Measuring Preferences for Local Public Goods

David Schönholzer*

December 31, 2021

Abstract

Public goods provided by local governments shape many fundamental aspects of life, such as access to education, safety, and neighborhood quality. But how much households value publicly provided goods compared to neighborhood amenities remains unclear. This paper uses a sample of 1.5 million houses in thousands of neighborhoods that straddle local government boundaries to isolate local government valuation. We find that households value access to specific local governments even when comparing homes on opposite sides of the same street but in different governments, suggesting an important role for excludable local public goods. White and Black households show little differential valuation, while Hispanic and especially Asian households exhibit lower and higher valuation, respectively. Local government valuation is mediated through the quality of schooling, free-riding on high-property tax payers, and the quality of peers with whom public goods are consumed.

Keywords: local governments, public goods, boundary discontinuity, variance components, house prices, race.

*Institute for International Economic Studies, Stockholm University (david.schonholzer@iies.su.se). I thank Steven Billings, David Card, Rebecca Diamond, Fernando Ferreira, Rebecca Fraenkel, Pat Kline, Stephen Redding, Raffaele Saggio, Jesse Shapiro, Danny Yagan, and Calvin Zhang as well as seminar audiences at UEA Europe 2021, SITE Housing and Urban Economics 2021, and UEA NA 2021, for useful comments and discussions. I am very grateful to the Real Estate and Financial Markets Lab (REFM Lab) at UC Berkeley for making data available, and especially Victor Couture, Paulo Issler, Christopher Palmer, Nick Tsivanidis, and Nancy Wallace. This paper significantly expands and revises parts of the first chapter of my dissertation titled "Valuing Local Public Goods Using Municipal Annexations".

1 Introduction

Local governments provide various essential services to households, such as education, public safety, and infrastructure. A rich history of research in economics attempts to estimate the value of these local public goods, going back to studies by Oates (1969), Brueckner (1979) and Epple and Sieg (1999), and for schools by Card and Krueger (1992), Bogart and Cromwell (1997), and Black (1999). How much households value these public goods has important implications for household sorting and the efficiency of public goods provision (Samuelson, 1954; Tiebout, 1956; Bewley, 1981; Hoxby, 1999), but compelling evidence on the valuation of publicly provided goods remains elusive.

One important challenge in valuing publicly provided goods is that it is hard to distinguish between households valuing access to local governments providing high-quality public goods from valuation of desirable neighborhoods. Black (1999) compares house prices on either side of school attendance boundaries and includes boundary fixed effects to control for unobserved neighborhood characteristics. But Bayer et al. (2007) show that household sorting plays an important role even on a relatively small geographic scale, which has put into question whether boundary discontinuities can serve as a way to estimate the valuation of local public goods. The concern is that differences in house prices across boundaries may in part reflect features such as the demographic composition of the immediate neighborhood.

This paper seeks to advance the boundary discontinuity approach by following directly the "idealized" approach suggested by Bayer et al. (2007, page 608): to "compare the prices of houses on opposite sides of a neighborhood street" that serves as boundary between adjacent jurisdictions. In this way, "any discontinuity would be almost completely attributable to differences in valuation" of the public goods provided by the two jurisdictions. As they point out, obtaining samples of house prices large enough to implement this approach is challenging. Due to the emergence of large-scale administrative databases with millions of property assessment records, this approach is now in reach.

The first contribution of this paper is to implement this idealized approach to boundary discontinuities at scale, using millions of property records near thousands of local government boundaries and comparing prices of similar houses on the same street. The second contribution is to demonstrate that unobserved local government and neighborhood valuations are separately identified in clusters of local governments connected by boundaries. Based on this insight, we can estimate the contribution of unobserved local government and neighborhood valuations to house price variation. Finally, the third contribution is to illuminate the role of local government quality, peer characteristics, and free-riding for the valuation of local governments.

We establish several new findings that are at the heart of the literatures on local public goods and school quality valuation. First, households exhibit substantial willingness to pay to access higher-quality, excludable public goods: comparing across thousands of boundaries, house prices jump discontinuously on opposite sides of the same residential street by about \$3,500 relative to an average difference of \$42,200, suggesting about 8% of housing costs are due to access to local governments providing better excludable public goods. Second, in contrast to earlier findings, there is no indication that White or Black home buyers are more likely to sort discontinuously on the high-quality side; however, Asian households exhibit substantially higher willingness to pay for access to high-quality local governments, while Hispanic households have slightly lower willingness to pay than other groups. These results are confirmed using unbiased variance component estimates of house prices for different groups of buyers. Finally, estimated local government valuations are highly correlated with both student achievement of the school district and the crime clearance rate of the city, but only student achievement is robust to the inclusion of peer characteristics. This result suggests that household valuation of governments is explained by school and peer quality for school districts, and primarily by free-riding on high-tax households for cities.

Section 2 lays out the construction of a new dataset of house prices and local government boundaries, combining 25 million administrative single-family residential assessment records with boundaries for more than 30,000 local governments, including all school districts and cities in the United States. Matching boundaries with houses nearby, we arrive at a sample of more than 1.47 million households within 500 meters (0.31 miles), for each of which we know the exact location and its street address. The boundary sample of houses connects more than a thousand school districts and cities to one another in hundreds of clusters.

In Section 3, we then show how boundary discontinuities that compare only properties on the same street identify household valuation of excludable local public goods.[1] Intuitively,

[1]The statement regarding the value of opposite-side-of-street comparisons by Bayer et al. (2007, page 608) is worth quoting in full: "the idealized use of a boundary discontinuity design would compare the prices of houses on opposite sides of a neighborhood street that served as a boundary between school attendance zones. Such a comparison would hold everything about the neighborhood as close to constant as possible, and any discontinuity in house prices would be almost completely attributable to differences in the valuation of the assigned schools. In reality, in order to generate large enough samples, researchers employing a BDD have typically used a sample of houses within a threshold distance of a boundary in the range of 0.15–0.35 mile".

the extent to which house prices jump at the boundary as we move from the lower average-price side to the higher side corresponds to the average value of access to the higher-price side. The assumption is that, comparing observably similar houses on opposite sides of the same street, bought in the same year by buyers of the same race/ethnicity, washes out any other discontinuous changes other than access to excludable public goods. We then show that the spatial network of local governments connected by boundary neighborhoods has the same structure as a sample of firms connected through worker mobility, opening the door to the approach developed by Abowd et al. (1999) in the worker-firm setting. Adapting this approach to the setting of neighborhoods and local governments, we can thus estimate unobserved neighborhood and local government effects as well as variance components of house prices using a two-way fixed effects model.

We implement the boundary discontinuity approach using street fixed effects in Section 4. Unlike commonly used estimators simply comparing means on either side within some distance such as 160 meters (0.1 miles), we are able to use local linear regressions that place no parametric restrictions on the shape of the average price function as it approaches the boundary. We use the bandwidth selection procedure proposed by Calonico et al. (2014) and allow errors to be correlated across all properties in the neighborhood (and across the boundary). We find precise discontinuities in house prices of $3,500 at the boundary, relative to an average difference of $42,200; no discontinuity in the shares of Black and White households; and moderately large discontinuities in Asian and Hispanic households. We get almost identical results when using separate quartic polynomials on both sides of the boundary.

The boundary discontinuity approach relies on the assumption that the high average-price side of the boundary indeed has higher-quality excludable public goods. In Section 5, we instead directly estimate both unobserved local government valuation and neighborhood valuation using the leave-one-out procedure developed by Kline et al. (2020). We do so first for each cluster of connected local governments separately, and then for all clusters simultaneously, normalizing relative to the largest city in the cluster. We find robust patterns of variance components across most of the twenty largest clusters, suggesting that about 12% of house price variation is due to valuation of excludable public goods provided by local governments. Unobserved neighborhood characteristics explain about 13% of house price variation. The covariance between local government effects and neighborhood effects is negative in most cases, suggesting that households trade off better excludable local public goods against better neighborhoods.

Finally, in Section 6, we explore what observable characteristics of cities and school districts are correlated with estimated local government valuation. Since city boundaries and school district boundaries overlap imperfectly in most states, we can compare differences between school districts within the same city, and vice versa compare differences between cities within the same school district. We find that student achievement in school districts is robustly associated with local government valuation, whereas the crime clearance rate as a measure of city quality is only weakly related to valuation. For both cities and school districts, peer quality as measured by average household income is strongly correlated with valuation, suggesting that households both value joint consumption of excludable public goods with richer peers and seek to free-ride on the additional tax revenues generated by richer peers.

Links to existing literature. This paper builds on a long literature studying how local public goods and property taxes capitalize in house prices (Oates, 1969; Brueckner, 1979; Berglas, 1984; Brueckner and Lee, 1989). The contribution to this literature is two-fold: first, due to the large sample of house sales available in this research, it is the first to use the across-the-street comparison to study how differences in public goods and property taxes are capitalized in the housing market, holding other components of neighborhood quality fixed. Second, it estimates valuation separately for thousands of cities and school districts, providing a broader assessment of the value of local governments than other studies. Earlier studies typically value only a single public good such as schooling or the full bundle of public goods without distinguishing between schooling and public goods provided by cities. Moreover, studies using fine-grained micro data are often constrained to a single region, whereas this study estimates public goods valuation across hundreds of metro areas.

Many studies use boundary discontinuities to value local public goods (Black, 1999; Bayer et al., 2007; Boustan, 2013; Gibbons et al., 2013; Turner et al., 2014; Caetano, 2019). These studies typically use bandwidths of 150 to 500 meters (0.1 to 0.3 miles), either simply comparing averages on either side of the boundary or parameterize distance using polynomials. In contrast, we estimate discontinuities non-parametrically using local linear regressions with automatic bandwidth selection. Together with the across-the-street comparison, this estimation strategy minimizes concerns that unobserved neighborhood features may be driving discontinuities and instead zeroing in on the component of valuation due to local governments.

Finally, a number of studies propose methods to capture unobserved neighborhood quality, amenities, and household sorting for valuation of local public goods (Gyourko and Tracy, 1991; Kahn, 1995; Bogart and Cromwell, 1997; Bayer et al., 2007; Albouy et al., 2020; Laliberté, 2021). Bogart and Cromwell (1997) are the first to exploit the fact that city and school district boundaries do not always overlap to estimate school valuation. The approach presented here is most similar to Laliberté (2021), who uses boundary discontinuities in student achievement across school attendance zones in Montreal combined with a student-mover design to separate neighborhood effects from school effects. In contrast, we show that connected sets of boundary discontinuities can be used directly to decompose neighborhood and local government valuation.

Section 7 concludes with a summary of implications of these findings for local public finance more broadly, and specifically considerations of efficiency and policy. We now begin by describing some facts about local governments in the United States that are relevant for our data and design.

2 Setting and Data

2.1 Local Governments in the U.S.

The bulk of local public goods in the U.S. in terms of expenditures are provided by school districts and municipalities (henceforth "cities"). Unlike in most countries in the world, school districts and cities are governed separately in most parts of the United States, with some school districts providing services to multiple cities, such as the Los Angeles Unified district to 26 cities in the region; and sometimes a city is serviced by multiple school districts, such as Phoenix or San Jose, each having multiple elementary school districts offering education services.

The top panel of Figure 1 shows the spatial distribution of all 19,539 cities and 13,193 school districts in the United States. As we can see, the distribution differs markedly by region: In New England, there are often more school districts than cities, while in the South there are relatively more cities and generally a sparser distribution of local governments. Major metropolitan areas are often densely packed with dozens of local governments clustered around a central city.

Local governments provide a wide variety of services (see Figure A.1). As shown in Table 1, both cities and school district vary enormously in size, demographic characteristics, and per capita expenditures. School districts are primarily responsible for K-12 education provision to any household in their jurisdiction. Cities have a broad portfolio of responsibilities, ranging from public safety and emergency services to infrastructure and health. Their largest expenditure item and their most important responsibility is public safety, including police and fire protection. On average, municipalities spend about $300 per capita on police protection and $100 per capita on fire protection. Local services include a number of other items too, such as street maintenance, utilities, parks and recreation, libraries, sewerage maintenance, and solid waste management.

Many of the public goods provided by school districts and cities are excludable to non-residents who do not live in the jurisdiction. While school districts sometimes have programs for non-residents to join (Bergman, 2018), residency in the district is usually required to attend a school in the district (Education Law Center, 2005). Similarly, access to services such as police, fire protection, and emergency medical services are usually tightly linked to residence as well: calls to 9-1-1 are routed to the jurisdiction associated with the address. Zoning, regulation, and other local ordinances are also sharply tied to residence, although they may exert local externalities in the neighborhood (Turner et al., 2014).

If a household highly values any of these excludable services, it is thus usually required to buy a house in the associated jurisdiction. The extent to which excludable public goods are important is then reflected in a sharp difference in capitalization of local public goods at the boundary. In contrast, other local public goods, primarily those provided by cities such as parks or public streets are not excludable. As a result, a household can still enjoy these public goods even when living nearby but outside of the jurisdiction, and they would be capitalized smoothly across local government boundaries.

2.2 Data

To investigate how excludable public goods are capitalized in the housing market, we combine two primary datasets. First, we use fine-grained local government boundaries from the Census TIGER/Line database. Second, we combine these boundaries with a large sample of individual property assessment records from ATTOM. We supplement these data with standard records from the American Community Survey (ACS), from the Stanford Education Data Archive (SEDA), and Uniform Crime Reporting (UCR). We describe each of the primary datasets in turn.

Local government boundaries. Local governance in the U.S. consists of various entities including counties, school districts, incorporated towns and cities, townships, and special districts. We focus on school districts and incorporated towns and cities (henceforth cities) as the two most important layers of local governance as they make up the bulk of local public employment, revenue and expenditures.

To measure the extent of these two jurisdictions, we use shapefiles from the Census's TIGER/Line place boundaries database. These consist of detailed boundaries for each place (i.e. cities and unincorporated Census Designated Places), for a total of more than 19,000 cities. School districts boundaries have been prepared by the NCES's EDGE school district boundaries program, which also draws on the TIGER/Line database. It combines elementary school districts and unified school district boundaries that jointly cover all of the U.S. with more than 13,000 districts.[2]

Local government fragments. We use these boundaries to create unique city-district pairs, which we call local government *fragments* or simply local governments. The territorial extent of fragments is a subset of a city territory and a district territory, and in case city and district boundaries do not coincide, the fragment is a strict subset. This means that neighboring fragments are often in the same city but different school districts, or they are in the same school district but in different cities. This is illustrated in Figure 2: the blue boundary in the Northwest separates the City of Cupertino in the North from the City of Saratoga in the South. The green boundary in the South divides the Cupertino Unified School District (CUSD) in the North from the Saratoga Unified School District (SUSD) in the South. Hence fragment ① consists of the Cupertino city-district pair; fragment ② is made up of the City of Saratoga but the Cupertino school district; and fragment ③ combines the Saratoga city-district pair. Panel A in Table A.1 shows summary statistics for local governments.

ATTOM assessor records. ATTOM offers a comprehensive database of individual property assessment records drawing primarily on county assessor records and covering a total of 118 million properties over 2003-2016, with near-universal coverage of properties for about half of all states. These records have been standardized and cleaned across jurisdictions,

[2]Outside of unified school districts, K-12 schooling is provided by an overlapping combination of elementary and high school districts. By using elementary school districts and unified school districts, we thus have a non-overlapping and geographically complete set of school districts.

allowing us to focus on comparable properties across the country. We restrict our sample to single family residential properties since multi-unit residential properties and commercial properties are harder to compare. For each property, we have information on the sales price at the time of the most recent transaction, the most recent property tax payment, its geocoded location, lot size, indoor square footage, the number of rooms, the number of bathrooms, and the number of stories. We also know the last names of the owner of each property, which allows us to probabilistically infer their race and ethnicity using the Bayesian procedure in Diamond et al. (2019). To this end, we merge property records with Census data assigning probabilities to last names according to race and ethnicity into five categories: White, Black, Asian, Hispanic, and Unknown. The latter category applies when no posterior probability of any race reaches 80%.

These steps result in a dataset of approximately 25 million single family homes across the 30 states shown in the bottom panel of Figure 1. As shown in Table 2, the average house price in 2010 dollars is about $235,000 and has been last sold in 2007. Around two-thirds of buyers have White surnames, 9% have Hispanic surnames, 5% each have Black or Asian surnames, and for about 15% of surnames there is no clear race/ethnicity. Houses have been built on average in 1977, have 2.25 bathrooms and 3.69 rooms in total. Neighborhood characteristics from the Census show slightly higher shares of all racial/ethnic categories other than Asian, in part due to the 15% of unknown buyer race/ethnicity. Average household income is $66,900 in the neighborhood, with around 20% of household heads being college educated.

