How Do Changes In Housing Voucher Design Affect Rent and Neighborhood Quality?

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June 2017

Abstract

U.S. housing voucher holders pay their landlord a fraction of household income and the government pays the rest, up to a rent ceiling. We study how two types of changes to the rent ceiling affect landlords and tenants. A policy that makes vouchers more generous across a metro area benefits landlords through increased rents, with minimal impact on neighborhood and unit quality. A second policy that indexes rent ceilings to neighborhood rents leads voucher holders to move in higher-quality neighborhoods with lower crime, poverty and unemployment.

Keywords: Incidence, Vouchers, Housing
JEL Codes: H22, H53, R21, R31

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1 Introduction

A central goal of U.S. low-income housing programs in recent years has been to improve neighborhood quality for assisted households. Recent evidence suggests this is a valuable goal, finding that neighborhood quality during childhood plays a role in determining labor market success as an adult (Chetty et al. 2016, Chetty and Hendren 2016, Chyn 2016). The Housing Choice Voucher program tries to achieve this aim by providing households with more choice over location (U.S. Department of Housing & Urban Development 2014). However, most housing voucher holders opt to live in neighborhoods of much lower quality than the average neighborhood, and typically live in neighborhoods similar to their neighborhood before receiving a voucher.[2] Various reforms to the generosity of vouchers have been proposed to address this problem, but little is known about whether these reforms achieve their goal of improving voucher holder neighborhood quality or are instead captured by landlords via higher rental prices.

We fill this void by evaluating two types of policy changes intended to spur moves to high-quality neighborhoods. The first increases the maximum per-unit government subsidy – which we refer to as the "rent ceiling" – uniformly in all neighborhoods in a metro area. The second increases the ceiling in higher-quality ZIP codes and lowers it in lower-quality ZIP codes. Each of these policy changes is depicted visually in Figure 1. We find that a policy of uniform increases in the ceiling raises the rents charged by voucher landlords to the government, with little impact on observed neighborhood quality. In contrast, a policy which establishes ZIP code-specific ceilings leads landlords to adjust rents, but also is a cost-effective way to increase neighborhood quality for voucher households.

Housing Choice Vouchers, also known as Section 8 vouchers, paid rent subsidies for 2.3 million low-income families in 2016. Voucher holders typically pay 30% of their income as rent and the government pays the rest up to a rent ceiling, which is usually set at the 40th percentile of metro area or countywide rents. Because a single uniform ceiling often applies to a broad geography, a much larger share of units are affordable with a voucher in low-quality neighborhoods. In 2013, Census rent data show that two-thirds of rental units were priced at or below the ceiling in low-quality neighborhoods, but only one-seventh of units were in high-quality neighborhoods, as shown in Figure 2.

In spite of the importance of high-quality neighborhoods for economic mobility, most voucher households occupy units in low-quality neighborhoods. For example, we document that voucher holders in Dallas live on average in neighborhoods that are one standard deviation below the mean in terms of a neighborhood quality index defined below. Other research has shown that housing vouchers do not lead households to move to substantially safer or less impoverished neighborhoods.[3]

[2] We use "low-quality" to refer to neighborhoods with low rents, high poverty, high crime and poor-performing schools.

[3] Two examples with random assignment of housing vouchers are a lottery in Chicago (Jacob et al. 2013) and

Who benefits from raising the rent ceiling uniformly is ambiguous.[4] It could benefit landlords, if they price discriminate by raising their rents to the new rent ceiling, or benefit voucher holders, if they use the more generous vouchers to move to better neighborhoods. Whether voucher holders move depends on the extent to which they value finding a unit in a high-quality neighborhood versus finding a unit at all. Ultimately, this is an empirical question, which we analyze using rich administrative and survey data.

In contrast to a uniform increase, tilting the rent ceiling so that it is higher in high-quality neighborhoods and lower in low-quality neighborhoods may be a cost-effective way to raise neighborhood quality.[5] Intuitively, the status quo penalizes searching in high-quality neighborhoods and the tilting policy raises optimal neighborhood quality by reducing this penalty. However, because the average rent ceiling does not increase, the scope for additional price discrimination is limited. In our empirical work, we investigate whether these predictions are supported.

We empirically estimate the impact of the two voucher policies described above: raising the rent ceiling uniformly and tilting the rent ceiling towards quality. To estimate the impact of uniform increases, we use two complementary research designs; the first precisely measures the policy's impact on neighborhood quality, while the second uses a dataset with rich measures of unit quality. The first research design uses sharp corrections to accumulated measurement error in the local rent ceiling and a national panel capturing the universe of voucher holders. We estimate that a $1 increase in the rent ceiling raises rents by 46 cents over the next six years, while a hedonic measure of unit and neighborhood quality rises by only 5 cents over the same time period. In addition, we estimate a precise zero for the impact on neighborhood quality as measured by Census tract median rent and tract poverty rate. These point estimates imply that the benefit of this policy to landlords is eight times as large as the benefit in terms of observed quality to tenants. Although this design has the advantage of generating statistically precise estimates of the impact on neighborhood quality in an event study framework, it uses unit quality measures that are quite limited.

The second research design for studying a uniform rent ceiling increase remedies the limited unit quality measures in the first by exploiting a unique survey of voucher recipients. This survey of over 300,000 voucher holders has excellent detail on unit quality, including 26 questions on time-varying unit quality. We use a difference-in-differences design to study how unit quality changes in 39 metro areas that saw an increase in rent ceilings. Here, we find that each $1 increase in the rent ceiling raised the rents paid on voucher units by 47 cents, with no significant impact

HUD's Welfare to Work Voucher Experiment (Eriksen and Ross 2013, Patterson et al. 2004). Two other studies which use matching methods are Carlson et al. (2012) and Susin (2002).

[4] In Appendix A, we theoretically analyze the impact of this policy using a model where voucher holders face a trade-off between finding a unit in a high-quality neighborhood and finding a unit at all and landlords can post higher rents in hopes of leasing to price-insensitive voucher holders. In such a model, whether landlords or tenants benefit more is ambiguous.

[5] The model in Appendix A predicts that tilting the rent ceiling is a cost-effective way to raise neighborhood quality.

on observed unit quality. These point estimates are very close to the point estimates from the first research design, although the estimates from the second research design are less precise. Two distinct research designs in two different time periods yield similar results: uniform increases in the rent ceiling appear to benefit landlords and not tenants.[6]

Finally, we study the effects of tilting the rent ceiling by examining a recent demonstration project in the Dallas, Texas metro area. Housing authorities in Dallas switched from a single metro-wide ceiling to ZIP-code-level ceilings in 2011. Much as with the uniform rent ceiling increase, we find empirically that landlords adjust rents – raising them in expensive ZIP codes and lowering them in low-cost ZIP codes. Because this policy makes vouchers more generous when they are used in high-quality neighborhoods, one might expect that it would improve neighborhood quality.

A difference-in-differences design using neighboring Fort Worth, Texas as a comparison group shows that new leases signed after the policy was implemented were in tracts where neighborhood quality is 0.23 standard deviations higher than leases signed prior to policy implementation. We construct a neighborhood quality index using the violent crime rate, test scores, the poverty rate, the unemployment rate and the share of children living with single mothers. Relative to other housing voucher policies, 0.23 standard deviations is a substantial improvement in neighborhood quality. It is about half the magnitude of the improvements in neighborhood quality for people currently living in public housing who are allocated vouchers (Kling et al. 2005) and larger than the improvement in neighborhood quality from allocating a voucher to previously-unsubsidized tenants (Jacob and Ludwig 2012).

The Dallas tilting policy is budget-neutral within the time period we study. Absent any tenant behavioral response, this policy would have been cost-saving for the government, because voucher holders tend to live in inexpensive neighborhoods and therefore rent increases in expensive ZIP codes were offset by larger decreases in low-cost ZIP codes. Incorporating tenants' improved neighborhood choices, the Dallas intervention had zero net cost to the government over the years that we study. Thus, our results show that a simple budget-neutral reform to housing voucher design has the potential to substantially improve voucher holder neighborhood quality.

The remainder of the paper is organized as follows. Section 2 reviews the voucher program and Section 3 describes the data. In Section 4, we show that a uniform increase in rent ceilings fails to raise neighborhood quality, but benefits landlords through increased voucher rents. In Section 5, we show that tilting rent ceilings is successful at inducing moves to higher-quality neighborhoods. Section 6 concludes.

[6]These research designs estimate who benefits from marginal changes to the rent ceiling. See Desmond and Perkins (2016) for estimates of differences in average rents between similar voucher and nonvoucher units in Milwaukee.

2 Housing Voucher Program

Housing Choice Vouchers use the private market to provide rental units for 2.3 million low-income households. There are four key actors in the voucher program: the U.S. Department of Housing and Urban Development (HUD), local housing authorities, private landlords and tenants. HUD funds local housing authorities who administer the voucher program, which includes making payments on behalf of tenants to landlords. Tenants search for units to lease on the private market.

The tenant pays at least 30% of her income in rent and the housing authority pays the difference, up to a rent ceiling. The local housing authority chooses a Payment Standard (which we refer to as the "rent ceiling") from 90%-110% of a federally-set "Fair Market Rent" (FMR) (U.S. Department of Housing and Urban Development 2001). HUD typically sets FMRs at the 40th percentile of area–by-bedroom level gross rent (rent to landlord plus utility costs). An FMR area is by default defined using county boundaries, but in urban areas there is often a single FMR for all counties in a metro area. We defer a discussion of how FMRs are updated until Section 4 where we describe the natural experiments which we exploit.

Voucher holders renting units below the rent ceiling pay nothing when rents rise, but remain below the ceiling, with the housing authority paying each extra dollar of an increase. This is important because when the rent ceiling increases landlords can increase rents without worrying that this will cause the voucher holder to move. Two institutional details limit the extent of rent increases when the rent ceiling rises. First, a small share of voucher holders lease units with rents above the rent ceiling, and they bear each dollar of a rent increase. Second, at initial lease signing, as well as with requests for rent increases, housing authority staff must certify that rent requests meet "rent reasonableness" standards.[7]

3 Data

The primary dataset we use in this paper is a HUD internal administrative database called PIC that covers the universe of voucher holders. It contains an anonymous household identifier, an anonymous address identifier, building covariates, the rent ceiling, the FMR, and the contract rent received by landlord, on an annual basis beginning in 2002. The data have two strengths that we exploit in our analysis. First, we can follow a household if they move in response to a policy change. Second, the address identifier, coded as a 9-digit ZIP code, enables us to follow a single address over time if it has multiple voucher occupants, which is useful for estimating the impact of an increase

[7]The typical rent reasonableness process entails local housing authority staff drawing a set of rent comparables for the unit in question from rental listing services. The housing authority staff will negotiate with a landlord requesting a rent substantially above the comparables, and may request evidence of other existing leases to establish that the requested tent is in line with market rents. The median housing authority rejects between one-quarter and one-half of units on the first inspection (Abt Associates (2001), Exhibit 3-5). One piece of evidence that the rent reasonableness process is effective is that empirically that rents are lower for units with lower hedonic unit and neighborhood quality (Appendix Figure B.1).

in the rent ceiling while holding constant many aspects of unit quality. Table 1 provides summary statistics and Appendix B.1 discusses sample construction.

We supplement PIC with four other datasets. To investigate the effects of rent ceiling changes on non-voucher rents, we draw on rent data from the American Community Survey (ACS). To measure housing quality, we compute hedonic quality measures using coefficients from hedonic regressions in the ACS (Section 4.1) and American Housing Survey (Section 4.2). Our analysis in Section 4.2 uses the predecessor to PIC, the Multifamily Tenant Characteristics System (MTCS), which contains information on voucher rents, location of voucher tenants, household size and bedroom count. It also uses the HUD Customer Satisfaction Survey (CSS) which includes detailed questions about housing unit quality ideally suited to measure within-unit quality changes. To evaluate the effects of tilting the rent ceiling in Section 5, we assemble detailed data on neighborhood quality: school-level test scores data from the Department of Education, geocoded address level crime data from the Dallas Police Department, and tract-level measures from the American Community Survey 2006/2010.

4 Impact of Raising the Rent Ceiling Uniformly

We estimate the causal effect of uniform rent ceiling changes on housing quality (unit and neighborhood) and voucher rents using two natural experiments. In Section 4.1, we study a 2005 change in FMRs due to availability of updated 2000 Decennial Census data. The primary advantage of this research design is that it uses exogenous variation across all U.S. counties, giving us enough statistical power to detect even small neighborhood quality responses. A secondary advantage is that by using unit fixed effects, we are able to examine the price response while holding physical structure quality and neighborhood quality constant. However, this design lacks detailed measures of within-unit quality changes arising from better management, maintenance, or unit upgrades.

In Section 4.2 we investigate these types of quality improvements. We make use of a detailed HUD survey which asked 26 questions about time-varying unit quality and was administered to voucher holders on a widespread basis from 2000 to 2003 to construct measures of housing quality. Here, we exploit a 2001 change which raised FMRs from the 40th percentile to the 50th percentile of rents in 39 metro areas. Across both research designs, we find similar results: raising the rent ceiling results in higher rents with little evidence of positive unit or neighborhood quality impacts. We discuss at the end of the section why we believe that price discrimination by landlords is the most reasonable interpretation of these empirical results.

4.1 Rebenchmarking of FMRs in 2005

For many years, data constraints meant that FMRs changed little in a typical year, punctuated by very large swings once every ten years. This offers useful variation for a quasi-experimental analysis.

In most years, FMRs are updated using local Consumer Price Index (CPI) rental measures for 26 large metro areas and 10 regional Random Digit Dialing (RDD) surveys for the rest of the country. The availability of new decennial Census data results in a "rebenchmarking." Because the local CPI and RDD estimates are so noisy, large swings in FMRs occurred from 1994 to 1996, when 1990 Census data were incorporated into FMRs, and again in 2005, when 2000 Census data were added in 2005. In non-rebenchmarking years, FMR changes are very crude estimates of the actual change in local rent; for example, they were a bit *worse* at predicting local rent changes than using a single national trend from 1997 to 2004.[8]

The 2005 rebenchmarking offers substantial variation in FMR changes, suitable for a quasi-experimental research design. As an example, in Map 1, we show FMR revisions for two-bedroom units in Eastern New England for 2003-2004 and for 2004-2005. From 2003 to 2004, FMRs rose by 5.5% in Eastern Massachusetts and rose by 1.6% in outlying areas. The next year shows large revisions, with Rhode Island experiencing 22% increases in 2-bedroom FMRs and Greater Boston experiencing 11% decreases. Map 2 shows national impacts of the rebenchmarking. Figure 3 shows an event study of FMRs for four groups of county-bed pairs, stratified by the size of their revision from 2004 to 2005. In nominal terms, the bottom quartile fell by 7%, while the top quartile rose by 24%. These four groups had similar trends in the six years after the revision, so we can study the rebenchmarking as a one-time, permanent change. Throughout the paper, all regression specifications studying rent or hedonic quality use a log transformation. The motivation for this log transformation is that there is tremendous heterogeneity in FMR levels; in 2004, FMR levels for a 2-bedroom unit ranged from \$370 in rural Alabama to \$1800 in San Jose. Clearly, a \$50 increase in the FMR would have a very different impact in percent terms in Alabama than in San Jose. Additional empirical details on our use of the rebenchmarking are provided in Appendix B.2.

