Does Rent Regulation Affect Tenant Unemployment? Evidence from New York City *

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Abstract

We study the effect of rent stabilization in NYC on tenant unemployment from 2002 to 2017. We show rent stabilization increases tenants' unemployment by six percentage points, more than double the average unemployment rate during the study period. Moreover, the effect is concentrated in traditionally privileged groups. We address the potential policy's endogeneity with an instrumental variable that captures neighborhood-level rent-stabilized units' relative vacancy when the tenant moves in. We develop a job-search model with rent stabilization that rationalizes the disincentive mechanism behind this effect. We confirm empirically heterogeneous effects across groups of workers in the directions suggested by the model.

Key Words: Rent Regulation, Rent Stabilization, Tenant Unemployment, Job Search Model, Policy Awareness, New York City.

JEL Codes: H70, L51, R21, R28, J60

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1 Introduction

Rent regulation[1] has become increasingly popular as a tool to keep rental housing affordable among state and local governments. Oregon, New York, Minnesota, and California have all passed rent-stabilization bills since 2019 with widespread support despite an historically unfavorable perception among economists who express concerns about its potential long-term effects on misallocation, housing supply and affordability for future renters (Glaeser, 2003; Autor et al., 2014; Diamond et al., 2019). With the severity of the housing-affordability crisis in the rental market on the rise,[2] we would not expect the interest in rent regulation to vanish.[3] Rent-regulation popularity has extended beyond the US, with Berlin imposing a five-year rent-freezing plan in 2020 (Hahn et al., 2021), and Barcelona, Amsterdam, and Paris implementing strict rent regulation in 2019. All measures have faced legal challenges but still hold popular support among tenants who state that housing affordability is crucial for human development (Meltzer and Schwartz, 2016; Farha et al., 2020). And yet, we know little about the effects of rent regulation on dimensions that go beyond housing.

This paper estimates the effect of rent stabilization on tenant unemployment for over 15 years in New York City (NYC) from 2002 to 2017, a unique laboratory that provides a large population covered by a binding rent-regulation policy,[4] a large share of renters, and a housing market with affordability issues. Our results show rent stabilization increases the unemployment of tenants by approximately six percentage points, more than double the average unemployment rate during the study period. Moreover, we find an effect concentrated among non-minority tenants, those with high education levels, and those with expected higher rent discounts. These results are consistent with the theoretical predictions of the job-search model with rent regulation we develop.

Rent stabilization limits rent increases, providing households with significant savings and housing stability, key factors that affect workers' decisions in the labor market (Jacob and Ludwig,

[1] Two types of rent regulation exist: rent stabilization and *hard price* rent control. The two terms are often used interchangeably, but with some important differences. Rent control restricts rent levels and is often discussed as a canonical textbook example of a price control (Arnott, 1995). Rent stabilization restricts rent growth.

[2] See America's Rental Housing (2021), a report written by the Joint Center for Housing Studies of Harvard University.

[3] For example, political support has grown in New York State, even for a *good cause* eviction reform that would effectively extend rent regulation (Capps, 2022) to most units in the state, including New York City (NYC).

[4] Based on the New York City Housing and Vacancy Survey (NYCHVS), NYC had about half of its rental units under the rent-stabilization policy between 2002 and 2017, with around 1 million units being rent stabilized in 2017.

2012). With lower rental payments, a worker may decide to forgo taking a job offer and search for longer. This behavior would be affected by the size of the rent discount and the expected gains in waiting for a future offer. This paper extends the standard job-search model to include rent stabilization to formalize this intuition and motivate our empirical work. The model predicts a positive effect of rent regulation on tenant unemployment, an effect that grows with the rent-discount size, expected wage, and workers' risk aversion.

Tenants living in rent-stabilized units enjoy a rent discount. Even though the rent discount does not depend on unemployment status, it can increase unemployment by increasing the value of unemployment relative to employment, because the tenant's consumption level is lower during unemployment. This effect of rent discount on unemployment depends on assuming a concave utility, that is, a diminishing marginal utility of consumption. Following the unemployment insurance (UI) literature, we refer to this mechanism as the *disincentive effect* throughout the paper.[5] Our model shows a rent discount affects job-search behaviors and unemployment outcomes through this disincentive effect as long as it changes the relative value of being unemployed to being employed. Theoretically, besides the disincentive channel, rent stabilization can also affect individual job search and unemployment through mobility[6] and liquidity[7] channels. Though we cannot completely rule out the mobility and liquidity mechanisms, we develop a model that shows how the disincentive channel works and provide empirical evidence supporting its existence.

This paper uses the New York City Housing and Vacancy Survey (NYCHVS) from 2002 to 2017. In addition to containing information about household characteristics and unemployment status, a unique feature of NYCHVS is that it identifies the rent-regulation status of each housing unit verified by administrative sources. Therefore, the treatment variable does not rely on self-reporting, which often leads to severe measurement errors. Empirically, endogeneity concerns arise

[5]It has long been recognized that although UI offers insurance and consumption smoothing, it also comes at the cost of distorting incentives to look for a job (Schmieder and Trenkle, 2020). This disincentive effect is present in the standard job-search model (McCall, 1970; Rogerson et al., 2005) in which UI increases the value of being unemployed relative to being employed, by directly increasing the unemployed disposable income. In this way, UI can increase unemployment without assuming a concave utility function.

[6]Rent stabilization increases dwelling stability by imposing benefits linked to specific housing units (Diamond et al., 2019). This stability growth may reduce job-search scope, resulting in less frequent job offers and higher unemployment (Svarer et al., 2005). The literature also cites mobility as a common reason that homeownership causes unemployment, the so-called Oswald hypothesis (see Oswald (1996); Munch et al. (2006, 2008); Coulson and Fisher (2009), and Morescalchi (2016)).

[7]Similar to unemployment benefits, rent stabilization can ease liquidity constraints and reduce job-search efforts, leading to longer unemployment spells, a liquidity channel (Chetty, 2008).

in estimating the causal impact of rent stabilization on tenant unemployment, because renters living in rent-stabilized units might be systematically different in observed and unobserved ways from those living in private market-rate units. Those differences may be correlated with rent-stabilization status and directly affect tenant unemployment.

To address these concerns, we first provide evidence of the difficulty in sorting into rent-stabilized units in this market.[8] We show that although tenants of rent-stabilized units are not observationally identical to tenants in market rates, the differences have minimal economic magnitude, particularly in terms of major socioeconomic characteristics such as education and income, assuaging the selection on observables concert. This pattern is distinct from what is commonly found in other assisted housing programs, especially those that are means tested, in which program beneficiaries have much lower socioeconomic status and educational attainment (Baum-Snow and Marion, 2009; Diamond and McQuade, 2019).[9]

Yet, one may still be concerned with an omitted-variable bias coming from the tenants' selection into rent-stabilized units based on unobservable characteristics that is correlated with their unemployment outcomes. For example, workers who expect to have more difficulties finding a job may spend more time searching for a rent-stabilized unit, because they have a more significant incentive to reduce housing expenditure. This selection into the policy would bias upwards the unemployment effect of rent stabilization. By contrast, those with high ability may expect better long-term local job prospects and find it worthwhile to search harder for rent-stabilized units to reap higher discounts in the long run. Alternatively, the skills required to search for jobs and rent-stabilized units could be related to unobserved skills and local networks. The two latter arguments suggest a downward bias of the effect from OLS estimation. Therefore, the direction of the bias is ex-ante ambiguous. Estimation that does not control for differences in those unobservables will incorrectly attribute a tenant's unemployment to the treatment variable (rent stabilization).

We address the endogeneity concerns by constructing an instrumental variable (IV) that measures the tightness of the rent-stabilized market at the neighborhood level, that is, the relative availability of rent-stabilized units that are vacant and available for rent in each borough, at the

[8] In fact, this paper documents a new empirical fact about the policy awareness of rent stabilization: not everyone knows about it!

[9] Also see "Public Housing and Public Schools: How Do Students Living in NYC Public Housing Fare in School?" a report from Furman Center of New York University.

time the tenant moved into the unit.[10] The tightness of the rent-stabilized market in each borough depends on many factors (e.g., rents of market-rate units (Early, 2000), prices of owner-occupied units, new units built, etc.) beyond the individual tenant control. Market tightness works like a lottery that changes the probability of getting a rent-stabilized unit for tenants who move in different years to the same neighborhood conditional on their individual characteristics.[11]

We show this market-tightness instrument indeed affects the probability of getting a rent-stabilized unit and provide tests for the conditional independence assumption, showing it does so due to factors exogenous to observable tenant characteristics (Bhuller et al., 2020). The causal interpretation of the rent stabilization obtained from our IV regression depends on satisfying the exclusion restriction. However, the multidimensionality of the treatment, that is, rent stabilization easing access to the neighborhood with a specific set of amenities correlated with the unobservable tenant characteristics, threatens the exclusion restriction. In robustness tests, we estimate measures of the unobserved quality of the neighborhood at the time of moving in following Sieg et al. (2002) and include these measures as controls to eliminate this source of bias. Additionally, we test the monotonicity assumption to ensure the validity of our estimates in the presence of heterogeneous effects. We also conduct placebo tests to show the causal effects are not obtained by chance.

We find rent stabilization increases the unemployment of tenants by approximately six percentage points, more than double the average unemployment rate during the study period. This effect is around half of the effect estimated by OLS. The theoretical model suggests the impact of rent stabilization will be larger for tenants with larger rent discounts, higher expected wage offers, and more risk-averse preferences. We explore these heterogeneous effects by running IV regressions on subsamples divided by observed characteristics: tenants of different tenure duration, because longer tenure is associated with larger rent discounts; tenants of different races and educational levels to test the effect of facing different wage-offer distributions conditional on observed demographics; and households of different ages to explore the impact of increasing risk aversion. We confirm all the model predictions. Although we cannot identify the relative strength of each mechanism, we provide contrast tests between our proposed disincentive mechanism and the mobility and liquidity

[10] Another approach would track workers as they change between rental units of different stabilization status while keeping these unobservable characteristics constant. Without longitudinal data at the tenant level, our identification relies on cross-sectional variation in multiple periods.

[11] This process is similar to how lottery drafts with different military draft-eligibility ceilings would change the probability of American men facing drafting in different years (Angrist et al., 2011; Angrist, 1990).

mechanisms.

Finally, this paper also documents a novel empirical fact about the policy awareness of rent stabilization: only about a third of the tenants in rent-stabilized units are aware of it, despite considerable rent discounts. By contrast, strikingly, one quarter of rent-stabilized tenants claim their units are unregulated.[12] A possible explanation in the literature for this phenomenon is policy opacity (DellaVigna, 2009). Our model suggests forward-looking strategic considerations when accepting or rejecting job offers are key for rent stabilization to have an effect on unemployment, which presupposes policy awareness. Results using the 2002 and 2005 data, for which the policy-awareness information is available, show the impact of rent stabilization on unemployment is concentrated among tenants correctly aware of the benefit.

The rest of the paper proceeds as follows: Section 2 discussed the literature contribution. Section 3 discusses the rent-stabilization policy's institutional details and the theoretical predictions of its effects on tenant unemployment. Section 4 describes the data. Section 5 presents the empirical design and its validity. Section 6 reports the main results about the impact of rent stabilization on unemployment, explores the heterogeneity of this effect and mechanisms, and provides robustness tests to the main results. Finally, section 7 discusses policy implications and concludes.

2 Literature Review

A significant number of studies have evaluated the impact of rent regulation (primarily the first-generation rent control) on housing-related outcomes such as housing prices and rents (Early, 2000; Sims, 2007; Autor et al., 2014), housing supply (Malpezzi and Ball, 1993; Diamond et al., 2019) and misallocation (Glaeser, 1996; Glaeser and Luttmer, 2003; Micheli and Schmidt, 2015; Favilukis et al., 2019), housing quality (Olsen, 1988; Gyourko and Linneman, 1990; Moon and Stotsky, 1993), and gentrification and displacement (Nagy, 1995; Munch and Svarer, 2002; Asquith, 2019; Diamond et al., 2019); for excellent reviews, see Arnott (1995); Turner and Malpezzi (2003); Metcalf (2018); Pastor et al. (2018). More recently, the scope of studies on rent regulation has expanded to broader outcomes beyond the housing market, such as crime Autor et al. (2017).[13] Yet, the effect of

[12] The remaining rent-stabilized tenants either don't know about their rent-regulation status or choose not to report.

[13] Other studies explore the effect of dimensions that are affected by rent regulation, for example, housing stability and affordability, on other outcomes such as educational attainment (Brennan et al., 2014; Newman and Holupka,

rent regulation on tenant employment in the US is largely unknown (Rajasekaran et al., 2019). Our paper contributes to the literature on rent regulation by estimating the causal impact of the rent-stabilization policy on tenant unemployment and by providing a model that rationalizes the channels through which the effect occurs.

To the best of our knowledge, Svarer et al. (2005) is the only paper that also investigates the effect of rent control on unemployment outcomes. They find rent control reduces the probability of accepting a *distant* job offer and increases the likelihood of accepting a *local* one in Denmark. Due to low mobility, the overall effect of rent control is positive on unemployment duration. Three main differences exist between this paper and ours. First, they analyze the Danish housing market, where rent-control policies present significant institutional differences compared with NYC's rent stabilization. In the Danish housing market, almost all rental units are subject to rent control[14] and the rent discount barely changes over time.[15] These institutional details call for their treatment variable to be an estimated continuous rent benefit.[16] By contrast, we measure rent discount as a discrete treatment and test its impact using workers in market-rate units as the control group. Second, their work focuses on the hampered mobility that rent control brings, affecting unemployment by reducing the geographical breadth of desirable job offers. We argue this channel is not the only one present.[17] We introduce the disincentive channel, which is testable with data on a single market. Third, they do not focus on the assignment of rent-controlled units across workers but instead estimate the effect of different rent-control intensities in a sample of rent-controlled households. We discuss the potential endogenous allocation of rent-stabilized units and identify the causal impact with an IV in a sample of renters with and without rent stabilization in the private market.[18] Also, they have access to a longitudinal panel to estimate the hazard rate out

2015) and health (Burgard et al., 2012), but they do not estimate the direct impact of rent regulation.

[14]Rental units are only 18% of all housing units in the Danish housing market in Svarer et al. (2005). Only 1% of all housing units are for rent and not rent controlled (those built since 1991). The remaining 82% of the market is owner occupied, cooperative, and public housing.

[15]Rents are cost based and include a capital charge based on the age of the unit. No allowances for inflation adjustment are permitted, and landlords cannot raise rents except when they make significant improvements.

[16]This benefit is estimated by comparing contract rent and the rent estimated by tax authorities using comparable owner-occupied units. This procedure may introduce a bias in estimating the discount if spillovers occur from the rental market to the owner-occupied market (Häckner and Nyberg, 2000). Furthermore, contract rent is not observed for half of their units and needs to be estimated.

[17]It cannot explain, for instance, why the effect of rent control on unemployment exists where job opportunities are abundant and moving is not required for getting employed.

[18]In results not reported, they include a selection equation into high versus low rent control and their results are unchanged.

of unemployment, track workers' moves across cities, and draw conclusions about unemployment duration. We do not observe a panel at the tenant level but only have repeated cross-section data. Thus, our identifying variation comes from cross-sectional differences over time. We see the papers as complementary, but we differ in the institutional context, the theoretical explanation, and the empirical analysis.

We also contribute to the literature that studies how housing affordability policies broadly affect individual labor market outcomes. We find public housing (Newman et al., 2009; Dujardin and Goffette-Nagot, 2009; Monkkonen, 2011), housing vouchers (Newman et al., 2009; Carlson et al., 2011), or both (Susin, 2005; Olsen et al., 2005; Wood et al., 2005), among the housing policies studied by this strand of literature.[19] Most existing studies find these housing policies can reduce workers' earnings and increase unemployment.[20] These studies highlight as mechanisms the mobility restrictions, possible stigma, and the incentives to keep income low to qualify for public housing and vouchers (Moffitt, 2014).[21] We contribute to this literature by estimating the effect on tenant unemployment of rent stabilization, another housing affordability program that differs fundamentally from other assisted-housing programs in that it is not means tested. We find rent stabilization reduces unemployment, suggesting the similar disincentive effect can apply beyond means-tested programs.

The theoretical model in our paper builds on the classic job-search models (McCall, 1970; Rogerson et al., 2005).[22] Those models point out that UI increases disposable income when the worker is unemployed, and therefore can affect unemployment by increasing the value of unemployment relative to employment (disincentive or moral-hazard effect).[23] Our model is different in the sense that the rent discount increases unemployment and employment disposable income equally. We point out that the rent discount can still increase the value of unemployment relative to employ-

[19] Related studies focus on the impact of these housing-affordability policies on labor supply (Carlson et al., 2011; Jacob and Ludwig, 2012).

[20] Dujardin and Goffette-Nagot (2009) and Newman et al. (2009) are notable exceptions that find the labor market effects of low-income housing programs are negligible.

[21] See Olsen and Zabel (2014) and Collinson et al. (2015) for comprehensive discussions on how low-income housing policies affect associated outcomes such as housing consumption, health, and child outcomes.

[22] Based on this theoretical job-search model, many studies have estimated the effect of UI on unemployment and its duration (Moffitt, 1985; Katz and Meyer, 1990; Meyer, 1990; Lalive et al., 2006; Card et al., 2007a; Lalive, 2008; Krueger and Mueller, 2010; Schmieder et al., 2012; Card et al., 2015; Landais, 2015; Le Barbanchon et al., 2019) as well as post-unemployment results (Card et al., 2007b; Lalive, 2007; Van Ours and Vodopivec, 2008; Lalive et al., 2015; Schmieder et al., 2016; Nekoei and Weber, 2017; Schmieder and Trenkle, 2020).

[23] As argued in Chetty (2008), UI can also affect unemployment through relaxing the liquidity constraint.

ment and induce the disincentive effect on job search as long as the marginal utility of consumption is diminishing.

Finally, we contribute to the literature on policy awareness and its implications. Policy opacity often explains poor policy knowledge or understanding. Policy opacity is usually related to decisions that are rarely taken, such as housing changes (DellaVigna, 2009), with limited scope for feedback and learning. It has also been associated with the complexities of contracts related to policies and their informational burden (Ulen, 2021; Hirshleifer et al., 2009) and how obscurely they are presented to potential beneficiaries (Chetty et al., 2009).[24] Despite its apparent simplicity and seeming fairness, rent-stabilization details are indeed hidden as supplements long leasing contracts signed by landlords with little incentives to advertise these benefits in a context of low vacancies. We contribute to this literature by showing how policy awareness can be low even if no extraneous application processes are required. We document that only about a third of the tenants in rent-stabilized units are aware of it. We find the effect of rent stabilization on unemployment is concentrated among tenants who are aware of the benefit.

3 Rent Stabilization and Unemployment

3.1 The Rent-Stabilization Policy in NYC

This paper focuses on rent stabilization in NYC, which began in 1969 and protects tenants in two major ways: (1) Tenants are protected from sharp rent increases; and (2) tenants have the right to renew their leases and be protected against arbitrary evictions.[25] Rent stabilization is the most popular form of regulation in the city. In 2017, of the 2 million rental-occupied units in NYC, almost 1 million were rent-stabilized apartments, while only about 22,000 were *hard price* rent-controlled (HPRC) apartments[26] and about 180,000 public housing units. On average, 50% of all

[24] These complexities reduce knowledge and participation of policy programs (Daponte et al., 1999; Duflo et al., 2006; Dynarski and Scott-Clayton, 2008; Chetty et al., 2013), especially among low-income households who are informationally overloaded (Horrigan, 2016).

[25] Another associated benefit is succession, which gives the right to a tenant's relatives to take over the lease without post-vacancy rent adjustments after a short cohabitation period.

[26] For an apartment to be under HPRC, a tenant must generally have been living there continuously since before July 1, 1971. HPRC limits the rent an owner may charge for an apartment and restricts the right of an owner to evict tenants (Arnott, 1995). HPRC units are mostly occupied by an elderly population. A brief comparison between these two types of rent regulation—rent stabilization and HPRC—is summarized in Appendix Table B1.

rental units are rent stabilized. This share is fairly constant across boroughs (see Appendix Table B2) except for Staten Island.

Eligibility. In NYC, rent stabilization applies to apartments in buildings (1) with six or more units built between February 1, 1947, and January 1, 1974; (2) with six or more units built before February 1, 1947, and with a tenant who moved in after June 30, 1971; and (3) with three or more units constructed or extensively renovated since 1974 with special tax benefits, such as J-51, 421a, or other programs. The third category only applies to the period when tax abatement is effective, which usually lasts 10-20 years. These multidimensional criteria are shown in Figure 1, wherein the shadowed areas represent rent-stabilized units.

