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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 by 6-7%, for an aggregate loss of $1.6 billion. A calibrated model of house prices under rent control attributes a third of these losses to indirect, negative externalities. Second, leveraging administrative parcel-level data, we find that the tenants who gained the most from rent control had higher incomes and were more likely to be white, while the owners who lost the most had lower incomes and were more likely to be minorities. For properties with high-income owners and low-income tenants, the transfer of wealth was close to zero. Thus, to the extent that rent control is intended to transfer wealth from high-income to low-income households, the realized impact of the law was the opposite of its intention.

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A data appendix is available at http://www.nber.org/data-appendix/w30083

I. Introduction

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 I 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 has simple, though extreme, provisions: annual rental growth is capped at 3% year-over-year, with no inflation-adjustment and no provision to allow rental prices to be reset to market prices upon vacancy, and all residential properties are covered by the law, with very few exceptions. 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.

Using a sample of nearly 150,000 real estate transactions over the period January 2018 to January 2022 in the five counties surrounding St. Paul, we first estimate the effect of St. Paul's rent control on property values. These difference-in-difference tests identify the change in transaction prices of residential real estate in St. Paul following the passage of rent control, relative to the change in prices during the same period in cities adjacent to St. Paul. The tests control for i) year-month fixed effects to absorb common time-series variation in prices in the St. Paul area, ii) detailed location fixed effects to absorb time-invariant cross-sectional variation in prices across granular geographic regions, and iii) property-level attributes, including building age and size, and whether the property is a multi-unit or single-family residence.

We find that the introduction of rent control caused an economically and statistically significant decline of 6–7% in the value of real estate in St. Paul. Because we control for year-month fixed effects, these results do not reflect seasonal changes caused by declining volume in winter months. Second, we run additional tests to account for changing preferences for suburbs over city centers using transactions from five Midwestern cities comparable to St. Paul. In triple-differences models that exploit variation in city centers versus suburbs, we still find that rent control caused a 6–8% decline in property values. Third, we find that rental properties in St. Paul experienced an additional 6% decline in value compared to owner-occupied properties in St. Paul, for a total loss of about 12%. These results imply that the value loss is caused by rent control rather than a spurious variable that affected rentals and non-rentals equally. Finally, we verify that our results are unlikely to be caused by selection bias.

To decompose the observed value loss into direct capitalization effects and indirect negative externalities, we derive 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.

The large decline in property values caused by rent control has significant consequences for St. Paul's economy. Assuming that the market transactions we observe represent the average residential property in St. Paul, rent control would have caused an aggregated loss of $1.57 billion in property value and a 4% expected shortfall in property tax revenue. Given that property taxes are the main form of revenue for the city and the school district, the shortfall in tax revenue is likely to lead to tax increases to maintain city services.

Next, we investigate our second research question: how does rent control redistribute wealth? The intention of St. Paul's rent control is to reduce the burden of housing costs for low-income renters. To study whether the law achieved its intended goal, we test whether the wealth transfers caused by the law are larger when owners have higher incomes and renters have lower incomes.

To test this hypothesis, we first 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. We show this relationship in a simple textbook model of rent control as well as a model of rent control with heterogeneous quality. These findings allow us to use variation in property value losses to proxy for variation in the size of transfers from owners to renters.

Next, we use a hedonic pricing model to predict the change in property value following rent control for over 60,000 residential parcels in St. Paul. Due to limitations in the administrative data, we focus on properties with three or fewer units owned by small landlords. This accounts for 90% of rental properties and 54% of all rental housing units in St. Paul. The large majority of residential parcels in St. Paul are single-family residences (89% of all parcels), of which 17% are rental properties. Of the rental properties in our sample, 43% are owned by small landlords.

To measure the traits of renters and owners, we use highly granular Census data. We proxy for the traits of renters based on Census data corresponding to the property address. To proxy for the traits of owners, we collect their addresses from the county assessor's office. To verify that

the address of the owner is residential, rather than commercial, we match owners' addresses to 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. For small landlords, we proxy for their demographic traits using the granular Census data that correspond with their home address.

To test whether transfers are larger when renters have lower incomes and owners have higher incomes, we create a 'high disparity' subsample of properties in which owners have incomes above the median-owner's income and renters have incomes below the median-renter's income. We also create a 'low disparity' subsample in which owners have below-median income and renters have above-median income. The differences between the two subsamples are stark. In the high disparity subsample, the median owner's income is more than double the income of the median renter, while in the low disparity subsample, owners' and renters' median incomes are statistically equivalent. Likewise, the fraction of minority renters is roughly 50% in the high disparity subsample, compared to 25% in the low disparity subsample. Similar variation exists for age and education.

In contrast to the stated goals of the rent control law, we find that the largest transfer of wealth occurred in the low disparity subsample (8.52%) in which renters are relatively wealthier, while the smallest transfer occurred in the high disparity subsample (0.89%), in which renters are relatively poorer. This pattern persists in cross-sectional regressions. Wealth transfers are positively related to renters' income and negatively related to owner's income.

We consider possible explanations for the poor targeting of rent control. If properties in neighborhoods with lower-income renters also have lower expected growth in future rents, then rent control would impose a smaller constraint, and hence a smaller transfer loss. Using our simple pricing model to help isolate transfers from negative externalities, we find evidence consistent with this hypothesis. In contrast, we find that negative externalities do not vary systematically with renters' backgrounds, suggesting that the externalities affect city-wide amenities, such as school quality or infrastructure. An alternative, untested explanation is that owners with low-income renters are more likely to be able to evade the law then owners with high-income renters.

The central contribution of this paper is to provide new evidence that rent control substantially reduces property values and that the transfer of wealth caused by rent control is poorly targeted.

Though there is a host of evidence on rent levels, housing supply, search costs, property maintenance, and tenant mobility (see Jenkins (2009) for a review of the literature), there are relatively few well-identified studies on the effect of rent control on property values. A notable exception is Autor, Palmer, and Pathak (2014), which finds a 22% increase in assessed values over a ten-year period and a 7% increase in yearly transaction prices following the end of rent control in Cambridge, Massachusetts in 1994. Other research that studies property values include Marks (1984) and Mense and Kholodilin (2019).

Our second set of results on the wealth transfers caused by rent control provides a novel contribution to existing research on the beneficiaries of rent control. Gyourko and Linneman (1989) show that rent control in New York City in 1968 was poorly targeted because low income tenants did not receive more benefits than high income tenants. Sims (2007) shows similar results for Cambridge, Massachusetts. Our results confirm that not only does St. Paul's rent control generate bigger benefits to higher income renters, but also provide new evidence that rent control imposes a larger burden on lower income owners. This finding adds to an older literature focused on New York City, including Olsen (1972) and Ault and Saba (1990) which find that the costs to landlords was substantially larger than the benefits to tenants. More recent, Favilukis, Mabille, and Van Nieuwerburgh (2021) show theoretically that the advantages of housing policies depend on successfully targeting the benefits to the neediest households.

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, Palmer, and Pathak, 2014), Vancouver (Marks, 1984), Toronto (Fallis and Smith, 1985), Los Angeles (Murray and Neels, 1991), and San Francisco (Diamond, McQuade, and Qian, 2019). As housing markets and political policies have become more integrated over time (Floetotto, Kirker, and Stroebel, 2016), and there is a growing debate on housing affordability (Ghent and Leather, 2021), we believe that studying a new rent control mandate, in a relatively large city, located in an area with no history of rent control, may provide important evidence for understanding the future of rent control.

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

II.A. Historical Context of Rent Control

Rent control laws in the United States have historically been implemented in a small number of states, most notably New York, New Jersey, and California. 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 relocation and economic uncertainty (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. Arnott (1995) argues that the relative flexibility of the second-generation laws allowed for the possibility that rent control laws could improve welfare in inefficient housing markets.

Following the second wave, there was a regulatory backlash to rent control laws and many states passed 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), Georgia (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 I, many states are revisiting their laws that preempt rent control or have enacted state-level rent control. Cities are also 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 in response to a proposed rent cap in Minneapolis, 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.[1]

II.B. St. Paul's Rent Control Ordinance

St. Paul's rent control ordinance is unique in its stringency. First, unlike most rent control laws which include vacancy decontrol provisions, rent increases in St. Paul are limited to 3% regardless of whether a property becomes vacant and is re-rented to new tenants. This means that there is no mechanism for rents to be adjusted to market prices. Second, unlike most rent controls that exempt new construction to encourage increases in supply, there is no exemption for new construction in St. Paul. All residential rental property is under the jurisdiction of the law. Similarly, there are no exemptions for small landlords or for properties with few units; no provisions for owner-occupants; and no provision for inflation adjustments, as are common in other rent control laws. This means that rent increases in St. Paul could be capped below inflation rates for an indefinite number of years. Thus, because St. Paul's rent control is possibly the strictest rent law in the country, it offers the chance to provide new evidence beyond what has been learned from the relatively moderate, second-generation rent control laws.

