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Despite being eligible for use in any neighborhood, housing choice vouchers tend to be redeemed in low-opportunity neighborhoods. This paper investigates how landlords contribute to this outcome and how they respond to efforts to change it. We leverage a policy change in Washington, DC, that increased voucher rental payments only in high-rent neighborhoods. Using two waves of a correspondence experiment that bracket the policy change, we show that most opportunity landlords screen out prospective voucher tenants, and we detect no change in average screening behavior after a $450 per month increase in voucher payments. In lease-up data, however, enough landlords do respond to increased payments to equalize the flow of voucher tenants into high- vs. low-rent neighborhoods. Using tax data and listings from a website specializing in subsidized housing, we characterize a group of marginal opportunity landlords who respond to higher payments. Marginal opportunity landlords are relatively rare, list their units near market rates, operate on a small scale, and negatively select into the voucher program based on hard-to-observe differences in unit quality.

Keywords: Housing Choice Voucher, landlord, opportunity neighborhood, mobility, Small Area Fair Market Rent (SAFMR).

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1 Introduction

Neighborhoods can provide or deny access to economic opportunity. The future economic success of otherwise similar children varies widely among neighborhoods in the same city (Chetty et al., 2018). Such neighborhood effects can combine with residential segregation to drive persistent racial inequity in economic opportunity (Wilson, 1997; Massey and Denton, 1993). Housing vouchers, which subsidize a share of private-market rent, can facilitate moves to opportunity neighborhoods and improve the later-in-life economic outcomes of children (Chetty et al., 2016; Chyn, 2018) and adults (Aliprantis and Richter, 2019). Housing vouchers' potential for generating moves to opportunity is typically unrealized, however, as most families with federally subsidized rental vouchers do not live in opportunity neighborhoods. The average voucher holder lives in a neighborhood with a high poverty rate and limited access to good schools (Galvez, 2010; Horn et al., 2014).

The success of different methods for encouraging moves to opportunity has varied widely. In the Moving to Opportunity (MTO) experiment, restricting vouchers to lower-poverty areas and providing mobility counseling to encourage opportunity moves resulted in fewer vouchers being used at all (Shroder, 2002; Galiani et al., 2015). A more recent experiment in Chicago providing cash incentives and counseling for tenants showed no effect on opportunity moves (Schwartz et al., 2017). More positively, paying landlords higher rent in expensive neighborhoods can encourage opportunity moves (Collinson and Ganong, 2018). Perhaps most promising, a combined intervention of paying higher rent, intensive counseling for tenants, flexible support for landlords, and short-term financial assistance showed success in Baltimore (DeLuca and Rosenblatt, 2017) and King County, WA (Bergman et al., 2019). In King County, the combined mobility intervention increased opportunity moves by 40 percentage points relative to a randomly selected control group only able to access increased rent payments. Why success varies so greatly is unclear, but successful interventions share a common element of engaging landlords, either directly through outreach and services or indirectly through paying higher rent. Direct evidence on whether and how landlords might shape access to opportunity, though, is scarce.

This paper investigates how landlords shape access to opportunity neighborhoods. We focus on *opportunity landlords*, defined as landlords in high-rent neighborhoods.[1] Our investigation uses both an experiment and a naturally occurring policy change. First, we conduct two waves of a correspondence experiment using online listings, measuring whether opportunity landlords penalize prospective tenants who signal a desire to rent with a voucher. These listings come from a large website ("majority market website") focused primarily on high-rent neighborhoods. Second, we observe a policy change between the two waves of the experiment in which the DC Housing Authority (DCHA) increased the voucher payment cap in some neighborhoods but not others. The geographic variation in this policy allows us to study its effects with both difference-in-differences

[1] We measure neighborhood opportunity using an index of the poverty rate, the unemployment rate, the employed to population ratio, the share with a HS diploma, the share with a BA, and the share of families with children under 18 that are single-headed. The correlation of median two bedroom rent with this index is 0.86 in DC.

and border discontinuity research designs. We test whether landlords' listing, pricing, and screening behavior responds to higher payments using several sources of new data on landlords in Washington, DC. We collect rental listings data from two websites: the majority market website with many opportunity landlords and an alternative website specializing in subsidized rentals. We link these data to property tax data that provide rich detail on the landlords who own listed properties.

We identify six facts about how opportunity landlords respond to the housing voucher program. First, most opportunity landlords avoid voucher tenants. Landlords on a majority market website respond positively to 20 percent of tenant inquiries signaling use of a voucher compared to 49 percent when not signaling voucher use. This gap is 7 times larger than the gap for black-indicating vs. white-indicating names in our experiment and is 12 percentage points larger at the 90th percentile of posted rent versus the 10th percentile. These facts match existing evidence from the literature (Phillips, 2017; Moore, 2018; Cunningham et al., 2018).

Second, most opportunity landlords are not on the margin between accepting and rejecting a voucher tenant. On average, the policy change we study increases voucher payments by $450 per month more in high-rent neighborhoods than low-rent neighborhoods. Despite this large increase in payments, the point estimate for the effect on landlords' positive response rates is actually negative, though statistically insignificant, and we can reject increases of more than 5 percentage points. This result is consistent across our difference-in-differences and spatial discontinuity models. Eliminating the 29 percentage point voucher penalty simply with larger payments would be daunting.

Third, we do, however, identify some *marginal opportunity landlords*, who begin targeting units in high-rent neighborhoods toward voucher tenants after payments increase. The number of rental listings on a voucher specialist website increases by 126 percent more in high-rent neighborhoods where voucher payments increase than in low-rent neighborhoods where they do not. Units listed between the old and new voucher payment limits compose the bulk of this increase.

Fourth, the existence of marginal opportunity landlords means that paying more in high-rent neighborhoods does indeed expand tenant choice, shifting lease-up locations to high-rent neighborhoods. This effect is large enough to equalize the flow of voucher tenants into high- and low-rent neighborhoods, which contrasts with the status quo of many years of voucher tenants disproportionately entering low-rent neighborhoods. However, since most voucher tenants do not move in our two-year time frame, the stock of vouchers remains concentrated in low-rent neighborhoods. Our results are qualitatively similar to those from Dallas (Collinson and Ganong, 2018) and Seattle's Family Access Supplement (Bergman et al., 2019), despite differences in context.[2]

Our fifth and sixth facts characterize marginal opportunity landlords. Our fifth fact is that marginal opportunity landlords observably differ from other landlords. Marginal opportunity landlords tend to operate at small scale; the majority own one property. They tend to be new to voucher specialization but not to owning property. While majority market and voucher specialist landlords sort into high-rent versus low-rent neighborhoods, respectively, marginal opportunity

[2]For example, our opportunity index has a correlation across census tracts with median two bedroom rent of 0.58 in King County, 0.71 in Dallas, and 0.86 in DC. These communities differ in the feasibility of moving to opportunity without paying more rent, but higher rent payments still have some effect in all three places.

landlords with multiple properties tend to have exposure in both types of neighborhoods.

Our last fact is that the prices listed by marginal opportunity landlords indicate that their units are listed near market prices but these landlords also negatively select into voucher specialization based on hard-to-observe characteristics. Voucher specialist landlords direct units with somewhat worse unobservable quality to the voucher program. Compared to majority market listings, these units are listed for lower rent, conditional on neighborhood fixed effects and observed unit characteristics. In principle, our estimates provide a lower bound for the extent of negative selection, since lower rent could include both negative selection and rent mark-ups for voucher tenants. However, we find no evidence that opportunity landlords mark up rent for voucher tenants. Among the broader set of voucher specialist listings, we find evidence that landlords charge voucher tenants above market rates; many units are listed at exactly the voucher payment ceiling. However, this tendency only exists in low-rent neighborhoods. Landlords in high-rent neighborhoods price voucher units about the same as observably similar units targeted to the majority market.

The facts we document indicate that engaging landlords is likely necessary for sustained improvement in access to neighborhood opportunity. This focus on landlords' role in moves to opportunity contrasts with a common approach in the literature of treating neighborhood location as being driven entirely by tenant choices and prices. For example, Shroder (2002) shows that restricting a voucher to low-poverty neighborhoods in MTO decreased the lease-up rate relative to an unrestricted voucher. If one assumes that landlords simply list a price and take any tenant who can pay that price, then low lease-up rates reflect tenant preferences; voucher tenants who do not lease-up do not wish to move to a low-poverty neighborhood (Galiani et al., 2015). Our results indicate that landlord barriers rather than tenant preferences likely drive this fact. More broadly, only about half of voucher recipients are able to successfully use their voucher in any location, despite long wait-lists and a high value of successfully leasing up (Chyn et al., 2018; Collinson et al., 2015). This fact likely reflects an inability to find a landlord willing to take a voucher. This interpretation matches the qualitative literature, which finds that landlords actively screen voucher tenants and select which units to market to cash versus voucher tenants (Popkin and Cunningham, 2000; Rosen, 2014; Greenlee, 2014; Desmond, 2016; Garboden et al., 2018). In this view, landlords may restrict tenants access to opportunity neighborhoods when tenants wish to move. We provide some of the first direct quantitative analysis on how landlords shape access to opportunity neighborhoods and on the extent to which policies can change landlord behavior.

Our results help rationalize the existing evidence on efforts to encourage moves to opportunity. Tenant-focused interventions may show disappointing results (Schwartz et al., 2017) or a trade-off between opportunity moves and lease-up (Shroder, 2002) because opportunity moves require a landlord partner, and most landlords do not wish to participate. Similarly, paying greater rent in expensive neighborhoods can facilitate some voucher moves (Collinson et al., 2015) because some landlords are on the margin between accepting and rejecting a voucher tenant. However, if most landlords do not respond to increased payments alone, then a combined package of services that includes high-touch landlord outreach can be more effective (Bergman et al., 2019).

Finally, our results raise a question about whether opportunity moves can significantly narrow the gap in equity of opportunity given the current set of marginal opportunity landlords. The success of a mobility effort hinges on its ability to induce participation from some group of opportunity landlords, and a policy that engages only a small and idiosyncratic set of landlords may not succeed at scale or over long periods of time. For example, we find that neighborhood-specific voucher payments increase the number of voucher households living in opportunity neighborhoods in DC by about 300. However, this progress is facilitated by relatively few landlords who operate at small scale. Changing lease-up locations for the stock of voucher tenants will require sustained impacts on the flow of new lease-up locations over many years, so the ability of increased rent payments to change where most voucher tenants live will depend on engaging a broader set of landlords than do current policies. Similarly, expanding from successful, small-scale mobility pilots to systematic policies requires the involvement of many more landlords. It is possible that additional time or greater landlord engagement will expand the set of interested landlords, but success on this point is key. Any systematic attempt to provide equity of opportunity through housing moves will depend on expanding the set of marginal opportunity landlords.

The remainder of the paper is organized as follows: Section 2 provides background on the HCV program, the determination of voucher values in the program, and related policy changes in DC. Section 3 describes three predictions from a model of landlord behavior to help interpret our empirical results. Section 4 describes our empirical strategy, providing details about the experiment, additional data sources, and the identification strategies we use in our analysis. Section 5 presents our empirical results and Section 6 concludes.

2 Background

2.1 Housing Vouchers and Neighborhood Opportunity

The Housing Choice Voucher (HCV) program is the United States' largest program for low-income housing assistance. HCV tenants lease up in rental units found on the private market. Tenants typically pay 30 percent of their income toward rent, with the HCV voucher subsidizing the remainder of the unit's rent. The value of the voucher subsidy is capped, typically near the median of recently leased rents in the tenant's metro.

In principle, voucher recipients can lease-up in any neighborhood. In practice, voucher tenants typically live in low-rent neighborhoods. Washington, DC, provides a clear illustration of where voucher recipients reside when a uniform subsidy is provided in a bimodal rental housing market. As shown in Figure 1a, in the 2012-2016 ACS many tracts in DC have a median rent below $1,000 per month and many others have a median rent above $1,800 per month, with relatively few tracts in between. High-rent neighborhoods tend to cluster to the North and West, while lower-rent neighborhoods cluster to the South and East. Figure 1b shows where in DC households with vouchers lease-up. People with vouchers are concentrated in lower-rent neighborhoods. This pattern matches national data. Compared to all low-rent housing units, units leased by HCV tenants tend to be in neighborhoods with similar poverty rates and lower-performing schools (Galvez, 2010; Horn et al., 2014; Davis et al., 2018).

Living in low-rent neighborhoods reduces economic opportunity for voucher holders. In DC and elsewhere, low-rent neighborhoods also tend to have lower income, labor force participation, and educational attainment. Living in neighborhoods with these characteristics reduces economic opportunity. Housing mobility programs have demonstrated that children moving to lower-poverty neighborhoods have higher income as adults (Chetty et al., 2016; Chyn, 2018), and that children moving to better schools are more likely to graduate from high school, attend college, and have positive labor market outcomes (Rosenbaum, 1995). Similarly, children have better outcomes after experiencing improvements to neighborhood opportunity through changes around their public housing building (Dastrup and Ellen, 2016), through gentrification (Brummet and Reed, 2019), or through local labor demand shocks (Baum-Snow et al., 2019). Adults experience better recovery from job displacement when living in a neighborhood with access to jobs (Andersson et al., 2018) and have improved labor market outcomes when living in a neighborhood with greater job referrals (Bayer et al., 2008). And while the Moving to Opportunity housing mobility program had limited effects on adult economic outcomes (Ludwig et al., 2013), some re-analysis of the original data indicates that these null effects result from relatively modest changes in neighborhood opportunity. Adults induced by the experiment to make larger changes in neighborhood conditions actually see improvements in labor market outcomes (Aliprantis and Richter, 2019; Pinto, 2019). All of this evidence implies that families with vouchers forgo economic benefits by leasing up in low-rent neighborhoods.

Many economic models infer that tenant preferences for other neighborhood attributes drive voucher lease-up locations toward neighborhoods with less economic opportunity. In a standard neighborhood choice model, tenants can lease any housing unit for which they are willing and able to pay the rent. Higher neighborhood opportunity simply implies a greater rent payment. If a tenant has a voucher that allows her to afford a neighborhood and she does not move, the model infers that the tenant intentionally chose a different neighborhood consistent with her preferences. Galiani et al. (2015) apply such a model to the Moving to Opportunity data. In that experiment, families were randomly assigned to public housing, a regular voucher, or a voucher that could only be used in a tract with a poverty rate less than 10 percent. Lease-up rates for the restricted voucher are much lower than those for the unrestricted voucher (Shroder, 2002). Hence, the model infers that tenants prefer not to move to such neighborhoods. Galiani et al. (2015) conclude that many tenants do not want opportunity moves and restrictions on move locations can be counterproductive.

However, landlord behavior could also prevent voucher tenants from accessing opportunity neighborhoods. While there is some evidence that people can move to opportunity through intensive mobility counseling programs (DeLuca and Rosenblatt, 2017; Bergman et al., 2019), a randomized control trial testing counseling and incentives in Chicago did not increase the rate of opportunity moves, and the authors concluded that a lack of willing landlords limited many families who wanted to move (Schwartz et al., 2017). A growing qualitative literature documents that, rather than passively accepting any tenant who can pay, landlords respond actively and strategically to the voucher program (Popkin and Cunningham, 2000; Rosen, 2014; Greenlee, 2014; Desmond, 2016; Garboden et al., 2018). These studies find that some landlords actively avoid vouchers due to concerns about property damage and regulatory burden or screen on other characteristics, such as family size, race, and public housing, that can correlate with use of a voucher. Other landlords specialize in receiving vouchers, steering tenants to units with lower market value or even renovating units to be lower in quality but more durable (e.g., walling off windows). Recently, a series of correspondence and audit studies have confirmed these qualitative observations, finding that landlords respond more negatively to prospective tenants who wish to use a voucher, all else equal (Phillips, 2017; Moore, 2018; Cunningham et al., 2018). Since vouchers typically expire if a tenant does not find a unit within a 90-day time limit and many tenants fail to lease-up at all (Chyn et al., 2018), these landlord restrictions could lead tenants to lease-up in poorer, lower-rent neighborhoods.

2.2 Housing Voucher Payment Limits

The rules determining the value of a housing voucher could drive high-rent landlords away from accepting voucher tenants. Tenants typically pay 30 percent of their income in rent while the housing authority pays the balance directly to the landlord. With income-based rents tenants have no incentive to economize, so the housing authority typically enforces two procedures to keep rents in check. First, the housing authority analyzes rent reasonableness, comparing a unit to other similar properties to assess if the listed rent is reasonable. We will return to rent reasonableness later. Second, the local housing authority will not pay rent that exceeds a cap, known as fair market rent (FMR).[3] Traditionally, vouchers are capped at a metro-wide FMR, with HUD setting FMR at the 40th percentile of rent for all units with the same number of bedrooms in the entire metro area.[4] Since the FMR limit is constant across the entire metro area, high-rent landlords could have very limited incentives to participate, and the qualitative literature argues that the value of FMR relative to market rents drives much of landlords' responses to vouchers (Garboden et al., 2018).

Attaching the value of a voucher to neighborhood-specific rents could provide voucher holders access to higher opportunity neighborhoods. Small area fair market rents (SAFMRs) set rent limits for the voucher by a smaller geography, e.g., ZIP code. Analysis of rental listings in California shows that setting FMRs by ZIP code rather than metro area increases the number of high-opportunity units below FMR in many cities (Palm, 2018). Dallas, Texas was forced by court order to implement small area FMRs at the ZIP code level. Collinson and Ganong (2018) compare voucher lease-up locations in Dallas and nearby unaffected Fort Worth. They find that setting ZIP-level FMRs

[3]If a tenant rents a unit above the FMR, the tenant is responsible for the remaining amount of rent. Voucher-eligible tenants are income constrained and have a strong incentive to rent units below the FMR. Also, exception payment standards require special approval for tenants to lease up in a unit between 110 and 120 percent of FMR, and will typically not allow for vouchers to be used in units that are more than 120 percent of FMR.

[4]The 40th percentile is for the distribution of gross rents paid by recent movers in the private market who are not voucher holders. Note that in 2001 HUD switched from setting FMRs at the 40th percentile to the 50th percentile in 39 metro areas; see Footnote 17 in Collinson and Ganong (2018) for a discussion.

shifts new voucher lease-ups to high-opportunity neighborhoods. Despite paying higher rents for opportunity moves, this change is actually budget neutral in the short run since it also reduces rents paid for the vast majority of voucher tenants who live in low-rent neighborhoods and do not move. Given the success of SAFMRs in Dallas, HUD modified FMR rules in early January 2017 to require 24 additional cities to move to SAFMRs and allowed all others to opt-in. After a protracted court battle between conflicting HUD administrations, this rule began implementation on January 1, 2018 (Howell, 2017).

2.3 Small Area Fair Market Rents in Washington, DC

Washington, DC, was also an early mover in attaching voucher payments to neighborhood market rents, and we investigate the impacts of small area FMRs on landlords and voucher holders in DC. Through a Moving to Work waiver in 2015 (Galvez et al., 2017), DC received permission from HUD to move from city-wide to neighborhood-specific rent limits. Since introducing this policy, the DC Housing Authority (DCHA) conducts a rental analysis that includes referring to existing data and canvassing neighborhoods. It uses these data to compute market rental comparisons by tax neighborhood and number of bedrooms, which are used to compute caps on payments for tax neighborhoods.

The SAFMR policy rolled out across DC neighborhoods in two stages, as illustrated in the timeline in Figure 2. The first stage occurred in early 2015, prior to the first wave of our experiment. At this time, HUD allowed DCHA to switch from a common metro-wide limit to neighborhood-specific limits up to 130 percent of the metro-wide FMR. Since DC exhibits a bimodal rental housing market with high-end rents that are higher on average than those in the remainder of the MSA, the 130 percent cap was binding for a large portion of the city but not all of it. Some low-rent neighborhoods had payment limits attached to neighborhood conditions in an unrestricted manner, while other neighborhoods had payments capped at 130 percent of metro-wide FMR during this time. The second stage of the policy occurred in January 2017 when DC obtained a waiver to increase neighborhood rent limits up to 175 percent of the metro-wide FMR. In high-rent neighborhoods, voucher payments that were previously at 130 percent of FMR increased up to 175 percent of FMR. However, in lower-rent neighborhoods voucher payment limits were unchanged. This latter policy change comes between the two waves of our experiment, allowing us to observe how conditions changed over time in neighborhoods affected vs. unaffected by higher voucher payments.

