Rent Control and the Supply of Affordable Housing

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Abstract

We generate the first cross-city panel dataset of rent control reforms and estimate their effect on rental housing supply across varying levels of affordability. To generate the reform data, we use machine learning algorithms to analyze over 76,000 newspaper articles from 7,000 news outlets spanning 27 metropolitan areas and over 4,000 census designated places across the US between 2000 and 2021. We then combine these data with the number of rental units within each city that are affordable to households of different incomes, created using Census microdata. Finally, we estimate the effect of rent control reforms on the supply of rental units using a two-way fixed effects model with place specific time trends and a difference-in-differences model with multiple treatment periods, following the approach outlined by Callaway and Sant'Anna. We find that rent control is associated with a reduction in the total number of rental units in a city. Stratifying our models by affordability, we estimate an increase in the availability of rental units affordable to extremely low-income households, offset by a decline in units affordable to higher income brackets. Our findings underscore the dynamics at play in the implementation of rent control regulations, suggesting differential impacts across income segments and highlighting the nuanced trade-offs inherent in such policy interventions.

DISCLAIMER: Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503).

Keywords: affordable housing, housing, land use, zoning
JEL Classification Numbers: R14, R31, H7

1 Introduction

Rent control is a long-standing and controversial policy. It was first implemented in the United States during World War II as a hard ceiling on rents (Arnott 1995; Fetter 2016). Over time, rent control evolved into a less stringent and more nuanced regulatory scheme. Modern rent control, also known as second-generation rent control or rent stabilization, generally involves a cap on annual rent increases, along with a set of accompanying regulations such as vacancy decontrol, condominium conversion regulation, hardship clauses, and exemptions based on building type and age (Been, Ellen, and House 2019).[9] Rent control has two primary goals: preventing rapid rent increases and keeping qualified units below market rates. Rent control has also been used to address concerns surrounding speculative real estate investment, maintaining economic and racial diversity, and providing renters with housing habitability and stability (Chew and Treuhaft 2019).

In recent years, rent control regulations have reemerged as a potential solution to the housing affordability crisis. Seven states have rent control policies in place at the state or local level, and more than 200 municipalities had some form of rent regulation in 2022 (Rajasekaran, Treskon, and Greene 2019; National Apartment Association 2024). Concurrently, many existing rent control policies have been repealed, weakened, or banned through local and state-level legislations. For instance, Montana and Florida preempted rent control in 2023, and Ohio preempted it in 2022 (National Apartment Association 2023).

Rent control policies are complicated and controversial due to mixed findings demonstrating positive, negative, or neutral effects on housing outcomes. For instance, Heskin, Levine and Garrett (2000), Diamond, McQuade, and Qian (2019), and Asquith (2019) find that the implementation of rent regulation decreases the existing rental supply, while Goetz (1995), Gilderbloom and Markham (1996), Glaeser (2002), Gilderbloom and Ye (2007), Sims (2007), Gaffney (2021) and Jofre-Monseny, Martinez-Mazza, and Segu (2023), and Ambrosious et al. (2015) find no significant effect or mixed effects on the quantity of rental housing stock or construction. The heterogeneity in findings are predominantly driven by the assumptions made in each study, the outcomes of interest, the institutional features of the policy, and the local housing market conditions where they are implemented (Been, Ellen, and House 2019; Gibb, Marsh, and Soaita 2022; Rajasekaran, Treskon, and Greene 2019). For instance, some rent control policies include vacancy decontrol, which allows landlords to raise rents to the market rate or exclude units from regulation after a vacancy, while others ban it, each yielding different results (Nagy 1997; Stacy et al. 2021).

[9] Although we use the terms rent control and rent stabilization interchangeably throughout this article, there are important differences between the two. Traditional rent control policies capped the rent landlords could charge for protected units. Rent stabilization allows rent to increase each year, with the increase limited and applied to the prior year's rent. Modern rent control policies often mimic rent stabilization, allowing for periodic increases to rent based on a predetermined formula.

The current body of research is also limited in geographical scope. Most of the literature focuses on only a few well-known rent-controlled jurisdictions or jurisdictions within a single state. New York City received much attention in the 1970s and 1980s, but there have been no empirical studies based on New York since 2000. Washington, DC — a large city that has had rent control for the last three decades — has received virtually no attention in the literature. Even for areas that have been studied thoroughly, there is virtually no data on the changes in rent control laws over time. For example, cities within New Jersey saw their rent control regimes watered down in a series of small changes throughout the early 2000s, making the policy significantly less restrictive (Ambrosius et al. 2015).

To begin to fill this gap and to better understand the effect of rent control across a wide variety of housing markets and policy regimes, we undertake the first cross-city panel analysis examining the effect of rent control reforms on rental housing supply across twenty-seven US metropolitan areas, encompassing over than 4,000 cities. To examine the relationship between rent control policies and rental housing supply, we generate two novel datasets. First, we use a machine-learning approach to identify rent control and rent stabilization reforms across multiple areas. Recognizing that rent control reforms across jurisdictions can vary in intent and effect, we also identify and separate reforms by those considered more and less restrictive. We define more restrictive reforms as those increasing ownership costs for landlords or creating more favorable conditions for tenants, and less restrictive reforms as those doing the opposite.

Our second novel dataset is a detailed dataset on the rental housing supply across affordability levels in Census-designated places (hereafter cities). We are able to identify affordability levels using Census microdata, specified as the number of rental housing units – both vacant and occupied – within each affordability range based on the Department of Housing and Urban Development's (HUD) measure of Area Median Income (AMI). Specifically, we use rental housing counts affordable to households that make less than 30% of AMI (extremely low-income households), those affordable to households that make between 30 and 50% of AMI (very low-income households), those affordable to households that make between 50 and 80% of AMI (low-income households), those affordable to households that make between 80 and 100% of AMI (moderate income households), those affordable to households that make between 100 and 120% (upper moderate income households), and those affordable to households that make above 120% of AMI (higher income households). Our intent is to not only examine how differences in rent control policies affect supply overall (e.g., more vs. less restrictive), but also the effect of these different policies across the spectrum of rental prices.

