Reclaiming Local Control:
School Finance Reforms and Housing Supply Restrictions

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Abstract

Local policies preventing new home construction have severe negative spillover effects on housing affordability, geographic mobility, and macroeconomic growth. This paper examines the loss of local control over public good financing as a new causal channel behind the rise of such housing supply restrictions. I develop a model of local public good provision, showing that without the ability to price a public good like education (using property taxation), localities use quantity controls (housing supply restrictions) to prevent congestion and attract high-wealth households. I empirically test the new mechanism by exploiting California's mid-1970s landmark school finance equalization, which prevented jurisdictions from increasing property taxes to meet their desired level of education spending. Using linked historical data on school district finances, housing, and municipal land use policies, I show in a difference-in-differences framework that school districts with larger exclusionary motives–those that benefited most from local control of education funding–enacted more stringent land use controls and built less housing after the reforms. I find suggestive evidence of the exclusionary effects of supply restrictions: house prices increased and minority population share decreased in wealthy school districts constrained by the equalization. These findings have implications for the unintended effects of fiscal federalism on the housing market, namely that fiscal policy affects new development in the short run and the urban form in the long run.

Keywords: Land Use Restrictions, Local Public Finance, Fiscal Policy, Housing Supply, Education Finance

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1 Introduction

Local regulations preventing new home construction are at the root of some of the most urgent issues facing advanced economies today. These housing supply restrictions cause housing shortages and affordability issues for metropolitan areas, exacerbate existing geographic inequality and racial segregation within cities, and reduce aggregate productivity and growth at the macroeconomic level.[1] Explicit supply regulation is not endemic to the urban political economy either; unlike municipal zoning ordinances which first appeared in the US in the early 20th century, housing supply controls came to prominence only in the 1970s.[2] Though supply restrictions are a relatively new phenomenon, they have become widespread in the last 40 years.

Unlike their consequences, the causes of restrictive housing supply policies and their rise over time are not well established. From a theoretical standpoint, endogeneity between supply controls, house prices, and household sorting confound the aims of supply restrictions. Empirically, a dearth of natural experiments to address the endogeneity plus the lack of longitudinal regulation data has prevented researchers from identifying local governments' motivations for limiting new housing.

In this paper, I propose and test a new causal channel behind the widespread adoption of housing supply restrictions: the loss of local control over property taxation. This erosion of local autonomy isolates localities' desire to maintain quality public goods as a particular motive for enacting supply restrictions. I first show in a model that without the ability to price a congestible local public good like education, localities turn to coarser quantity controls (housing supply restrictions) to prevent overcrowding and attract high-wealth households. I then test the empirical relevance of this channel by exploiting California's mid-1970s landmark school finance equalization as a natural experiment. Leveraging new panel data on regulation and housing outcomes, I overcome the typical measurement and identification challenges and show that high-spending localities benefiting most from decentralized school finance are more likely to enact stringent housing supply controls in response to losing local control.

Drawing on seminal local public finance literature (Tiebout, 1956; Hamilton, 1975; Ellickson,

[1] On housing shortages and affordability issues, see for example Glaeser & Gyourko (2003, 2018), Turner et al. (2014) on the local welfare consequences. On geographic inequality and segregation, see for example Trounstine (2018, 2020), Shertzer et al. (2016, 2018, 2021). On macroeconomic effects, see for example Hsieh & Moretti (2019), Duranton & Puga (2019)

[2] In California, the most supply-constrained state today (Gyourko et al., 2006; Gyourko et al., 2021), just 5 percent of its local municipal governments had adopted a supply restriction by 1970. This rate increased nearly five-fold to 24 percent by the end of the 1970s and more than doubled again to 58 percent by 1990 (see Appendix Figure A1).

1971), my model illustrates why communities might adopt stringent land use policies in response to losing funding authority over a valuable public amenity like education. In a system with local control, local governments rely on both fiscal and land use policy to provide quality public goods. These policy levers jointly impact local revenues and the size or density of the jurisdiction. Since education is a congestible local public good, for a fixed per capita funding level, quality declines as the population increases (Wildasin, 1987).[3] Fiscal policy (property tax rates) directly impacts local revenues, while indirectly affecting enrollments (through its function as the price of education). Conversely, land use policy (housing supply restrictions) directly impacts density and enrollments, while indirectly affecting local revenues (through their effect on house prices).

Without fiscal autonomy, however, localities lose not only the ability to improve public good quality through additional tax revenue, but also the market-like benefit of screening households by flexibly setting entry fees. Unable to increase revenues, the quantity and characteristics of students and neighbors becomes more salient to incumbent residents. Revenue constrained districts therefore reclaim local control over school quality by adopting housing supply restrictions. These regulations function as brute force quantity controls while indirectly affecting access fees, as supply restrictions increase house prices.[4] The resulting price increases may have racially exclusionary effects, as minority households historically tend to have considerably less wealth than white households.[5]

To explore this mechanism empirically, I exploit California's 1973 school finance equalization reform as a natural experiment. Before the reform, localities simultaneously set education funding, property tax rates, and land use policy. Under the reform, all school districts lost fiscal autonomy over education spending and property tax rates. To equalize funding across school districts, California's state government set annual per-pupil revenue limits for each district, preventing jurisdictions from choosing their desired level of education spending. The reform adjusted these limits in a progressive fashion over time, such that funding for wealthy districts

[3] In other words, additional resources on a *per capita basis* are required if one is to expand the population (number of students) provided education in a given jurisdiction while keeping the quality of education constant (Wildasin, 1987 p. 1135)

[4] This is theoretically consistent and analogous to the "ticket" price of Banzhaf and Mangum's (2019) two-part tariff model for how local amenities are capitalized into house prices. They find that most variation in house prices across neighborhoods can be explained by the access fee household pay to enter a neighborhood and consume local public services; moreover, "ticket prices" or access fees increase sharply as land use regulations become more stringent.

[5] Urban Institute calculations from Survey of Financial Characteristics of Consumers 1962 (December 31), Survey of Changes in Family Finances 1963, and Survey of Consumer Finances 1983-2016. https://apps.urban.org/features/wealth-inequality-charts/

was constrained as poorer districts caught up (Sonstelie et al, 2000). In order to cross-subsidize poorer districts, wealthier districts essentially had their funding levels frozen in place in real terms. The reform was intended to enhance equity, but the lack of flexibility over education financing created an incentive for previously high-tax, property-rich districts to prevent new families from moving in.

In practice, the school finance equalization introduced exogenous temporal variation in local control over education revenues and cross-sectional variation in terms of which districts were most helped or hurt by the policy change. This allows me to test whether localities enacted housing supply controls in response to losing autonomy over local public good financing. Using both administrative and survey data, I create a novel school district level panel dataset linking local fiscal data, municipal housing supply regulations, and housing and demographic characteristics. I merge newly-digitized district fiscal policy records with hand-collected revenue equalization schedules to determine which school districts were constrained by the school finance reform. Using a difference-in-differences framework, I then compare constrained versus unconstrained districts before and after the equalization reform on housing market and sorting outcomes, including housing supply restrictions, new construction, house prices, and racial composition.

I find that school districts with larger exclusionary motives – those constrained by the reform's revenue limits–enacted more housing supply restrictions and built fewer new homes in the short run. Fiscally constrained locales were differentially 66 percent (5.8 ppt) more likely to adopt a supply restriction in the five years following the implementation of the 1973 school finance reform.[6] These districts also constructed about 1% fewer housing units per year (as a share of pre-period stock) over this same time frame. These two results suggest the loss of local control over education funding prompted wealthier localities to use quantity controls in order to maintain higher quality schools.

In a heterogeneity analysis, I find the effect sizes on supply constraints are even larger for higher-income and highly-educated localities. These types of school districts were perhaps motivated both by the fear of overcrowding and by the desire to maintain wealthier, more highly educated neighborhoods. Importantly, the results on housing show no evidence of pre-trends and are robust to the exclusion of major coastal metro areas known for their natural amenities and

[6] All main results are in the short run (5 years, 1973-1978). These short-run effects precede the passage of Proposition 13, a major property tax limitation, which only further constrained local autonomy over taxation and school funding. See section 3.2 for a further discussion of Proposition 13.

high land values. The results and trends are specifically due to localities' pre-reform reliance on fiscal policy, rather than the natural correlation between property values and school spending. In a placebo test, I show there is no relationship between a jurisdiction's pre-reform assessed property value per-pupil and its likelihood of adopting a land use control after the reform.

As a result of the adoption of the supply restrictions, the wealthier, fiscally constrained school districts also became more socioeconomically and racially exclusive. In the short run (5 years), house prices in constrained districts increased 7 percent relative to unconstrained districts. Consistent with the house price increases (given correlations between race and wealth/income), Black and Hispanic population share differentially fell 17 percent (3.5 ppts) in constrained districts following the reform.

The loss of local control over public good financing significantly changed the supply side of the housing market in the short run. I also find suggestive evidence for long run consequences. In the 20 years following the school finance equalization, wealthier, fiscally-constrained districts continued to adopt additional housing supply restrictions. Differential changes in house prices and racial composition persisted and even further widened, an indication of the lingering effects of restrictive land use controls.

This is the first paper to my knowledge to quasi-experimentally identify a source of a statewide increase in land use regulations over time. The temporal and cross-sectional variation generated by the school finance reform exactly match the initial rise of land use regulations in California both in terms of timing and geography. This new causal channel can help explain the origins of restrictive land use policy in the most supply constrained state today. By exploiting California's school finance equalization, this paper also underscores that the *system* of local public finance is important for understanding not only the theoretical relationship between local public good provision and land use controls, but also the widespread empirical increase in housing supply regulations over time. In particular, the centralization of school finance and the loss of local control over education revenue magnified congestion and peer effect concerns. My findings highlight that fiscal federalism and centralization in one market (education) has persistent unintended consequences on another (housing) even for the same geographic area. Because of the durability of housing, short-run changes to the supply side of the housing market – whether from new construction or policies that prevent it – have long-run impacts on the city growth, house prices, and residential sorting.

The central finding of this paper – that localities benefiting most from local control over public education financing quickly adopted housing supply restrictions after losing fiscal autonomy – informs three distinct literatures at the intersection of urban, housing, and public economics. First, a long literature in urban and housing economics is concerned with identifying the empirical determinants of land use controls (Molotch (1976); Bogart (1993); Evenson and Wheaton (2003); Glaeser & Ward (2009); Shertzer et al (2016)). Much of the literature in urban and housing economics concludes incumbent residents (homeowners) adopt land use controls to maintain stable home values and preserve neighborhood characteristics.[7] My proposed causal channel is a refinement of these hypotheses, offering a better-identified mechanism. Localities and homeowners in part maintain value through the stability of quality local public goods. When a rivalrous public amenity like schools faces an increased threat of congestion, localities react by restricting future housing supply.

Second, understanding the interactions between local public goods, fiscal policy, and land use controls has been an important topic at the intersection of local public finance and urban economics for almost fifty years (Mills & Oates (1975), Buchanan and Goetz (1972), Hamilton (1975), White (1975)). Yet empirical research on these endogenous relationships is "virtually non-existent" (Blochiger et al, 2017). This paper provides some of the first empirical evidence on the effects of public good revenues on land use controls (see also Lutz (2015) on the connection between property taxes and new home construction).