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. Historically, Minneapolis and St. Paul tend to enact similar laws (e.g., minimum wages, COVID masking policies, and paid employee leave), so we might imagine that, if Minneapolis were to adopt rent control in the future, it would be similar to the policy in St. Paul. However, the mayor of Minneapolis, who was re-elected in November, has been a vocal opponent of rent control. Thus, the future of rent control in Minneapolis is unclear. For these reasons, we focus this paper on St. Paul's rent control law.

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

It is important to note that 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, we find 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 (see Figure I).

As of the time of writing, St. Paul's final rent control policy is uncertain. In February 2022, the city formed a community working group to help decide how to implement the law, which would go into effect in May 2022. The Department of Safety and Inspections also solicited comments from the public in April on its proposed rules. On April 29, 2022, the city issued a set of rules that substantially weakened the terms of the law as passed 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. The legal uncertainty continues as the Mayor of St. Paul is pursuing an exemption for new construction and the Minnesota Senate approved a bill that would retroactively ban rent control, even if passed in a ballot measure.

For the purposes of our study, if investors anticipated the weakening of the law, the impact of the law would be smaller. Thus, our results reflect the net effect of the law, given the anticipation of its weakening.

III. 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:

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

The direct effect of rent control on 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. We use the term capitalization to denote the direct effect because it reflects the future loss of income that is capitalized into current prices.

In contrast to the direct effect, the indirect effect of rent control on existing property values is caused by negative externalities in the city. Numerous studies report that lower valued properties cause negative spillover effects on other properties (Rossi-Hansberg, Sarte, and Owens III, 2010; Autor, Palmer, and Pathak, 2014). These effects could be driven by changes in such attributes as crime or school quality (Autor, Palmer, and Pathak, 2019; Cellini, Ferreira, and Rothstein, 2010). Because these externalities make the property less desirable, both for renters and owner-occupants, they represent a deadweight loss without any transfers.[2]

[2]Rent control also creates deadweight losses by reducing the incentive to supply new housing. In this paper, we focus on value changes of existing properties and do not study the effects on new supply.

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.

III.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. To the degree that market prices are not forward-looking, our results will be biased towards zero.[3]

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,

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

As illustrated in this equation, the value of rental housing is affected through three channels: the level of current rents, the growth rate of future rents, and the size of the discount rate. Thus, rent control can affect prices through any of these three variables.

Most directly, rent control restricts the growth rate of future rents. If rent control is strictly enforced in St. Paul, the growth rate will be capped at 3% per year. However, landlords and tenants 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, as suggested in Breidenbach, Eilers, and Fries (2022). The growth rate of net rents may also be

[3]While there is evidence of price inefficiencies and behavioral biases in real estate, it is reasonable to expect that landlords will look at their properties as income-producing investment assets, and will price them as the present value of a stream of future cash flows (Clayton, 1996).

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, as mentioned above. 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. In results reported in the Internet Appendix, we find no significant increase in rents immediately following the passage of rent control.

Third, rent control could change the discount rate of local real estate by increasing the risk that the city will pass even stricter 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.

IV. Identification Strategy

The first step of our analysis is to identify the causal relationship between rent control and property values. In an ideal experiment, we would randomly assign some properties to be rent controlled (treated) and others to be non-rent controlled (control). With perfect random assignment, the average pre-treatment property values would be identical between the treated and control groups,

and any subsequent differences in the market value of the two groups could be attributed to rent control. The passage of rent control in St. Paul presents a setting that has similarities to the ideal experiment, with some important deviations.

IV.A. Cross-Sectional Variation

First, rent control is not randomly assigned to a sample of properties. Instead, rent control is assigned to all properties in St. Paul, without exception. In contrast to studies of San Francisco (Diamond, McQuade, and Qian, 2019) and Cambridge, Massachusetts (Autor, Palmer, and Pathak, 2014), in which two properties on the same block could have different exposure to rent control based on building traits or ownership status, there are no control group observations within the city of St. Paul.

Instead, our control sample is restricted to properties located in cities in the five counties surrounding St. Paul. This has both advantages and disadvantages for our identification. The advantage 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, we need to worry less that owners will take actions to remove their properties from rent control, which could bias our treatment sample.

The disadvantage in 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 any 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 of suburbs are not comparable to properties in St. Paul, we identify whether a property is a rental or owner-occupied. As our conceptual model shows, we expect that rental properties are likely to 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 assigned to 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.

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, Palmer, and Pathak, 2014; Campbell, Giglio, and Pathak, 2011; Anenberg and Kung, 2014). If all St. Paul properties are required to charge below-market rents, in the short-run, competitive market forces will also drive down the rents for properties located near the border of St. Paul. This spillover effect will bias the effects of rent control on property values towards zero.

A final threat to our identification is that the difference in real estate prices in St. Paul compared to the surrounding control cities may reflect changes in 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. To provide variation across multiple city centers, we collect additional data on real estate transactions in metro areas comparable to the Twin Cities: St. Louis, Kansas City, Indianapolis, Nashville, and Denver. Each of these areas has roughly the same population size as the Twin Cities area and is geographically proximate.

IV.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 both anticipation of the law, one-time confounding events in control cities, and general time trends.

First, it appears that there was relatively little 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 the Internet Appendix (Figure I), 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 is unlikely to have influenced 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] Second, 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.

Second, we need to control for any one-time confounding events in control cities. Most notably, Minneapolis would be a natural control for St. Paul. However, it has important confounding events at the same time as St. Paul's ballot measure. In addition to the ballot measures on rent control, Minneapolis's ballot also included referenda on mayoral power and policing. These confounding

[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/

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, we use real estate in cities adjacent to Minneapolis and St. Paul as control cities, excluding Minneapolis. In the control cities, there were no ballot measures and only routine school board elections. Moreover, because the city boundaries of St. Paul are not driven by geographic boundaries that could influence property values, the real estate markets are contiguous and integrated across St. Paul's city limits.

Finally, to control for macroeconomic variation in the time-series, we include year-month fixed effects for each month from January 2018 to January 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.

IV.C. Econometric Specification

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 in November 2021, through January 2022; X_i is a vector of characteristics including the log of the building age, the log size of the building in square feet, 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. Because the main effect of $StPaul_i$ and $Post_t$ are spanned by the geographic fixed effects and the year-month fixed effects, they are omitted.

The β coefficient on the interaction term in Equation 3 reflects the difference-in-difference estimate of the effect of rent control on the value of real estate in St. Paul. Because the dependent variable is logged, β reflects a percentage change in property prices within St. Paul, relative to the change in property values in the control cities.

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

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

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. (As discussed previously, we omit Minneapolis from our sample.) For the control cities, the city center (downtown) is the main city area as defined by Census. The geographic and year-month fixed effects in Equation 4 absorb all of the main effects and the cross-sectional interaction terms.

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. If $\beta < 0$, then the price change in the post period in St. Paul relative to the price change in the suburbs of St. Paul is more negative than the same change in prices between the downtown areas of the control cities and their suburbs. Thus, this estimate controls for an overall change in the prices of real estate in large Midwestern city centers relative to their suburbs that may have occurred at the same time that rent control was passed in St. Paul.

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

V.A. Data

We construct a comprehensive micro-dataset of real estate prices and rents, covering house sales and rental listings for the metropolitan area of Minneapolis-St. Paul, consisting of the counties of Anoka, Dakota, Hennepin, Ramsey, and Washington. The counties included in the five comparable metro areas (St. Louis, Kansas City, Indianapolis, Nashville, and Denver) are reported in the

Internet Appendix.[5] Data on house sales and listings cover the period from January 2018 to January 2022, and are downloaded from Redfin. Data on rental listings for the period from October 2018 to December 2021 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.

For all house sales and rental properties, Redfin provides information on 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. Our final sample include 149,480 transactions in the Twin Cities and 680,193 transactions in the comparable metro areas.

