The Incidence of Housing Voucher Generosity

Robert Collinson and Peter Ganong

October 2014

Abstract

What is the incidence of housing vouchers? Housing voucher recipients in the US typically pay their landlord a fixed amount based on their income and the government pays the rest of the rent, up to a rent ceiling. We consider a policy that raises the generosity of the rent ceiling everywhere, which is equivalent to an income effect, and a policy which links generosity to local unit quality, which is equivalent to a substitution effect.

Using data on the universe of housing vouchers and quasi-experimental variation from HUD policy changes, we analyze the incidence of these policies. Raising the generosity of the rent ceiling everywhere appears to primarily benefit landlords, who receive higher rents with very little evidence of medium-run quality improvements. Setting ZIP code-level rent ceilings causes rent increases in expensive neighborhoods and decreases in low-cost neighborhoods, with little change in aggregate rents. The ZIP code policy improves neighborhood quality as much as other, far more costly, voucher interventions.

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

1Email: ganong@fas.harvard.edu (Department of Economics, Harvard University, Littauer Center, 1805 Cambridge Street, Cambridge, MA 02138) and rcollinson@nyu.edu (Furman Center, 139 MacDougal St, Suite 2, New York, NY 10012). We thank Geoff Newton, Lynn Rodgers and Lydia Taghavi at HUD, and MaryAnn Russ and Matt Hogan at DHA for answering many questions. We thank Nathaniel Baum-Snow, Raphael Bostic, Raj Chetty, Denise D’Alesio, Ingrid Gould Ellen, Michael Eriksen, Dan Fetter, Edward Glaeser, Adam Guren, Nathan Hendren, Jeff Liebman, Bruce Meyer, Larry Katz, Pat Kline, Ed Olsen, Jesse Rothstein, Barbara Sard, Dan Shoag, and seminar participants for their valuable feedback. Peter Ganong gratefully acknowledges residence at MDRC when working on this project, as well as funding from the NBER Pre-Doctoral Fellowship in Aging and Health, the Joint Center for Housing Studies, and the Taubman Center on State and Local Government. The views expressed here are those of the authors and should not be construed as representing those of the U.S. Department of Housing and Urban Development or MDRC.
1 Introduction

Over half of all US transfer program expenditures to the nonelderly take the form of targeted subsidies for purchases of privately-provided goods or services.\footnote{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.} Every year, marginal changes to these programs’ generosity is a major focus of Congressional debate. Who benefits when these transfers are made more generous? Traditionally, targeted subsidies have been thought to be highly efficient because they “tag” needy recipients who make up a small fraction of consumers and so policy changes have little impact on market-clearing prices (Akerlof (1978)). However, subsidized vouchers typically make recipients insensitive to the market price of the good and often involve search frictions in the purchasing decision. These features may undermine the effectiveness of “tagging.” In this paper, we investigate how price insensitivity and search frictions affect the incidence of vouchers.

We study the extent to which changes in the generosity of a large US transfer program, housing vouchers, accrue to subsidy recipients (tenants) or suppliers (landlords). 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. Like other targeted transfers in health care, employment and post-secondary education, housing vouchers operate within a market where search is costly.

We develop a realistic search model to analyze voucher recipient behavior. In our model, voucher recipients choose a quality submarket (defined by neighborhood, structure type, number of bedrooms) in which to search for housing. The probability of finding a unit is higher in low-quality submarkets. We use the model to analyze two policy changes to the rent ceiling, which is the highest rent which the government will pay for a unit: an across-the-board increase in the ceiling, and “tilting” the ceiling to pay more in high-quality submarkets. Both policies result in higher prices paid to landlords, even when recipients are a very small share of the total market. When tenants receive an across-the-board increase, tenants’ quality response is formally equivalent to an income effect in a consumer demand model. The impact of tilting the price ceiling is equivalent to a substitution effect. We estimate the price response as well as income and substitution effects for unit quality using administrative data on the universe of housing vouchers and three quasi-experimental research designs.

We analyze the impact of a county or metro-wide increase in the rent ceiling. Our first source of variation is a revision to county-level rent ceilings in 2005 which used 2000 Census data to correct for a decade of accumulated forecast error. Vouchers account for only 2% of the US housing market, and the 2005 rent ceiling change had no significant correlation with
contemporaneous changes in nonvoucher prices. First, we test for voucher-specific markups, by looking at rental price changes within a single unit address to hold quality constant. We estimate that a $1 increase in the rent ceiling caused same-address rents to rise by 13-20 cents over the next six years.³ Looking at the full sample of units, we estimate that a $1 increase in the rent ceiling caused aggregate rents to rise by 41 cents, while hedonic unit quality rose by only 5 cents over the next six years.⁴ Our second source of variation in metrowide rent ceilings comes from a policy change in 2001 where HUD began setting rent ceilings on the basis on the 50th percentile of local rents rather than the 40th percentile. The advantage of this research design is that we can examine program impacts through the lense of a 28-question HUD survey, with detail comparable to the American Housing Survey, which allows us to capture time-varying quality within an address. Again, we find substantial price increases, with no impact on unit quality. Our finding that rent ceiling changes accrue mostly to landlords is similar to work on UK housing subsidies by Gibbons and Manning (2006). Our model provides a useful framework for understanding these empirical results; because the quality channel operates through an income effect, it is not surprising that the magnitude is small.

We also analyze the impact of “tilting” the rent ceiling toward higher-quality neighborhoods. Housing authorities in Dallas, Texas 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 of children living with single mothers. A difference-in-difference design with Fort Worth, Texas as a comparison group shows that new leases signed after the policy were 0.23 standard deviations higher. Using results from Chetty et al. (2014), we calculate this improvement would likely raise the income rank of a child raised in a family with a voucher by 4.3 percentage points.

This is a substantial improvement, comparable in magnitude to other randomized voucher interventions for public housing residents (Kling et al. (2005); Jacob et al. (2013)) and larger than interventions for unsubsidized tenants (Jacob and Ludwig (2012)). Because price increases in expensive ZIP codes were offset by larger decreases in low-cost ZIP codes, absent any behavioral response, this policy would have been cost-saving for the government. Incorporating tenants’ improved neighborhood choices, the Dallas intervention had zero net cost to the government, while other randomized voucher interventions with similar program impacts cost upwards of $3,000 per year.

³Bayer et al. (2013) similarly use address fixed effects to identify price differences in home sales by race.
⁴Consumer incidence in this context is the benefit to tenants (5 cents) divided by the change in government expenditure (41 cents). If the hedonic measure fully captures the benefits to tenants, then 89% of the increase in government expenditure went to landlords. This differs from standard incidence calculation because a $1 increase in the policy parameter – the rent ceiling – need not imply a $1 increase in government expenditure.
An extensive literature in public economics focuses on how tax changes affect marketwide prices.\(^5\) Although marketwide price impacts should be small for targeted subsidies, our results suggest that voucher-specific markups can seriously undermine subsidies’ effectiveness.\(^6\) Additionally, subsidy policies for markets with search frictions can be thought of in terms of conventional income and substitution effects. Intuitively, the policy which lowers the implicit price of quality improvements may be more effective than the policy which increases generosity unconditionally. These insights may be relevant for other targeted transfer programs where the government offers voucher-like subsidies.

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

\section{Description of Housing Choice Vouchers and Data}

Housing Choice Vouchers use the private market to provide rental units for 2.2 million low-income households. There are four key actors in the voucher program: the Department of Housing and Urban Development (HUD), local housing authorities, private landlords and tenants.

Each year, HUD announces “Fair Market Rents” (FMRs) for every metro- and county-bedroom pair in the US. HUD typically sets FMRs at the 40th percentile of county-level gross rent (rent to landlord plus utility costs). Beginning in 2001, HUD set rents at the 50th percentile in 39 metro areas in an endeavor to promote access to higher-quality neighborhoods. 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 surveys are used to produce adjustment factors which modify the base, not to provide a new estimate of the level. These estimates are very coarse, and in fact were a bit worse at

\(^5\)Most existing work on the incidence of housing subsidies and place-based policies uses a framework with market-clearing prices (Eriksen and Ross (2014); Susin (2002); 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.

\(^6\)Other recent empirical work has also documented price differences for subsidy recipients. Turner (2014) 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, allow for search frictions, and show that changes in subsidy policy are equivalent to income and substitution effects, which are not explicit considerations in this prior work.
predicting local rent changes than using a single national trend from 1997 to 2004.\footnote{See Appendix Figure 1.}

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.\footnote{See Appendix Figure 1 for a plot of changes in FMR by year.} The local housing authority chooses a local rent ceiling $r$ (or “Payment Standard”), as 90%, 100% or 110% of the federally-set FMR (U.S. Department of Housing and Urban Development (2001)). Housing authorities are typically allocated a fixed budget for vouchers, and this budget does not vary with FMR changes (McCarty (2006)). When a housing authority increases its rent ceiling, it is able to finance fewer vouchers. Although an FMR increase allows housing authorities to increase the rent ceiling, housing authorities may use their discretion to smooth out FMR changes.

Local housing authorities are also responsible for finding eligible tenants. These vouchers are often issued using lotteries from a pool of very low income applicants. Once a tenant is issued a voucher, she typically has three months to use it or lose it. Voucher recipients face high search costs and audit studies have shown discrimination by landlords against voucher recipients (Lawyers Committee for Better Housing Inc (2002)). The most recent comprehensive study found that about 70% of households issued vouchers successfully lease units (Abt Associates (2001)). Take-up rates are even lower for households offered vouchers in high-quality neighborhoods, an issue modeled carefully by Galiani et al. (2012).

