ROBBING PETER TO PAY PAUL?
THE REDISTRIBUTION OF WEALTH CAUSED BY RENT CONTROL

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Abstract

We use the price effects caused by the passage of rent control in St. Paul, Minnesota in 2021, to study the transfer of wealth across income groups. First, we find that rent control caused property values to fall sharply. A calibrated model of rent control attributes a third of these losses to indirect, negative externalities. Second, leveraging administrative parcel-level data, we find that tenants who gained more from rent control had higher incomes. Thus, to the extent that rent control is intended to benefit low-income households, the realized impact of the law was not consistent with its intention. (*JEL* D61, D62, G51, H23, R23, R31, R38)

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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 February 2022, the CoreLogic single-family rent index grew by 13.1% year-over-year, the fastest increase in almost two decades.

As housing becomes more expensive, rent control is making a resurgence. Table 1 shows that starting in 2019, 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. Given the importance of housing for consumption inequality and wealth accumulation, it is imperative to provide well-identified empirical evidence on the economic consequences of these new rent control laws.

This paper investigates two of the most important consequences of rent control: changes in property values and the redistribution of wealth caused by rent control. While basic economic analysis indicates that the outcomes of rent control include reduced supply, deadweight loss, and a transfer of wealth from property owners to renters, it is challenging to establish the causal effect of rent control on these outcomes. First, landlords endogenously respond to rent control by evading the law, neglecting maintenance, or removing properties from the rental market (Autor, Palmer and Pathak, 2014; Diamond, McQuade and Qian, 2019). Second, these outcomes are difficult to observe directly and occur gradually over many years. Similarly, a city's rent control law may evolve slowly over time. Studying market values offers a potential solution to these challenges. Because market prices are forward-looking and respond quickly to new information, they offer the opportunity to immediately observe the long-run and endogenous impacts of rent control.

To provide new evidence on the effect of rent control on property values and wealth transfers, we study the enactment of rent control in St. Paul, Minnesota in November, 2021. This is an ideal setting for a number of reasons. First, there was little anticipation of the law and no other confounding laws were passed at the same time. Second, relative to existing rent control laws in other cities, St. Paul's new law had simple, though extreme, provisions: with very few exceptions, rent growth for all residential properties in the city was capped at 3% per year without allowances for inflation nor the ability to reset rents to market prices upon vacancy. Third, the real estate located outside of St. Paul's city limits provides a similar control sample for comparison. Finally, St. Paul is a large, diverse city that allows us to study the heterogeneous impact of rent control across different property types, locations, tenants, and owners.

First, in difference-in-differences tests, we find 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 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. Using our most conservative estimate, rent control caused an aggregated loss of $1.1 billion in property value, born in large part by owner-occupants of single-family homes.

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. We obtain consistent results when we use the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020) to control for unbalanced traits in control and treated groups. In addition, 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. Finally, we verify that our results are unlikely to be caused by selection bias.

Next, we decompose the observed value loss into direct capitalization effects and indirect negative externalities. Consistent with a direct capitalization effect, we find that rental homes experienced an additional 7%–8% decline in value compared to similar owner-occupied properties, and apartment buildings with at least eight units experienced losses of more than 13% in value. We rationalize these results in a simple model of rent control that allows for stochastic growth rates and probabilistic transitions between owner-occupied and rental housing. Matching the model's parameters to the St. Paul market, we estimate that about two-thirds of the value loss is driven by capitalization effects and one-third is driven by externalities. These results suggest that capitalization effects of rent control can have a large impact on prices even for owner-occupied properties with a small likelihood of switching to the rental market.

We then investigate our second research question: how does rent control redistribute wealth? The stated intention of St. Paul's rent control is to reduce the burden of housing costs for low-income renters. To investigate whether the law achieves this objective, we first estimate the size of the wealth transfer for each parcel in St. Paul, then proxy for the incomes of renters and owners at the parcel level, and finally, test whether the wealth transfers caused by the law are larger when renters have lower incomes.

First, we argue that variation in property value losses are reliable proxies for variation in the size of transfers from owners to renters. In both a simple textbook model of rent control and a model with heterogeneous quality, we show theoretically and empirically that the cross-sectional variation in value losses we observe is driven by transfers from owners to renters, rather than deadweight losses from reduced supply.

Next, we estimate individual value losses for over 72,000 residential parcels in St. Paul using our transaction-level data as inputs into a hedonic pricing model based on location and property characteristics. The sample includes 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. The hedonic model predicts an average decline in prices of 4.6%, consistent with our transaction-level estimates.

To measure the incomes of renters and owners, we use highly granular Census data. To proxy for the incomes of renters, we use the income of the average renter living in the block group of the property address. To proxy for the incomes of owners, we first collect their addresses from the Ramsey County assessor's office. Next, we verify that the owner's address is residential, rather than commercial, using the US Postal Service's residential delivery indicator (RDI). We then classify rental properties 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 these estimates of transfers and incomes, we study the redistribution of wealth across renters and owners. After double-sorting renters and small landlords into 25 bins by joint-income levels, we find, as expected, that landlords have higher incomes than renters, on average. This indicates that rent control caused a transfer of wealth from higher income to lower income individuals. However, we find that the transfer received by renters increases monotonically with their 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. This pattern is identical if we sort renters by the likelihood of being white or having a bachelor's degree. In contrast, the size of transfers varies little with the landlords' incomes. Small landlords with incomes less than $90,000 lose 4.1% of value compared to a loss of 4.6% for large landlords of multi-unit properties. These results also hold in multivariate regressions. Thus, in contrast to the stated goals of the rent control law, we find that the largest transfers of wealth are received by renters with the highest incomes, though the burden is shared equally across all owners.

To better understand the targeting of the law, we use our simple pricing model to compare the targeting of transfers versus negative externalities. We find that properties with lower-income renters have lower expected growth in future rents. Thus, a uniform rent cap imposes a smaller constraint on these properties, which causes a smaller transfer to lower-income renters. In contrast, we find that negative externalities do not vary systematically with renters' incomes, suggesting that the externalities affect city-wide amenities, such as school quality and infrastructure.

The first contribution of this paper is to provide some of the only evidence that rent control substantially reduces property values. While prior research provides extensive evidence on rent control's influence on rent levels, housing supply, search costs, property maintenance, and tenant mobility (see Jenkins (2009) for a review of the literature), there is surprisingly little evidence on property values. To our knowledge, the only other paper that studies the effect of rent control on market values of existing dwellings is Autor, Palmer and Pathak (2014). Autor et al. 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. Our paper extends their results in important dimensions. First, while Autor et al. study the abolishment of a 1970s-era rent control law in a city with less than 100,000 residents, we study the initiation of a new, stricter rent control law in 2021 in a city of more than 300,000. Second, our larger setting allows us to include apartment buildings in our sample, which Autor et al. exclude because of data limitations. Thus, our results generalize Autor et al. to a different time period, geographic region, and institutional setting using more comprehensive data of rental real estate.[1]

The second contribution of this paper, beyond studying the effects on prices and spillovers as in Autor et al., is to provide new evidence on the targeting of rent control, including both renters and owners. 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. Sims (2007) shows similar results for Cambridge, Massachusetts in the 1990s. 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. 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). More recently, Favilukis, Mabille and Van Nieuwerburgh (2021)

[1] The only other paper we know that studies property values is Mense et al. (2019), who show that vacant land prices increased in Germany in 2015 after the passage of rent control that exempted new construction.

show theoretically that the advantages of housing policies depend on successfully targeting the benefits to the neediest households. More generally, our results on wealth transfers complement research on the effects of rent control on tenant mobility and misallocation (Glaeser and Luttmer, 2003; Diamond et al., 2019).

To our knowledge, 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 in other locations, including Cambridge (Sims, 2007; Autor et al., 2014), Vancouver (Marks, 1984), Toronto (Fallis and Smith, 1985), Los Angeles (Murray, Rydell, Barnett, Hillestad and Neels, 1991), and San Francisco (Diamond et al., 2019). Amidst a growing debate on housing affordability and regulation (Floetotto, Kirker and Stroebel, 2016; Ghent and Leather, 2021) and a proliferation of new, stricter rent control laws, we believe that studying a new rent control mandate, in a relatively large city, located in an area with no history of rent control, helps generalize past findings and also provides important evidence for understanding the future of rent control.

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). During the post-War housing boom, rents declined and the temporary rent control laws were not renewed, except in New York City (Arnott, 1995).

The second generation of rent control laws were enacted in the 1970s in response to growing inflation and as part of a general regulatory practice of price controls. New laws were passed in Massachusetts, Washington DC, and California. These second generation laws were less restrictive than the first generation of rent control laws. They allowed landlords to pass some costs on to tenants; rents to be set to market rates upon vacancies; exemptions for new construction and small landlords; and rent increases to be tied to the rate of inflation.

Following the second wave, a regulatory backlash led many states to pass laws that banned or limited rent control at the local level, including Massachusetts (1989), California (1995), and Illinois (1997). This trend continued in recent years in a wide range of states, including

Colorado (2010), Mississippi (2013), Indiana (2017), Iowa (2017), and Florida (2018). By 2019, 37 states had passed laws that preempted rent control at the local level.

Recently, as housing costs increase, the pendulum appears to have swung back in favor of rent control. As shown in Table 1, many states are revisiting their laws that preempt rent control or have enacted state-level rent control. 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 statue 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 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.[2]

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 which include vacancy decontrol provisions, 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. This means that 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 controls 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

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

properties with few units and no provisions for owner-occupants, as are common in other rent control laws.

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 take 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. 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.

II. Conceptual Framework of Rent Control and Property Values

Basic economic theory predicts that rent control causes both transfers of wealth and deadweight losses (DWL) for property owners. These losses can be divided into a direct capitalization loss and an indirect negative externality loss. The sum of these effects is observable as a decline in the market value of real estate, as follows:

$$\begin{aligned} \textit{Value Loss} = {} & \Pr(\textit{Rented}) \times (\textit{Capitalization Transfer} + \textit{DWL}) && \text{(Direct Effect)} \\ & + \textit{Negative Externality} && \text{(Indirect Effect)} \end{aligned} \tag{1}$$

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 wealth from owners to renters 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. Both of these two components of the direct effect represent a loss to owners. However, the transfer component represents a gain to renters.

The direct effect only occurs if a property is rented. 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.

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 et al., 2014). These effects could be driven by changes in such attributes as crime or school quality (Autor, Palmer and Pathak, 2019; Cellini et al., 2010). Because these externalities make the property less desirable, both for renters and owner-occupants, they represent a deadweight loss without any transfers.[3]

We use this simple conceptual framework to guide our analysis. The first step of this paper is to identify the left hand side of Equation 1, the total value loss caused by rent control. Once we have established this, the second step is to estimate the relative importance of the direct capitalization effect compared to the indirect externality effect. Finally, the third step is to decompose the direct effect into a transfer component and a deadweight loss component so that we can identify how the transfer of wealth correlates with the demographic traits of owners and renters.

A. Market Prices Capitalize Endogenous Future Expected Rents

Our empirical analysis focuses on the market value of real estate because it offers important advantages over studying rent levels, supply, or maintenance. In particular, market prices provide an easily observable summary statistic of all of the endogenous responses to rent control that are capitalized into prices, both in the short and long-run.

[3]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.

To think about the effect of rent control on property values, consider the following simple pricing model for rental housing. Assuming that net rents, $R_{i,t}$ grow at a constant expected rent growth g_i, and are discounted at rate r_i, the value of property i at time t in an uncontrolled market is,

$$V_{i,t} = \frac{R_{i,t}(1+g_i)}{r_i - g_i}. \tag{2}$$

Thus, rent control could affect the value of rental housing through three channels: the growth rate of future rents, the level of current rents, and the size of the discount rate.

Most directly, rent control restricts the growth rate of future rents. Under the law passed by ballot in St. Paul, the growth rate would have been capped at 3% per year. However, landlords and tenants would have an incentive to negotiate side payments to evade rent controls when rental housing is in short supply, such as charging high rents for furniture or appliances, or tenants offering discounts on services provided to the landlord. Similarly, the enforcement of rent control laws may be lax. The growth rate of net rents may also be impacted by maintenance costs. Gyourko and Linneman (1990) show that rent control leads owners to reduce maintenance expenditures, though Olsen (1988) argues that tenants of rent controlled units are likely to endogenously increase maintenance in response. Finally, the growth rate of rents could be affected by negative externalities from nearby properties. All of these effects will be impounded into the price, even though they may take years to be realized and are impossible for the econometrician to observe directly.

In addition, owners may endogenously exit from the rental market in response to rent control by selling rental properties to owner-occupants. In our framework, this lowers the probability of being a rental which will reduce the exposure to rent control. By studying forward-looking transaction prices, our results capture the net effect after controlling for the probability that a property exits the rental market.

Second, landlords in St. Paul have an incentive to increase current rents immediately before the passage of the law. These increases may be difficult to observe if rental contracts are privately renegotiated outside of new listings. However, the market price of real estate will incorporate the new, higher rent level, even if they are not observed by the econometrician.

Third, rent control could change the discount rate of local real estate by increasing the risk that the city will pass future rent controls. If the city is likely to pass stricter rent control laws in response to future recessions, the discount rate could increase, reducing the value of real estate. A spillover effect could also change the relative value of rental property to owner-occupied property, which could impact the riskiness of real estate (Early, 2000). Changes in discount rates are not observed directly, but they will be incorporated in prices.

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. In contrast to studies of San Francisco (Diamond et al., 2019) and Cambridge, Massachusetts (Autor et al., 2014), in which two properties on the same block could have different exposure to rent control based on building traits or ownership status, all properties within the city of St. Paul are subject to rent control. Therefore, we restrict our control sample to properties located in the five counties surrounding St. Paul. 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 could 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 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.

As an additional test to alleviate concerns that properties located in the control sample are not comparable to properties in St. Paul, we identify whether a property is a rental or owner-occupied. Following our conceptual model, we expect that rental properties will be more impacted by rent control than owner-occupied properties. The comparison between rental and owner-occupied properties 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.