Third, my empirical setting has implications for better understanding the full picture of fiscal federalism type policy interventions (Oates (1972); Brunner & Sonstelie (2006); Weingast (2009)). Work in this area is often concerned with efficiency gains or losses from centralizing vs. decentralizing public good provision (Ellickson, 1971, for example). Decentralized authority over public good provision produces wide variation in taxes and public good quality, offering allocative efficiency gains (e.g. Ellickson, 1971) at the expense of horizontal equity (Berne & Stiefel, 1999). The resulting inequities, have prompted higher levels of government to intervene using fiscal federalism (Oates 1972, 1999). Still, we cannot understand the full impacts of these interventions reforms without considering that localities might react to losing autonomy over one policy lever by reclaiming control using another. My findings highlight the potential for adverse unintended consequences of fiscal federalism interventions, particularly in the housing market. Finally, the paper's insights also relate to work on school finance reforms and their

[7] see for example: Fischel (1985, 2001), Glaeser et al. (2005), Hilber & Rober-Nicoud (2013), Ortalo-Magne & Prat (2014), Hall & Yoder (2019), Bogart (1993)

effect on house prices (Hoxby & Kuziemko (2004); Lutz (2009); Bayer et al. (2020); Chakrabarti & Roy (2015).

The paper proceeds as follows: section 2 establishes a model connecting school finance and housing supply restrictions; section 3 introduces the institutional setting and details the school finance reform; section 4 describes the empirical design, including the data and identification strategy; section 5 provides the results; and section 6 concludes.

2 Model of Local Land Use and School Finance

The purpose of this stylized model is to relate systems of school finance to local land use policy. These systems may either be decentralized with local control its local education budget and property property taxes, or centralized with state control over school funding. Compared to the decentralized case, land use becomes more restrictive (lower density) under a centralized funding regime, where education revenue is equalized across locations. This increase in housing supply restrictions is driven by wealthier localities, whose per-pupil education revenue is constrained under the equalization relative to under local control.

2.1 Set Up

The model below combines elements of Ellickson's (1971) theory on jurisdictional fragmentation and the benefits of fiscal decentralization with Diamond's (2017) model of local government public good quality and housing supply.

The set up of the stylized model is as follows:

Let there be a single metropolitan area M with two municipalities indexed by $j = \{1, 2\}$. Municipalities' objective is to maximize local public school quality denoted by Φ_j (one can think of this being done by a representative median voter or by a municipal government). The two municipalities are identical in geography and natural amenities.

School quality (Φ_j) is a function of per-pupil spending (s_j). Under local school finance, s_j is determined by local property tax revenues; under centralized school finance, s_j is set by the state and its level is constant across municipalities. At this constant level, it will be denoted as $\overline{s}$.

Local public education is a congestible public good, meaning that as the number of consumers (students) increases, overall quality for all will marginally decrease. This congestion externality can be offset by increases in per-pupil spending or by reductions in the number of students. In economic terms, local public education is non-rivalrous only up to a point. Examples of congestion could be larger class sizes, which lower the quality of education for all (on average). At some point, the municipality may have to build an additional school because of overcrowding, so the costs of congestion are both non-pecuniary and pecuniary.

Importantly, because of the congestibility, school quality also depends on the number of students, or equivalently the density of homes in the municipality. Following Ellickson (1971), congestion enters through the municipality's budget constraint. The cost of providing public education services of quality $\Phi(s)$ to a population N is sN. The municipality's total revenue is then $\tau r D$, where τ is the property tax rate, r is the price of housing, and D is the amount of residential land (identical for all municipalities). Formally then, the total budget constraint is $sN = \tau r D$. In other words, the total cost of providing education of quality $\Phi(s)$ to N students is equal to the municipality's total tax revenue $\tau r D$.

In addition to setting fiscal policy (property tax rate τ), municipalities also choose land use policy. Let the land use policy parameter (γ) set the residential housing density requirement for the municipality, where $\gamma = \frac{D}{N}$. Then the budget constraint can be re-written as $s = \tau r \gamma$.

Land use parameter γ is equivalent to the number of acres per home, making it akin to a minimum lot size requirement. Because γ sets the density requirement in the municipality, it in turn affects congestion and school quality. The price of housing r is given by the following housing supply function: $r = \gamma N$. House prices are therefore increasing in γ and decreasing in density.[8]

Under the local finance regime, municipalities maximize (congestible) school quality by choosing property tax rate (τ) and housing supply regulation (γ)

$$\begin{aligned} \max_{\tau,\gamma} \quad & \Phi(s) \\ \text{s.t.} \quad & s = \tau r \gamma \end{aligned} \tag{1}$$

[8] Also, all houses are identical and are owned by absentee landlords. As in Diamond (2017), houses are sold at the marginal cost of production, then rented to households by the absentee landlords; house prices paid are then the present discounted value of rents.

The metropolitan area also contains households, who are identical in preferences but heterogeneous in wealth ($w = \{H, L\}$). For simplicity, let half of the households be rich (high wealth, $w = H$) and half poor (low wealth, $w = L$). Wealth is exogenously determined and is essentially an endowment. There is no labor market in the model, so there are no firms or commuting. In this way, the spatial distance between the two municipalities is irrelevant and I assume households can move costlessly from one municipality to the other.

Household utility is given by:

$$U_i = w_i - (1 + \tau_j)r_j + \Phi_j$$

In words, utility for household of wealth type i is equal to the after tax price of housing in municipality j plus the benefit of school quality in j.

In equilibrium, households are indifferent between locations, so

$$U_i = w_i - (1 + \tau_j)r_j + \Phi_j = \overline{U}$$

Households take the behavior of the municipalities as given and choose a place to live weighing the quality of local schools against the after tax cost of living.[9]

The model evolves over two school finance regimes: decentralized (period 1) then centralized (period 2). These regimes occur sequentially such that municipalities' and households' decisions under the decentralized regime carry over into the initial state of the centralized regime. However, the regime change is an exogenous shock not foreseen by municipalities or households, so there are no dynamic elements other than the outcome of period 1 setting the initial conditions for period 2.

2.2 Decentralized Regime: Local School Finance

Under local school finance, municipalities have two levers to provide quality public education – set the cost of access: (1) fiscal policy (τ), which determines property tax revenues collected (τr); and (2) land use policy (γ), controls density and population growth (N)

Figure 1 shows the equilibrium of local public good quality and pricing under decentralized school finance. The diagonal line represents the municipal budget feasibility set where quality

[9] For simplicity, assume all households have exactly 1 school age child that must attend the local public schools.

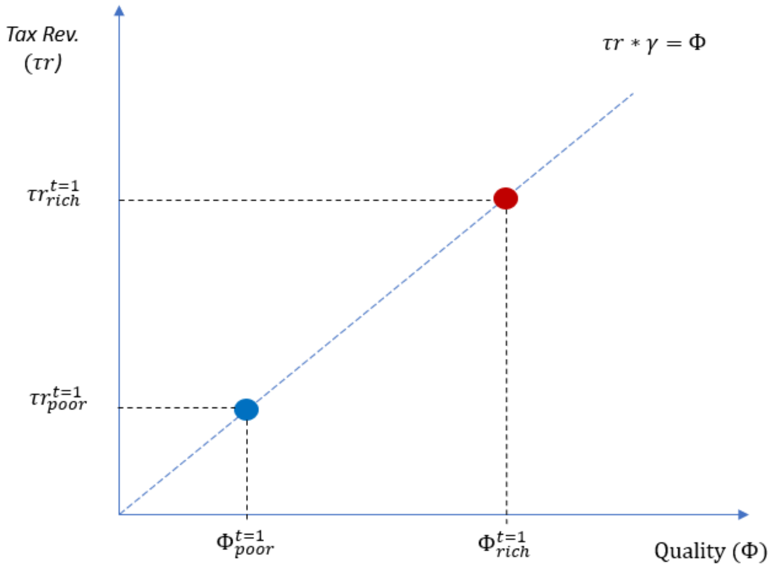


Figure 1: School quality and per-pupil tax revenue under local control

(Φ) is equal to per-pupil tax revenue (τr) scaled by the density control parameter (γ). The slope of the feasibility set then depends on γ. In the simplest case as shown, both municipalities choose the same density control parameter $(\gamma = 1)$. Each municipality then faces the same feasibility set and quality is exactly equal to the per-pupil tax revenue raised $(\Phi = \tau r)$. The figure shows the result of perfect sorting by wealth along the budget feasibility set. The blue and red dots represent the municipalities containing low wealth and high wealth individuals, respectively. At a given γ, higher quality public schooling requires higher revenues; the locality with high-wealth households raises more local revenue and thus has higher-quality schools. This equilibrium is stable, as wealthy households' utility gains from higher quality schools are exactly offset by the higher after-tax price of housing. Under the decentralized regime, municipality wealth, house prices, property taxes, and school quality are collinear. In the general case with many municipalities and a continuum of wealth levels, τ is pinned down by household demand. While raising τ will increase quality, holding γ constant, it also increases the after-tax price of housing.

2.3 Centralized Regime: School Finance Equalization

In the second period, the state government exogenously imposes a funding equalization reform, such that each municipality receives the same revenue per student $(\bar{s})$ regardless of the municipalities' previous or current tax base. The equalization is represented by the horizontal green bar in Figure 2. All else equal, school quality will converge across municipalities in the long run at $\Phi_{poor=rich}^{t=2}$ (Figure 2, left panel). The equalization level $\bar{s}$ acts as a binding revenue constraint for the wealthier municipality.

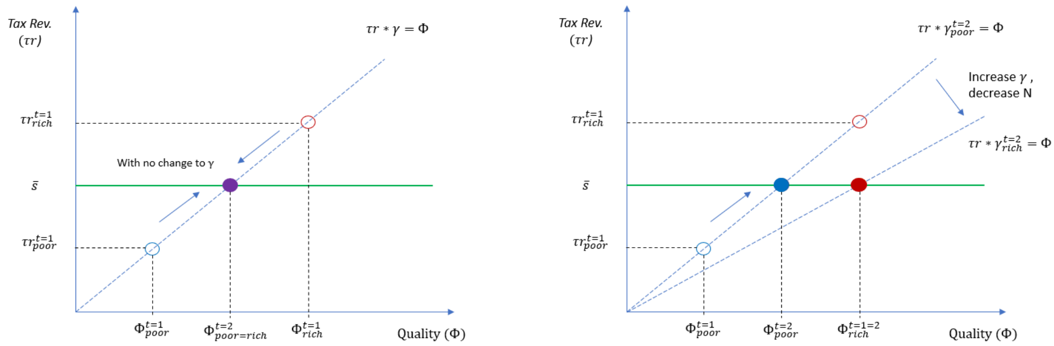


Figure 2: School Finance Equalization ($\bar{s}$) Increases Housing Supply Restrictions (γ)

Faced with the prospect of a decline in quality from $\Phi^{t=1}_{rich}$ to $\Phi^{t=2}_{poor=rich}$ due to the imposition of a real revenue constraint, the wealthier municipality will choose to increase γ, thus flattening its budget feasibility set. This will reduce congestion by reducing N in the long run. As shown in the right panel of Figure 2, increasing γ and flattening the budget set allows the wealthier municipality to maintain its pre-reform quality level despite operating with fewer real dollars per-pupil. The size of the γ increase depends on the degree to which the municipality's funding is constrained relative to its pre-reform optimum. Graphically, that means it depends on the both the level of $\bar{s}$ and initial quality level $\Phi^{t=1}_{rich}$.