Figure 1c summarizes the geography of the 2017 policy change. It displays 2017 voucher limits, relative to metro-wide FMR, for all neighborhoods. We display values by tax neighborhood, which is the definition of neighborhood used by the DCHA. As is apparent, the limits closely correlate with neighborhood rent levels. The neighborhoods colored in red represent those neighborhoods affected by the policy change. In 2017, red neighborhoods' FMRs are greater than 130 percent of the metro-wide FMR, indicating voucher values would have typically been below market rents in these neighborhoods in the absence of the policy change. Within this group, those neighborhoods in dark red have limits exactly at the 175 percent cap, indicating that neighborhood market rents are actually still higher than the current neighborhood FMR. Finally, those neighborhoods in blue have limits below 130 percent and hence were unaffected by the change. Our analysis will investigate how the policy change in 2017 affected landlord behavior and voucher holders' outcomes in red-shaded neighborhoods where the policy change had bite, relative to the blue-shaded neighborhoods where it did not.

As will be shown more formally later, this policy increases lease-up rates for vouchers in high-rent relative to low-rent neighborhoods. Figure 1d provides some initial graphical evidence. We map how the number of newly arriving vouchers in a census tract changed between 2012-2016 and 2017-2018. Relative to historical norms, newly moving voucher tenants tend to move into higher-rent neighborhoods in Northwest DC rather than cheaper neighborhoods in Southeast and Northeast DC. Similar to the results of Collinson and Ganong (2018) for Dallas, we find that paying more in high-rent neighborhoods can facilitate moves to opportunity. Similar to efforts in other cities, DC's expansion of neighborhood-based voucher payments shows promise for encouraging moves to opportunity in the short run, allowing our investigation to shed light on how landlords respond to such a change.

3 A Model of Landlord Decisions

The workhorse neighborhood choice model in economics treats rental housing as a competitive market with landlords accepting any tenant who can pay market rent (e.g. Galiani et al. (2015)). Such a model simplifies two aspects of the voucher program that are the focus of our empirical analysis. First, landlords may actively screen tenants based on voucher status. While the qualitative literature provides many examples of such behavior (Popkin and Cunningham, 2000; Rosen, 2014; Greenlee, 2014; Desmond, 2016; Garboden et al., 2018), this issue has been largely ignored by the economics literature.[5] Second, landlords might change the rent they charge in response to the voucher program. There is contrasting quantitative evidence on the prevalence of this behavior. On one hand, Collinson and Ganong (2018) find that increasing metro-wide voucher payment limits increases rent paid without much effect on unit or neighborhood quality. Desmond and Wilmers (2019) use hedonic regressions to show that vouchers in Milwaukee over-pay by about 10 percent relative to observably similar units. McMillen and Singh (2018) find some evidence from Los Angeles that equilibrium rents cluster around voucher limits set by FMR. On the other hand, Olsen (2019) summarizes a series of HUD studies that show little voucher premium. He argues that the voucher program pays market rent on average due to sufficient enforcement of rent reasonableness and offsetting voucher premia in high-rent vs. low-rent neighborhoods. Eriksen and Ross (2015) examine how increasing the number of vouchers in a market affects pricing. They find no overall effect on rents charged but do find rent increases near FMR when housing supply is inelastic.

We build a theoretical model in which the way landlords screen and set prices in response to

[5] A notable exception is Geyer (2017).

the voucher program depends on market conditions. We develop a formal theory in Appendix C that models the landlord's choices regarding posted rent and screening tenants. Our model makes three predictions:

1. Landlords screen out the least attractive prospective tenants, particularly when facing a rental market with high demand.

2. Some landlords will price units at exactly the FMR that applies to their unit, regardless of the unit's market rent. This scenario will only obtain with weak enforcement of rent reasonableness and for landlords who rent primarily to HCV tenants.

3. Increasing voucher payment caps may attract two types of landlords to the voucher program: First, landlords who anticipate no additional cost of working with voucher tenants but who have units priced at market rates just above the old payment cap and, second, landlords who view participation as costly but are enticed by charging above-market rent at the new payment limit. Other landlords do not respond.

While the full specification of the model and our numerical results can be found in Appendix C, here we outline the reasoning leading to these predictions. We suppose that landlords screen, or choose whether to accept or reject a tenant, based on the expected maintenance cost m the tenant will generate due to factors such as late rent; damage to the property; externalities operating through the preferences of the landlord's other tenants; the pecuniary, time, and energy costs of complying with program regulations; and contributions to utility arising from non-pecuniary factors such as altruism or prejudice. We show that in an extension of the McCall (1970) model the landlord's decision rule is based on a reservation expected maintenance cost where the landlord accepts if $m < m^*$ and rejects if $m > m^*$.

Landlord value functions are of the form

$$v(m) = \max \left\{ \frac{r - m}{1 - \beta}, \quad \beta \, \pi \int v(m') dF(m') \right\} \tag{1}$$

where the maximization is over accepting the tenant or rejecting him and waiting to draw a new tenant with expected maintenance m' next period. Here π is the probability of encountering a tenant next period, β is the discount rate, r is posted rent, and $F(m')$ characterizes the landlord's beliefs about the distribution of maintenance costs he may encounter next period. Some related value functions are shown in Figure 3a, where the kink in these figures occurs at m^*.

Prediction 1 is illustrated in Figure 3a. As π increases, the reservation maintenance cost m^* will become smaller and smaller. This predicts that in a rental market with high demand such as the market in Washington, DC, landlords will tend to reject all but the most attractive prospective tenants. This type of screening could very easily lead to screening out voucher tenants, particularly in high-rent neighborhoods. If voucher tenants have higher maintenance costs on average, landlords will screen them out, particularly when landlords are in the most advantageous situations. We formalize this idea in Appendix C by allowing $F(m)$ to depend on observable characteristics.

Proceeding by backward induction, we cast the landlord's pricing decision in terms of choosing the slack s to add to market rent, $r = r^m + s$, to solve the problem

$$\max_s \mathbb{E}[v(m|r^m, s, r^{FMR}, \ell)]. \tag{2}$$

The goal is to choose slack to maximize expected value for the landlord. In Appendix C we illustrate this problem graphically as finding the highest point on an expected value curve.

In the model we allow for two types of tenants τ, "cash" and "voucher." Cash tenants are always driven away by rent increases, while voucher tenants only respond to rent increases when required to by payment limits or rent reasonableness enforcement. We also allow for two types of landlords ℓ, cash and voucher specialists, who differ in whether they view voucher tenants as adding substantially to maintenance costs.

Prediction 2 is illustrated in Figure 3b. A voucher landlord with a unit below the fair market rent (FMR) will set s so that $r = r^m + s = r^{FMR}$. In other words, voucher specialists will tend to list their unit at the FMR, since the benefit of increased rental income will outweigh the costs from driving away cash tenants. In contrast, a cash landlord with a unit below FMR will list his unit near the market rent. For these landlords the benefit of increased rental income must also be weighed against the increased maintenance costs associated with the voucher tenants they are more likely to encounter after raising the listed rent.

Figure 3b also provides the basis for Prediction 3. Consider a landlord who owns a unit with market rent of \$1,250. The landlord will not rent to voucher tenants when facing an FMR of \$1,200, regardless of the enforcement of rent reasonableness or the type of landlord. If the FMR applied to the unit were to increase to \$1,400, then a voucher landlord would rent the unit to a voucher tenant at the new FMR. Such an increase in FMR would also induce a cash landlord to rent the unit to a voucher tenant if he believed that voucher tenants had maintenance costs similar to those of cash tenants.

4 Empirical Strategy

4.1 Correspondence Experiment

We conducted two waves of a correspondence experiment examining how landlords respond to tenants who state a desire to pay with a subsidized housing voucher. Research assistants sent e-mails from fictional applicants to real rental housing listings from an online classified ads site we refer to as the "majority market site." Since the inquiries were from fictional people, we could control and randomly assign the entire content of the initial e-mail from the applicant to the landlord. In the first wave of the experiment, we sent 2,668 fictional inquiries to 1,336 real rental listings during May and June of 2015. The resulting data are the same as those considered in Phillips (2017). In the second wave, we sent 4,264 inquiries to 1,810 rental listings during July to August 2017. The two waves are identical unless otherwise noted.

We sent inquiries to rental units in Washington, DC, that list monthly rent appropriate for a voucher. For a given inquiry, the research assistant first identified all units eligible for the experiment. In the first wave, units listed for rent greater than $1,500 were excluded. The second wave targeted any units whose rent was less than or equal to the highest voucher limit in the city for the unit's size. For efficiency units, the highest rent limit during the study was $2,560; for five-bedroom units, it was $5,766. Eligible units also had to be monthly rentals, listed since the previous work-day (typically the previous 24 hours), of known location, located inside the boundaries of the District of Columbia, not obviously a scam, and not a re-posting previously applied to. During the second wave, we also eliminated postings for roommates and ads by recognized landlords to whom we had already applied. Once a set of units had been screened for eligibility, a subset was randomly selected to receive inquiries. A given unit may have received multiple inquiries. During the first wave, there was an initial period where each unit was randomly selected to receive only one or two inquiries, which was later changed to two or four. In the second wave every unit had an equal chance of receiving one, two, or four inquiries. For units receiving multiple inquiries, the e-mails were sent in random order with at least one hour between them.

Our analysis focused on a signal statement indicating that the fictional tenant wished to use a housing voucher to subsidize rent. Since most people refer to the Housing Choice Voucher program by its prior name, Section 8, we focused on this language. In particular, selected inquiries received one of the following statements:

- I'm looking for a place that takes Section 8.
- I would also like to know if you accept Section 8 vouchers.
- Also, I would plan to pay with a Section 8 voucher.
- I plan to pay with Section 8.

We randomly and independently selected inquiries to include this statement versus omitting it. An inquiry without reference to Section 8 was intended to indicate a cash tenant. In the first wave, one-quarter of all e-mails included a voucher statement; in the second wave this increased to one-half.

Signaling voucher status in an initial inquiry is within common practice. Practitioner organizations that work with tenants give conflicting advice. Some recommend disclosing one's voucher status immediately to avoid wasting time and resources pursuing dead-ends; this is particularly important for clients who lack private transportation. Others recommend delaying disclosure to avoid a negative first impression. In any case, all academic correspondence and audit studies on vouchers signal it at first contact (Phillips, 2017; Moore, 2018; Cunningham et al., 2018). Likewise, non-academic organizations that use audits for compliance purposes similarly signal vouchers at first contact (Scott et al., 2018).

The language of an inquiry to a particular rental listing was a randomly generated message comparable to those used in Hanson and Hawley (2011) and Ewens et al. (2014). See Appendix

Figure 11 for an example. All other characteristics were assigned randomly and orthogonal to the main voucher signal treatment. As in Ewens et al. (2014) we randomly and independently assigned one-third of the applicants to include positive quality signals (professional employment, good references, and/or good credit), one-third to include negative signals (smoker and/or bad credit), and one-third to have no signal statement of quality. Names were chosen at random from the same list as in Bertrand and Mullainathan (2004). The sex signaled by the name was chosen randomly, independently, and in equal proportion. Name race was also assigned randomly, though the exact assignment rule varied. In the first wave, we assigned black-indicating names randomly and independently with a probability of 0.50 for half of all units. For the other half of units, we stratified or matched treatment, assigning black names at random but guaranteeing that each unit received half black and half white names. In the second wave, we similarly stratified assignment of name race for half of all units. For the other units, we assigned black names to all inquiries. We also assigned greetings, valedictions, etc. randomly. We avoided detection by drawing the components of each e-mail at random and without replacement so that landlords receiving as many as four e-mails in the experiment received truly unique e-mails. In 2017, minor differences from the text of messages used in 2015 were introduced to help obscure the experiment from detection.

Table 1 summarizes the design of the experiment. In the 2015 wave, the proportion of applicants listing a black or female name is 50 percent. The average unit in the first wave rents for $1,253 per month and has one bedroom. In the bimodal DC rental housing market, online listings tend to come from the upper mode in high-rent, high-opportunity neighborhoods. Thus, our experiment provides a good test of how landlords in such neighborhoods respond to voucher tenants. Since the voucher signal is assigned randomly and independently of all characteristics of the unit and all other components of the messages, means for these baseline characteristics are the same for inquiries including the voucher statement (voucher) and those not (cash). Baseline balance is similar for the 2017 wave, though the proportion of inquiries with black names is higher by construction, and the average unit is more expensive and larger because we increase the rent limit for entering the experiment.

We measure how landlords respond to the fictional inquiries via e-mail. Most often, landlord responses can be linked to the original inquiry because landlords respond through the listing service's system and/or because the listing number is referenced. In the few cases where this is not possible, the inquiry e-mail accounts are uniquely matched to applicant names. We then match manually given the timing of the inquiry, the timing of the response, and the listing location. Following Ewens et al. (2014), we focus on only positive responses in which a landlord invites the applicant to see the unit, explicitly provides a means for further contact, or responds that the unit is available while providing or requesting more information. We code as negative those responses indicating the unit is no longer available or that some stated trait of the applicant is incompatible with the listing. We also observe neutral responses, where landlords provide or request more information but do not describe availability or reply only with availability.

4.2 Listings Data

The rental listings used for the correspondence experiment provide one useful set of rental listings. The website is large; it listed units at a rate of about 80,000 per year during our 2017 sample period, compared to 162,670 rented units in DC reported by the 2017 ACS. For both waves we can observe posted rents, locations, and some unit characteristics (e.g., number of bedrooms, square feet). As noted above, we avoid duplicate correspondence with the same landlord, limit to a range of rents relevant to the experiment, and otherwise restrict the sample. However, for the 2017 wave we recorded a large sample of rental listings on the website for the city of Washington, DC, that did not fit the screening criteria for the experiment. This larger set of listings can be used to better characterize the state of the full rental market. Unfortunately, we do not have a similar sample of listings for the 2015 wave.

We supplement the majority market site listings with rental listings from a voucher specialist website. SocialServe.com is a non-profit organization that operates DCHousingSearch.org with funding from the DC Department of Housing and Community Development (DHCD). The site specializes in hosting listings for subsidized tenants and income-restricted housing units. SocialServe.com provided a database of all units listed on its site between 2010 and 2018. This specialized nature makes the site less extensive; there are 453 total listings in 2017. The data include information that can be observed publicly on its website: address, posted rent, number of bedrooms, etc. These listings include many landlords seeking voucher tenants, which provides a useful sample of listings targeted specifically at subsidized households. Local government also encourages landlords affected by certain affordable housing initiatives to list on the site. For example, new development in DC often falls under inclusionary zoning restrictions that require developers to reserve a certain number of rent-restricted units for low- or moderate-income households. Since we wish to focus on listings with market rent, we always restrict these data to exclude any listings tagged as corresponding to inclusionary zoning or posted with a rent schedule other than simple rent (e.g., income-based rent). We also eliminate units with six or more bedrooms, for which voucher payment limits are not clearly defined.

4.3 Property Tax Data

Rental listings do not provide information on landlords, so we match the rental listings described above to property assessments from DC property tax records. We match properties between the listing and tax data sets using a fuzzy matching algorithm based on the address. Within the tax assessment data we identify properties owned by the same landlord using the address to which the DC tax authority sends the property tax bill. For a given property matched to an online rental listing, we identify all other properties in DC with the same tax bill address and hence the same assumed owner. To the extent that a single property manager pays tax bills for multiple owners, we will measure property manager networks rather than owner networks, but it seems reasonable to treat the property manager as the landlord who both pays taxes and also screens potential tenants. The tax data also include owner names; however, many of these are LLC shell names that would lead to under-identification of multi-property ownership.

4.4 Lease-Up Data

We use data from HUD's Picture of Subsidized Households. This data set indicates the number and characteristics of households receiving various HUD-supported programs by census tract. We extract data on the number of households leased-up with Housing Choice Vouchers in each census tract of Washington, DC, in each year between 2012 and 2018. One of our main outcomes is the number of vouchers leased up in a tract in a given year. The data list the "number of subsidized units available" in a given tract. To be consistent with city-wide values of vouchers available, the underlying data take the number of vouchers actually in use and inflate them proportional to vouchers not in use.[6] To recover the actual number of vouchers leased up, we reverse this process and deflate the tract-level voucher numbers by the city-wide voucher usage rate. Another outcome of interest is the number of new vouchers moving into a tract in a given year. This value is directly observed in the data for most tracts. For a few tracts, though, it is censored due to small values. For these few tracts, we impute the number of new vouchers with the difference in the number of leased-up vouchers between the present year and the previous year. Finally, the HUD data are at the tract level rather than the tax neighborhood. We map the policy variation to census tracts as shown in Appendix Figure 12.

4.5 Summary Statistics

Table 2 presents summary statistics about neighborhoods and HUD voucher recipients. Means are weighted by one of four different weights: census tract population, number of HUD voucher recipients, number of units included in the experiment, and number of DCHousingSearch.org listings, respectively. These weights provide a snapshot of the relative differences in neighborhood context for the average DC resident, HCV recipient, listing in the experiment, and listing on DCHousingSearch.org for the year 2017. Statistics are from the American Community Survey at the census-tract level. For instance, the average neighborhood median household income across all DC residents is $83,000, whereas the average neighborhood income to which all HUD voucher recipients are exposed is just $48,000. In contrast, the rental units in the experiment are exposed to average neighborhood incomes of $99,000. Listings on DCHousingSearch.org are much more similar to a typical voucher lease-up location with neighborhood income near $57,000. Other key indicators of neighborhood opportunity, such as employment, poverty, and education, show that current voucher holders and units listed on DCHousingSearch.org are more exposed to lower-opportunity neighborhoods than is the typical resident. Our experiment's listings are drawn from even higher-opportunity neighborhoods than the city average, which reflects the composition of the online rental market for voucher holders. Since the goal of neighborhood-based rent limits is to extend the voucher program to new neighborhoods and landlords, this sample provides a useful one in

[6] Thanks to Ed Olsen for bringing this fact to our attention.

which to test the effects of the policy.

These large differences result from DC's underlying bimodal rental housing market. To summarize DC's geographic divide, we summarize opportunity as an index of neighborhood characteristics.[7] Figure 4 shows two-way histograms of rental units, counting by rent and opportunity. Relative to the national distribution in 4a, the city of Washington, DC, has a bimodal distribution of high-rent units in high-opportunity neighborhoods and low-rent units in low-opportunity neighborhoods but few low-rent units in mid-opportunity neighborhoods (4b). While the middle of the rent and opportunity distribution exists in DC, HCV residents did not live in such neighborhoods in 2016 (4c). Units listed in the experiment, though, come from the upper mode of the distribution (4d).

The context of DC proves particularly useful for studying neighborhood opportunity. As Chetty and Hendren (2018) document, economic mobility is much lower in the South and upper Midwest. These are exactly the places where cities divide most clearly along economic and racial lines. One way of measuring the geographic concentration of opportunity in a city is to measure the gap, or difference, in neighborhood opportunity between the median non-poor resident and the median poor resident. Appendix Figure 13a shows the distribution of these gaps as measured in the largest county of each of the 54 metro areas with at least 1 million residents in the 2012-2016 ACS. Among these central counties, DC has the 7th largest gap between poor and non-poor neighborhood quality. The remainder of the top 20, shown in Appendix Figure 13b, is populated by other Midwest and Southern cities. DC is between more unequal cities such as Atlanta (1st), Cleveland (4th), and Charlotte (6th) and less unequal cities such as Columbus (8th), St. Louis (10th), and Phoenix (13th). Neighborhood conditions correlate more with a person's economic situation in DC than in most large US cities, which makes it precisely the type of place to study interventions designed to target moves to opportunity and intergenerational mobility.