2.3 Boundary sample

Next, we link these 25 million houses to each of the nearest local government boundary, restricting the matching to properties within 500 meters (0.31 miles) of a boundary. We include only houses that are near boundaries with a sufficient number of houses on either side of the boundary. Specifically, we require at least 100 houses on each side of the boundary, with further restrictions on sufficient density of houses as detailed in the appendix. We place *boundary points* every 500 meters along the boundaries (the white diamonds in Figure 2) and associate each house with the nearest one of these boundary points. Boundary points with no properties in the immediate vicinity (100m), which may be due to a physical barrier running along the boundary, are dropped together with their associated houses. We also drop houses near boundary points that are near a highway or other major roadway, as can be seen in the center of Figure 2.

This results in a boundary sample of 1.47 million properties, which are slightly more expensive and less likely to have been bought by a buyer with a White surname. Since there are many more houses near boundaries in dense urban areas, houses have on average substantially smaller lot sizes and slightly older, although they have a higher average number of rooms. They are in neighborhoods that are less White and substantially more Hispanic and Asian. Neighborhood household income is slightly higher than houses overall at around $71,100.

Clusters of connected local governments. As each local government may border several other local governments, they form metropolitan *clusters* in the form of a spatial network of connected local governments. These clusters arise entirely endogenously based on the availability of ATTOM property records across states as well as sufficient density of houses near boundaries. As shown in the bottom panel of Figure 1, they are similar to standard definitions of metropolitan areas such as Consolidated Metropolitan Statistical Areas (CMSAs) as defined by the Census, but diverge in important ways. First, large CMSAs often have multiple clusters within them that are not connected. For example, there are two large clusters with more than 20 connected local governments each on Long Island. As shown in Panel B of Table A.1, there are 444 clusters with an average number of 4.43 local governments. The largest cluster is Los Angeles with 73 local governments.

As an example of a cluster, see Figure 3, the cluster around San Jose, CA (and the second-largest in the country). We can see that most of Silicon Valley is connected through residential neighborhoods as demonstrated in the Cupertino-Saratoga example in Figure 2, which can be found in the Southwest on the map in Figure 3. The red boundaries show connected LGs that are separated by stacked city-district boundaries, such as the boundary between Palo Alto and Los Altos in the West. Blue boundaries separate cities, with either side of the boundary belonging to the same school district; and green boundaries separate districts, with either side receiving services from the same city.

After connecting local governments through houses near boundaries, there are just over a thousand cities and school districts in the data, as shown in columns 4-6 in Table 1. These cities and school districts are substantially larger than the average city and school district across the country. They are also much more diverse, have higher average household income, higher share of college-educated residents, and higher revenue and expenditures, as one would expect from local governments in high-density urban areas. Comparing proxies of government quality in the sample relative to the population, we can see that crime clearance rates are similarly distributed in sample as in the population, while student achievement is more dispersed in the sample than the population.

3 Empirical Framework

We now discuss how we use this dataset of houses near boundaries to identify and estimate the valuation of non-excludable local public goods provided by cities and school districts. We begin with the simple case of a single boundary; we then describe how to stack those boundaries to estimate a discontinuity in house prices; and finally, we present the two-way fixed effects model that we use to separately estimate neighborhood and local government fixed effects.

3.1 One Boundary

Consider a single neighborhood indexed by the distance $s \in [-0.5, 0.5]$ in kilometers from a boundary located at $s = 0$. The boundary may either coincide with a residential street or adjacent parcels, ensuring there is some continuity in neighborhood character across the boundary. Without loss of generality, we use the index $j = 0$ for the local government on the left of the boundary with $s < 0$, and $j = 1$ for the one on the right. Local governments provide a bundle of public goods, some of which are excludable. We denote by ψ_j the average valuation of excludable public goods provided by local government j and we normalize the valuation of the local government on the left by setting $\psi_0 = 0$. Hence, ψ_1 is the premium the average household is willing to pay to get access to the excludable public goods provided by $j = 1$ rather than $j = 0$.

Individual houses $i = 1, ..., I$ have a distance $S_i \in [-0.5, 0.5]$ to the boundary. We assume that a government has a constant and additively separable effect of ψ_j on house prices in the neighborhood, such that the potential home price $Y_i(j)$ if i were to receive excludable public goods by j is given by

$$Y_i(j) = \alpha + \mu\left(S_i\right) + \psi_j + \xi_i$$

where α is the mean house price value in the neighborhood. $\mu(s)$ captures all amenities other than excludable public goods that affect house prices at s in the same way, such as hilltop views, distance to downtown, or non-excludable public goods such as access to parks. We assume that $\mu(s)$ is continuous in s, capturing that any amenities other than access to the excludable bundle of public goods would be similar on opposite sides of the same residential street or for adjacent parcels. While we impose continuity, $\mu(s)$ does not have to be differentiable, allowing for an abrupt change in the slope of spatial trends of house prices near the boundary.

The term ξ_i captures property-specific characteristics, such as lot size or the number of rooms, some of which may be unobserved. We assume that, conditional on observable characteristics, unobservable house characteristic do not change discontinuously at the boundary.

The assumption that local government valuation is additively separable from other amenities is not innocuous: for example, it excludes the possibility that the valuation of excludable public goods differs on opposite ends of the same school district. This assumption also does not allow match effects between neighborhood quality and government quality. We can test this assumption by comparing the additively separable model to a match model with unrestricted government-neighborhood interactions. The average valuation of being in $j = 1$ as opposed to $j = 0$ at some distance s of the boundary can be expressed as a function of average potential house prices:

$$\psi_1 = \mathbb{E}\left[Y_i(1) - Y_i(0) | S_i = s\right].$$

Under the stated assumptions, this parameter is locally identified at the boundary akin to standard regression discontinuity designs (Lee and Lemieux, 2010). Let $D_i = \mathbf{1}\left[S_i \geq 0\right]$ be a dummy for being in $j = 1$ as opposed to $j = 0$. The observed house price is given by $Y_i = D_i Y_i(1) + (1 - D_i)\, Y_i(0)$. Under the assumption that $\mathbb{E}\left[Y_i(j) | S_i = s\right]$ is continuous around zero we can identify the LATE of being governed by $j = 1$ at the boundary:

$$\psi_1 = \lim_{s \downarrow 0} \mathbb{E}\left[Y_i | S_i = s\right] - \lim_{s \uparrow 0} \mathbb{E}\left[Y_i | S_i = s\right].$$

The key identification assumption is that assignment to local governments is by law discontinuous, while other neighborhood amenities and non-excludable public goods are continuous. One threat to this assumption is that households sort along unobservable characteristics across the boundary, and that households may have different valuations that are correlated with the unobservable characteristics. This threat may be less relevant in our case, for a number of reasons. First, since we are comparing houses on the same street, it is unlikely that households value neighborhoods conditional on which side of the boundary they are on. Second, using surnames of buyers as a way to classify race and ethnicity as proposed by Diamond et al. (2019), we can directly control for one important dimension of heterogeneity. Third, in the work by Bayer et al. (2007), which explicitly accounts for sorting, mean valuation of school quality is not dramatically different than in standard hedonic regressions with a similar set of fixed effects.

3.2 Many Stacked Boundaries

Instead of estimating boundary discontinuities separately for individual pairs of local governments, we begin by stacking all boundaries in our data on top of one another after orienting them such that the side with the lower average price is on the left and the side with the higher average price on the right. Consequently, while house prices increase mechanically as we move from the left to the right, this setup allows us to estimate the extent to which house prices are on average indeed discontinuous at a local government boundary, and how much of the increase is due to continuous change in neighborhood quality. Concretely, we run

$$Y_{in} = \alpha_n + \psi_{\text{high}} D_{in} + f(S_{in}) + X_{in}'\gamma + \epsilon_{in}, \tag{1}$$

where houses $i = 1, ..., I_n$ are nested in $n = 1, ..., N$ neighborhoods. Y_{in} is the house price at the time of last transaction. α_n is a neighborhood fixed effect, for which we use street fixed effects as well as boundary points placed every 500 meters along each boundary. We also include cluster-by-year-of-transaction fixed effects throughout to account for different house price trends across metro areas. ψ_{high} is the size of the average discontinuity in house prices at the boundary going from the lower to the higher average-price side, which is indicated by $D_{in} = \mathbf{1}[S_{in} \geq 0]$. We allow substantial flexibility for the shape of $f(s)$, using both local linear regressions as well as quartic polynomials. X_{in} captures observable characteristics of the house and the buyer, including the house age, lot size, interior square feet, and fixed effects for the number of rooms, the number of bathrooms, and the number of stories. We also include fixed effects for the likely race or ethnicity of the buyer as proxied by surname.

3.3 Spatial Network of Connected Boundaries

Stacking boundaries ordered by average price allows us to detect a discontinuity, provided that the discontinuity is in the direction of the average price change. However, a lot of potentially useful information is lost by organizing the data in this way. Specifically, as discussed in Section 2.3, the sample of neighborhoods form a spatial network of local governments connected by mutual boundaries. It turns out that this data structure can be organized along the same lines as two-way fixed effects models estimating unobserved worker and firm fixed effects in labor economics (Abowd et al., 1999, henceforth AKM), whereby we use neighborhoods "moving" across local governments over space instead of workers moving across firms over time. As we now demonstrate, the conditions for separate identification of unobserved neighborhood and local government effects are identical.

Identification. Consider again many neighborhoods $n = 1, ..., N$, each of which cuts across exactly two of $J+1$ possible local governments. We assume for now that every local government shares a boundary with at least one other one. Under this assumption, the difference between any pair of local governments is identified, including non-adjacent pairs. To see this, consider two neighborhoods $n = 1, 2$ and three local governments, A, B, and C, with the first neighborhood cutting across the A-B boundary and the second across B-C. Note that A and C do not border each other. However, letting property i in neighborhood n be at distance S_{in} from the boundary, we can identify the relative premium of A over C by computing

$$\begin{aligned}\psi_A - \psi_C &= (\psi_A - \psi_B) + (\psi_B - \psi_C)\\ &= \lim_{s\downarrow 0} \mathbb{E}\left[Y_{i1}|S_{i1} = s\right] - \lim_{s\uparrow 0} \mathbb{E}\left[Y_{i1}|S_{i1} = s\right] + \lim_{s\downarrow 0} \mathbb{E}\left[Y_{i2}|S_{i2} = s\right] - \lim_{s\uparrow 0} \mathbb{E}\left[Y_{i2}|S_{i2} = s\right].\end{aligned}$$

This is illustrated in Figure 4. Identification of pairwise differences between $J+1$ local government effects is equivalent to identifying J local government effects after a normalization such as $\psi_0 = 0$, whereby we need to normalize one LG effect per metro cluster. Hence, each ψ_j captures the premium relative to the baseline local government. As will become clear now, the identification argument is the same as in AKM.

Let the mapping $j(\cdot,\cdot) : \{1, ..., \max_n I_n\} \times \{1, ..., N\} \to \{0, ..., J\}$ allocate any house i in neighborhood n to one of $J+1$ local governments. Just like in the single-boundary case, we can then identify the difference between two adjacent local governments at the boundary limit by comparing two houses i and i' in the same neighborhood on opposite sides of a boundary:

$$\psi_{j(i,n)} - \psi_{j(i',n)} = \lim_{s \downarrow 0} \mathbb{E}\left[Y_{in} | S_{in} = s\right] - \lim_{s \uparrow 0} \mathbb{E}\left[Y_{i'n} | S_{i'n} = s\right].$$

We can implement this strategy empirically through the regression

$$Y_{in} = \alpha_n + \psi_{j(i,n)} + f\left(S_{in}\right) + X'_{in}\gamma + \varepsilon_{in} \tag{2}$$

where we have now written local government valuation as fixed effects. For a given set of assignments $D_{in}(j) = \mathbf{1}[j(i,n) = j]$ of properties to local governments, it holds that $\psi_{j(i,n)} = \sum_{j=1}^{J} \psi_j D_{in}(j)$. We gain from this formulation the insight that this is in fact a two-way fixed effects model along the lines of AKM.

The key mean independence assumption to identify α_n and ψ_j is then:

$$\mathbb{E}\left[\varepsilon_{in} | X_{11}, ..., X_{IN}, \alpha_1, ..., \alpha_N, \psi_1, ..., \psi_J, j(1,1), ..., j(I,N)\right] = 0.$$

Assuming that errors are conditionally mean independent rules out that, conditional on house and buyer observables as well as neighborhood and local government fixed effects, house prices gradually increase as opposed to jump as we cross a boundary.[3] The assumption is supported by the evidence that there are indeed such discontinuities as shown in the stacked design in equation (1), as long as there are no unobservables that change discontinuously as we cross the boundary. We allow errors to be correlated within neighborhoods by clustering standard errors by neighborhood.

Variance decomposition. Having shown how to separately identify neighborhood and local government effects, we can then decompose the variance in log house prices into

$$\begin{aligned}\text{Var}\left(Y_{in}\right) = {} & \text{Var}\left(\alpha_n\right) + \text{Var}\left(\psi_{j(i,n)}\right) + \text{Var}\left(X'_{in}\gamma\right) + \text{Var}\left(\varepsilon_{in}\right) \\ & + 2\text{Cov}\left(\alpha_n, \psi_{j(i,n)}\right) + 2\text{Cov}\left(\psi_{j(i,n)}, X'_{in}\gamma\right) + 2\text{Cov}\left(\alpha_n, X'_{in}\gamma\right)\end{aligned} \tag{3}$$

where we are particularly interested in the contributions of the neighborhood component $\text{Var}\left(\alpha_n\right)$ and the local government component $\text{Var}\left(\psi_{j(i,n)}\right)$. These components tell us how much of the variation in house prices is explained through differences in neighborhood

[3] This is equivalent to the assumption that wages jump up when a worker moves to a higher paying firm but changes little in the years before and after the move.

valuation as well as differences in local government valuation. In addition, $\mathrm{Cov}\left(\alpha_n, \psi_{j(i,n)}\right)$ indicates the extent to which home buyers seem to treat neighborhoods and local governments as complements or substitutes, depending on whether it is positive or negative.

One complication specific to our setting is that, unlike in typical administrative worker-firm datasets, there is no single connected set that makes up the bulk of the observations, but rather a large number of fairly small unconnected sets of metro clusters. We proceed in two ways to address this. First, we estimate the model on each metro cluster separately. Second, we residualize house prices with respect to metro clusters and set each of the largest fragments in terms of city population to be the reference local government.[4]

Estimates of variance components in AKM are known to suffer from bias due to a small number of connections of firms by moving workers (Andrews et al., 2008). The same phenomenon is likely to be an issue here, as a small number of neighborhoods may connect each adjacent pair of local governments. We address this issue by using leave-one-out corrected estimates of variance components developed by Kline et al. (2020).

Linear projection of estimated local government effects. In addition to estimating variance components using equation (2), we can also investigate how the estimated local government effects correlate with observed characteristics of corresponding cities and school districts. While these estimates are not causal, they demonstrate what aspects of local governments are valued in equilibrium. We study this by running

$$\widehat{\psi}_j = \phi_{h(j)} + Z_j'\beta + r_j,$$

where Z_j is a vector of observable city and school district characteristics and r_j is an orthogonal error term by construction. These correlations are particularly interesting because we can include city or school district fixed effects $\phi_{h(j)}$, due to the fact that some local government j is in the same city or school district as a neighboring local government, separated only by one type of boundary.

[4]This is akin to using firms in the restaurant industry as the reference category across clusters in typical AKM studies.

4 Discontinuities at Local Government Boundaries

4.1 Price Discontinuities

Table 2 presents a simple comparison of houses on the low and high average-price side of local government boundaries. Houses on the high side sell on average for \$291,500, which is \$42,200 higher than on the low side. Buyers on the high side are slightly more likely to be White or Asian, and slightly less likely to be Black or Hispanic. Houses on the high side are slightly newer and larger. Average neighborhood characteristics are also quite different, with households on the high side more likely to be non-minority, slightly more likely to be college educated, and higher household income. Since it is unclear how much of the simple price difference is due to gradually changing neighborhood characteristics or local government characteristics, we now go beyond this broad comparison within 500 meters (0.31 miles) to zero in on the local discontinuity at the boundary.

Figure 5 shows nonparametric estimates of house price discontinuities according to equation (1). We use 2% sample bins along the distance to the boundary and local linear regressions with a 160m bandwidth (which is the MSE-minimizing bandwidth) and a rectangular kernel. In each of these estimates, we control for cluster-by-year fixed effects, boundary point fixed effects, street fixed effects, race of buyer, and cluster-by-housing characteristics fixed effects, where housing characteristics include age, lot size, interior square feet, and fixed effects for the number of rooms, bathrooms and stories. This means the comparison here is between houses on the same street with buyers of the same race, living in observably similar houses bought in the same year, but in different local governments.

In the panel on the top left, we stack all boundaries, no matter whether they are city-only boundaries with the same district on either side; district-only boundaries with the same city on either side; or joint city-district boundaries. We can see that house prices on the low average-price side are around \$268,000 from 500 meters to the boundary all the way to about 200 meters, from when on they drop to just over \$266,000. Average house prices then jump to just under \$270,000 as we cross to the high average-price side, rising slightly as we move further into the high-average price local government. This figure indicates that there are indeed price discontinuities at boundaries, even "within neighborhood", meaning comparing observably similar properties sold in the same year on opposite sides of the same street. Due to the drop in average prices as we approach the boundary *from either side*, there is little evidence that valuation merely reflects gradually improving neighborhood quality towards the high average-price side.

