Estimation of Welfare Effects in Hedonic Difference-in-Differences: The Case of School Redistricting*

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Abstract

Difference-in-differences (*DID*) estimation that identifies the capitalization of amenities through changes in housing prices has been widely used. However, there are concerns about how to interpret the estimates of capitalization from *DID* as changes in welfare. Here we show how this divergence between capitalization and welfare might arise. Following Banzhaf (2021), we estimate the capitalization of school redistricting in a *DID* framework that incorporates general equilibrium effects. When comparing estimates from our generalized *DID* model to the conventional *DID* model, we find significant differences in both the capitalization effects and welfare changes associated with the school redistricting.

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1 Introduction

Hedonic models, specifically those focused on the determinants of housing prices, have been used extensively to elicit estimates of the value of goods and services in the absence of explicit market prices for these goods. Within this literature, there has been a trend of implementing difference-in-differences (*DID*) models to value quality-differentiated goods, such as air quality (Chay and Greenstone, 2005), water quality (Muehlenbachs, Spiller, and Timmins, 2015), brownfield (Ma, 2019), flood risk (Bakkensen, Ding, and Ma, 2019), and school quality (Collins and Kaplan, 2022). In a framework for hedonics outlined in Rosen (1974), the *DID* estimand that identifies the changes in housing prices associated with changes in amenities, the "capitalization effect."

However, the assumptions that the control group is stable over time (*SUTVA*) or the gradient of price is time-constant (*TCGA*) in traditional *DID* hedonic models are likely to be violated if changes in local amenities are large or if there is resorting of residents. This raises a question about how reliable the standard *DID* estimate is in capturing the capitalization effect when local policies create spillovers (Clarke, 2017; Butts, 2021; Alves, Burton, and Fleitas, 2024).

Additionally, capitalization is not the same as the marginal willingness to pay (*MWTP*) (Klaiber and Smith, 2013; Kuminoff and Pope, 2014) because the changes in prices mix information from two cross-sectional hedonic price functions. This may not be an issue if the hedonic price function is stable over time and changes in house attributes and shocks to amenities are small or if a small share of the housing market is "treated."[1] However, if the shocks or the treatment group are large, general equilibrium spillovers are likely to exist and *SUTVA* is violated – there is a shift of the hedonic price functions and *DID* estimates of capitalization do not equal *MWTP*. This means we cannot interpret coefficient estimates from the *DID* directly as measures of welfare changes.

In this paper, we embrace the challenges around estimating both the capitalization and welfare effects in *DID* hedonics, focusing on recent school redistricting in Fayette County, Kentucky. This redistricting changed school boundaries for five existing high schools and opened a new high

[1]For instance, Koster and van Ommeren (2022) examine the neighborhood changes in Netherlands and argue the percentage of treated houses is only 4-5%, which is less likely to bias the results.

school. In addition to being a example of a discrete revision in local policy well-suited for *DID* as seen in studies including Ries and Somerville (2010) and Collins and Kaplan (2022), changes in school catchment areas and the opening of new schools occur frequently – over 1,000 schools changed boundaries and 258 new schools opened in 2020-21 alone. These changes in schooling can mean significant changes in school quality, housing prices, and welfare for households directly affected by the changes and, importantly, possibly for other households in the same housing market not directly affected by the changes. Importantly for this study, these boundary changes often affect a large share of the households in the market. In our application, the revision of high school catchment areas and the opening of a new high school in Fayette County (Lexington), Kentucky, over twenty percent of all households in the county were redistricted to a different high school with forty percent of households in one high school redistricted to other high schools.

To understand the implications of these general equilibrium effects on both estimates of capitalization and welfare, we engage in two alternative exercises. First, we construct a simple general equilibrium model of household location choices when districts differ in their provision of a public good (educational quality). This model and the numerical examples based on it focus on two reasons for the divergence between capitalization and welfare changes: 1) a change in preferences of the marginal individual, "Tiebout bias" (Goldstein and Pauly, 1981; Rubinfield, Shapiro, and Roberts, 1987); and 2) the jurisdiction changing policy has a large share of the market's population or "market power" (Hoyt, 1991; Agrawal, Hoyt, and Wilson, 2022). These examples show how an assumption underlying a *DID* hedonic model, the stability of property values in districts not changing policies (the comparison group) or *SUTVA*, is violated. Furthermore, the assumption of a time-constant gradient (*TCGA*) is subject to similar concerns as *SUTVA*. We demonstrate that failing to account for shifts in preferences among both the control and treatment groups over time can lead to a biased estimate of the capitalization effect within a simple *DID* framework.

From this model we also generate a sufficient statistic that we operationalize to obtain our welfare estimates that arise due to redistricting. Although our policy, altering the boundaries (area) of school catchment zones, differs from that examined by Banzhaf (2020), changes in public services in a single jurisdiction, our sufficient statistic demonstrates that aggregate, general equilibrium welfare effects can also be calculated by summing the changes in house values within the area directly affected by the policy.

To obtain welfare estimates of this redistricting we follow the methodology proposed by Banzhaf (2021) to control for changes in house attributes and amenities whenever possible. We first employ a discrete, semi-parametric approach to measure a school quality. Specifically, we utilize school dummies both before and after the school redistricting to capture the general equilibrium effects. In contrast to a single measure of school quality, such as test scores, these dummy variables capture the bundle of amenities that home buyers value in a school zone. We include a set of interactions between time, house attributes, and school characteristics to account for potential endogenous changes in the effects (coefficients on) these variables. The differences between two school dummies post-redistricting would imply the capitalization effect associated with switching schools while considering potential general equilibrium effect from shifts in preferences and changes in home and neighborhood characteristics. We find significant increases in property values in areas that have been rezoned from lower-performing schools to better-performing ones, with the magnitude of these changes aligning with school rankings based on test scores. Conversely, we find a similar but opposite effect for homes rezoned to lower-performing schools. To calculate the welfare effects, we multiply the number of homes, average home values, and the capitalization effect for each school-rezoning pair. These results constitute our baseline for assessing the general equilibrium welfare effects.

In addition, we also estimate a standard *DID* model without time-varying coefficients and preferences and compare the resulting estimates to those obtained using our semi-parametric approach. We observe substantial differences in the estimates of capitalization and their associated impacts on welfare, particularly concerning the new high school.

In contrast to our semi-parametric approach to characterize school quality (school dummy variables), numerous hedonic studies of schooling have measured quality in terms of test scores, school report cards, or racial composition (Figlio and Lucas, 2004; Clapp, Nanda, and Ross, 2008;

Ries and Somerville, 2010; Kuminoff and Pope, 2014). We, too, follow this approach using a mean *ACT* score as our measure of school quality. Analogous to our semi-parametric approach, we allow the coefficient on *ACT* as well as coefficients on other house attributes to vary between the pre- and post-redistricting periods. Subsequently, we calculate changes in welfare associated with redistricting across schools with varying *ACT* scores. Our findings show significant discrepancies in the welfare effects compared to our model using school dummy variables. The generalized *DID* model with school dummies has an estimated welfare effect of -$5.25 million; in contrast to the standard *DID* model that does not account for shifts in attributes and preferences has a welfare effect of $27.95 million. In an effort to reconcile these differences, we introduce specifications that incorporate additional school characteristics, including student demographics, graduation rates, student-to-teacher ratios, and behavioral events — factors frequently used in other studies to assess school quality (Downes and Zabel, 2002). After incorporating these factors, our welfare estimates align more closely with those derived using school dummy variables.

We see four important contributions. First, we contribute to the literature addressing concerns with using of *DID* models in hedonic estimation by showing that failure to account for the general equilibrium effects of large policy changes result in biased estimates of capitalization. Second, complementing the discussions found in Klaiber and Smith (2013), Kuminoff and Pope (2014), and Banzhaf (2021), we construct a simple general equilibrium model and use it to demonstrate how imprecise welfare evaluations may arise when using conventional *DID* methods. Third, we extend the framework in Banzhaf (2021) and construct a sufficient statistic to measure the welfare effect of a large policy change. Finally, educational quality, our application, is an important local policy and significant expenditure that has been the focus of a voluminous literature. Specific to the literature that employs hedonic estimation to evaluate school quality, we uncover substantial disparities in welfare estimates when comparing our semi-parametric approach to quantifying school quality with methods relying on test scores.

In the next section, we provide a review of related literature and offer some key distinctions between the approaches in these studies and the approach we take. In Section 3, we offer background information on school redistricting in Fayette County, Kentucky. We provide a discussion of the issues that arise in estimating *DID* hedonic models as well as two simple examples of when they occur in Section 4. Section 5 summarizes our data and discusses our empirical strategy. We present our results of estimation and welfare estimates in Section 6. Section 7 concludes.

2 Related Literature

A voluminous literature, spanning over fifty years, employs hedonic estimation to infer the valuation of public policies and amenities through their impacts on housing prices. Here we focus on hedonic studies of educational policies and, in particular, recent studies employing *DID* or other quasi-experimental approaches.

Difference-in-Differences Hedonics Pioneered by Black (1999), a large strand of literature has utilized boundary discontinuities to study the capitalization of school quality (Kane, Riegg, and Staiger, 2006; Dhar and Ross, 2012). One issue that arises in the estimation of boundary fixed effect models is the sorting of home buyers across district boundaries (Bayer, Ferreira, and McMillan, 2007). More recently, another strand of literature that utilizes exogenous changes in educational quality to identify differences in property values between those areas subject to the reforms and those areas that are not to alleviate the concerns of residential sorting has emerged. Bogart and Cromwell (2000) study the impact of redistricting schools on house values in Ohio and find that school closings resulted in dramatic decreases in house values. Ries and Somerville (2010) use a *DID* hedonic with repeated sales and find significant effects of the redistricting for top-quartile of homes. In a recent work, Collins and Kaplan (2022) look into school redistricting in Shelby County, Tennessee and they find that homes rezoned to higher-quality schools has a 2-3% appreciation in sale prices with a one standard deviation increase in test scores.

Even though *DID* hedonics have distinct advantages in overcoming several empirical challenges in cross-sectional hedonic estimation and boundary fixed effect models, two concerns remain when interpreting the estimated effects of redistricting. First, the timing and the scope of redistricting matters when estimating capitalization.[2] If redistricting is a lengthy process, with possibly years between its announcement and implementation, a simple two-period *DID* hedonic estimation may underestimate the true effect (Ding et al., 2024).[3] Further, while small adjustments along the existing school boundaries may not affect how homes capitalize school quality (Koster and van Ommeren, 2022), large changes in school catchment areas may affect the *SUTVA* assumption as highlighted in Banzhaf (2021). As well, the time-constant gradient assumption (*TCGA*) should also be tested before invoking it (Kuminoff and Pope, 2014). If redistricting results in a large share of homes being reassigned to different schools, the failure to account for shifts in the hedonic function and spillover effects from redistricted (treated) areas to (comparison) areas where there was no change in high school will introduce bias into the results, resulting in the estimates from the hedonic model to deviate from the actual capitalization effect and the *MWTP*. Finally, it is difficult to make welfare interpretations through quasi-experimental methods. While *DID* estimations are informative in understanding the average treatment effect, it is unclear about the welfare benefits from the *DID* estimand. Banzhaf (2021) shows the *DID* estimates represent a lower bound on the total welfare effects of the policy for all households and researchers should account for non-marginal changes in amenities and general equilibrium price effects, mobility responses, and endogenous responses to house attributes. We provide a more complete discussion of Banzhaf's explanation of the shortcomings of traditional *DID* in hedonics in Section 4.2 and follow his application on toxic air emissions to examine school quality.

What School Characteristics Affect House Values In contrast to estimating the value of a bundle of services and attributes of public schools using discrete changes (Ding et al., 2024), many studies relate school quality to specific school characteristics. One attributes that has received a great deal of attention is racial and ethnic composition. Bogart and Cromwell (2000) includes percent of nonwhite students in school as a control variable. Boustan (2012) finds that following desegregation of public schools housing prices in desegregated urban areas fell by 6 percent

[2] In a recent work, Bishop and Murphy (2019) discuss forward-looking hedonic models.

[3] In the case of the redistricting in Fayette County considered in both (Ding et al., 2024) and here, the interval between the announcement of redistricting and its implementation was over three years.

relative to its neighboring suburbs.

Test scores are a widely-used measure of school quality. Figlio and Lucas (2004) utilize school report cards that provide grades to represent the quality of schools. In a recent paper, Beracha and Hardin (2018) also use school grade to study the impact of school quality on the premium of renters and owners. They find that the price premium for school quality for owners exceeds the premium for renters. Liu and Smith (2023) uses Criterion-Referenced Competency Test (CRCT) scores in Georgia to construct both normalized test scores and percent of students did not meet the standard to represent school quality. Clapp, Nanda, and Ross (2008) shows that both test scores and racial composition affect property values. Utilizing boundary discontinuities Gibbons, Machin, and Silva (2013) use English and Math scores to represent school quality and find one standard deviation in these scores increases house prices by three percent.

Our paper complements these existing studies and further reveals that test scores may not be the single attribute to be considered by parents when school zones are subject to changes. As we show later, student behavior and graduation rate also significantly affect the capitalization effect of school quality.

3 Background of Redistricting in Fayette County

We utilize recent school redistricting in Fayette County, Kentucky to examine the welfare effects of changes in school catchment areas (zones) on the local housing market. Fayette County has a single school district, Fayette County Public Schools, that administers school assignment policies. As Fayette County has no open enrollment program nor any charter schools most students attend schools based on where they live.[4] Prior to 2014, there had been an average increase in enrollment of 600 to 750 students a year in the district.[5] Given these enrollment pressures, a redistricting process and planning for a new high school began in late 2013 with the opening of

[4]Fayette County does, however, have magnet programs that allow a limited number of students to attend schools other than the school to which they are zoned.

[5]In Appendix Figure B1, a plot of annual enrollment for each high school, the upward trend of increasing enrollment in most of the public high schools prior to 2016 is evident.

the new high school, Frederick Douglass, and the new school boundaries implemented in August 2017. As we have addressed the timing of the redistricting in Ding et al. (2024), in this study we restrict our sample to property sales that occurred prior to the announcement of redistricting (April 29, 2014) and that followed the approval of the plan (June 2015).

Figure 1 shows these changes in school boundaries with the dashed lines representing the pre-2017 catchment boundaries and the solid lines representing the post-2017 catchment boundaries from the redistricting. Under the new plan, the southeast part of the original Bryan Station High School was redistricted to the proposed school, Frederick Douglass. There are small geographical changes in the catchment areas of the other four high-schools. Based on these changes, we are able to determine the school catchment area for each house sold before and after the redistricting process. Appendix Table C1 reports the share of redistricted homes in each original high school zone using 2013 housing stock information from Fayette County assessment. Almost forty percent of Bryan Station homes were rezoned to a different school. Other high schools are also affected with vary degrees of homes affected by this change.

