Is There Discrimination in Property Taxation?

Evidence from Atlanta, Georgia, 2010-2016

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Abstract: In the past, some localities have taxed blacks at higher real rates than whites by over-assessing property values in predominately blacks neighborhoods while taxing those properties at the same nominal rates. In 1974, the NAACP sued Fulton County, Georgia – the principal county of the Atlanta metro area – over this very issue. In 1991, a mass reappraisal intended to remedy this discrimination incited a tax revolt in Fulton. However, there are few recent studies of whether discrimination is still taking place. Using assessment data from Fulton County 2010-2016, I find little to no evidence of any racial or socioeconomic discrimination in ratios of property assessment to sale prices. This suggests that (1) the assessment process is uniform and non-discriminatory, and/or (2) the process and fee for appealing one's assessment is not inaccessible to a degree that would allow any disparities to persist.

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I. Introduction

In the past, some local governments have taxed properties in predominately black and/or low-income neighborhoods at higher real rates than properties in predominately white and/or high-income neighborhoods (Rothstein 2017: 169-172, Lyons 1982, Baar 1981). Black and/or low-income residents would be taxed at the same nominal property tax rate as white and/or high-income residents, but property values in the former neighborhoods would be over-assessed, causing their real property tax rate to be higher. For example, in Fort Worth, Hendon (1968, table 3) found that assessment ratios – i.e. the ratios of assessed value to sale price – was between 0.93 and 1.03 for blacks but between 0.49 and 0.74 for whites. In Boston in 1960, Black (1972, table 2) found that homes in the neighborhoods with the greatest non-white percentage were assessed at a ratio of 0.64 while the rest of the city was assessed at a ratio of 0.37. In New Haven, Lee (2004, table 6) found that single-family homes in majority-black neighborhoods were assessed at a ratio of 1.2494 while single-family homes in majority-white neighborhoods were assessed at a ratio of 0.775. Thus, discriminatory property taxation was not a matter of a few percentage points or a few dollars. Black residents were sometimes taxed at twice the effective property tax rate as whites were, even though the same nominal rate applied to everyone.[1]

Over- and under-assessment are not problematic as long as everyone is equally mis-assessed. This is because the property tax rate is set to raise some budgeted revenue. For example, if everyone's property values are assessed at double their true values, then everyone's nominal tax rate will be cut in half and the same revenue will be raised. However, if different properties are over- or under-assessed differently, then the tax revenue will be disproportionately raised from some people more than others, notwithstanding the equal nominal tax rate. According to Rothstein (2017: 172), this violates the Fourteenth Amendment's due process clause. Atuahene and Hodge (2018: 264) consider discriminatory assessment a form of "stategraft," which they define as "when state agents transfer property from residents to the state in violation of the state's own laws and to the detriment of a vulnerable group."[2]

However, there few studies of this issue which use recent data. Faulk and Hicks (2015) - studying the entire state of Indiana over the period 2005-2012 – find that low-valued properties are over-assessed while high-valued properties are

1 Locations where property taxation was found to be discriminatory include – but are not limited to – Boston (Townsend 1951, Oldman and Aaron 1965, Black 1972, Engle 1975), 1970s Chicago (Little 1973, Bremer et. al. 1979, Capps 2015, Kahrl 2018), Baltimore and Philadelphia (Little 1973), Detroit (Atuahene 2018, Atuahene and Hodge 2018), New Haven (Lee 2004), Jim Crow-era Mississippi (Kahrl 2016), Fort Worth (Hendon 1968), Buffalo (Haettenschwiller 1973), Norfolk (Pearson 1979), and 1970s-1990s Atlanta (Atlanta Urban League 1971, Price 1975, Holmes and Pinner 1975, Connor 2018, Kahrl 2018).

2 Atuahene and Hodge (2018: 295) say that stategraft is similar to corruption except that the beneficiary is not a corrupt official, but the state or an agency thereof. For example, they note that civil asset forfeiture is used against those not convicted of any crime to enrich the police department as a whole, not any individual police officer (*ibid.* p. 297).

under-assessed. They also find that non-homestead properties – which are more likely to be rental properties or second homes – are over-assessed relative to homestead properties. McMillen and Singh (2019) – studying Baltimore, Cleveland, Denver, and Philadelphia – find that property taxes are regressive with respect to both sale price and income. However, Lee (2004) and Atuahene (2018) are the only recent quantitative papers which explicitly study the racial aspect of discriminatory property taxation. Every other quantitative study of the relationship between race and property assessment predates the literature reviews by Baar (1981) and Lyons (1982). Rothstein (2017: 172) concludes, “There are no contemporary studies of assessed-to-market value ratios by community and by race, so we cannot say whether discriminatory tax assessments persist to the present time, and if so, in which communities.”

In this paper, I use data from Fulton County – the principal county of Atlanta, Georgia – to test whether there is any contemporary evidence of racial or socioeconomic discrimination in housing assessment. Fulton has its own history of inequitable assessments, making it a suitable location to study (Simmonds 1991). In 1971, an Atlanta Urban League study found discriminatory assessment-to-sale ratios due to assessment lag (Connor 2018: 986). In 1973, the Georgia Revenue Commissioner ordered Fulton County to factor its tax rolls to bring them into compliance with a 40% assessment-to-sale standard (Connor 2018: 987).[3] The following year, the Atlanta chapter of the NAACP sued the Joint Fulton County Board of Tax Assessors in the Superior Court of Fulton County for the over-assessment of properties in black neighborhoods of Atlanta (Price 1975 cited by Kahrl 2018: 389). A 1975 study by Holmes and Pinner found that lower-priced properties and properties in inferior physical condition were assessed at disproportionately higher rates (Connor 2018: 1000 n. 7). Finally, in 1988, the Georgia state General Assembly passed House Bill (HB) 1279 which gave local governments until 1991 to bring their appraisals into compliance, or else they would face fines and penalties (Connor 2018: 987). In 1991, Fulton County initiated a mass reappraisal which incited a tax revolt. Opponents of the reappraisal claimed that the reappraisal was intended to redistribute wealth from whites to blacks and encourage wasteful government over-spending (Connor 2018). A 2007 study of Fulton County’s property assessment procedures by Almy, Gloudemans, Jacobs, and Denne found significant variation in assessment ratios and recommended a variety of technical improvements and changes, such as replacing their CAMA (computer-assisted mass appraisal) system with a newer CAMA system from IAS, and supplementing the CAMA system with more generalized regression analysis using SPSS or SAS. Thus, Fulton County is a suitable location to study whether discriminatory assessment persists.

3 Georgia state law requires taxable assessments to be 40% of the fair market value – where fair market value is equal to the 100% appraised value.

In theory, regression-based mass appraisal techniques could be sufficient to eliminate discrimination and other disparities. Most US counties and states modernized their property tax systems in the 1960s and 1970s, sparking the tax revolts of the 1970s and 1980s (Martin 2008). Customarily, many residential properties had been under-assessed – relative to commercial properties – by corrupt assessors in return for bribes and votes.[4] Modernization included centralization, systematization, professionalization, and computerization (Martin 2008, Baar 1981: 352ff.). Modernized assessment caused homes to be assessed at closer to their true market values, resulting in unexpectedly high tax bills for many – and thereby sparking tax revolts (Martin 2008). But since the early 1980s, there has been little research on whether modernized assessment has indeed reduced the prevalence of discriminatory assessment. Baar (1981: 394ff.) observes that discriminatory assessment in Chicago survived the advent of modern, regression-based methods. Using relatively recent data, several authors find discrimination by race (Lee 2004, Atuahene 2018) and by sale price (Atuahene and Hodge 2018, Faulk and Hicks 2015, McMillen and Singh 2019), suggesting that modern appraisal techniques are not necessarily sufficient to eliminate disparities.

Lee (2004: 33-39) summarizes why racially discriminatory assessment may exist: (1) to favor curry favor with influential voting constituencies – especially the whiter and wealthier – who are more likely to challenge assessments and property tax increases,[5] (2) because there is a greater incentive to appeal the assessment for high-valued property, (3) because of assessment lag – meaning that infrequent assessments benefit appreciating properties whose assessments are not updated,[6] and hypothetically (4) to reduce white flight from the city to the suburbs.

This study is related to ongoing, recent research exploring how blacks and members of other socioeconomic groups may be subject to discrimination or disadvantages in the housing market. Several authors have cataloged the myriad ways in which local, state, and federal government have encouraged or even mandated segregated housing over the course of many decades (Rothstein 2017, Armborst *et. al.* 2017, Trounstine 2018). Perry *et. al.* (2018) find that in a sample of U.S.

4 Martin (2008: 9) calls the under-assessment of residential properties relative to commercial properties "among the largest categories of social expenditure of any kind," within a degree of magnitude of the expenditure on Social Security.

5 Regarding political pressures which make officials reluctant to update assessments, cf. Baar (1981: 341).

6 Kahrl (2018: 388) argues that assessment lag was deliberately intended to benefit whites in Chicago. Thus, the desire to curry favor with influential constituencies may be a cause of assessment lag. On assessment lag, cf. Connor (2018: 986), Kahrl (2018: 388), Simmonds (1991: 162), Baar (1981: 334, 341), Haettenschwiller (1973: 431f.), and Townsend (1951: 362). Engle (1975) uses time-series regression to measure assessment lag as the elasticity of assessment value with respect to sale price. Pearson (1979: 200) rejects assessment lag as an explanation for Norfolk and instead argues that the replacement-cost method of assessment used had failed to account for changes in demand conditions and neighborhood effects in appreciating neighborhoods. Therefore, we should be skeptical of Lee's (2004: 4) proposal to replace the fair market value approach with a historical purchase-cost approach. Lee is correct that his historical purchase-cost approach would benefit individuals on low or fixed-incomes who suffer from illiquidity when their property assessments increase. But it would do so at the cost of discriminating against those with depreciating properties, in favor of those with appreciating properties.

metropolitan areas, homes in neighborhoods with more than 50% black population sell at half the price of similar homes in neighborhoods with no black residents, controlling for other neighborhood characteristics. Others find that black individuals pay higher prices than whites for identical houses within the same neighborhoods (Bayer *et. al.* 2017, Ihlanfeldt and Mayock 2009, Myers 2004). Audit and correspondence studies consistently find that landlords respond more positively to emails when the potential renter has a “white” name rather than a “black” or “Arab” name (Öblom and Antfolk 2017, Ewens *et. al.* 2014, Hanson and Hawley 2011, Carpusor and Loges 2006). Minorities list lower rental prices on Airbnb (Edelman and Luca 2014, Wang *et. al.* 2015, Kakar *et. al.* 2018, Marchenko 2019) and minorities are less likely to be accepted by Airbnb hosts (Edelman *et. al.* 2017). However, the acceptance rates for white and minority Airbnb guests becomes statistically indistinguishable when the guest has at least one review, suggesting the discrimination is statistical, not taste-based (Cui *et. al*. 2016).

As we shall see, I find no evidence of racially or socioeconomically discriminatory property assessment in Fulton County / Atlanta, Georgia. This suggests that assessments are uniform and/or that the appeals process is not so inaccessible that any disparities are able to persist. It is important to recognize, of course, that absence of evidence is not evidence of absence. I cannot state that there is no racial or socioeconomic discrimination in Fulton County’s assessment processor. All I can say is that I could not find any evidence of such discrimination. Nevertheless, it is encouraging to find absence of evidence for a historically pervasive form of discrimination. Future research should study other cities as well so that we may determine whether absence of discriminatory assessment is the exception or the rule.

In fact, I find that homes in black neighborhoods of Fulton County are consistently *under*-assessed. This may merely indicate lack of discrimination. On the other hand, this could be evidence of a different problem. Banks often rely on county assessments when evaluating mortgage applications. If homes in black neighborhoods are under-assessed, applicants may face difficulty accessing credit.[7] Future studies should combine data on county assessments with data on private assessments and loans to determine whether county under-assessment impedes access to credit, controlling for sale price.

Furthermore, null results are important for at least two reasons: first, as Rothstein (2017: 172) notes, there are few contemporary studies of this issue, and so our knowledge is very limited. Second, if it turns out that discriminatory assessment is still a problem in some cities, it is important for us to know where it is *not* a problem, so that we know where to find models for a non-discriminatory assessment process. In other words, it is easier to fix problems if we know

7 I thank Jonathan Rothwell of Perry *et. al.* (2018) for this point.

where those problems are absent. For example, if we find discrimination in some cities but not others, we may wish to compare the assessment notices which are mailed to residents in each city, to see whether some notices are easier to read or understand than others. Therefore, the null result for Fulton County is still noteworthy.

The outline of this paper is straightforward: in section II, I explain a few more institutional details about property assessment in Fulton County. In section III, I detail my data and methods. Sub-sections III.a through III.c present results: in sub-section a, I present simple measurements of discrimination: the PRD and COD descriptive statistics, Gini coefficients, and tabulated mean assessment-sale ratios by different quantiles of home characteristics. In sub-section b, I present OLS regression results with spatial controls for all sales. Sub-section c presents results using a smaller sample of repeat sales with individual house fixed effects and socio-demographic changes between one sale and the next. Section IV concludes.

II. Assessment in Atlanta

As noted in the introduction, Fulton County has a history of discriminatory assessment (Atlanta Urban League 1971, Price 1975, Holmes and Pinner 1975, Simmonds 1991; Almy, Gloudemans, Jacobs, and Denne 2007; Connor 2018). In 1991, Fulton County initiated a mass reappraisal which incited a tax revolt. Opponents of the reappraisal claimed that the reappraisal was racially motivated, meant to redistribute wealth from whites to blacks (Connor 2018). Thus, Fulton County is a suitable location to study whether discriminatory assessment persists. Whereas most US local governments modernized their assessments in the 1960s and 70s and experienced their tax revolts in the 1970s and 1980s (Martin 2008), Fulton's reassessment and revolt occurred much more recently, in 1991. With discriminatory assessment being more recent in Fulton, discrimination may be more likely to still be found in Fulton today – if it is still to be found anywhere.