The pattern of average price movement also suggests that observably similar properties very close to local government boundaries are less desirable than those further away from boundaries. Evidently, boundaries cross through sections of neighborhoods that are on average less sought after than those in the center of jurisdictions. This may be due to the availability of commercial zones (restaurants, shopping) being more concentrated in town centers.

We can also estimate these discontinuities separately for district-only and city-only boundaries (bottom left and top right of Figure 5). They exhibit similar patterns with slightly smaller discontinuities. The slopes of average price changes of district-only are more concave, dropping more rapidly as we approach the boundary from either side, while the average price drops more linearly for city-only boundaries. Apparently, the pattern of decreasing house prices near the boundary is more pronounced near school district boundaries than cities. Finally, the bottom right panel shows overlapping city-district boundaries only, which show a large discontinuity of around $8,000 (from $264,000 to $272,000) with mostly flat average price trends other than a slight drop towards the boundary from the low side.

Table 3 summarizes these price discontinuities for different control sets and specifications across the four different samples of boundaries. Each cell is a separate estimate of equation (1), with rows indicating the set of included controls and columns indicating the sample of boundaries. We use two different regression discontinuity estimators: a nonparametric version using a local linear estimator with optimal MSE-minimizing bandwidth as proposed by Calonico et al. (2014) ("OLL"); and a parametric version with separate quartic polynomials on either side of the 500m distance to the boundary ("Quartic"). Reassuringly, even though these two specifications are quite different, they deliver very similar results.

In the first row, we include boundary-point and cluster-by-year fixed effects. As a result, the average price gap between the low side and high side drops from $42,200 to about half of that ($20,900 to $27,100). Including race/ethnicity of buyer interact with state fixed effects

[5]For unobservable property characteristics to substantially drive down the discontinuity any further, they would have to be largely uncorrelated with observable housing characteristics, which would otherwise absorb some of that variation. For example, if the probability of having a recently renovated kitchen were to jump at the boundary, then the estimated discontinuity would be upward-biased. However, it is likely that recent kitchen renovations would be smoothly correlated with e.g. the number of rooms: households that can afford larger houses are also more likely to be able to afford a kitchen upgrade.

on the second row leaves these estimates largely unchanged. In the third row we include observable property characteristics interacted with state fixed effects instead of race: age, lot size, interior square feet, and fixed effects for the number of rooms, bathrooms, and stories. This captures quite a bit of variation in house prices on either side, cutting down the discontinuity to about $10,300 to $12,300. [5]

In the fourth row, we include street fixed effects interacted with boundary fixed effects instead of race or property controls: comparing only properties on the same street in the same 500-meter section of a boundary (due to the inclusion of boundary points) sold in the same year, the discontinuity reduces to about $4,800. Combining all these control sets into one regression, the discontinuity is about $3,300 to $3,500. All discontinuities are significantly different from zero, with standard errors clustered on the neighborhood level. As shown in Figure 5, the different subsets of boundaries show some deviations from the overall estimate: city-only discontinuities are slightly smaller at around $1,500 to $2,000; and district discontinuities are around $2,700. In contrast, city-district discontinuity estimates are substantially larger, at around $7,700 to $8,100.

Together, these estimates provide strong evidence for households valuing excludable public goods provided by the higher average-price jurisdiction relative to the lower side. The average discontinuity of about $3,500 compared to the initial difference of $42,200 suggests that about 8% of house price differences across neighboring jurisdictions is due to the excludable public goods provided by the two sets of local governments. The fact that joint city-district boundaries show substantially larger jumps than city-only or district-only boundaries – and in fact, the jump is significantly higher than the sum of city-only and district-only jumps – suggests that there is some complementarity in having local governments aligned across excludable public goods. For instance, perhaps the return to higher quality of schooling is higher when public safety provision is better.

4.2 Property Taxes

In addition to different bundles of public goods, local governments also offer different property tax rates. Hence, to account for the difference in housing cost beyond the transaction price on either side of a boundary, we also have to account for differences in annual property tax payments. The full household willingness to pay to live in a local government is then the sales price plus the present-discounted value of property tax payments.

Appendix Figure A.3 and Appendix Table A.2 present discontinuities in property taxes paid in 2016 across the stacked boundaries of each type, controlling for the same set of observables and fixed effects as before (boundary and cluster-by-sales-year fixed effects; state-by-race fixed effects; boundary point and street fixed effects; and property observables). On average, this discontinuity is about $33: slightly higher for district-only boundaries; slightly lower for city-only boundaries, and substantially higher at about $78 for city-district boundaries. Unsurprisingly, the patterns of average property taxes as a function of distance to the boundary are very similar to the patterns of house prices, as we would expect if all houses were assessed at a similar rate. The implied property tax rate would be just under 1%.[6] This is slightly lower than the average rate of around 1.5% (Harris et al., 2013), which suggests that property taxes rates are somewhat lower on the high-price side. Assuming households discount all future property tax payments at a rate of 0.95, the implied willingness to pay to live on the high-price side rises by $660, for a total of about $4,200, or 10% of the difference across the two sides of the boundary.[7]

4.3 Race/Ethnicity of House Buyer

The evidence provided so far suggests there are significant discontinuities in prices across boundaries, comparing houses on the same street bought by a household of the same race in the same year. We now investigate to what extent households of a different race/ethnicity value access to the higher average-price jurisdictions differently. Figure 6 shows the share of home buyers of a certain race/ethnicity near a boundary using the same nonparametric estimation approach as in Figure 5, again including boundary-point, cluster-by-year, and street fixed effects as well as property controls.

Surprisingly, and in contrast to findings in Bayer et al. (2007), there is little evidence of racial sorting across the boundary for White and Black households. The share of buyers with a White surname increases gradually from about 55% to 57% at the boundary, and then continues growing smoothly up to about 58% at 500 meters into the high average-price side. The share of Black households exhibits the exact reverse pattern: it drops smoothly from just under 6% to about 5% near the boundary, from where on it continues to drop smoothly to about 4.5%.[8]

[6]The average discontinuity in taxes is $33 and in prices is $3,500, so the implied average property tax rate is 0.94%.

[7]The present-discounted value of the higher tax payments in years after the purchase of a house on the higher-price (and hence higher-tax) side can be calculated as $\$660 = \$33/(1-\delta)$ for $\delta = 0.95$.

[8]Inspecting the top-right panel of Figure 4 in Bayer et al. (2007) and the corresponding t-statistic in their

In contrast to these smooth changes in race/ethnicity of White and Black buyers near the boundary, the share Hispanic and especially the share Asian households exhibit a substantial discontinuity at the boundary. The share of Asian buyers falls gradually from about 8.5% to 8% moving towards the low side of the boundary. At that point, it jumps discontinuously to more than 9% and continues growing to about 10%. Hispanic buyers exhibit the reverse pattern: Their share starts off at around 15%, gradually dropping to about 14% as we approach the boundary. It then drops and remains constant at around 12.5%.

Table 4 confirms these patterns in the form of point estimates. Columns (3) to (7) show local linear discontinuity estimates with optimal bandwidth of buyer race/ethnicity indicators. White and Black households have precisely estimated near-zero coefficients (-0.001). In contrast, Asian households are 1.3 percentage points more likely to be on the high-price side, which is an 18% increase relative to the low-price side. Hispanic households exhibit a more noisy 1.2 percentage points drop towards the high-price side, a 10% drop.

These patterns suggest that Asian households have a significantly higher willingness to pay for excludable public goods offered by the high-price local government than other race/ethnicity groups, and Hispanic households may have a slightly lower willingness to pay. This is confirmed in columns (1) and (2) of Table 4: the average discontinuity for Asian buyers is about $5,100 higher than for White households (i.e. the reference category). Hispanic buyers exhibit a somewhat noisy $1,100 lower willingness to pay than White households. Black households also have slightly lower willingness to pay than White households, but this result is sensitive to the specification: using a local linear regression instead of the quartic polynomials shows no difference of Black households relative to White ones.

Importantly, discontinuities in Asian and Hispanic households are not a concern for the validity of the price discontinuities shown in Section 4.1. Recall that price discontinuities (and race/ethnicity discontinuities) were estimated using street fixed effects. This includes comparisons of neighbors on opposite sides of the same residential street as well as neighbors on adjacent parcels (when the boundary crosses the street perpendicularly). It is unlikely that a household would value a property differently depending on whether a neighbor is in

Table 1, it is possible that the significant discontinuity estimates in their work is due to these regressions essentially comparing means within 0.1 and 0.3 miles on either side of the boundary respectively – a distance at which we non-parametrically estimate a substantial slope as we approach the boundary, with little discontinuity remaining. Alternatively, it could be that the different result emerges to the difference in the sample (Bay Area versus many urban areas across the U.S.) or the type of boundaries (school attendance boundaries versus district and city boundaries).

the same jurisdiction as the property under consideration – except for the purposes of joint consumption of excludable public goods such as schooling, which is part of what the price discontinuities intend to capture.

5 Decomposing House Price Variation

The stacked boundary discontinuities presented so far provide evidence for the existence and the magnitude of price discontinuities due to excludable public goods and differences between households in willingness to pay for access to these public goods. Building on these results, we now investigate how much of the variation in house prices within and across clusters of local governments is due to valuation of excludable public goods, how much is due to unobserved neighborhood quality, and how much due to the correlation between the two. To this end, we estimate variance components along the lines of equation (3). Since plug-in estimates as in AKM or Card et al. (2013) are known to be biased, we also present estimates using the leave-one-out estimator developed by Kline et al. (2020, henceforth KSS), which estimates bias-corrected variance components with unrestricted heterogeneity.

5.1 Variance Decomposition by Cluster

Table 5 presents variance component estimates for each of the twenty largest clusters. For each cluster, we show the (leave-one-out) number of local governments (J), the number of neighborhoods as defined by boundary points (N), and the number of individual properties (I). We then present estimates of $\text{Var}(\psi_j)$, $\text{Var}(\alpha_n)$, and $\text{Cov}(\psi_j, \alpha_n)$ using both the plug-in AKM estimator as well as the unbiased KSS estimator.

The largest leave-one-out cluster is Los Angeles, with a total of 72 local governments connected by 73,961 houses in 510 neighborhoods. The variance in log price is 0.517. We find that access to local governments explains about 11% of house price variation using the KSS estimate of $\text{Var}(\psi_j)$, which is 0.059. Neighborhoods explain only slightly more of the variance, about 13%, with an estimate of $\text{Var}(\alpha_n)$ of 0.004.

Other clusters exhibit similar patterns of price variance decompositions. The median large cluster (bottom row of Table 5) has a local government variance estimate of 0.036 relative to a log price variance of 0.4, suggesting that 9% of price variation is due to access to excludable local public goods. Neighborhoods explain a comparable share of about 10%. The covariance is small but negative, a result we find in most but not all clusters. We interpret the negative correlation we find in most clusters as evidence for households treating neighborhood quality and access to high-quality local governments as substitutes rather than complements: for instance, they trade off living in a good neighborhood but worse schools against a bad neighborhood with better schools. This can be true even if neighborhood quality and school quality are highly positively correlated.

A few clusters show patterns that diverge from the median. For example, the cluster around Boston, MA shows estimates for the contribution of access to local governments that are substantially larger than the median, making up almost a fifth of the house price variation in the cluster. These large estimates of $\mathrm{Var}(\psi_j)$ are accompanied by large estimates for neighborhood variance $\mathrm{Var}(\alpha_n)$ as well, and a strongly negative covariance between the two. Boston has a central city and school district that is thought of as having relatively low quality, while the connected suburbs have some of the highest-quality schools and city services in the country. This suggests that, conditional on some housing budget, households face a particularly stark tradeoff between neighborhood quality and excludable public goods quality in these clusters. On the other end of the spectrum, Seattle, GA, Denver, CO, and Phoenix, AZ show very little contribution of access to local governments to overall house price variation.

Overall, these variance component estimates confirm the important role of excludable public goods for house prices documented in the prince boundary discontinuities. The median estimate of 9% contribution of access to excludable public goods coincides with the 9% estimate in the boundary discontinuity approach. Unobserved neighborhood characteristics make up a slightly larger portion of house price variation. In contrast to typical worker-firm settings, covariance estimates are negative on average even when bias-corrected with KSS. Given the robustness of this pattern across almost all clusters, it is unlikely to be driven by a bias specific to an individual cluster.

5.2 Connected Cluster Variance Decomposition

Estimating variance components by cluster allows us to document how much access to excludable public goods and neighborhoods matters for individual clusters, but it is difficult to draw inferences about the country as a whole or for specific subsets of the data other than geographic proximity. To create connections between clusters, we first residualize house prices with respect to cluster fixed effects, so that variation is not driven by between-cluster differences in log house prices. We then normalize each cluster with respect to the largest city in the cluster, similar to using the establishments in the restaurant industry as the benchmark against which to compare other firms in a connected set in the worker-firm setting.

We begin by providing evidence for the validity of the additive model connected through the largest city per cluster as normalization by comparing it to a model with unrestricted neighborhood-government interactions in Table A.3. The adjusted R^2 from the additive model after residualizing by cluster fixed effects is 0.6450, while the R^2 from the match model is 0.6593. This is a mere 2.2% improvement, indicating that the match model captures only marginally more variation in log house prices. Appendix Figure A.4 shows mean residuals by cells defined as interactions of estimated neighborhood valuations and estimated local government valuations. Reassuringly, these mean residuals are small relative to the variation in log house prices (with variance 0.37), and there is no obvious pattern of positive or negative mean residuals.

Finally, to diagnose the extent to which the model captures discontinuous changes in house prices near the boundary without spatial pre-trend, in Appendix Figure A.5 we inspect the change in average house prices in four 100-meter distance bins closest to the boundary for transitions from the lowest quartile of estimated local government valuation to each of the four quartiles on the other side of the boundary. There is little indication that house prices evolve differentially in the four quartiles as they approach the boundary; the dip on the lowest-quartile side is small and insignificant. This pattern provides evidence for the conditional mean independence assumption underlying the model.

Table 6 shows results for the entire connected set and then for subsets by period, by boundary type, and by race/ethnicity. The variance of residualized house prices is 0.375, only slightly smaller than the median in the twenty largest clusters shown in Table 5. The KSS estimate of the contribution of access to local governments is slightly larger, at 0.044, explaining about 12% of house price variation. Neighborhoods make up about 19% of the variation. The covariance of the local government component and neighborhood component is small and negative.

Comparing variance components for three sub-periods, 1990-1998, 1999-2007, and 2008-2016, we see a substantial increase in the importance of local governments in house price variation, from 11% in the first period to more than 15% in the last period. Neighborhood variation also contributes more in the most recent sub-period than in the 1990s. The covariance becomes slightly more negative over time but remains small.

Turning to different types of boundaries, local governments are deemed much more important using only the connected set of joint city-district boundaries as opposed to the set connected by city-only or district-only boundaries. This again supports the complementarity of aligned city-district local governments in household valuation. Neighborhoods also explain a larger amount of variation in city-district boundaries compared to the other types of boundaries.

Finally, we can also investigate how variance component estimates differ if we study only the set connected by buyers of a specific race/ethnicity. Even though White and Black buyers connected a vastly different number of local governments – White households connect about eight times as many as Black households – the contribution of both local governments and neighborhoods to house price variation is very similar among the two groups.

In contrast, the set connected only by Asian buyers exhibits a much higher contribution of the role of access to local governments, consistent with patterns documented in the stacked boundary discontinuities. Neighborhoods also explain a lot of variation for Asian households, and the covariance between local governments and neighborhoods is strongly negative, suggesting a very strong tendency to trade off the two types of amenities. The set of local governments and neighborhoods connected by Hispanic buyers exhibits a similar pattern as Black and White buyers, although they value neighborhoods and trade-off neighborhoods and local governments more like Asians.

6 Valuation of Local Government Characteristics

The apparent importance of excludable local public goods in housing choices raises the question what observable characteristics of local governments are correlated with household valuation in equilibrium. Three groups of characteristics are potentially important here: first, the quality of government (output), expenditure and revenue patterns (input), and the quality of peers in joint consumption of excludable local public goods (peers).

We begin with simple correlations of government valuation with government quality and peer characteristics presented in Figure 7. On the top left, we see that estimated government valuation using AKM across the whole sample is highly correlated with standardized test score achievement. The correlation with crime clearance rate as a measure of city quality is weaker but still positive. Government valuation is also highly correlated with peer quality as measured by household income.

Table 7 shows coefficient estimates from regressions of the estimated local government valuations $\widehat{\psi}_j$ on observable characteristics of both the city and the district associated with the local government fragment, controlling for cluster fixed effects throughout.[9] Panel A shows that student achievement (in standard deviations) is strongly and robustly correlated with local government valuation: a one-standard deviation increase in student achievement is associated with a 14% (column 4) higher valuation (about $38,000). This is robust to including household income both of the city and the district. On the other hand, while crime clearance is also strongly correlated with local government valuation within clusters (column 5), this correlation disappears once we account household income, especially of the city (column 7).