To further address the concern that properties in St. Paul might be systematically different than those outside of St. Paul, 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.

A final threat to our identification is that real estate prices in St. Paul may reflect preferences for urban versus suburban locations. Though we control for geographic fixed effects which absorb time-invariant differences in demand for particular locations, if there was a coincidental increase in demand for suburban real estate at the time of the rent control vote, we could falsely attribute lower property values in St. Paul to rent control, when in fact it represents an unrelated shift in demand. Prior work demonstrates a surge in demand for suburban real estate by residents of large urban cities during the Covid pandemic (Gupta, Mittal, Peeters and Van Nieuwerburgh, Forthcoming; Ramani and Bloom, 2022). It is possible that a similar shift in preferences and reallocation of housing demand occurred in November 2021 for St. Paul buyers.

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. We choose these areas because they have roughly the same population size as the Twin Cities area and are geographically proximate. Internet Appendix Table 1 shows that the five comparable areas have similar demographic backgrounds, incomes, and housing markets as in St. Paul. In particular, though St. Paul has a higher fraction of white residents and a higher median income than the other cities, housing costs are roughly equal in the comparable cities as a fraction of income. In addition, St. Paul's population growth, immigrant growth, and income growth is in the middle of the distribution across the comparable cities.

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.

First, 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 to prices following the passage of rent control will reflect abnormal changes relative to seasonal norms and average yearly changes.

Second, we test for anticipation of the passage of the law. As noted, the ordinance was passed with a relatively close vote of 53% to 47% with 58,546 total votes cast, out of about 210,000 voting-age citizens. In Internet Appendix Figure 1, we show that media coverage of rent control issues in the St. Paul area only increased significantly in October 2021. Given that escrow periods are about four to six weeks, media coverage might have had only limited influence on the transactions that occurred before the election. In addition, to our knowledge, there was no public polling of the law in advance of the vote which could have led to substantial anticipation and response to the passage of the law.[4]

In addition, St. Paul and Minneapolis 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, using data from HousingLink, Internet Appendix Figure 2 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.

Finally, we need to 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. 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.

Similarly, we need to consider any one-time confounding events in control cities. Most notably, 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. In the control cities, there were no ballot measures and only routine school board elections.

[4]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/

To provide more evidence to support the assumption of parallel trends, we run an event study to identify if transaction prices in St. Paul followed a parallel trend with the control cities in the period before rent control was passed. To increase the credibility of the parallel trends assumption, we also 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 conditional on the covariates.

C. Econometric Specifications

Following this discussion, we estimate the following difference-in-difference equation using only data from the St. Paul area:

$$\ln(price)_{ikt} = \beta \cdot StPaul_i \times Post_t + \gamma X_i + \alpha_k + \tau_t + \varepsilon_{ikt}, \tag{3}$$

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.

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

$$\begin{aligned} \ln(price)_{ikmt} = {} & \beta \cdot TwinCities_m \times Downtown_i \times Post_t \\ & + \lambda \cdot TwinCities_m \times Post_t + \delta \cdot Downtown_i \times Post_t \\ & + \gamma X_i + \alpha_k + \tau_t + \varepsilon_{ikmt}, \end{aligned} \tag{4}$$

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.

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

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.[5] 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 (latitude and longitude). 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 the transaction of all real estate in Minnesota including address, parcel number, property usage, square footage of the buildings, 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.[6] 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. 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.

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.

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

[5] Internet Appendix Table 2 lists the number of transactions for each control city in the St. Paul area and Internet Appendix Table 3 lists the number of transactions for each county in the comparable metro areas.
[6] Non-clean sales include sales between relatives, sales of partial interest in a property, sales by government agencies, estate sales, 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 do not include details on non-market sales. In transactions of sales of multiple parcels, we read the notes of the eCRV to 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.

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

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 prices of single-family properties in the suburbs of St. Paul.

A. Estimates of the effect of rent control on transaction values

Table 3 presents estimates of Equation 3 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%.

Next, we control for migration from downtown areas into suburban areas. We first estimate a placebo tests in Panel A of Table 4 in which the sample only includes the five comparable metro areas. Across three out of four specifications of location fixed effects, we find positive and significant changes in property values for downtown vs. suburban areas following the passage of rent control in St. Paul. In the fourth specification using city-level fixed effects, we find no statistical significance.

Panel B of Table 4 estimates the triple-difference effect in Equation 4 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.

B. Robustness Tests

First, to provide corroborating evidence that the decline in real estate prices is caused by rent control, we show alternative evidence that the law is binding. First, using a difference-in-differences framework, Internet Appendix Table 4 shows that over the course of the post-period, rents decreased significantly in St. Paul, relative to its suburbs. Thus, the rent control law appears to be effective at limiting rent increases.

To test the prediction that rent control reduces the supply of new housing, we collect data from the US Department of Housing and Urban Development (HUD) on the number of monthly building permits by city. We estimate double-differences and triple-differences effects as in our main tests, replacing the dependent variable with the logged number of monthly building permits. The results in Internet Appendix Table 5 show that rent control caused a statistically significant decline in building permits in St. Paul. These findings provide additional support to our claim that the decline in property values in St. Paul was caused by rent control.

Second, to show our results are not driven by poorly matched control groups, we restrict the control observations to the cities that are directly adjacent to St. Paul or Minneapolis. Internet Appendix Table 6 reports estimates that are slightly muted compared to the main results, with a range of −3.0% to −4.5%. While using proximate properties as control observations helps alleviate concerns about omitted variables, it raises the concern that spillovers can reduce the distinction between treated and control properties (Autor et al., 2014; Campbell, Giglio and Pathak, 2011; Anenberg and Kung, 2014). This spillover effect will bias the effects of rent control on property values towards zero. To address this concern, we follow Kline and Moretti (2014) and estimate the difference-in-differences effect using a control sample that only includes observations from the five comparable metro areas excluding all observations from Minnesota. The estimates in Internet Appendix Table 7 are negative and statistically significant, ranging from −3.9% to −6.5%. In Internet Appendix Table 8, we exclude the suburbs of the five comparable metro areas and compare transactions only in the downtown areas to those in St. Paul. The effects range from −12.5% to −15.3%.

Third, to address concerns from using transaction values as observations, in Internet Appendix Table 9, we re-estimate the difference-in-differences effect using observations that are averaged over ZIP code, city, and block group geographic levels. The estimates are nearly identical to the transaction-level tests.

Fourth, to provide evidence to support our assumption of parallel time trends conditional on covariates, Figure 2 presents results from an event study of property values in St. Paul. In particular, we re-estimate Equation 3, but replace the dummy variable indicating the post rent-control period with dummy variables indicating year-months over the entire time period. Compared to the pre-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, with a slight rebound starting around April 2022, corresponding to the beginning of the reform of the law. 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 10 reports estimates of the average treatment effect on the treated (ATT) using the doubly robust improved estimator of Sant'Anna and Zhao (2020). This estimator addresses concerns that the characteristics of treated and control observations are unbalanced and may influence selection in the sample. The estimator incorporates both the inverse probability-weighting approach of Abadie (2005) and the outcome regression approach of Heckman, Ichimura and Todd (1997) to control for covariate-specific trends. The covariates are the control variables in our main tests. We normalize transaction prices by year-month and geographic fixed effects to control for time-series and spatial level changes.

The ATT estimates range from -3.5% to -4.5% across different geographic normalizations and are robust to clustered and bootstrapped standard errors. We find similar results 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.

Fifth, we address selection bias concerns with a range of empirical strategies detailed in the Internet Appendix. Internet Appendix Table 11 reports a battery of difference-in-differences regressions in which the dependent variable is an observable property characteristic, including size, number of bedrooms or bathrooms, age, and dummies for property type. We find that the difference-in-difference coefficients are economically small and statistically insignificant for all property traits. Second, Internet Appendix Figure 4 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. This procedure measures how much a regression coefficient shrinks in relation to the increase in R^2 as more control variables are included. 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.

C. Aggregate Effects

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 months since its passage. Using the upper-range of 8.1% from the triple-difference tests, the aggregate loss is \$1.96 billion dollars. Because property taxes are based on estimated market values, this decline could have significant implications for tax revenue, the dominant form of revenue for the city of St. Paul. Though St. Paul does not have fixed property tax rates, it is reasonable to expect that residents will eventually expect lower tax bills if property values decline.[7]

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.

Table 5 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

[7]Extra sales tax revenue from higher renters' spending will be largely offset by decreases in owners' spending.

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.[8]

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. Below, we refine these estimates in a calibrated model to better estimate the size of the externality.

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. For instance, a policy change that affected commute times, school quality, or public safety would not be expected to have a stronger impact on rental properties than owner-occupied properties.

A. Calibration to a Simple Model of Rental Housing Value

To connect our results to theory, we derive an extension of the simple pricing model in Equation 2 that accounts for rent control, stochastic growth rates, and the endogenous choice to supply rental housing. Using parameters based on the market in St. Paul, we use the model to predict the direct capitalization loss. Then, we compare these values to observed losses to back out the indirect externality loss. We provide a sketch of the model here, but present the full details in the Internet Appendix.

As in Equation 2, the extended model assumes that the present value of real estate equals the sum of discounted future rents. If a property is owner-occupied, we assume the implicit value received by the owner equals to the rent. We also assume the non-controlled growth rate of rents is stochastic and identical for owner-occupied and rental properties.

In a rent-controlled market, the growth rate of the implicit value to owner-occupants is not capped, but the growth rate of rent is capped, creating a capitalization loss for rental properties. However, based on historical evidence from St. Paul, we assume there is a small probability (3.18%) that an owner-occupied property switches to become a rental, and vice versa (13.28%). This means that properties that are currently owner-occupied also suffer a capitalization loss in expectation, though of smaller magnitude. In addition, because growth rates are stochastic, even if the expected growth rate is below the cap, rent control still creates a capitalization loss because the right tail of the growth rate distribution is truncated.

[8]The complete regression results for these results are reported in the Internet Appendix.

Next, we calibrate this model to the St. Paul market using the original terms of the law and estimates of value losses from the first quarter after the passage of the reform. This is for two reasons. First, in the first quarter, owners were most likely responding to the law as passed. After the city considered weakening the law, owners' expectations may have changed, which could bias our calibration. Second, evidence from Internet Appendix Table 4 suggests that in the first quarter, rent levels had not yet been affected by the reform, which helps simplify the calibration of our model. Based on practitioner surveys and Census data, we set the capitalization rate (net rent divided by property price) to be 5% and the discount rate to be 8%, based on the historical growth rate of rents of 3%.

After fixing these parameters, the model generates the capitalization loss as a function of the expected non-controlled growth rate. When the growth rate is 4.5%, the capitalization losses are roughly 10% for rentals and 5% for owner-occupied properties. These model-implied losses are similar to what we find in the data, suggesting that our empirical estimates can be rationalized in a pricing model calibrated to the St. Paul market. Second, the calibration shows that rent control can cause sizable capitalization losses even for owner-occupied properties with a relatively small probability of transitioning to become a rental.

Next, we use our model-implied capitalization losses to estimate the indirect externality losses. In particular, we identify the expected non-controlled growth rate such that the difference in the predicted value losses of rental properties and owner-occupied properties matches our empirical estimates. Under the assumption that the size of the negative externality of rent control is the same for owner-occupied and rental properties, the difference between the observed value loss in the data and the model-implied capitalization loss is an estimate of the negative externality loss.

Assuming that the transition probabilities between owner-occupied and rentals are not affected by rent control, we estimate that approximately 90% of the total value loss are capitalization losses and the remaining 10% are indirect externality losses. However, it is reasonable to assume that rent control reduces the probability that owner-occupied properties become rentals. Therefore, we re-estimate the size of the externality loss for a range of transition probabilities. The upper value of 3.18% is the historical average in St. Paul. The middle value of 2.45% corresponds to a 20% drop in the supply of rentals in the steady state and is similar to the decrease in rental supply in San Francisco reported in Diamond et al. (2019). The lower value of 1.70% is the transition probability that would create a decomposition of negative externalities similar to the one found in Autor et al. (2014).

Figure 3 presents the fraction of observed value loss attributable to the model-implied capitalization loss versus the residual externality loss. As the probability of switching from owner-occupied to rental decreases, the fraction of the observed loss attributable to a direct capitalization loss diminishes. If the probability of switching matches the evidence from San Francisco, we expect that roughly two-thirds of the value loss is attributable to direct capitalization losses, and the remainder is indirect negative externalities.

VI. The Redistribution of Wealth Caused by Rent Control

In this section of the paper, we further decompose the direct capitalization effect of rent control. As our conceptual framework in Equation 1 shows, capitalization effects include both a transfer of wealth from owners to renters and a deadweight loss. We first show theoretically and empirically that the direct capitalization loss in value caused by rent control in St. Paul is driven by transfers, not deadweight loss. We then use a hedonic model of property values to study the variation in the size of transfers by the incomes of landlords and renters.

A. Transfers vs. Deadweight Loss: Theory and Evidence

Property value losses are a useful proxy for wealth transfers if losses are positively correlated with wealth transfers. To verify this condition, we develop two alternative theoretical models, one based on the textbook model of rent control and the second 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.[9]

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, 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

[9]If rent control increases landlord-tenant matching costs, owners may bear DWL from longer vacancy periods.

more binding have bigger losses, which according to the textbook model, reflect transfers from owners to renters.

The second model 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. Initially, rent control creates a transfer of wealth from owners to renters, with no deadweight loss because quality is not immediately reduced. Over time, as owners allow quality to erode, the transfer diminishes and the deadweight loss increases. Eventually, new owners enter the market to supply more housing units of lower quality.

We extend Frankena's model to a dynamic setting and derive the present value of the transfer and the deadweight loss of owners, normalized by the producer surplus that would have been generated without rent control. We show that deadweight losses, as a percentage of non-controlled surplus, decline exponentially towards zero as supply elasticity increases, but transfer losses increase linearly as supply elasticity increases. Thus, Frankena's model predicts that areas with more elastic supply have larger percentage losses from transfers.

