Built Out Cities? A New Approach to Measuring Land Use Regulation

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Abstract:

We introduce a new way to measure the stringency of housing regulation. In lieu of a standard regulatory index or a single aspect of regulation like Floor Area Ratio, we draw on California cities' self-reported estimates of their total zoned capacity for new housing. This measure offers a more accurate way to assess the potential for new development in cities with substantial multifamily zoning but few undeveloped parcels. We show, in regressions analyzing new housing permitting, that our measure has associations with new supply that are as large or larger than conventional, survey-based indexes of land use regulation. Moreover, unbuilt zoning capacity interacts with rent to predict housing production in ways conventional measures do not. Specifically, interacting our measure with rent captures the interplay of regulation and demand: modest deregulation in high-demand cities is associated with substantially more housing production than substantial deregulation in low-demand cities. These findings offer a more comprehensive explanation for the historically low levels of housing production in high cost metros.

Keywords: Land Use Regulation, Measurement, Housing Production

JEL Codes: R15; R31; R38; R52

1. Introduction

This article examines how local regulations restrict housing supply, using California as a case study. By now most researchers agree that regulation plays some role in high housing prices (Kahn et al., 2010; Kok et al., 2014; Jackson, 2018). But regulation is both a broad term and a black box. A wide variety of regulations exist, and while the literature has found a strong correlation between regulatory stringency and housing prices, these findings are accompanied by two caveats: Regulation is hard to measure consistently, and the mechanisms through which it increases price remain opaque.

The opacity arises for two related reasons. First is the sheer variety of regulations, and regulatory decisions, that localities can choose from. Cities can use different regulations to arrive at the same outcome of low housing production. One local government might have large minimum lot sizes, another high minimum parking requirements, while a third might look lenient on paper but then review each project slowly, and use its discretion to discourage or reject applications for entitlement. These three cities could be equally adept at blocking development, but analysts correlating any *given* regulation with housing supply and price across these cities might find only weak relationships, even if they measured that regulation perfectly.

The second reason, which flows from the first, is that individual regulations might be interchangeable proxies for a larger phenomenon, which is an underlying and hard-to-observe political antipathy to new housing. A high minimum lot size can be a legitimately binding constraint. But suppose a city with such a requirement was forced to remove it. Would this city just welcome more housing? Or would it increase its parking requirements, reduce its height limits, or start slow-walking permit applications? Regulations are adopted for a reason, and may be more symptom than source.

Our primary contribution in this paper is a novel way to measure the source—the city's underlying sentiment toward housing development. Our measure is the city's subjective (but quantified) judgment of how close it is to its own buildout point, or what we call its "unbuilt capacity." This measure is available in California as a result of the state's Housing Element law. The law requires all local jurisdictions in California regularly estimate their unbuilt capacity and report it to the state. This requirement is compulsory, municipal governments take it seriously, and—crucially—because the reporting has real-world consequences, cities are strategic when they do it, and many try to keep their estimates low.

Strategic behavior is possible - and likely - because buildout estimates are fundamentally subjective. Estimates may often rest on objective measures (e.g., the availability of vacant land, and units allowed under existing zoning), but in point of fact all of California's cities could physically hold more housing *if* they decided to allow it.

Allowing housing is primarily a political decision. When cities report their ability to hold new housing, therefore, they reveal an overall tolerance for development—a tolerance that is reflected in different forms of regulation across both places and time.

Here is an example. In 1960 the total zoned capacity of the city of Los Angeles suggested it could hold roughly 10 million people. By 1990 it had fallen to about 4.5 million, where it remains today (Morrow, 2013). What accounts for this 55% decline? Los Angeles did not add mountains or water between 1960 and 1990. It did not lose land area. Structural engineers did not determine that buildings in Los Angeles could not be as tall as was once thought (if anything the opposite was true; improvements in seismic safety made taller structures more feasible). What changed was residents' attitudes toward development–prompted, in part, by development itself (Morrow, 2013; Whittemore, 2011). These changed attitudes came to be reflected, in different ways, in the city's regulations. Los Angeles had once determined that it could hold twice as much housing as it says it can now. It could return to that higher determination *if it wanted to*.

Unbuilt capacity is not a land use regulation. It is a number that estimates the underlying political attitude that land use regulations make manifest. In this way, it accomplishes what many indices of regulation attempt. We show in this paper that in simple models of new housing supply (as measured by building permits), unbuilt capacity performs comparable to a conventional index of regulation. We also show, however, that unbuilt capacity has an explanatory power the conventional index lacks as it offers a better window into the relationship between regulation and demand. When we interact a lagged rent variable with conventional metrics of regulation, the interaction term is statistically insignificant. When we do the same with unbuilt capacity, in contrast, the results are significant both economically and statistically. The results suggest that unbuilt capacity is strongly associated with permitting in high rent cities, but *not* in low rent cities. Specifically, we see that when demand is low, even dramatic changes in regulatory stringency (as measured by buildout) yield relatively little new housing, while when demand is high, even modest deregulation is associated with more permitting. Our results are not causal. However, because our dependent variable is supply rather than price, any endogeneity present in our regressions is likely nullifying, meaning our results may plausibly be underestimates.

Our findings about the interaction of demand and regulation are at once economically unsurprising and counterintuitive to policymakers. It is economically unsurprising because development is more likely to occur in higher-demand (higher-priced) neighborhoods, but development also creates its own opposition: a political constituency that wants to defend a low density status quo. When these constituencies gain sufficient power, they enact regulations that slow or prevent development (Ellickson 2022; Fischel 2009). The net result is a city where housing construction is less likely to occur than it would have been absent regulation, but *more* likely to occur than in other places where demand is lower. A naïve correlation between development and regulation, in this situation, would be positive, suggesting that regulation encourages housing supply. While economists are unlikely to be misled by such a correlation, skeptics in both the broader housing literature and in policy debates will sometimes observe that the most regulated cities also build the most (Baxamusa, 2020; Rodriguez-Pose and Storper, 2020).

This skepticism, moreover, is not easy to diffuse with conventional metrics of regulation, because some cities that are less regulated have little demand, and some cities that are more regulated have many vacant parcels. The former experience little development despite their leniency (because they lack demand), while the latter see ample development despite their stringency (because they are statutorily built out). Controlling specifically for buildout overcomes this problem, and also offers a metric of regulation that lends itself more naturally to policy. There is no obvious path to a city lowering its score on a regulatory index score (particularly if the score is a metropolitan area average) but a city *can* expand its zoning envelope.

The paper's next section sets the stage for our analysis by reviewing previous research on land use regulations and housing markets. Section 3 turns to our data; we describe California's Housing Element law and emphasize localities' strategic behavior in complying with it, and also outline our regression approach. Section 4 presents our results, and in the conclusion we discuss policy implications and future research.

2. Regulatory Barriers: Price and Supply

The empirical literature on zoning's role in distorting housing markets is by now substantial.[1] This literature differs along two important dimensions: the outcome of interest (prices or production) and how regulation is measured. We discuss each in turn.

2.1 Prices or Production?

Prices and quantity change in the same set of equations, but price is the dependent variable in most land use and housing research.[2] On the one hand, this disproportionate attention to prices is understandable.[3] Prices are clearly the relevant outcome for policy. High prices have an intrinsic impact on welfare that low production does not; scholars

[1] Quigley and Rosenthal (2005) identified 50 papers in this literature. We identified another 25, which we summarize in Appendix A.

[2] Appendix A shows that supply is the outcome in only 12 of the 80 papers we examined.

[3] The literature tends to measure prices using purchase prices or home values, even though rents are arguably a better measure of housing's consumption cost. None of the studies we reviewed since 2005 have used rents as an independent variable.

are more likely to initiate studies because prices are high than because production is low. Production from this perspective is an intermediate outcome, a necessary step toward the outcome of interest. As a result, the idea that regulation suppresses supply is almost always implied, but only sometimes explicitly tested.

Good reasons exist, however, to explicitly carry out that test. One involves the endogeneity between prices and regulation. A strong association between regulation and prices might reflect regulation inhibiting production, but also a price premium commanded by regulated environments (Katz and Rosen, 1987; Fischel, 1990). Demonstrating that regulation is in fact associated with less supply would not rule out regulation creating an amenity premium, but would offer some simple support for the mechanism implied in price regressions: regulations raise prices by constraining supply.

Using supply as the dependent variable creates a more manageable endogeneity problem than with price as the dependent variable. As mentioned above, if regulation can increase prices, but regulated areas also command higher prices (and for reasons unrelated to lower production), then tests of regulation's supply-induced effect on price will have confirming bias. For any given increase in demand, regulation will make prices rise because it slows building, but also because consumers have a taste for regulation. Left unaddressed, this bias could generate a false positive in a significance test, inflate the coefficients on measures of regulation, or both.

From a policy perspective, this endogeneity may not be a large concern. A regression that incorrectly attributes some or all of regulation's effect on price to a supply response, when in fact some of that effect is an amenity response, is a mistaken regression. This mistake does not imply, however, that regulation is not increasing prices, or that less regulation will not make housing more affordable. The pathway toward lower prices would be different—the area would become less desirable, holding supply constant, rather than have more supply, holding desirability constant—and this difference could imply different distributional consequences, but fundamentally regulation is still pushing prices up.

From an academic perspective, of course, proper identification is important, and distributional consequences often matter, so researchers do attempt, usually with lags and instrumental variables, to address the endogeneity between regulations and prices (Malpezzi et al., 1998; Mayer and Somerville, 2002; Malpezzi, 2002; Saiz, 2010).

A different approach is to avoid price regressions and simply model production. Production, relative to price, is a more straightforward outcome to interpret. This is so because endogeneity, given the hypothesized relationship between regulation and supply, will create a *nullifying* bias. Where regulation, for any given increase in demand, will make prices increase by more than they would otherwise, they will make supply rise by less.

This works as follows. We expect more expensive places to produce more housing, *ceteris paribus*. A simultaneity threat arises because regulation can reduce supply (a negative relationship), but if neighbors become concerned about growth, new supply could also increase regulation (a positive relationship). Left unaddressed, this bias could create a null significance test and/or a positive coefficient on measures of regulation. These results would be unreliable. If, however, the biased regression yielded a negative and statistically significant coefficient, that coefficient would if anything be too small. Thus in situations where endogeneity is hard to control and identification matters, negative coefficients on supply are more compelling than positive coefficients on price.

2.2 Measuring Regulation

Housing developments face scores of regulations, not all of them present in all cities, not all enforced with equal intensity in the cities that have them, and not all requiring the same time and effort to satisfy when enforced. Not every city, for instance, demands a traffic analysis for multifamily development. Such an analysis in one city might require a single trip to a single department, but require multiple reviews by multiple departments in another city. For that matter the same regulation within a city might impose much higher costs on some projects than others (e.g., requiring two parking spaces per housing unit matters little for detached single family homes, but can be a binding constraint for small apartments).

Given this variety and complexity, there is virtually no way to fully and consistently capture regulatory stringency in a single measure. Scholars have, however, come up with reasonable proxies. They have examined local zoning codes for the presence of specific measures, or observed changes in cities after new regulations are added (Downs, 2002; Schuetz, 2009; Jackson, 2016), measured the difference between the average and marginal value of land (Glaeser et al., 2005), tracked the frequency of development litigation over time (Ganong and Shoag, 2017), recorded the frequency with which developers request discretionary approvals (Ben-Joseph, 2003), or simply assessed the role of specific bulk regulations like Floor Area Ratio (FAR) in models of stringency (Brueckner et al., 2017; Brueckner and Singh, 2020; Zhang, 2022). While none of these approaches are perfect, they yield broadly consistent results: regulation is significantly associated with less housing production and higher prices.

Nevertheless, a skeptic combing the extant literature could find some reason for doubt. One of the more common approaches to measuring regulation is to survey planning staff.[4] These surveys generally ask about the presence of common regulations, the cost and time to get building permits for different types of development, and rates of enforcement (Glickfeld and Levine, 1992; Ihlanfeldt, 2007; Gyourko, 2008; Kok et al., 2014; Pendall et al., 2018; Jackson, 2019; Hilber and Vermeulen, 2016; Gyourko et al., 2021). Researchers use responses to these surveys to build indices of regulation, which they then correlate with housing outcomes.

