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Essays on Land Use Regulation and Charitable Giving

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Kristoffer R. Jackson

Dissertation Committee:
Professor Jan Brueckner, Chair
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2015

DEDICATION

This work is dedicated to my sweet wife, Alisha.

Though it will never compare with the work she does everyday to shape our three beautiful masterpieces, I hope that it makes her proud.

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ABSTRACT OF THE DISSERTATION

Essays on Land Use Regulation and Charitable Giving

By

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Doctor of Philosophy in Economics

University of California, Irvine, 2015

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The three chapters in this dissertation can be separated into two sections, each seeking to answer a different question. The first question (in Chapter 1) is whether information about the behavior of others affects charitable giving from lapsed donors. The second question (in Chapters 2 and 3) is whether (and to what extent) land use regulations affect housing prices and housing construction.

Using data from an experiment carried out by a large nonprofit organization, Chapter 1 finds that lapsed donors who received a solicitation letter referencing a relatively high donation made by another donor (high social information) were more generous in giving, but overall less likely to make a donation, relative to the baseline (low social information) group. Thus, high social information can have potentially offsetting effects when applied to lapsed donors. Nonprofits should consider this trade-off when employing social information fundraising techniques to solicit donations from lapsed donors.

Chapter 2 estimates the extent to which the supply of new housing is restricted by land use regulations, using a panel of California cities from 1970-1995. While land use regulation is found to significantly reduce residential development, controlling for unobserved heterogeneity using city and year (two-way) fixed effects reduces the magnitudes of the estimates by 50-75%. Attenuation bias from measurement error can only account for a small proportion of this reduction, suggesting that studies based on cross-sectional policy variation, which predominate this literature, may overestimate the effects of land use regulation.

Using data from a survey of top land use officials in communities across the state of California, Chapter 3 provides a measure of both local regulatory stringency and the degree to which geographic constraints inhibit local development. After exploring differences in regulatory patterns across the state, the index is applied to a model of housing prices. Land use regulation in California is related to the level of that state's housing prices, but not the elasticity of housing supply. Instead, where housing demand increased through the expansion of subprime lending, geographic constraints exacerbated the run-up and subsequent crash of local housing prices.

I acknowledge financial support from the Charles Koch Foundation.

Chapter 1

The Effect of Social Information on Giving from Lapsed Donors: Evidence from a Field Experiment

1.1 Introduction

In 2012, nonprofit organizations received more than $316 billion, with about 72% of that coming from individuals (Giving USA (2013)). Although charitable giving in the United States has hovered around 2 percent of U.S. GDP each year for the last five decades, the ease with which donors can shift their loyalty means individual organizations must work hard to retain their active donors, recruit new donors, and re-engage those who have lapsed.

Reactivating lapsed donors is a first-order concern for organizations that depend heavily on private donations. Since these individuals are aware of the organization and sympathetic to the cause (or, at least, were at some point), efforts spent on reactivating lapsed donors may be more fruitful than those spent on acquiring new donors (see Blackbaud (2013), as well as Barber and Levis (2013)). Despite the growing literature exploring the factors that drive charitable giving, very little empirical work attempts to link these factors with giving from lapsed donors, in particular.

Researchers have identified the provision of social information—that is, information about the donations of others—as one important factor that affects the rate at which people give (Andreoni

(2006a), Vesterlund (2006)). This paper contributes to the literature on donors' behavioral response to social information by exploring this effect for lapsed donors (i.e., those who have not given in more than three years).

This study uses data from an experiment carried out during a regularly occurring fundraising campaign for a large health-related nonprofit organization in the United States (referred to hereafter as "the NPO"). The experimental treatment was a reference to another donor's relatively high donation (i.e., an amount that is well above average for the donors used in the experiment, but still within the usual range of their donations). This experimental setting provides an ideal opportunity to test for the effects of high social information on lapsed donors' giving. The results of the experiment yield evidence that social information of the form employed here may reduce the probability lapsed donors contribute, but those who do choose to give, do so more generously.

By referencing a relatively high contribution made by another donor, the NPO in this study increased the average donation by $12 (or about 37%), relative to the average contribution among those who received information about a lower donation amount. However, there are clear and important differences in the composition of the two experimental groups. These imbalances are remedied first by controlling for the differences using regression, then by comparing donations after matching individuals in each experimental group with similar donors in the other group. Both methods yield results that are largely similar to the raw difference in means; however, after adjusting the standard errors to account for heteroskedasticity, the regression estimate of the treatment effect is not statistically significant at conventional levels. Using the preferred—matching—method, high social information is found to increase the average donation amount by $14.95, while reducing the probability a lapsed donor will give anything by 4.1%. Thus, while high social information may increase generosity among those donors who choose to contribute, some of these benefits are potentially offset by individuals being deterred from donating anything. Nonprofit organizations should consider this tradeoff when determining the most effective fundraising technique to use with lapsed donors.

1.2 Related Literature

Much of the past research on charitable contributions has primarily focused on the myriad reasons why individuals might give to charity, but a relatively small number of researchers have focused their attention on lapsed donors. This is an important distinction to make since individuals who have not given recently may be systematically different from active donors. Thus, although a given fundraising strategy may be effective in raising money from active donors, it is not clear that it would work the same way for those who have not given in some time.

Eckel and Grossman (2008) evaluate the effect of matching and rebate subsidies for continuing, lapsed, and prospective donors. They find no significant effect of rebates on lapsed donors, but matching subsidies are found to crowd out a portion of these individuals' giving. Aldrich (2000) evaluates the effect of a set of telemarketing and direct mail campaigns directed at lapsed donors of Sight Savers International and finds that the telemarketing campaigns are the more effective method of donor reactivation. Prokopec and De Bruyn (2009) find that suggesting a relatively high donation amount to infrequent and lapsed donors increases the donations of those who give, but reduces the likelihood of giving anything. Verhaert and Van den Poel (2011) (discussed in more detail below) carry out a similar field experiment, but also incorporate social comparisons (i.e., social information).[1]

Since nonprofit organizations are a major provider of public goods and social services, and because the private provision of these goods and services depends on a degree of social cooperation, previous studies indicate that various types of social information, pressures, and norms direct (or, at least, influence) the behavior of potential donors, in general. First, it has been shown that information and signals about the effectiveness of charities matter to donors. Such signals and information may come from third-party ratings of nonprofits (e.g., Grant (2010), Bhattacharya and Tinkelman (2009), Cnaan et al. (2011)) or through the organization's accounting information (e.g., Parsons (2003), Jacobs and Marudas (2009), Yetman and Yetman (2013)). Vesterlund (2003) and Andreoni (2006b) suggest that charities may increase donations by announcing large past contributions, or 'leadership' donations, as a signal that they provide a beneficial (or, high quality) public good. Smith, Windmeijer, and Wright (2013) find strong peer effects in giving, but little evidence of the quality-signaling role of donations.

Secondly, it is well established in the literature that individuals are much more generous when they can observe the actions of others and know that their actions can also be observed (Andreoni and Petrie (2004), Harbaugh (1998), Silverman et al. (1984), Soetevent (2005)). Romano and Yildirim (2001) suggest that by announcing donations, charities induce a sequential-move game, where donors in later rounds of fundraising can take into consideration what those in previous rounds contributed. They indicate that sequential play can be beneficial

[1] Another branch of this literature evaluates survey and focus group responses in an attempt to discover what affects individuals' decision to continue or discontinue donating to an organization (e.g., Beldad et al. (2014), Beldad et al. (2012), Bennett (2009), Germain et al. (2007), Mathew et al. (2007), Sargeant and Jay (2004), Sargeant (2001a), and Sargeant (2001b)).

for charities if donations are driven by either warm glow preferences or an effort to conform to the actions of others – making donations complements. Reinstein and Riener (2012) use experimental data to show that the donations of the leaders in a sequential-move game have a strong impact on the donations of the followers, especially when the leader's identity and donation amount is reported.

Despite the abundance of research investigating the role of social information in charitable giving, there is no general consensus in the existing literature as to whether an informational treatment would result in more (or higher) donations. Instead, the literature suggests that knowing how much another donor has contributed may have one of two opposing effects. If the provision of the public good is only conditional on total contributions passing a given threshold, then knowing that another donor has given a large amount may decrease an individual's donation (Andreoni (1998), Cornelli (1996), Romano (1991)). Romano and Yildirim (2001) note that this is most likely to happen if the value of a contribution is transmitted to donors only through its effect on the total amount of public good that is provided. In other words, high social information (that is, information about a relatively high amount another donor has given) could have a negative impact if the other (larger) donation has pushed total contributions sufficiently close to the required threshold that the individual feels less pressure to contribute. In this way, charitable contributions can be substitutes (Warr (1982), Roberts (1984)).

Alternatively, some have argued a peer-pressure or conforming motive to charitable giving, in which knowledge about the contributions of others may induce similar donations from an individual (Frey and Meier (2004)). Croson and Shang (2008) find this effect to be especially strong when the reported donations of others are likely to be less than what the individual had planned to give, as the knowledge of lower donations of others could be used to justify a smaller contribution. However, they also find a similar effect in the other direction, where reporting larger donations from others induced higher contributions. Although the literature provides important insight regarding the effects of social information, the vast majority of this work fails to address the potential for differential effects based on donor status (i.e., prospective, lapsed, or active).

The experiment in this study is similar to one carried out by Verhaert and Van den Poel (2011) using a charitable organization in Europe. In their experiment, the organization suggested donation amounts in direct mail solicitations with and without references to the behavior of others (i.e., with and without social information). They find that while suggested donations significantly affect the size of contributions lapsed donors make, the presence or absence of social information is irrelevant for these individuals. Additionally, these authors find the provision of social information to significantly reduce the probability a lapsed donor will give at all.

This paper provides additional evidence of the effect of social information on lapsed donors, with two distinct differences in the context of the experiment performed by Verhaert and Van den Poel (2011). First, this experiment is carried out in the United States. While there are certainly similarities between charitable giving in the United States and many European countries (see, for example, Charities Aid Foundation (2011)), cultural and institutional differences make it unclear whether the findings of Verhaert and Van den Poel (2011) can be generalized to populations outside of Europe. The second difference is the nature of the organizations with which the experiments were carried out. While the previous authors used a charitable organization whose purpose is “helping the needy,” the experiment evaluated in this paper was carried out by a health-related nonprofit, which focuses on funding research. In the latter case, donors to the cause—or, perhaps, those close to the donor—are likely stricken with the targeted infirmity. Thus, individuals donating to such an organization may be direct beneficiaries of their own donations. On the other hand, donors who give to purely charitable organizations do not receive a direct private benefit, in addition to the so-called warm glow of altruism. Similarly, these different types of nonprofits may be targeting entirely different populations. These propositions suggest that the sensitivity of contributions to social information may be very different for purely charitable organizations as compared to those that provide some direct benefit to their benefactors.

1.3 Experimental Design

This study evaluates the effects of two levels of social information on the giving behavior of lapsed donors using data from a quasi-natural experiment carried out by a large nationwide health-related nonprofit organization (“the NPO”) in the United States. With over 50 chapters nationwide, the NPO has raised more than $500 million ($36 million annually) to fund research initiatives. They currently fund over 134 research studies, 71 prominent research institutions and hospitals worldwide, including 15 full research centers. The NPO raises money using a variety of different events and direct (electronic) mail solicitations throughout the year, but the setting of this experiment was limited to one of their direct e-mail fundraising campaigns.

Since the NPO frequently sends solicitation letters to past donors, this provides an optimal scenario for them to control for many factors that may confound the effects of the experimental treatment—receiving the information about a relatively high contribution made by another donor with the NPO's usual solicitation letter. Specifically, if donors are conditioned to the usual timing of solicitations, they may respond differently to a mailer that is sent at an irregular time relative to the usual timetable. This possible source of bias is absent in this study, since the NPO regularly solicits its donors and this study was carried out as part of a previously scheduled, annually-occurring direct e-mail fundraising campaign. No effort was made to contact potential donors after sending the solicitation letter. Although research suggests that fundraising is more effective when charity recipients verbally communicate with their benefactors (Andreoni and Rao (2011)), the NPO does not usually attempt to contact individuals, except through solicitation letters. Moreover, direct e-mail solicitations limit the potential for bias coming from human interaction—whether intentional or unintentional.

The NPO used, as subjects, all lapsed donors (i.e., those who had not given in more than three years) who had previously given a positive amount less than $100 and for whom they had a current e-mail address – 15,166 in total. All letters were sent via electronic mail. The solicitation letters went out in seven separate waves over a four-week period of time. All subjects were solicited initially, and each donor who gave in some wave was excluded from all subsequent waves. The experiment coincided with a matching campaign wherein donors were informed that contributions made during the campaign would be matched at $2 for every $1 donated. Using lapsed and active donors, Karlan and List (2007) find matching offers to increase both the response rate to solicitations and revenue per solicitation. Additionally, as mentioned above, Eckel and Grossman (2008) find matching subsidies to crowd out a portion of the donations made by lapsed donors. While there is evidence that matching offers affect lapsed donors' giving, there is no reason to think that it would influence the two experimental groups differentially, so this should not affect the results of this paper.

Rather than use a baseline of no social information, the NPO implemented two treatments—referencing a relatively high donation for one group (called the "high treatment" or "high social information") and a lower donation for the other group (called the "low treatment" or "low social information"). The information given to those assigned to the high treatment group was a reference to the 80th percentile of the distribution of most recent donation amounts for the 15,166 donors involved in the study, while the low treatment group received a reference to the 40th percentile donation amount. Thus, the amounts referenced in the high and low treatments represent donations that are, respectively, above and below the median gift amount, but still within the usual range of contributions for these donors. Although the most recent donation amounts range from $0.01 to $99.82, the 80th percentile donation amount reported to the donors assigned to the high treatment group is $50. Assuming that, in the absence of an experimental intervention, the distribution of current donations would be similar to that of past donations, this means that, for the subjects used in this experiment, $50 is more than 80 percent of these individuals would have given. Thus, the treatment effect measures the differential impact of knowledge about a high donation made by some other donor, relative to the donor-response to information about a low donation.

The informational treatment in the solicitation letters sent to high treatment group members is contained in the following statement: "I've already received a contribution of $50 from a gracious donor like you, and I'm counting on you to join this person in helping us fight [omitted for anonymity]." Those in the low treatment group received a letter containing the same statement, except that the donation amount referenced was $25 (the 40th percentile donation amount) instead of $50.

Although the treatment assignment was random, it resulted in experimental groups that were not balanced in terms of the subjects' observable characteristics (i.e., their past giving to this organization). This is made evident in Table 1.1, which displays summary statistics for the three pre-treatment variables the NPO tracks for its donors. The first two columns contain statistics for the high and low treatment groups, respectively, and the third column tests for significance in the difference in means for the two groups. Ideally, this analysis would include comparisons of many other demographic variables (such as age, education, and socioeconomic status), but this information is absent from the data. However, insofar as these variables are correlated with past giving behavior, they may be partially (though indirectly) accounted for using the observable characteristics in the data.

As indicated in Table 1.1, the only pre-treatment variable that is balanced for the high and low treatment groups is gender, as indicated by the negligible (and non-significant) difference in the share of men in each group. This is important, since research suggests that there are differences in the charitable behavior of men and women (Cadsby and Maynes (1998), Andreoni and Vesterlund (2001), Piper and Schnepf (2008)). However, for the other two pre-treatment variables (most recent donation and highest donation), the difference in means for the two treatment groups is positive and significant at the 1% level. This means that prior to the experimental intervention, individuals in the high treatment group were, on average, more generous than those in the low treatment group in terms of their most recent and highest donation amounts. Thus, there are systematic differences in the observable characteristics of the two groups. This issue is adjusted for first by using ordinary least squares regression, then using propensity score matching.

Table 1.1
Summary statistics for pre-treatment donation history variables by treatment assignment

		High Treatment	Low Treatment	Difference (High Treatment – Low)
Share of Men		0.335	0.339	-0.004
				(0.008)
Most recent donation				
	Mean	$29.87	$26.80	$3.07***
				(0.259)
	Minimum	$0.01	$0.25	-$0.24
	Maximum	$96.26	$99.82	-$3.56
Highest donation				
	Mean	$30.46	$29.72	$0.74***
				(0.27)
	Minimum	$0.01	$0.25	-$0.24
	Maximum	$96.26	$99.82	-$3.56
N		7,712	7,454	

Notes: Standard errors are in parentheses. Significance in this table indicates that the assignment of treatment status (into either the high or the low treatment group) resulted in groups that were unbalanced with respect to the amount each donor has most recently given and in the highest amount each donor has ever given. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4 Results from Raw Data

Of the 15,166 solicitations sent (and resent) over the seven waves in this study, only 55 individuals (called "experimental donors") contributed any money (called "experimental donations"). This equates to a 0.36% donation rate, which is much lower than the 2-5% they usually obtain from direct e-mail fundraising campaigns to all donors (i.e., active and lapsed). However, this low donation rate is not terribly surprising given the fact that, as manifested by their relatively small and infrequent past donations, the individuals solicited as part of this experiment can be considered some of the least committed of all the donors to whom this organization petitions for contributions. The difference in the overall donation rates within the two treatment groups (0.08% from a baseline of 0.32%) is substantial, but too imprecisely estimated to be statistically significant (see Table 1.2).

Table 1.2
Summary statistics for donations made as part of the experiment

	High Treatment	Low Treatment	Difference (High Treatment – Low)
Mean – conditional on subjects making a donation	\$45.52	\$33.28	\$12.24*
			(6.79)
Mean – not conditional on making a donation	\$0.18	\$0.10	\$0.08
			(0.046)
Donation rate	0.40%	0.32%	0.08%
			(0.1%)
Minimum donation	\$10	\$10	\$0
Maximum donation	\$100	\$100	\$0
Sum of donations	\$1,411.25	\$798.75	\$612.50
Share of positive donors that were men	0.355	0.458	-0.103
			(0.135)
Number of donors	31	24	

Notes: Standard errors are in parentheses. Donation rate indicates the proportion of all subjects in the respective groups that gave a positive donation as part of the experiment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1.1 shows histograms of the experimental donations for each treatment assignment. Summary statistics for the experimental donations (i.e., those made as part of this experiment) are displayed in Table 1.2. The first row shows that, conditional on subjects making an experimental donation, the raw treatment effect is \$12.24. This represents a 37% increase over the average for the low treatment group and is significant at the 10% level. If the randomization were successful and if there were no other differences between the two treatment letters, the interpretation of this raw treatment effect would be that, conditional on making a positive donation, the higher informational treatment induced a \$12.24 increase in the average donation amount given to the NPO.

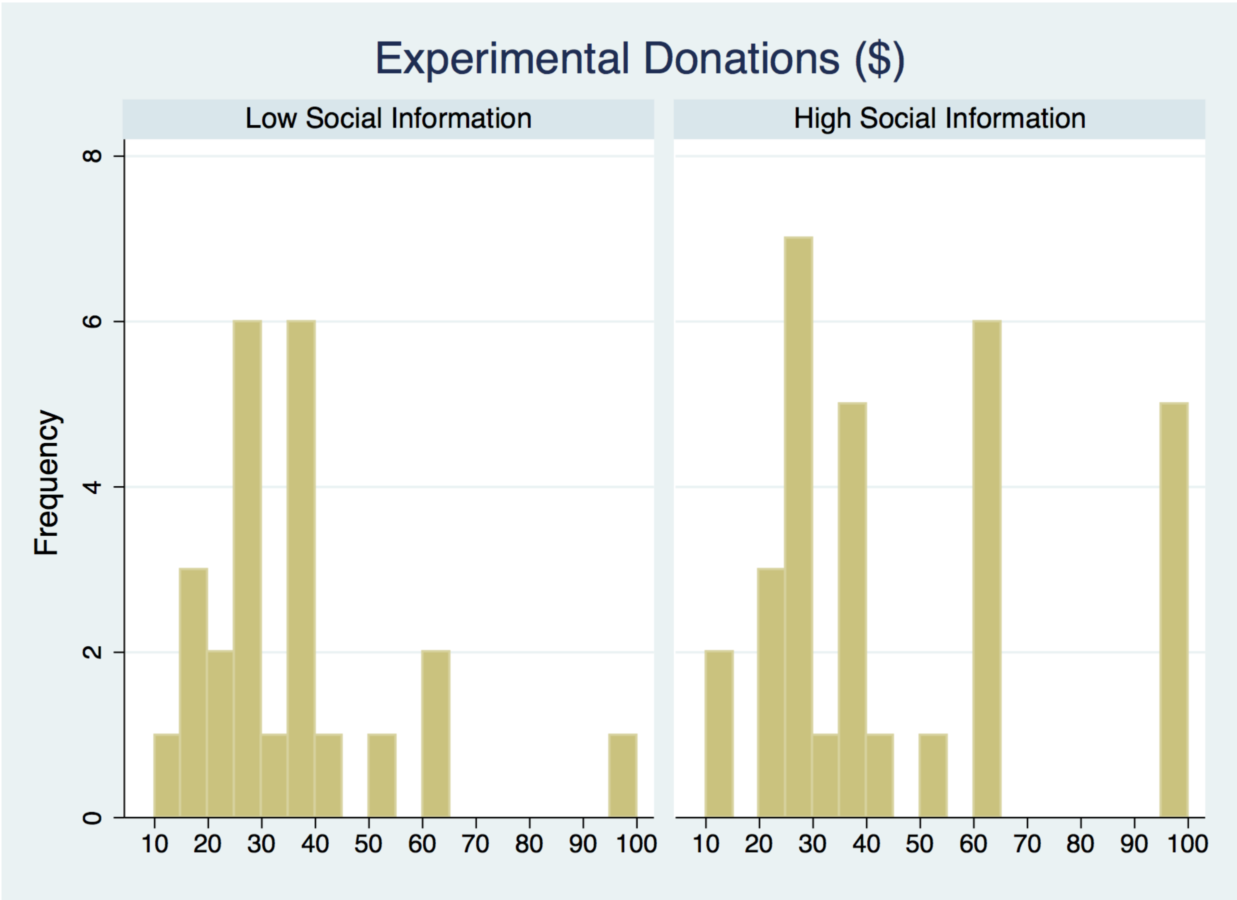

Figure 1.1: Donations made as part of the experiment for the low and high social information treatment groups

As shown in Figure 1.1, only one of the 31 subjects in the high treatment group who made a donation as part of this experiment gave exactly $50 (the donation amount referenced in the high treatment letter). Six of the 24 subjects in the low treatment group gave exactly $25 (the donation amount referenced in the low treatment letter), but seven of those in the high treatment group also gave that amount.

The share of men within the experimental donors is 10% lower for the high treatment group than the low treatment group, but this difference is not statistically significant.

Table 1.3
Experimental donation by wave in which subject donated

	High Treatment	Low Treatment	Difference (High Treatment – Low)
Wave 5			
Mean donation	\$57.40	\$31.25	\$26.15 (20.42)
Share of men	0.3077	0.6667	-0.3590 (0.317)
Donation rate	0.17%	0.04%	0.13%** (0.05%)
Number of donors	13	3	
Wave 6			
Mean donation	\$46.67	\$43.57	\$3.10 (15.35)
Share of men	0.5	0.5714	-0.0714 (0.301)
Donation rate	0.08%	0.09%	-0.01% (0.05%)
Number of donors	6	7	
Wave 7			
Mean donation	\$32.08	\$28.57	\$3.51 (5.87)
Share of men	0.3333	0.3571	-0.0238 (0.195)
Donation rate	0.16%	0.19%	-0.03% (0.07%)
Number of donors	12	14	

Notes: Standard errors are in parentheses. Donation rate indicates the proportion of all subjects solicited in the respective waves that gave a positive donation as part of the experiment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All of the subjects who gave as part of this experiment did so in the last three waves of the seven solicitation mailings. Table 1.3 displays summary statistics and differences for the experimental donors, separated by the wave in which they made their donation. Within each wave, the difference is positive, but generally imprecisely estimated. The only significant difference in Table 1.3 is the 13% difference in donation rates in the fifth wave of the solicitation mailings (which is the first wave in which any experimental donations were made). A regression of the wave in which donations were made on the treatment indicator shows that those who received the higher social information donated, on average, about half (0.49) a wave sooner than those in the baseline group. This estimate is statistically significant at the 5% level, but given the short amount of time between waves, it represents a very small reduction in the amount of time lapsed donors wait before contributing.