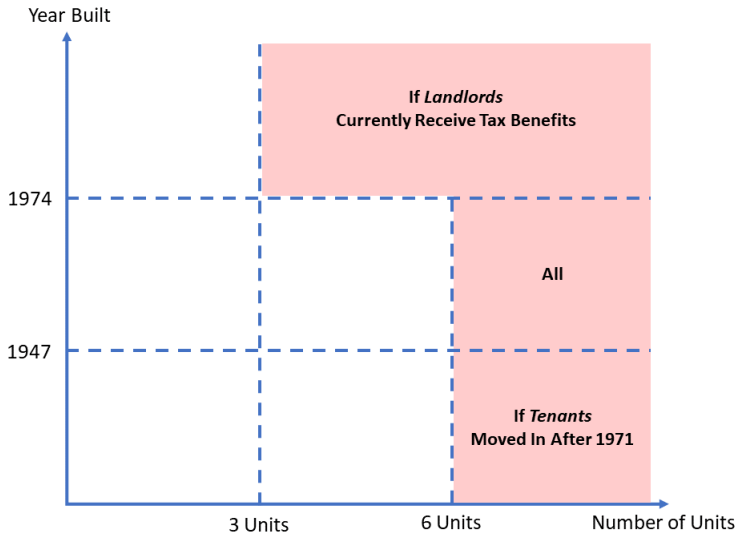
Rent stabilization differs in critical ways from how other assisted housing programs are allocated.[27] Rent regulation is not a means-tested welfare program targeted at the low-income population. As a result, rent stabilization does not apply only to low-income tenants. Most assisted housing programs have income eligibility rules. Also, assisted housing programs commonly set rent payments from participants relative to their income, usually about 30% of their adjusted income. Rents of rent-stabilized units bear no connection to tenants' income. Furthermore, rent-stabilized units do not require application by beneficiaries.

Rent Determination. The annual rent increases of rent-stabilized units are capped and adjusted by the Rent Guidelines Board (RGB). For example, between October 1, 2019, and September 30, 2020, a maximum 1.5% increase was allowed for a one-year lease. Appendix Table B3 shows the allowed stabilized rent growth rates since 1979. The RGB selects the growth cap considering housing and financing market conditions, owner costs and revenues, and rental vacancy rates, among other factors. Greater, but still capped, rent increases are allowed (1) if significant capital improvements are undertaken in the unit or building and (2) when turnover of tenants occurs.[28]

[27] Regarding U.S. federal assisted housing programs and the related policy discussion, see Green and Malpezzi (2003); Olsen (2003); Gyourko and Glaeser (2008); Collinson et al. (2015) for excellent reviews.

[28] In a few cases, a rent-stabilized unit is returned to the private market or is deregulated. This scenario happens when the rent reaches the deregulation rent threshold (DRT) (in 2019, e.g., the DRT was $2,774.76) and simultaneously meets one of the following two conditions: (1) a vacancy exists or (2) the household's income is above the deregulation income threshold (DIT), which has been $200,000 since 2011. A rent-stabilized unit may also be deregulated upon vacancy during a co-op or condo conversion. Lastly, deregulation may occur when tax abatements or exemptions (i.e., J-51 or 421a tax benefits) that the owner received expire. Rental units with active tax benefits are stabilized, regardless of whether the DRT has been exceeded. Deregulation is done at the unit level and could apply to some units in a building independently of the status of the other unit. This measure helps satisfy the stable unit treatment value assumption (SUTVA) that states that the potential outcomes for each tenant are unaffected by the particular assignment of treatment to the other units.

Figure 1: Eligibility of Rent Stabilization



Notes: Authors' own summary based on legal regulation documents from the NYC Rent Guidelines Board. Rent stabilization in NYC generally applies to apartments in buildings: (a) six or more units built between February 1, 1947, and January 1, 1974; (b) six or more units built before February 1, 1947, and tenants who moved in after June 30, 1971; and (c) three or more apartments constructed or extensively renovated since 1974 with special tax benefits such as J-51, 421a, or other programs. This measure only applies to the period when tax abatement is effective, which usually lasts 10-20 years. This figure only considers rent-stabilized units that are not subject to deregulation.

The policy is binding in practice when the permitted growth rate is lower than the observed rental growth in private markets, which has been common since 1979. In this context, rents in private and stabilized markets diverge (see Appendix Figure E8).[29]

Rent Discount. Whether rent stabilization actually implies a price discount is an empirical question. When the regulated price is below the market equilibrium price, landlords may decide to recover some of the foregone income by reducing housing quality in rent-regulated properties, for example, by refraining from performing maintenance (Diamond et al., 2019; Moon and Stotsky, 1993). If price is proportional to quality, a landlord may be fully compensated for the wedge between the equilibrium price and regulated price by sufficiently reducing housing quality. In such a scenario, tenants living in rent-stabilized units may not benefit from the policy. In a recent study, Chen et al. (2022) find that rent-stabilized units do have lower quality-adjusted rents and the rent discount increases with time in residence (in the last two decades in NYC). On average, the monthly

[29]The cap for the latest two periods in the available NYCHVS data, 2014-2017, is 4% from October 2013 to September 2014, 1% from October 2013 to September 2015, and 0% afterward. In this period, data show a growth of private market rents of 7% in the mean and 8.2% in the median.

quality-adjusted contract rent is $370 lower for a newly moved-in tenant, and it declines $20 more every year between 2002 and 2017.[30]

3.2 Theoretical Discussion

This section provides theoretical intuition on how the rent-stabilization policy affects job-search behaviors and unemployment outcomes. We also discuss potential factors that influence the effects of rent stabilization.

3.2.1 Setup

We consider a job-search model in the spirit of McCall (1970), extending to incorporate rental costs. Suppose a worker has to pay instantaneous rent r each period. For simplicity, normalize the rent for private market renters to be zero. We have

$$r = \begin{cases} r_m = 0 \\ r_s = -r_d, \end{cases} \tag{1}$$

where $r_d >= 0$ is the rent discount that captures the fact that rent is lower for a stabilized renter (with subscript s) than for a private market renter (with subscript m), as discussed in the earlier section 3.1 (also see Appendix Figure E8). We assume the rent discount is constant over time.[31]

All workers participate in the labor force: they are either employed or unemployed. Unemployed workers receive UI benefits, b. Cash flows are discounted by ρ. No on-the-job search occurs. Jobs are assumed to last forever, as in the standard job-search model. NYC has one labor market. The labor market prospects do not depend on workers' rent-regulation status. Thus, the arrival rate for

[30] Chen et al. (2022) investigates the value of rent stabilization as the price difference between the actual contract rent and the predicted counterfactual market rent for rent-stabilized units, using a hedonic pricing model. They also exploit the longitudinal data structure at the dwelling level for 2002-2008 NYCHVS and the variation in rent-stabilization status, which shows robust results.

[31] Our main model predictions still hold when one assumes growing rent discounts instead of constant discounts. These results are available upon request. We assume constant discounts instead of growing rent discounts for two reasons, admitting that growing rent discounts may be more consistent with the rent-stabilization policy in NYC. First, a constant discount leads to a stationary model that provides intuition and allows for constructive proofs of the propositions that link the effect of rent discount on unemployment. Instead, a growing rent discount leads to a non-stationary model where the age effect can confound the unemployment effect of a rent discount. Second, the assumption of a constant rent discount is consistent with models of rent-control policies studied in the literature. For example, Basu and Emerson (2000) and Basu and Emerson (2003) assume a constant rent discount and study the impact of a rent discount or rent-control policies on housing market equilibrium. Our model instead focuses on the effects of rent-control policies on the unemployment of tenants.

job offers is α and wage offers w are generated from a same exogenous wage-offer distribution $F(w)$ for both private market and rent-stabilized tenants. In this setup, the worker has only one choice variable in the period of unemployment: conditional on receiving a job offer, the worker decides whether to accept it. The utility function $u(\cdot)$ is a concave function of disposable income reflecting the preferences of a risk-averse worker.

3.2.2 Effect of Rent Stabilization on Unemployment

As derived in Appendix A, this decision is determined by a reservation wage expressed in the following equation:

$$\underbrace{u(w^* + r_d) - u(b + r_d)}_{\text{cost of rejecting a job offer}} = \underbrace{\alpha \frac{(1-\rho)}{\rho} \int_{w^*}^{\bar{w}} [u(w + r_d) - u(w^* + r_d)] dF(w)}_{\text{expected discounted benefit of rejecting a job offer}}, \tag{2}$$

where the left-hand side of equation (2) captures the cost of rejecting a wage offer w^* to continue searching, and the right-hand side of equation (2) captures the expected discounted benefit of rejecting a wage offer to continue searching. Based on equation (2), a rent discount can clearly reduce the cost and benefit from waiting for a new job. Following this intuition, in Lemma 1 below, we demonstrate the effects of rent discount r_d on the job search is theoretically ambiguous.[32]

Lemma 1. *If the utility function $u(\cdot)$ is concave (the worker is risk averse), the effect of rent discount r_d on the reservation wage depends on the relative magnitude of $u'(b+r_d) - u'(w^*(r_d)+r_d)$ and $\int_{w^*}^{\bar{w}} [u'(w + r_d) - u'(w^* + r_d)] dF(w)$. That is,*

$$\frac{\partial w^*(r_d)}{\partial r_d} \begin{cases} \geq 0, & if \alpha \frac{(1-\rho)}{\rho} \int_{w^*}^{\bar{w}} [u'(w + r_d) - u'(w^* + r_d)] dF(w) \geq [u'(w^* + r_d) - u'(b + r_d)] \\ < 0, & if \alpha \frac{(1-\rho)}{\rho} \int_{w^*}^{\bar{w}} [u'(w + r_d) - u'(w^* + r_d)] dF(w) < [u'(w^* + r_d) - u'(b + r_d)]. \end{cases} \tag{3}$$

Proof: See Appendix A.

Though Lemma 1 demonstrates the effect of rent discount r_d on the reservation wage is am-

[32] This result highlights a unique feature of rent stabilization that does not depend on tenants' employment status. Unemployment insurance, by contrast, is available only when the worker is unemployed and the theoretical prediction of its impact is straightforward, resulting in higher reservation wages and longer unemployment duration (McCall, 1970).

biguous, we can still derive useful predictions by making reasonable assumptions about the utility function. Because the marginal utility of the rent discount diminishes with disposable income, the effect of the rent discount becomes smaller when the worker has a higher wage. Thus, we can reasonably assume the effect of the rent discount is only evident or non-negligible when the worker is unemployed, so we have

$$u'(w^* + r_d) - u'(b + r_d) < 0, \quad \text{and} \quad u'(w + r_d) - u'(w^* + r_d) = 0.$$

In this case, the rent discount affects only the cost and not the benefit of turning down a job offer, and its impact on the job search becomes similar to the effect of UI. That is, an increase in r_d will increase the reservation wage. We present the impact of the rent discount on the reservation wage in Lemma 2 and the following propositions.[33]

Lemma 2. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w+r_d)-u'(w^*+r_d) = 0$, the rent discount r_d increases the reservation wage; that is,*

$$\frac{\partial w^*(r_d)}{\partial r_d} > 0.$$

Proof: See Appendix A.

Lemma 2 demonstrates that when the worker is risk averse and the effect of the rent discount on the utility is negligible when the worker is employed, the rent discount increases the reservation wage. Using Lemma 2, the following propositions predict how living in a rent-stabilized unit affects job-search outcomes.

Proposition 3. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w + r_d) - u'(w^* + r_d) = 0$, a rent-stabilized tenant has a higher reservation wage and is less likely to exit unemployment relative to a private market tenant.*

Proposition 3 suggests rent-stabilized renters have higher reservation wages and are more likely to stay unemployed than private market renters, which motivates our empirical analysis of the unemployment effect of rent stabilization.

[33] In Appendix A, we explore how the size of the rent discount, the wage distribution, and risk-aversion parameters affect the unemployment effect, with numerical simulations based on a more general model in which the assumption $u'(w+r_d)-u'(w^*+r_d) = 0$ is relaxed. Simulation results are consistent with the model predictions presented in this section.

3.2.3 Factors that Affect the Unemployment Effect

In this section, we demonstrate that the unemployment effect of rent stabilization depends on the size of the rent discount, wage distribution, and workers' risk aversion. We describe the three factors and provide testable implications for our empirical analyses.

Size of Rent Discount. So far, we have only discussed the model implications on the extensive margin of rent stabilization ($r_d > 0$) relative to private rental markets ($r_d = 0$). Proposition 1 also shows the intensive margin, a larger rent discount, is associated with greater unemployment effects. However, directly quantifying this intensive margin is challenging.[34] Considering that the rent discount tends to increase the longer someone stays in a unit, we use rent tenure to proxy for rent discount. Thus, we test the intensive margin by comparing the unemployment effect among workers who recently moved to those with longer tenures in the unit. We find the model predictions are supported, namely, that higher predicted discounts among tenants with longer tenure are associated with greater unemployment effects.

Wage Distribution. When workers expect to have a higher wage with a larger variation (the wage distribution is skewed to the left or shifted to the right), they have a higher potential return of turning down a job offer, and thus are more likely to respond to rent discount. In Proposition 4, we use the $\bar{w}$ to measure the expectation of the wage distribution, and it shows the rent-discount effect increases with $\bar{w}$.

Proposition 4. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w + r_d) - u'(w^* + r_d) = 0$, the rent-discount effect is greater when workers expect higher wages in the future. That is,*

$$\frac{\frac{\partial w^*(r_d)}{\partial r_d}}{\bar{w}} > 0.$$

Proof: See Appendix A.

We test the above prediction by comparing the unemployment effects between college and non-college workers, because college workers are more likely to have a higher $\bar{w}$. Appendix Figure E9 confirms the differences in wage distributions between college and non-college workers. Indeed, we

[34] Munch and Svarer (2002) calculate rent-discount magnitudes by comparing contract rents with the estimated unit value appraised by tax authorities and then calculating the rent implied by a calculated user cost. Chen et al. (2022) calculate the distribution of rent-discount magnitudes implied by rent stabilization using hedonic models.

find the unemployment effect is larger among college workers, consistent with our model prediction. Additionally, we test the above prediction by comparing the unemployment effects among white and non-white workers. Considering that white workers are more likely to have higher wages (Card and Lemieux, 1994; Heckman et al., 2000), our empirical results confirming that effects are larger for white workers are consistent with our model prediction.

Risk Aversion. So far, we assume workers are risk averse with a concave utility function. Here, we show workers' attitudes toward risk are essential to the unemployment effect of rent discounts. In particular, in the following Lemma 5, we show that when the worker is risk neutral with a linear utility function, the rent discount r_d has *no* impact on the job search. The intuition behind this finding is straightforward. When the worker is risk neutral, the rent discount does not affect the costs or benefits of turning down a wage offer, leaving the reservation wage unchanged.

Proposition 5. *If the utility function $u(\cdot)$ is linear (the worker is risk neutral), the rent discount r_d has no impact on the job search; that is,*

$$\frac{\partial w^*(r_d)}{\partial r_d} = 0.$$

Proof: See Appendix A.

Though we do not have direct measurements of risk aversion, older workers are known to be generally more risk averse than younger workers (Barsky et al., 1997; Albert and Duffy, 2012; Sahm, 2012; Dohmen et al., 2017). Thus, we test the importance of risk aversion by exploring the heterogeneity of unemployment effects among older and younger tenants in our empirical section. Consistent with our prediction here, we find the unemployment effect is significant and larger among older workers and insignificant among younger workers.

4 Data

In this paper, we take advantage of an under-explored micro dataset, the NYCHVS. The NYCHVS is designed and implemented by the U.S. Census Bureau to comply with New York State and NYC's rent-regulation laws. The rent-regulation status of each housing unit in this survey is verified

through administrative sources.[35] This verification makes the NYCHVS a uniquely accurate tool to measure the rent-stabilization policy and evaluate its impacts on tenant outcomes.

The NYCHVS has a high interview rate of 98% and is conducted approximately every three years. We use all the available waves of NYCHVS data since 2000 for the empirical analysis. Specifically, we use the 2002, 2005, 2008, 2011, and 2017 waves.[36] Each wave of the NYCHVS contains approximately 18,000 units, between occupied ones (either rental-occupied or owner-occupied) and vacant ones.[37] For both occupied and vacant units, the survey includes accurate rent-regulation status and detailed housing characteristics. For occupied units, the survey includes comprehensive information about household characteristics.

In addition, we also use almost four decades of the NYCHVS data beginning from 1978 and exploit its vacancy module to construct IVs for causal identification. The vacant module contains detailed information about vacancy status, rent-regulation status among vacant units, and the reasons units are vacant. Details about how the vacant module is used for the instrument's construction are discussed in section 5.2.

The Analytic Sample. To construct the analytic sample for empirical analysis, we focus on renters who live in either rent-stabilized or private market-rate units. We restrict our analytic sample to be working-age tenants between ages 26 and 54 who are not benefiting from any housing subsidies and whose non-labor income is not too high (less than $100,000). We also exclude households who have lived in their current units for less than one year to alleviate simultaneity bias concerns. And we exclude tenants who moved into their current housing units before 1978, due to data limitations.[38] Detailed descriptions of the step-by-step sample construction are documented in Appendix C.1. Our final analytic sample contains 24,210 householders, with 21,906 in the labor force.

[35] As elaborated in section 6.3.3, the distinction between the legal regulation status and self-reported regulation status shows many households are not aware of being beneficiaries of this policy.

[36] Previous studies using NYCHVS have almost exclusively focused on housing market outcomes. The earliest NYCHVS used in the literature was the 1968 wave (Olsen, 1972; Gyourko and Linneman, 1989, 1990; Ault et al., 1994) in which researchers focused on the role of rent control (instead of rent stabilization) on rents. Other issues, such as tenant turnover, have been analyzed in later waves. For example, Linneman (1987); Moon and Stotsky (1993); Nagy (1995, 1997) use 1978-1987 and Sieg and Yoon (2020) uses the 2011 NYCHVS. We exclude the 2014 from our analysis because the variable that captures the year the tenant moved into the unit is not coded consistently with the other waves. This variable is crucial to our identification strategy.

[37] Each sample unit represents around 180 similar housing units.

[38] The earliest NYCHVS survey available was carried out in 1978. Because our IVs are constructed using vacancy rates and rent-regulation information at the move-in time, 1978 is the earliest move-in year that satisfies our research design.

Descriptive Statistics. Table 1 shows descriptive statistics of the analytic sample used in our empirical analysis.[39] Panel A shows rent-stabilized tenants have a higher unemployment rate (5.2%) than private market-rate tenants (4.1%). The demographic differences between tenants in rent-stabilized and market-rate units from Panel A play an important role in the analysis of the causal impact of rent stabilization on unemployment. We analyze these differences in section 5.1 in the context of the endogeneity threat to our estimation. Housing characteristics are shown in Panel B of Table 1. As expected, rents of rent-stabilized units are smaller than rents in the private market. They pay lower rents, with an average monthly difference of $465. They are also located in neighborhood with lower reported quality by tenants. Tenure of residence is longer in rent-stabilized units, with tenants staying in their units more than two years longer.

5 Research Design

In this section, we describe our research design. We begin by reviewing how rent-stabilized units are allocated among potential tenants, which is informative for understanding the sources of endogeneity concerns and the directions of potential biases. We then describe how we use the relative vacancy availability of rent-stabilized units in the rental market to estimate the effect of rent stabilization on participants' unemployment.

5.1 Allocation of Rent-Stabilized Units

Sorting into rent-stabilized units is difficult, making it less of a problem for empirical analysis than other welfare or assisted housing programs. As discussed in section 3.1, rent stabilization is not a means-tested program directed only at low-income tenants, and it does not require application by beneficiaries. Many tenants obtain their rent-stabilized units through good fortune and may not even know it. If an apartment is rent stabilized, landlords should attach to the lease the rent stabilization "lease rider." However, how strictly such requirements are disclosed is unclear, because they happen near the end of the leasing process. One of the novel empirical findings of this paper is that many rent-stabilized tenants are indeed not aware of their good fortune. Among all tenants

[39] More detailed summary statistics containing standard deviation, minimum, and maximum are available in Table C5 and Table C6, respectively.

Table 1: Summary Statistics by Rent Stabilization

	Rent Stabilized		Market Rent		
	Mean	N	Mean	N	Difference
	Panel A. Household Characteristics				
Currently Unemployed	0.052	11555	0.041	10351	0.012***
Female	0.479	11555	0.431	10351	0.047***
Age	38.965	11555	37.864	10351	1.100***
Black	0.221	11555	0.203	10351	0.017***
Hispanic	0.313	11555	0.209	10351	0.104***
Asian	0.111	11555	0.137	10351	-0.025***
Married	0.349	11555	0.406	10351	-0.057***
High School Dropout	0.137	11555	0.102	10351	0.034***
High School Graduate	0.221	11555	0.208	10351	0.013**
Non-labor Income	0.260	11555	0.299	10351	-0.039***
Spouse: Black	0.176	4033	0.161	4199	0.016*
Spouse: Hispanic	0.363	4033	0.243	4199	0.120***
Spouse: Asian	0.180	4033	0.197	4199	-0.018**
Spouse: High School Dropout	0.188	4033	0.137	4199	0.051***
Spouse: High School Graduates	0.287	4033	0.269	4199	0.017*
Spouse: Unemployed	0.045	4033	0.031	4199	0.014***
Any Child in Household	0.345	11555	0.368	10351	-0.023***
Household Size	2.375	11555	2.556	10351	-0.181***
Other Household Member Total Income	2.171	11555	3.182	10351	-1.011***
	Panel B. Housing Characteristics				
Monthly Contract Rent	1.286	11555	1.753	10351	-0.467***
Monthly Gross Rent	1.395	11555	1.918	10351	-0.524***
Tenure of Residence	7.060	11555	4.813	10351	2.246***
Num. of Rooms	3.158	11555	3.740	10351	-0.582***
Num. of Bedrooms	1.337	11555	1.763	10351	-0.427***
Number of Unit Problems	0.655	11555	0.331	10351	0.324***
Number of Building Problems	0.112	11555	0.086	10351	0.027***
Neighborhood Rate: Fair	0.211	11555	0.118	10351	0.093***
Neighborhood Rate: Poor	0.039	11555	0.018	10351	0.021***

Notes: Data come from the 2002, 2005, 2008, 2011, 2014, and 2017 waves of the NYCHVS. All income-related variables are in 2017 real dollar and are in $10,000. All rent-related variables are in 2017 real dollar and are in $1,000. Unit problems include the presence of rodents, water leakage, broken plaster or peeling paint, plumbing, and kitchen issues. Building problems include issues with external walls, building windows, and stairways. * p<0.10, ** p<0.05, *** p<0.010.

occupying rent-stabilized units, more than 60% cannot identify that they are in a rent-regulated unit (see Table 2).