We utilize both fixed effects with place specific time trends and difference-in-differences with multiple treatment periods to estimate the effect of rent control reforms on the supply of rental units across affordability levels. We find that rent control is associated with a reduction in the total number of units in a city. This confirms theoretical expectations regarding the impact of rent control on total supply. However,

we are also able to determine whether all income levels are affected uniformly by focusing on the number of rental units affordable at different AMI's. We find this not to be true: rent control is associated with statistically significant increases in units affordable to extremely low-income households (those making less than 30% of AMI), with decreases to the total number of units driven by reductions to those affordable to people with incomes above 120% of AMI.

Our findings suggest that rent control may be able to increase affordability for people at the very low end of the rental distribution, but longer-term impacts on affordability driven by supply are unclear. If rent control reduces supply enough that it drives up prices overall, it may have detrimental effects on people with extremely low incomes in the long run. In addition, if rent control reduces the supply of rental units affordable to higher income households, there may be a filtering up effect where higher income households occupy units that are affordable to lower income households. Nonetheless, more research is needed to determine these long run impacts, and which types of rent control regimes are most effective at increasing affordability while minimizing negative impacts on overall supply.

2 Literature on Rent Control and Housing Supply

Rent control and rent stabilization have been studied extensively by academics and other researchers, with a recent resurgence in interest in the wake of the housing affordability crisis and newly enacted rent control policies across the US.[10] For brevity, we focus here on studies examining the relationship between rent control, housing supply, and affordability.

2.1 Supply

From a theoretical perspective, there is a growing consensus among economists that rent control decreases the availability of rental housing in both the short and long term (Kearl et al. 1979; Alston, Kearl, and Vaughan 1992; Arnott 1995; Jenkins 2009). In the short term, landlords with property subject to rent control laws may opt to avoid these laws by either converting or redeveloping their property into other uses. This stems from specific requirements on the types of property subject to rent control embedded in the

[10] Research examining rent control is extensive, with a large number of topics outside the purview of the current study. Such topics include the effects of rent control on: different income groups or the racial mix of an area (Ault and Saba 1990; Early 2000; Gilderbloom and Ye 2007; Glaeser 2002; Gyourko and Linneman 1989; Heskin, Levine, and Garrett 2000; Levine, Grigsby, and Heskin 1990; Linneman 1987; Olsen 1972; Sims 2007, 2011), commute times (Krol and Svorny 2005), unemployment duration (Svarer, Rosholm, and Munch 2005), property maintenance or quality (Breidenbach, Eilers, and Fries 2022; Gilderbloom and Ye 2007; Gyourko and Linneman 1989; Mengle 1985; Moon and Stotsky 1993; Sims 2007), mobility (Ault, Jackson, and Saba 1994; Bonneval, Goffette-Nagot, and Zhao 2022; Clark and Heskin 1982; Karpestam 2022; Munch and Svarer 2002; Nagy 1995), tenure (Arnott 2003; Ault and Saba 1990; Gardner 2022), rental rates (Autor, Palmer, and Pathak, 2014; Diamond, McQuade, and Qian 2019; Caudill 1993; Early 2000; Early and Phelps 1999; Fallis and Smith 1984; Gilderbloom and Markham 1996; Gyourko and Linneman 1989; Heskin, Levine, and Garrett 2000; Malpezzi 1996; Nagy 1997; Sims, 2007), and vacancy (Rapaport 1992). For a succinct overview of rent control research, we recommend Sturtevant (2018), Nelson (2024), and Kholodilin (2024).

policy. For example, landlords could convert rent-controlled units into condominiums, which are usually exempt from rent control, thereby removing rental units from the market. In the long run, theory suggests there will be a decrease in the production of rental housing. This could result from rent control exerting downward pressure on market rents, making development less profitable, or developers anticipating future regulations that might subject new units to rent control. In such scenarios, developers may be deterred from building new housing altogether if investment strategies incorporate anticipated rent regulations into the valuation for new development. Newer policies are beginning to require that new construction be subject to rent control on a rolling basis, which may have an even larger detrimental effect on new development (e.g., Oregon exempts new units from state rent regulation for 15 years).

Empirically, Heskin, Levine and Garrett (2000) provided an early examination of the changes in the number of rental units after rent control was enacted in four California cities between 1980 and 1990. An important component of these rent regulations was the inclusion of vacancy control, a restriction that requires rent control to remain even when new tenants occupy the rent-controlled unit. Their findings showed the implementation of rent regulation with vacancy control reduced the number of rental units by 7%, with some units converted to owner property. Similarly, Diamond, McQuade, and Qian (2019) and Asquith (2019) find that rent control policies in San Francisco incentivized landlords to decrease the existing rental supply by converting units to condominiums or redeveloping buildings (and their use) throughout the city. In particular, Diamond, McQuade, and Qian estimate apartment buildings under rent control were 8% more likely to be converted to condominiums than uncontrolled buildings and landlords reduced the total supply of available rental housing by 15%. On the opposite coast, Sims (2007, 2011) examined the elimination of rent regulation in three Massachusetts cities in 1995: Boston, Cambridge, and Brookline; areas where rent control regulations were removed had a 7% greater chance of having housing units become rental units than uncontrolled areas, increasing rental housing choices in these areas.

There is evidence that the link between rent control and supply reductions are not as clear as the aforementioned research indicates. For example, Goetz (1995) concluded that multifamily housing production increased after rent control was introduced in San Francisco, although these findings have been subject to scrutiny based on the techniques employed (Kholodilin and Kohl 2023). Gilderbloom and Markahm (1996) surveyed 60 New Jersey cities and found no significant effect on the quantity of rental housing stock and Glaeser (2002) found mixed results concerning statistical significance in New Jersey and California. Glaeser's findings were dependent on controlling for city size. In later research comparing jurisdictions in New Jersey with and without rent control, Gilderbloom and Ye (2007) and Ambrosious et al. (2015) found no relationship between rent control and the number of units constructed. As a separate view, Sims (2007) compared construction rates before and after the end of rent control in three Massachusetts localities and found the policy did not significantly affect short-term construction rates.