Empirical evidence supports Frankena's model. Using Census tract-level measures of supply elasticity from Han and Baum-Snow (2021), Internet Appendix Table 15 reports a positive and significant correlation between value loss and supply elasticity, as predicted.[10] 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 losses indicate larger transfers.

B. The Winners and Losers of Rent Control

The stated goal of St. Paul's rent control law is to improve the welfare of the residents of the city by reducing the burden of housing costs, especially for "persons in low and moderate income households" (Saint Paul Legislative Code, 2021). Unstated in the law, but implied, is the intention that the costs of rent control should be borne by higher income households, presumably the owners of rental real estate. Thus, rent control is intended as a transfer mechanism from higher income owners to lower income renters, ignoring any potential spillover effects on non-rental property.

In this section of the paper, we test whether transfers are larger when owners have higher incomes and renters have lower incomes, as intended by the law. It is important to note

[10]Internet Appendix Table 16 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.

that it is not necessary that we quantify the size of transfers nor isolate deadweight costs. Instead, we require only that variation in the predicted value losses is a valid proxy for the cross-sectional variation in the size of transfers across different areas of St. Paul.

1. Hedonic Model for Estimating Value Changes

To study transfers between owners and renters, we use a hedonic pricing model to predict the change in value for each residential parcel in St. Paul. In particular, we modify Equation 3 by replacing the dummy variable for St. Paul with a set of dummy variables for Census block groups in St. Paul, as follows:

$$\ln(price)_{izt} = \beta_z \cdot \alpha_z \times Post_t + \gamma X_i + \alpha_z + \tau_t + \varepsilon_{izt}. \tag{5}$$

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. 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 5 provides estimates of property values that allow for location fixed effects at a highly detailed level.

Next, we use the coefficients of Equation 5 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 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. Using these estimates, we define the transfer as the negative of the value loss caused by rent control.

2. The Demographic Traits 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). The demographic traits that we focus on are household income, race, and education. 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.[11] Race is defined as the probability of being white and education is defined as the probability of having a bachelors degree.

We proxy for a renter's characteristics using the block group-by-tenure level data from the ACS.[12] The ACS provides race separately for renters and owners at the block group level, but income and education 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, race, and education at the parcel level separately for owners and renters.

Unlike renters, to estimate owners' demographic traits 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' demographic profile would incorrectly attribute the demographics 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' demographic data. If the RDI data indicate that it is a residential address, we assume that this is the owner's residence and use the demographic data for the Census block group associated with this address for the owner. As for renters, we use block group-by-tenure level data to proxy for owner's race, and impute education and income using tract level-by-tenure data.

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.[13] 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.

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

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

[13]We assume owner-occupied multi-unit properties are more similar to rentals than to owner-occupied homes.

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.

3. Summary Statistics of Parcel-Level Data

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, 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.

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: rented, 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%.

Figure 4 presents a map of the estimated value loss at the census block group level, based on the average parcel loss calculated with Equation 5. There is some clustering of large losses in the northwestern part of the city and lower losses in the eastern part of the city. However, there is not an obvious geographic pattern to the losses, with areas of smaller losses located close to areas with larger losses.

4. Wealth Transfers Caused by Rent Control: Univariate Evidence

To test whether rent control benefits lower income renters at the cost of higher income landlords, we double sort rental parcels by the incomes of the renters and landlords to reflect the joint distribution of small landlords' and renters' incomes. Using the joint distribution of incomes, we then study whether transfers are larger when owners have relatively higher incomes and renters have relatively lower incomes.

Panel A of Table 6 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 6 reports the transfer of wealth from owners to renters based on owners' and renters' incomes. As renters' incomes increase, the size of the transfer received by renters 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. 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, renters with higher incomes receive larger transfers.

In contrast, Panel B also reveals that the size of transfers varies 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.

Next, Panel C shows 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. Among properties owned by large landlords, renters in the lowest income bracket live in properties with 15.8 units, decreasing to 7.4 for the middle bracket, and increasing to 26.3 units for the renters with the highest incomes. Thus, both the highest and lowest income renters tend to live in large apartment buildings. Comparing Panel C to the size of transfers in Panel B, we find that low income renters living in large apartment buildings receive small benefits, while wealthy renters living in large apartment buildings receive large benefits. More generally, we do not see that owners of large apartment buildings are especially targeted by the law.

Finally, Panel D shows that renter income and race is highly correlated. The likelihood of a renter being white increases monotonically with income from 33.5% for the lowest income renters up to nearly 100% for the highest income renters. Comparing these results to Panel B shows that the largest transfers to renters occurs when renters are white. In untabulated results, we find a nearly identical pattern for education.

In sum, the univariate results provide evidence that rent control is poorly targeted. Though rent control tends to transfer wealth from higher incomes residents (landlords) to lower income residents (renters), renters with the highest incomes receive the largest benefits, while low income renters receive the smallest benefits. At the same time, there is little variation in losses across landlords of widely varying income. If rent control increases the likelihood of foreclosure, small landlords with less financial means may bear additional costs (Diamond, Guren and Tan, 2020). In addition, owner-occupied housing also loses value, imposing costs on a large fraction of property owners in St. Paul.

5. Wealth Transfers Caused by Rent Control: Multivariate Regression Evidence

To better understand the explanatory power of each demographic trait, Table 7 presents cross-sectional regressions of the demographic traits of owners and renters on the loss caused by rent control at the parcel level.[14] We include income, race, and education separately because they are highly correlated. In all regressions we control for the fraction of rental housing in the block group of the property to absorb neighborhood effects.

In columns 1 through 3, we find that transfers from large owners to renters are statistically larger when renters have higher incomes, are more likely to be white, or are more educated. The magnitudes of the effects are economically meaningful. In columns 4 through 6, we find similar correlations between transfers and renters' demographics for parcels owned by small landlords. In contrast, the correlation between transfers and owner's demographics are weaker. Neither owner's income nor education are statistically related to the sizes of transfers. Small landlords that are white make statistically larger transfers to their tenants, but the magnitude of the effect is about a third of the magnitude for white renters.[15]

C. A Model of Cross-Sectional Variation in Transfers

We use the quantitative model presented earlier to study the cross-sectional variation in direct and indirect losses from rent control. Details of the model are in the Internet Appendix. Similar to our approach before, we use rent-to-price ratios to estimate expected growth rates in the quarter after rent control was passed to predict the direct capitalization loss for owner-occupied and rental properties. Assuming these direct effects are primarily transfers from

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

[15]Internet Appendix Table 18 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.

owners to renters, as discussed above, we then calculate the indirect externality loss for each block group as the difference in the observed loss and the predicted capitalization loss.

Consistent with our prior findings, Internet Appendix Table 19 shows that the model-implied transfers are strongly positively associated with the income of renters. For a one-standard deviation increase in renters' income, the size of the transfer increases by two percentage points, relative to a mean of six percentage points. These results are consistent with endogenous gentrification in which wealthier neighborhoods have higher growth rates of real estate prices (Guerrieri, Hartley and Hurst, 2013; Couture, Gaubert, Handbury and Hurst, 2021). In contrast, the estimated indirect negative externality component is not statistically related to the income of renters. These results imply that the indirect negative spillovers are not localized, consistent with city-wide changes in crime, educational quality, or other city-wide quality of life traits.

VII. Conclusion

Economists and policymakers have long disagreed about the benefits of rent control. Over 70 years ago, in response to the first generation of rent control in New York City, Grampp (1950, p. 425) writes, "*[The economic principles of rent control] are so obvious that one would feel the greatest reluctance to repeat them on the pages of a professional journal were it not that a great public policy has been erected upon either ignorance or a repudiation of them.*" Today, as a third generation of rent control laws are enacted, the debate continues.

This paper provides a new contribution to this debate by studying the immediate effect of St. Paul's rent control law on market valuations. Market valuations provide a summary statistic that accounts for all future costs and benefits of the new provision in the short and long term, including endogenous responses of owners, renters, and policy makers.

We find that the introduction of rent control in St. Paul in November 2021 caused statistically significant and economically large declines in property values. This result is robust to general trends in market prices, local fixed effects, and property traits.

While the costs the law imposes on owners are substantial, our results show that its benefits are poorly targeted. Though the intention of the law is to benefit lower income renters, we find that transfers to renters are largest in the neighborhoods of the city in which renters have higher incomes, are less likely to be minorities, and have more education.

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 show that this is unlikely to occur in St. Paul. Second, our results suggest future research on the political economy of rent control. 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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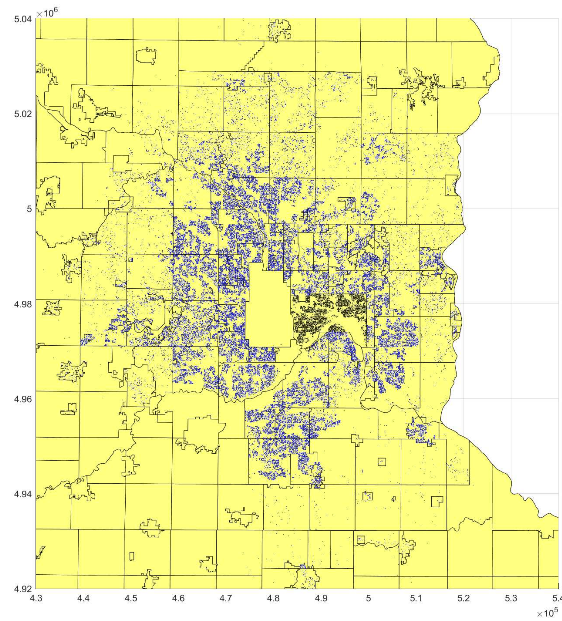


Figure 1. Location of Real Estate Transactions in St. Paul vs. Suburbs
Notes: The location of house sales in St. Paul (black) and surrounding cities (blue) in the Metropolitan area of the Twin Cities (excluding the city of Minneapolis) over the period from January 2018 to July 2022.

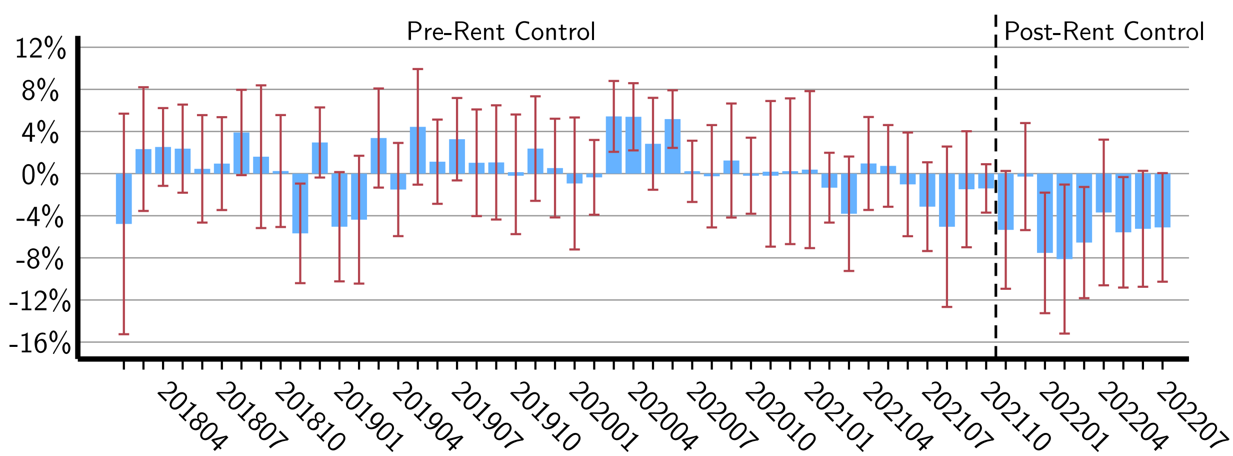


Figure 2. Real Estate Prices in St. Paul vs. Its Suburbs by Month
Notes: This figure presents coefficient estimates and their 95% confidence intervals from the interaction between dummy variables for year-months and a dummy variable for property located in St. Paul, controlling for property size, age, 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.

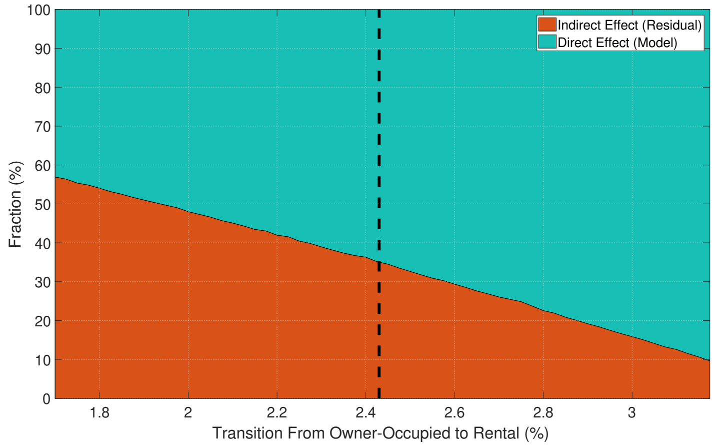


Figure 3. Decomposition of Losses for Owner-Occupied Houses
Notes: This figure presents estimates of the decomposition of value losses for owner-occupied houses into direct capitalization effects and indirect, negative externalities, based on the probability of transitioning from an owner-occupied house into a rental property. The dashed vertical line at 2.43% indicates the probability of transitioning to a rental as computed from the supply effects in San Francisco reported in Diamond et al. (2019). The highest transition probability of 3.18% is the historical average transition probability in St. Paul during the pre-rent control period 2010 to 2020.