Table 2: Legal Status versus Self-reported Regulation Status

Self-Reported Status	Legal Status		
	Unregulated	Rent-stabilized	Total
Rent-controlled	70	474	544
	1.79%	8.60%	5.77%
Rent-stabilized	136	1409	1545
	3.48%	25.55%	16.40%
Unregulated	2317	1338	3655
	59.29%	24.27%	38.79%
Don't Know	621	1198	1819
	15.89%	21.73%	19.31%
Not Reported	764	1095	1859
	19.55%	19.86%	19.73%
Total	3908	5514	9422
	100.00%	100.00%	100.00%

Notes: Data come from the pooled 2002 and 2005 waves of the NYCHVS. The self-reported rent-regulation status is not available in wave 2008 and onward.

Obscurity permeates the process of leasing a rent-stabilized unit, which is in the interest of landlords, who, in a market with low vacancy rates, such as NYC, do not have incentives to advertise this status, precisely to discourage sorting of tenants. Despite being around half of the rental units, rent-stabilized units are notoriously difficult to target in searches. For example, less than 3% of online postings (see Appendix Figure B7) contain information on rent stabilization, although historical NYCHVS data suggest at least 25% of vacant-for-rent units are rent stabilized. Furthermore, although a building may include rent-stabilized units, not all units in the same building have the same rent-stabilization status, making its identification more difficult.

These institutional characteristics imply performing targeted searches for rent-stabilized units is difficult. Furthermore, the recipients of the benefits do not face any incentives to reduce their income or to change their employment status as a strategy to get a rent-stabilized unit. This reduces somewhat the endogeneity concerns coming from correlation between treatment and labor market

prospects. Nevertheless, we address the potential endogeneity issue using the relative availability of rent-stabilized units as an instrument in later sections.

Who Lives in Rent-Stabilized Units? We first investigate whether rent-stabilized tenants have different socioeconomic and demographic characteristics than tenants living in private market-rate units. Then, we perform regression analyses in which the outcome variable is the binary indicator for rent-stabilization status. Differences in means are shown in Table 1 and regression results are presented in columns 1 and 2 of Table 3. Although most individual characteristics are significantly associated with rent-stabilized status, significance in correlations is mainly coming from very small standard errors, likely due to a large sample size. Point estimates, by contrast, are small, especially those related to critical factors affecting unemployment, such as age, education, and income. For example, the statistically significant age difference is only one year. The difference in the share of high school graduates is only one percentage point. The difference in non-labor income is only $ 400 annually. This pattern is distinct from other assisted housing programs, such as public housing or housing voucher programs, in which tenants with lower education and income are most likely to participate. The only distinct factors are gender and family size.[40] Once location unobserved characteristics are controlled for with sub-borough and year fixed effects, some coefficients lose significance, suggesting treated and control populations should be more comparable once we control for them in our regressions.

Tenants in rent-stabilized units seem to be comparable to tenants in private market-rate units in many respects. We have also highlighted that selecting into these units is, in practice, very difficult. Yet, one may still be concerned with other sources of endogeneity that are unobservable to researchers but affect tenant job search and unemployment outcome directly, that is, beyond variables included in Table 3.

One such concern is the canonical skill bias in the labor market.[41] The direction of this bias is not clear ex ante. For instance, given that rent-stabilized units have lower rents on average, tenants with lower unobserved skills may have stronger incentives to search for cheaper housing units. Such tenants with lower unobserved skills may also be more disconnected with the labor

[40]Occupation and industry indicators are not available in wave 2017. We conduct robustness check using pre 2017-waves and include occupation and industry as control variables. Our main results on the unemployment effect of rent stabilization remain the same. Those results are available upon request

[41]For example, the omitted-variable bias due to unobservable skills (or abilities) is extensively discussed in the wage return to schooling literature, which is summarized in Card (2001), among others.

market, and therefore stay unemployed. OLS will overestimate the impact if rent stabilization is negatively associated with being unemployed. On the other hand, given that rent-stabilized units are challenging to find, tenants with higher unobserved skills may more easily locate such units. For example, such tenants may be good at online searching or have a strong local network, which may also help with job search and improve other labor market outcomes. In this case, OLS may be underestimating the effect of the policy.

Reverse causation will be another potential concern if tenants choose their units mainly based on their job prospects. However, we analyze data on reported reasons for moving and conclude most moves are not related to job-related reasons, using the classification in Newman and Wyly (2006) (see Appendix Table D7). Only around 20% of households moved to their units for employment-related reasons, and this number is indistinguishable when comparing workers moving into rent-stabilized and market-rate units. This finding, together with the fact that within NYC, most jobs should be accessible from all locations, assuages this specific issue somewhat.

To address these endogeneity concerns, we develop a market-tightness measure of rent-stabilized units being vacant for rent as an instrument and discuss its validity in the next section.

5.2 IV Model

We are interested in the causal effects of rent stabilization on a tenant's unemployment status. This effect can be captured by the regression model in equation 4

$$Y_{it} = \beta D_{it} + X_{it}\theta' + U_s + W_t + \epsilon_{it}, \tag{4}$$

where i indexes households in units in a time period,[42] s indexes the sub-borough, and t indexes survey years. Y_{it} is the outcome variable of interest, that is, unemployed or not. D_{it} is the key variable of interest, which equals 1 when the unit is rent stabilized and equals 0 when the unit is private market rate. The estimate of interest is β. X_{it} is a vector of individual, household, housing, and neighborhood characteristics.[43] Particularly, we control for the total number of units in the

[42] Recall that our data are repeated cross-sectional. We cannot identify households separately from units or follow the same households over time, so i indices observation of a household in a unit in a time period.

[43] Demographic and socioeconomic characteristics are gender, ethnicity, marital status, educational attainment, and the household's non-labor income, whether any child is present, total family size, and total income of all the other family members. If applicable, the spouse's ethnicity, educational attainment, and labor market status are also

Table 3: Testing For Random Assignment of the Instrumental Variable

	Rent Stabilized				Market Tightness IV			
	(1)		(2)		(3)		(4)	
Female	0.028***	(0.004)	0.013**	(0.004)	0.004	(0.003)	0.004	(0.003)
Age	0.039	(0.027)	0.014	(0.028)	0.009	(0.008)	0.009	(0.008)
Black	0.025	(0.012)	0.011	(0.008)	0.003	(0.003)	0.002	(0.002)
Hispanic	0.090***	(0.013)	0.039*	(0.018)	0.008	(0.004)	0.007	(0.003)
Asian	0.003	(0.032)	0.023	(0.017)	-0.008	(0.004)	-0.006	(0.003)
Married	-0.028	(0.029)	-0.023	(0.013)	-0.004	(0.002)	-0.004	(0.002)
High School Dropout	0.018***	(0.003)	0.001	(0.004)	0.001	(0.002)	0.001	(0.002)
High School Graduate	0.021*	(0.007)	0.007	(0.004)	0.002	(0.001)	0.002	(0.001)
Non-labor Income	-0.003	(0.003)	0.000	(0.002)	0.001	(0.000)	0.000	(0.000)
Spouse: Black	0.041	(0.022)	0.011	(0.015)	0.005	(0.003)	0.005	(0.003)
Spouse: Hispanic	0.026	(0.035)	0.019	(0.018)	0.001	(0.002)	0.001	(0.002)
Spouse: Asian	0.037	(0.047)	0.010	(0.010)	0.002	(0.003)	0.003	(0.004)
Spouse: High School Dropout	0.022	(0.025)	0.008	(0.010)	0.002	(0.003)	0.001	(0.002)
Spouse: High School Graduates	0.013	(0.016)	0.012	(0.008)	-0.000	(0.002)	-0.001	(0.002)
Spouse: Unemployed	0.049*	(0.022)	0.002	(0.018)	0.004	(0.006)	0.002	(0.005)
Any Child in Household	0.009	(0.010)	0.006	(0.004)	0.001	(0.001)	0.000	(0.001)
Household Size	-0.033**	(0.008)	0.003	(0.003)	0.001	(0.001)	0.001	(0.001)
Other Household Member Total Income	-0.002	(0.001)	-0.001	(0.001)	-0.000	(0.000)	-0.000	(0.000)
Neighborhood Rate: Fair	0.108***	(0.010)	0.012*	(0.005)	0.006	(0.004)	0.003	(0.002)
Neighborhood Rate: Poor	0.110**	(0.028)	-0.008	(0.013)	0.005	(0.004)	-0.000	(0.003)
Observations	21906		21906		21906		21906	
R^2	0.189		0.635		0.437		0.455	
Sub-borough and Year Fixed Effects	Yes		Yes		Yes		Yes	
Housing Controls	No		Yes		No		Yes	

Note: The data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. Columns (1) and (2) each adopt a linear probability model (OLS) in which the outcome variable is whether a housing unit is rent stabilized or not. Results are robust with probit or logit models and are available upon request. Columns (3) and (4) each adopt a linear OLS model in which the outcome variable is the IV. The F-statistics range from 22 to 28 in the second stage of IV estimation. Each column has sub-borough and survey-year fixed effects. Housing controls include when a building was built, numbers of units and stories, numbers of rooms and bedrooms, whether the building owner lives in the building, lease types, fuel and heating types, existence of housing issues such as rats, water leakage, broken plaster or peeling paint, plumbing and kitchen issues, external wall, window, and stair issues. Standard errors are in parentheses and clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

building and the year of construction as those variables affect the rent-stabilization eligibility of the unit. We pool all the survey waves (2002, 2005, 2008, 2011, and 2017) and include a year fixed effect W_t. U_s is a sub-borough fixed effect. Last, the sub-borough and year fixed effects are included in all model specifications to control for unobservable local shocks. The underlying identifying assumption here is that, conditional on regressors X_{it}, U_s and W_t, rent stabilization is randomly assigned.

Our research design addresses the endogeneity-bias concern from unbalanced observable characteristics (Table 1) and other sources of endogeneity discussed in section 5.1 by exploiting the fact that local availability of rent-stabilized units at the time of moving in depends on factors exogenous to the tenants.[44] These factors lead to random variation in the probability that an individual will be in a rent-stabilized unit. We use this probability as our instrument, exploiting its variation to infer the causal effects of rent stabilization on tenant unemployment.

We measure local availability of rent-stabilized units as the ratio of the total number of vacant-for-rent units that are rent stabilized, $VR^{stab}_{b,n-1}$, and the total number of vacant-for-rent units, $VR^{all}_{b,n-1}$:

$$Z_{b,n} = \frac{VR^{stab}_{b,n-1}}{VR^{all}_{b,n-1}}, \tag{5}$$

where b indexes different boroughs (Bronx, Brooklyn, Manhattan, Queens, and Staten Island) and $n-1$ indexes the year before tenant i moves into the current dwelling. Notice the instrument value will vary across households in the same borough if they move into their units at different times.[45]

To construct this ratio, we need to know the number of total vacant units that are available for rent the year before the tenant moved in. This information is accurately measured by the NYCHVS,

included among the controls. Housing and neighborhood controls including when a building was built, numbers of units and stories, numbers of rooms and bedrooms, whether the building owner lives in the building, lease types, fuel and heating types, existence of housing issues such as rats, water leakage, broken plaster or peeling paint, plumbing and kitchen issues, external wall, window, and stair issues and neighborhood ratings.

[44] Another common approach to controlling for demographic differences would be to test for common pre-trends between households before they move into a new unit, and use difference-in-differences to estimate the effect of moving into a rent-stabilized unit. We cannot use this approach in our case, because we do not observe households longitudinally. No alternative data source follows households over time and accurately registers the rent-stabilization status.

[45] Similarly, we construct another instrument $\text{IV}^{mkt}_{b,n-1}$ as the ratio of the total number of vacant-for-rent units that are private market-rate $VR^{mkt}_{b,n-1}$ and the total number of vacant-for-rent units $VR^{all}_{b,n-1}$. Results are robust to the different IVs.

because the survey's main objective is to inform housing authorities in NYC about vacancy rates. We use all vacant units available for rent, which excludes vacant units being unavailable in the rental market, vacant units due to seasonal occupation and vacant units being offered for sale, which the NYCHVS also identifies. We also use the official rent-regulation status of all the vacant units in each borough to determine this *historical* availability of rent-stabilized units available for rent. Additionally, we use sampling weights to construct population-level statistics.

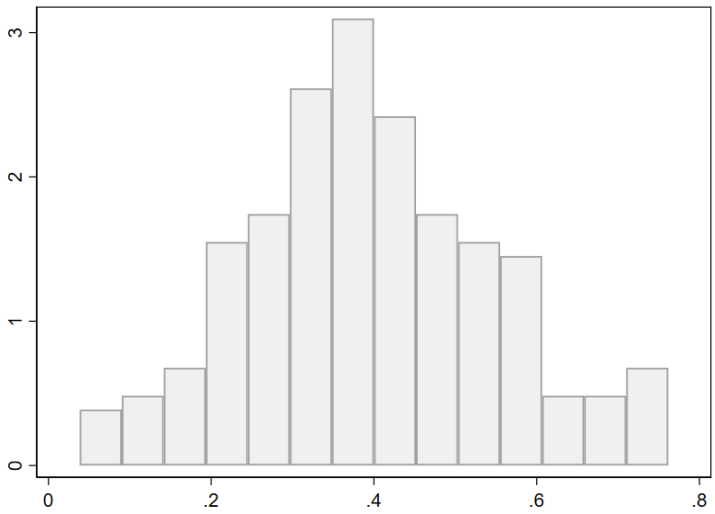
Figure 2 and 3 show the significant identifying variation in our instrument for the overall analytic sample and over time within the same borough. The mean of the market-tightness instrument is 43%, with a standard deviation of 16%. The histogram in Figure 2 reveals large variation in the rent-stabilized unit's market's tightness. For example, about 63% of the vacant rental units in a market at the 90th percentile are rent-stabilized rentals. This availability is much lower, 23%, for a market at the 10th percentile of the instrument distribution. Recall that the market tightness varies for each household in a borough depending on the year they moved in, guaranteeing significant variation within the borough.[46] Figure 3 shows the time series of the market tightness that households face, depending on the borough and the year they move in. Overall, markets have become tighter in recent years.

Exogeneity of our instrument requires that individual tenants cannot manipulate it. Some markets are tighter than others for many reasons. The instrument varies due to the interactions of the total number of new units built, the net number of units added to the rental market, the net number of units that remain vacant, and the number of units that leave and enter the rent-stabilization status. These factors result from aggregate demand and supply factors that are not manipulated by individual households. Although various reasons can explain these market dynamics, these reasons are not relevant for our analysis as long as they are randomly distributed across types of households.[47] The possibility that these factors can be affected by an individual tenants' decisions seems unlikely.

[46] Each borough has a large number of values for the instrument, but we are using only one instrument, because only one moment condition exists: each household's instrument value is determined by their year of moving in.

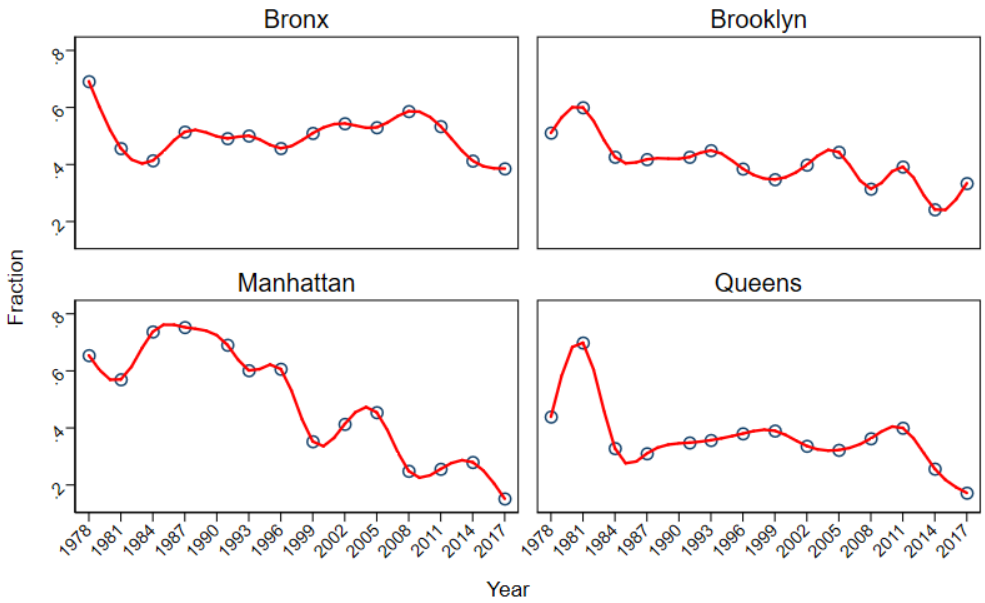
[47] The denominator in equation (5), the total number of vacant units for lease, varies when units switch from being offered for lease to offered for sale, when landlords stop listing units for rent, due to beginning renovations on them, when legal disputes prevent their immediate lease, and when they are held for occasional, seasonal, or recreational use. The numerator of equation (5), the total number of rent-stabilized vacant units for lease, may vary when rent-controlled units change status to become rent stabilized, when recently built units receive tax benefits, and when rent-stabilized units become deregulated (see section 3.1 for more details).

Figure 2: Histogram of the Instrumental Variable



Notes: The IV is defined as the ratio of the total number of vacant-for-rent units that are rent stabilized $N^{stab}_{b,t-1}$ and the total number of vacant-for-rent units $N^{all}_{b,t-1}$. We construct it using the vacant-housing modules from the 1978-2011 waves of NYCHVS.

Figure 3: Variation of the Instrumental Variable over Time across Boroughs



Notes: The IV is defined as the ratio of the total number of vacant-for-rent units that are rent stabilized $N^{stab}_{b,t-1}$ and the total number of vacant-for-rent units $N^{all}_{b,t-1}$. Blue dots are directly obtained and calculated from the 1978-2011 waves of NYCHVS, and the red lines represent fitted values based on cubic spline interpolation. Staten Island has a fairly small share of rent-stabilized rental units and its IV plots are available upon request.

To show instrument relevance, we show the market tightness is correlated with the probability of obtaining a rent-stabilized unit in section 5.3. To show instrument validity, we test the assumption of conditional independence, that is, in our case, that this market tightness is uncorrelated with worker characteristics that could affect future labor outcomes. We show in section 5.4.1 that the instrument is randomly distributed across households conditional on observable characteristics. In section 5.4.2 and 6.4.1, we argue in favor of the exclusion restriction by showing that estimates do not change appreciably when we augment the baseline model to control for other aspects that are chosen simultaneously with moving. Finally, we address the potential heterogeneity of effects by showing we satisfy monotonicity, that is, the requirement that a tenant who gets a rent-stabilized unit in a tight market would also get it in a looser market, in section 5.4.3.

We follow a two-stage least squares (2SLS) estimation of β, with equation (4) as the second-stage equation and a first-stage equation specified by

$$D_{it} = \lambda Z_{b(i),n(i)} + X_{it}\omega' + U_s + W_t + v_{i,t}, \tag{6}$$

where the scalar variable $Z_{b(i),n(i)}$ denotes the relative tightness of the market for rent-stabilized units faced by household i when they moved in at time n to a unit in borough b. Under the assumptions of instrument exogeneity and monotonicity, the 2SLS estimand can be interpreted as a weighted average of the causal effect - the local average treatment effects (LATEs) - of rent stabilization among the subgroup of tenants who could have received a different rent-regulation status had they faced a different housing market in borough b and period t when moving in.

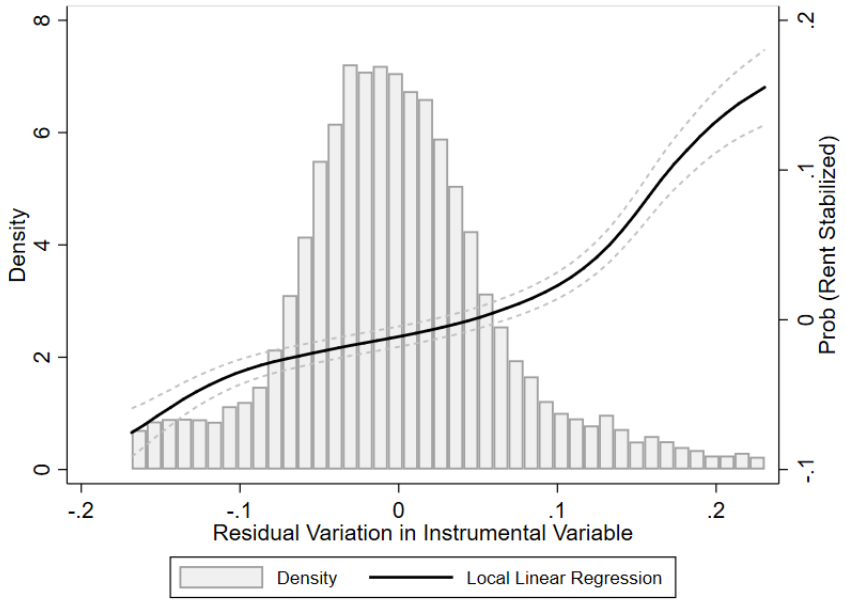
5.3 Assessing the Instrument: Instrument Relevance

The market-tightness instrument is positively correlated with our treatment variable. Facing a looser market is associated with a higher likelihood of occupying a rent-stabilized unit, even after controlling for household and unit characteristics, as well as year and sub-borough fixed effects. The black line in Figure 4 shows the implied probability of being in a rent-stabilized unit for each instrument value in the horizontal axis. This graph is a flexible representation of the first-stage equation (6) with estimates from a local linear regression, following Dahl et al. (2014); Arni and Schiprowski (2019); Bhuller et al. (2020), that regresses the instrument on the treatment variables

residualized to control for the covariates included in the estimation. The likelihood of being in a rent-stabilized unit is monotonically increasing with the rent-stabilized market tightness. The relationship is close to linear.