Figure II 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 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 II reports sample statistics for the period January 2018 to October 2021. Panel A shows that the average transaction price in St. Paul over the entire pre-rent control period is $282,112 and the median is $242,400. This represents a price per square foot of $174 (average) and $134 (median). The most common type of properties in St. Paul are single-family detached houses (79% of sample), followed by condos (10%), multi-family properties (8%), and townhouses (3%). 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, transaction prices are higher in the suburbs of St. Paul, though the price per square foot is lower and the properties are larger. As expected, there are fewer multi-family properties, condos, and rental properties in the suburbs than in St. Paul. The properties in the suburbs also have considerably newer construction.

[5]Internet Appendix Table I lists the number of transactions for each control city in the St. Paul area and Internet Appendix Table II lists the number of transactions for each county in the comparable metro areas.

Panel C provides summary statistics for the five comparable metro areas. On average, real estate prices are slightly higher in the comparable cities, with a higher variance in prices. However, overall the comparable cities have similar prices per square foot, distribution of building types, and age as in the Twin Cities area.

V.B. Estimates of the effect of rent control on transaction values

Panel A of Table III provides univariate tests of the difference in price per square foot in St. Paul compared to its suburbs for transactions before and after rent control was passed. The average price per square foot in St. Paul before rent control was \$174.18, significantly higher than the price per square foot in the suburbs of \$164.50. After rent control was passed, the average price per square foot in St. Paul increased by \$10.84 to \$185.02. Over the same time period, the price per square foot in the suburbs increased by \$23.01. The difference-in-difference of the price increase following rent control in St. Paul compared to the price increase in the suburbs over the same period is −\$12.17, a statistically significant difference. This represents a decline of 7% from the average price in St. Paul before rent control was passed.

Panel B presents the same analysis using transactions from the five comparable metro areas. As in the Twin Cities, prices increased after November 2021 for both the downtown and suburban areas. However, in the comparable cities, prices increased faster in the downtown area than in the suburban areas. The difference-in-difference is \$3.48, or an increase of 2% relative to the prices in the period before November, 2021.

Panel C presents the difference-in-differences between the Twin Cities area and the comparable metro areas. First, the increase in the average price following rent control is statistically smaller in St. Paul compared to the downtowns of the comparable metro areas. As well, the increase in prices in the suburbs of St. Paul is statistically smaller compared to the suburbs of the other metro areas. The triple-difference across time, downtown vs. suburb, and the Twin Cities versus the comparable cities is a statistically significant decrease of \$15.54 per square foot, or 9% of St. Paul's average pre-rent control price.

These results present initial evidence that rent control caused an economically meaningful and statistically significant decline in the price of real estate in St. Paul. We next run multivariate regressions that control for potentially confounding factors.

Table IV presents estimates of Equation 3 using data from the Twin Cities area that control for building size, age, type (multi-family, single-family, etc.), year-month fixed effects, and geographic location fixed effects.[6] Throughout the paper, standard errors are double-clustered by year-month and by the geographic level of the fixed effects.

Across the three specifications, we find that rent control caused a statistically significant decline in transaction prices of about 6%. This estimate does not vary across the three types of geographic fixed effects. Additionally, the magnitude of the results is similar to the univariate estimates. This suggests that the negative effect of rent control is not confounded by location or the traits of the properties. In Internet Appendix Table III, we find similar results when the control sample is restricted to the cities neighboring St. Paul, with prices falling between 4.5% and 5% in St. Paul following the reform. The slightly muted response in the closer cities is consistent with a spillover effect from St. Paul onto neighboring areas.

Next, we address the concern that rather than rent control, the decline in property values in St. Paul reflects a concurrent, nationwide trend of migration out of downtown areas and into suburban areas. Panel A of Table V presents a placebo test in which we estimate the difference-in-difference in property values between downtown and suburban regions before November 2021 to afterward for the five comparable metro areas. Across three specifications of location fixed effects, we find no changes in property values for suburban vs. downtown areas. When we control for block group fixed effects, we find a significant and positive effect of 2.2%. This means that in the five comparable cities, property values in downtown areas did not decline relative to suburban areas, as they did in St. Paul.

Panel B of Table V runs a triple-difference test that directly controls for the comparable cities. The triple-interaction term ranges from a statistically and economically significant decline of 6.3%

[6]In unreported tests, we present estimates that control for property-type × geographic location fixed effects to control for the possibility that single family homes in downtown areas are valued differently than in suburban areas. Our results are nearly identical.

to 8.2% for St. Paul. This means that the decline in property values in St. Paul does not reflect a general trend common to the downtown areas of other Midwestern cities of the same size.

Finally, we investigate the time-series of property values in St. Paul relative to values in its suburbs. We re-estimate Equation 3, but replace the dummy variable indicating the post rent-control period with a set of dummy variables that indicate quarterly periods for the entire time period. We define quarters beginning on the 2nd, 5th, 8th, and 11th months to match the period in our sample with rent control from November 2021 to January 2022. The interaction terms reflect the difference in property valuations in St. Paul, relative to its suburbs in each quarter, controlling for size, age, building type, and geographic fixed effects.

Figure III presents the estimates of the difference-in-difference interaction terms over the period from May 2018 through January 2022. The figure confirms our prior findings of a decline in property values of 6% in the period following the passage of rent control. Moreover, the figure shows that this is the largest change in absolute prices over the prior three years. Even with some seasonal patterns, the decline in property values starting in November 2021 is considerably larger than normal. These results indicate that the decline in property values does not reflect a long-term real estate trend.

V.C. Selection Bias

A potential threat to identification is that the passage of rent control may have created selection in the kind of properties transacted in St. Paul. To the extent that the properties transacted after the passage of rent control have lower unobserved quality, beyond our observable control variables, lower transaction prices after the ballot would reflect a change in the composition of transacted properties rather than a change in the value of equivalent properties.

We address this concern with a range of empirical strategies detailed in the Internet Appendix. First, we show that there is no change in the observable characteristics of properties sold after rent control was passed. In particular, we run a battery of tests in which the dependent variable in a difference-in-differences regression is an observable property characteristic, including size, number of bedrooms or bathrooms, age, and dummies for property type. The difference-in-difference coefficients are economically small and statistically insignificant for all property traits except size. For size, we find that properties transacted after the ballot are slightly larger than before, which

suggests higher, rather than lower prices. 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.

We also compare the change in the entire distribution of property traits of properties in St. Paul following the ballot. 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. Likewise, the fraction of sales that were single family residences, townhouses, multifamily buildings, and condos are nearly identical in the two quarters before the ballot and the quarter after the ballot.

Last, we use the methodology in Oster (2019) to bound the magnitude of unobservable differences that would be needed to shrink our estimates of the effects of rent control to zero. 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 11 times the impact of observables, which include micro-location, property size, and age. We interpret this as evidence that our estimates are robust to even large amounts of unobservable bias in the data.

V.D. Aggregate Effects

To put the loss of value in a real-world context, according to the Ramsey County Assessor's Office, there are 73,088 private residential parcels in St. Paul, with an aggregated estimated market value of $24.2 billion. Assuming a loss in value of 6.5%, our estimates imply that rent control caused an aggregate loss of $1.57 billion dollars to property owners in St. Paul over the three months since its passage.

Because property taxes are based on estimated market values, this decline also has significant implications for tax revenue. Using the tax calculators provided by the Ramsey County Assessor's Office, we calculate that rent control would cause a shortfall of roughly 4% in property tax receipts.[7] This represents a significant impact on local government and school revenues.

[7]The shortfall is smaller than 6.5% because of various caps and exemptions in the tax calculation.

VI. 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 VI shows that rent control had a larger negative impact on rental properties than owner-occupied properties. In particular, controlling for location, size, building type, age, and neighborhood income and rental intensity, we find that following rent control, owner-occupied properties in St. Paul transact at prices that are about 6% less than owner-occupied properties outside of St. Paul. However, rental properties in St. Paul transact at prices that are an additional 6–7% lower, on average, following the passage of rent control, then non-rentals. This implies that rental properties in St. Paul have a total loss of about 12%.

The finding of negative effects for both owner-occupied and rental properties suggests that rent control caused both a direct capitalization loss and an indirect loss from negative externalities. This result provides coarse evidence that the direct capitalization effect of rent control on property values is substantially large. In the next section, we refine these estimates in a calibrated model.