More recently, Gaffney (2021) and Jofre-Monseny, Martinez-Mazza, and Segu (2023) find that rent control policies did not yield any changes in the supply of rental units in East Palo Alto, California, and Catalonia, Spain, respectively.

Given the empirical findings, it is becoming clearer that there is nuance in rent control and stabilization policies and their effects may vary based on policy design. For example, older policies exempted new construction while some of the newer policies provide a grace period for newly constructed units before their rents are stabilized. The research literature is mostly based on older policies that completely exempt new construction from rent stabilization. These heterogeneous findings could also be attributed to the choice of housing supply measurement. While it is common to analyze the effect of rent control on overall construction rates or changes to the current housing stock, the effect of these policies on different types of rental units is important as well.

2.2 Affordability

We are also interested in how rent control affects the supply of rental housing across the distribution of affordability levels. Theory predicts that rent control will decrease the availability of rental housing in both the short and long term, but that it may increase the number of units affordable to people with low incomes (Arnott 1995; Jenkins 2009). Although not directly related to the supply of affordable units, a number of the aforementioned studies indirectly examine the effects of rent control on affordability. Empirically, rent control has been shown to lower rent for tenants in controlled units (Autor, Palmer, and Pathak 2014; Gyourko and Linneman 1989; Diamond, McQuade, and Qian 2019; Sims 2007) and increase the rents for uncontrolled units (Caudill 1993; Early 2000; Fallis and Smith 1984). There have also been a few studies measuring how the nuance of rent control policies affects these outcomes. For example, Heskin, Levine, and Garrett (2000) conclude that vacancy control contributed to lower rents when compared with non-vacancy-controlled areas, and Autor, Palmer, and Pathak (2014) showed decontrolled units experienced increases in market value. Finally, Glaeser (2002) found evidence supporting the notion that rent control provides low rent options in expensive cities. It is not clear whether these were the result of supply changes or if the policy was simply maintaining units at low rent levels per implemented guidelines.

2.3 Contribution

Our paper extends the previous literature in two important ways. First, while other studies focus on rent control's effects for, at most, a few cities at a time, there is currently a dearth of research examining the effects of rent control across different cities, making broad statements about the overall efficacy of rent control unproductive at best. We undertake the first cross-city panel analysis examining the effect of rent control reforms on rental housing supply across twenty-seven US metropolitan areas. Related to increasing the geographic focus, we identify rent control policies with nuance not separately examined in previous research. We focus on more restrictive policies, where we define more restrictive as reforms that made rent

control requirements more beneficial for renters and less beneficial for landlords, and use a machine-learning approach to identify reforms across the US to examine overall effects in multiple cities simultaneously.[11]

Our second contribution is our examination of the impact of rent control reforms on the distribution of rental units affordable to households of different incomes, rather than focusing solely on the overall supply of rental units. This is an important consideration; overall rental units may decrease, but there may still be increases for different affordability bands. A common criticism of rent control is that it shrinks the supply of rental units, encouraging landlords to convert rental stock to condos or making developers less inclined to build new housing. We not only recognize a potential reduction in overall housing supply, but we are also interested in understanding which types of units the reduction affects. Understanding if there are different effects across this distribution of affordable units provides insight into whether rent control may have positive effects to those for whom the program may be targeted.

3 Data & Descriptive Statistics

We generate two novel datasets to estimate the effect of rent control reforms on housing supply: 1) data on rent control reforms collected using machine learning algorithms to analyze newspaper articles for 27 metropolitan regions between 2000 and 2021; and 2) data on the number of rental units within each city that are affordable to households of different incomes, created using Census microdata at the Federal Statistical Research Data Center (RDC). We also use one supplementary dataset to create a Bartik instrument for our Callaway and Sant'Anna regressions. Each dataset and its collection process is discussed below.

3.1 Rent Control Reforms

To generate a dataset of rent control reforms, we build on the work of Stacy et al. (2023) by using natural language processing to scan a comprehensive database of major newspapers across the US and identify reforms. This is similar to the data-generating approach of textual data proxies utilized in other studies culling newspapers to examine urban phenomena (Ginsberg et al. 2009; Saiz and Simonsohn 2013). We first selected metropolitan regions (and the cities within them) based on news coverage in the NewsBank database, prioritizing regions with greater coverage and higher population growth. It is reasonable to expect regions with larger populations (positively correlated with more news coverage) and higher population growth to have a greater likelihood of experiencing affordability challenges and therefore to undergo rent control reform. We also considered regions based on rent control restrictions. As of 2020, 31 states had preempted local use of rent control. Metropolitan regions within these states were excluded from our analysis. With these considerations, we accessed raw text for approximately 7,000 local news

[11] We also collected data on less restrictive reforms, but there were a limited number of reforms that created less restrictive rent-control policies during our period of analysis (14), making the power too low to report those estimates.

outlets in the NewsBank database and assigned each newspaper to their respective 27 US metropolitan regions. Table 1 highlights the metropolitan regions included, as well as the newspaper count in the database for each region.