The tenant pays at least 30% of her income in rent and the housing authority pays the difference, up to the rent ceiling. For most tenants, when rents rise by $1, the housing authority pays an extra dollar and the tenant pays nothing. When tenants rent units with costs higher than the rent ceiling, they pay the difference out of pocket.\footnote{There is debate within HUD over how common it is for tenants to pay the final dollar of rent. Our tabulation of the micro-data shows that 30% of voucher recipients have rents greater than the rent ceiling. However, we suspect that these estimates are inflated by measurement error in rents and in rent ceilings in the administrative records. As another example of measurement error in these records, HUD requires in most cases that out-of-pocket rents be no more than 40% of a household’s income, but 12% of observations appear to have rent greater than this threshold.} To the extent that tenants who pay the final dollar out-of-pocket behave like price-sensitive private tenants, our rent estimates will understate the true magnitude of voucher-specific markups.

When a housing voucher recipient finds a suitable unit, she asks the housing authority to perform an inspection to check that the unit is up to code and to check for “rent reasonableness”. The median housing authority rejects between one-quarter and one-half of units on the first inspection (Abt Associates (2001), Exhibit 3-5). Housing authorities have strong incentives to negotiate down rents, both because holding down per-unit rents enables
them to serve more tenants and because they are reimbursed for administrative expenses on a per-unit basis. HUD routinely audits housing authorities’ leasing process, and rent reasonableness is consistently found to be one of the inspection categories with the highest compliance rates (ICF Macro (2009)). We conducted interviews with several experts to learn more about this process. One housing authority official described the following rent reasonableness process:

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

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

We analyze housing vouchers using a partial equilibrium framework, assuming that they have no impact on general equilibrium rents. Vouchers account for only 2% of the U.S. housing market. If average voucher rents in a tract rose by 30% (a change larger than any of the variation we study in this paper), the average user cost of housing in the tract would rise by only 0.6%.\(^{11}\) We therefore find it unlikely that the policy variation we study had substantial impacts on nonvoucher rents. However, we note that other researchers using other variation have found general equilibrium impacts of the housing voucher program (Susin (2002); Eriksen and Ross (2014)), and so we conduct robustness checks which examine the potential role of general equilibrium effects.

We use a HUD internal administrative database called PIC which contains an anonymous household identifier, an address, building covariates, contract rent received by landlord, and landlord identifier, on an annual basis beginning in 2002. The address, coded as a 9-digit ZIP code, enables us to follow a single unit over time if it has multiple voucher occupants. Appendix B.1 discusses sample construction.

---

\(^{10}\) Appendix Figure 2 shows empirically that rents are lower for units with lower hedonic quality.

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

Finding an apartment is hard, especially for voucher recipients. We build a partial equilibrium directed search model with price posting to analyze the incidence of changes in voucher generosity. Although this model is written in terms of tenants and landlords, it is potentially applicable to a broad variety of voucher programs.\footnote{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)).} People issued a voucher choose a quality submarket in which to search for housing. Only some voucher recipients are able to find units because of search frictions. Higher quality units are more attractive, but it is harder to find a unit in a higher-quality submarket, generating a compensating differential (Rosen (1986)). While private tenants are price-sensitive – making them reject a potential match if the markup is too large – voucher tenants will accept any unit priced below the ceiling. In this section, we describe the targeted subsidy counterparts to income and substitution effects algebraically. Then in Sections 4 and 5, we estimate their magnitudes empirically.

3.1 Environment

There is a continuum of rental submarkets with heterogeneous quality $q$ where $q$ is an observable, dollar-denominated index with positive measure for all $q \geq q_{\text{min}}$. 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. A subset of renters, too small to have any general equilibrium impact on rents, is offered a voucher.

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

\underline{Private Tenants} Because this analysis is primarily focused on vouchers, we do not model private tenants’ choice of submarket. They are randomly matched to units in submarket $q$ and have a dollar-denominated willingness to pay markups of $\eta \sim G$, again arising from differences in outside options.
Voucher Recipients: People offered a voucher are not price sensitive so they will rent any unit which costs less than the rent ceiling. Voucher recipients choose a quality level $q$ to maximize utility, subject to the constraint imposed by the rent ceiling $\bar{r}$ in conjunction with landlord markups. Recipients solve:

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

Recipients maximize expected utility. Let $V(q)$ (with $V'(q) > 0$ and $V''(q) < 0$) denote the relative utility gain from finding a unit with quality $q$ over remaining unmatched, which occurs with probability $P(q)$. Finally, assume that the rent ceiling has a linear structure $\bar{r} = r_{base} + cq$ with $c \in [0, 1)$. The tenant’s problem can be rewritten as

$$\max_q \frac{F(r_{base} + cq - q)}{V(q)} \text{ subject to } P(q) = F(\bar{r}(q) - q)$$

Goverment: There is a unit mass of low-income households potentially eligible for vouchers. The government has an exogenous budget constraint $G$ and only share $s$ of low-income households are offered vouchers, so $sP_{\mu_{\text{voucher}}} = G$. The welfare gain from the program is $Welfare = sP V(q)$.

### 3.2 Solution

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

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

The solution $q = q^*$ is unique.\(^{14}\)

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

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

\(^{13}\)We maintain the assumption that the share of the total population with vouchers is too small to have general equilibrium impacts on prices of all rental units.

\(^{14}\)This follows from the negative second-order condition in the maximand $U_{qq} = (-1 + c)^2 \frac{dF(.)}{dq} V(q) + 2f(.)V'(q)(-1 + c) + F(.)V''(q) < 0$. The first term is negative because $\frac{dF(.)}{dq}$ is negative by assumption, the second term is negative because $c < 1$ and the third term is negative because $V'' < 0$ by assumption.
Finally, we compute rents paid on behalf of voucher units in $q$. Voucher tenants will accept any unit offered to them with rent less than $\bar{r} - q$, so:

$$\mu_{\text{voucher}} = \int_{m_{\text{low}}}^{\bar{r}-q} mf(m)dm / \left( \int_{m_{\text{low}}}^{\bar{r}-q} f(m)dm \right) + q$$  \hfill (2)

The average voucher-specific markup in submarket $q$ is

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

Intuitively, the gap in average rents is larger when private tenants are more price sensitive ($g(m)$ falls rapidly in $m$) and when the rent ceiling is higher.\textsuperscript{15}

### 3.3 Comparative Statics

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

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

Proof: Differentiate equation 2 with respect to $\bar{r}$.

The size of the change in average voucher rents depends on how many landlords in $q$ are on the margin of renting, with markups equal to $\bar{r} - q$. This comparative static will understate the extent to which rents rise if landlords deliberately raise rents in response to changes in the rent ceiling. Any attempt to price discriminate will be limited to the extent that the rent reasonableness process described in Section 2 is effective.

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

\textsuperscript{15}Our model also implies that holding quality fixed, the average rent paid by a voucher recipient may be higher than the average rent paid by a private tenant, but we do not examine this empirically. See Table 6.7 in Olsen (2003) for a summary of older studies comparing differences in average costs and ORC/Macro (2001) for more recent evidence. From conversations with practitioners, we learned that some landlords perceive voucher recipients to be more costly than other tenants due to the risk of damage to the unit, while other landlords prefer voucher recipients because the housing authority guarantees a steady stream of rental payments. Both the costs and benefits of renting to a voucher recipient relative to a private tenant are difficult to quantify. For this reason, we focus instead on policy changes to the rent ceiling, rather than differences in average costs.
Across-the-board $\bar{r}$ increase

Rent Ceiling $\bar{r}$

Quality $q$

Tilting $\bar{r}$

Rent Ceiling $\tilde{r}$

Quality $q$

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

$$
\text{Income Effect } \frac{\partial q^*}{\partial r_{\text{base}}} \propto -(1-c) \frac{\partial f(\cdot)}{\partial r_{\text{base}}} V(\cdot) + f(\cdot) V'(\cdot) \frac{U_{pp}}{U_{pq}}
$$

$$
\text{Substitution Effect } \frac{\partial q^*}{\partial c} \propto f(\cdot) V(\cdot) - (1-c) \frac{\partial f(\cdot)}{\partial r_{\text{base}}} V(\cdot) q^* + f(\cdot) V'(\cdot) q^*
$$

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

Proof: Differentiate equation 1 with respect to $r_{\text{base}}$ and $c$.\(^{16}\)

Across-the-board increases are like an income effect in that voucher recipients may use the funds for moves to a better submarket or improved matching probability in the previously-chosen submarket. Raising the base rent ceiling raises quality, but only through second-order terms $U_{pp}$ and $U_{pq}$. Just as in a consumer demand problem where expanding a household’s budget set will raise their consumption through diminishing marginal utility of each good, quality here increases only through diminishing marginal utility of matching probability and the complementarity between matching probability and unit quality. In contrast, raising the funds for moves to a better submarket or improved matching probability in the previously-chosen submarket. This model has first-order condition of $-U_c(W(T-\ell^*(Z)) + Y, \ell^*(Z))W + U_1(W(T-\ell^*(Z)) + Y, \ell^*(Z)) = 0$ where $Z$ captures exogenous parameters $Y$ and $W$. Differentiation gives

\[ \frac{\partial q^*}{\partial c} \propto -U_c + WU_{cc} \ell^* + U_{cd} \ell^* \]

This is formally isomorphic to the model above with $\ell = q$, $c = P$ and $W = -(1-c)$.\(^{16}\)
subsidy for high-quality units also works through a first-order effect $U_p$, whereby the penalty for moving to a higher-quality unit, which takes the form of a lower matching probability, is diminished. This suggests that tilting the rent schedule may be more effective at improving quality than raising the base rent ceiling.

**Proposition 3** When the base rent ceiling rises, welfare rises for incumbent recipients due to increases in unit quality and increases in the matching probability. Examining welfare aggregated over all low-income eligibles: (1) increases in landlord markups reduce welfare through lower enrollment, (2) increases in the matching probability have no impact on welfare, and (3) increases in unit quality depend on the marginal value of unit quality versus the marginal value of enrollment.

Proof: See Appendix A.1.