In principle, an index offers a way around the problem of regulatory diversity. Particularly when they are derived with factor analysis, indices can isolate common underlying trends that might plausibly represent regulatory stringency. In practice, the accuracy of the survey responses, and thus the utility of indices built from them, is an open question. One issue is the surveys' implied faith in planners' knowledge. Planners preside over the regulatory landscape, so arguably they know that landscape better than others. But superior knowledge is not complete knowledge. As regulations become more complex, it becomes less likely that any one person fully understands them, and perhaps less likely still that this person, if they exist, will be the one to fill out an academic survey.[5]

When researchers directly test for survey errors, the results are not encouraging. Lewis and Marantz (2019) examined eight separate land use regulation surveys in California, and found that the same cities would, in different surveys, report different answers to similar questions. These differences, moreover, could not be explained by the time elapsing between surveys. The clearest case comes from 1988: scholars from two separate research projects surveyed the same municipalities. Nine municipalities reported having an urban growth boundary in the first survey but not the second, while five reported an urban growth boundary in the second survey but not the first.[6]

O'Neill et al. (2019), similarly, studied survey responses from eight California cities and found answers that were just wrong. Murray and Schuetz (2019) found the same. To be clear: the direction of error in these cases, when it can be identified, suggests that surveys *underestimate* regulation's impact. So our point is not that survey errors threaten the literature's broader conclusions. They do, however, point to the continuing difficulty of accurate measurement.

A final, intriguing twist in the literature is that even when planners respond inaccurately to specific objective questions, their broad *impressions* of the regulatory environment

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[4] Surveys account for over 20 of the 80 papers we reviewed.

[5] Some surveys, such as Jackson (2018) succeed in having planning directors or other high-level officials respond to them. Even these surveys, however, may not accurately capture the many regulations and processes that planning departments essentially outsource to sister agencies, such as a Building and Safety Department or a Department of Public Works.

[6] For more discussion, see the comments on Lewis and Marantz (2019) in the *Journal of the American Planning Association* (volume 85 issue 4).

seem accurate. Researchers have found, for example, that planners' responses to subjective questions about development constraints predict housing outcomes better than responses about the presence or absence of specific regulations (Jackson, 2018; Lewis and Marantz, 2019). More specifically, in cities where planners agree that "density restrictions" or "land constraints" are large impediments to new housing, new housing is in fact less likely to be built. This is true even when those same planners do not appear to know what the specific constraints or restrictions in their own cities are.

One interpretation of these results is that planners, even when they don't know every aspect of their development codes, do know if their city is a hard or easy place to build. This interpretation is bolstered when we consider that in some ways even these subjective impressions are often incorrect. As we discuss more below, no California city actually has a "land constraint" that prevents building. Some cities have little vacant land, but an absence of vacant land is only a hard constraint if redevelopment is physically impossible, which it almost never is. Redevelopment is instead often *legally* difficult; a point we turn to next.

3. Data and Methods

3.1 Measurement: Process and Prohibition

Our empirical approach begins with a basic observation: the extent and pace of developers' response to rising prices is determined by a market's price elasticity of supply. Supply elasticities have multiple determinants — including the availability of raw materials and capacity of the development sector – but cities influence two of them: the complexity of the production process and the availability of a major input (land zoned for residential development).[7] City policies that complicate the development process (adding hearings or fees), or that reduce the availability of residential land (density restrictions or apartment bans) will make supply less elastic. We can categorize these policies, broadly, as *process* policies and *prohibition* policies. In principle, we can measure them, together or separately, and correlate them with housing outcomes. Additionally, we advance this approach by measuring unbuilt zoned capacity, which captures not only these regulatory prohibitions but also existing structures that will also limit new construction.

In practice, as we mentioned above, such measurement is difficult. A further point is that if we roll process and prohibition policies together into a single metric, as many indices do, we might obscure the mechanisms by which a locality suppresses housing production. This is so for two reasons. First, process policies may matter more when

[7] An additional factor under city control is the ease of entry for firms, which we cannot directly measure but which we assume is roughly the same across California municipalities.

prohibition policies are weaker. In cities where few apartments are allowed, the process for permitting apartments, no matter how cumbersome, is unlikely to be a binding constraint on apartment development. A regression measuring both prohibition and process might show, correctly, that what matters is an overall density restriction, but may also suggest (incorrectly) that a cumbersome process is immaterial. For developers who *do* propose apartments, the process will matter.

Second, process constraints are probably harder to measure than prohibitions. Particularly in places where prohibition does more to limit development (e.g. a typical suburb restricted to single-family homes), process measures may be more prone to reporting errors, because planners will be less familiar with them (e.g. in cities where almost no land is zoned for apartments, planners may not know the steps needed for apartment developers to win approval). Additionally, prohibition measures rarely change. A city's share of land zoned for single-family housing may change once a decade or less (Gabbe, 2019), and zoning maps that show where apartments aren't allowed are usually easily interpretable.[8] Process conditions, in contrast, such as the steps required to obtain a variance, or the circumstances that lead a city to impose cash impact fees or in-kind exactions, change more frequently, and in fact often change from development to development. Process constraints can change noticeably with each new election to the zoning board or city council, and with changes in executive leadership positions like City Planning Director or City Manager. Folding process and prohibition together may thus combine a set of measurements that are systematically more accurate (though not perfectly so) with a second set that is systematically less so.

In combination, these factors also suggest a third problem: process will be more endogenous to supply than prohibition. Places where developers rarely request approvals are unlikely to adopt complex approval procedures. This is the case regardless of demand. If demand is high but a city mostly prohibits apartments, and if no one proposes apartments because of that prohibition, then a complicated process for approving apartments is unnecessary. Similarly, if zoning is permissive but demand is low, and no one proposes apartments because of that low demand, a cumbersome process is also superfluous. Places where developers *do* apply to build, conversely, are more likely to respond with process changes than new prohibitions, precisely because process constraints can change more readily. Blanket downzoning is complicated and time-consuming, but cities can quickly respond to unwanted development pressure by enacting more procedures, enacting moratoria, requiring more exactions, or taking longer to review projects (e.g., Brasuell, 2022).

The net result is a measurement error that probably biases the size of process coefficients toward zero, and an endogeneity that makes their sign more likely to be

[8] Although as we have seen, planners do still err when reporting shares of land zoned multi- or single-family.

positive (places with more permitting have more onerous processes). The takeaway here is twofold: first, measures of prohibition will suffer from less error than measures of process, although both will be prone to error. Second, because of these issues, and because regulations likely represent an underlying attitude toward new housing, researchers might be able to avoid these problems by finding an alternative approach that measures that attitude more directly. It is this latter point that motivates our use of unbuilt zoned capacity metrics.

3.2 Estimates of Unbuilt Capacity

Our primary measure of regulation is unbuilt capacity. California law mandates that jurisdictions periodically estimate their unbuilt capacity for new housing. The state requires these estimates as part of the Regional Housing Needs Assessment (RHNA) planning process, which is in turn a part of the state's Housing Element (HE) law.

Roughly, the RHNA/HE process works as follows. The state assigns each of California's regional governments a target number of units— a regional "housing need"—that is broken down by household income level.[9] This target is based on a projection of future growth, and represents the state's estimate of how much new housing each region will need in the next eight years. The regional governments divide these targets up among their constituent local jurisdictions. The local jurisdictions must then update the HE portion of their general plans to demonstrate that they can "feasibly" add at least as many units as their assigned housing need. "Feasible" has no strict definition, but jurisdictions must identify specific parcels with the potential to hold new housing. Cities complete this exercise, and comply with the law, by presenting an analysis of their unbuilt capacity. The latter must exceed the state-mandated housing target, and it becomes our independent variable of interest.

Compared to typical measures of land use regulation, the HE is useful for three reasons. First, it is compulsory.[10] Local jurisdictions are legally obligated to complete a housing element, making the nonresponse problem that plagues academic land use surveys largely disappear.

Second, the HE is consequential. The HE is the only part of the general plan subject to state review.[11] It can affect local growth in the subsequent eight years, and jurisdictions that do not complete HEs face legal consequences. For these reasons, cities give the

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[9] State guidelines use density as a proxy for household income. Income requirements thus require zoning at density thresholds, e.g. jurisdictions in metropolitan statistical areas must have sites developable at 30 units per acre to satisfy targets for low-income housing.
[10] As we will discuss, not every city reports multifamily capacity. But every city *is* required to estimate total capacity, and virtually all do.
[11] See the website of HCD for details on the HE guidelines: http://www.hcd.ca.gov/community-development/building-blocks/index.shtml

HE focused attention. Where jurisdictions may devote little time or resources to completing academic surveys, they regularly assign senior planners and/or hire consultants to complete their HEs. The resulting studies are often thorough and lengthy.[12]

Third and most important, the HE is fundamentally a political exercise. Cities draw on technical information to justify their Housing Elements but their estimates of unbuilt capacity reflect strategic behavior at both stages of the process (Monkkonen et al., 2019). In principle, regional governments assign housing allocations to local governments based on need. In practice, local governments lobby regional governments to minimize their allocations—a process enabled in part by local elected officials comprising most regional government leadership. The HE also lends itself to manipulation because allocations of need are based on projections of growth rather than prices, and the projections of future growth are based on past growth (Dillon, 2017). The RHNA thus rewards cities that resist housing; blocking growth in one RHNA cycle yields a lower allocation in the next.

Once assigned a target, cities again have an opportunity to behave strategically, this time when they prepare their HE. The easiest way to demonstrate unbuilt capacity is to identify parcels of vacant land. This is what most low-demand cities do. Almost by definition, however, vacant land is less common in higher-demand cities. A dearth of vacant land does not preclude increasing unbuilt capacity: cities with little vacant land can meet their allocations by rezoning some low-density parcels to allow denser redevelopment. This approach is fiscally almost costless--it can be done with the stroke of a pen—but is only appealing if cities are open politically to increasing their densities. Until recently they were not: few cities rezoned to meet their housing targets in 2014 (Monkkonen et al., 2023).

Expensive cities that are not open to higher density, and want to avoid rezoning, take a different path: identify sites already zoned for multifamily housing that currently hold existing, viable business, and predict that these sites will be redeveloped into apartments (e.g., rather than allow duplexes in a single-family neighborhood, cities can claim that shopping malls will be demolished and replaced with apartments). These predictions are notoriously inaccurate. A study of sites project to hold new housing San Francisco Bay Area HEs found that only 10% were actually developed over the planning period, and that a majority of the development that did occur in these cities took place on sites not even listed in the HEs (Kapur et al., 2021).

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[12] For example, the 2021 report for West Hollywood, a small city in Los Angeles County, is 28 pages of main prose with a 154-page technical appendix. The city hired a consulting firm, assigned full-time planners, and assembled a working group of residents to complete it, and the process took over a year. The typical land use survey, in contrast, is 14-30 pages long and usually filled out by one person.

This inaccuracy could arise from error, from strategic behavior, or both. Anecdotally, strategic behavior appears the more likely culprit. Early in the 6th HE cycle, for example, the City of Vista listed both its own City Hall and public library as sites likely to be torn down and redeveloped as high-density housing. South Pasadena predicted that every major grocery store would be demolished and redeveloped as affordable housing. And the pastor of a church in the affluent suburb of La Canada Flintridge told a local newspaper that she agreed to let the city list church property as a site for future affordable housing, since “everyone knows” it will “never be built.” (An exact quote: “We thought … ‘what’s the harm in letting our property be listed as one of the imaginary sites where it could be built, for the sake of submission to the state’? So we did the city a favor” (Pringle 2022).

