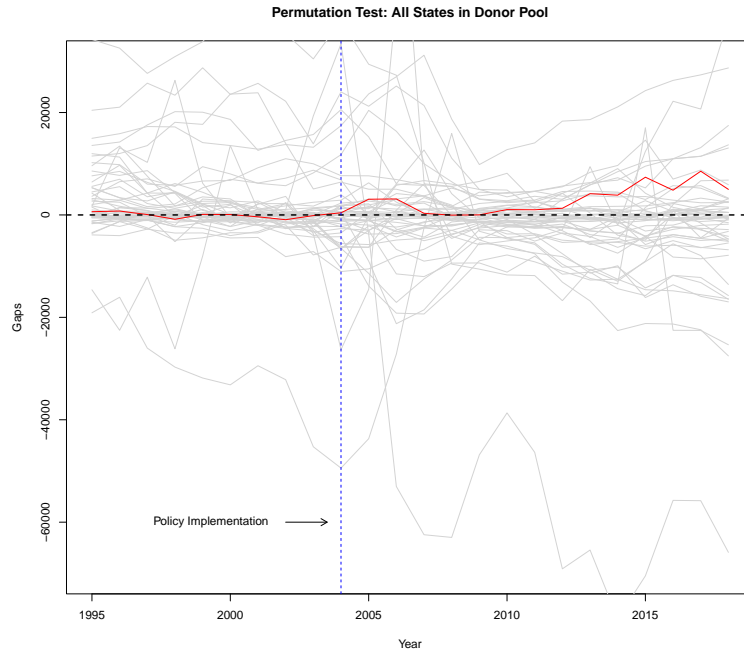
Figure A1: *Treatment effect estimates for Massachusetts and placebo units (all states in the donor pool).*
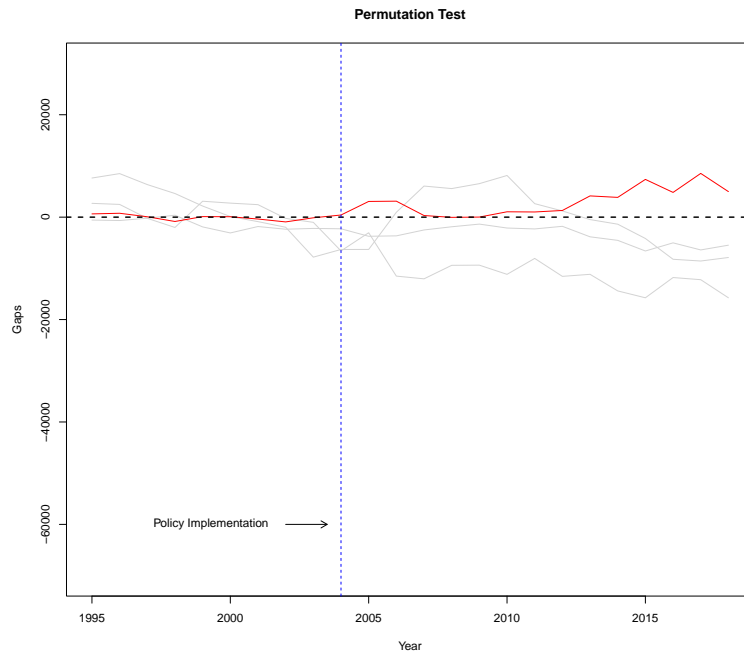
Figure A2: *Treatment effect estimates for Massachusetts and placebo units (including states that form the synthetic unit).*

Permutation Test
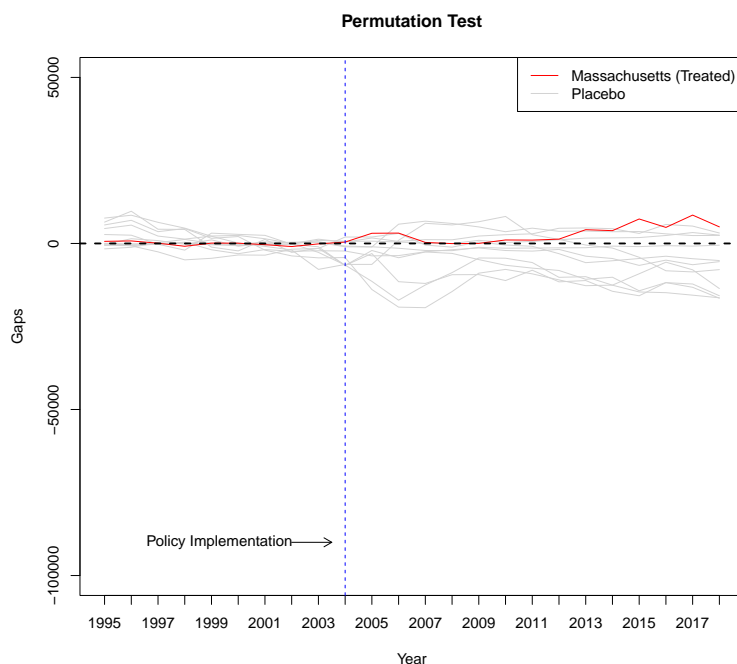
Figure A3: *Treatment effect estimates for Massachusetts and placebo units (excluding states with MSPE values above lowest 25 percent of the distribution, inclusive of donor states included in synthetic unit).*