This stylized example show the loss of flexibility over education pricing results in municipalities instead controlling the quantity margin. The policy instrument at their disposal is of course housing supply restrictions. These restrictions function as brute force quantity controls while indirectly affecting access fees, as supply restrictions increase house prices.

The implications of the model are as follows: (1) changes in γ and N depend on district's pre-reform spending relative to $\bar{s}$ (i.e. whether district is constrained by reform); (2) constrained districts will increase γ; (3) constrained districts will decrease N (new construction). As a result, the additional empirical predictions to be tested are: (1) house prices will differentially increase in constrained districts; and (2) Constrained districts will have a lower minority population share due to the pre-existing correlation racial wealth gap.

The goal of the research design of this paper (Sections 4) is to determine the empirical threshold above which localities are revenue constrained by the policy change (i.e. the empirical analogue of $\bar{s}$). With this threshold in hand, the empirical section (Section 5) then tests the implications of the model–namely that land use controls will increase under centralization and differentially so among constrained localities.

3 Background

3.1 California School Finance Under Local Control

Prior to the 1970s, California's schools–like most across the US–were financed primarily by local property taxes.[10] In the final years of this decentralized system, the pairwise correlation between per-pupil property tax revenues and overall per-pupil spending was 0.85. This relationship was driven primarily by heterogeneity in assessed property values rather than in property tax rates.[11]

The wide variation in property values across districts, coupled with a school finance regime reliant on local funding, generated large disparities in per-pupil education funding across school districts. Figure 3 documents the relationship between school district property value, property taxes raised, and per-pupil base revenue.[12] The left and right panels show this relationship for unified and elementary school districts, respectively.[13]

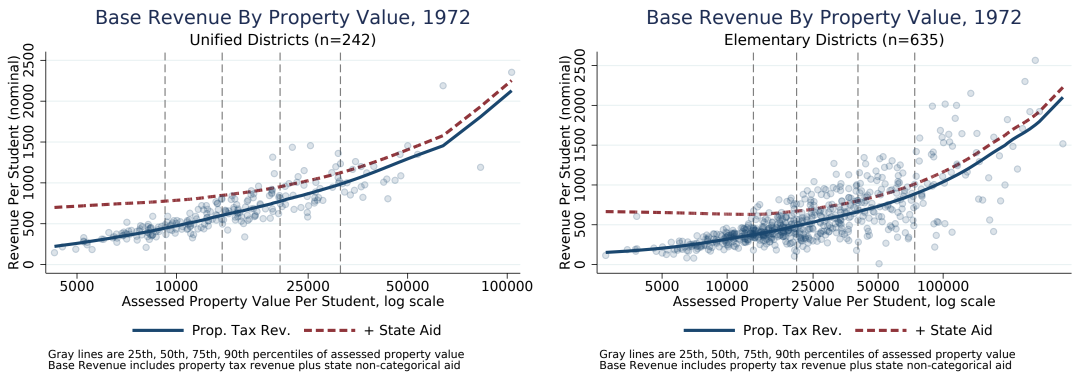


Figure 3: Base Revenue Per Pupil by Per-Pupil Assessed Property Value Under Pre-Reform Local School Finance (1972) shown separately for Unified and Elementary School Districts. Base Revenue is non-categorical per-pupil revenue shown as the sum of property tax revenue and state equalization and foundation aid. All lines are lowess smoothed estimates and each dot is a school district. Only the scatter plot for property tax rev by assessed value is shown.

[10] In 1967, the earliest year for which funding data are available at the school district level, local revenue sources accounted for 60% of overall funding (Picus, 1991 via *NEA Estimates of School Statistics*) By 1972, 64% of the median district's total revenue came from local sources

[11] For unified and elementary districts in 1972, the coefficient of variation of per-pupil assessed property values was 1.19 (mean=$30,158; SD=$35,786) while the coefficient of variation of property tax rates was 0.38 (mean=2.99%; SD=1.13%).

[12] Note that this is non-categorical per-pupil revenue. That is, it excludes additional funding districts receive from the state or federal governments based on student characteristics, such as special needs students. I exclude categorical aid here because districts have no control over these students' characteristics and because categorical aid was never factored into the eventual school finance equalization formula.

[13] In California, each house (student) is either zoned to a unified school district (which operates and finances both primary and secondary schools) or an elementary *and* a high school district. Multiple elementary school districts feed into the same high school district, so high school districts' financing information contains property values from multiple elementary districts. Elementary and unified districts, therefore, are the smallest levels of non-overlapping districts; together they cover the entire state. I drop high school districts from all analysis in this paper in order to prevent double-counting the same properties that result from the elementary-high school district overlap. Importantly, doing so does not drop any geographic areas or households; it only prevents double counting.

There are two important takeaways from this figure. First, both panels illustrate the strong positive relationship between assessed property value (x axis) and property tax revenue raised (solid blue line and blue scattered dots). The unconditional correlations between these two are 0.85 for unified districts and 0.74 for elementary districts. This is true because of property values, not tax rates. In fact, there is a negative correlation (not shown) between property tax rates and district property wealth. Second, the addition of some progressive state aid (red dotted line) does very little to break the overall relationship between property wealth and education revenue: the correlation between property value and base revenue (property tax revenue plus state aid) shrink to just 0.79 and 0.7, respectively. The addition of state aid is only significant for the bottom quartile of the distribution. At the median, state aid accounted for just 25% of base per-pupil revenue (about $200 per student in nominal 1972 dollars).[14]

3.2 *Serrano v. Priest* and School Finance Reform

This inequitable funding arrangement led to a major lawsuit (*Serrano v. Priest I*[15]) brought by parents of children in low property wealth districts. In late 1971, California's Supreme Court ruled that the quality of public education may not be a function of district property wealth, equating education quality to per-pupil revenue. The ruling stated that differences in property wealth "must not lead to significant differences in district revenue" (Sonstelie et al., 2000).

The Courts struck down the system of local control, but left it to the legislature to design a new system of school finance with more state involvement. In 1972 and 1973, the California legislature passed major funding reforms to begin in the 1973-1974 school year with the aim of equalizing per-pupil funding across school districts by de-coupling local property tax revenue form local education spending.[16] To achieve this, the equalization reform established for each school district an annual "revenue limit" per-pupil and introduced a progressive growth schedule such that these "revenue limits" would equalize over time. Revenues in property-poor, low-spending districts would increase over time while funding for property-rich, high-spending districts would be constrained by the state-imposed revenue limits. Revenue limits were determined by the state according to a district's pre-reform funding under local control. A full description of this process and its results can be found in the Empirical Design section.

Furthermore, school districts lost autonomy over setting property tax rates under the reform.

[14] State aid formula calculated via Sonstelie et al., 2000 and sources cited therein

[15] Serrano v. Priest, 5 Cal. 3d 584; 487 P.2d 1241; 96 Cal. Rptr. 601; hereafter Serrano I.

[16] SB 90 and AB 1267. For details see Sonstelie et al., 2000; Elmore & McLaughlin (1982); Picus (1991).

If a district's tax base exceeded its revenue limit (i.e. if its per-pupil assessed property value increased faster than its scheduled per-pupil revenue growth), property tax rates would be reduced such that the district could not raise more local revenue than its limit. This mechanism ensured two things: (1) revenue limits would bind; and (2) districts could not use property taxes to screen out potential new residents.[17]

As a result of state intervention, per-pupil revenues began to equalize over time. Figure 4 below illustrates the change in per pupil revenue (relative to the top revenue quartile) before and after the reforms by plotting difference-in-differences coefficients from the following equation:

$$PerPupilRevenue_{it} = \beta_1 PreReformQuartile_i \times Year_t + SchDistrict_i + Year_t + \epsilon_{it} \quad (2)$$

Prior to the reform, revenue for the bottom 3 quartiles were falling relative to the top quartile (left side of graph). Then in the first 5 years after the introduction of revenue limits and progressive growth rates, per-pupil revenues grew considerably for the bottom 3 quartiles relative to the top. The relative increases were monotonic, as intended by the reforms' progressive growth rates.[18] Bottom quartile per-pupil revenues increased 23% between 1973 and 1978; second quartile increased 18%; and third quartile increased 11%.

While this figure shows the progressive nature of California's school finance reform, it does not reveal which school districts were helped or hurt by the reform or show to what extent they were. The next section on Empirical Design uses new microdata on the reform to make these district-level delineations and then relates these findings on constrained versus unconstrained districts to the question of housing supply restrictions.

[17] The reforms did allow districts to vote to override their revenue limit by majority vote. The practice was rare, however. Using data on overrides, I find this occurred in less than 5% of districts. Additionally, all analysis is done with revenue limit overrides, so any estimates are an upper bound. For wealthier school districts, the impacts of the reform were twofold: first, the spending caps and lower scheduled growth rates substantially decreased per pupil funding (in real terms); second, requiring households pay property taxes to fund schools according to the state-determined formula made the current property tax system politically untenable (Fischel, 1989). On the second point, breaking the link between property taxes and education spending put downward pressure on property tax rates. As a result, there was a massive tax revolt in California in 1978; a statewide ballot initiative was passed to limit property taxes to 1% of assessed value. In turn, the state took control over the property tax system, and public education effectively became a state-financed program. After Proposition 13, the share of education spending coming from state sources jumped from around 40 percent to around 75 percent. Please note that this version of the paper will not examine Proposition 13 and any corresponding effects on the housing market. The focus will be strictly limited to the pre-prop 13 time period to show the causal effects of funding reforms on the housing and land markets. Prop 13 will be incorporated in future versions of this paper, and I expect it will only amplify the results and general thrust of my argument.