4 Hedonics in General Equilibrium

In this section we first summarize the discussion from Banzhaf (2021) on *DID* in hedonic models when the *SUTVA* assumption is violated, that is, when there are general equilibrium effects from policy changes in a single jurisdiction. Specifically, policy changes in one jurisdiction or, in our case, school zone, affect housing prices in other zones where there were no policy changes. These change in housing prices are a violation of *SUTVA*. Following a summary of Banzhaf's discussion, we present a simple model that provides an example of when and how estimates of the capitalization of a policy changes into housing prices cannot be interpreted as marginal willingness to pay (*MWTP*) for the policy.

4.1 Interpreting Difference-in-Differences Hedonics

Greenstone (2017), among others, notes there are a number of advantages of employing quasi-experimental estimation techniques such as regression discontinuity, border fixed-effects, or as done here, *DID* with hedonics. However, as noted by a number of studies, including Kuminoff, Parmeter, and Pope (2010), Klaiber and Smith (2013), Kuminoff and Pope (2014), and Banzhaf (2021), the coefficient on the *DID* term, that is the interaction of the variable denoting the treatment group and the treatment period in a regression on, in our case, log of sale price, cannot be directly interpreted as an estimate of *MWTP*. As Banzhaf (2021) notes, in terms of the vocabulary of the program evaluation literature, *SUTVA* is likely to be violated – even properties whose amenities, specifically school zones, are not changed will incur changes in their value.

As these studies point out, *DID* estimates confound *MWTP* estimates, movements along hedonic frontiers as in Rosen (1974), with shifts between hedonic frontiers caused by general equilibrium changes within the housing market. This point is nicely illustrated in Figure 2, a replication of Figure 1 in Banzhaf (2021).[6] In our case, a treated (rezoned) and matched control property both start at a price of p_A and have identical amenities, including schools. With rezoning the price of the untreated house (not rezoned) increases to p_B (distance IE), the indirect effect. This represents the shift in the hedonic function, the general equilibrium effect on housing prices throughout Fayette County. As Banzhaf (2021) argues, the total effect cannot be identified through the *DID* model if there is a temporal shift of the non-treated homes in the hedonic price function.

As the indirect effect is a change in housing price without any change in housing characteristics or amenities, it is simply a transfer between owner and renter with no associated welfare effects. However, for the treated (rezoned) property, educational quality increases from e^0 to e'. The distance DE is the partial equilibrium, utility-constant price change, the change in price that provides a lower bound on the welfare measure, *Hicksian equivalent surplus*. The total effect (TE)

[6] Appendix Figure B2 presents the original Figure 1 in Banzhaf (2021). We also find evidence that there is a shift in hedonic price functions in our data, as shown in Figure B3 where we graph the pre- and post-redistricting hedonic functions of housing prices and school ACT composite scores in Fayette County, Kentucky using local polynomial regressions.

includes both the direct effect and the indirect or general equilibrium effect. As both Kuminoff and Pope (2014) and Banzhaf (2021) demonstrate, the estimate of capitalization based on the difference-in-difference can severely underestimate the welfare effects of the treatment as it confounds the direct and indirect effects.

Another implicit assumption used in a standard hedonic *DID* model is the time-constant gradient assumption (*TCGA*). When *TCGA* holds, even though the capitalization effect is not consistent with the *MWTP*, it still reflects the correct capitalization. However, if *TCGA* fails, then the shape of the two hedonic price functions will change, and the estimated capitalization may not correctly reflect the direct effect.

4.2 A Simple Model of General Equilibrium Price Changes

We present the interpretation of hedonic estimates when districts are not small, "utility takers" (Hoyt, 1991; Agrawal, Hoyt, and Wilson, 2022) and when tastes for the public service (educational quality) are heterogeneous. Both the "market power" of the districts and the heterogeneous tastes or "Tiebout bias" (Goldstein and Pauly, 1981; Rubinfield, Shapiro, and Roberts, 1987) result in the coefficients from a hedonic equation not directly giving the marginal willingness to pay. To see this consider the (indirect) utility of residents of two districts be given by

$$V(e_i, p_i, \alpha(n)) = y + \alpha(n)\, g(e_i) - p_i \tag{1}$$

where e_i is educational quality and p_i is the price of a housing ***unit*** in district i.[7] The term α is a "taste" parameter for educational quality distributed across the population (n) with $\alpha' > 0$. We further assume the supply of housing units in each district is given by $H_i(p_i) = h_i(p_i)L_i$, $i = 1, 2$ where $h_i(p_i)$ is housing (and households) per unit of land and L_i is total in district i.

4.2.1 Equilibrium Conditions and Comparative Statics

Equilibrium requires that individuals choose the district in which their utility is maximized with the individual with tastes $\alpha(n_1)$ indifferent between the two districts,

[7] To simplify, we assume a homogeneous and inelastic demand for housing per resident, normalized to unity. Then underlying an indirect utility function of this form is a utility function of the form $U_i = x_i + \alpha g(e_i)$ with a budget constraint of $y_i = x_i + p_i$ where y_i is income and. x_i is consumption of a private commodity.

$$\alpha(n_1)g(e_1) - \gamma(p_1) = \alpha(n_1)g(e_2) - \gamma(p_2) \tag{2}$$

Given the identical demands for housing and absence of other local amenities, if $e_1 < e_2$ then individuals with $\alpha < \alpha(n_1)$ reside in district 1 and those with $\alpha > \alpha(n_1)$ reside in district 2 with the individual(s) with $\alpha = \alpha(n_1)$ indifferent between the two districts. In addition to the equal utility condition, the housing market needs to clear,

$$n_1(p_1) + n_2(p_2) = N, \tag{3}$$

where $n_i, i = 1, 2$, is the population of district *i* with $\frac{\partial n_i}{\partial p_i} > 0$ [8] and N is total population. Totally differentiating (3) we have

$$\frac{dp_2}{dp_1} = -\frac{n_1\theta_1}{n_2\theta_2} \tag{4}$$

where $\theta_i = \frac{\partial n_i}{\partial p_i}\frac{1}{n_i}$ is the semi-elasticity of population with respect to the price of housing. Then differentiating (2) with respect to e_1 and applying (4) gives

$$\underbrace{\frac{\partial p_1}{\partial e_1}}_{TE} = \underbrace{\alpha g'(e_1)}_{DE} + \underbrace{\left[\underbrace{-\frac{n_1}{n_2}}_{\frac{dp_2}{dp_1}} + \underbrace{\alpha(-1)^a\left[g(e_1) - g(e_2)\right]\varepsilon\theta}_{\frac{\partial\alpha(n_1)}{\partial n_1}\frac{\partial n_1}{\partial p_1}}\right]\frac{\partial p_1}{\partial e_1}}_{IE} \tag{5}$$

[9]

where $\varepsilon = \frac{\alpha'(n)}{\alpha(n)}/\frac{1}{n_1}$ is the elasticity of the taste for educational quality (α) with respect to population and $a = 1(2)$ if $e_1 < (>)e_2$. In terms of Banzhaf (2021), the direct effect (DE) is the term $\alpha g'(e_1)$ in (5).

The "indirect" term is composed of two terms. The first is effect of changes in p_1 on p_2, dp_2/dp_1. As seen in (5) this term depends on ratio n_1/n_2 and, as $\partial p_1/\partial e_1 > 0$, it acts to reduce the magnitude of TE with the larger the market share of district 1, the less the increase in education quality is capitalized into property values. The second term is the change in the difference in the valuation of

[8] That $\frac{\partial n_i}{\partial p_i} > 0$ follows from the fact that $n_i = H(p_i)$ and $\frac{\partial H_i}{\partial p_i} > 0$.

[9] Solving (5) for $\frac{\partial p_1}{\partial e_1}$ gives $\underbrace{\frac{dp_1}{de_1}}_{TE} = \underbrace{\alpha g'(e_1)}_{DE} - \underbrace{\frac{\left[\frac{n_1}{n_2}\frac{p_2}{p_1} - \alpha(-1)^a\left[g(e_1) - g(e_2)\right]\frac{\varepsilon}{p_1}\right]\theta}{\left[1 + \frac{n_1}{n_2}\frac{p_2}{p_1} - \alpha\left[g(e_1) - g(e_2)\right]\frac{\varepsilon\theta}{p_1}\right]}(\alpha g'(e_1))}_{IE}$

education quality between the two district, $\alpha(n)[g(e_1) - g(e_2)]$, with changes in the preferences of the "marginal" individual. How much the preferences of the marginal individual will change depends on the product of the elasticity of α, (ε), the elasticity of population, (θ), and the change in p_1, $\partial p_1/\partial e_1$. If $e_1 > (<) e_2$ α for residents of 1 are greater (less) than α for residents of 2 and the increase in n_1 will decrease (increase) $\alpha(n_1)$ requiring p_1 to decrease (increase).

Then from (5) we see that changes in property values in district 1, $\frac{\partial p_1}{\partial e_1}$, do not equal DE *(MWTP)*, if it has a significant market share, $n_1/n_2 \gg 0$, or if the tastes of the marginal individual and population changes when p_1 changes, $\varepsilon\theta \neq 0$, and the difference in educational quality, $\alpha(n)[g(e_1) - g(e_2)]$, is large.

4.3 Numerical Examples

We consider two simple numerical examples to illustrate to what extent the slope of the hedonic estimated may deviate from the constant-utility hedonic that has the *MWTP* as its slope. In our first example, we assume identical preferences but allow for the district changing its education quality to have a significant share of the market population. Our second example assumes that districts are small (atomistic) but tastes vary across the population, resulting in sorting of the population by tastes for education and changes in the valuation of education quality by the (marginal) resident indifferent between the two districts.

We assume a quasi-linear utility function of the form $U(x, e, h) = x + \alpha ln(e)$ with $\alpha = .2$ and income $y = 1$. Then elasticity of housing supply and, therefore, population, is $\theta = -1$. We set $e_2 = 1$ then vary e_1 in the range $[0.4,\ 1.8]$, solving for the equilibrium values of p_1 and p_2 for different population shares of the two districts. We can see the results of these simulations in Figure 3a. In the figure we can see that when district 1 has a small share of the population, 10% or less, the price lines are quite close to the atomistic case $(n_1 = 0)$ particularly for relatively small changes in e_1 with more pronounced differences with the atomistic case when the population of district 1 is 50% as shown in the figure.

In Figure 3b we highlight the distinction between the price gradient when district 1 is atomistic

$(\frac{n_1}{n_2} \to 0)$ (dark blue line) and utility is constant and when it has 50% of the market population (gray line) and utility varies with the level of e_1. The lighter blue line gives the constant utility price/public service curve for utility when $p_1 = 1.09$ and $q_1 = 1.4$, $p_1\left(U'\right)$. As is evident from the figure, changes in p_1 with changes in e_1 are of a greater magnitude along the constant utility price lines, $p_1\left(U^0\right)$ and $p_1\left(U'\right)$ than when the two districts are the same size and utility is not constant, $p_1(ES)$. As well, when the two districts are of equal size, changes in e_1 changes in e_1 also change in p_2 in the opposite direction of the change in p_1 as shown with the line $p_2(ES)$. Consider the increase in e_1 from 1 to 1.4. In Banzhaf's terms, the observed change in price, the total effect, TE, is found along the line $p_1\left(ES\right)$, the direct effect, DE, is the change in p_1 along the line $p_1\left(U'\right)$ and the distance between the two lines at $q_1 = 1.4$ is the indirect effect, IE and, as shown by both Kuminoff and Pope (2014) and Banzhaf (2021), the total effect on prices, TE, is less than the direct effect on price, DE, the change in welfare.[10]

Our second example mirrors the first, but in this case we assume a small share of population for the jurisdiction changing educational quality $\left(\frac{n_1}{n_2} \to 0\right)$ but allow for the taste for educational quality (α) to vary with a constant slope $\left(\frac{\partial \alpha}{\partial n_1} = k\right)$.[11] In Figure 4a we can see that the more elastic the taste for educational quality, the greater the difference with the constant-taste price line (solid). Note that with increases in e_1 above the base of $e_1 = 1$, when not controlling for sorting, the impact of increases in educational quality on *MWTP* are overestimated. In contrast, when education quality is decreased *MWTP* is underestimated. Intuitively, with increases in educational quality, sorting results in the (marginal) resident indifferent between the two districts having a higher taste for educational quality (α) than the marginal resident at $e_1 = 1$ and therefore having a higher *MWTP*. With decreases in educational quality, the marginal resident now has a lower taste for educational quality, so reductions in educational quality result in smaller reductions in *MWTP*. Figure 4b is analogous to Figure 3b, decomposing the change in price into the direct, taste-constant effect (DE), the total effect (TE), and the indirect effect (IE) for a change in e_1

[10]As utility is quasi-linear in this example, there are no income effects for housing or public service demand. This being the case, the lines $p_1\left(U^0\right)$ and $p_1\left(U'\right)$ are parallel making the difference between the prices at $e_1 = 1$ and $e_1 = 1.4$ the same on both lines.

[11]The elasticity ε evaluated at $e_1 = 1$ and $n_1 = 1$.

from 1 to 1.6. The direct effect is for $\alpha = .2$ with constant utility while total effect is the price gradient when the marginal resident has $\alpha = .275$ at $e_1 = 1.6$.

4.4 A Sufficient Statistics Approach to Welfare Estimation

As our discussion of Banzhaf (2021) and the examples above illustrate, the appropriate measure of the welfare effect of a change in school quality is based on differences in property values along an utility-constant hedonic. However, in contrast to most hedonic applications, the change in the amenity that we examine, educational quality, does not arise because of a change in the quality within a given school district (zone) but in changes in the boundaries of school zones.[12] To derive the welfare effects of these boundary changes, we posit a social welfare function that includes both renter and landowner utility. We then show how a change in school boundaries affects social welfare and, in doing so, derive a sufficient statistic. In Section 6.2, we operationalize our sufficient statistic to obtain welfare estimates of the opening of a new school and changing of school boundaries in Fayette County, Kentucky.

We employ the same model as in Section 4.2. Again, the total population and land area of the two districts are fixed with equilibrium again characterized by the equal utility condition, (2), and clearing in the housing market, (3). As housing prices in both districts are simultaneously determined, both are a function of the land and educational quality in both zones, that is, we have $p_i = p_i\left(e_i, e_j, L_i, L_j\right), i, j = 1, 2$. Let social welfare be given by the sum of renter and landlord utility in both school zones,

$$SWF = \int_0^{n_1} \left[y - p_1 + \alpha\left(n\right) g(e_1)\right] \mathrm{d}n + \int_{n_1}^{N} \left[y - p_2 + \alpha\left(n\right) g(e_2)\right] \mathrm{d}n + p_1 H_1(p_1, L_1) + p_2 H_2\left(p_2, L_2\right) \quad (6)$$

where housing supply in district i depends on both the land available for housing and the price of a unit of housing in district.[13] Then, of course, it follows that social welfare depends on the

[12]In fact, the quality of schools is likely to change as a result of changes in the number and characteristics of students as a result of the boundary changes. As we discuss, our welfare estimates implicitly include these quality changes across the school zones.