These results also provide additional evidence that our results are not driven by spurious correlations. 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.

VI.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. 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, in the extended model, 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, but identical for owner-occupied and rental properties. Therefore, in a non-controlled market, owner-occupied and rental houses that have the same rents have the same prices.

In a rent-controlled market, the growth rate of rent is artificially capped at 3%, but the growth rate of implicit value to owner-occupants is not capped. This creates a capitalization loss for properties that are rented but not for properties that are owner-occupied. However, based on empirical evidence, we assume there is a small, but positive probability that an owner-occupied property switches to become a rental, and vice versa. 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 3%, rent control still creates a capitalization loss because the right tail of the growth rate distribution is truncated.

To calculate predicted losses, we calibrate this model to the St. Paul market. 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%. We set the probability of switching from owner-occupied to rental to be 3.18% and from rental to owner-occupied to be 13.25%, which match the historical annual probabilities calculated from parcel-level data in St. Paul from 2010 to 2020. Below, we allow for these probabilities to change.

After fixing these parameters, the model generates the capitalization loss as a function of the expected non-controlled growth rate. When the expected growth rate is 3.5%, the model predicts a capitalization loss of 4.75% for rentals and 2.5% for owner-occupied properties. When the growth rate is 4.5%, the capitalization losses are roughly 10% for rentals and 5% for owner-occupied properties. The fact that these expected losses are similar to what we find in the data suggests 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 non-negligible 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 growth rate such that the difference in the predicted value losses

of rental properties and owner-occupied properties matches our empirical estimates in Table VI. 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 value loss is in the form of capitalization losses and the remaining 10% is indirect externality loss. 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 probabilities from 3.18% (the historical average in St. Paul) to 1.70%. This range is centered around the value of 2.45%, which corresponds to a 20% drop in the supply of rentals in the steady state and is similar to the decrease in rental supply reported in Diamond, McQuade, and Qian (2019) after the expansion of rent control in San Francisco. The 1.70% lower bound in our range is the transition probability that would create a decomposition of negative externalities found in Autor, Palmer, and Pathak (2014).

Figure IV 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.

In sum, this section shows that the magnitudes of the losses we observe can be rationalized in a pricing model with few parameter assumptions. Second, the model decomposition predicts that 67% of observed losses are direct capitalization losses and 33% are indirect, negative externalities. These expected negative externalities are less than the 56% reported by Autor, Palmer, and Pathak (2014) for Cambridge, Massachusetts, but still represent a substantial deadweight loss. Using our model estimates of externalities, at least $518 million of the aggregate loss in St. Paul is deadweight loss.

VII. 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 identities of landlords and renters. We also use our pricing model to show that the cross-sectional variation is driven by direct effects rather than indirect negative externalities.

VII.A. Transfers vs. Deadweight Loss: Theory and Evidence

Property value losses are a useful proxy for wealth transfers under the condition that 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. The more constraining is the rent cap, the larger is the transfer. 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 the value loss 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.

Empirical evidence supports the textbook model of rent control. We use variation in current rent-to-price ratios across St. Paul to proxy for cross-sectional variation in the expected growth rate of rents. We find a positive relationship between these expected growth rates and the value loss caused by rent control. This supports the claim that the areas where rent control is expected

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

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. Under rent control, 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. To measure supply elasticity, we use the Census tract-level measures provided by Han and Baum-Snow (2021).[8] In the Internet Appendix, we report a positive and significant correlation between value loss and supply elasticity, as predicted. 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 heterogeneous quality model which implies that larger losses indicate larger transfers.

In sum, we show that the direct capitalization effect is driven mainly by transfers, rather than deadweight losses, and that value losses are a reliable proxy for transfers.

[8]In the Internet Appendix, we validate the supply elasticity measures of Han and Baum-Snow (2021) by showing a strong and statistically significant relationship between building permits issued in St. Paul between October 2018 and October 2021 and Han and Baum-Snow's measures of supply elasticity of new floor space and new units.

VII.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 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. To conduct these tests, we measure the size of transfers for a cross-section of residential properties in St. Paul and identify the demographic traits of their owners and renters. We discuss each of these measurements below.

B.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, 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 estimated coefficients of Equation 5 to predict the property values for all residential parcels in St. Paul using administrative data from the Ramsey County Assessor's office. For the 73,103 residential parcels in St. Paul, 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. The data do not include square footage for apartment buildings with four or more units. This prevents us from estimating these properties' values. Because the Redfin transaction data do not include large, multi-unit properties either, our estimates would not be reliable even with data on square footage for apartment buildings. Therefore, our analysis focuses on properties with three or fewer units.

To estimate changes in property values caused by rent control, we calculate the predicted value of each parcel at two dates: October 2021 and January 2022. Using these estimates, our main variable is the negative of the difference in the logged values of these pre- and post-rent control estimated values, which we denote as *loss*. A larger loss represents a bigger decline in log prices from before to after rent control.

B.2. The Demographic Traits of Owners and Renters

In an ideal setting, we would directly observe the demographic traits of owners and renters at the parcel level. We could then compare these to the parcel-level estimates of value loss. Because we cannot observe the traits at the parcel level, we perform our analysis at the most granular level possible, the Census block group level, using data from the 2019 five-year estimate from the American Community Survey provided by the Census Bureau.

Because renters live in the block group where the property is located, we proxy for a block group's renters' demographic traits using the block group level data from the Census Bureau in which the property is located. This assumes that the average renter in the block group is similar to the average Census responder in the block group. If renters are systematically different from the average resident, our estimates could be biased. The relatively small areas covered by each block

group helps mitigate this issue. We are unaware of any other source of data that would allow us to more precisely measure the demographic traits of renters.

Measuring owners' demographic traits is less straightforward. We use the administrative assessor's 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 include the owners' demographic data in the sample. 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.

Next, we classify properties as rental properties or owner-occupied properties. First, using the administrative data from the Assessor's office, we assign all properties with more than one unit to be a rental. It is possible that an owner occupies one unit in a multi-unit property. However, we expect that the impact of rent control for an owner-occupied multi-unit property is more similar to a rental than to an owner-occupied single family home. 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. It is possible that some landlords rent their properties without getting a certificate of occupancy. Therefore, we also identify rental properties as any property that has been offered for rent in the last three years, as covered by the HousingLink data described above. We classify single family homes as rentals if either the property has been offered for rent or has received a fire certificate of occupancy.

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. We use the property address as the owner's address. 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.

Thus, for each parcel in St. Paul owned by an owner-occupant or a small landlord, we have an estimate of the owner's demographic profile. We then aggregate these profiles to the block group level by taking the average of the individual owner's profile, filtering by the type of owner. In the end, we have data for each block group in St. Paul that describes the demographic profile of renters and owners.

B.3. Summary Statistics of Parcel-Level Data

There are 78,221 parcels in St. Paul, including 73,103 residential parcels and 2,148 commercial parcels. Of the residential parcels, 65,180 are single-family residences, 6,064 are multi-unit parcels with two or three units, and 1,859 are apartments with four or more units. Due to missing fields in the administrative data, we can calculate the value loss for 59,466 single family residences and 3,001 two-to-three unit parcels. As mentioned above, because the data do not include square footage for apartment buildings, we do not estimate their values.

Of the 59,466 single family residences, 53,718, or 90%, are owner-occupied, 4,165 are rentals with small landlords, and 1,583 are rentals with large landlords. Of the two-to-three unit parcels, 2,259, or 75%, are owned by small landlords, and the remaining 25% 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 or the directly adjacent cities, and 41% live in St. Paul.

Across all single-family homes and two-to-three unit properties, the average value loss is 4.5% and the median is 2.9%. These estimates are slightly smaller than the 6% average loss estimated from the transaction data. This is explained by aggregating differences in characteristics between the sample of transacted properties and the stock of parcels. Across property types, there is relatively little variation in value loss, though the loss is largest, on average, for small landlords of single family residences with a 5.3% loss, while owner-occupied properties had the smallest loss at 4.1%.

Figure V 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.

B.4. The Redistribution of Wealth Caused by Rent Control: Univariate Evidence

As discussed above, rent control is intended to benefit lower income renters at the cost of higher income owners. Thus, if rent control has its intended effect we expect to see a larger transfer from owners with relatively higher incomes to renters with relatively lower incomes. Though a common narrative is that landlords are much wealthier than tenants, this is not obviously true. Though no recent work on this subject is available, Johnson (1951) shows that when rent control was imposed in New York City in the 1940s, landlords were not significantly wealthier than tenants. Therefore, to test whether rent control achieves its intended effect, we separate block groups into high income and low income block groups according to the median household income for renters. We do the same for owners and form four subsamples by the combination of owner and renter incomes.