Next, I conduct a placebo-in-time test, assigning treatment before the treatment actually occurred to ensure that the trajectories of the synthetic unit and the treated unit follow the same path beyond the assigned point in time. The placebo-in-time test is displayed in Figure A4. Assigning the treatment to 2002 allows me to assess whether the trajectories of the observed and imputed outcomes are aligned in between 2002 and 2004. The plot shows that the lines follow the same trajectory, indicating that the relationship between the treatment and the observed treatment effect is robust. These results are consistent with the trends one would expect to see if the introduction of Chapter 40R had an effect on housing outcomes.

Finally, I replicate the main synthetic control analysis while excluding Connecticut from the donor pool. As displayed in Table A2, Connecticut is assigned a weight of 0.748, which is very high. To test the robustness of the analysis, I model the synthetic unit without Connecticut.[14] Figure A5 displays the outcomes for the treated and the synthetic

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[14]Instead of only excluding the state from modelling the outcome, I run the entire analysis without
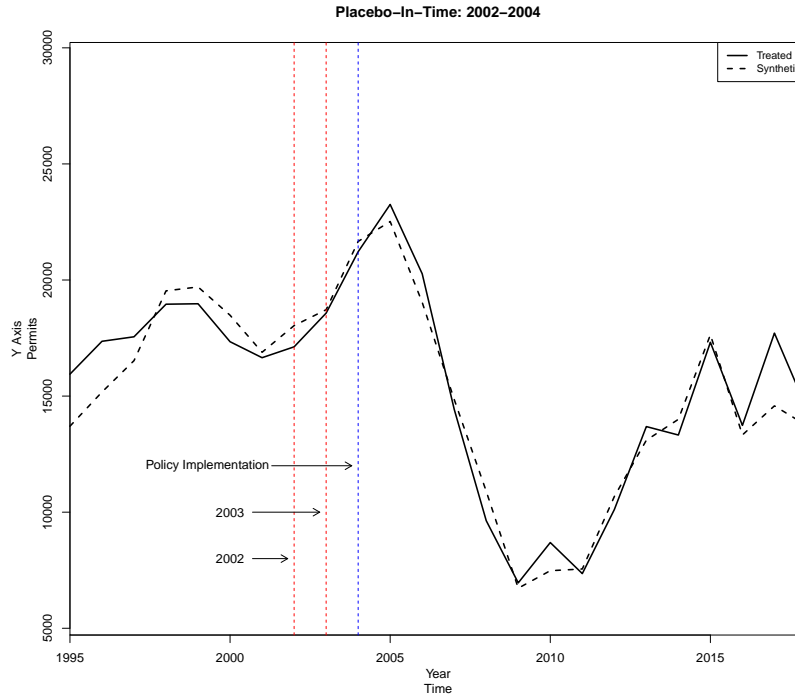
Figure A4: *Placebo-in-Time: Placebo test for observed and imputed outcomes, treatment assigned to 2002.*

unit over time. This test confirms the general pattern of the original analysis, albeit the post-reform gaps (specifically in the immediate post-reform period) are less pronounced. This in turn is likely driven by a worse pre-reform fit. Table A3 displays the weights assigned to each of the predictor variables in modeling the synthetic control unit from the donor pool without Connecticut.

	v.weights	Treated	Synthetic	Sample Mean
Urbanization	0.52	874.56	861.02	690.98
Population	0.03	6250015.56	5429336.76	5538913.10
Income	0.09	219795464.56	173072427.60	156701106.46
PCE	0.01	28918.78	24581.82	22316.63
Race	0.23	801.78	756.66	739.25
Vote	0.12	0.00	0.01	0.50

Table A3: *Pre-treatment characteristics of Massachusetts, the synthetic unit, and the population-weighted average of the states in the donor pool, by predictor variable, when exlcuding Connecticut from donor pool.*