[13]Formally, we have $H_i(p_i) = h_i(p_i)L_i$ where $h_i(p_i)$ housing per unit of land.

supply of land and educational quality, $SWF(e_1, e_2, L_1, L_2)$. Then, as shown in Appendix Section A, differentiating (6) with respect to L_1 and simplifying gives

$$\frac{\partial SWF}{\partial L_1} = (p_1 - p_2)h(p_1) \tag{7}$$

where $h(p_1)$ is the number of households per unit of land (density). Then integrating over the change in the size of the district 1 gives

$$\Delta SWF = \int_{L_1^o}^{L_1^{'}} (p_1 - p_2)\, dL_1 = (p_1 - p_2)\, \Delta n_1 \tag{8}$$

where L_1^o and $L_1^{'}$ are land in district 1 before and after redistricting.

The interpretations of (8) is quite straightforward and intuitive – it is equal to the product of the number of houses rezoned from district 1 to district 2 and the difference in the prices of houses in the two zones.

Equation (7) is the change in social welfare from a marginal change in land distribution evaluated at a given distribution of housing and educational quality. With discrete changes in the amount of land (housing) in each school zone, this could be the distribution of housing and educational quality either prior to or following the redistricting. Letting the superscript "o" and "$'$" refer values before and after the redistricting, estimation of the welfare effects requires we estimate the difference in prices at same equilibrium, that is, we estimate $p(e_1, U^o) - p(e_2, U^o)$ or $p(e_1, U^{'}) - p(e_2, U^{'})$ and not use estimates of $p(e_1, U^o) - p(e_2, U^{'})$ or $p(e_1, U^{'}) - p(e_2, U^o)$.

5 Data and Empirical Strategy

As explained in Section 4, standard *DID* estimates cannot be used to obtain meaningful measures of welfare and capitalization when *SUVTA* is violated. In this section, we outline the empirical strategy we employ, following Banzhaf (2021), to obtain estimates of capitalization and the welfare effects of school redistricting in Fayette County, Kentucky. We first discuss the data on housing and schools used in this study. Then we discuss a simple two-period *DID* model frequently used in the literature of school boundary changes and housing prices and its limitations in addressing the general equilibrium effects. Next, we address the issues discussed in Banzhaf (2021) and our theoretical model by a semi-parametric *DID* model and compare it with alternative specifications. Last, we also show the hedonic *DID* with continuous school quality measures.

5.1 Data

5.1.1 Housing Data

Our housing sales data are obtained from Fayette County Property Valuation Administrator (PVA) office. They have detailed information about the sale date, sale price, parcel identifier, and structure characteristics such as the number of bathrooms, square footage, and exterior finish for the years between 2010 and 2020. We restrict our sample to arm's length transactions of single-family residential houses.

Table 1 shows the summary statistics for major house attributes. Columns (1) and (2) present the averages for houses in rezoned and nonrezoned areas prior to the announcement of redistricting respectively and column (3) shows the differences. Important for identification, it is clear from this table that the redistricting did not select certain types of houses given that we do not find any statistically significant or economically large differences between the two groups of homes. The only exception is distance to schools where rezoned homes are 1.1 mile farther away from schools compared to homes in nonrezoned areas, which is consistent with the idea that houses that are distant from schools and close to the boundaries have more uncertainty in changing school boundaries (Cheshire and Sheppard, 2004).

5.1.2 Measures of School Performance and Environment

ACT Scores Our data on Fayette County public high schools are from Kentucky Department of Education and National Center for Education Statistics (NCES) Common Core Data (CCD). The school level average ACT scores are accessed from School Report Card Datasets for school years of 2011-2012 through 2018-2019.[14] Since 2008, ACT tests are required state-wide and around 98% of high school students took ACT tests making school-level bias on the type and percentage of students taking the test less of a concern. We use the composite ACT score to measure the performance of high schools.[15] Appendix Figure B4 plots the average ACT composite score for each school by year. We do not see significant changes in scores across the existing five high schools. Paul Dunbar, Henry Clay, and Lafayette have similar test scores, the highest in the district. Tates Creek follows these schools and Bryan Station has the lowest ACT scores. Frederick Douglass only has two data points and performs slightly higher than Bryan Station following its opening.

School Environment In addition to the test score data, we also collect information on school environment. Following Downes and Zabel (2002) among others. We measure the school environment using racial composition and percentage of free and reduced lunch participants. Figures B5 and B6 present selected school characteristics. As can be seen from the two figures, the percentage of white students steadily decreased over time without pronounced changes at the time of redistricting. The percentage of free and reduced lunch students gradually increased across over time and then began to decline in recent years. In our empirical analysis, we include these variables to account for school environment.

5.2 Empirical Strategy

One of our objectives is to compare estimates of the capitalization and welfare estimates due to changes in high school boundaries using standard *DID* model with our more general *DID* model that follows the approach outlined in Banzhaf (2021). We then briefly explain why the capitalization estimates obtained with a standard *DID* do not provide consistent estimates of *MWTP* when *SUTVA* assumptions are violated while our generalized *DID* will.

[14] See https://openhouse.education.ky.gov/Home/SRCData.

[15] There are four subjects including English, Reading, Math, and Science reported in the ACT data set, along with a composite score that is the average of all four sections.

5.2.1 A Standard Difference-in-Differences Model

Consider a simple *DID* model in which one area is rezoned from one high school to another, our treatment, while the high school for another area, our control, is unchanged. We express the model by

$$P_{ijnt} = \mathbf{X}_{it}\beta + \mathbf{Z}_{it}\delta + \sum_{m=1}^{6} Rezoned_m(\eta_m + Post_t\theta_m) + \sum_{j=2}^{5} \phi_j HS_j^O + \zeta_n + \zeta_t + u_{ijnt}, \tag{9}$$

where P_{ijnt} is log sale price of house i in original high school j neighborhood n at time t. The vector $\mathbf{X}_{it}$ is a set of variables controlling for house attributes such as log of square footage, number of bathrooms, number of stories, house age and age square, whether the house is all brick, and whether the house is located in the urban area. Location amenities include distance to parks, distance to urban service boundary, as well as neighborhood demographics such as racial composition and median household income, which are denoted by the vector $\mathbf{Z}_{it}$. HS_j^O denotes a set of original school fixed effects. The terms ζ_n and ζ_t denote location and time fixed effects respectively, accounting for the aggregate shocks and neighborhood heterogeneity and the term u_{ijnt} is the error term.

The subscript m now denotes the school rezoning pairs that is different from single school fixed effect j. $Rezoned_m$ is a set of binary indicators of school rezoning pairs. The term η_m captures the effect of rezoning areas before the approval of redistricting plan and θ_m delivers the *DID* estimate of the average treatment effect for each rezoning pair after approval as compared to nonrezoned areas. One advantage of estimating (9) is that it closely parallels our alternative, the generalized *DID* model. The binary variable $Post_t$ that equals to one if house i sold in time t was after the approval of the redistricting plan and zero if it was sold before. Finally, θ is the parameter that reflects the effect of switching school zones on housing prices.

5.2.2 A Generalized Difference-in-Differences Hedonic Model

A Semi-Parametric *DID* Model As suggested by our discussion of Banzhaf (2021) and our example in Section 4.2, because of potential general equilibrium effects of the redistricting, the returns to housing and locational characteristics may change along with the returns to schooling, that is, the coefficients in our *DID* may not be time-invariant as suggested by (9). To consider this possibility we follow Kuminoff and Pope (2014) and Banzhaf (2021) and estimate two alternative "generalized" *DID* models. First, we consider a *DID* of the form

$$P_{ijnt} = \mathbf{X}_{it}\left(\beta + Post_t\tilde{\beta}\right) + \mathbf{Z}_{it}\left(\delta + Post_t\tilde{\delta}\right) + \sum_{j=2}^{5}\phi_j HS_j^O + \sum_{j=2}^{6}\tilde{\phi}_j HS_j^N + \zeta_n + \zeta_t + u_{ijt}, \quad (10)$$

where $Post_t$ equals one for sales after approval. The variables HS_j^O and HS_j^N refer to the school catchment area for house i before and after the rezoning. Note that while we include the interactions between the coefficients on house and locational characteristics and the timing variable $Post_t$, the coefficients β, δ, $\tilde{\beta}$, and $\tilde{\delta}$'s can also be estimated separately from two cross-sections.[16] The parameter ϕ_j captures the relative difference in house values between the base Bryan Station High School ($j = 1$) and high school j in the pre-redistricting period and $\tilde{\phi}_j$ is the parameter of interest that represents the difference in the post-redistricting period. The terms ϕ and $\tilde{\phi}$ can be interpreted as the fixed effects of schools;[17] we also include neighborhood fixed effects (ζ_n) to account for location heterogeneity and time fixed effects (ζ_t) to absorb common shocks to the housing market.[18]

A Model of Generalized *DID* with Continuous Measures of Quality The advantage of the semi-parametric estimation of school quality in the context of school redistricting is that we are able to identify the bundle of aggregate changes within a school while still incorporating general

[16]We thank Ed Coulson for pointing out this.

[17]Alternatively, we could express the post-reform coefficients on schools as $\phi + \tilde{\phi} \times Post$ in (10) to make it appear more like a standard *DID*:

$$P_{ijnt} = \mathbf{X}_{it}\left(\beta + Post_t\tilde{\beta}\right) + \mathbf{Z}_{it}\left(\delta + Post_t\tilde{\delta}\right) + \sum_{j=2}^{6}\left(\phi_j + Post_t\tilde{\phi}_j\right) HS_j + \zeta_n + \zeta_t + u_{ijt}.$$

[18]Following Banzhaf (2020) and Bishop and Timmins (2018), we can estimate demand curves under the assumptions that the distribution of demand types active in the market does not change over time and use the single-crossing property that households can be ordered by their *MWTP* for the amenity, and the ordering will be the same evaluated at any level of the amenity and under any equilibrium price function.

equilibrium effects happened across school catchment zones. However, it remains a question as to what extent does the change come from different aspects of the school. Test scores are commonly used value-added measures to evaluate changes in school quality. But if redistricting significantly changes student body composition and other school and neighborhood characteristics the valuation of school test scores could be potentially biased.

Then, following numerous studies that have examined the relationship between property values and characteristics of schools we estimate equations of the form

$$P_{ijt} = \mathbf{X}_{it}\left(\beta + Post_t\tilde{\beta}\right) + \mathbf{Z}_{it}\left(\delta + Post_t\tilde{\delta}\right) + \mathbf{S}_{jt}\left(\gamma + Post_t\tilde{\gamma}\right) + \zeta_t + u_{ijt} \tag{11}$$

where $\mathbf{S}_{jt}$ is a vector of school characteristics that includes measures of student performance (composite ACT score, graduation rate), student characteristics (racial composition, percent free or reduced lunch, percent having behavior incidents), and resources (student-teacher ratio).

5.2.3 Bias in *DID*

To better understand the distinctions between the estimation equations, (9) and (10), and what they imply for our estimates of the effects of redistricting on property values on capitalization and welfare, consider a simple example, more consistent with a traditional *DID* framework, in which there is an existing school high school and a new high school opening at time T. For houses that are redistricted into the new schools let R=1. Then the standard *DID* is

$$P_{it} = X_{it}\beta + R_i\left(\eta + \theta \times Post_t\right) + \zeta_t + \mu_{it} \tag{12}$$

where we can think of X_{it} being a single attribute of a house. In this simple framework the analog to (10) is

$$P_{it} = X_{it}\left(\beta + \beta' \times Post_t\right) + R_i\left(\eta + \theta \times Post_t\right) + \zeta_t + \mu_{it} \tag{13}$$

Then the omitted variable in (12) is $X_{it} \times Post_t$. Letting β^*, η^*, and θ^* be the estimates for (12) and letting $X_{it}^T = Post_t \times X_{it}$, given the true specification is given by (13) we have

$$\beta^* = \beta + \beta' \delta_{X_{it},X_{it}^T},\ \eta^* = \eta + \beta' \delta_{R_i,X_{it}^T},\ \text{and } \theta^* = \theta + \beta' \delta_{Post_t \times R_i, X_{it}^T} \tag{14}$$

where $\delta_{X_{it},X_{it}^T} = \frac{Cov(X_{it},\tilde{X}_{it}^T)}{V(\tilde{X}_{it}^T)}$, $\delta_{R_{it},X_{it}^T} = \frac{Cov(R_{it},\tilde{X}_{it}^T)}{V(\tilde{X}_{it}^T)}$, and $\delta_{Post_t \times R_{it},X_{it}^T} = \frac{Cov(Post_t \times R_{it},\tilde{X}_{it}^T)}{V(\tilde{X}_{it}^T)}$ and where $\tilde{X}_{it}^T$ is the residual from a regression of X_{it}^T on R_i. Then as (14) suggests the bias in the estimate of the *DID* term, θ, arises because of a change in the return to (coefficient on) X_{it} following treatment (redistricting) and the covariance of X_{it} and R_i, housing characteristics and treatment or, more to the point, the houses in the area that is treated.

While (12) and (13) are simplified versions of (9) and (10) they indicate how some of the biases in the estimation of (12) may arise – changes in the returns to housing and locational attributes that affect property values in both rezoned areas and those that were not rezoned.

5.3 Identification

Key to identification in *DID* models is the parallel trend assumption, which implies that in the absence of the redistricting the trend of log sale price for rezoned and nonrezoned homes would have behaved similarly. Figure 5 shows that the trend of sale prices for the two groups is parallel before the announcement/approval and starts to diverge after the approval of redistricting. Appendix Figure B7 plots the event-study style test for the pre-trend assumption. In the aggregate level, we do not find evidence on diverging trends between rezoned and non-rezoned areas.

Equally important is the assumption of the exogeneity of school redistricting. As suggestive evidence of exogeneity, we compare neighborhood characteristics on both sides of the new boundaries following rezoning and find that they are not statistically different as seen in Table 2. In each column, we regress housing prices, percent of white, percent of bachelor degree holders, and median household income separately on a dummy indicating rezoning status. All regressions control for boundary fixed effect, school fixed effect, and year fixed effect. Within a quarter-mile of the new boundaries, homes in rezoned areas are 6.9 percent higher in value compared to those on the opposite side, although the difference is not statistically significant. Moreover, areas that have undergone rezoning display a 4.7 percentage point decrease in white households, an increase of 5.5 percentage points in bachelor's degree holders, and a minimal $74.9 gap in household income. Upon expanding our sample to include more locations farther from the new boundaries, the disparities in sale prices diminish.

We also perform a pairwise comparison for each new school zone boundary with results found in Table 3. In the table, the first school is the high school of attendance following rezoning and the latter is high school prior to rezoning. The coefficients are the differences in housing prices and neighborhood demographics along the boundary. While some of these differences are statistically-significant, with the possible exception of the Tates Creek-Henry Clay boundary, in none of the boundaries is more than a single measure statistically-different.