Most of these results are straightforward and intuitive – the government budget constraint implies that higher spending per voucher will result in fewer vouchers, with adverse effects on both landlords and tenants. Offer share $s$ and match probability $p$ vary one-for-one. While incumbent recipients value changes in the matching probability $p$, the pool of all low-income eligibles places no value on this change. This is because the voucher recipient’s decision to “spend” additional funds on matching probability has a negative externality on other potential recipients and fewer offers are extended to other eligibles. Similarly, the sign of the welfare impact of unit quality improvements depends on $V'(q) - \frac{V(q)}{\mu_{voucher}}$, the relative value of a dollar spent on unit quality for incumbents versus a dollar spent on increasing enrollment. Olsen (2008) forcefully argues that there are welfare gains from smaller, more universal housing subsidies.

In the remainder of the paper, we estimate the impact of changes in the rent ceiling on markups $\mu_{voucher}$ and quality $q$ using three complementary research designs.

## 4 Income Effects: Impact of Raising the Base Rent Ceiling

We estimate the causal effect of rent ceilings on voucher rents and unit quality. Total rent changes can be decomposed into changes in voucher-specific markups and changes from quality improvements ($\frac{d\mu_{voucher}}{\bar{r}} = \frac{\partial \mu_{voucher}}{\partial \bar{r}} + \frac{\partial q^*}{\partial \bar{r}}$). We estimate the partial effect on rents holding quality fixed ($\frac{\partial \mu_{voucher}}{\partial \bar{r}}$), the total effect on rent ($\frac{d\mu_{voucher}}{\bar{r}}$), and the effect on unit quality $\frac{\partial q^*}{\partial \bar{r}}$.

Due to data constraints, we use two different identification strategies. In Section 4.1, we study a 2005 change in FMRs due to availability of new highly-granular rental data.
in the 2000 Census. We study this change using rich data on the universe of housing vouchers including the ability to track households and addresses over time. Unfortunately, this database only came into widespread use in 2003. In Section 4.2, we study a 2001 change which raised FMRs from the 40th percentile to the 50th percentile of rents in 39 metro areas. We study this change using a detailed HUD survey, which was administered to voucher recipients on a widespread basis from 2000 to 2003. The advantage of this research design is that the survey offers an in-depth look at unit quality, including quality attributes which might vary over time within the same unit. Across both research designs, we find similar results: raising the rent ceiling accrues largely to landlords rather than tenants.

4.1 Rebenchmarking of FMRs in 2005

The availability of new Census data results in a “rebenchmarking.” Gordon (2004) and Suarez-Serrato and Wingender (2014) also use decennial Census rebenchmarkings as source of exogenous variation to examine the incidence of federal expenditures. 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, 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. Define $exp(\sigma_t)$ as an annual estimate of rental changes based on a regional RDD or CPI survey from year $t-1$ to $t$. Define $exp(r_t + \varepsilon_t)$ as an observation from decennial Census data, where $r_t$ is the true rent and $\varepsilon_t$ is measurement error. We can use these definitions to write $\log FMR^{2004} = \sum_{t=1991}^{2004} \sigma_t + r_{1990} + \varepsilon_{1990}$, and $\log FMR^{2005} = \sum_{t=2001}^{2005} \sigma_t + r_{2000} + \varepsilon_{2000}$. Taking the difference gives

$$\Delta FMR = \frac{r_{2000} - r_{1990}}{\text{true price change}} + \sigma_{2005} - \sum_{t=1990}^{1999} \sigma_t + \frac{(\varepsilon_{2000} - \varepsilon_{1990})}{\text{Census meas error}}$$

(3)

There are three sources of variation in the rebenchmarking: changes in nonvoucher rents, changes in nonvoucher rents, and census measurement error.

17More institutional details on the rebenchmarking are provided in Appendix B.2.
measurement error from annual updates, and measurement error in the Census. Consistent with measurement error as a source of variation, places where FMRs drifted upward due to noise over the prior ten years were subject to downward revisions in 2005, and places where FMRs drifted downward due to noise were subject to upward revisions.

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

Identification Assumption in Rebenchmarking Research Design

$$E(\Delta r_{2001-t}^{\text{nonvoucer}} | \Delta \text{FMR}) = 0$$

As detailed above, $\Delta \text{FMR}$ consists of measurement error, which is by construction orthogonal to future trends, and the true nonvoucher rent change, $r_{2000} - r_{1990}$. Note that this research design allows the rebenchmarking to bring rental rents closer in line with the level of market fundamentals. We require only that the change in FMR be uncorrelated with the subsequent change in nonvoucher rental rents. Available empirical evidence supports this identification assumption. Contemporaneous changes in nonvoucher rents have no significant correlation with the FMR change.\(^{18}\)

We use two stage least squares to address endogeneity, because local housing authorities have some discretion in setting rent ceilings. The bottom panel of Figure 1 shows an event study with the path of local rent ceilings around the rebenchmarking. Housing authorities use their discretion to offset the immediate impact of FMR changes, but a $1$ increase in the FMR from 2004 to 2005 corresponded to a 67 cent increase in the regional rent ceiling by 2010, so it takes time for FMR changes to absorb into local policy. In regression form, with $j$ indexing county-bed FMRs, our empirical strategy is

First Stage: $\Delta \bar{r}_j = \alpha + \gamma \Delta \text{FMR}_j + \varepsilon_j$  \hspace{1cm} (4)

Second Stage: $\Delta y_j = \alpha + \beta \Delta \bar{r}_j + \eta_j$  \hspace{1cm} (5)

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

\(^{18}\)Appendix B.3 analyzes prior and contemporaneous changes in nonvoucher rents in more detail and Appendix Table 2 shows the relevant regression results.
We examine the effect of rent ceiling increase on voucher rents 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 voucher-specific markups. These could arise through deliberate price discrimination, or, as in the model, through price-insensitive voucher recipients not avoiding units whose markups were rising due to random variation.

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 of the rent ceiling for stayers using \( \Delta y_{ij} = r_{2010,ij}^{\text{voucher}} - r_{2004,ij}^{\text{voucher}} \) in equation 5, where \( i \) indexes households. Table 1 column (1) shows the results – a $1 change in the rent 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 rents for addresses which were occupied by different households before and after the rebenchmarking. We exploit the fact that about one-third of movers and new admits from 2005-2010 went to an address that was occupied by a different voucher recipient in 2003 or 2004. We calculate mean pre-2005 rent at every address (9 digit ZIP code-bedroom) and then merge this file with the addresses of voucher recipients in later years. Formally, we estimate equation 5 with \( \Delta y_{hj} = r_{2010,h'i}^{\text{voucher}} - r_{2004,hi}^{\text{voucher}} \) where \( i \) changes to \( i' \), to reflect a change in household, while address \( h \) is constant. For these movers, we find that a $1 increase in the rent ceiling caused rents to rise by 20 cents, as reported in Table 1 column (2). We believe that these estimates are slightly larger than the stayers estimates because of tenure discounts, where landlords are less likely to raise rents for a tenant renewing their lease.

We conducted several robustness checks to critically assess our result that landlords raise rents for tenants at the exact same address. First, we add county fixed effects, so that identification comes only from within-county variation comparing the FMR change for 1-bedroom units to the FMR change for 4-bedroom units, and not at all from differences in secular trends across counties. Again, we find that a $1 increase in rent ceiling raises rents

\[ \Delta y_{ij} = r_{2010,ij}^{\text{voucher}} - r_{2004,ij}^{\text{voucher}} \]

Point estimates and standard errors are in Appendix Table 3.
for stayers. Second, recall that most tenants pay 30% of their income as rent, but some paid 30% of their income plus the difference between the unit’s rent and the local rent ceiling. We build a sample of households which are very unlikely to be the residual payer in 2010 using baseline characteristics in 2004, and find a substantial increase in rents, combined with no change in tenant payments. Third, we attempt to test for kickbacks. While it would be easy for a mom-and-pop operation to give kickbacks, it would be much more difficult for a large business with accountants and auditors to do so. We think that kickbacks from landlords to voucher recipients are unlikely to explain the results, because we find substantial rent increases among these larger landlords.

On a theoretical and empirical level, our results are consistent with voucher-specific markups and inconsistent with general equilibrium effects. Theoretically, since each housing authority’s budget is fixed, places that had a rent ceiling increase saw decreases in the number of vouchers. If units were priced competitively within a segmented market for vouchers and the number of vouchers fell, then voucher rents would have fallen, not risen. Empirically, we examine whether rent increases are larger in tracts with (relatively) high voucher concentrations, and find a similar rent increase in tracts with high voucher concentrations.

4.1.2 Impacts on Voucher Rents and Quality at All Units

Next, we assess the impact of the rebenchmarking on rents \( \left( d\mu_{\text{voucher}}/d\bar{r} \right) \) and quality \( (\partial q^* / \partial \bar{r}) \) at all voucher units. Formally, we use a slightly different estimation strategy with first stage \( \bar{r}_t = \alpha + FMR2005 + FMR2004 + \bar{p}_{2004} \) and second stage \( \Delta y_{t,j} = \alpha + \beta \bar{r}_{t,j} + FMR_{t,j} + \bar{r}_{2004,j} + \varepsilon_j \), where \( FMR2005 \) is the excluded instrument.\(^{20}\) For rents, we set \( \Delta y_{t,j} = r_{t,j}^{\text{voucher}} - r_{2004,j}^{\text{voucher}} \) where \( r_{t,j}^{\text{voucher}} \) is the unconditional average of rents in county-bed \( j \), including units that newly entered and exited the sample. For quality, we run a hedonic regression in the American Community Survey using covariates for structure age, structure type (e.g. single-family, multi-family, or apartment building) and local rent. We then constructed a dependent variable \( \Delta y_j = \beta_{\text{hedonic}}(x_{t,j} - x_{2004,j}) \) using covariates \( x_{t,j} \) on structure type and median tract rent from the voucher data.\(^{21}\) Census tracts typically

---

\(^{20}\)Using equation 4 as our first stage, we found that prior to the FMR change, average rents across all units were rising for places about to receive a downward revision and that rents were falling for places about to be revised upward, as shown in Appendix Figure 3. In order to ensure that we have consistent estimates of the policy impact, we use a different estimating equation from Section 4.1.1 which controls for FMR and the rent ceiling in 2004. Explicitly controlling for \( FMR_{2004} \) is unattractive for our baseline same-address strategy in Section 4.1.1, because it eliminates measurement error \( \sigma_t \) and \( \varepsilon_{1990} \) as a source of variation, but is necessary here to ensure that there is no pre-trend in the outcome variable.