Neighborhood conditions also correlate more strongly with rent in DC than in most large US cities. If one is worried about voucher holders being priced out of opportunity neighborhoods, DC is one of the cities where this is most likely to happen. Appendix Figure 14a shows a random sample of 1,000 tracts from the largest counties in each of the 54 MSAs with populations of at least 1,000,000 in the 2012-2016 ACS. This figure is representative of what one would see if conducting this same exercise for any particular metro area - most cities have high-opportunity neighborhoods that are also low-rent. Appendix Figure 14b shows again that DC represents a set of cities with more extreme sorting. There is a steeper gradient of rent as a function of opportunity with a relatively high correlation. This means that DC is an ideal place to study SAFMRs, as it represents cities with stronger cost constraints.

[7] We follow Aliprantis and Richter (2019) and measure neighborhood quality as the first principal component of the poverty rate, the unemployment rate, the employed to population ratio, the share with a HS diploma, the share with a BA, and the share of families with children under 18 that are single-headed. Each of these variables is first put into percentiles of the national distribution (in terms of population living in census tracts with these characteristics). We denote opportunity as the tract's percentile in the distribution of the resulting index/principal component.

4.6 Identification Strategies

We examine whether a change in neighborhood voucher values in Washington, DC, affected landlords' acceptance of vouchers. As discussed above, Washington, DC, received a waiver from HUD to set different voucher limits at the neighborhood level, as shown in Figure 1c. Prior to 2017, DC's waiver allowed it to set neighborhood limits based on neighborhood rent conditions up to a cap of 130 percent of fair market rent. In 2017 the cap expanded to 175 percent. Hence, rent limits increased in neighborhoods for which the city's preferred cap is above 130 percent of fair market rent. Most of the western portion of the city, along with Capitol Hill and surrounding areas, saw rent limits increase. Rent limits in other neighborhoods were not affected by this policy change. We can use this variation to test whether increasing neighborhood rent limits affects landlord behavior and lease-up locations.

We estimate the effects of higher rent limits in a difference-in-difference framework. First, consider a simple difference-in-difference specification estimating the magnitude of the change in voucher payment limits. We estimate this model by ordinary least squares:

$$P_{ijt} = \beta_0 + \beta_1 T_j + \beta_2 Post_t + \beta_3 T_j * Post_t + \epsilon_{ijt}.$$

P_{ijt} measures the value of the neighborhood-specific voucher payment limit for unit i in tax neighborhood j during year t. T_j is a dummy for whether the unit is in a neighborhood affected by the policy change, that is, with a 2017 rent limit above 130 percent of HUD's city-wide fair market rent. $Post_t$ is a dummy for years 2017 or later. The coefficient of interest is on the interaction of the two, β_3, which measures by how much more voucher payment limits increased in treatment neighborhoods compared to control neighborhoods.

We estimate a similar reduced form specification for final outcomes, again estimated by ordinary least squares:

$$Y_{ijt} = \gamma_0 + \gamma_1 T_j + \gamma_2 Post_t + \gamma_3 T_j * Post_t + \eta_{ijt}.$$

Y_{ijt} is an outcome indicator, for example, whether a landlord responds positively to an inquiry to unit i in tax neighborhood j during year t. Other variables are defined as before. In the correspondence experiment data, γ_3 measures whether callback rates to voucher tenants increase more in neighborhoods where rent limits went up relative to neighborhoods where they did not. We estimate similar specifications in using rental listings and voucher lease-up outcomes. When examining the lease-up outcomes, the level of observation is the neighborhood-year, and j indexes census tracts rather than tax neighborhoods.

The difference-in-difference specification relies on the typical parallel trends assumption. We assume that, in the absence of a policy change, the gap in outcomes between treated and control neighborhoods would remain constant over time. This assumption could be false if, for example, the neighborhood rental market is evolving differently in high-rent versus low-rent neighborhoods over time. To relax this assumption, we consider a triple-difference specification. For the correspondence experiment only, we can exploit the experimental variation in whether the fictional inquiry signals a desire to use a voucher, denoted by τ_{ijt}. More formally, we estimate:

$$Y_{ijt} = \psi_0 + \psi_1 T_j + \psi_2 Post_t + \psi_3 \tau_{ijt} + \psi_4 T_j * Post_t + \psi_5 T_j * \tau_{ijt} + \psi_6 Post_t * \tau_{ijt} + \psi_7 T_j * Post_t * \tau_{ijt} + \nu_{ijt}$$

The coefficient of interest is ψ_7, which measures whether the gap between voucher and cash inquiries decreases over time in neighborhoods that receive rent limit increases, relative to those that do not.

The difference-in-differences and triple difference specifications described above could confound the effect of increased rent limits with other changes that particularly affect voucher tenants' access to high-rent neighborhoods in DC. For example, the DC Housing Authority introduced other policies and landlord outreach programs aimed at moving tenants to higher-rent neighborhoods. To guard against this possibility, we consider an alternative identification strategy that focuses on the spatial discontinuity in rent limits near the border of the policy change. As shown in Figure 1c, several neighborhoods affected by the policy change border neighborhoods where rent limits were unaffected. Housing units, neighborhood conditions, and other policies will likely be similar on either side of these borders. If this is true, focusing on a narrow window around the border and comparing outcomes across the border will measure the effect of the policy in isolation from other policies or changes impacting both sides of the border.

We measure this spatial discontinuity using a simple linear regression.

$$Y_{ijt} = \phi_0 + \phi_1 T_j + \phi_2 Dist_i + \phi_3 T_j * Dist_i + \xi_{ijt}$$

In this specification, $Dist_i$ measures the distance between unit i and the policy border, measured as negative on the low side of the border and positive on the high side. Our coefficient of interest is ϕ_1, which measures the discontinuity in the outcome at the border. We implement this regression using 2017 data, since the border is created by the post-period variation in policy, and focus on the sample within 1 kilometer of the border. We use a parametric specification for simplicity; results are similar if we use common non-parametric regression discontinuity designs with optimal bandwidth selection.

5 Results

5.1 Landlords Avoid Voucher Tenants

We find that landlords avoid voucher tenants. In Table 1, the row labeled Positive Response shows landlords' interest in tenants requesting to pay by voucher and those that do not. In the first wave, landlords respond positively 50 percent of the time to cash tenants but only 23 percent of the time to voucher tenants, for a gap of 27 percentage points. This gap remains in the second wave at 29 percentage points. This effect is large. Figure 5 compares response rates based on signals of voucher use and race, as indicated by the name. An inquiry with a black-indicating name receives an economically and statistically significant 4 percentage point penalty, which is similar to racial discrimination measured in other studies.[8] The voucher penalty is 7 times larger. There is no significant interaction between the voucher and race signals, which indicates that landlords do not simply use voucher status to proxy for race.

Both theory and the qualitative literature provide many possible reasons for the voucher penalty. In the theory, voucher tenants could on average have worse draws from the "maintenance cost" distribution, broadly interpreted. This could include actual damage to the unit, compliance costs associated with inspections by the housing authority, preferences of neighbors, landlord prejudice, or incorrect information about all of the above. Qualitatively, most inquiries not receiving a positive response receive no response at all or an uninformative response. When landlords do directly address issues related to vouchers, the most common concerns are that the voucher would not cover the rent or that the landlord wishes to avoid the hassle of working with local government.

The voucher penalty increases with rent. Figure 6 displays this result graphically. The dashed lines show average (lowess smoothed) callback rates from landlords at different rent levels to tenants wishing to pay by voucher in the two waves of the experiment. Solid lines show the same for cash tenants. The gap between lines from the same wave measures the voucher penalty at a particular rent for that wave. At the lowest rent units, callback rates are more similar, and in some cases voucher tenants receive higher response rates. However, as rent increases, a gap appears such that tenants signaling a desire to pay by voucher receive much lower callback rates than those who give no such signal. For each \$100 that the posted rent of a unit increases, the gap between landlord responses to voucher and cash tenants widens by 0.75 percentage points. In the 2017 data, the 10th and 90th percentiles of the rent distribution in our data are \$1,279 and \$2,950 per month, respectively. Hence, the voucher penalty will be about 12 percentage points larger at the 90th percentile of the rent distribution than at the 10th percentile.[9]

As predicted by the theory, these results indicate that landlord behavior could help explain why voucher tenants tend to lease up in low-opportunity neighborhoods. The existence of a unit and a voucher to pay the rent do not guarantee that a tenant will be able to rent that unit. Since the voucher penalty increases with posted rent, the gap between the number of listed units truly accessible to voucher tenants grows with neighborhood rent. Even absent tenant preferences about different neighborhoods, voucher tenants would be directed to lower-rent, lower-opportunity neighborhoods on average.

5.2 Most Opportunity Landlords Do Not Respond to Increased Payments

5.2.1 Difference-in-Differences Results

Paying landlords market rent in high-rent neighborhoods does not eliminate the voucher penalty. We study this response using the 2017 increase in voucher payment limits affecting only high-rent neighborhoods in Washington, DC. Table 3 helps quantify this result. The first column gives a sense

[8] We note that this is the initial inquiry, which does not account for racial discrimination that may occur at subsequent points in the lease-up process. See (Curley et al., 2019) for related evidence.

[9] See Appendix Table 10 for regression results supporting both figures.

of the magnitude of the policy change. We estimate the relationship between the voucher value and the policy change in a simple difference-in-differences framework. The outcome is the monthly voucher rent limit in dollars. We find a coefficient of 450 on the interaction between treatment and the year 2017 dummy. This value indicates that the voucher limit increased by on average $450 per month more in treated tracts than in control tracts. This change is large, statistically significant, and passes all standard weak instrument tests.

The remaining columns of Table 3 show how landlord responses to voucher tenants changed in response to the policy. The second column estimates a simple difference-in-differences specification on the correspondence experiment data, restricting the sample to inquiries that request to pay by voucher. Prior to the policy change, callback rates were 8.8 percentage points lower in the high-rent neighborhoods. The coefficient on the interaction term shows that this gap does not close significantly in 2017. The positive coefficient of 0.024 indicates that the gap may have closed slightly, but this is not statistically significant. Column (3) estimates a placebo test of the same model for inquiries not requesting to pay by voucher. The interaction coefficient of 0.10 indicates that the gap between high- and low-rent neighborhoods actually does close for these tenants. This result suggests that it is important to control for other factors that change in high-rent neighborhoods over time other than the voucher payment limits. Any inference from the results in column (2) would overstate the benefits of increased rent limits. Thus, our preferred triple-difference specification in column (4) finds no evidence of positive landlord responses to higher payment limits. Taken literally, the triple interaction term of -0.080 indicates that the voucher penalty assessed by landlords actually became larger over time in neighborhoods with increased rent limits, relative to neighborhoods that did not change. Though this estimate is not statistically significant, its 95 percent confidence interval rules out large improvements in landlord response. At the most optimistic end of this confidence interval, paying landlords $450 more per month increases positive landlord response rates by at most 5 percentage points, which is only 17 percent of the 29 percentage point voucher penalty applied by landlords. These results indicate that the housing authority would likely have to increase voucher payments enormously to eliminate the voucher penalty among the landlords we test.

5.2.2 Spatial Regression Discontinuity Results

Results are similar if we test for discontinuities across the border between the areas affected and not affected by the policy change. Figure 7 displays this result graphically. Panel (a) shows that the monthly voucher limit increases by roughly $1,000 per month at the border. To the left, neighborhoods unaffected by the policy change have voucher limits that average just below $2,000 per month. On the right, neighborhoods affected by the policy change have voucher limits close to $3,000 per month. Panel (b) verifies the validity of this research design using tenants who do not signal a desire to pay by voucher. Positive response rates from landlords are similar on either side of the border for tenants who do not mention the voucher program, as expected. Finally, panel (c) previews the main result. Landlord responses to tenants signaling a desire to pay by voucher also show no discontinuity at the border, despite the large change in rent limits.

Table 4 quantifies the spatial discontinuity estimates. The coefficient on the treatment neighborhood dummy is the focus. Column (1) shows that voucher limits increase by $1,028 per month on average at the border. The main test for policy impacts is in column (2). The next three columns verify that housing units on either side of the border are similar in terms of how landlords respond to cash tenants (3), rent (4), and number of bedrooms (5). The negative and statistically insignificant coefficient in column (2) does not provide evidence that landlords respond more positively to voucher tenants on the side of the border with greater rent limits. At the edge of the 95 percent confidence interval, roughly $1,000 per month in voucher payments buys at most 14.0 percentage points of positive responses. As with estimates from the difference-in-differences design, spatial RD estimates suggest that eliminating the voucher penalty would require an exorbitant increase in the rent limit.

5.2.3 Heterogeneity by Posted Rent

There are at least two reasons to examine effects by posted rent. First, the two waves of the experiment impose different sample restrictions based on posted rent. The greatest rents in the 2015 wave of our experiment equal 130 percent of the metro-wide FMR, while the 2017 wave includes amounts up to 175 percent of FMR. Imposing common limits can ensure that sample differences do not drive our results. Second, theory predicts that landlords may respond differently to vouchers depending on the posted rent of their unit. In a model where posted rents reflect a unit's quality, some landlords will avoid vouchers because the voucher fails to pay the going market rate for that unit. The simplest version of this model predicts that only landlords posting rent above the payment limit will avoid voucher tenants. Then, raising the voucher payment improves landlord responses but only among those units with posted rent between the old and new payment limits.

However, we find consistent results that, regardless of posted rent, landlords respond little to higher voucher payments. Table 5 estimates the triple-difference specification in various subsamples depending on posted rent relative to the metro-wide FMR. The first column replicates the full-sample main result. The triple-interaction coefficient of -0.080 indicates that, if anything, landlords respond less positively to voucher tenants after the policy change. The second column imposes common support between the two waves of the experiment, limiting the sample to units posting rent no more than 130 percent of the metro-wide FMR. The negative and statistically insignificant triple-interaction coefficient of -0.046 matches our prior result that greater voucher payments show no sign of narrowing the voucher penalty imposed by landlords. The final three columns examine samples ranging from low to high posted rent. The triple-interaction coefficient for each sub-sample continues to be negative and statistically insignificant. Graphically, panel (d) of Figure 7 shows similar results limiting the border discontinuity design to units listed above 130 percent of FMR. Overall, we find no evidence that landlords respond to higher voucher payments differently depending on posted rent.

5.3 Detecting and Describing Marginal Opportunity Landlords

We define *marginal opportunity landlords* as landlords who own units in high-rent neighborhoods and become more receptive to renting to voucher tenants when voucher payments increase. The correspondence experiment results indicate that most high-rent landlords are not marginal opportunity landlords. However, we use data from lease-up locations, specialty rental listings, and property tax records to detect and describe a subset of landlords who are on the margin of facilitating opportunity moves.

5.3.1 Detecting Changes in Lease-Up Locations

The first evidence that some landlords respond to higher payments is that equilibrium lease-up locations change; new movers are now just as likely to move to high-rent and low-rent neighborhoods. Figure 8 shows how voucher lease-up locations have evolved over time for high-rent versus low-rent tracts. Each sub-figure splits tracts into high-rent neighborhoods affected by the increased rent limits versus those not affected. As before, treatment tracts are those for which the 2017 limit is greater than 130 percent of FMR. In the left panel, the dashed and solid lines show the average number of voucher tenants leased-up in neighborhoods that were and were not affected by the policy change, respectively. High-rent neighborhoods have far fewer voucher tenants, and we see only a small decrease in this gap after 2017. However, these numbers include both tenants remaining in their existing leases and tenants who move. The right panel focuses on just those tenants who move because they either change units or have a new voucher. While high-rent neighborhoods attracted far fewer new voucher tenants than low-rent neighborhoods between 2012 and 2016, this trend dramatically changed in 2017, closing the gap.

Table 6 quantifies these results. Each column estimates a simple difference-in-difference specification on a panel of census tracts with a different outcome related to voucher lease-up. Column (1) examines the total number of vouchers in the tract. The first row shows the coefficient on the interaction between a dummy for whether the tract was affected by the policy change and a dummy for whether the observation comes from after the change went into effect. The coefficient of 3.58 indicates that the number of voucher tenants increased by about 3.6 households in tracts with increased limits, relative to tracts where the policy change did not affect the rent limit. This effect is small in absolute terms relative to the mean of 60.6 tenants per tract and not statistically significant. However, column (2) indicates that this effect exists and is quite large for newly arriving voucher tenants. The coefficient of 3.25 is statistically significant at the 1 percent level and indicates a large effect among this group. Columns (3) and (4) show that the increase in vouchers is large in percentage terms and opens up new neighborhoods that previously had no voucher tenants. The likelihood that a tract has any voucher households increases by 13 percentage points in tracts with higher payment limits. Among the group of voucher tenants who are moving, higher rent limits affect lease-up locations and appear to induce moves into tracts where vouchers were totally absent.[10]

Because the flow of voucher tenants is relatively low, the changes in lease-up locations we observe suggest the existence of a relatively small number of marginal opportunity landlords. The average tract receives about three new voucher tenants in a given year, and the policy change has an effect roughly equal to this mean. With 96 treatment tracts, we estimate the policy moves about 300 households per year to the treatment neighborhoods. This is small relative to the entire stock of 11,612 vouchers in DC in 2017. On the other hand, 300 households per year is similar to the CMTO experiment in which just over 200 new voucher families received treatment, of whom 40 percent were induced into opportunity moves (Bergman et al., 2019). Similarly, the Baltimore Regional Housing Partnership has existed for about 15 years and currently serves about 4,000 families, which yields 267 families per year.

If these magnitudes represent the upper range of programmatically and politically feasible housing mobility programs, such programs will need to persist over many years to change the stock of voucher tenant lease-up locations. Will the equalization of the flow of voucher tenants we documented persist? The answer depends in part on whether more marginal opportunity landlords can be drawn into the voucher program. Are they rare and unusual or relatively common? Who are the marginal opportunity landlords? The remainder of this section addresses this latter question.

5.3.2 Detecting Changes in Listings

While most landlords' responses to voucher tenants do not change with voucher payments, we identify a set of landlords who do respond to higher payments by targeting listings to voucher tenants. Table 7 shows results from listings on DCHousingSearch.org, a website specializing in subsidized and/or income-restricted rental housing. For each tax neighborhood-year between 2010 and 2018, we count the number of unit listings. The first column shows a difference-in-difference regression with the inverse hyperbolic sine of the number of listings as the outcome. The coefficient of -2.07 on the Above 130 percent dummy indicates that high-rent neighborhoods see far fewer postings than low-rent neighborhoods prior to the policy change we study. The positive coefficient on the interaction between Above 130 percent and being after the 2017 policy change indicates that the number of listings increases in high-rent neighborhoods relative to low-rent neighborhoods after 2017. The value of 0.48 indicates that the number of listings increased by 126 percent in treatment neighborhoods after the policy change relative to before it.[11] The panel of voucher specialist listings also spans the limited introduction of neighborhood-based voucher payments in 2015, allowing us to test for its effect as well. For our treatment neighborhoods, this policy increased voucher payments from 100 percent of FMR to 130 percent of FMR. The coefficient on the interaction between being a treatment tract and the years 2015-2016 indicates a similar increase in listings for this policy

[10] Appendix Table 11 confirms that these results hold if we restrict the sample to tracts on the border of the policy change.

[11] As with log transformations, inverse hyperbolic sine coefficients only approximate percentages when working with dummy variables and large changes. Following Bellemare and Wichman (2019), we calculate percent changes as $\frac{sinh(\hat{y}_1)}{sinh(\hat{y}_2)} - 1$

change.