Connecticut, which reconfigures the synthetic unit.
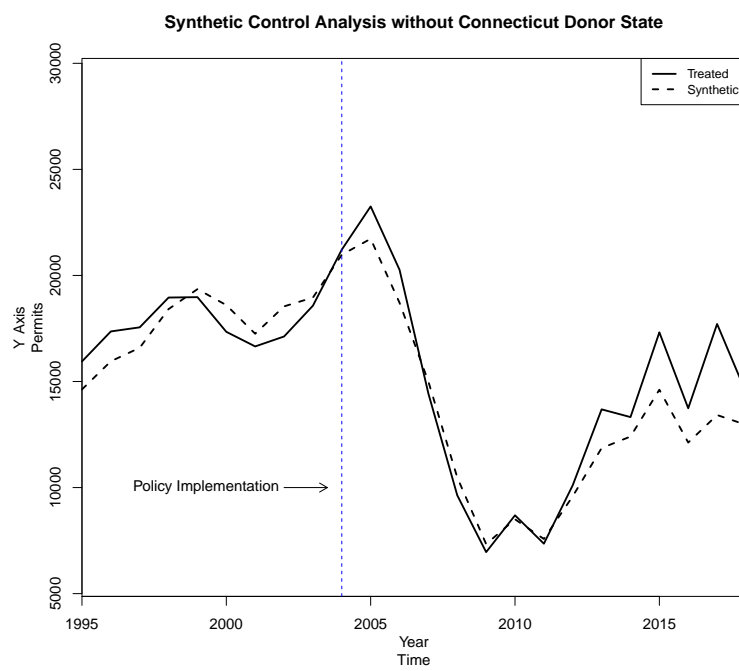
Figure A5: *Observed and Imputed Outcomes for Massachusetts, Model excludes Connecticut from Donor Pool.*

B Difference-in-Difference Estimation

This appendix provides additional detail on the DiD estimation strategy, placebo tests, and robustness checks. Results are reported using the Callaway–Sant'Anna estimator with different specifications of anticipation.

Baseline Estimates without Anticipation

	ATT	Std. Error	95% CI		Sig.
	15.06	4.21	6.81	23.31	*

Notes: Callaway–Sant'Anna DID, simple aggregation. Control group = Not Yet Treated; anticipation = 0; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A4: *Baseline: Overall average ATT across groups and times (no anticipation)*

	ATT	Std. Error	95% CI		Sig.
	14.89	4.04	6.97	22.81	*

Notes: Control group = Not Yet Treated; anticipation = 0; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A5: *Baseline: Dynamic (event-time) ATTs with 95% simultaneous confidence bands (no anticipation)*
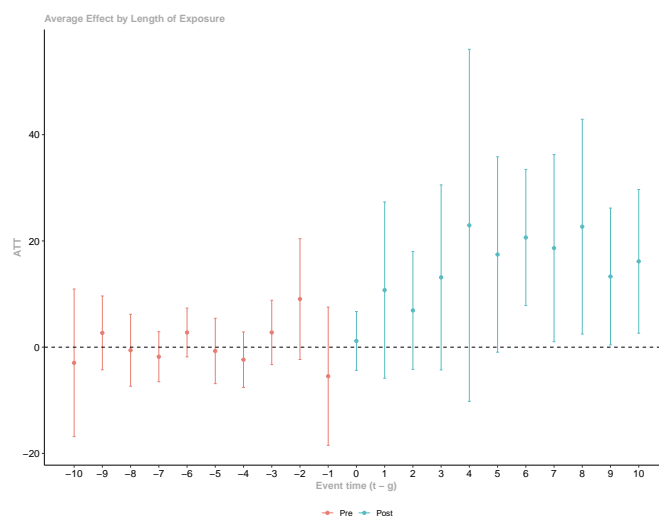
Figure A6: *Baseline event-study plot: DiD result without adjustment for anticipation.*

Placebo Test for Anticipation (Lead Falsification)

Next, I test for potential anticipation effects using a placebo design: treatment is artificially shifted $K = 3$ years earlier, and estimates are computed only on pre-treatment periods. Significant positive estimates would indicate anticipation.

Event time	Estimate	Std. Error	Lower	Upper	Sig.
-10	-2.95	5.44	-16.84	10.95	
-9	2.69	2.72	-4.25	9.63	
-8	-0.58	2.65	-7.35	6.19	
-7	-1.80	1.85	-6.51	2.91	
-6	2.76	1.80	-1.82	7.35	
-5	-0.72	2.41	-6.87	5.43	
-4	-2.36	2.05	-7.58	2.87	
-3	2.77	2.37	-3.28	8.83	
-2	9.05	4.46	-2.32	20.43	
-1	-5.47	5.10	-18.48	7.54	
0	1.18	2.17	-4.36	6.71	
1	10.74	6.50	-5.85	27.33	
2	6.92	4.35	-4.18	18.01	
3	13.14	6.82	-4.27	30.55	
4	22.94	12.99	-10.20	56.07	
5	17.45	7.21	-0.95	35.84	
6	20.65	5.02	7.83	33.46	*
7	18.64	6.90	1.04	36.24	*
8	22.69	7.92	2.48	42.90	*
9	13.30	5.05	0.42	26.18	*
10	16.16	5.30	2.63	29.68	*

Notes: Control group = Not Yet Treated; anticipation = 0; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A6: *Placebo (lead K = 3): Dynamic event-time ATTs with 95% confidence bands*

ATT	Std. Error	95% CI		Sig.
7.04	2.59	1.96	12.12	*

Notes: Placebo test with $K = 3$ leads. Control group = Not Yet Treated; estimator = Outcome Regression.