Discriminatory assessment could exist in Fulton if the more politically influential elements of the electorate – often the whiter or wealthier – successfully protest against upward assessments. Although Plummer (2014) finds that appeals promote assessment uniformity in Harris County, Texas, discrimination could exist because there is a greater incentive to appeal an assessment the higher the property value (Lee 2004: 38). The appeals process entails a fixed cost that may not be justifiable for properties with relatively low values. The owners of less valuable houses may therefore be less likely to appeal an over-assessment, and more likely to suffer from an uncorrected over-assessment. The appeals process may also be less accessible or comprehensible to those of lower means or lower education. Most local governments have a formal process for appealing one's assessment. However, there is often a filing fee, and one must often hire a private assessor.

In Fulton County, there is an established procedure for helping residents understand their annual property assessments and appeal them if necessary. In late May, all residents are mailed an annual "Notice of Assessment" (Fulton County Board of Assessors 2018, 2019a). This Notice of Assessment is not a bill because city, county, and school millage (property tax) rates will not have been set yet.[8] However, this Notice does notify the resident that a bill will be coming, and it clearly states the 100% fair market value (FMV) *appraisal*, the 40% taxable *assessment*, the deadline for appeal, instructions for appeal, and an estimated property tax bill based on the previous year's millage rates. Appeals may be filed online, in person, or by mail. A resident's appeal may be heard before a Board of Equalization, by non-binding arbitration, or by a hearing officer, depending on the issue being appealed. Some appeals can be quickly granted without a fee or hearing if the error is obvious (Kass 2018). An individual appearing before that board may wish to bring an attorney, an appraiser, or statistical data to substantiate their claim. If one is dissatisfied with the decision by the Board of Equalization, then appealing to the Superior Court costs $25. Finally, the Georgia state Department of Revenue reviews the Fulton County Digest Data annually, testing the assessment-sale ratio, the coefficient of dispersion (COD), and the price-related differential (PRD) for compliance.[9] The Fulton County Board of Assessors meets twice a month to approve changes to the Data Digest.[10] Although this process appears to be comprehensive and transparent, it is possible that individuals from certain socioeconomic classes are unable to afford this process, or perhaps they do not understand how to navigate the appeals process successfully. Therefore, over-assessment may be more likely in neighborhoods where the average income or education is lower.

Another source of discrimination may be assessment lag. If property assessments are not updated sufficiently frequently, then homes in appreciating neighborhoods will be taxed relatively less and homes in depreciating neighborhoods relatively more (Kahrl 2018: 388; Connor 2018: 986, Lee 2004: 37; Simmonds 1991: 162; Baar (1981: 334, 341; Haettenschwiller 1973: 431f.; Townsend 1951: 362; Engle 1975). In Fulton County, assessment is performed annually, and so assessment lag is a less likely explanation. Finally, Faulk and Hicks (2015: 5) argue that homes at either

8 In most local government jurisdictions, millage rates are set by dividing the total budgeted government expenditure by the total sum of all assessed property values. This is why it does not matter whether a property is over- or under-assessed as long as all other properties are equally mis-assessed.

9 Personal communication with Constance Mackey, Open Records Custodian of the Fulton County Board of Assessors. For details on the computation of the COD and the PRD, see section III.

10 Personal communication with Constance Mackey, Open Records Custodian of the Fulton County Board of Assessors. Cf. Almy, Goudemans, Jacobs, and Denne (2007: 5).

end of the statistical distribution of price are more difficult to accurately assess because there are fewer sales of comparable homes.

III. Data and Methods

Sales and assessment data are obtained from the Fulton County Board of Assessors.[11] This is a relatively standard set of tax assessor data and it includes all of the expected variables, such as parcel ID, sale price, sale date, sale type (e.g. ordinary sale, sale to a charity, foreclosure, etc.), assessed value, and physical characteristics (such as acreage, stories, etc.). The data include sales for calendar years 1999 through 2016, but to mitigate the effects of the 2008 housing crisis, and to ensure that I use only sales for which the most precise demographic data are available, I restrict the data to sales from 2010 onward.

Although Georgia state law requires all properties to be *assessed* at 40% of their *appraised* fair market value (FMV), this dataset reports the appraised FMV only, not the assessment. For tax purposes, the Fulton County Board of Assessors multiplies the appraised FMV by 0.4 to obtain the assessment.[12] Because assessment is precisely 40% of appraised FMV, the two are perfectly collinear. In my analysis, I use FMV as reported.

Fulton County offers homeowners several types of homestead exemptions, which are subtracted from the assessed value, i.e. from the 40% appraised FMV. For example, if the appraised FMV is $100,000, then the assessed value is $40,000, and after subtracting a $30,000 homestead exemption, the taxable value is $10,000 (Fulton County Board of Assessors 2019b). Each city and school system sets its own tax rate, in addition to the county-wide tax rate. Homestead exemptions increase the progressivity of the tax because the exemption reduces the taxable property value by a larger percent for low-priced homes than for high-priced homes. Different homestead exemptions exist for different regions of Fulton County, and they apply to different parts of the property tax bill – county, city, and school. Fulton County also offers several income- and age-based homestead exemptions. All of these different homestead exemptions are *not* accounted for in this analysis. This is because our data do not contain enough information to determine how each exemption would apply. For example, the basic Fulton County homestead exemption of $30,000 applies only to the county portion of one's property tax. Within the City of Atlanta, the $50,000 schools exemption applies only to the

11 https://fultonassessor.org/

12 Communication with Constance Mackey, Open Records Custodian of the Fulton County Board of Assessors. Cf. Fulton County Board of Assessors (2018, 2019).

schools portion of one's property taxes. Each city has its own separate homestead exemption, which applies only to the city portion of the property tax bill. Thus, to determine how each homestead exemption affects the final property tax bill, we would have to know each of the separate tax rates in each jurisdiction. And to determine the effect of the income- and age-based exemptions, we would have to know the age and income each homeowner. Therefore, in this analysis, I use only the 100% FMV without any adjustment for homestead exemptions. This is therefore a worst-case scenario, because adjusting for homestead exemptions would introduce progressivity. Furthermore, homestead exemptions are ideally not necessary to remove discrimination or disparity. If assessments are performed accurately and without bias, then the ratio of assessed value to sale price should not be correlated with income, race, etc. The homestead exemptions should make the tax more progressive by easing the burden for those with lower incomes, but there should not have been any discrimination or disparity to begin with.

A "Property Profile" file obtained from the Fulton County GIS Portal[13] contains data matching every parcel ID to spatial data such as (X,Y) coordinates and school attendance zones.[14] Demographic data – at the Census block group level were obtained from the U.S. Census Bureau's American Community Survey (ACS). The ACS comes in 5 year waves, and I used two waves: 2008-2012 and 2012-2016. Using ArcMap software – part of the ArcGIS suite – and the (X,Y) coordinates of each home, sales from 2010-2011 were spatially joined with the 2008-2012 wave, while sales from 2012 onward were joined with the 2012-2016 wave. In the final dataset, each observation is a sold house, each associated with Census block group demographics.

Next, the data were cleaned to remove unusable observations.[15] For example, to ensure that all sales are "arms-length" – i.e. ordinary sales between two parties who are not familiar with one another – I eliminated houses which were sold to charities, schools, and churches; houses which were sold under stressed conditions such as foreclosure or bankruptcy; houses which were sold as part of a divorce, etc. I also eliminated houses which were sold for implausible values, which I defined as less than $20 thousand or more than $10 million. But even after eliminating implausible or unusual sales, there remained implausible assessed-to-sale ratios. The largest ratio was approximately 170, meaning that some houses were assessed at approximately 170 times their sale price. Following the recommendation of the

13 http://gisdata.fultoncountyga.gov/. GIS stands for "geographic information systems."
14 X and Y coordinates refer to the Georgia State Plane West coordinate system, which projects the curved earth onto a flat Cartesian grid.
15 I also dropped duplicate observations. This was a complicated procedure, detailed in appendix 1.

International Association of Assessing Officers (IAAO 2013: 53), I eliminated all observations who assessed-to-sale ratios fell outside the 1.5xIQR range.[16]

Almy, Goudemans, Jacobs, and Denne (2007) express concern about so-called "sale chasing," in which property records are adjusted to ensure that assessments are close to sale prices. For example, they say, they have heard anecdotal evidence that physical condition (CDU) of a home may be changed in order to ensure that the estimated assessment is close to the sale price. This means that observed assessment-sale ratios may be systematically different from true (but unobserved) assessment-sale ratios, because unsold homes are not adjusted in this manner. The assessment-sale ratios may appear to be uniform and close to 100%, but unsold homes may be systematically mis-assessed in an unobservable way. To account for this potential bias, I use only sales from June onward, because assessment notices are mailed in late May. Thus, I use only sales which occur after the assessors office has already computed that year's assessment, ensuring that the assessor cannot adjust the assessment to match the sale price.[17] It is still possible for homeowners to appeal their assessments after this date. However, any changes made to assessments due to an appeal would *not* constitute a bias. On the contrary, it is highly desirable that homeowners are capable of successfully appealing erroneous assessments.

Ideally, I would control for the number of living units in each home, because Oldman and Aaron (1965), Black (1972), and Lee (2004) each found that multi-family properties were over-assessed (cf. Baar 1981: 335). Similarly, Faulk and Hicks (2015) find that non-homestead properties – which are often rented – are over-assessed. Unfortunately, our data consist almost entirely of single-family properties. Of 52,798 remaining observations, 51,635 are single-family, 729 are duplexes, 80 have three living units, and only 354 have either more than three living units or else a missing value. Therefore, I have chosen to restrict my data to single-family units only. Unfortunately, these data do not help us determine whether multi-family properties are over-assessed.

In the remaining sample, there are 51,635 observations, all single-family. The mean assessment-to-sale ratio is 0.9022, and the standard deviation is 0.1526. This ratio ranges between a minimum of 0.4667 and a maximum of 1.320. If every property were perfectly appraised at its sale price, the ratio would be 1.0, so a mean ratio of 0.9022 appears

16 That is: the interquartile range (IQR) is equal to the 75th percentile minus the 25th percentile. The IQR is multiplied by 1.5. Any observations whose ratio is less than the 25th percentile minus 1.5xIQR or more than the 75th percentile plus 1.5xIQR are eliminated.

17 This procedure is not used in any of the assessment-ratio studies cited in this paper except Almy, Goudemans, Jacobs, and Denne (2007). However, because the potential bias is enormous, this procedure appears unambiguously necessary. Furthermore, dropping sales which occur before notices are mailed does not appear to introduce any additional bias.

relatively reasonable.[18] Keep in mind that mis-assessment will not result in discriminatory taxation as long as every house is equally mis-assessed. If every house were assessed at exactly 0.9022 of its true value, there would be no discrimination.

Historically, most papers studying discrimination in housing assessment have tabulated mean assessment-to-sale ratios by location or by price range (e.g. Townsend 1951, Oldman and Aaron 1965, Haettenschwiller 1973, Pearson 1979, Lee 2004, Faulk and Hicks 2015, Atuahene 2018, Atuahene and Hodge 2018).[19] This is equivalent to a univariate regression of assessment-to-sale ratio on binned dummies of price range or location, without controls. Most of these studies use data at the Census tract level, because this was the smallest geographic region for which data were available. For the sake of consistency with prior studies, I use this same method as well, except I take advantage of the new availability data at the Census block group level – where two to four Census block groups compose a single Census tract.

In addition, following contemporary practice in most studies of property value, I use multiple regression methods adapted from hedonic OLS methods – in which logged sale price is regressed on a vector of housing characteristics (Freeman *et. al.* 2014: 310-359; Haab and McConnell 2002: 245-267). Recent studies which use hedonic OLS regression to test the effect of race on sale prices include Perry *et. al.* (2018), Bayer *et. al.* (2017), and Myers (2004). I simply replace the dependent variable, logged sale price with the assessed-to-sale ratio.

Special attention must be paid to spatial dependence and autocorrelation. A house's price is not merely a function of its physical characteristics (acreage, stories, etc.) but also its location. This spatial dependence can cause both bias as well as inefficiency. For example, a house's value will be affected by the crime rate of its neighborhood and proximity to desirable amenities. Failure to include such neighborhood effects will induce omitted variables bias (Basu and Thibodeau 1998: 61, 63). Meanwhile, a house's value is not only determined by itself, but also by the values of its neighboring houses. This could be for two reasons: first, houses near each tend to share a common architectural style (Basu and Thibodeau 1998: 61). Second, a seller or realtor will often use historical sale prices of neighboring houses to help determine an asking price. These comparable prices are often referred to as "comps." Thus, each house is not a unique observation, and wrongly treating each house as unique will over-estimate the sample size and therefore the precision of the coefficients. Thus, the standard errors will be biased by spatial autocorrelation.

18 Again, although I refer to this as an assessment ratio, I am really using the appraised FMV, whereas the true assessment is 40% of FMV.

19 Black (1972) is one of the few studies to perform regression analysis. He regresses mean assessment-to-sale ratio of each Census tract on mean sale price, mean income, mean depreciation, and mean race of those same Census tracts. Engle (1975) uses time-series regression to measure assessment lag. Lee (2004) mostly relies on tabulated means but does briefly present one regression.

Locational fixed effects – especially ZIP code fixed effects – and distance to the CBD (central business district) are often used to define housing submarkets and thereby correct for both omitted variables bias as well as spatial autocorrelation (Goodman and Thibodeau 2003; Bourassa, Cantoni, and Hoesli 2007).[20] Myers (2004) calls attention to the fact that neighborhood amenities are often correlated with race, and that failure to control for neighborhood effects will create a correlation between the treatment variable and the error term. She finds that even when controlling for neighborhood observables such as average race, income, and education, the Hausman test still rejects random effects in favor of fixed effects.[21] Similarly, Bayer *et. al.* (2017) use neighborhood-by-time fixed effects, where each neighborhood is a Census tract.[22] Perry *et. al.* (2018) include neighborhood observables at the Census tract and ZIP code levels, as well as city fixed effects.[23] Following these examples, my regressions will include demographic observables at the Census block group level, as well as fixed effects for cities, parcel districts, school attendance zones, and/or Census tracts.[24]

Following Black (1972), I also include average structural characteristics of the homes in each neighborhood. Specifically, I include log median sale price, median age, log median acreage, median stories, and the percentages of homes that have above or below "average" CDU (depreciation)[25] – for each Census block group, for each of the two

20 ZIP code fixed effects appear to be popular because the street address of a house already includes them. Because I have the option of using Census tract and block group fixed effects if necessary, I see no reason to use ZIP codes specifically.