Panel B restricts the comparison to districts within the same city (columns 1-4) by adding city fixed effects; and between cities within the same district (columns 5-8) by adding district fixed effects. Student achievement continues to be strongly correlated with local government valuation, although estimates get somewhat noisy when including average household income of the district as a proxy for peer quality. This is not surprising, given that the correlation between student achievement and average household income is very high.

The log of household income of the district itself also continues to be important (although noisily), even when including race/ethnicity demographics of under-represented minorities (Black and Hispanic). These demographics are not strongly correlated with school district valuation. This is not necessarily in contrast to findings in Bayer et al. (2007), as the importance of race there is due to neighborhood sorting, while here it is about demographic characteristics of local governments primarily *away* from the boundary. The log of household income of the city, controlling for district fixed effects (columns 6 and 8), continues to be strongly correlated with local government valuation, again with little apparent importance of average race/ethnicity characteristics of cities (primarily away from the boundary).

Summarizing these findings so far, they suggest that school quality is indeed an important mechanism underlying the valuation of excludable public goods: the correlation is fairly strong, even when comparing districts in the same city, and controlling for differences in peer quality. In contrast, the quality of cities as measured by their police department's crime clearance rate is not a strong predictor of government valuation once controlling for peer quality. However, peer quality as measured by average household income (again, primarily away from the boundary) is a very robust predictor of city valuation, even when controlling

[9] Recall that some fragments are in the same district or city as an adjacent fragment because boundaries of cities and districts do not always overlap. Hence observables from both cities and districts are not collinear.

for racial/ethnic demographics.

How do we interpret the importance of peer characteristics for government valuation? One channel may be that richer peers pay more taxes, and hence the local government can provide higher-quality excludable public goods through higher spending. We investigate this possibility in Table 8. Hence, we include expenditures and revenue per capita to see how important additional resources are for the valuation of local governments.

In Panel A, we use city fixed effects, meaning we are comparing only school districts in the same city to one another. Consistent with the recent wave of school finance equalizations (Jackson et al., 2016; Lafortune et al., 2018), per capita expenditures and revenue are not correlated with valuation, whether or not we control for household income. This suggests households primarily value richer peers rather than more resources, which is in line with recent evidence on the absence of parental valuation of school quality directly but rather valuation of peers (Rothstein, 2006; Abdulkadiroğlu et al., 2020; Lafortune and Schönholzer, 2021).

In Panel B, we use district fixed effects, comparing only cities within the same school district to one another. Here, there is some evidence that expenditures and in particular property tax revenue matters, in addition to household income. Since expenditures and revenues are in logs, these estimates imply that a 10% increase in per capita expenditures of the city are associated with a 0.4% in valuation (column 4). Similarly , a 10% increase in property tax increase is associated with a 0.2% higher valuation (column 6).

In sum, government valuation is related to all three factors considered at the outset of this section: it is related to output in the form of school quality; to input in the form of higher city per capita spending through higher per capita property tax revenues; and to peers in the form of richer households in the same jurisdiction.

7 Conclusion

This paper provides precise estimates of house price discontinuities at local government boundaries, comparing only houses on the same street. The willingness to pay for households to access certain local governments is at least in part mediated by the quality of services offered by those governments as well as the peers with whom they share excludable local public goods. We briefly discuss several implications regarding efficiency and policy.

Samuelson's 1954 Condition suggests that (local) public goods are efficiently provided when a dollar increase in expenditures leads exactly to a dollar increase in willingness to pay to live in the jurisdiction. Doing this on the basis of the results presented here alone is difficult and would require an economic model to interpret these reduced-form parameter estimates. Such a model would have to pay special attention to the peculiar structure of local governance in the U.S., whereby education and other local public goods are provided by separate entities. In addition, it is unclear how to value the non-excludable part of local public goods provided by local governments, which are not discontinuous at the boundary between adjacent jurisdictions.

The other major touch stone in local public finance is Tiebout (1956), which posits that household sorting leads to efficient provision of local public goods. However, more recent theoretical findings, including those by Hamilton (1975) and Bewley (1981) shed doubt on the extent of sorting-induced efficiency. The results presented here may inform a model of sorting by documenting heterogeneous preferences for (excludable) local public goods in much greater detail, as well as their correlation with observed and unobserved neighborhood characteristics. Estimating patterns of household sorting explicitly in the dataset presented here would be an interesting avenue for future research.

Finally, while the primary contribution of this paper is to estimate preferences for excludable local public goods, it raises important considerations for local government policy as well as federal incentives guiding local policy. It is unlikely that the patterns of household sorting documented here are welfare-maximizing, especially for under-represented minority groups. Even though Black buyer shares do not exhibit a jump at local government boundaries, there is a striking gradient whereby Black households live in worse neighborhoods, even when controlling for a substantial amount of observable and unobservable variation, in line with Bayer et al. (2007). This paper is also the first to document a substantial drop in the share of Hispanic households on the higher-price side of local government boundaries. Hence, both of these groups face barriers to access higher-quality local public goods, but perhaps for slightly different reasons: Black households seem to struggle accessing high-quality neighborhoods, while Hispanic households seem to have less opportunities to access better school districts and cities. Federal policy may consider providing incentives or support that is targeted along these dimensions.

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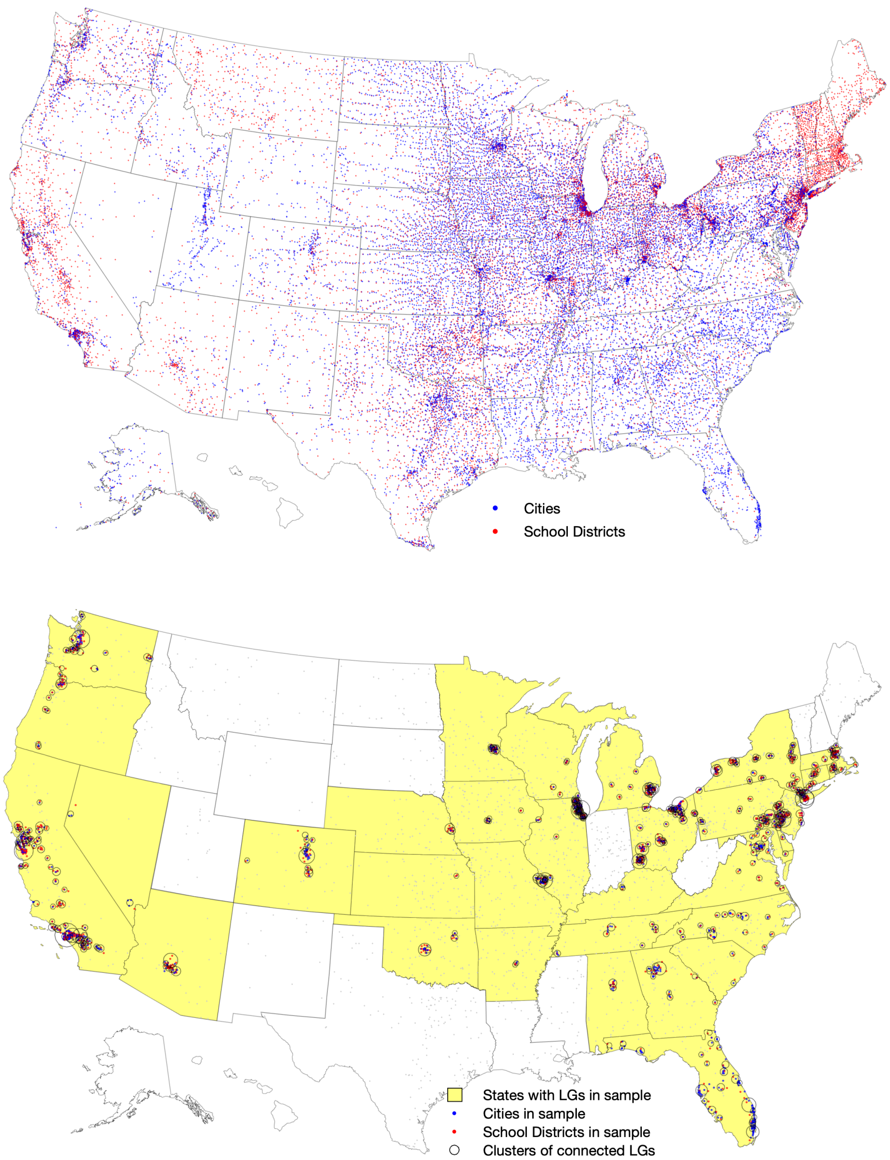
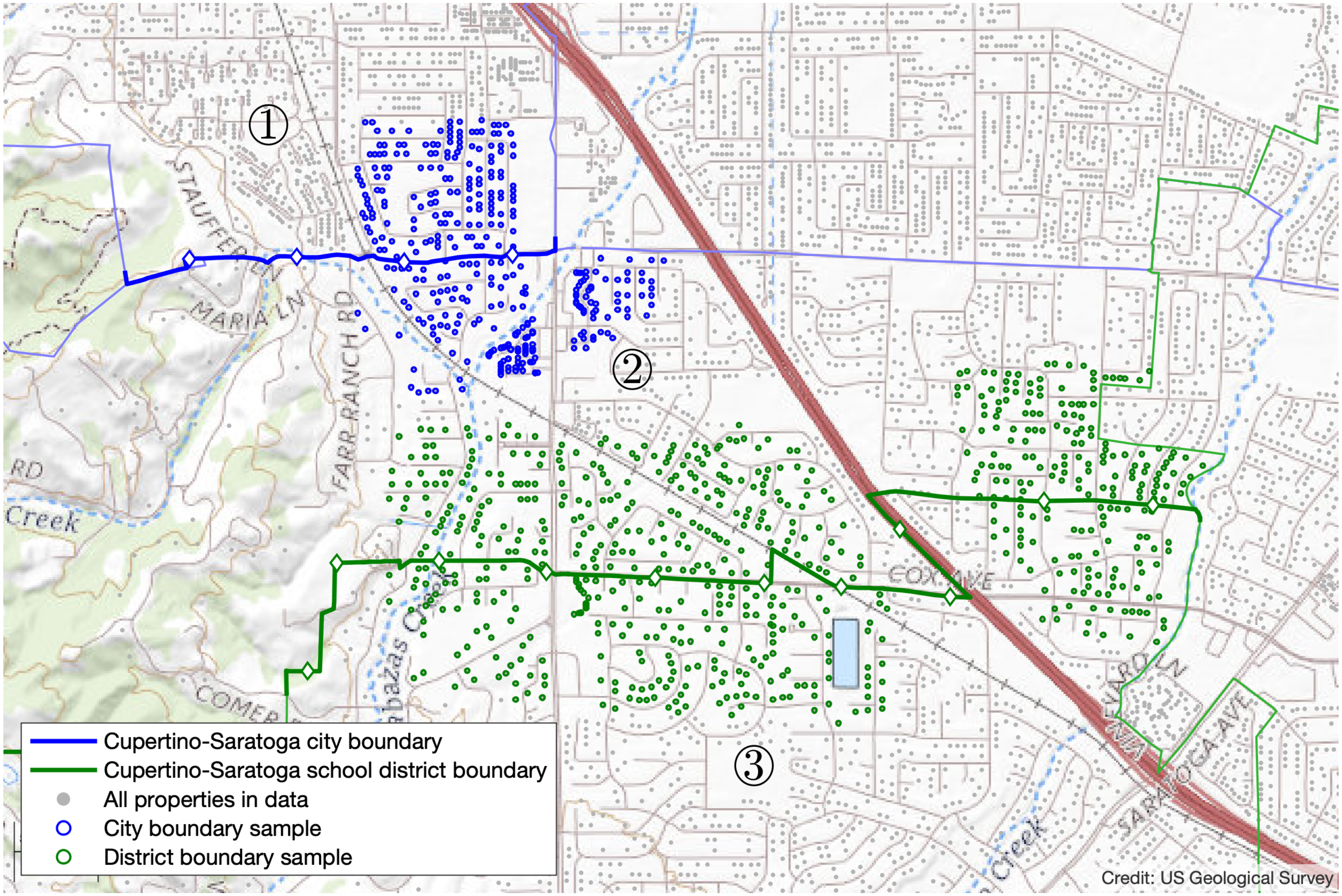
Figures

Figure 1: All and included local governments



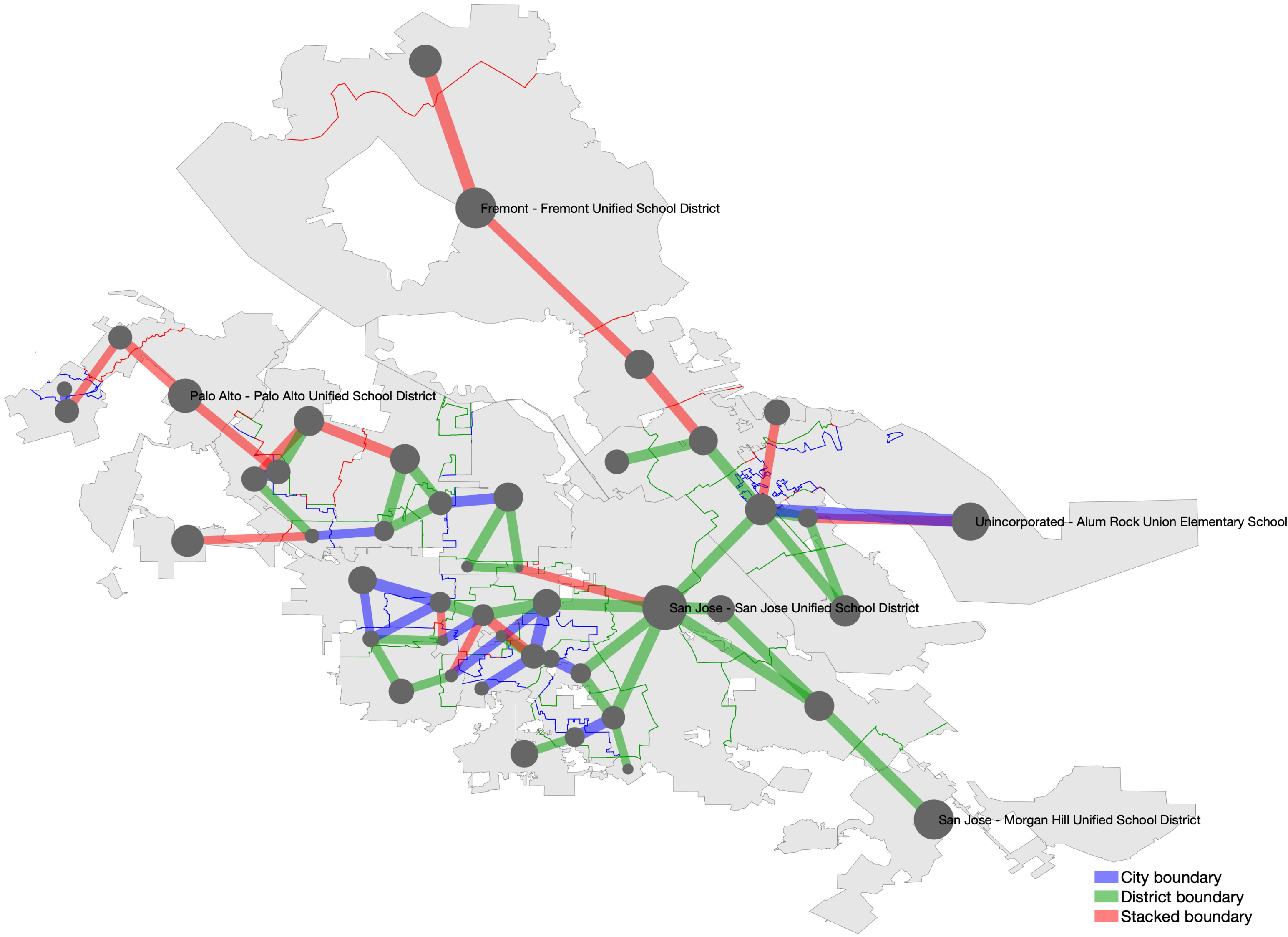
Notes: Centroids of all cities and school districts ("local governments") in the U.S. (top panel). Clusters of local governments connected by adjacent residential neighborhoods (bottom panel) included in the sample. The states in yellow have at least one cluster. Circles representing clusters are scaled according to the number of unique local governments in them.