\(^{21}\)We estimate our hedonic coefficients in the American Community Survey, where the smallest geographic units are Public Use Microdata Areas (PUMAs) with about 150,000 residents. However, when predicting hedonic quality for voucher units, we use median tract rent (tracts have about 4,000 residents), which provides much more geographic detail than PUMAs. More details on construction of the hedonic measure are provided in Appendix B.4.
have 4,000 residents and 77% of voucher moves cross tract boundaries, so this measure reflects even very short-distance moves to higher-quality neighborhoods or higher-quality units within the same neighborhood.

Figure 3 plots the year-by-year coefficients. By 2010, a $1 increase in the rent ceiling raised average rents by 41 cents (Table 1, column 3). In contrast, there was virtually no impact on observed quality, with an increase of just 5 cents. Either tenants saw big increases in unobserved quality or landlords saw increases in profits of 36 cents for each $1 change in the rent ceiling. If this hedonic measure completely captures changes in quality, then landlords captured 89% of the increased government expenditure, with the only 11% going to tenants.

How much do landlords benefit from a $1 rent ceiling increase? The estimate of 36 cents here is twice as big as the estimates of 13-20 cents with address fixed effects in the previous section. The “rent reasonableness” process discussed in Section 2 is likely to be particularly salient when a voucher recipient has already leased a unit rather than when starting a lease at an address previously unoccupied by a voucher. Therefore, we believe that 36 cents is an upper bound – because the hedonic quality measure in this section may understate the true quality impacts – while 13-20 cents is a lower bound. In both specifications, we find evidence consistent with voucher-specific markups.

4.2 40th → 50th Percentile FMRs in 2001

A concern with the first research design is an inability to measure detailed elements of unit quality which might vary over time at the same address. This motivates a second identification strategy based on a policy change in 2001, when HUD switched from setting FMRs at the 40th percentile of the local nonvoucher rent distribution to the 50th percentile in 39 MSAs. This policy was implemented not in response to recent housing market conditions, but rather with the explicit goal of “deconcentration” of vouchers from the poorest neighborhoods.

From 2000 to 2003, HUD conducted a Customer Satisfaction Survey (CSS) of repeated cross-sections of about 100,000 voucher households. This survey included numerous questions on unit quality and came close to matching the level of detail in the American Housing Survey (AHS), which is the state-of-the-art data source on housing quality in the US. In particular, it asked many questions about unit attributes which could plausibly vary at the same address over time including: “How satisfied are you with your unit?” “Over the last year, how many times have you called for maintenance or repairs?” “Do you think management is responsive to your questions and concerns?” and “If you had a problem with electricity or heat, how long did it take to fix?” To compute hedonic quality, we identified
the 26 questions on time-varying quality in the CSS which also appeared in the AHS. We ran a hedonic regression in the AHS using these 26 questions, building age, and building type and then used tenants’ responses in the CSS to predict hedonic quality.

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

First Stage: \[ \bar{r}_{ijt} = \alpha + \gamma 1(FMR = 50)_j Post_t + 1(FMR = 50)_j + Post_t + \varepsilon_{ijt} \] (6)

Second Stage: \[ r_{ijt} = \alpha + \beta \bar{r}_{ijt} + 1(FMR = 50)_j + Post_t + \eta_{ijt} \] (7)

Our identification condition is the standard difference-in-difference condition: \[ E(\eta_{ijt}|1(FMR = 50) \times Post) = 0. \] The bottom panel of Figure 3 shows the results visually and Table 2 shows regression results. Setting FMRs at the 50th percentile of the local nonvoucher rent distribution raised rent ceilings by an average of 15 percent. For every $1 increase in FMRs, rents rose by 47 cents and hedonic quality rose by less than 1 cent, with a standard error of 4 cents. The results from this analysis reinforce the conclusions from the prior section that increases in FMRs seem to accrue to landlords rather than tenants.

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

Following a court settlement, HUD replaced a single metro-wide FMR with ZIP code-level FMRs in early 2011. The demonstration caused sharp changes in local rent ceilings, ranging from a decrease of 20% to an increase of 30%, as shown in the top panel of Figure 4.

Identification Assumption in ZIP Code-Level Research Design

\[ E(\Delta r_{\text{nonvoucher}}^{2010-2013} | \Delta FMR) = 0 \]

The identifying assumption is that the FMR change had no differential impact across zip codes on changes in nonvoucher rents from the base year (2010) to the most recent data available (2013).

5.1 Impacts on Voucher Rents and Building Quality

We examine the impacts of this policy change on rent and building quality. Because FMR in 2010 was constant across Dallas, using the 2011 FMR level as the regressor is the
same as using the change from 2010 to 2011 as the regressor. With $j$ indexing ZIP codes and $Post_t$ as a dummy for 2013, we estimate

First Stage: \[
\bar{p}_{ijt} = \alpha + \gamma FMR_j Post_t + FMR_j + b_{ijt} + \varepsilon_{ijt}
\] (8)

Second Stage: \[
y_{ijt} = \alpha + \beta \bar{p}_{ijt} + FMR_j + b_{ijt} + \eta_{ijt}
\] (9)

Rents at the ZIP code-level were highly responsive to the policy change, as shown in Figure 4. Table 3 reports results from equations 8 and 9. Changes in FMRs are a strong predictor of changes in rent ceiling, with coefficients around 60 cents. We find that for every dollar increase (decrease) in FMR, rents for stayers rose (fell) by 13 cents. Among addresses where the tenants changed, we find a much stronger effect of 56 cents. Evidently, rent reasonableness is enforced much more seriously in Dallas for lease renewals than for new leases, even when the new leases occur at addresses previously occupied by other voucher tenants. Finally, looking across all tenants who moved, we find substantial rent increases in more expensive areas and rent decreases in cheaper areas; every $1 change in FMR was associated with a 57 cent change in rents. This could reflect changes in landlord pricing or unit quality.

We examine whether this change in the schedule led voucher recipients to move to higher-quality buildings. We predict physical structure quality by applying the hedonic coefficients to data in Dallas on number of bedrooms, structure type, and structure age (but not building location). In 2010, voucher recipients who lived in higher-quality neighborhoods had lower structure quality, as would be expected given the existence of a single, metro-wide rent ceiling. We find that for every dollar change in the rent ceiling, structure quality for movers changed by 19 cents, as reported in Table 3. If this hedonic measure fully reflects quality, then landlords at the ZIP code-level captured two-thirds of the gains and bore about two-thirds of the losses from the policy change. However, the policy had another important benefit for voucher recipients, which we examine in the next section.

5.2 Impacts on Neighborhood Quality and Average Rents

We assemble data on five measures of neighborhood quality: poverty rate, 4th grade test scores at zoned school, unemployment rate, share of children in families with single mothers, and the violent crime rate. 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

---

22See Appendix B.4 for details.
neighborhoods, often on the south side of the city. Map 3 also shows the change in voucher counts at the tract level from 2010 to 2013. A black dot indicates a net increase, a white dot represents a net decrease, and the size of the dot indicates the magnitude of the change. Voucher recipients exited the lowest-quality neighborhoods in the inner city, moving further south and east to better neighborhoods. Map 3 shows that the improvement in neighborhood quality was broad-based, and not driven by moves to or away from a single neighborhood. In addition, we find that movers in Dallas from 2007 to 2010 were not choosing higher-quality neighborhoods, as shown in Figure 5.

To formally estimate the impact of the change to ZIP code-level FMRs, we use a simple difference-in-difference design with a comparison group of Fort Worth – a nearby city which continued to have a single metro-wide rent ceiling. The identifying assumption is that quality difference between Dallas voucher tenants and Fort Worth voucher tenants would have been stable absent the policy intervention. Specifically, we estimate

\[ Y_{it} = \alpha + \text{Dallas}_i + \text{Post}_t + \beta_{\text{post}} \text{Dallas}_i \text{Post}_t + \varepsilon_{it} \]

where \( i \) indexes households and \( t \) indexes years. The results are shown in Table 4, where \( \beta_{\text{post}} \) shows an intent-to-treat (ITT) improvement of 0.1 standard deviations in quality. This estimate is statistically precise, with a t-statistic greater than 3 using standard errors clustered at the tract level. Of course, neighborhood quality could only improve for tenants who moved. From 2010 to 2013, 44% of continuing voucher recipients moved units, so the impact estimate for treatment-on-the-treated (TOT) is 0.23 standard deviations.

Table 4 also provides impacts separately for each of the five quality measures. We find small and statistically insignificant improvements of 0.09 SD in test scores at zoned schools and 0.05 SD in the rate of children living with single mothers. We find medium-sized improvements of 0.19 SD in the poverty rate and 0.21 in the unemployment rate. The largest improvements are in the violent crime rate, which improves by 0.33 SD. If these relative improvements reflect voucher recipients’ valuations, then it seems that voucher recipients prioritize getting away from high crime areas. This is consistent with evidence from the

---

23 We study voucher recipients’ locations three years after the policy change, which is important because Eriksen and Ross (2013) find that it takes several quarters before the full neighborhood impact of a voucher subsidy is realized.