Table 4 reports first-stage estimates for the whole sample. The dependent variable is our treatment variable, that is, a binary indicator for whether a unit is rent stabilized. The estimated coefficient for the instrument is between 0.8 and 0.6, depending on whether we include housing and neighborhood controls. The preferred specification in column 2 indicates that when a tenant faces a market with a share of vacant rent-stabilized units 10 percentage points higher, the probability of getting a rent-stabilized unit is 6% higher. A tighter market works like a lottery with fewer winning tickets.[48]

Figure 4: First-Stage Graph of IV on Rent Stabilization



Note: The probability of living in a rent-stabilized unit is plotted on the right y-axis against our proposed instrument shown along the x-axis. The plotted values are residuals from regressions on all variables listed in Table 3. The solid line shows a local linear regression of our instrument on rent stabilization. Dashed lines show a 90% confidence interval. The histogram shows the density of the instrument along the left a-xis. This graph follows Dahl et al. (2014); Arni and Schiprowski (2019); Bhuller et al. (2020).

[48]The change in the market tightness does not mechanically imply a proportional increase in the probability of being in a rent-stabilized unit, that is, a first-stage coefficient of 1. The reason is that additional covariates exist and the market tightness measure is constructed using units not included in the estimation; that is, a vacant unit is included in the instrument construction but not in the unemployment regression.

Table 4: First-Stage of IV Estimation

	Full Sample	
	(1)	(2)
IV	0.813***	0.639***
	(0.155)	(0.135)
$\overline{Y}$	0.527	0.527
N	21906	21906
R^2	0.206	0.645
F Statistic	28.634	22.310
Sub-borough and Year Fixed Effects	Yes	Yes
Tenant controls	Yes	Yes
Housing and Neighborhood Controls	No	Yes

Note: The data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. The outcome variable is the treatment variable, that is, a tenant's rent stabilization status, and the key variable of interest is the IV. Each column has sub-borough and survey-year fixed effects. Each column has sub-borough and survey-year fixed effects. Tenant controls include gender, race and ethnicity, age, marital status, non-labor incomes, presence of children, household size, and total income of all the other household members. The spouse's race and ethnicity, education, and labor market status are also included for married tenants. Housing and neighborhood controls including when a building was built, numbers of units and stories, numbers of rooms and bedrooms, whether the building owner lives in the building, lease types, fuel and heating types, existence of housing issues such as rats, water leakage, broken plaster or peeling paint, plumbing and kitchen issues, external wall, window, and stair issues and neighborhood ratings. Standard errors are in parentheses and clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

5.4 Assessing the Instrument: Instrument Validity

5.4.1 Conditional Independence

We worry about the endogeneity of the rent-stabilization status. If the policy assignment is determined by individual characteristics that also affect labor market performance, an OLS estimate of the treatment on unemployment will suffer from bias. As discussed in section 5.1, although demographics seem somewhat comparable, differences remain. Our measure of market tightness provides variation that is exogenous to these individual characteristics. To assess this validity, we run a regression of a tenant's observable characteristics on the market-tightness instrument following Bhuller et al. (2020). The results are in columns 3 and 4 of Table 3. This balanced panel test is comparable to a test of random assignment of treatment in a randomized controlled trial. The significance of all variables disappears, and the economic magnitude decreases by several orders of

magnitude. This finding suggests households and units are as good-as-randomly assigned to markets of different tightness, conditional on the fixed effects used in our primary estimation. Tenants do not strategically seek markets (location and move-in time) of different tightness levels based on their demographic characteristics. Variation in market conditions indeed changes the probability of getting rent-stabilized units in ways that are exogenous to the worker characteristics.[49]

5.4.2 Exclusion

Conditional random assignment of these tenants to markets of different tightness validates the causal interpretation of the effect of the instrument on our outcomes of interest. However, our question is not about the effect of facing a tighter market but about the effect of rent stabilization. To interpret the IV estimates as causal effects of the rent-stabilization status, we further need to provide evidence of the exclusion restriction, namely, that this market-tightness instrument should affect a tenant's unemployment status only by changing the probability that they obtain a rent-stabilized unit. Suppose the choice leading to the rent-stabilized unit affects workers' potential unemployment in other ways, we have the issue of multidimensionality of the treatment Bhuller et al. (2020). In that case, we face a threat to the exclusion restriction. One possibility is that the availability of units when moving into a unit can be correlated with the neighborhood's amenities. This correlation could happen, for example, if rent-stabilized units' availability, conditional on the observables included in our 2SLS estimation, indicates low neighborhood quality, which could in turn be correlated with unobservable characteristics of the workers moving there. The main concern here refers to the correlation between neighborhood quality and unobserved worker heterogeneity *at the time of moving in* that could be both persistent over time[50] and correlated with contemporary (in our estimation period) outcomes Y_{it}.[51] [52]

[49] We obtain a similar conclusion by running first-stage regressions without any controls beyond the fixed effects and sequentially adding these worker observable characteristics. When we do so, the estimate remains stable around the 0.8 result reported in Table 4.

[50] This persistence here would occur among the workers, not at the location. We have evidence that residents choose locations for seemingly exogenous reasons (see section 5.1 and Appendix Table D7). Because neighborhood quality changes over time, the residents of a neighborhood today face amenity levels that were difficult to predict from their time of moving in.

[51] The effect of contemporaneous neighborhood quality on the outcome variable is less concerning because (1) it is controlled for by the sub-borough fixed effects, (2) affects all workers in a neighborhood equally, regardless of the time they moved in, which is the time variable we are proposing could be correlated with unobservable endogenous characteristics, and (3) affects all workers in the borough equally regardless of the rent-stabilization status.

[52] Factors directly implied by the rent-stabilization benefits, that is, an increased ability to save, are potential mechanisms for the effect of rent stabilization that we analyze in our theoretical model in section 3.2 and empirically

In sum, in this context, the IV estimates based on equations (4) and (6) could be biased because they do not include a neighborhood-quality indicator. This omission violates the exclusion restriction because market tightness may affect contemporaneous labor market performance Y_{it} not only through influencing the rent-stabilized treatment D_{it}, but also through its correlation with neighborhood quality holding D_{it} fixed. Controlling for neighborhood quality at the time of moving in in equations (4) and (6) eliminates this source of bias. We measure unobserved neighborhood quality and include it as a control in section 6.4.1. Robust results ease this concern affecting the exclusion restriction.

Finally, we worry the instrument may affect workers differently depending on their expected response to the treatment.[53] We run first-stage regressions dividing the sample into workers who are aware and unaware of their rent-stabilized status (see discussion of awareness in Table 2 and section 6.3.3) and find the instrument strongly predicts the probability of being in a rent-stabilized unit for both aware and unaware tenants in the same direction, despite coefficients around twice as large for the aware tenants. This result shows the instrument affects the allocation of rent-stabilized units for those looking for one endogenously (aware) and those that are not (unaware). This result further supports the argument that the instrument affects this probability exogenously through changing availability exclusively.

5.4.3 Monotonicity

If the effect of being in a rent-stabilized unit is constant across tenants, conditional independence and the exclusion restriction guarantee our estimate gives a causal effect. Otherwise, we can still perform estimation, but monotonicity must be assumed. In our case, monotonicity amounts to assuming a tighter market reduces the probability of getting a rent-stabilized unit (the opposite for looser markets) for all tenants in (weakly) the same direction, excluding the presence of defiers (Imbens and Angrist, 1994; Angrist et al., 1996).[54] Although market tightness does not need to

in section 6.2. These mechanisms do not pose a threat to the exclusion restriction, because they are downstream consequences of the treatment Angrist et al. (2011).

[53] Sorting into the treatment depending on the idiosyncratic heterogeneous response to it is the essential heterogeneity issue in Heckman et al. (2006). We worry this sorting will happen on the instrument as well. For example, workers who expect to benefit more from rent stabilization observe lower availability of these units and decide to search harder for them, increasing their probability of getting one.

[54] Tenants are unlikely to refuse to choose to live in rent-stabilized units when local availability of vacant-for-rent units is higher, because rent-stabilized units cover a wide range of prices and quality, even including somewhat luxury apartments. Chen et al. (2022) calculate the propensity of rental units in NYC using all available unit and

affect all potential tenants, it should affect all those it does affect in the same direction (Cunningham, 2021). This further ensures the IV estimate gives us a weighted average of the underlying causal effects of the affected group (Imbens and Angrist, 1994). This LATE shows the average causal effect among the tenants who would have moved to a different rent-regulation type had they faced a market of different tightness.

To test this monotonicity assumption, we show first-stage estimates are nonnegative for multiple subsamples following Bhuller et al. (2020). Appendix Table D8 shows the first-stage estimates of the coefficient of our instrument on the treatment for different demographic types of tenants. These divisions are based on characteristics related to a potential heterogeneous effect of the rent stabilization, such as race, education, age, and length of time in the unit.[55] We also include a subsample division into groups across the distribution of the propensity to be rent stabilized (see Appendix Table D9). This propensity is estimated as the probability of being in a rent-stabilized unit, calculated using an OLS regression on the characteristics included in our model from equation (4), including the fixed effects. This approach allows the heterogeneity of effects to be an index of all the observed tenant and location characteristics. All coefficients are large, positive, and highly significant, supporting the monotonicity assumption.

Another implication of this assumption is that a tighter market for a type of unit (i.e., large rent-stabilized apartments) should also be tighter for other types (i.e., small ones). Testing this is relevant if tenants receive heterogeneous benefits from the treatment depending on the type of unit they demand, in ways that are not captured by the demographic characteristics used as controls or by the composite index implied by the propensity calculation above. To test this implication, we implement a reverse-sample instrument analysis as in Arni and Schiprowski (2019), where we redefine the instrument to be the market tightness of large units (number of rooms above

neighborhood characteristics to be rent stabilized and find rent-stabilized and private market rental units for all propensity values. In other words, observable characteristics share common support between these two markets. Furthermore, unlike public housing, we do not expect the stigma associated with rent-stabilized units. As described in section 5.1, the rent-stabilization status of a unit is difficult to observe and can be heterogeneous across units in the same building. If monotonicity is violated, IV estimates an average response of those induced to switch into the policy and those induced to switch out of the policy by the change in the market tightness (Heckman et al., 2006). Angrist et al. (1996); Yitzhaki (1996) analyze the case in which the monotonicity assumption is violated and measure the contradicting two-way flow effects.

[55] We also report the heterogeneous effects of rent stabilization across demographics groups as predicted by our theoretical model in section 6.2.

the median)[56] and use it as an instrument for units outside this subsample (units with number of rooms equal to or below the median), and vice versa. We show first-stage estimates for this reverse-sample instrument in the second panel of Appendix Table D9.[57]

6 The Effect of Rent Stabilization on Unemployment

In this section, we first present the main estimation results on the unemployment effect of rent stabilization. Consistent with the job-search model prediction in section 3.2, we find rent stabilization increases unemployment. We then discuss the heterogeneity of this unemployment effect by tenure duration, race, education, and age as predicted by the model. We also discuss alternative mechanisms and robustness checks for our main results in this section.

6.1 Baseline Results

In columns (1) - (3) of Table 5, we present OLS results that add control variables progressively. In column (1), only sub-borough and year fixed effects are included. In column (2), we add householder and household controls, including gender, race, and ethnicity, age, marital status, non-labor incomes, presence of children, household size, and total income of all the other household members. The spouse's race and ethnicity, education, and labor market status are also included for married tenants. Income-related variables are defined in $10,000 (2017 real). In column (3), we add housing and neighborhood controls, considering that some housing characteristics such as the number of units and construction year influence whether a building is rent stabilized (see section 5.1). We include the year of construction, numbers of units and stories, numbers of rooms and bedrooms, whether the building owner lives in the building, lease types, fuel and heating types, housing issues such as rodents, water leakage, broken plaster, or peeling paint, plumbing and kitchen issues, external wall, window, and stair issues, and neighborhood ratings. Overall, the probability of being

[56] Using similar notation as in section 5.2, we define this reverse-sample instrument as

$$Z_{b,n} = \frac{VR^{stab,L}_{b,n-1}}{VR^{all,L}_{b,n-1}},$$

where $VR^{stab,L}_{b,n-1}$ is the total number of vacant-for-rent units that are rent stabilized and large, and $VR^{all}_{b,n-1}$ is the total number of units that are vacant-for-rent and large.

[57] We cannot follow a reverse-sample instrument strategy based on the demographic characteristics, because all units are potentially open to being leased by all tenants.

unemployed is positively associated with living in a rent-stabilized unit across all specifications. Still, it is significant only when housing and neighborhood controls are included. These results show the probability of being unemployed is 1.3 percentage points higher when a tenant lives in a rent-stabilized unit versus in a private market-rate unit.

As discussed in sections 5.1 and 5.2, the OLS coefficients are likely biased due to endogeneity concerns, the direction of which is ex-ante unclear. Columns (4) - (6) of Table 5 present the results of the 2SLS analysis using our market-tightness instrument. We find that living in a rent-stabilized unit increases unemployment by more than 5 percentage points from our IV estimates. This result is significant economically, given that the unconditional average share of unemployed is 5% (6% and 5% for rent-stabilized and private market tenants, respectively). In addition, the IV coefficient is about four times larger than the OLS coefficient. These results suggest our OLS estimates are biased downward. One possible explanation is that tenants in rent-stabilized units are more likely to have unobserved characteristics that lower their unemployment, for example, higher unobserved skills. In this case, we can conclude that the higher unemployment observed in rent-stabilized units is a consequence of the policy effect and not selection. Another possible explanation for the IV and OLS differences is heterogeneity effects, which we explore in later sections. Also, we note the F-statistics range from 22 to 28, indicating weak IV is not a problem here. More validation tests of our instrument are provided in section 5.2.

6.2 Heterogeneous Effects

In section 3.2.3, we showed the disincentive effect of rent stabilization on tenant unemployment should theoretically be stronger if rent discounts are larger, expected wages are higher, or tenants are more risk averse. This section tests those model predictions. Throughout this section, we adopt the main estimation specification as in Table 5 column (6), where the share of rent-stabilized vacant-for-rent ratio is used as an IV for rent-stabilization status.

6.2.1 Size of Rent Discount: Tenure

Though considering that larger rent discounts can lead to greater disincentive effects is intuitive, the empirical testing of this model prediction is challenging, considering that measuring the magnitude of the rent discount and quantifying this intensive margin is hard. Unlike other welfare programs

Table 5: The Effect of Rent Stabilization on Tenant Unemployment

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Rent Stabilized	0.010	0.010	0.013**	0.063***	0.054***	0.056***
	(0.007)	(0.006)	(0.004)	(0.005)	(0.004)	(0.010)
$\overline{Y}$	0.047	0.047	0.047	0.047	0.047	0.047
N	21906	21906	21906	21906	21906	21906
R^2	0.012	0.019	0.026			
F-Statistics				28.008	28.634	22.310
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Tenant controls	No	Yes	Yes	No	Yes	Yes
Housing and Neighborhood Controls	No	No	Yes	No	No	Yes

Notes: The data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. The outcome variable is a tenant's unemployment status; that is, Y=1 if a tenant is currently unemployed and Y=0 if a tenant is currently employed. Columns (1)-(3) each adopt a linear OLS model, and columns (4)-(6) each adopt our proposed IV model. Each column has sub-borough and survey-year fixed effects. Tenant controls include gender, race and ethnicity, age, marital status, non-labor incomes, presence of children, household size, and total income of all the other household members. The spouse's race and ethnicity, education, and labor market status are also included for married tenants. Housing and neighborhood controls including when a building was built, numbers of units and stories, numbers of rooms and bedrooms, whether the building owner lives in the building, lease types, fuel and heating types, existence of housing issues such as rats, water leakage, broken plaster or peeling paint, plumbing and kitchen issues, external wall, window, and stair issues and neighborhood ratings. Standard errors are in parentheses and are clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

where by the benefit levels are calculated from the recipient's characteristics based on formulas, the magnitude of the rent discount depends not only on the rent of rent-stabilized units but also on the rent of counterpart private units. To overcome this challenge, we use the number of years the tenant has been living in their current unit as a proxy for the rent discount, considering the rent discount increases with tenure duration. Appendix Figure E8 shows that when stabilization policy is binding, rent discounts increase with a tenant's length of stay in the unit (Chen et al., 2022). Thus, we test whether the unemployment effect increases with the rent discount, by comparing the impact of rent stabilization among workers who recently moved in relative to those who have had long tenures in the unit.

In Panel A of Table 6, we divide the sample into short-term and long-term tenants, where we use five years, the median of tenure duration, as the cutoff length.[58] We find the unemployment

[58]Results of using other numbers of years as cutoff length are similar and are available upon request.

effects of rent stabilization are not only positive and significant but also much larger in magnitude among long-term tenants (0.173) and are negative and insignificant among short-term tenants (-0.013). These estimation results suggest the size of the rent discount plays an important role in the disincentive effect and are consistent with our model predictions that a larger rent discount can lead to a greater unemployment effect.

6.2.2 Wage Distribution: Education and Race

Our model predicts that the unemployment effect should be larger among workers with a higher expectation of future wages, because the return of staying unemployed is larger for those workers. We divide our sample by education level and race to test this prediction. The motivation is that white workers and college workers are more likely to have a higher expectation of future wages. The conditional wage distribution in Appendix Figure E9 confirms that white workers and college workers are more likely to receive high-wage jobs, because their wage distribution is skewed to the left.

In Panel B of Table 6, we divide the sample by worker's education level, where high-skilled workers are those with a college degree or above, and low-skilled workers are those without a college degree. The subsample results show the unemployment effects are mainly evident among workers with a college degree, whose unemployment is increased by 6.1 percentage points (statistically significant at the 1% level). By contrast, the effect among low-skilled tenants is smaller in magnitude and not statistically significant. In Panel C of Table 6, we divide the sample into white and non-white workers. We find the unemployment effect of rent stabilization is mainly driven by white tenants, whose probability of unemployment increases by 7.8 percentage points (statistically significant at the 1% level). By contrast, non-white tenants' probability of unemployment is insignificant and increases only by 3.1 percentage points.

Those results are consistent with our model's theoretical implications that the disincentive effects are more substantial among workers with better labor market perspectives who are more willing to wait longer for better job offers when unemployed. Note the heterogeneous effects among different racial and education groups can also be driven by forces other than wage distribution. For example, race and education are also important determinants of the rent discount (Chen et al., 2022), which can lead to the observed heterogeneous effects as discussed in section 6.2.1.

6.2.3 Risk Aversion: Age

As demonstrated in Proposition 5, risk aversion plays a vital role in determining the disincentive effect of the rent discount, because the cost and expected benefit in the future will cancel each other out if the worker is risk neutral. Though we do not have direct measurements of risk aversion, the general belief is that older workers may become less willing to take risks, due to biological aging processes (Barsky et al., 1997; Albert and Duffy, 2012; Sahm, 2012; Dohmen et al., 2017). Thus, we test the role of risk aversion by exploring the heterogeneity of unemployment effects among older and younger workers.

In Panel D of Table 6, we split our sample by age and find the unemployment effects of rent stabilization are much stronger among workers who are older than 40 than among workers who are younger than 40. Notably, we find the unemployment effect of rent stabilization increases the probability of unemployment by 6.1 percentage points (significant at the 10% level) among workers who are older than 40. By contrast, the likelihood of unemployment increases only by 2.7 percentage points among younger workers, and the estimated effect is not statistically significant. Those results are consistent with our model predictions: the unemployment effect increases when workers are more risk averse.[59]

6.3 Discussion on Alternative Mechanisms

As discussed in section 3.2, the unemployment effect of rent stabilization is driven by declining unemployment costs, inducing longer jobs searches. This section discusses alternative mechanisms and explains why they are less likely to be the main driving forces behind the unemployment effect of rent stabilization in our sample. We also discuss the role of policy awareness in intensifying this unemployment effect.

[59] Considering that older tenants stay in rent-stabilization units for longer and thus enjoy greater rent discounts, we further split the sample by the length of their tenure. Those results suggest that even among workers with longer tenures, the effect of rent stabilization is much larger and more precisely estimated among workers older than 40 than among workers younger than 40. These results further confirm the critical role of risk aversion and are available upon request.

