Online Appendix
The Assessment Gap: Racial Inequalities in Property Taxation

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A. Equitable Tax Null

We formalize the intuition behind our null hypothesis of an equitable tax as follows. We consider first a property tax system that does not establish individual tax exemptions, and then show the theory easily incorporates an arbitrary exemption structure. Let i denote property, j taxing jurisdiction, and t year. Further, let V^* be the true value of the property being taxed. Given an intended rate of taxation r_{jt}, by definition an ad valorem tax must satisfy:

$$equitable\ tax_{ijt} = r_{jt}V^*_{ijt}. \tag{1}$$

Note that r is an effective tax rate. Let c be the local target assessment ratio, and let r^{pol} be the policy tax rate that rationalizes equation 1: $r_{jt} = r^{pol}_{jt}c_{jt}$. This last equation simply reflects that if assessments are deliberately scaled to be half of market value, the policy rate must double in order to achieve the level of tax burden implied by r.

Property tax bills are generated by applying the policy rate to an assessed valuation, A_{ijt}:

$$actual\ tax_{ijt} = r^{pol}_{jt}A_{ijt}. \tag{2}$$

Our equitable tax null is simply that $actual\ tax_{ijt} = equitable\ tax_{ijt}$. We observe A_{ijt}, the realized assessed valuation assigned to the house. We observe market prices for homes, M_{ijt}, and accordingly will let $M_{ijt} = V^*_{ijt}$.[1] Equating 1 and 2, and taking logs yields a necessary condition for equitable administration of an ad valorem tax:

$$ln(A_{ijt}) - ln(M_{ijt}) = ln(c_{jt}) := \gamma_{jt} \qquad \forall i. \tag{3}$$

Equation 3 is a theoretical statement that does not allow any errors at all in assessments.

Empirically, we define a deviation from our fair tax benchmark in context of arbitrary delineations. Partition the homes of any jurisdiction into M subsets, and denote by $m \in \{1, 2...M\}$. Let $\bar{c}_{mjt} := \frac{1}{N}\sum_{i\in m} c_{ijt}$. Our fair taxation null is:

$$\bar{c}_{mjt} = \bar{c}_{m'jt} \qquad \forall m, m'. \tag{4}$$

Equation 3 states that assessment ratios should not vary at all within jurisdiction. While strictly true, this represents unattainable precision. Equation 4 says that average assessment ratios should not vary within jurisdiction for any arbitrary group. Our central estimating equation is the empirical counterpart of the theoretical statement:

$$ln(A_{ijt}) - ln(M_{ijt}) = \gamma_{jt} + \beta^r race_{ijt} + \epsilon_{ijt}. \tag{5}$$

[1] It is worth reiterating that state laws regularly and explicitly state that property taxation should be levied upon the "fair cash value" that would be received in an arm's-length transaction. Therefore, our reliance on market prices is not a strong statement about market efficiency, but rather a reflection of the legal intent underlying the taxation.

Here *race* is a vector of indicator variables for racial and ethnic groups. The fixed effect γ_{jt} absorbs the realized average assessment ratio within jurisdiction. Then, since *race* is a categorical variable, β^r is a vector of estimated group-level deviations from average realized assessment ratio.

The derivation above abstracts away from tax exemptions. As noted in Section 2.1 of the paper, most jurisdictions establish individual-level criteria for tax exemptions. Incorporating these exemptions, the expressions for equitable tax and actual tax bills become:

$$actual\ tax_{ijt} = r^{pol}_{jt}(A_{ijt} - E_{jt}(i)) \tag{6}$$

$$equitable\ tax_{ijt} = r_{jt}(V^*_{ijt} - E^*_{jt}(i)). \tag{7}$$

$E_{jt}(i)$ is the homeowner-level exemption established by law, and is written as a function of i to highlight dependency on personal characteristics (e.g. age or residency status). $E^*_{jt}(i)$ is the corresponding portion of the market value shielded by tax. This differs from E_{jt} only due to the scaling factor c_{jt}. If assessments in a given jurisdiction are done at 50% of market value, an exemption that reduces assessed value by \$10,000 corresponds to a reduction in market value of \$20,000: $E^*_{jt} = c_{jt}E_{jt}$. Given this relationship, the equitable tax benchmark implied by equations 6 and 7 is equivalent to equation 3.

B. Data Construction

B.i Taxing Jurisdictions

Local governments are highly spatially complex. Across the U.S. more than 75,000 entities potentially impose a property tax. Homeowners typically face taxation from multiple local units simultaneously. Cities and counties are key examples of local government units. However, it is very common for regions to have a range of separate autonomous taxing entities. Chief examples here are: school districts, park districts, and municipal utility districts. Taxing authority may also be embedded in a special purpose district like an airport authority or regional economic development initiative. As a rule, the boundaries of these units are not naturally coincident. Counties are a complete partition of space in the US: every point in a given state lies in exactly one county. However, no such logical precision applies to other local entities. Cities often lie across county boundaries. In low-population-density areas, school districts often cover multiple towns (and potentially portions of different counties); in urban areas, there may be multiple school districts within a given metropolitan region. Units like park districts or utility districts typically have a delineation governed by a service area that reflects physical geography and may have little to do with nearby civic boundaries. Excluding state governments, the average home in the United States is touched by 4.5 local entities, all of which potentially levy a property tax.[2]

Panel A of Figure A1 illustrates our approach in a stylized example. There are three govern-

[2] Author's calculations using Atlas Muni Data shapefiles.

ments in this example: the county, which contains a city and an independent school district. The city and the school district have partial overlap. This spatial overlay of governments generates 4 taxing jurisdictions. Jurisdiction one contains those homes which receive services from, and are taxed by, the county alone. Homes in jurisdiction two are served and taxed by both the county and the city; homes in jurisdiction three are served and taxed by all governments; and homes in jurisdiction four are served and taxed by the school district and the county. Panel B of Figure A1 highlights our focus on within-jurisdiction inequality. In this stylized example, the county realizes assessment ratios of either 50% or 20%. This generates inequality in the taxing jurisdiction comprised of just the county: there is large (binary) variation in assessment ratio. This does not generate inequality in the jurisdiction served by both the city and the county: everyone paying taxes and receiving public services in this region has the same assessment ratio. For any cross-jurisdiction comparisons, we cannot rule out Tiebout sorting along preferences for public goods or intended levels of property tax. Our focus is solely on inequality between residents who are subject to the same set of taxes and who have access to the same bundle of public goods.

The example in Panel A of Figure A1 is, in fact, quite common across the county. However, jurisdictions can be complex, especially in more urban regions. Figure A2 shows the example of Harris County, Texas. Including the county, there are 12 local units of government which overlap in varying combinations. Each combination forms a distinct jurisdiction. One such jurisdiction is the region defined by the nexus of all 12 governments (this region is not visually identifiable in Figure A2). In our full sample, we observe a market transaction (paired with an assessment) for approximately 100 homes within this particular jurisdiction. This is a relatively small jurisdiction. Others are the size of cities and encompass tens of thousands of home transactions.[3]

While our jurisdictions have both a natural economic and political interpretation, it is certainly reasonable to wonder whether our results are driven in any way by the partitioning of geography. We can test this fairly directly. Practically speaking, assessments are most commonly done at the county level. Often this is a provision of state law, but even when not required, it seems that either custom or natural considerations of efficiency and resource management often result in counties "owning" assessments. While it does not make any sense to compare effective tax rates within county (because so many sub-county units impose other property taxes and provide services), if target assessment ratios are unlikely to vary within county, we can meaningfully compare assessment ratios within county instead of within jurisdiction. In Section C., we show that our baseline results establishing racial differences in assessment ratio are robust to conducting our analysis within county. Within county estimations, in fact, generate slightly higher estimates. Our preferred specifications all use the more rigorous partitioning into jurisdictions of unique overlapping governments.

[3] In some regions, all substate units of government are spatially aligned; Philadelphia is one such example: the county and city of Philadelphia, along with the school system, are all entirely coincident. This is relatively rare.

B.ii Constructing Assessment Ratios

We obtain property-level records for both market transactions and assessed valuations from ATTOM Data Solutions. We use two linked datasets from ATTOM: (i) the Recorder Deeds data, which contains the near universe of real estate transactions; and (ii) the Assessor/Tax data, which contains an annual panel of property attributes including property assessments for the near-universe of residential properties.

We construct property-level assessment ratios as follows. Transactions are identified by a date-of-sale and a unique (static) property identifier. Starting from every residential transaction listed in the Recorder data, we exclude: (i) any transaction other than a resale, (ii) any transaction flagged as a partial sale, and (iii) any transaction for less than full consideration. We also exclude any record with zero reported transaction value.[4] We further remove any property for which multiple transaction records exist for the same day, unless the price information is exactly duplicated.[5] The result is 124M resale observations spanning 59.8M distinct properties.