Table VII presents the average value loss by owner and renter incomes. In particular, we denote the subsample of block groups with high-income owners and low-income renters in Column 2 as 'high disparity' areas and the subsample in column 3 with low-income owners and high-income renters as 'low disparity' areas. To complete the sample, column 1 presents averages for block groups with high-income owners and renters and column 4 for low-income owners and renters. Observations are block group level averages from properties with small landlords of any size property.

The results in Table VII indicate that the winners and losers from rent control are the opposite of its intended goal. In contrast to the intended transfer from higher-income owners to lower-income renters, column 2 shows that the value loss for the high disparity subsample is 0.89%, below the average value loss of 4.5%. This effect is statistically smaller than the effect for the other three subsamples. In contrast, column 3 shows that the statistically largest effect of rent control, at 8.52%, occurs in the low disparity parts of the city where renters have higher incomes and owners

have lower incomes. This implies that the impact of rent control is poorly targeted: the largest transfer of wealth is from relatively low income owners to relatively high income renters.

Table VII also reveals that the magnitude of the transfer from owners to renters varies with neighborhood and demographic factors. First, the transfer is smallest in areas that have the highest intensity of rental housing. In the areas with high owner incomes and low renter incomes, in which the transfer is small, 57% of housing units are rentals. In contrast, in areas with low owner incomes and high renter incomes, in which the transfer is high, the fraction of rentals is only 34%. Thus the gains from rent control are received by a relatively small number of higher income renters.

Table VII also presents variation in income, age, race, and education across owners and renters and by income groups. First, the household income of owners is statistically higher than renters on average, as expected. However, in the subsample of higher income renters and lower income owners, the incomes of renters and owners are statistically equivalent. This means that the transfer caused by rent control is largest when owners and renters have statistically equivalent incomes. In contrast, owners have incomes more than double renters' incomes when the transfer is the smallest.

Next, owners are older than renters in all subsamples. As before, owners are especially older than renters when owners have high incomes and renters have low incomes. This result follows from the fact that income is positively related to age.

There are also significant differences in the race of owners and renters across the four subsamples. Owners live in neighborhoods with a higher percentage of white people than do renters for all subsamples of income. In the subsamples that include low income renters, the fraction of white people is significantly less. The same result is found for education. Owners are more likely to have bachelors degrees than renters for all subsamples, but the effect is most pronounced when renters have low income.

These results show that the transfer of wealth caused by rent control is largest in the areas of St. Paul in which owners and renters are most alike in terms of income, age, race, and education. The areas where the transfer is the smallest are the areas in which owners and renters are least alike: owners have higher income and education and are more likely white and older than renters. The results also reveal that the demographics profile of the winners and losers from rent control are highly correlated across income, age, race, and education.

B.5. The Redistribution of Wealth Caused by Rent Control: Multivariate Regression Evidence

To better understand the explanatory power of each demographic trait, columns 1 through 4 of Table VIII present cross-sectional regressions of the demographic traits of owners and renters on the loss caused by rent control at the block group level.

Each regression includes the fraction of housing that is rental as a control; the correlation is always positive and significant, which suggests that, even after controlling for differences in income, neighborhoods that have a higher fraction of rental properties experiences higher value losses. As mentioned in the discussion of Table VII, these are block groups in which owners are more likely to have low income and to be minorities.

The first specification in Table VIII also shows a strong positive correlation between renters' household income and the value loss caused by rent control. Consistent with the univariate evidence, as renters' incomes decline, the transfer they receive from rent control diminishes. The income of owners is not significant after controlling for the income of renters.

The second specification in Table VIII adds the additional demographic traits of age, race, and education to the regression. None of the traits are significantly related to the size of the transfer. However, we still find that the income of renters is positively related to the size of the transfer and the incomes of owners is now significantly negatively related to the size of the transfer. In the third specification, we use the difference between owners and renters' incomes as an explanatory variable. Without any additional controls, this difference is negatively related to the size of the transfer. This means that as the disparity between owners and renters increases, the size of the transfer to renters decreases. The fourth specification includes the other control variables, of which none are significant, while income differences are still significantly related to the value loss.

Finally, in column 5, we use the difference in incomes of owners and renters as the dependent variable. We find that this disparity is larger when owners are more likely to be white and renters are less likely to be white, owners are more likely to have a bachelors degree and renters are less likely to have a bachelors. The ages of owners and renters is uncorrelated with the difference in incomes. These results highlight that income, race, and education are highly correlated. However, controlling for all variables at the same time, income has the most precise explanatory power for the variation in value loss across block groups in St. Paul.

In the Internet Appendix, we provide robustness tests that control for supply elasticity, using the measure provided by Han and Baum-Snow (2021). While the supply elasticity measures have positive coefficients, they are not statistically significant, and their inclusion leaves the coefficient for the income difference between owners and renters significant. We also show that the relationship between value losses and renter income, and the relationship between value losses and the income delta, are robust to controlling for different measures of block group level transaction volume, and for the concentration of large apartment buildings in the block.

VII.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. We provide an overview here, but provide details on the model in the Internet Appendix. For each of 209 Census block groups in St. Paul, we derive an expected growth rate based on current, observable rent-to-price ratios and an assumed discount rate of 8%. Similar to our approach before, we estimate each block group's expected growth rate using rent-to-price ratios to predict the direct capitalization loss for owner-occupied and rental properties. We take the weighted average of these losses based on the fraction of rentals within a block group to construct a block group-level of predicted direct, capitalization losses. Based on the arguments presented above, these direct effects are primarily transfers from owners to renters. As before, 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 findings above, we find that the model-implied transfers are strongly positively associated with the income of renters and negatively associated with the difference between owner and tenant income. 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. In contrast, the estimated indirect negative externality component is not statistically related to the income of renters. These results imply that though higher income renters receive a larger transfer of wealth than lower income renters, the indirect negative spillover affects all residents of the city relatively equally.

In sum, the results in this section provide consistent evidence that the rent control ordinance in St. Paul caused a transfer of wealth from owners to renters in the opposite neighborhoods as intended. Instead of wealthier owners transferring wealth to poorer renters, we find that the wealth transfers were greatest when owners were relatively poorer and renters were relatively wealthier. Because income is highly correlated with race and education, these results also show that the renters more likely to benefit from rent control were less likely to be minorities and were more likely to be highly educated. Finally, the results indicate that the negative externalities are not highly localized and may relate to city-wide changes in crime, educational quality, or other city-wide quality of life traits.

VIII. 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.*" The debate continues today as policymakers and voters enact a third generation of rent control laws.

We believe 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.

First, 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. These declines are larger for rental properties, but also spill over onto owner-occupied properties. We estimate that about two-thirds of the value loss is driven by a direct capitalization effect. The aggregate loss in property value in St. Paul is $1.6 billion.

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 and are less likely to be minorities, and in which the income difference between owners and renters is the smallest.

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. As shown in Table I, the majority of rent control laws are passed by legislative bodies, but are more likely rejected in ballot measures. 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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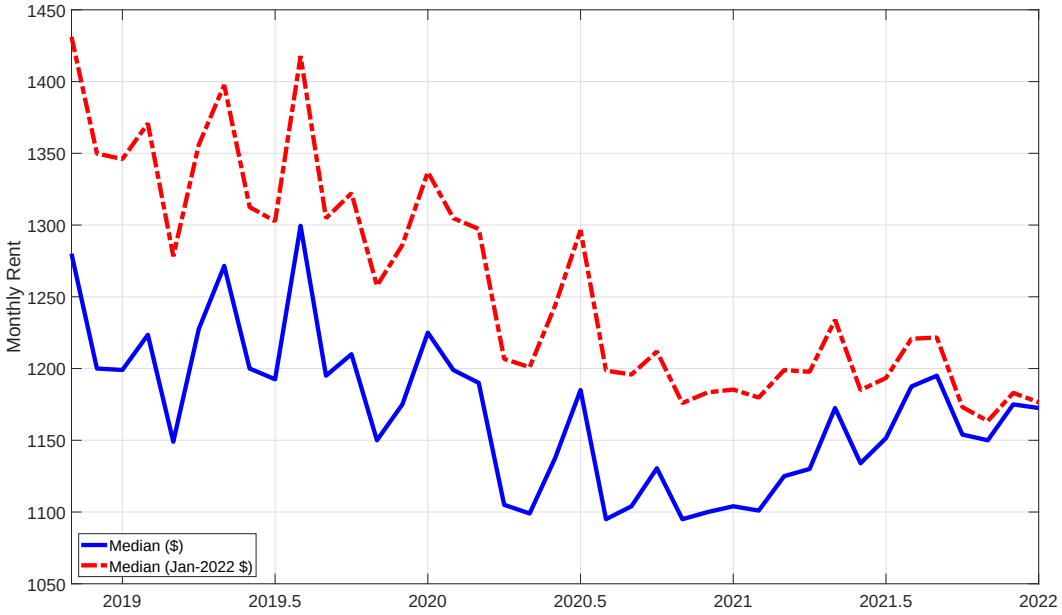

FIGURE I
RECENT TIME SERIES OF MEDIAN RENTS IN ST. PAUL

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.