Table 6: Heterogeneous Treatment Effects

	Panel A: By Tenure				Panel B: By Skill			
	Short Tenure		Long Tenure		High Skill		Low Skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rent Stabilized	-0.007	-0.013	0.140***	0.173**	0.058***	0.061***	0.023	0.038
	(0.045)	(0.049)	(0.038)	(0.072)	(0.011)	(0.016)	(0.029)	(0.045)
$\overline{Y}$	0.045	0.045	0.051	0.051	0.036	0.036	0.057	0.057
N	14739	14739	7167	7167	10433	10433	11473	11473
F Statistics	7.775	8.487	18.468	9.080	42.293	75.058	30.483	6.682
	Panel C: By Race				Panel D: By Age			
	White		Non-white		Younger		Older	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Rent Stabilized	0.068***	0.078***	0.033	0.031	0.027	0.027	0.055***	0.061*
	(0.023)	(0.029)	(0.040)	(0.065)	(0.023)	(0.027)	(0.015)	(0.032)
$\overline{Y}$	0.037	0.037	0.053	0.053	0.044	0.044	0.051	0.051
N	8581	8581	13325	13325	13552	13552	8354	8354
F Statistics	66.795	75.117	26.462	11.972	22.654	25.185	29.748	14.615
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenant controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. In Panel A, the short tenure (long tenure) subgroup includes tenants who live less than or equal to 6 years (more than six years) in their current dwellings. The average housing tenure is six years in our sample. In Panel B, the high skill (low skill) subgroup includes tenants who have at least college degrees (lower than college degrees). In Panel D, the younger (older) subgroup includes younger or equal to age 40 (older than age 40). Each column has sub-borough and survey-year fixed effects. Tenant controls and housing and neighborhood controls are defined similarly in Table 5. Standard errors are in parentheses and clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

6.3.1 The Mobility Channel

First, previous studies on rent regulation have highlighted that rent control reduces mobility. For example, Svarer et al. (2005) described this mobility channel in a two-market (local vs non-local) job-search model. They argue rent control can prevent workers from moving for jobs in non-local labor markets and force them to face a limited amount of job opportunities in local labor markets, leading to inferior labor market outcomes. They find empirical support for the mobility channel, based on Danish household panel data.[60]

However, this mobility channel is less likely to be relevant for our results, considering the abundant job opportunities provided in NYC. Also, to identify the mobility effect, one needs a longitudinal dataset that follows tenants and compares labor market outcomes of stayers and movers as housing policies make them face different labor market opportunities. But our results are solely derived from stayers who face the same local labor market. Thus, attributing their differences to the mobility channel is difficult.[61]

Nevertheless, we further test the mobility channel by dividing our sample into Manhattan and non-Manhattan tenants. Considering that jobs are concentrated in Manhattan,[62] rent-stabilized tenants that live in non-Manhattan areas are more likely to be prevented from getting access to job opportunities. If the mobility channel is a major driving force behind the estimated unemployment effect in our sample, we should observe a stronger effect among non-Manhattan tenants. However, in Panel A of Table 7, we see the unemployment effect is insignificant among non-Manhattan tenants, suggesting the mobility channel is not likely to be the main driving force.

[60] Diamond et al. (2019) also finds rent control limits renters' mobility. But their discussion is focused on the rental market instead of labor market implications. In addition, the reduced mobility has often been used to explain why home ownership causes unemployment, the so-called Oswald hypothesis (Oswald, 1996; Munch et al., 2006, 2008; Coulson and Fisher, 2009).

[61] Theoretically, the mobility and disincentive channels are not exclusive. Our model can be extended to include both local and non-local job market searches to demonstrate rent discounts can increase unemployment by restricting mobility and the disincentive effect.

[62] According to a report published by the Bureau of Labor Statistics(See https://www.bls.gov/opub/ted/2022/employment-peaks-by-county-1975-2020.htm), during 1975–2020, the employment peaks for Manhattan, Brooklyn, Queens, and Bronx are 2,527,343, 795,104, 717,786, and 324,446, respectively. Roughly speaking, in the New York metropolitan area, jobs are heavily concentrated in Manhattan, which provides more than half the number of jobs.

Table 7: Evidence on Alternative Mechanisms

	Panel A: By Manhattan			
	Manhattan		Non-Manhattan	
	(1)	(2)	(3)	(4)
Rent Stabilized	0.071***	0.069***	0.074	0.169
	(0.017)	(0.025)	(0.122)	(0.298)
$\overline{Y}$	0.042	0.042	0.049	0.049
N	6032	6032	15874	15874
F Statistics	36.649	31.331	14.984	5.676
	Panel B: By Non-Labor Income			
	Non-labor Income: ≤ 0		Non-labor Income >0	
	(1)	(2)	(3)	(4)
Rent Stabilized	0.019***	0.023**	0.100***	0.089***
	(0.005)	(0.010)	(0.005)	(0.023)
$\overline{Y}$	0.036	0.036	0.104	0.104
N	18296	18296	3610	3610
F Statistics	21.893	15.113	168.954	229.248
	Panel C: By Awareness			
	Aware		Unaware	
	(1)	(2)	(3)	(4)
Rent Stabilized	0.127***	0.208***	0.097***	0.149
	(0.015)	(0.056)	(0.026)	(0.102)
$\overline{Y}$	0.064	0.064	0.060	0.060
N	3786	3786	4308	4308
F Statistics	40.624	21.094	19.536	5.681
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes
Tenant controls	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes

Notes: In Panels A and B, the data come from the 2002, 2005, 2008, 2011, and 2017 waves of the NYCHVS. In Panel C, the data only come from the 2002 and 2005 waves of NYCHVS because the question about self-reported rent-regulation status is not available starting from the 2008 wave and onward. "Aware" refers to tenants who live in rent-stabilized units and claim their housing units are either "rent controlled" or "rent stabilized" when asked to self-report the housing regulation status. "Unaware" refers to tenants who live in rent-stabilized units but claim their housing units are either "unregulated" or "don't know" when asked to self-report the housing regulation status. The control group refers to tenants who live in unregulated, private market-rate units and correctly claim their housing units are "unregulated" when asked to self-report the housing regulation status. Each column has sub-borough and survey-year fixed effects. Tenant controls and housing and neighborhood controls are defined in the same way as in Table 5. Standard errors are in parentheses and clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

6.3.2 The Liquidity Channel

The other mechanism mentioned in the literature is the liquidity channel. Chetty (2008) adopted it to explain the effect of unemployment benefits on unemployment. They argue unemployment benefits increase cash-on-hand and consumption while unemployed for workers who cannot smooth consumption perfectly. In this way, unemployment benefits reduce unemployed workers' pressure to find a new job quickly, leading to more prolonged unemployment. Suppose the liquidity channel is the primary driving force behind the unemployment effect observed in our main estimation. In that case, we should expect this effect to be stronger among households without non-labor income who have less ability to smooth consumption and should exhibit stronger liquidity effects holding all else constant. However, the opposite is true according to Panel B of Table 7. We find that among tenants with non-labor income, the probability of unemployment is increased by 8.9 percentage points (statistically significant at the 1% level), while among tenants without non-labor income, the likelihood of unemployment grows less at 2.3 percentage points (statistically significant at the 5% level). Those results suggest the liquidity channel is not the primary driving force behind the observed unemployment effect, though we cannot completely rule out this channel.

6.3.3 Does Policy Awareness Matter?

In section 5.1, we discussed that rent-stabilization status is not always salient. Because it does not require an explicit application process, beneficiaries are often unaware of their rent-stabilization benefit. Landlords attach to the lease a "lease rider" explaining rent stabilization in regulated units, which could be placed at the end of a long contract and only revealed near the end of the leasing process. Regardless, the landlord is restricted by potential substantial penalties from increasing the rent beyond the cap posted by RGB every year. And still, they may not have incentives to reveal that their unit is rent stabilized. In a low-vacancy market such as NYC, stable tenants in a rent-stabilized unit mean forgone revenue from more frequent post-vacancy rent adjustments for landlords. The 2002 and 2005 NYCHVS report both the self-reported regulation status by the tenant and the legal rent regulation status verified from administrative sources.[63] Among all tenants occupying rent-stabilized units, we find that more than 60% of tenants cannot identify that they are in a rent-regulated unit (see Table 2).

[63]The self-reported regulation status is not available since 2008 and onward in NYCHVS.

We divide the sample into an *Aware* and an *Unaware* subsample, both of which include all tenants in market-rate units who correctly self-report that they occupy unregulated units. Among those in rent-stabilized units, we only keep tenants who self-reported that their housing units were either rent controlled or rent stabilized in the Aware subsample. By contrast, we only keep tenants who self-reported that they lived in unregulated units or did not know in the Unaware subsample. We report the IV estimates for each subsample in Panel C of Table 7. When controlling for housing and neighborhood controls, the preferred specification shows the point estimate is much larger in the Aware subsample and the coefficient is more precisely estimated.[64]

Recall that Aware tenants will know not only that their rent may be cheaper today, but also that it will not grow as much in the future. The concentration of the effect in the Aware group supports that tenants' strategic forward-looking considerations when accepting or rejecting job offers play a pivotal role, precisely the type of mechanism motivated by the model. This result also suggests the effect does not only work through providing liquidity and easing the budget constraint (Chetty, 2008), which applies equally to aware and unaware tenants of rent-stabilized units. Furthermore, the F-statistic suggests the instrument is more likely to change exogenously the probability of getting a rent-stabilized unit for those aware and potentially seeking.

Besides the mechanism, information about policy awareness can shed light on endogeneity. Appendix Table D10 shows the OLS results when dividing the sample into Aware and Unaware. The OLS point estimate is twice as large in the OLS results when comparing Aware with Unaware. The estimated coefficient increases significantly for both subsamples when using the instrument, showing the endogeneity imposes a downward bias in OLS estimates. However, when comparing the IV and the OLS estimates, only those from the Aware subsample are statistically different from the OLS result. This finding suggests the bias is coming mainly from the Aware. One would expect that, indeed, they are the group more actively seeking to sort into the treatment; that is, for them, the observed treatment is further away from being as-good-as-randomly allocated.

Future work could leverage the awareness data to investigate bias coming from essential heterogeneity (Heckman et al., 2006). In our context, essential heterogeneity amounts to heterogeneous

[64] In unreported results, we acknowledge that awareness of the policy itself may be endogenous to unemployment. Following Chetty et al. (2013), we build an additional instrument that measures the aggregate local knowledge about rent-stabilization awareness as an exogenous determinant of an individual's policy awareness. The results are consistent with the discussion in this section and are available upon request.

interest in rent stabilization stemming from partial knowledge of the idiosyncratic tenant response; that is, tenants who expect to benefit more seek a rent-stabilized unit aggressively, increasing the likelihood of accessing the treatment. If gains from treatment heterogeneity happen across subgroups, we have addressed this possibility with the subgroup analysis presented in section 6.2. Remaining essential heterogeneity that leads to the problematic sorting could be addressed with the awareness subsamples. Because tenants unaware of their rent benefits by definition are not sorting based on their unobserved gain from treatment, they can serve as a control group to show housing choice in the absence of the rent-stabilization distortion.

6.4 Robustness Tests

6.4.1 Threats to the Exclusion Restriction

As discussed in section 5.4.2, the exclusion restriction can be threatened if our instrument reflects neighborhood quality and this quality is in turn correlated with persistent unobservable characteristics of workers. This situation violates the exclusion restriction, because market tightness would affect contemporaneous unemployment Y_{it} not only through influencing the rent-stabilized treatment D_{it} but also through inducing workers with different characteristics to move to neighborhoods with different tightness values Z_{bn}. This section shows that controlling for neighborhood quality to eliminate that source of bias does not change our main results.

Neighborhood quality is unobserved. To measure it, we follow Sieg et al. (2002) and construct it from the following rental hedonic pricing function estimated using only private market units, separately for every year t and borough b:

$$\ln P_{jbn}^{mkt} = \beta X_{jbn} + B_{bn} + \psi_n, \tag{7}$$

where b indexes borough, n indexes year of moving in, and P_{ibn}^{mkn} is the monthly rental price for private market-rate unit j in borough b at year n. X_{jbtn} contains all the available *physical housing traits* at the unit and building level. B_{bn} is a borough fixed effect. No constant is included, to allow for the estimation of the fixed effects for all boroughs. Heuristically, $\hat{B}_{bn}$ captures all borough-level unobserved factors, such as school quality, crime rate, and pollution, after controlling for differences in the housing stock. The private market rent captures the value of housing services provided by

the unit, which is the sum of housing characteristics and neighborhood quality. Appendix Figure E10 shows the evolution of the average rent and of the estimated neighborhood quality $\hat{B}_{bn}$. We use cubic spline interpolation for years that are not available in NYCHVS.

To test for exclusion, we show estimates do not change appreciably when our model is augmented to control for other dimensions simultaneously determined by the unit choice, namely, local neighborhood quality, in columns (1) and (2) of Table 8.[65]

Table 8: Robustness Checks

	Unobserved Nbhd Quality		Alternative Instrument		Two Instruments Jointly	
	(1)	(2)	(3)	(4)	(5)	(6)
Rent Stabilized	0.060***	0.064***	0.059***	0.064***	0.056***	0.059***
	(0.008)	(0.014)	(0.011)	(0.018)	(0.007)	(0.012)
$\overline{Y}$	0.047	0.047	0.047	0.047	0.047	0.047
N	21906	21906	21906	21906	21906	21906
F Statistics	22.928	17.873	36.243	22.109	35.992	23.715
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Tenant controls	Yes	Yes	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes	No	Yes

Notes: The data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. Tenant controls and housing and neighborhood controls are defined in Table 5. Standard errors are in parentheses and clustered at the borough level, which is consistent with the geographic level of our IV. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

6.4.2 Alternative Instruments

As a robustness check, we construct an alternative IV that exploits exogenous variation in the private, market-rate market. We construct $\text{IV}^{mkt}_{b,n-1}$ as the ratio of the total number of vacant-for-rent units that are private market-rate $VR^{mkt}_{b,n-1}$ and the total number of vacant-for-rent units $VR^{all}_{b,n-1}$:

$$\text{IV}^{mkt}_{b,n} = \frac{VR^{mkt}_{b,n-1}}{VR^{all}_{b,n-1}} \tag{8}$$

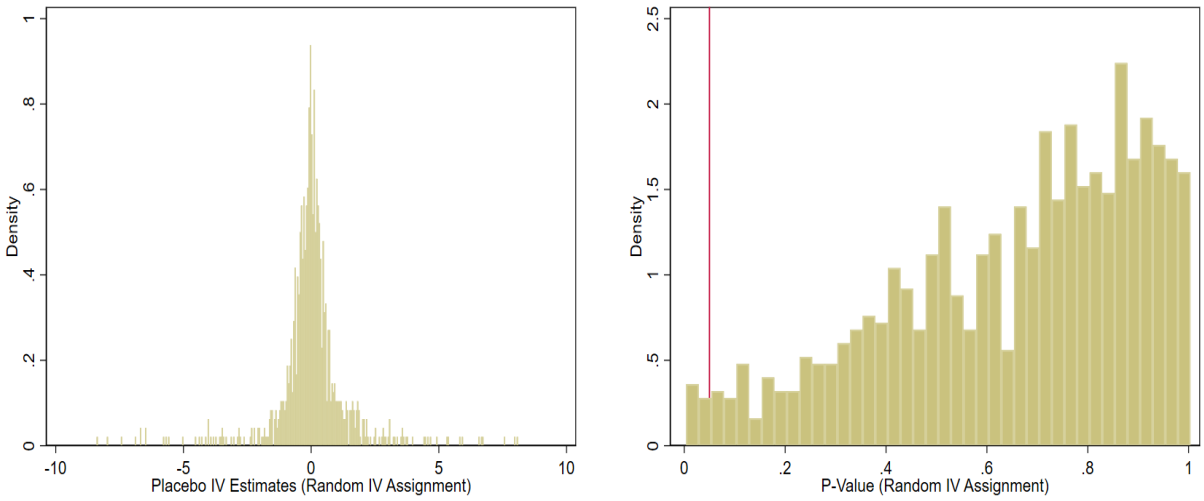
[65] This analysis is similar to sensitivity analyses to determine bias due to remaining confounders (Rosenbaum and Rubin, 1983; Peisakhin and Rozenas, 2018; Cinelli and Hazlett, 2020). Another potential approach can be extending the IV model to explicitly include neighborhood quality at the move-in time as another endogenous variable and use an instrument for the probability of choosing this neighborhood quality, such as overall (not only rent-stabilized) market tightness.

2SLS regression results with this instrument are in columns (3) and (4) of Table 8. Results are comparable to those using our main IV (columns (5) and (6) in Table 5). The F-statistic is higher than 22, suggesting this IV is also not weak. In columns (5) and (6) of Table 8, we use both instruments jointly. Our estimation results are robust with estimated coefficient around 0.6.

6.4.3 Placebo Test

Finally, we conduct a placebo test to strengthen confidence in our causal estimate. The placebo test shows the positive impact of rent stabilization on unemployment is not obtained by chance (Eggers et al., 2021). We randomly reshuffle 1,000 times the IVs across individual tenants. For each reshuffling, we estimate a causal estimate based on the reshuffled sample. Figure 5 shows the distribution of the point estimates obtained in these placebo regressions (left panel) and the corresponding p-values (right panel). The placebo IV point estimates are centered at 0. Out of 1,000 p-values, only 15 are smaller than 0.05. Therefore, we can be confident that the significant effects of rent stabilization on unemployment found in this paper are not likely to be driven by chance.

Figure 5: Placebo Test



(a) (b)

Notes: The figure in the left panel presents the distribution of placebo IV estimates, where we randomly reshuffle the IV and re-estimate the baseline IV model 1,000 times. The figure in the right panel presents the distribution for p-values corresponding to the IV estimates in the left panel. Out of 1, 000 p-values, only 15 are smaller than 0.05.

7 Conclusion

Local governments are increasingly using rent stabilization that slows down rent growth relative to market-rate units as a tool to keep rental units affordable. At the same time, the understanding of the effect of this policy beyond housing outcomes is limited.

This paper provides novel evidence on the causal impact of rent stabilization on tenant unemployment in NYC, using data between 2002 and 2017. Our baseline result shows rent stabilization increases the unemployment of tenants by approximately six percentage points, more than double the average unemployment rate during the study period. This finding is an unintended consequence of rent stabilization, a policy that is not seemingly connected to the labor market but affects tenant job-search behavior and unemployment outcomes. We develop a job-search model with rent discounts that illustrates the effect. We also find this effect is concentrated in traditionally privileged groups: non-minority tenants and those with high education levels.[66]

We discuss evidence of how the difficulty in sorting into rent-stabilized units in the NYC market. Yet, rent-stabilized units are not randomly allocated by housing authorities, so we still worry this treatment may be correlated with tenants' characteristics that also determine unemployment through other channels. We address this endogeneity concern by constructing an instrumental variable that measures the local relative market tightness of the rent-stabilized units tenants faced when they initially moved into the unit. This instrument presents significant variation: depending on the neighborhood and the year of moving in, tenants could easily face a market with 20% percent (at the 10th percentile) to 60% (at the 90th percentile) of vacant rental units being rent stabilized. The sources of variation of this market-tightness instrument, related to both the stabilized and private markets, are exogenous to individual tenants' decisions. The instrument is relevant, and we provide evidence to argue it satisfies monotonicity, conditional independence, and exclusion-restriction assumptions.

The proposed theory predicts the effect of rent stabilization on unemployment will be larger for tenants with higher rent discounts, higher expected income, and higher risk aversion. We empirically confirm these predictions with subgroup analyses: the effect is concentrated among white and highly educated tenants and tenants living in neighborhoods with high rent discounts.

[66] Svarer et al. (2005) find a positive effect of rent control on unemployment duration but in a very different market structure, where nearly all rental units are controlled.

We also test for heterogeneity of effects across households who are aware and unaware (cannot correctly identify they are beneficiaries of rent stabilization). Surprisingly, we find that one-third of policy beneficiaries are unaware of their rent-stabilized status. The effects on unemployment are stronger on those who are aware. This finding supports the model's mechanism, which relies on tenants' strategic forward-looking considerations at the time of accepting or rejecting job offers. The latter presupposes policy awareness and not simply easing the budget constraint, which applies equally to aware and unaware tenants.

Normative implications of the unemployment effect of the policy require a nuanced discussion of what the effect means. On one end, the policy could be discouraging job-search efforts. In this case, the policy would be *inefficient*. On the other end, the longer implied unemployment duration allows longer and better searches. In this case, the inefficiency statement is weakened, but the implications for inequality and *equity* of the policy are heightened. Recall the effect on unemployment is concentrated in traditionally privileged groups with lower unemployment levels, namely, white, high-education tenants. Chen et al. (2022) provide evidence showing these privileged groups benefit disproportionately more from rent stabilization and enjoy higher rent discounts. These groups outperform others in the labor market, further supporting the finding that the unemployment impact helps smooth consumption and improve their job search. Housing authorities would improve equity if the benefits for job search mentioned above targeted tenants who may need it most.

The large share of unaware tenants brings up a related concern about policy implications. It highlights the obscurity of this policy, which could be detrimental if it does not reach the populations that are most in need of rental-housing affordability. If rent regulation provides a buffer for job-search, policy opacity would further deter those in need of such a benefit from being aware of this possibility. These arguments go back to the widespread critique of rent control's poor targeting, one of the main policy lessons to be drawn.

Multiple avenues are available for future work that would complement our results and require better data. Access to longitudinal data at the individual-tenant level would allow observation of unemployment spells, wage dynamics, and movement across different tenures. It would permit comparison between our disincentive mechanism and the mobility mechanism in Svarer et al. (2005).[67] Moreover, improved data with detailed information on job applications could also allow researchers

[67]The NYCHVS does have a longitudinal feature before 2011 at the dwelling level but not at the tenant level, i.e., we cannot follow tenants over time.

to determine whether a rent discount serves as a benefit for job search that improves labor market matches or as a subsidy that distorts search effects and leads to labor market inefficiency.

The data on policy awareness provide additional areas for future work, especially given the role that the unawareness plays as a reverse placebo (they receive the treatment but do not know it). These data could enrich the previous work on the effect of policy opacity and its implications. They could also be used to investigate bias coming from essential heterogeneity (Heckman et al., 2006). Because tenants unaware of their rent benefits by definition are not sorting based on their unobserved gain from treatment, they can serve as a control group to show housing choice in the absence of the rent-stabilization distortion. This approach could extend the results Glaeser and Luttmer (2003) on the effects of rent stabilization on housing misallocation by comparing consumption between those aware and those unaware of the rent stabilization.

