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Incidence and Price Discrimination: Evidence from Housing Vouchers

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The Incidence of Housing Voucher Generosity

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Abstract

Most housing voucher recipients live in low-quality neighborhoods. We study how changes in voucher generosity affect neighborhood poverty, unit-quality and rents using administrative data. We examine a policy making vouchers more generous across a metro area. This policy had no impact on neighborhood poverty, little impact on observed quality, and increased rents. A second policy, which indexed rent ceilings to neighborhood rents, led voucher recipients to move to higher quality neighborhoods with lower crime, poverty and unemployment. These results are consistent with a model where the first policy acts as an income effect and the second as a substitution effect.

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1 Introduction

Who benefits from a change in housing voucher generosity? If tenants use their more generous voucher to lease a unit in a better neighborhood or a higher quality unit, then the incidence falls on tenants. If, on the other hand, landlords are able to raise rents without improving the quality of their unit, then the incidence falls on landlords. In this paper, we empirically estimate the incidence of changes in voucher generosity using natural experiments and administrative data on the universe of housing vouchers. We find that a policy of across-the-board increases in the rent ceiling increases voucher rents, with little impact on observed quality, but that a policy which incentivizes moves using ZIP code-specific rent ceilings is a cost-effective way to increase neighborhood quality.

Housing Choice Vouchers, formerly known as Section 8, paid rent subsidies for 2.2 million low-income families in 2015. Voucher recipients 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. We show empirically that a voucher covers the cost of 68% of units in a low-quality neighborhood, but only 15% of units in a high-quality neighborhood. In principle, a voucher recipient could rent a unit of typical quality in a neighborhood where the median rent was at the 40th percentile of countywide rents or a unit of very high quality in a low-quality neighborhood.

In fact, housing voucher recipients seem to leave money on the table. They overwhelmingly live in low-quality neighborhoods and a majority lease units with rents below the rent ceiling. For example, voucher recipients in Dallas live on average in neighborhoods one standard deviation below the mean in terms of a neighborhood quality index (defined below). These neighborhoods offer limited economic opportunity (Chetty and Hendren, 2015). Moreover, several recent studies show that giving a family a housing voucher yields very little improvement in neighborhood quality as measured by poverty rates and crime rates.[2]

There are a few reasons why it may be hard for a voucher recipient to find a unit in a good neighborhood. First, audit studies have found that landlords discriminate, refusing to rent to people with a voucher (Lawyers Committee for Better Housing Inc (2002); Perry (2009)). Second, many voucher recipients have high transportation costs; participants with cars in the Moving to Opportunity experiment seemed to move to and stay in higher-quality neighborhoods in terms of crime and school quality(Pendall et al. (2014)). Third, while a voucher can theoretically be used at any rental unit which meets some minimum standards, in practice they often are steered towards a short list of units by public housing authority (PHA) recommendations (Abt Associates (2001)).

In an environment where it is hard to find a good unit, it is theoretically ambiguous whether landlords or tenants benefit from an increase in the generosity of housing vouchers. In a world

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

without frictions, more generous housing vouchers would benefit tenants through increased neighborhood quality and increased structure quality. However, housing is costly to search for, with prices that can be negotiated, and is indivisible. These features mean that an increase in housing voucher generosity may result in landlords raising rents without improving unit quality.

We develop a model to examine how the incidence of a change in housing voucher generosity depends on the ease with which voucher recipients can find units in good neighborhoods. The first policy lever we consider is an across-the-board increase in the rent ceiling across all neighborhoods. This acts like an income effect in a consumer demand model because voucher recipients can choose to allocate this increase to increasing the chance of finding a unit or to finding a unit in a better neighborhood. The second policy lever we consider is a "tilting" of the rent ceiling so that it is higher in high-quality neighborhoods and lower in low-quality neighborhoods, which acts like a substitution effect.

Using two natural experiments and a variety of quality measures, we show that across-the-board increases in the rent ceiling increase rental prices with a minimal impact on observed unit quality. In 2005, HUD revised county-level rent ceilings to correct for a decade of accumulated forecast error. We estimate that a \$1 increase in the rent ceiling caused aggregate rents to rise by 46 cents, while hedonic unit quality rose by only 5 cents over the next six years. Our hedonic model includes both neighborhood quality – as measured by median tract rent – and physical structure quality – as measured by structure age and structure type. These empirical results could reflect unmeasured quality increases or landlords price discriminating. One piece of evidence consistent with the price discrimination story is when we include address fixed effects in an attempt to hold unit quality constant, we still find that rents increase. Nevertheless, our quality measures in this research design are quite limited, which motivates an alternative research design.

Using a second research design with richer unit quality measures, we also find that prices respond more than observed quality to an across-the-board increase in the rent ceiling. We use a difference-in-difference strategy to examine a policy change in 2001 where HUD began setting rent ceilings on the basis of the 50th percentile of local rents rather than the 40th percentile. The advantage of this research design is that we can capture time-varying unit quality within an address using a 28-question HUD survey, with detail comparable to the American Housing Survey. We find that for each \$1 increase in the rent ceiling, rents paid on voucher units rose by 47 cents, with no significant impact on observed unit quality. This is consistent with *marginal* changes in voucher generosity benefiting landlords who are price discriminating or benefiting tenants through increased in unobserved unit quality; our results do not speak to whether on *average* landlords receive more from vouchers or private tenants for the same unit.[3]

[3]We have deliberately chosen to focus on marginal changes rather than average differences, because the latter involves more significant empirical hurdles. Both the costs and benefits of renting to a voucher recipient relative to a private tenant are difficult to quantify. From conversations with practitioners, we learned that some landlords perceive voucher recipients to be more costly than other tenants due to the risk of damage to the unit, while other

Unlike across-the-board increases in the rent ceiling, we find from a third natural experiment that tilting the rent ceiling toward higher-quality neighborhoods raises neighborhood quality. Housing authorities in Dallas, Texas switched from a single metro-wide ceiling to ZIP-code-level ceilings in 2011, giving voucher recipients a stronger incentive to move to higher-quality neighborhoods. 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. A difference-in-difference design using neighboring Fort Worth, Texas as a comparison group shows that new leases signed after the policy were in tracts where quality was 0.23 standard deviations higher. This is a substantial improvement, comparable in magnitude to other randomized voucher interventions for public housing residents (Kling et al. (2005); Jacob et al. (2013)) and larger than interventions for unsubsidized tenants (Jacob and Ludwig (2012)) or across-the-board increases in housing voucher generosity.

This policy appears to have been budget-neutral in Dallas. Absent any tenant behavioral response, this policy would have been cost-saving for the government, because rent increases in expensive ZIP codes were offset by larger decreases in low-cost ZIP codes and voucher recipients tend to live in inexpensive neighborhoods at baseline. Incorporating tenants' improved neighborhood choices, the Dallas intervention had zero net cost to the government.

In this paper, we show empirically that an across-the-board increase in rent ceilings fails to raise neighborhood quality, but that a tilting of rent ceilings is successful. In our model, these two policies correspond to income and substitution effects respectively. For many consumer goods, economists think that substitution effects are larger than income effects. In the case where housing voucher recipients worry about their ability to find a unit, our model can explain three empirical facts: (1) why an across-the-board increase does not raise neighborhood quality (2) why tilting the rent ceiling does raise neighborhood quality and (3) why voucher recipients live in low-quality neighborhoods to begin with.

Section 2 describes the model, Section 3 reviews the program and data, Section 4 studies changes in county and metro-wide rent ceilings, Section 5 studies the Dallas ZIP code-level demonstration, and Section 6 concludes.

2 Summary of Model

We build a model to understand why voucher recipients leave money on the table and what policies benefit voucher recipients versus landlords. This model is in Appendix A and here we provide a verbal summary. Our key assumption is that it is harder for a new voucher recipient to find a

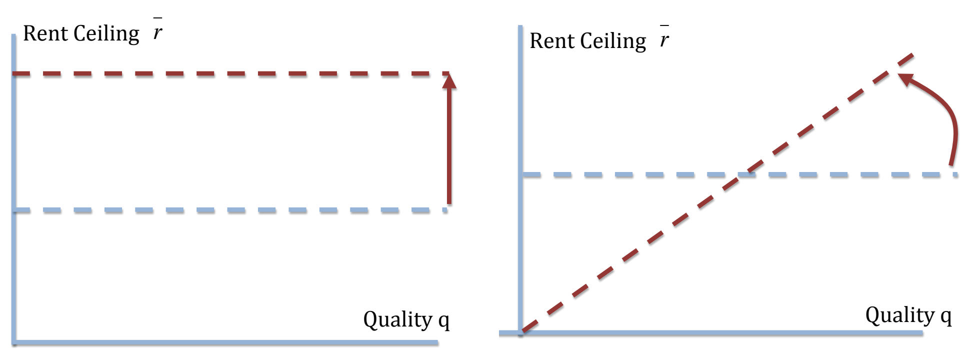
landlords prefer voucher recipients because the housing authority guarantees a steady stream of rental payments. See Table 6.7 in Olsen (2008) for a summary of older studies comparing differences in average costs and ORC/Macro (2001) for more recent evidence.

unit in a high-quality neighborhood than in a low-quality neighborhood, which is supported by several pieces of empirical evidence. First, because vouchers typically pay a flat amount across a metro area, a voucher can cover the cost 68% of units in the lowest-rent neighborhoods but only 15% of units in higher-rent neighborhoods, as shown empirically in Figure 1. Second, once a tenant is issued a voucher, she typically has three months to use it or lose it. These challenges are exacerbated for reasons unique to housing voucher recipients such as discrimination and high transportation costs. Given these constraints, it is not surprising that roughly one-in-three families issued a voucher are unable to lease-up in the allotted time (Abt Associates (2001)).

Voucher recipients face a trade-off between finding a unit at all and finding a unit in a high-quality neighborhood. In the model, a larger fraction of units in low-quality neighborhoods have rent below the ceiling than units in high-quality neighborhoods, which generates a compensating differential. Because of this trade-off, voucher recipients choose to look in lower-quality neighborhoods than they otherwise would. We use the model to examine two policy levers.

The first policy lever we consider is an increase in the rent ceiling across all neighborhoods. Voucher recipients can choose to allocate this increase to increasing their chance of finding a unit or to finding a unit in a better neighborhood. If raising the matching probability is an attractive "good" for voucher recipients to "buy" then increasing the rent ceiling will do little to improve quality. Formally, this policy is like an income effect in a Marshallian consumer demand model – only through second-order terms does it increase chosen neighborhood quality.The second policy lever we consider is a "tilting" of the rent ceiling so that it is higher in high-quality neighborhoods and lower in low-quality neighborhoods. This acts like a substitution effect in a consumer demand model. Unlike the metro-wide increase in the price ceiling, tilting the rent ceiling causes a first-order improvement in the voucher recipient's choice of optimal neighborhood quality.

Across-the-board $\bar{r}$ increase   Tilting $\bar{r}$



Increasing the rent ceiling also raises the rent paid for voucher units. Voucher recipients typically pay 30% of their income in rent, meaning that they are less price-sensitive than private tenants. Within each level of neighborhood quality, we assume that there is an exogenous distribution of markups. Because voucher recipients are not price-sensitive, they are more willing to accept markups and average rents rise when the price ceiling rises, even within a neighborhood.[4]

As far as we know, our emphasis on the challenge of finding a suitable unit 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)). This model predicts that housing voucher recipients should rent units with prices at least as high as the rent ceiling. In fact, 60 percent of housing voucher recipients rent units below the ceiling (Figure 1). Another explanation for why families with vouchers choose low-quality neighborhoods is preferences. For example, in Geyer (2011) and Galiani et al. (2015), voucher recipients have a preference for neighbors of the same race and also a preference for high-poverty neighborhoods.