21 Myers (2004) is concerned that the Census tract – the usual neighborhood indicator – is too large. She takes advantage of special data available only for 1985, 1989, and 1993, which observed demography at a special cluster level, even smaller than a Census block group. However, she worries that the cluster may actually be too small. In this paper, I use the Census block group, whose size is intermediate between too-large tracts and too-small clusters.

22 In addition, Bayer *et. al.* (2017) are concerned that within any given neighborhood, blacks may purchase homes whose architecture or depreciation is systematically different in ways which are not observable to researchers and assessors. For example, blacks may purchase homes which are less improved or less well-maintained on the inside, spuriously suggesting that blacks pay less than whites for equivalent homes. They take advantage of their large dataset of repeat sales by including individual house fixed effects as well.

23 Ihlanfeldt and Mayock (2009) are similarly concerned that failure to control for neighborhood quality will create a downward-biased estimate of discrimination. They first use a "traditional" approach of hedonic OLS with neighborhood observables and fixed effects. Then, they use a "Harding" approach, which uses the sums and differences of racial dummies as regressors. These racial dummies refer to the race of the buyer and of the seller. The difference of dummies indicates whether the buyer and seller were of different races, creating potential discrimination. The sums of dummies indicate whether the buyer and seller might share certain tastes and preferences. Thus, including both regressors attempts to measure the effect of discrimination and bargaining power while controlling for unobserved tastes and preferences. Remarkably, they find that the estimates of the Harding approach have the opposite signs of the "traditional" approach, casting doubt on whether locational fixed effects and observables are enough. However, I do not possess data on the ethnicities of buyer and seller, so I cannot use the Harding approach. Furthermore, Ihlanfeldt and Mayock (2009) have a different research question than mine, and therefore, different econometric concerns. Their question is whether blacks pay a higher price than whites for a given house within a given neighborhood. By contrast, I am concerned with discrimination between one neighborhood and another.

24 Because my treatment variables are demographic observables at the Census block group level, including block group fixed effects is not reasonable. Because each block group is observed twice – once in the 2008-2012 ACS wave and once in the 2012-2016 ACS wave – there is technically some variation in each block group that would not be absorbed by a fixed effect. However, this variation is so small that I would not place any trust in such a regression.

25 "CDU" stands for "condition, desirability, utility." Here, "average" CDU is *not* used in a statistical sense. Houses are rated "unsound," "very poor," "poor," "fair," "average," "good," "very good," and "excellent." Thus, above or below average CDU merely refers to quality that is above or below the middle ordinal ranking – *not* above or below a statistical mean or median.

waves of the ACS.[26] Of course, each home also includes observable Census block group demographics, including education, race, and income. The log median sale price is of direct interest as an indicator of discrimination. The average structural variables – median age, acreage, stories, and depreciation – may capture differences in kinds of neighborhoods. For example, Grace and Hall (2019) argue that it is important to include age in all hedonic OLS regressions because a home's age often proxies for its physical location.

Instead of using the distance to the CBD, I follow Farmer, Shiroya, and Naithani (2019), using the X and Y distance and distance squared from an arbitrary point. Thus, the mean value of the dependent variable allowed to vary separately with the latitudinal and longitudinal distances from some arbitrary point, and the squared terms add curvature. These four terms – X, X^2, Y, and Y^2 – should help control for spatial characteristics which may affect property values, such as proximity to the central business district (CBD). Because the locations of our houses are already coded according to a Cartesian grid, where the origin (0,0) lies to the southwest, outside the study area, I simply use (0,0) as my arbitrary point.

Because it is not clear whether and to what extent assessment-sale ratios are spatially dependent, and because there is a risk of multicollinearity between locational fixed effects and our demographic treatment variables, my regressions will proceed from models without any spatial controls to models with increasing levels of spatial controls. This will allow us to evaluate whether different kinds of spatial controls have any effects on the fit or the coefficients. There is probably a high degree of multicollinearity between Census block group observables and Census tract fixed effects, and therefore, I would be hesitant to trust the coefficients estimates when Census tract fixed effects are included. But including these fixed effects will give us an indication of omitted variables bias, via the R^2. If the R^2 does not greatly increase as we progressively add spatial controls, this will suggest that omitted variables bias is not severe, and that we can place a reasonable degree of trust in our previous coefficient estimates without these Census tract fixed effects. Finally, all standard errors are clustered by Census block group-wave.

One clarification of terminology: the progressivity or regressivity of a tax will depend on the choice of benchmark. For example, the sales tax is neutral insofar as the same nominal tax rate applies to everyone, but it is regressive with respect to the consumer's income. McMillen and Singh (2019) evaluate the regressivity of the property tax with respect to

26 That is, each block group is summarized in 2008-2012 and again in 2012-2016. For example, a home sold in 2009 would be associated with the median age, acreage, stories, etc. of all homes sold in its own Census block group over the entire period 2008-2012. Because each block group is so physically small, there may not be very many sales in a particular block group in a particular year, and therefore, taking medians of homes sold in a particular block group in a particular year may induce bias from outliers or small sample sizes. Because homes are durable, structural characteristics should not change very much over time, and therefore, it seems reasonable to average structural characteristics over 5 years of sales.

sale price and with respect to income. I will generally evaluate the regressivity or progressivity of the property tax in two different senses: first, in nominal terms: are homes of a certain kind taxed at a higher or lower nominal rate? Second, I will judge progressivity with respect to the county budget. The property tax will be progressive in this sense if owners of certain homes pay a larger property tax bill and contribute more to the county budget, thus cross-subsidizing those who pay less, even if their nominal tax rate is lower. Suppose that when a home's price doubles, its tax rate falls by 5%. Nominally, this tax is regressive, because the owner of a more expensive home is taxed at a lower rate. However, this tax is progressive with respect to the county budget. The owner of the more expensive home will owe a larger property tax bill and subsidize the owner of the less expensive home – assuming that both consume the same value of government services.

Finally, I assume that we hope to fail to reject the null hypothesis that there is no discrimination. Therefore, I will use a less stringent critical p-value than the standard 10%, 5%, or 1%. A smaller – and more stringent – critical p-value is preferable when one wishes to reject the null hypothesis, because the lower critical p-value makes it harder to reject the null hypothesis and obtain the results one expects or hypothesizes. In other words, a lower critical p-value reduces the odds of obtaining significant results. But in our case, we hope to obtain null results. It would be possible to guarantee a failure to reject the null hypothesis of zero discrimination merely by setting a very low critical p-value, such as 0.1%. A lower critical p-value – i.e. a higher confidence level – implies a wider confidence interval, and we could easily guarantee that zero is always within the confidence interval by using arbitrarily large confidence levels. In our case, we wish to make failure to reject the null hypothesis less likely by choosing narrower confidence intervals with a higher critical p-value. Therefore, I have selected a critical p-value of 20%, with a confidence level of 80%. This means that the evidence of discrimination will not have to be as statistically significant for us to reject the null hypothesis and conclude that there is evidence of racial or socioeconomic discrimination. In short, I have biased my methodology in favor of finding discrimination, in the hope that I will not find any.

In the final dataset, there are 51,635 observations, with 45,606 unique parcel IDs. Thus, about 88% of the houses were sold once, while about 12% were sold more than once. There are 471 Census block groups in the dataset, and therefore, each block group contains about 110 sales. There are 182 Census tracts, so every Census tract fixed effect covers approximately 284 sales. The full set of variables is listed and described in table 1. See table 2 for summary statistics, and figures 1 and 2 for chloropleth (color-coded) maps of Fulton County depicting the distributions of black population and median income.[27]

III. Results

a. Descriptive Statistics and Tabulated Means

First, we can estimate the degree of discrimination – or lack thereof – with three sets of descriptive statistics, indicated in table 3. These statistics are the COD, the PRD, and the Gini coefficient. The coefficient of dispersion (COD) is a measure of horizontal equity, and it is the “average percentage deviation of the [assessment-to-sale] ratios from the median ratio” (IAAO 2013: 13).[28] An acceptable COD is 5-10% for newer or homogeneous areas and 5-15% for older or heterogeneous areas (IAAO 2013: 17). In our sample, the COD is 12.0302%. This indicates an acceptable degree of horizontal equity – meaning that not too many houses are deviating too much from the median assessment-to-sale ratio. This does not indicate whether or not discrimination is present, but it at least indicates that in general, the assessment process is consistent. In Marion County – where Indianapolis is located – Faulk and Hicks (2015: 14f.) find a COD of 18.517% in a trimmed sample. McMillen and Singh (2019) find CODs between 22.480% in Denver and 31.648% in Cleveland. According to Baar (1981: 340f.), COD values of 30% to 40% used to be common, and the median COD nationwide was 22%. Thus, Fulton County’s COD of 12.0302% is relatively low.[29]

The price-related differential (PRD) is a measure of vertical equity, measuring whether higher-priced houses are assessed differently than lower-priced houses (IAAO 2013: 13f.). This measure will indicate whether discrimination is present or not. First, we calculate the sale price-weighted mean of assessment ratios, or equivalently, the sum of all assessments (in dollars) divided by the sum of all sale prices. Second, we obtain the PRD by dividing the mean ratio by the weighted mean ratio. A PRD greater than 1.0 indicates assessment regressivity, while a PRD less than 1.0 indicates

27 White areas of the map indicate missing data.

28 First the assessment-to-sale ratio is calculated for each house. Second, the absolute value of the deviation of each house’s ratio from the median ratio is calculated. Third, these absolute deviations are summed and divided by the number of houses to give a mean absolute deviation. Finally, the mean absolute deviation is divided by the median ratio and multiplied by 100 to obtain a percentage.

29 Note that Faulk and Hicks (2015) and McMillen and Singh (2019) use slightly different methods of removing outliers and accounting for homestead exemptions. Therefore, statistics are not directly comparable, but they do give an idea of what a “large” or “small” value is.

assessment progressivity. In our sample, the mean assessment ratio is 0.9022 while the weighted mean ratio is 0.8975, so our PRD is 1.00533. Technically, this indicates regressivity, but the two means only differ by a fraction of a percent, and this probably means there is no significant discrimination by sale price. For comparison, Faulk and Hicks (2015: 11), studying the entire state of Indiana, find PRD values between 1.04 and 1.26, depending on the year and sample. In Marion County – where Indianapolis is located – Faulk and Hicks (2015: 14f.) find a PRD of 1.072 in a trimmed sample. McMillen and Singh (2019) find PRDs between 1.015 in Denver and 1.316 in Baltimore. Thus, Fulton County's PRD of 1.00533 is comparatively close to one.

Finally, following McMillen and Singh (2019), a set of Gini coefficients can be used as a complement to the PRD, to measure assessment regressivity. First, the data are sorted by sale price. Second, Gini coefficients of both sale price and of assessment value are estimated. The same sorting order is used so that the two Gini coefficients are directly comparable. Finally, the statistic of interest is the Gini coefficient of assessments minus the Gini coefficient of sale prices. The Gini coefficient measures inequality, where a value of zero indicates perfect equality and a value of one indicates perfect inequality. If sale prices and assessments have the same degree of inequality as one another, this indicates that assessments track sale prices closely, which in turn is evidence against regressivity. But a value less than zero indicates that assessments are less unequal than sale prices, which in turn indicates regressivity. In other words, a negative difference of Gini coefficients means that assessments are relatively equal (closer to zero) while sale prices are relatively unequal (closer to one). In Fulton County, our difference of Gini coefficients is -0.0018, which indicates regressivity. For comparison, McMillen and Singh (2019) obtain values between -0.010 and -0.121 – for Denver and Baltimore respectively. Regarding the value of -0.010 for Denver, they say "the value is close to zero . . . despite being statistically significant." Fulton County's difference of -0.0018 is about one-fifth ($^1/_5$) the size of Denver's (-0.010) and $^1/_{67}$-th the size of Baltimore's (-0.121). Thus, Fulton County's regressivity is much smaller than even the smallest degree of regressivity found by McMillen and Singh (2019).

Next, following the method of most studies of discriminatory assessment, we can observe the mean assessed-sale ratio for stratified subsamples of the data. I divide percent black, percent renter, median income, sale price, median sale price, and education into quartiles. Discrimination will be present if the mean assessed-sale ratio differs for these quartiles.[30] I also tabulate mean ratios by city, to see if residents of different cities are assessed differently. See table 4a for

30 Because education is not a continuous variable, it is difficult to estimate a single measure of education. I estimated education as (1 * percent HS diploma + 2*percent college degree + 3*percent graduate degree). Although this measure is imperfect – for example, it assumes that a graduate degree is worth precisely three times as much as a HS diploma – I hope that dividing the result

the full set of tabulated mean assessment ratios by quartiles and by city. Note that a higher mean ratio indicates greater taxation because one's effective property tax rate is increasing. In each case, a oneway ANOVA indicates statistically different means for each quartile.

The signs of the changes in means from one quartile to the next often indicate the opposite of regressive discrimination. For example, as we move from the first quartiles to the fourth quartiles of percent black and percent renter, the mean ratios decrease, meaning that blacks and renters are taxed proportionately less. Conversely, as we move from the first quartiles to the fourth quartiles of median income, median sale price, and weighted education, the mean ratios generally increase, meaning that those with higher incomes, more education, and in more expensive neighborhoods are taxed proportionately more. This is the opposite of what we would expect if there is regressive discrimination.

By contrast, the first quartile of sale price is assessed at a higher ratio than the other three quartiles. This suggests that lower-priced homes are taxed proportionately more than higher-priced homes. This is consistent with our PRD of 1.00533, which indicates regressivity. However, it contradicts the pattern we saw for median sale price, which showed progressivity. Thus, it is unclear whether assessment in Fulton County is progressive or regressive with respect to sale price.

The clearest indicator of some sort of discrimination appears in the list of cities. If we disregard "Fulton County" with 3 observations and "Mountain Park" with 41 observations, we still see wide variation in means. For example, Sandy Springs and Alpharetta are assessed at 0.92 while Hapeville and East Point are assessed at only 0.87. This indicates some sort of disparity, but it is difficult to interpret its meaning. We have already failed to find any clear evidence discrimination along racial and socio-demographic lines, so this disparity among cities is apparently due to some other difference.