1.5 Adjusting for Differences in Pre-Treatment Observables

1.5.1 OLS Regression

Though unintentional, this experiment's treatment allocation process created the problem of selection on pre-treatment observables. One way to purge the estimated treatment effect of potential sources of bias—when these sources are observed—is by controlling for them using ordinary least squares (OLS) regression. If correctly specified, the regression coefficient on the treatment variable will give an unbiased estimate of the treatment effect under two conditions. First, donors' most recent and highest donation amounts must be the only relevant omitted variables (i.e., the only omitted variables that are correlated with treatment assignment and affect the outcome variable). Second, the joint distribution of donors' most recent and highest donation amounts must be similar for both experimental groups (i.e., the sample must contain sufficient overlap across the two groups with regard to these variables). As argued above, if past giving behavior is related to demographic characteristics not measured in the data, this method should indirectly account for potential imbalances with respect to these variables also.

Table 1.4
OLS regression results

Dependent variable: Experimental donation amount	
High treatment indicator	11.21
	(7.45)
Highest donation	0.39
	(0.32)
Most recent donation	0.20
	(0.40)
Intercept	9.61
	(6.42)
N	55
R-sq.	0.201

Notes: Heteroskedasticity-robust standard errors are in parentheses. High treatment indicator = 1 (0) for subjects who received a reference to the 80th (40th) percentile donation amount from the distribution of past donations.

Table 1.4 contains estimates from the regression of the experimental donation amounts on an indicator for being in the high treatment group (the variable of interest), as well as the two pre-treatment variables for which there is an imbalance in the two treatment groups – the highest donation ever given and the amount of the most recent donation. Controlling for differences in pre-treatment variables reduces the treatment effect to $11.21. The data show some evidence of heteroskedasticity, so Table 1.4 reports heteroskedasticity-robust standard errors. While the use of traditional standard errors suggests that the estimated treatment effect is statistically significant at the 10% level, the effect is not significant using robust standard errors.

If the distributions of pre-treatment variables are similar for the two experimental groups within the sample and the model is correctly specified, OLS regression adequately corrects for the imbalances within these groups. Thus, the regression-adjusted estimates of the treatment effect would be adequate. However, if the distributions of pre-treatment variables are not similar for the two groups, regression is not appropriate. In these cases, regression masks the fact that there may not be good comparisons, within the sample, for individuals across the two groups. As an example of this issue, Table 1.5 displays the joint distribution of experimental donors' most recent and highest donation amounts. In the top-left cell of this table (corresponding to those whose most recent and highest donations were each \$50 or more), there are five experimental donors from the high treatment group, but none from the low treatment group. Therefore, the sample does not contain an adequate counterpart for these five individuals.[2] For this reason, an estimator based on matching similar individuals is preferable to OLS regression (see Austin (2011) for an accessible discussion of matching methods and why they may be preferable to OLS regression; Imbens (2014) provides a more technical consideration of the subject).

Table 1.5
Overlap of treatment groups by past giving behavior for experimental donors

		Most recent donation		
Highest donation		\$50+	< \$50	Total
\$50+	# High treatment:	5	2	7
	# Low treatment:	0	4	4
< \$50	# High treatment:	0	24	24
	# Low treatment:	0	20	20
Total		5	26	31
		0	24	24

Notes: The top and bottom number in each cell represents the number of experimental donors assigned to the high treatment and low treatment groups, respectively. The presence of cells where one of the numbers is zero and the other is non-zero suggests that a matching method may be more appropriate than OLS regression.

[2] For simplicity, these variables are discretized into two categories, though the same pattern exists with more categories.

1.5.2 Propensity Score Matching

In order to estimate the degree of similarity between two individuals, propensity scores (i.e., the probability of being assigned to the high treatment group, conditional on observed donor characteristics) are estimated for each individual using a probit model.[3] The propensity score is a balancing score, meaning that when it is conditioned upon, the distributions of the covariates used to construct the scores are the same across experimental groups (Rosenbaum and Rubin (1983)). Thus, propensity score matching adjusts for the pre-treatment differences in the two groups, making observed variation in giving between them more likely to be the result of the experimental treatment.

Using the propensity score matching approach, the effect of high social information is estimated by comparing the donations of individuals in the high treatment group with the most similar one in the low treatment group.[4] This is clearly preferable to OLS regression, which simply compares the overall averages of the two groups.

[3] The results from this estimation and summary statistics for the estimated propensity scores are included in Appendix 1.C. To ensure that the results would not be sensitive to different probit model specifications, variants of this model were estimated (e.g., including interactions), but the predicted probabilities were nearly identical to those from the original model (i.e., the correlation coefficient was never lower than 0.998).

[4] This method of implementing the propensity score matching estimator is known as the nearest-neighbor match. Kernel matching yields nearly identical results.

Table 1.6
Estimates from propensity score matching

	(I)	(II)
Dependent variable:	Experimental donation amount	Probability of making a donation
High treatment indicator	14.95*	-0.041***
	(8.48)	(0.006)
Observations	55	15,166

Notes: Bootstrapped standard errors are in parentheses. Regression (I) contains only the subjects who made a positive donation as part of the experiment, while regression (II) contains all subjects (i.e., experimental donation amount = $0 for non-donors). High treatment indicator = 1 (0) for subjects who received a reference to the 80th (40th) percentile donation amount from the distribution of past donations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.6 contains estimates from the propensity score matching procedure. After matching individuals in the two experimental groups to account for differences in group composition, the effect of high social information increases to $14.95.[5] This estimate is somewhat larger than, but consistent with the raw difference in means and the OLS estimate. Within the propensity score matched sample, the effect of high social information on the probability a lapsed donor will contribute is negative. In particular, referencing the 80th percentile donation amount to lapsed donors reduces the probability of donation by 4.1%, relative to a reference to the 40th percentile amount. Thus, while high social information may increase generosity among those donors who choose to contribute, this effect may be countered (to some degree) by deterring some lapsed donors from giving anything.

[5] An alternative method of controlling for selection bias is using Heckman Two-Stage regression analysis (Heckman (1979)), which accounts for the selection process that led to only a small number of the solicited donors making a contribution during this experiment. This method produces a very similar estimate of $14.58, which is significant at the 5% level.

1.6 Limitations

While many of the potential concerns with this analysis have been addressed and corrected using statistical techniques, a few limitations to the generalizability and validity of this analysis warrant further discussion. First, because the NPO chose to focus the experiment on only those lapsed donors who had previously given less than $100, the generalizability of the results may be limited to only low-contribution donors. While it is not clear that more generous donors would respond differently to the high social information, this concern cannot be fully resolved here.

The second major limitation that remains has to do with the internal validity of the study. This limitation stems from the fact that, in addition to the different informational treatments, the NPO made other changes to the two treatment letters (see Appendices 1.A and 1.B). Although many of these differences are only in the wording, the NPO also changed the placement of the statement referencing the donation amount. For the high treatment group, the statement is in the concluding paragraph of the letter, but for the low treatment group, it is in the middle of the letter – immediately following the explanation of the matching offer. Since the high informational treatment is contained in a paragraph of its own at the bottom of the letter, those who received this letter may have been more likely to receive and process the information than those who received the low treatment letter. In the extreme, this would mean that the treatment effect measures the impact on donations of the high informational treatment relative to a somewhat 'pure' control group that received no social information.[6]

[6] Although it seems like this is the most likely effect, it may also be true that because the low informational treatment is earlier in the letter, it could have had a greater impact on the giving behavior of potential donors than did the high treatment. In the extreme case of this possibility, the treatment effect would then measure the impact on donations of the low informational treatment relative to a control group that received no social information.

Regardless of how the variation in the letters and the placement of the treatment statement affected donors, these differences mean that it is impossible to completely isolate the effects of the high social information from those caused by the differences in the letters. Insofar as these differences between the high and low treatment letters affect the decision to donate on the extensive and/or intensive margin, this will bias the estimation of the treatment effect. The differences do not appear substantive enough to invalidate the results of this paper, but this assumption cannot be tested.

1.7 Conclusion and Discussion

The results of this experiment support the hypothesis that the provision of high social information affects lapsed donors' giving behavior, but in potentially offsetting ways. On the intensive margin of giving, this study suggests that for somewhat less-committed donors, information about another person's relatively high donation (i.e., the 80^{th} percentile donation amount from the distribution of past donations) induces a more generous contribution than does a reference to a lower donation amount. This finding is consistent with research showing that high social information increases donations among active donors (Croson and Shang (2008), Shang and Croson (2009)).

While high social information is found to increase generosity among those who donate, it is also found to have an opposing effect on the extensive margin of giving. After correcting for differences in the composition of the two experimental groups, the high social information reduces the probability a lapsed donor will make a contribution. Although Croson and Shang (2008) find that social information does not affect donation participation rates and Frey and Meier (2004) find that high social information increases the probability of donating, neither of these studies distinguishes between lapsed and active donors. Since active donors are much more likely to give than are lapsed donors, it seems likely the observed effects in these two studies are driven by active donors. The results of this paper are consistent with those of Prokopec and De Bruyn (2009), who find that suggested donation amounts reduce the likelihood lapsed donors will give. Although Verhaert and Van den Poel (2011) did not find social information to affect the average donation given by lapsed donors, this paper's conclusion on the more relevant margin for lapsed donors – the donation participation rate – matches their results.

As mentioned above, there was not much clustering around the actual donation amounts referenced in the solicitation letters (i.e., $50 for the high treatment group and $25 for the low treatment group). Thus, although this hypothesis cannot be tested with these data, the observed effect does not seem to be driven by a desire to match the amount reported in the letter. Instead, it seems more plausible that the information was viewed as a benchmark from which donors determined how much to give – leading those in the high treatment group to be, on average, more generous than those in the low treatment group. Shang and Croson (2009) suggest that new donors may need a point of reference to help determine the "appropriate" donation amount, and this appears to also be true of those who have not given in several years.

While nonprofit organizations can use high social information to boost the average contribution given by lapsed donors, such references may reduce the probability they make a donation. The tension between these two results is especially noteworthy, since nonprofits are concerned with both increasing donations and reactivating lapsed donors. Nonprofits that use social information in fundraising campaigns should consider whether this is appropriate for both active and lapsed donors. If the primary goal is to get lapsed donors to start giving again (at any level), high social information does not appear to be an appropriate technique.

If the primary concern of a nonprofit is to maximize revenue, the estimates from this paper and a simple back-of-the-envelope calculation can inform whether high or low social information is more appropriate, given expectations of donors' behavior. Suppose n donors are solicited, each with probability p of donating an average of d under the baseline condition (i.e., having received low social information), then the expected revenue for the baseline group would equal $n * p * d$. If these individuals had, instead, been given high social information, the propensity score matching estimates suggest the probability of donating would be $p - 0.041$ and the average donation amount would be $d + 14.95$. Thus, the revenue for the high social information group would equal $n * (p - .041) * (d + 14.95)$. Using the values of n and d from this experiment (15,166 and 33.28, respectively), high and low social information would yield the same revenue if the probability of donating for the baseline group is 0.1322.[7] If the probability of giving is lower (higher) than this, the increase in the average donation induced by high social information is smaller (greater) than the cost of deterring some donors from giving, so revenue is maximized with low (high) social information. In general, if the probability of giving is low (as is usually the case for lapsed donors), the costs of potentially further reducing this probability swamp the value of increased donations given by those who chose to contribute.

In implementing these social information fundraising strategies, nonprofits should also consider whether it fits with their nature to induce higher contributions than would have been received in

[7] Note that n can be normalized to one without loss of generality. In general, the "breakeven" baseline response rate (i.e., the point at which low and high social information yield the same revenue) for any given average donation amount is computed as $p = \frac{1}{14.95}(0.041 * d + 0.61295)$. This is found by setting the expected revenue for low social information equal to that for high social information and solving for p.

the absence of the social information. While some donors may appreciate the sort of benchmark provided by social information, others may prefer to not receive such prompts. By using social information, nonprofits may lose credibility with donors. Sargeant (2001a) suggests this may happen if donors perceive the organization as having what is termed a "focus on transactions," rather than the "focus on relationships" that fosters long-term loyalty to the organization. These considerations, together with the results of this study, should help inform nonprofits as to the benefits (and costs) of using social information in soliciting donations from lapsed donors.

Chapter 2

Do Land Use Regulations Stifle Residential Development? Evidence from California Cities

2.1 Introduction

Since the beginning of the 20th century, cities and counties across the United States have turned to land use regulation in various forms to manage the location, rate, and type of development that occurs in their communities. These policies are among the most controversial aspects of local political action – sometimes even affecting outcomes of local council and mayoral elections (Lewis and Neiman, 2000).

The effects of land use restrictions have been explored extensively, but primarily in terms of their impact on housing prices. Recent additions to this literature find land use regulation to positively affect housing prices. While this positive relationship may stem from an increased willingness-to-pay for housing in communities that more strictly control development, many researchers take it as support for the theoretical prediction that land use regulation restricts the supply of new housing. This paper focuses on the extent to which this restriction actually occurs.

Relatively few studies have attempted to estimate the extent to which land use regulation stifles new residential development. Moreover, the majority of those papers that have endeavored to do so rely upon cross-sectional policy variation, which precludes the ability to control for unobserved local characteristics. This paper shows, among other things, that the effect of land use regulation on residential development may be overestimated if unobservables are not taken into account. Using a panel of regulatory data, the paper estimates the effects of various land use regulations, individually and collectively, on residential development in California cities from 1970-1995. Given California's rapid population growth during much of this period, along with the extensive use of voter initiatives and the localized nature of its land use authority, many growth controls and other land use regulations were adopted across the state during these years.[8] Using city and year (two-way) fixed effects, the approach employed in this paper effectively compares the changes in residential development in cities that raised the restrictiveness of their land use regulations to the changes in development in cities that did not.

The data suggest that the implementation of an additional land use regulation reduces the housing stock by an average of 0.2% per year. Residential permits are reduced by an average of about 4% per restriction. Land use regulation reduces new construction for both single and multi-family housing, but the effect on the latter is much larger. Of the regulations measured, those categorized as zoning and general controls have the strongest effects, again with much stronger effects on multi-family dwellings. An analysis of the partial effects of each regulation shows the important result that while some policies reduce residential development, others actually increase it. Thus, although the regulatory indices that dominate the literature may offer

[8] Glaeser (2013) discusses the role these regulations likely played in the dramatic price growth experienced in California between 1970-1990.

the best measure of the stringency of a community's regulatory environment, this sort of aggregation masks some important underlying effects.

The next section of this paper gives a brief review of the existing literature. Section 2.3 contains a description of the dataset employed in this study. The formal analysis of the data is contained in Section 2.4. Section 2.5 concludes.

2.2 Relevant Literature

Over the last four decades, researchers have developed an enormous literature empirically exploring the effects of local land use regulation. The vast majority of these studies have focused on the correlation between housing or land prices and the presence of land use regulation. While there is not strong consensus in the early literature, many recent studies find housing prices to be positively related to land use regulation.[9] Although this positive correlation is thought to be driven (at least partially) by supply-side factors, relatively few researchers have attempted to actually quantify the supply restriction that theory suggests would occur in the market for new housing following the adoption of (more) land use regulation.[10]

The bulk of the current literature exploring this relationship uses cross-sectional variation in local regulatory regimes and finds that land use regulation (measured in several different ways) significantly reduces housing construction. Thorson (1997) finds that an increase in the minimum lot size significantly reduced housing starts in rural areas of McHenry County, Illinois.

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[9] See Fischel (1990) for a review of the early literature. This literature is also summarized well by Quigley and Rosenthal (2005), which contains more recent contributions. See also Glaeser and Gyourko (2003), Ihlanfeldt and Shaughnessy (2004), Glaeser et al. (2005), Mostafa et al. (2006), Hui et al. (2006), Ihlanfeldt (2007), Chakraborty et al. (2010), Zabel and Dalton (2011), Caldera and Johansson (2013), and others.
[10] Ihlanfeldt (2004) provides a brief summary of the literature relating land use restrictions to residential housing development.

Mayer and Somerville (2000) use data from 44 U.S. metropolitan areas to show that areas with more stringent regulatory environments issue up to 45 percent fewer single family housing permits than less-regulated areas. Levine (1999) estimates that each additional land use regulation adopted by cities and counties in California led to 884 fewer housing units being built across that state between 1980-1990. Quigley and Raphael (2005) use an earlier version of the regulatory data from Levine (1999) and find that land use regulation reduces the stock of single family housing, while having no effect on multi-family housing.

Although most of the existing work suggests land use regulations restrict growth, some studies have found evidence to the contrary. In their 1992 monograph, Glickfeld and Levine describe the immense population growth that took place in California in the 1980s, as well as the land use restrictions that followed. They run a few basic time series regressions of residential permits from 1973-1988 on the annual number of land use regulations enacted statewide and then separately for various metropolitan areas throughout the state. These regressions lead them to conclude that the regulations did not significantly affect new construction. Pendall (2000) uses cross-sectional data from over 1,000 jurisdictions in the 25 largest metropolitan areas to estimate the effect of various land use regulations on housing starts and affordability. He finds that while residential construction is reduced by zoning laws that only allow for low-density development, urban growth boundaries, adequate public facilities ordinances, and building permit caps have little or no effect on the construction of new housing.

A handful of authors have used panel techniques to examine the relationship between various land use regulations and housing construction, but with no less discordant results than from the cross-sectional studies. While Dempsey and Platinga (2013) find that urban growth boundaries reduce the probability of development, Sims and Schuetz (2009) show that wetland protection bylaws do not significantly impact residential development. Skidmore and Peddle (1998) and Burge and Ihlanfeldt (2006) examine the effects of impact fees on housing construction using panel regulatory data for jurisdictions in Illinois and Florida, respectively. The former study finds that the adoption of impact fees reduces residential development, while the latter finds that impact fees increase construction of single family housing.[11]

The approach taken in this paper is most similar to that of Glaeser and Ward (2009). These authors use a panel of regulatory data to determine the effects of minimum lot sizes, stringent wetlands bylaws, septic regulations, and subdivision rules in Greater Boston. The effects of the latter three regulations are analyzed individually and collectively by way of a dynamic regulatory index, which sums the values of indicators for each of the three regulations. They find that land use regulation significantly reduces the issuance of building permits, with the effect coming primarily through subdivision rules. Despite the thoroughness of this study, the data only cover the Boston metropolitan area, so the generalizability of its findings may be limited.

This paper fills a void in the current literature by more accurately estimating the effects of land use regulation on the type and amount of new housing development in California. By exploiting within-city variation in the timing of adoption for various land use regulations, this paper uses two-way fixed effects regressions to identify the effects of land use regulation on residential development. Additionally, the novel dataset used in this study documents the annual number of permits issued for each city in California between 1970-1995.

[11] The theoretical model in Burge and Ihlanfeldt (2006) predicts this would occur when impact fees reduce exclusionary regulations and increase the percentage of proposed projects that are approved for construction.

2.3 Data Description

2.3.1 Regulatory Data

The data utilized here come from several different sources. The regulatory data are composed of responses to two surveys of California land use officials. The first survey was administered in 1989 (Glickfeld and Levine, 1992) and the other in 1992 (Levine et al., 1996). The jurisdictions represented in these two surveys account for 99.9% of the land area of California and 99.4% of the 1990 population (Levine, 1999). The data contain eighteen dummy variables indicating which of the various land use restrictions had been adopted in each jurisdiction as of 1992. Table 2.1 displays the eighteen regulations measured in the data, as well as the variable names used in this paper. The policies are grouped by whether they are intended to regulate residential or non-residential development. Additionally, the residential land use regulations are categorized according to the nature of each policy. These classifications essentially follow those put forth by Glickfeld and Levine (1992).[12]

[12] There are a few notable differences between Glickfeld and Levine's (1992) classification and the one used in this paper. First, the previous authors only included the first of the two surveys, so the variables representing subdivision limits and infill requirements are not included in their listing. Limitations on the number of subdivisions that can be created within a given time frame clearly fit with the population control devices, since these attempt to stunt growth. Policies requiring that developed areas are substantially developed before new construction can occur (i.e., infill development requirements) fit most naturally with the zoning control policies, since these are intended to affect the way in which development occurs within the city, rather than to stop its growth. When infill requirements are grouped as a population control, rather than a zoning control, the estimates in Section 2.4.3 are stronger, but the results of the paper are unchanged. The second difference between Glickfeld and Levine's (1992) categorization and the one used here is that the previous authors classify residential and non-residential adequate public facilities ordinances (APFOs) in a category of their own. Since this paper is primarily concerned with the effects of residential land use regulations, residential APFOs are classified as zoning controls, given that they are most similar in nature to those policies. They may also fit with population control regulations, since they essentially cap the size of the city until adequate infrastructure is in place. When residential APFOs are, instead, treated as a population control, the estimates in Section 2.4.3 are somewhat stronger, but the results of the paper remain unchanged.

Table 2.1
Land use restrictions and variable names

Residential

Population control

Population growth limits	[population limit]
Restrictions on the number of residential building permits	[residential permit limit]
Established urban limit line or greenbelt beyond which development is not permitted	[urban growth boundary]
Restrictions on number of new subdivision lots that can be created within given time frame	[subdivision limit]

Zoning control

Restrictions on structural floor area that can be built on a given parcel	[floor area ratio]
Reduced permitted residential density	[reduce density]
Rezoned residential land to open space or less intense use	[open space]
Phased development areas where development approval is deferred until existing developed areas are substantially developed	[infill]
Requirement of adequate service levels as a condition for approval of a residential development (i.e., adequate public facilities ordinances)	[adequate public facilities]

Political control

Requires voter approval to increase residential densities	[voter approval]
Requires super-majority council vote to increase residential densities	[supermajority approval]

General control

Adopted growth management element in general plan	[growth mgmt]
Other measure to control rate, intensity, type and distribution of development	[other]

Commercial/Industrial

Adequate service levels required as a condition for approval of commercial or industrial development	[commercial adequate public facilities]
Reduced permitted height of commercial/office buildings	[reduce height]
Rezoned commercial/industrial land to less intense use	[less intense]
Restricts commercial square footage that can be built within given time frame	[sqft commercial]
Restricts industrial square footage that can be built within given time frame	[sqft industrial]

Glickfeld et al. (1999) and Levine (1999) also use the regulatory data employed in this analysis, while Glickfeld and Levine (1992), Landis (1992), Brueckner (1998), and Quigley and Raphael (2005) each use an earlier version of the dataset, containing information from the first survey only. The main explanatory variable used by all of these researchers is a static index for the stringency of land use regulation in each jurisdiction at the time of the survey, constructed by summing the number of restrictions in place out of the total number of restrictions measured.[13] [14] The analysis in this paper exploits an underutilized aspect of this dataset – the reported year in which each restriction was adopted. Assuming survey respondents accurately reported the years in which the various land use regulations were adopted, these data are as if the survey was implemented each year during the panel.[15] Using information on the timing of adoption, dynamic indicators are constructed for the presence of each land use restriction. Following Glaeser and Ward (2009), these indicators are then used to construct dynamic indices of regulatory stringency. The effects of these policies on housing development are then estimated – individually and collectively (through the indices).

As shown in Figure 2.1, while some form of land use regulation was in place in many California cities in 1970, the adoption of these policies increased dramatically starting in the mid-1980s. The most common residential land use regulations adopted since 1985 are floor area ratio restrictions, adequate public facilities ordinances, and reductions in the permitted density of the city (see Figure 2.2). By 1993, each of these policies had been implemented in 118 or more of the cities in the sample.