Our model is better than a preference model at fitting our empirical findings for two reasons: neighborhood quality improves over time for voucher recipients and increases in across-the-board generosity have little impact on observed unit quality. First, the dynamic path of voucher recipients' neighborhood choices is consistent with it being hard to find a good unit upon initial lease-up rather than a preference for low-quality neighborhoods. Eriksen and Ross (2013) document that in the Welfare to Work Voucher experiment, voucher recipients 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 recipients worry about finding a unit to lease and only then worry about neighborhood quality.[5] A preference model with voucher recipients valuing structure over neighborhood quality predicts that voucher recipients in low-quality neighborhoods will live in high-quality units. However, as mentioned above, voucher recipients actually live in units with rents below the ceiling and as we document below, when there is an across-the-board increase in the rent ceiling, there is at most a modest improvement in observable structure quality.

3 Description of Housing Choice Vouchers and Data

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 Department of Housing and Urban Development (HUD), local housing authorities, private landlords and tenants.

[4] Rents may also rise if landlords *deliberately* raise rents in response to changes in the rent ceiling, but this is outside of our model. Any attempt to price discriminate will be limited to the extent that the rent reasonableness process described in Section 3 is effective.

[5] One interesting question is why voucher rents do not gradually asymptote to the rent ceiling as tenure rises. One possibility is that once a lease is signed in a bad neighborhood, inertia may lead some people not to move yet again to a better neighborhood.

Each year, HUD announces "Fair Market Rents" (FMRs) for every metro- and county-bedroom pair in the US. The geographic level at which FMRs are set is usually the metropolitan area in urban places and the county in rural places. HUD typically sets FMRs at the 40th percentile of area-level gross rent (rent to landlord plus utility costs). We defer a discussion of how FMRs are updated until Section 4 where we describe the natural experiments which we exploit in two research designs.

The local housing authority chooses a local rent ceiling $\bar{r}$ (or "Payment Standard") from 90%-110% of the federally-set FMR (U.S. Department of Housing and Urban Development (2001)).[6] Housing authorities are typically allocated a fixed budget for vouchers, and this budget does not vary with FMR changes (McCarty (2006)). When a housing authority increases its rent ceiling, it is able to finance fewer vouchers. Although an FMR increase *allows* housing authorities to increase the rent ceiling, housing authorities may use their discretion to smooth out FMR changes. Local housing authorities are also responsible for finding eligible tenants. Housing assistance is frequently oversubscribed, so housing authorities ration vouchers using preferences or lotteries to select tenants from a pool of very low income applicants (Collinson et al. (2015)).[7]

The tenant pays at least 30% of her income in rent and the housing authority pays the difference, up to the rent ceiling. For tenants renting units below the rent ceiling, when rents rise by $1, the housing authority pays an extra dollar and the tenant pays nothing. When tenants rent units with costs higher than the rent ceiling, they pay the difference out of pocket.[8] To the extent that tenants who pay the final dollar out-of-pocket behave like price-sensitive private tenants, our rent estimates will understate the extent to which landlords raise prices when housing vouchers become more generous.[9]

When a housing voucher recipient finds a suitable unit, she asks the housing authority to perform an inspection to check that the unit is up to code and to check for "rent reasonableness". The median housing authority rejects between one-quarter and one-half of units on the first inspection (Abt Associates (2001), Exhibit 3-5). Housing authorities have strong incentives to negotiate down rents, both because holding down per-unit rents enables them to serve more tenants and because they are reimbursed for administrative expenses on a per-unit basis. HUD routinely audits housing

[6] Housing authorities may request higher or lower "exception" payment standards from HUD. Exception payment standards below 120% of FMR may be approved by HUD Field Offices, exception requests above 120% FMR require approval from the Assistant Secretary for Public and Indian Housing.

[7] In a 2012 HUD survey, housing authorities reported more than 4.9 million households on waitlists for housing vouchers. Though this count likely includes some duplicate due to households appearing on multiple housing authorities' waitlists. (Collinson et al. (2015)). Among the 20 largest voucher-issuing housing authorities, 40 percent use a lottery-based system to select among eligible tenants.

[8] There is debate within HUD over how common it is for tenants to pay the final dollar of rent. Our tabulation of the micro-data shows that 40%of voucher recipients have rents greater than the rent ceiling. However, we suspect that these estimates are inflated by measurement error in rents and in rent ceilings in the administrative records.

[9] An earlier design of the housing voucher program, operated from 1983 until the early 2000s, eliminated this price insensitivity by offering tenants a fixed subsidy equal to the payment standard minus a fixed percentage of tenant's adjusted income (Olsen 2003). In this design, payment standards were constrained to be less than the applicable FMR.

authorities' leasing process, and rent reasonableness is consistently found to be one of the inspection categories with the highest compliance rates (ICF Macro (2009)). We conducted interviews with several experts to learn more about this process. One housing authority official described the following rent reasonableness process:

> [we] contract with Go-Section-8 [a web portal] to identify comparables. Go-Section-8 has over 20,000 listings in our area... We enter information on bedrooms, size and age, and Go-Section-8 provides the three closest listings with similar characteristics... We select the median of the three listings and use that as the rent we could offer.

When landlords request rents above comparables, the housing authority will begin a negotiating process where they exchange rent offers with the landlord. One housing authority we interviewed required that landlords asking for rents above their comparables furnish "three current leases for unsubsidized tenants" in the building as evidence that the asking rent is in line with market rent.[10]

We analyze housing vouchers using a partial equilibrium framework and changes in voucher generosity are unlikely to have much impact on general equilibrium rents. Vouchers account for only 6% of the U.S. rental housing market. If average voucher rents in a tract rose by 30% (a change larger than any we observe in this paper), the average user cost of housing in the tract would rise by only 1.8%.[11] We therefore find it unlikely that the policy variation we study had substantial impacts on nonvoucher rents. However, we note that other researchers using other variation have found general equilibrium impacts of the housing voucher program (Susin (2002); Eriksen and Ross (2015)), and so we conduct robustness checks which examine how non-voucher rents change with a change in FMRs in Appendix Table 3.

We use a HUD internal administrative database called PIC which contains an anonymous household identifier, an anonymous address identifier, building covariates, contract rent received by landlord, and landlord identifier, on an annual basis beginning in 2002. 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. Appendix B.1 discusses sample construction.

4 Income Effects: Impact of Raising the Base Rent Ceiling

We estimate the causal effect of across-the-board rent ceiling changes on housing quality (unit and neighborhood) and voucher rents using two natural experiments. In Section 4.1, we study a 2005

[10] Appendix Figure 2 shows empirically that rents are lower for units with lower hedonic quality.

[11] Of course, there is some heterogeneity in the concentration of vouchers, but even relatively concentrated voucher households are still a small share of the market. For example, for a voucher household at the 90th percentile of the voucher concentration distribution, 9% of all units in its tract are vouchers.

change in FMRs due to availability of updated 2000 Decennial Census data. We examine this change using rich data on the universe of housing vouchers, which includes the ability to track households and addresses over time. Unfortunately, this database only came into widespread use in 2003. The advantage of this research design is that the it uses variation across all counties giving us enough statistical power to detect even small quality and rent responses. In Section 4.2, we study a 2001 change which raised FMRs from the 40th percentile to the 50th percentile of rents in 39 metro areas. We use a detailed HUD survey, which was administered to voucher recipients on a widespread basis from 2000 to 2003 to evaluate the effects of this change on housing quality. The advantage of this research design is that the survey offers an in-depth look at unit quality, including quality attributes which might vary over time within the same unit. Across both research designs, we find similar results: raising the rent ceiling results in higher rents with little evidence of positive quality impacts.

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, which offers useful variation for a quasi-experimental analysis. In most years, FMRs are updated using local CPI rental measures for 26 large metro areas and 10 regional Random Digit Dialing (RDD) surveys for the rest of the country. These estimates are very coarse; for example, they were a bit *worse* at predicting local rent changes than using a single national trend from 1997 to 2004. 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.[12]

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 2 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.[13]

[12] See Appendix Figure 1 for a plot of changes in FMR by year as well as projected revisions under a counterfactual of a single national trend from 1997 to 2004.

[13] Throughout the paper, all regression specifications studying rent or hedonic quality use a log transformation.

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.[14] 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 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

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

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.[15]

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.

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

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

4.1.1 Impacts on Housing Quality and Voucher Rents

First, we assess the effects of across-the-board rent ceiling changes on the housing quality and rents of all voucher holders. Our unit of analysis is the county-bed, summary statistics for our sample appear in Table 1. We present three measures of quality: median tract rent, tract poverty rates, and a measure of hedonic housing quality.[16] To construct our hedonic quality measure, we run a hedonic regression in the American Community Survey using covariates for structure age, structure type (e.g. single-family, multi-family, or apartment building) and neighborhood rent. We then constructed our dependent variable quality measure $\Delta y_j = \hat{\beta}_{hedonic}(x_{t,j} - x_{2004,j})$ using covariates $x_{t,j}$ on structure type and median tract rent from the voucher data where $x_{t,j}$ is the unconditional average of x in county-bed j, including units that newly entered and exited the sample.[17] Census tracts typically have 4,000 residents and 77% of voucher moves cross tract boundaries, so this measure captures even very short-distance moves to higher-quality neighborhoods or higher-quality units within the same neighborhood. We construct our voucher rent measure in a similar fashion as $\Delta y_{t,j} = r^{voucher}_{t,j} - r^{voucher}_{2004,j}$.

We estimate our model 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:

$$\bar{r}_j = \alpha + \gamma FMR_{2005j} + FMR_{2004j} + \bar{r}_{2004j} + \varepsilon_j \tag{1}$$

where the exogenous variation comes from FMR in 2005, we control for FMR in 2004, the rent ceiling $\bar{r}$ in 2004, and ε is an error term.[18] Housing authorities use their discretion 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. It takes time for FMR changes to absorb into local policy, as shown in the bottom panel of Figure 2. We estimate our second stage:

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

Table 2 columns (1)-(3) show the effects of a \$1 chance in the rent ceiling on neighborhood and

[16] 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.

[17] 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. However, when predicting hedonic quality for voucher units, we use median tract rent (tracts have about 4,000 residents), which provides much more geographic detail than PUMAs. The results from our hedonic regression in the ACS appear in Appendix Table 1. More details on construction of the hedonic measure are provided in Appendix B.4.

[18] 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.

housing quality. There is virtually no impact of raising the ceiling on observable quality. A $1 increase in the ceiling has no detectable 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. In contrasts, average rents by 46 cents in response to a $1 increase in the rent ceiling (Table 2, column 4). Figure 3 plots the year-by-year coefficients of the reduced 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 hedonic quality rises minimally throughout this period. Either tenants saw big increases in unobserved unit quality or landlords saw increases in profits of roughly 40 cents for each $1 change in the rent ceiling.