Table A7: *Placebo (lead K = 3): Overall ATT from dynamic aggregation*
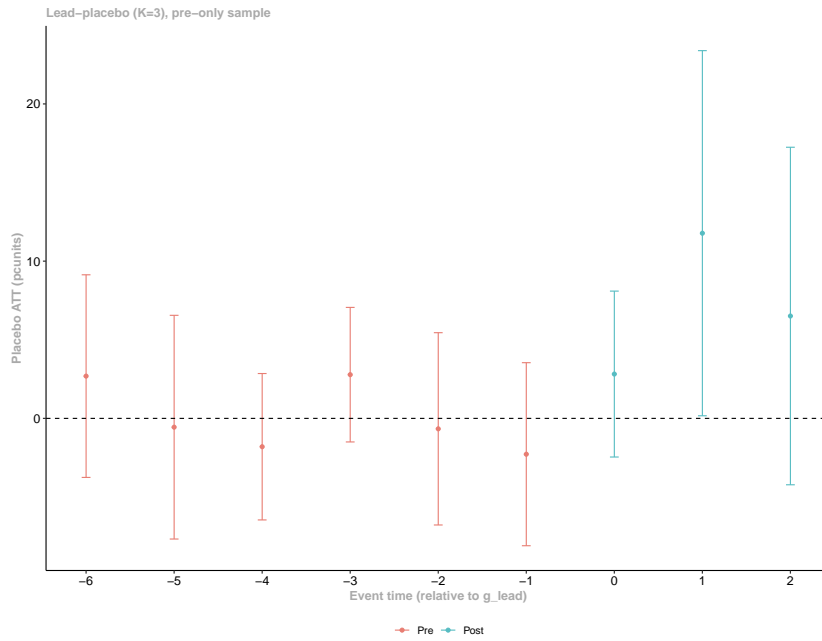
Figure A7: *Placebo event-study plot (lead K = 3). Evidence of pre-trends suggests anticipation.*

Which Years and Cohorts Drive the Placebo ATT?

To diagnose which periods and cohorts drive the placebo effect, I report dynamic and cohort-specific ATTs. Table A10 shows lead-placebo falsification: overall ATT when adoption is reassigned K years earlier and estimation is restricted to true pre-treatment periods year < actual adoption. Positive values indicate pre-adoption movement within the K-year window.

Event time	Estimate	Std. Error	Lower	Upper	Sig.
Overall (avg.)					
–	7.04	2.62	1.90	12.17	*
-10	3.98	7.24	-13.41	21.38	
-9	0.12	5.88	-14.01	14.24	
-8	4.03	7.38	-13.70	21.77	
-7	-2.92	5.66	-16.51	10.67	
-6	2.69	2.69	-3.76	9.14	
-5	-0.56	2.73	-7.13	6.01	
-4	-1.80	1.76	-6.03	2.42	
-3	2.78	1.74	-1.39	6.95	
-2	-0.67	2.42	-6.48	5.15	
-1	-2.28	2.18	-7.52	2.97	
0	2.82	2.29	-2.69	8.33	
1	11.78	4.85	0.12	23.43	*
2	6.51	4.71	-4.81	17.83	

Notes: Control group = Not Yet Treated; anticipation = 0; estimator = Outcome Regression. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A8: *Placebo analysis: event-time ATTs (which years drive the placebo ATT)*

Cohort (first treated year)	Estimate	Std. Error	Lower	Upper	Sig.
Overall (avg.)					
–	7.04	2.52	2.09	11.98	*
2003	-4.04	6.14	-18.07	9.99	
2004	5.95	5.87	-7.46	19.35	
2005	11.88	2.64	5.84	17.91	*
2006	43.90	2.54	38.10	49.70	*
2007	1.60	7.00	-14.39	17.60	
2011	5.04	1.64	1.29	8.78	*
2012	21.07	10.67	14.62	27.52	*
2014	0.86	6.77	-14.60	16.33	
2015	-4.00	3.01	-10.88	2.89	
2016	12.41	11.96	-14.92	39.73	

*Notes*: Control group = Not Yet Treated; anticipation = 0; estimator = Outcome Regression. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A9: *Placebo analysis: cohort (group) ATTs (which cohorts drive the placebo ATT)*

K	Placebo ATT
1	-5.5
2	6.3
3	7.0
4	3.0
5	1.7

Notes: Lead-placebo falsification test. ATT when adoption is reassigned *K* years earlier.