From our perspective, this inaccuracy—and the fact that strategic behavior likely contributes to it—is a feature rather than a bug. One reason the predictions are inaccurate is that many local governments do not want to allow more housing; they want to meet their planning targets with minimal risk of actual redevelopment. Anecdotally, these cities often report only their legally required level of unbuilt capacity: in each cycle they have just enough room to hit their assigned housing targets, but no more. They are, in their own judgment, “built out.”[13]

It is worth reiterating that buildout is a subjective judgment. No California cities are in fact physically built out. Even in places with topographical constraints, at a minimum almost every single-family home can be replaced with a duplex. The modern elevator was invented in 1903, and engineers have known for half a century how to construct buildings over 100 stories tall. Most of urban California is between one and two stories. Buildout is a political construct, and cities that determine themselves closer to buildout are presumably more hostile to development.[14]

Understanding this point can help resolve some otherwise unusual findings in the literature. Jackson (2018), for instance, argues that “Housing supply in California cities

[13] The RHNA process lends itself to such manipulation in part because it was designed in an era when almost all cities had abundant vacant land, and were thus expected to meet their targets by identifying vacant parcels (Baer, 2008).

[14] Beverly Hills illustrates our reasoning here. The city is well known for behaving strategically in the RHNA process (Dillon, 2017). In 2019, the city protested its multifamily housing allocation and warned, in a letter to the regional government, that forcing it to add apartments would actually undermine affordability. The city argued that as a result of its land constraints, it could only add more multifamily housing if it demolished its existing rent controlled apartment buildings and redeveloped those sites at higher densities. This statement is absurd. Most of Beverly Hills’s residential land is on flat terrain zoned for detached single-family housing, the vast majority of which sells for well over $2 million. Nothing but the city’s zoning stops these sites from being redeveloped at higher densities. The city’s warning about apartment demolition is only true if it chooses not to rezone, and nothing, other than politics, stops it from rezoning.

is made inelastic by land constraints, not regulation." But most land constraints *are* regulations. Jackson draws his conclusion from responses to a survey question about "developable land." With few exceptions, however, (such as steep hills and seismic areas), "developable" is a subjective determination, and it often rests on an assumption that once a parcel has a structure on it, it cannot be redeveloped at higher density. Virtually the entire history of urbanization, of course, suggests otherwise—Manhattan once had large parcels dotted with farmhouses. Planners responding to surveys might *believe* buildout represents a land constraint, but it is more accurately thought of as a political constraint placed on land.

We obtained the estimates of unbuilt capacity for 414 out of 482 of California's cities for the period 2014-2021, and estimates specific to unbuilt multifamily capacity for 346 cities. The state government had not yet standardized the reporting format in 2014, so some cities did not report their multifamily capacity independently.

3.3 Regulatory Process and Prohibition Indices

We supplement our unbuilt capacity measure with two conventional metrics of regulation, one measuring process and one measuring prohibition.

We build our Process Index from eight questions in Jackson's 2018 survey of 450 California municipalities. These questions ask about the number of regulatory bodies that must grant permission for a residential development to proceed, whether planning staff can grant some development approvals or if elected officials or commissions must weigh in, how often permit-granting entities meet, and so on. We transformed responses to each question into binary variables, to reflect above or below average burden, and then sum the variables to obtain our index. The index ranges from one (low process burden) to eight (high) and the median city scores four. Appendix B contains more detail.

Our Prohibition Index comes from the Mercatus-Augmented Terner California Housing Regulation (MATCHR) survey, which covers over 250 California jurisdictions (Furth and Gonzalez 2019). MATCHR is, as its name suggests, an expanded version of a land use survey carried out by UC Berkeley's Terner Center. MATCHR's creators followed a procedure similar to the one used to build the Wharton Residential Land Use Regulation Index (Gyourko et al 2008); a factor analysis that first aggregates responses about zoning rules, and then condenses them into a single number.

Specifically, MATCHR's creators use five inputs: single-family minimum lot sizes, single-family parking requirements, an index of single-family setbacks, the share zoned for multi-family housing, and an index of sixteen multifamily regulation variables (including parking requirements and development standards like setbacks and open space rules). The factor analysis identified a single latent (unobserved) factor underlying these six inputs (which represent more than six regulations, since two the inputs are indexes). For the roughly 250 cities with data, the MATCHR index ranges from -1.5 to 3.9 with a median of -0.14.

We expect the Prohibition Index to be negatively correlated with unbuilt capacity, since places with strict density restrictions often have less space for new housing. We see that this is so. We also observe, however, that unbuilt capacity captures more information about development potential than the Prohibition Index, because it accounts for existing buildings. Some cities zoned entirely for single family homes could have plentiful undeveloped land. The Prohibition Index might record these cities as stringently regulated, yet they could still add housing.

The MATCHR Index illustrates both the advantages and disadvantages of regulatory indices. It condenses a tremendous amount of information into a single number. It also, however, replaces the black box of regulation with the black box of an index, and appears to be sensitive to outliers (Furth and Gonzalez, 2019). Furth and Gonzalez (2019) report that MATCHR's correlation with housing development appears to be driven primarily by "extreme values"—a handful of affluent suburbs with very low-density zoning.

3.4 Models

We assume that more housing construction will occur in cities with higher rents (a proxy for returns on development) unless regulation in some form prevents it. Our regressions thus examine associations between permitting, regulation, and demand. Our models do not control for endogeneity, but our decision to model supply should mean that any endogeneity will favor smaller coefficients or null significance tests.

In our first regressions, we test three variations of the hypothesis that cities with more onerous regulations will permit less new housing. (The three variations are less unbuilt capacity, more prohibitions, and more complex processes. As discussed above, we expect the Process Index to be biased toward zero). Our regressions separately examine all building permits issued in a city between 2014 and 2019, and multifamily permits. The models take the following form:

$$\text{Permits } 2014\text{-}2019 = \alpha + \beta_1(\ln(\text{Rent}) + \beta_2\text{Reg} + \beta_3\text{City} + \beta_4\text{Dem} + \text{Metro} + e$$

Where permits are either total permits or multifamily permits, rents are measured in 2013, *Reg* is one of three regulatory indexes (process, prohibition, or unbuilt capacity), *City* denotes a vector of city controls including city size, job accessibility, and population density, and *Metro* is a metropolitan area fixed effect.

We use negative binomial regressions given that permits are an overdispersed count variable, and that in our sample multifamily permitting has a substantial number of 0 values. Multifamily housing production was roughly half of total housing production in California during our study period, but nearly one third of the cities in our sample built no multifamily units during that time.[15]

The second set of regressions interact our measures of regulation with rent. These terms offer an explicit test of the idea that regulation binds in the presence of demand. We hypothesize that this relationship will be harder to demonstrate with a conventional regulation index. Cities that are permissive on paper may have limited undeveloped parcels, and thus fewer places to put buildings that they nominally allow. Two cities that allow multifamily development on half their land might look almost identical in a Prohibition Index. But if one has an abundance of vacant land and the other has none, the latter is excessively regulated and the former may not be.

Our modeling strategy and measure is complementary to other approaches, especially the papers using versions of what Zhang (2022) calls the shadow price approach. Existing work using this identification strategy, like Brueckner and Singh (2020) yields many insignificant models and some implausible results (e.g. San Francisco has less stringent regulations than Chicago). These likely result from their measure of regulation (FAR), which does not capture the cumulative impact of the many different avenues cites can take to constrain production. Our approach, which explicitly accounts for any given regulation being endogenous to a political environment—and thereby likely correlated with other regulatory characteristics, be they observed or unobserved—should overcome this problem.

<<Table 1 here>>

We report descriptive statistics for the variables we will use in our analysis in Table 1.

4. Results

4.1 Understanding Unbuilt Capacity

Our first results simply describe the unbuilt capacity variable and consider its determinants. As a share of existing housing units, the median jurisdiction reports sufficient unbuilt capacity to grow its housing stock by 13%. Considerable variance exists around this median. The city with the lowest unbuilt capacity reports only being

[15] All but three municipalities that did not permit new multifamily housing do have existing multifamily housing. In fact, 22% of the housing stock of the median municipality that did not permit any during this time period was multifamily housing, only slightly lower than the median municipality that did permit new multifamily (at 31% of stock).

able to add 1% of its current stock, while the city with the highest unbuilt capacity can grow its stock over 200%. Appendix C Figure 1 is a density plot.

Figure 1 is a choropleth map of California counties that illustrates the median city's capacity for new housing as a share of existing housing. The map clearly shows that coastal counties, where the demand for housing is higher, report substantially less capacity for new housing development. Figure 2 also shows unbuilt capacity as a share of existing units, but does so for cities in the Southern California Association of Governments (SCAG) region. The same pattern exists within regions. Coastal and expensive jurisdictions report less space for growth in their zoning codes, with the exception of large cities like Los Angeles and San Diego.

<<Figures 1 and 2 here>>

Comparing Figures 1 and 2 also suggests that unbuilt capacity varies less across regions than within them. A coefficient of variation confirms this: the coefficient for unbuilt capacity as a share of existing housing across regions is 0.73, whereas within regions it is 1.16 on average. Only in the San Diego region is there less variation across municipalities than across the state's regions, at 0.63. In Appendix C Figure 2, we present a graph of the distribution of unbuilt capacity as a share of existing housing across regions.

The varied distribution of unbuilt capacity is consistent with the idea that these numbers arise from a combination of history and politics. Because the RHNA process only asks that cities find *room* for new housing (it does not demand physical units) absent strategic behavior the variation across cities in estimated capacity should be low (cities can always rezone). That is not the case.

We can, moreover, further examine the idea that places with more demand and more existing density report less capacity. Previous work has shown that much of the state's total unbuilt capacity of 2.8 million new housing units is in the counties where demand is low (Monkkonen and Friedman, 2019).

In Table 2, we present results from regressions of unbuilt capacity on potential determinants like existing density, housing values, an index of regulation, the age of housing stock, and demographic characteristics often associated with opposition to new housing: share of residents over 65 years old, share homeowners, and share White (Fischel 2001; Einstein et al.,2020; Fang et al., 2022). We include scatterplots between unbuilt capacity and these potential correlates such as the two indexes of regulation, permitting activity, population density, housing values, and the age of the housing stock in Appendix C Figure 3.

<<Table 2 here>>

The results show that controlling for population, higher density cities have less unbuilt capacity, as do cities with more valuable housing, a larger share of housing built before 1990, and more homeowners. Neither of our regulatory indexes have statistically significant associations with unbuilt capacity, even in more parsimonious regressions (not shown) that include only population as a control. This illustrates the importance of measuring build out. Cities can score low on a regulation measure because their zoning allows a substantial amount of multifamily housing, yet if all or most multifamily zoned parcels are built out, they will have limited capacity to add new housing. This is borne out in the data. There are cities in our data with strict regulations but many vacant parcels, as well as cities with permissive regulatory environments but parcels built out to their zoned maxima.[16]

Neither the share over age 65 nor the share White are not associated with unbuilt capacity, suggesting that if regulatory buildout reflects NIMBYism it is more closely aligned with home values and ownership, than with demographic attributes that tend to be correlated with conservative preferences (Fischel, 2001; Kahn, 2011; Manville 2021).

The importance of buildout explains Murray and Schuetz' (2019) finding that cities in California with high rents did not build more apartments than others. Expensive cities tend to have built to their zoned capacity and do not change zoning, or downzoned in a way that gives them relatively little room to grow.[17]

4.2 Analyzing Permitting

We now turn to actual supply. Table 3 reports the results of five models, where the dependent variable is all permits issued, regressed on our measures of regulation with controls for population, population density, rents in the year preceding the permit data, jobs accessibility, share multifamily in the city, median household incomes, race/ethnicity, education levels, and the recent (2009-2013) change in rents. We report the full model results in Appendix C Table 2.

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[16] Palmdale, for example, is in the top 5% of stringency as measured by the Prohibition Index (multifamily housing is not allowed in most of the city), but also top 5% of unbuilt capacity (it has a lot of vacant parcels). Rancho Santa Margarita, in contrast, scores near the bottom on the Prohibition Index, but is by its own judgment almost completely built out. It reported only two units of unbuilt capacity in its housing element. Nearly 50 of the 250 cities for which we have regulation data score below average on regulatory stringency, above average in share multifamily, but have a below average level of unbuilt capacity.

