Incidence and Price Discrimination: Evidence from Housing Vouchers

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Abstract

What is the incidence of housing vouchers? In a frictionless, price-taking equilibrium, increased generosity of a narrowly-targeted subsidy causes increases in unit quality. However, search frictions may limit quality improvements and subsidies may accrue to landlords through price discrimination.

Analyzing a 2005 formula change for Housing Choice Vouchers, we estimate that a $1 increase in the county-wide price ceiling raised same-address voucher rents by 13-20 cents. For tenants who moved, quality improvements were minimal. Second, we find that a Dallas pilot which replaced a metro-wide price ceiling with ZIP-code-specific ceilings improved tenants' chosen neighborhood quality by 0.2 standard deviations.

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1 Introduction

What is the incidence of an increase in the generosity of a narrowly-targeted subsidy, such as housing vouchers? Using the tax incidence framework in the *Handbook of Public Economics* (Kotlikoff and Summers (1987)), a subsidy's incidence is determined by its impact on market-clearing prices. A targeted subsidy has little impact on market-clearing prices, because recipients are typically a small fraction of consumers. However, such subsidies may be captured by suppliers in a setting with search frictions and price discrimination, with the potential to undermine the redistributive value of "tagging" (Akerlof (1978)).

Housing Choice Vouchers, formerly known as Section 8, is the largest program funded by the Department of Housing and Urban Development (HUD), with spending of $18 billion in 2010 to house 2.2 million families. Voucher recipients typically pay rent equal to 30% of their income. The government negotiates rents with landlords, constrained by a price ceiling ("Fair Market Rent") which is set separately for every metro- and county-bedroom pair in the U.S. This feature means tenants have little incentive to keep down prices.

We examine the extent to which vouchers accrue to landlords or tenants. We fix ideas using a theoretical model of housing search. Voucher recipients choose a quality submarket in which to search for housing. The probability of finding a landlord who will accept a voucher is higher in low-quality submarkets. When a voucher recipient is successfully matched, the landlord and the government bargain over the rental price. We consider two changes to voucher policy: an increase in generosity in all submarkets, akin to an income effect, and an increase in generosity targeted to high-quality submarkets, analogous to a substitution effect. In the former case, the quality impact is ambiguous, while quality always improves in the latter case. We analyze the questions posed by the theoretical model using administrative data on the universe of housing vouchers and two quasi-experimental research designs.

In our first research design, we study a revision to county-level price ceilings in 2005 which used 2000 Census data to correct for a decade of accumulated forecast error. We estimate that a $1 increase in the price ceiling caused quality-adjusted rents to rise by 13-20 cents by 2010, using same-address changes in voucher prices to identify price discrimination.[2] Looking at the full sample of units, we estimate that a $1 increase in the price ceiling caused quality-unadjusted rents to rise by 41 cents, while hedonic unit quality rose by only 14 cents and median tract rent was constant. Using a new, unit-specific quality estimator, we find that the quality change was not significantly different from zero. Vouchers account for 2% of the U.S. housing market, and the price ceiling change had roughly zero correlation with contemporaneous changes in nonvoucher prices. Our finding that price ceiling changes accrue mostly to landlords is similar to work on UK housing subsidies by Gibbons and Manning (2006).

In our second research design, housing authorities in Dallas switched from a single metro-wide ceiling to ZIP-code-level ceilings in 2011, giving voucher recipients an 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 with a bachelor's degreee. Quality rises by 0.2 standard deviations, an improvement at least as big as that from giving vouchers to families living in private housing (Jacob and Ludwig (2008); Abt Associates (2006)). Because price increases in expensive ZIP codes were offset by larger decreases in low-cost ZIP codes, this policy had no net cost to the government.

An extensive literature in public economics focuses on how tax changes affect *marketwide* prices and quantities.[3] In contrast, we focus on price discrimination directed specifically at targeted beneficiaries who face search frictions.[4]

[2]Bayer et al. (2012) use a similar approach to identify price discrimination in home sales by race.

[3]Most existing work on the incidence of housing subsidies and place-based policies uses a framework with market-clearing prices (Susin (2002); Eriksen and Ross (2012); Fack (2006); Busso et al. (2013)). As another example, existing models used to analyze the Earned Income Tax Credit assume that the credit depresses wages for all low-skill workers (Rothstein (2008); Leigh (2010)). In supermarkets, where price discrimination based on subsidy status is very difficult, Hastings and Washington (2010) show that demand shocks from subsidy recipients have a limited impact on storewide prices.

[4]Other recent empirical work has also documented price discrimination against subsidy

Even when voucher recipients are a small share of the market, price discrimination can undermine the effectiveness of targeted subsidies. This insight may be relevant for other targeted transfer programs where the government offers voucher-like subsidies, such as health care, employment, food, and college.[5]

Section 2 describes the model, Section 3 reviews the program and data, Section 4 studies the national 2005 change in price ceilings, Section 5 studies the Dallas ZIP code-level demonstration, and Section 6 concludes.

2 Model

Three features of the housing voucher program differentiate it from a textbook subsidy – search frictions, negotiated prices, and small program market share.[6] First, voucher recipients have high search costs and face substantial discrimination. When a voucher is issued, voucher recipients typically have three months to use it or lose it.[7] Audit studies have shown discrimination by

recipients. Turner (2013) shows that colleges' net tuition prices respond to individual-level differences in Pell Grants and Azmat (2012) argues that a tax credit for low-skill workers in the U.K. lowered recipients' wages. We introduce a quality margin and allow for search frictions to limit the scope of quality improvement from the marginal subsidy, which are not explicit considerations in this prior work.

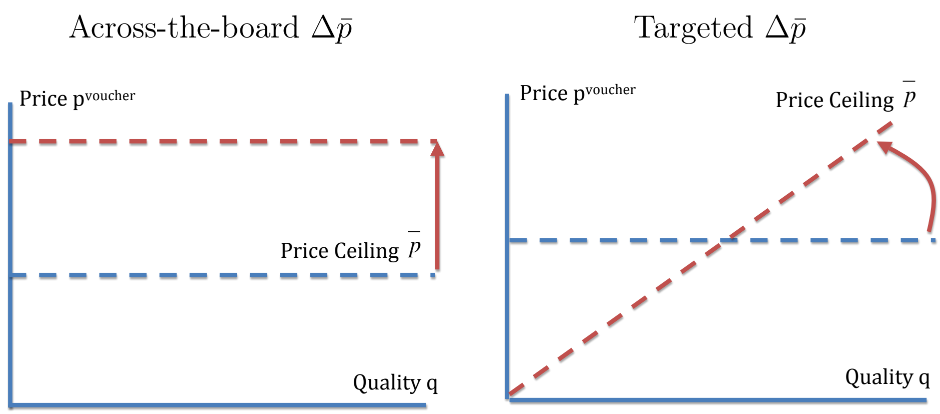
[5]More than half of US transfer program expenditures to the nonelderly were for purchases of privately-provided services, which have the potential for price discrimination. All of these programs use tagging, rather than offering a universal benefit. We calculate this number using data from Currie and Gahravi (2008) who review transfer programs in the US, and exclude public K-12 expenditures which are mostly funded at the local level and therefore have limited redistributive properties.

[6]Although this model is written in terms of tenants and landlords, it is potentially applicable to a broad variety of voucher programs. For example, the analysis could be applied to a program where the government subsidized wages of some low-skill jobseekers. For other voucher programs, the same considerations of price discrimination and quality remain important, but a microfoundation using other frictions may be more appropriate. For example, there is evidence that difficulty with complex calculations are important for health insurance (Abaluck and Gruber (2011)).

[7]Using data from the Survey of Income and Program Participation, we find that more than half of households receiving housing assistance do not own a car. Not surprisingly, evidence from interviews also suggests that apartment search is unusually costly for this population, even after they have signed their first lease (Rosen (2012)).

landlords against voucher recipients (Lawyers Committee for Better Housing Inc (2002)), and tenants with African-American sounding names (Hanson and Hawley (2011)). Second, if a voucher recipient finds a unit, the government and landlord negotiate prices, subject to a government-set price ceiling and a landlord participation constraint. A government inspector visits the unit in part to ensure that the rent is "reasonable" given the quality of the unit. This enforcement mechanism attempts to prevent landlord markups on low-quality units. Third, voucher recipients are on average only about 2% of all units in a Census tract.[8]

We build a simple positive model 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 are able to find units, because of search costs and landlord discrimination. It is more difficult to find a unit in a higher-quality submarket. When a landlord accepts a voucher, she bargains with the government over the price of the unit.



We consider two policy changes – an across-the-board increase in the price ceiling throughout all submarkets and an increase in the price ceiling targeted to high-quality submarkets. Across-the-board increases are like an income effect in that voucher recipients may use the funds for moves to a better sub-

[8]Even in tracts where vouchers are most concentrated, they are still a small share of all units. We index voucher units by "concentration," the share of units in each Census tract which are vouchers in 2004. For a voucher household at the 90th percentile of the concentration distribution, 9% of all units in its tract are vouchers.

market or improved matching probability in the previously-chosen submarket. Impacts on prices are determined by landlords' ability to raise markups. A targeted price change affects the relative price of quality, like a compensated substitution effect, and therefore unambiguously causes an increase in quality. Incidence on landlords is determined by the extent to which landlords tolerate price cuts in low-quality submarkets and raise prices in high-quality submarkets.

2.1 Environment

There exists a static private market for rental units. There is a unit mass of tenant and a unit mass of landlords. Each landlord owns a unit of quality $q \in [q_{min}, \infty)$, which represents the flow utility of their unit to any tenant.[9] q is observable, and has positive measure for all $q \geq q_{min}$. When occupied, a landlord receives rent p, and when vacant, a landlord receives no rent.

A subset of renters, too small to have any general equilibrium impact on prices, has vouchers. We model voucher recipients as choosing quality submarket q using a directed search framework; they trade off the probability of successfully finding a unit with the quality of the unit. If they find a landlord who will accept them, then landlord and government bargain over prices.

Private Tenants: When landlord and tenant are matched, all match surplus is allocated to the tenant, so $p = q$.[10] Because our analysis is focused on the voucher program, we do not model the allocation of private tenants to units.

[9]Conceptually, q should be thought of as a summary measure of many different inputs to quality such as neighborhood, building type, and unit size. We do not allow the landlord to change the quality of her unit. We critically examine this assumption in Section 4.2.

[10]One way to justify this assumption is that private renters typically have good outside options, while for landlords, having a vacant unit is very costly. Allocating all the match surplus to private tenants is equivalent to allowing for generalized Nash bargaining with any set of weights, while allowing the tenant's outside option to asymptotically approach the flow utility of the unit q.

Voucher Recipients: Voucher recipients have an outside option of zero and choose exactly one quality level q to maximize expected quality. They are randomly matched to units within q and get K draws of potential matches before their voucher expires. Higher quality units are more attractive, but it is harder to find a landlord that will take a voucher recipient in a higher-quality submarket.[11] Voucher recipients solve

$$V_{voucher}(q;\bar{p}) = \max_{q} \underbrace{\mathbb{P}(q,\bar{p})}_{\text{match probability}} \underbrace{q}_{\text{quality}}$$

Landlord Acceptance of Vouchers: Landlords have a taste for voucher recipients $\delta \in [0,\infty) \sim G$, which is independent of quality q.[12] They accept a voucher if they can receive payment of at least $q+\delta$.

Voucher Prices: If a landlord decides to accept a voucher recipient, then the landlord and government engage in generalized Nash bargaining, with reservation values of $q+\delta$ and price ceiling $\bar{p}$, respectively, and bargaining weight $\beta \in [0,1]$.[13]

[11] This is a compensating differentials argument, as in Rosen (1986). Moen (1997) calls this a "competitive search equilibrium." This set up uses four assumptions to clarify exposition: linear utility in unit quality, a representative agent, an outside option of zero, and search in exactly one submarket. In Appendix A.1, we relax each of these assumptions.

[12] Our assumption that the taste distribution is independent of voucher levels is not restrictive; what matters in each submarket q is the measure of landlords on the margin $g(\bar{p}-q)$, which can take any value. We impose a regularity condition: the distribution G is twice-differentiable, with $\frac{\partial \log g(\bar{p}-q)}{\partial \delta} > \kappa = \frac{-K(1-G(\bar{p}-q))^{K-1} g(\bar{p}-q)}{1-(1-G(\bar{p}-q))^{K}} - (K-1)\frac{g(\bar{p}-q)}{1-G(\bar{p}-q)} \forall q$. This condition is analogous to the strict monotone likelihood ratio property ($\frac{\partial g(x)}{\partial x} > 0$), but slightly weaker, since κ is negative.

[13] This assumption allows for clear exposition, but a wider set of assumptions are feasible, including rendering δ unobservable so that it cannot be used in bargaining, and allowing bargaining weight β to vary with q. We discuss these extensions in Appendix A.2.

2.2 Solution

We solve the model in four steps. First, the government announces a price ceiling $\bar{p}$. A randomly chosen landlord in submarket q will accept a tenant with probability $G(\delta^*(q,\bar{p})) = G(\bar{p}-q)$.

Second, we aggregate over multiple tenant draws. The probability of a voucher recipient signing a lease in submarket q is

$$\mathbb{P}(q,\bar{p}) = 1 - (1 - G(\bar{p}-q))^K \tag{1}$$

$\mathbb{P}(q,\bar{p})$ is the cumulative distribution function of a geometric distribution with parameter $G(\bar{p}-q)$, evaluated at K, the voucher recipient's number of draws. Equation 1 highlights two distinct forces which limit voucher recipients' ability to successfully find units. Only landlords with low discrimination levels $\delta < \bar{p} - q$ will accept them as tenants and voucher recipients have a limited number of draws K before their voucher expires. This formulation nests a frictionless benchmark, either when landlords no preference between private tenants and vouchers ($P(\delta = 0) = 1$) or search costs are approaching zero (so $K \to \infty$).

Next, we solve the voucher recipient's problem of $\max_q \mathbb{P}(q,\bar{p})q$ with first order condition:

$$q + \frac{\mathbb{P}(q,\bar{p})}{\frac{\partial}{\partial q}\mathbb{P}(q,\bar{p})} = 0 \tag{2}$$

Claims: (i) A solution $q = q^*$ exists for equation 2 and is unique. (ii) $\frac{\partial q^*}{\partial p}$ exists.

Proofs: See Appendix A.3.

Finally, we characterize transaction prices as a weighted average of the price ceiling and unit quality: $p^{voucher}(q,\bar{p},\delta) = \beta\bar{p} + (1-\beta)(q+\delta)$. Integrating over part of the δ distribution, we can define

$$p^{voucher}(q,\bar{p}) = \int_0^{\bar{p}-q} (\beta\bar{p} + (1-\beta)(q+\delta))\, dG(\delta)/G(\bar{p}-q) \tag{3}$$

.

2.3 Comparative Statics

Across-the-Board Price Ceiling Increase: Price Impact

The impact of an increase in the price ceiling on voucher prices is

$$\begin{aligned}\frac{dp^{voucher}(q^*(\bar{p}),\bar{p})}{d\bar{p}} &= \underbrace{\frac{\partial p^{voucher}(q^*(\bar{p}),\bar{p})}{\partial \bar{p}}}_{\text{price increase from markup}} + \underbrace{\frac{\partial p^{voucher}(q^*(\bar{p}),\bar{p})}{\partial q^*(\bar{p})}\frac{\partial q^*(\bar{p})}{\partial \bar{p}}}_{\text{price increase from quality}} \\ &= \beta + (1-\beta)\frac{\partial q^*(\bar{p})}{\partial \bar{p}}\end{aligned} \tag{4}$$

The first term reflects price discrimination. If landlords are able to negotiate higher prices with the government than they would receive from a private tenant, then an increase in the price ceiling will directly raise transaction prices. The sign of the second term depends upon whether quality goes up or down. If quality increases, then the government will pay more to cover the cost of higher-quality units for recipients.

Across-the-Board Price Ceiling Increase: Quality Impact

Quality is determined by the first order condition in equation 2. Differentiating with respect to $\bar{p}$ and solving for $\frac{\partial q^*}{\partial \bar{p}}$ gives

$$\frac{\partial q^*}{\partial \bar{p}} = \frac{1}{-\frac{\partial \mathbb{P}(q^*,\bar{p})}{\partial q}}\left[\underbrace{\frac{\partial \mathbb{P}(q^*,\bar{p})}{\partial \bar{p}}}_{\text{increased avg acceptance } >0} + \underbrace{\frac{\partial^2 \mathbb{P}(q^*,\bar{p})}{\partial q \partial \bar{p}}}_{\text{marg LL participation } \lesseqgtr 0} q^*\right]$$

The probability of matching is lower in higher-quality neighborhoods $\frac{\partial \mathbb{P}(q^*,\bar{p})}{\partial q} < 0$. We focus our analysis on the terms in square brackets.

Increased Average Acceptance Since $\mathbb{P}(q^*;\bar{p}) = 1-(1-G(\bar{p}-q^*))^K$,

$$\frac{\partial \mathbb{P}(q^*,\bar{p})}{\partial \bar{p}} = K\left(1-G(\bar{p}-q^*)\right)^{K-1} g(\bar{p}-q^*) > 0$$

Raising the price ceiling raises a voucher recipient's probability of acceptance, so recipients search in better submarkets. Intuitively, the magnitude of the improvement in the matching probability depends on how many additional landlords are willing to rent to voucher recipients ($g(\bar{p}-q)$).