Finally, while we do not restrict our analysis to rezoning along "straight lines" as in Turner, Haughwout, and van der Klaauw (2014) where land regulations are examined, as can be seen in Appendix Figure B8, in fact, almost all the boundaries between school zones are straight lines along major arteries in Lexington. The exception is, again, the Tates Creek-Henry Clay boundary.

6 Results

Here we report the results of the estimation of our empirical models. First, we present the results of estimating our three alternative empirical specifications, comparing the results from our "standard" models, the pooled standard *DID* (9), with the results of our "generalized" models with time-varying coefficients, our semi-parametric model (school dummies) (10) and our model that includes measures of school quality (11). Next, we follow Banzhaf (2021) to show the welfare effects associated with different methodologies.

6.1 Capitalization with Difference-in-Differences Hedonics

6.1.1 Discrete Measures

Standard *DID* In column (1) of Table 4 we report the results of estimating (9) which pools all school rezoning in a single regression. The coefficient on $Rezoned \times Post$, θ_m, for each school rezoning pair in equation (9) are presented. We report the full results of other coefficients in Appendix Table C2. The estimation of rezoning effects compares homes in the same school zones before redistricting but in different zones following redistricting. In the table, the corresponding rankings of schools based on ACT scores are shown in parentheses. As the estimates indicate, while the direction of capitalization generally aligns with the test score performance of the school, some results display opposite signs to what we would expect, though these are not statistically significant. For instance, moving from Bryan Station to Frederick Douglass, the new and slightly better high school, results in a small decline in house values, while moving from Paul Dunbar, the highest-ranked school, to Lafayette leads to a positive gain in property values. As discussed in Section 5.2, the general equilibrium effects of rezoning may change how the original school is valued over time, and the price gradient of house attributes and other dimensions of schools may also be shifted.

To mitigate these concerns, we conduct several additional analyses in columns (2) through (5), based on the specification of Equation (9). First, we interact the original high school fixed effect with $Post$ to allow for changes in location-specific heterogeneity. Next, we interact all house attributes with $Post$ to account for potential shifts in the price gradients of house characteristics, which may correlate with changes in the hedonic function of school quality. In column (4), we include the specified time-varying effects. Column (5) introduces an interaction between $Post$ and local demographics, such as median household income and the percentage of white households, to account for Tiebout bias in residential sorting following redistricting.

As seen from a comparison of columns (1) and (5), there are a few differences in the results of the two alternative approaches. In the standard *DID* model for rezoning from Bryan Station to Paul Dunbar, there was a 1.5% increase in housing prices, consistent with the ranking in ACT scores of the two schools. However, in the specification in which we account for the time-varying effect, there was a statistically significant increase of 2.8%, almost doubling the effect. In contrast, while there was an insignificant reduction of -0.8% in the values of homes rezoned from Bryan Station to Frederick Douglass in the standard model, the full specification showed an increase of 0.1%.

Generalized *DID* As discussed in Section 5.2, following Banzhaf (2021), one way to account for the general equilibrium effects of rezoning is including dummies for pre- and post-rezoning schools as well as time-varying coefficients on house and neighborhood characteristics. This allows for the value of schools to change following redistricting and capture the potential spillover effects of rezoning on homes that were not redistricted. We aggregate sales in the post-approval and post-opening period into a single treatment period and exclude sales during the post announcement period from the sample.

Table 5 reports the estimated school fixed effects for both pre-rezoning and post-rezoning periods in Panel A, as well as the other coefficients in Panel B, according to our generalized *DID* regression model specified in Equation (10). Column (1) includes only house attributes, while column (2) incorporates tract-level demographic data, such as the percentage of white residents and median household income, to account for neighborhood characteristics. Column (3), our preferred specification, adds a set of interactions between $Post$ and both house and tract attributes. This flexible approach allows for time-varying coefficients on house and location characteristics. We also control for the elementary school effect which accounts for potential interactions between elementary and high school quality.[19] The Bryan Station zone serves as the base group for both pre- and post-redistricting comparisons. Analysis of column (3) reveals, for example, that prior to redistricting, a house in the Henry Clay High School zone is valued 0.9 percent higher than a comparable house in the Bryan Station zone, after adjusting for all observed house and neighborhood characteristics. This disparity widens to 2.4 percent post-redistricting. Similarly, a house in the Paul Dunbar zone is 1.7 percent more valuable than one in Bryan Station before rezoning, with the gap increasing to 2.3 percent afterwards. In contrast, homes reassigned

[19]We are grateful to Sebastien Bradley for highlighting this aspect.

from Bryan Station to the newly established Frederick Douglass school show a marginal 0.4 percent increase in value relative to those remaining in Bryan Station, though this difference is not statistically significant.

To more readily compare the coefficients from estimation of Equation (10) with those from estimating our standard *DID* models, Equation (9), we obtain the school rezoning effect by calculating the difference between the estimated coefficients of two school dummies post rezoning in Table 5 column (3). These results are presented in column (6) of Table 4. Table C2 provides the estimates for all parameters, including the coefficients on interactions with *Post* ($\tilde{\beta}$ and $\tilde{\delta}$), addressing the violations of *SUTVA* and *TCGA*.

Analysis of column (6) reveals that homes rezoned from Bryan Station to Paul Dunbar show a post-redistricting property value appreciation of 2.3 percent. This contrasts with a 1.5 percent increase from the traditional *DID* estimate in column (1) and a 2.8 percent increase in the more refined specification of column (5). Similarly, the generalized school dummies *DID* analysis indicates a 0.8 percent decrease in value for homes rezoned from Bryan Station to Frederick Douglass, as opposed to the 0.1 and 0.3 percent increases reported in columns (5) and (6), respectively.

Further, we calculate the capitalization effects for other school pairs by comparing the post-rezoning school dummies using the delta method, with results shown in the subsequent rows of column (6). For instance, being rezoned from Henry Clay to Tates Creek results in a property value decline of 1.8 percent, and a move from Henry Clay to Frederick Douglass leads to a two percent decrease, figures that are comparable to those in the full specification of column (5). The effects observed for Lafayette and Henry Clay are lower at 2.2 percent in the generalized *DID* model compared to 5.1 percent in the standard *DID* and four percent in the *DID* with time-varying effects. Notably, a rezoning from Paul Dunbar to Lafayette results in a significant two percent depreciation, contrasting with the positive, though not statistically significant, effect observed in the standard approach.

A Comparison of Methods and Estimates In Table 4, we offer a comparative analysis of the estimation results from alternative *DID* models. The first model, which does not incorporate general equilibrium spillovers as per Equation (9) and detailed in column (1), is contrasted with adjustments in columns (2) through (5) and the second model governed by Equation (10) and reported in column (6). After accounting for changes in house attributes and neighborhood characteristics, as well as shifts in the price gradient, we can see the disparity between the two models are smaller. This is evidenced by the inspection of Table C2 where it shows the detailed estimates for our β, δ, $\tilde{\beta}$, and $\tilde{\delta}$'s.

As we can see, there is no significant differences in terms of the β's and δ's, as shown across columns in the upper panel. However, we do find that two coefficients of $\tilde{\beta}$ and $\tilde{\delta}$, the interaction between the $Post$ variable and house size and distance to park are statistically significant, suggesting potential violations of time-constant gradient assumption (Kuminoff and Pope, 2014).

In contrast to the consistent relationship between school ranking (based on mean ACT score) and the direction of housing price changes in the general equilibrium model, homes redistricted from Paul Dunbar to Lafayette and homes redistricted from Bryan Station to Frederick Douglass have different signs across the two models. It is important to highlight that the failure to account for the spillover effects of school redistricting on the original schools not only introduces bias to the estimates but may also lead to changes in the signs of the effects.

6.1.2 Continuous Measures of School Characteristics

The previous results pose a question relevant to any hedonic estimation of school quality and the impact of school boundary changes–"what school characteristics matter?" Our preferred model with school dummies shows the value of the bundle of all attributes attached to a school. Our estimates of school quality from this approach are likely to differ from those estimated using a single measures or set of measures of school quality. To examine the extent of differences between the the two approaches, we estimate a set of hedonic models (Equation (11)) and report the results in Table 6. Columns (1) and (2) estimate two cross-sectional regressions in which we only include school characteristics. This is a more flexible way of estimating a *DID* model because we allow the marginal willingness to pay for each school characteristic to vary over time with the difference between each coefficient represents the change in the marginal willingness to pay for a specific school quality attribute. Essentially we are estimating both pre and post-redistricting hedonic functions separately as shown in Figure B3. In column (3) we pool pre-redistricted and post-redistricted sales and interact all the school characteristics with $Post$ to account for the time-varying preferences for school characteristics – an application of the Banzhaf (2021) approach. In this case, the coefficients for the school characteristics will be similar to the pre-period estimates and the interaction terms represent the *DID* estimates, which would be close to the differences between the first two columns.[20]

The results align with literature findings that the student body and school quality affect school valuations. However, post-redistricting, the influence of student demographics on housing prices becomes less pronounced, while the importance of graduation rates and behavioral incidents significantly increases. Although there is a decrease in the marginal willingness to pay for test scores, this decrease is not statistically significant. We use these estimates as our baseline parameters to calculate the welfare effects of various rezoning pairs.

In column (4), we include only the ACT scores and their interaction with the $Post$ variable, neglecting the evolving preferences for other attributes of houses and schools over time, a point of criticism by Kuminoff and Pope (2014). In this scenario, the coefficient for the ACT score (0.003) is lower than when other school characteristics are controlled for (0.008) before redistricting, and it has a statistically insignificant impact after redistricting.

6.2 Evaluating Welfare Effects Using Alternative Methodologies

Then, as shown by the sufficient statistic derived in Section 4.4, the welfare benefit of redistricting is the differences in housing prices between the treated and comparison, post-treatment.

[20]Though it seems puzzling not to see the *MWTP* for ACT score in the pre-period is significant, it is likely due to the complementarity between elementary and high school quality. Once we drop elementary proficiency measures, the coefficient on ACT is 0.017 and statistically significant.

In our case, we obtain welfare estimates using two approaches: 1) the effect of being rezoned to another school on housing prices based on the estimated coefficients on post-approval school dummies (Table 4); and 2) the effect that a change in mean school ACT through rezoning has on housing prices (Table 6).

To obtain our welfare estimates, we apply our *DID* estimates in the post period to the assessed value of houses in 2013, the year prior to the redistricting. Row A in Table 7 shows the number of houses in each area and row B lists the average assessed value of those homes. Clearly, the Bryan Station and Henry Clay zones were subject to the largest changes as a result of construction of the Frederick Douglass. Row C presents the difference in average ACT score between the school rezoning pair after redistricting.

6.2.1 Discrete Measures

Rows D and F of Table 7 report the corresponding estimates of rezoning from columns (6) and (1) in Table 4 separately. We multiply the number of houses, average assessed value, and the percent change of those homes due to redistricting, to get the welfare measures and report them in rows E and G. 90% confidence intervals are in brackets. Based on column (6) of Table 4, the coefficients from our preferred estimate, being rezoned from Bryan Station (the base school) to Frederick Douglass increases housing prices by 0.4 percent. Then as seen in rows A and B in Table 7, as the average assessed value in 2013 was $164,262 and there are 7,912 houses in the rezoned area this translates to an increase in welfare of $5.20 million. In contrast, the difference in the coefficients on Henry Clay Post and Frederick Douglass Post (-2.03%), with an average assessed value of $248,370 and 2,783 houses rezoned from Henry Clay to Frederick Douglass, this results in a loss of $14.06 million in welfare. In total, the estimated welfare loss from the rezoning and opening of Frederick Douglass was $8.86 million. The estimated construction cost of Frederick Douglass was $82 million (Kennedy, 2017). Inspection of row D for the welfare effects from redistricting of other zones reveals different welfare effects. In column (2) houses redistricted from Bryan Station to Paul Dunbar received the largest return of redistricting, a 2.28% increase in property value, but the associated welfare is around $3.56 million due to smaller number of homes redistricted. Homes redistricted from Henry Clay to Tates Creek had declines in property value by 1.78%, and resulted in a decrease in welfare of $3.44 million. Lafayette to Henry Clay rezoning has gained 2.16% and the total welfare is $9.4 million and statistically significant. However, it is also partially offset by the depreciation for homes rezoned from Paul Dunbar to Lafayette. In total, redistricting was estimated to decreased welfare by $5.25 million meaning that the redistricting unrelated to the opening of Frederick Douglass increased welfare by $8.86 - $5.25 =$3.61 million.

When we compare our welfare results from estimates of our generalized discrete *DID* model (row E) to the standard *DID* or *DID* without time-varying coefficients (row G) we see much different estimates of the welfare effects, consistent with the estimates of capitalization (row F) and in the cases of houses redistricted from Bryan Station to Frederick Douglass and those redistricted from Paul Dunbar to Lafayette different signs on the capitalization and welfare effects. Most pronounced are the differences in the welfare effects of redistricting from Bryan Station to Frederick Douglass ($5.2 million with GE vs. -$0.41 without GE), Henry Clay to Frederick Douglass (-$14.06 vs. -$5.05), and Paul Dunbar to Lafayette ($-5.86 vs. $4.88). One exception to the smaller magnitude of capitalization and welfare effects is for homes redistricted from Lafayette to Henry Clay, which, as discussed in Ding et al. (2024), may reflect an anticipatory effect that may bias the estimate. The welfare change with the standard *DID* has $27.95 million appreciation.

6.2.2 Continuous Measures of School Characteristics

In contrast is the estimated impact on welfare based on mean school ACT scores. Again, following Banzhaf (2021), in Table 6 we report the estimate effect of mean school ACT score in column (4). We estimate that in the post-approval period, the coefficient on ACT score is 0.002, that is, a point increase in the mean school ACT score increases housing prices by 0.2 percent. We then multiply the difference in ACT scores between the schools and, as with the dummy variable approach, calculate the effect for each rezoned area based on the number and average assessed value of houses in each of the rezoned areas with the results reported in row I. In contrast to the results based on our estimation with school dummies, rezoning resulted in an estimated $7.57 million decrease in total welfare and is statistically significant, compared to $5.25 million decrease in the semi-parametric *DID* model.[21]

The most significant differences in welfare changes were found in the areas rezoned from Lafayette to Henry Clay ($9.40 million vs. $0.11 million). In the other direction, the estimated effect of rezoning from Paul Dunbar to Lafayette based on mean ACT score was a loss of $0.89 million versus a loss of $5.86 million using school dummies. Other school pairs also have discrepancies between the two models. One obvious explanation for the differences associated with the rezoning from Bryan Station to Frederick Douglass is the value of attending a new high school independent of the difference in mean ACT score. Of course, this explanation would seem to be inconsistent with the greater estimated loss with the school dummies rather than with mean ACT score for rezoning from Henry Clay to Frederick Douglass. Perhaps it is important to bear in mind that particularly for Frederick Douglass the first school ACT was only available in 2018 and might have carried less weight to potential homeowners in its zone post-opening as a result.