For each of these properties, we then pull the full time-series of assessed valuations, each associated with an assessment year in the underlying administrative records. We remove properties with missing assessment information, along with any records which duplicate over property and assessment year while diverging on assessment value. We then merge the assessment and transaction records by property ID and year. In the recorder data, year comes directly from transaction date; and in the assessment data, year comes from the stated assessment year. Assessment ratios are formed as assessed value divided by market (transaction) value. We restrict attention to assessment ratios from 2005 onward, to match availability of tract-level data from the American Community Survey 5-year estimates.[6] As described in the paper, we remove California properties from our core dataset.[7] At this stage, we have 28.8M assessment ratios associated with 21.7M unique properties.

We implement the following final cleaning steps: (i) remove any property which we are unable to match to a census tract, (ii) remove any property for which the sale value is less than $500, (iii) remove any property which we are unable to associate with government shapefiles, (iv) remove any property with a residential classification other than: single family home, condominium, duplex, or apartment, (v) trim any observation with an assessment ratio less than 3 or 0.01.[8] Collectively, these steps remove 4.3M observations spanning 3.0M properties from our final sample. We are left with 24.5M assessment ratios associated with 18.6M properties.

[4] Several states either do not mandate disclosure of sales price, or do not distribute the records publicly.

[5] This occurs in 0.8% of the properties that transact.

[6] This restriction removes 5.8 million assessment ratios from 2003-2004. The ATTOM assessor records extend back to 2003. The recorder data extends back substantially further into the 1900s; counts become substantially lower prior to the late 1980s.

[7] This removes 4.19M transactions spanning 3.10M properties. Standalone results for CA are presented in this Online Appendix in Section C..

[8] There are regions which target assessment ratios of 10% or less. We are unaware of any region targeting a ratio exceeding 100%. This step removes 1.2M observations across 1.0M properties.

B.iii Associating Assessment Ratios with Homeowner Race and Ethnicity

To establish homeowner race and ethnicity, we merge the ATTOM dataset with Home Mortgage Disclosure Act (HMDA) records. These records include HMDA applies to financial institutions meeting certain criteria – the major one being an asset threshold which is currently \$46M for depository institutions and \$10M for for-profit mortgage lenders. During the 2005–2016 period we consider, between 6,900 and 8,900 institutions reported loans ranging in number from 14.3 to 33.6M annually.[9]

HMDA loan records are identified by: year, census tract, lender name, and dollar amount (rounded to thousands). The ATTOM data contains: transaction date, latitude and longitude of the property, lender name, and dollar amount. We restrict our sample to the highest quality matches, requiring an exact match on year (permitting a one-month overlap between December and January), an exact match on tract, an exact match on (rounded) transaction amount, and a fuzzy string match on lender name. The diversity of retail-outlet names within a single financial institution can make exact string-matching a challenge in some regions. We rely on a natural language algorithm developed by the Real Estate and Financial Markets Laboratory at the Fisher Center for Real Estate and Urban Economics to match names. The algorithm trains itself within region on perfect singleton matches across all variables other than name, and then uses that mapping to assign a confidence index to each HMDA-ATTOM string-pairing.

Our central challenge is that HMDA records pin down the race and ethnicity of the individual establishing a mortgage. We wish to associate assessment ratios with the race and ethnicity of the home *seller*, since this is the owner at the time when the relevant assessment was generated. We proceed as follows. For every property in the final sample of assessment ratios (described in prior section), we extract *every* transaction associated with that property. We match each transaction to a HMDA record, if possible. Out of the 18.6M properties in the final set of cleaned assessment ratios, we are able to match 14.7M properties to at least one HMDA record.

For every transaction denoted in ATTOM as "resale" or "refinance," we associate the property with primary applicant's race and ethnicity listed in HMDA, for the transaction year. We code race and ethnicity as unknown for any resale or refinancing transaction which does not match the HMDA data, for any instance where the HMDA record itself reflects unknown race or ethnicity, or for any instance where multiple HMDA records match a single transaction and conflicting race and ethnicity information is given.[10] For multiple transactions within a year, we associate race and ethnicity with each transaction (including the unknown designation, as necessary) and then sort by date so that we have race/ethnicity at both year beginning and year end.

This leaves us with an incomplete panel of property-year-race/ethnicity observations. We transform this into a complete panel by filling race/ethnicity, including the unknown designation: (i)

[9] Summary statistics from www.ffiec.gov.

[10] This latter case does not necessarily denote an error. It could arise, for instance, from applicant and co-applicant switching on a given loan record. In the case of two records, one of which has missing race/ethnicity information, we do use the data from the populated record.

forward from resale transactions until the next observed transaction, and (ii) filling backwards from refinance transactions until a previously observed transaction. When multiple transactions occur within a year, we fill forward from the last transaction, and backward from the first transaction (only if that first transaction is a refinance).

Finally, using the sample described in Section B.ii, we associate each assessment ratio arising from a transaction in year t with the race and ethnicity of the homeowner in year $t-1$. For public officials, producing assessments is a process of designing and validating a model, disseminating new values to homeowners, and often allowing for a set period for homeowners to appeal assessments before they are final. All of this takes time, which means that assessments applying to tax year t are, in general, produced towards the end of year $t-1$: therefore the relevant race/ethnicity for a home selling in year t is the race/ethnicity of the individual who owns the home in $t-1$. We exclude from our sample homes that sell in year t and also in year $t-1$, because multiple homeowners in year $t-1$ means that we cannot be sure which individual owned the home when the assessment was generated (we observe only the year of the assessment, not a precise date of estimation). We do not use observations with unknown race/ethnicity in our regressions.

The result is 6.99M observations spanning 6.11M homes. The major factor driving the reduction from 14.7 million properties which we match to HMDA is the need to observe two transactions in order to pin down race/ethnicity of home seller: either two sales, or a refinance transaction preceding a sale. Our sample is roughly evenly split between these two cases.

B.iv Attribute-Bundle Fixed Effects and Attribute-Implied Prices

We extract the following property-level characteristics from the assessor portion of the ATTOM dataset: square footage of livable space on the property, number of bathrooms, number of stories, year built, and three separate indicators for the presence of a pool, patio, and fireplace. We trim the sample to remove outliers, restricting attention to properties with fewer than 20 bedrooms, fewer than 20 bathrooms, less than 50,000 square feet, and less than 10 stories. This removes less than half a percent of available observations. We exclude any observations listing zero square feet, both zero bedrooms and zero bathrooms, or a number of stories greater than the total number of rooms, as well as any observation missing information in the six attribute fields.

We create categorical variables from the continuous measures by binning properties. For size: between 0 and 6,000 square feet, cutpoints are every 500 square feet; between 6,000 and 10,000 square feet, cutpoints are every 1,000 square feet; and from 10,000 to 50,000 cutpoints are every 5,000 square feet. For year built: cutpoints are every 10 years; we also group together all homes built before 1900. For bathrooms, cutpoints are: 0, 1, 2, 3, 4, 5, 7, 10, 15, 20.

We then create an overall attribute bundle variable by interacting: square footage bin, bathrooms bin, year built bin, and each of the three amenity indicators. This yields 5,450 distinct attribute-bundle fixed effects, in a sample with 4.67M assessment-ratio observations across 4.11M properties. The reduced sample relative to our baseline dataset is due to missing housing stock attributes in the ATTOM dataset.

We also construct a continuous measure of price based on housing stock attributes. At a high level, this variable is the inner product of a given home's attributes and the implied prices of those attributes:

$$\hat{p}_{ijt} = X'_{ijt}\hat{\beta}^X_{t,-s(j)} \tag{8}$$

where X is a vector of property attributes for house i in jurisdiction j during year t. $beta^X$ is a vector of estimated hedonic prices for each attribute. Crucially, these hedonic prices are estimated from transactions in <u>other states</u>, as denoted by $-s$ in the subscript. We write $s(j)$ to make explicit that a taxing jurisdiction defines a state by construction. The resulting price estimate, $\hat{p}$ therefore contains no local market information. Hedonic prices are estimated according to:

$$p_{ijt,-s(j)} = \alpha_{jt} + Z'_{ijt,-s(j)}\beta^Z_{t,-s(j)} \tag{9}$$

where $Z = [X\ W]$, is a vector that includes the property attributes X as well as W, the same set of tract-level covariates we use in the hedonic analysis of Section 5.3.1, and $\beta^Z = [\beta^X\ \beta^W]$. That is: for every house, we estimate attribute implied prices from transactions in all other states with a jurisdiction fixed effect, property-level characteristics, and neighborhood-level characteristics as independent variables. We estimate this specification separately for each year. Then, to construct $\hat{p}$, we take only the implied prices for the property attributes and multiply those by actual property characteristics. Without loss of generality, we can omit any jurisdictional scaling, because every subsequent regression using $\hat{p}$ includes a jurisdiction-year fixed effect.