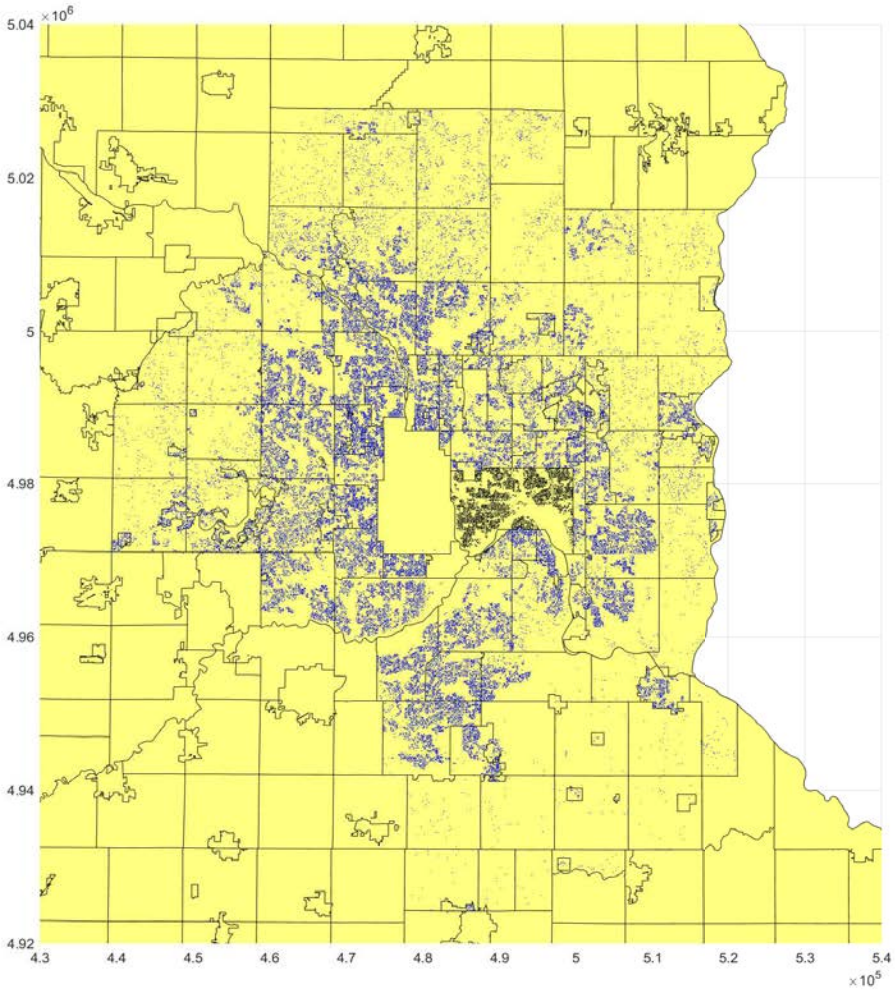

FIGURE II
LOCATION OF HOUSE SALES IN ST. PAUL VS. SUBURBS

This figure shows the location of house sales in the Redfin sample for the Metropolitan area of the Twin Cities (excluding the city of Minneapolis) used in our analysis. The data cover the period from January 2018 to January 2022. Sales within the city of St. Paul are highlighted in black, while sales in the surrounding cities are highlighted in blue.

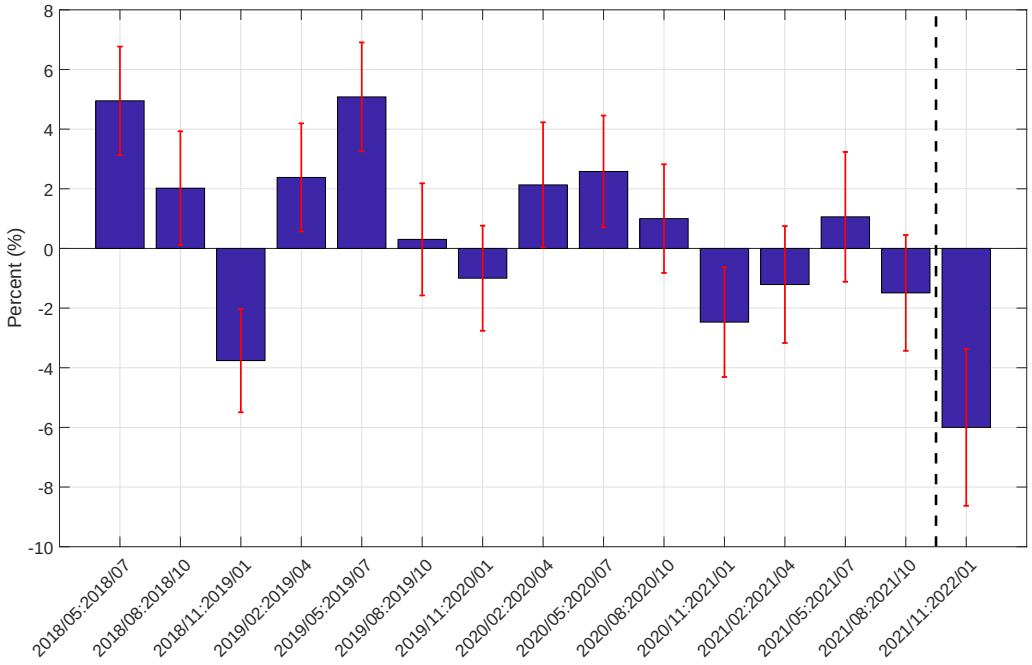

FIGURE III
TIME-SERIES OF PRICES IN ST. PAUL VS. SUBURBS

This figure presents coefficient estimates and their 95% confidence intervals from the interaction between dummy variables for quarters and a dummy variable for property located in St. Paul, controlling for property size, age, type, and city fixed effects. Confidence intervals are based on standard errors that are double-clustered by city and year-month. Quarters start on the 2nd, 5th, 8th, and 11th months and the benchmark quarter is 2/2018 – 4/2018.

