THE REDISTRIBUTION OF HOUSING WEALTH CAUSED BY RENT CONTROL

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Abstract

This paper studies the effects of rent control on the housing wealth of renters, landlords, and homeowners. Following the passage of rent control in St. Paul, Minnesota in 2021, average property values fell by 4.4% to 5.8%. Leveraging administrative parcel-level data, we show that in the aggregate, renters gained and landlords lost, though upper-income renters gained more than lower-income renters, while small landlords lost the same as large landlords. Owner-occupants' wealth also fell significantly from direct capitalization effects and negative externalities. These results provide the first evidence on the heterogeneous wealth effects of a wave of new rent control laws. (*JEL* D61, D62, G51, H23, R23, R31, R38)

February 2024

Keywords: Rent control, real estate valuation, wealth transfers, externalities

We thank Tom Chang, Katie Galioto, Andra Ghent, Richard Green, Arpit Gupta, Adam Guren, Isaac Hacamo, Erica Xuewei Jiang, Matthijs Korevaar, Lu Liu, Song Ma, Tim McQuade, Stijn Van Nieuwerburgh, Emily Nix, Chris Palmer, Chris Parsons, Kate Pennington, Jacob Sagi, Selale Tuzel, and participants at USC, Yale University, 2022 Innovations in Housing Affordability Summit, 2022 Real Estate Research Symposium at UNC, 2022 Northern Finance Association Annual Meeting, 2022 ESCP-TAU-UCLA Conference on Housing Affordability, 2023 American Real Estate and Urban Economics Association Meetings, 2023 AEA Annual Meeting, and the 2023 WFA Annual Meeting. Kenneth Ahern discloses that he has ownership interests in real estate in California covered by rent control. He has no ownership interest in real estate located in Minnesota. Marco Giacoletti discloses that he is a tenant. He has nothing else to disclose.

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Rental housing is one of the most important markets in the economy. In 2019, out of 123 million housing units in the United States, 44 million units, or 36%, were occupied by renters (U.S. Census Bureau, 2019). The median household spent 35% of income on rent, while 22% of households spent more than 50% of income on rent. Moreover, rents are increasing at a record pace. In March 2022, the CoreLogic single-family rent index grew by 13.6% year-over-year, the fastest increase in almost two decades.

In response to surging rents, Table 1 shows that a wave of new rent control laws have been enacted in cities across the country, including areas with no history of rent control, such as Maine and Minnesota. For the first time in 70 years, rent control has been enacted at the state level in Oregon and California, and state legislatures are debating similar laws in New York, Illinois, and Massachusetts. In addition, new rent control laws are becoming stricter with rental increases limited to rates below the rate of inflation. In sum, rent control is emerging as a significant and increasingly prevalent policy to regulate the costs of housing.

While it is evident that in broad terms, rent control impacts renters and landlords, it is not obvious how the effects of rent control are distributed at a more granular level. For example, though rent control is typically focused on benefiting low-income renters, it is possible that high-income renters receive the majority of the benefits. Similarly, it is not obvious whether larger, wealthier landlords bear more of the burden of rent control than do smaller, less wealthy landlords. Moreover, externalities from rent control could impact owner-occupants, who do not directly participate in the rental market. Given that housing costs are the largest expense of the average household and that housing wealth has an outsized influence on consumption inequality and wealth accumulation (Adelino, Schoar, and Severino, 2015; Favilukis, Ludvingson, and Van Nieuwerburgh, 2017), it is imperative to provide well-identified empirical evidence on the economic consequences of these new rent control laws for a diverse set of constituents.

Providing credible estimates of the causal effect of rent control is challenging. First, landlords endogenously respond to rent control by evading the law, neglecting maintenance, or removing properties from the rental market (Diamond, McQuade, and Qian, 2019a,b). Second, the effects of rent control occur gradually over many years and rent control laws evolve slowly over time. Third, data on rents and housing quality are scarce and imprecise.

To address these challenges, we study the effects on property values in an event study of the enactment of rent control in St. Paul, Minnesota in November 2021 through a ballot measure. Though there exists evidence on the effect of rent control on rent levels (Jenkins, 2009), the evidence of the effect of rent control on property values is extremely limited. However, studying property values, rather than rents, is advantageous because property

Table 1 – Recent Rent Control Laws

Government	Year	Source	Outcome	Description
State				
California	2018	Ballot measure	Rejected	Allow local government to enact rent control
Oregon	2019	Legislature	Passed	Rent control (7% + CPI)
Florida	2019	Legislature	Pending	Repeal statewide ban on rent control
California	2020	Ballot measure	Rejected	Allow local government to enact rent control
California	2020	Legislature	Passed	Rent control (5% + CPI, maximum 10%)
Colorado	2021	Legislature	Passed	Allow local government to enact rent control
New York	2021	Legislature	Pending	Rent control (higher of 3% or 1.5×CPI)
Illinois	2021	Legislature	Pending	Allow local government to enact rent control
Massachusetts	2021	Legislature	Pending	Repeal statewide ban on rent control
Local				
Santa Rosa, CA	2017	Ballot measure	Rejected	Rent control (3%)
Santa Cruz, CA	2018	Ballot measure	Rejected	Rent control (CPI)
Anaheim, CA	2019	City council	Rejected	Allow local gov. to enact temporary rent control
Oakland, CA	2019	City council	Passed	Extend existing rent control to more properties
Inglewood, CA	2019	City council	Passed	Rent control (higher of 3%-5% or CPI)
Sacramento, CA	2019	City council	Passed	Rent control(5% + CPI, maximum 10%)
Portland, ME	2020	Ballot measure	Passed	Rent control (CPI, 5% maximum for new tenants)
Montclair, NJ	2020	City council	Passed	Rent control (2.5% for seniors & 4.25% others)
Philadelphia, PA	2020	City council	Pending	Allow local government to enact rent control
Los Angeles Co., CA	2020	City council	Passed	Rent control (CPI, 8% maximum)
Culver City, CA	2020	City council	Passed	Rent control (CPI, 5% maximum)
Jersey City, NJ	2020	City council	Passed	Extend existing rent control to more properties
Sacramento, CA	2020	Ballot measure	Rejected	Rent control (CPI, 5% maximum)
Berkeley, CA	2020	City council	Passed	Extend existing rent control to more properties
Asbury Park, NJ	2021	City council	Passed	Rent control (higher of 3.5% or CPI)
Tampa Bay, FL	2021	City council	Rejected	Rent control ballot initiative
St. Petersburg, FL	2021	City council	Rejected	Rent control ballot initiative
Santa Ana, CA	2021	City council	Passed	Rent control (lower of 3% or 80% of CPI)
Minneapolis, MN	2021	Ballot measure	Passed	Allow local government to enact rent control
St. Paul, MN	2021	Ballot measure	Passed	Rent control (3%)
Bell Gardens, CA	2022	City council	Passed	Rent control (lower of 4% or 50% of CPI)
Antioch, CA	2022	City council	Passed	Rent control (lower of 3% or 60% of CPI)
Pomona, CA	2022	City council	Passed	Rent control (lower of 4% or 100% of CPI)
Kingston, NY	2022	City council	Passed	Rent control (limits determined by board)
Richmond, CA	2022	Ballot measure	Passed	Rent control (lower of 3% or 60% of CPI)
Orange County, FL	2022	Ballot measure	Passed	Rent control (CPI)
Portland, ME	2022	Ballot measure	Passed	Rent control (70% of CPI, 5% maximum for new tenants)
Santa Monica, CA	2022	Ballot measure	Passed	Rent control (lower of 3% or 75% of CPI)
Pasadena, CA	2022	Ballot measure	Passed	Rent control (75% of CPI)

values reflect changes in future rents while accounting for the endogenous responses of renters and owners. For renters, price changes capture the present value of their future expected rent savings. For landlords, price changes also capture negative externalities and deadweight losses. Furthermore, only property values can provide evidence on the spillover effect of rent control on owner-occupied properties. Finally, compared to the scarcity of data on private rental agreements, we can observe precise information on prices, geographic locations, and property attributes for the near universe of real estate transactions.

The St. Paul law is an ideal event study for a number of reasons. First, there was little anticipation that the ballot measure would pass, no other confounding laws were passed at the same time, and there was no history of rent control in any city or state close to St. Paul. Thus, compared to most regulatory changes that involve substantial debate and revision before passage, the St. Paul ballot measure provides a relatively clean experimental setting. Second, St. Paul is a large, diverse city that allows us to study the heterogeneous impact of rent control across different property types, locations, and constituents. Finally, the real estate located outside of St. Paul's city limits provides a similar control sample for comparison.

We first show that the introduction of rent control caused an economically and statistically significant decline of 4.4% to 5.8% in the value of real estate in St. Paul over the nine months after the law was enacted, compared to adjacent areas. These results are estimated in a difference-in-differences framework using a sample of nearly 170,000 real estate transactions, including single-family owner-occupied houses, duplexes, triplexes, and large apartment buildings, over the period January 2018 to July 2022. The tests control for year-month fixed effects, granular location fixed effects, and property-level attributes, including building age, size, number of units, and property type.

These results are robust to a range of potential confounding factors. In triple-differences models that control for preferences for city centers versus suburbs, we estimate that rent control caused an 8% decline in property values in St. Paul relative to five comparable Midwestern cities. Event study tests show a sharp decline in property values after the introduction of rent control with no pre-trend, supporting a parallel trends assumption. We also show our results are robust to unbalanced traits in control and treated groups using the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020) and to deviations from the parallel trends assumption using the method of Rambachan and Roth (2023). Finally, we verify that our results are unlikely to be caused by selection bias.

Next, we show that the decline in property values reflects a mix of direct capitalization effects and indirect externalities. Using administrative data and rental listings to identify rental properties, we find that rental properties experienced an additional 7–8% decline in value compared to similar owner-occupied properties. Apartment buildings with at least eight units experienced losses of more than 13% in value. These results are consistent with the law having large direct capitalization effects, reflected in the larger price decreases for rentals. Accounting for the historically small, but positive transition rate from owner-occupied to rental property, the effects on owner-occupied properties can be interpreted as a mix of direct capitalization effects and indirect effects, such as negative externalities on neighborhood quality and public finances.

Finally, in our main set of results, we show that the impacts of St. Paul's rent control law varied significantly by the income levels of renters, landlords, and owner-occupants. To derive these results, we first estimate wealth effects for all properties in St. Paul, then identify the income levels of renters, landlords, and owner-occupants at a highly granular level. In particular, we use the transaction-level data as inputs into a hedonic pricing model based on location and property characteristics to estimate individual value losses for over 72,000 residential parcels in St. Paul, including 1,958 apartment buildings with four or more units, 6,093 small, multi-unit properties, and more than 64,000 single-family residences, of which 10% are rental properties. To measure the incomes of renters and owners, we use highly granular Census data at the block group level, which represent an area of 0.01 square miles with 414 households, on average. Using administrative data on property owners' addresses, we classify rental property owners as small landlords if their listed address is residential and different than the property address, and as large landlords if their listed address is commercial. We proxy for the incomes of small landlords using the average income of homeowners that live in the block group of their home address.

Using our estimates of household income, we first study how the costs and benefits of rent control are distributed in the aggregate. First, as a group, renters gain from rent control, while owners lose, as expected. Second, the largest fraction of the total losses were borne by owner-occupants even though they do not directly participate in the rental market. Third, across income brackets, the aggregate effects of rent control were distributed progressively, except for the lowest quintile of income, for which the effects were distributed regressively.

Next, we study the within-group effects of rent control to determine whether higher income renters receive more benefits than lower income renters and whether higher income landlords bear larger costs than lower income landlords. While the legal implications of the law are delineated by broad definitions of renters and owners, within each of these groups, incomes vary considerably. Thus, within-group effects are a critical component of the progressivity of rent control. Studying within-group distributions of public policy follows a long tradition in economics, including studies of affirmative action (Bertrand, Hanna, and Mullainathan, 2010), minimum wage (MaCurdy, 2015), and the burden of taxes (Piketty and Saez, 2007).

Within renters, the price effects are consistent with a regressive distribution of benefits. In particular, the price effects increase monotonically with renters' income level, from 2% of property value for renters with incomes less than \$22,500 up to 8% for renters with incomes above \$90,000. The corresponding dollar values are \$2,667 for low-income renters and \$15,563 for high-income renters. Thus, high-income renters received larger absolute benefits than low-income renters. In contrast, the magnitude of the wealth effect varies little with landlords' incomes, consistent with a flat distribution of costs. Small landlords with incomes less than \$90,000 lost 4.1% of value compared to a loss of 4.6% for large landlords of multi-unit properties. Normalizing by income, we find that landlords with lower incomes lost more wealth per dollar of income than landlords with higher incomes, consistent with a regressive tax burden. Finally, in addition to income, we also find evidence of a regressive tax burden with respect to wealth, where landlords' wealth is measured by the number and aggregate value of parcels they own in the five counties surrounding St. Paul.

In the last section of the paper, we explore several alternative explanations for the cross-sectional differences in wealth effects across income levels. First, we construct a simple pricing model calibrated to the data with a stochastic discount factor and stochastic growth rates. The model allows for probabilistic transitions between owner-occupied and rental housing, consistent with empirical observation, to account for missed future rent growth, both for properties that are currently rented and for owner-occupied properties that may transition into the rental market in the future. As validation, we show that the model predicts sizable value losses for owner-occupied properties, matches the difference in the average price effects of owner-occupied and rental properties, and also matches the cross-sectional patterns in value losses across Census block groups. Using the model's estimates, we infer that roughly two-thirds of the price effect for owner-occupied properties is driven by indirect effects and externalities. Second, we infer that properties in higher-income areas experienced larger losses because they have higher expected rent growth, and thus are more strongly affected by the uniform rent cap imposed by the law.

Though our results suggest that St. Paul's rent control law generates regressive benefits in which low-income renters receive smaller benefits than high-income renters, this finding relies on the assumption that the present values of landlords' losses and renters' gains are equal. However, if tenants and landlords have different discount factors, price changes may not reflect wealth effects across income groups. When we consider this possibility, we show that to generate a progressive redistribution in which low-income tenants receive larger benefits than high-income tenants, we must assume that low-income tenants have implausibly low discount factors. We also show that potentially non-priced tenant benefits, such as insurance against forced evictions, require implausible assumptions to make benefits non-regressive. Thus, while it is true that low-income tenants receive positive wealth effects from the law and that the losses caused by the law are imposed on landlords who have higher incomes than tenants on average, we believe our results show that the distribution of the benefits of the law is regressive within renters and not progressive within landlords.

The primary contribution of this paper is to provide some of the first evidence on the diverse effects of rent control on housing wealth. While there exists extensive evidence on rent control's influence on rent levels, housing supply, search costs, property maintenance, and tenant mobility (Jenkins, 2009), there is almost no evidence on property values. To our knowledge, the only other paper that studies the effect of rent control on market values of housing is Autor, Palmer, and Pathak (2014). They find that property values increased substantially following the end of rent control in Cambridge, Massachusetts in 1994, including spillovers to properties not covered by the law.[1] Given the importance of housing wealth and the changing nature of rent control laws, there is a critical need for additional research on property values beyond the single study of Cambridge, Massachusetts.

This paper advances the literature on rent control and property values in at least three ways. First, compared to Autor et al., who study the abolishment of an older law, we study the initiation of a new, stricter rent control law in 2021 in a larger, more diverse city, in a region with no history of rent control. In particular, St. Paul's rent control law reflects a new generation of increasingly strict laws with fixed rental caps set explicitly below the rate of inflation, as shown in Table 1. Second, our larger setting allows us to include apartment buildings in our sample, which Autor et al. exclude. This is important because apartment buildings tend to house the lowest income renters but are owned by the highest income landlords. Third, in contrast to Autor et al., our paper provides new evidence on the redistribution of housing wealth caused by rent control across income groups for both renters and owners.

More generally, our results provide the first evidence on new rent control laws in the US since the mid-1990s. This is important because the vast majority of existing empirical evidence on rent control is concentrated on New York City's historical law (e.g., Glaeser and Luttmer, 2003), with a few papers studying rent control laws from the 1970s to the 1990s

[1] Mense et al. (2019) study rent control's effect on vacant land prices in Germany in 2015.

in other locations.[2] Gyourko and Linneman (1989) show that rent control in New York City in the 1960s was poorly targeted because low-income tenants did not receive more benefits than high-income tenants. Similarly, an older literature focused on New York City finds that the costs to landlords were substantially larger than the benefits to tenants (Olsen, 1972; Ault and Saba, 1990). Our results confirm that St. Paul's rent control generates bigger benefits to higher income renters, as in prior settings, but also provide new evidence that the burden of rent control falls equally on professional landlords of large apartment buildings as on "mom and pop" owners of small properties. More recently, Favilukis, Mabille, and Van Nieuwerburgh (2023) provide a general equilibrium model of housing affordability, including rent control, calibrated to New York City. In contrast, our goal is to provide well-identified, empirical estimates of the causal impact of rent control on housing wealth. Finally, our results on wealth effects complement research on the effects of rent control on tenant mobility and misallocation (Glaeser and Luttmer, 2003; Diamond, McQuade, and Qian, 2019a).

I. Background: Saint Paul and the Rent Control Ballot Measure

A. Historical Context of Rent Control

The so-called first generation of rent control laws were enacted by the federal government during World War II as a temporary method to stabilize rental markets during a period of mass relocation (Pastor, Carter, and Abood, 2018). In the 1970s, the second generation of rent control laws were enacted in select states in response to growing inflation. These laws were less restrictive than the first generation of rent control laws. In particular, rents could be set to market rates upon vacancies, rent increases were tied to the rate of inflation, and exemptions were granted for new construction and small landlords. Following the second wave, a regulatory backlash led many states to pass laws that banned or limited rent control at the local level.

Recently, as housing costs increase, many states are revisiting their laws that preempt rent control or have enacted state-level rent control (see Table 1). Cities have also been exploring options for enacting rent control, including Minneapolis and St. Paul, Minnesota. Though the Minnesota state legislature preempted rent control at the local level in 1984, the state statute had a provision that allowed local governments to enact rent control if approved in a general election. On November 2, 2021, Minneapolis and St. Paul residents voted on two separate rent control measures. St. Paul's ballot measure was a vote for a specific rent control

[2]These include Cambridge (Sims, 2007; Autor, Palmer, and Pathak, 2014), Vancouver (Marks, 1984), Toronto (Fallis and Smith, 1985), Los Angeles (Murray, Rydell, Barnett, Hillestad, and Neels, 1991), and San Francisco (Diamond, McQuade, and Qian, 2019a).

law that capped rental increases at 3% per year, with few exemptions. The law passed with a 53% to 47% split. Minneapolis's ballot measure was an amendment to the city charter allowing for the possibility of introducing a new, unspecified, rent control law in the future. This provision was also approved with a 53% to 47% split.[3]

In contrast to St. Paul's stringent rent control, Minneapolis's ballot measure did not create any new laws. Because no law was actually enacted, we cannot know what market participants anticipate about future provisions. Though Minneapolis and St. Paul tend to enact similar laws (e.g., minimum wages, COVID masking policies, and paid employee leave), the mayor of Minneapolis, who was re-elected in November, has been a vocal opponent of rent control. In addition, as discussed below, Minneapolis had confounding measures on the ballot when it passed its limited rent control law. For these reasons, this paper focuses on St. Paul's rent control law.

B. St. Paul's Rent Control Ordinance

At the time of its passage in November 2021, St. Paul's rent control ordinance was unique in its stringency. First, unlike most rent control laws, St. Paul's law did not include vacancy decontrol provisions. This meant that rent increases in St. Paul were originally limited to 3%, independent of inflation and regardless of whether a property became vacant and was re-rented to new tenants. Thus, there was no mechanism for rents to be adjusted to market prices and rent growth could be capped below inflation rates for an indefinite number of years. Second, unlike most rent control laws that exempt new construction to encourage increases in supply, there was no exemption for new construction in St. Paul. All residential rental properties were under the jurisdiction of the law. Similarly, there were no exemptions for small landlords or for properties with few units and no provisions for owner-occupants, as are common in other rent control laws.