In table 4b, we repeat the same procedure, except we tabulate by deciles rather than quartiles. As we move from the first decile of percent black to the last, the mean ratio generally decreases, meaning that residents of black neighborhoods are taxed less. A similar pattern exists for percent renter. As we move from the first deciles to the last deciles of median income, median sale price, and weighted education, the mean ratios increases, meaning that residents of more affluent neighborhoods are taxed more. So far, the deciles do not show any evidence of discrimination.

For sale price, nearly all the mean ratios are the same in every decile – about 0.89 or 0.90 – which is about the same as the overall mean ratio. The single exception is the first decile, which has a mean ratio of 0.9362. This may indicate that homes in the lowest 10% of sale price are taxed at a higher than average rate, while all other homes are taxed at the

into quartiles will meaningfully capture differences in mean education across our sample.

average rate. Alternatively, this may indicate an informational problem rather than discrimination. At the tail-end of the distribution, there may be more variation and fewer homogeneous comparables, making it harder to assess these homes (Faulk and Hicks 2015: 5). Furthermore, the difference between a ratio of 0.9362 and a ratio of 0.89 may be very small in the context of the differences found elsewhere. Faulk and Hicks (2015: 6) – studying the whole state of Indiana – find mean ratios of 2.9287 and 0.8752 for the lowest- and highest priced homes (respectively) in their untrimmed sample, and mean ratios of 1.4654 and 0.8597 for the lowest- and highest-priced homes in their trimmed sample. Relative to the variation that Faulk and Hicks found, our ratios of 0.9362 and 0.89 for the lowest- and highest-priced homes suggest relatively uniform and non-discriminatory assessment. Nevertheless, there may be room for improvement here.

So far, we do not see any clear evidence of discrimination. Neighborhoods with a higher percentage of blacks and/or renters are actually assessed and taxed less, not more. And neighborhoods with higher median income, median sale price, and education are assessed and taxed more, not less. The only suggestion of discrimination we can find is with regard to sale price, where it appears that lower-priced homes may be assessed and taxed proportionately more. However, this conflicts with the effect for median sale price. Therefore, we proceed to investigate these results more deeply with multivariate regression analysis.

b. OLS Regression

Although most studies of assessment discrimination rely on tabulated means of the sort I have just presented, it is possible that evidence of discrimination may be exaggerated or attenuated by omitted variable bias. Therefore, we proceed to examine the evidence for discriminatory assessment using multiple regression analysis, in which we simultaneously control for all of the separate variables we individually investigated in the previous section.

Before we proceed, however, there is one change to our sales price variables which we must make for clarity. In the previous section, we saw that the effect of median neighborhood price was the opposite of individual home sale price: a higher median price was associated with higher assessment and taxation, while a high sale price was associated with lower assessment and taxation. If we control for these two variables simultaneously, the coefficients will be difficult to interpret. Therefore, in our regressions, we replace log sale price with the percent deviation of a house's individual sale price from its neighborhood's median sale. Thus, rather than separately controlling for individual and average sale prices, we control for the average sale price (of each Census block group) and the individual deviations from the average. This should help us determine whether property taxes are progressive or regressive with respect to sale price.

A second issue that arises, however, is that there are many extreme outliers of percent difference from the median sale price. The maximum value of percent difference is 2,287.006, meaning that one house sold for 2,287.006 percent more than the median house in its neighborhood. Therefore, we trim outliers by dropping all houses whose percent deviation is outside 1.5xIQR. Summary statistics for the percent difference before and after trimming outliers are presented in table 5. Next, in table 6, we see summary statistics of price for the trimmed sample.

Now, we are ready to obtain our regression estimates. In the set of seven regressions in table 7, we regress the assessed-sale ratio on our set of socioeconomic and demographic variables and controls. Because our variables are all measured in different units, all coefficients are standardized. Thus, a one standard deviation change in the independent variable is associated with a beta standard deviation change in the dependent variable. To estimate whether property taxation is progressive or regressive with respect to sale price, we look to the coefficients on neighborhood median sale price, an individual home's percent deviation above, and percent deviation below.

As we move across the columns of table 7, we add additional controls to see whether they change the coefficients or remarkably increase the R^2. The first column is our baseline regression, controlling only for socio-demographics (percent black, percent renter, log median income, education), sale price (log median and percent difference above/below), sale characteristics (revision code, deed type, sale type, sale evaluation), and time of sale (month and year). The R^2 in this first regression is 0.300. The second column adds individual house characteristics, with the R^2 increasing to 0.348. In the third column, we add neighborhood characteristics to control for spatial dependence – specifically, dummy variables for type of location, lot, LUC (land-use code), and zoning. The R^2 increases to 0.359. In the fourth column, we add neighborhood observables – specifically median age, log median acres, median stories, and the percent of homes with an above or below "average" CDU. The R^2 increases to 0.363. In the fifth through eighth regressions, we respectively add (5) X and Y distance and distance squared, from an arbitrary point, (6) city fixed effects, (7) parcel district and school fixed effects, and (8) Census tract fixed effects. The R^2 increases from (5) 0.365, to (6) 0.367, to (7) 0.382, to (8) 0.392. This gradual increase in R^2 suggests that there are some spatially distributed omitted variables. However, the R^2 does not greatly increase from regression 6 to regression 7, and many of the coefficients are remarkably consistent across columns. This suggests that Census tract fixed effects are not contributing an enormous amount of additional information. This is important because Census tract FE may be highly multicollinear with Census block group observables, and so we would prefer to be able to interpret the coefficients without Census tract FE.

Summarizing our results, we see:

- Across all eight regressions, percent black is always significant, but its sign is always negative, indicating that blacks are assessed – and therefore taxed – at a *lower* effective rate.
- Percent renter is usually positive but sometimes negative. Furthermore, the coefficients are too small to be practically meaningful. A coefficient of 0.0424 in column 2 indicates that a one standard deviation increase in percent renters is associated with a 0.0424 standard deviation increase in the assessment ratio. This is less than 5% of a standard deviation – probably too small to be meaningful. The standard deviation of the assessment ratio is about 0.15 or 15%, so 5% of that is 0.0075 or 0.75%. In other words, a neighborhood with a one standard deviation increase in the number of renters is subject to about a 0.75% increase in their taxable property values. This is the difference between an annual property tax bill of $1,000 versus $1,007.50. And merely adding neighborhood controls in column 4 causes the effect to diminish to 0.0241. This suggests that any discrimination against renters is so small as to be practically meaningless.
- Log median income is positive but not significant in column 1, whereas it is negative and statistically significant in all other columns. These negative coefficients mean that residents of higher-income neighborhoods are taxed at lower effective rates. However, the magnitudes are too small to be meaningful. Across the last seven columns, the coefficients are all between approximately -0.03 and -0.05. This means that a one standard deviation increase in log income is associated with a 0.03 to 0.05 standard deviation decrease in assessment. This is approximately the same magnitude as we saw with percent renter. Thus, a one standard deviation increase in log median income is associated with approximately a $7.50 reduction to a $1,000 property tax bill.
- Education is sometimes statistically significant, but the significant coefficients are almost always positive. This means that more highly educated populations are taxed more. Moreover, the coefficients are all very small – somewhere between approximately 0.02 and 0.07. Thus, it does not appear that less educated residents suffer from higher effective tax rates.
- The effect of the percent of homes with a CDU (depreciation) above or below "average" is never significant.

So far, we see little evidence of any socioeconomic discrimination. If anything, blacks are taxed less than whites, and less educated neighborhoods are taxed less than more educated neighborhoods. While we do see some evidence of regressive discrimination in favor of those with higher incomes and regressive discrimination against neighborhoods with more renters, the effects are too small to be meaningful – a one standard deviation change in either variable is responsible for a change of about $7.50 for every $1,000 of property taxation.

However, there is some limited evidence of discrimination with regard to log median sale price and an individual home's percent deviation of its price above or below the median. In the first column, the coefficient on log median sale price is positive and significant, estimated at 0.0442. This coefficient is too small to be meaningful, but the fact that it is positive means that if anything, higher-priced homes are taxed at a higher rate, which is the opposite of regressive discrimination. This is consistent with our tabulated means, where we saw that homes in more expensive neighborhoods are assessed and taxed more. However, adding additional controls in columns 2 through 8 causes the coefficient estimate to become negative and significant. The absolute values of the coefficients on log median sale price are all between approximately 0.2 and 0.4, which is *not* negligible. This means that homes in more expensive neighborhoods are assessed and taxed less.

Furthermore, within any given neighborhood, homes priced above average are under-assessed while homes priced below average are over-assessed. Perhaps what this means is that assessments tend towards the average assessments in that neighborhood. This could be a result of computerized mass-appraisal techniques, which tend to use regression analysis to match homes to similar homes in their neighborhoods.

This appears to suggest some regressive taxation. Let us therefore examine the meanings of these coefficients. Consider a house in an average neighborhood with a median price of exp(12.262) or $211,504.17. The largest coefficient on log median sale price is negative 0.456. This means that a one standard deviation increase in the log median sale price is associated with a 0.456 standard deviation decrease in the assessment ratio. The standard deviation of log median sale price is 0.805, while the standard deviation of the assessment ratio is 0.153. Therefore, a one standard deviation increase in the log median sale price implies a neighborhood median price of exp(12.262+0.805) or $473,070.64. The average house is assessed at a ratio of 0.902, while the above-average house is assessed at 0.456 standard deviation less, or (0.902 - 0.456*0.153) = 0.832. Thus, the first house is assessed at $211,504.17 * 0.902 = $190,776.76, while the second house is assessed at $473,070.64 * 0.832 = $393,594.77. The percent difference between the two neighborhoods' median prices is

123%, while the percent difference between the two assessments is 106%. Thus, the property tax is still somewhat progressive. In one neighborhood, homes are worth double plus 23% more, while they are assessed at double plus 6% more. In other words, in neighborhoods where homes sell for more than double, they are also assessed more than double. On the other hand, the ratios do appear quite different – 0.902 versus 0.832.

Thus, the property tax may be progressive from one perspective but regressive from another. Residents whose homes are about twice as valuable do pay about twice the total property taxes, but at a lower rate. Thus, the property tax is regressive in the sense that (presumably) wealthier homeowners are taxed at a lower rate, but it is progressive in the sense that wealthier homeowners contribute a larger total to the county government's revenue, thus subsidizing the provision of public services to residents who pay less.

Now consider a house that is one standard deviation above the median sale price in its own neighborhood. The largest coefficient on percent difference above is negative 0.203. This means that a one standard deviation increase in the percent difference above the median price is associated with a 0.203 standard deviation decrease in the assessment ratio. The standard deviation of percent difference above is 27.175%. Thus, the baseline house in the median neighborhood will sell for exp(12.262) or $211,504.17, while a house one standard deviation above that will sell for exp(12.262) * 1.27175 = $268,980.43. As before, the mean assessment ratio is 0.902 and its standard deviation is 0.153. Thus, the assessment ratio will fall from 0.902 to (0.902 - 0.203 * 0.153) = 0.871. The first house is assessed at 211,504.17 * 0.902 = $190,776.76, while the second house is assessed at 268,980.43 * 0.871 = $234,281.95. The percent difference in sale prices is 27.175% while the percent difference in assessments is 22.804%. Thus, the more expensive home is still assessed at a higher value, and pays higher taxes. Once again, this is not perfectly progressive, but it is still generally progressive – in the sense that owners of more expensive homes pay more, albeit at a lower rate. Finally, if we considered a house that is one standard deviation *below* the median sale price in its own neighborhood, we would obtain qualitatively similar results, because the coefficients and standard deviation of percent different below are similar to those of percent different above.

Whether these results indicate progressivity or regressivity is a matter of interpretation. Our results are consistent with the null hypothesis that the less expensive home is assessed at a lower total value than the more expensive home, so that owners of more expensive homes pay more taxes. Although this property tax is not perfectly progressive – because the more expensive homes are assessed at lower ratios than the less expensive homes – the tax is still somewhat progressive because the proportional decrease in the assessment ratio is less than the increase in the house's sale price. In other words, sale prices increase faster than assessment ratios decrease, so that more expensive homes still owe more property taxes. And recall that our PRD – the price-related differential – was 1.00533, which is barely more than a perfect PRD of 1.0. This means that the property tax is only very slightly regressive. It is probably unreasonable to expect perfect progressivity because regression-based mass appraisal techniques must be used to assess unsold houses. In other words, there must be some method for estimating the fair market values of homes that have not been sold in many years. Regression must be used to compare unsold homes to comparable sold homes, and one reasonable consequence is that the assessments of above- and below- average homes tend towards their means. On the other hand, because higher priced homes are assessed at lower ratios, there may be some room for improvement. A higher-priced home owes a larger property tax bill, but its tax rate is slightly lower than that of a lower-priced home.

The bottom line, so far, is that we have failed to find any significant evidence of racial or socio-demographic discrimination. The coefficient on race is significant but negative – meaning that blacks pay lower effective tax rates. The coefficients on income and renter are too small to be meaningful. Coefficients on education are usually positive, indicating that more highly educated neighborhoods are taxed more. Given that our PRD and tabulated means failed to find evidence of discrimination, we should conclude that we have failed to find significant discrimination. This does not mean that no discrimination exists – only that we have failed to find it if it does exist.

On the other hand, we may have found some limited evidence of discrimination with respect to sale price. More expensive homes and homes in more expensive neighborhoods are taxed at higher total dollar values, but at lower rates. This implies that the property tax is progressive but not perfectly progressive. Sale price increases faster than the assessment ratio decreases, meaning that more expensive homes are taxed more, but not as much as they could be. Whether this implies progressivity or regressivity is a matter of interpretation.

For robustness, let us examine whether our results depend on the region of the county being examined. So far, we have used the entire county as our dataset, but perhaps this large dataset obscures heterogeneity within the data. In table 8, we present OLS regressions using sub-samples of the data. We use a minimum of geographic controls because our purpose is to compare different geographic regions to one another, so we do not want geographic controls within each region to obscure any differences among regions. The following regression specification is equivalent to column 4 of the previous regression (table 7) except now, each column is a different sub-sample.[31] Our regions are: the full county, the red boxed sub-sample which includes the city of Atlanta and its immediate suburbs (see the maps in the "Data" section), the

31 Thus, column #3 of table 8 is identical to column #1 of table 9.

city of Atlanta by itself, and the suburbs of Atlanta by themselves.[32] We see that comparing these four sub-samples, the estimated coefficients do not vary greatly from one sample to another. Thus, our results are not being driven by the choice of sample.