To clarify the sources of variation that we use for identification, we show that the rebenchmarking can be decomposed into three pieces: changes in nonvoucher rents, measurement error from annual updates, and measurement error in the Census. Define σ_t as an annual estimate of the change in log rents based on a regional RDD or CPI survey from year $t-1$ to t.[9] Define $exp(r_t + \varphi_t)$ as an observation from decennial Census data, where $exp(r_t)$ is the true rent and $exp(\varphi_t)$ is Census measurement error. We can use these definitions to write $\log FMR^{2004} = \sum_{t=1991}^{2004} \sigma_t + r_{1990} + \varphi_{1990}$, and $\log FMR^{2005} = \sum_{t=2001}^{2005} \sigma_t + r_{2000} + \varphi_{2000}$. Taking the difference gives

$$\Delta FMR = \underbrace{r_{2000} - r_{1990}}_{\text{true rent change}} + \underbrace{\sigma_{2005} - \sum_{t=1990}^{1999} \sigma_t}_{\text{annual meas error}} + \underbrace{(\varphi_{2000} - \varphi_{1990})}_{\text{Census meas error}}$$

[8]The top of panel of Appendix Figure B.2 shows that the variance of FMR changes is much larger in rebenchmarking years. The bottom panel shows that using a single national trend instead of actual FMR changes would have resulted in smaller swings in rent in the 2005 rebenchmarking.

[9]The RDD and CPI surveys are used to produce adjustment factors which modify the base, not to provide a new estimate of the level.

Consistent with measurement error as a source of variation, places where FMRs drifted upward due to noise over the prior ten years were subject to *downward* revisions in 2005, and places where FMRs drifted downward due to noise were subject to *upward* revisions.

Suppose that outcomes y such as unit and neighborhood quality or voucher rents may be affected by the rent ceiling $\bar{r}$ as well as contemporaneous shocks to supply and demand η, as expressed by the empirical model $\Delta y = h(\bar{r}) - h(\bar{r}_{2004}) + \eta$. Our identifying assumption is the shocks *after* 2004 were orthogonal to the level of FMRs in 2005, conditional on their 2004 level.

Identification Assumption in Rebenchmarking Research Design

$$\eta \perp FMR_{2005} | FMR_{2004}$$

As detailed above, ΔFMR consists of measurement error, which is by construction orthogonal to future trends, and the true nonvoucher rent change, $r_{2000} - r_{1990}$. Note that this research design allows the rebenchmarking to bring rental rents closer in line with the *level* of market fundamentals. We require only that the *change* in FMR be uncorrelated with the subsequent shocks η. Available empirical evidence supports this identification assumption. First, rents are about flat from 2002 to 2004, prior to the policy change. Second, contemporaneous changes in nonvoucher rents have no significant correlation with the FMR change.[10]

We estimate an empirical specification using two stage least squares, because local housing authorities have some discretion in setting rent ceilings, as discussed in Section 3. Formally, we estimate a first stage:

$$\text{First Stage } \bar{r}_j = \alpha + \gamma FMR_{2005j} + \omega FMR_{2004j} + \kappa \bar{r}_{2004j} + \varepsilon_j \tag{1}$$

where we predict the rent ceiling for county-bed j with the FMR for j in 2004 (FMR_{2004j}), the rent ceiling $\bar{r}$ for j in 2004, and exogenous variation from the 2005 FMR for j (FMR_{2005j}) with error term ε.[11] In the short term, housing authorities use their discretion in setting rent ceilings to offset the immediate impact of FMR changes, but a \$1 increase in the FMR from 2004 to 2005 corresponded to a 58 cent increase in the rent ceiling by 2010, as estimated by coefficient $\hat{\gamma}$. We estimate our second stage where j indexes county-bed pairs, $\widehat{\bar{r}_j}$ is the fitted value from the first

[10] Appendix B.3 analyzes prior and contemporaneous changes in nonvoucher rents in more detail and Appendix Table 1 shows the relevant regression results.

[11] The motivation for controlling for 2004 FMR is driven by the nature of our quasi-experimental variation. Prior to the FMR change, average rents across all units were *rising* for places about to receive a downward revision and that rents were *falling* for places about to be revised upward; this was likely because of mean reversion in regional rents combined with infrequent FMR resets. Controlling for the 2004 FMR level eliminates this pretrend. We also try the following first-differences specification. We estimate a first stage: $\Delta\bar{r}_j = \alpha + \gamma\Delta FMR_j + \varepsilon_j$, where $\Delta\bar{r}_j = \bar{r}_j - \bar{r}_{2004j}$ and second stage: $\Delta y_j = \alpha + \beta\widehat{\Delta\bar{r}_j} + \eta_j$. This specification produces very similar point estimates.

stage equation and the coefficient of interest is β the effect of rent ceiling changes on the outcome Δy_j:

$$\text{Second Stage } \Delta y_j = \alpha + \beta\widehat{\bar{r}_j} + \lambda FMR_{2004j} + \pi\bar{r}_{2004j} + \eta_j \tag{2}$$

We assess the effects of uniform rent ceiling changes on neighborhood quality as measured by median tract rent, neighborhood quality as measured by tract poverty rate, rents received by landlords, and a "composite" hedonic measure of unit and neighborhood quality, and rents.[12] Tract-level measures are a good way to detect even small improvements in neighborhood quality because Census tracts typically have 4,000 residents and 77% of voucher moves cross tract boundaries.[13] We construct our voucher rent measure as $\Delta y_{t,j} = r^{voucher}_{t,j} - r^{voucher}_{2004,j}$.

To construct our composite hedonic quality measure, we run a hedonic regression in the American Community Survey using covariates for the number of bedrooms, structure age, structure type (e.g. single-family, multi-family, or apartment building) and neighborhood rent.[14] We then construct our dependent variable quality measure $\Delta y_j = \hat{\beta}_{hedonic}(x_{t,j} - x_{2004,j})$ using covariates $x_{t,j}$ on the number of bedrooms, structure type, structure age and median tract rent from the voucher data where $x_{t,j}$ is the unconditional average of x in county-bed j in year t.[15]

The impact of raising the ceiling on observable quality is very small. Table 2 columns (1)-(3) show the effects of a \$1 change in the rent ceiling on neighborhood and unit quality. A \$1 increase in the ceiling has no economically significant impact on the neighborhood quality of voucher tenants, as measured by neighborhood rents (column 1) or poverty rates (column 3), and raises composite hedonic quality by a mere 5 cents (column 2).

In contrast to the quality results, average rents by 46 cents in response to a \$1 increase in the rent ceiling (Table 2, column 4). Figure 4 plots the year-by-year coefficients of the reduced

[12]We use the term "composite" hedonic quality when the measure incorporates characteristics of both the unit (such as building age and type) and neighborhood (such as tract median rent).

[13]The tract rent measure is $\Delta y_{t,j} = log(tract\,rent_{t,j}) - log(tract\,rent_{2004,j})$, the difference in average median tract rent for vouchers in county-bed j from year 2004 to year t. The census tract poverty rate is $\Delta y_{t,j} = tract\,pov_{t,j} - tract\,pov_{2004,j}$ where $tract\,pov_{t,j}$is the average tract poverty rate of voucher holders in county-bed j.

[14]Voucher holders are assigned an appropriate number of bedrooms according to a fixed schedule based on household size. We use this assigned bedroom count to construct our instrument values and in our county-bed definitions. A voucher holder can choose to lease a larger unit –for example, a family eligible for two bedrooms can lease a three bedroom unit – but the payment will be according to their eligible number of bedrooms. To capture moves to larger units, we include the actual number of bedrooms in the leased apartment as a quality measure.

[15]We estimate our hedonic coefficients in the American Community Survey, where the smallest geographic units are Public Use Microdata Areas (PUMAs) with about 150,000 residents. The results from our hedonic regression in the ACS appear in Appendix Table 2. When predicting composite hedonic quality for voucher units, we measure neighborhood quality using median tract rent. Substituting median tract rent for a PUMA fixed effect offers a much more granular neighborhood quality measure and likely has little impact on the other hedonic coefficients. To assess the potential change in hedonic coefficients from using median tract rent instead of a PUMA fixed effect, we re-run our hedonic regression using the median PUMA rent in lieu of the PUMA fixed effect. We find that the hedonic coefficients are largely unchanged, the coefficient on PUMA median rent is approximately \$1 and the constant shrinks from \$900 to \$50 dollars. More details on construction of the hedonic measure are provided in Appendix B.4.

form impact of the FMR change on rents, and shows rents rise steadily in response to the rent ceiling increase through the first four years after the re-benchmarking, while composite hedonic quality rises minimally throughout this period. These results imply that only (0.05/0.46=) 11% of the increased government expenditure went to improvements in observable unit or neighborhood quality.

We conduct three robustness checks of our finding that landlords adjust rents in response to rent ceiling changes.[16] First, we address the concern that places revised upward might have different rent fundamentals than places revised downward. To do this we add county fixed effects to equations 1 and 2 so that identification comes only from within-county variation comparing the FMR change by bedroom count. Our point estimates from the model with county fixed effects of 50 cents are remarkably similar to our baseline estimate of 46 cents.

Second, we show that it is the government and not voucher holders who pay more when the rent ceiling rises. Recall from Section 2 that some voucher holders choose to rent a unit that costs more than the rent ceiling and then pay more than 30% of their income. In this case, when the landlord raises rents, it is the voucher holder and not the government that pays an additional dollar, potentially undermining the interpretation that landlords are price discriminating on the basis of voucher receipt. We address this concern by building a sample of tenants that are unlikely to be the residual payer.[17] For this sub-sample, we can be confident that when rent rises by $1 that the government pays $1 additional dollar. A $1 increase in the rent ceiling raises rents by a similar amount to our baseline specification.

Finally, to address concerns about whether rent increases may reflect quality improvements not captured by our hedonic measure, we estimate a model with address fixed effects. The sample consists of 800,000 units continuously occupied by a voucher tenant (either a new voucher tenant or an existing tenant). Here, we find rent increases of 15 cents for each dollar increase in the rent ceiling. The address fixed effects specification indicates that rents increase when the rent ceiling rises, even after holding constant neighborhood quality and permanent unit attributes. There are two potential reasons why the address fixed effects estimate (15 cents) is smaller than the full sample estimate (46 cents). One explanation is that the government is more easily able to enforce the "rent reasonableness" restrictions discussed in Section 2 when the same unit was previously leased to a voucher recipient and so the government has an easily-available benchmark for what the unit's rent should be. A second explanation, which we investigate in detail in the next section and do not find any evidence for, is that increased rents pay for improvements in time-varying unit quality.

[16] The regression results are shown in Appendix Table 3.

[17] To identify households that are unlikely to be the residual payer we examine two variables: the gap between gross rents and the rent ceiling, and the number of bedrooms in 2004. We use voucher holders with two or fewer bedrooms and a value of rent minus rent ceiling in the bottom three quintiles in 2004. The probability that these households have rent higher than the rent ceiling – and therefore pay more when the landlord raises the rent – is 11%.

4.2 40th → 50th Percentile FMRs in 2001

A concern with the first research design is an inability to measure detailed elements of quality which might vary over time within the unit. In a different dataset, HUD measured unit quality in much more detail from 2000 to 2003. Using this dataset requires a different identification strategy based on a policy change in 2001, when HUD switched from setting FMRs at the 40th percentile of the local nonvoucher rent distribution to the 50th percentile in 39 metro areas. This policy was implemented not in response to recent housing market conditions, but rather with the explicit goal of "deconcentration" of vouchers from the lowest-quality neighborhoods.[18]

From 2000 to 2003, HUD conducted a Customer Satisfaction Survey (CSS) of about 100,000 voucher households each year. This survey included numerous questions on unit quality and came close to matching the level of detail in the American Housing Survey (AHS), which is the state-of-the-art data source on housing quality in the U.S. In particular, it asked many questions about unit attributes which could plausibly vary at the same address over time including: "How would you rate your satisfaction with your unit?", "Has your heat broken down for more than 6 hours?", "Does your unit have mildew, mold, or water damage?" and "Have you spotted cockroaches in your home in the last week?" A full list of quality measures is in Appendix B.4. We transform these questions into a hedonic unit quality measure and a composite (unit and neighborhood) hedonic quality measure which includes tract median rents from the 2000 Census. Our analysis pools these county-year observations from 1999-2003. To compute hedonic quality, we identified the 26 questions on time-varying quality in the CSS which also appeared in the AHS.[19] We run a hedonic regression in the AHS using these 26 questions, number of bedrooms, building age, and building type and a measure of median neighborhood rent, and then use tenants' responses in the CSS to predict composite hedonic quality. We also assess the effects of the intervention on voucher rents using administrative records from PIC and its predecessor, the Multifamily Tenant Characteristics System.[20] To construct our rents measure we calculate the average by county-year for all tenants.

We estimate the impacts of the 40th to 50th percentile policy change on Fair Market Rents, actual voucher rents and composite quality. In order to assess the impact of the rent ceiling increase,

[18]The 39 metro areas were chosen on the basis of three factors, which are not obviously related to the *trend* in voucher rents or neighborhood quality:

- a size requirement (must contain at least 100 census tracts)
- an FMR neighborhood access measure – 70 percent or fewer census tracts with at least 10 two bedroom rental units are census tracts in which at least 30 percent of the two bedroom rental units have gross rents at or below the two bedroom FMR
- a high concentration of voucher holders in a limited number of census tracts – 25 percent or more of tenant-based voucher holders reside in 5% of tracts with FMR area with largest number of participants

[19]Appendix Table 4 compares the predictive performance of our hedonic characteristics across data sets. In the AHS, the CSS variables perform nearly as well as the "kitchen sink" AHS model (R-squared 0.31 for CSS variables compared to 0.42 for the full AHS model).

[20]We use the administrative data on rents because they covers the universe of voucher tenants. The CSS contains rents for survey respondents but the values are top-coded at $500 and reported in bands of $100.

we implement a difference-in-differences model using an instrumental variable specification.[21] Our estimates of the policy's effects on housing quality use individual-level survey data from the CSS, and the effects on rents use administrative data aggregated to the county-level. In our first stage in equation 3, we predict the endogenous rent ceiling for household i in FMR area j and time t ($\bar{r}_{ijt}$) using an indicator for being in an FMR area subject to the 50th percentile FMR policy ($\mathbf{1}(FMR = 50_j)$), an indicator for whether time period t is after the policy change ($\mathbf{1}(Post_t)$) and the excluded instrument: an indicator for the whether the observation is in FMR Area subject to 50th percentile FMR after the policy change ($\mathbf{1}(FMR = 50_j \times Post_t)$).[22] Our second stage question, is represented by equation 4, where $\widehat{\bar{r}_{ijt}}$ is the fitted value from the first-stage (the predicted payment standard) and β is the parameter of interest, the effect of a policy-induced change in the rent ceiling on the outcome.

$$\text{First Stage: } \bar{r}_{ijt} = \pi + \gamma\mathbf{1}(FMR = 50_j \times Post_t) + \mathbf{1}(FMR = 50_j) + \mathbf{1}(Post_t) + \varepsilon_{ijt} \quad (3)$$

$$\text{Second Stage: } y_{ijt} = \alpha + \beta\widehat{\bar{r}_{ijt}} + \mathbf{1}(FMR = 50_j) + \mathbf{1}(Post_t) + \eta_{ijt} \quad (4)$$

Our identification condition is the standard difference-in-differences condition: $E(\eta_{ijt}|\mathbf{1}(FMR = 50 \times Post)) = 0$. Figure 5 shows the results visually and Table 3 shows regression results. Setting FMRs at the 50th percentile of the local nonvoucher rent distribution raised rent ceilings by an average of 11 percent. For every \$1 increase in FMRs, rents rose by 47 cents (column 5), which is very similar to our estimate of 46 cents when using the rebenchmarking research design. In comparison, composite hedonic quality rose by less than 5 cents (Table 3, column 3), with a standard error of 9 cents. Although the estimate for the impact on quality is less precise than in the rebenchmarking research design, the results from this analysis reinforce the conclusions from the prior section that uniform rent ceiling increases in FMRs do not seem to improve quality.