While St. Paul's law did not include vacancy decontrol, it also did not include 'just cause' eviction controls. Eviction controls give more rights to tenants by restricting landlords from evicting tenants except for a limited number of predefined reasons, such as non-payment of rent, violation of other terms of the lease, and in some cases, the personal use of the landlord. This means that under just cause eviction controls, tenants in good standing have the right to remain in a rental unit indefinitely. Taken together, St. Paul's absence of vacancy decontrol meant that landlords could not increase rents by evicting current tenants, but the absence of eviction controls meant that landlords could evict tenants for other reasons. This

[3]For comprehensive information on Election Day results, see https://electionresults.sos.state.mn.us/20211102

institutional detail will be important when we consider whether the price effects of rent control include a benefit to renters by preventing forced evictions.

To much controversy, on April 29, 2022, the city government issued a set of implementation procedures that substantially weakened the terms of the law as passed by the voters in November 2021. In particular, the new rules would allow landlords to increase rent in order to maintain an inflation-adjusted constant net operating income based on the property's operating income in 2019. Any rent increase below 8% per year could be self-certified by the landlord, with the possibility of an audit. Increases between 8% and 15% would need to be approved by the city. The maximum allowable rent increase in one year would be 15%, but increases in excess of 15% could be deferred to future years.

After the end of our sample period in July 2022, the City Council made further amendments in September that took effect in January 2023. Most notably, the amended law exempts new construction for 20 years retroactively and allows for partial vacancy decontrol following a just cause vacancy.[4] Beginning with the implementation of the law starting in May 2022, these amendments have made the law more comparable to existing laws in other jurisdictions. It is possible that real estate participants anticipated the weakening of the law before May 2022. On the other hand, it is possible that real estate prices respond slowly to new information (Cellini, Ferreira, and Rothstein, 2010). To the degree that market prices impound future expectations, if investors anticipated the weakening of the law, then we can consider our estimates as a lower bound for the effects of the original terms of the law and as accurate estimates for the effects of more typical rent control laws.

Though St. Paul's initial rent control law was especially severe, it is a good representation of the national trend towards stricter rent control laws. Older laws typically allow rent to increase at the rate of the local consumer price index (CPI). However, more recent laws limit rent increases to a fraction of CPI or a fixed percentage, as in St. Paul. For example, Los Angeles's 1978 law limits increases to 100% of CPI. In contrast, Santa Ana's 2021 law limits rent increase to 80% of CPI or 3%, whichever is lower and Pasadena's 2022 law limits rent increase to 75% of CPI. Similarly, there is a trend towards providing fewer exemptions from rent control. For example, in Los Angeles, single-family residences are exempt from rent caps, as are properties built after 1995. In contrast, Portland, Maine's 2022 law has no age exemption and only exempts small multi-unit buildings if they are owner-occupied. Given

[4]Prior to rent control, the St. Paul city council passed just cause eviction protections in early 2021 but rescinded the law shortly after it was adopted following a lawsuit and a court ruling. In November 2021, when rent control was passed, there was no just cause eviction protections and the new law did not include just cause eviction protections either. When the city council revised the law to be enacted in January 2023, they re-instituted just cause eviction protections. Thus, during our sample period, there was no just cause protection in place.

this trend toward stricter rent control laws, evidence from St. Paul can help shed light on the effect of the next generation of rent control laws.

II. Conceptual Framework of Rent Control and Property Values

Economic theory predicts that rent control affects property prices through both direct and indirect effects, which we can be characterized as follows:

$$\begin{aligned} \textit{Value Loss}_t = & \sum_{\tau} E_t \left\{ \frac{I^{Rental}_{t+\tau} \times \left[\textit{Rent Saving}_{t+\tau} + DWL_{t+\tau}\right]}{(1+r_{t+\tau})^{t+\tau}} \right\} && \text{(Direct Effect)} \\ & + \textit{Expected Negative Externality}_t, && \text{(Indirect Effect)} \quad (1) \end{aligned}$$

where $\textit{Value Loss}_t$ is the property value loss realized after the passage of the law at time t, I^{Rental}_t is an indicator for rental properties at time t, and r_t is the discount rate. The direct effect of rent control on existing property values includes two different components. The first component of the direct effect is a transfer of housing wealth from owners to renters, which is equal to the expected rent savings caused by rents that are constrained to be lower than free-market rents. The second component of the direct effect is a deadweight loss caused by a reduction in the level of housing quality, relative to the free-market level. In particular, landlords have an incentive to reduce maintenance expenses and let their properties deteriorate if rents are kept artificially low by rent control. Gyourko and Linneman (1990) show that rent control led owners to reduce maintenance expenditures, though Olsen (1988) argues that tenants of rent controlled units are likely to endogenously increase maintenance in response. Both components of the direct effect represent a housing wealth loss to owners. However, the transfer component represents a housing wealth gain to renters.

The direct effect only occurs if a property is rented ($I^{Rental} = 1$). If the property is owner-occupied, the owner enjoys the full value of the property, even under rent control, and there is no loss. Therefore, the expected direct effect of rent control on the present value of the property is moderated by the probability that the property is rented now or in the future. As we show below, there is a positive transition probability from owner-occupied to rental housing which means that in expectation the direct capitalization effect also impacts properties that are currently owner-occupied.

Owners may endogenously exit from the rental market in response to rent control by selling rental properties to owner-occupants, which reduces the exposure to rent control. Moreover, landlords in St. Paul have an incentive to increase current rents immediately before the passage of the law. These increases are difficult to observe if rental contracts are privately renegotiated outside of new listings. By studying forward-looking transaction prices, our results capture these effects.

In contrast to the direct effect, the indirect effect of rent control on existing property values is caused by negative externalities in the city. Numerous studies report that lower valued properties cause negative spillover effects on other properties (Rossi-Hansberg, Sarte, and Owens III, 2010; Autor, Palmer, and Pathak, 2014). These effects could be driven by changes in such attributes as crime or school quality (Autor, Palmer, and Pathak, 2019; Cellini, Ferreira, and Rothstein, 2010). Because these externalities make the property less desirable, both for renters and owner-occupants, they represent value loss without any transfers.[5]

III. Identification Strategy

The first step of our analysis is to identify the causal relationship between rent control and property values. Though the passage of rent control in St. Paul presents a setting that has similarities to an ideal experiment, there are important deviations.

A. Cross-Sectional Variation

First, rent control is not randomly assigned to a sample of properties. Instead, all properties within the city of St. Paul are subject to rent control. Therefore, we use properties located in the five counties surrounding St. Paul as a control sample. The advantage of this approach is that we do not need to be concerned that an omitted variable, like building age, could determine both the assignment to the treatment group and also a change in market value. Likewise, because there are no exemptions, owners cannot easily remove their properties from rent control, which would bias our treatment sample. Moreover, because the city boundaries of St. Paul are not driven by geographic boundaries that could influence property values, areas adjacent to St. Paul represent contiguous and integrated real estate markets.

The disadvantage of our setting is that we have to be concerned that the treated properties within St. Paul may not be comparable to the control properties outside of St. Paul. To address this concern, we use three different specifications of location fixed effects to capture time-invariant cross-sectional differences between treated and control groups: city, ZIP code, and Census block group. These fixed effects capture the large majority of potential cross-sectional time-invariant confounding differences in property values across city boundaries, such as school districts, tax rates, and urban density. Because the geographic boundaries

[5]Rent control also creates deadweight losses by reducing the incentive to supply new housing. Though we focus on value changes of existing properties, we also provide additional evidence on changes in the supply of new housing.

are narrowly defined, the fixed effects also absorb more nuanced variation that may affect property values, such as commuting time, neighborhood feel, and architectural styles. We also control for individual property traits, including square footage, number of units, and building age, to absorb other sources of price variation unrelated to rent control.

To further alleviate concerns that treated and control properties are not comparable, we provide robustness tests that limit the properties in the control sample to those that are geographically close to the border of St. Paul. Control properties located near the border of St. Paul are likely to share many of the same qualities as the treated properties located inside St. Paul, such as commuting times, quality of construction, and local amenities, though they are not directly affected by rent control. Second, we test whether rental properties are more impacted by rent control than owner-occupied properties, as would be expected if rent control is causing changes in values. This allows us to compare the changes in property values of two properties within the same small geographic region within St. Paul, similar to prior research on rent control in Cambridge and San Francisco (Autor, Palmer, and Pathak, 2014; Diamond, McQuade, and Qian, 2019a).

A final threat to our identification is that real estate prices in St. Paul may reflect preferences for urban versus suburban locations. If there was a coincidental increase in demand for suburban real estate at the time of the rent control vote, similar to the surge during the Covid pandemic (Gupta, Mittal, Peeters, and Van Nieuwerburgh, 2022; Ramani and Bloom, 2022), we could falsely attribute lower property values in St. Paul to rent control, when in fact it represents an unrelated shift in demand. To address this concern, we control for the location of real estate in city centers versus suburban areas using data from five metro areas comparable to the Twin Cities: St. Louis, Kansas City, Indianapolis, Nashville, and Denver. These cities are geographically proximate to the St. Paul area and are roughly equivalent in population size, housing costs, and in their growth rates of population, immigrants, and income. Internet Appendix Table 2 presents complete statistics.

B. Time-Series Variation

While fixed effects and property traits account for cross-sectional confounding variables, we also need to control for confounding time-series variation in market prices unrelated to rent control. This includes general time trends, anticipation of the law, and deviations from an assumption of parallel trends between treated and control groups.

To control for macroeconomic variation in the time-series, we include year-month fixed effects for each month from January 2018 to July 2022. These fixed effects absorb both seasonal variation and yearly variation for the average property in the sample. Thus, estimated changes in prices following the passage of rent control will reflect abnormal changes relative to seasonal norms and average yearly changes.

Next, we test for anticipation of the passage of the law. First, St. Paul did not have excessive rent before the passage of rent control. According to Census Bureau estimates, the median gross rent as a percentage of household income in the Minneapolis-St. Paul metro area was 28.4% in 2019, which places it at the 47th percentile in a sample of over 900 metro and micro Census areas. In addition, Internet Appendix Figure 6 shows that the median inflation-adjusted rent for a two-bedroom unit in St. Paul has remained roughly the same from January 2019 to November 2021, when rent control was approved. Second, in Internet Appendix Figure 7, we show that media coverage of rent control issues in the St. Paul area only increased significantly in October 2021. Third, there was no public polling of the law in advance of the vote which could have led to substantial anticipation.[6] Fourth, the ordinance passed with a relatively close vote of 53% to 47% with 58,546 total votes cast, out of about 210,000 voting-age citizens. Finally, it is important to note that to the extent that the law was anticipated, it will bias our tests towards finding no results.

We also need to consider any one-time confounding events in St. Paul or the control cities. First, we note that the rent control law was the only initiative on the November 2 ballot in St. Paul, so its passage was not accompanied by the passage of any related laws. The only other elections in St. Paul in November 2021 were a landslide win for the incumbent mayor and contests for four school board seats. Likewise, in the control cities, there were no ballot measures and only routine school board elections. As noted above, Minneapolis would be a natural control for St. Paul. However, in addition to the ballot measure on rent control, Minneapolis's ballot also included referenda on mayoral power and policing. These confounding events mean that if property values in St. Paul changed relative to Minneapolis, we could not attribute the change to rent control. Therefore, for all of our tests, our control sample excludes real estate in Minneapolis.

Finally, we provide evidence that the transaction prices in St. Paul would have followed a parallel trend with the controlled properties if rent control had not been passed. In particular, we run an event study to identify any differential pre-trends in St. Paul compared to the control cities. Second, we estimate the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020) to reduce biases caused by using time-varying covariates under the assumption of parallel trends. Finally, we test whether our results are robust to deviations from the parallel trends assumption following Rambachan and Roth (2023).

[6]See the discussion in the public press: https://minnesotareformer.com/briefs/heres-the-rent-control-question-st-paul-will-vote-on-this-fall/ and https://myvillager.com/2021/10/13/st-paul-debates-merits-of-rent-control-measure-on-ballot/

C. Econometric Specifications

Based on the discussion in the previous sections, we estimate the following difference-in-difference equation using only data from the St. Paul area:

$$\ln(price)_{ikt} = \beta \cdot StPaul_i \times Post_t + \gamma X_i + \alpha_k + \tau_t + \varepsilon_{ikt}, \tag{2}$$

in which $StPaul_i$ is a dummy variable equal to one for properties located in St. Paul and zero for properties outside of St. Paul; $Post_t$ is a dummy variable equal to one for transactions that closed after the passage of the law; X_i is a vector of characteristics including the log of the building age, the log size of the building in square feet, the log number of units, and dummies for different property types (apartments, townhouses, single family residences); and α_k and τ_t are families of geographic and year-month fixed effects. The coefficient β reflects a percentage change in property prices within St. Paul, relative to the change in property values in the control cities. Throughout the paper, standard errors are double-clustered by year-month and by the geographic level of the fixed effects.[7]

To control for changes in preferences for downtown versus suburban areas, we also estimate a triple-differences model as shown in the following equation:

$$\begin{aligned}\ln(price)_{ikmt} = {} & \beta \cdot TwinCities_m \times Downtown_i \times Post_t \\ & + \lambda \cdot TwinCities_m \times Post_t + \delta \cdot Downtown_i \times Post_t \\ & + \gamma X_i + \alpha_k + \tau_t + \varepsilon_{ikmt},\end{aligned} \tag{3}$$

where $TwinCities_m$ is a dummy variable equal to one for properties located in the Twin Cities metro area and zero for properties located in the other five metro areas; $Downtown_i$ is a dummy variable equal to one for properties located in the downtown area of its metro, and zero for properties located in suburban areas; and $Post_t$ is defined as before. For the Twin Cities area, downtown is defined as St. Paul. For the control cities, the city center (downtown) is the main city area as defined by Census. The triple interaction coefficient β reflects whether the difference-in-differences effect in Saint Paul versus the surrounding area is equal to the difference-in-differences effect in the downtown of the control cities.

[7]A potential concern is that clustering at more granular levels than the metro area incorrectly assumes that errors within subregions of a metro area (such as zip codes) are uncorrelated. Barrios, Diamond, Imbens, and Kolesøar (2012) show that if the covariate of interest is randomly assigned at the cluster level, ignoring correlations between clusters and differences in within-cluster correlations provides valid confidence intervals. Moreover, in Table 4 we cluster at the metro area level.

IV. The Effect of Rent Control on Real Estate Values in St. Paul

A. Data

We construct a comprehensive micro-dataset of real estate prices, covering both single-family houses and multi-unit properties in the five counties surrounding St. Paul and in the counties surrounding the five comparable metro areas. For sales of houses and small multi-unit properties, we download data from Redfin, which includes property types (single-family residence, townhouse, multifamily, etc.), characteristics (square footage and age), addresses, and precise geo-location. We exclude properties with missing or nonsensical geo-locations, with missing prices, with missing number of bathrooms or bedrooms, with number of bedrooms exceeding 10, and with number of bathrooms exceeding eight, or equal to zero.

Data on transactions of larger multi-unit properties are from Electronic Certificates of Real Estate Value (eCRV) collected by the Minnesota Department of Revenue. These certificates provide details on all real estate transactions in Minnesota including address, parcel number, property usage, square footage of the building, building age, number of rental units, sales price, and date. We only include transactions in which the current use and the intended use are both residential apartment buildings with four or more units. We also only include 'clean' transactions with complete eCRVs as defined by the Department of Revenue.[8] We omit duplicate copies of transactions that appear in both Redfin and the eCRV data.

Our final sample includes 169,119 transactions in the Twin Cities (including 16,943 in St. Paul with 2,564 in the post-period), and 805,271 total transactions in the comparable metro areas over the period from January 2018 to July 2022. Internet Appendix Tables 3 and 4 provide a complete breakdown of observations by city and county. To our knowledge, between the Redfin data and the eCRV data, our sample includes the near-universe of all residential properties sold in the Twin Cities area.[9]

Figure 1 provides a map of the transactions in the Twin Cities sample. Transactions in St. Paul are indicated by black dots. Transactions in the suburbs are indicated by blue dots. The empty space next to St. Paul is Minneapolis. This figure shows that the large

[8]Non-clean sales include sales between relatives, sales of partial interest, sales by government agencies, and other forms of non-market influences. A small number of the most recent transactions in the sample period are recorded only in preliminary eCRVs, which exclude details on non-market sales. In transactions of multiple parcels, we ensure the transaction price, number of units, and square footage are for the entire transaction. We also exclude assisted living facilities, mobile home parks, and mixed residential-retail properties.

[9]A repeated home sale approach, as in the Case Shiller index, would introduce selection bias away from large multi-unit buildings that transact infrequently and towards transactions less likely to be arms-length. For these reasons, the Case Shiller index excludes properties that sell more than once within six months.

majority of the control transactions are located close to St. Paul and the city boundaries appear arbitrary.

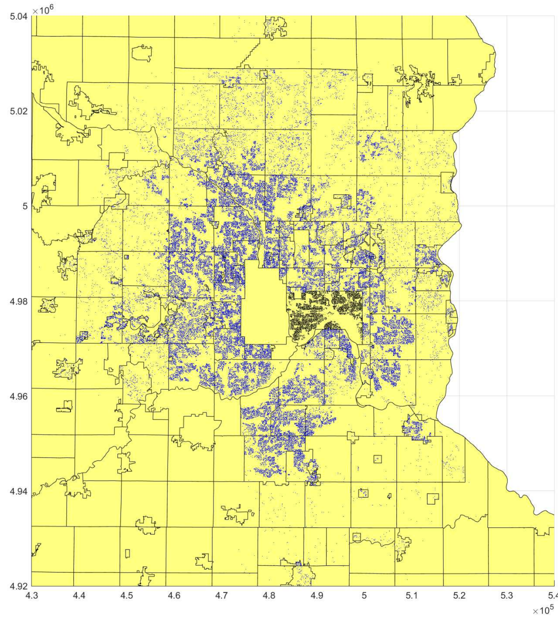


Figure 1. Location of Real Estate Transactions in St. Paul vs. Suburbs
Notes: The location of house sales in St. Paul (black) and surrounding cities (blue) in the Metropolitan area of the Twin Cities (excluding the city of Minneapolis) over the period from January 2018 to July 2022.

To provide a pre-rent control benchmark, Table 2 reports sample statistics for the period January 2018 to October 2021. Panel A shows that the average transaction price of a single family home in St. Paul over the pre-rent control period is \$280,395 and the median is \$240,400. This represents a price per square foot of \$178 (average) and \$170 (median). Multi-unit properties in St. Paul sell for \$616,146 on average (\$292,500 at the median). The average property has 5 units and sells for \$134,139 per unit, while the median has two units and sells for \$122,450 per unit. Nearly 7% of the transactions in St. Paul are rental properties, with an average rent of \$1,620 per month, and \$1,375 at the median.

In comparison, in the suburbs of St. Paul, transaction prices of single-family properties are higher though the price per square foot is lower and the properties are larger. Multi-unit properties in the suburbs of St. Paul have more units and transact at higher prices, on average. The properties in the suburbs also have considerably newer construction. Panel C provides summary statistics for single-family and small multi-family residences in the five comparable metro areas. On average, the transaction prices, sizes, and ages of transactions in the comparable areas are nearly identical to single-family transactions in St. Paul.

Table 2 – Summary Statistics of Transactions Before Rent Control

	Mean	Standard Deviation	Percentile 25th	Percentile 50th	Percentile 75th
Panel A: City of Saint Paul					
Single-Family Residences (Observations = 13,058)					
Price ($)	280,395	150,380	195,000	240,000	315,500
Square feet	1,605	673	1,174	1,495	1,876
Price per square foot ($)	178	54	137	170	211
Building age (years)	85	32	66	94	109
Multi-Unit Properties: 2+ units (Observations = 1,339)					
Price ($)	616,146	3,061,913	222,000	292,500	425,000
Square feet	5,279	26,272	1,808	2,235	3,337
Number of units	5	22	2	2	3
Price per square foot ($)	129	41	98	123	150
Price per unit ($)	134,139	56,025	96,667	122,450	158,000
Building age (years)	104	30	95	111	129
Panel B: Suburbs of Saint Paul					
Single-Family Residences (Observations = 126,606)					
Price ($)	365,987	215,196	245,000	315,000	422,000
Square feet	2,234	995	1,570	1,987	2,683
Price per square foot ($)	165	45	136	157	183
Building age (years)	37	25	19	34	54
Multi-Unit Properties: 2+ units (Observations = 1,198)					
Price ($)	2,443,837	8,745,832	309,342	390,000	545,000
Square feet	12,687	48,362	2,134	2,903	4,082
Number of units	16	53	2	2	4
Price per square foot ($)	137	41	110	132	156
Price per unit ($)	147,542	52,806	107,500	140,000	178,689
Building age (years)	60	24	46	57	63
Panel C: Comparable Metro Areas					
Single-Family and Small Multi-Family Residences (Observations = 677,649)					
Price ($)	370,911	273,873	204,000	315,371	456,995
Square feet	2,260	1,154	1,410	2,000	2,833
Price per square foot ($)	168	86	114	151	201
Building age (years)	37	30	14	29	56

Notes: Observations are completed real estate transactions in the pre-rent control period from January 2018 to October 2021. The suburbs of St. Paul exclude Minneapolis for reasons discussed in the paper.