4.1.2 Impacts on Same-Address Voucher Rents

How much do landlords benefit from a $1 rent ceiling increase? To explore this question further we examine the effect of rent ceiling increase on voucher rents at a given address. One empirical strategy uses people who stayed at the same address throughout the sample period ("stayers"). A complementary strategy uses data on voucher recipients who moved into a unit previously occupied by another voucher recipient ("movers"). If time-varying unit quality is constant, then landlords are capturing any increase in rents we observe. This could arise through deliberate price discrimination, or, as in the model, through price-insensitive voucher recipients not avoiding units whose markups were rising due to random variation. This could also be explained by within-unit changes in quality.

Table 3 column (2) shows the results – a $1 change in the rent ceiling corresponded to a 9 cent increase in rents for stayers from 2004 to 2010. This estimate is economically quite small and statistically precise, with a standard error of three cents. The magnitude of the point estimate suggests that the "rent reasonableness" policy discussed in Section 3 may be effective at regulating rent increases for incumbent tenants.

We also examine changes in rents for addresses which were occupied by different households before and after the rebenchmarking. We exploit the fact that about one-third of movers and new admits from 2005-2010 went to an address that was occupied by a different voucher recipient in 2003 or 2004. We calculate mean pre-2005 rent at every address (9 digit ZIP code-bedroom) and then merge this file with the addresses of voucher recipients in later years. Formally, we estimate equation 2 with $\Delta y_{hj} = r^{voucher}_{2010,hi'j} - r^{voucher}_{2004,hij}$ where i changes to i', to reflect a change in household, while address h is constant. For these movers, we find that a $1 increase in the rent ceiling caused rents to rise by 20 cents, as reported in Table 3 column (3). We believe that these estimates are slightly larger than the stayers estimates because of tenure discounts, where landlords are less likely to raise rents for a tenant renewing their lease.

We conduct several robustness checks to assess our result that landlords raise rents for tenants at the exact same address.[19] First, we add county fixed effects, so that identification comes only from within-county variation comparing the FMR change for 1-bedroom units to the FMR change for 4-bedroom units, and not at all from differences in secular trends across counties. Again, we find that a \$1 increase in rent ceiling raises rents for stayers. Second, recall that most tenants pay 30% of their income as rent, but some paid 30% of their income plus the difference between the unit's rent and the local rent ceiling. We build a sample of households which are very unlikely to be the residual payer in 2010 using baseline characteristics in 2004, and find a substantial increase in rents, combined with no change in tenant payments.[20] Third, we attempt to test for kickbacks. While it would be easy for a mom-and-pop operation to give kickbacks, it would be much more difficult for a large business with accountants and auditors to do so. We think that kickbacks from landlords to voucher recipients are unlikely to explain the results, because we find substantial rent increases among these larger landlords.

In this section, we used two empirical strategies to assess incidence with apparently disparate results – comparing total price increases to hedonic quality increases and controlling for quality with unit fixed effects – but this difference can likely be explained by some institutional details of the voucher program. In Section4.1.1, we showed increases in rents for all addresses of 46 cents with just 5 cents in quality improvements for voucher holders. In contrast, Section 4.1.2, our findings from the two address fixed effects specifications suggest rent increases of 9-20 cents for each dollar increase in the rent ceiling. One possibility is that when a unit is leased to a voucher recipient for the first time that a landlord can justify a wide range of rents in the "rent reasonableness" process, but that once it has been leased then a PHA staff member will reject a large increase in rent for a unit where rent reasonableness was previously established.

4.2 40th → 50th Percentile FMRs in 2001

A concern with the first research design is an inability to measure detailed elements of unit quality which might vary over time at the same address. In a different dataset, HUD measured 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 MSAs. 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.[21]

[19] Point estimates and standard errors are in Appendix Table 4.

[20] We plot tenant payments to landlords and housing authority payments to landlords against the FMR change from rebenchmarking in Appendix Figure 3. Tenant payments are unresponsive to changes in FMR, while payments from the government to landlords rise substantially.

[21] 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

From 2000 to 2003, HUD conducted a Customer Satisfaction Survey (CSS) of repeated cross-sections of about 100,000 voucher households. 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 US. 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 quality measure along with tract median rents from the 2000 Census. To compute hedonic quality, we identified the 26 questions on time-varying quality in the CSS which also appeared in the AHS.[22] We ran a hedonic regression in the AHS using these 26 questions, building age, and building type and a measure of median neighborhood rent then used tenants' responses in the CSS to predict hedonic quality.

We estimate the impacts of this policy change on Fair Market Rents, actual voucher rents and unit quality using a difference-in-difference model. Our estimation equations are

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

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

Our identification condition is the standard difference-in-difference condition: $E(\eta_{ijt}|\mathbf{1}(FMR = 50) \times Post) = 0$. Figure 4 shows the results visually and Table 4 Panel A 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) and composite hedonic quality rose by less than 5 cents (Table 4, panel A, column 3), with a standard error of 9 cents. The results from this analysis reinforce the conclusions from the prior section that increases in FMRs do not seem to improve quality. We also estimate the average effect of the policy (δ) in Table 4 panel B using:

$$y_{ijt} = \alpha + \delta \mathbf{1}(FMR = 50_j \times Post_t) + \mathbf{1}(FMR = 50)_j + Post_t + \eta_{ijt} \tag{5}$$

Moving from the 40th to 50th percentile FMR raises the rent ceiling by 11 percent with no measurable improvement in neighborhood quality, as measured by tract median rents and poverty

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 recipients reside in 5% of tracts with FMR area with largest number of participants

[22] Appendix Table 2 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).

rates, or in composite hedonic quality. We can reject improvements in the neighborhood poverty rates of voucher holders of more than a half a percentage point.

Our empirical results from two separate natural experiments which raised county and metro rent ceilings suggest that across-the-board changes in the ceiling act like an income effect doing little to improve either neighborhood or observed unit quality for voucher tenants while rents increase substantially.

5 Substitution Effects: 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 prices and quality. Following a court settlement, HUD replaced a single metro-wide FMR with ZIP code-level FMRs in early 2011. 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 5. In Section 5.1, we build a neighborhood quality index and document an improvement in quality of 0.23 standard deviations. In Section 5.2, we document that voucher rents and unit quality rose in ZIP codes where FMRs rose and fell in ZIP codes where FMRs fell. Finally, in Section 5.3, we establish that the effects on neighborhood quality are comparable to the results from more costly alternative interventions. . Appendix B.5 contains added supplementary empirical details.

5.1 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 recipients tend to live in lower-quality neighborhoods, often 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. Voucher recipients exited 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.

To formally estimate the impact of the change to ZIP code-level FMRs, we use a simple

[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.

difference-in-difference design with a comparison group of Fort Worth – a nearby city which continued to have a single metro-wide rent ceiling. The identifying assumption is that quality difference between Dallas voucher tenants and Fort Worth voucher tenants would have been stable absent the policy intervention. We estimate

$$Y_{it} = \alpha + \delta Dallas_i Post_t + Dallas_i + Post_t + \eta_{it} \tag{6}$$

where i indexes households and t indexes years. The results are shown in Table 5, where δ shows an intent-to-treat (ITT) improvement of 0.1 standard deviations in 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, 44% of continuing voucher recipients moved units, so the impact estimate for treatment-on-the-treated (TOT) is 0.23 standard deviations.[25]

Table 5 also provides impacts separately for each of the five 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 rate of children living with single mothers. We find medium-sized improvements of 0.19 SD in the poverty rate and 0.21 in the unemployment rate. In Appendix Figure 4 we contrast these improvements in poverty reduction with our findings from both across-the-board policy changes. The largest improvements are in the violent crime rate, which improves by 0.33 SD. If these relative improvements reflect voucher recipients' valuations, then it seems that voucher recipients 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 in Table 5 to the impact of the policy. Figure 6 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 7 shows that 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.

[25] 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 (non-Walker) vouchers in 2010 and took advantage of these counseling services by the end of 2012. Appendix Table 5 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 shrinks by about 20%.

5.2 Impacts on Voucher Rents and Building Quality

We examine the impacts of this policy change on building quality and voucher rents across all tenants, and separately for rents paid by stayers and for address with a voucher tenant change. The identifying assumption for this analysis is that the FMR change had no differential impact across zip codes on changes in nonvoucher rents from the base year (2010) to the most recent data available (2013):

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. With j indexing ZIP codes and $Post_t$ as a dummy for 2013, we estimate

$$\text{First Stage: } \bar{p}_{ijt} = \alpha + \gamma FMR_j Post_t + FMR_j + b_{ijt} + \varepsilon_{ijt} \tag{7}$$

$$\text{Second Stage: } y_{ijt} = \alpha + \beta \widehat{\bar{p}_{ijt}} + FMR_j + b_{ijt} + \eta_{ijt} \tag{8}$$

Rents at the ZIP code-level were highly responsive to the policy change, as shown in Figure 5. Table 6 reports results from equations 7 and 8. Changes in FMRs are a strong predictor of changes in rent ceiling, with coefficients around 60 cents. We find that for every dollar increase (decrease) in FMR, rents for stayers rose (fell) by 13 cents. Among addresses where the tenants changed, we find a much stronger effect of 56 cents. Evidently, rent reasonableness is enforced much more seriously in Dallas for lease renewals than for new leases, even when the new leases occur at addresses previously occupied by other voucher tenants. Finally, looking across all tenants who moved, we find substantial rent increases in more expensive areas and rent decreases in cheaper areas; every \$1 change in FMR was associated with a 62 cent change in rents. This could reflect changes in landlord pricing or unit quality.

Across Dallas average voucher rents were roughly constant (Table 5), but given the tendency of voucher recipients to live in low-quality neighborhoods, it is surprising that instituting ZIP code-level FMRs did not save money. Two statistical properties of the rent distribution in Dallas help to explain this. First, the share of renters is sharply declining in block group income, from 70% for the lowest-income neighborhoods to 10% for the highest-income neighborhoods. As a result, the median rent of all units in Dallas is substantially lower than the rent paid in a neighborhood of median quality. Second, the data suggest that there is a minimum cost to rental housing; median rents are the same in neighborhoods with a quality index of -4 and an index of -1. Finally, implementation costs were also minimal, at only about $10 per household.[26]

We also examine whether this change in the schedule led voucher recipients to move to higher-quality buildings. We predict physical structure quality by applying the hedonic coefficients to data in Dallas on number of bedrooms, structure type, and structure age (but not building location).[27] In 2010, voucher recipients 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 for movers changed by 19 cents, as reported in Table 6. This may understate the true effect on unit quality - our hedonic measure doesn't capture unobserved quality changes to units (reductions or improvements). This measure also does not incorporate the improvements in neighborhood quality detailed in 5.1.

5.3 Comparing Policies to Improve Neighborhood Quality

The impact on neighborhood poverty rates for voucher recipients of the Dallas policy is substantial in comparison with the across-the-board increases studied in Section4. 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.[28]

We compare the neighborhood quality impacts in Dallas to other randomized housing interventions in Table 7. Voucher recipients' access to areas with good schools 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 MTO experiment and voucher random assignment in Chicago (Jacob and Ludwig (2012), Jacob et al. (2013)). We consider 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 recipients were leaving distressed public housing with a high concentration of poverty.

[26] Implementation cost estimate comes from correspondence with Matthew Hogan of Dallas Housing Authority, October 23, 2012.

[27] See Appendix B.4 for details.

[28] The results are shown in a bar graph in Appendix Figure 4.