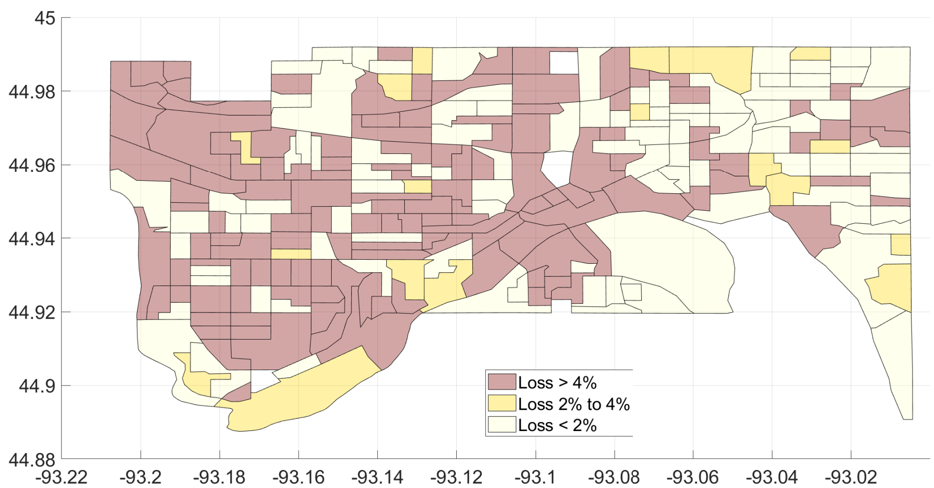


Figure 4. Distribution of Value Losses across St. Paul Census Block Groups.
Notes: This figure presents the average value loss generated by the rent control law at the block group level, estimated using the specification in Equation 5 based on the entire sample from November 2021 to July 2022.

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
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 (3%)
Pasadena, CA	2022	Ballot measure	Pending	Rent control (75% of CPI)

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.

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)
ln(square feet)	0.710	0.719	0.643
	(0.019)	(0.031)	(0.007)
ln(building age)	−0.081	−0.082	−0.090
	(0.005)	(0.005)	(0.003)
ln(units)	0.166	0.160	0.222
	(0.019)	(0.032)	(0.010)
Property type: Multi-family	0.088	0.031	0.150
	(0.035)	(0.102)	(0.020)
Property type: Single-family	0.299	0.255	0.358
	(0.035)	(0.083)	(0.019)
Property type: Townhouse	0.124	0.085	0.168
	(0.031)	(0.073)	(0.018)
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. 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.

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

	Dependent variable: ln(price)			
	(1)	(2)	(3)	(4)
Panel A: Placebo Tests in Comparable Metro Areas				
Downtown × Post	0.035	0.021	0.007	0.027
	(0.014)	(0.010)	(0.015)	(0.005)
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	0.705	0.860	0.791	0.898
Observations	801,054	800,885	800,947	800,636
Panel B: Triple Difference Tests of St. Paul vs. Comparable Metro Areas				
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 over the period January 2018 to July 2022. Panel A only includes observations from 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, December 2021, or January 2022, after rent control is passed in St. Paul. Panel B includes observations from all five comparable Metro Areas and the Twin Cities area, excluding Minneapolis. *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.

Table 5 – 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.

Table 6 – Wealth Redistribution 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	>90.0	464	1,833	1,019	1,275	15	4,679
landlord	47.5–90	174	898	387	409	4	1,895
income	37.5–47.5	22	70	22	10	0	125
($1,000s)	22.5–37.5	15	14	5	4	0	38
	≤ 22.5	2	1	2	0	0	5
Panel B: Wealth transfer from landlords to renters (% of property value)							
	All landlords	2.0	3.4	5.8	7.1	8.0	4.7
	Large landlords	1.7	3.3	5.8	7.2	8.5	4.6
Small	>90.0	2.8	3.6	6.5	7.0	7.7	5.2
landlord	47.5–90	1.5	3.1	4.2	7.2	8.0	4.1
income	37.5–47.5	-0.2	1.9	3.2	9.1		2.5
($1,000s)	22.5–37.5	5.9	3.4	13.4	2.0		5.6
	≤ 22.5	2.4	6.9	0.0			2.3
Panel C: Number of rental units on property							
	All landlords	7.3	4.7	4.0	5.1	8.2	5.2
	Large landlords	15.8	9.1	7.4	12.4	26.3	11.1
Small	>90.0	2.5	2.6	2.5	2.2	1.5	2.4
landlord	47.5–90	2.9	2.2	2.1	1.7	1.3	2.1
income	37.5–47.5	1.6	3.8	1.7	1.3		2.8
($1,000s)	22.5–37.5	1.4	1.8	1.4	1.3		1.5
	≤ 22.5	13.5	1.0	13.0			10.8
Panel D: Race of renters (% white)							
	All landlords	35.5	40.7	54.1	75.0	98.3	51.8
	Large landlords	33.5	38.4	53.8	75.0	100.0	49.7
Small	>90.0	37.4	44.0	56.3	76.1	97.8	55.4
landlord	47.5–90	35.1	38.2	50.6	72.3	100.0	48.3
income	37.5–47.5	29.9	32.9	41.1	62.4		36.5
($1,000s)	22.5–37.5	35.9	31.7	59.6	77.3		41.8
	≤ 22.5	56.2	36.4	16.5			36.4

Notes: This table presents average statistics of parcels with rental units based on owner and renter incomes. 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 reports the average wealth transfer, proxied by the estimated property value loss from November 2021 to July 2022. Panel C reports the number of units on each parcel, on average. Panel D reports the likelihood that the renter is white, based on block group averages.

Table 7 – Owners and Renters' Demographics and the Transfer of Wealth

Dependent variable:	Transfer from Owners to Renters					
Sample:	Large Landlords			Small Landlords		
	(1)	(2)	(3)	(4)	(5)	(6)
Renters ln(income)	0.053			0.055		
	(0.019)			(0.023)		
Renters that are white (%)		0.072			0.079	
		(0.019)			(0.023)	
Renters with bachelors (%)			0.131			0.154
			(0.031)			(0.033)
Owners ln(income)				0.008		
				(0.005)		
Owners that are white (%)					0.027	
					(0.011)	
Owners with bachelors (%)						−0.007
						(0.014)
Rental housing (%)	0.057	0.041	0.023	0.080	0.066	0.047
	(0.045)	(0.040)	(0.032)	(0.047)	(0.039)	(0.028)
Constant	−0.534	−0.010	−0.004	−0.655	−0.048	−0.018
	(0.212)	(0.028)	(0.023)	(0.247)	(0.033)	(0.024)
Adjusted R^2	0.048	0.058	0.118	0.062	0.080	0.166
Observations	3,341	3,366	3,380	6,583	6,998	7,014

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.

Internet Appendix for
"Robbing Peter to Pay Paul?
The Redistribution of Wealth Caused by Rent Control"
For Online Publication

This Internet Appendix contains additional material to support the results presented in the main text. Section I 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 II discusses selection bias concerns. 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 discusses additional tests not reported in the main paper. Section V provides additional figures and tables referenced in the main text and also in the Internet Appendix.

I. Simple Model of Housing Value

In this section, we develop a simple pricing framework, with two aims: 1) to verify whether the magnitudes of the price drops that we observe in the data can be rationalized, and 2) to provide a benchmark for the relative contribution of direct capitalization effects and indirect externalities to the price drops, as in Equation 1 in the paper.

Following a Gordon growth model, the price of a property at time t is:

$$P_{s0,t} = \left(\sum_{n=1}^{N} \frac{E_t\left[Inc_0(1+g_{sn,e})^n\right]}{(1+r)^n} \right) + \frac{E_t\left[Inc_0(1+g_{sN,e})^{N+1}\right]}{(1+r)^N(r-g_{sN,e})}$$

where the first term of the right accounts for the income stream earned by the owner over the following N years, and the second term is a terminal value; Inc_0 is the current income of the property if rented, r is the discount rate, and $g_{sn,e} = g_{sn} + e$ is the stochastic growth rate of income. It has two components: g_{sn}, which is the expected growth rate depending on the state at time $t+n$ (sn), and a mean zero noise component e, perfectly correlated over time. We include the shock e to reflect the fact that, even conditional on the state s, the true growth rate of income is unknown.

At each future time $t+1, ..., t+N$, the state $sn \in \rho, \omega$ is equal to either rented (ρ) or owner-occupied (ω). The expectation $E_t[\cdot]$ is computed based on the probability that, at each future date, the property will be rented or owner-occupied, and on the distribution of the shock e. Transitions between the rented and owner-occupied states are governed by a Markov process with positive probabilities placed on the transition from owner-occupied to renter and vice versa.

While owner-occupied properties do not earn income, they provide implicit income to their owners in the form of housing services. Though it is frequently assumed in the literature that the implicit income offered to the owner is larger than the financial income that can be extracted from rental, due to the unique consumption value of owner-occupied housing (the "warm glow"), for simplicity we assume that the implicit value of owner-occupancy is the same as the value of rents.

Next, we make the stylized assumption that, in the absence of the rent control reform, the growth rate of income and the growth of housing services would be the same, as would be the realizations of the shock e, so that: $g_{\rho n,e} = g_{\omega n,e} = g_e$. Thus, before rent control, the only relevant source of uncertainty on future growth is e, and is perfectly correlated between rentals and owner-occupied.

We use this model to predict the direct capitalization effects of the rent control proposition on both properties that are currently rented and owner-occupied. We model the effects of rent control as follows. Rent control does not affect the growth rate of housing services for owner-occupied housing, so that $g_{\omega n,e} = g_e$. However, rent control does affect the growth rate of income for rental properties. In particular, rent control sets a threshold $\bar{g}$ such that if $g_e > \bar{g}$, then $g_{\rho n,e} = \bar{g}$ and $g_{\rho n,e} < g_{\omega n,e}$. Instead, if $g_e \leq \bar{g}$, then $g_{\rho n,e} = g_{\omega n,e} = g_e$.

Thus, 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 does not include indirect externality effects. It only includes the direct capitalization effects of rent control.

A. Calibration Results

We calibrate the simple model presented in the previous section to the data. First, a crucial aspect in the calibration of our framework is the modeling of transitions between owner-occupied and rentals. To model these transitions, we collect data on the use of residential parcels in St. Paul from administrative data over the years 2010 to 2020. In particular, we use a flag for whether the property claims a homestead tax exemption as a proxy for owner-occupied properties. In Minnesota, owners may only claim this exemption for one property per year. The average annual transition probability from owner-occupied to rental is 3.18%. For transitions from a rental to owner-occupied, the average is 13.25%. Since our transition probability estimates are based on a 10-year period, and 10 years is frequently the horizon used by real estate investors in their cash flow projections to model property prices and internal rates of return, we also choose the horizon of our pricing model to be equal to 10 years ($N = 10$).

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 can then calculate the price drops generated by the rent control provision for different values of the expected growth rate. The "uncontrolled" expected growth rate is the same for rented and owner-occupied properties. The shock e, which captures the fact that the true expected growth rate is unknown by investors, has a truncated normal distribution, with mean 0, an upper bound equal to 8% (expected growth cannot exceed the discount rate), and variance equal to 0.7%, which is equal to the standard error of the average growth rate of rents in Minneapolis and St. Paul in the 10 years preceding the ballot. Rent control is equivalent to setting the rent growth cap $\bar{g}$ equal to 3%.

Internet Appendix Figure 5 shows the price drop generated by rent control for rentals and owner-occupied properties. More precisely, we calculate, for $s0 = \rho$ and $s0 = \omega$, the percentage drop from $Ps0, t$ to $Ps0, t'$, where the former is the uncontrolled market price, and the latter is the price with rent control. On the horizontal axis, we have different values of the uncontrolled annual expected growth rate of income (ranging from 2% to 5%), which in the pre-ballot period is the expected growth rate for both rental-income and owner-occupied housing services, and after the ballot period is the expected growth rate for owner-occupied housing services. The model, in spite of its stylized form, generates a variety of interesting effects.

First, we can see that price drops take place even when the uncontrolled expected growth rate is below 3%. This is because the true growth rate is not known, as captured by the stochastic component e, and rent control constrains the right tail of the distribution of the growth rate. Of course, as the expected growth rate increases, the likelihood that rent control will constrain growth increases, and the price drops become larger.

Second, and most importantly, the model generates price drops of different magnitudes for rental and owner-occupied housing. The drop is larger for rentals, and the gap in price changes between rentals and owner-occupied increases with the expected uncontrolled growth rate. For instance, when the uncontrolled expected growth rate is 3.5%, the model predicts a drop in prices of 4.75% for rentals, and close to 2.5% for owner-occupied. When the expected growth rate is 4.5% the drops are roughly 10% for rentals and 5% for owner-occupied, close to what we observe in the data.

Thus, the model highlights that even in the absence of externalities, capitalization effects alone can generate non-negligible price drops for owner-occupied, while at the same time matching the difference in the responses of rental and owner-occupied housing.

B. Decomposing the Effect on Owner-Occupied Housing

We use the model developed in the previous sections to decompose the observed price drop for owner-occupied properties into direct capitalization and indirect externality components. The model predicts the size of the direct component based on the calibrated parameters. To infer the size of the indirect component, we compare the observed price drop to the model-implied direct effect. Assuming that indirect externalities are equivalent for rentals and owner-occupied properties, the indirect externalities are estimated as the empirically observed value loss minus the model-implied direct value loss.

To discipline the model, we identify the growth rate required to match the difference in the price drop between renter occupied and owner-occupied properties in the empirical results. Since the model-implied spread between the loss of owner-occupied and rental properties is monotonically increasing in the uncontrolled expected growth rate, the value of the spread uniquely identifies a value of the expected growth rate. At this growth rate, we calculate direct and indirect losses. We conservatively set the spread to be 5%. A larger spread would generate even larger capitalization effects for owner-occupied properties.

We first run this exercise keeping transition probabilities fixed at pre-ballot values. However, it is likely that the introduction of rent control will make rental housing in St. Paul less attractive, and thus will endogenously reduce the likelihood that properties transition into the rental market. Thus, we repeat the exercise over a grid of different values for the probability of owner-occupied properties transitioning into rentals. We choose the range from 3.18% (the historical value) to 1.70%. This range is centered around the value of 2.45%, for which the steady-state fraction of rental would experience a relative drop of 20% with respect to the pre-ballot fraction. This is roughly the magnitude of the contraction in rental supply that is measured by Diamond, McQuade, and Qian (2019) in response to rent control expansion in San Francisco.