Finally, in table 9, we show the coefficients on city fixed effects using regression analysis, to compare the results to our tabulated mean assessment ratios by city. We wish to see whether residents of different cities are taxed differently, controlling for other variables. Here, we report Y-standardized coefficients, meaning that a one-*unit* change in the X-variable is associated with a beta standard deviation change in the Y-variable. Because our interest is in comparing geographic regions to one another, geographic controls are kept to a minimum. The regression equation in table 9 is equivalent to column 4 of the first regression (table 7). The omitted base-case is Atlanta. We see that there does appear to be some discrimination among cities. Even ignoring "Fulton County" with 2 observations and "Mountain Park" with 19 observations, we still see some large coefficients. Several cities have coefficients with absolute values of more than 0.1, meaning that in some cities, residents are taxed at more than $^1/_{10}$ of a standard deviation above or below the mean assessment ratio. It is not clear what is causing this disparity because we are controlling for both socio-demographic and architectural differences. Thus, the differences in assessment ratios among these cities are *not* due to differences in income, race, education, architecture, etc. Further research could explore this puzzle.

c. OLS Repeat Sales Model

One may be concerned with omitted variables bias, however. Bayer *et. al.* (2017), Ihlanfeldt and Mayock (2009), and Myers (2004) all argue that race may be correlated with omitted neighborhood characteristics, biasing all estimates. Therefore, following Bayer *et. al.* (2017), I proceed to estimate two repeat sales models. By using only observations of homes which sold multiple times, we may better control for unobservable characteristics, at the expense of a smaller sample size.

To obtain our sample of repeat sales, I drop all homes which did not sell in both the 2008-2012 period and the 2012-2016 period. This ensures that every home sold during each of the two waves of the American Community Survey, permitting variation in the demographic variables. Next, I regress the assessed-sale ratio on the same set of variables as all

32 The red-boxed sub-sample begins at 2,153,326 feet in the west (on the X-axis) and ends at the eastern boundary of Fulton County, at 2,241,567 feet. On the Y-axis, the sub-sample extends from 1,326,068 feet to 1,434,734 feet.

our previous regressions. However, following Bayer *et. al.* (2017), I include individual house fixed effects to capture unobserved time-invariant neighborhood characteristics. The Hausman test of fixed effects versus random effects is Prob>chi2 = 0.0000, indicating that random effects are *not* consistent, and that fixed effects *are* capturing significant unobserved variables.

The regression in table 10 compares ordinary – i.e. non-standardized – coefficient estimates for FE vs. RE. However, it is very difficult to interpret these coefficients. Because I am including individual house fixed effects, I am estimating the effects of changes in socioeconomic or demographic variables within an individual house. Therefore, it is inappropriate to use the sample standard deviation of percent black or log median income, for example. The sample standard deviation of median income is $46,083.88, while the sample standard deviation of percent black is 35.55%. It is absurd to imagine a neighborhood undergoing such a drastic change between 2010 and 2016 that the median income or percent black increase by that much for an individual house between one sale and the next.

In other words, we must be careful to consider a reasonable counterfactual and not extrapolate beyond the convex hull of our data (Neter, Wasserman, and Kutner 1989: 262; King and Zeng 2006, 2007). For all houses in Fulton County as a whole, it is reasonable to imagine the median income between one neighborhood and another differing by $46,083.88. But it is absurd to imagine the median income for one particular house changing by $46,083.88 from one sale to the next. Thus, the inclusion of individual house fixed effects force us to reconsider what is a reasonable change in any independent variable. Using the sample standard deviations to interpret a fixed effects model – i.e. holding a particular house dummy constant while varying an independent variable by as much as it varies over the whole sample – would require extrapolation beyond the convex hull of our data.

Therefore, we must obtain new summary statistics to determine what is a reasonable change within a given house. In table 11, I present estimates of the mean spread and of the spread of spread for each independent variable, within individual houses, over time. To obtain these estimates, I first estimate the spread of each variable over time, within individual parcel IDs. Then I obtain the mean of each spread for the whole sample. This will tell us how much each variable varies, on average, within any arbitrary individual house. I use three different measures of spread: the standard deviation (SD), the median absolute deviation (MAD), and the mean absolute deviation (MDEV). I use three measures of spread because they may be affected differently by outliers, and it is extremely important to ensure that outliers do not bias our estimates of spread and cause us to inadvertently extrapolate beyond the convex hull.

In table 11, "full sample" refers to the ordinary sample mean and sample standard deviation, as one might find in an ordinary table of summary statistics. If one naively attempted to interpret the FE results using ordinary summary statistics for the full sample, these are the values one would use. One would estimate a treatment effect by comparing the outcome for a unit with a mean value of the treatment variable, and the outcome for a unit with a (mean+st. dev.) value. For example, the full sample mean of percent black is 33.37% and its standard deviation is 35.75%. By contrast, "SD," "MAD," and "MDEV" refer to measures of spread within individual houses, as previously described. The mean of SD, MAD, and MDEV refers to the mean value of within-house spread. The standard deviation of SD, MAD, and MDEV refers to the spread of that spread. For example, mean SD of pct black refers to the mean standard deviation of percent black, for any individual house, estimated as a mean of all the standard deviations estimated for each house in the sample. The standard deviation of SD of pct black refers to the standard deviation of this standard deviation.

We see that within any given house, the variables vary far less than they do in the full sample. For example, within the full sample, the standard deviation of percent black is 34.75%. This means that is reasonable to compare two otherwise-identical houses in two neighborhoods whose percent black differ by 34.75%. However, within an individual house, the mean SD of percent black is 4.08%, the MAD is 2.39%, and the MDEV is 2.83%. This means that for an individual house, percent black will tend to vary over time by about 2 to 4 percent. These three measures of spread – SD, MAD, and MDEV – are themselves means. Therefore, the standard deviations of these three measures tell us how much the mean of each spread varies. For example, the mean SD of percent black is 4.08%, while the SD of that mean SD is 3.72%. This means that it is reasonable to vary percent black not only by 4.08%, but also by (4.08+3.72) = 7.80%. The percent black of the neighborhood of an individual house will tend to change by about 2 to 4 percent on average, but a change of 8 percent is also common.

Using these measures of spread within an individual house, we can interpret our FE regression results in table 10. First, percent black and percent renter are negative, meaning that neighborhoods with more blacks or renters are taxed less. This is true even when controlling for individual house fixed effects. Second, log median income is not statistically significant in the FE regression – not even at the 0.2 level – suggesting that homes in appreciating neighborhoods – in terms of income – are not assessed differently than homes in stable neighborhoods.

Next, we examine log median sale price. The FE coefficient is -0.221. This means that a one unit change in log median sale price decreases the assessment-sale ratio by 0.221. We see that the mean within-house SD of log median sale price is 0.121, while the SD of that mean SD is 0.102. This means that the largest reasonable change of log median sale price is (0.121+0.102) = 0.223. If anything, this is a liberal overestimate, because the MAD and MDEV are smaller than the SD. Using a change in log median sale price of 0.223, we find that the assessments-sale ratio decreases by 0.221 * 0.223 = 0.050. This implies a change in the assessment ratio from 0.902 to to (0.902 - 0.050) = 0.852. This is a rather large change, implying that homes in gentrifying neighborhoods are assessed and taxed significantly less. In the most rapidly appreciating neighborhoods, the assessment rate will fall from about 90% to about 85%. In such a neighborhood, the median sale price rose from exp(12.262) = $211,504.17 to exp(12.262+0.223) = $264,342.26 – an increase of 25%. Thus, when the median sale price increases by 25%, the effective tax rate falls by 5 percentage points.

The meaning of these results may be a matter of interpretation. Homes in relatively rapidly appreciating neighborhoods are assessed at a rate of about 85% instead of the normal rate of 90%. This means they save about 5% on their property taxes – a sizable savings, but not enormous. The sample standard deviation of the assessment ratio is 0.153 or 15.3%. This means that the reduction in taxes for the most rapidly appreciating neighborhoods is about $^1/_3$ of the reduction in taxes that might be expected due to ordinary variation. The saving is real, but modest. And the total tax bill is still increased, because the increase in the median sale price – 25% – is more than the decrease in the tax rate – 5%.

Next, we examine the percent difference above the median sale price. The FE coefficient is -0.00235. The mean standard deviation of percent difference price above is 9.952 and the standard deviation of that standard deviation is 14.921. Therefore, let us consider a home that is priced (9.952+14.921) = 24.873% above the neighborhood median. The assessment ratio will fall from 0.902 to (0.902 - 0.00235*24.873) = 0.844. Thus, when an individual home's price increases by 25%, its tax rate falls by about 6%. Once again, the increase in sale price is more than the decrease in the tax rate, so the total tax bill will still increase.

Finally, we examine education. Percent with a HS diploma and with a college degree are not statistically significant, but percent with a graduate degree is both negative and statistically significant, suggesting that more highly-educated neighborhoods are taxed less. Let us therefore interpret the magnitudes. The within-house mean SD of percent graduate degree is 3.947, and the SD of the SD is 3.580. The sum of these is 7.527, so let us interpret the consequence of a 7.527% increase in the percent with a graduate degree for a single house. The FE coefficient is -0.00128, so the effect is -0.00128 * 7.527 = -0.00963. In other words, when the percentage of a neighborhood with a graduate degree increases by 7.527%, the assessment ratio decreases by -0.00963 or about 1%. This is probably not a meaningful change in taxation.

Thus, our repeat sales model with individual house fixed effects shows no discrimination in terms of percent black, percent renter, median income, nor education. We do find modest discrimination in that homes in appreciating neighborhoods – in terms of sale price – are assessed at a moderately lower rate. The meaning of these results is a matter of interpretation. The use of individual house fixed effects should mitigate concerns of omitted variables bias.

Finally, we estimate one more repeat sales model, presented in table 12. This time, we use only the first and the last sale of each individual house, and we estimate the change in percent black and in log median sale price between the first and the last sale. In addition, we divide these changes by the number of years between one sale and the next to obtain annual rates of change.[33] Thus, we can determine whether the assessment-sale ratio is affected by changes in percent black or median sale price – both in absolute changes and in rates of change. We do not include fixed effects because these would be perfectly collinear with the changes.

In the first column, we regress the assessment-sale ratio on the absolute changes in percent black and log median sale price, along with their initial values and the number of years between the two sales. In the second column, we replace absolute changes with rates of change. Both columns include a full set of controls except for Census tract FE.

Our results show that percent black is not significant – neither as an absolute change nor as a rate of change. Only the absolute and relative changes in the log median sale price is statistically significant. The coefficient on the absolute change in log median sale price is -0.0735, meaning that a one unit change in the log median sale price is associated with a 0.0735 unit decrease in the assessed-sale ratio. The standard deviation of the absolute change in the log median sale price is 0.223. Thus, the change in the assessment ratio caused by a one standard deviation change in the log median sale price is -0.0735 * 0.223 = -0.0164. Thus, in a neighborhood where the log median sale price rises from exp(12.262) = $211,504.17 to exp(12.262+0.223) = $264,342.26, the assessed-sale ratio falls by 0.0164. In other words, when the median sale price rises by 25%, the tax rate falls by 1.64%. This is somewhat consistent with our earlier result, where we found that when the median sale price increases by 125%, the total assessment increases by "only" 107%. When a neighborhood's median price increases, the tax rate will fall slightly, but the decrease in tax rate is less than the increase in sale price, and so the total assessment will increase. This may indicate progressivity or regressivity, depending on one's interpretation.

The coefficient on the rate of change of log median sale price is -0.0778, while the standard deviation of the rate of change is 0.0863. This means that the log median sale price may increase by 0.0863 per year, implying the median sale

33 Years between sales is equal to months between sales divided by 12, so years between sale is a floating point number.

price increases by 9% per year from a base of exp(12.262) = $211,504.17. This implies a change in the assessment ratio of -0.0778 * 0.0863 = -0.00671414. Thus, when the median sale price increases 9% annually, the tax rate falls by 0.67%. This is too small to be meaningful.

Thus, we have failed to find any significant discrimination using repeat sales and the changes in percent black from one sale to the next. However, we have found some modest reduction in effective tax rates in appreciating neighborhoods. However, the decrease in effective tax rate is proportionally less than the increase in sale price, and so the total property tax bill still increases. The property tax may be regressive or progressive, depending on one's perspective.

IV. Conclusion

Rothstein (2017: 169-172) notes that according to several decades-old studies, some local governments had taxed blacks at effectively higher property tax rates than whites by over-assessing their property values while charging them the same nominal tax rates. However, Rothstein (2017: 172) concludes, "There are no contemporary studies of assessed-to-market value ratios by community and by race, so we cannot say whether discriminatory tax assessments persist to the present time, and if so, in which communities."

Therefore, I have tested whether this phenomenon occurs in contemporary Fulton County, the principal county of Atlanta, Georgia. In 1974, the NAACP sued Fulton County for racial over-assessment, making Fulton a suitable location to study (Price 1975). Using assessor data with assessed values and sale prices, and U.S. Census block group demographic data from the American Community Survey (ACS), I have tested whether there is any racial or socioeconomic discrimination in terms of the ratio of assessed-to-sale value. Two descriptive statistics – the COD (coefficient of dispersion) and the PRD (price-related differential) – show no evidence against horizontal and vertical equity, respectively. Tabulations of mean assessment ratios suggest that homes in neighborhoods with high percentages of blacks and/or renters are actually assessed and taxed at lower rates. These tabulations also suggest that homes in neighborhoods with higher average income and education are assessed and taxed at higher rates. Results are mixed, however, for sale price. Tabulations show that homes in neighborhoods with higher median sale prices are taxed more, but that individual homes with higher sale prices are taxed less.