In panels J and K we include all school characteristics and also allow them to vary over time to account for the general equilibrium effect of rezoning. As can be seen in column (1), the estimated welfare effect for Bryan Station to Frederick Douglass is much closer ($5.95) to the one we obtain from the discrete model with GE ($5.20). Similar results are also found in Bryan Station to Paul Dunbar rezoning pair where the two models yield similar aggregate gains in property values for the rezoned area. Other school pairs also see improvements in terms of the point estimates of welfare effects once we account for more school level characteristics. Looking at column (7) and Figure 8, the aggregate welfare associated with the *DID* model accounting for all school characteristics yields a much similar result compared to the other two methods. This set of results shows that using only test scores for school quality could have potential biases, especially when changes in school zones are large and the inclusion of school attributes both before and after redistricting helps reduce the gap between these models.

[21] Figure 7 shows the comparison of estimated welfare effects and their corresponding confidence bands for different models. In Figure B9 we present the welfare estimates for each school pair separately.

7 Conclusion

Utilizing school redistricting reform in Fayette County, Kentucky, we employ a *DID* hedonic model to examine the capitalization effects and welfare changes of school quality. Following Banzhaf (2021), we estimate a discrete, semi-parametric *DID* hedonic model that uses school dummies in both pre- and post-redistricting periods to measure school quality. We include a flexible set of interactions between house attributes and school characteristics and the post-treatment variable to incorporate general equilibrium effects. We also estimate an alternative *DID* model that does not have time-varying coefficients and compare the estimates from this model to estimates using our approach. We find that the estimated capitalization is much larger under our approach. As well, the welfare changes found using the conventional *DID* model differ greatly from those found with our general equilibrium specification. Using the housing stock in 2013 (one year prior to the redistricting announcement) in Fayette County we find that rezoning amounts to approximately $5.25 million loss. The loss from differences in ACT scores is around $7.57 million and the loss from changes in all school and housing time-varying attributes is $2.61 million. In stark contrast, the standard *DID* assuming *SUTVA* and *TCGA* reveals a $27.95 million gain in welfare.

In addition to the *DID* models that use discrete, semi-parametric measures of school quality, we also follow the literature that uses test scores and other dimensions of school characteristics such as demographics, graduation rates, and behavior events to measure school quality (Downes and Zabel, 2002; Clapp, Nanda, and Ross, 2008; Ries and Somerville, 2010). In the case of redistricting in Fayette County, we find large discrepancies in the estimates of welfare changes from redistricting based on changes in mean ACT score and those obtained using our semi-parametric approach. However, the inclusion of a more comprehensive set of school characteristics and their time-varying effects to the model with ACT scores leads to a closer estimate to the welfare effects found using our semi-parametric approach.

Our research contributes to several strands of literature. First, we address concerns related to *DID* models in hedonic estimation by demonstrating that neglecting to factor in the general equilibrium effects of major policy changes can lead to imprecise estimates of capitalization. Second, we present an example that illustrates the inaccurate nature of the welfare assessments associated with the standard *DID* methodology when general equilibrium effects are present. Our study is particularly pertinent to local policy of school redistricting and the establishment of new schools, which has attracted considerable attention in the literature owing to its substantial expenditure. In particular, with respect to the literature utilizing hedonic estimation for evaluating school quality, our semi-parametric approach to assessing school quality and measuring it through test scores reveals substantial variations in the welfare evaluations.

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8 Figures

Figure 1: Changes in High School Catchment Area Boundaries

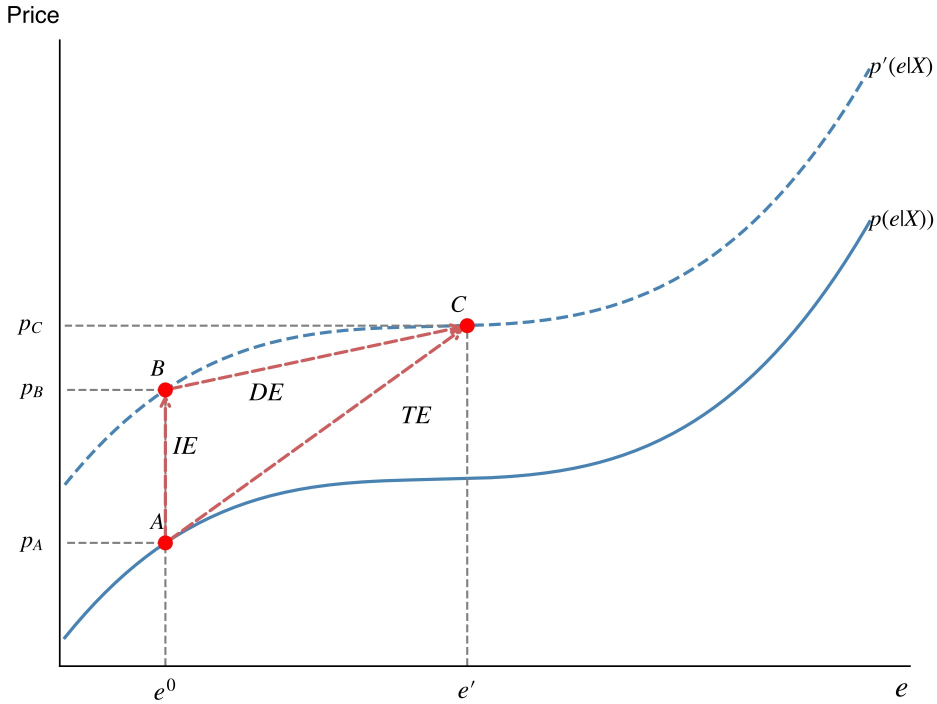
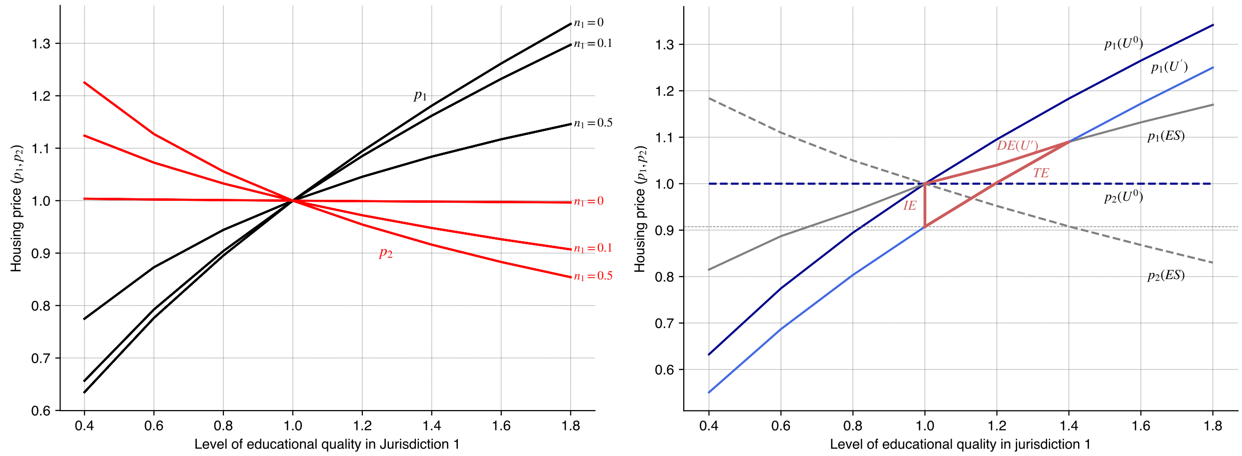


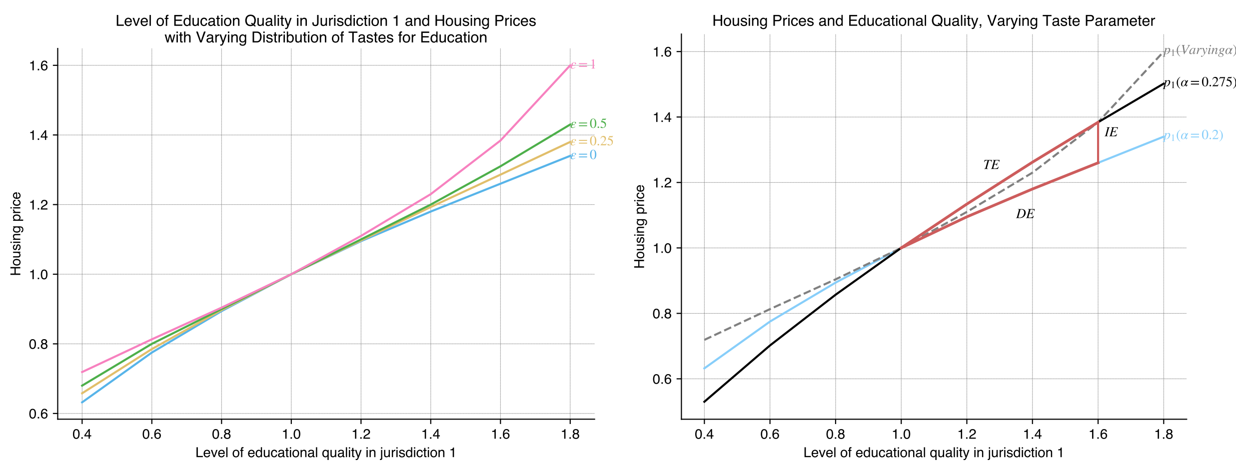
Figure 2: Replication of Figure 1 in Banzhaf (2021)



(a) Educational Quality in 1 and Prices in 1 & 2

(b) Constant Utility and Equal Share Districts

Figure 3: Capitalization with Alternative Shares of Population



(a) Varying Distribution of Tastes for Education

(b) 2 Alternative Taste Parameters

Figure 4: Capitalization with Alternative Taste Distributions

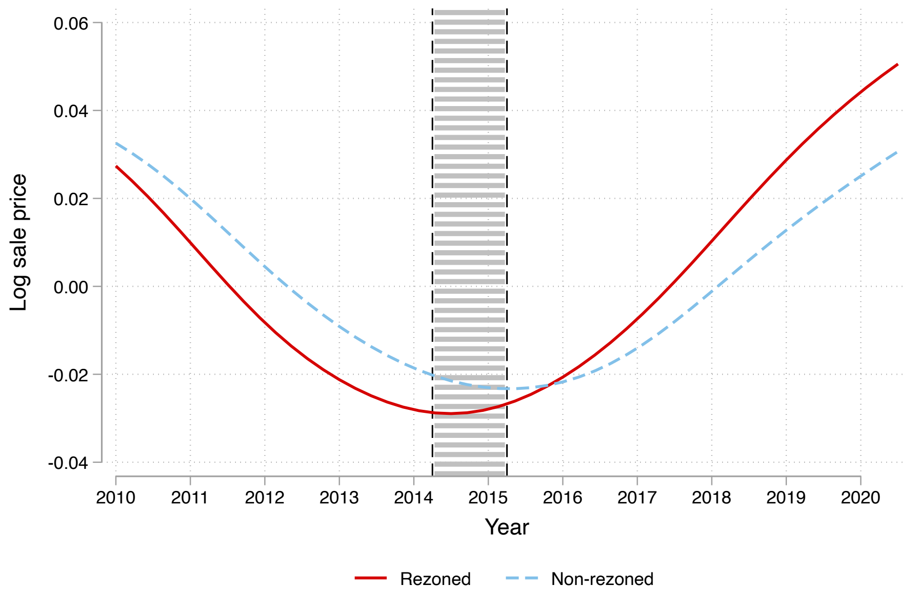


Figure 5: Sales Price Trends for Rezoned and Nonrezoned Homes

Notes: This figure compares the trend of log sales prices in rezoned area and non-rezoned area. Houses sold in areas that are subject to redistricting are in rezoned group and houses that are not subject to redistricting in included in non-rezoned group. We first regress log sale price on house attributes and obtain the residuals. Then we use local polynomial regressions to quarterly smooth the residuals. Shaded area refers to the period after announcement and before approval. We drop sales in this period in our empirical analysis for the ease of interpretation.

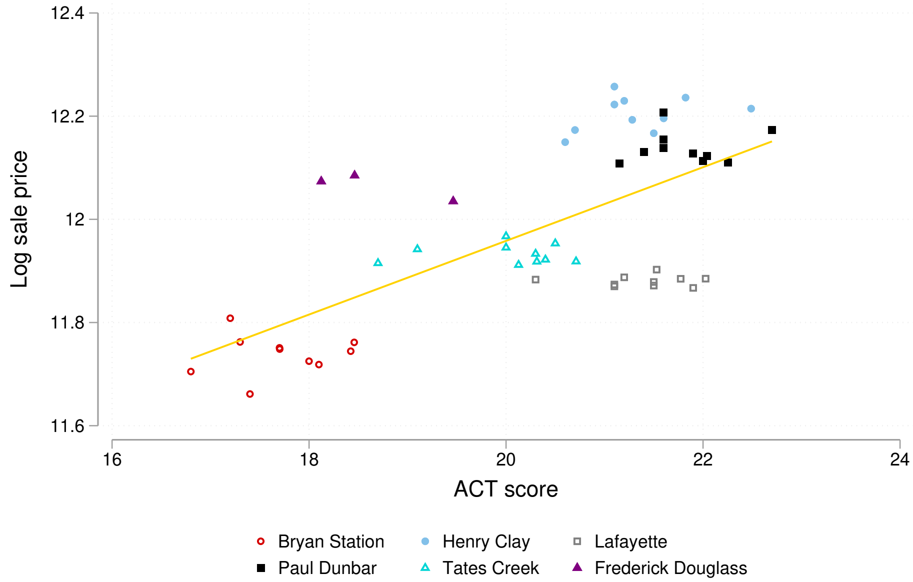


Figure 6: Scatter Plot of Mean Price and Composite ACT Score by High School and Year

Notes: This figure shows the scatter plot of average log sale price and high school ACT scores by school zone and year.