B. Estimates of the Effect of Rent Control on Transaction Values

Table 3 presents estimates of Equation 2 using data on all transactions, including large multi-unit properties, from the Twin Cities area, and controlling for different levels of location fixed effects. Across the three specifications, the results show that rent control caused a statistically significant decline in transaction prices over the entire nine-month post period. The estimate of the average decline varies across the three types of geographic fixed effects from −4.4% to −5.8%.

Table 3 – Difference-In-Difference of Rent Control on Transaction Prices

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	−0.058***	−0.044***	−0.056***
	(0.012)	(0.005)	(0.007)
Additional controls	Yes	Yes	Yes
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.856	0.842	0.886
Observations	169,000	168,994	168,990

Notes: Observations include all residential real estate transactions, including single-family and apartment buildings from the Twin Cities Metro Area, excluding Minneapolis, over the period January 2018 to July 2022. *St. Paul* is a dummy variable equal to one for properties in the city of St. Paul. *Post* is a dummy variable equal to one for transactions that occur in November 2021 or later, after rent control is passed in St. Paul. Additional controls include size, age, units, and property type (multi-family, townhouse, etc.) Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Next, we control for migration from downtown areas into suburban areas. Table 4 estimates the triple-difference effect in Equation 3 using observations from St. Paul and the five comparable areas. The estimated effect is statistically and economically significant, ranging from −5.2% to −8.1%. These results imply that the decline in property values in St. Paul following rent control does not reflect a general trend common to the downtown areas of other Midwestern cities of the similar sizes.

According to the Ramsey County Assessor's Office, there are 73,103 private residential parcels in St. Paul, with an aggregated estimated market value of $24.2 billion. Using the most conservative estimate of a value loss of 4.4%, our estimates imply that rent control caused an aggregate loss of $1.06 billion dollars to property owners in St. Paul over the nine

Table 4 – Triple-Difference Effect of Rent Control on Transaction Prices for Downtown vs. Suburban Housing

	Dependent variable: ln(price)			
	(1)	(2)	(3)	(4)
Twin Cities × Post	−0.056	−0.069***	−0.071***	−0.068***
	(0.032)	(0.007)	(0.010)	(0.004)
Downtown × Post	0.034*	0.021**	0.006	0.027***
	(0.014)	(0.010)	(0.016)	(0.005)
Twin Cities × Downtown × Post	−0.079***	−0.078***	−0.052***	−0.081***
	(0.014)	(0.017)	(0.014)	(0.009)
Additional controls	Yes	Yes	Yes	Yes
Location fixed effects	Metro area	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month	Year-month
Adjusted R^2	0.710	0.858	0.793	0.896
Observations	969,346	969,175	969,235	968,915

Notes: Observations include single-family and small multi-unit real estate transactions in the St. Paul area and the five comparable metro areas over the period January 2018 to July 2022. *Downtown* is a dummy variable equal to one for properties located in the central city area of each Metro Area. *Post* is a dummy variable equal to one for transactions that occur after rent control is passed in St. Paul. *Twin Cities* is a dummy variable equal to one for properties in the Minneapolis-St. Paul Metro Area. All regressions include ln(square feet), ln(age), and dummy variables for property types. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

months since its passage. Using the upper-range of 8.1% from the triple-difference tests, the aggregate loss is $1.96 billion dollars.

C. Robustness Tests

First, to provide corroborating evidence that rent control caused values losses, we provide evidence that the law is binding. Using a difference-in-differences framework, Internet Appendix Table 5 shows that rents significantly declined by 3.6% to 8.9% relative to surrounding areas after the law was passed.[10] Next, using US Department of Housing and Urban Development (HUD) data on monthly building permits, Internet Appendix Table 6

[10]Data on rental listings for the period from October 2018 to July 2022 come from HousingLink, a not-for-profit organization created to collect information on rental markets in Minnesota and to collaborate with policy makers on housing affordability initiatives.

shows that rent control caused a statistically significant decline in building permits in St. Paul, relative to comparable areas.

Second, we show our results are not driven by poorly matched control groups. Restricting the sample to the 54,701 transactions in cities that are directly adjacent to St. Paul or Minneapolis (as mapped in Internet Appendix Figure 8), Internet Appendix Table 7 shows that property values declined by −3.0% to −4.5%, which is slightly muted compared to the main results though still highly statistically significant. Spillover effects from directly adjacent areas in these tests will reduce the distinction between treated and control properties and bias the results towards zero (Autor, Palmer, and Pathak, 2014; Campbell, Giglio, and Pathak, 2011; Anenberg and Kung, 2014). Following Kline and Moretti (2014), Internet Appendix Table 8 addresses this concern by using a control sample that only includes observations from the five comparable metro areas excluding all observations from Minnesota. The estimates range from −3.9% to −6.5% and are statistically significant. Internet Appendix Table 9, uses transactions only in the downtown areas of the comparable metro areas as controls and finds effects of −12.5% to −15.3%. Finally, Internet Appendix Table 10, shows nearly identical results when we use observations that are averaged over ZIP code, city, and block group geographic levels, rather than transaction level observations.

Third, to provide evidence to support our assumption of parallel time trends, Figure 2 presents results from an event study in which we replace the post-law dummy variable in the main regression with year-month dummy variables over the entire time period. The monthly estimates show that transaction prices in St. Paul relative to the suburbs were persistently and statistically negative following the passage of rent control. In contrast, in 41 out of 45 months prior to the passage of rent control, transaction prices in St. Paul were statistically equivalent to prices in the suburbs, conditioning on the covariates. These results indicate that the decline in property values is unlikely to reflect a long-term trend in prices and that St. Paul and its suburbs followed parallel trends prior to the introduction of rent control.

To provide additional credibility to the parallel trends assumption, Internet Appendix Table 11 reports estimates of the average treatment effect on the treated of −3.5% to −4.5% using the doubly robust improved estimator of Sant'Anna and Zhao (2020).[11] Results are similar when we restrict the control sample to the adjacent cities. This estimator increases the credibility of the parallel trends assumption because it finds similar results but only requires that the parallel trends assumption holds conditional on the covariates of the model.

[11]This estimator incorporates both the inverse probability-weighting approach of Abadie (2005) and the outcome regression approach of Heckman, Ichimura, and Todd (1997) to addresses concerns that the characteristics of treated and control observations are unbalanced and may influence selection in the sample.

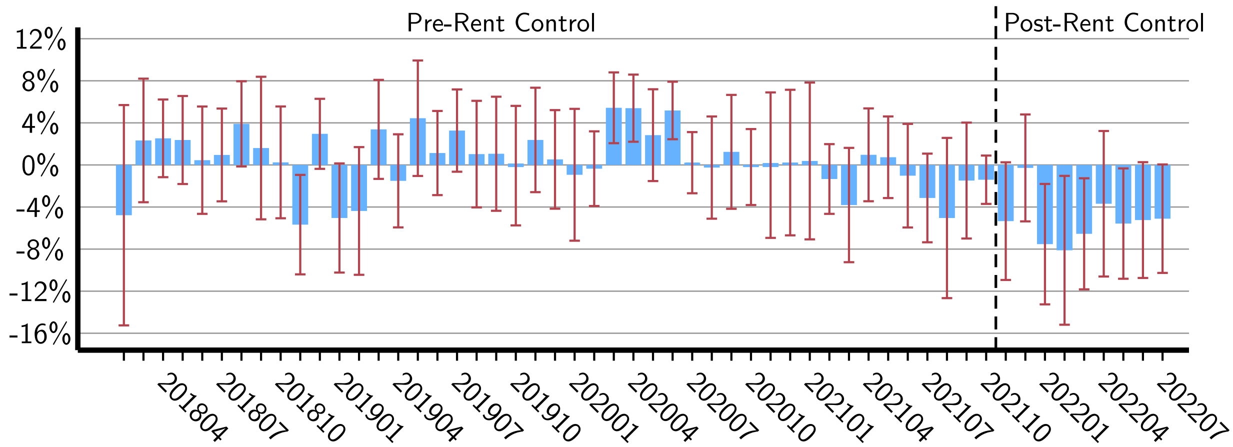


Figure 2. Real Estate Prices in St. Paul vs. Its Suburbs by Month
Notes: This figure presents coefficient estimates and their 95% confidence intervals from the interaction between dummy variables for year-months and a dummy variable for property located in St. Paul, controlling for property size, age, type, number of units, and ZIP code fixed effects. Confidence intervals are based on standard errors that are double-clustered by city and year-month. The benchmark month is 1/2018.

Similarly, Internet Appendix Figure 9 provides tests of the sensitivity of our estimates to potential differential parallel trends that evolve smoothly over time using the method of Rambachan and Roth (2023). First, we find that the negative effects in the post-period are robust to allowing for potential differential linear pre-trends in property values between St. Paul and its surrounding areas. These results are also robust to using a pre-period of nine months to match the length of the post-period. Second, following Alpert, Evans, Lieber, and Powell (2021), we conduct a placebo test where we assign transactions from the five largest cities in the St. Paul area, excluding St. Paul, to a hypothetical treatment group. We show that the maximum deviation from a linear trend for which it is possible to reject a zero effect on prices (the 'breakdown value' following Rambachan and Roth) is smaller in this placebo sample than in the true treated area of St. Paul. This indicates that the estimated treatment effect for St. Paul is highly unlikely to occur solely due to deviations from a preexisting linear trend specific to St. Paul.

Fourth, Internet Appendix Table 1 addresses selection bias concerns using a battery of difference-in-differences regressions in which the dependent variable is an observable property characteristic, such as size and age. The estimates are economically small and statistically insignificant for all property traits. Internet Appendix Figure 1 shows that the distributions of observable traits of properties sold in the two quarters preceding the ballot are nearly identical to the distributions in the quarter following the ballot. To the extent that unobservable and observable characteristics are correlated, this finding indicates that the properties that were sold after the ballot are comparable to the ones that were sold before.

Last, we use the methodology in Oster (2019) to show that our estimates are robust to even large amounts of unobservable bias in the data. We find that in order to shrink our estimates of the effect of rent control to zero, unobservables would need to have an impact on prices that is 19 times the impact of observables, which include micro-location, property size, and age.

V. Direct and Indirect Effects of Rent Control

Following our conceptual framework, the next step in our analysis is to test whether the observed decline in property values is driven by direct capitalization effects or indirect externality effects. As discussed above, the capitalization effect is amplified by the probability that a property is rented. Therefore, we test whether rental properties realize larger losses than owner-occupied properties.

Internet Appendix Table 12 shows that rent control had a larger negative impact on rental properties than owner-occupied properties. In Panel A, we find that single-family residences that are rented experience an additional loss of 7.4% to 8.2% in value beyond single-family owner-occupied properties. This implies that single-family rental properties in St. Paul have a total loss of about 12%. In Panels B and C, we show that multi-unit properties also experience negative and significant price drops. Panel B includes all multi-unit properties and Panel C limits the sample to large multi-unit buildings with at least 8, 12, or 16 units. The effects are negative and range from −4.8% loss up to −21% loss for larger units. Given the relatively small sample size in these tests, the statistically and economic significance of the results indicates that rental properties were especially impacted by rent control. Because the results are stronger for rental properties than owner-occupied properties within St. Paul, it is less likely that the results are caused by a coincident policy change specific to St. Paul that affected all properties equally.

Negative effects for both owner-occupied and rental properties are consistent with the notion that rent control caused both a sizable, direct capitalization loss as well as an indirect loss from negative externalities. In Section VII, we provide quantitative estimates of average direct effects and externalities within the city, and of differences across neighborhoods, using a calibrated model.

VI. The Redistribution of Housing Wealth Within and Across Neighborhoods

The stated goal of St. Paul's rent control law is to reduce the burden of housing costs, especially for "persons in low and moderate income households" (Saint Paul Legislative Code, 2021). It is clear from the results in the previous sections that the law generated a substantial decrease in housing wealth for properties across the entire city, including owner-occupied units. It is then natural to ask how the decrease in wealth was distributed across owners and renters, and whether the law met its goal of creating expected rent savings for low and moderate income households. The answers to these questions are not obvious, since the provisions of the law are uniform across the entire city. If the law binds differently in different neighborhoods, the most affected neighborhoods might not be locations in which low-income tenants live.

To answer these questions, this section of the paper presents a hedonic model of property values. Using the estimated changes in housing wealth, we provide evidence of the distribution of gains and losses of rent control by income and ownership status (renters, landlords, and owner-occupants). The aim of this analysis is to understand the extent to which rent control represents a progressive transfer of wealth from higher income individuals to lower income individuals. First, because landlords are likely to have higher incomes than renters, we expect that rent control is likely to be progressive at the aggregate level.

Second, beyond progessivity at the aggregate level, we also study the progressivity of the benefits of rent control within the broad classes of renters and landlords. Within-group progressivity is an essential outcome of any redistribution program. Even if aggregate transfers are progressive, it is not obvious whether the benefits and costs of rent control are progressive within classes of renters and landlords. Therefore, it is important to study whether lower-income renters receive larger benefits than higher-income renters and whether higher-income landlords bear a larger cost than lower-income landlords.

Our analysis of both aggregate and within-group progressivity follows a long tradition in economic research. For example, MaCurdy (2015) shows that though minimum wage policies are slightly progressive in the aggregate, the benefits are distributed evenly by income, while the costs are distributed regressively. Similarly, Bento, Freedman, and Lang (2015) studies the within-group costs and benefits of air quality regulations and Bertrand, Hanna, and Mullainathan (2010) consider similar issues for affirmative action in education. Finally, a long literature studies the progressivity of taxes within the class of taxpayers, independently from the distribution of their benefits (Piketty and Saez, 2007; Bento, Goulder, Jacobsen, and von Haefen, 2009; Allcott, Lockwood, and Taubinsky, 2019). Our analysis at both the aggregate and within-class levels follows this tradition.

1. Hedonic Model for Estimating Parcel-Level Value Losses

There are 78,221 parcels in St. Paul, including 73,103 residential parcels. Of the residential parcels with available data on the number of units, 64,960 are single-family residences, 6,093 are multi-unit parcels with 2–3 units, and 1,958 are apartments with four or more units. Due to missing fields in the administrative data, we can calculate the value loss for 64,654 single family residences, 5,926 two-to-three unit parcels, and 1,925 parcels with four or more units.

Of the single family residences, 90%, are owner-occupied, 7% are rentals with small landlords (defined below), and 3% are rentals with large landlords. Of the two-to-three unit parcels, 74%, are owned by small landlords, and the remaining 26% are owned by large landlords. Of parcels with four or more units, 39% are owned by small landlords and 61% are owned by large landlords. The majority of small landlords live in or near St. Paul. For all properties owned by small landlords, 89% of owners live in Minnesota, 63% live in the Twin-Cities area, and 41% live in St. Paul.

To estimate a hedonic pricing model for each residential parcel in St. Paul, we modify Equation 2 by replacing the dummy variable for St. Paul with a set of dummy variables for Census block groups in St. Paul, as follows:

$$\ln(price)_{izt} = \beta_z \cdot \alpha_z \times Post_t + \gamma X_i + \alpha_z + \tau_t + \varepsilon_{izt}. \tag{4}$$

All properties located outside of St. Paul are assigned to the same aggregate block group. This means that the β_z coefficients measure the change in prices for block group z following rent control, relative to the change in prices for the average property in the Twin Cities metro area located outside of St. Paul.[12] These regressions use the same controls as before: property type, square footage, number of units, building age, and year-month fixed effects.

Census block groups are the smallest geographic districts for which the Census Bureau publishes a wide range of demographic data. In St. Paul, there are 255 Census block groups, and the median block group represents an area of 0.01 square miles with 1,118 residents and 414 households. Thus, Equation 4 provides estimates of property values that allow for location fixed effects at a highly detailed level.

Next, we use the coefficients of Equation 4 that are estimated from transaction-level data to predict the property values for all residential parcels in St. Paul using administrative data from the Ramsey County Assessor's office. These data provide the property address, building age, and property type. For all parcels with three or fewer units, the data also provide the size in square feet. For parcels with four or more units, we use the price per unit

[12]The β_z coefficients for block groups in St. Paul would be identical if we assign all properties outside of St. Paul to their corresponding block group.

from transactions to estimate the values of these parcels. To estimate changes in property values caused by rent control, we calculate the predicted value loss of each parcel as the difference in the logged price in the post period relative to the pre-period.

Across all residential properties in St. Paul, the average predicted loss from the hedonic model is 4.6% and the median is 4.3%. These estimates fall within the range estimated from the transaction data. There is relatively little variation in value loss across property types: single family homes have losses of 4.3%, properties with two or three units have losses of 3.8%, and properties with four or more units have losses of 5.2%.

2. The Incomes of Owners and Renters

Because we cannot observe the incomes of owners and renters at the parcel level, we perform our analysis at the most granular level available, the Census block group level, using data from the 2019 five-year estimate from the American Community Survey (ACS). Household income is defined as wages, salary, interest, dividends, net rental income, retirement, and public assistance. Thus, the measure accounts for landlords whose wealth is driven by rents rather than wages.[13]

We proxy for a renter's income using the block group-by-tenure level data from the ACS.[14] The ACS provides income only at the census tract level. To exploit block group-level variation in transfers, we estimate renters' income as the average block group-level income scaled by the ratio of renter income to all residents' income at the census tract-level. This provides proxies for income at the parcel level separately for owners and renters.

Unlike renters, to estimate owners' incomes we need to first identify where they reside. To do so, we use the assessor data to identify the address of each parcel's owner and map these addresses to block group-level Census data. However, we first need to verify whether the owner's address is residential or commercial. It is possible that an address is located in a commercial building on a residential block, such as an office building or mail center. Using this address to identify the owners' income would incorrectly attribute the income of the office location to the owners themselves. Therefore, we collect the US Postal Service's residential delivery indicator (RDI) for all of the owners' addresses in St. Paul using an address verification service. If the RDI indicates that an owners' address is a commercial address, we do not record the owners' income data. If the RDI data indicate that it is a residential address, we assume that this is the owner's residence and use the income data for the Census block group associated with this address for the owner. As we do for renters, we

[13]Data on household wealth would also be relevant, but they are not available.

[14]The Census denotes an individual's renter status as "tenure."

use block group-by-tenure level data to proxy for owner's income using tract level-by-tenure data. As an alternative to Census data, we also proxy for landlords' wealth using information on their total holdings and valuations of parcels in the St. Paul metro area.

Next, we classify properties as rental properties or owner-occupied properties. First, we assign all properties with more than one unit to be a rental.[15] For single family homes, we identify rental properties in two ways. First, St. Paul requires that all rental properties receive a fire certificate of occupancy. We collect these certificate data from the St. Paul city government. To account for properties rented without a certificate of occupancy, we also identify rental properties if they have been offered for rent in the last three years, as covered by the HousingLink data described above.

We classify owners of properties into three types: owner-occupant, small landlord, or large landlord. Owner-occupants are single family homes that are not rentals. A property has a small landlord if the property is a rental and the owner's address is residential and not the same as the property address. A property has a large landlord if the property is a rental and the owner's address is commercial. Thus, the key determinant of large versus small landlords is whether the owner's address is residential or commercial. This allows small landlords to own multi-unit properties and large landlords to own single family residences. One concern with our approach is that a 'small' landlord might have a non-residential mailing addresses like a P.O. Box, which could create a smaller distinction in the data between the outcomes of small and large landlords. However, because we find that parcels owned by large landlords have about twice as many units as parcels owned by small landlords, our classification algorithm seems reliable.