Figure 2: Boundary samples with separate city and school district boundaries



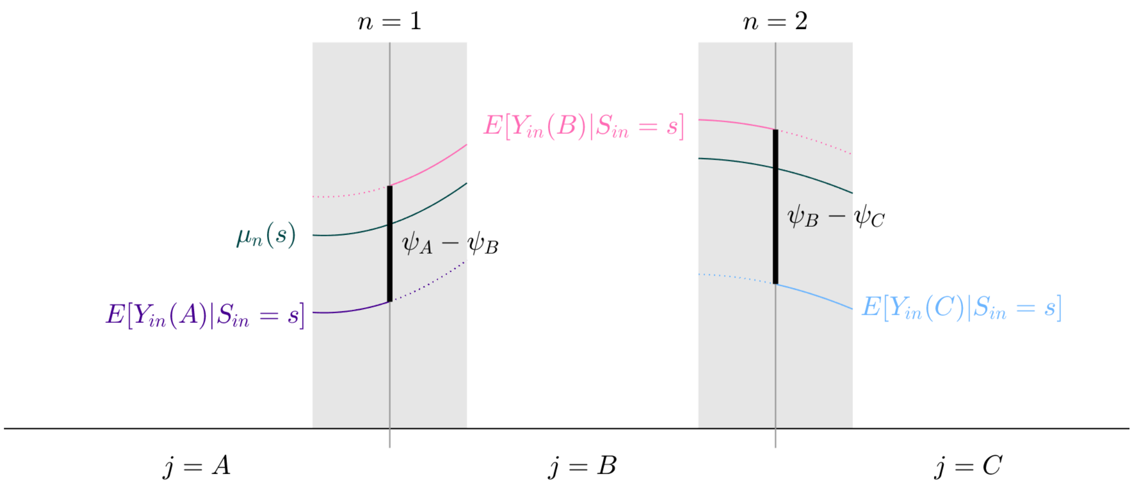
Notes: Fragment ① consists of the Cupertino city-district pair; fragment ② consists of the City of Saratoga but the Cupertino school district; and fragment ③ combines the Saratoga city-district pair. Properties in gray are in the data; blue-highlighted properties are associated with the Cupertino-Saratoga city boundary, whereas green-highlighted properties belong to the Cupertino-Saratoga district boundary.

Figure 3: Example of cluster of connected boundary discontinuities: San Jose, CA



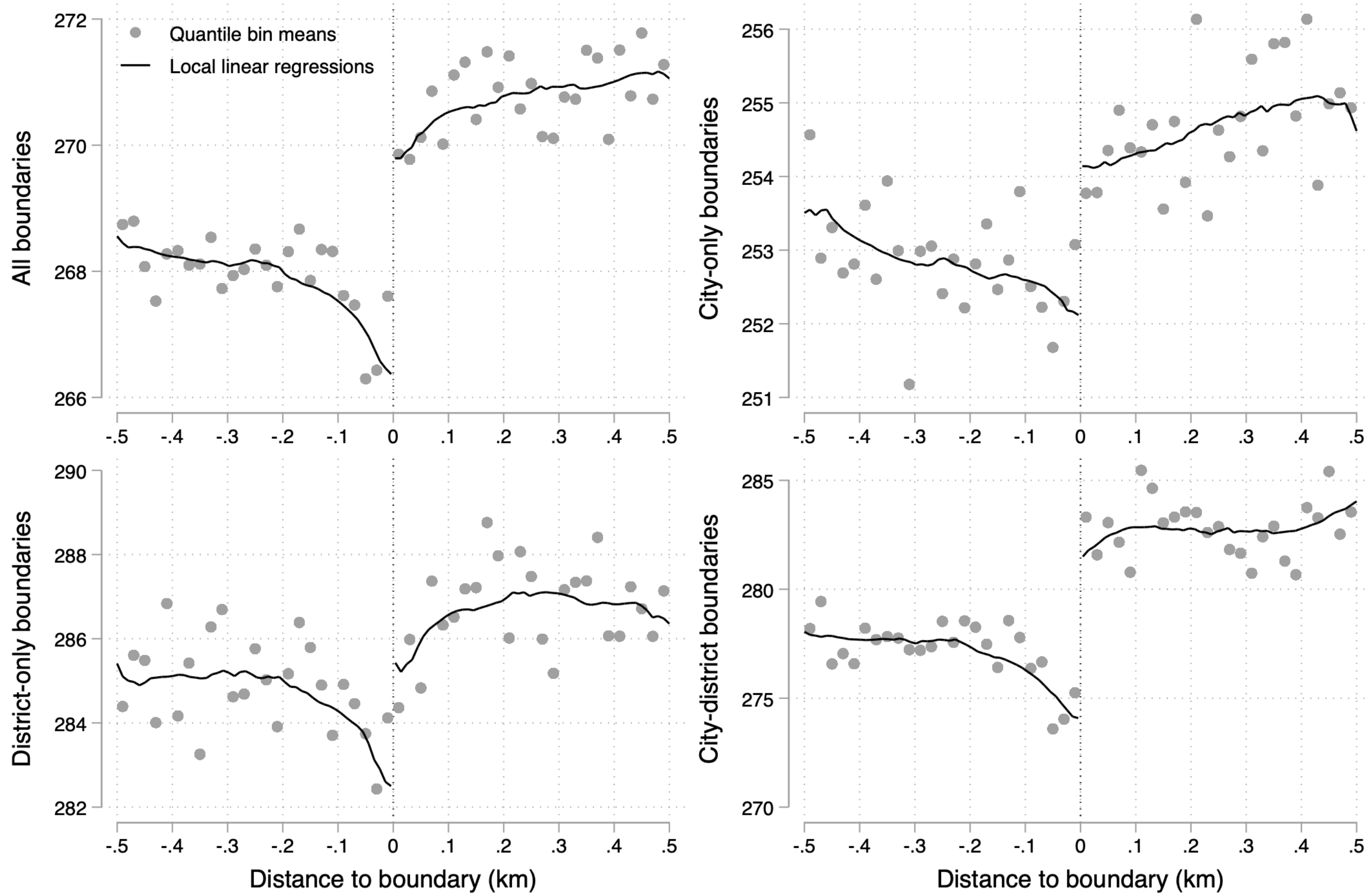
Notes: Each boundary in blue, green, or red has at least 100 single family houses on either side, with sufficient density of houses within 100 meters of the boundary. The dark gray dots represent unique city-school district pairs and are scaled relative to the size of the territory of the corresponding pair of local governments. The thickness of the blue, green and red lines is scaled relative to the number of houses near the boundary.

Figure 4: Identification of non-adjacent local government effect differences



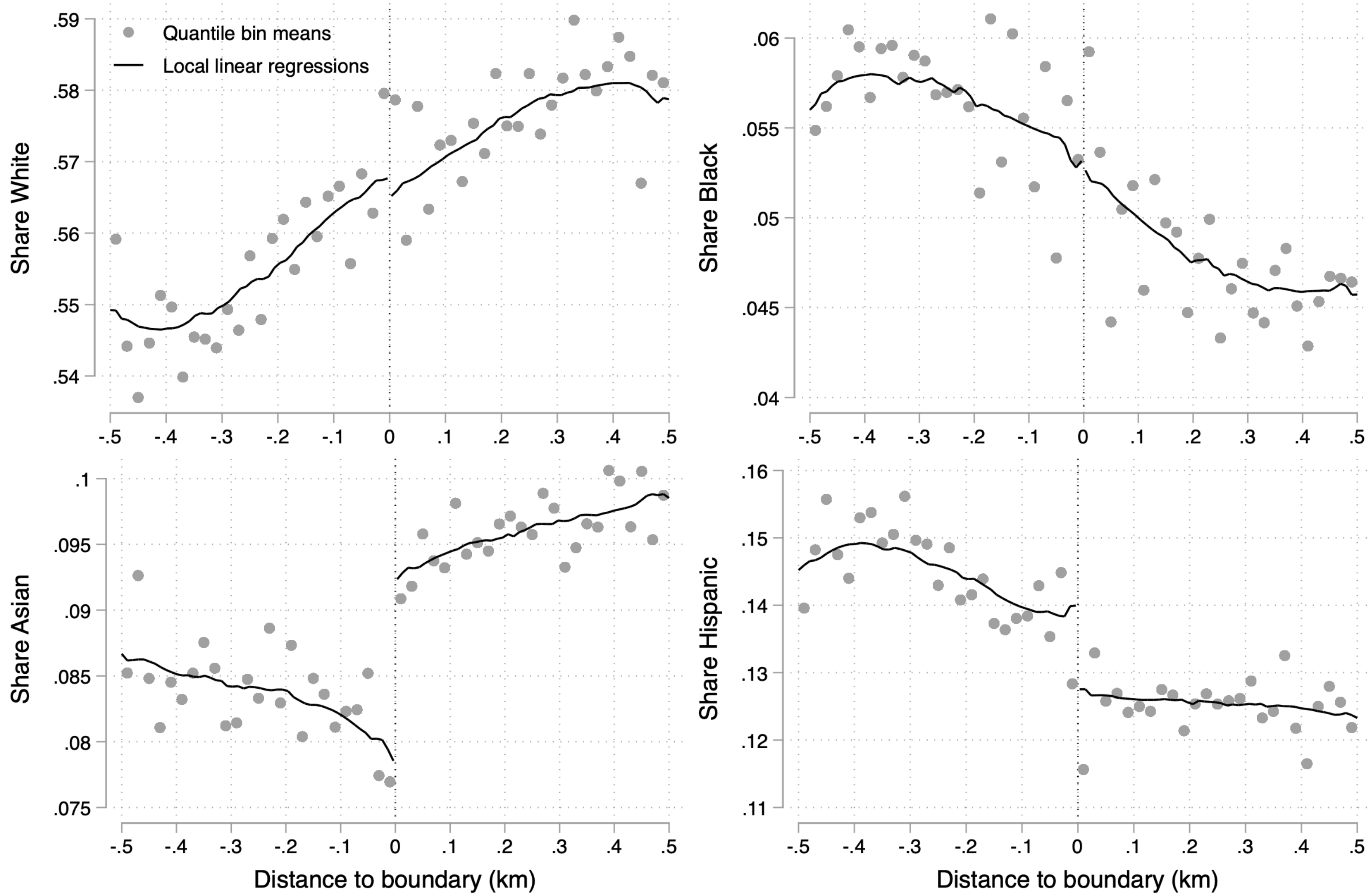
Notes: Three local governments A, B, and C are connected by two neighborhoods 1 and 2. In purple, pink and light blue are mean potential valuations of houses with access to excludable public goods provided by the corresponding local government. See main text for further details.

Figure 5: Residualized price discontinuities around mean ($1,000s) by boundary type, with controls including street FE



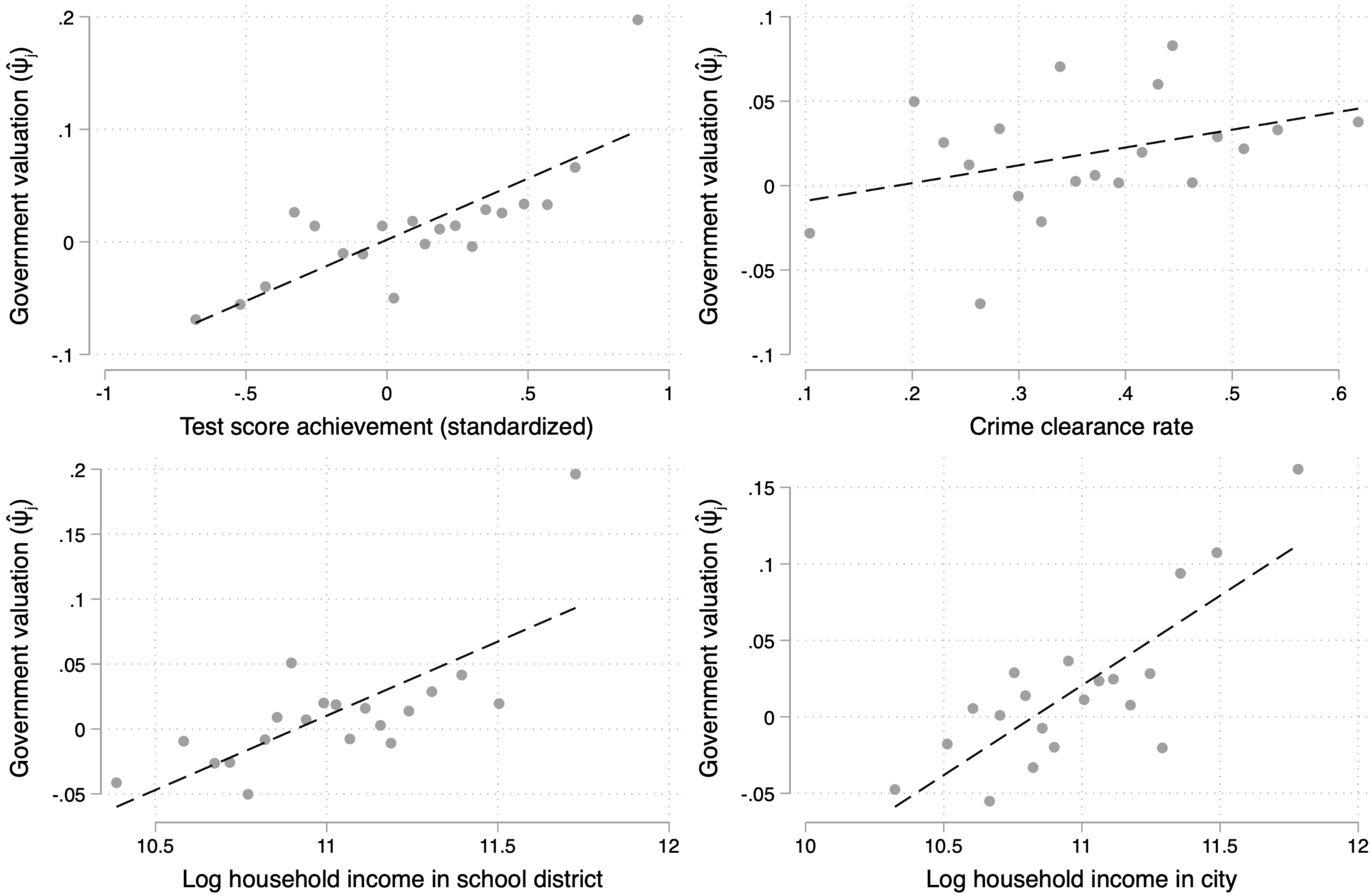
Notes: Nonparametric boundary discontinuity estimates of stacked boundaries in house prices (2010 dollars, in thousands), using 2% quantile bins (dots) and local linear regressions with a MSE-minimizing bandwidth of 160 meters as using the estimator proposed by Calonico et al. (2014). The negative distances are on the low average-price side, and the positive distances are on the high side. The top-left panel shows all types of boundaries; the top right shows city-only boundaries (same school district on either side); the bottom left shows district-only boundaries (same city on either side); the bottom right shows coinciding city-district boundaries.

Figure 6: Boundary discontinuities in race/ethnicity shares with controls including street FE



Notes: Nonparametric boundary discontinuity estimates of stacked boundaries in share of buyers with a White, Black, Asian, or Hispanic surname, using 2% quantile bins (dots) and local linear regressions with a MSE-minimizing bandwidth of 160 meters as using the estimator proposed by Calonico et al. (2014). The negative distances are on the low average-price side, and the positive distances on the high side.

Figure 7: Quality and household income correlates of estimated local government effects



Notes: Binned scatterplots of estimated local government valuation using equation (2) against proxies of government quality as measured by standardized student achievement and crime clearance rate of the police department; and peer quality as measured by household income of the city or the district.

Tables

Table 1: Summary statistics of local governments

	All local governments			Included in sample		
	P(10)	P(50)	P(90)	P(10)	P(50)	P(90)
Panel A: Cities						
Population (ths.)	0.1	1.2	17.0	5.0	27.8	136.5
White share	0.50	0.94	1.00	0.23	0.73	0.93
Black share	0.00	0.00	0.25	0.01	0.06	0.40
Hispanic share	0.00	0.02	0.20	0.01	0.08	0.39
Asian share	0.00	0.00	0.03	0.01	0.03	0.15
Household income (ths.)	26.2	41.1	69.1	37.5	55.4	91.3
Share college educated	0.07	0.15	0.32	0.13	0.25	0.45
Revenue p.c. (ths.)	0.23	0.83	2.29	0.57	1.20	2.62
Expenditures p.c. (ths.)	0.30	1.02	2.92	0.67	1.40	3.28
Crime clearance rate	0.18	0.39	0.60	0.21	0.38	0.54
Number of cities	19,494			1,041		
Panel B: School districts						
Population (ths.)	1.1	7.4	47.1	12.1	42.4	198.9
White share	0.54	0.93	0.99	0.28	0.76	0.94
Black share	0.00	0.01	0.15	0.01	0.04	0.29
Hispanic share	0.00	0.03	0.25	0.02	0.07	0.40
Asian share	0.00	0.00	0.04	0.01	0.03	0.14
Household income (ths.)	33.1	46.6	73.8	40.5	60.7	94.8
Share college educated	0.10	0.18	0.33	0.13	0.26	0.44
Revenue p.c. (ths.)	1.41	3.96	11.34	1.73	5.23	13.82
Expenditures p.c. (ths.)	7.81	10.88	18.94	7.42	11.46	19.17
Student achievement (std. devs.)	-0.38	0.03	0.42	-0.49	0.12	0.63
Number of school districts	12,240			1,079		

Notes: 10^{th}, 50^{th} (i.e. median), and 90^{th} percentiles of distribution of characteristics of incorporated places ("cities", Panel A) and school districts (Panel B). The first three columns show statistics for all local governments in the U.S., while the second three columns are only those local governments included in the sample (i.e. those with sufficient sample near a boundary neighboring another local government, as described in the text). Population through share college educated are from the ACS; revenue and expenditures per capita are from the Census of Governments; crime clearance fate is from UCR; and student achievement is from SEDA.