These specialist listings respond to higher payments with more units in exactly the rent range affected by the policy. The remaining four columns of Table 7 estimate the same difference-in-difference specification counting only subsamples of listings in specific ranges with respect to FMR. For example, the fourth column counts only units listed between 130 percent and 175 percent of the metro-wide FMR. Units listed in this range were more expensive than neighborhood payment standards prior to 2017 but within them after 2017. The positive coefficient of 0.40 is statistically significant and the largest among the various rent ranges for 2017. This result indicates that the policy change not only generated voucher specialist listings in high-rent neighborhoods overall but particularly for units between the old and new voucher payment ceilings. The second row of Table 7 shows a similar pattern for the 2015 policy change with payments up to 130 percent of FMR generating listings up to 130 percent of FMR.

The increase in the number of voucher specialist listings contrasts with what we observe for the broader population of landlords. Figure 9 shows kernel densities of posted rent relative to FMR split out by year, treatment vs. control neighborhoods, and website. Panel (a) visually replicates the quanitative results from above. In high-rent treated neighborhoods, the distribution of posted rents shifts from being centered at FMR before 2015 to 130 percent of FMR in 2015 to values above 130 percent of FMR in 2017. Panel (b) shows that a similar shift does not happen in low-rent control neighborhoods. Panels (c) and (d) show corresponding information for the full set of listings on the mainstream website used for our correspondence experiment during our 2017 sample period. While we cannot compare trends over time for this website due to data limitations, the larger population of landlords shows neither a concentration of listings around neighborhood rent limits nor a contrast between treatment and control neighborhoods.

5.3.3 Describing Marginal Opportunity Landlords

If most landlords do not respond to greater voucher payments, who are the landlords on the margin of accepting voucher tenants when payments increase? Landlords in high-rent neighborhoods who are on the margin of accepting voucher tenants differ from both majority market and voucher specialist landlords on multiple dimensions. To identify these differences, we turn to property tax data. We focus only on listings from 2017, which is the year for which all of our data sets overlap. Table 8 displays summary information for the tax assessment data. Each column corresponds to landlord characteristics for a different set of listings. The first column shows listings associated with "marginal opportunity landlords." These are high-rent units listed on DCHousingSearch.org in high-rent neighborhoods after DCHA increased voucher payments in these neighborhoods. More precisely, they are units listed for 130 percent to 175 percent of FMR in treatment neighborhoods. The second column provides a comparison to all listings on DCHousingSearch.org. The final column shows all listings on the mainstream website.

We document three facts about these marginal opportunity landlords:

First, marginal opportunity landlords have relatively little experience specializing in voucher tenants despite being existing property owners. The first two rows of Table 8 show landlords' experience with listing units on the voucher specialist website prior to the policy change in 2017. The vast majority had no prior experience. Only 24 percent of marginal opportunity landlords had ever listed a unit on DCHousingSearch.org before 2017, and on average they had only listed 9 percent of their properties. This lack of experience is not due to general inexperience with being a landlord; only a quarter of these landlords had purchased the listed property since 2015. The past experience of marginal opportunity landlords contrasts with the landlords behind the broader set of voucher specialist listings. These landlords were nearly twice as likely to have some prior experience (40 percent). Marginal opportunity landlords do have somewhat more experience with vouchers than the general population of landlords, though. Of listings on the mainstream site, 21 percent come from a landlord who has ever listed a property on DCHousingSearch.org, and mainstream landlords have only listed 1 percent of their properties on average, suggesting a handful of large landlords with many mainstream listings and few specialist listings. Altogether, marginal opportunity landlords have more experience with vouchers than most landlords but still only limited background.

Second, marginal opportunity landlords are uniquely exposed to both parts of DC's segmented housing market. On average, landlords listing high-rent units in high-rent neighborhoods on a voucher specialist site have 52 percent of their other units in high-rent treatment neighborhoods and 48 percent in low-rent control neighborhoods. This even split is unusual. Landlords listing on the voucher specialist site have only 28 percent of their units in high-rent neighborhoods, while landlords listing on the majority market site have 85 percent of their units in high-rent neighborhoods. Most landlords appear to specialize in high- vs. low-rent neighborhoods, but marginal opportunity landlords are more likely to have exposure to both parts of the market.

Third, marginal opportunity landlords operate on a surprisingly small scale. They own few properties. They own a mean of 11 properties and a majority own only one; 79 percent own 5 properties or fewer. The properties that marginal opportunity landlords own are also small. Only 13 percent are multi-family high-rise buildings with more than three stories. In their scale, marginal opportunity landlords are similar to other voucher specialists. The average landlord listing on DCHousingSearch.org owns 8 properties, 77 percent own 5 properties or fewer, and only 9 percent of their properties are high rise. By constrast, the typical majority market listing is associated with a much larger landlord. Those landlords own a mean of 127 and a median of 9 properties. More than half of their advertised units are in high rises.

5.3.4 Pricing by Marginal Opportunity Landlords

In addition to these observable characteristics, we find some evidence from posted rents that marginal opportunity landlords are negatively selected on hard-to-observe unit characteristics. We use a simple hedonic model to test whether units on DCHousingSearch.org are listed for lower rent, conditional on characteristics observable in the listing and tax data. Using OLS, we estimate a simple regression on the pooled sample of listings from both the majority market site and

DCHousingSearch.org:

$$ln(R_{ijt}) = \alpha_0 + \alpha_1 S_i * T_i + \alpha_2 S_i * (1 - T_i) + \alpha_3 T_i + X_{it}\delta + \mu_j + \epsilon_{ijt}$$

In this regression, $ln(R_{ijt})$ is the log posted rent of unit i in neighborhood j at time t. S_i is a dummy for being listed on the specialist site, and T_i is a dummy for being listed in a high-rent treatment neighborhood. We control for tax neighborhood fixed effects, μ_j, as well as X_i, which includes number of bedrooms dummies, a quadratic in square footage, a dummy for square footage missing, tax assessment amounts, land area, time since last sale, last sale price, the interaction of time since last sale and sale price, dummies for building use code, and listing month dummies. We are interested in the coefficients α_1 and α_2, which measure the rent premium or discount for specialist listings relative to majority market listings in high- and low-rent neighborhoods, respectively. These conditional differences in rent can be interpreted as selection on unobservables into the specialist market, a rent premium charged to voucher tenants, or a combination of the two.[12]

Table 9 shows the results of this regression. The first column shows the unconditional difference in log rent between specialist and housing listings. The coefficient of -0.17 in the first row indicates that in treatment neighborhoods, listings on DCHousingSearch.org are on average listed for 17 percent less rent than those on the majority market website. Observable characteristics account for about half of this difference. The remaining columns of Table 9 progressively add observable characteristics. In columns (2)-(4) we add unit characteristics from listings, property characteristics from tax assessments, and tax neighborhood fixed effects. Adding these covariates reduces the coefficient to -0.07. This discount remains the same when allowing the neighborhood fixed effects to interact linearly with latitude and longitude in the final column. A unit on DCHousingSearch.org gets listed for about 7 percent lower rent than a unit on the majority market site with comparable location, listing characteristics, and property tax assessment characteristics. This remaining discount for voucher specialist listings likely indicates that negative selection on hard-to-observe characteristics exists and outweighs any premium charged by landlords to voucher tenants. Since we control flexibly for unit location and for many primary unit characteristics, this difference likely reflects idiosyncratic unit quality, e.g., furnishings and building amenities. The second row of Table 9 shows the same discount in low-rent control neighborhoods; if anything, negative selection is less pronounced in high-rent neighborhoods than in low-rent neighborhoods.[13]

Lower prices, though, could hide a combination of negative selection on unobserved characteristics and a smaller magnitude of non-competitive pricing. As discussed in the theory section, landlords may charge a premium over market rate to voucher tenants if the housing authority does not strictly enforce rent reasonableness. The above results indicate that any premium charged would be more than offset by negative selection, but both could be significant. This distinction

[12] The rent premium, or slack, should not be negative; a landlord whose unit would draw *lower* rent for a voucher tenant than for a cash one would simply move to the cash market. A negative coefficient for α_1 or α_2 implies that the value of slack is no greater than the value of the unobserved quality difference.

[13] We obtain similar results after using the propensity score to impose common support.

matters for policy. Whether higher payments provide an efficient means to move voucher tenants into different neighborhoods or an inefficient transfer of economic rents to landlords depends on whether landlords change pricing behavior in response to higher payments. If landlords always list units targeted to voucher tenants at market values, then higher payments simply draw in landlords with higher quality units. On the other hand, if higher payments (combined with lax enforcement of unit-specific rent reasonableness requirements) incentivizes landlords to list units at the voucher payment cap instead of at market rent, resources will be transferred to landlords.

We find direct evidence that some landlords targeting voucher tenants post rent exactly equal to neighborhood rent limits; however, marginal opportunity landlords appear less likely to do so. Figure 10 provides the evidence. Panel (a) displays rental listings used in the correspondence experiment, while panel (b) shows those on DCHousingSearch.org. Each figure plots a histogram of the difference between rent and the neighborhood voucher payment limit. The value of zero indicates that the posted rent exactly equals the limit. As is apparent, the majority market rental listings show no sign of clustering around neighborhood rent limits, while voucher specialist listings show a clear spike.[14]

However, listings with posted rents matching voucher limits primarily appear in low-rent neighborhoods. In panels (c) and (d), we split the DCHousingSearch.org listings by control and treatment neighborhoods. The spike only appears in low-rent control neighborhoods. As predicted by the theory, a significant fraction of landlords price units exactly to payment limits, but this practice is concentrated in low-rent neighborhoods. This fact, combined with the discount observed for DCHousingSearch.org listings in hedonic regressions, suggests that marginal opportunity landlords lease to voucher tenants at rent close to market rates. Thus, the policy we observe, which increases payments in high-rent neighborhoods and leaves them unchanged in low-rent neighborhoods, does not appear to transfer economic rents to opportunity landlords.

6 Summary and Concluding Remarks

This paper documents how landlords shape access to neighborhood opportunity and the extent to which increasing voucher payments can encourage landlords to facilitate moves to opportunity. We conduct two waves of a correspondent experiment, sending inquiries from fictional voucher tenants to rental housing listings in Washington, DC. These two experiments bracket a policy change in which DC increased maximum voucher payments in high-rent neighborhoods. We find that *opportunity landlords*, i.e., landlords in high-rent neighborhoods, often screen out voucher tenants, and most landlords do not change their screening decisions in response to increased payments. However, a few landlords do respond positively to the policy change, and their response is enough to equalize the flow, though not stock, of voucher tenants into high- versus low-rent neighborhoods. Using rental listing and property tax data, we show that these *marginal opportunity landlords* are

[14] Appendix Figure 16 shows results for different bin widths. Also, this spike is not due to particular importance of any particular number to the DCHousingSearch.org website. Appendix Figure 17 shows a placebo test matching 2017 rent limits to 2015 listings with no such spike.

unusual. They are few in number, operate at small scale, and have properties exposed to a wide variety of neighborhoods. They negatively select into voucher specialization based on hard-to-observe unit features but appear to list their voucher-targeted units at market prices.

Although the evidence we have collected is limited to a particular policy in a particular city, we believe this evidence is generalizable and informative for current policy. While Washington, DC, has a unique context, the city's sharp economic and racial sorting is comparable to a broad set of cities in which the discussion of neighborhood opportunity likely matters most. Likewise, the policy of neighborhood-specific voucher payment limits is but one of many potential policies that could persuade opportunity landlords to lease to voucher tenants. Yet this policy is also of special interest given the major expansion of small area fair market rents (SAFMRs) that has just begun. The interaction of the DC context and the SAFMR policy is of particular interest. Because DC is a city in which rent and opportunity are closely linked across neighborhoods, we are studying SAFMRs in an environment where it could be a vital policy tool.

Our results should encourage innovative policymakers to expand engagement with landlords. Indirect landlord engagement through SAFMRs has shown success in multiple, widely varying contexts. More active landlord outreach may be even more effective, though less evidence is available here. Housing authorities have already begun experimenting with a variety of interventions, including landlord outreach and education, larger security deposits, insurance against damage to units, faster and more predictable inspections, payments that offset the opportunity cost of a vacant unit, certification of tenants' preparedness for renting, and active matching of landlords with tenants. Our results suggest this emphasis on experimenting with how to engage landlords is well-placed.

More evidence is needed on how landlords shape access to opportunity and what interventions are most effective in engaging landlords. We find some positive evidence that paying higher rent can encourage moves to opportunity, but we also find concerning evidence that few opportunity landlords are on the margin of accepting a voucher. Those marginal opportunity landlords are few and unusual, which raises a question of whether positive effects can be sustained. More work is needed to understand how landlords respond to mobility interventions at scale and over longer periods of time. Similarly, little is known about the relative effectiveness of and the complementarities between the many other policies targeted at landlords. We do not know if they induce a similarly small and unusual set of marginal opportunity landlords to engage with voucher tenants or if these higher-touch interventions reach a broader group of landlords. Evidence from this study and other recent work provides evidence that public policy can help families access different neighborhoods, but it remains to be seen whether such policies can draw in enough landlords to support residential moves as a systematic response to unequal opportunity.

7 Figures

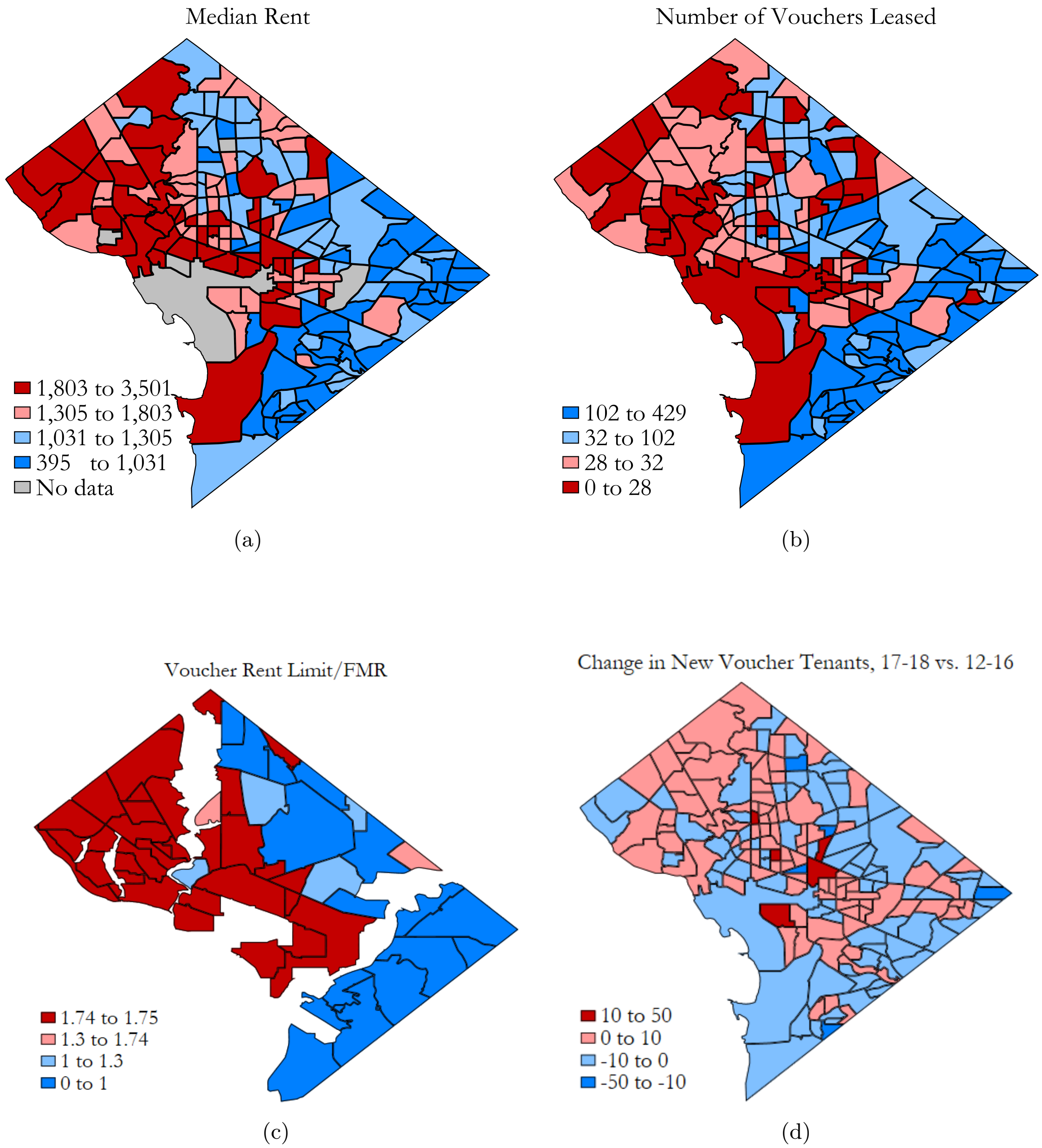


Figure 1: Neighborhoods in Washington, DC

Notes: Each figure shows a map of Washington, DC. Figure (a) shows median rent by census tract from the 2012-2016 ACS. Figure (b) shows the number of HCV residents leased-up by tract from HUD's 2015 Picture of Subsidized Households. Figure (c) shows the ratio of 2017 neighborhood voucher payment limits from DCHA and metro-wide FMR from HUD, by tax neighborhood. Figure (d) shows data from the 2012-2018 HUD Picture of Subsidized Households. It calculates the difference between the number of new vouchers in each tract in 2017-2018 vs. 2012-2016. Source: US Census Bureau, US Department of Housing and Urban Development.