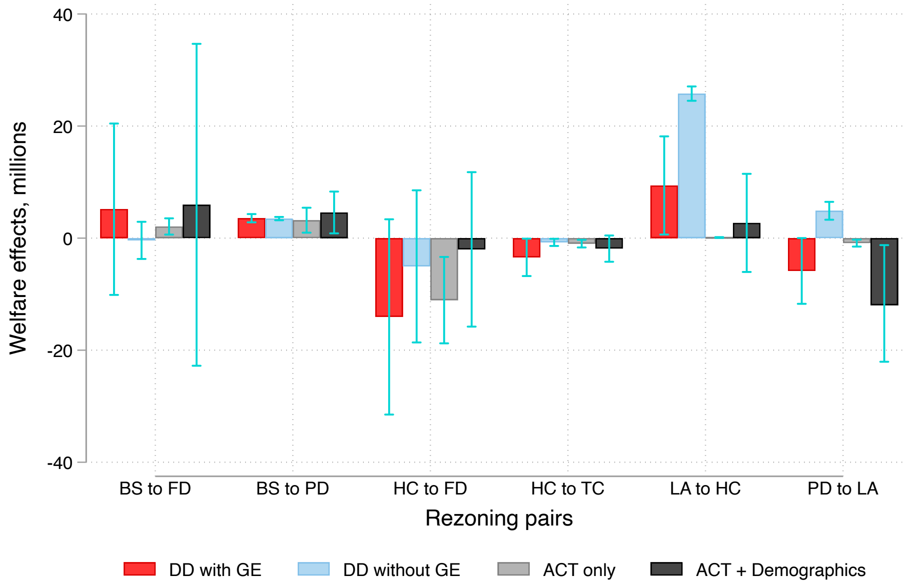


Figure 7: Comparison of Welfare Estimates for School Rezoning Pairs

Notes: This figure shows the estimated welfare effects from four models and their corresponding 90% confidence intervals.

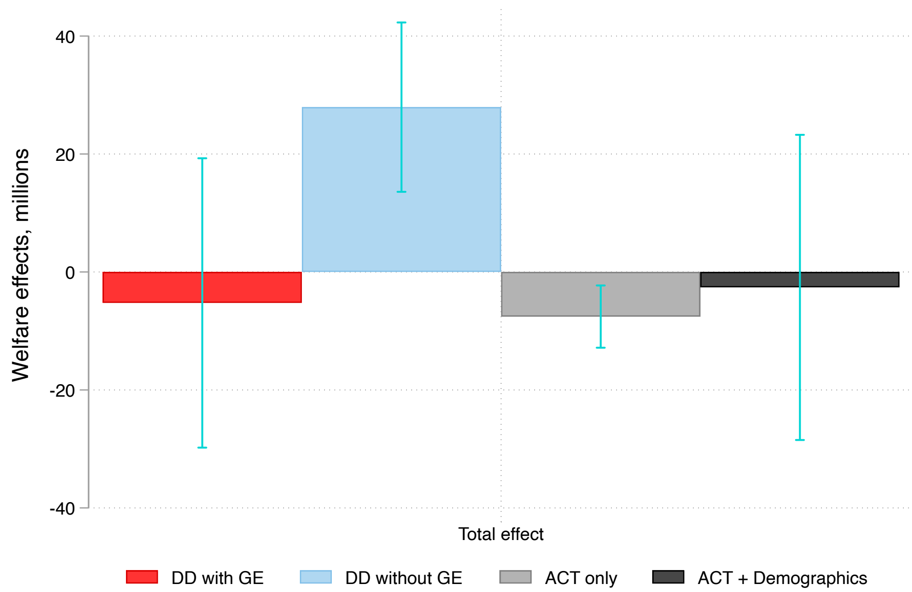


Figure 8: Aggregate Welfare Estimates Combining All School Rezoning Pairs

Notes: This figure shows the aggregate welfare effects from four models and their corresponding 90% confidence intervals.

9 Tables

Table 1: Summary Statistics for Houses before Announcement

	(1) Rezoned	(2) Nonrezoned	(3) Difference
Price	156,511.2	159,853.1	-3,341.924
	(81005.6)	(84573.8)	(16,719.243)
Log Price	11.86	11.87	-0.012
	(0.436)	(0.463)	(0.089)
Square footage	1784.0	1808.6	-24.558
	(623.5)	(667.6)	(129.580)
Log square footage	7.431	7.437	-0.005
	(0.327)	(0.353)	(0.067)
Age	0.243	0.312	-0.068
	(0.209)	(0.244)	(0.078)
Stories	1.400	1.419	-0.019
	(0.451)	(0.454)	(0.071)
No. Fullbath	1.994	1.908	0.087
	(0.640)	(0.660)	(0.157)
All brick	0.343	0.379	-0.035
	(0.475)	(0.485)	(0.106)
Urban	0.992	0.992	-0.000
	(0.0904)	(0.0878)	(0.004)
Distance to school	3.267	2.129	1.138**
	(1.304)	(1.439)	(0.297)
Distance to park	0.360	0.335	0.025
	(0.282)	(0.283)	(0.053)
Distance to urban boundary	1.237	1.163	0.074
	(0.850)	(1.010)	(0.346)
Observations	2,668	7,983	10,651

Notes: This table reports the summary statistics of major house attributes. Columns (1) and (2) report the mean for houses in rezoned and nonrezoned areas respectively. Column (3) reports the estimated difference between the two columns. Standard deviations are in parentheses in the first two columns and robust standard errors are clustered at the old school level in column (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Exogeneity Test: Differences of Sale Price and Demographics along New School Boundaries

	(1) Log price	(2) White	(3) Bachelor	(4) Median income
A. 0.25 mile				
$Rezoned$	0.069	-0.047	0.055	74.921
	(0.104)	(0.024)	(0.043)	(8,504.608)
Observations	1,898	1,898	1,898	1,898
R^2	0.247	0.553	0.529	0.409
B. 0.5 mile				
$Rezoned$	0.056	-0.030	0.066	-3,171.243
	(0.123)	(0.024)	(0.046)	(10,272.591)
Observations	4,178	4,178	4,178	4,178
R^2	0.206	0.497	0.474	0.303
C. 0.75 mile				
$Rezoned$	0.005	-0.015	0.060	-3,615.671
	(0.154)	(0.028)	(0.048)	(11,019.697)
Observations	6,094	6,094	6,094	6,094
R^2	0.209	0.463	0.428	0.273

Notes: This table reports the results of our exogeneity test of random boundaries using sales prior to the approval. Each column shows the mean difference for houses in rezoned areas compared to houses stay in the original school zones in terms of sale prices, census tract level percent of white, percent of bachelor's degree holders, and median household income. Sample consists of houses located within 0.25, 0.5, and 0.75 mile from the boundary. Robust standard errors are clustered at old school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Exogeneity Test for School Pairs

(New School − Old School)	(1) Log price	(2) White	(3) Bachelor	(4) Median income
Frederick Douglass-Bryan Station	0.065	-0.068	-0.041	-5,846.707
	(0.038)	(0.040)	(0.149)	(12,193.943)
Observations	642	642	642	642
R^2	0.701	0.198	0.025	0.026
Paul Dunbar-Bryan Station	0.015	-0.127***	0.030	16,531.503
	(0.009)	(0.021)	(0.122)	(11,251.038)
Observations	544	544	544	544
R^2	0.691	0.613	0.012	0.231
Henry Clay-Frederick Douglass	0.060*	-0.027	-0.089	19,009.347*
	(0.034)	(0.044)	(0.071)	(10,857.514)
Observations	1,106	1,106	1,106	1,106
R^2	0.767	0.015	0.061	0.088
Tates Creek-Henry Clay	-0.125**	0.060**	0.229***	-7,497.070
	(0.054)	(0.027)	(0.053)	(11,967.814)
Observations	953	953	953	953
R^2	0.755	0.043	0.192	0.015
Henry Clay-Lafayette	0.142*	-0.035	0.058	-10,815.968
	(0.074)	(0.046)	(0.128)	(10,859.380)
Observations	1,030	1,030	1,030	1,030
R^2	0.700	0.025	0.018	0.053
Lafayette-Paul Dunbar	0.067	-0.030	-0.015	-35,859.870***
	(0.051)	(0.051)	(0.063)	(11,070.042)
Observations	794	794	794	794
R^2	0.831	0.014	0.004	0.305

Notes: This table reports the results of our exogeneity test of random boundaries using sales prior to the approval within each school rezoning pair. The coefficient reports the mean difference between rezoned and nonrezoned homes within 0.5 miles from the redistricting boundaries. We control for sale year fixed effect. Robust standard errors are clustered at census tract level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Estimated Results for Standard *DID* and Generalized *DID*

	(1) Standard *DID*	(2) Extensisons of Standard *DID*	(3)	(4)	(5)	(6) Generalized *DID*
Bryan Station(6)-Paul Dunbar(1)	0.015*	0.022***	0.019**	0.024**	0.028***	0.023***
	(0.006)	(0.001)	(0.007)	(0.007)	(0.007)	(0.011)
Bryan Station(6)-Frederick Douglass(5)	-0.008	-0.000	-0.005	0.005	0.001	0.004
	(0.006)	(0.001)	(0.006)	(0.005)	(0.007)	(0.006)
Henry Clay(2)-Tates Creek(4)	-0.011*	-0.020***	-0.016**	-0.033*	-0.029	-0.018*
	(0.005)	(0.003)	(0.005)	(0.016)	(0.017)	(0.009)
Henry Clay(2)-Frederick Douglass(5)	-0.015	-0.024**	-0.005	-0.018	-0.016	-0.020
	(0.012)	(0.009)	(0.013)	(0.011)	(0.011)	(0.013)
Lafayette(3)-Henry Clay(2)	0.051***	0.035***	0.046**	0.040**	0.040**	0.022*
	(0.006)	(0.001)	(0.014)	(0.012)	(0.011)	(0.010)
Paul Dunbar(1)-Lafayette(3)	0.009	0.018***	-0.001	0.004	0.011	-0.020*
	(0.008)	(0.002)	(0.011)	(0.009)	(0.011)	(0.010)
$Post \times$New High Schools						✓
$Post \times$Rezoned Pairs		✓		✓	✓	✓
$Post \times$House Attributes			✓	✓	✓	✓
$Post \times$Local Demographics					✓	✓
Observations	22,288	22,288	22,288	22,288	22,288	22,288
R^2	0.906	0.906	0.906	0.906	0.906	0.906

Notes: This table shows the estimated results for our *DID* equations. Column (1) shows the estimated *DID* effects for rezoning pairs in Equation (9). Columns (2) through (5) show the estimated *DID* effects when we control time-varying school fixed effect, house attributes, and neighborhood demographics such as percentage of white and median household income at the census tract level. Column (6) is the differences between estimated coefficients of corresponding schools in which robust standard errors are estimated through delta method. All regressions control for neighborhood fixed effect, elementary school fixed effect, year, and season fixed effect. Robust standard errors are clustered at the school zone level.

Table 5: Generalized *DID* with School Dummies Results

	(1)	(2)	(3)
Panel A.			
Henry Clay Pre	0.014	0.015	0.009
	(0.009)	(0.010)	(0.012)
Lafayette Pre	-0.017*	-0.015	-0.019
	(0.008)	(0.011)	(0.011)
Paul Dunbar Pre	0.014**	0.016**	0.017
	(0.004)	(0.006)	(0.009)
Tates Creek Pre	0.020*	0.020	0.012
	(0.009)	(0.010)	(0.011)
Henry Clay Post	0.030**	0.029**	0.024*
	(0.008)	(0.009)	(0.011)
Lafayette Post	0.008	0.009	0.003
	(0.008)	(0.010)	(0.009)
Paul Dunbar Post	0.018***	0.020**	0.023***
	(0.004)	(0.006)	(0.002)
Tates Creek Post	0.018*	0.016	0.007
	(0.008)	(0.010)	(0.008)
Frederick Douglass Post	0.002	0.001	0.004
	(0.002)	(0.002)	(0.006)
Panel B.			
Log square footage	0.577***	0.576***	0.600***
	(0.024)	(0.024)	(0.026)
Age	-0.328***	-0.328***	-0.387**
	(0.072)	(0.072)	(0.103)
Age square	0.174***	0.174***	0.227**
	(0.037)	(0.037)	(0.067)
Stories	-0.032***	-0.032***	-0.038***
	(0.005)	(0.005)	(0.009)
No. fullbath	0.091***	0.091***	0.090***
	(0.005)	(0.005)	(0.009)
All brick	0.021*	0.021*	0.022
	(0.010)	(0.010)	(0.012)
Urban	-0.127*	-0.123*	-0.133**
	(0.057)	(0.056)	(0.051)
Distance to park	0.013	0.012	0.025**
	(0.010)	(0.010)	(0.008)
Distance to urban boundary	-0.003	-0.001	-0.001
	(0.008)	(0.009)	(0.011)
Median income		0.004*	0.002
		(0.002)	(0.002)
% White		-0.026	-0.021
		(0.015)	(0.013)
Log square footage $\times Post$			-0.037**
			(0.011)
Age $\times Post$			0.078
			(0.097)
Age square $\times Post$			-0.068
			(0.065)
Stories $\times Post$			0.010
			(0.010)
No. fullbath $\times Post$			0.002
			(0.009)
All brick $\times Post$			-0.001
			(0.007)
Urban $\times Post$			0.015
			(0.042)
Distance to park $\times Post$			-0.018*
			(0.009)
Distance to urban boundary $\times Post$			0.000
			(0.003)
Median income $\times Post$			0.002
			(0.002)
% White $\times Post$			-0.006
			(0.018)
Observations	22,288	22,288	22,288
R^2	0.906	0.906	0.906

Notes: This table reports estimates of redistricting effects based on the generalized *DID* model in Equation (10). Each column is a separate regression. Neighborhood fixed effect, elementary school, year, and seasonal fixed effects are also included. Robust standard errors are clustered at school zone level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: *DID* Hedonics with Continuous Measures of School Quality

	(1)	(2)	(3)	(4)
	All School Attributes			Only ACT
	Pre	Post	All Time-Varying	
% White student	-0.049	-0.280	-0.287**	
	(0.249)	(0.222)	(0.098)	
% Hispanic student	0.143	0.313	0.022	
	(0.355)	(0.240)	(0.169)	
% Lunch program	-0.138	-0.071	-0.084	
	(0.193)	(0.102)	(0.173)	
% Behavior incident	-0.025**	-0.266***	-0.019	
	(0.008)	(0.044)	(0.010)	
Distance to school	-0.009	-0.015	-0.017	
	(0.018)	(0.010)	(0.010)	
Graduation rate	-0.056	0.396	-0.109	
	(0.190)	(0.211)	(0.146)	
ACT	0.010	0.003	0.008	0.003
	(0.007)	(0.004)	(0.007)	(0.002)
$Post\times$% White student			-0.003	
			(0.076)	
$Post\times$% Hispanic student			0.033	
			(0.084)	
$Post\times$% Lunch program			0.057	
			(0.152)	
$Post\times$% Behavior incident			-0.165***	
			(0.022)	
$Post\times$Distance to school			0.009**	
			(0.002)	
$Post\times$Graduation rate			0.541**	
			(0.184)	
$Post\times$ACT			-0.005	0.002
			(0.007)	(0.002)
Observations	8,423	13,861	22,288	22,288
R^2	0.910	0.908	0.906	0.906