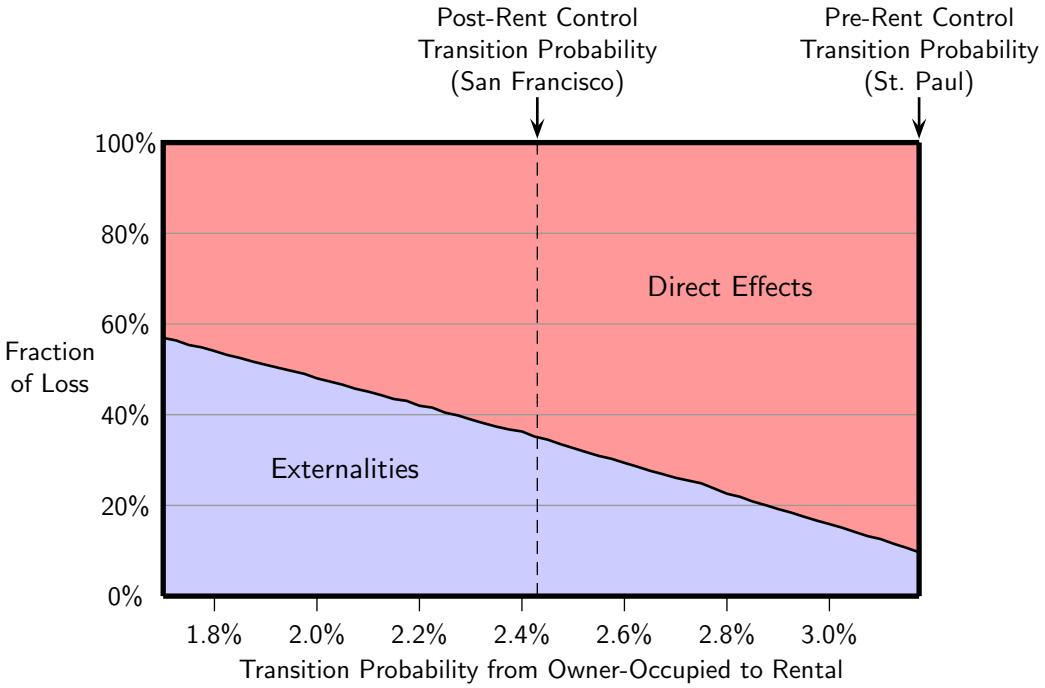

FIGURE IV
DECOMPOSITION OF LOSSES FOR OWNER-OCCUPIED HOUSES

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 after rent control is imposed as computed from the supply effects in San Francisco reported in Diamond et al. (2019). The highest transition probability indicated is 3.18% which is the historical average transition probability in St. Paul during the pre-rent control period 2010 to 2020.

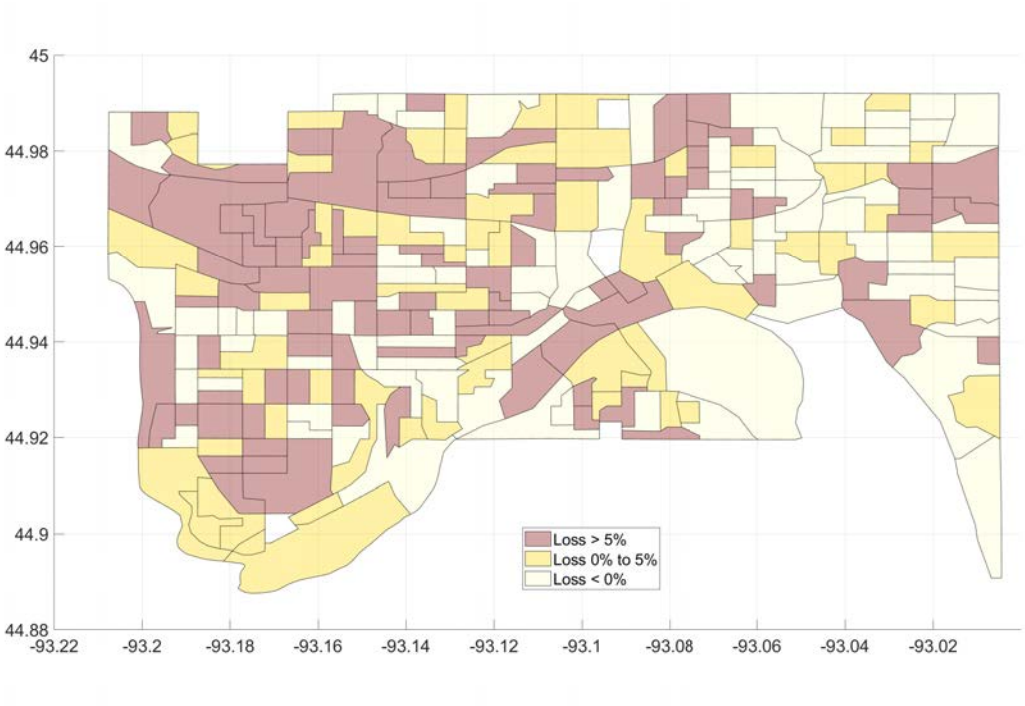

FIGURE V
DISTRIBUTION OF VALUE LOSSES ACROSS ST. PAUL CENSUS BLOCK GROUPS
This figure presents the average value loss generated by the rent control law at the block group level, estimated using the specification in Equation 5.

TABLE I
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 government 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 and 4.25% otherwise)
Philadelphia, PA	2020	City council	Pending	Allow local government to enact rent control
Los Angeles County, 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	Passed	Explore options for one-year rent freeze
St. Petersburg, FL	2021	City council	Passed	Explore options for rent control
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%)

TABLE II
SUMMARY STATISTICS OF REAL ESTATE TRANSACTIONS BEFORE RENT CONTROL

	Mean	Standard Deviation	Percentile 25th	Percentile 50th	Percentile 75th
Panel A: City of Saint Paul (Observations = 14,178)					
Price ($)	282,112	149,747	195,500	242,400	320,000
Square feet	1658.54	710.38	1202.53	1533.74	1948.53
Price per square foot ($)	174.07	54.04	134.00	166.00	208.00
Building age (years)	86.93	32.57	67.00	95.00	110.00
Property type: Condo/Co-op (%)	10.19	30.26	0.00	0.00	0.00
Property type: Multi-family (%)	8.20	27.43	0.00	0.00	0.00
Property type: Single-family (%)	78.88	40.82	100.00	100.00	100.00
Property type: Townhouse (%)	2.74	16.32	0.00	0.00	0.00
Rental property (%)	6.92	25.38	0.00	0.00	0.00
Rent ($)	1620.34	849.01	1150.00	1375.00	1622.50
Panel B: Suburbs of Saint Paul (Observations = 129,413)					
Price ($)	365,854	214,407	245,000	315,000	421,834
Square feet	2236.62	995.42	1572.17	1989.97	2686.54
Price per square foot ($)	164.56	44.25	136.00	156.00	183.00
Building age (years)	37.30	24.89	19.00	34.00	54.00
Property type: Condo/Co-op (%)	4.69	21.15	0.00	0.00	0.00
Property type: Multi-family (%)	1.21	10.95	0.00	0.00	0.00
Property type: Single-family (%)	72.05	44.87	0.00	100.00	100.00
Property type: Townhouse (%)	22.04	41.45	0.00	0.00	0.00
Rental property (%)	2.93	16.87	0.00	0.00	0.00
Rent ($)	1522.44	520.03	1195.00	1395.00	1750.00
Panel C: Comparable Metro Areas (Observations = 680,193)					
Price ($)	370,507	272,477	204,000	315,000	456,000
Square feet	2258.76	1154.45	1408.05	1999.31	2831.33
Price per square foot ($)	168.11	85.74	114.00	151.00	201.00
Building age (years)	37.05	30.49	14.00	29.00	56.00
Property type: Condo/Co-op (%)	8.51	27.90	0.00	0.00	0.00
Property type: Multi-family (%)	0.79	8.87	0.00	0.00	0.00
Property type: Single-family (%)	85.34	35.37	100.00	100.00	100.00
Property type: Townhouse (%)	5.35	22.51	0.00	0.00	0.00

Notes: Observations are completed real estate transactions in the pre-rent control period from January 2018 to October 2021. Data are from Redfin.com and HousingLink. The suburbs of St. Paul exclude Minneapolis for reasons discussed in the paper. Comparable Metro Areas include St. Louis, Kansas City, Indianapolis, Denver, and Nashville.