Figure 3 in the main paper reports the results from this exercise. We can see that for the historical value of the transition probability (3.18%), we can rationalize approximately 90% of the drop in the data as driven by direct capitalization effects. However, as we move to the left, and reduce the likelihood of owner-occupied properties transitioning into the rental market, the fraction attributable to externalities increases. The vertical line in the middle of the figure corresponds to the value of the transition probabilities that would generate long run effects consistent with Diamond, McQuade, and Qian (2019). For this value, roughly two thirds of the effect in the data can be explained by the model as a capitalization effect. Finally, for the lowest value of the probability, equal to 1.7%, only 45% of the effect can be explained by capitalization, and the majority of the price drop for owner-occupied is potentially tied to externalities. This decomposition is similar to what Autor et al. (2014) report for Cambridge, Massachusetts.

In unreported tests, we find that while changes in transition probabilities generate substantial differences in the magnitude of capitalization effects, they generate limited variation in the corresponding expected growth rates, which range from 4.15% (for the 1.7% transition probability) to 4.35% (for the 3.18% transition probability).

Overall, our calculations suggest that the capitalization effects induced by the law can be sizable even for owner-occupied housing. Our simple model can rationalize between 45% and 90% of the price drop for owner-occupied properties as capitalization-driven. While this is a large range, we believe that the mid-point estimate of roughly 67% could be a reasonable benchmark.

C. Direct Effects and Externalities in the Cross-Section

We use the quantitative model of the prior section to identify direct capitalization effects and indirect externality effects at the Census Block Group-level. We start by constructing block group level estimates of expected growth rates. We estimate Census Block Group-level rent-to-price ratios for 209 blocks, using information on sales and rental listings over the period from June 2018 to June 2021. These ratios are larger than cap rates (earnings-to-price ratios) of real estate properties for several reasons. First, they are based on listed rents, which are likely higher than actual rents. Second, landlords may face vacancies. Third, rents are gross of recurring expenses faced by landlords, such as periodic maintenance and property taxes. To convert rent-to-price ratios into cap rates, we first apply a 10%

downward adjustment, which accounts for the discount between listed and actual rents, and for vacancies (annual vacancies are frequently approximated to be 5% of rents, and we assume a 5% spread between listed and actual rents). Then, we assume that expenses account for a third of the remaining gross rent, consistent with the estimates constructed by Demers and Eisfeldt (2022). Given the block group level cap rates, we calculate expected growth rates as the difference between discount rates, which we again set equal to 8%, and cap rates. We find an average expected growth rate across block groups of 4.1%, with a standard deviation across the city of 1%.

Using the estimated expected growth rate for each block group, we obtain block group specific projections of capitalization-driven price drops for both rentals and owner-occupied properties. We calculate the weighted average of direct effects using the fraction of parcels that are rentals and owner-occupied in each block group to construct an average model-implied direct effect at the block group-level. As before, we estimate the indirect effect by subtracting the model-implied direct effect from the observed value loss at the block group level.

Consistent with our conjecture that the law is generating transfers at odds with its aims, Internet Appendix Figure 6 shows that the model-implied direct losses, which proxy for transfers, are strongly positively associated with the log income of tenants and negatively associated with the difference between owner income and tenant income.

In Internet Appendix Table 19, we explore more in depth the relationship between the block group level losses estimated in the data, the model implied direct effects (the transfers), and the income of tenants and owners. We restrict the sample of block groups to the 209 blocks for which we have constructed model-based estimates. For our empirical tests, we then estimate value losses using equation 5, but with the post-reform period ending in February 2022. This is because the model is calibrated to the initial terms of the law, and until February 2022 there was no public process in place to adjust the terms. Moreover, we aggregate value loss estimates at the block group-level, since this is the level at which we can construct model-based estimates. In column 1, we replicate the regressions in Table 7 of the main paper. We find a positive relationship with the log income of renters, with slopes similar to the ones estimated at the parcel level and over the longer sample. In column 2, the dependent variable is the model-implied loss, which we can interpreted as the expected transfer. Consistent with what is shown in Figure 6, this variable is positively correlated with tenant income, and negatively correlated with the income difference.

In column 3, the dependent variable is the difference between the loss estimated in the data, and the loss predicted by the model, for each block group. These difference can be interpreted as the component of the loss that is not explained by transfer effects, and is instead potentially related to negative externalities. Interestingly, while this residual component is positively correlated with tenant income, the coefficient is not statistically significant. Thus, to the extent that the model implied effects do capture transfers in the data, the relationship between losses and tenant income appears to be mainly driven by cross-sectional differences in transfers. The magnitude of the sensitivity of the transfer to income is also non-negligible. The standard deviation of tenant income across block groups is 46%, so that a one standard-deviation difference in income is equal to a 2% difference in transfers, relative to an average of 6%.

II. Selection Bias Tests

A threat to our identification strategy is that the passage of the rent control provision may create selection in the kind of properties transacted in St. Paul after the ballot. In particular, the properties transacted after the passage of the provision may be of lower quality. Then, lower prices after the ballot may not reflect lower valuations, but rather a change in the composition of transacted properties. Note that this is a concern to the extent that the ballot induces changes in characteristics that are unobservable in our data, since our controls already account for key observable features, such as micro-location, size, and age.

We address this concern with a range of empirical strategies. First, we show that there is no change in the composition of sold properties based on observable characteristics. We begin by using the difference-in-difference setup in our main regression, to run a battery of tests in which the dependent variable is set equal to one of the main property characteristic: log square feet size, log number of bedrooms or bathrooms, log age, and dummies equal to one for single family residences, townhouses, and other properties. In all regressions, we include year-month and census block group fixed effects. When the dependent variable is log square feet size, log number of bedrooms or bathrooms, and log age, we also include dummies for the different property types. Our results are reported in Internet Appendix Table 11. We find no statistically significant and very small coefficients when using the entire post-reform sample from November 2021 to July 2022.[1]

[1]When the sample is limited to November 2021 to January 2022, we find a significant coefficient for the interaction term between the St. Paul and the post ballot dummy only when the dependent variable is log size. However, in this case the effect is positive, suggesting that properties transacted after the ballot are larger than before, which would suggest higher, rather than lower prices. Moreover, the magnitude of this effect is relatively small, equal to only 1.6%.

We then directly inspect the entire distributions of characteristics for properties transacted in St. Paul, and how they changed around the ballot. In Internet Appendix Figure 4, we show 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. In Figure 4, we also show the fraction of sales that were single family residences, townhouses, multifamily buildings, and condos. Also these fractions are stable when comparing 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 unobservable controls would shrink the coefficient of interest to zero, if the sensitivity of the coefficient to unobservables is at least X times the one to observables.

When we apply this framework to our data, the short regression only includes year-month fixed effects, a dummy equal to one for sales in St. Paul, and an interaction between the St. Paul dummy and the post ballot dummy. Our estimate of $|\delta|$ is approximately 11. This suggests that, in order to shrink our estimates of the effect of St. Paul ballot to zero, unobservables would need to have an impact on prices which is 11 times the one of observables, which already include micro-location, property size and property age. We interpret this as evidence that our estimates are robust to even large amounts of unobservables bias in the data.

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

A. Textbook Model of Rent Control

In Figure 8 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 ($R_1 > R_0$).

We then introduce a rent control provision that will take effect in period 1. For simplicity, we assume that rent control imposes 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 supply deadweight losses.

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). 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 make this framework more tangible, we assume linear supply and demand curves as follows:

$$P_d = \alpha - \beta Q_d, \tag{IA.1}$$

where Q_d is the quantity of housing services demanded by renters at price P_d per unit of housing services and β represents the slope of the demand curve. Likewise, the supply of housing services is defined by

$$P_s = \delta + \gamma Q_s, \tag{IA.2}$$

where γ represents the slope of the supply curve.

In a free market equilibrium, the market-clearing quantity supplied is $Q^\star = \frac{\alpha-\delta}{\beta+\gamma}$ at the market price of $P^\star = \alpha - \beta Q^\star = \delta + \gamma Q^\star$. The producer surplus is $\frac{1}{2}\gamma(Q^\star)^2$. See panel (a) of Internet Appendix Figure 9 for a graphical representation of this equilibrium.

In this setting, the textbook treatment of rent control would state that rent control caps the price per housing services below the market price. However, if a rent control law does not force landlords to maintain a certain quality, the rent control actually caps the revenue received by the owner, not the price per unit of housing service provided. Thus, if a landlord reduces the quantity supplied of housing services by allowing the property to deteriorate in quality, but still receives the same total rent, the price per service increases. Even with rent control provisions that attempt to require landlords to maintain quality standards, the enforcement of such a requirement is infeasible. When features of the housing wear out and need to be replaced, such as flooring, windows, or appliances, the landlord can replace them with lower quality features. It is unlikely that a rent control law could prevent a landlord from replacing a double-paned window with a single-paned window, or hard-wood floors with carpet.

Instead, Frankena argues that because rent control limits rent revenue, instead of prices, the limit imposed by rent control is represented by a rectangular hyperbola such that the maximum rent payment is $\bar{R} = pq$, as shown in Internet Appendix Figure 9. When rent control is imposed, we assume that the price for housing services is constrained to be ω below the market price, which fixes the hyperbola $\bar{R}$ in the (p, q) space. Frankena argues that after rent control is imposed, the new supply curve will be the backward-bending rent control hyperbola, moving from more quantity at lower prices to less quantity at higher prices per housing service. After a transition period, the new short-run equilibrium will be at $\bar{E}$ in panel (d) of Internet Appendix Figure 9.

In particular, as shown in panel (a), immediately after rent control is imposed, the price per service falls to $(1-\omega)P^{\star}$, but quantity is fixed. This generates a transfer from the landlord to the renter, represented by the green rectangle. At this price, the landlord is oversupplying housing services. Because the quantity did not decrease, there is no deadweight loss. However, over time, as landlords allow their properties to deteriorate (panel b), the quantity of housing services decreases and the price per housing service increases. This has two effects. First, the transfer from the landlord to the renter decreases. Second, there is a deadweight loss borne by renters and landlords caused by the reduction in housing, as illustrated by the yellow triangle. This reflects the lost surplus from the provision of housing services that are now foregone by allowing properties to deteriorate.

As time continues and quality decreases further, the price per services exceeds the market price $P^{\star}$ and there is a transfer from renters to landlords, indicated by the blue rectangle in the figure. This is because renters who previously enjoyed a relatively low price for housing services relative to their willingness to pay, now pay a higher price. Second, as the provision of housing services decreases, the deadweight loss grows. Finally, at point $\bar{E}$, the supply equals demand, and the market attains a new equilibrium. Landlords maintain their properties at the lower level of housing services and renters pay a higher price per service than in the free market equilibrium. In addition, both renters and owners suffer a deadweight loss.

As Frankena argues, this new short-run equilibrium is not sustainable in the long-run if new entrants face the same rent control policy. New entrants are motivated to supply housing because landlords receive abnormal profit in the new short-run equilibrium. As new entrants increase supply, the long-run equilibrium will return to the original equilibrium before rent control was imposed by an increase in the quantity of low-quality supply.

Because the abnormal profit earned by landlords in the short-run is created by the slow deterioration of the quality of properties, it is not easy to predict the dynamics of the transition from the pre-rent control equilibrium to the new short-run equilibrium, and then to the long-run equilibrium. Immediately after rent control is passed, and before the price rises above the free-market price, landlords lose value in every period. After the price rises above the free-market price, landlords gain value because the transfer is larger than the deadweight loss borne by the landlords. Because we observe empirically that the present value of real estate falls after the imposition of rent control, for the theory to hold, the first phase of the transition must have more weight than the second phase. This could happen for at least two reasons: a large discount rate or new entrants keep the price low.

First, if discount rates are high enough, the positive gains received by landlords from a higher price per quality in later periods could be discounted enough that the losses from transfers to renters in the early phase account for the lion's share of the effects of rent control. In simulations using linear supply and demand curves or constant elasticity supply and demand curves, where we attempt to calibrate the model to the data, we find that the discount rate alone would need to be infeasibly large to explain the negative effect on prices in the data.

Second, it is possible that landlords never receive transfers from renters because new low-quality supply increases at the same rate as the depreciation of existing units. It is reasonable to assume that though rent control can be implemented quickly, existing landlords are unlikely to quickly decrease the quality of their properties to capture higher rents. Instead, existing units will naturally deteriorate. However, new, low-quality supply may enter the market to try to capture the gains created by rent control. Depending on the speed of new supply, the introduction of new lower-quality units is likely to occur at such a rate that the price per quality never rises above $P^{\star}$. Therefore, the first phase in the transition, when prices are less than the free-market price, is the most relevant for predicting losses caused by rent control.

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:

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

$$P(t) = \frac{(1-\omega)Q^{\star}P^{\star}}{Q^{\star} - \lambda t} \tag{IA.4}$$

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:

$$Transfer(t) = (P^{\star} - P(t))\, Q(t) \tag{IA.5}$$

$$= \omega P^{\star} Q^{\star} - \lambda P^{\star} t. \tag{IA.6}$$

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

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

$$DWL_s(t) = \frac{1}{2}\,(P^{\star} - P(Q(t))\,(Q^{\star} - Q(t)) \tag{IA.7}$$

$$= \frac{1}{2}\gamma\lambda^2 t^2. \tag{IA.8}$$

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

$$DWL_d(t) = \frac{1}{2}\beta\lambda^2 t^2. \tag{IA.9}$$

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

$$PV(Transfer) = \int_0^{\bar{t}} e^{-rt}\,(\omega P \star Q^{\star} - \lambda P^{\star} t)\, dt, \tag{IA.10}$$

and the present value of the deadweight loss to landlords is

$$PV(DWL_s) = \int_0^{\bar{t}} e^{-rt}\left(\frac{1}{2}\gamma\lambda^2 t^2\right) dt. \tag{IA.11}$$

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 assume $P^\star = 1,375$ and $Q^\star = 6,875$, which match the rent price and number of rental units in small properties in St. Paul.[2] 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, holding all else fixed.

Panel (a) of Internet Appendix Figure 10 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 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.