C. Results - Extensions

C.i Additional Baseline Results

Table A1 shows estimates of inequality for all non-Hispanic homeowners identified as a racial minority in HMDA other than Black or Hispanic. The included racial designations in HMDA records are: (i) American Indian or Alaskan Native, (ii) Asian, and (iii) Native Hawaiian or Other Pacific Islander. Column (1) presents inequality within taxing jurisdiction, and columns (2) and (3) estimate inequality within census tract and census block group respectively. Inequality is substantially smaller for this grouping: just below 3% on average within a taxing jurisdiction, and approximately 2% within neighborhoods.

Table A2 shows estimates of inequality for California. Assessment ratios are 4.13% higher for Black homeowners, 10.6% higher considering Black or Hispanic homeowners together, and 6.5% higher for other non-black, non-Hispanic racial minorities. Results are presented separately because of how stringently assessment growth in governed by the provisions of Proposition 13. In this setting, the most relevant restriction is that assessments can grow only at the lessor of inflation or 2% during a given homeowner's tenure. During our sample period, home prices exceeded these caps in the majority of regions.[11] As a result, misalignment between assessed values and market values is largely a mechanical function of homeowner tenure, making a subsequent exploration of how inequality arises less relevant in California. Proposition 13 is a canonical example of an administrative policy creating inequality that correlates with race. Other states impose policies with the potential to cap assessment growth, but California is unique both for its size (if included in the national sample, the 1.8M observations reflected in Table A2 would be 20% of the total) and for the frequency with which the administrative cap binds.[12]

In Table A3, we re-estimate the assessment gap using county-year fixed effects rather than jurisdiction-year. The point of this exercise is to show that our careful partitioning of space into taxing jurisdictions is not somehow mechanically driving our results. Differing levels of intended taxation by cities, towns, schools and others makes a within-county analysis of effective tax rate meaningless. However, counties are most often the entity which produces assessments. We can therefore reasonably consider assessment ratio variation within county-year. The results are very consistent with our baseline finding. Inequality in assessment ratios is approximately 4% higher within-county than it is within-jurisdiction. Our preferred specifications all employ the more rigorous within-jurisdiction analysis, not only because it is more likely to hold local assessment practices fixed, but more importantly because jurisdictions are able to hold fixed intended level of taxation and the set of entities providing public services.

We also split the national sample into quintiles based on minority population share at the county-level. The first quintile contains counties with the smallest minority share and the 5th quintile is comprised of counties with the largest. We estimate the assessment gap in each of these sub-samples. Figure A3 shows results from these regressions graphically, and Table A4 shows the regression estimates. The assessment gap is clearly increasing in minority population share. Since we have shown that a large portion of the assessment gap is linked to spatial sorting, this finding is unsurprising: it has been documented that spatial sorting increases as minority population increases (Card, et. al 2008).

For completeness, Table A5 shows the estimated hedonic prices associated with the results in Figure 4, and Table A6 shows the results of adding the neighborhood-level covariates used in our hedonic pricing analysis to the baseline estimation of the assessment gap. As implied by our findings in Section 5.3.1, spatial variation across the range neighborhood attributes induces misalignment between assessments and market values. The effect of racial demographics is still statistically and economically significant with the inclusion of these other controls; but more importantly, as a consequence of racial segregation in the U.S., exposure to neighborhood traits that generate assessment inequality (as a consequence of assessors failing to mirror the market's pricing of these

[11] Author's calculation using Zillow's zip-code ZHVI index for single family residences, computed January to January.

[12] For example, Oregon's Measure 50 establishes a Maximum Assessed Value that grows at 3% annually. This cap may not bind even with growth above 3%, if home prices have recently declined. Florida's Save Our Homes amendment to the state constitution caps assessment growth at the minimum of 3% or the CPI inflation rate. This policy applies only to properties designated as a homeowner's primary residence.

traits) is highly correlated with race.

Also for completeness, Table A7 shows the regression output underlying Figure 4 in the paper: the assessment gap estimated within deciles of county-level racial segregation.

Our test for racial differences in transaction prices (Table 4) necessarily relies on observing multiple sales, because we take an initial observed sale price, grow that sale price according to a local Home Price Index, and then measure whether race correlates with the difference between expected sale price and realized sale price in a subsequent transaction. Repeat sales are a distinct subset of the market, and may be a selected sample. Table A8 explores robustness on this margin. Our test of transaction prices is based on 2.1 million observations that also enter our core dataset. We can compare both balance and racial inequality between this subset ("test-sample"), and the other 4.9 million observations which are not used for the transaction price test because we don't observe a sufficient number of sales ("non-test sample"). Columns (1)–(4) estimate the assessment gap in each subsample. In the set of homes used for our transaction test, we find no evidence that minority sellers receive lower prices, thereby pushing inequality up (Table 4). If this pattern were reversed in the other sample, and all else remains the same, inequality would be higher as a matter of algebra. However, we find that inequality is actually lower in the non-test sample. This is not dispositive evidence. It is possible that Black sellers receive lower prices in the non-test sample – which would algebraically suggest inequality above 14.4% – but then some other unobserved difference between the two samples brings inequality back down to 11.7%. Our paper shows that one major factor driving inequality is racial demographics. Columns (5)–(6) show that the test-sample and non-test sample are evenly balanced on both Black and Hispanic share: homes in the test-sample are in regions with 1 percentage point lower Black or Hispanic share. We cannot directly test for racial differences in transaction prices within the non-test sample, but the results of Table A8 shows that the evidence we can examine does not strongly point to different racial transaction price dynamics between the test-sample and non-test sample.

Tables A9 and A10 explore the relationship between homeowner tenure and the assessment gap. The evidence on assessment appeals in Section 5.3.3, suggests that the assessment gap will increase in homeowner tenure. However, inequality arising through the neighborhood composition channel would not vary with homeowner tenure. Therefore, we would expect a large portion of the assessment gap to remain even while controlling for tenure. The data does not permit us to know the homeowner tenure for our entire sample: for about 40% of the sample, we pin down race and ethnicity using HMDA records from a refinancing transaction, and therefore do not observe original purchase (the transaction data in ATTOM becomes scarce prior to the late 1990s). For the remaining 60%, which represents just over 4 million transactions, we observe both the initial purchase and the subsequent sale which generates the assessment ratio. This permits us to observe tenure directly. Table A9 shows the results of augmenting our baseline specification with a control for homeowner tenure. The baseline assessment gap remains large and highly statistically significant. In this subsample of our full data, in fact, the assessment gap is approximately 3 percentage points greater than in the full sample. The estimate on tenure implies that the assessment gap increases by approximately 50bps per year. Table A10 relaxes the linearity assumption and estimates inequality across three tenure-bins: 1-5 years, 6-10 years, and >10 years. Estimates suggest inequality has an inverse U-shaped pattern with respect to tenure — rising by a total of approximately 2% from the short-tenure bin to the medium-tenure bin, but then decreasing in the longest-tenure bin. The decision to appeal is likely a function both of current (perceived) inequality and anticipated future time in the home. The non-linear dynamics suggested by Table A10 may reflect this complexity.

Finally, Table A11 builds on our analysis of within-neighborhood inequality in Section 5.3.3. The evidence from analyzing appeals within a single, large county shows a racial differential in appeals outcomes that will, over time, generate different assessment growth rates. White homeowners appeal with greater frequency and success, which will generate lower assessment growth relative to black or Hispanic homeowners. Absent other data on appeals, we cannot directly test the assessment appeals channel in other jurisdictions. We can, however, test whether the national data shows evidence of the patterns which this channel would generate. We exploit the time-series structure of assessments in the ATTOM dataset to ascertain whether assessment growth varies by homeowner race or ethnicity.

We will exploit the fact that for a large number of homes in our sample, the racial ownership changes pursuant to a transaction. We test for a racial differential in the trajectory of assessments over time, using a generalized difference in differences model:

$$a_{icjt} = \alpha_i + \gamma_{cjt} + \beta^r race_{icjt} + \epsilon_{icjt}. \tag{10}$$

In equation 10, a is the log assessment ratio, α_i is a property-level fixed effect, and γ_{cjt} is a jurisdiction-tract-year fixed effect, and *race* is the usual categorical variable. Each property in this sample is sold at some point. β^r is identified from properties which undergo a change in racial ownership as a consequence of the transaction. Property fixed effects absorb the between-home variation, and the geographic fixed effects absorb local housing market variation.