24 The court settlement which precipitated the policy change also funded voluntary mobility counseling, provided by Inclusive Communities Project, the organization which filed the lawsuit. There were 303 voucher households who already had conventional (non-Walker) vouchers in 2010 and took advantage of these counseling services by the end of 2012. Appendix Table 4 shows that households which received counseling showed dramatic improvements in neighborhood quality of 1.17 standard deviations. These large impacts may reflect self-selection or the causal impact of the intervention. If the quality improvement for these 303 households is entirely attributable to the causal impact of mobility counseling (and not to the ZIP code-level FMRs), then our estimates for the impact of ZIP code-level FMRs shrinks by about 20%.
Moving to Opportunity (MTO) experiment, where treatment households chose tracts with much lower crime rates, less graffiti, and better police response when a call was made (Kling et al. (2005)).

Finally, Table 4 shows that across Dallas, average voucher rents were about constant. 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. As a result, the median rent of all units in Dallas is substantially lower than the rent paid in a neighborhood of median quality. Second, the data suggest that there is a minimum cost to rental housing; median rents are the same in neighborhoods with a quality index of -4 and an index of -1. Finally, implementation costs were also minimal, at only about $10 per household.\footnote{Implementation cost estimate comes from correspondence with Matthew Hogan of Dallas Housing Authority, October 23, 2012.}

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

For each intervention, we construct a cost estimate and summary measure of the change in opportunity for a child affected by the policy. Chetty et al. (2014) document heterogeneity in intergenerational mobility across US commuting zones. Chetty and Hendren (2014) estimate that two-thirds of the cross-sectional variation is causal. We regress the predicted income rank of child whose parents are at the 10th percentile of the income distribution on local violent crime and poverty rates.\footnote{To be precise, across commuting zones $j$ we regress $E(rank|\text{parentRank}_j = 0) + 0.1 \cdot E(\Delta\text{rank}|\text{parentRank}_j) = \alpha + \beta\text{Crime}_j + \delta\text{Poverty}_j$ and then predict the impact of an intervention} To predict the causal impact of voucher interventions on children’s outcomes, we assume: (1) the child lived in the new location from...
birth to age 18 and (2) the cross-Commuting-Zone coefficients are accurate for the causal impacts of tract-level variation in neighborhood quality. The Chetty et al. (2014) results, combined with our assumptions, suggest that their children’s income rank at around age 30 would rise by 4.3 percentage points, so from the 39th percentile to the 43rd percentile. This improvement for Dallas is smaller than the predicted improvement for the MTO Experimental group (20 percentage points), but similar in magnitude to offering vouchers to public housing residents, and larger than offering vouchers to unassisted tenants.27 Offering vouchers, however, is very costly to unassisted renters, and more expensive than maintaining the existing public housing stock (Abt Associates (2010)). The Dallas ZIP-level FMRs, in contrast, appear to thus far have had no net cost to the government.

Finally, we note that policies setting ZIP-level FMRs on the basis of data on opportunity, rather than just data on local rents, may be a more effective way to help people move to high-opportunity neighborhoods. In particular, Chetty and Hendren (2014) report that about 20% of the variation in neighborhood quality is due to observables, which rise with rent, but 80% of the variation is uncorrelated with rents. If tying FMRs to ZIP code-level rents mean that the policy only captured 20% of the opportunity differences across ZIP codes, then the gains under an alternative policy tying FMRs directly to opportunity measures could be an order of magnitude larger.

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

6 Conclusion

We examine the incidence of a narrowly-targeted voucher program, allowing for consumer search frictions. Our assumptions provide a realistic description of housing vouchers in the US. Holding quality constant using address fixed effects, an across-the-board $1 increase in the rent ceiling raises rents by 13-20 cents. Across all units, a $1 increase in the rent ceiling raises rents by 41 cents; consistent with this policy change acting like an income effect, we

\[ \Delta \text{Rank} = \frac{2}{3} (-21.8 \times \Delta \text{Crime} - 0.231 \times \Delta \text{Poverty}) \]

where the crime rate is measured as violent crimes per 10,000 residents and poverty rate is the fraction of residents with incomes below the federal poverty line.

27This 20 percentage point prediction is if the policy moved children at birth and they stayed in the same neighborhood until age 18. In fact, the improvement neighborhood quality for the MTO experimental group decayed by about 80%, so the quality impact of MTO was smaller than the impact of the hypothetical policy considered here which permanently implemented voucher restrictions.
find very small quality increases of around 5 cents, meaning that as much as 89% of the increase in government expenditure accrued to landlords. A tilting of the rent ceiling, which is equivalent to a substitution effect, increases neighborhood quality substantially. The latter policy, without any net cost to the government, appears to have raised a neighborhood quality index by 0.23 standard deviations and the predicted income rank of children in families with vouchers by 4.3 percentage points.

Our emphasis on voucher-specific markups and search frictions 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. 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 has already provided an estimated 7 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 voucher-specific markups and the impact of voucher generosity on quality for other voucher programs.
References


Turner, L. J. (2014). The Road to Pell is Paved With Good Intentions: The Economic Incidence of Federal Student Grant Aid. *mimeo*.

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 as part of a “rebenchmarking” to incorporate newly-available data from the 2000 Census. The top panel plots demeaned changes in the Fair Market Rent for four quartiles of county-bed observations, stratified by the change from 2004 to 2005. Local housing authorities administer the vouchers, and have discretion to set the local rent ceiling at 90%, 100% or 110% of Fair Market Rent. The bottom panel plots local rent ceilings, using the same grouping of county-beds as in the top panel. By 2010, for every $1 increase in the Fair Market Rent, local rent ceilings rose by 70 cents.
FIGURE 2 – Voucher-Specific Markups After Rebenchmarking

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 changes in FMR from 2004 to 2005 due to the rebenchmarking. In the bottom panel, each point represents coefficient $\beta$ from the IV regression with second stage $r_t - r_{2004} = \alpha + \beta \Delta RentCeiling_t + \varepsilon$, and first stage $\Delta RentCeiling_t = \delta + \gamma \Delta FMR + \eta$. 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 – Full Sample Rent and Quality Impacts for Rebenchmarking and 40th→50th Percentile FMRs

Notes: The top panel plots β coefficients using variation from the 2005 rebenchmarking for the IV regression with second stage $\Delta y_{t,j} = \alpha + \beta \bar{r}_{t,j} + FMR_{2004,j} + \bar{r}_{2004,j} + \varepsilon_j$ and first stage $\bar{r}_t = \alpha + FMR_{2005} + FMR_{2004} + \bar{r}_{2004}$. Hedonic quality is measured using number of bedrooms, structure type, structure age and median tract rent. Shaded area / dashed lines indicate 95% confidence intervals. Rental data from 2002 and 2003 are a test for pretrends, and the 2004-2005 first stage is used.

The bottom panel shows an event study for changes in rent and quality around the introduction of 50th percentile FMRs in 2001. Hedonic quality is measure using number of bedrooms, structure type, structure age and 26 survey questions about unit quality and maintenance. Shaded area / dashed lines indicate 95% confidence intervals. See notes to Table 2 for details.
Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs. The top panel shows that this policy raised rent ceilings in expensive neighborhoods and lowered rent ceilings in cheap neighborhoods. Dots reflect means for 20 quantiles of the ZIP code-level FMR distribution conditional on bedroom-year. We show data only for households which moved from 2010 to 2013. The bottom panel shows that mean rents were quite responsive to the new rent ceiling schedule.
FIGURE 5 – Neighborhood Quality Impacts for Dallas ZIP-level Rent Ceiling Demo

Notes: In 2011, Dallas replaced a single, metro-wide FMR with ZIP code-level FMRs, raising rent ceilings in expensive neighborhoods and lowering rent ceilings in cheap neighborhoods. We construct a neighborhood quality index as an equally-weighted sum of tract-level poverty rate, test scores, unemployment rate, share of kids with single mothers, and violent crime rate. The index is normalized to have mean zero and unit standard deviation with respect to the entire Dallas metro area. The top panel shows the distribution of destination quality for people who moved from 2007 to 2010 (before the policy) and people who moved from 2010 to 2013 (after the policy). There is a broad-based improvement in destination quality in Dallas, with no change in nearby Fort Worth, which did not implement the policy.

FMR Changes 04-05 (2BD)
Quintiles of 04-05

- 18% - 66%
- 11% - 17%
- 6% - 10%
- 1% - 5%
- -28% - 0%
Table 1 - Effect of County/Metrowide Rent Ceiling Increase on Rents and Quality [Rebenchmarking]

<table>
<thead>
<tr>
<th>Policy Variation</th>
<th>Rebenchmarking of FMRs in 2005</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Same Address w/Same Voucher Tenant$^a$</td>
</tr>
<tr>
<td>Sample</td>
<td>(1)</td>
</tr>
<tr>
<td>ΔLog FMR, 2004-2005</td>
<td>0.675</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
</tr>
</tbody>
</table>

**First Stage**

Y: ΔLog Rent Ceiling, 2004-2010

<table>
<thead>
<tr>
<th>ΔLog Rent Ceiling, 2004-2010</th>
<th>0.125</th>
<th>0.199</th>
<th>0.414</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.036)</td>
<td>(0.029)</td>
</tr>
</tbody>
</table>

**IV Rent Estimate**

Y: ΔLog Voucher Rent, 2004-2010

Quality Measures: Bldg Age, Bldg Type & Median Tract Rent

<table>
<thead>
<tr>
<th>Unit of Observation</th>
<th>Address</th>
<th>Address</th>
<th>County-Bed</th>
</tr>
</thead>
<tbody>
<tr>
<td>n</td>
<td>290,731</td>
<td>553,577</td>
<td>12,375</td>
</tr>
</tbody>
</table>

Notes: This table shows the rent and quality impacts of a countywide or metrowide increase in the rent ceiling using variation from the 2005 Fair Market Rent (FMR) rebenchmarking. Standard errors shown in parentheses are clustered at FMR group level. See Section 4.1 for details.