Though linear supply and demand curves are easy to visualize, their elasticities are not constant. Therefore, to provide an alternative simulation, we compute the same comparative statistics assuming constant elasticity supply and demand curves. In particular, we assume

[2]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.

the following:

$$P_d = \alpha Q^{\frac{1}{\beta}} \tag{IA.12}$$

$$P_s = \delta Q^{\frac{1}{\gamma}} \tag{IA.13}$$

$$Q^\star = \left(\frac{\alpha}{\delta}\right)^{\frac{\beta\gamma}{\beta-\gamma}} \tag{IA.14}$$

$$P^\star = \alpha \left(\frac{\alpha}{\delta}\right)^{\frac{\gamma}{\beta-\gamma}}, \tag{IA.15}$$

where $\beta < 0$ is the elasticity of demand and $\gamma > 0$ is the elasticity of supply. Internet Appendix Figure 11 presents the results using the constant elasticity supply and demand curves, assuming the same parameters.

The constant-elasticity simulations generate results consistent with the linear supply and demand analysis. Transfers dominate deadweight losses; deadweight loss is nearly flat for elasticities above 0.4; and transfer losses, relative to landlord surplus, increase with the elasticity of supply.

In conclusion, whether we use linear supply and demand or constant-elasticity supply and demand curves, 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.

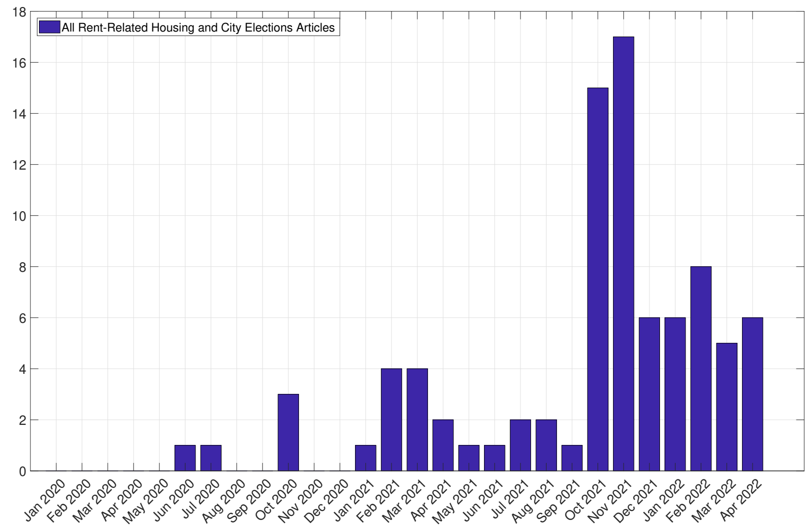
IV. Additional Tests

A. Estimates of the effect of rent control on current rents

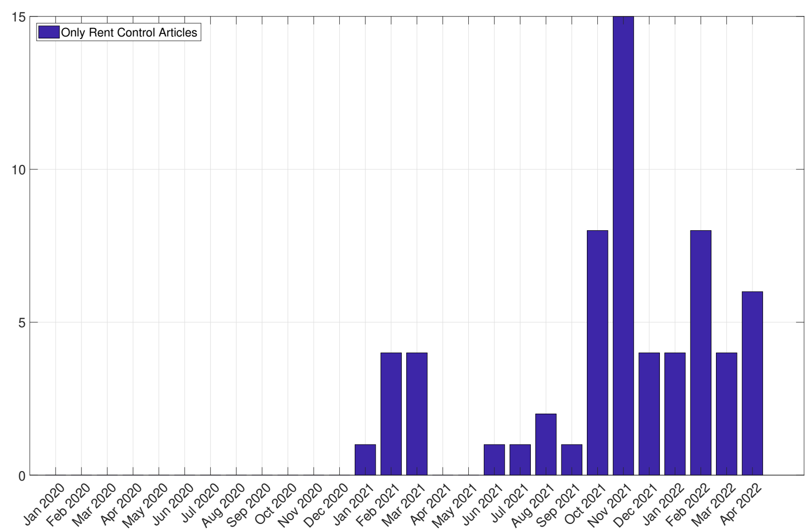
We test whether asked rents changed around the passage of rent control. While there was conflicting information on the date of enactment of the law, in the weeks following the referendum it was announced that the law would be enacted starting from May 2022. Thus, owners may have raised rents after November 2021, to counteract the effects of the cap on future rent growth. However, the extent to which landlords are able to attenuate the effects of the law by increasing rents depends on the competitiveness of the rental market, and on the demand for rental space. Higher rents may translate into higher vacancy, thus not necessarily leading to higher income.

Internet Appendix Table 4 presents estimates of a differences-in-differences model where the dependent variable is logged monthly rent. Controlling for location using city fixed effects, we find that rents are statistically lower in St. Paul in the quarter following the passage of rent control. However, when we use ZIP code or block group level fixed effects, the estimate shrinks and becomes statistically insignificant. Thus, it appears that there is no significant change in rents immediately following the passage of the ballot proposal. However, there is a negative and significant effect across all specification for the quarter from May 2022 to July 2022.

V. Additional Figures and Tables

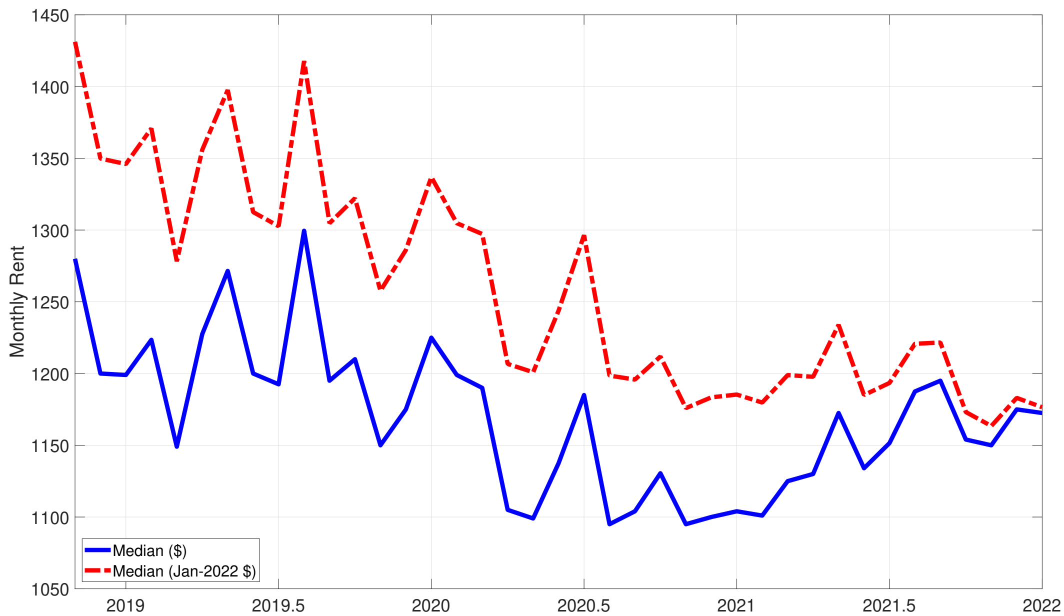


(a) Rent Control Articles

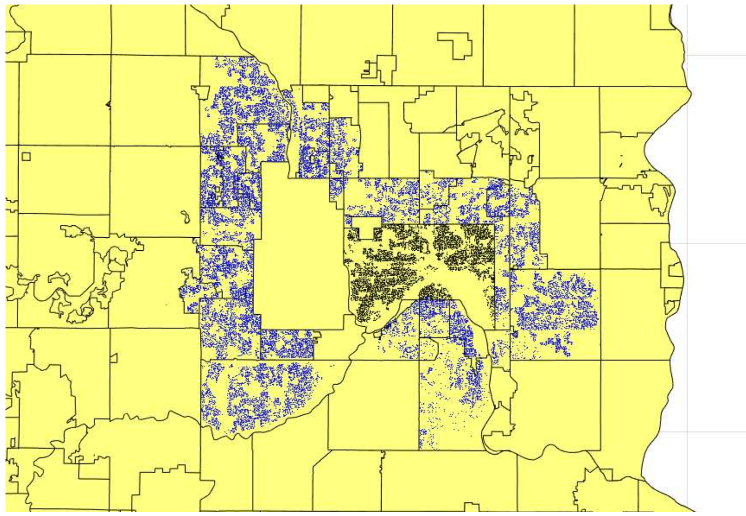


(b) Rent-related and Election News

Internet Appendix Figure 1. 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.

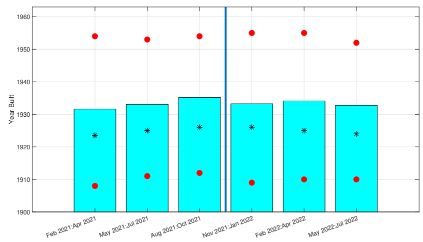


Internet Appendix Figure 2. Recent Time Series of Median Rents in St. Paul
This figure presents the monthly time series of median rents in St. Paul, based on the micro-data available from HousingLink over the period from October 2018 to December 2021. We report nominal and real monthly rents. The latter are expressed in terms of January 2022 dollars, using CPI for all Urban Consumers in the Minneapolis-St. Paul-Bloomington metropolitan area.

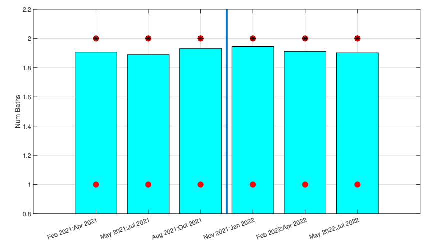


Internet Appendix Figure 3. Location of House Sales in St. Paul vs. Suburbs: Adjacent Cities

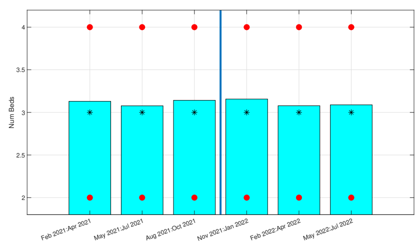
This figure shows the location of house sales in the Redfin sample for St. Paul and its close surrounding cities (Brooklyn Park, Woodbury, Brooklyn Center, Fridley, Bloomington, North Saint Paul, South Saint Paul, New Brighton, Crystal, Hopkins, Edina, Saint Louis Park, Maplewood, Saint Anthony, Roseville, Columbia Heights, Robbinsdale, Inver Grove Heights, New Hope, Golden Valley, Oakdale, West Saint Paul, Little Canada, Richfield, Lauderdale, Lilydale, Newport, Mendota Heights, Sunfish Lake). The data cover the period from January 2018 to July 2022. Sales within the city of St. Paul are highlighted in black, while sales in the surrounding cities are highlighted in blue.