[17] Beverly Hills again provides an illustration. The city, which had 13,000 housing units and a median rent of $2,043 in 2013, was given a RHNA allocation of two units and reported an unbuilt capacity of 732 (Dillon, 2017). In contrast, in the same year the City of Coachella, which had 15,000 housing units and a median rent of $897, was assigned over 5,000 units and reported an unbuilt capacity of over 10,000.

<< Table 3 here >>

Cities with less unbuilt capacity permitted less housing. A one standard deviation decrease in unbuilt capacity is associated with an 84% decrease in housing permits (Model 1). Cities with more stringent prohibitions issued significantly fewer housing permits. A one standard deviation increase in the Prohibition Index was associated with a 27% reduction in building permits (Model 2).[18] The Process Index is not statistically significantly associated with permitting levels (Model 3). The Process coefficient, moreover, is nearly zero, suggesting that an onerous permitting process has little economic significance with respect to housing production. As discussed previously, this result likely reflects a bias toward zero.

Model 4 combines all three measures of regulation. Doing so reduces the sample size dramatically (to 135 observations), mostly as a result of missing values for the Process Index. The results show a strongly increased correlation between the Prohibition Index and permitting This result, however, appears primarily to be a result of the constrained sample. In Model 5 we exclude the Process Index and estimate a regression with Prohibition and unbuilt capacity. In this model with a larger sample size, a one standard deviation increase in the Prohibition Index is associated with 17% less permitting, whereas the relationship to a one standard deviation increase in unbuilt capacity remains similar to our when we do not control for Prohibition or Process (Model 1), 80% fewer permits issued.

Table 4 essentially repeats the analysis of Table 3, but uses as the dependent variable multifamily permits from 2014-2019 (the controls are the same as in Table 3). Fewer cities reported multifamily capacity so the sample size is smaller. We find no statistically significant difference in the population, incomes, or permitting levels of cities for this smaller sample of cities. Full results in Appendix C Table 3.

<<Table 4 here>>

The coefficient on unbuilt capacity decreases when associated with multifamily permitting, though it remains high. A one standard deviation decrease in a city's unbuilt capacity is associated with 45% fewer multifamily permits issued. The coefficient on the Prohibition Index, on the other hand, is larger than in the models predicting all permits, with a one standard deviation increase in the Prohibition Index associated with 47% fewer permits. Given that several components of the Prohibition Index are directly related to restricting multifamily housing, this result is expected.

[18] Given Furth and Gonzalez's (2019) finding of influential outliers in the Prohibition Index, we re-estimated model 2 on a sample restricted to observations within one standard deviation of the Prohibition Index's mean. In this estimation the Prohibition coefficient is no longer statistically significant. Unbuilt capacity is not sensitive to outliers in the same manner.

As with the previous models, we run a model with the Prohibition Index as well as the measure of unbuilt capacity (including the Process Index reduces the sample size substantially, in this case to only 111). In this model (model 5), the coefficients change only slightly, with unbuilt capacity increasing to 51% and the Prohibition Index decreasing to 45%. The Process Index is not significantly associated with multifamily permitting.

4.3 Interacting Rents with Regulation and Unbuilt Capacity

In Table 5 we present regressions that interact regulation with rents. Here we see our most substantial finding: unbuilt capacity matters much more in expensive cities, and rents are more likely to predict new supply in cities with space for new housing in their zoning code. Cities that have ample space for new housing in their zoning code but also have low rents do not permit much housing. It is only in cities that are both high rent and have unbuilt capacity that permitting happens in substantial numbers. This result is intuitive: developers want to build where returns are highest, so cities with low rents permit very little housing regardless of their regulatory landscape.

<<Table 5 here>>

The interaction term is not significant for the two survey-based measures of regulation, suggesting that these measures correlate with production in the same way in cities with varying rent levels. This finding, in turn, suggests a deficiency in the prohibition measure: land use regulations are unlikely to bind equally regardless of demand. This deficiency may owe to the issue we mentioned earlier: some tightly zoned cities have many vacant parcels, while some cities with many parcels zoned for multifamily may not.

We illustrate these interactions in Figures 3 and 4, which show the models' predicted permitting levels (of all housing in and of multifamily housing) as a function of both rents *and* unbuilt capacity. The positive interaction term means that unbuilt capacity matters more at higher rent levels. As an example, consider two cities with rents at the 30th percentile (roughly \$1,100) but unbuilt capacity at the 20th and 80th percentiles. The city with more space is predicted to permit well over twice as many housing units between 2014 and 2019, 860 compared to 390 units. However, for two cities at the 70th percentile of rents (roughly \$1,650) with the same distinction in unbuilt capacity, the city with more space is predicted to permit nearly *four times* as many units, 1,660 units compared to 460.

The model predicts housing will only be permitted in large numbers by cities where rents are high *and* the zoning code has space. We observe an inflection point for cities above the 60th percentile in rent, which in 2013 was roughly \$1,520. Above this rent level, the relationship between unbuilt capacity and permitting increases substantially.

There are not very many of these places, however. Of the over 100 cities in the top quartile of rent, only 13 are also in the top quartile of unbuilt capacity and only five are in the top quartile of capacity as a share of existing stock[19]. (This may help explain why, of the 40 cities with the highest rents, 29 were in the bottom quartile for housing growth). Only one city, Irvine, is in the top 10% of both rents and capacity. We present the joint frequency distribution of rents and unbuilt capacity in Appendix C Figure 4.

<<Figures 3 and 4 here>>

Figure 4 shows that the interaction between rent and capacity in predictions of permitting is even more important for multifamily housing—a result that owes largely to the limited association between unbuilt capacity and permitting in cities with low rents. The model predicts that cities in the 10th percentile of rents with less unbuilt capacity will actually have higher levels of permitting. At the 30th percentile of rents, however, moving from the 20th to 80th percentile of unbuilt multifamily capacity is associated with a 40% increase in permitting, whereas at the 70th percentile of rents the model predicts a 120% higher level of permitting when moving from 20th to 80th percentile capacity.

As robustness checks, we run two sets of models. One uses housing prices to capture demand instead of rents and the other excludes cities that did not permit any multifamily housing. The results of these checks, reported in Appendix C Tables 4 and 5, are similar in both the significance and direction of coefficients.

5. Discussion and Conclusions

There is strong reason to believe that land use regulations increase housing prices, but testing that relationship empirically is difficult. Regulation is notoriously difficult to measure: different cities have different rules, not every regulation is equally burdensome in every context, and regulations that are individually benign can, in combination, become large barriers. Even assuming regulation can be measured, moreover, endogeneity can confound attempts to persuasively link it with price. Regulated places might, for reasons unrelated to supply, command a price premium.

We address these obstacles in two ways. First, we model regulation's impact on supply, rather than price. Supply is also endogenous to regulation, but its endogeneity creates a nullifying rather than a confirming bias, meaning that our results may if anything conservative estimates of regulation's effect.

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[19] The 13 cities with high rents and high absolute capacity are Brentwood, Fremont, Hermosa Beach, Irvine, Milpitas, Newport Beach, San Jose, San Mateo, San Ramon, Santa Clara, Santa Clarita, and Sunnyvale. The five cities with high rents and high capacity relative to size are: Emeryville, Hercules, Hermosa Beach, La Habra Heights, and Milpitas.

Second—and this is our primary contribution—we eschew conventional measures of land use regulation and instead measure the underlying political sentiment that regulation represents, which is a locality's willingness to accept new housing. A proxy for that sentiment is available to us because California state law requires cities to periodically estimate how much additional housing they can hold. The judgment that informs those estimates is largely political. Even cities with steep terrain and little vacant land can substantially increase their unbuilt capacity by allowing parcels zoned to hold one unit to instead hold two. Given that many local governments zone mostly for single-family homes, such a change could almost double allowed housing capacity. Differences in unbuilt capacity represent differences in openness to development, and these attitudes manifest in (hard-to-measure) regulations.

Using simple models, we show that this measure of openness to new development has associations with new housing supply that are equal to or larger than conventional regulatory indices. We also show that this buildout measure captures regulation's interaction with demand in ways that conventional indices do not.

One could draw the wrong lesson from these results, and arrive at the zoning version of "guns don't kill people, people kill people." Such a conclusion, that regulations are irrelevant, would be mistaken. The fact that underlying causes matter doesn't mean proximate causes don't. Some regulations really are more burdensome than others. Our point is only that a regulatory regime is the legal embodiment of a political atmosphere, and metrics that better reflect that reality may also better predict housing outcomes.

Such metrics may also lend themselves more readily to policy. The literature on regulation and housing supply is not new, but a political movement determined to apply its lessons is (e.g. Dougherty, 2020). The rise of pro-housing activists determined to act on economic research casts into sharp relief one limitation of regulatory indices: it is difficult for local or state officials to understand and change them. A zoning envelope, as a simple measure of how much housing a city allows, is different. It is imperfect, but almost certainly more tractable.

Our results suggest, in fact, that state governments interested in more housing production overall would do well to focus on increasing the zoned capacity in expensive cities (and the expensive neighborhoods of these cities). States could directly incorporate this lesson, for example, into the allocation methodologies of fair share housing plans, by allocating higher shares to places with higher rents. Alternatively, if state governments considered targeted preemption of local zoning based on proximity to public transportation, they should do so guided by not just proximity to transit but also demand. State preemption of exclusionary zoning will have a larger impact in high rent cities and neighborhoods. (Our analysis does not tell us directly that the relationships we observe apply to high rent neighborhoods in relatively inexpensive cities, but we anticipate that they do.)

For the same reason, our results also speak to concerns that zoning reform will harm lower-income communities, by subjecting them to waves of development and gentrification. This concern often leads to debates over whether new development does in fact harm lower-income neighborhoods, and whether it accelerates or delays gentrification. Our analysis, however, suggests that widespread upzoning would concentrate new development in places where rents (and thus presumably incomes) are already high.

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Table 1. Descriptive Statistics of Main Variables

Variable	Median	Standard Deviation
Total Unbuilt Capacity	1,641.00	18,345.50
Multifamily Unbuilt Capacity	956.00	7,962.00
Unbuilt Capacity / Housing Units	0.13	0.56
Prohibition Index (MATCHR)	-0.14	0.87
Process Index	4.00	1.76
All permits 2014-2019	271.00	4,712.71
Multifamily permits 2014-2019	42.00	3,929.70
Median rent, 2013	1,374.50	500.04
Median home value, 2013	403,100.00	361,532.09
Share of housing built before 1940	0.05	0.09
Share of housing built before 1990	0.61	0.20
Population density (per square mile)	3,607.99	3,453.60
Total population	31,864.50	211,072.92
Job Accessibility (within 20 minutes)	14,910.33	28,576.71

Source: California Housing Elements, Furth and Gonzalez (2019); Jackson (2016); US Census.

Table 2. OLS Results; DV = Unbuilt Capacity 2014

Variable	1	2	3	4
Pop. Density (log)	-0.530***	-0.440***	-0.408***	-0.476***
	(0.074)	(0.075)	(0.146)	(0.076)
Median Hsg. Value (log)	-0.696***	-0.614***	-0.631***	-0.390***
	(0.116)	(0.115)	(0.181)	(0.147)
Job Accessibility	-0.056	0.020	0.115	-0.031
	(0.062)	(0.064)	(0.106)	(0.068)
Share housing built pre 1940	0.634			
	(0.602)			
Share housing built pre 1990		-1.036***	-1.003**	-1.245***
		(0.296)	(0.436)	(0.309)
Prohibition Index			-0.122	
			(0.092)	
Share homeowners				-1.441***
				(0.396)
Share of pop. over 65				-0.593
				(0.837)
Share of pop. White				-0.118
				(0.307)
Constant	10.73***	9.588***	9.253***	8.496***
	(1.531)	(1.515)	(2.454)	(1.653)
Observations	413	413	228	413
R-squared	0.640	0.650	0.633	0.664

Notes: Log of Population is included as a control, along with region fixed effects.