a. Sample contains households whose address (9-digit zip code) was unchanged from 2004 to 2010.
b. Sample contains addresses where a new voucher recipient arrived in 2005 or later and a different voucher recipient was observed in 2003 or 2004.
c. Sample contains all tenants. Hedonic coefficients are estimated on nonvoucher units in the American Community Survey; these coefficients are applied to the voucher units to predict unit quality. See Appendix B.4 for details on hedonics.
Table 2 - Effect of County/Metrowide Price Ceiling Increase on Prices and Quality [50th Percentile FMRs]

<table>
<thead>
<tr>
<th></th>
<th>Set Fair Market Rent at 50th Percentile of Local Nonvoucher Rents Instead of 40th Percentile</th>
<th>All Tenants</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Policy Variation</strong></td>
<td><strong>First Stage</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1(50th Pctl) x Post</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>Unit of Observation</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>County-Year</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>n</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>11829</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>IV Rent Estimate</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Log Rent Ceiling</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y: Log Voucher Rent</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.467</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.106)</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>Unit of Observation</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>County-Year</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>n</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>11829</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>IV Quality Estimate</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Log Rent Ceiling</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y: Log Unit Quality</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.0160</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>Unit of Observation</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Household</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>n</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>351039</td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table shows the rent and quality impacts of a metrowide increase in the rent ceiling using the introduction of 50th percentile FMRs in 39 metro areas using a difference-in-difference design. From 2000 to 2003, HUD surveyed a repeated cross-section of vouchers with detail comparable to the American Housing Survey, including questions such as “Over the last year, how many times have you called for maintenance or repairs?” and “If you had a problem with electricity or heat, how long did it take to fix?”. Because of data constraints for this period, we conduct an analysis with repeated cross-sections. We construct a money-metric in hedonic quality using data from HUD’s detailed Customer Satisfaction Survey and a hedonic regression for nonvoucher units in the American Housing Survey. Standard errors shown in parentheses are clustered at FMR group level. See Section 4.2 for details.
### Table 3 - Effect of Tilting Rent Ceilings to ZIP-level on Rents and Building Quality in Dallas

<table>
<thead>
<tr>
<th>Policy Variation</th>
<th>Set Fair Market Rent in Dallas Using ZIP-Level Data</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Same Address w/Same Voucher Tenant</td>
</tr>
<tr>
<td>Sample</td>
<td>(1)</td>
</tr>
<tr>
<td>First Stage</td>
<td></td>
</tr>
<tr>
<td>Log ZIP FMR*Post</td>
<td>0.572</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
</tr>
<tr>
<td>IV Rent Estimate</td>
<td>Y: Log Price Ceiling</td>
</tr>
<tr>
<td>Log ZIP Rent Ceiling*Post</td>
<td>0.126</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
</tr>
<tr>
<td>IV Quality Estimate</td>
<td>Y: Log Hedonic Quality</td>
</tr>
<tr>
<td>Log ZIP Rent Ceiling*Post</td>
<td>--</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>Control for ZIP FMR</td>
<td>Yes</td>
</tr>
<tr>
<td>Indicators for Bedroom-Year</td>
<td>Yes</td>
</tr>
<tr>
<td>n</td>
<td>21020</td>
</tr>
</tbody>
</table>

Notes: This table shows the rent and building quality impact of moving from a single, metro-wide FMR in Dallas to ZIP-level FMRs using a balanced panel of units in 2010 and 2013. The first panel shows the coefficient \( b \) from the first stage equation: \( \text{Rent}_\text{Ceiling} = a + b*\text{FMR}*\text{post} + \text{FMR} + e \). The second and third panels show the coefficient \( b \) from the second stage equation \( y = a + b*\text{Rent}_\text{Ceiling:\hat{}}*\text{post} + \text{FMR} + e \) where \( \text{FMR}*\text{post} \) is the instrument for \( \text{Rent}_\text{Ceiling:\hat{}}*\text{post} \). This coefficient is the treatment estimate for the effect of a $1 rent ceiling change on rents and unit quality. Column 1 sample uses observations where the tenant stayed in the same unit from 2010 to 2013. Column 2 sample analyzes addresses which had different voucher tenants in 2010 and 2013. Column 3 sample uses tenants who moved from 2010 to 2013. The quality measure in the bottom panel is the same ACS model used in Table 1. Standard errors are clustered by ZIP (n=135 for stayers, 132 for movers). See Section 5.1 for details. See Appendix B.4 for details on hedonics.
<table>
<thead>
<tr>
<th></th>
<th>Fort Worth (Control)</th>
<th>Dallas (Treatment)</th>
<th>Differences</th>
<th>Diff-in-Diff (ITT)</th>
<th>Diff-in-Diff (TOT)</th>
<th>St'dized Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre</td>
<td>Post</td>
<td>Pre</td>
<td>Post</td>
<td>(2)-(1)</td>
<td>(4)-(3)</td>
</tr>
<tr>
<td>Poverty Rate(^{a})</td>
<td>0.174</td>
<td>0.172</td>
<td>0.210</td>
<td>0.199</td>
<td>-0.001</td>
<td>-0.011</td>
</tr>
<tr>
<td>Test Scores(^{b})</td>
<td>-0.719</td>
<td>-0.707</td>
<td>-0.494</td>
<td>-0.445</td>
<td>0.012</td>
<td>0.049</td>
</tr>
<tr>
<td>Unemployment</td>
<td>0.096</td>
<td>0.097</td>
<td>0.107</td>
<td>0.104</td>
<td>-0.001</td>
<td>-0.003</td>
</tr>
<tr>
<td>Single Mothers</td>
<td>0.363</td>
<td>0.356</td>
<td>0.381</td>
<td>0.370</td>
<td>-0.008</td>
<td>-0.011</td>
</tr>
<tr>
<td>Violent Crime(^{c})</td>
<td>0.0067</td>
<td>0.0066</td>
<td>0.0151</td>
<td>0.0138</td>
<td>-0.0001</td>
<td>-0.0013</td>
</tr>
<tr>
<td>Nhood Index(^{d})</td>
<td>-0.700</td>
<td>-0.684</td>
<td>-1.105</td>
<td>-0.986</td>
<td>0.017</td>
<td>0.118</td>
</tr>
<tr>
<td>Rent (2010 $)</td>
<td>709</td>
<td>700</td>
<td>796</td>
<td>777</td>
<td>-8</td>
<td>-19</td>
</tr>
<tr>
<td>n</td>
<td>7,203</td>
<td>7,038</td>
<td>19,315</td>
<td>19,399</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Moved</td>
<td>3,041</td>
<td>8,899</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

a. Poverty rate, unemployment, and share of kids in families with single mothers 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 metro 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. Intent-to-Treat Estimates. Standard errors for Diff-in-Diff estimate in column (7) are clustered at the tract level are in parentheses.
f. Treatment-on-Treated Estimates. Column (7) divided by the fraction of Dallas tenants who moved to a new unit.
g. 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 metro area.
## Table 5 - Comparison of Policies to Improve Neighborhood Quality

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Voucher with ZIP-Level FMR vs. Metrowide FMR</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tilting Rent Ceiling (Dallas)</td>
<td>21.0%</td>
<td>18.9%</td>
<td>151</td>
<td>125</td>
</tr>
<tr>
<td><strong>Voucher vs. Public Housing</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Moving to Opportunity Experimental</td>
<td>42%</td>
<td>18%</td>
<td>234</td>
<td>128</td>
</tr>
<tr>
<td>Moving to Opportunity Section 8</td>
<td>42%</td>
<td>28%</td>
<td>234</td>
<td>211</td>
</tr>
<tr>
<td>Lottery from Chicago Public Housing</td>
<td>48%</td>
<td>22%</td>
<td>219</td>
<td>201</td>
</tr>
<tr>
<td><strong>Voucher vs. No Voucher</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lottery from Chicago Private Housing</td>
<td>25.7%</td>
<td>24.6%</td>
<td>167</td>
<td>166</td>
</tr>
</tbody>
</table>

Notes: "Treat" is constructed as control mean plus impact estimate for Treatment-on-Treated. Poverty Rate and Violent Crimes per 10,000 residents are tract level data. Cost: Annual cost of Dallas program is from Table 4. Annual cost of a voucher subsidy is equal to 12 times contract rent plus utility allowance minus tenant contribution from Table 1. Annual cost of moving someone from public housing to a voucher is cost of voucher subsidy from Table 1 minus annual ongoing maintenance cost of a public housing unit (estimated as $3,155/year by Abt Associates, 2010).

**Predicted Impact on Child Income Rank** Chetty, Hendren, Kline and Saez (2014) document heterogeneity in intergenerational mobility across US commuting zones. Chetty and Hendren (2014) estimate that 2/3 of the cross-sectional variation is causal. We estimate the impact of the poverty rate and the violent crime rate on the income rank of a child whose parents are at the 10th percentile of the income distribution using their published data. Under the assumption that the cross-CZ coefficients are accurate for the causal impacts of tract-level variation in neighborhood quality, we can calculate the impact of each mobility policy on income of a child who experiences each policy at age 0 and stays in that location until age 18.