Table A11 shows the results. As homeowners typically can appeal their assessments each year, the channel we posit is most relevant to growth. Accordingly, columns (1) and (2) use the assessment growth (log differences) as the dependent variable. The coefficient in column (1) says that assessment growth is 7bps higher when a black person owns a property, relative to when a white person owns the same property. This is significant only at the 10% level. For black or Hispanic residents the difference in growth is 41bps, and is strongly statistically significant. Given that the assessment dataset spans only 13 years, and that an initial transaction is necessary to pin down the race and ethnicity of the homeowner (which further reduces the T-dimension of the usable sample), our estimating sample is large in the cross-section, but is on average fairly short in the T-dimension. This reduces the power of our estimation. Estimating growth rates exacerbates this challenge. In columns (3) and (4), we use (log) levels as the dependent variable instead. The level difference is 29bps and 79bps, respectively. This is consistent with the growth evidence. Within property, assessment levels are higher for minority residents. Given the length of our sample, the estimates in columns (3) and (4) should be thought of as reflecting two to three assessment cycles, which suggests reasonable consistency between the growth estimates and level estimates.

C.ii Pass-Through of Assessment Ratios to Tax Burden

As a matter of theory, any wedge between assessments and market prices must create a distortion in an ad valorem tax. We are able to observe taxes paid, and therefore can provide the empirical evidence showing that this theoretical relationship does, in fact, hold. Our central focus on assessment ratios is deliberate. Assessed values and market prices are observable by the econometrician with little ambiguity. Taxes are more complicated, chiefly due to exemptions.

Every state provides for a variety of tax exemptions in state legislative codes. Most localities have further autonomy to create exemptions. A common example would be a principal residence exemption: Michigan, for example, exempts primary homes from school taxation up to the amount of 18 mills (180bps).[13] Another very common exemption holds for residents of retirement age: New York State permits an exemption of up to 50% for residents over 65 whose income is between $3,000 and $29,000.[14] Within these parameters, local units have autonomy to select the precise cutpoints. While these are relatively straightforward, many exemptions are much more complicated. Even at a state level, the list of exemptions tends to be very long and complex. With tens of thousands of local authorities also potentially creating additional exemptions, even observing these exemptions becomes a significant challenge. While the ATTOM data includes a field for exemptions, it is unclear how consistently or accurately this data is reported. We show results: (i) using the reported gross tax bill directly, and (ii) removing the reported exemptions to create a post-exemption tax bill.

Exemptions matter in general because spatial distribution of the exemptions may very well be correlated with racial demographics. If some parts of Florida have more elderly white residents than young Black residents, a senior citizen exemption policy would create something that looks like a distortion in the tax burden, but which would be entirely consistent with the legislative intent and public administration of the tax system. We are unable to observe, and thus control for, age of the homeowner – let alone any other individual-level drivers of more complicated exemption policies. The strength of considering the assessment ratio is that none of these confounding factors matter. Using tax dollars paid, we are less able to rigorously strip out potential confounding factors.

Another complicating factor is partial-year tax bills. In some jurisdictions the homeowner of record on a certain date is liable for a full year's worth of property taxes. In others, a partial year of ownership would result in a tax bill spanning only that portion of the year. We do not observe this policy choice at a local level. To provide robustness around this issue, we will compute effective tax burden during the sale year, as well as one year before and one year after sale.

We first estimate the pass-through of the assessment ratio to the effective tax rate. We regress the log effective tax rate on the log assessment ratio. The mechanics of property tax administration

[13] Michigan Compiled Laws, Section 211.7cc and 380.1211.
[14] https://www.tax.ny.gov/pit/property/exemption/seniorexempt.htm.

would suggest a coefficient of 100%, unless homeowners have not fully exhausted available exemptions. If a region permits homeowners to deduct $5,000 from the assessed value of their primary residence before computing the tax bill and many homes are assessed at less than $5,000 then the pass-through would be less than 100%. Table A12 shows these estimated pass-through rates. Column (1) presents estimates for all homeowners in aggregate, and columns (2) and (3) show results by racial and ethnic grouping. Results for Black residents alone are very similar, and we do not include them here. Columns (1) and (2) use the gross (pre-exemption) tax bill. Column (3) uses the computed post-exemption tax bill. In all columns, estimates are very close to 1, as predicted. Across columns (2) and (3), differences by racial or ethnic identity are not evident.

Tables A13 – A15 extend the analysis of effective tax rates shown in Table 3. This is a robustness exercise to rule out bias arising from partial-year tax bills. We construct effective tax rates using tax bills from the year prior to sale and the year post-sale. The denominator remains the sale price of the home. Columns (1) and (4) show the estimated assessment gap in this reduced sub-sample (restricted to homes where tax bills and exemptions are observed in all three years): 13.7% for Black homeowners, and 10.1% for Black and Hispanic homeowners. For Black residents, we estimate an effective tax rate that is 15.9% higher in the actual tax bill and 15.4% higher before exemptions. Considering Black or Hispanic residents together, we find a 11.6% higher effective tax rate from tax bills and 11.3% increase before exemptions. Appendix Tables A14 and A15 show very similar patterns using tax bills one year on either side of the sale.

C.iii Formula-Driven Assessments Can Reduce Inequality

Having carefully documented the extent and magnitude of the distortion, it is natural to ask how easily the problem could be fixed. Perhaps it is the case that market prices are so sensitive to geographic variation and property prices so temporally unpredictable, that even the most skilled and attentive assessors office would not be able to equalize tax burdens by racial status. In this section, we show that a relatively simple approach can address a large portion of this inequality.

As more than half of the assessment gap relates to mispricing of local characteristics, we explore whether small-geography home price indexes (HPIs) can be used to reduce inequality. We use zip-code level HPIs to produce imputed assessments, and then compare the racial variation in assessment ratios obtained using our synthetic assessments to the variation obtained using true assessments. We find this simple procedure reduces inequality by 55–70%. The average zip code is about twice as large as a census tract. We conjecture that more geographically precise HPIs would be additionally effective in removing assessment ratio variation.

We use publicly available zip-code level HPIs from Zillow to construct assessments. Zillow constructs these HPIs monthly for 15,500 zip codes. This covers 84% of the U.S. population.[15] As some transaction density is needed for a sample size sufficient to produce a reasonable HPI index, these zip codes are highly skewed towards more populous urban areas. The monthly time-series

[15] Author's calculations using 2010 decennial census data.

from 1996 can be directly downloaded from Zillow's website at no cost. Zillow began providing these indexes in 2006 and has backwards constructed them to 1996. Zillow has also been increasing its coverage over time.

We construct synthetic assessments using the zip-code HPIs. The algorithm for a synthetic assessment is simple: in any zip code, we take the first observed transaction price and allow this to be the assessment in the month-year of sale. Then we grow that assessment according to the relevant monthly HPI. That is:

$$\hat{A}_{ijzt} = M_{ijz0} \frac{HPI_{ijzt}}{HPI_{ijz0}} \tag{11}$$

where 0 denotes the base month-year of the 1st transaction, z denotes zip code, and M_{ijz0} is the observed transaction price in the base year.

We next test the inequality which would be generated by using these synthetic assessments as the basis for property taxation. To do this, we apply the algorithm to carry the synthetic assessment forward in time until we arrive at the month-year of a subsequent transaction. We then form a synthetic assessment ratio at that time t by taking the log difference between our synthetic assessment and the observed transacted price: $\hat{ar}_{ijzt} = log(\hat{A}_{ijzt}) - log(M_{ijzt})$. We evaluate the success of this algorithm for generating assessments by comparing inequality in synthetic assessment ratios to inequality in the realized assessment ratios. Because this simple approach requires two transactions, and is by construction limited to the zip codes that Zillow covers, we end up with a significantly smaller subsample of 2.1M homes. We first document that the assessment gap still exists – and looks similar – in this subsample. Then we document that using synthetic assessments reduces inequality by 55–70%.

The first three columns of Table A16 show the assessment gap in the subsample covered by Zillow HPIs. Magnitudes are similar to our baseline findings. The figures in columns (1) and (2) are respectively 1.7% and 1.4% larger than the findings in our baseline sample. Columns (4) & (5) repeat the same regressions using our synthetic assessments. A perfect procedure would produce zeros on the racial and ethnic variables. The synthetic assessments completely reverse the assessment gap, and in fact overshoot. The estimates in columns (4) & (5) of Table A16 reflect a *lower* tax burden on minority residents. Of course, this is also an inequality in the tax burden. However, the overall distortion is much smaller in magnitude: 4.1% for Black homeowners and 5.1% for Black or Hispanic homeowners.

Two things are worth emphasizing here. One is that such a straightforward approach is only feasible if some valid HPI exists for small geographic regions. We use Zillow's zip-code HPIs to demonstrate that inequality can be reduced by using publicly available, easy to obtain data. Zip codes are, however, well known to be formed with little consideration for the institutions and characteristics of the underlying geography. Also, the average zip code contains 9,000 people. This is relatively large: our results suggest that there is meaningful spatial variation between tracts, which are less than half this size on average. We think this is likely to be one important reason that this simple implementation still generates a 4–5% racial difference in assessment ratios. The discussion in Section 5.2.1 also suggests that a racial or ethnic difference in transaction prices could explain 2–3 percentage points of the remaining inequality.