Table A10: *Lead-placebo falsification: overall ATT when adoption is reassigned K years earlier*

Event time	Number of Cohorts
-6	1
-5	1
-4	1
-3	1
-2	1
-1	1
0	1
1	1
2	1
3	1
4	1
5	1
6	1
7	1
8	1

Notes: Each event time shows how many cohorts contribute to that lead/lag.

Table A11: *Support per event time (number of cohorts contributing to each lead/lag)*

Main Estimates with Two-Year Anticipation

Given the placebo evidence, the main specification includes a two-year anticipation window. Results remain positive and statistically significant.

	ATT	Std. Error	95% CI		Sig.
	18.82	6.70	5.70	31.95	*

Notes: Control group = Not Yet Treated; anticipation = 2 years; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A12: *Main specification: Overall ATT (dynamic aggregation) with 2-year anticipation*

Event time	Estimate	Std. Error	Lower	Upper	Sig.
-10	-2.93	5.67	-17.03	11.16	
-9	2.68	2.77	-4.20	9.57	
-8	-0.56	2.70	-7.26	6.14	
-7	-1.80	1.84	-6.37	2.76	
-6	2.77	1.66	-1.36	6.90	
-5	-0.69	2.33	-6.48	5.11	
-4	-2.31	2.21	-7.80	3.17	
-3	2.82	2.30	-2.89	8.53	
-2	8.92	4.55	-2.40	20.23	
-1	-5.29	4.94	-17.58	7.00	
0	4.86	4.69	-6.79	16.52	
1	14.57	7.86	-4.96	34.10	
2	11.36	5.57	-2.50	25.21	
3	19.38	7.75	0.10	38.65	*
4	29.24	10.96	2.01	56.48	*
5	20.35	8.86	-1.67	42.37	
6	23.82	6.88	6.71	40.93	*
7	21.89	8.40	1.01	42.76	*
8	25.85	9.69	1.77	49.92	*
9	16.63	7.96	-3.15	36.42	
10	19.11	8.58	-2.20	40.42	

Notes: Control group = Not Yet Treated; anticipation = 2 years; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A13: *Main specification: Dynamic event-time ATTs with 95% confidence bands (2-year anticipation)*

Robustness Checks

I report robustness checks using calendar-time aggregation and leave-one-cohort-out estimation.

	ATT	Std. Error	95% CI		Sig.
	16.4045	6.4369	3.7883	29.0207	*

Notes: Control group = Not Yet Treated; anticipation = 2 years; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A14: *Calendar-time aggregation: overall ATT with 2-year anticipation*

Year	Estimate	Std. Error	Lower	Upper	Sig.
2006	-3.86	7.03	-20.76	13.03	
2007	-4.32	15.13	-40.68	32.04	
2008	18.31	12.38	-11.43	48.05	
2009	19.90	9.71	-3.42	43.23	
2010	23.36	13.00	-7.87	54.60	
2011	17.66	6.18	2.82	32.50	*
2012	30.67	10.67	5.03	56.31	*
2013	20.30	7.28	2.82	37.78	*
2014	20.36	7.01	3.51	37.20	*
2015	20.84	8.19	1.16	40.52	*
2016	19.28	7.69	0.82	37.75	*
2017	19.88	7.19	2.60	37.16	*
2018	15.01	6.43	-0.45	30.47	
2019	12.26	5.73	-1.51	26.03	

Notes: Control group = Not Yet Treated; anticipation = 2 years; estimator = Doubly Robust. Asterisk (*) indicates the (uniform) confidence band does not cover 0.

Table A15: *Calendar-time event-study ATTs with 95% confidence bands*

Dropped Cohort	ATT
2006	10.1
2007	16.1
2008	13.6
2009	15.3
2010	17.2
2014	15.3
2015	16.3
2017	15.1
2018	15.4
2019	15.4

Notes: Leave-one-cohort-out estimation with 2-year anticipation.

Table A16: *Leave-one-cohort-out analysis: ATT after dropping individual cohorts (2-year anticipation)*

C   Time to Event Analysis

I report the full results for the cox regression model in Table A17. To assess the robustness of the results, I additionally estimate Ridge and Lasso penalized models using the same covariates and include these coefficients here. Ridge regression addresses potential multicollinearity by shrinking coefficients toward zero, while Lasso regression both shrinks and selectively eliminates less predictive variables.