Multiple regression results similarly show that homes neighborhoods with high percentages of blacks are taxed and assessed at lower rates. Coefficients on percent renter, median income, and education are too small in magnitude to be important, suggesting absence of discrimination. Homes in neighborhoods with a higher median sale price are assessed and taxed less. Homes that sell above the median in their neighborhood are taxed and assessed less, while homes below the median are taxed and assessed more. However, changes in sale price are larger than the associated changes in the assessment ratio, meaning that the property tax is still progressive, albeit less progressive than it could be. For example, a home in a neighborhood whose median price is 125% above the median will be assessed at 107% above the mean. In other words, a home that sells for more than double is still assessed at more than double. Under-assessment of above-average homes and over-assessment of below-average homes is a reasonable consequence of regression-based mass appraisal, because regression estimates will tend towards the mean of comparable homes.

A repeat sales model with individual house fixed effects still finds that homes in neighborhoods with more blacks are actually assessed and taxed less. Homes in appreciating neighborhoods enjoy a statistically significant but modestly sized tax discount between approximately 1% and 5%. Finally, a repeat sales model using changes in percent black and median sale price from the first sale to the last sale finds that when the median sale price increases by 25%, the effective tax rate falls by 1.64%. Once again, the property tax is progressive from one perspective but regressive from another, depending on one's perspective. But in general, owners of much more expensive houses will owe much larger property tax bills.

Thus, we have failed to find evidence of racial or socioeconomic discrimination. This does not mean that no discrimination exists – only that we have failed to find any. Nevertheless, it is encouraging to find that at least one of Rothstein's enumerated methods of discrimination does not appear to be active in Fulton County nor Atlanta, Georgia. If there is any discrimination, it appears to be too subtle to detect using my methods. This suggests that computerized, regression-based mass-appraisal may be sufficient to eliminate the type of discrimination that used to be common. This also suggests that the process for appealing one's assessment may be sufficiently accessible to most portions of the population. We cannot assert that computerized, regression-based mass-appraisal and/or the appeals process is/are the cause of the failure to find discrimination, but we can at least assert that there are no obvious flaws in these two institutions.

These null results are important for at least three reasons: first, as Rothstein (2017: 172) observes, there are few contemporary studies of this issue, and so even null results add to our knowledge. Discriminatory assessment was common several decades ago, but we have no knowledge of its frequency today. Second, if in fact discriminatory assessment is still a problem in some cities or counties, it is important for us to identify those jurisdictions where it is *not* a problem, so that we can identify models for non-discriminatory assessment process. It is easier to fix problems – if they exist – if we know where those problems do not exist. For example, McMillen and Singh (2019) find regressivity in Baltimore, Cleveland, Denver, and Philadelphia; Atuahene and Atuahene and Hodge (2018) in Detroit; Faulk and Hicks (2015) in the state of Indiana; and Lee (2004) in New Haven. It might be useful to compare the notices mailed to residents to see whether Fulton County's notices are easier to understand, and to compare the mass appraisal techniques to see whether there are any significant differences in their procedures and methods.

Third, the fact that residents of black neighborhoods are under-assessed could be evidence of a different problem. Some banks rely on county assessments when extending credit for mortgages. If homes in black neighborhoods are under-assessed, purchasers and owners may face a greater difficulty obtaining credit.[34] Future studies should combine data on county assessments with data on private assessments and loans to determine whether county under-assessment impedes access to credit, controlling for sale price.

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34 I thank Jonathan Rothwell of Perry *et. al.* (2018) for this point.

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Appendix 1: Details on data and methods

Removing duplicate observations was a complicated procedure. First, I dropped all blatant duplicate observations (in Stata, “duplicates drop”). Second, I concatenated parcel ID and sale date to create ID-by-date clusters. I dropped all clusters for which the sale price differed within the cluster. That is, I dropped all observations for which another observation existed for the same parcel ID and sale date but with a different sale price. If a home were recorded as being sold twice (or more) on the same day for different prices, I considered *both* observations to be unreliable and I dropped *both* of them. But remaining duplications were difficult to deal with. Some observations were near duplicates of each other, containing nearly identical values across all variables but one. For example, two observations might be identical in every way except for the name of the mortgage lender. With two observations being so nearly identical, there was little way to privilege one observation over another. At the same time, if two observations were nearly identical, it was unlikely that their values were corrupted or mis-measured, and so dropping such observations seemed unreasonable. To deal with such duplications, I used the same clusters of ID-by-date. Within each cluster, I sorted each observation by the number of missing values. Then I dropped all observations within each cluster except for the first observation. If these observations had different numbers of missing values, then – because of the sorting – the first observation would always be the one with the fewest missing values.

Note that sale price is inflated in 2016 dollars when used as an independent variable. In a typical hedonic regression, in which logged sale price is the dependent variable, including year fixed effects automatically accounts for price inflation. However, because our dependent variable is the assessment ratio, this strategy is not possible. Therefore, I use the CPI to inflate sale prices as an independent variable, in June 2016 dollars.[35] But when calculating the assessed-sale ratio, un-inflated values are used. Inflation factors are: 1.11 in 2010, 1.07 in 2011, 1.05 in 2012, 1.03 in 2013, and 1.01 in 2014 and 2015. I use June for any given year.

Several variables were used for two purposes: both to eliminate unsuitable sales as well as to control for characteristics of remaining sales. For example, sale evaluation was used to drop sales to charities and bankruptcies, and LUC was used to drop non-residential properties. At the same time, there are several different LUC and zoning classifications consistent with residential use, and these several residential types were used as control variables.

The ACS demographic variables I use to calculate my regressors are:

- Total population for all races (c02003e1)

35 Using https://data.bls.gov/cgi-bin/cpicalc.pl.

- Total black population (c02003e4)
- Median income in the last 12 months, in inflated dollars (b19013e1)[36]
- Renter-occupied housing units (b25014e8)[37]
- Total occupied housing units (b25014e1)
- Total population for all educational attainments, ages 25 and up (b15003e1)
- Population with HS diploma (b15003e17)
- Population with GED (b15003e18)
- Population with some college, < 1 year (b15003e19)
- Population with some college, ≥ 1 year (b15003e20)
- Population with associate's degree (b15003e21)
- Population with bachelor's degree (b15003e22)
- Population with master's degree (b15003e23)
- Population with professional degree (b15003e24)
- Population with doctoral degree (b15003e25)

The ACS gives total populations, while I convert these into percentages between 0 and 100.

In the ACS, each educational category includes only those with exactly that qualification. For example, those with a HS diploma includes only those with exactly a HS diploma and excludes those with any college education. But when I coded educational variables for regression, I coded each educational category as including all those above it. For example, the percent of the population with a HS diploma will include those with any college education or degree. Thus, each of my categories codes the percent of the population with at least that qualification. My educational categories are:

- Percent with a graduate degree – including master's, professional, or doctorate
- Percent with a degree – including associate's or bachelor's – plus the percent with a graduate degree
- Percent with some college – including more or less than one year – plus the percent with a degree[38]
- Percent with a HS diploma – including GED – plus the percent with some college
- Percent with no HS diploma – equal to 100 minus the percent with a HS diploma

36 The 2008-2012 wave of the ACS uses 2012-inflated dollars while the 2012-2016 wave uses 2016-inflated dollars. According to https://data.bls.gov/cgi-bin/cpicalc.pl, using the CPI, $1.00 in June 2012 was worth $1.05 in June 2016. Therefore, for all homes sold in 2010-2011 – which had been joined to the 2008-2012 wave of the ACS, median income was multiplied by 1.05.

37 Lee (2004) finds evidence of discrimination against neighborhoods with higher percentages of renters.

38 Percent with some college is not used as a regressor, to reduce multicollinearity.

Table 1: List of variables

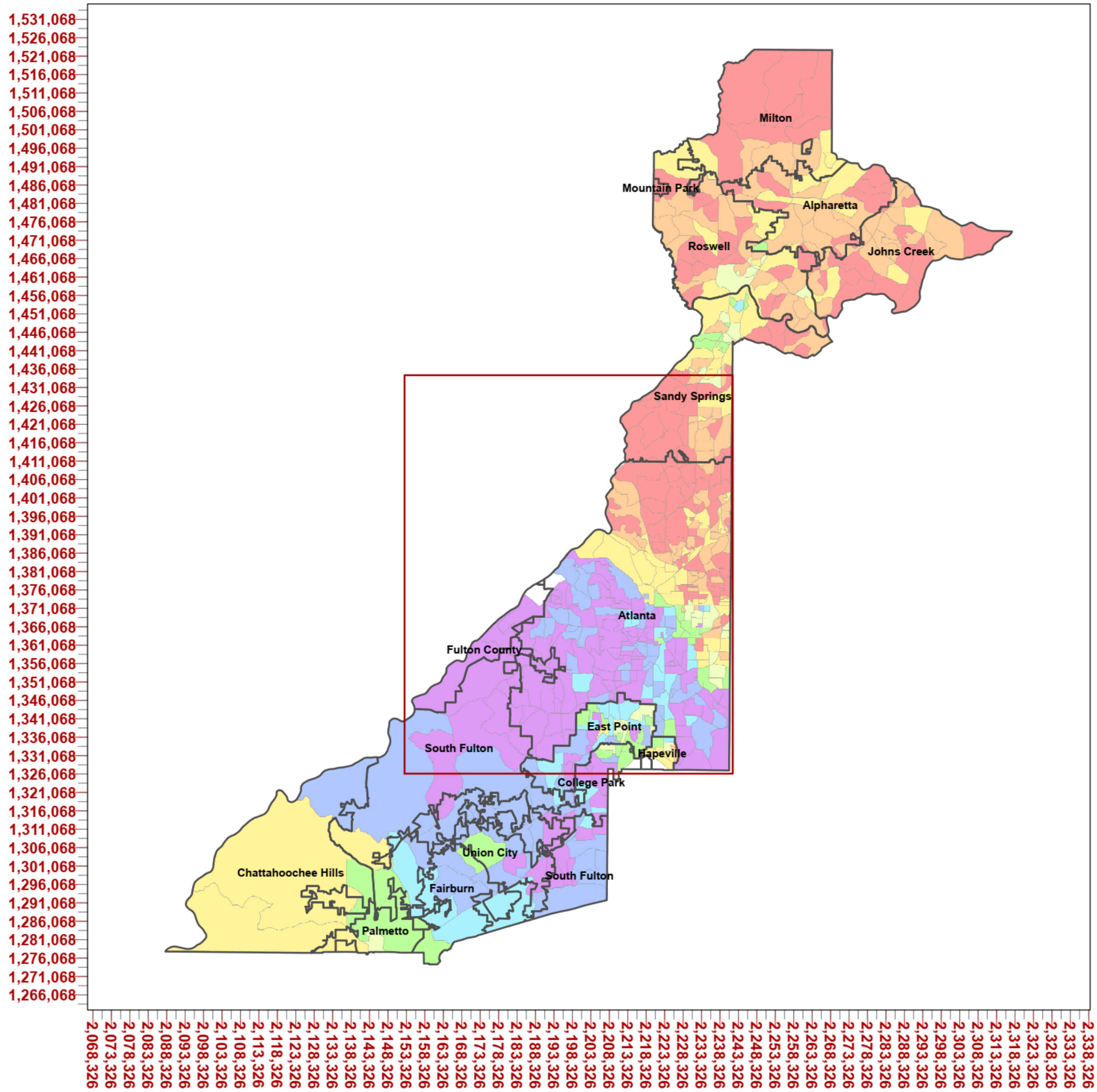
Variable	Description	Source
Assessed-sale ratio	100% fair market value (FMV) divided by sale price.	Fulton County Board of Assessors
Log sale price (infl)	As an independent variable, inflated in 2016 dollars and logged.	Fulton County Board of Assessors
Age & age sq	Age of individual home.	Fulton County Board of Assessors
Years since remodel	Year built minus year remodeled; if not remodeled, then years since remodel equals age	Fulton County Board of Assessors
Log acres	Acreage of individual home, logged.	Fulton County Board of Assessors
Stories	Number of stories of an individual home.	Fulton County Board of Assessors
Rooms	Separate variables for numbers of bedrooms, family rooms, and bedrooms.	Fulton County Board of Assessors
Fixtures	Plumbing fixtures: separate variables total fixtures, full fixtures, and half fixtures.	Fulton County Board of Assessors
Style	Style of individual home; categorical variable.	Fulton County Board of Assessors
Ext wall	Material construction of exterior wall of individual home; categorical variable.	Fulton County Board of Assessors
Heating	Type and presence of heating system of individual home; categorical variable.	Fulton County Board of Assessors
Fuel	Type of fuel system of individual home; categorical variable.	Fulton County Board of Assessors
Attic	Type and presence of attic of individual home; categorical variable.	Fulton County Board of Assessors
Fronting	Type of street fronting of individual home; categorical variable.	Fulton County Board of Assessors
Street	Type of street of individual home; categorical variable.	Fulton County Board of Assessors
Topo	Topography of individual home; categorical variable.	Fulton County Board of Assessors
Utility	Type of utility connection; categorical variable.	Fulton County Board of Assessors
Parking	Type, quantity, and proximity of parking; three categorical variables.	Fulton County Board of Assessors
Sale year & month	Dummy variables for each month and for each year.	Fulton County Board of Assessors
CDU	"Condition, desirability, utility" – i.e. physical depreciation. Categorical.	Fulton County Board of Assessors
Revision code	Codes for method of appraisal. Categorical.	Fulton County Board of Assessors
Deed type	Types of deeds. Categorical.	Fulton County Board of Assessors
Sale type	Types of sales. Categorical.	Fulton County Board of Assessors
Sale eval	Evaluation of how valid or invalid a sale is. Categorical.	Fulton County Board of Assessors
Location	Types of locations, e.g. residential, central business district, etc. Categorical.	Fulton County Board of Assessors
Lot type	Type of lot. Categorical.	Fulton County Board of Assessors
LUC	Land-use code. Categorical.	Fulton County Board of Assessors
Zoning	Zoning designation. Categorical.	Fulton County Board of Assessors
Condo	Binary variable indicating a condominium.	Fulton County Board of Assessors
Parcel district FE	Parcel district fixed effects.	Fulton County Board of Assessors
XY dist & dist sq	X and Y coordinates, in feet, according to the Georgia State Plane West coordinate system.	Fulton County GIS Portal.
City FE	City fixed effects.	Fulton County GIS Portal.
School FE	School attendance zone fixed effects.	Fulton County GIS Portal.
Census tract FE	Census tract fixed effects.	U. S. Census Bureau – American Community Survey
Pct black	The percent of residents in a Census block group who are black.	U. S. Census Bureau – American Community Survey
Pct renter	The percent of residents in a Census block group who are renters.	U. S. Census Bureau – American Community Survey
Log median income	Median income of a Census block group, logged.	U. S. Census Bureau – American Community Survey
Pct HS diploma	The percent of residents of a Census block group, ≥ 25 years, who possess a high school diploma.	U. S. Census Bureau – American Community Survey
Pct college degree	The percent of residents of a Census block group, ≥ 25 years, who possess a college degree.	U. S. Census Bureau – American Community Survey
Pct graduate degree	The percent of residents of a Census block group, ≥ 25 years, who possess a graduate degree.	U. S. Census Bureau – American Community Survey
Log median sale price (infl)	The median sale price of homes, by Census block group – wave, inflated in 2016 dollars, logged.	Author's calculation.
Pct diff price above	The percent difference between an individual home's sale price and the median sale price, if >0; else =0 if below	Author's calculation.
Pct diff price below	The percent difference between an individual home's sale price and the median sale price, if >0; else =0 if above	Author's calculation.
Pct CDU > "Avg"	The percent of homes with a depreciation rating better than "Avg," by Census block group-wave.	Author's calculation.
Pct CDU < "Avg"	The percent of homes with a depreciation rating worse than "Avg," by Census block group-wave.	Author's calculation.
Median age	The median age of homes, by Census block group – wave.	Author's calculation.
Log median acres	The median acreage of homes, by Census block group – wave, logged.	Author's calculation.
Median stories	The median stories of homes, by Census block group – wave.	Author's calculation.