To our knowledge, this is the most detailed data on the incomes of renters and landlords possible using publicly-available, non-administrative Census data. Other papers separate landlords into corporate owners and individual owners by the name of the owner listed in tax records (Gurun, Wu, Xiao, and Xiao, 2023). Our goal is to identify the traits of landlords, regardless of whether they are organized as a corporation or not. Therefore, individual landlords listed as owners may be corporations, but if their mailing address is residential, we can impute their incomes. A related paper by Diamond, McQuade, and Qian (2019b) proxies for the race of landlords based on their first and last names. While this algorithm can reliably predict race, it cannot be used to predict income, which we proxy using the granular Census data available to the public.

[15]We assume owner-occupied multi-unit properties are more similar to rentals than to owner-occupied homes. To the degree that we have misclassified these properties as rentals, when they should have been classified as owner-occupied properties, we will find smaller empirical differences in the price effects of the average rental property versus the average owner-occupied property.

3. Aggregate Costs and Benefits of Rent Control

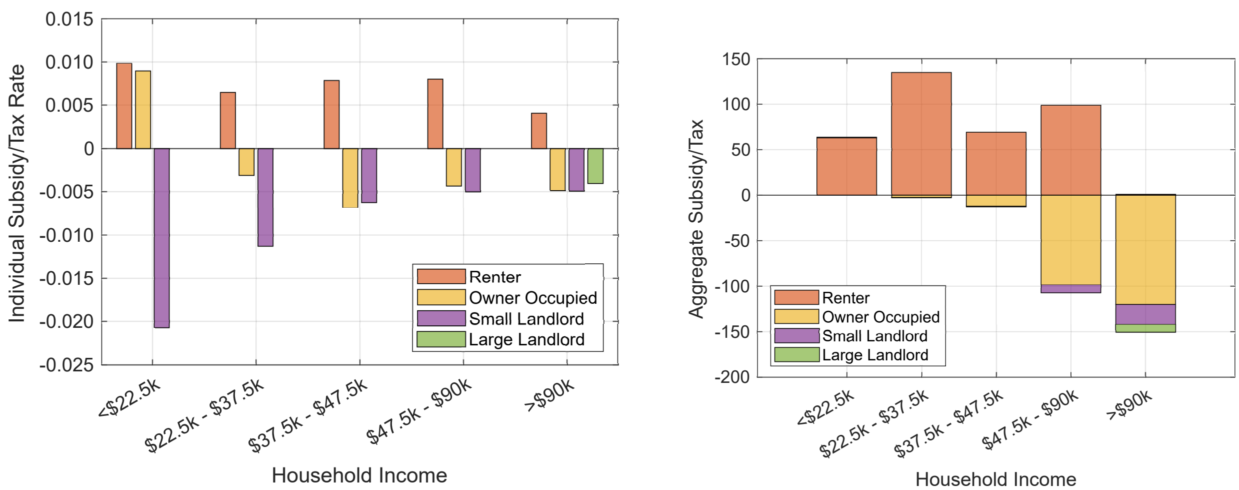
We first provide evidence on the aggregate costs and benefits of rent control by income group for all types of market participants (renters, owner-occupants, small landlords, and large landlords). To build a representative sample of the entire population, we generate observations from the parcel-level data. For owner-occupied properties, we generate one observation for the owner that includes the owner's estimated household income and the predicted loss in property value. For rental properties, we generate separate observations for renters and owners. For a property with n units, we generate n identical observations of renter household income and the negative of the per-unit estimated loss in property value. For landlords, we generate one observation of the landlord's income and the per-parcel estimated change in property value. To measure small landlords' incomes we use the Census-based measure described above. To measure the incomes of large landlords, we fit a censored lognormal distribution of incomes using the incomes of owner-occupants, small-landlords, and renters. Because large-landlords make up about 5% of the sample, we randomly draw estimates of their incomes from the top fifth percentile of the fitted distribution of incomes. Complete details are presented in the Internet Appendix and Internet Appendix Figure 2 presents the income distribution by type of participant. The median renter's income is at the 22nd percentile, the median owner-occupant's is at the 71st percentile, the median small-landlord's is at the 80th percentile, and the median large landlord's predicted income is at the 97th percentile.

For each observation, we calculate a 'subsidy/tax' rate of rent control. In particular, we calculate the yearly cash flow value of a perpetuity with a 5% discount rate where the estimated property loss is the present value of the perpetuity. We divide the yearly cash flow by household income to generate a subsidy rate if the cash flow is positive (for renters) and a tax rate if the cash flow is negative (for landlords).[16]

Figure 3 presents the tax/subsidy rates by income group and type of participant. Panel A shows that individual tax and subsidy rates vary across the income distribution. For renters with the lowest income, the subsidy rate is the highest, though for renters with household incomes above $22,500, the subsidy rate follows a hump-shaped pattern with an increasing subsidy rate between $22,500 and $90,000. In contrast, small landlords have a monotonically decreasing tax rate with income. By assumption, large landlords only appear in the highest income bracket, but their tax rates are less than small landlords of all income levels. Finally,

[16]The purpose of this procedure is to normalize the present value of property value changes by income. The choice of a 5% discount rate provides plausible cash flow values, as argued later in this paper, but the pattern of relative gains and losses by income group we document here is unaffected by this choice.

owner-occupants realize a hump-shaped tax, where the largest tax rate is for owners with incomes between $37,500 and $47,500. For the very few observations of owner-occupants in the lowest income bracket, we observe a positive effect on property values.



(a) (b)

Figure 3. Aggregate Progressivity

Notes: Individual tax/subsidy rate is the yearly cash flow equivalent of property loss caused by rent control divided by household income. The yearly equivalent is the yearly cash flow assuming property loss is the present value of a perpetuity. For renters, the subsidy is the negative of this value. Panel A presents the subsidy/tax rate for the average individual by income bracket. Panel B scales the individual tax/subsidy rates by the number of individuals of each market participant type (renter, owner-occupant, etc.) and income bracket.

To account for unequal distributions of owners, renters, and landlords in each income bracket, Panel B of Figure 3 presents the total sum of subsidies received and taxes paid per income bracket. This figure reflects the aggregate progressivity of St. Paul's rent control law. The overall net pattern of costs and benefits follows a hump-shaped pattern. The net gains are positive for renters in the lowest income bracket, though a larger share of the total gains are received by higher-income renters. As income increases, the net gains fall and those in the highest income bracket share the largest burden of the costs.

These results are consistent with a progressive impact of the law at the aggregate level, except for the lowest income bracket, for which the effects are regressive. As a class, renters benefit from the law, while owners absorb the cost, as expected. However, this analysis also shows that as a class, owner-occupants bear the largest burden of rent control, while large landlords bear a relatively small fraction.

4. Heterogeneous Wealth Effects Caused by Rent Control

To further understand the heterogeneity in wealth effects, we next focus on the within-class distributions of costs and benefits for landlords and renters. We first present univariate tests of value losses based on owner and renter incomes. Afterward, we discuss multivariate tests that account for explanatory variables simultaneously.

Panel A of Table 5 reports the number of parcels by owner and renter income categories. Among all landlords, 11% of renters have incomes below \$22,500, 42% have incomes between \$22,500 and \$37,500, 21% have incomes between \$37,500 and \$47,500, and 24% have incomes between \$47,500 and \$90,000. This pattern is nearly identical for large landlords only. Among all renters, 65% of small landlords have incomes above \$90,000, though 26% have incomes between \$47,500 and \$90,000. Thus, landlords have higher incomes than renters, on average.

Panel B of Table 5 reports that property value losses increased with renters' incomes. The size of these losses increases monotonically from 2% of the property value for renters with incomes less than \$22,500 up to 8% for renters with incomes above \$90,000. These are precise estimates with standard errors of 0.1% to 0.2%. The same pattern holds in parcels owned by large and small landlords alike and across all income levels of small landlords. Thus, across all owners, the largest value losses occurred where renters had higher incomes.

Table 5 – Property Value Loss by Owner and Renter Income

		Renter Income ($1,000s)					
		≤22.5	22.5–37.5	37.5–47.5	47.5–90.0	>90.0	All
Panel A: Number of parcels							
	All landlords	1,152	4,527	2,298	2,559	26	10,726
	Large landlords	415	1,507	774	758	7	3,519
Small landlord income ($1,000s)	>90.0	464	1,833	1,019	1,275	15	4,679
	47.5–90	174	898	387	409	4	1,895
	37.5–47.5	22	70	22	10	0	125
	22.5–37.5	15	14	5	4	0	38
	≤ 22.5	2	1	2	0	0	5

Continued on next page

		Renter Income ($1,000s)					
		≤22.5	22.5–37.5	37.5–47.5	47.5–90.0	>90.0	All
Panel B: Property value loss (% of property value)							
	All landlords	2.0	3.4	5.8	7.1	8.0	4.7
		(0.2)	(0.1)	(0.2)	(0.2)	(0.1)	(0.1)
	Large landlords	1.7	3.3	5.8	7.2	8.5	4.6
		(0.4)	(0.2)	(0.3)	(0.3)	(0.3)	(0.1)
Small landlord income ($1,000s)	>90.0	2.8	3.6	6.5	7.0	7.7	5.2
		(0.4)	(0.2)	(0.3)	(0.2)	(0.1)	(0.1)
	47.5–90	1.5	3.1	4.2	7.2	8.0	4.1
		(0.6)	(0.3)	(0.4)	(0.4)	(0.4)	(0.2)
	37.5–47.5	-0.2	1.9	3.2	9.1		2.5
		(1.8)	(1.1)	(2.2)	(2.8)		(0.9)
	22.5–37.5	5.9	3.4	13.4	2.0		5.6
		(1.6)	(2.6)	(4.9)	(3.1)		(1.4)
	≤ 22.5	2.4	6.9	0.0			2.3
		(0.3)	—	(5.5)			(2.1)
Panel C: Property value loss ($/unit)							
	All landlords	2,667	5,078	9,993	11,497	15,563	7,532
		(419)	(229)	(353)	(280)	(1,145)	(152)
	Large landlords	1,661	4,623	9,693	10,913	16,294	6,811
		(687)	(368)	(598)	(504)	(2,999)	(255)
Small landlord income ($1,000s)	>90.0	4,399	5,605	11,540	11,760	14,847	8,608
		(618)	(361)	(546)	(393)	(1,230)	(232)
	47.5–90	1,902	4,673	6,931	11,430	16,969	6,490
		(1,112)	(538)	(788)	(732)	(3,378)	(366)
	37.5–47.5	-1,288	1,225	5,135	15,029		2,794
		(3,077)	(2,346)	(3,525)	(4,705)		(1,633)
	22.5–37.5	9,327	6,100	20,008	4,294		9,093
		(2,550)	(5,236)	(6,319)	(6,318)		(2,408)
	≤ 22.5	2,855	10,326	-1,490			2,611
		(164)	—	(8,127)			(3,357)

Notes: This table presents average statistics of parcels with rental units based on owner and renter incomes. Standard errors are in parentheses. Renter incomes are denoted by column headings. Owner incomes are on rows. Large landlords do not have income data available. Panel A reports the number of parcels in St. Paul that correspond to row and column headings. Panel B (Panel C) reports the average estimated property value loss from November 2021 to July 2022, as a percentage of property value (in dollars per unit).

In contrast, Panel B also reveals that value losses vary little with owners' incomes. Large landlords have losses of 4.6% across all renters, compared to 5.2% for small landlords with incomes above \$90,000 and 4.1% for small landlords with incomes between \$47,500 and \$90,000. Comparing small landlords in the top income bracket to those in the \$47,500–\$90,000 bracket, losses are larger for higher income landlords when renters' incomes are lower, but losses are smaller for higher income landlords when renters' incomes are higher.

Panel C reports losses in dollars per unit. Across the full sample, the dollar value loss per unit is \$7,532, with a standard error of \$152. This value rises monotonically with renter income, from \$2,667 for renters with incomes below \$22,500 up to \$15,563 for renters with incomes above \$90,000. These differences are economically and statistically significant. If we compare the dollar value losses to renters' incomes, we find a near uniform ratio across all income levels of about 15%. Thus, in absolute terms, areas with higher income renters experienced substantially larger value losses. If we interpret these losses as wealth transfers from landlords to tenants, they show that higher-income tenants received larger benefits than lower-income tenants.

In contrast, Panel C shows that the dollar value loss paid by large landlords (\$6,811) is smaller than the one paid by the average landlord (\$7,532). This is driven by the fact that there are significantly smaller losses when the lowest income renters are matched with large landlords, compared to the average landlord. In general, Panel C shows that the absolute dollar value of losses is larger when renters and landlords have more similar income levels.

In unreported tests, we find that the number of units in an average property follows a U-shaped pattern across the distribution of renters' incomes, especially if landlords are large. Thus, both the highest and lowest income renters tend to live in large apartment buildings. This implies that small losses are experienced by large apartment buildings with low-income renters, while large losses are realized in large apartment buildings with high-income renters. More generally, we do not see that owners of large apartment buildings are especially targeted by the law.

A potential concern with these results is that landlord income is measured imprecisely. In particular, we cannot directly measure income for large landlords and though we use income estimates from the Census that are conditioned on being a property owner within a very small Census region, it is possible that the average owner's income in these regions is not representative of small landlords' incomes. To address this concern, we estimate losses to all landlords by wealth level, rather than income, where we estimate the wealth for individual landlords, both small and large, using tax records from the five counties surrounding St. Paul. To proxy for individual wealth we use a landlord's total number of parcels owned, total number of units owned, and the tax assessor's estimated market value of all property owned in the five-county area. Consistent with the Census-based results, the estimates in Internet Appendix Table 13 show that percentage and dollar losses per unit are roughly equal across widely varying estimates of landlord wealth, for both large and small landlords. Thus, our results on the distribution of costs across landlords is robust to using a completely different measure of landlord wealth.

To better understand the explanatory power of income, Internet Appendix Table 14 presents cross-sectional regressions of the incomes of owners and renters on the loss caused by rent control at the parcel level.[17] We find that value losses for large owners are statistically larger when renters have higher incomes, consistent with the univariate evidence. The correlations between value losses and renters' incomes for parcels owned by small landlords are nearly identical to losses of large landlords. Consistent with this, owner's income is not statistically related to the size of losses. In untabulated regressions, we also find that landlords' wealth, as proxied by number of parcels owned, is either uncorrelated or negatively correlated with landlords' losses, after controlling for property characteristics.[18]

VII. Interpretation of the Empirical Evidence

Our empirical results suggest that if we interpret value losses as wealth transfers, at the aggregate level, rent control tends to transfer wealth from higher income residents (landlords) to lower income residents (renters). In contrast, within the class of renters, those renters with the highest incomes receive the largest reductions in housing costs, while low-income renters receive the smallest, consistent with a regressive distribution of benefits. At the same time, there is little variation in losses within the class of landlords across widely varying income and wealth, consistent with a flat distribution of costs.

However, there are caveats to interpreting landlords' value losses as gains for renters because the losses may reflect other effects, such as deadweight losses, externalities, or relocation insurance. In addition, if the discount rates of owners and renters vary, the present values of future rent savings may be priced differently by renters and owners. In this section of the paper, we discuss these alternatives interpretations.

[17] Internet Appendix Table 15 reports similar results using observations aggregated to the block group level.

[18] Internet Appendix Table 16 shows that these results are robust to controlling for local housing supply elasticity, using the measure provided by Han and Baum-Snow (2021), for the number of parcels in the block group, and for the sales market liquidity (measured using the historical sales volume) of the block group.

A. Transfers vs. Deadweight Loss: Theory and Evidence

Differences in property value losses are a useful proxy for renters' housing wealth gains if losses are positively correlated with rent savings rather than deadweight losses. To verify this condition, we consider two alternative theoretical models, one based on the textbook model of rent control and the other based on a model that includes heterogeneous quality. We briefly outline the theoretical and empirical evidence here, but provide an in-depth discussion in the Internet Appendix.

In the textbook model of rent control, when demand causes market rents to increase beyond the rent cap, there are two effects. First, controlled rents are artificially low, which causes a transfer of wealth from the existing owners to the existing tenants. Second, rent control reduces the incentive to supply new housing to meet the higher demand, which causes a deadweight loss borne by new suppliers of housing. Thus, the textbook model implies that the transfer loss is borne solely by existing owners, whereas the deadweight loss is borne solely by the suppliers of new housing. Because we only estimate value losses for existing properties in St. Paul, the textbook model indicates that this loss is entirely in the form of a transfer from owners to renters, with no deadweight losses.

Empirical evidence supports the textbook model of rent control. Using variation in current rent-to-price ratios to proxy for cross-sectional variation in the expected growth rate of rents (Clark, 1995), we find a positive relationship between expected growth rates and the value loss caused by rent control. This supports the claim that the areas where rent control is expected to be more binding have bigger losses, which according to the textbook model, reflect transfers from owners to renters. These results are consistent with prior evidence that wealthier neighborhoods have higher growth rates of real estate prices (Moretti, 2013; Guerrieri, Hartley, and Hurst, 2013; Couture, Gaubert, Handbury, and Hurst, 2021).

Note that this pattern is not unique to St. Paul. Internet Appendix Figure 10 shows the relationship between rent grow between 2015 and 2021 and average income in 2015 across Zip Code Tabulation Areas from the American Community Survey, both in the raw data (Panel a), and after subtracting state and county means (Panels b and c).[19] The figure shows a strong positive association between realized rent growth and log income, within state and county. Thus, even in other jurisdictions, a rent stabilization policy with a uniform rent cap is likely to generate larger housing wealth effects in areas with higher-income residents.

[19]The sample is restricted to zip codes that experienced rent growth above 2% (approximately the average national inflation rate over the period), and excludes states with active rent control provisions (California, New York, New Jersey, Maryland, Maine, Oregon, and Minnesota).

An alternative theory of rent control is based on the model of heterogeneous quality in Frankena (1975). Rent control is set at the unit level, but the quality of housing services provided per unit varies. Thus, owners have an incentive to allow properties to deteriorate in order to charge higher prices per level of quality, while still abiding by the maximum rent allowed per unit. More elastic markets with lower barriers to entry limit the decline in quality. In the Internet Appendix, we extend Frankena's model to a dynamic setting and show that deadweight losses, as a percentage of non-controlled surplus, decline exponentially towards zero as supply elasticity increases, but transfers increase linearly. Thus, areas with more elastic supply are predicted to have larger transfers and smaller deadweight losses.

Empirical evidence shows that the larger price adjustments take place in the more elastic neighborhoods. Using Census tract-level measures of supply elasticity from Han and Baum-Snow (2021), Internet Appendix Table 17 reports a positive and significant correlation between value loss and supply elasticity, as predicted.[20] This relationship is robust to controlling for the fraction of rental housing, the volume of sales, and the number of properties with four or more units. These results support the prediction that larger price drops are correlated with larger rent savings, rather than deadweight losses.

B. A Model of Cross-Sectional Variation in Capitalization Losses

Given that value losses reflect the capitalization of future rents, rather than deadweight losses, we develop a simple pricing model to quantify the extent to which cross-sectional differences in value losses can be attributed to differences in expected rental growth rates. We provide a sketch of the model here with full details in the Internet Appendix.

There are two kinds of properties in the model: owner-occupied and rentals. We assume that the stream of rents from a rental property is equal to the consumption stream that a household can extract from owner-occupied housing, and we normalize rents to be equal to \$1 in the initial period. Following empirical observation, properties transition between rentals and owner-occupied following a Markov probability process each year. Thus, in expectation, the present value of properties that are currently owner-occupied may be affected by the future loss of rents caused by rent control.

We assume that growth rates are stochastic and that average growth rates vary across Census blocks. In particular, the logarithm of the growth rate in rents is,

$$g_{block,t+1} = \tilde{\mu}^{g}_{block} + \sigma_g e_{g,t+1},$$

[20]Internet Appendix Table 18 shows statistically significant relationships between building permits issued in St. Paul and Han and Baum-Snow's measures of supply elasticity, which helps to validate their measure.

where $\tilde{\mu}^g_{block} = \mu^g_{block} - \frac{1}{2}\sigma^2_g$, and $e_{g,t+1}$ is an *i.i.d.* Gaussian shock with mean zero and variance one. Rent control is modeled as a cap on rent growth realizations above 3%, following the initial version of the law in St. Paul.