Predictor	Cox Coef	Exp (coef)	se (coef)	z	p-value	Ridge	Lasso
Share Rent Burdened	0.01	1.01	0.27	0.03	0.97	0.19	0.20
New Construction	-0.15	0.86	0.37	-0.42	0.68	-0.25	-0.15
Affordability	-1.39	0.25	0.58	-2.40	0.02	-1.50	-1.55
Median Income	-0.00	1.00	0.00	-1.87	0.06	-0.00	-0.00
Operating Budget	-0.00	1.00	0.00	-0.99	0.32	-0.00	-0.00
Neighbor	0.34	1.40	0.14	2.33	0.02	0.08	0.11

Table A17: *Full Cox Model Results with Ridge and Lasso coefficients.*

Additionally, to ensure the robustness of the results presented, I estimate a series of alternative model specifications of the cox regression model in Table A18. Model 1b is the primary model presented in the manuscript. Model 1 includes the three key predictor variables—share rent burdened, new construction, and affordability—along with the neighborhood adoption variable, while controlling for the share of owner-occupied housing (a control variable dropped in the other models), median household income, and the municipality's operating budget. Model 2 is a more parsimonious specification, including only the three key predictors without additional controls. To test the sensitivity of the results to different operationalizations of housing market conditions and local opposition, Model 3 replaces the housing burden predictor with an alternative measure of local housing market pressure, the vacancy rate. Model 4 substitutes the new construction predictor with an alternative indicator of local opposition, the share of single-family homes in the municipality. Model 5 provides an alternative operationaliza-

tion of affordability by treating it as a continuous variable rather than a binary indicator. Model 6 includes only the alternative predictors (vacancy rate, share of single-family homes, and continuous affordability), while Model 7 incorporates both the original and alternative predictors in a combined specification. Models 3 through 7 all include the neighborhood adoption variable and control for median household income and the municipality's operating budget.

The results indicate that the association between affordability and adoption remains statistically significant across specifications, suggesting that the findings are robust to different measurement choices. The coefficients for the alternative predictors provide insight into their relationships with the outcome, with the vacancy rate and share of single-family homes showing some associations with adoption but not altering the main findings. The concordance statistic across models remains relatively stable, suggesting that the predictive power of the models does not diminish substantially when alternative measures are introduced. Overall, these alternative specifications reinforce the central conclusion of the analysis: affordability is consistently and significantly associated with adoption, and the results hold across alternative model specifications.

In addition to the main models, I include both a standard logistic regression and a Firth logistic regression (a penalized likelihood based method) to account for potential bias due to rare events (Firth, 1993). These models exclude the neighborhood variable and use the continuous version of the affordability measure, with all predictors averaged across the study period. Table A19 presents a comparison of the coefficient estimates from both models. The following tables, Table A20 and Table A21 report the full regression output, including model fit statistics and additional diagnostics.

	(1)	(1b)	(2)	(3)	(4)	(5)	(6)	(7)
Share Rent Burdened	0.98	1.01	1.14	–	1.03	1.00	–	0.98
	(0.27)	(0.27)	(0.27)	–	(0.26)	(0.27)	–	(0.27)
New Construction	0.62	0.86	0.49	0.85	–	0.85	–	0.79
	(0.49)	(0.37)	(0.36)	(0.38)	–	(0.37)	–	(0.41)
Affordability	0.25*	0.25*	0.27*	0.25*	0.25*	–	–	1.83
	(0.58)	(0.58)	(0.56)	(0.58)	(0.58)	–	–	(0.94)
Vacancy Rate	–	–	–	1.01	–	–	1.02	1.11
	–	–	–	(0.30)	–	–	(0.30)	(0.33)
Share Single Family	–	–	–	–	1.83	–	1.84	2.03
	–	–	–	–	(0.79)	–	(0.82)	(0.84)
Affordability (alt)	–	–	–	–	–	1.22***	1.22***	1.29*
	–	–	–	–	–	(0.06)	(0.06)	(0.10)
Share Owner-Occupied	1.54	–	–	–	–	–	–	–
	(0.42)	–	–	–	–	–	–	–
Median Income	1.00*	1.00.	–	1.00.	1.00.	1.00.	1.00.	1.00.
	(0.00)	(0.00)	–	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Operating Budget	1.00	1.00	–	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	–	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighbor	1.43*	1.40*	–	1.40*	1.37*	1.41*	1.38*	1.38*
	(0.15)	(0.14)	–	(0.14)	(0.14)	(0.14)	(0.14)	(0.15)
Concordance	0.85	0.85	0.70	0.85	0.84	0.86	0.86	0.86
	(0.04)	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1   Standard errors in parentheses.