**Sources for Poverty and Crime Impacts**: Moving to Opportunity results from Table 2, Kling Ludwig and Katz (2005). Lottery from Chicago Public Housing from Table 2, Jacob, Ludwig, and Miller (2013). Lottery from Chicago Private Housing from Table V, Jacob and Ludwig (2012).
A Model Appendix

A.1 Welfare

The welfare gain of someone offered a voucher is $W_{\text{offered}} = \mathbb{P}V(q)$ and the low-income population’s gain from the program is $W = s\mathbb{P}V(q)$.

\[
\frac{dW_{\text{offered}}}{dp_{\text{base}}} = \frac{\partial \mathbb{P}}{\partial p_{\text{base}}} V(q) + \mathbb{P}V'(q) \frac{\partial q^*}{\partial p_{\text{base}}}
\]

\[
\frac{dW}{dp_{\text{base}}} = s \frac{dW_{\text{offered}}}{dp_{\text{base}}} + \frac{\partial s}{\partial p_{\text{base}}} \mathbb{P}V(q)
\]

Recall that the government budget constraint is $s\mathbb{P}_{\mu_{\text{voucher}}} = G$. Totally differentiating the budget constraint with respect to $p_{\text{base}}$ gives

\[
\frac{\partial s}{\partial p_{\text{base}}} = \frac{s}{\mathbb{P}_{\mu_{\text{voucher}}}} \left( -\frac{\partial \mathbb{P}}{\partial p_{\text{base}}} \mu_{\text{voucher}} - \mathbb{P} \left( \frac{\partial \mu_{\text{voucher}}}{\partial p_{\text{base}}} + \frac{\partial q^*}{\partial p_{\text{base}}} \right) \right)
\]

Aggregate welfare changes by

\[
\frac{1}{s} \frac{dW}{dp_{\text{base}}} = \left( \frac{\partial \mathbb{P}}{\partial p_{\text{base}}} V(q) + \mathbb{P}V'(q) \frac{\partial q^*}{\partial p_{\text{base}}} \right) + \mathbb{P}V(q) \left( -\frac{\partial \mathbb{P}}{\partial p_{\text{base}}} \frac{1}{\mathbb{P}} - \frac{1}{\mu_{\text{voucher}}} \left( \frac{\partial \mu_{\text{voucher}}}{\partial p_{\text{base}}} + \frac{\partial q^*}{\partial p_{\text{base}}} \right) \right)
\]

\[
= \frac{\partial \mathbb{P}}{\partial p_{\text{base}}} \left( V(q) - \frac{\mathbb{P}V(q)}{\mu_{\text{voucher}}} \right) + \frac{\partial q^*}{\partial p_{\text{base}}} \frac{1}{\mathbb{P}} \left( \Delta \text{Incumbent Quality} - \frac{V(q)}{\mu_{\text{voucher}}} \frac{\partial \mu_{\text{voucher}}}{\partial p_{\text{base}}} \right) - \frac{\mathbb{P}V(q)}{\mu_{\text{voucher}}} \frac{\partial \mu_{\text{voucher}}}{\partial p_{\text{base}}}
\]

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.

Throughout the paper, all specifications use log rent 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.
B.2 2005 FMR Rebenchmarking

Constructing the FMR Cells: We use HUD’s published Fair Market Rent rates, with slight modifications (http://www.huduser.org/portal/datasets/fmr.html). Fair Market Rents are published on an annual basis corresponding to the federal fiscal year, so FY2005 rents were effective from October 1, 2004 to September 30, 2005. FMR geographies are largely stable over time; HUD added 14 new city geographies in Virginia, and we code prior FMRs for these cities using the county-level FMRs. Our policy variation is at the county-bed cell level and measurement error $\varepsilon_{2000} - \varepsilon_{1990}$ 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. 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. All documentation associated with the rebenchmarking is posted at http://www.huduser.org/portal/datasets/fmr/fmr2005r/index.html. For these counties, we recode the FMR back to its pre-lobbying level. Coincident with the rebenchmarking, HUD administered Random Digit Dialing (RDD) surveys in 49 metropolitan areas. The results from these surveys, where available, superseded the results from the 2000 Census. Since these surveys were initiated and administered by HUD, we are less concerned about endogeneity of this data source, and we use the post-RDD FMRs for these areas. For these areas, the orthogonality restriction is that rental market changes from 1990 to 2004 need to be uncorrelated with subsequent short-run changes (\(E(\Delta r_{Nonvoucher}^{2004-1990} | \Delta FMR) = 0\)). Finally we drop eight geographies, with specific reasons listed below.

Places Dropped – Reason

- Miami, FL, Honolulu, HI, Navarro County, TX, and Assumption Parish, LA – rebenchmark in 2004
- Okanogan County, WA – Lobbied for higher FMR in 2005, no counterfactual available
- Louisiana – Hurricane Katrina severely disturbed rental markets (among other things)
- Kalaawo County, HI – No FMR published before 2005

Measuring the First Stage: The administrative data report the rent ceiling \(\bar{r}\) at the household level. Although much of our analysis limits the voucher sample in various ways (e.g. stayers, movers), we always compute \(\bar{r}_{jt}\) as the unconditional mean of all observations in a county-bed-year cell.

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

B.3 Nonvouche Rents and 2005 FMR Rebenchmarking

In Section 4.1, our key identification condition is

\[ E(\Delta r_{Nonvoucher}^{2004-1990} | \Delta FMR) = 0 \]

Here we examine the correlation of the FMR change with contemporaneous changes in nonvoucher rents. Data availability make it difficult to measure nonvoucher rents at a high frequency and with a high degree of geographic specificity. (Recall that these difficulties are exactly what generated the policy variation we study here!) Using the notation developed in Section 4.1,

\[ \text{Cov}(\Delta \bar{r}_t, \Delta FMR) = \text{Cov}(\bar{r}_t + \varepsilon_t - r_{2000} - \varepsilon_{2000}, \Delta FMR) = \text{Var}(\varepsilon_{2000}) < 0 \]  \hspace{1cm} (10)

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

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

$$r_{2005-2009,i}^{\text{Nonv}} - r_{2000,i}^{\text{Nonv}} = \alpha + \beta_1 \Delta FMR_j + \varepsilon_{ij}$$

where $\Delta FMR_j$ 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 $\Delta FMR_j$ is a function of the change in Census rents from 1990 to 2000, there is a mechanical negative correlation between FMR changes and Census rent changes from 2000 to a later date. This generates a sharp contrast – places with relative increases in voucher rents had relative decreases in nonvoucher rents. This mean reversion pattern is most pronounced in rural areas. When we limit the sample to counties with at least 100,000 residents, we find that $\beta_1$ is not statistically different from zero (column 4). Finally, we pool the observations in columns 1 and 2 to estimate $\Delta r_{ij}^{\text{Voucher,Nonv}} = \alpha + \beta_1 \Delta FMR_j + \beta_2 \Delta FMR_j \times Voucher_{ij} + \varepsilon_{ij}$ where $Voucher_{ij}$ is an indicator for whether the rental change is observed for voucher stayers or nonvouchers. Then, we compute the probability that we would observe data like this or more extreme, under the null hypothesis that the two coefficients are equal ($\beta_1 = \beta_2$), and find $p < 0.01$. Likewise, we find that the probability $\beta_1 = \beta_2$ for in the urban sample is very low.

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

### B.4 Hedonic Quality

We 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,

---

28 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.

29 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 $\Delta Y_{j,\text{tract}} = \alpha + \beta \Delta Y_{j,\text{county}} + \varepsilon_j$ would yield a biased estimate $\hat{\beta} - \beta = -\frac{\text{Var}_{\text{tract}}(\eta)}{\text{Var}_{\text{county}}(\eta)}(1 - \rho)$, and $\text{Var}(\eta) \approx \text{Var}(\Delta Y_{j-1})$. Analyzing tract-level rent changes indicates that $\text{Var}(\eta) \approx \text{Var}(\Delta Y_{j-1})$, $\rho = 0.88$. Tracts in counties with 40,000 units or more have small values of $\text{Var}_{\text{tract}}(\eta)$, such that $\hat{\beta} - \beta = -0.005$ and tracts in counties with less than 40,000 units have large $\text{Var}_{\text{tract}}(\eta)$, resulting in $\hat{\beta} - \beta = -0.070$. 

---

41
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

\[ \log(\text{Rent}_{ijklm}) = \alpha + \text{Bed}_j + \text{StructType}_k + \text{Age}_l + \text{PUMA}_m + \varepsilon_i \] (11)

where \( \text{Bed}_j \) is a set of indicators for 5 possible numbers of bedrooms, \( \text{StructType}_k \) is a set of indicators for 6 possible structure types, \( \text{Age}_l \) is a set of indicators for 10 possible structure age bins, and \( \text{PUMA}_m \) is a set of indicators for 2,069 PUMAs. This regression computes a vector of hedonic coefficients \( \hat{\beta}_{\text{census}} \). 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 for bedrooms, structure type and building age to construct a measure of hedonic unit quality \( q^{\text{hedonic}} = \hat{\beta}_{\text{census}} x_{\text{voucher}} + r_{\text{tract}}^{\text{voucher}} \) where \( r_{\text{tract}}^{\text{voucher}} \) is the median tract rent. The standard deviation of actual rent is $497 and the standard deviation of predicted rent is $331.

For our Dallas analysis in Table 3, where we are interested in only structure quality and not neighborhood quality, we instead compute \( q^{\text{hedonic}} = \hat{\beta}_{\text{census}} x_{\text{voucher}} \), omitting neighborhood quality.

### B.5 Dallas ZIP-Level FMRs

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

**Constructing the Neighborhood Quality Measures:** Tract-level data on poverty rate, unemployment rate, and share with a bachelor’s degree are for 2006-2010 in the American Community Survey. Tract-level 2010 violent crime offense data was provided to HUD by the Dallas Police Department under a privacy certificate between HUD and Dallas (March 2012). Data on the percent of 4th grade students’ scoring proficient or higher on state exams in the 2008-2009 academic year was provided to HUD by the U.S. Department of Education. We map these scores to zoned schools at the block group level. “Single Mothers” is defined as share of own children under 18 living with a female householder and no husband present.
Notes: The top panel plots average Fair Market Rent (FMR) changes at the county-level within year-specific quartiles. The large swings in 1994-1996 and 2005 reflect decennial rebenchmarkings, when new Census data from 1990 and 2000 respectively were incorporated into the FMRs.