Marginal Landlord Participation

$$\frac{\partial^2 \mathbb{P}(q^*,\bar{p})}{\partial \bar{p}\partial q} = \left(\frac{\partial g(\bar{p}-q^*)}{\partial q} + g(\bar{p}-q^*)^2 \frac{K-1}{1-G(\bar{p}-q)}\right) K\left(1-G(\bar{p}-q)\right)^{K-1}$$

We cannot sign $\frac{\partial^2 \mathbb{P}(q^*,\bar{p})}{\partial \bar{p}\partial q}$, but the first term conveys an important idea about how the distribution of landlord preferences affects quality. Suppose that on the margin landlords with low-quality units are more willing to participate than landlords with high-quality units. In this case, it is possible that the value of searching in a low-quality submarket may rise more than the value of searching in a high-quality submarket and then the recipient's optimal quality choice will decrease. Because the two quality effects discussed above have potentially opposite signs, it is impossible to sign $\frac{\partial q^*}{\partial \bar{p}}$, the effect of an across-the-board price ceiling increase on realized quality.

Targeted Price Ceiling Changes

Consider an alternative policy which raises the price ceiling in high-quality submarkets and lowers it in low-quality submarkets.[14] To the extent that there is price discrimination, this will result in price decreases in low-quality submarkets and increases in high-quality submarkets. The consequences for quality, where recipients maximize $\mathbb{P}(q;\bar{p})q$ choosing across all submarkets, are more subtle. In terms of the equation above for $\frac{\partial q^*}{\partial \bar{p}}$, this amounts to

[14] Expressing this policy in terms of the model requires tweaking equation 1 to $\mathbb{P}(q;\bar{p}) = 1-(1-G(\bar{p}(q)-q))^K$ where $\bar{p}(q)$ is the price ceiling for submarket q. A sufficient condition for the existence of a unique solution q^* is to modify the regularity condition in footnote 12 to $\frac{\partial \log g(\bar{p}-q)}{\partial \delta} > (1-\frac{\partial \bar{p}}{\partial q})\kappa$.

setting "increased average acceptance" $\frac{\partial \mathbb{P}(q^*,\bar{p})}{\partial \bar{p}}$ to zero. By making new units accessible in high-quality neighborhoods and making fewer units accessible in low-quality neighborhoods, such a policy ensures that marginal landlord participation $\frac{\partial^2 \mathbb{P}(q^*(\bar{p});\bar{p})}{\partial \bar{p} \partial q}$ will be positive, and consequently that quality will rise.

In the remainder of the paper, we estimate these empirical quantities using two complementary research designs.

3 Data and Program Description

We use a HUD internal administrative database called PIC which contains a household identifier, address, building covariates, contract rent received by landlord, and landlord identifier, on an annual basis beginning in 2002. Appendix B.1 discusses sample construction. Summary statistics support the key features of the model: hedonic quality is well below the price ceiling (search frictions), prices exhibit substantial bunching at the ceiling (price discrimination), and among units with the same price ceiling, prices are lower for units with lower hedonic quality (rent reasonableness).[15]

HUD typically sets Fair Market Rents (FMRs) at the 40th percentile of county-level gross rent (contract rent plus utility costs). Sometimes HUD will set a single FMR for a metro area and in some metro areas HUD sets rents at the 50th percentile. Within each geography, there are different FMRs for studios through 4-bedroom units. In most years, FMRs are updated using local CPI rental measures for 26 large metro areas and 10 regional Random Digit Dialing surveys for the rest of the country. These intercensal estimates are very

[15] The top panel of Appendix Figure 1 plots the distribution of prices and hedonic quality, relative to the price ceiling. Voucher recipients are allowed to lease units with rents that are higher than the price ceiling and pay the difference out of pocket, but this is uncommon. Appendix Figure 1 shows that 11% of units appear to have rents of at least \$100 greater than the price ceiling. This estimate is an upper bound on the share of voucher recipients who are the residual payer, since it also reflects household-level measurement error (rent miscoded as too high, price ceiling miscoded as too low). If voucher recipients are able to hold down price increases on their units, then our reduced form estimates mask even larger rental impacts on those voucher recipients who are not the residual payer. The bottom panel shows of Appendix Figure 1 shows evidence for rent reasonableness.

noisy. However, when new micro data from the Census become available, these data are used to update FMRs. 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 (see Appendix Figure 2). The local housing authority chooses a local price ceiling $\bar{p}$ (or "Payment Standard"), as 90%, 100% or 110% of the federally-set FMR. Although an FMR increase *allows* housing authorities to increase the price ceiling, housing authorities may use their discretion to smooth out FMR changes.[16]

4 Results from County-Level Rebenchmarking

We estimate the causal effect of price ceilings on voucher prices and unit quality. In Section 4.1, we describe our identification strategy using the 2005 rebenchmarking. Recall that total price changes can be decomposed into changes in price discrimination and changes from quality improvements (equation 4: $\frac{dp^{voucher}}{d\bar{p}} = \frac{\partial p^{voucher}}{\partial \bar{p}} + \frac{\partial p^{voucher}}{\partial q}\frac{\partial q^*}{\partial \bar{p}}$). We estimate $\frac{\partial p^{voucher}}{\partial \bar{p}}$ in Section 4.2, $\frac{dp^{voucher}}{d\bar{p}}$ in Section 4.3, and $\frac{\partial q^*}{\partial \bar{p}}$ in Section 4.4. Finally, in Section 4.5, we compare our empirical results to predictions from our model with price discrimination and to a frictionless benchmark.

4.1 Identification

The availability of new Census data results in a "rebenchmarking."[17] 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 1 shows an event study of FMRs for four groups of county-bed pairs,

[16]Housing authorities are typically allocated a fixed budget for vouchers, and this budget does not vary with FMR changes. When a housing authority increases its price ceiling, it is able to finance fewer vouchers.

[17]More details on the rebenchmarking are provided in Appendix B.2. Gordon (2004), Suarez-Serrato and Wingender (2011) and Bhutta (2013) also use decennial Census rebenchmarkings as a source of variation in federal policies.

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, unconfounded by subsequent, serially correlated policy changes. Define $exp(\sigma_t)$ as an annual estimate of rental changes based on a regional RDD or CPI survey from year $t-1$ to t.[18] Define $exp(p_t + \varepsilon_t)$ as an observation from decennial Census data, where p_t is the true price and ε_t is measurement error at time t. We can use these definitions to write $\log FMR^{2004} = \sum_{t=1991}^{2004} \sigma_t + p_{1990} + \varepsilon_{1990}$, and $\log FMR^{2005} = \sum_{t=2001}^{2005} \sigma_t + p_{2000} + \varepsilon_{2000}$.[19] Taking the difference gives

$$\Delta FMR = \underbrace{p_{2000} - p_{1990}}_{\text{true price change}} + \underbrace{\sigma_{2005} - \sum_{t=1990}^{1999} \sigma_t}_{\text{annual meas error}} + \underbrace{(\varepsilon_{2000} - \varepsilon_{1990})}_{\text{Census meas error}} \tag{5}$$

There are three sources of variation in the rebenchmarking: changes in nonvoucher prices, measurement error from annual updates, and measurement error in the Census.[20] 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, consistent with measurement error σ_t and ε_{1990} (see Figure 1 and also Appendix Figure 2). In addition, changes in measured nonvoucher prices from 2000 forward are negatively correlated with the rebenchmarking, consistent

[18] CPI and regional RDD surveys are used to produce adjustment factors which modify the base, not a new estimate of the level. These estimates are very coarse, and in fact a bit worse at predicting local price changes than using a single national trend over this period, as shown in Appendix Figure 2.

[19] Throughout the paper, all specifications use log price or log quality. 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.

[20] Our policy variation is at the county-bed cell level and measurement error $\varepsilon_{2000} - \varepsilon_{1990}$ 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 throughout our analysis in Section 4, with exception of a robustness check which weights each voucher equally in Appendix Table 3.

with measurement error ε_{2000} (see Appendix Table 2, column 5).

Suppose that outcomes such as voucher price or unit quality may be affected by the price ceiling $\bar{p}$ as well as contemporaneous nonvoucher prices $p^{nonvoucher}$, as expressed by the empirical model $y = h(\bar{p}, p^{nonvoucher})$. Our identifying assumption is that local rental trends after 2004 were orthogonal to the FMR change from 2004 to 2005.

Identification Assumption in County-Bed Research Design

$$E(\Delta p_{2004-t}^{nonvoucher} | \Delta FMR) = 0$$

As detailed above, ΔFMR consists of measurement error, which is by construction orthogonal to future trends, and the true nonvoucher price change, $p_{2000} - p_{1990}$ for which we assume that $E(\Delta p_{2004-t}^{nonvoucher} | p_{2000} - p_{1990}) = 0$. Note that this research design allows the rebenchmarking to bring rental prices closer in line with the *level* of market fundamentals. We require only that the *change* in FMR be uncorrelated with the subsequent *change* in nonvoucher rental prices. Available empirical evidence supports this identification assumption.[21] Contemporaneous changes in nonvoucher prices have no significant correlation with the FMR change. Prior changes in voucher prices show either zero or negative correlation with the FMR change. In addition, we relax this assumption by adding county fixed effects. These results reinforce the plausibility of this assumption.

We use two stage least squares to address endogeneity, because local housing authorities have some discretion in setting price ceilings. The bottom panel of Figure 1 shows an event study with the path of local price ceilings around the rebenchmarking. Housing authorities use their discretion to offset the impact of FMR changes, so it takes time for FMR changes to absorb into

[21] Appendix B.3 analyzes prior and contemporaneous changes in nonvoucher prices in more detail. Glaeser and Gyourko (2006) report serial correlation in housing price changes and rent changes at one-, three-, and five-year horizons in Table 2 and if anything they find negative serial correlation at five-year horizons.

local policy. In regression form, with j indexing county-bed FMRs, our first stage equation estimates

$$\text{First Stage: } \Delta\bar{p}_j = \alpha + \gamma\Delta FMR_j + \varepsilon_j \tag{6}$$

We find that a \$1 increase in the FMR from 2004 to 2005 corresponded to a 67 cent increase in the regional price ceiling from 2004-2010, with standard errors of about 5 cents.[22] In other words, housing authorities changed their price ceilings to absorb one-third of the increase, in an attempt to partially preserve the status quo. Our second stage estimating equation is

$$\text{Second Stage: } \Delta y_j = \alpha + \beta\widehat{\Delta\bar{p}_j} + \eta_j \tag{7}$$

Under our identification assumption, these equations identify the causal impact of changes in the price ceiling on prices and unit quality.

4.2 Impacts on Same-Address Voucher Prices

We examine the effect of price ceiling increase on voucher prices at a given address. Our basic 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 these estimates constitute evidence of price discrimination.

Figure 2 shows an event study of impacts on rents for stayers: rents rose in places which had FMR revised upward and fell in places which had FMR revised downward in relative terms. In regression form, we estimate the impact

[22] The administrative data report the price ceiling $\bar{p}$ at the household level. Although much of our analysis limits the voucher sample in various ways (e.g. stayers, movers), we always compute $\bar{p}_{jt}$ as the unconditional mean of all observations in a county-bed-year cell. The estimates vary a bit depending on the geographic distribution of the estimation sample (see Table 1). We re-estimate equation 6 over alternative time horizons, and the coefficients are shown in Appendix Figure 3.

of the price ceiling for stayers using $\Delta y_{ij} = p^{voucher}_{2010,ij} - p^{voucher}_{2004,ij}$ in equation 7, where i indexes households. Table 1 shows the results – a \$1 change in the price ceiling corresponded to a 13 cent increase in rents for stayers from 2004 to 2010. This estimate is economically quite small and statistically precise, with a standard error of less than three cents. Figure 2 also shows the time path of impacts for stayers. Consistent with our identification assumption, rents for stayers are about flat from 2002 to 2004, with no statistically significant change.

We also examine changes in prices for addresses which were occupied by different households before and after the rebenchmarking. We exploit the fact that 41% 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 7 with $\Delta y_{hj} = p^{voucher}_{2010,hi'j} - p^{voucher}_{2004,hij}$ 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 price ceiling caused rents to rise by 20 cents, as reported in Table 1.

As a robustness check, we add county fixed effects, so that identification comes only from within-county variation comparing, for example, the FMR change for 1-bedroom units to the FMR change for 4-bedroom units. Here, we find that a \$1 increase in price ceiling raises prices for stayers by 19 cents (Table 1). Appendix Table 3 shows that the stayers estimate is stable under several additional robustness checks.

Finally, we look for evidence of whether the observed rent changes may reflect changes in tenant payments or time-varying unit quality. 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 completely explain the results, because we find substantial price increases among these larger landlords (Appendix Table 3, row 9). Last, we examine whether within-unit quality changes in response to the rebenchmarking. Using the American Housing Survey, which contains a panel of housing units and time-varying quality measures, we find no significant impact on unit quality, though the estimates are imprecise (Appendix Table 4).

An increase in the price ceiling is strongly associated with an increase in same-unit prices. We interpret this result as evidence of price discrimination, where landlords are able to charge more for the exact same units when the price ceiling rises.[23] "Rent reasonableness" restrictions likely make it difficult to raise prices all at once. Because inspectors have access to prior prices paid for the same unit, the same-address estimates may understate the extent of price discrimination.

4.3 Impacts on Unconditional Voucher Prices

Next, we estimate the total derivative of prices with respect to the price ceiling: $\frac{dp^{voucher}}{d\bar{p}}$. We estimate equation 7 with $\Delta y_j = p^{voucher}_{t,j} - p^{voucher}_{2004,j}$, where $p^{voucher}_{t,j}$ is the unconditional average of rents in county-bed j, including units that newly entered and exited the sample. We find that by 2010, a $1 increase in the price ceiling had raised prices by 41 cents (Table 1, column 4).

In the top panel of Figure 3, we see that prior to the FMR change, prices are *rising* for places about to receive a downward revision and that prices are *falling* for places about to be revised upward. These pretrends suggest an alternative empirical strategy with first stage $\bar{p}_t = \alpha + FMR_{2005} + FMR_{2004} + \bar{p}_{2004}$ and second stage $\Delta p_{t,2004} = \alpha + \beta\widehat{\bar{p}}_t + FMR_{2004} + \bar{p}_{2004} + \varepsilon$, where FMR_{2005} is the excluded instrument. The results are shown in the bottom panel of Figure 3; this empirical strategy makes the pretrends statistically indistinguishable

[23] Other affordable housing researchers have argued that there is a submarket for subsidized housing, with fixed landlord participation costs (Rothenberg et al. (1991)). The existence of such submarkets could mean that price increases documented here reflect movement along the supply curve, and not price discrimination. Our empirical results, however, are more consistent with tenant-specific price discrimination. Since each housing authority's budget is fixed, places that had a price ceiling increase saw *decreases* in the number of vouchers. If units were priced competitively within the voucher submarket and the number of vouchers fell, then voucher prices should have fallen, not risen.

from zero, and delivers very similar point estimates in subsequent years.[24]

The estimate of 41 cents for unconditional price impacts is larger than the estimates of 13-20 cents with address fixed effects. The same-address estimates are closer to zero either because it is easier for an inspector to reject a proposed price increase from a landlord who previously has accepted a lower price or because voucher recipients moved to better units.

4.4 Impacts on Quality of Voucher Units

We now turn our attention to unit quality impacts, $\partial q^*/\partial \bar{p}$. We use two complementary methods for measuring unit quality – conventional hedonic measures which are available for all units and a new unit-level measure of quality which is available only for some units.

First, we construct a hedonic measure using structure age, structure type (e.g. single-family, multi-family, or apartment building) and Public Use Microdata Area.[25] In regression form, we estimate equation 7, with $\Delta y_j = q_{t,j}^{hedonic} - q_{2004,j}^{hedonic}$ and Figure 4 plots the year-by-year coefficients. A $1 increase in the price ceiling raises hedonic quality by 14 cents by 2010 (Table 2, column 1). As a supplementary test, in column 2 we analyze changes in median tract-level rent and find precisely zero impact from price ceiling increases.[26]

A measure of unit-specific quality comes from using pre-2005 rents by address. Suppose that in 2004, Mr. Smith ended his lease of a house, and in 2005, Ms. Jones left her old apartment and signed a lease at Mr. Smith's old house. We compute the quality improvement for Ms. Jones as the difference between the rent she paid in her apartment and the rent that Mr. Smith

[24] Our baseline empirical strategy uses both variation in true trends ($p_{2000} - p_{1990}$) as well as measurement error (σ_t and ε_t), as shown in equation 5. Explicitly controlling for FMR_{2004} is unattractive, because it eliminates measurement error σ_t and ε_{1990} as a source of variation. We report it as a robustness check only, rather than using it as our main specification. Appendix Table 3, row 3 reports shows that this specification for stayers generates similar estimates to the baseline stayers estimate.

[25] Public Use Microdata Areas are geographic units with about 150,000 residents. Details on construction of the hedonic measure are provided in Appendix B.4.