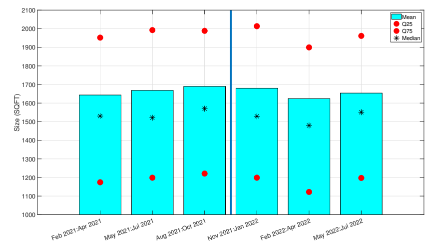
(a) Year Built



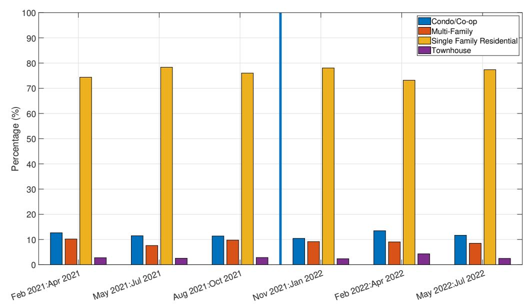
(b) Number of Bathrooms



(c) Number of Bedrooms

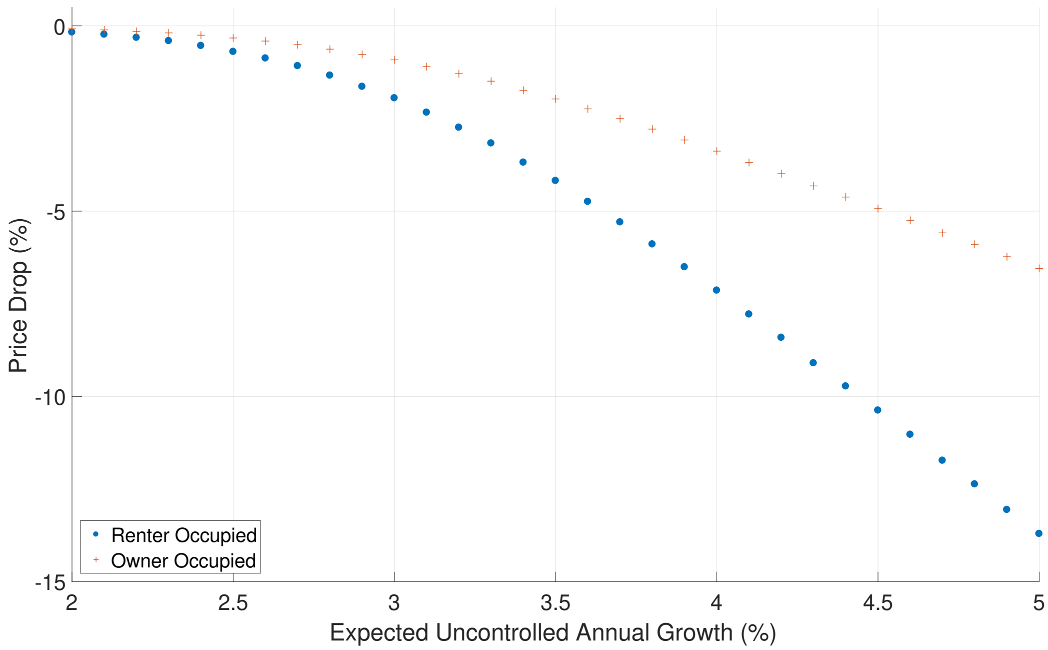


(d) Size



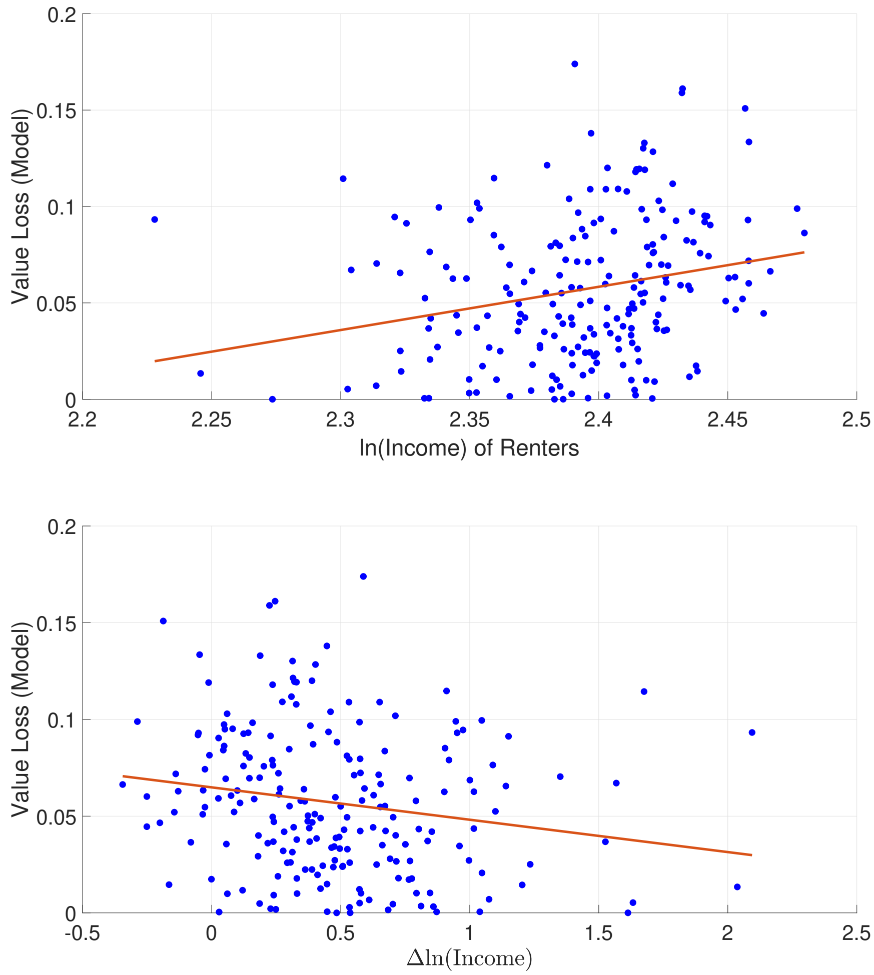
(e) Property Type

Internet Appendix Figure 4. 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.



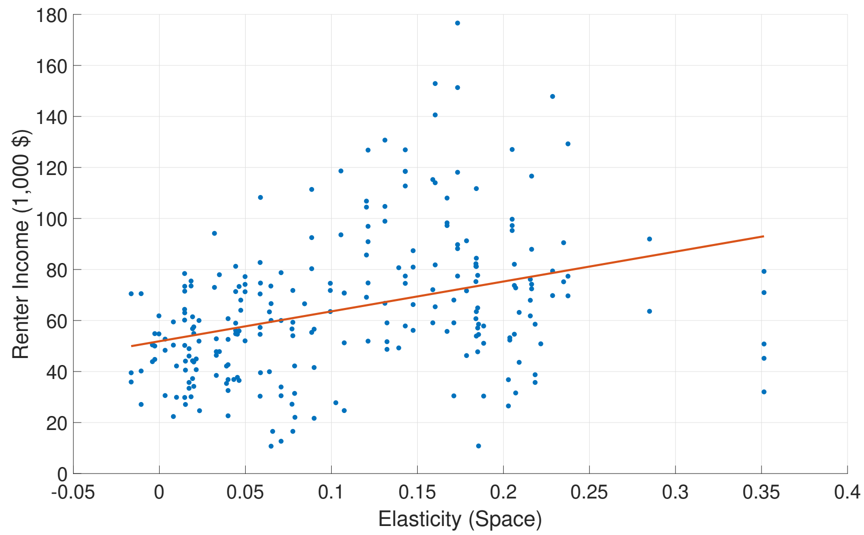
Internet Appendix Figure 5. Model Implied Price Changes of Rental and Owner-occupied Properties

This figure presents the price drops predicted by the calibrated pricing model for rental and owner occupied properties, for different values of expected net rental income growth in the absence of controls.

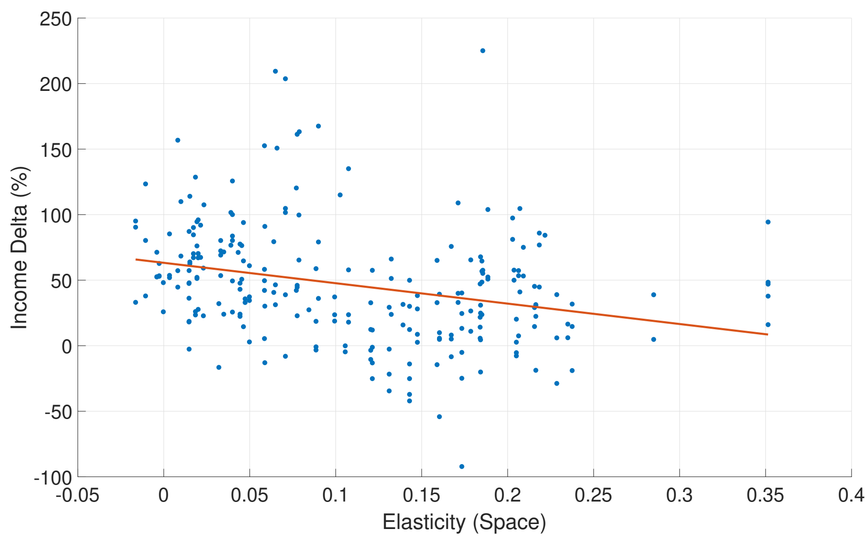


Internet Appendix Figure 6. Model-Implied Value Loss and Block Group Income

This figure presents bloc group level scatter plots, depicting the relationship between the average value loss due to the capitalization effects predicted by the pricing model, and the log income of renters (top panel), or the difference between the log income of owners and the log income of renters (bottom panel).



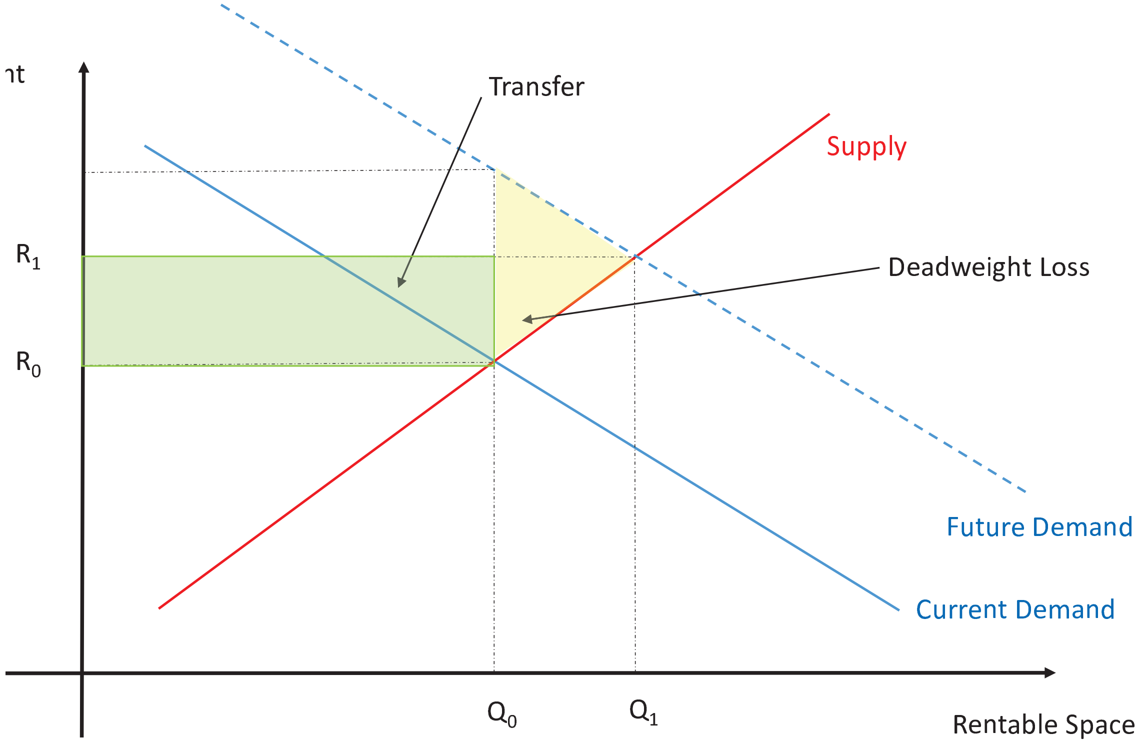
(a) Supply Elasticity and Income of Renters



(b) Supply Elasticity and Income Delta

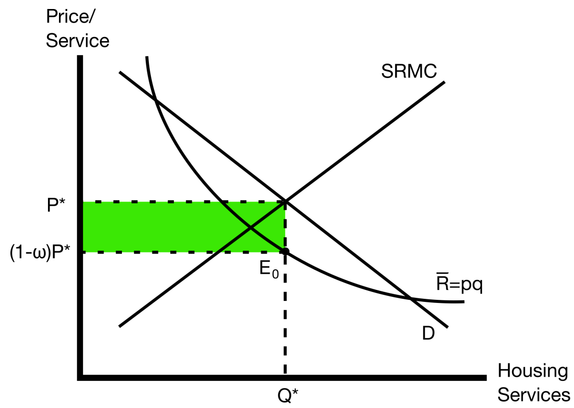
Internet Appendix Figure 7. Supply Elasticity and Income Distributions across St. Paul

This figure shows, across block groups within St. Paul, the association between supply elasticity (measured in terms of floorspace, using the methodology in Han and Baum-Snow, 2021), and, respectively, renters' income (panel a), and the income delta between renters and owners (panel b).

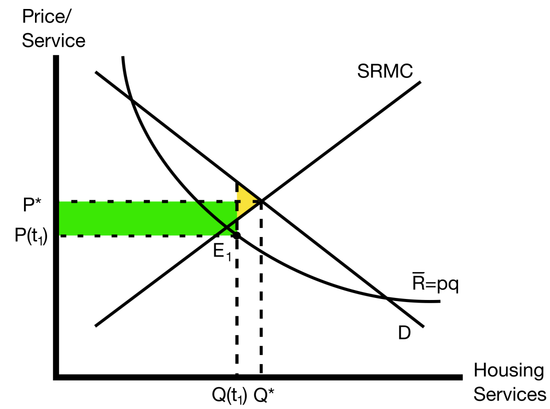


Internet Appendix Figure 8. Graphical Analysis: Supply Elasticity and Rent Control

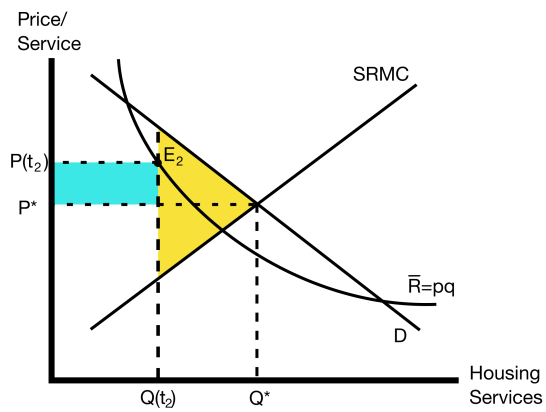
This figure presents the effects of price controls on future rents, transfers and deadweight losses, in a simple textbook framework.



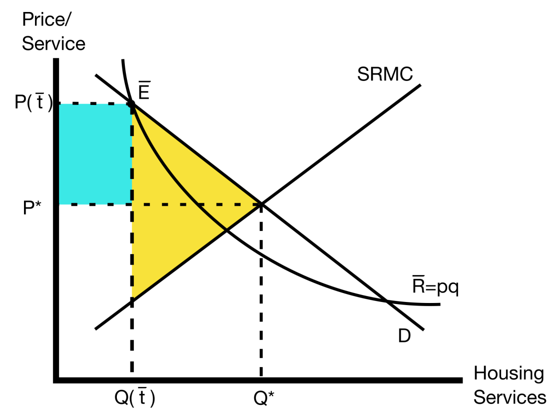
(a) Rent control imposed at $t = 0$



(b) At $t = 1$



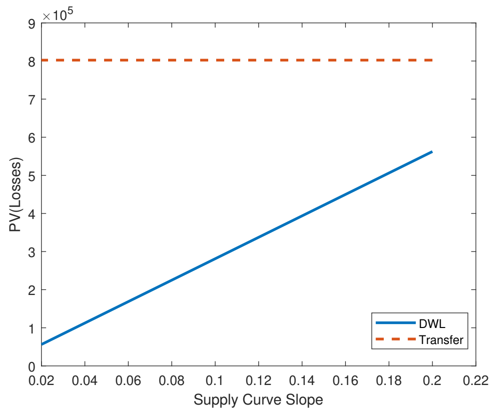
(c) At $t = 2$



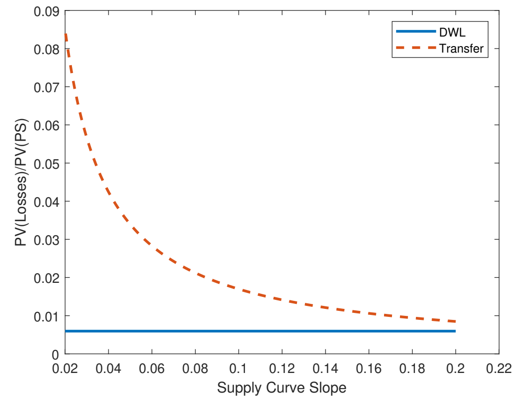
(d) New equilibrium at $t = \bar{t}$

Internet Appendix Figure 9. Deadweight Loss and Transfers Following Rent Control

This figure presents the change over time in price, quality, deadweight loss, and transfer following rent control. The horizontal axis represents housing services, or quality of housing. The vertical axis is the price per housing service. $SRMC$ is the short-run marginal cost curve of providing housing services. D is the demand for housing services. $\bar{R} = pq$ represents the rent controlled maximum rent per housing unit and replaces the $SRMC$ as the supply curve of the landlord. We assume landlords can only reduce housing services over time. In panel (a), rent control lowers the price for housing services from the market price $P^\star$ to $(1-\omega)P^\star$. The green rectangle is the transfer from landlords to tenants. Over time, the landlord will reduce quantity supplied and the price increases. Panel (b) represents the market at $t = 1$, as housing services decline. The yellow triangle is the deadweight loss caused by rent control. Panel (c) represents a continuation in the transition to a new equilibrium. The blue rectangle represents a transfer from renters to landlords because $P(t_2) > P^\star$. Panel (d) represents the steady-state equilibrium at $\bar{E}$ in which the landlord has no incentive to reduce housing services.