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[13] Levine (1999) also explores the effect of various land use restrictions individually.
[14] Using other data sources, Ihlanfeldt (2007), Malpezzi (1996), and several other researchers construct similar additive indices using cross-sectional regulatory data.
[15] It is likely that respondents were able to report these years accurately, given the amount of regulatory adoption that occurred in the years just prior to the administration of the surveys (see third column of Table 2.1 and Figures 2.1 and 2.2).

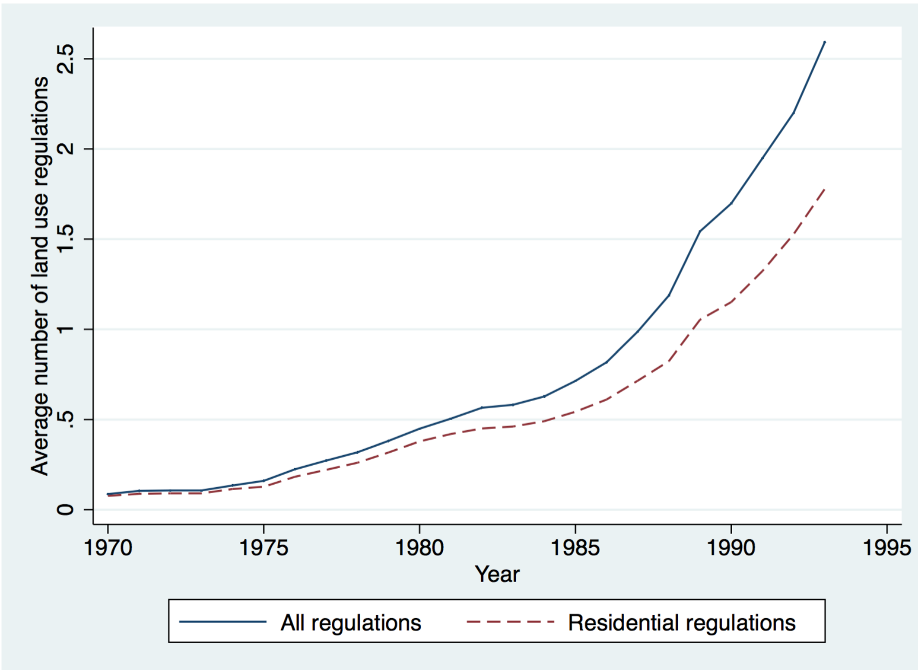

Figure 2.1: Average number of land use regulations adopted over time

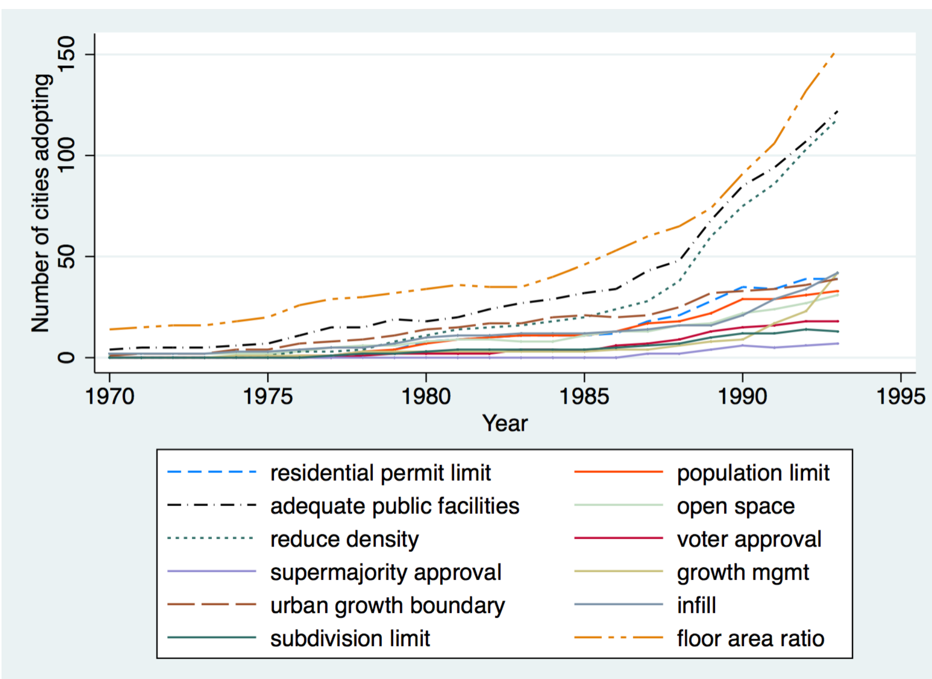

Figure 2.2: Number of cities adopting each residential land use regulation over time

Do neighboring cities adopt similar regulatory patterns? Brueckner (1998) answers this question using an earlier version of the regulatory data used in this paper. He finds the level of land use regulation employed in cities across the state of California to be positively related to levels in nearby cities. Likewise, the data used for this analysis, show a positive and statistically significant relationship in the regulatory environment of neighboring cities. Logistic regressions show that the presence of a particular regulation in a given city significantly increases the probability that city's nearest geographical neighbor has also adopted the policy for all but six of the regulations measured.[16] Moreover, the number of land use regulations adopted in these cities is significantly related to the number adopted in their nearest-neighboring city.[17] Coastal cities have an average of 0.518 more land use regulations than those not on the coast. The most heavily regulated areas are in the southern coastal region and the San Francisco Bay area.[18] [19]

2.3.2 Residential Building Permit and Other Data

Annual data on the number of new construction permits issued in each city in California come from the California Housing Foundation's Construction Industry Research Board (CIRB). CIRB's California Construction Review provides insight regarding the health and activity of the home building industry in the state. In particular, they maintain data on the annual number of

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[16] Given the interdependencies in the regulatory regimes of nearby locales, these regressions are not used to make causal claims, but only to explore correlations in the adoption of land use regulation. The policies that spill over less often are: reductions in the permitted height of commercial buildings, subdivision limits, rezoning residential land to open space, infill development requirements, and residential and commercial adequate public facilities ordinances.

[17] In particular, each additional land use regulation adopted by a city is associated with an average increase of about a third of one land use regulation in the city's nearest neighbor.

[18] Each of these regions contains about one-third of the more heavily regulated cities, where "more heavily regulated" is defined using various threshold numbers of regulations enacted.

[19] While both of these regions experienced positive population growth between 1970-1995, both saw slower growth rates after the proliferation of land use regulation in the late 1980s. Moreover, these more regulated areas have experienced much less population growth in the last decade than areas farther inland, providing some evidence that land use regulation continues to affect growth patterns. However, the link between population growth and land use regulation is likely through prices, which, as discussed above, reflect both supply- and demand-side factors.

single family, multi-family, and total residential permits issued in every city and county in California from 1970 to present. These data can be used to investigate whether different land use regulations have a differential impact on the type of development that occurs within a given jurisdiction. Table 2.2 contains summary statistics for the housing stock and permit issuance during the time of the analysis.

Table 2.2
Summary statistics for housing data

	Mean	Standard Deviation	Min	Max	N
Total residential permits	365	1,094	1	23,783	8,545
Multi-family permits	189	816	0	22,124	8,544
Single family permits	176	399	0	8,784	8,545
Housing stock	21,740	76,688	281	1,318,835	7,961

Permit data are used to construct several outcome variables for the analysis. While the majority of the existing literature explores the impact of regulation on changes in new construction (e.g., Glaeser and Ward (2009), Mayer and Sommerville (2000), and others), some authors have considered its effect on changes in the housing stock (e.g., Quigley and Raphael (2005), Pendall (2000)). Effects of both kinds are estimated in Section 4 of this paper.

After matching incorporated cities in the regulatory data with 1970-2000 Census data, the sample contains 402 cities. The latest year of enactment for any of the land use regulations measured in the data is 1993. Therefore, the panel dataset created for this analysis spans the period 1970-

1995.[20] The State of California Department of Finance provides annual population estimates for each city during this time frame, but any other potential covariates that are available for every city in California must come from the decennial Census data.[21] For non-Census years, covariate values are obtained via linear interpolation. This method assumes that demographic changes occur linearly over time within each city. This may be a strong assumption, but the fact that the interpolated population values are almost perfectly correlated with the actual state estimates (correlation = 0.9999) lends some credibility to the method.[22]

2.4 Analysis

A key advantage of using panel data is the ability to control for unobserved characteristics that may contaminate estimates in a cross-sectional analysis. If not taken into account, these unobservables could lead to biased estimates of the effect of land use regulation, since they are likely to affect the rate of new construction and to be correlated with the adoption of regulation. To the extent that these unobservables are time-invariant and/or contemporaneously common to all cities, the two-way fixed effects approach employed in this paper produces unbiased estimates of the effect of interest. Using this approach, the effects of land use regulations are identified by comparing within city changes in residential development in cities that adopted more regulation in that year to associated changes in cities that did not. The base model specification for this analysis is of the form:

[20] While this analysis implicitly assumes that there were no changes in regulation between 1993-1995, results from specifications excluding years after 1993 yield very similar results to those presented below.
[21] The U.S. Census Bureau also produces annual population estimates, but for years prior to 1990, these are only available online at the county, state, and national geographic levels.
[22] This finding is especially promising given that the state's estimates come from the average of several independent and fairly comprehensive methods that account for, among other things, changes in school enrollment, births, voter registration, and California drivers' license address change filings.

(2-1) $$Y_{ct} = \alpha + R_{ct}\beta + \varphi_c + \tau_t + \varepsilon_{ct} ,$$

where Y_{ct} is either a measure of the percentage change in the housing stock or the natural logarithm of permits issued (either total residential, multi-family, or single family) in city c and year t. R_{ct} represents the regulatory measure in city c in year t. φ_c is a vector of city dummies intended to capture unobserved characteristics that are city-specific and constant over time (such as local weather, established reputation of schools, and other fixed amenities). τ_t represents a vector of year dummies to control for time-varying factors that affect housing construction and are contemporaneously common to all cities (such as interest rates, costs of construction, and other cyclical factors).

It is important to note that although two-way fixed effects regressions eliminate potential sources of bias stemming from city-specific time-invariant factors, as well as contemporaneous factors common to all cities, they do not account for the possibility of dynamic selection in the adoption of land use regulation. That is, since cities may change idiosyncratically over time, and because current local conditions potentially affect both the local regulatory environment and housing development, city and year fixed effects may fail to capture all potential sources of bias.

The next specification adds time-varying demographic and housing characteristics in an attempt to purge the estimates of any bias stemming from dynamic selection in the adoption of land use regulation. This specification takes the form:

(2-2) $$Y_{ct} = \alpha + R_{ct}\beta + \varphi_c + \tau_t + X_{ct}\theta + \varepsilon_{ct} ,$$

where each variable is as defined above, and X_{ct} is a vector of demographic and housing control variables for city c in year t. In particular, this vector includes median income, percent white, percent black, percent owner-occupier, percent foreigner, percent of housing units that are currently occupied, the percent of housing in rural areas, and, for some of the specifications, population size.

Lastly, proper consideration must be given to the correct adjustment of the standard errors obtained through this analysis. Although land use regulation is generally implemented at the city level, growth is a regional phenomenon (Glickfeld and Levine (1992)). This fact suggests the error terms in the city-level regressions above are likely correlated among nearby cities. Similarly, observations of the same city over time are not independent. The presence of spatial and/or serial autocorrelation makes inference based on standard OLS estimates of the covariance matrix incorrect. To allow for heteroscedasticity, cross-sectional spatial correlation, and city-specific serial correlation, standard errors are adjusted following the approach of Conley (1999) and Conley (2008).[23] This method consists of estimating standard errors using a weighted average of spatial and serial autocovariances. The weights, which come from Bartlett kernels, decline linearly from 1 to 0, with a weight of 0 assigned to cities beyond prespecified threshold distances (in space and time). The thresholds used throughout this analysis are 100 km (or, about 62 miles) and 10 years.[24]

2.4.1 Effects of Regulatory Stringency – General Index

The first set of specifications measures the stringency of land use regulation (that is, the variable R_{ct} in equations (2-1) and (2-2)) using a dynamic regulatory index, which sums the number of restrictions in place out of the eighteen that are measured in the data. Although, as discussed

[23] This analysis was carried out using the *ols_spatial_HAC* Stata program from Hsiang (2010).
[24] Other thresholds were tested for robustness, but none yielded meaningful differences from the results discussed below.

above, numerous scholars have used this simple additive measure of land use regulation, there is no reason to think this is the best measure of each city's regulatory environment. An alternative method of data reduction, used by Malpezzi (1996), Gyourko et al. (2008), and others, is to construct a measure of regulatory stringency using weights from a factor analysis of the regulatory data. When carried out upon these data, the resulting factor scores are quite highly correlated with the simple sum, so the additive index is used throughout this paper.[25] Given the construction of the regulatory index, the estimated effect is the expected impact of imposing an additional land use restriction within a city.

The effect of land use regulation is estimated first using a measure of the annual percentage change in the housing stock, which is computed as

$$\text{Percentage change in housing stock}_{ct} = \frac{\text{Total Residential Permits}_{ct}}{\text{Housing stock}_{ct-1}}.$$

Since housing stock counts are only available for census years, annual estimates are obtained following the approach of Saks (2008a) and Saks (2008b). This method estimates each year's housing stock as the stock in the previous year plus the number of permits issued that year, minus an annual adjustment factor set to equate the housing stock estimates in each census year to the counts reported by the U.S. Census Bureau. Disaggregated housing stock data (i.e., the number of single family and multi-family structures) are not available from each of the required censuses, so this variable is only computed for overall residential development.

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[25] Pearson's correlation coefficient = 0.7027 if factors are rotated and 0.9646 if they are not. Using the first factor from the factor analysis (which accounts for 59% of the variance in the data), estimates of the effect of land use regulation are uniformly larger in magnitude for each specification, but the results are not qualitatively different from those reported here.

Since the housing stock is proportional to population, an alternative way to construct the previous measure is to divide permits by lagged population.[26] This method, which is roughly equivalent to permits per capita, is preferred to the former one, since the state of California provides rigorous annual population estimates for each city over the relevant time frame. Estimates of the effect of land use regulation using both measures are presented in the top two panels of Table 2.3. The bottom three panels of Table 2.3 contain similar estimates corresponding to regressions with the dependent variable being the natural logarithm of permits (either total residential, multi-family, or single family).

[26] A regression of population on the census reported housing stock yields an extremely precise estimate of 2.58 with an R-square of 0.9951.

Table 2.3
Coefficient on number of land use regulations enacted (1970-1995)

Dependent Variable	(1)	(2)	(3)
$\frac{\text{Total Residential Permits}_t}{\text{Housing stock}_{t-1}}$	-0.0045**	-0.0021**	-0.0020**
	[0.0005]	(0.0003)	(0.0003)
N	7849	7849	7849
$\frac{\text{Total Residential Permits}_t}{\text{Population}_{t-1}}$	-0.0017**	-0.0008**	-0.0007**
	[0.0002]	(0.0001)	(0.0001)
N	8472	8472	8472
Log(Total Residential Permits)	-0.156**	-0.046**	-0.039**
	[0.02]	(0.01)	(0.01)
N	8545	8545	8545
Log(Multi-Family Permits)	-0.239**	-0.065**	-0.060**
	[0.02]	(0.02)	(0.02)
N	6272	6272	6272
Log(Single Family Permits)	-0.065**	-0.040**	-0.031**
	[0.01]	(0.01)	(0.01)
N	8456	8456	8456
City FEs	No	Yes	Yes
Year FEs	No	Yes	Yes
Controls	No	No	Yes

Notes: Cluster-robust standard errors are shown in square brackets. Conley standard errors are in parentheses. The key variable in each of these regressions is a dynamic index that sums the number of land use regulations that have been enacted in each city. Control variables for the top two panels include median income, percent white, percent black, percent owner-occupier, percent foreigner, percent of housing units that are currently occupied, and the percent of housing in rural areas. For the bottom three panels, the controls also include population size. Significance at the 5% and 1% level is denoted by * and **, respectively.

To highlight the contribution of fixed effects regressions, column (1) of Table 2.3 shows, for the different outcome variables, the estimated effect of land use regulation using a pooled model similar to the cross-sectional ones typically used in the existing literature.[27] The pooled estimates are uniformly larger in magnitude than those from the two-way fixed effects regressions (in columns (2) and (3) of Table 2.3).[28] This disparity likely stems from one of two potential sources (or, presumably, a combination of the two). First, to the extent that unobserved heterogeneity affects both permit issuance and the stringency of local regulation, models that fail to account for this heterogeneity will overstate the negative effect of land use regulation on housing development. Second, the smaller estimates in columns (2) and (3) may reflect attenuation bias due to measurement error, which is amplified by the use of fixed effects. The role of measurement error is discussed in more detail below.

Fixed effects regressions are employed in this analysis to eliminate (or reduce) omitted variables bias. One potential source of bias is the presence of fixed geographic constraints. Insofar as these constraints are positively correlated with regulatory stringency, the effects of regulation will be overstated. This is the case for coastal California cities, which face geographic constraints to development, and are, as previously mentioned, more likely to adopt land use regulation.[29]

[27] Cluster-robust standard errors are shown in column (1) of Tables 2.3 and 2.4 because the program used to produce Conley standard errors is not compatible with pooled regression models. The pooled estimates are shown only to contrast with the fixed effects estimates and are not to be interpreted themselves, so these potentially incorrect standard errors do not affect the conclusions of the paper.

[28] When control variables are included in the pooled regression models, the estimated effects of regulation are somewhat less negative than those reported here, but still uniformly larger (in magnitude) than in the fixed effects regressions, so the qualitative results are the same.

[29] To the extent that geographic constraints can be adequately measured (e.g., Saiz (2010)), this source of bias can be controlled for in cross-sectional analyses. However, the fixed effects estimator removes the effects of these time-invariant attributes whether or not they are even observable.

Another important source of heterogeneity that could bias cross-sectional estimates is unobserved attitudes about growth. Local governments in cities (or years) where growth is perceived negatively are likely to adopt a greater number of land use regulations. Land use officials in these cities are also more likely use informal, ad hoc forms of regulation to hinder development.[30] Thus, we would expect city-specific attitudes about growth to be positively correlated with the adoption of land use regulation and negatively related to permit issuance. These relationships suggest that estimates of the effect of regulation on residential development using cross-sectional variation are likely to be biased downward by omitted factors (making estimates more negative). In other words, cross-sectional estimates may overestimate the negative effect of formal land use regulations on residential construction, since they capture the effects of both formal and informal kinds of regulation. To the extent that attitudes about growth and other potential sources of bias are city or year specific, the two-way fixed effects estimator resolves this issue.[31]

The results in Table 2.3 show that even after controlling for time-invariant city characteristics and contemporaneous factors common to all cities, regulation has a sizable and statistically significant impact on the growth of both the stock and flow of housing. In particular, the top panel of Table 2.3 suggests that a one-unit increase in the regulatory index reduces housing supply by an average of 0.2% per year. Using the estimates in the second panel of Table 2.3, each additional land use regulation is leads an average reduction of 0.7-0.8 permits per 1,000 residents. For both of these dependent variables, the estimates imply that for the "average" city

[30] Landis (1992) discusses various ways in which local governments can use informal regulation to retard growth; for example, they may refuse to annex vacant land or extend utility services to developing areas.
[31] While we may be interested in the combined effect of formal and informal land use regulation on residential development, the latter is difficult to measure and, in many cases, beyond the purview of policymakers. Hence, the focus of this paper is on the effects of formal land use regulation.

in the sample (with 21,740 housing units and population of about 55,140), each land use regulation reduces the housing stock by about 40 housing units per year.

While the top two panels of Table 2.3 contain estimates of the effect of land use regulation on growth in the housing stock, the bottom three display estimates of the effect on residential permit issuance. These estimates indicate that each additional land use regulation leads to about a 4% reduction in the number of new residential permits issued. Although the effect is significant for both multi-family and single family dwellings, the former is more affected by land use regulation, with 6-6.5% fewer permits issued per regulation.

For each of the different outcome variables, results from specifications that include the (admittedly sparse and imprecisely measured) set of time-varying controls are similar to those from specifications that leave out the controls. Estimated coefficients and standard errors for the control variables from the regressions in column (3) of Table 2.3 are contained in Appendix 2.A. This table indicates that, with the exception of population size, the coefficient on each of the controls is statistically significant for at least some of the specifications.

As mentioned above, the difference in estimates from the pooled and fixed effects regressions hints at omitted variables bias in the pooled regressions. However, it is important to note that the attenuating effect of measurement error from misreported years of adoption is exacerbated by the use of fixed effects. Thus, some of the observed difference between the pooled and fixed effects regression estimates may be due to measurement error.[32]

[32] In an attempt to verify the timing of the regulatory data used in this paper, land use officials in nearly every city across the state of California were contacted. Very few were willing (or able) to verify the years of adoption for the various regulations. Those who were willing to cooperate were generally only able to provide rough estimates for

While there is probably some misreporting in the years of adoption recorded in these data, it is likely to be less prevalent in the years closest to when the surveys were administered. That is, because recently adopted regulations are presumably more salient, survey respondents are more likely to report the correct years of adoption for these policies. Thus, an analysis focused on the years closest to the when the surveys were administered can be helpful in determining whether the smaller panel estimates are primarily the result of attenuation bias or a smaller underlying causal effect. To this end, Table 2.4 reports results from specifications identical to those in Table 2.3, but which drop observations prior to 1985.[33] Despite reducing the sample sizes by more than 50%, the estimates in Table 2.4 are similar to those in Table 2.3. More importantly, even after focusing on those observations least likely to be mismeasured, panel estimates are remarkably smaller, in magnitude, than those from the pooled regressions in column (1). This finding lends credibility to the hypothesis that the true causal effect of (formal) land use regulation on housing construction is smaller than cross-sectional analyses suggest.

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the years of adoption, which, despite being broadly consistent with the years reported in the data used here, are not precise enough to allay concerns regarding the presence of measurement error.

[33] The results are not qualitatively different when the threshold is 1986, 1987, or 1988.

Table 2.4
Coefficient on number of land use regulations enacted (1985-1995)

Dependent Variable	(1)	(2)	(3)
$\frac{\text{Total Residential Permits}_t}{\text{Housing stock}_{t-1}}$	-0.0037**	-0.0013**	-0.0012**
	(0.0004)	(0.0003)	(0.0003)
N	3473	3473	3473
$\frac{\text{Total Residential Permits}_t}{\text{Population}_{t-1}}$	-0.0014**	-0.0004**	-0.0003*
	(0.0002)	(0.0001)	(0.0001)
N	3982	3982	3982
Log(Total Residential Permits)	-0.205**	-0.038**	-0.028**
	[0.02]	(0.01)	(0.01)
N	3868	3868	3868
Log(Multi-Family Permits)	-0.287**	-0.061*	-0.043
	[0.03]	(0.02)	(0.02)
N	2566	2566	2566
Log(Single Family Permits)	-0.108**	-0.049**	-0.041**
	[0.02]	(0.01)	(0.01)
N	3821	3821	3821
City FEs	No	Yes	Yes
Year FEs	No	Yes	Yes
Controls	No	No	Yes

Notes: Cluster-robust standard errors are shown in square brackets. Conley standard errors are in parentheses. The key variable in each of these regressions is a dynamic index that sums the number of land use regulations that have been enacted in each city. Significance at the 5% and 1% level is denoted by * and **, respectively.

2.4.1.1 The Role of Measurement Error: A Simulation

This section of the paper uses a brief simulation to assess the degree to which the smaller magnitude of the panel estimates can be attributed to attenuation bias from measurement error. In order to approximate the amount of measurement error that exists within the kind of regulatory data used in this paper, responses to the Wharton Survey on Residential Land Use Regulation (Gyourko et al. (2008)) were matched to data from the Pioneer Institute for Public Policy Research (PIPPR) (2005), which is used in Glaeser and Ward (2009). While the latter dataset captures localities in Greater Boston, the Wharton survey covers communities throughout the country. Both of these sources contain data on local regulations as of 2004, and there are 48 overlapping cities. While most of the particular policies recorded in the two datasets are different, two of them are similar: minimum lot size requirements and residential permit limits (see Appendix 2.B).[34] To the extent that these questions were perceived and treated the same in both data collection processes, any inconsistencies in the data can be treated as measurement error.