For each intervention, we construct a cost estimate and summary measure of the change in opportunity for a child affected by the policy. Chetty et al. (2014) document heterogeneity in intergenerational mobility across US commuting zones. Chetty and Hendren (2015) estimate that two-thirds of the cross-sectional variation is causal. We regress the predicted income rank of child whose parents are at the 10th percentile of the income distribution on local violent crime and poverty rates.[29] To predict the causal impact of voucher interventions on children's outcomes, we assume: (1) the child lived in the new location from birth to age 18 and (2) the cross-Commuting-Zone coefficients are accurate for the causal impacts of tract-level variation in neighborhood quality. The Chetty et al. (2014) results, combined with our assumptions, suggest that their children's income rank at around age 30 would rise by 4.3 percentage points, so from the 39th percentile to the 43rd percentile. This improvement for Dallas is smaller than the predicted improvement for the MTO Experimental group (20 percentage points), but similar in magnitude to offering vouchers to public housing residents, and larger than offering vouchers to unassisted tenants.[30] Offering vouchers, however, is very costly to unassisted renters, and more expensive than maintaining the existing public housing stock (Abt Associates (2010)). The Dallas ZIP-level FMRs, in contrast, appear to thus far have had no net cost to the government.

The neighborhood quality improvements here stand in sharp contrast to the county-level rent ceiling results in Section 4. However, our model offers a straightforward reconciliation. Across-the-board rent ceiling increases operate like an income effect, with a minimal impact on quality. Tilting the rent ceiling, however, operates like a substitution effect and tenants substitute to higher quality.

6 Conclusion

We examine how changes in housing voucher generosity affect voucher rents and unit quality. Across all units, a $1 increase in the rent ceiling raises rents by 46 cents; consistent with this policy change acting like an income effect, we find very small observed quality increases of around 5 cents. A tilting of the rent ceiling, which is equivalent to a substitution effect, increases neighborhood quality substantially. The latter policy, without any net cost to the government, appears to have raised a neighborhood quality index by 0.23 standard deviations.

[29] To be precise, across commuting zones j we regress $E(rank|parentRank_j = 0) + 0.1 * E(drank/dparentRank_j) = \alpha + \beta Crime_j + \delta Poverty_j$ and then predict the impact of an intervention as $\Delta Rank = \frac{2}{3}(-21.8 \times \Delta Crime - 0.231 \times \Delta Poverty)$ where the crime rate is measured as violent crimes per 10,000 residents and poverty rate is the fraction of residents with incomes below the federal poverty line.

[30] This 20 percentage point prediction is if the policy moved children at birth and they stayed in the same neighborhood until age 18. 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.

A simple model built around an assumption that it is more difficult to find a unit in a high-quality neighborhood can explain our empirical findings, as well as why voucher recipients tend to live in low-quality neighborhoods. Although the tilting of the rent ceiling is highly cost-effective and voucher recipients 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 recipients.

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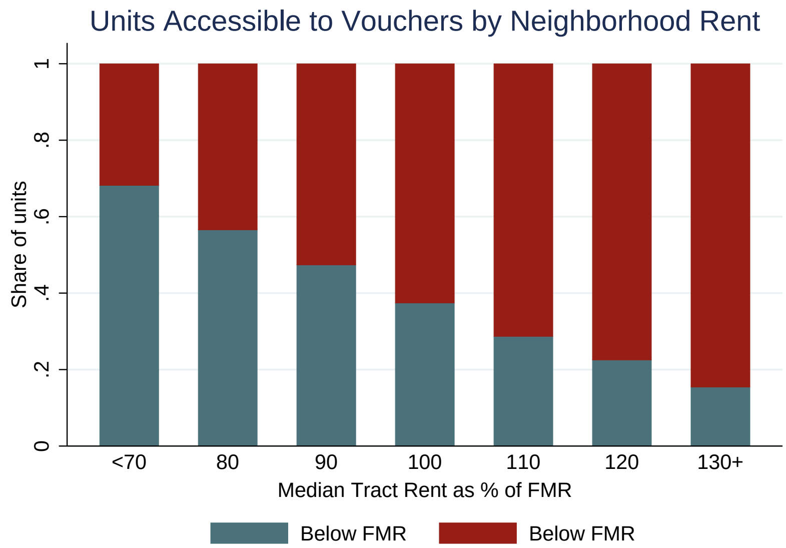
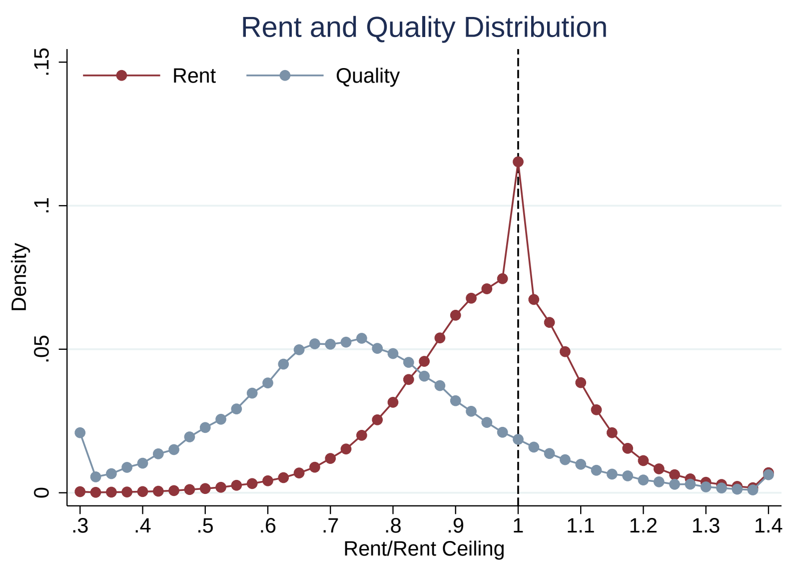
FIGURE 1 – Unit Availability and Rent Distribution

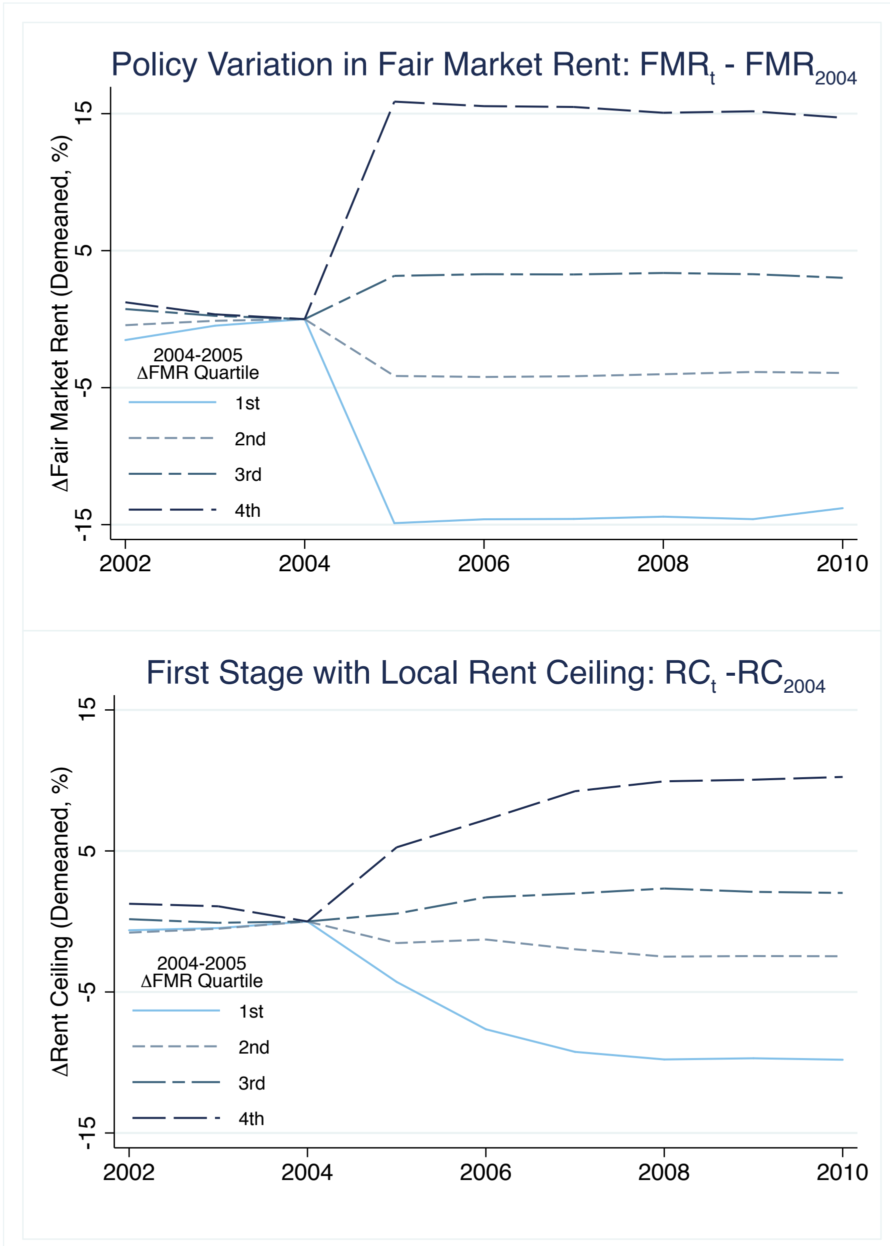




Notes: Each year, the federal government publishes "Fair Market Rents." These are typically estimated as the 40th percentile of rent in a county for studios, 1 bedroom, 2 bedroom, 3 bedroom and 4 bedroom units. The top panel reports the census tract share of standard rental units with rents below the 40th percentile rent metro area rent by the ratio of the census tract rent to the metro area rent. Data is drawn from a special tabulation of the 2009-2013 ACS five-year estimate and FY2013 fair market rents.