[26] Census tracts typically have 4,000 residents. 77% of voucher moves cross tract boundaries, so this measure should pick up localized moves to higher-quality neighborhoods.

paid in the house: $\Delta y_{ij} = p_{2004}^{\text{old rent at new unit}} - p_{2004}^{\text{old unit}}$. This approach is attractive because it captures changes in unit quality not detected by hedonic methods. However, this strategy is only feasible for people who moved to an address previously used by another voucher; this measure will not include quality increases from new units entering the sample.[27]

Formally, we estimate a first stage equation $\bar{p}_{2010,j'} - \bar{p}_{2004,j} = \alpha + \gamma \Delta FMR_j + FMR_{j,2004} + \eta_j$ and a second stage of $\Delta y_{ij} = \alpha + \beta \left(\widehat{\bar{p}_{2010,j'} - \bar{p}_{2004,j}} \right) + FMR_{2004} + \varepsilon$. We use variation in changes to FMR where the voucher recipient lived in 2004 (place j) as an instrument for the realized change in the price ceiling, $\bar{p}_{2010,j'} - \bar{p}_{2004,j}$, where j' reflects the household's current county residence.[28] We report year-by-year regressions in the bottom panel of Figure 4. Changes in the price ceiling have no statistically significant impact on this measure of unit quality (Table 2, column 3 and 4). The estimate for within-county moves is a precise zero, while the estimate for cross-county moves is imprecise.

Across-the-board increases in the price ceiling did not raise neighborhood quality and may have slightly improved structure quality. It could be that recipients decided to "spend" their new subsidy on increased acceptance probabilities in their current neighborhood, rather than quality improvement. As

[27]Two threats to this quality measure are distortions in voucher prices and sample selection. If voucher prices are distorted relative to nonvoucher prices, as in equation 3, this specification will consistently test for the existence of quality improvements, but understate their magnitude. Regarding sample selection, if FMR increases cause every voucher recipient to move to a unit with higher quality by \$50, then people moving from good units to great units drop out of the sample. However, so long as the people moving from mediocre units to good units choose places that were previously occupied by other voucher recipients, this method will still detect a quality increase.

[28]This specification mirrors the empirical strategy discussed in footnote 24. Here, we include $FMR_{2004,j}$ as a control variable to address a mechanical correlation between the instrument and the outcome. For voucher recipients who change counties, Δy is mechanically correlated with ΔFMR. This arises because ΔFMR_j is mechanically negatively correlated with $FMR_{2004,j}$ due to mean reversion, and $FMR_{2004,j}$ is mechanically positively correlated with the $p_{2004}^{\text{old unit}}$, because FMR and average rents are highly correlated in the cross-section. This was not necessary for the address-level research design developed in the previous section, because in that case both pre- and post-rents were measured within the same FMR area. We also implement this strategy for within-county movers because any measurement error in destination location generates the same biases.

a robustness check, we regress the change in new landlord participation on the change in FMRs, and find a precisely estimated zero coefficient (Appendix Table 5). We also regress the change in the share of households moving to a new unit on the change in FMRs, and again find a precise zero.

4.5 Comparison of Model and Frictionless Benchmark

Now, we compare the predictions of our theory with those under the frictionless benchmark.

	Frictionless Benchmark	Model with Frictions	Point Estimates
Price $p^{voucher}(q, \bar{p})$	$p^{voucher} = \bar{p}$	$p^{voucher} \approx \bar{p}$	$p^{voucher} = 714,\ \bar{p} = 747$
$\partial p^{voucher}(q, \bar{p})/\partial \bar{p}$	0	$\uparrow$ or 0	0.13 to 0.20
Quality q^*	$q^* = \bar{p}$	$q^* < \bar{p}$	$q = 618, \bar{p} = 747$
$\partial q^*/\partial \bar{p}$	1	$\uparrow$ or $\downarrow$	-0.02 to 0.14

Notes: We report average gross rent (contract rent + utilities) and ceiling for 2009. We omit the imprecise quality estimate using prior prices (Table 2, column 4).

Average voucher hedonic quality is well below the average price ceiling, while prices are much closer.[29] We find strong evidence of price increases, holding unit address fixed. Increases in the price ceiling yield at best a modest improvement in structure quality and no improvement in neighborhood quality. Taken together, our empirical results reject this frictionless benchmark, and suggest that suppliers capture a significant portion of the increased price ceilings through price discrimination. We explicitly analyze the welfare consequences of a price ceiling increase in Appendix C. We find welfare gains from cutting price ceilings and using the extra revenue to expand voucher enrollment.

[29] Our results to date have focused on *marginal* incidence. If unobserved quality for voucher units is the same as for nonvoucher units, this table can also be used to estimate *average* incidence. Average markups are about $100 in 2009, and tenants receive 87% of the rental payment of their voucher in hedonic housing quality units.

5 Results from ZIP-Level Demo in Dallas

In this section, we present evidence from a second, complementary research design in Dallas which switched from a single metro-wide price ceiling to ZIP code-level price ceilings. Following a court settlement, HUD replaced a single metro-wide FMR with ZIP code-level FMRs in early 2011.[30] The demonstration caused sharp changes in local price ceilings, ranging from a decrease of 20% to an increase of 30%, as shown in the top panel of Figure 5. All tenants in expensive ZIP codes saw increased ceilings. Voucher recipients renewing leases in ZIP codes which experienced a decrease were given a one-year grace period, but new leases signed in low-cost ZIP codes saw decreased ceilings. We focus our analysis primarily on movers, who were completely exposed to the new price schedule, and had strong incentives to change their search strategy, substituting to higher building quality and better neighborhoods.

Identification Assumption in ZIP Code-Level Research Design

$$E(\Delta p^{nonvoucher}_{2010-2011} | \Delta FMR) = 0$$

The identifying assumption is that the FMR change had no impact on changes in nonvoucher prices from 2010 to 2011. Since all areas in Dallas had the same FMR in 2010, using the FMR level in 2011 in a regression is equivalent to using the FMR change from 2010 to 2011 ($\Delta FMR_j = FMR_j^{2011} - const$).

5.1 Impacts on Voucher Prices and Building Quality

We examine the impacts of this policy change on price and building quality. Our estimating equation is

$$y_{ijt} = \alpha + Post_t + \beta_{pre} FMR_j + \beta_{post} FMR_j Post_t + \varepsilon_{ijt} \tag{8}$$

[30]This policy 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. This policy is known as the "Small Area FMR Demonstration."

where j indexes ZIP codes and $Post_t$ is a dummy for 2011.[31]

Rents were highly responsive to the policy change, as shown in Figure 5 for 2-bedroom units. Pooling variation from all bedroom sizes, Table 3 reports results from equation 8, with the price of voucher units at the dependent variable. We find that for every dollar increase in FMR, prices for stayers rose by 5 cents. Among movers, we find substantial price increases in more expensive areas and price decreases in cheaper areas; every \$1 change in FMR was associated with a 37 cent change in rents.[32] This could reflect changes in landlord pricing or unit quality. Average rents among movers remained about constant and implementation cost about \$10 per household.[33]

We examine whether this change in the schedule led voucher recipients to move to higher-quality buildings. Again, we use hedonic estimates, as described in Appendix B.4. 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). Figure 5 plots hedonic structure quality as a function of ZIP code-level FMR for 2-bedroom units. Physical structure quality rises where FMR increases and falls where FMR decreases. Next, again pooling variation from all bedroom sizes, we re-estimate

[31]Since this intervention was done with the cooperation of local housing authorities, there were no changes in local price ceilings to offset the FMR changes. Instead of using FMR changes to instrument for price ceiling changes, as we did in the previous section, we simply use FMR changes as the regressor.

[32]When we compute the analogous statistic (the price change for *all* movers) using the county-level research design, we find that a \$1 increase in the price ceiling also caused a 39 cent increase when comparing movers in 2004 to movers in 2005.

[33]Given the tendency of voucher recipients to live in poor, 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 poorest neighborhoods to 10% for the most wealthy neighborhoods. When FMR was calculated as the median rent of all units in Dallas, this estimate was 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; using the neighborhood quality index described below, median rents are the same in neighborhoods with an index of -4 and an index of -1. Together, these forces meant that had no one moved to better neighborhoods, voucher recipients would have experienced only small FMR decreases on average (median \$24, mean \$18). Implementation cost estimate comes from correspondence with Matthew Hogan of Dallas Housing Authority, October 23, 2012.

equation 8, using hedonic quality as the dependent variable. In 2010, voucher recipients who lived in higher-quality neighborhoods had lower structure quality, as would be expected with the existence of a single, metro-wide price ceiling. We find that for every dollar increase in the FMR, structure quality for movers rose by 13 cents, as reported in Table 3.

5.2 Impacts on Neighborhood Quality

We assemble data at on five measures of neighborhood quality: poverty rate, 4th grade test scores at zoned school, unemployment rate, share with a bachelor's degree, and violent crime rate.[34] We compute a neighborhood quality index, which equally weights all five measures. 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 (see Appendix Map 1). Map 3 also shows the change in voucher counts at the tract level from 2010 to 2011. A black dot indicates a net increase, a green dot represents a net decrease, and the size of the dot indicates the magnitude of the change. Voucher recipients appear to be exiting the lowest-quality neighborhoods in the inner city, moving further south and east to better neighborhoods.

To formally estimate the impact of the change to ZIP code-level FMRs, we use a simple difference-in-difference design to ask whether movers from 2010 to 2011 chose better neighborhoods than those chosen by movers from 2009 to 2010. We compute a second difference by comparing quality for stayers from 2009 to 2010 and stayers from 2010 to 2011. The identifying assumption is that quality difference between movers and stayers would be stable absent the

[34] 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.

policy intervention. Specifically, we estimate

$$Quality_{ij} = \alpha + Move_{ijt} + Post_t + \beta_{post} Move_{ijt} Post_t + \varepsilon_{ijt}$$

where i indexes households and j indexes tracts. The results are shown in Table 4, where β_{post} shows an improvement of 0.2 standard deviations in quality. This estimate is statistically precise, with a t-statistic greater than 5 using standard errors clustered at the tract level and also using Conley (1999) standard errors.[35]

Three additional pieces of evidence are consistent with attributing this quality increase to the introduction of ZIP code-level FMRs. Figure 6 shows that this is the biggest single-year quality increase for movers observed since 2005 in Dallas.[36] In contrast, average neighborhood quality in Fort Worth, Texas, which had no policy change, is largely stable. Finally, Map 3 shows that the improvement in neighborhood quality was broad-based, and not driven by moves to or away from a single neighborhood.

We compare the neighborhood quality impacts in Dallas to other policy interventions; they are smaller than those for vouchers offered to people exiting public housing and compare favorably to offering vouchers to people living in private housing.[37] Two prominent studies with random assignment of vouchers to people exiting public housing are the Moving to Opportunity (MTO) experiment (Kling et al. (2005)) and voucher random assignment in Chicago (Jacob et al. (2013)). The improvements in those studies were unusually large and dif-

[35]We thank Pat Kline for generously sharing his code with us to compute spatially-correlated standard errors. Appendix B.5 discusses our computation procedure and choice of bandwidth.

[36]There is more volatility in the movers series than in the stayers series in Dallas. In particular, there is a notable increase in destination quality for movers from 2008 to 2009. Raw sample counts for stayers plus movers from 2007, 2008, and 2009 are 26,722, 23,609, and 28,630. It appears that under-coverage in 2008 may have generated additional volatility in the movers index, and is reflected in the larger standard errors for 2008.

[37]In Appendix Table 6, we provide more detail on these other studies. Ideally, we would compare the Dallas results to impacts of other mobility assistance programs (e.g. counseling), but, in the words of one recent literature review, "[t]here is very little empirical research on the efficacy of housing mobility assistance programs" (Cunningham et al. (2010)).

ficult to scale up because participants were leaving very-distressed public housing, and in MTO, some participants were mandated to choose a low-poverty tract.[38] Other studies consider random assignment of vouchers to people without any subsidized housing. Jacob and Ludwig (2008) find in Chicago that tract poverty rates are 1 percentage point lower for voucher recipients (not significant). Abt Associates (2006) and Eriksen and Ross (2011) find in a six-site study that poverty rates are 2 percentage points lower, with no significant change in crime victimization rates. In contrast, the early results from Dallas which show moves to places with significantly improved poverty and violent crime rates make this intervention seem attractive. However, more years of follow-up data are needed to fully assess the impacts of ZIP-level FMRs.

The neighborhood quality improvements here stand in sharp contrast to the county-level price ceiling results in Section 4. However, our model offers a potential reconciliation. Across-the-board price ceiling increases may raise the matching probability more in low-quality neighborhoods than in high-quality neighborhoods, leading to an ambiguous effect on quality. Targeted price ceiling increases, however, always increase incentives to move to high-quality neighborhoods.

6 Conclusion

We examine the incidence of a narrowly-targeted voucher program, relaxing the textbook assumption that suppliers are price-takers, and allowing for consumer search frictions. Our assumptions provide a realistic description of housing vouchers in the U.S. Using quasi-experimental variation, we find that rental prices are responsive to price ceiling changes, while quality is less responsive. In particular, we find evidence of price discrimination, because prices respond even for tenants who stayed at the same address. There may be welfare gains to reducing price ceilings and using the savings to pay for more vouchers.

[38] See Galiani et al. (2012) for recent work on the difficulty of implementing location constraints.

A new HUD demonstration in Dallas which linked price ceilings to neighborhood quality shows dramatic first year results. After this intervention, voucher recipients in Dallas chose neighborhoods with substantially lower violent crime rates and lower poverty rates, and the net cost of the intervention was zero. While more years of follow-up data are needed, the initial results are promising.

Our emphasis on price discrimination may be useful for studying other voucher-like programs, including college financial aid, the Earned Income Tax Credit, federal nutrition programs, and child care vouchers. We estimate that more than half of existing transfers to the nonelderly are characterized by tagging and private provision. Policymakers' interest in vouchers is growing; the Affordable Care Act will provide an estimated 20 million people with subsidized vouchers, and several recent proposals have discussed turning Medicare into a voucher. As vouchers become increasingly prevalent, future research should try to estimate the extent of price discrimination and the impact of voucher generosity on quality for other voucher programs.