Our empirical results from two separate natural experiments show that uniform changes in the ceiling does little to improve either neighborhood or observed unit quality for voucher tenants while rents increase substantially. We interpret our findings as likely reflecting landlords price discriminating by raising rents in response to rent ceiling changes. Our empirical findings are also consistent with landlords improving unmeasured aspects of unit quality and raising rents to cover the cost of these improvements. However, we view unmeasured quality improvements as unlikely to fully explain the estimated rent increases because we have very detailed measures of unit quality, and if a landlord decides to make unit improvements at least some of them would show up in the

[21] A difference-in-difference specification estimating the average effect of the policy δ using the following equation $y_{ijt} = \alpha + \delta\mathbf{1}(FMR = 50_j \times Post_t) + \mathbf{1}(FMR = 50)_j + \mathbf{1}(Post_t) + \eta_{ijt}$, appears in Appendix Table 5.

[22] In the case where the outcome is the voucher rent our regressions are at the county-year level: $\bar{r}_{jt} = \pi + \gamma\mathbf{1}(FMR = 50_j \times Post_t) + \mathbf{1}(FMR = 50_j) + \mathbf{1}(Post_t) + \varepsilon_{jt}$ where j now index counties. Again, $\mathbf{1}(\cdot)$ denotes the indicator function, taking the value equal to 1 if the statement is true and zero otherwise.

observable dimensions of unit quality.

5 Tilting the Rent Ceiling with ZIP-Level FMRs in Dallas

In contrast to the results in the previous section, we find that tilting the rent ceiling has a big impact on neighborhood quality. Following a court settlement, HUD replaced a single metro-wide FMR in Dallas with ZIP code-level FMRs in early 2011. The new ZIP code-level FMRs were set by multiplying the metro-wide FMR in Dallas by the ratio of the median gross rent of rental units in the ZIP code to median gross rent of units in the metro area. The demonstration caused sharp changes in local rent ceilings, ranging from a decrease of 20% to an increase of 30%, as shown in the top panel of Figure 6.

In Section 5.1, we validate that landlords in Dallas behave similarly to landlords nationally in response to uniform increases: voucher rents rose in ZIP codes where FMRs rose and fell in ZIP codes where FMRs fell. In Section 5.2, we build a neighborhood quality index and show that households who moved located in neighborhoods 0.23 standard deviation higher in quality. Finally, in Section 5.3, we compare the effects on neighborhood quality to the results from more costly alternative interventions. Appendix B.5 contains supplementary empirical details.

5.1 Impacts on Voucher Rents and Building Quality

We document that the ZIP-level elasticity of rents and building quality in response to changes in the rent ceiling in Dallas is similar to the responses to uniform rent ceiling increases. The rent results provide validation that landlords in Dallas respond similarly to landlords nationally when the rent ceiling changes. The identifying assumption for this analysis is that the relationship between the ZIP FMR and our outcomes (housing quality and voucher rents) would be unchanged from the base year (2010) to the most recent data available (2013), but for the policy change:

Identification Assumption in ZIP Code-Level Research Design

$$\eta \perp FMR \times Post | FMR$$

Because FMR in 2010 was constant across Dallas, using the 2011 FMR level as the regressor is the same as using the change from 2010 to 2011 as the regressor. Our sample consists of voucher holders in 2010 and 2013. In our first stage we predict the payment standard for voucher holder i in ZIP code j at time t ($\bar{r}_{ijt}$) using equation 5. For voucher household i in ZIP code j in year $t \in \{2010, 2013\}$, $\mathbf{1}(Post_t)$ is a dummy for 2013, FMR_j is the applicable FMR level in 2011 for ZIP code j, and b_{ijt} is set of dummy variables for the number of bedrooms, we estimate

$$\text{First Stage: } \bar{r}_{ijt} = \alpha + \gamma FMR_j \mathbf{1}(Post_t) + \omega FMR_j + b_{ijt} + \varepsilon_{ijt} \tag{5}$$

$$\text{Second Stage: } y_{ijt} = \alpha + \beta \widehat{\bar{r}_{ijt}} + \lambda FMR_j + b_{ijt} + \eta_{ijt} \tag{6}$$

Our second stage equation 6 estimates the effect of policy-induced changes in the payment standard β on voucher rents or building quality (y_{ijt}). Rents at the ZIP code-level were highly responsive to the policy change, as shown in Figure 6. Appendix Table 6 reports results from equations 5 and 6. Changes in FMRs are a strong predictor of changes in rent ceiling, with a coefficient of 62 cents. We find substantial rent increases in more expensive areas and rent decreases in cheaper areas; every \$1 change in the rent ceiling caused a 57 cent change in rents. This estimate is quite similar to the estimates in Section 4 that a \$1 change in the rent ceiling raised rents by 46-47 cents.

We also examine whether this change in the schedule led voucher holders to move to higher-quality buildings. We predict physical structure quality by applying the hedonic coefficients from Section 4.1 to data in Dallas on number of bedrooms, structure type, and structure age (but not building location). In 2010, voucher holders who lived in higher-quality neighborhoods had lower structure quality, as would be expected given the existence of a single, metro-wide rent ceiling. We find that for every dollar change in the rent ceiling, structure quality changed by 19 cents, as reported in Appendix Table 6. This evidence re-affirms that the tilting policy muted the trade-off between unit quality and neighborhood quality. However, this measure does not incorporate the improvements in neighborhood quality which we explore in the next section.

5.2 Impacts on Neighborhood Quality

We assemble data on five measures of neighborhood quality: poverty rate, 4th grade test scores at zoned school, unemployment rate, share of children in families with single mothers, and the violent crime rate.[23] We compute a neighborhood quality index, which equally weights all five measures.[24] Map 3 shows Dallas, with the neighborhood quality index colored from red (lowest) to blue (highest). Voucher holders tend to live in lower-quality neighborhoods, usually on the south side of the city. Map 3 also shows the change in voucher counts at the tract level from 2010 to 2013. A black dot indicates a net increase, a white dot represents a net decrease, and the size of the dot indicates the magnitude of the change. After the policy change, voucher holders exit the lowest-quality neighborhoods in the inner city, moving further south and east to better neighborhoods. Map 3 shows that the improvement in neighborhood quality was broad-based, and not driven by moves to or away from a single neighborhood.

[23] Poverty rate, unemployment, and share of kids in families with single mothers are ACS tract-level data from 2006 to 2010. Test scores are the percent of 4th grade students' scoring proficient or higher on state exams in the 2008-2009 academic year at zoned school. Violent crime is number of homicides, non-negligent manslaughter, robberies, and aggravated assaults per capita in 2010, and is calculated over the tract level for tracts in the city of Dallas, and at the jurisdiction level (city or county balance) for suburban voucher residents.

[24] Each component is standardized to have mean zero and unit standard deviation over the Dallas metro area.

To formally estimate the impact of the change to ZIP code-level FMRs, we use a simple difference-in-differences design with a comparison group of Fort Worth – a nearby city which continued to have a single metro-wide rent ceiling. We construct a balanced panel of voucher holders in the eight affected counties from 2010 to 2013 to mitigate any unrelated composition changes over time.[25] The identifying assumption is that neighborhood quality difference between Dallas voucher tenants and Fort Worth voucher tenants would have been stable absent the policy intervention. We estimate

$$Y_{it} = \alpha + \delta \mathbf{1}(Dallas_i \times Post_t) + \mathbf{1}(Dallas_i) + \mathbf{1}(Post_t) + \eta_{it} \tag{7}$$

where i indexes households and t indexes years, $\mathbf{1}(Dallas_i)$ is an indicator taking the value 1 if the voucher holder i is with an affected Dallas housing authorities, and zero if the voucher holder is with a Fort Worth housing authority, and $\mathbf{1}(Post_t)$ is an indicator if the observation is after the policy change became effective. The results are shown in Table 4, where δ shows an intent-to-treat (ITT) improvement of 0.10 standard deviations in neighborhood quality. This estimate is statistically precise, with a t-statistic greater than 3 using standard errors clustered at the tract level. Of course, neighborhood quality could only improve for tenants who moved. From 2010 to 2013, 46% of continuing voucher holders moved units, so the impact estimate for treatment-on-the-treated (TOT) is 0.23 standard deviations.[26]

Table 4 also provides impacts separately for each of the five neighborhood quality measures. We find small and statistically insignificant improvements of 0.09 SD in test scores at zoned schools and 0.05 SD in the neighborhood rate of children living with single mothers. We find medium-sized improvements of 0.19 SD in the neighborhood poverty rate and 0.21 in the neighborhood unemployment rate. The largest improvements are in the violent crime rate, which improves by 0.33 SD. If these relative improvements reflect voucher holders' valuations, then it seems that voucher holders prioritize getting away from high crime areas. This is consistent with evidence from the Moving to Opportunity (MTO) experiment, where treatment households chose tracts with much lower crime rates, less graffiti, and better police response when a call was made (Kling et al. 2005).

The timing and distribution of neighborhood choices is consistent with attributing the results

[25] We use a balanced panel to isolate the effects of the intervention on neighborhood quality. During this period, some housing authorities changed the allocation rules for new vouchers. For example, beginning in 2009 the Dallas Housing Authority allocated many of its new vouchers to homeless individuals. These individuals needed other non-housing services and are a very different population from standard voucher holders. Nevertheless, when we analyze impacts for new voucher recipients, they also show improved neighborhood quality after the policy change.

[26] The court settlement which precipitated the policy change also funded voluntary mobility counseling, provided by Inclusive Communities Project, the organization which filed the lawsuit. There were 303 voucher households who already had conventional vouchers in 2010 and took advantage of these counseling services by the end of 2012. Appendix Table 7 shows that households which received counseling showed dramatic improvements in neighborhood quality of 1.17 standard deviations. These large impacts may reflect self-selection or the causal impact of the intervention. If the quality improvement for these 303 households is entirely attributable to the causal impact of mobility counseling (and not to the ZIP code-level FMRs), then our estimates for the impact of ZIP code-level FMRs shrink by about 20%.

in Table 4 to the impact of the policy. Figure 7 shows that neighborhood quality moves in tandem for Dallas and Fort Worth through 2010; beginning in 2011, there is an immediate and sustained increase in Dallas which does not appear in Fort Worth. Figure 8 shows the distribution of neighborhood qualities chosen by movers; movers after the policy change appear to have a broad-based monotonic shift away from lower-quality neighborhoods and to higher-quality quality neighborhoods. No such change is evident for the control group in Fort Worth.

Averaging across the entire Dallas metro area, average voucher rents are essentially unchanged after tilting the rent ceiling, as shown in Table 4. Given that average neighborhood quality rose, it is somewhat surprising that this policy was budget neutral. The reason for this is that there is heterogeneity in where voucher holders live and they usually live in low-quality neighborhoods. Because they are concentrated in low-quality, inexpensive neighborhoods, the policy would have saved money absent any behavioral response in terms of improved neighborhood quality. Coincidentally, the additional expenditure on improved neighborhoods exactly offsets the cost savings from the policy.

5.3 Comparing Policies to Improve Neighborhood Quality

The impact on neighborhood poverty rates for voucher holders of the Dallas policy is substantial in comparison with the uniform increases studied in Section 4. We consider three scenarios: (1) a 10% increase in the rent ceiling, multiplied by the coefficient from the rebenchmarking estimate, (2) a shift of FMRs from the 40th to the 50th percentile, and (3) the Dallas policy. The rebenchmarking yields a precise zero, the shift to the 50th percentile yields an imprecise zero, and the Dallas policy yields an improvement which is statistically large and economically significant.[27]

We also compare the neighborhood quality impacts in Dallas to other randomized voucher interventions in Table 5. Voucher holders' access to areas with good schools, low poverty and low crime has been a major focus of research in recent years (Lens et al. 2011; Horn et al. 2014). Two prominent studies with random assignment of vouchers where the tract-level poverty rate and violent crime rate are available as outcome measures are the Moving to Opportunity (MTO) experiment and voucher random assignment in Chicago (Jacob and Ludwig 2012, Jacob et al. 2013). These studies are informative about two types of policy interventions: giving a voucher to someone in public housing and giving a voucher to someone receiving no housing assistance. From largest to smallest, the improvements are largest for the MTO experimental group, who were *required* to move to low-poverty tracts, medium-sized for people leaving public housing with unrestricted vouchers and zero for unassisted tenants given unrestricted vouchers. The improvements for people leaving public housing are unusually large in part because holders were leaving distressed public housing with a high concentration of poverty.

For each intervention, we construct a cost estimate and summary measure of the change in

[27] The results are shown in a bar graph in Appendix Figure B.3.

opportunity for a child affected by the policy. We construct our summary measures as an estimated effect on children's income rank as adult at age 30. Chetty and Hendren (2017) document heterogeneity in intergenerational mobility across US commuting zones. We use estimates from Chetty and Hendren (2017) of the cross-sectional relationship between the causal estimates of a childhood spent in a county and that county's violent crime and poverty rates to generate these predictions. To be precise, we take estimates from Table XII which reports the results of univariate regressions of the estimated "place effect" on a county characteristic (within commuting zone): for violent crime β^{sd}_{crime} (-1.99) and poverty β^{sd}_{pov} (-1.44). We then calculate the effects of each voucher intervention as:

$$\Delta Rank = \frac{\Delta Crime}{\sigma_{crime}}\beta^{sd}_{crime} + \frac{\Delta Pov}{\sigma_{Pov}}\beta^{sd}_{Pov}$$

where $\Delta Crime$ is the treatment and control difference from the intervention in the violent crime rate and σ_{crime} is the standard deviation of the violent crime rate in the Chetty and Hendren (2017) sample, similarly ΔPov is the intervention's effect on tract poverty rates and σ_{pov} is the standard deviation of poverty.

Our estimates of the causal impact of voucher interventions on children's outcomes make the following assumptions: (1) the child lived in the new location from birth to age 20 (2) the cross-sectional relationship between the county characteristics and estimates of the causal effect of places from Chetty and Hendren (2017) are accurate for the causal impacts of tract-level variation in neighborhood quality, and (3) the interventions only affect a child's adult earnings through impacts on neighborhood poverty and violent crime rates. The Chetty and Hendren (2017) results, combined with our assumptions, suggest that tilting the rent ceiling in Dallas with ZIP-level rent ceilings would raise a child's income rank at age 30 by 3.1 percentile points, from the 39th percentile to the 42nd percentile. This improvement for Dallas is smaller than the predicted improvement for the MTO Experimental group (17 percentage points), about one-half of the impact of offering unrestricted vouchers to public housing residents in MTO, and larger than offering vouchers to unassisted tenants.[28] We approximate the cost of receiving a voucher from public housing with the difference between the average annual cost of a voucher in our sample and an accounting estimate of the per unit cost to maintain the existing public housing inventory (Abt Associates 2010).[29] Based on these simple cost comparisons, tilting the rent ceiling in Dallas was a cost-effective way to improve opportunity in Dallas.