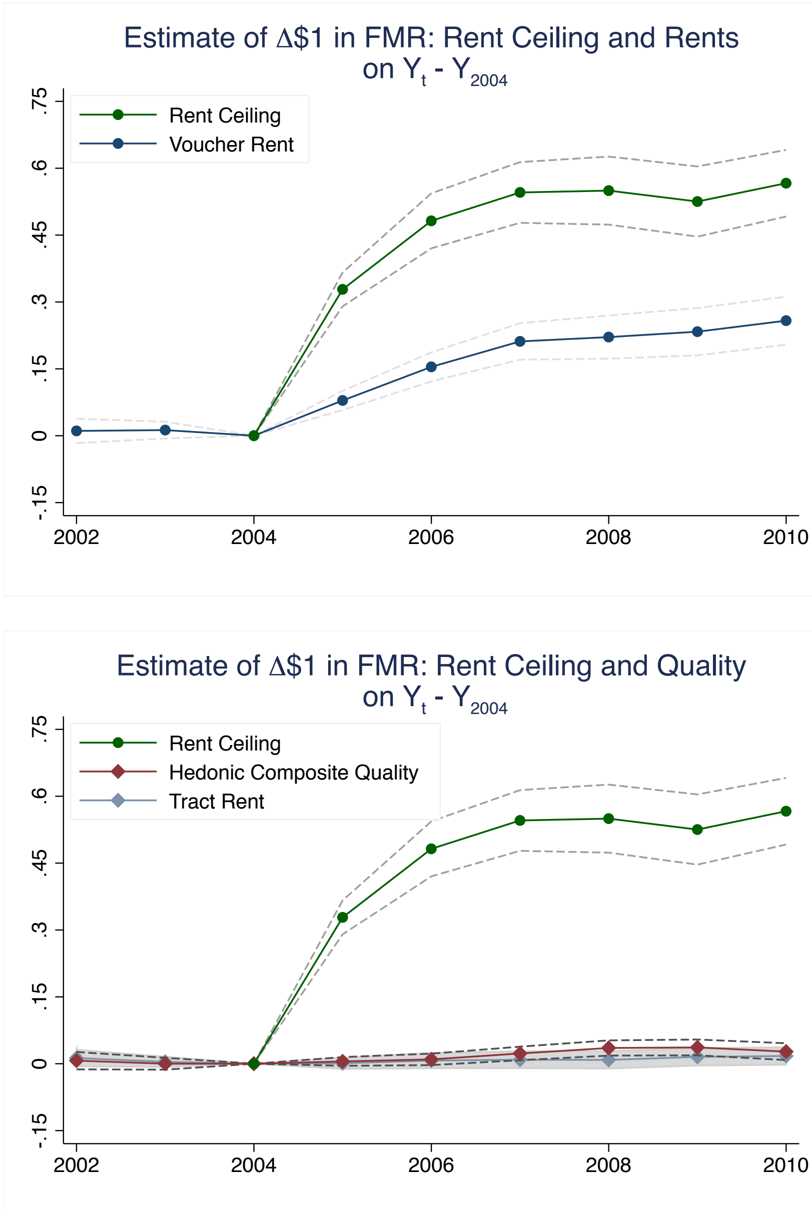
The bottom panel plots rents and hedonic quality relative to the local rent ceiling. Of rent observations, 0.03% are left censored and 0.62% are right censored. Of quality observations, 1.8% are left censored and 0.58% are right censored. We report gross rent (contract rent + utilities) to facilitate 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 Appendix B.4.

FIGURE 2 – Event Study for Rebenchmarking



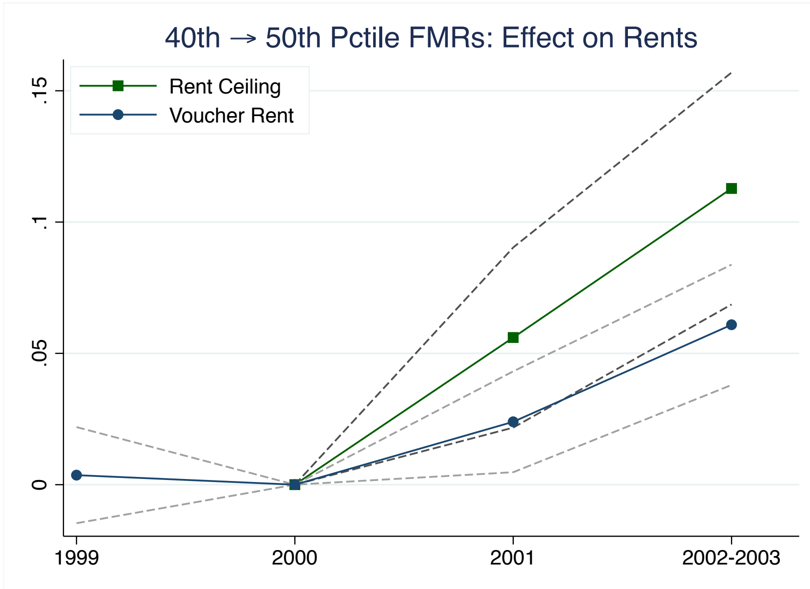
Notes: In 2005, the government made large revisions as part of a "rebenchmarking" to incorporate newly-available data from the 2000 Census. The top panel plots demeaned changes in the Fair Market Rent for four quartiles of county-bed observations, stratified by the change from 2004 to 2005. Local housing 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. By 2010, for every $1 increase in the Fair Market Rent, local rent ceilings rose by 70 cents.

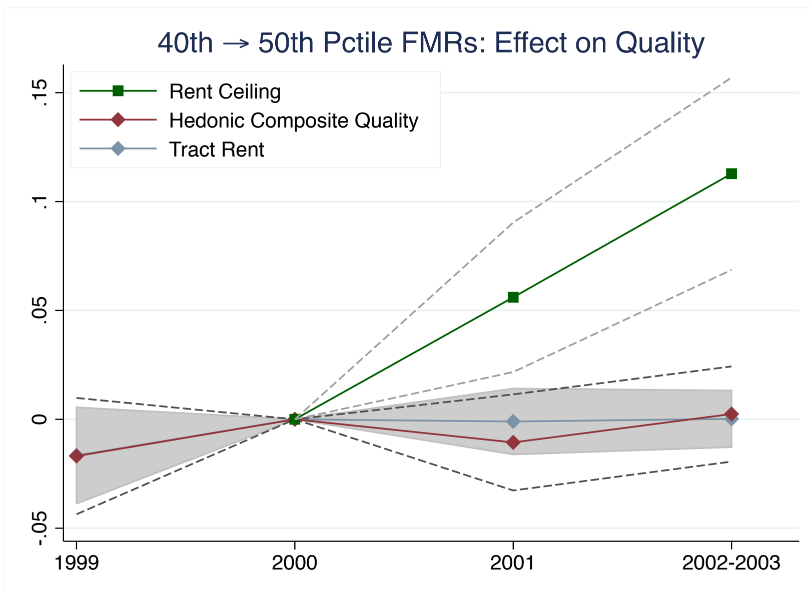
FIGURE 3 –Impacts of Rebenchmarking: Rents and Quality



Notes: The top panel plots β coefficients using variation from the 2005 rebenchmarking . The rent ceiling series plots the β coefficients from the following regression: $\bar{r}_t = \alpha + \beta FMR_{2005} + FMR_{2004} + \bar{r}_{2004}$. We plot a reduced form regression for rents and quality using the following equation $\Delta y_{t,j} = \alpha + \beta FMR_{2005,j} + FMR_{2004,j} + \bar{r}_{2004,j} + \varepsilon_j$ to facilitate comparison between the rent ceiling and rents/quality response to a $1 increase in FMR.. 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 4 – Impacts of 40th→50th Percentile FMRs: Rents and Quality

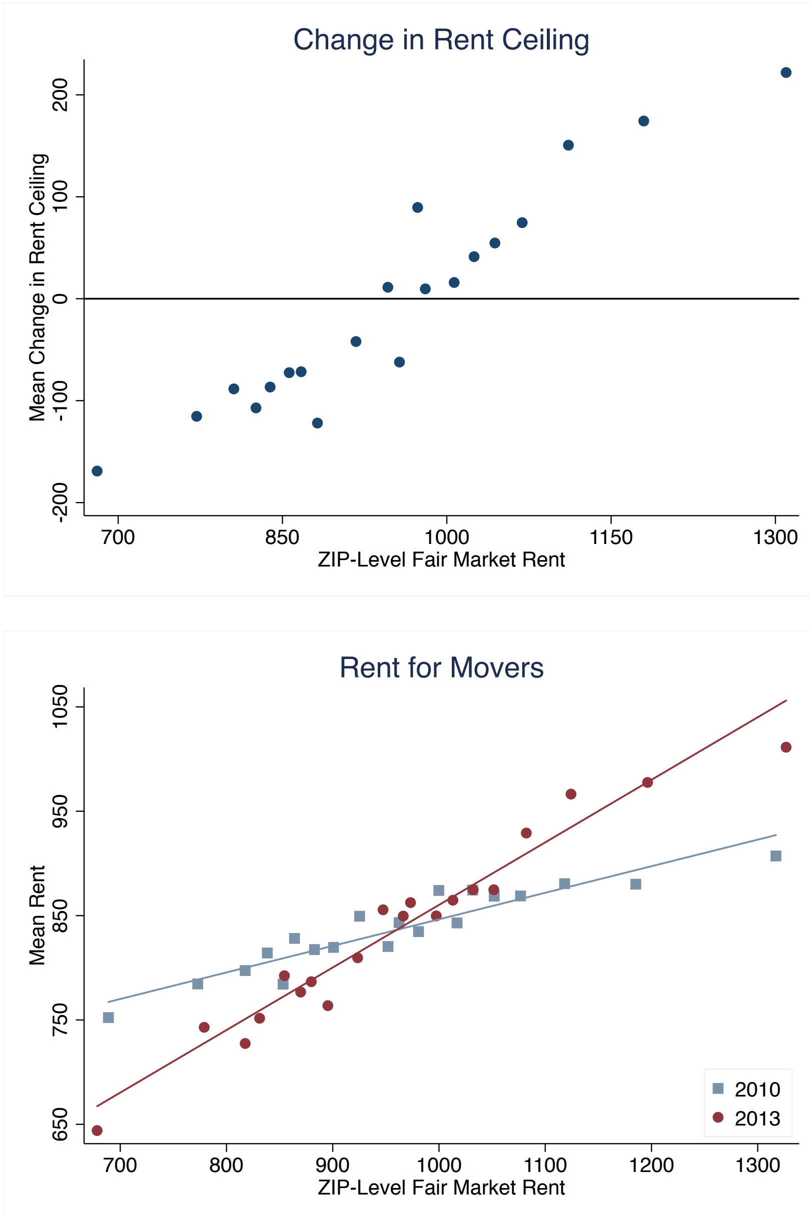




Notes: The top panel shows an event study for changes in rent and quality around the introduction of 50th percentile FMRs in 2001. Hedonic quality is measure using number of bedrooms, structure type, structure age, median tract rent, and 26 survey questions about unit quality and maintenance.

Shaded area / dashed lines indicate 95% confidence intervals.The bottom panel plots the same event study for changes in census tract poverty rates of voucher holders around the introduction of 50th percentile FMRs in 2001. Shaded area / dashed lines indicate 95% confidence intervals. See notes to Table 4 for details.

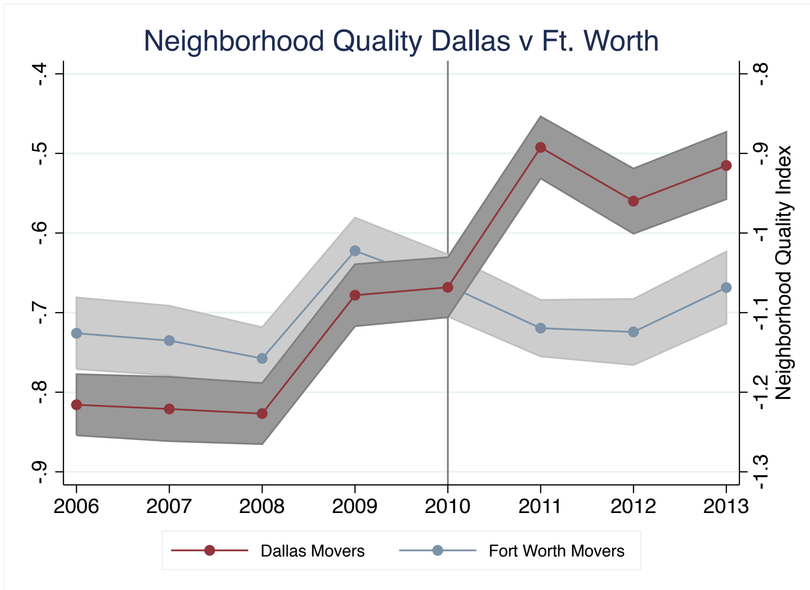
FIGURE 5 – Impact of Dallas "Tilting" on Rent Ceiling and Rents



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. Dots reflect means for 20 quantiles of the ZIP code-level FMR distribution conditional on bedroom-year. We show data only for households which moved from 2010 to 2013.