Table 3. Negative Binomial Models; DV: All Permits 2014-2019

Variables	1	2	3	4	5
Unbuilt Capacity (log)	0.405***			0.340***	0.388***
	(0.065)			(0.095)	(0.071)
Prohibition Index		-0.263***		-0.349***	-0.160**
		(0.087)		(0.135)	(0.081)
Process Index			0.056	-0.014	
			(0.041)	(0.047)	
Median Rent, 2013 (log)	0.801*	0.0854	0.073	0.384	0.490
	(0.436)	(0.525)	(0.663)	(0.763)	(0.464)
Population (log)	0.773***	1.171***	1.059***	0.881***	0.818***
	(0.089)	(0.061)	(0.094)	(0.118)	(0.088)
Population density (log)	-0.184**	-0.499***	-0.478***	-0.465***	-0.334***
	(0.092)	(0.142)	(0.182)	(0.154)	(0.125)
Job Accessibility (log)	-0.009	-0.159	-0.111	-0.237*	-0.072
	(0.101)	(0.136)	(0.175)	(0.132)	(0.123)
Multifamily Housing (%)	0.713	1.355*	1.521**	1.051	1.063
	(0.495)	(0.720)	(0.706)	(0.692)	(0.649)
Lnalpha	-0.175**	-0.258**	-0.119	-0.366**	-0.381***
	(0.082)	(0.104)	(0.107)	(0.155)	(0.111)
Constant	-9.235***	-2.276	-0.917	-3.327	-6.060*
	(3.000)	(3.657)	(3.713)	(4.606)	(3.298)
Observations	412	227	231	135	227
Pseudo R2	0.089	0.088	0.060	0.075	0.096

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses. Models include controls for median household income, share of the population with a BA or higher education, share Black, share Latino, share Asian, and the percent change in median rent from 2009 to 2013. Full results reported in the Appendix.

Table 4. Negative Binomial Models; DV: Multifamily Permits 2014-2019

Variables	1	2	3	4	5
Unbuilt Capacity (log)	0.267***			0.272	0.295***
	(0.102)			(0.187)	(0.105)
Prohibition Index		-0.448**		-0.574*	-0.431***
		(0.198)		(0.329)	(0.131)
Process Index			0.128	0.055	
			(0.079)	(0.102)	
Median Rent, 2013 (log)	2.094**	0.203	-0.788	3.424**	1.309
	(1.031)	(0.989)	(1.387)	(1.629)	(0.876)
Population (log)	1.063***	1.518***	1.619***	1.135***	1.131***
	(0.153)	(0.129)	(0.167)	(0.279)	(0.141)
Population density (log)	0.029	-0.370	0.329	-0.466	-0.244
	(0.241)	(0.255)	(0.342)	(0.415)	(0.206)
Job Accessibility (log)	-0.077	-0.075	-0.451*	-0.080	0.088
	(0.177)	(0.199)	(0.256)	(0.296)	(0.170)
Multifamily Housing (%)	2.799**	2.782**	3.238**	2.638	1.924
	(1.127)	(1.315)	(1.386)	(1.984)	(1.203)
Lnalpha	1.088***	0.742***	1.035***	0.615***	0.689***
	(0.078)	(0.092)	(0.093)	(0.132)	(0.136)
Constant	-23.76***	-10.730	-8.587	-28.50***	-17.84***
	(7.115)	(6.852)	(8.699)	(10.290)	(6.106)
Observations	333	227	231	111	184
Pseudo R2	0.062	0.082	0.056	0.067	0.073

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses. Models include controls for median household income, share of the population with a BA or higher education, share Black, share Latino, share Asian, and the percent change in median rent from 2009 to 2013**.** Full results reported in the Appendix.

Table 5. Negative Binomial Models; DV: All Permits and Multifamily Permits with Interaction Terms

	All Permits 2014-2019			Multifamily Permits 2014-2019		
Variables	**1**	**2**	**3**	**1**	**2**	**3**
Unbuilt Capacity (log)	-3.56***			-5.14***		
	(0.77)			(1.59)		
Prohibition Index		-0.85			0.35	
		(1.42)			(3.11)	
Process Index			0.42			-0.72
			(1.00)			(2.03)
Rent * Unbuilt Capacity (log)	0.55***			0.75***		
	(0.11)			(0.22)		
Rent * Prohibition Index		0.07			-0.13	
		(0.18)			(0.41)	
Rent * Process Index			-0.05			0.12
			(0.14)			(0.28)
Median Rent, 2013 (log)	-2.81***	0.72**	0.84	-3.93***	0.44	0.07
	(0.79)	(0.32)	(0.57)	(1.46)	(0.57)	(1.13)
Population (log)	0.71***	1.13***	1.00***	0.99***	1.41***	1.52***
	(0.09)	(0.06)	(0.09)	(0.14)	(0.12)	(0.16)
Population density (log)	-0.21**	-0.53**	-0.55**	0.30	-0.18	0.31
	(0.10)	(0.13)	(0.17)	(0.22)	(0.25)	(0.32)
Job Accessibility (log)	0.04	0.02	0.05	0.15	0.20	0.01
	(0.08)	(0.10)	(0.15)	(0.14)	(0.17)	(0.20)
Lnalpha	-0.21**	-0.23**	-0.08	1.12***	0.80***	1.08***
	(0.08)	(0.10)	(0.11)	(0.08)	(0.09)	(0.09)
Constant	17.39***	-6.64***	-6.57*	17.42	-13.19**	-14.63*
	(5.95)	(2.49)	(3.94)	(10.88)	(4.26)	(8.41)
Observations	412	227	231	333	227	231
Pseudo R2	0.09	0.09	0.06	0.06	0.08	0.05

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses.

Figures

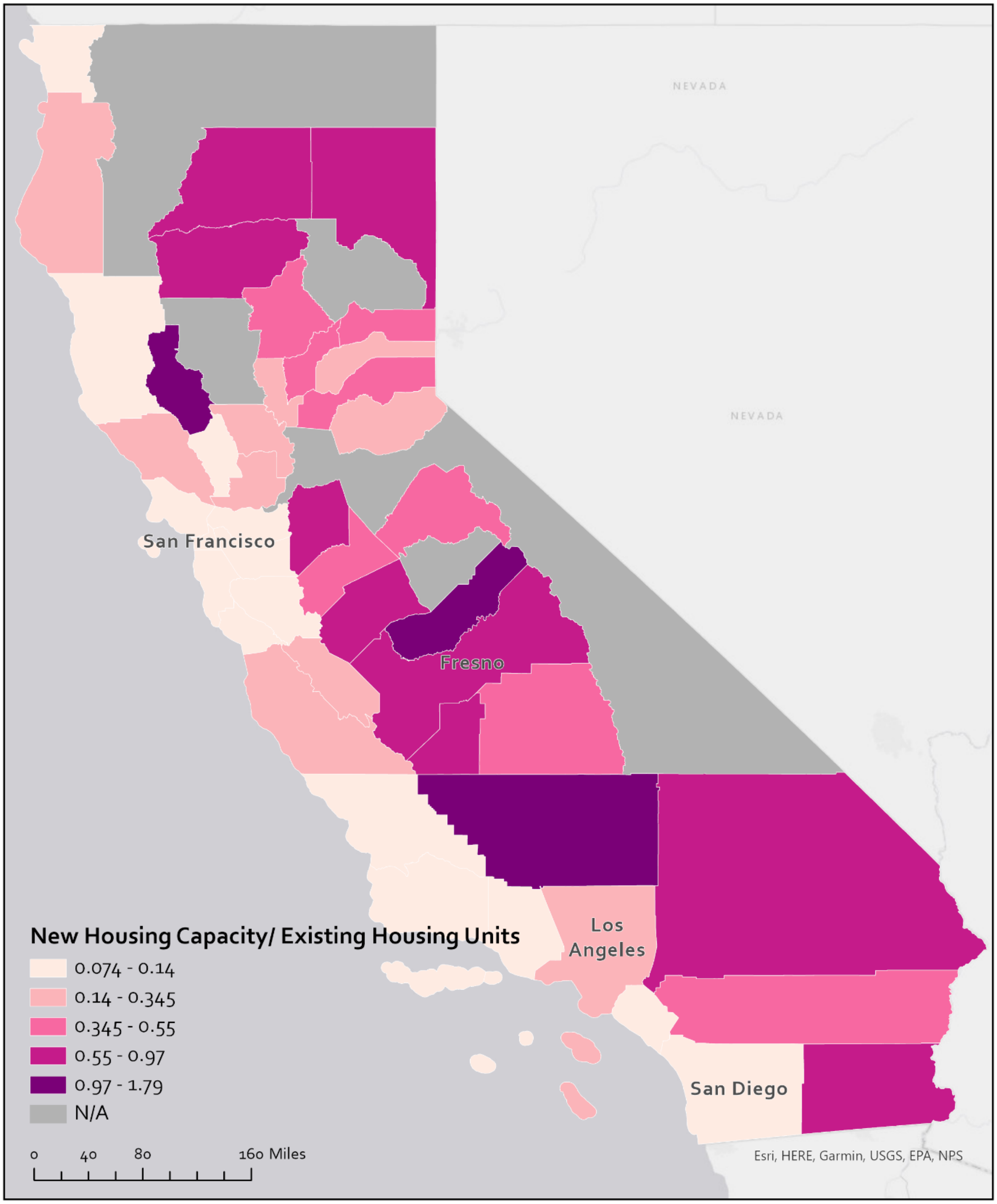


Figure 1. Median values of unbuilt capacity as a share of existing housing units by County

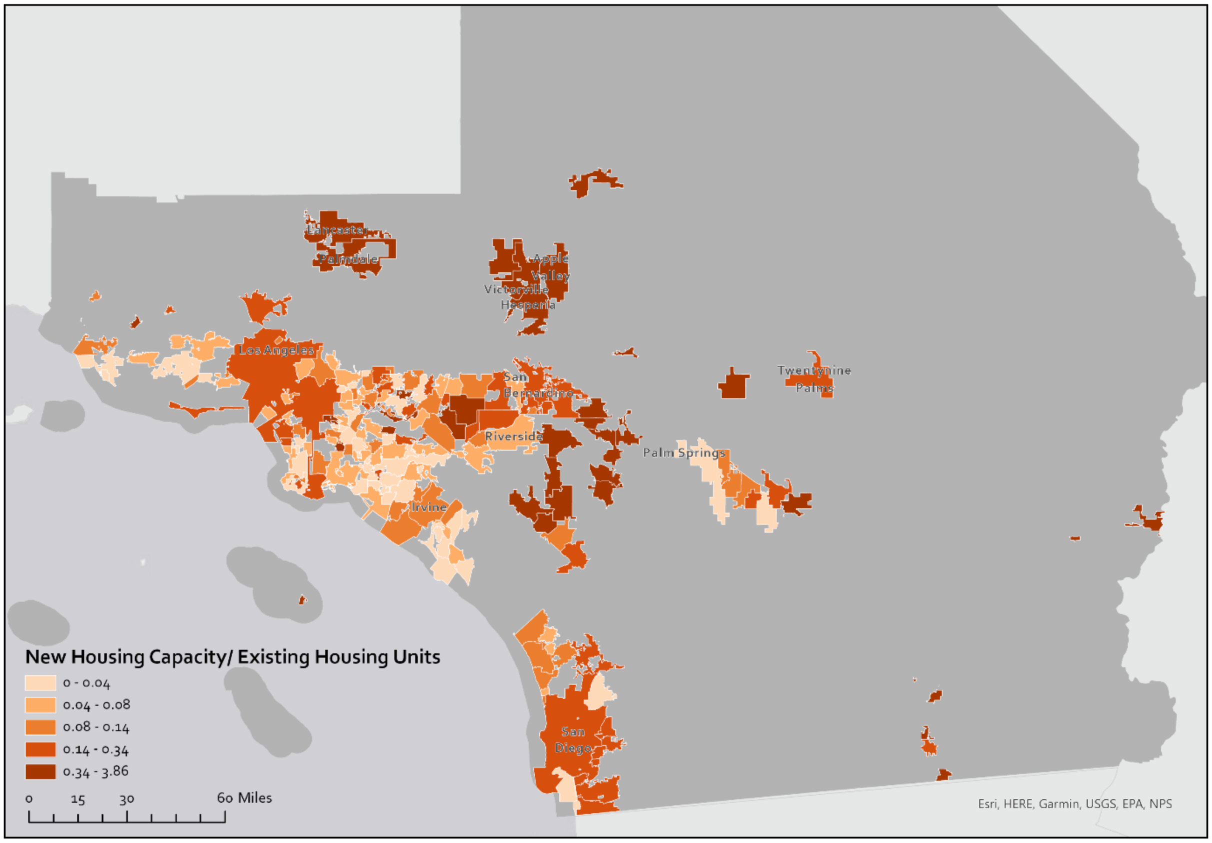


Figure 2. Unbuilt capacity as a share of existing housing units in the cities of the Southern California Association of Governments (SCAG) region