[28]This 2 percentage point prediction is if the policy moved children at birth and they stayed in the same neighborhood until age 20. In fact, the improvement neighborhood quality for the MTO experimental group decayed by about 80%, so the quality impact of MTO was smaller than the impact of the hypothetical policy considered here which permanently implemented voucher restrictions.

[29]This cost comparison makes no attempt to adjust for housing quality. Also, a more comprehensive cost comparison would take into account the opportunity cost of public housing land and structures, which are not reflected here. The per-family cost of providing a voucher is typically less costly than providing a new public housing unit. For a comprehensive review of studies on the cost of providing voucher and project-based subsidies see Olsen (2008).

6 Conclusion

We examine who benefits from two policies designed to improve the neighborhood quality of voucher holders: raising the rent ceiling uniformly and tilting the rent ceiling so that it is higher in high-quality neighborhoods and lower in low-quality neighborhoods. Across two separate research designs we find that increasing the rent ceiling uniformly by $1 raises voucher rents by roughly 46 cents with no commensurate improvements in housing or neighborhood quality. In contrast, tilting the rent ceiling in Dallas causes voucher families to move to notably safer and less impoverished neighborhoods at zero net cost to the government. Although tilting the rent ceiling is highly cost-effective and voucher holders move to *better* neighborhoods, the destination neighborhoods are still of a relatively low quality relative to the distribution for Dallas as a whole. Future research should seek to identify other barriers or preferences which affect the neighborhood quality of voucher holders.

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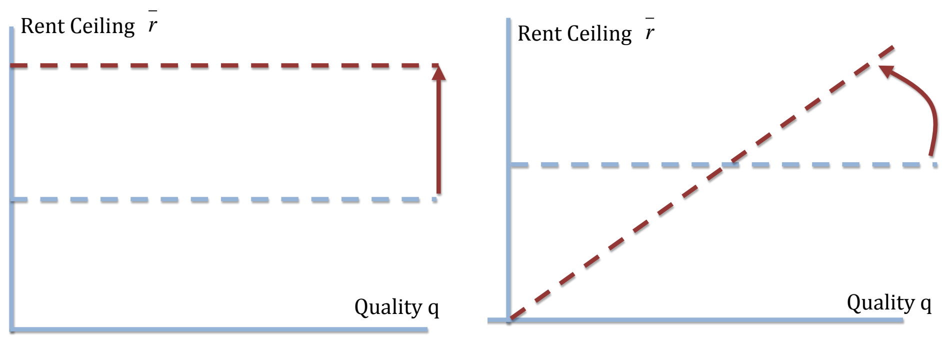
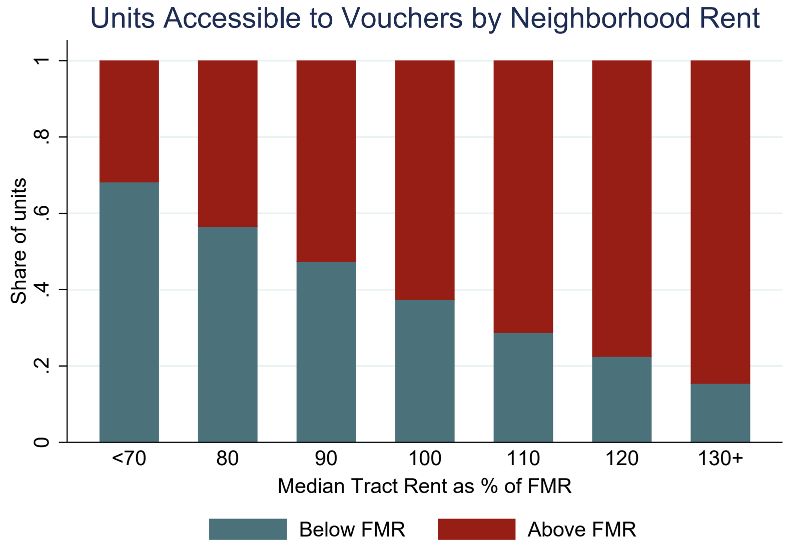
Figure 1: Changes in Rent Ceiling

Uniform increase of $\bar{r}$ Compensated Tilt of $\bar{r}$



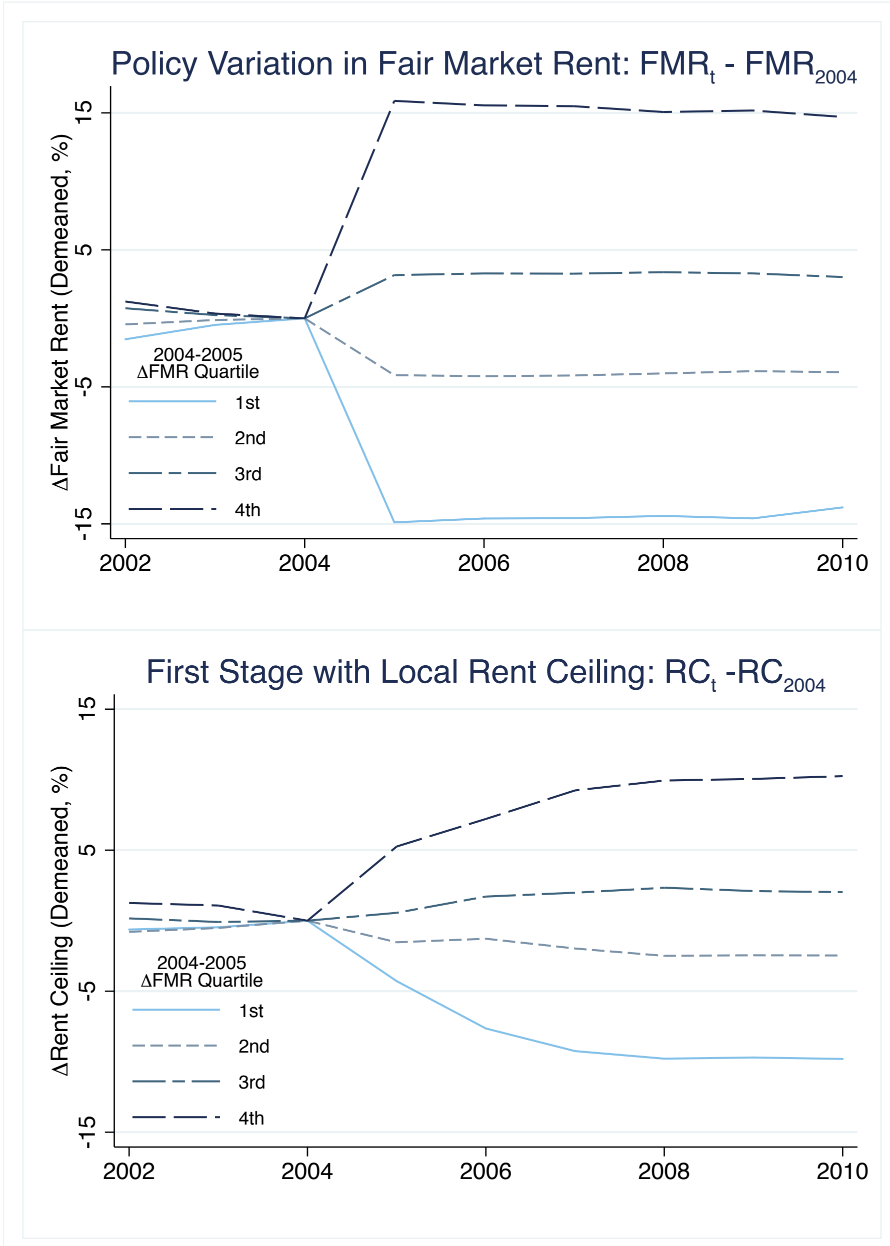
Notes: This figure shows the two changes in voucher generosity that we study in this paper. The first increases the maximum per-unit government subsidy – which we refer to as the "rent ceiling" – uniformly in all neighborhoods in a metro area. The second increases the ceiling in higher-quality ZIP codes and lowers it in lower-quality ZIP codes.

Figure 2: Unit Availability and Rent Distribution



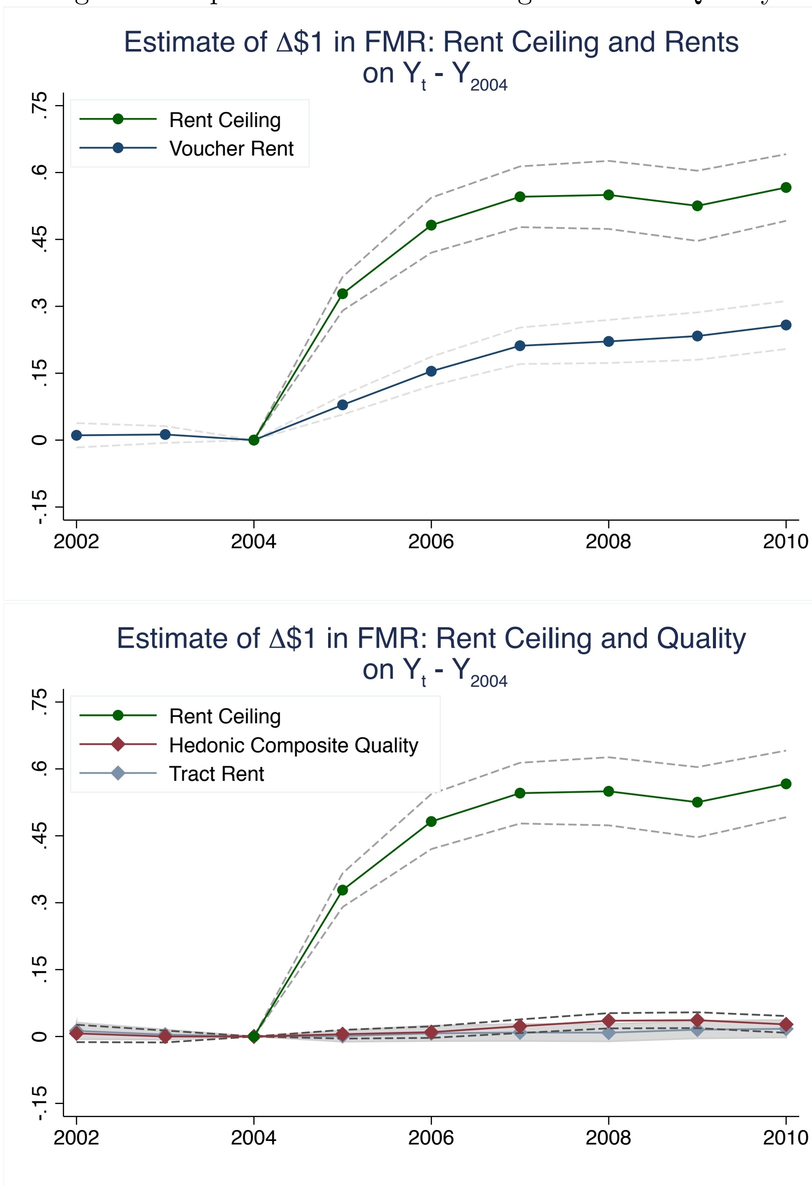
Notes: Each year, the federal government publishes "Fair Market Rents." These are typically estimated as the 40th percentile of rent in a county for studios, 1 bedroom, 2 bedroom, 3 bedroom and 4 bedroom units. For each Census tract, we compute the share of rental units priced at or below the 40th percentile of the metro area rent distribution. The top panel shows the average fraction of units priced below the rent ceiling as a function of median tract rent. Data are drawn from a special tabulation of the 2009-2013 ACS five-year estimate and FY2013 Fair Market Rents.

Figure 3: Event Study for Rebenchmarking



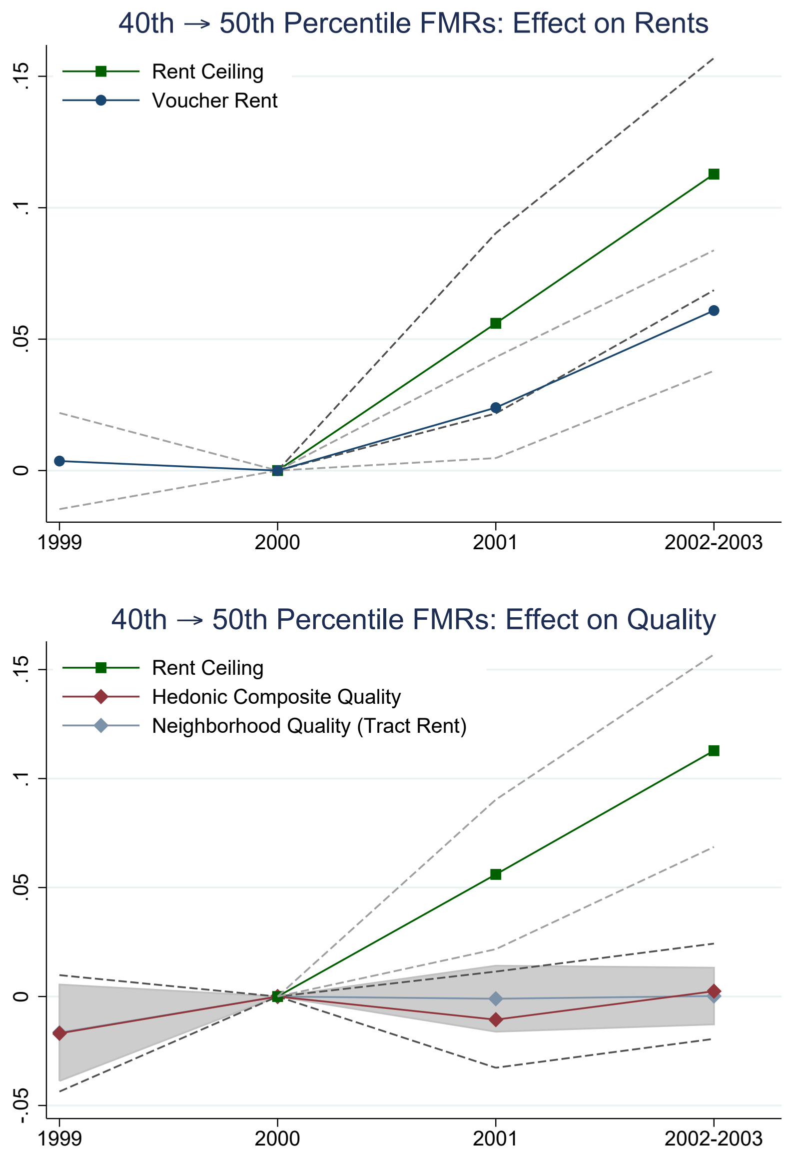
Notes: In 2005, the government made large revisions as part of a "rebenchmarking" to incorporate newly-available data from the 2000 Census. The top panel plots demeaned changes in the Fair Market Rent for four quartiles of county-bed observations, stratified by the change from 2004 to 2005. Local housing authorities administer the vouchers, and have discretion to set the local rent ceiling at 90%, 100% or 110% of Fair Market Rent. The bottom panel plots local rent ceilings, using the same grouping of county-beds as in the top panel.