Following prior literature on the valuation of real assets (Korteweg and Nagel, 2016), market participants discount future cash flows using an exponentially-affine stochastic discount factor (SDF) :

$$M_{t+1} = \exp(a - \gamma f_{t+1}), \tag{5}$$

where f_{t+1} is an *i.i.d.* factor. This SDF is similar to the discount factor obtained from CRRA preferences, where γ can be interpreted as the coefficient of relative risk aversion.

Finally, we calculate property values as the present value of the cash flows (or consumption streams) from each property in a census block as:

$$P_{0,block} = \sum_{\tau=1}^{T} E_0\left[M_{t+\tau} exp(g_{block,t+\tau})\right] + E_0\left[M_{t+T} P_T\right],$$

$$P_{T,block} = \frac{exp(g_{block,t+T})}{r - \mu^{g,log}_{block}},$$

where we obtain P_T by imposing the restriction that the rent-price ratio at time T has to be equal to the one currently observed in the data.

To calibrate the model, we use administrative data and estimates from prior research. In particular, the average annual transition probability from owner-occupied to rental is 3.18% and from rental to owner-occupied is 13.25%. Since our transition probability estimates are based on a 10-year period from 2010 to 2020, and a 10-year horizon is frequently used by real estate investors, we set $T = 10$.

We calibrate the expected growth rate μ^g_{block} at the Census Block-level using the perpetuity $Price_{block} = \frac{Rent_{block} - Costs}{r - \mu^g_{block}}$, where the discount rate r is 8%, following capitalization rates reported in practitioner surveys on multifamily housing in the St. Paul area, and operating costs are 2%, following the average total operating expense reported in the Rental Housing Finance Survey conducted by the US Census Bureau in 2021.

The parameters of the discount factor are calibrated to prior estimates of relative risk aversion (Cocco, Gomes, and Maenhout, 2005; Gomes and Michaelides, 2005; Chetty, Sandor, and Szeidl, 2017). We then set a so that $E_t[M_{t+1}] = exp(-3\%)$. The parameters governing the dynamics of the discount factor are calibrated to estimates of income/consumption volatility and the correlation between income and rent growth as reported in Cocco, Gomes, and Maenhout (2005) and Lockwood, Kueng, Ong, and Baker (2023).

The calibrated model is able to match the patterns we see in the data prior to rent control. In particular, we find that mean and median growth rates across blocks are approximately 3%, with an interquartile range of 2% to 4%, similar to our empirical estimates. In the Internet Appendix, we show that the model closely approximates the cross-section of rent/price ratios before the rent control provision.

Before presenting the results of the calibrated model, it is important to note that in the model, rent control only affects prices through its impact on future expected capitalized rents. In particular, the model does not capture the effect of externalities on prices. Thus, the difference between the calibrated value losses and the observed value losses can be attributed to indirect, non-capitalization effects of rent control.

First, we find that losses for owner-occupied properties (1.6%) are smaller than for rental properties (7.2%), with a difference of 6.6%, comparable to the observed difference in the data of 7–8%. The gap between the baseline prediction of 1.6% for owner-occupied properties and the observed value loss of about 4-5% implies that negative externalities explain about 66%–75% of the value loss of owner-occupied properties, consistent with Autor, Palmer, and Pathak (2014). However, the magnitude of the capitalization effect is non-trivial, suggesting that the law generates large direct effects, even for owner-occupied properties.

Second, Figure 4 shows that value losses are increasing as renters' incomes increase, consistent with the data, with stronger effects for rental properties. This is because the fraction of owner-occupied properties is higher in higher-income neighborhoods, and losses for owner-occupied properties are smaller than for rentals. Since the model only captures effects from capitalized future rents, the cross-sectional evidence shows that differences in expected rent growth can account for the cross-sectional variation by income that we observe in the data.

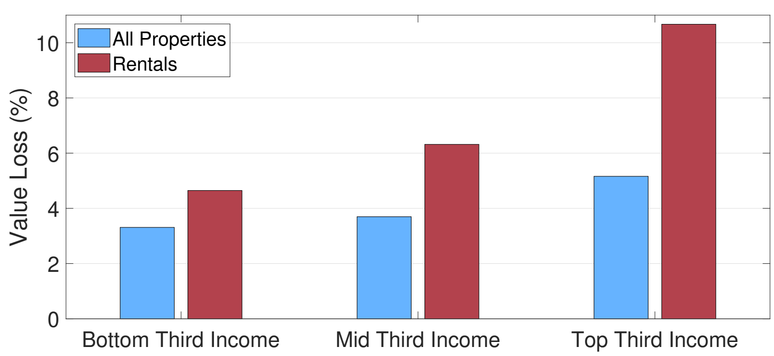


Figure 4. Model Value Losses by Census Block Group Income

Notes: This figure reports average value losses (price decreases) across Census Block Groups by income generated by the rent control law in St. Paul, according to the model developed in Section VII.

If value losses are equivalent to the present value of rent savings for tenants, then the positive relationship between renters' incomes and value losses is consistent with a regressive distribution of benefits from rent control within the renters group. The equivalence between value losses and renter benefits holds under the assumption that owners and tenants have the same discount factor for future rents. However, given that real estate markets are incomplete, and tenants may have different endowment and consumption processes than their landlords, this assumption may be violated. Instead, if tenants' and owners' have different discount factors, it is possible that the present value of the benefits received by renters could be distributed progressively, such that low-income renters receive larger benefits than high-income renters.

To evaluate the likelihood that the benefits of rent control are distributed non-regressively, we identify the assumptions on tenants' discount factors that are necessary to generate a flat distribution of benefits across income groups. First, Figure 5 shows that if we allow the volatility of the discount factor to increase with renters' incomes, it is possible to generate a flat distribution of benefits. In particular, the volatility is 11.5% for the bottom tercile of income, 15% for the mid-tercile (equal to the volatility for landlords), and 20% for the top tercile, nearly twice as high as the low-income tercile. Because the factor $f(t+\tau)$ in an equilibrium model would typically be associated with the individual's consumption process, the discount factor volatility is correlated with consumption volatility. This means that to generate a non-regressive distribution of benefits within renters, we must assume that consumption volatility is substantially larger for high-income tenants than for low-income tenants. This assumption contradicts prior empirical evidence that finds the opposite: consumption volatility is higher for low-income individuals than for high-income individuals (Cocco, Gomes, and Maenhout, 2005).[21] Thus, the assumptions about the volatility of tenants' discount factors needed to generate a non-regressive distribution of benefits are implausible.

Our second approach to generate non-regressive benefits is to allow the correlation between the discount factor and rent growth to increase with income. In order to generate the flattest distribution possible, as shown in Figure 5, we must assume that the correlation between the discount factor and rent growth is -0.45 in the bottom tercile of income, 0.7 in the mid tercile (equal to the correlation for landlords), and 1.0 in the top tercile. This generates the flattest possible distribution of benefits, though the distribution is still regressive within renters because the effects are largest in the top tercile of income for any correlation value. These

[21]The same considerations apply if we assume that the factor $f(t+\tau)$ is instead associated with individual's wealth dynamics.

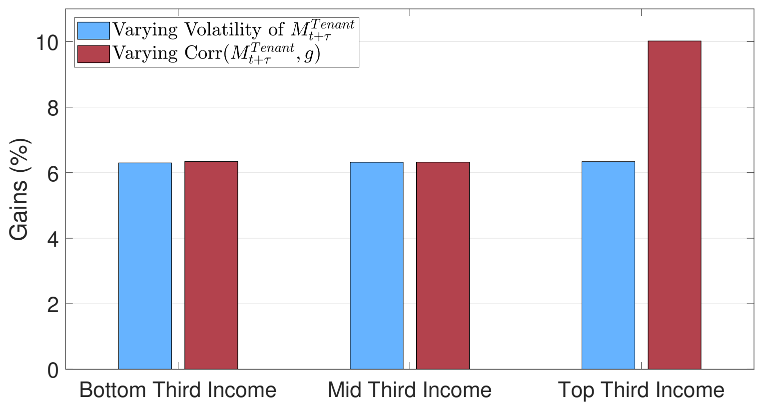


FIGURE 5. TENANT GAINS ASSUMING DISCOUNT FACTORS CORRELATE WITH INCOME
Notes: This figure reports the present value of expected tenant gains by tenant income (by Census Block average) according to the model developed in Section VII with alternative specifications of the discount factor. The first bar assumes the volatility of the tenant's discount factor increases with income. The second bar assumes that the correlation between the discount factor and rent growth increases with income.

results imply that to make the distribution of benefits less regressive, we must assume that the discount factors of lower income renters are more negatively correlated with rent growth than high income renters. This assumption seems implausible because it requires that the consumption process of lower-income tenants is more negatively correlated with local rent growth. Instead, it is more reasonable to assume that consumption and rental growth rates exhibit greater positive correlations for low-income renters than for high-income renters. Thus, the assumptions about the correlation between rent growth and tenants' discount factors needed to generate a non-regressive distribution of benefits are also implausible.

Overall, the results from the model suggest that capitalization effects can explain, at least in part, the gap between price decreases in low- and high-income neighborhoods, and that these gaps are likely correlated with differences in the positive wealth effects received by tenants across neighborhoods. This supports the claim that the distribution of benefits from St. Paul's rent control law are poorly targeted and likely regressive within tenants.

C. Non-Priced Effects

Besides differences in the pricing of future rents, other wealth effects and transfers not incorporated in prices could determine the true distribution of benefits to renters. Perhaps the most important non-priced effect is the insurance benefit against forced relocations that could occur without rent control. Though it is reasonable to assume that investors consider rent control's effect on future cash flows when purchasing rental property, it is possible that renters place a higher value on the insurance component of lower rent than do investors.

We believe the presence of unpriced insurance benefits would not reverse our findings about mistargeting for two reasons. First, the magnitude of the insurance benefits would need to be implausibly large. Panel C of Table 5 shows that the wealth transfer across all landlords was \$15,563 for renters with the highest incomes compared to \$2,667 for renters with the lowest incomes. For rent control to be targeted flatly across all income groups, the unpriced insurance benefit to the lowest income renters would need to be equal to the difference in their transfers, or \$12,896. This is an implausibly large number given that the direct, priced benefit to the lowest-income renters is only \$2,667. Moreover, for rent control to be progressive, the unpriced insurance benefit would need to be in excess of \$12,896.

Alternatively, consider the following numerical example. First note that the maximum monthly insurance benefit of rent control is the difference between market rent prices and controlled prices, not the full monthly rent. To be conservative, assume controlled prices are on average 20% below the market rent in perpetuity. Assume every year there is a 10% chance of a negative income shock that prevents a renter from paying rent for six months. Again, to be conservative, assume the lowest income renters pay the median rent in St. Paul of \$1100. In expectation, the yearly expected benefit of insurance is $20\% \times \$1100 \times 6$ months $\times 10\% = \$132$. To match the \$12,896 unpriced transfer requires a discount rate of 1.02% per year in perpetuity for a yearly benefit of \$132. This is an implausibly low discount rate. Moreover, given that all of our assumptions are extremely conservative and we are only considering the unpriced component of the wealth transfer, it seems unreasonable to imagine that the insurance benefit could be large enough so that low-income renters actually receive more benefits than high-income renters.

The second reason to doubt that unpriced insurance benefits are large enough to reverse mistargeting is based on the legal framework of the law. As discussed in Section I, during our sample period, St. Paul did not have statutory eviction control as part of its rent control law. Therefore, the insurance benefit in St. Paul is entirely provided through below market rental rates, not through statute. As we showed previously, even though landlords could not increase rents, they still had an incentive and legal right to evict low-income tenants and replace them with higher income tenants to insure themselves against non-payment of rent. Thus, in the absence of eviction control, if there is any non-priced relocation insurance, it is likely to make lower-income renters worse off than higher income renters.

D. Potential Evasion of the Law

An alternative explanation for the difference in gains between high-income and low-income renters is based on the enforcement of rent control. Landlords have an incentive to evade the law and increase rents beyond the statutory limits. If rent control causes a shortage of rental units, renters also have an incentive to offer side payments to landlords to evade the law. Higher income renters may have more resources to offer side payments. On the other hand, high-income renters also have more resources to protect their rights than low-income renters. For example, high-income renters may have more time and resources to learn about the law, to file violations with regulators, and to hire attorneys to represent their claims against unlawful rent increases.

The likelihood of evasion or lax enforcement of rent control is not observable in our data and therefore not directly testable. However, we offer indirect evidence consistent with a relationship between renters' income and evasion of the law. In St. Paul, each rental property must obtain a Fire Certificate of Occupancy through an inspection process. After inspection, a property is assigned a letter grade from A to D based on violations of safety standards. In Internet Appendix Figure 11, we show that properties located in higher income areas have statistically higher safety grades. The median income of households declines monotonically from \$62,010 for properties with A grades to \$46,708 for properties with D grades. If landlords that violate municipal safety codes are also more likely to violate rent control laws, we expect that the value transfer caused by rent control will be smaller for lower income renters.

VIII. Conclusion

This paper provides a new contribution to the debate on rent control by studying the immediate impact of St. Paul's rent control law on housing wealth. We provide evidence that the law caused statistically significant and economically large declines in property values for both rental and owner-occupied properties. This result is robust to general trends in market prices, local fixed effects, property traits, and a host of alternative controls and methods.

Using Census-based measures of household income and estimates of property value losses from administrative parcel-level data, we find that at the aggregate level, the net impacts of rent control are distributed progressively, except for the lowest income levels. In particular, between the bottom two income brackets, we find that the net benefits increase with income, while for the top three income brackets, the net benefits decline with income. This largely reflects that renters tend to have lower incomes than landlords.

In contrast to the aggregate level, within the broad classes of renters and landlords, the distribution of benefits and costs are not progressive. In particular, though the intention of the law was to benefit lower income renters, our results suggest that the largest rental cost savings were realized in the neighborhoods with the richest renters. At the same time, the absolute costs born by landlords vary little with landlord income or wealth. Thus, normalizing the absolute effects by landlord income, the tax rate on landlords is regressive. To explain the regressivity of benefits, we show that neighborhoods in St. Paul with higher incomes also have higher expected rent growth, which causes larger wealth effects when rent control is implemented with a uniform rent cap. Given that the positive correlation between income and rent growth is not unique to St. Paul, the introduction of uniform rent control will likely be poorly targeted in other cities, too.

Our results help inform future research and policy. The costs imposed by rent control provisions are typically justified towards the goal of reducing consumption inequality and increasing wealth accumulation for low-income tenants. Our results may help policy makers understand the costs and benefits of the new generation of rent control laws that are currently proliferating. Second, our results suggest that future research on the political economy of rent control is needed. Given the resurgence in rent control laws and its poor targeting, it is important to understand who votes in favor of rent control, their perception of the benefits of rent control, and the size of the benefits they actually receive.

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Online Appendix for
"The Redistribution of Housing Wealth Caused by Rent Control"
Kenneth R. Ahern and Marco Giacoletti

This Online Appendix contains additional material to support the results presented in the main text. Section I discusses selection bias concerns. Section II describes the procedure for estimating household incomes for large landlords. Section III presents two different theoretical models of rent control that help explain how deadweight loss and transfers are related to the elasticities of supply and demand. Section IV presents a simple valuation model of real estate with rent control that is calibrated to the data to provide numerical support for our empirical estimates in the main paper. Section V provides additional figures and tables referenced in the main text.

I. Selection Bias Tests

To address the threat to our identification strategy that the passage of the rent control led to selection in the kind of properties transacted in St. Paul after the ballot, we employ a range of empirical strategies. First, we show that there is no change in the composition of sold properties based on observable characteristics using difference-in-difference regressions on log square feet, log number of bedrooms or bathrooms, log age, and dummies equal to one for single family residences, townhouses, and other properties. All regressions include year-month and census block group fixed effects. Internet Appendix Table 1 shows that the coefficients are not statistically or economically significant using the entire post-reform sample from November 2021 to July 2022.

Next, Internet Appendix Figure 1 presents the mean, median, 25th, and 75th percentile of size, number of bedrooms, number of bathrooms, and construction year for properties sold in St. Paul in the two quarters preceding the ballot, and in the quarter following the ballot. The distributions appear nearly identical across quarters. Figure 1 shows that the fraction of sales that were single family residences, townhouses, multifamily buildings, and condos were nearly identical in the two quarters before the ballot and the quarter after the ballot.

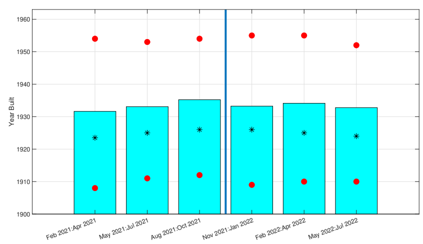
Finally, we turn to the methodology developed in Oster (2019). The procedure is analogous to, 1) estimating regressions with progressively more controls, starting from a "short regression" with only a limited set of controls, and 2) measuring how much the coefficient of interest shrinks as the R-square increases, subject to an assumption on the maximum R-square attainable (typically assumed to be 100%) in a regression that controls for all relevant observable and unobservable factors. The key statistic is the sensitivity of the magnitude of the coefficient of interest to changes in R-square, called δ. If $|\delta| = X$, then including all

Internet Appendix Table 1 – Difference-In-Difference Effect of Rent Control on Sold Property Characteristics

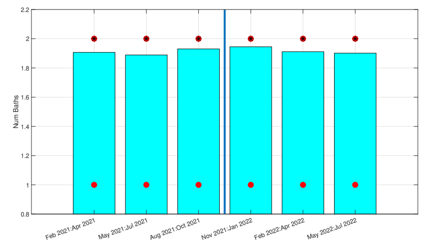
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln(square feet)	ln(building age)	ln(beds)	ln(baths)	SFR Dummy	TWNH Dummy	Other Dummy
St. Paul × Post	0.007	−0.011	0.001	−0.001	0.005	−0.002	−0.003
	(0.006)	(0.017)	(0.006)	(0.005)	(0.009)	(0.004)	(0.009)
Property type: Multi-family	0.556***	0.372***	0.583***	0.387***			
	(0.026)	(0.046)	(0.020)	(0.024)			
Property type: Single-family	0.738***	0.293***	0.747***	0.496***			
	(0.021)	(0.041)	(0.012)	(0.021)			
Property type: Townhouse	0.389***	0.002	0.345***	0.348***			
	(0.023)	(0.050)	(0.013)	(0.022)			
ln(units)	0.620***	−0.027	0.685***	0.426***			
	(0.022)	(0.016)	(0.021)	(0.025)			
Location fixed effects	Block group	Block group	Block group	Block group	Block group	Block group	Block group
Time fixed effects	Year-month	Year-month	Year-month	Year-month	Year-month	Year-month	Year-month
Adjusted R^2	0.517	0.565	0.508	0.396	0.346	0.341	0.385
Observations	168,299	169,324	168,134	168,299	169,350	169,350	169,350

Notes: Observations include real estate transactions from the Twin Cities Metro Area, excluding Minneapolis, over the period January 2018 to January 2022. *St. Paul* is a dummy variable equal to one for properties in the city of St. Paul. *Post* is a dummy variable equal to one for transactions that occur between November 2021 and July 2022, after rent control is passed in St. Paul. The omitted property type category is Condo/Co-op. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

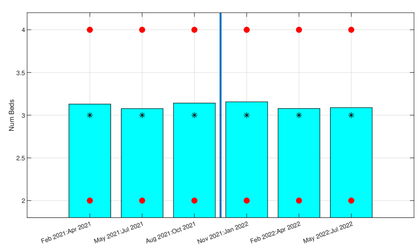
unobservable controls would shrink the coefficient of interest to zero, if the sensitivity of the coefficient to unobservables is at least X times the sensitivity of prices to observables. Our estimate of $|\delta|$ is approximately 11. Thus, to shrink our estimates to zero, unobservables would need to have an impact on prices which is 11 times as large as the impact of observables, which already include micro-location, property size and property age. This shows that our estimates are robust to even large amounts of unobservables bias in the data.