Notes: This table shows hedonic estimation of school attributes including ACT scores and their impact on housing prices. Columns (1) and (2) are two cross-sectional regressions using sales from pre and post periods separately. Column (3) combines the first two columns in one regression where we allow all attributes to change over time by interacting them with the $Post$ dummy. Column (4) excludes all school characteristics and neighborhood demographics except the ACT score. $Post = 1$ if houses were sold after the approval date. Sales between announcement date and approval data are dropped. House attributes are omitted in the reported table for space saving purpose. Neighborhood fixed effect, elementary school fixed effect, year, and seasonal fixed effects are included. Robust standard errors are clustered at census tract level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Capitalization of Rezoning Using Post-Approval School Dummies and School Characteristics

	(1) Bryan Station to Frederick Douglass (6)→ (5)	(2) Bryan Station to Paul Dunbar (6)→(1)	(3) Henry Clay to Frederick Douglass (2)→(5)	(4) Henry Clay to Tates Creek (2)→(4)	(5) Lafayette to Henry Clay (3)→(2)	(6) Paul Dunbar to Lafayette (1)→(3)	(7) Total
A. No. Houses	7,912	1,291	2,783	1,633	2,066	2,384	18,069
B. Avg Assessed Value in 2013	$164,262	$121,033	$248,370	$118,458	$210,912	$122,690	
C. ACT Difference Post	0.33	4.21	-3.30	-1.05	0.05	-0.63	
D. $\%\Delta P$: Generalized *DID*	0.40%	2.28%	-2.03%	-1.78%	2.16%	-2.00%	
E. Welfare Effect (mil)	$5.20	$3.56	-$14.06	-$3.44	$9.40	-$5.86	-$5.25
	[-$10.14, $20.45]	[$2.82, $4.29]	[-$31.49, $3.36]	[-$6.77, -$0.10]	[$0.64, $18.16]	[-$11.73, $0.01]	[-$29.78, $19.28]
F. $\%\Delta P$: Standard *DID*	-0.03%	2.23%	-0.73%	-0.39%	5.92%	1.67%	
G. Welfare Effect (mil)	-$0.41	$3.49	-$5.05	-$0.76	$25.79	$4.88	$27.95
	[-$3.72, $2.91]	[$3.19, $3.78]	[-$18.62, $8.53]	[-$1.40, -$0.12]	[$24.51, $27.07]	[$3.28, $6.47]	[$13.59, $42.30]
H. $\%\Delta P$: *DID* with ACT Only	0.16%	2.04%	-1.16%	-0.51%	0.02%	-0.31%	
I. Welfare Effect (mil)	$2.08	$3.19	-$11.07	-$0.99	$0.11	-$0.89	-$7.57
	[$0.63, $3.53]	[$0.97, $5.42]	[$-18.78, -$3.36]	[-$1.67, -$0.30]	[$0.03, $0.18]	[-$1.51, -$0.27]	[$-12.84, -$2.29]
J. $\%\Delta P$: *DID* with All School Vars	0.46%	2.93%	-0.29%	-0.97%	0.62%	-4.09%	
K. Welfare Effect (mil)	$5.95	$4.58	-$2.02	-$1.87	$2.72	-$11.97	-$2.61
	[-$22.78, $34.67]	[$0.85, $8.31]	[-$15.80, $11.77]	[-$4.22, $0.47]	[-$6.04, $11.47]	[-$22.70, -$1.25]	[-$28.49, $23.25]

Notes: This table shows the welfare measures of school redistricting. Each column is a school-pair rezoning. Row A shows the number of houses in each rezoned area prior to the rezoning. Row B shows the average assessed value for those homes affected by the rezoning. Row C presents the change in the average ACT score after rezoning. Row D uses coefficients from the rezoning effects in Table 4. Rows E, G, I, and K show the predicted property value changes based on rezoning estimates by multiplying rows A, B, and the corresponding percentage changes. Row H uses coefficients for ACT and $Post\times$ ACT from column (4) Table 6. Row J uses coefficients of ACT and demographics from column (3) Table 6. 90% confidence interval is in bracket.

Appendices

A Derivations for Section 3 of the Sufficient Statistic

Differentiating the social welfare function, (6), with respect to L_1 gives

$$\begin{aligned}\frac{\partial SWF}{\partial L_1} = & \underbrace{\frac{\partial n_1}{\partial L_1}\left[y - p_1 + \alpha\left(n_1\right) g\left(e_1\right)\right] - \frac{\partial n_1}{\partial L_1}\left[y - p_2 + \alpha\left(n_1\right) g\left(e_2\right)\right]}_{(a)} \\ & \underbrace{-\left(n_1 \frac{\partial p_1}{\partial L_1} + n_2 \frac{\partial p_2}{\partial L_1}\right) + \left(H_1 \frac{\partial p_1}{\partial L_1} + H_2 \frac{\partial p_2}{\partial L_1}\right)}_{(b)} + \underbrace{p_1 h_1 + \frac{\partial L_2}{\partial L_1} p_2}_{(c)} h_2\end{aligned} \tag{A1}$$

In (A1) there are three distinct effects on social welfare: a) the change in utility for households moving from zone 1 to zone 2; b) the change in rents paid by residents and received by landlords as a result of change in housing prices; and c) the change in rents received by landlords in the area rezoned from zone 2 to zone 1. As utility for the resident with $\alpha = \alpha\left(n_1\right)$ is the same in both districts by (2), term (a) of (A1) must equal zero. Term (b) also equals zero – the changes in rents to residents is also the change in income to landlords $\left(n_j = H_j\right)$. With $e_1 \neq e_2$, housing prices in the two districts are not equal and therefore term (c) does not equal zero. As $\frac{\partial L_2}{\partial L_1} = -1$ it follows that the marginal change in social welfare simplifies to (7).

B Additional Figures

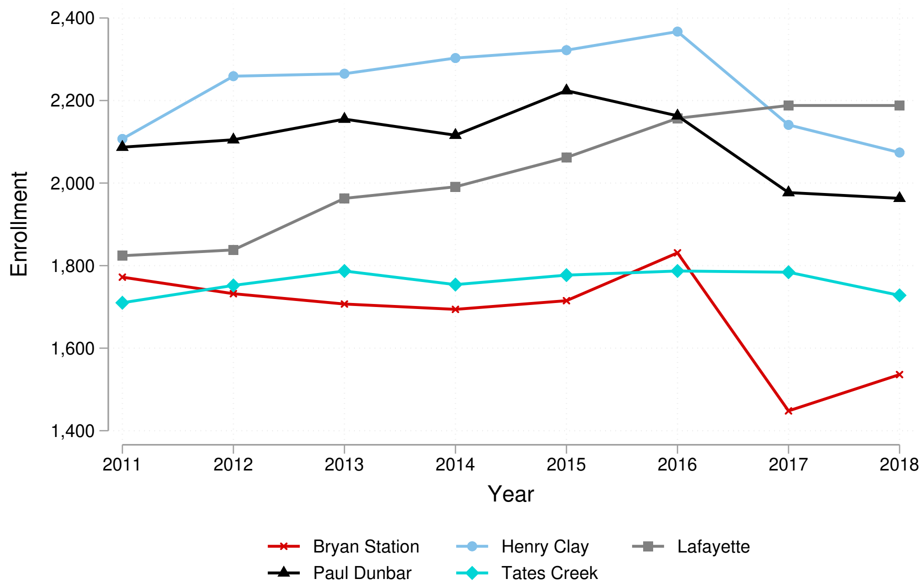


Figure B1: Annual Enrollment in Fayette County High Schools

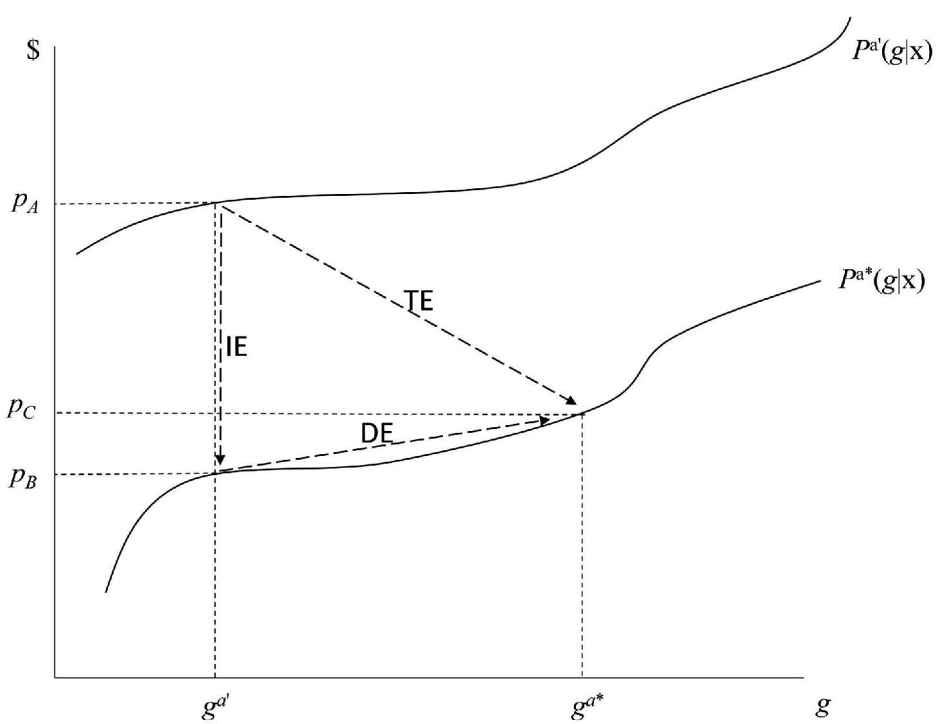


Figure B2: Figure 1 in Banzhaf (2021)

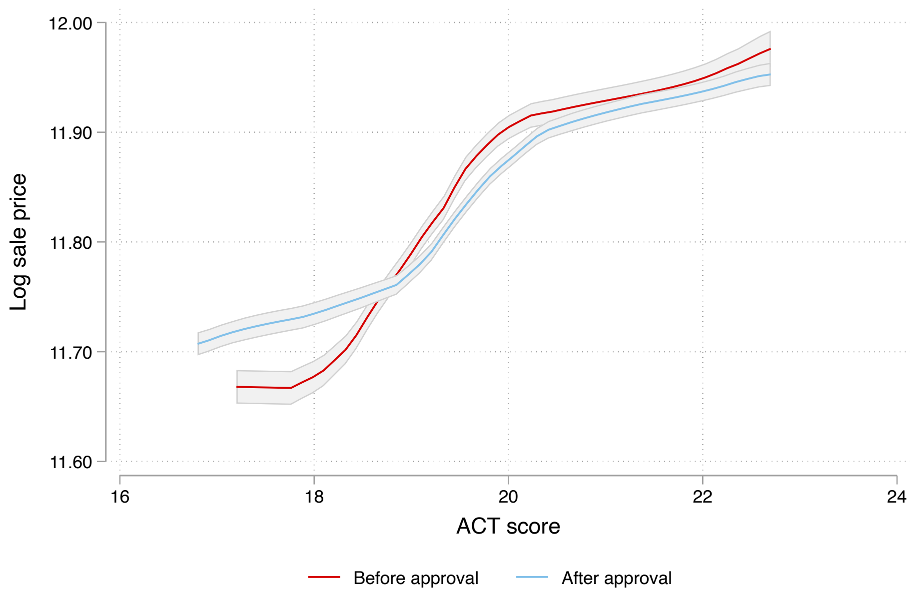


Figure B3: Hedonic Price Functions before and after Approval

Notes: This figure plots the hedonic price functions of school quality for sales before and after approval of the redistricting plan separately using local polynomial regressions. Shaded areas are 95 percent confidence interval bands.

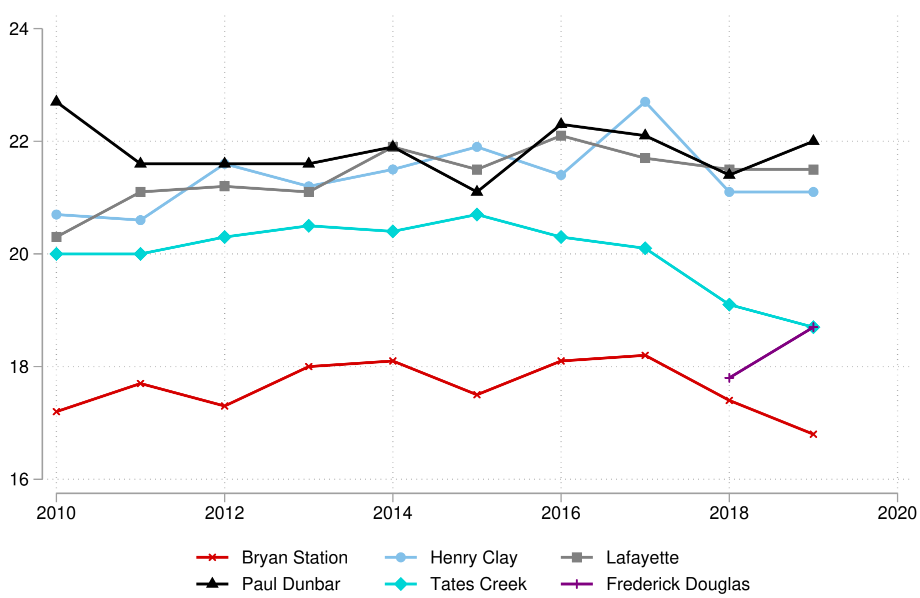


Figure B4: ACT Composite Scores by High School Catchment Area and Year

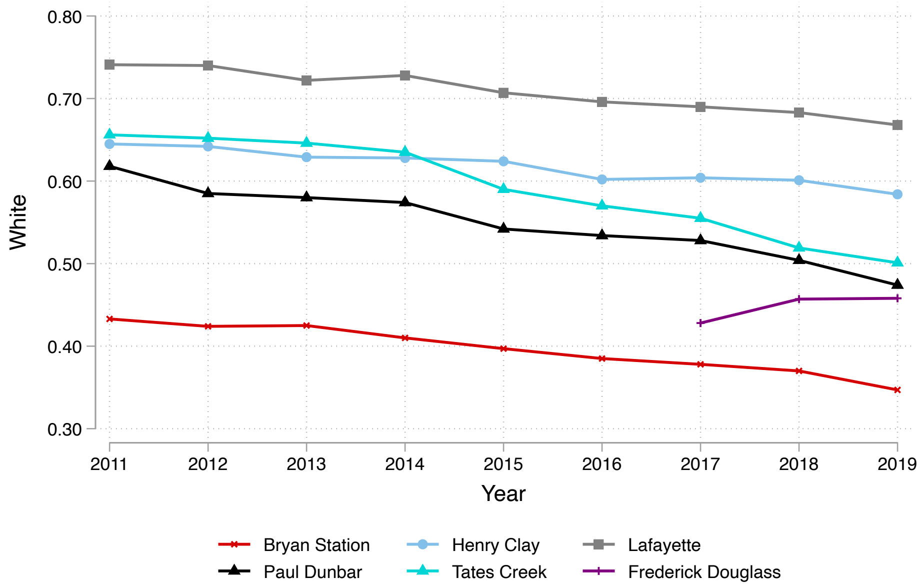


Figure B5: Percent of White Students

Notes: This figure plots the percentage of white students in each high school.