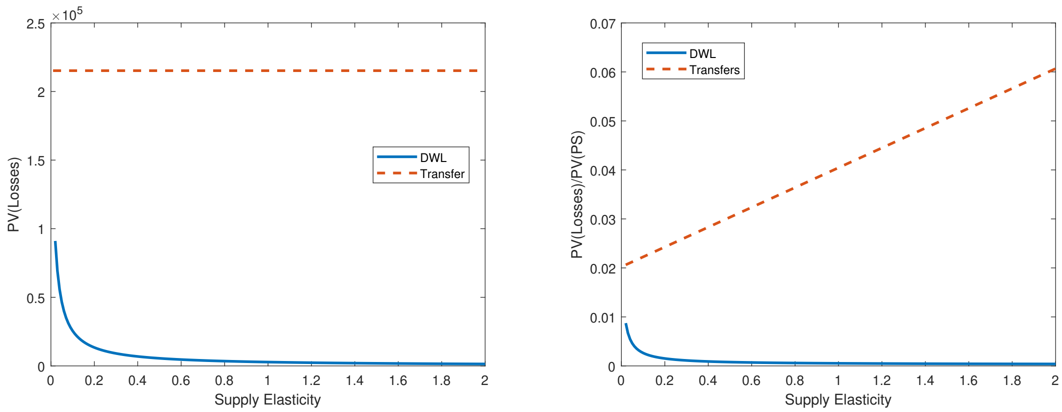
(a) Losses by Supply Curve Elasticity



(b) Losses Normalized by Producer Surplus

Internet Appendix Figure 10. Present Value of Landlord Losses: Linear Supply

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. Parameters are set such that $Q^\star = 6,875$, $P^\star = 1,375$, with $\alpha = 4,125$, $\delta = 0$, $\beta = 0.4$, and $\gamma = 0.2$. Rent control is assumed to reduce the market price by $\omega = 4\%$. The discount rate $r = 5\%$. The quality depreciation per year is set to 3.636% of $Q^\star$.



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

Internet Appendix Figure 11. Present Value of Landlord Losses: Constant Elasticity Supply

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. Parameters are set such that $Q^{\star} = 6,875$, $P^{\star} = 1,375$, with a supply curve of $P_s = \delta Q^{\frac{1}{\gamma}}$, where δ is a scaling factor and γ is the elasticity of supply, which both adjust to maintain the same $Q^{\star}$ and $P^{\star}$. Rent control is assumed to reduce the market price by $\omega = 4\%$. The discount rate $r = 5\%$. The quality depreciation per year is set to 3.636% of $Q^{\star}$.

Internet Appendix Table 1 – 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 2 – Sal*es 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 3 – 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 4 – 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)
Property type: Condo	−0.034	−0.018	0.000
	(0.020)	(0.036)	(0.016)
Property type: Duplex	0.007	−0.043	0.057
	(0.013)	(0.018)	(0.010)
Property type: Single Family Home	0.042	0.024	0.080
	(0.016)	(0.026)	(0.013)
Property type: Townhouse	0.062	0.049	0.074
	(0.016)	(0.017)	(0.014)
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. 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.

Internet Appendix Table 5 – 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.

Internet Appendix Table 6 – 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)
ln(square feet)	0.698	0.747	0.607
	(0.029)	(0.060)	(0.011)
ln(building age)	−0.097	−0.088	−0.104
	(0.009)	(0.016)	(0.007)
ln(units)	0.179	0.143	0.254
	(0.028)	(0.056)	(0.013)
Property type: Multi-family	0.108	−0.007	0.186
	(0.046)	(0.139)	(0.024)
Property type: Single-family	0.338	0.234	0.409
	(0.048)	(0.127)	(0.024)
Property type: Townhouse	0.142	0.056	0.191
	(0.040)	(0.104)	(0.023)
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 3, 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 through 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.

Internet Appendix Table 7 – 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)
ln(square feet)	0.720	0.752	0.612
	(0.033)	(0.033)	(0.009)
ln(building age)	−0.084	−0.073	−0.108
	(0.009)	(0.009)	(0.003)
ln(units)	0.184	0.242	0.305
	(0.044)	(0.090)	(0.016)
Property type: Multi-family	−0.234	−0.155	−0.378
	(0.070)	(0.082)	(0.019)
Property type: Single-family	−0.330	−0.287	−0.261
	(0.036)	(0.079)	(0.011)
Property type: Townhouse	−0.186	−0.130	−0.212
	(0.047)	(0.032)	(0.006)
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. *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 through 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 clustered at the year-month level are presented in parentheses.

Internet Appendix Table 8 – 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)
ln(square feet)	0.771	0.893	0.676
	(0.018)	(0.046)	(0.007)
ln(building age)	−0.068	−0.071	−0.069
	(0.006)	(0.021)	(0.003)
Property type: Multi-family	−0.107	−0.431	0.055
	(0.044)	(0.158)	(0.015)
Property type: Single-family	0.129	−0.085	0.187
	(0.024)	(0.056)	(0.011)
Property type: Townhouse	−0.039	−0.196	0.016
	(0.018)	(0.088)	(0.011)
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 cities of the five comparable metro areas and excluding all transactions in suburban areas of all cities 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 through 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.

Internet Appendix Table 9 – 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)
ln(square feet)	0.720	0.752	0.612
	(0.033)	(0.033)	(0.009)
ln(building age)	−0.084	−0.073	−0.108
	(0.009)	(0.009)	(0.003)
ln(units)	0.184	0.242	0.305
	(0.044)	(0.090)	(0.016)
Property type: Multi-family	−0.234	−0.155	−0.378
	(0.070)	(0.082)	(0.019)
Property type: Single-family	−0.330	−0.287	−0.261
	(0.036)	(0.079)	(0.011)
Property type: Townhouse	−0.186	−0.130	−0.212
	(0.047)	(0.032)	(0.006)
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. *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 through 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 clustered at the year-month level are presented in parentheses.

Internet Appendix Table 10 – 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. Standard errors are either clustered at the location level or are computed using the wild bootstrap method with 999 repetitions.

Internet Appendix Table 11 – 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.

Internet Appendix Table 12 – Triple-Difference Effect of Rent Control on Transaction Prices for Single-Family Rentals

	Dependent variable: ln(price)		
	(1)	(2)	(3)
Rental	−0.063	−0.090	−0.071
	(0.022)	(0.037)	(0.014)
St. Paul × Post	−0.051	−0.037	−0.050
	(0.012)	(0.005)	(0.006)
St. Paul × Rental	0.001	0.241	−0.035
	(0.102)	(0.034)	(0.054)
Post × Rental	−0.060	−0.066	−0.051
	(0.014)	(0.014)	(0.015)
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

Notes: Observations include real estate transactions of single-family residences 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. All regressions include ln(square feet) and ln(age). 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.

Internet Appendix Table 13 – Difference-In-Difference Effect of Rent Control on Transaction Prices for Multi-Unit Housing

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	−0.057	−0.059	−0.048
	(0.020)	(0.022)	(0.025)
ln(square feet)	0.542	0.613	0.497
	(0.034)	(0.067)	(0.019)
ln(building age)	−0.180	−0.160	−0.142
	(0.016)	(0.012)	(0.014)
ln(units)	0.333	0.277	0.343
	(0.032)	(0.065)	(0.018)
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

Notes: Observations include real estate transactions of multi-unit properties 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. 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.

Internet Appendix Table 14 – Difference-In-Difference Effect of Rent Control on Transaction Prices for Apartment Buildings

	Dependent variable: ln(price)		
	(1)	(2)	(3)
	8+ units	12+ units	16+ units
St. Paul × Post	−0.138	−0.209	−0.183
	(0.052)	(0.061)	(0.085)
ln(square feet)	0.278	0.219	0.183
	(0.082)	(0.103)	(0.102)
ln(building age)	−0.191	−0.198	−0.211
	(0.036)	(0.035)	(0.033)
ln(units)	0.731	0.805	0.868
	(0.084)	(0.108)	(0.116)
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 of apartment buildings from the Twin Cities Metro Area, excluding Minneapolis, over the period January 2018 to July 2022. The sample in column 1 (2, 3) includes properties with 8 (12, 16) or more units. *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. Standard errors double-clustered at the year-month and location level are presented in parentheses.

Internet Appendix Table 15 – 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	1.597	1.786	1.702	1.308				
	(3.99)	(3.57)	(2.20)	(6.06)				
New Units Elast					1.763	2.057	1.773	1.514
					(3.94)	(3.78)	(1.98)	(6.28)
Constant	5.880	5.309	2.340	0.750	5.954	5.398	2.412	0.815
	(181.56)	(125.66)	(41.32)	(38.80)	(134.77)	(96.47)	(30.18)	(31.38)
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.

Internet Appendix Table 16 – 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)
Housing that is rental (%)	0.062	0.061	0.066	0.082	0.080	0.089
	(0.042)	(0.042)	(0.045)	(0.039)	(0.039)	(0.041)
ln(Sales Volume) 2018Q1:2021Q3	0.048	0.048	0.044	0.057	0.056	0.053
	(0.027)	(0.027)	(0.029)	(0.029)	(0.029)	(0.029)
ln(Num Parcels)	−0.031	−0.030	−0.029	−0.025	−0.024	−0.022
	(0.034)	(0.034)	(0.037)	(0.028)	(0.028)	(0.032)
Constant	0.013	0.003	−0.005	−0.054	−0.070	−0.088
	(0.148)	(0.147)	(0.150)	(0.128)	(0.128)	(0.140)
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). *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.

Internet Appendix Table 17 – Owners and Renters' Demographics and the Transfer of Wealth: Block Group Level

Dependent variable:	Transfer from Owners to Renters					
Sample:	Large Landlords			Small Landlords		
	(1)	(2)	(3)	(4)	(5)	(6)
Renters ln(income)	0.033			0.031		
	(0.015)			(0.016)		
Renters that are white (%)		0.052			0.018	
		(0.018)			(0.022)	
Renters with bachelors (%)			0.109			0.103
			(0.022)			(0.031)
Owners ln(income)				0.038		
				(0.038)		
Owners that are white (%)					0.342	
					(0.120)	
Owners with bachelors (%)						0.028
						(0.074)
Rental housing (%)	0.042	0.032	0.026	0.053	0.053	0.035
	(0.027)	(0.023)	(0.021)	(0.027)	(0.023)	(0.022)
Constant	−0.316	0.004	−0.001	−0.743	−0.282	−0.019
	(0.169)	(0.017)	(0.013)	(0.433)	(0.100)	(0.039)
Adjusted R^2	0.015	0.024	0.079	0.018	0.063	0.081
Observations	245	244	248	246	245	249

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.

Internet Appendix Table 18 – Robustness: Renter's Income Results

Dependent variable:	Value loss					
	(1)	(2)	(3)	(4)	(5)	(6)
Renters ln(income)	0.047			0.042		
	(0.019)			(0.019)		
Renters that are white (%)		0.058			0.059	
		(0.019)			(0.021)	
Renters with bachelors (%)			0.131			0.137
			(0.035)			(0.031)
Owners ln(income)				0.004		
				(0.005)		
Owners that are white (%)					0.022	
					(0.011)	
Owners with bachelors (%)						−0.007
						(0.010)
Housing that is rental (%)	0.076	0.064	0.033	0.094	0.090	0.060
	(0.042)	(0.038)	(0.030)	(0.042)	(0.037)	(0.027)
New Units Elasticity	0.188	0.142	0.022	0.310	0.225	0.122
	(0.087)	(0.092)	(0.102)	(0.082)	(0.108)	(0.106)
ln(Sales Volume) 2018Q1:2021Q3	0.037	0.044	0.045	0.046	0.048	0.048
	(0.025)	(0.026)	(0.023)	(0.028)	(0.027)	(0.024)
ln(Num Parcels)	−0.034	−0.033	−0.046	−0.030	−0.020	−0.033
	(0.034)	(0.033)	(0.038)	(0.029)	(0.029)	(0.031)
Constant	−0.425	0.008	0.075	−0.473	−0.108	−0.013
	(0.240)	(0.132)	(0.144)	(0.242)	(0.121)	(0.119)
Adjusted R^2	0.073	0.077	0.136	0.111	0.123	0.188
Observations	3,314	3,339	3,353	6,579	6,994	7,010

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.

Internet Appendix Table 19 – Renters' Income and the Transfer of Wealth

Dependent variable:	Loss	Model Loss	Residual
	(1)	(2)	(3)
ln(income) of renters	0.074	0.043	0.031
	(0.028)	(0.009)	(0.028)
Housing that is Rental (%)	0.085	0.062	0.023
	(0.053)	(0.017)	(0.053)
Constant	−0.804	−0.442	−0.362
	(0.320)	(0.109)	(0.320)
R-Square adj	0.018	0.126	−0.004
N	209	209	209

Notes: Observations are at the block group level in St. Paul. *Loss* is the estimated loss in the average parcel in a block group caused by rent control, based on data over the period from November 2018 to February 2022. *Model Loss* is the loss predicted by the pricing model, and *Residual* is the difference between the two. Standard errors adjusted for heteroskedasticity are presented in parentheses.