[18] This figure was inspired by Bayer, Blair, Whaley (2020)

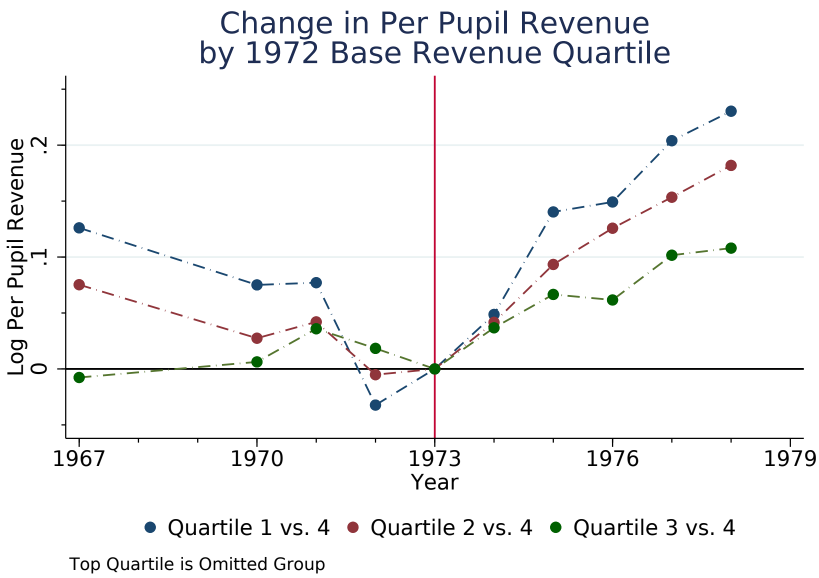


Figure 4: Plots the coefficients from a difference in difference regression, before and after the school financing reforms were implemented. The base year is 1973 and the omitted group is the top quartile of districts. The regression includes district and county fixed effects

3.3 The Rise of Land Use Regulations in California

In addition to school finance, residential land use policy in California also underwent rapid change in the 1970s. Locally adopted housing supply restrictions became mainstream, as the share of California municipalities with a land use regulation went from just 5 percent in the late 1960s to 24 percent by 1980. (See Appendix Figure A1 for the statewide trend over time.) The figure below shows the geographic proliferation of such regulations, mapping the share of municipalities in each county with a land use regulation for years 1967 (prior to the school finance reform), 1973 (the year of the reform), and 1978 (5 years after the reform).[19]

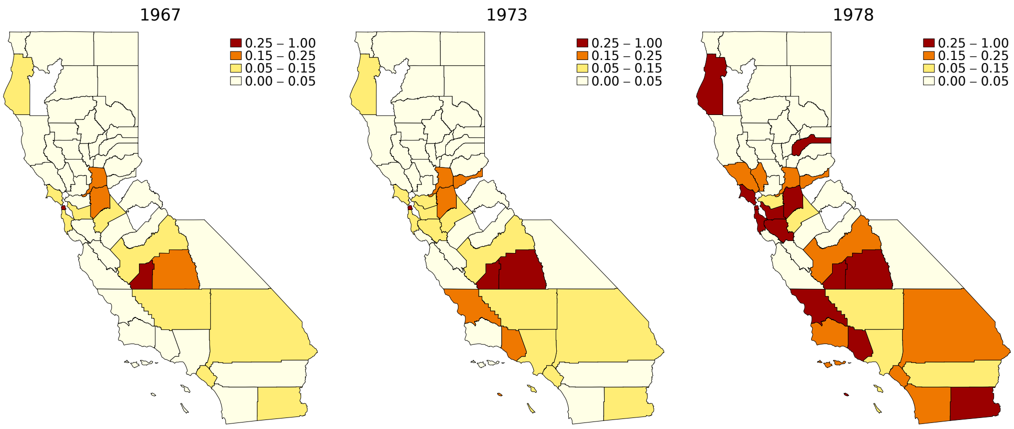
Several patterns emerge. First, the increase over time is obvious and especially pronounced between 1973 and 1978 (the second and third panels). This timing lines up with the 5 years following the implementation of the school finance reform (Figure 4 above). Second, visual inspection confirms there was very little change in the prevalence of regulations prior to the onset of the school finance reform. The aggregate data series (Figure A1) shows little movement between 1967 and 1973, and only a few counties show increases between those two years (the left and middle maps of Figure 5). This descriptive evidence suggests there are not geographically concentrated pre-trends prior to the school finance reform. Third, the patterns do not tell

[19] The legend/color palettes are identical for each panel. Counties are shaded depending on whether a land use regulation is present in >5%, 5-15%, 15-25%, or >25% of its municipalities. Data are from Jackson (2016). See section 4.1.1 and references therein for a full description

a particularly clear geographic story. Despite the fact that housing supply constraints today are especially prominent in large coastal cities, these maps show that, at least initially, the San Francisco, Los Angeles, and San Diego metro areas were neither early adopters of land use regulations nor disproportionately likely to have them compared to other regions. By 1978, supply constraints are somewhat prominent in counties in all regions of California. The phenomenon is not limited to coastal counties or major metro areas. San Bernardino County, for example, has about the same share of cities with a regulation as Sonoma County. However, the increases between 1973 and 1978 *are* especially pronounced in the Bay Area.

These maps show the regional patterns of local regulatory policy over time. But they do not shed light on the local drivers of land use regulations or their association with school finance. The following section examines the connections between school finance and housing supply restrictions at the local level. The rest of the paper explores how localities whose funding was constrained by the school finance equalization discussed in Section 3.2 were more likely to adopt restrictive land use controls to limit the supply of new housing, reduce the potential for congested schools and neighborhoods, and attract wealthier, more highly educated households.

Figure 5: Geography of California Land Use Regulations Over Time



Share of California cities in each county with at least one land use regulation, 1967, 1973, 1978. Author's calculations; data via Jackson (2016). Balanced panel of 411 cities incorporated prior to 1973.

4 Empirical Design

The goal of the empirical section is to understand how the loss of local fiscal autonomy differentially affected certain districts, and to determine whether this differential impact had spillover effects onto local housing supply policy. How did the new regime of school finance impact district revenues? Which districts were constrained by the state-imposed revenue limits? What would they have spent absent the reforms? This section uses pre-reform school finance data combined with new school district level administrative data on the revenue limits to determine an empirical threshold above which districts were financially constrained by the reform. In other words, I determine which districts were hurt most by the funding centralization. These districts benefited most under local control and were therefore more likely to adopt restrictive housing supply policies after losing the ability to impact their own revenues.

4.1 Data

Estimating how the overhaul of California's school finance system impacted local housing supply and land use policy requires data from several disparate sources. In short, I link school district level data from the Census of Governments and the California Department of Education to decennial US census data on district housing and demographics. After creating a crosswalk to match school districts to their municipality (or municipalities), I then merge on city-level data on land use regulation over time. The master dataset contains information on: (1) the finances and funding sources of California's school districts before and after the funding reforms; (2) the mechanics of the reforms, including the statutory education revenue limits placed on each school district over time and the associated changes in property tax rates; (3) historical housing stock and demographic variables at the school district level; (4) land use policies enacted at the municipality level over time.

4.1.1 Housing Data

Data on municipal land use regulations over time come from Jackson (2016), who combined responses from two surveys of California land use officials in 1989 (Glickfield and Levine, 1992) and 1992 (Levine et al., 1996).[20] The data contain 18 dummy variables indicating whether certain land use regulations were adopted by each municipality at the time of the survey. The types of regulations include: direct population/housing supply controls, zoning regulations, political controls, growth management plans, and commercial/industrial limitations.[21] The surveys used by Jackson also include year in which each regulation was adopted, which allows

[20] I am very grateful to Kristoffer (Kip) Jackson for generously sharing his data.
[21] For a full list of the land use restrictions measured and for a more thorough treatment of the dataset, see Jackson (2016).

for the creation of a balanced panel of regulations adopted by each jurisdiction, by regulation type, from 1965 to 1995. In this paper, I do not distinguish between regulation types; that is, the outcome variables in regressions and figures will either be an extensive margin measure of whether the municipality adopted any land use regulation or the intensive margin measure of the number of regulations adopted at a given year.

I use decennial US census data for housing and demographic data at the school district level. Specifically, I use the 1970 Fourth and Fifth Count Data for School Districts, the 1980 Census of School Districts, and the 1990 School District Data Book.[22] Merging these three survey waves together yields a decennial panel of district demographic data including information on population, income, race, house value, and housing stock, and year of construction. I use the year of home construction variable to impute each school district's housing stock in intercensal years. The census typically asks whether the respondent household's housing unit was built in the last two years, the last six years, the last ten years, etc. Using these questions for the 1980 census, for example, I can impute a school district's housing stock for 1978 and 1974; I then use the 1970 census for the 1970 housing unit count and continue to impute backwards in time into the 1960s. In total, I have snapshots of each district's housing stock for years: 1960, 1964, 1967, 1970, 1974, 1978, 1980, 1984, 1988, 1990.

4.1.2 Education Data

Data on school district finances comes from both survey and administrative data sources. The Census of Governments' (COG) School District Finance Data contains historical revenue, expenditure, and enrollment data for all school districts nationwide from FY1967 through 1991 (note that FY 1967 is the 1966-1967 school year, and so on).[23] I use the COG data for measures of district's total revenue, revenue raised by local property taxes, by other local sources, and revenue from intergovernmental transfers. Between 1967 and 1980, the COG conducted a full census of districts only in years ending in 2 and 7. For all non-census years, about a third of

[22] United States. Bureau of the Census. Census of Population and Housing [United States], 1970: Fifth-Count Tallies: Sample Data for School Districts . [distributor], 2003-01-02. https://doi.org/10.3886/ICPSR03524.v1
United States Department of Education. National Center for Education Statistics. User's Manual for 1970 Census Fourth Count (Population): School District Data Tape . [distributor], 2004-05-28. https://doi.org/10.3886/ICPSR03525.v1
United States. Bureau of the Census. Census of School Districts, 1980: [United States] . Inter-university Consortium for Political and Social Research [distributor], 2003-05-09. https://doi.org/10.3886/ICPSR02537.v1
United States Department of Education. National Center for Education Statistics. School District Data Book (SDDB), 1990: [United States]. [distributor], 2006-10-27. https://doi.org/10.3886/ICPSR02953.v2

[23] https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html

districts were sampled. The rotating sample in non-census years means I do not have a balanced panel on revenue data. Fortunately, my identification strategy relies only on knowing a district's pre-reform revenue levels and sources (which I do know for the full sample of districts in 1972). Additionally, administrative data from the State of California allows me to create a fully balanced panel for revenue and enrollment data.

For years 1972, 1973, 1974, and 1976, I digitized and hand-collected California Controller's Office data on districts' assessed property values, property tax rates, and average daily attendance.[24] I also hand-collect school districts' statutory revenue limits, property tax rates, assessed property values, and attendances for years 1974 through 1977 by digitizing archival data from the California Department of Education.[25]

After merging the COG-Census linked data with the administrative finance data, I am fully able to illustrate each step of the school finance reform (see Sonstelie et al, 2000; Picus, 1991; Elmore and McLaughlin (1982); Carroll and Park (1983) for more details). Starting with the top left panel Figure 6, the reforms set a more generous state noncategorical aid (that is, the a per-pupil transfer given to all students regardless of student characteristics) formula, where districts with lower than average per-student property wealth would receive additional lump sum revenue. Next, the reforms created "revenue limits" based on a district's pre-reform "base revenue," which is the sum of its per-student property tax revenue and its state noncategorical aid. Figure 6's top right panel shows a binned scatter of the revenue limits for 1975 as a function of districts' pre-reform base revenue.[26] A district's 1972 base revenue (via COG data) nearly perfectly predicts its 1975 revenue limit (via CA Controller data). To sum up, the top two panels show that given a district's pre-reform 1972 base revenue (property tax revenue + non-categorical state aid), one can map not only the district's 1975 revenue limit, but its future revenue growth path as given by the state scheduled increases (see below).