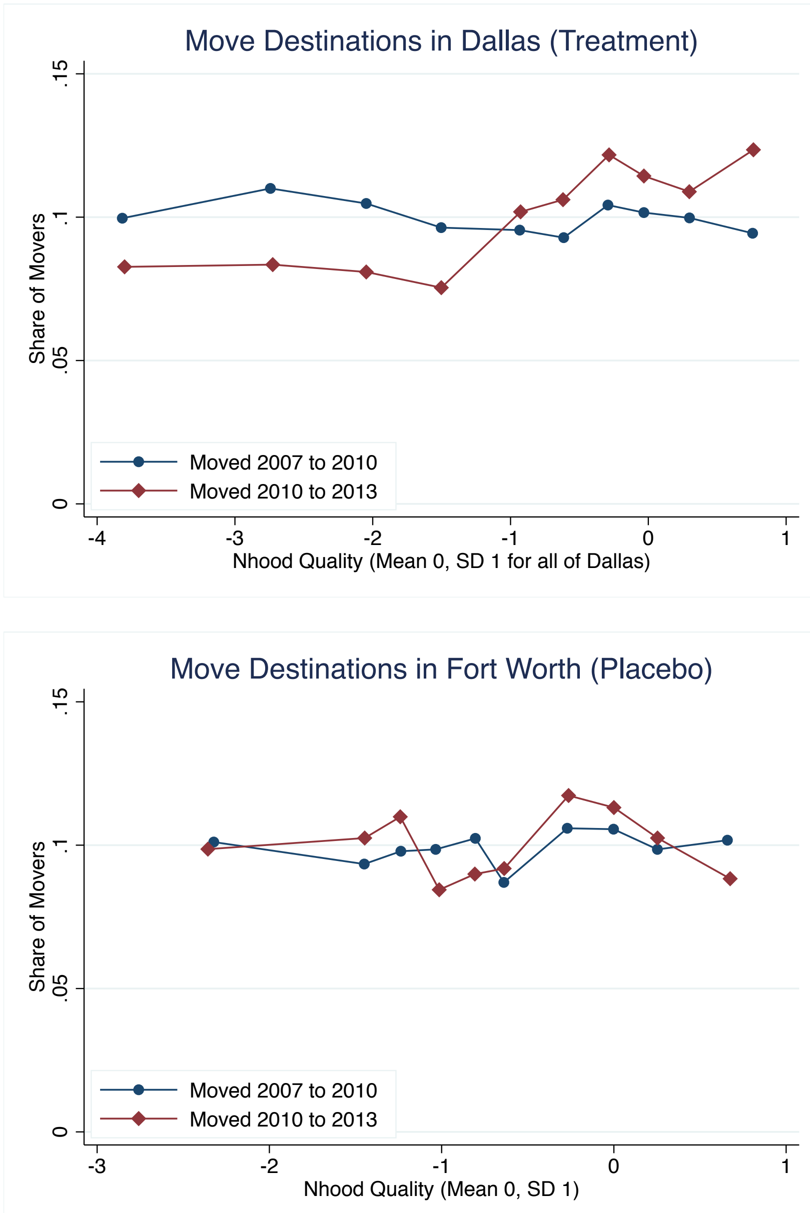
This bottom panel plots mean rents against the zip-code level FMR for movers from 2010-2013 at their 2010 and 2013 zip codes. Dots reflect means for 20 quantiles of the ZIP code-level FMR distribution conditional on bedroom-year in 2010 and in 2013. Rents were quite responsive to the new rent ceiling schedule.

FIGURE 6 – Impacts of Dallas "Tilting" on 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 7 – Impacts of Dallas "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

Finding an apartment is hard, especially for voucher recipients. We build a partial equilibrium directed search model with price posting to analyze the incidence of changes in voucher generosity.People issued a voucher choose a quality submarket in which to search for housing. Only some voucher recipients are able to find units because of search frictions. Higher quality units are more attractive, but it is harder to find a unit in a higher-quality submarket, generating a compensating differential (Rosen (1986)). We develop two propositions which examine how rent and quality change in response to an increase in the rent ceiling as well as a tilting of the rent ceiling with respect to neighborhood quality.

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 $q \geq q_{min}$.[31]A subset of renters, too small to have any general equilibrium impact on rents, is offered a voucher.

Landlords There is a unit mass of landlords in each neighborhood q who each choose rent markups (or discounts) $m \sim F$ with $m \in [m_{low}, m_{high}]$. Assume that F is twice-differentiable with $\frac{df(m)}{dm} < 0$, so that $\frac{f(.)}{F(.)}$ exhibits the monotone likelihood ratio property. Heterogeneity in m can be thought of as arising from differences in landlord's outside options. When occupied, a landlord receives rent equal to the markup plus the base quality index $m + q$, and when vacant, a landlord receives no rent.

Private Tenants Because this analysis is primarily focused on vouchers, we do not model private tenants' choice of neighborhood. They are randomly matched to units in neighborhood q and have a dollar-denominated willingness to pay markups of $\eta \sim G$, again arising from differences in outside options.

Voucher Recipients People who accept a voucher are not price sensitive so they will rent any unit which costs less than the rent ceiling. Voucher recipients consume one unit of housing. Voucher recipients choose a quality level q to maximize utility, subject to the constraint imposed by the rent ceiling $\bar{r}$ in conjunction with landlord markups. Landlords have the policy:

$$\text{Accept voucher if } q + m < \bar{r}(q)$$

so the fraction of landlords in neighborhood q who will accept a voucher is $F(\bar{r}(q) - q)$. Recipients solve:

$$\max_q U(P(q), q) \text{ subject to } P(q) = F(\bar{r}(q) - q)$$

Recipients maximize expected utility. 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, which occurs with probability $P(q)$. Finally, assume that the rent ceiling has a linear structure $\bar{r} = r_{base} + cq$

[31]We define q as a neighborhood because definition best matches our empirical work for the natual experiment in Dallas. However, it is possible to also think of q as a summary measure of many different inputs to quality such as neighborhood, building type, and unit size, so long as the landlord cannot change the quality of her unit. This alternative definition of q generates an additional empirical prediction which is that across-the-board increases in voucher generosity may not have much impact on unit quality in the presence of search frictions.

with $c \in [0,1)$. The tenant's problem can be rewritten as

$$\max_q \underbrace{F(r_{base} + cq - q)}_{\text{Match Probability}} \quad \underbrace{V(q)}_{\text{Utility if Matched}}$$

A.2 Solution

Voucher Tenants' Quality Choices We solve the voucher recipient's problem using the first order condition:

$$(1-c) = \frac{U_q}{U_P} = \frac{F(r_{base} + cq - q)}{f(r_{base} + cq - q)} \frac{V'(q)}{V(q)} \tag{9}$$

The solution $q = q^*$ is unique.[32]

Markups Private tenants observe markup m and rent the unit if it is better than their outside option (i.e. the rent is lower than their willingness to pay): $\eta - m > 0$. The share of the private tenant population that will accept an offer of m is $G(m)$. Average transacted prices are

$$\mu_{private} = \int_{m_{low}}^{m_{high}} mG(m)f(m)dm / \left(\int_{m_{low}}^{m_{high}} G(m)f(m)dm \right) + q$$

Finally, we compute rents paid on behalf of voucher units in q. Voucher tenants will accept any unit offered to them with rent less than $\bar{r} - q$, so:

$$\mu_{voucher} = \int_{m_{low}}^{\bar{r}-q} mf(m)dm / \left(\int_{m_{low}}^{\bar{r}-q} f(m)dm \right) + q \tag{10}$$

The average difference in rents between voucher and private units in neighborhood q is

$$\Delta(q) = \frac{\int_{m_{low}}^{\bar{r}-q} mf(m)dm}{\int_{m_{low}}^{\bar{r}-q} f(m)dm} - \frac{\int_{m_{low}}^{m_{high}} mG(m)f(m)dm}{\int_{m_{low}}^{m_{high}} G(m)f(m)dm}$$

Intuitively, the gap in average rents is larger when private tenants are more price sensitive ($g(m)$ falls rapidly in m) and when the rent ceiling is higher.[33]

A.3 Comparative Statics

Proposition 1 *Within a neighborhood q, the average voucher rents rise when the rent ceiling rises.*

$$\frac{\partial \mu_{voucher}}{\partial \bar{r}} = [\bar{r} - \mu_{voucher}] \frac{f(\bar{r}-q)}{F(\bar{r}-q)}$$

[32] This follows from the negative second-order condition in the maximand $U_{qq} = (-1+c)^2 \frac{df(.)}{dq} V(q) + 2f(.)V'(q)(-1+c) + F(.)V''(q) < 0 \forall q$. The first term is negative because $\frac{df(.)}{dq}$ is negative by assumption, the second term is negative because $c < 1$ and the third term is negative because $V'' < 0$ by assumption.

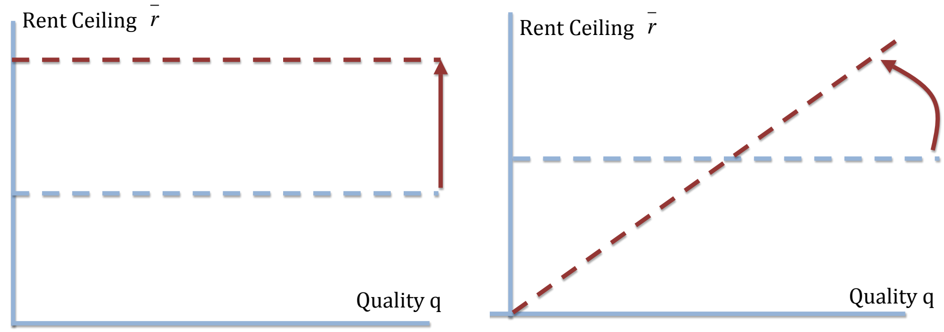
[33] Our model also implies that holding quality fixed, the average rent paid by a voucher recipient may be higher than the average rent paid by a private tenant, but we do not examine this empirically. See Table 6.7 in Olsen (2003) for a summary of older studies comparing differences in average costs and ORC/Macro (2001) for more recent evidence. From conversations with practitioners, we learned that some landlords perceive voucher recipients to be more costly than other tenants due to the risk of damage to the unit, while other landlords prefer voucher recipients because the housing authority guarantees a steady stream of rental payments. Both the costs and benefits of renting to a voucher recipient relative to a private tenant are difficult to quantify. For this reason, we focus instead on policy changes to the rent ceiling, rather than differences in average costs.

Proof: Differentiate equation 10 with respect to $\bar{r}$.

The size of the change in average voucher rents depends on how many landlords in q are on the margin, with markups equal to $\bar{r} - q$. This comparative static will understate the extent to which rents rise if landlords *deliberately* raise rents in response to changes in the rent ceiling. Any attempt to price discriminate will be limited to the extent that the rent reasonableness process described in Section 3 is effective.