For each of the regulations appearing in both datasets, error rates are computed as the proportion of responses that are inconsistent across the two sources. The error rates for minimum lot size requirements and residential permit limits are, respectively, 7.41% and 6.25%. Given these error rates, the simulation that follows first assumes that 7% of responses are incorrect, and then

[34] It is worth noting that one other variable is similar across the two datasets: the presence of affordable housing mandates. However, in the Wharton survey, these are referred to directly as requirements of "affordable housing (however defined)," while the PIPPR data refer to whether the bylaw includes "any provisions for inclusionary zoning." While these questions are similar in purpose, the difference in wording presents some concerns about whether they capture the same information. The error rate for this question is 29.79%, which, given the consistency of the other two (more comparable) questions, is likely (at least partially) due to the different wording. Nonetheless, the results of the simulation suggest that even if this higher error rate were included, attenuation bias is not still strong enough to explain the smaller panel estimates presented in this paper.

allows this proportion to rise in order to determine whether the smaller panel estimates in Tables 2.3 and 2.4 could be entirely due to measurement error.

For each of the eighteen regulations in Table 2.1, cities are randomly assigned to be adopters or non-adopters such that the proportion of cities adopting each regulation is the same in the simulated data as in the real data. Next, for each regulation, years of adoption (for cities categorized as adopters) are randomly generated such that the mean and standard deviation are the same in the simulated data as in the real data.

Using the "correct" regulatory index (summing the eighteen regulation variables using the years of adoption initially assigned), the dependent variable is generated as follows:

$$Y_{ct} = a + b*(\text{correct index}) + e_{ct} \ ,$$

where a = 10, b = -1, and e ~ N(0,1).

For the 7% of cities assigned to have incorrect years of adoption, the initially assigned adoption dates are adjusted by adding 1 year or subtracting 1 year, each with 20% probability, adding or subtracting 2 years, each with 15% probability, adding or subtracting 3 years, each with 10% probability, and the same for 4 years, each with 5% probability. This method of adjustment leverages the seemingly reasonable assumption that the probability a given year is reported as an adoption-year rises with temporal proximity to the correct date. [35]

[35] Simulations were also carried out where 1, 2, 3, 4, or 5 years were either added or subtracted from the assigned year of adoption, each with 50% probability. Given that the method described in the paper puts 70% weight on 1- and 2-year errors, the simulation results reported here are similar to those from the simulations with 2-year errors. Even when using the 5-year errors, the observed reductions in the panel estimates from Table 2.3 could only be fully attributed to attenuation bias if 50% of the data were misreported.

Table 2.5
Simulation results: Estimated slope coefficient from pooled and fixed effects regressions

Error rate	0.07	0.25	0.50	0.75	0.99
Pooled regression	-1.005	-1.020	-1.039	-1.055	-1.073
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Fixed Effects	-0.958	-0.867	-0.748	-0.649	-0.568
	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Percent attenuation	4.68%	15.00%	28.01%	38.48%	47.06%

Notes: This table reports means and standard deviations for the slope coefficient from 1,000 runs of the simulation described in the paper. The error rate is the proportion of observations with mismeasured years of adoption for each of the eighteen regulations used to create the regulatory index. Disparities in the pooled and fixed effects estimates show that for any given amount of measurement error, panel estimates are smaller in magnitude, relative to cross-sectional ones, and this effect is stronger where there is more error.

Finally, in order to determine how much the slope estimate is attenuated by measurement error in the panel setting, I estimate pooled and two-way fixed effects specifications using the “wrong” regulatory index (i.e., the one with measurement error). Table 2.5 contains the mean and standard deviation of the slope coefficient from 1,000 runs of this simulation. Each column of this table reports the average pooled and fixed effects estimate (and standard deviation) for different error rates. As shown in the first column, if 7% of the data are mismeasured in the way described here, the panel estimate is only reduced by 4.68%. This is markedly smaller than the 40-75% reductions in the panel estimates in Table 2.3. The other columns in Table 2.5 indicate that while more error leads to more attenuation in the panel setting, the smaller panel estimates in Table 2.3 are probably not driven by measurement error alone. Even when there is measurement error in 99% of the data, the simulation suggests that the panel estimate is reduced by less than 50%, which is a smaller reduction than for any of the outcome variables in Table 2.3, except for the log of single family permits. Thus, at least some of the reduction in the panel estimates in

Table 2.3 (and those that follow) is likely due to the elimination of unobserved heterogeneity, which causes bias in cross-sectional estimates.

2.4.2 Effects of Regulatory Stringency – Disaggregated Residential Indices

In order to identify which types of policies are particularly deleterious to new construction, the regulatory measure is decomposed into the categories in Table 2.1. As before, the panel structure of the data allows for the construction of dynamic indices for each of the categories, so identification of the estimated effects comes from within-city and within-year variation. Table 2.6 contains regression estimates for each of the separate indices with the dependent variables measuring growth in multi-family and single family permits separately.

Table 2.6
Results of disaggregated indices regressions

Dependent Variable:	Log(Multi-Family Permits)		Log(Single Family Permits)	
Population controls	0.006	0.032	0.014	0.004
	(0.04)	(0.04)	(0.03)	(0.03)
Zoning controls	-0.099**	-0.099**	-0.049**	-0.022
	(0.04)	(0.04)	(0.02)	(0.02)
Political controls	-0.189	-0.194	0.078	0.057
	(0.15)	(0.14)	(0.08)	(0.08)
General controls	-0.192*	-0.186*	-0.097	-0.074
	(0.08)	(0.08)	(0.06)	(0.06)
City FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	6272	6272	8456	8456

Notes: Conley standard errors are in parentheses. The key variables in each of these regressions are dynamic indices that sum the number of different types of land use regulations that have been enacted in each city. Significance at the 5% and 1% level is denoted by * and **, respectively.

The results in Table 2.6 show that one of the strongest forces in restricting housing construction is local zoning policy. The estimated effect of those zoning ordinances measured in the data is a reduction of about 10% for multi-family development. For single family development, the estimated effect is a reduction of about 2-5%, but when time-varying controls are included, this estimate is not statistically distinguishable from zero. The other important restriction index, *General controls*, represents the number of general or miscellaneous land use restrictions enacted in each city. This index sums indicators for the adoption of a growth management element in the city's general plan and the presence of "other" restrictions (i.e., ones other than those asked about in the two surveys). Although the estimated effects of this index are large and statistically significant for multi-family development, they are difficult to interpret, since the data contain no information as to the nature of the "other" restrictions reported.

2.4.3 Effects of Individual Land Use Regulations

Finally, the partial effects of each land use regulation are estimated.[36] For these regressions, R_{ct} in equations (2-1) and (2-2) is a vector of indicators for the land use policies in place in city *c* in year *t*. For the most part, the expected sign of the coefficient on each of these regulations depends on the particular implementation of each policy. For example, if a city caps the number of building permits issued within a given time frame, does this limit apply to single family units or multi-family units, or both? Unfortunately, specific information of this type was not collected with the regulatory surveys, so it is difficult to say, *a priori*, what sign we should expect to see for the coefficient on each regulation. Thus, the analysis in this section is primarily exploratory.

As shown in Table 2.7, several of the restrictions have a significant negative impact on new construction, while others have a significant positive effect. This important result is obfuscated by the use of more aggregated regulatory measures, like those that dominate the literature. Aggregate measures are generally used (as in this paper) on the grounds that they may be the best proxy available for the overall regulatory environment of a community. However, the fact that development is stimulated by some regulations and stymied by others suggests that null results from the use of aggregate regulatory indices may be due to offsetting effects, rather than the absence of any effect.

[36] The indicator for "Other measure to control rate, intensity, type and distribution of development" is omitted from these regressions, given the ambiguity of this variable. However, the regression coefficients are almost identical when this variable is included as a regressor, so the results remain unchanged.

Table 2.7
Results of individual restrictions regressions

Dependent Variable:	Log(Multi-Family Permits)		Log(Single Family Permits)	
Residential				
Population growth limit	0.283	0.259	0.032	0.021
	(0.17)	(0.17)	(0.11)	(0.11)
Residential permit limit	-0.029	0.067	-0.343**	-0.256**
	(0.15)	(0.15)	(0.09)	(0.09)
Urban growth boundary	-0.057	-0.056	0.305**	0.244**
	(0.12)	(0.12)	(0.06)	(0.05)
Subdivision limit	-0.509**	-0.514**	0.178	0.090
	(0.20)	(0.20)	(0.15)	(0.15)
Floor area ratio restriction	-0.095	-0.052	-0.237**	-0.090*
	(0.08)	(0.08)	(0.04)	(0.04)
Reduction in permitted residential density	-0.380**	-0.432**	0.055	0.007
	(0.09)	(0.09)	(0.06)	(0.06)
Rezoned residential land to open space	-0.175	-0.123	-0.128	-0.102
	(0.13)	(0.13)	(0.11)	(0.11)
Infill development requirement	0.221*	0.259**	0.374**	0.415**
	(0.09)	(0.10)	(0.06)	(0.06)
Adequate public facilities (residential)	-0.016	-0.027	-0.026	-0.040
	(0.11)	(0.11)	(0.07)	(0.07)
Voter approval required to increase density	-0.528**	-0.490*	0.057	0.054
	(0.20)	(0.19)	(0.08)	(0.08)
Supermajority council vote to increase density	1.280**	1.171**	0.622**	0.576**
	(0.34)	(0.33)	(0.22)	(0.22)
Growth management	-0.340*	-0.325*	-0.413**	-0.344**
	(0.15)	(0.15)	(0.08)	(0.08)
Non-Residential				
Adequate public facilities (commercial)	-0.095	-0.066	-0.017	0.000
	(0.11)	(0.12)	(0.05)	(0.06)
Reduce permitted height of comm. buildings	-0.258**	-0.293**	-0.190**	-0.252**
	(0.10)	(0.10)	(0.05)	(0.05)
Rezoned comm. land to less intense use	0.006	0.035	-0.091	-0.061
	(0.12)	(0.12)	(0.06)	(0.06)
Commercial square footage limit	0.485**	0.444*	-0.038	-0.096
	(0.18)	(0.18)	(0.13)	(0.12)
Industrial square footage limit	-0.306	-0.279	-0.185	-0.092
	(0.20)	(0.20)	(0.13)	(0.13)
City FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	5884	5884	7937	7937

Notes: Conley standard errors are in parentheses. The key variables in each of these regressions are indicator variables for the various land use regulations. Significance at the 5% and 1% level is denoted by * and **, respectively.

The key variables in these regressions are indicators for each regulation, and the estimated equations are semi-logarithmic, so the precise interpretation of the coefficients in Table 2.7 is not quite as straightforward as before. As shown by Kennedy (1981) and discussed by Giles (2011), the estimated proportional impact, $\hat{p}_j$, of a dummy variable, D_j, on the dependent variable, Y, is $\hat{p}_j = \left[\exp\left(\hat{c}_j - 0.5\hat{V}(\hat{c}_j)\right)\right] - 1$, where $\hat{c}_j$ is the estimated coefficient on D_j and $\hat{V}(\cdot)$ is the estimated variance. While Table 2.7 contains the estimated coefficients ($\hat{c}_j$), the reported effects that follow are the estimated proportional impacts, calculated as shown above.

Restrictions on the number of residential building permits that can be issued in a given time frame could potentially constrain both single and multi-family development, but the estimates in Table 2.7 suggest that it works entirely through single family housing. The same is true for restrictions on the floor area ratio of buildings. After controlling for the effects of the other land use regulations, the proportional impact of building limits on single family housing construction is between 26-32%. For floor area ratio restrictions, this proportional impact is a reduction between 10-23%.

If binding, a reduction in the permitted residential density of a city is expected to significantly reduce new construction of multi-family housing units, and this expectation is borne out in the data. In terms of the proportional impact, cities that reduced permitted densities experienced about a 36% reduction in the issuance of multi-family housing permits.

Cities that enacted either subdivision limits or requirements mandating voter approval in order to increase residential densities also saw large reductions in the construction of multi-family dwellings. In particular, the estimates and standard errors in Table 2.7 suggest the proportional impact of each of these policies is about a 45% reduction in multi-family permits issued.

Even after controlling for the effects of the other land use restrictions, the adoption of a growth management element in a city's general plan has a large impact on both single and multi-family housing construction (about a 34% reduction in each). This finding is consistent with the idea that additional costs associated with bureaucratic uncertainty in the development process may be substantial enough to significantly reduce or displace new construction (Staley, 2001). Also consistent with this interpretation is Mayer and Somerville's (2000) finding that regulations that add delays to the development process are especially harmful to residential development. Alternatively, this estimated effect might actually reflect the impact of some other land use regulations brought about by the adoption of the growth management element, but not measured in these data. Unfortunately, further exploration into this hypothesis is not possible using these data, since there is no information regarding the content of the growth management elements that were adopted.

The estimates in Table 2.7 suggest that several of the residential regulations measured in the data increase residential construction. For example, the implementation of an urban growth boundary boosts the construction of single family homes. This result supports the conjecture that, if not binding initially, zoning may appear to follow the market immediately after a zoning change (see Thorson (1994)). Urban growth boundaries have been shown to increase the value of new homes (e.g., Cho et al. (2008)), so it is not surprising that development would accelerate until abutting on the established boundary.

Residential development also increases following the adoption of a policy requiring infill development, as well as one that requires a super-majority council vote to increase residential density. While more research is needed to help explain the former correlation, the latter one may suggest that in the face of future political uncertainty, developers hasten to build on developable land in the present.

Interestingly, Table 2.7 also reveals that even after controlling for residential land use regulations, two policies not explicitly intended to affect residential development do just that. Cities that restricted the amount of commercial square footage that could be built in a given time frame saw multi-family permits rise considerably following the implementation of this constraint. In addition, those communities that adopted a policy reducing the permitted height of commercial or office buildings experienced a decrease in the construction of both single and multi-family housing. If such a policy drives out commercial employment, it should not be surprising that cities that enact these policies see less residential construction. Nevertheless, the link between non-residential land use regulations and residential development should be explored in future work.

2.5 Conclusion and Discussion

This study has shown that land use restrictions significantly reduce cities' housing supply and new construction, but to a lesser degree than suggested by cross-sectional analyses. While the smaller—panel—estimates may be, in part, due to attenuation bias, results from a simple simulation suggest that the reduction is probably not entirely attributable to measurement error. Instead, there is reason to believe that cross-sectional analyses overestimate the effects of formal regulation by not accounting for unobserved factors, such as negative attitudes about growth. The panel estimates suggest that each additional land use regulation reduces multi-family and single family permits by an average of more than 6% and 3%, respectively. Consequently, housing supply is reduced by an average of 0.2% per year, or more than 0.7 units per 1,000 residents.

The data reveal that those regulations defined as zoning controls and general controls are the strongest deterrents to development, resulting in substantial reductions primarily in multi-family construction. In contrast, the regulations classified as population controls do not have a significant impact on development. This result is surprising, since these policies appear to be explicit restrictions on growth. While this finding may suggest that these constraints were either not binding, or not enforced, disaggregating the index into its component parts shows offsetting effects from some of the individual policies. In particular, for single family housing, the positive effect of urban growth boundaries almost exactly offsets the effect of residential permit limitations.

Although Pollakowski and Wachter (1990) suggest that "land use constraints collectively have larger effects than individually," this study has shown that individual constraints can have rather sizable effects. Moreover, since some of the regulations curtail development, while others boost it, aggregate measures of land use regulation potentially mask important elements of the relationship between land use regulation and residential development.

The results of this paper are consistent with the idea that bureaucratic and political uncertainties are important factors in residential development. The effect of bureaucratic uncertainty may contribute to the large reductions in residential building permit issuance following the incorporation of a growth management element into a city's general plan. Political uncertainty is represented by the adoption of a policy requiring a supermajority council vote in order to increase residential density, which has huge effects on development.

This paper's findings provide grounds for future work exploring the relationship between these various land use regulations and residential development. For example, future work could investigate the link between commercial and industrial land use regulation and residential development.

Chapter 3

A New Regulatory Index for California Cities and Counties With Application to the Boom and Bust

3.1 Introduction

Understanding local housing market conditions and their determinants should be a first-order concern of researchers and policymakers for a variety of reasons. First, as recent experience attests, volatility in housing markets has serious repercussions for macroeconomic stability. Leamer (2007) provides evidence that, as the title of his paper suggests, "Housing *is* the business cycle." In addition, given that the most valuable asset owned by most households is their home, large swings in housing prices can seriously impact microeconomic decision-making, from consumption behavior (e.g., Campbell and Cocco 2007) to charitable giving (Do and Paley 2011) to mobility and labor market outcomes (Ferreira et al. 2010; Black et al. 1996; Charles et al. 2013). Finally, housing affordability is a serious issue in some communities across the United States, as skyrocketing housing prices make homeownership unattainable for many individuals and families. An important determinant of housing prices, and local housing market conditions, more generally, is the way in which communities' regulate land use.

The localized nature of most land use regulation means that, in general, little is known about the kinds of policies and procedures local governments have adopted. Without a central source of information about the regulatory environment of localities, researchers, policymakers, developers, and other interested parties are dependent on occasional surveys and other laborious data collection processes in order to understand local regulatory regimes. In an effort to obtain more complete and current information than previously existed regarding the stringency of local regulatory environments in California, I administered a statewide online survey regarding the land use policies and practices in each locality. California is known for its uniquely extensive utilization of land use regulation and the remarkable autonomy with which its communities manage local land use (e.g., Fischel 1995, Chapter 6), making an extensive database of local regulatory regimes all the more valuable.[37]

The California Land Use Survey was sent to top land use officials in nearly every city and county in California.[38] The questions extract information about five distinct aspects of local land use regulation. First, there are questions about each community's zoning code. For example, respondents were asked whether mobile or manufactured homes are allowed, and whether there are density or building-height restrictions. Next, there is a series of questions that explore the permitting process. This series includes questions about how many regulatory bodies are involved in the approval process, how often they meet, and the respondents' perception of how long it would take for approval on a relatively straightforward project. The third category of survey questions deals with affordable housing mandates, which are quite common in California. The next category of questions asks about explicit growth controls, such as limitations on the

[37] Malpezzi et al. (1998) finds evidence in favor of what he terms the "California is different" hypothesis.
[38] The only jurisdiction not solicited for this survey was the small town of Isleton, where contact information for a public official was unavailable.

number of permits issued within a given time frame, urban growth boundaries, and population growth caps. Finally, information regarding several other constraints on development is captured. These additional constraints include physical (i.e., natural) barriers to development, as well as local opposition to growth.

As indicated above, the survey contains questions about the specific policies that have been formally adopted, in addition to some that elicit respondents' opinions regarding several factors relevant to residential and other development, like their perceptions of approval delays and uncertainty in the permitting process. The combination of both types of questions is an important improvement over many previous surveys, since different communities may interpret and/or enforce the same set of regulations very differently. Additionally, the capricious and parochial nature of informal, ad hoc forms of regulation may be more burdensome than the policies that are actually on the books.[39]

Following Quigley et al. (2008) and Gyourko et al. (2008), responses to the California Land Use Survey are collapsed into several sub-indices, which are then aggregated to compose the California Land Use Regulatory Index (CaLURI). This succinct measure of land use regulation provides a clear ranking of California communities with regard to the stringency of their regulatory environments. Jurisdictions in the San Francisco Bay area tend to score highest on the CaLURI, indicating that they are some of the most stringently regulated. Despite this pattern, there is a significant amount of spatial heterogeneity in local regulatory regimes across the state. Moreover, regulation does not uniformly increase with proximity to the coast; the two least

[39] Landis (1992) discusses various ways in which local governments can use informal regulation to retard growth; for example, they may refuse to annex vacant land or extend utility services to developing areas.

regulated localities in the data are both coastal cities (Long Beach and Imperial Beach), and some inland communities are among the most regulated.

The method of this paper builds off of that used by previous researchers, who have conducted similar surveys on the incidence of growth controls and other land use regulations.[40] These antecedent surveys have occurred at different points in time and for various geographical regions. Pendall et al. (2006) and Gyourko et al. (2008) administered surveys in communities across the country in 2003 and 2005, respectively.[41] The latter of these two studies yielded the Wharton Residential Land Use Regulatory Index (WRLURI), which was constructed by combining survey data with state-level data on the activity level of the judicial, legislative, and executive branches with regard to land use regulation.

After constructing the CaLURI in a fashion similar to the WRLURI, this paper examines the connection between the stringency of land use regulation and housing prices. A regression of housing prices on the regulatory index and a number of controls indicates a strong positive relationship between land use regulation and the level of housing prices in various stages of the recent housing market cycle. However, despite this correlation with the level of prices, first-difference regressions suggest that land use regulation was not a significant driver in the boom or the bust experienced by California cities. Some previous research suggests that regulation, as a proxy for supply inelasticities, fuels housing market bubbles following shifts in demand. However, this hypothesis is not consistent with the manner in which the recent housing market boom and bust played out in California, where more-regulated cities were largely sheltered from

[40] Indeed, many questions on the present survey are based on some of those used by previous researchers, such as Levine et al. (1996), Lewis and Neiman (2000), Gyourko et al. (2008), and the U.S. Department of Housing and Urban Development's National Land-Use Regulations Survey (Pendall and Rosenthal 2008).
[41] The U.S. Department of Housing and Urban Development (2008) also carried out a national survey of land use regulation, but only in five U.S. cities: New Brunswick, Portland, Atlanta, Minneapolis-St. Paul, and Boston.

the substantial price swings experienced elsewhere. Instead, much of the spatial variation in housing price dynamics can be explained by geographic constraints on development, as quantified using a novel city-level measure, together with relevant demand factors, such as the prevalence of subprime lending and changes in the local unemployment rate.

The next section of this paper describes the administration of the California Land Use Survey. Section 3.3 describes the California Land Use Regulatory Index (CaLURI) and sub-indices of which it is comprised. Section 3.4 introduces a simple model relating land use regulation to housing prices. Additional data, not coming from the California Land Use Survey, are discussed in Section 3.5. Section 3.6 explores the empirical link between regulation and the level of housing prices, while Section 3.7 looks at the role of regulation in the recent housing price cycle in California cities. Section 3.8 concludes.

3.2 The California Land Use Survey

The California Land Use Survey is the most recent, but not the first, California-specific survey of land use regulation. Previous questionnaires were administered in 1989 (Glickfeld and Levine 1992), 1992 (Levine et al. 1996), 1998 (CA Dept. of Housing and Community Development 2000), and 1998-1999 (Lewis and Neiman 2000). In addition, Quigley et al. (2008) carried out an extensive survey of land use policy and procedures for the greater San Francisco area, which yielded the Berkeley Land Use Regulatory Index (BLURI). While the BLURI provides valuable information about the regulatory environment in that region of California, the present study explores broader regional differences within the state.

As with many of the previous surveys, the California Land Use Survey was administered to top land use officials in cities and counties across the state. Three such individuals from communities in the greater Los Angeles area pretested the survey, prior to it being launched online. The preliminary mailing list for the survey was obtained from the California Governor's Office of Planning and Research (OPR). This list, which required a significant number of updates, contains the contact information for individuals in each city and county whom OPR solicits for response to its occasional surveys. The survey was sent primarily to Directors of Community Development, Planning Directors, and Planning Managers, where these positions exist. For small communities, solicited individuals were often contract planners or those involved in city administration. To improve the response rate among county governments, the California State Association of Counties was recruited to e-mail county officials a week before the survey was launched to encourage their participation.

After following up with solicited individuals several times via e-mail and telephone, usable responses were elicited for 420 of the 540 cities and counties in California, yielding an overall response rate of 77.9%. The responding localities contain 91.9% of the California population, according to 2010 Census data. Responses were obtained from local land use officials in 75.9% of the cities that were incorporated by 2013 (366 out of 482) and 94.8% of California counties (55 out of 58). In California, the jurisdiction of county governments is restricted to unincorporated parts of their county, so cities and counties are distinct and independent political units, and they are treated as such in the data. Communities of all sizes are reasonably well represented among survey respondents. Table 3.1 displays survey response rates by population size for the sample of responding communities. Even in the least represented categories (containing the smallest of jurisdictions), responses were elicited from almost 60% of localities.