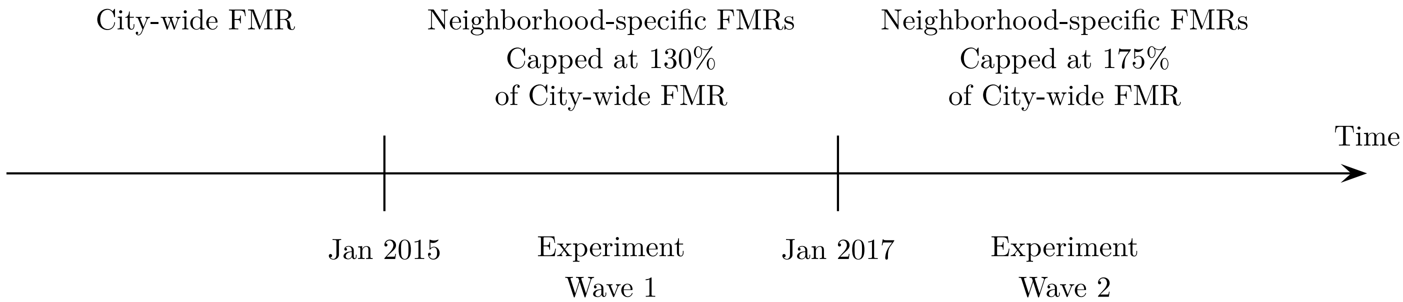
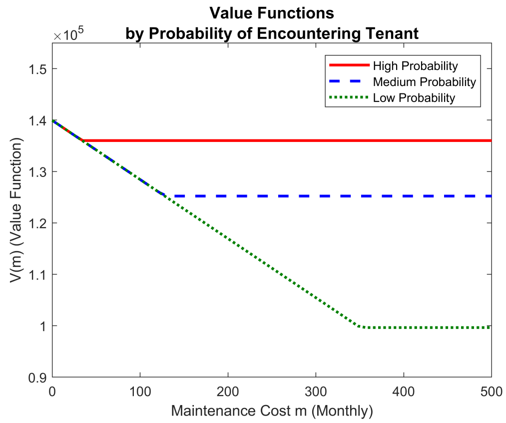
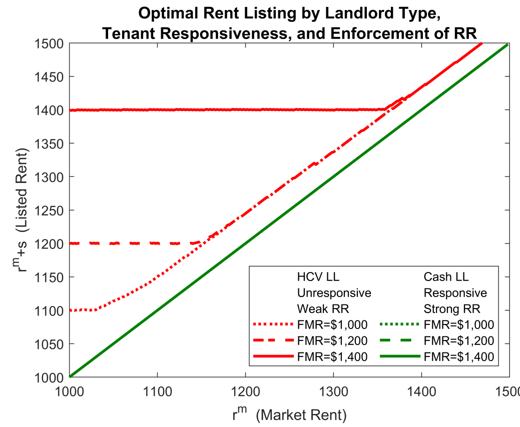


Figure 2: Timeline of Small Area Fair Market Rent (SAFMR) Policy in DC



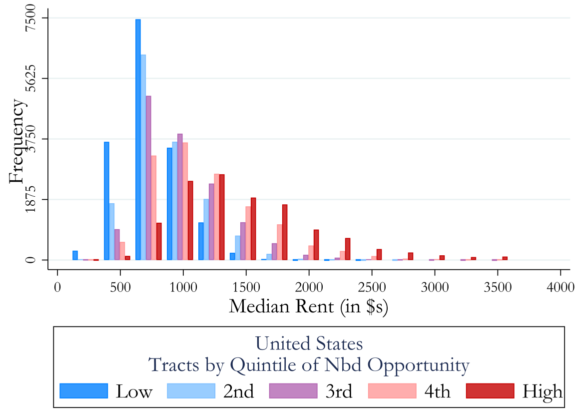
(a) Landlord's Accept/Reject Decision



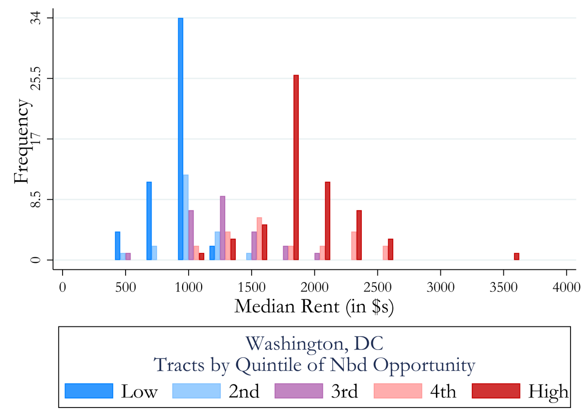
(b) Landlord's Pricing Decision

Figure 3: The Landlord's Decisions

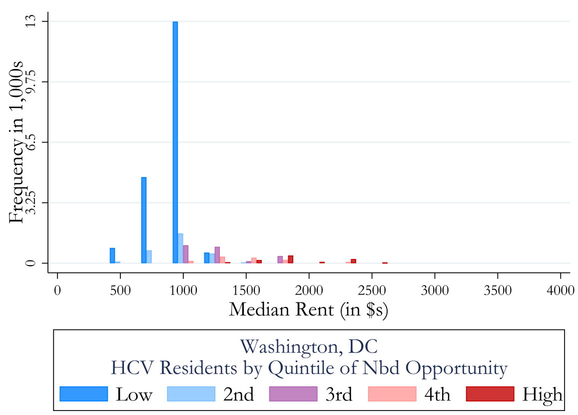
Note: The left panel shows the value functions of a cash landlord in Case 2 of Assumption A1 with weak enforcement of rent reasonableness and slack of $35. The right panel shows the optimal pricing decision rule for voucher and cash specialist landlords under various assumptions about the responsiveness (as measured by $\pi(s)$) of cash tenants to slack (pricing above market rent) and the enforcement of rent reasonableness (the highest level of slack permitted for voucher tenants). Source: Author calculations using model data.



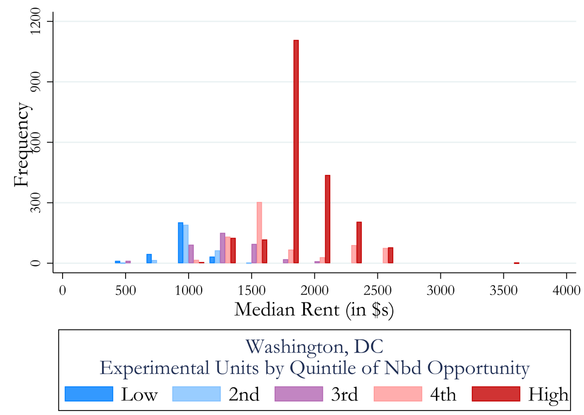
(a) United States



(b) DC - All Residents



(c) DC - HCV Residents



(d) DC - Experimental Units

Figure 4: Joint Distribution of Median Rent and Neighborhood Opportunity

Notes: Each figure shows a two-way histogram counting the frequency of census tracts with a particular rent range and opportunity index quintile. Figures (a) and (b) assign one observation per tract in the US and DC, respectively. Figures (c) and (d) weight tracts by the number of vouchers and number of listings used in the experiment, respectively. The opportunity index is the first principal component of the poverty rate, the unemployment rate, the employed to population ratio, the share with a HS diploma, the share with a BA, and the share of families with children under 18 that are single-headed from the ACS. Each of these variables is first put into percentiles of the national distribution (in terms of population living in census tracts with these characteristics). We denote opportunity as the tract's percentile in the distribution of the resulting index/principal component. Source: US Census Bureau, US Department of Housing and Urban Development, correspondence experiment.

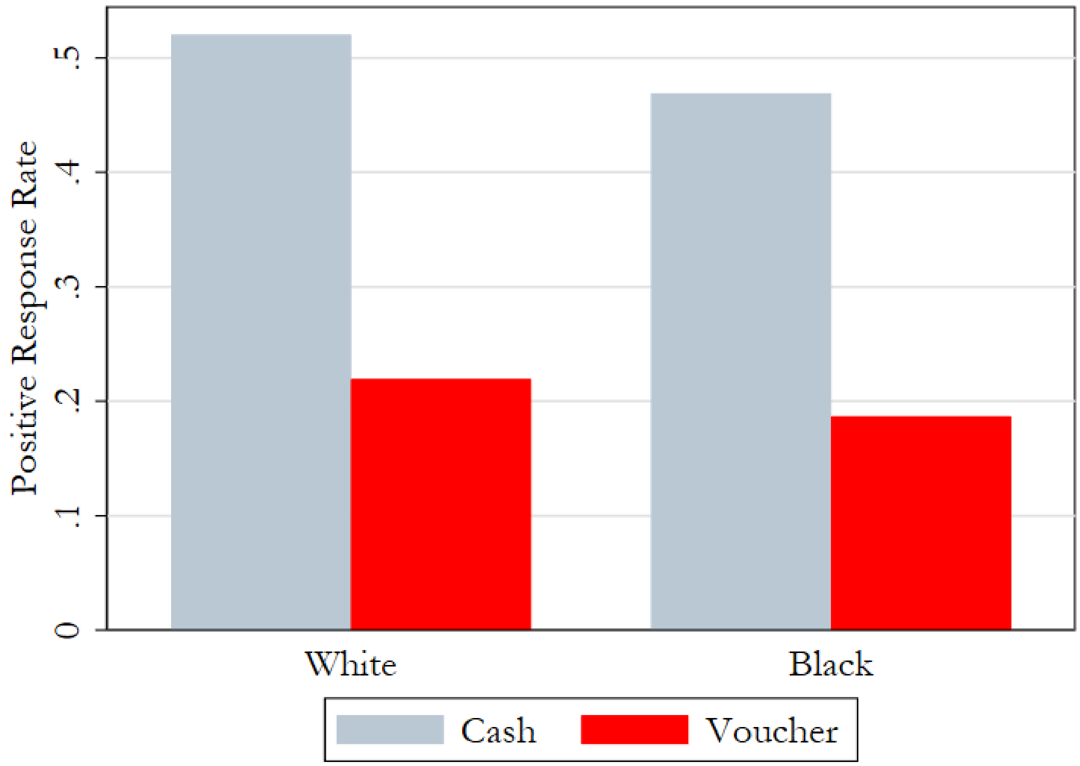


Figure 5: Probability of Positive Landlord Response, by Voucher Signal and Name
Notes: The bars show data from the correspondence experiment on the proportion of fictional inquiries receiving positive responses by sub-group, pooling across 2015 and 2017 waves. Voucher vs. cash indicates whether the voucher signal is present in the inquiry. Black vs. white indicates the race signalled by the name attached to the inquiry. Source: Correspondence experiment.

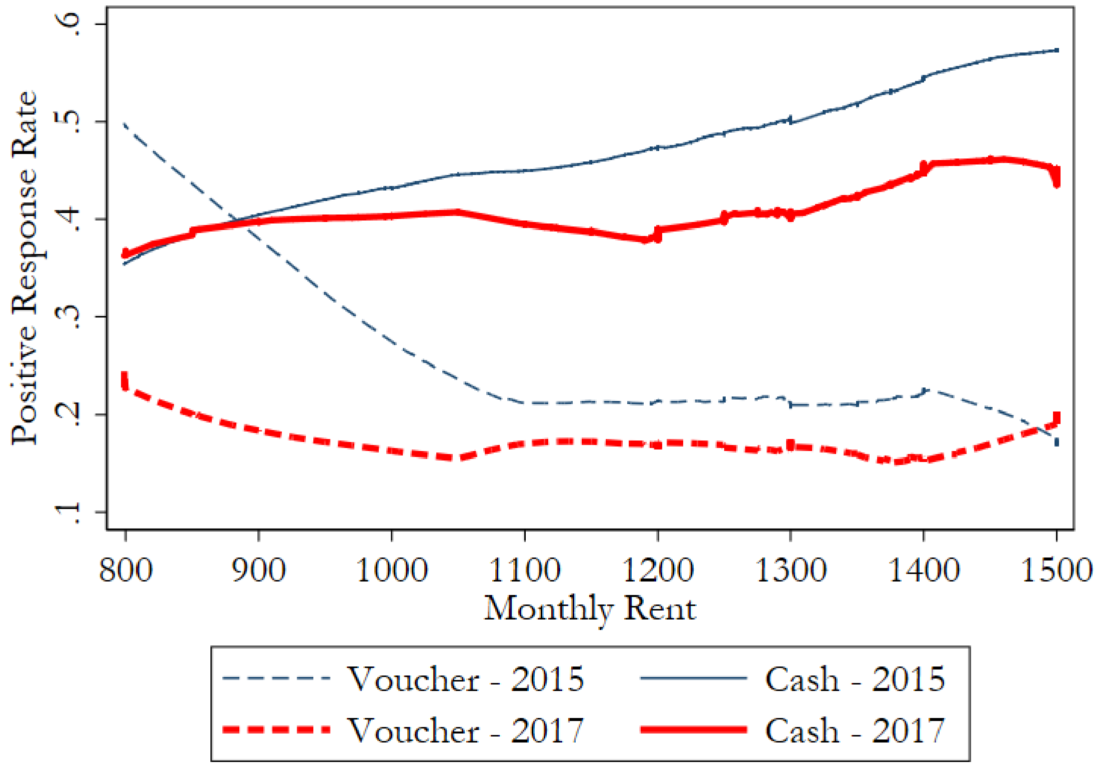
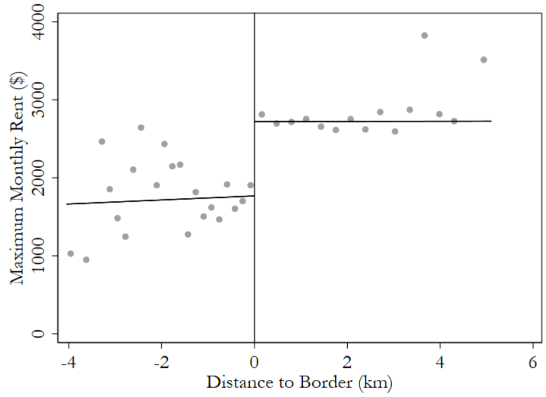
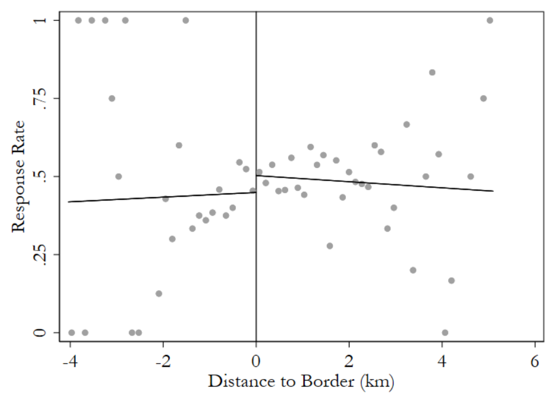


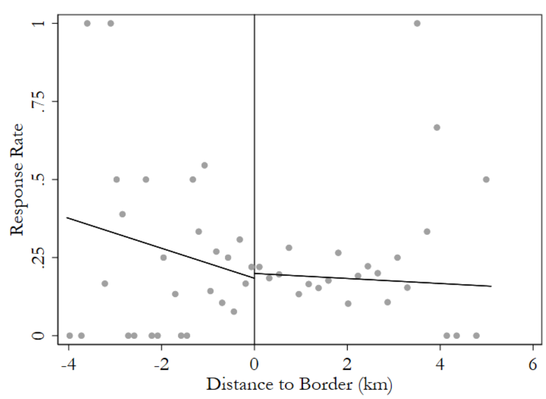
Figure 6: Probability of Positive Landlord Response, by Voucher Signal and Posted Rent
Notes: Each line shows data from the experiment and is a lowess smooth of the relationship between a landlord positive response dummy and the posted rent of the unit. Each line is limited to the sub-sample of the indicated year and voucher signal treatment. The sample is also limited to units with rent between the 1st and 99th percentiles of the rent distribution of both waves. Source: Correspondence experiment.



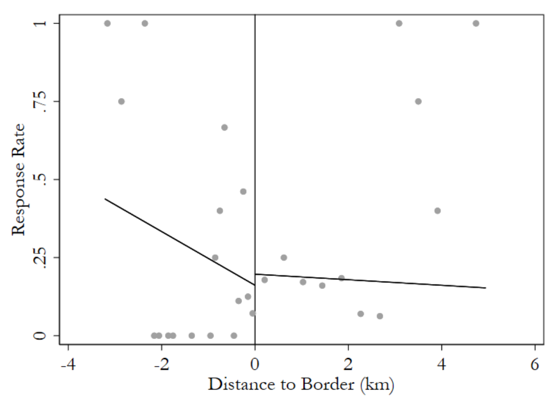
(a) Voucher Limit



(b) Landlord Responses - Cash Tenants



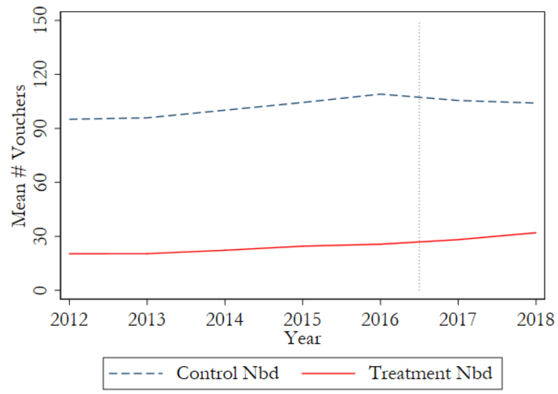
(c) Landlord Responses - Voucher Tenants



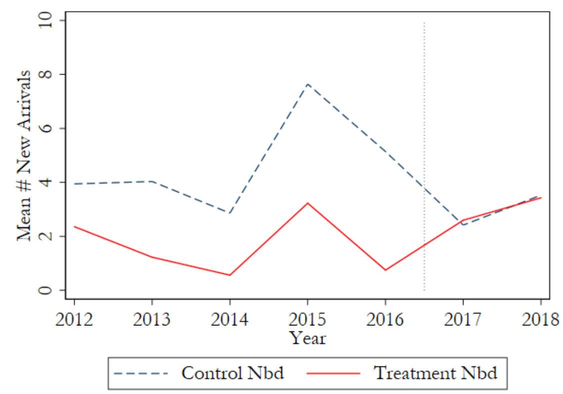
(d) Landlord Responses - Voucher, Rent > 130% FMR

Figure 7: Border Discontinuity Effects

Notes: Each sub-figure shows a regression discontinuity plot with optimal bandwidth and bin width selection from Calonico et al. (2017). Treatment neighborhoods are defined as in Figure 1 1c. The running variable is distance to the nearest border with a tax neighborhood of different treatment status. The running variable is negative in control neighborhoods and positive in treatment neighborhoods. Each plotted point shows the average of the outcome for a particular bin, and the two lines shows the best local linear fit within the given bandwidth. The outcome in (a) is the value of the neighborhood-specific voucher payment limit; all others use a landlord positive response dummy. All figures limit the sample to 2017 inquiries that send a voucher signal, except (c) which limits the sample to those that do not. Figure (d) also limits the sample to units with posted rent above 130 percent of FMR. Source: Correspondence experiment.



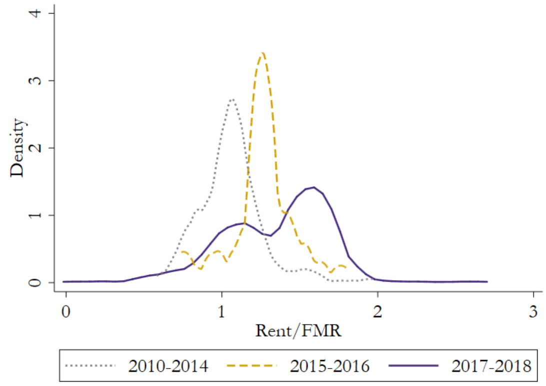
(a) Number of Vouchers



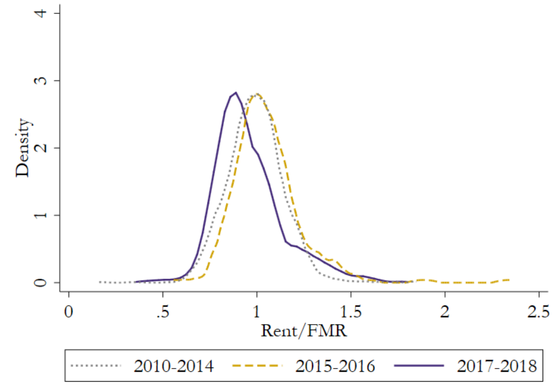
(b) Number of Newly Leased Vouchers

Figure 8: Number of Vouchers, Tracts Affected vs. Unaffected by Policy Change

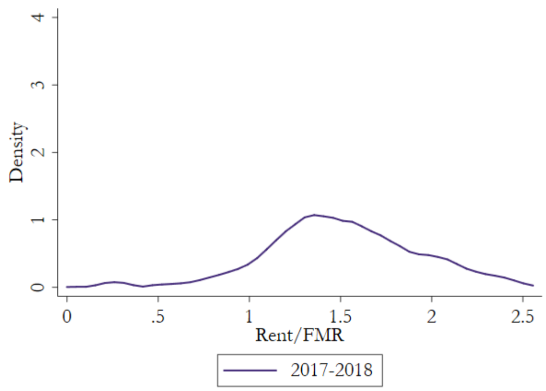
Notes: Data are from the HUD Picture of Subsidized Households. Each line shows an average across census tracts by year and treatment status. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. The left pane shows the number of vouchers in use, deflating the "number of vouchers available" in the data by usage and reporting rates. The right pane shows the number of newly leased vouchers. Source: US Department of Housing and Urban Development.