The bottom two panels show the progressive increases in state-mandated revenue limits from 1975 to 1976 (left in blue) and from 1976 to 1977 (right in red). Each dot represents a district

[24] California State Controller, Annual Report of Financial Transactions Concerning School Districts of California, Sacramento, California, various years.

[25] California State Department of Education, A Compilation of School District 1975-76 Revenue Limits Computed Pursuant to Senate Bill 90 and Assembly Bill 1267 by County Superintendents of Schools, Sacramento, California, 1976 (and other years).

[26] The 1974-1975 school year is the first time revenue limits were implemented. The 1973-1974 school year was the first post-reform year as far as the passing of legislation, but the state was still gathering data during time and was thus unable to formally implement revenue limits. See note in California State Department of Education, A Compilation of School District 1975-76 Revenue Limits Computed Pursuant to Senate Bill 90 and Assembly Bill 1267 by County Superintendents of Schools, Sacramento, California, 1976. Note to include this in full in next draft.

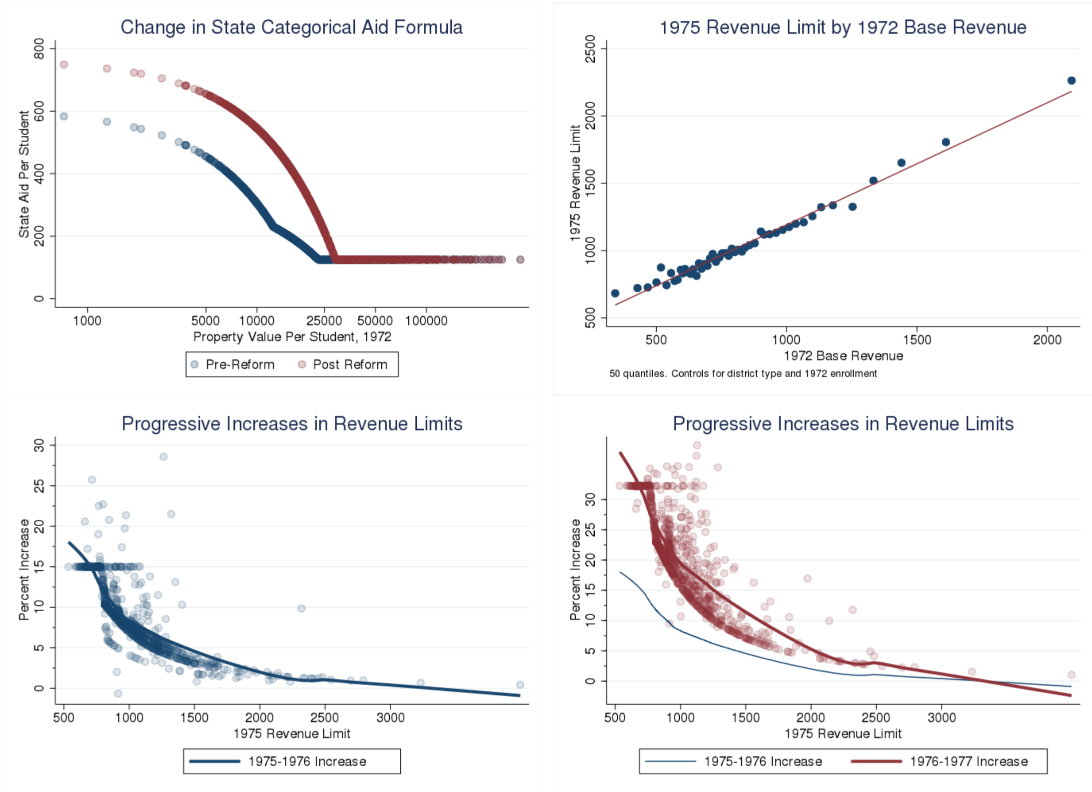


Figure 6: The four steps of California's SFR: (top left) More generous state aid; (top right) Impose revenue limits according to pre-reform revenue; (bottom left) Implement a progressive revenue increase schedule; (bottom right) Continue progressive increases until funding equalizes

and the lines are lowess estimated fits. While there is some variation around the regression estimated lines, the pictures tell a simple story: initially low base revenue districts would see their revenue limits rise year over year at a much higher rate than initially wealthier districts. For example, future revenue would rise 8 percent per year for districts with a $1,000 per pupil revenue limit in 1975 compared to 3 percent per year for districts with an initial limit of $2,000 per pupil. If this were to continue, the initial $1,000 funding gap between these two districts would be halved in 10 years and completely disappear within 15.

4.2 Identification Strategy

Typically in the US, and as was the case prior to California's school finance reform, local governments make joint decisions over education quality (raising local property tax revenues by choosing property tax rates) and urban growth (permitting new construction by setting land use and zoning policy). Because each of these choices may be made with the other in mind, simultaneity bias and reverse causality are major threats to identifying causal effects of local public good financing on land use and housing supply. Exploiting California's SFR sidesteps these classical endogeneity concerns since localities lose fiscal control under the new school finance regime. Conditional on pre-reform property wealth and tax rates, post-reform education revenue is determined completely by a centralized formula. But while the adoption of the reform offers temporal variation over which to examine the effect of local fiscal control on land use and housing, this alone is insufficient for identifying the mechanisms at play.

The policy change dictates that all localities lost autonomy over school spending and property tax rates at the same time, but the loss of control was accompanied by real funding increases for some districts and funding decreases for others. My identification strategy leverages this cross-sectional variation as well. The key then to estimating the differential effects according to district education funding is to find a natural and transparent measure for districts most-impacted by the loss of local control over their finances. It is not just the loss of local control over revenues that matters for housing supply outcomes, but also the districts' inability to re-optimize when revenues decrease.

Identification therefore depends on determining which districts experienced a decrease in revenue precisely due to the policy change. Empirically, I find that school districts at or above the 75th percentile in terms of pre-reform per-pupil revenues were constrained by the equalization reform, whereas districts below that threshold were unconstrained or beneficiaries of centralization of school finance. Districts in the top quartile then are the treatment group in my difference-in-differences specifications, with those below that threshold the control observations.

I determine the 75th percentile as the empirical threshold in two ways. First, and most simply, I calculate for each district its real per-pupil revenue before versus after the reform. Specifically, I take a district's 1972 base revenue (propert tax revenue + state non-categorical aid, per pupil) and compare that to its revenue limit four years into the reform (in 1977). The pre-reform base revenue is under the system of local control over financing, so it is presumably at a district's optimal funding level for that time. The post-reform revenue limit is the statutory funding level dictated by the equalization reform (which is a function of the same 1972 pre-reform funding levels). Figure 7 shows the real per-pupil revenue increases between 1972 and 1977, by 1972 base revenue. The figure plots both a linear and a lowess regression line fit to show the by-design negative relationship under funding equalization. School districts at the 25th (50th) percentile saw on average a 20% (10%) increase in per-pupil funding. Districts at or above the 75th percentile, however, experienced no real base revenue increases in the four years after the reform. Below the 75th percentile threshold, the reform increases district revenue; above it, revenue decreases. This is the first indication of which districts' funding was most constrained by the policy change.

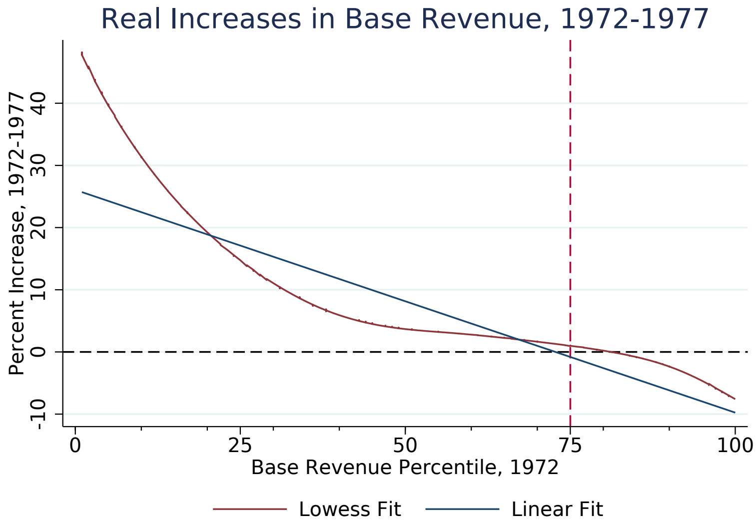


Figure 7: Determining Treatment and Control Groups. This figure pools unified and elementary school districts by 1972 base revenue percentile. It shows that districts above the 75th percentile experienced real revenue decreases between 1972 (pre-reform) and 1977 (4 years into the reform). Districts above this 75th percentile threshold are 'constrained' by the policy and are considered the treatment group for the empirical analysis.

Second, I conduct a stylized counterfactual exercise. Conceptually, districts "most-impacted" are those whose per-pupil funding under the state-financing regime is most different from what it would have been under pre-reform local financing. While this true counterfactual is unknown, Figure 8 computes a stylized counterfactual for all districts using their pre-reform property tax rates, while letting their assessed property values evolve as they did in reality. In effect, the difference between the red counterfactual lines and the blue revenue limit is the difference in per-pupil revenue due to losing autonomy over property tax rates. Again, districts above the 75th percentile (gray dashed line) would have better-funded schools under the local-financing counterfactual.

The reforms had differential effects on wealthy versus poorer districts by design. Revenues in poorer districts increased in large part due to progressive state aid. State subsidies meant that residents of poorer districts got better funded schools without bearing the cost of high property taxes. Meanwhile, wealthier districts' revenues were constrained by the limits.

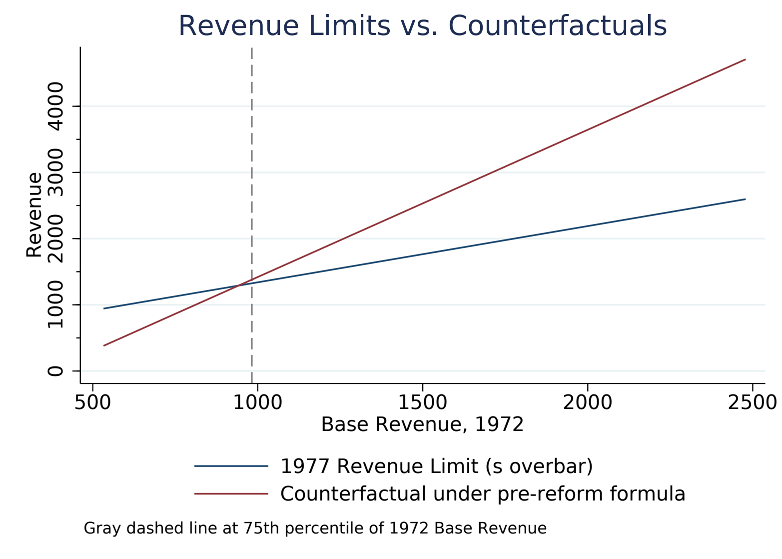


Figure 8: Determining Treatment and Control Groups.