Table 1: Metropolitan Regions Included in Analysis

Metropolitan Region	Newspaper Count
Atlanta-Sandy Springs-Alpharetta, GA	78
Baltimore-Columbia-Towson, MD	47
Birmingham-Hoover, AL	21
Boston-Cambridge-Newton, MA-NH	215
Chicago-Naperville-Elgin, IL-IN-WI	270
Denver-Aurora-Lakewood, CO	43
Durham-Chapel Hill, NC	16
Los Angeles-Long Beach-Anaheim, CA	142
Miami-Fort Lauderdale-Pompano Beach, FL	83
Minneapolis-St. Paul-Bloomington, MN-WI	69
New Orleans-Metairie, LA	25
New York-Newark-Jersey City, NY-NJ-PA	336
Ocean City, NJ	6
Oxnard-Thousand Oaks-Ventura, CA	7
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	182
Pittsburgh, PA	48
Portland-Vancouver-Hillsboro, OR-WA	37
Richmond, VA	14
Riverside-San Bernardino-Ontario, CA	35
Sacramento-Roseville-Folsom, CA	21
San Diego-Chula Vista-Carlsbad, CA	33
San Francisco-Oakland-Berkeley, CA	81
San Jose-Sunnyvale-Santa Clara, CA	29
Seattle-Tacoma-Bellevue, WA	64
Tallahassee, FL	8
Trenton-Princeton, NJ	12
Washington-Arlington-Alexandria, DC-VA-MD-WV	87

Source: Author analysis of NewsBank Data.

Next, we narrowed the full set of articles in the database to those published between 2000 and 2021, and used a search string to identify each article containing information related to rent control. These years were selected based on coverage (sparse prior to 2000) and timing (data collection occurred in 2021). In consultation with the research team and a group of external experts in housing advocacy, policy, and research, we chose keywords or phrases for our search string that would encompass all rent control reforms. Some of the most frequently used words and phrases related to rent control reform included “rent

regulation," "rent ceiling," "rent controlled," "rent stabilization," "rent cap," "rent board," as well as phrases related to specific types of reforms (e.g., "permitted rent increases," "eviction and harassment protections," "vacancy decontrol," etc.). We also combined these into larger search terms, adding "AND" and "OR" statements. After identifying possible search terms relevant to rent control reform passage, we tested the search terms on news outlets from a random subset of metropolitan areas where the "true" rent control reforms were already known. This allowed us to identify combinations of search terms that maximized coverage of all true reforms while minimizing inclusion of extraneous articles. Our final search string extracted 2,957 articles from the NewsBank database.

We then used machine learning techniques to identify articles in our extract that not only discuss hypothetical or proposed rent control and related reforms, but which reforms were actually passed in a jurisdiction. We trained a team of four manual taggers with a background in housing policy to tag 501 randomly selected articles. If the article represented a true reform, the taggers would record when and where the reform took place, the geographic extent to which the reform applied, and if the reform made the regulation more or less restrictive. More restrictive reforms are those increasing ownership costs for landlords or creating more favorable conditions for tenants (e.g., reduced caps on allowable rent increases, barring landlords from passing improvement costs to renters, implementing just cause eviction rules, enacting landlord appeal restrictions, instituting rent control boards, or implementing vacancy control);[12] less restrictive reforms work in the opposite direction, including the implementation of vacancy decontrol or the allowance of higher annual caps on changes to rent. We then split the tagged data into two datasets: a "training set" the machine learning model would use to "learn" how to identify relevant reforms in the articles, and a "testing set" excluded from the training set and used to evaluate the model's performance on unseen data.

In addition to the words, phrases, and combinations discussed above, our training set considered the proximity of keywords to each other and the presence of words differentiating reforms. We also tested different machine learning algorithms and model hyperparameters to identify the model specification that maximized the percentage of true reforms identified in the training data.[13] Our final model, a random forest model, classified whether or not the remaining un-tagged articles contained information about a successful

[12] Just cause eviction requires evidence of nonpayment or substantial breach of the tenant's lease for rent-controlled units to be evicted. Landlord appeal restrictions prevent landlords from appealing legislative or policy decisions regarding rent stabilization measures. Vacancy control requires the landlord to keep rent control in place at the same level after a tenant is replaced. Finally, rent control boards are an established group to enforce programs and hear disputes between landlords and tenants.

[13] We used grid search to test a variety of hyperparameters for naive bayes, support vector classifiers, and random forest models using the Python sklearn library. We also used fast.ai to train an AWD-LSTM deep learning model (see Merity, Keskar, and Socher 2018). We selected the model-hyperparameter combination that maximized training recall, or the number of true reforms identified in the training data.

rent control reform. Finally, the results were cross-validated by a team of land-use experts (including the authors of this study) and deduplicated using data cleaning techniques. Team members read each article and corrected any remaining errors generated by the machine-coded data, providing confidence in our final results.

Table 2 displays the number of rent control reforms by metropolitan statistical area (MSA), and Figure 1 highlights the timing of reform implementation.[14] As shown in the figure, the majority of identified reforms were in the latter half of the 2010s. This coincides with housing market changes across the US and the growing discussion concerning affordability. Likewise, the table highlights reforms in places where affordability concerns were likely the greatest during this period (e.g., California and New York).