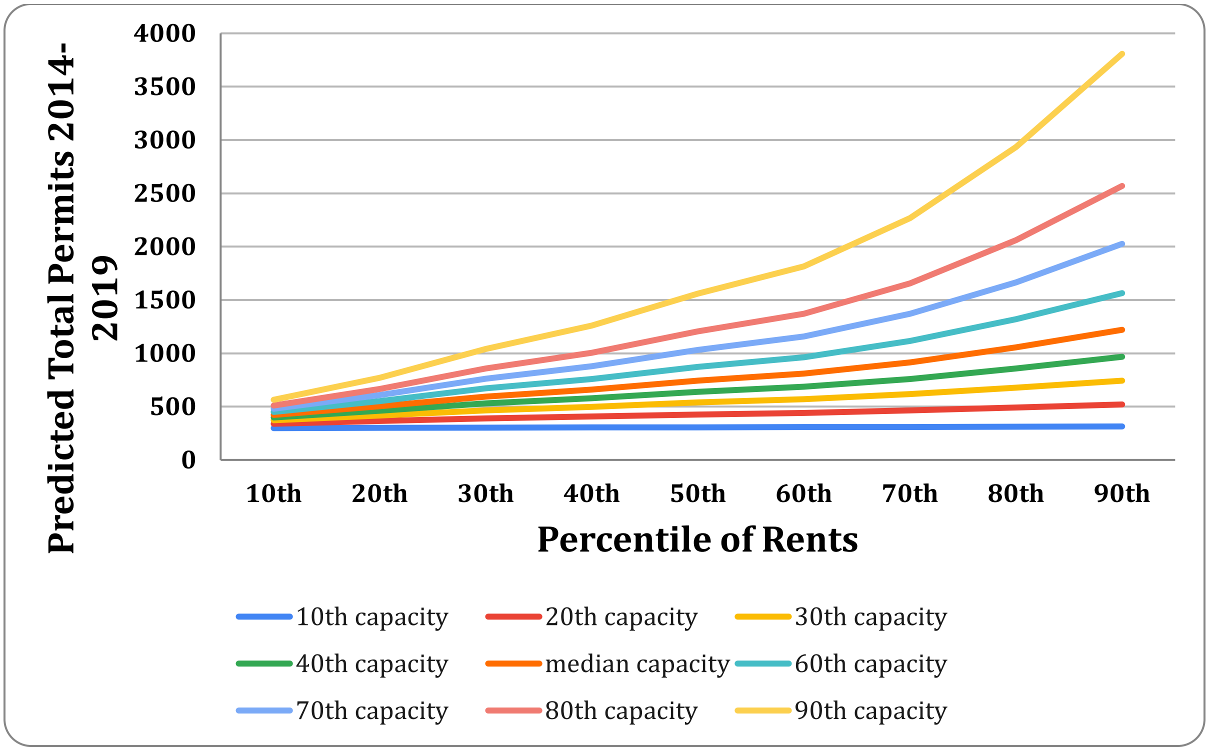


Figure 3. Predicted Total Permits as a Function of Rent and Unbuilt Capacity

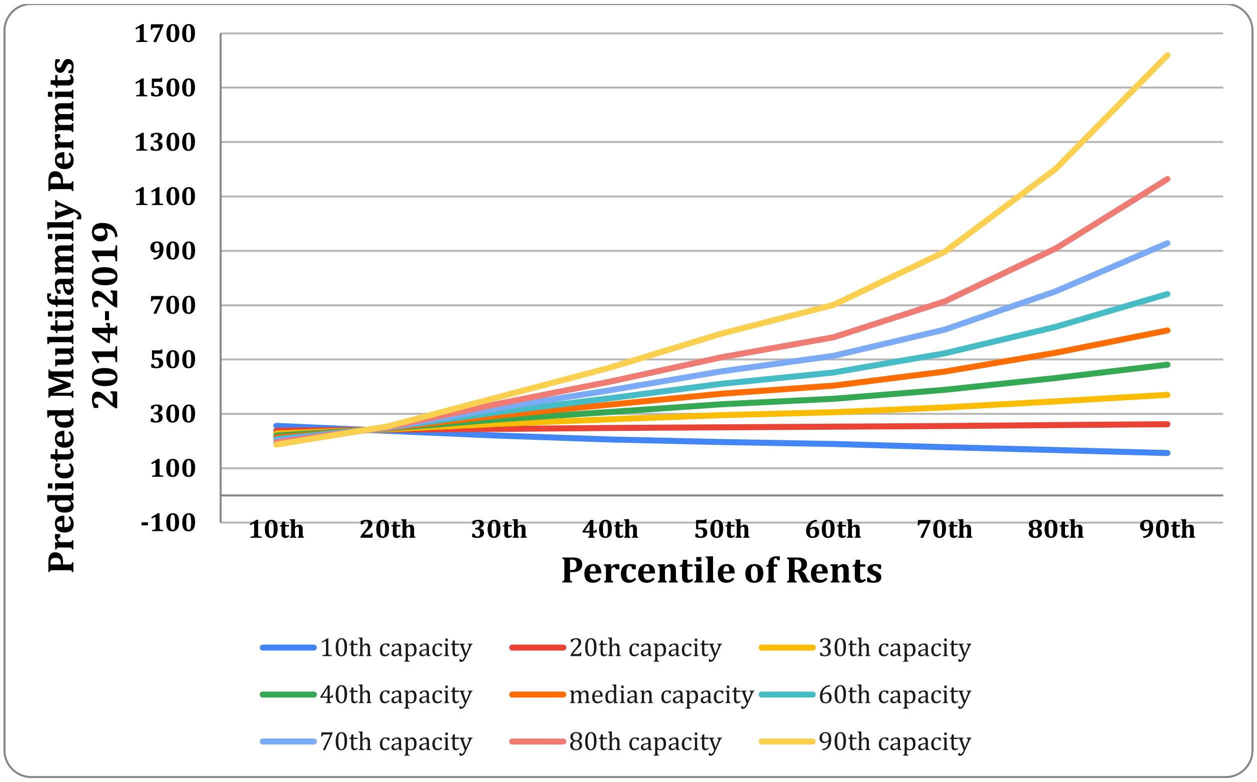


Figure 4. Predicted Multifamily Permits as a Function of Rent and Unbuilt Capacity

Appendix A Table 1: Summary of Empirical Literature Linking Land Use Regulation to Housing Supply

Author(s)	Year	Geography covered	Dependent Variable	Regulation measure	Model type
Thorson	1997	Parcels in IL	Permits (log)	Specific: Agricultural downzoning	Stock-flow, 1979-1984 & 1985-1994
Skidmore & Peddle	1998	Cities in IL	Change in Units (using permits)	Specific: Development Impact Fees	Fixed effects panel
Levine	1999	Cities in CA	Change in Units (using Census data)	An index: Surveys of several areas	OLS, change 1980-1990
Mayer & Somerville	2000	U.S. metros	Permits (log)	Specific: Development or impact fees, delays	Panel (quarterly) 1985 to 1996
Quigley, Raphael & Rosenthal	2004	Cities in CA	Change in Units (using permits)	An index: Surveys of several areas	OLS, 1990-2000
Zabel & Paterson	2006	Cities in CA	Single family permits	Specific: Critical habitat designation	Difference in difference 1990-2002
Glaeser & Ward	2009	Municipalities in Boston	Permits (log)	Specific: Lot sizes, wetlands by-laws, septic regulations, and subdivision rules	OLS, three cross sections
Schuetz	2009	Municipalities in MA	Permits (log)	Specific: Multifamily permitting rules	IV using historical characteristics
Kahn	2011	Cities in CA	Permits (log)	An index: Political ideology	Panel (annual) 2000 to 2008
Dempsey & Platinga	2013	Cities in OR (parcels)	A plot being developed	Specific: Urban growth boundaries	Difference-in-difference
Jackson	2016	Cities in CA	Permits (log)	An index: Surveys of several areas	Panel, 1970–1995
Murray & Schuetz	2019	Cities in CA	Permits per 10,000 people	Specific: Maximum density, height, and % zoned multifamily	Tobit, change 2013-2018

Appendix A Table 2: Summary of Empirical Literature Linking Land Use Regulation to Housing Prices (building on the summary in Quigley and Rosenthal (2005))

Author(s)	Year	Geography covered	Dependent Variable	Regulation measure	Model type
Quigly & Raphael	2005	Cities in CA	House price (1990 and 2000)	Index: Based on surveys	Hedonic model
Green, Malpezzi, & Mayo	2005	45 US metro areas	Supply elasticity	Index: Based on surveys	OLS
Ihlanfeldt	2007	Cities and counties in FL	House and land price (2000-2002)	Index: Based on surveys	Two stage least squares
Glaeser & Ward	2009	Cities & towns in Greater Boston	House price (2000 and 2005)	Index: Based on surveys	OLS
Saiz	2010	MSAs in the USA	Supply elasticity	Index: Based on surveys and a measure of developable land	Two stage least squares
Ball	2010	Southern England	Time to receive residential development approval	Specific: sites features, proposed buildings, local approval authorities, developers	OLS
Kahn, Vaugh, & Zasloff	2010	Parcels in CA	Housing units and house prices (2008)	Specific: Coastal boundary zone	Regression discontinuity
Zabel & Dalton	2011	Towns in MA	House prices (1987-2006)	Specific: Minimum lot size	OLS and difference-in-difference
Huang & Tang	2012	Cities in the US	House prices (2000 and 2009)	Index: Based on surveys	Fixed effects model
Kok, Monkkonen, & Quigley	2014	Cities in the San Francisco Bay Area	Land prices	Index: Based on surveys	OLS

Appendix A Table 2 (Continued): Summary of Empirical Literature Linking Land Use Regulation to Housing Prices (building on summary in Quigley and Rosenthal (2005))

Author(s)	Year	Geography covered	Dependent Variable	Regulation measure	Model type
Munneke, Sirmans, Slade, & Turnbull	2014	Housing near Brigham Young University	Housing prices	Specific: University policy limiting student housing location	Flexible hedonic model
Hilber & Vermeulen	2016	Planning authorities in England	Mixed-adjusted house price index	Specific: Refusal rate of large residential projects	Panel (1974 to 2008)
Jackson	2018	Cities and Counties in CA	Zillow Home Value Index (ZHVI)	Index: Based on surveys	Two-way fixed effects model
Gyourko & Krimmel	2021	CBSAs in the US	Land prices	Index: Based on surveys	Zoning tax estimates

Appendix B. Components of the Regulatory Process Index

The process index is a sum of eight binary variables created from questions in Jackson (2016). We coded variables 1 if they were above or below the median value in the direction of "more restrictive". For example, in question 1 fewer meetings is more restrictive, and the median value is twice per month. So we coded cities with less than one or one meeting per month of their permit-granting entity are coded as 1 for this variable, and the rest are 0.