Table A18: *Alternative Model Specifications for Cox Regression Results. Table reports the Hazard Ratios (Exp (Coef).*

Predictor	Logistic Model	Firth Model
Intercept	-9.61 (6.04)	-9.32 (5.55)
Share Rent Burdened	0.16. (0.10)	0.16. (0.09)
New Construction	4.19*** (1.14)	3.92*** (1.03)
Affordability	0.16* (0.07)	0.15* (0.06)
Median Income	-0.00** (0.00)	-0.00** (0.00)
Operating Budget	-0.00 (0.00)	0.00 (0.00)

Significance codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $\cdot$ $p < 0.1$ Standard errors in parentheses.

Table A19: *Comparison of Logistic and Firth Model Coefficients*

Variable	Estimate	Std. Error	z value	Pr(> $|z|$)
(Intercept)	-9.61	6.04	-1.59	0.11
Share Rent Burdened	0.16	0.10	1.67	0.10.
New Construction	4.19	1.14	3.68	<0.01***
Affordability	0.16	0.07	2.44	0.02*
Median Income	-0.00	0.00	-2.96	0.00**
Operating Budget	-0.00	0.00	-0.25	0.80

Table A20: *Full Logistic Regression Output. Dependent variable: municipal adoption of Chapter 40R.*

Variable	Estimate	Std. Error	95 percent CI Lower	95 percent CI Upper	Chi-sq (χ^2)
(Intercept)	-9.32	5.55	-20.98	2.06	2.59
Share Rent Burdened	0.16	0.09	-0.03	0.34	2.71
New Construction	3.92	1.03	1.95	6.26	16.62
Affordability	0.15	0.06	0.03	0.28	5.63
Median Income	-0.00	0.00	-0.00	-0.00	10.88
Operating Budget	0.00	0.00	-0.00	0.00	0.07

Note: Estimates based on Firth logistic regression with profile penalized log-likelihood.

Table A21: *Firth Logistic Regression Output. Dependent variable: municipal adoption of Chapter 40R.*

D Data and Descriptives

This section provides additional detail on the data sources and variables used in the analyses. It also presents descriptive information on key municipal characteristics and trends in the outcome variable at the municipal level over time.

Data Sources

Data on housing permits comes from the Census Bureau and is collected from the "Historic Permits by State" archive (`https://www.census.gov/construction/bps/historical.html`). I utilize the monthly data on new privately owned housing units authorized, and calculate annual values for each state. Data on state population counts (`https://www2.census.gov/programs-surveys/popest/tables/`), on urbanization rates, and on the racial composition of the population comes from the Census Bureau. Urbanization (`https://www.icip.iastate.edu/tables/population/urban-pct-states`) is here operationalized as the percentage of the total population in urbanized areas and urban clusters. Racial composition (`https://www.census.gov/data/tables/time-series/demo/popest/2010s-national-detail.html`) is calculated as the percentage of the population that is white. Due to missing data, values for 1994-2000 are based on the recorded value for 2000, and values for 2001-2009 are based on the recorded value for 2010. Data on personal consumption expenditures and on state annual personal income comes from the U.S. Bureau of Economic Analysis. Personal consumption expenditures by state (`https://apps.bea.gov/regional/histdata/releases/1215pce/index.cfm`) are a measure of goods and services purchased by or on behalf of households, and provide insight into the regional economy. Values are reported in current dollars. Due to missing data, the values for 1994-1996 are based on 1997 data. State annual personal income (`https://apps.bea.gov/regional/histdata/releases/0910spi/index.cfm`) is the income received by all persons from all sources, and is reported in current dollars. Data on state-level voting outcomes comes from the MIT Election lab (`https://electionlab.mit.edu/`) and is based on

popular votes for the Republican and Democratic candidates in presidential elections. For years without a presidential election, values were imputed from the most recent election.

For municipal-level data, data on housing permits comes from the Metropolitan Area Planning Council (`https://datacommon.mapc.org/browser/datasets/384`). Data on the operating budget, municipal population, and intergovernmental transfers is from the Massachusetts Division of Local Services database (`https://www.mass.gov/info-details/division-of-local-services-municipal-databank`), and data on the composition of the housing market is from the American Community Survey (`https://data.census.gov/table?q=DP04`). For data from the American Community Survey, values from 2010 were applied to all observations from 2002 to 2009. For the municipal operating budget per capita, values from 2004 were used for 2002 and 2003, while net state aid was imputed using 2013 values for the years 2002–2012 and 2014–2022. For variables capturing housing units, land area, and housing density, values from 2009 were used for all other years. Data on communities' stock of low-and moderate-income housing as well as data on the adoption of Chapter 40R is from the Massachusetts Government Executive Office of Housing and Livable Communities (`https://www.mass.gov/orgs/executive-office-of-housing-and-livable-communities`). Missing values for low- and moderate-income housing stock were imputed using the previous year's value for the years 2015–2016, 2018–2019, and 2021–2022. Data on voting outcomes comes from the MIT Election lab (`https://electionlab.mit.edu/`).