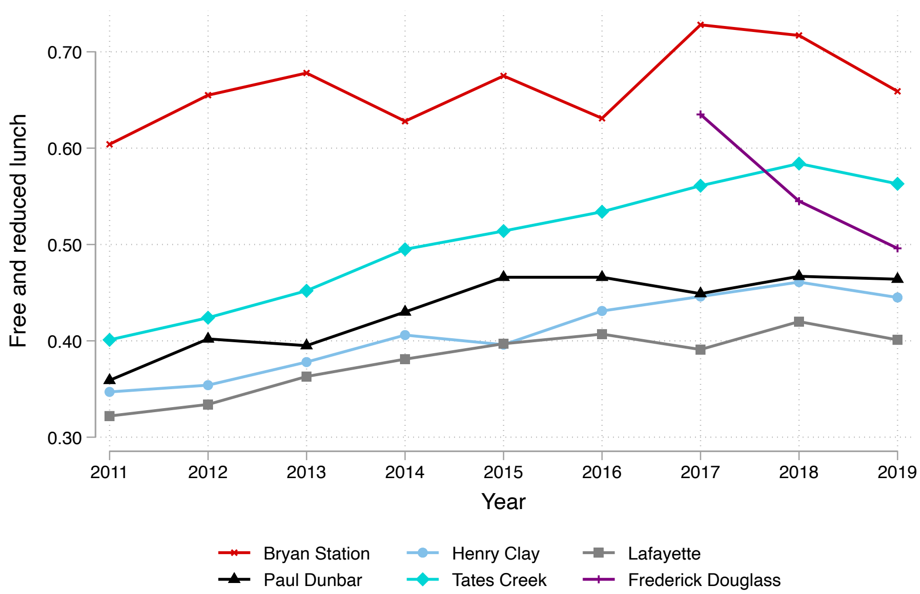


Figure B6: Percent of Free and Reduced Lunch Students

Notes: This figure plots the percentage of students participating in the lunch program.

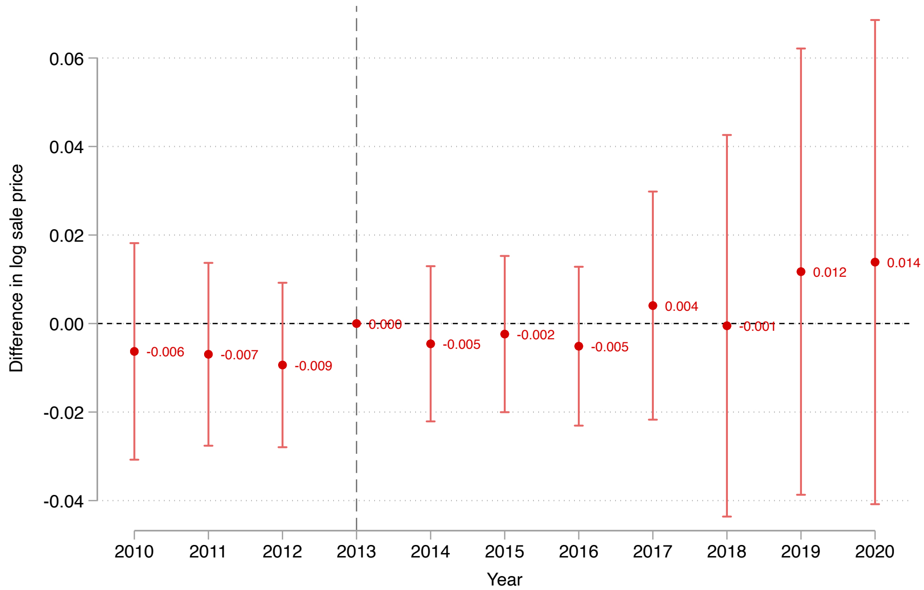


Figure B7: Parallel Trend Test

Notes: This figure plots the event-study style parallel trend test of the difference in log sale price between rezoned and non-rezoned homes relative to their difference in 2013.

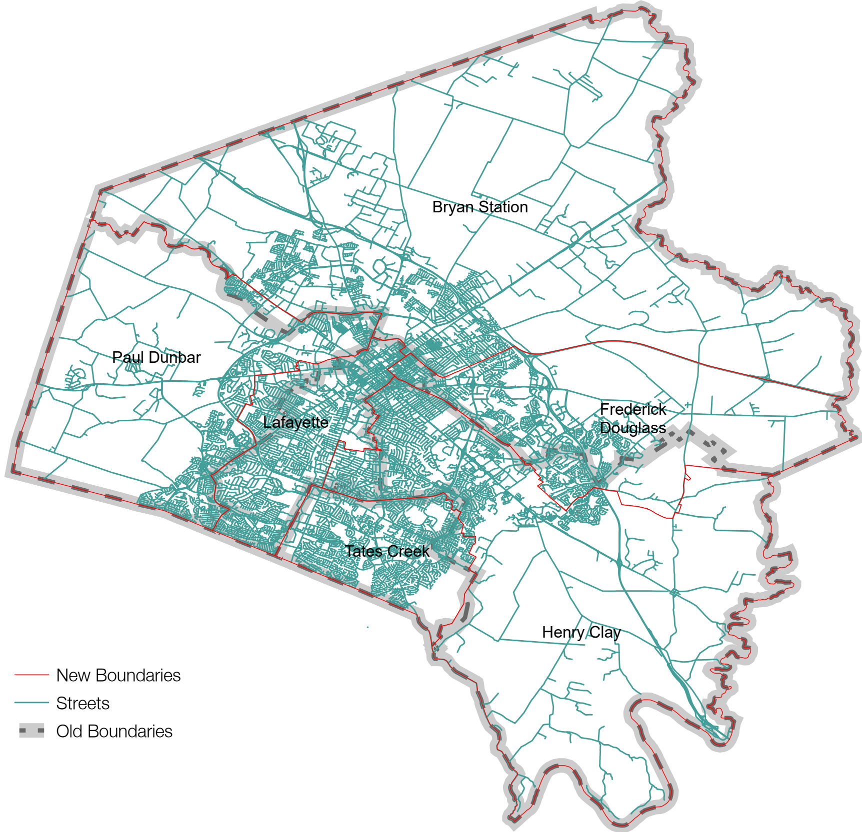
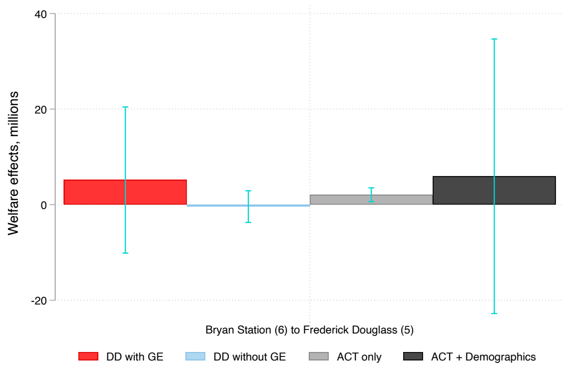
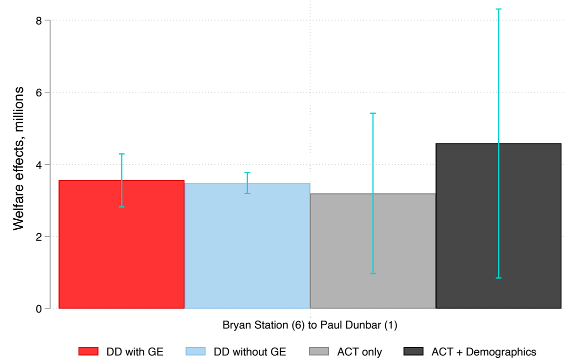


Figure B8: School Boundaries and Streets

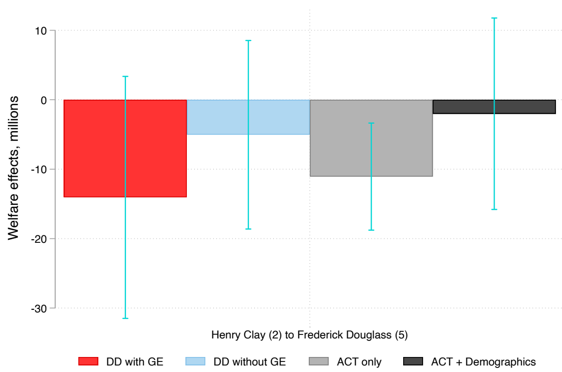
Notes: This figure shows the overlap of old and new school boundaries and main streets in Fayette County, KY.



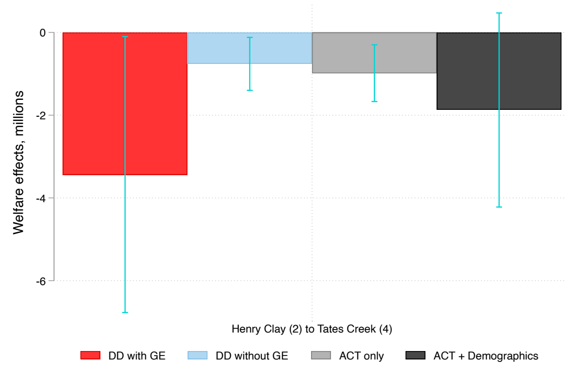
(a) Bryan Station (6) to Frederick Douglass (5)



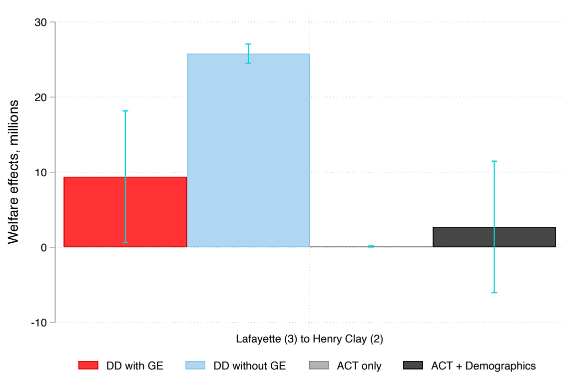
(b) Bryan Station (6) to Paul Dunbar (1)



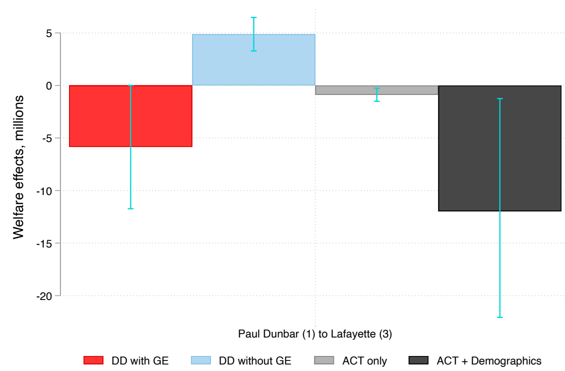
(c) Henry Clay (2) to Frederick Douglass (5)



(d) Henry Clay (2) to Tates Creek (4)



(e) Lafayette (3) to Henry Clay (2)



(f) Paul Dunbar (1) to Lafayette (3)

Figure B9: Welfare Effects for Each School Rezoning Pair

C Additional Tables

Table C1: Percent of Rezoned Homes

	Percent of rezoned homes
Bryan Station	39.87%
Henry Clay	22.77%
Lafayette	18.38%
Paul Dunbar	19.39%
Tates Creek	2.31%

Notes: This table shows percentage of rezoned homes in each original school zone prior to the redistricting.

Table C2: Estimated Coefficients for Additional Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Log square footage	0.577***	0.576***	0.598***	0.599***	0.601***	0.600***
	(0.024)	(0.024)	(0.025)	(0.024)	(0.026)	(0.026)
Age	-0.329***	-0.323***	-0.367**	-0.371**	-0.374**	-0.387**
	(0.073)	(0.074)	(0.103)	(0.104)	(0.104)	(0.103)
Age square	0.178***	0.172***	0.210**	0.216**	0.217**	0.227**
	(0.038)	(0.039)	(0.076)	(0.073)	(0.070)	(0.067)
Stories	-0.032***	-0.032***	-0.039***	-0.039***	-0.039***	-0.038***
	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)	(0.009)
No. fullbaths	0.091***	0.091***	0.090***	0.089***	0.090***	0.090***
	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)	(0.009)
All brick	0.021*	0.021*	0.022	0.022	0.022	0.022
	(0.010)	(0.010)	(0.013)	(0.013)	(0.013)	(0.012)
Urban	-0.143*	-0.143*	-0.166**	-0.152**	-0.145**	-0.133**
	(0.058)	(0.058)	(0.051)	(0.051)	(0.052)	(0.051)
Distance to park	0.014	0.014	0.020*	0.024*	0.026**	0.025**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)
Distance to urban boundary	-0.004	-0.004	-0.005	-0.002	-0.001	-0.001
	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)
Median income					0.002	0.002
					(0.002)	(0.002)
White					-0.020	-0.021
					(0.018)	(0.013)
$Post\times$Log square footage			-0.033*	-0.036**	-0.040**	-0.037**
			(0.013)	(0.013)	(0.012)	(0.011)
$Post\times$Age			0.059	0.064	0.066	0.078
			(0.099)	(0.102)	(0.102)	(0.097)
$Post\times$Age square			-0.049	-0.057	-0.057	-0.068
			(0.077)	(0.074)	(0.070)	(0.065)
$Post\times$Stories			0.010	0.011	0.011	0.010
			(0.010)	(0.010)	(0.010)	(0.010)
$Post\times$No. fullbath			0.002	0.003	0.002	0.002
			(0.009)	(0.009)	(0.009)	(0.009)
$Post\times$All brick			-0.002	-0.001	-0.001	-0.001
			(0.007)	(0.007)	(0.007)	(0.007)
$Post\times$Urban			0.031	0.012	0.009	0.015
			(0.039)	(0.045)	(0.042)	(0.042)
$Post\times$Distance to park			-0.008	-0.013	-0.017	-0.018*
			(0.008)	(0.010)	(0.009)	(0.009)
$Post\times$Distance to urban boundary			0.002	-0.003	-0.001	0.000
			(0.005)	(0.004)	(0.004)	(0.003)
$Post\times$Median income					0.002	0.002
					(0.002)	(0.002)
$Post\times$White					-0.009	-0.006
Time-Varying School Effect		✓		✓	✓	✓
Time-Varying House Attributes			✓	✓	✓	✓
Time-Varying Demographics					✓	✓
Observations	22,288	22,288	22,288	22,288	22,288	22,288
R^2	0.906	0.906	0.906	0.906	0.906	0.906

Notes: This table provides the full set of estimated coefficients for Table 4. Robust standard errors are clustered at the school zone level. $^{*}\ p<0.10$, $^{**}\ p<0.05$, $^{***}\ p<0.01$