4.3 Estimation

Using the 75th percentile empirical threshold, I then assign each district to either the treatment (Funding 'Constrained') or control ('Unconstrained') group.[27] To find the differential impact of the policy change on constrained versus unconstrained school districts, I first estimate the following non-generalized difference-in-differences equation:

$$\begin{aligned} y_{idmct} &= \beta_1 Constrained_i \times Post_t + \beta_2 Constrained_i + \beta_3 Post_t \\ &+ Controls_i' \beta + LatLong_m + \delta_c + \eta_d + \epsilon_{idmct} \end{aligned} \tag{3}$$

where y_{idmct} is an indicator for whether a land use regulation has ever been adopted in district i of type d by municipality m in county c at time t; $Constrained$ is a dummy variable equal to 1 if a district is in the top quartile of 1972 base revenue for its district type (i.e. there are different quartile cut offs for elementary and unified districts). $Post$ is a dummy variable for years after the reform (post-1973); $Controls$ are: school district assessed property value per pupil in 1972, housing units in 1960, enrollment in 1972, percent Black in 1970, percent Hispanic in 1970, and median income in 1970; $LatLong_m$ are controls for the municipality's latitude and longitude; δ_c is a county fixed effect; and η_d is a district type (unified or elementary) fixed effect; and ϵ_{idmct} is the error term.

[27] Note that I compute percentile cutoffs separate for the distributions of unified and elementary school districts, but the 75th percentile threshold is the same.

I estimate this equation over years 1966 through 1978 to understand the short run effects of the funding reform on changes in housing supply restrictions.[28] The coefficient of interest is β_1, which is the difference-in-difference term for the differential likelihood of treated districts to adopt a housing supply restriction after the reform.[29]

My preferred specification is a generalized difference-in-differences version of 3 above:

$$\begin{aligned} y_{idmct} = \beta_1 Constrained_i \times Post_t + \\ \alpha_i + \gamma_t + \delta_c + \eta_d + \epsilon_{idmct} \end{aligned} \tag{4}$$

where α is now a district fixed effect and thus collinear with district controls and the treatment variable. The coefficient of interest is again β_1, which represents the differential effect of moving from a local to state finance regime ($Post$) for initially high base revenue school districts ($Constrained$).

4.4 Identification: Assumptions and Threats

The identifying assumption of a difference-in-differences estimator is one of parallel trends: absent the change in school funding policy, districts above and below the 75th percentile revenue cut point would have continued on their pre-reform land use and housing supply trends. In other words, had it not been for the reforms, *pre-existing differences* in housing supply restrictions would have remained at the same level over time. One piece of suggestive evidence for parallel trends is the absence of pre-trends, which can be verified in figure 9 and 10. Prior to the policy change, there are no differential changes between the treatment and control group in terms of likelihood of adopting a housing supply restriction or rates of new construction. Treatment and control districts only diverged in their likelihood of enacting supply restrictions after losing fiscal

[28] Passed in 1978, ballot Proposition 13 took effect in the 1979 school year. As a result of Prop. 13's property tax limitation, there were state and local budget shortfalls, and the school finance system was reformed once again (with equalization still a key policy goal). The property tax system was fully centralized by the state, who enacted even more extreme funding equalization. This version of the paper will focus on housing impacts prior to Proposition 13 to limit complications. Though I suspect incorporating Prop. 13 into the model and empirics will only amplify the results.

[29] Estimating a land use regulation equation requires I overcome an important complication: regulations are enacted by municipal governments, not school districts. These are different geographies, and while every city will contain at least one school district, not every school district will be within or overlap with an incorporated municipality with authority over land use. Using shapefiles from the 1990 census, as well as matching districts to municipalities based on their listed city in the 1980 school district data book, I create a two-way district to municipality crosswalk. I find there are 416 elementary and unified districts in incorporated places, 377 of which (91%) match to exactly one city. Similarly, I find 411 cities incorporated by 1973, the time of the reform; 362 (88%) match to exactly one school district. This yields 472 district-city pairs, which I then classify according to the district's pre-reform revenue quartile.

autonomy over local education financing. Additionally, the policy change was truly unforeseen by school districts and local governments. Though the *Serrano v. Priest* opinion was handed down 2 years before the funding reform was enacted, the nature of the reforms mechanisms were not known or settled until enacted by the state legislature. This was the first statewide school finance reform of its time as well, so it is unlikely districts knew ex ante what to expect or how their budgets would be affected.

The precise timing of the measured effects is highly suggestive, but cannot on its own rule out potential confounders. For example, there are several concurrent trends that could be argued as alternative mechanisms explaining why high-wealth localities might have differentially begun adopting restrictive land use regulations in the mid-1970s. First, one hypothesis is that the environmental movement was in part behind the adoption of growth controls (Fischel, 2005). It could be the case that localities with natural amenities attracted high-wealth households, whose high property taxes funded schools. Then as environmental preservation gained momentum as a social movement, these high-wealth households petitioned local governments to enact stringent land use controls to preserve nature. Under this hypothesis, the timing with California's school finance reform is only coincidental. However, all of my specifications include geographic controls, including county fixed effects, longitude-latitude coordinates, and district fixed effects (in the preferred specification). Thus, the treatment effect is within-county and/or district, so point estimates are averaged across counties – coastal and non-coastal, north and south. This makes the environmental confounder less likely, as identifying variation comes not from geography, but from differences in pre-reform per-pupil revenue, conditional on property wealth.

A second alternative mechanism is that demographic trends during this time, such as black middle class suburbanization or Hispanic immigration into California, are actually behind the rise in land use restrictions. This is a narrative of racial/ethnic motivated exclusionary zoning whereby wealthier white communities, feeling threatened by the prospect of more integrated schools or neighborhoods, enacted land use restrictions. While certainly plausible, this is unlikely to confound my results due to the sharp timing of my policy change and the use of county and year fixed effects. Demographic trends were evolving slowly over this time period, so year fixed effects, plus the pre-post nature of the difference-in-differences estimation show that timing of my results are coincident with the school finance reform rather than decades-long trends like suburbanization or immigration.

A final set of identification concerns has to do with the effect of Proposition 13's tax limitation on the supply side of the housing market and a localities' likelihood of limiting development. A tax limitation could certainly have effects on housing supply (Lutz, 2015); for this reason, my main results stop at 1978, the year Prop. 13 was passed by California voters. Importantly though, my results show that housing supply restrictions, were increasing prior to the property tax limitation, particularly in localities with well-funded schools and high property tax revenues.

5 Results

5.1 Housing Supply Restrictions

Table 1 presents the main difference-in-differences results for the effect of the loss of local control on housing supply restrictions in reform-constrained school districts. Column (1) shows the results of equation (3) without county fixed effects, while Column (2) adds county fixed effects as in estimating equation. The difference-in-differences coefficient shows that after the 1973 funding reforms were enacted, constrained school districts were 5.8 percentage points more likely to have any land use regulation relative to control districts. Moving down the column, the post period (1974-1978) is associated with 7.4 percentage point increase in likelihood of regulation. Interestingly, constrained districts were actually 4.2 percentage points less likely than unconstrained districts to have adopted a housing supply restriction in general. This latter point, however, might be due to district-level unobservables, which are accounted for by adding district fixed effects in columns 2 and 3. Column 3 includes district fixed effects and the point estimates are unchanged. The post period is associated with an increase in regulations for all districts, on average, regardless of pre-reform revenue. This is suggestive evidence that loss of fiscal autonomy in general causes localities to restrict land use.

Column (4) presents the preferred generalized difference-in-differences specification (equation (4), adding year fixed effects to control for time trends that may have confounded column 3. The point estimate remains unchanged. Note that just 11 percent of districts had a land use control in 1973, the year of enactment. The 5.8 percentage point coefficient suggests a 52% increase in likelihood of housing supply restriction in the post period among revenue constrained school districts. The pre-period sample mean (1966 through 1973) is 8.8 percentage points, which implies a 66 % increase relative to entire pre-period.

Figure 9 below plots the annual event studies of the generalized difference in difference from 1966 (the omitted base year) through 1978 (before the passage of Proposition 13). In addition

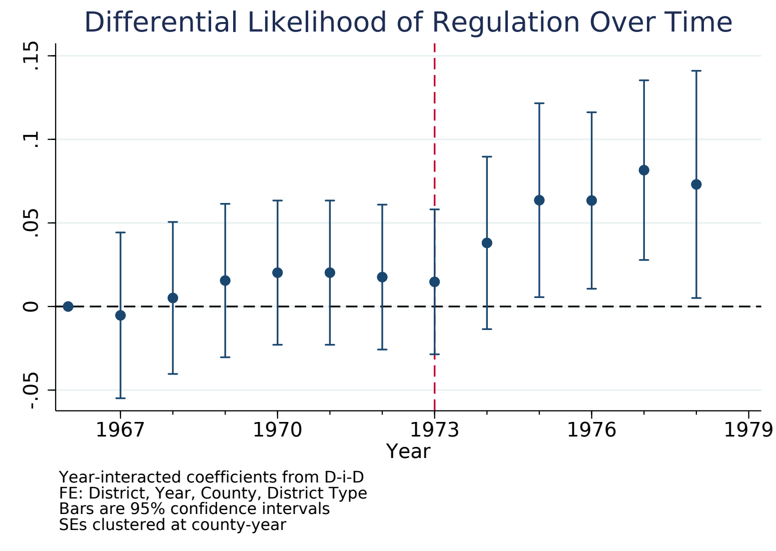
Table 1: Effect of School Finance Reform on Housing Supply Restrictions

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
Post X Constrained	0.058**	0.058**	0.058***	0.058***
	(0.025)	(0.027)	(0.015)	(0.014)
Post-Reform	0.074***	0.074***	0.074***	
	(0.019)	(0.011)	(0.010)	
Constrained District	0.039***	-0.042***		
	(0.012)	(0.014)		
Observations	5447	5447	5447	5447
R^2	0.098	0.250	0.693	0.699
Mean DV, 1973	0.11	0.11	0.11	0.11
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

to showing no differential pre-trend, the Figure shows a statistically significant (95% confidence intervals in bars) differential increase in the likelihood of adoption of a land use regulation in high-revenue school districts after California's SFR policy beginning in 1975 through 1978. The point estimates are generally increasing over time as well, though not statistically significant from one another. The point estimate in 1978 is 0.073; so relative to the pre-period sample average of 0.088, revenue constrained localities were about 83% more likely to have adopted a supply restriction 5 years after the reform relative to the pre-reform average.

Figure 9:



5.1.1 Robustness

A natural concern is that these main results are unique to or driven by particular geographic areas. One could argue, for example, that expensive coastal metropolitan areas like San Francisco and Los Angeles contain both elite school districts and–because of natural amenities and pre-existing high land values–are more likely to enact housing supply restrictions. By this logic, the timing of the school finance reform and its constraining effects on school district revenues is merely coincident. Even though the main results include county fixed effects, and thus exploit within-county identifying variation, it could be the case that a handful of counties are driving the effects.