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Does Rent Regulation Affect Tenant Unemployment? Evidence from New York City (**Online Appendix**)

Appendix A Details on the Theoretical Model

Appendix A.1 Value Functions

Value Function for the Employed

Suppose the instantaneous utility function is $u(\cdot)$, an increasing function of disposable income. Following the standard job search theory, the expected discounted lifetime utility for an employed worker (with superscript E) who receives a constant rent discount r_d (living in a rent-stabilized unit) is

$$\begin{aligned} V^E(w, r_d) &= \underbrace{u(w + r_d))}_{t=0} + \underbrace{(1-\rho)u(w + r_d))}_{t=1} + \underbrace{(1-\rho)^2 u(w + r_d))}_{t=2} + \cdots \\ &= \sum_{t=0}^{t=\infty} (1-\rho)^t (u(w + r_d))) \\ &= \frac{u(w + r_d)}{\rho} \end{aligned} \tag{Appendix A.1}$$

Different from the standard job search model, the value function for a employed worker is not only a function of wage w but also a function of rent discount r_d. In particular, $V^E(w, r_d)$ increases with wage w and rent discount r_d. By letting $r_d = 0$, the above equation also describes the expected discounted lifetime utility for an unemployed worker who lives in a private market unit.

Value Function for Unemployed

The expected discounted lifetime utility for an unemployed worker (with superscript U) is

$$V^U(r_d) = u(b + r_d) + (1-\rho)\left[\alpha Emax\Big(V^E(w, r_d), V^U(r_d)\Big) + (1-\alpha)V^U(r_d)\right] \tag{Appendix A.2}$$

where $u(b+r_d)$ captures the current utility of staying unemployed and the rest part of the equation Appendix A.2 captures the future expected utility. Different from the standard job search model, the value function for an unemployed worker is a function of rent discount r_d instead of being constant. In particular, $V^U(r_d)$ increases with rent discount r_d. By letting $r_d = 0$, the above equation also describes the expected discounted lifetime utility for an employed worker who lives in a private market unit.

Rearrange equation Appendix A.2, we have

$$V^U(r_d) = u(b + r_d) + (1-\rho)\left[\alpha Emax\Big(V^E(w, r_d), V^U(r_d)\Big) + (1-\alpha)V^U(r_d)\right]$$

$$V^U(r_d) = u(b + r_d) + (1-\rho)\left[\alpha Emax\Big(V^E(w, r_d) - V^U(r_d), 0\Big) + V^U(r_d)\right]$$

$$V^U(r_d) = u(b + r_d) + (1 - \rho)\left[\alpha Emax\Big(V^E(w, r_d) - V^U(r_d), 0\Big)\right] + (1 - \rho)V^U(r_d)$$

$$\rho V^U(r_d) = u(b + r_d) + (1 - \rho)\left[\alpha Emax\Big(V^E(w, r_d) - V^U(r_d), 0\Big)\right]$$

$$V^U(r_d) = \frac{u(b + r_d)}{\rho} + \frac{(1 - \rho)}{\rho}\left[\alpha Emax\Big(V^E(w, r_d) - V^U(r_d), 0\Big)\right] \qquad \text{(Appendix A.3)}$$

Appendix A.2 Rent Discount and Job Search

The worker's job search behavior can be captured by reservation wage which equalizes value of being unemployed and the value of being employed, that is

$$V^U(r_d) = V^E(w^*(r_d), r_d) \qquad \text{(Appendix A.4)}$$

where w^* denotes reservation wage and it is function of r_d. The reservation wage exists because the value of employment increases in the wage, whereas the value of unemployment does not. Using Equations Appendix A.3 and Appendix A.4, we have

$$V^E(w^*(r_d), r_d) = \frac{u(b + r_d)}{\rho} + \frac{(1 - \rho)}{\rho}\left[\alpha \int_{w^*(r_d)}^{\bar{w}} [V^E(w, r_d) - V^E(w^*(r_d), r_d)]dF(w)\right]$$

Inserting Equation Appendix A.1, we have

$$\frac{u(w^* + r_d)}{\rho} = \frac{u(b + r_d)}{\rho} + \frac{(1 - \rho)}{\rho}\left[\alpha \int_{w^*}^{\bar{w}} [\frac{u(w + r_d)}{\rho} - \frac{u(w^* + r_d)}{\rho}]dF(w)\right]$$

$$\underbrace{u(w^* + r_d) - u(b + r_d)}_{\text{cost of rejecting a job offer}} = \underbrace{\alpha\frac{(1 - \rho)}{\rho}\int_{w^*}^{\bar{w}} [u(w + r_d) - u(w^* + r_d)]dF(w)}_{\text{expected discounted benefit of rejecting a job offer}}$$

Thus, we derived the Equation 2 in the Section 3.2. Based on this equation, the following Lemmas and propositions demonstrates the effects of rent discount r_d on job search with proofs.

Lemma 1. *If the utility function $u(\cdot)$ is concave (the worker is risk averse), the effect of rent discount r_d on the reservation wage depends on the relative magnitude of $u'(b + r_d) - u'(w^*(r_d) + r_d)$ and $\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w)$. That is,*

$$\frac{\partial w^*(r_d)}{\partial r_d}\begin{cases} \geq 0, & if \alpha\frac{(1-\rho)}{\rho}\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w) \geq [u'(w^* + r_d) - u'(b + r_d)] \\ < 0, & if \alpha\frac{(1-\rho)}{\rho}\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w) < [u'(w^* + r_d) - u'(b + r_d)]. \end{cases} \qquad (3)$$

Proof. Let

$$h(w*, r_d) = \int_{w^*}^{\bar{w}} [u(w + r_d) - u(w^* + r_d)]dF(w),$$

apply Leibnitz's rule for differentiating functions with integrals, we have

$$\frac{\partial h(w*, r_d)}{\partial r_d} = \frac{\partial h(w*, rd)}{\partial w^*}\frac{\partial w^*(r_d)}{\partial r_d} + \frac{\partial h(w*, rd)}{\partial r_d},$$

where

$$\begin{aligned}\frac{\partial h(w*, rd)}{\partial w^*} &= -[u(w^* + r_d) - u(w^* + r_d)]\frac{\partial w^*(r_d)}{\partial r_d} + \int_{w^*}^{\bar{w}}[0 - u'(w^* + r_d)]dF(w) \\ &= -u'(w^* + r_d)[1 - F(w*)],\end{aligned}$$

and

$$\frac{\partial h(w*, rd)}{\partial r_d} = \int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w).$$

So we have

$$\frac{\partial h(w*, r_d)}{\partial r_d} = -u'(w^* + r_d)[1 - F(w*)]\frac{\partial w^*(r_d)}{\partial r_d} + \int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w).$$

Meanwhile, taking derivative with respect to r_d on both sides of Equation 2, we have

$$u'(w^*(r_d) + r_d)[1 + \frac{\partial w^*(r_d)}{\partial r_d}] - u'(b + r_d) = \alpha\frac{(1-\rho)}{\rho}\frac{\partial h(w*, rd)}{\partial r_d}.$$

Plugging the expression of $\frac{\partial h(w*, rd)}{\partial r_d}$ into the above equation, we have

$$\begin{aligned}&u'(w^*(r_d) + r_d)[1 + \frac{\partial w^*(r_d)}{\partial r_d}] - u'(b + r_d) = \\ &- \alpha\frac{(1-\rho)}{\rho}u'(w^* + r_d)(1 - F(w*))\frac{\partial w^*(r_d)}{\partial r_d} + \alpha\frac{(1-\rho)}{\rho}\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w),\end{aligned}$$

Rearrange the above equation, we have

$$\frac{\partial w^*(r_d)}{\partial r_d} = \frac{\frac{\alpha(1-\rho)}{\rho}\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w) - [u'(w^* + r_d) - u'(b + r_d)]}{u'(w^* + r_d)[1 + \alpha\frac{(1-\rho)}{\rho}(1 - F(w*))]} \quad \text{(Appendix A.5)}$$

where $u'(w^*(r_d) + r_d)$ is positive, so the sign of $\frac{\partial w^*(r_d)}{\partial r_d}$ depends on the the relative magnitude of reduction of costs and benefits, $u'(b + r_d) - u'(w^*(r_d) + r_d)$ and $\int_{w^*}^{\bar{w}}[u'(w + r_d) - u'(w^* + r_d)]dF(w)$. □

Lemma 2. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w+r_d) - u'(w^*+r_d) = 0$, the rent discount r_d increases the reservation wage; that is,*

$$\frac{\partial w^*(r_d)}{\partial r_d} > 0.$$

Proof. Plugging in $u'(w + r_d) - u'(w^* + r_d) = 0$ into Equation Appendix A.5, we have we have

$$\frac{\partial w^*(r_d)}{\partial r_d} = \frac{u'(b + r_d) - u'(w^*(r_d) + r_d)}{u'(w^*(r_d) + r_d)[1 + \alpha\frac{(1-\rho)}{\rho}(1 - F(w*))]}.$$

Since u is concave, we have the numerator $u'(b + r_d) - u'(w^*(r_d) + r_d) > 0$. Since u is a increasing function,

we have $u'(w^*(r_d)+r_d) > 0$ and the denominator is also positive, so we have

$$\frac{\partial w^*(r_d)}{\partial r_d} > 0.$$

□

Proposition 3. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w+r_d)-u'(w^*+r_d) = 0$, a rent-stabilized tenant has a higher reservation wage and is less likely to exit unemployment relative to a private market tenant.*

Proof. Since $r_{ds} > r_{dm} = 0$, according to Lemma 2, we have $w_s^* > w_m^*$. By definition, the exit rate from unemployment is the product of the arrival rate of job offer and the probability that the offer is accepted. So the hazard rate out of unemployment to a job for a rent-stabilized tenant is θ_s and the hazard rate to a job for a private market-rate tenant is θ_m:

$$\theta_s = \alpha[1 - F(w_s^*)],$$
$$\theta_m = \alpha[1 - F(w_m^*)].$$

It follows trivially that $F(w_s^*) > F(w_m^*)$ and $1 - F(w_s^*) < 1 - F(w_m^*)$, and that $\theta_s < \theta_m$. □

Proposition 4. *If the utility function $u(\cdot)$ is concave (the worker is risk averse) and $u'(w+r_d)-u'(w^*+r_d) = 0$, the rent-discount effect is greater when workers expect higher wages in the future. That is,*

$$\frac{\frac{\partial w^*(r_d)}{\partial r_d}}{\bar{w}} > 0.$$

Proof. Let

$$h(w*, \bar{w}) = \int_{w^*}^{\bar{w}} [u(w + r_d) - u(w^* + r_d)]dF(w),$$

apply Leibnitz's rule for differentiating functions with integrals and consider w^* as a function of $\bar{w}$ we have

$$\frac{\partial h(w*, \bar{w})}{\partial \bar{w}} = \frac{\partial h(w*, \bar{w})}{\partial w^*}\frac{\partial w^*}{\partial \bar{w}} + \frac{\partial h(w*, \bar{w})}{\partial \bar{w}},$$

where

$$\begin{aligned}\frac{\partial h(w*, \bar{w})}{\partial w^*} &= -[u(w^* + r_d) - u(w^* + r_d)]\frac{\partial w^*(r_d)}{\partial r_d} + \int_{w^*}^{\bar{w}} [0 - u'(w^* + r_d)]dF(w) \\ &= -u'(w^* + r_d)[1 - F(w*)],\end{aligned}$$

and

$$\frac{\partial h(w*, \bar{w})}{\partial \bar{w}} = u(\bar{w} + r_d) - u(w^* + r_d).$$

So we have

$$\frac{\partial h(w*, \bar{w})}{\partial \bar{w}} = -u'(w^* + r_d)[1 - F(w*)]\frac{\partial w^*}{\partial \bar{w}} + u(\bar{w} + r_d) - u(w^* + r_d).$$

Take derivative with respect to $\bar{w}$ on both sides of Equation 2, we have

$$u'(w^* + r_d)\frac{\partial w^*}{\partial \bar{w}} = \alpha\frac{(1-\rho)}{\rho}\frac{\partial h(w*, \bar{w})}{\partial \bar{w}}.$$

Plugging the expression of $\frac{\partial h(w*,\bar{w})}{\partial \bar{w}}$ into the above equation, we have

$$u'(w^*+r_d)\frac{\partial w^*}{\partial \bar{w}} = \alpha\frac{(1-\rho)}{\rho}[-u'(w^*+r_d)[1-F(w*)]\frac{\partial w^*}{\partial \bar{w}} + u(\bar{w}+r_d) - u(w^*+r_d)].$$

Rearrange the above equation, we have

$$\frac{\partial w^*}{\partial \bar{w}} = \frac{\alpha\frac{(1-\rho)}{\rho}[u(\bar{w}+r_d)-u(w^*+r_d)]}{u'(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}(1-F(w*))]} > 0.$$

Let

$$g(w^*) = \frac{\partial w^*(r_d)}{\partial r_d} = \frac{u'(b+r_d)-u'(w^*+r_d)}{u'(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}(1-F(w*))]}.$$

We have

$$\begin{aligned} g'(w^*) = & \frac{-u''(w^*+r_d)}{u'(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}(1-F(w*))]} \\ & - \frac{u'(b+r_d)-u'(w^*+r_d)}{\left[u'(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}(1-F(w*))]\right]^2}\left[u''(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}[(1-F(w*))-u'(w^*+r_d)F'(w^*)]]\right] \\ & > 0. \end{aligned}$$

Thus, we have

$$\frac{\frac{\partial w^*(r_d)}{\partial r_d}}{\bar{w}} = g'(w^*)\frac{\partial w^*}{\partial \bar{w}} > 0.$$

□

Proposition 5. *If the utility function $u(\cdot)$ is linear (the worker is risk neutral), the rent discount r_d has no impact on the job search; that is,*

$$\frac{\partial w^*(r_d)}{\partial r_d} = 0.$$

Proof. A constructive proof for this can be obtained conditional on a simple function form assumption. Suppose $u(x) = a_1x + a_2$ where x is the disposable income, we have

$$u'(w^*+r_d) - u'(b+r_d) = 0,$$

and

$$u'(w+r_d) - u'(w^*+r_d) = 0.$$

Plugging in Equation Appendix A.5, we have

$$\frac{\partial w^*(r_d)}{\partial r_d} = \frac{\alpha\frac{(1-\rho)}{\rho}\int_{w^*}^{\bar{w}}[u'(w+r_d)-u'(w^*+r_d)]dF(w) - [u'(w^*+r_d)-u'(b+r_d)]}{u'(w^*+r_d)[1+\alpha\frac{(1-\rho)}{\rho}(1-F(w*))]} = 0.$$

That is, r_d has no impact on job search outcomes. □

Appendix A.3 Numerical Simulation

Our propositions so far rely on the assumption that the effect of rent discount on utility is negligible when the worker is employed, that is

$$u'(w + r_d) - u'(w^* + r_d) = 0.$$

In this section, we relax this assumption and demonstrate that predictions from our propositions can still hold using numerical simulations. In particular, we solve our model numerically and use simulations to demonstrate the potential mechanisms behind the unemployment effect and how this effect varies with three factors: the size of rent discount level, risk aversion level, and wage parameters.

To get the numerical solution of the model, we assume the utility function to be a constant relative risk aversion (CRRA) utility function:

$$u(c) = \frac{c^{1-\delta}}{1-\delta} \tag{Appendix A.6}$$

We assume the wage distribution to be beta-binominal with $n = 100$ and $a = b = 10$. In addition, we set discount rate $\rho = 0.02$, offer arrive rate $\alpha = 0.7$, unemployment benefits $b = 5$, rent discount $r_d = 5$, and risk averse parameter $\delta = 3$ as our baseline parameter values.

Figure A1 shows the difference between the effects of rent discount on employed and unemployed value functions. It shows when rent discount r_d increases from 0 to 2, value function V^E (in blue) and V^U (in green) both increased. But V^U increases to a higher level, that is, $\partial V^U(r_d)/\partial r_d > \partial V^E(w^*, r_d)/\partial r_d$. Consistent with Lemma 2, reservation wage w^* (the interaction of V^U and V^E) also increased. That is, rent discount increases the value of unemployed more than the value of employed resulting in an increase in reservation wage (the interactions of the dashed green line and the dashed blue line is to the right of the interactions of the sold green line and the solid blue line).

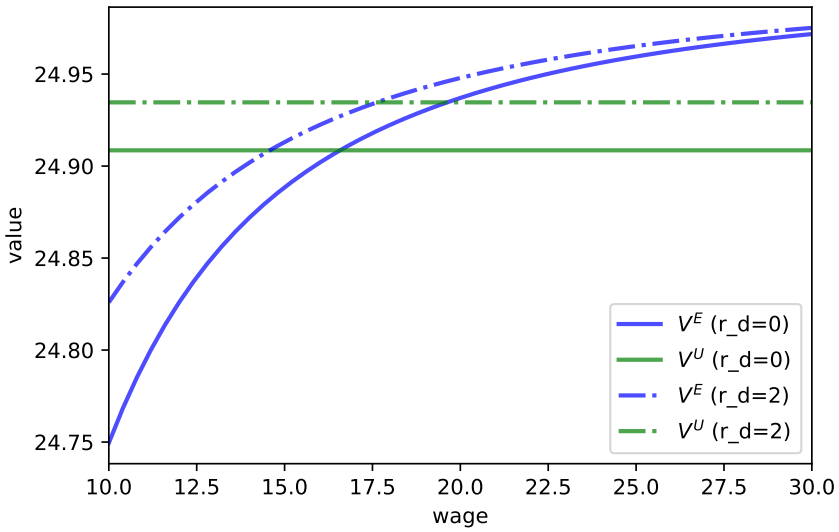
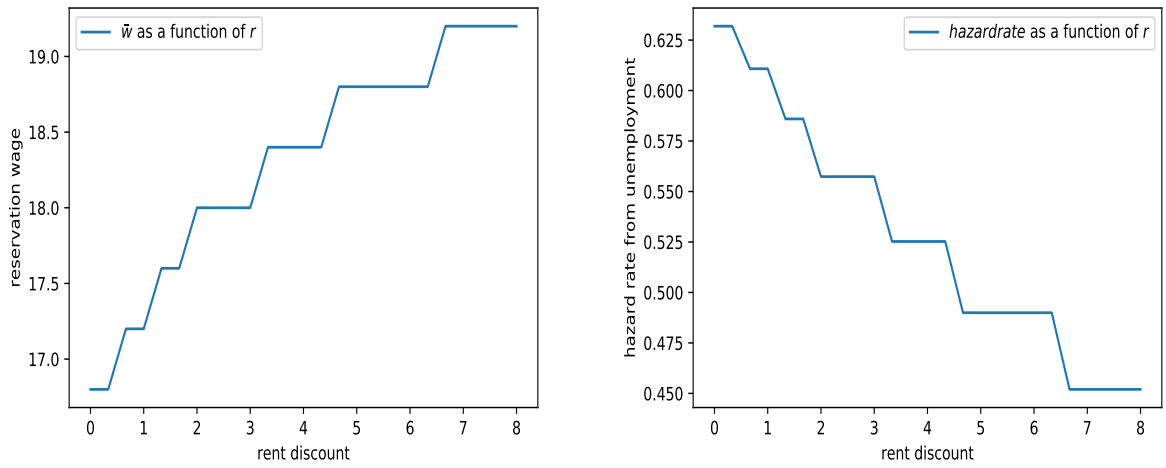


Figure A1: Rent discounts and value functions

Appendix A.3.1 Size of Rent Discount

Figure A2 shows that the reservation wage $w*$ increases with r_d (panel a). Accordingly, the hazard rates out of employment decrease with r_d (panel b). This figure is consistent with the scenario described in Propositions 3 that rent discounts increases reservation wage and decrease hazard rate out of unemployment. Our empirical results showing rent stabilized renters ($r_d > 0$) are more likely to be unemployed than private market renters ($r_d = 0$) are consistent with these simulations in Figure A2.



(a) (b)

Figure A2: Rent discount, reservation wage, and hazard rate out of unemployment

Appendix A.3.2 Wage Distribution

This section is motivated by the fact that the effect of rent discount is stronger among high-skilled workers than among low-skilled workers. Considering that high-skilled workers have a higher probability of getting high-wage jobs, we assume high-skilled workers have a wage distribution skewed to the right with a larger means and a larger variance as presented as the ones simulated in Figure A3 (orange line). These two distributions are consistent with the observed wage distributions in the data as in Figure E9.

Figure A4 shows that the effect of r_d is more evident among high-skilled workers (orange line). Differences in the shapes of job offer distributions can be a key diver here. For low-skilled workers, the probability of getting a better job offer in the future is low, so there are few benefits for them to stay unemployed no matter the rents are low or high. Thus, the effects of rent discounts are small. On the contrary, for high-skilled workers, the possible benefits of waiting are high and rent discounts can make it even higher. This explains why we observe high-skilled workers living in rent-stabilized units are more likely to stay unemployed compared with low-skilled workers.