Given that these data are applied to a model of housing prices below, it is important to note that local housing price growth is not significantly related to whether or not a community responded to the survey.

Table 3.1
Representation of localities of different sizes

2010 Population	% Response rate (Total)
Less than 5,000	59.3
5-10,000	59.3
10-20,000	63.2
20-50,000	83.7
50-100,00	86.4
100-250,000	95.7
250-500,000	95.7
More than 500,000	100
No. observations	420

The vast majority of respondents are in top positions in local Planning and Community Development Departments (see Appendix 3.A). Consequently, they are likely the most knowledgeable in their community regarding local land use policies and practices, and thus the most able to provide accurate responses to the survey's questions. Given the coverage of this survey and the plausible reliability of responses, these data provide valuable information regarding the current state of local land use regulation in California.

3.3 Regulatory and Geographic Constraints

3.3.1 California Land Use Regulatory Index and Sub-indices

Following Malpezzi (1996), Levine (1999), Mayer and Sommerville (2000), Ihlanfeldt (2007), Gyourko et al. (2008), Quigley et al. (2008), Glaeser and Ward (2009), and others, data from the California Land Use Survey are combined into an index that reflects the overall stringency of the regulatory environment in each jurisdiction. Similar to the WRLURI, the aggregate index in this study is comprised of several sub-indices, each representing the prevalence of different forms of land use regulation. For each sub-index and for the aggregate index, larger values indicate more restrictiveness. For the equations in this section, $\mathrm{STD}\{\cdot\}$ indicates standardization, so the given variable has a mean of zero and standard deviation of one. The data used to create the sub-indices consist of several ordinal categorical variables, so standardization does not remove meaningful information regarding the empirical magnitudes of the underlying data. By expressing variables as standardized deviations from the mean, they can be combined in a meaningful way to create the sub-indices and, ultimately, the aggregate index. For simplicity, variables that are measured on the same scale are combined prior to standardization as shown below.[42]

The sub-indices are as follows: Low-Cost Alternative Index, Residential Structure Requirement Index, General Residential Zoning Index, Political Tension Index, Development Uncertainty Index, Regulatory Delay Index, Building Limitations Index, Non-Residential Building Limitations Index, and Affordable Housing Index. For brevity, only the Residential Structure

[42] If, instead, each variable is standardized before being summed, the resulting sub-indices and aggregate index are very highly correlated with those used in this paper (Pearson's correlation coefficient is at least 0.90 for each index). Moreover, the estimates produced in this paper are nearly identical using either method.

Requirement Index, Building Limitations Index, Development Uncertainty Index, and Regulatory Delay Index are discussed below, while a discussion of the others is contained in Appendix 3.B. A description of each variable is found in Appendix 3.C.

Residential Structure Requirement Index (RSRI). The RSRI measures the extent to which the height, size, and form of residential structures are regulated. The components of this index are indicators for height limitations for single-family units, requirements that single-family units have garages, minimum square footage requirements for single-family units, and floor area ratio restrictions. Using the variable definitions in Appendix 3.B, this index is computed as follows:

$$\text{RSRI} = \text{STD}\{\mathit{maxhtD} + \mathit{garage\_reqD} + \mathit{min\_sqftD} + \mathit{farD}\}.$$

Building Limitations Index (BLI). The BLI quantifies the presence of regulations that, if binding, directly restrict the supply of residential housing units (i.e., growth controls). This index is the standardized sum of indicators for building permit caps for all residential units, permit caps for multi-family dwellings, population growth limitations, and urban growth boundaries. Thus, the index is computed as follows:

$$\text{BLI} = \text{STD}\{\mathit{bldglimitD} + \mathit{mflimitD} + \mathit{poplimitD} + \mathit{ugbD}\}.$$

Development Uncertainty Index (DUI). The DUI measures the amount of uncertainty in the development approval process. Similar to the so-called tragedy of the anticommons (Heller 1998), uncertainty in land development is assumed to be an increasing function of the number of individuals or organizations with veto-power. Thus, the DUI quantifies the extent to which land use decisions rely upon coordination between multiple groups of individuals. In particular, the index contains indicators for whether zoning changes require voter approval and whether they require a supermajority council vote. It also includes an ordinal variable representing the number of boards or regulatory bodies that must grant permission before a typical single-family development is approved. This index is computed as follows:

$$\text{DUI} = \text{STD}\{\text{STD}(voter\_appD + supermaj\_appD) + \text{STD}(num\_boards)\}.$$

Regulatory Delay Index (RDI). The RDI measures the time delay in the permit application process. While many of the other sub-indices measure formal policies intended to restrict or manage growth, delays in the development process may be even more harmful to residential development than explicit growth control policies (see Mayer and Somerville 2000). Included in the RDI is the frequency with which each community's permit granting entity meets, as well as the typical time delay in approving relatively straightforward developments (i.e., where no rezoning, zoning amendments, bulk variance, etc. is required), averaged over single-family, multifamily, and townhouse developments. Additionally, the index contains two ordinal variables for the perceived importance of the review process and lack of personnel to review projects in constraining residential growth. The RDI is computed as follows:

$$\text{RDI} = \text{STD}\{\text{STD}(freq\_permit\_mtg) + \text{STD}[(sf\_time + mf\_time + town\_time)/3] + \text{STD}(imp\_review\_proc + imp\_staff)\}.$$

California Land Use Regulatory Index (CaLURI). The aggregate California land use regulatory index (CaLURI) is computed as the standardized sum of the three sub-indices described above and the six others described in Appendix 3.B.[43] Table 3.2 shows pairwise

[43] An alternative method of data reduction, used by Malpezzi (1996), Gyourko et al. (2008), and others, is to create a weighted sum where the weights come from a factor analysis of the nine sub-indices. When carried out upon these data, the resulting factor scores are quite highly correlated with the standardized sum of sub-indices (r = 0.75 for the

correlation coefficients for the aggregate index, as well as each of the sub-indices. The vast majority of these pairwise correlations are positive, indicating that communities that are heavy-handed in one aspect of land use regulation are likely to be so in other aspects as well. As expected, the Affordable Housing Index (AHI) and the Low Cost Alternative Index (LCAI) are negatively related, which suggests that localities with affordable housing mandates tend to be more permitting of less-expensive housing options.

Table 3.2
Pairwise correlations with aggregate land use regulatory indices and sub-indices

	CaLURI	LCAI	RSRI	GRZI	PTI	DUI	RDI	BLI	NBLI
LCAI	0.27								
RSRI	0.35	0.24							
GRZI	0.47	-0.03	0.04						
PTI	0.52	-0.07	0.01	0.20					
DUI	0.49	0.04	0.10	0.13	0.20				
RDI	0.41	0.02	0.02	0.07	0.31	0.03			
BLI	0.53	-0.07	-0.01	0.33	0.19	0.18	0.07		
NBLI	0.38	0.08	0.05	0.06	-0.01	0.08	-0.02	0.09	
AHI	0.39	-0.17	-0.10	0.02	0.17	0.12	0.11	0.23	0.12

Notes: The California Land Use Regulatory Index is computed as the standardized sum of the nine sub-indices. See the text and Appendix B for descriptions of each sub-index.

first principal factor), so the more straightforward sum is used to create the CaLURI. When the first principal factor replaces the standardized sum, the results are not qualitatively different from those reported here.

3.3.2 Differences in Regulation Across Regions of California

California is generally regarded as one of the most highly regulated states in the union, but the CaLURI indicates a fair amount of intrastate variability in the stringency with which communities manage local land use. Figure 3.1 illustrates how regulation varies spatially across the state of California. The colors in this choropleth map (and those that follow) indicate quartiles from the relevant distributions, with darker shades indicating more stringent land use regulation, with the relevant values averaged across all localities within each county.[44] Figures 3.1 and 3.2 illustrate data from all communities for which the relevant index can be computed from complete survey responses or imputed given partial responses.[45] From Figure 3.1, it is clear that the most highly regulated communities lie in the San Francisco Bay area. The next most regulated region appears to be the southern coast and other coastal areas, followed by the Central Valley. While Figure 3.1 shows some general patterns, there is a remarkable amount of regulatory variation even within the same region of the state. The finding that a few inland counties have some of the most draconian land use regulation contradicts the idea that regulation is the result of the geographic constraints and high amenity values created by the Pacific Coast. In fact, while the average coastal city scores slightly higher on the CaLURI than does the average non-coastal city, this difference is not statistically significant.

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[44] Readers familiar with California's geography may wonder about the island depicted off the coast of San Francisco. While not drawn to scale, this island represents the Farallon Islands, which are officially part of the City and County of San Francisco.

[45] Missing values are imputed for observations that are missing less than four of the sub-indices. These imputations are estimated at the sub-index level, using ordinary least squares to predict each missing value from non-missing sub-indices. It is important to note that the single imputation method used here is only to provide stylized facts about regional patterns of land use regulation across the state of California. These imputed values are not used in this paper's application of these data to housing prices and the housing price cycle of the 2000s.

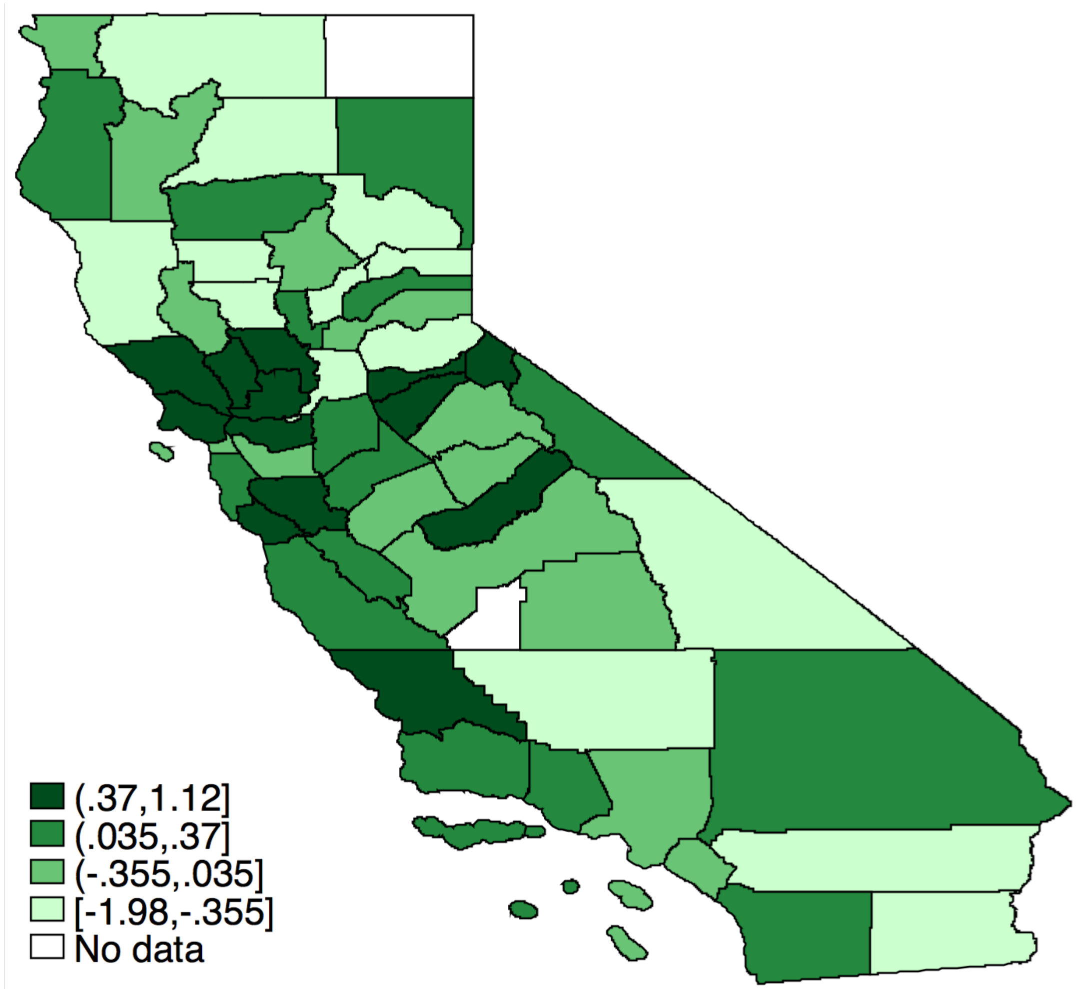


Figure 3.1: Average California Land Use Regulatory Index (CaLURI) value by county

The data show evidence of spatial correlation in the adoption of land use regulation across jurisdictions. A one standard deviation increase in a community's CaLURI score is associated with an average 0.14 standard deviation increase in that of the locality's nearest neighbor. This estimate is significant at the 1% level.[46] Moreover, communities' scores on the LCAI, RSRI, DUI, BLI, and AHI are significantly related to those of neighboring localities.

[46] Given the interdependencies in the regulatory regimes of nearby locales (Brueckner 1998), this estimate is not used to make a causal claim, but only to explore correlations in the adoption of land use regulation.

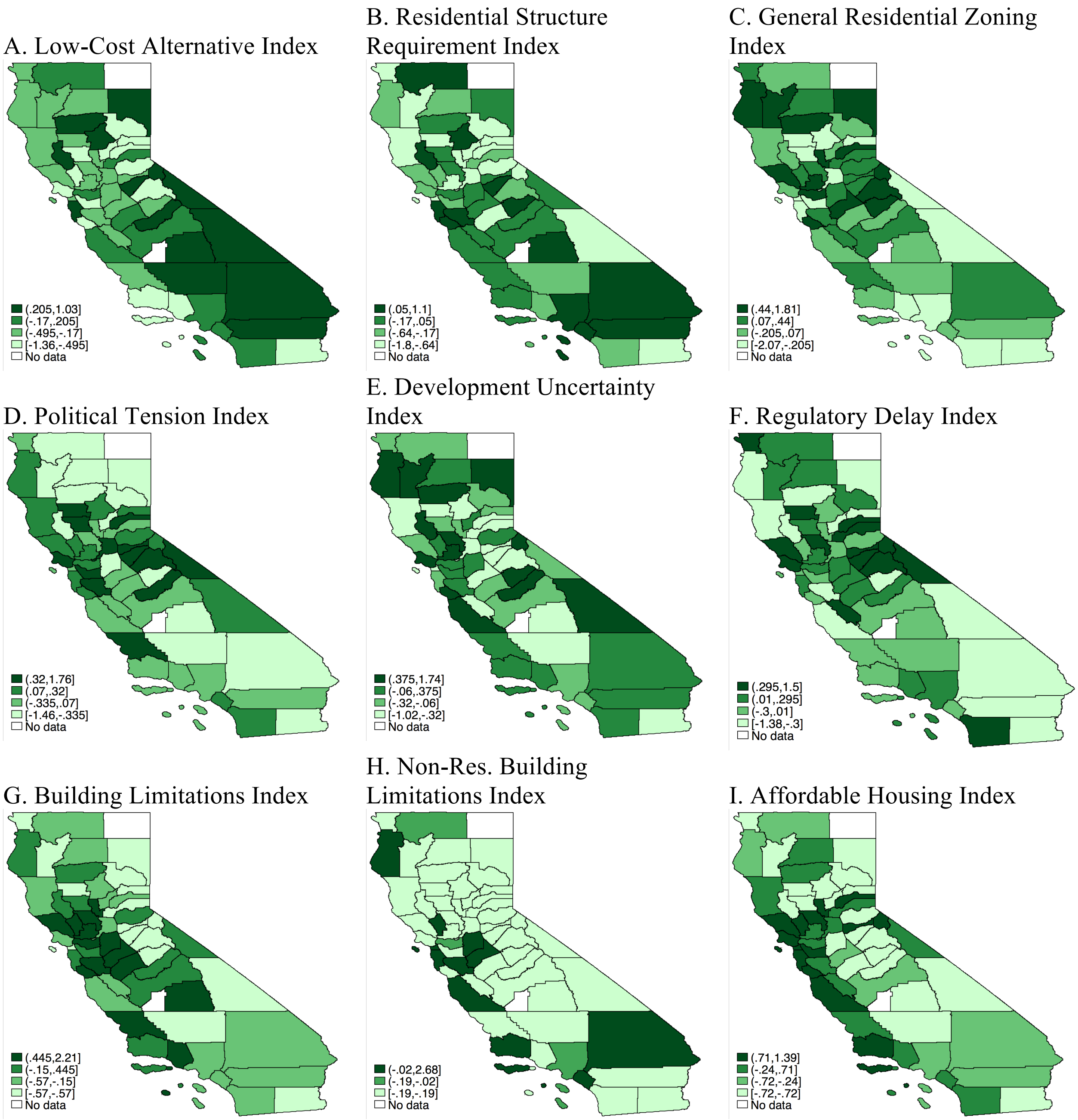


Figure 3.2: Average sub-index values by county

Figure 3.2 contains choropleth maps similar to those in Figure 3.1, but for each sub-index. The clearest regional patterns displayed in these graphics are with regard to the LCAI and the AHI (Panels A and I of Figure 3.2, respectively). These two indices are, as already mentioned, negatively related, with inland counties regulating the availability of mobile and manufactured homes much more frequently and mandating affordable housing less often than coastal counties (where housing is generally much more expensive). Additionally, Panels B and G of Figure 3.2 show that localities in Southern California (on the coast and inland) tend to restrict the *form* of new residential dwellings, while those in the San Francisco Bay area prefer to limit their construction altogether.

3.3.3 Geographic Constraint Index (GCI)

In addition to providing information regarding the regulatory environment of cities and counties across the state, the California Land Use Survey yields a measure of the extent to which geographic constraints inhibit growth in these communities. This estimate of local geographic constraints (i.e., the GCI) is derived from responses to the following question on the land use survey (five-point Likert scale):

"Please rate each of the following factors in terms of their importance in constraining or slowing residential growth in your jurisdiction: Supply of land".

Clearly this is not a very precise measure of local geographic constraints, but its value comes primarily from it being measured at the same level at which most land use regulation is enacted: the city level. Thus, the GCI can be directly employed in studies of the effects of land use regulation. On the contrary, Saiz (2010) measures geographic constraints by using satellite-generated topographical data to estimate the amount of developable land for 95 U.S. metropolitan areas (MSAs), including eleven in California.

While the Saiz measure is more precise than the GCI, it has faced some criticism.[47] Additionally, since many land use regulations are implemented at the city level and geographic constraints can vary across jurisdictions within the same MSA, a more disaggregated measure of geographic restrictiveness may be more appropriate. For example, in the Los Angeles–Long Beach MSA, the Saiz measure of geographic constraint assigns the same score to coastal communities such as Santa Monica and Palos Verdes Estates as it does to communities that face much weaker geographic constraints, such as the inland communities of Lancaster and Azusa. Using the GCI, the former two cities receive the highest score, 5, while the latter two receive the lowest score, 1.

To the extent that survey respondents objectively identified the importance of the availability of land in constraining growth, the GCI should correlate strongly with the measure put forth by Saiz. Indeed, for the California MSAs included in the Saiz study, the correlation between the average GCI for communities within each metropolitan area and the amount of developable land (as estimated by Saiz) is 0.80. This strong relationship lends credibility to the GCI as an adequate city-level measure of geographic constraint.

Figure 3.3 shows the average GCI value by county. As expected, coastal counties and those bordering the Sierra Nevada mountain range tend to score highest on this index.

[47] Cox (2011) points out that by only analyzing geographic constraints in a 50 kilometer radius around the urban center, the Saiz measure fails to account for vastly different geographical sizes of metropolitan areas. Moreover, Ihlanfeldt and Mayock (2014) advise researchers to exercise caution in using the Saiz measure, after finding it to not explain new construction in Florida counties or show a significant relation to their measure of housing supply elasticity.

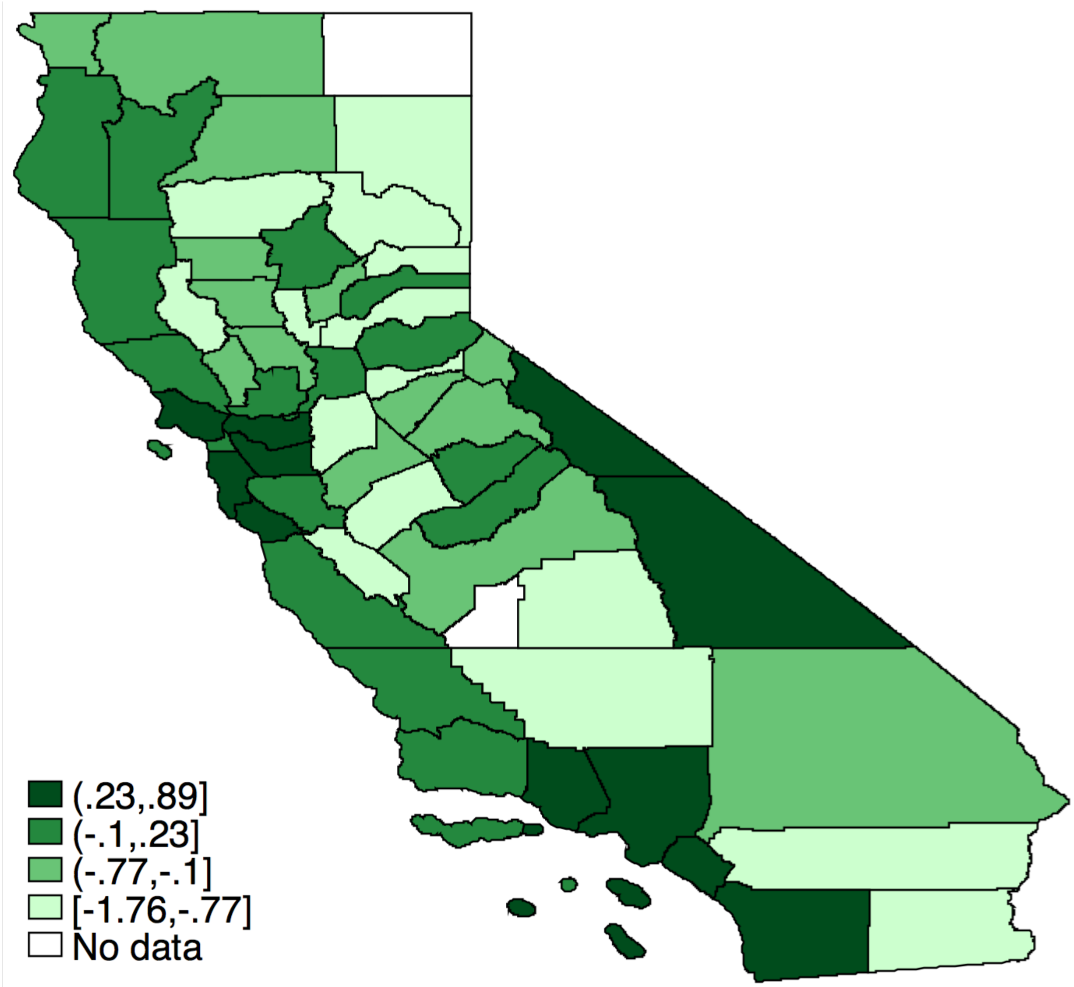


Figure 3.3: Average Geographic Constraint Index value by county

3.4 A Model of Regulation and Housing Prices

A large body of papers shows that land use regulation and other supply constraints exhibit a strong positive correlation with the level of housing prices.[48] Moreover, by potentially making

[48] This literature relating regulation to housing prices is summarized well by Quigley and Rosenthal (2005). See also Glaeser and Gyourko (2003), Ihlanfeldt and Shaughnessy (2004), Glaeser et al. (2005), Mostafa et al. (2006), Hui et al. (2006), Ihlanfeldt (2007), Chakraborty et al. (2010), Zabel and Dalton (2011), Caldera and Johansson

the supply of housing more inelastic, these constraints can also affect the rate at which prices rise (or fall) for a given demand shock (e.g., Malpezzi et al. 1998; Mayer and Somerville 2000; Green et al. 2005; Saiz 2010; Paciorek 2013; Ihlanfeldt and Mayock 2014). In this section of the paper, a model is developed to test for both the level effect and the elasticity effect of land use regulation and other supply constraints (i.e., geographic constraints on development) on California housing prices during the recent housing market boom and bust in that state. In particular, this paper focuses attention on three points in time: January 2000, April 2006, and January 2012. There was some local variation in the exact timing of the boom and bust, but housing prices in California as a whole rose rapidly and steadily from the beginning of 2000 to April 2006, when they began to plummet until January 2012 (see Figure 4).