Table 3 below assuages this concern by excluding the San Francisco and Los Angeles metropolitan areas from the sample.[30] Within these two large coastal metros, the point estimates on the difference-in-differences coefficient are nearly identical to those with the full sample (0.60 vs. 0.58, respectively). Despite fewer observations, they remain statistically significant at the 5% level. In Appendix Table A1, I also drop the San Diego metro area. The point estimates are just slightly smaller (0.47), but statistically indistinguishable from the main results. The main results are not driven by major coastal metropolitan areas. Taken together, these robustness exercises mean the proposed causal channel is not unique to the high house price, high natural amenity cities that are especially supply constrained today.

Table 2: Robustness Test: Excluding San Francisco and Los Angeles Metro Areas

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
Post X Constrained	0.060	0.060	0.060^{**}	0.060^{**}
	(0.040)	(0.045)	(0.027)	(0.025)
Post-Reform	0.086^{***}	0.086^{***}	0.086^{***}	
	(0.024)	(0.015)	(0.014)	
Constrained District	0.020	-0.113^{***}		
	(0.016)	(0.024)		
Observations	3419	3419	3419	3419
R^2	0.130	0.306	0.741	0.747
Mean DV, 1973	0.13	0.13	0.13	0.13
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

Another identification concern is that school districts or localities with higher property

[30] Specicially, I drop the following counties: San Francisco, Marin, San Mateo, Alameda, Contra Costa, Los Angeles, Orange.

values will naturally be more likely to adopt land use controls in order to block new supply from tempering price growth and changing the physical landscape as well. Under this argument, the reason localities adopt land use restrictions has nothing to do with local public goods, property taxes, or fiscal constraints imposed by the school finance reform; the fact that a district is constrained by the school finance reform is merely coincident, as property values are highly correlated with pre-reform school spending. To rule out this competing (or complementary) channel, I conduct a placebo test where treated localities are those in the top quartile for assessed per-pupil property value, instead of those in the top quartile of school spending. While these two measures are correlated, the difference is driven by localities' preferences for better funded schools vis a vis their pre-reform property tax rates. If the local public goods channel dominates the property value channel, we would expect to see no change in local land use policy around the policy change. Indeed this is exactly shown in Appendix Figure A2.

5.1.2 Heterogeneity and Peer Effects

The previous sections have established that high education revenue localities ("Constrained" districts) are differentially more likely to adopt housing supply restrictions upon losing control over school funding. Theory established in Section 2 shows this is because districts unable to adequately price a congestible local public good, like education, respond by using direct quantity controls to prevent future overcrowding. In addition to the *number* of students contributing to local congestion, constrained localities might also be especially concerned about the *characteristics* of such students and their families.

Peer effects in education and neighborhood social networks are extremely powerful (Epple & Romano, 2011; Sacerdote, 2011; Calvo-Armengol et al., 2009) and valuable to households (Calabrese et al., 2006; Brasington & Haurin, 2009). High-achieving (low-achieving) students confer positive (negative) externalities to their peers; parents too might gain (lose) from wealthier (poorer) social networks. Under local school finance (pre-reform), districts set the price of access via property taxes to impact both the quantity of potential entrants and the "quality" (i.e. income level) of district. Under the reform, losing the ability to price education might therefore make peer effects even more salient, in addition to exacerbating congestion concerns. One would then expect Constrained localities with high income and/or highly educated families to be even more likely to enact restrictive land use policies. Implicitly, this assumes elite school districts with wealthier, highly-educated families would be most concerned with poorer and/or less-educated families moving in and lowering average student "quality" after the school finance reform.

Tables in Appendix A.3 test this heterogeneous effects hypothesis by setting up the following triple difference regression:

$$\begin{aligned} y_{idmct} = {} & \beta_1 Constrained_i \times Post_t + \beta_2 Constrained_i + \beta_3 Post_t \\ & + \beta_4 AboveMedian_i + \beta_5 AboveMedian_i \times Post_t \\ & + \beta_6 AboveMedian_i \times Constrained_i \\ & + \beta_{\mathbf{7}} \mathbf{AboveMedian_i} \times \mathbf{Constrained_i} \times \mathbf{Post_t} \\ & + Controls_i' \beta + LatLong_m + \delta_c + \eta_d + \epsilon_{idmct} \end{aligned} \tag{5}$$

where β_7 is the triple difference coefficient of interest that interacts the usual difference-in-difference interaction ($Constrained_i \times Post_t$) with an indicator variable equal to one if the school district is above the statewide median income or college attainment before the reform (1970).[31] If β_7 is positive and significant, funding-constrained districts with higher parental education (for example) are even more likely to adopt housing supply restrictions after the policy change. Indeed this pattern is exactly what we find. Table A2 shows the triple-difference coefficient for above median income is 0.081 and statistically significant. By comparison, the baseline result in Table 1, Column 4 was 0.058; so constrained districts above the median income districts were even more likely to adopt restrictive land use policies. Table A3 tells a similar story for constrained districts above the median college attainment. The statistically significant point estimate on the triple difference coefficient is 0.098, suggesting an differentially stronger effect in highly-educated districts. These heterogenous effects are consistent with the idea that more elite constrained districts are sensitive to peer effects in education above and beyond the first order congestion concerns.

5.2 New Construction

A second set of housing supply results examines new construction at the school district level as the dependent variable. If the supply restrictions bind, new construction should differentially

[31] School district level data on median income and college attainment are from the 1970 Census. I compute separate median cutoff points depending on the district type. For instance, I pool all unified school districts, compute the median of district median income, and assign a value of 1 if the district is above that cut point. College attainment is measured as the share of households in the district whose household head has ever attended college

decrease in constrained districts, as stringent regulatory policies are designed to prevent new supply in some way. Put another way, land use regulations might be thought of as the mechanism behind a differential decline in new construction or urban growth. Proving this sequence of causality is beyond the scope of this paper at this time, however.

I estimate this effect only with housing unit data supplied in decennial census years or in years for which I can credibly impute from census questions (years: 1967, 1970, 1974, 1978). Figure 10 shows the results from the following generalized difference-in-differences event study:

$$\begin{gathered} y_{idct} = \beta_1 Constrained_i \times Year_t + \\ \alpha_i + \gamma_t + \delta_c + \eta_d + \epsilon_{idct} \end{gathered} \tag{6}$$

where dependent variable is the school district's percent change in housing since 1967 (thus 1967 is the omitted group in the plot). Importantly these percentages are calculated off of each district's housing stock in 1967 and are cumulative over time. The specification includes district, district type, year, and county fixed effects. The plot again shows now evidence of differential

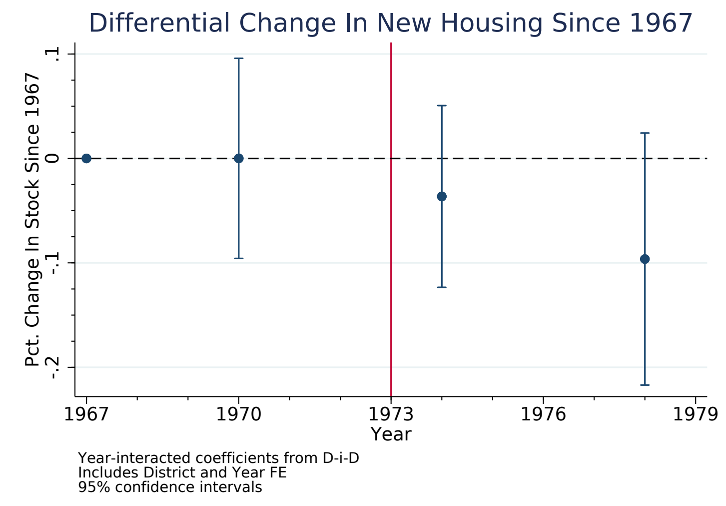


Figure 10:

pre-trends, and a gradual differential decline in new housing during the post period (1973-1978). The 1974 point estimate is -0.036 and the 1978 point estimate is -0.096.[32] Interpreting the 1978 point estimate, school districts that were revenue constrained by the policy change added differentially 9.6% fewer homes relative to their housing stock in 1967. Note, however, that this was during a building boom. The median district's housing stock grew by 42% between

[32] Though neither are statistically significant, a pooled difference in difference (non-event study) produces a -.066 point estimate, which is significant at the 10% level.

1967 and 1978, so constrained districts were still adding homes, just not at the rate of their revenue unconstrained peer districts.

In 1970, the median (mean) district in this sample had about 3,700 (11,750) homes; from 1970 through 1978, constrained districts constructed about 1.2% fewer homes per year, or about 45 (140) fewer homes per year evaluated at the median (mean).

5.3 House Prices and Racial Composition

Following the adoption of housing supply restrictions and the differential effects on new home construction, one would expect correspondent economic impacts on house prices and residential sorting. Land use controls make the local housing supply curve more inelastic, and absent a negative demand shock, house prices in revenue-constrained (treatment) school districts should differentially increase relative to revenue-unconstrained (control) districts. Table 2 below examines the short-run (column 1) and long-run (column 2) impacts on average house value. These specifications are the same as equation (6), though they include only decennial census years. Because of this, we are unable to examine pre-trends or the time series around the 1973 policy change. While the results stand on shakier causal ground, they are informative and suggestive nonetheless.

The table shows short run and persistent differential changes in house value. Despite real cuts to school funding and the loss of control over future per-pupil revenues, house prices went up in constrained school districts. Between 1970 and 1980 (column 1), average house price increased differentially by 6.8% in Constrained districts. House price gains widened further to 11.2% when the analysis is extended to 1990 (column 2).

Table 3: Differential House Price Increases in Revenue-Constrained School Districts

	(1) (ln) Real Mean House Value	(2) (ln) Real Mean House Value
Post X Constrained	0.068^{***}	0.112^{***}
	(0.021)	(0.032)
Observations	1114	1660
R^2	0.978	0.948
Mean DV, 1970	147,000	147,000
Years	1970-1980	1970-1990
FE	District, Year	District, Year

Standard errors in parentheses
District and Year FE, standard errors clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

Newly-restricted land use, declining rates of construction, and increasing house prices should also have effects on residential sorting. Here I examine the effect of the school finance reforms on minority population share across constrained versus unconstrained districts over time. Columns (1) and (2) of Table 4 show short run differential declines in Black and Hispanic population share, respectively. The differential declines in column 1 are economically but not statistically significant. Note the mean district Black population share was just 2% in 1970, so a differential decline of 0.5 percentage points is small, but it has an effect size of 25% relative to the pre-reform mean. Hispanic population share fell differentially by 2.9 percentage points (on a pre-reform district-level mean of 15%). Taken together, this 3.4 percentage point differential decline means Black and Hispanic population share fell 17% in constrained school districts after the funding reforms.

Table 4: Minority Population Share Declines in Constrained Districts

	1970-1980		**1970-1990**	
	(1) Pct. Black	(2) Pct. Hispanic	(3) Pct. Black	(4) Pct. Hispanic
Post X Constrained	-0.005	-0.029***	-0.004	-0.038***
	(0.003)	(0.010)	(0.003)	(0.011)
Observations	1118	1118	1666	1666
R^2	0.932	0.943	0.913	0.922
Mean DV, 1970	0.02	0.15	0.02	0.15

Standard errors in parentheses
District and Year FE, standard errors clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

6 Conclusion

Restrictive land use polices are adopted at the local level, but their cumulative effects have first order impacts on spatial economics at a broader scale. From their effects on housing affordability (Glaeser and Gyourko, 2018) to economic opportunity (Trounstine, 2020) and mobility (Ganong and Shoag, 2017) to GDP growth (Hsieh and Moretti, 2019), the negative externalities of land use restrictions have been well documented. This research has focused on these ill effects, taking the origins of the policies as given. Yet identifying and understanding the local motivations for land use controls will be key to increasing access to quality neighborhoods and solving housing affordability crises throughout the country.