Figure 4: Impacts of Rebenchmarking: Rent and Quality



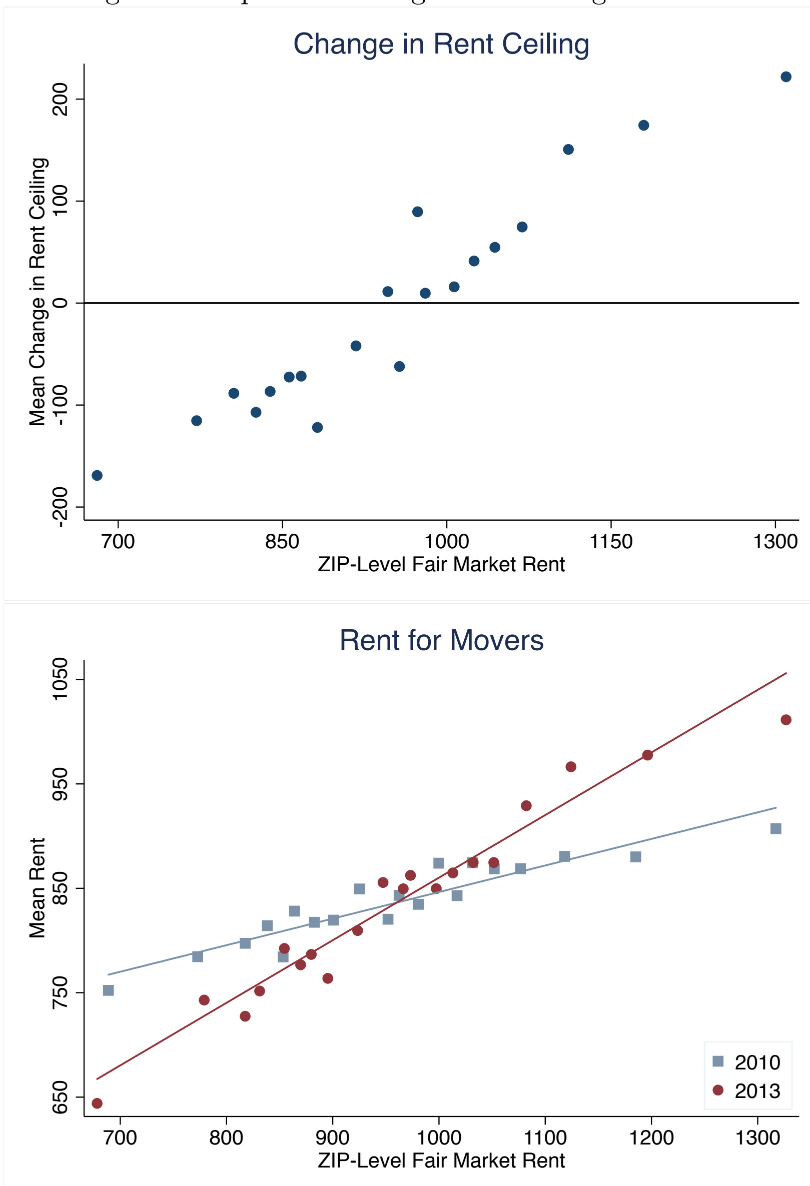
Notes: We plot β coefficients from a reduced form regression for rent ceilings, rents and quality using the following equation $\Delta y_{t,j} = \alpha + \beta FMR_{2005,j} + \phi FMR_{2004,j} + \upsilon \bar{r}_{2004,j} + \varepsilon_j$. The coefficient β captures the impact of a $1 increase in the FMR on each variable. Hedonic quality is measured using number of bedrooms, structure type, structure age and median tract rent. Shaded area / dashed lines indicate 95% confidence intervals. Rental data from 2002 and 2003 are a test for pretrends, and the 2004-2005 first stage is used. See Section 4.1 for details.

Figure 5: Impacts of 40th → 50th Percentile FMRs: Rent and Quality



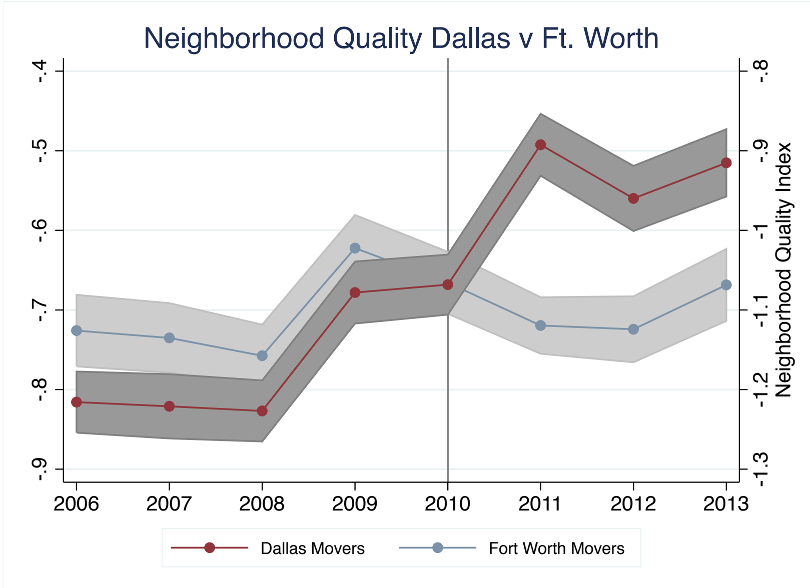
Notes: The top panel shows an event study for changes in the rent ceiling and voucher rents around the introduction of 50th percentile FMRs in 2001. The bottom panel plots the same event study for changes in quality: hedonic composite quality and neighborhood quality. Hedonic composite quality is measured using number of bedrooms, structure type, structure age, median tract rent, and 26 survey questions about unit quality and maintenance. Neighborhood quality is measured using median tract rents. Shaded area / dashed lines indicate 95% confidence intervals. See notes to Table 3 for details.

Figure 6: Impact of Tilting: Rent Ceiling and Rents



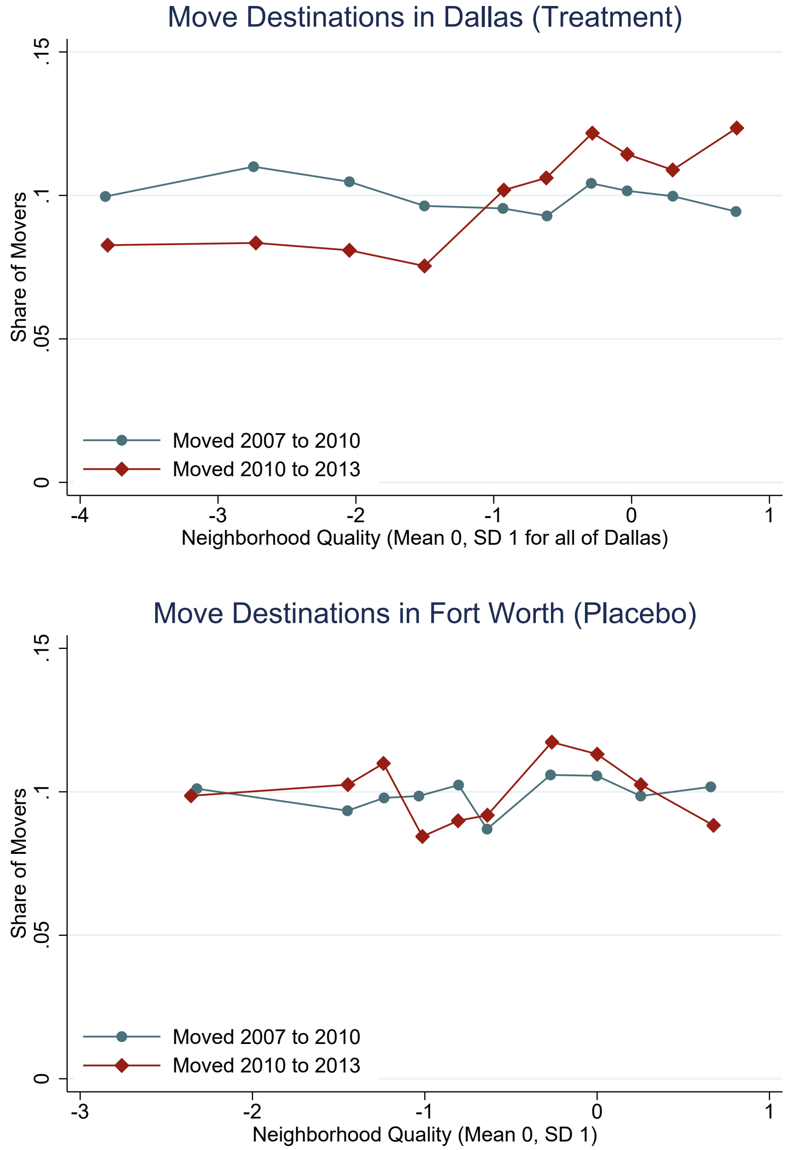
Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs. The top panel shows that this policy raised rent ceilings in expensive neighborhoods and lowered rent ceilings in cheap neighborhoods. Using a sample of households which moved from 2010 to 2013, we residualize ZIP FMRs and tenants' rent ceiling by the number of bedrooms, add back the unconditional mean for each, and plot conditional mean rent ceilings for 20 quantiles of residualized ZIP code-level FMR.
This bottom panel plots mean rents against the zip-code level FMR for movers from 2010-2013 at their 2010 and 2013 zip codes. We follow the same procedures as above using residual voucher rents by bedroom size. Each dot reflect means for 20 quantiles of the ZIP code-level FMR distribution conditional on bedroom-year in 2010 and in 2013. Rents were quite responsive to the new rent ceiling schedule.

Figure 7: Impact of Tilting: Neighborhood Quality (Timeseries)



Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs, raising rent ceilings in expensive neighborhoods and lowering rent ceilings in cheap neighborhoods. We construct a neighborhood quality index as an equally-weighted sum of tract-level poverty rate, test scores, unemployment rate, share of kids with single mothers, and violent crime rate. The index is normalized to have mean zero and unit standard deviation with respect to the entire Dallas metro area. The above figure plots the average neighborhood quality for movers in each year in the Dallas metro area and the Fort Worth metro area. The left vertical axis is the quality level of Fort Worth movers, the right vertical axis reports the quality level of Dallas movers, and both axes share the same scale.

Figure 8: Impact of Tilting on Neighborhood Quality (Distribution)



Notes: The top panel shows the distribution of destination quality for people who moved from 2007 to 2010 (before the policy) and people who moved from 2010 to 2013 (after the policy). There is a broad-based improvement in destination quality in Dallas, with no change in nearby Fort Worth, which did not implement the policy.

A Model Appendix (For Online Publication)

We build a model to understand how changes in voucher generosity may accrue to landlords or tenants. The model contains two key features. First, landlords post prices, and may adjust their posted price based on the government-set rent ceiling. In particular, they may post a price equal to the rent ceiling and actively recruit voucher holders; together, these activities act as a means of price discrimination. Second, it is harder for a new voucher holder to find a unit in a high-quality neighborhood than in a low-quality neighborhood.

The assumption that voucher holders face a trade-off between finding a unit in a high-quality neighborhood and finding a unit at all is motivated by three features of the institutional context. First, because vouchers typically pay a flat amount across a metro area, a voucher can cover the cost of 68% of units in the lowest-rent neighborhoods but only 15% of units in higher-rent neighborhoods, as shown empirically in Figure 2 (top panel). Second, once a tenant is issued a voucher, she has 60-90 days to "use or lose it". These challenges are exacerbated for reasons unique to housing voucher holders such as discrimination, high transportation costs, and steering to specific units.[30] Given these constraints, it is not surprising that roughly one-in-three families issued a voucher are unable to lease a unit under the program in the allotted time (Abt Associates 2001).

Two lessons emerge from the model's comparative statics. Historically, HUD has attempted to improve neighborhood quality using uniform increases in voucher generosity (U.S. Department of Housing and Urban Development 2000). The model's first lesson is that theory does *not* provide a clear prediction whether a uniform increase in voucher generosity will accrue to landlords or tenants. Tenants will benefit if the probability of matching is already high such that they use the more generous vouchers to move to better neighborhoods. On the other hand, landlords will benefit if they can raise their rents without tenants moving to quality.

The second lesson is that tilting the rent ceiling is a cost-effective way to raise neighborhood quality. A policy lever which HUD has piloted in recent years is tilting the rent ceiling so that it is higher in high-quality neighborhoods and lower in low-quality neighborhoods. Intuitively, this policy reduces the penalty for searching in high-quality neighborhoods which is implicit in the status quo policy. This policy is cost-effective because it changes the incentives voucher holders face when searching, without increasing the opportunity for price discrimination by raising the average rent ceiling.

A.1 Environment

There is a continuum of neighborhoods with heterogeneous quality q where q is an observable, dollar-denominated index with positive measure for all $q \geq q_{min}$ and zero measure for $q < q_{min}$. Our model focuses on differences in neighborhood quality because improving neighborhood quality is the explicit objective of the rent ceiling policies we study (U.S. Department of Housing and

[30] Audit studies have found that landlords discriminate, refusing to rent to people with a voucher (Lawyers Committee for Better Housing Inc 2002; Perry 2009). Voucher recipients also seem to have high transportation costs; participants with cars in the Moving to Opportunity experiment moved to and stayed in higher-quality neighborhoods in terms of crime and school quality (Pendall et al. 2014). Voucher holders are often steered towards a short list of units by housing authority recommendations (Abt Associates 2001).

Urban Development 2000). However, our empirical analysis also estimates improvements in unit quality because this is one way that increases in voucher generosity can accrue to tenants rather than landlords.

A.1.1 Housing Demand

In each neighborhood q, there are private nonvoucher (NV) tenants whose demand is decreasing in rental price r. Their housing demand gives rise to a reduced-form demand curve $D_{NV}(r;q)$. Because the focus of this paper is the neighborhood choices of voucher recipients, we take the demand of nonvoucher recipients as exogenous.[31] Voucher holders demand is not price sensitive, and they will lease any unit at or below the government-set voucher rent ceiling of $\bar{r}$. Voucher demand is given by

$$D_V(r,q) = \begin{cases} 0 & r > \bar{r} \text{ or } q \neq q^* \\ \alpha \tilde{D}_V & r = \bar{r} \text{ and } q = q^* \\ \tilde{D}_V & r < \bar{r} \text{ and } q = q^* \end{cases}$$

where q^* is the neighborhood that voucher holders rent in (the optimal choice of q^* is described in Section A.1.3), and $\tilde{D}_V$ is the endogenously-set share of units leased to voucher holders with $r < \bar{r}$.[32] In Section A.1.2, we explain that landlords making an active choice to set their rent at the rent ceiling also engage in recruiting activity which results in additional voucher holder demand, reflected in the exogenous parameter $\alpha > 1$. The total occupancy rate of units in q renting at price r is

$$D(r;q) = D_{NV}(r;q) + D_V(r,q).$$

and is assumed to be between 0 and 1.