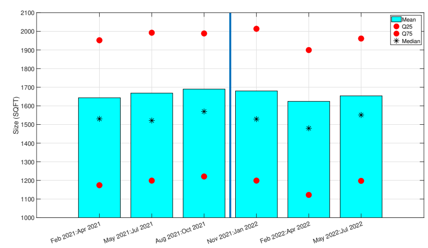
(a) Year Built



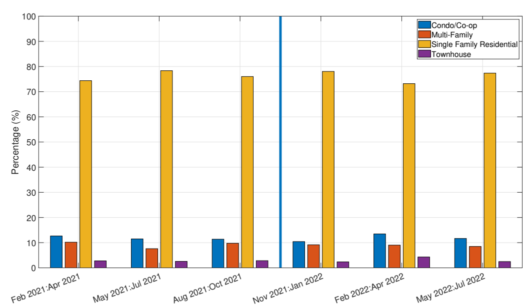
(b) Number of Bathrooms



(c) Number of Bedrooms



(d) Size

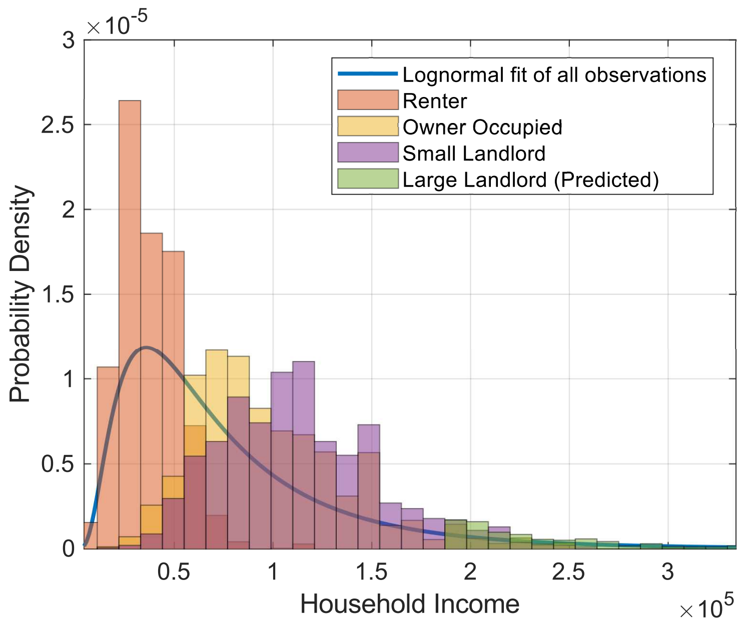


(e) Property Type

Internet Appendix Figure 1. Sample Comparison: Pre vs. Post-Rent Control
This figure presents summary statistics of properties transacted within the city of St. Paul in the three quarters before and in the quarter after the passage of the rent control provision.

II. Income Distribution

We do not have a Census-based measure of large landlords' incomes as we do for small landlords because we cannot identify a geographic location for large landlords. Therefore, to measure the incomes of large landlords, we assume income follows a lognormal distribution, as is common, and fit a censored lognormal distribution of incomes using the incomes of owner-occupants, small-landlords, and renters. We assume censoring occurs for incomes above \$250,000 and estimate the parameters of the lognormal distribution assuming all large landlords have incomes above \$250,000. We randomly draw income values for large landlords in the top 5.4% of the estimated distribution to match the sample property that 5.4% of individuals in our simulated population are large landlords. This assumes that all large landlords are in the very top of the income distribution.



Internet Appendix Figure 2. Income Distribution

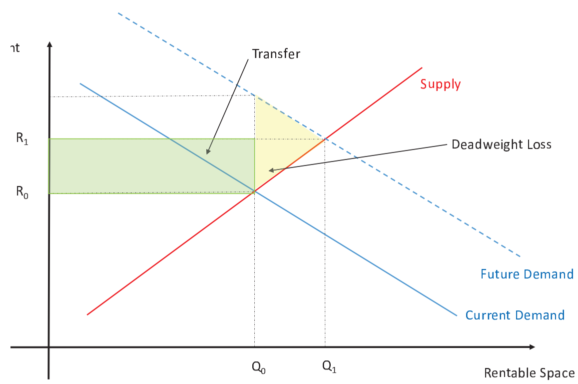
Notes: Income distribution of the simulated population of St. Paul real estate participants. The lognormal fit is estimated assuming that incomes are censored above \$250,000 to account for missing observations for large landlords. Large landlords incomes are randomly drawn from the top 5.4% of the distribution to match their fraction of the population.

Internet Appendix Figure 2 represents the income distribution by type of participant as well as the fitted lognormal distribution. Renters make up the majority of lower-income residents, followed by owner-occupants, small landlords, and large landlords at the top. The figure shows that owner-occupants' and small landlords' income distributions have a large degree of overlap. The median renter's income is at the 22nd percentile, the median owner-occupant is at the 71st percentile, the median small-landlord is at the 80th percentile and the median large landlord is at the 97th percentile.

III. Theoretical Models of Rent Control: Transfers vs. Deadweight Loss

A. Textbook Model of Rent Control

In Figure 3 we consider a stylized representation of the rental market in St. Paul, with a downward sloping demand curve and an upward sloping supply curve. Assume that there are only two periods, that quantity can adjust instantly, and that the market is in equilibrium at time 0 with rent R_0 and supply of rentable space Q_0. If an investor purchases a rental property at time 0, after the first rental payment R_0, the price of the property equals the discounted rent received in period 1. Also, assume that in period 1 there will be an increase in demand, shifting the demand curve to the right. This will lead in period 1 to larger supplied quantity ($Q_1 > Q_0$) and higher rent prices ($R_1 > R_0$).



Internet Appendix Figure 3. Supply Elasticity and Rent Control
This figure presents the effects of price controls on future rents, transfers and deadweight losses, in a simple textbook framework.

We then introduce a rent control provision that will take effect in period 1 such that $R_1 \leq R_0$. Then, in period 1 we will still have $R_1 = R_0$ and $Q_1 = Q_0$. Rent control generates at time 1 a transfer, from the landlords that were already in the market at time 0, to their tenants. Moreover, it generates a deadweight loss, due to foregone supply that is no longer added to the market at time 1. However, notice that, for the supply that was already present at time 0, the only effect of the rent control policy is the lower rent at time 1. This will in turn determine a drop in property prices already at time 0, which is going to be proportional to the difference between the controlled and the free market rent at time 1. Thus, drops in the prices of existing properties do not internalize future deadweight losses from reduced supply.

B. A Model of Heterogenous Quality

As an alternative to the textbook model, in this section, we extend the model of rent control with heterogeneous quality presented in Frankena (1975). We prevent a sketch of the model here; a full analysis is available from the authors. As Frankena argues, rental housing is not homogenous across units of housing. Instead, for each housing unit, various levels of housing services may be provided, which can be thought of as the quality of housing. With heterogeneous quality, rent per unit varies because the amount of housing services (quality) varies across units. Therefore, Frankena argues that rent is a revenue payment equal to price times quantity. The price is the price per unit of housing services, not per housing unit, and the quantity is the amount of housing services provided by the landlord. This distinction allows for landlords to supply heterogeneous housing at different price levels.

To understand how rent control affects the value of rental real estate, we analyze the aggregate value of real estate as the present value of future producer surplus. We allow quality to deteriorate at the rate of λ per unit of time t. This provides a parameterization of quantity and price as follows:

$$Q(t) = Q^\star - \lambda t \tag{IA.1}$$

$$P(t) = \frac{(1-\omega)Q^\star P^\star}{Q^\star - \lambda t} \tag{IA.2}$$

for $t = 0, \ldots, \bar{t}$, where $\bar{t}$ is such $P(t) = P^\star$. This is found as $\bar{t} = \frac{Q^\star \omega}{\lambda}$. Intuitively, $\bar{t}$ is decreasing in λ because if depreciation is faster, the market will reach the low-quality equilibrium sooner. Also, $\bar{t}$ is increasing in ω because the more restrictive is the rent cap, the longer it will take to return to the market price.

The transfer from landlords to renters at time t is as follows:

$$Transfer(t) = \omega P^\star Q^\star - \lambda P^\star t. \tag{IA.3}$$

Therefore, the size of the transfer is linear in time.

The deadweight loss of rent control to landlords at time t is as follows:

$$DWL_s(t) = \frac{1}{2}\gamma\lambda^2 t^2. \tag{IA.4}$$

Similarly, the deadweight loss to renters at time t is:

$$DWL_d(t) = \frac{1}{2}\beta\lambda^2 t^2. \tag{IA.5}$$

In contrast to the transfer, the DWL is increasing exponentially with time. The deadweight loss for owners increases as the supply curve become steeper and more inelastic. Similarly, the deadweight loss for renters increases as the demand curve becomes steeper.

The present value of the transfer is

$$PV(Transfer) = \int_0^{\bar{t}} e^{-rt}\left(\omega P \star Q^\star - \lambda P^\star t\right) dt, \tag{IA.6}$$

and the present value of the deadweight loss to landlords is

$$PV(DWL_s) = \int_0^{\bar{t}} e^{-rt}\left(\frac{1}{2}\gamma\lambda^2 t^2\right) dt. \tag{IA.7}$$

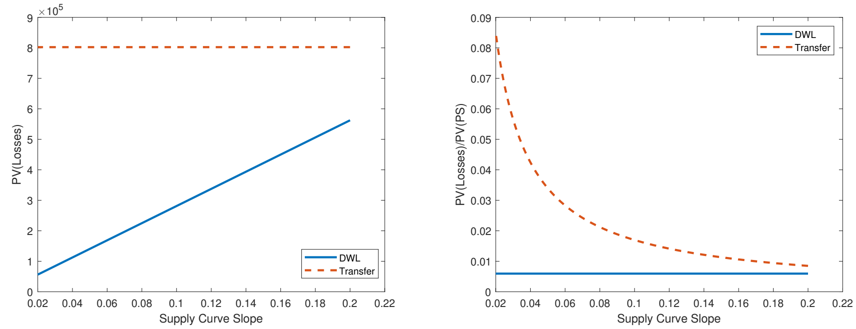
Notice that for a given free-market equilibrium $(Q^\star, P^\star)$, the shape of the demand curve is unrelated to transfers or deadweight losses to landlords in this model. The only determinants of the size of transfers and deadweight loss are the shape of the supply curve (γ), the constraint imposed by rent control (ω), and the depreciation rate (λ).

We simulate this market by setting parameters to match the data from St. Paul. Specifically, we set $P^\star = 1,375$ and $Q^\star = 6,875$, which match the rent price and number of rental units in small properties in St. Paul.[1] We set the rate of depreciation $\lambda = 3.636\%$ to match the IRS depreciation schedule for rental real estate. We assume that rent control constrains rental prices by 4%, which is based on current inflation of about 7% and a rent cap of 3%. We assume the discount rate is 5%. We allow the slope and intercept of the supply curve to vary, while holding constant the free market equilibrium $(Q^\star, P^\star)$. This allows us to show how changes in supply elasticity influence the losses experienced by landlords.

Panel (a) of Internet Appendix Figure 4 presents a graph of the present value of losses attributed to deadweight loss and transfers for changes in the slope of the supply curve. For supply curve slopes of zero to 0.2, transfer losses constitute the majority of losses. Second, the present value of deadweight losses increase linearly with the steepness of the supply curve. In contrast, transfers are unrelated to supply curve inelasticity.

Steeper supply curves affect not only the change in surplus, but they also affect the free-market surplus. Therefore, in panel (b), we normalize losses by the present value of the landlord surplus in the free-market equilibrium. This provides a simulation that more closely

[1] We use data on unit prices and quantities as a benchmark, but the model uses units of housing services (quality) which are not directly observable.



(a) Losses by Supply Curve Elasticity (b) Losses Normalized by Producer Surplus

Internet Appendix Figure 4. Present Value of Landlord Losses
This figure presents the present value of losses in the form of transfers and deadweight loss. Panel (a) presents raw losses. Panel (b) presents losses normalized by the free-market landlord surplus.

matches our empirical evidence on percentage changes in property values. Normalizing the losses, we see that deadweight loss is constant across supply curves slopes. This is because the deadweight loss is proportional to the size of the landlord surplus. In contrast, as the slope of the supply curve decreases (more elastic), the total losses increase. Because surplus is increasing with the slope of the supply curve, in relative terms, the losses caused by transfers decrease as the supply curve becomes more inelastic.[2]

In conclusion, we find that transfers are much larger than deadweight losses and that deadweight losses vary relatively little with supply elasticity. In contrast, transfers increase as supply becomes more elastic. This analysis supports our assumption that our empirical estimates of changes in property values proxy for transfers from landlords to renters.

IV. Housing Valuation Model

We assume that agents discount future cashflows using an exponentially-affine stochastic discount factor (SDF), such that between years t and $t+1$ the discount factor is:

$$M_{t+1} = \exp(a - \gamma f_{t+1}),$$

where f_{t+1} is an *i.i.d.* factor. The SDF specification is similar to the one that would obtain from CRRA preferences, and γ can be interpreted as the coefficient of relative risk aversion.

[2]All of these results hold if we assume constant elasticity supply and demand curves.

Over a period of $T = 10$ years, the discount factor is:

$$M_{t+T} = \prod_{\tau=1:T} M_{t+\tau} = \exp(aT - \gamma \sum_{\tau=1:T} f_{t+\tau}).$$

We consider two kind of assets: owner occupied properties and rental properties. For both assets, we normalize rents to be equal to \$1. Before rent control, rents are equal to the consumption stream that household can extract from owner-occupied housing. Log rent growth follows the dynamics $g_{block,t+1} = \tilde{\mu}^g_{block} + \sigma_g e_{g,t+1}$, where $\tilde{\mu}^g_{block} = \mu^g_{block} - \frac{1}{2}\sigma^2_g$, and $e_{g,t+1}$ is an *i.i.d.* Gaussian shock with mean zero and variance one. Over multiple years: $g_{block,t+T} = \sum_{\tau=1:T} g_{block,t+\tau}$. We calculate the present value of the cash flows (or consumption streams) from each property as:

$$P_0 = \sum_{\tau=1:T} E_0\left[M_{t+\tau} exp(g_{block,t+\tau})\right] + E_0\left[M_{t+T} P_T\right] \text{ and } P_T = \frac{exp(g_{block,t+T})}{r - \mu^{g,log}_{block}},$$

where we obtain P_T by imposing that the rent-price ratio has to be the same as today.

The rent control provision is modeled as a 3% cap on rent growth. We assume that there is no (direct) effect of the law on owner-occupied housing. Thus, growth dynamics for owner occupied are unchanged, while dynamics for rentals are:

$$\begin{cases} g^c_{block,t+1} & = g_{block,t+1} \quad if \quad g_{block,t+1} \leq 3\% \\ g^c_{block,t+1} & = 3\% \quad\quad\quad if \quad g_{block,t+1} > 3\% \end{cases}$$

Since the dynamics are now different between owner-occupied and rentals, it becomes critical to model transitions of properties between rental and owner-occupied. First, we model transitions for owner-occupied properties. For these properties, we use an annual transition matrix:

$$ProbTr = \begin{bmatrix} P(1,1) & P(1,2) \\ P(2,1) & P(2,2) \end{bmatrix} = \begin{bmatrix} P(Own, Own) & P(Own, Rent) \\ P(Rent, Own) & P(Rent, Rent) \end{bmatrix}$$

Transitions over multiple years are $ProbTr_\tau = ProbTr^\tau$. For rentals, we assume that ϕ of rentals are exposed to transitions into owner-occupied, based on the same matrix as owner-occupied properties, and $1 - \phi$ are not, and will remain rentals with probability one. Then, for the average owner-occupied property, the price under rent control is:

$$\begin{aligned} P^c(OwnerOcc)_0 = & \sum_{\tau=1:T} ProbTr_\tau(1,1) E_0\left[M_{t+\tau} exp(g_{block,t+\tau})\right] + ProbTr_T(1,1) E_0\left[M_{t+T} P_T\right] \\ & + \sum_{\tau=1:T} ProbTr_\tau(1,2) E_0\left[M_{t+\tau} exp(g^c_{block,t+\tau})\right] + ProbTr_T(1,2) E_0\left[M_{t+T} P^c_T\right], \end{aligned}$$

where $P_T^c = \frac{exp(g_{block,t+T}^c)}{r-\mu_{block}^{g,c,log}}$ is the price under rent control for a rental; $\mu_{block}^{g,c,log} = \mu_{block}^{g,log}$ if $\mu_{block}^{g,log} \leq 3\%$, otherwise $\mu_{block}^{g,c,log} = 3\%$.

$$
\begin{aligned}
P^c(Rental)_0 = & \sum_{\tau=1:T} (1-\phi)ProbTr_\tau(2,1)E_0\left[M_{t+\tau}exp(g_{block,t+\tau})\right] + (1-\phi)ProbTr_T(2,1)E_0\left[M_{t+T}P_T\right] \\
& + \sum_{\tau=1:T} \left[(1-\phi)ProbTr_\tau(2,2)+\phi\right]E_0\left[M_{t+\tau}exp(g_{block,t+\tau}^c)\right] \\
& + \left[(1-\phi)ProbTr_T(2,2)+\phi\right]E_0\left[M_{t+T}P_T^c\right],
\end{aligned}
$$

We then measure the effects of rent control as the change in property values:

$$
ValueLoss = -\left(\frac{P_0^c}{P_0} - 1\right)
$$

Rent control reduces the future expected growth rate of income for rental properties. Since properties transition probabilistically between the rental and owner-occupied state, both rentals and owner-occupied properties experience a price decline with rent control. Note that this model only includes direct capitalization effects, not indirect externality effects.

A. Calibration and Model Fit

We infer rent growth from Census Blockgroup-level rent/price ratios in the year before the passage of rent control. We set the discount rate r to be 8%. We believe this value is reasonable based on two different back-of-envelope calculations. First, the CBRE Cap Rate Survey for the summer of 2020 estimates a capitalization (cap) rate of 4.75%–5.25% for suburban multifamily properties in the Minneapolis-St. Paul metro area. Therefore, we set the cap rate at 5%. If we assume expected income growth rates between 3% and 4% (3% is the historical growth rate of rents in the metropolitan area over the last 10 years based on the American Community Survey, and rent growth over the year from January 2021 to January 2022 was roughly 5%), and rely on the fact that cap rates in a simple dividend discount model would be roughly equal to the difference between the discount rate and expected growth, we then obtain an estimate of the discount rate equal to roughly 8%. Second, given that the 10-year Treasury was roughly equal to 2% at the time of the rent control ballot, a discount rate of 8% implies a beta of roughly 0.75, which is in line with estimates of unlevered betas for multifamily Real Estate Investment Trusts (REITs).

We assume annual costs equal to 2% of the property value. To estimate these costs, we use data from the Rental Housing Finance Survey (RHFS) conducted by the US Census Bureau.

In 2021, the average total operating expenses in the RHFS is 2.1%. We then calculate:

$$\mu^g_{block} = r - \left(\frac{Rent_{block}}{Price_{block}} - costs\right),$$

so that μ^g_{block} is annualized expected net rent growth. Mean and median growth are close to 3%, and the interquartile range is 2% to 4%.

Prior literature has used exponential affine discount factors in the valuation of real assets such as real estate (see Korteweg and Nagel, 2016). We set the coefficient $\gamma = 6$, which is within the range of values used for relative risk aversion (RRA) in the literature (see Cocco, Gomes, and Maenhout, 2005, Gomes and Michaelides, 2005, and Chetty, Sandor, and Szeidl, 2017). We set a so that $E_t[M_{t+1}] = exp(-3\%)$.

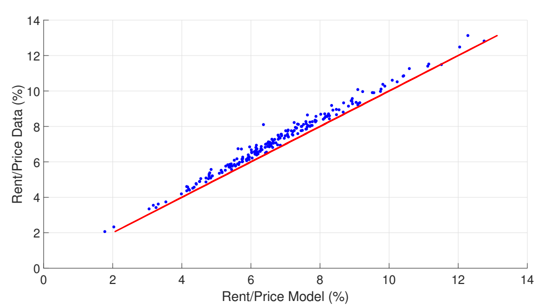
We calibrate the dynamics of f_{t+1} using data on consumption volatility from Cocco, Gomes, and Maenhout (2005), and set $\sigma_f = 15\%$, and data on the correlation between income and rent growth from Lockwood, Kueng, Ong, and Baker (2023), setting $\rho_{f,g} = 0.7$. We then set $\sigma_g = 6\%$, while μ^g_{block} for each *block* is estimated as described as above.