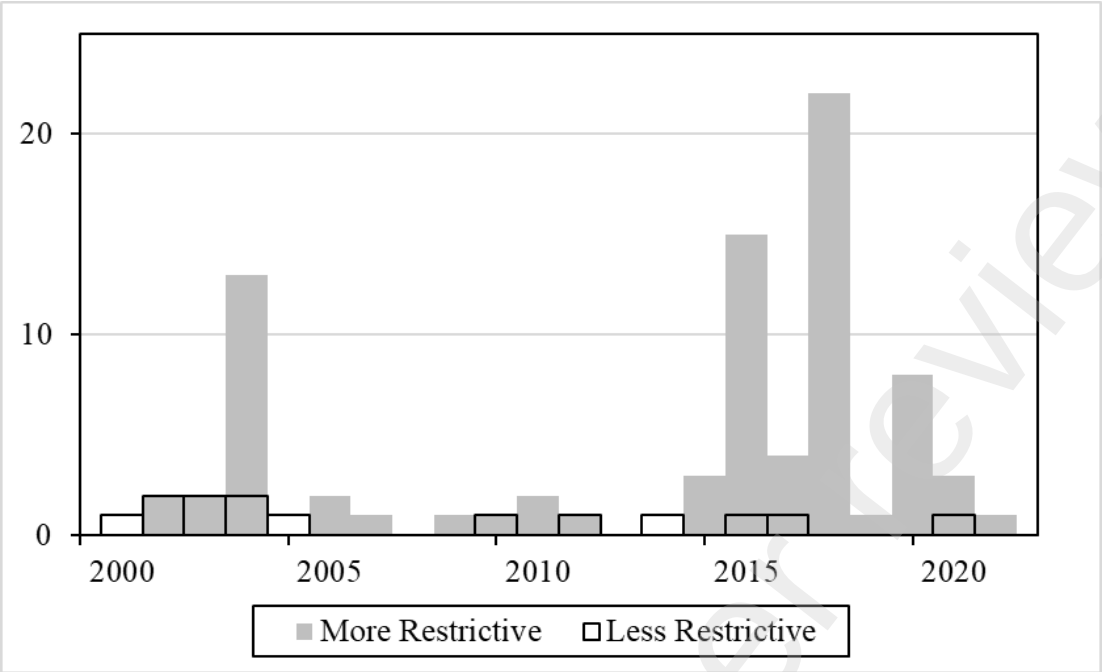
Table 2: Rent Control Reforms and Type by MSA

	Frequency		
	Total Reforms	**More Restrictive**	**Less Restrictive**
Boston-Cambridge-Newton, MA-NH	1	1	0
Los Angeles-Long Beach-Anaheim, CA	9	9	0
New York-Newark-Jersey City, NY-NJ-PA	13	13	8
Sacramento--Roseville--Arden-Arcade, CA	1	1	0
San Diego-Carlsbad, CA	3	2	1
San Francisco-Oakland-Fremont, CA	57	53	4
San Jose-Sunnyvale-Santa Clara, CA	2	2	0
Trenton-Princeton, NJ	1	0	1
Washington-Arlington-Alexandria, DC-VA-MD-WV	1	1	0
Total	96	82	14

Source: Author analysis of NewsBank Data.

[14] It is worth noting that in the article reading process, team members identified 7 counties that instituted a reform. In these cases, one of the members contacted the counties for further detail and the reform was assigned to all places within the county that would be affected by it.

Figure 1: Rent Control Reform Types by Year



Source: Author analysis of NewsBank Data.

3.2 Census Microdata

To conduct our analysis, we needed a measure of housing affordability that reflects both the number and affordability of rental units in cities over time. Unfortunately, accurate measures concerning the number of affordable housing units over time are not widely available. While the Census Bureau offers information related to the number of households that pay within specified rental bins, these do not include rents high enough to approximate affordable units in high-cost areas. For example, 80 percent of Area Median Income (AMI) in New York City in 2020 was $90,950, which would equate to an affordable rent of $2,274.[15] However, the highest bin publicly available for the years in our study is $2,000.[16] Even if higher bins were available, inflation may prevent comparison between the number of units in a bin for a given year with the number of units within the same bin for other years. In addition, the publicly available data do not account for the number of bedrooms in each rental unit, making accurate counts of affordability difficult. For example, a studio apartment may appear affordable or a four-bedroom apartment may appear unaffordable without considering for the potential makeup of renters based on unit size. Finally, the data do not contain rental information at the level of granularity needed to generate estimates for cities over time using

[15] Homes and Community Renewal, NY State.gov. 2020. "Eligible Income Limits (80% AMI), NYS COVID Rent Relief Program 2020." https://hcr.ny.gov/system/files/documents/2020/07/crrp2020_eligible_income_80ami.pdf

[16] AMI is defined by the US Department of Housing and Urban Development's Section 8 Voucher program requirements.

consistent boundaries.

We also considered a special dataset HUD receives and publishes from the Census called the Comprehensive Housing Affordability Strategy (CHAS) data. These data show the number of units affordable to people at different income levels up to 80% of AMI. Using Census microdata allowed us to create a more precise and comprehensive dataset, since CHAS data are often rounded or suppressed for smaller places and the data are frequently missing for the year 2000. In addition, we were able to observe the full distribution of units by affordability using the microdata, while the CHAS data only include units up to 80% of AMI. Finally, using microdata allowed us to maintain constant boundaries over time, rather than including units in places as they change as areas are annexed or de-annexed.

As alluded, we overcame the challenges with the publicly available datasets by obtaining approval to access the restricted Census microdata at the Federal Statistical Research Data Center (RDC) and generating the appropriate metrics on affordable housing units. In the RDC, we merged Section 8 income limit data with rental data, including vacant rental units, from the 2000 Decennial Census and each five-year American Community Survey (ACS) available when the data was collected (2005-2009 to 2017-2021) at the county subdivision level. We assign each data point to the middle year of the corresponding ACS. Since affordability thresholds vary by family size (i.e., AMI data developed by HUD sets different thresholds based on the number of family members that live within the unit), we use family size to adjust for the capacity of the unit as a more accurate measure of rental unit capacity. We then calculated gross rent using electricity, natural gas, water, fuel, and rent, and compared these to the Section 8 income limits to identify units within the AMI thresholds. This was more complicated for vacant rental units; we approximated the number of people that might live in the unit using the number of bedrooms and applied the percentage of occupied units by AMI level to the total number of vacant units to create an estimate of the number of vacant rental units by affordability bucket. Finally, we added occupied rental units and vacant rental units together to get the total number of units by AMI level, aggregating to the place by year level, and linked these data to our MSAs using an MSA-to-place crosswalk.