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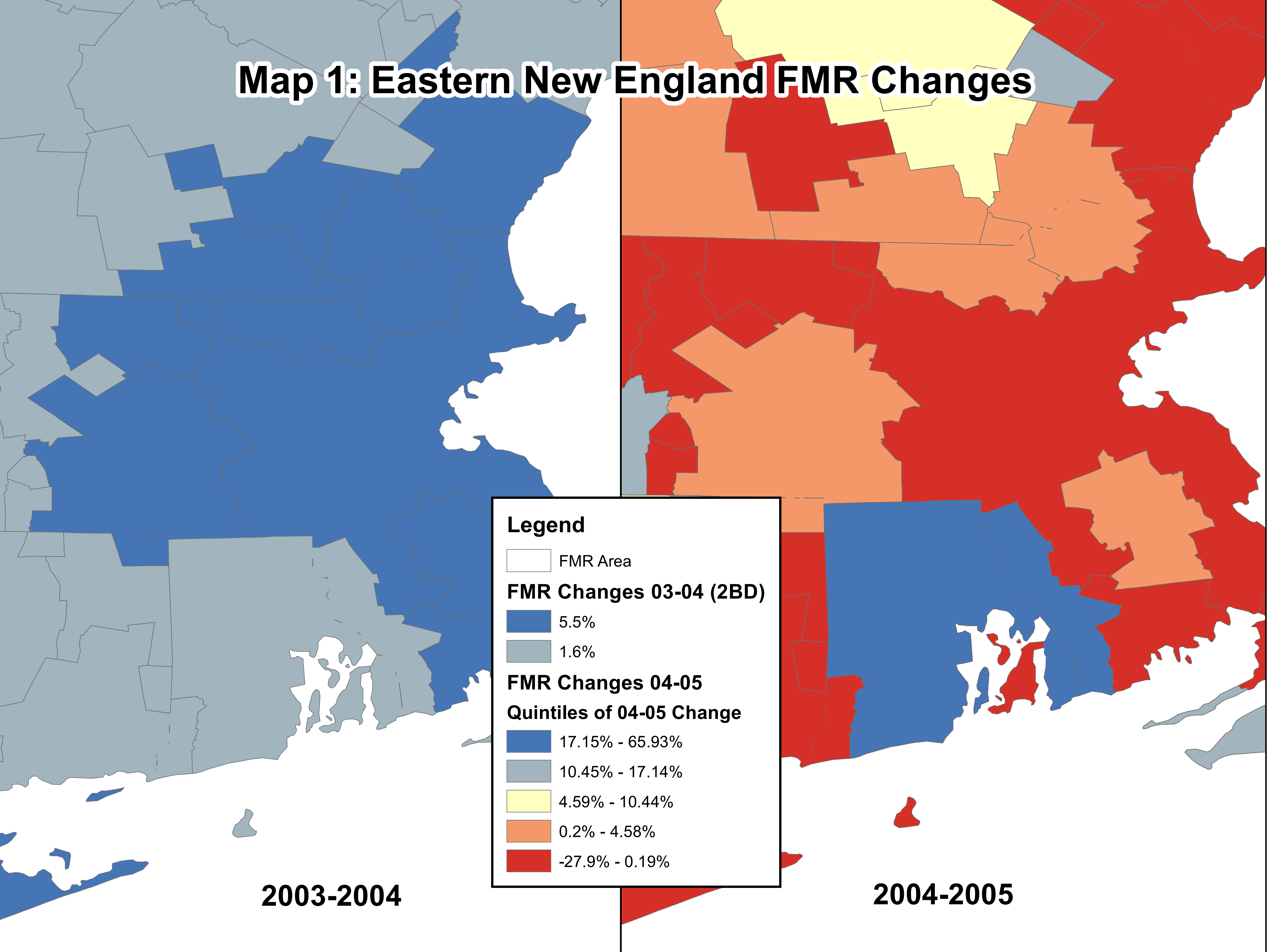
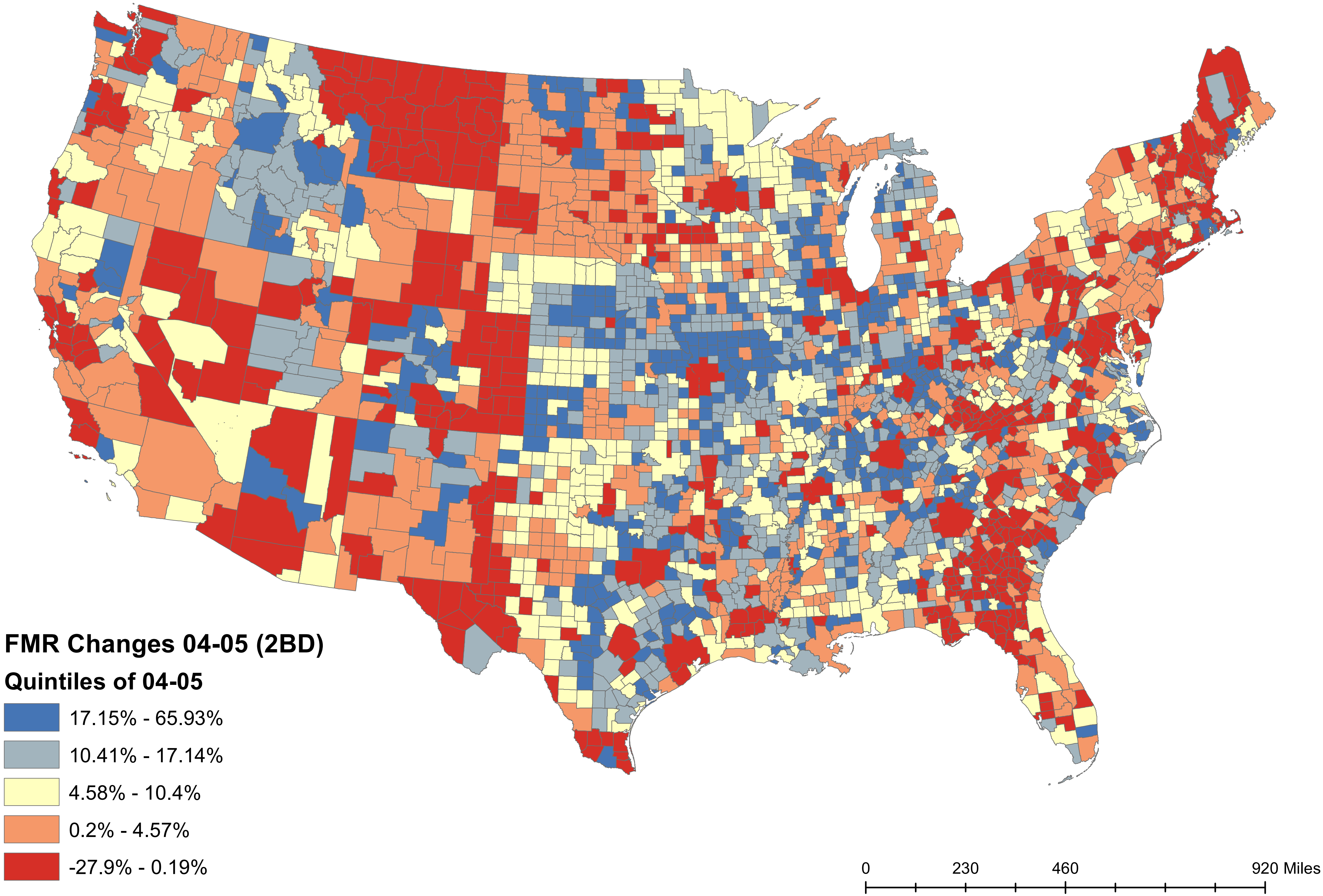
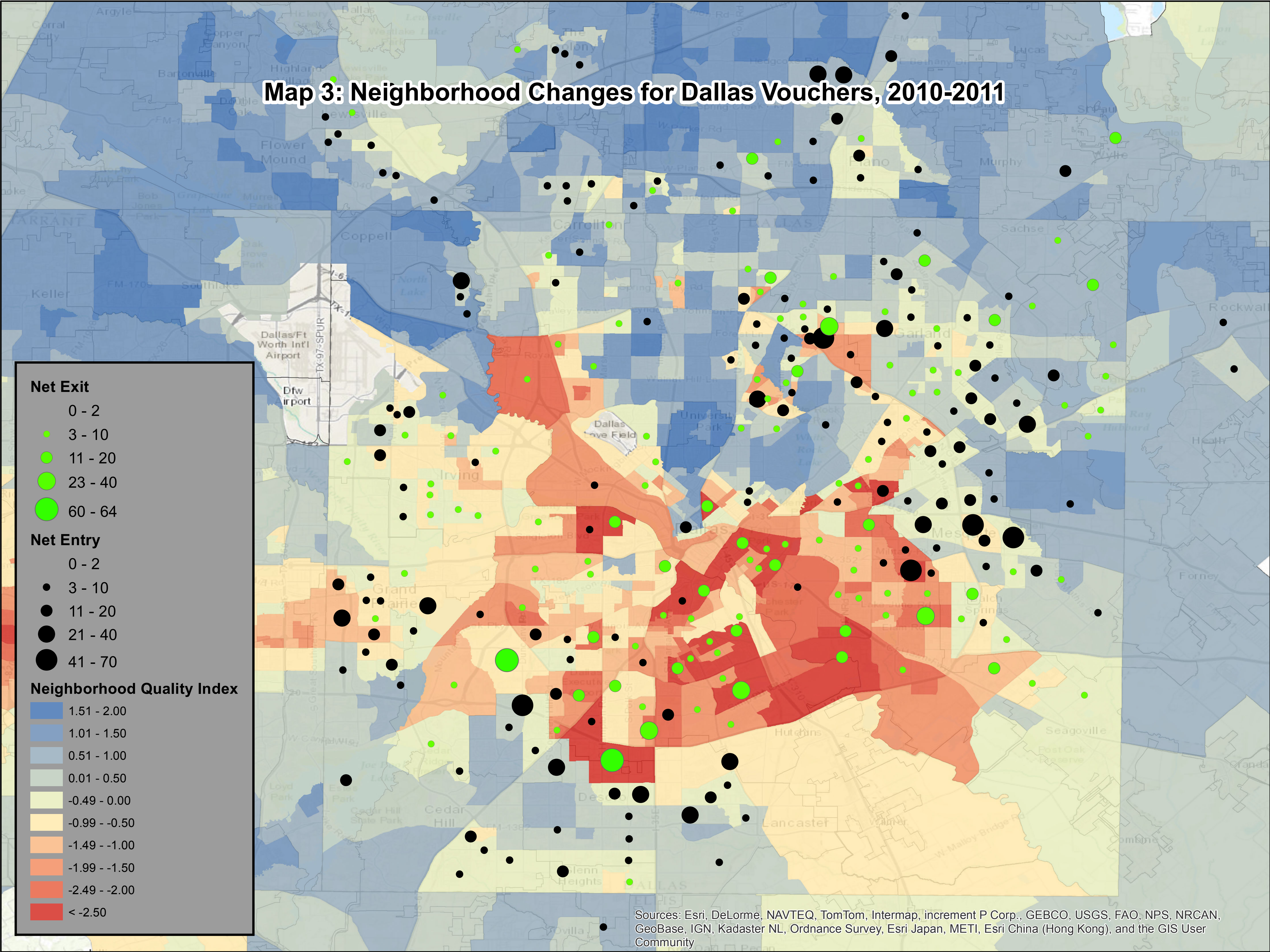
Map 1: Eastern New England FMR Changes



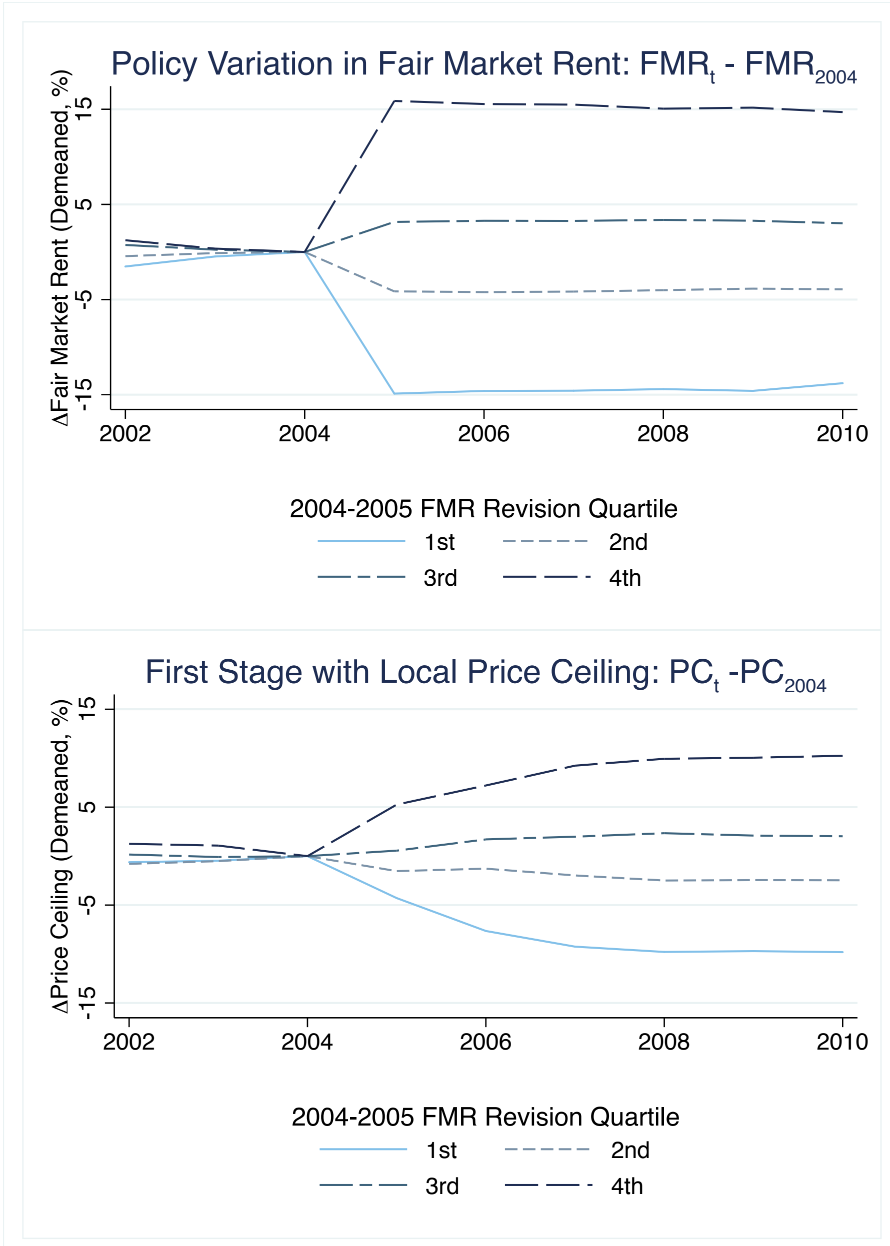
Map 2: National Fair Market Rent Rebenchmarking, 2004-2005





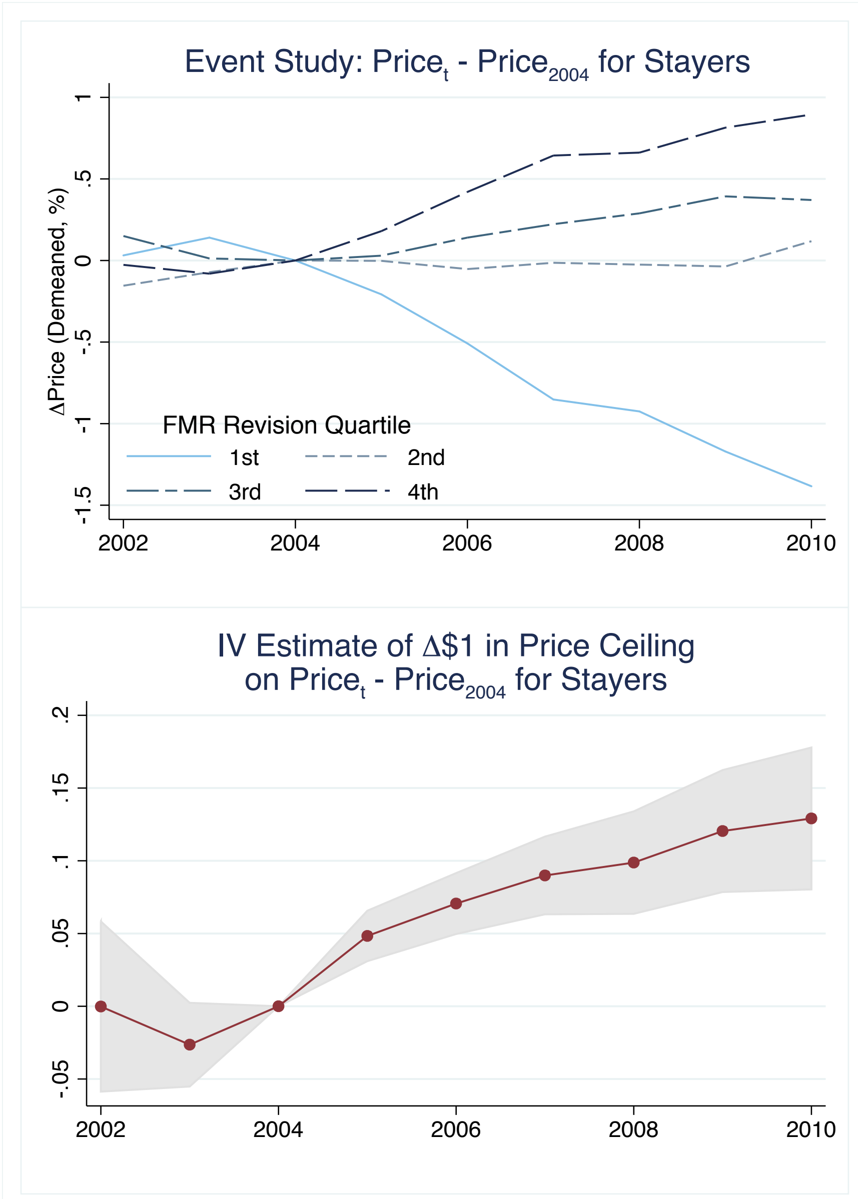
Map 3: Neighborhood Changes for Dallas Vouchers, 2010-2011

FIGURE 1 – Event Study for County-Level Changes



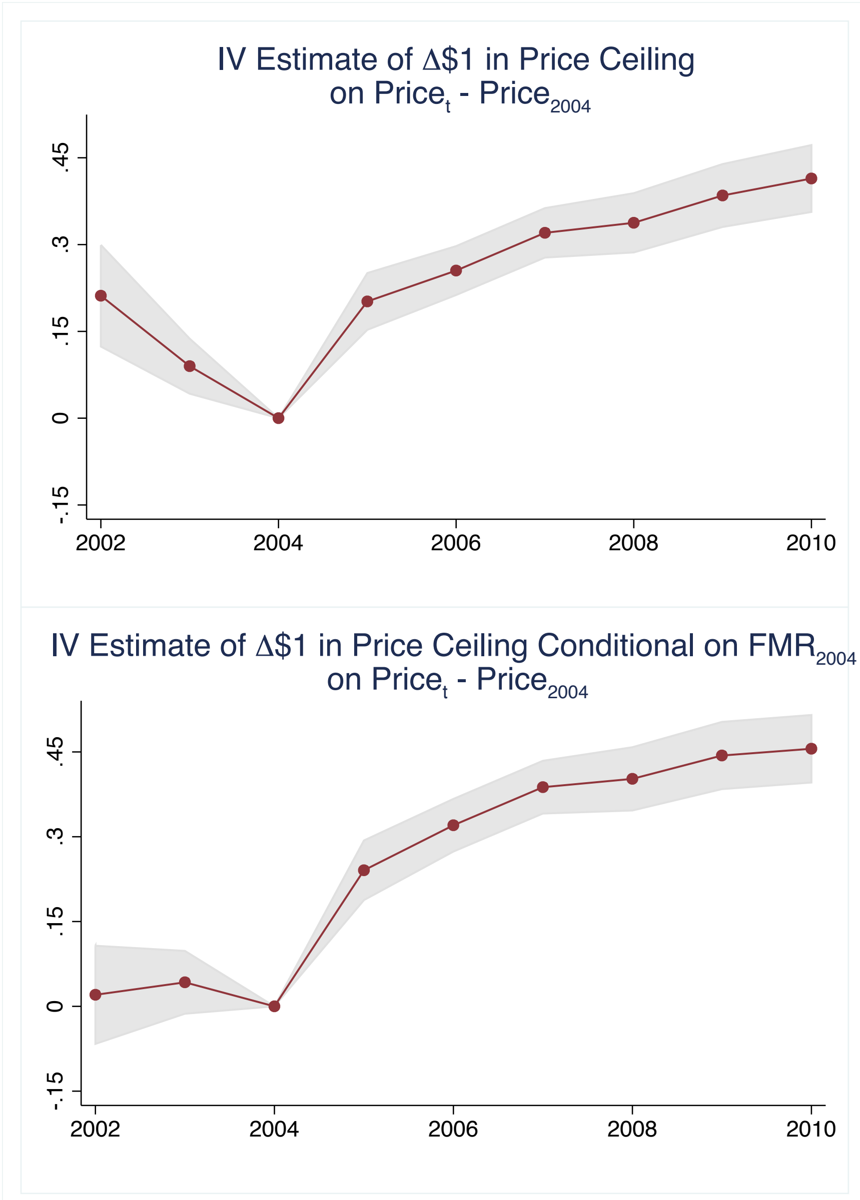
Notes: Each year, the federal government publishes "Fair Market Rents." These are typically estimated as the 40th percentile of rent in a county for studios, 1 bedroom, 2 bedroom, 3 bedroom and 4 bedroom units. In 2005, the government made large revisions to incorporate newly-available data from the 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 price ceiling at 90%, 100% or 110% of Fair Market Rent. The bottom panel plots local price 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 price ceilings rose by 70 cents.