Descriptive Statistics

Table A22 displays average per capita permits per year among adopters and non-adopters (remainers), as well as absolute and cumulative adoptions over time. The adopter and non-adopter groups in a given year are made up of those municipalities that have (not yet) adopted in the current or a previous year. Table A23 presents descriptive statistics for units permitted at the municipal level (absolute values) over the time period 1995-

2019. Table A24 compares average characteristics across adopting and non-adopting municipalities, as well as the overall sample, using data averaged across all years.

	Counts			Means (total units)		Means (per capita)	
Year	Adopt	Cumulative	Remaining	Adopters	Remain	Adopters	Remain
1995	0	0	347	NA	47	NA	47
1996	0	0	347	NA	49	NA	49
1997	0	0	347	NA	49	NA	49
1998	0	0	347	NA	55	NA	52
1999	0	0	347	NA	54	NA	53
2000	0	0	347	NA	52	NA	48
2001	0	0	347	NA	49	NA	46
2002	0	0	347	NA	50	NA	52
2003	0	0	347	NA	58	NA	51
2004	0	0	347	NA	64	NA	55
2005	0	0	347	NA	70	NA	53
2006	5	5	342	30	56	18	41
2007	7	12	335	57	43	21	37
2008	12	24	323	76	25	20	26
2009	1	25	322	47	21	15	19
2010	4	29	318	56	23	21	21
2011	0	29	318	58	19	11	17
2012	0	29	318	122	24	26	19
2013	0	29	318	144	32	20	24
2014	1	30	317	160	30	21	25
2015	2	32	315	212	33	23	25
2016	0	32	315	159	35	22	26
2017	4	36	311	194	34	23	25
2018	3	39	308	143	37	21	27
2019	4	43	304	122	39	19	NA

Table A22: *Municipal Adoption - Adoptions and average permits by year*

Year	Mean Units	Min Units	Max Units
1995	46.8	0	336
1996	49.2	0	379
1997	49.0	0	333
1998	54.9	0	757
1999	54.0	0	1147
2000	51.3	0	575
2001	48.5	0	883
2002	49.8	0	772
2003	57.7	0	1508
2004	64.0	0	1079
2005	69.9	0	1156
2006	55.8	0	2419
2007	43.8	0	1249
2008	28.2	0	513
2009	22.6	0	457
2010	25.9	0	386
2011	22.0	0	785
2012	31.7	0	1776
2013	41.5	0	2561
2014	41.3	0	2841
2015	49.6	0	4955
2016	46.4	0	3348
2017	50.5	0	5085
2018	48.6	0	3602
2019	49.5	0	2993

Table A23: *Summary Statistics: Housing Units Authorized per Municipality, by year.*

Predictor	Non-adopter	Adopter	Sample
Net Aid Receipt	8.558.446	9.577.210	8.615.395
Operating Budget	3.315	2.716	3.234
Population	643.02	557.83	637.25
Unemployment Rate	5.25	5.41	5.27
Median Age	40.9	40.6	40.9
Share Population over 65	15.6	15.0	15.6
Share African Americans	4.47	4.50	4.48
Share Uninsured	1.55	1.03	1.52
Median Income	69.220	64.148	68.877
Share in Poverty	9.59	9.99	9.61
Republican Vote Share	34.8	34.5	34.8
Total Buildings	23.3	51.2	27.1
Total Units	270.18	231.78	267.58
Housing Per SQM	491	659	503
Total Area	25.3	23.8	25.2
Land	21.9	21.9	21.9
Median Home Value	335.386	309.273	333.619
Median Rent	1.056	963	1.049
Share Affordable Housing	4.96	7.50	5.30
Rent Burden	50.3	51.5	50.3
Share Overcrowded	0.421	0.360	0.417
Share New Construction	1.25	1.39	1.26
Rental Vacancy	5.02	5.34	5.04
Share Owner-Occupied	67.6	67.8	67.6
Single Family Buildings	22.0	45.9	25.2
Share Single Family Home	59.2	58.3	59.2
Share Attached Single Family Homes	4.65	4.58	4.64
Multifamily Buildings	1.37	5.32	1.90
Multifamily Units	14.0	35.6	16.9
Share Mobile Homes	1.20	1.44	1.22
Share Buildings 2 Units	9.38	10.2	9.43
Share Buildings 3-4 Units	9.07	8.98	9.06
Share Buildings 5-9 Units	5.27	5.29	5.27
Share Buildings 10-19 Units	3.57	3.62	3.58
Share Buildings over 20 Units	7.59	7.52	7.59

Table A24: *Comparison of Municipal Characteristics by Adoption Status (adopters, non-adopters and the sample overall). Values are averaged across all years.*