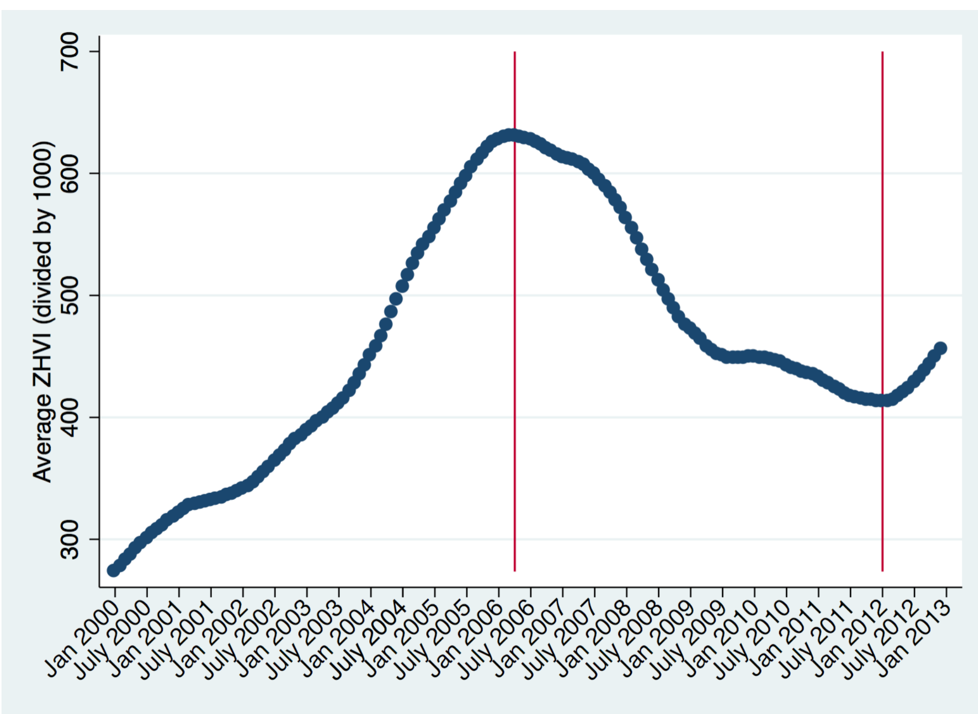


Figure 3.4: Average California housing price over time

(2013), and others. Saiz (2010) is the seminal work on geographic supply constraints. See also Ihlanfeldt and Mayock (2014).

In the stylized model below, supply is expressed as a function of price (P_{it}), a time-invariant measure of the stringency of land use regulation (Reg_i), and the extent to which fixed geographic constraints inhibit development (Geo_i). In particular, regulation and geographic constraints are allowed to affect both the level of local housing prices and the elasticity of housing supply. Demand is a function of price and city-level demographic, employment, and housing characteristics (X_{it}). A key factor in housing demand during the housing cycle of the 2000s was the availability of credit for high-risk borrowers. Thus, the model also includes, as a determinant of demand, the potential prevalence of subprime borrowing (Sub_i), measured prior to the loosening of mortgage approval standards. This variable can be thought of as pent-up demand for housing among low credit-quality borrowers and will be approximated using a measure of the proportion of each city's population that would likely take advantage of subprime lending practices.

The effect of Sub_i is allowed to vary over time, permitting risky lending practices to have different impacts during the boom (when demand was rising) and the bust (when demand was falling). The time-varying effect of Sub_i on housing demand reflects the fact that, during the boom years, pent-up demand for housing was released over time as lower-quality borrowers gained access to mortgages through the expansion of subprime lending. Similarly, during the bust, housing demand tumbled in high-subprime areas as many subprime borrowers defaulted on their mortgages, leading to rampant foreclosures.

The housing supply and demand equations are expressed as follows:

$$Q_{it}^{S} = \upsilon_t P_{it}^{\pi+\gamma Reg_i+\theta Geo_i} e^{\lambda Reg_i+\tau Geo_i}$$

$Q_{it}^{D} = \omega_t P_{it}^{-\eta} e^{\phi X_{it} + \rho_t Sub_i}$,

where i indexes cities and t indexes time (i.e., January 2000, April 2006, or January 2012). Thus, Q_{it}^{S} and Q_{it}^{D} indicate, respectively, housing supply and housing demand in city i in time period t. Although the regulatory data includes both cities and counties, many of the other variables are only available for cities, so this analysis and that which follows focuses solely on California cities. Both supply and demand are allowed to have time-varying intercepts, captured by υ_t and ω_t, respectively. As described above, the effect of Sub_i on housing demand is time-dependent, and this dependence is captured by the coefficient on that variable, ρ_t. The model isolates the demand-side effects of subprime borrowing by using cities' potential proclivity toward these types of loans, prior to the widespread adoption of risky lending practices, rather than the actual issuance of subprime loans, which may reflect both supply and demand conditions.

Note that both regulation and geographic constraints enter the supply equation in two ways: as exponents on both e and P_{it}. The exponents on P_{it} are price elasticities, and, in logs, those on e capture the level effects of Reg_i and Geo_i on supply. By characterizing the stringency of land use regulation as time-invariant, this model assumes that regulatory regimes are fixed over time. This assumption does not require that particular policies are time-invariant (as they certainly are not), but only that the general regulatory milieu in each locality is stable. This assumption is revisited later when the model is applied to the data.

Equating supply and demand, taking logarithms, and solving for P_{it} yields

(3-1) $$logP_{it} = \frac{\mu_t + \phi X_{it} + \rho_t Sub_i - \lambda Reg_i - \tau Geo_i}{\delta + \gamma Reg_i + \theta Geo_i},$$

where $\mu_t = log(\omega_t) - log(\upsilon_t)$ and $\delta = \pi + \eta$.

Note that, because of the ratio form of (3-1), the effects of Reg_i and Geo_i on housing prices are interactive, with the effect of Reg_i depending on the levels of X_{it}, Sub_i, and Geo_i. Similarly, the effect of Geo_i on housing prices depends on the levels of X_{it}, Sub_i, and Reg_i. In a linear specification, these interdependencies can be approximated by using interaction terms. The model in equation (3-1) is, therefore, approximated using the following second-order Taylor series expansion:

(3-2)
$$logP_{it} = \alpha_{0t} + \alpha_1 Reg_i + \alpha_2 Geo_i + \alpha_3 X_{it} + \alpha_{4t} Sub_i + \alpha_5 Geo_i Reg_i + \alpha_6 X_{it} Reg_i$$
$$+ \alpha_7 X_{it} Geo_i + \alpha_8 Sub_i Reg_i + \alpha_{9t} Sub_i Geo_i + \alpha_{10}(Reg_i)^2 + \alpha_{11}(Geo_i)^2 + \varepsilon_{it} .$$

Assuming the model is correctly specified, we can test whether regulation affects the level of housing prices in California cities using equation (3-2). Moreover, using the interaction terms, we can test whether there are elasticity effects from Reg_i or Geo_i. This proposition follows from the fact that if there are no elasticity effects from Reg_i or Geo_i (i.e., if, in the model above, $\gamma = \theta = 0$), the denominator in equation (3-1) is a constant, so that there is no need for interactions in the linear specification. A more direct and intuitive way of testing for elasticities is to test whether these supply constraints affect the changes in prices, following a shock in housing demand. Taking first differences in equation (3-2) yields a model that will allow for such a test:

(3-3)
$$\Delta logP_{it} = \beta_{0t} + \beta_1 \Delta X_{it} + \beta_{2t} Sub_i + \beta_3 \Delta X_{it} Reg_i + \beta_4 \Delta X_{it} Geo_i + \beta_{5t} Sub_i Reg_i$$
$$+ \beta_{6t} Sub_i Geo_i + u_{it} ,$$

where the following relationships hold:

$\beta_{0t} = \alpha_{0t} - \alpha_{0t-1}$

$\beta_1 = \alpha_3$

$\beta_{2t} = \alpha_{4t} - \alpha_{4t-1}$

$\beta_3 = \alpha_6$

$\beta_4 = \alpha_7$

$\beta_{5t} = \alpha_{8t} - \alpha_{8t-1}$

$\beta_{6t} = \alpha_{9t} - \alpha_{9t-1}$.

First differencing removes time-invariant factors from the model, except for those that are interacted with factors that change over time, or have time-dependent effects. In particular, note that because regulation is assumed to be time-invariant, Reg_i enters equation (3-3) only through interactions. The same is true for Geo_i. While Sub_i is also time-invariant, it represents a shift in demand and is included in the first-differenced model because its effect varies with time. Price changes should not be affected by the presence of low-quality borrowers (as quantified through Sub_i), per se. Rather, as risky lending practices became more common, during the boom, these previously unqualified borrowers gained access to the credit necessary to compete in the housing market and, thereby, bid up housing prices. After the housing bubble burst, this pattern was reversed, as many subprime mortgages fell into default, sending home prices plummeting.

The interaction terms in equation (3-3) connect shifts in housing demand (stemming from the expansion of subprime lending and the factors in X_{it}) to local regulatory and geographic constraints. The coefficients on these interactions allow us to test whether regulatory or geographic constraints are significant determinants of California housing supply elasticities. Using the CaLURI, the GCI, and the data described below, this paper tests for both the level and elasticity effects of regulation.

3.5 Data

3.5.1 Home Price Data

Housing prices are measured using the Zillow Home Value Index (ZHVI) produced by Zillow.com. Mian and Sufi (2009) and Huang and Tang (2012) also use this measure of housing prices. The ZHVI is a hedonic price index, so home values are estimated given the relative contribution of various home attributes in the sale-price of similar homes in the area. There are several advantages to using a hedonic price index, rather than median sales price or repeat sales indices (e.g., the Case-Shiller Home Price Indices and the Federal Housing Finance Agency's House Price Index).[49] First, home values are estimated for all homes in a given region, which eliminates the need to assume the homes that are sold are representative of the entire population. This fact also means that, even though hedonic price estimates will be more accurate where there are more home sales, index values can still be produced where relatively few transactions have occurred.[50] Second, hedonic price estimates are tied to current tastes for local amenities and the characteristics of each home and neighborhood. For example, at different points in time, the same neighborhood may be more attractive simply due to changes in preferences. Repeat sales indices rely on the assumption that all of these factors remain constant between sales in order to attribute observed differences in sale prices to changes in the price level. Third, the mix of homes that are sold in a region exerts much less influence on indices from hedonic price estimates. If a greater proportion of smaller (larger) homes are currently being sold, repeat sales

[49] Despite the methodological differences, the ZHVI is generally consistent with the Case-Shiller Home Price Index. Using a sample of 2,248 ZIP codes, Mian and Sufi (2009) report that house price changes for the Case-Shiller Home Price Index and Zillow's index have a correlation coefficient of 0.91.

[50] The Federal Housing Finance Agency requires 1,000 total transactions in an area before data are published.

indices and median home prices will indicate that home prices have fallen (risen), when this may not actually be the case.[51]

3.5.2 Quantifying the Demand for Subprime Borrowing

As already discussed, the lax mortgage approval standards that were common throughout the first several years of the 21st century provided a strong impetus for heightened housing demand in those years (e.g., Mayer and Sinai 2009; Sinai 2013). Thus, any analysis of the housing market during the boom or bust must account for the change in demand coming from the local prevalence of subprime borrowing. This paper follows Mian and Sufi (2009) and Huang and Tang (2012) in measuring local dependence on subprime mortgages using each city's 1996 mortgage application rejection rate from the Loan Application Registrar (LAR) of the Home Mortgage Disclosure Act (HMDA).[52] This proxy represents the presence of low-quality borrowers, who would be most likely to take advantage of mortgage credit expansions like those that occurred during the heyday of subprime lending.[53] Thus, where higher proportions of 1996 mortgage applications were denied, one would expect a greater increase in housing demand as mortgage approval standards were loosened. Similarly, where there are larger pools of these low credit-quality borrowers, one would expect a greater decline in housing demand during the bust. This conjecture follows from the fact that low-quality borrowers already face credit constraints,

[51] A key limitation of hedonic price indices is that additions made to existing homes can increase the index values, even though the overall price level may not have necessarily increased. An alternative measure of housing prices that controls for this issue is the median ZHVI per square foot. Using this constant-quality measure, the results are not qualitatively different from those presented in this paper.
[52] These data area available at the ZIP code level, so city-level estimates come from averaging over all ZIP codes within each city.
[53] In fact, Mian and Sufi (2009) show that the 1996 mortgage application denial rate is strongly positively correlated with the fraction of the population with credit scores below 660.

so that the credit-impairment costs of default are lower for these individuals, making default and foreclosure more likely, after a collapse in housing prices (see Brueckner et al. 2012).

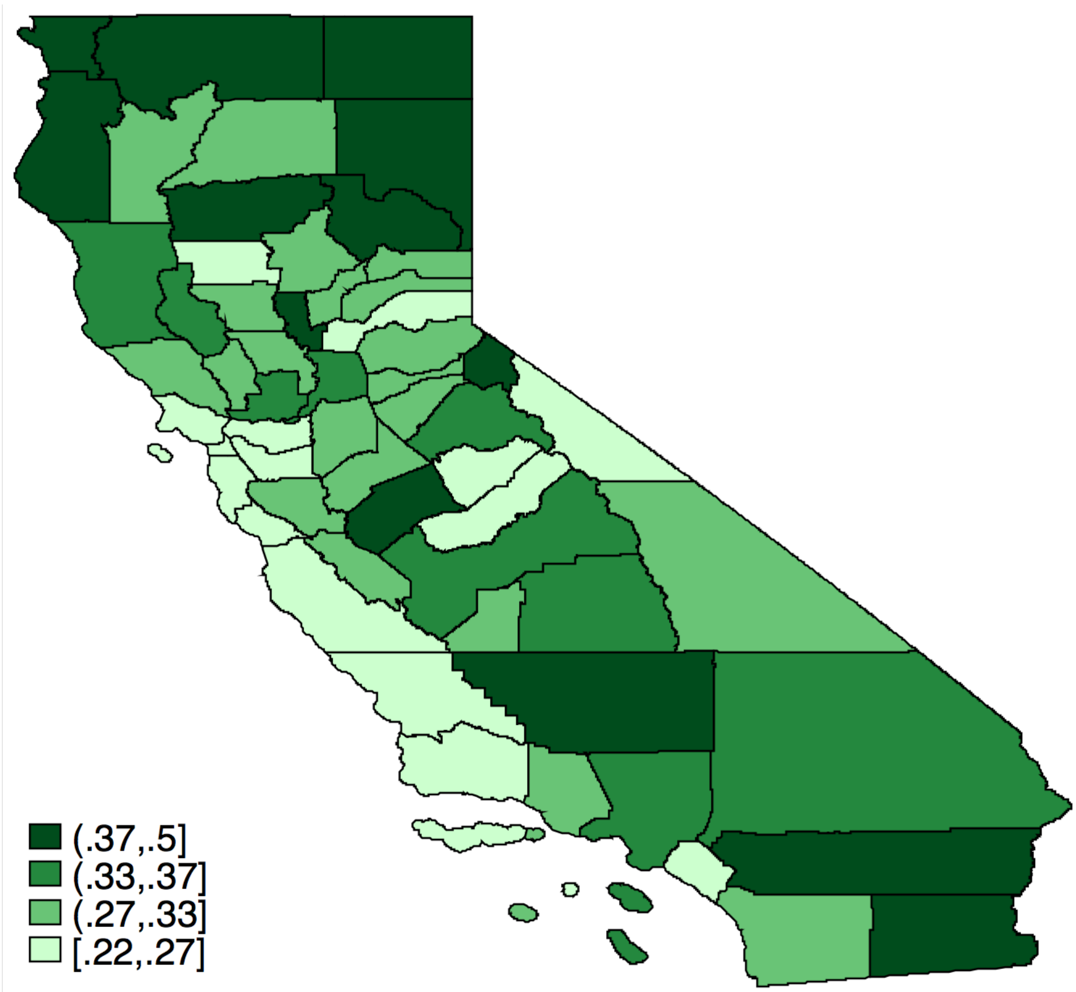


Figure 3.5: Average 1996 mortgage application rejection rate by county

Figure 3.5 shows the average 1996 mortgage rejection rate by county for the state of California. Higher rejection rates prevail in the northernmost part of the state, the Central Valley, and some parts of southern California (the Inland Empire, in particular). Housing prices before, during, and after the boom are negatively correlated with the 1996 mortgage rejection rate, so there were more potential subprime borrowers where prices were (and are still) lowest.[54]

3.5.3 Other Data

The vector X_{it} in equation (3-2) contains city-level housing, employment, and demographic variables. To control for locational amenities, the vector includes an indicator for whether the city directly borders the Pacific Coast. In addition, this vector includes 2000 Census values for median income, population density, percent of residents with a bachelor's degree, percent white, percent black, percent Asian, and median year of construction of the city's housing stock. There are a few reasons why 2000 Census values for these variables may be preferred to annual estimates. First, these variables are likely endogenous to changes in housing prices, so it is preferable to measure them at their initial values, which predate much of the price escalation and the eventual crash of the housing market cycle. Second, annual estimates for each city would require linear interpolation of Census data, but, given the volatility of the housing market during these years, it is unlikely that the variables changed linearly over this time frame.[55] The sole annually measured variable in X_{it} is each city's unemployment rate. While the unemployment rate suffers from the same endogeneity concerns as the other potentially time-varying variables in X_{it}, annual estimates are available for each city from the State of California's Employment Development Department, eliminating the need for interpolation. Moreover, unlike many of the other variables in X_{it}, the unemployment rate fluctuated significantly over the housing cycle, so

[54] When computed using housing prices at different points in time, Pearson's correlation coefficient always lies between -0.41 and -0.45.
[55] Alternatively, a more precise method, such as spline interpolation, could be used with more Census years, but it is not clear how much better this would be since it would still likely miss important changes occurring between survey years.

this will be an important variable in the first-difference specification estimated in Section 3.7 of this paper.

3.6 Regulation and the Level of Housing Prices

The specification in equation (3-2) allows for the estimation of the effect of regulation on the level of housing prices. Inserting variable names where appropriate, this specification is as follows:

$$\text{(3-4)} \qquad logP_{it} = \alpha_{0t} + \alpha_1 CaLURI_i + \alpha_2 GCI_i + \alpha_3 X_{it} + \alpha_{4t} Reject_i + \alpha_5 GCI_i CaLURI_i$$
$$+ \alpha_6 X_{it} CaLURI_i + \alpha_7 X_{it} GCI_i + \alpha_8 Reject_i CaLURI_i + \alpha_{9t} Reject_i GCI_i + \varepsilon_{it} \,.$$

The dependent variable in equation (4) is the natural logarithm of the Zillow Home Value Index for city i in time period t. The regulatory measure, $CaLURI_i$, is each city's score from the California Land Use Regulatory Index, and GCI_i is its score from the Geographic Constraint Index. The variable $Reject_i$ is the proportion of 1996 mortgage applications that were denied, which proxies for the prevalence of subprime borrowing in each city. The vector of local control variables, X_{it}, consists of the city-level characteristics described in the previous section. Interactions of X_{it} with $CaLURI_i$ and GCI_i are suppressed, except for those involving the unemployment rate. The primary reasoning behind this decision is that, as already mentioned, many of the other variables in X_{it} did not likely see considerable change over the housing cycle, but the unemployment rate oscillated dramatically. Thus, this variable provides a valuable proxy for shifts in housing demand to be used to estimate the elasticity effects in equation (3-3). Additionally, the first-difference model requires reliable estimates of changes in X_{it} for each city, and such estimates can be computed for the unemployment rate using reliable annual data from the State of California.[56]

Recall from Section 3.4 that the model relies on the assumption that cities' regulatory environments did not change over the time of analysis. Although there were undoubtedly a number of changes in particular policies in some cities over this time frame, the regulatory index attempts to measure the general tendencies toward regulation in each locality. It seems reasonable to assume that, although a few policies may have been adopted or repealed, the overall regulatory environment remained relatively constant. The data allow, to some extent, for the evaluation of this assumption, since respondents were asked for the year of adoption for any regulations they indicated were in place in their jurisdiction. However, only around 20% of respondents provided these particulars for any given question. Of those who gave this information, generally less than a quarter reported policy adoptions after 1999. This finding provides some evidence that the regulatory environments in cities across the state did not change drastically during the period of interest. Additionally, the few responses that were received are likely to overstate recent regulatory activity, since respondents are more likely to remember and report a year of adoption for a recently enacted policy than for one that has been in place for a relatively longer period of time.

In a second attempt to verify that regulatory regimes are fairly stable over time, responses to the 2013 survey were matched to those from a survey of California land use officials in 1992

[56] The State of California also releases annual population estimates for each city, so changes in population density can also be computed. However, it is not clear how useful this variable is as a proxy for shifts in housing demand, so the interaction is suppressed. Nonetheless, the results are not qualitatively different when interactions of population density with $CaLURI_i$ and GCI_i are included in the model in equation (3-4) or the first-difference specification in equation (3-5).

(Levine et al. 1996).[57] The 1992 survey was an important input in the creation of the 2013 survey, so there are fifteen overlapping questions, each regarding a different policy. For each of these policies, the percent of localities that adopted the regulation between survey years was computed. For twelve of the fifteen regulations, fewer than 5% of those localities that currently have the policy adopted it after 1992. While the policy-adoption patterns of the intervening years cannot be explored with these data, the fact that so few communities had policies in place in 2013 that were not in place twenty years prior suggests that it may be reasonable to assume that local regulatory environments are stable over time.

3.6.1 Results of Levels Regressions

Table 3.3 shows the results from the pooled regression in equation (4). Interacted variables are demeaned so that the main effect can be interpreted as the marginal effect of each variable evaluated at sample means. In order to allow the proxy for subprime borrowing (i.e., *Reject* in equation (3-4)) to have time-dependent effects, this variable is interacted with a full set of time dummies (i.e., one each for January 2000, April 2006, and January 2012). Standard errors in Table 3.3, and throughout this paper, are clustered at the county level, which allows for heteroskedasticity and arbitrary correlation between errors for cities within the same county.[58]

[57] While the 1992 survey is not the most recent, it provides the greatest overlap of questions for comparing responses with the 2013 California Land Use Survey.
[58] This method of clustering is not perfect, since cities that lie on the county border will have neighboring cities in different counties. However, clustering according to any other grouping criteria would suffer from a similar limitation.

As discussed above, the regulatory environments of nearby cities are related, so it is important to not treat cities as wholly independent observations.[59]

The estimated coefficient on *CaLURI*, in Table 3.3, indicates that an additional standard deviation increase in regulation is associated with a 4.7% increase in housing prices for the average city in the sample. This estimate is consistent with the findings of Quigley and Raphael (2005), who relate 1990 and 2000 housing prices to regulatory data from California cities in the early 1990s. These authors find the adoption of an additional land use control to be associated with a 3.1% increase in housing prices in 1990 and a 4.5% increase in 2000. It is noteworthy that although the data reveal a positive correlation between land use regulation and housing prices, this cross-sectional analysis does not account for unobserved heterogeneity in California cities, which precludes a causal interpretation of the relationship.