Next, we analyze the impact on quality of raising r_{base} versus the impact of raising c (with a compensating change in r_{base}), which can be depicted visually as:

Across-the-board $\bar{r}$ increase   Tilting $\bar{r}$



Inside the model, these comparative statics correspond to an income effect and a substitution effect.

$$\begin{array}{lccc} & & \text{First-Order} & \text{Second-Order} \\ \text{Income Effect } \frac{\partial q^*}{\partial r_{base}} & \propto & & -(1-c)\underbrace{\frac{\partial f(.)}{\partial r_{base}} V(.)}_{U_{PP}} + \underbrace{f(.)V'(.)}_{U_{Pq}} \\ \text{Substitution Effect } \frac{\partial q^*}{\partial c} & \propto & \underbrace{f(.)V(.)}_{U_P} & -(1-c)\underbrace{\frac{\partial f(.)}{\partial r_{base}} V(.)}_{U_{PP}} q^* + \underbrace{f(.)V'(.)}_{U_{Pq}} q^* \end{array}$$

Proposition 2 *Raising the rent ceiling in a search model affects quality chosen in the same way that an income effect does in a consumer demand model. Tilting the rent ceiling in a search model affects quality chosen in the same way as a substitution effect.*

Proof: Differentiate equation 9 with respect to r_{base} and c.[34]

Across-the-board increases are like an income effect in that voucher recipients may use the funds for moves to a better neighborhood or improved matching probability in the previously-chosen neighborhood. Raising the base rent ceiling raises quality, but only through *second-order* terms

[34] To see the exact analogy with for a model with labor and leisure, assume agent has utility $U(c, \ell)$ where c is consumption and ℓ is leisure. Assume $c = W(T - \ell) + Y$ where W is the wage, time spent working is $T - \ell$ and Y captures unearned income. This model has first-order condition of $-U_c(W(T - \ell^*(Z)) + Y, \ell^*(Z))W + U_\ell(W(T - \ell^*(Z)) + Y, \ell^*(Z)) = 0$ where Z captures exogenous parameters Y and W. Differentiation gives

$$\begin{array}{lccc} & & \text{First-Order} & \text{Second-Order} \\ \text{Income Effect } \frac{\partial \ell^*}{\partial Y} & \propto & & +WU_{cc} + U_{c\ell} \\ \text{Substitution Effect } \frac{\partial \ell^*}{\partial W} & \propto & -U_c & +[WU_{cc} + U_{\ell c}][T - \ell^*] \end{array}$$

This is formally isomorphic to the model above with $T - \ell = q$, $c = P$ and $W = -(1 - c)$.

U_{PP} and U_{Pq}. Just as in a consumer demand problem where expanding a household's budget set will raise their consumption through diminishing marginal utility of each good, quality here increases only through diminishing marginal utility of matching probability and the complementarity between matching probability and unit quality. In contrast, raising the subsidy for high-quality units also works through a *first-order* effect U_P, whereby the penalty for moving to a higher-quality unit, which takes the form of a lower matching probability, is diminished. This suggests that tilting the rent schedule may be more effective at improving quality than raising the base rent ceiling.

A.4 Robustness

Two of the the simplifying assumptions in the baseline model are the use of a representative agent and focusing on voucher units below the rent ceiling. Here, we show how the model changes when we relax these assumptions. Our key conclusions remain unchanged. For simplicity, we focus on the case where there is one constant rent ceiling $\bar{r}$across a metro area, rather than letting the rent ceiling vary with quality q.

A.4.1 Heterogeneity in Outside Options

Our baseline model examines a representative agent, while in fact voucher recipients choose a wide variety of neighborhoods. Adding heterogeneity in a voucher recipient's outside option generates heterogeneity in neighborhood choices. Voucher recipients with better outside options will search in better neighborhoods and their neighborhood choice will be more responsive to changes in the rent ceiling. Formally, let i index different individuals, and let $\underline{q}_i$ be individual i's outside option. All individuals have utility over unit quality $u(q)$, with $u' > 0, u'' < 0$. Now the tenant's maximization problem and first order condition become:

$$\begin{aligned} V_i &= \max_q F(\bar{r}-q)V(q) + (1-F(\bar{r}-q))V(\underline{q}_i) \\ \text{First Order Condition} \quad & \quad -f(\bar{r}-q_i^*)\left(V(q_i^*) - V(\underline{q}_i)\right) + F(\bar{r}-q_i^*)V'(q_i^*) = 0 \end{aligned}$$

Under the regularity condition already specified in Section A.1, there is a unique, global solution, with $q_i^* > \underline{q}_i$. Choosing to search in a higher quality neighborhood q means a decreased chance of matching. This is most painful for someone with low $V(\underline{q}_i)$ and so people with worse outside options use their voucehr in worse neighborhoods. Differentiating with respect to $\bar{r}$ and solving for $\frac{\partial q_i^*}{\partial \bar{r}}$ gives

$$\frac{\partial q_i^*}{\partial \bar{r}} \propto -\frac{\partial f(.)}{\partial \bar{r}}\left(V(q_i^*) - V(\underline{q}_i)\right) + f(.)V'(q_i^*)$$

These terms are the same as in the baseline model, except that the utility *gain* $V(q^*) - V(\underline{q}_i)$ now affects the responsiveness to a price ceiling increase, whereas in the baseline model $V(\underline{q}_i)$ was normalized to zero. People with a lot to lose from failing to find a unit with their voucher will be less responsive to the increase in the price ceiling.

A.4.2 Out-of-Pocket Payments for Expensive Housing

One important institutional feature of the housing voucher program which is omitted from the baseline model is that a voucher recipient can sometimes rent a unit above the rent ceiling. Adding this feature does not change the core results from the model: that the impact of a rent ceiling increase on neighborhood quality is blunted by search frictions and that increases in the price ceiling raise markups. In particular, if a voucher recipient new to the program finds a unit whose rental cost is greater than the rent ceiling but lower than the rent ceiling plus 10% of her income then she can choose to rent it and pay the difference between the rent ceiling and the unit's rent out of pocket.[35]

In the baseline model above with only housing consumption, voucher recipients solved:

$$\max_q \int_{\eta_{min}}^{\bar{r}-q} V(q)dF(\eta)$$

Now, redefining V to have two arguments, q for housing quality and c for non-housing consumption, voucher recipients instead solve:

$$\max_q \int_{\eta_{min}}^{\bar{r}-q} V(q, 0.7y)dF(\eta) + \int_{\bar{r}-q}^{\bar{r}+0.1y-q} V(q, 0.7y - \eta - (\bar{r} - q))dF(\eta)$$

where voucher recipients have non-housing consumption of at most 70% of their income y. The optimal choice of quality q^* is given by the first-order condition:

$$\underbrace{F(\bar{r} + 0.1y - q^*)V_q}_{\text{choosing higher q improves quality...}} - \underbrace{f(\bar{r} + 0.1y - q^*)V(.)}_{\text{...but risks not matching}} - \underbrace{\int_{\bar{r}-q^*}^{\bar{r}+0.1y-q^*} V_c dF(\eta)}_{\text{...and lowers non-housing cons}} = 0$$

Raising the rent ceiling affects quality through the same second-order terms as in the baseline, plus a new term which captures utility gain from reduced out-of-pocket payments for housing. Intuitively, the new term blunts the impact of the recent ceiling increase on housing quality because adding non-housing consumption to the model gives voucher recipients another "good" to buy other than housing quality. The comparative static of quality with respect to the price ceiling is:

$$\frac{\partial q^*}{\partial \bar{r}} \propto -f(.)V_q + \frac{\partial f(.)}{\partial \bar{r}}V + \frac{d}{d\bar{r}}[\int_{\bar{r}-q^*}^{\bar{r}+0.1y-q^*} V_c dF(\eta)]$$

The new term $\frac{d}{d\bar{r}}[\int_{\bar{r}-q^*}^{\bar{r}+0.1y-q^*} V_c dF(\eta)]$ is most likely negative because its first and third components are negative and because the second term is small. Using the Leibniz rule, it is equal to

$$\int_{\bar{r}-q^*}^{\bar{r}+0.1y-q^*} V_{cc}(1 - \frac{\partial q^*}{\partial \bar{r}}) + V_{cq}\frac{\partial q^*}{\partial \bar{r}} dF(\eta) + [F(\bar{r} + 0.1y - q^*) - F(\bar{r} - q^*)]V_c(.)(1 - \frac{\partial q^*}{\partial \bar{r}})$$

The first term is negative under the assumption of diminishing marginal utility. The sign of the second term is ambiguous and depends on whether housing and non-housing consumption are complements or substitutes. Even in the case where they are complements, the term is still proportional to $\frac{\partial q^*}{\partial \bar{r}}$, which we show empirically in the paper to be small. The third term is negative under our distrbutional assumption from the baseline model that F has decreasing mass further into the tail of markups.

Raising the rent ceiling affects prices entirely through changes in the set of units rented. Because there is no bargaining between landlords and tenants in this model, adding out-of-pocket tenant payments does not affect the economic conclusions from this comparative static.

$$\frac{\partial \mu_{voucher}}{\partial \bar{r}} = [\bar{r} + 0.1y - \mu_{voucher}]\,\frac{f(\bar{r} + 0.1y - q)}{F(\bar{r} + 0.1y - q)}$$

[35]This rule does not apply to voucher recipients who are renewing their lease.

B Data 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 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 at

http://www.huduser.org/portal/datasets/fmr/fmr2005r/index.html 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 below.

Places Dropped – Reason
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 (among other things)
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. Although much of our analysis limits the voucher sample in various ways (e.g. stayers, movers), we always 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. (Recall that these difficulties are exactly what generated the policy variation we study here!) Using the notation developed in Section 4.1,

$$Cov(\Delta\hat{r}_t, \Delta FMR) = Cov(r_t + \varepsilon_t - r_{2000} - \varepsilon_{2000}, \Delta FMR) = Var(\varepsilon_{2000}) < 0 \tag{11}$$

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.[36] 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 3, column 2. This is consistent with measurement error, since ΔFMR_j is a function of the change in Census rents from 1990 to 2000, there is a mechanical negative correlation between FMR changes and Census rent changes from 2000 to a later date. 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).[37] 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 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

[36] The Census estimates include voucher recipients 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 recipients are unlikely to change in response to changes in the FMR.

[37] 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 $\hat{\beta} - \beta = -0.070$.

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-bencmaking 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

$$Rent_{ijklm} = \alpha + Bed_j + StrucType_k + Age_l + PUMA_m + \varepsilon_i \tag{12}$$

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 1. 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 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. We compare the predictive 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 2. The ACS variables approximate 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 coefficient $\hat{\gamma}$. 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, 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 2). 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. Rents in the AHS appear to be substantially higher variance than voucher rents in the CSS. Impressively, our hedonic measures explain nearly 70 percent of the cross sectional variation in voucher rents in the CSS.