We use data on individual property occupancy (owner-occupied or rental) over the period from 2010 to 2020 to estimate the annual transition probabilities:

$$ProbTr = \begin{bmatrix} P(Own, Own) & P(Own, Rent) \\ P(Rent, Own) & P(Rent, Rent) \end{bmatrix} = \begin{bmatrix} 0.9682 & 0.0318 \\ 0.1325 & 0.8675 \end{bmatrix},$$

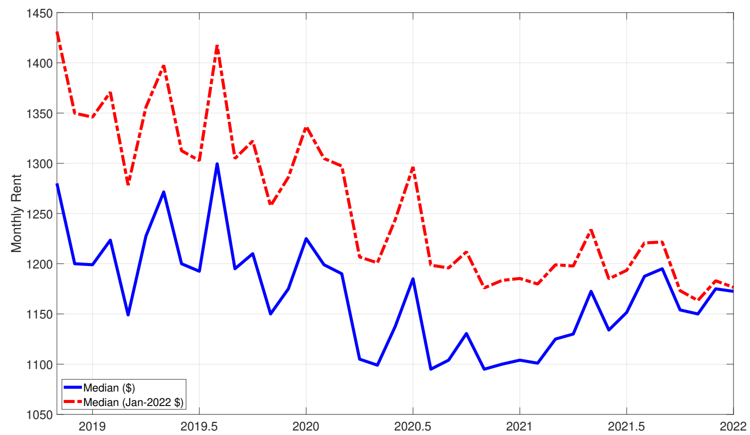
while ϕ is chosen so that the unconditional probability of being a rental unit is 55%.

We verify the fit of the model to the data by comparing (gross) rent-price ratios generated by the model before the reform to the estimates from the data. Figure 5 shows the comparison at the Census blockgroup-level, the diagonal line corresponds to a perfect match between the model and the data. Overall, the model matches well the cross-section of rent-price ratios before the reform.

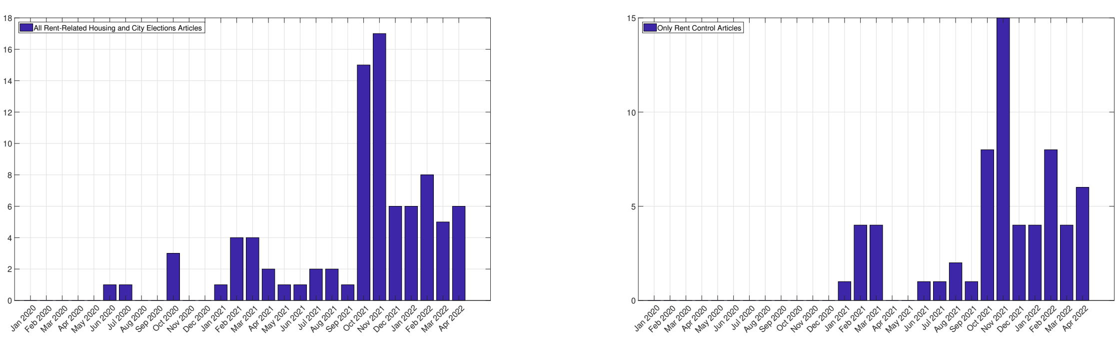


INTERNET APPENDIX FIGURE 5. RENT/PRICE RATIOS: MODEL AND DATA
The figure reports rent/price ratios estimated from the data (y-axis) and the model (x-axis) across St. Paul Census Block Groups. The solid line is the 45-degrees line.

V. Additional Figures and Tables

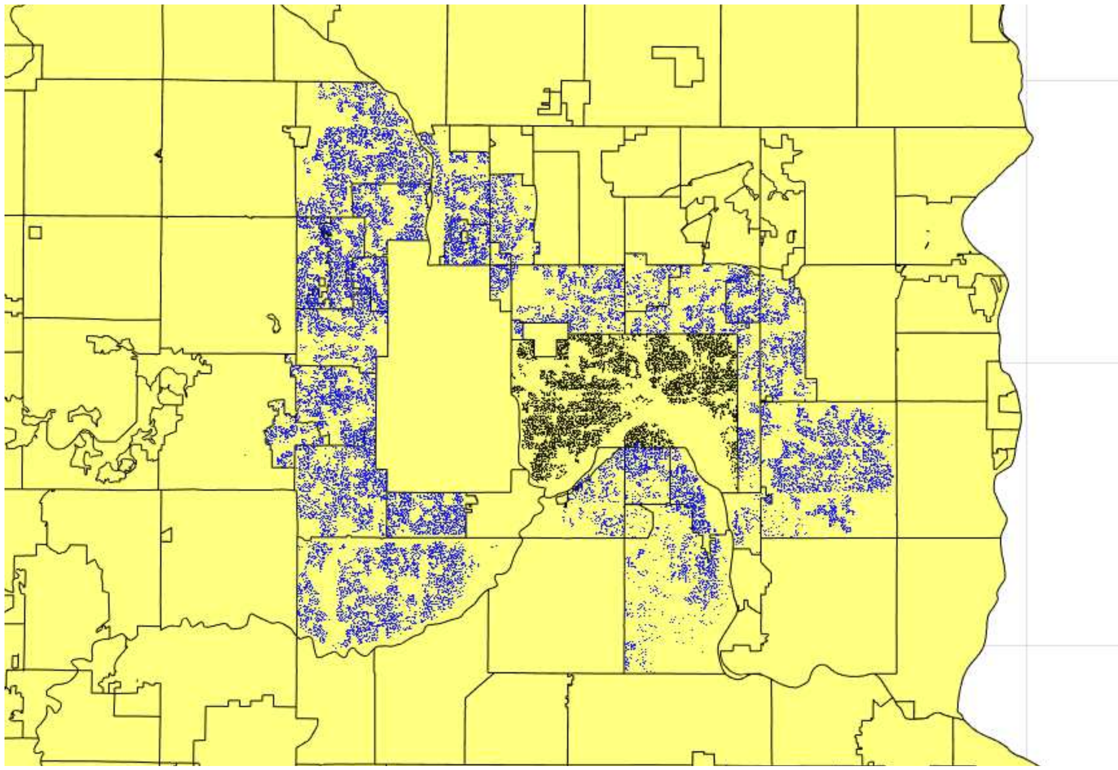


Internet Appendix Figure 6. Recent Time Series of Median Rents in St. Paul
Monthly median rents in St. Paul, based on data from HousingLink, October 2018 to December 2021. Real rents are expressed in terms of January 2022 dollars, using CPI for all Urban Consumers in the Minneapolis-St. Paul-Bloomington metro area.



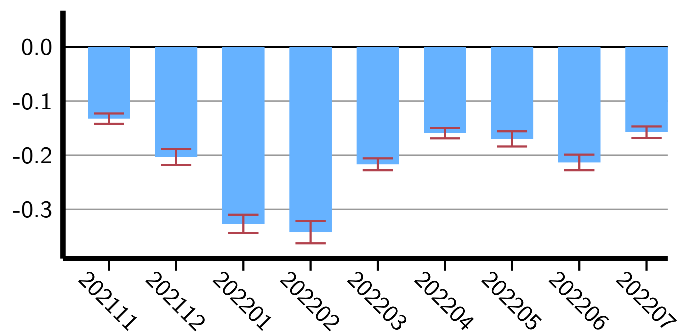
(a) Rent Control Articles (b) Rent-related and Election News

Internet Appendix Figure 7. Media Coverage of Rent Control and Elections
This figure presents time series counts of the number of news articles per month from Factiva that mention issues related to elections or rent control. Panel (a) includes searches for articles on mayoral elections, housing, and rents. Panel (b) includes searches that specifically discuss rent control.

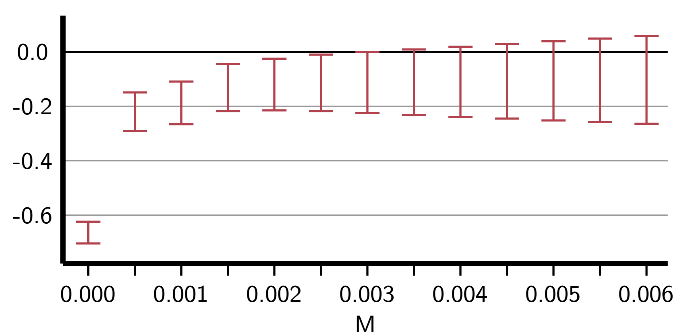


Internet Appendix Figure 8. Location of House Sales in St. Paul vs. Suburbs: Adjacent Cities

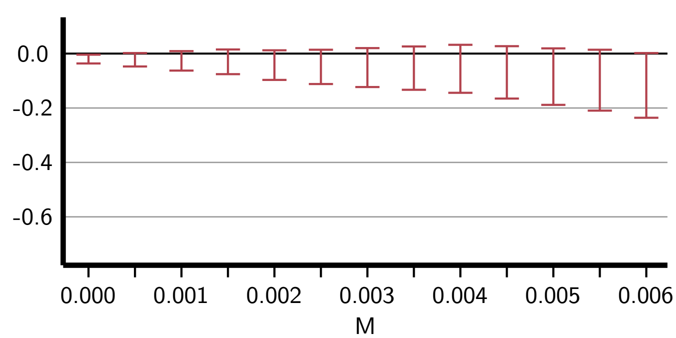
Sales within the city of St. Paul are highlighted in black, while sales in the surrounding cities are highlighted in blue.



(a) Robustness to Linear Pre-Trend

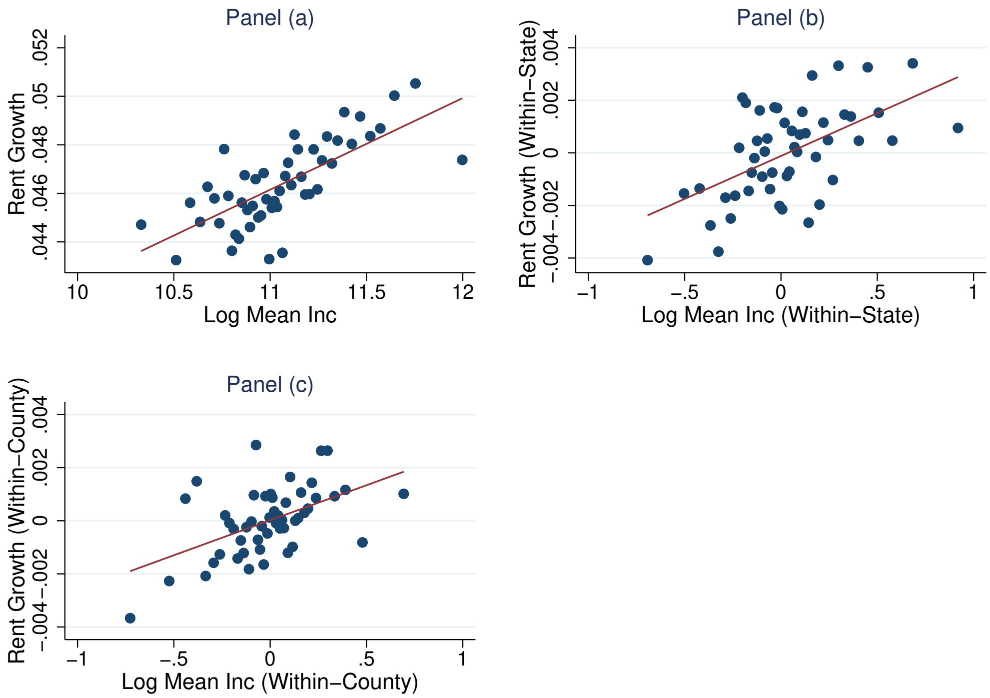


(b) St. Paul Sensitivity to Non-Parallel Trends



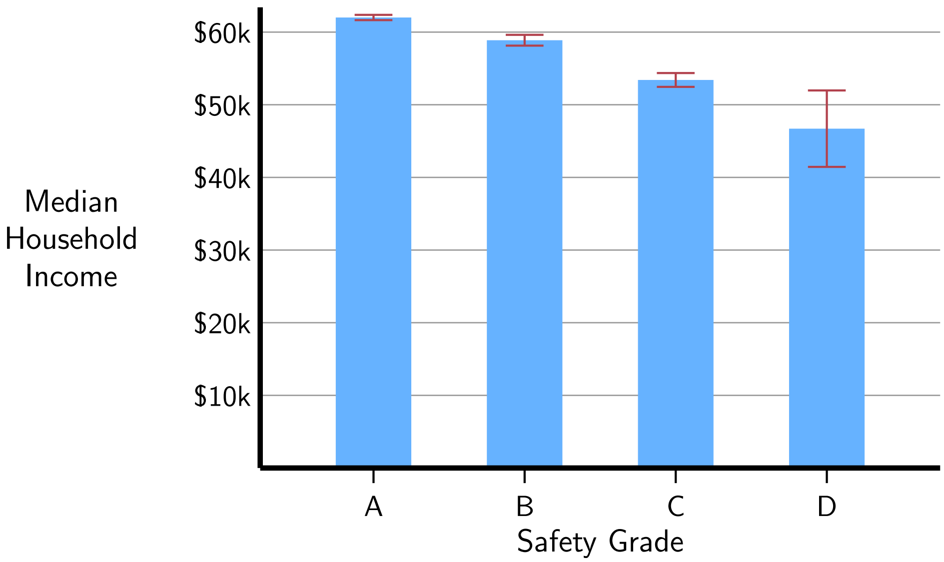
(c) Placebo Sensitivity to Non-Parallel Trends

Internet Appendix Figure 9. Sensitivity to Non-Parallel Trends
This figure uses the procedure in Rambachan and Roth (2023) to test the sensitivity of the event study results to violations of the parallel trends assumption. Panel a provides fixed length 95% confidence intervals (FLCIs) of the difference-in-difference coefficients for each month in the nine-month post-period, using the nine-months ending on October 2021 as the pre-period, where we allow for a linear time trend specific to St. Paul. Panel b provides estimates for the average treatment effect over the first three months following the passage of rent control for a post-treatment differential monthly secular growth between $\theta - M$ and $\theta + M$. M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods. Panel c provides estimates from a placebo test that replicates the results in Panel b in which we assign the five largest cities in the St. Paul area to be the treated observations, omitting observations from St. Paul.



Internet Appendix Figure 10. Log Mean Zip Code Income and Annualized Rent Growth

Notes: The binned scatter plots are based on Zip Code Tabulation Area (ZCTA) level data from the American Community Survey (ACS). Income information is from the 2015 ACS, while annualized rent growth is calculated using rents for 2 bedroom units between 2015 and 2021. The sample is restricted to states that had no active rent control regulations (thus, excluding California, New York, New Jersey, Maryland, Maine, Oregon, and Minnesota), and to ZCTAs with annualized rent growth above 2% (above U.S. inflation). In panel b, the data are demeaned by state. In panel c, the data are demeaned by county.



Internet Appendix Figure 11.
Median Household Income by Housing Code Safety Grade

Notes: This figure presents the median renter income, averaged across properties within the same safety grade. Safety grades are reported for all rental properties with a cerificate of occupancy. Properties with fewer violations have higher grades, where a grade of A is highest. Red bars indicate 95% confidence intervals.

Internet Appendix Table 2 – Demographic, Income, and Housing Statistics by Metro Area

	MSP	DEN	IND	KS	NASH	STL
Demographics						
Population (1,000,000s)	3.6	2.9	2.0	2.1	1.9	2.8
Population growth (%)	10.7	17.4	18.2	6.2	21.4	0.5
Race (% of total population)						
White	84.3	84.5	79.3	80.6	79.4	77.8
Black or African American	7.2	5.3	15.1	12.7	15.4	17.8
Asian	4.7	3.6	2.6	2.3	2.2	2.3
Hispanic or Latino origin (of any race)	3.8	17.0	4.5	6.4	4.9	2.2
White Alone, not Hispanic or Latino	82.2	71.8	76.3	76.5	75.6	76.3
Foreign-born Population (%)	10.8	12.2	7.1	7.2	9.4	4.8
Asia	4.2	3.3	2.7	2.3	2.8	2.2
Africa	2.9	1.3	1.1	0.9	1.2	0.5
Americas	2.5	6.1	2.7	3.3	4.3	1.1
Foreign-born population growth (%)	27.2	17.3	38.9	20.8	38.9	17.1
Income						
Median household income ($1,000s)	80.4	79.7	61.6	66.6	66.3	63.7
Median household income growth (%)	23.4	32.5	15.9	19.5	28.6	19.7
Below 100 percent of poverty level	8.0	8.4	10.6	9.7	10.6	10.6
Gini Index of income inequality	44.5	45.1	47.1	45.4	46.4	46.6
Housing						
Population per housing unit	2.5	2.5	2.4	2.3	2.4	2.2
Housing units growth (%)	2.3	4.7	3.8	-4.9	8.6	-8.2
Owner-occupied (%)	70.0	63.9	65.2	65.1	65.6	68.9
Fraction of housing units						
1 unit detached (%)	61.1	59.1	69.1	69.7	65.3	69.9
1 unit attached (condos)	10.4	8.2	6.0	6.4	5.0	3.7
2 units	2.4	1.3	1.7	1.9	3.0	4.0
3 or 4 units	2.0	2.7	4.0	3.9	2.6	5.4
5 to 9	2.2	4.7	6.5	5.3	5.0	4.7
10 to 19	3.7	7.2	4.9	4.4	6.2	3.5
20 or more units	16.6	15.2	5.4	6.5	8.0	5.4
Rental vacancy rate	3.5	4.3	7.0	5.4	6.3	6.5
Housing costs						
Renters						
Median rent	1,102	1,380	916	961	1,073	902
Median Gross Rent (% of hh income)	28.0	29.5	28.4	27.3	28.2	27.3
Owners						
Median monthly housing cost w/ mortgage	1,730	1,877	1,280	1,491	1,462	1,420
Median owner costs (% of hh income)	19.7	21.1	18.3	19.4	20.1	19.3

Notes: Data from the 2019 American Community Survey. Growth rates are over 2010–2019.

Internet Appendix Table 3 – Sales by Sample Cities in Minnesota

City	Sales	City	Sales	City	Sales
Afton	167	Greenfield	188	Oak Park Heights	223
Andover	2,283	Greenvale Twp	3	Oakdale*	1,906
Annandale	1	Greenwood	53	Orono	665
Anoka	1,104	Grey Cloud Island Twp	11	Osseo	110
Apple Valley	3,873	Ham Lake	851	Otsego	194
Arden Hills	442	Hampton	63	Oxford Twp	1
Bayport	235	Hampton Township	4	Pine Springs	18
Baytown Twp	45	Hampton Twp	5	Plymouth	6,120
Bethel	52	Hanover	104	Ramsey	2,281
Birchwood Village	55	Hastings	1,715	Randolph	38
Blaine	5,288	Hilltop	2	Randolph Twp	4
Bloomington*	5,097	Hopkins*	979	Ravenna Twp	17
Bradford Twp	1	Hugo	1,709	Richfield*	2,239
Brooklyn Center*	1,919	Independence	207	Robbinsdale*	1,279
Brooklyn Park*	5,267	Inver Grove Heights*	2,091	Rockford	67
Buffalo	2	Isanti	124	Rogers	1,105
Burnsville	4,135	Lake Elmo	1,246	Rosemount	2,208
Cannon Falls	63	Lake Saint Croix Be..	72	Roseville*	2,132
Castle Rock Twp	8	Lakeland	107	Saint Anthony*	486
Centerton	1	Lakeland Shores	16	Saint Bonifacius	187
Centerville	271	Lakeville	5,555	Saint Francis	699
Champlin	1,752	Lauderdale*	107	Saint Louis Park*	3,907
Chanhassen	268	Lexington	77	Saint Mary's Point	22
Chaska	246	Lilydale*	72	Saint Michael	231
Chisago City	60	Lino Lakes	1,550	Saint Paul	16,950
Circle Pines	446	Linwood Twp	138	Saint Paul Park	390
Coates	5	Little Canada*	531	Scandia	224
Cologne	1	Long Lake	122	Sciota Twp	1
Columbia Heights*	1,450	Loretto	61	Shoreview	1,765
Columbus	208	Mahtomedi	505	Shorewood	614
Coon Rapids	4,420	Maple Grove	5,981	South Saint Paul*	1,515
Corcoran	472	Maple Plain	101	Spring Lake Park	432
Cottage Grove	3,163	Maplewood*	2,374	Spring Park	71
Crystal*	1,818	Marine On Saint Croix	97	St. Paul	2
Dayton	1,113	Marshan Township	1	Stacy	149
Deephaven	254	Marshan Twp	5	Stillwater	1,805
Delano	109	May Twp	60	Stillwater Twp	34
Dellwood	74	Medicine Lake	10	Sunfish Lake*	30
Denmark Twp	24	Medina	602	Tonka Bay	105
Douglas Twp	3	Mendota	9	Vadnais Heights	782
Dundas	2	Mendota Heights*	700	Vermillion	12
Eagan	4,217	Miesville	4	Vermillion Twp	7
East Bethel	709	Minnetonka	3,867	Victoria	19
Eden Prairie	4,399	Minnetonka Beach	51	Waconia	130
Edina*	3,959	Minnetrista	849	Waterford Twp	1
Elk River	181	Mound	913	Watertown	35
Empire Twp	28	Mounds View	576	Wayzata	379
Eureka Township	1	New Brighton*	1,194	Welch	7
Eureka Twp	13	New Hope*	1,325	West Lakeland Twp	120
Excelsior	135	New Trier	8	West Saint Paul*	1,161
Falcon Heights	233	Newport*	361	White Bear Lake	1,800
Farmington	2,349	Nininger Twp	6	White Bear Twp	544
Forest Lake	1,525	North Oaks	395	Willernie	43
Fridley*	1,789	North Saint Paul*	870	Woodbury*	6,482
Gem Lake	45	Northfield	273	Woodland	31
Golden Valley*	1,661	Nowthen	175	Wyoming	97
Grant	198	Oak Grove	529		

Notes: Cities in the adjacent subsample are indicated by *.