1. How many times a month (including special meetings) does your permit-granting entity typically meet to consider development applications?

(Less than once per month, Once, Twice, Three times, Four times, More than four times a month)

2. Within how many days to you consider a typical single-family development application?

(0-14, 15-29, 30-44, 45-59, 60 or more)

Questions 3-5: For developments on land needing no rezoning, zoning amendment, bulk variance, etc., what is the typical time to secure preliminary plat/plan approval for the most common applications for the following types of development, starting from the time the application is deemed complete?

3. Single Family Detached Development
4. Townhouse residential development
5. Multifamily residential development

6. Apart from the body that grants preliminary plat/plan approval of the single-family detached development application, how many other boards and/or regulatory bodies immediate to the local jurisdiction must grant permission or preliminary approval before a typical residential development is approved in your jurisdiction?

(None, One, Two or Three, Four or Five, More than Five)

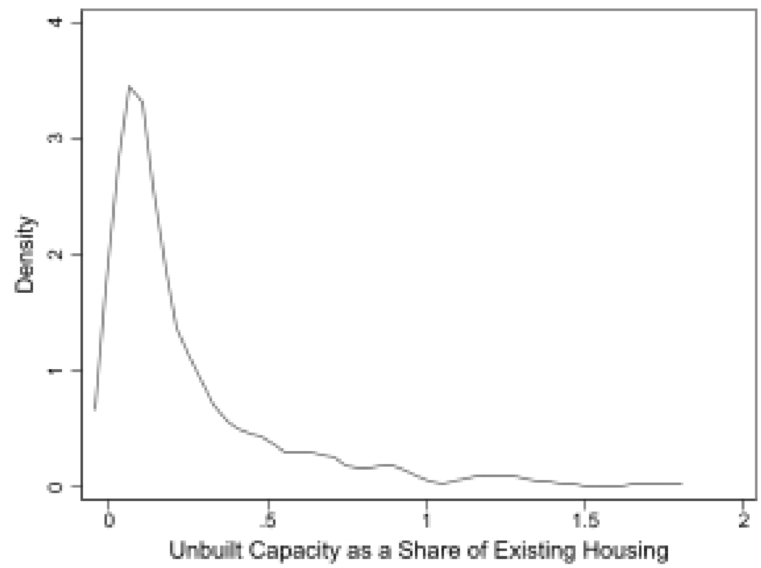
7. Does your jurisdiction offer pre-application conferences, sketch/concept reviews, or similar measures designed to expedite or resolve conflicts about residential development approval? If so, how long do these last?

(No, Yes: One meeting, Yes: Several meetings, Yes: But the number of meetings varies so much it is impossible to say)

8. Who is typically authorized to grant preliminary plat/plan approval (at time of vested rights) for single family detached development application?

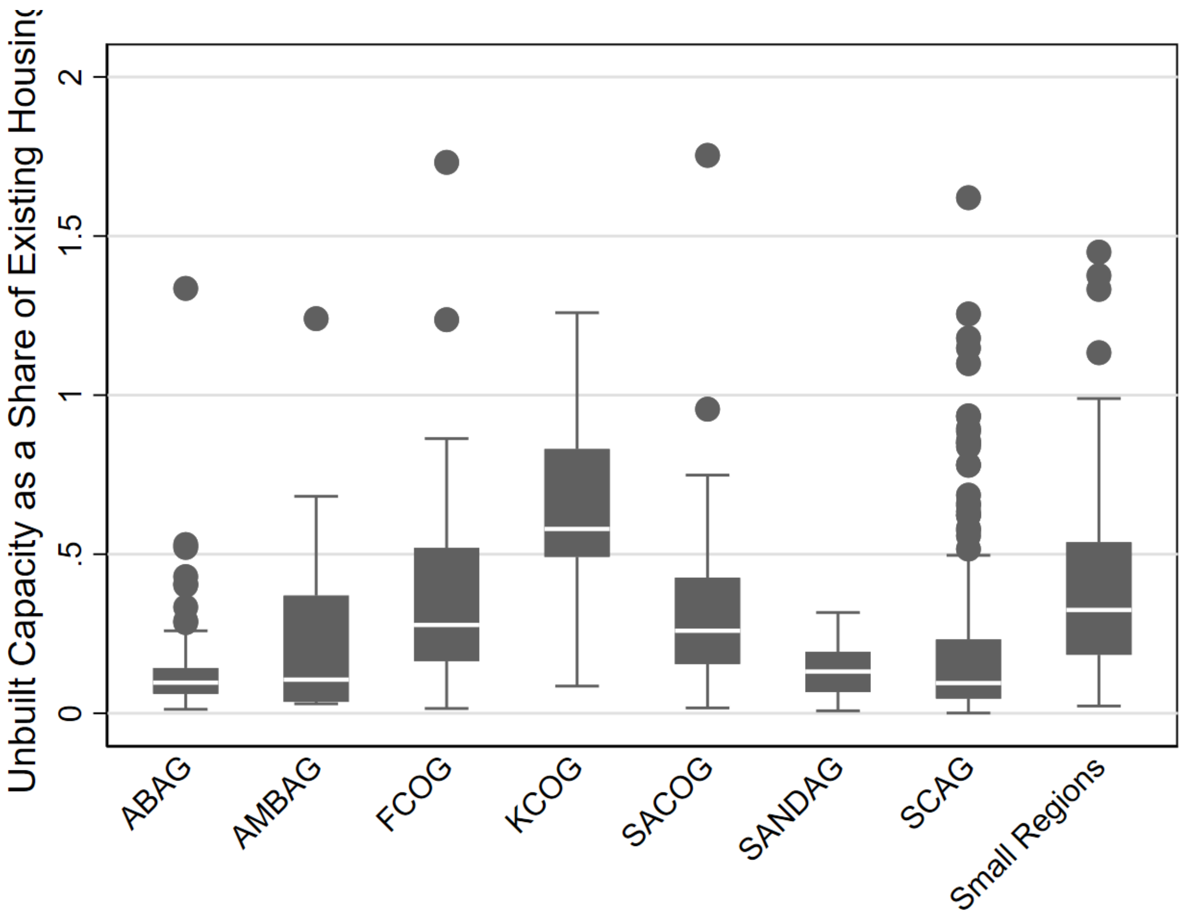
(No local approvals are required for subdivisions in this jurisdiction, Staff, Appointed or elected citizen board (planning board or commission), Elected legislative body)

Appendix C

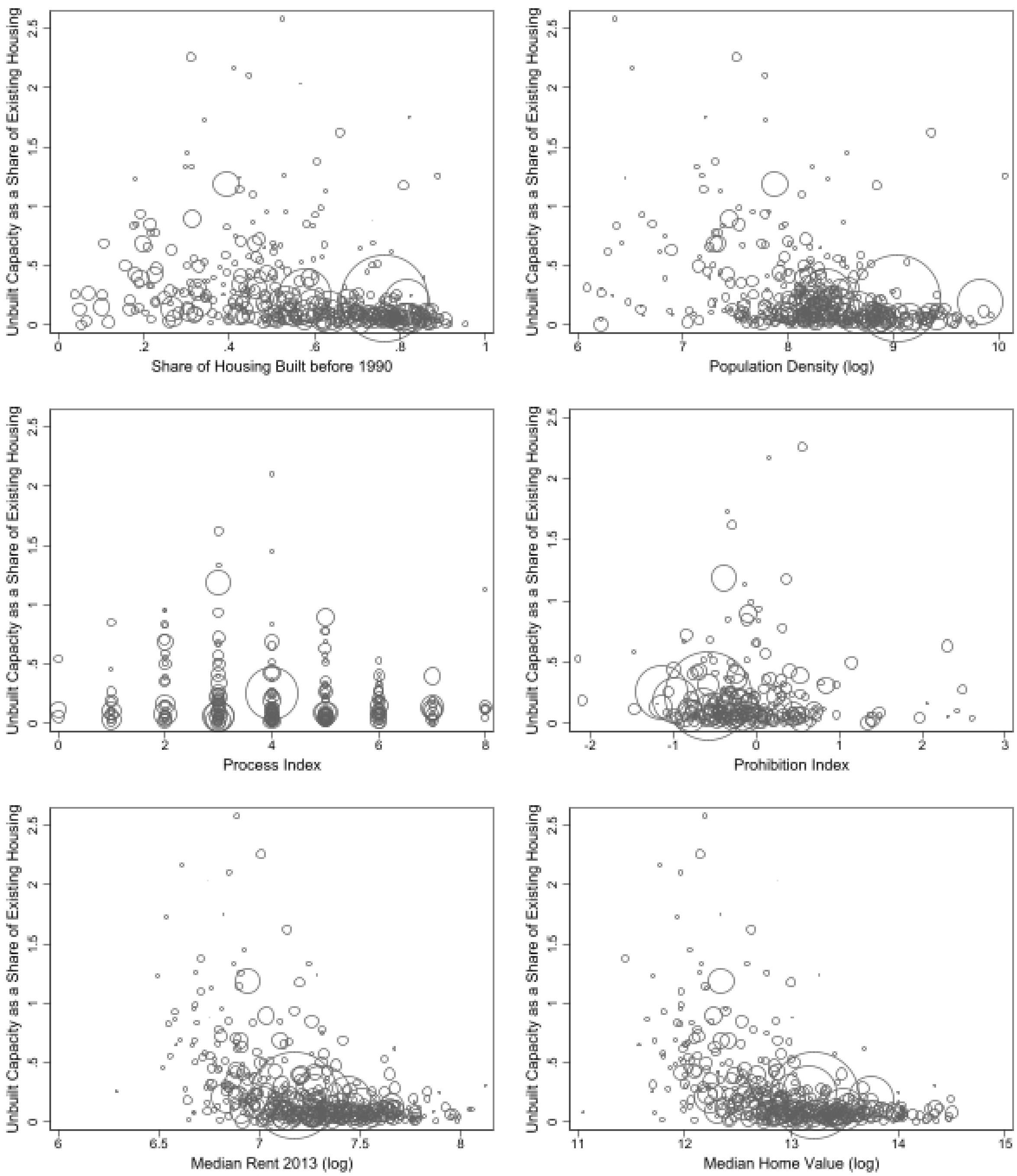


Appendix C Figure 1. Density plot of unbuilt capacity as a share of existing housing units