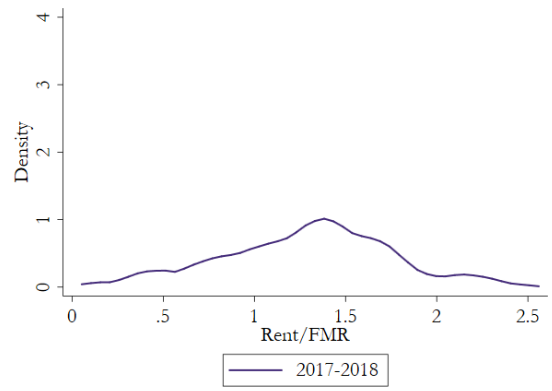
(a) DCHousingSearch - Treatment Nbds



(b) DCHousingSearch - Control Nbds

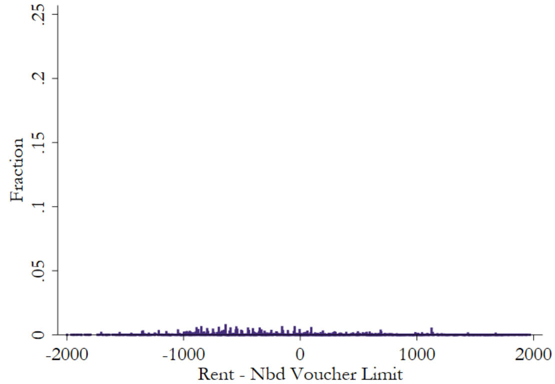


(c) Majority Market - Treatment Nbds

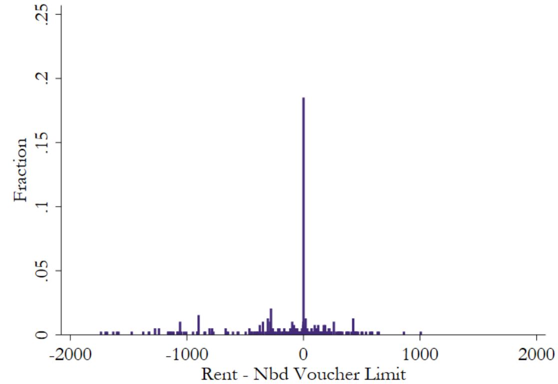


(d) Majority Market - Control Nbds

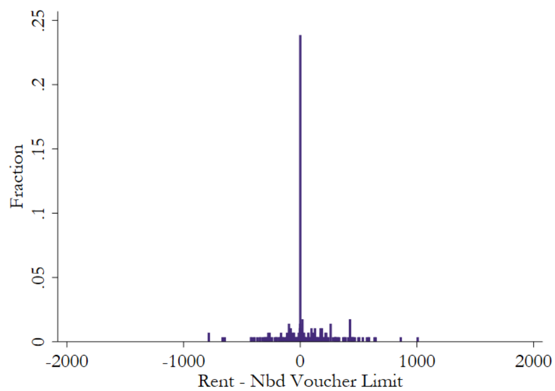
Figure 9: Density of Posted Rent Relative to Fair Market Rent
Notes: Each line shows a kernel density function of the ratio of posted rent and metro-wide FMR estimated with an Epanechnikov kernel. Each estimation examines a subset of observations from rental listings over the listed years, data sources, and neighborhoods. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Source: Correspondence experiment, Social Serve.



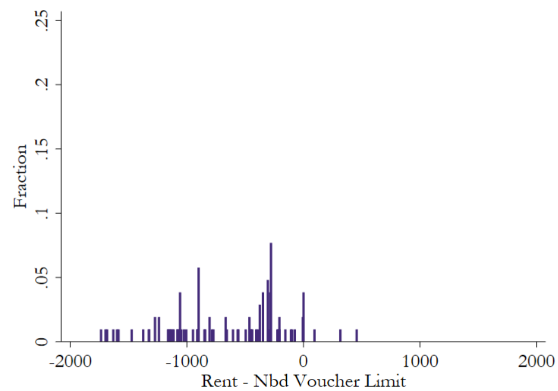
(a) Majority Market



(b) DCHousingSearch



(c) DCHousingSearch - Control Neighborhoods



(d) DCHousingSearch - Treatment Neighborhoods

Figure 10: Frequency of Listings, by Posted Rent Relative to Neighborhood Rent Limit in 2017
Notes: Each graph shows a histogram for data from 2017. The horizontal axis measures the simple difference between the posted rent and the neighborhood-specific voucher payment limit for that unit's neighborhood, i.e. zero indicates a unit listed for exactly the voucher limit. Bin width is 1. Each sub-figure examines a subset of observations from rental listings over the listed data sources and neighborhoods. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Source: Correspondence experiment, Social Serve.

8 Tables

Table 1: Correspondence Experiment Summary Statistics

	2015			2017		
	Cash	Voucher	All	Cash	Voucher	All
Voucher	0.00	1.00	0.25	0.00	1.00	0.50
	(0.00)	(0.00)	(0.43)	(0.00)	(0.00)	(0.50)
Black	0.49	0.50	0.49	0.75	0.75	0.75
	(0.50)	(0.50)	(0.50)	(0.43)	(0.43)	(0.43)
Female	0.50	0.50	0.50	0.49	0.51	0.50
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Monthly Rent	1,252	1,255	1,253	2,046	2,056	2,051
	(212)	(221)	(214)	(701)	(663)	(682)
Bedrooms	0.9	1.0	0.9	1.3	1.3	1.3
	(0.7)	(0.8)	(0.8)	(1.0)	(0.9)	(0.9)
Positive Response	0.50	0.23	0.43	0.48	0.19	0.33
	(0.50)	(0.42)	(0.50)	(0.50)	(0.39)	(0.47)
Sq. Ft.				849	881	865
				(500)	(909)	(735)
N	2,010	658	2,668	2,115	2,149	4,264

The sample comes from two correspondence experiments. The left panel shows the first wave from May to June 2015 and the right panel shows the second wave conducted in July and August 2017. Each cell shows means with standard deviations in parentheses. The first and second columns split out inquiries not signaling use of a voucher vs. signaling use of a voucher, and the final column shows statistics for the combined sample. Source: Correspondence experiment.

Table 2: Neighborhood Summary Statistics

	All	Voucher	Experiment	DCHousingSearch
Med HH Income	82.9	47.8	98.7	56.8
	(42.3)	(25.2)	(37.1)	(35.6)
Med Home Value	522	346	592	375
	(228)	(134)	(191)	(157)
Median Rent	1,386	1,002	1,596	1,106
	(524)	(332)	(441)	(440)
Share Employed	64	53	71	55
	(14)	(12)	(11)	(14)
% Poverty	17.5	27.6	13.6	25.3
	(11.8)	(12.3)	(9.0)	(13.6)
Share College	53.4	26.7	68.8	33.2
	(28.6)	(20.1)	(21.7)	(25.5)
HCV Tenants	150	354	78	274
	(175)	(222)	(115)	(195)
% HCV Moved Last Year	7.0	4.4	9.7	5.4
	(9.9)	(5.7)	(13.7)	(6.9)
N	371,739	25,591	2,213	417

Neighborhood statistics and tract-level characteristics from the 5-year 2013-2017 ACS. The last two HCV variables are from the 2017 HUD Picture of Subsidized Households. Statistics are means with standard deviations in parentheses. The columns use different weights. The columns weight respectively by tract's total population (ACS 2013-2017), number of voucher holders (HUD 2017), number of experimental inquiries in 2017, and number of unit listings in DCHousingSearch.org in 2017. Source: US Census Bureau, US Department of Housing and Urban Development, correspondence experiment, Social Serve.

Table 3: Effect of Increasing Rent Limits on Landlord Voucher Penalty, Triple Difference

	All Voucher Limit ($)	Voucher Response	Cash Response	All Response
Treatment Nbd X 2017	454.8***	0.024	0.10**	0.10**
	(54.8)	(0.048)	(0.048)	(0.048)
Treatment Nbd	668.0***	-0.088*	-0.067*	-0.067*
	(34.6)	(0.047)	(0.034)	(0.034)
2017	286.4***	-0.047	-0.090**	-0.090**
	(42.7)	(0.043)	(0.040)	(0.040)
Treatment Nbd X 2017 X Voucher				-0.080
				(0.064)
Voucher X 2017				0.043
				(0.056)
Treatment Nbd X Voucher				-0.021
				(0.054)
Voucher				-0.26***
				(0.047)
Mean of Dep. Var.	2241.3	0.20	0.49	0.37
R^2	0.66	0.0074	0.0025	0.092
N	6857	2778	4079	6857

The sample comes from the two correspondence experiments. Each column shows the results of a linear regression (or linear probability model). The outcome is a landlord positive response dummy except in the first column, which uses the voucher payment limit as the outcome. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. The voucher dummy is an indicator that the inquiry states a desire to pay by voucher. The sample for the final three columns is inquiries signalling use of a voucher, those without such a signal, and the full sample, respectively. Coefficients for all covariates are listed. Standard errors are in parentheses and are clustered by tax neighborhood. Source: Correspondence experiment.

Table 4: Effect of Increasing Rent Limits on Landlord Voucher Penalty, Border Discontinuity

	Voucher Voucher Limit ($)	Voucher Response	Cash Response	Voucher Rent	Voucher Bedrooms
Treatment Nbd	1028.1***	-0.026	0.041	-9.55	-0.11
	(85.5)	(0.064)	(0.074)	(121.1)	(0.16)
Distance to Border (km)	180.2	0.073	0.073	448.4**	-0.091
	(137.7)	(0.094)	(0.11)	(201.3)	(0.26)
Treatment Nbd X Distance	-318.0**	-0.081	-0.11	-547.9**	-0.32
	(155.6)	(0.11)	(0.13)	(226.3)	(0.29)
Mean of Dep. Var.	2493.2	0.21	0.48	2110.3	1.34
R^2	0.47	0.00070	0.0027	0.018	0.041
N	882	882	854	882	882

The sample comes from inquiries in the 2017 correspondence experiment, restricted to those within 1 km of the border between tracts treated and untreated by the policy. Each column shows the results of a linear regression with the variable listed in the column headings as the outcome. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Distance is negative for control neighborhoods and positive for treatment neighborhoods. All columns use only the sub-sample of inquiries sending a voucher signal, except the third column, which uses only those not sending the signal. Coefficients for all covariates are listed. Standard errors are in parentheses and are clustered by listing. Source: Correspondence experiment.

Table 5: Effect of Increasing Rent Limits on Landlord Voucher Penalty, Triple Difference, Heterogeneity by Posted Rent

	All Response	Under 130% FMR Response	40% to 70% FMR Response	70% to 100% FMR Response	100% to 130% FMR Response
Treatment Nbd X 2017 X Voucher	-0.080	-0.046	-0.13	-0.12	-0.12
	(0.061)	(0.065)	(0.19)	(0.098)	(0.10)
Voucher X 2017	0.043	0.058	-0.11	0.046	0.18*
	(0.053)	(0.056)	(0.13)	(0.079)	(0.095)
Treatment Nbd X Voucher	-0.020	-0.020	0.091	-0.038	0.13*
	(0.047)	(0.047)	(0.15)	(0.068)	(0.072)
Treatment Nbd X 2017	0.10**	0.078	-0.17	0.25***	0.13*
	(0.044)	(0.048)	(0.16)	(0.071)	(0.074)
2017	-0.090**	-0.11***	0.11	-0.13**	-0.21***
	(0.039)	(0.041)	(0.11)	(0.054)	(0.068)
Treatment Nbd	-0.068**	-0.068**	0.037	-0.20***	-0.14***
	(0.030)	(0.030)	(0.094)	(0.046)	(0.045)
Voucher	-0.26***	-0.26***	-0.073	-0.19***	-0.46***
	(0.039)	(0.039)	(0.098)	(0.055)	(0.065)
Mean of Dep. Var.	0.37	0.38	0.33	0.34	0.40
R^2	0.091	0.077	0.035	0.080	0.11
N	6932	5122	415	1734	2942

The sample comes from the two correspondence experiments. The sample varies across columns, limiting the sample to listings with posted rent relative to the city-wide FMR in the range shown at the top of the column. The first column replicates column (4) of Table 3. Each column shows the results of a linear regression (linear probability model) with a landlord positive response dummy as the outcome. The outcome is a landlord positive response dummy. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. The voucher dummy is an indicator that the inquiry states a desire to pay by voucher. Standard errors are in parentheses and are clustered by tax neighborhood. Source: Correspondence experiment.

Table 6: Effect of Increasing Rent Limits on Number of Vouchers Leased-Up per Tract

	Vouchers	New Arrivals	arsinh(Vouchers)	Any Vouchers
Treatment Nbd X Post-2017	3.58	3.25***	0.50***	0.13***
	(2.84)	(0.77)	(0.086)	(0.031)
Post-2017	3.88*	-1.77***	0.0035	-0.016
	(2.09)	(0.45)	(0.033)	(0.011)
Treatment Nbd	-78.3***	-3.20***	-2.43***	-0.22***
	(8.95)	(0.80)	(0.24)	(0.044)
Mean of Dep. Var.	60.6	3.07	3.57	0.87
R^2	0.29	0.025	0.32	0.085
N	1253	1192	1253	1253

The sample comes from the HUD Picture of Subsidized Households. The unit of analysis is a tract-year. Each column shows the results of a linear regression with the outcome listed at the top of the column. The function 'arsinh' refers to inverse hyperbolic sine, which is similar to a log transformation but accounting for zeros. The treatment dummy indicates that the tract of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Coefficients for all covariates are listed. Standard errors are in parentheses and are clustered by tract. Source: US Department of Housing and Urban Development.

Table 7: Effect of Increasing Rent Limits on Number of Voucher Specialist Listings (Inverse Hyperbolic Sine), by Rent Relative to FMR

	All arsinh(Listings)	< 100% FMR arsinh(Listings)	100-130% FMR arsinh(Listings)	130-175% FMR arsinh(Listings)	> 175% FMR arsinh(Listings)
Treatment Nbd X Post-2017	0.48***	0.10	0.32	0.40**	0.031
	(0.15)	(0.12)	(0.19)	(0.16)	(0.031)
Treatment Nbd X 2015-2016	0.33**	0.20*	0.37*	0.059	0.025
	(0.13)	(0.11)	(0.18)	(0.057)	(0.026)
Treatment Nbd	-2.07***	-2.01***	-0.83***	-0.0084	0.0084
	(0.36)	(0.35)	(0.20)	(0.031)	(0.0084)
Year FE	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	1.27	0.98	0.54	0.16	0.013
R^2	0.37	0.43	0.15	0.12	0.017
N	513	513	513	513	513

The sample comes from listings posted to DCHousingSearch.org between 2010 and 2018. In the first column, the outcome is the inverse hyperbolic sine of the number of listings by tax neighborhood and year. The inverse hyperbolic sine is similar to a log transformation but accounts for zeros. The latter columns restrict this count to listings with posted rent (relative to fair market rent) in the listed range. Each column shows the results of a linear regression. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Coefficients for all covariates are listed unless indicated. Standard errors are in parentheses and are clustered by tax neighborhood. Source: Social Serve.

Table 8: Summary of Property Holdings for Landlords Listing Units for Rent

	Marginal Opportunity	DCHousingSearch.org	Majority Market Website
Pre-2017 DCHousingSearch Listing: Any	0.24	0.40	0.21
Pre-2017 DCHousingSearch Listing: Proportion	0.09	0.21	0.01
Listed Property Sold After 2015	0.24	0.22	0.13
Proportion in Treatment Tracts (if >1 Unit)	0.52	0.28	0.85
# Properties	11	8	127
# Properties: 1	0.55	0.44	0.31
# Properties: 2-5	0.24	0.33	0.16
# Properties: 6-25	0.14	0.18	0.21
# Properties: 26+	0.07	0.05	0.32
Type: Single Family	0.22	0.26	0.09
Type: Multi-Family 0-2 Stories	0.65	0.65	0.33
Type: Multi-Family 3+ Stories	0.13	0.09	0.58
Avg. Lot Area (sqft)	7,178	8,134	13,608
Avg. Assessed Value (millions)	2.0	3.6	15.8
Avg. Assessed Value: Land (millions)	0.7	0.8	3.2
Avg. Assessed Value: Improvements (millions)	1.3	2.8	12.6
N	42	341	3696

This table shows the results of matching rental listings to DC property tax records. For any given listing, we match it to a DC property tax record. Within property tax records, we identify other properties with the same owner address, which we use to calculate landlord holdings information for any given listing. There are two exceptions: sale date information is only for the listed property, not the landlord network, and property location excludes the listed property. Each column summarizes landlord characteristics for a different sample of rental listings. The first column shows all listings on DCHousingSearch.org in treatment tracts with posted rent between 130 percent and 175 percent of FMR. The second columns shows all listings on DCHousingSearch.org. The final column shows all listings on the majority market website. All columns are restricted to listings in the year 2017 that successfully match to DC property tax records (which primarily requires having an exact address on the listing). All reported statistics are means. Source: Majority market website, Social Serve, Washington, DC, Integrated Tax System Public Extract.

Table 9: Hedonic Regression Comparing Majority Market to Specialist Listings

	Ln(Rent)	Ln(Rent)	Ln(Rent)	Ln(Rent)	Ln(Rent)
DCHousingSearch X Treatment Nbd	-0.17***	-0.15***	-0.11***	-0.074*	-0.078*
	(0.044)	(0.036)	(0.039)	(0.045)	(0.045)
DCHousingSearch X Control Nbd	-0.27***	-0.36***	-0.28***	-0.13***	-0.13***
	(0.029)	(0.030)	(0.028)	(0.039)	(0.042)
Treatment Nbd	0.15***	0.19***	0.15***		
	(0.023)	(0.020)	(0.017)		
Unit Char. - Listing	No	Yes	Yes	Yes	Yes
Month Indicators	No	Yes	Yes	Yes	Yes
Unit Char. - Tax	No	No	Yes	Yes	Yes
Tax Nbd X Lat	No	No	No	No	Yes
Tax Nbd X Long	No	No	No	No	Yes
Tax Nbd FE	No	No	No	Yes	Yes
Mean of Dep. Var.	7.72	7.72	7.72	7.72	7.72
R^2	0.091	0.50	0.64	0.70	0.71
N	4030	4030	4030	4030	4030

The sample combines 2017 listings from DCHousingSearch.org and the majority market website. We limit the sample to listings that can be successfully matched to DC property tax records (which primarily requires having an exact address on the listing), units priced between $400 and $15,000 per month, and units smaller than 20,000 square feet. The difference between listing on the two websites is denoted by the DCHousingSearch dummy. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Unit characteristics from the listing include number of bedrooms dummies, a quadratic in square footage (zero if missing), and an indicator for square footage missing. Tax assessment unit characteristics are past values of land, past values of improvements, last sale price (zero is missing), last sale date (zero if missing), ever sold dummy, interaction of sale price and sale date, lot size, and use code dummies. Coefficients for all covariates are listed unless indicated. Standard errors are in parentheses and are heteroskedasticity-robust. Source: Majority market website, Social Serve, Washington, DC, Integrated Tax System Public Extract.

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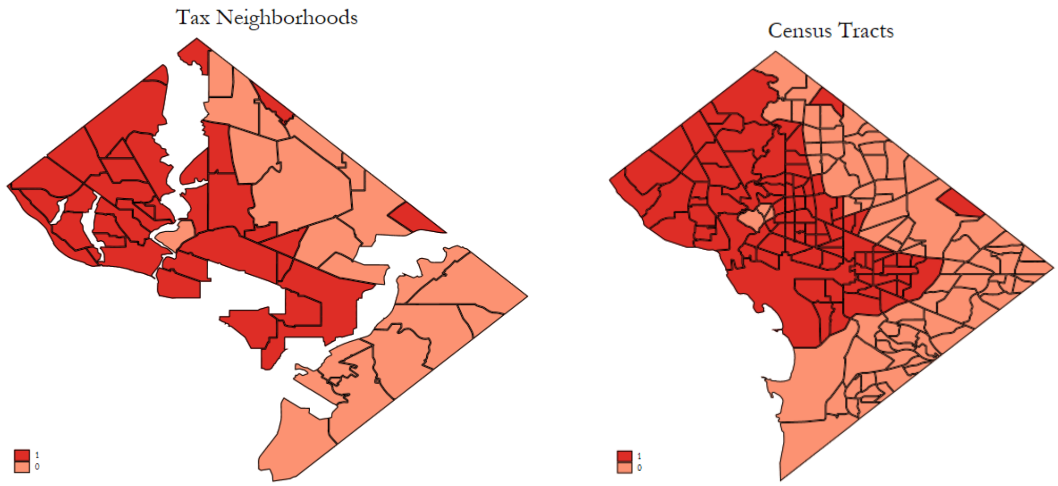
A Appendix: Figures

Hi,

My name is Meredith O'Brien, I am responding to your [redacted] posting for an apartment listed at INSERT RENT AMOUNT/month. I have a long and consistent rental history if you would like references. I plan to pay with Section 8. Is the place still available?