FIGURE 2 – Price Discrimination for County-Level Changes



Notes: This figure analyzes changes in rents for voucher recipients who lived at the same address in 2002-2003 and 2005-2010 as they did in 2004. The top panel plots conditional means in four bins, stratified by the size of the FMR change from 2004 to 2005. In the bottom panel, each point represents coefficient β from the IV regression with second stage $p_t - p_{2004} = \alpha + \beta \Delta PriceCeiling_t + \varepsilon$, and first stage $\Delta PriceCeiling_t = \alpha + \gamma \Delta FMR + \eta$. The shaded area is a 95% confidence interval. Rental data from 2002 and 2003 are a test for pretrends, and the 2004-2005 first stage is used. The sample size is shrinking over time: n=938,803 in 2005 and shrinks in each subsequent year to n=290,731 in 2010. See notes to Table 1 for details on estimates and standard errors.

FIGURE 3 – Price Impacts for County-Level Changes

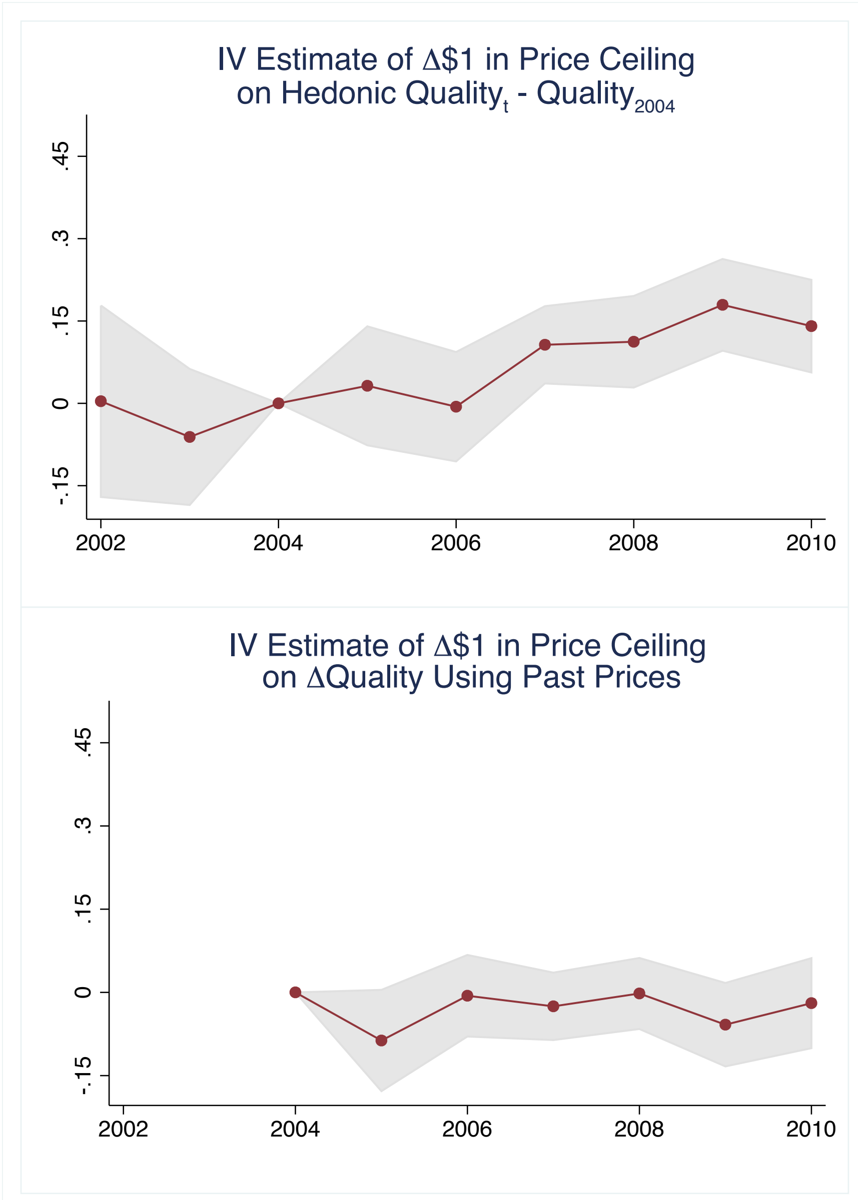


Notes: This figure analyzes changes in rents for the full voucher sample.

The top panel plots β coefficients from the IV regression with second stage $p_t - p_{2004} = \alpha + \beta \Delta PriceCeiling_t + \varepsilon$, and first stage $\Delta PriceCeiling_t = \alpha + \gamma \Delta FMR + \eta$. The shaded area is a 95% confidence interval. Rental data from 2002 and 2003 are a test for pretrends, and the 2004-2005 first stage is used. Prices were *rising* in places which received a negative shock to FMR in 2005 and *falling* in places which received a positive shock to FMR.

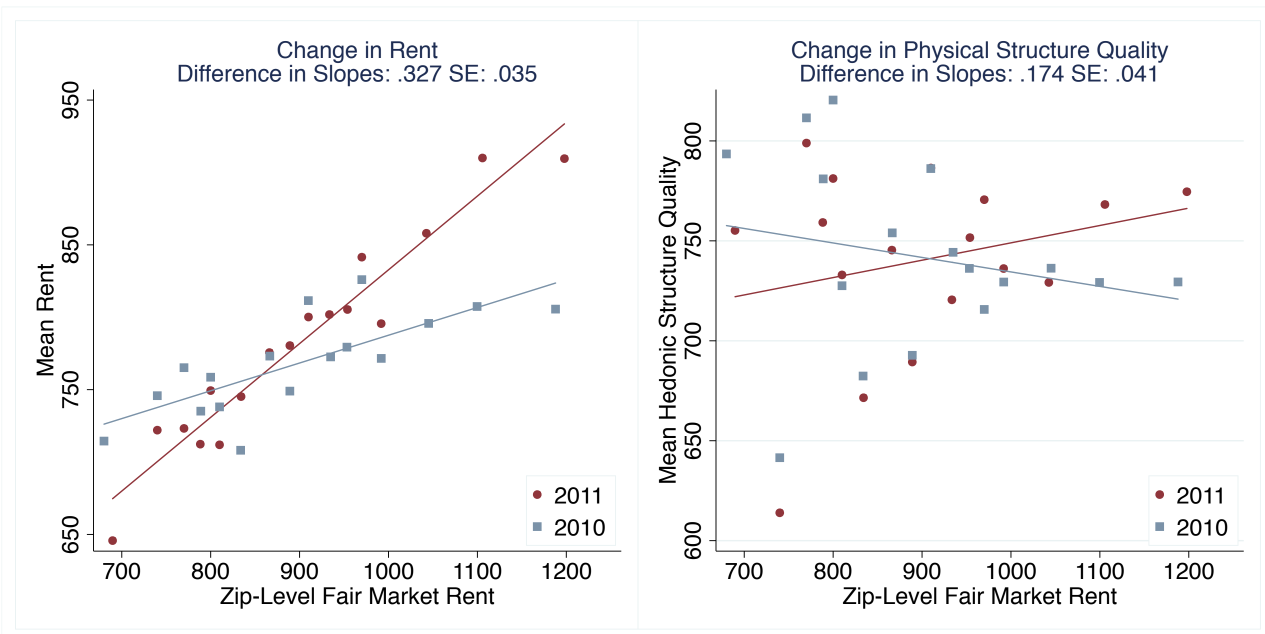
The bottom panel uses first stage $PC_t = \alpha + FMR_{2005} + FMR_{2004} + PC_{2004} + \eta$ and second stage $p_t - p_{2004} = \alpha + \beta \widehat{PC}_t + FMR_{2004} + PC_{2004} + \varepsilon$, where FMR_{2005} is treated as the excluded instrument. This strategy corrects for mean reversion due to measurement error, and the pretrends are much less pronounced.

FIGURE 4 – Quality Impacts for County-Level Changes



Notes: This figure analyzes changes in unit quality associated with the 2005 FMR rebenchmarking. The top panel plots coefficients from the IV regression with second stage $\Delta q_t = \alpha + \beta \Delta PriceCeiling_t + \varepsilon$, and first stage $\Delta PriceCeiling_t = \alpha + \gamma \Delta FMR + \eta$, where Δq is the change in hedonic quality. Hedonic coefficients are estimated on nonvoucher units in 2005-2009 ACS using PUMA, number of bedrooms, structure type, and structure age; these coefficients are applied to the voucher data to predict unit quality. In the bottom panel, the change in unit quality is measured for within-county movers as the difference between rent paid by a different pre-2005 tenant at a voucher recipient's new residence and the rent paid at a voucher recipient's pre-2005 residence. See notes to Table 2 for details.

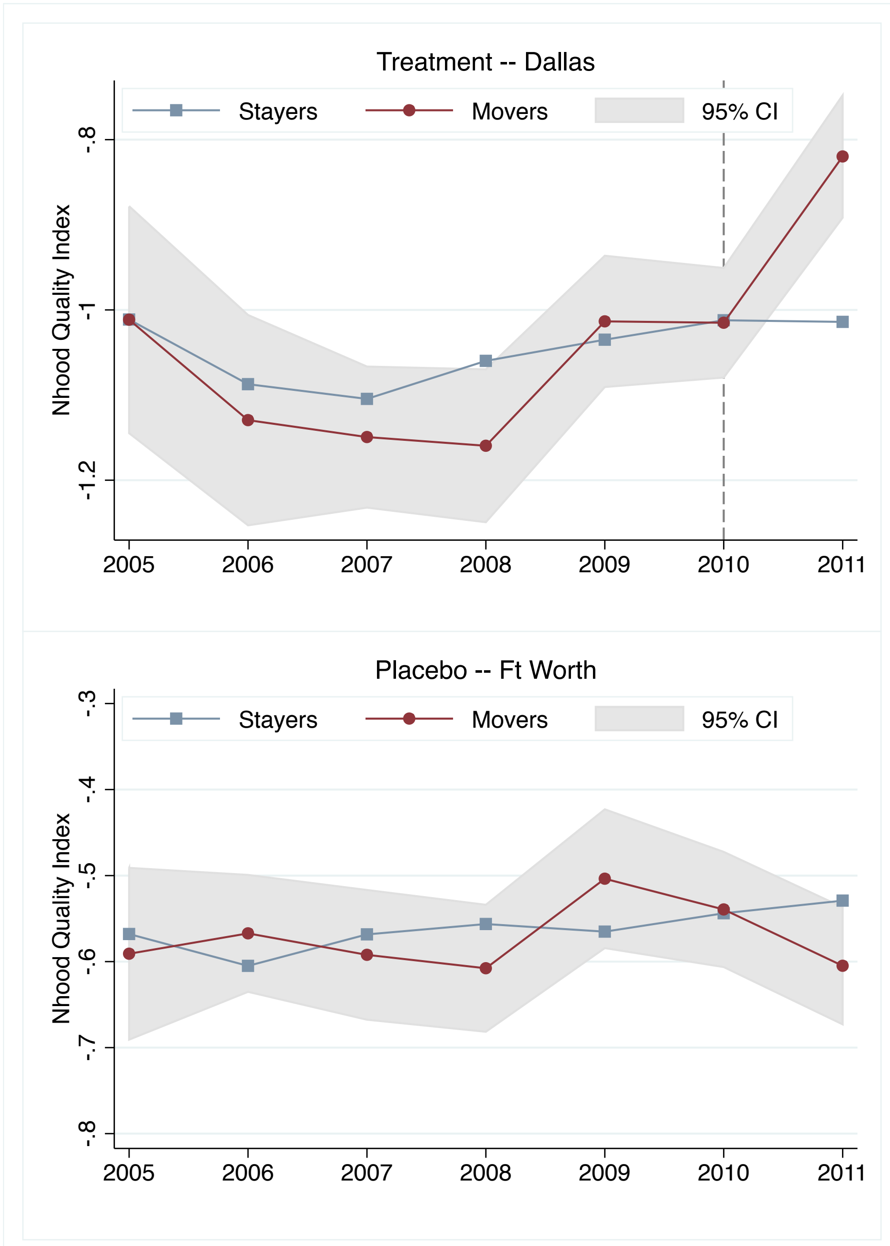
FIGURE 5 – Rent and Building Quality Impacts for Dallas ZIP-level Price Ceiling Demo



Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs. The top panel shows that this policy raised price ceilings in expensive neighborhoods and lowered price ceilings in cheap neighborhoods. Dots reflect conditional means for 2-bedroom units for 20 quantiles of the ZIP code-level FMR distribution. We show data only for households which moved in 2010 or 2011.

The bottom left panel shows that mean rents were quite responsive to the new price schedule. The right panel shows modest responses in hedonic structure quality. Hedonic coefficients are estimated on nonvoucher units in 2005-2009 ACS and applied to the voucher data to predict the quality of the physical structure. See Section 5.2 for details.

FIGURE 6 – Neighborhood Quality Impacts for Dallas ZIP-level Price Ceiling Demo



Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs, raising price ceilings in expensive neighborhoods and lowering price 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 BA, and violent crime rate. The index is normalized to have mean zero and unit standard deviation with respect to the entire Dallas metro area. This figure shows average destination neighborhood quality for households that stayed in place and moved in the two-year period ending with the year listed on the x-axis. We report a 95% confidence interval for destination quality for movers using standard errors clustered at the tract level. It appears that ZIP code-level price ceilings caused voucher recipients to move to higher-quality neighborhoods.

Table 1 - Effect of County-Level Price Ceiling Increase on Voucher Prices

Sample	Stayers[a] (1)	Movers[b] (2)	Stayers[c] (3)	All Tenants (4)
First Stage	Dep Var: ΔLog Price Ceiling, 2004-2010			
ΔLog Fair Market Rent, 2004-2005	0.675	0.701	0.847	0.666
	(0.048)	(0.049)	(0.040)	(0.0422)
Two Stage Least Squares	Dep Var: ΔLog Voucher Rent, 2004-2010			
ΔLog Price Ceiling, 2004-2010	0.125	0.199	0.185	0.414
	(0.025)	(0.036)	(0.024)	(0.029)
Unit of Observation	Address	Address	Address	County-Bed
County Fixed Effects	No	No	Yes	No
n	290,731	553,577	290,731	12,375

Notes: This table shows the impact of a countywide increase in the price ceiling on rents, using variation from the 2005 Fair Market Rent (FMR) rebenchmarking. Standard errors shown in parentheses are clustered at FMR group level (n=1,484). Rent changes in the address-level sample are winsorized at the 1st and 99th percentile. Regressions give equal weight to each county-bed pair. See Sections 4.2 and 4.3 for details.

a. Stayers sample contains households whose address (9-digit zip code) was unchanged from 2004 to 2010.

b. Movers sample contains addresses where a new voucher recipient arrived in 2005 or later and another voucher recipient was observed in 2003 or 2004. 41% of movers and new admits from 2005-2010 went to an address that was occupied by a different voucher recipient in 2003 or 2004.

c. This specification adds county fixed effects to the stayers sample, so that identification comes from within-county cross-bedroom changes in FMR (e.g. the change in FMR for 1-bedroom units compared to the change in FMR for 2-bedroom units within the same county).