Table 3 shows the total number of housing units, the total number rental units (occupied or vacant), the number of rental units by affordability level from the Census microdata by reform type, as well as the number of units per household by affordability levels. Places that never had a reform have a greater ratio of housing units to rental units than those with a reform, but places having instituted a reform of any type tend to have more housing and rental units than those without an RC reform. These are unsurprising and coincide with our previous statement, “It is reasonable to expect regions with larger populations (positively correlated with more news coverage) and higher population growth to have a greater likelihood of experiencing affordability challenges and therefore to undergo rent control reform.” However, it is important to recognize these differences could be generated from the data collection process itself. Smaller cities are less likely to

offer robust news coverage and thus were not covered. Despite these differences, there are important similarities between each of these groups; the largest category for each group is the number of units affordable to people making between 50% and 80% of AMI while the smallest category is the number of units affordable to people making between 100% and 120% of AMI. Furthermore, while places that had reforms were much larger than places that did not have reforms, the number of units per person within each bucket were more similar. In total, there are no striking or apparent differences in the provision of affordable units for places with RC reforms in comparison to places without a reform.

Table 3: Baseline Number of Housing Units by Affordability Level by Whether a city ever had a Rent Control Reform, 2000

Means of:	Never had a reform	Had a more restrictive reform	Had a less restrictive reform
Total number of housing units	6,187	116,800	274,700
Total number of rental units (vacant or occupied)	2,377	75,020	188,700
Number of units affordable to people making less than 30% of AMI	229	7,887	22,730
Number of units per household affordable to people making less than 30% of AMI	0.03	0.04	0.04
Number of units affordable to people making between 30% and 50% of AMI	553	12,000	29,420
Number of units per household affordable to people making between 30% and 50% of AMI	0.08	0.08	0.09
Number of units affordable to people making between 50% and 80% of AMI	1,013	29,910	74,940
Number of units per household affordable to people making between 50% and 80% of AMI	0.11	0.15	0.23
Number of units affordable to people making between 80% and 100% of AMI	239	10,490	24,850
Number of units per household affordable to people making between 80% and 100% of AMI	0.02	0.05	0.04
Number of units affordable to people making between 100% and 120% of AMI	87	4,520	9,772
Number of units per household affordable to people making between 100% and 120% of AMI	0.01	0.03	0.01
Number of units affordable to people making greater than 120% of AMI	257	10,210	27,030
Number of units per household affordable to people making greater than 120% of AMI	0.04	0.05	0.06

Notes: Estimates derived from American Community Survey Microdata from the Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503)

3.3 Longitudinal Employer-Household Dynamics (LEHD)

Finally, we use data from the Longitudinal Employer-Household Dynamics (LEHD) to generate a Bartik instrument to use as a control variable in our Callaway and Sant'Anna regressions to account for exogenous changes in housing demand and prices. LEHD is a program of the US Census Bureau that compiles tabulated and modeled administrative data on employers and employees from states, censuses, and surveys to estimate employment, earnings, and job flows at detailed levels of geography. We use LEHD Origin-Destination Employment Statistics (LODES) which contains information on the total number of jobs in tracts where people work. In the shift-share variable creation we use the 2-digit NAICS codes (20 industries) and a base year of 2002.

4 Empirical Model

To estimate the effect of rent control reforms on the supply of rental housing at different levels of affordability, we use counts for various affordability brackets at the city level as our dependent variable, as described above and shown in Table 4. The following equation serves as our baseline model:

$$Outcome_{it} = \rho Implementation_{it} + \beta PostImplementation_{it} + \lambda_t + \delta_i + \tau_{it} + \varepsilon_{it}$$

where $Outcome_{it}$ is the inverse hyperbolic sin of the count of rental housing units in city *i* during year *t*, both overall and stratified based on affordability levels, as defined by HUD for the Section 8 voucher program. We take the inverse hyperbolic sin of the outcome measures to account for the fact that they cannot be negative, that there are zeros within some supply buckets, and that there is overdispersion (Chen and Roth 2024). We use the 2000 decennial census and ACS 5-year micro data, where the outcome variables for each year *t* is the middle year of the 5-year sample. For example, the ACS 2015-19 sample is specified as city *i* in year 2017. We also include time fixed effects (λ_t) for each calendar year to control for aggregate shocks affecting all places, place fixed effects (δ_i) to account for time-invariant heterogeneity, and place by year fixed effects (τ_{it}) to account for place-specific linear trends. Standard errors are clustered at the city level and are robust to heteroskedasticity and arbitrary forms of error correlation within each city.

Table 4: Outcome Measures

Measure	Definition
Total	Total number of rental units in a city
0-30% AMI	Number of rental units affordable to households that make between 0 and 30% of Area Median Income, or extremely low-income households
30-50% AMI	Number of rental units affordable to households that make above 30% through 50% of Area Median Income, or very low-income households
50-80% AMI	Number of rental units affordable to households that make above 50% through 80% of Area Median Income, or low-income households
80-100% AMI	Number of rental units affordable to households that make above 80% through 100% of Area Median Income, or moderate-income households
100-120% AMI	Number of rental units affordable to households that make above 100% through 120% of Area Median Income, or upper moderate-income households
>120% AMI	Number of rental units affordable to households that make greater than 120% of Area Median Income, or high-income households

We include an implementation period, $Implementation$, to control for the 2 years before a reform, the year of the reform, and the 2 years after a reform. This helps control for anticipation effects that could cause an Ashenfelter dip or spike. That is, it mitigates effects from discussion about a reform that has not yet been approved that could influence developer or landlord behavior. It also allows time for reforms to take effect as knowledge about reforms requires time to spread. Finally, it helps account for the five-year averages in the ACS data to ensure that outcome years do not include pre-periods due to averaging.

Our key explanatory variable, $PostImplementation$, is a stock variable of the number of reforms in year *t*-3. More restrictive reforms add 1 to this variable and less restrictive reforms subtract 1. That is, this variable would go up by 1 if there was a reform three years prior that was more restrictive, and would go down by 1 if there was a less restrictive reform three years prior. As represented, estimates for β are the average effect of a more restrictive rent stabilization reform on housing supply and affordability.