Table 2: Summary statistics

	Obs	Mean	St. Dev.	Min	Max
Sale price (inflated)	51635	318908.4	343654.9	20000	6800850
Log sale price (inflated)	51635	12.2425	.9590783	9.903487	15.73256
Assessed-sale ratio	51635	.9022401	.1525802	.4666776	1.319863
Age	51635	30.2936	27.00812	0	195
Years since remodel	51635	26.44931	24.79585	0	162
Acres	51453	.3109345	.7627094	0	90.1
Stories	51635	1.454508	.5040533	1	3.5
Condo	51635	.2380362	.4258855	0	1
Rooms (total)	51453	6.593163	2.194919	1	77
Rooms (bed)	51602	2.977423	1.176443	0	14
Rooms (family)	51231	.4866585	.5313632	0	5
Fixtures (bath)	51635	2.16667	.98028	0	10
Fixture (half)	50660	.5210817	.5664806	0	16
Fixtures (total)	51635	11.12118	4.812136	0	58
Pct black	51635	34.91058	35.54655	0	100
Pct renter	51635	38.35807	24.03172	0	100
Pct HS diploma	51635	93.40051	7.499483	37.48933	100
Pct college degree	51635	78.78941	17.08339	10.68493	100
Pct college degree	51635	63.00347	21.8645	.9208103	100
Pct graduate degree	51635	24.07899	12.61343	0	73.45132
Median income	51346	85027.34	46083.88	6052.2	262501.1
Log median income	51346	11.20459	.559581	8.708178	12.47801
Median sale price (inflated)	51635	280206.8	209939.5	20200	1444500
Log median sale price (infl)	51635	12.25411	.808671	9.913438	14.18327
Median age	51635	28.49488	23.65598	0	99
Median acres	51634	.2211017	.2395197	.01425	2.4
Median stories	51635	1.453588	.4863964	1	3
Pct CDU > “Avg”	51635	83.22649	22.16947	0	100
Pct CDU < “Avg”	51635	3.565411	8.088582	0	100

Figure 1

Fulton County - Percent Black by Census Block Group



Percent Black by Census Block Group
Classification: Natural Breaks (Jenks)

C02003e4 / C02003e1

- 0% - 6.82%
- 7.18% - 17.56%
- 17.74% - 32.33%
- 33.26% - 50.38%
- 50.84% - 67.18%
- 67.92% - 80.84%
- 82.07% - 92.21%
- 92.53% - 100%

Projection: Georgia State Plane West, feet

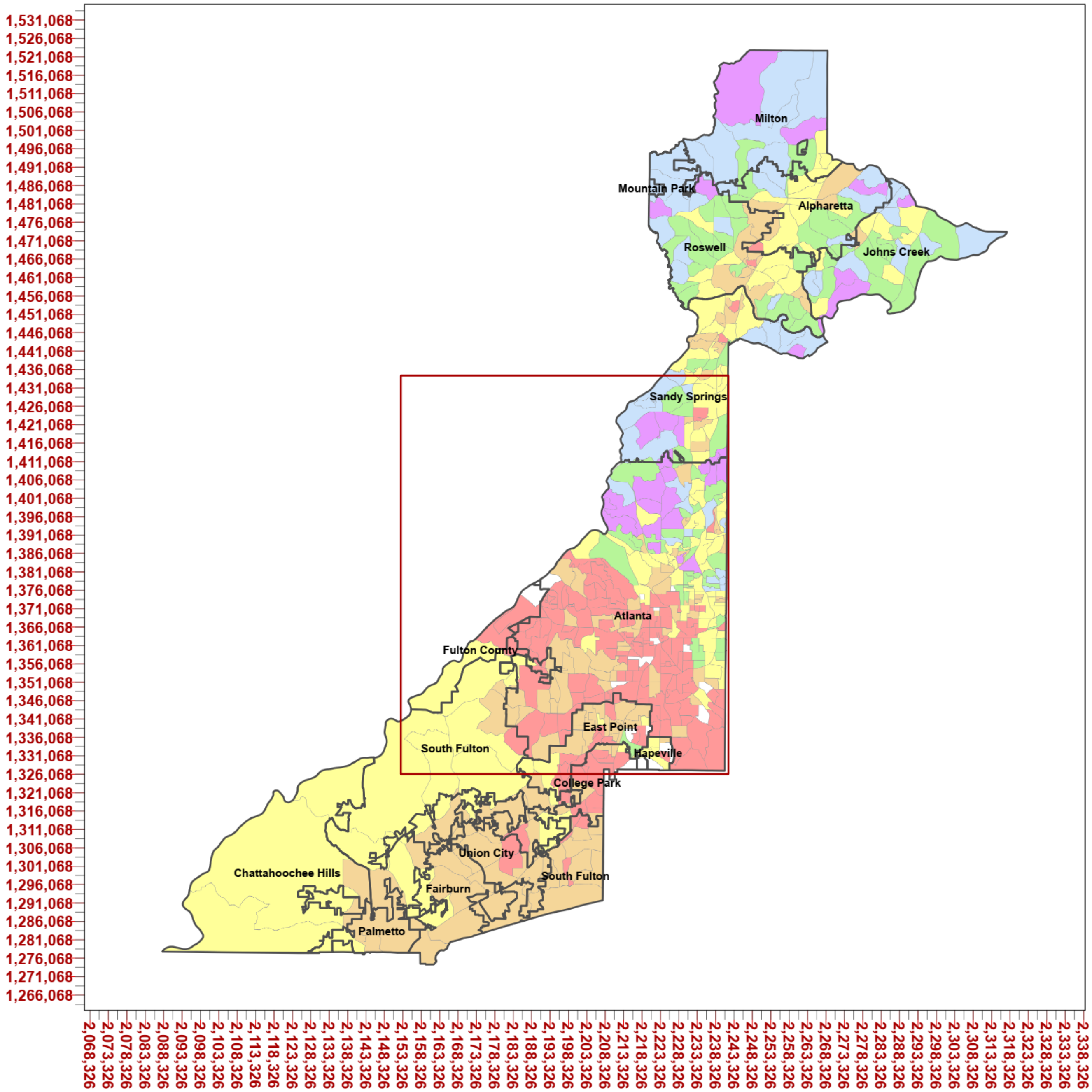
City Limits: Fulton County GIS Portal

Census Block Groups: ACS 2012-2016 - TIGER/Line
with Selected Demographic and Economic Data

Red box = Sub-sample
Lower left corner = (2,153,326;1,326,068) feet
Upper right corner = (2,241,567;1,434,734) feet

Figure 2

Fulton County - Median Income by Census Block Group



Median Income by Census Block Group
Classification: Natural Breaks (Jenks)

ACS 2012-2016

B19013e1

- 6815 - 34643
- 35032 - 58438
- 58657 - 87740
- 88727 - 125492
- 126071 - 178750
- 182833 - 250001

Projection: Georgia State Plane West, feet

City Limits: Fulton County GIS Portal

Census Block Groups: ACS 2012-2016 - TIGER/Line
with Selected Demographic and Economic Data

Red box = Sub-sample
Lower left corner = (2,153,326;1,326,068) feet
Upper right corner = (2,241,567;1,434,734) feet

Table 3: Measures of horizontal and vertical equity

COD	12.0302	PRD	1.0053	Gini assessment (G_a)	0.4774
	(0.0615)		(0.0008)		(0.0017)
		Mean ratio	0.9022	Gini price (G_p)	0.4792
			(0.0007)		(0.0016)
		Weighted mean ratio	0.8975	G_a - G_p	-0.0018
			(0.0009)		(0.0005)

Bootstrapped standard errors in parentheses (reps=1000).

COD: Coefficient of dispersion – a measure of horizontal equity.
PRD: Price-related differential – a measure of vertical equity.
Mean ratio: Numerator of PRD.
Weighted mean ratio: Denominator of PRD.
Gini coefficients are estimated by sorting by sale price, following McMillen and Singh (2019).
The difference – G_a – G_p – is a measure of vertical equity.

Table 4a: Mean assessment-sale ratios by quartile

Pct black Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.91443847	0.13015968	12945
2	0.91362428	0.13081102	12881
3	0.89361152	0.15975932	12930
4	0.88725611	0.1816542	12879
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Pct renter Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.9163842	0.13904005	12983
2	0.90410422	0.15051478	12922
3	0.89586964	0.15659538	12884
4	0.8924594	0.1622326	12846
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Median Income Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.88067549	0.18120747	12874
2	0.90461282	0.15072812	13123
3	0.905472	0.141932	12588
4	0.9196142	0.12742629	12761
Total	0.90254993	0.15234132	51346

Oneway Prob > F: 0.0000

Sale price Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.91087418	0.17348347	12909
2	0.8980461	0.16301648	12940
3	0.90165994	0.13823451	12879
4	0.8983884	0.13123528	12907
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Median Sale price Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.8790316	0.18390835	12923
2	0.9045858	0.1568924	12962
3	0.9081732	0.13553697	12878
4	0.91724268	0.12435846	12872
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Weighted Edu Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.88140274	0.18386293	12934
2	0.91267104	0.14852611	13010
3	0.90728104	0.13820845	12785
4	0.9076141	0.1323254	12906
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

City	Assessed-sale Ratio Mean	Std. Dev.	Freq.
Alpharetta	0.91613339	0.12692721	1807
Atlanta	0.894176	0.1558841	25603
Chattahoochie Hills	0.91929445	0.15065892	159
College Park	0.86741883	0.17613751	393
East Point	0.86810295	0.19140536	1386
Fairburn	0.88250526	0.1937308	669
Fulton County	1.1253456	0.17726551	2
Hapeville	0.86927624	0.19601126	241
Johns Creek	0.92913165	0.11842384	4956
Milton	0.92002873	0.11984392	1292
Mountain Park	0.88197426	0.20353965	19
Palmetto	0.91113987	0.19587524	216
Roswell	0.92392867	0.1144082	1823
Sandy Springs	0.91651991	0.13294119	6352
South Fulton	0.89958714	0.17474952	5747
Union City	0.88941254	0.17686939	970
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Table 4b: Mean assessment-sale ratios by decile

Pct black Decile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.91807556	0.12871452	5228
2	0.9139094	0.13219087	5726
3	0.91218711	0.12997527	4586
4	0.91792542	0.12640896	5226
5	0.90764581	0.13464627	5060
6	0.90072809	0.14399008	5214
7	0.88988741	0.16273619	5118
8	0.88052341	0.18328921	5175
9	0.89058229	0.17573554	5174
10	0.89039528	0.18578474	5128
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Pct renter Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.9194407	0.13236554	5237
2	0.91842189	0.1380996	5111
3	0.90780204	0.1537867	5153
4	0.90175563	0.15476796	5176
5	0.90392016	0.14392411	5228
6	0.89181478	0.16649793	5203
7	0.8979812	0.1516265	5051
8	0.89654063	0.15215346	5167
9	0.89445591	0.16024882	5156
10	0.89013439	0.16576408	5153
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Median Income Decile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.87553424	0.18530606	5143
2	0.88415398	0.17906733	5182
3	0.89683201	0.16946428	5255
4	0.90319957	0.14972217	5390
5	0.90379826	0.14588046	5027
6	0.90682538	0.14373542	4944
7	0.90021032	0.14551182	5074
8	0.91672002	0.13038248	5093
9	0.92309122	0.12330023	5205
10	0.91570071	0.13008592	5033
Total	0.90254993	0.15234132	51346

Oneway Prob > F: 0.0000

Sale price Decile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.93623057	0.16098491	5169
2	0.89682035	0.178578	5167
3	0.89029993	0.18180201	5155
4	0.89740835	0.16586022	5186
5	0.90148664	0.14904632	5172
6	0.90175914	0.1412184	5132
7	0.90218734	0.13839091	5166
8	0.90172908	0.13057458	5161
9	0.90250429	0.12315839	5171
10	0.89192713	0.13953123	5156
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Median Sale price Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.8676604	0.19142888	5261
2	0.88782021	0.18283946	5105
3	0.89427985	0.17144341	5140
4	0.90789033	0.15552528	5193
5	0.90177596	0.14909643	5186
6	0.90312197	0.13928905	5102
7	0.90752822	0.13369672	5193
8	0.92336924	0.12361037	5133
9	0.9177365	0.12223654	5367
10	0.91151036	0.13055159	4955
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Weighted Edu Quartile	Assessed-sale Ratio Mean	Std. Dev.	Freq.
1	0.87236045	0.19043149	5176
2	0.88397804	0.17995278	5209
3	0.90191066	0.16994401	5130
4	0.91136823	0.14646847	5222
5	0.91568415	0.14338275	5207
6	0.90737505	0.1409158	5163
7	0.90740619	0.13604772	5092
8	0.90947766	0.13283682	5316
9	0.91196087	0.13186753	4972
10	0.9011098	0.13432768	5148
Total	0.90224012	0.15258019	51635

Oneway Prob > F: 0.0000

Table 5: Summary statistics for percent difference between individual sale price and median neighborhood sale price, before and after trimming to remove outliers.