The bottom panel plots FMR changes for the same sample within quartiles defined over the 2004-2005 FMR change, as in Figure 1. The four groups exhibit similar trends in terms of changes prior to the rebenchmarking. There is some evidence of mean reversion: places which had higher revisions from 1997 to 2004 were revised downward in 2005. The dashed lines represent a counterfactual of what the magnitude of annual changes would have been if a single national index had been applied from 1997 through 2004, followed by an update which brought FMRs to observed 2005 levels. Observed revisions are larger than the counterfactual revisions, indicating substantial measurement error in intercensal FMR changes.
APPENDIX FIGURE 2 – Distribution of Rent and Quality

Notes: 2009 data, n=1.7 million. The top panel plots rents and hedonic quality relative to the local rent 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 rent ceiling, which is set in terms of gross rent. In the rest of the paper, we use contract rent alone, to focus on landlord behavior.

The bottom panel plots conditional means of unit rent for twenty quantiles of hedonic quality. We include fixed effects for the number of bedrooms interacted with the county, because each voucher recipient’s number of bedrooms is fixed by family size and it is usually quite difficult to switch counties. We find that a $1 increase in hedonic quality is associated with a 36 cent increase in rents. This indicates that even for a fixed rent ceiling, the government paid less for lower-quality units.
APPENDIX FIGURE 3 – First Stage for Rebenchmarking

Notes: The top panel of this figure shows the impact of the 2005 FMR rebenchmarking on the locally-set rent ceiling. FMR varies at the county-by-bedroom level. We regress $Rent_{Ceiling_t} - Rent_{Ceiling_{2004}} = \alpha + \beta \Delta FMR_{2004-2005} + \varepsilon$ and plot coefficients and a 95% confidence interval for $\beta$. Rent ceilings were falling in places that were subsequently revised up and rising in places that were subsequently revised down.

The bottom panel of this figure analyzes changes in rents for the full voucher sample and plots $\beta$ coefficients from the IV regression with second stage $r_t - r_{2004} = \alpha + \beta \Delta Rent_{Ceiling_t} + \eta$. 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. Rents were rising in places which received a negative shock to FMR in 2005 and falling in places which received a positive shock to FMR.
### Appendix Table 1 - Summary Statistics for Across-the-Board Rent Ceiling Changes

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>S.D.</th>
<th>Mean</th>
<th>S.D.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td><strong>Rebenchmarking -- National Sample</strong>&lt;sup&gt;a&lt;/sup&gt;</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Voucher Characteristics 2004 (n = 1,578,124) &amp; 2010 (n=1,665,868)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Contract Rent</td>
<td>495</td>
<td>238</td>
<td>586</td>
<td>266</td>
</tr>
<tr>
<td>Utility Allowance</td>
<td>106</td>
<td>65</td>
<td>144</td>
<td>89</td>
</tr>
<tr>
<td>Rent Ceiling (Contract Rent + Utility)</td>
<td>618</td>
<td>278</td>
<td>762</td>
<td>296</td>
</tr>
<tr>
<td>Tenant Payment</td>
<td>238</td>
<td>154</td>
<td>288</td>
<td>184</td>
</tr>
<tr>
<td>Tenant HH Income (Annual)</td>
<td>9683</td>
<td>6358</td>
<td>11567</td>
<td>7347</td>
</tr>
<tr>
<td>Share Moved</td>
<td>Nonattrit</td>
<td>0.21</td>
<td>0.41</td>
<td>0.16</td>
</tr>
<tr>
<td><strong>Tract Characteristics</strong>&lt;sup&gt;b&lt;/sup&gt;</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Median Contract Rent (2005-2009)</td>
<td>473.70</td>
<td>196.26</td>
<td>479.55</td>
<td>197.97</td>
</tr>
<tr>
<td>Share Voucher (2004)</td>
<td>0.021</td>
<td>0.024</td>
<td>0.019</td>
<td>0.022</td>
</tr>
<tr>
<td><strong>County Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fair Market Rent</td>
<td>628</td>
<td>312</td>
<td>802</td>
<td>326</td>
</tr>
<tr>
<td>40th -&gt; 50th Pctile FMRs -- National Sample&lt;sup&gt;c&lt;/sup&gt;</td>
<td>Pre (n = 171,248) &amp; Post (n = 285,279)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gross Rent</td>
<td>547</td>
<td>167</td>
<td>620</td>
<td>213</td>
</tr>
<tr>
<td>Hedonic Quality (using 28 survey vars)</td>
<td>613</td>
<td>237</td>
<td>628</td>
<td>247</td>
</tr>
<tr>
<td>Fair Market Rent</td>
<td>589</td>
<td>186</td>
<td>648</td>
<td>242</td>
</tr>
</tbody>
</table>

Notes:

a. Voucher and tract characteristics are computed giving equal weight to each county-bed pair.
### Appendix Table 2 - Placebo Tests with Nonvoucher Rents [Rebenchmarking]

<table>
<thead>
<tr>
<th>Policy Variation</th>
<th>Dep Var: Change in Log Rent</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Voucher</td>
</tr>
<tr>
<td>Sample</td>
<td>All Units</td>
</tr>
<tr>
<td>Time Horizon</td>
<td></td>
</tr>
<tr>
<td>04-09</td>
<td>00-09</td>
</tr>
<tr>
<td>Data Source</td>
<td>HUD Admin(^a)</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
</tr>
</tbody>
</table>

\[ \Delta \text{Log FMR, 2004-2005} \]

- (1) 0.0831  -0.046  0.175  0.066  -0.193  0.021
- (2)  (0.0179)  (0.020)  (0.049)  (0.049)  (0.102)  (0.099)

- Voucher Coef \(\neq\) Nonvoucher Coef
- 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 rents. Regressions give equal weight to each county-bed pair. Standard errors shown in parentheses are clustered at FMR group level (n=1,484). See Appendix 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.
### Appendix Table 3 - Robustness Checks for Voucher Prices [Rebenchmarking]

#### Policy Variation: Rebenchmarking of FMRs in 2005

\[
\beta \text{ from } \Delta \text{Rent, 2004-2010} = \alpha + \beta \ast \Delta \text{Rent Ceiling, 2004-2010} + \eta \quad \text{(Second Stage)}
\]
\[
\Delta \text{Rent Ceiling, 2004-2010} = \alpha + \gamma \ast \Delta \text{FMR, 2004-2005} + \varepsilon \quad \text{(First Stage)}
\]

#### Rent Baseline from Table 1

<table>
<thead>
<tr>
<th>Description</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) ΔRent winsorized at 1st and 99th percentile</td>
<td>0.129</td>
<td>(0.0249)</td>
</tr>
<tr>
<td>Lived at same 9-digit zip in 2004 &amp; 2010</td>
<td>0.0859</td>
<td>(0.0348)</td>
</tr>
<tr>
<td>Weight each county-bed pair equally (n=290,731)</td>
<td>0.0871</td>
<td>(0.0329)</td>
</tr>
</tbody>
</table>

#### Add Controls

- (2) Add County Fixed Effects
- (3) IV for current price ceiling with 2005 FMR, controlling for 2004 price ceiling and FMR

<table>
<thead>
<tr>
<th>Description</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>(4) Units unlikely to be paying final dollar (n=127,092)(^a)</td>
<td>0.149</td>
<td>(0.0379)</td>
</tr>
<tr>
<td>(5) Units with low kickback potential (Owner has at least 10 voucher units, n=109,075)</td>
<td>0.0913</td>
<td>(0.0473)</td>
</tr>
<tr>
<td>(6) Units with above median concentration of voucher units (n=132,314)</td>
<td>0.157</td>
<td>(0.0434)</td>
</tr>
</tbody>
</table>

#### Subsample

- (7) Weight every household equally

<table>
<thead>
<tr>
<th>Description</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>(4) Units unlikely to be paying final dollar (n=127,092)(^a)</td>
<td>0.280</td>
<td>(0.0606)</td>
</tr>
</tbody>
</table>

#### Placebo Dependent Variable: Tenant Portion of Rent

- (8) Units unlikely to be paying final dollar with nonmissing tenant income (n=126,146)

<table>
<thead>
<tr>
<th>Description</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>(8) Units unlikely to be paying final dollar with nonmissing tenant income (n=126,146)</td>
<td>-0.0116</td>
<td>(0.0404)</td>
</tr>
</tbody>
</table>

**Notes:** This table shows robustness checks for estimating the impact of a countywide increase in the rent 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).

\(^a\) Units unlikely to be paying the "final" dollar of rent in 2010 are those with two or fewer bedrooms and a value of rent minus rent ceiling in the bottom three quintiles in 2004. The probability that these households have rent higher than the rent ceiling -- and therefore pay more when the landlord raises the rent -- is 11%.
### Appendix Table 4 - Mobility Counseling in Dallas

<table>
<thead>
<tr>
<th>Sample</th>
<th>N</th>
<th>Before Move</th>
<th>After Move</th>
<th>Change</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Total Movers</td>
<td>8189</td>
<td>-1.10</td>
<td>-0.92</td>
<td>0.19</td>
</tr>
<tr>
<td>(2) Movers With Mobility Counseling</td>
<td>303</td>
<td>-0.94</td>
<td>0.23</td>
<td>1.17</td>
</tr>
<tr>
<td>(3) Movers Without Mobility Counseling</td>
<td>7886</td>
<td>-1.11</td>
<td>-0.96</td>
<td>0.15</td>
</tr>
</tbody>
</table>

Notes: This table decomposes the neighborhood quality improvement in Dallas for households which received vouchers in 2010 and moved by 2012 by receipt of voluntary mobility counseling. This counseling was offered to all voucher Data in row (1) are locations in 2010 and 2012 for all movers and come from HUD administrative records. Data in row (2) are locations immediately prior to and after moving and come from the Inclusive Communities Project, which provided the counseling. Data in row (3) are calculated as $y_{notCounseled} = (y_{all} - shareCounseled*y_{counseled})/(1 - shareCounseled)$.