The adoption of rent control policies is staggered over time, which can generate bias in traditional two-way fixed effects estimates since the specification requires the relatively strong assumptions about homogeneity of treatment effects (Goodman-Bacon 2021). In particular, Goodman-Bacon (2021) shows that two-way fixed effects estimates for policies with staggered adoption, as in the varied implementation dates of rent control reforms, use comparisons between early treatment groups with untreated and later treatment groups, and later treatment groups with untreated and early treatment groups. Each group will at some point act as a control group and the coefficient generated from traditional difference-in-differences

will be a variance-weighted average of many different treatment effects. A large body of methodological research has emerged accounting for staggered implementation effects that allow for careful comparison of treated units to not-yet-treated, or treated to never-treated, or both. In addition to the traditional two-way fixed effects approach, we use the approach proposed by Callaway and Sant'Anna (2021) to estimate a difference-in-differences estimator with multiple time periods and focus on comparing treated cities with not-yet-treated cities. Furthermore, we include an exogenous shift-share variable for local employment growth (the local employment growth rate predicted by interacting local industry employment shares with national industry employment growth rates) to control for changing economic activity and to aid in achieving parallel pre-trends (the key identifying assumption for difference-in-difference estimation). The shift-share variable follows Bartik (1991) and uses workplace area characteristic (WAC) data from LODES. In the shift-share variable creation, we use the 2-digit NAICS codes (20 industries) and a base year of 2002. The shift-share variable is the predicted employment level in each city based on its local exposure to national industry level employment changes. Finally, as a falsification test, we estimate both models with lead variables (i.e., dummy variables for our policies starting 3 years early) to probe the parallel pre-existing trends assumption.

5 Results

Table 5 presents our results using the model described by equation 1 (the two-way fixed effects model with place specific time trends). We find that rent control is associated with a reduction in total rental units, statistically significant at the 5% level, driven by a decrease in units affordable to people making greater than 120% of AMI, significant at the 5% level as well. However, these results are not universal as there is a large and statistically significant increase in the number of rental units affordable to those making 30% or less of the area median income (extremely low-income households), significant at the 1% level. Estimates for the implementation period are not statistically significant for the three groups affected by rent control, but is positive and statistically significant for units affordable to people making between 80 and 100% of AMI. This may reflect some anticipatory effects for developers and a shorter-term impact of the reform on rental prices, construction, and/or development deals.

While the overall relationship between rent control and total rental units is negative and statistically significant, the falsification test for this estimate fails. Table 6 shows there was a statistically significant increase in the total supply in cities three years prior to a reform. This suggests that the effect may be caused by selection bias and pre-existing effects rather than due to the reforms themselves; cities that were already growing in total rental units were more likely to institute a reform. While our fixed effects model with place specific time trends helps to account for some of this lack of parallel trends, it does not completely remove it. However, the pre-trend is positive and the estimate is negative, suggesting that the

estimate could be a lower bound on the true effect size. Further, we do not see similar pre-trends for the statistically significant estimates for our AMI thresholds that provide a dichotomous breakdown of the overall effect.

Table 5: The Effect of Rent Control Reforms on the Supply of Rental Housing using a Two-Way Fixed Effects Model

		Arcsin of units affordable at:					
	Total	**0-30% AMI**	**30-50% AMI**	**50-80% AMI**	**80-100% AMI**	**100-120% AMI**	**>120% AMI**
Rent Control	-0.099*	0.421**	-0.144	-0.031	-0.026	-0.076	-0.380*
	(0.039)	(0.158)	(0.156)	(0.127)	(0.158)	(0.191)	(0.176)
Implementation Period	-0.048	0.090	0.030	0.155	0.224*	0.134	-0.146
	(0.028)	(0.093)	(0.093)	(0.093)	(0.096)	(0.123)	(0.122)
N	53,500	53,500	53,500	53,500	53,500	53,500	53,500
adj. R-sq	0.980	0.847	0.872	0.921	0.878	0.830	0.819

Notes: Results are from a fixed effects model with random trends that includes place and year fixed effects, as well as city specific time trends. These estimates shown are for more restrictive reforms, with less restrictive reforms included as a control. Robust standard errors are in parentheses, clustered at the place level. Sample sizes are rounded to the nearest one hundredth to comply with Census disclosure requirements. ** $p \leq 0.01$; * $p \leq 0.05$. Data are sourced from the Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503).

Table 6: Falsification Test for the Two-Way Fixed Effects Model

		Arcsin of units affordable at:					
	Total	**0-30% AMI**	**30-50% AMI**	**50-80% AMI**	**80-100% AMI**	**100-120% AMI**	**>120% AMI**
Three Years Before Rent Control Reform	0.074*	0.050	0.197	0.164	0.265*	0.151	0.162
	(0.036)	(0.110)	(0.144)	(0.136)	(0.134)	(0.184)	(0.175)
N	53,500	53,500	53,500	53,500	53,500	53,500	53,500
adj. R-sq	0.980	0.847	0.872	0.921	0.878	0.830	0.819

Notes: Results are from a fixed effects model with random trends that includes place and year fixed effects, as well as city specific time trends. The model includes all of the variables from the main model, but for which results are suppressed since they cannot be causally interpreted. Robust standard errors are in parentheses, clustered at the place level. Sample sizes are rounded to the nearest one hundredth to comply with Census disclosure requirements. ** $p \leq 0.01$; * $p \leq 0.05$. Data are sourced from the Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503).

Using the Callaway and Sant'Anna (2021) estimator, we continue to find that rent control reforms that are more restrictive reduce the total number of rental units and the number of units affordable to households

making more than 120% of AMI (Table 7), with no indication of a significant pre-trend (Table 8). There is no effect in this model of rent control on units affordable to extremely low-income households (i.e., those making less than 30% of AMI).