A.1.2 Landlord's Problem

There is a unit mass of landlords indexed by i in each neighborhood q. For simplicity, we suppress the q argument in this subsection. Landlords each own one unit of housing, and landlords may choose one of two rents: $\{r_i, \bar{r}\}$:

1. r_i is the landlord's reservation rent if they were renting only to private tenants. As with private nonvoucher tenant demand, landlord r_i is set outside the model. The variable $x = r_i - q$ embodies the markup or discount charged by the landlord relative to the quality in the neighborhood. We assume x has univariate distribution F in all neighborhoods q. As a regularity condition, assume that F is twice-differentiable with $\frac{df(x)}{dx} < 0$. Later in our

[31] In practice, it seems likely that any re-optimization by nonvoucher recipients in response to housing voucher policy changes will be small because voucher holders are only 6% of U.S. renters.

[32] A small fraction of voucher recipients choose to rent a unit priced above the rent ceiling and pay more out-of-pocket, as discussed in Section 2. This could be incorporated into the model by allowing for modest unit demand in the case when $r > \bar{r}$. Because few voucher recipients rent units above the ceiling and those that do will be price-sensitive, incorporating these tenants into the model would have little impact on the landlord's incentives in setting pricing.

analysis, we use this assumption to generate a trade-off between the probability of finding a unit and neighborhood quality, which ensures a unique solution to the voucher holder's problem.

2. The landlord may also set rent at $\bar{r}$, which is the voucher rent ceiling.[33] Landlords who choose this rent also engage in activities to recruit voucher holders and ensure that their unit would pass the inspection for Housing Quality Standards mandated by the voucher program.[34] Recruiting activity increases demand from voucher holders $\tilde{D}_V$ to $\alpha \tilde{D}_V$ where $\alpha > 1$ is an exogenous parameter. However, this activity has effort cost e_i. As a regularity condition, we assume that $e_i > \bar{r}(\alpha \tilde{D}_V + D_{NV}(\bar{r})) - r_i D_{NV}(r_i)$. The intuition for this assumption is that recruiting activities are sufficiently costly that a landlord whose reservation rent r_i is greater than $\bar{r}$ will not lower her rent in order to attract voucher holders. These assumptions are consistent with qualitative evidence that some landlords in low-quality neighborhoods specialize in recruiting voucher holders (Rosen 2014, Turner 2003).

Landlord profits $\Pi(r)$ are rent times the occupancy rate minus any recruiting costs. The landlord chooses rent to maximize profits:

$$\begin{aligned} \Pi(r) &\equiv rD(r) - e_i \mathbf{1}(r = \bar{r}) \\ r^* &= \max_{r \in \{r_i, \bar{r}\}} \Pi(r). \end{aligned} \tag{8}$$

Conditional on revenue, landlords are indifferent between leasing to a voucher tenant or a private tenant. Note that in neighborhoods without any voucher holders, $D_V = 0$ and so it is always the case there that $r_i^* = r_i$.

A.1.3 Voucher Holder's Problem

There is a representative agent for voucher holders. Recall that the agent is not price sensitive, so she will rent any unit which costs less than or equal to the rent ceiling. The probability of finding a unit is $\mathbb{P}(\bar{r}, q)$ in the neighborhood she choses to search in. The probability is increasing in $\bar{r}$ and decreasing in q. Let $V(q)$ (with $V'(q) > 0$ and $V''(q) < 0$) denote the relative utility gain from finding a unit with quality q over remaining unmatched. The agent chooses to search in a neighborhood of a quality level q to maximize utility:

$$\begin{aligned} q^* &= \max_q U(\mathbb{P}, q) \\ &= \max_q \underbrace{\mathbb{P}(\bar{r}, q)}_{\text{Match Probability}} \quad \underbrace{V(q)}_{\text{Utility if Matched}} . \end{aligned} \tag{9}$$

The utility function as defined above yields a trade-off between match probability and neighborhood quality. Higher-quality neighborhoods q are more attractive to voucher holders, but it is

[33] Housing authorities are required to verify that the rent on the unit is reasonable as described in Section 2. This could be modeled as the housing authority rejecting voucher leases among some units which the landlord priced at $\bar{r}$. The housing authority would be most likely to reject when the distance between r_i and $\bar{r}$ is large.

[34] In principle, the decision to set rent at the rent ceiling and the decision to actively recruit voucher holders are separable. However, separating these decisions complicates the algebra and our simpler model contains sufficient conditions for price discrimination.

harder to find a unit in those neighborhoods. Define $F^*(x)$ as the distribution of optimal rents in q^* with $x = r^* - q^*$. The voucher holder's probability of finding a unit is:

$$\mathbb{P}(\bar{r}, q) \equiv \begin{cases} F^*(\bar{r} - q) & \text{if } q = q^* \\ F(\bar{r} - q) & \text{if } q \neq q^* \end{cases}.$$

It will be convenient to define the joint distribution (e_i, r^*) as G. There are measure $\bar{V}$ of voucher holders who successfully lease a unit. This is the sum of voucher holders renting units priced at the ceiling and voucher holders renting units priced below the ceiling:

$$\alpha \left(F^*(\bar{r} - q^*) - F(\bar{r} - q)\right) \tilde{D}_V + F(\bar{r} - q)\tilde{D}_V = \bar{V}. \tag{10}$$

A.1.4 Policy Parameters

Assume that the rent ceiling has a linear structure $\bar{r}(q) = r_{base} + cq$ with $c \in [0, 1)$. Historically, HUD has used a single rent ceiling r_{base} across an entire metro area, with $c = 0$. However, this formulation is useful because in Section 5, we analyze a recent HUD policy innovation that tilted the rent ceiling to lower r_{base} and make c positive.

A.2 Equilibrium Definition and Solution

Equilibrium Definition - Given occupancy rates, a measure of vouchers, a distribution of effort costs and landlord reservation rents, recruiting technology, and voucher holder utility $\{D(r; q), \bar{V}, \{e_i, r_i\}, \alpha, V(.)\}$, an equilibrium is defined by three conditions:

1. Landlords price optimally using equation 8.

2. Voucher holders choose neighborhoods optimally using equation 9.

3. The market for vouchers clears using equation 10.

Solution – We show that each of the three conditions holds so an equilibrium exists. To show that the first condition is satisfied, note that landlords can only choose two possible rent levels in equation 8, so a landlord will choose $\bar{r}$ if

$$\begin{aligned} \Pi(\bar{r}) &> \Pi(r_i) \Rightarrow \\ \bar{r}\alpha D_V + \bar{r} D_{NV}(\bar{r}) - e_i &> r_i D_V + r_i D_{NV}(r_i) \Rightarrow \\ \underbrace{(\bar{r} - r_i)}_{\text{higher rent}} \left(\tilde{D}_V + D_{NV}(\bar{r})\right) + \underbrace{\bar{r}(\alpha - 1)\tilde{D}_V - e_i}_{\text{gain from recruiting vouchers}} &> r_i \underbrace{\left(D_{NV}(r_i) - D_{NV}(\bar{r})\right)}_{\text{lower occupancy rate}} \end{aligned} \tag{11}$$

The first term on each side of the inequality in equation 11 reflects the classic price versus quantity trade-off for a monopolistic supplier. Raising the posted price raises revenue conditional on occupancy, but reduces the occupancy rate. The second term on the left-hand side of the inequality reflects benefits and costs unique to the voucher market.

By charging $\bar{r}$ and actively recruiting voucher holders, our model effectively allows landlords to price discriminate. Comparative advantage dictates that only some landlords price discriminate. Specifically, by setting $\Pi(\bar{r}) = \Pi(r_i)$, it is possible to trace out a frontier of effort costs and reservation rents $(\hat{e}, \hat{r})$ where the landlord is indifferent about which price to choose. Landlords with (e_i, r_i) below this frontier, meaning that they have a combination of low recruiting effort costs and/or low reservation rents in the private market, will optimally set rents at the rent ceiling.

The second equilibrium condition is that voucher holders choose their preferred neighborhood. It is convenient to make two algebraic substitutions in the tenant's problem in equation 9: $\bar{r}(q) = r_{base} + cq$ and $\mathbb{P}(\bar{r}, q) = F(\bar{r} - q)$. The first substitution comes from the definition of the rent ceiling in Section A.1.4. For the second, recall from Section A.1.3 that $\mathbb{P} = F(\bar{r} - q)$ for all q except q^*, where it is $F^*(\bar{r} - q^*)$. However, because of the regularity assumption on e_i in the landlord's problem, only landlords with $r_i < \bar{r}$ will consider raising their prices to $\bar{r}$ and no landlords with $r_i > \bar{r}$ will lower their price to $\bar{r}$. This implies that $F^*(\bar{r} - q^*) = F(\bar{r} - q)$. Next, differentiate the voucher holder utility function with respect to q. The unique solution of optimal neighborhood choice $q = q^*$ is implicitly defined by[35]

$$\underbrace{(1-c)}_{\text{Penalty for Better Neighborhood}} \times \underbrace{f(r_{base} + cq - q)V(q)}_{\text{Increased Matching Probability}} = \underbrace{F(r_{base} + cq - q)V'(q)}_{\text{Increased Neighborhood Quality}}. \tag{12}$$

Equation 12 reveals that tenants choose a neighborhood q^* by trading off the left-hand side – which is the increased probability of finding a unit from choosing a lower q^* – with the right-hand side, which is additional utility from living in a higher-quality neighborhood.

The third equilibrium condition, which is the market-clearing condition for vouchers, is given by equation 10. $\bar{V}$ and α are fixed exogenously, and $(F^*(\bar{r} - q^*) - F(\bar{r} - q))$ is set by equation 10. The market-clearing equation can be solved by setting the free parameter $\tilde{D}_V$ as $\bar{V}/[\alpha\left(F^*(\bar{r} - q^*) - F(\bar{r} - q)\right) + F(\bar{r} - q$ This equilibrium $\left(\left(F^*(\bar{r} - q^*) - F(\bar{r} - q)\right), \tilde{D}_V\right)$ is unique.[36]

Remark – Recall that G is the joint distribution of optimal rents and effort costs (e_i, r^*). The average rent paid on voucher units is

$$\mathbb{E}_G r^* \equiv \int_{-\infty}^{\bar{r}-q} \int_{e_{min}}^{\hat{e}} \left[1 + \alpha \mathbf{1}(x = \bar{r} - q^*)\right](x + q^*) dG(e_i, r^*). \tag{13}$$

A.3 Comparative Statics

We first characterize how an increase in housing voucher generosity affects average voucher rents.

Proposition 1 *Raising the rent ceiling increases the average rent paid on voucher units.*

$$\frac{\partial \mathbb{E}_G(r^*; q)}{\partial \bar{r}} = \underbrace{\alpha f^*(\bar{r} - q)}_{\text{units at rent ceiling}} + \underbrace{\left(\bar{r} - E\left[r|(e_i, r_i) = (\hat{e}, \hat{r})\right]\right)}_{\text{gap in rents relative to ceiling}} \underbrace{g(\hat{e}, \hat{r})}_{\text{units re-pricing to rent ceiling}}$$

[35] Proof: Differentiate equation 9 twice with respect to q. The second-order condition in the maximand $U(\mathbb{P}, q)$ is negative: $U_{qq} = (-1+c)^2 \frac{df(\cdot)}{dq} V(\cdot) + 2f(\cdot)V'(\cdot)(-1+c) + F(.)V''(\cdot) < 0 \forall q$. The first term is negative because $\frac{df(\cdot)}{dq}$ is negative by assumption, the second term is negative because $c < 1$ and the third term is negative because $V'' < 0$ by assumption.

[36] The equilibrium is unique because landlord price discrimination $f^*(\bar{r} - q^*)$ is strictly increasing in $\tilde{D}_V$ and the market-clearing condition implies that $f^*(\bar{r} - q^*)$ is strictly decreasing in $\tilde{D}_V$. For the first clause, note that increased $\tilde{D}_V$ increases the incentive to price discriminate, thereby raising $f^*(\bar{r} - q^*)$. For the second clause, totally differentiate the market clearing condition with respect to f^* and solve for $\frac{d\tilde{D}_V}{df^*}$. This yields $\frac{d\tilde{D}_V}{df^*} = \frac{(1-\alpha) - f^* \tilde{D}_V}{(\alpha - 1) f^* + F^*}$, which is negative because the numerator is negative and the denominator is positive.

Proof: Differentiate equation 13 with respect to $\bar{r}$.

This proposition applies to every neighborhood q. However, the expression collapses to zero in a neighborhood with no voucher holders. Proposition 1 shows that average rents rise most when there are already many units priced at the rent ceiling which will increase their rents, and when there are many landlords who re-price their units from the prior rent r_i to the new ceiling $\bar{r}$.

Next, we consider how two changes to the schedule of rent ceilings across a metro area affect optimal neighborhood quality chosen by voucher holders. Recall that the rent ceiling can be expressed as a constant r_{base} and a linear slope c: $\bar{r}(q) = r_{base} + cq$. We analyze the impact on quality of raising r_{base} and the impact of raising c.

Proposition 2 *Starting from a constant rent ceiling ($c = 0$), the impact on neighborhood quality of raising the rent ceiling r_{base} or raising it by c is*

$$\frac{\partial q^*}{\partial r_{base}} = \frac{\overbrace{\frac{\partial f(.)}{\partial x} V(\cdot)}^{U_{\mathbb{PP}}} - \overbrace{f(\cdot)V'(\cdot)}^{U_{\mathbb{P}q}}}{SOC} > 0$$

$$\frac{\partial q^*}{\partial c} = \frac{\frac{\partial f(.)}{\partial x} V(\cdot)q^* - f(\cdot)V'(\cdot)q^* - \overbrace{f(\cdot)V(\cdot)}^{U_{\mathbb{P}}}}{SOC} > 0$$

where second-order condition $SOC \equiv \frac{\partial f(.)}{\partial x} V(\cdot) - 2f(\cdot)V'(\cdot) + FV''(\cdot) < 0$.

Proof: Differentiate equation 12 with respect to r_{base} and with respect to c.

When the rent ceiling increases uniformly ($\frac{\partial q^*}{\partial r_{base}}$), absent any behavioral change, the probability of finding a unit rises in every potential neighborhood. Two forces lead the voucher holder to substitute to a higher-quality neighborhood. The first term in the numerator, $U_{\mathbb{PP}}$, leads to increased quality because as the probability of finding a unit approaches 1, additional increases in the probability of matching do little to increase utility. The second term in the numerator, $U_{\mathbb{P}q}$, leads to increased quality since an additional unit of quality is more valuable when the probability of successfully leasing is higher. However, if tenants put little value on improving neighborhood quality and the policy change substantially increases the probability of finding a unit, then raising r_{base} will have little impact on neighborhood quality.

When the rent ceiling tilts toward higher-quality neighborhoods ($\frac{\partial q^*}{\partial c}$), the neighborhood quality rises even more sharply than from a uniform rent ceiling increase. Algebraically, $\frac{\partial q^*}{\partial c}$ can be decomposed as

$$\frac{\partial q^*}{\partial c} = \underbrace{\frac{\partial q^*}{\partial r_{base}} q^*}_{\text{Uniform ceiling increase}} + \underbrace{\frac{-f(.)V(.)}{SOC}}_{\text{Decreased penalty for good neighborhoods}} . \tag{14}$$

The impact of a tilt in the rent ceiling is equal to the sum of (1) a uniform increase in the rent ceiling and (2) a policy which lowers the probability of matching in low-quality neighborhoods and raises it in high-quality neighborhoods. We call this second policy a "compensated tilt". Each of these policy changes are depicted visually in Figure 1.