Table 2: Summary statistics of property records

	Property sample			Within 500m		Difference within	
	All	500m	100m	Low price	High price	500m	100m
Sales and tax characteristics							
Sales price (ths.)	234.5	269.8	262.1	249.3	291.5	42.2	29.2
Sales year	2006.9	2006.7	2006.8	2006.7	2006.7	0.1	0.1
White (buyer surname)	0.672	0.566	0.569	0.556	0.577	0.021	0.004
Black (buyer surname)	0.049	0.052	0.052	0.056	0.048	-0.009	-0.003
Hispanic (buyer surname)	0.087	0.135	0.132	0.144	0.125	-0.019	-0.013
Asian (buyer surname)	0.049	0.089	0.086	0.083	0.096	0.012	0.012
2016 property tax (ths.)	3.18	3.79	3.62	3.52	4.07	0.55	0.35
Property characteristics							
Year built	1976.5	1971.5	1973.6	1969.9	1973.1	3.2	2.8
Lot size (ths. sqft)	24.71	9.32	9.89	9.15	9.5	0.35	0.28
Building square feet (ths.)	1.8	1.7	1.7	1.62	1.78	0.15	0.12
Bathrooms	2.25	2.18	2.19	2.1	2.26	0.17	0.13
Rooms	3.69	4.34	4.16	4.28	4.42	0.14	0.09
Stories	1.44	1.42	1.41	1.4	1.43	0.03	0.03
Neighborhood characteristics							
White (block)	0.706	0.619	0.623	0.609	0.63	0.022	0.007
Black (block)	0.105	0.112	0.112	0.118	0.105	-0.013	-0.005
Hispanic (block)	0.118	0.168	0.164	0.177	0.158	-0.019	-0.011
Asian (block)	0.047	0.076	0.075	0.071	0.082	0.01	0.01
Household income (ths., block group)	66.9	71.1	70.1	68	74.4	6.4	4.6
College educated (block group)	0.2	0.21	0.21	0.2	0.22	0.02	0.01
Observations	25,141,841	1,471,325	329,486	757,534	713,791	—	—

Notes: Means of property characteristics of all properties ("All"); within 500 meters of a local government boundary ("500m"); and within 100 meters ("100m"). All prices and taxes in constant 2010 dollars. The next two columns show the means on the low-price side and the high-price side within 500 meters of a boundary; the last two columns are the difference between high-price mean minus low-price mean. All differences are significant at 1% level. Properties include all single-family residences in the thirty states in which any boundaries are included in the dataset.

Table 3: Price regression discontinuity estimates at local government boundaries

Price discontinuity at boundary type:	All		City-only		District-only		City-district	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1): Boundary-point and cluster-year FE	22.2	27.2	22.3	21.9	17.5	21.1	33.8	47.7
	(1.7)	(1.7)	(1.7)	(2.1)	(2.5)	(2.6)	(4.7)	(4.7)
(2): (1) + race/ethnicity of buyer FE	22.0	26.8	22.0	21.7	17.5	20.7	34.5	46.7
	(1.6)	(1.7)	(1.7)	(2.1)	(2.5)	(2.6)	(4.5)	(4.6)
(3): (1) + property controls	10.7	12.3	8.9	8.4	5.6	7.3	23.3	27.8
	(1.1)	(1.1)	(1.2)	(1.4)	(1.8)	(1.4)	(3.1)	(3.3)
(4): (1) + street FE	4.9	5.0	3.3	2.9	3.7	4.2	10.4	10.9
	(0.7)	(0.6)	(0.8)	(0.6)	(1.4)	(0.9)	(1.6)	(1.5)
(5): (2) + (3) + (4)	3.4	3.2	2.1	1.4	2.8	2.5	8.1	8.1
	(0.5)	(0.5)	(0.6)	(0.5)	(1.1)	(0.7)	(1.6)	(1.4)
RD estimator	OLL	Quartic	OLL	Quartic	OLL	Quartic	OLL	Quartic
Avg. bandwidth (km)	0.14	0.50	0.14	0.50	0.14	0.50	0.14	0.50
N boundaries	1744	1744	665	665	548	548	531	531
N properties	511,870	1,451,468	245,951	675,568	124,520	397,437	96,893	378,463

Notes: Each cell is a separate stacked boundary discontinuity estimate. "City-only" subsets the data to city boundaries only, with the same school district on either side; "District-only" subsets to school district boundaries, with the same city on either side; "City-district" subsets to boundaries that are both city and school district boundaries. Each row includes a different set of control variables, included in regressions using the residualized procedure proposed by Lee and Lemieux (2010), and they comprise of boundary-point and cluster-by-year fixed effects; race/ethnicity by state fixed effects; age, lot size and interior square feet interacted with state fixed effects; number of rooms, bathrooms, and stories interacted with state fixed effects; and street fixed effects. "OLL" is a local linear estimator with optimal bandwidth (Calonico et al., 2014); "Quartic" uses separate quartic polynomials on each side. Reported average bandwidths are averages across the five different control sets. Standard errors are clustered by boundary.

Table 4: Price discontinuities by race and race discontinuities

	House price		Race/ethnicity				
			White	Black	Asian	Hispanic	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discontinuity	3.2	3.0	-0.003	-0.001	0.013	-0.012	0.001
	(0.6)	(0.5)	(0.014)	(0.006)	(0.006)	(0.009)	(0.006)
Discontinuity × Black	0.1	-0.9					
	(0.9)	(0.6)					
Discontinuity × Asian	4.5	5.1					
	(1.5)	(1.1)					
Discontinuity × Hispanic	-0.8	-1.1					
	(0.7)	(0.5)					
Discontinuity × Unknown	0.4	-0.1					
	(0.6)	(0.4)					
Black	0.8	1.0					
	(0.7)	(0.5)					
Asian	-4.4	-3.0					
	(1.3)	(1.0)					
Hispanic	0.7	0.3					
	(0.6)	(0.4)					
Unknown	-0.5	-0.5					
	(0.5)	(0.4)					
RD estimator	OLL	Quartic	OLL	OLL	OLL	OLL	OLL
Bandwidth (km)	0.16	0.50	0.18	0.21	0.19	0.17	0.15
N boundaries	1744	1744	1744	1744	1744	1744	1744
N properties	525,650	1,451,468	603,325	687,051	617,102	561,207	505,230

Notes: Stacked boundary discontinuity estimates of price (2010 dollars, in thousands) in columns 1 and 2; discontinuity estimates in race/ethnicity shares in columns 3-7. Race/ethnicity is proxied by buyer surname similar to Diamond et al. (2019). Regressions use the residualized procedure proposed by Lee and Lemieux (2010). "OLL" is a local linear estimator with optimal bandwidth (Calonico et al., 2014); "Quartic" uses separate quartic polynomials on each side. Standard errors are clustered by boundary.

Table 5: House price variance components for the 20 largest clusters

	Metro cluster statistics				$Var(\psi_j)$		$Var(\alpha_n)$		$Cov(\psi_j, \alpha_n)$	
	J	N	I	$Var(Y_{in})$	AKM	KSS	AKM	KSS	AKM	KSS
Los Angeles, CA	72	510	73961	0.517	0.06	0.059	0.07	0.068	0.004	0.004
San Jose, CA	46	341	56168	0.605	0.049	0.046	0.117	0.113	-0.024	-0.021
West Long Island, NY	44	202	33314	0.337	0.054	0.053	0.018	0.016	-0.01	-0.009
Chicago, IL	36	165	31614	0.398	0.045	0.044	0.159	0.158	-0.03	-0.029
Seattle, WA	39	331	41979	0.327	0.01	0.008	0.038	0.036	-0.005	-0.004
Denver, CO	33	329	57495	0.313	0.019	0.018	0.037	0.036	-0.009	-0.008
Phoenix, AZ	30	177	31067	0.428	0.016	0.015	0.09	0.089	-0.009	-0.009
Cleveland, OH	29	171	26936	0.517	0.035	0.034	0.062	0.06	0.019	0.02
West Covina, CA	27	170	25213	0.251	0.008	0.007	0.005	0.004	-0.001	0
Boston, MA	26	100	15365	0.395	0.077	0.074	0.042	0.039	-0.028	-0.025
Riverside County, CA	25	126	23024	0.278	0.013	0.012	0.014	0.013	0.002	0.002
Central Long Island, NY	25	160	22818	0.309	0.008	0.007	0.008	0.007	0.001	0.002
Cincinnati, OH	23	161	22462	0.306	0.023	0.023	0.043	0.041	-0.005	-0.004
Atlanta, GA	21	291	29646	0.434	0.038	0.038	0.106	0.105	0.017	0.017
Oklahoma City, OK	21	156	22567	0.348	0.011	0.01	0.031	0.029	-0.002	-0.002
Contra Costa County, CA	17	145	16783	0.423	0.048	0.047	0.039	0.037	-0.025	-0.024
East Bay, CA	19	185	31430	0.577	0.06	0.059	0.161	0.159	-0.036	-0.035
North Miami, FL	19	150	18604	0.617	0.039	0.038	0.132	0.13	0.007	0.007
Port St. Lucie, FL	18	121	14744	0.661	0.099	0.097	0.144	0.14	-0.048	-0.045
Ballwin, MO	17	92	10917	0.403	0.009	0.008	0.042	0.04	0.013	0.014
Median	25.5	167.5	26074.5	0.4	0.037	0.036	0.042	0.04	-0.005	-0.004

Notes: Variance component estimates separately in each of the 20 largest clusters of connected local governments and neighborhoods according to equation (3) using both plug-in estimates as in Abowd et al. (1999) ("AKM") as well as bias-corrected leave-out-estimates proposed by Kline et al. (2020) ("KSS"). J, N, and I are the number of local governments, neighborhoods, and houses, respectively; Var(Y_{in}) is log house price variance; Var(ψ_j) is the variance of the local government component; Var(α_n) is the variance of the neighborhood component; and Cov(ψ_j, α_n) is their covariance.

Table 6: Connected house price variance components

	Metro cluster statistics				$Var(\psi_j)$		$Var(\alpha_n)$		$Cov(\psi_j, \alpha_n)$	
	J	N	I	$Var(Y_{in})$	AKM	KSS	AKM	KSS	AKM	KSS
All	1292	9903	1362137	0.375	0.045	0.044	0.074	0.072	-0.018	-0.017
Panel A: By sub-period										
1990-1998	1181	8108	231396	0.28	0.051	0.032	0.094	0.066	-0.03	-0.013
1999-2007	1287	9667	441574	0.31	0.033	0.024	0.065	0.049	-0.014	-0.007
2008-2016	1292	9890	621546	0.354	0.06	0.052	0.107	0.091	-0.023	-0.016
Panel B: By boundary type										
City-only	316	3224	384204	0.368	0.011	0.009	0.06	0.051	-0.004	-0.002
District-only	240	1443	208349	0.31	0.01	0.007	0.048	0.041	-0.005	-0.003
City-district	297	1661	258290	0.431	0.06	0.055	0.091	0.08	-0.013	-0.01
Panel C: By race/ethnicity										
White	1143	8573	764690	0.586	0.047	0.037	0.153	0.138	-0.022	-0.014
Black	223	1192	58455	0.671	0.043	0.034	0.136	0.117	-0.02	-0.012
Asian	646	3781	115484	0.599	0.07	0.051	0.256	0.232	-0.045	-0.027
Hispanic	714	4384	170425	0.46	0.068	0.033	0.197	0.155	-0.063	-0.029

Notes: Variance component estimates using the largest city per cluster as reference local government, according to equation (3) using both plug-in estimates as in Abowd et al. (1999) ("AKM") as well as bias-corrected leave-out-estimates proposed by Kline et al. (2020) ("KSS"). J, N, and I are the number of local governments, neighborhoods, and houses, respectively; Var(Y_{in}) is log house price variance; Var(ψ_j) is the variance of the local government component; Var(α_n) is the variance of the neighborhood component; and Cov(ψ_j, α_n) is their covariance. Boundary types and buyer race/ethnicity as described in main text.

Table 7: Projection of government valuation on government quality

Panel A: Cluster FE	Dependent variable: $\widehat{\psi}_j$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Student achievement	0.21	0.16	0.13	0.14				
	(0.04)	(0.04)	(0.04)	(0.04)				
Crime clearance rate					0.23	0.11	0.03	0.02
					(0.12)	(0.10)	(0.10)	(0.10)
Household income (district)		0.07		-0.02		0.26		0.14
		(0.06)		(0.07)		(0.07)		(0.07)
Household income (city)			0.18	0.18			0.28	0.21
			(0.04)	(0.04)			(0.04)	(0.04)
R^2	0.15	0.15	0.18	0.18	0.06	0.13	0.15	0.17
Number of metro clusters	286	286	286	286	228	228	228	228
Number of LG fragments	1,300	1,300	1,300	1,300	1,081	1,081	1,081	1,081
Panel B: City/district FE	City FE				District FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Student achievement	0.12	0.07	0.11	0.07				
	(0.02)	(0.04)	(0.03)	(0.05)				
Crime clearance rate					0.11	-0.04	0.04	-0.04
					(0.12)	(0.10)	(0.11)	(0.10)
Household income (district)		0.08		0.08				
		(0.07)		(0.06)				
Share Black (district)			-0.04	-0.05				
			(0.08)	(0.08)				
Share Hispanic (district)			0.01	0.02				
			(0.11)	(0.11)				
Household income (city)						0.18		0.19
						(0.04)		(0.05)
Share Black (city)							-0.15	0.06
							(0.13)	(0.14)
Share Hispanic (city)							-0.24	-0.04
							(0.08)	(0.13)
R^2	0.64	0.64	0.64	0.64	0.54	0.58	0.56	0.58
Number of metro clusters	290	290	290	290	180	180	180	180
Number of LG fragments	1,051	1,051	1,051	1,051	661	661	661	661

Notes: Regression of estimated local government valuation $\widehat{\psi}_j$ on observable characteristics of cities and school districts, with a focus on government quality. Student achievement from SEDA in standard deviations; crime clearance rate from UCR; household income in logs. Share Black and Hispanic as a share of the total population of the city or district. Cluster fixed effects in Panel A; city or district fixed effects in Panel B. Standard errors are clustered by cluster of connected local governments.

Table 8: Government valuation, government spending, and household income

Panel A: City FE	Dependent variable: $\widehat{\psi}_j$					
	(1)	(2)	(3)	(4)	(5)	(6)
Household income (district)		0.15		0.15		0.15
		(0.04)		(0.04)		(0.04)
Expenditures p.c. (district)	-0.02	-0.02				
	(0.02)	(0.02)				
Revenue p.c. (district)			0.04	0.00		
			(0.02)	(0.02)		
Property tax revenue (district)					0.03	-0.01
					(0.02)	(0.02)
R^2	0.63	0.65	0.63	0.65	0.63	0.65
Number of metro clusters	265	265	265	265	265	265
Number of LG fragments	994	994	994	994	994	994
Panel B: District FE	(1)	(2)	(3)	(4)	(5)	(6)
Household income (city)		0.19		0.18		0.16
		(0.04)		(0.04)		(0.04)
Expenditures p.c. (city)	0.02	0.04				
	(0.02)	(0.02)				
Revenue p.c. (city)			0.02	0.03		
			(0.02)	(0.02)		
Property tax revenue (city)					0.03	0.02
					(0.01)	(0.01)
R^2	0.54	0.58	0.54	0.58	0.55	0.58
Number of metro clusters	219	219	219	219	216	216
Number of LG fragments	794	794	794	794	779	779

Notes: Regression of estimated local government valuation $\widehat{\psi}_j$ on observable characteristics of cities and school districts, with a focus on expenditures and revenue. Household income, expenditures per capita revenue per capita, and property tax revenue in logs. Standard errors are clustered by cluster of connected local governments.

Online Appendix for

Measuring Preferences for Local Governments

Contents

A Further Datasets

American Community Survey (ACS). To measure observable characteristics of cities, school districts, and block groups, we use the 5-year data from the ACS, specifically total population, counts by Hispanic or Latino origin and race (forming four non-exclusive groups: White, Black, Hispanic, and Asian), educational attainment for the population 25 and over, and median household income in the last 12 months (in 2010 dollars). College educated is coded as having attended least some college. The data were obtained through NHGIS (Manson et al., 2017; SABINS, 2011).