As shown in Table 3.3, housing prices are, as expected, significantly higher where the unemployment rate is lower and where residents are wealthier and more educated. The 2000 racial composition of cities is also significantly related to housing prices, but in a surprising way: housing prices appear to be lower where there were higher proportions of whites, blacks, and Asians. Housing prices are negatively correlated with the median year of construction of cities' 2000 housing stock and positively correlated with 2000 population density, so more dense cities and those with older homes tend to be more expensive. Unsurprisingly, coastal cities command a significant price premium.

[59] When the CaLURI value for each city's nearest neighbor is included as a control, the estimated effect of regulation in Table 3.3 is somewhat stronger, but the general results are the same as from those specifications reported here.

Table 3.3
Pooled cross-sectional regression

Dependent Variable:	LogPrice
CA Land Use Regulatory Index	0.047***
(CaLURI)	(0.01)
Geog. Constraint Index (GCI)	-0.001
	(0.02)
Reject*Jan2000	-0.336**
	(0.15)
Reject*April2006	-0.048
	(0.17)
Reject*Jan2012	-0.384*
	(0.21)
CaLURI*Reject*Jan2000	0.031
	(0.12)
CaLURI*Reject*April2006	0.001
	(0.14)
CaLURI*Reject*Jan2012	0.035
	(0.16)
GCI*Reject*Jan2000	-0.055
	(0.06)
GCI*Reject*April2006	0.065
	(0.07)
GCI*Reject*Jan2012	-0.132
	(0.09)
GCI*CaLURI	-0.004
	(0.01)
$CaLURI^2$	-0.010
	(0.01)
GCI^2	-0.005
	(0.01)
unemployment rate (urate)	-3.970***
	(0.58)
urate*CaLURI	-0.104
	(0.30)
urate*GCI	-0.231
	(0.24)
2000 median income (‘000)	0.011***
	(0.00)
2000 pct college graduate	1.371***
	(0.17)
2000 pct White	-0.452**
	(0.19)
2000 pct Black	-0.970***
	(0.33)
2000 pct Asian	-0.427**
	(0.20)
2000 med. year structure built	-0.005***
	(0.00)
2000 population density	0.022***
	(0.01)
coastal city	0.201***
	(0.04)
N	784

Notes: This regression includes three time-specific intercepts (i.e., fixed effects). Standard errors (shown in parentheses) are clustered at the county level to account for spatial correlation in the adoption of land use regulation. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Recall from Section 3.4 that the interaction terms in equation (3-2) (and, therefore, equation (3-4)) come from the elasticity effects of Reg_i and Geo_i. If there are no elasticity effects, the impact on housing prices of each right-hand side variable in equation (1) would not depend on any other variable, so there would be no need for interactions in the linear specification. Given that none of the interactive coefficients in Table 3.3 is statistically distinguishable from zero, the results from this specification suggest that there are no elasticity effects from either Reg_i or Geo_i. However, given the cross-sectional nature of these regressions, the estimates may be biased by unobservable characteristics of cities that affect both local regulatory environments and housing prices.

The next section of this paper briefly discusses the boom and bust in California, and then tests for elasticity effects using the first-difference model in equation (3-3). This model removes the effects of time-invariant factors, including fixed unobservable characteristics that, if correlated with regulation and housing prices, may lead to biased estimates in the cross-section. The elasticity effects are identified, in the first-difference model, by determining whether variation in the supply constraints lead to differential affects on housing-price changes, following a shift in housing demand.

3.7 Regulation and the Housing Market Boom and Bust

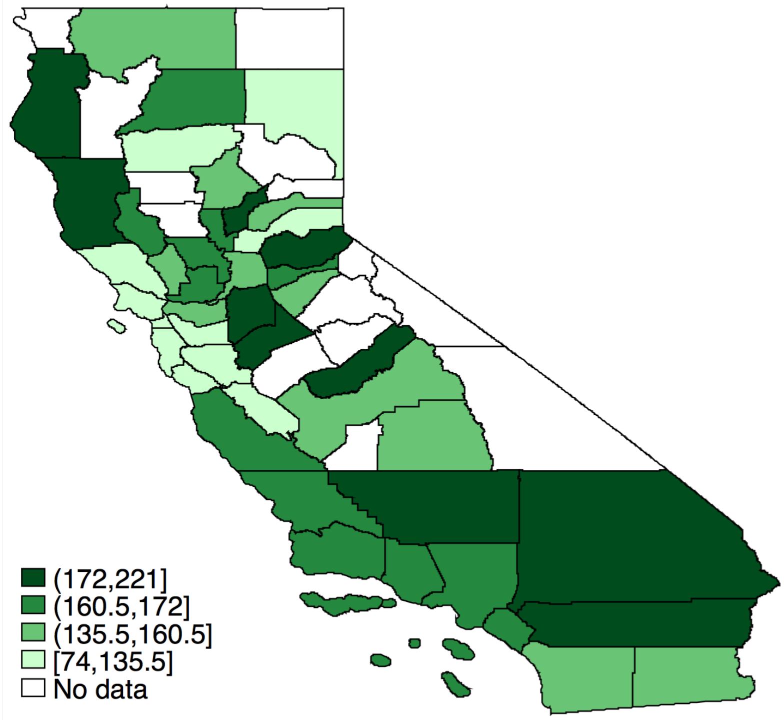
During the late 1990s and early 2000s, the demand for homes increased dramatically in communities across the United States. This increase in demand was fueled by historically low interest rates, speculative fever, and, as already mentioned, lax mortgage approval standards (e.g., Sinai 2013; Mayer 2011). The boom, and eventual bust, experienced in the U.S. housing market during the mid-2000s triggered the stock market crash of 2008 and lead to the Great Recession (e.g., Taylor 2009; Verick and Islam 2010; Farmer 2012).

Although housing prices rose throughout the country, there was a remarkable amount of idiosyncratic variation in the run-up and subsequent crash experienced in particular U.S. cities. This observation has lead researchers to ask why housing bubbles grew so much larger and burst with so much more fervor in some markets than in others. This section of the paper applies the CaLURI to the question of whether the stringency of local regulatory regimes can account for these differences within the state that most actively regulates land use: California.

3.7.1 California's Housing Market Boom and Bust

Before turning to the potential role of regulation in the boom and bust in California cities, it is helpful to first discuss some stylized facts about the housing bubble in that state. Panel A of Figure 3.6 shows variation in the average price increase during the boom (January 2000 – April 2006) for counties across the state. The region that appears to have been most sheltered from the price run-up is the San Francisco Bay area. Of the forty California cities with the smallest percentage increase in housing prices, only two lie outside of the Bay area. Indeed, the twenty cities that experienced the smallest boom all lie within the Bay area counties of Santa Clara, Marin, and San Mateo. There were large price increases in some northern coastal and central inland communities, but those in Southern California, particularly the Inland Empire, saw some of the biggest jumps. While cities like Palo Alto and Cupertino saw prices rise by just over 50% during the boom, housing prices in places like San Bernardino rose by over 250%.

A. Price Growth During January 2000 – April 2006



B. Price Growth During April 2006 – January 2012

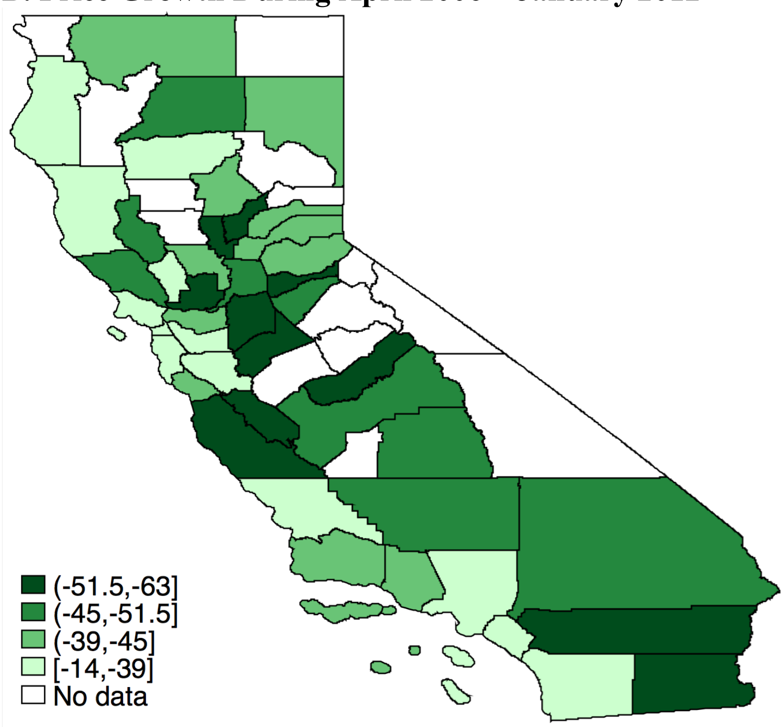


Figure 3.6: Average percentage change in housing price by county

As seen in Panel B of Figure 3.6, many of the areas that saw the most dramatic price increases during the boom also experienced the largest price declines during the bust. This pattern is especially apparent for the central inland counties of San Joaquin, Stanislaus, and Madera, as well as some southern inland counties like Riverside. Figure 3.7 shows the strong relationship between the magnitude of the boom and bust in the 349 cities for which data is available (Pearson's correlation coefficient = -0.67). The contrast between the bust in Silicon Valley and that in the Inland Empire is as stark as it is with the boom; While prices fell by over 60% in San Bernardino between April 2006 and January 2012, they actually increased slightly for Palo Alto and Cupertino during the same time frame.[60]

Figure 3.7: Percentage changes in housing prices during boom and bust

[60] Palo Alto and Cupertino both saw modest price declines during 2008 and the beginning of 2009, but prices started rising again in both of these places in mid-2009.

3.7.2 The Role of Land Use Regulation

Can differences in local housing-price volatility be attributed to variation in the stringency of land use regulation? A few authors have attempted to answer this question, but there is no consensus in the existing literature, which is briefly reviewed below.

Malpezzi and Wachter (2005) develop a model that predicts, among other things, that price cycles are more volatile in housing markets with more-inelastic supply. As suggestive evidence, they show that, for U.S. metropolitan areas during 1979-1996, the level of local land use regulation is positively correlated with the standard deviation of annual house-price changes.

Similarly, Paciorek (2013) develops a structural dynamic model of housing supply to explore the relationship between house price volatility and local regulatory regimes in U.S. metropolitan areas. Using component parts of the Wharton Residential Land Use Regulatory Index (WRLURI), together with a measure of geographic constraint similar to that of Saiz (2010), he finds that supply constraints increase volatility in house prices. This relationship emerges as a result of regulation decreasing housing supply elasticities and is magnified by the lessening of housing-investment in geographically constrained areas.

Glaeser et al. (2008) explore how supply elasticities affect housing prices and construction over the housing cycle. While these authors only report results using Saiz's (2010) measure of undevelopable land as proxy for supply inelasticities, they note that their findings are qualitatively similar when they instead use the Wharton Residential Land Use Regulatory Index (WRLURI) (Gyourko et al. 2008). They find that U.S. metropolitan areas with relatively inelastic housing markets experienced more price appreciation and a dampened construction response during the boom of 1982-1989 and during the post-1996 boom, compared to more elastic housing markets. However, these authors do not find strong evidence connecting housing supply elasticities to price declines during the bust of 1989-1996.

In a similar paper, Huang and Tang (2012) ask whether supply elasticities can account for spatial differences in the boom (2000-2006) and bust (2006-2009) in U.S. cities. These authors use both the Saiz (2010) measure of geographic constraint and the WRLURI (Gyourko et al. 2008) to proxy for supply inelasticities. Acknowledging the role of looser mortgage approval standards in contributing to the recent U.S. housing price cycle, Huang and Tang (2012) also include the 1996 mortgage application rejection rate as a proxy for the increase in local housing demand stemming from the availability of subprime mortgages. They find that housing markets with more-inelastic supply saw larger price swings in both the boom and the bust.

Davidoff (2013) uses the same proxies for supply inelasticities as Huang and Tang (2012), in addition to an indicator for whether the metropolitan area is a coastal market (à la Rappaport and Sachs 2003). Across three sets of specifications each allowing for different sets of assumptions about local demand and supply conditions, he finds consistent evidence that, conditional on demand, supply inelasticities are not positively correlated with the severity of the recent housing price cycle in U.S. cities. While Davidoff (2013) concedes that the assumptions required for his first two specifications are fairly strong, the third set of specifications regresses different measures of the severity of the housing price cycle on each of his supply inelasticity proxies. State fixed effects are included in some of these regressions to control for variation in the demand for housing. Without state fixed effects, the results of Davidoff's (2013) third set of specifications are consistent with Huang and Tang (2012): each of the supply inelasticity proxies shows a positive and significant relationship with price cycle severity. However, after controlling for differences across states, this positive relationship vanishes, leading him to conclude that supply inelasticities did not contribute to the housing price cycle.

While Huang and Tang (2012) do not discuss whether their results are robust to the inclusion of state fixed effects, they mention that when MSA fixed effects are included, "most estimates become small and indistinguishable from zero." Thus, the determination of whether supply-elasticity proxies played a meaningful role in the boom and bust relies heavily on the kind of variation used to identify these effects (i.e., between cities across the U.S., between cities within each state, or between cities within each MSA).

One paper, by Ihlanfeldt and Mayock (2014), uses intrastate variation in supply elasticities to show that Florida counties with more inelastic housing supply saw greater price swings in the run-up of the early 2000s, although not in the subsequent crash. Rather than use a proxy, these authors estimate short-run elasticities of housing supply using a 21-year pooled panel regression of annual single-family home completions on annual housing prices, average construction financing interest rates, land prices, construction costs, and the previous year's stock of single-family housing. Ihlanfeldt and Mayock (2014) also develop measures of land availability and two measures of land use regulation: counties' expenditures on comprehensive planning and minimum lot size requirements. The authors find that these factors are significant determinants of their estimated elasticities. While expenditures on comprehensive planning and minimum lot size requirements are helpful in understanding local regulatory conditions, the narrowness of their scope suggests that they likely fail to account for at least some factors relevant to restricting land use.

3.7.3 Application of the CaLURI to the Boom and Bust

If demand for housing increased uniformly across the state, and if regulation is a significant determinant of housing supply elasticities, we would expect housing prices in the more regulated markets to rise more precipitously. From a quick examination of Figures 3.1 and 3.6, the relationship between price appreciation and regulation appears weak, at best. Figure 3.8 replicates Figure 3.7 for those cities for which the CaLURI can be computed, after distinguishing cities by their level of regulation. If regulation increased the magnitude of the boom and bust in California cities, we would see a cluster of the "Most regulated" cities in the top-left corner of the scatterplot and those classified as "Least regulated" in the bottom right corner. This pattern does not arise in Figure 3.8. In fact, as indicated in panels A and B of Table 3.4, the most regulated cities had somewhat smaller booms and busts than did those that take a more *laissez faire* approach to restricting land use. However, this preliminary analysis operates under the unlikely assumption that housing demand rose homogenously across California's vastly heterogeneous communities.

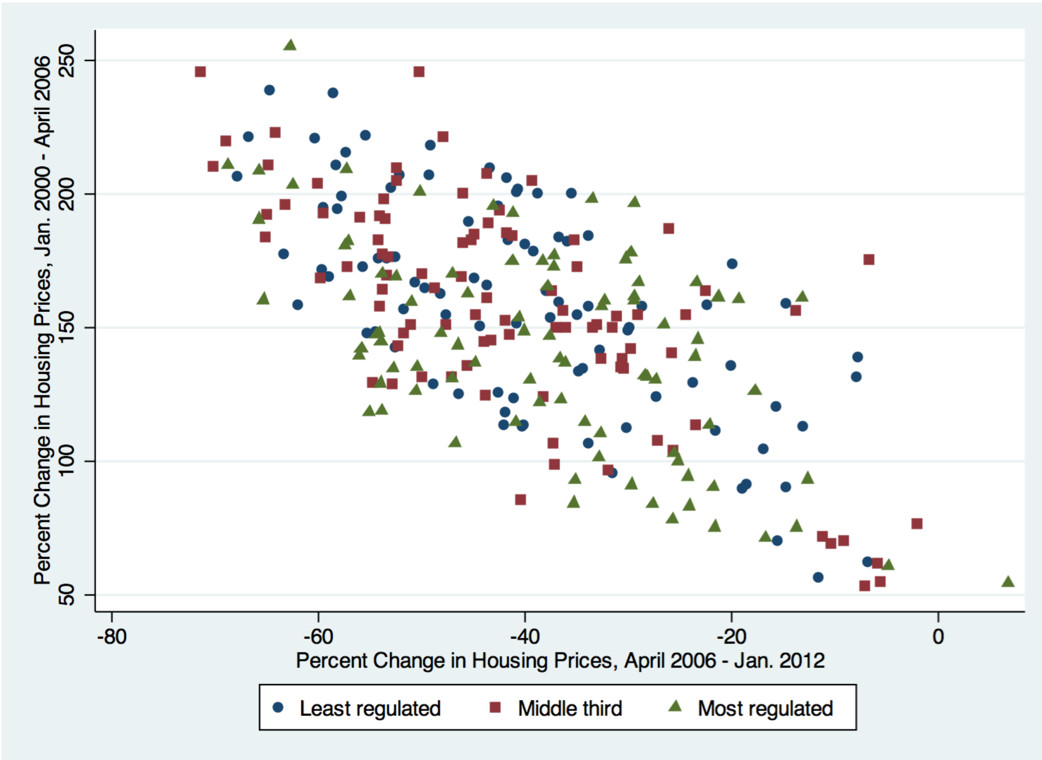


Figure 3.8: Percentage changes in housing prices by level of regulation

Changes in local labor markets and the availability of subprime mortgages likely created variation in the magnitude of housing demand shocks felt across California. These differences are taken into account by the specification in equation (3-3). Moreover, as mentioned above, the interaction terms in this equation allow us to test whether land use regulation or geographic constraints affect housing supply elasticities. As before, we focus attention on the unemployment rate as the primary time-varying demand factor in X_{it}. Following Huang and Tang (2012), separate regressions are estimated for the boom and bust periods. Inserting variable names, the specification in equation (3-3) is as follows:

(3-5) $\Delta logP_{i,\tau} = \beta_{0,\tau} + \beta_{1,\tau}\Delta urate_{i,\tau} + \beta_{2,\tau}Reject_i + \beta_{3,\tau}\Delta urate_{i,\tau}CaLURI_i + \beta_{4,\tau}\Delta urate_{i,\tau}GCI_i$

$$+ \beta_{5,\tau} Reject_i CaLURI_i + \beta_{6,\tau} Reject_i GCI_i + u_{i,\tau} \ ,$$

where, as before, i indexes California cities. The subscript τ indicates either the boom period (January 2000 to April 2006) or the bust period (April 2006 to January 2012). As noted above, in the first-differenced model, the main effects of the time-invariant $CaLURI_i$ and GCI_i are differenced out, and these variables enter only through their interactions with the two variables representing shifts in demand. The variable $Reject_i$ is included in the model because its effect varies with time. In particular, $Reject_i$ captures the shift in demand stemming from the expansion of subprime lending in the boom years and from the downturn that followed.

By estimating the model in equation (3-5) for the boom and bust periods separately, the coefficient on each term is allowed to vary over the housing cycle. However, the underlying model in equation (3-3) calls for a pooled regression, where the effect of each variable and interaction is constrained to be constant over the housing cycle. This assumption is unreasonable, given that subprime lending fueled the demand for housing during the boom and the ensuing defaults and foreclosures dramatically reduced housing demand during the bust. The assumption can be relaxed by estimating a pooled first-difference specification with interactions involving indicators for the boom and bust periods and those terms involving *Reject*. The results from such a specification are shown following the separate boom and bust regressions in the next section of this paper.

3.7.4 Results of First-Difference Regressions

Table 3.5 contains results from the regressions in equation (3-5). The first column displays results from the boom regression and the second column shows those from the bust regression.

As before, interacted variables are demeaned so that main effects are interpreted as marginal effects at sample means. The main result from the first column of Table 3.5 is that local land use regulation is not a significant determinant of housing supply elasticities in these California cities. This finding is evidenced by the insignificant coefficients on the interactions involving the CaLURI and the two demand shifters. Whereas regulation does not affect supply elasticities, there is evidence that geographic constraints, as measured via the GCI, have a significant impact on elasticities. This conclusion follows from the significant coefficient estimates on the interaction terms of GCI with both demand shifters in the boom regression. Where housing demand rose through the expansion of subprime lending or strengthening labor markets, geographic constraints exacerbated the price run-up during the boom. In particular, the positive and significant coefficient on the interaction of *GCI* and *Reject* suggests that, during the boom, geographic constraints lead to significantly larger price appreciation where there was a larger pool of potential subprime borrowers (and, thus, a larger shift in housing demand). Similarly, the negative and significant coefficient on the interaction of *GCI* and *Δurate* indicates that where local housing demand increased due to an expansion of local employment (i.e., a decrease in the unemployment rate), geographic constraints cause prices to rise more rapidly during the boom.

In the first column of Table 3.5, the estimated coefficient on *Reject* indicates a clear relationship between the housing price boom in California cities and the proportion of likely candidates for subprime mortgages in those localities. Moreover, the negative and significant coefficient on the change in the unemployment rate suggests that where labor markets were becoming weaker (stronger) as indicated by increasing (decreasing) unemployment rates, housing prices increased at a slower (faster) rate during the housing price run-up.

Table 3.5
Boom and bust first-difference regressions

Dependent Variable:	ΔlogPrice, Jan. 2000 – April 2006	ΔlogPrice, April 2006 – Jan. 2012
1996 mortgage rejection rate (Reject)	0.244***	-0.202
	(0.07)	(0.13)
Reject*CaLURI	-0.023	-0.005
	(0.05)	(0.10)
Reject*GCI	0.090*	-0.160*
	(0.05)	(0.09)
Δunemployment rate (Δurate)	-4.688***	-5.369***
	(1.44)	(0.81)
Δurate*CaLURI	0.081	0.250
	(0.87)	(0.48)
Δurate*GCI	-1.114**	-0.580
	(0.47)	(0.43)
intercept	0.931***	-0.587***
	(0.02)	(0.04)
N	260	261
adj. R-sq	0.342	0.456

Notes: Standard errors (shown in parentheses) are clustered at the county level to account for spatial correlation in the adoption of land use regulation. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

As shown in the second column of Table 3.5, many of the factors that drove housing prices up during the bubble deflated them after the downturn. The insignificant coefficient estimates on the interactions involving the CaLURI suggest that, as with the boom, the magnitude of the bust is not related to the stringency of land use regulation. However, the coefficient on the interaction between *GCI* and *Reject* provides evidence that, where subprime lending fueled an increase in demand during the boom, more geographically constrained markets saw larger price declines when the housing market crashed. The negative and significant coefficient on *Δurate* suggests that, during the bust, prices declined fastest where local labor markets were hit the hardest by the economic downturn.