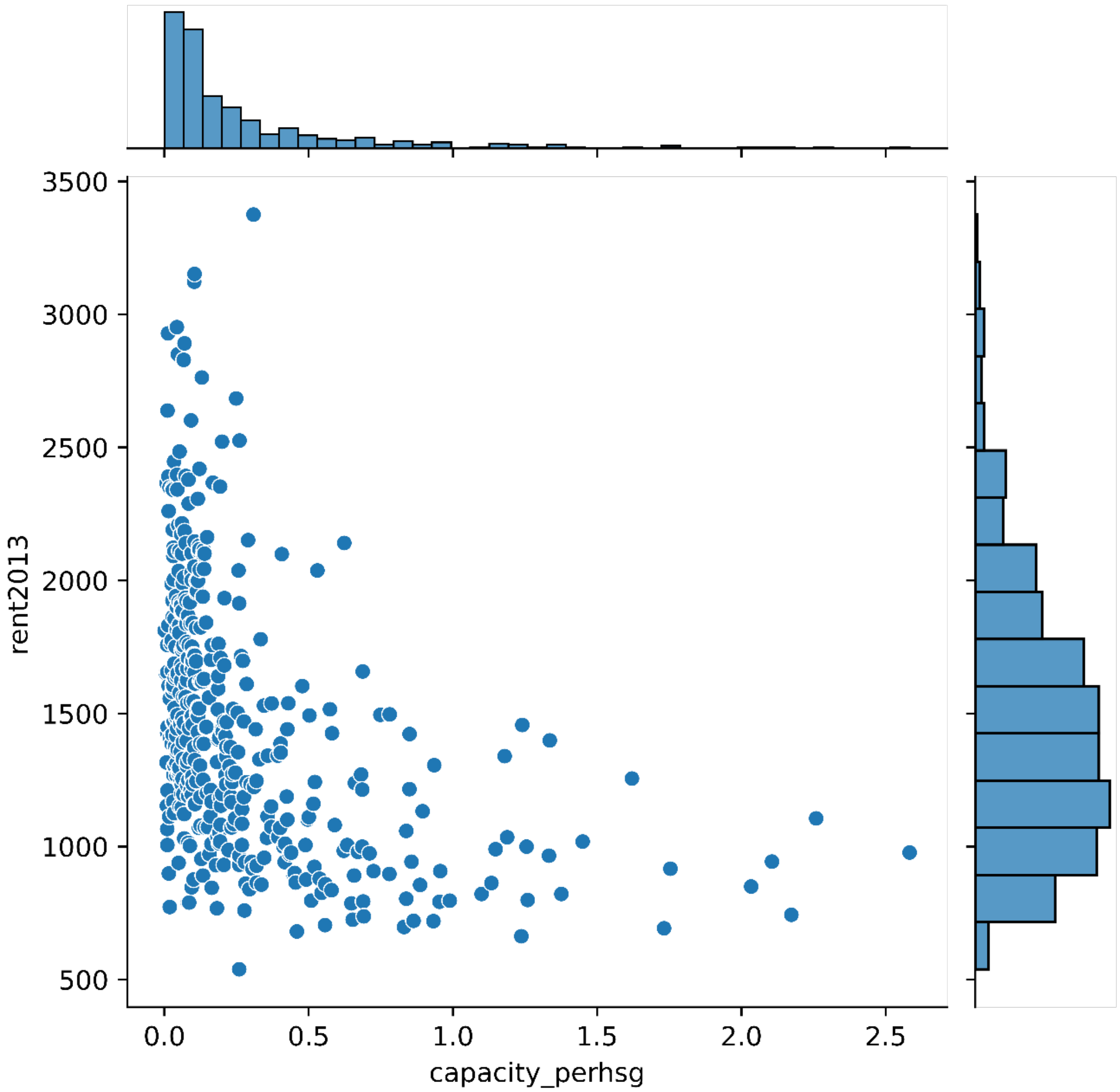
Notes: Excludes nine jurisdictions with a capacity that is over twice the number of existing units



Appendix C Figure 2. The distribution of unbuilt capacity as a share of existing housing across regions



Appendix C Figure 3. Scatterplots of unbuilt capacity with potential correlates: Regulatory prohibitions, regulatory process, population density, rents, value, and the age of the housing stock



Appendix C Figure 4. Joint Frequency Distribution of Rent in 2013 and Unbuilt Capacity as a Share of Existing Housing

Appendix C Table 1. Data availability for Metropolitan cities by California region

Region	Number of cities	Process Index*	Prohibition Index	Total Unbuilt Capacity	MF Unbuilt Capacity
Greater LA	191	109	99	182	138
SF Bay Area	108	54	67	96	78
San Diego	19	8	13	18	18
Sacramento	29	11	10	23	20
Monterey Bay	17	8	9	15	11
Fresno	16	8	5	15	13
Small Metros	81	33	25	65	57
Total cities	461	231	228	414	335

Notes: This table shows availability by city for each metropolitan region of California for data from three different data sources – two surveys (Jackson, 2016; Furth and Gonzalez, 2019 based on Mawhorter and Reid, 2018) and unbuilt capacity estimates from cities' Housing Elements.

Appendix C Table 2. Full results for Table 3. Negative Binomial Models; DV: All Permits 2014-2019

Variables	1	2	3	4	5
Unbuilt Capacity (log)	0.405***			0.340***	0.388***
	(0.065)			(0.095)	(0.071)
Prohibition Index		-0.263***		-0.349***	-0.160**
		(0.087)		(0.135)	(0.081)
Process Index			0.056	-0.014	
			(0.041)	(0.047)	
Median Rent, 2013 (log)	0.801*	0.0854	0.073	0.384	0.490
	(0.436)	(0.525)	(0.663)	(0.763)	(0.464)
Population (log)	0.773***	1.171***	1.059***	0.881***	0.818***
	(0.089)	(0.061)	(0.094)	(0.118)	(0.088)
Population density (log)	-0.184**	-0.499***	-0.478***	-0.465***	-0.334***
	(0.092)	(0.142)	(0.182)	(0.154)	(0.125)
Job Accessibility (log)	-0.009	-0.159	-0.111	-0.237*	-0.072
	(0.101)	(0.136)	(0.175)	(0.132)	(0.123)
Multifamily Housing (%)	0.713	1.355*	1.521**	1.051	1.063
	(0.495)	(0.720)	(0.706)	(0.692)	(0.649)
Median Household Income	0.002	0.008	0.000	0.001	0.004
	(0.006)	(0.004)	(0.003)	(0.006)	(0.004)
Population Black (%)	-2.060	-1.108	-3.070***	-2.429	-2.538*
	(1.594)	(1.545)	(1.081)	(1.806)	(1.382)
Population Asian (%)	0.113	0.547	-0.019	0.985*	0.462
	(0.593)	(0.580)	(0.455)	(0.542)	(0.502)
Population Hispanic (%)	-1.018	-0.024	-0.077	0.494	0.115
	(0.658)	(0.648)	(0.429)	(0.855)	(0.588)
Change in Rent 2009-2013	1.215	0.559	-0.007	0.292	-0.226
	(0.784)	(0.508)	(0.434)	(0.967)	(0.466)

Education: BA or greater (%)	-0.742	-0.367	0.024	0.799	-0.060
	(1.034)	(0.832)	(0.684)	(1.015)	(0.769)
Lnalpha	-0.175**	-0.258**	-0.119	-0.366**	-0.381***
	(0.082)	(0.104)	(0.107)	(0.155)	(0.111)
Constant	-9.235***	-2.276	-0.917	-3.327	-6.060*
	(3.000)	(3.657)	(3.713)	(4.606)	(3.298)
Observations	412	227	231	135	227
Pseudo R2	0.089	0.088	0.060	0.075	0.096

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses.

Appendix C Table 3. Full results for Table 4. Negative Binomial Models; DV: Multifamily Permits 2014-2019

Variables	1	2	3	4	5
Unbuilt Capacity (log)	0.267***			0.272	0.295***
	(0.102)			(0.187)	(0.105)
Prohibition Index		-0.448**		-0.574*	-0.431***
		(0.198)		(0.329)	(0.131)
Process Index			0.128	0.055	
			(0.079)	(0.102)	
Median Rent, 2013 (log)	2.094**	0.203	-0.788	3.424**	1.309
	(1.031)	(0.989)	(1.387)	(1.629)	(0.876)
Population (log)	1.063***	1.518***	1.619***	1.135***	1.131***
	(0.153)	(0.129)	(0.167)	(0.279)	(0.141)
Population density (log)	0.029	-0.370	0.329	-0.466	-0.244
	(0.241)	(0.255)	(0.342)	(0.415)	(0.206)
Job Accessibility (log)	-0.077	-0.075	-0.451*	-0.080	0.088
	(0.177)	(0.199)	(0.256)	(0.296)	(0.170)
Multifamily Housing (%)	2.799**	2.782**	3.238**	2.638	1.924
	(1.127)	(1.315)	(1.386)	(1.984)	(1.203)
Median Household Income	0.010	0.000	-0.014	-0.030	0.004
	(0.014)	(0.009)	(0.009)	(0.020)	(0.004)
Population Black (%)	-1.925	-3.748	-5.015*	-5.699	-2.538
	(3.055)	(2.361)	(3.034)	(4.428)	(1.382)
Population Asian (%)	1.726	0.839	1.340	1.281	0.462
	(1.188)	(0.948)	(1.021)	(1.495)	(0.502)
Population Hispanic (%)	0.835	1.304	1.501	1.956	0.115
	(1.340)	(1.182)	(1.064)	(1.654)	(0.588)
Change in Rent 2009-2013	1.697	0.801	0.847	-2.269	-0.226
	(1.716)	(0.847)	(0.877)	(2.511)	(0.466)
Education: BA or greater (%)	1.135	1.544	1.168	1.846	-0.060
	(2.050)	(1.750)	(1.767)	(2.499)	(0.769)
Lnalpha	1.088***	0.742***	1.035***	0.615***	0.689***
	(0.078)	(0.092)	(0.093)	(0.132)	(0.136)
Constant	-23.76***	-10.730	-8.587	-28.50***	-17.84***
	(7.115)	(6.852)	(8.699)	(10.290)	(6.106)
Observations	333	227	231	111	184

Pseudo R2	0.062	0.082	0.056	0.067	0.073

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses.

Appendix C Table 4. Robustness check using Housing Prices instead of Rents: Negative Binomial Models; DV: All Permits and Multifamily Permits with Interaction Terms

	All Permits 2014-2019			Multifamily Permits 2014-2019		
VARIABLES	**1**	**2**	**3**	**1**	**2**	**3**
Zoned Capacity (log)	-2.98***			-6.26***		
	(0.73)			(1.60)		
Prohibition Index		-2.45			2.99	
		(1.53)			(3.95)	
Process Index			0.79			-0.31
			(0.88)			(1.86)
Value * Zoned Capacity (log)	0.26***			0.50***		
	(0.06)			(0.12)		
Value * Prohibition Index		0.16			-0.27	
		(0.11)			(0.29)	
Value * Process Index			-0.06			0.03
			(0.07)			(0.14)
Median Value, 2013 (log)	-1.27***	0.21	0.43	-2.99***	0.21	0.18
	(0.41)	(0.19)	(0.31)	(0.84)	(0.32)	(0.64)
Population (log)	0.71***	1.16***	1.02***	1.04***	1.39***	1.54***
	(0.09)	(0.07)	(0.10)	(0.14)	(0.13)	(0.16)
Population density (log)	-0.20**	-0.52***	-0.55***	0.16	-0.22	0.33
	(0.09)	(0.13)	(0.17)	(0.23)	(0.25)	(0.32)
Job Accessibility (log)	-0.04	0.00	0.06	0.14	0.22	-0.03
	(0.08)	(0.12)	(0.16)	(0.15)	(0.18)	(0.22)
Lnalpha	-0.20**	-0.22**	-0.07	1.11***	0.79***	1.08***
	(0.08)	(0.10)	(0.11)	(0.08)	(0.09)	(0.09)
Constant	13.86**	-4.26*	-6.23	28.76**	-12.52***	-16.38*
	(5.57)	(2.46)	(4.20)	(11.44)	(4.06)	(8.38)
Observations	412	227	231	333	227	231

Pseudo R2	0.09	0.09	0.06	0.06	0.08	0.05

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses.

Appendix C Table 5. Robustness check, sample restricted to municipalities that permitted multifamily housing, Negative Binomial Models; DV: Multifamily Permits with Interaction Terms

	Multifamily Permits 2014-2019		
Variables	**1**	**2**	**3**
Unbuilt Capacity (log)	-3.568***		
	(1.120)		
Prohibition Index		1.163	
		(2.346)	
Process Index			-0.582
			(1.327)
Rent * Unbuilt Capacity (log)	0.530***		
	(0.153)		
Rent * Prohibition Index		-0.238	
		(0.307)	
Rent * Process Index			0.096
			(0.181)
Median Rent, 2013 (log)	-2.270**	0.570	0.733
	(1.066)	(0.408)	(0.740)
Population (log)	0.802***	1.257***	1.163***
	(0.092)	(0.084)	(0.098)
Population density (log)	0.204	-0.151	-0.012
	(0.153)	(0.186)	(0.220)
Job Accessibility (log)	0.052	0.135	0.015
	(0.099)	(0.123)	(0.144)
Lnalpha	0.062	-0.102	0.028
	(0.081)	(0.094)	(0.096)
Constant	9.327	-12.000***	-12.641**
	(7.854)	(3.104)	(5.556)
Observations	244	186	174
Pseudo R2	0.085	0.113	0.075

Notes: ***, ** and * indicate significance at the 0.01, 0.05, and 0.10 levels. Robust standard errors in parentheses.