Internet Appendix Table 4 – Sales by Counties in Full Sample

County	Sales	County	Sales
Minneapolis-St. Paul		*Denver*	
Anoka County, MN	24,931	Adams County, CO	36,253
Dakota County, MN	30,017	Arapahoe County, CO	48,120
Hennepin County, MN	60,384	Boulder County, CO	21,273
Ramsey County, MN	30,847	Broomfield County, CO	4,966
Washington County, MN	20,999	Denver County, CO	45,575
Total	167,178	Douglas County, CO	32,590
		Jefferson County, CO	42,204
Indianapolis		Weld County, CO	29,398
Boone County, IN	5,305	Total	260,379
Hamilton County, IN	30,228		
Hancock County, IN	6,418	*Kansas City*	
Hendricks County, IN	13,072	Clay County, MO	18,934
Johnson County, IN	11,713	Jackson County, MO	45,442
Madison County, IN	7,488	Johnson County, KS	44,113
Marion County, IN	62,548	Platte County, MO	7,646
Morgan County, IN	4,598	Wyandotte County, KS	7,334
Shelby County, IN	2,375	Total	123,469
Total	143,745		
		St. Louis	
Nashville		Madison County, IL	16,109
Cheatham County, TN	2,834	Monroe County, IL	1,856
Davidson County, TN	56,810	St. Charles County, MO	22,891
Robertson County, TN	5,169	St. Clair County, IL	14,843
Rutherford County, TN	29,391	St. Louis City, MO	18,226
Sumner County, TN	16,710	St. Louis County, MO	56,780
Williamson County, TN	23,757	Total	130,705
Wilson County, TN	14,243		
Total	148,914	Grand Total	974,390

Internet Appendix Table 5 – Difference-In-Difference Effect of Rent Control on Rents

	Dependent variable: ln(rent)		
	(1)	(2)	(3)
St. Paul × Q(2021/11:2022/01)	−0.022	-0.054^{***}	−0.022
	(0.018)	(0.012)	(0.014)
St. Paul × Q(2022/02:2022/04)	−0.017	-0.056^{***}	−0.013
	(0.022)	(0.008)	(0.014)
St. Paul × Q(2022/05:2022/07)	-0.036^{**}	-0.089^{***}	-0.041^{***}
	(0.017)	(0.008)	(0.011)
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
R-Square adj	0.562	0.518	0.710
N	79,793	79,784	79,716

Notes: Observations include real estate transactions from St. Paul and the Twin Cities market, excluding Minneapolis, over the period January 2018 to July 2022. *St. Paul* is a dummy variable equal to one for properties in the city of St. Paul. *Post* is a dummy variable equal to one for transactions that occur between November 2021 and July 2022, after rent control is passed in St. Paul. All regressions included property type (Condo, Duplex, Single family, Townhouse, Apartment Building). Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 6 – Effect of Rent Control on Building Permits from HUD Data

Estimation Method:	OLS	PPML	OLS	PPML
Dependent variable:	ln(1+Permits)	Permits	ln(1+Permits)	Permits
	(1)	(2)	(3)	(4)
St. Paul × Post	−0.463***	−1.171***		
	(0.075)	(0.143)		
Twin Cities × Post			−0.011	−0.026
			(0.059)	(0.135)
Downtown × Post			0.079	−0.244
			(0.128)	(0.183)
Twin Cities × Downtown × Post			−0.542***	−0.927***
			(0.085)	(0.156)
Location fixed effects	City	City	City	City
Time fixed effects	Year-month	Year-month	Year-month	Year-month
Adjusted R^2	0.637		0.825	
Observations	8,140	7,150	31,790	22,935

Notes: Observations are number of building permits issued per city by month from January 2018 to July 2022. Columns 1 and 2 only include observations from the Twin Cities Metro Areas. Columns 3 and 4 include the five comparable Metro Areas. *Downtown* is a dummy variable equal to one for properties located in the central city area of each Metro Area. *Post* is a dummy variable equal to one for transactions that occur in November 2021 through July 2022, after rent control is passed in St. Paul. *Twin Cities* is a dummy variable equal to one for properties in the Minneapolis-St. Paul Metro Area. Ordinary least squares (OLS) is used to estimate coefficients in columns 1 and 3. Pseudo-Poisson Maximum Likelihood (PPML) is used to estimate coefficients in columns 2 and 4 to account for count data. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 7 – Difference-In-Difference Effect of Rent Control on Transaction Prices Restricting to Adjacent Cities

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	-0.045^{***}	-0.030^{***}	-0.042^{***}
	(0.012)	(0.008)	(0.007)
Additional controls	Yes	Yes	Yes
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.850	0.807	0.889
Observations	71,588	71,594	71,581

Notes: Observations include all real estate transactions, including single-family and multi-family properties, from the Twin Cities Metro Area, excluding Minneapolis, and restricting control cities to those adjacent to St. Paul and Minnesota, as depicted in Internet Appendix Figure 8, over the period January 2018 to July 2022. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 8 – Difference-In-Difference Effect of Rent Control on Transaction Prices Including Observations from Comparable Metro Areas and Excluding Twin Cities Control Observations

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	-0.065^{***}	-0.039^{***}	-0.051^{***}
	(0.017)	(0.003)	(0.006)
Additional controls	Yes	Yes	Yes
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.931	0.912	0.906
Observations	6,449	6,322	64,171

Notes: Observations include all real estate transactions, including single-family and multi-family properties, from St. Paul and the five comparable metro areas, over the period January 2018 to July 2022. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 9 – Difference-In-Difference Effect of Rent Control on Transaction Prices Including Only Observations from the Downtowns of the Comparable Metro Areas

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	-0.150^{***}	-0.125^{***}	-0.153^{***}
	(0.017)	(0.018)	(0.010)
Additional controls	Yes	Yes	Yes
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.848	0.720	0.897
Observations	227,650	227,684	227,691

Notes: Observations include all real estate transactions, including single-family and multi-family properties, from St. Paul and the downtown areas of the five comparable metro areas, over the period January 2018 to July 2022. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 10 – Difference-In-Difference Effect of Rent Control on Transaction Prices Using Geographic-Level Averages

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	-0.065^{***}	-0.039^{***}	-0.051^{***}
	(0.017)	(0.003)	(0.006)
Additional controls	Yes	Yes	Yes
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.931	0.912	0.906
Observations	6,449	6,322	64,171

Notes: Observations include the monthly geographic averages of all real estate transactions, including single-family and multi-family properties, from the Twin Cities Metro Area, excluding Minneapolis, over the period January 2018 to July 2022. The geographic averages are at the geographic region level of the location fixed effects. Therefore, the unit of observation is the region level-year-month. Standard errors clustered at the year-month level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 11 – Doubly Robust Difference-in-Difference Estimate of Average Treatment Effect of Treated (ATT)

	(1)	(2)	(3)	(4)	(5)
Panel A: Entire Twin-Cities Area					
ATT	-0.042^{***}	-0.042^{***}	-0.035^{***}	-0.045^{***}	-0.045^{***}
	(0.012)	(0.008)	(0.008)	(0.010)	(0.009)
Location fixed effects	ZIP code	ZIP code	City	Block group	Block group
Time fixed effects	Year-month	Year-month	Year-month	Year-month	Year-month
Standard errors	Clustered	Bootstrap	Bootstrap	Clustered	Bootstrap
Observations	169,004	169,004	169,004	169,004	169,004
Panel B: Adjacent Cities					
ATT	-0.045^{***}	-0.045^{***}	-0.034^{***}	-0.030^{***}	-0.030^{***}
	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)
Location fixed effects	ZIP code	ZIP code	City	Block group	Block group
Time fixed effects	Year-month	Year-month	Year-month	Year-month	Year-month
Standard errors	Clustered	Bootstrap	Bootstrap	Clustered	Bootstrap
Observations	71,594	71,594	71,594	71,594	71,594

Notes: The dependent variable is a normalization of ln(price) in all real estate transactions, including single-family and multi-family properties, from the Twin Cities Metro Area, excluding Minneapolis over the period January 2018 to July 2022. Panel A includes all observations in the five counties surrounding St. Paul. Panel B restricts observations of control cities to those adjacent to St. Paul and Minnesota. ATT is the estimate of the average treatment effect on the treated, using the improved doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020). To control for geographic and time fixed effects, the dependent variable is normalized by demeaning at the year-month level and by location fixed effects listed in the column. The covariates of the estimation are ln(square footage), ln(age), ln(units), and dummies for property types. The treatment variable is a dummy variable equal to one for properties in the city of St. Paul and the time difference is a dummy variable equal to one for transactions that occur after rent control is passed in November 2021 through July 2022. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***. Standard errors are either clustered at the location level or are computed using the wild bootstrap method with 999 repetitions. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 12 – Effect of Rent Control on Transaction Prices for Rental Housing

	Dependent variable: ln(price)		
	(1)	(2)	(3)
Panel A: Single-Family Residences			
St. Paul × Post	-0.051^{***}	-0.037^{***}	-0.050^{***}
	(0.012)	(0.005)	(0.006)
St. Paul × Post × Rental	-0.082^{***}	-0.074^{***}	-0.081^{***}
	(0.027)	(0.012)	(0.022)
Additional controls	Size, age, type	Size, age, type	Size, age, type
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.851	0.839	0.883
Observations	166,112	166,108	166,102
Panel B: All Multi-Unit Residences			
St. Paul × Post	-0.057^{***}	-0.059^{***}	-0.048^{*}
	(0.020)	(0.022)	(0.025)
Additional controls	Size, age, units	Size, age, units	Size, age, units
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.948	0.927	0.951
Observations	2,881	2,875	2,688
Panel C: Large Apartment Buildings			
	8+ units	12+ units	16+ units
St. Paul × Post	-0.138^{**}	-0.209^{***}	-0.183^{**}
	(0.052)	(0.061)	(0.085)
Additional controls	Size, age, units	Size, age, units	Size, age, units
Location fixed effects	ZIP code	ZIP code	ZIP code
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.977	0.976	0.977
Observations	322	212	157

Notes: Observations include real estate transactions from the Twin Cities Metro Area, excluding Minneapolis, over the period January 2018 to July 2022. *St. Paul* is a dummy variable equal to one for properties in the city of St. Paul. *Post* is a dummy variable equal to one for transactions that occur between November 2021 and July 2022, after rent control is passed in St. Paul. *Rental* is a dummy variable equal to one for transactions of rental properties. Panel A only includes single family residences. Panel B includes properties with two or more units. Panel C includes apartment buildings with the number of units indicated at the column heading. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 13 – Loss by Estimated Landlord Wealth

	Loss per unit in dollars			Loss as a percentage of property value		
	Small Landlords	Large Landlords	All Landlords	Small Landlords	Large Landlords	All Landlords
Panel A: Total number of parcels owned by landlord						
1	9,338	7,209	8,876	5.6	4.9	5.4
	(291)	(482)	(251)	(0.2)	(0.3)	(0.1)
2	7,827	7,520	7,765	5.1	5.5	5.1
	(413)	(704)	(359)	(0.2)	(0.5)	(0.2)
3–5	6,866	6,495	6,750	4.2	4.6	4.3
	(445)	(604)	(359)	(0.2)	(0.4)	(0.2)
6–10	5,897	5,966	5,922	3.7	4.1	3.8
	(557)	(715)	(439)	(0.3)	(0.4)	(0.3)
> 10	4,388	6,757	5,996	2.6	4.2	3.7
	(639)	(469)	(379)	(0.3)	(0.3)	(0.2)
Panel B: Total number of units owned by landlord						
1	11,215	8,547	10,833	6.4	4.7	6.2
	(394)	(949)	(364)	(0.2)	(0.5)	(0.2)
2	6,309	6,240	6,297	3.9	3.8	3.9
	(378)	(854)	(346)	(0.2)	(0.5)	(0.2)
3–5	6,014	5,891	5,989	3.9	3.6	3.8
	(409)	(770)	(362)	(0.2)	(0.4)	(0.2)
6–10	5,777	6,965	6,115	3.9	4.6	4.1
	(486)	(868)	(427)	(0.3)	(0.5)	(0.2)
> 10	7,856	6,745	7,187	5.1	4.9	5.0
	(424)	(313)	(253)	(0.3)	(0.2)	(0.2)
Panel C: Estimated market value of all parcels ($1,000s)						
< 200	6,947	5,289	6,662	5.1	3.8	4.9
	(419)	(847)	(376)	(0.3)	(0.5)	(0.2)
200–300	10,319	9,053	10,145	6.0	5.2	5.8
	(495)	(1,241)	(460)	(0.3)	(0.7)	(0.2)
300–500	8,725	7,591	8,541	4.9	4.4	4.8
	(440)	(934)	(398)	(0.2)	(0.5)	(0.2)
500–1,000	7,812	6,641	7,547	4.5	4.1	4.4
	(415)	(778)	(366)	(0.2)	(0.4)	(0.2)
$> 1,000$	6,369	6,767	6,592	4.0	4.8	4.4
	(365)	(306)	(235)	(0.2)	(0.2)	(0.1)

Notes: Landlord wealth is estimated using three proxies from tax assessors' data. For each landlord in St. Paul, including corporate landlords, we record the total number of parcels, the total number of units, and the estimated market value for all parcels located in the five counties surrounding St. Paul: Anoka, Dakota, Hennepin, Ramsey, and Washington. The table provides averages of dollar loss per unit and percentage loss for bins of landlord wealth and standard errors in parentheses.

Internet Appendix Table 14 – Owners and Renters' Demographics and Property Value Losses

Dependent variable:	Property value loss (% of property value)	
Sample:	Large Landlords	Small Landlords
	(1)	(2)
Renters ln(income)	0.053**	0.055**
	(0.019)	(0.023)
Owners ln(income)		0.008
		(0.005)
Rental housing (%)	0.057	0.080
	(0.045)	(0.047)
Constant	−0.534**	−0.655**
	(0.212)	(0.247)
Adjusted R^2	0.048	0.062
Observations	3,341	6,583

Notes: The dependent variable is the estimated loss in property values caused by rent control, and the unit of observation is a rental residential parcel. Standard errors are clustered at the ZIP code level. Rental housing is the fraction of renter occupied housing units in the block group where the parcel is located, based on the 2019 ACS. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 15 – Owners and Renters' Demographics and Property Value Losses: Block Group Level

Dependent variable:	Property value loss (% of property value)	
Sample:	Large Landlords	Small Landlords
	(1)	(2)
Renters ln(income)	0.033**	0.031**
	(0.015)	(0.016)
Owners ln(income)		0.038
		(0.038)
Rental housing (%)	0.042	0.053*
	(0.027)	(0.027)
Constant	−0.316*	−0.743*
	(0.169)	(0.433)
Adjusted R^2	0.015	0.018
Observations	245	246

Notes: The dependent variable is the estimated loss in property values caused by rent control. Observations are at the block group level. Standard errors are adjusted for heteroskedasticity. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 16 – Robustness: Renter's Income Results

Dependent variable:	Value loss	
	(1)	(2)
Renters ln(income)	0.047**	0.042**
	(0.019)	(0.019)
Owners ln(income)		0.004
		(0.005)
Housing that is rental (%)	0.076*	0.094**
	(0.042)	(0.042)
New Units Elasticity	0.188**	0.310***
	(0.087)	(0.082)
ln(Sales Volume) 2018Q1:2021Q3	0.037	0.046
	(0.025)	(0.028)
ln(Num Parcels)	−0.034	−0.030
	(0.034)	(0.029)
Constant	−0.425*	−0.473*
	(0.240)	(0.242)
Adjusted R^2	0.073	0.111
Observations	3,314	6,579

Notes: The dependent variable is the estimated loss in property values caused by rent control, and the unit of observation is a rental residential parcel. Standard errors are clustered at the ZIP code level. Demographic characteristics are at the block group-level, based on data from the 2019 American Community Survey (ACS). Rental housing is the fraction of renter occupied housing units in the block group where the parcel is located, based on the 2019 ACS. *New Units Elasticity* is the measure of supply elasticity for new housing units developed by Han and Baum-Snow (2021). *ln(Sales Volume) 2018Q1:2021Q3* is the log of the number of house sales in the block group where a parcel is located, over the period from January 2018 to October 2021. *ln(Num Parcels)* is the log number of residential parcels in the block group. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 17 – Building Permits and Supply Elasticity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	ln(Num)	ln(ResNum)	ln(NewResNum)	ResPerParcel	ln(Num)	ln(ResNum)	ln(NewResNum)	ResPerParcel
New Space Elast	1.6***	1.8***	1.7**	1.3***				
	(4.0)	(3.6)	(2.2)	(6.1)				
New Units Elast					1.8***	2.1***	1.8**	1.5***
					(3.9)	(3.8)	(2.0)	(6.3)
Constant	5.9***	5.3***	2.3***	0.8***	6.0***	5.4***	2.4***	0.8***
	(181.6)	(125.7)	(41.3)	(38.8)	(134.8)	(96.5)	(30.2)	(31.4)
Adjusted R^2	0.047	0.034	0.015	0.091	0.048	0.038	0.013	0.101
Observations	246	246	246	246	246	246	246	246

Notes: Observations include total block group-level permitting activity from St. Paul, over the period from October 2018 to October 2021, and census tract level supply elasticity. We measure permitting activity as the log of the total number of issued permits (Columns 1 and 5), the log of the total number of issued residential permits (Columns 2 and 6), the log of the total number of residential new construction permits (Columns 3 and 7), the total number of residential permits per residential parcel (Columns 4 and 8). We measure census tract level elasticity using the floorspace and unit elasticity measures constructed by Han and Baum-Snow (2021). Standard errors adjusted for heteroskedasticity are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

Internet Appendix Table 18 – Value Loss and Supply Elasticity

Dependent variable:	Value loss					
	(1)	(2)	(3)	(4)	(5)	(6)
New Units Elasticity	0.235**			0.335***		
	(0.093)			(0.103)		
New Space Elasticity		0.205**			0.285***	
		(0.077)			(0.088)	
Development Elasticity			0.389			0.483
			(0.233)			(0.276)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.046	0.044	0.035	0.089	0.085	0.063
Observations	3,353	3,353	3,353	7,130	7,130	7,130

Notes: The dependent variable is the estimated loss in property values caused by rent control, and the unit of observation is a rental residential parcel. Standard errors are clustered at the ZIP code level. We measure supply elasticity at the census tract level using the new units, new floorspace, and land development elasticity measures, developed by Han and Baum-Snow (2021). All regressions include % of housing that is rental, ln(Sales Volume) which is the log of the number of house sales in the block group where a parcel is located, and ln(Num Parcels) by block group. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.