Thank you for your time,
Meredith O'Brien

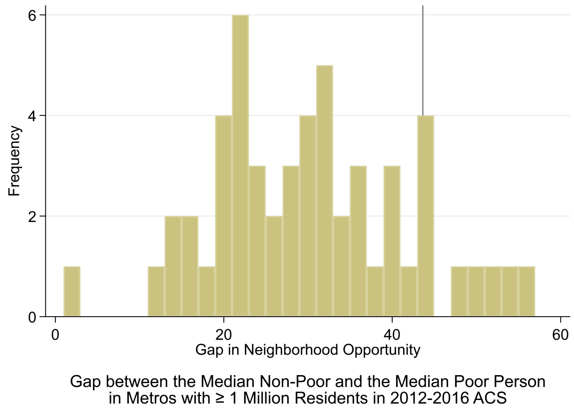
Figure 11: Example Inquiry



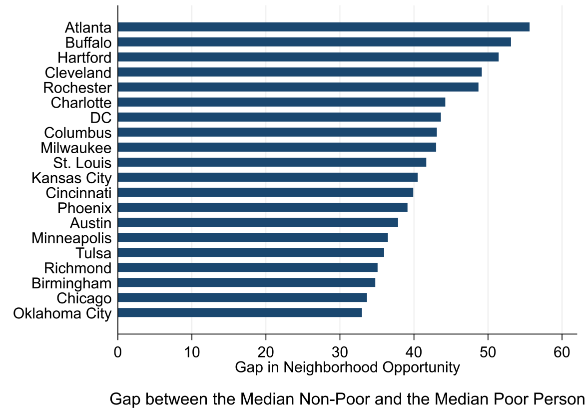
(a) Tax Neighborhoods

(b) Census Tracts

Figure 12: Changes in Rent Limits Notes: Neighborhoods in dark red indicate those that had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Neighborhoods in light red had a voucher payment limit less than or equal to 130 percent of metro-wide FMR in 2017. Source: Author calculations.



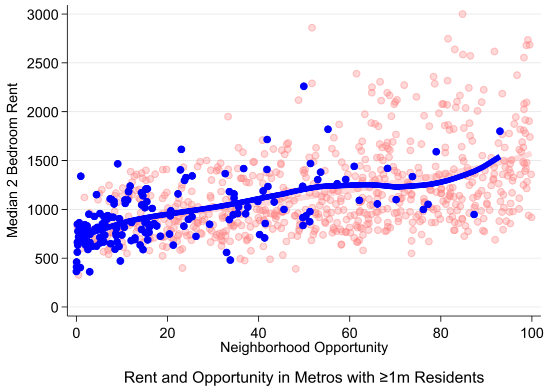
(a) The Distribution of within Metro Gaps



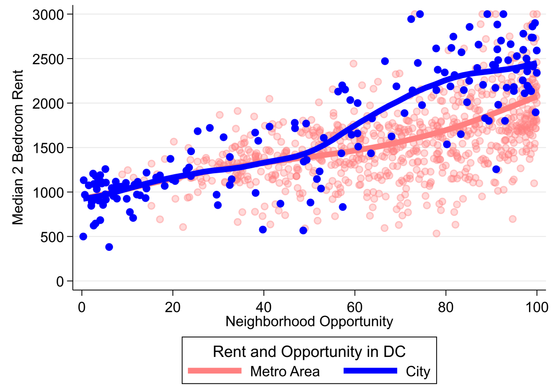
(b) The Top 20 Metro Gaps

Figure 13: Gaps in Neighborhood Opportunity between Poor and Non-Poor Residents

Note: This figure shows the gaps in the neighborhood opportunity of a metro's median poor and median non-poor person. The left panel shows the distribution of the 54 metros in the US with populations of at least 1 million residents in the 2012-2016 ACS. The dotted line shows where DC resides in the distribution. The right panel labels the 20 metros with the largest gaps. Source: US Census Bureau.



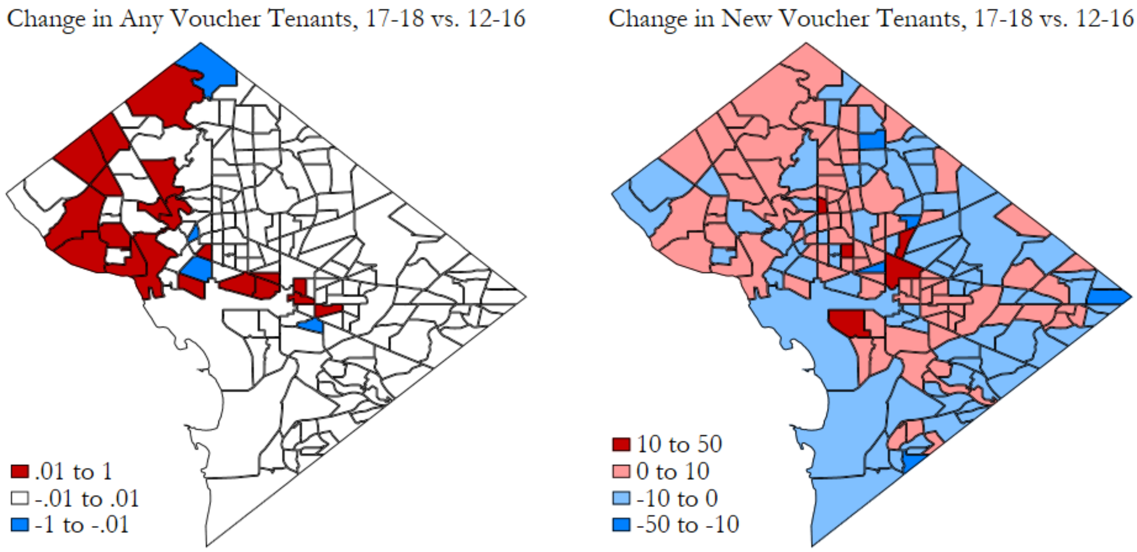
(a) Largest Counties in Metros with $\geq 1m$ Population



(b) Washington, DC

Figure 14: Neighborhood Opportunity and Median Rent

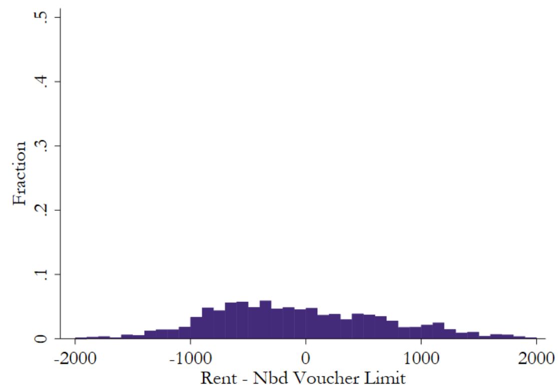
Note: The left panel shows a random sample of 1,000 tracts from the 54 MSAs (the largest county in each MSA) with populations of at least 1,000,000 in the 2012-2016 ACS. The right panel shows tracts in the city of Washington, DC, and the entire DC metro area. The opportunity index in both figures is the first principal component of the poverty rate, the unemployment rate, the employed to population ratio, the share with a HS diploma, the share with a BA, and the share of families with children under 18 that are single-headed from the ACS. Each of these variables is first put into percentiles of the national distribution (in terms of population living in census tracts with these characteristics). We denote opportunity as the tract's percentile in the distribution of the resulting index/principal component. Source: US Census Bureau.



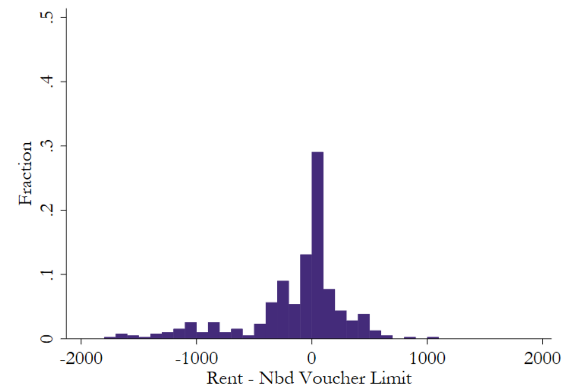
(a) Any Voucher Residents

(b) Number of New Arrivals

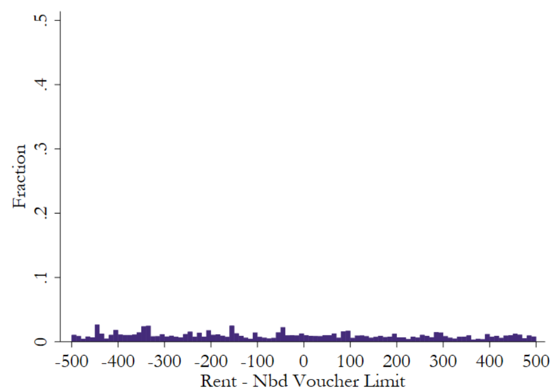
Figure 15: Change in Mean Voucher Outcome by Census Tract, 2017-2018 vs. 2012-2016
Notes: Each figure shows a map of Washington, DC. Both figures show data from the 2012-2018 HUD Picture of Subsidized Households. Figure (a) shows the difference between the fraction of years with any vouchers in the tract in 2017-2018 vs. 2012-2016. Figure (b) shows the difference between the number of new vouchers in each tract in 2017-2018 vs. 2012-2016. Source: US Department of Housing and Urban Development.



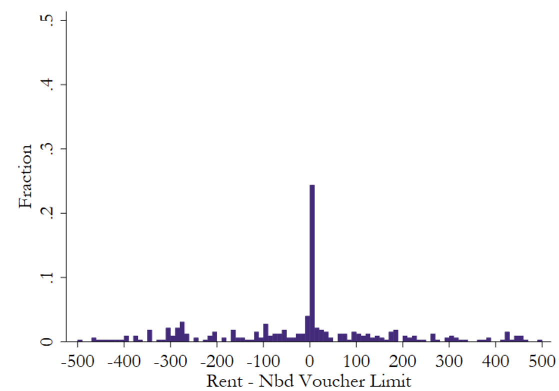
(a) Experiment



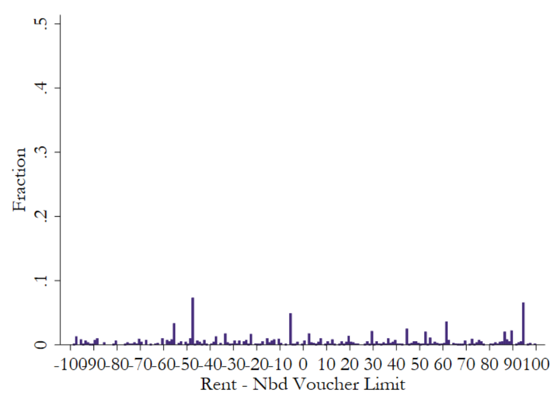
(b) DCHousingSearch



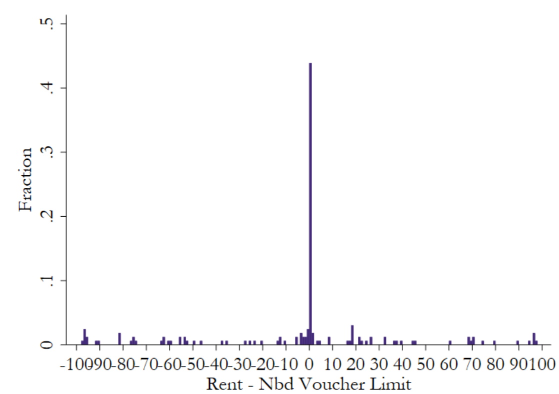
(c) Experiment



(d) DCHousingSearch

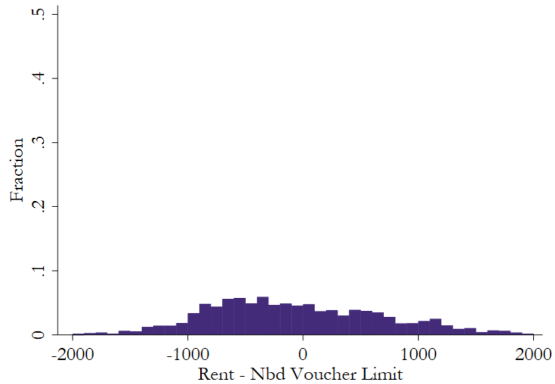


(e) Experiment

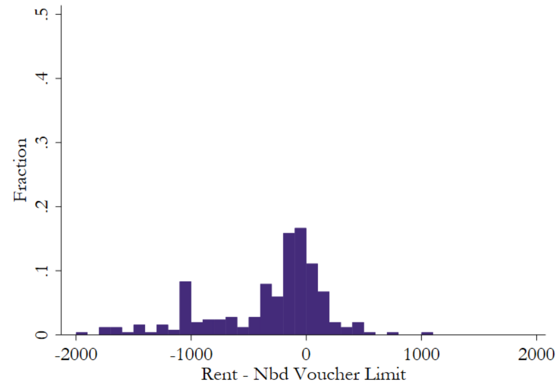


(f) DCHousingSearch

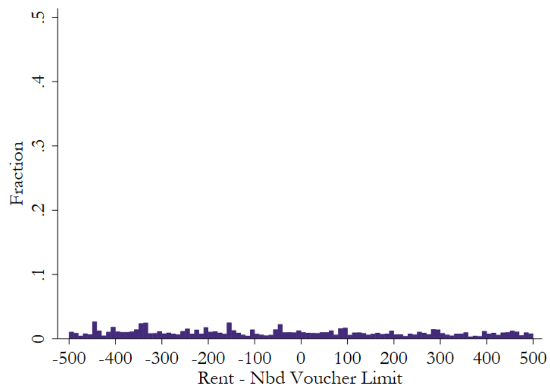
Figure 16: Frequency of Listings, by Posted Rent Relative to Neighborhood Rent Limit
Notes: These figures replicate Figure 10 with different bin widths. Source: Correspondence experiment, Social Serve.



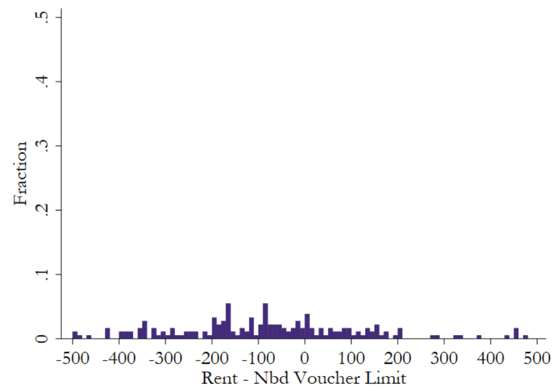
(a) Experiment



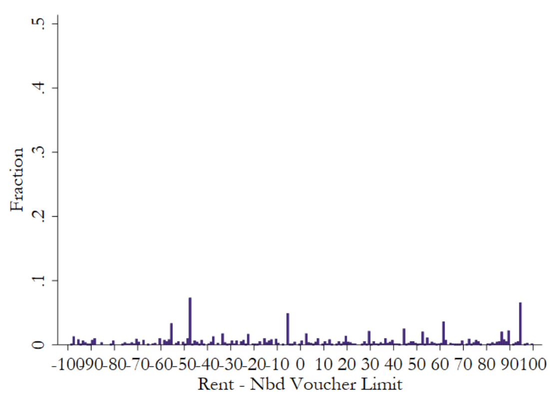
(b) DCHousingSearch - 2015



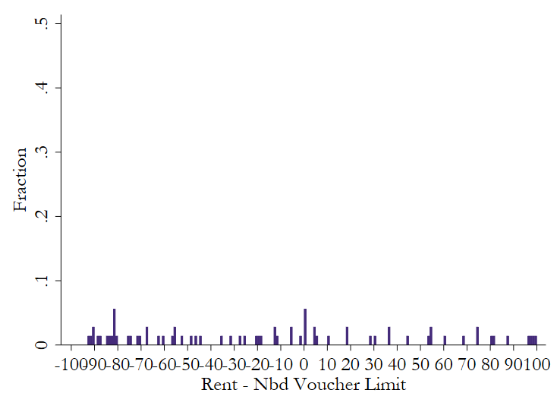
(c) Experiment



(d) DCHousingSearch - 2015



(e) Experiment



(f) DCHousingSearch - 2015

Figure 17: Frequency of Listings, by Posted Rent Relative to Neighborhood Rent Limit

Notes: These figures provide a placebo test. They are exact replications of Appendix Figure 16 except that the DCHousingSearch figures use listings from 2015 matched to 2017 voucher limits as a placebo test. Source: Correspondence experiment, Social Serve.

B Appendix: Tables

Table 10: Linear Probability Model of Landlord Response Varies on Inquiry Characteristics

	Positive Response	Positive Response	Positive Response	Positive Response
Voucher	-0.29***	-0.30***	-0.15***	0.027
	(0.012)	(0.020)	(0.032)	(0.10)
Black Name	-0.041***	-0.048***	-0.041***	-0.041***
	(0.012)	(0.016)	(0.012)	(0.012)
Voucher X Black		0.018		
		(0.023)		
Monthly Rent - 100s			0.0057***	0.011***
			(0.0016)	(0.0020)
Voucher X Rent			-0.0075***	-0.012***
			(0.0016)	(0.0022)
BedroomXVoucher Dummies	No	No	No	Yes
Year Dummy	Yes	Yes	Yes	Yes
Mean of Dep. Var.	0.37	0.37	0.37	0.37
R^2	0.090	0.090	0.093	0.099
N	6932	6932	6932	6932

The sample comes from the two correspondence experiments. Each column shows the results of a linear regression (linear probability model) with a landlord positive response dummy as the outcome. The voucher treatment is an indicator that the inquiry states a desire to pay by voucher and the black name variable is an indicator for an inquiry with a black-indicating name. Coefficients for all covariates are listed unless indicated. Standard errors are in parentheses and are clustered by rental listing. Source: Correspondence experiment, Social Serve.

Table 11: Effect of Increasing Rent Limits on Number of Vouchers Leased-Up per Tract, Only Border Tracts

	Vouchers	New Arrivals	arsinh(Vouchers)	Any Vouchers
Treatment Nbd X Post-2017	11.7**	4.44***	0.52***	0.16**
	(5.69)	(1.54)	(0.16)	(0.069)
Post-2017	-0.38	-1.15	-0.058	-0.048
	(2.49)	(0.75)	(0.066)	(0.034)
Treatment Nbd	-32.0**	-1.00	-0.83*	-0.026
	(14.7)	(1.14)	(0.49)	(0.079)
Mean of Dep. Var.	52.7	2.80	3.69	0.91
R^2	0.068	0.033	0.043	0.019
N	371	356	371	371

The sample comes from the HUD Picture of Subsidized Households. The unit of analysis is a tract-year. Each column shows the results of a linear regression with the outcome listed at the top of the column. The function 'arsinh' refers to inverse hyperbolic sine, which is similar to a log transformation but accounting for zeros. The treatment dummy indicates that the tax neighborhood of the unit had a voucher payment limit greater than 130 percent of metro-wide FMR in 2017. Coefficients for all covariates are listed. Standard errors are in parentheses and are clustered by tract. We restrict the sample to tracts that border a tract with a different value of the treatment indicator. Source: US Department of Housing and Urban Development.

C Appendix: Model

Consider the following model of landlord decision making. Suppose there is a fixed supply of housing units, and that landlord ℓ owns unit i in neighborhood j, and no other units. In the remainder of the analysis we will assume that we are focusing on one landlord ℓ with one unit of quality $q_i \in [0, 100]$ in a neighborhood of opportunity $opp_j \in [0, 100]$. We therefore suppress notation specifying ℓ, i, and j until we explicitly begin to investigate heterogeneity along these dimensions.