Table 2 - Effect of County-Level Price Ceiling Increase on Voucher Quality

Time Horizon	2004-2010			
Dependent Variable	ΔLog Hedonic Quality[a]	ΔLog Median Tract Rent[b]	Log Prior Price Arrival Unit - Log Price Departed Unit[c]	
	(1)	(2)	(3)	(4)
ΔLog Price Ceiling, 2004-2010	0.141	-0.005	-0.019	-0.370
	(0.043)	(0.013)	(0.041)	(0.251)
Control for Log FMR, 2004	No	No	Yes	Yes
Unit of Observation	County-Bed		HH Moving to Unit Previously Occupied by Voucher	
Move Destination	N/A	N/A	Same County	Different County
n	12,096	12,333	104,024	82,505

Notes: This table shows the impact of a countywide increase in the price ceiling on rents, using variation from the 2005 Fair Market Rent rebenchmarking. Standard errors shown in parentheses are clustered at FMR group level (n=1,484). See Section 4.4 for details.

a. Hedonic Quality in column 1 is measured for all units. Hedonic coefficients are estimated on nonvoucher units in 2005-2009 ACS using Public Use Microdata Area, number of bedrooms, structure type, and structure age; these coefficients are applied to the voucher units to predict unit quality. See Appendix B.4 for details on hedonics. We then collapse the data to the county-bedroom level.

b. Tract Quality in column 2 is measured by median rent of a unit's Census tract, as estimated in the 2005-2009 ACS. We then collapse the data to the county-bedroom level.

c. Unit-Level Quality is measured for voucherholders who moved to a unit that was previously occupied by a different voucherholder. We use the realized price ceiling change from place of origin in 2004 to destination in 2010, instrumenting with the 2004-2005 FMR change in place of origin. 41% of movers and new admits from 2005 to 2010 went to a unit that was occupied by a different voucherholder in 2003 or 2004. This measure of quality change is quite volatile, and is winsorized at the 5th and 95th percentile. Regressions give equal weight to each county-bed pair.

Table 3 - Effect of Dallas ZIP-level Price Ceiling Demo on Prices and Building Quality

Dependent Variable	Log Rent[a]		Log Hedonic Physical Structure Quality[b]
Sample	Stayers, ΔFMR > 0	Movers	Movers
	(1)	(2)	(3)
Log ZIP FMR*2011	0.052	0.371	0.134
	(0.015)	(0.027)	(0.027)
Log ZIP FMR	0.353	0.255	-0.127
	(0.052)	(0.031)	(0.044)
Indicators for Bedroom-Year	Yes	Yes	Yes
n	15,644	9,049	9,049

Notes: This table shows the price and building quality impact of moving from a single, metro-wide FMR in Dallas in 2010 to ZIP-level FMRs in 2011. The coefficient in the first row, "Log ZIP FMR*2011", is the treatment estimate for the effect of a $1 price ceiling change on rents and unit quality. The coefficient in the second row, "Log ZIP FMR" gives the baseline, pre-treatment relationship of the dependent variable with future ZIP-level FMRs. Standard errors are clustered by ZIP (n=116 for stayers, 132 for movers). See Section 5.1 for details.
a. Rent Column 1 sample uses observations where the tenant stayed in the same unit from 2010 to 2011. FMRs were allowed to rise but not fall for stayers in the first year of the new policy because of a "hold-harmless" provision. Sample is limited to places where FMRs rose.
Column 2 sample uses movers in 2010 and 2011. No hold-harmless provision applied to movers.
b. Hedonic Physical Structure Quality Column 3 repeats column 2 with hedonic physical structure quality. Hedonic coefficients are estimated on nonvoucher units in 2005-2009 ACS using Public Use Microdata Area (PUMA), number of bedrooms, structure type, and structure age; the coefficients for building-related covariates (but not PUMA) are applied to the voucher data to predict building quality. See Appendix B.4 for details on hedonics.

Table 4 - Effect of Dallas ZIP-level Price Ceiling Demo on Neighborhood Quality

	Stayers		Movers		Differences		Diff-in-Diff	Std Error[e]	St'dized Effect[f]
	Pre	Post	Pre	Post	(2)-(1)	(4)-(3)	(6)-(5)		(7)/SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
n	18,711	18,927	4,923	4,170					
Poverty Rate[a]	0.209	0.209	0.208	0.187	0.000	-0.021	-0.021	(0.004) [0.004]	0.187
Test Score[b]	-0.499	-0.498	-0.519	-0.450	0.001	0.069	0.068	(0.032) [0.025]	0.068
Unemployment	0.107	0.106	0.104	0.100	0.000	-0.004	-0.004	(0.002) [0.002]	0.096
Share BA	0.184	0.185	0.180	0.198	0.001	0.018	0.017	(0.004) [0.004]	0.081
Violent Crime[c]	0.0147	0.0149	0.0151	0.0129	0.0002	-0.0022	-0.0023	(0.0005) [0.0005]	0.312
Nhood Index[d]	-1.010	-1.012	-1.013	-0.818	-0.002	0.195	0.197	(0.036) [0.037]	0.197

Notes: This table shows the neighborhood quality impact of moving from a single, metrowide FMR in Dallas in 2010 to ZIP-level FMRs in 2011. See Section 5.2 for details.

a. Poverty rate, unemployment, and share with a bachelor's degree are ACS tract-level data from 2006 to 2010.

b. Percent of 4th grade students' scoring proficient or higher on state exams in the 2008-2009 academic year at zoned school. Proficiency rates are standardized to have mean zero and unit standard deviation over blockgroups in the Dallas Core Based Statistical Area.

c. Violent Crime is number of homicides, nonnegligent 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.

d. Index is an equally-weighted sum of the five measures, standardized to have mean zero and unit standard deviation.

e. Standard errors for Diff-in-Diff estimate in column (7) are clustered at the tract level are in parentheses and Conley (1999) standard errors with a bandwidth of ten miles are in square brackets. See Appendix B.5 for details.

f. Standardized effect is Diff-in-Diff estimate with each measure re-oriented so that positive indicates an improvement, divided by standard deviation for all census tracts in the Dallas Core Based Statistical Area.

A Theory Appendix

A.1 Generalizations of Tenant's Problem

The model in Section 2 uses this functional form to describe voucher recipients' quality choice

$$\max_q \underbrace{\mathbb{P}(q,\bar{p})}_{\text{match probability}} \underbrace{q}_{\text{quality}}$$

$$\text{First Order Condition} \qquad q + \frac{\mathbb{P}(q,\bar{p})}{\frac{\partial}{\partial q}\mathbb{P}(q,\bar{p})} = 0$$

which assumes linear utility in unit quality, a representative agent, an outside option of zero, and that tenants can search in only one quality submarket. We relax the first three assumptions all at once. Then, separately, we allow tenants to search sequentially in submarkets of different qualities.

A.1.1 Alternative Objective Function

Let i index different individuals, and let $\underline{q}_i$ be individual i's outside option. Heterogeneity in outside options generates heterogeneity in quality choices. All individuals have utility over unit quality $u(q)$, with $u' > 0, u'' < 0$. Now the tenant's maximization problem is

$$V_i = \max_q \mathbb{P}(q,\bar{p})u(q) + (1 - \mathbb{P}(q,\bar{p}))u(\underline{q}_i)$$

$$\text{First Order Condition} \qquad \left(u(q) - u(\underline{q}_i)\right) + \frac{\mathbb{P}(q,\bar{p})}{\frac{\partial}{\partial q}\mathbb{P}(q,\bar{p})}u'(q) = 0$$

If $\frac{\mathbb{P}(q,\bar{p})}{\frac{\partial}{\partial q}\mathbb{P}(q,\bar{p})}$ is strictly decreasing in q, then there is a unique, global solution, with $q^* > \underline{q}_i$. This requires the regularity condition already specified in Section 2.1. Next, we use the implicit function theorem to define $q^*(\bar{p})$ and use $\mathbb{P} = \mathbb{P}(q,\bar{p})$ for shorthand. The first order condition can be written as

$$\frac{\partial \mathbb{P}}{\partial q}\left(u(q^*(\bar{p})) - u(\underline{q}_i)\right) + \mathbb{P}u'(q^*(\bar{p})) = 0$$

Differentiating with respect to $\bar{p}$ and solving for $\frac{\partial q^*}{\partial \bar{p}}$ gives

$$\frac{\partial q^*}{\partial \bar{p}} = \frac{-\frac{\partial \mathbb{P}}{\partial \bar{p}}u'(q^*(\bar{p})) - \frac{\partial^2 \mathbb{P}}{\partial q \partial \bar{p}}\left(u(q^*(\bar{p})) - u(\underline{q}_i)\right)}{\frac{\partial \mathbb{P}}{\partial q}u'(q^*(\bar{p})) + \mathbb{P}u''(q^*(\bar{p}))}$$

The terms in the numerator are the "increased average acceptance" effect and the "marginal landlord participation" effect, just as before, although they are now rescaled by utility values. The denominator is still negative, although the algebra is a bit more involved.

A.1.2 Finite Sequence Problem

Tenants have K draws as before, but now we allow them to draw sequentially from different quality submarkets. We also assume that the hazard rate of G is weakly monotone decreasing ($\partial g(\delta)/\partial\delta \leq 0$). Formally, the tenant solves

$$\max_{\{q_1 \dots q_K\}} \sum_{j=1}^{K} \underbrace{\prod_{i=1}^{j-1} (1 - G(\bar{p} - q_i))}_{\text{Probability of reaching draw j}} \underbrace{G(\bar{p} - q_j) q_j}_{\text{Expected utility of draw j}}$$

This problem can be solved using backwards induction.

Date K The tenant solves

$$\begin{aligned} V_K &= \max_{q_k} G(\bar{p} - q_K) q_K \\ \text{First Order Condition} \quad & g(\bar{p} - q_K^*) q_K^* = G(\bar{p} - q_K^*) \end{aligned}$$

which has a unique global solution q_K^*.

Date $K-1$ The tenant solves

$$\begin{aligned} V_{K-1} &= \max_{q_{K-1}} G(\bar{p} - q_{K-1}) q_{K-1} + (1 - G(\bar{p} - q_{K-1})) V_K \\ \text{First Order Condition} \quad & g(\bar{p} - q_{K-1}^*)(q_{K-1}^* - V_K) = G(\bar{p} - q_{K-1}^*) \end{aligned}$$

Proceeding by induction, this defines a unique sequence of quality choices $\{q_1^* \dots q_K^*\}$.

Next, we consider the effect of a change in the price ceiling on quality choices. Differentiating the first order condition for q_K^* with respect to $\bar{p}$ gives

$$\frac{\partial q_K^*}{\partial \bar{p}} = \frac{g(\bar{p} - q_K^*) - \frac{\partial g(\bar{p} - q_K^*)}{\partial \delta}}{2g(\bar{p} - q_K^*) - \frac{\partial g(\bar{p} - q_K^*)}{\partial \delta}} > 0$$

Differentiating the first order condition for q_{K-1}^* with respect to $\bar{p}$ gives

$$\frac{\partial q_{K-1}^*}{\partial \bar{p}} = \frac{g(\bar{p} - q_{K-1}^*) - \frac{\partial g(\bar{p} - q_{K-1}^*)}{\partial \delta} + \frac{dV_K}{d\bar{p}}}{2g(\bar{p} - q_{K-1}^*) - \frac{\partial g(\bar{p} - q_{K-1}^*)}{\partial \delta}}$$

Using the envelope theorem

$$\frac{dV_K}{d\bar{p}} = g(\bar{p} - q_K^*) > 0 \Rightarrow \frac{\partial q_{K-1}^*}{\partial \bar{p}} > 0$$

Proceeding by induction, an increase in the price ceiling raises quality chosen at every date

j. This is a consequence of our assumption that $(\partial g(\delta)/\partial\delta \leq 0)$. However, much as in simple model, the size of the increase is determined by the measure of landlords on the margin $g(\bar{p}-q)$.

A.2 Price Determination Alternative

Here, we define a broader set of admissible price schedules.

> $p^{voucher}(q;\bar{p}) \in [q,\bar{p}]$, and has two weak monotonicity restrictions
>
> Weak Monotonicity in Quality: $\frac{\partial}{\partial\bar{p}}p^{voucher}(q;\bar{p}) \geq 0$. When the price ceiling goes up, local government's offers will go up weakly.
>
> Weak Monotonicity in Price Ceiling $\frac{\partial}{\partial q}p^{voucher}(q;\bar{p}) \in [0,1)$. Local government pays weakly more for higher quality units. However, the payment schedule rises less than 1-for-1 with quality increases.

This description admits a wide variety of price-setting mechanisms. The remainder of the model is solved by substituting $G(p^{voucher}(q;\bar{p})-q)$ for $G(\bar{p}-q)$. $p^v(q)$ is an entire schedule and so the existence of $\frac{\partial q^*}{\partial\bar{p}}$ requires the regularity condition in footnote 14.

A.3 Existence of a Unique Solution

Voucher recipients trade off increased quality (first term) with a lower probability of matching (second term). The value of q where the benefits of searching in a higher submarket q are equated with the costs is the q^* which solves this equation.

$$\text{First Order Condition} \qquad q + \frac{\mathbb{P}(q,\bar{p})}{\frac{\partial}{\partial q}\mathbb{P}(q,\bar{p})} = 0$$

We can show that q^* exists and is unique if the second term is strictly decreasing in q. Intuitively, there will only be a unique interior solution if searching in a higher submarket induces a sufficiently large decrease in the probability of matching. Our choice of $\mathbb{P} = 1-(1-G(\bar{p}-q))^K$ shows that there is a decreased probability of matching in high quality submarkets. Now, we need to be sure that this first-order probability decrease dominates any second-order effects. To ease notation, let $\mathbb{P}$ denote $\mathbb{P}(\bar{p},q)$.

$$\begin{aligned}
\frac{\partial}{\partial q}\frac{\mathbb{P}}{\frac{\partial}{\partial q}\mathbb{P}} &= \frac{1}{\frac{\partial}{\partial q}\mathbb{P}}\left(\frac{\partial\mathbb{P}}{\partial q} - \frac{\mathbb{P}}{\frac{\partial}{\partial q}\mathbb{P}}\frac{\partial^2\mathbb{P}}{\partial q^2}\right) < 0 \iff \\
\frac{\frac{\partial}{\partial q}\mathbb{P}}{\mathbb{P}} &< \frac{1}{\frac{\partial}{\partial q}\mathbb{P}}\frac{\partial^2\mathbb{P}}{\partial q^2} \\
&= \frac{-K\left(1-G(\bar{p}-q)\right)^{K-1}g(\bar{p}-q)}{-K\left(1-G(\bar{p}-q)\right)^{K-1}g(\bar{p}-q)}\left((K-1)\frac{g(\bar{p}-q)}{1-G(\bar{p}-q)} - \frac{\partial\log g(\bar{p}-q)}{\partial q}\right)
\end{aligned}$$

The probability of failing to sign a lease is $(1-G(\bar{p}-q))^K$. The hazard rate of this fail-to-sign distribution is $h_{lease}(q) = -\frac{\frac{\partial}{\partial q}\mathbb{P}}{\mathbb{P}}$. The hazard rate of the G distribution is $h_G(q) = \frac{g(\bar{p}-q)}{1-G(\bar{p}-q)}$. Substituting these terms into the above inequality yields the more intuitive representation:

$$-h_{lease}(q) < (K-1)h_G(q) - \frac{\partial \log g(\bar{p}-q)}{\partial q}$$

This constraint says that if the hazard rate for the failure to sign a lease is sufficiently low in q, then there is a unique solution q^* to the FOC. Finally, by plugging in for these hazard rates, we can recover the restrictions implied by this condition on G.

$$\begin{aligned}
\frac{-K\left(1-G(\bar{p}-q)\right)^{K-1} g(\bar{p}-q)}{1-\left(1-G(\bar{p}-q)\right)^K} &< (K-1)\frac{g(\bar{p}-q)}{1-G(\bar{p}-q)} - \frac{\partial \log g(\bar{p}-q)}{\partial q} \Rightarrow \\
\frac{\partial \log g(\bar{p}-q)}{\partial \delta} &> \frac{-K\left(1-G(\bar{p}-q)\right)^{K-1} g(\bar{p}-q)}{1-\left(1-G(\bar{p}-q)\right)^K} - \frac{(K-1)g(\bar{p}-q)}{1-G(\bar{p}-q)}
\end{aligned}$$

This is the regularity condition on G assumed in Section 2.1. It is weaker than the monotone likelihood ratio condition, which would simply assume $\frac{\partial \log g(\bar{p}-q)}{\partial \delta} > 0$.

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 Sample: We use HUD's published Fair Market Rent rates, with slight modifications.[39] 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. 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.