TABLE III
UNIVARIATE TRIPLE-DIFFERENCE TESTS OF PRICE PER SQUARE FOOT

	Pre-Rent Control	Post-Rent Control	Difference
Panel A: Twin Cities Metro Area			
Downtown (St. Paul)	174.18	185.02	10.84***
	(0.453)	(2.124)	(2.171)
Suburbs	164.56	187.56	23.01***
	(0.123)	(0.635)	(0.647)
Difference	9.63***	−2.55	−12.17***
	(0.470)	(2.217)	(2.264)
Panel B: Comparable Metro Areas			
Downtown	180.40	214.38	33.98***
	(0.265)	(1.087)	(1.119)
Suburbs	163.72	194.22	30.50***
	(0.104)	(0.460)	(0.472)
Difference	16.68***	20.16***	3.48***
	(0.285)	(1.180)	(1.214)
Panel C: Differences			
Downtown	−6.22***	−29.27***	−23.04***
	(0.525)	(2.393)	(2.449)
Suburbs	0.83***	−6.67***	−7.50***
	(0.161)	(0.784)	(0.800)
Difference	−7.06***	−22.60***	−15.54***
	(0.550)	(2.517)	(2.576)

Notes: Table entries are average transaction price per square foot across all transactions, with standard errors in parentheses. The pre-rent control period is from January 2018 to October 2021. The post-rent control period is from November 2021 to January 2022. Data are from Redfin.com. The Twin Cities Metro Area excludes Minneapolis for reasons discussed in the paper. Comparable Metro Areas include St. Louis, Kansas City, Indianapolis, Denver, and Nashville. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

TABLE IV
DIFFERENCE-IN-DIFFERENCE EFFECT OF RENT CONTROL ON TRANSACTION PRICES

	Dependent variable: ln(price)		
	(1)	(2)	(3)
St. Paul × Post	−0.060***	−0.066***	−0.061***
	(0.019)	(0.010)	(0.009)
ln(square feet)	0.710***	0.718***	0.644***
	(0.018)	(0.031)	(0.007)
ln(building age)	−0.082***	−0.082***	−0.090***
	(0.005)	(0.006)	(0.003)
Property type: Multi-family	0.176***	0.115	0.274***
	(0.040)	(0.104)	(0.020)
Property type: Single-family	0.289***	0.244***	0.351***
	(0.035)	(0.084)	(0.018)
Property type: Townhouse	0.115***	0.075	0.164***
	(0.031)	(0.074)	(0.018)
Location fixed effects	ZIP code	City	Block group
Time fixed effects	Year-month	Year-month	Year-month
Adjusted R^2	0.848	0.833	0.880
Observations	149,480	149,476	149,472

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 in November 2021, December 2021, or January 2022, after rent control is passed in St. Paul. The omitted property type category is Condo/Co-op. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

TABLE V
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	0.011	0.011	−0.002	0.022***
	(0.015)	(0.008)	(0.011)	(0.004)
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	0.700	0.859	0.787	0.898
Observations	714,694	714,558	714,629	714,646
Panel B: Triple Difference Tests of St. Paul vs. Comparable Metro Areas				
Twin Cities × Post	−0.050	−0.061***	−0.059***	−0.062***
	(0.027)	(0.006)	(0.008)	(0.004)
Downtown × Post	0.010	0.011	−0.003	0.021***
	(0.015)	(0.008)	(0.011)	(0.004)
Twin Cities × Downtown × Post	−0.071**	−0.070***	−0.063***	−0.082***
	(0.019)	(0.020)	(0.006)	(0.010)
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	0.704	0.858	0.790	0.896
Observations	864,175	864,040	864,108	864,118

Notes: Observations include real estate transactions over the period January 2018 to January 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. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

TABLE VI
TRIPLE-DIFFERENCE EFFECT OF RENT CONTROL ON TRANSACTION PRICES FOR RENTAL HOUSING VS. NON-RENTAL HOUSING

	Dependent variable: ln(price)		
	(1)	(2)	(3)
Rental	−0.067***	−0.095**	−0.072***
	(0.022)	(0.038)	(0.014)
St. Paul × Post	−0.057***	−0.061***	−0.057***
	(0.018)	(0.013)	(0.008)
St. Paul × Rental	0.013	0.213***	−0.002
	(0.083)	(0.030)	(0.043)
Post × Rental	−0.010	−0.007	−0.003
	(0.011)	(0.012)	(0.010)
St. Paul × Post × Rental	−0.066***	−0.064***	−0.073**
	(0.023)	(0.015)	(0.035)
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	0.849	0.834	0.881
Observations	149,480	149,476	149,472

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 in November 2021, December 2021, or January 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), ln(age), and dummy variables for property types. Block group is the 2019 Census block group geographic area. Standard errors double-clustered at the year-month and location level are presented in parentheses. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

TABLE VII
THE TRANSFER OF WEALTH CAUSED BY RENT CONTROL: DISPARITIES BY OWNER AND RENTER INCOME

Owner Income:	High	High	Low	Low						
Renter Income:	High	Low	High	Low	Differences					
	(1)	(2)	(3)	(4)	(1) − (2)	(1) − (3)	(1) − (4)	(2) − (3)	(2) − (4)	(3) − (4)
Neighborhood										
Value loss (%)	4.30	0.89	8.52	3.72	3.41*	−4.22*	0.58*	−7.63***	−2.83*	4.80*
Rental units (%)	30.82	57.39	33.97	56.44	−26.57***	−3.15*	−25.62***	23.42***	0.94*	−22.47***
Space Elasticity	0.14	0.079	0.123	0.085	0.063***	0.016	0.056***	−0.047***	−0.006	0.041***
Income ($1,000s)										
Owners	103.72	100.34	85.16	84.10	3.38*	18.56***	19.62***	15.18***	16.24***	1.06
Renters	87.73	44.45	82.36	42.89	43.27***	5.36	44.84***	−37.91***	1.57	39.48***
Difference	16.08***	55.88***	2.79	41.21***	−39.81***	13.28***	−25.13***	53.09***	14.67***	−38.41***
Age										
Owners	40.84	40.51	40.14	39.09	0.33	0.70	1.75***	0.37	1.42***	1.05**
Renters	35.93	31.82	35.63	31.40	4.11	0.30	4.53***	−3.81***	0.42	4.23***
Difference	4.98***	8.69***	4.50***	7.69***	−3.71***	0.48	−2.70**	4.19***	1.00	−3.18***
Race: White (%)										
Owners	85.02	79.38	78.99	74.08	5.64***	6.03***	10.94***	0.39*	5.30***	4.91***
Renters	78.15	51.72	74.00	43.48	26.43***	4.14*	34.67***	−22.29***	8.24**	30.52***
Difference	6.61***	27.66***	4.99*	30.60***	−21.06***	1.62*	−23.99***	22.68***	−2.93*	−25.61***
Education Attained: Bachelors Degree (%)										
Owners	33.25	30.17	29.98	27.24	3.08***	3.26***	6.01***	0.18*	2.93***	2.75***
Renters	31.23	20.20	29.63	16.88	11.03***	1.60*	14.36***	−9.43***	3.33*	12.76***
Difference	1.91*	9.96***	0.35*	10.36***	−8.05***	1.56*	−8.45***	9.61***	−0.40*	−10.01***

Notes: This table presents averages of block group-level observations for St. Paul. Column 1 includes block groups in which owners and renters have income above the median owner and renter income. Column 2 includes block groups in which owners have above median income and renters have below median income. Columns 3 includes block groups in which owners have below median income and renters have above median income, and Column 4 includes block groups in which both owners and renters have income below median. *Value loss* is the estimated average value loss for residential properties. *Rental units* is the fraction of residential units that are rented. *Owners* observations are based on the average block group variables from the addresses of small landlords. Statistical significance of differences in means at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.

TABLE VIII
OWNERS AND RENTERS' DEMOGRAPHICS AND THE TRANSFER OF WEALTH

Dependent variable:	Value loss				Δln(Income)
	(1)	(2)	(3)	(4)	(5)
ln(Income) of owners	−0.103	−0.206**			
	(0.074)	(0.100)			
ln(Income) of renters	0.085***	0.060*			
	(0.025)	(0.032)			
Δln(income)			−0.086***	−0.077**	
			(0.024)	(0.030)	
ln(age) of owners		0.064		0.014	0.220
		(0.225)		(0.234)	(0.375)
ln(age) of renters		−0.006		0.004	0.109
		(0.069)		(0.067)	(0.149)
Owners that are white (%)		0.266		0.235	0.720*
		(0.226)		(0.223)	(0.405)
Renters that are white (%)		0.023		0.020	−0.792***
		(0.074)		(0.073)	(0.137)
Owners with bachelors (%)		0.181		−0.062	1.301*
		(0.363)		(0.341)	(0.664)
Renters with bachelors (%)		−0.060		−0.041	−0.800***
		(0.127)		(0.128)	(0.226)
Rental housing (%)	0.105**	0.092*	0.109**	0.123**	0.930***
	(0.050)	(0.055)	(0.049)	(0.053)	(0.094)
Constant	0.242	1.218	0.035*	−0.211	−1.426
	(0.860)	(1.464)	(0.019)	(0.744)	(1.166)
Adjusted R^2	0.031	0.032	0.034	0.026	0.623
Observations	246	246	246	246	247

Notes: Observations are at the block group level in St. Paul. *Value loss* is the estimated loss in the average parcel in a block group caused by rent control. Δln(Income) is ln(income) of owners minus ln(income) of renters. Standard errors adjusted for heteroskedasticity are presented in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is indicated by *, **, and ***.