Appendix A.3.3 Risk Aversion

Figure A5 shows that r_d has no effect on reservation wage or hazard rate when $\delta = 0$, the worker is risk-neutral (the blue line is not changing in responding to rent discount), which are consistent with proposition

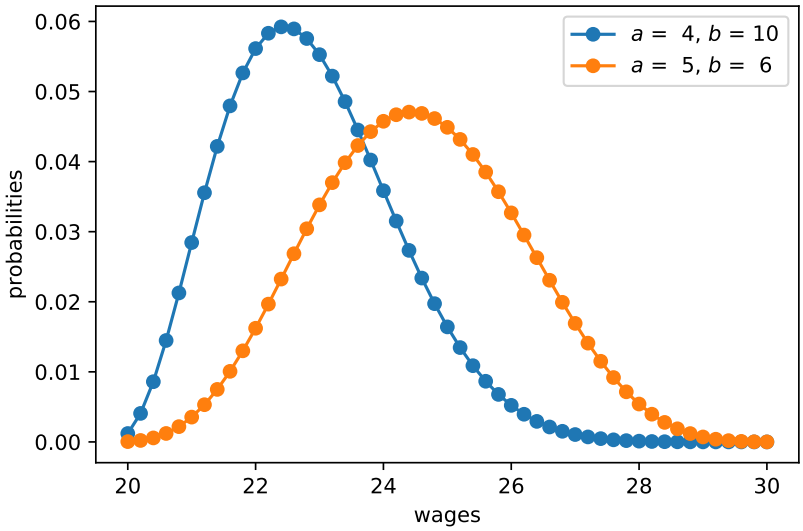
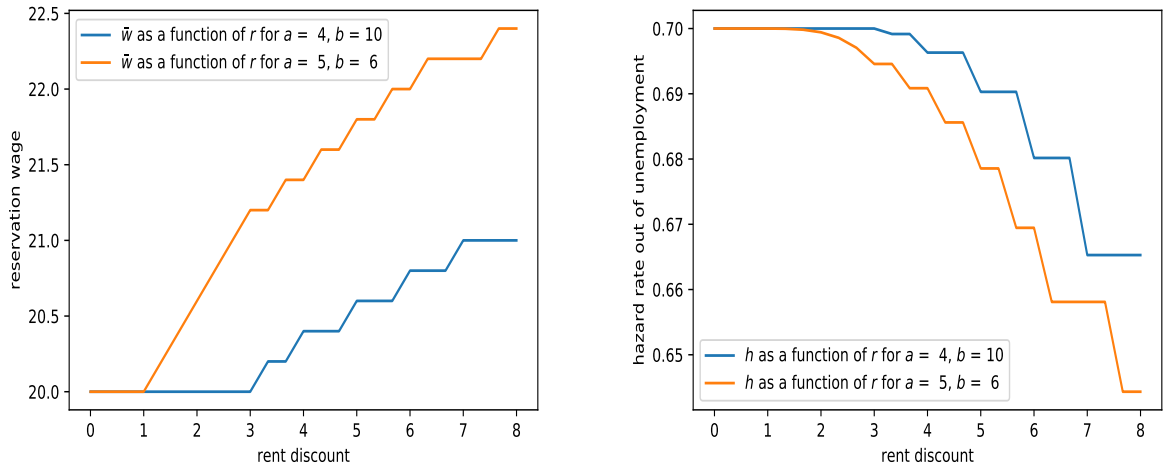


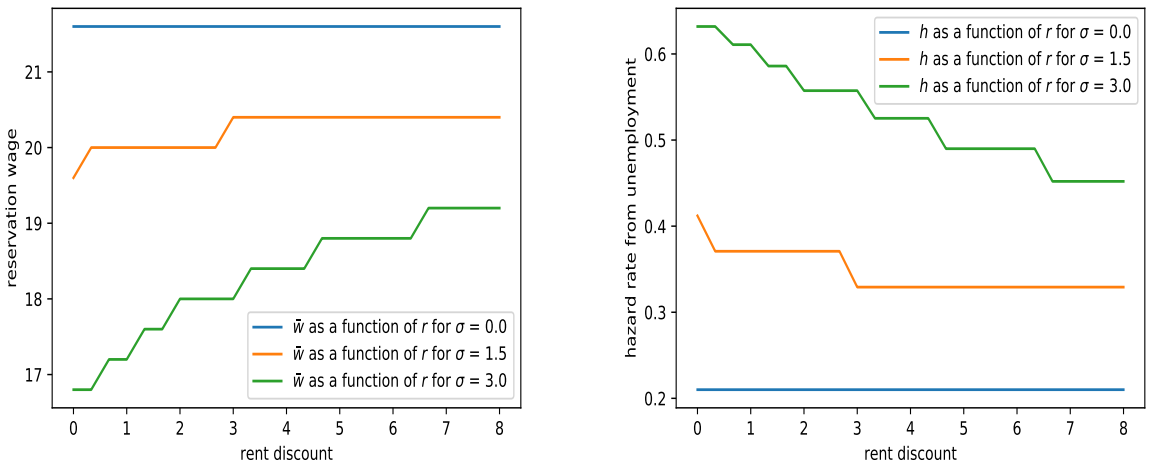
Figure A3: High and low skilled wage distributions



(a) (b)

Figure A4: Different wage distributions

5. Also, this figure shows that unemployment effect is greater when δ is larger.



(a) (b)

Figure A5: Different risk-averse parameters

Appendix B The Rent Stabilization in NYC

Table B1: Rent Stabilization v.s. Hard Price Rent Control (HPRC) in NYC

	Rent Stabilization	Rent Control
Enact Time	1969	1943
Current Stock (2017)	966,000 out of 2,183,064 (44%)	22,000 of 2,183,064 (1%)
Rent Increase Operation	Rent Guidelines Board (RGB)	Maximum Base Rent (MBR)
Succession Right	Yes	Yes
Criterion 1: Construction Time	(1) Between 2/1/1947 and 1/1/1974 (2) Post-1974 units with tax benefits	Only before 2/1/1947
Criterion 2: Number of Units	Mainly ≥ 6	Mainly ≥ 3
Criterion 3: Move-in Time	Move-in after 6/30/1971 for units built before 2/1/1947	Move-in before 7/1/1971
Vacancy De-regulation	(1) Deregulation if *exceeds* High-Rent threshold (2) "Vacancy Bonus" if *below* High-Rent threshold	(1) Deregulation if < 6 units (2) Become rent stabilized if ≥ 6 units
Other De-regulation	(1) Post Condo or Co-op conversion (2) Exceeds High-Income threshold (3) Tax benefit expires	

Notes: Authors' own summary based on legal regulation documents from NYC Rent Guidelines Board.

Table B2: Shares of Different Types of Rental Units by Borough

	New York City		Bronx		Brooklyn		Manhattan		Queens		Staten Island	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Controlled	0.019	0.14	0.009	0.09	0.015	0.12	0.033	0.18	0.014	0.12	0.000	0.00
Stabilized	0.460	0.50	0.569	0.50	0.424	0.49	0.467	0.50	0.447	0.50	0.148	0.36
Public Housing	0.085	0.28	0.116	0.32	0.094	0.29	0.088	0.28	0.039	0.19	0.062	0.24
Market Rental	0.347	0.48	0.205	0.40	0.401	0.49	0.264	0.44	0.470	0.50	0.716	0.45
N	52855		9558		15736		15728		10412		1421	

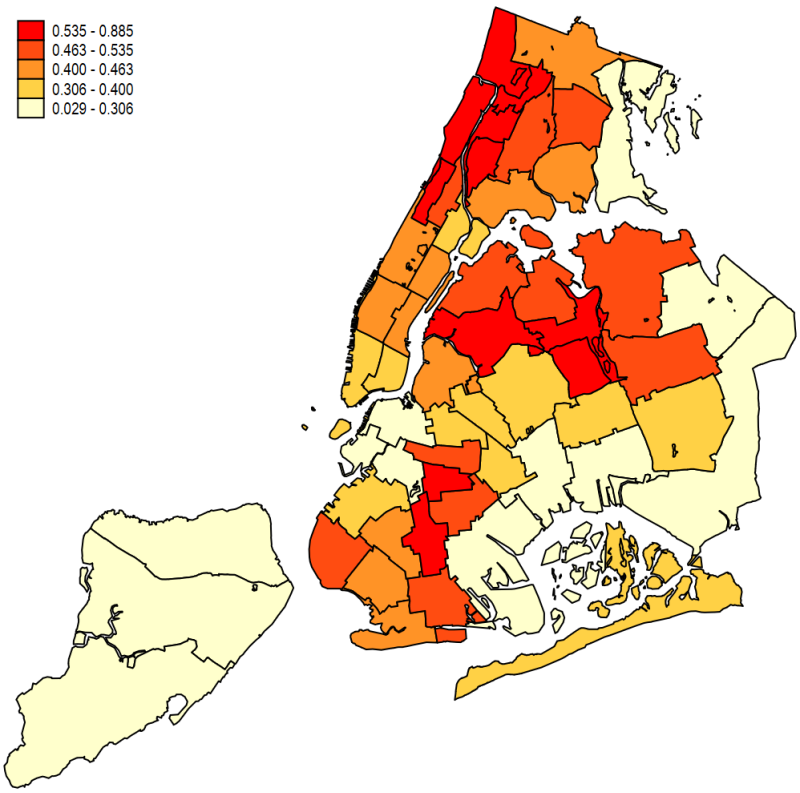
Notes: Data come from the 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS data and include all rental units. Other rental units not shown in the table include Article 4 or 5 building, HUD regulated, Loft Board regulated building, Michell Lama rental, and In Rem. Technical details about these other rental units are available from NYC Rent Guuidelines Board.

Table B3: Permitted Annual Increase of Rent Stabilized Apartments (1978-2020)

Renewal Leases Time	One Year Lease	Two Year Lease
10/1/19 to 9/30/20	1.5%	2.5%
10/1/18 to 9/30/19	1.5%	2.5%
10/1/17 to 9/30/18	1.25%	2%
10/1/16 to 9/30/17	0%	2%
10/1/15 to 9/30/16	0%	2%
10/1/14 to 9/30/15	1%	2.75%
10/1/13 to 9/30/14	4%	7.75%
10/1/12 to 9/30/13	2%	4%
10/1/11 to 9/30/12	3.75%	7.25%
10/1/10 to 9/30/11	2.25%	4.5%
10/1/09 to 9/30/10	3%	6%
10/1/08 to 9/30/09	4.5%	8.5%
10/1/07 to 9/30/08	3%	5.75%
10/1/06 to 9/30/07	4.25%	7.25%
10/1/05 to 9/30/06	2.75%	5.5%
10/1/04 to 9/30/05	3.5%	6.5%
10/1/03 to 9/30/04	4.5%	7.5%
10/1/02 to 9/30/03	2%	4%
10/1/01 to 9/30/02	4%	6%
10/1/00 to 9/30/01	4%	6%
10/1/99 to 9/30/00	2%	4%
10/1/98 to 9/30/99	2%	4%
10/1/97 to 9/30/98	2%	4%
10/1/96 to 9/30/97	5%	7%
10/1/95 to 9/30/96	2%	4%
10/1/94 to 9/30/95	2%	4%
10/1/93 to 9/30/94	3%	5%
10/1/92 to 9/30/93	3%	5%
10/1/91 to 9/30/92	4%	6.5%
10/1/90 to 9/30/91	4.5%	7%
10/1/89 to 9/30/90	5.5%	9%
10/1/88 to 9/30/89	6%	9%
10/1/87 to 9/30/88	3%	6.5%
10/1/86 to 9/30/87	6%	9%
10/1/85 to 9/30/86	4%	6.5%
10/1/84 to 9/30/85	6%	9%
10/1/83 to 9/30/84	4%	7%
10/1/82 to 9/30/83	4%	7%
10/1/81 to 9/30/82	10%	13%
7/1/80 to 6/30/81	11%	14%
7/1/79 to 6/30/80	8.5%	12%
7/1/78 to 6/30/79	2.5%	2%

Notes: Data come from NYC Rent Guidelines Board.

Figure B6: Share of Rent Stabilized Units in NYC Rental Market (2002-2017)



Notes: Authors' own calculation based on 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. The map shows the percentage of rent-stabilized units among all rental units in NYC.

How to Find a Rent Stabilized Apartment?

Now days, there are different web sites containing tips to help prospective tenants to find a rent stabilized apartment. We paste from some most well-known ones to better understand the self-selection problem.

↪ From The New York Times[68]:

1. **Know what to look for.** Rent stabilized units are mostly in buildings that were built before 1974, have six or more units, have not been previously deregulated, and are not a co-op or condo.
2. **Search smart.** Do an advanced search using the keywords "rent stabilized" in real estate listing sites like RentHop, StreetEasy, Zillow or Craigslist. If you save a search on some sites, you'll receive a daily email with new listings that match your criteria.
3. **Zero in on odd prices...** "Look for a funny price point, like $1993.64 instead of $2,000," said Douglas Wagner, director of brokerage at Bond New York. "Anything with change attached to it at a lower price range is probably old-school rent stabilized."
4. **... But ignore listings over $2,733.75.** That's the current price when an apartment can be deregulated.
5. **Search city data.** The Rent Guidelines Board has listings of the city's RS buildings. If you think a building might have rent stabilized units, you can look it up here, or call Homes and Community Renewal.
6. **Schmooze everyone.** "Meet your broker in person and make friends with them," Mr. Wagner said. "Brokers tend to respond to people who respond to them," he added, and will have you in mind when a new listing becomes available. Talk to doormen, longtime neighborhood residents, and friends and co-workers who live in RS apartments, said Neeta Mulgaokar, an associate broker at Mirador Real Estate. Contact their landlords and follow up with them regularly.
7. **Give yourself time.** Begin your search six to eight weeks before you will move, said Eva Nowakowski, a real estate saleswoman at Citwe Habitats. And move quickly. Below-market RS apartments are often snatched up the same day they are listed.
8. **Don't move.** Once you find a place, you can stay as long as you want. As long as you pay rent and follow the rules, you're guaranteed a renewal of the lease.

↪ From Curbed New York[69]:

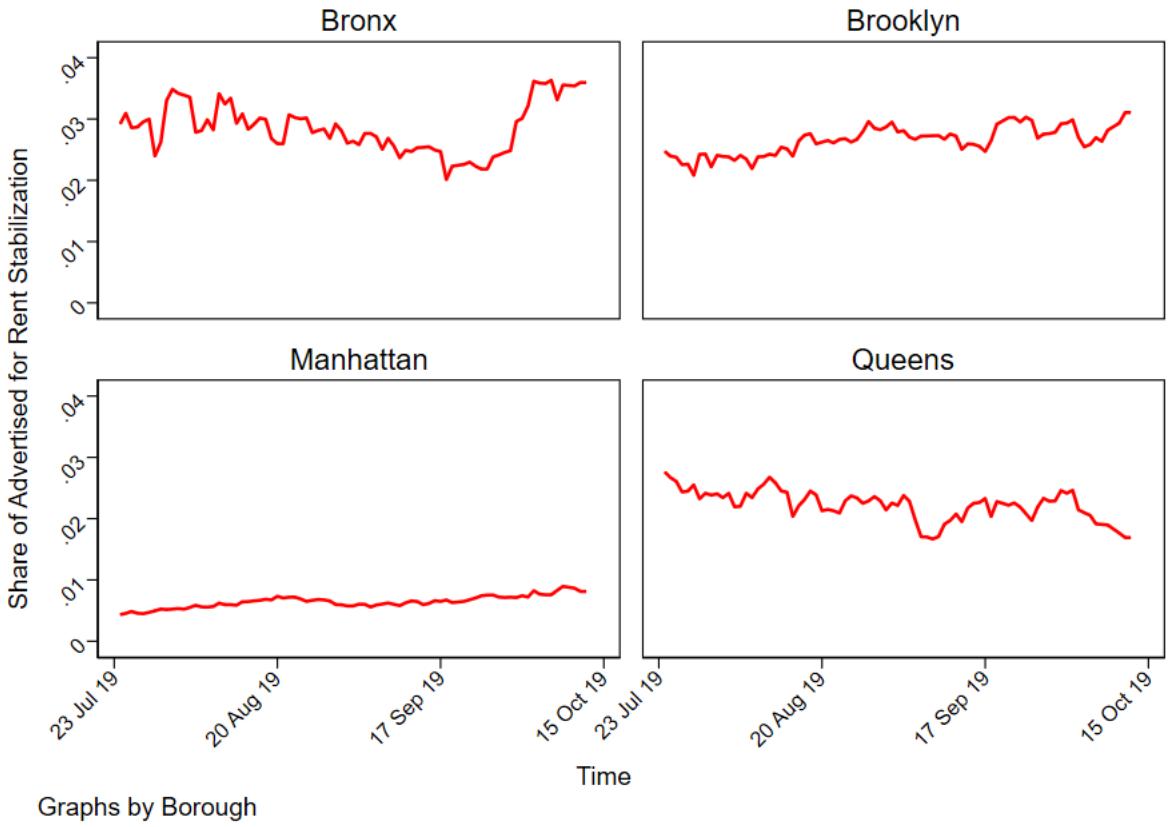
1. **Search official databases**: The Rent Guidelines Board maintains a list of all buildings registered with the DHCR. It's a fantastic resource, but the PDF files are unruly. They're broken out into borough, then sorted by zip code, but they are still huge. (The document for Brooklyn is 339 pages.) However, the document does note if a building is stabilized because of a tax abatement, which is useful. Alternatively, you can search by address on DHCR's website, so if you spot a listing that you think might be rent stabilized, you can look it up. One important thing to note: Neither of these resources says which units in these buildings remain stabilized.
2. **Go where the stabilized units are** : The data wizards at NYU's Furman Center For Real Estate and Urban Policy released a report in January 2015 charting where New York's subsidized housing is located. It shows the highest concentrations to be in Upper Manhattan (Harlem), the South Bronx, and central Brooklyn (Crown Heights and Prospect-Lefferts Gardens), so these areas are a good place to start your search. Use the tips suggested above to narrow the results in these specific areas.

[68]Source: `https://www.nytimes.com/2018/04/17/nyregion/new-york-today-how-to-find-a-RS-apartment.html`

[69]Source: `https://ny.curbed.com/2017/4/13/15264890/nyc-apartments-guide-tips-new-york`

3. **Narrow your search criteria**: Since most buildings constructed between February 1, 1947 and January 1, 1974 contains RS apartments, hone in on older buildings. Zillow lets you search by the date the building was constructed, and a recent search shows more than 1,000 listings. But not all of these will be stabilized, so your best bet is to cross-check with the databases referenced above.
4. **Literally search for rent stabilized apartments**: On StreetEasy, searching for rentals with the keyword "stabilized" brings up close to 200 options. Ditto Craigslist, although the usual caveats when searching for apartments on that platform apply. Point being, you can use the keyword "rent stabilized" or some combination thereof to narrow your options when searching.

Figure B7: Share of Online Postings with Rent Stabilization Advertisement



Notes: Data are obtained by author's manual collection from `StreetEasy` website (`https://streeteasy.com`) during the specified period. An online posting is considered to have rent stabilization advertisement if any of these word appear in the description section: "rent stabilization", "rent stabilized", "stabilized", etc. Historical New York City Housing and Vacancy Survey (NYCHVS) data suggests that at least 25% of total vacant-for-rent units are rent stabilized. This suggests only a small minority of actual rent stabilized, vacant apartments are advertised for their stabilization status.

Appendix C The NYC Housing and Vacancy Survey (NYCHVS)

Appendix C.1 Construction of Analytic Sample

To construct the analytic sample for empirical analysis, we follow these restrictions:

1. Only rental housing units that are either rent stabilized or private market-rate are kept. This excludes rent-controlled, public housing, and other subsidized rental units, as well as the owner-occupied units.
2. Households where household heads are younger than age 26 or older than 54 are excluded.[70]
3. Households benefiting from any federal, state, or city housing subsidy programs, such as the federal Section 8 housing choice voucher program, are excluded.[71]
4. Households where the household heads' nominal non-labor incomes are greater than $100,000 are excluded.
5. Households that have lived in their current units for less than one year are excluded to alleviate simultaneity bias concerns. Results are robust to including households that moved less than one year ago as well.
6. Households that moved into their current housing units before 1978, due to data limitations, are excluded.[72]

Table C4: Sample Size

Steps	Description	Sample Size
1	Keep only rent stabilized and private market-rate units	42,656
2	Exclude householders who are younger than age 26 or older than 54	28,913
3	Exclude householders who benefit from any housing subsidy programs	26,193
4	Exclude householders whose non-labor incomes are greater than $100,000	25,956
5	Exclude householders who have lived for less than 1 year	24,686
6	Exclude householders who have moved in the current dwelling before 1978	24,210
7	Exclude householders who currently participate in the labor force	21,906

Notes: The data come from the 2002, 2005, 2008, 2011, 2014, and 2017 waves of NYCHVS.

Appendix C.2 Summary Statistics

[70] Empirical results are robust if the upper bound of the age range is extended to 64.

[71] We thank Ingrid Ellen for suggesting this point.

[72] The earliest NYCHVS survey available was carried out in 1978. Since our instrumental variables are constructed using vacancy rates and rent regulation information at the time of move-in, 1978 is the earliest year of move-in that satisfies our research design. This step makes our final sample size 24,210.