We model two choices made by the landlord. The first choice is the rent at which to advertise their unit. We frame this choice of the listed rent $r = r^m + s$ in terms of the slack s added to the competitive market rent of the unit, r^m, with the enforcement of rent reasonableness entering through the maximum slack $\overline{s}$ in the landlord's choice set. The second choice of the landlord is a discrete time optimal stopping problem of when to accept a tenant. Proceeding in terms of backward induction, we first assume that the landlord has set some rent level r and must decide whether or not to accept a tenant he has encountered.

Our model has two types of tenants, $\tau \in \{C, H\}$ (cash and HCV), and two types of landlords, $\ell \in \{C, H\}$ (cash and HCV specialists).[15]

C.1 The Landlord's Problem of Whether to Accept or Reject a Tenant

After their choice of posted rent, r, the landlord must make a second decision as to whether to accept renter k with observed characteristics X_k as the tenant of his unit. Upon accepting an applicant as his tenant, each month the landlord will expect to earn

$$r - \overline{m}(X_k), \qquad \text{where} \qquad \overline{m}(X_k) = \int_{\mathcal{M}} m \; dF(m|X_k)$$

where m arises from factors such as late rent, maintenance to the unit after damage (possibly caused by tenants), and the monetary, time, and energy costs of complying with government regulations. Note that the expected maintenance cost is a function of the landlord's beliefs about the distribution of maintenance costs conditional on a tenant's observed characteristics, F_ℓ .

In a simple search model where accepted matches continue in perpetuity, the landlord will follow a reservation maintenance cost strategy. That is, upon matching with renter k, the landlord will accept the renter as a tenant if the expected maintenance cost is less than a reservation value m^*, or if

$$r - \overline{m}(X_k) > r - m^*.$$

To see that the landlord follows a reservation maintenance cost decision rule in our model, consider the case of just cash renters. The landlord's value function when encountering renter k is

$$v(\overline{m}(X_k)) = \max\left\{\frac{r - \overline{m}(X_k)}{1-\beta}, \beta \int v(m')dF(m')\right\}, \tag{3}$$

[15] We refer to housing choice vouchers (HCV) and vouchers interchangeably.

where the maximization is over accepting the tenant or rejecting him and waiting to draw a new tenant with expected maintenance m' next period. The textbook results from the McCall model can be extended to this model.[16] This establishes that the landlord's decision rule is based on a reservation expected maintenance cost where the landlord accepts if $\overline{m}(X_k) < m^*$ and rejects if $\overline{m}(X_k) > m^*$, and that one can characterize the reservation maintenance cost using the equation

$$m^* = r - \beta \int_0^{m^*} \left(m^* - m'\right) dF(m') \quad \text{where} \tag{4}$$
$$f(m') = \int_{\mathcal{X}} f(m|x) f(x|r) dx.$$

In the case of heterogeneity of tenants along the dimension of either paying with cash or a voucher, $\tau \in \{C, H\}$, let $\pi^C(r)$ be the probability of encountering a cash tenant and $\pi^H(r)$ be the probability of encountering an HCV tenant, where we assume that both of these probabilities are functions of the rent already listed by the landlord and that the landlord can encounter at most one tenant per period. We will at times denote the total probability of encountering a tenant next period by $\pi(r) = \pi^C(r) + \pi^H(r)$. We generalize the landlord's value function to

$$v(\overline{m}(X_k)) = \max \left\{ \frac{r - \overline{m}(X_k)}{1 - \beta}, \beta \left[\pi^C(r) \int v(m') dF(m'|\tau = C) + \pi^H(r) \int v(m') dF(m'|\tau = H) \right] \right\}, \tag{5}$$

and characterize the reservation maintenance cost in Equation 4 as

$$m^* = r - \beta \left[\pi^C(r) \int_0^{m^*} \left(m^* - m'\right) dF(m'|\tau = C) \quad + \quad \pi^H(r) \int_0^{m^*} \left(m^* - m'\right) dF(m'|\tau = H) \right],$$

both subject to the constraint that $\pi^C(r) + \pi^H(r) \leq 1$.

C.2 The Landlord's Rent Listing Problem

Proceeding by backward induction (i.e., assuming the landlord's optimal decision rule in the second period), consider the landlord's problem of the rental price at which to list his unit. We assume that the rent listed by the landlord r is

$$r = r^m + s,$$

a combination of the market rent for his unit and some amount of slack s. We suppose there are two types of landlords: cash specialists and HCV specialists, which we will denote by $\ell \in \{C, H\}$. Landlord types differ in the responsiveness of their probability of encountering tenants as a function of the slack they choose, $\pi^C(s|\ell)$ and $\pi^H(s|\ell)$. As well, different types of landlords have different beliefs about the distribution of HCV tenants' maintenance costs, $F(m|\ell, \tau = H)$.

This landlord heterogeneity results in a generalized version of the value function specified in

[16] See Chapter 5 of Ljungqvist and Sargent (2000).

Equation 5. We now also account for the institutional rule that landlords cannot accept voucher tenants at a rent above the FMR that applies to their unit's local area. The resulting value function is:[17]

$$v(m|r^m, s, r^{FMR}, \ell, \tau) = \max \Bigg\{ \mathbb{1}\{\tau = C\} \times \frac{r^m + s - m}{1-\beta} + \mathbb{1}\{\tau = H\} \times \min \left\{ \frac{r^{FMR} - m}{1-\beta}, \left(\frac{r^m + s - m}{1-\beta} \right) \right\}, \tag{6}$$

$$\beta \Big[\pi^C(s|\ell) \int v(m'|r^m, s, r^{FMR}, \ell, \tau = C)\, dF(m'|\ell, \tau = C) \; +$$

$$\pi^H(s|\ell) \int v(m'|r^m, s, r^{FMR}, \ell, \tau = H)\, dF(m'|\ell, \tau = H) \Big] \Bigg\}.$$

The slack decision faced by a landlord is

$$\max_s \mathbb{E}[v(m|r^m, s, r^{FMR}, \ell)] = \max_s \;\; \pi^C(s|\ell) \int_{\mathcal{M}} v(m|s, \ell, \tau = C)\, dF(m|\ell, \tau = C) \; + \tag{7}$$

$$\pi^H(s|\ell) \int_{\mathcal{M}} v(m|r^m, s, r^{FMR}, \ell, \tau = H)\, dF(m|\ell, \tau = H).$$

C.3 Landlord Types

Landlord types differ along two dimensions: the responsiveness of their probability of encountering tenants as a function of the slack they choose, $\pi^C(s|\ell)$ and $\pi^H(s|\ell)$, as well as their beliefs about the distribution of HCV tenants' maintenance costs, $F(m|\ell, \tau = H)$. We formally specify these differences in terms of the following assumptions:

Assumption A1: Differences in $\pi(s|\ell)$

We consider two cases of $\pi(s|\ell)$. In both cases, charging greater slack drives away cash tenants. In Case 1, the housing authority strictly enforces rent reasonableness, and the landlord is less likely to encounter both cash and HCV tenants if he increases the slack in his rent listing. In Case 2, the landlord is less likely to encounter cash tenants after increasing slack, but HCV tenants tend to fill this void. In this context we could think about π^C as representing the probability of the event "the most attractive/lowest $\overline{m}$ tenant encountered by the landlord is a cash tenant" and π^H analogously. These cases are summarized in Table 12 and shown in Figure 21 for high, medium, and low levels of responsiveness to slack (i.e., elasticity of π with respect to slack).

[17] Under this specification, HCV tenants are able to rent up in a unit listed above the FMR at the FMR. We also investigated specifying the first line of Equation 6 as $\left(\frac{r^m+s-m}{1-\beta}\right) \times \left(1 - \mathbb{1}\{r^m + s > r^{FMR},\ \tau = H\}\right)$ to capture an HCV tenant being unable to lease up in a unit listed above the FMR. We found that the choice between these specifications had no qualitative implications for our simulation results.

Table 12: Cases by Tenant Responsiveness to Slack

Case	Overall Probability	Cash Tenants	HCV Tenants
Case 1	$d\pi(s)/ds < 0$	$d\pi^C(s)/ds < 0$	$d\pi^H(s)/ds < 0$
Case 2	$d\pi(s)/ds \approx 0$	$d\pi^C(s)/ds < 0$	$d\pi^H(s)/ds \lesseqgtr 0$

Note: In Case 1 cash and HCV tenants both respond negatively to slack. In Case 2 cash tenants respond negatively to slack, but HCV tenants replace the "missing" cash tenants. In this sense one can think of π^τ as the probability that the most attractive tenant encountered is of type τ.

Assumption A2: Differences in $F(m|\ell, \tau = H)$

The distributions of expected maintenance costs $F(m|\ell, \tau)$ used in our simulations are shown in Figure 19. The expected maintenance costs of cash tenants is the same for all landlord types. For HCV specialist landlords, the expected maintenance cost distribution for HCV tenants is assumed to be slightly higher than the distribution for cash tenants. For these landlords the relatively similar maintenance cost distributions for cash and HCV tenants could be driven by beliefs about actual costs, experience in screening HCV applicants, or an addition of warm glow utility to the actual costs faced by HCV tenants. This last interpretation seems quite plausible; Greenlee (2014) documents that one third of landlords renting to HCV tenants report doing so out of a desire to help individuals they consider less fortunate than themselves, and Rosen (2014) interviews landlords who renovate units to tailor them to renting to voucher tenants.

For cash landlords, the expected maintenance costs of cash and HCV tenants are quite different. These landlords may not be familiar with the HCV program and might consider the prospect of an initial inspection especially burdensome. Also plausible is that these landlords have previously participated in the HCV program and found the experience costly; Garboden et al. (2018) find that two thirds of landlords who refuse voucher tenants had once accepted them, and Zuberi (2019) finds that many landlords express frustration in their interactions with their local PHA.

C.4 Model Predictions

We consider three model predictions. The most important components of these predictions are shown in Figure 20, but details about the components of this figure are shown in Figures 21-23.

First, our model predicts that as the overall probability of encountering a tenant $\pi = \pi^C + \pi^H$ goes up, the reservation maintenance cost m^* goes down. The reason can be seen from looking at the hypothetical value functions in Figure 20a. The downward sloping line is the value from accepting a tenant with maintenance cost m, and the horizontal line is the continuation value of rejecting a tenant (of any maintenance cost m). Increasing the probability of encountering a tenant raises the horizontal line, which in turn decreases the m at which the downward sloping and horizontal lines intersect. Since this point of intersection occurs at m^*, raising π decreases m^*.

Second, our model makes predictions about rent listing when rent reasonableness is strictly enforced (implying that HCV tenants are also deterred by slack, resulting in Case 1). The green lines in Figure 20b shows objective functions $\mathbb{E}[v(m|r^m, s, r^{FMR}, \ell)]$ from Problem 7 for cash and HCV landlords when rent reasonableness is strictly enforced. We can see that the objective functions are both maximized by setting $s = 0$. Thus, the model predicts that with strict enforcement of rent reasonableness, cash and HCV landlords behave similarly in that they both list their unit at the market rent.

Third, our model makes predictions about rent listing decisions when rent reasonableness is weakly enforced (implying that HCV tenants need not be deterred by slack, resulting in Case 2). Under weak enforcement HCV landlords would also be less likely to encounter cash tenants after increasing the slack in their listed rent, but this decrease would be offset by HCV tenants able to fill this absence (Case 2 of A1). The red line in Figure 20b shows the resulting objective function $\mathbb{E}[v(m|r^m, s, r^{FMR}, \ell)]$ from Problem 7 for an HCV landlord. We can see that HCV landlords will maximize their objective function by setting s to list their unit at its FMR, or by choosing s so that $r = r^m + s = r^{FMR}$. By increasing slack, HCV landlords increase the income stream from a leased-up unit. While they face a tradeoff in that increasing slack chases away cash tenants, HCV tenants will fill this void. And since HCV landlords do not face such a different maintenance cost distribution for HCV tenants than cash tenants, this tradeoff will initially increase their objective function. Once slack hits the maximum allowed under rent reasonableness (or overall rent hits the unit's FMR), the HCV landlord will no longer benefit from increasing slack. Doing so will chase away cash tenants but will no longer increase the stream of income associated with a lease-up, and therefore will decrease the landlord's objective function.

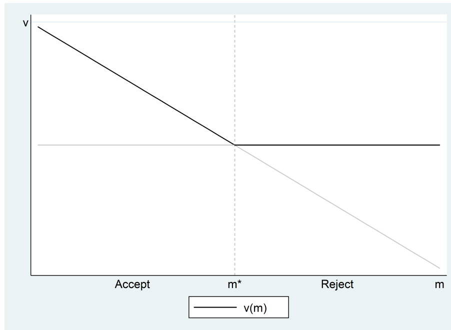


Figure 18: The Function $v(m)$

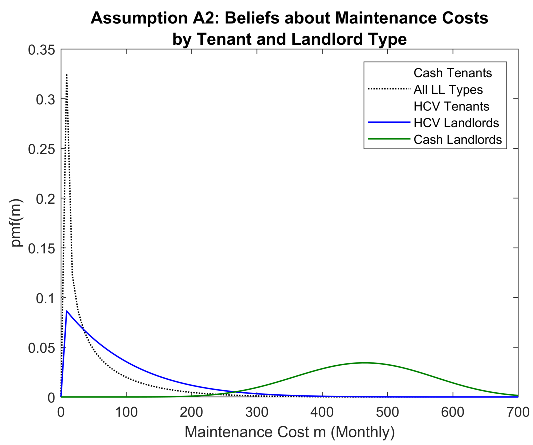
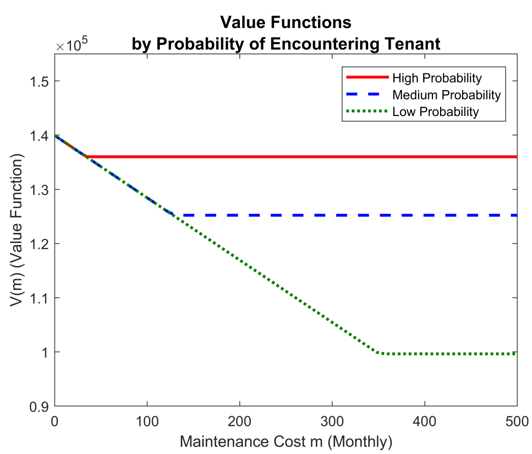
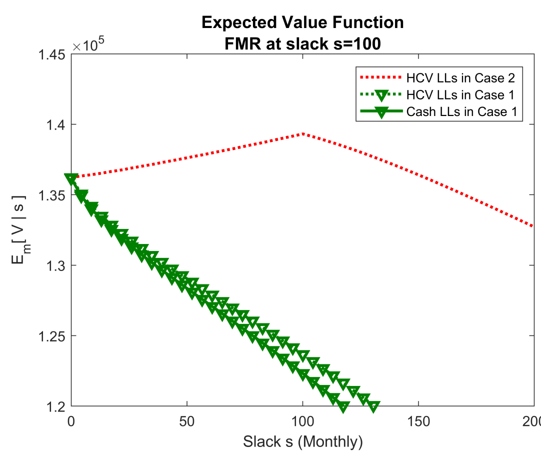


Figure 19: Assumption A2 by Landlord Type



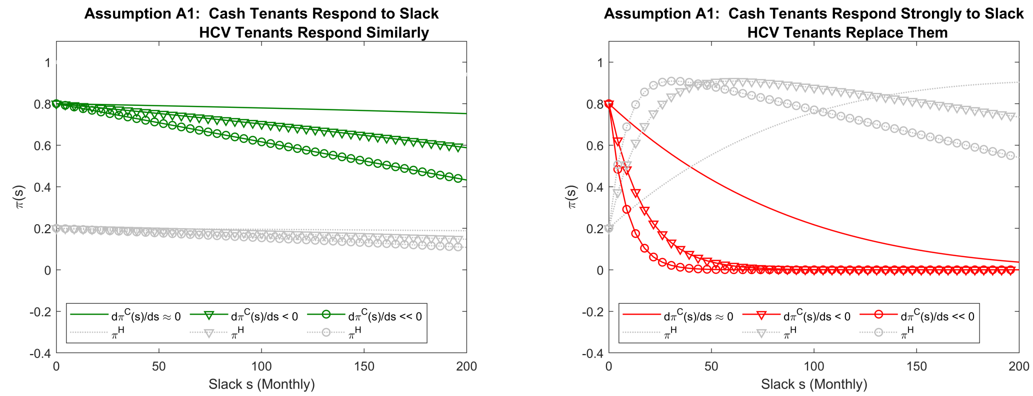
(a) Landlord's Accept/Reject Decision



(b) Landlord's Slack Decision

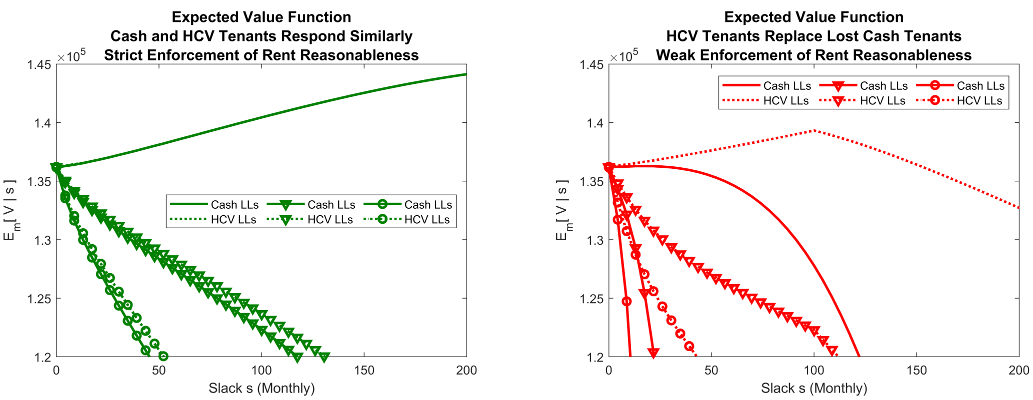
Figure 20: The Landlord's Decisions

Note: The left panel shows the value functions of a cash landlord in Case 2 of Assumption A1 with weak enforcement of rent reasonableness and slack of \$35. The right panel shows the expected value function as a function of slack s for landlords under Case 2, in which cash tenants respond strongly and negatively to increased slack, but HCV tenants are unresponsive to slack and therefore make up the difference. Source: Author calculations using model data.



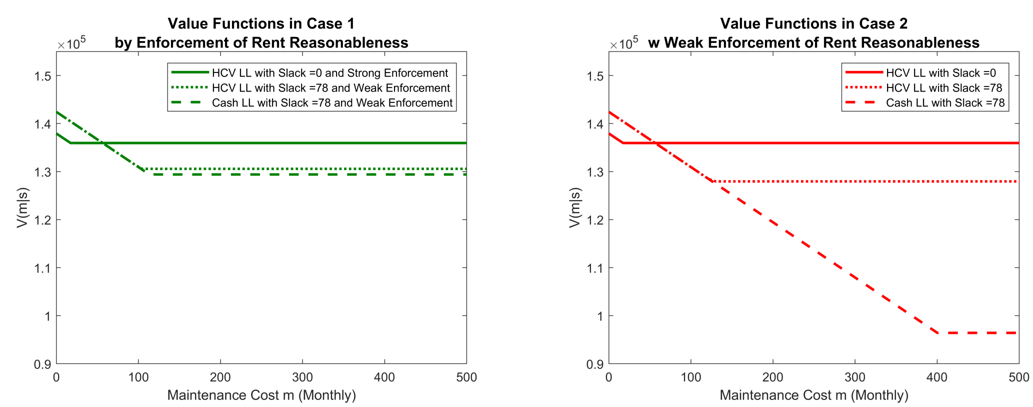
(a) Case 1 (b) Case 2

Figure 21: Two Cases of π when $\pi^C(0) = 0.80$ and $\pi^H(0) = 0.20$



(a) Case 1 w Strong Enforcement (b) Case 2 w Weak Enforcement

Figure 22: Expected Value Function in the Two Cases from Figure 21



(a) Case 1 by Enforcement (b) Case 2 w Weak Enforcement

Figure 23: Value Functions in the Two Cases from Figures 21 and 22