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.[40] 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 ($E(\Delta p_{2004-t}^{Nonvoucher}|\Delta p_{1990-2004}^{Nonvoucher}) = 0$). 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

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 Prices and 2005 FMR Rebenchmarking

In Section 4.1, our key identification condition is

$$E(\Delta p_{2004-t}^{Nonvoucher}|\Delta FMR) = 0$$

Here we examine the correlation of the FMR change with contemporaneous changes in nonvoucher prices. Data availability make it difficult to measure nonvoucher prices at a high

[39] FMRs are posted at http://www.huduser.org/portal/datasets/fmr.html

[40] All documentation associated with the rebenchmarking is posted at http://www.huduser.org/portal/datasets/fmr/fmr2005r/index.html

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,

$$Cov(\Delta\hat{p}_t, \Delta FMR) = Cov(p_t + \varepsilon_t - p_{2000} - \varepsilon_{2000}, \Delta FMR) = Var(\varepsilon_{2000}) < 0 \qquad (9)$$

Even if $E(\Delta p_t|\Delta p_{t-1}) = 0$, we estimate a negative covariance because of the negative autocorrelation of gains measured with error.

First, we compare changes in voucher prices to changes in tract-level median rents published by the Census.[41] 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

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

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 2, 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 prices had relative *decreases* in nonvoucher prices. 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).[42] Finally, we pool the observations in columns 1 and 2 to estimate $\Delta p^{\{Voucher,Nonvoucher\}}_{ij} = \alpha + \beta_1 \Delta FMR_j + \beta_2 \Delta FMR_j \times Voucher_{ij} + \varepsilon_{ij}$ 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

[41] 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.

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

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 of $E(\Delta p^{Nonvoucher}_{2004-t}|\Delta FMR) = 0$, 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 prices. The answer appears to be no. Since vouchers make up 6% of rental units in most Census tracts, we would expect to find little impact on rental prices. Susin (2002) reports that that voucher program expansions resulted in higher nonvoucher rents. Our finding that FMR changes have no impact on nonvoucher prices is more similar to results in Eriksen and Ross (2012), who finds that voucher program expansions had no measurable impact on average nonvoucher rents.

B.4 Hedonic Quality and Engel Curves for Housing

We use 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 1,458,750 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

$$\log(Rent_{ijklm}) = \alpha + Bed_j + StrucType_k + Age_l + PUMA_m + \varepsilon_i \tag{10}$$

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 2,069 PUMAs. This regression computes a vector of hedonic coefficients $\hat{\beta}_{census}$. This hedonic regression has substantial predictive power, with an R-squared of 0.45. We then apply the coefficients from this hedonic regression to the voucher covariates to construct a measure of hedonic unit quality $q^{hedonic} = \hat{\beta}_{census} x_{voucher}$. The standard deviation of actual rent is \$497 and the standard deviation of predicted rent is \$331.

B.5 Conley Standard Errors

We allow for spatial dependence neighborhood quality which decays linearly with distance using the method of Conley (1999). This method involves multiplying every observation by every other observation, which means that computational demands grow at rate $O(n^2)$ where n is the number of observations. We found that Stata crashed when attempting to compute standard errors for the analysis sample. We took a random subsample of 1/3 of the analysis sample, computed standard errors for the specification in Section 5.2, and multiplied them by $\sqrt{1/3}$. In Table 4, we report the average of 10 repetitions to estimate the standard errors. The standard deviation of latitude and longitude for the analysis sample was 0.15. Changing longitude by 0.15, holding constant latitude, is a distance of ten miles. We use a bandwidth of ten miles for computing standard errors.

C Welfare

We highlight the price ceiling as a key policy instrument for the social planner designing a voucher program. We use our empirical elasticites $(\frac{\partial p^{voucher}}{\partial \bar{p}}, \frac{dp^{voucher}}{d\bar{p}}, \frac{\partial q}{\partial \bar{p}})$ to examine the welfare impact of a 10% increase in the regional price ceiling. For a fixed expenditure level, the social planner trades off program generosity, by setting a higher price ceiling, and enrollment, by adding more people to the program.

We assume that the government solves

$$\begin{aligned} \text{Social Welfare} = \max_{\bar{p},\alpha,q,B} \int_0^1 f(x) V_i(q;\bar{p}) dx \quad &- \quad c(B) \\ \text{with } V_{voucher}(q;\bar{p}) = \mathbb{P}(q,\bar{p})q \text{ and } V_{nonvoucher}(q;\bar{p}) = 0 & \\ \text{subject to Incentive Compatibility} \quad & \quad q = q^*(\bar{p}, K, G) \\ \text{Resource Constraint} \quad & \quad \alpha p^{voucher}(q,\bar{p})\mathbb{P} = B \end{aligned}$$

where $c(B)$ is the cost of raising public funds B. Social welfare weights $f(x)$ are indexed from most needy $(f(0))$ to least needy $(f(1))$, with $f'(x) \geq 0$. The government offers vouchers to the neediest households first; this feature incorporates the insights of Akerlof (1978) about why tagging is attractive. Share α are offered a voucher, and share $\mathbb{P}$ are able to use the voucher. We maintain our assumption that the number of recipients is small relative to the general population of tenants, and put no weight on the welfare of landlords.

By focusing on a budget-neutral policy change, we can make welfare statements which require only comparisons between current and potential program recipients, without any assumptions on the marginal cost of public funds. Differentiating the social welfare function with respect to the price ceiling and taking a first order Taylor approximation, the effect of a 10% increase in the price ceiling on social welfare is

$$\frac{\Delta \text{Welfare}/f(\alpha)q^*}{\Delta 10\%\bar{p}} \approx \underbrace{\frac{E(f(x)|x<\alpha)}{f(\alpha)}}_{\text{Need Ratio}} \underbrace{\frac{\Delta \log q^*(\bar{p})}{\Delta 10\%\bar{p}}}_{\text{Quality Benefit}} - \underbrace{\frac{\Delta \log p_{voucher}}{\Delta 10\%\bar{p}}}_{\text{Enrollment Cost}} + \underbrace{\left(\frac{E(f(x)|x<\alpha)}{f(\alpha)} - 1\right)\alpha\frac{\Delta\mathbb{P}}{\Delta 10\%\bar{p}}}_{\text{Targeting Gain}}$$

This formula has three key inputs. Increasing generosity helps the average recipient, who is weakly needier than the marginal recipient by assumption ($E(f(x)|x < \alpha) \geq f(\alpha)$). The extent of the benefit depends upon the quality increase $\frac{\partial q^*(\bar{p})}{\partial \bar{p}}$, and the improvement in the matching probability, which improves the targeting of vouchers to needy recipients ($\left(\frac{E(f(x)|x<\alpha)}{f(\alpha)} - 1\right)\frac{d\mathbb{P}}{d\bar{p}}$). The cost of the generosity increase is that, with a fixed budget, enrollment must be cut to finance this change. We consider each of these inputs in turn to calculate the welfare impact.

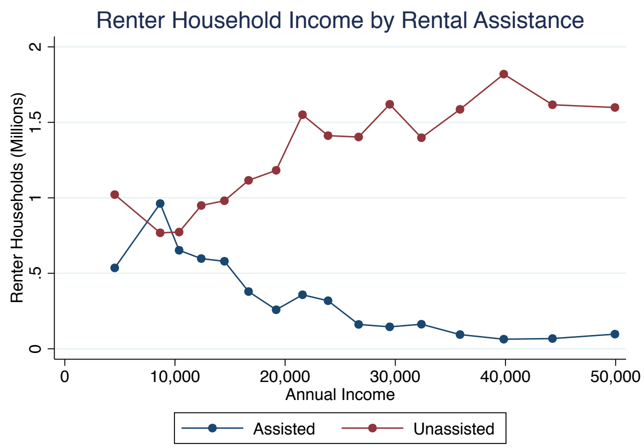
First, we found that a 10% increase in the price ceiling caused a 4.1% increase in per-unit expenditures. With a fixed budget, this implies a 4.1% decline in enrollment.

Second, we found that changes in the price ceiling had little impact on unit quality. We use three methods to estimate the quality change: hedonics, prior prices, and address fixed effects. First, a 10% increase in the price ceiling was associated with a 1.4% increase in hedonic unit quality, based on limited observables. A conservative approach would assume that spending on observable quality is accompanied by spending on unobservable quality. We estimate two additional equations in the American Community Survey:

$$\begin{aligned} \log Rent_i &= \alpha + \beta_{actual} \log HHIncome_i + \varepsilon_i \\ \log q_i^{hedonic} &= \alpha + \beta_{hedonic} \log HHIncome_i + \varepsilon_i \end{aligned}$$

We find that a \$1 increase in spending on actual rent is associated with a 50 cent increase in spending on hedonic rent, so we multiply our quality estimate by two. Second, a 10% increase in the price ceiling was associated with a 0.2% decrease in quality as measured in prior prices. Finally, a 10% increase in the price ceiling raises prices with address fixed effects by 1.7%. This is the sample-size-weighted average of the estimates for stayers and movers reported in Table 1. If this estimate reflects the full extent of price discrimination, then the rest of the increase in prices (4.1% - 1.8%) is a quality increase.

Third, data on low-income households show that marginal recipients are as needy as incumbents ($\frac{E(f(x)|x<\alpha)}{f(\alpha)} = 1$). In particular, using data from the most recent SIPP, we estimate that there were 3.1 million households receiving no housing assistance with income lower than half of the national median and housing expenditures equal to at least half of their income. Here, we plot the distribution of income by receipt of rental assistance.



Notes: Data are from the 2004 Survey of Income and Program Participation. We use sample weights to construct population estimates. Annual household income is estimated using a 32 month average. A household is treated as "assisted" if it reports rental assistance (including public housing or Section 8) in any of the 12 waves.

In addition, the most recent point-in-time estimate shows that there are about 600,000 homeless people in the U.S. (Witte, 2012). If $E(f(x)|x < \alpha) = f(\alpha)$, then there is no targeting gain from changes in the matching probability $\frac{\Delta \mathbb{P}}{\Delta 10\% \bar{p}}$.

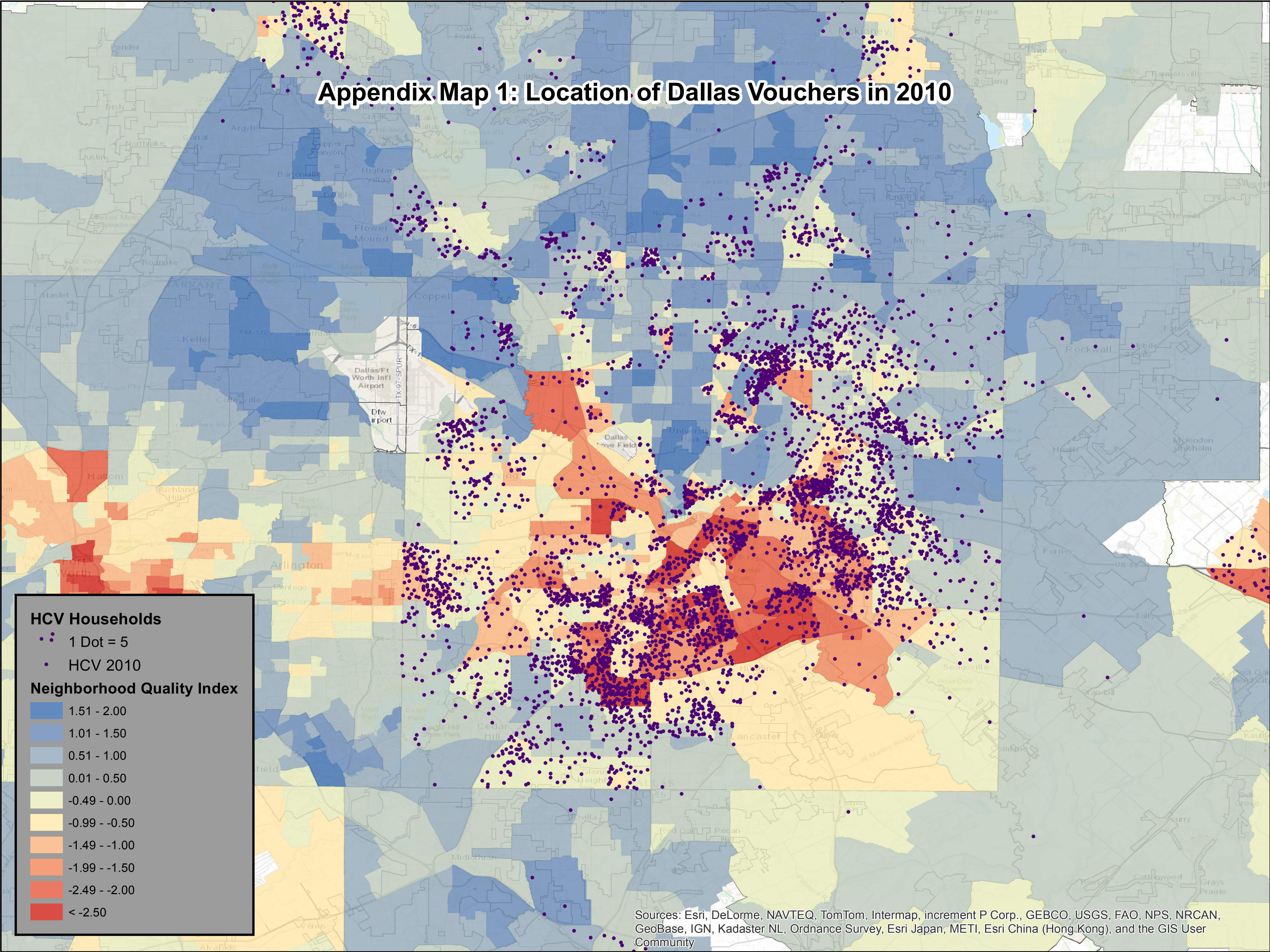
Under these assumptions, we calculate that a 10% increase in the price ceiling reduces welfare by 1.3%-4.3%, measured in terms of the welfare of current and potential participants.

Estimated Welfare Impact	Impact of Δ10% Price Ceiling on:		
	ΔQuality	ΔEnrollment	ΔWelfare
	(1)	(2)	(1)+(2)
Hedonic Coef 0.14*Ratio Actual to Hedonic 2	2.8%	-4.1%	-1.3%
Prior Price -0.02	-0.2%	-4.1%	-4.3%
Address FEs: Expenditure 0.41 - Price Discrim 0.17	2.4%	-4.1%	-1.7%

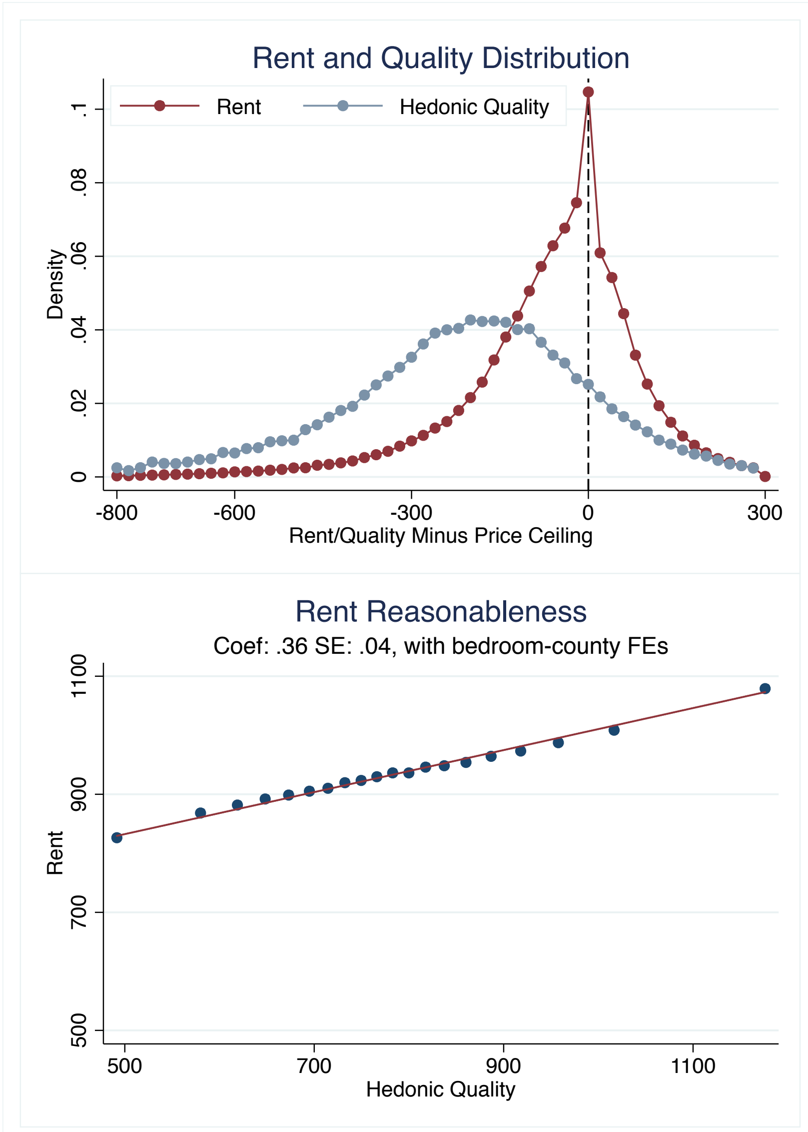
These estimates are likely a lower bound on the true welfare impact because they rely on a linear utility assumption. Adding any concavity to the utility function to acknowledge the serious need of unassisted renters would make the welfare losses even larger. Intuitively, because raising the price ceiling is quite costly to the government and has only modest quality impacts, and there are many needy people who are currently not enrolled in the program, the formula suggests that there are substantial gains to lowering the price ceiling by 10%. In related work, Olsen (2008) has advocated a universal, smaller housing subsidy. Our findings provide evidence that policy changes in that direction may raise welfare. In terms of this social welfare function, the program is too generous in terms of each voucher and too stingy in terms of enrollment.