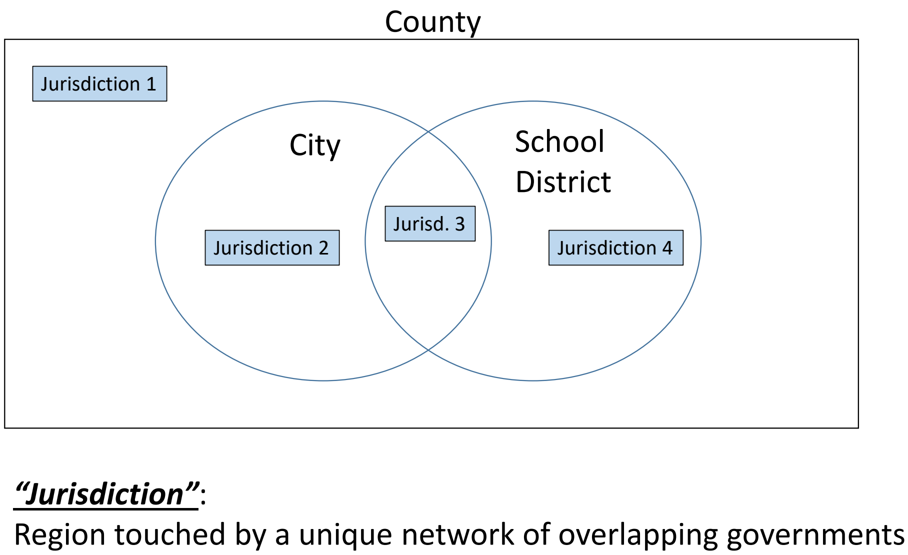
In addition, as a practical matter, assessment values need to be set at the beginning of the tax year, and sales may occur at any time during the next 12 months. Accordingly, racial sorting into areas of higher or lower growth would cause some amount of measured inequality in the realized assessment ratio to arise within the year. To see how important this channel would be, we reproduce a set of synthetic assessments where the assessment is set annually in January of each year. Every transaction then includes up to 12 months of home price growth which is not reflected in the assessment. Appendix Table A17 shows results from this exercise. The estimates are almost unchanged.

The second point of emphasis is that our procedure uses an observed transaction price for the base year value. In order to apply to all properties within a jurisdiction, assessors would need some method for imputing a base-year price for properties which have not sold at any point during the period spanned by the HPI index. Our neighborhood composition findings suggest that this will require assessors to permit prices to vary between small geographic regions. However, racial equity in the initial values is empirically observable and testable. So assessors should be able to iterate a model for initial pricing to land on an equitable distribution of base-year assessments, and then grow those by using some HPI index.[16] The point remains that assessors can make significant strides towards equity by linking assessment growth to small geographic regions within their jurisdiction.

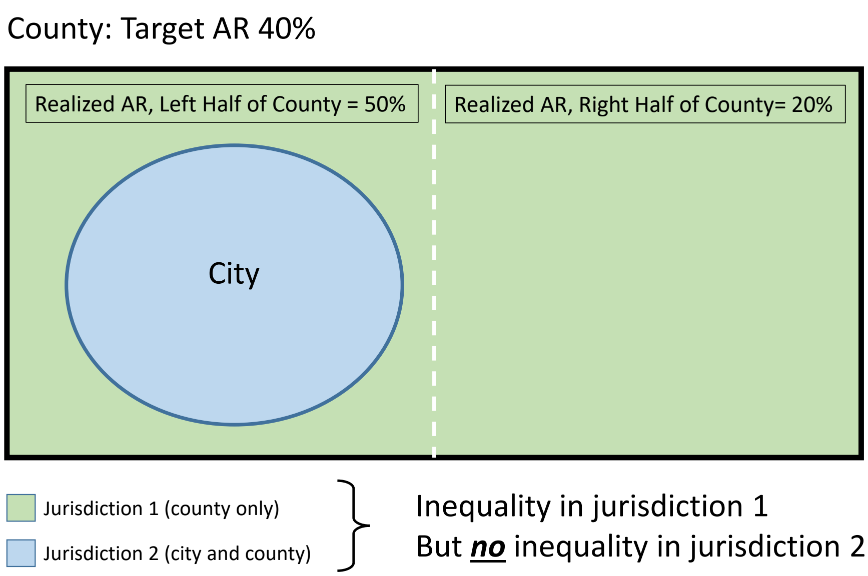
[16] This is, in fact, not particularly dissimilar from the process advocated by IAAO (2018) and other professional guides. However the bulk of this paper serves to show that regardless of process, the outcomes articulated in standards like these are not being widely achieved.

Figure A1: Taxing Jurisdiction Stylized Examples

Panel A

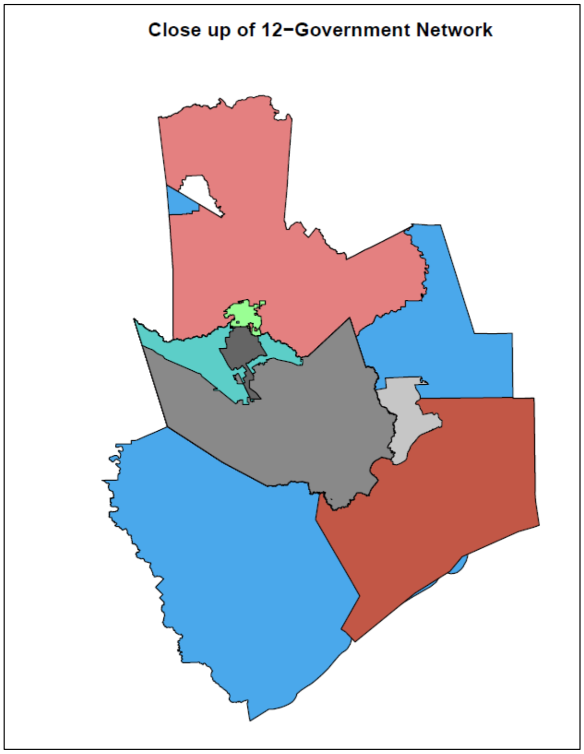


Panel B



Note: This figure shows two examples to illustrate how we form taxing jurisdictions. Panel A shows a stylized example with 3 governments: a county (the large rectangle) which fully contains a city and a school district. The latter two units of government are not spatially coincident. This spatial overlay generates 4 distinct jurisdictions. Panel B presents an example with two governments: the county is again the large rectangle, and a city is entirely contained within the left (blue) portion of the county. In this example, we assume that the county is targeting a 40% assessment ratio, but realizes 50% for every home in the blue region, and realizes 20% for every home in the green region.

Figure A2: 12-Government Network in Texas



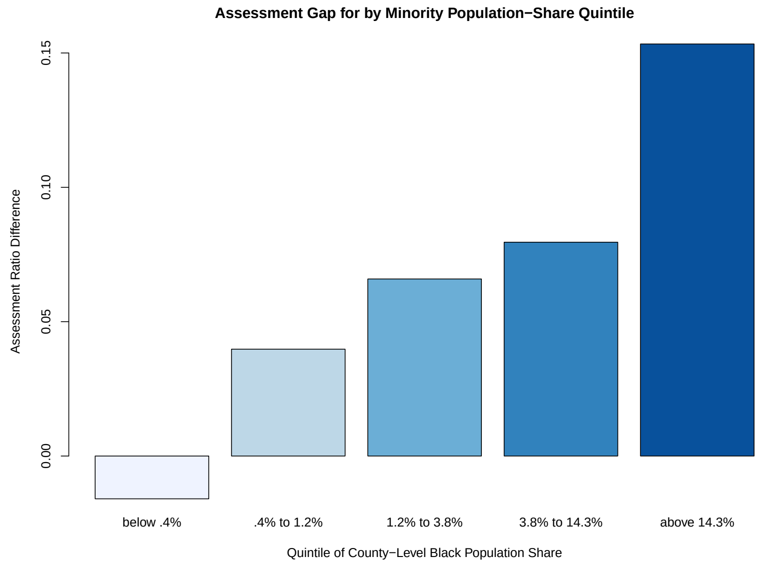
Harris County
City of Houston
Katy Independent School District
Houston Community Colleges
Harris County Flood Control
Port of Houston
Gulf Coast Waste Disposal
Coastal Water Authority
Willow Fork Drainage District
Cinco MUD
North Fort Bend Water Authority
Multi-County Economic Dev. Entity