Table 7: The Effect of Rent Control Reforms on the Supply of Rental Housing using Callaway and Sant'Anna (2021)

	Arcsin of units affordable at:						
	Total	**0-30% AMI**	**30-50% AMI**	**50-80% AMI**	**80-100% AMI**	**100-120% AMI**	**>120% AMI**
Rent Control	-0.116**	-0.162	0.034	-0.213	-0.027	0.166	-0.268*
	(0.043)	(0.186)	(0.146)	(0.112)	(0.122)	(0.312)	(0.119)
N	47,000	47,000	47,000	47,000	47,000	47,000	47,000

Notes: Results are from a staggered treatment difference-in-differences model proposed by Callaway and Sant'Anna (2021). The model also includes Bartik instruments as control variables to account for exogenous changes in economic activity at the place level. Robust standard errors are in parentheses, clustered at the place level. Sample sizes are rounded to the nearest one hundredth to comply with Census disclosure requirements. ** $p \leq 0.01$; * $p \leq 0.05$. Data are sourced from the Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503).

Table 8: Falsification Test for Callaway and Sant'Anna Model

	Arcsin of units affordable at:						
	Total	**0-30% AMI**	**30-50% AMI**	**50-80% AMI**	**80-100% AMI**	**100-120% AMI**	**>120% AMI**
Three Years Before Rent Control Reform	0.003	0.074	0.105	-0.020	0.059	-0.010	0.006
	(0.021)	(0.061)	(0.072)	(0.072)	(0.062)	(0.087)	(0.101)
N	47,000	47,000	47,000	47,000	47,000	47,000	47,000

Notes: Results are from a staggered treatment difference-in-differences model proposed by Callaway and Sant'Anna (2021). The model also includes Bartik instruments as control variables to account for exogenous changes in economic activity at the place level. It also includes all of the variables in the main model, but for which results are suppressed since they cannot be causally interpreted. Robust standard errors are in parentheses, clustered at the place level. Sample sizes are rounded to the nearest one hundredth to comply with Census disclosure requirements. ** $p \leq 0.01$; * $p \leq 0.05$. Data are sourced from the Federal Statistical Research Data Center under FSRDC Project Number 2661 (CDRB-FY24-P2661-R11503).

Overall, we find that rent control reduces the total number of rental units in a city, driven by a reduction in units affordable to people with high incomes. There is also evidence that rent control is associated with a statistically significant increase in the number of units affordable to households making less than 30% of AMI, but this effect is only present in the two-way fixed effects model. Because we have a large control

group, the bias inherent in two-way fixed effects models with staggered treatment is minimized, giving us trust in those results. Furthermore, we lose some variation inherent to places that had more than one reform using the Callaway and Sant'Anna (2021) estimator because it only allows for a single treatment in a city and it does not allow us to control for place specific time trends. Finally, the Callaway and Sant'Anna estimates do not allow for us to control for an implementation period. For these reasons, the two-way fixed effect model is our preferred specification.

6 Conclusion

This study highlights the tradeoffs inherent to rent control. Increasing the restrictiveness of rent control appears to increase the number of units affordable to people with extremely low incomes (those making less than 30% of AMI); however, increases to rent control restrictiveness also reduce the total number of rental units and the number of rental units affordable to people with higher incomes. With our already constrained supply and construction rates not keeping up with demand, reducing this overall supply might harm households with low incomes in the long-run. Supply side policies such as upzoning and increasing the speed of development might be needed in tandem with rent control to ensure that rent control does not further reduce the supply of rental units and potentially affordability in the long run.

Two caveats concerning our results should be highlighted. First, while our findings show an increase in the supply of units affordable to people with extremely-low incomes, they do not indicate by whom those units are being occupied; that is, who is benefitting from these low-cost units. This is an oft-cited issue with rent control. By design, rent control protects incumbency and provides benefits to those living in rent-controlled units, and it is not targeted to households with the most need since it is not means tested. There is a strand of literature that supports these ideas. Gyourko and Linneman (1989) find that many people with low incomes benefit from rent control, but that the same is true for people with middle and upper incomes. They conclude that rent control benefits low, middle, and high-income tenants equally. Similarly, Linneman (1987) and Early (2000) found that rent control and rent stabilization benefitted some low-income households, but did a poor job targeting those most in need. Finally, Ault and Saba (1990) found that the rent control subsidy in NYC was greater for higher-income households than for lower-incomes or minority households and Sims (2007) finds renters in the bottom quartile of household income distribution occupied 26 percent of rent-controlled units while tenants in the top half occupied 30 percent. Over time, even if residents in rent-controlled units benefit, the policy may not be distributing benefits equitably if people with middle or high incomes are disproportionately housed in controlled units and if, like the literature suggests, people not lucky enough to obtain a rent-controlled unit face constrained supply and increasing rents.

Second, while our findings indicate that rent control leads to a greater supply of rental units affordable to extremely low-income individuals, we do not know the quality of the housing stock nor the mechanisms

through which this supply increases. It could be that the increase in lower cost units reflects a reduction in housing quality, loosely matching previous research that finds evidence that rent control policies discourage landlords from maintaining the quality of their units (Arnott and Shevyakhova 2014; Gyourko and Linneman 1989). Or, the housing stock and prices could remain relatively stable with higher income residents moving into the area, changing the definition of 0-30% AMI over time (Diamond, McQuade, and Qian 2019).

Considering our findings and these caveats, more research is needed both on rent control and on supply side impacts to determine what combinations of policies can best increase affordability for households making low or moderate incomes in both the short and long run. Research is needed to determine who benefits from various types of rent control regulations in different cities and which components of rent control regulations harm or benefit various community members, such as vacancy decontrol, different types of allowable rent increases, and exemptions. Research is also needed to determine who benefits from and who is harmed by supply effects. Given these considerations and our findings, it is up to local leaders and community members to decide whether rent control is the correct policy to address their community's goals, how to design it, and which policies should be adopted in tandem with rent control to ensure both short- and long-term benefits.

Funding: This research received funding from the Robert Wood Johnson Foundation's Policies for Action Program. Any errors are our own.

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