This paper argues that the desire to safeguard local public goods is a key motivator behind the adoption of supply restrictions. To solve the natural endogeneity between public goods and the housing market, I exploit an exogenous shock to the system of local public finance. I theoretically propose and then empirically test the loss of local fiscal control over school finance as a new causal channel for housing supply restrictions. Finally, I provide new evidence consistent with supply constraints' effects on new home construction, house prices, and neighborhood demographics.

The theoretical model illustrates that communities constrained by the fiscal centralization are forced to substitute from price controls (property taxation) to quantity controls (land use restrictions). Without the ability to influence public good quality via the fiscal channel, communities rely more heavily on land use instruments. This behavior can be further generalized: when one set of public policy instruments is taken away, economic agents will adjust by finding a new set. In this case, the new set of instruments are housing supply restrictions, which are have distortionary and durable impacts on the urban form, neighborhood accessibility, and housing affordability.

Using California's 1973 school finance equalization reform as a natural experiment, I show that when communities lose the ability to set their desired level of school funding, they respond by adopting housing supply restrictions and build less housing as a result. School districts with larger exclusionary motives–those whose school spending was constrained by the reform–were 66% more likely to enact a housing supply restriction in the five years after reform. These same localities also built about 1% fewer new homes per year over this same time frame. Consistent with the predicted effects of supply regulations, house prices in constrained districts increased differentially 7 percent relative to unconstrained districts in the short run. Price increases had predictably exclusionary effects: Black and Hispanic population share differentially fell 17 percent. The house price and district demographic effects persisted into the long run (20 years), indicating the lingering effects of land use controls.

This is the first paper to empirically test any causal channel behind the widespread adoption of housing supply regulations across space and over time. Additionally, the proposed mechanism–the loss of local control over school finance–helps explain the origins of supply controls in California. I also provide new empirical support to seminal local public finance theory on the interactions between public good provision, property taxation, and land use/zoning (Mills and Oates, 1975; Hamilton, 1975; White, 1975). Documenting this connection and identifying determinants of restrictive land use policies is especially timely. Many metropolitan areas are facing serious housing affordability crises, and supply constraints are seen as a primary driver.

In addition to addressing the connection between land use and public goods, this work also highlights the importance of examining the unintended consequences of equity policies and fiscal federalism interventions. The intention of centralizing education finance was to create a more equitable system. Instead, limiting per-pupil revenue created an incentive to limit new housing. Policy interventions in other contexts may engender local adjustments along other policy margins or backlash in other forms; in this instance, localities reclaimed control via land use controls – policies with severe long-run consequences for households and the urban form.

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A Appendix

A.1 The Rise of Land Use Regulations in California

California is the most housing supply-constrained state in the US today (Gyourko et al., 2019). However, this is relatively recent phenomenon; land use regulations became prominent in California only in the 1970s.

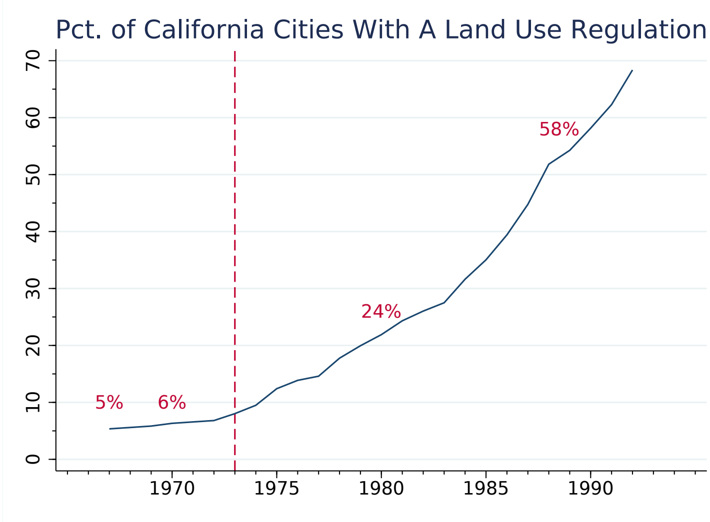


Figure A1: Share of California cities with at least one land use regulation, 1967-1992. Author's calculations using data via Jackson (2016). Red vertical line at 1973, the year California's school finance reform was implemented. Includes 411 cities incorporated prior to 1973.

A.2 Robustness

Table A1: Effect of School Finance Reform on Housing Supply Restrictions - *Excluding SF, LA, SD*

	(1) Any Reg	(2) Any Reg	(3) Any Reg	(4) Any Reg
Post X Constrained	0.047	0.047	0.047*	0.047*
	(0.041)	(0.046)	(0.028)	(0.026)
Post-Reform	0.088***	0.088***	0.088***	
	(0.025)	(0.015)	(0.014)	
Constrained District	0.012	-0.129***		
	(0.017)	(0.025)		
Observations	3237	3237	3237	3237
R^2	0.133	0.311	0.749	0.756
Mean DV, 1973	0.14	0.14	0.14	0.14
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses

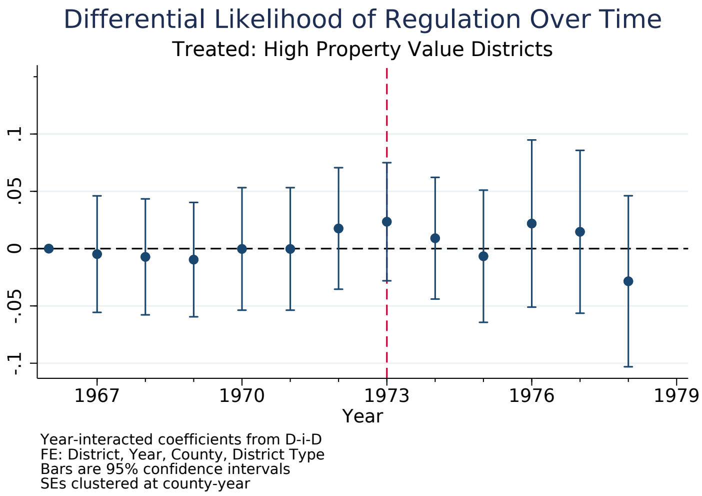
All specifications include district controls, city coordinates controls, district type FE

Standard Errors Clustered at County-Year level.

Sample includes only unified and elementary districts

* $p < .10$, ** $p < .05$, *** $p < .01$

Figure A2: Placebo Test: Assessed Property Value Per-Pupil



A.3 Heterogeneous Effects

Table A2: Triple Difference: School District Above Median Income, 1970

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
High Income X Constrained X Post	0.081	0.081	0.081**	0.081**
	(0.065)	(0.056)	(0.037)	(0.036)
Income X Post	0.025	0.025	0.025	0.025
	(0.036)	(0.024)	(0.022)	(0.022)
Income X Constrained	-0.040	0.047		
	(0.034)	(0.029)		
High Income	-0.000	-0.041**		
	(0.020)	(0.017)		
Post X Constrained	-0.007	-0.007	-0.007	-0.007
	(0.036)	(0.032)	(0.017)	(0.016)
Post-Reform	0.068***	0.068***	0.068***	
	(0.018)	(0.009)	(0.008)	
Constrained District	0.068***	-0.065***		
	(0.022)	(0.022)		
Observations	5447	5447	5447	5447
R^2	0.100	0.253	0.695	0.700
Mean DV, 1973	0.11	0.11	0.11	0.11
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

Table A3: Triple Difference: School District Above Median College Educated, 1970

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
High College X Constrained X Post	0.098**	0.098**	0.098***	0.098***
	(0.039)	(0.047)	(0.022)	(0.022)
College X Post	0.041*	0.041**	0.041***	0.041***
	(0.024)	(0.018)	(0.012)	(0.012)
College X Constrained	0.075***	0.119***		
	(0.018)	(0.028)		
High College	0.032**	0.014		
	(0.014)	(0.015)		
Post X Constrained	-0.036	-0.036	-0.036**	-0.036**
	(0.027)	(0.036)	(0.016)	(0.016)
Post-Reform	0.058***	0.058***	0.058***	
	(0.020)	(0.011)	(0.009)	
Constrained District	-0.021	-0.131***		
	(0.016)	(0.027)		
Observations	5447	5447	5447	5447
R^2	0.109	0.259	0.696	0.701
Mean DV, 1973	0.11	0.11	0.11	0.11
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

Table A4: Triple Difference: School District Above Median Pct. White 1970

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
High White X Constrained X Post	0.067	0.067	0.067***	0.067***
	(0.050)	(0.049)	(0.025)	(0.024)
High White X Post	0.028	0.028*	0.028**	0.028**
	(0.025)	(0.016)	(0.012)	(0.012)
High White X Constrained	0.008	0.088***		
	(0.023)	(0.024)		
High White Share	0.067***	0.089***		
	(0.014)	(0.013)		
Post X Constrained	0.003	0.003	0.003	0.003
	(0.036)	(0.034)	(0.016)	(0.015)
Post-Reform	0.062**	0.062***	0.062***	
	(0.025)	(0.013)	(0.011)	
Constrained District	0.036*	-0.099***		
	(0.018)	(0.020)		
Observations	5447	5447	5447	5447
R^2	0.107	0.270	0.695	0.700
Mean DV, 1973	0.11	0.11	0.11	0.11
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$

Table A5: Triple Difference: School District Above Median Enrollment, 1972

	(1) Any Reg.	(2) Any Reg.	(3) Any Reg.	(4) Any Reg.
High Enrollment X Constrained X Post	-0.004	-0.004	-0.004	-0.004
	(0.050)	(0.051)	(0.029)	(0.028)
High Enrollment X Post	0.019	0.019	0.019	0.019
	(0.022)	(0.018)	(0.014)	(0.013)
High Enrollment X Constrained	-0.040*	-0.076***		
	(0.021)	(0.023)		
High Enrollment	0.040***	0.033**		
	(0.015)	(0.014)		
Post X Constrained	0.061*	0.061*	0.061***	0.061***
	(0.036)	(0.035)	(0.020)	(0.020)
Post-Reform	0.064***	0.064***	0.064***	
	(0.017)	(0.011)	(0.010)	
Constrained District	0.059***	-0.001		
	(0.015)	(0.017)		
Observations	5447	5447	5447	5447
R^2	0.100	0.252	0.694	0.699
Mean DV, 1973	0.11	0.11	0.11	0.11
District Controls	Yes	Yes	Yes	Yes
FE	District Type	County	District	District, Year

Standard errors in parentheses
All specifications include district controls, city coordinates controls, district type FE
Standard Errors Clustered at County-Year level.
Sample includes only unified and elementary districts
* $p < .10$, ** $p < .05$, *** $p < .01$