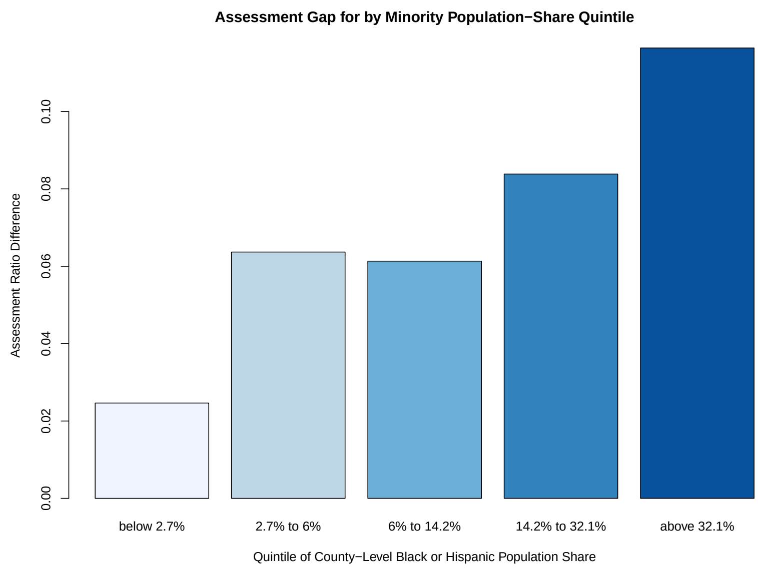
Twelve potential taxing units:

- Every unique combination of these overlapping entities is a jurisdiction
- *One example*: the intersection of all 12 entities

Note: This figure shows the spatial overlay of 12 different local government units in Texas. Some units are proper subsets, and thus fewer than 12 colors are evident in the figure at right. All 12 are listed at upper right. They include "standard" local governments: a county (Harris) and a city (Houston) plus two independent school districts. In addition, there are a range of entities which are related to municipal utilities or economic development initiatives. Each entity listed may, or may not, levy a property tax. Our empirical strategy generates no bias by including an entity as a taxing unit even if it does not, in fact, levy a tax in any particular year. Each unique overlapping combination of these units defines a taxing jurisdiction.

Figure A3: Sample Split by County-Level Minority Population Share





Note: These graphs show results from estimating the assessment gap in sub-samples by minority population share at the county level. We split the sample into quintiles by on average county black or black and Hispanic population share between 2005 to 2016. The quintile range is reflected below each bar. The regression output underlying this table is shown in Table A4.

Table A1: Inequality for all other minority homeowners

	log(Assessment Ratio)		
	(1)	(2)	(3)
Other Nonwhite Mortgage Holder	0.0278***	0.0198***	0.0190***
	(0.0016)	(0.0006)	(0.0007)
Fixed Effects	Jurisd-Yr	Tract-Yr	BG-Yr
No. Clusters	37723	37723	37723
Observations	6,987,915	6,987,915	6,987,915
R^2	0.8798	0.9005	0.9166
Note:			*p<0.1; **p<0.05; ***p<0.01

Note: This table complements Table 2 and shows our baseline findings of a racial assessment gap for all other minority homeowners. We regress the log assessment ratio on a set of fixed effects at the year × geography level and on categorical groupings by racial and ethnic identity. Columns (1), (2), and (3) show results using fixed effects at the jurisdiction-year, jurisdiction-tract-year, and jurisdiction-block group-year level, respectively. In all columns, the reference group is non-Hispanic white residents. Standard errors are clustered at the jurisdiction level.

Table A2: Assessment Ratio Differentials in California

	log(Assessment) - log(Market)		
	(1)	(2)	(3)
Black Mortgage Holder	0.0413***		
	(0.0101)		
Black or Hispanic Mortgage Holder		0.1060***	
		(0.0044)	
Other Nonwhite Mortgage Holder			0.0653***
			(0.0030)
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	5603	5603	5603
Observations	1,186,388	1,186,388	1,186,388
R^2	0.3816	0.3820	0.3820
Note:			*p<0.1; **p<0.05; ***p<0.01

Note: This table shows the results of our baseline assessment gap analysis for California alone. We regress the log assessment ratio on a jurisdiction-year fixed effect and on categorical groupings by racial and ethnic identity. In all columns, the reference group is non-Hispanic white residents, and for clarity coefficients for groups not being considered in a given column are not reported. The estimates in this table reflect an assessment ratio differential for the given grouping of minority residents relative to non-Hispanic white residents. Standard errors are clustered at the jurisdiction level.

Table A3: Assessment Gap, Using Counties instead of Taxing Jurisdictions

	log(Assessment Ratio)		
	(1)	(2)	(3)
Black Mortgage Holder	0.1687***		
	(0.0187)		
Black or Hispanic Mortgage Holder		0.1356***	
		(0.0138)	
Other Nonwhite Mortgage Holder			0.0321***
			(0.0024)
Fixed Effects	County-Year	County-Year	County-Year
No. Clusters	1982	1982	1982
Observations	6,987,915	6,987,915	6,987,915
R^2	0.8507	0.8508	0.8508
Note:		*p<0.1; **p<0.05; ***p<0.01	

Note: This table repeats our baseline assessment gap analysis, but uses county-year fixed effects rather than jurisdiction-year. We regress the log assessment ratio on a county-year fixed effect and on categorical groupings by racial and ethnic identity. In all columns, the reference group is non-Hispanic white residents, and for clarity coefficients for groups not being considered in a given column are not reported. The estimates in this table reflect an assessment ratio differential for the given grouping of minority residents, relative to non-Hispanic white residents. Standard errors are clustered at the county level. This specification shows that our results are not driven by the way we form jurisdictions. Our preferred specifications all use the more rigorous within-jurisdiction analysis.

Table A4: Sample Split by County-Level Minority Population Share

Panel A

	Assessment Value / Market Value				
	Quintile of County-Level Minority Population Share				
	(1)	(2)	(3)	(4)	(5)
Black Mortgage Holder	−0.016	0.040***	0.066***	0.080***	0.153***
	(0.054)	(0.007)	(0.004)	(0.006)	(0.021)
Fixed Effects	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr
No. Clusters	2087	6718	9619	12876	6445
Observations	54,188	412,164	919,591	3,129,016	2,472,956
R^2	0.857	0.938	0.905	0.888	0.847
Note:					*p<0.1; **p<0.05; ***p<0.01

Panel B

	Assessment Value / Market Value				
	Quintile of County-Level Minority Population Share				
	(1)	(2)	(3)	(4)	(5)
Black or Hispanic Mortgage Holder	0.025*	0.064***	0.061***	0.084***	0.116***
	(0.013)	(0.006)	(0.003)	(0.006)	(0.018)
Fixed Effects	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr
No. Clusters	3452	6097	11116	12122	4969
Observations	78,526	303,353	1,443,303	2,803,100	2,359,633
R^2	0.816	0.784	0.860	0.879	0.881
Note:					*p<0.1; **p<0.05; ***p<0.01

Note: Each panel shows the results from estimating the assessment gap on sub-samples based on county-level demographics. For Panel A, we split our baseline sample into quintiles by average county black population share. In Panel B the sample is split by black or Hispanic population share. In each panel, column 1 shows the estimated assessment gap within the lowest minority-population quintile, and column 5 shows results for the highest quintile. Regressions are run separately rather than pooled. We include jurisdiction-year fixed effects in all specifications. Standard errors are clustered at the jurisdiction level.

Table A5: Hedonic Prices

	Market	Assessment	Market	Assessment
	(1)	(2)	(3)	(4)
Black Share	−0.092***	−0.056***		
	(0.004)	(0.004)		
Black or Hispanic Share			−0.117***	−0.078***
			(0.006)	(0.005)
Median HH Income	0.157***	0.144***	0.145***	0.135***
	(0.008)	(0.008)	(0.008)	(0.008)
Unemployment	−0.027***	−0.013***	−0.030***	−0.015***
	(0.003)	(0.002)	(0.004)	(0.002)
SNAP Share	−0.089***	−0.061***	−0.075***	−0.050***
	(0.006)	(0.004)	(0.006)	(0.004)
Owner Share	−0.049***	−0.032***	−0.053***	−0.035***
	(0.005)	(0.003)	(0.005)	(0.004)
GINI	0.066***	0.059***	0.058***	0.053***
	(0.004)	(0.004)	(0.004)	(0.004)
Square Feet	0.256***	0.264***	0.256***	0.264***
	(0.029)	(0.030)	(0.029)	(0.030)
Bathrooms	0.107***	0.103***	0.107***	0.103***
	(0.017)	(0.017)	(0.017)	(0.017)
Year Built	0.031***	0.028***	0.030***	0.028***
	(0.003)	(0.003)	(0.003)	(0.003)
Other Attributes	Y	Y	Y	Y
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	26152	26152	26152	26152
Observations	4,877,658	4,877,658	4,877,658	4,877,658
R^2	0.773	0.942	0.773	0.942
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table reports estimated hedonic prices from two separate hedonic models. The first model uses (log) market as the dependent variable. These estimates are reported in columns 1 and 3. The second model uses (log) assessed values as the dependent variable. These estimates are reported in columns 2 and 4. Otherwise, the two hedonic models are identical: all regressors are the same. The table omits estimated coefficients for indicator variables stating whether a property has a patio, pool, or fireplace. Standard errors are clustered at the jurisdiction level. Figure **??** shows the difference between attribute-coefficients graphically.