Table C5: Summary Statistics of Household Characteristics

	Rent Stabilized				Market Rental			
	mean	sd	min	max	mean	sd	min	max
Currently Unemployed	0.052	0.223	0.00	1.00	0.041	0.198	0.00	1.00
Female	0.479	0.500	0.00	1.00	0.431	0.495	0.00	1.00
Age	38.965	7.872	26.00	54.00	37.864	7.796	26.00	54.00
Black	0.221	0.415	0.00	1.00	0.203	0.403	0.00	1.00
Hispanic	0.313	0.464	0.00	1.00	0.209	0.407	0.00	1.00
Asian	0.111	0.314	0.00	1.00	0.137	0.343	0.00	1.00
Married	0.349	0.477	0.00	1.00	0.406	0.491	0.00	1.00
High School Dropout	0.137	0.343	0.00	1.00	0.102	0.303	0.00	1.00
High School Graduate	0.221	0.415	0.00	1.00	0.208	0.406	0.00	1.00
Non-labor Income	0.260	0.988	0.00	8.97	0.299	1.077	0.00	8.97
Spouse: Black	0.176	0.381	0.00	1.00	0.161	0.367	0.00	1.00
Spouse: Hispanic	0.363	0.481	0.00	1.00	0.243	0.429	0.00	1.00
Spouse: Asian	0.180	0.384	0.00	1.00	0.197	0.398	0.00	1.00
Spouse: High School Dropout	0.188	0.391	0.00	1.00	0.137	0.344	0.00	1.00
Spouse: High School Graduates	0.287	0.452	0.00	1.00	0.269	0.444	0.00	1.00
Spouse: Unemployed	0.045	0.207	0.00	1.00	0.031	0.174	0.00	1.00
Any Child in Household	0.345	0.475	0.00	1.00	0.368	0.482	0.00	1.00
Household Size	2.375	1.393	1.00	11.00	2.556	1.493	1.00	13.00
Other Household Member Total Income	2.171	3.701	0.00	32.01	3.182	5.049	0.00	32.01

Notes: The data come from 2002, 2005, 2008, 2011, 2014, and 2017 waves of NYCHVS. All the monetary values are in 2017 real dollar and are in $10,000.

Table C6: Summary Statistics of Housing Characteristics

	Rent Stabilized				Market Rental			
	mean	sd	min	max	mean	sd	min	max
Monthly Contract Rent	1.286	0.570	0.02	4.70	1.753	0.915	0.07	4.70
Monthly Gross Rent	1.395	0.599	0.14	7.55	1.918	1.047	0.17	7.55
Tenure of Residence	7.060	6.295	1.00	38.00	4.813	4.764	1.00	38.00
Num. of Rooms	3.158	1.148	1.00	8.00	3.740	1.347	1.00	8.00
Num. of Bedrooms	1.337	0.800	0.00	7.00	1.763	0.938	0.00	8.00
Number of Unit Problems	0.655	0.912	0.00	5.00	0.331	0.662	0.00	4.00
Number of Building Problems	0.112	0.392	0.00	3.00	0.086	0.357	0.00	3.00
Neighborhood Rate: Fair	0.211	0.408	0.00	1.00	0.118	0.323	0.00	1.00
Neighborhood Rate: Poor	0.039	0.194	0.00	1.00	0.018	0.132	0.00	1.00

Notes: The data come from 2002, 2005, 2008, 2011, 2014, and 2017 waves of NYCHVS. All the monetary values are in 2017 real dollar. Rent-related variables are in $1,000. Unit problems include toilet breakdown, heating equipment breakdown, presence of mice and rats, cracks or holes in interior walls, holes in floors, broken plaster or peeling paint on inside walls, water leakage, etc. Building problems include issues related to external walls, building windows, stairways, floors, etc.

Appendix D Additional Tables

Table D7: Reasons for Moving to Current the Housing

	Rent Stabilized	Market-Rate	Difference
Reverse-Causation Related	0.217	0.221	-0.004
Education Related	0.027	0.022	0.005
Demographic Related	0.134	0.142	-0.009
Neighborhood Related	0.105	0.122	-0.018***
Housing Related	0.235	0.229	0.006
Displacement, Eviction, and Disaster	0.014	0.014	-0.000
Other	0.017	0.014	0.003
Not Reported	0.251	0.234	0.017**

Notes: "Reverse-Causation Related" reasons are mainly related to labor market outcomes and incomes, such as job transfer/new job, looking for work, commuting reasons, wanted less expensive residence, and other reasons potentially related to employment and income. "Education Related" reasons include attend schools. "Demographic Related" reasons include widowed, separated/divorced, newly married, moved to be with or closer to relatives, and other family reasons. "Neighborhood Related" reasons include neighborhood overcrowded, change in racial or ethnic composition of neighborhood, wanted better neighborhood services, crime or safety concerns, and other neighborhood reasons. "Housing Related" reasons include wanted to own residence, wanted to rent residence, wanted better quality residence, poor building condition/services, needed housing accessible for persons with mobility impairments, and other housing reasons. "Displacement, Eviction, and Disaster" related reasons include evicted, harassment by landlord, displaced by urban renewal/highway construction/other public activity, displaced by private action, natural disaster/fire. This question is only asked for tenants who moved to their current dwelling in the past three years.

Table D8: First Stage of Instrumental Variable Estimation by subgroups

	Panel A: By Tenure				Panel B: By Education			
	Short Tenure		Long Tenure		High Skill		Low Skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IV	0.468**	0.369**	0.576**	0.477**	0.967***	0.756***	0.476***	0.322*
	(0.165)	(0.126)	(0.137)	(0.158)	(0.148)	(0.088)	(0.087)	(0.124)
$\overline{Y}$	0.467	0.467	0.652	0.652	0.496	0.496	0.556	0.556
N	14739	14739	7167	7167	10433	10433	11473	11473
R^2	0.178	0.592	0.246	0.765	0.173	0.534	0.253	0.764
F Statistics	7.775	8.487	18.468	9.080	42.293	75.058	30.483	6.682
	Panel C: By Race				Panel D: By Age			
	White		Non-white		Younger		Older	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
IV	1.067***	0.811***	0.496***	0.377**	0.725***	0.588***	0.858***	0.608**
	(0.133)	(0.093)	(0.098)	(0.109)	(0.154)	(0.117)	(0.163)	(0.158)
$\overline{Y}$	0.468	0.468	0.566	0.566	0.504	0.504	0.566	0.566
N	8581	8581	13325	13325	13552	13552	8354	8354
R^2	0.166	0.548	0.244	0.720	0.193	0.609	0.240	0.718
F Statistics	66.795	75.117	26.462	11.972	22.654	25.185	29.748	14.615
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenant controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: The data come from 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. In Panel A, the short tenure (long tenure) subgroup includes tenants who live less than or equal to 6 years (more than 6 years) in their current dwellings. The average housing tenure is 6 year in our sample. In Panel B, the high skill (low skill) subgroup includes tenants who have at least college degrees (lower than college degrees). In Panel D, the younger (older) subgroup includes tenants who are younger or equal to age 40 (older than age 40). Each column has sub-borough and survey year fixed effects. Tenant controls and housing and neighborhood controls are defined in the same way as in Table 5. Standard errors are in parentheses and clustered at borough level, which is consistent with the geographic level of our instrumental variable. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

Table D9: First Stage of Instrumental Variable Estimation Robustness Check

	Panel A: Test by Propensity Score			
	Lower 50%		Higher 50%	
	(1)	(2)	(3)	(4)
IV	0.347	0.207	0.856**	0.511**
	(0.201)	(0.209)	(0.223)	(0.114)
$\overline{Y}$	0.437	0.179	0.642	0.900
N	9138	9140	9139	9139
R^2	0.140	0.452	0.206	0.160
	Panel B: By Reversing Sample			
	IV ≥3 Rooms		IV ≥4 Rooms	
	(1)	(2)	(3)	(4)
IV (Reversed Sample)	0.829***	0.623***	0.689**	0.461*
	(0.080)	(0.058)	(0.243)	(0.195)
$\overline{Y}$	0.648	0.648	0.637	0.637
N	3595	3595	9796	9796
R^2	0.182	0.532	0.171	0.589
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes
Tenant Controls	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes

Note: The data come from 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. In Panel A, the subsample division of first stage analysis is based on the distribution of the propensity to be rent-stabilized. The propensity is estimated as the probability of being a rent-stabilized unit, calculated using an OLS regression on the characteristics included in above model. In Panel B, the reverse-sample instrument analysis as in Arni and Schiprowski (2019) is conducted, where the instrument is redefined to be the market tightness of large units and use it as an instrument for the unit outside this subsample, and vice-versa. Each column has sub-borough and survey year fixed effects. Tenant controls and housing and neighborhood controls are defined in the same way as in Table 5. Standard errors are in parentheses and clustered at borough level, which is consistent with the geographic level of our instrumental variable. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

Table D10: Rent Stabilization, Policy Awareness, and Tenant Unemployment (OLS)

	Aware		Unaware	
	(1)	(2)	(3)	(4)
Rent Stabilized	0.019	0.033	0.007	0.017
	(0.014)	(0.017)	(0.009)	(0.012)
$\overline{Y}$	0.064	0.064	0.060	0.060
N	3786	3786	4308	4308
R^2	0.113	0.133	0.132	0.147
Sub-borough and Year Fixed Effects	Yes	Yes	Yes	Yes
Tenant controls	Yes	Yes	Yes	Yes
Housing and Neighborhood Controls	No	Yes	No	Yes

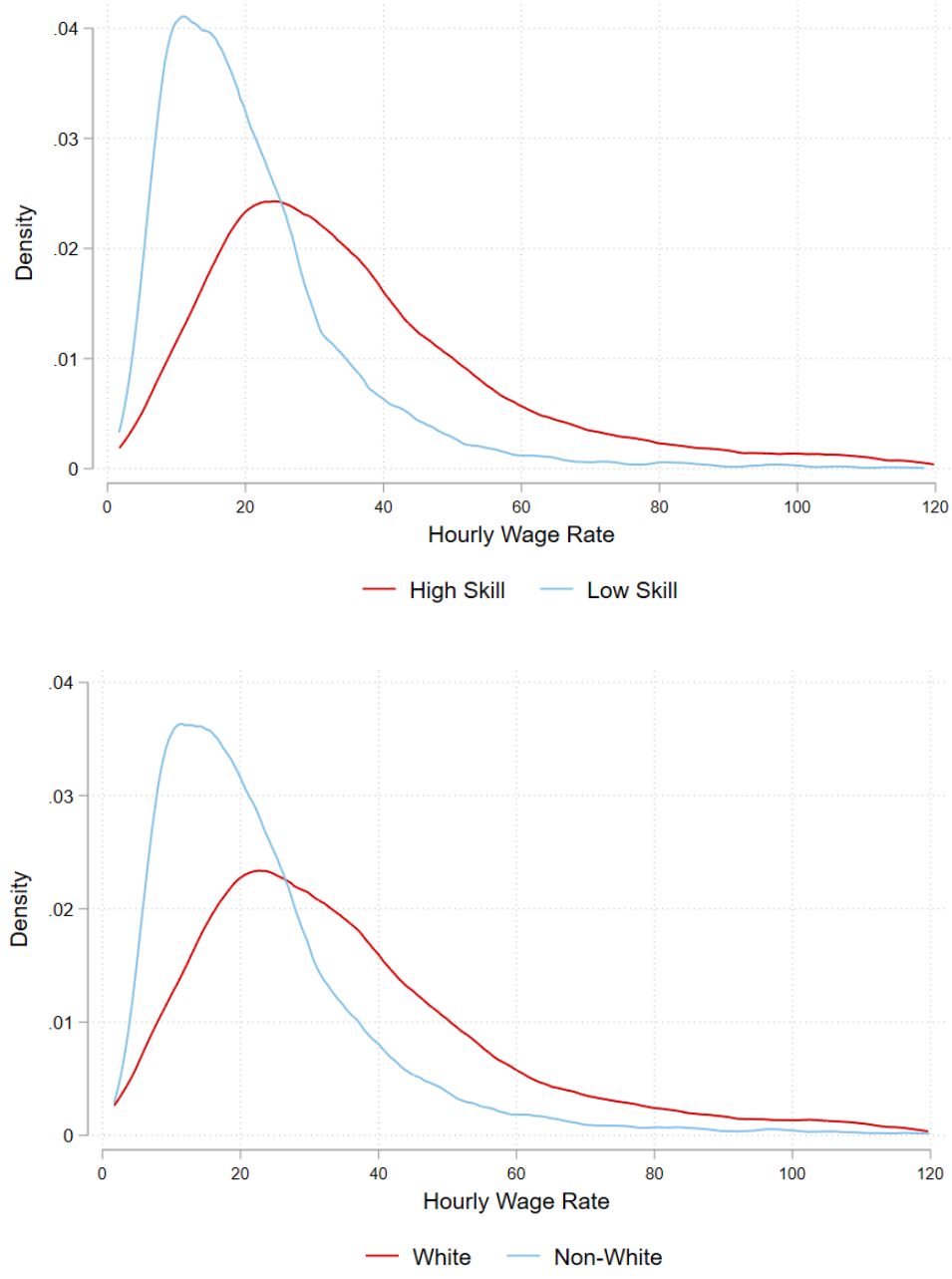
Notes: The data only come from 2002 and 2005 waves of NYCHVS, because the question about self-reported rent regulation status is not available starting from the 2008 wave and onward. "Aware" refers to tenants who live in rent stabilized units and claimed their housing units to be either "rent controlled" or "rent stabilized" when asked to self report the housing regulation status. "Unaware" refers to tenants who live in rent stabilization units but claimed their housing units to be either "unregulated" or "don't know" when asked to self report the housing regulation status. The control group refers to tenants who live in unregulated, private market-rate units and correctly claim their housing units to be "unregulated" when asked to self report the housing regulation status. Each column has sub-borough and survey year fixed effects. Tenant controls and housing and neighborhood controls are defined in the same way as in Table 5 Standard errors are in parentheses and clustered at borough level, which is consistent with the geographic level of our instrumental variable. * p<0.10, ** p<0.05, *** p<0.01. Full results containing coefficients of other variables are available upon request.

Appendix E Additional Figures

Figure E8: Average Rent Comparison in New York City

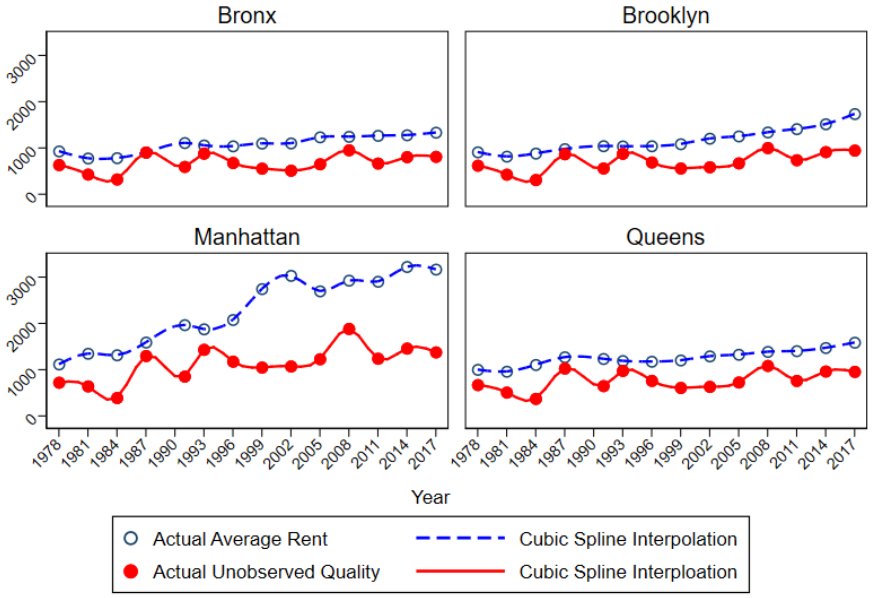
Notes: The data come from 1978 - 2017 waves NYCHVS. Rent is measured as average monthly contract rent in 2017 $1,000 real value. The solid line denotes the average monthly contract rent for RS units while the dashed line denotes the one for private market-rate units.

Figure E9: Wage Distribution by Education and Wage



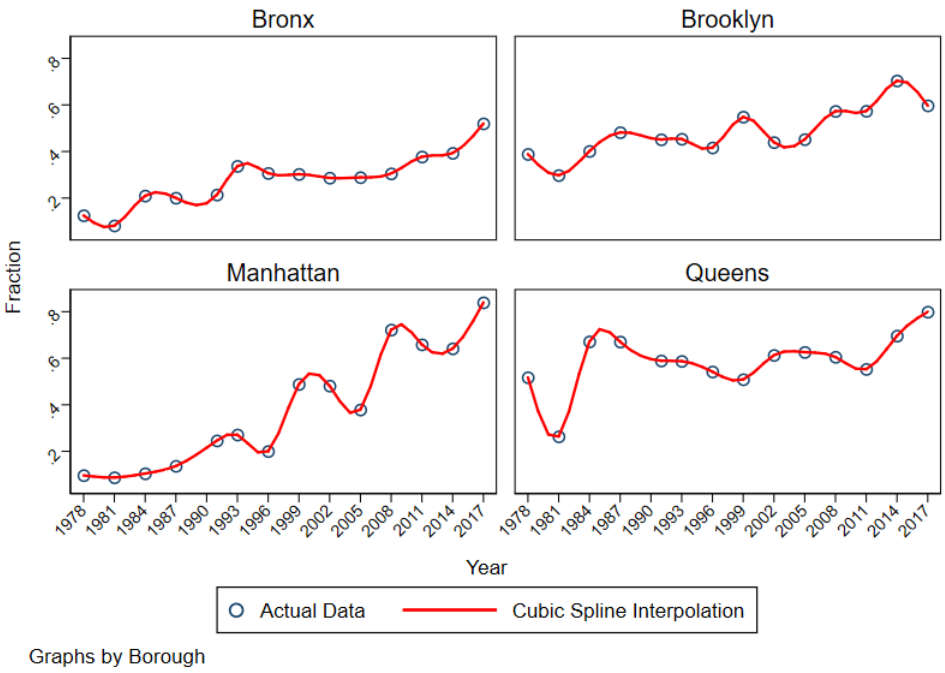
Notes: Authors' calculation based on pooled 2002, 2005, 2008, 2011, and 2017 waves of NYCHVS. Hourly wage rate is calculated as total labor income divided by total annual hours of work. Total annual hours of work is calculated as the product between annual weeks of work and average weekly hours of work.

Figure E10: Estimated Neighborhood Quality by Boroughs



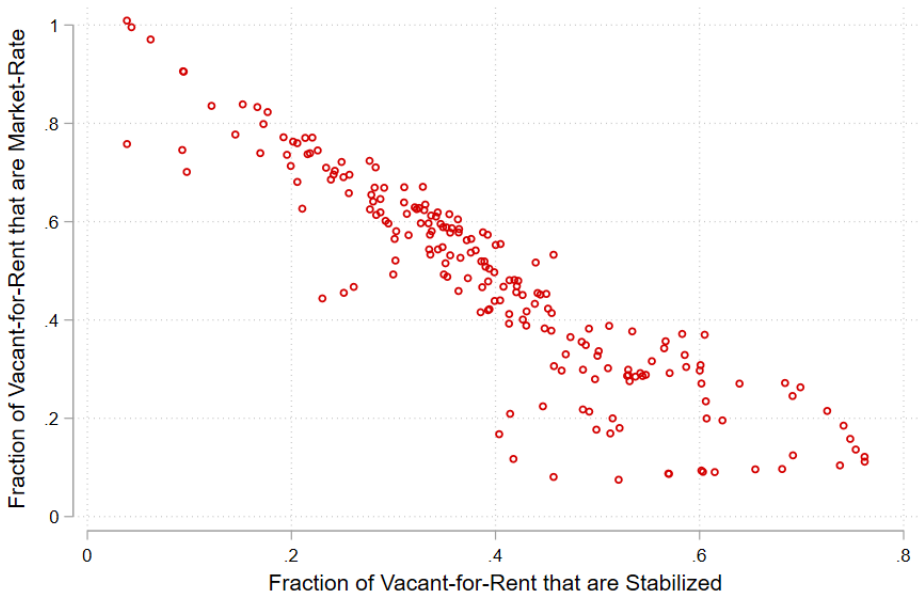
Notes: Y-axis is measured in terms of 2017 U.S. dollar value. Dots are directly obtained and calculated from 1978-2005 waves of the New York City Housing and Vacancy Survey (NYCHVS). Red solid lines represent fitted value for unobserved neighborhood quality based on cubit spline interpolation in each borough. Blue dashed lines represent fitted value for average rental price based on cubit spline interpolation in each borough. The result of Staten-Island is available upon request.

Figure E11: The Variation of $\text{IV}^{mkt}_{b,t-1}$ over Time by Borough



Notes: The instrument $\text{IV}^{mkt}_{b,t-1}$ is defined as the ratio of the total number of vacant-for-rent units that are private market-rate $N^{mkt}_{b,t-1}$ and the total number of vacant-for-rent units $N^{all}_{b,t-1}$. Blue dots are directly obtained and calculated from 1978-2017 waves of NYCHVS, while the red lines represent fitted value based on cubit spline interpolation. Staten-Island has fairly small share of rent stabilized rental units and is available upon request.

Figure E12: Scatter Plot of $\text{IV}^{stab}_{b,t-1}$ and $\text{IV}^{mkt}_{b,t-1}$



Notes: This figure plots the scatter figure between $\text{IV}^{stab}_{b,t-1}$ (x-axis) and $\text{IV}^{mkt}_{b,t-1}$ (y-axis). The instrument $\text{IV}^{stab}_{b,t-1}$ is defined as the ratio of the total number of vacant-for-rent units that are rent stabilized $N^{stab}_{b,t-1}$ and the total number of vacant-for-rent units $N^{all}_{b,t-1}$. The instrument $\text{IV}^{mkt}_{b,t-1}$ is defined as the ratio of the total number of vacant-for-rent units that are private market-rate $N^{mkt}_{b,t-1}$ and the total number of vacant-for-rent units $N^{all}_{b,t-1}$.