Two lessons emerge from the comparative statics. The first major lesson from our model is that a uniform increase in the rent ceiling may accrue to landlords through higher voucher rents (Proposition 1) or to tenants if they optimally decide to search in a higher-quality neighborhood (Proposition 2). The voucher rent response is larger when when the effectiveness of recruiting activities α is higher and when the cost of recruiting activities e_i is lower. The quality response is larger when tenants put a relatively high weight on neighborhood quality (embodied by $V(q)$) or when the probability of finding a unit is already high.

The second major lesson is that a compensated tilt – unlike a uniform increase – is a cost-effective way to raise neighborhood quality. Algebraically, by subtracting the impact of the change in r_{base} in equation 14, the expected change in neighborhood quality is

$$\frac{\partial q^*}{\partial c}\Big|^{Compensated} = \frac{-f(\cdot)V(\cdot)}{SOC}.$$

To be specific, consider a policy that decreases r_{base} by Δr and increases c by $\Delta r/q^*$. This policy is cost-effective because it holds $\bar{r}(q^*)$ constant ($\bar{r}(q^*) = r_{base} - \Delta r + (c - \Delta r/q^*)q^* = r_{base} + cq^*$) and since $\bar{r}(q^*)$ is unchanged, there is no opportunity for increased price discrimination. Nevertheless, optimal neighborhood quality rises because the penalty for searching in a higher-quality neighborhood ($1 - c$ from the left-hand side of the tenant's first-order condition in equation 12) is diminished. Government expenditure increases only if q^* rises. This ensures that every dollar of extra government expenditure goes to neighborhood quality.

A.4 Relation to Prior Models

As far as we know, our emphasis on price discrimination and search frictions is new to the literature studying vouchers and does a better job of explaining this paper's empirical findings than two existing benchmark models. In one benchmark model, people frictionlessly trade-off housing and non-housing consumption and housing vouchers introduce a kink into the budget constraint (Collinson et al. 2015, Olsen (2003)). This model predicts that housing voucher holders should rent units with prices at least as high as the rent ceiling. This prediction is inconsistent with the data. In fact, 60 percent of housing voucher holders rent units below the ceiling (Figure 2, bottom panel).

A second class of benchmark model argues that voucher holders derive relatively more utility from living in low-quality neighborhoods (Geyer 2011, Galiani et al. 2015). This model makes two predictions which are inconsistent with research on housing vouchers. The first prediction that differs from the data is that a preference model with voucher holders valuing structure over neighborhood quality predicts that voucher holders in low-quality neighborhoods will live in high-quality units. However, as shown in Figure B.4, voucher holders actually live in units with rents below the ceiling and as we document in Section 4, when there is a uniform increase in the rent ceiling, there is at most a modest improvement in observable structure quality. Second, the dynamic path of voucher holders' neighborhood choices is consistent with it being hard to find a good unit upon initial admission to the voucher program rather than a preference for low-quality neighborhoods. Eriksen and Ross (2013) document that in the Welfare to Work Voucher experiment, voucher holders signed their first lease in neighborhoods of no better quality than their prior residence (as measured by poverty and employment rates); however, neighborhood quality improved subsequently over the next four years. This is qualitatively consistent with a model where at first voucher holders worry about finding a unit to lease and only then worry about neighborhood quality.[37]

B Empirical Appendix

B.1 Sample Construction

We use HUD's "PIH Information Center" database, also known as PIC. In principle, every voucher is supposed to appear in PIC when admitted, when leaving the voucher program, for a regularly scheduled annual recertification, and for any unscheduled interim recertification due to, for example, a change in tenant payment or a move. Coverage is quite good for an administrative dataset with decentralized data entry; HUD estimates that in 2012, some record appeared in PIC for 91% of vouchers (Public and Indian Housing Delinquency Report (2012)). We construct years according to the federal government's fiscal year (e.g. FY2012 starts in October 2011), since this is the calendar used for applying Fair Market Rent changes. We consider observations with non-missing rent, household id, address text, and lease date (also known as "effective date"). Addresses are standardized using HUD's Geocoding Service Center, which uses Pitney and Bowes' Core-1 Plus address-standardizing software. For each raw text address, this produces a cleaned text address, a 9-digit ZIP code and an 11-digit ZIP code. Within each household-year, we choose the observation with the most recent lease date and most recent server upload date. Our final step is to drop duplicate household-year observations, which amount to 2.3% of the sample and project-based vouchers, where the housing authority chooses the unit, rather than the tenant, which are less than 1% of the sample. This leaves us with a sample of about 1.6 million annual household records. Conditional on appearing in the sample in 2004, the probability of that household appearing in 2005 is 75%, and the probability of appearing in 2005, 2006, or 2007 is 84%, indicating that there often are substantial lags between appearances in PIC.

B.2 2005 FMR Rebenchmarking

Constructing the FMR Cells: We use HUD's published Fair Market Rent rates, with slight modifications (http://www.huduser.org/portal/datasets/fmr.html). Fair Market Rents are published on an annual basis corresponding to the federal fiscal year, so FY2005 rents were effective from October 1, 2004 to September 30, 2005. FMR geographies are largely stable over time; HUD added 14 new city geographies in Virginia, and we code prior FMRs for these cities using the county-level FMRs. Our policy variation is at the county-bed cell level and measurement error $\varphi_{2000} - \varphi_{1990}$ is larger for thinner cells. To maximize the variation in our instrument which can be attributed to classical measurement error, we weight each county-bed equally. In New England, FMRs are set by NECTAs, which cross county lines and we merge on FMRs to the appropriate sub-state geographies there. However, we weight each county-bed pair equally everywhere, including New England; were we to give equal weight to each geographic unit, then 1/3 of the sample weight would be in New England. Gordon (2004) and Suarez-Serrato and Wingender (2014) also use decennial Census rebenchmarkings as source of exogenous variation to examine the incidence of federal expenditures.

Sample Restrictions: The rebenchmarking resulted in large swings in local rents, and many housing authorities lobbied HUD for upward revisions to their local FMRs. In a revision to the 2005 FMRs, HUD accepted proposals from 14 counties. All documentation associated with the rebenchmarking is posted here. For these counties, we recode the FMR back to its pre-lobbying level. Coincident with the rebenchmarking, HUD administered Random Digit Dialing (RDD) surveys in 49 metropolitan areas. The results from these surveys, where available, superseded the results from the 2000 Census. Since these surveys were initiated and administered by HUD, we are less concerned about endogeneity of this data source, and we use the post-RDD FMRs for these areas. For these areas, the orthogonality restriction is that rental market changes from 1990 to 2004 need to be uncorrelated with subsequent short-run changes ($E(\Delta r^{Nonvoucher}_{2004-t} | \Delta r^{Nonvoucher}_{1990-2004}) = 0$). Finally we drop eight geographies, with specific reasons listed for each geographic unit:

- Miami, FL, Honolulu, HI, Navarro County, TX, and Assumption Parish, LA – rebenchmarked in 2004
- Okanogan County, WA – Lobbied for higher FMR in 2005, no counterfactual available
- Louisiana – Hurricane Katrina severely disturbed rental markets

[37] One interesting question is why, after voucher holders find their first unit, they do not then move later on to units priced more closely to the rent ceiling.

- Kalawao County, HI – No FMR published before 2005

Measuring the First Stage: The administrative data report the rent ceiling $\bar{r}$ at the household level. We compute $\bar{r}_{jt}$ as the unconditional mean of all observations in a county-bed-year cell.

Trimming and Standard Errors: We winsorize county-by-bed FMR changes at the 1st and 99th percentile, so that our results will not be unduly influenced by outliers. While FMRs are published at the county-bed level, sometimes counties are grouped together for the purpose of setting a common FMR. Throughout our rebenchmarking analysis, we cluster our standard errors at the FMR group level (n=1,484).

B.3 Nonvoucher Rents and 2005 FMR Rebenchmarking

In Section 4.1, our key identification condition is

$$\eta \perp FMR_{2005}|FMR_{2004} = 0$$

Here we examine the correlation of the FMR change with contemporaneous changes in nonvoucher rents. Data availability make it difficult to measure nonvoucher rents at a high frequency and with a high degree of geographic specificity. Using the notation developed in Section 4.1,

$$Cov(\Delta\hat{r}_t, \Delta FMR) = Cov(r_t + \varphi_t - r_{2000} - \varphi_{2000}, \Delta FMR) = Var(\varphi_{2000}) < 0 \tag{15}$$

Even if $E(\Delta r_t|\Delta r_{t-1}) = 0$, we estimate a negative covariance because of the negative auto-correlation of gains measured with error. Similarly, Glaeser and Gyourko (2006) calculate serial correlation in housing price changes and rent changes at five-year horizons and find negative serial correlation.

First, we compare changes in voucher rents to changes in tract-level median rents published by the Census.[38] Data at the tract level are available from the 2000 Census (Minnesota Population Center (2011)) and the 2005-2009 American Community Survey with a consistent geographic identifier. In regression form, with i indexing tracts and j indexing counties, we estimate

$$r^{Nonvoucher}_{2005-2009,ij} - r^{Nonvoucher}_{2000,ij} = \alpha + \beta_1 \Delta FMR_j + \varepsilon_{ij}$$

where ΔFMR_j is the average FMR change across bedroom sizes. We find that rent changes from 2000 onward are negatively correlated with FMR changes ($\beta_1 < 0$), as reported in reported in Appendix Table 1, column 2. This is consistent with measurement error, as described in equation 15. This generates a sharp contrast – places with relative *increases* in voucher rents had relative *decreases* in nonvoucher rents. This mean reversion pattern is most pronounced in rural areas. When we limit the sample to counties with at least 100,000 residents, we find that β_1 is not statistically different from zero (column 4).[39]

Finally, we pool the observations in columns 1 and 2 to estimate $\Delta r^{\{Voucher,Nonvoucher\}}_{ij} = \alpha + \beta_1 \Delta FMR_j + \beta_2 \Delta FMR_j \times Voucher_{ij} + \varepsilon_{ij}$ where $Voucher_{ij}$ is an indicator for whether the rental change is observed for voucher stayers or nonvouchers. Then, we compute the probability that we would observe data like this or more extreme, under the null hypothesis that the two coefficients are equal ($\beta_1 = \beta_2$), and find $p < 0.01$. Likewise, we find that the probability $\beta_1 = \beta_2$ for in the urban sample is very low.

Another source of data on nonvoucher rents comes from the ACS public use microdata. These data are preferable because they more closely correspond to the time horizon of interest (data observed in 2000 and annually from 2005

[38] The Census estimates include voucher holders themselves, making this an imperfect measure of nonvoucher rent changes. Internal HUD data indicate that subsidized households typically report their rental payment (30% of income) in the Census, rather than the total rent received by the landlord. This measurement error means that rent reports by voucher holders are unlikely to change in response to changes in the FMR.

[39] This is consistent with plausible parameterizations of a tract-level data-generating process. Suppose that tract-level rents follow an auto-regressive process, with $Y_j = \rho Y_{j-1} + \eta_j$. A regression of *tract-level* rent changes from 2000 to 2005-2009 on *county-level* FMR changes, which are effectively rent changes from 1990 to 2000, of the form $\Delta Y^{tract}_j = \alpha + \beta \Delta Y^{county}_{j,t-1} + \varepsilon_j$ would yield a biased estimate $\hat{\beta} - \beta = -\frac{n_{tract}}{n_{county}}(1-\rho)\frac{Var(\eta)}{Var(\Delta Y_{j,t-1})}$. Analyzing tract-level rent changes indicates that $Var(\eta) \approx Var(\Delta Y_{j,t-1})$, $\rho = 0.88$. Tracts in counties with 40,000 units or more have small values of $\frac{n_{tract}}{n_{county}}$, such that $\hat{\beta} - \beta = -0.005$ and tracts in counties with less than 40,000 units have large $\frac{n_{tract}}{n_{county}}$, resulting in $\hat{\beta} - \beta = -0.070$.

to 2009) and because they identify the number of bedrooms the unit has, rather than just the location, allowing us to exploit the county-by-bed variation in FMR changes. However, since this is a public use file, geographic identifiers are available only for units located in counties which have more than 100,000 residents. We find a strong negative coefficient from 2000 to 2005 (column 5), consistent with measurement error at the bedroom level within counties. Analyzing the correlation of rent changes from 2005 to 2009 with FMR changes, which is perhaps our strongest test of $E(\Delta r^{Nonvoucher}_{2004-t}|\Delta FMR) = 0$, we find a coefficient of 0.02, very close to zero, although the estimate is imprecise. These estimates offer a joint test of two distinct hypotheses: (1) selection – contemporaneous neighborhood trends were correlated with FMR changes and (2) general equilibrium spillovers – FMR changes causally affected nonvoucher rents. The data are not consistent with these hypotheses.

B.4 Hedonic Quality

We build our hedonic quality measure using regression coefficients from a model of rents in the ACS along with building age, structure type, number of bedrooms and median tract rent. For our hedonic measures in the analyses of the re-benchmarking change and the Dallas ZIP-level ceiling change, we use administrative data from our PIC database and coefficients from a model of rents in the 2005-2009 public use sample of the American Community Survey, inflated to 2009 \$ (Ruggles et al. (2010)). The following unit covariates appear in both the Census and in PIC: Public Use Microdata Area (PUMA), number of bedrooms, structure type, and structure age. The PIC file reports an exact building age, which we code into the 10 bins for structure age available in the ACS. The PIC file reports 6 different structure categories and the ACS has 10 categories. We crosswalk these categories as best as we can, as

PIC	ACS 2005-2009
Single family detached	Single family detached
Semi-detached	1-family house, attached, 2-family building
Rowhouse/townhouse	3-4 family building
Low-rise	5-9 family building, 10-19 family building
High-rise	20-49 family building, 50+ family building
Mobile home or trailer	Mobile home or trailer

We have 710,957 observations of households with positive cash rent in the ACS. Unfortunately, we have no way to drop subsidized renters (13% of sample). This is an added source of measurement error. We estimate using least squares

$$Rent_{ijklm} = \alpha + Bed_j + StrucType_k + Age_l + PUMA_m + \varepsilon_i \tag{16}$$

where Bed_j is a set of indicators for 5 possible numbers of bedrooms, $StrucType_k$ is a set of indicators for 6 possible structure types, Age_l is a set of indicators for 10 possible structure age bins, and $PUMA_m$ is a set of indicators for 2,067 PUMAs. The results from this regression appear in Appendix Table 2. This regression computes a vector of hedonic coefficients $\hat{\beta}_{census}$. This hedonic regression has substantial predictive power, with an R-squared of 0.48. We then apply the coefficients from this hedonic regression to the voucher covariates for bedrooms, structure type and building age to construct a measure of hedonic unit quality $q^{hedonic} = \hat{\beta}_{census} x_{voucher} + r^{tract}_{voucher}$ where $r^{tract}_{voucher}$ is the median tract rent. The standard deviation of actual rent is \$497 and the standard deviation of predicted rent is \$331. For our Dallas analysis in Appendix Table 6, where we are interested in only structure quality and not neighborhood quality, we instead compute $q^{hedonic} = \hat{\beta}_{census} x_{voucher}$, omitting neighborhood quality. To evaluate whether these limited variables can approximate more detailed measures of unit quality, we compare the explanatory power of these same covariates in the American Housing Survey against a benchmark "kitchen-sink" regression of all hedonic characteristics in the AHS (60+ variables) in Appendix Table 4. The AHS hedonic regression using the subset of variables in the ACS approximates the full model fairly well with an R^2 of 0.30 compared to 0.42 with the full model.