Table A6: All Neighborhood Correlates

	log(Assessment Ratio)	
	(1)	(2)
Black Mortgage Holder	0.077***	
	(0.003)	
Black Share	0.027***	
	(0.005)	
Black or Hispanic Mortgage Holder		0.065***
		(0.003)
Black or Hispanic Share		0.035***
		(0.006)
Median HH Income	−0.021***	−0.015***
	(0.005)	(0.004)
Unemployment	0.015***	0.017***
	(0.004)	(0.004)
SNAP Assistance	0.033***	0.030***
	(0.004)	(0.003)
Owner Percentage	0.021***	0.020***
	(0.004)	(0.004)
GINI Coef	−0.011***	−0.009***
	(0.002)	(0.002)
Median Age	0.003*	0.008***
	(0.002)	(0.003)
Fixed Effects	Jurisd-Year	Jurisd-Year
No. Clusters	37679	37679
Observations	6,944,439	6,944,439
R^2	0.881	0.881
Note:	*p<0.1; **p<0.05; ***p<0.01	

Note: This table augments our baseline assessment gap findings in Table 2 with several measures of spatial characteristics. All regressors are tract-level variables from the American Community Survey 5-year estimates. Standard errors are clustered at the jurisdiction level. We continue to hold homeowner race fixed in this regression: those coefficients are reported in the first line of notes immediately under the estimated coefficients. Standard errors are clustered at the jurisdiction level.

Table A7: Assessment Gap by Segregation Decile

Panel A: Black Homeowners

	log(Assessment Ratio)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black Mortgage Holder	0.0585***	0.0650***	0.0492***	0.0668***	0.0709***	0.0757***	0.0950***	0.0973***	0.1248***	0.1904***
	(0.0140)	(0.0063)	(0.0083)	(0.0051)	(0.0076)	(0.0069)	(0.0176)	(0.0101)	(0.0103)	(0.0371)
Fixed Effects	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr
No. Clusters	418	1265	2036	3517	3454	4348	4341	6096	5875	6348
Observations	28,109	124,642	254,298	466,978	632,892	911,707	698,160	946,883	1,252,737	1,670,456
R^2	0.9246	0.8592	0.9008	0.9093	0.8849	0.9443	0.8785	0.8268	0.8603	0.8233

Note: *p<0.1; **p<0.05; ***p<0.01

Panel B: Black or Hispanic Homeowners

	log(Assessment Ratio)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black or Hispanic Mortgage Holder	0.0718***	0.0464***	0.0518***	0.0503***	0.0461***	0.0565***	0.0531***	0.0586***	0.0838***	0.1509***
	(0.0242)	(0.0057)	(0.0038)	(0.0033)	(0.0047)	(0.0043)	(0.0033)	(0.0054)	(0.0062)	(0.0242)
Fixed Effects	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr	Jur-Yr
No. Clusters	359	1393	2489	2513	3556	3125	3805	5329	6318	8811
Observations	11,241	66,821	210,686	239,072	376,861	329,217	595,845	1,166,829	1,672,469	2,317,821
R^2	0.9146	0.8489	0.9311	0.8870	0.8859	0.8956	0.9226	0.8598	0.9054	0.8332

Note: *p<0.1; **p<0.05; ***p<0.01

Note: This table provides point estimates for Figure 4 on the paper.

Table A8: Robustness for Test of Transaction Prices

	Assessment Gap				Black Share	B/H Share
	(1)	(2)	(3)	(4)	(5)	(6)
Black Seller	0.144***	0.117***				
	(0.015)	(0.015)				
Black or Hispanic Seller			0.110***	0.089***		
			(0.011)	(0.011)		
Test Sample					−0.011***	−0.011***
					(0.002)	(0.002)
Sample	Test	Not Test	Test	Not Test	Full	Full
Fixed Effects	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr	Jurisd-Yr
No. Clusters	18854	37193	18854	37193	37723	37723
Observations	2,135,966	4,851,949	2,135,966	4,851,949	6,987,915	6,987,915
R^2	0.910	0.870	0.910	0.870	0.618	0.686
Note:						*p<0.1; **p<0.05; ***p<0.01

Note: This figure splits our core dataset to compare the assessment gap within the sample of homes used for our test of racial differences in transaction prices (columns 1 and 3), and within the set which does not enter this test (columns 2 and 4). Columns 5 and 6 regress Black share and Black or Hispanic share respectively on an indicator for whether the observation is used in the test of transaction prices. All specifications include jurisdiction-year fixed effects and standard errors are clustered at the jurisdiction level.

Table A9: Assessment Gap by Homeowner Tenure (Continuous)

	log(Assessment) - log(Market)	
	(1)	(2)
Black Mortgage Holder	0.1533***	
	(0.0180)	
Black or Hispanic Mortgage Holder		0.1173***
		(0.0120)
Years Since Sale	0.0049***	0.0052***
	(0.0003)	(0.0003)
Fixed Effects	Jurisd-Year	Jurisd-Year
No. Clusters	32705	32705
Observations	4,216,379	4,216,379
R^2	0.8939	0.8939
Note:	*p<0.1; **p<0.05; ***p<0.01	

Note: This table estimates the assessment gap with a continuous control for homeowner tenure (years since purchase). Standard errors are clustered at the jurisdiction level.

Table A10: Assessment Gap Homeowner Tenure Bin

Panel A: Black Homeowners

	log(Assessment) - log(Market)		
	1-5 Years	6-10 Years	10+ Years
Black Mortgage Holder	0.1435***	0.1630***	0.1368***
	(0.0189)	(0.0188)	(0.0162)
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	28682	27330	14874
Observations	2,313,454	1,546,116	356,809
R^2	0.9038	0.8866	0.9012
Note:	*p<0.1; **p<0.05; ***p<0.01		

Panel B: Black or Hispanic Homeowners

	log(Assessment) - log(Market)		
	1-5 Years	6-10 Years	10+ Years
Black or Hispanic Mortgage Holder	0.1087***	0.1237***	0.0933***
	(0.0122)	(0.0133)	(0.0106)
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	28682	27330	14874
Observations	2,313,454	1,546,116	356,809
R^2	0.9037	0.8866	0.9011
Note:	*p<0.1; **p<0.05; ***p<0.01		

Note: This figure estimates the assessment gap by three homeowner tenure bins: 1-5 years, 6-10 years and greater than 10 years. Regressions are run separately, rather than pooled. Standard errors are clustered at the jurisdiction level.

Table A11: Effect of Black or Hispanic Ownership on Assessments

	Assessments			
	Growth		Levels	
	(1)	(2)	(3)	(4)
Black Mortgage Holder	0.0711*		0.2917***	
	(0.0386)		(0.0415)	
Black or Hispanic Mortgage Holder		0.4103***		0.7923***
		(0.0255)		(0.0274)
Fixed Effects	Two-Way	Two-Way	Two-Way	Two-Way
No. Clusters	12268641	12268641	12268641	12268641
Observations	54,970,191	54,970,191	54,970,191	54,970,191
R^2	0.6925	0.6925	0.9910	0.9910
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table shows the results of a generalized difference-in-differences estimation. The dependent variable is logged assessment value. Every home in this sample is transacted at least once. Fixed effects are two-way: property and tract-year. In columns 1 and 2, the dependent variable is growth rates (log difference in assessed value). In columns 3 and 4, the dependent variable is the logged assessment. Standard errors are clustered at the property level.

Table A12: Assessment Ratio Pass Through to Tax Bill

	Effective Tax Rate - Year of Sale (%)		
	Before Exemptions	Before Exemptions	Tax Bill
	(1)	(2)	(3)
All Mortgage Holders	0.9842***		
	(0.0042)		
White Mortgage Holder		0.9858***	0.9941***
		(0.0038)	(0.0041)
Black or Hispanic Mortgage Holder		0.9773***	0.9836***
		(0.0067)	(0.0069)
Other Nonwhite Mortgage Holder		0.9823***	0.9892***
		(0.0042)	(0.0043)
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	34776	34776	34776
Observations	5,574,777	5,574,777	5,574,777
R^2	0.9096	0.9097	0.8658
Note:			*p<0.1; **p<0.05; ***p<0.01

Note: This table shows the results of regressing log effective tax rate on log assessment ratio. Column 1 presents estimates for all homeowners. Columns 2 and 3 show a breakdown by racial and ethnic grouping. Results for black homeowners alone are very similar to those reported here. In columns 1 and 2, the dependent variable is an effective rate formed using the gross (pre-exemption) tax bill reported in the ATTOM dataset. Column 3 computes a post-exemption effective rate by subtracting reported exemptions from the reported tax bill. The effective rate is computed by using the tax bill reported in the same year as the sale. All specifications use jurisdiction-year fixed effects. Standard errors are clustered at the jurisdiction level.