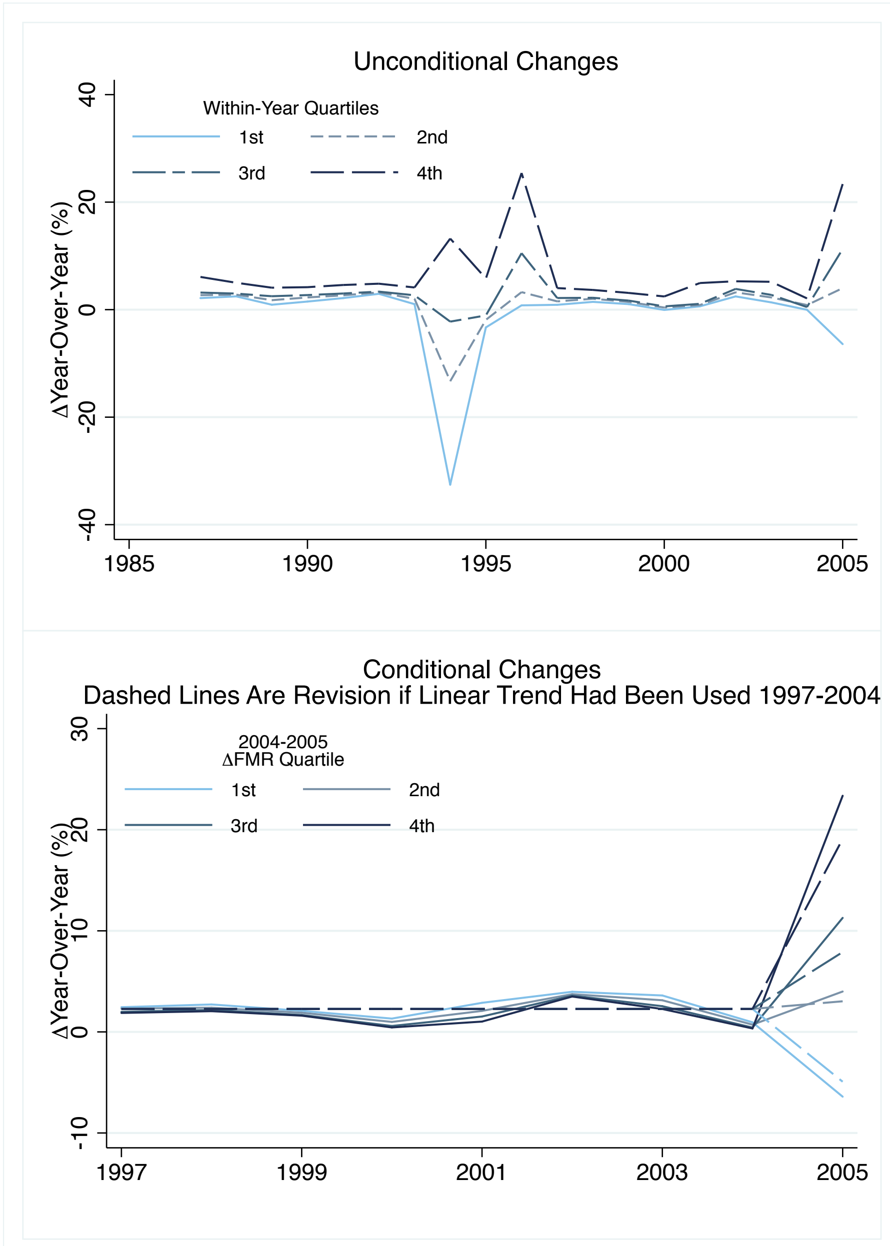
$$Rent_{ijklm} = \pi + s_i'\gamma + \varepsilon_i \tag{13}$$

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 recipients.

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). 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.

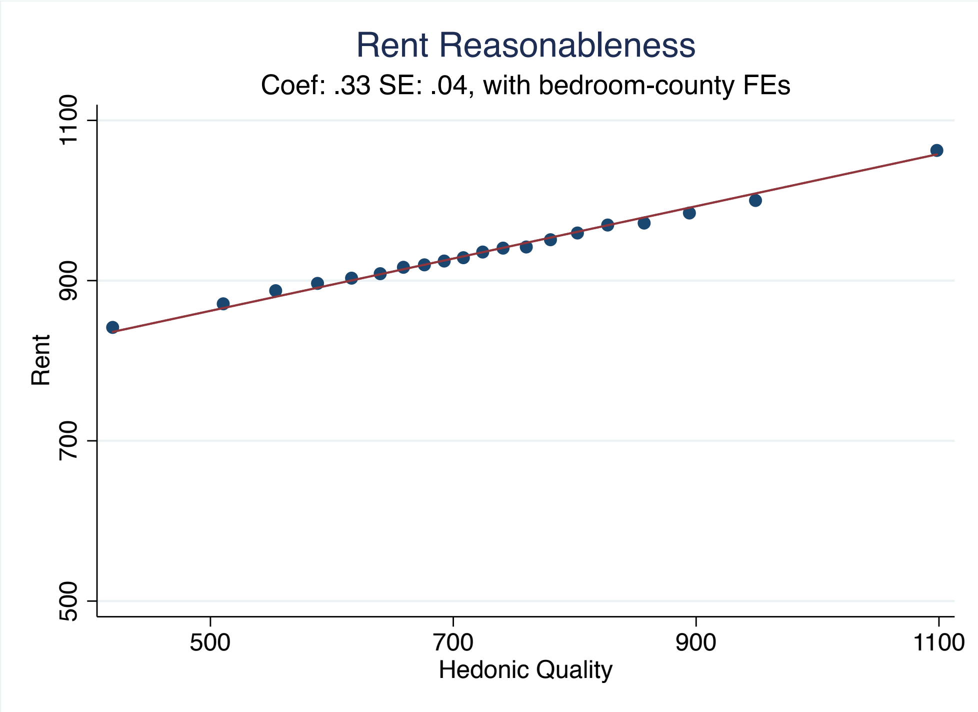
APPENDIX FIGURE 1 – County-Level FMR Changes



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.

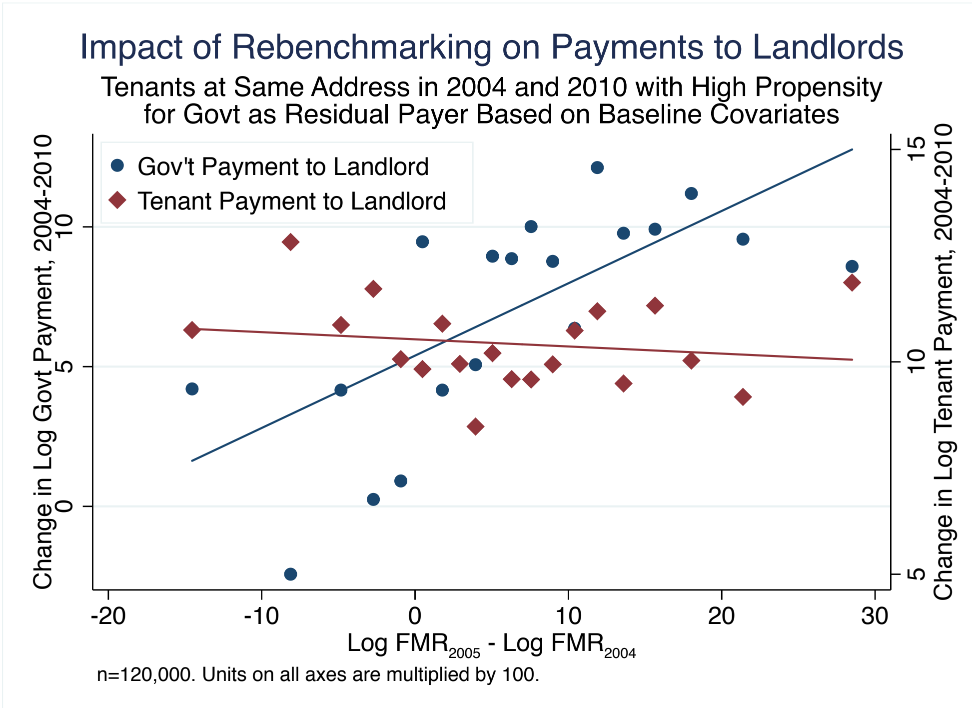
The bottom panel plots FMR changes for the same sample within quartiles defined over the 2004-2005 FMR change, as in Figure 1. The four groups exhibit similar trends in terms of changes prior to the rebenchmarking. There is some evidence of mean reversion: places which had higher revisions from 1997 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 larger than the counterfactual revisions, indicating substantial measurement error in intercensal FMR changes.

APPENDIX FIGURE 2 – Rent Reasonableness



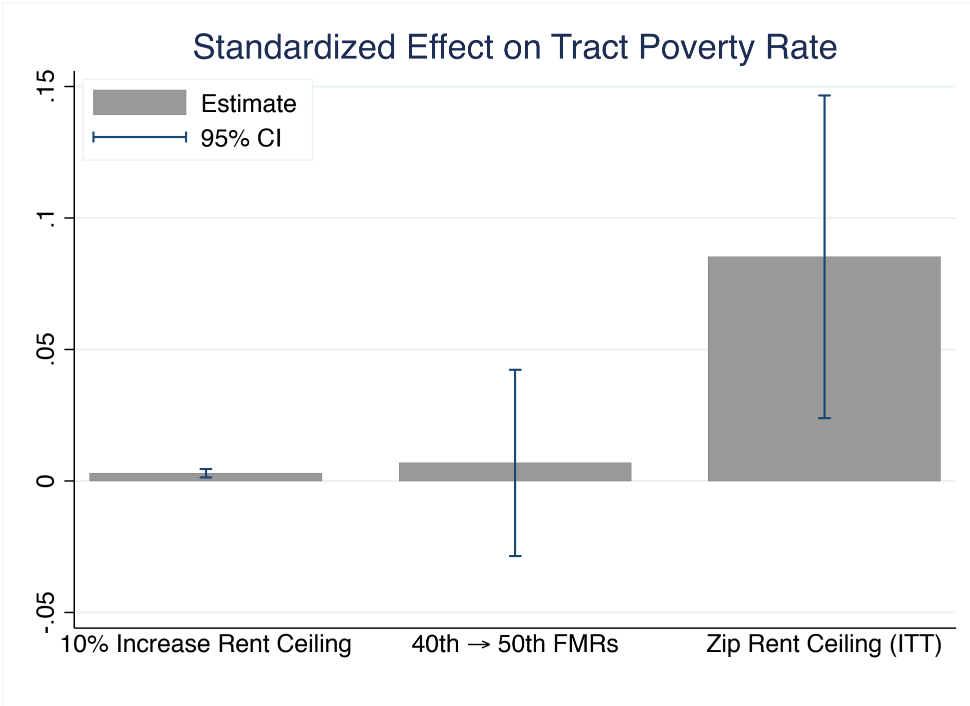
This figure plots conditional means of unit rent for twenty quantiles of hedonic quality. We include fixed effects for the number of bedrooms interacted with the county, because each voucher recipient's number of bedrooms is fixed by family size and it is usually quite difficult to switch counties. We find that a $1 increase in hedonic quality is associated with a 36 cent increase in rents. This indicates that even for a fixed rent ceiling, the government paid less for lower-quality units.

APPENDIX FIGURE 3 – Who Pays When Rent Ceiling Increases?



Notes: This figure plots payments to landlords by tenants (red) and the housing authority (blue) by re-benchmarking change in FMR for households that are unlikely to be the residual payer at baseline (2004). To identify households that are unlikely to be the residual payer we examine the gap between gross rents and the payment standard and the number of bedrooms in 2004. We use voucher recipients 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%. We estimate the effects of the re-benchmarking separately on tenant payments to landlords and government payments to landlords for these price insensitive tenants. Tenant payments are unresponsive to changes in FMR, while payments from the government to landlords rise substantially.

APPENDIX FIGURE 4 – Policy Comparison - Neigborhood Poverty



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