To evaluate the effect of the 40th to 50th percentile FMR policy change on housing quality we construct a quality measure with building age, structure type, number of bedrooms and median tract rent plus 26 questions from HUD's Customer Satisfaction Survey (CSS) and hedonic coefficients from a model of rents in the 2011 American Housing Survey (AHS). We identify 26 quality measures which can be matched to variables in the AHS. These are:

- Building has working elevator
- Working cooktop/burners
- Unit lacks hot water
- Access to a laundry room
- Working outlets
- Unit has safe porch or balcony
- Working refrigerator
- Use oven to heat the unit
- Large open cracks
- Windows have broken glass
- Roof sagging, holes, or missing roofing
- Home has cockroaches
- Home has rodents
- Home cold for 24 hours or more
- Fuses blown or circuit breakers tripped regularly
- Heating break down for 6 hours or more
- Wiring metal coverings
- Water leaking inside
- Mildew, mold ,or water damage
- Smell bad odor such as sewer, natural gas
- Large peeling paint
- Toilet not working for 6 hours or more
- Unsafe handrails, steps or stairs
- Electrical outlets/switches have cover plates
- Rate unit good
- Rate unit poor

We estimate the contribution of unit characteristics to rent using equation 13 where vector s includes the 26 measures listed above along with the number of bedrooms, age of housing, structure type and is a set of indicators for the American Housing Survey "Zone" a coarser analog to ACS Public Use MicroData Areas (the coefficient on median Zone rents is approximately \$1). This regression produces a vector of coefficients $\hat{\gamma}$. We then construct our hedonic measure: $q_{css}^{hedonic} = \hat{\gamma}_{AHS} x_{css} + r_{voucher}^{tract}$. The CSS adds many more time-varying quality factors, and together with the basic ACS variables this model achieves about 75 percent of the predictive performance of the full "kitchen-sink" AHS model (Appendix Table 4). We believe that our actual hedonic measure, which uses tract rent rather than PUMA or Zone rents, likely explains much more of the actual variation in cross-sectional rents than the AHS R^2 numbers suggest. Impressively, our hedonic measures explain nearly 70 percent of the cross sectional variation in voucher rents in the CSS.

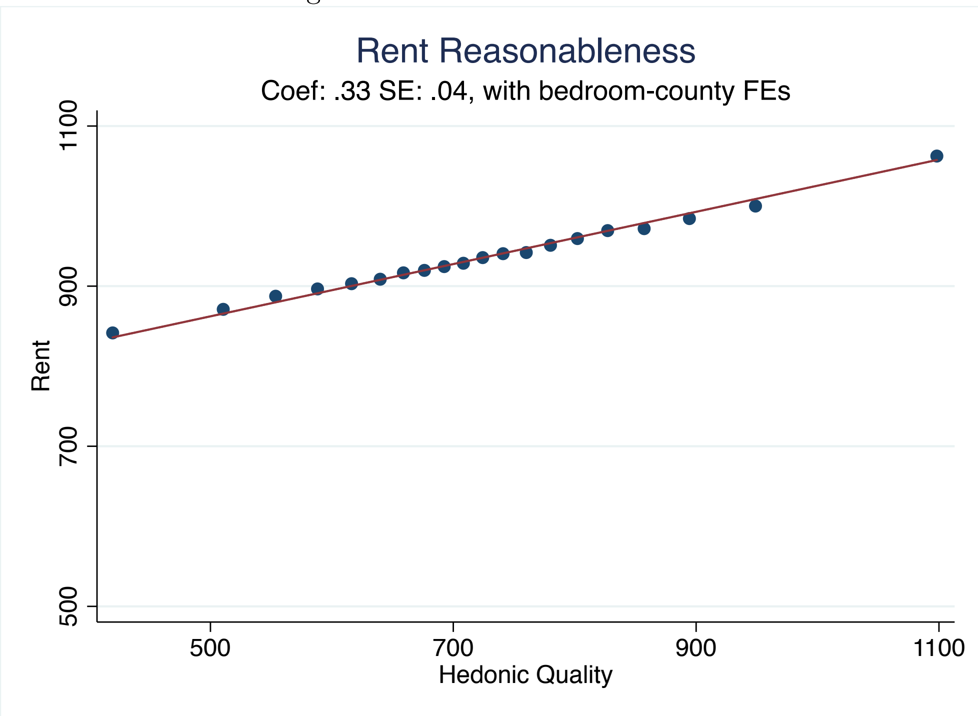
$$Rent_{ijklm} = \pi + s_i'\gamma + \varepsilon_i \tag{17}$$

B.5 Dallas ZIP-Level FMRs

Constructing the Analysis Sample: This Dallas "Small Area FMR Demonstration" applied to eight counties: Collin, Dallas, Delta, Denton, Ellis, Hunt, Kaufman, and Rockwall. Several housing authorities administer vouchers in these counties. Most adopted the new policy in December 2010, but the Dallas Housing Authority adopted the policy in March 2011. We use a balanced panel of all vouchers in these eight counties from 2010 to 2013 because beginning in 2009 the Dallas Housing Authority allocated many of its new vouchers to homeless individuals. These individuals also needed other non-housing services and are a very different population from standard voucher holders.

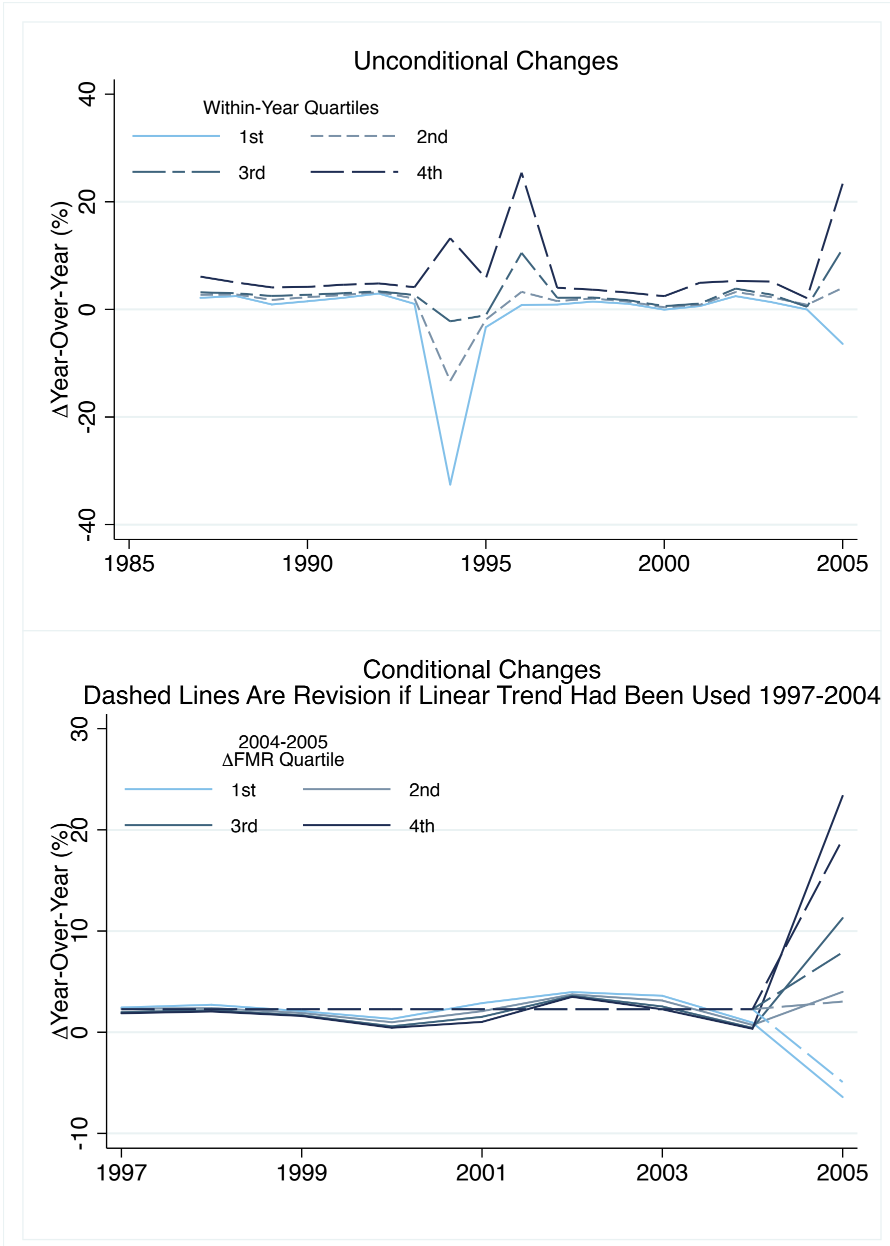
Constructing the Neighborhood Quality Measures: Tract-level data on poverty rate, unemployment rate, and share with a bachelor's degree are for 2006-2010 in the American Community Survey. Tract-level 2010 violent crime offense data was provided to HUD by the Dallas Police Department under a privacy certificate between HUD and Dallas (March 2012). For crime data outside the city of Dallas, crime is measured at the jurisdiction level using the FBI's Uniform Crime Reports from 2010. Data on the percent of 4th grade students' scoring proficient or higher on state exams in the 2008-2009 academic year was provided to HUD by the U.S. Department of Education. We map these scores to zoned schools at the block group level. "Single Mothers" is defined as share of own children under 18 living with a female householder and no husband present.

Figure B.1: Rent Reasonableness



This figure plots conditional means of unit rent for twenty quantiles of hedonic quality. The method for constructing hedonic quality is described in Section 4.1. We include fixed effects for the number of bedrooms interacted with the county, because each voucher holder's number of bedrooms is fixed by family size and it is usually quite difficult to switch counties. We find that a $1 increase in hedonic quality is associated with a 33 cent increase in rents. This indicates that even for a fixed rent ceiling, the government paid less for lower-quality units.

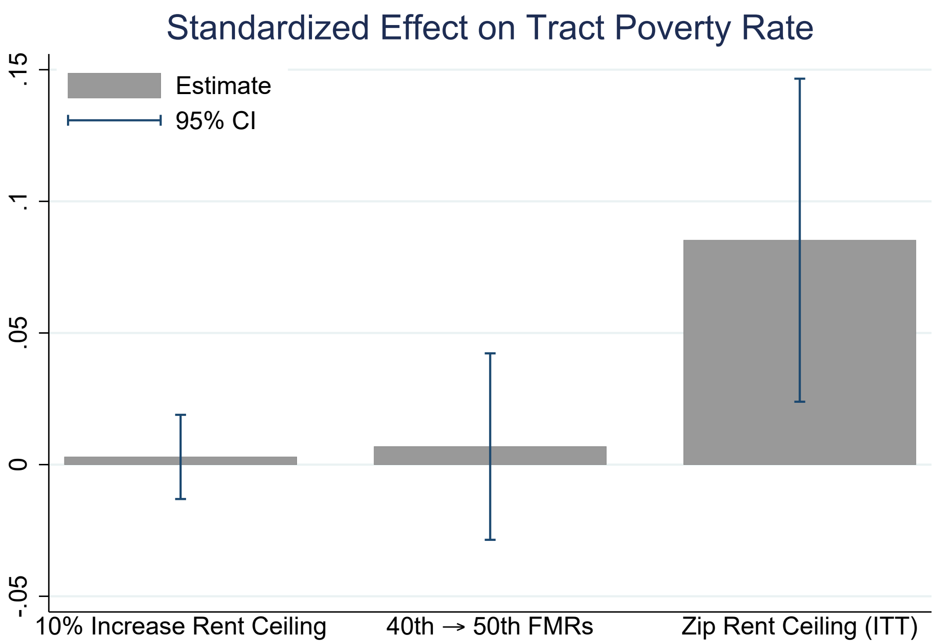
Figure B.2: County-Level FMR Changes



Notes: The top panel plots average Fair Market Rent (FMR) changes at the county-level within year-specific quartiles. The large swings in 1994-1996 and 2005 reflect decennial rebenchmarkings, when new Census data from 1990 and 2000 respectively were incorporated into the FMRs.

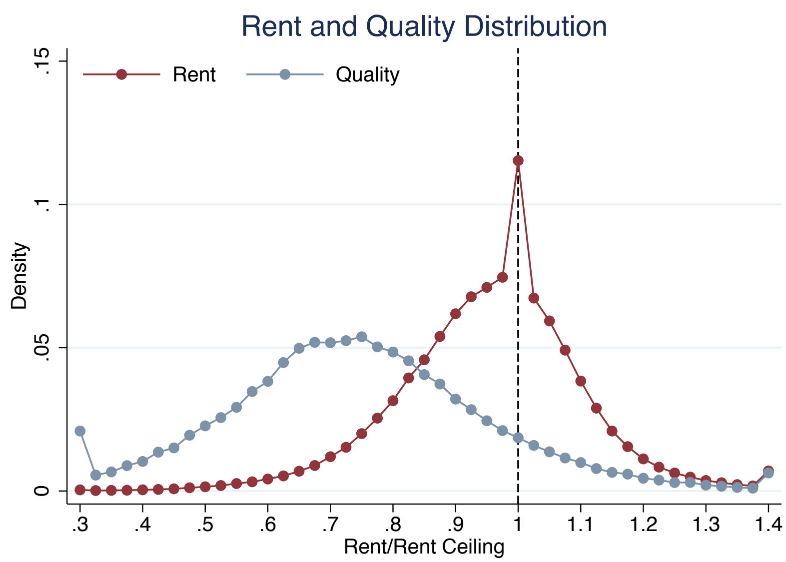
The bottom panel plots FMR changes for the same sample within quartiles defined over the 2004-2005 FMR change, as in Figure 3. The four groups exhibit similar trends in terms of changes prior to the rebenchmarking. There is some evidence of mean reversion: places which had higher revisions from 1997 to 2004 were revised downward in 2005. The dashed lines represent a counterfactual of what the magnitude of annual changes would have been if a single national index had been applied from 1997 through 2004, followed by an update which brought FMRs to observed 2005 levels. Observed revisions are more dispersed than the counterfactual revisions, indicating substantial measurement error in intercensal FMR changes.

Figure B.3: Policy Comparison – Impact on Neighborhood Poverty



Notes: This figure plots the standardized impact of three policies on census tract poverty rates of voucher holders: 1) a 10% increase in the rent ceiling using the 2005 re-benchmarking variation from Section 4.1, 2) the 40th →50th percentile FMR change from Section 4.2 3) Dallas ZIP Code-Level rent ceiling from Section 5. Positive standardized effects represent *reductions* in the tract poverty rate.

Figure B.4: Distribution of Rents



The bottom panel plots rents and hedonic quality relative to the local rent ceiling. Of rent observations, 0.03% are left censored and 0.62% are right censored. Of quality observations, 1.8% are left censored and 0.58% are right censored. We report gross rent (contract rent + utilities) to facilitate comparison with the rent ceiling, which is set in terms of gross rent. In the rest of the paper, we use contract rent alone, to focus on landlord behavior. Notes: 2009 data, n=1.7 million. Our methods for constructing hedonic quality are described in Section 4.1.