This welfare statement comes with three caveats which we leave to future work. First, we neglect recipients' potential time savings from shorter searches. Second, we lack data to consider heterogeneous impacts of price ceiling changes. These could matter if, for example, low price ceilings have particularly adverse effects on the matching probability of the elderly and large families. Third, our welfare estimates are partial equilibrium, in the sense that they ignore welfare impacts on private tenants. While we have argued that the voucher program is so small that it is unlikely to have meaningful general equilibrium impacts, and empirically there is no evidence that FMR increases raised nonvoucher prices, further treatment of these issues would be valuable.

We can do a similar exercise for the ZIP code-level FMRs in Dallas, and the result is immediately apparent. Instituting this FMR schedule had substantial positive impacts for movers on neighborhood quality (0.2 standard deviations), with no net cost to the government. The welfare function suggests that there are gains to instituting ZIP-level price ceilings more broadly.


Appendix Map 1: Location of Dallas Vouchers in 2010
HCV Households
1 Dot = 5
HCV 2010
Neighborhood Quality Index
1.51 - 2.00
1.01 - 1.50
0.51 - 1.00
0.01 - 0.50
-0.49 - 0.00
-0.99 - -0.50
-1.49 - -1.00
-1.99 - -1.50
-2.49 - -2.00
< -2.50
Sources: Esri, DeLorme, NAVTEQ, TomTom, Intermap, increment P Corp., GEBCO, USGS, FAO, NPS, NRCAN, GeoBase, IGN, Kadaster NL, Ordnance Survey, Esri Japan, METI, Esri China (Hong Kong), and the GIS User Community
Arlington
Dallas/Ft Worth Int'l Airport
Rockwall
Lancaster
Cedar Hill
Coppell
Flower Mound
Forney
Seagoville

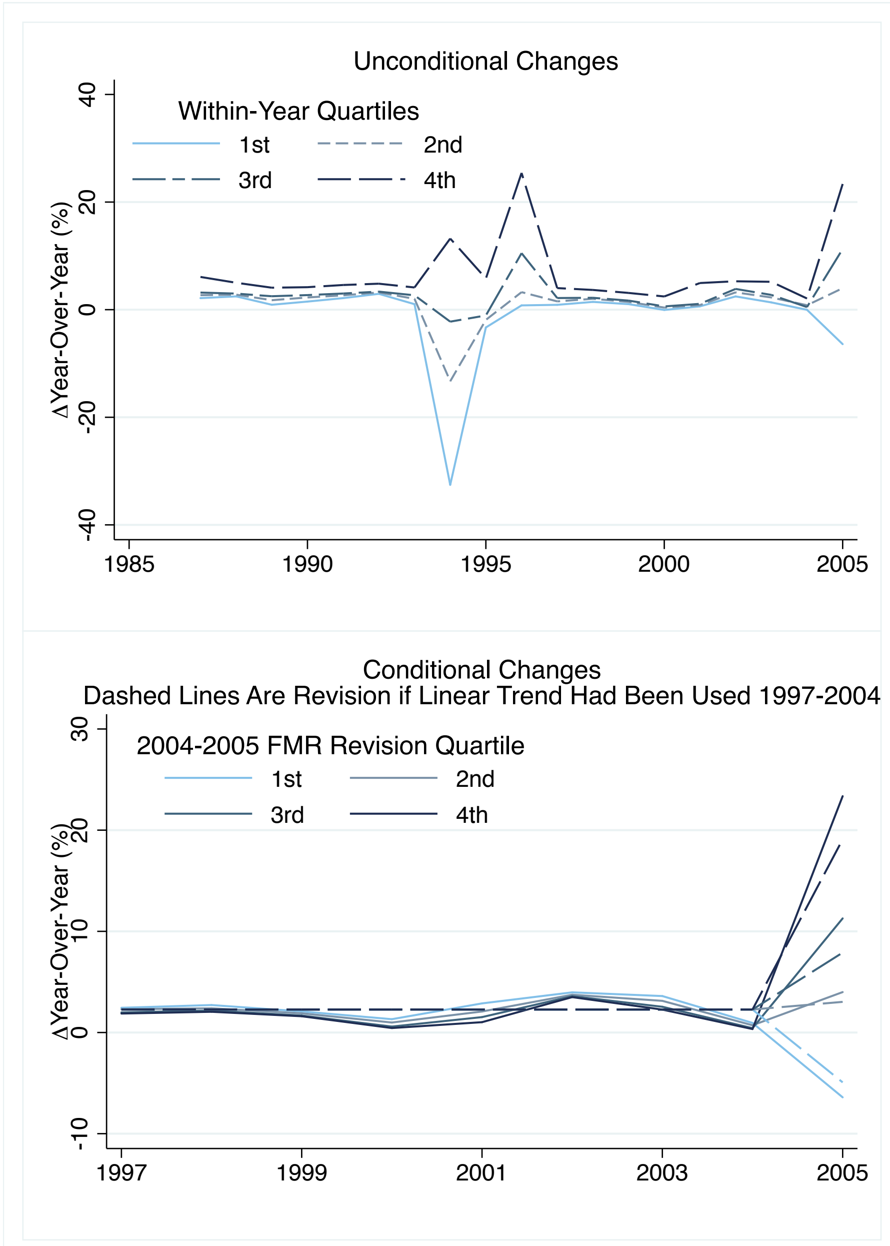
APPENDIX FIGURE 1 – Distribution of Rent and Quality



Notes: 2009 data, n=1.7 million. The top panel plots rents and hedonic quality relative to the local price ceiling. Of rent observations, 0.2% are left censored and 1.1% are right censored. Of quality observations, 1.5% are left censored and 1.4% are right censored. We report gross rent (contract rent + utilities) to facilitate comparison with the price 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.

The bottom panel plots conditional means of unit price 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 prices. This indicates that even for a fixed price ceiling, the government paid less for lower-quality units.

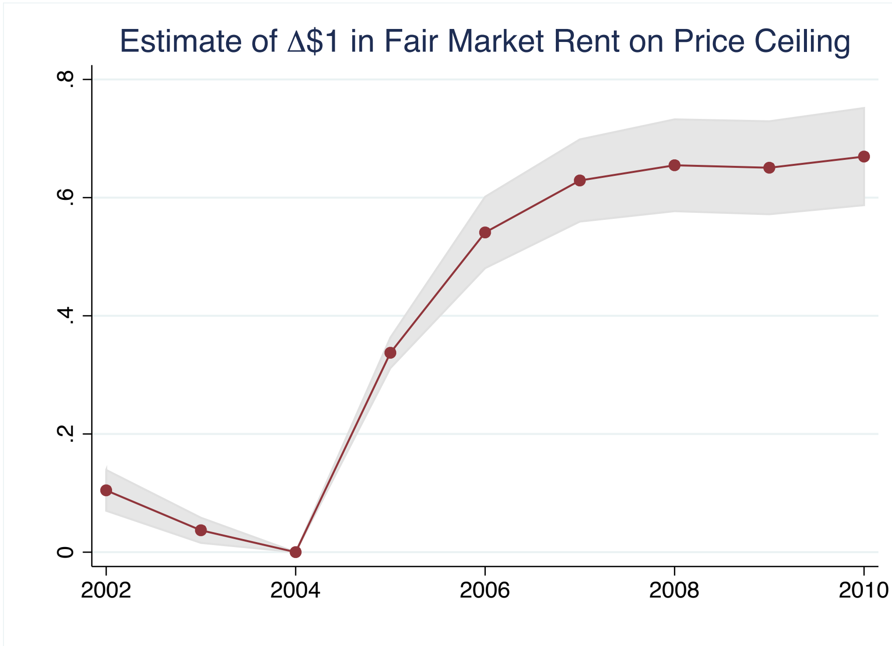
APPENDIX FIGURE 2 – County-Level FMR Changes



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

The bottom panel plots FMR changes for the same sample within quartiles defined over the 2004-2005 FMR change, as in Figure 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 3 – First Stage for County-Level Changes



Notes: This figure shows the impact of the 2005 FMR rebenchmarking on the locally-set price ceiling. FMR varies at the county-by-bedroom level. We regress $PriceCeliling_t - PriceCeiling_{2004} = \alpha + \beta \Delta FMR_{2004-2005} + \varepsilon$ and plot coefficients and a 95% confidence interval for β. Price ceilings were falling in places that were subsequently revised up and rising in places that were subsequently revised down.

Appendix Table 1 - Summary Statistics

County-Level Variation -- National Sample[a]	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)
Voucher Characteristics	2004 (n = 1,578,124)		2010 (n=1,665,868)	
Contract Rent	495	238	586	266
Utility Allowance	106	65	144	89
Price Ceiling (Contract Rent + Utility)	618	278	762	296
Tenant Payment	238	154	288	184
Tenant HH Income (Annual)	9683	6358	11567	7347
Share Moved | Nonattrit	0.21	0.41	0.16	0.36
Tract Characteristics[b]				
Poverty Rate (2000)	16.31	9.13	16.02	9.07
Median Contract Rent (2005-2009)	474	196	480	198
Share Voucher (2004)	0.021	0.024	0.019	0.022
County Characteristics				
Fair Market Rent, Studio	400	158	553	170
Fair Market Rent, 1BR	477	184	623	192
Fair Market Rent, 2BR	596	229	755	229
Fair Market Rent, 3BR	774	294	975	282
Fair Market Rent, 4BR	893	350	1105	343
ZIP-Level Variation -- Dallas Sample	2010 (n = 23,384)		2011 (n=22,866)	
Contract Rent	796	202	807	207
Price Ceiling	1006	237	952	263
Share Moved	0.21	0.41	0.18	0.38

Notes:
a. Voucher and tract characteristics are computed giving equal weight to each county-bed pair.
b. Poverty rate from 2000 Census, ACS survey responses from 2005 to 2009, with rent values inflated to 2009 $.

Appendix Table 2 - Rebenchmarking: Placebo Tests with Nonvoucher Prices

	Dep Var: Change in Log Rent					
Sample	All Units		Units in Counties with 100K+ Residents			
	Voucher	Nonvoucher	Voucher	Nonvoucher		
Time Horizon (2000)	04-09	00-09	04-09	00-09	00-05	05-09
Data Source	HUD Admin[a]	Tract[b]	HUD Admin	Tract	IPUMS[c]	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔLog Fair Market Rent, 04-05	0.0831	-0.046	0.175	0.066	-0.193	0.021
	(0.0179)	(0.020)	(0.049)	(0.049)	(0.102)	(0.099)
Voucher Coef not equal to Nonvoucher Coef[d]						
F-statistic		28.9		5.7		2.3
p-value		<0.0001		0.0174		0.129
n	365,667	312,045	240,525	144,920	1,778	1,772

Notes: This table shows the correlation of the 2005 Fair Market Rent rebenchmarking with contemporaneous changes in nonvoucher prices. Regressions give equal weight to each county-bed pair. Standard errors shown in parentheses are clustered at FMR group level (n=1,484). See Section B.3 for discussion of these results.

a. Voucher estimates in columns (1) and (3) are from HUD Admin data for stayers.

b. Tract-level estimates in columns (2) and (4) use the change in log median rent from the 2000 Census to the 2005-2009 ACS.

c. Change in log rent at the county-bed level constructed from public-use micro data. These data only identify counties with more than 100,000 people due to confidentiality restrictions.

d. F-statistic is calculated by pooling voucher and nonvoucher rent changes and testing the two-sided probability that we would observe data like this or more extreme, under the null hypothesis that the two coefficients are equal.

Appendix Table 3 - Rebenchmarking: Robustness Checks for Voucher Prices

Estimates of β from ΔRent, 2004-2010 = $\alpha + \beta * \Delta$Price Ceiling, 2004-2010 $+ \eta$ (Second Stage) ΔPrice Ceiling, 2004-2010 = $\alpha + \gamma * \Delta$FMR, 2004-2005 $+ \varepsilon$ (First Stage)	
Price Baseline from Table 2	
(1) ΔRent winsorized at 1st and 99th percentile Lived at same 9-digit zip in 2004 & 2010 Weight each county-bed pair equally (n=290,731)	0.125 (0.0252)
Add Controls	
(2) Control for prior rent trends	0.126 (0.0254)
(3) IV for current price ceiling with 2005 FMR , controlling for 2004 price ceiling and FMR	0.0871 (0.0329)
Recode ΔRent	
(4) Winsorize at 5th and 95th percentile	0.103 (0.0218)
(5) No winsorization	0.119 (0.0283)
Alternate Sample	
(7) Define stayers using 11-digit zip code and number of bedrooms (n=268,205)	0.144 (0.0241)
(8) Define stayers using same text address (n= 278,289)	0.137 (0.0249)
(9) Units with low kickback potential (Owner has at least 10 voucher units, n=109,075)	0.0913 (0.0473)
Alternate Weights	
(10) Weight every household equally	0.282 (0.0594)

Notes: This table shows robustness checks for estimating the impact of a countywide increase in the price ceiling on rents for stayers, using variation from the 2005 Fair Market Rent rebenchmarking. Each row shows coefficient and standard error from a separate regression. Standard errors shown in parentheses are clustered at FMR area level (n=1,484).

Appendix Table 4 - Rebenchmarking: Time-varying Unit Quality

Data Source: American Housing Survey, 2003 & 2005

Panel A: Hedonic Estimates of Unit Quality	
Time-varying Covariates	
R-squared (Adj)	0.10
N questions	26
All Covariates	
R-squared (Adj)	0.27
N questions	42

Panel B: Correlation of ΔFMR and ΔHedonic Quality for Stayers

Dependent Variable	ΔLog Quality
ΔLog Fair Market Rent, 2003-2005	-0.04 (0.10)
n	243

Notes: This table shows the impact of Fair Market Rent (FMR) changes on time-varying unit quality, using variation from the 2005 FMR rebenchmarking and data from the American Housing Survey. Analysis sample includes units which self-reported as vouchers in 2003 and 2005. Standard errors clustered at the MSA level.

Hedonic estimation sample is all unassisted observations in 2003. User cost of housing is assumed to be 5% of value for owner-occupied housing. Time-varying covariates include working dishwasher, broken window, toilet issues, holes in walls or roof, running water, rodents, paint, and exposed wiring. Fixed covariates include structure type, square feet, central air, and porch. Categorical covariates are converted into a set of dummies.

Appendix Table 5 - Rebenchmarking: Landlord Entry and Tenant Move Rates

Dependent Variable	ΔPr(New Landlord)	ΔPr(Move)
	(1)	(2)
ΔLog Price Ceiling, 2004-2010	0.00032	0.00032
	(0.00051)	(0.00053)
n	12,375	11,617
Dependent Variable Mean in 2010	0.16	0.17
Unit of Observation	County-Bed	County-Bed
Sample	2004, 2010	2004, 2010

Notes: This table shows the impact of a countywide increase in the price ceiling on landlord entry and tenant move rates, using variation from the 2005 Fair Market Rent (FMR) rebenchmarking. Standard errors shown in parentheses are clustered at FMR group level (n=1,484).
We use data on landlord tax ids to analyze landlord entry. For each county-bed cell, we estimate in 2004 the share of leases signed with a landlord who did not rent to any voucher recipients in 2003 and the share of nonattriters from 2003 who moved to a new address in 2004. We repeat the exercise to estimate new landlord probability and move rates for 2009 to 2010. For column (2), we limit the sample to cells where at least one household could have moved from 2003 to 2004 and from 2009 to 2010.

Appendix Table 6 - Comparison of Nhood Improvement from Dallas with Other Policies

Neighborhood Measure	Control	Treat	Policy Variation and Source
Voucher with ZIP-Level FMR vs. Metrowide FMR			
Poverty Rate	20.8%	18.8%*	Dallas ZIP-Level FMR Demo, Table 4 of this paper
Violent Crimes per 10K ppl	155	131*	
Voucher vs. Public Housing			
Poverty Rate	42%	28%*	Moving to Opportunity Experiment, Table 2, Kling Ludwig and Katz (2005)
Violent Crimes per 10K ppl	234	211*	
Poverty Rate	42%	18%*	
Violent Crimes per 10K ppl	234	128*	
Poverty Rate	48%	22%*	Voucher Applicants in Chicago Public Housing in 1997, Table 2, Jacob, Ludwig, and Miller (2013)
Violent Crimes per 10K ppl	219	201	
Voucher vs. No Voucher			
Poverty Rate	25.7%	24.6%	Voucher Applicants in Private Housing in Chicago in 1997, Table V, Jacob and Ludwig (2008)
Poverty Rate	27.2%	25.4%*	Welfare to Work Experiment, Exhibits 3.6 and 3.8, Abt Associates (2006)
Any Crime Victimization in Last 6 Months	15.0%	21.5%	

Notes: Estimates are control mean and impact estimates for Treatment-on-Treated. All statistics are tract level, except crime victimization in Welfare to Work. * different from control with p <0.01. All other differences are not significant at 10% level.