Table 3.6
Pooled first-difference regression

Dependent Variable:	ΔlogPrice
1996 mortgage rejection rate (Reject)*boom	0.219***
	(0.08)
1996 mortgage rejection rate (Reject)*bust	-0.245
	(0.15)
Reject*CaLURI*boom	-0.045
	(0.06)
Reject*CaLURI*bust	-0.000
	(0.11)
Reject*GCI*boom	0.131**
	(0.05)
Reject*GCI*bust	-0.205**
	(0.09)
Δunemployment rate (Δurate)	-4.951***
	(1.02)
Δurate*CaLURI	0.171
	(0.24)
Δurate*GCI	-0.008
	(0.17)
boom	0.596***
	(0.07)
bust	-0.457***
	(0.04)
N	521
adj. R-sq	0.955

Notes: Standard errors (shown in parentheses) are clustered at the county level to account for spatial correlation in the adoption of land use regulation. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Table 3.6 shows results from the pooled first-difference specification. As before, all interacted variables are demeaned so that the main effects can be interpreted as the marginal effects at sample means. Unsurprisingly, the results from the pooled specification are not qualitatively different from those in Table 3.5. In Table 3.6, as in Table 3.5, all of the interactions involving the CaLURI have insignificant coefficient estimates, suggesting that regulation does not affect housing supply elasticities in California cities. However, as before, geographic constraints appear to be an important determinant of these elasticities. Relative to the estimated coefficients on the interactions between *GCI* and *Reject* reported in Table 3.5, those in Table 3.6 are very similar, though more precise and larger in magnitude. Similarly, the sign, magnitude, and significance for the boom- and bust-specific intercepts and *Δurate* are quite consistent across specifications. The only meaningful difference between the pooled model and the previous one is that, in the pooled model, the interaction between *Δurate* and *GCI* shrinks considerably and becomes indistinguishable from zero. This disparity is not surprising, given that the overall mean is removed from this variable in the pooled model, whereas the boom- and bust-specific means are removed in the earlier regressions.[61]

The results in Tables 3.5 and 3.6 are not entirely consistent with those in Table 3.3. In particular, the first-difference regressions in Tables 3.5 and 3.6 suggest that geographic constraints significantly impact the elasticity of housing supply in California cities, but the cross-sectional regressions in Table 3.3 indicate that there is no such effect. This divergence of results is likely due to omitted variables bias, in the cross-sectional regressions, stemming from the presence of time-invariant factors that affect both the stringency of local regulatory regimes and housing prices. As noted above, the first-difference regressions remove any such sources of bias, and, thus, provide more reliable results regarding the potential elasticity effects of regulation and geographic constraints.

While supply constraints contributed to local housing market bubbles, we cannot ignore the role of demand factors, such as the availability of subprime mortgages, in the precipitous rise of some

[61] The results from Table 3.6 are nearly identical when *Reject* is used as the only demand shifter and regressors involving the (potentially endogenous) unemployment rate are omitted.

cities' housing prices. The local dependence on such lending practices was probably not homogenous across the state, and the cities where there was likely more subprime lending are those that experienced the most violent house price cycles in California and across the country (see Mian and Sufi 2009). Table 3.7 confirms that as the proportion of low quality borrowers increased, the magnitudes of the boom and bust were amplified.

Table 3.7
Distributions of price growth by 1996 mortgage rejection rates

	n = 90 Lowest rej. rates (%)	n = 85 Middle third (%)	n = 94 Highest rej. rates (%)
Price Growth, Jan. 2000 – Apr. 2006			
Mean	135.25	155.36	167.87
Std. Dev.	38.86	35.88	41.25
Price Growth, Apr. 2006 – Jan. 2012			
Mean	-32.20	-40.88	-46.15
Std. Dev.	15.12	14.38	13.54

3.7.5 First-Difference Regressions Using Saiz's Measure of Geographic Constraints

Given the central role of geographic constraints in this analysis, it is important to explore how the key results are affected by the use of alternative measures of these constraints. To this end, Table 3.8 reports results from the same specification as in Table 3.6, except that the proportion of undevelopable land from Saiz (2010) replaces the GCI as measure of geographic constraint. Of the three regressors involving the Saiz measure, only the interaction with *Reject* during the boom period yields a statistically significant coefficient. The estimated coefficients on each of the interactions involving the Saiz measure are markedly smaller in magnitude than their counterparts in Table 3.6, using the GCI. That the estimates using the Saiz measure are attenuated toward zero is not surprising given the error that is introduced by using an MSA-level proxy for local geographic constraints.

Table 3.8
Pooled first-difference regression using Saiz's measure of undevelopable land

Dependent Variable:	ΔlogPrice
1996 mortgage rejection rate (Reject)*boom	0.251**
	(0.11)
1996 mortgage rejection rate (Reject)*bust	-0.345*
	(0.18)
Reject*CaLURI*boom	0.020
	(0.08)
Reject*CaLURI*bust	0.059
	(0.13)
Reject*Undevelopable Land*boom	0.014**
	(0.01)
Reject*Undevelopable Land*bust	-0.004
	(0.01)
Δunemployment rate (Δurate)	-6.109***
	(1.08)
Δurate*CaLURI	-0.032
	(0.33)
Δurate*Undevelopable Land	-0.027
	(0.02)
boom	0.502***
	(0.08)
bust	-0.422***
	(0.05)
N	346
adj. R-sq	0.948

Notes: Standard errors (shown in parentheses) are clustered at the county level to account for spatial correlation in the adoption of land use regulation. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

3.7.6 Estimating Elasticities Directly From Supply Equation

While the first-difference of the linear approximation provides an intuitive test for the elasticity effects of regulation and geographic constraints, an alternative approach is to estimate the parameters of the supply equation directly. Since X_{it} and Sub_i appear in the demand equation only, these can be used as instrumental variables to estimate the supply equation. In log-differences, and with variable names inserted, the supply equation can be written as follows:

$$(6) \quad \Delta log Q^S_{it} = \psi_t + (\pi + \gamma CaLURI_i + \theta GCI_i)\Delta log P_{it} \,,$$

where $\psi_t = \Delta log(\upsilon_t)$. In order to capture the change in price stemming from demand-related factors, those variables that only enter the first-differenced demand equation (i.e., $Reject_i$ and $\Delta urate_{it}$) are used to get predicted log-price changes.[62] These predicted price changes are then interacted with the CaLURI and the GCI to yield a set of instruments for identifying equation (6) using two-stage least squares. City-level housing stock data come from the California Department of Finance (2012).

Results from the two-stage least squares estimation are contained in Table 3.9. The Cragg-Donald F-statistic, which can be used to test for weak instruments, is 85.16, which is well above the critical values presented in Stock and Yogo (2005) for both bias and size. Consistent with the earlier findings of this paper, the insignificant estimate of γ from equation (3-6) suggests that the stringency of local land use regulation does not have a significant impact on housing supply elasticities in California cities. However, as before, Table 3.9 indicates that geographic constraints significantly reduce housing supply elasticities. The precise interpretation of the

[62] This regression produces an F-statistic of 2432.16 ($p < 0.0001$), and each estimated coefficient is significant at the 0.01% level.

estimate for θ is difficult, since the GCI is an ordinal variable. Interpreted literally, this coefficient indicates that a 1-unit increase in the level of local geographic constraint (i.e., moving from one classification of geographic constraint to the next, more-constrained, level) reduces the elasticity of housing supply by an average of 0.02.

Table 3.9
Instrumental variable estimation of supply equation

Dependent Variable:	ΔlogQ
ΔlogPrice	0.010
	(0.027)
CaLURI*ΔlogPrice	0.004
	(0.007)
GCI*ΔlogPrice	-0.0197***
	(0.004)
Cragg-Donald F-statistic	85.16
N	445

Notes: IV using linear projections of *ΔlogPrice* onto *Reject* and *Δurate* instruments, as described in the text. The first-stage F-statistics on the excluded instruments for *ΔlogPrice* and its interactions with *CaLURI* and *GCI* are 20.78, 279.10, and 138.78, each of which is significant at the 0.01% level. This regression includes time-period fixed effects. Standard errors (shown in parentheses) are clustered at the county level. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

3.8 Conclusion

This paper develops a new land use regulatory index for cities and counties across the state of California. Using this index and component sub-indices, the paper discusses spatial patterns in the adoption of regulation across the state. The San Francisco Bay area contains many of the most stringently regulated communities, but there is considerable spatial variation in regulatory regimes across the state. Communities in the Bay area are more prone to adopt outright limitations on development, while those in Southern California (on the coast and inland) tend only to restrict the *form* of new residential dwellings.

Applying the regulatory index, this paper shows that while land use regulation may be associated with higher housing prices in levels, it did not play a meaningful part in either the dramatic rise or fall in prices that characterize the recent housing market bubble in California cities. These results are consistent with Davidoff's (2013) finding that regulation did not contribute to the severity of the housing price cycle. Davidoff (2013) interprets the finding as suggesting that supply inelasticities were irrelevant in the boom and bust. This paper finds evidence of an alternative explanation, which is that, for California cities, regulation may not be an adequate proxy for supply inelasticities.

Using a city-level measure of geographic constraint, with considerable coverage over the state of California, this paper finds that physical impediments to development lead to more dramatic booms and busts in local housing markets. This result can be taken as evidence that, while not true for regulation, geographic constraints appropriately proxy for local housing supply inelasticities in California cities. Indeed, structural estimates indicate that geographic constraints significantly reduce housing supply elasticities.

The novel dataset employed in this analysis provides useful information for researchers and decision-makers in the public and private sectors as to the regulatory environment in localities across the state of California. The California Land Use Regulatory Index (CaLURI), together with the sub-indices of which it is comprised, can be used in future studies of the causes and effects of land use regulation. In particular, given the recent surge in residential construction, the

CaLURI can be used to explore spatial patterns in residential development during the post-recession era, together with the resulting changes in local demographics.

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Appendix 1.A
High Treatment Letter for the NPO

[redacted]

Don't miss the Chairman's Match!
Donate by August 31 and your gift will be tripled.

Dear [redacted],

Chairman [redacted]'s offer to match any contribution you give to fight [redacted] this summer ends soon.

You must make your gift online before midnight, August 31st, to take advantage of his offer to TRIPLE the resources to develop [redacted] treatments and cures.

Your matched gift will help us fund even more cutting-edge research. You will ensure that the best scientists have the resources they need to move promising research from the laboratory through human clinical trials as quickly as possible.

Seven potential treatments are currently in human clinical trials – more than at any other time in our history.

But each human clinical trial can cost several million dollars.That's why [redacted] are promising to give $2 for every $1 you give today – no matter what amount.

So don't delay. I've already received a contribution of $50 from a gracious donor like you, and I'm counting on you to join this person in helping us fight [redacted]. Make your tax-deductible gift online now.

[redacted]

Sincerely,

[redacted]

[redacted]

P.S. Time is running out to provide TRIPLE the resources for vital research. Make your gift before midnight, August 31st. Thank you.

Appendix 1.B
Low Treatment Letter for the NPO

[redacted]

Don't Miss the Chairman's Match

Dear [redacted],

Thanks to you [redacted] continues to make scientific advancements in our fight to [redacted]. **Potential new therapies and treatments are coming out of labs across the globe, and we are moving them to the clinical trial phase as fast as we possibly can.**

But I need your support more than ever before to keep moving this work forward as rapidly as possible.

That's why I urge you to make a generous gift by August 31st to take advantage of a special opportunity to TRIPLE the value of your contribution.

Our Chairman, [redacted] are willing to contribute $2 for every $1 you give – but only if you act by midnight August 31st. I've already received a contribution of $25 from a gracious donor like you, and I'm counting on you to join this person in helping us fight [redacted].

Please give today before this special matching opportunity disappears.

Your contribution by August 31st will be put to work immediately and will provide three times the vital resources to find treatments to prevent [redacted] for millions of people.

But you can't delay. This match opportunity will end in just a few days.

[redacted]

With gratitude,

[redacted]

P.S. Time is running out on the Chairman's Summer Match. Make your gift online before midnight August 31st.

Appendix 1.C
Probit Regression to Obtain Predicted Propensity Scores

A. Coefficients and standard errors

	Dependent variable: Probability of being assigned into the high treatment group
Most recent donation	0.049***
	(0.002)
Highest donation	-0.044***
	(0.002)
Gender	-0.027
	(0.021)
Intercept	0.021
	(0.075)
N	15,166

B. Summary statistics for predicted probabilities (i.e., propensity scores)

	High Treatment	Low Treatment	Difference (High Treatment – Low)
Mean	0.530	0.487	0.042***
			(0.002)
Minimum	0.002	0.0004	0.0016
Maximum	0.685	0.692	-0.007
N	7,712	7,454	

Notes: Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 2.A
Control Variable Coefficients and Standard Errors from Column (3) of Table 2.3

Dep. Variable:	$\frac{\text{Permits}_t}{\text{Housing stock}_{t-}}$	$\frac{\text{Permits}_t}{\text{Population}_{t-1}}$	Log(Permits)	Log(MF Permits)	Log(SF Permits)
% white	-0.0605**	-0.0245**	-1.2304**	-1.9045**	-0.7041**
	(0.0085)	(0.0032)	(0.2881)	(0.3508)	(0.2568)
% black	-0.0862**	-0.0343**	-2.2888*	-1.7897	0.2678
	(0.0208)	(0.0083)	(1.0159)	(1.3267)	(0.7496)
% owner-occupier	-0.0002	-0.0001	-0.0347**	0.0414	-0.0447**
	(0.0004)	(0.0001)	(0.0130)	(0.0213)	(0.0145)
% foreigner	-0.0062**	-0.0015**	-0.2536**	0.0470	-0.3667**
	(0.0011)	(0.0005)	(0.0827)	(0.1761)	(0.0706)
% housing occupied	-0.4029**	-0.1643**	-3.9276**	-3.6774*	-6.0370**
	(0.0769)	(0.0248)	(1.2508)	(1.6957)	(1.2005)
% housing rural	-0.0015	-0.0005	-0.1918	-0.0294	-0.1705*
	(0.0020)	(0.0009)	(0.1009)	(0.1381)	(0.0839)
median income ('000)	-0.0002**	-0.00003	-0.0098**	0.0012	-0.0157**
	(0.0001)	(0.00002)	(0.0021)	(0.0039)	(0.0016)
population ('000)	–	–	-0.0003	-0.0006	-0.0006
			(0.0005)	(0.0005)	(0.0004)
N	7849	8472	8545	6272	8456

Notes: Conley standard errors are in parentheses. Significance at the 5% and 1% level is denoted by * and **, respectively.

Appendix 2.B
Variable Descriptions for Similar Questions from Two Datasets of Land Use Regulation

Wharton Survey on Residential Land Use Regulatory	Pioneer Institute for Public Policy Research	Matched responses	Error rate
Annual limit on the total allowable number of building permits for *either single family or multi-family homes*.	Limitation on the annual number of *residential permits* issued	48	6.25%
Minimum lot size requirement	Minimum lot size requirement *under flexible development*	27	7.41%

Notes: The Wharton survey and the data collected by the Pioneer Institute for Public Policy Research both capture information on local regulatory regimes in 2004. The former dataset covers localities across the U.S., while the latter one is concerned with cities in the Greater Boston area. Thus, the matched responses enumerated in this table are all communities in Greater Boston. Italics are used to emphasize differences in how the variables are defined in the two datasets. Error rates are computed as the proportion of responses that are inconsistent across the two sources.

Appendix 3.A
Survey Respondents' Job Titles

Job Title	No. Respondents
Director of Community/Economic Development or Development Services	151
Director of Planning or Resource Management or Public Services	81
Assistant Director of Community Development or Planning	17
Planning Manager/Principal Planner (Planner IV)	53
Senior Planner (Planner III)	37
Associate City Planner (Planner II)	10
Assistant City Planner (Planner I)	12
Planning Technician	6
Contract Planner or "Planner" (of unidentified level)	29
City Clerk/Administrator	9
City Manager/Director	11
Assistant City Manager	4
Total	420

Appendix 3.B
Descriptions of Six Regulatory Sub-indices

Low-Cost Alternative Index (LCAI). There is an extensive literature suggesting that land-use regulation increases local housing prices. In highly restrictive areas, low-cost alternatives to traditional housing units may be tightly regulated or not permitted at all, exacerbating these effects. The LCAI measures the extent to which communities restrict low-cost alternatives (i.e., mobile and manufactured homes). It is computed as the standardized sum of indicators for whether mobile or manufactured homes are allowed in the jurisdiction, whether they are allowed outside of mobile home parks, whether mobile or manufactured homes must meet minimum size or width requirements, and whether they must meet any other specific provisions, such as pitched roofs, attached garages, etc. Using the variables definitions from Appendix B, this index is calculated as the following:

$$\text{LCAI} = \text{STD}\{(1 - \textit{mobile\_allowedD}) + (1 - \textit{mobile\_moveD}) + \textit{mobile\_minsizeD} + \textit{mobile\_provisionsD}\}.$$

As mentioned in the text, larger values indicate a more stringent regulatory environment, so the first two components of the LCAI are transformed as shown above to preserve this interpretation.

General Residential Zoning Index (GRZI). The GRZI provides a measure of the overall stringency of zoning regulation with respect to residential development. While the RSRI captures the extent to which individual dwellings are regulated, the GRZI captures general restrictions on residential development in the community. This index is comprised of indicators for minimum density requirements, infill development requirements, whether the jurisdiction has adopted growth management element in its General Plan, and an ordinal variable for the maximum density permitted in the highest density zone. This index is computed as:

$$\text{GRZI} = \text{STD}\{\text{STD}(\textit{min\_densD} + \textit{infillD} + \textit{growth\_mgmtD}) + \text{STD}(\textit{max\_dens})\}.$$

As mentioned in the text, variables measured on different scales are standardized in order to be combined in a meaningful way.

Political Tension Index (PTI). The PTI measures the local political activity level concerning land-use decisions and the extent to which these decisions are perceived as controversial. More precisely, this index captures the survey respondents' perception of the local political milieu with regard to land-use issues. Although a more objective measure may be more apt for this component, it is likely that, given the strong role local politics play in land-use decisions (particularly in California), survey respondents should be able to provide reliable information in this regard. The first set of components that make up this index includes ordinal variables (five point Likert scale) indicating how important the respondents feel the following issues are in constraining residential growth: The cost of new infrastructure, citizen opposition to growth, school crowding, sewer capacity limits, city budget constraints. The index also includes an ordinal variable (four point Likert scale) indicating how controversial the respondents' perceive residential growth issues to have been in the recent past, as well as an indicator for whether property owners have appealed regulatory attempts to encourage more housing. The PTI is computed as follows:

PTI = STD{STD(*imp_infra* + *imp_cit_opp* + *imp_schl_crowd* + *imp_sewer* + *imp_budget*) + STD(*controversial*) + STD(*appealD*)}

Non-Residential Building Limitations Index (NBLI). Similar to the BLI, the NBLI measures supply restrictions on non-residential development (i.e., industrial and commercial buildings). This index combines indicators for limitations on the amount of square footage that can be built in a given time frame for commercial and industrial development. The index is calculated as:

NBLI = STD{*sqft_commD* + *sqft_industD*}.

Affordable Housing Index (AHI). The AHI reflects the presence of restrictions that require residential developers to provide affordable housing as a condition to project approval. This index is calculated as follows:

AHI = STD{*afford_reqD*}.

Appendix 3.C
Variable Descriptions

Variable name	Sub-Index	Definition	Code
mobile_allowedD	Low Cost Alternative Index (LCAI)	Mobile or manufactured homes allowed in jurisdiction	0 = no; 1 = yes
mobile_moveD	LCAI	Mobile homes allowed outside mobile home parks	0 = no; 1 = yes
mobile_minsizeD	LCAI	Mobile homes must meet minimum size or width requirements	0 = no; 1 = yes
mobile_provisionD	LCAI	Mobile homes must meet specific provisions	0 = no; 1 = yes
LCAI = STD{(1 – *mobile_allowedD*) + (1 – *mobile_moveD*) + *mobile_minsizeD* + *mobile_provisionsD*}			
maxhtD	Residential Structure Restriction Index (RSRI)	Maximum building height requirements for single-family (SF) units	0 = no; 1 = yes
garage_reqD	RSRI	Garages required for SF units	0 = no; 1 = yes
min_sqftD	RSRI	Minimum square footage requirements for SF units	0 = no; 1 = yes
farD	RSRI	Floor area ratio restrictions	0 = no; 1 = yes
RSRI = STD{*maxhtD* + *garage_reqD* + *min_sqftD* + *farD*}			
min_densD	General Residential Zoning Index (GRZI)	Minimum residential density requirements	0 = no; 1 = yes
infillD	GRZI	Restriction of residential development to areas that are already developed	0 = no; 1 = yes
growth_mgmtD	GRZI	Adoption of a growth management element to General Plan	0 = no; 1 = yes
max_dens	GRZI	Maximum density (per acre) permitted in the highest density zone	0 = No maximum specified; 1 = Over 30 units; 2 = 16-30 units; 3 = 8-15 units; 4 = 5-7 units; 5 = 1-4 units; 6 = Less than 1 unit per acre
GRZI = STD{STD(*min_densD* + *infillD* + *growth_mgmtD*) + STD(*max_dens*)}			
imp_infra	Political Tension Index (PTI)	Importance in constraining residential growth – cost of new infrastructure cost	1 = Not at all important; … 5 = Very important
imp_cit_opp	PTI	Importance in constraining residential growth – citizen opposition	1 = Not at all important; … 5 = Very important
imp_schl_crowd	PTI	Importance in constraining residential growth – school crowding	1 = Not at all important; … 5 = Very important

imp_sewer	PTI	Importance in constraining residential growth – sewer capacity limits	1 = Not at all important; … 5 = Very important
imp_budget	PTI	Importance in constraining residential growth – city budget constraints	1 = Not at all important; … 5 = Very important
controversial	PTI	In the past and more recent periods, how controversial are residential growth issues in the jurisdiction?	1 = Not at all controversial; … 4 = Almost always controversial
appealD	PTI	Property owners appealed regulatory attempts to encourage more housing	0 = no; 1 = yes

PTI = STD{STD(*imp_infra* + *imp_cit_opp* + *imp_schl_crowd* + *imp_sewer* + *imp_budget*) + STD(*controversial*) + STD(*appealD*)}

voter_appD	Development Uncertainty Index (DUI)	Voter approval required for some or all zoning changes	0 = no; 1 = yes
supermaj_appD	DUI	Supermajority council vote required for some or all zoning changes	0 = no; 1 = yes
num_boards	DUI	Number of boards or regulatory bodies immediate to local jurisdiction that must grant permission or preliminary approval before a typical SF development is approved (apart from the body that grants preliminary plat/plan approval)	0 = None; 1 = One; 2 = Two or three; 3 = Four or five; 4 = More than five

DUI = STD{STD(*voter_appD* + *supermaj_appD*) + STD(*num_boards*)}

freq_permit_mtg	Regulatory Delay Index (RDI)	How many times a month (including special meetings) permit-granting entity typically meets to consider development applications	0 = More than four times; 1 = Four times; 2 = Three times; 3 = Twice; 4 = Once; 5 = Less than once a month
sf_time	RDI	How long to obtain approval for single-family projects where no rezoning, zoning amendments, bulk variance etc. is required	0 = N/A or Unsure; 1 = Less than 2 months; 2 = 2-6 months; 3 = 6-12 months; 4 = 1-2 years
mf_time	RDI	How long to obtain approval for multi-family projects where no rezoning, zoning amendments, bulk variance etc. is required	0 = N/A or Unsure; 1 = Less than 2 months; 2 = 2-6 months; 3 = 6-12 months; 4 = 1-2 years

town_time	RDI	How long to obtain approval for townhouse development projects where no rezoning, zoning amendments, bulk variance etc. is required	0 = N/A or Unsure; 1 = Less than 2 months; 2 = 2-6 months; 3 = 6-12 months; 4 = 1-2 years
imp_review_proc	RDI	Importance in constraining residential growth – review process	1 = Not at all important; … 5 = Very important
imp_staff	RDI	Importance in constraining residential growth – lack of personnel to review projects	1 = Not at all important; … 5 = Very important

RDI = STD{STD(*freq_permit_mtg*) + STD(*sf_time* + *mf_time* + *town_time*)/3 + STD(*imp_review_proc* + *imp_staff*)}

bldglimitD	Building Limitations Index (BLI)	Permit cap for residential units	0 = no; 1 = yes
mflimitD	BLI	Permit cap for multi-family dwellings	0 = no; 1 = yes
poplimitD	BLI	Population growth cap	0 = no; 1 = yes
ugbD	BLI	Urban growth boundary	0 = no; 1 = yes

BLI = STD{*bldglimitD* + *mflimitD* + *poplimitD* + *ugbD*}

sqft_commD	Non-Residential Building Limitations Index	Limit on square footage that can be built for commercial development	0 = no; 1 = yes
sqft_industD	NBLI	Limit on square footage that can be built for industrial development	0 = no; 1 = yes

NBLI = STD{*sqft_commD* + *sqft_industD*}

afford_reqD	Affordable Housing Index (AHI)	Affordable housing required as a condition to project approval	0 = no; 1 = yes

AHI = STD{*afford_reqD*}