Table A13: Effective Tax Rate, Sale Year

	Effective Tax Rate - In Sale Year (%)					
	Assmt. Gap	Before Exemptions	Tax Bill	Assmt. Gap	Before Exemptions	Tax Bill
	(1)	(2)	(3)	(4)	(5)	(6)
Black Mortgage Holder	13.6796***	15.3594***	15.8591***			
	(2.0953)	(2.1055)	(2.1254)			
Black or Hispanic Mortgage Holder				10.1349***	11.2948***	11.6403***
				(1.5904)	(1.5689)	(1.5320)
Jurisd-Year FE	Y	Y	Y	Y	Y	Y
No. Clusters	25267	25267	25267	25267	25267	25267
Observations	3,027,748	3,027,748	3,027,748	3,027,748	3,027,748	3,027,748
R^2	0.8956	0.6961	0.6488	0.8955	0.6958	0.6484
Note:						*p<0.1; **p<0.05; ***p<0.01

Note: This table repeats our baseline estimation, but uses effective tax rate as the dependent variable instead of assessment ratio. Coefficients are percentages. For each racial and ethnic grouping, we present two sets of results. In odd columns, we show results using an effective rate computed using the gross (pre-exemption) tax bill and observed market value in the same year. In even columns, we compute a post-exemption effective tax rate, by subtracting reported exemptions from the gross tax bill, and then dividing by market value. We trim any observation above a calculated effective tax rate of 25% both before and net of exemptions. We believe this to be a conservative choice as 25% is far higher than any property tax rate of which we are aware (the national median is approximately 1.4%), and is more likely than not to be a data error. All specifications use jurisdiction-year fixed effects to hold constant the level of intended taxation. Standard errors are clustered at the jurisdiction level.

Table A14: Effective Tax Rate, One Year Before Sale

	Effective Tax Rate - One Year Before Sale (%)			
	Before Exemptions	Tax Bill	Before Exemptions	Tax Bill
	(1)	(2)	(3)	(4)
Black Mortgage Holder	15.8285***	16.5085***		
	(2.2103)	(2.2371)		
Black or Hispanic Mortgage Holder			11.6723***	12.2055***
			(1.6216)	(1.6311)
Jurisd-Year FE	Y	Y	Y	Y
No. Clusters	25267	25267	25267	25267
Observations	3,027,748	3,027,748	3,027,748	3,027,748
R^2	0.6798	0.6324	0.6795	0.6321
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table repeats our analysis in Table A13, but uses the tax bill from the year before sale. The denominator for computing the effective tax rate remains the observed market value. Coefficients are percentages. For each racial and ethnic grouping we present two sets of results.In odd columns, we show results using an effective rate computed using the gross (pre-exemption) tax bill and observed market value in the same year. In even columns, we compute a post-exemption effective tax rate, by subtracting reported exemptions from the gross tax bill, and then dividing by market value. We trim any observation above a calculated effective tax rate of 25% both before and net of exemptions. We believe this to be a conservative choice as 25% is far higher than any property tax rate of which we are aware (the national median is approximately 1.4%), and is more likely than not to be a data error. All specifications use jurisdiction-year fixed effects to hold constant the level of intended taxation. Standard errors are clustered at the jurisdiction level.

Table A15: Effective Tax Rate, One Year After Sale

	Effective Tax Rate - One Year After Sale (%)			
	Before Exemptions	Tax Bill	Before Exemptions	Tax Bill
	(1)	(2)	(3)	(4)
Black Mortgage Holder	13.6175***	13.9837***		
	(1.9898)	(1.9776)		
Black or Hispanic Mortgage Holder			9.9325***	10.1185***
			(1.4818)	(1.4179)
Jurisd-Year FE	Y	Y	Y	Y
No. Clusters	25267	25267	25267	25267
Observations	3,027,748	3,027,748	3,027,748	3,027,748
R^2	0.7155	0.6599	0.7152	0.6595
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table repeats our analysis in Table A13, but uses the tax bill from the year after the sale. The denominator for computing the effective tax rate remains the observed market value. Coefficients are percentages. For each racial and ethnic grouping we present two sets of results. In odd columns, we show results using an effective rate computed using the gross (pre-exemption) tax bill and observed market value in the same year. In even columns, we compute a post-exemption effective tax rate, by subtracting reported exemptions from the gross tax bill, and then dividing by market value. We trim any observation above a calculated effective tax rate of 25% both before and net of exemptions. We believe this to be a conservative choice as 25% is far higher than any property tax rate of which we are aware (the national median is approximately 1.4%), and is more likely than not to be a data error. All specifications use jurisdiction-year fixed effects to hold constant the level of intended taxation. Standard errors are clustered at the jurisdiction level.

Table A16: Synthetic Assessments Using Zip Code HPIs

	log(Assessment) - log(Market)			
	Real Assessments		Synthetic Assessments	
	(1)	(2)	(3)	(4)
Black Mortgage Holder	0.144***		−0.041***	
	(0.015)		(0.003)	
Black or Hispanic Mortgage Holder		0.110***		−0.051***
		(0.011)		(0.003)
Jurisd-Year FE	Y	Y	Y	Y
No. Clusters	18853	18853	18853	18853
Observations	2,135,943	2,135,943	2,135,943	2,135,943
R^2	0.910	0.910	0.712	0.713
Note:		*p<0.1; **p<0.05; ***p<0.01		

Note: This table shows the results from our proposed approach for correcting the assessment gap. Using the algorithm described in Section **??**, we construct synthetic assessments using zip-code-level HPIs. We use Zillow's publicly available ZHVI series by zip-code. Our approach uses an initial transaction to pin down the base assessment value. At every subsequent transaction, we observe a realized assessment ratio along with our synthetically constructed assessment ratio. Columns 1 & 2 show that the overall assessment gap looks similar in the subset of homes to which can we apply this approach (smaller chiefly because the first transaction is not included in the analysis). Columns 3 & 4 show the assessment gap using our synthetic assessment ratios. All specifications include jurisdiction-year fixed effects. Standard errors are clustered at the jurisdiction level.

Table A17: Synthetic Assessments, Stopping Growth in January Each Year

	log(Assessment) - log(Market)			
	Real Assessments		Synthetic Assessments	
	(1)	(2)	(3)	(4)
Black Mortgage Holder	0.144***		−0.040***	
	(0.015)		(0.003)	
Black or Hispanic Mortgage Holder		0.110***		−0.049***
		(0.011)		(0.003)
Jurisd-Year FE	Y	Y	Y	Y
No. Clusters	18853	18853	18853	18853
Observations	2,135,943	2,135,943	2,135,943	2,135,943
R^2	0.910	0.910	0.692	0.693
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table shows an alternative implementation of our proposed approach for correcting the assessment gap. The analysis in Table A16 uses constructed assessments which increase with the zip-code HPI until the month of sale. In this table, we use constructed assessments which change only in January of each year. This more closely parallels the actual assessment practice of generating a single value each year. In this approach, when a sale occurs, the assessment is out of date by up to 12 months. Columns 1 & 2 are identical to Table A16 and show that the overall assessment gap looks similar in the subset of homes to which we can apply this approach. Columns 3 & 4 show the assessment gap using January-revised synthetic assessments. All specifications include jurisdiction-year fixed effects. Standard errors are clustered at the jurisdiction level.

Table A18: Effect of Assessment Caps on Inequality

	log(Assessment Ratio)		
	No Cap	Cap Exists	Cap Exists and Binds
	(1)	(2)	(3)
Panel A: Black Homeowners			
Black Mortgage Holder	0.1384***	0.1366***	0.0860***
	(0.0115)	(0.0373)	(0.0089)
Panel B: Black or Hispanic Homeowners			
Black or Hispanic Mortgage Holder	0.1065***	0.1080***	0.0607***
	(0.0084)	(0.0230)	(0.0052)
Fixed Effects	Jurisd-Year	Jurisd-Year	Jurisd-Year
No. Clusters	28589	9374	4492
Observations	4,025,841	2,295,890	509,245
Note:			*p<0.1; **p<0.05; ***p<0.01

Note: This table shows our findings of a racial assessment gap in areas with different policies regarding a cap rate of growth. Panel A presents our results for Black homeowners, and Panel B presents our results for Black or Hispanic homeowners. In all specifications, we regress the log assessment ratio on jurisdiction-year fixed effects and on categorical groupings by racial and ethnic identity. Column (1), (2), and (3) respectively present results for areas with no known cap policy, areas with a cap, and areas with a cap that is binding. In all columns, the reference group is non-Hispanic white residents, and for clarity coefficients for groups not being considered in a given column are not reported. The estimates in this table reflect an assessment ratio differential for the given grouping of minority residents relative to non-Hispanic white residents. Standard errors are clustered at the jurisdiction level.