Variable	Note	Obs	Mean	Std. Dev.	Min	Max
Pct diff price	Before 1.5xIQR trim	51635	15.77934	82.95127	-96.5812	2287.006
Pct diff price	After 1.5xIQR trim	48401	0.7984574	41.52634	-96.5812	122.0163

Table 6: Summary statistics for sample trimmed to remove outliers of percent difference sale price. Percent difference (between individual sale price and median neighborhood) is separated into percent difference above and percent difference below.

Variable	Obs	Mean	Std. Dev.	Min	Max
Log median sale price	48401	12.26197	0.8052479	9.913438	14.18327
Pct diff price above	48401	16.61964	27.17498	0	122.0163
Pct diff price below	48401	15.82118	21.44906	0	96.5812

Table 7: OLS regression using percent difference between individual sale price and median neighborhood sale price.

Depvar: Assessed-sale ratio	(1) Baseline	(2) Individual house characteristics	(3) Location	(4) Avg nbhd	(5) XY	(6) City FE	(7) Parcel & school FE	(8) Census tract FE
Pct black	-0.0491****	-0.203****	-0.224****	-0.181****	-0.187****	-0.177****	-0.0575**	-0.0826***
	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.086)	(0.034)
Pct renter occupied	-0.0150	0.0424****	0.0509****	0.0241*	0.0209*	0.0269**	-0.0130	0.00139
	(0.211)	(0.002)	(0.000)	(0.102)	(0.154)	(0.069)	(0.338)	(0.928)
Log median income	0.0197	-0.0509***	-0.0409**	-0.0317*	-0.0289*	-0.0324*	-0.0500***	-0.0305*
	(0.348)	(0.023)	(0.067)	(0.151)	(0.181)	(0.138)	(0.013)	(0.181)
Pct HS diploma	0.0214**	0.0196*	0.0317***	0.0243**	0.0257***	0.0237**	0.0109	0.00599
	(0.085)	(0.131)	(0.015)	(0.065)	(0.049)	(0.072)	(0.413)	(0.688)
Pct college degree	0.0455***	0.0740****	0.0752****	0.0575***	0.0474***	0.0397*	0.0292	0.0196
	(0.045)	(0.001)	(0.002)	(0.016)	(0.047)	(0.102)	(0.218)	(0.466)
Pct graduate degree	-0.0754****	0.0286**	0.0140	-0.00469	-0.0177	-0.00332	0.00550	-0.00284
	(0.000)	(0.086)	(0.412)	(0.788)	(0.310)	(0.851)	(0.751)	(0.876)
Log median sale price (infl)	0.0442****	-0.253****	-0.285****	-0.210****	-0.227****	-0.230****	-0.375****	-0.456****
	(0.009)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pct diff price above	-0.103****	-0.164****	-0.166****	-0.182****	-0.181****	-0.182****	-0.196****	-0.203****
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pct diff price below	0.0914****	0.234****	0.256****	0.273****	0.275****	0.278****	0.307****	0.320****
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pct CDU > "Avg"				-0.0162	-0.00659	-0.00353	-0.0178	-0.0214
				(0.232)	(0.629)	(0.809)	(0.227)	(0.219)
Pct CDU < "Avg"				0.00779	0.00952	0.00611	0.00680	-0.00339
				(0.433)	(0.341)	(0.550)	(0.494)	(0.765)
Revision code	YES	YES	YES	YES	YES	YES	YES	YES
Deed type	YES	YES	YES	YES	YES	YES	YES	YES
Sale type	YES	YES	YES	YES	YES	YES	YES	YES
Sale eval	YES	YES	YES	YES	YES	YES	YES	YES
Revision code	YES	YES	YES	YES	YES	YES	YES	YES
Sale month & year	YES	YES	YES	YES	YES	YES	YES	YES
Individual house characteristics	NO	YES	YES	YES	YES	YES	YES	YES
Location type	NO	NO	YES	YES	YES	YES	YES	YES
Lot type	NO	NO	YES	YES	YES	YES	YES	YES
LUC	NO	NO	YES	YES	YES	YES	YES	YES
Zoning	NO	NO	YES	YES	YES	YES	YES	YES
Median age, log acres, stories	NO	NO	NO	YES	YES	YES	YES	YES
XY dist & dist sq	NO	NO	NO	NO	YES	YES	YES	YES
City FE	NO	NO	NO	NO	NO	YES	YES	YES
Parcel district FE	NO	NO	NO	NO	NO	NO	YES	YES
School FE	NO	NO	NO	NO	NO	NO	YES	YES
Census tract FE	NO	NO	NO	NO	NO	NO	NO	YES
N	48132	46769	46769	46769	46769	46769	46769	46769
R-sq	0.300	0.348	0.359	0.363	0.365	0.367	0.382	0.392
adj. R-sq	0.299	0.346	0.355	0.359	0.361	0.362	0.376	0.383

Standardized coefficient estimates
Standard errors clustered by Census Block Group-Wave
P-values in parentheses
Individual house characteristics include: age & age sq, remodel, log acres, stories, rooms, (total, bedroom, bathroom), fixtures (total, full, half), style, exterior wall, heating, fuel, attic, fronting, street type, topography, utilities, parking type & proximity, and CDU (depreciation).
* p<0.2 ** p<0.1 *** p<0.05 **** p<0.01

Table 8: OLS regression of sub-samples

Depvar: Asssessed-sale ratio	(1) Full county	(2) Sub-sample	(3) City of Atlanta	(4) Suburbs
Pct black	-0.181****	-0.125****	-0.109****	-0.228****
	(0.000)	(0.000)	(0.002)	(0.000)
Pct renter	0.0241*	0.0326**	0.0275*	0.0137
	(0.102)	(0.050)	(0.129)	(0.524)
Log median income	-0.0317*	-0.0298	-0.0311	-0.0316
	(0.151)	(0.221)	(0.262)	(0.351)
Pct HS diploma	0.0243**	0.0256*	0.0400***	0.00986
	(0.065)	(0.156)	(0.046)	(0.534)
Pct college degree	0.0575***	0.0727***	0.0567*	0.0601***
	(0.016)	(0.020)	(0.131)	(0.040)
Pct graduate degree	-0.00469	0.0109	0.00771	-0.0195
	(0.788)	(0.625)	(0.766)	(0.367)
Log median sale price (infl)	-0.210****	-0.213****	-0.187****	-0.255****
	(0.000)	(0.000)	(0.000)	(0.000)
Pct diff price above	-0.182****	-0.175****	-0.163****	-0.213****
	(0.000)	(0.000)	(0.000)	(0.000)
Pct diff price below	0.273****	0.275****	0.275****	0.304****
	(0.000)	(0.000)	(0.000)	(0.000)
Pct CDU > "Avg"	-0.0162	-0.00196	0.00422	-0.0377***
	(0.232)	(0.920)	(0.868)	(0.019)
Pct CDU < "Avg"	0.00779	-0.00130	0.0255*	-0.00410
	(0.433)	(0.924)	(0.101)	(0.767)
N	46769	29546	22955	23814
R-sq	0.363	0.374	0.385	0.361
adj. R-sq	0.359	0.368	0.378	0.354

Standardized coefficient estimates
Standard errors clustered by Census Block Group-Wave
P-values in parentheses
Specification is the same as in first regression table, column 4, "Avg nbhd."
* p<0.2 ** p<0.1 *** p<0.05 **** p<0.01

Table 9: OLS city fixed effect estimates

	(1) Assessment-sale ratio
Alpharetta	-0.114***
	(0.044)
Atlanta	
	(.)
Chattahoochee Hills	0.175***
	(0.034)
College Park	-0.135**
	(0.065)
East Point	-0.102*
	(0.112)
Fairburn	-0.172***
	(0.050)
Fulton County	0.955**
	(0.054)
Hapeville	0.00638
	(0.964)
Johns Creek	0.0772*
	(0.130)
Milton	-0.152***
	(0.013)
Mountain Park	-0.436***
	(0.026)
Palmetto	0.0308
	(0.837)
Roswell	0.0885*
	(0.188)
Sandy Springs	0.0524*
	(0.170)
South Fulton	-0.0167
	(0.723)
Union City	-0.0422
	(0.684)
Observations	46769
R-squared	0.366
Adjusted R-squared	0.361

Y-standardized OLS coefficient estimates
Standard errors clustered by Census Block Group-Wave
P-values in parentheses
Specification is the same as in first regression table, column 4, “Avg nbhd.”
* p<0.2 ** p<0.1 *** p<0.05 **** p<0.01

Table 10: Repeat sales model, individual house random effects and fixed effects

Depvar: Assessed-sale ratio	(1) FE	(2) RE
Pct black	-0.000808*	0.000113
	(0.100)	(0.733)
Pct renter	-0.000830***	-0.000173
	(0.033)	(0.401)
Log median income	-0.00461	-0.00655
	(0.806)	(0.635)
Pct HS diploma	-0.000121	0.000787
	(0.884)	(0.209)
Pct college degree	-0.000278	0.000455
	(0.602)	(0.209)
Pct graduate degree	-0.00128***	-0.000400
	(0.022)	(0.349)
Log median sale price (infl)	-0.221****	-0.121****
	(0.000)	(0.000)
Pct diff price above	-0.00235****	-0.00164****
	(0.000)	(0.000)
Pct diff price below	0.00483****	0.00296****
	(0.000)	(0.000)
Pct CDU > "Avg"	-0.000484	-0.000289
	(0.340)	(0.252)
Pct CDU < "Avg"	0.000765	-0.000132
	(0.543)	(0.791)
N	4025	4025
R-sq	0.486	

Hausman FE vs. RE: Prob>chi2 = 0.0000
Ordinary (non-standardized) coefficient estimates
Standard errors clustered by parcel ID (individual home)
P-values in parentheses
Variables are the same as in first regression table, column 7, "Parcel & school FE."
* p < 0.2 ** p<0.1 *** p<0.05 **** p<0.01

Table 11: Measures of spread within individual houses, across time – for interpreting individual house FE estimates. "Full sample" refers to ordinary summary statistics of independent variables. "SD," "MAD" and "MDEV" refer to measures of spread within houses. E.g., mean SD is the mean standard deviation within houses, across time. Thus, the standard deviations of SD, MAD, and MDEV are measures of spread of spread.

	Obs	Mean	St. Dev.	Min	Max
Assessed-sale ratio	4025	.9140848	.1580771	.4670732	1.319863
Pct black:					
Full sample	4025	33.36762	34.74757	0	100
SD	3893	4.078097	3.718928	0	35.57287
MAD	4025	2.385683	2.709038	0	25.15382
MDEV	4025	2.829571	2.663838	0	25.15382
Log median sale price (infl):					
Full sample	4025	12.25746	.7422592	10.12663	14.0486
SD	3893	.1214219	.1023298	0	.7319448
MAD	4025	.0697887	.0753088	0	.5175631
MDEV	4025	.0844199	.0737388	0	.5175631
Pct diff price above					
Full sample	4025	16.03176	26.81876	0	121.8887
SD	3893	9.95236	14.92079	0	85.89686
MAD	4025	6.243135	10.21967	0	60.73826
MDEV	4025	6.87676	10.55277	0	60.73826
Pct diff price below					
Full sample	4025	16.55497	22.10864	0	96.5812
SD	3893	8.478216	10.13073	0	59.803
MAD	4025	5.191274	7.007012	0	42.28711
MDEV	4025	5.873931	7.208257	0	42.28711
Log median income:					
Full sample	4025	11.19549	.5143531	9.246552	12.47801
SD	3893	.1219405	.1018242	0	.7547078
MAD	4025	.0710057	.0750362	0	.533659
MDEV	4025	.0845758	.0731296	0	.533659
Pct graduate degree:					
Full sample	4025	24.37361	12.36089	0	73.45132
SD	3893	3.947197	3.579852	0	27.74923
MAD	4025	2.307248	2.58385	0	19.62167
MDEV	4025	2.736767	2.565504	0	19.62167

Table 12: Repeat sales model, using absolute changes and rates of change

Depvar: Assessed-sale ratio	(1) Absolute changes	(2) Rates of change
Pct black – abs change	0.0000571	
	(0.911)	
Log median sale price (infl) – abs change	-0.0735****	
	(0.002)	
Pct black – rate of change		0.000486
		(0.706)
Log median sale price (infl) – rate of change		-0.0778*
		(0.139)
Pct black – initial value	-0.000118	0.0000269
	(0.785)	(0.949)
Log median sale price (infl) – initial value	-0.0407***	-0.0154
	(0.022)	(0.297)
Years btw sale	-0.00876**	-0.00999**
	(0.091)	(0.056)
Pct diff price above	-0.00104****	-0.00100****
	(0.000)	(0.000)
Pct diff price below	0.00142****	0.00133****
	(0.000)	(0.000)
Pct renter	-0.000151	-0.000217
	(0.562)	(0.396)
Log median income	-0.0331**	-0.0436***
	(0.062)	(0.012)
Pct HS diploma	0.000828	0.000675
	(0.315)	(0.409)
Pct college degree	0.000310	0.000298
	(0.500)	(0.514)
Pct graduate degree	0.000180	0.000256
	(0.762)	(0.667)
Pct CDU > "Avg"	0.000331	0.000148
	(0.320)	(0.644)
Pct CDU < "Avg"	0.000366	0.000285
	(0.588)	(0.674)
N	1852	1852
R-sq	0.579	0.576

Ordinary (non-standardized) coefficient estimates
Standard errors clustered by Census Block Group-Wave
P-values in parentheses
Variables are the same as in first regression table, column 7, "Parcel & school FE."
* p<0.2 ** p<0.1 *** p<0.05 **** p<0.01