Census of Governments (CoG). Fiscal data for cities and school districts comes from the "Data Files on Historical Finances of Individual Governments" (U.S. Census Bureau Government Division, 2015). We use the data from the 2012 Census of Governments. The primary variables include general revenues per capita, general expenditures per capita, and property tax revenue per capita.

Stanford Education Data Archive (SEDA). To measure school district quality, we use standardized test scores assembled by SEDA (Reardon et al., 2018). We use version 2.1 of pooled achievement data on the cohort scale and school district level.

Uniform Crime Reports (UCR). To measure city quality, we use data from UCR (United States Department of Justice, 2015). The clearance rate is the count of cleared crimes over the count of total crimes in each category. We average crime clearance rates first across all crimes within the property crime and violent crime categories and then across the two categories, for years 2005-2014.

National Transportation Dataset (NTD). To assess whether a particular segment of a boundary coincides with a road, we use data from the NTD, provided by the USGS (U.S. Geological Survey, 2020). There are eight types of segments encoded in these data: (1) federal highways/freeways, (2) state highways, (3) state routes, (4) residential streets, (5) onramps/offramps, (6) rural roads, (7) bridges, and (8) tunnels. Residential streets make up 92.8% of segments.

B Data Preparation

Data preparation involves four steps: the raw assessor records from ATTOM, the boundary data from various sources as described in the main text, the spatial match of ATTOM and boundary data, and addressing noisy boundaries. We describe each of these steps in turn.

B.1 Preparation of Raw Data from ATTOM

We proceed state by state. We keep only properties categorized as single-family residences (codes "R" and "VRES" in ATTOM). We keep only properties whose sales value is between \$5,000 and \$10,000,000 and sold between 1990 and 2017. We then winsorize sales prices at the 1st and 99th percentiles by year of sale and drop any properties without a sales price. Residential addresses have been geocoded by the Fisher Center for Real Estate & Urban Economics at UC Berkeley.

Turning to housing characteristics, we drop properties without year-built information. We replace year built by "effective" year-built whenever year built is missing or outside the 1950-2017 range. The building age is then the difference between the year of assessment and the year built, and we drop properties with negative age. We keep only properties whose lot size in square feet is between 100 and 1,000,000 and again winsorize at the 1st and 99th percentiles, dropping properties with no lot size information. We proceed the same for interior square feet, again winsorizing at the 1st and 99th percentiles after dropping properties outside the 100-1,000,000 range. We only keep properties with at least 0.25 and at most 20 bathrooms; at most 50 rooms; and at least 1 and at most 5 stories.

Race/Ethnicity Classification of Buyers. To classify buyers by race/ethnicity, we proceed along the lines of Diamond et al. (2019), although we use only 2010 Census genealogy information without the supplementary information from NamePrism. Their procedure builds in turn on a methodology developed by CFPB (2014).

In Step 1, using the data on the frequency of each surname with at least 100 counts in the Census and self-reported race/ethnicity, we first compute each surname's share of respondents in the six race/ethnicity categories (White, Black, American Indian and Alaskan Native, Asian/Pacific Islander, two-or-more races, and Hispanic). We impute missing shares by assigning nationwide means per race so that shares add up to one for each surname. We then keep only the four main categories of interest (White, Black, Asian, and Hispanic). We then merge surname race/ethnicty shares to the surnames in ATTOM, either to the primary entry of buyer surname, or, if there is no match, to the secondary buyer.

In Step 2, we compute a posterior probability of the race/ethnicity of the buyer by using Bayes' rule and data on the distribution of the four race/ethnicity categories across Census blocks. Concretely, we compute:

$$\Pr(r|g,s) = \frac{\Pr(r|s)\Pr(g|r)}{\sum_{r'\in R}\Pr(r'|s)\Pr(g|r')},$$

where $\Pr(r|s)$ is the probability that a buyer is of race/ethnicity r if their surname is s, and $\Pr(g|r)$ is the probability of living in Census block g if their race/ethnicity is r. R denotes the four race/ethnicity categories. In this way, we update probabilities to reflect the geographic distribution of race/ethnicity categories across Census blocks. As in Diamond et al. (2019), we then assign a race/ethnicity category if its posterior probability is above 80%.

B.2 Preparation of Boundary Data

As described in the main text, we use 2010 TIGER/Line place boundaries as a basis of city boundaries, and NCES EDGE school district boundaries for school districts. For place boundaries, we restrict to the class "C" (cities). Because not all areas of the U.S. belong to a city, we use 2010 TIGER/Line county boundaries to assign territories without cities to their corresponding county as the primary entity providing local services. For school districts, we use both elementary school districts and unified school districts (which together cover the United States). We keep only boundaries that fall within the boundaries of a 2010 Core-Based Statistical Area (CBSA), which excludes boundaries in rural parts of the U.S. where boundaries are unlikely to have sufficient density of housing nearby.

We then create fragments of unique city-district pairs by finding the intersection of each city territory with each school district territory in each state, keeping all non-empty intersections. Territories not included in this set of intersections are residual district territories, which are subsequently added to the data, and for which we assign the county as the corresponding "city".

B.3 Spatial Match of ATTOM and Boundaries

The most involved part of the build consists of the steps involved in spatially matching geocoded ATTOM data to the boundary fragments. This involves five steps which we describe in turn:

1. *Match ATTOM to Census blocks and block groups*: We begin by assigning to each ATTOM property a corresponding Census block using the Census block shapefile provided by the U.S. Census and subsequently merging all corresponding Census block information to each property. We then update race/ethnicity categories of buyers using Census block information as described above. Using the Census block identifiers, we can also match Census block group information to each ATTOM property.

2. *Identify each adjacent boundary pair and assign houses to boundary*: We then loop through each city-district fragment and find the segment of its boundary that is nearby another fragment's boundary. This creates 500-meter buffer zones around each boundary segment, which is associated with a unique fragment pair. We then match houses to each buffer zone, so that a single house may belong to more than one boundary buffer zone as long as it is within 500 meters of that boundary segment.

3. *Place evenly-spaced boundary points along boundary*: We then create boundary points by moving along each fragment-pair boundary segment and placing a point every 500 meters. We drop boundary points that are closer than 250 meters to one another (which can happen when the boundary meanders strongly, or when one fragment fully

surrounds another).

4. *Match boundary points to USGS roads data*: We assign each boundary point to a highway if it is within 100 meters of a highway segment (road segment types 1-3), or to a residential road if it is within 50 meters of such a segment (type 4).

5. *Measure distance of house to boundary and apply inclusion criteria*: For each property in each boundary buffer, we then measure the distance to its corresponding boundary. We also assign the nearest boundary point to each property and assign spatial averages values of price, year built, lot size, interior square feet, share White, Black, Hispanic and Asian households within 100 meters, 250 meters, and 500 meters of each property. We include a boundary in the sample only if there are sufficient properties associated with the boundary, each boundary point, on each side, and for each distance bin. Concretely, we require:

   (a) at least 100 houses on either side of the boundary;

   (b) there are at least 10 houses on each side of the boundary for each boundary point;

   (c) no less than 10% of houses near a boundary point are on one side of the boundary;

   (d) the boundary point is not associated with a highway (segment types 1-3);

   (e) at least 10 properties in each of the ten 100-meter distance bins on either side of the boundary.

B.4 Dealing with Boundary Noise

TIGER/Line boundary data are known to be unsuitable for high-precision measurement applications such as engineering problems or property transfers (U.S. Census Bureau, 2016). As can be seen in the Northeast of Figure 2, this can result in inaccurate assignment of properties to each boundary side for properties that are very close to the boundary.[10] We interpret these caveats by the Census Geography division as limited precision at scales below 30 feet (10 meters).

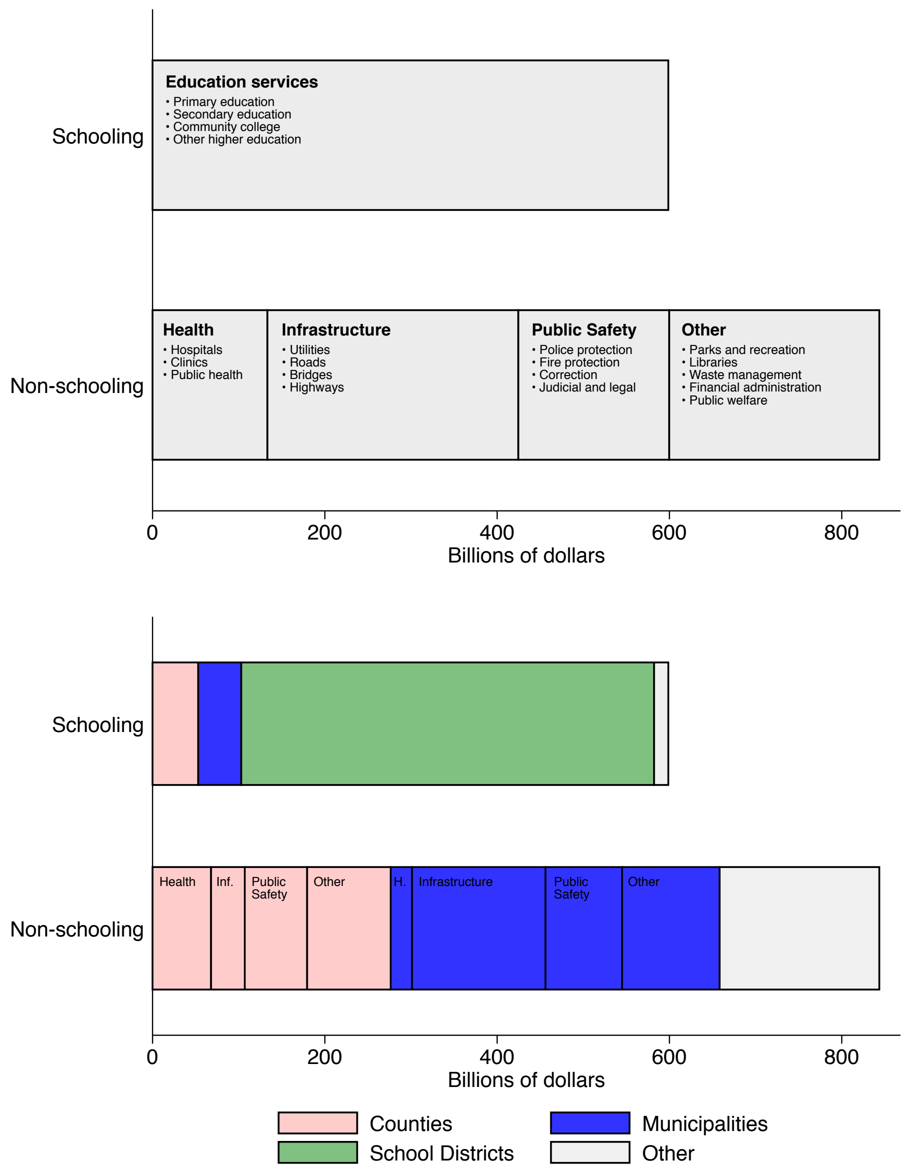
To deal with this kind of boundary noise, we drop properties whose centroids are very close to the boundary, where "very close" is defined as having a parcel size and a centroid

[10]When the green boundary turns Eastward in the Notheast of the map, it can be seen cutting across five properties, and it is unclear to which side of the boundary they belong.

distance to the nearest boundary such that the parcel centroid is less than half of a lot width from boundary, assuming that a parcel's lot width is twice as large as its lot length. The logic is that if the centroid were any closer than that, the boundary would intersect with the lot, and so there would be no clear assignment to either side of the boundary. This procedure identifies 0.7% of properties (10,356 out of 1,481,681) that are too close to a boundary to be reliably assigned to one side and thus subsequently dropped.

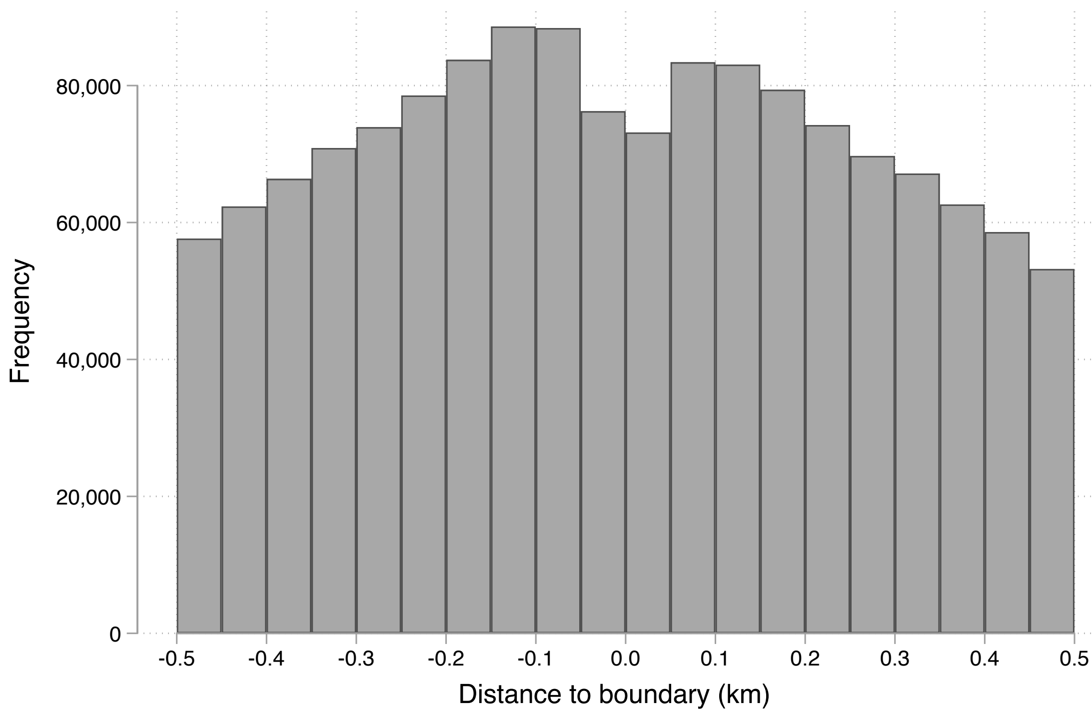
Appendix Figures

Figure A.1: Schooling and local public goods expenditures.



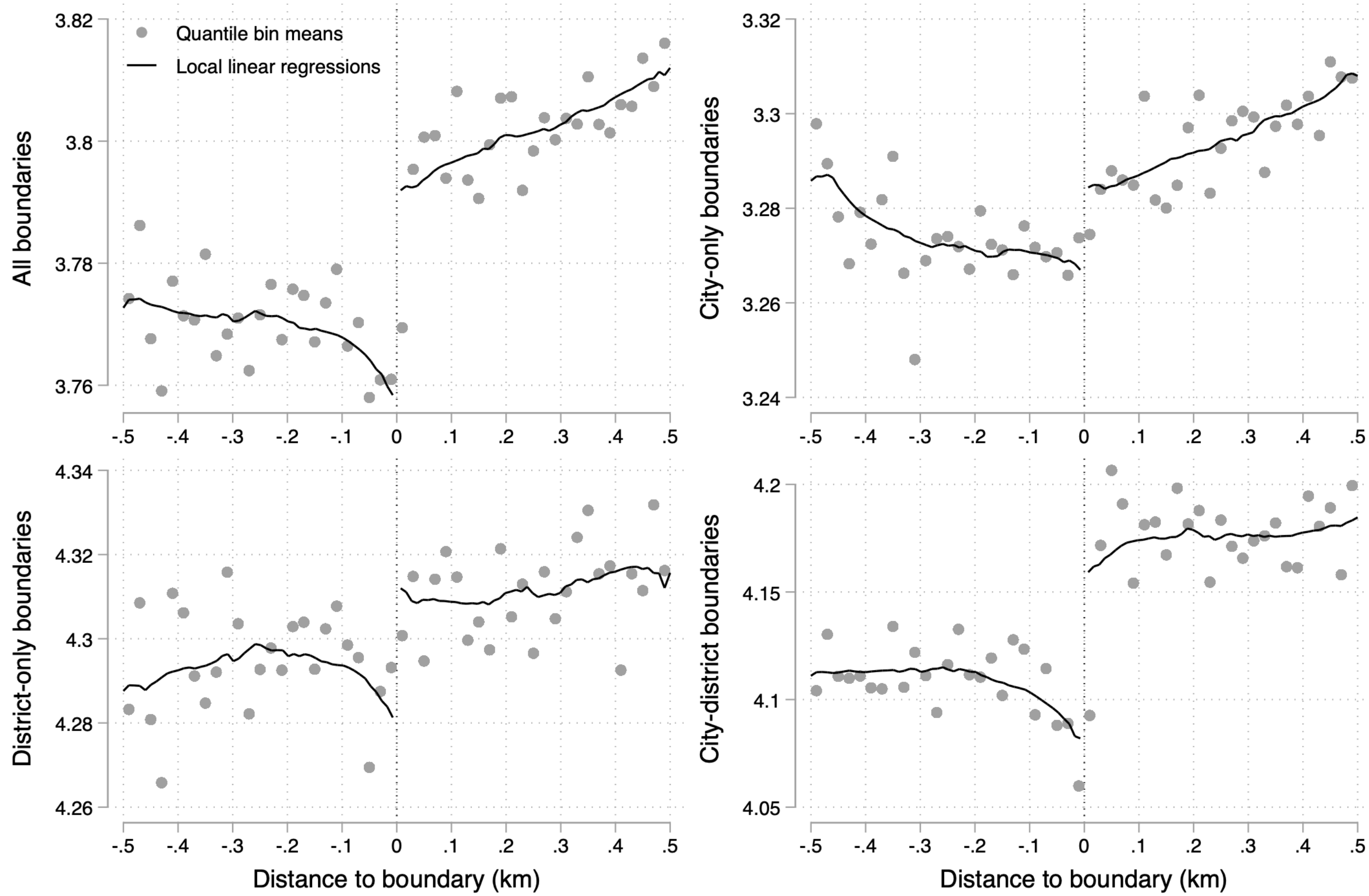
Note: Types and quantities of local public goods expenditures, divided into schooling and local public goods. In the top panel, we show expenditures by expenditure category. In the bottom panel, the same categories are organized by the type of local government providing them. "Other" includes townships and special districts.

Figure A.2: RD density plot: Number of observations in distance-to-boundary bins



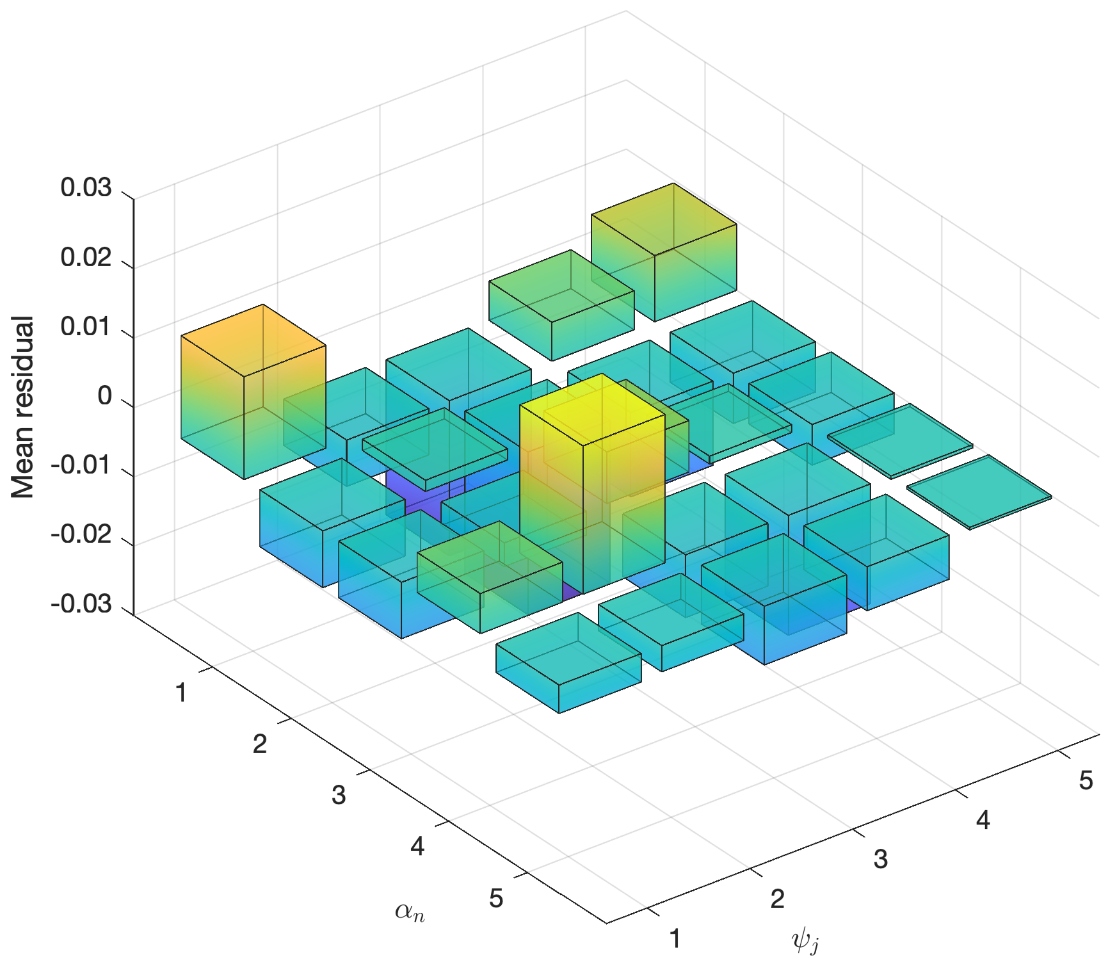
Notes: Number of properties near the stacked boundaries. The two 50-meter bins closest to the boundary have a lower number of observations since a parcel centroid cannot be infinitely close to the boundary. Properties closer than 7.21 meters to the boundary are dropped, as described in Appendix B for data preparation and inclusion criteria.

Figure A.3: Discontinuity in property taxes



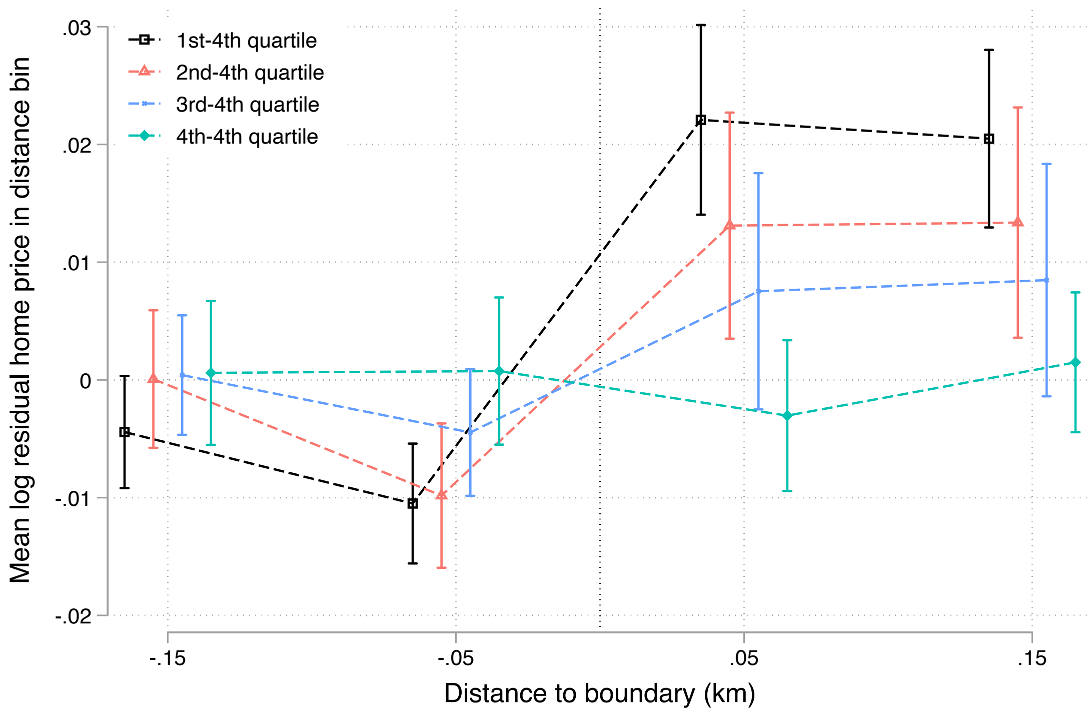
Notes: Nonparametric boundary discontinuity estimates of stacked boundaries in 2016 property tax payments (2010 dollars, in thousands), using 2% quantile bins (dots) and local linear regressions with a MSE-minimizing bandwidth of 160 meters as using the estimator proposed by Calonico et al. (2014). The negative distances are on the low average-price side, and the positive distances are on the high side. The top-left panel shows all types of boundaries; the top right shows city-only boundaries (same school district on either side); the bottom left shows district-only boundaries (same city on either side); the bottom right shows coinciding city-district boundaries.

Figure A.4: Mean residuals by neighborhood/government quintiles



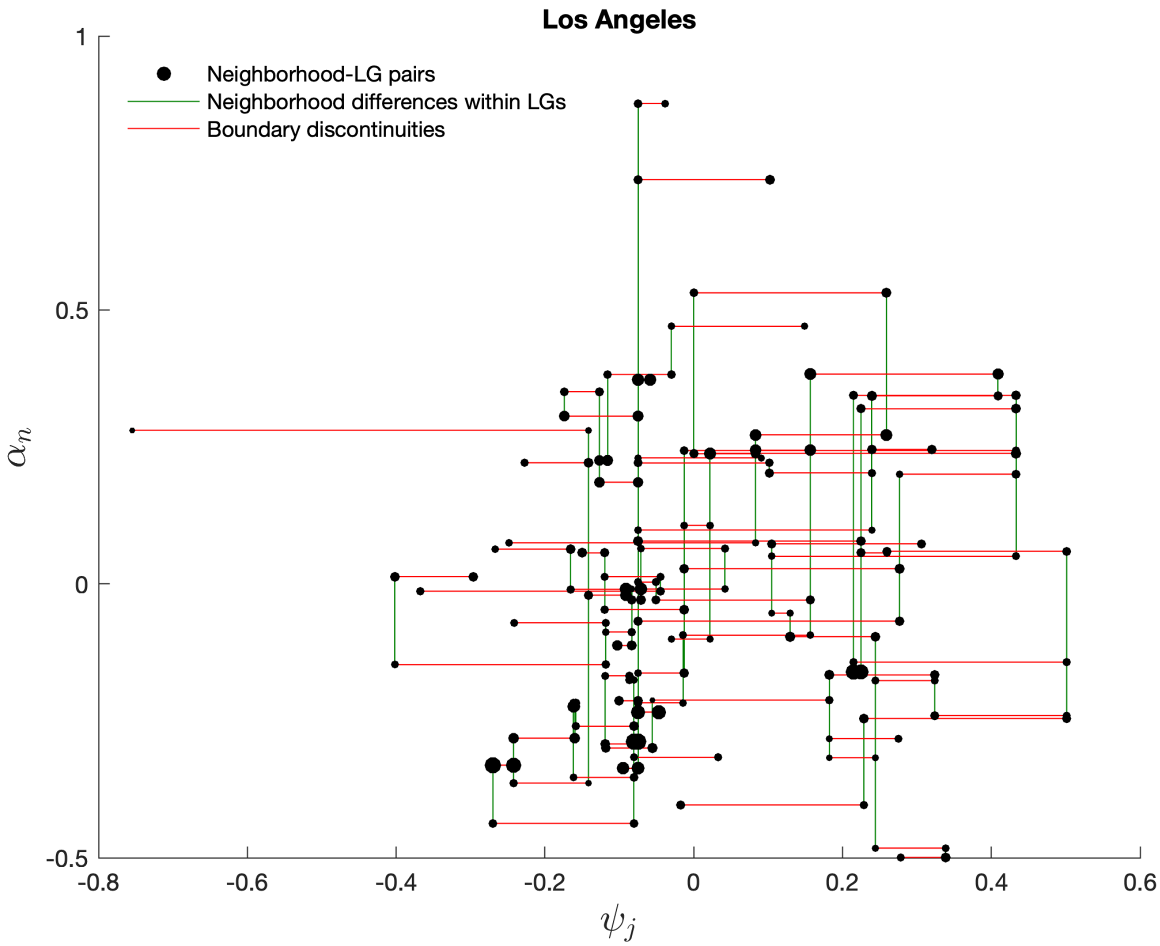
Notes: Mean residuals $Y_{in} - \left(\alpha_n + \psi_{j(i,n)} + \mathbf{X}'_{in}\gamma\right)$ per cell defined by interactions of quintiles of estimated local government valuations $\widehat{\psi}_j$ and estimated neighborhood valuations $\widehat{\alpha}_n$. Y_{in} is the log house price residualized with respect to cluster fixed effects.

Figure A.5: Mean residual house prices after hedonic regression by distance bin



Notes: Estimated increase in residual house price near the boundary, going from a the lowest quartile of estimated local government valuation on the left to any of the four quartiles of the neighboring jurisdiction.

Figure A.6: Estimates of ψ_j and α_n for largest cluster (L.A.)



Notes: Example of estimated neighborhood effects and local government effects using AKM. Neighborhoods here are aggregated to one side of the boundary in a given pair of local governments.

Appendix Tables

Table A.1: Summary statistics of fragments and clusters

	Mean	P(50)	Min	Max
Panel A: Fragments				
Number of other fragments in same city	1.92	1	0	18
Number of other fragments in same district	3.18	1	0	43
City and district are equal	0.12	0	0	1
Fragment is district and strict subset of city	0.34	0	0	1
Fragment is city and strict subset of district	0.21	0	0	1
Fragment is strict subset of city and district	0.32	0	0	1
Fragment is unincorporated	0.27	0	0	1
Number of fragments	1,968			
Panel B: Clusters				
Fragments in cluster	4.43	2	2	73
City and district are equal	0.55	0	0	17
District is strict subset of city	1.51	1	0	21
City is strict subset of district	0.94	0	0	16
Fragment is strict subset of city and district	1.43	0	0	35
Fragment is unincorporated	1.21	1	0	20
Number of clusters	444			

Notes: Summary statistics of local government fragments and clusters of connected fragments. “City and district are equal” means that the boundaries of a city and the school district perfectly coincide. “Fragment is unincorporated” means that the part of the school district in the fragment under consideration lies outside any incorporated (i.e. city) boundary.

Table A.2: Regression discontinuity estimates of property tax amounts

	All		City-only		District-only		City-district	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discontinuity	0.034	0.029	0.013	0.011	0.029	0.017	0.079	0.082
	(0.009)	(0.009)	(0.008)	(0.009)	(0.017)	(0.016)	(0.029)	(0.033)
RD estimator	OLL	Quartic	OLL	Quartic	OLL	Quartic	OLL	Quartic
Bandwidth (km)	0.13	0.50	0.13	0.50	0.13	0.50	0.13	0.50
N boundaries	1744	1744	665	665	548	548	531	531
N properties	458,817	1,447,970	241,685	674,086	110,989	396,211	88,695	377,673

Notes: Dependent variable is the 2016 property tax payment in thounds of 2010 dollars. "City-only" subsets the data to city boundaries only, with the same school district on either side; "District-only" subsets to school district boundaries, with the same city on either side; "City-district" subsets to boundaries that are both city and school district boundaries. Controls included in regressions using the residualized procedure proposed by Lee and Lemieux (2010), and they comprise of boundary-point and cluster-by-year fixed effects; race/ethnicity by state fixed effects; age, lot size and interior square feet interacted with state fixed effects; number of rooms, bathrooms, and stories interacted with state fixed effects; and street fixed effects. "OLL" is a local linear estimator with optimal bandwidth (Calonico et al., 2014); "Quartic" uses separate quartic polynomials on each side. Reported average bandwidths are averages across the five different control sets. Standard errors are clustered by boundary.

Table A.3: Plug-in AKM variance components of house prices

		Sub-periods			Type of boundary		
	All years	1990-1998	1999-2007	2008-2016	City	District	Stacked
Parameter estimates							
SD neighborhood effects	0.415	0.409	0.417	0.491	0.384	0.404	0.415
SD local government effects	0.172	0.225	0.204	0.190	0.068	0.078	0.143
SD property characteristics	0.506	0.401	0.467	0.525	0.475	0.489	0.650
Corr(α_n, ψ_j)	-0.175	-0.338	-0.314	-0.149	-0.040	-0.132	-0.030
Corr$(\psi_j, X'_{in}\gamma)$	0.018	-0.044	0.008	0.022	0.037	0.023	0.026
Corr$(\alpha_n, X'_{in}\gamma)$	0.107	0.046	0.090	0.096	0.129	0.051	0.079
Model evaluation							
RMSE of AKM residual	0.313	0.286	0.301	0.291	0.327	0.280	0.306
Adjusted R^2	0.800	0.668	0.764	0.844	0.769	0.805	0.845
Comparison match model							
RMSE of match model	0.301	0.268	0.284	0.273	0.314	0.272	0.297
Adjusted R^2	0.810	0.702	0.780	0.856	0.782	0.813	0.851
SD of match effect	0.437	0.411	0.426	0.518	0.408	0.413	0.447
Model statistics							
SD home prices	0.761	0.637	0.712	0.815	0.729	0.707	0.868
Number neighborhoods (N)	10,772	7,717	10,252	10,627	5,614	2,724	2,434
Number local governments (J)	1,968	1,641	1,936	1,958	1,061	811	726
Number properties	323,586	46,747	103,234	149,799	171,383	78,882	73,175

Notes: Plug-in estimates of variance components along the lines of equation (3), normalizing clusters relative to the cluster average of house prices. The match model allows for unrestricted neighborhood-government interactions.